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MINIMUM WAGES AND LOW WAGE WORKERS: COMPLIANCE ASNON-EMPLOYMENT MARGIN[1]

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ABSTRACT: This paper examines the impact of a substantial minimum wage increase in Uruguay—a middle-income developing economy—on wages and employment. Using administrative data and a difference-in-differences approach, we analyze the consequences of a 2005 policy reform that raised the real minimum wage by 80% within a year. Our findings show that the reform led to significant wage gains for low-wage earners, with at most minimal negative effects on employment. Survey data further reveal no significant changes in unemployment or informality, suggesting the reform did not distort labor market dynamics. To contextualize these results, we investigate compliance with minimum wage laws and document a post-reform decline in compliance, particularly among low-wage workers. This pattern aligns with firms' cost-benefit trade-offs under weak enforcement of wage regulations. Our study contributes to the literature by providing causal evidence on the labor market effects of minimum wage policies in a developing economy, underscoring the pivotal role of enforcement in shaping policy outcomes.

JEL Codes: J23, J38, J88
Keywords: Minimum wage, labour market, compliance with the law.

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1. Introduction

Despite over a century of minimum wage legislation, the employment effects of minimum wage policies remain a subject of ongoing debate among economists. Early studies, predominantly conducted before the 1990s, generally concluded that minimum wage increases reduce employment Neumark and Wascher (1992); Currie and Fallick (1996). However, seminal work by Card and Krueger (1994, 1995) challenged this consensus, reporting non-negative employment effects. More recent research indicates that the impact of minimum wages on employment often depends on firms' ability to pass labor costs onto consumers.[1] In developing countries, where evidence is comparatively limited, findings are even more mixed Neumark and Munguía Corella (2021). Institutional differences –such as a high prevalence of low-wage workers, segmented labor markets Fields (2011), and weak enforcement of minimum wage laws Rani et al. (2013)– contrast with settings in developed countries. Recent studies highlight the trade-off between employment and minimum wage compliance, noting potential unintended consequences of stricter enforcement in developing countries (e.g., Badaoui and Walsh, 2022). However, evidence on employment effects remains inconclusive and sector-specific.[2] These findings underscore the need for further empirical research to gain a better understanding of the nuanced effects of minimum wage policies in developing countries.

This paper provides novel empirical evidence on the long-standing debate over how minimum wage policies affect wages and employment in middle-income developing economies, using Uruguay as a case study. Our analysis examines a substantial increase in the real minimum wage in 2005—an 80% rise within that year—which remained effective in subsequent years. The reform significantly raised the Kaitz ratio—the minimum-to-median wage ratio—from roughly 25% to over 40%. As doc-

[1] For instance, in the U.S., Cengiz et al. (2019) find negative employment effects in tradable sectors with higher demand elasticities but negligible effects in non-tradable sectors. Similarly, for Hungary, Harasztosi and Lindner (2019) report larger employment losses in manufacturing and export-oriented industries, where cost pass-through is limited. By contrast, for China, Mayneris et al. (2018) find no heterogeneous effects across sectors, suggesting that firms in developing countries may adjust to wage hikes through other margins.

[2] For example, in India's construction sector, Soundararajan (2019) finds that under weak enforcement regimes, minimum wage hikes have no impact on wages and exert negative or negligible effects on employment. Similarly, for domestic workers in South Africa, Dinkelman and Ranchhod (2012) reports that minimum wage increases lead to higher wages but do not significantly affect employment in contexts of low enforcement.

umented in Messina and Silva (2018), while Uruguay's minimum wage was initially modest, this increase was among the largest in Latin America during the 2000s.

This sudden and significant change in minimum wage policy makes Uruguay a compelling case for analyzing the labor market consequences of a reform driven by a substantial minimum wage increase, implemented just before other measures took effect. At the end of 2005, following the wage hike, the newly elected center-left government reinstated a tripartite collective wage bargaining process through the so-called "Wage Councils". These Councils gradually raised wage floors for formal workers starting in the last quarter of 2005, marking a transition from the labor market deregulation that had prevailed since 1990. In terms of enforcement, the number of labor inspectors and workplace inspections—key factors influencing compliance with minimum wage laws—rose by approximately 50% and 600%, respectively, between 2005 and 2009. Although Uruguay conducts a relatively high number of labor inspections compared to other Latin American and Caribbean countries, it imposes one of the region's lowest financial penalties for noncompliance Kanbur and Ronconi (2018); Gindling and Ronconi (2023), lowering firms' expected costs of failing to comply with minimum wage regulations.

Drawing on individual-level administrative records and exploiting variation in workers' exposure to the minimum wage reform, we examine its impact on wages and employment—capturing both intensive and extensive margins.[3] We implement a difference-in-differences (DiD) approach, comparing outcomes—such as hourly wage growth and employment changes—for workers earning at or near the minimum wage (treated group) with those earning substantially more (control group), before and after the 2005 policy change. By examining wage growth and employment changes across these groups and over time, this strategy helps us isolate the impact of the policy from broader labor market trends, addressing concerns such as differential exit rates among low-wage workers Manning (2021).

Our main findings indicate that the minimum wage policy led to substantial wage increases for low-wage earners, particularly for those near the minimum wage, compared to higher-wage workers. Regarding employment, we find at most minor neg-

[3]Many related studies rely on cross-sectional survey data and identify effects using variation in the minimum wage bite across regions. This strategy may suffer from measurement error and be confounded by shifts in the composition of local labor markets Monras (2019).

ative effects among directly affected workers compared to their less-affected peers. Moreover, we observe no significant impact on the intensive margin of employment —i.e., hours worked—. Overall, our results align with prior research, such as Engbom and Moser (2022), showing that minimum wage increases contributed to reducing wage inequality without significant adverse effects on employment.[4]

Our main analysis relies on data from formal workers, based on work histories from social security records. However, this approach has a potential limitation: workers who leave the formal sector may either remain unemployed or inactive or transition into the informality, making them absent from the records.[5] Our findings, which indicate almost no effect on employment in the formal sector, suggest that an increase in informality is a less likely response to the policy. In any case, using repeated cross-sectional survey data and leveraging regional variation in the minimum wage bite, we confirm this by estimating no significant effects of the minimum wage policy change on either informality or unemployment.

To better understand our findings, particularly the large and significant impact on wages and the limited effects on employment, we explored a well-documented hypothesis in developing countries: the extent to which compliance with minimum wage laws helps explain our results. Specifically, we constructed a measure of compliance based on whether workers' hourly wages fell above or below the minimum wage threshold. A key advantage of using administrative data is that it is less susceptible to measurement error, a major concern in compliance analysis, as noted by Clemens and Strain (2022). Interestingly, our analysis revealed a substantial decline in compliance following the policy change, particularly among wage earners near the minimum wage, compared to workers in higher wage brackets. The

[4]Based on our baseline estimates, we calculate an own-wage elasticity (OWE) of employment ranging from -0.022 to -0.063, implying that 2.2% to 6.3% of potential earnings gains from minimum wage hikes are offset by associated job losses. The OWE of employment is computed by dividing the employment effect estimates by the wage effect estimates. For comparison, the median OWE across 72 published studies, mostly from the U.S., is -0.14, though more recent research published since 2010 reports estimates closer to zero Dube and Zipperer (2024). A regularly updated repository compiles estimates of OWE of employment from minimum wage studies. See: https://economic.github.io/owe/ (accessed in March 2025).

[5]According to dual-sector models with formal and informal employment and low transition costs, workers displaced from the formal sector due to a minimum wage increase are expected to move into informality Gramlich (1976); Mincer (1976). However, a recent study by Samutpradit (2024) develops a framework in which, under certain conditions, the formal sector could actually expand following a minimum wage increase.

non-compliance hypothesis is confirmed when using survey data. These findings are likely to be explained by the initial lack of enforcement. Collecting data on two aggregate measures of enforcement, namely the number of labor inspectors and work inspections, we document that both remained unchanged right after the minimum wage increase, while it increases sharply after 2007.

The decline in compliance is consistent with recent evidence on non-employment responses to minimum wage increases (Clemens, 2021). According to classic economic models, firms evade minimum wage regulations through a cost-benefit calculus similar to tax evasion, where detection risk is a central determinant (e.g., Ashenfelter and Smith, 1979). In our context, two factors likely explain the post-reform compliance drop: (1) lax enforcement under the new administration, and (2) firms' short-term rigidity in labor cost adjustment (Clemens, 2021). This pattern is particularly relevant in developing economies, where minimum wage hikes often impose substantial operational constraints on firms. Policymakers may consequently adopt deliberate enforcement leniency post-reform to mitigate excessive burdens on businesses (Basu et al., 2010; Badaoui and Walsh, 2022). Existing evidence indicates that non-compliance is more widespread in developing countries with weak enforcement, especially during minimum wage hikes (Ham, 2018; Bhorat et al., 2017; Soundararajan, 2019). Because our study period overlapped with a transition from weaker to stronger labor regulations, we anticipated that some firms, particularly in the immediate post-reform years, would only partially comply with the new minimum wage laws. Our findings ultimately provide important empirical insights into the short-term trade-off between employment effects and non-compliance (see, for instance, Metcalf, 2008; Clemens and Strain, 2022).

This study advances the minimum wage literature in three important dimensions. First, we analyze the labor market effects of a large, persistent minimum wage increase using a quasi-experimental design, complementing established evidence from developed economies (e.g., Card and Krueger, 1994; Machin and Manning, 1994; DiNardo et al., 1996; Neumark and Wascher, 2000; Neumark et al., 2014; Autor et al., 2016; Cengiz et al., 2019; Clemens and Wither, 2019; Dustmann et al., 2021). Second, and more importantly, we extend the relatively limited research on minimum wage effects on middle-income developing economies (e.g., Bell,

1997; Rama, 2001; Lemos, 2009; Dinkelman and Ranchhod, 2012; Jales, 2018; Neumark and Munguía Corella, 2021; Engbom and Moser, 2022; Lombardo et al., 2024). Most studies on the impact of minimum wage policies in developing countries rely on survey data with self-reported earnings and employment, which may be prone to measurement error.[6] Third, we examine developing-country-specific adjustment channels, particularly compliance behavior, a mechanism often overlooked despite its policy relevance.[7] Our focus on Uruguay offers particularly valuable insights, as developing economies typically exhibit: (1) a larger share of workers affected by minimum wage policies, (2) substantial informal sectors, and (3) weaker enforcement capacity. By combining causal estimates with analysis of mechanisms, we provide novel insights into how minimum wage policies operate in such contexts (e.g., Borraz and González-Pampillón, 2017; Katzkowicz et al., 2021).

The remainder of the paper is organized as follows. Section 2 discusses the institutional setting, the policy change, and the datasets used. Section 3 outlines our identification strategy and empirical approach. Section 4 presents the findings on labor market outcomes, while Section 5 explores potential explanations for these results. Finally, Section 6 concludes.

2. Background

2.1. Institutional setting: changes in the minimum wage policy

The minimum wage (*salario mínimo nacional*) was introduced in Uruguay in November 1969 through Decree 1534/969, with the aim of guaranteeing a wage floor at the national level. The public sector and workers in the rural and domestic sectors were exempt from this legislation, each having its own minimum wage set in 1978 and 1990, respectively.[8] From its inception until 2005, the minimum wage was largely ineffective at setting wage floors. It primarily served as a reference for calculating social security benefits, including family allowances, unemployment insurance, and pensions, as well as for determining tax brackets on wages. In the 1990s, it also played a role in reducing public expenditure. Additionally, the mini-

[6] A notable recent exception is Engbom and Moser (2022), who examines the effects of minimum wage policies on wage inequality and employment in Brazil.

[7] For empirical evidence and theoretical mechanisms, see Bhorat et al. (2015); Ham (2018); Badaoui and Walsh (2022).

[8] Both sectors have a minimum wage similar to the national one.

mum wage lost purchasing power over the years, as it was not indexed to inflation and remained significantly below the median wage.

The panels in Figure 1 provide a detailed view of the evolution of real wages and their growth rates over the analyzed period. Panel A depicts the trends in real wage indices—normalized to 100 in December 2004—for the minimum wage (MW), private sector wages, and public sector wages. This panel highlights a substantial increase in the minimum wage in January 2005, followed by a pronounced rise after the policy change. In contrast, while private and public sector wages also grew, their indices remained relatively more stable. Panel B of Figure 1, which presents the growth rates of these wage indices over two-year windows, offers a clearer perspective on the dynamics at play. It reveals a sharp spike in the minimum wage growth rate in the two years immediately following the policy shift, whereas private and public sector wage growth fluctuates more moderately without exhibiting the same abrupt increase. Notably, after the initial two years, the trajectories of the three wage indices begin to converge. Together, these panels underscore the scale of the minimum wage policy change and its potential impact, particularly on workers earning close to the minimum wage.

The real minimum wage in Uruguay more than doubled between 2002 and 2012 (see Panel A of Figure 1), outpacing the growth of real wages in both the private and public sectors.[9] In December 2004, the monthly minimum wage was 1,310 UY$ (51 US$). One year later, in December 2005, it rose to 2,500 UY$ (109 US$), marking an 80% real increase (see Panel B of Figure 1). This policy shift coincided with a change in government and was implemented within a favorable macroeconomic context. Following the 2002 economic crisis, Uruguay experienced a period of strong economic and employment growth. Between 2003 and 2012, real GDP expanded at an average annual rate of 5.2%, the employment rate increased from 49% to 61%, and the unemployment rate declined from 17.2% to 6.5%.

Although the ratio of the minimum wage to the median wage in Uruguay may be lower than in other Latin American countries—such as Argentina, Brazil, Chile, and Ecuador, where the ratio ranged between 60% and 70% in 2006 (see Chapter 6 of

[9]During the 2000s, most Latin American countries significantly increased their national minimum wages in both level and coverage. Notably, Uruguay recorded the highest real minimum wage growth rate in the region during the first decade of the 2000s (Messina and Silva, 2018).

Messina and Silva, 2018)—its increase has been particularly noteworthy. As shown in Figure 1, the Uruguayan minimum wage experienced a significant increase, with the ratio of the minimum to median wage (in the private sector) climbing from approximately 26% at the end of 2004 to 40% in the following years. This substantial increase presents a unique opportunity to analyze its impact on the Uruguayan labor market.

Under the new policy, Uruguay's minimum wage established a floor for the monthly nominal earnings of private formal workers, applying fully to those with full-time contracts and being proportionally adjusted for part-time workers. Additionally, it became a key reference for setting wage floors negotiated in the Wage Councils, which were reinstated at the end of 2005. Furthermore, it has acted as a benchmark for wage increases among informal workers—those not covered by the social security system—consistent with the so-called "lighthouse effect" as documented by Maurizio and Vázquez (2016) and Jales (2018).

2.2. Data

2.2.1. Administrative records

Our empirical analysis is based on a large administrative database from the Uruguayan social security system. It contains work histories from 1996 to 2016 for a random sample of approximately 300,000 workers (about 15% of the Uruguayan labor force) who were registered in social security for at least one month during this period. Using this database, we construct an unbalanced panel of individuals, from which we observe monthly reported earnings, worked hours, gender, date of birth, contribution periods, contribution regimes, payment type, and firm characteristics such as ownership type, number of employees, number of branches, and 5-digit industry codes.

Following Dean et al. (2024), we exclude public sector and rural workers from our sample, restricting it to individuals receiving a monthly salary, as this is the most common and reliable method of reporting earnings. As a robustness check, we include daily and weekly paid workers -who are more frequently employed in domestic services and the construction sector- to assess the consistency of our results.[10]

[10]In the database, 99% of workers in the domestic service and construction social security contribution schemes are classified as daily or weekly paid.

We then focus on individuals affiliated with the primary social security contribution scheme (industry and services, "*Industria y Comercio*"), which covers private-sector employees in the manufacturing and service industries. For workers holding multiple jobs, we only consider the job with the highest wage. Additionally, we exclude individuals with reported wages equal to zero. Our analysis centers on prime-age workers, defined as individuals aged 20 to 55. The final sample consists of 92,856 individuals, evenly distributed by sex, with an average of 43.8 months of social security contributions recorded between 1997 and 2010.

Table 1 presents descriptive statistics for workers who, in 2004 (before the policy change), earned below or above the hourly minimum wage. We categorize workers into five groups: those earning between .75 and 1 minimum wage; those just above that threshold but still close to the minimum wage (1 to 1.75 minimum wages); those earning between 1.75 and 2.5 minimum wages; those between 2.5 and 4 minimum wages; and those earning above 4 minimum wages. We exclude workers earning more than the 75th percentile of the wage-bin distribution. Overall, we observe only minor differences between the first two groups and the third and fourth groups. Workers in the first two groups, who are more likely to be affected by changes in the minimum wage policy, are slightly younger, work a similar number of hours while earning below the average, and are more likely to reside outside the capital compared to those in the third and fourth groups, who are less likely to be affected by the minimum wage.[11] Furthermore, workers in the first two groups share a similar sectoral distribution with those earning between 1.75 and 4 minimum wages, with most employed in retail, manufacturing, and professional services. However, workers in the upper tail of the wage distribution—i.e., those earning above four minimum wages but less than the 75th percentile—differ the most from the rest. They are older, earn substantially higher wages, and are more concentrated in the health and transport sectors.

Panel A of Figure 2 shows the wage-to-minimum wage ratio in logarithms for 2004 and 2005. In 2005, following the minimum wage hike, the distribution exhibits a strong density spike near the minimum wage, indicating that the minimum wage

[11]Table A.1 in A presents the descriptive statistics, including daily and weekly paid workers. In this case, the first wage group has a higher proportion of female workers (60%) and is, on average, older than the second wage group.

became binding. Moreover, the distribution appears more compressed in 2005 relative to 2004, suggesting a reduction in wage inequality. Another notable feature is the increase in the share of workers earning below the minimum wage, rising from 6% in 2004 to 11% in 2005.

Panel B of Figure 2 displays the unconditional means of the outcome variables —wage and employment probability changes— by bin group, following the aforementioned categorization.[12] Triangle markers represent wage or employment changes between 2002 and 2004 —i.e., before the policy change— while squared markers indicate changes between 2003 and 2005 —i.e., after the policy change. The vertical gray bars measure the within-bin difference between these changes, serving as a before-and-after comparison for each bin group. For wages, the gray bars are larger for bins closer to the minimum wage and gradually decrease as we move further away. For instance, comparing the first bin group —most likely affected by the minimum wage policy— with the fourth bin group —less likely affected— we observe an approximate 30% wage increase. Regarding employment, we detect minimal changes in employment probability across bin groups before and after the policy change, suggesting no significant negative employment effects.[13]

While this exercise provides informal evidence of the wage and employment effects of the minimum wage, it also helps illustrate the intuition behind our empirical strategy.

2.2.2. Household survey data

To complement our main analysis based on administrative data of formal-sector workers, we use the annual Uruguayan Household Survey (*Encuesta Continua de Hogares*, ECH) from 1996 to 2011, conducted by the National Statistical Office of Uruguay (INE). The ECH is the primary source of socioeconomic information on Uruguayan households and, importantly, includes both formal and informal sector workers.[14] As with the administrative data, we focus on individuals aged 20 to 55,

[12]For the outcome variables, we consider changes in real hourly wages and employment probability, the latter defined as a binary variable equal to one if individual i is employed in the formal sector in the baseline period t and remains employed two years later (at $t+2$), and zero if employed at t but not at $t+2$. To minimize measurement error, we first construct the outcome variables before imposing sample restrictions, ensuring that the number of observations coded as zero is not artificially inflated.

[13]In Figure A.1 in A, we repeat this exercise but including daily/weekly paid workers.

[14]Since 2006, the survey has included rural areas. Before this, it covered only urban areas. To ensure comparability across years, we restrict our sample to urban areas with more than 5,000 in-

excluding public sector workers—who are largely unaffected by minimum wage policies—as well as rural and domestic workers, who are primarily paid on a daily or weekly basis.[15] The ECH provides information on self-reported monthly earnings after taxes and social security contributions, along with hours worked, allowing us to construct real hourly wages. We merge this data with minimum wage information for our analysis.

In the ECH analysis, we define informal workers as employed individuals without access to social security. Based on this definition, we find that one-quarter of employed workers in our sample work in the informal sector. Additionally, the unemployment rate stands at 8.6%.[16]

Uruguay is divided into 19 regions (departments) with varying living costs and employment opportunities, resulting in differences in real hourly wage distributions across regions. As a result, the national minimum wage policy is more binding in some departments than in others. The exposure to the minimum wage varies significantly across departments, ranging from approximately 5% to 25% when use .[17]

Figure 3 presents the distribution of the wage-to-minimum wage ratio in logarithms, grouping departments into three levels of exposure —low, mid, and high— in 2004 and 2005.[18] In the low-exposure group (Panel A), we observe no significant density concentration near the minimum wage in either 2004 or 2005. However, in the mid- and, particularly, the high-exposure groups, a substantial proportion of workers are clustered near the minimum wage in 2005, indicating that these departments were more directly affected by the policy change.

Panel A of Figure 4 displays the evolution of informality rates for the three groups with varying exposure to the minimum wage policy. Throughout the period, there is a persistent gap of approximately 10 percentage points between the informality rate in highly exposed departments and that in low-exposure departments. How-

habitants per department, covering over 80% of the total departmental labor force and thus serving as a representative sample of the overall workforce.

[15]Unemployed individuals are included to assess potential minimum wage effects on unemployment.

[16]Table A.2 in A presents descriptive statistics for various variables in 2004, prior to the minimum wage increase.

[17]Figure A.2 in A shows the share of workers in 2004 earning below the 2005 minimum wage in each department, providing a measure of their exposure to the policy.

[18]For these three groups, the average share of workers in 2004 earning below the 2005 minimum wage is 6%, 11%, and 18%, respectively.

ever, aside from this level difference across groups, we do not observe substantial variations in growth rates or shifts in trends following the 2005 minimum wage increase. This suggests that the policy change did not lead to a significant expansion of the informal sector in highly exposed departments.

Panel B of Figure 4 presents the evolution of the unemployment rate, showing similar levels across exposure groups. However, after 2004, the unemployment rate in the highly exposed group appears to increase at a slightly higher rate compared to the mid- and low-exposure groups. In Section 5, we formally test the effect of the minimum wage on informality and unemployment.

3. Identification

To analyze the effects of the minimum wage, we employ a difference-in-differences approach, comparing the labor market outcomes of workers earning close to the minimum wage with those earning significantly more, before and after the minimum wage increase —referred to as the individual-level approach. Figure 5 presents the wage distribution relative to the minimum wage in 2003, two years before the policy change, to illustrate our treatment group definition. The vertical dashed lines indicate the treatment group boundaries used in our main specifications, while the solid vertical line represents the 2005 minimum wage relative to its 2003 level.

As shown in the figure, our primary treatment group comprises individuals who, in 2003, earned between the lower bound and the 2005 minimum wage, as well as those earning slightly above this threshold, up to the upper bound.

Concretely, we analyze the impact of the minimum wage increase on wage growth, employment probability, and changes in hours worked over a two-year period. Our focus is on dynamic effects, which we estimate using the following model:

$$\Delta Y_{i,t} = \alpha T(w_{i,t-2}) + \sum_{\substack{k=1999 \\ k\neq 2004}}^{2012} \beta_k T(w_{i,k-2}) \times \mathbb{1}\{t=k\} + \theta_t X_{i,t-2} + \lambda_t + \varepsilon_{i,t} \tag{1}$$

where $Y_{i,t}$ represents one of our three outcomes —hourly wage growth, employment probability, or hours-worked growth— for individual i at time t. We consider two-year changes (i.e., $\Delta Y_{i,t} = Y_{i,t} - Y_{i,t-2}$). The treatment indicator, $T(w_{i,t-2})$, equals one for workers earning around the minimum wage in $t-2$, as defined in Figure 5, and

$w_{i,t-2}$ denotes wages in $t-2$ relative to the minimum wage in $t-2$.[19] The vector $X_{i,t-2}$ includes individual-level characteristics in $t-2$, such as age, sex, nationality, firm's two-digit industry, and region (capital vs. rest of the country), while λ_t captures year dummies. Our dataset covers the period 1997–2012.[20]

Our coefficients of interest, β_k, measure the labor market effects of minimum wage increases on the treated group relative to the control group, using 2004 as the baseline period. For instance, β_{2005} captures the effect of the sharp minimum wage increase between 2003 and 2005 on wage and employment changes for the treatment group during this period, relative to the control group, as well as to wage/employment changes between 2002 and 2004. We present results as event study graphs, allowing us to test for pre-trends between 1999 and 2004, when the Kaitz ratio remained stable and low. To obtain magnitudes, we focus on the period 2001–2006, specifically comparing changes in 2002–2004 to those in 2001–2003, 2003–2005 and 2004–2006.

In a second exercise, we examine the labor market effects of the minimum wage across the wage distribution, following Dustmann et al. (2021). Specifically, we compare changes in labor market outcomes —wage growth, employment probability, and hours worked growth— over two-year windows (between $t-2$ and t) along the wage distribution in $t-2$ before and after the policy change.

To implement this approach, we divide workers into equally sized bins based on their wages relative to the minimum wage ($w_{i,t-2}$). Each bin represents a wage range of one-quarter of the minimum wage, starting from .75 times the minimum wage and going up to four times the minimum wage, resulting in 13 bins. For example, individuals earning exactly one minimum wage in $t-2$ ($w_{i,t-2}=1$) are assigned to the first bin. Workers earning above four times the minimum wage but below the 75th percentile of the wage-bin distribution —who are unlikely to be affected by the policy— serve as the baseline group.

We then estimate the effect of the minimum wage policy by regressing the two-year change in wages, employment probability, or hours worked on a set of bin dum-

[19] Our main definition sets $T(w_{i,t-2})=1$ if $.75 \leq w_{i,t-2} < 2.5$ and $T(w_{i,t-2})=0$ if $w_{i,t-2} \geq 2.5$. In C, Figure C.1 shows results using an alternative definition: $T(w_{i,t-2})=1$ if $.75 \leq w_{i,t-2} < 1.75$ and $T(w_{i,t-2})=0$ if $w_{i,t-2} \geq 1.75$. Moreover, Figures C.2 and C.3 present results using one-year changes.

[20] While our baseline estimates focus on data from October of each year, robustness checks including estimates for all months are provided in C, Figure C.4.

mies, with one dummy for each bin —a method referred to as the bins approach.

$$\Delta Y_{i,t} = \sum_{k=1}^{13} (\mathbb{1}\{\underline{b}_k < w_{i,t-2} \leq \bar{b}_k\}\gamma_{k2004} + \mathbb{1}\{\underline{b}_k < w_{i,t-2} \leq \bar{b}_k\}\delta_{kt}) + \beta X_{i,t-2} + \lambda_t + \varepsilon_{i,t} \tag{2}$$

The coefficients δ_{kt} capture wage growth (or changes in employment probabilities or hours worked) for workers in wage bin k at $t-2$, relative to both the baseline period (2002–2004) and those earning more than four times the minimum wage. We estimate this regression over the 2001–2007 period, allowing us to analyze one pre-policy period (2001–2003) as a placebo test and three post-policy periods (2003–2005, 2004–2006, and 2005–2007), since there was a significant minimum wage increase in 2005.

3.1. Exploiting variation in the exposure

A common strategy in the minimum wage literature is to leverage regional variation in exposure to the policy. Studies relying on survey data—i.e., repeated cross-sections—often adopt this identification strategy due to the lack of data on workers' employment histories. Since earnings in survey data are typically self-reported, they may be subject to measurement error. However, an advantage of survey data is that it enables us to identify workers in the informal sector. In economies with dual labor markets, workers who lose formal employment due to a minimum wage increase can decide to move to the informal sector, leading to a rise in the share of informal workers. To test this hypothesis, we exploit the observed variation in exposure to the minimum wage policy across departments, as shown in Figure 3. Specifically, we estimate the following equation:

$$Y_{i,d,t} = \alpha_d + \lambda_t + \sum_{\substack{k=1996 \\ k \neq 2004}}^{2011} \beta_k E_{d,t} + \varepsilon_{i,d,t} \qquad E_{d,t} = I_d \times post_t \tag{3}$$

Where $Y_{i,d,t}$ is a binary variable that takes the value of one if worker i in department d is employed in the informal sector at time t, and zero if employed in the formal

sector.[21] We include additive department fixed effects (α_d), and year fixed effects (λ_t). I_d represents the continuous and time-invariant measure of treatment intensity – i.e., the exposure measure – for each department, defined as the share of workers in 2004 earning below the 2005 minimum wage. We also use a discrete version of I_d categorizing departments into three groups – high, mid, low exposure –using the low-exposure category as the reference.[22] $post_t$ is a dummy taking the value of one from 2005 onward.

Our coefficients of interest are β_k, which capture the impact of the minimum wage policy on informality, conditional on the fixed effects and relative to the reference year 2004. Department fixed effects account for time-invariant unobservable factors at the department level, while year fixed effects control for time-varying factors common to all departments. This approach relies on the assumption that, in the absence of the policy, any pre-existing trends across departments with different levels of exposure would have evolved at the same rate.

4. Results: Impact on labor market outcomes

In this section, we present graphical evidence alongside our main point estimates to illustrate the impact of the policy change on wages, employment, and hours worked, following the empirical strategies outlined in Section 3.

4.1. Individual approach

Figure 6 presents the dynamic treatment effects of minimum wage increases on labor market outcomes—wage growth, employment probability, and changes in hours worked— obtained from estimating equation 1. In all cases, the coefficients β_k capture differences in the evolution of labor market outcomes for workers earning around the minimum wage versus those earning above it, relative to the reference period 2002–2004—prior to the minimum wage policy change.

Panel A illustrates a positive effect of the minimum wage increase on the wage growth of workers near the minimum wage compared to those earning more. Specifically, in the first two post-policy periods (2003–2005 and 2004–2006), which were

[21] We also test the effect on employment using a binary outcome variable that takes the value of one if individual i in department d is employed at time t, and zero if unemployed.

[22] In addition, we use a time-varying version of the exposure measure, which allows treatment to switch on and off from 2005 onward. For this case, we use the discrete version of I_d with two categories—low exposure and mid/high exposure—using the low-exposure category as the reference.

directly affected by the policy change, wage growth for workers around the minimum wage increased by nearly 28% relative to unaffected workers.[23] In subsequent periods, when the minimum wage growth did not experience another substantial hike, the estimated coefficients become statistically insignificant. This finding is consistent with prior research on how minimum wages shape the lower tail of the wage distribution (e.g., DiNardo et al., 1996; Autor et al., 2016; Cengiz et al., 2019).

Despite observing effects on wages, there is no clear evidence that the minimum wage increase affected the extensive margin—i.e., employment. Panel B of Figure 6 shows a small negative effect on employment probability, but estimates are not statistically significant, ranging between 0 and -0.02 percentage points. Firms may respond to rising labor costs by adjusting hours worked rather than reducing employment. Panel C of Figure 6 suggests that the minimum wage increase has a mild negative effect—i.e., 1.5% reduction—on the intensive margin of employment—i.e., hours worked. Labor market rigidities in the formal sector may limit firms' ability to adjust hours in response to minimum wage changes unless in the short-run.[24]

All graphs in Figure 6 show that differences in wage growth, employment probability, and hours worked between workers with different levels of minimum wage exposure follow a parallel trend before the minimum wage hike. Nonetheless, in Section 4.3, we formally test whether the parallel trends assumption holds prior to the minimum wage policy change.

We summarize our main findings in Table 2, which reports estimates for the three main outcomes based on regression equation 1, using data from 2001 to 2007. The first row presents results for the pre-policy period (2001–2003), used as an unaffected benchmark and labeled as *placebo*. The second and third rows show estimates for two directly affected periods (2003–2005 and 2004–2006), capturing the immediate impact of the minimum wage increase. The fourth row provides results for a mildly affected period (2005–2007), allowing us to assess potential longer-term adjustments. The 2002–2004 period is used as the reference.

We confirm the patterns previously observed in the event study graphs. Specif-

[23] Given that the minimum wage increased by 80%, this implies an elasticity of $(\frac{0.28}{0.80}) \cong 0.35$.

[24] Given that our results may mask worker heterogeneity, in B, we apply the same approach for all three outcomes to different worker subgroups—specifically, females and males, young and old, and those living in the capital versus outside.

ically, we find a positive impact on wages during the two directly affected periods, with no significant effects in the placebo or mildly affected period. Regarding employment, estimates fluctuate around zero across all periods. For hours worked, the estimates are generally not statistically different from zero, except for the 2005–2007 period, where a small but significant effect suggests minor adjustments in working hours.

4.2. Bins approach

We further examine the impact of the minimum wage policy using our second approach, which involves estimating effects across thirteen wage-to-minimum wage bins. Workers earning more than four times the minimum wage but below the 75th percentile of the wage-bin distribution are used as the reference group. Panel A of Figure 7 presents estimates of two-year log wage growth by bin for the pre-policy period (2001–2003) and two post-policy periods (2003–2005, 2004–2006 and 2005–2007), relative to the 2002–2004 period, obtained from estimating Equation 2. Consistent with our findings from the event study graphs, the figure shows that wage growth following the minimum wage increase significantly outpaced that of 2002–2004. The effect is strongest for workers in bins closest to the minimum wage, with wage growth increasing by approximately 40% in the first three bins. Additionally, the effect diminishes as wages rise further above the minimum wage.

In Panel B of Figure 7, we present estimates of the probability of remaining employed in year t based on workers' wage bin in year $t-2$, relative to the 2002–2004 period. The results, if anything, suggest a very small negative effect on workers slightly above the minimum wage, particularly in the first year following the policy change. Panel C of Figure 7 shows estimates for hours worked, also derived from Equation 2. Here, the findings are more clear-cut: there are no discernible differences in hours worked between affected and unaffected workers, both before and after the policy change.

We summarize the findings for labor market outcomes using the bin approach in Table 3. Results are obtained by estimating Equation 2 with data from 2001 to 2007. Specifically, workers are categorized into five wage-to-minimum wage bins: [.75, 1), [1, 1.75), [1.75, 2.5), and [2.5, 4). Workers earning more than four times the minimum wage but less than the 75th percentile of the wage bin distribution form

the reference group. The first three bins are considered the "treated" groups, while the fourth serves as the "control," following the same classification as in Table 1. We examine effects for four periods: 2001–2003 (placebo), 2003–2005, 2004–2006, and 2005-2007 (mildly affected), using the 2002–2004 period as the benchmark.

Consistent with the patterns observed in Figure 7, Table 3 shows that wage effects are most pronounced for workers in the first and second treated bins (Panels A and B), with smaller, yet still statistically significant, effects on the third treated bin (Panel C), and no impact on the fourth bin (Panel D). For all four bins, no detectable impact is observed in the pre-policy period (row one of each panel, 2001 vs. 2003). Most of estimates in the mildly affected period (row four of each panel, 2005 vs. 2007) are not statistically significantly different from zero. Regarding employment and hours worked, we find no significant effects of the minimum wage policy change, as shown in column 2 and column 3, respectively.

In conclusion, both the individual and bins approaches yield very similar results, which can be summarized as follows: the minimum wage increase has a positive impact on wages, but no discernible effects on either the intensive or extensive margins of employment.

4.3. Robustness of parallel trends assumption

A key identification assumption in our difference-in-differences approach is that, in the absence of the policy change, the outcomes of workers affected and unaffected by the minimum wage hike would have followed parallel trends. While this assumption cannot be directly tested, we conduct two exercises to assess the robustness of our main findings.

First, we perform a joint test for the equality of pre-policy coefficients. Specifically, we test whether the coefficients β_{1997} through β_{2001} are not significantly different from one another. The resulting p-values do not reject the null hypothesis at conventional significance levels for employment and hours worked. For wages, we reject the null at the 10 percent significance level (p-value = 0.09).[25]

[25]The p-values for the joint test over the entire pre-policy period are 0.09 for wages, 0.50 for employment, and 0.38 for hours worked. Excluding β_{1997} from the test increases the p-values to 0.36, 0.54, and 0.70, respectively. In the case of wages, the rejection may be driven by the 2002 economic crisis, which likely affected workers differently across the wage distribution. If this introduces a negative pre-trend, our estimates may be downward biased, suggesting that our findings provide a lower bound.

Standard tests of the parallel trends assumption—such as checking whether pre-treatment coefficients are statistically insignificant—can suffer from low power, meaning that violations of the assumption may go undetected (Roth et al., 2023).

As a second exercise, we implement the "honest approach" developed by Rambachan and Roth (2023), which constructs confidence intervals that remain valid under potential violations of parallel trends. This method assumes that deviations from parallel trends in the post-treatment period should not be substantially different from those observed before the policy change. By modeling these deviations as a smooth extension of pre-treatment trends—allowing for both linear and non-linear adjustments—it provides a more robust assessment of policy effects.

Figure 8 presents the results of the "honest test" for the three main outcomes. The key input for this exercise is the sequence of β coefficients obtained from estimating Equation 1. Specifically, we use all pre-treatment coefficients —β_{97-99}, β_{98-00}, β_{99-01}, β_{00-02}, and β_{01-03}— to construct post-treatment violations. The vertical red segment represents the 95% confidence interval for the estimated effect of the minimum wage policy change, considering the two most affected periods, 2003–2005 and 2004–2006. The parameter M controls the degree of restrictions imposed on violations of pre-trends: $\text{M} = 0$ assumes a linear extrapolation, while $\text{M} > 0$ allows for non-linear deviations.

In Panel A, we find that the effect on wages remains significant under $\text{M} = 0$ and persists even for higher values of M. The threshold at which we can no longer reject the null hypothesis appears to be around $\text{M} = 0.25$. For employment and hours worked (Panel B and C), the absence of significant effects holds across different values of M. These results reinforce our main findings: the minimum wage increase has led to wage gains while showing no clear evidence of adverse effects on employment and hours worked.

5. Discussion

5.1. Informality and Unemployment

One main concern regarding our findings—a substantial increase in wages, accompanied at most by a slight decrease in employment—is that low-wage workers may have transitioned out of formal employment due to the minimum wage increase (i.e., higher labor costs for firms). Specifically, these workers might have shifted to

informal employment or even unemployment. This hypothesis is particularly relevant for dual labor markets that include both formal and informal sectors.

To test this, we use household survey data (*Encuesta Continua de Hogares*, ECH) from 1996 to 2011, which allows us to classify workers as formal, informal, or unemployed. If this explanation holds, we would expect an increase in informality, unemployment, or both among affected workers following the minimum wage policy change. [26]

Panel A of Figure 9 presents a dynamic difference-in-differences analysis of informality, which is defined as a binary variable equal to one for workers without access to social security and zero for those with access. Estimates are obtained from Equation 3—i.e., the sequence of β coefficients—using a discrete exposure measure at the department level, categorized into low, mid, and high exposure, with low exposure serving as the reference category. The year 2004 is used as the reference period.

Given the 19 departments and the use of clustered standard errors at the department level, we report wild bootstrap 95% confidence intervals (vertical segments). Overall, the point estimates are not statistically significant for either the mid- or high-exposure groups relative to the low-exposure group, suggesting that the minimum wage policy change did not affect informality.[27] While the point estimates for 2010 and 2011 are negative and appear sizable, the wide confidence intervals indicate a lack of precision.

In Panel B of Figure 9, we conduct the same analysis using unemployment as the outcome variable. Unemployment is defined as a binary variable, taking the value of one if the individual is unemployed and zero if employed. We find no evidence of changes in unemployment status for individuals in mid-exposure departments relative to those in low-exposure departments. For the high-exposure group, the estimates suggest a mild increase in unemployment after 2005 compared to the low-exposure group. However, most point estimates are not statistically significant,

[26] The main drawback of using survey data, however, is that it requires an alternative empirical strategy that exploits regional variation in exposure to the policy, which is driven primarily by differences in living costs.

[27] We perform two robustness checks in Figure C.5. First, we exclude the capital city, Montevideo—a low-exposure department—from the analysis (Panel A). Second, we use a time-varying exposure measure that allows for treatment switching on and off (Panel B). In Figure C.6, we conduct the same robustness checks using unemployment as the outcome variable.

indicating limited evidence of a meaningful effect.

Table 4 presents estimates using both the discrete exposure measure (columns 1 and 3) and its continuous counterpart (columns 2 and 4), capturing the average effect of the minimum wage policy for the period 2005–2011. Wild bootstrap 95% confidence intervals are reported in squared brackets. We find no impact on informality using either the discrete or continuous exposure measures. Although results indicate an increase in unemployment for the highly exposed group relative to the low-exposure group, this effect is only marginally significant. Estimates using the continuous exposure measure are not statistically significant.

Overall, our analysis based on survey data provides strong evidence that the small reduction in employment observed cannot be attributed to a shift toward informality or unemployment. This suggests that the small decline in formal employment was not offset by an increase in informal sector participation or joblessness, implying that other factors explain the effects of the minimum wage policy change.

5.2. Compliance with the MW Law

A plausible explanation for the findings in Section 4—significant effects on hourly wages with minimal, if any, impact on employment—relates to government enforcement of the minimum wage laws and firm compliance. We collected aggregate data on two enforcement indicators before and after the policy change: the number of labor inspectors and workplace inspections conducted. As shown in Figure 10, the number of inspectors increased substantially nearly two years after the minimum wage adjustment, with workplace inspections beginning to diverge from previous trends in 2008. This suggests that enforcement efforts were intensified approximately 2–3 years after the policy change. These findings align with theoretical predictions from previous research such as Basu et al. (2010); Badaoui and Walsh (2022), which argues that governments—particularly in developing countries—often allocate limited resources to labor inspections, making non-compliance more appealing to firms. Motivated by this, we conduct a more systematic investigation of enforcement dynamics and firm compliance within the context of a middle-income developing country like Uruguay.

To systematically examine compliance as an alternative margin of adjustment, we constructed an individual-level compliance indicator. Specifically, for each worker,

we defined a binary variable that equals one if the hourly wage remains above the minimum wage in both years, $t-2$ and t, and zero if the hourly wage was above the minimum wage in $t-2$ but dropped below it in t. Using this compliance indicator, we estimate the probability of compliance by applying both identification approaches discussed in Section 3.

Panel A of Figure 11 illustrates the dynamic effects of minimum wage policy changes on the probability of compliance. Estimates are obtained from Equation 1, using 2002–2004 as the reference period. Compliance declines during the two periods most affected by the policy change (2003–2005 and 2004–2006). Beyond this, we observe no discernible impact on compliance, as most point estimates remain close to zero and are not statistically significant. This pattern may be attributed to an initially weak enforcement of the minimum wage law, followed by a subsequent increase in enforcement efforts (see Figure 10) and the full implementation of the collective bargaining process. Additionally, there is no evidence of differing pre-trends between affected and unaffected workers before the policy change.[28]

Our second approach corroborates these findings. Specifically, Figure D.1 (included in the appendix for brevity) shows a decline in compliance around the minimum wage during the two periods affected by the policy change: 2003–2005 (blue) and, more prominently, 2004–2006 (green). In contrast, the estimates for 2001–2003 (red) and 2005-2007 (black) are not statistically different from the baseline period of 2002–2004.[29]

Panel B of Figure 11 presents evidence on compliance using survey data (ECH). As with labor market outcomes, we follow an intensity-of-treatment approach. Specifically, results are obtained by estimating Equation 3 using a discrete exposure measure at the department level, categorized into low, mid, and high exposure, with low exposure as the reference category. We confirm a decline in compliance following the minimum wage policy change in 2005, primarily among workers in highly exposed departments. Differences in treatment effect dynamics across panels may stem from variations in specification and outcome construction—changes vs. levels. Relative to 2004—the base year—the effect diminishes over time post-policy change, which may

[28] Our joint test for the equality of coefficients β_{1997} through β_{2001} yields a p-value of 0.31.

[29] In this figure, we began the bin analysis with the second bin (between 1 and 1.25 minimum wages) because, by construction, the variable is not defined between 0.75 and 1 minimum wage.

be attributed to increased enforcement efforts after 2007. We perform two robustness checks—excluding the capital and using a time-varying exposure measure—and find that the results remain unchanged.[30]

In Table 5, we present point estimates using administrative data (Panel A) and survey data (Panel B). Panel A shows that compliance declined by 3.6 percentage points in 2003–2005 (second row) and by 4 percentage points in 2004–2006 (third row). For 2005–2007, the estimate is near zero and no longer statistically significant, with all estimates measured relative to the base period 2002–2004. Additionally, point estimates for the placebo period 2001–2003 remain close to zero.[31]

Panel B further confirms that the policy change reduced compliance with the minimum wage. Specifically, we observe a 1.8 percentage point decrease in the probability of compliance for workers in mid-exposed departments (first row), while the decline nearly doubles to 3.4 percentage points for workers in highly exposed departments (second row).[32] In the second column of Panel B, where we use a continuous exposure measure, we also find a negative and statistically significant effect on compliance.

In the short run, this negative and significant effect on compliance may be seen as a non-employment margin of adjustment, allowing firms to evade the minimum wage law and, to some extent, acting as a release valve that mitigates the wage floor's adverse effects on employment (Clemens, 2021). A key question at this stage is whether these effects on compliance represent a widespread phenomenon or are concentrated within specific types of firms. To explore this further, we use an indicator of firm size—specifically, the number of employees.[33] Based on previous theoretical predictions (see, for example, Basu et al., 2010; Badaoui and Walsh, 2022), our hypothesis is that small firms, which are often low-productivity firms, are more likely to be non-compliant.

Table 6, column 1, presents estimates on compliance for firms above and below the median number of employees. The results indicate no statistically significant

[30]Robustness checks in Figure C.7.

[31]Estimates from the bin approach are presented in Table D.1 in the Appendix D.

[32]We report 95% confidence intervals in squared brackets using wild bootstrap standard errors clustered at the department level.

[33]Table D.3 in the D reproduces the estimates using the number of branches as alternative measure of firm-size.

differential impact on compliance by firm size. However, in the two treated years, the point estimates of compliance for small firms are larger than those for large firms. Specifically, for 2003–2005 (third and fourth rows), compliance decreases by approximately 3.3 percentage points for affected workers, with 3.0 for large and 3.6 in small firms. For 2004–2006 (fifth and sixth rows), compliance declines by 4.3 percentage points in small firms and 3.1 percentage points in large firms.

Additionally, we estimate the impact of the minimum wage increase on three key labor market outcomes, distinguishing between small and large firms. Columns 2–4 of Table 6 present these results. The evidence suggests that large firms respond to the minimum wage increase by reducing compliance without cutting employment or hours worked. Similarly, small firms adjust by lowering compliance, with minimal effects on employment.We estimate a statistically significant reduction in hours worked in the second and third years after the policy change, particularly in the third year, when government enforcement intensified.

Finally, we replicate the specifications from Table 6, this time dividing the sample into top and bottom quartiles. Table D.2 shows that the compliance effect was clearly more prevalent in the smallest firms, not in the largest. Additionally, the wage effect was statistically significant only for small firms. Moreover, we estimate that the negative effect on employment in small firms (bottom quartile) is more pronounced in the second year after the policy change.

6. Conclusions

This paper examines the consequences of a substantial increase in the minimum wage in Uruguay, using an administrative database from the Uruguayan social security system. The dataset includes work histories from 1996 to 2016 for a random sample of approximately 300,000 workers, representing around 15% of the Uruguayan labor force.

Our findings reveal a significant impact on wages with almost no discernible effect on employment, at either the extensive margin (employment probability) and the intensive margin (hours worked). Additionally, using survey data, we find that the policy change did not affect informality or unemployment.

To shed light on these results, we provide causal evidence showing that the policy change reduced short-run compliance with minimum wage laws. Furthermore,

we document two key findings: (i) non-compliance was more prevalent among small firms than among large ones, suggesting that vulnerable firms may use this as an adjustment margin; and (ii) some small firms instead (or additionally) adjusted by reducing employment, particularly when the government strengthened labor law enforcement. We argue that this evidence aligns with hypotheses discussed in (Basu et al., 2010; Clemens, 2021; Badaoui and Walsh, 2022), suggesting that non-compliance serves as a de facto adjustment margin for vulnerable firms—particularly in contexts where authorities may tolerate enforcement lapses for small firms after the reform.

Tables

TABLE 1

DESCRIPTIVE STATISTICS BY WAGE GROUPS RELATIVE TO THE MINIMUM WAGE - YEAR 2004

ADMINISTRATIVE DATA

Variables / Wage bins	(1) [.75 - 1]	(2) (1 - 1.75]	(3) (1.75 - 2.5]	(4) (2.5 - 4]	(5) >4
Men	.47	.50	.51	.49	.49
Age	32.66	33.10	33.18	33.24	34.55
below 30 y.o.	.44	.41	.40	.39	.32
30 and 45 y.o.	.44	.46	.48	.49	.54
above 45 y.o.	.12	.13	.12	.12	.14
Hours/week	40.90	41.61	40.6	40.43	40.34
Real hourly wage	5.80	8.65	13.59	20.30	33.20
Firms' sector					
Manufacturing	.17	.19	.19	.14	.14
Regulated	.02	.02	.02	.02	.02
Construction	.02	.02	.02	.01	.02
Commerce	.34	.41	.38	.39	.27
Transport	.08	.06	.06	.07	.10
HR	.07	.05	.06	.07	.05
Communication	.02	.02	.02	.02	.03
Finance	.01	.01	.01	.02	.02
Professional services	.11	.09	.09	.10	.12
Public sector	.00	.00	.00	.00	.00
Education	.03	.02	.03	.04	.05
Health	.03	.01	.02	.03	.09
Other services	.10	.11	.10	.10	.09
n.d.	.00	.00	.00	.00	.00
Regions					
Capital	.36	.38	.54	.60	.69
Rest of the country	.44	.39	.31	.30	.25
n.d.	.20	.24	.16	.10	.06
Nbr. of workers	1,522	6,385	6,853	8,491	8,584

NOTE: Table 1 compares workers located at different points in the hourly wage distribution in 2004 relative to the minimum wage. Columns 1, 2, and 3 display the characteristics of workers in the bins between 0.75 and 1, 1 and 1.75, and 1.75 and 2.5 times the minimum wage, respectively—i.e., the "treated bins." Columns 4 and 5 include bins less likely to be affected by the minimum wage policy, i.e., the "control bins." We exclude workers earning more than the 75th percentile of the wage-bin distribution. Source: Labor and Social Security Ministry.

Table 2

Impact of the MW on Labor Market Outcomes
Individual Approach

	(1) ΔLog(Wages)	(2) Pr(Employed)	(3) ΔLog(Hours Worked)
$T(w_{i,2001}) \times \mathbb{1}\{t = 2003\}(placebo)$	-0.021	-0.009	-0.006
	(0.017)	(0.013)	(0.005)
$T(w_{i,2003}) \times \mathbb{1}\{t = 2005\}$	0.278	-0.006	-0.009
	(0.023)	(0.014)	(0.007)
$T(w_{i,2004}) \times \mathbb{1}\{t = 2006\}$	0.272	-0.017	-0.009
	(0.023)	(0.014)	(0.006)
$T(w_{i,2005}) \times \mathbb{1}\{t = 2007\}$	0.005	-0.019	-0.016
	(0.018)	(0.014)	(0.007)
Obs.	41236	58467	41236

NOTE: Estimates are based on individual-level regressions using data from 2001 to 2007. For wages and hours worked, two-year changes—i.e., $\Delta Y_{i,t} = Y_{i,t} - Y_{i,t-2}$—were used. For employment, the outcome is a binary indicator equal to one if individual i is employed in both $t-2$ and t, and zero if the individual was employed in $t-2$ but no longer employed in t. Effects are estimated for three periods: 2001–2003 (placebo), 2003–2005, 2004–2006, and 2005–2007, using the 2002–2004 period as the reference. Standard errors are clustered at the 2-digit economic sector level, interacted with a binary variable for geographic location (capital vs. rest of the country/missing), and reported in parentheses.

Table 3
Impact of the MW on Labor Market Outcomes
Bins Approach

	(1) ΔLog(Wages)	(2) Pr(Employed)	(3) ΔLog(Hours Worked)
Panel A: Wage bin in t-2 (0.75, 1)			
$2001 vs. 2003 (placebo)$	-0.078	-0.022	0.017
	(0.114)	(0.044)	(0.027)
$2003 vs. 2005$	0.421	0.041	0.003
	(0.104)	(0.041)	(0.038)
$2004 vs. 2006$	0.402	-0.019	0.018
	(0.104)	(0.046)	(0.033)
$2005 vs. 2007$	0.116	-0.011	-0.034
	(0.104)	(0.039)	(0.039)
Panel B: Wage bin in t-2 (1, 1.75)			
$2001 vs. 2003 (placebo)$	-0.015	-0.016	-0.007
	(0.028)	(0.019)	(0.008)
$2003 vs. 2005$	0.428	-0.017	-0.003
	(0.032)	(0.015)	(0.008)
$2004 vs. 2006$	0.441	-0.046	-0.009
	(0.032)	(0.025)	(0.008)
$2005 vs. 2007$	0.049	-0.029	-0.010
	(0.023)	(0.018)	(0.008)
Panel C: Wage bin in t-2 (1.75, 2.5)			
$2001 vs. 2003 (placebo)$	-0.001	-0.009	-0.017
	(0.021)	(0.015)	(0.008)
$2003 vs. 2005$	0.221	-0.024	-0.010
	(0.023)	(0.014)	(0.007)
$2004 vs. 2006$	0.215	0.001	-0.002
	(0.025)	(0.014)	(0.008)
$2005 vs. 2007$	0.027	0.015	-0.020
	(0.022)	(0.017)	(0.007)
Panel D: Wage bin in t-2 (2.5, 4)			
$2001 vs. 2003 (placebo)$	0.006	-0.001	-0.009
	(0.021)	(0.014)	(0.006)
$2003 vs. 2005$	0.084	-0.017	0.007
	(0.019)	(0.016)	(0.006)
$2004 vs. 2006$	0.108	-0.010	0.009
	(0.019)	(0.017)	(0.006)
$2005 vs. 2007$	0.044	0.021	0.002
	(0.019)	(0.014)	(0.005)

NOTE: Estimates are based on bin approach using data from 2001 to 2007. Workers are grouped in five wage-to-minimum wage bins, using the fifth bins - those earning above four times the minimum wage- as the base category. For wages and hours worked, two-year changes - i.e., $\Delta Y_{i,t} = Y_{i,t} - Y_{i,t-2}$- were used. For employment, the outcome is a binary indicator that takes the value one if individual i is employed at both $t-2$ and t, while it takes the value zero if the individual was employed at $t-2$ but no longer employed at t. Effects are estimated for three periods: 2001–2003 (placebo), 2003–2005, 2004–2006, and 2005–2007, using the 2002–2004 period as the reference. Standard errors are clustered at the 2-digit economic sector level interacted with a binary variable for geographic location (i.e., capital vs. rest of the country/missing), and are reported in parentheses.

Table 4

Impact of the MW on unemployment & Informality Using Survey Data

	(1) Pr(Informal)	(2) Pr(Informal)	(3) Pr(Unemployed)	(4) Pr(Unemployed)
$\mathbb{1}\{I_d = \text{Mid}\} \times post_t$	-0.004		-0.002	
	[-0.038 ; 0.032]		[-0.022 ; 0.015]	
$\mathbb{1}\{I_d = \text{High}\} \times post_t$	-0.010		0.027	
	[-0.052 ; 0.034]		[-0.008 ; 0.069]	
$I_d \times post_t$		-0.118		0.142
		[-0.318 ; 0.293]		[-0.034 ; 0.467]
Obs.	202795	202795	224387	224387

NOTE: Estimates are based on individual-level regressions using household survey data (Encuesta Continua de Hogares, ECH) from 1996 to 2011. The estimates are obtained using a discrete exposure measure at the department level, categorized into low, mid, and high exposure, with low exposure serving as the reference category. For informality, the outcome is a binary indicator that takes the value of one for workers without access to social security, and zero for those employed with access to social security. For unemployment, the outcome is a binary indicator that takes the value of one if the individual is unemployed, and zero if employed. $post_t$ takes the value of one for years 2005 and thereafter. 95% confidence intervals (CIs) using wild bootstrap standard errors clustered at the department level are reported in square brackets.

Table 5
Impact of the MW on Compliance

	(1)	
Panel A: Using Administrative Data		
$T(w_{i,2001}) \times \mathbb{1}\{t = 2003\}(placebo)$	-0.009	
	(0.006)	
$T(w_{i,2003}) \times \mathbb{1}\{t = 2005\}$	-0.036	
	(0.008)	
$T(w_{i,2004}) \times \mathbb{1}\{t = 2006\}$	-0.040	
	(0.007)	
$T(w_{i,2005}) \times \mathbb{1}\{t = 2007\}$	0.003	
	(0.005)	
Obs.	40781	
	(1)	(2)
Panel B: Using Survey Data		
$\mathbb{1}\{I_d = \text{Mid}\} \times post_t$	-0.018	
	[-0.036 ; -0.007]	
$\mathbb{1}\{I_d = \text{High}\} \times post_t$	-0.034	
	[-0.060 ; -0.014]	
$I_d \times post_t$		-0.240
		[-0.402 ; -0.130]
Obs.	200110	200110

NOTE: Panel A: Estimates are based on individual-level regressions using administrative data from 2001 to 2006. Compliance is defined as a binary variable equal to one if individual i's hourly wage remains above the hourly minimum wage in both years $t-2$ and t, and zero if the hourly wage was above the minimum wage in $t-2$ but dropped below it in t. Effects are estimated for four periods: 2001–2003 (placebo), 2003–2005, 2004–2006, and 2005–2007, using 2002–2004 as the reference. Standard errors are clustered at the 2-digit economic sector level and interacted with a binary variable for geographic location (capital vs. rest of the country/missing), they are reported in parentheses. Panel B: Estimates are based on survey data from 1996 to 2011. Compliance is a binary variable equal to one if individual i's hourly wage is above the hourly minimum wage and zero otherwise. In column 1, we use a discrete exposure measure at the department level, categorized into low, mid (blue markers), and high (blue markers) exposure, with low exposure as the reference category. In column 2, we use a continuous exposure measure. $post_t$ takes the value of one for 2005 and thereafter, with 2004 as the reference year. 95% confidence intervals (CIs) using wild bootstrap standard errors clustered at the department level are reported in squared brackets.

Table 6

Impact of the MW on Compliance & Labor Market Outcomes by Firm Size

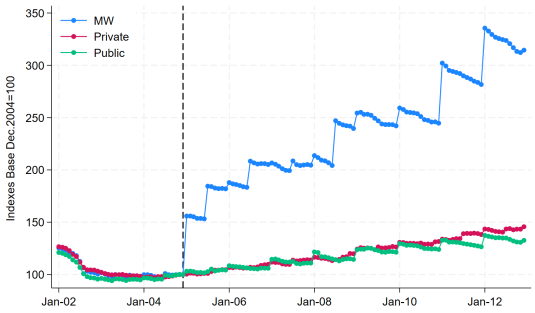
	(1) Pr(Compliance)	(2) ΔLog(Wages)	(3) Pr(Employed)	(4) ΔLog(Hours Worked)
$T(w_{i,2001}) \times \mathbb{1}\{t = 2003\} \times \mathbb{1}\{Size > Med\}$	-0.012	-0.016	-0.018	-0.006
	(0.014)	(0.034)	(0.024)	(0.008)
$T(w_{i,2001}) \times \mathbb{1}\{t = 2003\} \times \mathbb{1}\{Size < Med\}$	-0.005	-0.028	-0.015	-0.006
	(0.008)	(0.018)	(0.016)	(0.007)
$T(w_{i,2003}) \times \mathbb{1}\{t = 2005\} \times \mathbb{1}\{Size > Med\}$	-0.030	0.216	-0.023	-0.001
	(0.012)	(0.041)	(0.030)	(0.007)
$T(w_{i,2003}) \times \mathbb{1}\{t = 2005\} \times \mathbb{1}\{Size < Med\}$	-0.036	0.283	-0.007	-0.011
	(0.010)	(0.031)	(0.016)	(0.011)
$T(w_{i,2004}) \times \mathbb{1}\{t = 2006\} \times \mathbb{1}\{Size > Med\}$	-0.031	0.210	0.005	0.007
	(0.012)	(0.049)	(0.021)	(0.008)
$T(w_{i,2004}) \times \mathbb{1}\{t = 2006\} \times \mathbb{1}\{Size < Med\}$	-0.043	0.272	-0.031	-0.018
	(0.009)	(0.025)	(0.022)	(0.008)
$T(w_{i,2005}) \times \mathbb{1}\{t = 2007\} \times \mathbb{1}\{Size > Med\}$	0.006	-0.054	0.021	-0.003
	(0.010)	(0.040)	(0.021)	(0.008)
$T(w_{i,2005}) \times \mathbb{1}\{t = 2007\} \times \mathbb{1}\{Size < Med\}$	0.007	0.031	-0.036	-0.039
	(0.007)	(0.018)	(0.020)	(0.010)
Obs.	40780	41237	58471	41237

NOTE: Estimates are based on individual-level regressions using data from 2001 to 2007. For compliance, the outcome is a binary indicator equal to one if the hourly wage of individual i remains above the hourly minimum wage in both years, $t-2$ and t, and zero if the hourly wage was above the minimum wage in $t-2$ but dropped below it in t. For wages and hours worked, two-year changes—i.e., $\Delta Y_{i,t} = Y_{i,t} - Y_{i,t-2}$—were used. For employment, the outcome is a binary indicator equal to one if individual i is employed in both $t-2$ and t, and zero if the individual was employed in $t-2$ but no longer employed in t. $\mathbb{1}\{Size > Med\}$ equals one for workers in firms with a number of workers above the median (which is equal to one). Effects are estimated for four periods: 2001–2003 (placebo), 2003–2005, 2004–2006, and 2005–2007, using the 2002–2004 period as the reference. Standard errors are clustered at the 2-digit economic sector level and interacted with a binary variable for geographic location (capital vs. rest of the country/missing), they are reported in parentheses.

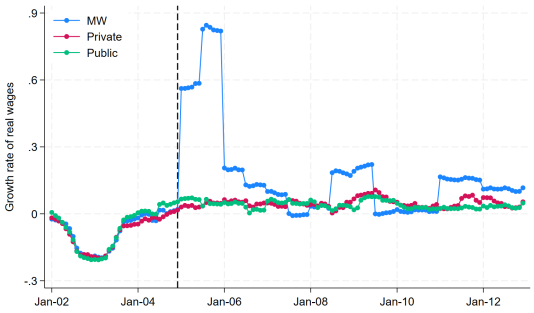
Figures

Figure 1

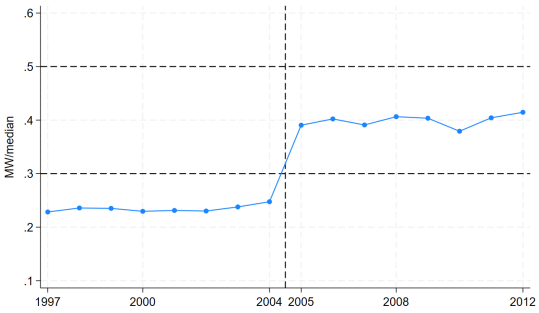
Evolution of the minimum wage (MW), real wages, and the Kaitz ratio



Panel A: Wages and the MW in levels



Panel B: Wages and the MW in changes

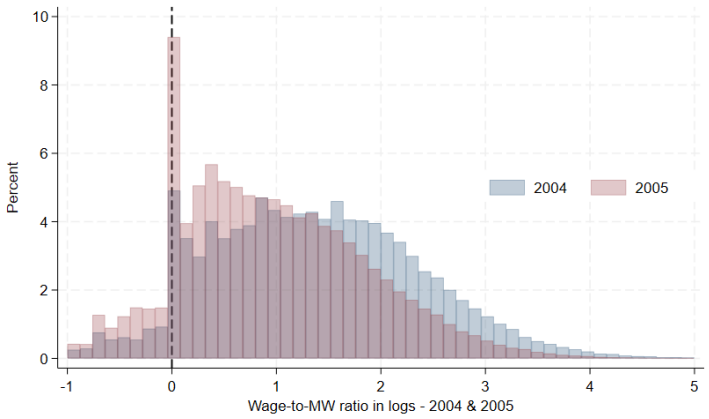


Panel C: Kaitz ratio

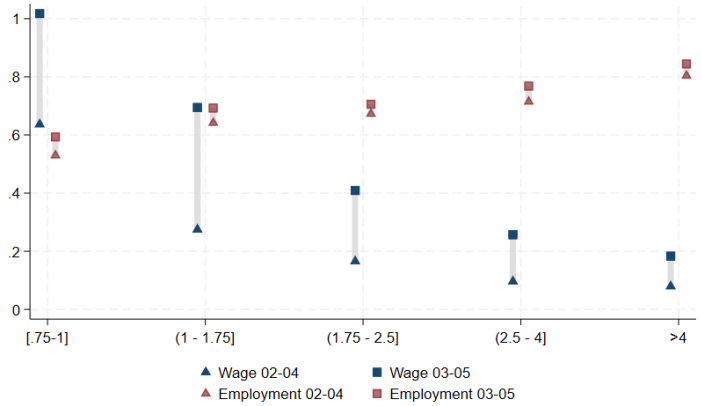
NOTE: Panel A presents the indexes of real wages normalized to 100 in December 2004. Panel B shows the growth rates, in a 2 years window of the real wages. In both cases the dotted vertical line divide the pre and post policy period. Panel C shows the kaitz ratio – minimum wage to the median wage – for wage earners. We consider all workers in the industry and services social security contribution scheme – i.e., "*Industria y Comercio*", most of them are monthly paid workers (around 60%). We exclude public employees, as well as the domestic service and construction social scurity schemes. Source, Labor and Social Security Ministry. Sources: National Statistics Institute (INE) and Labor and Social Security Ministry.

Figure 2

Wage-to-MW ratio & labor market outcomes



Panel A: Histogram of the wage-to-MW ratio



Panel B: Wage & employment changes by bin group

NOTE: Panel A shows the distribution of the wage-to-minimum wage (MW) ratio in logs for 2004 and 2005. Panel B displays the two-year growth rates of real hourly wages, along with changes in employment probability –i.e., labor market outcomes–. Squared markers represent the 2004–2002 change –i.e., before the substantial minimum wage hike– while triangle markers correspond to the 2005–2003 change, which captures the sharp minimum wage increase in 2005. Source: Labor and Social Security Ministry. Sources: National Statistics Institute (INE) and Labor and Social Security Ministry.

Figure 3

Wage-to-MW ratio by level of exposure to the minimum wage policy

Panel A: Low Exposure

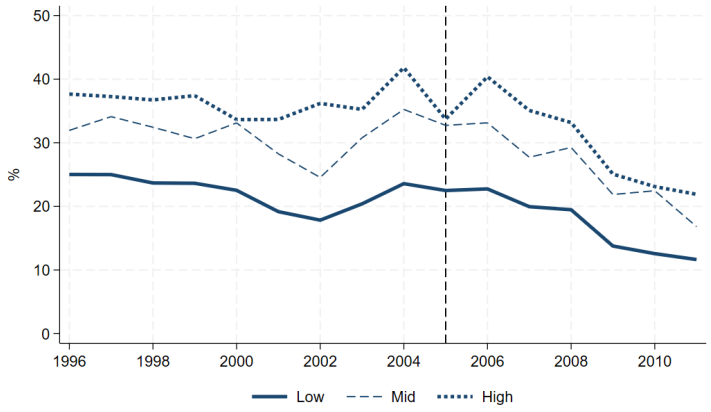
Panel B: Mid Exposure

Panel C: High Exposure

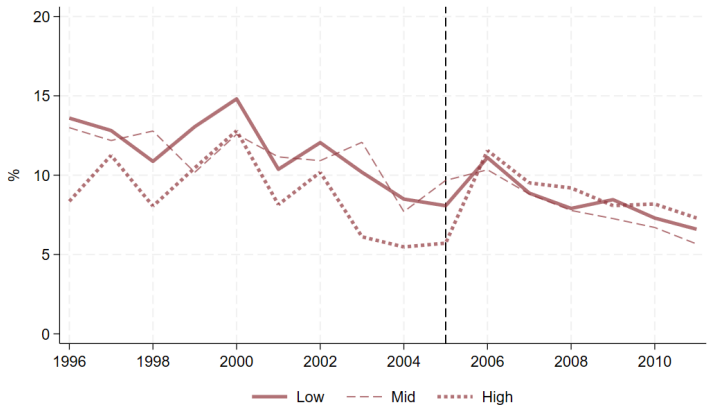
NOTE: Figure 3 displays the wage-to-minimum-wage ratio by level of exposure—low, mid, and high—to the minimum wage policy. Exposure is defined based on the share of workers in 2004 earning below the 2005 minimum wage. The number of departments in each exposure group is as follows: low (7), mid (6), and high (6). Source: Uruguayan Household Survey (ECH) – National Statistics Institute (INE).

Figure 4

Evolution of the informality rate and unemployment rate by level of exposure to the minimum wage policy



Panel A: Informality rate

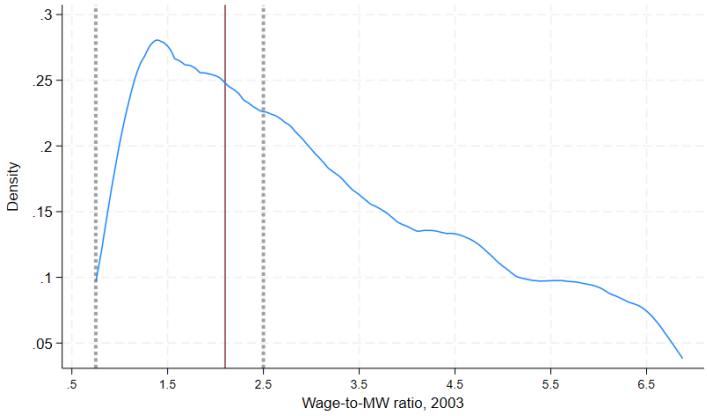


Panel B: Unemployment rate

NOTE: Panel A displays the informality rate defined as the share of workers without access to social security, while Panel B shows unemployment rate, by level of exposure—low, mid, and high—to the minimum wage policy. Exposure is defined based on the share of workers in 2004 earning below the 2005 minimum wage. The number of departments in each exposure group is as follows: low (7), mid (6), and high (6). Source: Uruguayan Household Survey (ECH) – National Statistics Institute (INE).

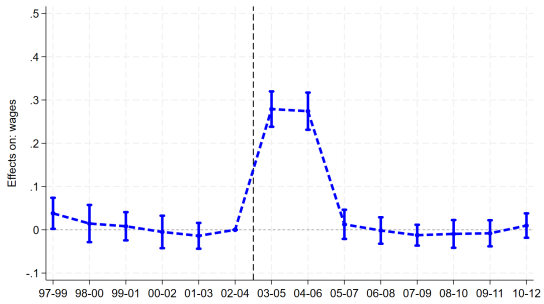
Figure 5

Definition of the treatment

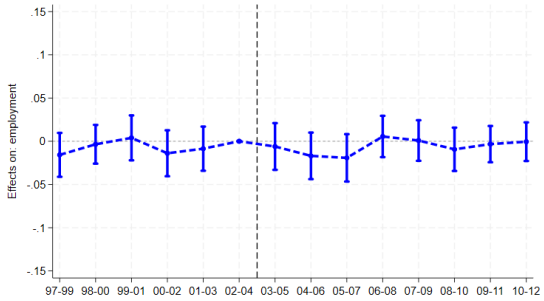


NOTE: Figure 5 displays the density of the wage-to-minimum wage ratio in October 2003, two years before the substantial minimum wage increase. The two vertical dashed lines represent the treatment definition used in our baseline estimates, corresponding to 0.75 and 2.5 times the minimum wage. The solid vertical line indicates the 2005 minimum wage expressed in terms of the 2003 minimum wage. Source: Labor and Social Security Ministry.

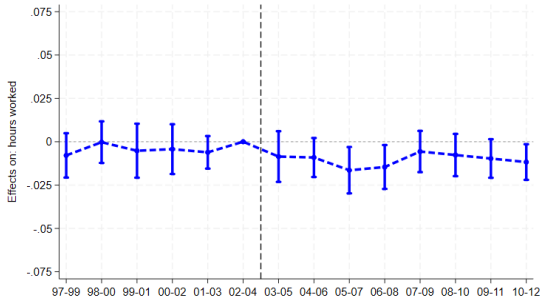
Figure 6
Event-Study Graph: Labor Market Outcomes



Panel A: Wages



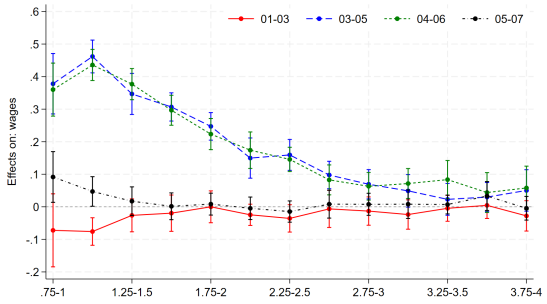
Panel B: Employment



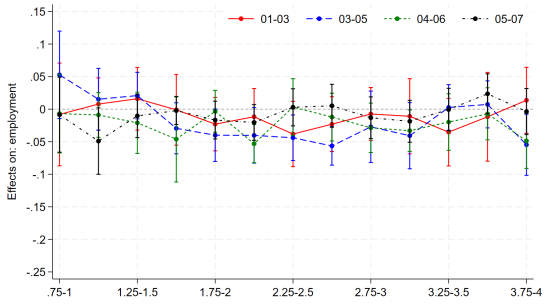
Panel C: Hours worked

NOTE: Event-study graphs for changes in labor market outcomes. Round markers represent estimates for the sequence of β coefficients from Equation 1. Panel A shows estimates for wage growth (wages in logs). Panel B displays estimates for employment probability. Panel C presents estimates for hours growth (hours in logs). Vertical segments indicate 95% confidence intervals, using standard errors clustered at the 2-digit economic sector level interacted with a binary variable for geographic location (capital vs. rest of the country/missing). The dashed vertical line separates the pre- and post-minimum wage policy change periods.

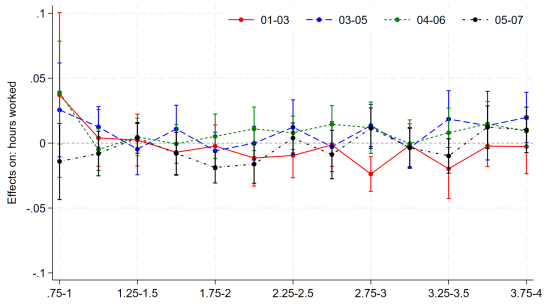
Figure 7
Bins Approach: Labor Market Outcomes



Panel A: Wages



Panel B: Employment

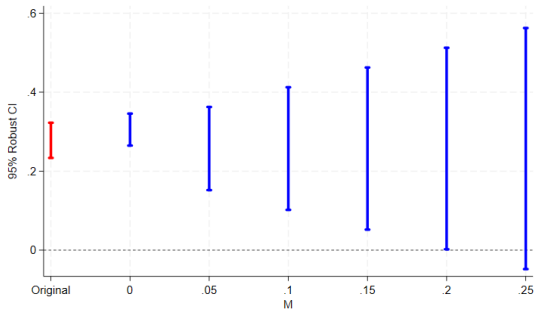


Panel C: Hours Worked

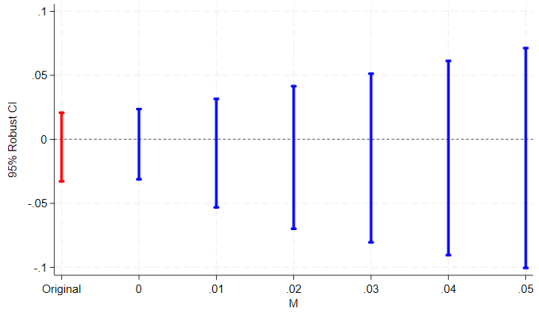
NOTE: Results from the bin approach obtained by estimating Equation 2. Panel A presents estimates for wage growth (log wages) over a two-year window, capturing excess wage growth by wage bin for the periods 2001–2003 (in red), 2003–2005 (in blue), 2004–2006 (in green), and 2005–2007 (in black), relative to 2002–2004. Panel B reports estimates for the probability of employment in period t for workers employed in $t-2$, by wage bin, for the same periods. Panel C presents estimates for hours worked growth (log hours) over a two-year window for the same periods. In all figures, wage bins on the horizontal axis are measured in minimum wage units—e.g., the bin 0.75–1 includes individuals who earned between 0.75 and 1 times the minimum wage in the base year. Workers earning more than four times the minimum wage but below the 75th percentile of the wage-bin distribution are used as the reference category. Vertical segments indicate 95% confidence intervals, using standard errors clustered at the 2-digit economic sector level interacted with a binary variable for geographic location (capital vs. rest of the country/missing).

Figure 8

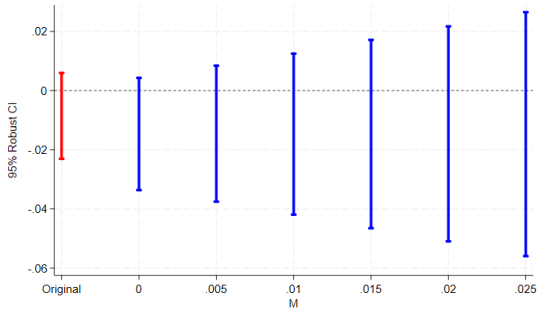
Sensitivity Analysis for Violations of Parallel Trends



Panel A: Wages



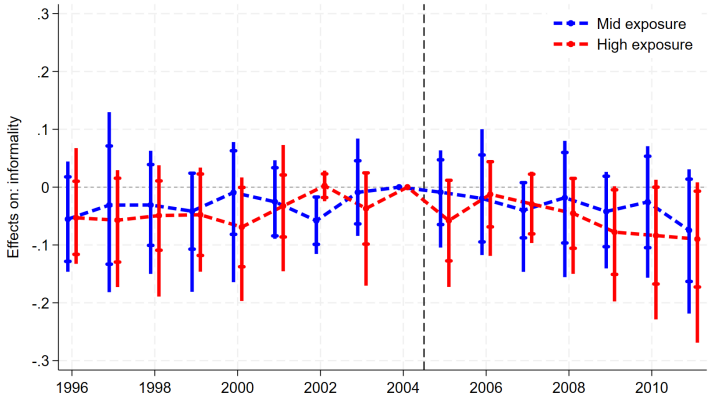
Panel B: Employment



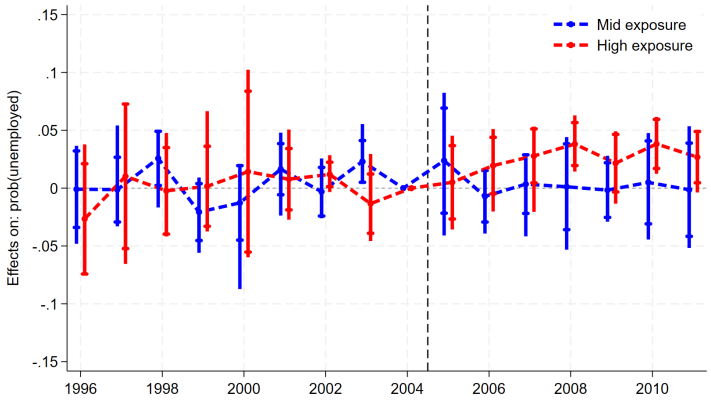
Panel C: Hours Worked

NOTE: This graph presents 95% confidence intervals that are robust to violations of the parallel trends assumption, following the approach developed by Rambachan and Roth (2023). The vertical red segment represents the confidence interval for the estimated effect of the minimum wage policy change, based on the two most affected periods, 2003–2005 and 2004–2006. The vertical blue segments show confidence intervals that account for potential violations of the parallel trends assumption using the smoothness restriction—i.e., post-treatment deviations from parallel trends are constrained not to diverge significantly from extrapolations of the pre-trend. M=0 assumes a linear extrapolation of the counterfactual difference in trends, while M>0 allows for non-linear extrapolations.

Figure 9
Event-Study Graph: effects on unemployment & informality
Using survey data



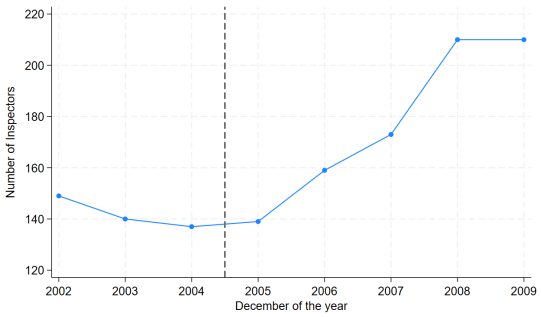
Panel A: Informality



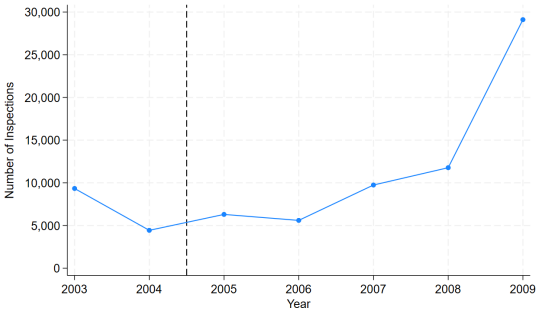
Panel B: Unemployment

NOTE: Event-study graphs for informality and unemployment probability using household survey data (Encuesta Continua de Hogares, ECH). Round markers represent estimates for the sequence of β coefficients from Equation 3 using a discrete exposure measure at the department level, categorized into low, mid (blue markers), and high (blue markers) exposure, with low exposure as the reference category. Panel A and B presents estimates for informality and unemployment, respectively. Vertical segments indicate 95% confidence intervals (CIs) using wild bootstrap standard errors clustered at the department level, while capped spikes illustrate CIs using conventional clustered standard errors. The dashed vertical line separates the pre- and post-minimum wage policy change periods.

Figure 10
Work Inspections



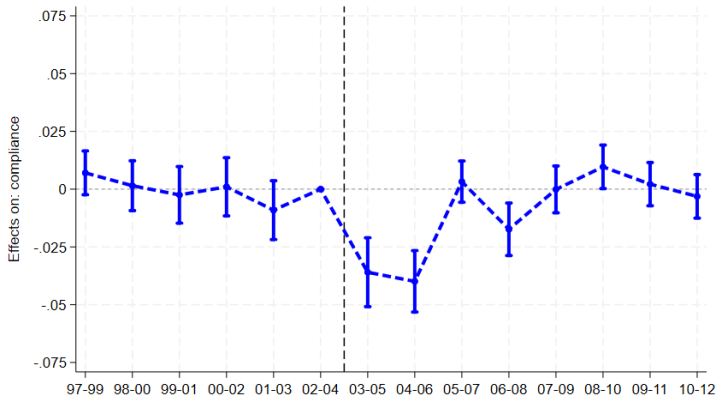
Panel A: # **of work inspectors**



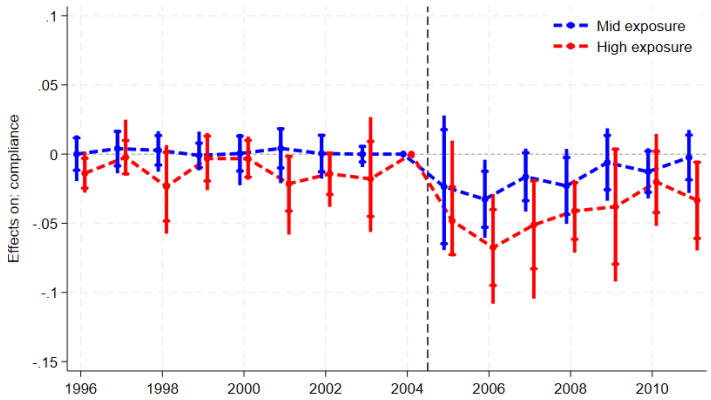
Panel B: # **of work inspections**

NOTE: The dotted vertical line divide the pre and post policy period. Social Security Annual Reports.

Figure 11
Event-Study Graphs: Compliance



Panel A: Using administrative data



Panel B: Using survey data

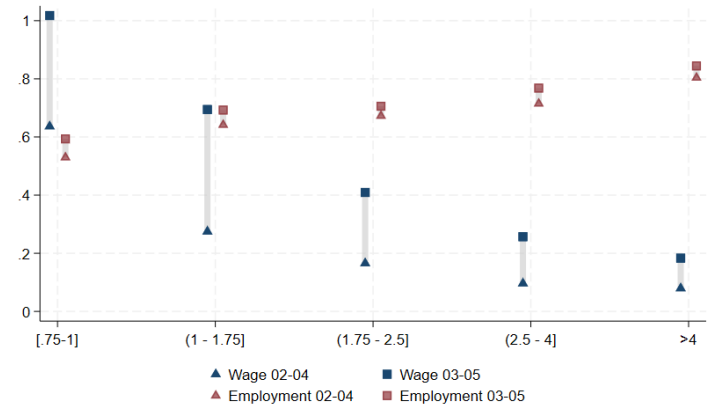
NOTE: Event-study graphs for compliance. Panel A: round markers represent estimates for the sequence of β coefficients from Equation 1. Vertical segments indicate 95% confidence intervals, using standard errors clustered at the 2-digit economic sector level interacted with a binary variable for geographic location (capital vs. rest of the country/missing). Panel B: round markers represent estimates for the sequence of β coefficients from Equation 3 using a discrete exposure measure at the department level, categorized into low, mid (blue markers), and high (blue markers) exposure, with low exposure as the reference category. Vertical segments indicate 95% confidence intervals (CIs) using wild bootstrap standard errors clustered at the department level, while capped spikes illustrate CIs using conventional clustered standard errors. The dashed vertical line separates the pre- and post-minimum wage policy change periods.

Appendices

A. Additional descriptive statistics

Figure A.1
Wage-to-MW ratio & labor market outcomes
Including daily/weekly paid workers

Panel A: Histogram of the wage-to-MW ratio



Panel B: Wage & employment changes by bin group

NOTE: Panel A presents the distribution of the wage-to-minimum wage (MW) ratio in logs for 2004 and 2005. Panel B displays the two-year growth rates of real hourly wages, along with changes in employment probability –i.e., labor market outcomes–. Squared markers represent the 2004–2002 change –i.e., before the substantial minimum wage hike– while triangle markers correspond to the 2005–2003 change, which captures the sharp minimum wage increase in 2005. Source, Labor and Social Security Ministry. Sources: National Statistics Institute (INE) and Labor and Social Security Ministry.

Table A.1

Descriptive statistics by wage groups relative to the minimum wage - year 2004
Administrative data - Including daily/weekly paid workers

Variables / Wage bins	(1) [.75 - 1]	(2) (1 - 1.75]	(3) (1.75 - 2.5]	(4) (2.5 - 4]	(5) >4
Men	.40	.49	.54	.56	.60
Age	34.34	33.81	33.65	33.76	34.72
below 30 y.o.	.36	.38	.38	.37	.30
30 and 45 y.o.	.47	.47	.48	.50	.55
above 45 y.o.	.17	.15	.14	.13	.15
Contribution scheme					
Industry & services	.66	.86	.91	.89	.86
Construction	.06	.03	.05	.09	.13
Domestic service	.28	.11	.04	.02	.01
Payment type					
Monthly-paid workers	.43	.68	.66	.62	.59
Daily/weekly-paid workers	.57	.32	.34	.38	.41
Hours/week	40.94	41.36	41.02	41.61	42.03
Real hourly wage	5.62	8.64	13.56	20.43	30.76
Firms' sector					
Manufacturing	.14	.19	.21	.21	.23
Regulated	.04	.03	.04	.02	.02
Construction	.07	.05	.06	.10	.14
Commerce	.18	.31	.29	.29	.23
Transport	.05	.06	.06	.06	.08
HR	.04	.04	.05	.05	.03
Communication	.01	.01	.01	.02	.02
Finance	.00	.01	.01	.01	.01
Professional services	.09	.09	.09	.09	.09
Public sector	.00	.00	.00	.00	.00
Education	.02	.02	.03	.03	.03
Health	.02	.01	.02	.03	.06
Other services	.34	.18	.13	.09	.06
n.d.	.00	.00	.00	.00	.00
Regions					
Capital	.34	.39	.55	.62	.70
Rest of the country	.33	.35	.30	.29	.24
n.d.	.33	.26	.15	.09	.06
Nbr. of workers	3,700	11,504	11,870	15,383	13,447

NOTE: Table A.1 compares workers located at different points in the hourly wage distribution in 2004 relative to the minimum wage. Columns 1, 2, and 3 display the characteristics of workers in the bins between 0.75 and 1, 1 and 1.75, and 1.75 and 2.5 times the minimum wage, respectively—i.e., the "treated bins." Columns 4 and 5 include bins less likely to be affected by the minimum wage policy, i.e., the "control bins." We exclude workers earning more than the 75th percentile of the wage-bin distribution. Source: Labor and Social Security Ministry.

Table A.2

Descriptive statistics - year 2004

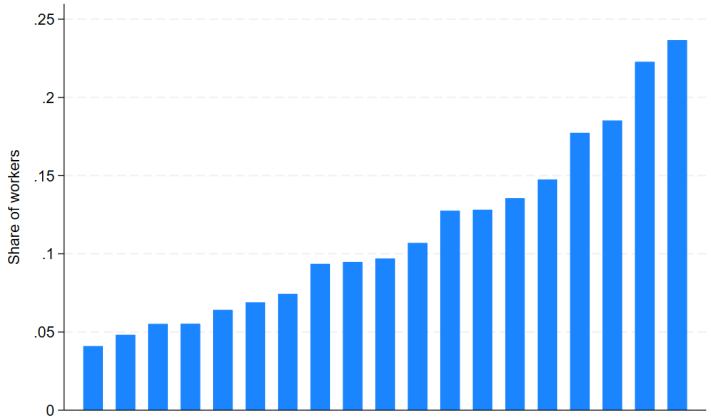
Survey data

Variables	(1) Mean	(2) Std. dev.	(3) Min.	(4) Max.
Men	.56	.50	.00	1.00
Age	34.73	10.09	20.00	55.00
below 30 y.o.	.39	.49	.00	1.00
30 and 45 y.o.	.40	.49	.00	1.00
above 45 y.o.	.22	.41	.00	1.00
Years of schooling	10.83	3.49	.00	20.00
Worked hours	42.68	13.29	1	98
Real hourly wage	58.17	55.5	4.81	930.99
Informal workers	.25	.43	.00	1.00
Regions				
Capital	.66	.48	.00	1.00
Rest of the country	.34	.48	.00	1.00
Nbr. of obs.(*)	8,268			

NOTE: (*) The sample includes employed (7,616) and unemployed (652) individuals. Source: Uruguayan Household Survey (ECH) - National Statistics Institute (INE).

Figure A.2

Proportion of workers earning below the 2005 minimum wage by department in 2004



NOTE: Figure A.2 displays the share of workers in each of the 19 departments who, in 2004, earned below the 2005 minimum wage. Source: Uruguayan Household Survey (ECH) - National Statistics Institute (INE)

B. Heterogeneity analysis

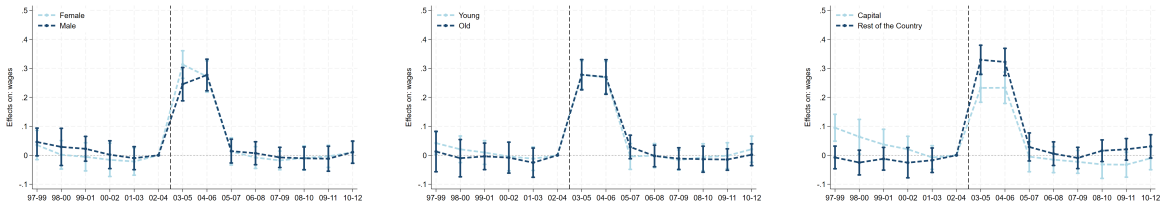
We leverage our rich dataset to explore potential heterogeneity in the minimum wage increase's impact across various worker groups. We begin by examining gender differences, as women tend to hold lower-paying jobs and might be more affected by the policy. Subsequently, we analyze whether variations exist based on the age (above and below 35 years old). Finally, we explore differences between those residing in the country's capital and those living elsewhere. In Figure B.1, we present the results of our heterogeneity analysis using our first identification approach.

Figure B.1 reveals no significant differences in the policy's impact across the groups we analyzed. Interestingly, for wages (Panel A), the increase appears slightly more pronounced for workers outside the capital city. This may be due to the higher concentration of low-wage workers residing outside the capital, as shown in Table 1. Reassuringly, for both employment probability (Panel B) and hours worked (Panel C), there remains no statistically significant effect across groups. However, regarding employment, in the second year after the minimum wage increase, there appears to be a slightly more negative effect for workers, particularly males, young individuals, and those residing outside the capital city. Finally, regarding hours worked, male workers and those living outside the capital may experience a more pronounced negative effect.

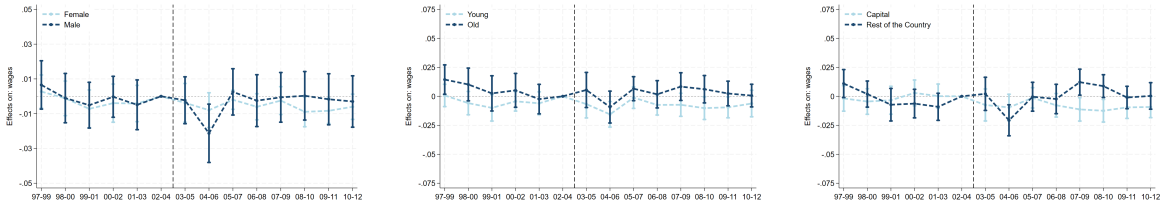
Figure B.1

Event Study: Labor Market Outcomes - Heterogeneous results

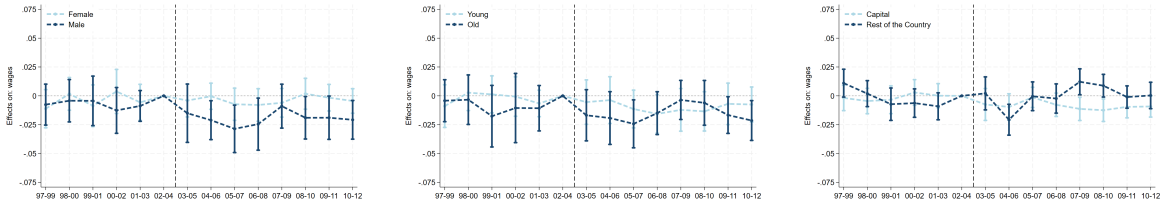
Panel A: **Wages**



Panel B: **Employment**



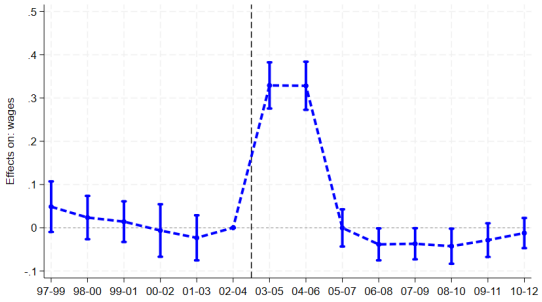
Panel C: **Hours Worked**



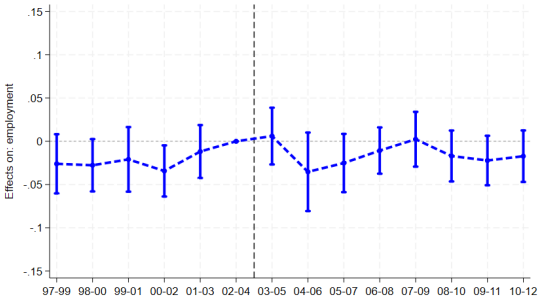
NOTE: Event-study graphs for changes in labor market outcomes across three worker groups, gender, age and residence. Round markers represent estimates for the sequence of β coefficients from Equation 1. Panel A shows estimates for wage growth (wages in logs). Panel B displays estimates for employment probability. Panel C presents estimates for hours growth (hours in logs). Vertical segments indicate 95% confidence intervals, using standard errors clustered at the 2-digit economic sector level interacted with a binary variable for geographic location (capital vs. rest of the country/missing). The dashed vertical line separates the pre- and post-minimum wage policy change periods.

C. Robustness checks

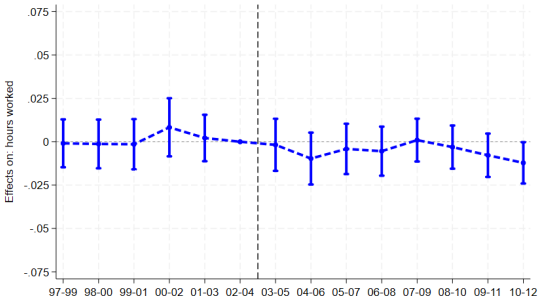
Figure C.1
Event Study: Labor Market Outcomes - Alternative treatment definition



Panel A: **Wages**



Panel B: **Employment**

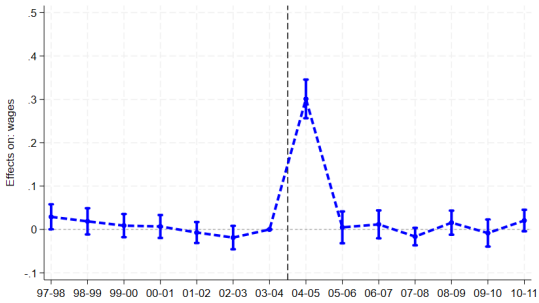


Panel C: **Hours Worked**

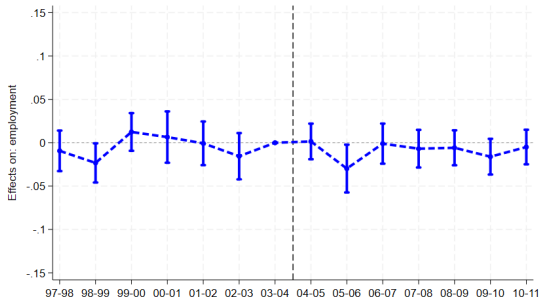
NOTE: Event-study graphs for changes in labor market outcomes using an alternative treatment definition, where the treatment group includes workers earning between 0.75 and 1.75 times the minimum wage (MW), while the control group consists of workers earning above 1.75 times the MW. Round markers represent estimates for the sequence of β coefficients from Equation 1. Panel A shows estimates for wage growth (wages in logs). Panel B displays estimates for employment probability. Panel C presents estimates for hours growth (hours in logs). Vertical segments indicate 95% confidence intervals, using standard errors clustered at the 2-digit economic sector level interacted with a binary variable for geographic location (capital vs. rest of the country/missing). The dashed vertical line separates the pre- and post-minimum wage policy change periods.

Figure C.2

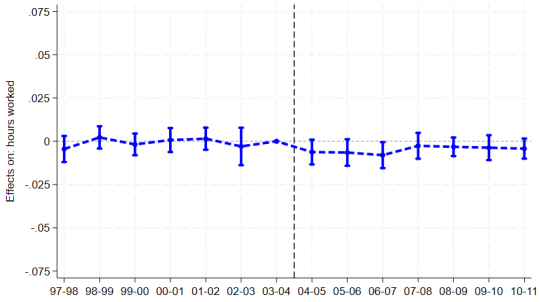
Event Study: Labor Market Outcomes - One year



Panel A: **Wages**



Panel B: **Employment**

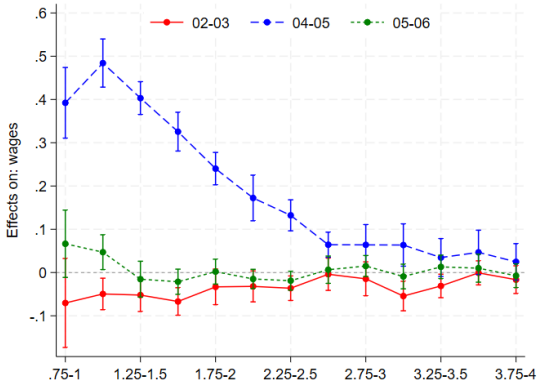


Panel C: **Hours Worked**

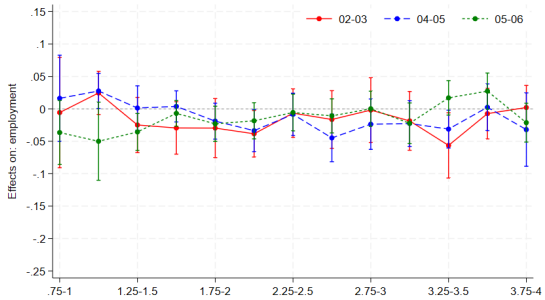
NOTE: Event-study graphs for one year changes in labor market outcomes (i.e., $\Delta Y_{i,t} = Y_{i,t} - Y_{i,t-1}$). Round markers represent estimates for the sequence of β coefficients from Equation 1. Panel A shows estimates for wage growth (wages in logs). Panel B displays estimates for employment probability. Panel C presents estimates for hours growth (hours in logs). Vertical segments indicate 95% confidence intervals, using standard errors clustered at the 2-digit economic sector level interacted with a binary variable for geographic location (capital vs. rest of the country/missing). The dashed vertical line separates the pre- and post-minimum wage policy change periods.

Figure C.3

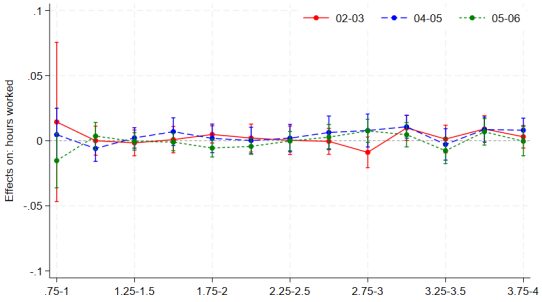
Bins Approach: Labor Market Outcomes - One year



Panel A: **Wages**



Panel B: **Employment**

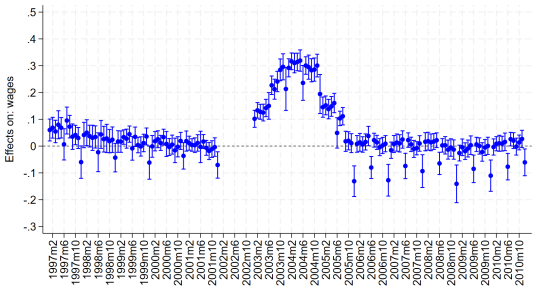


Panel C: **Hours Worked**

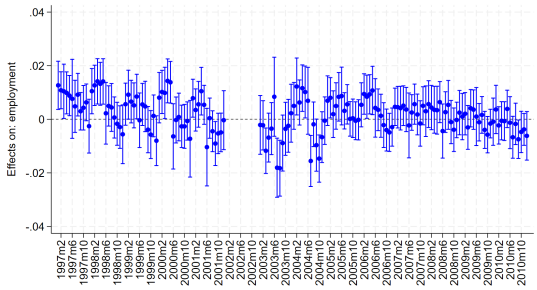
NOTE: Results from the bin approach obtained by estimating Equation 2 for one year changes in labor market outcomes (i.e., $\Delta Y_{i,t} = Y_{i,t} - Y_{i,t-1}$). Panel A presents estimates for wage growth (log wages) over a two-year window, capturing excess wage growth by wage bin for the periods 2002–2003 (in red), 2004–2005 (in blue), and 2005–2006 (in green), relative to 2003–2004. Panel B reports estimates for the probability of employment in period t for workers employed in $t-2$, by wage bin, for the same periods. Panel C presents estimates for hours worked growth (log hours) over a two-year window for the same periods. In all figures, wage bins on the horizontal axis are measured in minimum wage units—e.g., the bin 0.75–1 includes individuals who earned between 0.75 and 1 times the minimum wage in the base year. Workers earning more than four times the minimum wage but below the 75th percentile of the wage-bin distribution are used as the reference category. Vertical segments indicate 95% confidence intervals, using standard errors clustered at the 2-digit economic sector level interacted with a binary variable for geographic location (capital vs. rest of the country/missing).

Figure C.4

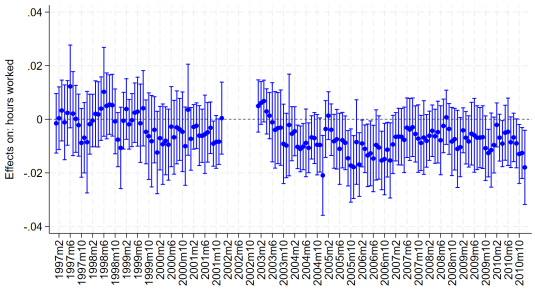
Event Study: Labor Market Outcomes - Two years by month



Panel A: **Wages**



Panel B: **Employment**

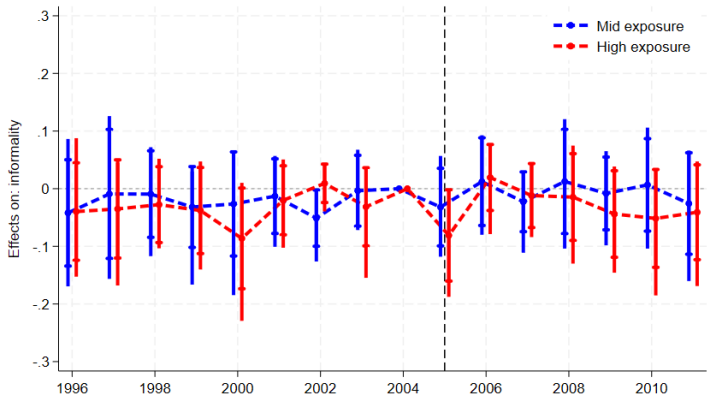


Panel C: **Hours Worked**

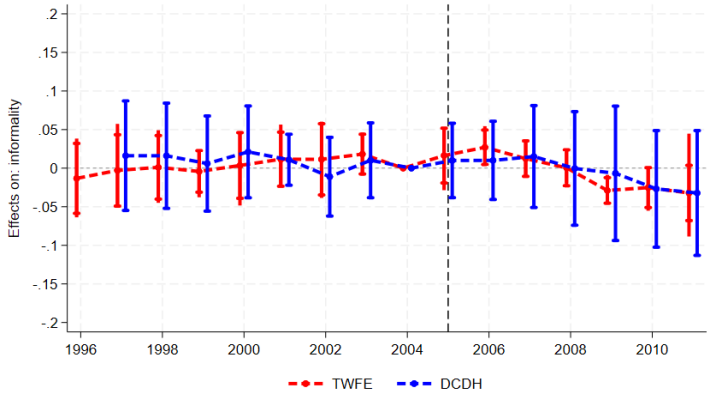
NOTE: Event-study graphs for changes in labor market outcomes by month. this Figure reproduce the Figure6 by each month of the year. Round markers represent estimates for the sequence of β coefficients from Equation 1. Panel A shows estimates for wage growth (wages in logs). Panel B displays estimates for employment probability. Panel C presents estimates for hours growth (hours in logs). Vertical segments indicate 95% confidence intervals, using standard errors clustered at the 2-digit economic sector level interacted with a binary variable for geographic location (capital vs. rest of the country/missing). The dashed vertical line separates the pre- and post-minimum wage policy change periods.

Figure C.5

Event-Study Graph: effects on informality Using survey data



Panel A: Excluding the capital (Montevideo)

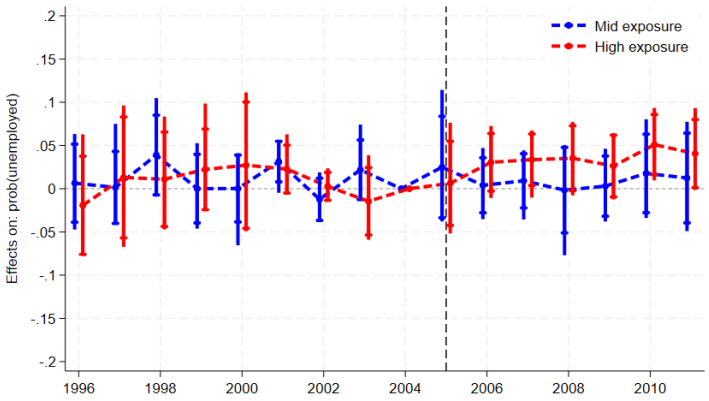


Panel B: Time-varying exposure measure

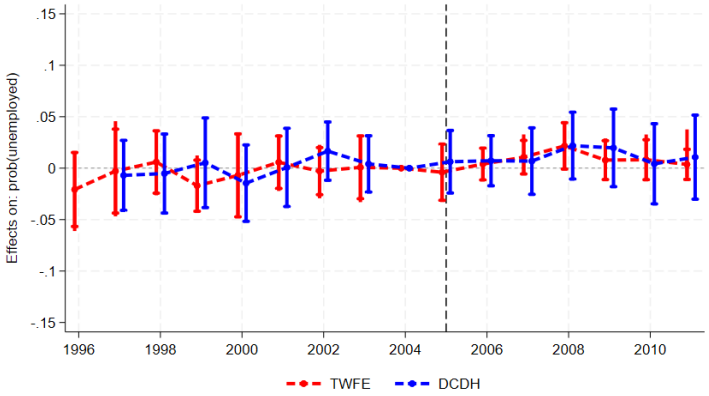
NOTE: Event-study graphs for informality using household survey data (Encuesta Continua de Hogares, ECH). In both panels, the outcome variable is binary, taking the value of one if workers do not have access to social security and zero if they do have. Panel A presents estimates for the sequence of β coefficients from Equation 3, excluding the capital (Montevideo), and using a discrete exposure measure at the department level with three categories—low, mid (blue markers), and high (red markers) exposure—with low exposure as the reference category. Panel B presents estimates using a time-varying exposure measure, meaning treatment can switch on and off after the 2005 policy change, and a discrete exposure classification with two categories (low and mid/high exposure) using low exposure as the reference category. Red markers represent estimates from the two-way fixed effects method, while blue markers correspond to the approach proposed by DCDH. Vertical segments indicate 95% confidence intervals (CIs) using wild bootstrap standard errors clustered at the department level, while capped spikes illustrate CIs using conventional clustered standard errors. When applying the DCDH method, bootstrap standard errors clustered at the department level are used. The dashed vertical line separates the pre- and post-minimum wage policy change periods.

Figure C.6

Event-Study Graph: effects on unemployment Using survey data



Panel A: Excluding the capital (Montevideo)

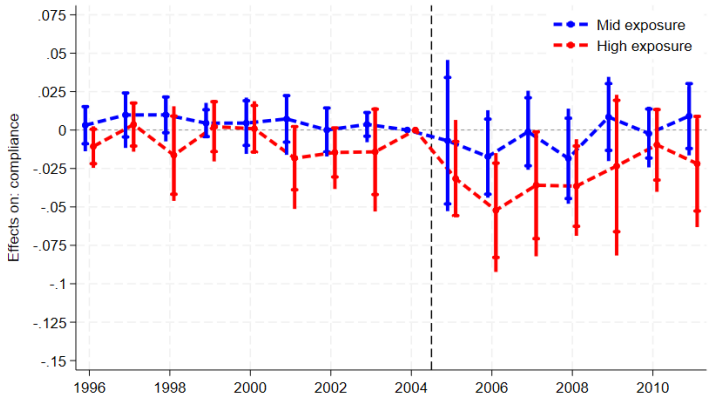


Panel B: Time-varying exposure measure

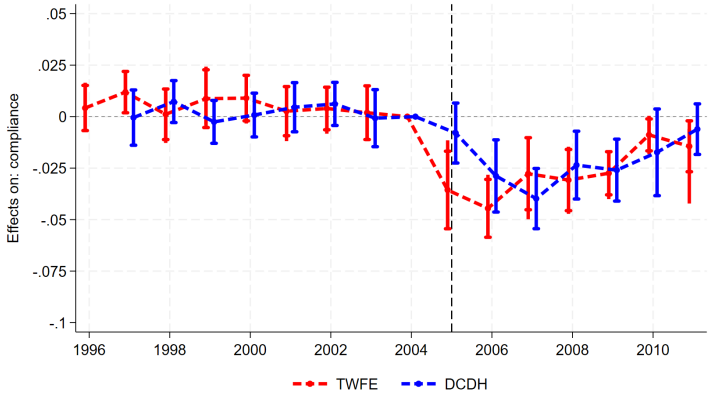
NOTE: Event-study graphs for unemployment probability using household survey data (Encuesta Continua de Hogares, ECH). In both panels, the outcome variable is binary, taking the value of one if individual i is unemployed and zero if employed. Panel A presents estimates for the sequence of β coefficients from Equation 3, excluding the capital (Montevideo), and using a discrete exposure measure at the department level with three categories—low, mid (blue markers), and high (red markers) exposure—with low exposure as the reference category. Panel B presents estimates using a time-varying exposure measure, meaning treatment can switch on and off after the 2005 policy change, and a discrete exposure classification with two categories (low and mid/high exposure) using low exposure as the reference category. Red markers represent estimates from the two-way fixed effects method, while blue markers correspond to the approach proposed by DCDH. Vertical segments indicate 95% confidence intervals (CIs) using wild bootstrap standard errors clustered at the department level, while capped spikes illustrate CIs using conventional clustered standard errors. When applying the DCDH method, bootstrap standard errors clustered at the department level are used. The dashed vertical line separates the pre- and post-minimum wage policy change periods.

Figure C.7

Event-Study Graph: effects on compliance Using survey data



Panel A: Excluding the capital (Montevideo)

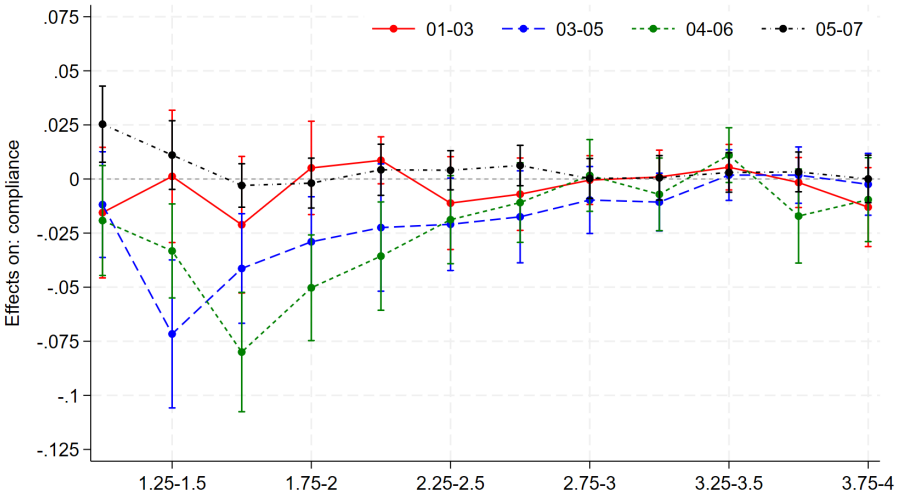


Panel B: Time-varying exposure measure

NOTE: Event-study graphs for compliance using survey data (Encuesta Continua de Hogares, ECH). The outcome variable is binary, taking the value of one if the hourly wage remains above the minimum wage in both years, $t-2$ and t, and zero if the hourly wage was above the minimum wage in $t-2$ but dropped below it in t. Panel A presents estimates for the sequence of β coefficients from Equation 3, excluding the capital (Montevideo), and using a discrete exposure measure at the department level with three categories—low, mid (blue markers), and high (red markers) exposure—with low exposure as the reference category. Panel B presents estimates using a time-varying exposure measure, meaning treatment can switch on and off after the 2005 policy change, and a discrete exposure classification with two categories (low and mid/high exposure) using low exposure as the reference category. Red markers represent estimates from the two-way fixed effects method, while blue markers correspond to the approach proposed by DCDH. Vertical segments indicate 95% confidence intervals (CIs) using wild bootstrap standard errors clustered at the department level, while capped spikes illustrate CIs using conventional clustered standard errors. When applying the DCDH method, bootstrap standard errors clustered at the department level are used. The dashed vertical line separates the pre- and post-minimum wage policy change periods.

D. Further results

FIGURE D.1
BINS APROACH: COMPLIANCE



NOTE: Results from the bin approach are obtained by estimating Equation 2. Compliance is measured as a binary variable that equals one if the hourly wage remains above the minimum wage in both years, $t-2$ and t, and zero if it was above the minimum wage in $t-2$ but dropped below it in t. Wage bins on the horizontal axis are measured in minimum wage units—for example, the bin 1.25–1.5 includes individuals who earned between 1.25 and 1.5 times the minimum wage in the base year. Twelve equal-sized wage bins were constructed—excluding the bin 0.75-1 minimum wage. By definition, there are no individuals in the bin between 0.75 and 1 minimum wage at $t-2$. The reference category consists of individuals earning more than four times the minimum wage but less than the 75th percentile of the wage bin distribution. Vertical segments indicate 95% confidence intervals, with standard errors clustered at the 2-digit economic sector level and interacted with a binary variable for geographic location (capital vs. rest of the country/missing).

Table D.1

Impact of the MW: Compliance - Bins Approach

	(1)
	$Pr(Compliance)$
Panel A: Wage bin in t-2 (1, 1.75)	
$2001 vs. 2003 (placebo)$	-0.007
	(0.011)
$2003 vs. 2005$	-0.039
	(0.011)
$2004 vs. 2006$	-0.035
	(0.010)
$2005 vs. 2007$	0.018
	(0.006)
Panel B: Wage bin in t-2 (1.75, 2.5)	
$2001 vs. 2003 (placebo)$	-0.006
	(0.006)
$2003 vs. 2005$	-0.031
	(0.007)
$2004 vs. 2006$	-0.038
	(0.007)
$2005 vs. 2007$	-0.005
	(0.006)
Panel C: Wage bin in t-2 (2.5, 4)	
$2001 vs. 2003 (placebo)$	0.003
	(0.004)
$2003 vs. 2005$	-0.005
	(0.004)
$2004 vs. 2006$	-0.002
	(0.005)
$2005 vs. 2007$	0.007
	(0.004)

NOTE: Estimates are based on a bin approach using data from 2001 to 2007. Workers are grouped into four wage-to-minimum-wage bins, with the fourth bin—those earning above four times the minimum wage—serving as the base category. The compliance outcome is a binary indicator that equals one if individual i has a wage above the minimum wage at both $t-2$ and t, and zero if the individual was above the minimum wage at $t-2$ but below it at t. By definition, there are no individuals in the bin between 0.75 and 1 minimum wage at $t-2$. Effects are estimated for three periods: 2001-2003 (placebo), 2003-2005, 2004-2006, and 2005-2007, using the 2002-2004 period as the reference. Standard errors are clustered at the 2-digit economic sector level interacted with a binary variable for geographic location (i.e., capital vs. rest of the country/missing), and are reported in parentheses.

Table D.2

Impact of the MW on Compliance & Labor Market Outcomes by Firm Size

	(1) Pr(Compliance)	(2) ΔLog(Wages)	(3) Pr(Employed)	(4) ΔLog(Hours Worked)
$T(w_{i,2001}) \times \mathbb{1}\{t = 2003\} \times \mathbb{1}\{Size > Top\}$	-0.033	-0.073	-0.058	-0.018
	(0.037)	(0.078)	(0.032)	(0.019)
$T(w_{i,2001}) \times \mathbb{1}\{t = 2003\} \times \mathbb{1}\{Size < Bottom\}$	-0.002	-0.026	-0.026	-0.005
	(0.011)	(0.021)	(0.022)	(0.009)
$T(w_{i,2003}) \times \mathbb{1}\{t = 2005\} \times \mathbb{1}\{Size > Top\}$	-0.024	0.038	-0.027	-0.010
	(0.024)	(0.085)	(0.050)	(0.014)
$T(w_{i,2003}) \times \mathbb{1}\{t = 2005\} \times \mathbb{1}\{Size < Bottom\}$	-0.030	0.311	-0.033	-0.014
	(0.015)	(0.034)	(0.026)	(0.020)
$T(w_{i,2004}) \times \mathbb{1}\{t = 2006\} \times \mathbb{1}\{Size > Top\}$	-0.031	0.045	0.030	0.007
	(0.022)	(0.104)	(0.033)	(0.012)
$T(w_{i,2004}) \times \mathbb{1}\{t = 2006\} \times \mathbb{1}\{Size < Bottom\}$	-0.033	0.290	-0.046	-0.025
	(0.013)	(0.029)	(0.027)	(0.014)
$T(w_{i,2005}) \times \mathbb{1}\{t = 2007\} \times \mathbb{1}\{Size > Top\}$	0.003	-0.250	0.057	0.003
	(0.018)	(0.094)	(0.033)	(0.013)
$T(w_{i,2005}) \times \mathbb{1}\{t = 2007\} \times \mathbb{1}\{Size < Bottom\}$	0.006	0.041	-0.084	-0.044
	(0.009)	(0.026)	(0.027)	(0.014)
Obs.	20182	20429	29367	20429

NOTE: Estimates are based on individual-level regressions using data from 2001 to 2007. For compliance, the outcome is a binary indicator equal to one if the hourly wage of individual i remains above the hourly minimum wage in both years, $t-2$ and t, and zero if the hourly wage was above the minimum wage in $t-2$ but dropped below it in t. For wages and hours worked, two-year changes—i.e., $\Delta Y_{i,t} = Y_{i,t} - Y_{i,t-2}$—were used. For employment, the outcome is a binary indicator equal to one if individual i is employed in both $t-2$ and t, and zero if the individual was employed in $t-2$ but no longer employed in t. $\mathbb{1}\{Size > Top\}$ equals one for workers in firms with a number of workers above the 75 percentile (which is equal to one) and $\mathbb{1}\{Size < Bottom\}$ equals one for workers in firms with a number of workers bellow the 25 percentile. Effects are estimated for four periods: 2001–2003 (placebo), 2003–2005, 2004–2006, and 2005-2007, using the 2002–2004 period as the reference. Standard errors are clustered at the 2-digit economic sector level and interacted with a binary variable for geographic location (capital vs. rest of the country/missing), they are reported in parentheses.

Table D.3

Impact of the MW on Compliance & Labor Market Outcomes by Firm Size (# of branches)

	(1) Pr(Compliance)	(2) ΔLog(Wages)	(3) Pr(Employed)	(4) ΔLog(Hours Worked)
$T(w_{i,2001}) \times \mathbb{1}\{t = 2003\} \times \mathbb{1}\{Size > Med\}$	-0.005	0.014	0.017	0.007
	(0.009)	(0.025)	(0.018)	(0.007)
$T(w_{i,2001}) \times \mathbb{1}\{t = 2003\} \times \mathbb{1}\{Size < Med\}$	-0.011	-0.043	-0.028	-0.016
	(0.009)	(0.022)	(0.016)	(0.007)
$T(w_{i,2003}) \times \mathbb{1}\{t = 2005\} \times \mathbb{1}\{Size > Med\}$	-0.034	0.306	0.030	-0.007
	(0.010)	(0.024)	(0.020)	(0.013)
$T(w_{i,2003}) \times \mathbb{1}\{t = 2005\} \times \mathbb{1}\{Size < Med\}$	-0.036	0.257	-0.029	-0.010
	(0.010)	(0.028)	(0.016)	(0.007)
$T(w_{i,2004}) \times \mathbb{1}\{t = 2006\} \times \mathbb{1}\{Size > Med\}$	-0.042	0.303	0.004	-0.001
	(0.011)	(0.025)	(0.026)	(0.010)
$T(w_{i,2004}) \times \mathbb{1}\{t = 2006\} \times \mathbb{1}\{Size < Med\}$	-0.037	0.251	-0.031	-0.016
	(0.010)	(0.030)	(0.014)	(0.007)
$T(w_{i,2005}) \times \mathbb{1}\{t = 2007\} \times \mathbb{1}\{Size > Med\}$	-0.000	0.022	-0.002	-0.011
	(0.008)	(0.025)	(0.026)	(0.010)
$T(w_{i,2005}) \times \mathbb{1}\{t = 2007\} \times \mathbb{1}\{Size < Med\}$	0.006	-0.005	-0.029	-0.023
	(0.006)	(0.020)	(0.014)	(0.008)
Obs.	40780	41237	58471	41237

NOTE: Estimates are based on individual-level regressions using data from 2001 to 2007. For compliance, the outcome is a binary indicator equal to one if the hourly wage of individual i remains above the hourly minimum wage in both years, $t-2$ and t, and zero if the hourly wage was above the minimum wage in $t-2$ but dropped below it in t. For wages and hours worked, two-year changes—i.e., $\Delta Y_{i,t} = Y_{i,t} - Y_{i,t-2}$—were used. For employment, the outcome is a binary indicator equal to one if individual i is employed in both $t-2$ and t, and zero if the individual was employed in $t-2$ but no longer employed in t. $\mathbb{1}\{Size > Med\}$ equals one for workers in firms with a number of branches above the median (which is equal to one). Effects are estimated for three periods: 2001–2003 (placebo), 2003–2005, 2004–2006, and 2005-2007, using the 2002–2004 period as the reference. Standard errors are clustered at the 2-digit economic sector level, interacted with a binary variable for geographic location (capital vs. rest of the country/missing), and reported in parentheses.

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