

LABOR MARKET REGULATIONS AND THE DURATION OF EMPLOYMENT IN BRAZIL

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Originally published by Ipea in October 1999 as number 676 of the series Texto para Discussão.

Brasília, January 2015

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1. The authors would like to thank the support of Phillippe Leite, from Diretoria de Política Social do Ipea, Rosane Mendonça, from Diretoria de Política Social do Ipea and UFF, and the comments of Ricardo Henriques da Diretoria de Política Social do Ipea and UFF and Sérgio Firpo from Diretoria de Política Social do Ipea and PUC-RJ.

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DISCUSSION PAPER

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Discussion paper / Institute for Applied Economic

Research.- Brasília : Rio de Janeiro : Ipea, 1990-

ISSN 1415-4765

1. Brazil. 2. Economic Aspects. 3. Social Aspects.
I. Institute for Applied Economic Research.

CDD 330.908

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RESUMO

O objetivo deste trabalho é estimar o impacto do aumento do valor das multas pagas aos trabalhadores demitidos na duração do emprego. Este valor foi bastante elevado pela Constituição de 1988. Além de uma simples comparação da duração em períodos anteriores e posteriores à Constituição, são implementadas duas alternativas para estimar o impacto mencionado.

A primeira faz uso de um método quase-experimental que permite isolar o impacto da regulação do impacto oriundo das transformações macroeconômicas experimentadas pelo Brasil. Este método se baseia em uma divisão da população em dois grupos denominados controle (não afetado pela regulação) e tratamento. O contraste entre a evolução dos dois grupos nos fornece uma estimativa do impacto da regulação.

A segunda alternativa se baseia em regressões envolvendo estimativas da duração de emprego bem como um indicador da mudança constitucional controlando por uma série de indicadores macroeconômicos. O coeficiente associado ao indicador da constituição é tomado como uma estimativa do impacto da regulação. Este procedimento também permite checar se os grupos usados como controle atendem os requisitos necessários para desempenhar este papel.

ABSTRACT

The objective of the paper is to estimate the impact of an increase of dismissal penalties, implemented by the 1988 Brazilian constitution, on the duration of employment spells. After a simple comparison of estimates of this variable in periods pre and post 1988 we made use of two alternative methods to estimate the impact we are looking for.

The first method use control groups in order to separate the impact of underlying changes in the macroeconomic environment from the impact of the constitutional change. Accordingly, we proceed by breaking down the overall population into two groups, the so-called the treatment and control groups and then apply what is known as *differences-in-differences* methodology.

The second method would essentially consist of regressing monthly or yearly estimates for the duration of employment spells on an indicator for the constitutional change, controlling for a set of macroeconomic indicators. The estimated coefficient on this indicator would then be an estimate of the impact of the constitutional change on the hazard rate. This procedure also allow us to test the validity of all control groups on which we base part of our empirical analysis.

1 - INTRODUCTION

Labor market regulations are invariably introduced with two objectives. To improve the welfare of the labor force is the first one, even at the cost of introducing some degree of economic inefficiency. The second consists on improving efficiency, when external factors and/or other labor market imperfections are present.

These regulations, may eventually become inadequate due to an unsuitable original design or unexpected changes in the economic environment. This inadequacy may lead to results contrary to the original goals of labor market regulations. Consequently, as a general rule, labor market regulations (as any other market regulation) need to be constantly evaluated and updated if their original goals are to be preserved.

However, any empirical study of the impact of labor market regulations on labor market performance faces three main difficulties. First, one has to face the facts that labor market regulations do not change very often and tend to apply universally to all sectors in the economy. Hence, variations in labor market regulations, that are necessary to identify their impact on labor market performance, are hard to find, both in time series and cross-sections.

Secondly, even when legislation varies over time, it is difficult to isolate its impact on labor market performance from the impact of other macroeconomic factors. This is particularly important in Brazil since, over the past two decades, macroeconomic instability has reached unprecedented levels. Inflation, economic growth, internal and external imbalances and the degree of openness of the economy have changed considerably. If one opts for using cross-section variations, the drawbacks are not less. In this case, it is necessary to isolate the impact of differences in regulations from all other sector-specific factors that could make performance measures different across sectors.

Finally, one must have measures of labor market performance. The problem here is that performance is a multi-dimensional aspect of labor market with no consensus about its precise definition. Hence there is not a single uni-dimensional measure for this aspect. The use of the measure for the (supposed) main dimension is usually implemented as a measure of labor market performance.

In respect to Brazilian labor market, many analysts have been very critical about the benefits of the prevailing labor market regulations.¹ On the whole these regulations were designed to improve the welfare giving the workers more protection. The analysts claim these regulations have not been wisely designed and, consequently, are failing to reach their objective. Actually their arguments go

¹ See Jatobá (1994) for a survey of those analysis that consider that a higher non-wage labor cost reduce the job creation. This survey includes the arguments of Bacha, Mata and Modenesi (1972), Camargo and Amadeo (1990), Almeida (1992), Chaad (1993), Macedo (1993), Pastore (1993) and World Bank (1991).

further claiming that the regulation worsened not only the welfare of the labor force, but also the efficiency, based on the observation of increasingly poor working conditions and lower wages and a drop in the degree of employability of the Brazilian labor force. They argue this occur in a new economic environment that increasingly requires greater labor flexibility. As a consequence, labor market regulation reform has become a central item on the current Congress agenda, particularly after the recent leap in unemployment.²

Despite the importance of evaluations of the impact of these regulations on labor market performance, the number of such studies focusing on Brazilian labor markets has been very limited.³ The three difficulties pointed out are not sufficient to justify the relatively few studies on the subject. First labor market regulations underwent considerable changes in 1988, when a new Constitution was enacted, containing most of the prevailing labor market regulations. Moreover the wealth of information available allows the implementation of promising methodological possibilities for identifying the impact of labor market regulations based on alternative proxies of labor market performance that can be obtained using the information available.

Hence, the objective of this paper is to identify if the prevailing Brazilian labor market regulations, in large extension originated by the 1988 constitutional change, has any impact on labor market performance. To reach this objective we will explore alternative sources of information.

The paper is organized in six sections including this introduction. In the following three sections we describe how we address each of the three difficulties mentioned above. First we briefly describe the 1988 constitutional change, with special emphasis on the topics related to labor costs which, basically, will be used as the main sources of variation on labor market regulations. Next, Section 3 describes the difference in difference methodology, used to estimate the impact of the constitutional change and separate it from the impact of underlying changes in the macroeconomic environment. Section 4 presents the necessary assumptions in order to obtain consistent estimates of measures for the duration of employment spells, our proxy for the labor market performance. Section 5 then describes our empirical strategy for estimating the impact of changes in legislation on the duration of employment spells. and presents an analysis of the empirical evidence. Section 6 summarizes the main findings and presents the main conclusions of the study.

² Deseasonalized unemployment in the six main Brazilian metropolitan regions increased from around 5,7% in October 1997 to 7,4% in June 1998.

³ Some examples are Amadeo, *et alii* (1995), Amadeo and Camargo, (1993 and 1996) and Málaga (1992).

2 - THE 1988 CONSTITUTIONAL CHANGE

A new Brazilian Constitution was enacted in 1988 as part of the process of re-democratization in Brazil during the second half of the 1980s. Traditionally, Brazilian constitutions are very detailed, stipulating not only general rules, but also many specific legal provisions. Most labor regulations, for instance, are written in the Constitution and are, consequently, very difficult to amend. The new Constitution of 1988, in particular, considerably affected labor regulations, causing changes in many labor codes that had remained intact since the 1940s.⁴ Most of these changes, in tune with the re-democratization environment, increased the degree of the workers' protection.

These changes, shown in Table 1, affected both individual rights and workers' organizations. The new Constitution gave more freedom and autonomy to unions. The possibilities for government intervention in unions were drastically reduced. In fact, many mechanisms of official interference were eliminated (e.g. the right of intervention by the Ministry of Labor and the need to be registered and approved at the same Ministry), as well as many restrictions of an institutional nature used to limit workers' organizations (representation scales; diversity of occupational categories). Many regulations on union management were also weakened, ensuring more autonomy to unions during elections of their representatives and in their decisions.

From the point of view of individual rights, we can perceive important changes that increase variable labor costs and the level of dismissal penalties. The increase in protection ensured by the new Constitution considerably increased a firm's costs of employment. The maximum number of working hours per week dropped from 48 to 44 hours; the maximum number of hours for a continuous work shift dropped from eight to six hours; the minimum overtime premium increased from 20% to 50%; maternity leave increased from three to four months; and the value of paid vacations increased from 1 to, at least, 4/3 of the normal monthly wage.

The new Constitution also considerably increased the level of dismissal penalties. This change in legislation will be one of the fundamental sources of variation used throughout this study to estimate the impact of regulations on labor market performance.

⁴ One major exception were the rules regulating dismissals that suffered major changes in 1966, when the *FGTS* was created.

Table 1

Changes Introduced by the New Constitution Promulgated in October 1988

Pre-Constitution	Post-Constitution
Individual Rights	
1- Maximum working hours per week = 48 hours	1- Maximum working hours per week = 44 hours.
2 - Maximum daily journey for continuous work shift = 8 hours.	2- Maximum daily journey for continuous work shift = 6 hours.
3 - Minimum over-time remuneration = 1,2 of the normal wage rate.	3- Minimum over-time remuneration = 1,5 of the normal wage rate.
4 - Paid vacations = at least the normal monthly wage.	4- Paid vacations = at least 4/3 of the normal monthly wage.
5 - Maternity license = 3 months (1 before and 2 after the birth).	5- Maternity license = 120 days.
6 - Previous notification of dismissal = one month.	6- Previous notification of dismissal = proportional to seniority (to be regulated by a future law).
7 - Fine for non-justified dismissal = 10% of Fundo de Garantia por Tempo de Serviço (FGTS)	7- Fine for non-justified dismissal = 40% of Fundo de Garantia por Tempo de Serviço (FGTS).
	8- Creation of paternity license of 5 days.
	9- Profit-sharing (regulated by a 1996/97 law).
Unions Organization	
A) The Ministry of Labor had the right to intervene in the unions and depose their board of directors.	A) The Ministry of Labor is forbidden to intervene in the unions.
B) Every union had to be registered and approved at the Ministry of Labor.	B) Unions do not need to be registered and approved at the Ministry of Labor.
C) National representation of unions was allowed only in exceptional cases.	C) National representation of unions is allowed.
D) Union's representatives were elected by a minimum quorum of 2/3 of the members in the first balloting, 1/2 in the second balloting and 2/5 in the third balloting. In the case of no minimum quorum for the election, the Ministry of Labor could chose union's directors and call another election.	D) Union's representatives are elected following union's own rules.
E) Workers (employers) unions were allowed to be formed by only one type occupational (economic) category.	E) Workers (employers) unions are allowed to be formed by different types of occupational (economic) categories.
F) Union's decision to go on strike had to be approved by a minimum quorum of 2/3 of union's members in the first calling and 1/3 in the second calling.	F) Union's decision to go on strike follows union's own criterias.
G) In case of strike, notification to the employer had to be done 5 days in advance.	G) In case of strike, notification to the employer has to be done 48 hours in advance.
H) Strikes were forbidden in activities considered fundamental (e.g. energy and gas services, hospitals, pharmacies, funeral services); public servants were not allowed to go on strike.	H) There are not any more sectors in which strikes are forbidden: in essential activities, workers and employers are responsible for the provision of minimum services; public servants (excluding military personnel) are allowed to go on strike.

Source: CAMARGO and AMADEO (1990) and NASCIMENTO (1993).

It is worth mentioning that the changes altered the level of the penalties but not their nature. Traditionally, Brazilian legislation affects the cost of dismissal through two channels. First, employers must give notice to their employees in the case of dismissal. Moreover, between the notice and actual dismissal workers are granted two hours per day to look for a new job, with no cut in wages. Secondly, the law states that all workers dismissed for no just cause must receive monetary compensation paid by the employer.

Prior to the 1988 Constitution, notice had to be given at least one month in advance. The 1988 Constitution states that the period of notice should be given in proportion to the worker's tenure. However, since no specific law has ever regulated this constitutional device, notice continues to be given, as before 1988, one month prior to dismissal for all workers, independent of their tenure. Hence, it cannot be used as our source of variation in labor regulations.

With respect to the monetary compensation for dismissed workers, the law states that a fixed percentage of the *Fundo de Garantia por Tempo de Serviço (FGTS)*, a sort of job security fund accumulated while the worker was employed by the firm, is to be paid to every worker dismissed for no just cause. There was a fourfold increase in the value of this penalty as a result of the 1988 Constitutional change.

FGTS basic characteristics are: *a)* each worker in the formal sector has his own fund, in other words, it is a private fund, instead of a single fund for the workers as a group; *b)* to build the fund of each individual worker, the employer must contribute every month with the equivalent of 8% of his employee's current monthly wage, consequently, the accumulated *FGTS* of a worker in any given firm is proportional to the worker's tenure and his/her average wage over his/her stay in the firm; *c)* the fund is administrated by the government; *d)* workers have access to their own fund only if dismissed without just cause or upon retirement,⁵ *e)* if they resign they are *not* granted access to this fund; and *f)* on dismissal, workers have access to their entire fund, including all funds accumulated in previous jobs, plus a penalty in proportion to their accumulated fund in the job from which they are being dismissed.⁶

Before 1988, this compensation was equal to 10% of the cumulative contribution of the current employer to the worker's *FGTS*. After 1988, this penalty was increased to 40% of the employer's cumulative contribution to the worker's *FGTS*. As the monthly rate is 8% of the monthly wage, the *FGTS* accumulates at a rate of approximately one full monthly salary per year in the job. So, quantitatively, the penalty accumulates in a rate equivalent to 40% (10% prior to 1988) of the worker's current monthly wage per year in the firm. This compensation was certainly very small prior to 1988. In fact, under the former Constitution, the worker had to be employed in the firm for at least ten years in

⁵ There are a few exceptions. Workers can use their *FGTS* as a part of the payment for acquiring their home. They also can use it to pay for large health expenses.

⁶ The *FGTS* is a fund created by the military regime in 1966 to serve as an alternative to the job security law prevailing at that time. In practice, all new contracts after 1966 adopted the new system, since it was preferred by both employees and employers.

order for the compensation to reach the magnitude of one monthly salary. Now it takes 2.5 years in the job for the compensation reach this value.

As far as incentives are concerned, it is worth emphasizing that the penalty is paid *by the employer to the employee*, as opposed to the employer's paying into a social fund held for all workers as a group. In other words, the dismissed worker receives the penalty on an individual basis. This characteristic of the law has well-established and major negative effects on the workers' behavior, giving them significant incentives to induce their own dismissal [see Macedo (1985) and Amadeo and Camargo (1996)]. There are two main reasons for these negative effects. On one hand, we see that the *FGTS* penalty is received individually by workers if they are dismissed. Furthermore, being fired is the chief mechanism to achieve access and control over their overall *FGTS*. On the other hand, there are strong incentives for workers to seek access to their *FGTS*. First, because the *FGTS* has been poorly managed by the government, typically generating negative real returns or returns well below market rates.⁷ Secondly, because, due to short-sightedness or credit constraints, workers may be heavily discounting the future. In short, the facts that *a)* all dismissal penalties are immediately received individually by the dismissed worker and *b)* being dismissed is the chief mechanism for workers to acquire control over their own fund that is poorly managed by the government, give them considerable incentives to induce their own dismissal after a certain time in any job.

Lastly, it is worth mentioning that, despite the 1988 fourfold increase in the *FGTS* penalty, it is not clear that, even now, this penalty constitutes a major constraint to dismissals or even a major fraction of overall dismissal costs. For instance, the cost of advance notice may be larger than the penalty. In principle, the need for notice would increase the cost of dismissal only to the extent that, for a period of one month, 25% of the hours of the dismissed worker would be paid but not worked. In practice, the productivity of a dismissed worker will drop once he/she has been given notice, implying an overall decline of well over 25% in his/her contribution to production. As a result, it is not uncommon for firms to pay a full salary to dismissed workers, without their being required to work a single hour. In other words, the cost of notice is actually between 25% and 100% of one month's salary, being in practice closer to 100% than to 25%.

Consequently, the costs of advance notice tend to be higher than the dismissal compensation paid to all workers with tenure of less than 2.5 years. Since most employment relationships in Brazil are short, employers may be more sensitive to the cost of advance notice than to the value of the dismissal compensation.

3 - THE DIFFERENCES IN DIFFERENCES METHODOLOGY

Accordingly to this methodology, we began by breaking down the overall population into two groups, the so-called the treatment and control groups. If this

⁷ See Almeida and Chautard (1976) for a broad analysis of the *FGTS*, including topics such as management of the fund and workers welfare.

partition attend some properties, the evolution of the performance for the control group would indicate what would have happened to the treatment group if the 1988 constitutional change had not occurred. Hence the contrast of the two evolutions is used as an estimate of the impact of the 1988 constitution. As the application of this methodology consist on taking differences twice, first with respect to time (between changes before and after the Constitution) and then between the treatment and control groups, that is why it is known as *differences-in-differences*.

Ideally, the treatment group would be the group most affected by the change in legislation. The control group, on the other hand, ideally must have two properties. First, contrary to the treatment group, it should not be affected at all by the change in legislation. Secondly, the impact of the underlying macroeconomic changes on the treatment and control groups must be very similar. To implement this methodology, we use three alternative ways to breakdown the population in treatment and control groups.

3.1 - Formal-Informal Dichotomy

The existence of the *carteira de trabalho* (a document which has a complete record of the main parameters of the worker's current and all previous formal labor contracts) permits an easy empirical separation of workers with formal labor contracts, that must comply with the labor laws, from workers with informal labor contracts, that are not under this legislation. Workers with formal labor contracts are all employees who have the terms of their current labor contract transcribed to their *carteira de trabalho*. Those, whose employers have not registered their labor contract in their *carteira de trabalho*, should be considered informal.

Moreover, this dichotomy is of the utmost importance, since around 25% of the urban occupied labor force is employed without a formal labor contract. So the formal and informal partition of the worker population correspond to our first alternative of treatment (formal) and control (informal) groups.

3.2 - Quits versus Layoffs

Data regarding the informal sector is not always available. This is particularly the case when administrative files are used. Hence, it is important to identify other sources of cross-section variation in the legislation. The dichotomy between quits and layoffs is one possibility.

In general, regulations involving quits are totally different from those regulating dismissals. In Brazil, quits remain essentially unregulated, while a considerable amount of legislation was designed to restrict dismissals without just cause. Moreover, the changes brought by the new Constitution are entirely related to dismissals. They are silent with respect to quits. Hence quits and layoffs corresponds to our second alternative for the partition between treatment and control groups.

3.3 - Short versus Long Employment Spells

Accordingly to the new and previous Constitutions, the entire regulation on dismissals without just cause only applies to employment spells that have lasted at least three months. Dismissals of workers that have not yet completed three months in the job have been and still are completely unregulated. Hence, an alternative partition in treatment and control groups can be achieved through the contrast between very short spells and other employment spells, where we consider as very short spells all those that last less than three months.

3.4 - Some Limitations of the Methodology

Although changes in the legislation would not have a *direct* effect on these control groups (one of the necessary conditions that the partition between treatment and control has to attend), it is very likely that they would be indirectly affected by the constitutional change. Quitting behavior, for instance, may be significantly influenced by an increase in dismissal compensation. Increases in this type of compensation are likely to reduce quits, as some workers prefer to wait or even to force their dismissal in order to collect the compensation.

The informal sector, on the other hand, is also likely to be indirectly affected through at least two channels. First, because changes in the formal sector tend to affect the informal sector through its effects on overall labor market conditions, for instance, as a result of its effect on the unemployment rate. Secondly, because changes in the legislation may play a role in the bargaining process even in the informal sector through their effect on the notion of fair labor relation.

The probability of separation among very-short spells may also be indirectly influenced by the legislation. On one hand, firms may increase the dismissal of employees before they complete three months in the job in order to avoid the payment of dismissal penalties later. On the other hand, as a result of the increase in dismissal penalties, firms become more selective in their hiring procedures, leading to an overall decline in dismissal rates.

There also exists another reason why the informal sector, quits, and very-short spells may not be an ideal control group. Ideally, it is also necessary that changes in the macroeconomic environment have identical impact on the control and treatment groups. However, there is no theoretical or empirical reason why the response of quits and layoffs, short and long spells and the formal and informal sectors to macroeconomic shocks should be of the same magnitude.

In fact, there are, for instance, good reasons why the sensitivity of the formal and informal sectors differs considerably to macroeconomic changes. For instance, changes in the degree of openness of the economy to foreign trade are bound to have very different effects in the two sectors, since the formal sector specializes in tradeables while the informal sector produces predominantly non-tradeables.

4 - MEASURING JOB TURNOVER AND DURATION OF EMPLOYMENT SPELLS

4.1 - Conceptual Preliminaries

As an indicator of the labor market performance we use, the probability that the employment spell will be terminated next month, conditional on its current duration. This probability as a function of the current duration of the spell is commonly referred as the *hazard function*. Evidently, in this case, the indicator, the hazard function, is inversely related to the duration of employment.

The hazard function is usually preferred to its complement, the probability that it will not be terminated next month, conditional on its current duration. In part, this preference derives from the fact that it can be broken down according to the nature of the separation. In fact, if an employment spell can only be terminated by a quit or dismissal, then the hazard function is equal to the sum of the probability that the employment spell will be terminated by a quit and the corresponding probability that it will be terminated by a dismissal, where both probabilities are conditional on the current duration of the spell. The probability that an employment spell will be terminated by a quit (dismissal), conditional on its current duration, is commonly referred to as the *transition intensity function*. Hence, the property just stated can be summarized by saying that the hazard equals to the sum of the transition intensities.

To define these measures precisely, some symbols must first be introduced. Accordingly, let Ω_t be the universe of all active employment relationships at time t , and for each ω in Ω_t , let $D_t(\omega)$ denote the incomplete duration of the relationship ω up to time t . Moreover, let $S_t(\omega)$ denote an indicator of whether ($S = 1$) or not ($S = 0$) the relationship ω will be terminated in the month beginning at time t . The hazard rate, $h_t(d)$, is then defined as the probability that an active employment relationship, which up to time t has already lasted d months, will be terminated next month, i.e.,

$$h_t(d) = P[S_t = 1 | D_t = d]$$

Moreover, let $Q_t(\omega)$ and $L_t(\omega)$ be indicators of whether the relationship ω will be terminated next month or not by a quit or a dismissal, respectively. The quit and dismissal transition intensities can be expressed, respectively, by

$$h_t^q(d) = P[Q_t = 1 | D_t = d]$$

and

$$h_t^l(d) = P[L_t = 1 | D_t = d]$$

If a separation can only occur due to a quit or a dismissal then

$$h_t(d) = h_t^q(d) + h_t^l(d)$$

These equations refer to the probability of separation for employment spells of a given duration in months. In practice, however, it is more convenient to use the probability of separation for all employment spells with the duration in any given interval.⁸ For instance, it may be more convenient to analyze the probability of separations of employment spells that have already lasted six months but have not yet completed one year, than the probability of separation of employment spells that have lasted up to now exactly seven months. Fortunately, the above equations can easily be adapted to define the probability of separations for all employment spells with the duration in any given interval. In short, we refer to these probabilities as the aggregated hazard and transition intensity rates.

To define them precisely, let $\{a_i : i=1, \dots, z\}$ be a partition of $N = \{0, 1, \dots\}$, so that

$$a_i = \{d_i, \dots, d_{i+1}\}$$

for all $i=1, \dots, z$, $0=d_1 < d_2 < \dots < d_z$, and $a_z = \{d_z, d_{z+1}, \dots\}$. Moreover, let H_{it} denote the probability that an active employment relationship that up to time t has already lasted between d_i and d_{i+1} months will be terminated next month. Then H_{it} is given by

$$H_{it} = P[S_t = 1 | d_i \leq D_t < d_{i+1}] = P[S_t = 1 | D_t \in a_i]$$

By analogy we can define the corresponding transition intensities, respectively, by

$$H_{it}^q = P[Q_t = 1 | d_i \leq D_t < d_{i+1}] = P[Q_t = 1 | D_t \in a_i]$$

and

$$H_{it}^l = P[L_t = 1 | d_i \leq D_t < d_{i+1}] = P[L_t = 1 | D_t \in a_i]$$

In this study we divide employment spells according to their duration in four intervals, i.e., we consider the case $n=5$. **These five intervals are determined by choosing $d_1=0$, $d_2=3$, $d_3=6$, $d_4=12$, $d_5=24$. Hence, spells in the first interval are those that have not yet lasted three months.** To simplify the exposition, we refer to them as the very-short spells. The second interval consists of all spells that have already lasted at least three months but have not yet reached six months. Spells in this interval are referred to as the short spells. The third interval is made of all spells that have already lasted at least six months but have not yet reached one year. Spells in this interval are referred to as the not-so-short spells. The fourth interval is made of all spells that have already lasted at least one year but have not yet reached two years. We are going to refer to them as the long spells. Finally, the

⁸ Conceptually d should refer to an instantaneously measure of time. In this sense the use of month is already a simplification for practical propose.

fifth interval is made of all spells that have already lasted at least two years. We are going to refer to them as the very-long spells.

To obtain estimates for hazard and transition intensities we combine three distinct data sets: *Rais*, *Caged* and *PME*. *PME* is a monthly employment survey, while *Rais* and *Caged* are administrative files.⁹ Three alternative empirical procedures are pursued to obtain consistent estimates of the temporal evolution of these probabilities from these data sets. The first two combine data from *Rais* and *Caged* while the other one rely exclusively on *PME* data. Not only the data used but also the nature of the hypothesis necessary to obtain consistent estimators vary considerably between these four procedures. We describe each one of the three empirical procedure in turn, in the next sub-section.

4.2 - Combining Data on Flows and Stocks from Administrative Files: January Rates

Our first empirical strategy was to combine data from two administrative files: *Rais* and *Caged*. *Rais* is an annual administrative file that provides, at December 31 every year, a complete list of all active employment relationships in the formal sector. The file also includes extensive characteristics of workers and firms. One of the characteristics available for every worker in the file is how long he has been in the current job, i.e., the worker's tenure. Hence, based on this information, it is possible to estimate the distribution of active employment relationships according to their incomplete duration up to December 31 of each year. *Rais* is available for all years from 1985 to 1996.

Cadastro Geral de Empregados e Desempregados (Caged) — is a monthly administrative file that provides data on the formal sector on all separations that occurred in a given month. This file also includes information about the nature of the separation and basic information on workers and firms. In particular, it is possible to identify in each separation the reason for the separation (quits *versus* dismissals) and the complete duration of the employment spell.

In this section, we describe how we combine these two data sets to estimate the measures of employment duration introduced in the previous sub-section. Due to the nature of the available data, t is always December 31 for any given year.

⁹ *Relação Anual de Informações Sociais (Rais)* is an annual administrative file that provides, at December 31 every year, a complete list of all active employment relationships in the formal sector. The file also includes extensive characteristics of workers and firms. One of the characteristics available for every worker in the file is how long he has been in the current job, i.e., the worker's tenure. Hence, based on this information, it is possible to estimate the distribution of active employment relationships according to their incomplete duration up to December 31 of each year. *Rais* is available for all years from 1985 to 1996.

Caged is a monthly administrative file that provides data on the formal sector on all separations that occurred in a given month. This file also includes information about the nature of the separation and basic information on workers and firms. In particular, it is possible to identify in each separation the reason for the separation (quits *versus* dismissals) and the complete duration of the employment spell.

Consequently, the month beginning at t is always January of the following year. Hence, all estimates will refer to the probability that a separation will occur in January, conditional on the duration of employment spells up to December 31. Therefore, even though we label these estimates by the year associated to time t , they do not reflect the average for this year. Actually, the estimates refer to January of the following years.

To estimate the transition probabilities, we use the information from *Rais* on the stock of active employment relationships classified by their incomplete duration up to time t , December 31 of a given year. **The number of active employment relationships that at time t has already lasted d months is represented by $N_t(d)$.**

We use the information from *Caged* on a) how many of these active employment relationships are terminated in January of the following year, $M_t(d)$, b) how many are terminated by quits, $M_t^q(d)$, and c) how many are terminated by dismissals, $M_t^l(d)$. More specifically, we have

$$N_t(d) = \#\{\omega \text{ in } \Omega_t \mid D(\omega) = d\}$$

$$M_t(d) = \#\{\omega \text{ in } \Omega_t \mid D(\omega) = d \text{ and } S(\omega) = 1\}$$

$$M_t^q(d) = \#\{\omega \text{ in } \Omega_t \mid D(\omega) = d \text{ and } Q(\omega) = 1\}$$

and

$$M_t^l(d) = \#\{\omega \text{ in } \Omega_t \mid D(\omega) = d \text{ and } L(\omega) = 1\}$$

Based on this information we obtain the hazard and the transition intensities functions for each year *via*

$$h_t(d) = M_t(d) / N_t(d)$$

$$h_t^q(d) = M_t^q(d) / N_t(d)$$

$$h_t^l(d) = M_t^l(d) / N_t(d)$$

These expressions provide a useful method for estimating the probability of separations of employment spells of a given duration in months. Estimates for the aggregated hazard and transition intensities can also be obtained by

$$H_{ti} = \frac{\sum_{s=d_i}^{d_{i+1}} M_t(s)}{\sum_{s=d_i}^{d_{i+1}} N_t(s)}$$

$$H_{ti}^q = \frac{\sum_{s=d_i}^{d_{i+1}} M_t^q(s)}{\sum_{s=d_i}^{d_{i+1}} N_t(s)}$$

and

$$H_{ti}^l = \frac{\sum_{s=d_i}^{d_{i+1}} M_t^l(s)}{\sum_{s=d_i}^{d_{i+1}} N_t(s)}$$

By combining data from *Caged* and *Rais* it is possible to estimate all these transition probabilities for each year between 1986 and 1995. Tables A.1 attached provide the corresponding estimates for the aggregated hazard and transition intensities.

4.3 - Combining Data on Stocks and Average Flows for the Year

As already mentioned, the estimates described in the previous section refer only to transitions taking place in January. Since these probabilities may follow a seasonal pattern during the year, it is important to verify to what extent our conclusions are sensitive to the choice of a reference month. Unfortunately, we cannot precisely compute these transition probabilities for each month, since we only have data for the stock at December 31. Nevertheless, an approximation of the average over the year can be obtained. In fact, since flows are observed for all months we can combine the average monthly flow for the year with the stock at December 31, in order to obtain an approximation for average monthly transition probabilities for the year.

Formally, the average hazard rate for the year, $\bar{h}_t(d)$, could be defined as

$$\bar{h}_t(d) = (1/12) \sum_{i=0}^{11} h_{t+i}(d)$$

hence,

$$\bar{h}_t(d) = (1/12) \sum_{i=0}^{11} M_{t+i}(d) / N_{t+i}(d)$$

If we take as an approximation that $N_t(d)$ does not change much over the year, than $\bar{h}_t(d)$ could be approximated by

$$\bar{h}_t(d) \approx (1/12) \sum_{i=0}^{11} M_{t+i}(d) / N_t(d)$$

Using similar arguments we can also obtain approximations for the yearly average for the transition intensities. We would like to stress that the assumption made is not related to the pattern of the hiring flow over the year but to the total amount of active employment relationships.

Estimates of the aggregated hazard rate can be obtained by

$$\bar{H}_{ti} = \frac{\frac{1}{12} \sum_{i=0}^{11} \sum_{s=d_i}^{d_{i+1}} M_{t+i}(s)}{\sum_{s=d_i}^{d_{i+1}} N_t(s)}$$

The aggregated transition intensities can be obtained by similar expressions. Table A.2 attached provides estimates for these approximations for the yearly average of the aggregated hazard and transition intensity rates. Despite differences in methodology, the estimates in this table are very similar to those obtained using only the January flows.

4.4 - Estimates Based on Flows from Administrative Files and Steady State Assumption

In steady state it is not necessary to have data on both stocks and flows, since in this case they are closely related to each other by a series of relationships. As a matter of fact, under the steady state assumption, either stock or flow data is sufficient to obtain estimates for transition probabilities.

We begin with the following identity to describe the methodology used in this section:

$$N_{t+1}(d+1) \equiv N_t(d) - M_t(d) \dots\dots\dots$$

By the stationary assumption, we have that $N_{t+1}(d) = N_t(d)$, therefore,

$$N_t(d+1) = N_t(d) - M_t(d)$$

and

$$N_t(d+1) = N_t(0) - \sum_{s=0}^d M_t(s)$$

Moreover, assuming that all employment spells will have a finite duration, we obtain $\lim_{d \rightarrow \infty} N_t(d) = 0$. As a result,

$$N_t(0) = \sum_{s=0}^{\infty} M_t(s)$$

and, in steady state, the hazard function can be obtained through

$$h_t(d) = M_t(d) / N_t(d) = M_t(d) / \left\{ N_t(0) - \sum_{s=0}^{d-1} M_t(s) \right\} = M_t(d) / \sum_{s=d}^{\infty} M_t(s)$$

In short, under the steady state assumption, the hazard rate can be obtained solely from flow data. Similar arguments imply that the transition intensities can be obtained through

$$q_t(d) = M_t^q(d) / \sum_{s=d}^{\infty} M_t(s)$$

and

$$l_t(d) = M_t^l(d) / \sum_{s=d}^{\infty} M_t(d)$$

We take similar steps to obtain the aggregated hazard rate. Since,

$$H_{ti} = \frac{\sum_{s=d_i}^{d_{i+1}} M_t(s)}{\sum_{s=d_i}^{d_{i+1}} N_t(s)}$$

and

$$N_t(d) = \sum_{s=d}^{\infty} M_t(s)$$

it follows that

$$H_{ti} = \frac{\sum_{s=d_i}^{d_{i+1}} M_t(s)}{\sum_{s=d_i}^{d_{i+1}} \sum_{r=s}^{\infty} M_t(r)}$$

To the extent that

$$\frac{\sum_{s=d_i}^{d_{i+1}} M_t(s)}{\sum_{s=d_i}^{\infty} M_t(s)}$$

is very small

$$\sum_{s=d_i}^{d_{i+1}} \sum_{r=1}^{\infty} M_t(r) \approx (d_{i+1} - d_i) \left[\sum_{s=d_{i+1}}^{\infty} M_t(s) + \frac{1}{2} \sum_{s=d_i}^{d_{i+1}} M_t(s) \right]$$

That allows us to simplify the expression for the aggregates hazard rate to

$$H_{ii} \approx \frac{\frac{1}{(d_{i+1} - d_i)} \sum_{s=d_i}^{d_i} M_t(s)}{\sum_{s=d_{i+1}}^{\infty} M_t(s) + \frac{1}{2} \sum_{s=d_i}^{d_{i+1}} M_t(s)}$$

Which is the expression we actually ended up using. By similar arguments we can estimate the aggregated transition intensities via

$$Q_{ii} \approx \frac{\frac{1}{(d_{i+1} - d_i)} \sum_{s=d_i}^{d_{i+1}} M_t^q(s)}{\sum_{s=d_i}^{\infty} M_t(s) + \frac{1}{2} \sum_{s=d_i}^{d_{i+1}} M_t(s)}$$

and

$$L_{ii} \approx \frac{\frac{1}{(d_{i+1} - d_i)} \sum_{s=d_i}^{d_{i+1}} M_t^l(s)}{\sum_{s=d_i}^{\infty} M_t(s) + \frac{1}{2} \sum_{s=d_i}^{d_{i+1}} M_t(s)}$$

Based on these equations and monthly flow data from *Caged*, we obtain estimates for the monthly evolution of aggregated hazard and transition intensities covering the period 1986/95. These estimates are presented in Table A.3 in Appendix.

4.5 - Estimates Based on Employment Surveys, Steady State Assumption, and Stochastic Independence of Employment and Unemployment Spells

In this section we use data from a monthly household survey (*Pesquisa Mensal de Emprego (PME)*) to estimate transition probabilities for the formal and informal sectors. *PME* is a typical employment survey covering the six major Brazilian metropolitan areas. For this study we use monthly data from this survey covering the period 1982/97. The important feature of this survey is the fact that it has information on the *complete duration of previous employment spell for those currently unemployed*. The survey also has information on whether these employment spells ended as quits or layoffs.

To obtain estimates for the hazard and transition intensities out of employment from this data source, we have to assume, in addition to the steady state hypothesis, that the duration of employment and unemployment spells are stochastically independent. This can be considered a strong assumption since one may expect that someone that have experienced a very long unemployment spell

has lower level of endowments such as ability or another unobserved productive characteristics and than would have shorter employment spell. But if this is not the case, we can obtain estimates for the hazard and transition intensities using exactly the same equations we developed in the previous sub-section.

More specifically, let $p_t(d, u)$ represent the probability that a worker, whose previous job lasted d months and who is unemployed at time t for u months, will not leave the unemployment pool next month. Then, at each moment in time t , the number of unemployed workers whose previous jobs last d months, $U_t(d)$, is given by

$$U_t(d) = \sum_{s=-\infty}^t \left\{ M_s(d) \left(\prod_{r=s}^{t-1} p_r(d, r-s) \right) \right\}$$

The steady state assumption implies that the time subscripts are not relevant. Thus, in particular, this equation can be re-written as

$$U(d) = M(d) \sum_{s=-\infty}^{\infty} \left\{ \left(\prod_{r=s}^{\infty} p(d, r-s) \right) \right\}$$

Moreover, the stochastic independence of the duration of the employment and unemployment spells implies that

$$p(d, r-s) = \lambda(r-s)$$

Therefore,

$$U(d) = M(d) \sum_{s=-\infty}^{\infty} \left\{ \left(\prod_{r=s}^{\infty} \lambda(r-s) \right) \right\} = \Lambda M(d)$$

where

$$\Lambda = \sum_{s=-\infty}^{\infty} \left\{ \left(\prod_{r=s}^{\infty} \lambda(r-s) \right) \right\}$$

Hence, we have established the useful result that, at each moment in time, the number of unemployed workers whose previous jobs lasted d months, $U_t(d)$, is proportional to the number of employment spells of duration d ending at time t , $M_t(d)$. Therefore, the hazard rate can be obtained from the data on unemployed workers by

$$h_t(d) = M_t(d) / \sum_{s=d}^{\infty} M_t(s) = U_t(d) / \sum_{s=d}^{\infty} U_t(s)$$

If it is also assumed that the duration of the unemployment spell is independent of whether the previous employment spell ended by a quit or dismissal, then the

transition intensities could also be obtained from data on unemployment workers by

$$q_t(d) = U_t^q(d) / \sum_{s=d}^{\infty} U_t^q(s)$$

and

$$l_t(d) = U_t^l(d) / \sum_{s=d}^{\infty} U_t^l(s)$$

where $U_t^q(d)$ is the number of unemployed workers at time t whose previous job lasted d months and ended in a quit, and $U_t^l(d)$ is the number of unemployed workers at time t whose previous jobs lasted d months and ended in dismissal. This assumption seems to be much weaker than the previous one since it can be argued that the dismissal was not necessary related to the quality of the worker, specially in adjustment periods for the firm like recessions.

Using the same arguments as in the previous section, we can show that the aggregated hazard rate can be approximated by the following equation

$$H_{ii} \approx \frac{\frac{1}{(d_{i+1} - d_i)} \sum_{s=d_i}^{d_{i+1}} U_t^l(s)}{\sum_{s=d_i}^{\infty} U_t(s) + \frac{1}{2} \sum_{s=d_i}^{d_{i+1}} U_t(s)}$$

Similar equations help estimate the aggregated transition intensities.

Using these equations and *PME* monthly data on the complete duration of the previous employment spell for those currently unemployed, we estimate the monthly evolution of the aggregated hazard and transition intensities for the formal and informal sectors and for each of the six metropolitan areas covered by the *PME*. Table A.4 in Appendix presents the yearly estimates of these rates for the six metropolitan areas as a whole. All data in this table was first estimated on a monthly basis for each of the six metropolitan areas. The values in the table are averages across months and metropolitan areas.¹⁰

5 - EMPIRICAL ANALYSIS

5.1 - Difference in Difference Results

To implement the difference in difference methodology we must specify a period before and after 1988. For a pre-1988 period we use the years 1986-1987. The choice of a post-1988 period is more difficult. In principle, one would like to pick

¹⁰ For the *PME* database we have estimates available from 1982 to 1997.

a period as close to 1988 as possible. On one hand, this choice would be useful for isolating the effect of the drastic change in the Constitution from the impact of other concomitant macroeconomic changes that occurred spread out over time. In other words, in terms of separating the impact of the constitutional change from the impact of changes in the macroeconomic environment, the closer the pre- and post-1988 periods the better. On the other hand, since the effects of the 1988 constitutional change may also be spread out over time, to capture a significant portion of them it would be necessary to use for a post-1988 period, a time not very close to 1988. In the latter case, however, there would be no guarantee that the effect of changes in the macroeconomic environment were properly separated. By virtue of this trade-off, we choose the years 1991/92 to represent the post-1988 period.

Estimates of the contrast between the aggregate hazard rates related to these two periods are given in Table 2. The estimates in this table indicate that the hazard rate, mainly for short spells, dropped considerably just after the constitutional change. Since the underlying macroeconomic environment did not remain constant over this period, this finding should be taken with caution. In order to achieve a more precise result we have to contrast the temporal difference of the hazards rates associated to the treatment groups with the analogous difference for the control group.

Table 2

First Differences of Hazard Rates: (1991/92) - (1986/87)

	1/4-1/2 year	1/2-1 year	1-2 years
<i>Administrative files</i>			
Rais and Caged (january flows)	-2.7	-0.3	-0.3
Rais and Caged (average flows)	-2.0	-0.5	-0.2
Caged	-1.6	-0.6	-0.3
<i>Employment survey</i>			
Formal	-1.7	-0.7	-0.1
Informal	-0.8	-0.3	0.0

Source: Based on Relação Anual de Informações Sociais (RAIS), Cadastro Geral de Empregados e Desempregados (Caged) and Pesquisa Mensal de Emprego (PME).

To describe the methodology more explicitly, let Y_0^r and Y_1^r be an indicator of the duration of employment for the treatment group before and after the constitutional change, respectively. Moreover, let Y_0^c and Y_1^c be the corresponding indicator of the duration of employment for the control group before and after the constitutional change, respectively. The impact of the constitutional change on the treatment group will be estimated by the difference in difference estimator, D , given by

$$D = (Y_1^r - Y_0^r) - (Y_1^c - Y_0^c)$$

To implement this methodology, we use the three alternative ways to breakdown the population in treatment and control groups presented in Section 3 (informal x formal, quits x dismissals and very short spells x other spells). The decision about when to use each of these alternative control and treatment groups was finally totally guided by data availability. In fact, we use every alternative that the available data permitted us to use.

When quits are used as controls, the equation for this estimator is simpler. To arrive at this result, we should first notice that the response of quits to macroeconomic changes behaves contrary to dismissals. In fact, as the economy moves into a recession, layoffs will increase while quits fall. Hence, when taking differences in differences we should change the sign of the first differences in quits before taking the second difference. More specifically, in this case the differences in differences estimator, D , is given by

$$D = (Y_1^l - Y_0^l) + (Y_1^q - Y_0^q)$$

or the equivalent

$$D = (Y_1^l + Y_1^q) - (Y_0^l + Y_0^q)$$

Since, in general, $Y_1^l + Y_1^q = Y_1$ and $Y_0^l + Y_0^q = Y_0$, where Y_0 and Y_1 are the corresponding indicator for all separations before and after the constitutional change, respectively. It follows that in this case $D = Y_1 - Y_0$, that is the simple difference estimator investigated in the previous section. In other words, all results presented in the previous section could be interpreted as being obtained from the differences in differences estimator that use quits as a control group.

Table 3 presents differences in differences estimates for the **impact of the constitutional change on employment duration**, using the informal sector and very-short spells as control groups. This table may be analyzed either by the control or by the interval considered. The second alternative shows to be more informative. It very clearly indicates a significant decline in separation among short employment spells, a pattern not observed among other employment spells.

This decline tend to be higher when quit is the control group (around 2 percentage points as reported on Table 2). When the very short spell is the control group the results of Table 3 varies from 0,5 to 1,2 percentage points. Finally the decline associated to the formal x informal partition is 0,9 percentage point. For this last result we can go a little further and explore the insights of the difference-in-difference methodology.

The hazard rate for short spells declined in both the formal and informal sector between before and after the constitutional change, but the decline was larger in the formal sector. Hence, under the assumption that the informal sector is an adequate control group, we have found evidence that the 2 percentage points drop

in the separation rate among short spells, associated to the formal sector, can be divided in two equally important components. Half this decline is common to the informal sector and should, therefore, be attributed to the concomitant macroeconomic changes. The other percentage point, however, can still be considered as a consequence of the constitutional change.

Table 3

Differences in Differences of Hazard Rates: Treatment - Control

Treatment	Control	
	0-1/4 year	Informal
<i>Rais and Caged (January flows)</i>		
1/4-1/2 year	-0,9	-
1/2-1 year	1,5	-
1-2 years	1,5	-
<i>Rais and Caged (average flows)</i>		
1/4-1/2 year	-1,2	-
1/2-1 year	0,3	-
1-2 years	0,6	-
<i>Caged</i>		
1/4-1/2 year	0,1	-
1/2-1 year	1,1	-
1-2 years	1,5	-
<i>Formal</i>		
1/4-1/2 year	-0,5	-0,9
1/2-1 year	0,6	-0,4
1-2 years	1,2	0,0

Source: Based on *Relação Anual de Informações Sociais (RAIS)*, *Cadastro Geral de Empregados e Desempregados (Caged)* and *Pesquisa Mensal de Emprego (PME)*.

However the results are distinct when we analyze other employment spells. There is an increase in the hazard rates (decrease in the duration of employment) for employment spell above six months and below two years, when very short spell is the control group, and insignificant (lower than 0,5 percentage points) when quits are the control group (see Table 2). Therefore it seems that the constitutional change had an effect on hazard rates for those with employment spell between three and six months.

It worthies mention that this conclusion may depend on two aspects of the methodology implemented. The first is the necessary assumptions made to allow us to measure hazard rates based on the available database. There is nothing to be said about this, besides the considerations already done on Sub-section 4.2. The other aspect consists on the adequacy of the control groups chosen. The next Sub-section describes an attempt of testing the adequacy of these controls for the employment survey based analysis.

5.2 - Testing the Validity of the Control Groups

The preceding empirical strategy requires data only for two periods in time: a pre- and a post-1988 period. However, if data is available for a large number of points in time and the macroeconomic changes can be characterized by measurable indicators, then it is possible to obtain estimates of the impact of the constitutional change and macroeconomic factors on hazard rates, through a regression analysis. Moreover, these impacts can be achieved separately for the treatment and the control group. Hence, according to this empirical procedure, it is possible to test the validity of the control group, imposing some restrictions on the estimated coefficients.

The procedure would essentially consist of regressing monthly estimates (based on the employment survey – *PME*) for the aggregated hazard rate on an indicator for the constitutional change (i.e., an indicator that will have 0 as the value before and 1 after the constitutional change), another indicator for the group (the treatment group is associated to the 0 value whether the control to 1), a set of macroeconomic indicators and interactions between the group indicator and each of the macroeconomic indicators and also the constitution indicator.

$$H_{ti} = \beta_0 + X_1'_{ti} \cdot \beta_1 + X_2'_{ti} \cdot \beta_2 + \beta_3 \cdot C_{ti} + \beta_4 \cdot I_{ti} + X_1'_{ti} \cdot \beta_5 \cdot I_{ti} + \beta_6 \cdot I_{ti} \cdot C_{ti} + \varepsilon_{ti} \quad (1)$$

X_1 is the matrix of the values obtained for the four macroeconomic indicators in each month: *a*) *GDP* real growth rate; *b*) degree of openness as measured by the ratio between total trade (import plus exports) and the *GDP*; *c*) inflation rate; and *d*) the volatility of the inflation rate as measured by its temporal standard deviation.

X_2 is a matrix of explanatory variables other than the constitutional indicator (C) and the group indicator (I). These variables are: a linear time trend, monthly seasonal dummies, and regional indicators. Finally, bold characters represent vector notation.

As already mentioned, for a control group to be valid it must satisfy two properties. First, it must not be affected by the constitutional change. Secondly, macroeconomic changes must have the same impact on the treatment and the control groups. Both properties are testable, under the assumption that we can explicitly control for macroeconomic changes.

If the first property is valid (i.e., the constitutional change had no effect on the control group), then the expected value for the hazard rates associated to the control group should be equal for periods pre and post constitutional change. It can be precisely written as:

$$Z = E(H / I = 1, C = 1) - E(H / I = 1, C = 0) = 0$$

Where according to (1):

$$E(H / I = 1, C = 1) = \beta_0 + E(X_1 \cdot \beta_1 / I = 1, C = 1) + E(X_2 \cdot \beta_2 / I = 1, C = 1) + \beta_3 + \beta_4 + E(X_1 \cdot \beta_5 / I = 1, C = 1) + \beta_6$$

and

$$E(H / I = 1, C = 0) = \beta_0 + E(X_1 \cdot \beta_1 / I = 1, C = 1) + E(X_2 \cdot \beta_2 / I = 1, C = 1) + \beta_4 + E(X_1 \cdot \beta_5 / I = 1, C = 1)$$

So, we have that

$$Z = \beta_3 + \beta_6$$

Our test statistic will be based on the analogous estimated coefficients, that is:

$$b_3 + b_6$$

Where we denoted as b the estimations of the true β coefficients. So, we have that if the constitutional change did not affect the control group, the sum of the coefficient on the indicator for the constitutional change and the coefficient on the interaction between the constitution and group indicators must be zero.

If the second property is valid (i.e., macroeconomic changes have the same effect on the control and treatment groups), then the expected value for the effect of the macroeconomic indicators on the hazard rates associated to the treatment and control groups must be equal. It can be precisely written as:

$$E\left(\frac{\partial H}{\partial X_1} / I = 1\right) = E\left(\frac{\partial H}{\partial X_1} / I = 0\right)$$

According to (1) we have:

$$E\left(\frac{\partial H}{\partial X_1} / I = 1\right) = \beta_1 + \beta_5$$

and

$$E\left(\frac{\partial H}{\partial X_1} / I = 0\right) = \beta_1$$

So, we want to see if $\beta_1 + \beta_5 = \beta_1$ that is equivalent to see if $\beta_5 = 0$. Once more we will base our test statistic on the analogous vector of estimated coefficients (b_5) to see if each of the elements of this vector is null ($b_5^1 = b_5^2 = b_5^3 = b_5^4 = 0$).

This procedure was done considering each one of the alternatives for control group and for each interval in turn, except for the very short spell (this interval is supposed to not be ever affected by the constitution, even when it is part of the control group). So we have results from nine regressions shown on Tables 4a-4c. They are based on hazard rates estimated from the employment survey. On the upper part of these tables we have the estimated values of the related coefficients of the regressions which we base this analysis. We can see that the hazard rates tends to be lower after 1988 for the short and not-so-short spells, as mentioned on the previous sub-section.

Table 4a

Pooling Harzard Rates for the Formal and Informal Sectors

	Short Spell		Not-so-Short Spell		Long Spell	
	Coefficient	P-value	Coefficient	P-value	Coefficient	P-value
<i>Variables</i>						
Indicator for the constitutional change (b_3)	-1.3	25.1	-1.1	40.4	2.9	9.0
Degree of openness	1.4	26.2	1.9	20.7	2.0	29.2
GDP growth rate	-7.8	17.4	3.0	67.0	-4.2	63.2
Inflation volatility	0.0	91.4	0.1	8.3	0.0	90.3
Inflation rate	1.7	39.0	3.4	15.8	4.0	17.6
Indicator for informal sector (b_4)	16.4	0.0	13.9	0.0	13.2	0.0
$B \times$ Indicator for the constitutional change (b_5^1)	4.0	1.2	2.5	19.0	-1.7	47.1
$B \times$ Degree of openness (b_5^2)	4.0	1.6	3.6	7.5	2.8	26.9
$B \times$ GDP growth rate (b_5^3)	-10.3	5.1	-6.1	34.0	10.6	18.6
$B \times$ Inflation volatility (b_5^4)	0.0	89.3	0.0	58.6	0.1	19.5
$B \times$ Inflation rate (b_5^5)	-7.1	1.0	-7.3	2.8	-3.1	44.9
<i>Tests for the validity of the informal sector as control group</i>						
H1: Informal sector not affected by the constitution ($b_3 + b_5^1 = 0$)		1.7		31.4		49.3
H2: Effects of macroeconomic indicators are identical in formal and informal sector ($b_5^2 = b_5^3 = b_5^4 = b_5^5 = 0$)		0.0		1.3		9.9

Source: Based on Pesquisa Mensal de Emprego (PME).

Table 4b

Pooling Harzard Rates for the Very Short and Other Spells

	Short Spell		Not-so-Short Spell		Long Spell	
	Coefficient	P-value	Coefficient	P-value	Coefficient	P-value
<i>Variables</i>						
Indicator for the constitutional change (b_3)	-1.6	1.5	-1.6	4.7	2.6	0.4
Degree of openness	2.0	0.5	2.1	1.6	1.7	9.1
GDP growth rate	-7.6	2.1	-3.2	42.4	-1.8	69.7
Inflation volatility	0.0	53.6	0.1	0.1	0.0	96.6
Inflation rate	2.5	2.4	4.5	0.1	4.7	0.2
Indicator for informal sector (b_4)	-7.0	0.0	-17.1	0.0	-27.9	0.0
B x Indicator for the constitutional change (b_5^1)	0.6	50.1	0.6	60.0	-3.7	0.3
B x Degree of openness (b_5^2)	-1.4	15.2	-1.3	25.6	-1.0	43.0
B x GDP growth rate (b_5^3)	-4.8	11.3	-8.3	2.3	-6.8	10.0
B x Inflation volatility (b_5^4)	0.0	82.8	-0.1	3.5	0.0	83.7
B x Inflation rate (b_5^5)	-0.7	63.5	-2.8	14.3	-2.6	22.8
<i>Tests for the validity of the informal sector as control group</i>						
H1: Informal sector not affected by the constitution ($b_3 + b_5^1 = 0$)		13.5		21.2		22.1
H2: Effects of macroeconomic indicators are identical in formal and informal sector ($b_5^2 = b_5^3 = b_5^4 = b_5^5 = 0$)		40.5		2.6		45.1

Source: Based on Pesquisa Mensal de Emprego (PME).

Note: Based on formal sector.

The p-value related to both test statistics mentioned above, for each of the nine regressions, are shown on the bottom part of Tables 4. If one of both null hypotheses is rejected there is evidence against the validity of the group as a control. This is what frequently appears on these tables. Considering a significance level of 10% we have only two cases of acceptance of both hypotheses: the short spell and long spell when we test the very short spell as the control group (Table 4b). This can be taken as an evidence that the very short spell is the best control among the alternative considered.

It worth mention that when we consider the long spell for the test of the informal sector and the short spell for the test of the quit, the test for $Z = 0$ is marginally rejected. We should also stress that the results of the test for a particular group vary considerably for different spells.

Table 4c

Pooling Harzard Rates for the Quit and Dismissals Separations

	Short Spell		Not-so-Short Spell		Long Spell	
	Coefficient	P-value	Coefficient	P-value	Coefficient	P-value
<i>Variables</i>						
Indicator for the constitutional change (b_3)	-1,5	0,0	-0,4	57,1	1,9	2,8
Degree of openness	2,4	0,0	3,2	0,0	3,2	0,1
GDP growth rate	-5,9	0,0	-1,9	59,1	1,9	66,6
Inflation volatility	0,0	0,7	0,0	17,3	0,0	17,1
Inflation rate	1,7	0,1	2,5	3,6	5,3	0,1
Indicator for informal sector (b_4)	-5,1	0,0	-8,1	0,0	-12,2	0,0
B x Indicator for the constitutional change (b_5^1)	1,5	0,0	-1,2	20,6	-2,7	3,2
B x Degree of openness (b_5^2)	-0,8	0,3	-1,8	7,5	-2,9	2,9
B x GDP growth rate (b_5^3)	-0,4	0,9	0,7	82,3	-3,5	39,1
B x Inflation volatility (b_5^4)	0,0	0,7	0,0	30,5	0,1	0,6
B x Inflation rate (b_5^5)	-3,4	0,0	-2,6	11,4	-6,9	0,1
<i>Tests for the validity of the informal sector as control group</i>						
H1: Informal sector not affected by the constitution ($b_3 + b_5^1 = 0$)		87,2		1,9		41,1
H2: Effects of macroeconomic indicators are identical in formal and informal sector ($b_5^2 = b_5^3 = b_5^4 = b_5^5 = 0$)		8,6		19,9		0,0

Source: Based on Pesquisa Mensal de Emprego (PME).

6 - SUMMARY AND CONCLUSION

In this study we investigated the impact of increasing firing costs on the duration of employment spells. The main identification device is the 1988 constitutional change. As a result of this change, the compensation for dismissals without just cause increased fourfold. To measure the duration of employment spells we used separation rates conditional on the current duration of the employment spell (hazard rates). We also broke down separations into dismissals and quits.

Overall, the evidence is consistent with the hypothesis that an increase in firing costs tends to reduce separation rates and so to increase the duration of employment spells. However, the large macroeconomic changes that occurred in Brazil at the end of the 1980s and beginning of the 1990s make it difficult to isolate the impact of the 1988 constitutional change.

We estimated hazard rates based on administrative files and household surveys. The evidence varies according to the interval considered. On the one hand it very clearly indicates a significant decline in separation among short employment spells associated to the constitutional change, independent of the choice for the

control group. On the other hand the variation, associated to the constitutional change, on the hazard rates related to longer employment spells are either insignificant (absolute value lower than 0,5%) or indicates that it increases, depending on the choice of the control group.

Hence we use monthly estimates for the separation rate coupled with time series macroeconomic indicators to test the validity of the alternative control groups used in our analysis. The tests are done separately for each spell. The three alternative for control are rejected in at least one of the three spells considered (short, not-so-short and long). The group that can be considered the best control is the very short spell.

Annex

Table A.1
Transition Probabilities — Based on January Flows

Duration of employment	Years									
	1986	1987	1988	1989	1990	1991	1992	1993	1994	1995
Hazard										
0-1/4	7,4	7,6	7,0	6,7	6,8	5,7	5,7	6,0	7,0	6,3
1/4-1/2	9,7	9,8	8,9	8,5	9,0	7,5	6,6	7,3	10,0	8,0
1/2-1	6,3	5,3	5,5	6,1	6,8	6,1	5,0	5,2	7,1	5,7
1-2	3,9	3,5	3,3	3,7	4,4	3,6	3,1	3,3	4,5	4,0
Dismissal										
0-1/4	3,8	4,6	4,2	3,9	4,5	3,9	3,9	4,2	4,1	4,1
1/4-1/2	5,4	6,4	5,8	5,4	6,4	5,6	4,7	5,4	6,6	5,7
1/2-1	3,8	3,7	3,8	4,2	5,3	4,8	3,9	4,1	5,4	4,5
1-2	2,4	2,5	2,3	2,6	3,4	2,9	2,5	2,6	3,4	3,2
Quit										
0-1/4	3,4	2,8	2,6	2,6	2,2	1,5	1,7	1,6	2,7	2,0
1/4-1/2	4,0	3,1	2,7	2,7	2,3	1,6	1,6	1,6	3,1	2,0
1/2-1	2,3	1,4	1,4	1,6	1,3	0,9	0,8	0,8	1,4	0,9
1-2	1,2	0,7	0,7	0,8	0,6	0,4	0,4	0,4	0,8	0,5

Source: Based on Relação Anual de Informações Sociais (RAIS) and Cadastro Geral de Empregados e Desempregados (Caged).

Table A.2
Transition Probabilities — Based on Average Flows

Duration of employment	Years									
	1986	1987	1988	1989	1990	1991	1992	1993	1994	1995
Hazard										
0-1/4	6,8	7,7	8,0	7,2	7,0	5,8	7,2	6,8	6,4	6,5
1/4-1/2	7,7	9,0	8,1	7,0	7,2	5,6	7,2	7,2	8,0	6,6
1/2-1	5,7	4,8	4,6	5,3	5,3	4,7	4,7	4,7	6,1	4,6
1-2	3,6	3,3	3,3	3,7	3,2	3,3	3,1	3,6	3,9	3,9
Dismissal										
0-1/4	3,5	4,4	5,1	4,1	5,0	5,1	4,6	4,3	3,3	4,8
1/4-1/2	4,3	6,0	5,8	4,6	5,7	5,3	5,4	5,0	4,6	5,8
1/2-1	3,2	3,7	3,4	3,4	4,9	3,9	4,3	3,5	3,8	3,9
1-2	2,3	2,2	2,5	2,4	2,8	2,9	2,6	2,8	2,6	2,8
Quit										
0-1/4	5,8	6,5	6,4	6,2	6,3	5,4	3,6	3,7	3,6	5,5
1/4-1/2	4,8	5,8	4,8	4,6	4,9	3,6	2,9	3,1	3,5	4,4
1/2-1	2,7	2,5	2,0	2,4	3,0	1,8	1,5	1,3	1,7	1,6
1-2	1,8	1,3	1,2	1,6	1,5	1,1	0,7	1,0	0,9	1,0

Source: Based on Relação Anual de Informações Sociais (RAIS) and Cadastro Geral de Empregados e Desempregados (Caged) .

Table A.3
Transition Probabilities — Based on Flows

Duration of employment	Years										
	1986	1987	1988	1989	1990	1991	1992	1993	1994	1995	1996
Hazard											
0-1/4	7,6	7,5	7,4	7,0	6,4	6,2	5,4	5,8	6,0	6,1	5,9
1/4-1/2	9,0	9,4	9,3	9,0	8,1	8,0	7,1	7,2	7,3	7,7	7,2
1/2-1	5,6	6,3	5,9	5,9	5,7	5,2	5,4	5,1	5,3	5,5	5,4
1-2	3,4	3,6	3,7	3,7	3,5	3,2	3,3	3,2	3,1	3,0	3,4
Dismissal											
0-1/4	4,1	4,3	4,5	3,9	3,9	4,0	3,8	3,9	3,8	3,8	3,9
1/4-1/2	5,6	6,2	6,4	5,8	5,5	5,8	5,4	5,3	5,1	5,4	5,3
1/2-1	3,8	4,6	4,4	4,2	4,2	4,1	4,5	4,1	4,1	4,3	4,4
1-2	2,3	2,6	2,8	2,6	2,6	2,5	2,7	2,5	2,5	2,3	2,8
Quit											
0-1/4	3,3	3,1	2,8	3,0	2,4	2,1	1,5	1,7	2,0	2,1	1,9
1/4-1/2	3,1	3,0	2,6	2,9	2,4	2,0	1,5	1,6	1,9	2,0	1,7
1/2-1	1,6	1,5	1,3	1,5	1,3	0,9	0,8	0,8	0,9	0,9	0,8
1-2	0,9	0,8	0,7	0,8	0,7	0,5	0,4	0,4	0,4	0,4	0,4

Table A.4
Transition Probabilities — Based on “Pesquisa Mensal de Emprego” (PME)

Duration of employment	Years								
	1982	1983	1984	1985	1986	1987	1988	1989	1990
Hazard									
0-1/4	4,8	5,2	5,8	5,5	4,5	4,2	4,1	3,9	4,0
1/4-1/2	4,9	5,2	5,2	5,2	5,0	5,1	4,9	4,8	4,8
1/2-1	3,7	4,0	3,8	3,6	3,3	3,9	3,7	3,6	3,9
1-2	2,4	2,6	2,6	2,5	2,2	2,5	2,6	2,6	2,6
Dismissal									
0-1/4	3,6	2,5	2,5	2,6	2,8	2,6	2,3	2,3	1,9
1/4-1/2	3,3	2,7	2,5	2,7	3,1	2,7	2,7	2,8	2,3
1/2-1	2,2	1,7	1,6	1,6	1,9	1,8	1,7	1,7	1,5
1-2	1,3	1,0	0,9	1,0	1,1	1,1	1,1	1,1	1,0
Quit									
0-1/4	8,5	7,7	8,3	8,0	7,3	6,7	6,4	6,3	5,9
1/4-1/2	8,3	7,9	7,7	7,9	8,0	7,8	7,6	7,5	7,1
1/2-1	5,9	5,7	5,4	5,3	5,2	5,7	5,4	5,4	5,4
1-2	3,7	3,6	3,5	3,5	3,4	3,6	3,8	3,7	3,6

(continua)

(continuação)

Duration of employment (years)	Year						
	1991	1992	1993	1994	1995	1996	1997
Hazard							
0-1/4	4,0	3,8	4,1	4,4	4,5	4,3	4,1
1/4-1/2	4,4	4,7	4,6	4,7	5,0	5,1	4,8
1/2-1	3,4	3,8	3,5	3,6	3,9	3,7	3,8
1-2	2,5	2,6	2,7	2,5	2,5	2,6	2,7
Dismissal							
0-1/4	1,8	1,2	1,2	1,3	1,4	1,3	1,1
1/4-1/2	2,0	1,5	1,4	1,6	1,7	1,6	1,5
1/2-1	1,3	1,1	1,0	1,1	1,1	1,1	1,0
1-2	0,9	0,7	0,7	0,7	0,7	0,7	0,7
Quit							
0-1/4	5,7	5,0	5,3	5,7	5,9	5,6	5,3
1/4-1/2	6,3	6,2	6,1	6,3	6,7	6,7	6,2
1/2-1	4,7	4,9	4,6	4,8	5,0	4,8	4,8
1-2	3,4	3,4	3,4	3,2	3,2	3,2	3,4

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