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Abstract

One of the main characteristics of the Brazilian labor market is its impressively high job and worker turnover rates. Although labor legislation in Brazil is very restrictive, dismissal costs are not high when compared with other Latin American countries. Moreover, many authors argue that the design of some job security programs creates perverse incentives that generate labor turnover. The objective of this paper is twofold. First, we describe Brazilian labor legislation, with emphasis on job security provisions and their incentives on workers reallocation. Then, after reviewing the most recent evidence on labor turnover in Brazil, we investigate the effects of changes in job termination costs implemented in the 1988 Constitution and in a Labor Law of September 2001 on employment duration. Both legislation changes increased firing costs and should have, therefore, reduced turnover for formal workers affected by them. A simple differences-in-differences methodology is applied to monthly individual data from *Pesquisa Mensal de Emprego (PME, IBGE)*, which has information on previous employment spells for those currently unemployed. The results establish that both changes reduced turnover for formal workers affected by the legislation. A significant increase in average employment duration of affected workers relative to not affected workers was observed after both legislation changes. We also provided evidence that the 1988 Constitutional change reduced the probability of fake layoffs, although there are still a high number of such agreements being made between workers and their employers.

Keywords: Labor Turnover, Employment Duration, and Job Security Provisions in Brazil

JEL Codes: J63, J64, J65 and J68

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I. Introduction

One of the main characteristics of the Brazilian labor market is its impressively high job and worker turnover rates. This contrasts with a vision of many analysts that Brazil has an overly regulated labor market. It is true that the Brazilian labor code is very restrictive. However, dismissal costs are not high when compared with other countries in the region (see Heckman and Pagés, 2000). Moreover, many authors claim that it is the design of some job security programs in Brazil that creates perverse incentives that stimulates labor turnover to levels greater than would otherwise be attained.

Critics of job security provisions usually argue that dismissal costs tend to create obstacles to functional labor market flexibility, while supporters of dismissal costs stress potential benefits coming from less income volatility and more investment in specific human capital that could increase productivity in the medium run. According to this latter view, excessively high turnover rates might be seen as a problem, since it might be causing under-investment in human capital and be signaling a low commitment between employers and employees.

Brazil displays one of the highest labor turnovers in the world for some comparable measures. An average of 3.4% of the formally employed are admitted and separated from their jobs every month. A high labor turnover can partially explain the low quality of jobs observed in Brazil, since labor productivity depends essentially on the level of human capital, either general - through basic education - or specific - through on-the-job training. Since a high labor turnover is a disincentive for training investment, it lowers specific human capital and, therefore, labor productivity.

In this paper, we fully describe the legislation on dismissal costs in Brazil. Following the argument of other authors, we conclude that the design of the FGTS (*Fundo de Garantia por Tempo de Serviço*) system, a seniority severance payment fund, is inefficient, is a source of conflict between firms and workers, and creates labor turnover.

According to the FGTS system, every firm in Brazil has to deposit 8% of its current formal employees wages (8.5% since September 2001) in an account opened in the worker's name in a state bank. Other than some exceptions, workers can only withdraw money from these accounts in case they are fired without a "justified reason". In this case, they have access to the FGTS account balance plus a firing penalty paid directly by the employer. Since the returns to the fund are much below market rates, there is a strong incentive for workers to get a hold on their FGTS accounts. Since the firing fine can be *de facto* negotiated between firms and workers, there is ample room for

fake dismissals, by which firms simulate they are firing workers for just cause and are paying the firing penalty. This tends to increase labor turnover. Every year in Brazil, there are around nine million withdrawals from FGTS accounts (Zylberstajn, 1999).

In light of this, the paper uses two episodes of increases in the fine to empirically identify the effects of firing costs on labor turnover. The idea is to study the effects of increases in job termination costs implemented in the 1988 Constitution and in a Labor Law of September 2001. These episodes are used to test the prediction that higher firing costs would, other things being equal, reduce turnover for formal workers affected by the legislation.

It is important to emphasize that the paper does not advocate that such firing cost increases are good policy responses to the original distortion, which causes turnover to be sub-optimally high in Brazil. In fact, a removal of the original distortion through a reform of the whole functioning of the FGTS system would be preferable.

A simple differences-in-differences methodology is used to test the implication described above in monthly individual data from *Pesquisa Mensal de Emprego (PME, IBGE)*, which has information on previous employment spells for those currently unemployed. The methodology exploits the fact that the legislation changes should have differently affected some groups of workers relative to two control groups: informal workers, and formal workers with low-tenure (less than three months).

The paper is organized as follows. Section II describes the Brazilian labor legislation, with emphasis on those measures that affect labor turnover, including the perverse incentives mentioned above. Section III reviews the evidence on labor turnover in Brazil found in the recent literature. Data from several sources, analyzed through the lenses of several methods, confirm the view of a "hyperactive labor market" (World Bank, 2002), with especially high turnover rates for the less educated. Section IV brings presents the results of the differences-in-differences methodology to assess the importance of changes in the labor legislation on employment duration. Section V concludes.

II. Labor Legislation in Brazil[1]

II.1 The development of social rights in Brazil and the CLT

To understand the evolution of labor legislation in Brazil across time, it is useful to place the country's long-term process of citizenship building in a broader perspective. Carvalho (2001) uses a

[1] Some parts of this section were largely based on Barros, Corseuil and Gonzaga (1999).

standard notion that citizenship is fully characterized when the society has completely developed civil, political and social rights. Although countries differ with respect to the sequence of development of each of these groups of rights, a typical pattern observed in most countries is one in which civil rights precede political rights, which in turn create the background for advancing social rights. Carvalho argues that Brazil is an exception to this sequence, with social rights preceding both civil and political rights. He provides detailed evidence that individuals in Brazil did not conquer social rights. On the contrary, social rights were obtained through concessions from paternalistic and/or authoritarian governments, and were usually accompanied by measures that tended to restrict labor and social movements.

The Brazilian labor code (*Consolidação das Leis do Trabalho, CLT*) is a clear example of this pattern (Amadeo and Camargo, 1996). The CLT was created in 1943 and has governed Brazilian labor-capital relations since then. It currently has more than 900 articles. When created in 1943, it consolidated many Labor Laws that had been progressively introduced since the beginning of the Getúlio Vargas government in 1930.[2]

The Brazilian labor legislation suffered just a few important changes over the last sixty years. The two main revisions were made in 1964 (by the military regime) and in 1988 (when a new Constitution was implemented).

Most changes introduced in 1964 had the objective of reducing the power of labor unions and their ability to organize (Amadeo and Camargo, 1996). The right to strike was severely reduced. In fact, this was a period in which many union leaders were persecuted. Also in the beginning of the military government, a wage policy was introduced. From 1965 to 1995, wage determination in Brazil was largely influenced by the official wage indexation policy, especially between 1965 and 1979, a period of very low labor union activity.[3]

The new Brazilian Constitution was implemented in November 1988 as part of the process of re-democratization of the country after the end of the military regime in 1985. The Constitution brought many changes to labor legislation (Barros, Corseuil and Gonzaga, 1999). In fact, many labor laws are written in the Constitution, which makes them much harder to change since at least 60% of the votes are required in two-round voting in both parliament houses to amend it. The aim

[2] The CLT is a highly restrictive and detailed body of law, clearly fascist in inspiration. Hall (2002), for instance, finds striking similarities between CLT and the Italian "Law on the Jurisdical Disciplining of Labor Relations", from 1926, a law that preceded Carta del Lavoro, from 1927.

[3] Mario Henrique Simonsen, one of the most brilliant Brazilian economists of his time and architect of the first wage policies, once wrote that the "beauty of the official wage policy was that it substituted a highly complex system of labor-capital negotiations by a simple arithmetic formula".

of the changes was to increase workers' benefits and reverse restrictions on workers' rights to organize that characterized the previous period. Most of the changes, however, represented a significant increase in labor costs. Among many other items, the maximum number of working hours per week without overtime pay was reduced from 48 to 44 hours; the minimum overtime premium increased from 20% to 50%; the maximum number of hours for a continuous work shift dropped from 8 to 6 hours; maternity leave increased from 3 to 4 months; and the value of paid one-month vacations increased from 1 to, at least, 4/3 of the normal monthly wage. There was also an increase in dismissal costs. Since this is the main focus of this paper, we will discuss the dismissal cost modifications in detail in the next sub-section.

Before moving on, let us briefly note that not many changes were introduced to labor legislation over the last fifteen years. Although the former president, Fernando Henrique Cardoso, advocated from time to time the need of labor legislation reforms, these never ranked high in his 1995-2002 two-term government priorities. For example, Congress never completed the voting process on four Constitutional amendments sent by the government – three in 1998 and one in 2001.

Some changes were nonetheless introduced in the 1990s. However, they had only minor effects on the functioning of the Brazilian labor market. The two main modifications were the introduction of fixed-term labor contracts in 1998 and the possibility that extended periods of time be considered for counting the average number of weekly hours.

Fixed-term labor contracts (*Contrato de Trabalho por Prazo Determinado*) were introduced in 1998 by Labor Law 9601. Under these contracts workers could be hired for a fixed number of months (up to two years) with much lower payroll charges and without any dismissal costs. The main restriction was that these contracts have to be approved in collective agreements with unions. The consequence was that only around 40.000 workers were hired under these contracts from 1998 to 2002. This contrasts with an average of 750.000-800.000 Brazilian workers, on average, being hired and fired from formal jobs *every month* over the same period. The current president, Luiz Inácio Lula da Silva, has already not renewed the law, which is not in effect since 01/23/2003.

Another labor legislation implemented in 1998 created the so-called "hours bank" (*Banco de Horas*), which allows firms to increase from one week to 4 months the period for calculating the average of the number of hours worked, and consequently the number of overtime hours. This gives more flexibility to firms in hiring overtime hours in high-demand weeks which can be compensated with undertime hours in low-demand weeks over a 4-month period. Contracts with this clause also

have to be approved in collective agreements with unions and have been widely used, especially in the industrial sector.[4]

II.2 Legislation on Dismissal Costs in Brazil

Job security provisions exist in Brazil since the early 1940s. According to the labor legislation prevailing from the early 1940s to 1966, workers with less than ten years and more than one year of tenure were entitled on dismissal to receive a severance payment of one monthly wage per year worked at the firm (Oliveira *et al.* 1999). Workers with more than ten years at the firm were granted job stability. These workers could only get fired for "justified reasons", which did not include absenteeism and low productivity. Other than just cause, the only way to terminate a labor contract was through a severance payment of two monthly wages per year on the job, but only in the case the worker agrees with it. As one would expect, this created many distortions affecting the productivity of workers with job stability, with very frequent cases ending up on the labor courts. Moreover, the absence of formal mechanisms assuring that dismissed workers would in fact receive the severance payments predicted in the legislation created additional problems. Some attempts to reform job security provisions in the direction of guaranteeing that the resources necessary for dismissal charges were provisioned by firms and deposited in federal funds were made in 1958 and 1964.

It was under this background that the FGTS (*Fundo de Garantia por Tempo de Serviço*) system was created in September 1966. FGTS is a seniority fund created by Law 5107 to replace, on a voluntary basis, these job security provisions. The main idea was to establish a legislation that would at the same time remove the distortion of full job stability for those with more than ten years of tenure, and assure the provision of funds to cover severance payments. In practice, all new contracts after 1966 were written under the FGTS system, which was preferred by both workers and firms, although several workers opted not to switch their old contracts to the new system.

According to the FGTS legislation, the employer has to deposit every month 8% (8.5% since September 2001) of his/her formal employee's monthly wage into an individual account, managed by a state bank, *Caixa Econômica Federal*.[5] Deposits are periodically adjusted to compensate for

[4] The majority of collective agreements that implements the hours bank mechanism also contains worksharing measures, such as the reduction of the length of working time, increases in overtime premium, and some wage restraint. This illustrates the change of focus of labor unions from wage adjustment until the early 1990s to a larger concern with employment.

[5] Before 1989, the FGTS system was managed by the extinct *Banco Nacional da Habitação*.

inflation plus a 3% annual interest rate.[6] Workers have access to their accounts only in the case of unjustified dismissals or upon retirement.[7] That means that workers that voluntarily quit are not granted access to their accounts.[8]

The law also states that all workers dismissed for unjustified reasons, with the exception of workers on probationary period, must receive a fine paid by the employer equivalent to a proportion of the FGTS balance accumulated during the period in which the worker was with the firm. From 1966 to 1988, this proportion was fixed at 10%. The 1988 Constitution increased this penalty from 10 to 40% of the FGTS balance. Finally, as discussed below, since September 2001 the firms have to pay an additional 10% of the FGTS balance to the government when dismissing a worker for unjustified reasons.

We should stress that workers have access to the entire individual fund on dismissal, including all deposits accumulated in previous jobs, plus the fine in proportion to the deposits accumulated only while on the job from which they are being dismissed.

The FGTS balance thus accumulates at a rate of approximately one basic monthly salary per year on the job, since the monthly deposit in the account corresponds to 8% of the monthly wage (8.5% since September 2001). Note that, by design, the FGTS system is intended to approximately match the same severance payment determined by the previous job security provisions.

The three main changes with respect to the previous legislation were: i) firms were forced to make a provision for the severance payment depositing it upfront in the worker's FGTS account; ii) under the new system, there is no more job stability for those with more than ten years of tenure; and iii) the introduction of the fine for unjustified dismissals, initially of 10% of the FGTS balance.[9]

In September 2001, a new labor law was introduced in order to deal with the effects of a Supreme Court decision that threatened the solvency of the FGTS system. The decision was to adjust all FGTS account balances that were active in 1990 by 68.6%, which corresponded to real

[6] From 1966 to 1971, interest rates were 3, 4, 5 or 6% for those workers that migrated from the previous job termination law, depending (proportionally) on the tenure at the current job at the time (Oliveira et al., 1999).

[7] Some exceptions have been introduced over time that also allow workers to withdraw money from their accounts: buying the first real state in the city the worker lives, suffering serious diseases like cancer and Aids, having an inactive account for more than five years, death, etc.

[8] Some other Latin American countries (Colombia, Ecuador, Panama, Peru, and Venezuela) have severance arrangements similar to FGTS (Heckman and Pagés, 2000). However, in these countries, workers have access to their seniority funds both in the case of voluntary quits and unjustified dismissals. In these countries, like in Brazil, the penalty is paid only in the case of unjustified dismissals.

[9] One can interpret the fine as a distortion deliberately introduced in the job security legislation to compensate for the removal of an even larger distortion – the job stability after 10 years on the job.

term losses incurred at the time of two stabilization plans (16.44% in *Plano Verão* in 1989 and 44.80% in *Plano Collor* in 1990).

After several months of negotiations, the government reached an agreement with workers and firms' representatives to share the costs of the judicial decision. As part of the agreement, as mentioned above, the fine for unjustified dismissals increased from 40 to 50% of the FGTS balance with the extra 10 percentage points paid to the government. The new law also determined that monthly deposits in FGTS accounts increase from 8 to 8.5% of current monthly wages.[10]

It is important to emphasize that this last fine increase is to be paid to the government instead of to the worker. Consequently, there was no change in the amount the worker receives as a compensation for unjustified dismissal after Law 110, which is still 40% of approximately one monthly wage per year in the firm. Labor Law 110, therefore, represented at the same time an increase in firing costs (since the extra 10% goes to the government) and a reduction in the incentives for workers to making agreements with their employers that would enable them to receive their FGTS balances, since these are now more expensive.

The other important component of job security legislation in Brazil is advance notification. Since the 1940s employers are required to give a one-month dismissal advance notice to their employees, with the exception of workers on probationary period - from zero to three months on the job.[11] During that month, workers are granted up to two hours per day (25% of a regular working day) to search for a new job. Since productivity of leaving workers tends to significantly drop during the advance-notice period, the law gives an option to the firm of incurring in the costs of paying an extra wage to the worker at the moment of layoff without requiring them to work. In other words, the cost of advance notice is actually between 25% and 100% of one month's salary, being in practice closer to 100% than to 25%.

To sum up, it is useful to introduce some notation. Total firing cost in Brazil is $\lambda w + (f+g)FGTS_{bal}$, where w is the monthly wage, λ is the proportion of the monthly wage that constitutes the cost of advance notice ($0.25 \leq \lambda \leq 1$), f is the proportion of the FGTS balance paid as a

[10] This change was implemented through Complementary Law 110 of 06/25/2001 and regulated by the Decree-Law 3914 of 09/11/2001, which came into effect on 09/28/2001.

[11] Before the 1988 Constitution, advance notifications had also to be given at least one month in advance. In fact, the 1988 Constitution states that the period of notice should be proportional to the worker's tenure and this change has to be regulated through ordinary legislation, which requires simple majority of votes in both houses. However, no specific law has ever regulated this constitutional device, and notice continues to be given one month prior to dismissal for all workers, independent of their tenure. In fact, even the fine increase from 10 to 40% is in the article 10 of the Transitory Constitutional Dispositions, which should remain in effect until a Complementary Law is promulgated, which never happened.

fine on dismissal to the worker, and g is the proportion of the FGTS balance paid to the government.[12]

The first term corresponds to the cost of one-month advance notice. Based on the discussion above, let us assume that λ is equal to one, since in practice the cost of advance notice is close to one monthly wage. The second component is the fine on unjustified dismissals. Since the FGTS balance accumulates at one monthly wage per year at the firm, total firing cost is approximately equal to $(1+(f+g)y)w$, where y is the number of years the worker was with the firm he/she is being fired.[13] Note that the worker is entitled to receive only $w+f.FGTS_{bal} \cong (1+f.y)w$, since the proportion g is paid directly to the government.

Figure 1 plots the total amount of dismissal costs (one-month advance notice plus the fine over the FGTS fund) in terms of the basic monthly wage for the three periods after the implementation of FGTS in 1966: i) from 1966 to the promulgation of the 1988 Constitution in November 1988, when according to our notation f=0.1 and g=0; ii) from November 1988 to the Law 110 of September 2001, when f=0.4 and g=0; iii) and after September 2001, when f=0.4 and g=0.1. Note that there are no costs of dismissing a worker on probation, i.e. a worker with less than 3 months (1/4 year) on the job.

Note that dismissal costs were very small prior to November 1988. In fact, before the Constitutional change, the worker had to be employed in the same firm for at least ten years in order that the fine reach the magnitude of one monthly salary in addition to the one received as advance notice. Between November 1988 and September 2001, it would take 2.5 years in the job for the fine to reach the value of one monthly wage. After September 2001, it takes 2 years of tenure for the penalty to amount to an additional monthly wage.

II.3 Perverse Incentives Implied by the Legislation on Dismissal Costs

Much has been written on the perverse incentives coming from the nature of the legislation on dismissal costs in Brazil (see, among others, Macedo (1985), Camargo (1996), Amadeo and Camargo (1996), Gonzaga (1998), and Barros, Corseuil and Bahia (1999)). The basic argument made by these authors is that the design of the legislation gives strong incentives for workers to induce their own dismissal. The three main features of the FGTS system that create these incentives are: i) there are strong incentives for workers to get a hold on their FGTS balances; ii) getting fired

[12] Note that the FGTS balance itself (the accumulation of the monthly deposits in the FGTS accounts of 8.5% of the wage) is not included among the firing costs. The deposits are, in fact, a static labor cost.
[13] In this approximation, we are also assuming that wages do not increase much with tenure, a hypothesis more realistic for workers with low human capital.

is the main mechanism to get access to the FGTS accounts; and iii) since the penalty is paid directly by the employer to the employee, there is ample space for fake dismissals.

The incentives to withdraw FGTS balances arise for two reasons. On one hand, the government has poorly managed the FGTS, typically paying negative real returns or returns well below market rates. This occurred mainly due to less than perfect inflation-indexation, especially on the occasions of many stabilization plans implemented in the late 1980s and early 1990s that involved currency and price index changes. Oliveira *et al.* (1999) show that the main losses occurred in the early 1980s and early 1990s. Negative real returns of approximately –60% were observed from 1977 to 1993.[14] Inflation stability after the Real Plan helped to obtain some recovery of real values of FGTS deposits since 1994, but still the 3% interest rate is much below market rates (riskless assets like savings accounts, for instance, pay 6% interest rate plus the same nominal indexation as FGTS).[15]

The design of the FGTS system creates an incentive for firms and employees to involve in rent-seeking activities. A fake layoff agreement can be described as follows. Using the notation from the previous sub-section, a worker that wants to quit can offer $b.FGTS_{bal} \cong b.y.w$ to the firm to fire him/her without just cause so that he/she is able to receive $(1-b)FGTS_{bal} \cong (1-b)y.w$, where $0 \leq b \leq 1$. Note that in the case the employer agrees, he/she has to simulate paying $(1+f.y)w$ to the worker and has to actually pay $g.y.w$ to the government.[16] Consequently, when $g>0$, b has to be at least larger than g to make it worthwhile for the employer to accept the offer from the employee. Therefore, the introduction of the penalty g to be paid to the government after September 2001 should have made these agreements of fake dismissals more costly to be implemented. Since the penalty does not go the worker, there is only a negative effect of the increase in g on labor turnover. The same reasoning applies to eventual future increases of g.

On the other hand, an increase in f, like the one observed in 1988, should have two effects. On the one hand, it should also reduce the incentive for the employers to engage in fake dismissals

[14] Before 1989, for instance, FGTS balances were adjusted at a quarterly frequency (annually, from 1971 to 1976). Only in 1989 the frequency of adjustment was increased to monthly, with inflation indexation following the same indexes as those governing nominal adjustment of savings accounts.

[15] Additionally, due to shortsightedness or credit constraints, workers may be heavily discounting the future. Some authors have argued that this would not be rational, given that there is a high premium for tenure in Brazil, unless discount rates are too high (Carneiro and Ramos, 2002). However, one should note that the tenure premium is much higher for the more educated in Brazil, as expected by theory. In fact, low educated workers gain very little from tenure in Brazil, with dim prospects for future earnings on the same job. At the same token, low educated workers are more likely to be credit constrained. Therefore, one should expect that the temptation to get access to the FGTS balance is higher for less-educated and for poorer workers.

[16] Note that in the fake layoff-agreement, the employer does not pay the fine f to the worker.

agreements, since it implies larger losses for employers in the case that the employee does not comply with the agreement, by for example not returning the money paid as dismissal penalty or bringing him/her to court alleging he/she has not paid it. On the other hand, an increase in f might eventually raise the desire of employees that would otherwise quit to get fired, since the short-run rewards are now larger. Note that this second effect exists only for workers that decided to get fired through a litigious process since the worker only gets the penalty if the employer decides to fire him/her and pay him/her the penalty. However, in the litigious case, the employer has nothing to gain from it. On the contrary, it is likely that these workers take actions to force their own dismissal.

The first effect implies a lower labor turnover and less fake layoff agreements. The second effect implies a higher labor turnover, also less fake layoff agreements, and certainly lower productivity since it makes labor-capital relations more litigious. In a multi-period framework, where references for future jobs are important, it is likely that the second effect is small being perhaps more significant, for example, for workers that would like to use the money for opening a new business. It is also likely that employers would resist in firing such a worker without just cause, to signal to other workers that they are tough. Bringing workers to court for just cause might be the optimal strategy for employers in this situation in order to avoid other cases in the future, even when it is likely that they would end up losing the judicial action. In any case, with regards to labor turnover, the net effect is not obvious and will depend on the factors behind the decision of workers to switch a job termination negotiation process from being non-litigious to being litigious.

As mentioned above, there was one episode of increasing f (from 0.1 to 0.4) in November 1988 and one episode of increasing g (from 0 to 0.1) in September 2001. According to our discussion, it is an empirical question to find the net effect of the November 1988 fine increase on labor turnover, although we expect to observe a negative effect on labor turnover, since we believe that the first effect described in the previous paragraph is more important than the second effect. As to the increase in g of September 1001, it should have unambiguously decreased labor turnover in Brazil.

One should thus expect that many workers that would otherwise like to quit a job would make efforts to get fired (for "unjustified reasons") in order to get a hold on their FGTS accounts, either with the agreement of their employers or not.[17] This is, in fact, very common in Brazil. There

[17] It is important to note that the Brazilian government has become aware of the existence of fake layoff agreements over time. The typical response to the problem has been the implementation of procedures that make these simulations of payments more costly and risky. For instance, in the 1990s, only workers with more than one year of tenure were required to receive the penalty payment in the labor union that represents him/her and to sign a document that he/she has received it. Since the early 2000s, a new legislation requires that the penalty be deposited in the worker's FGTS

is ample anecdotal evidence, for instance, that employees in human resources departments of large firms rob their own firms by sharing the rents coming from these fake layoff agreements with the employees. In some cases, workers are re-hired after 3 months, which is a legal procedure.

Camargo (2002) provides evidence that complaints involving costs related to dismissals not paid by employers are the second-most common reason for taking employers to judicial courts. On the other hand, there is evidence that a considerable number of workers succeed in making agreements with their employers about being fired. Barros, Corseuil and Foguel (2001) show that 62% of unemployed workers that answered the 1990 PNAD (*Pesquisa Nacional de Amostra por Domicílio*, the annual Brazilian household survey) that they have quit their previous formal jobs also answered that they have received their FGTS balance.[18] Since the legislation does not allow quitters to withdraw their FGTS balance, these seem to be fake layoffs, i.e. quitters that somehow were able to convince or induce their employers to dismiss them without a just cause.

This piece of information has been overlooked in the previous literature. The information on the proportion of fake layoffs (workers that voluntarily quit and withdrew FGTS) is available from PME on a monthly basis for the 1982-2002 period. Therefore, it is possible to exploit its temporal variation around the implementation of both the Constitutional change and Law 110 in order to test whether the incidence of this type of agreement decreased after the fine increases. This is accomplished in Section IV of this paper.

In sum, the facts that (a) the dismissal penalties are received individually by the dismissed worker and (b) being fired is the main mechanism for workers to acquire control over their FGTS accounts give workers considerable incentives to induce their own dismissal after a certain time in any job. The legislation changes introduced by the 1988 Constitution and by Labor Law 110 of September 2001 implied increases in firing costs that should have made it harder for workers to come to agreements of fake layoffs with their employers. A reduction in labor turnover (or equivalently, an increase in employment duration) and a decrease in fake layoffs are expected to be observed after both legislation changes. These implications are tested in Section IV of this paper.

III. Evidence from the Literature on Labor Turnover in Brazil

Over the last five years, many papers brought new evidence concerning labor turnover in Brazil exploiting old and recently available data sets. Some measures were constructed following

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account, which of course makes agreements harder to being implemented. A direction for future research on this topic is to study the effects of this temporal variation in these procedures on labor turnover and on the incidence of fake layoff agreements.

[18] The authors also report that this proportion in the *Pesquisa Mensal de Emprego (PME)* in 1998 was 68%.

methodologies that allow one to make international comparisons. This section reviews the recent literature, gathering new evidence on this topic.

III.1 Studies that Measure Job and Worker Turnover

Corseuil, Ribeiro and Santos (2003) use Ministry of Labor and Employment annual administrative files (*Relação Anual de Informações Sociais, RAIS*) to compute job and worker reallocation measures. The data set is organized in such a way that allows one to follow firms from 1991 to 1998. It covers all sectors and all regions of the Brazilian economy. It has detailed information on workers' characteristics, stocks of workers, and number of hirings and separations. The only drawback is that information is provided only for formal (registered) employees.

The authors report an annual job reallocation average of 33% over the 1991-98 period, a level substantially higher than the yearly average of net employment growth of 1.5%. Average job creation and job destruction rates were, respectively, 17.3% and 15.5%. Some sectors have astonishing job reallocation rates (a rate of 63.9% was found in Construction, maybe due to the characteristics of this sector, which tends to hire most workers for specific projects). All these measures place Brazil among the countries with the highest job turnover rates in the world.[19]

In line with the findings on job turnover rates, although not less impressive, are the evidence on worker reallocation. Average yearly hirings amount to 48.2% of the employment level. Average separations are 46.2%. These correspond to an average churning rate (worker turnover in excess of job turnover) of 61.8%, certainly among the highest in the world. Sectoral disaggregation shows that Construction hires every year 121.8% of the previous year-end employment level.

Menezes-Filho and Fernandes (2003) use the same data set (RAIS) to analyze the costs of job displacement in Brazil. They show significant earnings losses following job transitions. More importantly for the purposes of this section, they report evidence on job tenure (average employment spells for the currently employed). They find that, on average from 1992 to 1998, 29.9% of formal employees were with their employers for less than a year; 44.0% for less than two years; with only 34.1% with average tenure above 5 years. According to the authors, this represents a much shorter employment duration than observed in the U.S., where only 20% of workers have less than 1 year tenure. They also confirm a sharp increase in job tenure for the more educated.

More recent data for the period 1997-2002, available monthly from the Labor Ministry (*Cadastro Geral de Empregados e Desempregados, CAGED*), confirm the very high worker

[19] The study also confirms expected findings that job reallocation rates are negatively correlated with plant size and age, and with worker education.

turnover rates. An average of 746.2 thousand workers (3.4% of the formally employed) were admitted to a new job each month between 1997 and 2002, while 740.4 thousand workers (3.4% of the formally employed), on average, left their formal jobs each month during the same period. These measures correspond to annual rates of worker turnover around 40% for the more recent period.

Pazzelo, Bivar and Gonzaga (2000) use manufacturing establishment survey data from the *Pesquisa Industrial Anual* (*PIA, IBGE*) for the 1986-95 period. They report average job creation and job destruction rates of 9.8 and 13.2%, respectively, for the period.[20] Using a methodology that avoids pitfalls found in some studies on the effect of size on job turnover rates, the paper reports much higher job creation and destruction rates for micro and small firms, while micro, small and medium-sized firms account for about the same share of the volume of job creation and destruction as large firms.

Another evidence that firing costs seem to be perceived as low in Brazil, at least relative to the costs of adjusting average hours, is obtained from a simple decomposition of the variance of (the log of) total hours (hN) into the variance of (the log of) average hours (h), the variance of (the log of) employment (N) and two times the covariance of (the log of) hours and employment. Manufacturing data from *Pesquisa Industrial Mensal* (*PIM, IBGE*), available on a monthly basis from 1985 to 1997, shows that 98.6% of total hours variance is explained by employment variance. These results point to a much greater reliance on employment adjustment when compared to hours adjustment in Brazil.

III.2 Studies on the Determinants of Labor Turnover

Chahad, Orellano and Pichetti (2001) study the joint determinants of quits and dismissals based on a bi-variate probit model and on household data for the *São Paulo* metropolitan region (*Pesquisa de Emprego e Desemprego, PED, SEADE*). As far as I know, this is the only study using individual characteristics and macroeconomic variables as determinants of turnover in Brazil. One of the advantages of the PED data set is that it has information on job tenure for those currently employed as well as on complete employment spells on the previous job for those currently unemployed. The other main household surveys in Brazil, PNAD and PME, only have information on the previous employment spells of the currently unemployed. The main disadvantage of the data set for the purposes of this paper is that information on employment duration starts in February

[20] These numbers are smaller than those found in RAIS data set because i) they refer to the industrial sector only, ii) they refer to establishments that existed throughout the period, and iii) the sample of firms in the PIA data set is based on the 1985 Industrial Census which over represents large firms.

1988, which hurts the analysis of the 1988 Constitution impact. The analysis in their paper is carried out separately for five major economic sectors.

The results conform to expectations. The authors show that education reduces the probabilities of dismissals in all sectors and the probability of quits in the industrial sectors. Having a working card (or equivalently, being a formal worker) reduces both probabilities of separations; job tenure also reduces both probabilities; gender is not significant in most regressions; and age reduces both the probability of quits and dismissals. Unemployment increases the probability of dismissals and reduces the probability of quits; and GNP changes are significant with the correct sign only for manufacturing. The Constitution dummy (after December 1988) was found to increase the probability of dismissals, a result contrary to what was expected and that might be explained by not having enough information before the Constitutional change.

III.3 Studies that analyze the perverse incentives and the 1988 Constitutional change

Barros, Corseuil and Bahia (1999) analyze data on complete employment spells on the previous job for those currently unemployed from *Pesquisa Mensal de Emprego (PME*, IBGE), a monthly household survey that covers the six main metropolitan regions in Brazil.[21] The data has information on employment duration of the previous job, reason for separation (layoffs or quits), on whether it was a formal or an informal job (with or without a working card), and on whether the individual withdrew her FGTS balance (this question was only asked for formal workers).

Since most empirical exercises performed in the next section are inspired on this paper and are based on the same data set, we discuss it in more detail. The purpose of the study is to test whether the 1988 Constitution (and in particular the increase in the fine from 10 to 40%) affected labor turnover. This was obtained using a differences-in-differences methodology that compare employment turnover measures before and after the Constitutional change for control and treatment groups.[22]

As choices for the control group, supposedly not affected by the regulatory changes, they considered three options. The first control group used consists of workers whose previous employment tenure lasted less than three months, the so-called very short employment spells workers. As discussed above, these workers do not bear any firing costs, since tenures up to three months are considered as probationary periods. Note that these workers still have access to the

[21] The use of employment spells data reported by those that are currently unemployed provide adequate measures of employment duration under the hypotheses that the economy is in steady-state and that the duration of employment and unemployment spells are independent (for a thorough discussion on this, see Barros, Corseuil and Bahia, 1999).

FGTS deposited (in case of unjustified dismissal), but the firm does not have to pay the fine or to give them any advance notice.

The second choice for control group is composed of the informal workers, those employees that do not have a working card. This is also a natural choice, since these workers are obviously not directly affected by the legislation change.

The third control group consists of those workers that quit their previous job. This group was chosen because, as discussed, they have no right to withdraw their FGTS balance or to receive the job termination fine. However, the discussion from the previous section indicates that this is not an adequate control group, because the increase in the fine for unjustified dismissals from 10 to 40% should have made it harder for workers to make agreements with their employers. This, in turn, should have affected the number of people who quit. Consequently, it is expected that this control group be directly affected by the legislation changes, which makes it a non-suitable choice for a control group. In the next section we show that, indeed, the proportion of workers that voluntarily quit and receive FGTS *changed* with the legislation modifications.

Barros, Corseuil and Bahia arbitrarily chose 1986-87 and 1991-92 for representing the pre- and post- Constitution periods to be analyzed. They show that hazard rates dropped significantly, specially for short tenures (3 to 6 months). However, since the years of 1991-92 are very weak economic activity years, comparisons with the three control groups had to be made.

Since the hazard rates are aggregate measures that can only be computed for each month and for each metropolitan region, this is the only source of variation they could use. Consequently, the differences-in-differences analysis was performed by comparing the averages of the two periods, across months and regions within the two periods.

The results point to a significant drop in hazard rates for short employment spells relative to control groups for all three choices of control groups. The authors conclude that the drop in hazard rates observed for workers with short employment spells could be attributed to the 1988 legislation change, as expected by theory. However, the results for the other spells (6-12, 12-24 months) were mixed, depending on the choice of control groups.

Carneiro and Ramos (2002) present time series econometric evidence of a structural break after 1990 in a monthly labor turnover measure, based on administrative files from the Ministry of Labor and Employment (*Cadastro Geral de Empregados e Desempregados*, *CAGED*) available for

[22] The authors use as a measure of turnover the aggregate hazard rate - the probability that an employment relationship that has already lasted a certain number of months will be terminated next month,.

the 1985-2001 period. They use the minimum of hirings and separations divided by the previous employment level as a measure of turnover. They show that labor turnover decreased after 1990, even when one corrects for the business cycle (using regional unemployment rates as controls). The study also finds that labor turnover is procyclical, with regressions of labor turnover on lagged unemployment rates for each sector generating significantly different from zero coefficients ranging from –0.24 to –0.14.[23]

The authors interpret the result as evidence that the argument that FGTS promotes employment turnover is flawed. However, according to the discussion of the previous section, the increase in the fine should imply exactly what was found – a decrease in turnover – since it makes agreements between firms and workers harder to be implemented. Therefore, we interpret their results as evidence that the increase in dismissal costs brought less turnover, as expected.

IV. New Evidence on the Link between Labor Turnover and Labor Legislation in Brazil

IV.1 Data

The data source used in this section is taken from *Pesquisa Mensal de Emprego – PME*. PME is a monthly household survey covering the six main Brazilian metropolitan regions: Belo Horizonte, Porto Alegre, Recife, Rio de Janeiro, Salvador, and São Paulo. The survey is conducted by IBGE (Brazilian Census Bureau), interviewing about 38,500 households every month. It has information on the usual demographic and labor market indicators. For each individual, we use information on education, age, gender, region, and labor market status (employed, unemployed or out of the labor force).

If the individual is employed, there is information on whether she is working in the formal or informal sector, the number of hours worked, monthly earnings, sector of activity and occupation. If she is currently unemployed, there is data on her unemployment duration and the characteristics of the previous job, including employment duration, reason for job termination, and whether she received her FGTS balance. The sample period used runs from 1982 to August 2002.

IV.2 Average employment duration

Table 1 presents the average duration of employment spells from 1982 to 2002 for several groups of workers in the data set (formal/informal, layoff/quit, and breaking by receiving or not the FGTS balance). For all disaggregations, we also show the average duration for those with tenures of

[23] More specifically, regressions were run for each major sector, from 1985:2 to 2001:9, including a constant, seasonal dummy variables, a dummy for the period after 1990, one lag of the dependent variable and one lag of unemployment

less than 3 months (1/4 year) and more than 3 months. The table shows that employment duration on the previous job increased over time from 1.3 years in 1982 to 2.0 years in 2002 when one considers all unemployed workers in the sample. Average duration of very short employment spells remained almost constant, slightly increasing from 0.09 in 1982 to 0.10 years in 2002. In fact, average duration of very short employment spells displayed very little variation across time for all groups of workers, independently of disaggregation. By contrast, average tenure for all other workers tended to increase over time for all groups of workers and to show pronounced variation across time.

Figures 2 to 4 plot the average employment duration of unemployed workers by reason of separation (quits and layoffs), considering the full sample, formal and informal workers, respectively. Aggregate unemployment from the same data set is also included in all figures to get a sense of the state of the economy. The graphs illustrate better the different tendencies of rising tenures by reason of separation and reveal some cyclical swings, with average duration of employment spells increasing in good times, apparently somewhat more in the case of quits.

Figure 2 shows that there has been a convergence of employment duration by reason of separation. People that quit their previous job used to have shorter previous employment duration in the 1980s and early 1990s, but employment spells converged to the level observed for laid off workers, of about 2.0 years in 2002. Figures 3 and 4, however, suggest that this convergence is reflecting a composition effect. Figure 3 shows that the average employment duration of formal workers has always been higher for those that were laid off than for those that quit, while Figure 4 shows that the contrary is observed for informal workers: average employment tenure for those that quit is higher than for those that were laid off. Since a rise in the proportion of informal workers was observed over time, the weight of informal workers in the total sample increased, which explains the convergence found when we take the average for all workers. Nonetheless, average tenures increased for every sub-group in the sample (formal/informal – quit/layoff) over time.

Figure 5 shows how employment tenure of formal workers varies over time considering also the information on receiving or not the FGTS balance. Figures 5.1 to 5.3 contain the same information by level of education (three levels of education were used: 0-4, 5-10, 11 or more years of study). The figures reveal another interesting pattern: formal workers that withdrew their FGTS balances have longer tenure than those that have not, independent on whether they quit or were fired. This may be reflecting the fact that the amount in the FGTS accounts increases

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as explanatory variables. Regressions for manufacturing displayed the highest coefficients (in absolute value) for the structural change and for the cycle indicator.

proportionately with tenure and, consequently, workers will be more aware of it the longer they were in the job. Also note that the average employment duration of workers that were laid-off and did not receive their FGTS balances is high and increased from 1.4 years to 2.4 years over time. This group includes formal workers on probation that did not have the right to receive FGTS, workers that were in firms that did not properly deposit FGTS in the workers' accounts (this cases usually end up in labor courts), and workers that were fired for "justified reasons" (a small proportion of those fired). Figures 5.1 to 5.3 illustrate similar patterns of average employment duration for the three education groups. The figures also confirm the positive relationship of job tenure and education found in other studies.

IV.3 Number and proportion of unemployed workers in each category

Figure 6 displays the average number of unemployed workers who had a previous formal job in each of the four categories: quit or layoff, received or not the FGTS balance, for each year from 1982 to 2002 (the numbers for 2002 refer to the average from January to August). The Figure reveals that: i) most workers were fired and received their FGTS balances; ii) the number of laid off workers that received FGTS is counter cyclical and varies significantly with the economic cycle; iii) the number of unemployed workers that were fired and have not received their FGTS is fairly constant across the sample period. Figure 7 and Table 2 confirm these findings, by showing the proportions of unemployed workers with a previous formal job, also by reason of separation and by the FGTS withdrawal indicator.

IV.4 Differences-in-differences analysis of the effects of legislation changes on employment duration: identification strategy

The purpose of this sub-section is to study the effects of increases in dismissal costs mandated by the 1988 Constitution and the September 2001 Law 110 on average employment duration of the previous jobs of those currently unemployed. We use individual data from PME (described in Sub-Section *IV.1*) from 1982 to August 2002. The strategy is based on a differences-in-differences methodology that compares average employment duration for the periods before and after the legislation changes, for control and treatment groups. The idea is to test the prediction from the discussion in Section II that both legislation changes should have decreased labor turnover (or, equivalently, increased employment duration) for the treatment groups relatively to control groups.

As choices for the control groups, we use two of the three control groups proposed by Barros, Corseuil and Bahia (1999): informal workers and workers with employment spells lower than three months - workers on probation (see Sub-Section *III.3*).[24]

The main differences of our work from Barros, Corseuil and Bahia (1999) are: i) we use employment duration data directly, instead of aggregate hazards; ii) we use individual level data, which allows us to control for observed characteristics and to interact some characteristics with the legislation changes dates; iii) we use the full sample before and after the Constitution and not arbitrary years;[25] and iv) we also analyze the implications of the more recent period legislation changes (Law 110 of September 2001) on labor turnover.

The differences-in-differences estimator of the effect of the legislation changes (Constitution and Law 110) on employment duration are given, respectively, by the coefficients γ_2 and γ_4 in the following regression (Kugler, 2000):

$$d_{it} = \alpha + \beta X_{it} + \gamma_o Treat_{it} + \gamma_1 8901_{it} + \gamma_2 Treat_{it} * 8901_{it} + \gamma_3 Post01_{it} + \gamma_4 Treat_{it} * Post01_{it} + \delta Y_t + u_{it}$$

where *i* indexes each individual, *t* indexes each month, *d* is employment duration, *X* is a vector of observed characteristics (gender, age, education, sector, and metropolitan region), *Treat* is a dummy for the treatment group, *8901* is a dummy for the Constitutional change period, which equals one between January 1989 and September 2001, *Post01* is a dummy for the period after Law 110 came into effect, which equals one from October 2001 on, and *Y* is a vector of aggregate variables (unemployment rate, inflation, and a measure of openness). Seasonal dummies are also included in all regressions, which are run with and without controls for observable characteristics and aggregate variables.

Given the discussion of Section II, one should expect that coefficients γ_2 and γ_4 are positive, and that γ_4 is larger than γ_2, that is, average employment duration of the treatment groups should have *increased* relative to the control groups both after the Constitutional change and the implementation of Law 110.

There are a number of caveats that should be made before presenting the results. First, since this is a non-experimental study, estimated coefficients are subject to selection bias. Since people self-select to control and treatment groups, these are groups likely composed of people with different characteristics that could react differently to other factors that happened around the

[24] In a previous version of this paper, we also used a control group based on workers that quit and do not withdraw their FGTS balance. However, this group is also directly affected by the legislation changes, since both changes made agreements harder to be implemented.

legislation changes. The use of regressions with controls for observable characteristics and aggregate variables is an attempt to deal with this problem. Under the strong hypothesis that selection is also based on observed characteristics, and on a rigid linear parametric way, including these observed characteristics in the regression would lead to unbiased estimates. Since this is a strong hypothesis, some selection bias might still be present even after controlling for individual characteristics.[26]

Table 3, nonetheless, presents some summary statistics that describe basic characteristics of treatment and control groups before and after the legislation changes. Panel A displays the statistics for formal and informal workers, while Panel B presents data for long duration and short duration workers. Note that the composition of both pairs of treatment/control groups changed over time.

Panel A shows a decrease in the proportion of formal workers from 59.2 to 55.6% across the three periods. Formal workers are more educated, older, more likely to be male, and have a larger presence in the manufacturing sector than informal workers. However, the changes in characteristics across the three periods were very similar for the two groups. Over time, one can observe a decrease in the proportion of men, an increase in the proportion of those with more schooling, and an increase in average age in both groups (formal and informal workers). The only change was in sectoral allocation. There was a significant decrease of formal workers in manufacturing and a significant increase in services over time, while the share of informal workers in the manufacturing sector remained almost constant and there was only a slight increase in services.

Panel B shows that the size of the control group composed of workers with short tenure is much smaller and decreased across the three periods. The two groups (short and long duration workers), however, are much more similar to each other and their basic characteristics moved in the same direction over time.

The information presented in Table 3 makes us much more confident on the empirical strategy, since they suggest that the compositions of the two pairs of treatment/control groups moved in very similar ways across the three periods. Nonetheless, they suggest that controlling for observable individual characteristics is essential for correctly identifying the legislation changes effects.

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[25] We also chose different dates around the legislation changes for robustness, and the results are almost the same.

[26] Note that control groups could be at least indirectly affected by the legislation changes, especially when one takes into account general equilibrium effects. Kugler (2000) constructs a model to study general equilibrium effects of dismissal costs on formal and informal sectors (see also Barros, Corseuil and Bahia, 1999).

The use of aggregate variables is also intended to control for cyclical and structural changes that happened in the Brazilian economy around the legislation change periods that could have differently affected treatment and control groups, contaminating the identification of the dismissal cost effects. First, we control for unemployment, since turnover measures tend to display cyclical movements that are different for treatment and control groups as shown in Sub-Section IV.2.

Trade liberalization is another important change that occurred around the implementation of the 1988 Constitution.[27] We analyze its effects on employment duration by using a proxy for the degree of openness of the Brazilian economy, measured by the sum of imports and exports over GNP. We also interact the treatment dummies with the post legislation change dummies and sector dummies, in order to check whether the differences-in-differences estimators in tradable sectors (manufacturing) are different from non-tradable sectors.

Finally, we also include the aggregate inflation rate in the controlled regressions. Among the many possible effects of inflation on employment turnover, the discussion in Section II pointed to the fact that inflation acceleration tends to lead to a decline in the real purchasing power of FGTS balances, which should increase the desire of those that had plans of leaving a job to make efforts to get fired in order to withdraw the fund.

Nonetheless, we should warn that other structural changes not considered or inadequately measured here might still affect the results. In particular, we should note that there was a significant upgrade in 1990 of the existing unemployment insurance program created in 1986. Formal workers that became eligible to receive unemployment benefits would have an additional incentive to make agreements with their employers to get fired in order to collect them. Note, however, that this has the effect of increasing turnover rates. This occurs because this additional incentive to get fired is not offset by an increase in the degree of resistance of employers to make agreements with workers, since the unemployment insurance fund is financed by revenue taxes and does not depend on turnover rates. This is the opposite of the predicted effect of the dismissal cost increases implemented through the 1988 Constitution. Finally, note that the timing of the program expansion is different from the timing of the 1988 Constitution promulgation, and of course, very distant from the 2001 legislation change.

[27] Trade liberalization was implemented in Brazil first through a round of removal of non-tariff barriers in 1988-89 followed by an aggressive program of import tariff reduction, especially between 1990 and 1995.

IV.5 Differences-in-differences analysis: results

Tables 4 and 5 contain the results of the differences-in-differences analysis. Each Table presents the results of the regression estimation for one choice of control group: informal workers in Table 4, and short duration workers in Table 5. Each table has five columns. The first and second columns present, respectively, the estimation results without and with controls for observable characteristics and aggregate variables. The third, fourth and fifth columns present the results of including interactions of the reform changes effect term (treatment dummy interacted with post legislation changes dummies) with gender, education and sector dummies, respectively. The idea is to identify differential effects of the legislation changes for disaggregations of these variables. Standard errors are in italic below each coefficient.

The results in Table 4 show significant coefficients and expected signs for most of the variables included. Column (1) presents the results for the unconditional regressions. The differences-in-differences estimators γ_2 and γ_4 are 0.412 and 0.491, respectively, both significantly different from zero and estimated with small standard errors. Note that the effect of the more recent legislation change on increasing employment duration (γ_4) was found to be larger than the coefficient on the interaction of the formal dummy with the January 1989-September 2001 dummy (γ_2) that captures the Constitutional change effects. This implies that the more recent legislation change increased the average employment duration of formal workers relative to informal workers when compared with the previous period. Both results are expected from the previous discussion.

When controls for observed individual characteristics and aggregate variables are included in the regression (Table 4, column 2), the differences-in-differences estimators γ_2 and γ_4 dropped to 0.362 and 0.402, significantly lower than before but still representing a sizable increase in employment duration after both legislation changes. These results confirm that the increase in dismissal costs implied by the two legislation changes reduced turnover even when controlling for other macroeconomic changes and individual characteristics.

The three aggregate variables included in the regression are significant. The unemployment rate is found to positively affect employment duration, implying procyclical employment turnover, a typical finding in the literature. The inflation rate coefficient is negative and significantly different from zero, as expected from the discussion of the previous sub-section. The measure of openness is positively correlated with employment duration. Since the 1990s were characterized by higher unemployment, lower inflation and more openness, part of the increases in employment duration observed in the last decade are attributed to these effects, reversing the sign of the coefficients of the post legislation changes dummies alone (1989-2001 and Post 2001).

Table 4, column (2) also shows the effects of individual characteristics on employment duration. The results are also expected: male workers have longer employment tenures; employment duration significantly increases with education (workers with complete high school or more, for instance, have tenures 0.73 years longer than those with 0-4 years of study, the omitted group); employment spells tend to increase with age up to a certain point (65 years); the ranking of employment tenures places the construction sector in last position, then commerce, services, manufacturing and others (the omitted sector); and the more developed regions display longer tenures, even controlling for other observables, with the exception of Porto Alegre, which have the lowest employment duration.

Table 4, column (3) interacts the legislation changes effect term with gender. It reveals that the effect of the Constitutional change on average duration of formal workers was higher for male workers, and that there was no significant gender difference of the most recent legislation change.

Table 4, column (4) shows that the Constitutional change effect was also higher for the more educated workers, when compared with the low-educated workers, and much higher when compared with workers with intermediate-level of education. The 2001 legislation change, on the other hand, was found to significantly decrease the employment duration of workers with intermediate-level education with respect to the low-education group.

Column (5) of Table 4 presents the results of interacting the legislation changes effect term with sector. Note that the Constitutional change increased employment duration of formal workers relative to informal workers especially in the manufacturing sector. Coefficients in the other sectors and interacting with the 2001 legislation change indicator are not significant.

Similar results are obtained in Table 5, when workers with short duration (less than 3 months of tenure) are used as control group. The differences-in-differences estimators γ_2 and γ_4 in the unconditional regressions (Column 1) are 0.532 and 0.699, both significantly different from zero. The regressions with controls for observed characteristics and aggregate variables (Table 5, column 2) produce differences-in-differences estimators, γ_2 and γ_4, of 0.467 and 0.660. These results are evidence that the increase in dismissal costs implied by the two legislation changes increased employment duration of the affected group (workers with more than 3 months of tenure) relative to the control group, controlling and not for aggregate variables and individual characteristics.

The coefficients of the three aggregate variables and of all individual characteristics variables in Table 5 are very similar and have exactly the same signs of those presented in Table 4.

The fact that the size of this control group is much smaller than the previous one makes the coefficients on the interactions with disaggregations of gender, schooling and sector much less precisely estimated. The results of interactions of legislation changes terms with gender, schooling and sector (columns 3 to 5) show only significant effects for the disaggregations of schooling (column 4), which are very similar to those obtained for the previous control group: the Constitutional change effect was found to be higher for the more educated workers, when compared with the low-educated workers; and employment duration of workers with intermediate-level of education in the treatment group relatively decreased after both legislation changes.

IV.6 Differences-in-differences analysis for the probability of quitting and receiving FGTS

The idea of this sub-section is to exploit the temporal variation of the proportion of fake layoffs around the Constitutional change and the September 2001 periods in order to test whether the incidence of this type of agreement decreased after the implementation of the fine increases predicted by the two legislation changes. Table 6 presents the results of logit regressions based on data on the proportion of workers making fake layoff agreements presented in Table 2 and Figure 7. In particular, we run a logit regression, where the dependent variable is a dummy variable that takes the value of 1 when the formal worker quit and received the FGTS. The value of zero is assigned to the other formal workers (workers that quit and did not receive their FGTS balances and all workers that were laid off). Since people that quit do not have the right to withdraw their FGTS balances, the dependent variable is an indicator of a fake layoff.

Note that since PME is a household survey, the answer on reason for job termination is given from the perspective of the worker. The answers on some particular questions can be subjective. As discussed in Section II, the check on whether the worker received or not the FGTS balance is useful for inferences about the frequency of fake layoff agreements, but is an imperfect one. It should be expected that it works better for the non-litigious cases described in Section II.3 in which the firm agrees to fire with just cause. However, in the litigious case, in which the worker that wants to leave forces his/her own dismissal to receive the FGTS balance plus the penalty, it is not clear what he/she would respond in the survey. Consequently, one should expect that at least some workers that answer that they were laid off and received the FGTS balances were workers that wanted to leave and forced their own dismissal. This means that they were also affected by the legislation changes. Therefore, a note of warning should be used when interpreting the results of this sub-section.

The results in Column (1) of Table 6 show that both dummies representing the post legislation change periods are significant and negative. The coefficient on the dummy for the post-

September 2001 period is larger, in absolute value, than the one for the post-Constitutional change period. Both results are expected from the previous discussion, suggesting that the incidence of fake layoff agreements decreased after both legislation changes.

The regression also controls for the unemployment rate and individual characteristics. The results show that the probability of a fake layoff agreement is pro-cyclical, decreasing with the unemployment rate, meaning that in tough times, it is harder to make this kind of agreement. The coefficients on individual characteristics show that the probability of a fake layoff: i) is lower for male workers; ii) increases with years of schooling; iii) decreases with age; iv) is lower in construction and manufacturing; v) is higher in Belo Horizonte, Porto Alegre and São Paulo than in other metropolitan regions.

The result that those with more schooling are more likely to quit and receive their FGTS balances might be interpreted in two ways. On the one hand, since there is a high correlation between wages, schooling and employment duration, those with more years of schooling have probably more in their FGTS accounts, which implies that they should care more for withdrawing it. The other interpretation is that those with more years of schooling are more likely to occupy better jobs, closer to the high hierarchy of the firm, which increases their degree of persuasion that that they would not break the agreement.

Column (2) includes the interaction of the two dummies for the post-legislation change periods with years of schooling. The result shows a negative and significant effect in both periods, meaning that those with more schooling were more affected by the legislation changes, reducing more the proportion of fake layoff agreements.

V. Conclusions

This paper studied the links between labor legislation and labor turnover in Brazil. We presented the main features of the Brazilian labor legislation, with emphasis on those measures that affect labor turnover. In particular, we analyzed in detail the argument of many authors that the design of some job security provisions in Brazil creates perverse incentives that end up generating more labor turnover. We concluded that the design of the FGTS system not only creates labor turnover, but is also inefficient and a source of conflict between firms and workers. We also note some implications of these arguments that were overlooked in the previous literature and that could be tested in the data.

The two main features of the FGTS system that create perverse incentives are the below-market interest rates and the fact that the firing penalty is paid to the worker. Those features create

the desire of workers to access their FGTS accounts and the possibility of negotiation between employers and employees about not paying the penalty. Therefore, the main measures to be included in any FGTS overhaul package should revert these two main features.

In particular, a proposal would be to set returns on FGTS accounts balances that increase proportionately with employment tenure, so as to reward longer employment spells. Second and most important, the firing fine should not be paid directly to the employee. The FGTS system is already designed to provide a reasonable insurance for those that lose their jobs, although most would prefer to make private arrangements. The firing fine, however, is a big distortion. A proposal that still preserves dismissal costs should consider increasing the proportion that goes to the government (g, according to our notation) and decreasing the amount paid to the employee (f), preferably to zero.

After reviewing the most recent evidence on labor turnover in Brazil, which confirm the very high turnover rates, especially for the less educated, the paper used two episodes of increases in the firing penalty to empirically identify the effects of dismissal costs on labor turnover in Brazil. Using a simple differences-in-differences methodology applied to monthly individual data from *Pesquisa Mensal de Emprego (PME, IBGE)*, which has information on previous employment spells for those currently unemployed, we studied the effects of increases in job termination costs implemented in the 1988 Constitution and in a Labor Law of September 2001 on employment duration.

The methodology exploited the fact that those changes should have had different effects for different groups of workers. The exercises establish that, in fact, both changes reduced turnover for formal workers affected by the legislation. A significant increase in average employment duration of affected workers relative to control groups of workers was observed after both legislation changes.

We also provided evidence that both legislation changes reduced the probability of fake layoffs, although there are still a high number of such agreements being made between workers and their employers.

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Figure 1
Dismissal Costs

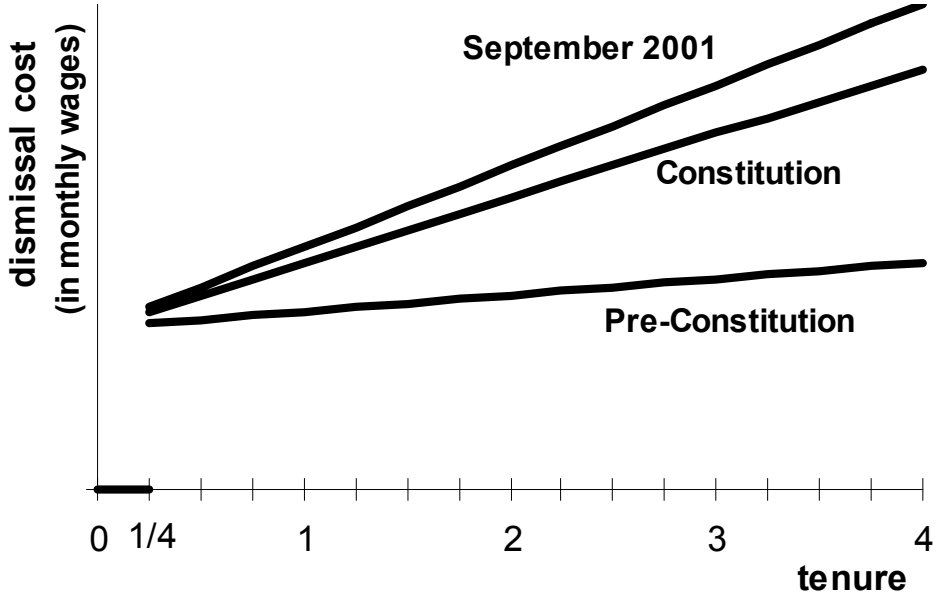

Figure 2
Average Duration of Employment

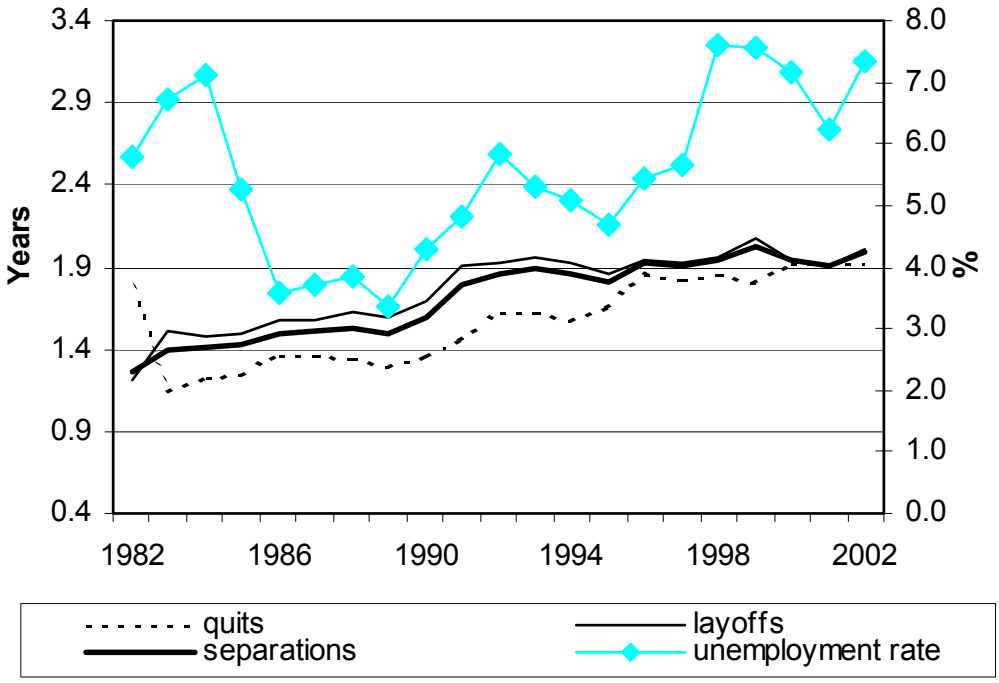

Figure 3
Average Duration of Employment – Formal Workers

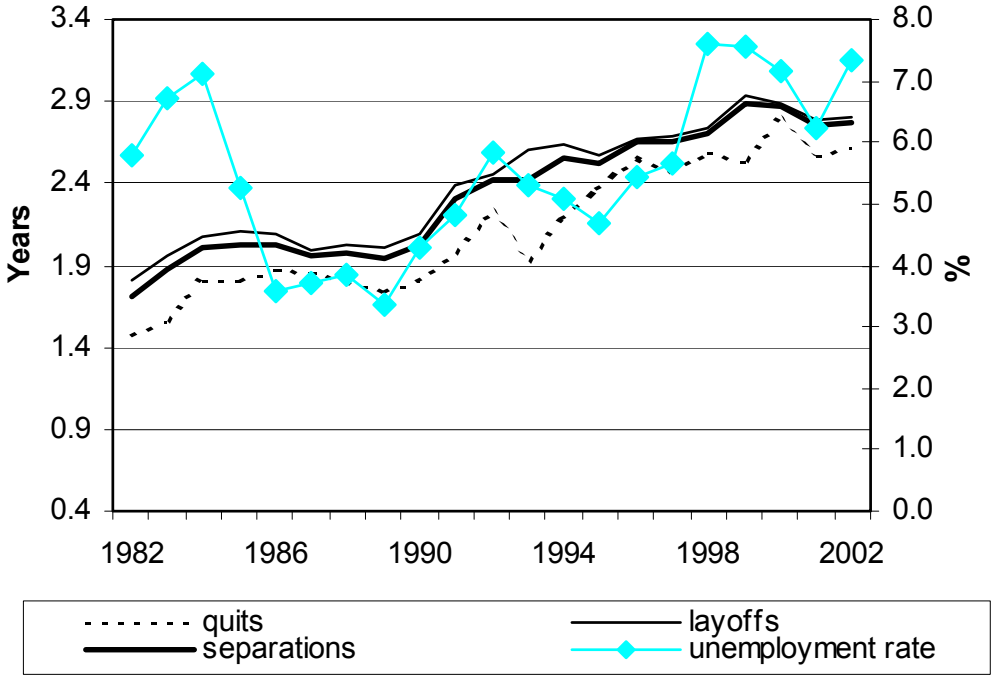

Figure 4
Average Duration of Employment – Informal Workers

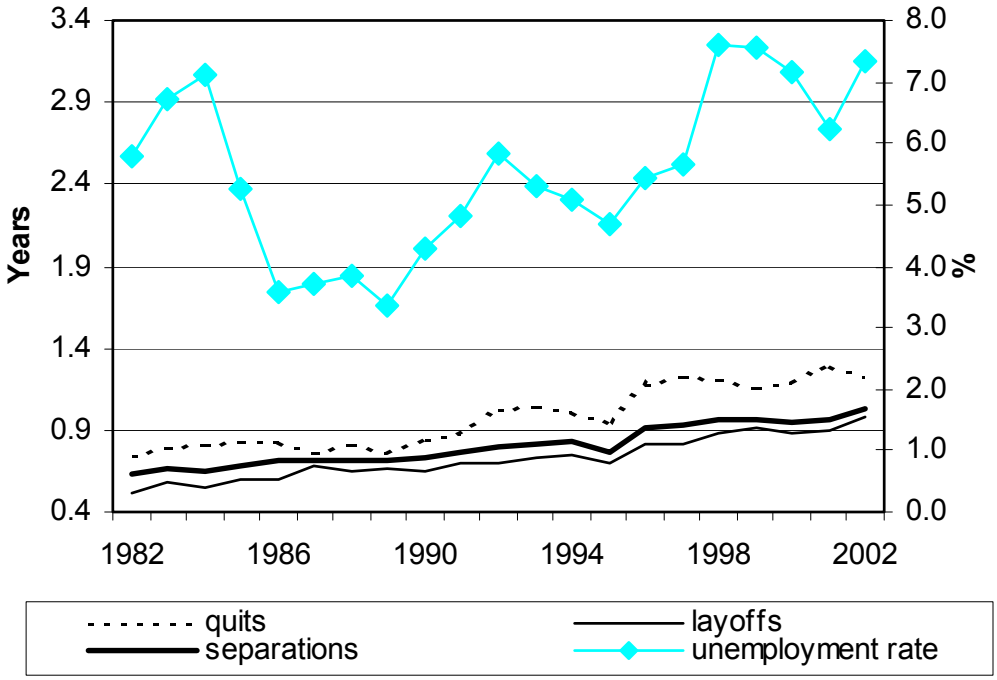

Figure 5
Average Duration of Employment

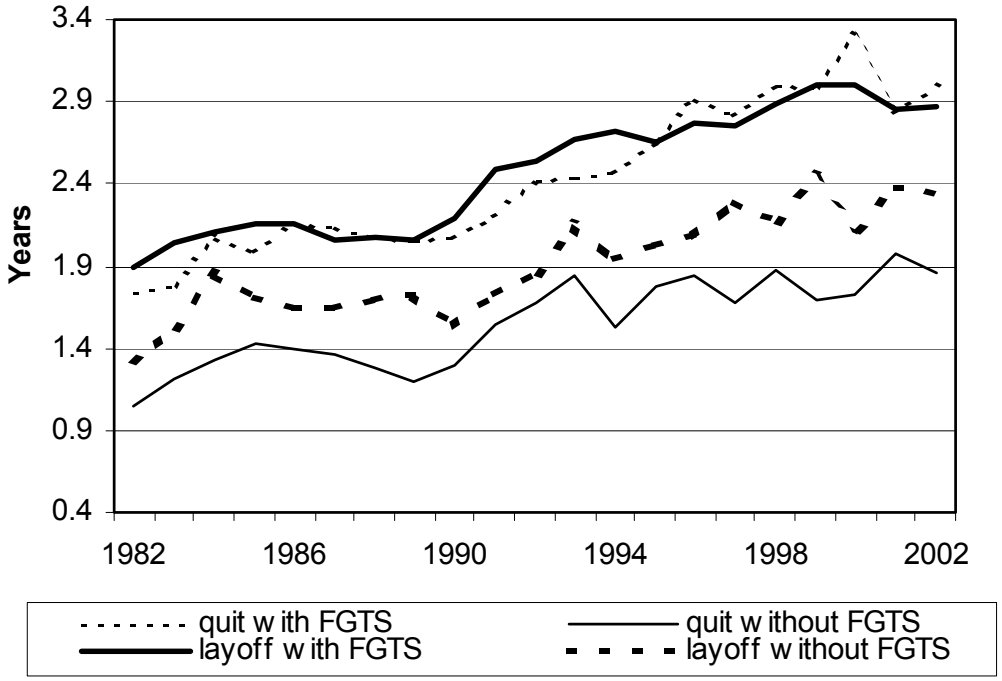

Figure 5.1
Average Duration of Employment
(0 to 4 years of schooling)

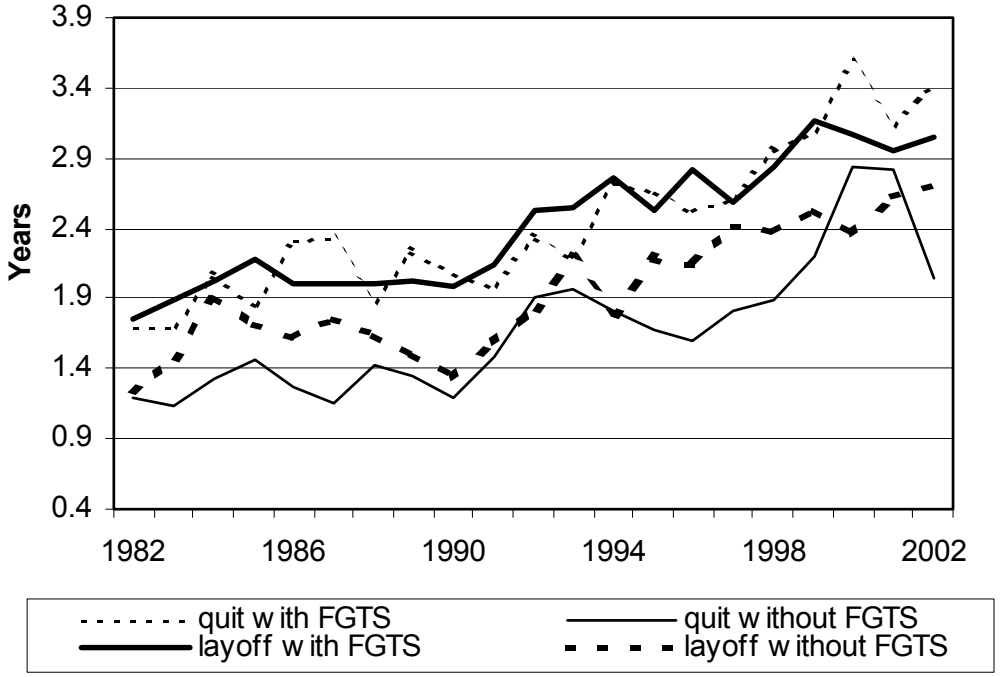

Figure 5.2
Average Duration of Employment
(5 to 10 years of schooling)

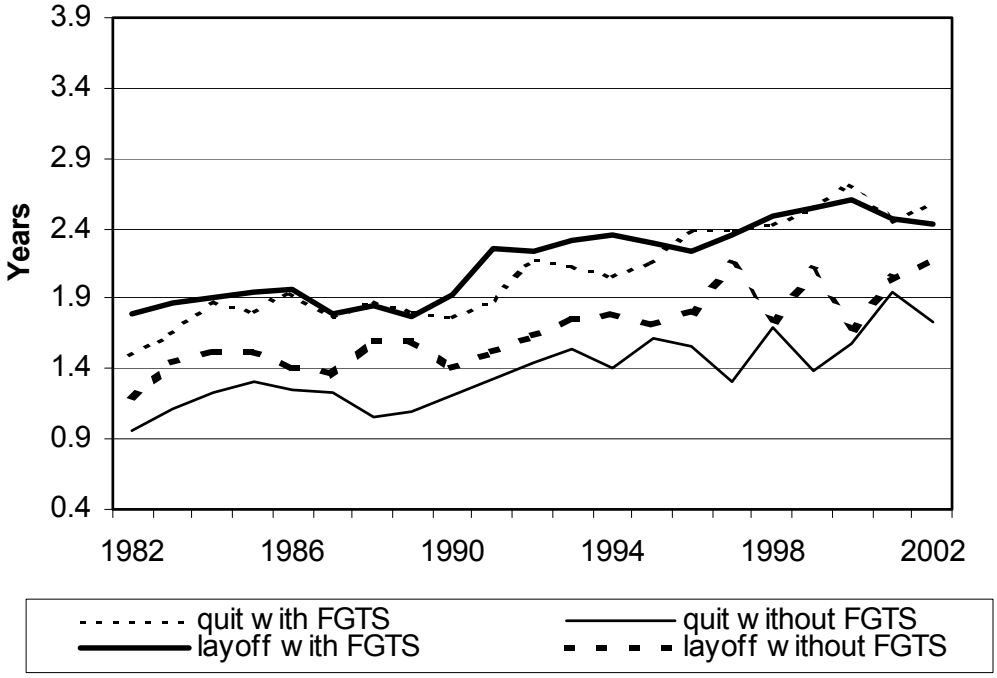

Figure 5.3
Average Duration of Employment
(11 and more years of schooling)

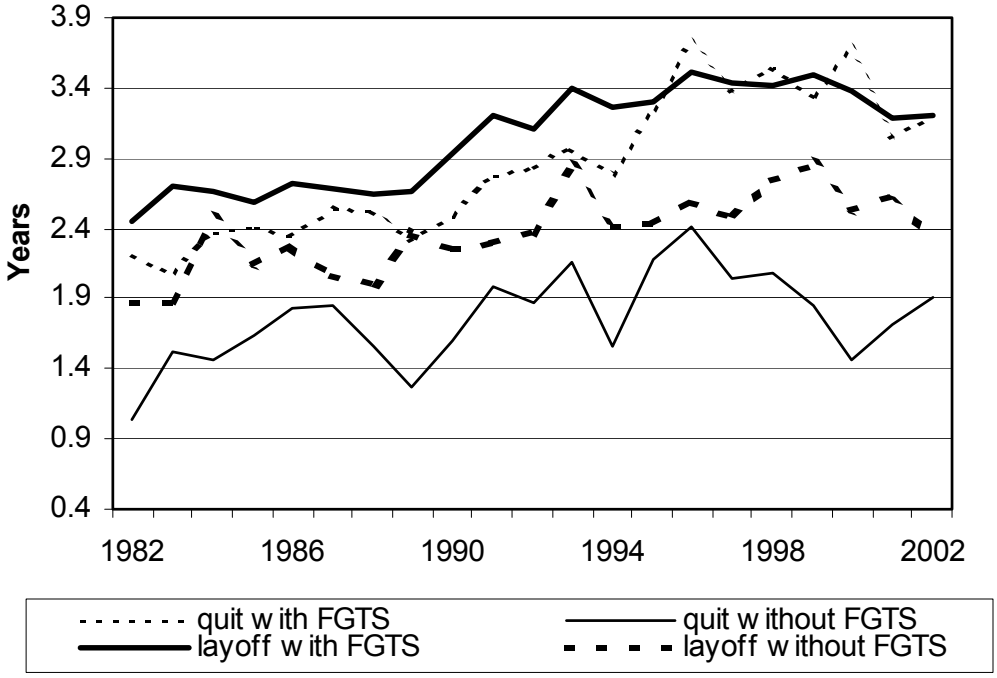

Figure 6: Number of Unemployed Workers by Reason of Separation and FGTS Withdrawal
(Layoff with FGTS: 1982=100)

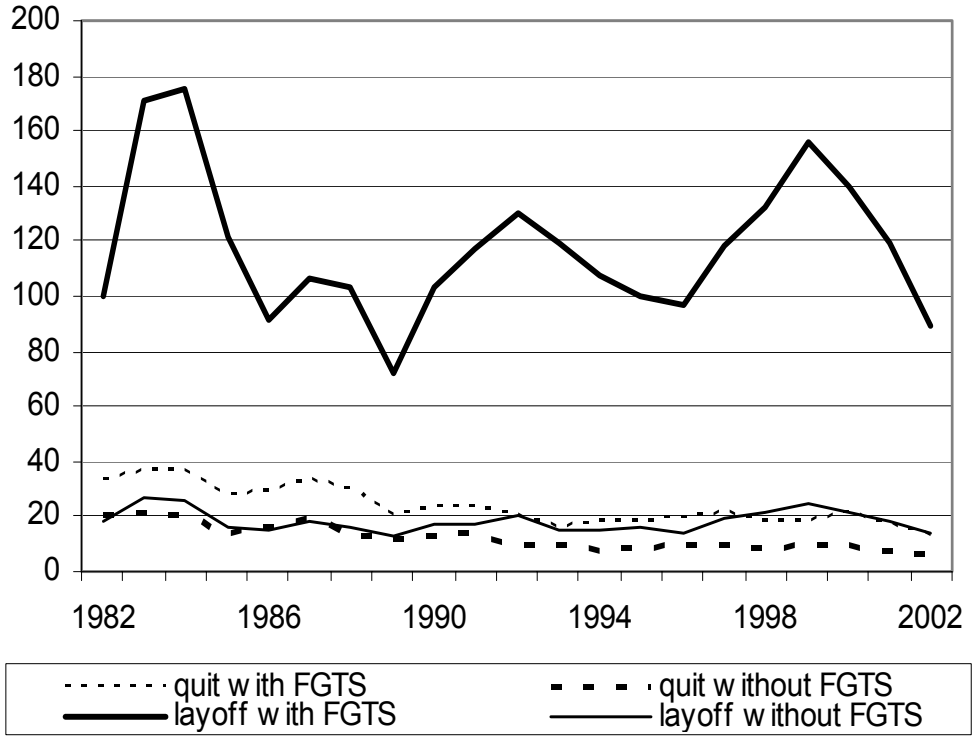

Figure 7: Proportions of Unemployed Workers by Reason of Separation and FGTS Withdrawal

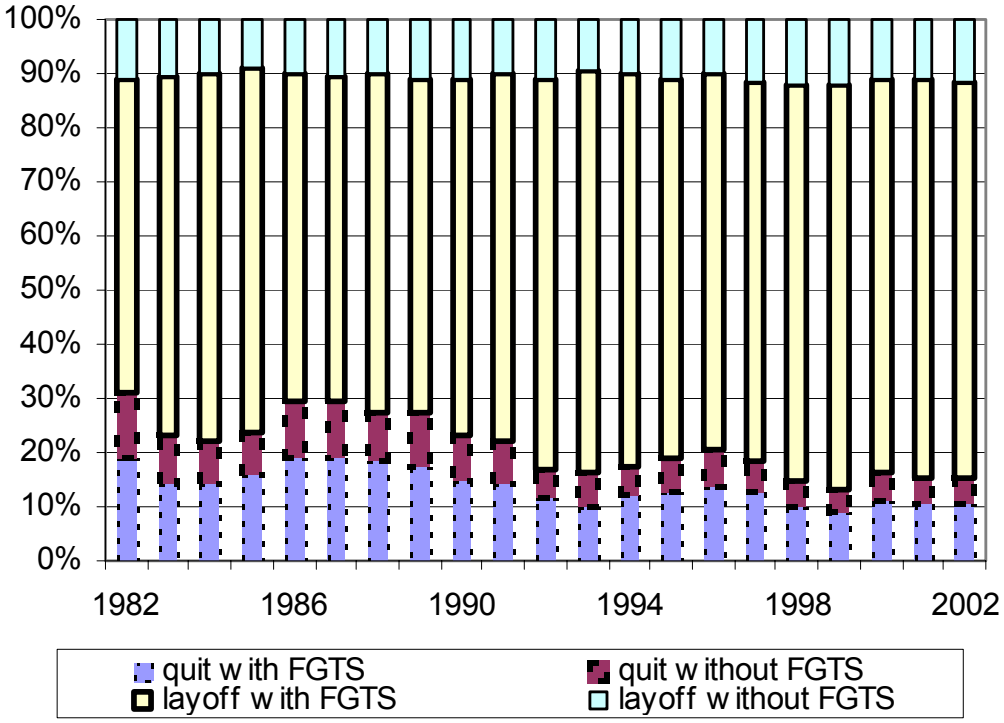

Table 1
Average Duration of Employment (years)

	1982	1983	1984	1985	1986	1987	1988	1989	1990	1991	1992	1993	1994	1995	1996	1997	1998	1999	2000	2001	2002
All																					
0-1/4	0.09	0.08	0.08	0.09	0.09	0.10	0.10	0.09	0.10	0.10	0.09	0.09	0.09	0.09	0.10	0.10	0.10	0.10	0.09	0.09	0.10
>=1/4	1.69	1.79	1.86	1.85	1.91	1.86	1.87	1.82	1.92	2.13	2.16	2.24	2.22	2.18	2.29	2.25	2.29	2.38	2.32	2.26	2.33
Total	1.27	1.39	1.41	1.42	1.50	1.51	1.53	1.49	1.59	1.78	1.84	1.88	1.85	1.81	1.92	1.91	1.93	2.03	1.94	1.91	2.00
Quit (all)																					
0-1/4	0.09	0.09	0.09	0.09	0.10	0.10	0.10	0.10	0.10	0.10	0.10	0.10	0.09	0.10	0.10	0.10	0.10	0.11	0.11	0.10	0.11
>=1/4	1.52	1.51	1.65	1.63	1.75	1.75	1.68	1.62	1.68	1.80	1.92	1.93	1.88	2.02	2.23	2.16	2.18	2.11	2.26	2.24	2.20
Total	1.09	1.14	1.23	1.24	1.35	1.37	1.34	1.29	1.36	1.46	1.61	1.60	1.56	1.66	1.86	1.83	1.85	1.81	1.93	1.92	1.93
Layoff (all)																					
0-1/4	0.08	0.08	0.08	0.09	0.09	0.09	0.10	0.09	0.10	0.09	0.09	0.09	0.09	0.09	0.09	0.10	0.09	0.09	0.09	0.09	0.10
>=1/4	1.79	1.91	1.94	1.94	1.99	1.92	1.95	1.92	2.02	2.25	2.23	2.32	2.31	2.22	2.30	2.28	2.32	2.43	2.34	2.27	2.35
Total	1.38	1.50	1.48	1.50	1.58	1.58	1.62	1.59	1.69	1.90	1.91	1.96	1.93	1.85	1.94	1.93	1.95	2.07	1.94	1.91	2.01
Quit (formal)																					
0-1/4	0.11	0.10	0.10	0.11	0.11	0.11	0.11	0.11	0.11	0.12	0.10	0.11	0.10	0.12	0.12	0.11	0.12	0.12	0.13	0.11	0.12
>=1/4	1.72	1.78	2.03	2.02	2.13	2.09	2.01	1.95	2.00	2.16	2.35	2.38	2.32	2.55	2.72	2.61	2.74	2.70	2.96	2.75	2.77
Total	1.48	1.57	1.81	1.81	1.88	1.86	1.81	1.74	1.80	1.96	2.19	2.21	2.18	2.37	2.56	2.46	2.59	2.55	2.79	2.57	2.63
Quit (informal)																					
0-1/4	0.08	0.08	0.09	0.09	0.09	0.09	0.09	0.09	0.09	0.10	0.10	0.10	0.09	0.10	0.10	0.10	0.10	0.10	0.11	0.10	0.10
>=1/4	1.24	1.21	1.26	1.25	1.24	1.16	1.18	1.12	1.20	1.25	1.37	1.40	1.37	1.30	1.59	1.63	1.58	1.49	1.53	1.65	1.51
Total	0.75	0.79	0.82	0.83	0.83	0.76	0.81	0.77	0.85	0.88	1.03	1.05	1.01	0.94	1.17	1.23	1.21	1.17	1.19	1.29	1.23
Layoff (formal)																					
0-1/4	0.11	0.12	0.12	0.12	0.12	0.12	0.12	0.11	0.12	0.12	0.12	0.12	0.12	0.13	0.13	0.13	0.12	0.12	0.12	0.11	0.12
>=1/4	2.01	2.15	2.26	2.30	2.29	2.17	2.20	2.17	2.27	2.54	2.58	2.75	2.77	2.70	2.80	2.81	2.86	3.03	3.00	2.92	2.93
Total	1.81	1.96	2.07	2.10	2.09	1.99	2.03	2.01	2.10	2.39	2.45	2.61	2.63	2.57	2.67	2.69	2.73	2.93	2.88	2.79	2.80
Layoff (informal)																					
0-1/4	0.07	0.07	0.07	0.08	0.08	0.08	0.08	0.08	0.08	0.08	0.08	0.08	0.08	0.09	0.09	0.09	0.09	0.09	0.09	0.09	0.10
>=1/4	0.98	1.07	1.04	1.03	1.02	1.09	1.05	1.05	1.02	1.13	1.05	1.23	1.15	1.05	1.19	1.17	1.27	1.30	1.27	1.24	1.33
Total	0.51	0.58	0.55	0.59	0.60	0.68	0.65	0.66	0.65	0.70	0.70	0.83	0.75	0.69	0.81	0.82	0.88	0.92	0.88	0.89	0.98

Table 1 (Continued)

Quit with FGTS																					
0-1/4	0.11	0.11	0.10	0.12	0.12	0.12	0.12	0.11	0.11	0.12	0.10	0.10	0.10	0.13	0.12	0.11	0.12	0.11	0.13	0.11	0.12
>=1/4	1.93	1.95	2.26	2.17	2.35	2.32	2.23	2.22	2.25	2.37	2.58	2.57	2.58	2.77	3.05	2.93	3.13	3.09	3.43	2.98	3.11
Total	1.75	1.78	2.08	2.00	2.16	2.14	2.07	2.05	2.08	2.21	2.43	2.44	2.47	2.65	2.92	2.82	3.01	2.98	3.30	2.83	3.00
Quit without FGTS																					
0-1/4	0.11	0.10	0.10	0.10	0.11	0.10	0.11	0.11	0.12	0.12	0.10	0.11	0.11	0.11	0.11	0.11	0.13	0.13	0.12	0.12	0.13
>=1/4	1.33	1.46	1.58	1.68	1.68	1.63	1.50	1.42	1.50	1.76	1.84	2.04	1.67	2.03	2.04	1.84	2.01	1.87	1.91	2.18	2.02
Total	1.05	1.21	1.33	1.43	1.39	1.37	1.27	1.20	1.30	1.54	1.67	1.83	1.53	1.77	1.84	1.67	1.88	1.69	1.72	1.98	1.85
Layoff with FGTS																					
0-1/4	0.12	0.12	0.12	0.12	0.13	0.12	0.12	0.12	0.12	0.12	0.12	0.12	0.13	0.13	0.13	0.13	0.13	0.13	0.12	0.11	0.12
>=1/4	2.07	2.19	2.27	2.32	2.34	2.21	2.23	2.20	2.34	2.62	2.65	2.79	2.84	2.76	2.89	2.86	2.98	3.10	3.10	2.97	2.98
Total	1.90	2.03	2.11	2.15	2.16	2.06	2.08	2.06	2.19	2.48	2.54	2.67	2.72	2.65	2.76	2.76	2.89	3.01	3.00	2.85	2.87
Layoff without FGTS																					
0-1/4	0.10	0.10	0.11	0.10	0.11	0.11	0.11	0.11	0.11	0.11	0.11	0.11	0.11	0.12	0.12	0.12	0.12	0.12	0.11	0.11	0.12
>=1/4	1.62	1.82	2.22	2.09	1.97	1.90	2.00	1.96	1.84	1.98	2.09	2.37	2.18	2.29	2.34	2.48	2.42	2.61	2.32	2.57	2.57
Total	1.30	1.53	1.85	1.71	1.65	1.64	1.70	1.72	1.55	1.72	1.85	2.15	1.94	2.02	2.09	2.28	2.18	2.44	2.12	2.37	2.34
Formal																					
0-1/4	0.11	0.11	0.11	0.11	0.12	0.12	0.12	0.11	0.12	0.12	0.11	0.1141	0.12	0.12	0.12	0.12	0.12	0.12	0.12	0.11	0.12
>=1/4	1.93	2.06	2.21	2.23	2.25	2.14	2.15	2.11	2.21	2.46	2.54	2.6885	2.69	2.68	2.79	2.77	2.84	2.99	2.99	2.90	2.91
Total	1.71	1.87	2.02	2.03	2.03	1.95	1.97	1.93	2.03	2.29	2.40	2.5442	2.55	2.53	2.65	2.65	2.71	2.88	2.86	2.76	2.77
Informal																					
0-1/4	0.08	0.07	0.07	0.08	0.08	0.09	0.09	0.09	0.09	0.09	0.09	0.09	0.08	0.09	0.09	0.09	0.09	0.09	0.09	0.09	0.10
>=1/4	1.12	1.14	1.14	1.13	1.13	1.12	1.11	1.08	1.10	1.18	1.16	1.28	1.22	1.12	1.31	1.30	1.35	1.34	1.33	1.31	1.37
Total	0.63	0.67	0.65	0.69	0.71	0.71	0.72	0.71	0.73	0.77	0.80	0.89	0.83	0.76	0.91	0.92	0.96	0.97	0.95	0.96	1.02

Table 2: Proportions of Unemployed Workers by Previous Employment Duration, Reason of Separation and FGTS Withdrawal

		1982	1983	1984	1985	1986	1987	1988	1989	1990	1991	1992	1993	1994	1995	1996	1997	1998	1999	2000	2001	2002
Duration (years)																						
0-1/4	Quit with FGTS	0.16	0.13	0.12	0.14	0.16	0.16	0.15	0.16	0.14	0.14	0.12	0.10	0.11	0.10	0.11	0.11	0.09	0.08	0.09	0.11	0.08
	Quit without FGTS	0.22	0.16	0.15	0.13	0.18	0.20	0.16	0.19	0.14	0.16	0.09	0.12	0.09	0.14	0.13	0.12	0.08	0.12	0.12	0.09	0.09
	Layoff with FGTS	0.43	0.52	0.54	0.55	0.49	0.48	0.51	0.49	0.51	0.51	0.56	0.61	0.60	0.53	0.56	0.56	0.54	0.59	0.56	0.62	0.60
	Layoff without FGTS	0.19	0.18	0.19	0.18	0.16	0.16	0.17	0.16	0.21	0.20	0.23	0.17	0.21	0.23	0.20	0.21	0.29	0.21	0.22	0.18	0.23
>=1/4	Quit with FGTS	0.20	0.15	0.14	0.16	0.19	0.19	0.19	0.18	0.15	0.14	0.12	0.10	0.12	0.13	0.14	0.13	0.10	0.09	0.11	0.10	0.11
	Quit without FGTS	0.10	0.08	0.07	0.07	0.10	0.10	0.08	0.09	0.08	0.08	0.05	0.06	0.05	0.06	0.06	0.05	0.05	0.04	0.05	0.04	0.05
	Layoff with FGTS	0.60	0.68	0.70	0.69	0.62	0.61	0.64	0.63	0.67	0.69	0.73	0.75	0.73	0.71	0.70	0.71	0.74	0.75	0.73	0.74	0.74
	Layoff without FGTS	0.10	0.10	0.09	0.08	0.09	0.10	0.09	0.11	0.10	0.09	0.10	0.09	0.09	0.10	0.09	0.11	0.11	0.12	0.11	0.11	0.11
Total	Quit with FGTS	0.19	0.14	0.14	0.16	0.19	0.19	0.18	0.17	0.15	0.14	0.12	0.10	0.12	0.13	0.14	0.13	0.10	0.09	0.11	0.10	0.10
	Quit without FGTS	0.12	0.08	0.08	0.08	0.11	0.11	0.09	0.10	0.08	0.08	0.06	0.06	0.05	0.06	0.07	0.06	0.05	0.05	0.05	0.05	0.05
	Layoff with FGTS	0.58	0.66	0.68	0.67	0.61	0.60	0.63	0.62	0.66	0.68	0.72	0.74	0.73	0.70	0.69	0.70	0.73	0.75	0.73	0.74	0.73
	Layoff without FGTS	0.11	0.11	0.10	0.09	0.10	0.10	0.10	0.11	0.11	0.10	0.11	0.10	0.10	0.11	0.10	0.11	0.12	0.12	0.11	0.11	0.12

Table 3: Summary Statistics of Treatment and Control Groups

A: Formal and Informal Workers

	Formal			Informal		
	Pre-Constitution	Post-Constitution	Post Law 110	Pre-Constitution	Post-Constitution	Post Law 110
Share in the Sample	59,24%	57,86%	55,62%	40,76%	42,14%	44,38%
Share of Men	63,51%	61,30%	58,28%	55,17%	52,76%	49,94%
Share with 0-4 Years of Schooling	32,89%	21,67%	14,18%	41,31%	27,21%	15,00%
Share with 5-10 Years of Schooling	41,55%	41,88%	34,50%	40,62%	43,02%	37,76%
Share with 11+ Years of Schooling	25,56%	36,45%	51,32%	18,07%	29,78%	47,24%
Average Age	27,55	30,09	32,00	23,69	26,35	27,99
Share in Manufacturing	35,93%	30,11%	24,70%	15,65%	15,58%	13,84%
Share in Construction	11,93%	7,68%	6,42%	14,47%	12,39%	9,81%
Share in Commerce	17,76%	19,99%	21,35%	14,61%	15,36%	16,40%
Share in Services	32,76%	40,69%	46,19%	49,47%	51,53%	54,69%
Share in Others	1,62%	1,53%	1,35%	5,81%	5,14%	5,26%

B: Long-Duration and Short-Duration Workers

	Short Duration			Long Duration		
	Pre-Constitution	Post-Constitution	Post Law 110	Pre-Constitution	Post-Constitution	Post Law 110
Share in the Sample	9,84%	5,75%	4,83%	90,16%	94,25%	95,17%
Share of Men	65,42%	62,44%	60,11%	63,29%	61,23%	58,16%
Share with 0-4 Years of Schooling	37,53%	26,37%	13,11%	32,39%	21,38%	14,24%
Share with 5-10 Years of Schooling	44,21%	44,88%	37,18%	41,26%	41,70%	34,37%
Share with 11+ Years of Schooling	18,26%	28,76%	49,72%	26,35%	36,92%	51,38%
Average Age	25,46	26,98	29,07	27,77	30,28	32,15
Share in Manufacturing	33,16%	28,78%	21,89%	36,24%	30,20%	24,86%
Share in Construction	19,46%	13,75%	11,59%	11,10%	7,30%	6,15%
Share in Commerce	20,94%	21,81%	28,04%	17,42%	19,88%	21,00%
Share in Services	25,74%	34,80%	37,20%	33,52%	41,05%	46,65%
Share in Others	0,70%	0,85%	1,29%	1,72%	1,57%	1,34%

Table 4: Determinants of Employment Duration
Control Group: Informal Workers*

Variable	(1)	(2)	(3)	(4)	(5)
Constant	0.757	0.808	0.832	0.896	0.995
	0.011	*0.038*	*0.040*	*0.039*	*0.041*
Formal	1.256	1.021	1.003	0.937	0.511
	0.014	*0.014*	*0.021*	*0.020*	*0.069*
1989-2001	0.177	-0.106	-0.085	-0.094	-0.119
	0.014	*0.017*	*0.022*	*0.019*	*0.019*
Formal x 1989-2001	0.412	0.362	0.300	0.294	0.260
	0.018	*0.017*	*0.026*	*0.024*	*0.084*
Post 2001	0.271	*-0.260*	*-0.276*	*-0.207*	*-0.288*
	0.030	*0.035*	*0.045*	*0.040*	*0.040*
Formal x Post 2001	0.491	*0.402*	*0.371*	*0.444*	*0.622*
	0.040	*0.038*	*0.056*	*0.069*	*0.212*
Aggregate Variables					
Unemployment rate	-	0.018	0.018	0.017	0.018
		0.004	*0.004*	*0.004*	*0.004*
Inflation rate	-	-0.001	-0.001	-0.001	-0.001
		0.000	*0.000*	*0.000*	*0.000*
Openness	-	0.004	0.004	0.004	0.004
		0.002	*0.002*	*0.002*	*0.002*
Gender					
Male	-	0.179	0.141	0.160	0.145
		0.008	*0.022*	*0.014*	*0.014*
Education					
5-10 years of schooling	-	0.222	0.223	0.250	0.222
		0.010	*0.010*	*0.015*	*0.010*
> 10 years of schooling	-	0.728	0.731	0.396	0.733
		0.011	*0.011*	*0.018*	*0.011*
Industry					
Manufacturing	-	-0.790	-0.798	-0.813	-1.326
		0.024	*0.024*	*0.024*	*0.031*
Construction	-	-1.915	-1.914	-1.958	-1.781
		0.025	*0.025*	*0.025*	*0.031*
Commerce	-	-1.126	-1.132	-1.159	-1.246
		0.024	*0.024*	*0.024*	*0.031*
Services	-	-1.004	-1.015	-1.033	-1.178
		0.023	*0.023*	*0.023*	*0.028*
Age					
26-35 years	-	0.708	0.708	0.708	0.708
		0.009	*0.009*	*0.009*	*0.009*
36-45 years	-	1.798	1.797	1.797	1.802
		0.012	*0.012*	*0.012*	*0.012*
46-55 years	-	2.668	2.666	2.673	2.682
		0.018	*0.018*	*0.018*	*0.018*
56-65 years	-	3.354	3.354	3.356	3.376
		0.035	*0.035*	*0.035*	*0.035*

> 65 years	-	0.827	0.827	0.822	0.823
		0.028	*0.028*	*0.028*	*0.028*
Metropolitan Regions					
Belo Horizonte	-	0.121	0.123	0.129	0.114
		0.014	*0.014*	*0.014*	*0.014*
Recife	-	0.085	0.086	0.084	0.074
		0.014	*0.014*	*0.014*	*0.014*
Rio de Janeiro	-	0.116	0.118	0.113	0.106
		0.014	*0.014*	*0.014*	*0.014*
Porto Alegre	-	-0.060	-0.056	-0.050	-0.073
		0.015	*0.015*	*0.015*	*0.015*
São Paulo	-	0.236	0.240	0.238	0.222
		0.013	*0.013*	*0.013*	*0.013*
Male x 1989-2001	-	-	-0.043	0.038	-
			0.027	*0.017*	
Male x Post 2001	-	-	0.027	0.057	-
			0.057	*0.038*	
Formal x Male	-	-	0.034	-	-
			0.028		
Formal x 5-10 years of schooling	-	-	-	0.019	-
				0.024	
Formal x >10 years of schooling	-	-	-	0.402	-
				0.029	
Formal x Manufacturing	-	-	-	-	0.963
					0.072
Formal x Construction	-	-	-	-	0.076
					0.075
Formal x Commerce	-	-	-	-	0.442
					0.074
Formal x Services	-	-	-	-	0.526
					0.071
Formal x 1989-2001 x Male	-	-	0.110	-	-
			0.035		
Formal x 1989-2001 x 5-10 years of schooling	-	-	-	-0.082	-
				0.025	
Formal x 1989-2001 x >10 years of schooling	-	-	-	0.184	-
				0.029	
Formal x 1989-2001 x Manufacturing	-	-	-	-	0.151
					0.085
Formal x 1989-2001 x Construction	-	-	-	-	-0.139
					0.089
Formal x 1989-2001 x Commerce	-	-	-	-	0.103
					0.086
Formal x 1989-2001 x Services	-	-	-	-	0.085
					0.084

Formal x Post 2001 x Male	-	-	0.053 *0.076*	-	-
Formal x Post 2001 x 5-10 years of schooling	-	-	-	-0.278 *0.074*	-
Formal x Post 2001 x >10 years of schooling	-	-	-	-0.073 *0.074*	-
Formal x Post 2001 x Manufacturing	-	-	-	-	-0.278 *0.215*
Formal x Post 2001 x Construction	-	-	-	-	-0.384 *0.230*
Formal x Post 2001 x Commerce	-	-	-	-	-0.303 *0.217*
Formal x Post 2001 x Services	-	-	-	-	-0.177 *0.213*
Seasonal dummies	yes	yes	yes	yes	yes
Number of Observations	502901	498581	498581	498581	498581
R^2	0.07	0.16	0.16	0.16	0.16

* Standard errors are in italic below each coefficient.

Table 5: Determinants of Employment Duration
Control Group: Short Duration Workers*

Variable	(1)	(2)	(3)	(4)	(5)
Constant	0.114	-0.622	-0.581	-0.504	-1.085
	0.032	*0.068*	*0.080*	*0.075*	*0.252*
Long Duration	2.093	1.703	1.679	1.626	2.232
	0.034	*0.032*	*0.055*	*0.045*	*0.259*
1989-2001	0.005	-0.354	-0.334	-0.343	-0.364
	0.046	*0.045*	*0.074*	*0.048*	*0.048*
Long Duration x 1989-2001	0.532	0.467	0.414	0.387	0.379
	0.048	*0.045*	*0.076*	*0.050*	*0.104*
Post 2001	0.002	*-0.738*	*-0.637*	*-0.607*	*-0.765*
	0.130	*0.126*	*0.198*	*0.131*	*0.130*
Long Duration x Post 2001	0.699	*0.660*	*0.518*	*0.604*	*0.985*
	0.134	*0.125*	*0.200*	*0.143*	*0.270*
Aggregate Variables					
Unemployment rate	-	0.020	0.020	0.020	0.019
		0.005	*0.005*	*0.005*	*0.005*
Inflation rate	-	-0.001	-0.001	-0.001	-0.001
		0.001	*0.001*	*0.001*	*0.001*
Openness	-	0.005	0.005	0.005	0.006
		0.003	*0.003*	*0.003*	*0.003*
Gender					
Male	-	0.261	0.197	0.210	0.208
		0.012	*0.064*	*0.020*	*0.020*
Education					
5-10 years of schooling	-	0.314	0.314	0.371	0.315
		0.015	*0.015*	*0.050*	*0.015*
> 10 years of schooling	-	0.982	0.982	0.352	0.983
		0.016	*0.016*	*0.061*	*0.016*
Industry					
Manufacturing	-	-0.202	-0.203	-0.207	-0.100
		0.046	*0.046*	*0.046*	*0.251*
Construction	-	-1.614	-1.610	-1.621	-0.435
		0.049	*0.049*	*0.049*	*0.253*
Commerce	-	-0.587	-0.588	-0.590	0.062
		0.047	*0.047*	*0.047*	*0.252*
Services	-	-0.552	-0.552	-0.554	-0.214
		0.046	*0.046*	*0.046*	*0.251*
Age					
26-35 years	-	0.992	0.992	0.992	0.990
		0.013	*0.013*	*0.013*	*0.013*
36-45 years	-	2.391	2.391	2.388	2.387
		0.017	*0.017*	*0.017*	*0.017*
46-55 years	-	3.353	3.352	3.351	3.350
		0.025	*0.025*	*0.025*	*0.025*
56-65 years	-	4.156	4.155	4.155	4.157
		0.050	*0.050*	*0.050*	*0.050*

> 65 years	-	1.214	1.214	1.216	1.220
		0.043	*0.043*	*0.043*	*0.043*
Metropolitan Regions					
Belo Horizonte	-	0.193	0.194	0.199	0.193
		0.020	*0.020*	*0.020*	*0.020*
Recife	-	0.080	0.080	0.083	0.078
		0.022	*0.022*	*0.021*	*0.021*
Rio de Janeiro	-	0.122	0.122	0.125	0.122
		0.021	*0.021*	*0.021*	*0.021*
Porto Alegre	-	-0.060	-0.060	-0.059	-0.058
		0.020	*0.020*	*0.020*	*0.020*
São Paulo	-	0.333	0.333	0.335	0.333
		0.019	*0.019*	*0.019*	*0.019*
Male x 1989-2001	-	-	-0.034	0.082	-
			0.091	*0.025*	
Male x Post 2001	-	-	-0.174	0.080	-
			0.250	*0.057*	
Long Duration x Male	-	-	0.034	-	-
			0.067		
Long Duration x 5-10 years of schooling	-	-	-	-0.003	-
				0.054	
Long Duration x >10 years of schooling	-	-	-	0.530	-
				0.066	
Long Duration x Manufacturing	-	-	-	-	-0.135
					0.262
Long Duration x Construction	-	-	-	-	-1.186
					0.265
Long Duration x Commerce	-	-	-	-	*-0.690*
					0.263
Long Duration x Services	-	-	-	-	-0.368
					0.262
Long Duration x 1989-2001 x Male	-	-	0.087	-	-
			0.095		
Long Duration x 1989-2001 x 5-10 years of schooling	-	-	-	-0.080	-
				0.030	
Long Duration x 1989-2001 x >10 years of schooling	-	-	-	0.202	-
				0.034	
Long Duration x 1989-2001 x Manufacturing	-	-	-	-	0.101
					0.097
Long Duration x 1989-2001 x Construction	-	-	-	-	-0.186
					0.103
Long Duration x 1989-2001 x Commerce	-	-	-	-	*0.046*
					0.099

Long Duration x 1989-2001 x Services	-	-	-	-	0.053 *0.097*
Long Duration x Post 2001 x Male	-	-	0.242 *0.257*	-	-
Long Duration x Post 2001 x 5-10 years of schooling	-	-	-	-0.250 *0.085*	-
Long Duration x Post 2001 x >10 years of schooling	-	-	-	0.016 *0.085*	-
Long Duration x Post 2001 x Manufacturing	-	-	-	-	-0.421 *0.248*
Long Duration x Post 2001 x Construction	-	-	-	-	-0.450 *0.266*
Long Duration x Post 2001 x Commerce	-	-	-	-	-0.416 *0.249*
Long Duration x Post 2001 x Services	-	-	-	-	-0.284 *0.245*
Seasonal dummies	yes	yes	yes	yes	yes
Number of Observations	310404	307790	307790	307790	307790
R^2	0.04	0.16	0.16	0.16	0.16

* Standard errors are in italic below each coefficient.

Table 6: Logit - Probability of Quitting with FGTS*

Variable	(1)	(2)
Constant	-1.184	-1.224
	0.058	*0.060*
1989-2001	-0.402	-0.345
	0.012	*0.027*
Post 2001	-0.508	-0.321
	0.030	*0.085*
Aggregate Variables		
Unemployment rate	-0.129	-0.128
	0.004	*0.004*
Male	-0.488	-0.489
	0.011	*0.011*
Years of Schooling	0.049	0.053
	0.002	*0.002*
Age	-0.005	-0.005
	0.001	*0.001*
Sector		
Manufacturing	-0.165	-0.163
	0.043	*0.043*
Construction	-0.457	-0.453
	0.048	*0.048*
Commerce	0.046	0.048
	0.044	*0.044*
Services	0.034	0.036
	0.043	*0.043*
Metropolitan Regions		
Belo Horizonte	0.675	0.674
	0.021	*0.021*
Recife	0.173	0.173
	0.023	*0.023*
Rio de Janeiro	0.187	0.187
	0.022	*0.022*
Porto Alegre	0.689	0.689
	0.020	*0.020*
São Paulo	0.324	0.325
	0.020	*0.020*
Years of Schooling x 1989-2001	-	-0.007
		0.003
Years of Schooling x Post 2001	-	-0.021
		0.009
Seasonal dummies	yes	yes
Log-likelihood	-116226.1	-116221.1
Number of Observations	302336	302336

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