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**Presentata da:** **DIOGO GERHARD CASTRO DE BRITTO**

**Coordinatore Dottorato** **Relatore**

**PROF. LUIGI FRANZONI** **PROF. GIULIO ZANELLA**

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# Essays on Unemployment Insurance

Diogo G. C. Britto[1]

[1]PhD Candidate at the European Doctorate of Law and Economics. E-mail: diogo.gerhardcastro5@unibo.it

*I dedicated this work to my family, Celia, Paulo, Douglas and Madalena, and to the memory of my dearest friend, Bruno Cangussu.*

*Dedico esse trabalho à minha família: Celia, Paulo, Douglas e Madalena e à memoria do meu grande amigo Bruno Cangussu.*

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# List of Abbreviations

LATE - Local Average Treatment Effect

MER - Minimum Eligibility Requirements

PSID - Panel of Social and Income Dynamics

RCT - Randomized Controlled Trial

RDD - Regression Discontinuity Design

RDK - Regression Kink Design

RMSE - Root Mean Square Error

UI - Unemployment Insurance

# Explanatory Note On Specific Terminology

- Bayesian Updating Process: it is a process of inference through which someone adapts its prior beliefs on the parameters of some stochastic variable. In this thesis' context, firms have some prior belief on the ability of workers to perform some job. Along time, by observing how much the employee is actually able to produce, firms update their belief on the employee's ability.

- Covered Unemployment Duration: this is the length of time which individuals spend unemployed conditional on receiving unemployment benefits.

- Experience Rated UI System: this is any UI system which sets higher contribution requirements for firms which lay-off more workers. The idea is making firms internalize the burden they impose on the UI system when they lay-off workers.

- Hazard Rate: it defines the risk of a failure event in a given period, conditional that the failure has not taken place up to that point.

- Lay-off Hazard Rate: this is risk that a worker is laid-off at a given tenure, provided that the worker has not been laid-off until that point in time. For example, it is the probability of being laid-off at the tenth month in a job, for those workers who have not been displace before the tenth month. Note that, during the paper, I use synonyms for this term such as job hazard rate and employment hazard rate.

- Lower order parameter of a model: it is a synonym to primitives of the model.

- Primitives of a model: In this thesis the primitives of a model are understood as in Chetty (2008). It refers to calibrations on the very basis of a model. For example, the specific functional form of the agent's utility function is a primitive of a model. Another examples are the degree of risk aversion or any specific parameters of the utility function.

- Quasi-Experimental Strategy: this refers to any econometric approach which resembles an ideal controlled experiment, where similar group of individuals are assigned to a treatment and a control group.

- Replacement ratio: this is the ratio between unemployment benefits and worker's previous earnings. In other words, it defines in percentage terms the amount of UI benefits which a unemployed worker receives with respect to his earnings before the job loss.
- Reservation Wage: it is the minimum wage offer which an unemployed workers is willing to accept.
- Spike: it refers to a sharp increase of something. During the text, I usually use together with job hazard rate. It, thus, means a sharp increase in the job hazard rate.
- Tenure: it is the length of a employment relationship. For instance, a worker with 12 months of tenure is someone who have worked for 12 months in a given firm.
- Unconstrained Unemployment Duration: it is the total length of time which an individual spends unemployed, irrespective of whether he expired or not unemployment benefits
- Unemployment inflow: this refers to the flow of workers coming into unemployment. During the text, I use this concept together with tenure. For example, unemployment inflow at the sixth month of employment, refers to the flow of workers becoming unemployed after having worked for six months in a firm. "Employment outflow" and "exit from employment" are also used as synonym for this term.

# Contents

# Chapter 1

# Introduction

Unemployment Insurance (UI) is a widespread policy around the world. It provides temporary income support to unemployed workers. Its main goal is to avoid that these workers experience large drops in consumption while unemployed. The policy was first introduced in the UK by the National Insurance Act in 1911 and slowly spread around the developed world. In the US, it was first introduced by the State of Wisconsin in 1932.[1] After the II World War, with the expansion of the welfare state, most of the developed world introduced unemployment insurance schemes. On the other hand, the adoption of such a policy in developing countries is definitely more recently and, generally, the generosity of benefits is low compared to the developed world.

Such far reaching policy has motivated the development of an extensive theoretical and empirical literature over at least the last five decades. It covers a variety of issues which arise when policy makers face the question of whether unemployment insurance should be in place and how it should be designed. This thesis is a combination of three independent stand-alone essays which aim to advance this literature and contribute to guiding policy on relevant aspects of unemployment insurance provision.

At this point, I introduce the research questions addressed in each of these three essays, the motivation and present my main findings. I also take the opportunity to discuss their relevance for policy. The introduction concludes with a section discussing the empirical methodology used throughout this thesis. This may be useful for the reader who is not familiar with techniques of causal inference. The following chapter discusses more in deep how these main findings relate and contribute to the existing literature on UI, and also discusses more in details how they may be relevant for policy. Then, the three essays are presented in chapter 3, 4 and 5. Finally, chapter 6 concludes by discussing the main results of this thesis. At the beginning of the book it is also possible to find a list of abbreviations and a explanatory note on some specific concepts with which a reader not familiar with the field may find useful.

[1] Zimmerman (1970)

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

The starting point for the first essay, presented in chapter III, consists of one observation regarding the literature on unemployment insurance as a whole. Probably its most well established empirical finding is that providing more generous unemployment benefits increases the length of time which individuals take to come back to the labor market. There is plenty of credible empirical evidence from a number of countries which consistently finds that UI affects the average duration of unemployment. More specifically, both the amount of unemployment benefits and the maximum potential duration raise the average duration of unemployment because of a moral hazard problem. The mechanism for this effect is simple: UI reduces the incentives for beneficiaries to actively search for job because once they accept a new job, their unemployment benefits are cut. In different words, UI constitutes part of the opportunity cost of starting a new job. A fairly large share of this literature is concerned with either measuring this effect or assessing the effectiveness of different policy designs in minimizing such distortion.

While moral hazard on search effort is clearly an extremely relevant issue, I notice that there are a number of straightforward reasons to believe that UI may also affect the behavior of workers while they are still employed. First, workers entitled to unemployment insurance should fear less unemployment risks because UI increases the value of their outside option. Therefore, they have less incentives to put effort into work in order to keep their jobs. Second, since the goal of UI is to cover workers who are unemployed against their will, the vast majority of UI programs do not grant workers who voluntarily quit their jobs with benefits. This feature provides a disincentive for quitting because as a worker voluntarily leaves his job, he is giving up unemployment benefits. Third, most systems have in place a minimum eligibility requirement (MER) where workers are only eligible for UI if they have been employed for a minimum length of time before dismissal. For example, in Brazil dismissed workers are only eligible for unemployment benefits if they have worked continuously in the last six months. Such feature provides incentives for individuals to work harder until meeting the minimum eligibility requirement. Fourth, and similarly, in many systems benefit generosity increases with tenure at displacement, providing again a further incentive for workers to keep their jobs for longer. For instance, again in the Brazilian system, maximum potential duration increases as workers reach certain specific levels of tenure.[2] Fifth, firms may also consider potential unemployment benefits of workers when taking lay-off decisions, even though the theoretical explanations are less clear cut. For instance, firms may take advantage of UI to temporarily lay-off workers. Also, they could expect that a worker is less likely to fight the dismissal in courts if entitled with unemployed benefits.[3]

[2] More specifically, worker are eligible to a maximum of 3 months of unemployment benefits if they are displaced with between 6 to 11 months of tenure. If they reach 12 and 24 months of tenure they are eligible to a maximum of 4 and 5 months of benefits, respectively.

[3] Tuit and van Ours (2010) suggest this last explanation.

Motivated by all these potential mechanisms, the first main question addressed in the first essay of this thesis is: can the level of potential unemployment benefits affect the average time which workers spend employed in an economically significant way? In other words, do employed workers (or firms) react to the potential level of unemployed benefits that they would receive in case they lose their jobs? If yes, is this reaction large enough to affect the average time that they spend employed in an economically meaningful way?

To answer to this question, my first essay exploits the Brazilian UI schedule linking workers' benefit level to their previous earnings. It takes advantage of an extremely rich administrative dataset on the Brazilian labor market to assess how (potential) benefit level affects the average duration of employment by applying a regression kink design,which is an econometric method to assess the effects of a given policy.[4] Surprisingly, I find that increasing benefit level by 1% increases the average time that workers spend employed by around 0.5% for low-income workers in Brazil, implying an elasticity of 0.5. This sizable estimate suggests that UI, at least in some context, can affect the average duration of employment spells in an economically sizable way. The follow-up question however is why should we care about this result and whether it is of any relevance for policy. There comes the second main question addressed in this paper: How does this effect matters for the optimal level of unemployment benefits, if it matters at all?

I address this question by building a theoretical model which generalizes Chetty (2008) to incorporate the effect described above. In his seminal work, Chetty makes two key contributions to this literature. First, he argues that the observed reaction of unemployment duration in response to unemployment benefits is not purely a result of moral hazard as pointed by the literature so far. Instead, such reaction is actually a composition between a liquidity and a moral hazard effect. Put differently, workers take longer to find new jobs when UI increases for two independent reasons: (i) UI alleviates their liquidity constrains, so that they can search for work "less desperately"; (ii) as previously known, UI creates a moral hazard problem because it is part of the opportunity cost of starting a new job. His estimates point that approximately 60% of the increase in unemployment duration as UI rises is due to the liquidity effect, while 40% is a moral hazard response. Moreover, the pure liquidity effect of UI relatively to the moral hazard effect is actually a measure of the welfare gains from providing the insurance. Such measure is the so-called *liquidity-to-moral hazard ratio*. Loosely speaking, it evaluates how much unemployment benefits help relaxing the liquidity constrains of unemployed workers and allow them to better smooth consumption.

Second, Chetty provides a sufficient statistics welfare formula to assess locally whether the current benefit level, in a given system, is above or below optimality. The key advantage of this formula is that it is built on a fairly general model in which no assumptions on the shape of the utility function

[4]The method is explained in details in Chapter 3.

are needed. There is no need to calibrate the model with primitives such as individual's degree of risk aversion[5]. The formula can be estimated by mainly recalling two key statistics from the data: (i) the liquidity-to-moral hazard ratio and (ii) the elasticity of unemployment duration to benefit level. Then it is possible to deliver a clear statement for policy on whether the current level of benefits should be marginally increased or decreased; or is close to optimality. The underlying intuition for the formula is that the optimal level of unemployment benefits comes from achieving the best balance between the welfare gains from liquidity provision (the liquidity-to-moral hazard) and the welfare losses due to moral hazard on search (the elasticity of unemployment duration to benefit level).

I depart from this model to assess how the optimality condition for UI benefit level changes when the duration of employment spells is allowed to vary with benefit level, as found in the empirical analysis. Analogously to Chetty (2008), a sufficient statistics formula assessing how welfare reacts to marginal changes in benefit level is derived. Similarly to Chetty, the derived formula depends on the liquidity-to-moral hazard ratio and on the elasticity of unemployment duration to benefit level. However, differently from Chetty, the elasticity of employment duration to benefit level plays a role: it actually affects welfare with the same order of magnitude as the elasticity of unemployment duration.[6] Put differently, the cost side of UI in Chetty's optimality condition depends only on how benefits affect the length of time which workers spend unemployed, which is characterized by the elasticity of unemployment duration to benefit level. In my essay, this optimality condition is affected by an additional element: how UI affects the length of time which workers spend employed, which is characterized by the elasticity of employment duration to benefit level.

When the elasticity of employment duration is positive, as in my estimates for low-income workers in Brazil, it positively affects welfare and constitutes a further reason for benefit level to be increased. The intuition behind this channel is: if raising benefits increases the average time which workers spend employed (and contributing to the UI system), when the policy maker raises benefit level, it needs to raise the tax levied on employed workers to finance the system by a lower amount than otherwise.[7] On the other hand, if this effect is found to be negative, it would call for a lower level of benefit than otherwise: if higher benefit level causes employment spells to be shorter, for a given increase in UI, the policy maker needs to increase by a higher amount the distortionary tax levied on workers while employed.

[5] Here the primitives of a model are understood as in Chetty (2008). It refers to calibrations on the very basis of a model. For example, the specific functional form of the agent's utility function is a primitive of a model. Another examples are the degree of risk aversion or any specific parameters of the utility function.

[6] Notice that Chetty's formula is fed by the traditional elasticity of unemployment duration to benefit level, as in most of the literature. This measures how unemployment benefits affect the length of time which workers spend unemployed. I instead look at the elasticity of employment duration to benefit level. It refers to how the (potential) availability of unemployment benefits affects the length of time which workers spend employed in a given job.

[7] It implicitly considers that the policy maker increases benefit level while keeping the government budget constrain balanced at the same time, by raising UI taxes levied on workers while employed.

Therefore, this theoretical result shows that the elasticity of employment duration to UI benefit level does matter for welfare. Combining this theory to the estimated elasticity of 0.5 for low-earners in Brazil indicates that welfare gains from marginally increasing UI benefits are higher with respect to an analysis which neglects this result. Therefore, the main contribution from this essay is twofold. First, it shows that, at least for some group of workers, UI may indeed affect the duration of employment. To the best of my knowledge, this is the first paper to directly estimate how the duration of employment spells reacts to unemployment benefits. Second, it provides a sufficient statistics welfare formula to assess the implications from the first result on optimal benefit level. It shows that when such elasticity is positive (negative) the welfare effect of a marginal increase in UI is higher (lower).

There also arises two further contributions from the theoretical model. First, in order to endogenize employment duration, I incorporate a minimum eligibility requirement to the model in such a way that workers are only granted with benefits if their employment spells last for a minimum of $k$ periods. In such a way, the relationship between MER and benefit level is also assessed in the welfare formula. Namely, welfare can also be affected by how the fraction of dismissed workers meeting minimum eligibility requirements reacts to benefit level. It imposes a cost on the system if more workers are granted as benefit increases because taxes on those working must be further increased. Thus, this is an addition to the original formula from Chetty. In this essay however, I do not attempt to estimate or assess whether such effects are of any relevance empirically.

Second, Chetty's welfare formula is actually composed by two elasticities related to unemployment: (i) the elasticity of unemployment duration conditional on receiving UI benefits to benefit level, and (ii) the elasticity of the full (unconditional) unemployment duration to benefit level. Differently, I argue that only the first term should matter for welfare, as shown by the formula derived in this first essay. This first of these terms in Chetty's formula correctly assesses how raising benefit level imposes an extra cost on the system, which must be compensated by higher distortionary taxes on employed workers. The second term instead captures the increased period of time during which workers remain out of the system: after benefits expire they neither contribute nor receive benefits. In Chetty (2008), this second elasticity matters because higher benefit level cause workers to contribute for less periods when unemployment extends beyond the maximum duration of benefits. I argue that this term should not matter because, while it is true that longer uncovered unemployment spells implies less contributions to the system, it is also true that uncovered unemployed workers are not at risk of requiring benefits from the system. In such way that they are neutral to the government budget. In this first essay I discuss at length this result.

In sum, the main contribution of this first essay is twofold. First, it shows that unemployment benefits can affect the duration of employment spells in an economically meaningful way. The literature so far has overlooked such effect. Second, since Chetty's welfare formula does not account for this finding, this essay generalizes Chetty's model to deal with this new effect and shows how the elasticity of employment duration to benefit level matters for the optimal level of unemployment benefits.

## 1.2 The Liquidity Gains from Unemployment Insurance in Markets with High Informality: Evidence from Brazil

In the second essay of this thesis, I turn my attention to the benefit side of providing unemployment benefits. As mentioned before, the key reason for providing such policy is avoiding that workers experience large drops in consumption because of unemployment. Indeed, one way to measure the welfare gains from UI is evaluating how large is the gap in consumption between employment and unemployment, as long noticed by Baily (1978). The market failure which justifies such intervention lies on imperfect credit and insurance markets. If workers were able to buy private insurance or to borrow freely while unemployed, there would be no need for public provision of UI, since they would be able to optimally smooth consumption across states. As briefly discussed above, Chetty (2008) provides a welfare formula for UI where he shows that welfare gains from UI can be evaluated by the liquidity effects on search. Unemployed workers react to the provision of liquidity by taking longer to find jobs because they can search less "desperately". This is actually a socially desirable response to the correction of imperfect capital and insurance markets.

Estimating such a response is therefore of paramount importance to correctly evaluate the overall welfare effects of providing UI. If liquidity responses happen to be small *vis-a-vis* moral hazard effects, which are detrimental to welfare, then unemployment insurance is probably not a desired policy. On the other hand, if liquidity effects are large with respect to moral hazard, providing and making UI more generous are a desirable course of action.

There is however rather scarce evidence on the liquidity effects.[8] Using data from Austria, Card et al. (2007) shows that granting unemployed workers with a severance payment equal to two monthly pre-taxes earnings decreases their average job-finding rate in the first 20 weeks of unemployment by 8%-12%. Basten et al. (2014) perform a similar application with data from Norway and estimate that a payment equivalent to 1.2 months decreases the average job-finding rate by 14%-16% within a year.

In this second essay, I take advantage of a "bonus policy" in Brazil which grants low-income workers a cash bonus of about half of their previous monthly earnings. I apply a regression discontinuity design (RDD) to assess the liquidity effect on unemployment outcomes. The RDD exploits the fact that only workers who have earned up to two minimum wages in the year before payment are eligible. Such eligibility condition allows me to compare workers who are barely eligible for the grant with workers

[8] Throughout this thesis, by "liquidity effects" I refer to the effect of providing liquidity to unemployed workers.

who are barely not eligible in order to assess the effects of the policy.[9] One advantage of this setup is that the cash grant is small with respect to previous earnings, and are also smaller than those explored in Card et al. (2007) and Basten et al. (2014). This mitigates the concern that estimations reflect not only a liquidity effect but also wealth effects. Put differently, if the cash grant is not negligible with respect to individual's life time earnings, their response may reflect changes in wealth rather than liquidity.[10]

A more interesting and key distinction from these two previous contributions is the Brazilian context from where estimations are derived. These are the first estimates of liquidity effects in a country where the share of informal labor market is large, as typical in the developing world. It is thus of primary importance for policy to assess whether and to what extent workers can benefit from unemployment insurance in such contexts. On one hand, liquidity constrains may be much more binding with respect to developed countries because capital markets are less developed and individuals in general are less wealthy.[11] This would strengthen the case for UI provision. On the other hand, the presence of a large informal labor sector in which entry and exit costs are much lower may help workers smoothing consumption while unemployed as they can more easily rely on informal jobs. This would call instead for a lower need for UI as a consumption smoothing device. Therefore, it is hard to evaluate the need for liquidity in such contexts by taking as a reference point the evidence from developed countries. Henceforth, the first contribution from this second essay is assessing the size of liquidity effects in a context of high informality.

I estimate that the probability of finding a new job within 12 weeks of unemployment decreases by around 0.65% baseline points in response to a cash grant of about half of worker's previous monthly earnings. Moreover, the effect on workers not entitled to unemployment benefits and with very low severance pay is about three times this figure. It suggests that, at least to some extent, UI in Brazil does accomplish the goal of alleviating liquidity constrains. The effects on the whole sample are substantial in comparison to Card et al. (2007) (about 120% larger) and similar to the estimates by Basten et al. (2014) (almost 20% higher).[12] Thus, unemployed workers in Brazil are liquidity constrained to a large extent, also in comparison to this previous evidence. This evidence suggests that there might be a substantial scope for UI provision in developing countries, where informal labor markets are prevalent.

This first finding just described is clearly relevant *per se*. However, a further question which naturally emerges is how large the liquidity effect is compared to the moral hazard effect. The key term which

[9]This strategy is discussed in details in chapter 4.

[10]In any standard life-cycle model of consumption behavior, the consumption of any income shock is smoothly spread over the individual's life if he is not liquidity constrained. Therefore, the wealth effect of a cash bonus which equals half-monthly earnings of an individual should have a negligible effect on consumption today because this amount is to be spread on consumption for the rest of the agent's life. Hence, any effect of a small (one period) cash grant should only significantly affect consumption today because of liquidity constrains rather than wealth effects.

[11]See Rajan and Zingales (1998) for the argument on capital markets.

[12]These are approximate comparisons based on a constant hazard ratio function.

determines the welfare gains from UI is actually the ratio between the liquidity and moral hazard effects, based on Chetty (2008) and in line with my first essay. Even if liquidity effects are found to be large in Brazil compared to developed countries, gains from UI provision might be low if it is the case that moral hazard effects are too large. Hence, the second contribution from this second essay is fully estimating the so-called liquidity-to-moral hazard ratio.

To evaluate this ratio, it is then necessary to estimate the moral hazard effect. Hence, a second regression discontinuity strategy is performed in this essay. I exploit the fact that workers dismissed with more than 24 months of previous tenure are entitled to one extra month of unemployment benefits.[13] Comparing workers dismissed slightly before and after the 24 months threshold allows me to estimate the effect of an extra month of benefits on job-finding probabilities which is a composition of liquidity and moral hazard. I combine this with the estimates on the pure liquidity effects (based on the bonus policy) to isolate the moral hazard effect based on theoretical results by Landais (2014). Then, it is possible to finally estimate the liquidity-to-moral hazard ratio.

I find that, around the tenure threshold, an extra month of potential duration decreases the job-finding probability by 1.9% in the first eight weeks of unemployment. This estimate suggests an elasticity of UI covered unemployment duration to potential duration of around 0.1.[14] An upper bound for the elasticity of unconstrained unemployment duration is around 0.15.[15] This is actually fairly lower than previously found in the US. For example, Landais (2014) finds a figure around 1.3 and 0.3 for the constrained and unconstrained elasticities, respectively. Such difference is in line with Gerard and Gonzaga (2013) who observe that most unemployed workers in Brazil do not come to back to the formal labor market. They argue that since most beneficiaries exhaust UI benefits anyways, there is little space for opportunistic behavioral responses to UI and therefore moral hazard is likely to be low.

Finally, I estimate the liquidity-to-moral hazard ratio to be around 0.98. There are only two estimates on this ratio in the literature, both for the US. Chetty (2008) exploits variations in severance payments addressing endogeneity by using a variety of controls and finds the ratio to be around 1.5. Landais (2014) estimates a lower ratio of around 0.88 by applying a regression kink design, which is likely to deliver more reliable estimates than Chetty (2008). Interestingly, overall benefits from providing liquidity to workers *vis-a-vis* moral hazard responses in Brazil are sizable and similar to the US. It indicates that about 50% of the increase in unemployment duration caused by UI is a response to relaxing workers' liquidity constrains, which is a socially desirable response to welfare. I further discuss in the essay that these

[13] I follow the same strategy proposed by Gerard and Gonzaga (2013) who are interested in different unemployment outcomes.

[14] The covered unemployment duration is the length of time which individuals spend unemployed conditional on receiving unemployment benefits

[15] Unconstrained unemployment duration is the total length of time which an individual spends unemployed, irrespective of whether he expired or not unemployment benefits

findings from local analysis are not likely to be driven by differences in UI generosity between Brazil and US: both system are similar in terms of potential duration (around 20 weeks) and, if anything, replacement ratios are higher in Brazil.[16] The key contribution of this essay is being the first to assess the liquidity effects of UI and the liquidity-to-moral hazard in a context of high informality.

Furthermore, these results go against the common suspicion that providing UI in developing countries is unlikely to be beneficial to welfare because informality is large. They complement the results by Gerard and Gonzaga (2013) who, differently from this essay, study the cost side of UI policies in Brazil. They argue that the efficiency cost of providing UI when informality is high are low because most workers do not come back to the formal labor market in any case. My results instead provide an answer to the natural question arising from Gerard and Gonzaga's analysis: are the welfare gains from UI provision also sizable in developing countries like Brazil? Putting together these results indicates that increasing unemployment benefits in Brazil may enhance social welfare.

## 1.3 Unemployment Insurance, Unemployment Inflow and Work Effort

In the first essay of this thesis, I study how employment spells react to unemployment insurance in terms of average duration. This is an interesting outcome *per se*, since the theory provided shows that this response has direct effects to welfare. Nevertheless, it is also interesting to go beyond this analysis and better understand the mechanisms behind this response. This third essay ought to contribute to better understanding how workers' behavior react to UI while still employed.

More specifically, this essay departs from a finding in the UI literature which relates the risk of a lay-off to minimum eligibility requirement for benefits. In different studies, it has been reported that the probability of a lay-off sharply spikes once workers reach the minimum eligibility requirement for UI. In other words, once employed workers qualify for unemployment benefits, the probability that they are fired sharply increases. This pattern has been documented at least for Canada and Spain.[17] However, the channels behind this effect are not fully understood. In broad terms, it could be driven by a labor demand or supply response. As regards the demand side story, one could argue that firms prefer to lay-off workers who are eligible for unemployment benefits, as already mentioned above. For example, firms may consider UI as an opportunity to temporarily lay-off workers. Or, firms may expect workers to be less likely to fight the dismissal if they are eligible for UI. On the supply side story, it could be that workers lower the effort they put into keeping their jobs once they are eligible for benefits, causing the spike in hazard rates.

[16] Replacement ratio is the ratio between unemployment benefits and worker's previous earnings.

[17] Christofides and McKenna (1995, 1996); Baker and Rea Jr (1998); Green and Sargent (1998) for Canada and Rebollo-Sanz (2012) for Spain.

Both explanations are by no means exclusive to each other. Also, from an empirical prospective it is hard to disentangle the role of each of them. Therefore, in this third essay, I focus on assessing the plausibility of the supply side story: I provide evidence suggesting that it is a relevant channel for explaining the spike of hazard rates at MER.[18]

First, using data from Brazil, I show that there is indeed a spike in lay-off probabilities at MER. In Brazil, workers qualify for UI once they reach six months of tenure in a given job. Thus, I show that there is a statistically significant discontinuity in lay-off hazard rates once workers reach six months of tenure. This extends the evidence that minimum eligibility requirements affect job hazard rates. It shows that such finding also holds in the Brazilian labor market, where informality is large.

Then I provide some further empirical results which allows one to gain some insight on the mechanism behind the effect. First, it is shown that the discontinuity is much larger for low-income workers with respect to high-earners. This is what one would expect since replacement ratios are much higher for low-earners and, therefore, UI incentives are stronger for this group of workers. Second, it is shown that the discontinuity is larger for small firms *vis-a-vis* large firms. If one accepts that unions are stronger in larger firms, this result goes against the idea that firms prefer to lay-off workers entitled to UI because of unions. Thus, it is at odds with one possible channel through which the demand side explanation may work.

Second, in this essay I develop a learning model with work effort. The goal is evaluating whether the supply side story can explain the whole hump-shaped pattern of lay-off hazard rates over tenure, as found in the data. A first underlying mechanism for firings in the model is borrowed from a shirking model. Firms use the threat of unemployment to incentivize workers to put effort in their jobs. In such model, MER causes a spike in lay-offs because once workers qualify for UI, they are less afraid of unemployment and, accordingly, they decrease their level of work effort. This causes hazard rates to discontinuously increase at MER. Another mechanism causing lay-offs in the model is a learning process where firms and workers learn about the quality (productivity) of their match over tenure. If at some point of this process the (updated) expected quality of the match becomes too low, the worker is dismissed. It is shown in chapter 5 that this second process is able to reproduced the hump-shaped pattern of lay-off probabilities over tenure. Overall, the model shows that the supply side story can consistently explain the pattern found in the data.

Finally, I use data on absenteeism as a proxy to work effort in order to assess the plausibility of the link between work effort and unemployment insurance. Weekly data on absenteeism was provided by

[18]In survival analysis, the hazard rate defines the probability of failure conditional on having survived until the previous period. In this context of job tenure, it characterizes the probability that a worker is dismissed in a given month of tenure, given that this worker is still employed at the previous month.

a Brazilian car dealer company employing around 400 workers. I show that there is a clear spike in absenteeism exactly at the sixth month of tenure, coinciding with MER. It suggests that work effort does decrease once workers become eligible for UI. Even though the external validity of this result is not clear, I interpret this as supporting evidence for the model.

Overall, the evidence provided in this essay suggests that the supply side explanation plays a role in explaining the spike in lay-off hazard rates caused by UI. These results might be relevant for policy as it indicates that there might be a link between unemployment insurance and work effort, which could harm productivity. Such evidence is rather scarce in the literature. If it is the case that UI negatively affects productivity, policy makers should also take this finding into account when setting the generosity of unemployment benefits. This evidence also links to the theoretical model by Wang and Williamson (1996), which is one of the few models in the literature to consider job-retention effort endogenous to unemployment insurance. They show that if, besides from affecting search effort, UI also increases the inflow into unemployment, the optimal profile of benefits may not be strictly decreasing, as found by other studies. Instead, they suggest that the optimal profile entails very low benefits at the very beginning of the unemployment spell in order to discourage low work effort and unemployment inflow. Then, benefits should increases up to a certain point and start decreasing in order to discourage too much moral hazard on search effort.

## 1.4 Discussion on Empirical Methodology

In order to answer many of the questions which are addressed in this thesis, it is necessary to evaluate empirically the effects of different policy features on relevant outcomes. For example, in my first essay I am interested on how the level of unemployment benefits set by the policy maker affects the length of time which workers spend in a given job. In other words, is it the case that offering more generous UI benefits changes how much time a worker stays in his job? To answer to this question I build on the empirical framework called causal inference.[19] Conceptually, this framework asserts that the effect of a given treatment (or policy) is the difference between what happens when the treatment is in place compared to what would have happened if the treatment were not in place. Still in the example, what is the effect of a $100 increase in UI benefit level on the length of time which a given worker spends in a job? Conceptually, it is the difference between the duration of his job when benefit level is increased and the duration of his job in the situation in which there is no benefit increase. Thus, to evaluate the effect of the increase in benefits it would be necessary to measure the time which this worker spends employed in two different worlds: one in which the UI increase is in place and the other one in which there is no increase - *the counterfactual world.*

[19] Angrist and Pischke (2008) is a key reference for a rather complete discussion of causality in econometrics

Of course, this is a conceptual exercise: once a policy change is implemented, it is impossible to observe the counterfactual world in order to know what would have happened without the policy change. A tempting, but misleading, strategy to overcome this problem is comparing groups of workers which are assigned by the policy with different levels of treatment. In the example, suppose that unemployment benefits is set by the policy as an increasing function of wages. One could compare job duration of workers with high and low benefits to estimate the effects of increasing UI benefits. However, in this case, workers who receive higher benefits are also workers who earn more. This means that these groups of workers are different with respect to some characteristics, and some of these might not even be observed by the researcher. For example, it could be that workers earnings more have higher ability than those with lower wages. It implies that differences in job duration between these two groups of workers cannot be attributed with certainty to differences in UI benefits. It could well be that any difference in behavior is driven by any other characteristic such as wages or ability. Put differently, by following such strategy one is comparing apples with bananas.

This is the reason why observing a correlation does not imply that the relationship between two variables is causal. Therefore, to assess causality one must overcome the problem of confounding factors described above. In other words, to infer causal relationships one cannot compare subjects which are different to each other because any differences in outcomes may be due to differences in a number of characteristics. Instead, one must compare apples with apples.

### 1.4.1 The Ideal Controlled Experiment

The solution to this problem which is considered to be ideal in econometrics is the so-called *randomized controlled trial* (RCT). This approach is used by the pharmaceutical industry to test the effects of medicines for instance. A RCT consists of randomly selecting two different groups from a given population. One of the groups receives the treatment and is called "treatment group" while the other group receives no treatment and is called "control group". Evaluating the effects of a treatment is simple in this case: it is enough to compare what happens to the treatment group *vis-a-vis* what happens to the control group. The key feature of this experiment is that since the two groups are chosen randomly, they are composed by individuals with the same (or very similar) characteristics.[20] Therefore, the underlying idea is that if the treatment group received no treatment, it would behave exactly in the same way as the control group. In other words, since the two groups are similar, they represent a credible counterfactual to each other. In such a way, any differences in outcomes between these two groups must be due to the treatment. Here is the comparison between apples and apples.

Back to the example, one could evaluate the effect of a $100 UI benefits increase using a RCT. It would consist of randomly selecting two groups from the population of workers in a given country and

[20] Provided that the sample is large enough.

assigning the first of them with the UI increase (treatment group) while leaving benefits for the second group unchanged (control group). Then, the effect of the increase on job duration could be measured by looking how the average time which workers of these two groups spend in their respective jobs.

### 1.4.2 Quasi-Experiments: The Regression Kink and Discontinuity Designs

As one can easily imagine, it is most often not possible to implement randomized controlled trials to evaluate most policies because of several moral and political constrains, for example. Therefore, econometricians have developed over the years a number of techniques to evaluate the effects of different policies without the need to rely on randomized trials. In general, these techniques are called "quasi-experimental techniques" because they consist of strategies which resemble randomized controlled trials where one group receives a treatment and a similar (control) group does not. If applied correctly, these techniques deliver an evaluation of policy effects which are as credible and reliable as in a RCT.

Throughout this thesis, I apply two of these techniques which follow a similar intuition. The first of them is called *Regression Discontinuity Design*. This strategy exploits a policy criteria used to define which subjects are and are not eligible for a given policy. The idea is that individuals barely ineligible for the policy are *ex-ante* similar to those barely eligible and can work then as good counterfactual.

For example, in chapter 4 I evaluate the effect of a policy grating a cash bonus to some workers in Brazil every year. I am interested in assessing how the cash grant affects the average time which unemployed workers need to find a new job. To do so, I apply a RDD which exploits the policy rule which states that only workers earning up to two minimum wages in the previous years are eligible to the policy. This design departs from the idea that the group of workers who earn just a little more than two minimum wages should be very similar to the group of workers earning just a little less than this value. In other words, if the minimum wages is \$150, those workers earning between \$300 and \$301 should be very similar to those workers earning between \$299 and \$300. The idea is that the selection into these two groups is as good as random. Yet, only the second group is eligible for the cash grant, according to the policy rule. Therefore, to evaluate the effect of this policy, I compare how the second group (treatment group), eligible for the cash grant, behaves with respect to the first group (control group), which is composed of similar workers but not eligible for the grant. At this point, it also becomes clear why the RDD falls within the group of quasi-experimental techniques: it resembles a randomized control experiment (or trial).

The *Regression Kink Design* follows the same idea with the difference that the policy threshold imposes only marginal differences in treatment. In other words, instead of having individuals around a threshold which are eligible or not for a cash grant, there are individuals which receive marginally different amounts of cash grant around the threshold. Then, the randomization around this threshold is exploited and the outcomes of groups with marginal different amounts of treatment are compared.

# Chapter 2

# Relevance for the Literature and Policy

In this chapter, I provide a selected review of the UI literature which relates to the essays developed in this thesis. The goal of this chapter is to pin down more clearly the contributions from these essays and how they relate to policy. I start with a brief exposition of the reasons motivating the public provision of unemployment insurance. For a more throughout review of literature, see Tatsiramos and Van Ours (2014); Fredriksson and Holmlund (2006).

## 2.1 Why Do We Need Unemployment Insurance?

The first fundamental question one should ask when analyzing the economic desirability of unemployment insurance policy is clear: how can social welfare gain from unemployment insurance? In other words, what is the benefit side of UI? It is possible to find an answer to this question already in Baily (1978). He studies the main aspects which determines the optimal provision of unemployment insurance. Interestingly, already at that time, he notices that a great deal of research effort has been made to evaluate the adverse aspects of providing unemployment insurance and how it increases unemployment. However, he argues, even though these results are certainly useful, "they do not go directly at the policy issue. They do not tell us whether or not the value of the existing UI program outweighs its costs". Therefore, in order to analyze the economically desirability of UI programs, it is first necessary to understand the rational for its provision.[1]

As pointed in Baily's work, the key welfare gain from this policy comes from consumption smoothing benefits: UI avoids that workers experience drops in consumption which are too large when unemployed. The reason why large discrete variations in consumption cause utility losses is simple. It comes from the standard result in microeconomics which shows that, if individuals are risk averse, their optimal consumption path over time is smooth. Hence, the goal of unemployment benefits is helping individuals to smooth consumption when in between employment spells.

[1] Also interesting is the fact that this seminal work in the UI literature by Baily was originally written for the Office of ASPER of the U.S. Department of Labor in 1975.

At this point, the next question which arises is: why is it the case that workers cannot do that by themselves? The first reason is that insurance markets are imperfect. It is well-known that private insurance markets suffer from adverse selection: riskier individuals self-select into the insurance, which in turn makes premiums more expensive and, thus, the insurance is not profitable for low risk individuals. As a result, private insurance markets are inefficient and public provision with mandatory insurance is more efficient. Of course, a large number of private insurance markets do exist despite this problem. As regards unemployment insurance specifically, such issue seems to be strong enough to prevent the private provision of the insurance. Indeed, it is rare to observe unemployment insurance policies provided by private insurance companies, even in countries in which (public) UI is not available or very limited. This is the first reason for UI to be provided by the government (Boeri and Van Ours, 2013).

The second reason lies on imperfect credit markets, which suffer both from adverse selection and moral hazard. Most often unemployed workers are unable to borrow in credit markets or can only borrow at rates which are too high. This implies that, in general, workers cannot fully resort to capital markets in order to smooth consumption while unemployed. This completes the rational for public provision of UI.[2] However, a further question which arises is: why workers cannot rely on savings? To some extent workers can indeed rely on savings, however it is not the optimal instrument to deal with unemployment risks. The reason is that savings are a good instrument to deal with expected and certain fluctuations of future income, as retirement for example.[3] It is not the best response to unemployment risks because while savings represent a certain loss in current consumption, unemployment is a contingent state: it happens with some probability but is not a sure thing. Furthermore, the duration of unemployment spells is also uncertain. Therefore, the most efficient instrument to deal with unemployment risks is insurance. This also partially answers the question of why the state should provide unemployment insurance instead of credit schemes for the unemployed.

In sum, the main benefit from providing unemployment insurance is consumption smoothing: avoid that unemployed workers experience drops in consumption which are too high. The rational for public intervention with UI policy is based on imperfect insurance and credit markets.

## 2.2 Why Full Insurance Is Not an Optimal Policy?

At this point, the natural question which emerges is how much insurance should be provided by the policy maker. Can we fully insure workers against unemployment risks? The answer is obvious not because the insurance also causes distortions on incentives which are detrimental to welfare. Public

[2]See Zeldes (1989); Johnson et al. (2006); Blundell et al. (2008) for more throughout discussions and evidence on imperfect credit and insurance markets.

[3]This naive example supposes that one can perfectly predicts the duration of his own life, in the sense that there is no uncertainty regarding the period in which she will not be working.

provision of UI solves the adverse selection problem to the extent that the program is usually mandatory. However, public provision yields no advantage over private markets as regards the also well-known problem of moral hazard.[4] This problem arises because once unemployment workers are granted unemployment benefits, they have less incentives to search hard for a job. In other words, the moral hazard problem is a consequence of the fact that once workers start a new job, they lose unemployment benefits. The fundamental source of this problem is the fact that search effort is not observable or verifiable by the policy maker. If it was possible to make unemployment benefits contingent on search effort, UI could be provided without yielding any distortion on search and optimal UI provision would be much higher.[5]

These considerations highlight the fact that optimal UI provision is a second best solution. The first best solution would be fully insuring workers and, at the same time, conditioning unemployment benefits payments on a given optimal level of search effort. Since this is not possible, in most of this literature the optimal level of UI is a partial insurance which balances the welfare gains from consumption smoothing against the distortions on search effort.[6]

Another reason why full insurance should not be a desired policy is that UI may also affect unemployment inflow. As mentioned before, UI affects the incentives of workers to keep a job and may induce higher inflow into unemployment because of low work effort. Since the policy maker cannot make unemployment benefits contingent on minimum levels of work effort because the latter is not observable, this causes an additional distortion which need to be taken into account when setting the optimal level of unemployment insurance. Furthermore, UI may also affect unemployment inflow by distorting firm's incentives to lay-off.

In summary, the main goal of providing unemployment insurance is allowing workers to better smooth consumption while unemployed. This constitutes the key welfare gain from the policy. However, it is not desirable to fully insure workers in such way that they do not suffer any drop in consumption while unemployed. The reason is that unemployment benefits provision creates a moral hazard problem. The traditional moral hazard problem identified in the literature regards search effort: once workers are granted with UI, they have less incentives to search since they lose benefits once they find a new job. Therefore, setting optimality the generosity of UI benefits hinges on achieving the best balance between welfare gains from consumption smoothing and the welfare costs created by the moral hazard effects.

[4] Moral hazard occurs when an agent does not fully internalize risks which depend on his actions.

[5] Of course most countries do make efforts to monitor search activities by workers receiving unemployment benefits. However, it is costly and far from a perfect monitoring.

[6] For example, see Hölmstrom (1979) for the trade-off between insurance and incentives.

## 2.3 How Much UI Do We Need?

An important strand of the UI literature explicitly approaches the question of what is the optimal level of unemployment insurance. What level of unemployment benefits achieves the best balance between consumption smoothing and the distortions caused by the policy? The first and second essays of this thesis aim to contribute to this literature in different ways. Before turning to that, I first summarize the main studies addressing this question.

### 2.3.1 The Traditional Approach

The seminal paper which served to some extent as the basis for the development of this literature in the last 50 years is Baily (1978). As mention before, Baily indicates two key aspects which should be balanced when setting the generosity of unemployment benefits: the gains from consumption smoothing versus the moral hazard on search where the latter is summarized by the elasticity of unemployment duration to benefit level. More importantly, for empirically assessing the optimal level of UI, he provides a formula defining the optimality condition:

$$\frac{\Delta C}{C_e} R(C_u) = \epsilon_{D,b} \tag{2.1}$$

where $C_e$ and $C_u$ are consumption levels while employed and unemployed, $R(C_u)$ is the degree of relative risk aversion, and $\epsilon_{D,b}$ is the elasticity of unemployment duration to benefit level. Hence, this approximate criterion to evaluate the optimal level of UI is composed by two main components: the marginal welfare gains from consumption smoothing - left side, and the marginal welfare costs due to increases in unemployment duration - right side. When the marginal gain of increasing benefit level equals the marginal cost, the optimal level is achieved.

The intuition behind the formula is that the gains from consumption smoothing can be measured by the percentage drop in consumption caused by unemployment, adjusted by the individual's degree of risk aversion. When the consumption loss caused by unemployment is small, there is not much role for UI: workers are already smoothing consumption across employment and unemployment quite well. Instead, when the drop is large, there is a large role for UI since it shows that workers are unable to smooth consumption across states. In turn, the degree of risk aversion is a measure of the utility cost caused by variations in consumption. A high degree of risk aversion implies that the utility loss from variations in consumption is sizable. On the cost side, the elasticity of unemployment duration to benefit level measures the efficiency cost arising from the moral hazard problem created by UI. The larger this response is, the larger the welfare cost of UI.[7]

[7]Notice that in Baily's model there are no liquidity issues, and therefore such elasticity is a pure consequence of the moral hazard problem.

In practice, empirically evaluating the formula requires estimating three key parameters. First, how large is the percentage drop in consumption when workers lose their jobs: $\frac{\Delta C}{C_e}$. Second, what is the relative degree of risk aversion, evaluated at the consumption level when unemployed: $R(C_u)$. Third, the elasticity of unemployment duration to benefit level: by how many percentage points unemployment duration increases when benefit level is raised by one percent: $\epsilon_{D,b}$. Notice that the background model does not impose *a priori* neither that consumption drops when workers lose their jobs nor that unemployment duration increases as a response to higher UI. These are left as empirical questions.

Once these statistics are estimated, it is possible to evaluate approximately whether the current benefit amount is close to the optimal level; or whether it is below or above optimality. Whenever the left and right hand side of the formula are (approximately) balanced, meaning that marginal benefits are close to marginal costs, it suggests that benefit level is close to optimal. When the left hand side is larger, it indicates that marginally raising unemployment benefits yields welfare gains since marginal benefits are larger than marginal costs. When the right side is larger, it suggests that marginally decreasing UI benefits is beneficial for welfare because at this point marginal costs of the policy are larger than marginal benefits.

Nevertheless, one certainly could argue that such welfare formula is derived under a very stylized model of the labor market and overlooks dimensions of the problem which could be important. Baily (1978) discusses at length several aspects which are neglected in this model and how they may affect the results. He also provides some extensions of this basic version to deal with some further issues. Despite all that, the model still provides a tool which offers a straight answer to an extremely important policy question: how much unemployment insurance do we need? Even if one acknowledges that this evaluation may miss some important aspects of the problem, the formula at least provides a benchmark estimate of whether policy generosity is reasonably set.

Interestingly, Chetty (2006) addresses exactly the concern that Baily's formula is derived under some specific assumption and may miss further important aspects of the problem. He shows that the formula, with an adjustment for precautionary savings motive, "holds in a general class of dynamic models subject to weak regularity conditions". In other words, his paper demonstrates that Baily's formula is robust to a wide array of different model setups. He then argues that many extensions which incorporated further complexity to Baily's analysis actually do not imply that the optimal formula changes. Namely, the adjusted expression remains robust to all the following notable cases (but not only). First, to any specific types of borrowing constrains that individuals may face. For example, the formula still holds if individuals can only borrow up to a certain limit. Second, to the presence of private insurance markets. Thus, the formula is robust even if workers can rely on private markets to insure against unemployment risks. Third, to the introduction of multiple consumption goods and also of durable goods. Hence, the formula remains the same if one allows for different types of consumption goods in the model. Fourth, to the existence of search and human capital benefits of UI. Thus, the formula does not change if one allows higher benefits in the model to improve the quality of the job matching. Fifth, to the introduction of leisure choices in the model, which addresses the concern that results would change if one consider the value of leisure for unemployed workers. Sixth, to endogenous savings behavior and more complex dynamics of the search process. Therefore, even if the model allows individuals to endogenously self insure against unemployment risks through savings, the formula still holds.

In sum, this contribution by Chetty shows that the parameters present in Baily's formula are able to capture all the relevant behavioral features which matter for welfare in an extremely generalized setup. The main take away message is that such formula is arguably a reasonable benchmark for policy when evaluating the optimal benefit level to be set.

Turning then to the effective application of the formula, Gruber (1997) is a reference in this literature for estimating the consumption benefits of UI and taking Baily's formula to the data in the US context. Interestingly, Gruber refers to two policy papers developed at the US Dept. of Labor, Employment and Training Administration which provide an analysis in the same direction: Burgess (1981); Kingston et al. (1978). This suggests that Baily's work indeed served as a benchmark for policy. In the paper, Gruber also notices that the distortions caused by UI are well-documented (already at that time) by an extensive empirical literature showing these to be sizable. However, he points, there is a gap in the empirical literature trying to assess the consumption benefits from UI. Hence, his contribution fills part of this gap.

By using US data from the Panel of Social Income and Dynamics (PSID) dataset and controlling for a number of confounding factors, he finds that the average consumption drop caused by a job loss in the US is of about 6.8%. Moreover, he estimates that the loss in consumption of workers not entitled for UI benefits is around 22.2%. It suggests that, at least partially, UI achieves the goal of helping workers to smooth consumption while unemployed. To apply the Baily's formula he recalls the elasticity of unemployment duration to benefits from Meyer (1989) which equals 0.9. Then the missing element in Baily's formula (2.1) is the coefficient of risk aversion. Gruber then performs the following exercise. He completes the formula with different degrees of risk aversion to assess what is the optimal benefit level for each of them, which is defined as the replacement ratio (benefit level divided by previous earnings). He finds that the optimal replacement ratio is always below 50% for any given plausible level of risk aversion. Given that the actual replacement ratio in his data for the US was on average 42.6%, he concludes that UI generosity can only be optimal if risk aversion coefficients are very high, hinting that benefits in the US were to generous at that time.

### 2.3.2 The Sufficient Statistics Approach

One subsequent extremely important development in the theory of optimal benefit level, which goes in the direction of the approach used in most of this thesis, is the contribution of Shimer and Werning (2007). They start by noticing two main drawbacks in Baily's approach. First, it requires properly estimating drops in total consumption caused by a job loss. They notice that data on effective total consumption do not exist. One to implement the formula is constrained to use data on food consumption as a proxy, as in Gruber (1997). This is a drawback because there exists evidence showing that food consumption is not strongly related to total consumption. Moreover, food consumption is likely to react less to income or wealth shocks. Even though Chetty (2006)'s generalization introduced above shows that one could use food consumption to apply his generalized version of Baily's optimality condition, doing so would require the estimation of the utility function curvature with respect to food instead of the general risk aversion parameter. Such estimation might be to a great deal more problematic than assessing the general degree of risk aversion, which is already an empirical challenge itself. In summary, Shimer and Werning highlight that a major problem for applying Baily's formula is that one cannot observe a measure of consumption in the data as required by the theory.

Second and related to Chetty's generalization, Baily's approach requires the estimation of the risk aversion parameter. This can be problematic because such estimate varies significantly across studies in different contexts. The contribution by Shimer and Werning (2007) provides a UI welfare formula which no longer depends on consumption reactions but on reservation wages, without the need to estimate risk aversion.[8] It shows that it is possible to assess the welfare gains from UI by only looking at how after-tax reservation wages react to changes in unemployment benefits. The intuition behind this result is that the minimum offer which workers are willing to accept in order to leave unemployment is informative on how they are struggling with unemployment in terms of consumption losses. If a worker is very liquidity constrained, he should be willing to accept a job with low pay rather than remaining unemployed. The value from UI policy can be then inferred from how benefits are able to avoid that workers have reservation wages which are too low. Therefore, the way workers' after-tax reservation wages react to an increase in unemployment benefits encode all the relevant information needed to assess the marginal welfare gains from higher UI.

Hence, Shimer and Werning (2007) show that it is possible to evaluate benefit level optimality by comparing how reservation wages (welfare gains) and unemployment duration (welfare costs) react to changes in benefit level. There is no need to rely on estimates of risk aversion. This is a very important advantage because this approach delivers a result which holds for whatever is the actual degree of workers' risk aversion. Thus, there is no risk of wrongly estimating this parameter. This characterizes the sufficient statistics approach to welfare. In other words, Shimer and Werning's welfare formula does

[8] Reservation wage is the minimum wage offer which an unemployed workers is willing to accept.

not require any estimates on lower order parameters of the model such as risk aversion or time discounting rate. The key advantage is that the policy conclusion holds regardless of which is the real preferences of individuals. More specifically, Baily's approach requires one to estimate a coefficient of risk aversion. If this coefficient is wrongly estimated, the results derived from his formula are no longer valid. Instead, Shimer and Werning's welfare formula is based on a general utility function which only assumes that agents have constant absolute risk aversion preferences, without specifying the risk aversion parameter. Therefore, results based on their formula hold regardless of which is the true parameter of risk aversion underlying agents' preferences. The intuition, again, is that the response of workers' reservation wages conveys all the relevant information for evaluating the welfare gains from marginal increases in benefits.

Turning to the empirical application of the formula, Shimer and Werning (2007) find that marginal welfare costs of providing a \$1 increase in benefits is of about \$0.11. This means that as long as the marginal response of reservation wages to benefits is above this figure, there are still welfare gains from increasing benefit level. Such result is based on their welfare formula. At this point, the main shortcoming of this approach comes up: estimates on reservation wages responses to UI are scarce and troublesome. A main important obstacle is that usually it is not possible to observe reservation wages directly, but only the effective wages accepted by workers. An alternative is relying on surveys asking workers the minimum wage which they would accept in a new job. This also may be troublesome as the reservation wages reported by workers may not exactly reflect the minimum wage that they would in fact accept. Shimer and Werning review the evidence and find figures in the range of \$0.047-\$0.44 for the response of reservation wages to an \$1 increase in benefits. This range of estimates do not deliver a clear policy conclusion regarding whether the current level of benefits are below, at, or above optimality because it is not clear whether the cost side of UI (\$0.11) is larger or smaller than the benefit side (\$0.047-\$0.44), on the margin. Furthermore, this approach is also at odds with the more recent evidence of UI effects on post-employment earnings, which has mixed results but mostly suggests that UI yields no relevant gains in re-employment wages (Card et al., 2007; Caliendo et al., 2013; Van Ours and Vodopivec, 2008).

Chetty (2008) proposes a new welfare formula which avoids the need to rely either on reservation wages or on consumption data, in a contribution which has become a reference point for the UI literature since then. The path breaking insight provided by Chetty is that not all the response in unemployment duration to UI is a result of moral hazard. He notices that the negative effect of UI on search can be decomposed into two completely independent channels. Formally, the decomposition takes the following form:

$$\frac{\partial s_t}{\partial b_t} = \frac{\partial s_t}{\partial A_t} - \frac{\partial s_t}{\partial w_t} \tag{2.2}$$

where $\frac{\partial s_t}{\partial b_t}$ is the worker's total response of search effort to UI, $\frac{\partial s_t}{\partial A_t}$ is the response of worker's search effort to the provision of liquidity, and $\frac{\partial s_t}{\partial w_t}$ is the response of worker's search effort to variations in reemployment wage, which is the pay-off of finding a new job.

The first channel of the total effect of increasing UI arises from the typical moral hazard problem pointed by most of the literature ($\frac{\partial s_t}{\partial w_t}$): workers granted with UI search less intensely for a new job because once they are reemployed, they lose the unemployment benefits that they would have received remaining out of a job. Indeed, this component is defined as the response of search effort to an increase in reemployment wage, with the opposite sign. The reemployment wage in turn is exactly the pay-off of finding a new job. The fact that part of the total UI effect is equivalent to a reduction in the pay-off of finding a new job highlights the intuition behind the moral hazard component of UI: since workers lose the UI benefits that they would receive had they remained unemployed, benefits represent the opportunity cost of finding a new job.

The second channel, which is the key new insight provided by Chetty, is the liquidity effect: UI also causes workers to search less intensively for a job because it relaxes their liquidity constrain. The idea is that workers also search less intensively because unemployment benefits alleviate their consumption loss while unemployed. Consider the extreme example in which an unemployed worker has no liquid assets at all and is not able to consume nothing until he does not find a job. This person will search incredibly intensively for a job. If you grant this person with a small cash grant, it allows her to consume more while unemployed. Then, this will cause this person to search less intensively because now she is less liquidity constrained and able to consume more while out a job. Therefore, Chetty's point is that UI affects search effort not only through moral hazard but also through liquidity. This implies that the total increase in unemployment duration in response to higher benefit level is partly due to moral hazard and partly due to liquidity. Chetty shows that while the moral hazard response is detrimental for welfare, the liquidity response represents exactly the welfare gains from UI provision. It is precisely the welfare enhancing response to the correction of market failures in insurance and credit markets.

Chetty's seminal contribution takes this insight to derive a welfare formula. He shows that the optimal benefit level can be evaluated by balancing the liquidity effects against the moral hazard effect. More specifically, the welfare gains in his formula is composed by the so-called liquidity-to-moral hazard ratio which is a ratio between the two effects. The welfare cost is composed by the elasticity of unemployment duration to benefit level. A key advantage of this approach is that there is no need to estimate directly consumption or reservation wages responses, which can be problematic due to the reasons discussed above. Instead, both the liquidity-to-moral hazard ratio and the elasticity of unemployment duration can be estimated by relying on labor market data with information on durations. This type data is definitely more precise than data on consumption or reservation wages especially because of the increasing availability of large administrative datasets on the labor market in a number of countries. Such datasets provide precise and reliable information on actual durations of employment and unemployment. This constitutes the first key advantage of Chetty's approach.

The second important advantage is that the formula is also based exclusively on sufficient statistics which can be directly recalled from the data. As in Shimer and Werning (2007), there is no need to estimate any lower order parameter of the model such as risk aversion coefficients or time discounting factors.[9] Actually, Chetty's approach is even more general than Shimer and Werning's one. He does not even need to assume that agents have constant absolute risk averse preferences. His formulation is based on a generic utility function which only needs to be weakly concave.

Chetty (2008) estimates the liquidity-to-moral hazard ratio to be around 1.5 using data from the US, which is characterized by a ratio of two marginal effects. It means that about 60% of the increase in unemployment duration caused by unemployment insurance is due to liquidity effects rather than moral hazard, which responds for the remaining 40% .[10] This shows that liquidity responses are actually quantitatively substantial and suggests that unemployed workers face indeed important liquidity constrains which seriously compromises their ability to smooth consumption while unemployed. Moreover, the paper estimates the elasticity of unemployment duration to benefit level to equal 0.53. Plugging this estimates in the welfare formula, Chetty finds that current replacement ratio in his data ($\approx$ 50%) is about optimal. His calibration suggests that raising benefit level by $1 would incur in a welfare gain of $0.04.

### 2.3.3 Implications of Employment Duration Responses to Welfare

In a footnote, Chetty points out that his analysis ignores firm lay-off behavior and general equilibrium effects on welfare and that "generalizing the formula to account for these factors would be very valuable". The first essay of this thesis is partly motivated by the first part of his observation on firm lay-off behavior. Some limited empirical literature shows that UI, besides from increasing unemployment duration, also affects firm's lay-off behavior (Topel, 1983; Feldstein, 1978). The idea departs from the fact that UI causes displaced workers to take longer to find new jobs. Firms exploit the insurance to temporarily lay-off workers because unemployment benefits decreases the probability that they find a new job any time soon. This implies that the firm is more likely to be able to recall these workers when needed. In turn, temporary lay-offs are profitable for firms because they save the costs of training new workers and the specific human capital which employees develop over time in a given function.

[9] Lower order parameters is a synonym to primitives of the model, which in turn refer to the elementary parameters of a model

[10] A ratio of 1.5 means that for a given unit of the moral hazard effect, there are 1.5 units of liquidity effect. Therefore, from a total reaction of 2.5 units, 1.5 is due to liquidity and 1 is due to moral hazard. In such way, 60% of the UI total effect is due to liquidity and 40% is due to moral hazard.

In this first essay, I notice that there are also a number of straightforward reasons to suspect that unemployment insurance may also affect the behavior of workers while employed. As discussed in the introduction section, there are potential channels which could both cause the duration of employment spells to be longer or shorter. First, UI may decrease work effort because it makes workers less afraid about unemployment. Second, since job quitters are not entitled to benefits, UI decreases the incentives for workers to quit because it causes workers to give up unemployment benefits. Third, most often workers are entitled to benefits only if they remain employed for a minimum length of time, thus it increases the incentives for them to retain their jobs until reaching MER. Fourth, since potential duration in some systems is increasing in tenure, it increases incentives for workers to remain employed longer.

If it is indeed the case that UI affects the length of time which workers spend in a given job, there arises the policy question of whether and how it affects the optimal level of unemployment benefits. Therefore, in this essay of chapter 3, I first approach the question of whether it is the case that UI affects the duration of employment spells in the data. So far, it is a suspicion based on theoretical reasoning. Then, still in the same essay, I approach the question of how such effect is relevant for the optimal level of unemployment benefits, if at all. These are clearly relevant questions for policy.

Hence, motivated by all these potential theoretical mechanisms, my first essay addresses the question of whether unemployment benefits affect the average duration of employment spells in an economically meaningful way. This is the first step of the policy question described above. Using data from Brazil, I provide evidence that employment duration does react to the level of (potential) unemployment benefits. By applying a regression kink design, I show that an 1% increase in benefits causes the average duration of employment spells to increase by around 0.5% for low-earners, while the evidence on other workers is less conclusive. To the best of my knowledge, this is the first paper to empirically assess the effects of UI benefit level on the duration of employment spells. This is the first contribution from my first essay. Later on in this section, I discuss some related evidence which assess the effects of UI minimum eligibility requirements on unemployment inflow and highlight the differences to this contribution.

The second main question addressed in this essay of chapter 3 is one of theoretical nature: how the empirical findings just described matter for welfare? This addresses the policy question of how the results from above matter for the optimal level of UI benefits. I do that by generalizing Chetty's welfare formula in such a way that employment duration is endogenous to unemployment benefits. Therefore, the second main contribution from this essay is providing a welfare formula to evaluate benefit level optimality which can deal with distortions on employment duration. I show that if higher unemployment benefits decrease the average duration which workers spend employed, this has a negative implication for welfare. The intuition is that if higher benefits cause shorter employment spells, it implies that workers contribute for a shorter period to the UI system, which in the model is financed by a tax levied on the employed. Therefore, to finance an increase in unemployment benefits, the policy maker has to increase the tax levied on the employed by higher amount than otherwise in order to keep a balanced budget. In other words, the policy maker would need to increase the UI tax by less in the case where the duration of employment spells were not reduced. Since this further rise in the tax levied on the employed is distortionary, when the elasticity of employment duration to benefit level is negative, increasing benefits imposes a larger burden on welfare.

A perfectly symmetric argument is true when the elasticity of employment duration to benefit level is positive, as shown in the empirical analysis of chapter 3. When this is the case, the policy maker can sustain a rise in unemployment benefits by increasing the UI tax by a lower amount, since higher UI causes workers to contribute for longer periods. The implication is that increasing UI yields lower costs on welfare than otherwise. Therefore, once one allows for this effect, the optimal level of unemployment benefits shows to be higher than otherwise. Furthermore, the estimated effect on duration of 0.5 is sizable compared to the well-known elasticity of unemployment duration to benefit level which ranges from 0.3 to 0.9 (Tatsiramos and Van Ours, 2014). Since these statistics weight in the same way on the welfare formula, it shows that this effect on employment duration estimated in this essay is certainly quantitatively relevant for welfare, at least in contexts similar to Brazil. In other words, it is a relevant variable if a policy maker is to set UI benefit level in such a way that it is best for society.

Hence, taking this theoretical results and the empirical estimates showing that UI increases employment duration together suggests that the optimal level of UI should be higher than previously thought, at least for the case of Brazil. Of course, it is necessary to evaluate the direction and the size of such effect in other countries to make properly specific policy evaluations. Nevertheless, it is worth noticing that even if such positive effect on employment duration is restricted to countries similar to Brazil, this finding is still relevant for the discussion on how large UI should be in developing countries. The common knowledge on the issue suggests that UI in countries where informal markets are prevalent should be low.[11] The finding here presented is a reason which goes on the opposite direction. It indicates that the positive effects on employment duration supports a higher level of unemployment benefits.

A further specific advantage of the empirical analysis implemented in this essay is that it does not rely on any policy change, which are not frequent. Henceforth, one could apply exactly the same method to estimate responses of employment duration to benefit level over time. It thus may serve as an useful benchmark for policy makers to constantly monitor over time whether benefit level is set close to optimal. Such constant evaluation of this piece of the welfare formula would not be possible if my estimates were based on policy changes.

[11] For example, see Vodopivec (2013) for a full assessment of UI in developing countries and a list of potential limitations.

### 2.3.4 Estimating the Welfare Gains from UI

The generalized welfare formula proposed in this first essay depends on three main statistics which should be recovered from the data. First, it depends on the liquidity-to-moral hazard ratio which measures the welfare gains from UI. Second, there is the elasticity of unemployment duration to benefit level, which accounts for the traditional welfare cost in the literature. Third, there enters the elasticity of employment duration to benefit level, which is the new contribution from my first essay. In the second essay of this thesis, my contribution is estimating the liquidity-to-moral ratio using the same data from Brazil. Even though learning about the size of this ratio is fundamental to evaluate the welfare gains and the optimal level of UI, the evidence on that is extremely scarce.

Chetty (2008) finds that this ratio to be close 1.5, which indicates that about 60% of the response of unemployment duration to benefit level is due to liquidity effects. His estimates are based on variations in severance payments using PSID data from the US. Even though he controls for a number of variables and provides a number of robustness check to arrive at these results, there is not a credible quasi-experimental strategy.[12] This means that part of these estimates might be biased due to the existence of confounding factors. In a follow up study, Card et al. (2007) apply a regression discontinuity design using data from Austria to exploit variations on severance payments. They find that liquidity effects are substantial. This is the first paper providing credible causal evidence that unemployment duration is affected by the provision of liquidity. It indicates that there may be large welfare gains from providing insurance to the unemployed. The only other direct evidence on this response comes from Basten et al. (2014) which finds even larger liquidity effects using data from Norway. A drawback from using these two studies to feed the welfare formula is that they do not fully estimate the ratio between liquidity and moral hazard, which is the effective statistic of interest for the welfare formula.

Chetty notices in his 2008 paper that more evidence is needed "to obtain more precise and compelling estimates of the liquidity effect in the US". This is addressed by Landais (2014) which proposes a method based on a regression kink design to back-out the liquidity-to-moral hazard ratio from variations in benefit level and potential duration. Using data from the US, he estimates a ratio of 0.88 which is fairly lower than the one found by Chetty (2008) but still substantial.

The contribution of my second essay is assessing this ratio in an extremely different context than those of these four previous studies, which are all based on data from developed countries. This is a fundamental step to answering the question of how much unemployment insurance do we need in developing countries. This constitutes this essay's contribution for policy. I apply a regression discontinuity design to exploit a "bonus policy" in Brazil granting some workers with a yearly cash bonus. This empirical strategy is discussed in details in the essay presented in chapter 4. The goal is to assess the liquidity

[12] A quasi-experimental strategy is any econometric approach which resembles an ideal controlled experiment, where similar group of individuals are assigned to a treatment and a control group

gains from UI in the context of a developing country where informal labor markets are prevalent. It is not obvious *a priori* whether such gains are larger or smaller than in developed countries. From one perspective, liquidity constrains in developing countries should be more binding because credit markets are less developed. This would lead one to believe that unemployed workers in these countries need more liquidity. From another perspective, the presence of large informal markets in which entry and exit costs are lower may constitute a faster source of income for unemployed workers. In other words, the possibility of finding a job more quickly in the informal market (even at a lower pay) may decrease the need of liquidity for the unemployed.

My results show that the liquidity effects are actually about 20% higher from those found by Basten et al. (2014) in Norway and 120% higher than those found by Card et al. (2007) in Austria. I also find the liquidity-to-moral hazard ratio to be close to 1, which is larger than the ratio estimated by Landais (2014) in the US. These results indicate that liquidity constrains are more binding in developing countries and that welfare gains from UI may be similar or larger with respect to developed countries. This provides another contribution to the discussion on the desirability of UI in such countries where informality is prevalent. Taken together with the evidence provided by Gerard and Gonzaga (2013) which show that the welfare costs of UI are likely to be small in Brazil, these findings suggest that there may be large welfare gains from increasing the generosity of the Brazilian UI system. It is also worth noticing that UI generosity in Brazil is roughly equal to the US. Thus these conclusions are not an artifact of local estimates from a system where benefits are too low.

### 2.3.5 UI Effects on Unemployment Inflow

At this point, I turn to the evidence on how features of the UI system affect the inflow into unemployment. The goal is to clarify the contribution by the last essay of this thesis. As pointed by Tatsiramos and Van Ours (2014) in their recent survey, "the empirical evidence on the inflow into unemployment is rather limited". This evidence can be divided in two strands. The first and larger one studies how eligibility requirements affect the incidence of unemployment. The second assesses the effects of benefit level and potential duration on unemployment inflow.

One of the first contributions in this literature is Christofides and McKenna (1995). Using data from Canada, for the years of 1988-1990, they show that employment hazard rates spikes exactly at the minimum eligibility requirement for UI. In other words, their evidence indicates that the risk that workers become unemployed displays a sharp increase exactly at the week in which workers qualify for UI. They notice that such spike might be particularly strong in Canada for three reasons. First, UI entry requirements are not much restrictive, since minimum eligibility requirements vary from only 10 to 14 weeks of work across Canadian regions. Second, voluntary job quitters are also eligible for the insurance. Three, UI is not experience rated in Canada, and therefore firms who lay-off more contribute just in the same way to the system.[13] In any case, their finding suggests that features of the UI system are relevant not only for job search behavior but may also influence the incidence of unemployment. In a follow up study, Christofides and McKenna (1996) provide a more throughout econometric analysis to better investigate whether such spike in employment hazard rates is indeed caused by MER. After controlling for a number of personal and job characteristics, they conclude that "a significant number of jobs terminate when they have reached the duration that would permit a UI claim" (pg. 286).

Green and Sargent (1998), always using Canadian data, complement this evidence by showing that UI minimum eligibility requirement plays an important role in tailoring seasonable jobs but has small effects on non-seasonable employment. In other words, they show that such effects are relevant for seasonable jobs whereas they are small for regular jobs. Baker and Rea Jr (1998) take advantage of policy change in Canada to assess the effects of minimum eligibility requirements on the same outcomes. In line with Christofides & McKenna, their findings suggest that UI causes a spike in employment hazard rates at the week in which workers qualify for the insurance.

The second strand of this literature studies how UI benefit level and potential duration affect the inflow into unemployment. The first well-known studies are the contributions of Feldstein (1976, 1978). In the first one, Feldstein provides a theory explaining why firms resource to temporary lay-offs and how it interacts with unemployment insurance. He shows that unemployment insurance represents an incentive to temporary lay-offs when the system has no perfect experience rating and benefits are not taxed. In the second follow-up paper, he goes to the data and estimate that a large share of temporary lay-offs are caused by unemployment insurance. His estimates point that a change in "replacement ratio from 0.4 to 0.6 raises the predicted temporary layoff unemployment ratio by about 0.5 percentage points, or one-third of the current average temporary lay-off unemployment rate of 1.6 percent".

More recent studies are Winter-Ebmer (2003) and Lalive et al. (2011), both using data from Austria. Winter-Ebmer exploits a temporary policy which extended potential benefit duration by 3 times in specific areas of the country for older workers. He finds that unemployment entry "has risen between 4 and 11 percentage points due to the new law". In a "preliminary" analysis of the channels behind this results, he indicates that it is probably driven by firms which try to get rid of high-tenured expensive workers. Lalive et al. (2011) analyze a different policy change which took place around the same years in Austria. In line with Winter-Ebmer's results, they find that an extension in benefits caused an increase in unemployment rate. Surprisingly, they find that most of this extension's effects on unemployment rate were due to responses in unemployment inflow rather than outflow.

[13] An experience rated UI system is characterized by higher contribution requirements for firms which lay-off more workers. The idea is making firms internalize the burden they impose on the UI system when they lay-off workers.

A somehow different contribution to this literature is Light and Omori (2004). In a theoretical model, they raise the idea that UI may decrease job quits because UI decreases the incentives for workers to perform on-the-job search. The mechanism in his model is that workers do on-the-job search more intensively when they expect to be laid-off soon. The reason is that when faced with a high risk of being displaced, workers try to find a new job before the lay-off takes place in order to avoid unemployment. Since UI makes unemployment less undesirable, higher potential benefits lead workers to search less while employed and, in turn, this causes less workers to quit in order to start new jobs. Using panel data from the US they find that higher benefit level "deters job quits by a small but significant amount". Another potential mechanism which can explain this result is that UI decreases incentives to quit since quitters in most US states are not entitled to benefits, as mentioned before.

Analyzing this evidence as a whole clearly points that different features of the UI system do affect the inflow into unemployment. Motivated by these results, the first essay of this thesis addresses the normative question of how UI distortions on the dynamic of employment spells affect welfare, or more specifically, how they affect the optimal level of unemployment benefits. No study in this strand of literature directly answer such question. Furthermore, the first essay in chapter III also provides direct evidence of how benefit level affects the average duration of employment spells, which is the effect of interested based on the theoretical framework there developed. None of these other studies provide a direct full estimate of how the level or potential duration of UI affect the average duration of employment spells.

As regards the fit of the third thesis' essay in the literature, it ought to enhance the understanding on the channels behind the effects just described above. As pointed before there are a number of straightforward reasons why UI may affect unemployment inflow, both from the supply and demand side. The third essay in chapter 5 starts by extending the literature presented above. It shows that minimum eligibility requirements in Brazil do cause an increase in the probability of a lay-off at MER. Second, it provides some further suggestive evidence on the mechanism behind this effect. Third, it develops a theoretical framework which shows that a model based on work effort can explain how lay-off hazard rates evolve over worker's tenure: it is able to reproduce the whole hump-shaped pattern of the lay-off risk during the employment relationship. Fourth, it takes advantage of an original dataset on absenteeism data and shows that worker's absenteeism sharply increases exactly in the month when workers qualify to unemployment insurance. Overall, the evidence there provided suggests that the supply side explanation plays a substantial role in explaining why UI affects the inflow into unemployment. More specifically, it suggests that work effort is a plausible channel.

# Chapter 3

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

## 3.1 Introduction

There is a large body of both theoretical and empirical literature studying a number of issues related to unemployment insurance (UI). Perhaps its most well-established result is that more generous UI increases the duration of unemployment spells. Instead, the question of whether (and how) unemployment insurance affects the dynamic of employment spells has been much less studied. In this paper, by applying a regression kink design (RKD) using Brazilian data, I present evidence that the level of unemployment benefits significantly affects the duration of employment spells for low skilled workers. Surprisingly, the effect is positive and yields an elasticity of around 0.3, which seems to be driven by a strong negative effect on job quitting probabilities. To assess whether the size of this effect is of any relevance for the optimal benefit level, I provide a welfare formula based on sufficient statistics. The formula shows that this job duration elasticity weights on welfare just in same way as the well-known elasticity of unemployment duration.

Even though there is rather limited evidence on the issue, there are at least few straight forward reasons for one to suspect that the availability of UI may affect the duration of employment, both positively or negatively. First, higher unemployment benefit increases the value of unemployment for employed workers. Therefore, it decreases the incentives for employed workers to put effort in keeping their jobs, potentially decreasing the duration of employment. Second, in the vast majority of UI systems, only workers laid-off against their will are eligible to unemployment benefits. Therefore, it should decrease the incentives for workers to quit because it means giving up unemployment benefits, especially if the reason for quitting is not starting a new job. Differently from the first, this mechanism would cause an increase in job duration. Third, most UI systems have in place minimum eligibility requirements (MER) in which only workers employed for a minimum length of time are eligible to benefits in the case of a lay-off. Such a feature creates incentives for workers to hold their jobs until the minimum eligibility period, and thus should increase the duration of the employment spells. Fourth, in many systems potential duration of benefits is an increasing function, often times discontinuous, of the duration of the employment spell prior to the dismissal. Similarly to MER, this provides an incentive for workers to put effort into holding their jobs for longer periods, increasing the duration of employment.

All these are simple theoretical predictions which can be made without the need to rely on any extreme assumption whatsoever. The real question however is whether one or more of these mechanisms are able to create any economically meaningful effect on the duration of employment spells. Notice that, in principle, such effect could be positive or negative. To answer to this question avoiding the interference of confounding factors, I exploit the assignment rule for benefit level in the Brazilian UI system by implementing a regression kink design. By taking advantage of eight years of linked employer-employee data from the whole Brazilian formal market, I assess the effect of benefit level around three different points of the earnings distribution. Even though the RKD is extremely data demanding, the very sizable dataset containing more than 50 millions observations per year allows me to have enough precision on the estimates. To the best of my knowledge, this is the first paper to address the question of how UI affects the average duration of employment spells with a credible quasi-experimental setup.

The remaining issue though is whether this result is of any relevance for welfare. To address this question, I generalize the reduced-form welfare formula provided by Chetty (2008) in a way that it can deal with UI distortions on the duration of employment. I show that the latter weights on welfare with the same order of magnitude as the well-known effect on unemployment duration, measured by the elasticity of unemployment duration to benefit level. Therefore, this result suggests that the effect of benefit level on the duration of employment spells is as relevant for policy as the moral hazard of search.

It is worth noticing that there exists a small literature which studies the effects of UI on few different aspects of unemployment inflow. Indeed, Feldstein (1976, 1978); Topel (1983, 1984) argue that when UI is not fully experience rated, it represents a subside for firms to temporarily lay-off workers. Using data from Canada, Christofides and McKenna (1995) shows that employment hazard rates spike exactly when employed workers qualify for unemployment benefits. In a follow up paper, they provide evidence that the spike is indeed caused by UI (Christofides and McKenna, 1996). Green and Sargent (1998) complements this evidence from Canada arguing that UI has a relevant effect on tailoring seasonable jobs but that the effect on non-seasonable jobs are small. Still with Canadian data, Baker and Rea Jr (1998) exploits a policy change of minimum eligibility requirements to assess the effects on the same outcomes. Winter-Ebmer (2003) exploits a temporary policy change in Austria and finds that unemployment entry increases by 4-11% as a response to a large increase in the potential duration of benefits. Interestingly, Lalive et al. (2011) assess the effects of a different policy change in Austria and find that an extension in UI potential duration led to a higher unemployment rate mostly due to a rise in unemployment inflow rather than outflow. Light and Omori (2004) study the effects of UI on job-to-job quit. They find that UI decreases quits because it provides less incentives for workers to perform on-the-job search in response to a expected lay-off.

Taken together, this literature indicates that UI affects the dynamics of employment spells, at least to some extent. A question which is not directly answered in none of these studies is whether UI can affect the average time which workers spend employed in an economically significant way. This is the first contribution provided by this paper: I present quasi-experimental evidence that UI benefit level actually increases the duration of employment in Brazil for low skilled workers. These estimates imply in an elasticity as large as 0.3 and seem to be driven a negative effect on quitting rates. Besides from satisfying standard test in RD design, these estimates are robust to permutation tests (Ganong and Jäger (2014)), and RK estimates in double and triple differences. A second and more important shortcoming of this small literature is that it provides little guidance on the normative question concerning the implications of these results for policy. This paper's second contribution is providing a welfare formula which explicitly indicates how such elasticity affects welfare. Interestingly, I find that a positive elasticity of UI duration to benefit level actually implies in a higher optimal benefit level. The intuition is that if higher benefit level induces longer employment spells, it implies that the government must raise by less the distortionary tax imposed on employed workers to sustain a benefit increase because these workers are now contributing for longer. The third contribution from this work is putting together these empirical results with the theory to show that the welfare effects of these responses on employment spells are sizable. I show that they affect welfare with the same order of magnitude as the well-known elasticity of unemployment duration to UI.

The paper is organized as follows. In section 2, I present the model from which the reduced-form welfare formula is derived and discuss its key differences and results with respect to Chetty (2008). In section 3, I describe the institutional background and present the identification strategy. In section 4, I show the results and provide evidence on the validity of the regression kink design. In section 5, I discuss the results and how they link to welfare.

## 3.2 Theory

The goal of this partial equilibrium model of labor supply is to derive a reduced form welfare formula which can deal with potential distortions of benefit level on the duration of employment spells. It generalizes the setup proposed by Chetty (2008) by allowing UI to affect employment duration. It features incomplete markets where workers are not able to privately insure against unemployment and have a limited ability to borrow against the future. These elements provide the rational for government intervention with unemployment insurance policy. If, otherwise, credit and insurance markets were complete, workers would be able to perfectly insure against unemployment and would face no liquidity constrains. In such world, there would be no reason for the government to intervene. Below, I present the model setup and the agent's problem.

### 3.2.1 Model Setup and Agent's Problem

The model runs in discrete time and agents live for 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, all agents start the model employed with a wage equal to $w$ and have to pay a tax $\tau$ which finances the UI system. They face a lay-off risk which negatively depends on the level of effort $e_t$ that they put in keeping their job. The idea is that workers can make costly decisions which may help them holding their jobs. For instance, workers can decide how punctual they are or how willing they are to do extra hours. It can also be understood under the framework of a standard shirking model: firms use the threatening of firing to motivate workers to exert effort. The more effort the worker puts into his job, the lower the probability of being fired. It is worth noticing, however, that this model is silent with respect to the fact that variations in effort may affect firm productivity. Work effort $e_t$ is costly for the workers 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 lay-off. The problem of the worker who keeps his job is given by:

$$V_t(A_t) = \max_{A_{t+1} \geq L} v(A_t - A_{t+1} + w_t - \tau) + J^V_{t+1}(A_{t+1}) \tag{3.1}$$

$$J^V_t(A_t) = \max_{e_t} e_t V_t(A_t) + (1 - e_t) U_t(A_t) - c(e_t) \tag{3.2}$$

$V_t$ defines the value of the job 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 the worker is able to borrow against the future. $v(.)$ defines the utility from consumption of the employed worker. With probability $e_t$ he keeps his job, 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 $U_t$.[1]

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

[1] A more intuitive and conventional assumption would be that a lay-off 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.

requirement (MER) for UI, which is a typical feature of many UI systems.[2] Nevertheless, since $k$ is a parameter which can take any value, the model is also able to suit the case of systems which do not have MER. At this point, the unemployed worker chooses his level of search effort $s_t$ in order to find a new job. As for work effort, $s_t$ is normalized to equal the probability that the worker finds a new job at period $t$. The cost of search effort is defined by $\psi(s_t)$ which is assumed to be continuous and convex ($\psi'(s_t) > 0$ and $\psi''(s_t) > 0$). Thus, with probability $s_t$ the unemployed worker finds a new job which immediately starts at period $t$ and yields value $E_t$. With probability $(1 - s_t)$ he fails to find a job at period $t$ and remains unemployed, which yields him the value $U_t$. His problem is given by:

$$U_t(A_t) = \max_{A_{t+1} \geq L} u(A_t - A_{t+1} + b_t) + J^U_{t+1}(A_{t+1}) \tag{3.3}$$

$$J^U_t(A_t) = \max_{s_t} s_t E_t(A_t) + (1 - s_t) U_t(A_t) - \psi(s_t) \tag{3.4}$$

$E_t$ is defined as the value of employment subsequent to unemployment. Following the same spirit of Chetty (2008), I assume this to be an absorbing state. It means that once an unemployed worker finds a new job, he remains employed indefinitely. Furthermore, once reemployed, workers no longer have to contribute for UI since his job now lasts forever.

$$E_t(A_t) = \max_{A_{t+1} \geq L} v(A_t - A_{t+1} + w_t) + E_{t+1}(A_{t+1}) \tag{3.5}$$

$$\tag{3.6}$$

The underlying idea of this setup is that the UI system can be properly represented by a initial period where employed workers contribute to the system, and a subsequent period where workers who have lost their jobs are benefited from the insurance. This also seems to be the appropriate order of facts because any UI system requires workers first to work and, only then, 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 enough 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 sum, 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 lay-off risk which negatively depends on their

[2] More precisely, to the best of my knowledge, I am not aware of any UI system which does not require a minimum number of working months for workers to be granted with UI benefits.

choice level of work effort, period after period. If a worker becomes unemployed, he has to choose a level of search effort in order 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. Once the worker leaves unemployment, he falls into an absorbing state where his new job lasts indefinitely and he has no longer to contribute for the UI system.

### 3.2.2 The Reduced-Form Welfare Formula

I leave the solution for the worker's problem in each state of the model to appendix 3.7.1 and 3.7.2, and move to the social planner's problem to derive the welfare formula. The social planner aims to maximize expected utility by choosing the level of unemployment benefits and a tax level $\tau$ on employed workers in order to finance the system. In principle, the profile of benefit levels and duration could vary over time, however for a matter of simplicity I focus on "constant benefit, finite duration", as in Chetty (2008).[3] Therefore, I here assume $b_t$ to be constant over time and that benefits last for a maximum of B periods.

The general social planner's problem is given below:

$$\max_{b,\tau} J_0^V(b,\tau) = e_0 V_0(b,\tau) + (1-e_0)U_0(b,\tau) - c(e_0) \tag{3.7}$$

$$s.t. \quad f^{UI} D_B b = D_E \tau \tag{3.8}$$

The goal of the social planner is to maximize $J_0^V$ which defines the representative worker's expected utility, which is assumed to start the model employed. Since the choice of effort at period 0 can lead to a lay-off at the same period as discussed before, expected utility is the weighted sum of the expected utility of workers who keep their jobs at the initial period, $V_0$, and of those who enter unemployment already at period 0, $U_0$; minus the cost of effort. Weights are of course the probability of keeping the job at the initial period, and the probability of being dismissed, respectively.

The constraint assures that the government budget is balanced. $D_E$ describes the expected duration of the agent's employment at the beginning of the model. Only this duration matters for the government budget's revenue because, as stated before, upon reemployment workers remain employed forever and no longer contribute to the system. $D_B$ defines the agent's expected unemployment duration under UI benefits and $f^{UI}$ is the fraction of workers meeting MER. The former differs from the simple unemployment duration because when the unemployment spell exceeds the maximum duration of benefits ($B$ periods), workers no longer receive benefits. Thus, once the unemployment spell exceeds the maximum

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

duration of benefits, its duration no longer matters for the government budget. Therefore, the left-hand-side of the budget constraint in (3.8) denotes the expected cost of the policy, while the right-hand-side represents the expected amount received in taxes, which are 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 of Chetty (2008), I assume that the consumption path during employment is constant since unemployment is unlikely to cause large losses on life cycle earnings. Furthermore, I assume both the liquidity to moral hazard ratio and the probability of finding a job to be independent to the period in which a worker is displaced. There is no reason to believe that these should vary according to the period of work in which individuals become unemployed. 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 3.7.4 for details):

$$\frac{dW}{db} = f^{UI}\frac{D_B}{D_E}\left\{\frac{1}{1-s_0}(\rho+1) - \left(1+\epsilon_{f^{UI},b}+\epsilon_{D_B,b}-\epsilon_{D_E,b}\right)\right\} \tag{3.9}$$

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

This formula shows the net welfare effect from increasing UI benefits by \$ 1. Welfare effects are a trade-off between the benefits from the liquidity provided to unemployed workers and the costs from higher taxes imposed on employed workers. The benefit from providing liquidity to the unemployed is captured by the liquidity-to-moral hazard ratio $\rho$. It is also weighted by the fraction of unemployed workers actually eligible for UI, since those not meeting MER are not entitled to benefits. On the cost side $\epsilon_{D_B,b}$ captures the behavioral response from higher benefits on the duration of unemployment under benefits; and $\epsilon_{D_E,b}$ 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, $\epsilon_{f^{UI},b}$. The two latter terms capture exactly the distortionary effect of UI on employment and is the key difference from this result to the original formula provided by Chetty (2008). They affect welfare because they change the length of time during which individuals contribute for the system. If UI causes employment spells to be longer, the policy marker needs to impose less distortionary taxes on employed workers in order to finance the system. This implies in a positive addition to the welfare effects of an increase in UI. If instead 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 benefit level. Whether the elasticity of employment duration to benefit level is positive or negative is an empirical question.

The formula shows that $\epsilon_{D_E,b}$ affects welfare exactly with the same magnitude of $\epsilon_{D_B,b}$. In the empirical section, I show that $\epsilon_{D_E,b}$ can take values as large as 0.3, which is clearly in the range of $\epsilon_{D_B,b}$ found by other studies, roughly going from 0.1 to 0.9.

## 3.3 Institutional Background and Identification 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 to explore kinks on the policy rule which conditions benefit level on previous earnings in Brazil. Throughout this section, I introduce the main characteristics of the UI system in Brazil and the benefit level schedule; explain the identification strategy - *the regression kink design*; and present the data.

### 3.3.1 UI Schedule in Brazil

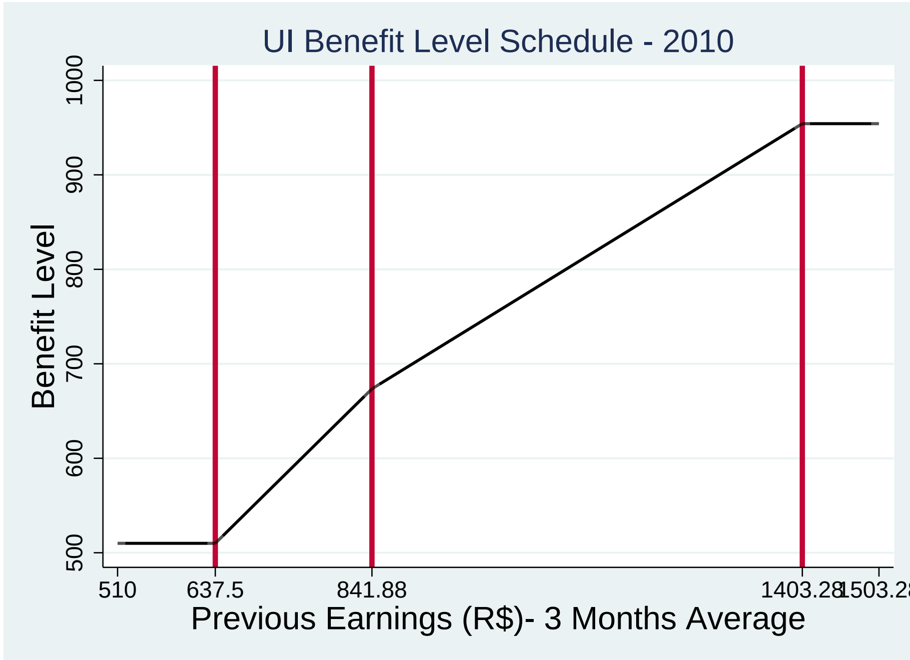
The Brazilian unemployment insurance system is a federal program established in 1986. It offers temporary income for formal sector workers who are dismissed against their will and meet minimum eligibility requirements. These are: (i) have been employed in all the last 6 months prior to the lay-off; (ii) have no other source of income ; (iii) have not been granted with UI benefits for the last 16 months, counting from the date of the last lay-off which enacted benefits. It is important to notice that benefits are granted only for workers dismissed *without a just cause*. This is the most common type of dismissal in Brazil, since employers by law are free to dismiss workers without a just cause in the sense that they need no authorization from any tribunal or government agency to do so. 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 hard to collect enough proof to back up cause.[4] Also notice that workers quitting their jobs are not entitled to benefits.

Benefit level is defined by a rule based on the 3-months average of previous earnings prior to the dismissal. The minimum wage and the UI schedule increase every year by the inflation rate in the previous year plus the average real growth rate of the economy in the two previous years. This causes the minimum wage and the kinks to move to the right in real terms at the same pace, in all years. On average these have increased by roughly 8% per year in the period from 2005-2012. I choose to present the schedule with numbers from 2010 for illustrative purpose, as the schedules for other years follow the very same shape. Figure 3.1 shows the schedule for the year of 2010.

Monthly benefits are a function of a reference wage (*r.w.*) which is given by the average monthly earnings in the last three months prior to the dismissal. Benefit level equals 80% of the reference wage

[4] In general, workers can only be fired on cause only if they: (a) are continuously absent from work (usually more than 30 days) ; (b) commit serious misconduct ; (c) go to work under the effect of alcohol; or (d) commit a large number of small infractions.

Figure 3.1: Benefit Level Assignment Rule - Year 2010




if it is lower than R$ 841.88. However, benefits can never be lower than minimum wage, R$ 510 for the year of 2010. This generates the first kink in the assignment rule which can be seen by the red line at the left side of figure 3.1 (at R$ 637.50). When the reference wage is higher than R$841.88 but not larger than R$1403.28, benefits are given by $[(r.w. - 841.88) * 0.5 + 841.88 * 0.8]$. It defines the second kink present in the assignment rule, at R$841.88, which is indicated by the second red line in figure 3.1. For reference wages larger than R$1403.28, benefits are always equal to R$954.20. This cap defines the last kink in the assignment rule, which is indicated by the red line at the right side of figure 3.1. These three kinks are the source of exogeneity which to be exploited to identify the effect of benefit level on employment duration. The time variation of the kink points are also used for identification as a robustness test. The details of the strategies are discussed in the next subsection.

As regards the maximum duration of benefits, it is a function of the number of months worked in the last 36 months prior to the lay-off. Table 4.1 presents the UI schedule of potential duration:

Table 3.1: Potential Duration Assignment Rule

| Months worked in the last 36 months | Months of Benefit |
|---|---|
| from 6 to 11 | 3 |
| from 12 to 23 | 4 |
| More or equal 24 | 5 |

### 3.3.2 The Regression Kink Design

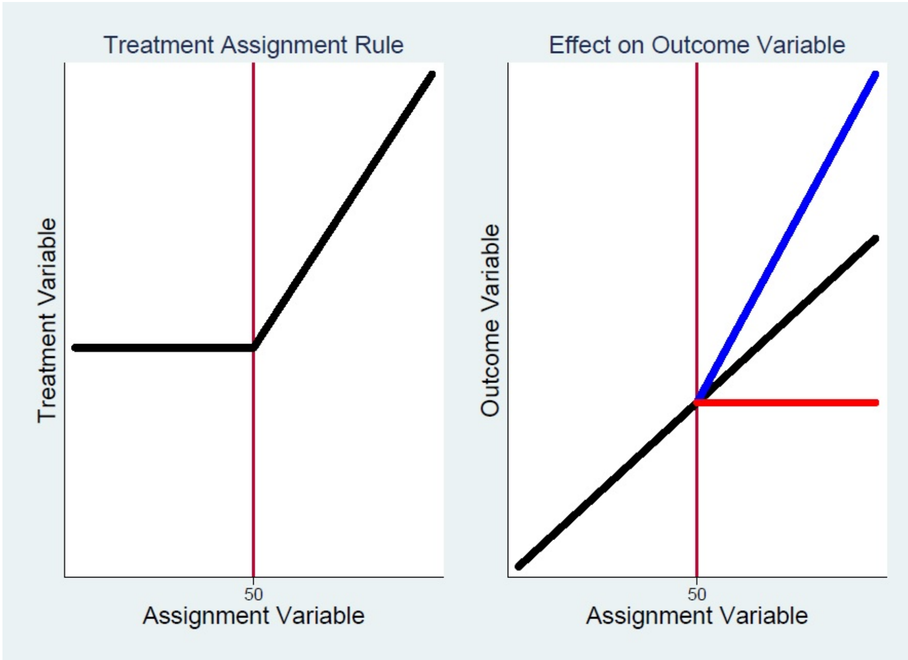
The idea of the regression kink design (RKD) is to exploit kinks in the relationship between an assignment variable and a treatment variable. These are the reference wage, based on previous earnings, and the level of unemployment benefits in this application, respectively. Such kinks are present in the relationship explained above and illustrated by figure 3.1. 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 kink in the relationship between the outcome variable and the assignment variable. Therefore, in our context, if we expect that there is a causal relationship between UI benefit level and employment duration, there should also be a kink relationship between employment duration and the reference wage (the assignment variable) at the same kink points marked in red in figure 3.1.

The idea of this design is similar to a regression discontinuity design (RDD), except that in this case there is not a discontinuity in the level of the assignment rule, but in its slope (or first derivative). The intuition of why it is able to identify the treatment causal effect is exactly that in the vicinity of the kink, subjects have the same pre-treatment characteristics *on the margin* but are however assigned to different levels of treatment *on the margin*.

In figure 3.2 it is possible to see a graphic example of the RKD. The graph on the left illustrates the kink relationship between the treatment and the assignment variable when it equals 50. It illustrates a hypothetical case in which individuals to the right of the kink receive a linearly increasing level of treatment. The graph on the right side shows the three possible results which can be found by analyzing the relationship between a given outcome variable and the assignment variable. If the treatment yields no effect on the outcome, one should find no kink in the relationship around the kink point, as shown by the black line. In case the treatment has a positive effect on the outcome, one should expect to find a positive change of slope around the kink point, as shown by the blue line. In the case where the treatment has a negative effect on the outcome, there should be a negative change in slope around the kink point, as shown by the red line.

The key assumption for the RKD is formalized by Card et al. (2015) 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 in the RDD, one crucial advantage for the credibility of this design is that its key assumption is testable in at least two ways. First, it is possible to test whether the empirical density function of the assignment variable is actually smooth around the kink point. I therefore provide evidence on whether the density of average previous wage is smooth around all the three kink points. Second, the key assumption described above implies that the conditional expectation function of any pre-determined characteristic is also smooth around the kink point. Therefore, I provide evidence on the smoothness of the conditional expectation function of pre-determined variables, such as previous tenure and years of schooling, around all the three kink points. Furthermore, I create a further

Figure 3.2: RKD Graphic Example




variable to test the validity of the design. I generate the best linear prediction based on a full set of pre-determined covariates around each kink to test whether it evolves smoothly around each kink. The idea is that if the design is valid, this linear prediction based on pre-determined covariates should not yield any kink around the points.

In order to test and identify the presence of kinks in the data, I apply a local regression in the following parametric form:

$$Y_i = c_0 + [\sum_{p=1}^{P} \gamma_p (w-k)^p + \beta_p (w-k)^p.D] \text{ where } |w-k| \leq h \tag{3.10}$$

where $w$ is the reference wage, based on previous earnings, in the year (the assignment variable) 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 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 $\beta_1$. As regards the bandwidth and polynomial order choice, I decided to conduct Monte Carlo simulations with a variety of proposed bandwidth selectors with linear and quadratic polynomials to evaluate how each of these perform, as suggested by Card et al (2015). This procedure is discussed in details in the results section.

I concentrate the analysis on the sample around the first kink as it yields more statistical power because of two reasons. First, the variation in the slope of the treatment is the largest (0.8 against -0.3

and 0.5) and there are more observations around this threshold (around 0.5 and 4 times more than the second and third kink). Therefore, I suppress the results around the second and third kinks. Overall, they indicate that it is not possible to robustly identify moderate effects around these points. Therefore, whether results on the first kink also apply to other more skilled workers is left as an open question.

### 3.3.3 Data

The data I use in this paper comes from the *Relação Anual de Informações Sociais* - RAIS. It is an administrative dataset covering all the employment relationships in the Brazilian formal Labor Market. I have access to this data from the year of 2005 to 2012. It contains detailed information on the characteristics of each labor contract such as start and end date, type of labor contract, type of termination, 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 identify workers and firms through an identification number.

## 3.4 The Effect of Benefit Level on Employment Duration

To assess the effect of benefit level on employment duration, I create three samples around each of the three kinks in the UI schedule pooling data from all years around each threshold. I consider all workers from the private sector which were employed at the first day in which the yearly schedule is introduced. Then, since the schedule is again updated 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 in 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 job until the last day of the year, I consider the last day of work as December $31^{st}$. Notice however that from 2005 to 2009, the yearly schedule was respectively introduced at the first day of May, April, April, March and February, therefore it would be possible to consider the duration until a date further than the December $31^{st}$ of the previous year in this period. However, I decide always to use the December $31^{st}$ as the last day of employment in the year because of the structure of 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 which report the data for only one of the years. As mentioned before, the analysis is concentrated on the first kink because of lack of statistical power around the other kinks.

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 previous to dismissal. Due to that limitation, I use the average wage in the year as assignment variable for the RKD and need to expect that wages do not change too fast within a given year. In case wage evolution over the year is too steep, it is likely that the RKD design would be compromised as the kink would likely be smoothed and it would be hard to identify any kink in the data.

### 3.4.1 Density Smoothness

To evaluate whether the necessary conditions for the RKD hold, the density function of reference wages must evolve smoothly around the kink. To test for 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 for the number of observations in each bin. Then, I run a regression as in equation (4.6) on the number of observations allowing for a slope change at the kink in order to test for the smoothness condition. I set the polynomial order of this regression to minimize the Akaike criterion. Figure 4.1 displays the density of average monthly earnings and the manipulation test result for the first kink.[5] From visual inspection, the density function seems to move quite smoothly around the kink and there seems not to be any evidence of slope changes. This impression is supported by the first-derivative test reported in the graph which does not allow one to reject the null hypothesis of no kink.

### 3.4.2 Graphical Evidence

A first and key piece of graphical evidence comes from observing how job duration evolves with earnings within a (schedule) year. Figures 3.4-3.5 display this evidence for 2005 and 2012. In both figures, this profile seems to evolve smoothly at all points except for the first kink. Job duration seems to be relatively flat between the minimum wage and the first kink, and starts increasing with earnings exactly at the first kink. Since potential UI 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 to highlight that the first kink changed in real terms by 45% between 2005 and 2012, suggesting that this is not a coincidence driven by a specific year.

In order to get 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, and the probability that a worker reaches MER evolve over earnings. Figures 3.6-3.7 presents this evidence on quits which is striking: in both years, the probability of quits display a clear negative slope change around kink 1. It strongly suggests that UI benefit level is causing job quits to decrease. As regards the probability of lay-off and the probability that a worker remains in the job until MER (6 months), the evidence displayed in figures A1-A4 is much less clear. Firing probabilities seem to evolve relatively smooth at all points. The share

[5] Same results for kink 2 and 3 are reported in the appendix figures B1 and B2.

of workers reaching MER seem to display a mild positive slope change around kink 1 in 2005 while the same pattern is less clear in 2012.

At this point, I pool the data for all years around the first kink and display it on figure 3.8. There emerges the same pattern observed for 2005 and 2012: employment duration is apparently flat before the first kink and starts to increase just after it. To investigate whether any of these results might be driven by kinks in pre-determined workers' characteristics, I build a linear prediction of job duration based on an extremely rich set of pre-determined covariates: age at hiring date, job tenure at the date of the yearly schedule introduction, decile of firm size and previous firm size at two different aggregation levels, and dummies for whether the worker was recalled for this job and whether the workers was still facing the waiting period for UI due to prior benefit claim (16 months minimum periods between UI claims), termination type at previous job, race, gender, weekly hours of work, years of schooling, industry, and federal state (27); all this interacted with yearly dummies. Notice that the dataset allows me to recall relevant workers' previous job characteristics, which are likely to be very informative on their current labor market outcomes. Figure 3.9 shows how this best linear prediction of job duration evolves around the kink. The first important thing to notice is that these predicted values seem to display a positive kink around the threshold. However, it is also worth noticing that this variable displays incredibly less variation for the same range of the running with respect to actual job duration ([36.2,36.6] vs. [35,38]).[6] This suggests that even if it were the case that covariates are driving the results on job duration, this bias is likely to be very small. In any case, it may lead one to be suspicious that non-observable could be driving this effect. I address this reasonable concern by analyzing how these predicted values evolve on a year to year basis.[7] I find that this apparent kink is driven by the years of 2005 and 2006. Therefore, to avoid concerns that results are driven by unbalances in covariates around the kink, I drop these years from the sample and run the regression analysis on a restricted sample containing data from 2007 to 2012.

Figure 3.11 shows how predicted job duration behaves in this restricted sample. The range of this outcome becomes even smaller ([36.2,36.6]) and there seems to be no evidence of the presence of any slope change around the threshold. On the other hand, actual employment duration displays exactly the same pattern as in the full sample, as shown by figure 3.10. This strongly suggests that the kinked pattern of job duration around kink 1 cannot be explained by unbalances in covariates. To gain further insight on such issue, it also useful to observe how the pre-determined covariates evolve around the threshold, which is shown by figure 3.12. As for the predicted job duration, these variables seem to display no slope change around the kink point.

[6]Indeed, if these two graphs are set with the same scale, it is not to possible to observe any slope change in the predicted job duration.

[7]These graphs are displayed in the appendix by figures A5-A12

Overall, I interpret these results as evidence that UI causes job duration to increase around the first kink and that unbalances in predetermined covariates are not driving this effect. As regards the channel of this effect, it seems to be through a decreased probability of job quits.

### 3.4.3 Bandwidth Choice

A key issue in the RKD 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 from now on, where they proposed a selector based on optimal mean square error. As in Card & al (2015) - CLPW from now on, I consider this selector with and without its regularization term, which I call "CCT" and "CCT no regularization".[8] Furthermore, I follow again CLPW and implement the FG bandwidth selector which is based on Fan and Gijbels (1996). 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. As regards polynomial order, I use a linear and quadratic specifications for each bandwidth selector. Therefore, in total I consider six specifications: FG linear, CCT linear, CCT linear without regularization, FG quadratic, CCT quadratic and CCT quadratic without regularization.

At this point, I argue that none of these selectors perform satisfactorily on my data. I provide two pieces of evidence which support this claim. The first is based on a Monte Carlo simulation exercise, as suggested by CLPW, in order to assess the performance of each specification considering two main criteria: the root mean-squared error (RMSE) and coverage rates. To run these simulations, I set the data generation process (DGP) using an approximation of data on job duration around the kink with a fifth order polynomial on each side of the threshold. 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 in order have an elasticity of 0.5. I generate 300 samples of the same size as the actual data by sampling the running variables and errors with replacement. On the top of assessing the performance of these six selectors, I decide also to test the performance of using fix bandwidths in a similar range to those picked on average by the selectors described above. Interestingly, some of these specifications perform sometimes better than any of the other selectors, which suggests that using constant bandwidths should also be considered.

The results from these simulation are shown in table 3.2. It is striking to see that 95% coverage rates are often very low and that RMSE with respect to the true kink value is never lower than 0.70. Another pattern arising from these regressions is that all the specifications seem to suffer from a strong negative bias ranging from 0.22 to 1.28 times the actual kink size. Overall, these simulations suggest that these

[8] The original selector contains a regularization term to avoid bandwidths which are "too large", as noted in Calonico et al. (2014)

specifications perform poorly in estimating slope changes on employment duration. Indeed, as shown by the appendix table 3.5, regression results based on these specifications vary to a great extent and do not allow for any conclusive assessment on the actual slope change. Around kink 1, three specification point for a statistically significant negative slope change, two for a statistically significant positive slope change, and one is not significantly different from zero. I argue that the instability of these results is driven by the extremely small bandwidths chosen by these selectors. In the first kink, except for the FG quadratic specification, all selectors pick bandwidths ranging from R$4 ($\approx$ $2) to R$12 ($\approx$ $2) monthly earnings, which is clearly an extremely narrow range. Such small choices are possibly caused by the small global range of the data around this kink.

Therefore, I present a second piece of evidence suggesting that such small bandwidths indeed pick up too much noise. I implement the permutation test proposed by Ganong and Jäger (2014) for a range of small bandwidth. The idea of the test is, for a given bandwidth, to estimate the slope change around as many placebo points as possible, where there is no actual policy kink. This procedure is useful to assess the performance of the local linear estimator for each bandwidth. Of course the assumption for this procedure to make sense is that these placebo points are reasonable counterfactuals of the actual kink point. Therefore, I use the distribution of these placebo estimates to construct critical values required for one to reject the null hypothesis of no slope change. Results are displayed in figure 3.15. The blue line report the elasticities based on the estimated slope change at the kink with nominal 95% confidence intervals in red based on the local linear regression. The gray lines display the critical values at the 90% and 95% confidence level based on the distribution of placebo estimates at which one would expect zero slope change. These results clearly suggest that such small bandwidth often lead to statistically significant results around the placebo points, where one should expect just the opposite. It is also important to notice that t-statistics for these 90% and 90% critical values are extremely above the expected one.

I interpret these two pieces of evidence as suggestive that such small bandwidth choices have unsatisfactory performance with this data. Therefore, in the following subsection I explore larger bandwidth choices and try to get insight on their significance with permutation tests.

### 3.4.4 Estimation Results

Figure 3.16 presents regression results for varying bandwidths, but now using larger bandwidths. For bandwidths larger than R$55 ($\approx$$27.5), estimates are statistically significant and positive, just in line with the graphical analysis. More importantly, they display some stability which suggests for the absence of bias within this range (Ruppert, 1997). A similar pattern is found on the t-statistics. Even though, the number of placebo points for the permutation tests becomes small, it is still worth to notice that the estimated slope is higher than any of those found by the placebo tests. Notice that when there are less than enough placebo estimates to draw the 90% and 95% critical value, gray lines display the largest and smallest placebo estimate results. On the other hand, the same results on predicted job duration point for a very small and most often statistically insignificant estimates. Also, these estimates are most often within the range of placebo estimates based on the permutation test. I interpret this as strong evidence of the presence of an actual and statistically significant slope change in employment duration around the first kink caused by the UI benefit level and pointing for a positive elasticity of around 0.3.

As regards the channel driving such effect, figure 3.18 displays the same estimates on quitting probability. Results clearly point for an statistically significant negative effect which also displays stability for bandwidths larger than R$55 (≈$27.5). Moreover, slope estimates and t-statistics are well outside the range found in the placebo tests. The same estimates for lay-off and MER probabilities are displayed in appendix figures A13-A14 and indicate small and statistically insignificant results for these variables.

Despite all this evidence, there could still be a concern that these results are capturing a simple predetermined kinked or quadratic relationship between job duration and the average earnings around the kink point. To address this issue and test for the robustness of the estimates presented above, I apply a RKD in double differences which explores the fact that each kink point changes every year in real terms, as suggested by Landais (2014). The idea is to compare the estimated slope change on the actual policy kink to the slope change estimate at the same point in previous years, where 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 around the kink the points, the diff-in-diff RKD estimates should help mitigating this bias. To implement this procedure I need a minimum interval between "treatment" and "control" in order to ensure that the kink moves enough in real terms, otherwise bandwidths around these points are too small and results will likely pick noise. Therefore, I use a three years lag to implement this procedure, and have also to drop the years of 2005 and 2011 because there is too little variation between kink points for 2008-2005 and 2011-2008. Therefore, the final sample contains the years of 2009, 2010 and 2012 as treatment years; and 2006, 2007, and 2009 as control years (there was no actual policy kink at these points in these years). Figure 3.19 shows how employment duration evolves around the years in which there was an actual policy kink (top panel), compared to the "control" years (bottom panel). This graphical evidence suggests that while there seems to be a kink on the top panel, job duration seems to evolve smoothly around the placebo points on the bottom panel. Such impression is confirmed by the regression results displayed in table 3.3. Estimates on employment duration and quitting probabilities yield very similar results to the standard RKD analysis, which are robust and relatively stable across bandwidths. No robust indication of slope change caused by UI benefit level is found on lay-off and MER probabilities. The same thing happens for the predicted job duration variable, indicating once again that the estimates on job duration and job quits are not driven by imbalances on pre-determined covariates.

Finally, I propose a further test to assess whether the differences in slopes on job duration detected in the DD-RKD is indeed caused the UI benefit schedule. I repeat the same procedure from above but now adding a placebo kink point to the analysis which is not an actual policy kink neither in the "treatment" nor in 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 "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 a three years distance in time is spurious. I set this fake point as close as possible to the actual kink point but considering the constrain that some minimal bandwidth is required to run these regressions. 3.4 presents these DDD-RKD results. Even though the range of possible bandwidths is smaller, these results point to a similar pattern as the one found in the previous analysis.

Overall, I find the results around the first kink very robust, indicating that the elasticity of benefit level is positive for the lower end of the skill distribution and its size around 0.3. This evidence is also robust for a substantial and negative effect of UI benefit level on quitting probabilities.

## 3.5 Conclusion and Implications for Welfare

Now I present a simple exercise on the evaluation of welfare effects based on the estimates of the previous section. Since estimates on the elasticity of UI covered unemployment duration to benefit level are not available for this data, I assume this to equal 1 which is line with the literature. For the liquidity to moral hazard ratio, I recover this estimate from Britto (2015). The goal of the exercise is to show that the size of the effect of UI on the duration of employment is clearly relevant for welfare.

The welfare formula as shown in (3.9) implies that are still gains from higher benefit level if:

$$\frac{dW}{db} > 0 \iff \frac{1}{1-s_0}(\rho+1) - \left(1 + \epsilon_{f^{UI},b} + \epsilon_{D_B,b} - \epsilon_{D_E,b}\right) > 0 \tag{3.11}$$

From estimates around kink 1, consider $\epsilon_{D_E,b} = 0.3$ and $\epsilon_{f^{UI},b}$ to equal zero. Consider $\epsilon_{D_B,b} = 1$, which is an upper bound in the empirical literature, and recover $\phi = 0.98$ from Britto (2015). Also, I estimate $\frac{1}{1-s_0} = \frac{1}{0.97} = 1.04$, where $s_0$ is the fraction of workers finding a new job within one week of unemployment. It implies that the effect of raising UI benefits on welfare by R\$1 is given by:

$$\frac{dW}{db} > 0 \iff (0.98+1) - (1+0+1-0.3) = 0.28 > 0 \tag{3.12}$$

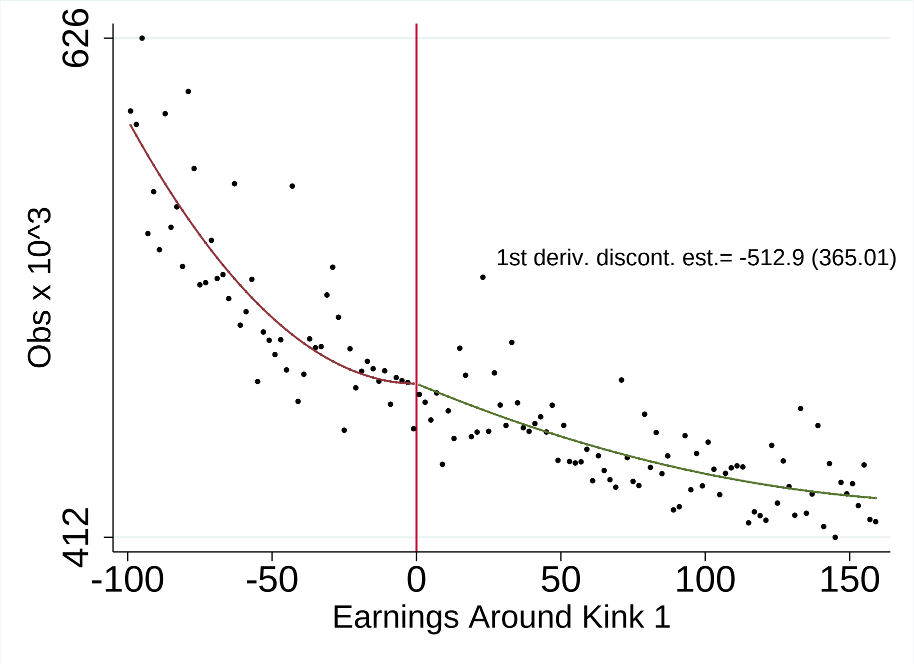
If the elasticity of unemployment duration to benefit level is indeed approximately equal to 1, the welfare formula suggests that there would still be gains from raising benefit level. If one instead neglects UI effects on the duration of employment, the result would point otherwise:

$$\frac{dW}{db} > 0 \iff (0.88 + 1) - (1 + 0.5 - \cancelto{0}{(0.3)}) = -0.02 < 0 \tag{3.13}$$

In any case, the main message from this simple exercise is that the effect found on the duration of employment is economically significant and clearly relevant for welfare. Recovering previous estimates from the literature on other elasticities, it becomes clear that it has at least the same magnitude of the other elements present in the welfare formula. Therefore, it strongly suggests that policy makers should be aware of such effect and take it into account in order to optimally set the level of unemployment benefits. Perhaps the most surprising result from this analysis is that unemployment benefit increases the length of employment spells. Introducing this into welfare raises the marginal value for society of providing UI, at least in this partial equilibrium analysis. Whether there are further elements to consider once we accept this effect to be relevant is certainly a question for future research. Such effect could have implications on the optimal job turnover rate and affect worker productivity since it distorts the incentives to keep a job.

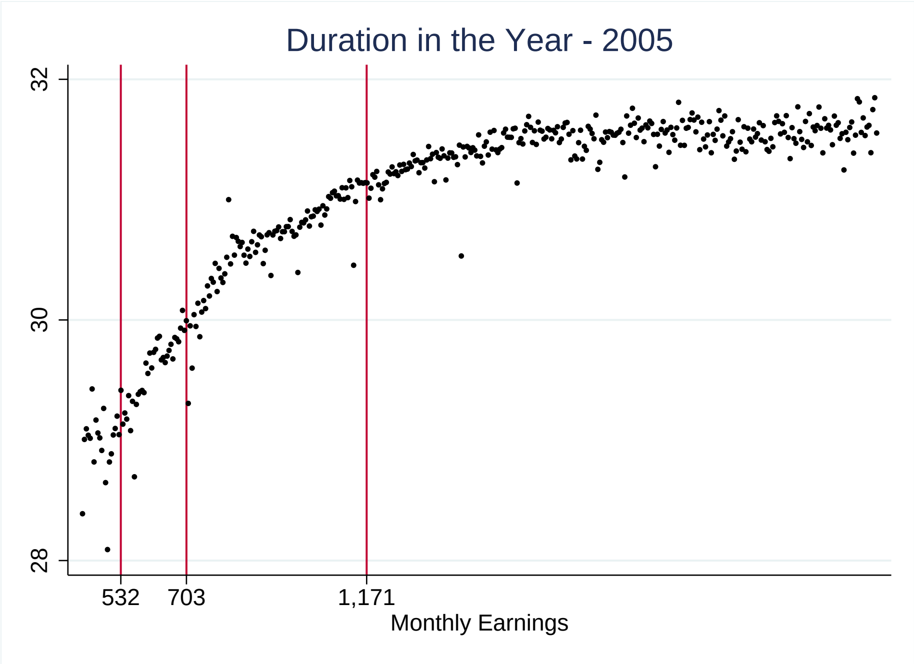
Of course, the analysis presented in paper applies to Brazil and represents LATE on workers on the very left of the skill distribution. However, this may not only be the case of a developing country. For instance, Rebollo-Sanz (2012) analyzes data from the Spanish labor market and supports the hypothesis that UI is related to labor turnover in Spain. He also reports spikes in lay-off probabilities once workers qualify for unemployment benefits. To conclude, I believe that the findings here presented suggest that future research should aim at evaluating whether the same effects are sizable for other groups of workers and in countries with different contexts.

Figure 3.3: Density of Wages the Around Kink 1



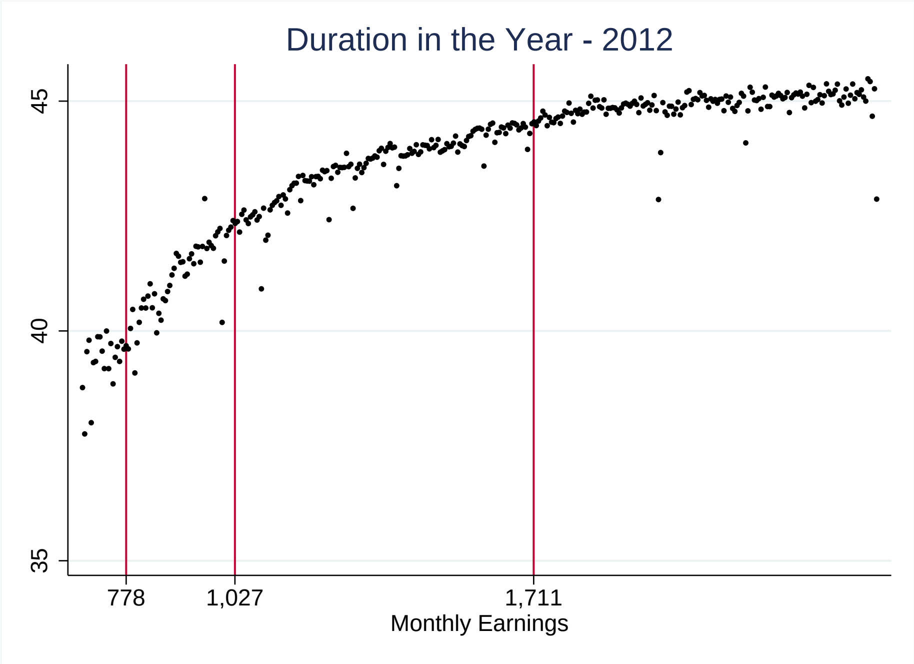

The graph displays how the density of earnings evolve around the kink. At each side of the kink, the density is approximated by the polynomial which minimizes the Akaike Criterion. The graph also displays the test statistics for the slope change of these polynomials at the kink. See the text for details.

Figure 3.4: Employment Duration



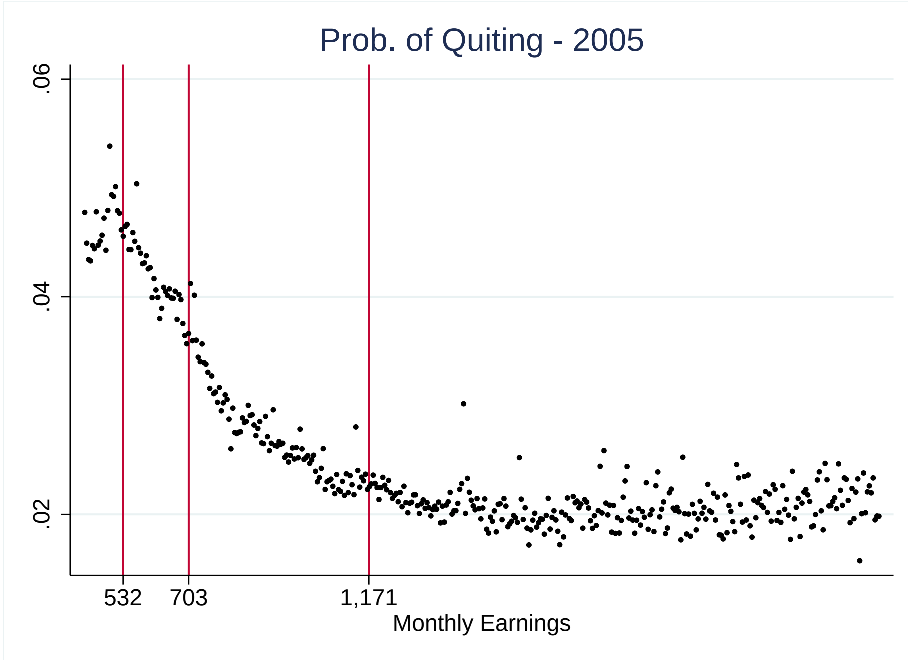

The graph displays how employment duration in the year evolves according to monthly average earnings, in 2012 prices. Duration is expressed in weeks.

Figure 3.5: Employment Duration



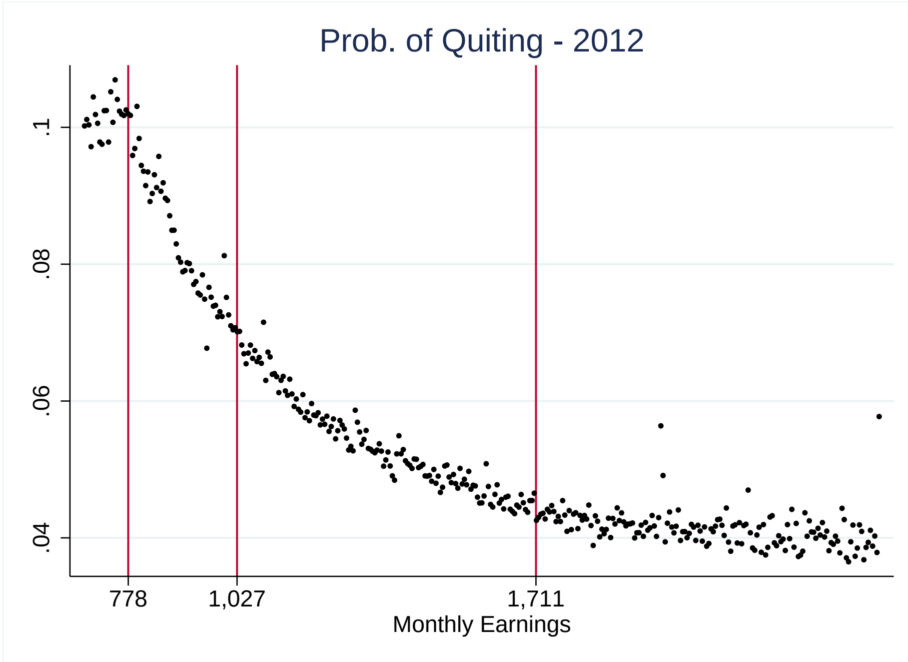

The graph displays how employment duration in the year evolves according to monthly average earnings, in 2012 prices. Duration is expressed in weeks.

Figure 3.6: Quitting Probability



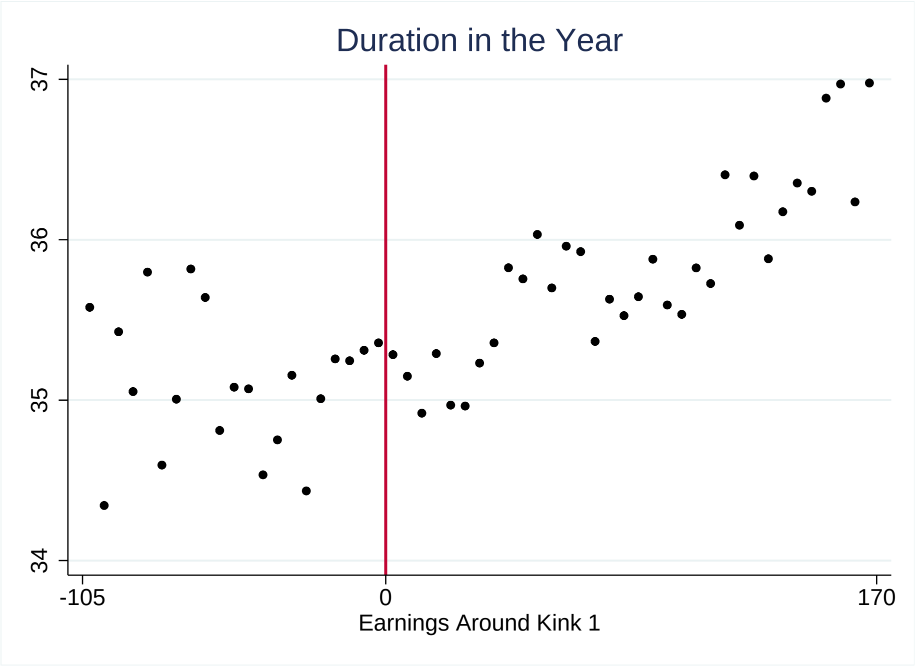

The graph displays how the prob. of quitting in the year evolves according to monthly average earnings, in 2012 prices. Duration is expressed in weeks.

Figure 3.7: Quitting Probability



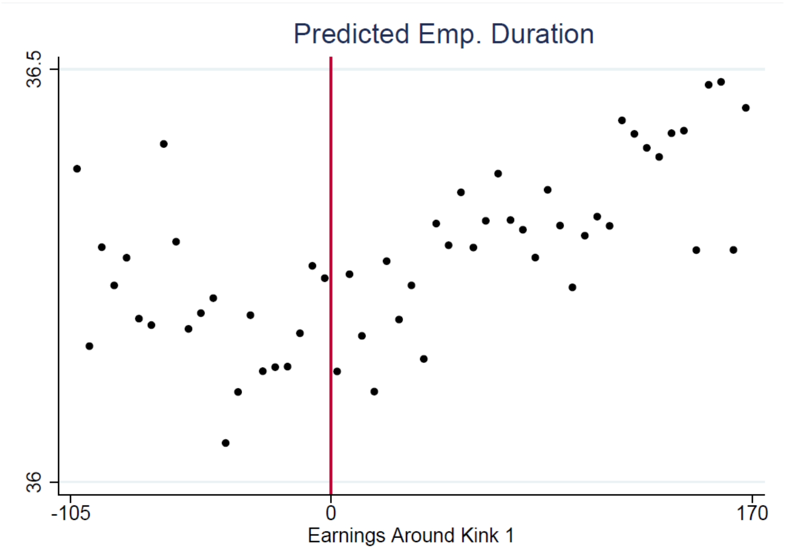

The graph displays how the prob. of quitting in the year evolves according to monthly average earnings, in 2012 prices. Duration is expressed in weeks.

Figure 3.8: Employment Duration Around Kink 1 - All Years



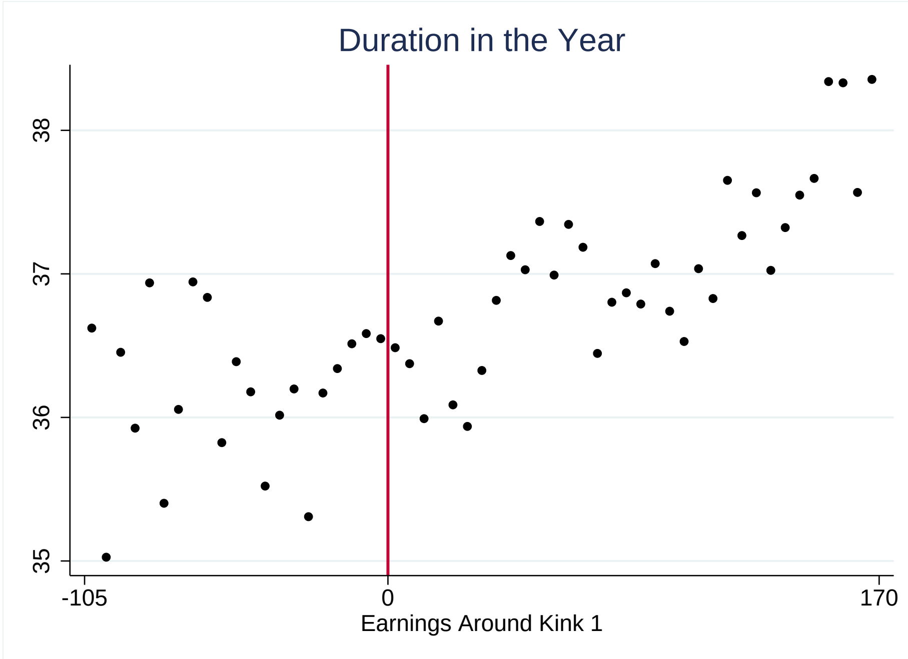

The graph displays how employment duration in the year evolves around the kink. Duration is expressed in weeks.

Figure 3.9: Predicted Employment Duration Around Kink 1 - All Years



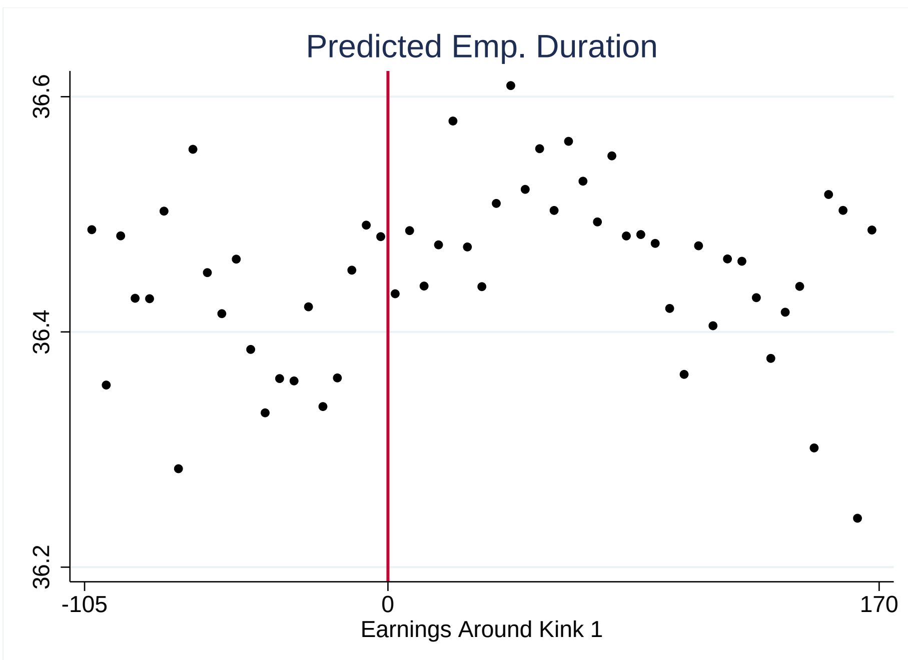

The graph displays how predicted employment duration in the year, based on a extremely rich set of covariates, evolves around the kink. Duration is expressed in weeks. See text for details on the construction of this predicted variabel.

Figure 3.10: Employment Duration Around Kink 1 - Restricted Sample



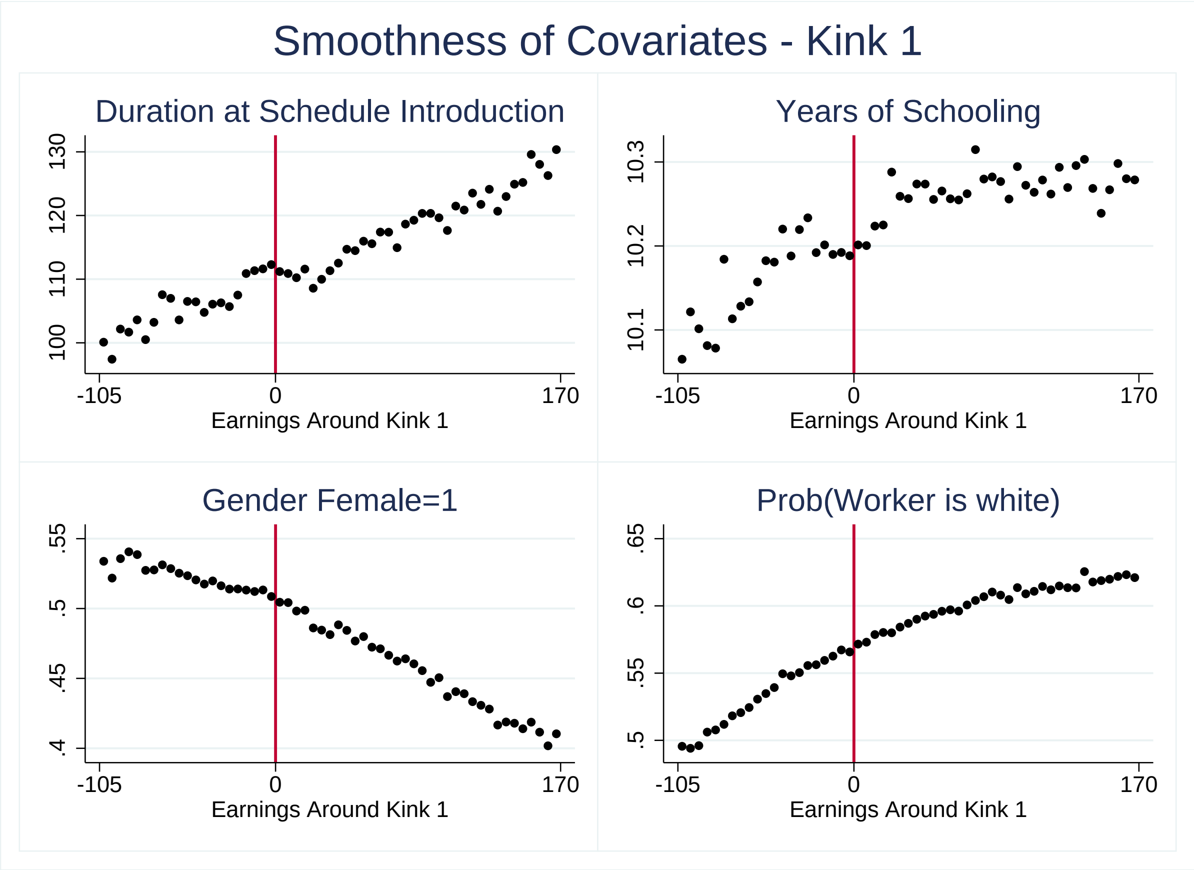

The graph displays how employment duration in the year evolves around the kink. This sample is restricted to the years from 2007 to 2012. Duration is expressed in weeks.

Figure 3.11: Predicted Employment Duration Around Kink 1 - Restricted Sample



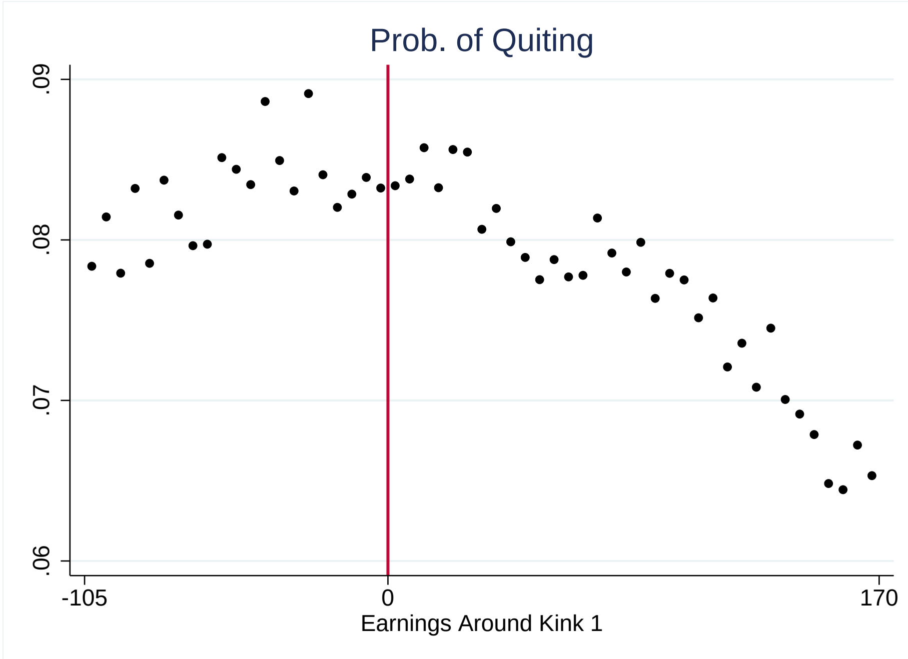

The graph displays how predicted employment duration in the year evolves around the kink. This sample is restricted to the years from 2007 to 2012. Duration is expressed in weeks. See text for details on the construction of this predicted variabel.

Figure 3.12: Covariates Around Kink 1 - Restricted Sample



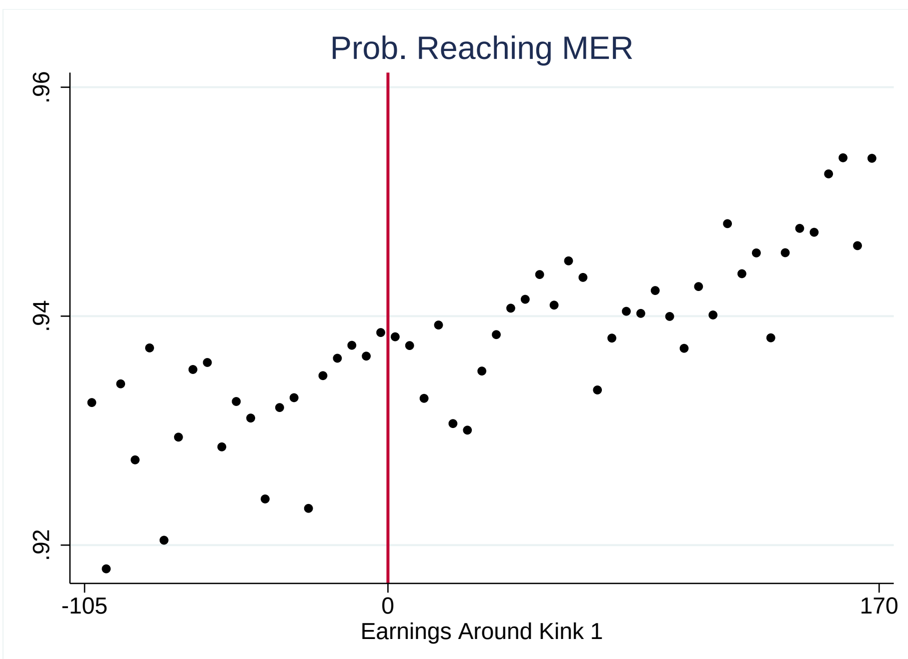

The graph displays how pre-determined covariates evolves around the kink. This sample is restricted to the years from 2007 to 2012. Duration is expressed in weeks.

Figure 3.13: Quitting Probabilities Around Kink 1 - Restricted Sample



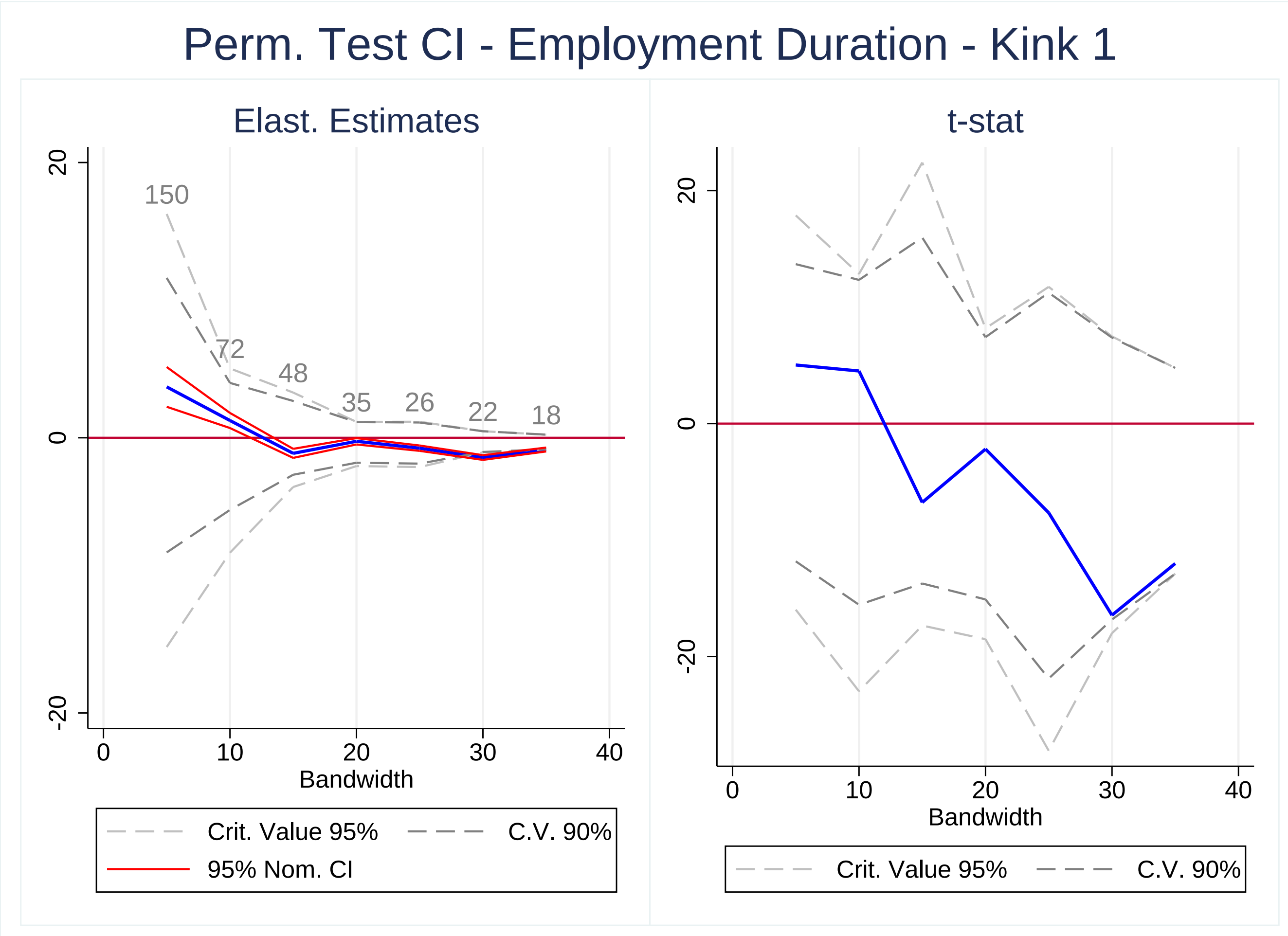

The graph displays how the prob. of quitting in the year evolves around the kink. This sample is restricted to the years from 2007 to 2012. Duration is expressed in weeks.

Figure 3.14: UI Min. Eligibility Requirement Around Kink 1 - Restricted Sample



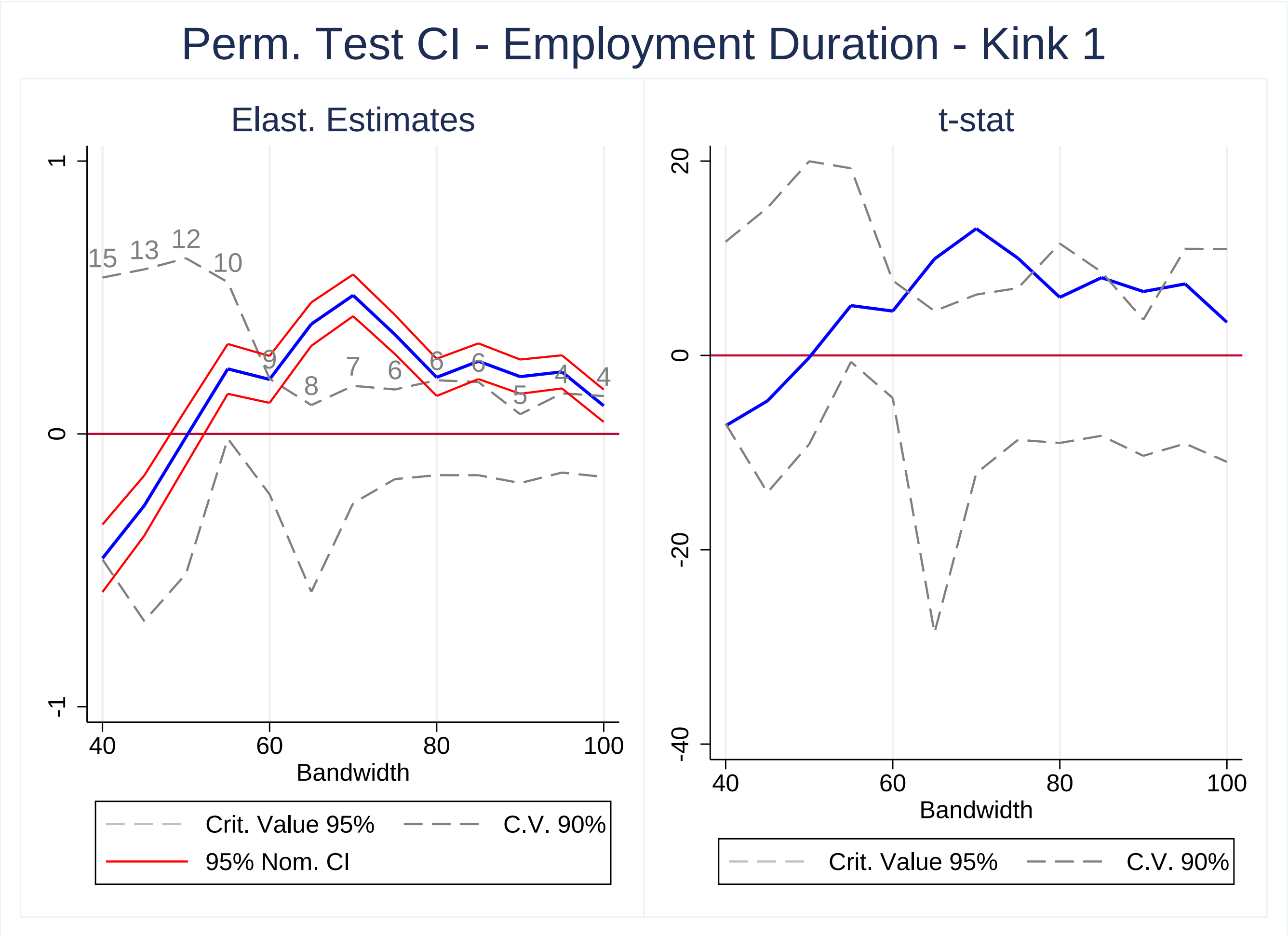

The graph displays how the prob. of reaching MER in the year evolves around the kink. This sample is restricted to the years from 2007 to 2012. Duration is expressed in weeks.

Figure 3.15: RKD Results with Permutation Test Critical Values Around Kink 1 - Small Bandwidths - Restricted Sample



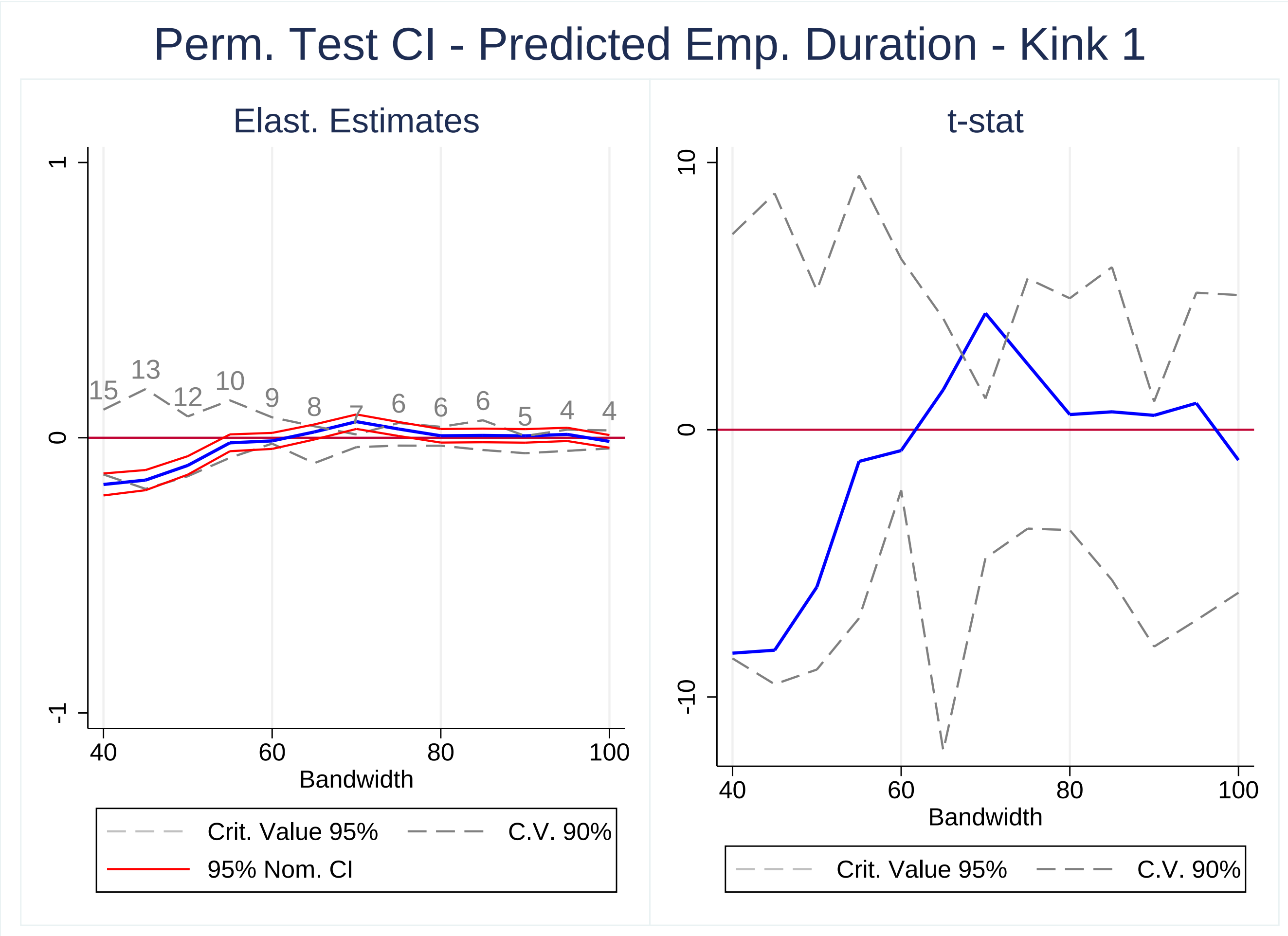

The graph displays elasticities estimates and t-statistics based on a local linear regressions around kink 1 for varying bandwidths (blue line). The gray lines display the critical value in a two-sided test for rejecting the null hypothesis of zero effect, based on permutation tests along as many as possible placebo points located between the minimum wage and R$2500 (2012 prices). The gray number displays the number of placebo points for this test, according to the LLR bandwidth. This sample is restricted to the years from 2007 to 2012. Duration is expressed in weeks. Standard errors are clustered at the firm level.

Figure 3.16: RKD Results with Permutation Test Critical Values Around Kink 1 - Restricted Sample



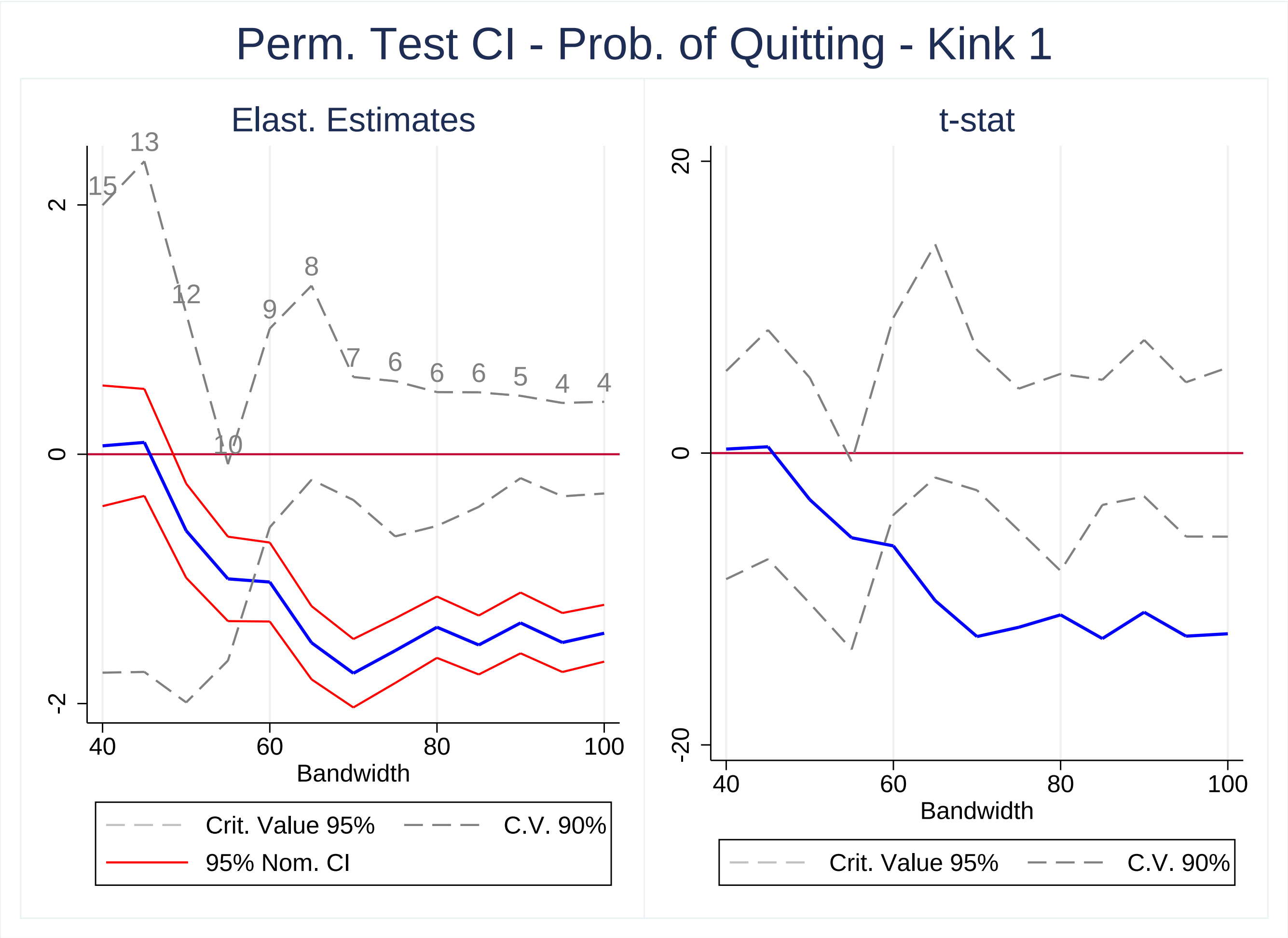

The graph displays elasticities estimates and t-statistics based on a local linear regressions around kink 1 for varying bandwidths (blue line). The gray lines display the critical value in a two-sided test for rejecting the null hypothesis of zero effect, based on permutation tests along as many as possible placebo points located between the minimum wage and R$2500 (2012 prices). The gray number displays the number of placebo points for this test, according to the LLR bandwidth. This sample is restricted to the years from 2007 to 2012. Duration is expressed in weeks. Standard errors are clustered at the firm level.

Figure 3.17: RKD Results with Permutation Test Critical Values Around Kink 1 - Restricted Sample



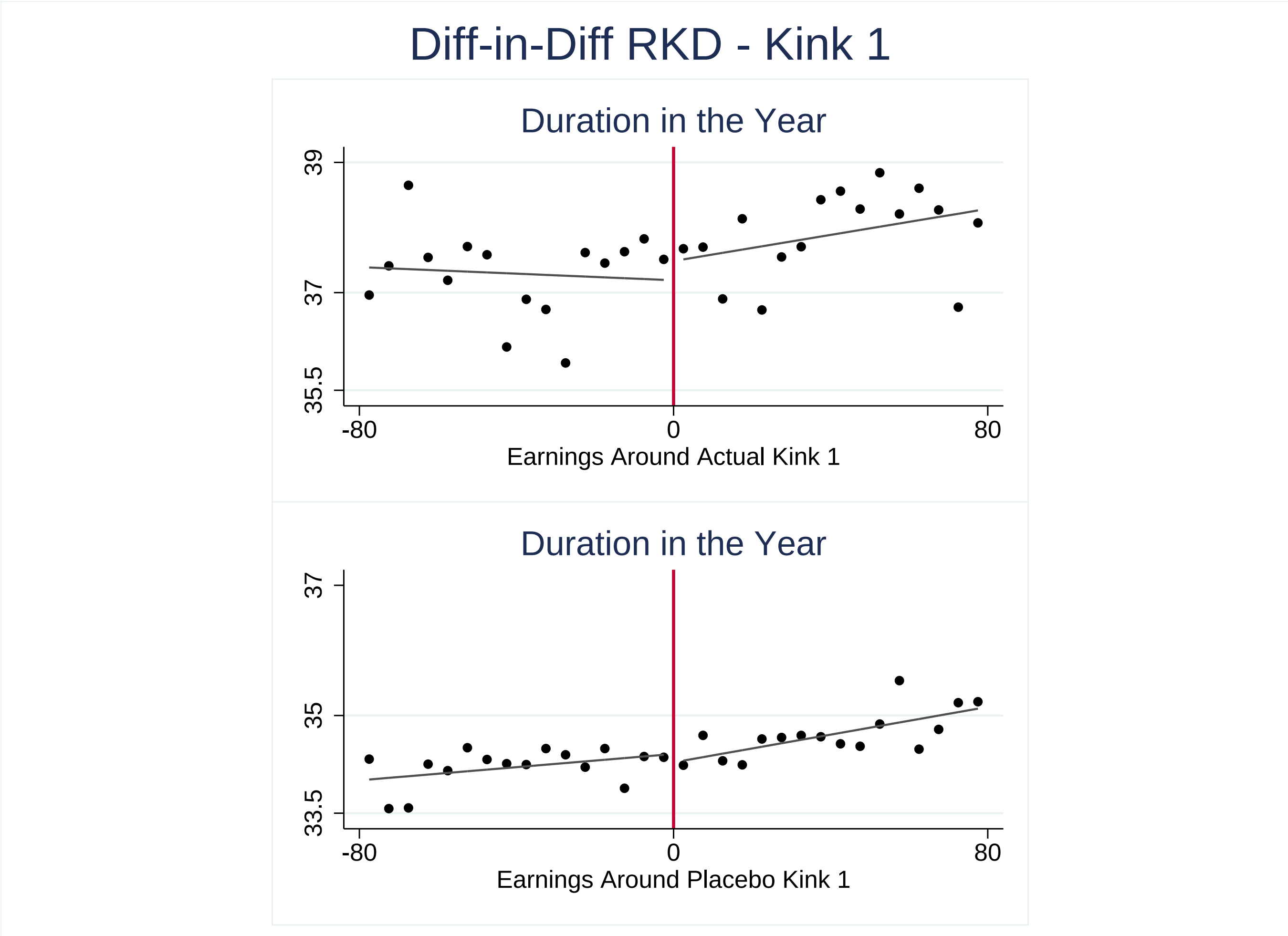

The graph displays elasticities estimates and t-statistics based on a local linear regressions around kink 1 for varying bandwidths (blue line). The gray lines display the critical value in a two-sided test for rejecting the null hypothesis of zero effect, based on permutation tests along as many as possible placebo points located between the minimum wage and R$2500 (2012 prices). The gray number displays the number of placebo points for this test, according to the LLR bandwidth. This sample is restricted to the years from 2007 to 2012. Duration is expressed in weeks. Standard errors are clustered at the firm level.

Figure 3.18: RKD Results with Permutation Test Critical Values Around Kink 1 - Restricted Sample




The graph displays elasticities estimates and t-statistics based on a local linear regressions around kink 1 for varying bandwidths (blue 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$2500 (2012 prices). The gray number displays the number of placebo points for this test, according to the LLR bandwidth. This sample is restricted to the years from 2007 to 2012. Duration is expressed in weeks. Standard errors are clustered at the firm level.

Figure 3.19: Diff. in Diff. RKD - Actual Policy Kink vs. Previous Years Placebo - Kink 1




Note: The table shows how employment duration evolves around actual policy kinks (top pane) compared to how the same outcome evolves three years before around the same points, when no actual policy kink was in place (bottom panel). These results consider the policy kink of the years 2009, 2010 and 2012, and use as placebo kinks the same points on data in years 2006, 2007, and 2009. It is necessary to use a three years and drop some years in order to have a minimum bandwidth range around the kink points.

Table 3.2: Monte Carlo Simulations - Bandwidth Selector Performance - Kink 1

| *First Kink* | | | | | |
|---|---|---|---|---|---|
| Bandwidth Selector | Mean b.w. | RMSE/true value | Coverage | Bias | Rejects null |
| FG linear | 9.3 | 1.08 | 0.30 | -0.81 | 0.14 |
| CCT linear, no regularization | 18.5 | 1.10 | 0.05 | -1.06 | 0.43 |
| CCT linear | 11.3 | 1.03 | 0.09 | -0.98 | 0.13 |
| FG quadratic | 20.2 | 1.15 | 0.55 | -0.64 | 0.25 |
| CCT quadratic, no regularization | 20.0 | 0.85 | 0.56 | -0.63 | 0.19 |
| CCT quadratic | 16.7 | 0.80 | 0.70 | -0.51 | 0.14 |
| CCT linear, no regularization | 26.3 | 1.01 | 0.23 | -0.93 | 0.19 |
| CCT linear | 24.9 | 0.92 | 0.29 | -0.83 | 0.11 |
| CCT quadratic, no regularization | 31.7 | 0.70 | 0.81 | -0.28 | 0.21 |
| CCT quadratic | 28.5 | 0.74 | 0.91 | -0.22 | 0.21 |
| Fix b.w. - linear | 5 | 0.95 | 0.88 | -0.57 | 0.08 |
| Fix b.w. - linear | 10 | 0.97 | 0.08 | -0.93 | 0.08 |
| Fix b.w. - linear | 15 | 1.16 | 0.00 | -1.15 | 0.18 |
| Fix b.w. - linear | 20 | 1.28 | 0.00 | -1.28 | 0.85 |
| Fix b.w. - quadratic | 5 | 3.23 | 0.92 | -0.28 | 0.08 |
| Fix b.w. - quadratic | 10 | 1.20 | 0.92 | -0.29 | 0.12 |
| Fix b.w. - quadratic | 15 | 0.78 | 0.85 | -0.49 | 0.16 |
| Fix b.w. - quadratic | 20 | 0.80 | 0.54 | -0.70 | 0.15 |

Note: Results are based on 300 simulations. DGP is based on $5^{th}$ degree polynomial approximation of the actual data around this kink. The actual sample size is draw for each simulation. The true elasticity is 0.5. Bias is reported as a proportion on the actual slope change size. Standard errors are clustered at the firm level.

Table 3.3: Diff. in Diff. RKD Estimates - Kink 1

| Elasticities Estimates | Bandwidth | | | | | | |
|---|---|---|---|---|---|---|---|
| | 60 | 65 | 70 | 75 | 80 | 85 | 90 |
| *Dep. Variable* | | | | | | | |
| Employment Duration | 0.02 | 0.24*** | 0.53*** | 0.32*** | 0.17*** | 0.25*** | 0.15*** |
| | (0.059) | (0.055) | (0.053) | (0.050) | (0.049) | (0.049) | (0.047) |
| Prob. Of Quitting | -1.55*** | -2.09*** | -2.51*** | -2.12*** | -1.68*** | -1.76*** | -1.44*** |
| | (0.224) | (0.218) | (0.203) | (0.192) | (0.183) | (0.185) | (0.166) |
| Prob. Reach MER | -0.03 | 0.07*** | 0.16*** | 0.06*** | -0.02 | 0.01 | -0.01 |
| | (0.024) | (0.021) | (0.020) | (0.019) | (0.018) | (0.017) | (0.017) |
| Prob. of Firing | 0.23 | -0.07 | -0.69*** | -0.23 | 0.12 | 0.01 | 0.2 |
| | (0.175) | (0.162) | (0.152) | (0.143) | (0.137) | (0.134) | (0.130) |
| Predicted Emp. Duration | -0.15*** | -0.1*** | -0.02 | -0.07*** | -0.02 | 0 | 0 |
| | (0.017) | (0.015) | (0.014) | (0.014) | (0.014) | (0.014) | (0.014) |
| *obs* | 21,400,000 | 23,300,000 | 25,200,000 | 27,100,000 | 28,900,000 | 30,800,000 | 32,700,000 |

Note: The table displays elasticities estimates using a RKD in double differences. 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. These results consider the policy kink of the years 2009, 2010 and 2012, and use as placebo kinks the same points on data in years 2006, 2007, and 2009. It is necessary to use a three years and drop some years in order to have a minimum bandwidth range around the kink points. Standard errors are clustered at the firm level.

Table 3.4: Diff. in Diff. in Diff. RKD Estimates - Kink 1

| Elasticities Estimates | | Bandwidth | |
|---|---|---|---|
| | 55 | 60 | 65 |
| *Dep. Variable* | | | |
| Employment Duration | 0.54*** | 0.44*** | 0.49*** |
| | (0.081) | (0.076) | (0.072) |
| Prob. Of Quitting | -3.92*** | -3.56*** | -4.23*** |
| | (0.416587) | (0.380598) | (0.386473) |
| Prob. Reach MER | 0.13*** | 0.09** | 0.19*** |
| | (0.033888) | (0.041618) | (0.039456) |
| Prob. of Firing | -1.47*** | -0.99*** | -1.99*** |
| | (0.27688) | (0.25716) | (0.24443) |
| Predicted Emp. Duration | -0.22*** | -0.19*** | -0.33*** |
| | (0.02475) | (0.02237) | (0.02189) |
| *obs* | 35,300,000 | 38,500,000 | 41,400,000 |

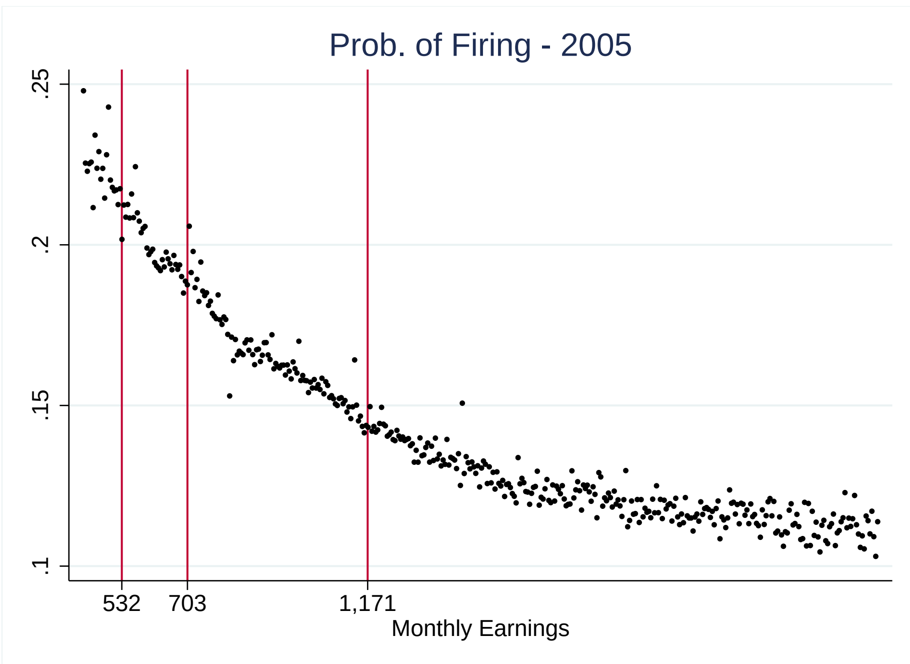
Note: The table displays elasticities estimates using a RKD in triple differences. These results are the difference between the DD-RKD from table (3.3) and the estimates from implementing the same DD-RKD in a point which had no actual policy kink in any year. These results consider the policy kink of the years 2009, 2010 and 2012, and use as placebo kinks the same points on data in years 2006, 2007, and 2009. It is necessary to use a three years and drop some years in order to have a minimum bandwidth range around the kink points. Standard errors are clustered at the firm level.

## 3.6 Appendix

### 3.6.1 Figures

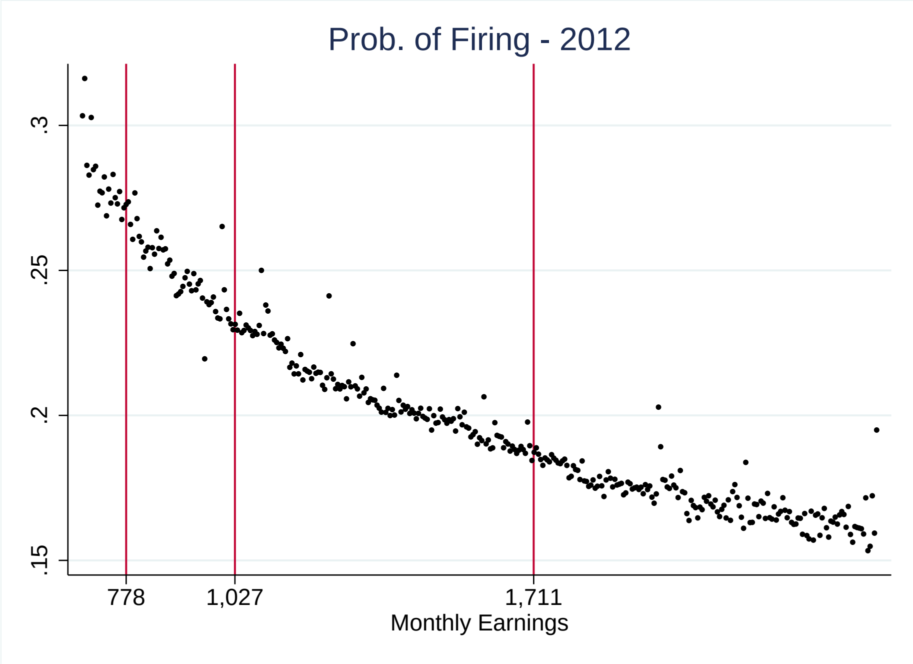
**Supplementary Results - Kink 1**

Figure A1



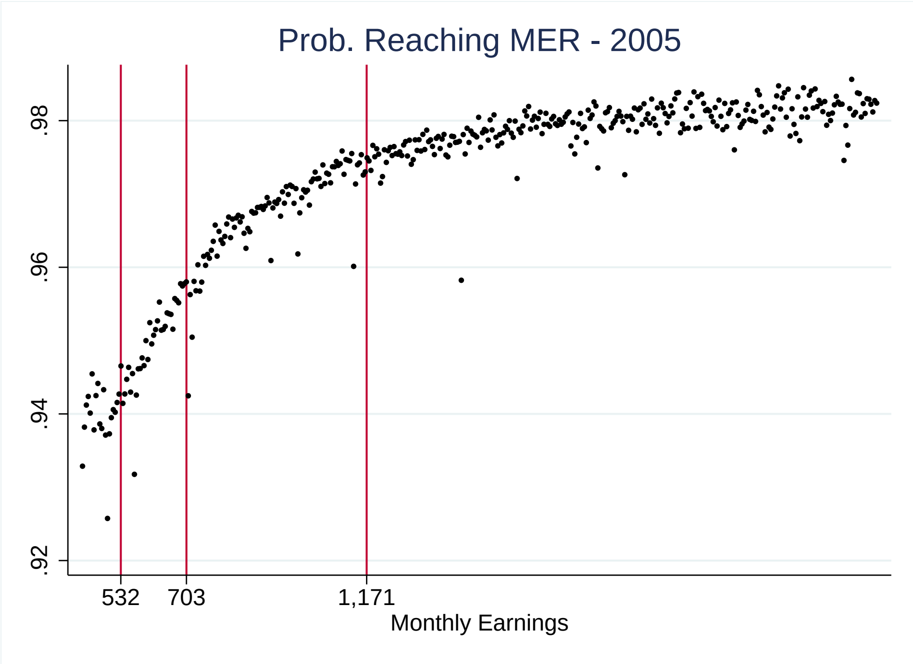

The graph displays how the prob. of lay-off in the year evolves according to monthly average earnings, in 2012 prices. Duration is expressed in weeks

Figure A2



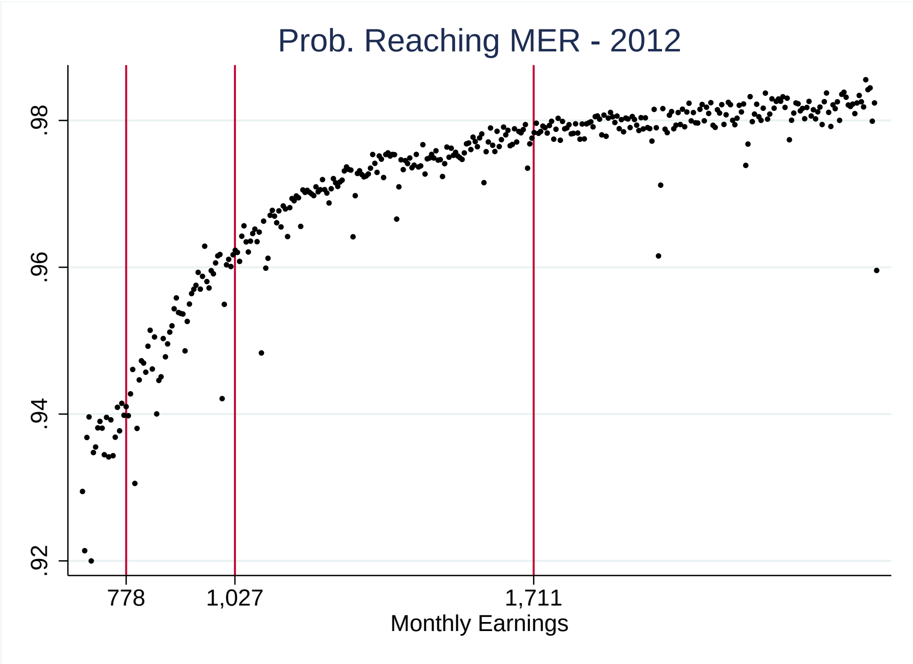

The graph displays how the prob. of lay-off in the year evolves according to monthly average earnings, in 2012 prices. Duration is expressed in weeks

Figure A3



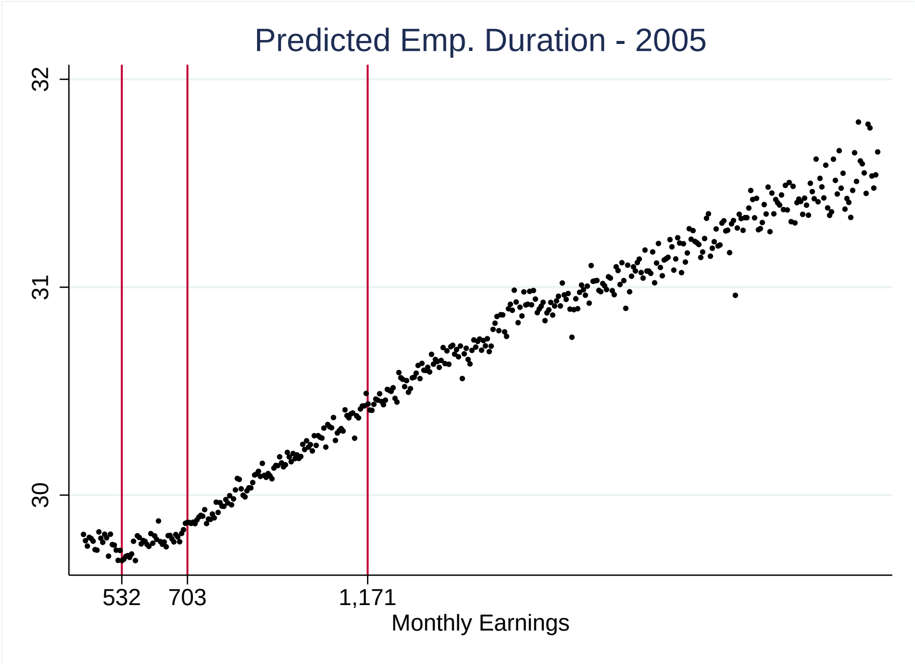

The graph displays how the prob. of reaching MER in the year evolves according to monthly average earnings, in 2012 prices. Duration is expressed in weeks

Figure A4



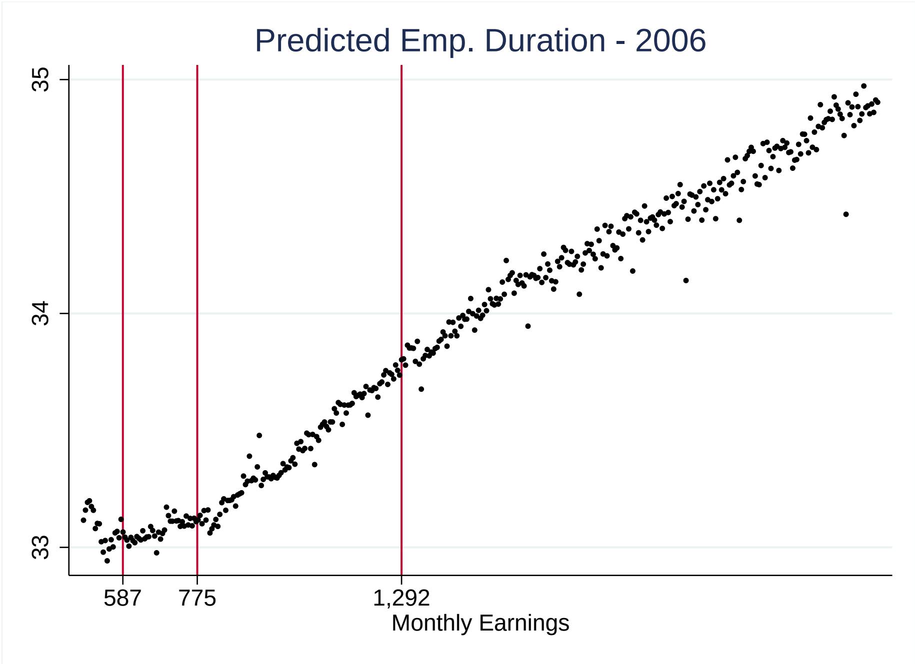

The graph displays how the prob. of reaching MER in the year evolves according to monthly average earnings, in 2012 prices. Duration is expressed in weeks

Figure A5



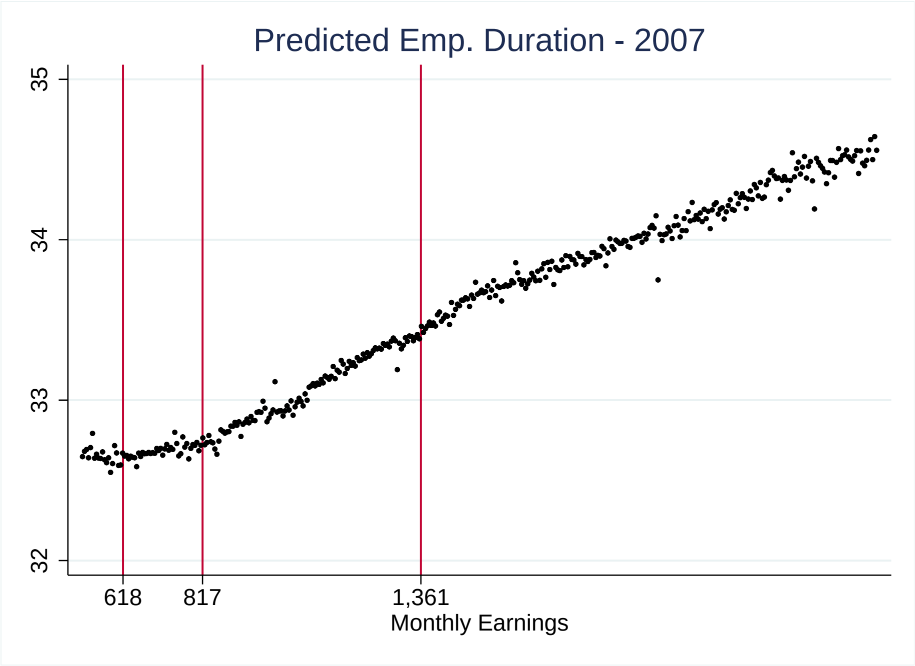

The graph displays how the predicted duration of employment in the year evolves according to monthly average earnings, in 2012 prices. Duration is expressed in weeks

Figure A6



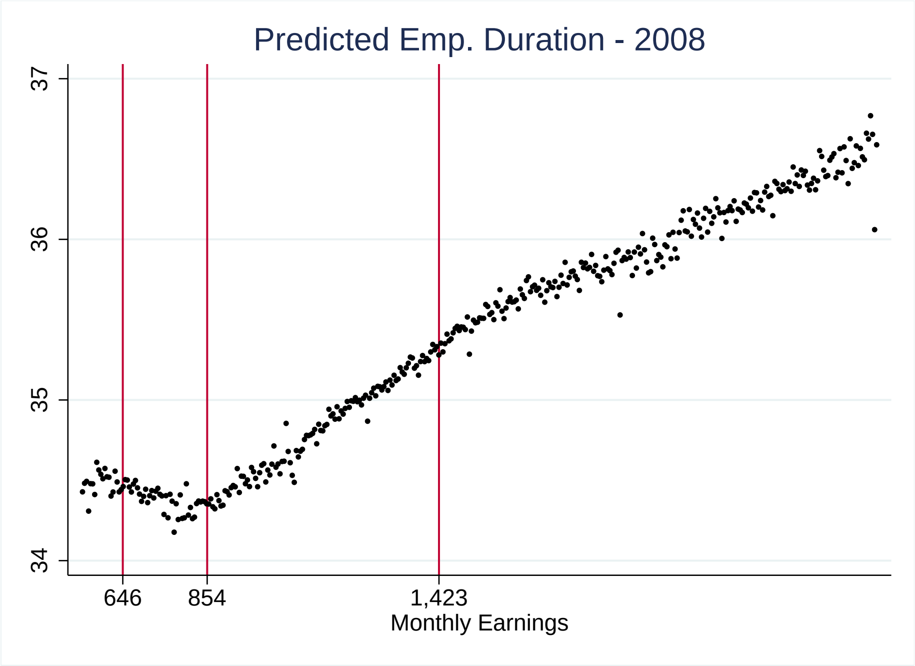

The graph displays how the predicted duration of employment in the year evolves according to monthly average earnings, in 2012 prices. Duration is expressed in weeks

Figure A7



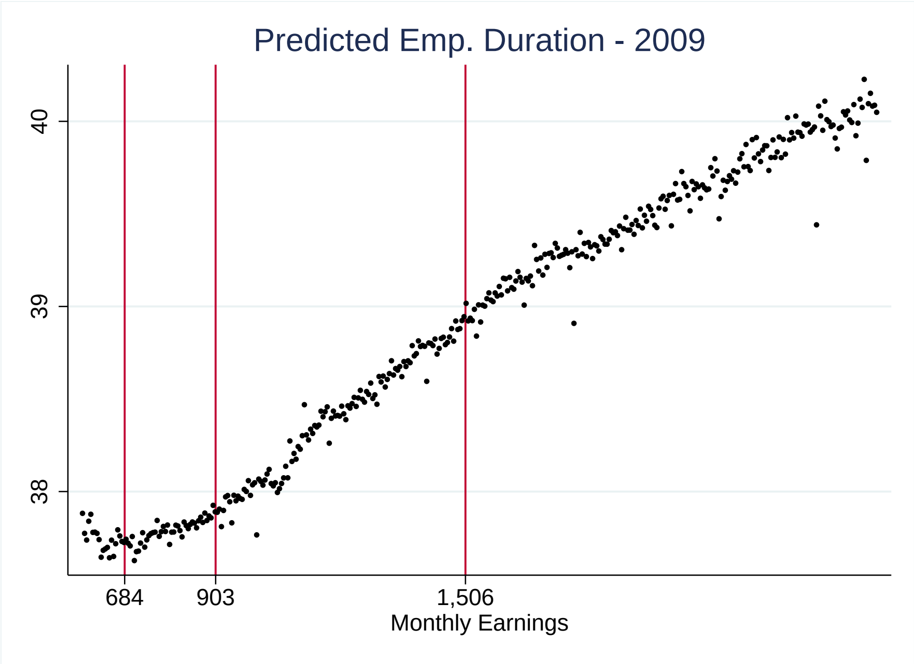

The graph displays how the predicted duration of employment in the year evolves according to monthly average earnings, in 2012 prices. Duration is expressed in weeks

Figure A8



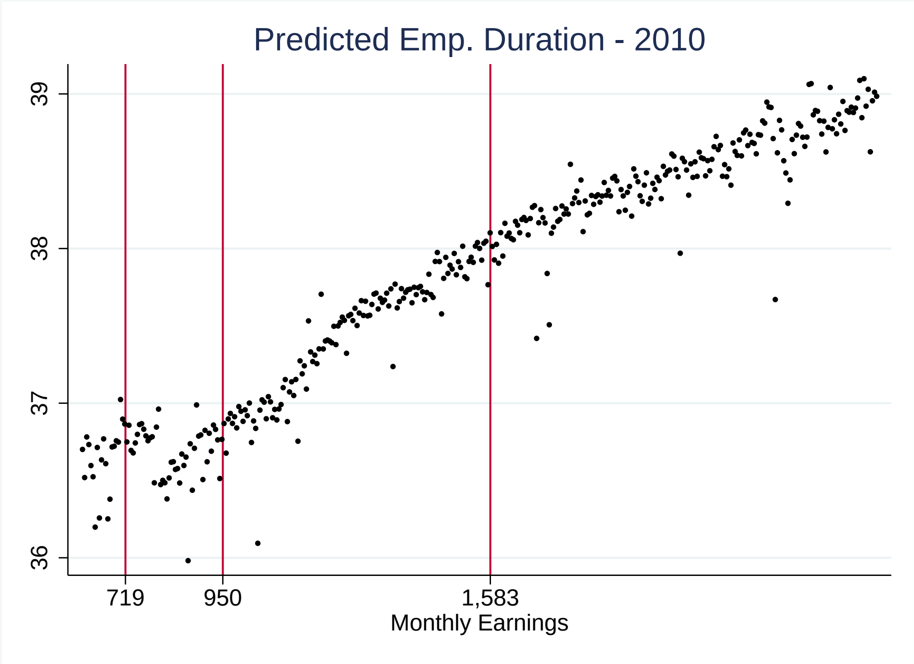

The graph displays how the predicted duration of employment in the year evolves according to monthly average earnings, in 2012 prices. Duration is expressed in weeks

Figure A9



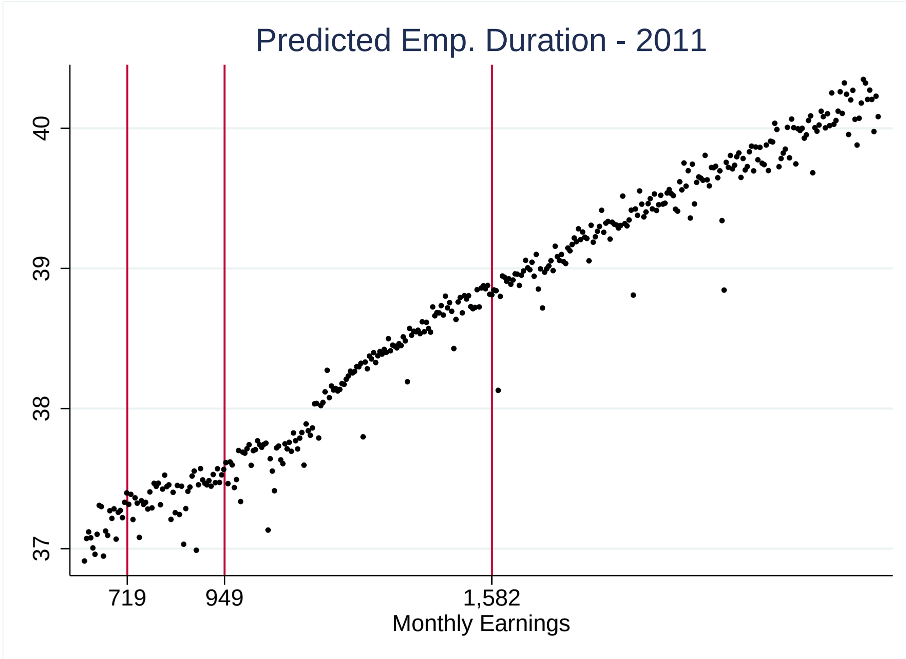

The graph displays how the predicted duration of employment in the year evolves according to monthly average earnings, in 2012 prices. Duration is expressed in weeks

Figure A10



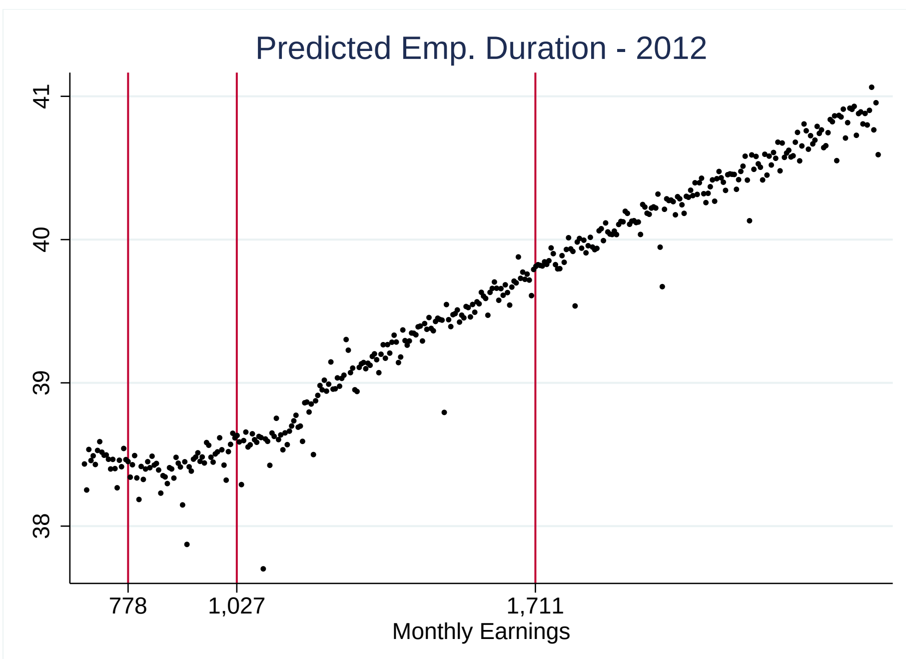

The graph displays how the predicted duration of employment in the year evolves according to monthly average earnings, in 2012 prices. Duration is expressed in weeks

Figure A11



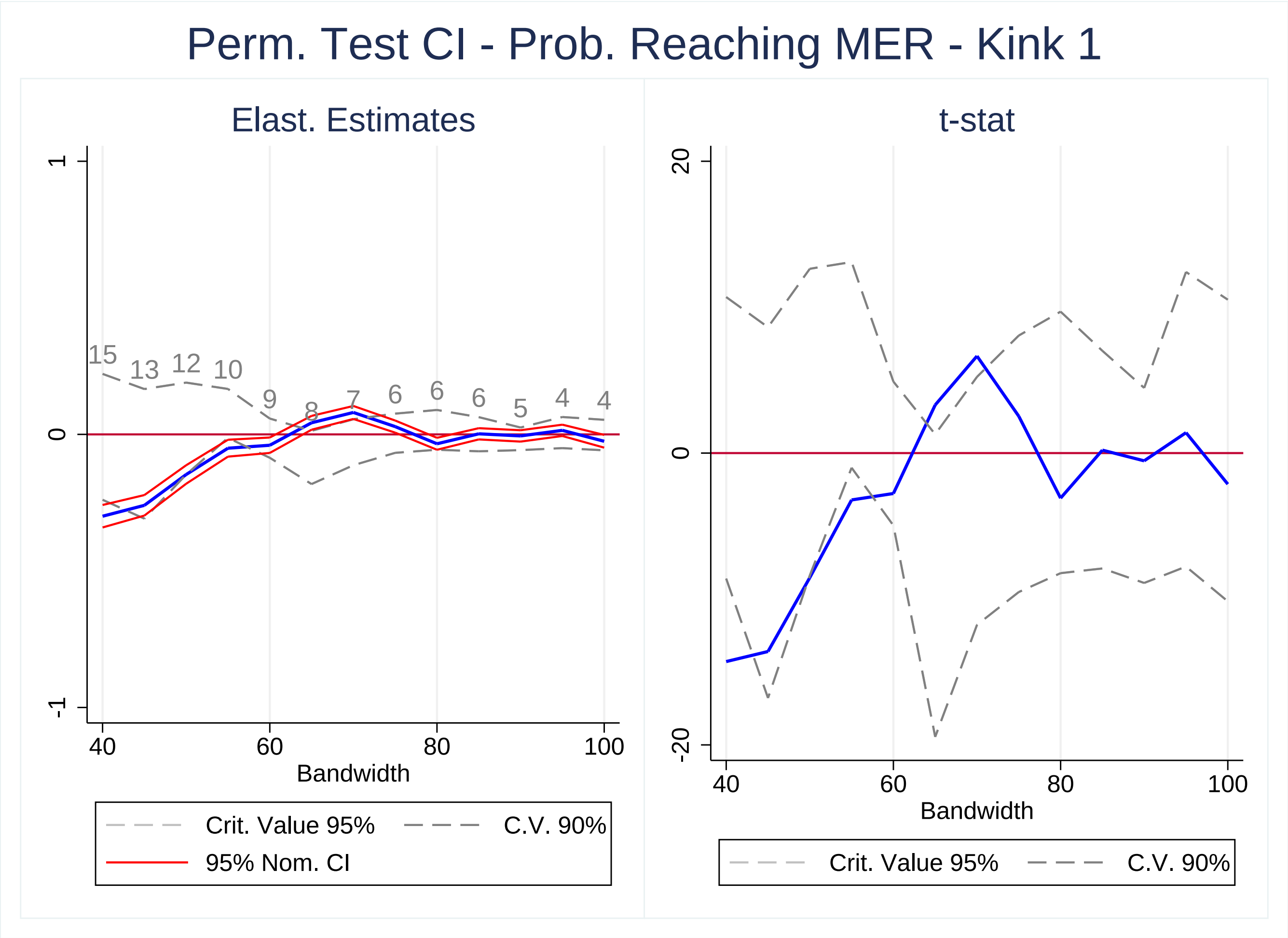

The graph displays how the predicted duration of employment in the year evolves according to monthly average earnings, in 2012 prices. Duration is expressed in weeks

Figure A12



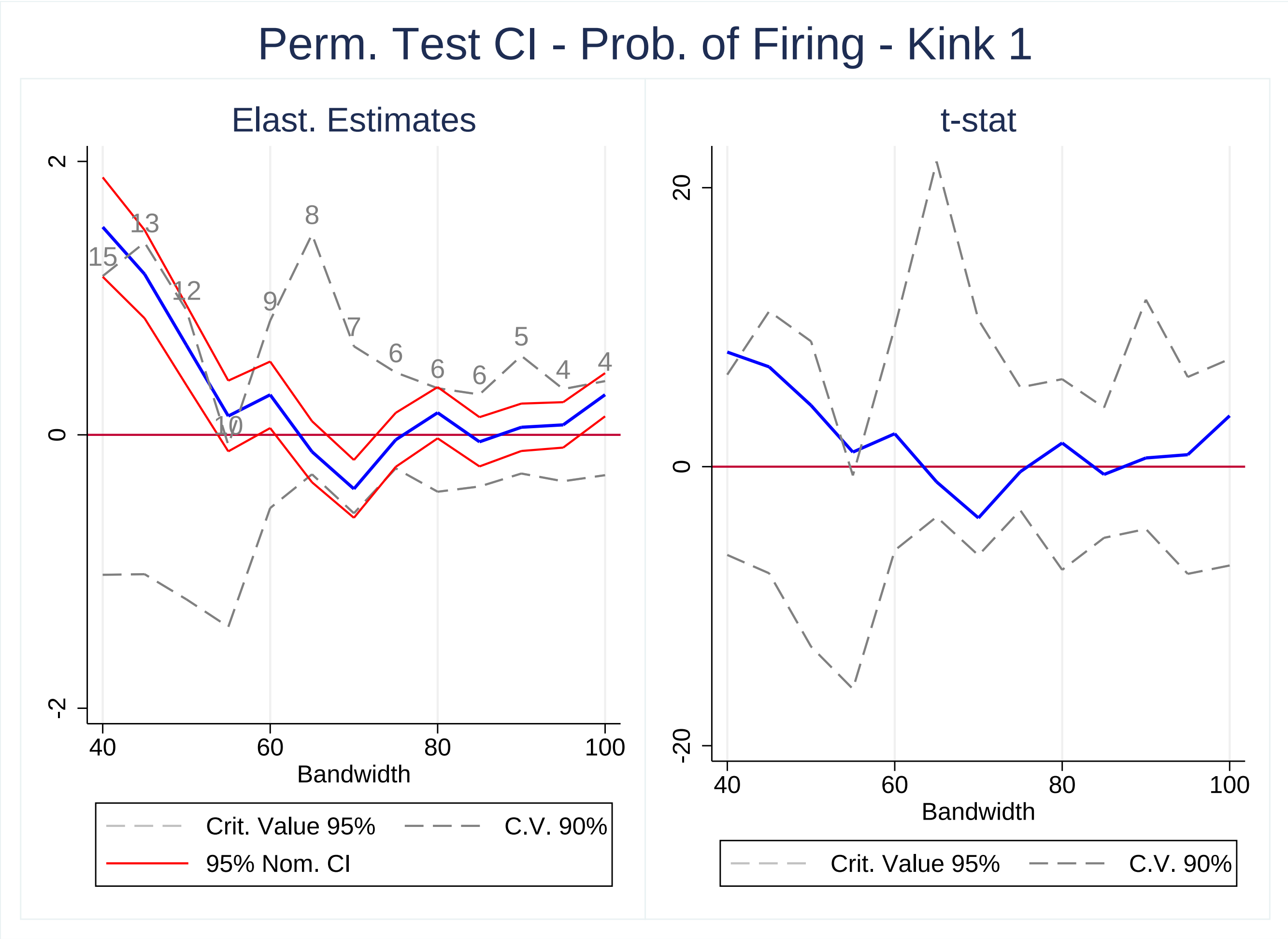

The graph displays how the predicted duration of employment in the year evolves according to monthly average earnings, in 2012 prices. Duration is expressed in weeks

Figure A13: RKD Results with Permutation Test Critical Values Around Kink 1 - Restricted Sample




The graph displays elasticities estimates and t-statistics based on a local linear regressions around kink 1 for varying bandwidths (blue 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$2500 (2012 prices). The gray number displays the number of placebo points for this test, according to the LLR bandwidth. This sample is restricted to the years from 2007 to 2012. Duration is expressed in weeks. Standard errors are clustered at the firm level.

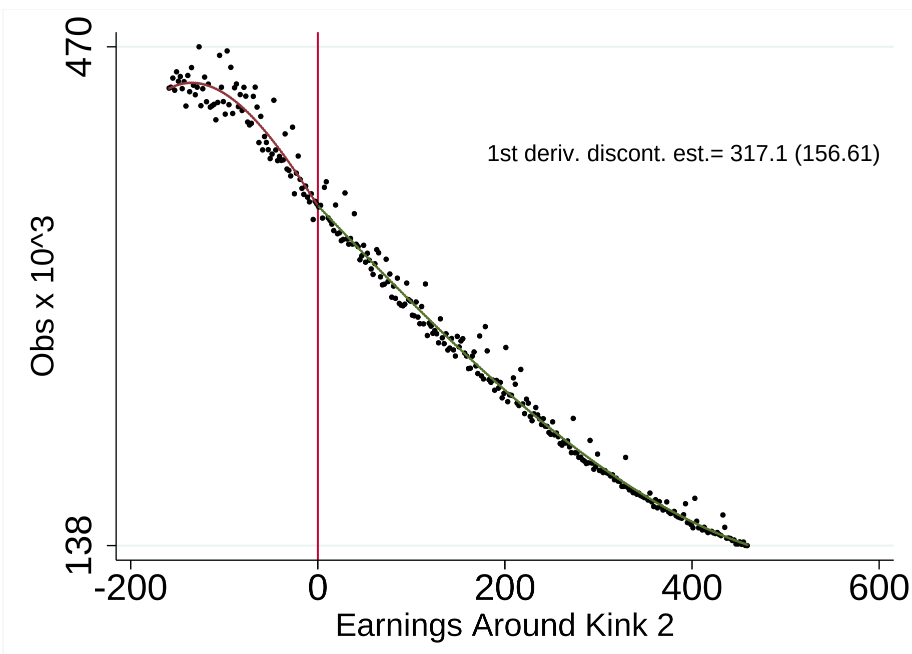
Figure A14: RKD Results with Permutation Test Critical Values Around Kink 1 - Restricted Sample




The graph displays elasticities estimates and t-statistics based on a local linear regressions around kink 1 for varying bandwidths (blue 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$2500 (2012 prices). The gray number displays the number of placebo points for this test, according to the LLR bandwidth. This sample is restricted to the years from 2007 to 2012. Duration is expressed in weeks. Standard errors are clustered at the firm level.

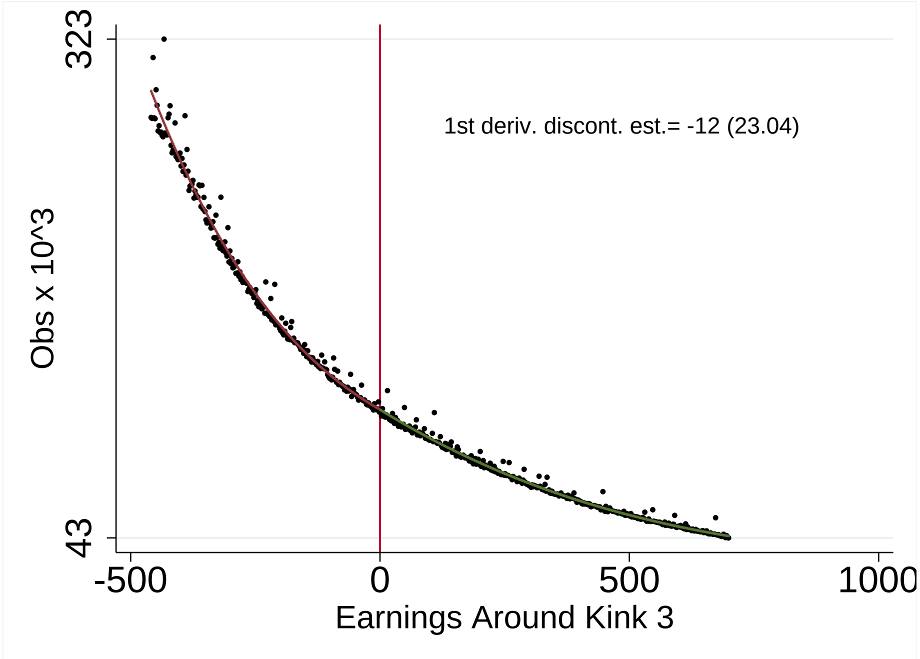
### 3.6.2 Supplementary Results - Kink 2 and 3

Figure B1: Density of Wages the Around Kink 2



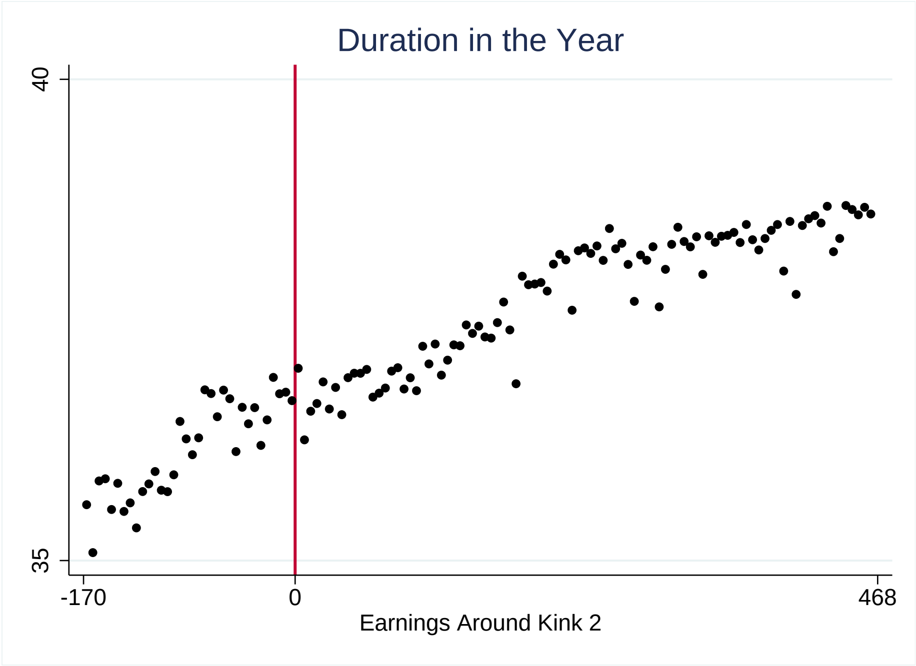

The graph displays how the density of earnings evolve around the kink. At each side of the kink, the density is approximated by the polynomial which minimizes the Akaike Criterion. The graph also displays the test statistics for the slope change of these polynomials at the kink. See the text for details.

Figure B2: Density of Wages the Around Kink 3



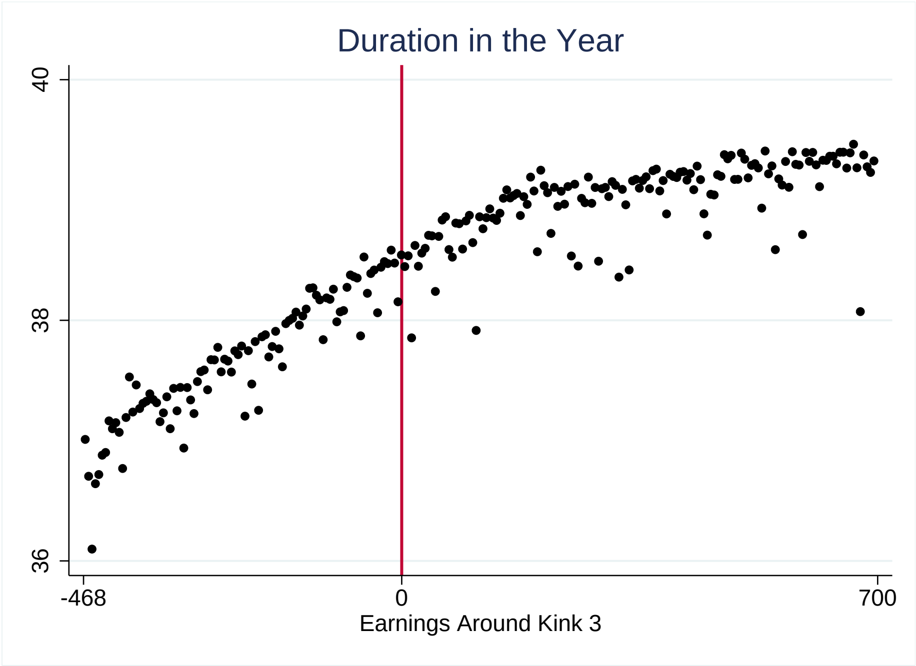

The graph displays how the density of earnings evolve around the kink. At each side of the kink, the density is approximated by the polynomial which minimizes the Akaike Criterion. The graph also displays the test statistics for the slope change of these polynomials at the kink. See the text for details.

Figure B3: Employment Duration Around Kink 2




The graph displays how employment duration in the year evolve around the kink. Duration is expressed in weeks

Figure B4: Employment Duration Around Kink 3




The graph displays how employment duration in the year evolve around the kink. Duration is expressed in weeks

### 3.6.3 Tables

Table 3.5: RKD Estimates based on standard Bandwidth Selectors

| Elasticity of Employment Duration with Respect to Benefit Level | | | | |
|---|---|---|---|---|
| Bandwidth Selectors | b.w. | Estimated Elasticity | std. Error | p-robust |
| *First Kink* | | | | |
| FG linear | 11.6 | 0.31*** | (0.103) | |
| CCT linear, no regularization | 5.0 | -0.23 | (0.367) | (0.125) |
| CCT linear | 4.6 | -1.35*** | (0.404) | (0.000) |
| FG quadratic | 207.7 | 0.86*** | (0.008) | |
| CCT quadratic, no regularization | 8.4 | -2.33*** | (0.659) | (0.000) |
| CCT quadratic | 9.1 | -3.05*** | (0.590) | (0.000) |
| *Second Kink* | | | | |
| FG linear | 33.9 | 0.75*** | (0.072) | |
| CCT linear, no regularization | 20.1 | 0.84*** | (0.157) | (0.000) |
| CCT linear | 19.6 | 1*** | (0.163) | (0.000) |
| FG quadratic | 72.3 | 1.33*** | (0.092) | |
| CCT quadratic, no regularization | 43.6 | 2.28*** | (0.197) | (0.000) |
| CCT quadratic | 29.1 | -0.48* | (0.361) | (0.067) |
| *Third Kink* | | | | |
| FG linear | 104.7 | -0.04** | (0.017) | |
| CCT linear, no regularization | 63.5 | 0.1*** | (0.037) | (0.162) |
| CCT linear | 66.3 | 0.04 | (0.035) | (0.934) |
| FG quadratic | 189.0 | -0.12*** | (0.029) | |
| CCT quadratic, no regularization | 59.8 | -0.13 | (0.162) | (0.945) |
| CCT quadratic | 53.8 | -0.05 | (0.190) | (0.584) |

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 (4.6). P-robust displays p-values based on CCT robust confidence intervals.

## 3.7 Appendix

### 3.7.1 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 for a given level of benefits as stated in equation (4). First-order conditions are given by: [9]

$$\psi'(s_t) = E_t(A_t) - U_t(A_t) \tag{3.14}$$

The optimal level of search intensity is simply the one where the marginal cost of search (left-hand-side of the equation) equals the net gain from finding a new job (right-hand-side of the equation). Such gains are given by the difference between the value of a new job $E_t(A_t)$ and the value of unemployment $U_t(A_t)$. The larger the value of finding a new job *vis-a-vis* the value of remaining unemployed, the greater the incentive to search.

At this point, it is possible to approach the question of how a small change in the level of benefits affects the incentives to search. From the previous first-order condition and by applying the envelope theorem we have: [10]

$$\frac{\partial s_t}{\partial b_t} = \frac{-u'(c_t^u)}{\psi''(s_t)} < 0 \tag{3.15}$$

It shows that an \$1 increase in UI benefits decreases search intensity by an amount which depends on the marginal utility of consumption of the unemployed worker adjusted by how the marginal cost of search is increasing at a given point. For instance, it means that if an unemployed worker is already enjoying a high level of consumption, his marginal utility of consumption is low and, thus, a small increase in benefits will not affect by much his level of search intensity.

As in Chetty (2008), it is possible to show that an increase in the level of benefits affects search through two distinct channels: a liquidity and a moral hazard effect. With this purpose, we notice that:

[9] 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.

[10] The envelope theorem states that for small changes of parameter values in an optimization problem, the relevant effects on the function of interest are the direct effects. It means that indirect effects should be ignored. In our case, $b_t$ is the changing parameters of the optimization problem. The envelope condition allows us to ignore the effects of $b_t$ on both $E_t(A_t)$ and $U_t(A_t)$. In other words, $\frac{\partial E_t(A_t)}{\partial b_t} = 0$ and $\frac{\partial U_t(A_t)}{\partial b_t} = 0$.

$$\frac{\partial s_t}{\partial A_t} = \frac{v'(c_t^e) - u'(c_t^u)}{\psi''(s_t)} \leq 0 \tag{3.16}$$

$$\frac{\partial s_t}{\partial w_t} = \frac{v'(c_t^e)}{\psi''(s_t)} \geq 0 \tag{3.17}$$

Equation (7) shows that the larger the gap between the marginal utilities of consumption when employed ($v'(c_t^e)$) and unemployed ($u'(c_t^u)$), the larger the effect of an increase in the agent's asset level on search intensity. This means that when unemployed workers are significantly liquidity constrained, and so the gap in consumption between unemployment and employment is sizable, providing an extra small amount of liquidity to this agent will lower his search intensity significantly. Equation (8), instead, shows that an increase in the pay-off of finding a new job (higher $w_t$) positively affects search intensity. The magnitude of this effect depends on the marginal utility of consumption when employed adjusted by the slope of the marginal cost of search at a given point. The intuition is that if consumption when employed is already large, so marginal utility is low, an small increase of $w_t$ does not change substantially the reward of finding a job and, thus, the change in search intensity is small.

By combining the results of (7) and (8) with (6), we can decompose the marginal distortion of unemployment benefits on search in two distinct elements: liquidity and moral hazard effect:

$$\frac{\partial s_t}{\partial b_t} = \frac{\partial s_t}{\partial A_t} - \frac{\partial s_t}{\partial w_t} < 0 \tag{3.18}$$

This is the core result provided by Chetty (2008). It highlights that the effect of UI benefits on 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 that fact unemployment benefits distort the pay-off 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.[11] 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.

For example, suppose an unemployed worker has zero liquid assets, no access to credit markets and is not entitled to UI benefits in this model. This worker searches very intensely for a new job because he

[11] 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.

cannot borrow against the future and, thus, he experiences a very low (zero) level of consumption while unemployed. If he is granted with a lump sum cash grant, he will decrease search effort not because the net benefit of finding a job changes (as is the case for UI benefits), but simply because he is less liquidity constrained and can now better smooth consumption across states. Therefore, a hypothetical cash grant unveils the liquidity effects on search. Such effect is embodied in the total distortion caused by UI benefits because it also provides more liquidity to workers.

The other part of the distortion relates to the fact that when such a worker is granted with UI, the net benefit of finding a new job decreases. This happens because, differently from the cash grant, benefits cease as soon as he comes back to employment, representing a decrease in the reward of finding a new job.

Equation (3.18), however, shows how the effect of an *one period* increase in UI benefits on search can be decomposed into a liquidity and a moral hazard effect. The expression bellow shows how the decomposition applies for a B-periods increase in the level of UI benefits, as shown in Appendix 3.7.3:

$$\frac{\partial s_t}{\partial b} = \frac{\partial s_t}{\partial a}|_B - \frac{\partial s_t}{\partial w}|_B \tag{3.19}$$

where $\frac{\partial s_t}{\partial b}$ denotes the effect on search at the initial period when the level of UI benefits increase for all the B periods. $\frac{\partial s_t}{\partial a}|B = \sum_{i=t}^{t+B-1} \frac{\partial s_t}{\partial a_i}$ and $\frac{\partial s_t}{\partial w}|B = \sum_{i=t}^{t+B-1} \frac{\partial s_t}{\partial w_i}$ describe, respectively, the liquidity and moral hazard effect on search in the first B periods of unemployment.

### 3.7.2 Benefit Level and the Choice of Work Effort by the Employed

Here I approach the problem of how variations in benefit levels affect the the choice of effort by the employed. This analysis is analytically symmetric to the one found in the previous subsection. Equation (2) states the problem faced by the employed worker. From there it is possible to derive the following first-order condition:[12]

$$c'(e_t) = V_t(A_t) - U_t(A_t) \tag{3.20}$$

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.

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

From the first-order condition above, it is possible to assess how work effort reacts to a small variation in the level of benefits:

$$\frac{\partial e_t}{\partial b_t} = \frac{-u'(c_t^u)}{c''(e_t)} < 0 \tag{3.21}$$

If the agent's level of consumption when unemployed is expected to be low, so that marginal utility is high, the distortion caused by a small variation in potential benefits will be large. Analogously to the case analyzed in the previous subsection, it is possible to decompose this distortion into a liquidity and a moral hazard problem. The derivatives below show how the level of work effort reacts to a small change in asset levels and wages, respectively:

$$\frac{\partial e_t}{\partial A_t} = \frac{v'(c_t^v) - u'(c_t^u)}{c''(e_t)} \leq 0 \tag{3.22}$$

$$\frac{\partial e_t}{\partial w_t} = \frac{v'(c_t^v)}{c''(e_t)} \geq 0 \tag{3.23}$$

By combining equation (12) and (13) with (11), we can decompose the effect of UI benefits on work effort as:

$$\frac{\partial e_t}{\partial b_t} = \frac{\partial e_t}{\partial A_t} - \frac{\partial e_t}{\partial w_t} < 0 \tag{3.24}$$

Similarly to the case of the unemployed, this equation shows that effects of benefits on work effort depends on two distinct factors: a liquidity ($\frac{\partial e_t}{\partial A_t}$) and a moral hazard ($\frac{\partial e_t}{\partial w_t}$) component. The moral hazard effect concerns the fact that UI benefits imply a decrease of the net loss caused by a lay-off. In other words, unemployment is less unattractive relatively to being employed. The liquidity effect, however, has to do with the fact that UI raises asset level and may help the worker to smooth consumption between employment and unemployment.

Once again, the decomposition above applies for a *one period* increase in benefit level. Below is the decomposition for a B periods increase in the level of benefits:

$$\frac{\partial e_t}{\partial b}|_B = \frac{\partial e_t}{\partial a}|_B - \frac{\partial e_t}{\partial w}|_B \tag{3.25}$$

where $\frac{\partial e_t}{\partial a}|B = \sum_{i=t}^{t+B-1} \frac{\partial e_t}{\partial a_i}$ and $\frac{\partial e_t}{\partial w}|B = \sum_{i=t}^{t+B-1} \frac{\partial e_t}{\partial w_i}$.

### 3.7.3 Liquidity and Moral Hazard in the T Periods Model

Let $x \in \{a, b, w\}$, $s \in \{0, 1, ..., T-1\}$, $\frac{\partial e_0}{\partial x}|s = \sum_{t=0}^{T-1} \frac{\partial e_0}{\partial x_t}$ and $\frac{\partial s_0}{\partial x}|s = \sum_{t=0}^{T-1} \frac{\partial s_0}{\partial x_t}$.
Exploiting the FOCs with envelope conditions, we have:

$$\frac{\partial e_0}{\partial x}|s = \frac{1}{c''(e_0)} \left\{ \frac{\partial V_0}{\partial x}|s - \frac{\partial U_0}{\partial x}|s \right\} \tag{3.26}$$

$$\frac{\partial s_0}{\partial x}|s = \frac{1}{\psi''(s_0)} \left\{ \frac{\partial E_0}{\partial x}|s - \frac{\partial U_0}{\partial x}|s \right\} \tag{3.27}$$

Notice that:

$$\begin{aligned}
\frac{\partial E_0}{\partial a}|_B &= \frac{\partial E_0}{\partial w}|_B \\
\frac{\partial U_0}{\partial a}|_B &= \frac{\partial U_0}{\partial w}|_B + \frac{\partial U_0}{\partial b}|_B \\
\frac{\partial V_0}{\partial a}|_B &= \frac{\partial V_0}{\partial w}|_B + \frac{\partial V_0}{\partial b}|_B
\end{aligned}$$

Combining these conditions, it follows that:

$$\frac{\partial e_0}{\partial b}|_B = \frac{\partial e_0}{\partial a}|_B - \frac{\partial e_0}{\partial w}|_B \tag{3.28}$$

$$\frac{\partial s_0}{\partial b}|_B = \frac{\partial s_0}{\partial a}|_B - \frac{\partial s_0}{\partial w}|_B \tag{3.29}$$

### 3.7.4 The Welfare Formula in the T Periods Model

$$\max_{b,\tau} J_0^V(b, \tau) = (1 - e_0)U_0(b, \tau) + e_0 V_0(b, \tau) - c(e_0) \tag{3.30}$$

$$s.t. D_B b = D_E \tau \tag{3.31}$$

Deriving with respect to the level of benefits:

$$\frac{dJ_0}{db} = (1 - e_0)\cancelto{0}{\frac{\partial U_0}{\partial b}} + e_0 \frac{\partial V_0}{\partial b} - \frac{d\tau}{db} \left[ (1 - e_0)\frac{\partial U_0}{\partial w} + e_0 \frac{\partial V_0}{\partial w} \right] \tag{3.32}$$

Notice that $\frac{\partial U_0}{\partial b} = 0$ because workers laid-off in the first period are not eligible for UI. Let $E_{0,T-1}v'(c_t^V)$ denote the unconditional average marginal utility while employed and $D_E$ the expected duration of (first) employment. Then:

$$E_{0,T-1}v'(c_t^V) = \frac{1}{D_E}\left[(1-e_0)\frac{\partial U_0}{\partial w} + e_0\frac{\partial V_0}{\partial w}\right] \tag{3.33}$$

Also:

$$e_0\frac{\partial V_0}{\partial b} = \sum_{i=k}^{T-1}\left[\Pi_{j=0}^{i-1}e_j\right](1-e_i)\frac{\partial U_i}{\partial B_i} \tag{3.34}$$

Where $\frac{\partial U_i}{\partial B_i}$ is the effect of raising UI benefits for workers entering unemployment at period $i$. Then, it implies:

$$\frac{dJ_0}{db} = \sum_{i=k}^{T-1}\left[\Pi_{j=0}^{i-1}e_j\right](1-e_i)\frac{\partial U_i}{\partial B_i} - \frac{d\tau}{db}(D_E)E_{0,T-1}v'(c_t^V) \tag{3.35}$$

Higher benefits increase the value of employment at $t=0$ by raising the value of subsequent unemployment after minimum eligibility requirement, at period $k$.

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

$$\frac{dJ_0}{dw} = (1-e_0)\frac{\partial U_0}{\partial w} + e_0\frac{\partial V_0}{\partial w} = (D_E)E_{0,T-1}v'(c_t^V) \tag{3.36}$$

Therefore:

$$\frac{dW}{db} = \frac{\frac{dJ_0}{db}}{\frac{dJ_0}{dw}} = \frac{\sum_{i=k}^{T-1}\left[\Pi_{j=0}^{i-1}e_j\right](1-e_i)\frac{\partial U_i}{\partial B_i}}{(D_E)E_{0,T-1}v'(c_t^V)} - \frac{d\tau}{db} \tag{3.37}$$

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

$$\frac{\partial s_i}{\partial B_i} = \frac{1}{\psi''(s_i)}\left\{\cancelto{0}{\frac{\partial E_i}{\partial B_i}} - \frac{\partial U_i}{\partial B_i}\right\} \tag{3.38}$$

$$\implies \frac{\partial U_i}{\partial B_i} = -\psi''(s_i)\frac{\partial s_i}{\partial B_i} \tag{3.39}$$

Then, it follows that:

$$\frac{dW}{db} = \frac{\sum_{i=k}^{T-1} \left[\Pi_{j=0}^{i-1} e_j\right](1-e_i)\left(-\psi''(s_i)\frac{\partial s_i}{\partial B_i}\right)}{(D_E)E_{0,T-1}v'(c_t^V)} - \frac{d\tau}{db} \tag{3.40}$$

Now since:

$$\frac{\partial s_i}{\partial B_i} = \frac{\partial s_i}{\partial A_i}|B - \frac{\partial s_i}{\partial W_i}|B \tag{3.41}$$

We have:

$$\frac{dW}{db} = \frac{\sum_{i=k}^{T-1} \left[\Pi_{j=0}^{i-1} e_j\right](1-e_i)\left[-\psi''(s_i)\frac{\partial s_i}{\partial W_i}|B\left(\frac{\frac{\partial s_i}{\partial A_i}|B}{\frac{\partial s_i}{\partial W_i}|B} - 1\right)\right]}{(D_E)E_{0,T-1}v'(c_t^V)} - \frac{d\tau}{db} \tag{3.42}$$

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

$$E_{i,i+B-1}v'(c_t^E) = \frac{1}{B - D_B}\left(s_i \frac{\partial E_i}{\partial W_i}|_B + (1-s_i)\frac{\partial U_i}{\partial W_i}|_B\right) \tag{3.43}$$

From (3.26) and (3.27), after some manipulation, it follows that:

$$\begin{aligned}
\frac{\partial s_i}{\partial W_i}|B &= \frac{1}{\psi''(s_i)}\frac{1}{1-s_i}\left\{\frac{\partial E_i}{\partial W_i}|_B - \left(s_i\frac{\partial E_i}{\partial W_i}|_B + (1-s_i)\frac{\partial U_i}{\partial W_i}|_B\right)\right\} && (3.44)\\
&= \frac{1}{\psi''(s_i)}\frac{1}{1-s_i}\left\{Bv'(c_i^E) - (B-D_B)E_{i,i+B-1}v'(c_t^E)\right\} && (3.45)\\
& && (3.46)
\end{aligned}$$

These results in $\frac{dW}{db}$ imply:

$$\frac{dW}{db} = \frac{\sum_{i=k}^{T-1} \left[\Pi_{j=0}^{i-1} e_j\right](1-e_i)\left[\frac{1}{1-s_i}\left\{Bv'(c_i^E) - (B-D_B)E_{i,i+B-1}v'(c_t^E)\right\}(-\rho_i - 1)\right]}{(D_E)E_{0,T-1}v'(c_t^V)} - \frac{d\tau}{db} \tag{3.47}$$

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 $i$.

Notice that from the government budget constraint:

$$\frac{d\tau}{db} = f^{UI}\frac{D_B}{D_E}\left\{1 + \epsilon_{f^{UI},b} + \epsilon_{D_B,b} - \epsilon_{D_E,b}\right\} \tag{3.48}$$

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 budget constrain, it implies that:

$$\frac{dW}{db} = \frac{D_B}{D_E}\left\{\sum_{i=k}^{T-1}\left[\Pi_{j=0}^{i-1}e_j\right](1-e_i)\frac{(\rho_i+1)}{1-s_i} - f^{UI}\left[1 + \epsilon_{f^{UI},b} + \epsilon_{D_B,b} - \epsilon_{D_E,b}\right]\right\} \tag{3.49}$$

The term $\sum_{i=k}^{T-1}\left[\Pi_{j=0}^{i-1}e_j\right](1-e_i)\frac{(\rho_i+1)}{1-s_i}$ is the weighted average of the liquidity-to-moral hazard ratio of a worker becoming unemployed at period $i > k$, divided by the probability that he does not find a job at the first period of the spell. If we assume that both the liquidity-to-moral ratio and $s_i$ at the first period of the spell do not vary with respect to the period in which workers become unemployed, as is implicity in Chetty (2008), it is true that $\rho_i = \rho$ and $s_i = s_0$. Then it follows our final welfare formula:

$$\frac{dW}{db} = f^{UI}\frac{D_B}{D_E}\left\{\frac{1}{1-s_0}(\rho+1) - \left(1 + \epsilon_{f^{UI},b} + \epsilon_{D_B,b} - \epsilon_{D_E,b}\right)\right\} \tag{3.50}$$

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

# Chapter 4

# The Liquidity Gains from Unemployment Insurance in Markets with High Informality: Evidence from Brazil

## 4.1 Introduction

There exists a rather large body of empirical evidence assessing how unemployment benefits affect unemployment outcomes. One established finding is that workers take longer periods to find a new job as UI increases, either in terms of levels or potential duration. However, one key issue for policy is evaluating to what extent such benefits achieve its main goal: avoiding that jobless workers face drops in consumption which are too large. In other words, the key efficiency gain from providing temporary income assistance to the unemployed is allowing workers to better smooth consumption when in between jobs. Nevertheless, it is an empirical challenge to assess how UI benefits affect consumption because data on the latter typically comes from surveys and is very imprecise. Chetty (2008) proposes a solution to this problem and shows that it is possible, and equivalent, to evaluate welfare gains from UI by looking at how unemployment duration reacts to changes in liquidity *vis-a-vis* to changes in unemployment benefits: the so-called *liquidity-to-moral hazard ratio*. He notices that unemployed workers take longer to find a new job when UI increases because of two reasons: (i) higher benefit level makes workers less liquidity constrained, so that they do not need to search "too fast" for a new job: *the liquidity effect*; and (ii) higher benefit level increases moral hazard because once workers find a new job they are no longer entitled to receive benefits: *the moral hazard effect*. The key theoretical finding from his work is that while the second effect is rather detrimental to welfare, the first effect is non-detrimental and is actually an alternative measure to the gains from consumption smoothing. Welfare gains from UI can then be evaluated by estimating the ratio between these two effects, based on Chetty's formula and in line with the generalization of the formula developed in the previous chapter. In this framework, optimal UI policy consists therefore of finding the right balance between providing liquidity to constrained job seekers and avoiding moral hazard effects which are too large.

There is however scarce credible evidence in the literature of how large the liquidity-to-moral hazard ratio really is. In this work, I first take advantage of a policy in Brazil which provides an unconditional "bonus income" to low income workers to estimate the liquidity effect: how unemployed workers react to a liquidity provision which yields no moral hazard. Unlike unemployment benefits, this policy does not create moral hazard because the unemployed do not lose this bonus in case they find a new job. Second, I explore the schedule of UI potential duration in Brazil to assess how potential duration affects the duration of employment. Such an effect is a composition of a liquidity and a moral hazard component. Finally, I use these two estimates to disentangle the moral hazard component from the liquidity effect by using a theoretical result due to Landais (2014). In such a way, it is possible to estimate the liquidity-to-moral hazard ratio, which I find to be close to 0.98 in Brazil, an estimate which is similarly to the one previously found by Landais (2014) using data from the US.

In order to estimate the liquidity effect, I exploit a policy in Brazil (*Abono Salarial*) which yearly grants low-income workers a "bonus" payment equal to one monthly minimum wage. To be eligible for the bonus, beneficiaries average earnings in the previous year must not have exceed two times the minimum wage at that time. The grant then is unconditional to employment status in the payment year. I use this condition to estimate how the unemployed react to receiving this bonus at the beginning of their spell by applying a regression discontinuity design, which will be discussed in details ahead. The intuition of the empirical analysis is comparing workers close to this two minimum wage threshold who are eligible and not eligible to the bonus. I find that granting unemployed workers around this eligibility threshold one monthly minimum wage decreases the probability of finding a new job in the first eight weeks of unemployment by 0.65%. This estimates the liquidity effect.

To assess the moral hazard effect, I first exploit the Brazilian UI schedule of potential duration. It grants displaced workers who have reached 24 months of tenure with an extra month of potential unemployment benefits. I use again a regression discontinuity to assess how workers around this threshold react to this extension and find that it decreases their probability of finding a new job in the first eight weeks of unemployment by 1.9%.[1] This estimate embodies both a liquidity and a moral hazard effect.

From the two empirical analyses briefly described in the two previous paragraphs, I draw estimates of the liquidity effect (from the bonus policy) and of the full effect of UI (from the UI schedule assigning potential duration) which is a sum of liquidity and moral hazard. From these two estimates it is possible to recover the liquidity-to-moral hazard ratio. It is however not immediate, because first it is necessary to isolate the moral hazard effect from the second estimate described in the previous paragraph above.

[1] This RDD strategy is the same used by Gerard and Gonzaga (2013) who are interested in a related but different outcome related to unemployment outcomes.

It is also important to notice that the bonus policy provides an increase in liquidity at the beginning of the unemployment spell, while an increase in potential duration works as an increase in benefit level at the end of the UI covered unemployment duration. Thus, in order to combine these results and isolate the moral hazard component, I take advantage of a theoretical result from Landais (2014) which links UI, liquidity and moral hazard effects at different points in time. Then, it is possible to estimate the moral hazard component and, finally, the liquidity-to-moral hazard ratio.

The main contribution of this paper is providing a credible estimate of how unemployed workers react to an increase in the provision of liquidity and estimating the liquidity-to-moral hazard ratio. Based on the framework presented in chapter 2 and 3, learning about this ratio is fundamental to evaluate the welfare effects of unemployment insurance policies; and to guide policy makers in setting a reasonable and close to optimal generosity level of unemployment benefits. To the best of my knowledge only Chetty (2008) and Landais (2014) estimate this ratio using data from the US. This is then the first paper to estimate the size of this ratio in a developing country, in which informal labor markets are prevalent. Furthermore, since the estimates here presented do not rely on policy changes, which are not frequent, they can deliver timely estimates from year to year. Therefore, these could be used by policy makers on a constant basis to evaluate the policy and optimally adjust UI over time.

This chapter is organized as follows. Section 2 presents the theoretical background with the goal of clearly stating the parameters of interest to be estimated in the empirical section. In section 3, the empirical analysis of the bonus policy is presented. There the liquidity effect is estimated. In section 4, variations in potential duration of UI benefits due to tenure are exploit in order to estimate the overall effect of UI on re-employment probabilities. This effect is a mix of a liquidity and a moral hazard effect. Section 5 takes the estimates from the two empirical sections and, based on the theory described in the next section, estimates the liquidity-to-moral hazard ratio. Section 6 concludes and discuss the main results.

## 4.2 Theoretical Background

The main goal of this work is to estimate the liquidity-to-moral hazard ratio as defined by Chetty (2008). Namely, it is defined by the following ratio:

$$\rho = \frac{-\frac{\partial s_0}{\partial a}|_B}{\frac{\partial s_0}{\partial w}|_B} \tag{4.1}$$

where $s_0$ defines the probability of finding a job in the first period of the unemployment spell, $B$ is the potential duration of benefits, $a$ is an annuity payment, $w$ is the wage rate and $\frac{\partial s_0}{\partial a}|_B = \sum_{t=0}^{B-1} \frac{\partial s_0}{\partial a_t}$

and $\frac{\partial s_0}{\partial w}|_B = \sum_{t=0}^{B-1} \frac{\partial s_0}{\partial w_t}$. The numerator is the liquidity effect: the marginal effect of providing liquidity to the unemployed during $B$ periods on the probability of finding a job at the first period of spell. The denominator defines the moral hazard: the marginal effect of increasing the pay-off of leaving unemployment (without providing liquidity) on the probability of finding a job at the first period of spell. Increasing the wage rate does not embed any liquidity effect for the unemployed today because it represents a future income flow.

By exploiting the bonus policy, it is possible to estimate $\frac{\partial s_0}{\partial A_0}$ where $A_0$ is a one time cash grant today: how the probability of finding a job at the first period of spell changes due to a windfall bonus at the start of the spell. From this result it is possible to recover the first term of the formula by applying the approximation that $-\frac{\partial s_0}{\partial a}|_B \approx -B\frac{\partial s_0}{\partial A_0}$ as discussed in Chetty (2008). This fact should not be a reason for any concern since $\frac{\partial s_0}{\partial A_t}$ should not vary over the first $B$ months of the spell if the budget constrain does not bind. Moreover, even if it does bind, it should not vary much since potential duration in Brazil varies only from 3 to 5 months, which is a fairly short period.

From the RDD analysis on potential duration, it is possible to recover $\frac{\partial s_0}{\partial b_t}$: the effect of raising potential duration by one period, which is equivalent to raising benefit level from zero to $b$ at the point where duration increases. This effect is a composition of moral hazard and liquidity reactions by the workers. In order to find which share of it is due to moral hazard, I use the following results from Landais (2014) to isolate the moral hazard effect from combining this paper's estimates of $\frac{\partial s_0}{\partial A_0}$ and $\frac{\partial s_0}{\partial b_t}$:

$$\frac{\partial s_0}{\partial b_t} = \frac{\partial s_0}{\partial A_0} - S_1(t)\frac{\partial s_0}{\partial w_0} \tag{4.2}$$

and

$$\frac{\partial s_0}{\partial w}|_B = \frac{\partial s_0}{\partial w_0}\sum_{t=0}^{B-1} S_1(t) \tag{4.3}$$

where $S_1(t)$ defines the survival rate in unemployment until period $t$ conditional on being unemployed at period 1. These formulas are an intertemporal extension of Chetty (2008)'s key decomposition:

$$\frac{\partial s_t}{\partial b_t} = \frac{\partial s_t}{\partial A_t} - \frac{\partial s_t}{\partial w_t} \tag{4.4}$$

This original (intratemporal) form describes how the marginal effect of an increase in UI benefits is a composition of a liquidity and a moral hazard effect. First, higher benefit level relaxes the budget constrain of unemployed workers, increasing liquidity, which causes lower search effort at the same period: the liquidity effect ($\frac{\partial s_t}{\partial A_t}$). Second, higher benefit level also creates a moral hazard problem because unemployment benefits are the opportunity cost of finding a job: once workers find a new job, unemployment insurance is suspended. This mechanism also causes them to search less intensively: the moral hazard effect ($\frac{\partial s_t}{\partial w_t}$).

Equation (4.2) simply describes how this decomposition works in a intertemporal fashion which is discussed at length in Landais (2014). Equation (4.3) specifically describes how moral hazard is connected over time. It states that the total moral hazard effect of a full decrease in the pay-off profile of finding a job for $B$ periods (similarly to what a raise in UI benefits for $B$ periods does) is equivalent to the moral hazard effect of decreasing the pay-off of finding a job at a single period adjusted by survival probabilities.

Once $\frac{\partial s_0}{\partial b_t}$, $\frac{\partial s_0}{\partial A_0}$ are estimated as described above and $S_1(t)$ can be easily recovered from the data, it is possible to derive $\frac{\partial s_0}{\partial w_0}$ using (4.2). Then, (4.3) allows me to derive the moral hazard effect as in the denominator of (4.1). Thus, using (4.2), (4.3) and the approximation $-\frac{\partial s_0}{\partial a}|_B \approx -B\frac{\partial s_0}{\partial A_0}$, the liquidity-to-moral hazard ratio is estimated as:

$$\rho = \frac{-\frac{\partial s_0}{\partial a}|_B}{\frac{\partial s_0}{\partial w}|_B} = \frac{-\frac{\partial s_0}{\partial A_0}}{\frac{\partial s_0}{\partial w_0}\bar{S}_1^B} \tag{4.5}$$

where $\bar{S}_1^B$ is the average survival rate between time 1 and $t$ conditionally on being unemployed at time 1.

In summary, the intuition behind this strategy to estimate the liquidity-to-moral hazard ratio is simple. The effect of an increase in unemployment benefits on labor supply is a composition of liquidity and moral hazard effects. I use the "bonus policy" to estimate the liquidity effect and the UI schedule to estimate the composed overall effect of increasing benefits. The strategy to disentangle the moral hazard effect is taking the second result of the UI extension (liquidity + moral hazard) and discounting it from the liquidity effect estimated from the "bonus" policy. The formula from above is also composed of a survival rate term in order to account for the fact that the "bonus" policy takes place at the beginning of the spell while potential duration is equivalent to an increase in benefit level at the end of the spell.

## 4.3 Empirical Analysis I: Liquidity Effect

In this section, I present the empirical analysis used to estimate the effects of liquidity provision on the probability that unemployed workers find a job at the beginning of their spells.

### 4.3.1 The "Bonus Policy"

The "Bonus Policy" (*Abono Salarial*) was introduced in Brazil by the Federal Law 7.998 of 1990 and is in place since then. The policy is financed by a fund supported by compulsory contributions from firms in the private sector. Contributions depend solely on gross revenues and are not related in any way to how firms manage their workforce. Every year, a sum equal to one monthly minimum wage is granted to all workers in the country satisfying the following conditions: (i) have been employed at any firm in the private sector for at least 30 days in the year before the payment year [2]; (ii) average monthly earnings in the year before payment has not exceed two monthly minimum wages in that year; and (iii) have started working in the formal labor market at least five years before the payment year. The cash grant does not depend on the workers' labor market outcomes in the payment year and, hence, is paid both to workers who happen to be employed and unemployed at the moment of the payment.

The exact timing of the payment during a year ranges from July to November and can happen in three different ways: (i) workers who hold an account at the state controlled bank (*Caixa Economica Federal*) responsible for the payments are paid in July; (ii) those employed at firms which have an agreement with the public controlled bank responsible for the payments are paid from July to August, and (iii) those not entitled to the two previous payment channels can withdraw the bonus starting from August to November according to their month of birth, and are able to do so until June of the next year.

### 4.3.2 Data and Identification Strategy

The data used for both empirical analyses is the RAIS (*Relação Anual de Informações Sociais*) for the years from 2005 to 2012. It is a linked employer-employee administrative dataset covering the whole Brazilian formal labor market. It contains more than 50 million employment contracts in each year. It allows one to track workers' formal employment history over the years and contains information such as earnings, tenure, years of schooling, race, gender, birth date and weekly workload, among others.

To estimate the effect of the grant on re-employment probabilities cleaning from confounding factor, I apply a regression discontinuity design to exploit the condition that only workers earning up to two minimum wages in the previous year are eligible to the grant (eligibility condition (ii)). The idea of this procedure is comparing workers just above and just below this earning threshold. These two groups should be very similar to each other but only the group earning just less than two minimum wages is entitled to the grant. Therefore, any differences in behavior can be plausibly attributed to the cash grant.

Because the data contains no explicit variable informing on workers' eligibility for the cash grant over the years, it is necessary to manually identify these workers. The sample for the analysis is built by first

[2] Workers hired by individuals are not entitled to the bonus.

restricting the dataset to workers who have worked at least 30 days in the year previous to the bonus payment (eligibility condition (i)), for each year from 2005 to 2011. Notice that I can only consider grants paid from 2006 to 2012, since eligibility criteria (i) and (ii) require data from the previous year. Second, I consider only workers who had only one employment contract during each eligibility year (the year before payment). This is to avoid the need to calculate average earnings in between jobs within a year. Such calculation is not exactly trivial because tenure in each job is different and minimum wage varied in the middle of some years while data only informs average earning (also in terms of minimum wages) in the year for each labor contract. Henceforth, by using this restriction, it is possible to rely on the data on average yearly earnings, already calculated in terms of minimum wages, without the need to average it over different employment contracts. This ensures precision on the running variable for the RDD, which is based exactly the average earnings in minimum wages in the eligibility year.

It is also reassuring to know that the state-controlled bank responsible for identifying and paying eligible workers rely exactly on the same dataset used in this work. This fact ensures that even in the extreme case where there is imprecision in this dataset (which is based on compulsory information reported by firms) affecting the assignment of the bonus, this will be the actual data used by the bank to effectively concede the cash grant. In other words, there is no asymmetry between the data used in this work and the data used by the state controlled bank to assign the cash grant in each year. In order to avoid considering workers too far away from the threshold, I drop workers with average monthly earnings in the eligibility year higher than 3 minimum wages or lower than one minimum wage.[3]

At this point, I further restrict the dataset to workers who are dismissed against their will in the payment year of the bonus.[4] I do not consider other forms of job termination to avoid endogeneity problems due to voluntary job loss: for example, workers may quit because they are entitled to the bonus. Such a mechanism would cause self-selection and could bias the results. Furthermore, I restrict the sample to workers displaced only in the month of June, which is just the month before the payment starts. In such a way, I avoid considering workers who become unemployed after receiving the bonus. This helps preventing endogeneity of lay-off probabilities to the actual payment of the grant.

Given these restrictions, the sample contains only workers who satisfy eligibility condition (i) and may or not satisfy the eligibility condition (ii) according to their average earnings in the previous year. As regards eligibility condition (iii), there are some workers in the sample who do not satisfy this condition. Namely, these are those who have first entered the (formal) labor market within four years before the payment. Since the data does not contain explicit information on the date which workers first enter the labor market, the only way to verify this condition would be restricting the analysis only to the

[3]Workers in the dataset can have average monthly earnings below the monthly minimum because not all of them work full-time.

[4]Workers dismissed against their and without a just cause, which is by far the most typical form of termination in Brazil.

payment years of 2010, 2011 and 2012. For these years, it is possible to check whether workers were already registered in the labor market five years before given the span of time available in the data. Such procedure however seriously decreases the number of observations in the dataset because it only considers three years of payment restricted to workers who were not unemployed or out of the labor force in the few years which can be used to verify this condition: 2005, 2006 and 2007[5]. This would imply in a significant loss of observations and statistical power in the analysis. Hence, since the share of workers in a given year who have first entered the labor market within the last four years is likely to be small, I decide to use the whole sample for all years of bonus payment (2006-12) in which some workers fail to satisfy eligibility condition (iii). In the RDD analysis, those should behave in the same way around the threshold and, if their share is large enough, it will bias estimates toward zero. Therefore, estimates on the whole sample used here should be a lower bound. In order to get a better sense of the actual effect, results on the restricted sample for the years of 2010, 2011 and 2012 are also reported and discussed in the next subsection: even though they are less precise, they are still valuable to better estimate the size of the actual liquidity effect.

Another point to highlight is that even though it is not possible to observe whether eligible workers actually withdraw the grant, data from the Ministry of Labor shows that take-up rate is around 95%.[6] Hence, every year the vast majority of entitled workers do receive the grant. A further point to remark is that it is only known that all beneficiaries have the grant available from July to November, without the precise date. It means that the cash grant is made available over the first five months of the spell. It is also true that a fair share of workers are likely to receive it on the first two months of unemployment (July and August) because the payment methods (i) and (ii) should cover a non-negligible share of beneficiaries.[7] Therefore, the effect I estimate on re-employment probabilities is a weighted average of liquidity provision over the first five months of the spell, with higher weights for the first two months. However, this should not be a reason of any concern for two reasons. First, $\frac{\partial s_0}{\partial A_t}$ should be close to constant over the beginning of the spell, for the reasons discussed in section 4.2. Second, even if it happens not to be close to constant, results will be an estimate which is closer to $\frac{\partial s_0}{\partial a}|_B = \sum_{t=0}^{B-1} \frac{\partial s_0}{\partial a_t}$, which is the actual statistic of interest arising from the background model.

In summary, the final sample contains workers who become unemployed against their will from 2006 to 2012 in the month of June, and have worked at least for 30 days in the previous year in a single employment spell. Those in the sample who had earned on average less than two minimum wages in the

[5]More precisely, I can only recall eligible workers for the cash grant in 2010 who were working in 2005. Therefore, all eligible workers to the cash grant in 2010, satisfying eligibility condition (iii) (who have entered the labor market before 2005), and who have not worked in the year of 2005 cannot be identified.

[6]This data is available on http://portal.mte.gov.br/imprensa/mais-de-1-milhao-nao-sacaram-abono-salarial/palavrachave/abono-salarial-prazo-final.htm

[7]It is hard to know exactly the number of payments made according to each category. News found on the internet suggest that, in 2007, 3.8 out of 11.1 million bonus were paid by the first two methods http://www.endividado.com.br/noticia_ler-17173,correntista-caixa-recebe-abono-do-pis-partir-hoje.html

previous year are entitled to the cash grant, which is made available for them from July to November, in each year.[8] Summary statistics are presented in table 4.2. On this sample, I run the RDD analysis using a local polynomial spline specification as in:

$$Y_i = \sum_{p=0}^{P} [\gamma_p (w-2)^p + \beta_p (w-2)^p . D] + Z\alpha + \epsilon_i \text{ where } |w-2| \leq h \tag{4.6}$$

where $Y_i$ is an outcome of interest, $w$ is the worker's average earnings in the (previous) eligibility year in minimum wages, $D$ is a dummy taking value 1 for $w-2>0$, $Z$ is a set of control variables including tenure at last and previous job, average earnings in the year, dummy variables for reaching minimum tenure requirement for UI, male, and for each category of schooling, race, industry and weekly hours of last and previous job, dismissal type at previous job and decile of age at dismissal date. The parameter of interest is $\beta_0$. It estimates the discontinuity when workers around the threshold are no longer eligible for the bonus. Thus, $-\beta_0$ identifies the local average treatment effect (LATE) of providing the cash grant to workers around the threshold. In the main specification, I use a local linear polynomial ($p=1$) which is the most common in the literature. It is also in line with Gelman and Imbens (2014) which suggests that higher order polynomials should be avoided in RD designs. As regards the bandwidth choice $h$, I use the selector proposed by Imbens and Kalyanaraman (2011).[9]

As standard procedure in RDD applications, I provide two main tests to assess the validity of the design. First, I employ the McCrary (2008) test for discontinuity in the density of the assignment variable, which aims to identify whether there is manipulation in the treatment. Second, I test whether pre-determined covariates evolve continuously around the threshold using the specification in (4.6). Moreover, to better evaluate whether any potential imbalances in covariates around the threshold may affect the outcomes of interest, I create a linear prediction for the main outcomes here analyzed by regressing each of them on a rich set of covariates: quintile of average earnings in the previous year, decile of age at hiring and dismissal, employment duration in the payment year, and dummies for each month of tenure in the last and previous job, industry of last and previous employer, separation cause at previous job, race, gender, weekly hours of work, year, calendar month of dismissal and federal state (27). Then, using (4.6), I test whether these predictions evolve continuously around the threshold.

I notice that it is possible to identify some small but visible bunching of earnings at two minimum wages. This may happen for two reasons. First, it is possible to observe some heaping of observations at multiples of minimum wages in the data, probably because it works as a reference point for wage

[8]except for those who do not satisfy eligibility condition (iii), which should represent a small share of all these workers. This issue is more extensively discussed in the results' section.

[9]I do not attempt to implement the selection procedure proposed by Calonico et al. (2014) because it is extremely computationally time consuming in a large dataset like the one used here.

bargaining. Second, it might happen because firms and workers agree to manipulate earnings in order to allow workers to receive the bonus. The second reason is one of more concern since it could compromise randomization around the threshold. To deal with that I drop workers whose average earnings lie exactly at this point. As one key advantage of RD designs is that identification assumptions are testable, there is no need to take by granted that this strategy works. In the next subsection, I show that this procedure is enough to ensure the balance of a rich set of covariates around the threshold and that the assignment variable displays no discontinuities around the cut-off. The intuition of this procedure is also similar to the one proposed by Barreca et al. (2015) which studies heaping issues in RD designs. They show that even when heaping is endogenous, it is still possible to estimate the causal effect of interest by dropping the points of heaping. Their key consideration is that in this case the estimated effect is the local average treatment effect (LATE) on the non-heaped types. Since in our case there is bunching at only one point and it is fairly small, the LATE recovered represents quite well the population of workers around that threshold.

### 4.3.3 Results

*Density Test* Before turning to the results, I present evidence that the density of the running variable evolves continuously around the two minimum wages threshold. Figure 4.1 displays the density of average earnings with a third degree polynomial fit, selected by minimizing the Akaike Criterion. The graph clearly suggests that there is no discontinuity around the cut-off point. This impression is supported by the McCrary density test reported in the same figure, which cannot reject the null hypothesis of no discontinuity.

*Graphical Evidence* Figure 4.2 displays how the probability of re-employment within 8 weeks ($s_0$) and overall unemployment duration evolve around the cut-off.[10] The top panel shows a clear discontinuity on $s_0$, suggesting that workers receiving the bonus (left side of the cut-off) are less likely to exit unemployment at the beginning of the spell. At the bottom panel, the cash grant seems not to affect the overall duration of unemployment. It is worth noticing that this graph is also fairly more noisy, what might be a result of the large average duration of the unemployment spell (around 51 weeks).

In figures 4.3-4.4 it is possible to observe how a variety of covariates evolve around the threshold. All of them seem to evolve clearly continuously around the threshold. Perhaps the only exception is tenure at previous job which displays a negative discontinuity to the right side of cut-off. In the regression results, I discuss whether this discontinuity is either statistically or economically significant and if it might be driving results. Overall, I see the graphical evidence on this rich set of covariates as clear evidence in favor of the validity of the design.

[10] I choose to define $s_0$ as the probability of re-employment in the first eight weeks of spell. Shorter definitions yield similar point estimates but imply in a significant loss of precision.

*Regression Results* Table 4.3 shows regression results for both outcome variables and pre-determined covariates. I report both results from a specification without controls and one with controls. Comparing them provides insight on the validity of the design as one expect results not to change much with the inclusion of pre-determined covariates if the design is valid. Both specifications indicate that the probability of exiting unemployment at the beginning of the spell is 0.5% lower on the left side of the cut-off. It thus suggests that providing workers with a cash grant equal to one monthly minimum wage decreases the probability of re-employment within eight weeks by half percentage point. This result is statistically significant at the 1% level in both specifications. Instead, results on the full duration of unemployment yield a positive value significant at the 10% level which is not robust to the inclusion of controls. Thus, this evidence, which also goes the theory, is not much conclusive and should be interpreted with extreme caution.

Still in the same table, results on the pre-determined covariates point that the seemingly discontinuity on tenure at previous job turns out not to be statistically significant. There are however four statistically significant discontinuities in tenure at last job, years of schooling, gender and age at dismissal. The last three are arguably not economically significant as the size of the discontinuity represents an imbalance of only 0.063 years of schooling, 0.4% in the probability of being a female and 0.246 age years of difference. The discontinuity estimate on tenure at last job instead is moderate (around two weeks) and could compromise the design. Six months of tenure constitutes the minimum eligibility requirement (MER) for unemployment benefits and UI potential duration increases from 3 to 5 months according to tenure. Hence, the main suspicion is that the interaction of tenure imbalances with UI eligibility and potential duration could directly affect unemployment outcomes.

A first evidence which suggests that this is not the case is the fact that there is no significant discontinuity on the probability of reaching MER. It suggests therefore that the same share of workers on both sides of the threshold are eligible for unemployment insurance. Second, if anything, workers on the right side of the threshold (not granted with the bonus) are dismissed with higher tenure and thus might have longer UI benefits and slightly larger severance payments. Because longer benefits and larger severance payments could only cause a decrease in the probability of exiting unemployment at the beginning of the spell, such imbalances could only bias the result on $s_0$ downwards, while the actual effect found is positive. Therefore, even if it is actually the case that there is a bias, in the worst case scenario estimates represent a lower bound of the effect.

To gain a sense of how these imbalances could actually be driving the results in any economically meaningful way, I create a linear prediction by regressing both outcomes on an extremely rich set of pre-determined covariates, as discussed in the previous subsection. Then, I test whether these predicted values are discontinuous around the cut-off to gain insight on how these imbalances could bias the results. In the same table, it is reassuring to see the linear prediction of $s_0$ presents no statistically significant discontinuity and, if anything, the sign of the bias goes to an opposite direction while its size is clearly of a smaller order of magnitude than the actual discontinuity found in $s_0$. I interpret this evidence as a strong sign that results on this variable are not biased in any economically significant order of magnitude, and, if anything at all, estimates represent a lower bound very close to the actual value.

On the other hand, the same exercise on the predicted overall duration of unemployment strongly suggests a sizable discontinuity on this outcome and that such results should be interpreted with much caution. As one could only expect individuals with less liquidity to take less time to exit unemployment, this imbalances in predicted unemployment duration may also explain why the estimated discontinuities on both specifications are positive, contrary to theory. It also worth noticing that in the specification with controls, the discontinuity in unemployment duration is no longer statistically significant, suggesting that adding controls to the regression effectively helps mitigating biases driven by imbalances in (observable) covariates.

*Heterogeneity* At this point I show that the need for liquidity is heterogeneous for groups of workers displaced at different tenure ranges. I split the sample in three groups and repeat the analysis performed in the full sample. The first and most liquidity constrained group (0-5.8 tenure months - *low tenure group*) contains dismissed workers who do not the reach MER and therefore are not entitled to UI benefits.[11] At the second group are workers dismissed with tenure ranging from 6 to 60 months (*mid tenure group*). All these workers reach MER and are also entitled to larger severance payments which vary from 0.8 to 7 times their average monthly earnings. At the third and less liquidity constrained group are workers dismissed with more than 60 months of tenure (*high tenure group*). These workers are all entitled to unemployment benefits with the longest potential duration (5 months) and receive severance payments starting from 7 times their average monthly earnings.

Figure 4.5-4.7 show the graphical evidence on outcomes of these three groups, while covariates are displayed in the appendix figures 4.11-4.16. The evidence in figure 4.5 for the low tenure group is striking: workers granted to the bonus are clearly less likely to exit unemployment right away and seem to remain on average two weeks longer unemployed. For the mid tenure group discontinuities are less clear but it still seems that the cash grant provision makes workers slightly less likely to find a new job at the beginning of the spell, suggesting that the liquidity provision still plays a role for these workers. For the high tenure group, there seems to be no discontinuity in $s_0$ and a negative jump on the duration of unemployment, even though the graph displays fairly more noise.

[11] Since tenure months in the data has a 30 days reference month with one-digit precision, I exclude workers with 5.9 months of tenure because some of them actually reach MER, which is counted according to calendar months.

Table 4.4 display regression results on the three groups without and with controls, respectively. Results on $s_0$ indicate that low tenure group is by far the most sensitive to the cash grant, as one could expect given that these workers are not entitled to unemployment benefits and have very low severance payments. Providing one monthly minimum wage for these workers decreases their probability of finding a new job within eight weeks by 1.3% and increases unemployment duration by roughly two weeks. Instead, the same effect on the mid tenure group, is of only -0.34% on $s_0$ and not statistically significant on unemployment duration after controls are added. On the high tenure group, neither of the effects are statistically significant, even though one should consider that the estimations are noisier. Balance on covariates are overall good and results on the linear predictions indicate that, if at all, any bias on $s_0$ goes the opposite directions of the results and yield a smaller order of magnitude. As regards unemployment duration, predicted discontinuities are larger, especially for the mid tenure group, and the direction of the bias is always on an opposite direction to the one suggested by the theory.

At this point, I address the limitation that some workers in the sample used so far are not eligible to the bonus due to eligibility condition (iii): those who have first entered the labor market within less than 5 years are not entitled, as discussed before. Hence, estimations discussed above represent a lower bound of the effect. To deal with that and better assess the actual absolute value of the effect, I further restrict the sample. I now consider only those workers who surely satisfy the eligibility condition (iii), i.e., who can be recalled working in the data at least five years before the grant payment. This procedure limits the analysis to only three years of bonus (2010, 2011 and 2012) for workers who are identified to have worked through the years of 2005, 2006 and 2007. Since the number of observations strongly decreases and much statistical power is lost, I redefine $s_0$ as the probability of finding a new job within twelve weeks instead of eight to improve precision.

Table 4.5 display the results for the linear specification without and with controls. Estimates on predicted $s_0$ in this specific sample point for a statistically significant and not small negative bias this outcome. It is probably the reason why estimates on $s_0$ without controls point for a non statistically significant effect which is smaller in magnitude than the one previous found on the full sample (0.43% versus 0.5%). The estimated effect on $s_0$ increases to 0.064% once controls are added and are border line statistically significant at the 10% level. They are about 20% larger than the same estimates on the full sample.

Table 4.6 shows results on tenure groups without and with controls. As expected, the estimated effects on $s_0$ increases in the specification with controls, again by about 20%, but results lose some statistical significance. Nevertheless, they are still border line significant at the 10% level for the low and mid tenure groups: t-statistics are 1.52 and 1.34 respectively. In appendix table 4.9, I provide the same analysis with a second degree polynomial which displays stronger results. Effects on $s_0$ are found to be larger and t-statistics increase to 2.12 and 1.57 for the low and mid tenure group respectively. Overall, point estimates of the linear and quadratic specifications suggest that the cash grant decreases the probability of exiting unemployment at the beginning of the spell by between 1.6% and 2.4% for the low tenure group, by between 0.67% and 0.87% for the mid tenure group and are statistically insignificant for the high tenure group. I interpret these as the best guesses of the true liquidity effect. Hence, the analysis on the liquidity-to-moral hazard ratio is based on these point estimates.

I draw four main conclusion from these results. First, unemployed workers are indeed liquidity constrained since their probability of exiting unemployment at the beginning of their spell reacts to the cash grant provision, which yields no moral hazard. Second, workers dismissed with low tenure and not entitled to UI react very strongly to the cash grant, and thus are very liquidity constrained. Third, the mid tenure group, which is eligible for UI and larger severance payment, still displays a mild reaction to the cash provision, indicating that they are still liquidity constrained to a fair extent. Fourth, since the low tenure group (not entitle to UI) is found to be way more liquidity constrained than the mid tenure group (entitled to UI), it suggests that UI provision is succeeding on its goal of providing liquidity for those in need.

## 4.4 Empirical Analysis II: Potential Duration Effect

In this section, I present the empirical analysis exploring the UI potential duration schedule. The goal is assessing how the probability of exiting unemployment at the beginning of the spell reacts to UI potential duration extensions. This effect is a composition of liquidity and moral hazard and, as described before, is needed to recall the liquidity-to-moral hazard ratio.

### 4.4.1 UI and Potential Duration Schedule in Brazil

Unemployment Insurance in Brazil is administered at federal level and was introduced by the Federal Law 7.998 in 1990. Important changes were implemented in 1994 by the Federal Law 8.900, which defines all the relevant features of the system for the period analyzed in this paper (2005-2012).[12] As is the case for the Bonus Policy, the system is finance by a compulsory contribution which is a percentage of firms' gross revenues and by no means is related to firms' behavior as regards labor force management. Therefore, there is no experience rating in place. To be eligible for unemployment benefits workers must satisfy the following conditions: (i) have worked continuously in the last 6 months (MER); (ii) have been dismissed against their will without a just cause, which is the most typical form of dismissal in Brazil ; there should be at least 16 months differences between the lay-off date and the date of the last previous lay-off which the worker used to claim UI benefits in the past, if it is not his first claim.

[12] At the beginning of 2015 new significant changes which are still being implemented were introduced.

The replacement ratio is 100% for workers earning close to minimum wages and decreases up to 68% for workers who are just at the benefit cap, whose earnings are equivalent to 2.75 minimum wages. From this point, replacement ratios steadily decrease for higher earnings since a benefit cap is in place. Maximum duration varies from three to five months according to tenure as in the following table:

Table 4.1: Potential Duration Assignment Rule

| Months worked in the last 36 months | Months of Benefit |
|---|---|
| from 6 to 11 | 3 |
| from 12 to 23 | 4 |
| More or equal 24 | 5 |

### 4.4.2 Data and Identification Strategy

In order to assess the effect of increasing potential duration on $s_0$, I apply again a regression discontinuity design to exploit the fact that workers laid-off with more than 24 months of tenure are eligible to five months of benefits instead of four. It is not possible to exploit neither the 6 nor the 12 months threshold, as similarly noticed by Gerard and Gonzaga (2013) who follow the same RD strategy to estimate UI effects on a different set of unemployment outcomes. When workers become eligible for UI at 6 months tenure, there is a large spike in lay-off hazard rates which causes selection around this cut-off. Around the 12 months tenure, when potential duration increases from 3 to 4 months, there is a sharp decrease in lay-offs because legislation imposes higher administrative costs for firing workers with more than one year of tenure.

Gerard and Gonzaga (2013), who use administrative data on the actual payment records of the UI system in Brazil, report that the potential duration rule presented above is not perfectly enforced. Even though the law only grants five months of potential duration to displaced workers with tenure higher than 24 months, in practice, a share of beneficiaries displaced with tenure between 22 and 24 months are granted with 5 months of potential duration. As it would cause the RD analysis to fail because there is no actual discontinuity in the treatment around the 24 months, I apply here the same strategy suggested by Gerard and Gonzaga (2013). In the RD analysis, I set to the right of the cut-off workers who were displaced with 24 months and more, and to the left workers displaced with 22 months and less. In such a way, on the right side of the threshold there are only workers eligible to five months of potential duration and on the left side are only workers eligible to four months of potential duration. For this strategy to succeed, workers around the 22 and 24 months thresholds should work as good counterfactuals to each other. As in any RDD, such requirement can be tested by checking for the existence of discontinuities in pre-determined covariates. As a further robustness check, I also provide results for two placebo thresholds around which there is no change in UI potential duration. Therefore, I implement the same procedure described above to compare workers displaced with 16 and 18 tenure months; and workers displaced with 30 and 32 tenure months. These further results provide insight on whether workers displaced with a difference of two tenure months can work as a good counterfactual to each other.

In order to assess the effect of benefit extension on a similar group of workers to those of the previous section, I build this second sample analysis departing from the sample used in the previous empirical analysis. Thus I depart from a sample of workers who are displaced in the calendar month of June and who had average earnings in the previous year from one to three minimum wages in a single employment spell. Then I reduce the sample to workers which are closer to the $22^{-}/24^{+}$ months threshold by keeping in data only those displaced with tenure higher than 14 months and lower than 32 months. Since the effects found in the previous section are local effects for workers earning on average 2 minimum wages, I restrict again the sample to workers earning on average strictly more than 2 and less than 2.25 minimum wages in the previous year. By doing so, the RDD analysis recovers the effect of benefit extension on a group of workers which has very similar earnings and are not entitled to the bonus policy.

### 4.4.3 Results

Figure 4.9-4.10 show how covariates evolve around the $22^{-}/24^{+}$ tenure months threshold. It is comforting to see that covariates overall evolve clearly continuously across the threshold. It strongly suggests that workers displaced with just less than 22 tenure months are good counterfactuals for workers displaced with just more than 24 tenure months. The only variable which seems to be an exception is average earnings in the year of displacement which seems to be higher on the right side. It is however important to notice that this sample contains only workers who lie in an extremely narrow window of earnings: between 2 and 2.25 minimum wages in the previous years. Therefore any statistically significant imbalance in earnings has a very low potential to be large enough to cause any economically significant difference on outcomes.

This intuition is confirmed by the regression results displayed in table 4.7. The only statistically significant imbalances are on earnings and age at dismissal, which are however economically small: only 13% higher earnings in terms of minimum wages (local average earnings is around 2.1 m.w.) and roughly half a year in age difference. The same exercise done in the previous empirical analysis is repeated here: I assess whether imbalances in pre-determined covariates can predict discontinuities in $s_0$ and unemployment duration. The same table shows that discontinuities estimate are not significant neither for predicted $s_0$ or unemployment duration. Moreover, if there is any bias at all, it goes to the opposite direction to the estimated effect. Therefore, I interpret this evidence on the continuity of covariates and predicted outcome values as strongly supportive for the validity of the design.

On the other hand, as shown by figure 4.8, the graphical evidence on $s_0$ and unemployment duration is striking. There is a clear negative jump on $s_0$ and a clear positive discontinuity in unemployment duration, as one would expect from theory. Results in table 4.7 suggest that an extra month of potential duration decreases the probability of exiting unemployment within 8 weeks by 1.9%, which is robust to the inclusion of controls. Results from the specification without controls point that unemployment duration increases by 2 weeks as potential duration is extended by a month. It yields a similar value to the one found by Landais (2014) with US data which points that an extra week of potential duration raises duration by around 0.4 weeks. This result on the full unemployment duration however should be taken with some caution as it is not robust to the inclusion of controls: in this regression the effect decreases by 50% and is no longer statistically significant. In any case, this last result is not the effect of interest for estimating the liquidity-to-moral hazard ratio.

In order to investigate whether these results are driven by the fact that the proposed design takes workers displaced with 22 tenure months as counterfactuals for those displaced with 24 months, I provide the following placebo test. I implement the same design at two thresholds in which there are no changes in UI potential duration. The first placebo test assess whether job finding rates of workers displaced with 16 tenure months are any different of those laid-off with 18 tenure months. The second placebo test repeats the same procedure for workers laid-off with 30 and 32 tenure months. These results are displayed in table 4.10. It is reassuring to see that, in absolutely all specifications, it is not possible to find any statistical significant result on $s_0$ in any of the two placebo tests. This strongly suggests that dropping workers displaced in between 22 and 24 months of tenure is not driving the results from above.

## 4.5 Liquidity-to-Moral Hazard Estimates

In the two previous section, I estimate the effect of providing liquidity and extending potential duration on the probability of re-employment at the beginning of the spell, $s_0$. The goal of this section is to use these estimates with the background theory from section 2 to estimate how large is the liquidity-to-moral hazard ratio in Brazil. I proceed in two steps. First, I use the estimates of $\frac{\partial s_0}{\partial A_0}$ and $\frac{\partial s_0}{\partial b_t}$ to recover the moral hazard effect $\frac{\partial s_0}{\partial w_0}$ by using equation (4.2). Second, I use the recovered moral hazard effect and liquidity effect estimated in section 4.3 to estimate the liquidity to moral hazard ratio by using equation (4.5).

Before moving to this two step procedure, I define the specific preferred estimates which are used to evaluate the ratio. From section 4.3, the liquidity effect is estimated for three different groups split by tenure. Since the overall UI effect on $s_0$ is a local effect for workers displaced with around $22^-/24^+$ tenure months, I decide to use the liquidity effects estimated for the mid tenure group (from 6 to 60 tenure months at displacement). This avoids concerns that both estimates are taken from groups of workers which are too different. Furthermore, I use the liquidity effect estimates on the sample of workers which surely satisfy eligibility condition (iii) for the cash grant, as in table 4.6. These estimates do not suffer from a downward bias for the reasons discussed in section 4.3. Therefore, my preferred estimate for the liquidity effect is that a cash grant equal to one minimum wage decreases $s_0$ by 0.67% for these group of workers. Nevertheless, in a second specification I also report estimates of the liquidity-to-moral hazard ratio based on the liquidity effect found for workers of all tenures, as in table 4.5 with controls. As regards $\frac{\partial s_0}{\partial b_t}$, my preferred estimate from table 4.7 is the one where controls are included, which should increase precision. It indicates that $s_0$ decreases by 1.9% when potential duration is extended by one month. This is equivalent to increasing potential benefits in the fifth month of unemployment from zero to 1.5 minimum wages, since replacement ratio is around 0.75 for workers earning around 2 m.w..

At this point, I use these estimates to feed equation 4.2 and recover the moral hazard effect $\frac{\partial s_0}{\partial w_0}$, as shown by table 4.8. Then, by using equation (4.5), the liquidity-to-moral hazard ratio is estimated. I find a ratio of 98% in the preferred specification and a similar value of 89% in the specification using the liquidity effect based on the sample with workers of all tenure groups. This estimate is very close to the one found by Landais (2014) which finds a ratio of 0.88% using data from the US. This means that roughly 50% of the marginal increase in duration when UI benefits are raised is due to liquidity constrains.

## 4.6 Conclusion

This paper exploits two different policies in Brazil to estimate the liquidity-to-moral hazard ratio. First, it exploits a bonus policy which grants some low-income earners with a cash grant to assess how unemployment outcomes react to the provision of liquidity. I find that providing a cash grant roughly equivalent to half of a monthly wage decreases the probability that unemployed workers find a new job within 8 weeks of spell by 0.65%. Second, by exploiting the UI potential duration schedule, I estimate that an extra month of potential duration decreases the probability that unemployed workers find a new job within 8 weeks by 1.9%. Then, I use results from Landais (2014) based in a partial equilibrium search model to put these estimates together and estimate the liquidity-to-moral hazard ratio in Brazil. I find this ratio to be close to 1, suggesting that half of the marginal response of unemployed durations to increases in UI are due to a liquidity effect.

This work extends the extremely scarce empirical evidence on the liquidity-to-moral hazard ratio, which is a key measure to evaluate the benefit side from providing unemployment insurance. An important advantage of the approach here proposed is that it is able to evaluate the liquidity-to-moral ratio without the need to rely on policy changes, which are infrequent. Therefore, it can serve policy makers as method to deliver timely estimates of this statistic over time.

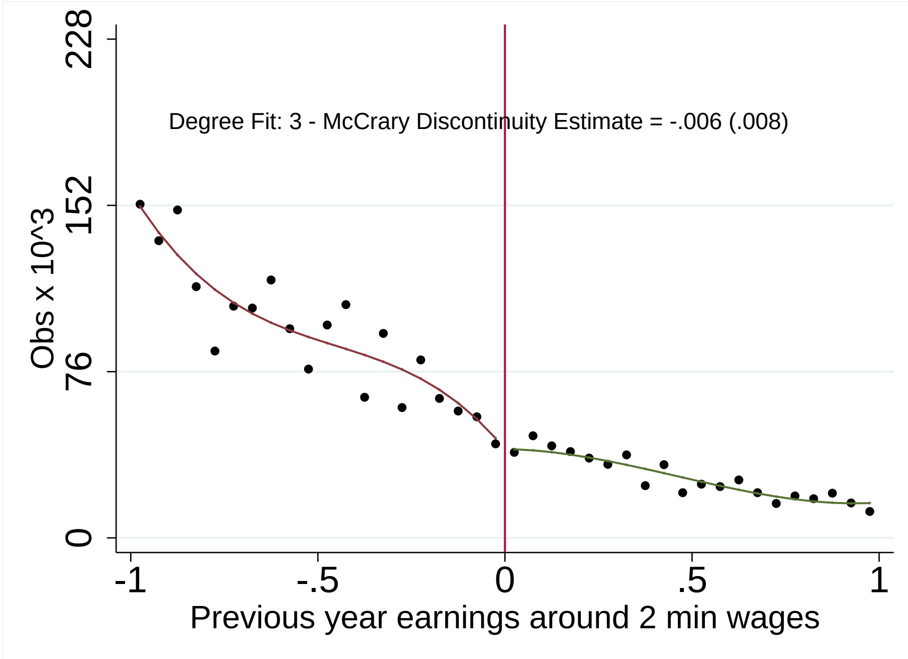
To the best of my knowledge, this is also the first paper to evaluate this ratio in a developing country, where informality is often very high. The results found in this paper challenge the common suspicion that providing unemployment insurance in contexts of large informality is not beneficial to welfare as moral hazard is expected to be too high.[13] The estimates presented in this paper show instead that moral hazard is actually not much different from previous figures found for the US, and, more importantly, the liquidity-to-moral hazard ratio seems to be about the same. A further relevant consideration is that this is not an artifact of local marginal effects due to differences in UI generosity between Brazil and the US. Both countries provide a similar potential duration and, if anything, replacement ratios are larger in Brazil.[14]

This work also complements results by Gerard and Gonzaga (2013). They show that since a large share of displaced workers in Brazil do not come back to the formal labor market, the efficiency-cost of providing UI is limited. They argue the most UI beneficiaries exhaust benefits independent of the insurance provision and, therefore, the behavioral cost of increasing UI benefits is low. As they show that only this behavioral cost is detrimental to welfare, increasing UI generosity yields limited welfare costs. The open question which remains from their analysis is whether the welfare gains from consumption smoothing are not small in a context where informality is prevalent. The results presented here fill this gap and show that UI benefits from consumption smoothing are as large as previously found in the US. Taken together, they suggest that, at least on the margin, raising the generosity of unemployment insurance in Brazil could generate significant welfare gains.

[13] See Vodopivec (2013) for a throughout analysis of UI in markets with large informality

[14] Potential duration ranges from three to five months in Brazil while the average in the US is roughly 22 weeks in the sample used by Landais (2014). Replacement ratios in Brazil are as high as 100% for workers in the low-end of the earnings distribution and are still around 68% at the benefit cap, while in the US it roughly average 50% and rarely ever exceed 60%.

Figure 4.1: Density of Previous Earnings around 2 m.w. Threshold

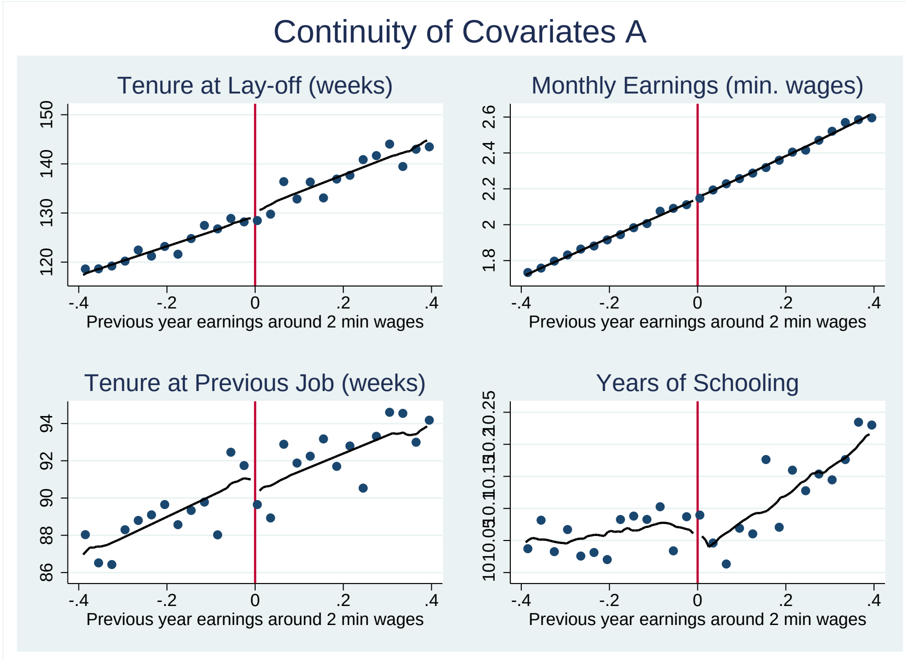



The graph displays how the density of previous earnings evolve around the threshold. At each side of the threshold, the density is approximated by the polynomial which minimizes the Akaike Criterion. The graph also displays the test statistic for the McCrary Discontinuity test.

Figure 4.2: Unemployment Outcomes around 2 m.w. Threshold

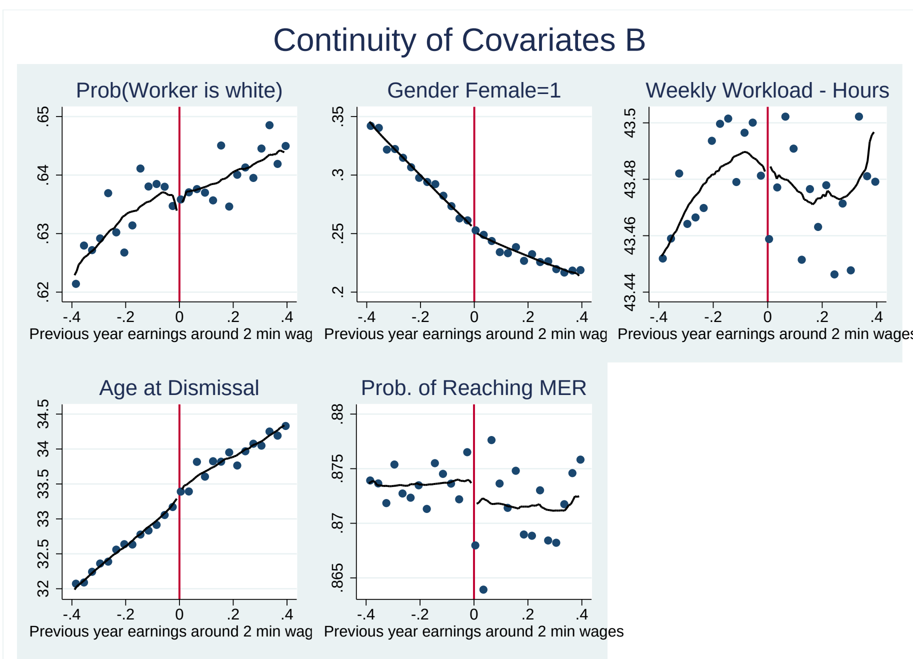

The graph displays how unemployment outcomes evolve around the threshold. At each side of the threshold, the conditional expectation function is approximated by a epanechnikov smoothed local linear polynomial with rectangular kernel. $s_0$ defines the probability of re-employment within 8 weeks.

Figure 4.3: Covariates A around 2 m.w. Threshold



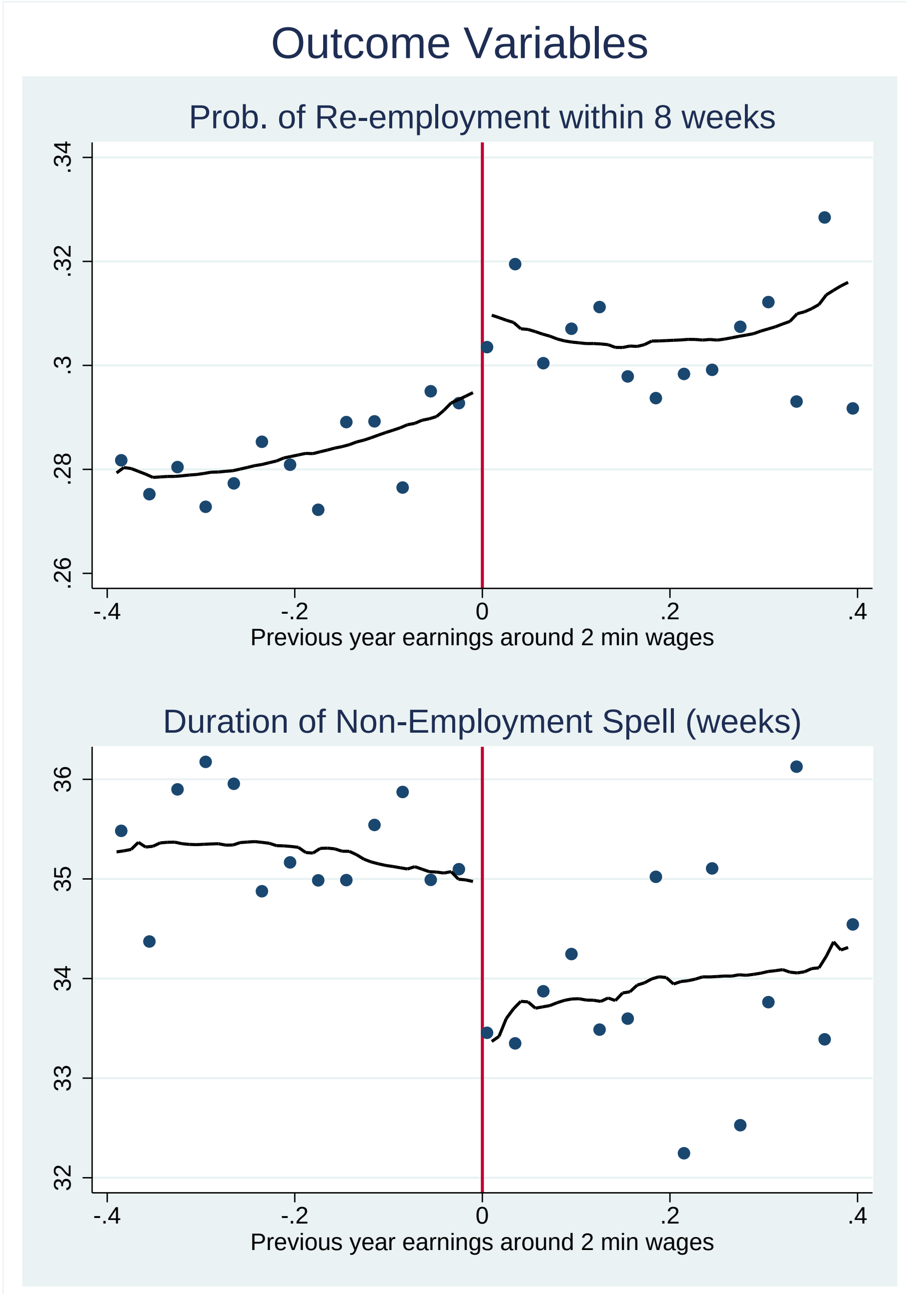

The graph displays how pre-determined covariates evolve around the threshold. At each side of the threshold, the conditional expectation function is approximated by a epanechnikov smoothed local linear polynomial with rectangular kernel.

Figure 4.4: Covariates B around 2 m.w. Threshold




The graph displays how pre-determined covariates evolve around the threshold. At each side of the threshold, the conditional expectation function is approximated by a epanechnikov smoothed local linear polynomial with rectangular kernel.

Figure 4.5: Unemployment Outcomes around 2 m.w. Threshold - Tenure 0-5.8 months

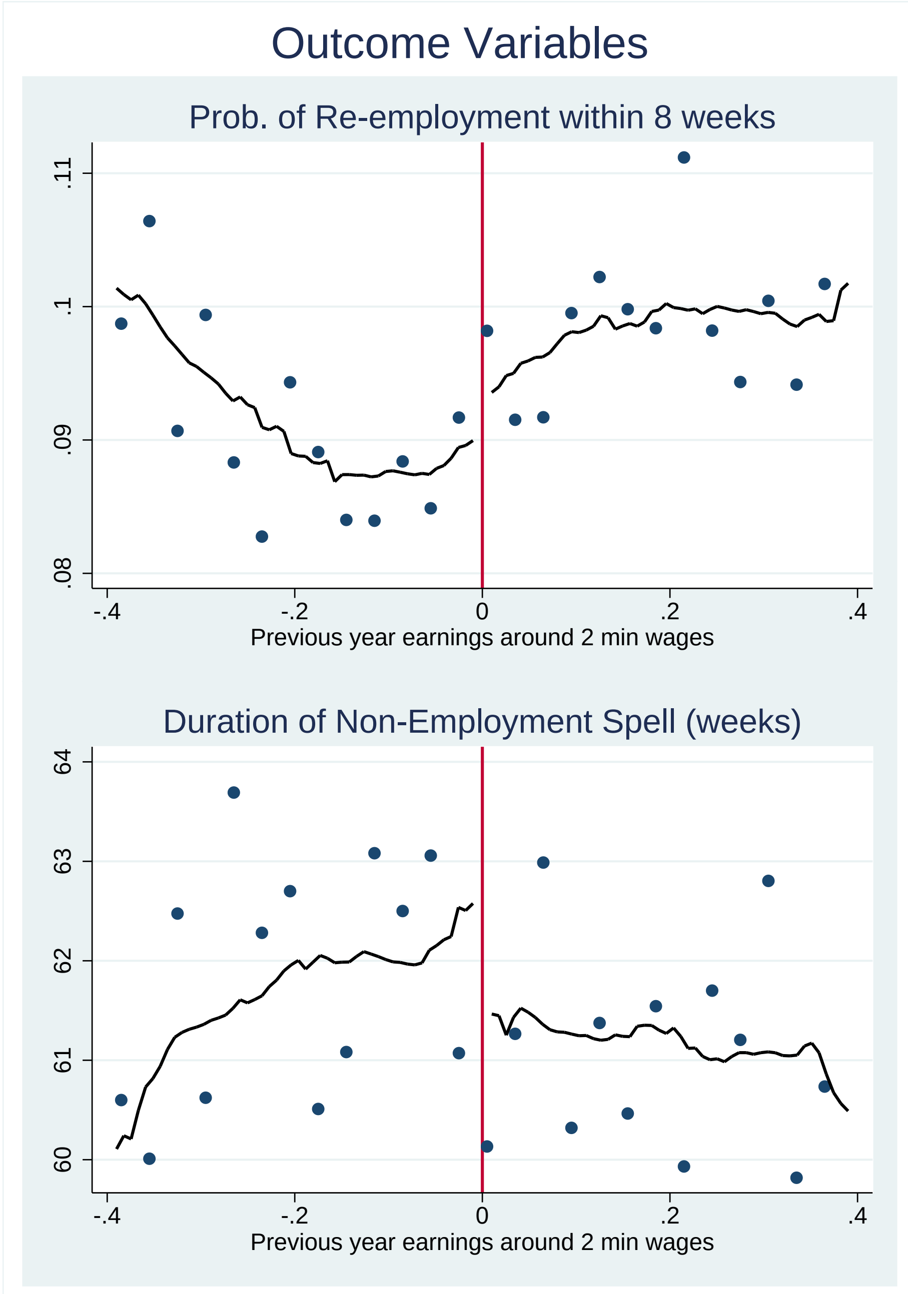



The graph displays how unemployment outcomes evolve around the threshold. At each side of the threshold, the conditional expectation function is approximated by a epanechnikov smoothed local linear polynomial with rectangular kernel. $s_0$ defines the probability of re-employment within 8 weeks.

Figure 4.6: Unemployment Outcomes around 2 m.w. Threshold - Tenure 6-60 months

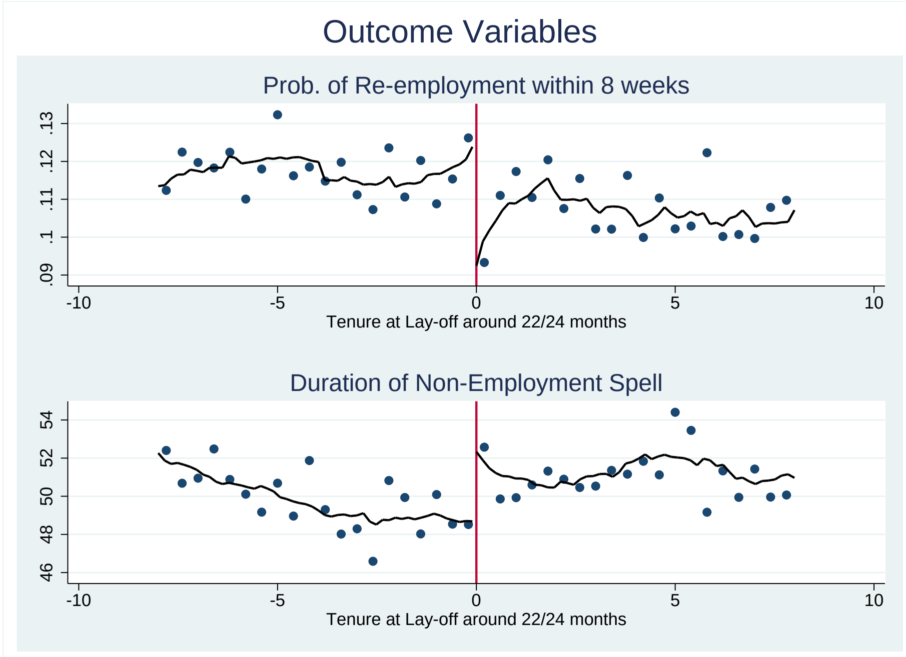

The graph displays how unemployment outcomes evolve around the threshold. At each side of the threshold, the conditional expectation function is approximated by a epanechnikov smoothed local linear polynomial with rectangular kernel. $s_0$ defines the probability of re-employment within 8 weeks.

Figure 4.7: Unemployment Outcomes around 2 m.w. Threshold - Tenure 60-$\infty$ months



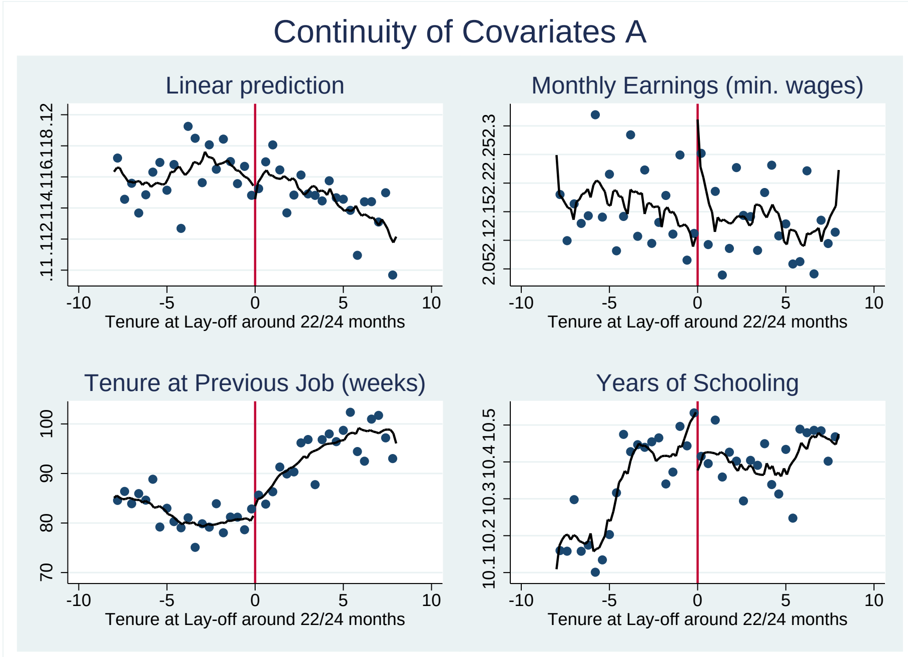

The graph displays how unemployment outcomes evolve around the threshold. At each side of the threshold, the conditional expectation function is approximated by a epanechnikov smoothed local linear polynomial with rectangular kernel. $s_0$ defines the probability of re-employment within 8 weeks.

Figure 4.8: Unemployment Outcomes around 22-/24+ Tenure Threshold



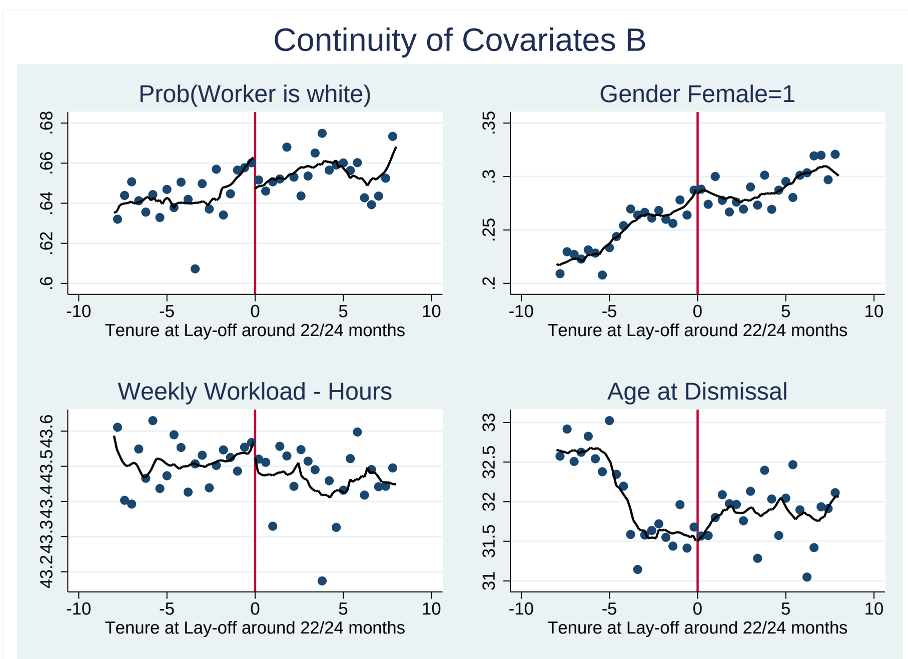

The graph displays how unemployment outcomes evolve around the threshold. At each side of the threshold, the conditional expectation function is approximated by a epanechnikov smoothed local linear polynomial with rectangular kernel. $s_0$ defines the probability of re-employment within 8 weeks. Unemployment duration is expressed in weeks.

Figure 4.9: Covariates A around 22-/24+ Tenure Threshold




The graph displays how pre-determined covariates evolve around the threshold. At each side of the threshold, the conditional expectation function is approximated by a epanechnikov smoothed local linear polynomial with rectangular kernel.

Figure 4.10: Covariates B around 22-/24+ Tenure Threshold




The graph displays how pre-determined covariates evolve around the threshold. At each side of the threshold, the conditional expectation function is approximated by a epanechnikov smoothed local linear polynomial with rectangular kernel.

Table 4.2: Summary Statistics

| Tenure range (months) | | | 0-5.8 | | 6-60 | | 60-99999 | | 14-30 | |
|---|---|---|---|---|---|---|---|---|---|---|
| Variable | Mean | Std. Dev. | Mean | Std. Dev. | Mean | Std. Dev. | Mean | Std. Dev. | Mean | Std. Dev. |
| *outcomes* | | | | | | | | | | |
| $s_0$ | 0.12 | (0.3) | 0.27 | (0.4) | 0.10 | (0.3) | 0.10 | (0.3) | 0.12 | (0.3) |
| Unemployment Duration | 53.1 | (52.0) | 36.0 | (44.7) | 54.8 | (51.7) | 62.3 | (57.6) | 50.1 | (50.1) |
| *covariates* | | | | | | | | | | |
| Tenure Last Job (weeks) | 120.1 | (122.2) | 13.7 | (6.4) | 101.4 | (55.5) | 404.8 | (170.4) | - | - |
| UI Min. Elig. Req. | 0.88 | (0.3) | 0.00 | (0.0) | 1.00 | (0.0) | 1.00 | (0.0) | 1.00 | (0.0) |
| Monthly Earnings | 1.8 | (1.5) | 1.9 | (1.7) | 1.7 | (1.4) | 2.0 | (1.8) | 2.3 | (1.7) |
| Years of Schooling | 10.1 | (285.7) | 9.8 | (307.9) | 10.2 | (278.7) | 9.5 | (303.2) | 10.4 | (282.5) |
| Worker is white | 0.59 | (0.5) | 0.54 | (0.5) | 0.60 | (0.5) | 0.63 | (0.5) | 0.65 | (0.5) |
| Gender | 0.36 | (0.5) | 0.25 | (0.4) | 0.37 | (0.5) | 0.41 | (0.5) | 0.24 | (0.4) |
| Weekly Hours | 43.4 | (2.8) | 43.5 | (2.7) | 43.4 | (2.8) | 43.3 | (3.2) | 43.5 | (2.6) |
| Age at Dismissal* | 32.5 | (9.8) | 31.3 | (9.3) | 32.0 | (9.6) | 38.0 | (10.2) | 32.4 | (9.5) |
| Observations | 2,416,709 | | 301,383 | | 1,871,615 | | 237,778 | | 60,893 | |

Note: Age at Dismissal is missing for the years of 2011 and 2012. Therefore it has only 1,654,241 observations in the full sample. $s_0$ defines the probability of re-employment within 8 weeks. Duration outcomes are expressed in weeks.

Table 4.3: RDD - Two Minimum Wages Threshold - Whole Sample

| | Discontinuity | s.e. | Discontinuity | s.e. | IK Bandwidth |
|---|---|---|---|---|---|
| *Outcomes* | | | | | |
| $s_0$ | 0.005*** | (.002) | 0.005*** | (0.002) | 0.36 |
| Unemployment Duration | 0.478* | (.289) | 0.029 | (0.274) | 0.29 |
| | | | | | |
| *Covariates* | | | | | |
| Predicted $s_0$ | -0.0002 | (.0004) | | | 0.35 |
| Predicted Unempl. Dur. | 0.599*** | (.129) | | | 0.25 |
| Tenure Last Job | 1.979*** | (.57) | | | 0.61 |
| UI Min. Elig. Req. | -0.0019 | (.0017) | | | 0.40 |
| Monthly Earnings | 0.002 | (.008) | | | 0.38 |
| Tenure Previous Job | -0.784 | (.613) | | | 0.61 |
| Years of Schooling | -0.063*** | (.018) | | | 0.25 |
| Worker is white | -0.003 | (.003) | | | 0.31 |
| Gender | -0.004** | (.002) | | | 0.44 |
| Weekly Hours | -0.011 | (.016) | | | 0.35 |
| Age at Dismissal | 0.246*** | (.071) | | | 0.26 |
| | | | | | |
| Controls | N | | Y | | |

Note: The table displays discontinuities' estimates for each variable on the right side of the threshold, where workers are not eligible to the bonus due to the earnings criterion. All specifications use linear splines, bandwidth is selected according to IK selector and standard errors are displayed in parentheses. $s_0$ defines the probability of re-employment within 8 weeks. Duration outcomes are expressed in weeks. Predicted $s_0$ and Unemployment Duration are obtained by regressing each variable on a rich set of pre-determined covariates: quintile of average earnings in the previous year, decile of age at hiring and dismissal, employment duration at the start of the year, and dummies for each month of tenure in the last and previous job, industry of last and previous employer, dismissal cause at previous job, race, gender, weekly hours of work, year, calendar month of dismissal and federal state (27).

Table 4.4: RDD - Two Minimum Wages Threshold - By Tenure Groups

| Tenure Range (months) | 0-5.8 | | 6.0-60 | | 60+ | |
|---|---|---|---|---|---|---|
| | Discontinuity | s.e. | Discontinuity | s.e. | Discontinuity | s.e. |
| *Outcomes* | | | | | | |
| $s_0$ | 0.013** | (.006) | 0.0037* | (0.002) | 0.006 | (0.006) |
| Unemployment Duration | -1.629*** | (.592) | 0.814** | (0.363) | -0.878 | (0.877) |
| Controls | N | | N | | N | |
| $s_0$ | 0.013** | (.006) | 0.0034* | (0.002) | 0.005 | (0.006) |
| Unemployment Duration | -1.924*** | (.572) | 0.334 | (0.347) | -0.212 | (0.798) |
| Controls | Y | | Y | | Y | |
| *Covariates* | | | | | | |
| Predicted $s_0$ | -0.002** | (.0009) | -0.001*** | (0.000) | 0.002*** | (0.001) |
| Predicted Unempl. Dur. | 0.395* | (.235) | 0.718*** | (0.138) | -0.499 | (0.366) |
| Tenure Last Job | -0.011 | (.082) | 0.474 | (0.389) | 0.328 | (2.133) |
| Monthly Earnings | -0.036 | (.023) | 0.004 | (0.009) | 0.03 | (0.025) |
| Tenure Previous Job | 1.12 | (1.155) | -0.657 | (0.811) | -1.882 | (2.969) |
| Years of Schooling | -0.064 | (.046) | -0.052*** | (0.018) | -0.026 | (0.036) |
| Worker is white | -0.003 | (.006) | 0 | (0.003) | -0.008 | (0.008) |
| Gender | -0.006 | (.006) | -0.002 | (0.003) | -0.014** | (0.007) |
| Weekly Hours | 0.028 | (.029) | -0.024 | (0.019) | -0.053 | (0.045) |
| Age at Dismissal | 0.091 | (.158) | 0.25*** | (0.079) | 0.239 | (0.188) |

Note: The table displays discontinuities' estimates for each variable on the right side of the threshold, where workers are not eligible to the bonus due to the earnings criterion. All specifications use linear splines, bandwidth is selected according to IK selector and standard errors are displayed in parentheses. $s_0$ defines the probability of re-employment within 8 weeks. Duration outcomes are expressed in weeks. Predicted $s_0$ and Unemployment Duration are obtained by regressing each variable on a rich set of pre-determined covariates: quintile of average earnings in the previous year, decile of age at hiring and dismissal, employment duration at the start of the year, and dummies for each month of tenure in the last and previous job, industry of last and previous employer, dismissal cause at previous job, race, gender, weekly hours of work, year, calendar month of dismissal and federal state (27).

Table 4.5: RDD - Two Minimum Wages Threshold - Only Eligible Workers

| | Discontinuity | s.e. | Discontinuity | s.e. | IK Bandwidth |
|---|---|---|---|---|---|
| *Outcomes* | | | | | |
| $s_0$ | 0.0043 | (.0044) | 0.0064 | (0.0043) | 0.27 |
| Unemployment Duration | -0.074 | (.339) | -0.402 | (0.320) | 0.26 |
| | | | | | |
| *Covariates* | | | | | |
| Predicted $s_0$ | -0.0042*** | (.0009) | | | 0.50 |
| Predicted Unempl. Dur. | 0.685*** | (.148) | | | 0.43 |
| Tenure Last Job | 5.251*** | (1.166) | | | 0.50 |
| UI Min. Elig. Req. | -0.0002 | (.0008) | | | 0.62 |
| Monthly Earnings | -0.005 | (.011) | | | 0.41 |
| Tenure Previous Job | 0.173 | (1.244) | | | 0.49 |
| Years of Schooling | -0.001 | (.025) | | | 0.42 |
| Worker is white | 0.002 | (.006) | | | 0.22 |
| Gender | 0.005 | (.004) | | | 0.32 |
| Weekly Hours | -0.009 | (.022) | | | 0.47 |
| Age at Dismissal | 0.153 | (.141) | | | 0.43 |
| | | | | | |
| Controls | N | | Y | | |

Note: This sample is restricted only to workers who surely attend the five years eligibility criterion. The table displays discontinuities' estimates for each variable on the right side of the threshold, where workers are not eligible to the bonus due to the earnings criterion. All specifications use linear splines, bandwidth is selected according to IK selector and standard errors are displayed in parentheses. $s_0$ defines the probability of re-employment within 12 weeks. Duration outcomes are expressed in weeks. Predicted $s_0$ and Unemployment Duration are obtained by regressing each variable on a rich set of pre-determined covariates: quintile of average earnings in the previous year, decile of age at hiring and dismissal, employment duration at the start of the year, and dummies for each month of tenure in the last and previous job, industry of last and previous employer, dismissal cause at previous job, race, gender, weekly hours of work, year, calendar month of dismissal and federal state (27).

Table 4.6: Effect of Bonus Provision - Only Eligible Workers by Tenure Group

| Tenure Range (months) | 0-5.8 | | 6.0-60 | | 60+ | |
|---|---|---|---|---|---|---|
| | Discontinuity | s.e. | Discontinuity | s.e. | Discontinuity | s.e. |
| *Outcomes* | | | | | | |
| $s_0$ | 0.013 | (.0112) | 0.0059 | (0.0052) | 0.005 | (0.0063) |
| Unemployment Duration | -0.1 | (.669) | 0.208 | (0.375) | -0.87 | (0.863) |
| Controls | N | | N | | N | |
| $s_0$ | 0.016 | (.0112) | 0.0067 | (0.0051) | 0.006 | (0.0061) |
| Unemployment Duration | -0.473 | (.651) | -0.099 | (0.362) | -0.582 | (0.788) |
| Controls | Y | | Y | | Y | |
| *Covariates* | | | | | | |
| Predicted $s_0$ | -0.007*** | (.0024) | -0.003*** | (0.001) | -0.001 | (0.002) |
| Predicted Unempl. Dur. | 0.966*** | (.369) | 0.71*** | (0.166) | 0.026 | (0.361) |
| Tenure Last Job | 0.03 | (0.166) | 1.659*** | (0.629) | -1.086 | (3.439) |
| Monthly Earnings | 0.026 | (.038) | 0.002 | (0.013) | -0.04* | (0.024) |
| Tenure Previous Job | 3.408 | (2.329) | -1.023 | (1.243) | -2.904 | (4.619) |
| Years of Schooling | 0.043 | (.073) | 0.03 | (0.033) | -0.009 | (0.067) |
| Worker is white | -0.007 | (0.012) | 0 | (0.005) | 0 | (0.011) |
| Gender | 0.012 | (.01) | 0.005 | (0.005) | -0.011 | (0.010) |
| Weekly Hours | 0.034 | (.05) | -0.039 | (0.026) | 0.059 | (0.062) |
| Age at Dismissal | -0.152 | (.371) | 0.156 | (0.162) | 0.079 | (0.323) |

Note: This sample is restricted only to workers who surely attend the five years eligibility criterion. The table displays discontinuities' estimates for each variable on the right side of the threshold, where workers are not eligible to the bonus due to the earnings criterion. All specifications use linear splines, bandwidth is selected according to IK selector and standard errors are displayed in parentheses. $s_0$ defines the probability of re-employment within 12 weeks. Duration outcomes are expressed in weeks. Predicted $s_0$ and Unemployment Duration are obtained by regressing each variable on a rich set of pre-determined covariates: quintile of average earnings in the previous year, decile of age at hiring and dismissal, employment duration at the start of the year, and dummies for each month of tenure in the last and previous job, industry of last and previous employer, dismissal cause at previous job, race, gender, weekly hours of work, year, calendar month of dismissal and federal state (27).

Table 4.7: Effect of Extra Month of Unemployment Benefits - Displaced workers with around $22^{-}/24^{+}$ months of tenure

| | Discontinuity | s.e. | Discontinuity | s.e. | IK Bandwidth |
|---|---|---|---|---|---|
| *Outcomes* | | | | | |
| $s_0$ | -0.018* | (.011) | -0.019* | (0.010) | 2.35 |
| Unemployment Duration | 2.188** | (.955) | 1.158 | (0.927) | 6.77 |
| | | | | | |
| *Covariates* | | | | | |
| Predicted $s_0$ | 0.0017 | (.002) | | | 2.31 |
| Predicted Unempl. Dur. | -0.6035 | (.673) | | | 2.15 |
| Monthly Earnings | 0.1308*** | (.05) | | | 2.19 |
| Tenure Previous Job | 4.2367 | (3.9931) | | | 3.81 |
| Years of Schooling | -0.1504 | (.0942) | | | 2.07 |
| Worker is white | -0.0201 | (.0176) | | | 2.28 |
| Gender | 0.0187 | (.0137) | | | 2.61 |
| Weekly Hours | -0.0701 | (.0677) | | | 3.82 |
| Age at Dismissal | 0.5978* | (.3261) | | | 3.20 |
| | | | | | |
| Controls | N | | Y | | |

Note: The table displays discontinuities' estimates for each variable on the right side of the threshold for an extra month of potential UI duration. On the left side of the threshold are workers with less than 22 months of tenure, while on the right side are workers with more than 24 months of tenure; workers in between are not included due to reasons discussed in the text. All specifications use linear splines, bandwidth is selected according to IK selector and standard errors are displayed in parentheses. $s_0$ defines the probability of re-employment within 8 weeks. Duration outcomes are expressed in weeks. Predicted $s_0$ and Unemployment Duration are obtained by regressing each variable on a rich set of pre-determined covariates: quantile of average earnings in the previous year, decile of age at hiring and dismissal, and dummies for industry of last employer, race, gender, weekly hours of work, year, calendar month of dismissal, federal state (27) and eligibility for yearly bonus.

Table 4.8: Liquidity to Moral Hazard Estimates

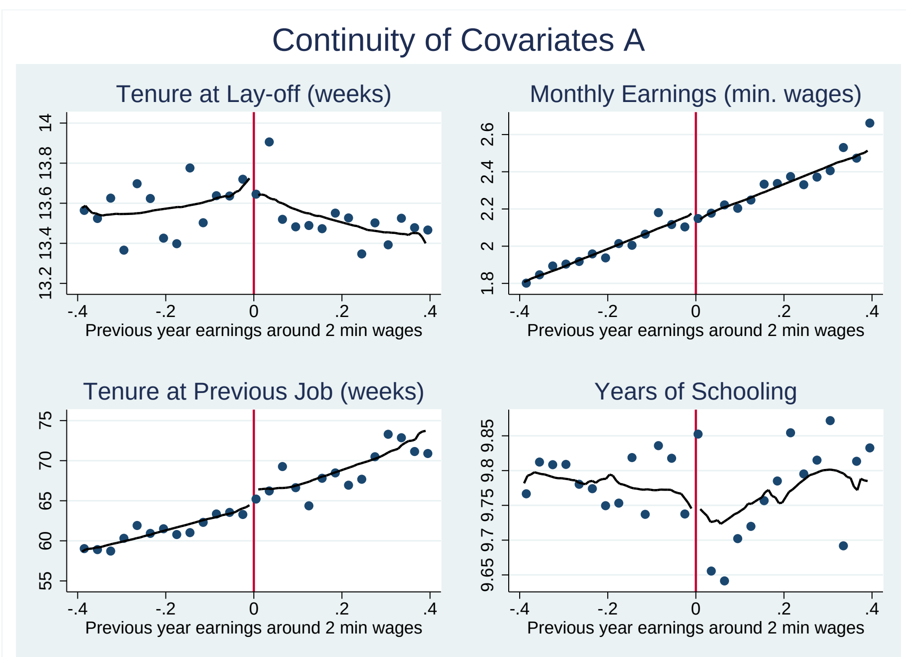
| | | |
|---|---|---|
| Empirics I - Liquidity Effect | | |
| *specification* | Tenure 6 - 60 | Whole Sample |
| $\Delta s_0$ | -0.0068 | -0.0065 |
| $\Delta A_0$ | 592.11 | 592.42 |
| $\frac{\partial s_0}{\partial A_0}$ x $10^3$ | -0.0115 | -0.0109 |
| Empirics II - Potential Duration Effect | | |
| $\Delta s_0$ | -0.0190 | -0.0190 |
| $\Delta b_t$ | 830.9 | 830.9 |
| $\frac{\partial s_0}{\partial b_t}$ x $10^3$ | -0.0229 | -0.0229 |
| Moral Hazard Estimate | | |
| $S_t$ | 0.857 | 0.857 |
| $\frac{\partial s_0}{\partial w_0}$ x $10^3$ | 0.0133 | 0.0140 |
| Liquidity-to-Moral Hazard Estimate | | |
| $\bar{S}_1^B$ | 0.879 | 0.879 |
| Liquidity-to-Moral Hazard Ratio $\rho$ | 0.98 | 0.89 |

Note: Currency values are expressed in reais in 2012 prices.

## 4.7 Appendix

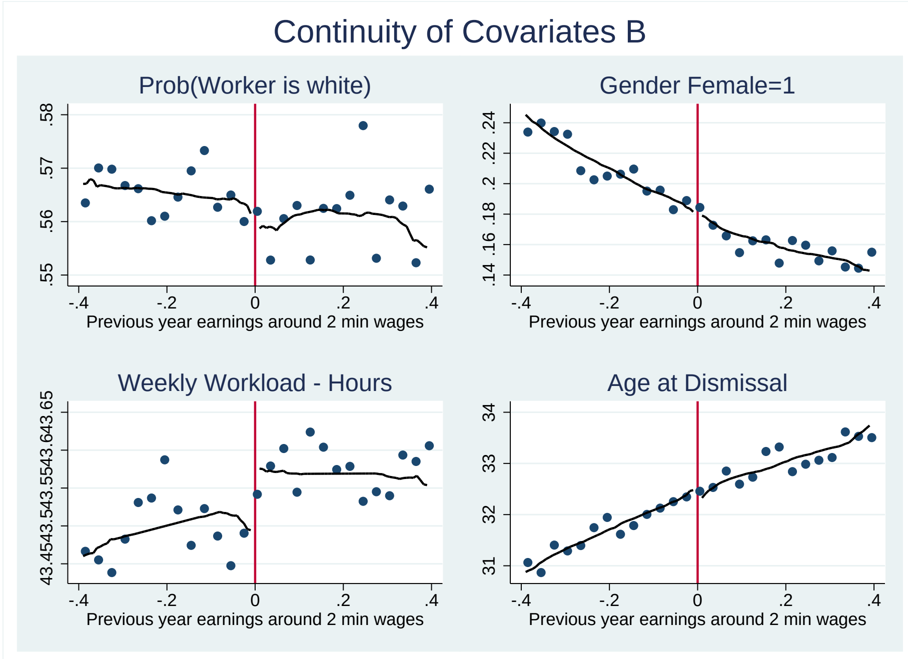
### 4.7.1 Figures

Figure 4.11: Covariates A around 2 m.w. Threshold - Tenure 0-5.8 months



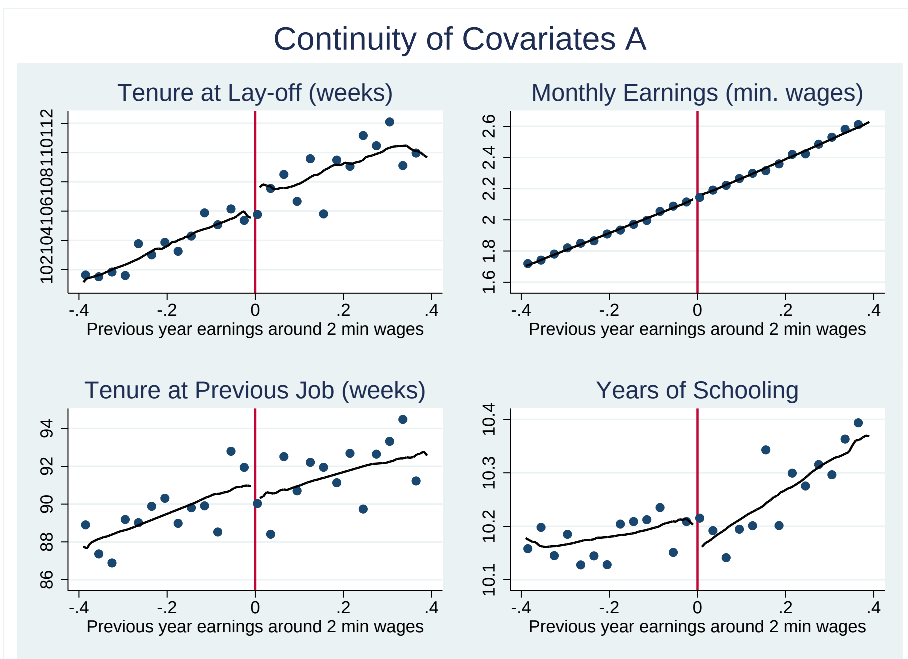

The graph displays how pre-determined covariates evolve around the threshold. At each side of the threshold, the conditional expectation function is approximated by a epanechnikov smoothed local linear polynomial with rectangular kernel.

Figure 4.12: Covariates B around 2 m.w. Threshold - Tenure 0-5.8 months



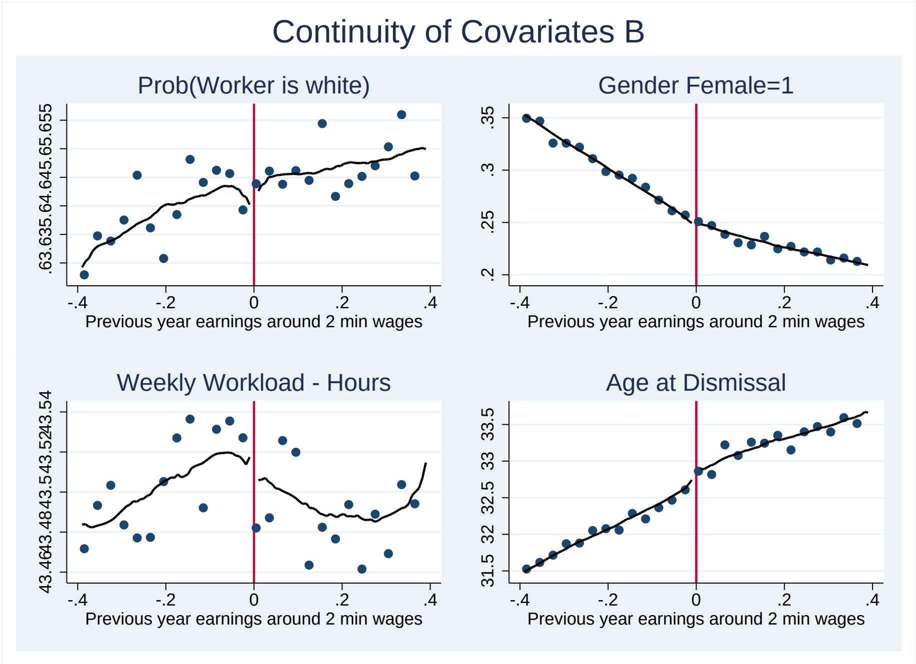

The graph displays how pre-determined covariates evolve around the threshold. At each side of the threshold, the conditional expectation function is approximated by a epanechnikov smoothed local linear polynomial with rectangular kernel.

Figure 4.13: Covariates A around 2 m.w. Threshold - Tenure 6-60 months



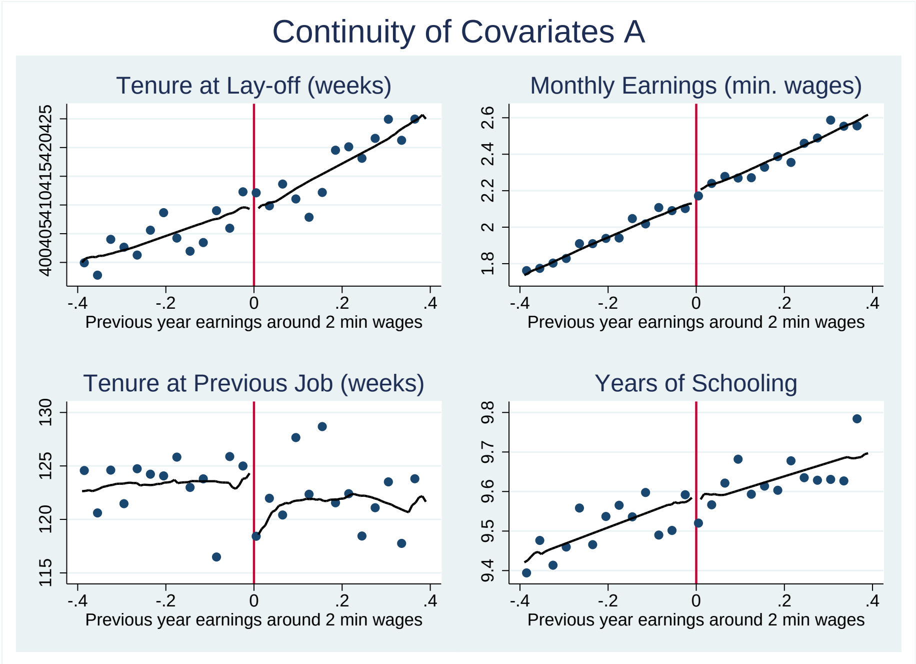

The graph displays how pre-determined covariates evolve around the threshold. At each side of the threshold, the conditional expectation function is approximated by a epanechnikov smoothed local linear polynomial with rectangular kernel.

Figure 4.14: Covariates B around 2 m.w. Threshold - Tenure 6-60 months



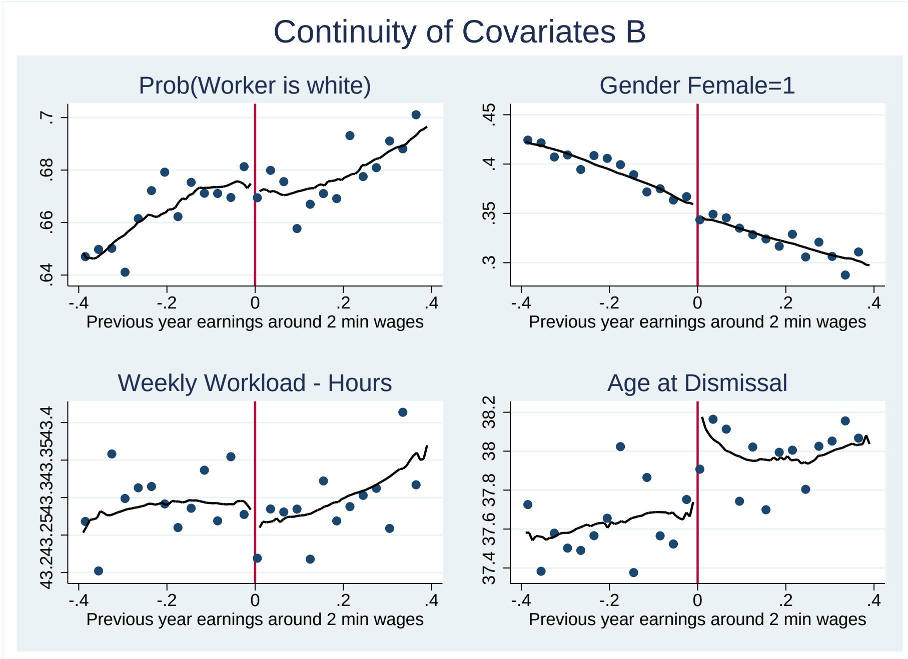

The graph displays how pre-determined covariates evolve around the threshold. At each side of the threshold, the conditional expectation function is approximated by a epanechnikov smoothed local linear polynomial with rectangular kernel.

Figure 4.15: Covariates A around 2 m.w. Threshold - Tenure 60-$\infty$ months




The graph displays how pre-determined covariates evolve around the threshold. At each side of the threshold, the conditional expectation function is approximated by a epanechnikov smoothed local linear polynomial with rectangular kernel.

Figure 4.16: Covariates B around 2 m.w. Threshold - Tenure 60-$\infty$ months




The graph displays how pre-determined covariates evolve around the threshold. At each side of the threshold, the conditional expectation function is approximated by a epanechnikov smoothed local linear polynomial with rectangular kernel.

### 4.7.2 Tables

Table 4.9: Effect of Bonus Provision - Only Eligible Workers By Tenure Group - Local Quadratic Regressions

| Tenure Range (months) | 0-5.8 | | 6.0-60 | | 60+ | |
|---|---|---|---|---|---|---|
| | Discontinuity | s.e. | Discontinuity | s.e. | Discontinuity | s.e. |
| *Outcomes* | | | | | | |
| $s_0$ | 0.019* | (.012) | 0.0083 | (0.0058) | 0.003 | (0.010) |
| Unemployment Duration | 0.546 | (.89) | -0.404 | (0.450) | -0.877 | (0.972) |
| Controls | N | | N | | N | |
| $s_0$ | 0.024** | (.012) | 0.0087 | (0.0056) | 0.004 | (0.010) |
| Unemployment Duration | 0.167 | (.87) | -0.742* | (0.437) | -0.798 | (0.884) |
| Controls | Y | | Y | | Y | |
| *Covariates* | | | | | | |
| Predicted $s_0$ | -0.006* | (.0037) | -0.003*** | (0.001) | 0 | (0.003) |
| Predicted Unempl. Dur. | 1.297** | (.581) | -0.249 | (0.276) | -0.305 | (0.557) |
| Tenure Last Job | 0.045 | (0.208) | 3.038*** | (0.966) | -3.448 | (5.183) |
| Monthly Earnings | 0.028 | (.052) | 0.009 | (0.018) | -0.041 | (0.032) |
| Tenure Previous Job | 2.738 | (3.162) | -1.86 | (1.770) | -3.286 | (6.328) |
| Years of Schooling | 7.363 | (10.665) | 4.703 | (4.177) | 1.148 | (8.518) |
| Worker is white | -0.005 | (0.014) | 0.004 | (0.007) | -0.001 | (0.013) |
| Gender | 0.01 | (.012) | 0.005 | (0.007) | 0.001 | (0.013) |
| Weekly Hours | 0.18** | (.081) | -0.047 | (0.032) | 0.071 | (0.079) |
| Age at Dismissal | -0.279 | (.486) | -0.218 | (0.266) | 0.216 | (0.412) |

Note: This sample is restricted only to workers who surely attend the five years eligibility criterion. The table displays discontinuities' estimates for each variable on the right side of the threshold, where workers are not eligible to the bonus due to the earnings criterion. All specifications use linear splines, bandwidth is selected according to IK selector and standard errors are displayed in parentheses. $s_0$ defines the probability of re-employment within 12 weeks. Duration outcomes are expressed in weeks. Predicted $s_0$ and Unemployment Duration are obtained by regressing each variable on a rich set of pre-determined covariates: quintile of average earnings in the previous year, decile of age at hiring and dismissal, employment duration at the start of the year, and dummies for each month of tenure in the last and previous job, industry of last and previous employer, dismissal cause at previous job, race, gender, weekly hours of work, year, calendar month of dismissal and federal state (27).

Table 4.10: Placebo Thresholds on Tenure Discontinuity with 2 months gap

| Dep. Var.: $s_0$ | Discontinuity | s.e. | Discontinuity | s.e. | Bandwidth |
|---|---|---|---|---|---|
| | Placebo A: $16^{-}/18^{+}$ Threshold | | | | |
| *linear specification* | | | | | |
| 0.5 IK Bandwidth | -0.027 | (.029) | -0.027 | (0.028) | 0.69 |
| IK Bandwidth | 0.001 | (.02) | 0 | (0.020) | 1.38 |
| 1.5 IK Bandwidth | -0.021 | (.016) | -0.018 | (0.015) | 2.07 |
| *quadratic specification* | | | | | |
| 0.5 IK Bandwidth | -0.02 | (.03) | -0.02 | (0.030) | 1.37 |
| IK Bandwidth | -0.001 | (.021) | 0 | (0.021) | 2.74 |
| 1.5 IK Bandwidth | -0.022 | (.017) | -0.02 | (0.017) | 4.10 |
| Controls | N | | Y | | |
| | Placebo B: $30^{-}/32^{+}$ Threshold | | | | |
| *linear specification* | | | | | |
| 0.5 IK Bandwidth | -0.027 | (.023) | -0.025 | (0.023) | 0.83 |
| IK Bandwidth | -0.003 | (.016) | -0.001 | (0.016) | 1.66 |
| 1.5 IK Bandwidth | 0.007 | (.013) | 0.008 | (0.013) | 2.50 |
| *quadratic specification* | | | | | |
| 0.5 IK Bandwidth | -0.01 | (.037) | -0.01 | (0.037) | 0.93 |
| IK Bandwidth | -0.005 | (.023) | -0.002 | (0.024) | 1.86 |
| 1.5 IK Bandwidth | -0.006 | (.019) | -0.004 | (0.019) | 2.80 |
| Controls | N | | Y | | |

Note: The table displays discontinuities' estimates on $s_0$ for two placebo thresholds at which there is no change in UI potential duration. On the left side of each threshold are workers with less than 16 and 30 months of tenure respectively, while on the right side are workers with more than 18 and 32 months of tenure respectively; workers in between are not included due to reasons discussed in the text. Bandwidth is selected according to IK selector and standard errors are displayed in parentheses. $s_0$ defines the probability of re-employment within 8 weeks.

# Chapter 5

# Unemployment Insurance, Unemployment Inflow and Work Effort

## 5.1 Introduction

Unemployment Insurance (UI) is a widespread policy around the world which aims to support unemployed workers with temporary income. The economic reasoning behind the policy is to avoid individuals to experience large drops in consumption when out of employment, allowing therefore for a smoother path of consumption. However, on the flip side of the coin, as with most policies providing income transfers, it causes distortions on incentives and thus must be carefully designed in order to provide the best balance between social costs and benefits. Therefore, optimally designing UI systems hinges on finding the best balance between consumption smoothing and distortionary effects on work.

Most of the theoretical literature on the issue is concerned with the adverse incentives on search effort, which cause longer periods of unemployment. This is a well established finding supported by a vast amount of empirical work. On the other hand, only a few number of works treat job destruction as endogenous. Furthermore, none of these studies takes seriously the problem of analyzing how UI may affect workers' behavior in the job.

The aim of this paper is to fill this gap: it investigates how UI affects job termination and the mechanisms behind it. First, using administrative data from the Brazilian labor market, I report a positive discontinuity in the lay-off hazard rate at the minimum eligibility requirement (MER) for UI. By applying a regression discontinuity design, it is shown that eligibility for unemployment insurance causes the spike in hazard rates. Then, I argue that UI affects lay-off probabilities by decreasing work effort. Second, I provide a learning model with work effort which is able to explain the whole path of the employment hazard throughout tenure and the discontinuity found in the data. Finally, with support from personeel data, I use worker's absenteeism as a proxy of work effort and show that it peaks exactly at the MER for unemployment benefits.

The fact that the literature overlooks such channel can be partially explained by the rather limited empirical evidence on the link between job termination and work effort: "It is difficult to assess the empirical plausibility of these results (UI on work effort). There is virtually no empirical evidence available on the relationship between job destruction and workers' choice of effort" (Fredriksson and Holmlund, 2006) - pg. 9.

Few more recent studies report spikes in the job hazard rate at MER for UI. However, as noticed by Tuit and van Ours (2010), the mechanism behind this effect is not clear. According to them, there are mainly two, and non exclusive, explanations to this. The first is that it becomes less expensive to firms to fire employees once MER are met. They argue, for instance, that a worker entitled to UI benefits would be less likely to fight the dismissal in court. Another possible reason in the same line is that firings after MER would create less attrition with unions. The second explanation is precisely that UI eligibility undermines work effort and, through this channel, increases job hazard rates.

The underlying idea presented in this paper is that the insurance increases the value of the outside option for employed workers and therefore also affects their behavior in the job. I argue that the higher value of the outside option causes a decrease in work effort. If, at least to some extent, workers put effort in their activities with the goal of securing their jobs, we should expect UI eligibility to decrease effort. This mechanism ultimately causes higher unemployment inflow once MER are met. A second consequence of a more attractive outside option it that it could lead workers to prefer to be readily dismissed in order to enjoy the benefits. The rational is that if someone has a low value job, she may prefer to enjoy UI benefits for some period, without working, and then look for a new job. Alternatively, in countries with sizable informal labor markets, the same person may prefer to move into a informal job and enjoy UI benefits at the same time. Of course, the size and likelihood of both effects should be directly related to the generosity of the UI system.

As regards the limited number of studies already showing positive effects of UI on unemployment inflow, the unresolved question seems to be through which mechanism this effect works. Therefore, the research questions analyzed in this paper are the following: Through what channel(s) unemployment benefits affect job destruction? Does UI affect work effort? The answer to such questions can unveil distortionary effects about which we know little and, therefore, may be relevant for the optimal design of UI. More specifically, if the spike in job hazard rates is purely a result of firms' action in marginally delaying layoffs, the remedy for it may involve measures which target the firm. For example, making firms internalize the cost that they impose on the UI system when workers are laid-off (experience rating). On the other hand, if the spike is a result of decreased work effort caused by the benefits, the remedy may be one targeting workers. For example, it could involve longer waiting periods for receiving unemployment benefits. Furthermore, the optimal benefit level could change once this distortion on the incentives to work hard is taken into account. It is also worth noticing that fully assessing the role of the demand side explanation goes beyond the scope of this essay.

This chapter is organized as follows. In Section 2, I provide a brief review of the UI literature and argue that better understanding the channels through which UI affects unemployment inflow is relevant for the optimal design of such programs. Section 3 provides a description of the Brazilian UI system and the relevant features of its labor market. In Section 4, the data is presented and evidence on a causal relationship between unemployment insurance and unemployment inflow is provided. In section 5, a model to study this effect is developed. In Section 6, I test the model's prediction of lower work effort after UI eligibility. Finally, section 6 concludes with policy implications.

## 5.2 Related Literature

Unemployment Insurance has received much attention from labor economists in the last decades, since then a large body of theoretical and empirical research on the topic has been developed. I very briefly summarize its main findings as follows, for a detailed review see Tatsiramos and Van Ours (2014). Firstly, there is wide agreement that the main distortion caused by UI schemes is the increase it causes on the average duration of unemployment. The two main channels for this effect are through potential higher reservation wages and reduced incentives to put effort in the search for a new job.

There is plenty of empirical evidence supporting this theoretical prediction with data from different countries and showing that the benefit level and maximum duration positively affect unemployment duration. For example, see Meyer (1990), Katz and Meyer (1990), Lalive and Zweimüller (2004), and Lalive et al. (2006). Nevertheless, part of these responses are actually due to liquidity effects, as noticed by the well-known work of Chetty (2008).

Secondly, more recent studies give attention to the potential positive effects on post-unemployment outcomes. The empirical evidence in this case is rather mixed. For example, while Belzil (2001) and Centeno (2004) find that higher benefit level increases the duration of subsequent employment, Card et al. (2007) finds no effect either in subsequent employment duration or wages.

Finally, few studies deal with the issue that the insurance may also positively affect unemployment inflow. Using Canadian survey data, Christofides and McKenna (1995) find large and significant spikes of employment hazard once workers become eligible for benefits. Green and Sargent (1998), also using survey data from Canada, find substantial tailoring of job durations only for seasonal employees and argue that, after all, the magnitude of the effects are small. Winter-Ebmer (2003) takes advantage of a policy change extending benefit duration for older workers in Austria and finds positive effects on unemployment entry. He argues that this effect can be best understood as a demand-side phenomenon in which firms try to get rid of expensive high-tenured workers. Lalive et al. (2011) takes advantage of the same policy change studied by Winter-Ebmer (2003) to estimate the causal effect of benefit duration on unemployment rate. He finds that the extension of benefits duration led to higher unemployment rate, and interestingly that most of this increase was due to higher exit rates from employment rather than lower exit rates from unemployment. Rebollo-Sanz (2012), using Spanish data, finds that UI appears to favor job turnover both through unemployment inflow and outflow, and that this seems to be a result of both firms' and workers' decisions.

This paper ought to contribute to this literature by: (i) extending the rather limited empirical evidence on how UI affects unemployment inflow and, to the best of my knowledge, being the first to explore this issue with data from a developing country; (ii) studying the channels through which this effect works and making an explicit link with work effort supported by personnel data; (iii) providing a formal model which is able to explain this effect and the lay-off hazard throughout the whole duration of a employment relationship.

## 5.3 Institutional Background

### 5.3.1 The Brazilian Unemployment Insurance System

The Brazilian Unemployment Insurance program dates back to 1986, it was introduced as a law in January 1990. In 1994, a new law changed the entrance requirements and the criteria for establishing benefits duration to the way it is today. The program is designed in such a way that it covers the whole formal sector of the labor market by providing from three to five months of benefits to laid-off workers who meet the minimum eligibility requirement and apply for the benefits within 7 to 120 days after dismissal. Such minimum conditions to access benefits require workers to: (i) have been employed in all the 6 months prior to the lay-off; (ii) have no other source of income; and (iii) have not already enjoyed the benefit in the last 16 months, counted from the date of the previous lay-off which granted him with the previous insurance (if that is the case). It is also important to notice that the benefit is exclusively conceded to employees dismissed by their employer without just cause and is suspended as soon as the worker finds a new job. Furthermore, even though the second requirement for eligibility refers to any other source of income, in practical terms it requires that applicants have no other formal employment relationship aside from the one which has been terminated. This happens because in order to assess whether the worker has other sources of income, the public authorities rely exclusively on a monthly mandatory report filled by firms (CAGED). This report allows the authorities to identify whether a worker has additional employment relationships. It is also worthy to notice that this monthly report covers only about 85 % of all new hirings and terminations of the formal sector, since firms often fill the report with some delay (Gerard and Gonzaga, 2011). This causes imperfect monitoring on insured workers who find new jobs before benefit exhaustion.

Now turning to the set of criteria defining the duration of benefits, it is based on the number of months that the applicant has worked in the last 36 months, considering all together the length of all his employment relationships in the formal sector. The maximum duration of benefits are the following:

| Months worked in the last 36 months | Months of Benefit |
| --- | --- |
| from 6 to 11 | 3 |
| from 12 to 23 | 4 |
| More or equal 24 | 5 |

To what refers to the amount of the benefit, it is a function of a reference wage, which is calculated as the average monthly earnings the worker received in the last three months prior to the lay-off. If this reference wage is up to R$841 (about €240) the monthly amount of benefits equals 80% of this value. For higher level of wages, there is an additional increase in benefit level which is calculated by multiplying the amount exceeding the previous threshold by 50%, but in such a way that benefits are capped at R$954 (about €272). On the top of that, the benefit can never be lower than the minimum wage (R$ 510 or €145), which implies that eligible workers to the benefits earning minimum wages are granted with benefits that equals their previous level of earnings. All these values reflect the 2010 schedule which updated on a yearly basis, but remains relatively stable in real terms. Figure 5.1 shows how the replacement ratio varies with the reference wage.

Finally, the program is funded by a flat tax of 0.65% on firm's gross revenues. An important feature is therefore that there is no experience rating. This means that firms' contributions to the system bear no relationship to how much their activities effectively cost to the unemployment insurance system.

### 5.3.2 Job Protection Institutions in Brazil

Now that the Brazilian UI system has been introduced, it is important to characterize the role of the job protection institutions in Brazil. Overall, firms are free to dismiss workers at will without providing a just cause. However, two peculiar features particularly increase the cost of lay-offs without just cause relatively to other types of job termination (lay-off with just cause and quit), and are especially relevant for job security. Before turning to them, I notice that the first three months of employment relationship in Brazil are considered probation period and, therefore, none of these two mechanisms apply previously to the beginning of the fourth month. Hence, during the probation period, the cost of termination of an employment contract is the same, regardless the cause (quit or lay-off, with or without just cause).

Turning to the mechanisms posing barriers to firings without cause, the first relates to the rule which determines that each worker must have a mandatory savings account at the expense of the firm. On a monthly basis, employers must contribute to this account with an amount that equals 8.5% of the worker's monthly gross wage. These funds can only be used when the employee is laid-off without just cause or in other very exceptional cases (e.g. to finance the acquisition of the worker's first real estate). In any other type of job separation (lay-off with just cause or quit) the worker cannot immediately access these funds. In such cases, he will be able to access the funds in an eventual future lay-off without just cause or after three years of waiting period. To what concerns firing costs, the important feature related to this savings account is a law granting workers a severance payment in the case of dismissal without just cause. In such cases, the employer must pay a severance amount equivalent to 40% of the savings account balance to the worker and an extra sum equivalent to 10% of the balance to the government.

The other mechanism protecting employees from being dismissed without cause requires that employers notify the lay-off within at least one month in advance (one month plus 3 days for each year of tenure exceeding the first one). From the moment the worker is notified, he has the right to choose either to decrease his daily workload by two hours or to work one week less at the end of the last month in the job, without any earnings decrease in both cases. Practically, it represents an extra burden on the employer since workers, after notified about the termination, have very little incentives to put effort in their activities. In fact, it is often the case that firms opt to fully pay the last month of contract without requiring the dismissed employee to work.

Finally, a last important characteristic of job protection in Brazil regards the reasons which can lead to a lay-off with just cause. The latter are rare compared to those dismissals without just cause, in our dataset the proportion is of about 1 to 20. In general, workers can only be fired on cause if they: (a) are continuously absent from work (usually more than 30 days) ; (b) commit serious misconduct ; (c) go to work under the effect of alcohol; or (d) commit a large number of small infractions. Furthermore, the firm must be able to provide clear evidence of any of these behaviors. In practice, since these conditions are extreme and very often hard to prove, firings with cause are rare in Brazil. Nevertheless, when a dismissal with just cause takes place, the worker loses the right of claiming unemployment benefits and is not entitled to the severance payment corresponding to 40% of his savings account balance.

## 5.4 The Data and The Discontinuity of Hazard Rates at MER

### 5.4.1 Data

The empirical analysis presented here is based on the RAIS (Relação Anual de Informações Sociais), a dataset provided by the Brazilian Ministry of Labor, for the years of 2010 and 2011. This is an administrative dataset containing the whole universe of employment relationships in the formal labor market. It provides a number of variables about each job such as: tenure, last wage, yearly average wage, tenure, occupation, sector, type of contract, reason of termination (if terminated at all). Furthermore, it is possible to know the age and schooling level of workers and firm size. Although it is a rich dataset containing information from the whole population of formally employed workers in Brazil, there is unfortunately a main feature which limits our analysis to some extent. It is not possible to follow the same worker over different jobs. This restriction, for instance, does not allow us to know for how long a worker remained unemployed between one job and another.

### 5.4.2 Identification Strategy

Ideally, to assess the effect of UI on job destruction, one would like to observe and compare the employment duration of workers with identical characteristics who are and who are not entitled to unemployment insurance at any given tenure. This would allow us to identify the causal effect of the insurance on the employment hazard during the whole path of tenure. Since our dataset limitations restrict our set of alternatives to estimate something in line with an ideal experiment, I use the following identification strategy to assess whether UI has or not a causal effect on job termination.

First, I estimate how the risk of a lay-off evolve over worker's tenure, aggregating the data on tenure in periods of 15 days (the empirical lay-off hazard rate over tenure). Notice that only dismissals without cause are considered here since only these allow workers to be granted with UI. Then, I test for a discontinuity in the hazard rate at the minimum tenure threshold required for eligibility to UI (6 months in our case). The statistically significant discontinuity then shows a causal effect of UI on the lay-off hazard rate at the minimum eligibility requirement for UI. The needed hypothesis for this regression discontinuity design to work is that the hazard rate is a continuous function with respect to tenure. The underlying idea is that there is no reason to the expect the hazard rate to be a discontinuous function of tenure. In fact, from the graph of the next subsection it is clear that the empirical job hazard rate is indeed a continuous function of tenure at any other level of tenure that is not directly influenced by the legislation.

### 5.4.3 Empirical Results

Figure 5.2 shows the empirical lay-off hazard rate according to tenure. In other words, it measures the probability of a termination by a lay-off (without just cause) for a given tenure, provided that the job was not terminated up to that point. From eye inspection, it is possible to see three points where the hazard rate takes a discrete jump. The first one, at three months, is the end of the probation period. The second one, at twelve months, is due to a law which increases the administrative cost of the dismissal procedure of workers with more than one year in the job. The last one, at six months, is the minimum eligibility required for unemployment insurance. Once this threshold is reached, the risk of a termination due to a lay-off without just cause discretely increases.

Before discussing this result from the graphical inspection, I provide a formal test to assess whether this is a statically significant discontinuity. Following a standard procedure from a sharp Regression Discontinuity Design, I run a linear regression with a quadratic polynomial as a function of tenure, before and after the discontinuity threshold (6 months) and, on top of that, add a dummy variable taking value one when tenure is greater than the threshold to test for the discontinuity.

Figure 5.3 shows the regression fit before and after the threshold. The regression results presented in Table 5.1 show that the dummy variable ("ui") is highly significant (p-value$\leq$0.01). This means that there is support from the data that the UI system indeed affect unemployment inflow. Although it allows us to argue that benefits eligibility indeed causes a rupture in the lay-off hazard rate, we should be careful not to wrongly extrapolate this result. It unfortunately does not allow us to derive the conclusion that the overall effect of the policy decreases job duration. The need for caution comes from the fact that our quasi-experimental design does not tell us anything about the counterfactual of the hazard rate at tenure levels distant from the threshold for eligibility. For example, it may well be that workers, while they have not yet reached eligibility, increase effort in order to make sure they will not be laid-off before becoming eligible for UI. Therefore, it is not possible to evaluate the effect of unemployment benefits eligibility at different points of tenure, but only on the hazard rate at the point in which workers reach MER.

### 5.4.4 Further Empirical Results

At this point, I report further empirical results with two main purposes. First, the aim of this subsection is to increase the robustness of the analysis just presented. Second, it should help clarifying for which subsample of the population this effect is stronger. The goal of this exercise is to provide insight on which are the relevant mechanisms behind the results of the previous subsection.

I begin by reporting how the hazard of other types of job termination behaves throughout tenure. More specifically, one would like to see how the risk of a lay-off or a quit evolves along the employment relationship. In principle, one should not expect any atypical behavior around the minimum eligibility requirements for UI, since these other forms of job termination never grant workers with unemployment benefits. Results are reported in Figure 5.4 and show that both types of hazard rates indeed evolve smoothly around MER for UI. Therefore, these graphs reinforce the hypothesis that the discontinuity found for lay-offs without cause are indeed caused by the eligibility for UI.

Now I turn to the question of whether the discontinuity found at MER depends on the generosity of benefits. In order to do so, I create two subsamples of High and Low wages. The former contains only workers with a monthly wage greater than R$4,000 (above €1142) while the latter consists of workers with wages below R$900 (below €330), close to minimum wages. The reason for such analysis is that low wage workers enjoy the highest replacement ratio while high wage workers have very low benefits. In other words, as a proportion of their salaries, the level of UI benefits for those workers earning close to minimum wages is significantly greater with respect to high wage workers. By observing how hazard rates evolve for these two groups, I assess how the level of the replacement ratio affects the size of the discontinuity.

In Figure 5.5, it is possible to observe that while the discontinuity for the low wage group is clearly sizable, the discontinuity for the high wage group seems to be rather small, if not inexistent. This follows with the work effort explanation: one would indeed expect that workers with a low value job would be those more strongly affected by UI eligibility.

Finally, I turn to the analysis of how the size of the discontinuity differs across firms of different sizes. It may help assessing the plausibility of the explanation along the line that unions exert pressures on firms against the lay-off of non-eligible workers. Departing from the hypothesis that larger firms have to deal with stronger union, I compute lay-off hazard rates for a sample of workers employed in small (bellow 50 workers) and large firms (above 500 workers). As it is possible to see in Figure 5.6, there does not to seem to be any differences in the relative size of the discontinuity between the small and large companies subsample. By noticing that hazard rates levels are significantly smaller for large firms, it turns out that the absolute size of the discontinuity is actually higher for small firms. Therefore, this simple analysis provides evidence against the "union" explanation, since one would expect that stronger unions would cause the discontinuity to be stronger in larger firms. However, it is worth to notice that this result does not contradict other possible channels for the broader demand side explanation.

In sum, I have shown so far that: UI eligibility threshold locally increases lay-off hazard rates without just cause; that the hazard of other types of job termination are not affected; and that the size of the effect is significantly greater for low wage workers. In the next section, I build a model with the purpose of showing that the pattern found in the data can be explained by the work effort explanation.

## 5.5 The Model

In this section, a model based on work effort is provided in order to explain the spike in the lay-off hazard found in the previous section. There are two relevant aspects that I want to explain with this model: (i) the discontinuity of the hazard rate at the unemployment benefits MER; and (ii) the hump shaped pattern found in the hazard rate over time. The intuition is that the first element is explained by the negative effect of unemployment insurance on work effort, leading to a higher probability of lay-off. The second element, instead, seems to be related to a learning process which takes place during the employment relationship. At the beginning, neither the worker nor the employer know with precision the quality of the match. The worker productivity in the job is revealed for both the firm and the worker throughout time. Even though applied to a very different context, the learning process present in the model is inspired in the original model of career concerns (Holmstrom 1999). It works as follows.

Firms hire workers without knowing their precise ability for carrying out the tasks of a given job. The output produced by a given worker in each period is given by $Y_t = \eta - k + e_t + \epsilon_t$. $\eta$ denotes worker's ability of carrying out the tasks of a given job; $e_t$ and $\epsilon_t$ refer to work effort and a noise in each period t, respectively. $k$ represents a fix cost of keeping a worker which is not related to wages. This can be interpreted as an administrative cost. In the absence of work effort, a match is profitable only if $\eta - k > 0$. Both the firm and the worker share the same belief about the distribution of ability in the population and the distribution of the errors:

$$Y_t = \eta - k + e_t + \epsilon_t \tag{5.1}$$

$$\eta \sim N(m_0, 1/h_0) \tag{5.2}$$

$$\epsilon_t \sim N(0, 1/h_\epsilon) \tag{5.3}$$

Differently from a model of career concerns, the level of effort provided by workers in each period is assumed to be observed by the firm. Wages are flexible and reset at the beginning of each period. At the beginning of each new period, the firm decides whether or not to fire the worker. In case it keeps the worker, it offers a new wage which equals expected output in the subsequent period since the market is competitive. Because wages are paid at the beginning of each period and firms can freely lay-off, workers have no incentives to put effort in the absence of further constraints. For this reason, firms commit to fire workers with some probability $p(e_t)$ at the end of each period, regardless on whether the worker's next period expected output is greater than zero. This characterizes a second channel through which jobs are terminated in this model. The function $p(e_t)$ obviously depends negatively on $e_t$, and is also assumed to be convex and twice differentiable ($p'(e_t) < 0$ and $p''(e_t) > 0$).

As regards the learning process, at the end of each period, both firms and workers observe a signal $s_t = y_t + k - e_t = \eta + \epsilon_t$ and, through a bayesian learning process, update their beliefs about the worker's ability $\eta$. At the end of each period, given the updated beliefs $m_t$ about the expected ability of each worker, the firm offers next period wages $w_{t+1} = E(Y_{t+1}|s_t) = E(\eta|s_t) - k + e_t^* = m_t - k + e_{t+1}^*$. Workers are fired whenever their expected output falls below zero. Notice that since there is no asymmetric information or hidden action, firms perfectly anticipates the level of effort provided by workers.[1] The competitive market ensures that the anticipated level of effort that will be provided by workers is incorporated into wages.

[1] Changing these assumptions on asymmetric information and/or hidden action would probably not add anything relevant to the story and, at the same, they would make the model untractable.

The learning process works exactly as in Holmström (1999), except for the fact that the level of effort is directly observed by the firm. Thus, the firm does not need to infer it in order to update its beliefs on workers' ability:

$$E(\eta|s_1) = m_1 = \frac{h_0 m_0 + h_\epsilon s_1}{h_0 + h_\epsilon} \tag{5.4}$$

$$E(\eta|s_t) = m_{t+1} = \frac{h_0 m_0 + h_\epsilon \sum_{t=1}^{t} s_t}{h_t + h_\epsilon} \tag{5.5}$$

The precision of the estimate of ability $h_t$ increases with time and is given by:

$$h_{t+1} = h_t + h_\epsilon = h_0 + t h_\epsilon \tag{5.6}$$

Therefore, as the duration of the employment relationship increases, both the firm and the worker increase their precision on the estimation of ability. The worker's recursive problem at each period is given by:

$$Max_{e_t} V_t^E = w_t - c(e_t) + \beta E\{(1 - p(e_t))V_{t+1}^E + p(e_t)V_{t+1}^U\} \tag{5.7}$$

$c(e_t)$,$V_t^E$ and $V_t^U$ are the cost of effort, the value of employment and unemployment at period t, respectively. The model is in infinite time. Notice that since workers have no ability to interfere on the learning process on their ability, because effort is not private information, the only benefit from choosing a higher level of effort is decreasing the probability of being fired. First-order conditions are:

$$c'(e_t^*) = -\beta p'(e_t^*))E\{V_{t+1}^E - V_{t+1}^U\} \tag{5.8}$$

Summarizing, the model works through the following steps:

1. Firms hire a population of workers at a flexible wage and are constrained by the market to pay for the expected output in each period.

2. Workers choose an strictly positive effort level due to the threat of dismissal. Output is then realized.

3. Firms use the signal contained in the previous period output and use it to update expectations on workers' abilities and next period output. Firms then fire workers through two distinct channels: (i) with probability $p(e_t)$ (notice that firms can observe the realized effort level), since they commit to it in order to promote effort, and (ii) when expected output is negative due to low updated beliefs on ability.

I notice that, in the model, when there is no UI at all ("No-UI" regime), the level of effort provided by each worker, for a given expectation on ability, is constant over time. The reason for that is that the worker's problem at each period is perfectly replicated and, thus, we have a recursive problem. The same applies for the UI regime after MER are met in the sixth month of employment (calibrating the model to the Brazilian case explored in the data). Effort becomes constant over time because the problem turns out to be recursive. However, in this case, the choice of effort is lower with respect to the "No-UI" regime since the outside option is worth more once workers becomes eligible for benefits.

Interestingly, before MER are met, the optimal level of effort is actually higher than under the "No-UI" regime because, even though next period $V^U$ is the same as in the no UI regime, the next period value of employment embodies the future increased value of unemployment - with UI. In other words, with respect to the "No UI" regime, workers actually provide a higher level of effort before they reach MER in order to increase their odds of becoming eligible for UI in the future. Therefore, the minimum eligible requirement constitutes a incentive for workers starting a new job to provide higher work effort with the goal of increasing the likelihood of being eligible for unemployment benefits.

Figures 5.7 and 5.8 present a Monte Carlo simulation of the model under the "No-UI" and UI regime, respectively. A Monte Carlo procedure replicates one million employment relationships within the model for each regime, and lay-off hazard rates are calculated. The goal is to understand which kind patterns of lay-off hazard rates would emerge if the world were like the model. The UI model closely replicates the pattern found in the data and shows that the "work effort" explanation for the spike in the hazard rate is able to explain the data. It suggests that indeed once UI benefits become available workers' effort level decreases, causing an increase in the number of dismissals. Predictions on whether UI actually decreases the duration of employment are not so clear and depend on the model's parameters. Such effect on the average duration of employment is not clear cut precisely because UI actually increase work effort at the beginning of the employment relationship.

In sum, this subsection presented a learning model with work effort which is able to explain the pattern found in the data. It shows that the "work effort" explanation is consistent with the data. In order to further test our theory, I use personnel data on absenteeism as a proxy of work effort and evaluate whether workers actually decrease their level of effort once they reach MER.

## 5.6 Personnel Data on Absenteeism

By using personnel data on absenteeism I evaluate the hypothesis that UI eligibility undermines work effort. The analysis is based on weekly observations on absenteeism due to "illness" for 653 workers from 2009 to 2012. Data was provided by a Brazilian Car Dealer firm employing around 400 workers. The analysis is performed separately by gender since the literature on absenteeism supports that it is a better proxy for male work effort with respect to female effort (Ichino and Moretti, 2009). Figures 5.9 and 5.10 show how monthly absenteeism evolves for male and female respectively.

From eye inspection, it is possible to check that male absenteeism peaks exactly at the sixth month, coinciding with MER for UI benefits. It thus seems to support the negative relationship between UI eligibility and the choice of effort, as predicted by the model. For females, the relationship is less clear. Absenteeism increases until the fifth month and then seems to remain relatively constant.

To better assess whether the increase of absenteeism right after MER is in fact a robust result, I run a fixed-effects regression controlling for time trends with polynomials of first and second degree. Table 5.2 and 5.3 report the results for male and female workers. The analysis suggests that UI eligibility have indeed a statistically significant effect on male absenteeism, while it is insignificant for female as suggested by pure eye inspection. The fact the female absenteeism does not significantly increase after MER should not be a reason for skepticism since there exists robust evidence showing that female absenteeism is not a very precise proxy for work effort. Overall, these results support the model prediction of lower work effort after UI eligibility.

## 5.7 Discussion and Policy Implications

This paper employs a Regression Discontinuity Design to show the existence of a positive causal effect of UI on lay-off hazard rates at MER. Then, it provides evidence supporting that this effects is driven by a decrease in the level of work effort, even though it is not possible to rule out that firm's decision may also play a role. Therefore, this work supports the hypothesis that the discontinuity in hazard rates at MER is, at least partially, a labor supply phenomenon driven by shirking decisions. The purpose of this section is to discuss the implications of these findings for policy.

The first remark is that the extensive literature assessing welfare effects of UI schemes does not take into account the UI distortion on work effort supported by this paper (see the seminal paper by Chetty (2008) for instance). Therefore, if it is the case that policy makers have so far set the generosity level of UI schemes disregarding the distortion on work effort, the straightforward implication is that UI generosity should be at least marginally decreased.

Nevertheless, it is worth noting that the evidence presented in this work concerns only the case of Brazil and that one should be cautious in extending these implications to other countries. However, it is also worth to note that previous research have also reported spikes in hazard rates at MER for UI in developed countries. Christofides and McKenna (1995) and Rebollo-Sanz (2012) report similar effects of MER on job termination rates for Spain and Canada. Furthermore, Lalive et al. (2011) support that an extension of benefits duration increased equilibrium unemployment in Austria mostly through higher employment hazard rates. Hence, there is evidence from other countries suggesting that UI does affect job destruction and that the size of this effect is not negligible. Therefore, the explanation for such distortionary effect provided in this paper might be also helpful for policy in other countries.

The second remark concerns the measures which could be taken by policy makers to mitigate such distortion. For such assessment, the key insight provided by this paper is that the discontinuity in the employment hazard rate is at least partially driven by the workers' decision. Such insight has implications for the assessment of the policy features which may be best suitable for mitigating this distortion.

Suppose that the discontinuity in the hazard rate was merely a result of firm's decision as consequence of the hypothesis that it becomes less expensive to firms to lay-off workers eligible for UI. It would imply that the reason for such distortion is that firms do not internalize the full costs of UI and, instead, enjoy some benefit from laying-off insured workers. Therefore, if the policy maker would transfer the full cost of UI to the firm according to the number of workers the firm dismisses (full experience rating), such distortion would no longer exist.

On the other hand, if it is the case that the spike in the hazard rate is driven by the worker's decision of decreasing work effort after UI eligibility binds, such measure of fully transferring the cost of UI to firms would not restore efficiency. Workers would still socially inefficiently decrease work effort as a result of a moral hazard problem and lay-off hazard rates would still spike at MER, as shown by the model of Section 3.

In this case, a more effective way of addressing this problem would be targeting policy measures at the worker. One possible way is strengthening UI recipients requirements to prove that they are actively searching for a job, since it would decrease worker's incentives to be laid-off and enjoy UI benefits. Such measures already exists in most countries in order to mitigate the moral hazard problem on search effort. However, in the light of this paper's finding, these requirements would serve a second purpose of turning unemployment insurance less attractive. Especially for the Brazilian case where there exists a sizable informal market, stricter requirements of search would also make it harder for UI recipients to be employed in the informal market while receiving UI benefits.

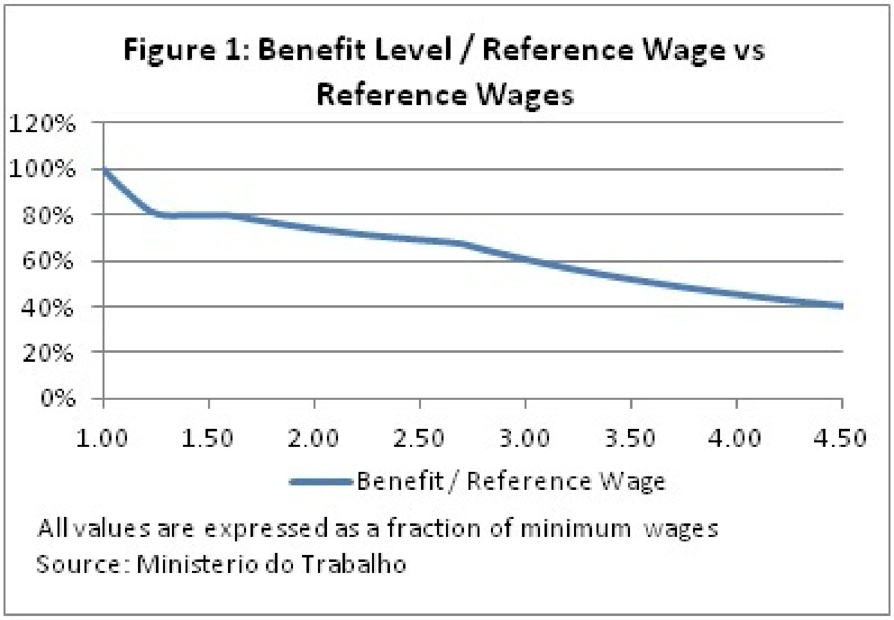
Another possible measure in a similar spirit is being implemented in Brazil. In the beginning of 2013 the government introduced a rule requiring that workers receiving unemployment insurance for the second time, or more, in the last ten years are required to undertake compulsory training while enjoying benefits. Such training program demands four hours in every week day. The idea is that these workers use the leisure time from unemployment to develop skills and that it makes unemployment less attractive as UI recipients can enjoy less time with leisure. Paradoxically, an interesting concern regarding this policy is that, if the training program turns out to provide high enough returns for unemployed workers, it might be the case that unemployment actually becomes more attractive. The success of such policy in mitigating the distortion reported in this work is still to be assessed and is a interesting avenue for future research.

To conclude, this paper suggests that policy should take into account the negative effect of UI on work effort. If it is the case that current policy so far neglects such distortion, as it is the case in the UI literature, the immediate policy implication is that UI generosity should be at least marginally diminished. Furthermore, the evidence provided in this paper linking UI to work effort suggests that policy measures to mitigate this distortion should be target at the worker and that making that firms fully internalize the cost of UI should not be enough.

## 5.8 Conclusion

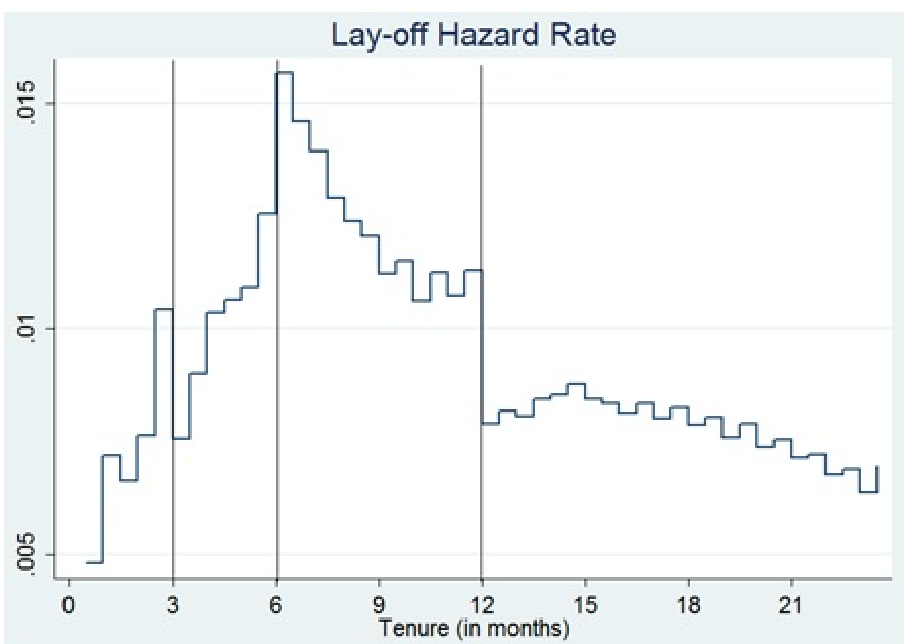
This work provides evidence that UI has distortionary effects on work effort, a fact that has been given little attention by the literature. It suggests that this effect should not be neglected by policy makers when designing such program. The paper further extends the evidence of the effects of UI eligibility on employment hazard rates and provides a robust explanation linking this evidence to work effort. The model of learning with work effort is shown to be able to explain the evolution of hazard rates throughout the employment relationship and the discontinuity found in the data. In order to directly test the prediction of lower work effort after UI eligibility, personnel data on absenteeism is presented and used as proxy of work effort. Our regression analysis shows that indeed absenteeism significantly increases after UI eligibility.

Figure 5.1: Replacement Ratio



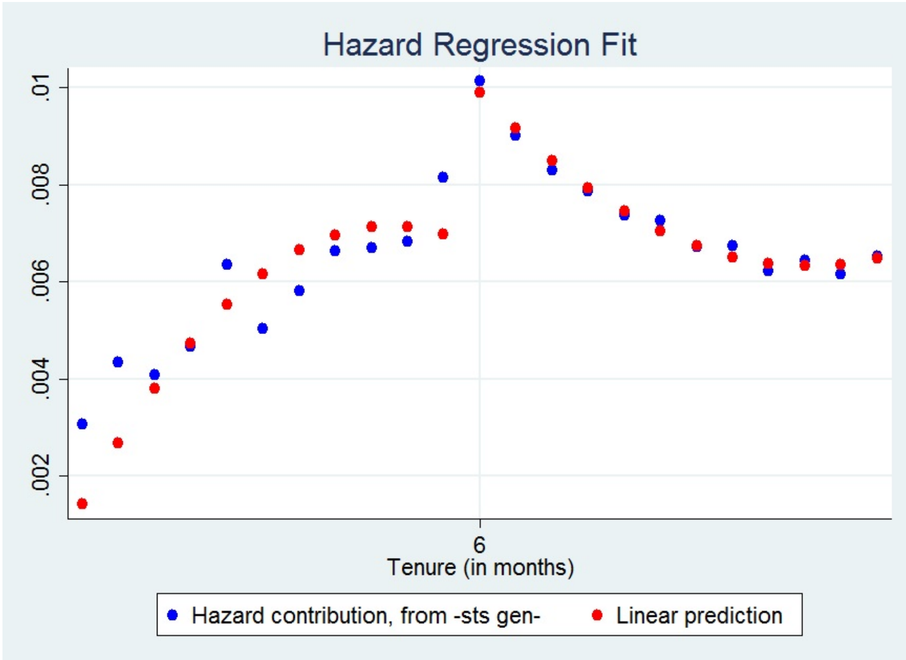

Reference wages are expressed in terms of minimum wages.

Figure 5.2



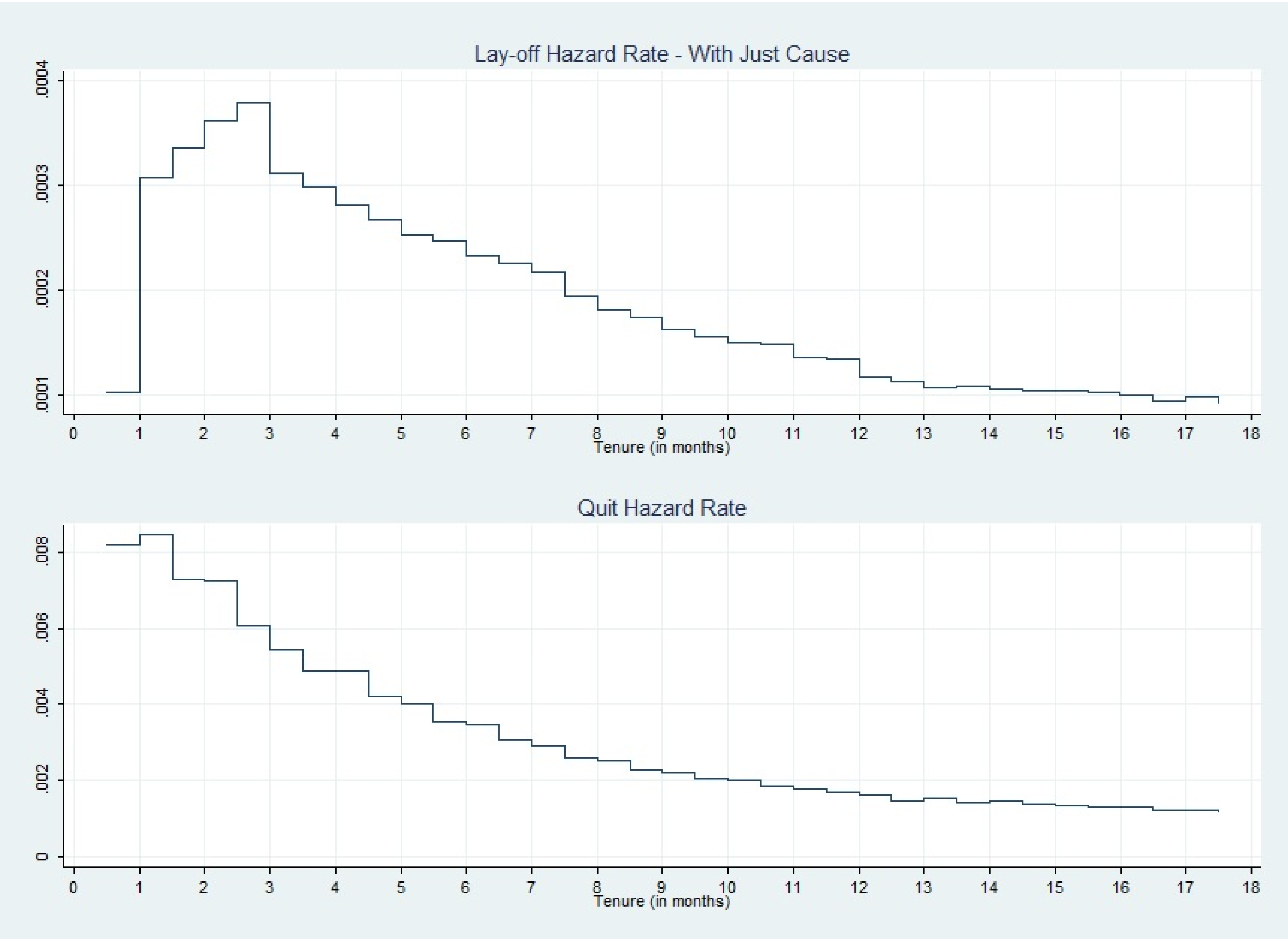

The graph displays the empirical lay-off hazard rate on the whole sample. It considers only lay-offs without a just cause.

Figure 5.3



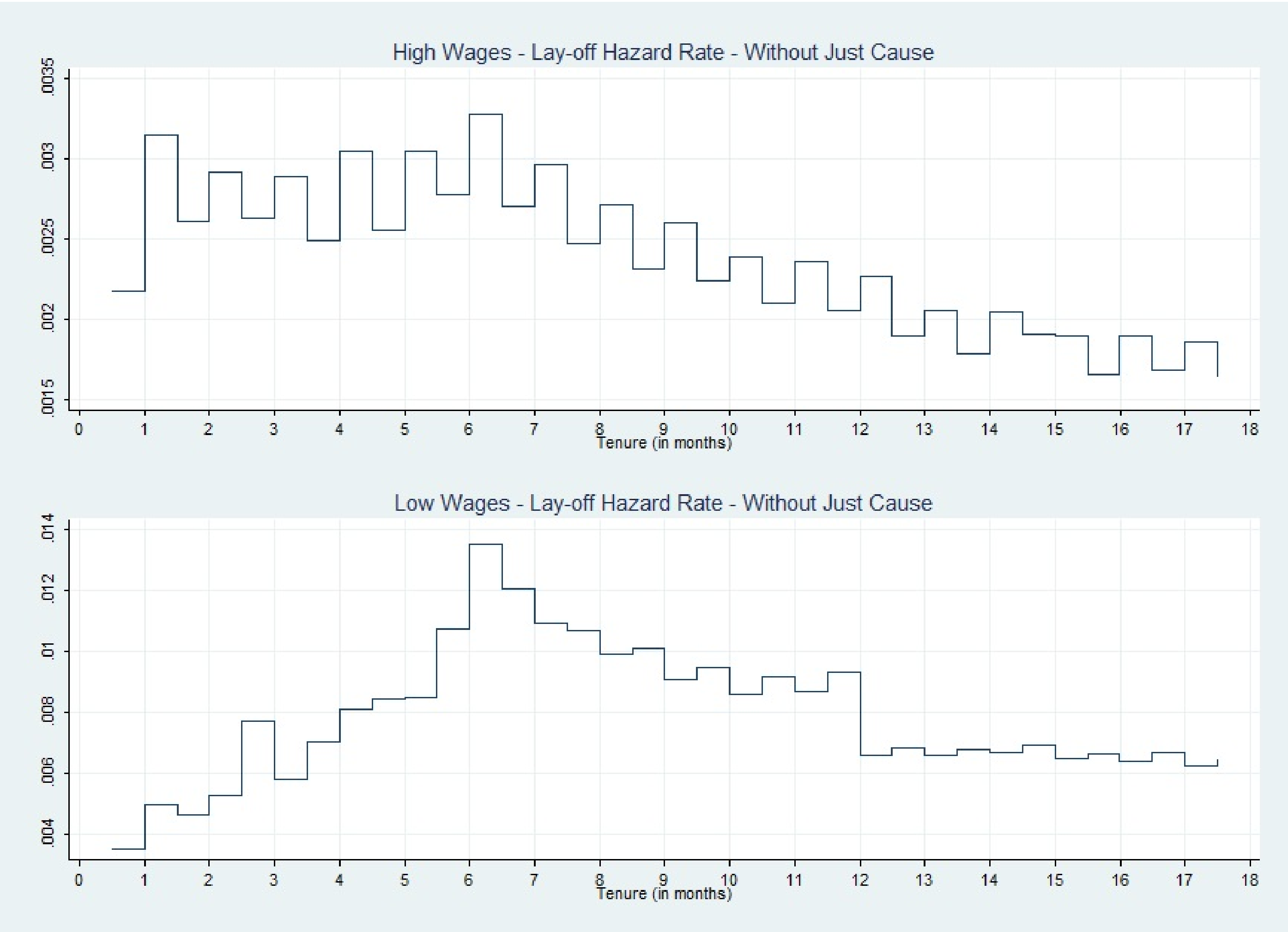

The graph displays the empirical (blue dots) and estimated fit (red dots) of the lay-off hazard rate on the whole sample. It considers only lay-offs without a just cause.

Figure 5.4



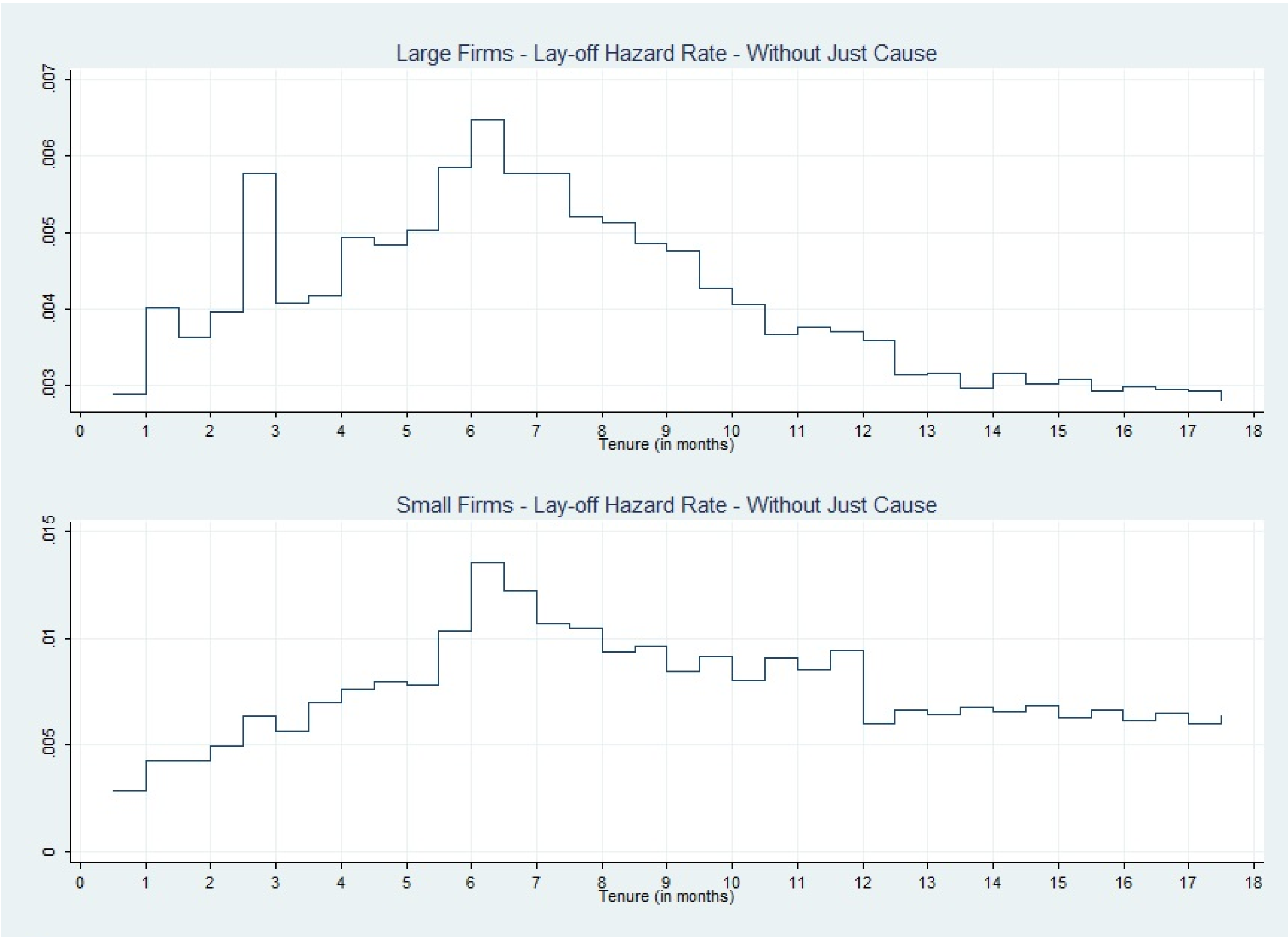

The graphs display the empirical lay-off and quit hazard rates on the whole sample.

Figure 5.5



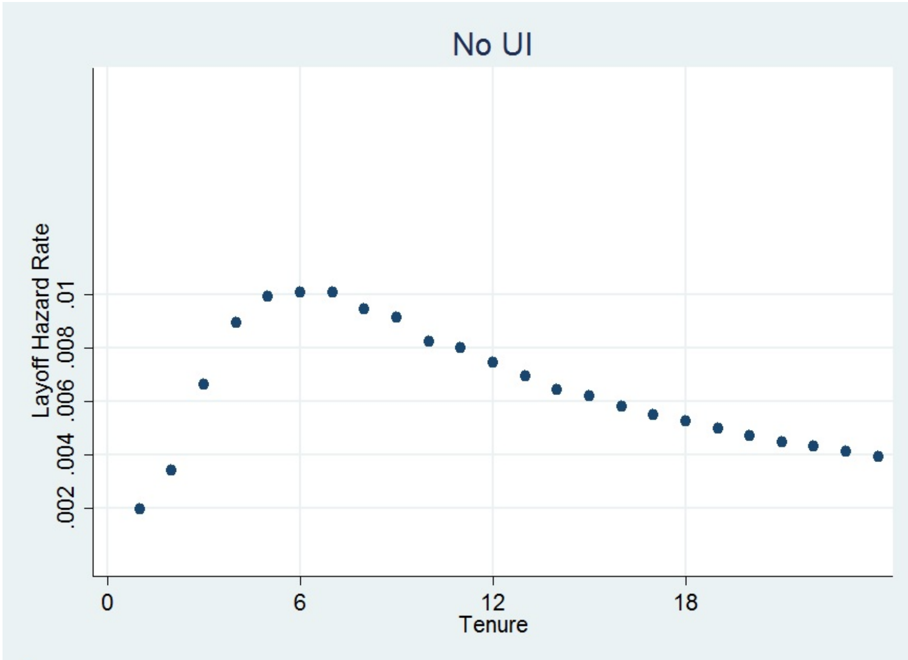

The graph displays the empirical lay-off hazard rate for workers with high (≥R\$4000) and low (≤R\$900) monthly earnings.

Figure 5.6



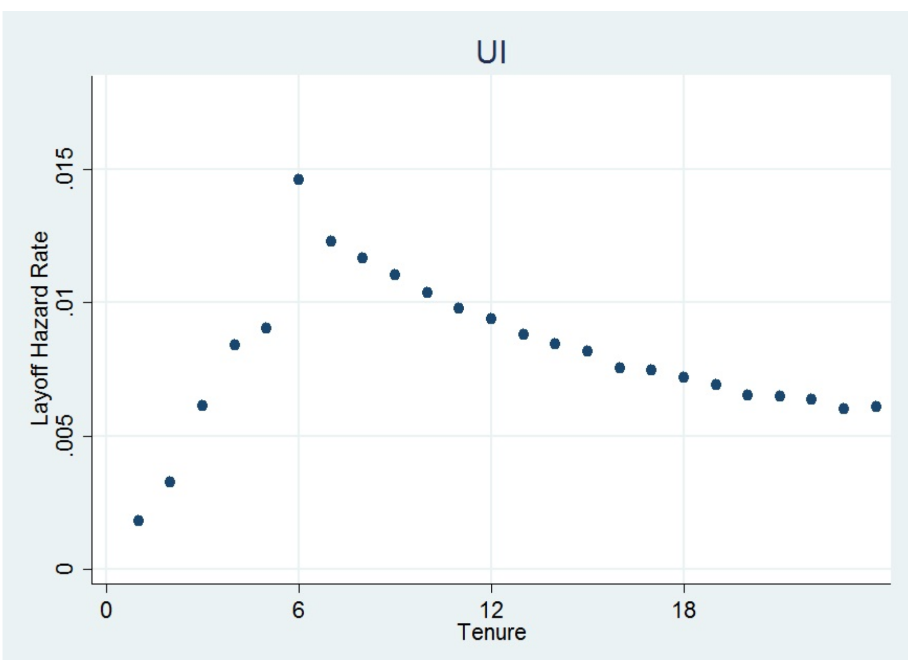

The graph displays the empirical lay-off hazard rate for workers employed in large (≥500 employees) and small (≤R$50) firms.

Figure 5.7: Model Simulation - No Unemployment Insurance



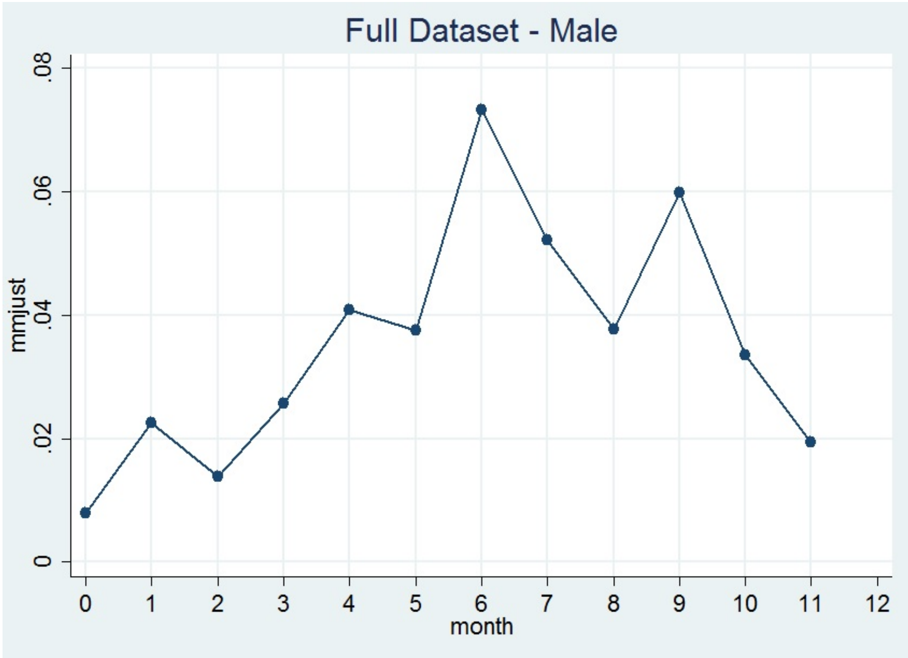

The graph display lay-off hazard rates derived from 1 million simulations of employment histories based on the model developed in the paper, when UI is absent.

Figure 5.8: Model Simulation - With Unemployment Insurance



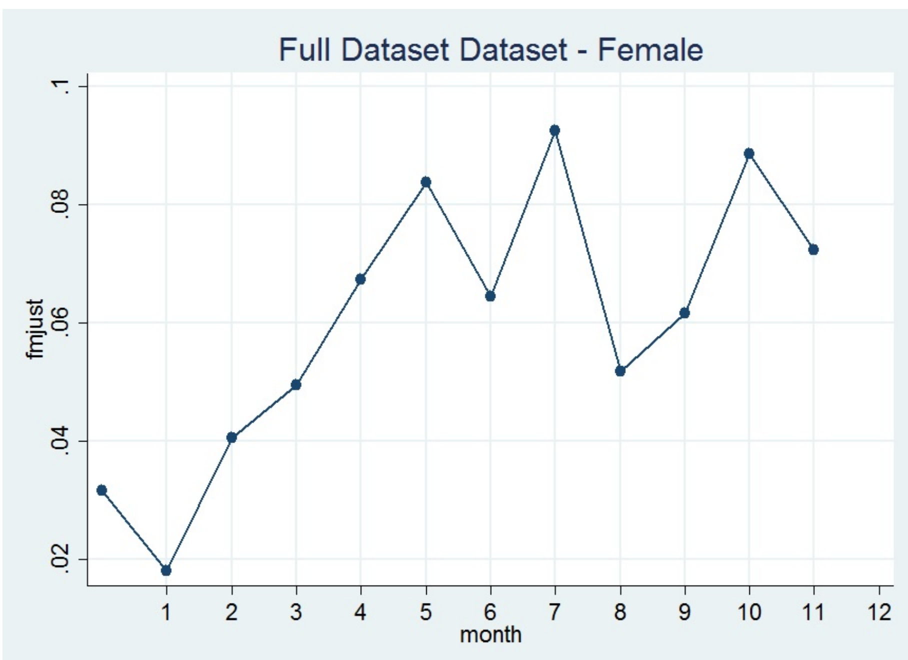

The graph display lay-off hazard rates derived from 1 million simulations of employment histories based on the model developed in the paper, when UI is present.

Figure 5.9




The graph displays the average rate of absenteeism according to tenure based on the male sample of the firm level personeel data.

Figure 5.10




The graph displays the average rate of absenteeism according to tenure based on the female sample of the firm level personeel data.

Table 5.1: Test for Discontinuity in Lay-off Hazard Rate Without Just Cause at MER

| Source | SS | df | MS | | | |
|---|---|---|---|---|---|---|
| | | | | Number of obs | = | 23 |
| | | | | F( 5, 18) | = | 379.75 |
| Model | .00275133 | 5 | .000550266 | Prob > F | = | 0.0000 |
| Residual | .000026082 | 18 | 1.4490e-06 | R-squared | = | 0.9906 |
| | | | | Adj R-squared | = | 0.9880 |
| Total | .002777413 | 23 | .000120757 | Root MSE | = | .0012 |

| deltach | Coef. | Std. Err. | t | P>\|t\| | [95% Conf. | Interval] |
|---|---|---|---|---|---|---|
| ui | .0047849 | .0013043 | 3.67 | 0.002 | .0020446 | .0075253 |
| tenure | .005875 | .000762 | 7.71 | 0.000 | .0042741 | .007476 |
| tenure2 | -.0009907 | .0003474 | -2.85 | 0.011 | -.0017205 | -.000261 |
| tenure3 | .0000513 | .0000491 | 1.04 | 0.310 | -.0000519 | .0001546 |
| tenure4 | -4.76e-07 | 2.23e-06 | -0.21 | 0.833 | -5.16e-06 | 4.20e-06 |

Note: The table displays regression discontinuity results on lay-off hazard rates at MER (6 months). The variable "UI" indicates the size of the estimates discontinuity.

Table 5.2: Fixed-Effects Regression on Absenteeism - Male Workers

Dep Var - Absences/week - Male

| | m1 | m2 | m3 |
|---|---|---|---|
| | b/p | b/p | b/p |
| aprob | 0.022*** | 0.030*** | 0.004 |
| | (0.01) | (0.01) | (0.78) |
| ui | 0.018** | 0.029** | 0.025** |
| | (0.02) | (0.02) | (0.05) |
| t | | -0.001 | 0.003* |
| | | (0.27) | (0.06) |
| t2 | | | -0.000** |
| | | | (0.02) |
| constant | 0.011* | 0.015** | -0.007 |
| | (0.06) | (0.03) | (0.56) |
| R-squared | 0.002 | 0.002 | 0.002 |
| observations | 14602 | 14602 | 14602 |

* p<0.10, ** p<0.05, *** p<0.01

Note: The table displays the results of a fixed effect model on male absenteeism. The variable "aprob" is a dummy taking value 1 when the worker end the probation period (first three months). The variable "UI" is a dummy taking value 1 when the worker reaches MER.

Table 5.3: Fixed-Effects Regression on Absenteeism - Female Workers

Dep Var - Absences/week - Female

| | m1 | m2 | m3 |
|---|---|---|---|
| | b/p | b/p | b/p |
| aprob | 0.039*** | 0.022 | 0.017 |
| | (0.00) | (0.10) | (0.34) |
| ui | 0.013 | -0.011 | -0.012 |
| | (0.18) | (0.47) | (0.46) |
| t | | 0.001* | 0.002 |
| | | (0.05) | (0.29) |
| t2 | | | -0.000 |
| | | | (0.72) |
| constant | 0.025*** | 0.015* | 0.011 |
| | (0.00) | (0.08) | (0.41) |
| R-squared | 0.003 | 0.004 | 0.004 |
| observations | 9128 | 9128 | 9128 |

* p<0.10, ** p<0.05, *** p<0.01

Note: The table displays the results of a fixed effect model on female absenteeism. The variable "aprob" is a dummy taking value 1 when the worker end the probation period (first three months). The variable "UI" is a dummy taking value 1 when the worker reaches MER.

## 5.9 Appendix

### 5.9.1 Model Solution

The worker problem is given by:

$$Max_{e_t} V_t^E = w_t - c(e_t) + E\{\beta[(1-p(e_t))V_{t+1}^E + p(e_t)V_{t+1}^U]\} \tag{5.9}$$

First order conditions are:

$$c'(e_t^*) = -\beta p'(e_t^*))\{V_{t+1}^E - V_{t+1}^U\} \tag{5.10}$$

This implies:

$$V_{t+1}^E = -\frac{c'(e_t^*)}{\beta p'(e_t^*)} + V_{t+1}^U \tag{5.11}$$

Second Order Conditions are satisfied as long as:

$$-c''(e_t^*) - \beta p''(e_t^*)[V_{t+1}^E - V_{t+1}^U] < 0 \tag{5.12}$$

Now, we split the solution in two cases: with and without unemployment insurance.

*No Unemployment Insurance Case*

In this case, we have a recursive problem and, at the optimal path of $e_t$ it holds that: $V_t^E = EV_{t+1}^E, \forall t$.

Therefore:

$$V_t^E = \frac{w_t - c(e_t{}^*) + \beta p(e_t{}^*)V_{t+1}^U}{1-\beta[1-p(e_t{}^*)]} \tag{5.13}$$

From the FOC and the last equation together, we have that:

$$g(e_t^*) = -c'(e_t^*)[1-\beta(1-p(e_t^*))] - \beta p'(e_t^*)[w_t - c(e_t^*) - (1-\beta)V_t^U] = 0 \tag{5.14}$$

This equation characterizes the worker's optimal choice of effort for a given wage. To insure the existence of a solution, we notice two things:

$$g(0) = -\beta p'(0)[w_t - (1-\beta)V_t^U] > 0, \text{ since } V_t^U = 0 \tag{5.15}$$

$$g(e^M) = -c'(e_t^*)[1-\beta(1-p(e_t^*))] < 0, \text{where } e^M \in \{e|[w_t - c(e_t^*) - (1-\beta)V_t^U]\} = 0 \tag{5.16}$$

$$g'(e_t^*) = -c''(e_t^*)[1-\beta(1-p(e_t^*))] - \beta c(e_t^*)p'(e_t^*) - \beta p''(e_t^*)[w_t - c(e_t^*) - (1-\beta)V_t^U] + \beta c'(e_t^*)p'(e_t^*) < 0, \tag{5.17}$$

$$\text{if } e_t^* < e^M \tag{5.18}$$

These conditions guarantee the existence of a unique solution in the interval $(0, e^M)$.

At this point, we turn to the firm which competitively sets wages at the beginning of each period. Therefore, $w_t = E[\eta|Y^{t-1}] + e_t^*$. Plugging the conditions into the solution to the worker's problem we have:

$$g(e_t^*) = -c'(e_t^*)[1-\beta(1-p(e_t^*))] - \beta p'(e_t^*)[E[\eta|Y^{t-1}] + e_t^* - c(e_t^*) - (1-\beta)V_t^U] = 0 \tag{5.19}$$

*Unemployment Insurance Case*

Now notice that since the value of unemployment from the 6th month onwards is constant, we have a perfectly recursive problem from the 6th month of employment. Therefore, for a given wage, the optimal level of effort is constant: $e_6^* = e_7^* = e_8^* = ... =$.

$$V_0^E = w_0 - c(e_0) + E\{\beta[(1-p(e_0))V_1^E - p(e_t)V_1^U]\} \tag{5.20}$$

$$V_1^E = w_1 - c(e_1) + E\{\beta[(1-p(e_1))V_2^E - p(e_t)V_2^U]\} \tag{5.21}$$

$$V_2^E = w_2 - c(e_2) + E\{\beta[(1-p(e_2))V_3^E - p(e_t)V_3^U]\} \tag{5.22}$$

$$V_3^E = w_3 - c(e_3) + E\{\beta[(1-p(e_3))V_4^E - p(e_t)V_4^U]\} \tag{5.23}$$

$$V_4^E = w_4 - c(e_4) + E\{\beta[(1-p(e_4))V_5^E - p(e_t)V_5^U]\} \tag{5.24}$$

$$V_5^E = w_5 - c(e_5) + E\{\beta[(1-p(e_5))V_6^E - p(e_t)V_6^U]\} \tag{5.25}$$

$$V_6^E = w_6 - c(e_6) + E\{\beta[(1-p(e_6))V_7^E - p(e_t)V_7^U]\} \tag{5.26}$$

$$V_6^E = E\{V_7^E\} = E\{V_8^E\} = ... = \tag{5.27}$$

# Chapter 6

# Conclusion

Unemployment insurance is a widespread policy around the world and is often at the core of the policy debate, especially in periods of economic crisis when workers' employment prospects largely deteriorate. It is certainly not by accident that it inspired a wide academic literature over at least the past 50 years. This thesis consists of three stand alone essays which all fit in the broad UI literature. More specifically, at the core of this thesis is the question of what is the optimal amount of unemployment insurance that should be provided by the government. In other words, how can the policy maker determine the optimal amount of unemployment benefits which achieves the best balance between welfare costs and gains for society? Therefore, the main broad contribution of this work is advancing the most recent literature at the research frontier addressing this question.

In particular, this thesis aims to advance the framework provided by the seminal contribution of Chetty (2008), which in few years has become a cornerstone benchmark for the UI literature. Such approach allows one to assess the marginal welfare effects of unemployment benefits by relying on a very general baseline model: *the sufficient statistics approach*. As discussed at length in chapter 2, the fundamental advantage of this framework is that there is no need to estimate lower-order statistics of the model. In such way, policy conclusions remain robust to whatever is the true underlying primitives of the model such as individuals' risk aversion.

The first essay of this thesis in chapter 3 studies the relationship between the duration of employment spells and the level of unemployment benefits. The first key original contribution of this work is an empirical one. I find that unemployment insurance can affect the duration of employment spells in an economically significant way. It shows that UI affects the dynamics of employment spells by altering workers and/or firms' incentives. Based on a quasi-experimental approach, I show that an 1% increase in benefit level leads to an increase of about 0.5% in employment duration for low-skilled workers in Brazil. To the best of my knowledge, this is the first study to assess such causal link with an empirically credible framework.

Nevertheless, simply showing the existence of this causal link between UI and employment duration falls short of addressing the real policy question: is this empirical finding of any relevance for social welfare? If yes, how does it affect social welfare? Therefore, the second key original contribution of this chapter is providing a framework which shows how this effect impacts welfare. I show that when UI increases employment duration, the optimal level of unemployment benefits is higher than otherwise. In this case, the intuition is that policy makers can provide higher UI by raising UI taxes by less when higher benefits cause workers to contribute for longer periods. In the case where this elasticity is negative, a perfectly symmetric argument applies and the optimal benefit level is lower than otherwise.

Moreover, a key feature of the theoretical framework developed in this essay is delivering a welfare formula bases on sufficient statistics. Thus, it can be directly linked to the data. Taking together this theoretical result with the empirical results leads to the third key contribution of this chapter. It allows one to answer the following question: does the effect on employment duration affect social welfare with the same order of magnitude of the remaining well-known elements of the formula? Put differently, is the impact of this effect small compared to other elements affecting welfare so that it can be neglected? At least in the context of Brazil, the answer is no. The elasticity of employment duration to benefit level impacts welfare in the same way as the elasticity of unemployment duration, and the size of the first effect found in the data is similar to previously estimated figures on the second effect.

Two further theoretical contributions naturally arise from the welfare formula provided in this first essay. First, since the model incorporates minimum eligibility requirements, the welfare formula unveils the role of MER for optimal benefit level. If it is the case that raising unemployment benefits increases the number of individuals reaching MER and thus being granted UI, it imposes an additional cost on the system in such way that the UI tax must be further increased, decreasing welfare. Assessing the empirical size and plausibility of such an effect goes beyond the scope of this work. Second, and differently from Chetty (2008), the welfare formula shows that only the UI covered elasticity of unemployment duration matters for welfare. The full (uncovered) elasticity of unemployment duration is not relevant for welfare because unemployed workers no longer drawing UI benefits are neutral to the government budget constrain.

Overall, the main conclusion from this essay is that UI can affect the duration of employment spells in an economically meaningful way. This is of direct interest for policy makers to evaluate the social costs and benefits of UI provision. This work provides a welfare formula which incorporates this effect and delivers a clear message on how it affects the optimal level of unemployment benefits. Furthermore, the empirical strategy proposed in this essay relies simply on the policy schedule determining benefit level in Brazil. Thus, together with the welfare formula, it could serve as tool for policy makers to constantly evaluate the optimality of benefit level over time.

The second essay present in this thesis turns the attention to the social benefit side of providing unemployment insurance. If on one hand there is a large empirical literature studying the adverse effects of UI, on the other hand the evidence on the liquidity gains from UI is rather scarce. Henceforth, the second essay of this thesis is concerned with advancing this extremely small literature. The first key contribution of this essay is assessing how large are the liquidity effects of unemployment insurance in a context of high labor informality. To the best of my knowledge, this is the first paper to assess this effect in such context. It fundamentally contributes to assessing whether it is desirable to provide unemployment benefits in developing countries, where labor informality is typically prevalent. I show that the liquidity effects for unemployed workers are similar to previously found in Norway and much larger than estimates for Austria.

The second contribution of this chapter is fully assessing the liquidity-to-moral hazard ratio. As discussed at length in chapter 2, this is the actual statistic measuring the welfare gains from liquidity provision in the framework developed by Chetty (2008). I find this to be similar to previously found in the US, the only country for which there is a full estimation of this statistic (Landais, 2014). These results suggest that the potential gains from providing UI in contexts where labor informality is prevalent are large and comparable to developed countries. If taken together with the results from Gerard and Gonzaga (2013) that the efficiency costs of UI under high informality are low, these results make a suggestive case: UI provision in developing countries is likely to be welfare enhancing. Once again, the empirical strategy developed to estimate these fundamental statistics for welfare evaluation does not depend on policy changes. Therefore, it allows one to monitor constantly such effects over time. Again, together with the theoretical framework, it may serve as an useful tool for policy makers to evaluate the optimality of benefit level over time.

While the first essay of this thesis shows that UI affects the average duration of employment, the third essay is concerned with the specific mechanisms through which unemployment benefits affect the dynamics of employment spells. My analysis starts from the common finding in the literature showing that employment hazard sharply increases once workers become eligible for unemployment insurance. The open question is whether this effect is driven by the firms or employees' behavior, if not both. The third essay of this thesis investigates whether a supply side explanation is plausible: can this effect be driven by the behavior of workers?

The first and marginal contribution is showing that such spike in hazard rates do exist in Brazil, just as previously found in developed countries. Then, I provide a learning model which shows that the supply side explanation is able to explain the whole profile of employment hazard rates. The underlying idea of the model is that once workers become eligible for UI, they decrease their level of work effort and thus hazard rates spike. To assess the empirical plausibility of this underlying mechanism, I provide firm level personeel data on absenteeism and show that worker's absenteeism sharply increases once workers qualify for UI. This is perfectly in line with the hypothesis that work effort decreases once workers qualify for benefits. The main contribution of this essay is thus providing evidence that the supply side explanation is at place. Understanding such underlying mechanisms may prove relevant for the design of UI features targeted at dealing with such effects. For example, there is a literature which claims that such spikes are caused by firms' lay-off behavior. Because UI is most often not fully experience rated, it creates incentives for firms to temporarily lay-off.[1] Hence, if the policy maker pursues the goal of diminishing such spikes, it should target firms. For example, it could make experience rating more stringent. However, if the spike in hazard rates is (also) caused by workers' behavior, probably the measure just described will not fully deal with the issue. This would call instead for measures targeted at the workers.

As a final concluding remark, I notice that a great deal of this thesis is dedicated to the study of how unemployment insurance influences the dynamics of employment spells. While the large literature on unemployment insurance has been extremely successful at providing insightful guidance for policy, the relationship here investigated seems to be understudied . The work here presented studies different aspects of this relationship and provides evidence that UI effects on the dynamic of employment spells may be not only quantitatively sizable, but also relevant for social welfare. At least to a minimal extent, I hope that this thesis can motivate further research on these issues which may help the pursuit of better policies for society.

[1] Feldstein (1976, 1978); Topel (1983, 1984)

# Summary


This thesis is a collection of three essays which study how unemployment insurance (UI) can be provided in the most beneficial way for society. In particular, a great deal of this work aims to advance the scientific understanding regarding the following question: how generous unemployment benefits should be in order to maximize social welfare? In other words, how much unemployment insurance do we need?

Chapter 1 and 2 are introductory to the thesis and to the specific topic. The first of them introduces the specific questions addressed in this thesis and presents the main results. Chapter 2 provides a more throughout review of the related literature and highlights how each of this thesis' essays contribute to advancing the scientific understanding on the topic.

Chapter 3 presents the first of the three essays. It studies the existence of a causal link between the availability of potential unemployment benefits for employed workers and the duration of their employment spells. After discussing few straightforward reasons why and how UI may affect employment duration, I apply a regression kink design to address this question using linked employer-employee data from the Brazilian labor market. Exploiting kinks in the Brazilian UI schedule, I find a statistically and economically significant effect of benefit level on the duration of employment spells at the lower end of the skill distribution. Surprisingly, the results for these workers indicate that the elasticity of employment duration to benefit level is positive and as large as 0.5. To assess the economic relevance of this result, I generalize the reduced welfare formula from Chetty (2008) to deal with this effect on employment duration and show that this elasticity is as relevant for welfare as the elasticity of unemployment duration to benefit level.

Chapter 4 contains the second thesis' essay. It first exploits a "bonus" policy providing low-income workers with cash grants in Brazil to study the effect of liquidity provision on unemployment outcomes. Based on a RDD, I find that granting unemployed workers with a bonus equal to half of their previous monthly earnings decreases the probability of exiting unemployment within 8 weeks by around 0.65%. Second, by exploiting the UI potential duration schedule, I find that granting workers with an extra month of unemployment benefits decreases the same outcome by 1.9%. Then, theoretical results from Landais (2014) are used to combine these estimates and disentangle liquidity and moral hazard effects

of UI. Based on these, I estimate the liquidity-to-moral hazard ratio in Brazil to be as large as 98%, similarly to values previously found in the US. It suggests that, contrary to common belief, providing UI in developing countries with large informal labor markets may be welfare increasing.

Chapter 5 is composed by the third and last essay. This work investigates how unemployment insurance (UI) affects unemployment inflow. By using administrative data from the Brazilian Labor Market and applying a Regression Discontinuity Design, I show that UI significantly increases the lay-off hazard rate at the minimum eligibility requirement for benefits. Then, I provide a learning model with work effort which is able to explain this finding and the hazard rate profile over time by relating unemployment benefits to work effort and lay-off hazard rates. The model supports the hypothesis that UI may increase unemployment inflow because it undermines work effort. Then, personnel data on absenteeism supporting this prediction is provided

The main conclusion from these three essay are summarized and related to each other in Chapter 6.

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