DOES LABOR REGULATION IMPEDE ADJUSTMENT TO PRODUCT MARKET REFORM? EVIDENCE FROM BRAZIL

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Labor reform is at the top of the policy agenda in Latin America due to the alarming loss of jobs in the region in the wake of product market reforms. This paper investigates whether rigid labor laws have impeded adjustment in Brazil, and finds that they did not. Using a large, panel survey of Brazilian households from 1982-2002, the paper shows that (i) the relationship between formal and informal wages is cyclical. Informal wages fall more in recessions than formal wages; (ii) the relationship between formal and informal wages is *only* cyclical. There is no evidence of divergence over the longer term during which unemployment has been rising and employment rates falling. I interpret these facts in light of a model in which short-run contracting inefficiencies can thwart efficient factor allocation in the medium run. Indeed, the Brazilian context, with its thick informal labor market, provides a fertile testing ground for these models because they are implicitly based on the idea that employment protection and other match-specific costs can segment the labor market for extended periods of time. The evidence in this paper suggests that employment protection laws by themselves are unlikely to generate the kind of segmentation of the labor market that is currently attributed to them. Other impediments to factor mobility are more likely to explain persistent lack of jobs in Brazil.

1. INTRODUCTION

In 1987, more than 40% of prime-age males in São Paulo – Brazil's largest city – were employed in industry. By 1999, after a decade of tumultuous market reforms, that number was reduced by half, to 20%. Sixteen years after reforms were initiated, the employment rate of prime-age males has declined by 15 percent. Unemployment, which has historically been quite low in Brazil, rose steadily throughout the 1990s and exceeded 10 percent by the end of the decade. Employment in the informal sector grew an astonishing 80 percent between the late 1980s and the late 1990s, although it has recently leveled off.

Addressing Brazil's, and Latin America's, poor post-market-reform labor market performance is at the top of the policy agenda for the region (Williamson, 2003). The most

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popular candidate explanation for this unexpected outcome has been the much-maligned firing costs in the region. In an ambitious project, Heckman and Pagés (2003), and the authors therein, showed that costs associated with labor turnover in Latin America are on a par with many European countries and they use various instances of labor reforms as exogenous variation to provide evidence that many of Latin America's labor market institutions reduce efficiency and increase inequality throughout the region. Williamson and Kuczynski (2003), reflecting on why some of the expectations of the so-called Washington consensus have missed the mark, also suggest that restrictive labor market institutions may underlie adverse developments in the labor market.

Prohibitive firing costs discourage firms from hiring workers in the formal sector. Indeed, a report by the Inter-American Development Bank (2004) finds that virtually all job creation in the 1990s has taken place in the informal sector. However, the idea that firing costs have impeded *aggregate* job creation is more controversial. The "labor regulation" account of sluggish adjustment relies crucially on the notion that workers with formal contracts are able to resist pressure exerted by unemployed or informally employed outsiders, not only in the short run but in the longer run as well. This paper provides evidence that workers have no such power at longer than business cycle frequencies. Thus, the "labor regulation" account of sluggish medium-run adjustment is not very compelling, at least for Brazil.

Most of the recent theoretical advances on labor market institutions have developed from the literature on European unemployment. The traditional arsenal of theories of unemployment that rely on cyclical shifts in labor demand along a fairly elastic effective labor supply relation have never really fit the facts very well in Europe, especially beginning in the mid-1980s. Recent developments in this literature have begun to model contracting inefficiencies explicitly from micro-foundations and trace out their effects on macroeconomic outcomes. (e.g., Caballero and Hammour, 1996, 1997, 1998; Blanchard, 1997; Blanchard and Giavazzi, 2003). The gist of these models is that contracting inefficiencies, by enabling workers in jobs to resist adjustment in the short run, can hijack the adjustment process to such an extent that they may trigger technological substitution away from labor altogether. Once begun, such a mechanism could plausibly take quite a long time to reach equilibrium. An attractive feature of this type of model is that it seems to capture the political economy of labor reform quite well. In the long run, labor can only gain by ceding its share of the rents to production. But in the short run, workers in jobs would lose. Since these are the very workers who have the power to resist adjustment, labor reforms that unambiguously improve welfare in the long run are politically very difficult to pass in the short run. Caballero and Hammour (1996, 2000) have suggested that this same mechanism – that institutions that impede transactions between firms and workers can lead to inefficient restructuring of resources at the macroeconomic level – may lie at the root of the problem of economic development and may explain the difficulty many countries have experienced adjusting to market reforms.

Although not explicit in most models of this type, the argument is based on labor market segmentation. As such, it is easier to discern whether institutions impede adjustment in the Latin American context because economic activity in the informal sector is observed. This paper uses a a large panel survey of Brazilian households that has been collected continuously from 1982 to 2002 to investigate wage-setting over the business cycle and the longer run in the context of a labor market with a thick informal labor market. This period encompasses several business cycles and covers the period before and after market reforms. Unlike most papers about labor market segmentation, this paper is not concerned with obtaining an accurate point estimate of the "wage gap" at a given point in time. Instead, the paper investigates cyclical and structural movements in wages over time. In this sense, the paper has more in common with panel studies of real wage cyclicality in the U.S. (Bils, 1985; Solon, Barsky, and Parker, 1994). Since the data have a panel component, the paper is able to base its results on observed wage flexiblity of fixed groups of workers over time.

There are two main findings. The first is that the relative wage of informal workers is cyclical. Although it is certainly possible that this observed cyclicality is the result of unobserved differences across individuals in *changes* in productivity over the business cycle, it is also consistent with any theory in which labor market institutions insure workers against cyclical changes in demand. The second finding is that labor market segmentation is *only* cyclical. There is no evidence of diverging wages of formal and informal workers over time. This finding is inconsistent with the idea that labor market institutions have thwarted adjustment to reforms in Brazil in the medium run, and suggests that we must look to other impediments to factor mobility to explain the persistent lack of jobs.

The paper proceeds as follows. The next section outlines a simple theoretical model of adjustment in the presence of contracting inefficiencies. Section 3 discusses the data used for the empirical work and gives a brief overview of the major macroeconomic developments during the past two decades. Section 4 outlines the econometric strategy. Section 5 presents the main results of the paper, and Section 6 concludes.

2. MACROECONOMIC EFFECTS OF LABOR AND PRODUCT MARKET REGULATION

In this section, I use a simple model developed in Blanchard and Giavazzi (2003) which illustrates as transparently as possible the mechanism by which contracting inefficiencies may affect adjustment in the presence of product and labor market regulation. Although the model was designed with unemployment in mind, it actually has a more natural application to a labor market with formal and informal sectors. This is because adjustment to shocks occurs primarily via a change in the degree of segmentation in the labor market. Although the labor market can be segmented between employed and unemployed workers, it is easier to model segmentation in the context of informal employment where there is a clear marginal product of labor for both "insiders" and "outsiders."[2]

The model revolves around the distribution of rents between firms and their workers. In order to introduce rents into the story, the market for firms' output is assumed to be monopolistically competitive, which drives a wedge between the price of output and the marginal product of labor. The model derives interesting dynamics from a tension between a *share* of rents that must be paid to formal labor versus an *absolute* return that firms must make on their capital. If formal labor's share of the rents increases, the total size of the rents must rise sufficiently to restore firms' required return on capital. The only way for the total size of rents to increase is for formal labor's outside option – that is, the informal wage – to fall. Thus, to restore firms' profitability after a rise in the share of rents accruing to formal labor, informal employment must rise to increase the gap between formal and informal wages.

2.1. Production

Suppose that there are many firms in the formal sector, each of which has one unit of capital. Formal sector production, y, requires fixed proportions of formal labor with this unit of capital:

$$y = \min\{f, 1\}$$

Thus, the number of firms is equal to the capital stock, K, and f is equal to both the ratio of formal labor to capital ($fK/K = f$) and the amount of formal labor used by a firm.

A unit of capital costs c – assumed to be a shadow cost – and once purchased it loses its value outside the firm. Firms are monopolistically competitive, and face a downward-sloping

[2] In an earlier draft of their paper, Blanchard and Giavazzi think of unemployment as 'backyard production', which is analagous to informal employment here. Unemployment can be modeled explicity by adopting a search framework, but this complicates the model considerably. In that case, increasing segmentation of the labor market occurs by increasing the duration of unemployment. See Blanchard and Portugal (2001) and Caballero and Hammour (1997, 1998) for models along these lines.

demand curve[3]:

$$y = \frac{Y(P)}{K} p^{-\sigma}$$

where y is firm output, p is the relative price charged by a firm, and σ is the elasticity of substitution between goods, which is assumed not to vary with the number of goods. Y is total output, which is a function of the world price, P. Thus, when the relative price is one, firms split the market. Since firms face a downward sloping demand curve, they produce at a point where price exceeds marginal cost. Let μ_p be the markup of price over marginal cost, which is greater than one. In what follows, it will be more convenient to work with the excess of the markup over one, denoted μ:

$$\mu_p = 1 + \mu = \frac{\sigma}{\sigma - 1} \Longrightarrow \mu = \frac{1}{\sigma - 1}$$

Furthermore, we assume that the elasticity of substitution is proportional to capacity, reflected in the capital stock: $\sigma = \bar{\sigma}K$. Substituting in for the "net markup", yields:

$$\mu = \frac{1}{\bar{\sigma}K - 1} \tag{1}$$

2.2. Labor

The economy has a labor force of size L, which can work either in the formal sector, where it joins with capital, or in the informal sector where it produces by itself. Labor in the informal sector is paid its marginal product, which is decreasing in the amount of labor informally employed:

$$\frac{w_i}{P} = \theta \left(\frac{L_i}{L}\right)^b \tag{2}$$

Here, θ is a shift parameter, L_i is the amount of labor employed informally, and $b < 0$ is the elasticity of the informal wage with respect to the rate of informal employment. In the formal sector, workers bargain with firms over the surplus from joint production, and receive a share, β, of this surplus. The wage is not fully allocative because of the specificity of capital – which increases the surplus – and because labor laws give insiders some bargaining power (e.g., via turnover costs) – which increases workers' share of a given

[3] Blanchard and Giavazzi (2003) derive this demand curve from the following utility function:

$$V = \left[K^{-1/\sigma} \sum_{i=1}^{K} C_j^{(\sigma-1)/\sigma}\right]^{\sigma/(\sigma-1)}$$

This formulation differs from the standard Dixit-Stiglitz utility by the addition of the $K^{-1/\sigma}$ term in the brackets, which implies that utility does not increase with the number of firms.

surplus. Thus, formal sector workers receive, in real terms:

$$\frac{w_f}{P} = \frac{w_i}{P} + \beta\left(p - \frac{w_i}{P}\right) = \frac{w_i}{P} + \beta\left(\frac{(1+\mu)w_i}{P} - \frac{w_i}{P}\right) = (1+\beta\mu)\,\frac{w_i}{P}$$

The wage premium for formal labor is therefore:

$$\frac{w_f}{w_i} = 1 + \beta\mu \tag{3}$$

2.3. Equilibrium

All firms use the same production technology, which implies that they all charge the same price in equilibrium. Thus $p = 1$. This allows us to solve for the informal wage, which is inversely proportional to the markup:

$$p = 1 = (1+\mu)w_i \Longrightarrow w_i = \frac{1}{1+\mu} \tag{4}$$

In the long run, the supply of capital is perfectly elastic; firms will enter until their profits just cover the fixed cost of a unit of capital. Since there is only capital and labor, what is not profit must be wage. Thus, formal labor is paid the excess of the surplus from joint production over the fixed cost of capital.

$$w_f = 1 - c \tag{5}$$

Firms' profits are equal to the share of the surplus not paid to labor:

$$(1-\beta)(p - w_i) = (1-\beta)\left[(1+\mu)w_i - w_i\right] = (1-\beta)\mu w_i = \frac{(1-\beta)\mu}{1+\mu}$$

which must equal the fixed cost of capital:

$$\frac{(1-\beta)\mu}{1+\mu} = c$$

Substituting equation (1) into this expression allows us to solve for the equilibrium capital stock:

$$\bar{K} = \frac{(1-\beta)}{\bar{\sigma}c}$$

Similarly, substituting equations (1) and (4) into equation (3), we can derive an expression for the formal wage premium:

$$\frac{w_f}{w_i} = \frac{(1-\beta)(1-c)}{1-\beta-c}$$

Workers in the informal and formal sectors receive the same wage under two circumstances: when formal workers receive none of the surplus from joint production, and when there are no sunk costs to producing in the formal sector. Suppose that one of these conditions holds, so that the wage is the same in the formal and informal sectors. In this case, there are no distortions, and the model simply reduces to a standard two-sector fixed-factor model; arbitrage ensures that labor is efficiently allocated across sectors.

Using the informal sector marginal productivity condition (2) and the formal wage (5) we can solve for the equilibrium size of the informal sector in this case when $w_f = w_i$:

$$\left(\frac{L_i}{L}\right)^* = \left(\frac{1-c}{\theta}\right)^{\frac{1}{b}}$$

More generally, we can solve for the size of the informal sector in terms of the wage premium and the rate of informal employment associated with no wage premium:

$$\left(\frac{L_i}{L}\right) = \left(\frac{\theta/(1-c)}{w_f/w_i}\right)^{\frac{1}{b}} = \left(\frac{L_i}{L}\right)^* \left(\frac{w_f}{w_i}\right)^{-\frac{1}{b}}$$

Taking logs yields:

$$\ln\left(\frac{L_i}{L}\right) = \ln\left(\frac{L_i}{L}\right)^* - \frac{1}{b}\ln\left(\frac{w_f}{w_i}\right) \tag{6}$$

Equation (6) states that there are two pieces contributing to the overall rate of informal employment: one part corresponding to the rate of informality associated with full employment and another part associated with rationing of formal jobs.

Equation (6) is a long-run equilibrium condition[4], which implies that firms are receiving the required rate of return on their capital investments. This is depicted in Figure 1. There are two points to take away from the long-run equilibrium in this model. First, bargaining has no effect on the formal wage. Its only effect is to drive a wedge between the formal and informal wage and hence reduce the formal employment rate. The reason for this is that the supply of capital is infinitely elastic while labor is in fixed supply. Any attempts by labor to appropriate rents in this model are destined to fail. By implication, in the long run, labor would lose nothing by ceding its share of the surplus from joint production.

Second, the presence of an informal sector does not imply market distortions *per se*. Given the production technology, it may be more efficient to produce in the informal sector. To be sure, this may largely reflect the many non-turnover costs associated with formal labor – income and payroll taxes, costs of evasion, etc. The point is merely that, given these costs, production will take place where it is most efficient. Where there is not much

[4] The "long run" in this model is best thought of as the medium run. It does not necessarily satisfy the conditions for balanced growth.

to be gained from the division of labor – for example, street vending – informal contracts will predominate, even in the absence of any rationing of formal jobs induced by specificity in employment relationships (legislated or otherwise).

2.4. Adjustment to shocks

In the short run, firms may not be earning the required rate of return on their capital investments. The capital stock is fixed, and since employment and capital are combined in fixed proportions, so are formal and informal employment. Furthermore, since the relative productivity of formal and informal workers is determined by the level of informal employment, the markup is also fixed. However, firms may earn positive or negative profits in the short run if their share of the surplus is larger or smaller than the cost of entry. Over time, free entry of firms drives profits to equality with entry costs, which entails adjustments in employment. Thus, by tying employment to capital stock adjustment, changes in profitability can drive long-lasting changes in the labor market.

2.4.1. Labor Resistance to Productivity Shock

The idea that rigid labor laws are responsible for sluggish adjustment hinges on the idea that labor is able to resist changes in the economic environment in the short run. This situation is presented in Figure 2. In the short run, quantities cannot adjust so labor as a whole is better off – formal workers' wages do not fall and the informal sector is unchanged. However, this cannot be a long-run equilibrium because firms are earning negative profits. To restore profitability, firms reduce employment until the informal wage falls by the amount that formal workers' wages should have fallen. In the end, formal workers are no better off and informal workers are worse off.[5]

This simple setup makes it clear what is driving this result – it is the assumption of Nash bargaining. Formal workers and firms each receive a constant share of the surplus from joint production, regardless of labor market conditions. A smaller share going to firms implies that the surplus must rise correspondingly. It is worth noting that this is not simply a result of this simplified model. All models in this literature derive their main results from this same mechanism.[6]

The key insight from this model is that the degree of segmentation of the labor market is a sufficient statistic for the extent to which product and labor regulations are binding. Moreover, this is true in both the short run and the long run. Thus, the observation

[5] Caballero and Hammour (1997, 1998), Blanchard (1998), and Acemoglu (2003) offer explanations of European unemployment along these lines.

[6] Ironically, it is precisely the assumption of Nash bargaining that results in excessive wage flexibility in search models without turnover costs. See Hall (2003) and Shimer (2004).

of economic activity in both formal and informal sectors allows for a direct test of the plausibility of any given interpretation of labor market evolutions based on institutional factors. This fact motivates the empirical analysis below.

3. DATA AND BACKGROUND

3.1. The PME

The data used in this paper come from the *Pesquisa Mensal de Emprego* (PME). The PME is a large monthly employment survey administered by the *Instituto Brasileiro de Geografía e Estatistica (IBGE)*, Brazil's government statistical agency. The survey has been implemented in the six largest cities in Brazil – São Paulo, Rio de Janeiro, Belo Horizonte, Salvador, Recife, and Porto Alegre – since 1980. However, due to inconsistencies in the questionnaire prior to 1982, this paper uses only data for February 1982 until December 2002, a span of nearly 21 years. The questionnaire remained unchanged throughout this time.[7] In the late 1990s, a seventh city – Curitiba – was added to the survey, but I do not use these data. The six cities account for rougly three-quarters of Brazilian GDP and more than half of the nation's manufacturing employment.

The questionnaire is fairly typical of an employment survey. Information is collected from every member of the household over age ten regarding standard demographics (sex, age, education, but not race), employment, hours, income, industry, and a series of questions about job search if unemployed.

An important feature of the data is that it allows us to distinguish between formal and informal employment. The survey asks employed workers if they have a signed "work card". All workers are supposed to be guaranteed certain constitutional rights, and the enforcement of these rights occurs through the use of work cards which all Brazilians over the age of 15 have. Workers are supposed to have their work card signed by their employer, which registers the employment relationship with the labor ministry and entitles them to full protection of labor laws. It also makes both employer and employee liable to pay taxes. Overhead costs for formal workers typically range from 70-100% of the wage (Pastore, 1995). In practice, many workers work without getting their work card signed. Workers without a signed work card and self-employed workers are counted as informally employed.

Another very attractive feature of the survey is that it follows individuals over time. The survey has a rotating panel design similar to the Current Population Survey: households are interviewed once a month for four months, dropped from the sample for eight months, and then re-interviewed a year later for four more months. Thus, households are tracked

[7] It has recently changed.

over a period of 16 months. Households not found for a return interview are replaced with new households, so that sample sizes are practically constant within a given rotation group.

All together, the data contain upwards of 20 million observations (nearly 1 million per year). In the analysis below, I create two sub-samples of the data which I refer to as the "working sample" and the "panel". The working sample consists of all employed men, ages 20 to 60, with non-missing wage and employment data. This sample contains about 4.7 million observations on 1.3 million individuals. The analysis is limited to males because important changes in female labor supply were taking place throughout this period. The working sample is used for the cross-section regressions.

The "panel" consists of all men in the full sample who are observed in the same month in two consecutive years. This sample contains about 2.2 million observations of 444,000 individuals. The PME does not track individuals from odd to even years beginning in 1993-4, so no changes could be observed for these years, which accounts for much of the sample reduction. Table 1 presents summary statistics for both samples.

3.2. Background, 1982-2002

The 21 year period spanned by the data was an exceptionally tumultuous time in Brazil's (and most of Latin America's) history. The major macroeconomic developments have been discussed extensively elsewhere; I present an overview here to put the results that follow into context. The period can be roughly divided into four partly overlapping phases: the Latin American debt crisis and its aftermath (1982-5), a "muddling through" period (1986-1990), first-round market reforms (1987-93), and inflation stabilization and second-round reforms (1994 and after).

Figure 3 presents some macroeconomic and labor market aggregates to put this period in perspective. Although some important achievements were made in the 1990s – notably, the conquest of inflation – from the perspective of the labor market, this was a very difficult period indeed. Economic growth was sluggish and unstable, aggregate employment fell, unemployment rose steadily and the share of the labor force employed in the informal sector to unprecedented levels.

There were two sharp recessions in the period, one in the aftermath of the debt crisis from 1982-4, and another after a failed attempt to stabilize inflation in the early 1990s. There were also two expansions. The first was associated with the significant, though short-lived, stimulus provided by the so-called Cruzado plan in 1986, which was Brazil's first "heterodox" attempt at stabilizing inflation by means of direct price controls. The second was associated with the *Real* plan in 1994, which successfully put an end to Brazilian hyperinflation and sparked an enormous consumption boom. The years after each of these expansions, however, was marked by relative stagnation. During the late 1990s, Brazil experienced a few quasi-recessionary years due to spillover from the East Asian financial crisis and, later, from the Argentine crisis, but Brazil emerged from both relatively unscathed. Growth has thus been both precarious and resilient in recent years.

This has also been a period of major structural change in the Brazilian economy, which appears to be the major source of trouble for the labor market. Figure 3b displays an impressive decline in employment rates of prime-age males, which coincided with first-round market reforms in the late 1980s. These included primarily trade liberalization as well as some early privatization of government enterprises. Other authors have linked industry-level changes in external tariffs in Brazilian manufacturing to declining employment and rising productivity at the firm level (Hay (2000), Muendler (2003)). These papers attribute a large productivity response to the reforms. In the first few years, output and employment fell, but after 1992, productivity increases outpaced employment declines so that output began to rise. That the same is true of *aggregate* employment is less well-known, and is surprising. Nations have more flexibility in changing the bundle of goods they produce than individual firms. To the extent that falling employment is attributable to changes in demand, this suggests that Brazil as a nation has had trouble changing its output mix. Structural changes appear to have swamped cyclical changes in the 1990s. Although there was a discernible rise in employment associated with inflation stabilization in the mid-1990s, this was little more than a blip on a persistent downward trend.

Declining employment was accompanied by rising unemployment especially in the latter part of the decade (Figure 3c). Historically, Brazil has had quite low unemployment rates, but by the end of the 1990s, unemployment exceeded eight percent in many cities. These rates are on the low end of what are considered reasonable estimates in Brazil. Some surveys record open unemployment in excess of 20 percent.

The persistent decline in employment was also matched by an equally impressive rise in the share of employment in the informal sector. Figure 3d plots the share of prime-age males working without a formal labor contract or self-employed, indexed to the first quarter of 1989. Part of the rise in informality was the mechanical result of declining manufacturing industries in which formal labor contracts predominate. To account for the compositional change in the structure of industry, as well as potential changes in the human capital attributes of the work force over the period, I applied OLS to a regression of a dummy variable indicating informality on a series of 47 industry dummies, 18 dummies for single years of education, 4 age group dummies, and a time series of dummies for each quarter in the sample period. The coefficients from the time dummies from this regression, indexed to the first quarter of 1989, are also plotted in Figure 3d. This figure shows an abrupt, persistent, expansion in the informal sector beginning in the late 1980s. About two-thirds of this expansion occurred *within* industries. In fact, informality rose within every single 2-digit industry over the period. Appendix Table 1 shows the proportion of employment in each industry accounted for by informal workers in 1986 and 1999.

4. EMPIRICAL FRAMEWORK

The following statistical model of formal and informal wages is convenient for understanding estimation issues and the potential for panel data methods to overcome some of them. Wages are modeled as a function of fixed and time-varying attributes, an indicator of the business cycle, and a polynomial time trend. In addition, the time trend and the cycle indicator are interacted with an individual's formal or informal work status:

$$\begin{aligned} w_{it} \quad = \quad & \alpha_0 + \alpha_1'\mathbf{t}_p + \alpha_2(U_t - \phi'\mathbf{t}_p) + T_{it}\{\alpha_3 + \alpha_4'\mathbf{t}_p + \alpha_5(U_t - \phi'\mathbf{t}_p)\} \\ & + \gamma_1 X_{it} + \gamma_2 X_{it}^2 + \gamma_{3t}' Z_i + \varepsilon_{it} \end{aligned} \tag{7}$$

Here, w_{it} is the log of the real wage of person i in time t, U_t is a business cycle indicator such as the unemployment rate, $\mathbf{t}_p$ is a polynomial time trend vector of order p, T_{it} is a dummy variable indicating informal work status, X_{it} is an individual's age, Z_i is a vector of fixed individual characteristics, and ε_{it} is a random error term. The time trends are included to distinguish between cyclical and structural changes in wages and unemployment.

Apart from the interaction terms, this specification is similar to one used by Bils (1985) and Solon, Barsky, and Parker (1994) in their studies of real wage cyclicality in the U.S. The focus of these papers was to account for a countercyclical bias in estimates of the cyclical real wage elasticity based on aggregate wage series. If the second line of the equation, averaged over all individuals, varies over time in a way that is correlated with the business cycle, then a cyclical elasticity estimated from aggregate wage series will be biased. Specifically, these papers show that since low-quality workers (for whom the value of $\gamma_1 X_{it} + \gamma_2 X_{it}^2 + \gamma_{3t}' Z_i$ is low) make up a larger share of the sample in expansions, the bias is countercyclical. Inference based on aggregate wage series therefore gives a very misleading picture of real wage cyclicality.

In the present context, the possible effects of composition bias are more complicated. As with the U.S. studies, there is likely to be an overall countercyclical bias since total employment rises in expansions and the distribution of characteristics of the whole population does not change much from year to year. Here, however, we must also worry about how the work force is selected into the informal sector from year to year. If selection into the informal sector is non-random and varies systematically over the business cycle or over time, then applying OLS to equation (7) will produce biased estimates of the cyclical elasticities and time trends. For example, if the informal sector is capable of absorbing marginal workers in recessions who would otherwise be unemployed, failure to control adequately for compositional changes in each sector will induce a *pro*cyclical bias on the cyclical elasticity of informal wages relative to formal wages.

One could imagine comparing the wages of a fixed group of formal and informal workers over a long period of time as a potential fix for this problem. Even if it were impossible to assign people randomly to each group, it would require a much shorter leap of faith to base inference about changes in wages over time on this sample since the composition of each sector would not change at all. This would still fall short of a perfect experiment because the characteristics of each group could be correlated with changes in productivity over time that are not the result of the type of labor contract *per se*.

From the perspective of equation (7), since the Z_i are likely to contain unobserved determinants of productivity, the panel aspect of the data can be used to "difference out" individual fixed effects. For the moment, it is convenient to assume that $\Delta T_{it} = 0$, so that individuals do not make transitions between the formal and informal sector. In this case, first-differencing equation (7) yields:

$$\begin{aligned} \Delta w_{it} = \beta_0 + \beta_1 \mathbf{t}_{p-1} + \beta_2 \Delta U_t + T_{it}\{\beta_3 + \beta_4 \mathbf{t}_{p-1} + \beta_5 \Delta U_t\} \\ + \gamma_2(2X_{it} - 1) + \Delta\gamma_{3t}' Z_i + \Delta\varepsilon_{it} \end{aligned} \tag{8}$$

where $\beta_2 = \alpha_2$ and $\beta_5 = \alpha_5$. Since the differenced equation is based on observed wage changes for a fixed group of individuals across years, unobserved determinants of productivity (e.g., motivation and ability) that are likely to be correlated with the business cycle are eliminated from the equation. Equation (8) will nevertheless yield inconsistent estimtates of the cyclical elasticities and time trends if unobserved determinants of wage *growth* are correlated with business cycle conditions and time. In terms of equation (8), a necessary condition for this to be true is that some of the elements of $\Delta\gamma_{3t} \neq 0$. Equation (8) also makes clear that this becomes more of an issue when the panel is unbalanced. Even though the sample is fixed for any given consecutive pair of years, the sample changes at longer than one-year horizons so that $\Delta\gamma_{3t}' Z_i$, averaged over all individuals, may change from year to year as well. Thus, the composition bias story also applies to the panel regression, only with less force since unobserved determinants of wage growth are likely to be of second order importance compared to unobserved determinants of wage levels. Apart from obtaining a balanced panel over the entire period, one way of gauging the importance of this type of bias, which is pursued below, is to see whether controlling for fixed characteristics that we do observe (e.g., education) changes the estimates of the cyclical and time elasticities from the panel regressions.

Further, as was discussed above, if selection into informal work is correlated with wage growth (e.g., if low wage-growth individuals tend to work informally) in a way that is correlated with the business cycle or time, the estimated cyclical elasticities and time trends may be biased. Thus, while it is undoubtedly useful to estimate wage flexibility with observed wage changes of individuals over time, this is not a cure-all empirical strategy. Nonetheless, it is worthwile to observe estimates from both the cross-section and the panel because the sources of bias in each case are different.

Finally, since U_t varies only over time, direct OLS estimation of (7) or (8) yields artificially low estimates of the standard errors for $\alpha_2 = \beta_2$ and $\alpha_5 = \beta_5$. Instead, I estimate these equations in two steps. In the first step, the wage is regressed on the individual-level variables and a vector of time dummies interacted with T_{it}. For the cross-section regression, this produces two time series of wages (one formal and one informal) that are, one hopes, purged of individual characteristics that are correlated with the business cycle and time trends. Depending on the specification, the time series may consist of 84 quarterly dummies or 21 year dummies. For the panel regression, the time dummies correspond to regression-adjusted yearly wage changes. In the second step, OLS is applied to the regression of each of these wage series on a cyclical indicator together with a time trend. This method yields identical estimates of the α's and β's in equations (7) and (8) with correct standard errors.

5. RESULTS

5.1. Cross-section Estimates

In practice, I apply OLS to the following regression in the first stage (dropping industry and city subscripts for simplicity):

$$w_{it} = \sum_{t=1982}^{2002} Y_{it}\{\delta_{0t} + ED_i'\boldsymbol{\delta}_{1t} + \delta_{2t}X_{it} + \delta_{3t}X_{it}^2 + \delta_{4t}T_{it} + f_j + f_c + \varepsilon_{it}\} \tag{9}$$

where i indexes individuals; Y_{it} are time dummies for each year of the survey; ED_i is a vector of dummies for 18 single years of education; f_j and f_c are 47 industry and six city fixed effects, respectively; and ε_{it} is a random error term.[8] The age-earnings profile is quadratic and allowed to vary over time. For the figures below, I interacted the informal dummy with quarterly instead of yearly dummies.

[8] The results are similar if separate dummies are included for the self-employed and informal employees. These workers have very similar wage distributions. For simplicity, we lump both types together into one category.

The coefficients of interest, δ_{0t} and δ_{4t}, are the time series of average log wages for formal workers and the log informal wage gap after controlling for industry and human capital controls. The time series of informal wages can therefore be constructed as $\delta_{0t} + \delta_{4t}$. In order to give an idea of the actual differences between formal and informal wages, Figure 4 plots the δ_{4t} from a regression on the combined sample of all the cities, first without controls, and then controlling sequentially for human capital and industry. For ease of exposition, I have omitted standard error bands. However, due to the very large size of the sample, these coefficients are quite precisely estimated. The standard error of the estimate of the informal wage gap for any given quarter is roughly .02. Figure 5 plots the separate formal and informal wage series from the regression with full controls, indexed to the first quarter of 1986.

The unadjusted wage gap is quite large, ranging from about -50% to -15%. Furthermore, human capital controls, which explain a very large share of the variation in wages in Brazil, have relatively little effect on the informal wage gap. Partly, this has to do with the fact that age and education work in opposite directions. Educated workers are far more likely to work formally, but older workers less so. Since age and education are both positively correlated with wages, including them in the wage regression has opposite effects on the estimated informal wage gap.

In contrast to human capital controls, which boost the R^2 up to nearly .5, industry controls explain much less of the variation in earnings but soak up much more of the variation in the informal wage gap. After inclusion of both human capital and industry controls, the informal wage gap varies between zero and -25%. Cross-industry movements represent a genuine form of arbitrage in the labor market, so it is not clear that industry fixed effects should be included in the model, especially to gauge real wage cyclicality. On the other hand, from the perspective of getting the level of the δ_{4t} right (to the extent that that is possible), within-industry differences in wages of formal and informal workers are probably more relevant because there are substantial differences in wages across industries that may or may not reflect unobserved productivity differences and that are correlated with informality.

The most striking aspect of Figure 4 is that the informal wage gap follows the business cycle quite closely. The downturns correspond to the recessions of 1982-4 and 1990-2. The upswings correspond to the two expansions in 1985-6 and 1994-5. Furthermore, at the peak of the expansions, the informal wage gap narrows to a precisely estimated zero. Although our empirical strategy does not address the potential bias in the *level* of the wage gap, it is nonetheless an interesting finding since this is exactly the condition that would be expected at full employment – that is, when the supply of unemployed workers is low relative to the number of job vacancies, formal jobs cease to be rationed. Figure 5 shows that the cyclicality of the informal wage gap occurs despite considerable cyclicality in formal wages. Formal and informal wages track each other quite closely, but formal wages are less flexible downwards in recessions.

To obtain estimates of the α's in equation (7), each of the wage series from the first-stage regressions are regressed on the unemployment rate and a cubic time trend. Table 2 presents the results from these regressions. Each set of three columns shows the cyclical elasticities and time trends for formal wages, informal wages, and the informal wage gap from a given cross-section wage regression. To show the impact of controlling for changes in the composition of observable characteristics on the cyclicality of wages, the results are shown for the unadjusted wage series, as well as regression-adjusted wages controlling for human capital variables and, finally, after including both human captial and inudstry controls. Since wages are typically highly serially correlated, it is preferable to estimate the second-stage in changes. However, the results are shown in levels because it is easier to interpret the time trend. (Results presented below show that the estimates of the cyclical elasticities are very similar whether or not the data are first-differenced.)

The regressions for the different wage series are remarkably similar. Comparing the unadjusted wage to the wage adjusted for the full set of human capital and industry controls, the point estimate of the cyclical elasticity falls from -.076 to -.066 for formal workers and from -.136 to -.111 for informal workers. The cyclical elasticity of the wage gap, which is just the difference between the two, falls from -.06 to -.045. Thus, a one percentage point rise in the unemployment rate is associated with a 4.5% fall in the relative wage of informal workers.

The fact that controlling for observable characteristics has so little influence on the estimates suggests that changes in the composition of the workforce are not a major source of variation over time in average wages of prime-age males. This is all the more true when we consider the fact that observables account for a very large share of the variation in log wages. In any given year, the R^2 rises from about 2% controlling for only informal work status to over 50% in the full regression.

Table 2 also shows no evidence of diverging trends in formal and informal wages over time. Although there are strong time trends in wage levels in each sector, the time variables are all insignificant in the wage gap regression. This result is robust to many variations in the time trend (not shown). The particular specification of the time trend affects the cyclical elasticity somewhat in the regressions for the formal and informal wage, but the point estimate for the wage gap is unaffected. There is therefore little evidence of a divergence between formal and informal wages over the sample period.

5.2. Panel estimates

The results of the preceding section suggest that omitting variables from the regression is a relatively minor crime from the perspective of measuring changes in wages over time, although it clearly affects measurement of relative wage levels. We now test for this directly by exploiting the panel aspect of the data. Since the data allow us to track individuals across years, estimating equation (9) in changes provides a partial fix for this type of bias. Abstracting from changes in industry, first-differencing (9) and adding and subtracting $\delta_{4t}T_{it-1}$ yields

$$\Delta w_{it} = \sum_{t=1983}^{2002} Y_{it}\{\Delta\delta_{0t} + ED_i'\Delta\delta_{1t} + \delta_{2t} + \delta_{3t}(2 \times X_{it} - 1) + \Delta\delta_{4t}T_{it-1} + \delta_{4t}\Delta T_{it} + f_c + \Delta\varepsilon_{it}\} \tag{10}$$

Theoretically at least, the level of δ_{4t} can be estimated from this regression by observing changes in wages of individuals moving between formal and informal contracts, but this strategy is likely to be very sensitive to deviations of the actual data generating process from the model as written. Changing informal contract status is likely to be correlated with changing jobs, and job tenure is something that we do not observe. Thus, it is unlikely that ΔT_{it} is uncorrelated with $\Delta\varepsilon_{it}$ in this case. A more robust alternative is to restrict the sample to "contract-stayers" ($\Delta T_{it} = 0$) and focus instead on estimating $\Delta\delta_{0t}$ and $\Delta\delta_{4t}$.[9] These estimates measure observed wage flexibility for a fixed sample of formal and informal workers across a given pair of years. We can then compare these changes with changes in the business cycle in the second stage. In order to gauge the potential importance of job-changing, the regression is run on three different samples: individuals who remain in the same industry and occupation, individuals who remain in the same industry, and all individuals matched across years. It turns out that this has virtually no effect on the estimates.

The full set of panel estimates of the $\Delta\delta_{4t}$ (the change in the relative wage of informal workers across years) is presented for each of these three regressions in columns 2 through 4 of Appendix Table 2. For purposes of comparison, the year-on-year change in the cross-section estimates with full controls is presented in column 1 of the table. The estimates are shown graphically in Figure 6 for each consecutive pair of years. Since the panel estimates are so similar, only the results of the third panel regression (column 4 in Appendix Table 2) are presented (with 95% confidence intervals) together with the change in the cross-section estimates (column 1). The PME does not track individuals from odd to even years beginning in 1993-4. There are therefore a total of 15 yearly changes observed in the panel.

[9] Specifications including contract-changers in the sample yielded virtually identical results for the $\Delta\delta_t$ coefficient.

Consistent with the view that compositional changes are not very important, the cross-section and panel results are quite similar. The panel estimates do show a smaller rise in relative informal wages in 1985-6, and substantially larger declines in 1987-8 and 1991-2. The point estimates from the panel regressions are quite precise. This is true even for the high-inflation years of 1989 and 1993, when measurement error in the dependent variable might have been expected to increase the standard errors of the estimates.

These results are borne out in the second stage regressions. Table 3 displays these results. The first set of three columns presents the coefficients from a regression of the time series of changes in formal and informal wages and the wage gap from the panel regression on the change in the unemployment rate and a quadratic time trend. In the second set of three columns, the first-differenced version of the final regression in Table 2 is presented, restricting the sample to the 15 year pairs for which the panel tracks individuals across years. The elasticity of the wage gap with respect to the unemployment rate is -.058 in the panel regression, compared to -.049 for the for the cross-section regression. The panel estimate is less precisely estimated, and falls just short of significance at the 5% level. The time trends in the wage gap are again small and insignificant.

Although the second stage estimates of the wage gap are very similar in the panel and cross-section regressions, the same is not true of the formal and informal wage series. The point estimates in Table 3 show quite a bit more wage flexibility with respect to the business cycle. For example, a one percentage point increase in the unemployment rate is associated with a 16% fall in informal wages in the panel regressions compared to an 11% fall in the cross-section. However, the reason for this discrepancy appears to be the change in the sample rather than the switch from cross-section to panel estimation. When the second set of regressions in Table 3 were re-estimated on the full sample of 20 yearly wage changes, the point estimates of the cyclical elasticities for formal and informal wages look like the corresponding estimates in Table 2. This suggests that the wage gap elasticity is robust to changes in the time period. It also suggests that measuring the cyclicality of real wages (as in Bils (1985) and Solon, Barsky, and Parker (1994)) is a much more fragile endeavor than measuring the cyclicality of relative wages. This underscores a point made earlier, that estimates of the cyclical elasticity of the formal and informal wage series is more sensitive to the choice of time trend than the cyclical elasticities of the informal wage gap.

The final set of three columns in Table 3 shows the second stage regression results corresponding to a first-stage in which education is omitted from the panel regression. The idea is to test whether unobservable "fixed wage-growth effects" might bias the cyclical elasticities and the time trends by omitting the observable fixed wage-growth effect (i.e., education) from the sample. These results are more similar to the first-differenced cross-section results than the panel regressions, although the cylical elasticities are quite similar in all cases.

5.3. Robustness

Finally, we present the results of a few sensitivity analyses of the cyclical elasticity estimates in Tables 2 and 3. First, the second-stage regressions were re-estimated at the city level instead of pooling all the cities together. Second, the regressions were run using a different indicator of the business cycle. The results are presented in Table 4.

Table 4 contains three sets of two columns, each corresponding to cyclical elasticities from Tables 2 and 3. The columns labeled "Aggregate" in the first row repeat the results from the corresponding sample in Tables 2 and 3. The columns labeled "City-level" present the corresponding elasticity when the second-stage regressions are estimated at the city level. To obtain these estimates, the first-stage regressions were re-estimated with formal×year×city interactions, which were used to derive six informal wage gap time series (one for each city). In the second stage, these were regressed on a business cycle indicator, together with city fixed effects and a city-specific time trend.

The city-level regressions would be expected to yield smaller estimates of the cyclical elasticities than the aggregate regressions for two reasons. First, to the extent that residents of a city can respond to local shocks by seeking job opportunities elsewhere, the wage elasticity with respect to local demand shocks would be smaller than the elasticity with respect to aggregate shocks. Second, it is well-known that local unemployment rates are measured with more error than aggregate unemployment rates and this tends to bias the coefficients downward. Consistent with this expectation, the city-level estimates in Table 4 are all somewhat smaller than the corresponding aggregate estimates, although the effects are generally quite small.

The second set of rows in Table 4 present the cyclical elasticities using the log of average per capita hours of work as an alternative indicator of the business cycle. Thus, the coefficient of 2.33 in the first column suggests that a cyclical increase of 10% in average hours worked is associated with a 23% rise in the relative wage of informal workers. The results are broadly consistent with the results using the unemployment rate.

6. IMPLICATIONS

The empirical results presented in this paper do not support the idea that labor market institutions are capable of segmenting the labor market for extended periods of time, at least in the Brazilian labor market. This is contrary to a strong presumption in academic and policy circles that labor market institutions – particularly those that introduce incomplete contract inefficiencies into transactions between workers and firms – are at the root of unexpectedly sluggish labor market adjustment to market reforms in Latin America. The theoretical framework developed above makes it clear that any explanation that relies on this type of mechanism should show up in the degree of segmentation of the labor market. Empirically, however, there is no evidence that labor market segmentation is anything but cyclical.

What accounts for the discrepancy between theory and evidence? The theoretical result that insiders with formal contracts can push for measures that have long-term effects on outsiders hinges critically on the assumption of Nash bargaining in employment relationships. To demonstrate this, it is useful to take the canonical model through its paces. A point of departure for most models of labor market fluctuations involves taking a stand on the how firms and workers decide to separate. Under one view, associated with Hall and Lazear (1984), Kahn and Huberman (1988), and Hall (1995), firms and workers are assumed to be able to shape employment bargains in advance to maximize the joint value of their match. Later, if conditions deteriorate, some separations will result that could have been avoided by renegotiating to a different wage. This view contrasts sharply with another, associated with Diamond (1982) and Mortensen (1982), in which firms and workers would not honor a commitment to a particular wage if both parties could be made better off renegotiating. Thus, a firm and worker will only stay together if they can agree on a wage. Since any wage in the bargaining set is acceptable to both firm and worker, the pair are assumed to divide the surplus according to some fixed bargaining share. This is the Nash bargaining assumption. Although separations are efficient, this type of arrangment is subject to a hold-up problem: anticipating that bargains will be renegotiated, the pair would be unable to invest efficiently in the match for fear of the other party appropriating part of the investment.

Although the framework developed in this paper is highly stylized, it implicitly takes the Diamond-Mortensen model as its point of departure (as do all models in this literature). To this framework is added the twist that firms are forced (presumably by organized labor) to pay a dissipative sunk cost specific to every match. In other words, it turns the hold-up problem on its head — rather than leaving match-specific investments to be (inefficiently) determined by firms and workers, it asks what happens in macroeconomic equilibrium when firms are forced to make a fixed match-specific investment. What comes out turns out to have a strong efficiency-wage flavor in the sense that workers must ultimately pay for the sunk cost imposed on firms in the form of a higher opportunity cost of working formally. Although the Nash bargaining assumption is critical to this result, it is essentially arbitrary. Regardless of the true nature of the bargaining process, the deeper point remains that it will show up in the degree of segmentation in the labor market.

The finding that labor market segmentation between formal and informal workers is cyclical is consistent with a variety of interpretations. In particular, it does not necessarily imply that formal jobs are rationed. For example, Munasinghe (2000) develops of model of on-the-job learning in which labor market equilibrium can sustain a variety of wage-tenure profiles. Competition drives the ex ante value of jobs to equality, but after a job has begun, the value of high-wage-growth jobs is higher than a comparable low-wage-growth job. Formal contracts may be a consequence rather than a cause of the hold-up problem in this case. In a similar vein, one very plausible explanation for the evidence in this paper is that formal contracts are a way for firms to insure workers' specific investments against business cycle fluctuations. Rather than a wasteful cost that cannot be "bonded away", the labor contract is itself the bond that allows firms and workers to overcome the hold-up problem. Under this interpretation, Brazil's rising informality may reflect the falling value of this insurance. On the other hand, the evidence in this paper is consistent with any theory in which formal workers are not easily replaced at shorter than business cycle frequencies. Examples would include the efficiency wage model of dual labor markets in Bulow and Summers (1986), implicit contract models (Azariadis, 1975), cyclical models with contracting inefficiencies (Acemoglu, 2001), and models of imperfect competition (Rotemberg and Woodford, 1991).

The fact that Brazil has experienced sluggish labor market adjustment at frequencies at which its labor regulations are non-binding points to other impediments to capital or labor mobility in the wake of market reforms. This remains an important topic for future research.

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Table 1. Sample Selection Criteria and Descriptive Statistics for Samples Used in the Paper

	"Working" Sample	"Panel" Sample
Sample Selection Criteria	Males, Ages 20-60, Employees or Self-employed in Private Sector	Males, Ages 20-60, Employees or Self-Employed in Private Sector, Observed in Adjacent Years
Descriptive Statistics		
Number of Observations	4,678,852	2,232,572
Number of Individuals	1,281,499	443,652
Sector Distribution of Employment		
Manufacturing	26.3	28.9
Construction	13.5	12.4
Merchandise/Sales	15.1	14.4
Services	45.1	44.3
Mean Education	7.0	6.8
S.D. Education	4.3	4.3
Mean Age	34.9	36.2
S.D. Age	10.3	9.9
Distribution Across Education-by-Age Groups		
Less than Primary		
Ages 15-24	2.3	1.5
Ages 25-40	8.3	8.5
Ages 40-60	7.9	9.0
At least Primary, Less than Secondary		
Ages 15-24	11.0	8.1
Ages 25-40	28.4	29.8
Ages 40-60	14.5	16.6
At least Secondary		
Ages 15-24	4.9	3.6
Ages 25-40	15.9	15.9
Ages 40-60	6.8	7.2

Source: Pesquisa Mensal de Emprego, 1982-2002

Table 2. Cross-Section Regressions, Second Stage: Cyclical and Structural Changes in Formal and Informal Wages and Informal Wage Gap, Brazil, 1982-2002, PME

	Source of Wage Estimate								
	Unadjusted			Human Capital Controls			HC/Industry Controls		
	Formal	Informal	Gap	Formal	Informal	Gap	Formal	Informal	Gap
Unemployment rate	-0.076 *	-0.136 **	-0.060 ***	-0.071 *	-0.126 **	-0.055 ***	-0.066 *	-0.111 **	-0.045 ***
	(0.044)	(0.055)	(0.015)	(0.038)	(0.049)	(0.015)	(0.037)	(0.045)	(0.012)
Time=Year-1982	-0.214 ***	-0.244 ***	-0.030	-0.216 ***	-0.237 ***	-0.022	-0.200 ***	-0.208 ***	-0.007
	(0.069)	(0.086)	(0.024)	(0.059)	(0.076)	(0.023)	(0.058)	(0.070)	(0.019)
$Time^2$	0.025 ***	0.030 ***	0.005 *	0.030 ***	0.034 ***	0.004	0.029 ***	0.031 ***	0.002
	(0.007)	(0.010)	(0.003)	(0.007)	(0.008)	(0.003)	(0.006)	(0.008)	(0.002)
$Time^3$	-0.001 ***	-0.001 ***	-0.0001	-0.001 ***	-0.001 ***	-0.0001	-0.001 ***	-0.001 ***	-0.0001
	(0.000)	(0.000)	(0.0001)	(0.000)	(0.000)	(0.0001)	(0.000)	(0.000)	(0.0001)
R^2	0.52	0.59	0.78	0.90	0.87	0.71	0.90	0.87	0.71
Sample Size	21	21	21	21	21	21	21	21	21

Notes: Significance: *** 1%, ** 5%, * 10%. Coefficients from second-stage regressions. The dependent variables are derived from year dummies and year*informal interactions in the first stage cross-section wage regressions. The wage series are regressed on an indicator of the cycle and a cubic time trend. Each set of three columns corresponds to a different first-stage regression, controlling sequentially for human capital and industry variables. Standard errors in parentheses.

Table 3. Panel Regressions, Second Stage: Cyclical and Structural Changes in Formal and Informal Wages and Informal Wage Gap, Brazil, 1982-2002, PME

	Source of Wage Estimate								
	Panel			ΔC-S (Panel Sample)			Panel (excl. education)		
	Formal	Informal	Gap	Formal	Informal	Gap	Formal	Informal	Gap
Δ Unemployment	-0.106 **	-0.164 ***	-0.058 *	-0.116 ***	-0.165 ***	-0.049 ***	-0.122 ***	-0.171 ***	-0.049 *
	(0.044)	(0.054)	(0.030)	(0.032)	(0.044)	(0.018)	(0.035)	(0.053)	(0.029)
Time=Year-1982	0.033	0.041	0.007	0.057 ***	0.060 **	0.003	0.035	0.044	0.009
	(0.027)	(0.033)	(0.019)	(0.020)	(0.027)	(0.011)	(0.022)	(0.033)	(0.018)
$Time^2$	-0.0013	-0.0016	-0.0003	-0.0026 ***	-0.0028 **	-0.0002	-0.0012	-0.0016	-0.0003
	(0.0013)	(0.0016)	(0.0009)	(0.0009)	(0.0013)	(0.0005)	(0.0011)	(0.0016)	(0.0009)
R^2	0.40	0.49	0.25	0.63	0.60	0.41	0.60	0.54	0.21
Sample Size	15	15	15	15	15	15	15	15	15

Notes: Siginificance levels:*** 1%, ** 5%, * 10%. Coefficients from second-stage regressions. The dependent variables are derived from year*informal interactions in first-stage wage regressions. The first set of three columns corresponds to panel regressioes: of wage changes controlling for age, education, and city and week of interview fixed effects in the sample of all observations observed in consecutive years without changing industry or occupation. The second set of three columns uses as the dependent varige cl the first-differenced wage series from the cross-section wage regression (with human capital and industry controls), and the sample is restricted to years for which panel data estiamtes of wage changes are available. The third set of three columns correspost-diff panel regressions of wage changes without controlling for education. These wage-change series are regressed on the change in the unemployment rae, time, and time squared. Standard errors in parentheses.

Table 4. Robustness Checks, Second Stage Regressions: Cyclical Elasticity of Informal Wage Gap, Brazil, 1982-2002, PME

	Source of Wage Gap Estimate					
	Cross-Section		Panel		π C-S (Panel Sample)	
Cycle Regressor	Aggregate	City-level	Aggregate	City-level	Aggregate	City-level
Unemployment rate	-0.045 ***	-0.040 ***	-0.058 *	-0.055 ***	-0.049 ***	-0.046 ***
	(0.012)	(0.005)	(0.030)	(0.011)	(0.018)	(0.006)
R^2	0.78	0.77	0.25	0.28	0.41	0.43
Sample Size	21	126	15	90	15	90
ln (Emp/Pop)*Avg. Hours	2.33 ***	2.02 ***	2.92 *	2.31 ***	2.80 ***	2.08 ***
	(0.43)	(0.20)	(1.61)	(0.57)	(0.90)	(0.36)
R^2	0.81	0.80	0.23	0.20	0.48	0.34
Sample Size	21	126	15	90	15	90

Notes: Siginificance levels:*** 1%, ** 5%, * 10%. Coefficients from second-stage regressions. (See notes from Tables 2 and 3.)

Columns labeled "Aggregate" are as in Tables 2 and 3. Columns labeled "City-level" present cyclical elasticity from regressions of city-specific wage gap series on city-specific cycle indicators, controlling for city fixed effects and city-specific time trends. The time trends were all statistically insignificant and were omitted.

Columns labeled "Panel" and "C-S (Panel Sample)" are estimated in first differences.

Figure 1. Long-run Equilibrium

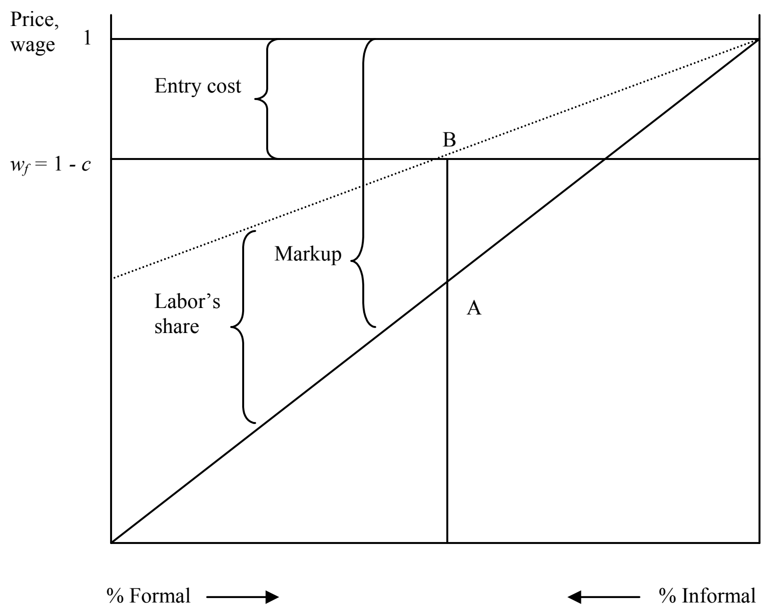


Figure 2. Labor Resistance to Demand Shock

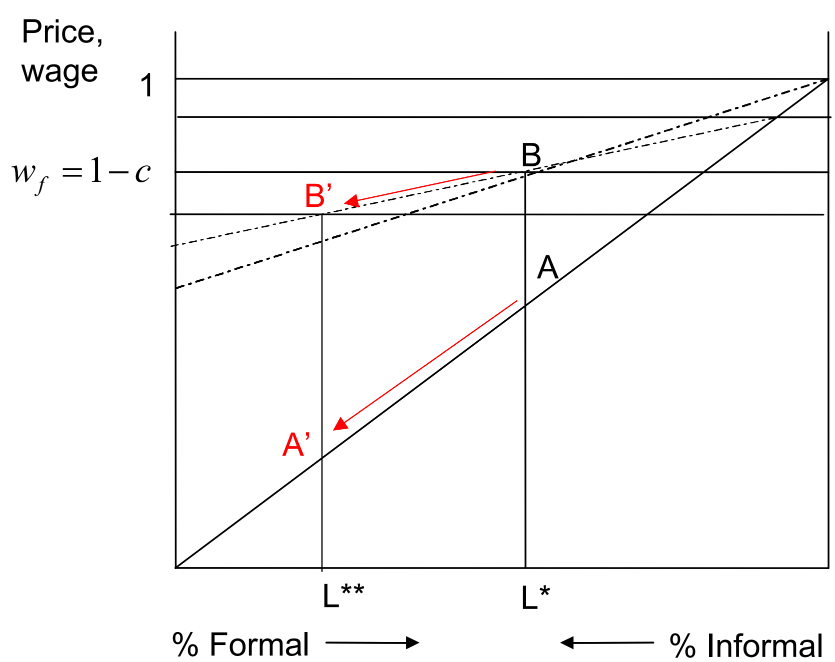


Figure 3. Labor Market Trends, 1982-2002, Brazil, PME

Figure 4. Cross-Section Estimates of Informal Wage Gap, Sequential Controls for Human Capital and Industry

Males Ages 20-60, 1982-2002, Brazil, PME, 3-Quarter Moving Average

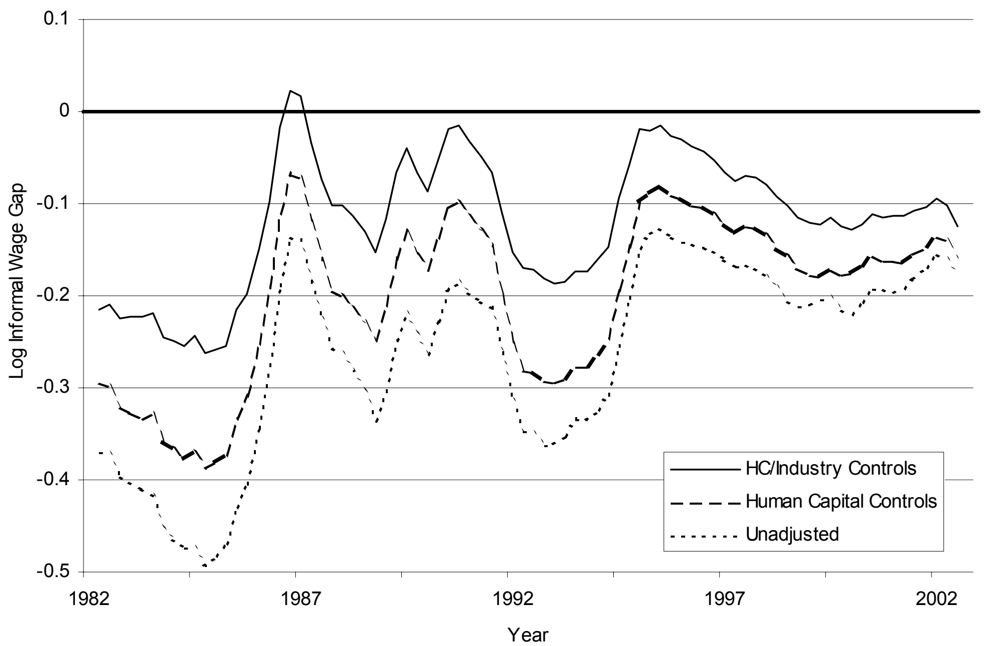


Figure 5. Log Wage Index of Formal and Informal Workers, Controlling for Human Capital and Industry

Males Ages 20-60, 1982-2002, Brazil, PME, 3-Quarter MA, Index[1986:1=0]

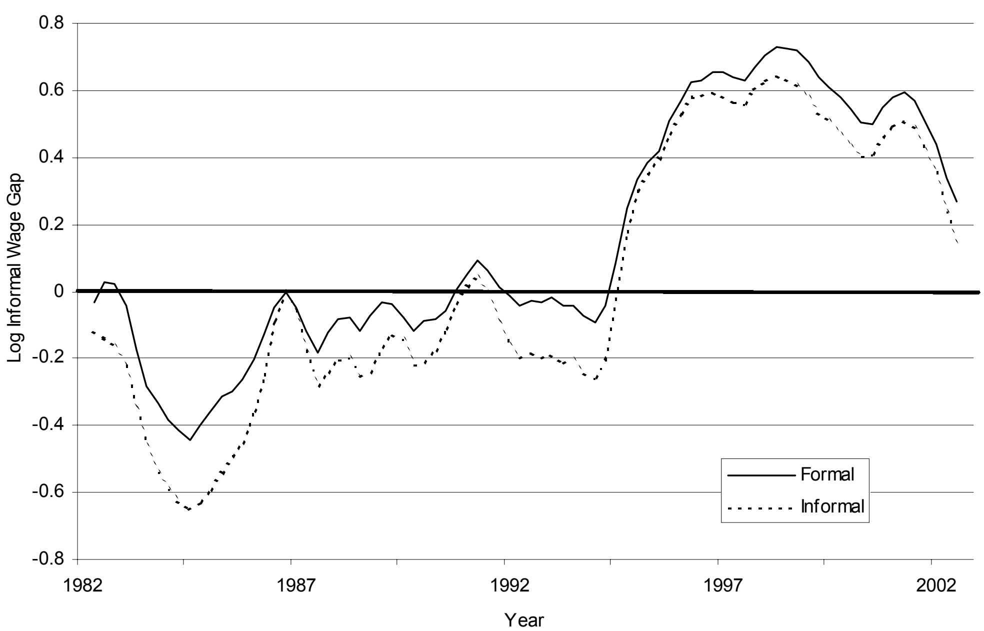
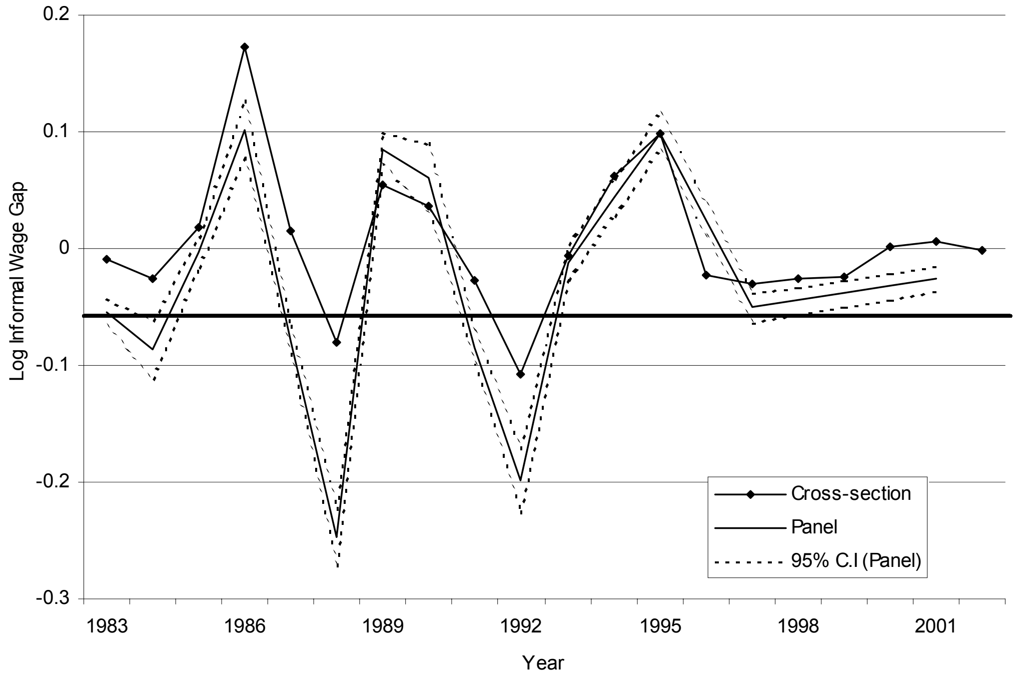


Figure 6. Cross-Section v. Panel Estimates of Wage Gap

Males Ages 20-60, 1982-3 to 2001-2, Brazil, PME



Appendix Table 1. Industry Informality and Employment Shares, Ranked by Proportion Informal in 1986, Selected Years

		Percent with Informal Labor Contract							Industry Share of Total Employment (Percent)						
Rank	**Industry**	**1986**	**1992**	**1995**	**1999**	$\Delta_{86,92}$	$\Delta_{86,95}$	$\Delta_{86,99}$	**1986**	**1992**	**1995**	**1999**	$\Delta_{86,92}$	$\Delta_{86,95}$	$\Delta_{86,99}$
1	Oil	0.66 %	0.75 %	2.14 %	7.29 %	0.14	2.24	10.01	0.09 %	0.07 %	0.04 %	0.03 %	-0.19	-0.59	-0.72
2	Metal Ore Mining	2.58	1.16	2.15	2.89	-0.55	-0.17	0.12	0.21	0.16	0.12	0.09	-0.21	-0.40	-0.56
3	Vehicle Manufacturing	2.59	2.86	3.87	8.78	0.10	0.49	2.39	2.11	1.62	1.67	1.21	-0.23	-0.21	-0.43
4	Petroleum-Based Products	2.75	3.58	7.04	6.33	0.30	1.56	1.30	0.37	0.33	0.23	0.23	-0.10	-0.38	-0.36
5	Communications	2.77	7.61	11.28	15.05	1.75	3.07	4.43	0.90	0.78	0.77	1.00	-0.13	-0.15	0.11
6	Financial Services	3.04	5.70	10.08	14.77	0.88	2.32	3.87	3.97	2.92	2.34	2.07	-0.26	-0.41	-0.48
7	Tobacco	3.06	1.35	3.07	11.01	-0.56	0.00	2.59	0.09	0.06	0.04	0.03	-0.33	-0.52	-0.68
8	Rubber Products	3.50	4.77	6.05	10.41	0.36	0.73	1.97	0.34	0.31	0.25	0.21	-0.07	-0.25	-0.37
9	Electronics/Electrical Products	4.00	6.93	9.73	18.12	0.73	1.43	3.53	1.41	0.93	0.80	0.74	-0.34	-0.43	-0.47
10	Chemicals	4.19	4.35	6.86	10.74	0.04	0.64	1.57	1.04	0.85	0.65	0.53	-0.18	-0.37	-0.49
11	Machinery/Tractors	4.80	9.44	12.76	17.52	0.97	1.66	2.65	1.34	0.93	0.83	0.56	-0.30	-0.38	-0.58
12	Utilities	4.91	6.88	11.30	18.03	0.40	1.30	2.67	1.46	1.47	1.20	1.01	0.00	-0.18	-0.31
13	Beverages	5.19	3.30	4.73	6.63	-0.36	-0.09	0.28	0.41	0.39	0.34	0.27	-0.05	-0.17	-0.34
14	Plastics	6.15	6.22	9.69	13.25	0.01	0.58	1.15	0.71	0.58	0.55	0.51	-0.19	-0.23	-0.28
15	Paper/Pulp	7.06	8.37	7.74	11.13	0.19	0.10	0.58	0.43	0.28	0.32	0.21	-0.34	-0.25	-0.52
16	Textiles	8.46	10.73	9.89	15.99	0.27	0.17	0.89	1.15	0.83	0.70	0.41	-0.28	-0.40	-0.65
17	Metal Products	8.62	15.86	16.71	24.60	0.84	0.94	1.85	3.30	2.58	2.54	2.17	-0.22	-0.23	-0.34
18	Pharmaceuticals/Perfumes/Soaps	9.80	12.31	10.67	15.91	0.26	0.09	0.62	0.58	0.47	0.44	0.50	-0.20	-0.24	-0.14
19	Nonmetallic Mineral Products	11.26	14.68	16.29	19.62	0.30	0.45	0.74	1.06	0.83	0.68	0.58	-0.21	-0.36	-0.46
20	Printing/Publishing	12.50	17.78	20.93	26.63	0.42	0.67	1.13	1.16	1.04	1.02	1.01	-0.11	-0.12	-0.13
21	Food Processing	12.97	15.60	17.63	20.66	0.20	0.36	0.59	2.01	1.96	1.96	1.92	-0.03	-0.03	-0.05
22	Shoe Manufacturing	18.75	22.54	15.92	21.80	0.20	-0.15	0.16	1.44	1.01	1.23	0.98	-0.30	-0.14	-0.32
23	Medicine	24.26	30.83	34.58	39.03	0.27	0.43	0.61	3.21	3.95	4.17	4.62	0.23	0.30	0.44
24	Sugar	24.60	21.91	17.85	55.26	-0.11	-0.27	1.25	0.20	0.23	0.08	0.04	0.13	-0.61	-0.80
25	Poultry	26.86	19.93	21.07	26.67	-0.26	-0.22	-0.01	0.09	0.07	0.07	0.06	-0.16	-0.21	-0.37
26	Community/Social Service	27.17	36.50	41.23	41.40	0.34	0.52	0.52	2.75	2.35	2.32	2.40	-0.15	-0.16	-0.13
27	Merchandise	27.60	34.43	36.31	37.36	0.25	0.32	0.35	10.41	11.10	11.14	11.22	0.07	0.07	0.08
28	Auxiliary Services	28.01	36.44	39.43	38.97	0.30	0.41	0.39	1.93	2.45	2.57	3.19	0.27	0.33	0.65
29	Transportation Services	28.90	29.49	34.38	42.95	0.02	0.19	0.49	4.86	5.18	5.10	5.41	0.07	0.05	0.11
30	Clothing Manufacturing	29.13	33.35	33.73	45.03	0.14	0.16	0.55	1.91	1.42	1.64	1.17	-0.26	-0.14	-0.39
31	Insurance	30.79	32.11	37.19	42.43	0.04	0.21	0.38	0.82	0.78	0.68	0.69	-0.05	-0.17	-0.17
32	Miscellaneous Manufacturing	31.94	40.09	44.95	48.91	0.26	0.41	0.53	0.84	0.71	0.70	0.64	-0.16	-0.16	-0.24
33	Technical/Professional Services	36.35	45.77	47.69	55.39	0.26	0.31	0.52	2.26	2.74	2.84	3.46	0.22	0.26	0.53
34	Public Administration	38.43	53.19	64.40	70.97	0.38	0.68	0.85	3.93	3.77	3.46	3.53	-0.04	-0.12	-0.10
35	Wood Products	38.82	53.72	54.08	58.18	0.38	0.39	0.50	1.09	0.96	0.94	0.93	-0.12	-0.13	-0.14
36	Entertainment	39.38	47.11	54.63	61.14	0.20	0.39	0.55	0.52	0.67	0.72	0.92	0.28	0.37	0.76
37	Mineral Extraction	40.17	43.31	43.99	45.07	0.08	0.10	0.12	0.16	0.14	0.11	0.10	-0.17	-0.33	-0.40
38	Teaching	43.70	48.64	55.50	58.80	0.11	0.27	0.35	4.92	5.46	5.52	6.11	0.11	0.12	0.24
39	Construction	47.25	58.44	65.48	70.58	0.24	0.39	0.49	7.28	7.84	7.50	7.10	0.08	0.03	-0.02
40	Food/Lodging	51.34	58.74	57.31	57.79	0.14	0.12	0.13	3.58	4.46	4.87	4.65	0.25	0.36	0.30
41	Domestic Service	60.58	55.98	53.23	48.78	-0.08	-0.12	-0.19	11.28	11.82	13.08	13.24	0.05	0.16	0.17
42	Beef	63.44	50.97	64.27	67.81	-0.20	0.01	0.07	0.16	0.11	0.17	0.11	-0.29	0.07	-0.33
43	Repairs	70.04	76.38	76.78	77.39	0.09	0.10	0.11	2.48	3.13	2.98	2.98	0.26	0.20	0.20
44	Agriculture/Traditional	80.94	88.65	88.35	88.45	0.10	0.09	0.09	0.58	0.51	0.51	0.44	-0.13	-0.12	-0.25
45	Defense	83.53	88.61	91.62	90.84	0.06	0.10	0.09	2.59	2.38	2.15	2.14	-0.08	-0.17	-0.17
46	Personal Services	92.18	93.35	92.37	89.19	0.01	0.00	-0.03	3.07	2.93	2.90	3.03	-0.04	-0.05	-0.01
47	Street Vending	98.85	99.24	99.31	99.17	0.00	0.00	0.00	1.93	2.47	2.89	2.85	0.28	0.50	0.48

Source: Author's calculations from *Pesquisa Mensal de Emprego* .
Notes: $\Delta_{86,92}$ refers to the percent change between 1986 and 1992 relative to 1986, and similarly for 1995 and 1999.

Appendix Table 2. Cross-Section and Panel Estimates of Yearly Changes in Log Informal Wage Gap, Brazil, 1982-2002

	(1)	(2)	(3)	(4)
		Panel Estimates (ΔT_{it}=0 for all regressions)		
Years	Change in Cross-Section Coefficients	Industry-occupation stayers	Industry-stayers	All matched observations
1982-3	-0.022	-0.055	-0.058	-0.061
	(0.011)	(0.006)	(0.006)	(0.004)
1983-4	-0.023	-0.087	-0.085	-0.095
	(0.009)	(0.013)	(0.012)	(0.010)
1984-5	0.000	-0.003	-0.002	0.001
	(0.009)	(0.007)	(0.006)	(0.005)
1985-6	0.133	0.101	0.095	0.072
	(0.009)	(0.012)	(0.012)	(0.010)
1986-7	0.000	-0.078	-0.073	-0.065
	(0.009)	(0.006)	(0.006)	(0.004)
1987-8	-0.053	-0.247	-0.227	-0.222
	(0.009)	(0.013)	(0.012)	(0.009)
1988-9	0.038	0.085	0.076	0.076
	(0.010)	(0.007)	(0.007)	(0.005)
1989-90	0.023	0.060	0.063	0.078
	(0.010)	(0.015)	(0.014)	(0.011)
1990-1	0.004	-0.086	-0.087	-0.077
	(0.010)	(0.007)	(0.007)	(0.005)
1991-2	-0.091	-0.199	-0.210	-0.204
	(0.010)	(0.014)	(0.013)	(0.010)
1992-3	-0.006	-0.012	-0.013	-0.024
	(0.010)	(0.008)	(0.007)	(0.006)
1993-4	0.044			
	(0.010)			
1994-5	0.091	0.099	0.102	0.099
	(0.010)	(0.008)	(0.008)	(0.006)
1995-6	-0.017			
	(0.009)			
1996-7	-0.021	-0.051	-0.052	-0.057
	(0.009)	(0.006)	(0.006)	(0.005)
1997-8	-0.018			
	(0.009)			
1998-9	-0.017	-0.039	-0.041	-0.044
	(0.009)	(0.006)	(0.006)	(0.004)
1999-2000	0.010			
	(0.009)			
2000-1	0.009	-0.026	-0.025	-0.022
	(0.009)	(0.006)	(0.005)	(0.004)
2001-2	-0.033			
	(0.010)			

Source: Pesquisa Mensal de Emprego, 1982-2002

Notes: Standard errors in parentheses. Column (1) presents the difference between the coefficients on an informal-contract dummy in cross-section regressions from two adjacent years, using the "working" sample. Columns (2), (3), and (4) present the coefficients for having an informal contract in two adjacent years. The samples are all individuals in the "working panel" sample who do not change informal-contract status (Column 4), and who stay in the same industry (Column 3), or the same industry and occupation (Column 2). No individuals were tracked across odd years after 1991 so there are no panel estimates for these years. Other regressors from cross-section regressions are education, age, age^2, and industry, occupation, and city fixed effects. Other regressors from panel regressions are education, 2*age-1, and city and week of interview fixed effects.