Wage Rigidity and Disinflation in Emerging Countries*

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Abstract

This paper examines the consequences of rapid disinflation for downward wage rigidities in two emerging countries, Brazil and Uruguay, relying on high quality matched employer-employee administrative data. Downward nominal wage rigidities are more important in Uruguay, while wage indexation is dominant in Brazil. Two regime changes are observed during the sample period, 1995-2004: (i) in Uruguay wage indexation declines, while workers' resistance to nominal wage cuts becomes more pronounced; and (ii) in Brazil, the introduction of inflation targeting by the Central Bank in 1999 shifts the focal point of wage negotiations from changes in the minimum wage to expected inflation. These regime changes cast doubts on the notion that wage rigidity is structural in the sense of Lucas (1976).

Keywords: *downward wage rigidity, indexation, matched employer-employee data, emerging economies*

JEL Classification: *J30, E24.*

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1 Introduction

The incidence and nature of downward wage rigidities varies markedly across countries. In the US, an ample literature suggests that workers resist nominal wage cuts (e.g. Kahn (1997), Altonji and Devereux (2000) and Lebow et al. (2003)). In contrast, downward nominal wage rigidities (DNWR) are lower in Europe, where real rigidities, introduced through different forms of explicit or implicit indexation of wages to prices, appear to be more relevant (Dickens et al., 2007).

An important aspect that has not been addressed in the empirical micro literature is whether downward wage rigidities represent a structural feature of the economy, in the sense of Lucas (1976). Gordon (1996) and Mankiw (1996), in their discussion of Akerlof et al. (1996) argue that the wage setting behavior of workers and firms is likely to change in response to changes in the macroeconomic environment. In particular, workers facing long periods of low inflation or price stability may get used to reductions in nominal wages, and hence offer less resistance to nominal cuts. A similar argument, in the context of the extent of wage-price indexation, is put forward by Bernanke (2004), who claims that explicit or implicit indexation clauses should ultimately depend on the monetary policy regime.

Previous empirical evidence suggests that wage rigidities are highly persistent within countries. Goette et al. (2007) show that workers' resistance to nominal wage cuts remains persistently high in Switzerland, in spite of a prolonged period of very low inflation. Studies by Barwell and Schweitzer (2007) in the UK, Bauer et al. (2007) in Germany, and Devicienti et al. (2007) in Italy show that real rigidities are more important than workers' resistance of nominal wage cuts in Europe. Moreover, the incidence of downward real wage rigidity in the three countries changes little over time. Hence, it is not surprising that macro models featuring nominal wage rigidities and partial indexation clauses (e.g., Christiano et al. (2005), Smets and Wouters (2007)) treat these parameters as fixed, independent of the macro and policy framework. However, previous empirical evidence may not be the best suited to study the question at hand; i.e., whether wage setting patterns vary with the macroeconomic environment. The macro and policy environment in the countries and periods previously studied were relatively stable, offering little variation to identify the impacts on wage setting of macro and policy changes.

This paper contributes to this debate by documenting the case of two countries in which the intensity of downward nominal wage rigidity and the indexation of nominal wages has varied dramatically with the economic and institutional environment. We study two middle-income countries, Brazil and Uruguay, both of which successfully reduced inflation after the introduction of stabilization policies in the 1990s -moving from three-digit levels (four, in Brazil) in the first years of the stabilization effort to one-digit levels by the end of the decade (see figure 1). Our period of analysis begins in the transition years -1995 in Brazil and 1996 in Uruguay. The movement from a high and volatile inflation environment to a low inflation regime allows us to assess how the structural features of wage setting changed with the macroeconomic environment.

While the two countries both experienced a transition in their rates of inflation, they differ in several institutional dimensions that render a comparative study interesting, and may turn out to be important for the prevalence of different types of rigidities. Regarding the monetary policy framework, Uruguay is characterized during most of the sample period by a system of exchange rate anchor, and then, after the crisis of 2002, by moving towards a flexible exchange rate regime coupled with overall price stability within a target rate. In Brazil, by contrast, there is a sharp regime shift during the period, with the adoption of inflation targeting in 1999. Two differences in the labor market institutions of the two countries are also important. First, unions in Uruguay steadily lose power while firm level bargaining gain weight dramatically during the sample period; while in Brazil union coverage remains fairly high and stable. Second, minimum wages in Uruguay are not binding, whereas in Brazil, workers earning a minimum wage represent 6.9 percent of the formal labor force in our sample. In most Latin American countries, however, changes in the minimum wage tend to be used as a signal for wage bargaining across all sectors (Maloney and Mendez, 2004). In this respect, such changes might be an important source of indexation in Brazil.

This paper also contributes to an understanding of the evolution of wage rigidities in micro-data for emerging countries, a topic that has received very little attention in the literature.[1] We rely on high-quality matched employer-employee data from administrative records in Brazil and Uruguay, which allows us to obtain accurate measures of wages and to properly track worker mobility. The analysis is carried out using the maximum likelihood estimator first proposed by Altonji and Devereux (2000), and extended to consider DRWR by Goette et al. (2007). The estimator relies on a model of wage changes that jointly estimates the relevant parameters governing the process of DNWR and wage indexation. Importantly, the price index underlying the indexation process is endogenously determined by the model and allowed to be heterogeneous across workers. This permits ex-post evaluation of the relationship between this price index and different prices in the economy, which is crucial for understanding the sources of indexation. The model also allows for errors in the measurement of wages. This is potentially important, since measurement error in wage data has been shown to be an source of downward bias in studies of DNWR (Gottschalk, 2005).

Our results indicate that Brazil and Uruguay present substantial differences in the nature of wage rigidities, and in their reaction to changes in the macroeconomic and institutional environment. In the private formal sector of Uruguay, the pegging of individual nominal wage growth to the rate of inflation falls dramatically over the sample period. Downward real wage rigidity affects on average 75 percent of the workforce in the period 1996-1999, reducing to a fairly stable 7 percent for the rest of the sample period (1999-2004). In parallel, the fraction of workers subject to DNWR increases from around 13 percent in the first three years to an average of 66 percent in the second part of the sample. Hence, disinflation is associated with a sharp movement away from wage indexation. However, this process does not lead the way to a frictionless wage setting regime. Instead, worker resistance to nominal wage cuts arises.

In contrast with Uruguay, the incidence of wage indexation in Brazil is fairly high and stable, affecting on average 43 percent of the workforce. There is, however, an important

[1] A notable exception is Castellanos et al. (2004), who study downward nominal wage rigidity and indexation to the minimum wage in Mexico. Our paper differs from this study in three aspects. First, we allow for the presence of measurement errors in the estimation, an aspect that has received considerable attention in the literature in the last few years. Second, we do not impose wage indexation to follow a pre-determined price index. Instead, the price index considered in wage negotiations is allowed to be heterogeneous across agents, and is endogenously determined. Finally, our model is estimated year by year in order to track changes in the distribution of wage changes over time.

regime shift underway in Brazil, and this change is related to the price index to which nominal wages are indexed. During the first years of the sample, up to 1999, the price index to which wages are indexed is the change in the minimum wage. Subsequently, and coincident with the introduction of inflation targeting in 1999, the price index that represents the focal point of wage negotiations moves away from the minimum wage and becomes closely related to the evolution inflation. Interestingly, the variance of this price index across agents also increases after 1999 -a feature that is consistent with the index being an indicator of inflation expectations.

The results in this paper are related to two recent macro studies. In the context of price setting, Benati (2008) shows that the rate of inflation persistence in different countries and time periods varies with the monetary policy regime, while Hofmann et al. (2010) show similar time variation in US wage dynamics. These two papers offer compelling time series evidence that challenges the notion that the persistence of inflation, or wage indexation to past inflation, are intrinsic to the deep structure of the economy, and invariant to changes in the monetary regime. Our work is a first attempt to examine the stability of downward nominal and real wage rigidities in an environment subject to policy changes and rapid macroeconomic stabilization. To this end, relying on matched establishment-worker data constitutes a great advantage. A general problem with the aggregation in macro series is that it assumes that the evolution of wage rigidities is the same for all establishments or groups of individuals. However, agents with different characteristics are likely to react differently to macro and policy changes. Some of these are subtle changes. For instance, our results for Brazil show that after the introduction of inflation targeting some agents appear to start negotiating wage changes as a function of expected inflation, while others remain pegged to changes in the minimum wage. Understanding the evolution of the focal point of wage negotiations is important for the design of monetary policy, and to guide the need for possible structural reforms in the labor market. However, these changes may be blurred by aggregation at the macro level. The use of micro data instead allows us to examine time-varying worker and establishment heterogeneity in wage setting.

The rest of the paper is organized as follows. Section 2 describes the data sets used and presents wage change histograms for selected years, discussing their main features and why they are suggestive of different types of rigidities. Section 3 discusses the methodology used to deal with measurement errors and the joint estimation of the different types of rigidity. Section 4 presents the main results of the paper. In section 5 we extend the analysis of the incidence of wage rigidities to differentiate across worker and firm characteristics. Section 6 concludes.

2 Data

2.1 Data sources and sample selection

We use annual administrative employer-employee matched data from Brazil (1995-2002) and Uruguay (1996-2004). The main difference between the two data sets is that while the former is comprehensive, including information for the universe of workers in the formal sector in Brazil, the latter provides us with information on a random sample of workers in Uruguay's formal sector.

Our data source for Brazil is the labor market census RAIS (*Relação Annual de Informações Sociais*), an administrative data set collected annually by the Brazilian Labor Ministry. By Brazilian law, all employers in the formal sector must report detailed information for all their workers to RAIS every year. RAIS includes information about workers (sex, age, education) and their jobs (type of contract, occupation, average monthly wage earned during the year, wage earned in December, and the amount of hours usually worked per week), as well as some characteristics of the establishment (sector, region, municipality). Importantly, RAIS also provides firm, establishment, and worker identifiers which, together with the dates of admission and separation, allow us to accurately identify year-to-year job stayers.

We restrict our Brazilian sample to the state of Minas Gerais. We do so for two reasons. First, using the whole RAIS sample, which is huge, would make data handling impracticable. Second, we have confirmed, using representative survey data, that Minas Gerais is well suited to represent the Brazilian economy. In particular, our tabulations, based on the Brazilian Labor Force Survey "*Pesquisa Mensal de Emprego*" for our sample years indicate that the shares of formal employment, gender, age, and industry distribution in Belo Horizonte (the capital of Minas Gerais) is very similar to those in other urban areas (see table 1).[2]

Our Uruguayan data consist of social security records from the *Banco de Previsión Social* (BPS). After the 1995 social security reform, the BPS started keeping employer and employee administrative records on a monthly basis. We exploit a random sample of the BPS records, which, like the Brazilian RAIS, contains information on establishments (number of employees, sector, region), workers (age, sex) and their jobs (occupation, weekly hours of work, monthly wages). Establishments and workers are uniquely tracked by an individual identifier and an establishment identifier. Each job within a worker-establishment pair is uniquely identified as well, which allows us to track year-to-year job stayers who have been neither promoted nor demoted. This job identifier, however, is not available in the Brazilian RAIS, so for the sake of comparability in our empirical analysis we define stayers as workers who are continuously employed in the same establishment for a year or more.[3]

Regarding the compensation measures used in our analysis, the Brazilian RAIS reports monthly wages earned in December, which include extraordinary additions, supplements and bonuses, tips and gratuities, commissions and fees, contracted premia, overtime compensation for contracted extra hours, and, in general, all forms of payment that are taxable income or are subject to Brazilian social security contributions. The "thirteenth salary" -the special December payment that is made in some sectors- as well as severance payments for layoffs and indemnity payments are not considered wage components. Therefore, we construct a comparable monthly wage measure from Uruguay's BPS that excludes severance payments and the special December payment. The number of hours worked per week is reported in both data sets. There are some differences, though, in the way hours are measured in each data set. The RAIS data

[2] The *Pesquisa Mensal de Emprego* is representative of urban areas in 8 districts of Brazil. We restrict the comparison to Belo Horizonte, where, according to our data, 28 percent of formal employees in Minas Gerais are employed.

[3] However, it is worth noting that our results are very similar when we exclude promoted or demoted employees from our Uruguayan sample of stayers. These resuls are available upon request from the authors.

reports contractual hours, while the BPS records actual hours worked during the week of reference.

Hourly wages in each data set are constructed using the compensation and hours worked in that data set. Note that none of our measures of hourly wages is, in principle, exempted from measurement errors. Since individuals' working time arrangements are not time invariant, even if they stay in the same job, some of the observed monthly wage changes will actually reflect changes in labor input. Computing hourly wages from RAIS will only partially mitigate this problem as long as changes in hours are reflected in the contract. The hourly wage measure in Uruguay might be more accurate, but variations in weekly hours of work throughout the month will result in measurement errors. In conclusion, even if our two data sources are less likely to be subject to measurement error than survey data, we cannot rule out the possibility of error. Hence, the model described in section 3 allows for the presence of measurement error in the estimation.

For both Brazil and Uruguay, we use the same sample selection criteria. First, we restrict our sample to full-time employees working in the private sector. Second, whenever individuals have more than one job, we always keep the observation with the highest paying job. Finally, our benchmark analysis of individual wage growth, which we define as the difference in log wages over a 12-month interval, is limited to job stayers, who are characterized by working in the same establishment in the same month in two adjacent years. These data restrictions leave 7 years of wage change observations in Brazil, containing between 857,589 and 1,200,120 individual cases, and 8 years in Uruguay, including between 68,494 and 97,721 individual wage changes.

2.2 Descriptive Evidence

In order to get an initial impression of the different types of rigidities that prevail under different inflation scenarios, we look in this section at individual wage change distributions from selected years (figures 2 and 3). In line with our previous discussion, we concentrate on the yearly nominal log-wage change distribution of private sector workers staying in the same job for two consecutive years. Each bin contains a one-percent variation in wages, and we have restricted nominal wage changes to be below 40 percent and above -10 percent in the figures, in order to highlight the distortions associated with DNWR and indexation. Two vertical lines are also shown in the graph. The solid line is the yearly inflation rate, while the dashed line shows the yearly growth rate of the minimum wage.

We start by discussing the earlier period in our sample, when inflation had already started coming down but was still at double-digit levels. Panel A of figure 2 shows the wage change histogram in 1997-1998 in Uruguay. This is the first year of moderate (10.81%) inflation after the introduction of the stabilization plan in 1990. The first notable aspect is a concentration of observations at zero wage changes (around 7 percent), and the relatively little mass below it, in what seems to be a sign of downward nominal wage rigidities. There is another important asymmetry in the distribution: a large mass of observations is clustered around the inflation rate, while the percentage of workers with a wage change right below the 10-11 percent bin is clearly lower than the percentage of wage changes above it. Naturally, economic agents do not necessarily have the same expectations about inflation, or the same reference value in wage negotiations. Hence, the distortion associated with indexation clauses cannot be seen as sharply in the histograms as the impact of the zero wage change boundary. This will be taken into account in the empirical analysis by allowing for inflation expectations or focal point in wage negotiations to differ across agents. However, the cluster of observations around the rate of inflation might be a sign of partial indexation clauses. Finally, note also that the growth of the minimum wage lies well above the rate of inflation, and there are no signs of distortion in this part of the distribution. As we will see, this is in sharp contrast with what is happening in Brazil during this period.

Panel B of figure 2 shows the wage change histogram in 1995-1996 in Brazil. This is right after the introduction of the stabilization plan in 1994, the Real Plan, which succeeded in sharply bringing down inflation from 2,075 percent in 1993-1994 to 15.75 percent in 1995-1996. The first noticeable aspect is a large spike in the 12 percent bin, which accounts for some 15 percent of the wage change observations and more than triples the mass in the adjacent bins. The 12 percent bin coincides with the growth of the minimum wage (11.99 percent), and forcefully suggests that the price index used to index wage contracts this year is the change in the minimum wage. Note also that there are no distortions associated with the rate of inflation. Quite surprisingly, we find very little zero wage changes, but the -1 bin shows a small spike. This concentration of tiny wage cuts is suggestive of measurement error.

We move next to the low-inflation years. Panel A of figure 3 shows the wage change distribution in Uruguay in 2000-2001, when inflation is at its low for the period (4.35 percent). The most striking feature with respect to the high-inflation year is the growth in the spike at zero, which now accounts for almost 25 percent of the wage change observations. As before, there is some concentration of observations around the rate of inflation, but to a much lesser extent than in 1997-1998. Panel B of figure 3 shows the 1999-2000 histogram in Brazil. As before, the change in the minimum wage is associated with a large spike, amounting to 14 percent of the wage changes. The novelty is that there appears to be a second distortion in the positive wage change range, this time associated with the rate of inflation. This could indicate that as inflation comes down, some agents start indexing their wages to expected inflation.

In sum, visual inspection of the wage change histograms suggests an important presence of downward rigidities and indexation in both countries in all years. However, obtaining precise measures of downward wage rigidities is complicated because of the presence of measurement error in the data and the existence of different focal points for wage indexation. The next section presents a model that deals with these two features and provides us with clean measures of DNWR and the extent of wage indexation.

3 Methodology

There are different approaches in the literature to estimating rigidities from individual data. Focusing on job stayers between two consecutive years, a number of studies draw inferences about rigidities based on asymmetries in the wage change distribution (see, e.g., Card and Hyslop (1997) and Dickens et al. (2007)). A second group of estimates is based on the assumption that, in the absence of changes in the extent of rigidity, the shape of the wage change distribution is constant over time.[4] A problem with these methods is that they do not directly take into account the impact of measurement errors. An alternative is first proposed by Altonji and Devereux (2000), who develop a wage-setting model in which the structural parameters of DNWR and measurement errors are jointly estimated via maximum likelihood. This methodology has been extended by Goette et al. (2007) to consider downward real wage rigidities (DRWR) or the resistance of nominal wages to falling below an estimated positive threshold, , and has been applied in the UK by Barwell and Schweitzer (2007), in Germany by Bauer et al. (2007), and in Italy by Devicienti et al. (2007). In what follows we present a brief description of the main elements of this model.

Each individual wage change observation can belong to one of three regimes: a regime subject to DNWR, a regime subject to wage indexation or DRWR, and a regime in which wages are fully flexible.[5] In the model we allow for the possibility (although this is not imposed) that observed wage changes are distorted by measurement error. Define Δy_{it}^{*} as the notional wage change distribution at time t, corresponding to wage changes between t and $t-1$. The notional is the distribution that would prevail if all wages were fully flexible and there were no measurement errors. Then, the notional wage change for a random draw i from the population can be modelled as a function of observable characteristics, as follows:

$$\Delta y_{it}^{*} = \mathbf{x}_{it-1}\boldsymbol{\beta}_t + \varepsilon_{it} \tag{1}$$

, where $\mathbf{x}_{it-1}$ is a vector of individual and job characteristics in the base year, and the error term ε_{it} is assumed to be normally distributed. The problem is that we do not observe this notional wage change. Rather than observing Δy_{it}^{*}, we observe a wage change distribution that is potentially distorted by both measurement errors and downward wage rigidities. We refer to this distribution as the observed (Δy_{it}^{o}), and we detail below how it relates to Δy_{it}^{*}.

[4] See Kahn (1997) and Castellanos et al. (2004) for detailed discussion.

[5] We use the terms DRWR and wage indexation interchangeably. There are important differences across countries, but indexation clauses tend to be asymmetric. If inflation happens to be above expectations, revision clauses are often in place, but the reverse is not true. See Babecky et al. (2009) for a discussion.

As we stated, the first difference between observed and notional wage changes is due to measurement error. The population measurement error is defined as the difference between the observed value, Δy_{it}^{o}, and the actual value, Δy_{it}. Hence, $\Delta e_{it} = \Delta y_{it}^{o} - \Delta y_{it}$, where e_{it} is modeled as a normally distributed shock to wage levels. However, not all observations are subject to measurement error. We assume that with probability q_t wages are correctly measured, and with probability $1 - q_t$, they are measured with error.

The second difference between observed and notional wage changes is due to the presence of rigidities in wage setting. Wage changes for the proportion of individuals subject to DNWR, denoted by p_t^N, will be governed by:

$$\Delta y_{it}^{o} = \left\{ \begin{array}{c} \mathbf{x}_{it-1}\boldsymbol{\beta}_t + \varepsilon_{it} + \Delta e_{it} \text{ if } \mathbf{x}_{it-1}\boldsymbol{\beta}_t + \varepsilon_{it} \geqslant 0 \\ \Delta e_{it} \text{ if } \mathbf{x}_{it-1}\boldsymbol{\beta}_t + \varepsilon_{it} < 0 \end{array} \right\} \quad (2)$$

Thus, for those individuals subject to DNWR, the observed wage change will coincide with the notional wage change plus some measurement error only when the programmed wage increase is above zero. Had the flexible wage change been negative, the worker would receive a wage freeze, and the observed wage change would only differ from zero if wages were measured with errors.

Similarly, the observed wage changes of the fraction of individuals prone to DRWR, denoted by p_t^R, will instead be given by:

$$\Delta y_{it}^{o} = \left\{ \begin{array}{c} \mathbf{x}_{it-1}\boldsymbol{\beta}_t + \varepsilon_{it} + \Delta e_{it} \text{ if } \mathbf{x}_{it-1}\boldsymbol{\beta}_t + \varepsilon_{it} \geqslant r_{it} \\ \Delta e_{it} + r_{it} \text{ if } \mathbf{x}_{it-1}\boldsymbol{\beta}_t + \varepsilon_{it} < r_{it} \end{array} \right\} \quad (3)$$

, where a straightforward interpretation (but not the only one) of r_{it} is that it represents individual-specific inflation expectations. Note that, in contrast with DNWR, the focal point for wage indexation is individual specific (r_i). In the empirical implementation of the model, we will assume that r_{it} is i.i.d. normal and will allow its mean and variance to vary over time.

As for the fraction of flexible individuals, denoted by $1 - p_t^N - p_t^R$, their actual wage changes, Δy_{it}, are equal to the notional wage changes, Δy_{it}^{*}. Differences between observed and notional wage changes are allowed if measurement error is present: $\Delta y_{it}^{o} =$

$\mathbf{x}_{it-1}\boldsymbol{\beta}_t + \varepsilon_{it} + \Delta e_{it}$.

It is worth stressing once more that measurement error is not forced onto the data. Assuming that Δe_{it} is normally distributed with a positive variance in the population would imply that the probability of a firm correctly reporting individual wages would be zero, a feature most likely inconsistent with the nature of our data. Instead, our assumption that a fraction, q_t, of the wages, is measured without error allows us to examine the relative importance of this feature in the two data sets we study.

Under these assumptions, the likelihood of each observation is calculated and all parameters are estimated by maximum likelihood. Intuitively, the estimator uses the local asymmetries in the wage change distributions that we have discussed in the descriptive section in order to identify DNWR and DNWR. However, instead of identifying them unconditionally, by, for example, visually inspecting the previous histograms, the estimator exploits the asymmetries in wage changes conditional on individual and job characteristics. As for the identification of potential measurement error, the estimator has two main features that help assess its relevance. First, since measurement error is expected to follow a continuous distribution, its presence is less likely when the size of the spike at zero is large. Second, since measurement error is expected to displace actual wage freezes, we should observe excess probability mass around zero and relatively smooth asymmetries in other parts of the wage change distribution when measurement error is important.

It is important to note that not all forms of measurement error will necessarily result in an underestimation of DNWR. Most notably, in the presence of rounding errors, wage freezes would actually be over-represented and not the opposite. For example, Smith (2000) uses data from the British Household Panel Survey (BHPS) and finds that self-reported reported wage freezes between two consecutive years are less likely to happen when individuals double check their pay slip before reporting wages. As a consequence, measurement of DNWR in the BHPS which does not take into account rounding errors would tend to over-estimate rigidities. We believe our data are unlikely to suffer from rounding errors since wage information is directly extracted from firms' individual pay records. In the case of Uruguay, the BPS data determine individual access to pension, health, and other benefits. Hence, it is in the worker's interest that all information is accurately reported by the firm. Moreover, the data are thoroughly checked by government officials. In the case of Brazil, wages are also directly extracted from company records and wage information is requested to the level of *centavos* (cents). On average, 75 percent of our wage observations include the cents paid.

Goette et al. (2007) note that their estimator may encounter identification problems when inflation is very low, since in that case, DNWR and DRWR are very close in the wage change distribution and are therefore hard to distinguish. We are rarely constrained by this issue and are therefore generally able to obtain accurate estimates because, even in the periods with lowest inflation in Brazil and Uruguay, the inflation rate was relatively high with respect to OECD countries previously studied. Also, and in contrast with previous studies, the rate of inflation observed during the period of study in both Uruguay and Brazil is fairly volatile. It is for this reason that we separately estimate our model year by year. This allows for a flexible specification, wherein both, the parameters of the notional and the different rigidity regimes may vary across years. A potential limitation of our analysis is that it does not consider the possibility of symmetric rigidities; for instance, those associated with the menu costs of changing wages. Our assumption is justified by the analysis of Dickens et al. (2008), who examines individual wage change distributions among job stayers in 17 OECD countries and finds no evidence of menu costs in wage setting. Given the inflation history of the two countries we are studying and the high volatility of inflation during the sample period we believe that menu costs are even less likely to be relevant in our case.

4 Main Results

4.1 Empirical implementation

In this section, we discuss some relevant details regarding the empirical implementation of the model and provide a first assessment of its functioning. We estimate an individual wage change model and the corresponding rigidity parameters year by year. It is important to note that, after applying the sample restrictions discussed in section 2, we do not eliminate any outliers from the sample, neither in wage levels nor in changes. We let our model of measurement error deal with this problem.

The set of covariates used to estimate the notional wage change distribution varies in the two countries, reflecting differences in data availability. The Brazilian data are more comprehensive and allow us to control for gender, age, tenure and their quadratic terms, educational attainment, occupation and sector of operation, establishment size, type of labor contract, and location of the firm (in Belo Horizonte or outside the state capital). In the case of Uruguay, we control for gender, age, tenure and their quadratic terms, sector of operation, white or blue collar status, establishment size, and the location of the firm (in Montevideo vs. the rest of the country).

Table 2 reports maximum likelihood estimates of the notional wage change equation parameters and their associated z-statistics for Uruguay and Brazil in 1998-1999.[6] The estimated coefficients are highly significant in almost all cases. Some commonalities within the two countries emerge. As expected, individual wage growth declines non-monotonically with age, is higher in the capital, and tends to increase with establishment size, although wages in medium/large brazilian establishments (those with the 50 to1000 employees) display higher wage growth than the largest firms in the sample (those with 1,000 employees or more). Perhaps surprisingly, the effect of education and type of contract on individual wage growth in Brazil is not clear, as suggested by inconsistent signs across years, and often coefficients not significantly different from zero.

Figure 4 presents the wage changes predicted by the model, as well as the observed wage changes in 1997-1998. Panel A displays the corresponding distributions for Uruguay and panel B for Brazil. The predicted histogram is obviously smoother, but the overall fit of the model to the shape of the observed wage growth histogram is quite good. If anything, the model tends to underestimate the concentration of observations around the focal points of rigidity. In the case of Uruguay, we slightly underestimate the spike at zero, and overestimate the share of wage growth observations immediately below the focal point of real rigidity. In the case of Brazil, we slightly underestimate

[6] Results for the other years are qualitatively similar to those of 1998-1999, unless otherwise stated in the text.

the concentration of observations around the realized rate of inflation. Table 3 shows the median and standard deviations of the observed and estimated distributions in every year. With very few exceptions, the model fit is very close to the actual summary statistics. We pin down the median and standard deviations with a precision up to the second decimal.

Tables 4 and 5 summarize some other important parameter estimates from our model, including the mean wage changes in the observed (Δy_{it}^{o}) and notional (Δy_{it}^{*}) distributions, the estimated focal point of DRWR, the percentages of workers subject to DNWR (p_t^N) and DRWR (p_t^R), and the extent of measurement error in the data (q_t). Regarding the latter, our results indicate that the extent of measurement error in the data is quite stable over time in both countries. The extent of measurement error estimated by the model is larger in Brazil, where he average share of correctly reported hourly wage changes according to the model is 79 percent, against 91 percent in Uruguay.

4.2 DNWR and DRWR

We begin the discussion of our main results by assessing the impact of measurement error on the shares of nominal wage freezes and cuts. In line with our expectations, the share of wage cuts declines and the share of wage freezes increases after measurement errors have been dealt with, as the comparisons plotted in Figures 5 and 6 show for Uruguay and Brazil, respectively. The average share of wage cuts in the data is quite large: 16 percent in Uruguay and 18 percent in Brazil. However, these shares are almost halved, to 9 percent in Uruguay and 10 percent in Brazil, when measurement error has been taken into account. Over time, the share of wage cuts gradually increases in Uruguay (figure 5), in accordance with the disinflation process. However, the increase in wage cuts in Uruguay should not necessarily be interpreted as a sign of increased wage flexibility, inasmuch as wage freezes also rise over the same period -a feature consistent with growing importance of downward nominal wage rigidities. In the data, wage freezes move from 3.6 percent in 1996-1997 to 25 percent in 2002-2003. The raise is even more dramatic after measurement error has been accounted for, moving from 4.4 percent to 31 percent within the same period. In contrast with the case of Uruguay, we do not observe a clear pattern in the time evolution of wage cuts and freezes in Brazil (figure 6).

The incidence of the two rigidity regimes in Uruguay clearly illustrates that the macroeconomic environment plays a crucial role in the determination of wage rigidities, as highlighted by a clear regime shift in 1999-2000 (figure 7). In the fist years of our sample period, DRWR is very important, most likely as a result of a recent history of high inflation and widespread indexation in labor contracts. The share of workers in the real rigidity regime $\left(p^R\right)$ is 72 percent in 1996-1997 and 88 percent in 1997-1998. As inflation goes down, however, real rigidity rapidly declines. In 1998-1999, the share of workers subject to the real rigidity regime is 65 percent; this plummets to 12 percent the next year, and remains relatively stable below 8 percent, for the remainder of the period.

The shift away from DRWR as inflation declines in Uruguay is accompanied by a sharp rise in DNWR. During the first two years of wage changes, DNWR is very low according to our estimates: the fraction of workers subject to DNWR is just 12 percent. After a mild increase in 1998-1999, DNWR jumps to a new equilibrium in 1999-2000, when the share of workers subject to DNWR stabilizes at around 65 for the remainder of the period. However, the relationship between the two types of rigidities and inflation is more complex than one might initially suspect. When inflation peaks again in 2001-2002 and 2002-2003, the share of workers subject to DRWR remains low, while DNWR is still binding for a majority of the labor force ($p^N_{2001-2002} = 62\%$ and $p^N_{2002-2003} = 78\%$). Note that this is the same period in which the share of both wage cuts and freezes grows sharply in Uruguay. The rationale for this behavior must be found in the severe recession in Uruguay during these years. Uruguay enters into a long recession in 1998Q4 which reaches its trough in 2002Q3, after a temporary upswing between 2000Q2 and 2001Q3.[7] In 2002, the severe crisis that affected the economy forced the central bank to let the exchange rate float freely, putting an end to the use of the exchange rate as a nominal anchor. Hence, even in the presence of high inflation during these years, firms are in need of downward wage adjustments. Perhaps more importantly, 2003-2004 is a year of

[7] Peaks and troughs are identified using the Bry-Boschan quarterly (BBQ) algorithm (Harding and Pagan, 2002) on quarterly real GDP data for the period 1988Q1-2010Q3.

very strong recovery in economic activity (real GDP growth is 12 percent) and relatively high inflation (9 percent). Even in this context wage indexation does not reappears, and DNWR remaining as the only noticeable friction in wage setting ($p^N_{2003-2004} = 78\%$ and $p^R_{2003-2004} = 4\%$)

The results in Brazil are in sharp contrast with those of Uruguay. The first important difference is that DWNR is of second order importance in Brazil, while, as table 5 indicates, there is a greater degree of DRWR. The importance of wage indexation remains relatively stable throughout our sample period. On average, 43 percent of the formal workforce in Brazil is subject to DRWR, while 10 percent is subject to DNWR. During the first half of the sample period, the share of workers whose wage are governed by the DRWR regime slowly declines, from 48 percent in 1995-1996 to 31 percent in 1998-1999. In 1999-2000, there is a rapid raise in DRWR, with p^R reaching its maximum at 66 percent, but this is rapidly reversed in 2000-2001, when real rigidity declines again. We will come back to this rapid reversal when we discuss, below, the role of the estimated focal point in wage negotiations. As for DNWR, it only becomes a relevant feature of wage setting in 2000-2001, where p^N is estimated to be equal to 35 percent, compared to 8 percent in the previous period.

4.3 Monetary Policy Regime and the Indexation of Wages

The stability of wage indexation in Brazil hides an important regime change. Figure 8 shows the evolution of our estimated focal point of DRWR (r_{it}) and its variance, together with the CPI inflation rate and the increase in the minimum wage. In the first years of our sample period r_i traces the increase in the minimum wage almost perfectly. The importance of the minimum wage is so powerful as a focal point of wage negotiations that the estimated bounds around r_i, as measured by its variance, are virtually zero. It is only starting in 1999-2000 that our estimate of r_i follows the rate of inflation and not the growth of the minimum wage. Incidentally, this is the year in which the Brazilian central bank introduced inflation targeting. The movement away from the minimum wage as the sole price index in wage negotiations, and towards expected inflation, speaks in favor of the role of inflation targeting in anchoring inflation expectations.

Interestingly, after the introduction of inflation targeting, the confidence bands around r_i widen. A plausible interpretation of the increase in the bands around r_i is that they illustrate the dispersion in inflation expectations across agents. Indeed, after 1999-2000 our estimator centers the focal point of wage negotiations or the price index used for wage indexation at the realized rate of inflation, reinforcing this interpretation. However the increase in the variance of r_i allows indexation to be driven by both distortions around the rate of inflation, and distortions associated with the increase of the minimum wage. The growth of the minimum wage falls outside the estimated range of r_i only in 2000-01, a year in which wage rigidities are narrowly pegged to the inflation rate. It is probably for this reason that we fail to capture the full picture of real rigidity this year, as illustrated by the rapid drop in the share of workers subject to DRWR and the corresponding wage sweep up.

In contrast with Brazil, the focal point of DRWR in Uruguay (figure 9) is never related to changes in the minimum wage. During the first three years of the sample, when indexation is high, r_i appears to trace movements of the inflation rate, although the standard deviation is quite large, in the range of 3 orders of magnitude with respect to the post-inflation targeting period in Brazil. The rest of the period is characterized by a low incidence of wage indexation, and hence the focal point of DRWR bears less relevance.

The contrast in the focal point of wage negotiations between the two countries illustrates two approaches to dealing with the considerable uncertainty associated with a fairly high and volatile inflation environment. In Brazil, during the years that followed the stabilization plan, the great uncertainty regarding inflation arguably forced agents to index wages to changes in the minimum wage. The introduction of a credible inflation target in 1999 had a clear anchoring effect. Some agents changed their focus in the wage negotiations to expected inflation. The credibility of the target rate can be seen by the progressive narrowing of the bands around r_i. In Uruguay, the stabilization plan is introduced earlier, in 1991, and is followed by a gradual but steady reduction in inflation, although inflation remains fairly volatile during the sample period. Not having a meaningful price index to which to anchor their expectations, economic agents index their wages to their forecast of inflation, which are fairly disperse.

5 Worker and Firm Heterogeneity

Wage setting theories provide different predictions regarding the expected incidence of wage rigidity across types of workers and firms. In this section, we examine how the estimated individual probabilities of being subject to each type of rigidity vary with worker and firm characteristics, in an attempt to shed some further light on the functioning of labor markets in the two countries.

5.1 Brief Theoretical Discussion

Efficiency wage considerations of various kinds have been put forward to explain why firms might be reluctant to cut wages. If wage rigidities arise in order to limit worker shirking behavior (Shapiro and Stiglitz, 1984), or as a result of a firm's gift in exchange for higher worker effort (Akerlof and Yellen, 1990), the wages of a firm's core workers should be more protected from nominal or real wage cuts. Since the effort of white collar workers is both more difficult to monitor and most likely of higher importance for the productivity of the firm, these theories predict that the wages of white collar workers should be more rigid. An additional prediction of the shirking model is that wages would tend to be more rigid in larger firms, where monitoring is more costly. A similar prediction of higher rigidity for white collar workers would be obtained if efficiency wages are paid in order to detract from costly turnover (Stiglitz, 1974), as white collar workers are more difficult and expensive to replace. Additionally, this model would predict wage rigidity to be higher among younger workers, as their turnover rate tends to be higher. Insider-outsider theories (Lindbeck and Snower, 1986) also predict higher wage rigidity for white collars, but in contrast with the turnover model, these theories predict more rigidity for older, prime-aged workers. Additionally, wage rigidity should be higher among workers with permanent contracts and workers with higher tenure, as they represent the primary workforce. A similar prediction would be obtained regarding tenure if tenure were a proxy for firm-specific human capital, which is costly to replace.

5.2 Measurement: The Individual Incidence of Rigidities

Our model classifies individuals as belonging to one of three different regimes: DNWR, DRWR or flexible wages. Having discussed, in the previous section, the estimated shares of individuals in the two rigidity regimes throughout our sample period, we now discuss now the *actual incidence* of DNWR and DRWR, which depends on individual characteristics and the macroeconomic environment that governs wage setting. Intuitively, in a high-growth environment DNWR is less likely to be binding; i.e., firms are expected to require fewer wage cuts, so even those workers who are potentially subject to DNWR $\left(p^N\right)$ are likely to receive wage increases in accordance with their notional wage growth. Similarly, even if highly productive individuals belong to the DNWR regime, the probability of this regime being binding, i.e., the probability that their notional wage change falls below 0, should be low.

Denoting the macroeconomic environment by M_t and a set of observable worker and firm characteristics by X_{it}, we can define $p_{it}^N(X_{it}, M_t)$ and $p_{it}^R(X_{it}, M_t)$ as the individual probabilities of being subject to DNWR and DRWR, respectively in year t:

$$\begin{aligned} p_{it}^N(X_{it}, M_t) &= E\left[\Delta y_{it}^* < 0 | p_{it}^j = p_t^N\right] \\ p_{it}^R(X_{it}, M_t) &= E\left[\Delta y_{it}^* < r_{it} | p_{it}^j = p_t^R\right] \end{aligned}$$

Our purpose is to describe the differences across types of workers and firms in their exposure to both types of rigidities, not to estimate underlying parameters of the models discussed above. Since some of these characteristics are correlated among one another, we prefer to present differences conditional on covariates rather than simpler differences in means. Our estimated models include all the covariates used to estimate the notional wage distribution, as described in section 4.1, and two additional sets of dummy variables. Since we now pool all years in the estimation, we include time dummies to capture any macroeconomic effects associated with wage rigidities. Additionally, we include four indicator variables to capture the quartile distribution of worker wage levels on in the base year. Note that DNWR is, in principle, related to the expected wage growth of individuals, but not their *wage levels*. In contrast, to the extent that DRWR is related to increases in the minimum wage, as seems to be the case in the first years of the sample in Brazil, its effects are expected to be more visible for low-earning workers. However, wage indexation associated with the minimum wage often extends beyond those workers strictly earning the minimum -the so-called numeraire effect (Cunningham, 2007). Moreover, both DNWR and DRWR associated with price changes are more likely to be binding for high-wage workers if insider-outsider mechanisms are at play.

5.3 Results

Tables 6 and 7 show OLS regressions of the probability of being subject to each type of rigidity as a function of worker and firm characteristics in Uruguay and Brazil, respectively. In both countries, wage rigidity rises with age and tenure at a decreasing rate, over the study period, as evidenced by the positive signs of the main effects and negative signs of the quadratic terms. Comparing a 50-year-old worker with a worker aged 25, the estimated probability of being subject to DNWR (DRWR) increases by 0.5pp (2pp) in Brazil, and by 0.7pp (2.7pp) in Uruguay. The estimated results for tenure are similar across both types of rigidities in the two countries, suggesting a difference between a worker with 15 years of tenure in a firm and one with 5 years of tenure in the range from 0.9 to 1.6pp. Both of these features are in line with insider-outsider considerations. This evidence appears in contrast to previous evidence for the US, where in a survey of firm managers, Campbell and Kamlani (1997) find strong support for explanations of DNWR based on efficiency wage considerations and adverse selection, but little evidence in favor of the insider-outsider theory. Interestingly, gender, education, and type of contract seem to have little impact on the probability of being subject to either type of rigidity, as suggested by the small magnitude of the estimated coefficients. In Uruguay, white collar workers are more likely to be subject to DNWR, in line with most of the models discussed in this section. In contrast, occupational dummies in the case of Brazil have all very small effects.

There is a strong negative association between establishment size and the probability of being subject to either form of rigidity. In Uruguay, the smallest establishments present a probability of being subject to DRWR (DRWR) that is 6.7pp (11pp) larger than the reference category (establishments with more than a 1,000 employees). In Brazil, size effects in the case of DNWR are of fairly small magnitude. In contrast, the likelihood that a worker's wage is subject to DRWR in the lowest size group is about 6.5pp lower than if that worker is employed in one of the largest firms in the sample. This evidence appears to be against shirking models of efficiency wages, but is in line with estimates of DRWR in Belgium reported in Du Caju et al. (2007) and with the greater flexibility in compensation policies of larger firms found in surveys of wage setters in European firms (Babecký et al., 2009).

As expected, the wage level in the base year, as measured by the three wage quartile dummies, has little impact on the probability of being subject to DNWR. Perhaps surprisingly, however, we also find very little impact of the wage quartile dummies in the case of DRWR in Brazil, where a concentration of rigidity across low-earning individuals might have been expected, given the importance of the minimum wage. The main rationale for this result lies in the fact that some of the covariates included in the regression, most notably age, education, and occupation dummies, are capturing the wage level of workers. An unconditional regression of the incidence of DRWR on the quartile dummies yields a highly significant and sizable negative association: workers whose wage level is in the fourth quartile have a probability of being subject to DRWR that is 3.6pp lower than workers earning wages in the first quartile. If we concentrate on the pre-1999 sample, the period where the focal point of DRWR coincides with the growth of the minimum wage, this difference increases to 5.8pp. As before, similar unconditional regressions show no observable differences in DNWR across wage quartiles.

In the case of Brazil, we take advantage of the fact that we observe every formal worker employed by each of the establishments in the census to repeat the estimation, controlling for establishment by time fixed effects. Hence, we are effectively comparing workers employed in the same firm and year to identify the effect of covariates on the incidence of each type of rigidity. This should take care of the possibility that some of our results may be driven by worker selection into different types of firms, depending on the degree of wage rigidity operating. The results, reported in table 8, are qualitatively similar to those previously discussed, which suggests that the differences across workers highlighted here are not driven by worker sorting and firm heterogeneity.

6 Conclusions

Emerging countries have moved from high inflation regimes in the 1980s to relatively low and stable inflation rates since the second half of the 1990s. This paper assesses how this disinflation process has shaped wage setting in two of these emerging economies: Brazil and Uruguay. This investigation is important from a policy perspective. The natural response of wage setters to high inflation regimes is wage indexation. This form of wage rigidity is likely to translate into inefficient allocations, as relative wages are not allowed to adjust to firms' productivity developments. These efficiency losses are, in turn, likely to result in lower levels of welfare and higher unemployment. Hence, from both a macro and labor economics perspective, it is important to know how downward real wage rigidities have evolved as inflation has fallen.

Measuring downward wage rigidities at the individual level imposes serious data requirements. In this paper, we have measured the incidence and nature of wage rigidities in Brazil and Uruguay using high quality administrative data, but it is still possible that measurement error might result in an underestimation of downward wage rigidities. Our model helps to deal with this possible bias by jointly estimating the parameters governing measurement error and the different types of rigidities we are considering. The model contrasts the individual wage changes observed in the data with a notional wage change, which is the wage change that would have prevailed in the absence of wage rigidities. Importantly, we take advantage of the richness of our administrative data sets to estimate this notional wage change distribution year by year as a function of individual and firm characteristics.

Our analysis uncovers several important findings. First, although a non-trivial share of individuals experiences wage cuts in emerging countries, there is substantial evidence of wage rigidity, either in the form of resistance to nominal wage cuts, or in the form of wage indexation. Second, the nature of wage rigidities is quite different in Brazil and Uruguay. Downward nominal wage rigidity is virtually nonexistent in Brazil, but as inflation lowers it becomes very important in Uruguay, affecting more than 70 percent of the workforce by the end of the sample period.

Our third finding is potentially important for the modelling of wage setting. We find that the nature of wage rigidities changes dramatically in response to changes in the macro and policy environment. Hence, the two cases we study challenge the view that wage rigidities in the labor market are structural in the sense of Lucas (1976). In Uruguay, downward real wage rigidities in the first years of the sample are rapidly transformed into resistance to nominal wage cuts. Hence, the disinflation process had a positive impact on breaking with indexation. In Brazil, wage indexation is important and highly persistent, but the introduction of inflation targeting by the central bank in 1999 shifts the focal point of wage negotiations. During the first years of analysis, wage indexation is pegged to the growth of the minimum wage. After 1999, workers and firms start centering wage negotiations on the expected rate of inflation.

The reasons for the decline of wage indexation in Uruguay, and the persistence of the same phenomenon in Brazil remain issues for future research. As a point of departure, these patterns appear to be consistent with the different trends of unionization in the two countries over the same period. While we observe a decline in union density and a strong movement towards decentralization in wage negotiations in Uruguay, union coverage remains fairly high and stable in Brazil.

Theories of wage rigidity based on insider-outsider considerations predict that the wages of blue collars, young workers and workers with lower tenure exhibit less rigidity than primary workers, who have greater bargaining power to exert against wage cuts. Our results confirm these predictions and lend thus support to these theories.

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Tables and Figures

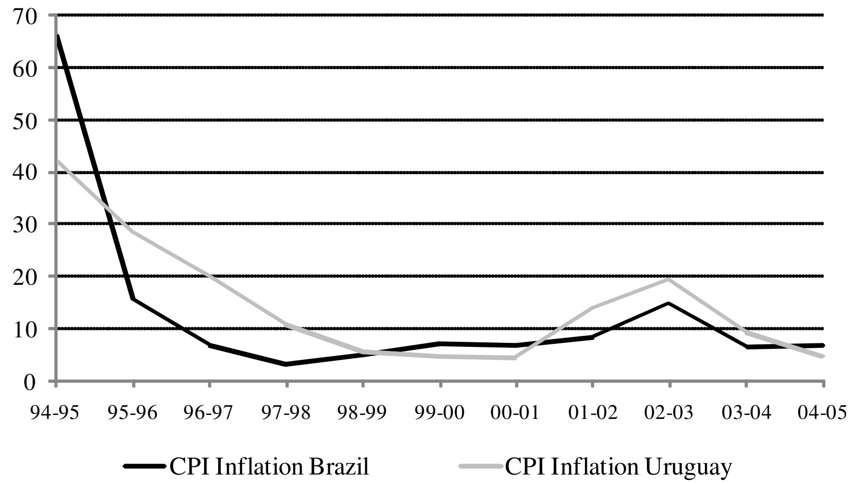

Figure 1: The evolution of inflation in Brazil and Uruguay. 1995-2005

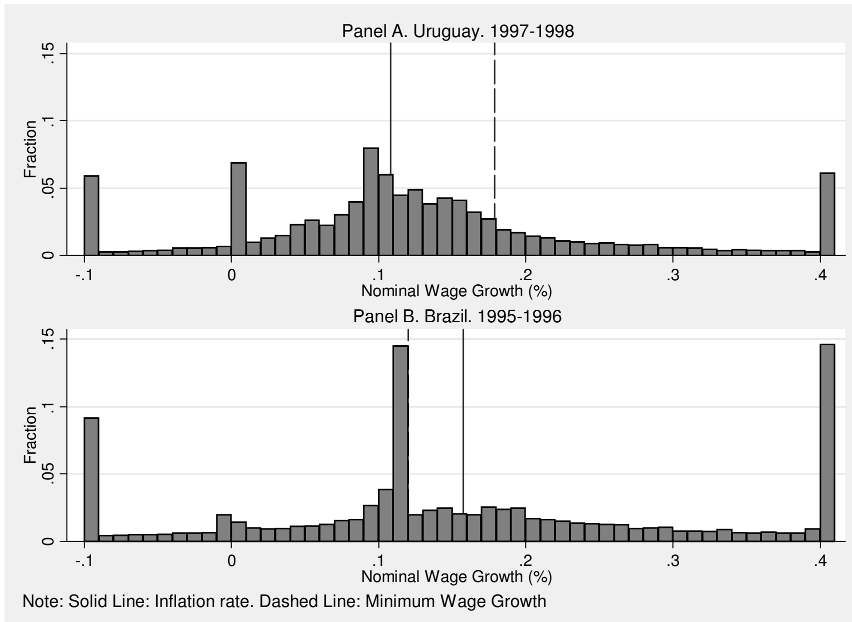

Figure 2: Histograms of Observed Log Hourly Wage Changes. Double-digit Inflation Years

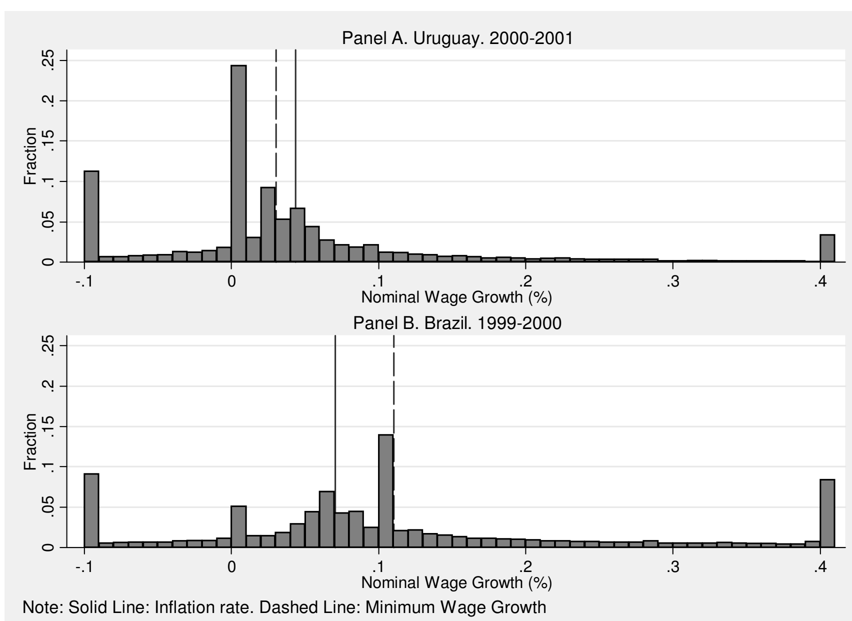

Figure 3: Histograms of Observed Log Hourly Wage Changes. Single-digit Inflation Years

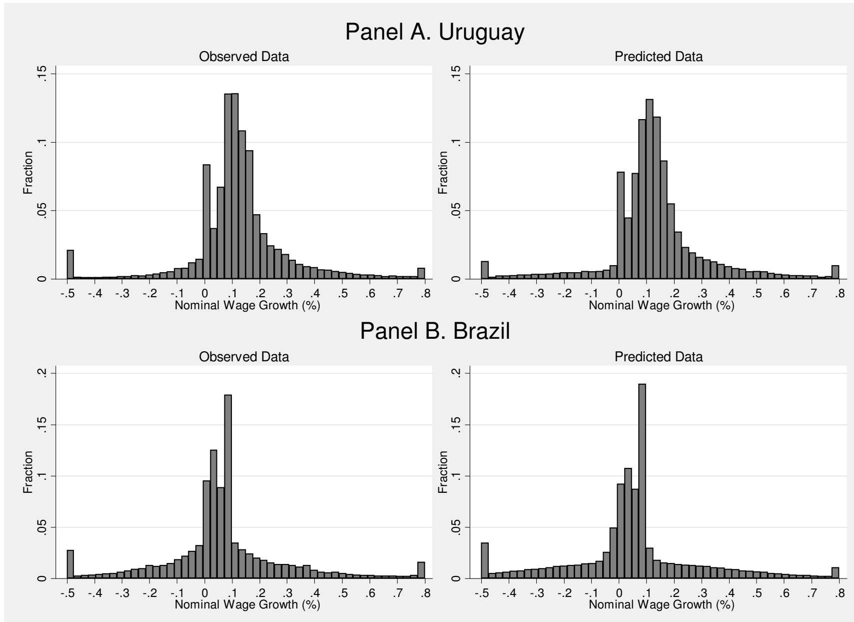

Figure 4: Observed vs. Predicted Log Hourly Wage Changes. 1997-1998

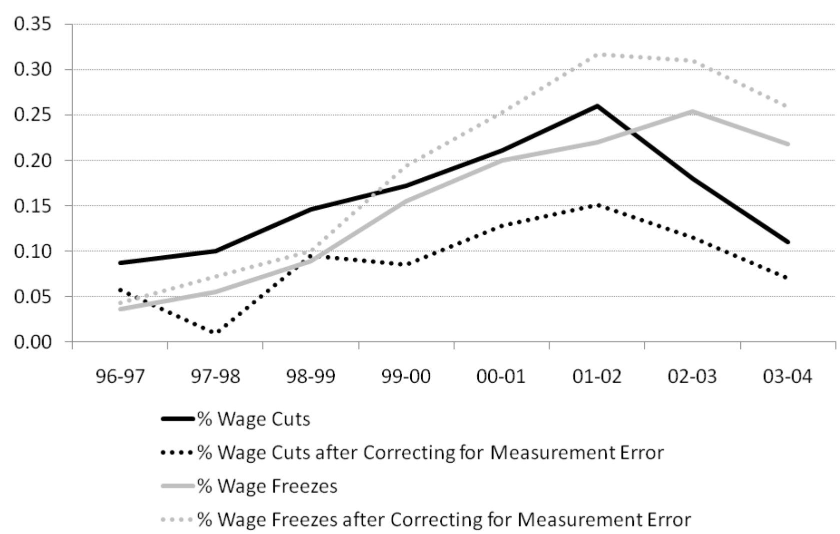

Figure 5: Wage Cuts and Freezes in Uruguay. The Impact of Measurement Error

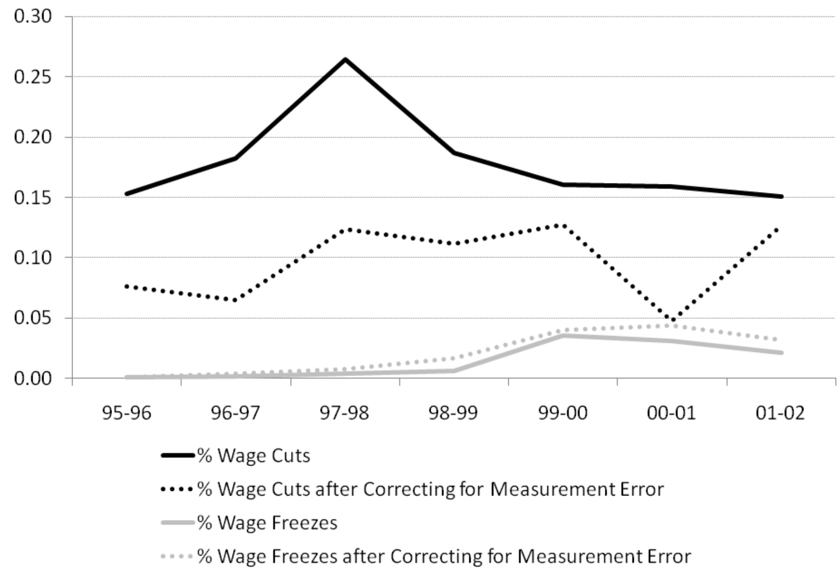

Figure 6: Wage Cuts and Freezes in Brazil. The Impact of Measurement Error

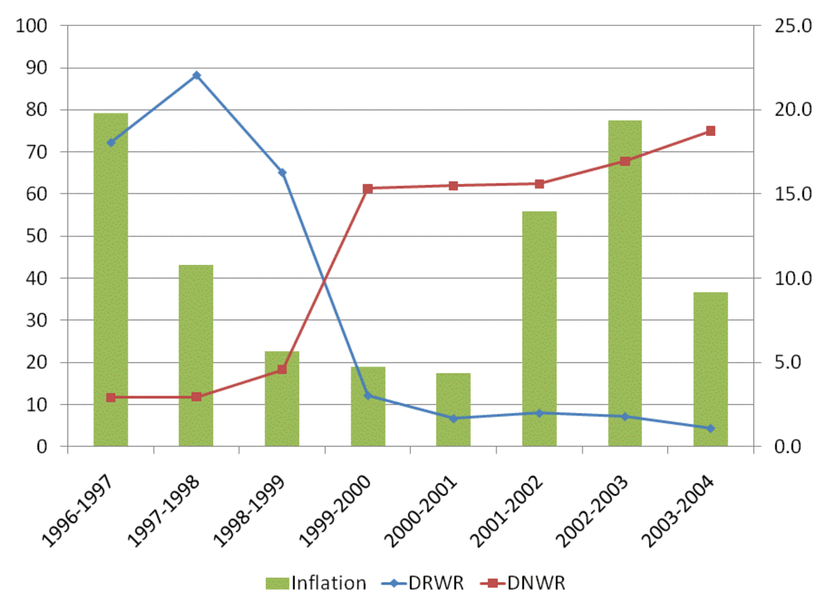

Figure 7: Rigidity Regimes and the Rate of Inflation in Uruguay

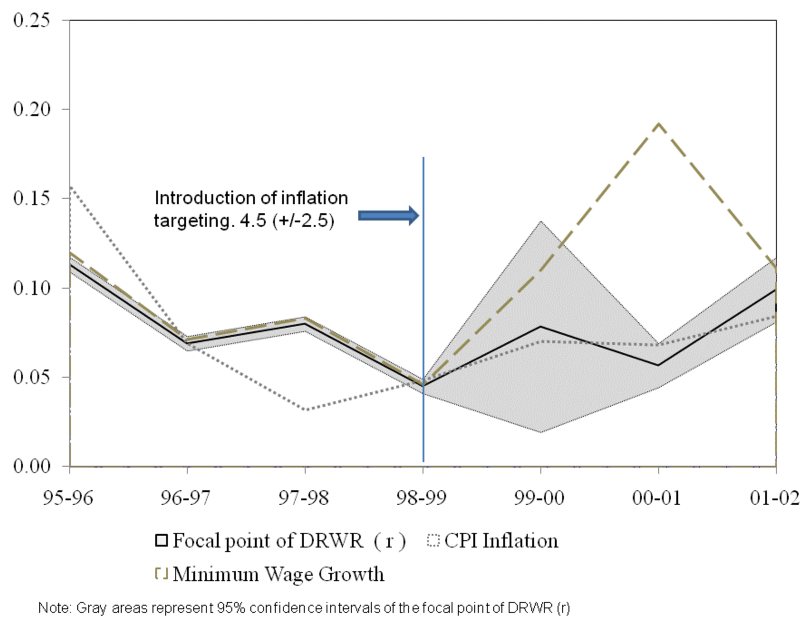

Figure 8: Estimated Focal Point of DRWR and the Rate of Inflation. Brazil

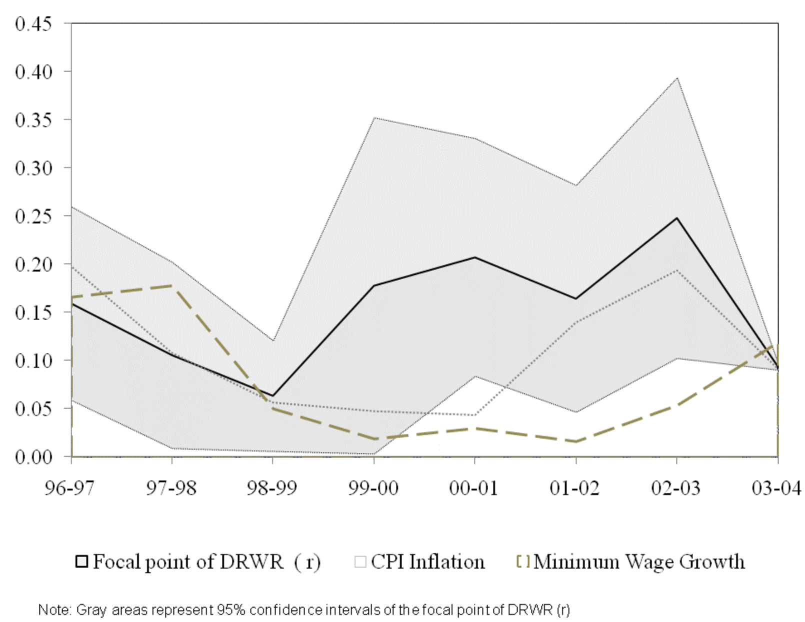

Figure 9: Estimated Focal Point of DRWR and the Rate of Inflation. Uruguay

Table 1: Summary Statistics. Belo Horizonte vs. Other Urban Areas. 1995-2000

	Belo Horizonte	**Other Urban Areas**
	(Averages)	
Age	33.7	35.4
Share of males	47.7%	46.9%
Share of Formal Workers	63.6%	63.9%
Years of education		
< 1	5.6%	6.1%
1 to 3	13.3%	12.1%
4 to 7	40.8%	36.0%
8 to 10	16.7%	18.2%
11 or more	23.7%	27.6%
Sector of Economic Activity		
Agriculture, hunting and forestry	0.9%	0.5%
Fishing	0.0%	0.1%
Mining and quarrying	0.6%	0.1%
Industry	16.1%	17.9%
Electricity, Gas and Water	1.0%	0.9%
Construction	9.5%	6.6%
Commerce	14.6%	15.8%
Hotels and restaurants	4.9%	5.2%
Transport	5.8%	6.0%
Financial intermediation	1.7%	2.5%
Real state, renting and related	0.8%	1.0%
Public administration and defense	4.7%	5.2%
Education	5.9%	5.7%
Health and social work	6.6%	6.9%
Other social activities	26.9%	25.7%
Extra territorial organizations	0.0%	0.0%

Table 2: Maximum Likelihood Estimates. Notional Wage Growth. 1998-1999

	Uruguay		**Brazil**	
	Coeff.	**z**	**Coeff.**	**z**
Age	-0.004***	-6.96	-0.001***	-7.94
$Age^2/1000$	0.032***	4.61	0.002***	4.33
Male	0.006***	3.10	-0.001***	-3.83
Tenure in months	-0.001***	-13.22	0.000***	4.94
$(\text{Tenure in months})^2/1000$	0.001***	7.74	-0.000***	-2.75
Establishment size:				
0-5 employees	-0.116***	-23.55	-0.011***	-26.08
5-15 employees	-0.091***	-20.31	-0.007***	-18.57
15-30 employees	-0.082***	-16.95	-0.005***	-11.56
30-50 employees	-0.047***	-9.63	-0.001***	-1.28
50-100 employees	-0.018***	-4.17	0.004***	9.28
100-200 employees	-0.022***	-4.68	0.004***	9.69
200-500 employees	-0.033***	-7.30	0.005***	13.28
500-1000 employees	-0.009**	-2.14	0.009***	18.90
Montevideo	0.009***	4.00	-	-
Belo Horizonte	-	-	0.004***	18.23
White collar worker	0.008**	2.41	-	-
Occupation dummies	-	-	yes	
Temporary contract	-	-	-0.002	-1.46
Education:				
Primary	-	-	-0.001***	-3.81
Secondary	-	-	0.000	0.06
N. Obs.	71,539		1,005,791	

Note: The reference groups for establishment size and education are more than 1000 employees and tertiary education, respectively. Additional control variables are 9 sector dummies for Uruguay and 14 sector dummies and 9 occupation dummies for Brazil. Levels of statistical significance: *** denotes significance at the 1-percent level; ** at the 5-percent level; and * at the 10-percent level.

Table 3: Observed vs. Simulated Wage Change Distributions

	Actual wage growth		Simulated wage growth	
	Median	Sd. Dev.	Median	Sd. Dev.
Year	**Uruguay**			
1996-1997	0.175	0.334	0.176	0.308
1997-1998	0.113	0.197	0.116	0.198
1998-1999	0.068	0.294	0.068	0.276
1999-2000	0.033	0.300	0.033	0.271
2000-2001	0.025	0.319	0.019	0.287
2001-2002	0.004	0.340	0.005	0.316
2002-2003	0.010	0.260	0.018	0.244
2003-2004	0.057	0.264	0.054	0.248
	Brazil			
1995-1996	0.134	0.292	0.140	0.287
1996-1997	0.077	0.282	0.075	0.274
1997-1998	0.055	0.265	0.052	0.254
1998-1999	0.045	0.281	0.045	0.262
1999-2000	0.094	0.267	0.090	0.263
2000-2001	0.100	0.248	0.101	0.240
2001-2002	0.104	0.248	0.103	0.243

Table 4: Downward Nominal and Real Wage Rigidity in Uruguay. Parameter Estimates

Year	Δy_i	Δy_i^*	r_i	p^R	p^N	q	N
1996-1997	0.182	0.072	0.160	0.722	0.117	0.952	52,222
1997-1998	0.120	-0.064	0.105	0.882	0.118	0.876	70,254
1998-1999	0.069	-0.018	0.063	0.651	0.183	0.933	71,556
1999-2000	0.036	0.024	0.178	0.122	0.613	0.898	69,352
2000-2001	0.015	0.014	0.207	0.067	0.620	0.899	68,149
2001-2002	-0.016	-0.001	0.164	0.080	0.624	0.865	66,525
2002-2003	0.041	0.009	0.248	0.072	0.678	0.907	63,996
2003-2004	0.088	0.0490	0.084	0.044	0.750	0.923	63,497

Note:This table displays the mean wage changes in the observed (Δy_i) and notional (Δy_i^*) distributions, the estimated focal point of DRWR (r_i), the percentages of workers in the DRWR (p^R) and DWNR (p^N) regimes, the share of wage change observations observed without error (q) and the number of observations (N) for each period.

Table 5: Downward Nominal and Real Wage Rigidity in Brazil. Parameter Estimates

Year	Δy_i	Δy_i^*	r_i	p^R	p^N	q	N
1995-1996	0.174	0.136	0.113	0.457	0.009	0.781	849,004
1996-1997	0.122	0.074	0.069	0.493	0.026	0.702	896,797
1997-1998	0.065	0.032	0.080	0.306	0.041	0.688	985,980
1998-1999	0.078	0.040	0.045	0.312	0.091	0.742	1,005,791
1999-2000	0.113	0.007	0.078	0.659	0.082	0.937	1,042,319
2000-2001	0.126	0.101	0.056	0.271	0.351	0.793	1,085,804
2001-2002	0.126	0.064	0.099	0.530	0.093	0.890	1,199,888

Note:This table displays the mean wage changes in the observed (Δy_i) and notional (Δy_i^*) distributions, the estimated focal point of DRWR (r_i), the percentages of workers in the DRWR (p^R) and DWNR (p^N) regimes, the share of wage change observations observed without error (q) and the number of observations (N) for each period.

Table 6: The incidence of wage rigidity in Uruguay. Worker and Firm Characteristics

	Real Rigidity		**Nominal Rigidity**	
	Coeff.	**t**	**Coeff.**	**t**
Age	0.296***	20.12	0.078***	6.27
$Age^2/1000$	-2.540***	-14.34	-0.703***	-4.54
Male	-0.228***	-5.14	-0.155***	-3.99
Tenure in months	0.022***	25.26	0.005***	8.96
$(\text{Tenure in months})^2/1000$	-0.036***	-15.01	0.001	0.86
Establishment size:				
0-5 employees	6.715***	58.42	10.869***	133.16
5-15 employees	5.551***	51.78	12.125***	147.31
15-30 employees	3.708***	34.55	9.719***	124.30
30-50 employees	1.487***	13.25	6.293***	91.50
50-100 employees	0.021	0.17	3.151***	41.53
100-200 employees	0.671***	5.55	0.836***	8.15
200-500 employees	0.129	1.09	0.722***	7.95
500-1000 employees	-0.415***	-3.34	0.683***	7.89
Montevideo	-0.980***	-20.75	0.699***	15.34
White Collar Worker	0.117*	1.62	1.676***	21.70
Wage quartile				
Quartile 2	0.008	0.13	0.043	0.64
Quartile 3	-0.098	-1.26	0.199***	2.85
Quartile 4	0.115	1.37	0.054	0.68
N. Obs.	525,551		525,551	

Note: The reference groups for establishment size and wage quartile are more than 1000 employees and quartile 4, respectively. Additional control variables are year dummies and 9 sector dummies. Levels of statistical significance: *** denotes significance at the 1-percent level; ** at the 5-percent level; and * at the 10-percent level. T-statistics are clustered by individual.

Table 7: The incidence of wage rigidity in Brazil. Worker and Firm Characteristics

	Real Rigidity		**Nominal Rigidity**	
	Coeff.	**t**	**Coeff.**	**t**
Age	0.182***	189.85	0.049***	279.41
$Age^2/1000$	-1.317***	-102.33	-0.425***	-178.89
Male	-0.575***	-157.07	-0.101***	-140.70
Tenure in months	0.029***	309.77	0.005***	304.90
$(\text{Tenure in months})^2/1000$	-0.072***	-171.48	-0.011***	-158.67
Establishment size:				
0-5 employees	6.464***	758.57	0.216***	133.39
5-15 employees	4.146***	515.09	-0.054***	-34.43
15-30 employees	2.725***	332.97	-0.178***	-109.85
30-50 employees	1.740***	204.33	-0.253***	-148.10
50-100 employees	0.771***	96.21	-0.291***	-181.08
100-200 employees	-0.047***	-5.65	-0.244***	-139.41
200-500 employees	-0.114***	-14.29	-0.183***	-111.61
500-1000 employees	-0.225***	-24.04	-0.138***	-66.70
Belo Horizonte	-1.514***	-380.79	0.228***	281.71
Occupation Dummies	yes		yes	
Education:				
Primary	0.139***	18.70	-0.088***	-48.50
Secondary	-0.459***	-64.25	-0.165***	-91.28
Temporary contract	-0.006	-0.19	-0.304***	-52.46
Wage quartile:				
Quartile 2	-0.134***	-26.00	-0.031***	-33.11
Quartile 3	-0.063***	-12.16	-0.008***	-8.83
Quartile 4	-0.050***	-8.56	0.020***	17.66
N. Obs.	7,065,583		7,065,583	

Note: The reference groups for establishment size and education are more than 1000 employees and tertiary education, respectively. Additional control variables are year dummies, 14 sector dummies and 9 occupation dummies. Levels of statistical significance: *** denotes significance at the 1-percent level; ** at the 5-percent level; and * at the 10-percent level. Levels of statistical significance: *** denotes significance at the 1-percent level; ** at the 5-percent level; and * at the 10-percent level. T-statistics are clustered by individual.

Table 8: The incidence of wage rigidity in Brazil. Within Establishment Estimates

	Real Rigidity		**Nominal Rigidity**	
	Coeff.	**t**	**Coeff.**	**t**
Age	0.178***	494.20	0.048***	473.83
$Age^2/1000$	-1.233***	-261.91	-0.409***	-307.25
Male	-0.648***	-368.80	-0.110***	-221.14
Tenure in months	0.029***	1006.35	0.005***	597.55
$(\text{Tenure in months})^2/1000$	-0.071***	-666.76	-0.010***	-361.27
Occupation Dummies	yes		yes	
Education:				
Primary	0.285***	85.09	-0.062***	-66.30
Secondary	-0.427***	-135.39	-0.137***	-154.01
Temporary contract	0.421***	34.92	-0.183***	-53.78
Wage quartile:				
Quartile 2	0.010***	3.96	0.002***	2.59
Quartile 3	0.039***	13.84	0.006***	8.14
Quartile 4	0.100***	31.03	0.034***	37.48
N. Obs.	7,065,583		7,065,583	

Note: The reference group for education is tertiary education. The regressions include establishment by year fixed effects. Additional control variables are 9 occupation dummies. Levels of statistical significance: *** denotes significance at the 1-percent level; ** at the 5-percent level; and * at the 10-percent level.