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Labor Turnover and Labor Legislation in Brazil

One of the main characteristics of the Brazilian labor market is its impressively high job and worker turnover rates. This contrasts with many analysts' view that Brazil has an overly regulated labor market. While the Brazilian labor code is, in fact, very restrictive, dismissal costs are not high when compared with other countries in the region.[1] Moreover, many authors claim that the design of some job security programs in Brazil creates perverse incentives that stimulate labor turnover to greater levels 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 the potential benefits of reducing income volatility and increasing investment in specific human capital, which could raise productivity in the medium run. According to this latter view, an excessively high turnover rate is a problem, since it might cause underinvestment in human capital and signal 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 percent of the formally employed enter and leave every month. A high labor turnover partially explains the low quality of jobs observed in Brazil, since labor productivity depends essentially on the level of human capital, either general (through basic

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1. See Heckman and Pagés-Serra (2000).

education) or specific (through on-the-job training). High labor turnover is a disincentive for training investment because it lowers specific human capital and, therefore, labor productivity.

This paper fully describes the legislation on dismissal costs in Brazil. Following the argument of other authors, I conclude that the design of the *Fundo de Garantia por Tempo de Serviço* (FGTS) 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 must deposit 8 percent of its current formal employees' wages (8.5 percent since September 2001) into accounts opened in each worker's name in a state bank. With a few exceptions, workers can only withdraw money from these accounts if they are fired without justification, in which 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, workers have a strong incentive to get hold of their FGTS funds. The firing fine can be de facto negotiated between firms and workers, creating ample room for fake dismissals, by which firms simulate they are firing workers without 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.[2]

The paper uses two episodes of increases in the dismissal fine to empirically identify the effects of firing costs on labor turnover. Specifically, the two episodes—namely, the 1988 Constitution and a labor law introduced in September 2001—are used to test the prediction that higher job termination costs would, other things being equal, reduce turnover for formal workers affected by the legislation. The paper does not advocate that such firing cost increases are good policy responses to the original distortion, which causes turnover to be suboptimally 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 difference-in-differences methodology is used to test the implication of the increased fines based on monthly individual data from the monthly employment survey (*Pesquisa Mensal de Emprego*, or PME) carried out by the Brazilian Geographical and Statistical Institute (IBGE); this household survey covers the six main metropolitan regions in Brazil

2. Zylberstajn (1999).

and includes information on previous employment spells for those currently unemployed. The methodology exploits the fact that the change to the legislation should have affected some groups of workers differently than two control groups: informal workers and formal workers with low tenure (less than three months).

The paper is organized as follows. The next section describes the Brazilian labor legislation, with emphasis on those measures that affect labor turnover, including the perverse incentives mentioned above. Subsequently, the paper reviews the evidence on labor turnover in Brazil found in the recent literature. Data from several sources, analyzed through different methods, confirm the view of a hyperactive labor market with especially high turnover rates for the less educated.[3] The paper then presents the results of the difference-in-differences methodology to assess the importance of changes in the labor legislation on employment duration. The final section concludes the paper.

Labor Legislation in Brazil

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.[4] Carvalho uses a standard notion that citizenship is fully characterized when the society has completely developed civil, political, and social rights.[5] 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 win social rights for themselves. On the contrary, social rights were obtained through concessions from paternalistic or authoritarian governments, and they were usually accompanied by measures that tended to restrict labor and social movements.

3. World Bank (2002).

4. Some parts of this section are largely based on Paes de Barros, Corseuil, and Gonzaga (1999).

5. Carvalho (2001).

The Brazilian labor code (*Consolidação das Leis do Trabalho*, or CLT) is a clear example of this pattern.[6] 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.[7]

The Brazilian labor legislation experienced 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 of the changes introduced in 1964 had the objective of reducing the power of labor unions and their ability to organize.[8] The right to strike was severely reduced, and many union leaders were persecuted during this period. A wage policy was also introduced at the beginning of the military government. From 1965 to 1995, wage determination in Brazil was largely influenced by the official wage indexation policy, especially between 1965 and 1979, when labor union activity was very low.[9]

The new Brazilian Constitution was implemented in November 1988 as part of the process of democratization after the end of the military regime in 1985. The Constitution brought many changes to labor legislation.[10] In fact, many labor laws are written into the Constitution. These are very difficult to change, since amending the Constitution requires at least 60 percent approval in two rounds of voting in both parliamentary houses. The aim of the changes was to increase workers' benefits and reverse the 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 forty-eight to forty-four hours; the minimum overtime premium increased from 20 percent to

6. Amadeo and Camargo (1996).

7. The CLT is a highly restrictive and detailed body of law, which is clearly fascist in inspiration. Hall (2002), for instance, finds striking similarities between the CLT and the 1926 Italian Law on the Juridical Disciplining of Labor Relations, law that preceded the Labor Charter of 1927.

8. Amadeo and Camargo (1996).

9. Mario Henrique Simonsen, one of the most brilliant Brazilian economists of his time and the 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 with a simple arithmetic formula" (Simonsen and Campos, 1974).

10. Paes de Barros, Corseuil, and Gonzaga (1999).

50 percent; the maximum number of hours for a continuous work shift dropped from eight to six hours; maternity leave increased from three to four months; and the value of paid one-month vacations increased from one normal monthly wage to, at least, one and a third. Dismissal costs also increased. Since this is the main focus of this paper, I discuss the dismissal cost modifications in detail in the next subsection.

Not many changes were introduced to labor legislation over the last fifteen years. Although the former president, Fernando Henrique Cardoso, advocated the need for labor legislation reforms, these never ranked high among the priorities of his two-term government (1995–2002). 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, although 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 of averaging the number of hours worked over an extended period to reduce the payment of overtime.

Fixed-term labor contracts 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 the contracts had to be approved in collective agreements with unions. Consequently, 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 who were hired and fired from formal jobs *every month* over the same period. The current president, Luiz Inácio Lula da Silva, did not renew the law, which has not been in effect since January 23, 2003.

Other labor legislation implemented in 1998 created the so-called hours bank, which allows firms to average the number of hours worked over a period of four months instead of one week, thus reducing the number of overtime hours. This gives firms more flexibility to use overtime in high-demand weeks, which can be compensated with cutbacks in low-demand weeks over a four-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.[11]

11. Most collective agreements that implement the hours-bank mechanism also contain work-sharing measures, such as the reduction of the length of working time, increases in the

Legislation on Dismissal Costs in Brazil

Job security provisions have existed in Brazil since the early 1940s. According to the labor legislation prevailing from the early 1940s to 1966, workers with more than one year but less than ten years of tenure were entitled on dismissal to receive a severance payment of one monthly wage per year worked at the firm.[12] Workers with more than ten years at the firm were granted job stability. These workers could only be fired for just cause, which did not include absenteeism or low productivity. Aside from 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 if the worker agreed. This created many distortions affecting the productivity of workers with job stability, with cases frequently ending up in the labor courts. Moreover, the absence of formal mechanisms ensuring that dismissed workers would, in fact, receive the severance payments dictated by the legislation created additional problems. Some attempts to reform job security legislation in the direction of guaranteeing that the resources necessary for dismissal charges were allocated by firms and deposited in federal funds were made in 1958 and 1964.

It was against this background that the FGTS 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 legislation that would simultaneously remove the distortion of full job stability for those with more than ten years of tenure and ensure 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 many established 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 percent (8.5 percent since September 2001) of his/her formal employee's monthly wage into an individual account, managed by a state bank, Caixa Econômica Federal.[13] Deposits are periodically adjusted to

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overtime premium, and some wage restraint. This illustrates labor unions' change of focus in the early 1990s, from wage adjustment to a larger concern with employment.

12. Oliveira and others (1999).

13. Before 1989, the FGTS system was managed by the now-defunct Banco Nacional da Habitação.

compensate for inflation plus a 3 percent annual interest rate.[14] Workers have access to their accounts only in the case of unjustified dismissal or retirement.[15] That means that workers who voluntarily quit are not granted access to their accounts.[16]

The law further states that all workers dismissed without just cause, with the exception of those on a 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. This proportion was fixed at 10 percent from 1966 to 1988, but the 1988 Constitution increased it to 40 percent of the FGTS balance. Finally, as discussed below, as of September 2001 the firms have to pay an additional 10 percent of the FGTS balance to the government when dismissing a worker without justification.

Workers have access to the entire individual fund on dismissal, including all deposits accumulated during previous jobs, plus the fine in proportion to the deposits accumulated while on the job from which they are being dismissed.

The FGTS system is designed to approximately match the severance payment determined by the previous job security provisions. 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 percent of the monthly wage (8.5 percent since September 2001). The three main changes with respect to the previous legislation were that firms were forced to make a provision for the severance payment by depositing it upfront in the worker's FGTS account; that job stability for those with more than ten years of tenure was eliminated; and that the

14. From 1966 to 1971, interest rates were 3, 4, 5, or 6 percent for those workers that migrated from the previous job termination law, depending (proportionally) on the tenure at the current job (Oliveira and others, 1999).

15. Some exceptions have been introduced over time that allow workers to withdraw money from their accounts: buying the first real estate in the city in which the worker lives, suffering a serious disease like cancer or AIDS, having an inactive account for more than five years, death, and so forth.

16. Some other Latin American countries (Colombia, Ecuador, Panama, Peru, and Venezuela) have severance arrangements similar to FGTS (Heckman and Pagés-Serra, 2000). In these countries, however, workers have access to their seniority funds in the case of voluntary quits as well as unjustified dismissals. As in Brazil, the penalty is paid only in the case of unjustified dismissals.

fine for unjustified dismissals was introduced, initially at 10 percent of the FGTS balance.[17]

In September 2001, Complementary Law 110 was introduced 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 percent, which corresponded to real losses incurred as a result of two stabilization plans (16.44 percent in the Summer Plan of 1989 and 44.80 percent in the Collor Plan of 1990). After several months of negotiations, the government reached an agreement with workers and firms' representatives to share the cost of the judicial decision. As part of the agreement, monthly deposits in FGTS accounts increased from 8.0 to 8.5 percent of current monthly wages.[18] The fine for unjustified dismissals also increased from 40 to 50 percent of the FGTS balance, with the extra 10 percentage points being paid to the government, as mentioned above.

Because this last fine increase is paid to the government instead of to the worker, the amount the worker receives as compensation for unjustified dismissal did not change after Law 110. It remains at 40 percent of approximately one monthly wage per year in the firm. Law 110, therefore, represented both an increase in firing costs (the extra 10 percent that goes to the government) and a reduction in the incentives for workers to make 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 their employees a one-month advance notice of dismissal, with the exception of workers on probation (from zero to three months on the job).[19] During that month, workers are granted up to two hours per day (25 percent of a regular workday) to search for a new job. Since the productivity of workers who are leaving tends to significantly drop during the advance-notice period,

17. 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—namely, the job stability after ten years on the job.

18. This change was implemented through Complementary Law 110 of 25 June 2001 and regulated by Decree Law 3914 of 11 September 2001, which came into effect on 28 September 2001.

19. Notification of dismissal also had to be given at least one month in advance before the 1988 Constitution. The 1988 Constitution states that the notification period should be proportional to the worker's tenure; this change has to be regulated through ordinary legislation, which requires a simple majority of votes in both houses. However, no specific law

the law gives the firm the option 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 percent and 100 percent of one monthly wage, with it being closer to 100 percent than 25 percent in practice.

To sum up, it is useful to introduce some notation. Total firing cost in Brazil is $\lambda w + (f + g)\text{FGTS}_{bal}$, 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 fine to the worker on dismissal, and g is the proportion of the FGTS balance paid to the government.[20]

The first term corresponds to the cost of the one-month advance notice. Based on the discussion above, I 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. Given that 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 from which he/she is being fired.[21] The worker is entitled to receive only $w + f \bullet \text{FGTS}_{bal} \cong (1 + f \bullet y)w$, 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 after the implementation of FGTS in 1966 for three periods: first, from 1966 to the promulgation of the 1988 Constitution in November 1988, when according to my notation $f = 0.1$ and $g = 0$; second, from November 1988 to the initiation of Law 110 in September 2001, when $f = 0.4$ and $g = 0$; and third, after September 2001, when $f = 0.4$ and $g = 0.1$. Note that there are no costs of dismissing a worker on probation, that is, a worker with less than 3 months on the job.

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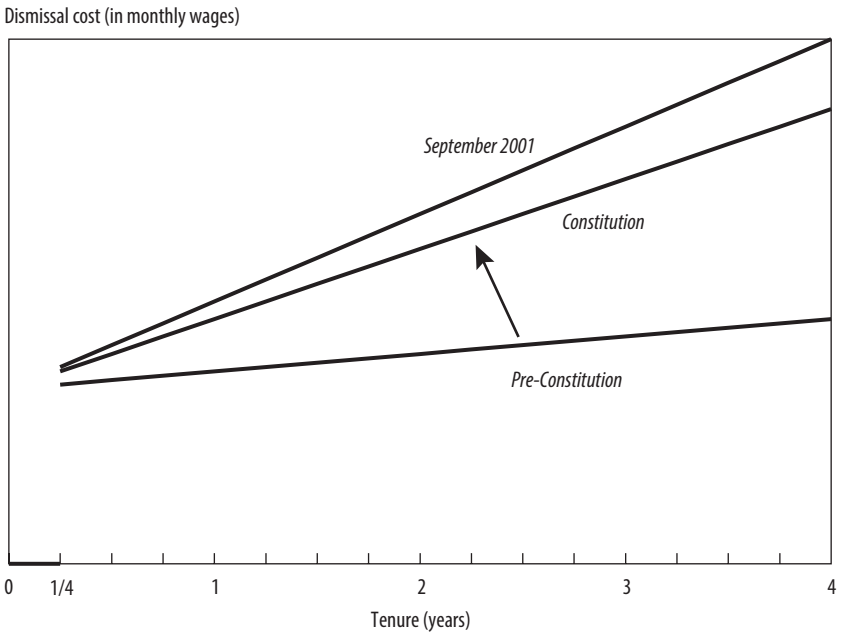
has ever regulated this constitutional device, and notice continues to be given one month prior to dismissal for all workers, independent of their tenure. Even the fine increase from 10 to 40 percent is a temporary mechanism established in Article 10 of the Transitory Constitutional Dispositions, which should remain in effect until a Complementary Law is promulgated, which never happened.

20. The FGTS balance itself (the accumulation of the monthly deposits in the FGTS accounts of 8.5 percent of the wage) is not included in the firing costs. The deposits are, in fact, a static labor cost.

21. In this approximation, I also assume that wages do not increase much with tenure, which is a realistic hypothesis for workers with low human capital.

FIGURE 1. Dismissal Costs



Source: Author's calculations.

Dismissal costs were very small prior to November 1988. Before the constitutional change, the worker had to be employed in the same firm for at least ten years in order for the fine to reach the magnitude of one monthly wage in addition to the one received as advance notice. Between November 1988 and September 2001, it took two and a half years in the job for the fine to reach the value of one monthly wage. It now takes two years of tenure for the penalty to amount to an additional monthly wage.

Perverse Incentives Implied by the Legislation on Dismissal Costs

Much has been written on the perverse incentives originating in the legislation on dismissal costs in Brazil.[22] The basic argument is that the design of the legislation gives workers strong incentives to induce their own

22. See, among others, Macedo (1985); Camargo (1996); Amadeo and Camargo (1996); Gonzaga (1998); Paes de Barros, Corseuil, and Foguel (2001).

dismissal. The three main features of the FGTS system that create these incentives are that the funds are maintained below market rates, which gives workers an incentive to take their money out of the system; getting fired is the main mechanism for gaining access to the FGTS accounts; and the dismissal penalty is paid directly by the employer to the employee, which establishes space for fake dismissals.

With regard to the return on the funds, the government has poorly managed the FGTS, typically paying negative real returns or returns well below market rates. This has resulted mainly from less-than-perfect inflation indexation, especially during the many stabilization plans implemented in the late 1980s and early 1990s involving currency and price index changes. Oliveira and others show that the main losses occurred in the early 1980s and early 1990s.[23] Real returns of approximately –60 percent were observed from 1977 to 1993.[24] Inflation stability after the Real Plan has helped recover some of the real value of FGTS deposits since 1994, but the 3 percent interest rate remains much below market rates (riskless assets like savings accounts, for instance, pay 6 percent interest plus the same nominal indexation as FGTS).[25]

The design of the FGTS system creates an incentive for firms and employees to engage in rent-seeking activities. A fake layoff agreement can be described as follows. A worker who wants to quit can offer $b \bullet \text{FGTS}_{bal} \cong b \bullet y \bullet w$ to the firm to fire him/her without just cause so that he/she is able to receive $(1 - b)\text{FGTS}_{bal} \cong (1 - b)y \bullet w$, where $0 \leq b \leq 1$. If the employer agrees, he/she has to simulate paying $(1 + f \bullet y)w$ to the worker and has to actually pay $g \bullet y \bullet w$ to the government.[26] Consequently,

23. Oliveira and others (1999).

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

25. Additionally, workers may be heavily discounting the future owing to shortsightedness or credit constraints. Some authors argue that this would not be rational, given the high premium for tenure in Brazil, unless discount rates are too high (Carneiro and Ramos, 2002). However, the tenure premium in Brazil is much higher for workers with more education than for those with less, as expected by theory. In fact, workers with a low level of education gain very little from tenure in Brazil, given their dim prospects for future earnings on the same job. These workers are also likely to be credit constrained. Therefore, one should expect that the temptation to gain access to the FGTS balance is higher for less educated and poorer workers.

26. In the fake layoff agreement, the employer does not pay the fine, f, to the worker.

when $g > 0$, b has to be larger than g to make it worthwhile for the employer to accept the employee's offer. The introduction of the penalty g to be paid to the government after September 2001 should have increased the cost of implementing these fake dismissal agreements. Since the penalty does not go to 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.

An increase in f, like the one observed in 1988, should have two effects. On the one hand, it should reduce the incentive for the employers to engage in fake dismissal agreements, since it implies larger losses for employers should the employee not comply with the agreement—for example, by not returning the money paid as dismissal penalty or by bringing the employer to court alleging that he/she did not pay it. On the other hand, an increase in f might eventually raise the desire of employees who would otherwise quit to get fired, since the short-run rewards are now larger. This second effect only applies to workers who 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. In the litigious case, however, the employer has nothing to gain, and it is likely that these workers take actions to force their own dismissal.

The first effect implies a reduction in both labor turnover and fake layoff agreements. The second effect implies an increase in labor turnover, a reduction in fake layoff agreements, and certainly a reduction in productivity, since it makes labor-capital relations more litigious. In a multiperiod framework, in which references for future jobs are important, the second effect is likely to be small and would mainly be significant, for example, for workers who would like to use the money to open a new business. It is also likely that employers would resist 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 if they are likely to lose the judicial action. In any case, the net effect on labor turnover is not obvious and will depend on the factors behind the worker's decision to undertake a litigious job termination negotiation process.

As mentioned above, f was increased once in November 1988 (from 0.1 to 0.4), and g was increased once in September 2001 (from 0 to 0.1). Finding the net effect of the November 1988 fine increase on labor turnover is an empirical question, although I would expect to observe a negative effect on labor turnover, since the first effect described in the last paragraph

should be more important than the second. The increase in *g* in September 2001 should have unambiguously decreased labor turnover in Brazil.

One should thus expect that many workers who would otherwise like to quit a job would make an effort to get fired (without justification) in order to get hold of their FGTS accounts, either with or without the agreement of their employers.[27] This is, in fact, very common in Brazil. There is ample anecdotal evidence, for instance, that employees in human resources departments of large firms rob their own firms by sharing the rents from these fake layoff agreements with the employees being fired. In some cases, workers are rehired after three months, which is a legal procedure.

Camargo provides evidence that complaints involving unpaid dismissal fines are the second-most-common reason for taking employers to judicial courts.[28] On the other hand, there is evidence that a considerable number of workers succeed in making agreements with their employers about being fired. Paes de Barros, Corseuil, and Foguel show that 62 percent of unemployed workers who answered the 1990 annual Brazilian household survey (*Pesquisa Nacional de Amostra por Domicílio*, or PNAD) that they had quit their previous formal jobs also answered that they received their FGTS balance.[29] Since the legislation does not allow quitters to withdraw their FGTS balance, these seem to be fake layoffs—that is, quitters who somehow were able to convince or induce their employers to dismiss them without just cause.

The previous literature overlooks this piece of information. Monthly data on the proportion of fake layoffs (workers who voluntarily quit and withdrew FGTS) are available in the PME for the 1982–2002 period. It is

27. The Brazilian government is aware of the existence of fake layoff agreements. The typical response to the problem has been the implementation of procedures that increase the costs and risk associated with these simulated payments. For instance, in the 1990s, only workers with more than one year on the job were eligible to receive the penalty payment, which was paid through the labor union representing the worker and which required a signed statement by the worker acknowledging receipt. In the early 2000s, new legislation was introduced requiring that the penalty be deposited in the worker's FGTS account, which makes agreements harder to implement. 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.

28. Camargo (2002).

29. Paes de Barros, Corseuil, and Foguel (2001). The authors also report that this proportion was 68 percent in the 1998 PME.

therefore possible to exploit the temporal variation around the implementation of both the constitutional change and Law 110 to test whether the incidence of this type of agreement decreased after the fines increased. This is accomplished later in 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 legislative changes introduced by the 1988 Constitution and by 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 should be observed after both legislative changes. These implications are tested later in the paper.

Evidence from the Literature on Labor Turnover in Brazil

Many papers written in the last five years bring new evidence concerning labor turnover in Brazil by exploiting both old and recently available datasets. Some measures were constructed following methodologies that allow one to make international comparisons. This section reviews the recent literature, gathering new evidence on this topic.

Studies That Measure Job and Worker Turnover

Corseuil, Ribeiro, and Santos use the Ministry of Labor's annual administrative record (*Relação Anual de Informações Sociais,* or RAIS) to compute job and worker reallocation measures.[30] The dataset is organized in such a way that one can follow firms from 1991 to 1998. It covers all sectors and all regions of the Brazilian economy, and it has detailed information on workers' characteristics, stocks of workers, and number of hirings and separations. The one drawback is that the information is provided only for formal (registered) employees.

30. Corseuil, Ribeiro, and Santos (2003).

The authors report an annual job reallocation average of 33 percent over the 1991–1998 period, which is substantially higher than the yearly average of net employment growth of 1.5 percent. Average job creation and job destruction rates were, respectively, 17.3 percent and 15.5 percent. Some sectors have astonishing job reallocation rates. For instance, a rate of 63.9 percent was found in construction; this may be 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.[31]

The evidence on worker reallocation is in line with the findings on job turnover rates. Average yearly hirings amount to 48.2 percent of the employment level, while average separations are 46.2 percent. These correspond to an average churning rate (worker turnover in excess of job turnover) of 61.8 percent, which is certainly among the highest in the world. Sectoral disaggregation shows that each year construction hires 121.8 percent of the previous year-end employment level.

Menezes-Filho and Fernandes use the same RAIS dataset to analyze the costs of job displacement in Brazil.[32] They show significant earnings losses following job transitions. With regard to job tenure (average employment spells for the currently employed), they find that, on average from 1992 to 1998, 29.9 percent of formal employees were with their employers for less than a year; 44.0 percent for less than two years; and only 34.1 percent for more than five years. According to the authors, this represents a much shorter employment duration than that observed in the United States, where only 20 percent of workers have a tenure of less than one year. 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's employment registry (*Cadastro Geral de Empregados e Desempregados,* or CAGED), confirm the very high worker turnover rates. An average of 746,200 workers (3.4 percent of the formally employed) were admitted to a new job each month between 1997 and 2002, while 740,400 workers (also 3.4 percent of the formally employed), on average, left their formal jobs each month during the same period. These measures

31. The study also confirms expected findings that job reallocation rates are negatively correlated with plant size and age, as well as with worker education.

32. Menezes-Filho and Fernandes (2003).

correspond to annual worker turnover rates of around 40 percent for the more recent period.

Pazello, Bivar, and Gonzaga use manufacturing establishment survey data from IBGE's annual industrial survey for the 1986–95 period.[33] They report average job creation and job destruction rates of 9.8 and 13.2 percent, respectively, for the period.[34] Using a methodology that avoids the pitfalls found in some studies on the effect of size on job turnover rates, the authors find much higher job creation and destruction rates for micro and small firms than for medium-sized and large 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.

Additional evidence that firing costs are 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. Monthly manufacturing data from IBGE's monthly industrial survey for 1985 to 1997 show that employment variance explains 98.6 percent of total hours variance. These results point to a much greater reliance on employment adjustment than hours adjustment in Brazil.

Studies on the Determinants of Labor Turnover

Chahad, Orellano, and Picchetti study the joint determinants of quits and dismissals based on a bivariate probit model, using household data for the São Paulo metropolitan region.[35] 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 dataset is that it has information on job tenure for those currently employed, as well as on complete employment spells in the previous job for those currently

33. Pazello, Bivar, and Gonzaga (2000).

34. These numbers are smaller than those found based on the RAIS dataset because they consider the industrial sector only, they refer to establishments that existed throughout the period, and the sample of firms in the dataset is based on the 1985 Industrial Census, which overrepresents large firms.

35. Chahad, Orellano, and Picchetti (2001). Data are from the *Pesquisa de Emprego e Desemprego* (PED) conducted by the State Data Analysis System (SEADE) of the State of São Paulo.

unemployed. The other main household surveys in Brazil—PNAD and PME—only have information on the previous employment spells of the currently unemployed and not on the tenure of the currently employed. The main disadvantage of the dataset for the purposes of this paper is that information on employment duration starts in February 1988, which hurts the analysis of the 1988 Constitution impact. Their analysis is carried out separately for five major economic sectors.

The results conform to expectations. The authors show that education reduces the probability of dismissal in all sectors and the probability of quits in the industrial sectors. Having a working card (that is, being a formal worker) reduces the probability of both quits and dismissals in all sectors, as does job tenure. Gender is not significant in most regressions, and age reduces the probability of both quits and dismissals. Unemployment increases the probability of dismissals and reduces the probability of quits. Gross national product (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, which is contrary to what was expected; it might be explained by the lack of data before the constitutional change.

Studies of the Perverse Incentives and the 1988 Constitutional Change

Paes de Barros, Corseuil, and Bahia analyze data from the PME on complete employment spells on the previous job for those currently unemployed.[36] The data include information on the duration of the previous job, on the reason for separation (laid off or quit), on whether it was a formal or an informal job (with or without a working card), and on whether the individual withdrew his/her FGTS balance (only formal workers were asked this question). Since most of the empirical exercises performed in the next section are inspired by this paper and are based on the same dataset, I now discuss it in detail. The purpose of the study is to test whether the 1988 Constitution (in particular, the increase in the fine from 10 to 40 percent) affected labor turnover. The authors use a difference-in-differences

36. Paes de Barros, Corseuil, and Bahia (1999). The use of employment spells data reported by those who are currently unemployed provides an adequate measure of employment duration under the hypotheses that the economy is in a steady state and that the duration of employment and unemployment spells is independent. See Paes de Barros, Corseuil, and Bahia (1999) for a thorough discussion.

methodology that compares employment turnover measures before and after the constitutional change for control and treatment groups.[37]

They use three control groups that supposedly were not affected by the regulatory changes. The first control group consists of workers whose previous employment tenure lasted less than three months, the so-called very short employment spells workers. These workers do not bear any firing costs, since the first three months on the job are considered a probationary period. They still have access to the FGTS deposited in their name (in case of unjustified dismissal), but the firm does not have to pay the fine or to give them any advance notice.

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

The third control group encompasses those workers who quit their previous job. This group was chosen because the workers have no right to withdraw their FGTS balance or to receive the job termination fine. This is not an adequate control group, however, because the increase in the fine for unjustified dismissals from 10 to 40 percent should have made it harder for workers to make agreements with their employers, which would have affected the number of people who quit. Thus this group is not a suitable control, as it would have been directly affected by the legislative changes. In the next section I show that the proportion of workers who voluntarily quit and yet received FGTS (my proxy for fake layoffs) did, in fact, change with the legislation modifications.

Paes de Barros, Corseuil, and Bahia arbitrarily choose 1986–87 and 1991–92 to represent the pre- and post-Constitution periods. They show that hazard rates dropped significantly, especially for short tenures (three to six months). Comparisons had to be made with the three control groups, however, since 1991–92 was a period of very weak economic activity. The hazard rates are aggregate measures that can only be computed for each month and for each metropolitan region, so this is the only source of variation they could use. Consequently, they perform the difference-in-differences analysis by comparing the averages of the two periods, across months and regions within the two periods.

37. The authors' measure of turnover is the aggregate hazard rate—that is, the probability that an employment relationship that has already lasted a certain number of months will be terminated next month.

The results point to a significant drop in hazard rates for short employment spells relative to all three control groups. The authors conclude that the drop in hazard rates observed for workers with short employment spells could be attributed to the 1988 legislative change, as expected by theory. The results for the other spells (six to twelve months and twelve to twenty-four months) were mixed, depending on the choice of control group.

Carneiro and Ramos 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's employment registry, available for the 1985–2001 period.[38] 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 they correct 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 coefficients that are significantly different from zero, ranging from –0.24 to –0.14.[39]

The authors interpret this result as evidence that the argument that FGTS promotes employment turnover is flawed. However, as discussed above, 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 implement. I thus interpret their results as evidence that the increase in dismissal costs brought less turnover, as expected.

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

The data used in this section are taken from the monthly employment survey (PME), which covers the six main Brazilian metropolitan regions: Belo Horizonte, Porto Alegre, Recife, Rio de Janeiro, Salvador, and São Paulo. The survey is conducted by the Brazilian Geographical and Statistical

38. Carneiro and Ramos (2002).

39. More specifically, regressions were run for each major sector, from February 1985 to September 2001, including a constant, seasonal dummy variables, a dummy for the period after 1990, one lag of the dependent variable, and one lag of unemployment as explanatory variables. Regressions for manufacturing displayed the highest coefficients (in absolute value) for the structural change and for the cycle indicator.

Institute (IBGE), which interviews about 38,500 households every month, and it contains information on the usual demographic and labor market indicators. For each individual, I use information on education, age, gender, region, and labor market status (employed, unemployed, or out of the labor force).

For employed individuals, the survey identifies whether they work in the formal or informal sector, the number of hours worked, monthly earnings, sector of activity, and occupation. For individuals who are currently unemployed, the data cover the unemployment duration and the characteristics of the previous job, including employment duration, reason for job termination, and whether the FGTS balance was received. The sample period used runs from January 1982 to August 2002.

Average Employment Duration

Table 1 presents the average duration of employment spells from 1982 to 2002 for several groups of workers in the dataset. The table shows that employment duration on the previous job increased 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 (less than three months) remained almost constant, increasing slightly from 0.09 a year in 1982 to 0.10 a year in 2002. This indicator displayed very little variation across time for all groups of workers, independently of disaggregation. By contrast, average employment tenure for all other unemployed workers tended to increase over time and to show pronounced variation across time.

Figure 2 plots the average employment duration of unemployed workers by reason of separation (quits and layoffs) for the full sample, formal workers, and informal workers. Aggregate unemployment from the same dataset is also included in all figures to provide a sense of the state of the economy. The three panels illustrate the different trends of rising tenures by reason of separation. They reveal some cyclical swings, with average duration of employment spells increasing in good times, which is relatively more pronounced in the case of quits.

Panel A reveals a convergence of employment duration by reason of separation. People who quit their previous job had shorter previous employment duration in the 1980s and early 1990s, but employment spells converged to the level observed for laid-off workers, at about 2.0 years in 2002. Panels B and C, however, suggest that this convergence reflects a

TABLE 1. Average Duration of Previous Employment Spells of Unemployed Workers, by Type of Separation and Formal versus Informal Contracts

Number of years

Type of separation, type of contract, and job duration category	*1982*	*1984*	*1986*	*1988*	*1990*	*1992*	*1994*	*1996*	*1998*	*2000*	*2001*	*2002*
All unemployed workers												
0–3 months	0.09	0.08	0.09	0.10	0.10	0.09	0.09	0.10	0.10	0.09	0.09	0.10
More than 3 months	1.69	1.86	1.91	1.87	1.92	2.16	2.22	2.29	2.29	2.32	2.26	2.33
Total	1.27	1.41	1.50	1.53	1.59	1.84	1.85	1.92	1.93	1.94	1.91	2.00
Quit: all workers												
0–3 months	0.09	0.09	0.10	0.10	0.10	0.10	0.09	0.10	0.10	0.11	0.10	0.11
More than 3 months	1.52	1.65	1.75	1.68	1.68	1.92	1.88	2.23	2.18	2.26	2.24	2.20
Total	1.09	1.23	1.35	1.34	1.36	1.61	1.56	1.86	1.85	1.93	1.92	1.93
Layoff: all workers												
0–3 months	0.08	0.08	0.09	0.10	0.10	0.09	0.09	0.09	0.09	0.09	0.09	0.10
More than 3 months	1.79	1.94	1.99	1.95	2.02	2.23	2.31	2.30	2.32	2.34	2.27	2.35
Total	1.38	1.48	1.58	1.62	1.69	1.91	1.93	1.94	1.95	1.94	1.91	2.01
Quit: formal workers												
0–3 months	0.11	0.10	0.11	0.11	0.11	0.10	0.10	0.12	0.12	0.13	0.11	0.12
More than 3 months	1.72	2.03	2.13	2.01	2.00	2.35	2.32	2.72	2.74	2.96	2.75	2.77
Total	1.48	1.81	1.88	1.81	1.80	2.19	2.18	2.56	2.59	2.79	2.57	2.63
Quit: informal workers												
0–3 months	0.08	0.09	0.09	0.09	0.09	0.10	0.09	0.10	0.10	0.11	0.10	0.10
More than 3 months	1.24	1.26	1.24	1.18	1.20	1.37	1.37	1.59	1.58	1.53	1.65	1.51
Total	0.75	0.82	0.83	0.81	0.85	1.03	1.01	1.17	1.21	1.19	1.29	1.23

(*continued*)

TABLE 1. Average Duration of Previous Employment Spells of Unemployed Workers, by Type of Separation and Formal versus Informal Contracts (*continued*)

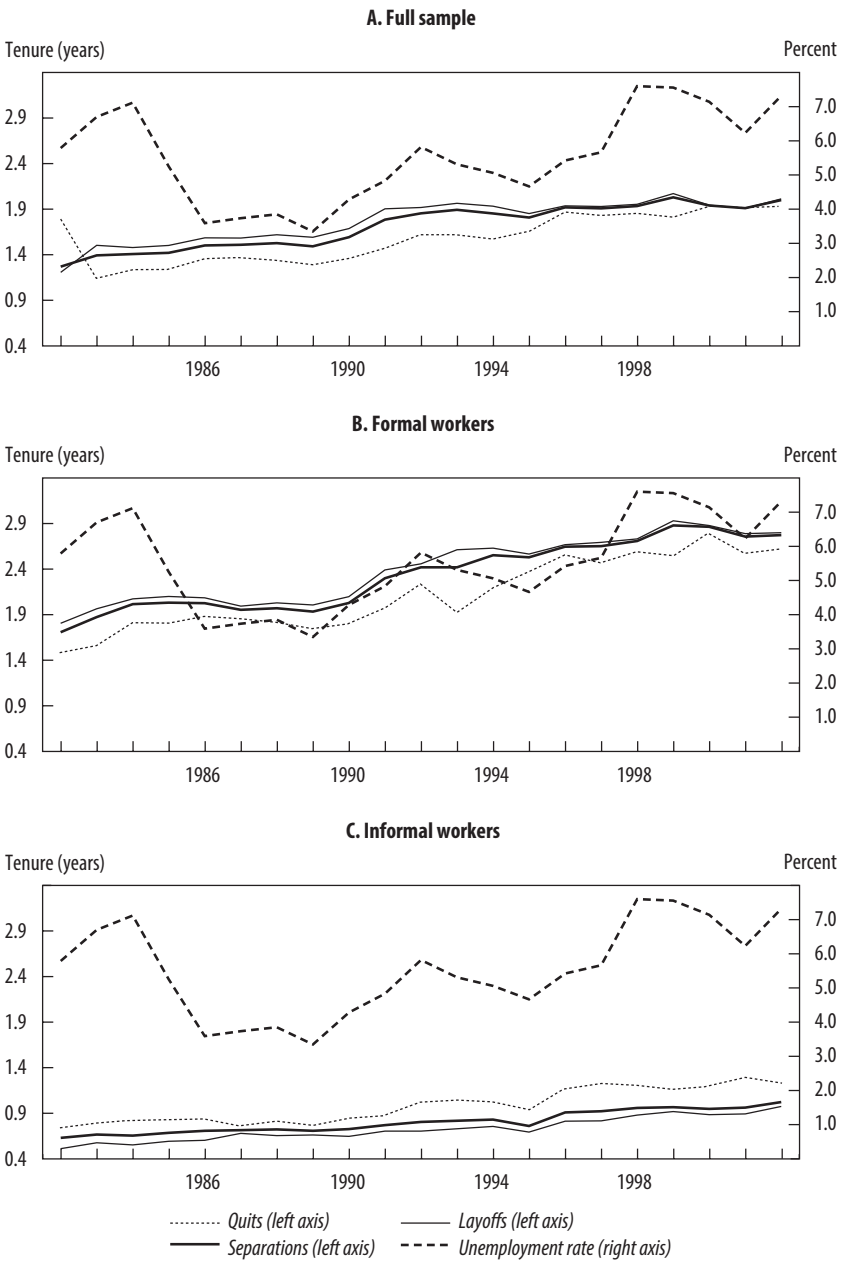
Number of years

Type of separation, type of contract, and job duration category	*1982*	*1984*	*1986*	*1988*	*1990*	*1992*	*1994*	*1996*	*1998*	*2000*	*2001*	*2002*
Layoff: formal workers												
0–3 months	0.11	0.12	0.12	0.12	0.12	0.12	0.12	0.13	0.12	0.12	0.11	0.12
More than 3 months	2.01	2.26	2.29	2.20	2.27	2.58	2.77	2.80	2.86	3.00	2.92	2.93
Total	1.81	2.07	2.09	2.03	2.10	2.45	2.63	2.67	2.73	2.88	2.79	2.80
Layoff: informal workers												
0–3 months	0.07	0.07	0.08	0.08	0.08	0.08	0.08	0.09	0.09	0.09	0.09	0.10
More than 3 months	0.98	1.04	1.02	1.05	1.02	1.05	1.15	1.19	1.27	1.27	1.24	1.33
Total	0.51	0.55	0.60	0.65	0.65	0.70	0.75	0.81	0.88	0.88	0.89	0.98
Quit with FGTS												
0–3 months	0.11	0.10	0.12	0.12	0.11	0.10	0.10	0.12	0.12	0.13	0.11	0.12
More than 3 months	1.93	2.26	2.35	2.23	2.25	2.58	2.58	3.05	3.13	3.43	2.98	3.11
Total	1.75	2.08	2.16	2.07	2.08	2.43	2.47	2.92	3.01	3.30	2.83	3.00
Quit without FGTS												
0–3 months	0.11	0.10	0.11	0.11	0.12	0.10	0.11	0.11	0.13	0.12	0.12	0.13
More than 3 months	1.33	1.58	1.68	1.50	1.50	1.84	1.67	2.04	2.01	1.91	2.18	2.02
Total	1.05	1.33	1.39	1.27	1.30	1.67	1.53	1.84	1.88	1.72	1.98	1.85

Layoff with FGTS												
0–3 months	0.12	0.12	0.13	0.12	0.12	0.12	0.13	0.13	0.13	0.12	0.11	0.12
More than 3 months	2.07	2.27	2.34	2.23	2.34	2.65	2.84	2.89	2.98	3.10	2.97	2.98
Total	1.90	2.11	2.16	2.08	2.19	2.54	2.72	2.76	2.89	3.00	2.85	2.87
Layoff without FGTS												
0–3 months	0.10	0.11	0.11	0.11	0.11	0.11	0.11	0.12	0.12	0.11	0.11	0.12
More than 3 months	1.62	2.22	1.97	2.00	1.84	2.09	2.18	2.34	2.42	2.32	2.57	2.57
Total	1.30	1.85	1.65	1.70	1.55	1.85	1.94	2.09	2.18	2.12	2.37	2.34
Formal workers												
0–3 months	0.11	0.11	0.12	0.12	0.12	0.11	0.12	0.12	0.12	0.12	0.11	0.12
More than 3 months	1.93	2.21	2.25	2.15	2.21	2.54	2.69	2.79	2.84	2.99	2.90	2.91
Total	1.71	2.02	2.03	1.97	2.03	2.40	2.55	2.65	2.71	2.86	2.76	2.77
Informal workers												
0–3 months	0.08	0.07	0.08	0.09	0.09	0.09	0.08	0.09	0.09	0.09	0.09	0.10
More than 3 months	1.12	1.14	1.13	1.11	1.10	1.16	1.22	1.31	1.35	1.33	1.31	1.37
Total	0.63	0.65	0.71	0.72	0.73	0.80	0.83	0.91	0.96	0.95	0.96	1.02

Source: Author's calculations based on data from Pesquisa Mensal de Emprego (PME), 1982–2002.

FIGURE 2. Average Duration of Employment



Source: Author's calculations based on data from Pesquisa Mensal de Emprego (PME), 1982–2002.

composition effect. Panel B shows that the average employment duration of formal workers has always been higher for those who were laid off than for those who quit, while Panel C indicates the opposite for informal workers: average employment tenure for those who quit is higher than for those who were laid off. Since the proportion of informal workers rose over time, the weight of informal workers in the total sample increased, which explains the convergence found among all workers. Nonetheless, average tenures increased over time for every subgroup in the sample.

Figure 3 shows how the employment tenure of formal workers varies over time considering also the information on whether the FGTS balance was received. Panel A contains the full sample, while panels B, C, and D present the same information by level of education (zero to four years of schooling, five to ten years, and eleven or more years). The figures reveal another interesting pattern: formal workers who withdrew their FGTS balances had longer tenure than those who did not, independent of whether they quit or were fired. This may reflect the fact that the amount in the FGTS accounts increases proportionately with tenure and, consequently, workers will be more aware of the funds the longer they are 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 the sample period. This group includes formal workers on probation who did not have the right to receive their FGTS, workers who were in firms that did not properly deposit the FGTS funds in the workers' accounts (these cases usually end up in labor courts), and workers who were fired with just cause (a small proportion of those fired). Panels B, C, and D illustrate similar patterns of average employment duration for the three education groups, and they confirm the positive relationship of job tenure and education found in other studies.

Number and Proportion of Unemployed Workers in Each Category

Figure 4 displays the average number of unemployed workers who had a previous formal job in each of the four categories—quit or layoff, received or did not receive 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 most workers were fired and received their FGTS balances; that the number of laid-off workers that received FGTS is countercyclical and varies significantly with the economic cycle; and that the number of

FIGURE 3. Average Duration of Employment and FGTS Withdrawal

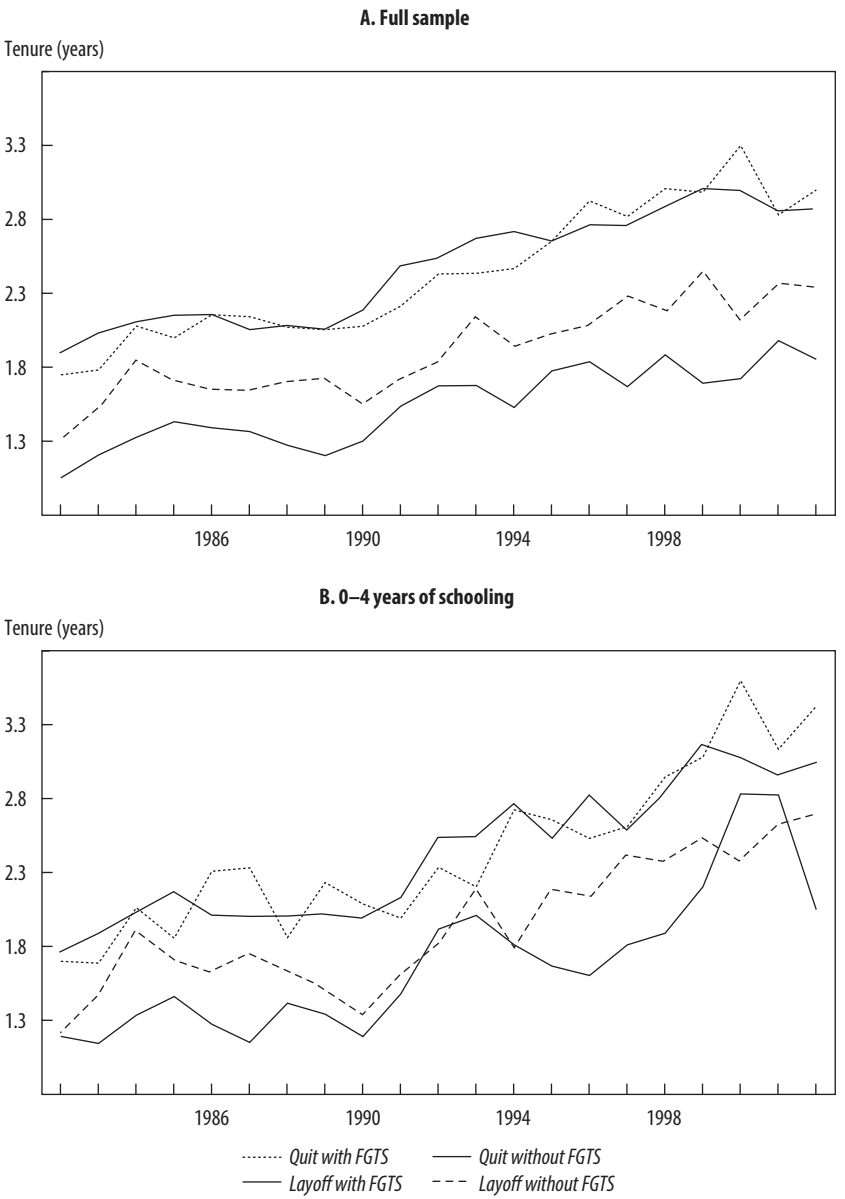
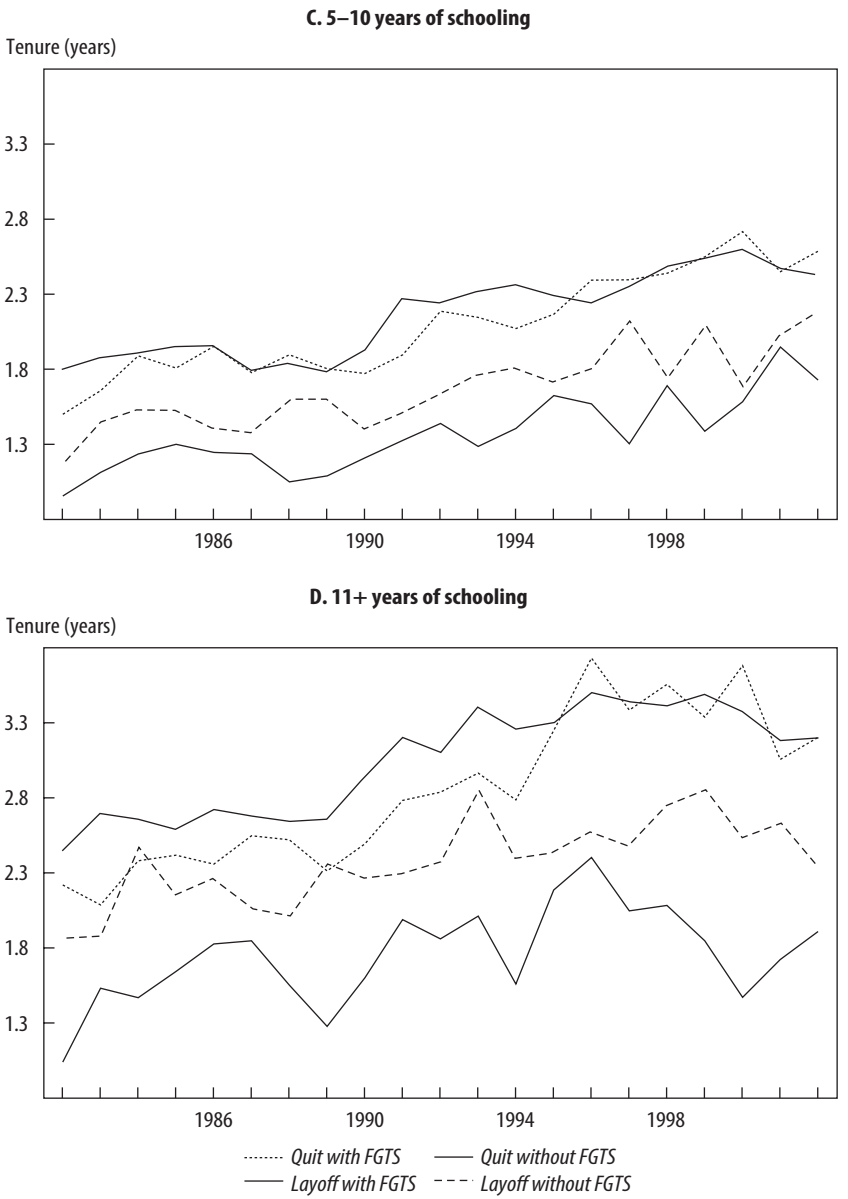


FIGURE 3. Average Duration of Employment and FGTS Withdrawal (*continued*)



Source: See figure 1.

FIGURE 4. Number of Unemployed Workers, by Reason of Separation and FGTS Withdrawal

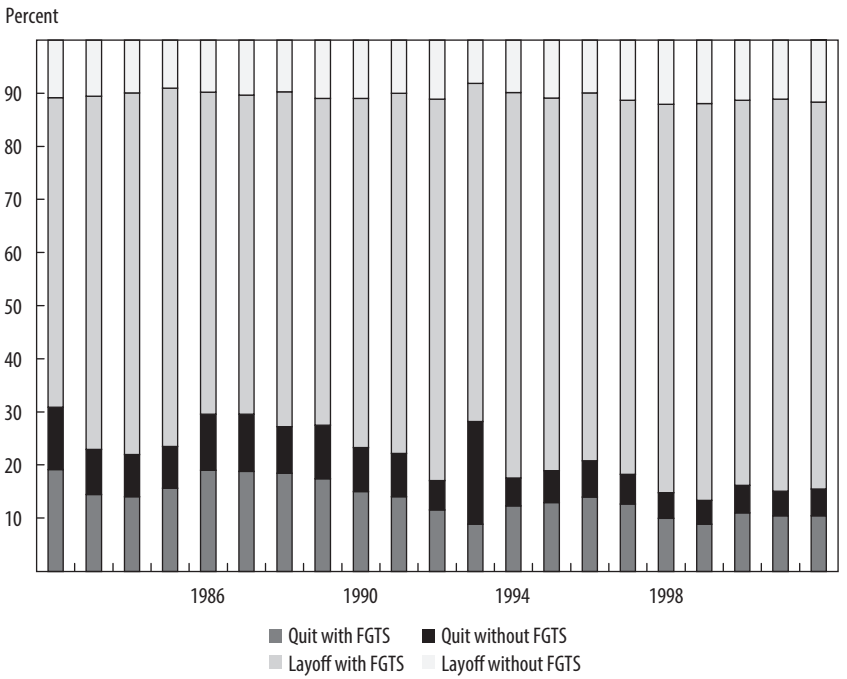
Source: See figure 1.

unemployed workers that were fired and did not receive their FGTS is fairly constant across the sample period. Figure 5 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.

Difference-in-Differences Analysis of the Effects of Legislative Changes on Employment Duration: Identification Strategy

This subsection studies 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 held by people who are currently unemployed, based on individual PME data from January 1982 to August 2002. The strategy is based on a difference-in-differences methodology that compares average employment duration for the periods before and after the legislative changes, for control and treatment groups. The idea is

FIGURE 5. Proportion of Unemployed Workers, by Reason of Separation and FGTS Withdrawal



Source: See figure 1.

to test the prediction that both legislative changes decreased labor turnover (or, equivalently, increased employment duration) for the treatment groups relatively to control groups. I use two of the three control groups proposed by Paes de Barros, Corseuil, and Bahia: informal workers and workers with employment spells shorter than three months (that is, workers on probation).[40]

The main differences between my work and that of Paes de Barros, Corseuil, and Bahia are as follows: I use employment duration data directly, instead of aggregate hazards; I use individual-level data, which allows me to control for observed characteristics and to interact some characteristics

40. Paes de Barros, Corseuil, and Bahia (1999). In a previous version of this paper, I also used a control group based on workers who quit and did not withdraw their FGTS balance, but this group is also directly affected by the legislative changes, since both changes made agreements harder to implement.

TABLE 2. Proportion of Unemployed Workers Whose Previous Job Had a Formal Contract, by Previous Employment Duration, Reason of Separation, and FGTS Withdrawal

Job duration and type of separation	*1982*	*1984*	*1986*	*1988*	*1990*	*1992*	*1994*	*1996*	*1998*	*2000*	*2001*	*2002*
0–3 months												
Quit with FGTS	0.16	0.12	0.16	0.15	0.14	0.12	0.11	0.11	0.09	0.09	0.11	0.08
Quit without FGTS	0.22	0.15	0.18	0.16	0.14	0.09	0.09	0.13	0.08	0.12	0.09	0.09
Layoff with FGTS	0.43	0.54	0.49	0.51	0.51	0.56	0.60	0.56	0.54	0.56	0.62	0.60
Layoff without FGTS	0.19	0.19	0.16	0.17	0.21	0.23	0.21	0.20	0.29	0.22	0.18	0.23
More than 3 months												
Quit with FGTS	0.20	0.14	0.19	0.19	0.15	0.12	0.12	0.14	0.10	0.11	0.10	0.11
Quit without FGTS	0.10	0.07	0.10	0.08	0.08	0.05	0.05	0.06	0.05	0.05	0.04	0.05
Layoff with FGTS	0.60	0.70	0.62	0.64	0.67	0.73	0.73	0.70	0.74	0.73	0.74	0.74
Layoff without FGTS	0.10	0.09	0.09	0.09	0.10	0.10	0.09	0.09	0.11	0.11	0.11	0.11
Total												
Quit with FGTS	0.19	0.14	0.19	0.18	0.15	0.12	0.12	0.14	0.10	0.11	0.10	0.10
Quit without FGTS	0.12	0.08	0.11	0.09	0.08	0.06	0.05	0.07	0.05	0.05	0.05	0.05
Layoff with FGTS	0.58	0.68	0.61	0.63	0.66	0.72	0.73	0.69	0.73	0.73	0.74	0.73
Layoff without FGTS	0.11	0.10	0.10	0.10	0.11	0.11	0.10	0.10	0.12	0.11	0.11	0.12

Source: See table 1.

with the dates of the legislative changes; I use the full sample before and after the Constitution and not arbitrary years; and I analyze the implications of the more recent legislative changes (Law 110 of September 2001) on labor turnover.[41]

The difference-in-differences estimators of the effect of the two legislative changes on employment duration are given, respectively, by the coefficients γ_2 and γ_4 in the following regression:[42]

$$d_{it} = \alpha + \beta \mathbf{X}_{it} + \gamma_0 \text{TREAT}_{it} + \gamma_1 8901_{it} + \gamma_2 \text{TREAT}_{it} * 8901_{it} + \gamma_3 \text{POST01}_{it} + \gamma_4 \text{TREAT}_{it} * \text{POST01}_{it} + \delta \mathbf{Y}_t + u_{it},$$

where i indexes each individual; t indexes each month; d is employment duration; $\mathbf{X}$ 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 and zero otherwise; POST01 is a dummy for the period after Law 110 came into effect, which equals one from October 2001 on and zero otherwise; and $\mathbf{Y}$ 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 earlier discussion, one should expect that coefficients γ_2 and γ_4 are positive, and that γ_4 is larger than γ_2. In other words, average employment duration of the treatment groups should have increased relative to the control groups after both the constitutional change and the implementation of Law 110.

An important caveat should be made before I present the results. Since this is a nonexperimental study, the estimated coefficients are subject to selection bias. Given that people self-select to control and treatment groups, these groups are likely to be composed of people with different characteristics that could react differently to other factors associated with the legislative 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

41. I also tried different dates around the legislative changes for robustness; the results are almost the same.

42. Kugler (2000).

TABLE 3. Summary Statistics of Treatment and Control Groups: Formal and Informal Workers

Percent share

	Formal workers			*Informal workers*		
Variable	*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
Men	63.51	61.30	58.28	55.17	52.76	49.94
0–4 years of schooling	32.89	21.67	14.18	41.31	27.21	15.00
5–9 years of schooling	41.55	41.88	34.50	40.62	43.02	37.76
10 + years of schooling	25.56	36.45	51.32	18.07	29.78	47.24
Average age (in years)	27.55	30.09	32.00	23.69	26.35	27.99
Manufacturing	35.93	30.11	24.70	15.65	15.58	13.84
Construction	11.93	7.68	6.42	14.47	12.39	9.81
Commerce	17.76	19.99	21.35	14.61	15.36	16.40
Services	32.76	40.69	46.19	49.47	51.53	54.69
Other sectors	1.62	1.53	1.35	5.81	5.14	5.26

Source: See table 1.

characteristics, and in 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.[43]

Tables 3 and 4 present some summary statistics that describe the basic characteristics of the treatment and control groups before and after the legislative changes. Table 3 displays the statistics for formal and informal workers and for short- and long-duration workers. Note that the composition of both pairs of treatment and control groups changed over time.

Table 3 shows a decrease in the proportion of formal workers from 59.2 to 55.6 percent 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, with a decrease in the proportion of men, an increase in the years of schooling, and an increase in the average age. The only important difference was in

43. Control groups could be at least indirectly affected by the legislative 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 Paes de Barros, Corseuil, and Bahia, 1999).

TABLE 4. Summary Statistics of Treatment and Control Groups: Short- and Long-Duration Workers
Percent share

	Short-duration workers			*Long-duration workers*		
Variable	*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
Men	65.42	62.44	60.11	63.29	61.23	58.16
0–4 years of schooling	37.53	26.37	13.11	32.39	21.38	14.24
5–9 years of schooling	44.21	44.88	37.18	41.26	41.70	34.37
10 + years of schooling	18.26	28.76	49.72	26.35	36.92	51.38
Average age (in years)	25.46	26.98	29.07	27.77	30.28	32.15
Manufacturing	33.16	28.78	21.89	36.24	30.20	24.86
Construction	19.46	13.75	11.59	11.10	7.30	6.15
Commerce	20.94	21.81	28.04	17.42	19.88	21.00
Services	25.74	34.80	37.20	33.52	41.05	46.65
Other sectors	0.70	0.85	1.29	1.72	1.57	1.34

Source: See table 1.

sectoral allocation. Formal workers significantly decreased their share in the manufacturing sector and significantly increased their share in services, while the share of informal workers remained almost constant in the manufacturing sector and increased slightly in services.

With regard to time on the job, table 4 shows that the size of the control group composed of workers with short tenure is much smaller than the group of workers with long tenure and decreased across the three periods. The two groups are very similar, however, and their basic characteristics moved in the same direction over time.

The information presented in tables 3 and 4 confirms the empirical strategy, since it suggests that the compositions of the two pairs of treatment and control groups moved in very similar ways across the three periods. They also suggest that controlling for observable individual characteristics is essential for correctly identifying the effects of the legislative changes.

The use of aggregate variables is intended to control for cyclical and structural changes in the Brazilian economy around the time of the legislative changes, as these could have affected treatment and control groups differently and thus contaminated the identification of the dismissal cost effects. I therefore control for unemployment, since turnover measures tend to display cyclical movements that are different for treatment and control groups. Trade liberalization is another important change that occurred

around the implementation of the 1988 Constitution.[44] I 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. I also interact the treatment dummies with the post-legislative-change dummies and sector dummies to check whether the difference-in-differences estimators in tradables sectors (manufacturing) are different from nontradables sectors. Finally, I also include the aggregate inflation rate in the controlled regressions. Among the many possible effects of inflation on employment turnover, inflation acceleration tends to lead to a decline in the real purchasing power of FGTS balances, which should increase the incentive of any workers who are thinking of leaving their job to make efforts to get fired in order to withdraw the fund.

Other structural changes not considered or inadequately measured here might still affect the results. In particular, the unemployment insurance program created in 1986 was significantly upgraded in 1990. Formal workers who became eligible for unemployment benefits would have an additional incentive to make agreements with their employers to get fired so as to collect them. This has the effect of increasing turnover rates, because this additional incentive to get fired is not offset by an increase in employers' resistance to making 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, the timing of the program expansion is different from the timing of the 1988 Constitution promulgation and very distant from the 2001 legislative change.

Difference-in-Differences Analysis: Results

Tables 5 and 6 contain the results of the difference-in-differences analysis. Each table presents the results of the regression estimation for one choice of control group: informal workers in table 5, and short-duration workers in table 6. Each table has two columns, presenting, respectively, the estimation results without and with controls for observable characteristics and

44. Trade liberalization was implemented in Brazil first through the removal of nontariff barriers in 1988–89 and then through an aggressive program of import tariff reduction, especially between 1990 and 1995.

TABLE 5. Determinants of Employment Duration—Control Group: Informal Workers[a]

	(1)		(2)	
Variable	*Coefficient*	*Standard error*	*Coefficient*	*Standard error*
Constant	0.757	0.011	0.808	0.038
Formal	1.256	0.014	1.021	0.014
1989–2001	0.177	0.014	–0.106	0.017
Formal × 1989–2001	0.412	0.018	0.362	0.017
Post-2001	0.271	0.030	–0.260	0.035
Formal × post-2001	0.491	0.040	0.402	0.038
Aggregate variables				
Unemployment rate			0.018	0.004
Inflation rate			–0.001	0.000
Openness			0.004	0.002
Gender				
Male			0.179	0.008
Education				
5–10 years of schooling			0.222	0.010
More than 10 years of schooling			0.728	0.011
Industry				
Manufacturing			–0.790	0.024
Construction			–1.915	0.025
Commerce			–1.126	0.024
Services			–1.004	0.023
Age				
26–35 years			0.708	0.009
36–45 years			1.798	0.012
46–55 years			2.668	0.018
56–65 years			3.354	0.035
> 65 years			0.827	0.028
Metropolitan regions				
Belo Horizonte			0.121	0.014
Recife			0.085	0.014
Rio de Janeiro			0.116	0.014
Porto Alegre			–0.060	0.015
São Paulo			0.236	0.013
Seasonal dummies	yes		yes	
No. observations	502,901		498,581	
R^2	0.07		0.16	

a. The dependent variable is employment duration. The model is estimated using OLS. The sample is the IBGE's monthly employment survey (PME) for January 1982 through August 2002.

TABLE 6. Determinants of Employment Duration—Control Group: Short-Duration Workers[a]

	(1)		(2)	
Variable	*Coefficient*	*Standard error*	*Coefficient*	*Standard error*
Constant	0.114	0.032	–0.622	0.068
Long duration	2.093	0.034	1.703	0.032
1989–2001	0.005	0.046	–0.354	0.045
Long duration × 1989–2001	0.532	0.048	0.467	0.045
Post-2001	0.002	0.130	–0.738	0.126
Long duration × post-2001	0.699	0.134	0.660	0.125
Aggregate variables				
Unemployment rate	–		0.020	0.005
Inflation rate	–		–0.001	0.001
Openness	–		0.005	0.003
Gender				
Male	–		0.261	0.012
Education				
5–10 years of schooling	–		0.314	0.015
> 10 years of schooling	–		0.982	0.016
Industry				
Manufacturing	–		–0.202	0.046
Construction	–		–1.614	0.049
Commerce	–		–0.587	0.047
Services	–		–0.552	0.046
Age				
26–35 years	–		0.992	0.013
36–45 years	–		2.391	0.017
46–55 years	–		3.353	0.025
56–65 years	–		4.156	0.050
> 65 years	–		1.214	0.043
Metropolitan region				
Belo Horizonte	–		0.193	0.020
Recife	–		0.080	0.022
Rio de Janeiro	–		0.122	0.021
Porto Alegre	–		–0.060	0.020
São Paulo	–		0.333	0.019
Seasonal dummies	yes		yes	
No. observations	310,404		307,790	
R^2	0.04		0.16	

a. The dependent variable is employment duration. The model is estimated using OLS. The sample is the IBGE's monthly employment survey (PME) for January 1982 through August 2002.

aggregate variables. In regressions not reported here, I also included interactions of the reform changes effect term (treatment dummy interacted with post-legislative-changes dummies) with gender, education, and sector dummies, respectively.[45] The idea is to identify differential effects of the legislative changes for disaggregations of these variables.

The results in table 5 show significant coefficients and expected signs for most of the variables included. Column 1 presents the results for the unconditional regressions. The difference-in-differences estimators γ_2 and γ_4 are 0.412 and 0.491, respectively. Both are significantly different from zero and estimated with small standard errors. Note that the effect of the more recent legislative change on increasing employment duration (γ_4) was found to be larger than the coefficient on the interaction of the formal dummy with the 1989–2001 dummy (γ_2), which captures the constitutional change effects. This implies that the more recent legislative 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 5, column 2), the difference-in-differences estimators γ_2 and γ_4 drop to 0.362 and 0.402, which is significantly lower than in column 1 but which still represents a sizable increase in employment duration after both legislative changes. These results confirm that the increase in dismissal costs implied by the two legislative changes reduced turnover even when the regressions control 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, which is a typical finding in the literature. The inflation rate coefficient is negative and significantly different from zero, as expected. The measure of openness is positively correlated with employment duration. Part of the increases in employment duration observed in the 1990s is thus attributable to the high unemployment, low inflation, and increased openness that characterized the decade, which reversed the sign of the coefficients of the post-legislative-changes dummies alone (1989–2001 and post-2001).

45. See Gonzaga (2003) for the full results.

The second column of table 5 also shows the effects of individual characteristics on employment duration. The results are also as expected: male workers have longer employment tenures; employment duration significantly increases with education (workers who have completed high school or more, for instance, have tenures 0.73 year longer than those with zero to four years of study, the omitted group); employment spells tend to increase with age up to a certain point (sixty-five years); the ranking of employment tenures places the construction sector in last position, preceded by 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 has the lowest employment duration).

Interacting the legislative changes effect term with gender reveals that the effect of the constitutional change on the average duration of formal workers was higher for male workers than for female workers. There was no significant gender difference for the most recent legislation change.

Interacting the legislative changes effect term with education shows that the constitutional change effect was also higher for more educated workers than for workers with low education and much higher than for workers with an intermediate level of education. The 2001 legislative change, on the other hand, significantly decreased the employment duration of workers with intermediate-level education relative to the low-education group.

Finally, the results of interacting the legislative changes effect term with each sector dummy reveals that the constitutional change increased the 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 6, which uses workers with short duration (less than three months of tenure) as the control group. The difference-in-differences estimators γ_2 and γ_4 in the unconditional regressions (column 1) are 0.532 and 0.699; both are significantly different from zero. The regressions with controls for observed characteristics and aggregate variables (column 2) produce difference-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 legislative changes increased the

employment duration of the affected group (workers with more than three months on the job) relative to the control group; the result holds when controlling for aggregate variables and individual characteristics.

The coefficients of the three aggregate variables and of all individual characteristic variables in table 6 are very similar to and have the same signs as those presented in table 5. The fact that the size of this control group is much smaller than the previous one means that the coefficients on the interactions with disaggregations of gender, schooling, and sector are much less precisely estimated. The results of interactions of the legislative changes terms with gender, schooling, and sector (not shown here) show only significant effects for the disaggregations of schooling, which are very similar to those obtained for the previous control group: the constitutional change effect is higher for the more educated workers than for workers with a low level of education; and the employment duration of workers with an intermediate level of education in the treatment group relatively decreased after both legislative changes.

Difference-in-Differences Analysis of the Probability of Quitting and Receiving FGTS

This subsection exploits the temporal variation of the proportion of fake layoffs around the time of the constitutional change and introduction of Law 110 to test whether the incidence of this type of agreement decreased after the dismissal fine was increased by the two legislative changes, as predicted. Table 7 presents the results of logit regressions based on data on the proportion of workers making fake layoff agreements (from table 2 and figure 5). In particular, I run a logit regression in which the dependent variable is a dummy variable that takes the value of 1 for formal workers who quit and received their FGTS balances; the value of zero is assigned to other formal workers (workers who quit and did not receive their FGTS balances and all workers who were laid off). Since people who quit do not have the right to withdraw their FGTS balances, the dependent variable is an indicator of a fake layoff.

Because PME is a household survey, the reason for job termination is given from the perspective of the worker and is thus subjective. The information on whether the worker received the FGTS balance is useful for inferences about the frequency of fake layoff agreements, but it is an

TABLE 7. Logit-Probability of Quitting with FGTS[a]

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)
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)
Manufacturing sector	–0.165	–0.163
	(0.043)	(0.043)
Construction sector	–0.457	–0.453
	(0.048)	(0.048)
Commerce sector	0.046	0.048
	(0.044)	(0.044)
Services sector	0.034	0.036
	(0.043)	(0.043)
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 × 1989–2001	—	–0.007
		(0.003)
Years of schooling × post-2001	—	–0.021
		(0.009)
Seasonal dummies	yes	yes
Log-likelihood	–116,226.1	–116,221.1
No. observations	302,336	302,336

a. The dependent variable is a dummy that takes the value of 1 for formal workers who quit and received their FGTS balance and a value of zero for other formal workers (workers who quit and did not receive their FGTS balances and all workers who were laid off). Standard errors are in parentheses.

imperfect check. It should work for nonlitigious cases in which the firm agrees to fire with just cause. In litigious cases, however, in which workers who want to leave force their own dismissal to receive the FGTS balance plus the penalty, it is not clear how these workers would respond in the survey. Consequently, at least some workers who answered that they were laid off and received the FGTS balances were probably workers who wanted to leave and forced their own dismissal. This means that they were also affected by the legislative changes.

The results in column 1 of table 7 show that both dummies representing the post-legislative-change periods are significant and negative. The coefficient on the dummy for the post-2001 period is larger, in absolute value, than the one for the post-constitutional-change period. Both results are expected from the previous discussion: they suggest that the incidence of fake layoff agreements decreased after both legislative changes.

The regression also controls for the unemployment rate and individual characteristics. The results show that the probability of a fake layoff agreement is procyclical, in that it decreases with the unemployment rate. This implies that it is harder to make this kind of agreement in tough times. The coefficients on individual characteristics show that the probability of a fake layoff is lower for male than for female workers; increases with years of schooling; decreases with age; is lower in construction and manufacturing than in other sectors; and is higher in Belo Horizonte, Porto Alegre, and São Paulo than in other metropolitan regions.

The result that workers with more schooling are more likely to quit and receive their FGTS balances than those with less schooling might be interpreted in two ways. First, since there is a high correlation between wages, schooling, and employment duration, those with more years of schooling probably have more money in their FGTS accounts, which implies that they should care more about withdrawing it. The second interpretation is that those with more years of schooling are likely to hold jobs that are higher up in the firm's hierarchy, which increases their probability of convincing their employers that that they would not break the agreement.

Column 2 includes the interaction of the two dummies for the post-legislative-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 legislative changes than those with less schooling, resulting in a greater reduction in the proportion of fake layoff agreements among this group.

Conclusions

This paper studied the links between labor legislation and labor turnover in Brazil. It presented the main features of the Brazilian labor legislation, with emphasis on those measures that affect labor turnover. In particular, I analyzed in detail the argument of many authors that the design of some job security provisions in Brazil creates perverse incentives that increase labor turnover. I 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. Some implications of these arguments that were overlooked in the previous literature could be tested in the data.

The three main features of the FGTS system that create perverse incentives are the below-market interest rates, the fact that workers access their FGTS money primarily by being fired, and the fact that the firing penalty is paid to the worker. Workers thus have a strong desire to access their FGTS accounts, combined with the possibility of negotiating with their employers about not paying the penalty. Any FGTS overhaul package should reverse these three main features. One proposal is to set returns on FGTS account balances that increase proportionately with employment tenure, so as to reward longer employment spells. A second, and more important, reform is that the firing fine should not be paid directly to the employee. The FGTS system is designed to provide reasonable insurance for those who lose their jobs, although most prefer to make private arrangements. The firing fine, however, is a big distortion. A proposal that preserves dismissal costs should consider increasing the proportion that goes to the government (g, in the paper's 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 confirms 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. A simple difference-in-differences methodology was applied to monthly individual data from the PME, which has information on previous employment spells for those currently unemployed. This provided the basis for studying the effects on employment duration of the increases in job termination costs implemented in the 1988 Constitution and in Complementary Law 110 of September 2001. The methodology exploited the fact that those changes should have had different effects for different

groups of workers. The exercises 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 control groups of workers was observed after both legislative changes. Finally, the paper also provided evidence that both legislative changes reduced the probability of fake layoffs, although there are still a high number of such agreements being made between workers and their employers.