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Political Variables as Instruments for the Minimum Wage

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The international literature on minimum wage greatly lacks empirical evidence from developing countries. In Brazil, not only are increases in the minimum wage large and frequent but also the minimum wage has been used as anti-inflation policy in addition to its social role. This paper estimates the effects of the minimum wage on employment using monthly household data from 1982 to 2000 aggregated at regional level. A number of conceptual and identification questions is discussed as tentative explanation of the non-negative estimates found in the literature, for example: (1) The use of political variables as excluded exogenous instruments for the minimum wage variable; (2) The superiority of "spike" over "fraction affected" and "Kaitz index" as a minimum wage variable; (3) The decomposition of the minimum wage employment effect into hours worked and number of jobs effects; (4) Robustness checks accounting for sorting into informal and public sectors. Robust results to various alternative specifications and instrumental variables indicate that an increase in the minimum wage has moderately small adverse effects on employment.

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There is currently not much consensus on the direction of the employment effects of the minimum wage. The old debate between Stigler (1946) and Lester (1946), dormant since the early 80s in an apparent consensus of negative significant but modest effects on employment (Brown, Gilroy and Kohen, 1982) has been re-awakened. On the one hand, Neumark and Wascher (1992) and Deere et al. (1995), among others, find results consistent with the standard model prediction of a negative employment effect. On the other hand, Card and Krueger (1995) and Dickens et al. (1999), among others, challenge such a prediction, unable to find disemployment effects. Explanations to non-negative effects range from theory to empirical identification and data issues (Card and Krueger, 1995; Brown, 1999). In a recent survey, Brown (1999, p.2154) remarks: "the minimum-wage effect is small (and zero is often hard to reject)". While there is yet no consensus, small employment effects, clustered around zero, are becoming prevalent in the literature (Freeman, 1994 and 1996; Brown, 1999).

In studies for Brazil, in line with the international empirical literature, an increase in the minimum wage does not always have a significant effect on employment and it is not always negative, despite sizeable wage effects (Camargo, 1984; Velloso, 1988; Neri, 1997; Carneiro, 2000; Carneiro, 2002; Corseuil and Servo, 2002). Using national aggregate data, this literature estimates average wage and employment effects imposing restrictions on time modeling, i.e. relying on the so-called *ad hoc* identification predominant in the early time series literature. This paper estimates the effects of the minimum wage on employment using panel data techniques and monthly Brazilian household data from 1982 to 2000 aggregated at regional level. It contributes to the extant literature in a number of ways.

First, data only recently released for the public and not yet used for minimum wage studies is here used.

Second, the international literature on minimum wage is scanty on non-US empirical evidence. This paper estimates minimum wage effects for a key non-US example. There are compelling reasons to study the minimum wage outside the US. "No single empirical study of an economic phenomenon is ever highly convincing" (Hamermesh, 2002, p. 4). Many data points are needed - many and independent data points are needed. Using non-US data is an unbiased way of extending the understanding of minimum wage effects and assessing the robustness of findings for the US. Hamermesh (2002, p. 15) argues for increased reliance on non-US data and policy evaluations: "policies like hours legislation and the minimum wage provide especially fruitful areas in which to apply the results of studying foreign experiences to the US".

Furthermore, Hamermesh (2002) calls attention for the evidence from developing countries, which is greatly lacking in the literature. Minimum wage increases in Brazil are large and frequent, unlike the typically small increases studied in most of the literature (Deere et al, 1996; Hamermesh, 2002; Castillo-Freeman and Freeman, 1992). Studying such increases allows a better possibility of observing the negative effects predicted by theory and thus the link between empirical data and theoretical models of the minimum wage. Furthermore, Hamermesh (2002) remarks that foreign experiences are especially fruitful if they generate exogenous shocks (an alternative to reliance on statistical methods to circumvent the problems arising from endogeneity), as in Brazil over the past 30 years. Moreover, special features of the Brazilian Economy are valuable for case studies of the minimum wage in presence of: a (low) high inflation; a public sector and a large informal sector both overpopulated by minimum wage workers; and a strong link between benefits and pensions with the minimum wage. This unique data is a result of the important role the minimum wage plays in Brazil, where it has been used as an anti-inflation policy in addition to its traditional social role (Macedo and Garcia, 1978, 1980; Camargo, 1984; Foguel, 1997; Carneiro, 2000).

Third, this paper discusses a number of conceptual and identification questions as tentative explanations of the non-negative employment effects found in the literature. For example:

(1) A national minimum wage cannot explain variation in employment across regions (Brown et al., 1982; Card and Krueger, 1995). Identification of the effect of the minimum wage separately from the effect of other variables on employment requires regional variation if no restriction on time modeling is imposed. This motivates the use of "spike" as a minimum wage variable, which is here argued to be superior to the commonly used "Kaitz index" and "fraction affected".

(2) The minimum wage variable and employment might be simultaneously determined. Identification of the effect of the minimum wage separately from the effect of unobserved variables on employment requires consistent estimation if such endogeneity bias is to be corrected for. Put differently, rather than capturing a descriptive relationship - which asks: *if a person is taken at random from the population, what is his/her expected hours of work, given the level of the minimum wage?* - the instrumented model captures a behavioural relationship - which asks: *if the same person is taken from the population, knowing which region he/she comes from (i.e., controlling for observed and unobserved regional effects), and the minimum wage is increased by 1%, by how much would his/her hours of work be expected to increase/decrease?* This paper suggests a number of political variables – not previously suggested in the literature - as excluded exogenous instruments to control for the endogeneity of the minimum wage variable.

(3) Identification of the effect of the minimum wage separately from the effect of unobserved regional macro fixed effects on employment requires modeling fixed effects. This paper uses panel data techniques, scarcely used in the minimum wage literature, to account for this.

(4) This paper formalizes an employment decomposition that separately estimates the effect of the minimum wage on hours worked and on the number of jobs; if the first is positive and the second is negative, this could be an explanation of non-negative (total) employment effects. Such decomposition has not been previously formalized in the literature.

(5) This paper performs robustness checks accounting for sorting into the informal and public sectors, scarce in the literature. Again, if formal sector employment effects are negative and informal sector positive, this could be an explanation of non-negative (net) employment effects.

This paper is organized as follows. Section 2 presents the data. Section 3 describes the minimum wage in Brazil (Section 3.1) and discusses identification (Section 3.2). Section 4 estimates descriptive models. Section 5 further discusses identification: lags of the endogenous variable are used as instruments under the assumption of errors serially uncorrelated (Section 5.1); and political variables are used instead as exogenous excluded instruments when this assumption is relaxed (Section 5.2). Further robustness checks, accounting for sorting into informal (Section 6.1) and public sectors (Section 6.2) are performed. Robust results indicate moderately small employment effects.

2. DATA

The data used is from PME (Monthly Employment Survey), a rotating panel data similar to the US CPS (Current Population Survey). Between 1982 and 2000, PME interviewed over 21 million people across the six main Brazilian metropolitan regions: Bahia (BA), Pernambuco (PE), Rio de Janeiro (RJ), Sao Paulo (SP), Minas Gerais (MG) and Rio Grande do Sul (RS). Its monthly periodicity is important because wage bargains during the sample period occurred annually, bi-annually, quarterly and even monthly, depending on the inflation level and indexation rules. Comparisons of demographic and economic characteristics across regions or waves show no

selectivity bias in any direction (Neri, 1996). The deflator, INPC (National Consumers Price Index), was regionally disaggregated to reduce measurement error.

3. MINIMUM WAGE VARIABLES

3.1 MINIMUM WAGE IN BRAZIL

The minimum wage was introduced in 1940 as a social policy to provide subsistence income (diet, transport, clothing, and hygiene) for an adult worker. The associated bundle varied across regions, which was reflected in 14 minimum wages - the highest (lowest) for the Southeast (Northeast) (Gonzaga and Machado, 2002). Wells (1983, p. 305) believes they were "generous relative to existing standards" since about 60% to 70% of workers earned below them; Saboia (1984) and Oliveira (1981) believe they legitimated the low wages of the unskilled.

The real minimum wage was decreased over time because of two main reasons. The first one has been the failure in adjustments to keep pace with inflation. After a steep decrease, the real minimum wage was adjusted and reached its peak during the boom of the 50s, when productivity was high, unions strong, and the Government populist. After that, it decreased as a result of the subsequent recession, rising inflation, and non-aggressive unions (Singer, 1975). The real minimum wage was then 40% lower than in the 50s.

The minimum wage social role changed when the dictatorship installed in 1964 associated high inflation with wage adjustments. Nominal minimum wage increases can be inflationary because they affect production costs and prices, not only through its direct effect on minimum wage workers, but also through indirect spillover effects (Brown, 1999). The dictatorship limited labour organization, reduced wage militancy, and implemented a centralized wage policy. One of the strategies of this policy was under-indexation of the real minimum wage, via erosion of the nominal minimum wage (Macedo and Garcia, 1978), which transformed the latter "from a social policy designed to protect the worker's living standard into an instrument for stabilization policy" (Camargo, 1984, p.19). The "Teoria do Farol" (Lighthouse Effect) associated the subsequent increase in inequality revealed in the 1970 Census with the pos-64 real minimum wage decrease (Souza and Baltar, 1979, 1980a and 1980b).

According to Carneiro and Faria (1998), the nominal minimum wage was used not only as a stabilization policy but also as a coordinator of the wage policy. One example is that other wages were set as multiples of the minimum wage. Another example is that in the early 80s, wages in the range 1 to 3 minimum wages were bi-annually adjusted by 110% of the inflation rate; the higher the worker's position in the wage distribution, the lower the percentage adjustment. Such increases immediately spilled over higher up the wage distribution; its effects were no longer limited to the bottom of the distribution as when it plays a social role. More generally, the minimum wage played an indexer role. In the presence of high inflation and distorted relative prices, rational agents took increases in the minimum wage as a signal for price and wage bargains - even after law forbade its use as "numeraire" in 1987. Minimum wage indexation and reinforced inflationary expectations was a phenomenon first noticed by Gramlich (1976), Cox and Oaxaca (1981), and Wolf and Nadiri (1981); and more recently discussed by Card and Krueger (1995) and Freeman (1996). Maloney and Nunes (2003) show that the "Efeito Farol" and the "numeraire" effect are a general phenomenon in Latin America.

The second main reason for the decrease of the real minimum wage over time has been its impact on the public deficit - uncontrollably large and growing in the 80s and 90s - via benefits,

pensions, and the Government wage bill.[2] This impact has often been the criterion for the affordable increase in the nominal minimum wage, resulting in under-indexation of the real minimum wage.

Because of its effects both on prices and on the public deficit, the under-indexation of the real minimum wage (by erosion of the nominal minimum wage) was used as a deflationary policy. However, when pressure was enough, the Government had to give in, allowing increases in the nominal minimum wage - the nominal minimum wage became the "messenger" of the inflation - which in turn severely affected both prices and the public deficit and were therefore inflationary. This effect was perpetuated in an inflation spiral. The anti-inflation policy became inflationary itself; the remedy became the disease. In this context, the minimum wage has been alternately used as social and anti-inflation policy. The policy choice depended (a) on the level of inflation, (b) on the bargaining power of the workers, and (c) on the party affiliation of the Government (Velloso, 1988; Bacha, 1979). The social role is associated with more populist Governments, lower inflation, and stronger unions.

Graph 1.a shows that the hourly real minimum wage decreased between 1982 and 2000. Its highest (lowest) level was in November 1982 (August 1991), before the acceleration of inflation. In political terms, three events were important in the 80s: (a) in 1984, the minimum wage became national, after slow regional convergence; (b) with the end of the military regime in 1985, the 1988 Constitution re-defined the subsistence income (diet, accommodation, education, health, leisure, clothing, hygiene, transport, and retirement) for an adult worker and his/her family - even though such a bundle was unaffordable at the prevalent minimum wage; (c) the union movement re-emerged and became ever stronger, reaching a high union density for a developing country (Carneiro and Henley, 1998; Amadeo and Camargo, 1993). In economic terms, despite the political changes, the minimum wage was still a component of the centralized wage policy. The 80s and 90s witnessed an exhausting battle against inflation. Five stabilization plans between 1986 and 1994 had different nominal minimum wage indexation rules depending on the inflation level. Since then, under reasonably stable inflation, the minimum wage has not been explicitly used as an anti-inflation policy.

The steady decrease of the real minimum wage over time suggests a move downwards along the labour demand curve. It is then not surprising that minimum wage employment effects in Brazil are non-negative (Lemos, 2003a and 2003c; Carneiro, 2000; Foguel, 1997; Neri, 1997; Amadeo et al., 1995; Camargo, 1984), despite sizeable wage effects. Graph 2 plots log employment rate against log real minimum wage suggesting a non-negative relationship between the two. Furthermore, Lemos (2003b) shows evidence of full pass-through effect of the minimum wage on prices in Brazil. Evidence of large wage effects, large price effects and small employment effects is consistent with an inelastic labour demand curve and a particularly rapid wage-price spiral under high inflation (note saw-toothed pattern in Graph 1a). Firms anticipate the wage-price spiral - encountering little resistance to upward prices adjustment, as nominal stickiness is smaller the higher inflation (Layard et al., 1991) - and do not adjust employment to avoid adjustment costs.

3.2 IDENTIFICATION

Within a month, the minimum wage is a constant and therefore cannot explain variations in employment across regions. The real minimum wage varies across regions simply because the nominal minimum wage has been deflated with regional deflators. This variation cannot be regarded as genuine, as it is completely driven by the variation in the deflators; the effect of the inverse of the

[2] In the sample period, 12% of the population are pensioners, 7% are civil servants.

deflator on employment is what is ultimately estimated (Welch and Cunningham, 1978; Freeman, 1982). In other words, once the numerator is constant, the variation in the deflator is what drives the estimated impact of the ratio on employment. Lacking genuine regional variation, identification depends on how time is modeled - the so-called *ad hoc* identification predominant in the early minimum wage literature.

Identification requires regional variation if no restriction on time is imposed. Many minimum wage variables with such a regional variation have been suggested in the literature. (1) The typically used is "Kaitz index" (Kaitz, 1970), defined as the ratio of the minimum wage to average wage adjusted for coverage of the legislation. The Kaitz index varies across regions and over time, but the above criticism applies because the variation in average wages is what drives the estimated impact of the ratio on employment. (2) Another minimum wage variable suggested in the literature is "fraction affected", defined as the proportion of people earning a wage between the old and the new minimum wage (Card, 1992; and Card and Krueger, 1995). (3) A variable closely related to fraction affected is "spike", defined as the proportion of people earning one minimum wage (Dolado et al., 1996).

Brown (1999, p. 2130) advocates that the "degree of impact" measures (e.g., fraction affected) are conceptually cleaner than the "relative minimum wage" variable (e.g., Kaitz index). He also notes that fraction affected is "not well-suited for studying periods when the minimum wage is constant, and so its impact should be declining. While there is more to be learned from a year in which the minimum wage increases by 10 or 15% more than average wages than from a year of modest decline, the periods between increases should together contain about as much information as the periods of increase." In other words, fraction is constant at zero regardless of how unimportant the minimum wage might become. As discussed thoroughly in Lemos (2003c), spike is superior to Kaitz index and fraction. That is because, on the one hand spike is conceptually related to fraction and is therefore methodologically clean; on the other hand spike does not suffer from the same drawback, as it can be defined even when the minimum wage is constant. Beyond statistical identification, an intuitive reason to use spike to measure the minimum wage impact on employment is that spike is a measure of those workers becoming more expensive; i.e., a measure of the extra employment costs. While spike was 4% for the US in 1993 (Dolado et al., 1996), it was 12% for Brazil, although as high as 25% in PE, a poor region. Its correlation with the real minimum wage in the sample period is 0.64.

Once regional variation has been ensured, no restriction needs to be placed on the time dummies. The typical annual data model in the literature includes year and regional dummies to model time and regional fixed effects (Brown, 1999). The monthly analogue of this model would require month in place of the year dummies. However, that would eliminate all the variation in the model because each dummy would capture all that affects employment in each month - including the discrete minimum wage increases. As a result, there would be no variation but noise left to identify the minimum wage effect (Burkhauser et al., 2000). If on the one hand month dummies eliminate all the variation, on the other hand year dummies alone are not sufficient to model time in a month model. An alternative is to include, in addition to year dummies, seasonal-month dummies to control for unobserved fixed effects across months, as in Burkhauser et al. (2000). Also,

stabilization plan dummies[3] are included to capture common macro shocks under each stabilization plan.[4]

4. DESCRIPTIVE MODELS

Changes in employment can be decomposed into changes in hours of work and changes in the number of jobs. Let average hours in the population ($\overline{T}$) be equal to the product of average hours for those working ($\overline{H}$) and the employment rate (E):

$$\overline{T} = \overline{H}E \text{ is } \frac{\sum_{i=1}^{N} hour_i}{N} = \frac{\sum_{i \in e}^{N} hour_i}{N_e} \frac{N_e}{N},$$

where N_e and N are sample sizes of the employed and labour force and *hour* is hours worked.

As noted by Brown et al. (1982, p. 497), "to measure the employment effect of the minimum wage, the ratio of employment to population is used most often as the dependent variable". However, the above decomposition suggests not only E, but also $\overline{T}$ and $\overline{H}$ as dependent variables; as a result, three specifications for the employment equation naturally arise. If a log-log or semi-log functional form is assumed, and the set of regressors is the same, the additivity property of OLS holds and the estimate in the $\overline{T}$ model equals the sum of estimates in the $\overline{H}$ and E models. Although this issue has not received much attention in the literature (Brown et al., 1982; Brown, 1999), more recent research (Michl, 2000; Zavodny, 2000; Card and Krueger, 2000; Neumark and Wascher, 2000) suggests that non-negative effects on jobs are sub-product of adjustments in hours. Zavodny (2000) and Machin et al. (2003) estimate job and hours effects, but do not formalize it as a decomposition.

Each of these three specifications was estimated for four alternative data filters: levels, first-difference, twelfth difference, and both first and twelfth differences. This is to account for Baker et al.'s (1999) criticism that negative or positive employment effects are found depending on whether short or long differencing is used.[5] For each of these filters, the following base model is estimated:

$$\log employment_{rt} = \alpha + \beta \log realMW_{rt} + \gamma \inf lation_{rt-1} + f_r + f_t + u_{rt},$$

where $employment_{rt}$ is taken in turn to mean E, $\overline{T}$ or $\overline{H}$;; f_r and f_t are regional and time fixed effects (Section 3.2), as discussed in Section 3.2; and u_{rt} is the error term. Past inflation, $\inf lation_{rt-1}$, was explicitly included because on the one hand, the macroeconomic policy, including the minimum wage policy, was aimed at stabilizing the inflation; thus, inflation is driving other variables. On the other hand, the minimum wage was used as indexer (Section 3.1); thus, past inflation captures the portion of the minimum wage increase that merely compensates for past inflation.

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[3] Each had very particular rules (Abreu, 1992); macro shocks were similar within, and different across plans. Additionally, a dummy was defined in October 1988, when the new Constitution: shortened the working week from 48 to 44 hours, and introduced an alternative working day of 6 consecutive hours.

[4] Wald and F tests were used to test whether spike had variation over and above the time dummies to explain employment. Both tests rejected the restricted model. This is reassuring that the variation captured by spike - further to that captured by the time dummies – is due to the minimum wage.

[5] Card and Krueger (1995) found positive results using one and two-year-differencing whereas Neumark and Wascher (1992) found negative results using long differencing. More technically, the aim is to reduce the variables to stationarity preventing spurious regression, which depends on the number of unit roots of the variables.

The standard neoclassical model underlies the above empirical equation. Assuming the production function depending on skilled and unskilled labour, with input and output prices W, MW, and p; maximization of profits at the (representative) firm level delivers the aggregate unconditional labour demand function $L^d=L(p,W,MW)$. As this is homogeneous of degree zero, all prices can be normalized by W, which is the reasoning for using Kaitz index in the literature (Card and Krueger, 1995), here replaced by spike (Section 3.2).[6] This is therefore the theoretical ground for modeling employment as a function of inflation and spike.

If the labour supply is perfectly elastic, the effect of the minimum wage on employment can be estimated using estimates of labour demand curve alone. If, however, labour supply is positively sloped, some sort of reduced form is what is being estimated, and supply shifters need to be included. Here, these are mainly population and institutional variables that control for region specific demographics potentially correlated with the minimum wage, the proportion of workers in the population who are: young, younger than 10 years old, women, illiterates, retired, students, in the informal sector, in urban areas, in the public sector, in the building construction industry sector, in the metallurgic industry sector, basic education degree holders, high school degree holders, and the proportion of workers with a second job.[7] Thus, the model was estimated with and without controls.

Dynamics, in the form of 24 lags of the dependent variable were also added because an increase in the minimum wage might not affect employment contemporaneously, but in future periods. This is because the inability to adjust other inputs instantaneously creates lagged responses in employment (Brown, 1982; 992; Hamermesh, 1995).[8]

By modeling regional and time fixed effects, including controls and dynamics, and differencing the data, the errors are no longer expected to be serially correlated; few authors worry about that (Brown, 1999).[9] This variety of specifications embraces the typical ones in the literature (Brown, 1999; Card and Krueger, 1995).[10]

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[6] In minimum wage studies, the interest is on the minimum wage bite (an how it varies across regions) rather than on the minimum wage itself - that is the intuition for normalizing the minimum wage by the average wage (Kaitz index). To measure the minimum wage bite, spike is just as good as any other empirical variable (Dickens at al., 1999; Williams, 1993). Moreover, assuming log wages normally distributed, no spillover effects, and a known cut off point (the minimum wage); spike summarizes all that there is to know about the employment effects of the minimum wage.

[7] There is some agreement that demand side variables should be held constant, but less agreement on whether supply side variables should be included as controls and, if so, which ones. The debate is about whether a reduced form or a demand equation is estimated, depending on whether the minimum wage is binding or not (Neumark and Wascher, 1992, 1995, 1996). For those who earn a minimum wage, employment is demand determined, but for those who earn more, relative supply and demand matter. Typically, employment equations in the literature have been interpreted as demand equations, even though many include supply side variables (Card and Krueger, 1995). Particularly debatable is the inclusion of a variable measuring enrolment rates in school (Card and Krueger, 1995; Neumark and Wascher, 1992). As claimed by Brown (1999), if minimum wage reduces both employment and enrolment, reduced form and enrolment rate constant employment equations have very different interpretations. In Brazil, a large number of minimum wage workers are adults no longer at school. Also, schooling is largely available outside working hours, and therefore working and schooling need not be exclusive alternatives; if present, the simultaneity bias will not be as severe. Due to these particularities and the unresolved debate, enrolment rate was not here included (Williams, 1993; Baker, 1999).

[8] Employment is reported to be AR(2) using annual data (Layard et al., 1991), which is equivalent to 24 lags on monthly data. Results were robust to including 12 lags only, but that was thought to prematurely censor the adjustment process because lags beyond 12 were still significant.

[9] The results were robust when re-estimating the models using Seemingly Unrelated Regression Estimation method.

[10] The models were White-corrected and sample size weighted, to correct for heteroskedasticity arising from the regional aggregation. Incidentally, weighting captures the relative importance of each region to the average coefficient if the sample size is proportional to the regional labour market (Card and Krueger 1995; Neumark and Wascher 1992).

Graph 2 plots log employment rate (E) against log real minimum wage. The suggested positive raw correlation in levels fades as the data is differenced; this offers no support for a negative effect of the minimum wage on employment - if anything, the correlation is weakly positive. Nonetheless, such raw correlations need to be proved robust when the effect of other variables (demand and supply shocks) on employment is controlled for. Graph 3 and corresponding panel 1 of Table A (in the appendix) show estimates for the models discussed above. In line with the plots, such estimates also give little support for a negative effect: they are mostly positive, statistically significant, and small. The spike coefficient for the total employment model ranges from –0.036 to 0.779, decomposed into (a) the hours coefficient ranging from 0.193 to 0.844 (darker bars); and (b) the jobs coefficient ranging from -0.232 to 0.104 (lighter bars). A 10% increase in the nominal minimum wage increases spike by 0.12 percentage points on average and is associated with a decrease in total employment of less than 0.01%. However, this is a correlation, once the model is purely descriptive; the next step is an attempt to estimate behavioural effects.

5. ROBUSTNESS CHECK

To summarize the identification discussion: (1) By using spike as a measure of the constant minimum wage, the effect of spike is not confounded with the effect of other regional macro variables on employment. (2) By accounting for regional fixed effects, the effect of spike is not confounded with the effect of unobserved regional macro fixed effects on employment. The last step is to control for simultaneity bias. (3) By correcting for simultaneity bias, the effect of spike is not confounded with the effect of unobserved regional macro variables on employment.

Even if the nominal minimum wage is assumed to be predetermined,[11] spike and employment are simultaneously determined. Once the minimum wage is increased, the relative wage bargains determine the workers' position in the wages distribution; this also determines who earns one minimum wage, i.e. who is at the spike. An exogenous or predetermined variable - that affects employment only via spike - was necessary to ensure identification. Lags of spike and political variables were proposed as such an instrumental variable.

Firstly, under the assumption of serially uncorrelated errors, lags of spike - naturally correlated with spike but uncorrelated with the error term - fulfill the properties of a valid instrument. Panel 2 of Table A (in the appendix) shows estimates, not always significant, of magnitude and signs not too different from the uninstrumented versions of Section 4. The total employment elasticity ranges from -0.327 to 0.956, decomposed into (a) hours elasticity, ranging from -0.023 to 0.975; and (b) jobs elasticity, ranging from -0.306 to 0.222. Other things constant, increasing the minimum wage by 10% (increases spike by 0.12 percentage points) decreases employment by 0.04% at the most.

5.1 SERIAL CORRELATION

Secondly, if serial correlation is relaxed, the structure of the errors is crucial in defining which - if any - lag of the endogenous variable can be used as a valid instrument. Assuming serial correlation due to mis-specified dependent variable dynamics, as its lags are included as regressors,

[11] The nominal minimum wage might be endogenous if its increases are related to regional macroeconomic performance (Card and Krueger, 1995; Dolado et al., 1996; William and Mills, 1998). Further endogeneity can be caused by the denominator of the real minimum wage, i.e. price or (average) wage deflators (Dolado et al, 1996; Zavodny, 2000). The most obvious instruments for spike are lagged real minimum wage and lagged Kaitz index along with lagged spike. However, (a) they do not ensure identification, as discussed in Section 3.2; and (b) they suffer from the same drawback as spike when serial correlation is relaxed (Section 5.1). Despite that, robustness checks using such instruments produced robust estimates.

serial correlation is expected to vanish. Furthermore, the Sargan test can be used as a model selection criteria, indicating which dynamics generate serially uncorrelated errors and validates lags (of the endogenous variable) as instruments (Andrews, 1999; Szroeter, 2000). Ultimately, an orthogonality condition must be made to produce an estimable equation and it is not too unrealistic to assume that serial correlation will vanish after differencing, adding dynamics, controls, regional and time dummies.

This was the presumption in Section 5. Panel 2 of Table B (in the appendix) shows the associated overidentifying restrictions (Sargan) test, Hausman test and F test (in the first step of the 2SLS) for the models in panel 2 of Table A. The Hausman test shows endogeneity, as anticipated in Section 5; the F test shows the instruments performed well; but the Sargan test fails even the dynamic models - this invalidates lags of spike as instruments. Only an excluded instrument with truly exogenous variation, uncorrelated with the error term and all its past lags, will ensure consistency. Political variables were used in an attempt to define such an instrument.

5.2 EXCLUDED EXOGENEOUS INSTRUMENTS

Three different sources of political variables were used as instruments. Appendix 1 and Table 1 give the institutional details underlying the validity of the instruments and their raw correlations.

Politicians Data - It is well established in the politics of the minimum wage literature that politicians might favour or oppose minimum wage increases depending on the overall macroeconomic performance in each region. Card and Krueger (1995, p. 134) argue, "Politicians from states in which an increase in the minimum wage is expected to have a strong effect on wages or employment opportunities might oppose the increase, whereas those from states in which the expected effect is smaller might support it." The final increase is the result of compromise between competing interest groups (regions) (Becker, 1983). Sobel (1999) argues that interest group pressure significantly influenced congressional voting on the passage of the minimum wage bills in the US. In other words, the final increase is a regional weighted average; the impact of the increase in each region determines the political support (the relative weight) of that region to the increase.

In Brazil, the Intersyndical Department of Parliamentary Consultancy (1) ranks the most influential congressmen according to political science criteria (debating, negotiating, voting, articulating, forming opinion, leading, etc.) rating their powers of persuasion; and (2) attributes marks to politicians voting in favour of workers in labour related bills. These are measures of regional weight and were here used as instruments. The more influential congressmen from a particular region, the more weight on the interests of that region; and the more pro-increase (contra-increase) these influential congressmen, the higher (lower) the minimum wage. First, the influential status is based on personal characteristics and there is no reason to believe they are endogenously determined with employment. Second, the pro-increase (pro-worker) status is acquired by consistently voting in favour of workers in workers related bills. Most of these bills are not directly related to employment, as for example: land reform, union leader tenure, president mandate length, etc. (see Appendix 1). Those bills not directly related to employment were used to re-construct the pro-worker status; therefore, this measure is not endogenously determined with employment (see Appendix 1).

Voting Data - Some might argue that voting data would measure the regional weight more directly associated with minimum wage increases. Card and Krueger (1995) used voting data to construct a measure of political support. Similar data, accounting for votes in favour and against a minimum wage bill, was collected for Brazil. Usually, pressure against the bill results in inflation erosion of the real minimum wage (Sobel, 1999). In Brazil, the centralized wage policy was

intended to be deflationary via under-indexation of the real minimum wage (Section 3.1). Opposing such a policy meant protecting the worker's living standard. Thus, the more congressmen against the increase, the more pressure for a bigger increase, and the higher the minimum wage (see Appendix 1).

Card and Krueger (1995, p. 135) used their political variable as a "proxy for otherwise unobservable factors in a state that might be related to the impact of the law", implicitly assuming a direct effect on employment over and above the indirect effect via the minimum wage. There is no reason to believe that at the time politicians are voting the bill this is having a simultaneous effect on employment in Brazil. (1) The minimum wage is more a political issue in Brazil - with huge repercussions for political stability - than it is in the US. The minimum wage is perceived as a source of political instability that affects the behaviour of voters and policymakers; it is, ultimately, a determinant of economic decisions (see Appendix 1). (2) The minimum wage is more related to the wage-price spiral than to employment. The wage-price spiral is a rapid phenomenon under high inflation. Firms anticipate the spiral and do not adjust employment to avoid incurring in adjustment costs (Section 3.1). Those who regard the potential correlation between political variables and employment as a source of endogeneity should note the robustness of the results across instruments. This suggests that any endogeneity is negligible in both spike and instruments; in presence of severe endogeneity, there is no reason why all instruments would produce bias in the same direction and magnitude. As an attempt to further measure the political bargaining process, data was collected on bills never submitted to voting, on the commissions formed to appreciate bills, and on the speeches of congressmen related to the bill (see Appendix 1).

An interesting feature of voting data is that voting can be non-secret (nominal), secret, or party oriented. During the dictatorship there was no voting, and when there was, it was symbolic - this is an exogenous instrument in itself. Parties orient the vote prior to voting; non-secret votes (only on demand) are usually a strategy of those opposing the increase (favouring a bigger increase) to expose their opponents (see Appendix 1). The lower the minimum wage, the more often non-secret votes are demanded. Block (1980 and 1989) and Card and Krueger (1995) discuss party influence on the passage of minimum wage bills in the US. Weighting the number and proportion of votes by the "voting dummy" generates an additional instrument. This places more weight on the more reliable non-secret votes data, which also represents more proactive pro-increase and democratic times. Incidentally, this interaction dilutes the potential endogeneity discussed above, as it introduces exogenous variation from the voting dummy.

Another way to measure the political bargaining process is to consider the frequency of increases. An increase occurred whenever the socio-economic-political tension became unbearable (81/217 months). The timing of the increases was regarded as a measure of tension and used to define a "voting cycle" variable. The more often bills are presented, the lower the minimum wage (the faster its inflation erosion). The voting cycle is assumed to be predetermined, as tension at each moment is a function of past information. Weighting the voting data by the voting cycle generates an additional political variable that measures regional political support over time. This places more weight on voting when it is most relevant (has just occurred).

Election Data - As a further attempt to collect data with independent variation consider political propaganda. Firstly, assume that incentives for more generous increases depend on the proximity of elections (Sobel, 1999). The basic assumption is that voters are myopic and opportunistic policymakers systematically manipulate macroeconomic policy right before elections to maximize their chances of re-election (Nordhaus, 1975; Lindbeck, 1976). Thus, the timing of elections was used to define an "election cycle" variable, as in political economy models (Carmignani, 2003). The closer the elections, the higher the minimum wage. The political cycle is an exogenous instrument,

as it is determined by regular intervals of time. Secondly, assume that left-wing politicians are in favour of more generous increases. The lower the minimum wage, the more popular discontentment, and the more left-wing politicians elected (see Appendix 1). Data on the number of (votes on) left wing politicians was used as an instrument. The underlying assumption is that any endogeneity coming from the simultaneous determination of the number of left wing politicians elected and employment is negligible on monthly data because elections only happen every 4 years. However, incentive for increases are bigger the more left wing politicians elected and the closer the elections; weighting the election data by the election cycle generates an additional political variable varying over time and across regions. Incidentally, this interaction dilutes any (already negligible) election data endogeneity, as it introduces exogenous variation from the election cycle. Thirdly, assume that incentives for increases are bigger the lower the minimum wage. Even if the proportion of left wing politicians is high and the next elections are close, not much political propaganda is made if the minimum wage is already at a relatively high level. Moreover, this additional political variable re-introduces the real minimum wage variation into the model (Card and Krueger, 1995; Machin and Manning, 1994).

The above instruments are strongly correlated with spike[12] (see Table 1) but not thought to be endogenously determined with employment. Furthermore, the Sargan test did not fail the specifications using such instruments (see Table B). This is supportive of the assumption that any correlation with past information is not too strong.

Some might argue that interactions "fake" the correlation with the endogenous variable and "create" a weak instrument; i.e. even if the instrument is uncorrelated with the endogenous variable in the population, correlation might not be zero in a finite sample (Nagar, 1959; Bound et al., 1995; Staiger and Stock, 1997). There is nothing intrinsic about interactions that produce nonzero correlations. In general, provided that there is some *a priori* economic reasoning in establishing the validity of the instruments (as exhaustively discussed in Appendix 1), and they pass the appropriate tests (see Table B), nothing particular about interactions invalidates instruments. The issue is about weak instruments, not interactions *per se* (Angrist and Krueger, 1995; Krueger et al., 1999). Interactions were here justified for a conceptual reason. Incidentally, they produce variation in both dimensions (over time and across regions) for instruments originally only varying in one dimension. In general, interactions did not produce stronger correlations; most of the above instruments are interaction-free, and yet correlated with spike (see bold in Table 1). Interactions were motivated as further robustness checks and were by no means crucial in defining the instruments.

These instruments were organized into four groups to account for potential criticisms on interaction, endogenous, and weak instrumenting contaminating the results: (a) only interaction-free instruments; (b) a subsample from the interaction-free instruments whose correlation with spike was higher than 0.30; (c) voting data interacted with voting cycle; (d) election data interacted with election cycle and real minimum wage. The full set of results is reported in Table A (in the appendix). The estimates are still clustered around zero but larger than before in absolute terms, suggesting that some bias was corrected. Estimates are both smaller and more significant when interaction-free instruments were used in panels 3 and 4; and larger, but not always significant, when interaction instruments were used in panels 5 and 6. Table 2 presents the interval that brackets the effect of a 10% increase in the minimum wage across specifications: the total employment elasticity ranges from –0.13% to 0.11%, decomposed into (a) hours elasticity, ranging from -0.07% to 0.20%; and (b) jobs elasticity, ranging from -0.10% to 0.09%. Holding other things constant, increasing the

[12] It was intuitively easier to discuss the sign of the correlation in relation to the minimum wage even though the above are instruments for spike. Both correlations should bare the same sign, because spike and minimum wage are positively correlated (see Section 3.1 and Appendix 1).

minimum wage by 10% (increases spike by 0.12 percentage points) decreases employment by 0.13% at the most. At a regional level, increasing the minimum wage by 10% increases spike by 0.19 (0.05) percentage points in PE (SP), a poor (rich) region, and decreases employment by 0.2% (0.05%) in PE (SP) at the most. In other words, it causes four times more disemployment in PE than it does in SP. Finally, the last two columns of Table A also show a less than 0.03% employment decrease in the long run.

The range of estimates produced is expected to embrace the true coefficient. The preferred specification is the one in first differences, instrumented with interaction-free political variables - i.e., column 3, row 2, panel 3 of Table A. This specification is expected to have the least serial correlation and use the less debatable set of political instruments. It also performs better in the overall tests: the Hausman test suggests endogeneity, but the Sargan test did not fail, and the F test showed the high explanatory power of the instruments - which is reassuring of the validity of the instruments. Thus, this specification is more reliable both conceptually and statistically; it is also more comparable with specifications in the existing literature, mostly in first differences. Incidentally this "preferred" specification produces estimates fairly similar to the other specifications.

Bracketing the employment elasticity below 0.13% across such a variety of models is reassuring; this number goes down to 0.04% in the preferred specification. These results were remarkably robust to changes in the specification and to various alternative instruments. They are also in line with the international and Brazilian literature. Furthermore, the results are in line with prior expectations discussed in the Introduction and in Section 3.1. Regarding the above as demand equations, the results are consistent with a fairly inelastic demand curve: minimum wage increases translate into small employment losses (Freeman, 1995). Barros et al. (2002) also estimated a fairly inelastic labour demand curve for the industry sector in Brazil.

6. FURTHER ROBUSTNESS CHECKS

6.1 FORMAL AND INFORMAL SECTORS

The standard Welch-Gramlich-Mincer Two Sector Model major prediction is that the uncovered sector wages fall as a result of covered sector displaced workers moving into uncovered sector employment. It follows that, in the uncovered sector, a spike should not be observed in the wage distribution and the labour demand curve should not be downwards sloping (Welch, 1976; Gramlich, 1976; Mincer, 1976). If additionally labour supply is assumed inelastic, the uncovered sector employment increase is just enough to off set the covered sector employment decrease ($\beta_F = -\beta_I$) and the net (full sample) employment effect is zero. This might offer an explanation for the clustered-around-zero employment effect found in the literature (Brown, 1999). It is therefore important to investigate the covered and uncovered sectors coefficients underlying the net coefficient - especially if the uncovered sector is large.

The predictions of the Two Sectors Model follow from the assumption of non-coverage. The Brazilian informal market suffers from non-compliance not non-coverage. That is the key difference between the US and Brazilian literature on uncovered and informal sector minimum wage effects. Informal sector wages and employment need not respond to an increase in the minimum wage in the same way uncovered sector wages and employment respond. First, a large spike is observed in the wages distribution of both sectors for Brazil (Lemos, 2003c; Maloney and Nunes, 2003; Neri, 1997; Foguel, 1997) and for other Latin America countries (Maloney and Nunes, 2003). Card and Krueger (1995) and Brown (1999) also document a spike in the uncovered sector wage distribution for the US. The same effective pay (one minimum wage) is paid in both sectors; non-compliance is

observed either as below minimum wage workers or on other aspects of the labour contract, such as social security taxes, paid holidays, health insurance, etc. (Amadeo et al., 1995). Maloney (1999) argues that the informal sector is a way of avoiding the inefficiencies of labour market regulation. Second, spillover effects are also observed in both sectors for Brazil (Lemos, 2003c; Maloney and Nunes, 2003; Neri, 1997; Foguel, 1997; Neri et al., 1999; Carneiro, 2000) and for other Latin America countries (Maloney and Nunes, 2003).

The presence of a spike and spillover effects in both sectors suggest employment decreases in both sectors. Maloney and Nunes (2003) question the validity of the standard Two Sector Model to explain the formal and informal sector in Latin America. Mincer (1976) notes that the prediction of uncovered sector wages fall is not robust to alternative assumptions on sectoral choice and unemployment. Card and Krueger (1995) show that the uncovered sector wages rise (and employment falls) if the covered sector labour demand curve is relatively inelastic. The labour demand curve for the industry (mainly formal sector firms) in Brazil is fairly inelastic (Barros et al., 2002). It is, however, important to investigate the formal and informal sectors coefficients underlying the net coefficient in Brazil – especially because the informal sector is large and overpopulated by minimum wage workers.

Panel 1 of Table 3 presents estimates of the employment effect by sector using the preferred employment specification (Section 5.2), here uninstrumented. The pattern of signs, significance, and magnitudes are remarkably similar in both sectors.[13] The null hypothesis of identical employment effects, $\beta = \beta_F = \beta_I$, could not be rejected. (This result is robust across the specifications discussed in Section 4.) The estimates for the formal and informal sectors therefore do not change the previous conclusions. As discussed above, negative employment (rate) effects in both sectors and positive unemployment effects (Table 3) are consistent with the presence of a spike and spillover effects in both sectors; these results are also in line with previous findings for Brazil (Foguel, 1997; Neri, 1997; Neri et al., 1999).

6.2 PRIVATE AND PUBLIC SECTORS

The employment effects predicted by the neoclassical model relies on a profit maximizing firm, not on a government employer that can cover the higher wage bill by raising taxes, reducing expenditure, or simply printing money, as in Brazil (see Section 3.1). This is not to claim there will be no adverse employment effect in the public sector. Even though they are not predicted by a specific theory, Hamermesh (1993) notes that institutional differences in developing countries do not require changes in the basic theory of labour demand. However, because evidence regarding the private sector need not carry over to the public sector, the same sort of robustness check for the formal and informal sectors in Section 6.1 is performed for the private and public sectors. Investigating the public sector employment effects is particularly relevant if the public sector, as in Brazil, is overpopulated by minimum wage workers and has no negligible spillover effects (Lemos, 2003c).

[13] As discussed in Section 4, the semi-log specification guarantees that . As a result, log of full sample employment rate (hours worked) no longer equals the sum of the log of formal and informal sectors employment rate (hours worked), and therefore no longer holds. This is a technical issue with no further implications; the functional form does not change the estimates magnitudes significantly, and it surely does not change their signs. Also note that, in line with previous work for Brazil (Menezes-Filho et al., 2002; Tannuri-Pianto and Pianto, 2002; Carneiro, 2002), the self-employed were dropped because the design of the survey does not allow their classification into salaried formal or informal sector workers.

Panel 2 of Table 3 presents the estimates. Once again the pattern of signs and magnitudes are similar in both sectors. The null hypothesis of identical employment effects could not be rejected. (This result is robust across the specifications discussed in Section 4, even though, in a few cases, this might be due to large standard errors). Nevertheless, once again, the estimates for the private and public sectors are not sufficiently strong to change the previous conclusions.

7. CONCLUSION

The international literature on minimum wage is scanty on non-US empirical evidence, in particular on developing countries evidence. Using Brazilian data is an unbiased way of extending the understanding of minimum wage effects and assessing the robustness of findings for the US. This paper estimates the minimum wage effects on wages and employment using Brazilian household data for the 80's and 90's recently released for the public and not yet used for studies of the minimum wage. Brazil's minimum wage policy is a distinctive and central feature of the Brazilian economy. Not only are increases in the minimum wage large and frequent, but also the minimum wage has been used as anti-inflation policy in addition to its social role. It affects employment directly and indirectly, through wages, pensions, benefits, inflation, the informal sector, and the public deficit. This confirms the importance of studying the minimum wage in Brazil.

The international literature on minimum wage is scanty on non-US empirical evidence, in particular on developing countries evidence. Using Brazilian data is an unbiased way of extending the understanding of minimum wage effects and assessing the robustness of findings for the US. This paper estimates the minimum wage effects on employment using Brazilian household data for the 80's and 90's recently released for public use and not yet used for studies of the minimum wage. Brazil's minimum wage policy is a distinctive and central feature of the Brazilian economy. Not only are increases in the minimum wage large and frequent, but also the minimum wage has been used as anti-inflation policy in addition to its social role. It affects employment directly and indirectly, through wages, pensions, benefits, inflation, the informal sector, and the public deficit.

Evidence of a moderately small adverse effect was uncovered. An increase of 10% in the minimum wage was found to decrease employment by 1% at the most. At a regional level, it was found to decrease employment by 0.2% (0.05%) at the most in PE (SP), a poor (rich) region, causing four times as much disemployment in PE than in SP. This result was shown to be robust to many alternative specifications, estimation techniques, and instruments. In presence of errors serially correlated, lagged endogenous variable was not a valid instrument. A number of political variables were used instead as exogenous excluded instruments uncorrelated with the error term and all its past history.

The above result is in line with the international and Brazilian empirical literature. It is also consistent with a fairly inelastic demand curve where minimum wage increases translate into small employment losses. To test further whether the minimum wage does not destroy too many jobs, this total effect was decomposed into hours and jobs effects. This is because the non-negative effects on jobs have been suggested to be a sub-product of adjustments in hours in the recent debate in the literature. Indeed, the total employment appears to be dominated by the hours rather than the jobs effects. This suggests that the minimum wage does not hurt as much where it hurts the most: causing disemployment.

Another tentative explanation to the non-negative employment effect found in the literature is that the underlying covered and uncovered sectors effects, if opposite-signed as predicted by the standard Two Sectors Model, might drive the net employment effect to be close to zero. The formal and informal sectors effects were both found to be negative, consistent with the presence of a large

spike and substantial spillover effects in both sectors. This suggests a downwards sloping labour demand curve in both sectors, challenging the standard Two Sectors Model as inadequate to explain the effect of the minimum wage on the formal and informal sectors in Brazil and in Latin America more generally.

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APPENDIX A – DEFINING EXCLUDED EXOGENEOUS INSTRUMENTS

A.1 Politicians Data

The intuition for the "degree of impact" variables discussed in Section 3.2 is that a national minimum wage increase affects a different proportion of people across regions depending on the overall macroeconomic performance in each region. Similarly, the intuition for the political variable instrument is that the underlying political bargaining process for the increase implicitly accounts for the overall macroeconomic performance in each region. Card and Krueger (1995, p. 134) argue, "Politicians from states in which an increase in the minimum wage is expected to have a strong effect on wages or employment opportunities might oppose the increase, whereas those from states in which the expected effect is smaller might support it." In Brazil, not only the direct, but also the indirect effect on employment via wages, pensions, benefits, inflation, informal sector, and the public deficit is on politicians' minds. The final increase is a regional weighted average. Becker (1983) argues that policy is the result of compromise between competing interest groups (regions); what matters is their relative strength. The impact of the increase in each region determines the political support (the relative weight) of that region to the increase.

In Brazil, once an year, the Intersyndical Department of Parliamentary Consultancy (DIAP) ranks the 100 most influential congressmen in the country according to political science criteria (debating, negotiating, voting, articulating, forming opinion, leading, etc.) rating their powers of persuasion (DIAP, 2001). These are personal characteristics and there is no reason to believe they are endogenously determined with employment. The more such congressmen from a particular region, the more weight on the interests (group interests) of that region. Sobel (1999) argues that interest group pressure significantly influenced congressional voting on the passage of the minimum wage bills in the US. DIAP's rank is then a measure of regional weight and was here defined as an instrument (IV1). The more pro-increase (contra-increase) influential congressmen, the higher (lower) the minimum wage. This can drive the correlation with real minimum wage to be either positive or negative, depending on the socio-economic-political context. However, Table 1 shows a strong positive correlation (0.62), which suggests a fairly stable correlation over the sample period. DIAP's rank was also re-defined as a proportion of congressmen from the sampled regions, which did not change the sign of the correlations (IV2).

Once a year, DIAP also attributes marks to politicians for each vote favouring workers in workers related bills. The pro-increase (pro-worker) status is acquired by consistently voting in favour of workers in labour related bills. Most of these bills are not directly related to employment,

as for example: land reform; union leader tenure; president mandate length; subsoil nationalization; unfair dismissal; wage, pension and benefits increases; petroleum state monopoly; work week shortening; 30 days minimum notice; centralized union; conditions of retirement; paid holidays; maternity leave; workers representative to company management; progressive tax bands; central bank independence; direct presidential elections; striking rights; civil servants tenure; political administrative reform; voluntary union contribution; president re-election; redundancy and dismissals legal process subject to union check; high education tuition free; public health insurance, etc.[14] A number of these are clearly not endogenously determined with employment; this should dilute the endogeneity of those which are. The pro-worker status was re-defined using solely those bills not simultaneously determined with employment but was not sensitive to that, suggesting that a considerable part of the variation is exogenous. The average mark for each region is a measure of how pro-workers (pro-increase) that region is (IV3); the higher the mark, the higher the minimum wage. Table 1 confirms a strong positive correlation with the real minimum wage (0.38), but not with spike. Dummies were also defined for whether these politicians are left or right wing, whether or not they hold a degree, and the number of mandates they hold (IV4 to IV6), which were then interacted (IV7).

A.2 Voting Data

Some might argue that voting data would measure the regional weight more directly associated with minimum wage increases. Card and Krueger (1995) used voting data to construct a measure of political support. Similar data was collected for Brazil from the National Congress Daily (Diario do Congresso Nacional, DCN). The number of congressmen votes in favour (IV8 and IV9), against (IV10 and IV11) and absent (IV12 and IV13) in each minimum wage bill during the sample period were collected in both the Federal Senate and the Deputy Chamber. Usually, pressure against the bill (pressure for no increase or a smaller increase) results in inflation erosion of the real minimum wage (Sobel, 1999). In Brazil, there are two distinct reasons to oppose the increase. In line with the above, pressure against the increase means that the increase cannot be afforded; this argument is usually related to the inflation impact or public deficit impact of the increase (Section 3.1). In contrast, pressure against the increase means that the increase is not large enough to even maintain the minimum wage purchase power; this argument is usually related to protecting the worker's standard of living. Examples of both arguments can be found in the newspapers:

> "The Government makes the minimum wage increase conditional upon the inflation level, the benefits and pension bill, the Estates and Cities finances… Most Congressmen know that a big increase would put at risk the economic stability of the country." (Estadao, 15th January 1998).
>
> "The minimum wage increase affected inflation… but the time is long gone when the increase would spread through the whole Economy, like the petrol increase" (Estadao, 13th May, 2000).
>
> "…congressmen are worried about finding the resources to afford the bill at the federal level, but have forgotten the municipal level… increasing the minimum wage to R$200 would increase the wage bill of most small and medium towns of the Northeast by 7.8%, where 60% to 70% of civil servants receive one minimum wage… if they do not find resources at a municipal level, mayors will have to fire civil servants" (Estadao, 11th December, 2001).
>
> "…to buy the same basket as in 1940, when it was introduced, the minimum wage would have to be R$517.55 [as opposed to the current R$130]" (Estadao, 10th May, 1998).

The underlying reason for being against the increase will depend on the political and economic context, party affiliation and workers' bargaining power, which naturally vary over time. As discussed in Section 3.1, the centralized wage policy was designed to control inflation and the public

[14] For a full list, see DIAP (1986, 1990, 1994, 2002). These publications are not part of a series; they have slightly different methodologies that required some adjustment. But the main idea is the same – grading politicians on how worker sympathetic they are.

deficit via under-indexation or the real minimum wage; Graph 1 shows supporting evidence of the resulting steady decrease. It is hard to think that being against these increases is being in favour of even smaller increases; it is more plausible to think that opposing such a policy meant favouring protection of the worker's living standard. Thus, the more congressmen against the increase, the more pressure for a bigger increase, and the higher the minimum wage. Absence (not justifiable absence through sickness, official mission, etc.) is also important because it might be a strategy against the passage of the bill (see newspaper citations below). Table 1 shows strong negative correlations, although a positive sign was expected for IV10 and IV11. This is either because of the underlying reason for opposing the increase, which can effectively drive the correlation to be negative or positive; or because of the definition of the instrument as "number" rather than "proportion". The number of congressmen both in favour and against the increase can move in the same direction as the minimum wage, but the proportions are not expected to. Thus, proportions were defined (IV14 to IV17), but although the correlation is stronger (ranging from 0.19 to 0.60), the sign did not change. Although IV15 and IV16 bare a negative sign, the correlations are robust across definitions and variables - they cannot have happened by chance alone. Most importantly, there is plausible economic reasoning for either a positive or a negative correlation. Provided the correlation is reliable - nonzero and stable over time - it suffices to establish a robust correlation.

Card and Krueger (1995, p. 135) used their political variable as a "proxy for otherwise unobservable factors in a state that might be related to the impact of the law", implicitly assuming a direct effect on employment over and above the indirect effect via the minimum wage. There is no reason to believe that at the time politicians are voting the bill this is having a simultaneous effect on employment in Brazil. (1) The minimum wage is more a political issue in Brazil - with huge repercussions for political stability - than it is in the US. The minimum wage is perceived as a source of political instability that affects the behaviour of voters and policymakers; it is, ultimately, a determinant of economic decisions (see Appendix 1). (2) The minimum wage is more related to the wage-price spiral than to employment. The wage-price spiral is a rapid phenomenon under high inflation, when firms are more able to adjust their prices. They anticipate the wage-price spiral and do not adjust employment to avoid incurring in adjustment costs (Section 3.1). Those who regard the potential correlation between political variables and employment as a source of endogeneity should note the robustness of the results across instruments. This suggests that any endogeneity is negligible in both spike and instruments; in presence of severe endogeneity, there is no reason why all instruments would produce bias in the same direction and magnitude.

An interesting feature of voting data is that voting can be non-secret (nominal), secret, or party oriented. During the dictatorship there was no voting, and when there was, it was symbolic - this is an exogenous instrument in itself. Parties orient the vote prior to voting; non-secret votes (only on demand) are usually a strategy of those opposing the increase (favouring a bigger increase) to expose their opponents. For example:

> "The popular movement against the minimum wage of R$151 toughens up in Brasilia on Easter, when a circus tent will be installed in front of the Congress to shelter 1,000 retired workers who will camp there until voting on the bill on the 26th. The vigil will include a mass for the "conversion" of deputies and senators in favour of a more generous minimum wage… The organizers of the movement [the Labour Party] want to install panels with the names of the Congressmen and their intentions of votes in Rio and Sao Paulo… as well as large screens for the people to watch the voting live" (Estadao, 19th April, 2000).
>
> "In a convoluted session stretching until early morning, the Government got the Congress to approve the R$151 minimum wage… after 3 months of fighting and thanks to a full day of intense lobbying. The session, due to start at 7pm, was postponed to 8pm, to prevent voting going live on television, exposing the 'situation' Congressmen [those in favour of the R$151 Government proposal] …who did not succeed in making a deal for symbolic voting, which guarantees the anonymity of votes. The opposition Congressmen… insisted on nominal voting". (Estadao,

11th May 2000). "By determination of the president… the general secretary will list the names of the Congressmen who will be punished for voting against the Government." (Estadao, 12th May 2000).

"The increasing tension between allies and adversaries of the Government because of the difficulties in finding a solution to the minimum wage increase might stop the voting… the leader of the Labour Party… announced yesterday that his party will be absent" (Estadao, 9th November, 2000).

The lower the minimum wage, the more pressure for a bigger increase, and the more often non-secret votes are demanded. Thus, a dummy (3 for non-secret, 2 for party oriented and 1 for secret/symbolic vote) was defined for the Federal Senate and Deputy Chamber (IV18 and IV19) to account for data reliability, pressure strategies, and democracy level. Table 1 shows strong correlations (0.49 and 0.59) and the expected negative sign. Block (1980 and 1989) and Card and Krueger (1995) discuss party influence on the passage of minimum wage bills in the US. Weighting the number and proportion of votes by the "voting dummy" is a natural step (IV20 to IV29). This places more weight on the more reliable non-secret votes data, which also represents more proactive pro-increase and democratic times. Table 1 shows strong negative correlations. Incidentally, this interaction dilutes the potential endogeneity discussed above, as it introduces exogenous variation from the voting dummy.

Another way to measure the political bargaining process is to consider the frequency of increases. An increase occurred whenever the socio-economic-political tension became unbearable (81/217 months); tension in the month immediately after the increase is low, reaching its peak the month before the next increase. The timing of the increases was regarded as a measure of tension and used to define a "voting cycle" variable (IV30).[15] The more often bills are presented, the lower the minimum wage (the faster its inflation erosion). Table 1 shows a strong correlation (0.29) and the expected positive sign. The voting cycle is assumed to be predetermined, as tension at each moment is a function of past information.

Weighting the voting data by the voting cycle generates a political variable that measures regional political support over time (IV35 to IV50). This places more weight on voting when it is most relevant (has just occurred), and less weight when the tension is such that new voting is imminent. Weighting is also expected to improve the instruments performance - it produces variation across regions and over time - although Table 1 shows the correlations to be again strong and negative, but not stronger.

As an attempt to further measure the political bargaining process, data was collected on bills never submitted to voting, on the commissions formed to appreciate bills, and on the speeches of congressmen related to the bill. Data on bills submitted by congressmen from the sampled regions to the Federal Senate and the Deputy Chamber was collected: the number of minimum wage bills presented (IV51), by left-wing[16] congressmen (IV52), and the number of minimum wage increase bills (IV53). The more bills presented, the lower the minimum wage. Table 1 shows strong negative correlations, as expected. For the same reasons as before, the last two instruments were defined as proportions of the first one (IV54 and IV55), which again did not change the sign of the correlation. Also, a dummy was defined for whether the bill was effective (never voted) (IV56). The more effective the bills, the higher (less inflation eroded) the minimum wage. Table 1 confirms the expected positive correlation. Two more variables were defined to measure the length of the passage of the bill: sum of days (if more than one bill per month) (IV57); and average days (IV58). The longer the passage, the more pressure (the less bargaining power), the higher (lower) the minimum

[15] Tension can only be measured when it reaches its peak triggering an increase. Assuming that tension grows linearly, the voting cycle was defined as a linear time trend between each of the two increases. Other functional forms (exponential, squared, squared root and log) were also experimented (IV31 to IV34).

[16] Left wing designation according to Figueiredo and Linomgi (1995).

wage. The correlation sign will depend on the underlying reason for the delay, but it should be the same for both spike and real minimum wage. Table 1 shows that the signs differ. This is either because of no genuine correlation (correlations are indeed low) or because of measurement error. Regarding the latter, this data was collected from the National Congress System Information (SICON) web page, and (a subsample) checked against data from the Section for Parliamentary Information (SEDOP). The data is assumed to be reliable and measurement error negligible (IV51 to IV53 show strong correlations). Also, the number of bills was weighted by the "effectiveness" (IV59) and "length" dummies (IV60 and IV61).

Data was also collected on the number of speeches by congressmen from the sampled regions in both the Federal Senate and the Deputy Chamber regarding the minimum wage (IV62), by left-wing congressmen (IV63), regarding a minimum wage increase (IV64), in favour (IV65) and against the increase (IV66). The more speeches needed, the lower the minimum wage. Table 1 confirms the expected negative correlations. This data was collected from the Shorthand Notes from the National Congress Sessions (and associated DCN); it is assumed to be reliable and the measurement error negligible for the first three, but not for the last two instruments. This is because the last two are subject to interpretation, aggravated by the complex socio-economic-political Brazilian context and by the number of variables affected by the minimum wage. Once more the last four instruments were defined as proportions of the first (IV67-IV70), and once more the sign of the correlation remained unchanged.

For most of the bills submitted, a commission would appreciate the impact of the increase prior to voting. Data on the (total, left and right-wing) number of congressmen from the sampled regions in each commission was collected (IV71 to IV73). The more congressmen in favour of the increase, the lower the minimum wage, as before. Table 1 shows negative correlations, even though a positive sign was expected for IV73, as before. Once more, proportions were defined (IV74 to IV77), which did not change the sign of the correlations with real minimum wage, but turned into positive the correlations with spike. As before, this is either because of no genuine correlation (correlations are indeed low) or because of measurement error. Regarding the latter, this data was collected from the SICON, and checked against the SEDOP, as before, and is assumed to be reliable. However, measurement error is not assumed to be negligible, because of the nature of the data (there was not always a commission, not always a minimum wage one, etc.). Even though these instruments were thought to capture the true underlying political bargaining process, not much confidence should be placed in them.

A.3 Election Data

However, regional affordability is not the only criteria for political support. As an attempt to collect data with independent variation to further test the robustness of the estimates, consider political propaganda:

> "…around 500 mayors will meet in Brasilia to discuss a strategy to pressure the Congress against … the minimum wage increase... [they] changed their strategy of pressure... mainly due to the proximity of the election campaign for the re-election of the congressmen, who dispute the support of the mayors in their electoral basis." (Estadao, 11th December 2001).

> "Usually, the minimum wage increase is defined… in December, but this year the elections anticipated the debate… the Government strategy is to postpone the increase above inflation until after October, when the new president will have been elected." (Estadao, 10th July 2002).

Firstly, assume that incentives for more generous increases depend on the proximity of elections. Sobel (1999, p. 766) specified a model that "shows an incentive for Congress to time changes in the minimum wage just before elections". He argues that this was the case over the entire

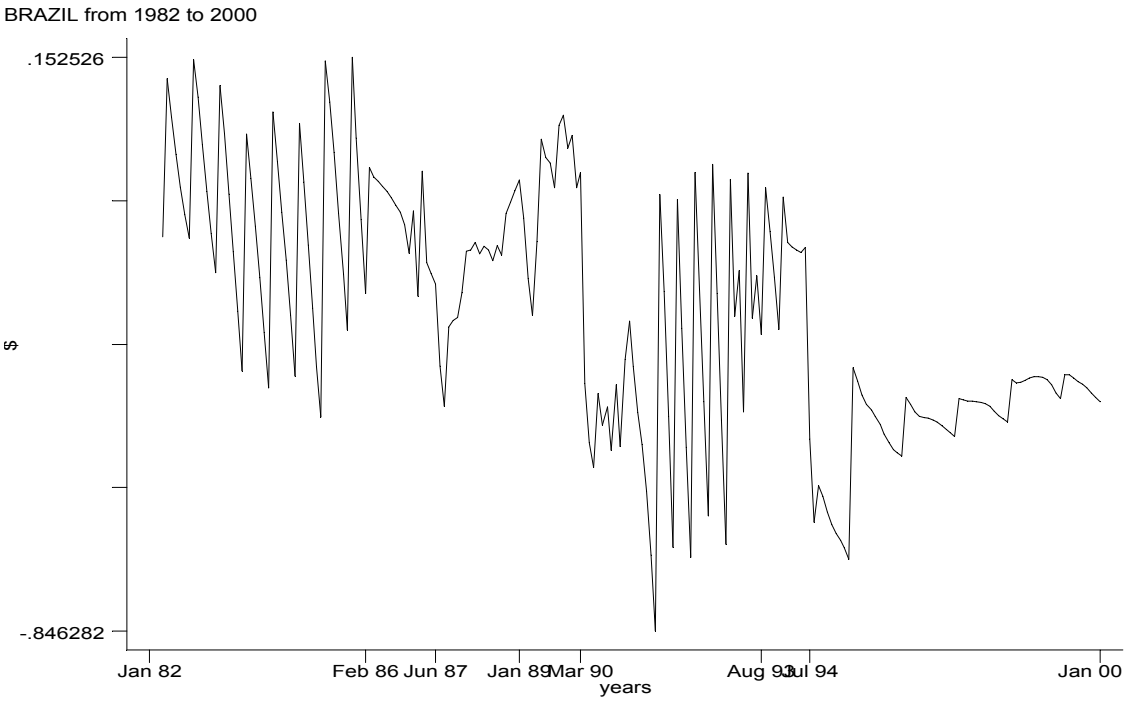
history of the minimum wage, starting with the Fair Labor Standards Act going into effect just eight days before election. Similarly, in Brazil, the Consolidacao das Leis do Trabalho introduced the minimum wage - on the 1st May 1943 - as a prelude to amending the Constitution to introduce presidential elections. In every single electoral year in the sample period there was a minimum wage increase - mostly either in the same month, or a couple of months before the election. This is reassuring evidence that the minimum wage is used as political propaganda. The basic assumption is that voters are myopic and opportunistic policymakers systematically manipulate macroeconomic policy right before elections to maximize their chances of re-election (Nordhaus, 1975; Lindbeck, 1976). Thus, the timing of elections was used to define an "election cycle" variable (IV78 for national, and IV79 for municipal elections)[17], as in to political economy models (Carmignani, 2003). The closer the elections, the higher the minimum wage. Table 1 confirms the expected negative correlations (0.34 and 0.04). The political cycle is assumed to be exogenous, as it is determined by regular intervals of time.

Secondly, assume that left-wing politicians are in favour of more generous increases. The lower the minimum wage, the more popular discontentment, and the more left-wing politicians elected. Data was collected (Nicolau, 1998) on the number (proportion) of left-wing candidates elected as president, federal deputy, governor, estate deputy, senator, and capital mayor (IV88 to IV99). Although not included in their final version, Baker et al. (1999) used a dummy for whether left wing politicians were in power as an instrument, a common procedure in political economy models (Carmignani, 2003). Similarly, data was collected on the number (proportion) of votes for left-wing candidates (IV100 to IV107). Table 1 shows strong negative correlations (0.11 to 0.69), as expected, stronger for proportions. The underlying assumption is that any endogeneity coming from the simultaneous determination of the number of (votes on) left wing politicians and employment is negligible on monthly data because elections only happen every 4 years. However, incentive for increases are bigger the more left wing politicians elected and the closer the elections; weighting the election data by the election cycle (IV108 to IV127) is expected to improve the instruments performance - it produces variation across regions and over time – although Table 1 shows correlations negative and strong, but not stronger. Incidentally, this interaction dilutes any (already negligible) potential endogeneity discussed above, as it introduces exogenous variation from the election cycle.

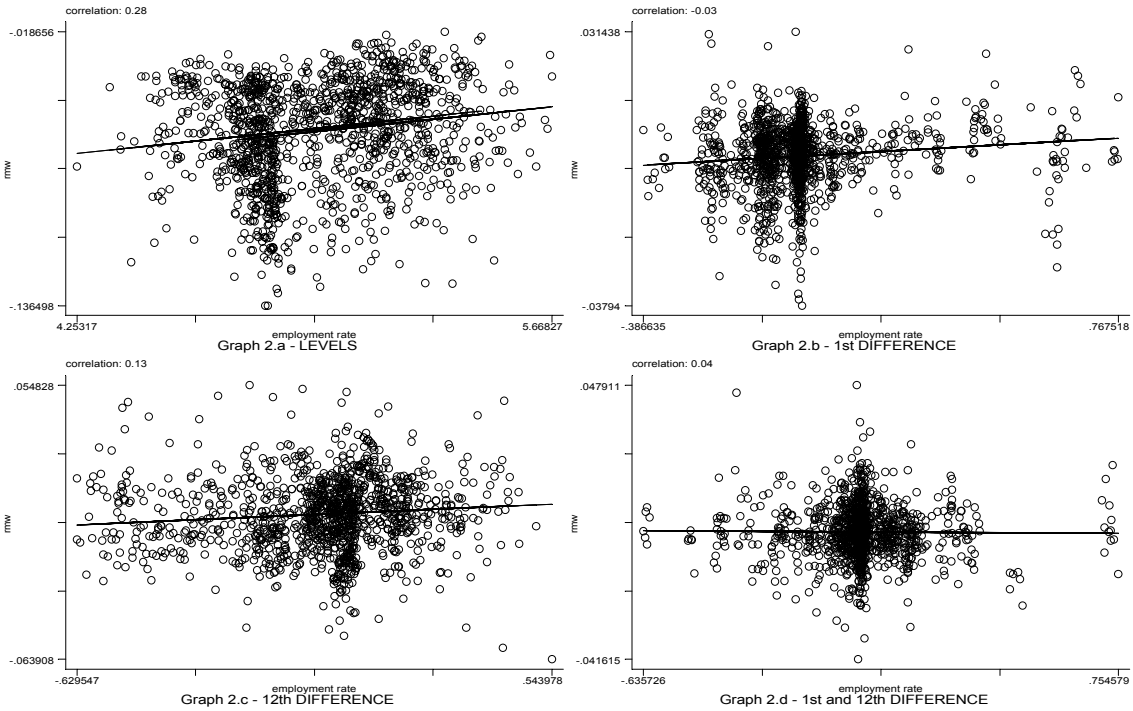
Thirdly, assume that incentives for increases are bigger the lower the minimum wage. Even if the proportion of left wing politicians is high and the next elections are close, not much political propaganda is made if the minimum wage is already at a relatively high level. Moreover, this re-introduces the real minimum wage variation into the model (Card and Krueger, 1995; Machin and Manning, 1994). Incidentally, Table 1 shows that this improves the correlations (IV128 to IV147).

The above instruments are strongly correlated with spike (see Table 1) but not thought to be endogenously determined with employment. Furthermore, the Sargan test - here regarded as a serial correlation test - did not fail the specifications using such instruments (see Table B). This is supportive of the assumption that any correlation with past information is not too strong.

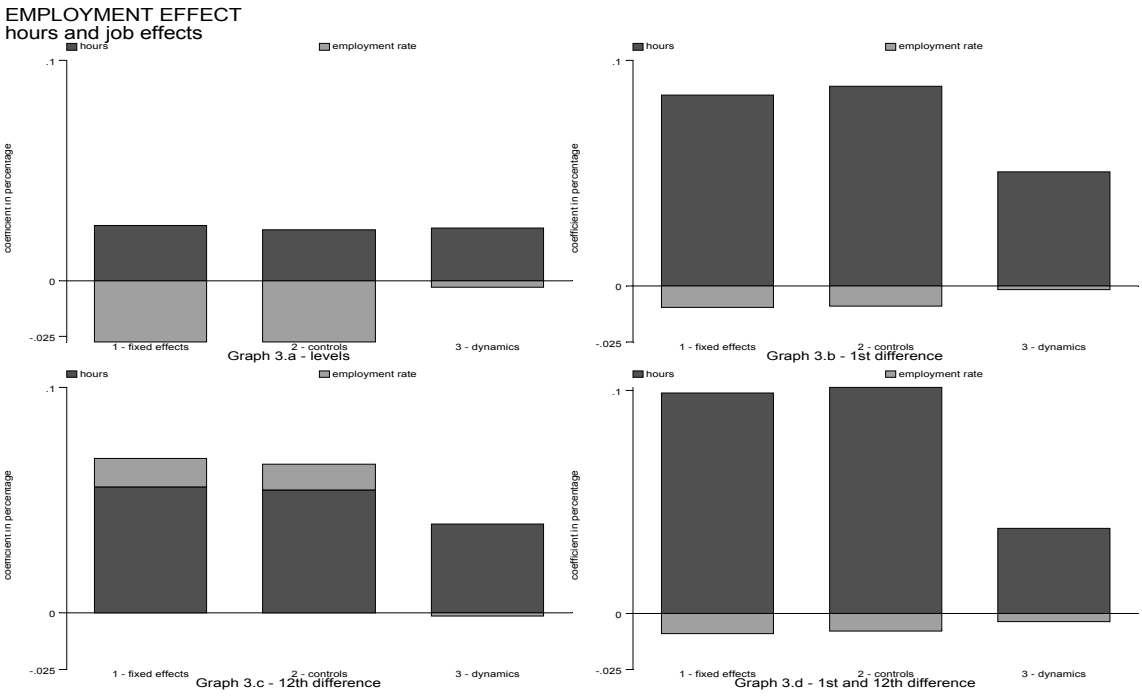
[17] Like the voting cycle, the political cycle is a linear (exponential, squared, squared root and log) time trend between two consecutive elections (IV80 to IV87).



Graph 1 - LOG REAL HOURLY MINIMUM WAGE



Graph 2 - LOG EMPLOYMENT RATE AND LOG REAL HOURLY MINIMUM WAG



ph 3 - EFFECT OF A 10% INCREASE IN THE MINIMUM WAGE ON EMPLOYME

Table 1 - CORRELATIONS BETWEEN THE MINIMUM WAGE, SPIKE AND POLITICAL INSTRUMENTS - continues

IV	spike	real MW	instrument (vary across regions and over time)	intuition
I - POLITICIANS DATA				
IV1	0.21	0.62	nb (out of 100) of most influential politicians in the country	the more influential the congressmen and the more pro-increase, the higher the mw
IV2	0.28	0.16	IV2 as a proportion of total influential politicians in the sampled regions	as above
IV3	0.01	0.38	measure of how worker-sympathetic congressmen are	the higher the mark, the more pro-increase, the higher the mw
IV4	-0.56	-0.51	dummy: 1 if left wing, 0 otherwise for congressmen in IV3 (average)	the more left wing, the higher the mw
IV5	0.16	0.04	dummy: 1 if university graduated, 0 otherwise for congressman in IV3 (average)	the more education, the bigger the support for a higher mw
IV6	-0.25	-0.54	nb of mandates for congressman in IV3 (average)	the longer in power, the less favourable of a higher mw
IV7	-0.42	-0.35	IV3*IV4*IV5*IV6	
II - VOTING DATA				
IV8	-0.44	-0.59	nb of senator votes in favour of the mw increase	the more congressmen in favour of the increase (as opposed to a bigger increase), the lower the mw
IV9	-0.56	-0.48	nb of deputy votes in favour of the mw increase	as above
IV10	-0.20	-0.17	nb of senator votes against the mw increase	the more congressmen against the increase (as opposed to an increase), the lower the mw
IV11	-0.50	-0.49	nb of deputy votes against the mw increase	as above
IV12	-0.07	-0.05	nb of senators absent when the mw increase was voted	the more congressmen absent (the less pressure for a bigger increase), the lower the mw
IV13	-0.13	-0.35	nb of deputys absent when the mw increase was voted	as above
IV14	-0.51	-0.60	IV8 as a proportion of total senator votes	as above
IV15	-0.42	-0.36	IV9 as a proportion of total deputy votes	as above
IV16	-0.20	-0.20	IV10 as a proportion of total senator votes	as above
IV17	-0.47	-0.60	IV11 as a proportion of total deputy votes	as above
IV18	-0.47	-0.49	dummy for senator vote: 3 non-secret, 1 secret, and 2 party oriented vote	the more non-secret votes, the lower the mw (non-secret votes expose those against it)
IV19	-0.59	-0.60	dummy for deputy vote: 3 non-secret, 1 secret, and 2 party oriented vote	as above
IV20	-0.38	-0.49	IV8*IV18	
IV21	-0.54	-0.47	IV9*IV19	
IV22	-0.19	-0.15	IV10*IV18	
IV23	-0.49	-0.49	IV11*IV19	
IV24	-0.07	-0.05	IV12*IV18	
IV25	-0.12	-0.34	IV13*IV19	
IV26	-0.44	-0.49	IV14*IV18	
IV27	-0.20	-0.17	IV15*IV19	
IV28	-0.44	-0.34	IV16*IV18	
IV29	-0.46	-0.57	IV17*IV19	
IV30	0.36	0.29	voting cycle (linear)	the more often mw bills are voted, the higher the mw
IV31	0.37	0.27	voting cycle (squared root)	as above
IV32	0.08	0.02	voting cycle (squared)	as above
IV33	0.34	0.22	voting cycle (log)	as above
IV34	0.08	0.02	voting cycle (exponential)	as above
IV35	-0.25	-0.42	IV8*IV30	
IV36	-0.40	-0.43	IV9*IV30	
IV37	-0.17	-0.14	IV10*IV30	
IV38	-0.31	-0.36	IV11*IV30	
IV39	-0.30	-0.44	IV14*IV30	
IV40	-0.34	-0.42	IV15*IV30	
IV41	-0.18	-0.17	IV16*IV30	
IV42	-0.28	-0.42	IV17*IV30	
IV43	-0.29	-0.45	IV20*IV30	
IV44	-0.40	-0.43	IV21*IV30	
IV45	-0.15	-0.11	IV22*IV30	
IV46	-0.31	-0.36	IV23*IV30	
IV47	-0.34	-0.47	IV26*IV30	
IV48	-0.33	-0.40	IV27*IV30	
IV49	-0.16	-0.15	IV28*IV30	
IV50	-0.28	-0.41	IV29*IV30	

Table 1 - CORRELATIONS BETWEEN THE MINIMUM WAGE, SPIKE AND POLITICAL INSTRUMENTS - continues

IV	spike	real MW	instrument (vary across regions and over time)	intuition
IV51	-0.30	-0.34	nb of mw bills by congressman from the sampled regions	the more mw bills, the lower is the mw
IV52	-0.11	-0.05	nb of mw bills by left wing congressman from the sampled regions	as above
IV53	-0.31	-0.38	nb of mw increase bills by congressman from the sampled regions	as above
IV54	-0.07	-0.02	IV52 as a proportion of IV51	as above
IV55	-0.29	-0.44	IV53 as a proportion of IV51	as above
IV56	0.06	0.14	dummy: 0 if bill not effective and 1 if effective (average)	the more effective the bills, the (less eroded) higher the mw
IV57	-0.06	0.04	nb days mw bills took to be appreciated (sum)	the longer to be appreciated, (the more pressure or the less bargaining power) the higher/lower the mw
IV58	0.00	0.11	nb days mw bills took to be appreciated (average)	as above
IV59	-0.32	-0.39	IV51*IV56	
IV60	-0.11	-0.08	IV51*IV57	
IV61	-0.08	0.00	IV51*IV58	
IV62	-0.15	-0.14	nb of congressman speechs regarding the mw	the more the need for speechs, the lower is the mw
IV63	-0.14	-0.14	nb of left wing congressman speechs regarding the mw	as above
IV64	-0.14	-0.15	nb of congressman speechs regarding a mw increase	as above
IV65	-0.14	-0.12	nb of congressman speechs favourable to a mw increase	as above
IV66	-0.10	-0.09	nb of congressman speechs against a mw increase	as above
IV67	-0.16	-0.16	IV62 as a proportion of speechs from the sampled regions	as above
IV68	-0.17	-0.18	IV63 as a proportion of speechs from the sampled regions	as above
IV69	-0.13	-0.13	IV64 as a proportion of speechs from the sampled regions	as above
IV70	-0.08	-0.06	IV65 as a proportion of speechs from the sampled regions	as above
IV71	-0.19	-0.23	nb of congressman in the mw comission	the more (the need of) congressmen in the comission, the lower the mw
IV72	-0.25	-0.19	nb of left wing congressman in the mw comission	as above
IV73	-0.12	-0.22	nb of right wing congressman in the mw comission	as above
IV74	-0.20	-0.30	IV71 as a proportion of comission congressmen	as above
IV75	0.10	-0.16	IV71 as a proportion of comission congressmen from the sampled regions	as above
IV76	0.04	-0.08	IV72 as a proportion of comission congressmen from the sampled regions	as above
IV77	0.10	-0.16	IV73 as a proportion of comission congressmen from the sampled regions	as above
III - ELECTIONS DATA				
IV78	-0.27	-0.35	national election cycle (linear)	the closer the elections, the lower the mw increase
IV79	-0.03	-0.05	municipal election cycle (linear)	as above
IV80	-0.22	-0.30	national election cycle (squared root)	as above
IV81	-0.04	-0.02	municipal election cycle (squared root)	as above
IV82	-0.31	-0.39	national election cycle (squared)	as above
IV83	-0.20	-0.22	municipal election cycle (squared)	as above
IV84	-0.15	-0.22	national election cycle (log)	as above
IV85	-0.05	-0.01	municipal election cycle (log)	as above
IV86	-0.03	-0.03	national election cycle (exponential)	as above
IV87	-0.05	-0.08	municipal election cycle (exponential)	as above
IV88	-0.38	-0.54	nb of left wing candidates to president elected	the lower the mw, the more left wing congressmen elected
IV89	-0.64	-0.34	nb of left wing candidates to federal deputy elected	as above
IV90	-0.34	-0.19	nb of left wing candidates to senator elected	as above
IV91	-0.23	-0.25	nb of left wing candidates to governor elected	as above
IV92	-0.49	-0.28	nb of left wing candidates to state deputy elected	as above
IV93	-0.02	-0.07	nb of left wing candidates to capital mayor elected	as above
IV94	-0.38	-0.54	proportion of left wing candidates to president elected	the lower the mw, the more left wing congressmen elected
IV95	-0.60	-0.48	proportion of left wing candidates to federal deputy elected	as above
IV96	-0.45	-0.46	proportion of left wing candidates to senator elected	as above
IV97	-0.28	-0.28	proportion of left wing candidates to governor elected	as above
IV98	-0.60	-0.48	proportion of left wing candidates to state deputy elected	as above
IV99	-0.02	-0.07	proportion of left wing candidates to capital mayor elected	as above

Table 1 - CORRELATIONS BETWEEN THE MINIMUM WAGE, SPIKE AND POLITICAL INSTRUMENTS - continued

IV	spike	real MW	instrument (vary across regions and over time)	intuition
IV100	-0.26	-0.25	nb of votes in left wing president candidates	as above
IV101	-0.16	-0.11	nb of votes in left wing federal deputy candidates	as above
IV102	-0.30	-0.12	nb of votes in left wing governor candidates	as above
IV103	-0.15	-0.06	nb of votes in left wing estate deputy candidates	as above
IV104	-0.54	-0.69	proportion of votes in left wing president candidates	as above
IV105	-0.63	-0.49	proportion of votes in left wing federal deputy candidates	as above
IV106	-0.39	-0.30	proportion of votes in left wing governor candidates	as above
IV107	-0.61	-0.49	proportion of votes in left wing estate deputy candidates	as above
IV108	-0.33	-0.46	IV78*IV98	
IV109	-0.49	-0.37	IV79*IV98	
IV110	-0.35	-0.23	IV80*IV98	
IV111	-0.25	-0.29	IV81*IV98	
IV112	-0.39	-0.34	IV82*IV94	
IV113	-0.03	-0.11	IV83*IV99	
IV114	-0.33	-0.46	IV84*IV98	
IV115	-0.46	-0.45	IV85*IV98	
IV116	-0.39	-0.48	IV86*IV98	
IV117	-0.31	-0.31	IV87*IV98	
IV118	-0.45	-0.46	IV88*IV98	
IV119	-0.03	-0.11	IV89*IV99	
IV120	-0.23	-0.26	IV90*IV98	
IV121	-0.20	-0.08	IV91*IV98	
IV122	-0.30	-0.19	IV92*IV98	
IV123	-0.20	-0.09	IV93*IV98	
IV124	-0.43	-0.55	IV94*IV98	
IV125	-0.45	-0.46	IV95*IV98	
IV126	-0.35	-0.34	IV96*IV98	
IV127	-0.44	-0.46	IV97*IV98	
IV128	-0.33	-0.45	IV108*rmw	
IV129	-0.46	-0.22	IV109*rmw	
IV130	-0.32	-0.16	IV110*rmw	
IV131	-0.21	-0.22	IV111*rmw	
IV132	-0.33	-0.16	IV112*rmw	
IV133	-0.03	-0.24	IV113*rmw	
IV134	-0.33	-0.45	IV114*rmw	
IV135	-0.42	-0.30	IV115*rmw	
IV136	-0.38	-0.39	IV116*rmw	
IV137	-0.27	-0.24	IV117*rmw	
IV138	-0.41	-0.30	IV118*rmw	
IV139	-0.03	-0.24	IV119*rmw	
IV140	-0.23	-0.24	IV120*rmw	
IV141	-0.17	-0.06	IV121*rmw	
IV142	-0.29	-0.13	IV122*rmw	
IV143	-0.16	-0.05	IV123*rmw	
IV144	-0.42	-0.50	IV124*rmw	
IV145	-0.41	-0.29	IV125*rmw	
IV146	-0.32	-0.24	IV126*rmw	
IV147	-0.40	-0.29	IV127*rmw	

source: IV1-IV44 National Congress; IV45-IV49 DIAP

1) instruments in bold are prior to interaction

Table 2 - EFFECT OF A 10% INCREASE IN THE MINIMUM WAGE ON EMPLOYMENT

dependent variable	data filter	interval lower	interval upper
total employment	(1) levels	**-0.05**	**0.05**
hours worked		**-0.02**	**0.05**
employment rate		**-0.06**	**0.01**
total employment	(2) first	**-0.13**	**0.08**
hours worked	difference	**-0.05**	**0.09**
employment rate		**-0.09**	**0.01**
total employment	(3) twelfth	**-0.04**	**0.09**
hours worked	difference	**-0.02**	**0.06**
employment rate		**-0.03**	**0.09**
total employment	(4) first and	**-0.09**	**0.11**
hours worked	twelfth	**-0.07**	**0.20**
employment rate	difference	**-0.10**	**0.02**

1) For full estimates see Table A in the Appendix. A 10% increase in the nominal minimum wage increases spike by 0.12 percentage points.
2) A 10% increase in the minimum wage decreases employment by less than 1% (lower end) across models.
3) The dependent variable is average hours worked for the working population, average hours worked for those employed and employment rate. Hours and Job elasticities add to Total elasticity for the static but not for the dynamic model.
4) Time effects are modelled with year, seasonal-month, stabilization and 1988 structural break dummies. Controls are population and institutional factors.

Table 3 - ESTIMATES OF THE COEFFICIENT OF SPIKE - formal and informal, private and public sectors

dependent variables	coef	se	coef	se	Chow test
	1 - formal		**informal**		
total employment	**0.489**	0.108	**0.537**	0.146	0.172
hours worked	**0.427**	0.090	**0.420**	0.107	0.129
employment rate	**-0.044**	0.034	**-0.001**	0.098	0.093
	2 - private		**public**		
total employment	**0.446**	0.094	**0.517**	0.209	0.202
hours worked	**0.415**	0.085	**0.526**	0.208	0.195
employment rate	**-0.013**	0.035	**-0.021**	0.041	0.054
unemployment rate	**0.129**	0.183			

1) These estimates are to be compared with the dynamic model in first differences in column 3, row 2, panel 1 of Table A (in the appendix). That is the preferred specification as discussed in Section 5.2, here uninstrumented. Dynamics are modelled as 24 lags of the dependent variable.
2) The dependent variable is average hours worked for the working population, average hours worked for those employed and employment rate. Hours and Job elasticities do not add to Total elasticity for the dynamic model.
3) Time effects are modelled with year, seasonal-month, stabilization and 1988 structural break dummies. Controls are population and institutional factors.

Table A - ESTIMATES OF THE COEFFICIENT OF SPIKE

dependent variable	data filter	fixed effects coef (1)	se	controls coef (2)	se	dynamics coef (3)	se	long run coef (4)
		1 - uninstrumented						
total employment	(1) levels	**-0.022**	0.095	**-0.036**	0.099	**0.203**	0.090	**0.108**
hours worked		**0.210**	0.080	**0.193**	0.084	**0.200**	0.080	**0.109**
employment rate		**-0.232**	0.042	**-0.230**	0.043	**-0.024**	0.025	**-0.013**
total employment	(2) first	**0.625**	0.111	**0.663**	0.111	**0.441**	0.098	**-0.117**
hours worked	difference	**0.704**	0.110	**0.737**	0.109	**0.422**	0.090	**-0.051**
employment rate		**-0.079**	0.034	**-0.074**	0.027	**-0.013**	0.030	**0.016**
total employment	(3) twelfth	**0.570**	0.102	**0.551**	0.103	**0.347**	0.083	**0.250**
hours worked	difference	**0.466**	0.095	**0.454**	0.096	**0.328**	0.074	**0.326**
employment rate		**0.104**	0.044	**0.097**	0.043	**-0.012**	0.031	**-0.008**
total employment	(4) first and	**0.747**	0.117	**0.779**	0.116	**0.317**	0.079	**-0.084**
hours worked	twelfth	**0.823**	0.116	**0.844**	0.115	**0.317**	0.071	**-0.059**
employment rate	difference	**-0.076**	0.032	**-0.065**	0.030	**-0.031**	0.027	**0.024**
		2 - IV: lagged spike						
total employment	(1) levels	**-0.327**	0.137	**-0.327**	0.139	**0.042**	0.126	**0.023**
hours worked		**-0.023**	0.121	**-0.021**	0.122	**0.060**	0.112	**0.033**
employment rate		**-0.304**	0.056	**-0.306**	0.056	**-0.011**	0.035	**-0.006**
total employment	(2) first	**0.675**	0.195	**0.628**	0.185	**0.527**	0.187	**-0.140**
hours worked	difference	**0.774**	0.191	**0.738**	0.181	**0.513**	0.166	**-0.062**
employment rate		**-0.099**	0.059	**-0.110**	0.047	**0.012**	0.052	**-0.016**
total employment	(3) twelfth	**0.635**	0.184	**0.590**	0.181	**0.242**	0.132	**0.174**
hours worked	difference	**0.414**	0.164	**0.379**	0.162	**0.217**	0.109	**0.215**
employment rate		**0.222**	0.081	**0.211**	0.080	**0.052**	0.053	**0.034**
total employment	(4) first and	**0.907**	0.166	**0.956**	0.171	**0.228**	0.126	**-0.059**
hours worked	twelfth	**0.954**	0.163	**0.975**	0.168	**0.192**	0.111	**-0.035**
employment rate	difference	**-0.047**	0.046	**-0.019**	0.043	**0.008**	0.043	**-0.006**
		3 - IV: interaction-free political instruments						
total employment	(1) levels	**-0.370**	0.148	**-0.317**	0.148	**0.156**	0.136	**0.083**
hours worked		**0.068**	0.124	**0.075**	0.127	**0.140**	0.117	**0.076**
employment rate		**-0.438**	0.070	**-0.392**	0.064	**0.033**	0.045	**0.017**
total employment	(2) first	**0.142**	0.297	**0.198**	0.295	**0.022**	0.297	**-0.006**
hours worked	difference	**0.346**	0.287	**0.442**	0.282	**0.176**	0.270	**-0.021**
employment rate		**-0.204**	0.079	**-0.244**	0.070	**-0.106**	0.089	**0.141**
total employment	(3) twelfth	**0.329**	0.200	**0.307**	0.200	**-0.161**	0.161	**-0.115**
hours worked	difference	**0.234**	0.187	**0.219**	0.188	**0.070**	0.142	**0.070**
employment rate		**0.094**	0.084	**0.088**	0.080	**-0.131**	0.061	**-0.086**
total employment	(4) first and	**0.666**	0.315	**0.814**	0.312	**-0.230**	0.244	**0.056**
hours worked	twelfth	**0.880**	0.315	**1.037**	0.307	**0.040**	0.206	**-0.007**
employment rate	difference	**-0.214**	0.071	**-0.223**	0.070	**-0.235**	0.077	**0.185**
		4 - IV: interaction-free whose correlation with spike was higher than 0.30						
total employment	(1) levels	**-0.255**	0.209	**-0.237**	0.206	**0.365**	0.165	**0.190**
hours worked		**0.268**	0.162	**0.282**	0.167	**0.191**	0.141	**0.104**
employment rate		**-0.523**	0.106	**-0.519**	0.103	**0.057**	0.061	**0.030**
total employment	(2) first	**0.035**	0.505	**0.288**	0.493	**-0.050**	0.450	**0.013**
hours worked	difference	**0.208**	0.473	**0.439**	0.469	**0.167**	0.410	**-0.020**
employment rate		**-0.173**	0.162	**-0.151**	0.131	**-0.101**	0.132	**0.132**
total employment	(3) twelfth	**0.766**	0.335	**0.633**	0.350	**0.070**	0.235	**0.051**
hours worked	difference	**0.462**	0.293	**0.296**	0.308	**0.229**	0.211	**0.227**
employment rate		**0.304**	0.147	**0.337**	0.145	**-0.063**	0.089	**-0.042**
total employment	(4) first and	**-0.145**	0.502	**0.077**	0.500	**-0.211**	0.346	**0.053**
hours worked	twelfth	**-0.338**	0.504	**-0.087**	0.505	**-0.188**	0.325	**0.032**
employment rate	difference	**0.194**	0.149	**0.164**	0.145	**-0.071**	0.112	**0.056**
		5 - IV: voting data interacted with voting dummy and voting cycle						
total employment	(1) levels	**-0.434**	0.268	**-0.029**	0.267	**0.424**	0.268	**0.185**
hours worked		**-0.109**	0.217	**0.192**	0.222	**0.380**	0.228	**0.158**
employment rate		**-0.326**	0.136	**-0.220**	0.130	**0.039**	0.081	**0.014**
total employment	(2) first	**-1.050**	0.764	**-0.745**	0.793	**0.105**	0.614	**-0.017**
hours worked	difference	**-0.337**	0.669	**-0.285**	0.714	**-0.081**	0.551	**0.014**
employment rate		**-0.714**	0.275	**-0.460**	0.239	**-0.193**	0.200	**0.146**
total employment	(3) twelfth	**0.402**	0.497	**0.475**	0.486	**-0.059**	0.295	**-0.030**
hours worked	difference	**0.319**	0.413	**0.486**	0.408	**-0.003**	0.260	**0.089**
employment rate		**0.083**	0.246	**-0.011**	0.228	**-0.107**	0.108	**-0.067**
total employment	(4) first and	**0.594**	0.796	**0.937**	0.780	**-0.564**	0.507	**0.166**
hours worked	twelfth	**1.411**	0.802	**1.628**	0.797	**-0.587**	0.501	**0.129**
employment rate	difference	**-0.817**	0.208	**-0.691**	0.198	**-0.229**	0.118	**0.125**
		6 - IV: election data interacted with election cycle and real minimum wage						
total employment	(1) levels	**-0.333**	0.327	**-0.423**	0.308	**0.161**	0.241	**0.085**
hours worked		**-0.115**	0.283	**-0.181**	0.270	**0.138**	0.203	**0.075**
employment rate		**-0.218**	0.104	**-0.242**	0.093	**-0.032**	0.061	**-0.017**
total employment	(2) first	**0.378**	0.647	**0.441**	0.695	**-0.753**	0.698	**0.190**
hours worked	difference	**0.318**	0.643	**0.428**	0.679	**-0.433**	0.624	**0.050**
employment rate		**0.060**	0.202	**0.013**	0.176	**-0.168**	0.149	**0.222**
total employment	(3) twelfth	**0.540**	0.546	**0.608**	0.545	**-0.335**	0.407	**-0.239**
hours worked	difference	**-0.180**	0.495	**0.011**	0.485	**0.005**	0.355	**0.005**
employment rate		**0.720**	0.222	**0.597**	0.204	**-0.222**	0.116	**-0.145**
total employment	(4) first and	**0.051**	0.658	**0.518**	0.671	**-0.768**	0.765	**0.181**
hours worked	twelfth	**0.059**	0.636	**0.520**	0.645	**-0.242**	0.599	**0.041**
employment rate	difference	**-0.008**	0.192	**-0.002**	0.178	**0.078**	0.177	**-0.061**

1) The dependent variable is average hours worked for the working population, average hours worked for those employed and employment rate. Hours and Job elasticities add to Total elasticity for the static but not for the dynamic model.

2) Column 1shows the base specification with region and time fixed (and past inflation); column 2 adds controls to the base specification; and column 3 adds controls and dynamics (24 lags of the independent variable). Column 4 shows the long run coefficient associated to the model in column 3. Each comun shows Time effects are modelled with year, seasonal-month, stabilization and 1988 structural break dummies. Controls are population and institutional factors.

3) Spike is endogeneous. Panel 1 shows uninstrumented, and panels 2 to 6 show instrumented estimates using lags of spike and political variables as instruments. Each panel has four rows: (1) within groups (levels), (2) first differences, (3) twefth differences, and (4) first and twelfth differences.

4) For full estimates see Table A in the Appendix. Because a 10% increase in the nominal minimum wage increases spike by 0.12 percentage points, all estimates were multiplied by 0.12 (see Section 4).

Table B - ESTIMATES OF THE COEFFICIENT OF THE SPIKE

dependent variable	data filter	fixed effects coef (1)	se	S	df	*H*	*se*	F	df	controls coef (2)	se	S	df	*H*	*se*	F	df	dynamics coef (3)	se	S	df	*H*	*se*	F	df
2 - IV: lagged spike																									
hours worked	(1) levels	**-0.023**	0.121	27.54	10	*0.27*	*0.17*	181.27	48/1013	**-0.021**	0.122	31.80	10	*0.23*	*0.17*	133.96	66/995	**0.060**	0.112	36.22	10	*0.30*	*0.16*	89.46	88/901
employment rate		**-0.304**	0.056	15.36	10	*0.08*	*0.08*	181.27	48/1013	**-0.306**	0.056	13.12	10	*0.09*	*0.08*	133.96	66/995	**-0.011**	0.035	13.40	10	*-0.02*	*0.05*	83.71	88/901
hours worked	(2) first	**0.774**	0.191	26.91	10	*-0.24*	*0.23*	8.99	48/1013	**0.738**	0.181	23.86	10	*-0.16*	*0.23*	7.91	66/995	**0.513**	0.166	24.80	10	*-0.13*	*0.20*	6.35	88/895
employment rate	difference	**-0.099**	0.059	16.33	10	*0.02*	*0.07*	8.99	48/1013	**-0.110**	0.047	12.86	10	*0.05*	*0.06*	7.91	66/995	**0.012**	0.052	14.50	10	*0.03*	*0.03*	33.21	88/895
hours worked	(3) twelfth	**0.414**	0.164	53.04	10	*0.09*	*0.20*	30.12	46/943	**0.379**	0.162	56.64	10	*0.12*	*0.20*	22.70	64/925	**0.217**	0.109	31.23	10	*0.17*	*0.14*	17.01	87/830
employment rate	difference	**0.222**	0.081	45.55	10	*-0.19*	*0.10*	30.12	46/943	**0.211**	0.080	43.57	10	*-0.18*	*0.10*	22.70	64/925	**0.052**	0.053	18.28	10	*-0.10*	*0.06*	16.64	87/830
hours worked	(4) first and twelfth	**0.954**	0.163	43.15	10	*-0.37*	*0.20*	12.59	46/943	**0.975**	0.168	39.81	10	*0.36*	*0.20*	10.42	64/925	**0.192**	0.111	29.16	10	*0.20*	*0.15*	8.79	86/825
employment rate	difference	**-0.047**	0.046	16.99	10	*0.00*	*0.06*	12.59	46/943	**-0.019**	0.043	13.58	10	*-0.01*	*0.06*	10.42	64/925	**0.008**	0.043	23.73	10	*-0.07*	*0.06*	8.46	86/825
3 - IV: interaction-free political instruments																									
hours worked	(1) levels	**0.068**	0.124	228.44	73	*0.23*	*0.15*	78.61	109/1015	**0.075**	0.127	225.91	73	*0.20*	*0.15*	74.37	127/985	**0.140**	0.117	151.63	73	*0.11*	*0.14*	41.47	148/832
employment rate		**-0.438**	0.070	396.26	73	*0.33*	*0.09*	78.61	109/1015	**-0.392**	0.064	404.31	73	*0.27*	*0.09*	74.37	127/985	**0.033**	0.045	121.34	73	*-0.09*	*0.06*	42.79	148/832
hours worked	(2) first	**0.346**	0.287	71.73	73	*0.26*	*0.31*	7.01	108/1015	**0.442**	0.282	75.23	73	*0.34*	*0.30*	5.82	126/985	**0.176**	0.270	136.43	73	*0.28*	*0.29*	2.78	147/826
employment rate	difference	**-0.204**	0.079	77.22	73	*0.11*	*0.09*	7.01	108/1015	**-0.244**	0.070	63.87	73	*0.20*	*0.07*	5.82	126/985	**-0.106**	0.089	105.62	73	*-0.01*	*0.03*	44.39	148/826
hours worked	(3) twelfth	**0.234**	0.187	153.83	73	*0.30*	*0.20*	9.43	108/944	**0.219**	0.188	145.72	73	*0.31*	*0.20*	7.82	126/920	**0.070**	0.142	141.01	73	*0.34*	*0.15*	9.54	146/761
employment rate	difference	**0.094**	0.084	250.11	73	*0.01*	*0.09*	9.43	108/944	**0.088**	0.080	261.93	73	*0.01*	*0.09*	7.82	126/920	**-0.131**	0.061	150.36	73	*0.16*	*0.07*	7.51	147/761
hours worked	(4) first and twelfth	**0.880**	0.315	147.75	73	*-0.08*	*0.33*	5.04	108/944	**1.037**	0.307	153.11	73	*-0.23*	*0.32*	4.57	125/920	**0.040**	0.206	133.76	73	*0.31*	*0.22*	3.23	146/756
employment rate	difference	**-0.214**	0.071	108.15	73	*0.16*	*0.08*	5.04	108/944	**-0.223**	0.070	96.75	73	*0.19*	*0.08*	4.57	125/920	**-0.235**	0.077	124.11	73	*0.23*	*0.08*	3.24	146/756
4 - IV: interaction-free whose correlation with spike was higher than 0.30																									
hours worked	(1) levels	**0.268**	0.162	131.28	24	*-0.07*	*0.18*	129.24	63/1070	**0.282**	0.167	128.70	24	*-0.11*	*0.18*	97.95	81/1040	**0.191**	0.141	59.67	24	*0.01*	*0.15*	61.72	102/887
employment rate		**-0.523**	0.106	284.53	24	*0.37*	*0.12*	129.24	63/1070	**-0.519**	0.103	285.47	24	*0.37*	*0.11*	97.95	81/1040	**0.057**	0.061	35.96	24	*-0.10*	*0.07*	59.07	102/887
hours worked	(2) first	**0.208**	0.473	21.24	24	*0.45*	*0.50*	3.61	63/1070	**0.439**	0.469	17.70	24	*0.31*	*0.49*	3.12	81/1040	**0.167**	0.410	35.68	24	*0.27*	*0.43*	2.68	102/881
employment rate	difference	**-0.173**	0.162	18.07	24	*0.12*	*0.17*	3.61	63/1070	**-0.151**	0.131	13.86	24	*0.08*	*0.13*	3.12	81/1040	**-0.101**	0.132	33.42	24	*0.00*	*0.03*	54.52	102/881
hours worked	(3) twelfth	**0.462**	0.293	60.05	24	*0.00*	*0.30*	12.60	62/999	**0.296**	0.308	51.18	24	*0.17*	*0.31*	9.84	80/975	**0.229**	0.211	68.00	24	*0.11*	*0.22*	7.31	101/816
employment rate	difference	**0.304**	0.147	130.57	24	*-0.22*	*0.15*	12.60	62/999	**0.337**	0.145	141.34	24	*-0.26*	*0.15*	9.84	80/975	**-0.063**	0.089	55.76	24	*0.06*	*0.09*	6.95	101/816
hours worked	(4) first and twelfth	**-0.338**	0.504	27.89	24	*1.23*	*0.51*	1.08	62/999	**-0.087**	0.505	30.63	24	*0.97*	*0.52*	1.07	80/975	**-0.188**	0.325	35.37	24	*0.53*	*0.33*	2.89	100/811
employment rate	difference	**0.194**	0.149	30.99	24	*-0.28*	*0.15*	1.08	62/999	**0.164**	0.145	31.42	24	*-0.24*	*0.14*	1.07	80/975	**-0.071**	0.112	33.62	24	*0.04*	*0.11*	1.36	100/811
5 - IV: voting data interacted with voting dummy and voting cycle																									
hours worked	(1) levels	**-0.109**	0.217	55.83	14	*0.35*	*0.22*	103.91	53/1080	**0.192**	0.222	54.97	14	*0.00*	*0.23*	74.54	71/1050	**0.380**	0.228	36.40	14	*-0.20*	*0.23*	40.59	92/897
employment rate		**-0.326**	0.136	95.68	14	*0.10*	*0.15*	103.91	53/1080	**-0.220**	0.130	93.27	14	*-0.01*	*0.14*	74.54	71/1050	**0.039**	0.081	19.46	14	*-0.07*	*0.08*	42.64	92/897
hours worked	(2) first	**-0.337**	0.669	6.42	14	*1.06*	*0.66*	3.80	53/1080	**-0.285**	0.714	5.85	14	*1.04*	*0.70*	3.16	71/1050	**-0.081**	0.551	31.42	14	*0.52*	*0.55*	2.61	92/891
employment rate	difference	**-0.714**	0.275	5.28	14	*0.66*	*0.23*	3.80	53/1080	**-0.460**	0.239	5.37	14	*0.39*	*0.21*	3.16	71/1050	**-0.193**	0.200	19.82	14	*0.02*	*0.03*	35.40	92/891
hours worked	(3) twelfth	**0.319**	0.413	71.33	14	*0.15*	*0.42*	14.35	52/1009	**0.486**	0.408	74.89	14	*-0.03*	*0.41*	11.09	70/985	**-0.003**	0.260	42.52	14	*0.36*	*0.26*	7.21	91/826
employment rate	difference	**0.083**	0.246	62.66	14	*0.02*	*0.25*	14.35	52/1009	**-0.011**	0.228	70.37	14	*0.11*	*0.23*	11.09	70/985	**-0.107**	0.108	48.42	14	*0.10*	*0.11*	7.33	91/826
hours worked	(4) first and twelfth	**1.411**	0.802	39.19	14	*-0.60*	*0.81*	1.30	52/1009	**1.628**	0.797	39.43	14	*0.81*	*0.80*	1.22	70/985	**-0.587**	0.501	27.28	14	*0.93*	*0.47*	3.03	90/821
employment rate	difference	**-0.817**	0.208	7.39	14	*0.76*	*0.17*	1.30	52/1009	**-0.691**	0.198	7.84	14	*0.65*	*0.17*	1.22	70/985	**-0.229**	0.118	25.43	14	*0.21*	*0.12*	1.72	90/821
6 - IV: election data interacted with election cycle and real minimum wage																									
hours worked	(1) levels	**-0.115**	0.283	102.93	16	*0.38*	*0.31*	93.50	51/1078	**-0.181**	0.270	102.91	16	*0.45*	*0.30*	69.20	69/1048	**0.138**	0.203	44.67	16	*0.08*	*0.23*	39.55	90/895
employment rate		**-0.218**	0.104	161.39	16	*-0.02*	*0.12*	93.50	51/1078	**-0.242**	0.093	165.38	16	*0.01*	*0.11*	69.20	69/1048	**-0.032**	0.061	30.61	16	*0.01*	*0.07*	40.78	90/895
hours worked	(2) first	**0.318**	0.643	9.35	16	*0.33*	*0.66*	4.02	51/1078	**0.428**	0.679	10.65	16	*0.32*	*0.68*	3.19	69/1048	**-0.433**	0.624	24.27	16	*0.89*	*0.58*	2.61	90/889
employment rate	difference	**0.060**	0.202	3.59	16	*-0.15*	*0.19*	4.02	51/1078	**0.013**	0.176	7.44	16	*-0.09*	*0.17*	3.19	69/1048	**-0.168**	0.149	22.91	16	*0.03*	*0.03*	34.11	90/889
hours worked	(3) twelfth	**-0.180**	0.495	31.01	16	*0.68*	*0.48*	12.42	50/1007	**0.011**	0.485	30.15	16	*0.47*	*0.47*	9.43	68/983	**0.005**	0.355	40.57	16	*0.34*	*0.35*	6.04	89/824
employment rate	difference	**0.720**	0.222	75.03	16	*-0.65*	*0.21*	12.42	50/1007	**0.597**	0.204	82.98	16	*-0.53*	*0.20*	9.43	68/983	**-0.222**	0.116	50.51	16	*0.22*	*0.11*	5.88	89/824
hours worked	(4) first and twelfth	**0.059**	0.636	10.06	16	*0.76*	*0.63*	0.85	50/1007	**0.520**	0.645	10.66	16	*0.33*	*0.66*	0.91	68/983	**-0.242**	0.599	13.94	16	*0.57*	*0.55*	2.89	88/819
employment rate	difference	**-0.008**	0.192	14.12	16	*-0.07*	*0.19*	0.85	50/1007	**-0.002**	0.178	12.69	16	*-0.06*	*0.18*	0.91	68/983	**0.078**	0.177	14.79	16	*-0.11*	*0.18*	1.33	88/819

1) The dependent variable is average hours worked for the working population, average hours worked for those employed and employment rate. Hours and Job elasticities add to Total elasticity for the static but not for the dynamic model.

2) Column 1shows the base specification with region and time fixed (and past inflation); column 2 adds controls to the base specification; and column 3 adds controls and dynamics (24 lags of the independent variable). Time effects are modelled with year, seasonal-month, stabilization and 1988 structural break dummies. Controls are population and institutional factors.

3) Each comun shows the spike coefficient and associated standard errors, Sargan test, Hausman test and F test (step of the 2SLS). Degree of freedom for Sargan and F test are indicated. Standard errors for Hausman test are indicated.

4) Spike is endogeneous. Panels 2-6 show instrumented estimates using lags of spike and political variables as instruments. Each panel has four rows: (1) within groups (levels), (2) first differences, (3) twefth differences, and (4) first and twelfth differences.

5) For full estimates see Table A in the Appendix. Because a 10% increase in the nominal minimum wage increases spike by 0.12 percentage points, all estimates were multiplied by 0.12 (see Section 4).