Minimum wage hikes and Employment Transitions in Brazil[1]

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ABSTRACT

This paper investigates the effect of the minimum wage on employment transitions in Brazil and, in particular, on the informal sector transitions. We estimate the probability of becoming nonemployed (unemployed or out of the labour force) and the probability of moving to the informal sector after minimum wage hikes. We estimate these effects separately for periods with high and low inflation to assess how agents react to minimum wage hikes under different inflationary expectations, particularly, under different degrees of wage indexation. Workers affected by minimum wage increases are compared with similar workers further up in the wage distribution. In order to account for heterogeneity between the treated minimum wage workers and the comparison groups we use a difference-in-differences approach that compares treated and comparison groups in periods with nominal increase in the minimum wage with periods with no increase. In this last case the comparison and treated groups are defined as if there had been an increase in the minimum wage (pseudo-experiment). Such strategy is applied in a parametric way via probit estimates and also in a nonparametric way using kernel propensity score matching method. Our findings suggested that disemploymet effects were more likely to be observed in the late 1990´s than in the early 1980´s. This negative effect affects in 1990 affects both informal and formal workers, but it is not a characteristic of all minimum wage hike episodes. We also find no robust evidence that minimum wage hikes lead to transitions from the formal to the informal sector.

Keywords: Minimum wage, employment transitions, informality, propensity score matching.

RESUMO

Este artigo investiga o efeito do salário mínimo sobre transições de emprego no Brasil, em particular, sobre o setor informal. Estimamos a probabilidade de um trabalhador se tornar não-empregado (desempregado ou sair da força de trabalho) e a probabilidade de transitar do setor informal para o formal depois de aumentos do salário mínimo. Este efeitos são estimados separadamente para período de alta e baixa inflação para testar como os agentes reagiram a aumentos do salário mínimo sob diferentes expectativas inflacionárias e sob diferentes regras de indexação salarial. Trabalhadores diretamente afetados por aumentos do salário mínimo são comparados com trabalhadores similares marginalmente acima deles na distribuição de salários. A fim de controlar pelo efeito da heterogeneidade entre trabalhadores afetados pelo aumento e trabalhadores no grupo de comparação, utilizamos o procedimento de diferenças em diferenças que compara indivíduos submetidos ao tratamento com o grupo de trabalhadores logo acima deles em períodos com aumentos nominais e em períodos sem aumento. Neste último caso, grupos de tratamento e comparação são definidos com se tivesse ocorrido um aumento do salário mínimo (pseudo-experimento). Esta estratégia é aplicada tanto de forma paramétrica via probit como também de forma não paramétrica através de *kernel propensity score matching*. Nossos resultados sugerem que efeitos negativos do salário mínimo sobre o emprego foram mais prováveis no final dos anos 90 do que no início dos anos 80, mas não ocorreu em todos os episódios de aumento do salário minimo (basicamente ocorre em 1996 e 1998). Este efeito negativo afeta tanto os trabalhadores informais do que os formais. Nós também não encontramos nenhuma evidência robusta de que aumentos do salário mínimo levem a transições do setor formal para o informal.

Palavras-chave: Salário mínimo, trasições ocupacionais, informalidade, propensity score matching.
JEL CODE: J6, J38, C21
ÁREA 12 - ECONOMIA DO TRABALHO

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1. Introduction

The minimum wage has been singled out in the literature as one of the causes of segmentation[3] between formal and informal sector workers in developing countries (Behrman,1999). The idea is that the informal sector is not covered by the minimum wage policy or that compliance is not enforced is that sector. Thus minimum wage hikes would lead to an increase in the informal sector because small firms that could not afford the wage increase would start hiring workers with a wage lower than the minimum wage[4] in the informal sector.

As seen in Soares(2004), the degree of non-compliance with the minimum wage decreased a lot from 1981 to 1999 in Brazil. In the informal sector the non-compliance fell from 61% to 35% in the informal sector and from 7% to 1% in the formal sector. Possibly this happened due to the fall in the real value of the minimum wage over this period. The high level of non-compliance in the informal sector has led some to believe, that minimum wage hikes were irrelevant for the determination of the wage of those workers. However, some models on the effect of the minimum wage in the covered and non-covered sectors challenged this common sense. Welch (1974) develops a model with two sectors: one covered by the minimum wage and the other not covered. In his model, minimum wage hikes lead to both an increase in the non-covered sector and a reduction in the participation rate, since workers for whom the new (lower) equilibrium wage in the non-covered sector is below their reservation wage would withdraw from the labour market. Mincer (1976) extends this model to allow the existence of unemployment. In his model the effect of minimum wage hikes on the non-covered sector will depend whether the turnover rate is higher or lower than the elasticity of demand in the protected sector. If it is higher, than the non-covered wage will increase, as workers would queue for covered jobs. If its lower, the wage in the non-covered sector would fall, as workers migrate to this sector.

Thus, workers affected by minimum wage hikes can move to different states after the increase. Minimum wage hikes can affect transitions to the informal sector, to unemployment, to inactivity and even transitions from inactivity to unemployment. What happens with a worker's mobility pattern after a minimum wage hike is mainly an empirical question.

Besides having a sizable informal sector, the Brazilian labour market has special features that make the study of the impact of the minimum wage on the labour market even more complex. First, the formal sector during the 1980's and the first half of the 1990's followed strict rules of wage indexation determined centrally by the federal government. In addition, during this period there was a high concentration of formal sector workers around multiples of the minimum wage, so that the spike on the value of minimum wage in the distribution of wages was not unique. Thus, minimum wage hikes were quite likely to have spillover effect in the wage distribution of formal sector workers. For the informal sector, such spikes were not observed in the 1980's. However, since 1995 there has been an increasing indexation of their wages to the minimum wage. Furthermore, it has been observed that a higher proportion of informal sector workers have received wages increases equal to the increase of the minimum wage than formal sector workers. Therefore, the minimum wage policy in Brazil is likely to have a direct effect on informal sector workers, at least, in the second half of the 1990's, in addition to the indirect effect pointed out in models like the ones developed by Welch (1974) and Mincer (1976).

In this paper we will investigate the effect of the minimum wage hikes on employment transitions in Brazil. We will estimate the probability of becoming nonemployed (unemployed or out of the labour force) and the probability of moving from the formal to the informal sector after minimum wage hikes[5]. Workers affected by minimum wage increases are compared with similar workers further up the wage distribution. In order to control for heterogeneity between the treated minimum wage workers and the comparison groups we use a difference-in-differences approach that compares treated and comparison

[3]See for instance Rauch's (1991).

[4]We are assuming that the cost of non-compliance is higher for large firms, since they are more easily caught by inspection than small firms.

[5]We estimate these effects for the joint sample of formal and informal sector workers and separately for each sector.

groups in periods with nominal increases in the minimum wage with periods with no increase. In this last case the comparison and treated groups are defined as if there had been an increase in the minimum wage (pseudo-experiment). This strategy is applied in a parametric way via probit and also in a nonparametric way using kernel propensity score matching methods.
The findings suggested that disemploymet effects were more likely to be observed in the late 1990´s than in the early 1980´s. This negative effect affects in 1990 affects both informal and formal workers, but it is not a characteristic of all minimum wage hike episodes. We also find no robust evidence that minimum wage hikes lead to transitions from the formal to the informal sector. Methodologically, probit and propensity score matching difference-in-difference approaches do not differ much in their estimates.

2. A Tale of Two Decades and Two Sectors

The role of the minimum wage in Brazil as a reference for wages in the formal sector is a major characteristic of the distribution of wages in Brazil due to the wage policy of the 1980's. However, its role as an index was not restricted to the formal sector. In this regard, there are two very different phases. One between 1982 and 1985 when the minimum wage was an index for the formal sector, and had little - if any - impact on the wage distribution of the informal sector, and the period 1995 - 1999, when the minimum wage seems to be much more important for the determination of the wages in the informal sector than in the formal sector. One possible explanation for this phenomenon was the fall in the real value of the minimum wage observed during the 1980's and the first half of the 1990's.
To illustrate these changes we will analyse the kernel densities of the log wages in May 1985 and in May 1995 and also the wage changes by percentiles in periods of minimum wage increases. These two minimum wage hike episodes are representative of the effects of minimum wage on the wage distribution for the early 1980's and for the late 1990's, respectively[6]. Figure 1 and Figure 2 show the distribution of log wages in the month before (April) and in the month after (May) a minimum wage increase for formal and informal sector workers in 1985 and 1995. The data come from the PME (Monthly Employment Survey) for the six main metropolitan areas.[7] The three bars in the graphs correspond (from left to right) to the minimum wage, to two times the minimum wage and to three times the minimum wage. Figure 1 reveals that the minimum wage produces a spike at its value in the distribution of wages for formal sector workers, and a second less pronounced mass at the value of two minimum wages. Moreover, there was a clear shift to the right from one month to the other after the minimum wage increase. The graphs in the second row of this figure show that this sort of truncation did not exist in the informal sector due to the high degree of non-compliance. More than a half of the informal sector workers earned less than the minimum wage in 1985. Nonetheless there is a spike at the value of the minimum wage, but not at its multiples.
Figure 2 differs from Figure 1 in several ways. First, non-compliance dropped considerably among both formal and informal sector workers between 1985 and 1995. Second, the most important spike in the wage distribution of formal sector workers occurs at two times the value of the minimum wage in 1995. There is still a spike at the minimum wage but it is less pronounced, particularly, in April 1995. Surprisingly, the graphs for the informal sector in 1995 resembles the ones for the formal sector in 1985. There is a spike at the minimum wage and a second spike at the value of two times the minimum wage. Moreover, there was a clear shift to the right of the distribution after the minimum wage increase. Therefore, the minimum wage seems to have affected the distribution of wages in both formal and informal sectors. However, during the late 1990's, it seems that the minimum wage became much more important for the distribution of wages in the informal than in the formal sector.

[6]Whenever there is a significant difference between the results we get for these two specific years and the other years that they are meant to represent, we will comment on the differences. However, most of the stylised facts found for these two years also apply for similar years in the 1980's and in the 1990's.
[7]See section 4 for more details on this data set.

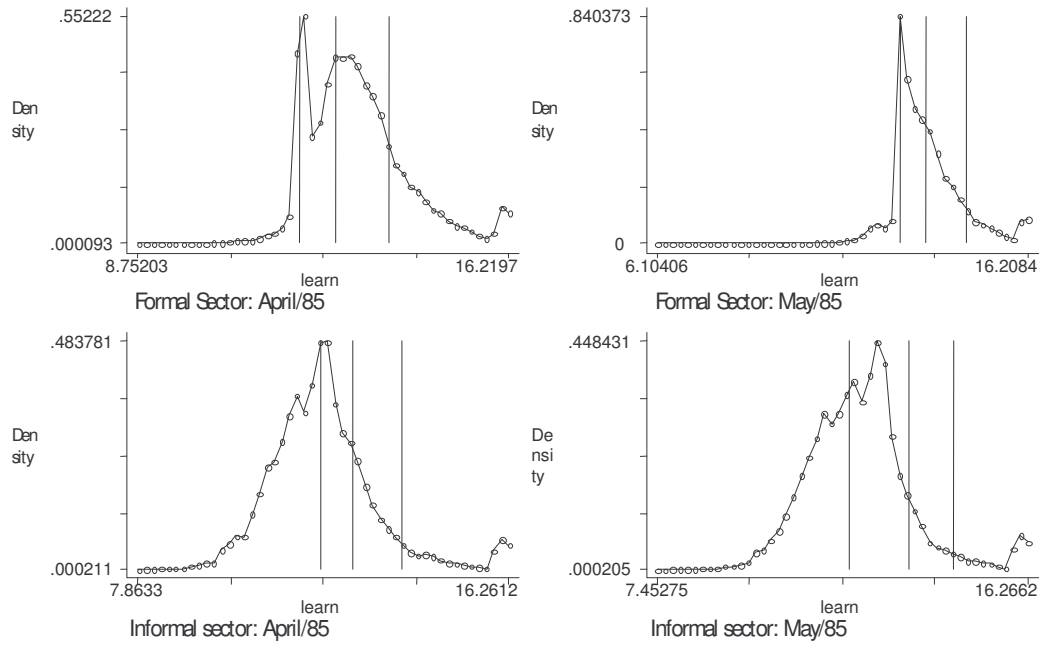

Figure 1. Kernel Density of Log Wages: 1985

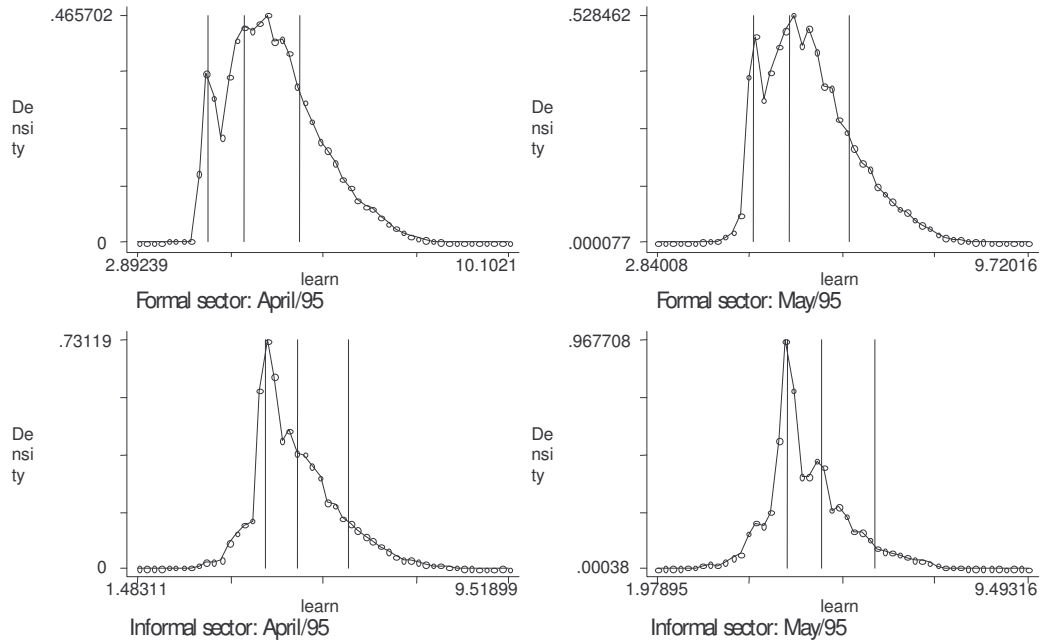

Figure 2. Kernel Density of Log Wages: 1995

In order to analyze the impact of minimum wage hikes on the entire wage distribution, Figure 3 plots the monthly increases in the average wage for each percentile (*diff1*) and compares them with the nominal minimum wage increase in May 1985 and May 1995 (the upper horizontal line in the graphs) for formal and informal workers, separately. In the case of absence of spillover effect, we expect to see spikes equivalent to the minimum wage hike for percentiles whose average wage are close to the minimum wage and no effect for highest percentiles. Due to the possibility that we would be capturing spurious increases in wages we also apply a differences-in-differences approach that controls for the variation in months

surrounding the minimum wage increase: *diff2* and *diff3*. For 1985, *diff2* is the difference between the increase in the mean of the percentile in the month after the minimum wage hike (June) and the month before the minimum wage hike (April), and *diff3* is the difference between the increase in the mean of the percentile in the month after the minimum wage increase (May) and the average mean increase one month before (April) and one month after the minimum wage hike (June). For the period 1995, *diff2* is the difference between the increase in the mean of the percentile in the month after the minimum wage increase (May) and the average mean increase one month before(April) and one month after (June) the minimum wage hike[8], and *diff3* is the difference in the increase in the mean of the percentile in the month after the minimum wage increase and five months later (October)[9].

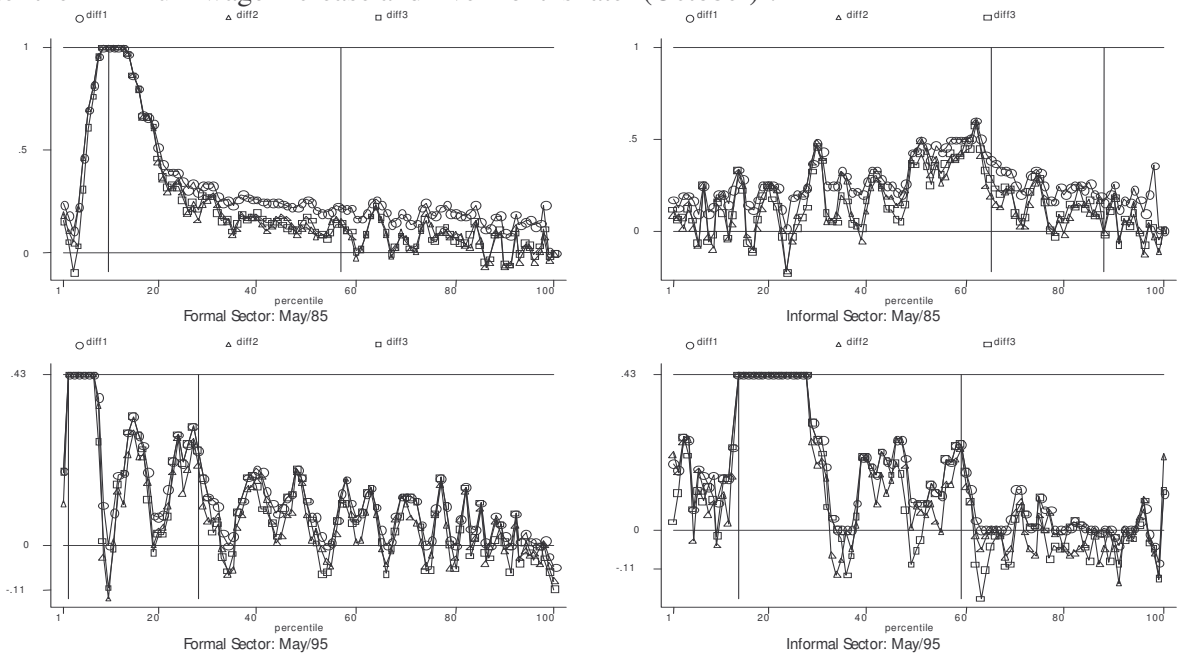

Figure 3. Wage Change by Percentiles due to Minimum Wage Increase: 05/1985 and 05/1995

From the graphs in Figure 3 we can observe many contrasts between the percentile wage variation for formal and informal workers. First, the minimum wage hike in May 1985 had no impact on wage increases in the informal sector. The percentiles close to the minimum wage (first vertical line from left to right) had a 50% increase - the larger increase observed for all percentiles -, whereas the minimum wage increased by 100% (in nominal terms). Second, formal sector workers in the lower percentiles of the distribution had wage increases equal to the increase of the minimum wage (this can be seen by the flat upper line in the graphs). Notice that in general this flat portion of the graph coincides with the vertical line on the left that indicates the first percentile whose average wage is equal to the minimum wage. This coincidence in nominal increases occurred in both periods. Third, in May 1995 many percentiles at the lower end of the wage distribution for informal sector workers had wage increases equal to the minimum wage. This represents a completely change in relation to May 1985. Actually, more percentiles in the wage distribution for informal sector workers had wage increases equal to the increase in the minimum wage than in the formal sector[10].

At a first glance, the graphs suggest that spillover effects were more likely to happen in 1995 than in 1985[11]. However, this only means that in the month of a minimum wage increase during the 1980's only the percentiles directly affect by the minimum wage seems to show an expressive positive variation.

[8]Note the definition of *diff2* for 1995 corresponds to the definition of *diff3* for 1985.

[9]As there was an increase in the minimum wage in November 1985, we cannot apply the control group *diff3* as defined for the year 1995, i.e, using months well ahead the May minimum wage hike, as a control group for the year 1985.

[10]This phenomenon was first reported in Neri(1997).

[11]Actually the figures for minimum wage increases from 1995 onwards show even stronger spillover effects.

Nothing prevents that the other percentiles catch up with the minimum wage increase over time. Nevertheless, this indicates that during the 1980's every minimum wage increase in the very short run implied that the relative wage of minimum wage workers did increase.
As for indexation of multiples of the minimum wage for both minimum wage hike episodes reported in the graphs, May 1985 and May 1995, there is no clear spike at two times the value of the minimum wage. However, graphs not shown here reveal that from 1996 onwards there is a spike equal to the increase in the minimum wage near the percentile correspondent to two times the minimum wage. Thus, it seems that multiples of the minimum wage in the late 1990's were much more indexed to it than in the 1980's.

3. Methodology: Difference-in-Differences with Pseudo-Experiment

The only difference between the traditional difference-in-difference approach and one with a pseudo-experiment is that in the standard case, the treated group is defined regardless of the existence of the treatment or not. For instance, union workers can be defined as the treated group in the case of an evaluation of a major union-rights reform: both unionized and non-unionized workers (comparison group) are clearly observed and defined before and after the treatment without any trick. This does not occur in the case of an evaluation of the effects of a minimum wage increase. In this case, one has to pretend that there had been a minimum wage increase in the period used as baseline (before or some time after the minimum wage increase) in order to define the treated group - workers between two different minimum wages - and the correspondent comparison group - workers marginally further up in the wage distribution. The (pseudo)increase used to define the treated and control group is arbitrary, which means that one can use different "(pseudo) minimum wage increases" in order to generate the counterfactual for the "actual minimum wage increase". [12]
A common characteristic of the strategies based on the idea of differences-in-differences with pseudo-experiment is that they rely on parametric models - mostly linear probability models, logit or probit analysis - in order to measure the impact of minimum wage hikes on both treated and comparison groups. The caveats of these parametric models in dealing with comparisons between treated and comparison groups are well-known. First there is the problem of the lack of common support for treated and comparison groups. This feature implies that the estimation of the effect of the minimum wage relies on linear (or other assumed functional form) extrapolations to evaluate the impact over regions where there is no available counterfactual in the comparison group for the treated group[13]. Second, in a difference-in-difference context, changes in group composition over time may lead to an imbalance between the observed characteristics of both comparison and treated groups after the treatment rendering the comparison meaningless[14]. Due to these shortcomings, this paper also applies a propensity score matching method to evaluate the impact of the minimum wage on employment transitions. In doing so, we will be building comparison groups that are more reliable than the ones available for the parametric analysis[15].
In a difference-in-differences framework both unobserved time-invariant heterogeneity and common time effects between treated and comparison groups could be eliminated by taking the differences between them in two different time periods, one period under the effect of the treatment and another period when the treatment did not occur, but when appropriate treated and control groups can be defined (pseudo-experiment), and then taking the difference of these differences.
The common trend assumption underlying the differences-in-differences approach implies that changes in

[12] For the period 1995-1999 we will apply to different pseudo-experiment to assess the impact of the minimum wage. For the period 1982-1985 this is not possible as the minimum wage was increased every 6 months. The availability of a pseudo-experiment far from the episode of a minimum wage hike is particular interesting for it avoid the problems of data contamination and/or anticipation effects when we only use a pair of months before the minimum wage increase.
[13]See Smith and Todd (2003) for an in-depth discussion of this issue.
[14]See Blundell et al. (2001) for the importance to control for compositional effects in a difference-in-differences context. However, this difficulty can be overcome in a parametric framework by using changes in the characteristics rather than the characteristics themselves as explanatory variables in case one has access to a panel.
[15]Brown (1999) argues that most of the early studies that applied difference-in-differences to the evaluate minimum wage effect had built naive comparison groups. In fact, when using workers further up the wage distribution the reliability of the comparison group is even more difficult to prove due to possible existence of both substitution and spill-over effects.

the outcome in the absence of a minimum wage hike for treated workers should be the same as the change observed for workers in the control group:

$$E(Y_{it} - Y_{it'}) = [E(Y_{it}^0 \mid I=1) - E(Y_{it'}^0 \mid I=1)] = [E(Y_{it}^0 \mid I=0) - E(Y_{it'}^0 \mid I=0)] \quad (1)$$

Where I is an indicator for treatment ($I=1$, treated and $I=0$ comparison) and Y is an the outcome of interest and $t'<t$.

The propensity score matching is necessary in order to make the chosen comparison group as similar as possible to the treated group. Thus the first step when applying matching is to estimate the propensity score associated to the treated group, which can be done parametrically via probit or logit estimation. The second step is to calculate the common support where the average treatment effect can be defined, based on the overlap region of distribution of the propensity scores for both treated and control groups. Finally, the third step is to build the estimator of the impact of the policy change

The differences-in-differences estimates in the non-parametric case will be based on kernel methods. The kernel propensity score matching builds the counterfactual based on a kernel weighted average over a set of individuals in the control group:

$$\alpha_{mdid} = \frac{1}{n_{1t}} \sum_{i \in I=1}^{n1t} [Y_{it} - \sum_{j \in I=0} W_{ij} Y_{jt}] - \frac{1}{n_{1t'}} \sum_{i \in I=1}^{n1t'} [Y_{it'} - \sum_{j \in I=0} W_{ij} Y_{jt'}] \quad (2)$$

Where the weight, Wij, corresponds to the following kernel function ratio: $\frac{K(\frac{P_j - P_i}{h_n})}{\sum_{k \in I=0} K(\frac{P_k - P_i}{h_n})}$, where $K(.)$ is a kernel function and h_n is a bandwidth parameter, this function gives weight to the comparison observations according to its distance in terms of propensity score in relation to the treated observations. Heckman et al.(1997) show that under standard conditions on the bandwidth and the kernel[16], the counterfactual generated by this method is a consistent estimator of the non-observed counterfactual: $E(Y^0 \mid P(X), I=1)$. The choice of the kernel and of its bandwidth will determine the number of control observations used to match each treated individual and the weight[17] with which each control observation will enter the calculation of the counterfactual. The Gaussian kernel, for instance, is unbounded which means that all control observations enter the calculation. In this paper we use a Epanechnikov kernel with constant bandwidth chosen according to Silverman's rule of thumb.

The problem here is how to determine the common support for the two periods of time under consideration, t and t'. One strategy would be to calculate the propensity score for the aggregate $P(I=1 \mid X)$, including observations of t and t' and ignoring that we would be pooling together the composition of the treated group in two different points in time. This procedure ignores the fact that the composition of the treated group after the treatment may have changed. Another alternative is to try to balance the distribution of X among the four cells, which means that we must take into account that X is also distributed between t and t'. Blundell et al. (2001) suggest that besides the usual propensity score $P(I=1 \mid X)$, the probability of being observed in period t (after the treatment), $P(t=1 \mid X)$, should also be used in the matching process[18].

We apply two matching procedures to our data. Following Blundell et al. (2001), first we use Euclidean Distance to combine the two propensity scores in the search for the closest matching for the NN matching case. Second we estimate a kernel estimator. We multiply the two kernel weights that we get from the two

[16]These conditions require that $K(.)$ integrates to one, has mean zero and that $h_n \to 0$ as $n \to \infty$ and $nh_n \to \infty$.

[17]The weight of each control observation decreases with its distance from the treated observation as measured by their propensity scores.

[18]Smith and Todd(2000) put forward a procedure to estimate the common support region. Basically they argue that the intersection of the two densities with some trimming for very low probabilities should be selected. However, the widely used user-written programs in Stata pscore.ado and attk.ado define the common support in a simpler way. Nonetheless they differ between them in the way that the distribution is trimmed. While psmatch.ado excludes those observations in the treated group whose probability (or linear index) is not within the common support, attk.ado excludes those observations in the control group whose probability (or linear index) is not within the common support. In this paper we exclude both treated and control observations that are not within the common support. We believe that this procedure is much more in line with Smith and Todd (2000) idea than the former ones.

propensity scores for each observation on the treated group, and divide its product by the product of their constant bandwidth given by $1.06\sigma_x N^{(-0.2)}$.

As for the choice of comparison group, even using the propensity score matching we face some challenges due to the specificity of a minimum wage treatment. In the perfect competition framework, the traditional theory of labour demand assumes that an increase in a binding minimum wage renders workers whose productivity is below the newly set value unemployable. Thus minimum wage increases would tend to increase non-employment either through increases in inactivity or in unemployment. A potential problem for the construction of good comparison groups for minimum wage workers is the fact that workers marginally further up the wage distribution can be close substitutes for minimum wage workers so that their employment probability can be (positively) affected by an increase in the minimum wage, depending on the elasticity of substitution between the two groups and on the magnitude of the increase.

If this is the case, the outcome trend before and after the treatment cannot be the same for both groups and (negative) minimum wage effects (on the treated) would be overestimated. The assumption here is that the difference in the outcome between the treated and the comparison groups should be constant in the absence of the treatment, so that the comparison group would offer a good approximation of what would have been the outcome for the treated group in the absence of the experiment - an increase in the minimum wage.

Kramarz and Phillipon (2000) point out that the demand for workers in the comparison group must not vary with changes in the minimum wage. One alternative control group would be workers who earn well above minimum wage workers, but in this case workers in the control group would not be comparable to minimum wage earners since their observable and unobservable characteristics would be quite different, making even worse the problem of lack of common support[19]. Testing whether or not comparison groups are affected by minimum wage hikes is important in the sense that depending on the elasticity of substitution between treated and comparison groups used in the analysis, we can be over- or underestimating the effect of the minimum wage. Kramarz and Phillipon (2000) put forward a test of the quality of the comparison group through the difference in elasticities of employment probability during minimum wage hikes and during pseudo-minimum wage hikes - the difference between the elasticities calculated in these two periods should be small and statistically insignificant for the control group to be considered a good one. This is particularly important in the case of Brazil, since as pointed out in the last section, there has been a history of wage indexation in relation to both inflation rate and minimum wage increases[20]. In this paper we also evaluate the appropriateness of the comparison group.

4. Data

The microdata used in this paper comes from the Brazilian Monthly Employment Survey (PME) published by IBGE (*Instituto Brasileiro de Geografia e Estatistica*). The PME collects monthly data in a similar way of the American CPS. Each household stays in the sample for 4 months, then "rests" for 8 months, and finally returns for extra 4 months. A worrying limitation of the PME is that the individual is asked about his/her monthly wage in the last month, and not in the month of the interview. Moreover, if the individual is unemployed or inactive in the month of the interview, he/she is not asked about the wage in the previous month. Thus, even if he/she had a job in $t-1$, we do not have information about his/her wage. For this reason, we do not know the wage of an individual who became unemployed in a month of a minimum wage increase, so we assume that his/her wage in the month previous to the minimum wage increase was equal to his/her wage in the month previous to that. This means that for an unemployed individual in t (month with minimum wage increase), the wage in $t-1$ is assumed, in our analysis, to be

[19]This problem is quite common in approaches that contrast the probability of employment in t for individuals that in $t-1$ earned between the old and the new minimum wage with the probability of employment of all other workers, the general procedure to overcome the criticism of bad quality comparison group has been the inclusion of dummies for the group marginally above the minimum wage and testing whether the impact was different for this group and the treated one, see for instance Currie and Fallick (1996) and Abowd et. al (1997).

[20]As seen in last section, spillover effect does not seem to be a major problem in Brazilian data. Lemos (2002) and Soares (2002b)also find small spillover effects beyond the 10th percentile of the wage distribution.

equal to the wage in $t-2$. For the individuals who are employed in t we do observe their wages in $t-1$. Our outcome variables are non-employment[21], unemployment, out of the labour force, non-registered (informal) and self-employed status in t, after the real or the pseudo increase in the minimum wage. For the period 1995 to 1999 the minimum wage was increased every May. So we will compare transitions between April and May $(t=1)$ with transitions for the pair of months immediately before: March to April and for a pair of months 5 months ahead of the last increase, the pair September/October. The treated group is made up of workers who earn between the current minimum wage and the new one, and the comparison groups are made up of workers who earn between the new minimum wage and twice the current one. For the September/October pair the pseudo-increase is equal to the increase that would be observed in May of the following year. Treated individual are the ones who earn between the current minimum wage and the future one, and control group individuals are the ones who earn between the future minimum wage and twice the current one.

5. Results

5.1. The period 1982-1985

Probit Analysis

All probit regressions were run with the following controls: gender, age, age squared, six education groups, region, and industry. The coefficients of interest is the interaction between the month of the minimum wage increase and the treated group (*t*m*) that gives us the sign of the difference in difference, i.e, whether or not workers affected by the minimum wage increases had a different (worse) transition pattern in the labour market in the month of the increase when compared to similar comparison groups. A positive and significant coefficient indicates that treated observations were affected by the minimum wage in the expected direction, i.e, the minimum wage hike caused disemployment or transitions to the informal sector or to self-employment, according to the specification in use.

Table 1 brings the results for the pooled sample of formal and informal workers and for each group separately of the marginal effect of the interaction term of the dummy for the treated group and the dummy for the month after the minimum wage increase *t*m*[22]. For the nonemployment equation there is no clear pattern for the sign of the marginal effect of interaction term: two out of eight coefficients display an unexpected negative sign, but only the one for May 1984 is statistically significant at 5%, which means that after the minimum wage increase there was a reduction in transitions to unemployment for treated workers when compared to the comparison group in that episode. The other 6 positive estimates are not statistically significant. All in all, it seems that minimum wage increases had no impact on the probability of moving from employment to nonemployment in the period 1982 - 1985, when using this specific comparison group. Not surprisingly, the estimates for both the probability of transition to unemployment and the probability of transition to inactivity (out of the labour force) do not show any clear effect either. Only the increase of the minimum wage in November 1983 seems to have had a positive and significant impact on transitions to inactivity (1.7%).

As for the sample of formal sector workers, the results are quite similar to the previous one for the whole sample. There is no evidence that minimum wage hikes increase the probability that treated workers move from employment to nonemployment relative to their control counterparts.

	Pooled			Formal			Informal		
Dep. Variable	Variable	t*m	z		t*m	z		t*m	z
1982 Transitions to:									
Non-employment	May	-0.003	[0.43]	May	-0.002	[0.19]	May	-0.016	[0.74]
Unemployment	(N=16369)	-0.001	[0.12]	(N=12720)	-0.001	[0.21]	(N=3647)	-0.003	[0.22]

[21]Non-employment state consists in the the sum of unemployment and out of the labour force states.

[22]Results not reported here show that the marginal effects of the dummy for the treated group tend to be positive and significant (but not always), this result means that workers between the old and the new minimum wage are more likely to move to nonemployment than the control group even without the presence of the treatment, i.e, even without the minimum wage hike.

Inactivity		-0.002 [0.35]		0.000 [0.04]		-0.011 [0.69]
Non-employment	Nov	0 [0.01]	Nov	0.004 [0.54]	Nov	-0.014 [0.67]
Unemployment	(N=18815)	-0.006 [1.37]	(N=15063)	-0.003 [0.56]	(N=3752)	-0.019 [1.66]
Inactivity		0.006 [1.18]		0.006 [1.20]		0.008 [0.48]
1983 Transitions to:						
Non-employment	May	0.01 [1.26]	May	0.002 [0.28]	May	0.037 [1.55]
Unemployment	(N=17974)	0.006 [1.16]	(N=14424)	0.002 [0.39]	(N=3550)	0.021 [1.52]
Inactivity		0.004 [0.74]		0.001 [0.11]		0.018 [1.03]
Non-employment	Nov	0.016 [1.55]	Nov	0.007 [0.76]	Nov	0.044 [1.42]
Unemployment	(N=17974)	0.001 [0.10]	(N=13965)	0.001 [0.11]	(N=3581)	0.003 [0.20]
Inactivity		0.017 [2.08]*		0.007 [0.98]		0.047 [1.81]
1984 Transitions to:						
Non-employment	May	-0.023 [2.08]*	May	-0.003 [0.23]	May	-0.084 [2.70]**
Unemployment	(N=16672)	-0.008 [1.16]	(N=14389)	0.004 [0.60]	(N=3395)	-0.05 [2.76]**
Inactivity		-0.013 [1.50]		-0.008 [0.94]		-0.022 [0.96]
Non-employment	Nov	0.015 [1.23]	Nov	0.011 [1.03]	Nov	0.021 [0.48]
Unemployment	(N=17202)	0.005 [0.72]	(N=13630)	0.007 [0.94]	(N=3572)	-0.008 [0.36]
Inactivity		0.008 [0.84]		0.003 [0.36]		0.051 [1.30]
1985 Transitions to:						
Non-employment	May	0.001 [0.12]	May	-0.008 [1.22]	May	0.068 [2.30]*
Unemployment	(N=24828)	-0.002 [0.35]	(N=20951)	-0.005 [1.10]	(N=3877)	0.021 [1.17]
Inactivity		0.002 [0.48]		-0.003 [0.67]		0.043 [1.92]
Non-employment	Nov	0.005 [0.43]	Nov	0.009 [0.82]	Nov	-0.013 [0.43]
Unemployment	(N=17557)	-0.001 [0.13]	(N=13949)	0.003 [0.49]	(N=3608)	-0.012 [0.97]
Inactivity		0.005 [0.54]		0.006 [0.68]		-0.002 [0.07]

*Significant at 5%, **Signifcant at 1%

Robust z statisitcs in brackets

Table 1. Changes in Transition to Non-employment: Probit (1982-1985)

As for the informal sector workers[23], we find significant marginal effects for the interaction term in May 1984 and May 1985. However, whereas for 1984 the coefficient is negative, indicating that the minimum wage increase made informal treated workers more employable than their control counterparts, in 1985 it is positive indicating that there was an increase in transition to non-employment for treated individuals[24]. But again there is no pattern in the sign of the interactions during this period.

As for the transitions to informality the only significant interaction terms were found in November 1984 and November 1985 and with a negative sign, implying that the minimum wage hike led to a reduction in transitions to the informal sector (non-registered state). As for transition to self-employment, only the interaction term for May 1982 is negative and statistically significant.

	Formal			
Dep.	Variable	t*m	se	
1982 Transitions to:				
Informal Sector	May	0.008 [1.02]	Nov	-0.007 [1.23]
Self-Employment	(N=12720)	-0.002 [2.91]**	(N=15063)	0.000 [0.20]
1983 Transitions to:				

[23]Similarly to the pooled sample and the sample for formal workers, results not reported here show that in general treated workers are more likely to move to non-employment than their counterparts in the control group. However, this result is not as strong as for the former samples.

[24]It is interesting to note that whereas the positive effect on nonemployment in 1985 seems to be determined by an increase in transitions to inactivity, the negative effect on nonemployment of 1984 was determined by a reduction in transitions to unemployment

	Informal Sector	May	-0.004 [0.68]	Nov	0.006 [0.69]
	Self-Employment	(N=14424)	-0.001 [0.68]	(N=13965)	0.002 [0.74]
1984	Transitions to:				
	Informal Sector	May	0.014 [1.27]	Nov	-0.021 [2.02]*
	Self-Employment	(N=14389)	-0.004 [1.25]	(N=13630)	-0.001 [0.36]
1985	Transitions to:				
	Informal Sector	May	0.001 [0.07]	Nov	-0.026 [2.81]**
	Self-Employment	(N=20951)	0.004 [1.16]	(N=13949)	0.000 [0.13]

*Significant at 5%, **Significant at 1%
Robust z statistics in brackets

Table 2. Changes in Transition to Informality: Probit

All in all, the above results for this sample period and for the parametric methodology suggest a negligible effect of minimum wage hikes on employment transitions.

Matching: Kernel-based results

The specification used to estimate the propensity score for both the treated group $P(I=1|X)$ and for the time after treatment $P(t=1|X)$ - to correct for compositional change[25] - was the same that we used for the probit estimates.
The estimates of interest here are the differences between treated and control groups in the probability of moving to the nonemployment, unemployment, inactivity, informality and self-employment in the months after minimum wage increases when compared to months when the increase did not take place.
As in the case of the probit estimates, the propensity score matching estimates for the pooled sample do not exhibit a common pattern on the effect of nominal minimum wage hikes on transitions to nonemployment. Table 3 shows that the sign of the differences in probabilities varies considerably. For November 1982 and May 1984 it seems to have occurred a decrease in the probability of nonemployment for treated workers, whereas for the other episodes over this period there has been an increase. Nevertheless these estimates are only statistically significant for May 1984. This result is similar to the one found by the probit estimates of the marginal effect of the difference-in-differences approach (Table 1). As for transitions to unemployment only the estimates for May 1983 are positive and significant and for transitions to inactivity there is a statistically significant decrease in May 1984.

	Difference in Differences in Probability		
	Nonemployment	Unemployment	Inactivity
Pooled sample	estimates se	estimates se	estimates se
May-82	0.005 [0.008]	0.001 [0.005]	0.004 [0.006]
Nov-82	0 [0.006]	-0.007 [0.004]	0.007 [0.005]
May-83	0.011 [0.007]	0.011 [0.005]**	0 [0.005]
Nov-83	0.013 [0.008]	0.006 [0.006]	0.007 [0.006]
May-84	-0.024 [0.009]**	-0.008 [0.007]	-0.015 [0.006]**
Nov-84	0.009 [0.009]**	0.006 [0.007]	0.003 [0.005]
May-85	0.012 [0.007]	0.002 [0.004]	0.01 [0.006]
Nov-85	0.007 [0.010]	0.006 [0.006]	0.001 [0.007]
Formal sector			
May-82	0.002 [0.008]	-0.001 [0.006]	0.003 [0.006]
Nov-82	-0.004 [0.081]	-0.007 [0.005]	0.003 [0.005]

[25]We do not report the results of the probit estimations used to calculate the propensity score indexes used in the matching process. But it is worth mentioning that in general the covariates were not significant in the equation for $P(t=1|X)$, so it seems the compositional effects are not a major issue in this particular application.

May-83	0.009 [0.079]	0.01 [0.006]	-0.001 [0.006]
Nov-83	0.01 [0.009]	0.004 [0.006]	0.005 [0.007]
May-84	-0.009 [0.011]	0.001 [0.007]	-0.01 [0.008]
Nov-84	0.003 [0.008]	0.004 [0.007]	-0.002 [0.006]
May-85	0.006 [0.007]	0.002 [0.004]	0.004 [0.005]
Nov-85	0.005 [0.008]	0.002 [0.006]	0.002 [0.082]
Informal sector			
May-82	-0.017 [0.021]	-0.013 [0.015]	-0.004 [0.017]
Nov-82	0.02 [0.024]	-0.009 [0.013]	0.029 [0.020]
May-83	0.041 [0.025]	0.032 [0.017]	0.009 [0.019]
Nov-83	0.043 [0.027]	0.004 [0.207]	0.039 [0.021]
May-84	-0.09 [0.029]**	-0.057 [0.021]**	-0.033 [0.023]
Nov-84	0.008 [0.029]	-0.018 [0.017]	0.026 [0.020]
May-85	0.024 [0.102]	-0.001 [0.012]	0.025 [0.022]
Nov-85	-0.023 [0.022]	0.001 [0.013]	-0.024 [0.017]

*Significant at 5%, **Significant at 1%
Bootstrapped standard errors

Table 3. Changes in Transition to Non-Employment: Kernel Propensity Score Matching (1982-1985)

As for the sample of formal workers, most of the estimates of differences in the probability of transition to nonemployment are positive, but there are negative signs for November 1982 and May 1984. Nevertheless none of the estimates are statistically significant like in the probit specifications. The same lack of pattern occurs again for the decomposition of this transitions into unemployment and inactivity.

	Difference in Differences in Probability			
	Informality		Self-employment	
Formal	estimates	se	estimates	se
May-82	0.002	[0.008]	-0.005	[0.002]*
Nov-82	0.003	[0.006]	-0.001	[0.002]
May-83	0.001	[0.007]	0.001	[0.003]
Nov-83	0.002	[0.008]	0.001	[0.002]
May-84	0.011	[0.011]	-0.004	[0.011]
Nov-84	-0.016	[0.008]*	-0.001	[0.003]
May-85	-0.006	[0.006]	-0.003	[0.003]
Nov-85	-0.027	[0.009]**	0.007	[0.004]

*Significant at 5%, **Significant at 1%
Bootstrapped standard errors

Table 4. Changes in Transition to Informality: Kernel Propensity Score Matching (1982-1985)

As for the sample of informal workers, most of the estimates of the difference in the transitions to nonemployment are positive, but there are some negative estimates: May 1982, May 1984 and November 1985. However, only the estimate for May 1984[26] is statistically significant, which means that the minimum wage hike of May 1984 led to a lower transition to unemployment to treated informal workers when compared to the control informal workers.

As with the probit estimates, the kernel estimates of changes in the probability of transition to the informal sector reported in Table 4 show a statistically significant effect of the minimum wage hike for November 1984 and November 1985 with reductions in this outflow as well as a reduction in changes to self-employment in May 1982[27]. Overall, the results for this period using the kernel estimates do not

[26]Again this result is in line with the probit estimates.
[27] The only difference between probit and kernel estimates is the reduction into transition to self-employment found in the

diverge much from the result of the probit analysis and point to the absence of negative effect of the minimum wage hikes on employment.

6.2. The period 1995-1999

The period 1995-1999 witnessed five minimum wage increases. Unlike the period 1982-1985, the minimum wage increased during this period in real terms. The impact of the increase in real terms, nonetheless, varied a lot from year to year, but the biggest real increase was in May 1995.
Besides using the pair of months just before the increase in the minimum wage - March/April - as contrast for the pair of months of the increase - April/May, we simulate an increase in the minimum wage between September and October and contrast the effect of this pseudo-increase with the actual increase of April/May. In the pseudo-increase for the pair September/October the treated group is defined as workers earning between the current minimum wage and the minimum wage of May of the following year, and the control group is defined as workers earning between the future minimum wage and two times the current one[28].

Probit Analysis

Table 5 shows that the marginal effect of the interaction *t*m* alternates positive and negative values when the pair of months immediately before the minimum wage increase is used March/April (*May*), but it is always positive when the pair September/October (*Oct*) is used as contrast. However for most specification the estimates are not significant. Only for 1996 and 1998 the interaction is statistically significant and only when the pair September/October is used as contrast[29]

		Pooled Sample			Formal Sector			Informal Sector		
	Dep. Variable		t*m	se		t*m	se		t*m	se
1995	Transtions to:									
	Nonemployment	May	-0.008	[0.73]	May	-0.002	[0.14]	May	-0.017	[0.72]
	Unemployment	(N=11948)	-0.004	[0.86]	(N=7714)	-0.001	[0.10]	(N=4234)	-0.007	[0.86]
	Inactivity		-0.003	[0.34]		0.000	[0.03]		-0.011	[0.49]
	Nonemployment	Oct	0.007	[0.60]	Oct	0.004	[0.31]	Oct	0.014	[0.60]
	Unemployment	(N=13206)	0.000	[0.06]	(N=8456)	0.004	[0.61]	(N=4750)	-0.004	[0.54]
	Inactivity		0.007	[0.65]		0.000	[0.01]		0.019	[0.89]
1996	Transtions to:									
	Nonemployment	May	0.003	[0.24]	May	0.002	[0.15]	May	0.01	[0.42]
	Unemployment	(N=12530)	0.004	[0.65]	(N=7762)	0.005	[0.87]	(N=4704)	0.007	[0.59]
	Inactivity		-0.002	[0.18]		-0.002	[0.22]		0.003	[0.16]
	Nonemployment	Oct	0.027	[2.03]*	Oct	0.001	[0.07]	Oct	0.06	[2.35]*
	Unemployment	(N=12084)	0.012	[1.84]	(N=7447)	0.001	[0.24]	(N=4637)	0.021	[1.78]
	Inactivity		0.015	[1.34]		-0.003	[0.28]		0.039	[1.76]
1997	Transtions to:									
	Nonemployment	May	0.017	[1.14]	May	0.014	[0.95]	May	0.016	[0.58]
	Unemployment	(N=10949)	-0.003	[0.43]	(N=6958)	0.015	[1.76]	(N=3991)	-0.014	[1.47]
	Inactivity		0.021	[1.59]		0.005	[0.42]		0.04	[1.51]
	Nonemployment	Oct	0.009	[0.63]	Oct	0.005	[0.37]	Oct	0.021	[0.78]

probit estimates, but not in the Kernel estimates.

[28]Notice that the results for October are not a different minimum wage increase as were the results for November for the period 1982-1985. The results reported on the rows *Oct* refer to the comparison between transitions from April to May and transitions from September to October.

[29]As it was the case for the period 1982-1985, estimates not shown here reveal that the treated group is in general more likely to move to nonemployment than the control group regardless of minimum wage hikes. Transitions to inactivity rather than transitions to unemployment is the major force driving this result.

	Unemployment	(N=11498)	-0.002	[0.24]	(N=7146)	0.005	[0.58]	(N=4352)	-0.007	[0.64]
	Inactivity		0.009	[0.81]		0.000	[0.01]		0.03	[1.25]
1998	Transtions to:									
	Nonemployment	May	0.018	[1.27]	May	0.001	[0.10]	May	0.034	[1.27]
	Unemployment	(N=11399)	0.013	[1.57]	(N=6938)	0.001	[0.20]	(N=4461)	0.025	[1.59]
	Inactivity		0.005	[0.41]		-0.001	[0.08]		0.009	[0.41]
	Nonemployment	Oct	0.035	[2.40]*	Oct	0.004	[0.27]	Oct	0.066	[2.39]*
	Unemployment	(N=11915)	0.02	[2.14]*	(N=7331)	0.009	[1.15]	(N=4524)	0.023	[1.49]
	Inactivity		0.015	[1.31]		-0.004	[0.44]		0.042	[1.77]
1999	Transtions to:									
	Nonemployment	May	-0.005	[0.35]	May	0.021	[1.25]	May	-0.029	[1.17]
	Unemployment	(N=11300)	-0.006	[0.90]	(N=7057)	0.001	[0.19]	(N=4243)	-0.014	[1.33]
	Inactivity		0.001	[0.12]		0.021	[1.41]		-0.012	[0.56]
	Nonemployment	Oct	0	[0.03]	Oct	0.005	[0.35]	Oct	-0.008	[0.31]
	Unemployment	(N=11515)	0.002	[0.28]	(N=7244)	0.006	[0.95]	(N=4271)	-0.004	[0.29]
	Inactivity		-0.001	[0.12]		0.000	[0.04]		-0.003	[0.16]

*Significant at 5%, **Significant at 1%.

Robust z statistics in brackets

Table 5. Changes in Transition to Non-employment: Probit (1995-1999)

Decomposing the impact on non-employment into its two components, unemployment and inactivity, we find even more volatility in the results with the sign of the interaction terms varying a lot. The significance of the positive interaction term for 1996[30] and 1998 discussed above is driven by the transitions to unemployment, which are positive and significant when the pair September/October is used as contrast. As for the effect on the probability of moving to inactivity the results of the interaction term were never significant.

As for the formal sector[31], the interaction term in the nonemployment equation, displays in general positive coefficients as expected, but they are never statistically significant. Interestingly, the large nominal (and also real) increase in the minimum wage observed in 1995 led to a negative (but not significant) sign for the interaction term when the pair of months just before the increase is used as contrast. Looking at the equations for unemployment and inactivity it is striking that the positive - but not significant - interaction terms observed for the nonemployment equation can be attributed to the effect of minimum wage hikes on unemployment. The coefficients of the interaction term on the inactivity equation display a much more volatile behaviour, changing from positive to negative, from year to year, whereas the non-employment equation display positive coefficients for all years, with the exception of 1995 when the pair March/April is used as contrast.

For the sample of informal workers, the results are much more in line with the ones found for the pooled sample than the ones found for formal workers. The coefficients of the interaction terms where positive for all years with the exception of 1995 (when using March/April as contrast) and 1999 (for both pairs of contrast). However, the most interesting fact is that for the year 1996 and 1998 when the pair September/October is used as contrast the positive coefficient is also statistically significant at 5%. For both years, this result seems to be driven by positive changes in the probability of moving to inactivity rather than moving to unemployment. Hence, according to the probit estimates, informal sector workers were the ones who were hit by minimum wage hikes after 1995.

As for the probability of moving to the informal sector (non-registered workers) Table 6 shows that again there is no pattern. The interaction assumes positive and negative values, in general statistically insignificant, from one year to another. Only in 1995 and in 1998, when the pair September/October is

[30]In this case the estimate is significant only at 10%.

[31]Results not reported here show that the treated group does not necessarily have a higher probability of moving to non-employment than the comparison group. This result is only observed for 1995 regardless of the pair of months used as control, whereas it was observed for most of the years in the period 1982-1985 and for the pooled sample as discussed above. Only transitions from employment to inactivity seems to be more likely for the treated group than in the control, even when the coefficient is not significant, it is always positive.

used as contrast, we do find positive and significant effect of the minimum wage hike on the probability of moving to the informal sector[32]. The same lack of pattern also occurs with the probability of moving to self-employment. In 1996, the minimum wage hike seems to have increased the probability of becoming self-employed[33], while in 1999 it had the opposite effect[34].

		Formal Sector					
	Dep. Variable	Month/N	t*m	se	Month/N	t*m	se
1995	Transitions to:						
	Informal Sector	May	-0.007	[0.50]	Oct	0.032	[2.21]*
	Self-Employment	(N=7714)	-0.004	[0.70]	(N=8456)	-0.007	[1.79]
1996	Transitions to:						
	Informal Sector	May	-0.011	[0.73]	Oct	-0.015	[1.04]
	Self-Employment	(N=7762)	-0.002	[0.36]	(N=7447)	0.021	[1.91]
1997	Transitions to:						
	Informal Sector	May	-0.002	[0.11]	Oct	-0.023	[1.50]
	Self-Employment	(N=6958)	-0.001	[0.14]	(N=7146)	-0.004	[0.77]
1998	Transitions to:						
	Informal Sector	May	0.021	[1.16]	Oct	0.042	[2.38]*
	Self-Employment	(N=6938)	0.006	[0.83]	(N=7331)	0.015	[1.68]
1999	Transitions to:						
	Informal Sector	May	-0.009	[0.54]	Oct	0.003	[0.15]
	Self-Employment	(N=7057)	-0.008	[1.60]	(N=7244)	-0.008	[2.57]*

*Significant at 5%, **Significant at 1%.
Robust z statistics in brackets

Table 6. Changes in Transition to Informality: Probit (1995-1999)

Matching: Kernel-based results

According to Table 7, the estimates using propensity score matching kernel for both contrast pairs, March/April and September/October indicate a negative and insignificant impact of minimum wage on transitions to non-employment for the pooled sample in 1995. For all other years the estimates show a positive effect of the minimum wage hikes on non-employment. This effect is statistically significant for 1996 regardless of the contrast pair of months, for 1997 (March/April as contrast) and for 1998 (Sept/Oct as contrast). These results for the pair Sept/Oct as contrast months are in line with the probit estimates. As for the decomposition on transitions to unemployment, the estimates are in general positive, but only in 1998 is the estimate statistically significant and positive (regardless of the pair of months used as contrast[35]). As for transition to inactivity there is a positive and significant effect only in 1997 when March/April is used as contrast.

	Difference in Differences in Probability					
	Nonemployment		Unemployment		Inactivity	
Pooled sample	estimates	se	estimates	se	estimates	se
May-95	-0.008	[0.012]	-0.003	[0.006]	-0.004	[0.011]
Oct-95	-0.003	[0.012]	0.004	[0.006]	-0.007	[0.011]
May-96	0.01	[0.014]	0.008	[0.006]	0.002	[0.012]
Oct-96	0.028	[0.014]*	0.01	[0.007]	0.018	[0.013]
May-97	0.037	[0.017]*	0.003	[0.007]	0.034	[0.017]*
Oct-97	0.015	[0.016]	0.004	[0.008]	0.01	[0.015]

[32]All negative estimates are non-significant.
[33]The coefficient is significant at 10%.
[34]Both significant effects are only found when the pair September/October is used as contrast.
[35]In the probit analysis this result holds only when the pair September/October is used as contrast.

May-98	0.041 [0.016]**	0.015 [0.008]*	0.026 [0.014]
Oct-98	0.035 [0.015]**	0.019 [0.007]**	0.016 [0.014]
May-99	0.021 [0.016]	-0.007 [0.007]	0.028 [0.016]
Oct-99	0.003 [0.014]	0.005 [0.008]	-0.003 [0.014]
Formal sector			
May-95	-0.006 [0.011]	0.002 [0.006]	-0.008 [0.108]
Oct-95	-0.005 [0.015]	0.005 [0.007]	-0.011 [0.013]
May-96	0.012 [0.013]	0.006 [0.006]	0.006 [0.013]
Oct-96	0.033 [0.016]*	0.01 [0.008]	0.023 [0.014]
May-97	0.021 [0.019]	-0.001 [0.010]	0.021 [0.017]
Oct-97	0.001 [0.021]	0.002 [0.010]	-0.001 [0.019]
May-98	0.02 [0.018]	0.005 [0.009]	0.0152 [0.016]
Oct-98	0.013 [0.020]	0.014 [0.012]	0 [0.016]
May-99	0.015 [0.015]	-0.003 [0.009]	0.018 [0.019]
Oct-99	0.008 [0.019]	0.009 [0.009]	-0.001 [0.016]
Informal sector			
May-95	-0.002 [0.026]	0 [0.014]	-0.002 [0.023]
Oct-95	-0.007 [0.025]	0.001 [0.012]	-0.008 [0.023]
May-96	0.02 [0.029]	0.011 [0.011]	0.009 [0.027]
Oct-96	0.006 [0.026]	0.018 [0.010]	-0.012 [0.023]
May-97	0.059 [0.031]	0.005 [0.013]	0.053 [0.028]
Oct-97	0.029 [0.033]	0.011 [0.018]	0.019 [0.029]
May-98	0.037 [0.023]	0.024 [0.013]	0.013 [0.021]
Oct-98	0.061 [0.029]*	0.017 [0.012]	0.044 [0.026]
May-99	0.013 [0.030]	-0.005 [0.016]	0.018 [0.027]
Oct-99	0.009 [0.029]	-0.004 [0.015]	0.013 [0.026]

*Significant at 5%, **Signifcant at 1%
Bootstrapted standard errors

Table 7. Changes in Transition to Non-Employment: Kernel Propensity Score Matching (1995-1999)

As for the results for the formal sector, they are quite similar to the ones for the pooled sample in terms of sign: negative effects on transitions to nonemployment for 1995 and positive effects for all other years, regardless of the pair of months used as contrast. However, only for 1996 when the pair September/October is used as contrast is the positive estimate significant. None of the estimates for transitions to unemployment and for transitions to inactivity are significant.

The same pattern of transitions to nonemployment is found in the case of informal workers. Only for 1995 there was a reduction in transitions to nonemployment. For all other years the estimates are positive. However, as was the case for the pooled sample, only for 1998 and when the pair September/October is used as contrast the positive estimate is significant. Moreover, none of the estimates for transitions to unemployment and for transitions to inactivity are significant.

As for transitions to informality, Table 8 shows that there is no pattern in the sign of the change. The only statistically significant estimate is a negative effect for 1997 when the pair September/October is used as contrast. As for transitions to self-employment the positive effect on transitions to self-employment found in 1996 when the pair September/October is used as contrast is statistically significant.

	Difference in Differences in Probability			
	Informality		Self-employment	
Formal	estimates	se	estimates	se
May-95	-0.0168	[0.015]	0.0004	[0.006]

Oct-95	0.0206 [0.015]	0.0005 [0.006]
May-96	-0.023 [0.019]	-0.0003 [0.008]
Oct-96	-0.0213 [0.017]	0.0177 [0.007]**
May-97	-0.0141 [0.019]	0.0017 [0.008]
Oct-97	-0.0608 [0.021]**	-0.0015 [0.007]
May-98	0.0012 [0.022]	0.0024 [0.007]
Oct-98	0.006 [0.021]	0.0064 [0.006]
May-99	0.0238 [0.020]	-0.0006 [0.008]
Oct-99	0.0043 [0.019]	-0.0118 [0.007]

*Significant at 5%, **Signifcant at 1%
Bootstrapted standard errors

Table 8. Changes in Transition to Informality: Kernel Propensity Score Matching (1995-1999)

Assessing Comparison Groups for the Non-parametric Results

A good comparison group should not display any strong effect after minimum wage increases. For instance, if workers marginally further up the wage distribution in relation to the minimum wage are good substitutes for minimum wage workers, one should expect their probability of moving to nonemployment to decrease, making the effect of the minimum wage even larger. Another possibility is that workers whose wages are indexed to the minimum wage would also be "at risk" of losing their jobs. If this were the case, the comparison group that includes multiples of minimum wage such as 1.5, 2, 2.5 or 3 times the minimum wage would also face a higher probability of moving to nonemployment after minimum wage hikes[36]. The real figure, however, must be a mixture of these two effects.
One way to assess how these effects jeopardize the quality of the comparison group is to estimate the differences in the probability of transition to nonemployment before and after the minimum wage increase for the comparison group sample. For the comparison group to be of good quality this difference must be small (close to zero) and not statistically significant. Tables 9 to 11 bring the results of these estimates.

	POOLED SAMPLE		
	Nonemployment	Unemployment	Inactivity
Kernel estimates	estimates se	estimates se	estimates se
May-82	-0.013 [0.006]*	-0.006 [0.003]	-0.007 [0.005]
Nov-82	-0.001 [0.005]	0.001 [0.004]	-0.002 [0.004]
May-83	-0.009 [0.005]	-0.011 [0.004]**	0.001 [0.004]
Nov-83	-0.008 [0.007]	-0.003 [0.005]	-0.006 [0.005]
May-84	0.016 [0.008]*	0.007 [0.006]	0.009 [0.005]
Nov-84	-0.006 [0.082]	-0.009 [0.007]	0.003 [0.004]
May-85	-0.01 [0.006]	-0.001 [0.003]	-0.009 [0.003]
Nov-85	-0.005 [0.009]	-0.009 [0.006]	0.004 [0.006]
May-95	0.012 [0.009]	0.004 [0.005]	0.008 [0.008]
Oct-95	0.002 [0.009]	-0.004 [0.005]	0.006 [0.008]
May-96	0.003 [0.009]	-0.002 [0.004]	0.005 [0.008]
Oct-96	-0.009 [0.010]	-0.004 [0.004]	-0.005 [0.009]
May-97	-0.009 [0.011]	0.008 [0.005]	-0.017 [0.011]
Oct-97	-0.007 [0.011]	-0.001 [0.006]	-0.005 [0.011]
May-98	-0.038 [0.011]**	-0.01 [0.005]*	-0.028 [0.009]**

[36]The wage indexation of the 1980's should have a similar effect. The lack of disemployment effect over this period may be due to shifts in the whole wage distribution caused by the indexation rules.

Oct-98	-0.017 [0.009]	-0.007 [0.005]	-0.01 [0.006]
May-99	-0.028 [0.011]**	-0.003 [0.005]	-0.025 [0.010]**
Oct-99	0.001 [0.010]	-0.005 [0.005]	0.007 [0.009]

Table 9 – Difference in Probability Before and After the Treatment for the Control Group (Pooled Sample)

The results in Tables 9 to 11 suggest that the negative effect in transitions to nonemployment found in May 1984 and the positive effect of the minimum wage on transitions to unemployment in May 1983 were due to statistically significant changes in the probability of transitions for the control group. The only robust result is the fall in transitions to self-employment for formal sector workers in May 1982. The results for the late 1990's are much more robust, since only the positive effect on transitions to nonemployment observed in 1998 does not pass the test. The positive effect on transitions to nonemployment found for 1996 and 1997 are robust to the test of quality of the control group.

	FORMAL SECTOR									
	Nonemployment		Unemployment		Inactivity		Informality		Self-employment	
Kernel Estimates	estimates	se	estimates	se	estimates	se	estimates	se	estimates	se
May-82	-0.01	[0.006]	-0.005	[0.004]	-0.005	[0.004]	-0.011	[0.006]*	0.003	[0.002]
Nov-82	0.005	[0.005]	0.002	[0.003]	0.002	[0.004]	-0.006	[0.005]	0.001	[0.002]
May-83	-0.01	[0.079]	-0.011	[0.005]*	0.001	[0.004]	0.003	[0.005]	-0.002	[0.003]
Nov-83	-0.007	[0.009]	-0.002	[0.006]	-0.005	[0.082]	-0.003	[0.007]	-0.003	[0.002]
May-84	0.005	[0.009]	-0.001	[0.005]	0.006	[0.007]	-0.005	[0.010]	0.007	[0.010]
Nov-84	-0.001	[0.007]	-0.007	[0.006]	0.006	[0.005]	0.015	[0.007]*	0.002	[0.002]
May-85	-0.008	[0.006]	-0.003	[0.004]	-0.004	[0.005]	0.007	[0.005]	0.003	[0.002]
Nov-85	0.001	[0.008]	-0.002	[0.005]	0.003	[0.005]	0.037	[0.010]**	-0.01	[0.005]*
May-95	0.006	[0.008]	0.003	[0.003]	0.003	[0.007]	0.021	[0.010]*	-0.001	[0.005]
Oct-95	-0.003	[0.009]	-0.004	[0.005]	0.001	[0.008]	0.004	[0.011]	0.001	[0.003]
May-96	0.003	[0.007]	-0.001	[0.004]	0.004	[0.007]	0.019	[0.012]	0.003	[0.004]
Oct-96	-0.014	[0.011]	-0.007	[0.006]	-0.008	[0.009]	0.028	[0.011]**	-0.004	[0.005]
May-97	0.012	[0.014]	0.01	[0.007]	0.002	[0.013]	0.017	[0.010]	-0.002	[0.006]
Oct-97	0.020	[0.016]	0.003	[0.009]	0.017	[0.014]	0.019	[0.013]	0.000	[0.005]
May-98	-0.023	[0.013]	-0.003	[0.007]	-0.02	[0.011]	-0.006	[0.013]	-0.004	[0.005]
Oct-98	-0.013	[0.013]	-0.008	[0.011]	-0.005	[0.008]	0.011	[0.010]	-0.001	[0.005]
May-99	-0.016	[0.012]	-0.002	[0.005]	-0.015	[0.011]	-0.002	[0.011]	-0.003	[0.006]
Oct-99	-0.009	[0.012]	-0.008	[0.007]	-0.001	[0.009]	0.009	[0.010]	0.006	[0.005]

*Significant at 5%, **Significant at 1%

Table 10. Difference in Probability Before and After the Treatment for Control Group (Formal Sector)

	Informal Sector					
	Nonemployment		Unemployment		Inactivity	
Kernel estimates	estimates	se	estimates	se	estimates	se
May-82	-0.001	[0.016]	0.008	[0.010]	-0.009	[0.013]
Nov-82	-0.033	[0.020]	-0.005	[0.011]	-0.028	[0.017]
May-83	-0.031	[0.019]	-0.026	[0.015]	-0.004	[0.015]
Nov-83	-0.039	[0.025]	-0.002	[0.019]	-0.037	[0.018]*
May-84	0.061	[0.026]**	0.044	[0.020]*	0.017	[0.021]
Nov-84	-0.008	[0.024]	0.018	[0.014]	-0.026	[0.017]
May-85	-0.009	[0.023]	0.012	[0.010]	-0.021	[0.021]

Nov-85	0.015 [0.018]	-0.01 [0.011]	0.025 [0.014]
May-95	0.004 [0.023]	-0.003 [0.011]	0.007 [0.020]
Oct-95	0.022 [0.020]	0 [0.009]	0.023 [0.018]
May-96	-0.015 [0.023]	-0.007 [0.007]	-0.008 [0.022]
Oct-96	0.018 [0.019]	-0.003 [0.006]	0.021 [0.018]
May-97	-0.033 [0.020]	0.002 [0.009]	-0.035 [0.018]
Oct-97	-0.034 [0.024]	-0.018 [0.013]	-0.015 [0.021]
May-98	-0.049 [0.017]**	-0.016 [0.008]	-0.033 [0.015]*
Oct-98	-0.032 [0.017]	-0.002 [0.008]	-0.031 [0.014]*
May-99	-0.032 [0.021]	-0.004 [0.012]	-0.028 [0.019]
Oct-99	-0.005 [0.020]	0 [0.010]	-0.005 [0.018]
Oct-99	-0.004 [0.029]	-0.006 [0.017]	0.001 [0.028]

*Significant at 5%, **Significant at 1%

Table 11. Difference in Probability Before and After the Treatment for Control Group (Informal Sector)

Overall a closer look at Table 19 suggests that the minimum wage hikes have provoked more disemployment effect on the late 1990's than in the early 1980's. This effect is stronger for the pooled sample, than for any of the separately sample we have investigated. Transitions to the informal sector or to self-employment were rarely significant for both sample periods. It seems that assessing the quality of the control group is a good way to avoid misleading results when evaluating the effect of minimum wage increase on employment transitions. From this section, we could conclude that several of our difference-in-difference matching estimates that seemed to lead to a positive effect of the minimum wage on transitions to nonemployment were due to significant changes in the transition probabilities of the comparison group. Whether this is due to spill-over effects or different reaction between treated and comparison group to simultaneous macroeconomic shocks is not clear. More research is needed to disentangle these two effects.

	Nonemployment	Unemployment	Inactivity	Informality	Self-Employment
Kernel					
Pooled sample					
Oct-96	YES				
May-97	YES				
Oct-98	YES				
Formal sector					
May-82					YES
Oct-96	YES				YES
Oct-97				YES(-)	
Informal sector					
Oct-98	YES				

Table 12. Summary of the Effect of the Minimum Wage Hikes

7. Conclusion

This paper aimed to evaluate the effect of several episodical minimum wage increases on employment transitions in the early 1980's and late 1990's in Brazil. We evaluate this effect for the pooled sample of formal and informal workers and for each group separately. We emphasize the need to incorporate the informal sector in the analysis for two reasons. First, due to possible dynamics effects of minimum wage hikes on the wage of informal sector workers as highlighted in Welch (1974) and Mincer (1976) models. Second due to the well-documented increase in the importance of the minimum wage in the determination

of wage or wage increases for informal sector workers. Thus minimum wage hikes are quite likely to affect the employment transition of informal sector workers either because of (indirect) dynamic effects or because of the informal indexation of the informal sector wages to the minimum wage.

We estimate the effect of the minimum wage hike using difference-in-differences in a parametric way (via probit) and in a non-parametric way (via propensity score matching). To define treated and control groups in the baseline period that was used as contrast (in the difference-in-differences) for the actual minimum wage increase we defined pseudo-experiments both before (in the previous month) and after the minimum wage increase[37] (5 months later).

For the pooled sample we only find evidences of disemployment effects for some of the episodical increases of the late 1990's. According to the probit estimates this occurred in 1996 and 1998 when the pair September/October is used as control. The kernel matching indicates that the increase in transitions to nonemployment was significant in 1996 for the pair September/October, in 1997 for the pair March/April and in 1998 for the pair September/October[38]. As for the formal sector, the results are in line with the probit estimates, there is no disemployment effect on the early 1980's regardless of the method used. For the late 1990's only the kernel matching point to a positive and significant effect in 1996 when the pair September/October is used as contrast. We also find no strong evidence that minimum wage hikes led to transitions to informality or to self-employment. As for the informal sector, the probit estimates are quite similar to the estimates for the pooled sample, indicating disemployment effects in 1996 and 1998 and no effect on the early 1980's. The kernel matching only shows significant disemployment effects in 1998 (when the pair September/October is used) whereas the NN matching indicates significant disemployment effects in 1997 (when the pair March/April is used) and in May 1983.

All in all it seems that the probit estimates and the two propensity score matching do not diverge very much in their estimates. The results indicate that the minimum wage hikes were more likely to have disemployment effects in the late 1990's. These results suggest that the wage indexation of the early 1980's avoided the disemployment effect of the minimum wage as long as it affected the whole wage distribution[39]. However, one puzzling result refers to the minimum wage increase of May 1995. The increase in the real value of the minimum wage observed in that date should make disemployment effects more likely to be found in that episode. However, all estimates point to a negative (but statistically insignificant) effect on transitions to nonemployment for that year[40]. One possible explanation for this result is that the minimum wage was so low before that increase that it was not binding for formal and informal workers.

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[37]Notice that this second pseudo-experiment was only possible for the sample of the late 1990's.

[38]The result for 1998 using the pair March/April as contrast is not robust to the quality of control group.

[39]More research is needed to establish a causal link between these two evidences.

[40] These results contrast with Neri (1997) findings that formal sector workers between the old and the new minimum wage of May 1995 have higher (double) probability to migrate to the informal sector than the group marginally above, and their probability of becoming unemployed or moving out of the labour force is also higher. Such evidence, however, is based on raw probability transitions. It does not control for observable and unobservable heterogeneity between treated and control groups, neither tests the statistical significance of the differences.

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