Minimum Wages and the Distribution of Firm Wage Premia*

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Abstract

This paper leverages a large minimum wage reform in Uruguay to study the effects of minimum wages on the distribution of firm wage premia. The reform significantly decreased wage inequality, mainly by reducing between-firm inequality. AKM and time-varying AKM analyses reveal a large compression in the distribution of firm fixed effects after the reform, driven by an increase in the fixed effects of low-premium firms. Firm-level and worker-level difference-in-differences analyses document a causal effect of the reform on the compression of firm fixed effects. Results suggest minimum wages can increase the supply of "good jobs" by "making bad jobs better".

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1 Introduction

Firms affect the levels and trends of wage inequality (Kline, 2024). This conclusion usually follows from the observation that, after fitting the two-way fixed-effects model of Abowd et al. (1999) (AKM), a non-trivial portion of the variance of log wages is explained by the variance of the estimated firm fixed effects (F-FEs). Recent work investigates the drivers of cross-sectional variation in firm wage premia, suggesting that heterogeneity in firm-level attributes such as productivity, rent-sharing, and compensating differentials may rationalize equilibrium dispersion in firm pay when labor markets have frictions. Most of the discussion about the determinants of F-FEs, however, abstracts from how labor market policies mediate the firms' ability to set wages. Including labor market policies in the analysis can deepen the understanding of equilibrium dispersion in firm pay for similar workers and offer a rationale for the differential role of F-FEs in explaining wage inequality across countries and within countries across time. For example, Card et al. (2013) argue that the increased importance of F-FEs in recent decades in Germany coincides with a decrease in the prevalence of collective bargaining, while Song et al. (2019) conjecture that the stable role of F-FEs in the US is related to the small role of labor market institutions. Do these hypotheses reflect a causal impact of labor market policies on the distribution of F-FEs?

To make progress on this question, this paper conducts a battery of descriptive and causal empirical analyses to investigate the effects of minimum wages on the distribution of firm wage premia. There are two channels through which minimum wages can mediate the effect of firms on aggregate wage inequality. First, there is a "composition" channel: minimum wages can induce employment reallocation across employers and between employment and non-employment (Dustmann et al., 2022; Engbom and Moser, 2022; Berger et al., 2025). Then, for a fixed set of F-FEs, reallocation effects impact how the cross-sectional dispersion in firm wage premia maps to the overall variance of wages. Second, there is a "causal" channel: minimum wages can generate within-firm wage spillovers, influence how rents are distributed between workers and employers (through, for example, profit effects), and impact the levels of rents to be split (through, for example, productivity effects) (Dube and Lindner, 2024). Through the lens of the AKM framework, this channel suggests that minimum wages can have a causal effect on the level of the F-FEs, thereby affecting their distribution even in the absence of employment reallocation.

We use matched employer-employee data from Uruguay, built from administrative social security records for the period 1997-2013, to explore the effects of minimum wages on the distribution of F-FEs. The raw data is monthly and contains information on earnings and hours, allowing us to measure job transitions and hourly wages precisely. Uruguay prominently increased the national minimum wage (NMW) in 2005 after several years of it being non-binding. In 2005, the nominal minimum wage increased by almost 80%, while annual inflation rates were around 5%. The government kept implementing nominal increases above inflation after 2005: in 2013, the real minimum wage was more than three times the 2004 level. These increases had implications for the "bite" of the minimum wage. Between 1997 and 2004, the Kaitz index (minimum wage as a share of the median wage) averaged 9.8%. The index rose sharply to 24.7% in 2005 and continued to increase after the reform, reaching 45.8% in 2013.

We start with a descriptive analysis of the evolution of wage inequality around the minimum wage reform. The reform coincided with a sharp and substantial decrease in wage inequality. The standard deviation of log hourly wages grew from 75 log points in 1997 to 80 log points in 2004. In 2005, it decreased to 74 log points – reversing, in one year, more than seven years of increased inequality – and it kept falling throughout the post-reform period, reaching 61 log points in 2013. The evolution of different wage percentiles reveals that the decrease in wage inequality after the minimum wage reform was driven by a higher wage growth of the lower percentiles. Importantly, almost all of the reduction in wage inequality observed after 2005 was explained by a decrease in between-firm (rather than within-firm) inequality.

These stylized facts are complemented with AKM estimates using overlapping 5-year periods around the minimum wage reform (1997-2001, 2001-2005, 2005-2009, and 2009-2013). Bias-corrected variance decompositions following Kline et al. (2020) show that the levels of all components of wage inequality fell after 2005, but that the decrease was particularly large for the firm component. As a result, the relative importance of firms in explaining wage inequality decreased after the minimum wage reform. Relative to the pre-reform period, the share explained by worker fixed effects increased in the post-2005 period, and the share explained by the covariance term remained constant. By contrast, the share explained by F-FEs decreased from almost 30% in the pre-2005 period to 22% in 2005-2009 and 19% in 2009-2013.

We then estimate a Time-Varying AKM model (Engbom et al., 2023; Lachowska et al., 2023, TV-AKM) that generalizes the AKM specification to allow for time-varying F-FEs, endowing us with F-FEs estimates that vary at the firm-by-year level. We first use corporate tax records for the period 2009-2013 to compute value added per worker and show, consistent with previous literature, that F-FEs are positively correlated with firm productivity (Kline, 2024). We show that this positive relationship remains large and significant after controlling for industry and firm indicators, suggesting that within-firm across-time variation in value added per worker predicts drifts in the F-FEs. However, the relationship disappears after including year fixed effects, suggesting that productivity-driven within-firm drifts in F-FEs likely represent aggregate productivity shocks. We also use the tax data to proxy for heterogeneous rent-sharing behavior by computing measures of firm-level labor shares (total gross wages over value added) and find that, conditional on value added per worker, F-FEs are positively correlated with firms' labor shares, with a steep slope that is robust to the inclusion of industry and firm indicators and year fixed effects. This pattern, which, to the best of our knowledge, has not been documented in the literature, suggests that firm-level variation in rent-sharing, both across firms and within firms over time, plays a crucial role in rationalizing the dispersion in F-FEs, conditional on the known role of heterogeneous productivities.

Using the time-varying F-FEs, we then show that, before the 2005 reform, the short-run within-firm persistence in pay premia was substantial: binned scatter plots of F-FEs in year t against F-FEs in year $t-1$ lie almost exactly on the 45-degree line, with some generalized cyclical component in recession years, echoing the findings documented in Engbom et al. (2023) and Lachowska et al. (2023). However, we document a trend break for low-paying firms after the reform. Starting in 2005, the same binned scatter plots start displaying a "hockey stick" pattern: the F-FEs of firms at the bottom of the distribution display large annual growth rates while firms up in the distribution remain close to the 45-degree line.

Through time, this differential growth pattern generates a sizable compression in the support of the F-FEs distribution, as firms with low initial pay premia catch up with firms up in the distribution. These patterns are not explained by composition effects, as the same behavior is observed in longer horizons when restricting to the balanced panel of firms that are active throughout our complete analysis period.

These analyses show that, after the NMW reform, the wage distribution was compressed because of a corresponding compression in the distribution of F-FEs. While the timing and mechanisms suggest the NMW reform played a crucial role in these dynamics, the descriptive facts are not sufficient to conclude a causal relationship. Moreover, the reform was enacted after a recession, so business cycle dynamics and mean reversion could also explain the observed outcomes. Given these confounders, the final part of the paper empirically tests whether the minimum wage causally affected the distribution of F-FEs.

We proceed with two designs. First, a firm-level design exploits variation in exposure to the NMW reform using the GAP strategy proposed in Card and Krueger (1994) and recently used by Draca et al. (2011), Dustmann et al. (2022), and Derenoncourt and Weil (2025) to estimate a difference-in-differences (DID) model. The (continuous) treatment variable is the increase in labor costs that firms would have to incur to comply with the new NMW based on pre-reform hourly wages, so firms with a higher gap between observed and counterfactual labor costs are said to be more exposed to the reform. The results show that more exposed firms had similar trends in real hourly wages relative to less exposed firms before 2005, but that real hourly wages increased sharply and permanently in the treated firms starting in 2005. We find that almost all of the increase in the firm-level average log real hourly wage is explained by an increase in the time-varying F-FE. We also show that, in the medium run, firms accommodate the NMW increase by changing the composition of workers, as the average worker fixed effect also increases in relatively more exposed firms after the NMW reform (Clemens et al., 2021; Butschek, 2022). However, the worker-composition effect is an order of magnitude smaller than the effect on the F-FE.

Finally, we implement a worker-level design, better suited for studying aggregate impacts and between- and within-firm spillovers, closely following Dustmann et al. (2022). We estimate the effect of the NMW reform on 2-year growth rates of different outcomes of interest using a DID design that sorts workers into wage bins based on their baseline real hourly wages. This design mimics a standard DID model where the unit of analysis is the initial wage bin, so treatment effects are recovered by comparing outcomes before and after the NMW reform between low real hourly wage workers (directly exposed to the NMW reform) and workers higher in the wage distribution. The focus on 2-year growth rates allows us to deal with the well-known fact that mean reversion implies that wage growth is faster at the bottom of the wage distribution, despite the weaker employment prospects (Ashenfelter and Card, 1982). The results show that the real hourly wage growth by wage bin was constant before 2005 in all wage bins but sharply increased in 2005 and 2006 at the bottom of the wage distribution, especially for workers earning below the new NMW before the reform. We find evidence of wage spillovers to workers earning more than the new NMW that decrease in the distance to the NMW and dissipate when wages reach about 1.5 times the NMW level. We estimate no effect on the probability of remaining employed and on hours worked by wage bin, suggesting that the increase in wages was not accompanied by disemployment effects.

With this design, we also provide evidence on the two channels through which the minimum wage can affect the distribution of F-FEs. First, we find evidence of reallocation ("composition") effects by replicating Dustmann et al. (2022) exercise. Job switchers at the bottom of the distribution moved to firms with higher F-FEs after the NMW reform. We also find evidence of productive reallocation in the pre-reform recession, consistent with the "cleansing" hypothesis based on Schumpeter (1939) tradition. However, job-stayers experienced substantially larger increases in their F-FEs and exhibited even larger wage spillovers throughout the wage distribution, consistent with a causal impact of the minimum wage on the level of the F-FE. Importantly, the wage spillovers on workers that earn significantly above the NMW are mainly observed in firms directly exposed to the reform (i.e., in firms with workers earning below the post-2005 NMW in the pre-period), further supporting the interpretation that the NMW reform changed the wage-setting and rent-sharing behavior of the firms beyond mere compliance with the policy. A simple decomposition exercise shows that this "causal" effect on the F-FEs of stayers accounts for more than 70% of the total wage effect across all exposed wage bins, while the relative importance of reallocation effects is concentrated at the bottom bin and accounts for around 20-30% of its wage effect.

In sum, this paper shows that public policy, in particular the minimum wage, can affect the wage-setting power of firms, having sizable implications for the aggregate effects of firms on wage inequality. Under the interpretation that F-FEs are proxies for job quality, our results suggest that minimum wages can increase the supply of "good jobs" (Acemoglu, 2001; Rodrik and Stantcheva, 2021) by "making bad jobs better" through increased rent-sharing, in addition to reallocating workers towards "good jobs".

Related literature. This paper contributes to a large and growing literature on firm wage setting power and F-FEs, recently surveyed in Kline (2024). This literature has gradually shifted from empirically documenting the role of firms in wage inequality by estimating the AKM model (and subjecting it to empirical scrutiny) to studying the structural drivers of equilibrium dispersion in pay premia. Recent work suggests that firm-level heterogeneity in productivity, rent-sharing, and non-wage amenities can rationalize equilibrium pay dispersion in the presence of imperfect labor market competition (Card et al., 2018; Sorkin, 2018; Engbom and Moser, 2022; Lamadon et al., 2022, 2025; Di Addario et al., 2023; Haanwinckel, 2025; Morchio and Moser, 2025). We contribute to this discussion by empirically showing that labor market institutions and, in particular, the minimum wage, can be important mediators of the wage setting power of firms, consequently affecting the distribution of F-FEs. Only a few empirical papers directly link AKM F-FEs with labor market policies and institutions (see Beauregard et al., 2025; Derenoncourt et al., 2025 for unions; Cruz and Rau, 2022 for equal pay laws). To the best of our knowledge, we are the first to empirically document the causal effect of minimum wage reforms on F-FEs, showing they compress their distribution by increasing the F-FEs of initially low-paying firms.

Within the AKM literature, Engbom et al. (2023) and Lachowska et al. (2023) study the time-series behavior of F-FEs, mostly motivated by the literature on rent-sharing of firm-specific productivity shocks (e.g., Kline et al., 2019; Garin and Silvério, 2024). Engbom et al. (2023) and Lachowska et al. (2023) find that, despite exhibiting some aggregate cyclical behavior, F-FEs are remarkably persistent at the firm level. Our results warn that the within-firm persistence in F-FEs can be broken by changes in labor market institutions: we document aggregate cyclicality and strong persistence before the NMW reform, but find a sharp trend break at the bottom of the distribution after the minimum wage increase.

Our paper also contributes more generally to the literature on rent-sharing and the minimum wage. A long empirical tradition documents significant wage effects of minimum wage reforms with elusive employment effects (Manning, 2021; Dube and Lindner, 2024). Our estimates of the NMW on several outcomes (wages, employment, hours, spillovers, worker composition, etc.) add to this vast empirical literature. More importantly, we show that the wage effects with no employment losses, usually interpreted merely as the mechanical effect of compliance, can be rationalized as an increase in rent-sharing within the firm, a mechanism suggested in Engbom and Moser (2022) and Bassier and Budlender (2025) and consistent with empirical evidence on profit effects of minimum wage reforms (Draca et al., 2011; Harasztosi and Lindner, 2019; Drucker et al., 2021; Vergara, 2026). Our estimates of wage spillovers further support this interpretation. Also, a long tradition documents wage compression effects of the minimum wage (e.g., Lee, 1999; Autor et al., 2016; Dube, 2019; Fortin et al., 2021; Giupponi et al., 2024). Our results suggest that changes in the cross-sectional profile of F-FEs are an important mediator of this compression.

2 Institutional Setting

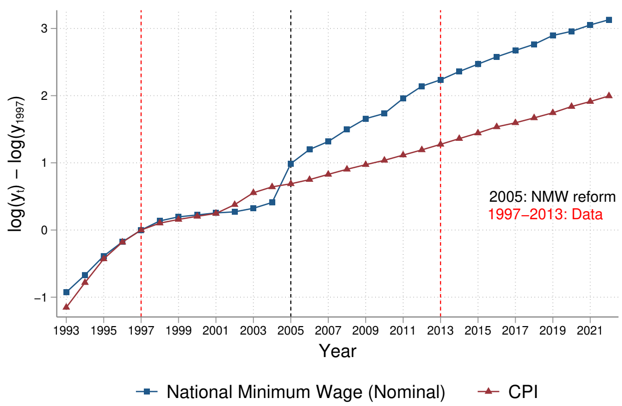
Uruguay is an upper-middle-income country in South America with a current population of almost 3.5 million. Uruguay performs well above the regional standards in terms of income, corruption, and human development.[1] The country's employment structure is centered around the services sector, with an approximate employment share of 70%. Agriculture and manufacturing account for about 10% and 20% of total employment, respectively. The public sector accounts for around 15% of employment, with special participation in education, healthcare, and public administration.

2.1 The National Minimum Wage and the 2005 reform

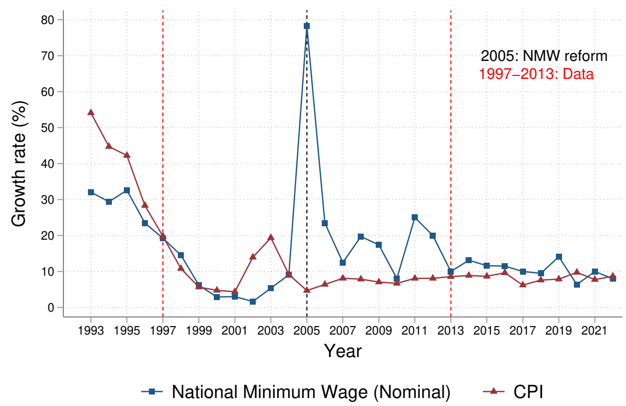
The National Minimum Wage (NMW) in Uruguay has a long history. However, for several decades, it was a non-binding policy. Established by Decree 1,534/969 on November 28, 1969, the NMW in Uruguay sets a nationwide wage floor for all private sector employees aged 18 and older. During the 1970s and 1980s, the real NMW substantially decreased since large annual inflation rates completely undermined its nominal raises. In the 1990s and early 2000s, inflation rates were controlled, but nominal NMW increases were also muted. The lack of relevant hikes in the NMW mainly relates to two reasons. First, the NMW was used as a reference point for calculating several social benefits provided by the central government. Second, personal income tax brackets were also indexed to the value of the NMW. Both factors led governments to avoid increasing the NMW. By 2003, the real NMW had fallen to just a quarter of its

[1] All these statistics were accessed in February 2026. According to the World Bank, Uruguay's GDP per capita (PPP) in 2021 US dollars was $32,039 in 2024 (https://data.worldbank.org/indicator/NY.GDP.PCAP.PP.KD). Transparency International ranks Uruguay 13th out of 180 countries in terms of corruption (https://www.transparency.org/en/cpi/2019/results), while the United Nations Development Programme ranks Uruguay 48th among 193 countries in terms of human development (https://hdr.undp.org/data-center/country-insights#/ranks).

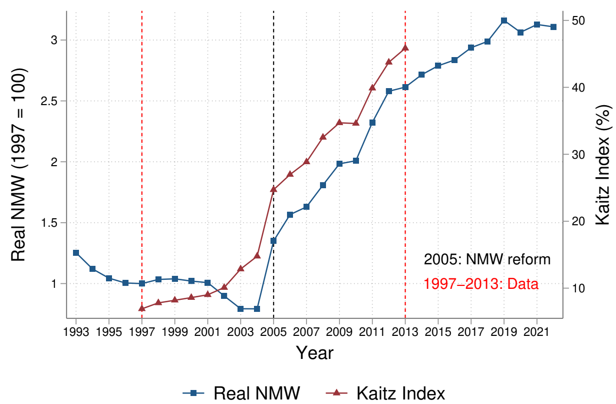
Figure 1: Evolution of the National Minimum Wage (NMW)



(a) Log difference relative to 1997



(b) Annual growth rates



(c) Real minimum wage and Kaitz index

Notes: These figures plot the evolution of the national minimum wage (NMW) between 1993 and 2022. In each figure, the black vertical line in 2005 accounts for the year of the minimum wage reform, and the red vertical lines in 1997 and 2013 account for the period for which we have access to the administrative records. CPI series comes from official reports of the Uruguayan National Institute of Statistics. In Panel (a), each dot represents the log difference relative to 1997. In Panel (b), each dot represents nominal annual growth rates. In Panel (c), the real minimum wage is normalized to be 1 in 1997, and the Kaitz index is computed as the hourly minimum wage divided by the median hourly wage computed using the administrative records, which is why the Kaitz index series is limited to the period 1997-2013. We compute the median hourly wage using our annual panel of formal workers before imposing restrictions specific to the AKM model (i.e., before dropping observations based on age, firm size, labor market attachment, and connected set).

1969 value, rendering it largely ineffective as a labor market institution.

A recession in the early 2000s led to substantial real wage decreases, especially at the bottom of the wage distribution (see Section 4). Motivated by this fact, the new government elected in 2004 reinstated the NMW as a key policy instrument. The government detached the NMW from the determination of social benefits and income tax brackets, freeing it from the constraints imposed by other fiscal policies. This shift triggered a period of robust real growth for the NMW, beginning in 2005.

Figure 1 shows the evolution of the NMW between 1993 and 2022. Panel (a) shows log differences relative to 1997 for the nominal NMW and the Consumption Price Index (CPI), while Panel (b) shows the annual growth rates of the same variables. Between 1993 and 2001, the NMW exactly traced the evolution of the CPI, meaning that the NMW stayed constant in real terms. Between 2002 and 2004, the real NMW experienced a decrease due to the increased inflation rates. In 2005, however, the nominal NMW increased by almost 80%, with an annual inflation rate on the order of 5%. Large nominal increases in the NMW above inflation rates persisted in the decade after the reform, stabilizing to approximately track inflation only around 2013. Panel (c) shows that this reform substantially increased the real NMW. The real increase in 2005 was around 70%, and in 2013 the real NMW was more than three times its 2004 value. This real increase was accompanied by an increase in the "bite" of the NMW. Between 1997 and 2004, the Kaitz index (minimum wage as a share of the median wage) was, on average, 9.8%. The index rose to 24.7% in 2005 and kept increasing after the reform, reaching 45.8% in 2013.[2]

The goal of this paper is to study the effects of this large NMW reform on wage inequality, focusing on the role of firms and their wage-setting policies as mediators. The 2005 reform coincided with other policy discussions, the two most important being the restoration of a collective bargaining system and a progressive income tax reform. We briefly discuss these institutions, their interactions with the NMW reform, and the concerns they may raise for our analysis below, concluding that they do not confound our analysis of the NMW.

Collective bargaining. In 1943, Law No. 10,449 established a contractual pay scheme that brought together representatives from workers, employers, and the government to negotiate industry-specific minimum wages above the NMW that are allowed to vary with occupation. These collective bargaining agreements (CBAs) experienced two long periods of inactivity: first, during the military dictatorship (1973–1984) and second, during liberal democratic governments between 1992 and 2005. Negotiations were gradually restored starting in the second half of 2005, with proper functioning after Law No. 18,566 was enacted in 2009. This law expanded the sectoral coverage and established concrete rubrics for the bargaining rounds. Collective bargaining directly interacts with the NMW, as the NMW works as the lower bound for the industry-level negotiations.

The presence of industry-specific wage floors above the NMW may affect our analysis below because the NMW may not be a binding restriction for firms participating in industries that set wage floors above the NMW. A few considerations follow. First, while the process of restoring sectoral negotiations began in 2005, it was not until 2009 that the system was operating widely. This timing contrasts with the NMW, whose most significant change happened in 2005. Our results below show immediate effects on our outcomes of interest around 2005, suggesting that the policy predominantly driving the action is the NWM. Second, if anything, this measurement problem should work against finding causal effects of the NMW because "control" firms and workers (i.e., not exposed to changes in the NMW because they have larger baseline wages) may experience wage increases dictated by their respective contracts. Third, our firm-level regressions control by firm fixed effects, and our individual-level regressions control by industry indicators. These fixed effects attenuate concerns about confounders arising from industry attachment. Finally, a heterogeneity analysis shows that our main findings are exhibited both in samples of workers attached to industries exposed to early negotiations and in samples of workers attached to industries not

[2] We compute the median wage using administrative records only available between 1997 and 2013 (see Section 3).

exposed to early negotiations, suggesting that CBAs cannot rationalize the results.

Income tax reform. The government elected in 2004 also introduced an income tax reform in the second half of 2007 through Law No. 18,083, which replaced a linear labor income tax (*Impuesto a las Retribuciones Personales*) with a progressive tax schedule (*Impuesto a la Renta de las Personas Físicas*). The post-2007 income tax applies to all forms of individual labor income, including wage earners and the self-employed, using a progressive tax scheme with six income brackets, annually adjusted by CPI, with marginal tax rates ranging from 0% to 25% (see Bergolo et al., 2021 for more details on the tax reform). While changes in income taxes can lead to changes in the distribution of pre-tax earnings because of different types of labor supply responses (Saez et al., 2012), we believe this reform does not confound our analysis of the NMW reform for the following reasons. First, as with the collective bargaining reform, the timing of the tax reform prevents it from rationalizing the immediate effects documented around 2005. Importantly, our worker-level causal results use data only through 2006, that is, before the tax reform. Second, in both the pre-2007 and post-2007 tax systems, workers earning around the NMW were exempt from income taxes, as both systems feature a zero-tax bottom bracket that covers around half of formal workers. This implies that the tax reform did not affect the labor supply incentives of our population of interest. Third, below we document that the decrease in wage inequality observed after 2005 is mainly explained by faster wage growth at the bottom and an increased pay premium for initially low-paying firms, with negligible changes in inequality in the top half of the wage distribution, a pattern unlikely to be tied to changes in marginal tax rates at the top. Then, we believe that the tax reform is not a relevant confounder of our NMW analysis.

2.2 Employment informality

We use administrative social security records that account for the universe of formal workers in Uruguay. In the presence of labor market informality, these data may introduce two concerns. First, inequality dynamics in the formal labor market may not be representative of those in the aggregate labor market if the informal sector is large and the formal and informal labor markets respond differently to minimum wage reforms (Parente et al., 2025). Second, reporting responses may explain changes observed within the population of formal workers if "payments under the table" (PUTs, i.e., the payment of part of the salary of formal workers "off the books") are prevalent, thereby affecting the interpretation of minimum wage effects on formal wages (Bíró et al., 2022; Gavoille and Zasova, 2023; Feinmann et al., 2024, 2025).

In terms of informality, Uruguay is an outlier within Latin America as it systematically performs well above the regional standards in different informality indicators (Maurizio, 2021; OECD, 2025; ILO, 2026). Appendix B provides further exploration leveraging data from the National Household Survey of Uruguay (ECH). Figure B.1 of Appendix B shows the evolution of the informality rate around the 2005 reform. The informality rate increased before 2005 (likely due to the recession), but steadily decreased starting in 2005, reaching approximately 7% for our population of interest in 2013. The relative size of the informal labor market is, then, small enough to affect aggregate trends in inequality (and even shrank after the 2005 reform). In fact, Figure B.2 of Appendix B shows that, while inequality within the informal sector slightly increased after the 2005 reform, the earnings inequality of formal workers in the ECH closely tracks the trends observed in the administrative records, and also is similar in levels and trends to the earnings inequality of the overall (formal and informal) labor market, a pattern consistent with the relatively small size of the informal sector in Uruguay. This contrasts with findings in other contexts where the size of the informal labor market is large enough to affect aggregate trends and levels (e.g., Parente et al., 2025). To further stress this point, Figure B.3 of Appendix B shows harmonized informality rates for 2023 for a subset of Latin American countries. In 2023, informality rates in Uruguay were below a fifth of those in Mexico and Peru, around 30% of those in Argentina and Colombia, around 40% of those in Brazil, and around 60% of those in Chile, confirming that Uruguay constitutes an exception in the region regarding labor market informality.

Regarding PUTs, while we don't have access to state-of-the-art surveys to estimate their prevalence (e.g., Feinmann et al., 2025), we leverage the fact that, since 2006, the ECH has asked respondents about underreporting behavior. We document two facts. First, the share of respondents underreporting formal wages remained stable between 2006 and 2013, and then decreased between 2013 and 2022, suggesting that the NMW reform of 2005 (and the subsequent increases between 2005 and 2013) did not increase PUTs. Second, leveraging the cross-country survey of Feinmann et al. (2024), we note that the levels of PUT in Uruguay estimated using the ECH are substantially lower than those estimated by the authors for other countries in the region. In 2023, only 1.4% of formal workers in Uruguay received PUTs according to the ECH. By contrast, the estimated rates by Feinmann et al. (2024) are 26% for Argentina, 25% for Brazil, 18% for Mexico, 10% for Peru, 6% for Colombia, and 4% for Chile.

Given these facts, we argue that concerns about employment informality do not pose a threat to our analysis using formal employment data. Then, we interpret the effects of the NMW on wages, inequality, and other outcomes as real effects that reflect the country's aggregate patterns of inequality.

3 Data

This section describes the data sources, variable definitions, and cleaning procedures. Additional details can be found in Appendix A.

3.1 Data sources

Our main data source is the individual-level administrative records from the Uruguayan Social Security Administration (*Banco de Previsión Social*, SSA). These records provide matched employer-employee labor histories constructed from the payroll tax forms that firms are required to file monthly to report social security contributions to the SSA. The dataset covers all formal workers who reported any positive earnings to the SSA for at least one month between 1997 and 2013 and includes monthly information on workers' gross earnings, hours worked, days worked within the month, and sociodemographic characteristics, including gender and birth date. The dataset also contains firm-level information, most importantly firm identifiers and industry indicators (5-digit ISIC codes).

We also access Corporate Income Tax (CIT) data from the Uruguayan Tax Authority (*Dirección General Impositiva*), which covers the universe of medium-sized and large private firms in Uruguay between 2009 and 2013. All firms in Uruguay, except for self-employed businesses and other small-sized firms subject to special regimes, must file an annual tax return to pay the CIT. These data provide information on balance sheets and income statements, allowing us to measure value added at the firm level. We merge the employer-employee labor history data with the CIT data using an auxiliary dataset from the SSA that merges worker identifiers from the income tax data with those from the SSA. This key enables us to utilize the income tax records to obtain the tax identifier of the firm in which the individual is employed to merge the firm-level CIT records with the SSA labor histories.

The tax data has two important limitations. First, the auxiliary dataset used to recover tax identifiers comprises a non-random subset of workers from the SSA records, resulting in our inability to match the CIT records for the universe of firms present in the SSA data. Using this procedure, we can match the tax records of 72% of the firm-year observations that appear in our final estimation sample between 2009 and 2013, which account for 78% of the total employment in our sample (after the sample restrictions).[3] Second, we only access the corporate tax records for 2009 onwards, which implies that we cannot use this data in our main causal estimates of the effects of the 2005 reform. Therefore, balance sheet variables are only used in descriptive exercises to validate our procedures.

3.2 Main variables

The main outcome used in our empirical analysis is the real hourly wage, which is built by dividing annual earnings by annual hours worked in each employer-employee match, properly adjusted by inflation. Monthly variables are annualized by adding up all monthly entries within a worker-firm-year combination. Our final dataset is a worker-level unbalanced panel that records the annual outcomes for the primary employer of each worker, defined as the employer that delivered the highest earnings each year.

Annual earnings comprise all forms of labor compensation subject to labor income and payroll taxes, including fixed and variable pay, before taxes and social security contributions. Our measure of annual hours is the total hours worked in a year, including legal vacation, holidays, and lunch breaks. When employers report both weekly hours worked and total days worked within a month, monthly hours can be computed without any imputation. This case represents roughly 75% of our baseline yearly panel observations. When employers report weekly hours but days worked are missing, the worker is assumed to have worked the complete month in the firm. This case represents roughly 5% of our baseline yearly panel observations. Finally, when employers report days worked but weekly hours worked are missing, the worker is assumed to work the standard labor contract of 8 hours per day, with a maximum of 200

[3] An individual appears in this auxiliary SSA dataset if they have enabled a dependent to access certain social security benefits. The most common scenario is when formal workers provide health insurance to other household members, typically their children. Other programs, such as conditional cash transfers, are also included.

monthly hours.[4] This case represents roughly 20% of our baseline yearly panel observations. As this latter imputation is prone to measurement error, we replicate our analyses using a panel of workers that drops observations with imputed hours.

A typical concern about data on days and hours worked is the quality of the reported information. In Uruguay, employers are required to report hours worked in the SSA records, and the reported information is subject to periodic controls by the SSA. Firms that are found to be providing erroneous information are subject to penalties and fines. Then, firms have an incentive to report hours worked accurately. Despite this institutional feature, we assess the hour data quality following Lachowska et al. (2022), who propose a sequence of validation tests of administrative data on hours. The results from these evaluations suggest that the quality of the Uruguayan records is generally high (see Appendix A.1 for details).

Finally, we closely follow Harasztosi and Lindner (2019) to process the CIT records and build, at the firm-by-year level, measures of value added per worker and labor shares. Value added is broadly defined as the value of production (revenue plus operational grants) minus production costs (all operating expenses excluding labor costs). The firm-level labor share is the ratio between total gross wages and value added, where total gross wages are computed by aggregating all employees' labor expenses using the SSA records before imposing our sample restrictions. More details can be found in Appendix A.2.

3.3 Annual panel and estimation sample

We follow standard practices (e.g., Sorkin, 2018; Lachowska et al., 2020) in the treatment of matched employer-employee data to process the monthly records to build an annual panel of hourly wages at the worker level with firm identifiers, where the relevant firm for each worker-year pair is the primary employer of the worker based on annual earnings. We provide details on the steps taken to go from the raw monthly records to the annual worker-level panel in Appendix A.3. One advantage of our data is that the raw entries are recorded at the monthly level, so we can observe exactly when job matches are created and destroyed. This implies we don't need to implement adjustments (needed in quarterly or annual data) to annualize outcomes when the start and end dates of job matches are not observed.

Descriptive statistics for the yearly worker-level panel are displayed in Panel (a) of Table 1. Pooling all years, we have an unbalanced panel of 10,434,988 worker-year observations, with 1,690,771 different workers who work in 114,817 different firms. The average log hourly wage decreased in the 2001-2005 period (relative to the 1997-2001 period) but then increased in the post-2005 period. The standard deviation of log wages decreased after 2005. The average worker is between 36 and 37 years old, and between 59% and 61% of the observations pertain to male workers.

Since our analysis below mostly uses the data to estimate AKM and TV-AKM models, we impose additional (standard) restrictions on the sample to make it suitable for the empirical analysis. First, to avoid labor market transitions driven by education or retirement decisions, we only keep job matches in

[4] The cap on 200 hours follows from the legal definition of the workweek. Imputing 8 hours per day without a cap may lead to implausibly large monthly hours in firms that report full-time workers as working 30 or 31 days a month.

Table 1: Descriptive Statistics

	Observations	Workers	Firms	log Wage Mean	log Wage SD	Age Mean	Age SD	Male Mean
Panel (a) Yearly Panel								
1997-2001	2,581,830	828,988	48,252	4.15	0.84	35.94	13.10	0.61
2001-2005	2,424,552	821,661	48,853	3.94	0.85	36.92	12.90	0.60
2005-2009	3,238,379	1,036,156	60,957	4.09	0.74	36.39	12.74	0.60
2009-2013	3,953,293	1,216,746	66,686	4.42	0.71	36.35	12.64	0.59
All years	10,434,988	1,690,771	114,817	4.19	0.80	36.37	12.82	0.60
Panel (b) Yearly Panel + Restrictions								
1997-2001	1,691,746	546,244	19,736	4.30	0.79	36.19	10.50	0.60
2001-2005	1,609,651	538,531	20,090	4.07	0.81	36.77	10.48	0.58
2005-2009	2,074,307	682,445	23,668	4.19	0.70	36.53	10.54	0.59
2009-2013	2,547,655	816,359	26,224	4.49	0.63	36.60	10.57	0.58
All years	5,782,162	1,070,488	33,108	4.33	0.73	36.26	10.17	0.58
Panel (c) Yearly Panel + Restrictions + LCS								
1997-2001	1,641,416	529,508	17,457	4.32	0.78	36.15	10.49	0.60
2001-2005	1,553,646	518,191	17,295	4.09	0.80	36.71	10.47	0.58
2005-2009	2,042,785	669,955	21,905	4.19	0.70	36.48	10.54	0.59
2009-2013	2,524,680	807,331	24,821	4.49	0.63	36.57	10.57	0.58
All years	5,776,762	1,068,100	32,682	4.33	0.73	36.26	10.17	0.58

Notes: This table presents descriptive statistics of the yearly worker-level panel built from the raw monthly administrative records of the SSA. Panel (a) displays statistics for the baseline yearly panel, where each observation reports annual outcomes for the primary employer, defined as the employer that had larger earnings. Panel (b) displays statistics for the yearly panel after imposing restrictions on age, labor market attachment, outliers, and firm size. Panel (c) further restricts the sample to firms belonging to the largest connected set (LCS). SD accounts for Standard Deviation. See Section 3 and Appendix A.3 for additional details on the panel construction.

which the worker is always between 20 and 60 years old. Second, we focus on workers with strong labor market attachment by dropping worker-year observations in which a worker worked fewer than 400 hours per year with the primary employer. Third, we drop outliers in hours worked (workers with average weekly hours greater than 60, $\approx 0.003\%$ of the sample) and hourly wages (workers with hourly wages less than 5 Uruguayan pesos, $\approx 0.23\%$ of the sample). Fourth, we only keep firms with four or more observations after the age and labor market attachment restrictions are imposed. This restriction ensures a minimum firm size to estimate within- and between-firm variances and the corresponding F-FEs. Panel (b) of Table 1 shows that when pooling all the years, the number of observations is reduced by 45% after imposing all these restrictions. The most important one is the firm size restriction since in Uruguay (as in many developing economies) the firm-size distribution is skewed to smaller firms. Indeed, we see that, after imposing the restrictions, the number of surviving workers is in the order of 63% relative to the baseline panel, while the number of surviving firms is slightly below 30%. The age and gender composition of this sample are comparable to the baseline panel, but we observe that the restricted sample has slightly higher wages and displays a slightly smaller wage variance. While this restriction is binding for all analyses involving F-FEs, in Section 7.2 and Appendix D we reassuringly estimate almost equivalent wage and employment effects when using the sample before and after imposing the firm size restriction, an exercise that attenuates concerns related to this attrition. We also report similar AKM results when using a panel that keeps firms with three or more observations after the other restrictions are imposed, an exercise that increases the number of firms by around 38%.

Finally, it is well known that AKM models are identified by job switchers in the largest connected set of firms (Card et al., 2013; Kline, 2024). Panel (c) of Table 1 shows descriptive statistics for the largest connected set after imposing the abovementioned restrictions. 99.9% of the observations of the yearly panel with restrictions are part of the largest connected set when pooling all years, which is not surprising given the firm-size restriction (mechanically, larger firms are more likely to be connected). We use this worker-level panel as the main input for all our exercises below.[5]

While mechanically displaying different sample sizes, Tables D.1 and D.2 of Appendix D show similar descriptive statistics for the worker-level panels that drop observations with imputed hours or keep firms with three or more observations after the other restrictions are imposed, respectively.

4 Stylized Facts

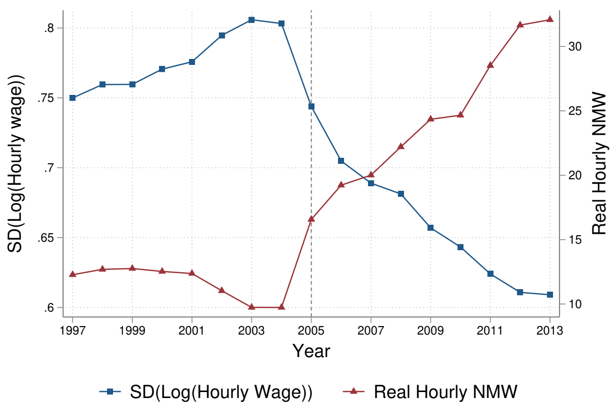
We present four stylized facts using our processed annual worker-level panel to motivate our analysis.

Panel (a) of Figure 2 plots the annual standard deviation of log hourly wages and the real hourly NMW. The evolution of wage inequality closely mirrors the evolution of the real NMW. The correlation between the two series is -0.99. The figure shows that, between 1997 and 2002, the real NMW remained constant while the standard deviation of log hourly wages increased by 6%, from 75 log points to 79 log points. In the years 2003 and 2004, the real NMW decreased while wage inequality slightly increased. However, in 2005, the real NMW increased by 70%, and the standard deviation of log hourly wages decreased by 7% (from 80 log points to 74 log points), more than reversing the increase in inequality experienced over the previous eight years. This pattern persisted during the post-reform period. In 2013, the real hourly NMW was more than three times the level prevailing in 2004, and the standard deviation of log hourly wages reached 61 log points, 76% of the level in 2004. In Figure D.1 of Appendix D, we extend Panel (a) of Figure 2 by including the standard deviation of log monthly earnings and log monthly hours. The figure reveals that baseline inequality in monthly hours is an order of magnitude smaller (below 0.3 log points) and does not exhibit a change after 2005. Consequently, the standard deviation of log monthly earnings closely follows the standard deviation of log hourly wages.

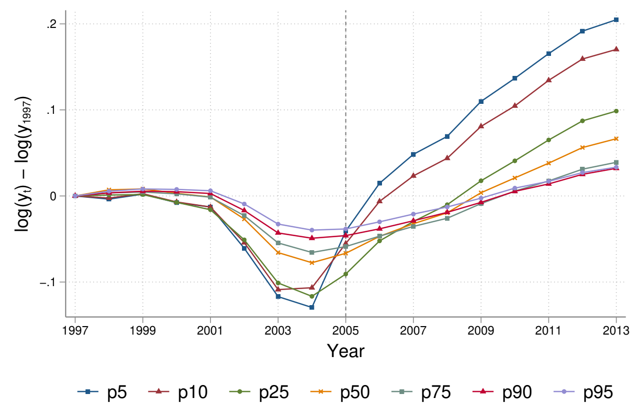
Panel (b) of Figure 2 plots the evolution of different wage percentiles relative to their values in 1997. Real wages decreased across the entire wage distribution before 2005, with lower percentiles (p5, p10, and p25) being particularly affected by the recession experienced in the first half of the 2000s. Starting in 2005, however, hourly wages at the bottom grew much faster than the rest of the wage distribution. By 2006, the p5 and p10 of the wage distribution had already recovered their pre-crisis real values despite the larger dip before the reform, and the faster growth rates persisted until 2013. On the contrary, higher

[5]When looking at specific 5-year windows, the connected set restriction has a larger impact. This happens because firms can be connected through moves that happen outside the interval considered. Except for the baseline AKM decompositions by 5-year period, all exercises below are based on the dataset that pools all years.

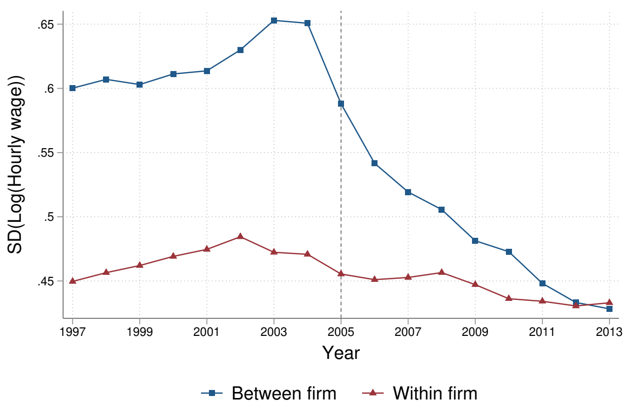
Figure 2: Stylized Facts on Wage Inequality Around the Minimum Wage Reform



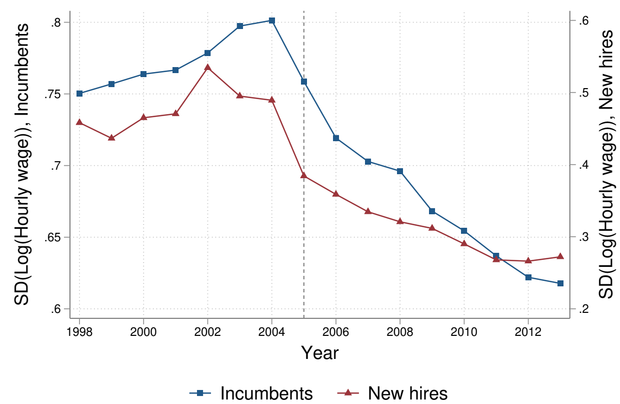
(a) Wage inequality and the real hourly minimum wage



(b) Wage growth by percentile



(c) Between- and within-firm inequality



(d) Incumbents versus new hires

Notes: This figure presents stylized facts using the processed yearly panel described in Section 3 (Panel (c) of Table 1). In all figures, SD accounts for Standard Deviation. Panel (a) plots the standard deviation of log hourly wages and the real hourly national minimum wage. Panel (b) plots the evolution of different percentiles of the wage distribution relative to their levels attained in 1997. Panel (c) plots an employment-weighted bias-corrected variance decomposition of the total variance of log hourly wages into a between-firm component and a within-firm component. Panel (d) plots the standard deviation of log hourly wages separately for new hires and incumbent workers, where a new hire is defined as a worker who either appears for the first time in the sample (after 1997), the primary employer is a different firm relative to the previous year, or appears in the sample after disappearing for at least one year. By construction, the year 1997 is omitted in Panel (d) since the new hire definition relies on the information from the previous year.

percentiles exhibited much slower growth rates, achieving full recovery to pre-crisis levels only around 2009-2010. These patterns suggest that the overall decrease in inequality documented in Panel (a) was mediated by wage compression at the bottom of the distribution. Indeed, Figure D.2 of Appendix D shows that lower-tail percentile ratios (p50/p5, p50/p10, and p50/p25) experienced mild trends before 2005 but exhibited dramatic decreases after the 2005 reform. This pattern contrasts with upper-tail percentile ratios (p95/p50, p90/p50, and p75/p50), which exhibit much smoother and smaller decreases after the 2005 reform, suggesting that dynamics at the top are not driving the aggregate patterns.

Panel (c) of Figure 2 provides a standard employment-weighted bias-corrected variance decomposition of the aggregate trend depicted in Panel (a), where the total variance in log wages is split into a between-firm component and a within-firm component.[6] To ease the interpretation of the levels, the components are expressed in standard deviations. Two aspects of the figure are worth discussing. First, before the reform, the between-firm component was substantially larger than the within-firm one. Between 1997 and 2004, the within-firm component averaged 0.47 log points, while the between-firm component averaged 0.62 log points. Second, almost all of the decrease in inequality observed starting in 2005 can be attributed to a reduction in between-firm inequality. Indeed, the decrease in the between-firm component is significant enough to completely close the gap between components in 2012 and even reverse it in 2013. It is well known that changes in between-firm inequality may arise from either changes in firms or changes in the sorting patterns of workers to firms (Song et al., 2019). The following sections explore which of these stories is more likely to rationalize this aggregate pattern.

Finally, Panel (d) of Figure 2 shows the evolution of the standard deviation of log hourly wages separately for new hires and incumbent workers. A new hire is a worker who appears for the first time in the sample (after 1997), the primary employer differs from the previous year, or appears in the sample after disappearing for at least one year. Two aspects are worth discussing. First, the cross-sectional inequality among incumbent workers is larger than that of new hires, as illustrated by the different levels on the axes. Second, both series show a slight increase in inequality prior to 2005 and a sharp decline in inequality thereafter. The fact that incumbent workers experience a decrease in inequality suggests that the wage-setting behavior of the firms they work for may be changing due to the NMW reform.

5 AKM Analysis

The previous section suggests that firms were important mediators of the change in inequality following the 2005 NMW reform. This section and the following descriptively assess this claim using the AKM (Abowd et al., 1999) and TV-AKM (Engbom et al., 2023; Lachowska et al., 2023) frameworks.

We first estimate standard AKM regressions using the estimation sample described in Section 3:

$$\log w_{it} = \alpha_i + \psi_{j(i,t)} + X_{it}'\beta + \varepsilon_{it}, \tag{1}$$

where w_{it} is the hourly wage of worker i in year t, α_i is a worker fixed-effect, $j(i,t)$ denotes the firm where worker i is employed in year t, $\psi_{j(i,t)}$ is the firm fixed effect (F-FE), X_{it} is a vector of time-varying covariates that include a (properly normalized) polynomial in age and year fixed-effects, and ε_{it} is the error term.[7] Following previous work (e.g., Card et al., 2013), we estimate equation (1) separately for overlapping 5-year periods: 1997-2001, 2001-2005, 2005-2009, and 2009-2013.

Identification restrictions for having unbiased estimates of the fixed effects are well understood in

[6] The bias correction follows standard practices (e.g., Krueger and Summers, 1988). The between-firm component is adjusted by the square of the standard error to avoid attributing sampling error to the true between-firm variance component.

[7] As discussed in Card et al. (2018), controlling by age is key but can lead to standard collinearity problems between age, year, and cohort effects, biasing the estimates of the worker fixed effects. The recommendation is to control by a polynomial centered around the age where the life-cycle pattern of hourly wages flattens. In our data, this happens approximately at age 53 (see Figure D.3 of Appendix D), so we include a third-degree polynomial in $\widetilde{\text{Age}}_{it} = (\text{Age}_{it} - 53)$.

the literature (see Kline, 2024 for a discussion). Assuming the model is correctly specified (i.e., fixed effects are time-invariant and worker and firm components are additively separable), it is identified by job switchers within a connected set of firms, as in the absence of these switches, worker and firm fixed effects cannot be separately identified. As a consequence, F-FEs are only identified up to a constant since switches only identify differences in pay premia. Importantly, the error needs to satisfy strict exogeneity:

$$\mathbb{E}\left[\varepsilon_{it}|i, j(i,s) = \iota, X_{is} = x\right] = 0, \tag{2}$$

for all $s \in \{1, ..., T\}$. In terms of the mobility network, strict exogeneity implies that firm switches can be selected on time-varying covariates and F-FEs, but they cannot be selected on time-varying shocks ε_{it}.

While these restrictions are inherently untestable, a typical practice to assess their plausibility is to report "event studies" of log hourly wages of switchers around job transitions (Card et al., 2013, 2018). We define as a switcher within a 5-year period a worker who stays for (at least) 2 consecutive years in a firm, switches to a different firm, and stays for (at least) 2 consecutive years. Then, we compute employment-weighted quartiles of firms based on the hourly wages of switchers' coworkers and plot the residualized (against year fixed effects and age controls) log hourly wage of switchers that experience transitions between firms in different quartiles. Under the assumptions of the model, there should be no trends before the switch, and the switch should generate a discrete and symmetric change in wages depending on the origin and destination firms (based on the coworkers' wages quartiles). Figure D.4 of Appendix D shows these event studies separately for each of the four 5-year periods. Pre-switch trends in hourly wages are essentially flat, and wage changes around moves are remarkably symmetric across quartiles, suggesting that the AKM assumptions may provide a reasonable approximation in our setting.

Another diagnostic to assess the separability assumption consists of comparing the adjusted R-squared of the AKM regression with the adjusted R-squared of a regression that replaces the worker and firm fixed effects with "match" fixed effects, i.e., a set of indicators $\varphi_{ij(i,t)}$ unique to each job match between a worker and a firm. Averaging across the four 5-year periods, the adjusted R-square of the AKM regression is 0.922, while the adjusted R-square of the "match effect" model is 0.942. The mild increase in regression fit suggests there is little loss in assuming the additive representation. In regressions using the whole period 1997-2013, the adjusted R-squares are 0.859 and 0.904, respectively.[8]

Using the AKM model estimates, it is standard to report a variance decomposition of log wages into worker and firm components. Abstracting from the role of time-varying covariates, we can write:

$$\mathbb{V}\left(\log w_{it}\right) = \mathbb{V}\left(\alpha_i\right) + \mathbb{V}\left(\psi_{j(i,t)}\right) + 2 \cdot \mathbb{C}\left(\alpha_i, \psi_{j(i,t)}\right) + \mathbb{V}\left(\varepsilon_{it}\right), \tag{3}$$

where $\mathbb{V}(.)$ denotes the variance operator and $\mathbb{C}(.)$ denotes the covariance operator.[9] Each variance

[8] The overall decrease in fit relative to the average across individual 5-year periods suggests that time-varying changes in F-FEs may be important. Indeed, the adjusted R-square of the TV-AKM model discussed in the next section, which allows for time-varying F-FEs, is 0.892, a fit that nearly matches that of the general match effects model.

[9] In principle, this decomposition should also include variance and covariance terms for the portion explained by $X_{it}'\beta$. Our results below, however, residualize against time-varying covariates to focus on the worker, firm, and sorting components.

component describes the share of wage inequality explained by heterogeneity in worker and firm fixed effects. The covariance component is usually interpreted as a measure of worker sorting: the share of wage inequality explained by high-α_i workers working in high-$\psi_{j(i,t)}$ firms. When the AKM identification assumptions hold, the estimated fixed effects are unbiased but the variance decomposition is not, a problem known as "limited mobility bias" (Andrews et al., 2008; Bonhomme et al., 2023). In simple words, estimated fixed effects are unbiased but noisy, so variance components square the standard error, spuriously attributing noise to the relative importance of firms. Since the identification of F-FEs is based on job switchers, this estimation error is inversely proportional to the number of movers per firm (Bonhomme et al., 2023; Lachowska et al., 2023). To deal with this problem, we report bias-corrected variance decompositions based on the leave-out method proposed in Kline et al. (2020).[10]

5.1 Results

Table 2 shows the results of the variance decomposition using the log hourly wage as the dependent variable. Panel (a) shows the bias-corrected results. Panel (b) shows the (biased) naive plug-in decompositions for reference. As expected, the bias correction decreases the relative importance of F-FEs and increases the relative importance of the covariance term. The bias, however, does not seem substantial enough to produce negative covariance terms. This feature reflects the non-trivial number of movers per firm present in our dataset, possibly facilitated by our restriction based on firm size. Table D.4 of Appendix D shows similar results using monthly earnings as the dependent variable. For the variance components, the tables also report standard deviations so values can have a log point interpretation.

Consistent with Figure 2, Table 2 shows that the variance of wages increased between 1997-2001 and 2001-2005 to exhibit then a substantial decrease in the 2005-2009 and 2009-2013 periods. The AKM decomposition reveals that firms played a significant role in the reduction in inequality observed after 2005. While all components faced a decrease in levels, the decrease was particularly large for the firm component. This implies that while the relative contribution of worker fixed effects increased after 2005 and the relative contribution of the covariance term remained constant, the relative contribution of firms decreased from around 30% in the pre-2005 period to 22% in the 2005-2009 period and to 19% in the 2009-2013 period.[11] These results suggest that the descriptive patterns discussed in Section 4 are related to a change in the distribution of F-FEs. We also observe that the total variance explained by the worker and firm fixed effects decreased throughout the period, suggesting that the NMW increase may have given low-wage workers a premium above the one that firms pay to all workers. Tables D.5 and D.6 of Appendix D show similar results when using the worker-level panels that drop observations with imputed

[10] The bias correction of Kline et al. (2020) further restricts the sample of firms to the leave-one-out largest connected set (LOO-LCS), a sample of firms that remain connected after randomly extracting one worker in each firm. Table D.3 of Appendix D compares the panel restricted to the LCS to the panel restricted to the LOO-LCS. Around 10% of the worker-year observations are lost in the LOO-LCS, with no detectable differences in average wages, age, or gender composition.

[11] Kline (2024) compares different bias-corrected variance decompositions and finds that developing economies generally exhibit a higher standard deviation of F-FEs. Our pre-reform estimates align with this pattern. However, the reduction experienced after the 2005 reform puts Uruguay closer to the levels observed in developed economies.

Table 2: Bias-Corrected Variance Decomposition: Log Hourly Wages

	1997-2001		2001-2005		2005-2009		2009-2013	
	Level	%	Level	%	Level	%	Level	%
Variance of Log Hourly Wages	0.602	100	0.647	100	0.495	100	0.395	100
Std. Dev.	0.776		0.804		0.704		0.629	
Panel (a) KSS (Bias corrected)								
$\mathbb{V}(\alpha_i)$	0.220	36.5	0.230	35.5	0.196	39.7	0.175	44.3
Std. Dev	0.469		0.479		0.443		0.418	
$\mathbb{V}(\psi_j)$	0.177	29.5	0.192	29.7	0.111	22.3	0.073	18.5
Std. Dev.	0.421		0.438		0.332		0.270	
$2 \cdot \mathbb{C}(\alpha_i, \psi_j)$	0.111	18.5	0.102	15.7	0.084	16.9	0.059	15.0
Share explained		84.5		80.9		78.9		77.8
Panel (b) Plug-in (Naive)								
$\mathbb{V}(\alpha_i)$	0.275	45.7	0.278	43.0	0.239	48.4	0.208	52.5
Std. Dev.	0.524		0.527		0.489		0.456	
$\mathbb{V}(\psi_j)$	0.211	35.1	0.219	33.9	0.131	26.4	0.086	21.7
Std. Dev.	0.459		0.468		0.361		0.293	
$2 \cdot \mathbb{C}(\alpha_i, \psi_j)$	0.048	8.0	0.050	7.7	0.047	9.4	0.036	9.2
Share explained		88.7		84.6		84.2		83.4
N Movers	106,364		97,629		168,667		217,186	
N Firms	14,706		14,322		19,560		22,549	
Movers/Firms	7.23		6.82		8.62		9.63	

Notes: This table presents AKM variance decompositions of log hourly wages. The AKM model is estimated separately for 5-year overlapping intervals –1997-2001, 2001-2005, 2005-2009, and 2009-2013– using the processed yearly panel described in Section 3 (Panel (c) of Table 1). Panel (a) shows bias-corrected variance decompositions using the leave-one-out method of Kline et al. (2020). Panel (b) shows naive plug-in decompositions that are contaminated by limited mobility bias. Each panel shows results in terms of variances, standard deviations, and share explained. N Movers is the number of movers used to identify the firm fixed effects. N Firms is the number of firms. Movers/Firms is the ratio between the two.

hours or keep firms with three or more observations after the other restrictions are imposed, respectively.

Table D.7 of Appendix D presents an analog variance decomposition using monthly hours as the dependent variable. The levels of dispersion are an order of magnitude smaller than the ones observed for hourly wages (around 27-28 log points), with no difference between the years before and after the reform. Compared to the results reported in Lachowska et al. (2026), the level of hours inequality is slightly smaller (around 27-28 log points versus 35 log points), but the share explained by our two-way fixed-effects model is much larger (around 70% versus 35%). The relative importance of workers and F-FEs is symmetric and also stable around the reform.

Finally, we complement the baseline AKM analysis with a distributional AKM regression:

$$1\{w_{it} \geq c\} = \alpha_i + \psi_{j(i,t)} + X_{it}'\beta + \varepsilon_{it}, \tag{4}$$

where the dependent variable is an indicator function that takes the value one if the wage is above a threshold c, and all other variables are defined as in equation (1). By estimating equation (4) for different values of c, variance decompositions can provide insight into the relative importance of individual and firm fixed effects in determining wages at various parts of the wage distribution.

We implement this exercise by setting c as real-wage percentiles within each 5-year period, with $c \in \{p10, p30, p50, p70, p90\}$. Table D.8 of Appendix D shows the results. There are three main takeaways. First, the pre-reform results indicate that the individual fixed effects are crucial for determining wages at the top of the distribution, but F-FEs matter the most at the bottom percentiles. The share explained by individual fixed effects is above 60% in the pre-2005 period when setting $c = p90$, but is only around 25% when setting $c = p10$. Conversely, F-FEs explain less than 7% in the pre-2005 period when setting $c = p90$, but their shares are above 40% in the pre-2005 period when setting $c = p10$. Second, while the variance decomposition for wages at the top ($c = p90$) is the same before and after 2005, it changes dramatically after 2005 for wages at the bottom ($c = p30$ and, especially, $c = p10$). In particular, the share explained by F-FEs decreases substantially for these percentiles after 2005. Third, the total share explained declines after the reform for most percentiles, but particularly when setting $c = p10$. These three findings together suggest that the aggregate patterns documented in Table 2 around the 2005 reform most likely reflect changes in the role of firms in determining wages at the bottom of the distribution.

6 TV-AKM Analysis

The previous analysis shows that the role of F-FEs in explaining wage inequality decreased after the NMW reform. It is, however, difficult to precisely dissect what is driving this evolution. The set of firms considered in each 5-year period and their employment shares may vary, implying that this evolution may reflect composition effects. The overlap of firms between periods, however, is substantial, which also allows for the possibility of within-firm changes between periods. This latter possibility is difficult to assess within the AKM framework because F-FEs are identified up to a constant, so the levels of F-FEs are not comparable between regressions without additional normalization restrictions.

To sidestep this normalization problem, we estimate the Time-Varying AKM model (TV-AKM) proposed by Engbom et al. (2023) and Lachowska et al. (2023). The TV-AKM model relaxes the assumption of constant F-FEs by allowing for within-firm drifts in wage premia. Then, the evolution of the role of firms can be studied using the complete sample (1997-2013) without needing arbitrary normalizations on the estimated F-FEs. As an additional benefit, using the 1997-2013 period instead of 5-year periods increases the potential number of movers per firm and expands the set of firms considered in the largest connected set, thus attenuating limited mobility concerns ubiquitous in AKM regressions.

Formally, the estimating equation is given by:

$$\log w_{it} = \alpha_i + \psi_{j(i,t)t} + X'_{it}\beta + \varepsilon_{it}, \tag{5}$$

where, now, the F-FEs are indexed by t. All other objects are defined as in equation (1) with the detail that, naturally, year fixed effects are excluded from the vector X_{it} since F-FEs now absorb the aggregate time variation. As thoroughly discussed in Engbom et al. (2023) and Lachowska et al. (2023), the identification argument is analogous to the one of the standard AKM model by reinterpreting firm-year combinations as different firms, meaning that stayers play the role of switchers between firm (j, t) and $(j, t+1)$. While firm stayers play no role in identifying F-FEs in the AKM model, they play a key role in the TV-AKM by pinning down the within-firm change in F-FEs. The strict exogeneity assumption is also required, but now conditional on the set of time-varying firm wage premia.

How much fit is gained by allowing the F-FEs to be time-variant? Figure D.5 of Appendix D shows projections between the AKM F-FEs effects estimated using the complete sample (1997-2013) and the TV-AKM F-FEs. Panel (a) shows a binned scatter plot representation of a projection of the TV-AKM F-FEs into the AKM F-FEs. Consistent with Engbom et al. (2023) and Lachowska et al. (2023), we find a projection slope of 0.99, with all bins aligned with the 45-degree line. That is, omitting time variation in F-FEs does not significantly affect estimates of the average pay premia over the period. Panel (b), however, shows a binned scatter plot representation of a projection of the AKM F-FEs into the TV-AKM F-FEs. Similar to what is found by Engbom et al. (2023), we estimate a much smaller projection slope of 0.63, suggesting substantial within-firm variation over time in pay premia.

6.1 Results

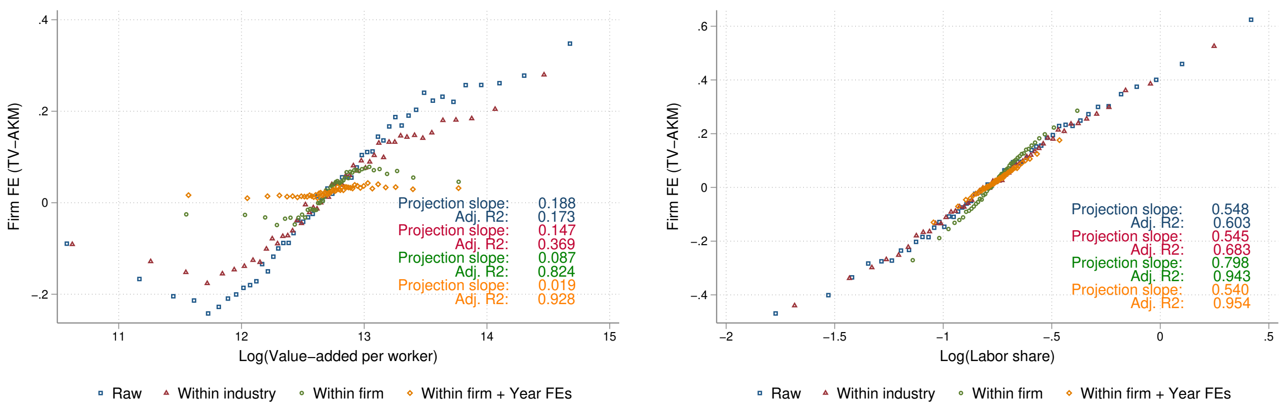
The TV-AKM model enables us to work directly with the estimated F-FEs for each firm and year, allowing for an internally consistent dynamic analysis of the role of firms in wage inequality. We therefore develop several descriptive exercises using the complete set of estimated F-FEs as the unit of analysis.

Correlating F-FEs with firm characteristics. We use the CIT data described in Section 3 and Appendix A.2 to compute, for 2009-2013, two variables at the firm-by-year level and explore whether they correlate with the time-varying F-FEs. First, we compute value added per worker as a proxy for firm productivity. Second, we compute firm-level labor shares that equal total gross wages over value added to test if, conditional on productivity, firms that share more rents display larger F-FEs. This variable proxies for firm-level rent-sharing, which may be heterogeneous across firms and time.

Figure 3 presents the results. Panel (a) shows a binned scatter plot that relates F-FEs with value added per worker. Panel (b) shows a binned scatter plot that relates F-FEs with the firm-level labor share (conditional on value added per worker). In each figure, the blue curve shows the raw correlations, the red curve controls by industry indicators (5-digit ISIC codes), the green curve controls by (time-invariant) firm indicators, and the orange curve controls by (time-invariant) firm indicators and year fixed effects. Then, in the green and orange curves, slopes are identified from within-firm comparisons across time.

Panel (a) shows that F-FEs are positively correlated with firm productivity. Consistent with findings in other contexts (see Kline, 2024 for a survey), the positive correlation displays a "hockey stick" pattern where the relationship is flat at the bottom but then positive and log-linear. When including industry fixed effects, the projection slope is attenuated but remains large and significant, suggesting that differences in productivity across industries explain a minor part of the baseline correlation. A novel insight from our analysis is that, after including firm indicators, the slope decreases to approximately 46% of the

Figure 3: Correlation Between TV-AKM F-FEs and Firm Attributes (2009-2013)



(a) Value added per worker (VA/L)

(b) Labor share (conditional on VA/L)

Notes: This figure plots binned scatter plots of the firm fixed effects estimated with the TV-AKM model against log value added per worker (Panel (a)) and log labor share conditional on value added per worker (Panel (b)). Value added is computed using corporate income tax records and is normalized in Panel (a) by the full-time equivalent workers in each firm, which is computed using the SSA records. Firm-specific labor shares are computed by dividing total gross wages (computed using the SSA data) by total value added. Details on the computation of these variables can be found in Appendix A.2. Given the limited availability of the tax data, these plots only consider the years 2009-2013 and focus on a balanced panel of firms observed in the 5 years. In Panel (a), the blue series includes no controls, the red series controls by industry indicators, the green series controls by firm indicators, and the orange series controls by firm indicators and year fixed effects. In Panel (b), the blue series controls by value added per worker, the red series controls by value added per worker and industry indicators, the green series controls by value added per worker and firm indicators, and the orange series controls by value added per worker, firm indicators, and year fixed effects. Projection slopes and adjusted R-squares are recovered from OLS regressions. Panels winsorize at the percentiles 1 and 99 of the value added per worker and labor share distributions, respectively.

baseline estimate and remains statistically significant. This finding suggests that within-firm changes in productivity across time have predictive power for changes in F-FEs. However, the positive relationship vanishes after the introduction of year fixed effects, suggesting that the productivity-driven within-firm drifts in F-FEs are likely a consequence of aggregate (rather than firm-specific) productivity shocks.

Panel (b) shows that, conditional on value added per worker, firms that share more rents with workers display larger F-FEs. This correlation is steeper than the ones displayed in Panel (a). This result – which, to the best of our knowledge, has not been documented in the literature – suggests that variation in rent-sharing plays an important role in explaining the dispersion in F-FEs. The inclusion of industry, firm, and year indicators does not attenuate the estimated slope, suggesting that idiosyncratic within-firm changes in rent-sharing over time seem important to predict changes in F-FEs. Labor market policies, and in particular the minimum wage, can have an effect on the rent-sharing policy within the firm (Engbom and Moser, 2022; Bassier and Budlender, 2025). Then, this descriptive fact helps motivate our research question, which asks whether minimum wages can causally affect the F-FEs.[12]

Revisiting dispersion in F-FEs. We extend the AKM decomposition reported in Table 2 by manually aggregating the estimated F-FEs year by year instead of by 5-year periods. This strategy allows the assessment of different aggregations of the estimates, informing about the importance of

[12] One caveat of this analysis is that we don't have data on non-wage amenities. Therefore, we cannot assess how much of the dispersion in F-FEs is driven by compensating differentials (Sorkin, 2018; Morchio and Moser, 2025).

the composition effects. Figure D.6 in Appendix D shows naive standard deviations (i.e., non-biased-corrected) of F-FEs for each year in our sample. Assuming that the limited mobility bias is relatively constant across time (Card et al., 2013; Bonhomme et al., 2023), the figure is valid to get insights into the drivers of the trends of F-FEs dispersion.

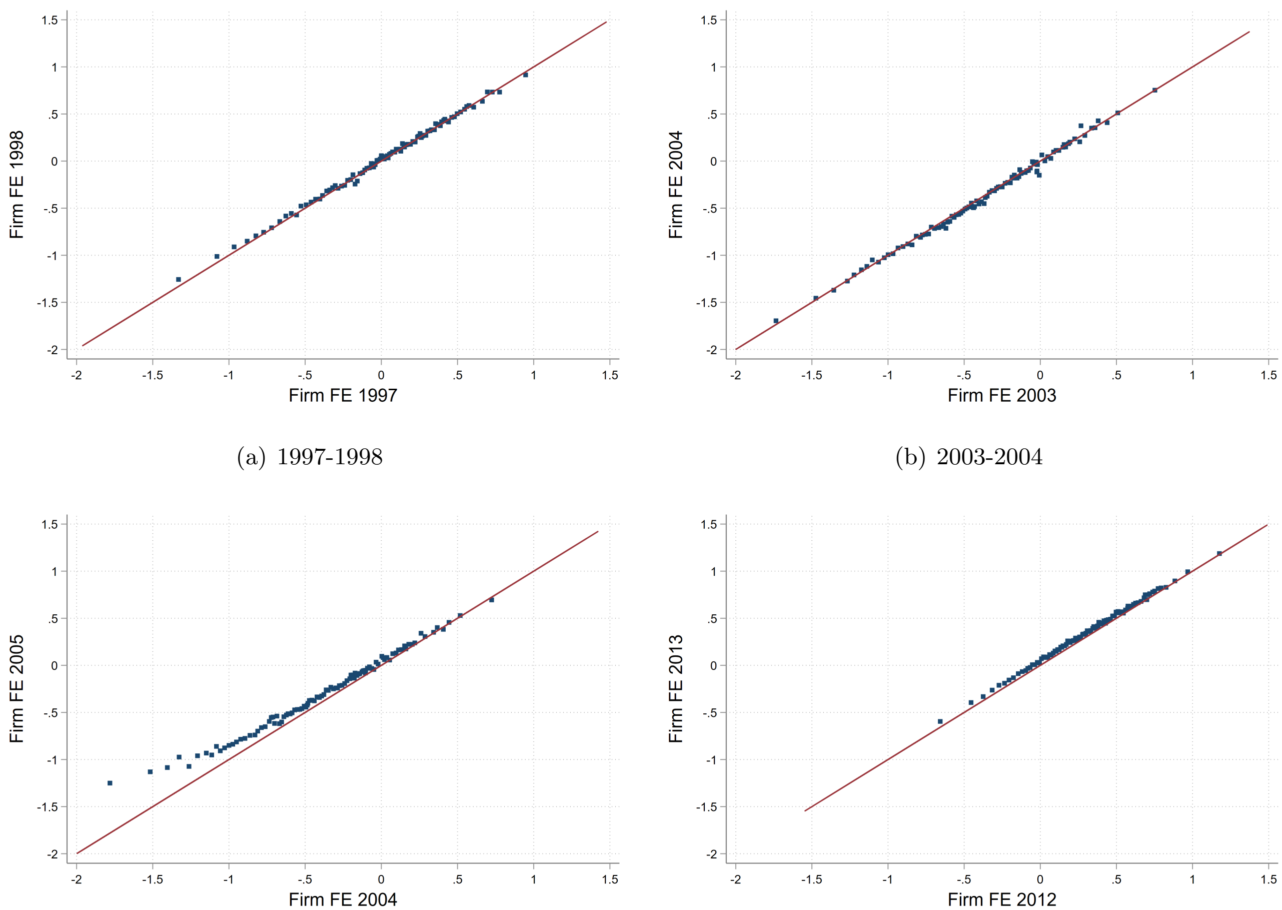
The figure plots four series: the employment-weighted standard deviation of F-FEs using all firms in the panel (unbalanced panel), the employment-weighted standard deviation of F-FEs using firms that appear in each of the 17 years of our panel (balanced panel), and corresponding versions that do not weight by employment. The unbalanced-weighted series encompasses all mechanisms through which firms can mediate impacts on aggregate inequality (changes in composition and within-firm F-FEs levels), thus being closer in spirit to the results presented in Table 2. The balanced-weighted series omits the effects of firm entry and exit, thereby maintaining a fixed composition of firms. The unbalanced-unweighted series abstracts from changes in the employment composition across firms. Finally, the balanced-unweighted series omits both composition effects (firms and employment). All series exhibit a substantial decrease after the 2005 reform that mimics the aggregate evolution of wage inequality documented in Figure 2, a pattern that is especially interesting for the balanced-unweighted series: holding constant the set of firms and the employment composition across them, dispersion in F-FEs decreased after the 2005 reform, suggesting that within-firm drifts affected the variance of F-FEs.

Persistence in F-FEs. The across-time comparability of time-varying F-FEs has been used by previous literature to assess how persistent F-FEs are (Engbom et al., 2023; Lachowska et al., 2023). Two main findings emerge from these papers. First, F-FEs are persistent within firms, especially in the short run. Second, F-FEs are cyclical, with firms paying smaller premia in recessions and vice versa. We use our time-varying F-FEs estimates to revisit these conclusions in our data. Figure D.7 of Appendix D shows, for each pair of consecutive years in our sample, the within-firm short-run persistence of the estimated F-FEs. Each plot shows a raw binned scatter plot of the F-FE in year t against the F-FE in year $t-1$. Figure 4 shows the plots for four selected pairs of years: the endpoints of our sample (1997-1998 and 2012-2013) and the pre- and post-NMW reform years (2003-2004 and 2004-2005).

In the pre-reform period, F-FEs are very persistent and slightly cyclical, mimicking the findings of Engbom et al. (2023) and Lachowska et al. (2023). In 1997-1998, 1998-1999, 1999-2000, and 2000-2001, the scatterplots lie almost exactly on the 45-degree line. As a consequence, the support of the F-FEs distributions is very stable. In 2001-2002 and 2002-2003, there is a decrease in the F-FEs levels across the whole distribution (a "year fixed effect"), possibly reflecting the effects of the recession. In 2003-2004, the cyclical shock stabilizes, and F-FEs are located again on top of the 45-degree line.

Starting in 2005, however, the perfect persistence breaks and the binned scatter plots start displaying a "hockey stick" pattern. In 2004-2005, firms at the bottom of the distribution of the F-FEs were located far above the 45-degree line, meaning they experienced faster growth in their F-FEs relative to firms up in the distribution that remained located on top of the 45-degree line. In the years that follow, the "hockey stick" pattern persists, although it gets attenuated through time. Close to the end of our analysis period, the short-run behavior returns to its baseline persistence, with binned scatter plots located again close

Figure 4: Within-Firm Short-Run Persistence in F-FEs: Selected Years



(a) 1997-1998

(b) 2003-2004

(c) 2004-2005

(d) 2012-2013

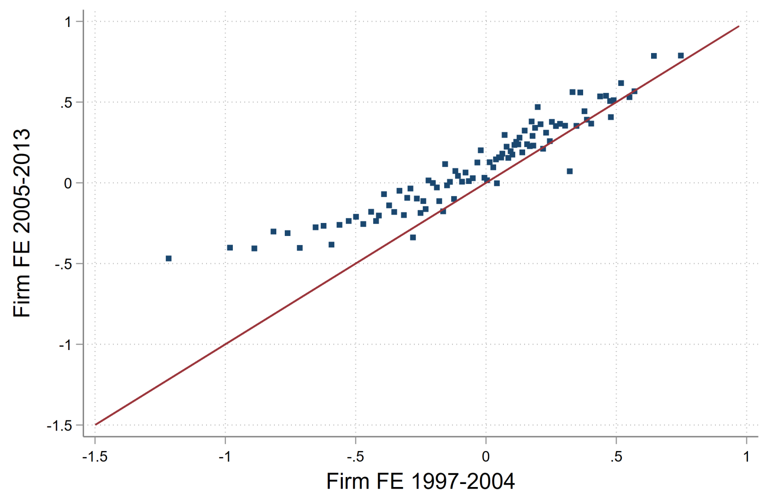
Notes: This figure presents binned scatter plots of the firm fixed effects (F-FEs) estimated with the TV-AKM model in year t against the F-FEs estimates in year $t-1$. This figure presents results for four selected pairs of years: 1997-1998, 2003-2004, 2004-2005, and 2012-2013. Figure D.7 in Appendix D shows the results for all pairs of years. Observations are weighted by employment in year $t-1$.

to the 45-degree line along the whole distribution. However, because of several years of "hockey stick" behavior, the support of the F-FEs was much more compressed in 2012 relative to before the 2005 reform.

To rule out that these patterns are an artificial consequence of transitory shocks and mean-reversion, Figure 5 shows long-run changes in the distribution of F-FEs using the balanced sample of firms for which we have F-FEs estimates for each of the 17 years of our sample. The figure plots the average F-FE between 2005 and 2013 against the average F-FE between 1997 and 2004. The hockey-stick pattern becomes clearer, with firms in the bottom half of the distribution showing a sizable departure from the 45-degree line, while firms up in the distribution showing a much smaller and evenly distributed increase in pay premia over their baseline level. Figure D.8 of Appendix D show similar results when using the F-FEs estimated with the worker-level panels that drop observations with imputed hours or keep firms with three or more observations after the other restrictions are imposed.

As a further exploration of the long-run changes in the distribution of F-FEs, we define unweighted

Figure 5: Long-Run Persistence in F-FEs: 1997-2004 vs 2005-2013



Notes: This figure presents a binned scatter plot of the average firm fixed effect (F-FEs) estimated with the TV-AKM model in years 2005-2013 against the average F-FEs estimates in years 1997-2004. This figure only considers firms in the balanced panel (i.e., observed in our data in all 17 years between 1997 and 2013). Observations are weighted by the average employment in the years 1997-2004.

deciles of firms based on their F-FE level in 2004 and analyze their evolution in time, holding the deciles fixed. Figure D.9 of Appendix D reports the results for both an unbalanced panel of firms (firms that were active in 2004, but may enter or exit at different years) and a balanced panel of firms (firms that were active during the 17 years of our sample). Both figures show that, before the reform, trends looked parallel across deciles and exhibited cyclical behavior but that, starting in 2005, low-paying firms exhibited a much faster growth in their average F-FE. At the end of the period, the cross-sectional dispersion in F-FEs was much smaller because of the compression of the distribution.

These findings suggest that the substantial persistence in F-FEs documented in previous work may not extend to all settings and policy contexts. If the NMW reform is causing these trend breaks around 2005, the results suggest that F-FEs can be idiosyncratically persistent, but also can be affected by policy reforms. The rest of the paper addresses this causal question using two different research designs.

7 The Causal Effect of the Minimum Wage

Sections 4, 5, and 6 document a large decrease in wage inequality after 2005 mediated by changes in the F-FEs. In this section, we provide evidence that the NMW reform had a causal role in this dynamic.

To develop intuition, Appendix C proposes a simple extension of Card et al. (2018) model (CCHK) where minimum wage increases can lead to increases in the F-FEs of exposed firms. We build on the CCHK model with linear technologies (which yields an AKM representation of log wages) and augment it with a minimum wage and heterogeneous, firm-specific wage-setting norms that mediate within-firm wage spillovers. In the model, a minimum wage introduction (or an increase of an already binding minimum wage) cannot be absorbed by the worker fixed effects because low-skill wages in unconstrained firms do not react to changes in low-skill wages in constrained firms and, therefore, a wage increase driven by a minimum wage reform is not portable for low-skill workers when switching to higher-paying firms.[13] This intuition implies that, conceptually, the minimum wage is a job (rather than a worker) attribute: the economy is populated by minimum wage jobs (taken more often by certain groups of workers) rather than by an intrinsic population of "minimum wage workers."[14]

If the minimum wage increase is not absorbed by worker fixed effects, it can be absorbed either by F-FEs or by a low-skill workers' residual. Appendix C provides closed-form equations that make explicit the two forces that determine the split of the minimum wage effect into F-FEs and residuals. First, workers' composition plays a key role, as the passthrough of the minimum wage to the F-FE is a function of the share of exposed workers. In the limit, a firm that offers only minimum-wage jobs will have perfect passthrough of the minimum wage to the F-FE. Second, given an employment composition, the degree of within-firm wage spillovers (e.g., due to strong wage-setting norms) is also key (e.g., Dube et al., 2019; Giupponi and Machin, 2024). The passthrough of the minimum wage to the F-FE will increase with the strength of the within-firm wage spillovers, especially in firms with higher shares of unexposed workers.

Motivated by this intuition, we now explore whether the NMW reform of 2005 had causal impacts on the F-FEs. We implement two different research designs, one at the firm level and one at the worker level. We directly test for effects on F-FEs and on wage spillovers, among other related outcomes.

7.1 Causal analysis: Firm-level design

Our firm-level exercise follows the GAP design first proposed by Card and Krueger (1994) and recently used by Draca et al. (2011), Dustmann et al. (2022), and Derenoncourt and Weil (2025). The idea of the GAP design is to proxy the firm-level exposure to the NMW reform by estimating how much firms should increase the labor costs to comply with the policy, using data from the pre-reform period.

Concretely, for each firm j, we compute the following statistic:

$$\overline{GAP}_j = \frac{1}{8}\sum_{t=1997}^{2004} GAP_{jt}, \tag{6}$$

[13]This argument does not hold if the minimum wage binds for all low-skill jobs, as in that case the minimum wage is not separately identified from the worker fixed effect. However, as long as there is wage dispersion among low-skill workers across firms, the mechanism holds. In our estimation sample, among all workers who earn around the minimum wage for at least one year, fewer than 25% earn around the minimum wage every year. The remaining group oscillates between jobs that pay the minimum wage and jobs that pay above. This pattern is not only explained by within-firm life-cycle wage dynamics, as we observe that a third of workers who earn around the minimum wage at least one year change firm, and, among all those firm switches, almost 70% represent discrete changes from minimum wage jobs to non-minimum wage jobs and vice-versa.

[14]This conclusion relies on the lack of wage responses in unaffected firms, which, in turn, relies on the absence of strategic interactions between firms. In a model with strategic interactions, wages in unconstrained firms could respond to the minimum wage, for example, because of general equilibrium effects generated by competition forces. A model with such characteristics can be found in Morchio and Moser (2025), who develop an equilibrium model based on Burdett and Mortensen (1998) that yields an equilibrium wage equation that has an AKM representation. A careful inspection of their identification result reveals that strategic interactions only affect the microfoundation of the F-FEs but do not affect the worker fixed effects, which are only proportional to individual productivity, as in CCHK (see their Proposition 1). This suggests that the logic developed in our model can go through in settings with strategic interactions.

where:

$$GAP_{jt} = \frac{\sum_{i \in j} h_{ij} \max\{0, 1.25 \cdot NMW_{2005} - w_{it}\}}{\sum_{i \in j} h_{it} w_{it}}, \tag{7}$$

where NMW_{2005} is the real hourly national minimum wage in 2005. For each firm j in a given year t, $\sum_{i \in j} h_{it} w_{it}$ are the total labor costs of the firm, with h_{ij} the hours worked by worker i in firm j and w_{ij} the corresponding real hourly wage. Then, GAP_{jt} measures how much firm j labor costs should increase in year t as a share of the total labor costs to ensure that everyone earns at least 125% of the counterfactual minimum wage. The firm-level time-invariant exposure measure ("treatment") is the average of GAP_{jt} across all pre-reform years (1997 to 2004). The rationale for this design is that firms that need to engage in higher payroll adjustments to comply with the minimum wage are the ones that will be more affected by the reform in 2005, so we can exploit variation in the level of exposure to recover the causal effect of the minimum wage reform using a difference-in-differences (DID) design.

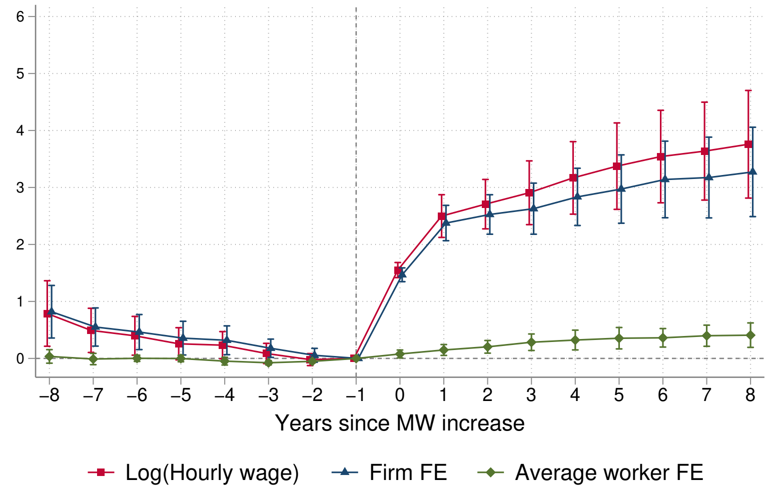
Two comments follow. First, we choose a wage threshold equal to 1.25 times the NMW in 2005 because the NMW experienced non-trivial increases in the years after 2005 (see Figure 1), so firms that, before the reform, paid slightly above the 2005 value were possibly also exposed. As a robustness check, we also proceed by defining the GAP measure using both the NMW in 2005 and 1.5 times the NMW in 2005 as the relevant hourly wage benchmark. Second, we follow the implementation in Dustmann et al. (2022) and Derenoncourt and Weil (2025) and use a time-invariant GAP measure that averages over several pre-reform years. Having a time-invariant measure based on pre-reform variables is cleaner for a DID design because post-reform GAPs are likely affected by the treatment effects of the 2005 reform. Also, we average the GAP measure over several years to better account for mean reversion, as considering a single year may capture a transitory shock in exposure, ultimately leading to a spurious treatment indicator. This concern is particularly relevant in our context, where a recession occurred in the pre-reform years, potentially leading to higher short-run volatility.

Estimating equation. Using the GAP exposure measure described in equations (6) and (7), we estimate the following DID specification:

$$y_{jt} = \gamma_j + \lambda_t + \sum_{t=1997}^{2013} \beta_t \cdot \overline{GAP}_j + \varepsilon_{jt}, \tag{8}$$

where y_{jt} is the outcome of interest in firm j in year t, γ_j and λ_t are firm and year fixed effects, and ε_{jt} is the error term. The coefficients β_t recover the causal treatment effect of the NMW reform on y_{jt} under the parallel trends assumption: firms with different GAP levels would have trended in parallel, absent the NMW increase in 2005. While this assumption is not testable, we use estimates of β_t for $t \leq 2004$ to assess its plausibility. Since $\overline{GAP}_j$ is a continuous treatment variable, β_t is interpreted as the effect of going from a 0% GAP to a 100% GAP (i.e., a situation in which the firm needs to double the labor costs to pay at least 1.25 times the 2005 NMW to all workers). We cluster the standard errors at the 2-digit industry level. We follow the standard practice of normalizing β_{2004} to 0. To avoid confounding treatment

Figure 6: Firm-Level Design: Hourly Wages, Firm FE, and Average Worker FE



Notes: This figure plots the estimated event study coefficients β_t of the firm-level equation (8) with their corresponding 95% confidence intervals. The regression considers a balanced panel of firms observed in our data in all 17 years between 1997 and 2013. Each series corresponds to a different regression with a different dependent variable. The red series uses the hours-weighted average log real hourly wage at the firm level, computed using the processed yearly panel described in Section 3 (Panel (c) of Table 1). The blue series uses the firm fixed effect (F-FEs) estimated with the TV-AKM model. The green series uses the hours-weighted firm-level average worker fixed effect estimated with the TV-AKM model. Standard errors are clustered at the 2-digit industry level.

effects with composition effects, we estimate equation (8) using a balanced panel of firms that were active between 1997 and 2013 (i.e., during 8 years before and 8 years after the reform). This restriction leaves us with 3,415 firms. We also report results using balanced panels for 7, 6, and 5-year windows around the NMW reform, which increases the number of included firms by 9.4%, 20.2%, and 32.4%, respectively.

To summarize the results, we also estimate the following pooled regression using the balanced panel:

$$y_{jt} = \gamma_j + \lambda_t + \beta \cdot \overline{GAP}_j \cdot \text{Post}_t + \varepsilon_{jt}, \tag{9}$$

where $\text{Post}_t = 1\{t \geq 2005\}$ and all other variables are defined as in equation (8). In this regression, β measures the average treatment effect of the policy relative to the pre-reform window.

We estimate regressions (8) and (9) for three different outcomes. First, we use the hours-weighted average log hourly wage at the firm level, built from the worker-level dataset used in Sections 5 and 6. Second, we use the time-varying F-FE estimated using the TV-AKM framework, ψ_{jt}. This is our main outcome of interest because we aim to test whether the NMW reform of 2005 had a causal impact on the F-FEs. Finally, we use the hours-weighted firm-level average worker fixed effect estimated using the TV-AKM framework, $\overline{\alpha}_{jt} \equiv \left(\sum_{i\in j} h_{it}\right)^{-1} \sum_{i\in j} h_{it}\alpha_i$. With this outcome, we can test whether firms adapt to the NMW reform by changing the α-composition of workers.

Results. Figure 6 shows the results. We plot the estimated $\{\beta_t\}_{t=1997}^{2013}$ coefficients with their corresponding 95% confidence intervals. The three series represent different regressions that use different dependent variables: the average log hourly wage of the firm, the time-varying F-FE, and the average worker fixed effect, the latter two estimated with the TV-AKM model described in Section 6.

Table 3: GAP Design: Results

	$\overline{\log(w)}$ (1)	ψ (2)	$\overline{\alpha}$ (3)	$\overline{\log(w)}$ (4)	ψ (5)	$\overline{\alpha}$ (6)	$\overline{\log(w)}$ (7)	ψ (8)	$\overline{\alpha}$ (9)	$\overline{\log(w)}$ (10)	ψ (11)	$\overline{\alpha}$ (12)
$\widehat{\beta}$	2.737	2.368	0.303	2.437	2.186	0.203	2.631	2.290	0.293	2.426	2.175	0.213
	(0.266)	(0.232)	(0.068)	(0.219)	(0.198)	(0.062)	(0.262)	(0.231)	(0.066)	(0.220)	(0.200)	(0.060)
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	No	No	No	No	No	No	No	No	No
Year x Size FE	No	No	No	Yes	Yes	Yes	No	No	No	No	No	No
Year x Age FE	No	No	No	No	No	No	Yes	Yes	Yes	No	No	No
Year x Size x Age FE	No	No	No	No	No	No	No	No	No	Yes	Yes	Yes
Observations	58,055	58,055	58,055	58,055	58,055	58,055	57,783	57,783	57,783	57,783	57,783	57,783

Notes: This table presents the estimated DID coefficient β of the firm-level equation (9). The regression considers a balanced panel of firms observed in our data in all 17 years between 1997 and 2013. Each column corresponds to a different regression with a different dependent variable. Columns (1), (4), (7), and (10) use the hours-weighted average log real hourly wage at the firm level, computed using the processed yearly panel described in Section 3 (Panel (c) of Table 1). Columns (2), (5), (8), and (11) use the firm fixed effect (F-FEs) estimated with the TV-AKM model. Columns (3), (6), (9), and (12) use the hours-weighted firm-level average worker fixed effect estimated with the TV-AKM model. Columns (1)-(3) include firm and year fixed effects. Columns (4)-(6) include firm and year-by-size fixed effects, where size corresponds to size quintiles based on the average size in the pre-reform period. Columns (7)-(9) include firm and year-by-age fixed effects, where age corresponds to age quintiles. Columns (10)-(12) include firm and year-by-size-by-age fixed effects, thus interacting the year indicator with all combinations of size-age quintiles. Standard errors (in parentheses) are clustered at the 2-digit industry level.

The figure shows that more exposed firms experienced a significant increase in the average log hourly wage after the 2005 reform, relative to less exposed firms. While there is a mild differential trend between more and less exposed firms several years before the reform (high-exposure firms had steeper downward trends in the recession years, consistent with Panel (b) of Figure 2), the difference in trends disappears in pre-reform years closer to 2005 and the change exhibited around 2005 is sharp, large, and immediate, providing suggestive evidence in favor of the parallel trends assumption. This "first stage" confirms that the NMW reform had an economic impact on wages of exposed firms.

Figure 6 shows that more exposed firms also experienced faster growth in their TV-AKM F-FEs. Almost all of the increase in the average log hourly wage due to the NMW reform can be attributed to the increase in the F-FE. This result suggests that the descriptive patterns exhibited in previous sections can be causally linked to the NMW reform. The figure also shows that, as time passes, the average worker fixed effect increases in more exposed firms, suggesting that more exposed firms accommodate part of the labor cost shock by changing the composition of workers within the firm, as in Clemens et al. (2021) and Butschek (2022). The results, however, suggest that the effect on the composition of workers is quantitatively an order of magnitude below the effect on the firm wage premium.

Figures D.10 and D.11 of Appendix D show similar results when considering (1) the balanced samples with more firms but shorter windows around the 2005 reform, (2) the worker-level panels that drop observations with imputed hours or keep firms with three or more observations after the other restrictions are imposed, and (3) the GAP measures that use both the NMW in 2005 and 1.5 times the NMW in 2005 as the relevant hourly wage benchmark.

To put magnitudes in context, Table 3 shows estimated β coefficients from equation (9). As a reference, 1.25 times the log real hourly NMW in 2005 was equal to 3.51. The baseline estimate of the effect on the average log hourly wage (Column (1)) is 2.74 log points. This coefficient is large and significant, despite being smaller than the benchmark wage, possibly because real wages also increased in control firms (see Figure 2). The baseline estimates of the effect of the NMW reform on the F-FE and the average worker fixed effect (Columns (2) and (3)) are 2.37 and 0.30, respectively, confirming that the majority of the wage increase can be attributed to the increase in the F-FE.

Table 3 also reports estimates of β that control for stricter fixed effects. Columns (4) to (6) include year fixed effects that vary by firm size. We do this by computing quintiles based on the average annual employment in the pre-reform period. Columns (7) to (9) include year fixed effects that vary by firm age, where, again, quintiles are used to classify firms. Finally, Columns (10) to (12) include year fixed effects that vary by both the size and the age quintiles. Reassuringly, results are stable across specifications.

7.2 Causal analysis: Worker-level design

Finally, we implement a worker-level design that closely follows Dustmann et al. (2022). This design is better suited for studying aggregate impacts and between- and within-firm spillovers.

We implement Dustmann et al. (2022) design as follows. Using our worker-year panel, we define bins of real hourly wages to allocate observations. We consider 11 bins indexed by k, with thresholds b_k so that a worker-year observation belongs to bin k if $b_{k-1} < w_{it} \leq b_k$. We set b_{11} equal to 48 Uruguayan pesos. As a reference, the real NMW in 2005 and 2006 was around 16.5 and 20 Uruguayan pesos, respectively, so the upper bound is almost 3 times the real NMW in 2005.[15] The first two bins are below the NMW in 2005. The third bin is between the NMW in 2005 and the NMW in 2006. Bins 4 to 11 are all above the NMW in 2006. Then, for $t \in \{2001, ...2006\}$, we estimate the following regression:

$$\begin{aligned} y_{it} - y_{it-2} \quad = \quad & \sum_k 1[b_{k-1} < w_{it-2} \leq b_k]\gamma_k + \lambda_t + \delta' X_{it-2} \\ & + \sum_k 1[b_{k-1} < w_{it-2} \leq b_k] 1[t \in \{2002, 2006\}]\beta_{kt} + \varepsilon_{it}, \end{aligned} \tag{10}$$

where $y_{it} - y_{it-2}$ is the two-year growth of an outcome of interest, γ_k are bin fixed effects, λ_t are year fixed-effects, X_{it-2} is a vector of worker-level controls in $t-2$ that includes gender, age, tenure, firm size, and firm industry (5-digit ISIC codes), β_{kt} are the analogs of event-study coefficients that vary by bin, and ε_{it} is the error term. The standard errors are clustered at the (lagged) industry level.

The sample includes workers who were employed in $t-2$. We consider five outcomes. First, we use log real hourly wages, so the dependent variable is the real wage growth. Second, we use an indicator of being employed, so the dependent variable measures whether the worker remains employed. Third, we use log monthly hours, so the dependent variable is the change in hours worked conditional on employment. Fourth, we use the time-varying F-FE estimated using the TV-AKM framework ψ_{jt}, so the dependent variable measures the change in the employer-specific wage premia. When using this outcome, we contrast stayers and switchers to decompose how much of the estimated effect on the F-FE is due to reallocation

[15] Dustmann et al. (2022) consider bins up to 2.3 times the minimum wage value they study.

effects specific to switchers and causal effects that benefit incumbents. Finally, we use the TV-AKM residual, which the model presented in Appendix C suggests can be affected by the reform when within-firm spillovers are limited. The employment status regression does not condition on the employment status at t. The other outcomes restrict the estimation to workers who remain employed at t.

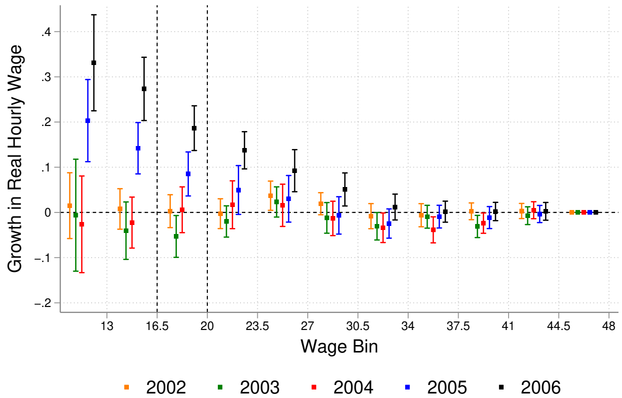
This design mimics the intuition of a DID model where the unit of observation is the wage bin but allows the event indicators to vary by wage bin, a feature possible given the use of worker-level data. The cost of this decision is the preclusion from using individual fixed effects, which implies that the model's fixed effects do not account for within-bin heterogeneity across workers, an issue we address with the inclusion of a rich vector of controls that vary at the individual level. We focus on growth rates rather than levels because of the well-known differences in wage and employment dynamics at different parts of the wage distribution (Ashenfelter and Card, 1982). Specifically, mean reversion dynamics typically imply higher wage growth and weaker employment attachment at the bottom of the wage distribution regardless of the minimum wage policy, so not properly accounting for these differences may lead to spurious conclusions with respect to the effects of the NMW reform. Focusing on outcome changes attenuates these concerns as the treatment effect is estimated on changes in the growth rates.

We estimate equation (10) for $t \in \{2001, 2006\}$. Assessing differences in the outcomes' changes between 2001-1999, 2002-2000, 2003-2001, and 2004-2002 works as a "pre-trends" diagnostic in event studies: the parallel trend assumption implies that, absent the NMW reform, there should be no differential growth rates in outcomes across bins relative to a baseline year. Then, the DID model tests for changes in growth rates by wage bin in 2005-2003 and 2006-2004 relative to a baseline year. We don't use years beyond 2006 because the two-year lagged wages starting in 2007 are affected by the reform and, therefore, are non-comparable to the baseline period. To properly interpret the coefficients, note that our implementation of equation (10) makes two normalizations. First, we normalize $\beta_{k2001} = 0$ for all k, meaning that the effects on outcome changes are estimated relative to the bin-specific two-year growth rates in 2001 (up to a constant common to all bins captured by the year fixed effect). Second, we normalize $\beta_{11t} = 0$ for all t, so within a year, event study coefficients represent differences with respect to the highest bin. The intuition is that wages far up in the distribution are not "treated" by the reform, an assumption supported by related literature where wage spillovers of minimum wage reforms are found to be decreasing in the distance to the minimum wage (e.g., Cengiz et al., 2019; Engbom and Moser, 2022).

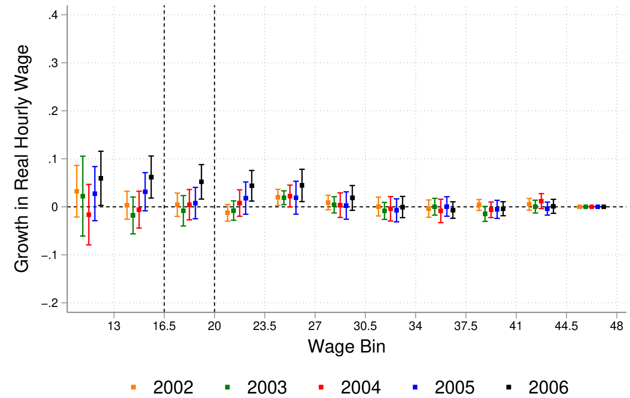
Results. Figures 7 and 8 show the results. We plot the estimated $\{\beta_{tk}\}_{t=2002,k=1}^{2006,11}$ with their 95% confidence intervals. Each plot represents a different outcome variable. Within each plot, the x-axis represents different wage bins, and the five sets of event study coefficients represent different years.

Figure 7 presents results on wages, employment, and hours. Panel (a) shows estimates using the two-year real hourly wage growth as the dependent variable. Relative to 2001, the bin-specific two-year real wage growth did not change in 2002, 2003, and 2004, providing support for the parallel trends assumption. In 2005 and 2006, however, the estimates show a significant change in the real hourly wage growth, especially at the bottom of the distribution. The effect is sharp starting in 2005 and is larger for wage bins at the bottom, especially the ones below the NMW levels of 2005 and 2006, suggesting

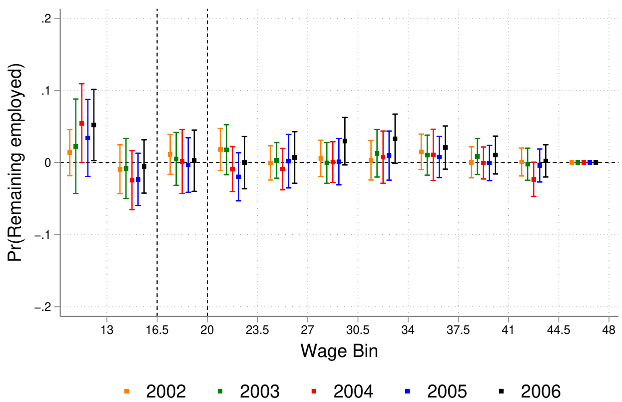
Figure 7: Worker-Level Design: Hourly Wages, Employment, and Monthly Hours



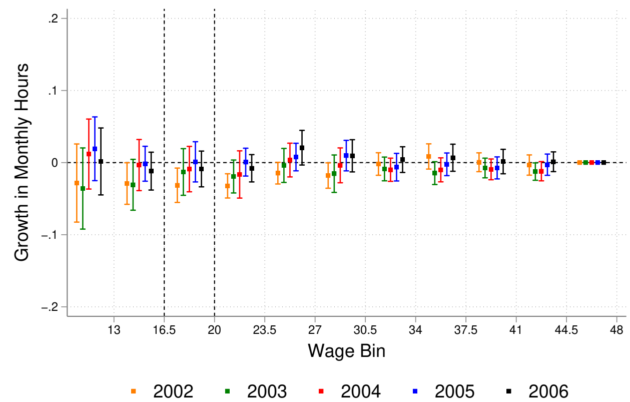
(a) Hourly wage growth



(b) Hourly wage growth (controlling by change in F-FE)



(c) Employment probability



(d) Monthly hours

Notes: This figure plots the estimated event study coefficients β_{kt} of the worker-level equation (10) with their corresponding 95% confidence intervals using the processed yearly panel described in Section 3 (Panel (c) of Table 1). Within each panel, the horizontal axis accounts for different wage bins (k), and each series represents a different year (t). Each panel uses a different dependent variable. Panel (a) uses the 2-year hourly wage growth. Panel (b) also uses the 2-year hourly wage growth, but controls by the 2-year growth in the attached F-FE. Panel (c) uses the 2-year change in employment status. Panel (d) uses the 2-year change in growth in monthly hours. All regressions control for a vector of baseline characteristics, including gender, age, tenure, firm size, and firm industry (5-digit ISIC codes). Standard errors are clustered at the (lagged) industry level.

that the NMW reform played a causal role in this increase. We estimate significant wage spillovers above the NMW levels. The spillovers decrease with the distance to the NMW and vanish in bins with wage levels around 1.5 times the NMW, validating our assumption that bins high up the distribution are valid controls for bottom wage bins. The effects are larger in 2006, consistent with the fact that the NMW exhibited two consecutive non-trivial increases. Panel (b), however, shows that the wage effect almost completely vanishes when controlling by the change in the F-FE attached to the worker. We further explore this result below to better understand the role of changing F-FEs in rationalizing the wage effect. Panel (c) shows that the increase in wages was not accompanied by detectable impacts on the probabilities of remaining employed. Relative to 2001, the bin-specific employment probabilities did not change in the years 2002, 2003, 2004, 2005, and 2006. If anything, we estimate a mild positive employment effect at the bottom bin. Similarly, Panel (d) shows that the increase in wages was also not accompanied by changes in the growth rate of monthly hours. These results suggests that the NMW increase did not destroy jobs on top of the natural bin-specific turnover rates, both at the extensive and intensive margins.[16]

Figure D.12 of Appendix D presents robustness checks. We first replicate the wage, employment, and hours results using both the worker-level panels that drop observations with imputed hours or keep firms with three or more observations after the other restrictions are imposed, finding similar results. Also, to assess whether the attrition of small firms resulting from our firm-size restrictions imposed to credibly estimate the AKM models biases our results, we replicate the wage, employment, and hours results using the panel of workers before the AKM restrictions are imposed. Reassuringly, we find similar results.

To explore the role of F-FEs in explaining the wage dynamics, Figure 8 presents results that use the TV-AKM F-FEs and residuals as dependent variables, separately for switchers and stayers. Panel (a) shows the change in the F-FE for job switchers. Consistent with Dustmann et al. (2022), we find evidence of reallocation effects: relative to 2001, workers at the bottom of the wage distribution who switch jobs are more likely to land a job in a firm with a higher wage premium after the NMW reform of 2005. This reallocation effect is concentrated at the bottom of the distribution. This figure also shows that the pre-reform recession induced productive reallocation in the Uruguayan economy, consistent with "cleansing" narratives built on Schumpeter (1939). Reallocation effects were positive in 2003 and 2004, suggesting that the recession damaged jobs in low-paying firms that were absorbed by better-paying firms.[17]

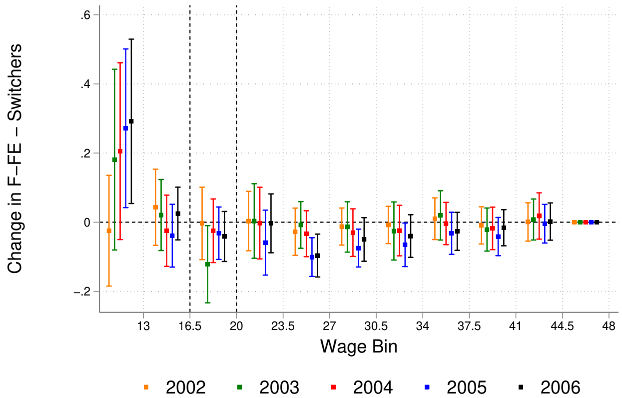
More importantly, Panel (b) of Figure 8 shows the change in the F-FE for job stayers. We find that the NMW reform of 2005 had a sizable effect on the F-FEs of incumbent workers, especially at the bottom of the distribution, but also with spillovers for workers earning above the NMW that are more pronounced than the ones exhibited in Figure 7. This result supports the hypothesis that the NMW reform reduced wage inequality primarily through a causal effect on the levels of the F-FEs. As further support of this interpretation, we estimate an extension of equation (10) that interacts the event coefficients of 2005 and 2006 with indicators of firm exposure. For all bins k, we allow the event coefficients β_{k2005} and β_{k2006} to be different for workers who work in exposed firms (i.e., firms with positive $\overline{GAP}_j$) and non-exposed firms (i.e., firms with $\overline{GAP}_j = 0$). Based on the model presented in Appendix C, wage spillovers are indicative of changes in F-FEs in firms with heterogeneous workforces. Consistent with this narrative, Figure 9 shows that the estimated changes in the F-FEs for non-exposed workers in 2006 (i.e., spillovers) are driven by workers earning above the NMW who work in firms with exposed workers (to properly interpret the figure, note that it only considers wage bins above the NMW of 2006).[18]

[16]Figures D.13, D.14, and D.15 of Appendix D present heterogeneity analyses. Effects are similar between male and female workers, and between younger and older workers. Effects are more precise and pronounced in smaller firms, possibly because larger firms pay higher wages to begin with and, therefore, are underrepresented in the bottom bins of the distribution.

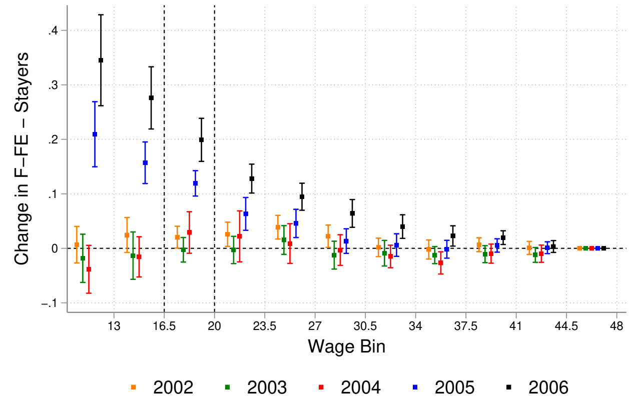
[17]Our definition of reallocation slightly differs from Dustmann et al. (2022) in that we use the TV-AKM F-FEs as the dependent variable. Reassuringly, Figure D.16 of Appendix D shows that using the average F-FE of the pre-2005 period as the dependent variable (as in Dustmann et al., 2022) yields the same results.

[18]As discussed in Section 2, Collective Bargaining Agreements (CBAs) were gradually implemented starting in the second half of 2005, reaching proper coverage and functioning in 2009. Because some industries may have been exposed to CBAs before 2009, we examine whether spillovers in F-FEs for stayers are driven by CBA exposure. Our data lacks information on the CBAs attached to each firm during our sample period; however, we access an auxiliary dataset covering 2017-2020 that

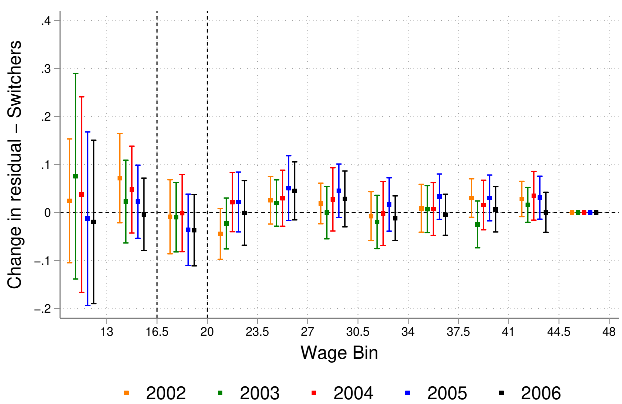
Figure 8: Worker-Level Design: TV-AKM Firm FE and TV-AKM Residual



(a) Change in F-FE (switchers)



(b) Change in F-FE (stayers)



(c) Change in TV-AKM residual (switchers)

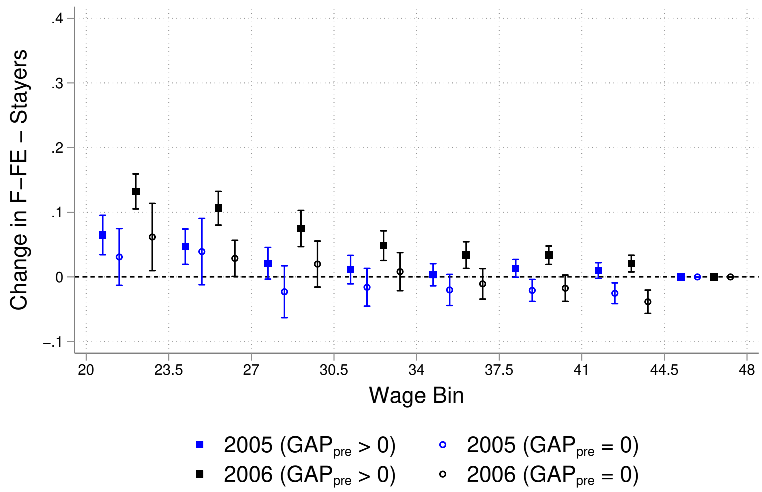
(d) Change in TV-AKM residual (stayers)

Notes: This figure plots the estimated event study coefficients β_{kt} of the worker-level equation (10) with their corresponding 95% confidence intervals using the processed yearly panel described in Section 3 (Panel (c) of Table 1). Within each panel, the horizontal axis accounts for different wage bins (k), and each series represents a different year (t). Each panel uses a different dependent variable. Panels (a) and (b) use the change in the firm fixed effect (F-FEs) estimated with the TV-AKM model for populations of job switchers and stayers, respectively. Panels (c) and (d) use the change in the TV-AKM residual for populations of job switchers and stayers, respectively. All regressions control for a vector of baseline characteristics, including gender, age, tenure, firm size, and firm industry (5-digit ISIC codes). Standard errors are clustered at the (lagged) industry level.

Finally, Panels (c) and (d) of Figure 8 show the change in the TV-AKM residual for the sample of switchers and stayers. The results for low-wage workers who switch jobs point towards no clear effects on their residuals, although the results are noisy. The pattern for job stayers is different and more precisely

links firms to CBAs, compute the modal CBA for each industry (5-digit ISIC codes), and assign it to the firms in our sample based on their industry codes. Using data on the wage floors posted (if any) by each CBA in 2005 and 2006, we divide our sample into terciles of exposure, where the bottom tercile is composed of workers working in firms with little exposure (no wage floor, or wage floor no larger than 10% above the NMW), while the top tercile considers workers in industries that may have been exposed to CBAs with nontrivial wage floors. We estimate the effect on F-FEs for stayers separately for the different groups; results are presented in Figure D.17 of Appendix D. While workers in the top tercile exhibit larger spillovers, F-FEs spillovers are also observed among workers with wages above the NMW in the bottom tercile, suggesting the NMW played an important role in this dynamic. Importantly, the larger spillovers in more exposed industries do not necessarily confirm the role of CBAs, as in 2005 and 2006, their implementation was still at an early stage. The exposure could capture industries with more bargaining power, explaining both the larger spillover from the NMW and also the stronger CBAs.

Figure 9: Worker-Level Design: Spillovers to Non-Exposed Workers by Firm Exposure



Notes: This figure plots the estimated event study coefficients β_{kt} of an extension of the worker-level equation (10) with their corresponding 95% confidence intervals using the processed yearly panel described in Section 3 (Panel (c) of Table 1), only for years 2006 and 2006 and for non-exposed bins $k \geq 4$. The dependent variable is the change in the firm fixed effect (F-FEs) estimated with the TV-AKM model for the population of job stayers. The extended equation (10) interacts the coefficients β_{k2005} and β_{k2006} with indicators of firm exposure to the NMW reform of 2005. Concretely, we allow the event coefficients of 2005 and 2006 to be different for workers who work in exposed firms (i.e., firms with positive $\overline{GAP}_j$, see equation (6)) and non-exposed firms (i.e., firms with $\overline{GAP}_j = 0$, see equation (6)). All regressions control for a vector of baseline characteristics, including gender, age, tenure, firm size, and firm industry (5-digit ISIC codes). Standard errors are clustered at the (lagged) industry level.

estimated. The model presented in Appendix C shows that if wage spillovers are limited, increases in wages after the NMW reform can be explained by a change in residuals. We find evidence of an increase in the residuals of stayers around the NMW levels, but the effect is an order of magnitude smaller than the change in the F-FE. This result suggest that, while minimum wage workers get rents from minimum wage increases above the firm-level premia, an important portion of the wage increase can be rationalized by an increase in the firm-level rent-sharing behavior, as captured by the change in the F-FE.

To assess the quantitative implications of our results, we implement the following decomposition of the wage effects between reallocation, causal, and residual effects. For the bottom four wage bins in the years 2005 and 2006, the bin-specific estimated change in wage growth is decomposed as follows:

$$\Delta\left(\log w_{it} - \log w_{it-2}\right) = \underbrace{\omega^{sw}\Delta^{sw}\left(\psi_{jt} - \psi_{jt-2}\right)}_{\text{Reallocation}} + \underbrace{\omega^{st}\Delta^{st}\left(\psi_{jt} - \psi_{jt-2}\right)}_{\text{Causal}} + \underbrace{\Delta\left(R_{it} - R_{it-2}\right)}_{\text{Residual}}, \tag{11}$$

where ω^{sw} is the share of switchers within the bin, $\omega^{st} = 1 - \omega^{sw}$ is the corresponding share of stayers, $\Delta^{sw}\left(\psi_{jt} - \psi_{jt-2}\right)$ is the change in the F-FE growth for switchers (Panel (a) of Figure 7), $\Delta^{st}\left(\psi_{jt} - \psi_{jt-2}\right)$ is the change in the F-FE growth for stayers (Panel (b) of Figure 7), and $\Delta\left(R_{it} - R_{it-2}\right)$ is the overall change in residuals (weighted average between Panels (c) and (d) of Figure 7). Based on this decomposition, we compute the share of the change in wage growth $\Delta\left(\log w_{it} - \log w_{it-2}\right)$ that can be attributed to each of the three components: reallocation, causal, and residual.[19]

[19]We note this coefficient identity does not hold exactly because coefficients on controls and fixed effects also vary by

Table 4: Worker-Level Analysis: Wage Growth Decomposition

	2005 Reallocation	2005 Causal	2005 Residual	2006 Reallocation	2006 Causal	2006 Residual
Wage bin 1	27.0%	63.1%	9.9%	19.7%	69.5%	10.8%
	[18.6%, 36.9%]	[54.0%, 76.5%]	[-6.0%, 21.8%]	[13.8%, 26.3%]	[62.1%, 77.8%]	[0.2%, 18.7%]
Wage bin 2	-7.6%	95.1%	12.5%	3.0%	85.0%	12.0%
	[-25.2%, 5.7%]	[77.4%, 120.5%]	[-8.4%, 27.3%]	[-6.2%, 11.0%]	[75.7%, 97.4%]	[0.3%, 20.2%]
Wage bin 3	-10.9%	114.1%	-3.1%	-8.3%	89.7%	18.6%
	[-40.4%, 8.7%]	[88.0%, 160.9%]	[-35.2%, 17.9%]	[-23.2%, 3.0%]	[77.2%, 108.3%]	[5.5%, 29.1%]
Wage bin 4	-32.3%	97.8%	34.5%	-0.8%	76.3%	24.5%
	[-97.5%, -0.5%]	[67.0%, 176.2%]	[-3.5%, 60.9%]	[-15.3%, 11.3%]	[64.6%, 92.4%]	[10.8%, 35.4%]

Notes: This table presents the decomposition of the estimated effect on changes in the two-year real hourly wage by wage bin based on equation (11). Reallocation is the share of the wage effect explained by workers switching to firms with higher TV-AKM fixed effects. Causal is the share of the wage effect explained by the change in the TV-AKM firm fixed effect for job stayers. Residual is the share of the wage effect explained by the change in the TV-AKM residual for both switchers and stayers. We report 95% confidence intervals computed using bootstrap with 1000 repetitions. The decomposition is performed separately for each of the bottom four wage bins and for the years 2005 and 2006.

Table 4 presents the decomposition with corresponding 95% confidence intervals. For each bin, the causal effect on the F-FEs of stayers is the most important driver of the increase in wage growth. Part of this result is explained by job switching being less prevalent than staying at the firm. At the bottom bin, the reallocation component accounts for 27% in 2005 and 20% in 2006, while the causal change for stayers is 63% and 70%, respectively. Residual effects in this bin represent around 10% of the total wage effect. In bins 2, 3, and 4, reallocation effects become very noisy, likely reflecting a zero effect, while the residual component becomes relatively more important, especially in 2006. The causal effect remains the most critical component, accounting for at least 76% of the change in wage growth, with many cases including the 100% share in the confidence interval.

8 Conclusion

This paper shows that minimum wages can have a significant impact on the distribution of F-FEs, not only due to reallocation effects but primarily because they causally affect the wage premia of low-paying firms. We demonstrate this empirically using administrative employer-employee data from Uruguay, employing a battery of descriptive analyses that build on the AKM and TV-AKM frameworks, and two causal designs that utilize both firms and workers as units of observation. The estimated causal effect on F-FEs is consistent with the minimum wage changing the wage-setting norms of firms by increasing their rent-sharing. Under the interpretation that F-FEs are proxies of job quality, the results are consistent with the minimum wage increasing the supply of good jobs by making "bad jobs better".

regression. We, therefore, use the change in wage growth implied by the reallocation, causal, and residual estimates as the share denominator, which closely aligns with the estimates reported in Panel (a) of Figure 7.

On a conceptual level, our results suggest that the vast evidence on wage compression effects after minimum wage reforms can be rationalized by the minimum wage limiting the role of firms in setting wages. As a consequence, our results suggest that F-FEs can be influenced by policy, even if they are rooted in firms' fundamentals that may persist over time like productivity. These conclusions provide support to the conjecture that differences in labor market institutions can rationalize the heterogeneous impact of firms on inequality, both across countries and within countries over time.

Two fruitful avenues of future research naturally follow from our empirical analysis. First, our results have implications for the development of labor market models that rationalize wage dispersion across firms for similar workers, as they indicate that the minimum wage plays a significant role in determining firm wage premia. Explicitly introducing minimum wages in frameworks that microfound wage dispersion for similar workers may improve the qualitative and quantitative performance of these models. Second, other policies and labor market institutions may also have an impact on the distribution of F-FEs. Testing for these effects using suitable reforms will enable a more comprehensive empirical understanding of the impact of public policies on wage inequality.

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Minimum Wages and the Distribution of Firm Wage Premia

Online Appendix

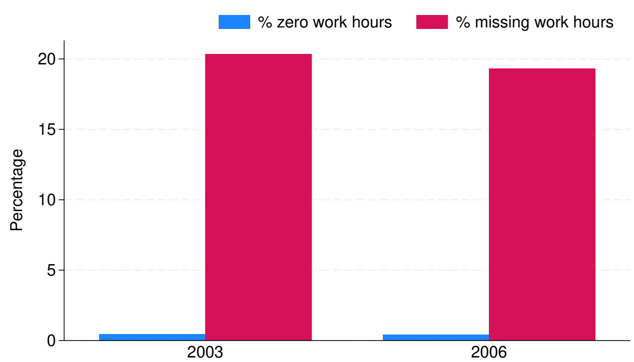
A Data Appendix

A.1 Quality tests of the administrative records on work hours

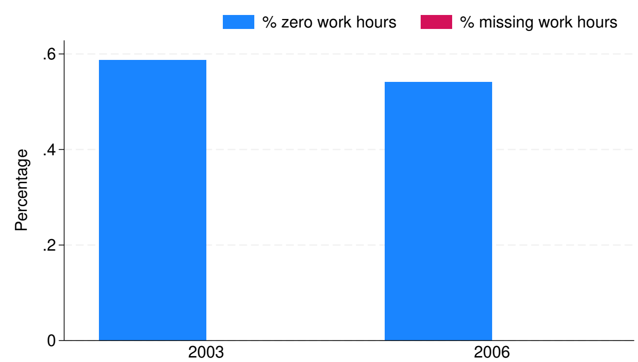
We implement a series of tests proposed in Lachowska et al. (2022) –for administrative records in the State of Washington– to examine the quality of the administrative records of Uruguay's SSA on hours. We present results on our estimation sample using two years as a reference period: 2003 (pre-NMW reform) and 2006 (post-NMW reform). As a reminder, our measure of annual hours is the total hours worked in a year. When employers report both weekly hours worked and total days worked within a month, monthly hours can be computed without any imputation. This case represents roughly 75% of our baseline yearly panel observations. When employers report weekly hours but days worked are missing, the worker is assumed to have worked the complete month in the firm. This case represents roughly 5% of our baseline yearly panel observations. Finally, when employers report days worked but weekly hours are missing, the worker is assumed to be under a standard 8-hour-per-day labor contract, up to a maximum of 200 hours per month. This case represents roughly 20% of our baseline yearly panel observations. We present our diagnostics separately for the raw hours measure (no imputation) and the final hours measure.

First, we analyze the extent to which employers report that a worker had positive earnings but zero or missing hours in the same period. Panel (a) of Figure A.1 shows the proportion of records that have zero (blue bars) or missing (red bars) working hours. In both years, about 20% of the raw SSA records have zero or missing hours worked despite showing positive values in the earnings records. These cases, however, mostly coincide with cases in which employers report missing hours worked but report the employee's monthly days worked. A potential explanation for this pattern is that, in Uruguay, workers in sectors such as manufacturing and services are subject to an 8-hour-per-day legal regime; thus, employers may only report the number of days worked to the SSA. Panel (b) of Figure A.1 reproduces Panel (a) but using the final measure of hours worked variable with imputed values. After the imputation, less than 1% of SSA records reported zero hours, and virtually 0% had missing hours worked. Importantly, there is no significant change in the pattern before and after 2005.

Figure A.1: Percentage of Observations with Zero or Missing Work Hours (SSA Records)



(a) Without imputations

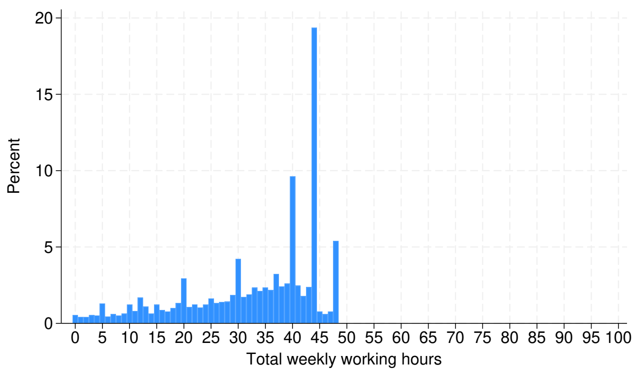


(b) With imputations

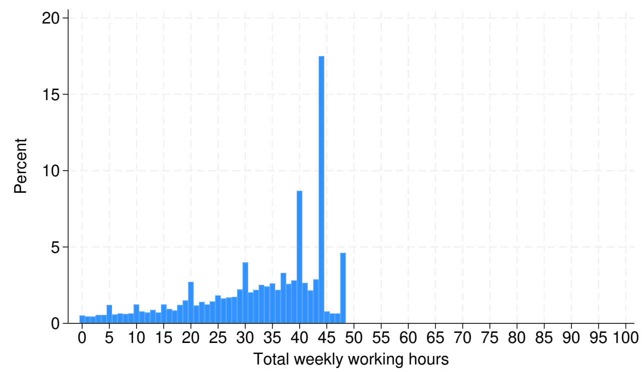
Notes: This figure plots the percentage of observations with positive earnings but zero or missing working hours. Panel (a) shows raw reported hours, while Panel (b) shows our final measure of hours with imputations based on days worked. The data comes from SSA administrative records for the years 2003 and 2006.

Second, we compare the distribution of hours from SSA records with that of working hours from the Uruguayan National Household Survey (ECH) for the sample of wage and salary workers in the private sector. The intuition is that if, for example, the administrative records display a much larger spike at 20 or 40 hours than the ECH, it may suggest that employers are unable to accurately track the actual hours worked by a substantial number of employees and instead report standard working hour packages (e.g., either part-time (20 hours) or full-time (40 hours)). Figure A.2 shows the distribution of weekly hours based on administrative records for the years 2003 and 2006, with and without the imputation procedure. Both distributions look similar, except for the 47-hour spike observed when applying the imputation procedure. This spike is due to the imposition of a 200-hour-per-month cap when the records report 7 days worked per week. In Figure A.3, we show the distribution of hours worked according to surveyors' responses to the ECH in the corresponding years. Similar to the Current Population Survey (CPS) in the US, the ECH asks respondents to report "usual" weekly hours at their main job, whereas the administrative data report workers' weekly hours worked for each employer. In the administrative records, we observe some spread in hours reported, with some "heaping" at around 20, 30, 40, 44, 48, and 56 hours per week. In particular, about 15% of the employer reports cluster at the 44-hour-per-week spike for both analyzed years, which corresponds to a standard 8-hour workday from Monday to Friday plus a half-day of work on Saturday. Although the distribution based on the ECH responses also shows a "heaping" pattern at those round numbers, the gaps between spikes are much more prominent than those observed in the administrative hours distribution. For example, in the ECH, almost 36% of wage and salary private sector workers reported 40 hours per week in 2003, while only 8% in the administrative reports clustered at that same hour-per-week spike. These results imply that employers in Uruguay do not merely artificially report a standard working week. Lachowska et al. (2022) observed qualitatively similar findings when comparing CPS data with administrative records in the State of Washington.

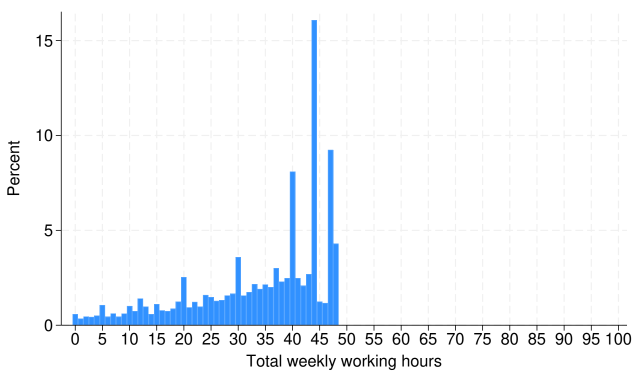
Figure A.2: Distribution of Weekly Hours Worked (SSA Records)



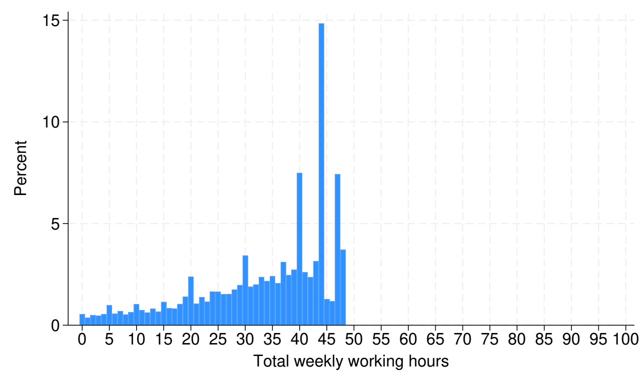
(a) Year 2003 (without imputations)



(b) Year 2006 (without imputations)



(c) Year 2003 (with imputations)

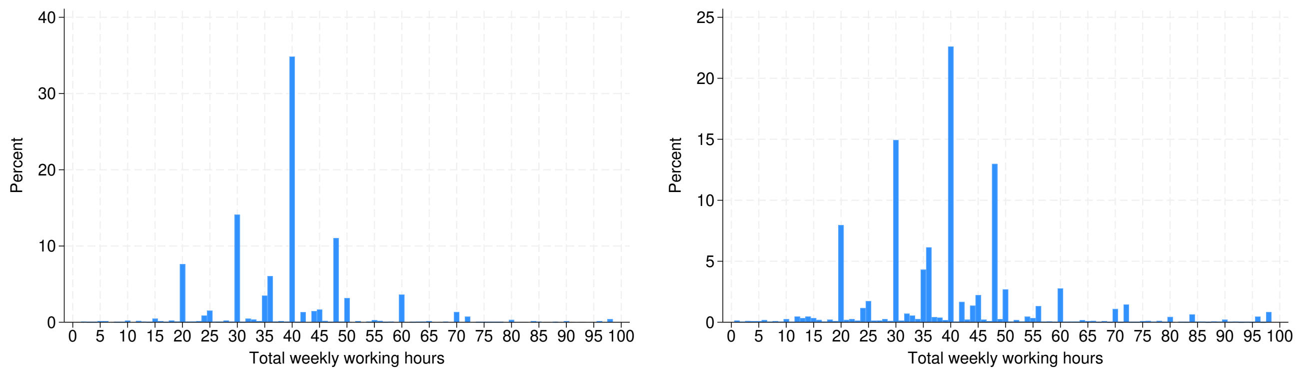


(d) Year 2006 (with imputations)

Notes: This figure plots the distribution of weekly working hours in the administrative data. Panels (a) and (b) show raw reported hours, while Panels (c) and (d) show our final measure of hours with imputations based on days worked. The data come from SSA administrative records for the years 2003 and 2006, respectively. Histograms are trimmed at 100 hours per week.

Third, we regress the yearly changes in log earnings on the changes in log hours. If hours are measured with substantial error, we would expect the slope coefficient in the regression to be attenuated, while it would be close to one if hours are measured accurately. Panel (a) of Table A.1 presents the estimated coefficient of such regressions using observations of workers who remained with the same employer in successive years (i.e., "stayers"). For estimates in years 2003 and 2006, the underlying samples correspond to stayers observed in years 2002–2003 and 2005–2006, respectively. Regressions use the hours worked variable constructed using the imputation procedure when the records show missing values for daily hours. Following Lachowska et al. (2022), Columns (1) through (4) show slope coefficients from models with and without employer fixed effects and with and without clustered standard errors. The results in Panel (a)

Figure A.3: Distribution of Weekly Hours Worked, (Household Survey Data, ECH)



(a) Year 2003

(b) Year 2006

Notes: This figure plots the distribution of usual weekly working hours reported in Uruguay's national household survey. The data come from ECH recorded by the National Institute of Statistics (*Instituto Nacional de Estadísticas*, INE) for the years 2003 and 2006, respectively. To make the household survey sample comparable to administrative records, we select workers in the ECH who are private-sector salaried employees aged 20-60. Histograms are trimmed at 100 hours per week.

of Table A.1 show estimated slope coefficients close to 1, which is consistent with what we expect from data with little measurement error in hours, given that most of our sample consists of salaried workers who presumably have proportional changes in earnings and hours from year to year. Panel (b) of Table A.1 presents estimates with the weekly hours variable constructed without the imputation procedure and shows similar results.

A.2 Building balance sheet variables from corporate tax data

The Corporate Income Tax Data (CIT) covers the universe of medium-sized and large private firms in Uruguay. The data contains information on balance sheets and income statements, allowing us to measure firms' income and cost structure. We have access to forms 2148 and 2149 for the period 2009-2017. The two forms are different but have the same cell numbering to be filled out. In the following, we list the definitions of our key variables and some details about their construction.

Value of Production. Corresponds to revenue plus operational grants. This is reported by firms in cell 100.

Total gross wages. Corresponds to total gross wages paid by the firm (before taxes and social security contributions). It comprises the sum of all wages, salaries, and bonuses paid to workers. The construction of this variable is based on the data from the SSA records before imposing our sample restrictions. We merge this variable with the CIT at the firm level using the procedure described in Section 3.

Production costs. Corresponds to all operating expenses (excluding labor costs). It includes net purchases, administrative costs, and other minor costs (e.g., services and third-party costs). Production and administrative costs are netted out of wage costs, as these cells in the CIT forms usually include

Table A.1: Estimates of the Change in Log Earnings on the Change of Log Hours (SSA Registers)

Panel (a) With imputations

	Change in Log Monthly Earnings			
	(1)	(2)	(3)	(4)
Change in Log Monthly Hours	1.070	1.070	1.117	1.117
	(0.008)	(0.008)	(0.009)	(0.009)
Constant	0.152	0.152	0.150	0.150
	(0.001)	(0.001)	(0.001)	(0.001)
Observations	375,649	375,649	368,311	368,311
R-sq	0.155	0.155	0.336	0.336
Employer fixed effects	No	No	Yes	Yes
Standard errors	Standard	Clustered by worker	Standard	Clustered by worker

Panel (b) Without imputations

	Change in Log Monthly Earnings			
	(1)	(2)	(3)	(4)
Change in Log Monthly Hours	1.144	1.144	1.204	1.204
	(0.010)	(0.010)	(0.011)	(0.011)
Constant	0.151	0.151	0.149	0.149
	(0.001)	(0.001)	(0.001)	(0.001)
Observations	284,086	284,086	277,804	277,804
R-sq	0.173	0.173	0.359	0.359
Employer fixed effects	No	No	Yes	Yes
Standard errors	Standard	Clustered by worker	Standard	Clustered by worker

Notes: This table shows the estimated slope coefficient from a regression of the change in log monthly earnings on the change of log monthly hours. It includes annualized data based on years 2002, 2003, 2005, and 2005 from the SSA administrative records. Panel (a) shows our final measure of hours with imputations based on days worked, while Panel (b) shows raw reported hours.

employee compensation. This is reported by firms in cell 177.

Value added. Corresponds to the value a firm can create from inputs during production. It is computed as follows: value of production minus production costs.

Employment. To normalize value added, we build a full-time equivalent employment measure as follows. From the SSA records, we identify at the firm-by-month level the number of workers employed at the firm. Then we sum all employed workers each month, weighting by 0.5 the ones that report working less than 40 hours a week. Then, the firm-by-year full-time equivalent employment is the average across the twelve months of the monthly measure.

Value added per worker. Corresponds to value added divided by employment.

Labor Share. Corresponds to total gross wages over total value added.

A.3 From raw data to annual panel

We impose the following restrictions when transitioning from the raw monthly data to a unique worker-year observation, in the following order. First, we drop observations for jobs in the public sector (on average, 28% of the initial observations each year). Second, we drop duplicate observations (0.4%) and observations for which monthly earnings are zero or missing (7%). Third, we drop observations attached to unipersonal firms, defined as firms where they have exactly one worker in each month (5%). Fourth, we drop observations for which monthly hours are zero or missing (0.3%).[1] Fifth, we only keep observations that pertain to job matches recorded as salaried workers or employees (that is, we drop observations associated with shareholders, internships, and explicitly short-term temporary work arrangements, 10%). After this basic cleaning, workers may appear more than one time in a firm-month pair (0.4%). In those cases, we keep the observation with maximum monthly earnings, leaving us with a panel with unique observations at the worker-firm-month level. Then, we drop worker-year combinations that exhibit 10 or more different employers within a year, where the total number of employers is recorded before applying the previous restrictions (0.02%). From this panel, we identify the primary annual employer as the employer that yielded the maximum annual earnings and drop the observations related to secondary employers (3.4%). After collapsing the data at the worker-year level, we also drop the observations for which the firm has a missing industry indicator (3%).

B Informal Employment and Wage Underreporting in Uruguay

This appendix uses the National Household Survey of Uruguay (ECH) to provide statistics related to employment informality and formal wage underreporting. Section 2 discusses in more detail the implications of these statistics for our analysis based on the SSA administrative records on formal employment and wages. The key takeaway is that the prevalence of informality in Uruguay is low enough to have a significant impact on our analysis. The ECH is a nationally representative household survey conducted by Uruguay's National Institute of Statistics (INE) in accordance with international standards since 1990. It combines elements of living standards and labor force surveys, and it is the main source of socioeconomic, labor, and demographic indicators in Uruguay. The microdata and supporting documents are publicly available. Similar in scope to the Current Population Survey (CPS) in the US, the ECH provides information on both formal and informal employment and wages. To ensure comparability with the sample used in our analysis based on the SSA records, which excludes marginal or transitory employment relationships, we restrict the analysis to private-sector employees who work at least 20 hours per week, are employed in firms with two or more workers, and have a minimum job tenure of three months.

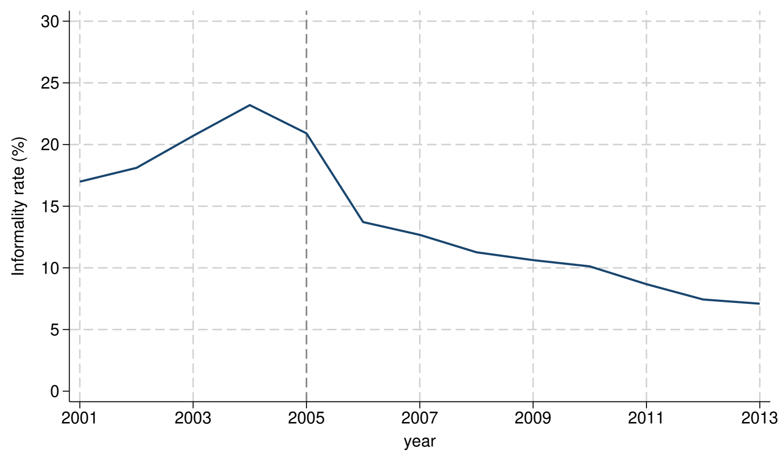
Informal Employment. The identification of informality in the ECH has varied over time. From 2001 onward, the survey allows for a direct classification based on explicit questions about social security contributions, as it is standard in the related literature (see, e.g., Gasparini and Tornarolli, 2009). For earlier years, this information is not available, so informality was identified indirectly through questions on health-care coverage in the private sector provided through formal employment. To ensure our informality definition is internally consistent, we focus on the post-2001.

Figure B.1 plots the informality rate for the period 2001-2013. We observe an increase between 2001 and 2004, which is associated with the business cycle. In 2004, the year before the national minimum

[1] When building the panel with no imputed hours, we drop the observations with imputed hours in this step.

wage (NMW) reform, the informality rate rose to about 23%. However, starting in 2005, the informality rate exhibited a sustained downward trend, reaching approximately 7% in 2013. Interestingly, the period of rapid NMW increase is descriptively associated with a decrease in the size of the informal sector.

Figure B.1: Informality Rate



Notes: This figure plots the percentage of informal workers from the National Household Survey (ECH) between 1997 and 2013. We consider only salaried workers aged 20 to 60 who work at least 20 hours per week, are employed in firms with at least two workers, and have a minimum job tenure of 3 months. The informality definition is based on explicit questions about social security contributions.

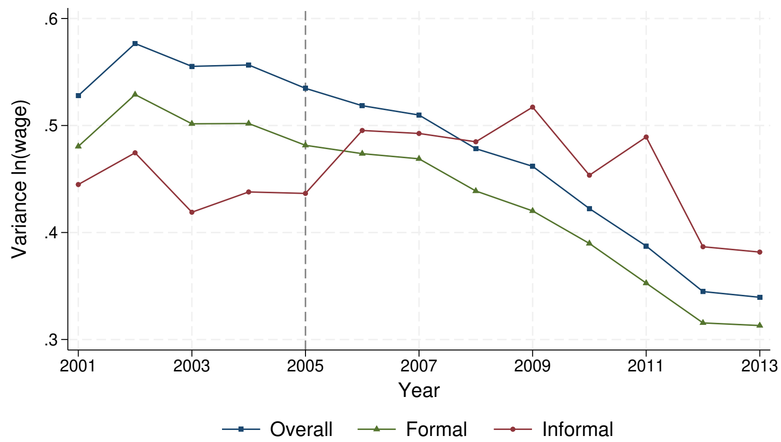
When focusing on wage inequality, measured as the variance of real log wages, Figure B.2 shows a pattern for formal workers similar to that obtained with the SSA data (see Panel (a) of Figure 2 and Figure D.1 of Appendix D), i.e., a strong decline in the variance of log wages starting in 2005. Within informal workers, wage inequality increased modestly in the years after the minimum wage increase. However, while the gap between formal and informal wage inequality widens after the NMW reform, these dynamics have no material impact on overall wage inequality (including both formal and informal workers), which closely tracks the evolution of the variance of log wages in the formal sector. Moreover, the gap between the overall and formal-sector series, which is already small, appears to narrow after the NMW reform, likely explained by the decrease in the size of the informal sector documented above.

Finally, Figure B.3 plots harmonized informality rates for a sample of Latin American countries for the year 2023, for both the total population of salaried workers and for salaried workers aged between 25 and 64. Data come from the Labor Database for Latin America and the Caribbean (LABLAC), constructed by CEDLAS and the World Bank.[2] The figure shows that informality rates are substantially lower in Uruguay than in all other countries in the region, and by a large margin. This pattern holds for both samples considered.

Wage underreporting. To identify wage underreporting, we use information from the ECH since 2006 onward, which directly asks to registered private-sector employees whether their salaried earnings are underreported to the tax and social security authorities. The specific question is: "Are your contributions

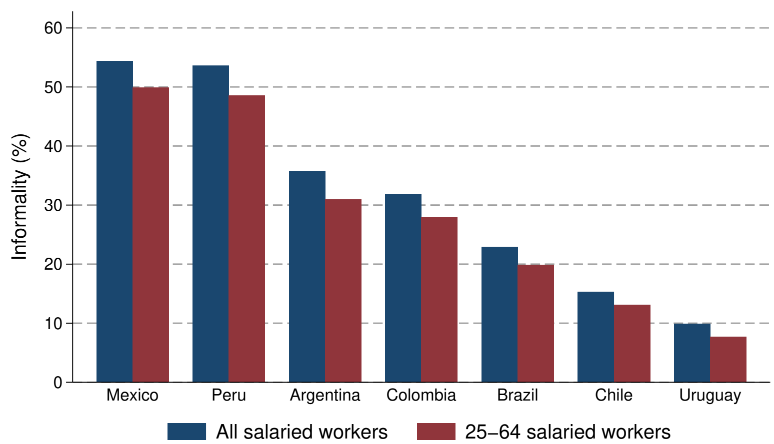
[2]The statistics are publicly available at this link (accessed on February 6, 2026).

Figure B.2: Earnings Inequality: Formal and Informal Workers



Notes: This figure plots the variance of wages for overall, formal, and informal workers from the National Household Survey (ECH) between 2001 and 2013. We consider only salaried workers aged 20 to 60 who work at least 20 hours per week, are employed in firms with at least two workers, and have a minimum job tenure of 3 months. The informality definition is based on explicit questions about social security contributions.

Figure B.3: Share of Informal Salaried Workers, Circa 2023



Notes: This figure plots the fraction of salaried informal workers. The blue bars are computed using all workers, while the red bars consider only workers aged 25 to 64. The data come from LABLAC (CEDLAS, and the World Bank). All countries account for 2023, with the exception of Mexico, which accounts for 2021.

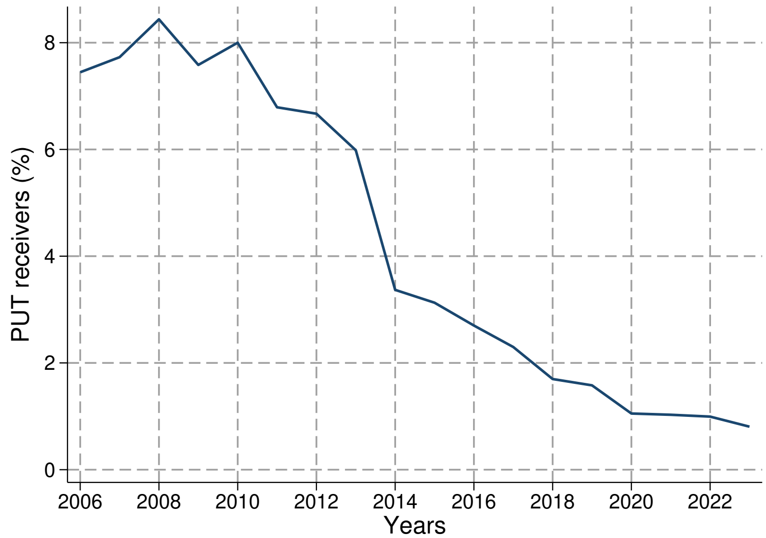
based on the total amount of your earnings from this job?”.[3] We use this variable to proxy for the prevalence of “payments under the table” (PUTs, Feinmann et al., 2025).

Figure B.4 plots the evolution of the share of respondents receiving PUTs over the period 2006–2013, showing a relatively stable pattern of around 6–8%. The share of respondents receiving PUTs sharply decreases starting in 2014, reaching 1.4% in 2023.

Finally, we benchmark the prevalence of PUTs in Uruguay to other countries in the region using the cross-country surveys of Feinmann et al. (2024). To compute the incidence of PUT among formal

[3]The specific question in Spanish reads: *“¿Aporta por la totalidad del salario en esa ocupación?”*

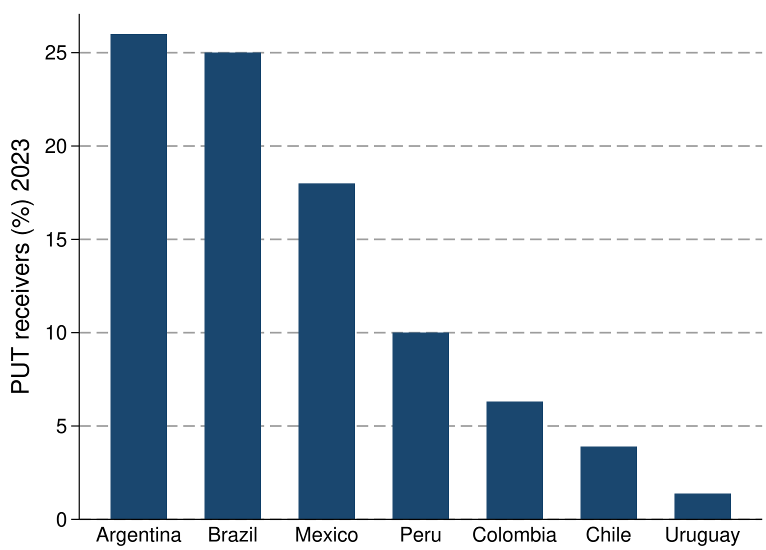
Figure B.4: Share of Formal Employees Receiving PUTs in Uruguay



Notes: This figure plots the fraction of formal workers who receive PUTs using data from the National Household Survey (ECH) between 2006 and 2023.

employees in Uruguay, we impose sample restrictions similar to those in Feinmann et al. (2024).[4] Figure B.5 shows the results. Similar to the figures on informality rates, the figure shows that the prevalence of PUTs in Uruguay is substantially lower than the rates estimated for the other countries in the region. The number is, by far, the lowest among the countries in the sample.

Figure B.5: Share of Formal Employees Receiving PUTs, Circa 2023



Notes: This figure plots the fraction of formal workers receiving PUTs. Data for Uruguay come from the National Household Survey (ECH), while data for other Latin American countries come from Feinmann et al. (2025).

[4]We compute the incidence only among full-time formal and private employees and above 18 years-old.

C Model

We propose a simple extension of Card et al. (2018) model. Workers are heterogeneous in skill and firms are heterogeneous in productivity. To simplify exposition, assume there are low- and high-skill workers indexed by $s \in \{l, h\}$, and low- and high-productivity firms indexed by $j \in \{1, 2\}$. Workers have heterogeneous preferences for firms (conditional on skill) and firms operate with linear technologies in labor, so production of firm j takes the form $Y_j = R_j(\theta_l L_j + \theta_h H_j)$, where R_j is the firm-specific productivity shifter, θ_s is the relative productivity of workers of skill s, and (L_j, H_j) is the number of low- and high-skill workers hired by firm j, respectively.

Card et al. (2018) shows that, if there are no strategic interactions between firms, a set of parametric assumptions yields an equilibrium equation for log wages that has an AKM representation:

$$\log w_{sj} = \alpha_s + \psi_j \equiv \log \theta_s + \log(1 + R_j), \tag{C.1}$$

where, for simplicity, we have normalized to 1 the constants that do not affect our argument below. For completeness, below in this appendix we derive equation (C.1) from first principles to explicitly state details on the baseline structure, the derivations, and the additional restrictions.

Wage setting norms. To analyze the effects of introducing a binding minimum wage on the structural equation (C.1), we extend the CCHK model to incorporate firm-specific wage setting norms that potentially mediate within-firm wage spillovers (e.g., Dube et al., 2019; Giupponi and Machin, 2024).

To fix ideas, assume that, in the absence of a minimum wage, $w_{l1} < w_{l2} < w_{h1} < w_{h2}$. From equation (C.1), it follows that $w_{hj}/w_{lj} = \theta_h/\theta_l$ for every j. Workers and firms internalize this equilibrium condition, which gives rise to a wage-setting norm of relative wages within the firm. We assume that, in addition to being heterogeneous in productivity, firms are heterogeneous in the extent to which they respect the wage-setting norm, which we parametrize by the parameter ρ_j. ρ_j affects wage-setting behavior as follows. Suppose that, because of some regulation (e.g., the NMW), w_{lj} exogenously increases to $w_{lj}(1 + a_j)$, for some a_j. The firm-specific wage-setting norm restricts the behavior of the high-skill wage, which is forced to increase to $w_{hj}(1 + \rho_j a_j)$. If $\rho_j = 0$, there is no within-firm spillover, and we are back to the baseline CCHK model. If $\rho_j = 1$, there is full passthrough to high-skill wages, so the new wages respect the benchmark θ_h/θ_l. In a case with $\rho_j \in (0, 1)$, there will be a partial spillover to the high-skill wage.

Introducing a minimum wage. We denote by $x^{\overline{w}}$ the value of a variable x when the minimum wage equals $\overline{w}$. Values of x when there is no binding minimum wage are denoted by x^0.

Assume that the government introduces a minimum wage $\overline{w} \in (w^0_{l1}, w^0_{l2})$, so $\overline{w}$ only binds low-skill workers in the low-paying firm. Since there are no strategic interactions between firms, wages in firm $j = 2$ are not affected by the minimum wage, so $w^{\overline{w}}_{s2} = w^0_{s2}$, for $s \in \{l, h\}$. Likewise, the firm-specific component is fixed in firm $j = 2$, so $\psi^{\overline{w}}_2 = \psi^0_2$. It follows from equation (C.1) that $\alpha^{\overline{w}}_s = \alpha^0_s$, for $s \in \{l, h\}$.

On the contrary, wages in firm $j = 1$ are affected by the minimum wage. The new equation for low-skill wages in firm $j = 1$ is given by $\log w^{\overline{w}}_{l1} = \alpha^{\overline{w}}_l + \psi^{\overline{w}}_1 + e_{l1} \equiv \log \overline{w}$, where e_{l1} is a potential residual generated by the introduction of the minimum wage. Using the restriction $\alpha_l^{\overline{w}} = \alpha_l^0$, we can write $\log \overline{w} - \log w_{l1}^0 = \psi_1^{\overline{w}} + e_{l1} - \psi_1^0$. While the behavior of firm $j = 2$ restricts this wage change to not be caused by a change in the worker fixed effect, it remains an open question whether it is driven by a change in the F-FE or the residual term.

Both terms can be separately identified using the wage equation of high-skill workers in firm $j = 1$. We know that $\log w_{h1}^{\overline{w}} - \log w_{h1}^0 = \psi_1^{\overline{w}} + e_{h1} - \psi_1^0$, where, by definition, $L_1 e_{l1} + H_1 e_{h1} = 0$. We note that $\log \overline{w} - \log w_{l1} = \log(1 + a_1(\overline{w}))$: the wage increase factor $a_1(\overline{w}) > 0$ after the minimum wage introduction is firm-specific and depends on the minimum wage. Putting all terms together, we have that:

$$\psi_1^{\overline{w}} + e_{l1} - \psi_1^0 \quad = \quad \log(1 + a_1(\overline{w})), \tag{C.2}$$

$$\psi_1^{\overline{w}} + e_{h1} - \psi_1^0 \quad = \quad \log(1 + \rho_1 a_1(\overline{w})). \tag{C.3}$$

Using $L_j e_{l1} + H_j e_{h1} = 0$ and solving for $\psi_1^{\overline{w}}$ yields:

$$\psi_1^{\overline{w}} = \psi_1^0 + \left(\frac{L_1}{H_1 + L_1} \log(1 + a_1(\overline{w})) + \frac{H_1}{H_1 + L_1} \log(1 + \rho_1 a_1(\overline{w})) \right), \tag{C.4}$$

which implies that:

$$\begin{aligned} e_{s1} \quad = \quad & \log\left(1 + a_1(\overline{w})\left(1\{s = l\} + 1\{s = h\}\rho_1\right)\right) \\ & - \left(\frac{L_1}{H_1 + L_1} \log(1 + a_1(\overline{w})) + \frac{H_1}{H_1 + L_1} \log(1 + \rho_1 a_1(\overline{w})) \right). \end{aligned} \tag{C.5}$$

The relative size of the change in the firm fixed effect and the residual is governed by ρ_1 and the within-firm employment shares. When $\rho_1 = 1$, all of the minimum wage increase is loaded into the firm fixed effect. Conditional on (L_1, H_1), the relative importance of the residual decreases with ρ_1.

C.1 Card et al. (2018) derivation of equation (C.1)

Setup. Consider J firms indexed by $j \in \{1, ..., J\}$. There are two types of workers heterogeneous in skill, $s \in \{l, h\}$, indexed by i. The total number of low- and high-skill workers is given by $\{\mathcal{L}, \mathcal{H}\}$. Firms post wages (w_{lj}, w_{hj}), which are observed by workers.

Workers' problem. Workers have preferences for firms, with ϵ_{isj} denoting the preference shock of worker i of skill s for firm j. When employed in firm j, worker i of skill s gets indirect utility:

$$u_{isj} = \eta_s \log(w_{sj} - b_s) + \nu_{sj} + \epsilon_{isj}, \tag{C.6}$$

where η_s mediates the skill-specific labor supply elasticity, b_s is an outside option equal to all workers within skill s, and ν_{sj} is a firm-specific amenity common to all workers of skill s.

Assuming that the $\{\epsilon_{isj}\}$ are independent draws from a type I extreme value distribution, the firm choice probabilities are given by:

$$p_{sj} \quad = \quad \Pr\left(\arg\max_{k}\{u_{isk}\} = j\right) = \frac{\exp\left(\eta_s \log(w_{sj} - b_s) + \nu_{sj}\right)}{\sum_k \exp\left(\eta_s \log(w_{sk} - b_s) + \nu_{sk}\right)}. \tag{C.7}$$

Assume away strategic interactions in wage setting between firms and J large. In that case:

$$p_{sj} \quad \approx \quad \lambda_s \exp\left(\eta_s \log(w_{sj} - b_s) + \nu_{sj}\right), \tag{C.8}$$

where λ_s is a constant common to all firms. Then, the firm-specific labor supply elasticities are given by:

$$\log L_j(w_{lj}) \quad = \quad \log(\mathcal{L}\lambda_l) + \eta_l \log(w_{lj} - b_l) + \nu_{lj}, \tag{C.9}$$

$$\log H_j(w_{hj}) \quad = \quad \log(\mathcal{H}\lambda_h) + \eta_h \log(w_{hj} - b_h) + \nu_{hj}. \tag{C.10}$$

Firms' problem. Firms have technology $Y_j = R_j f(L_j, H_j)$, with R_j a firm-specific productivity shifter and (L_j, H_j) the number of low- and high-skill workers hired by firm j. We assume $Y_j = R_j\left(\theta_l L_j + \theta_h H_j\right)$. The firms' problem is to set (w_{lj}, w_{hj}) to minimize costs internalizing the labor supply elasticities:

$$\min_{w_{lj}, w_{hj}} w_{lj} L_j(w_{lj}) + w_{hj} H_j(w_{hj}) \quad s.t. \quad R_j\left(\theta_l L_j + \theta_h H_j\right) \geq Y. \tag{C.11}$$

Firms don't observe $\{\epsilon_{isj}\}$, so they cannot perfectly price-discriminate. The FOCs are given by:

$$L_j + w_{lj}\frac{\partial L_j}{\partial w_{lj}} - \mu_j R_j \theta_l \frac{\partial L_j}{\partial w_{lj}} \quad = \quad 0, \tag{C.12}$$

$$H_j + w_{hj}\frac{\partial H_j}{\partial w_{hj}} - \mu_j R_j \theta_h \frac{\partial H_j}{\partial w_{hj}} \quad = \quad 0, \tag{C.13}$$

where μ_j is the constraint multiplier (marginal cost of production). Grouping terms yields:

$$w_{lj}\left(\frac{1 + e_{lj}}{e_{lj}}\right) \quad = \quad \mu_j R_j \theta_l, \tag{C.14}$$

$$w_{hj}\left(\frac{1 + e_{hj}}{e_{hj}}\right) \quad = \quad \mu_j R_j \theta_h, \tag{C.15}$$

where e_{sj} are the labor supply elasticities. From equations (C.9) and (C.10), these elasticities equal:

$$e_{Lj} = \frac{\eta_l w_{lj}}{w_{lj} - b_l}, \tag{C.16}$$

$$e_{Hj} = \frac{\eta_h w_{hj}}{w_{hj} - b_h}, \tag{C.17}$$

which together with the FOCs imply that:

$$w_{lj} = \frac{b_l}{1+\eta_l} + \eta_l \frac{\mu_j R_j \theta_l}{1+\eta_l}, \tag{C.18}$$

$$w_{hj} = \frac{b_h}{1+\eta_h} + \eta_h \frac{\mu_j R_j \theta_h}{1+\eta_h}. \tag{C.19}$$

When $b_s = 0$, the labor supply elasticity is equal to η_s and the equilibrium wage is equal to a fraction of the value of the marginal product (wage markdown), which is a function of the labor supply elasticity.

Assume that the reference wages, b_s, are proportional to the relative productivities, so $b_l = \theta_l b$ and $b_h = \theta_h b$. Let $T_j = \mu_j R_j / b$ be the relative productivity of firm j relative to the "outside sector". Then

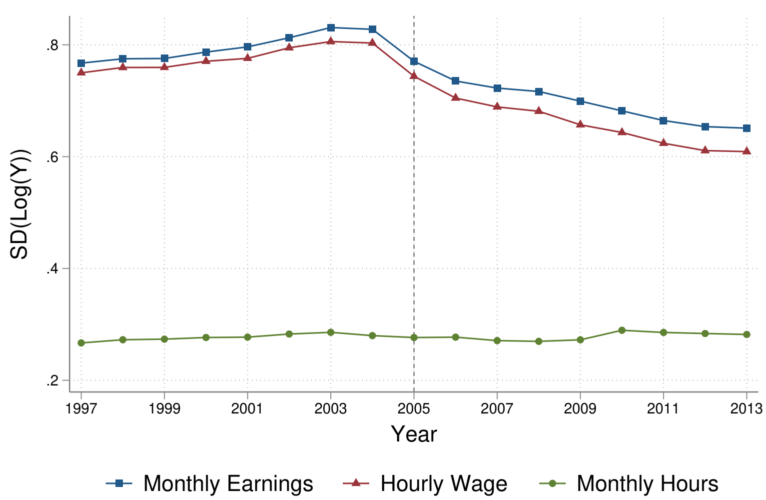
$$\log w_{lj} = \log \frac{\theta_l b}{1+\eta_l} + \log(1+\eta_l T_j), \tag{C.20}$$

$$\log w_{hj} = \log \frac{\theta_h b}{1+\eta_h} + \log(1+\eta_h T_j). \tag{C.21}$$

When $\eta_l \approx \eta_h$, then $\log w_{sj} \approx \alpha_s + \psi_j$, which coincides with an AKM decomposition. Normalizing $b = \eta_l = \eta_h = \mu_j = 1$ and ignoring the constant $\log(0.5)$ yields equation (C.1).

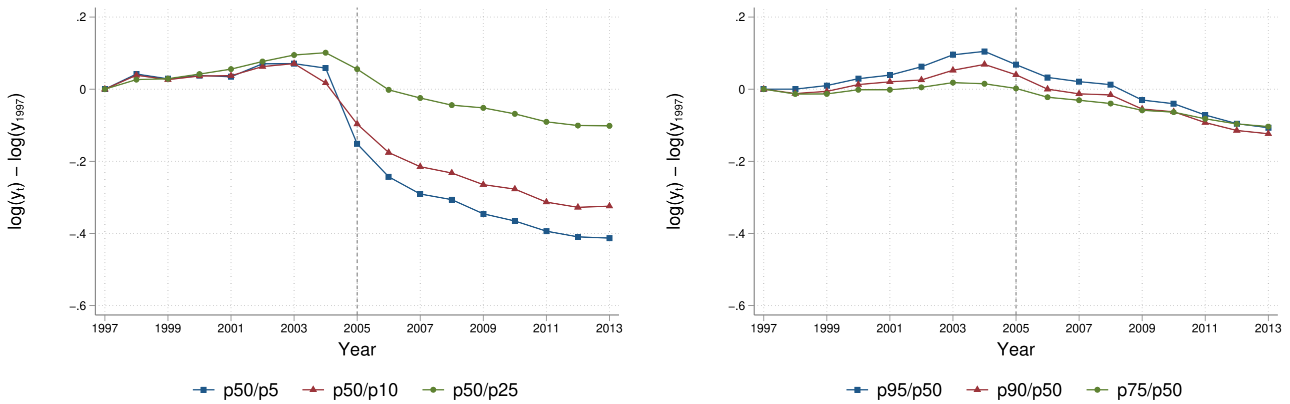
D Additional Figures and Tables

Figure D.1: Wage, Earnings, and Hours Inequality



Notes: This figure uses the processed yearly panel described in Section 3 (Panel (c) of Table 1) to extend Panel (a) of Figure 2. SD accounts for Standard Deviation. The blue series plots the standard deviation of the log monthly earnings. The red series plots the standard deviation of the log hourly wages. The green series plots the standard deviation of the log monthly hours.

Figure D.2: Evolution of Percentile Ratios

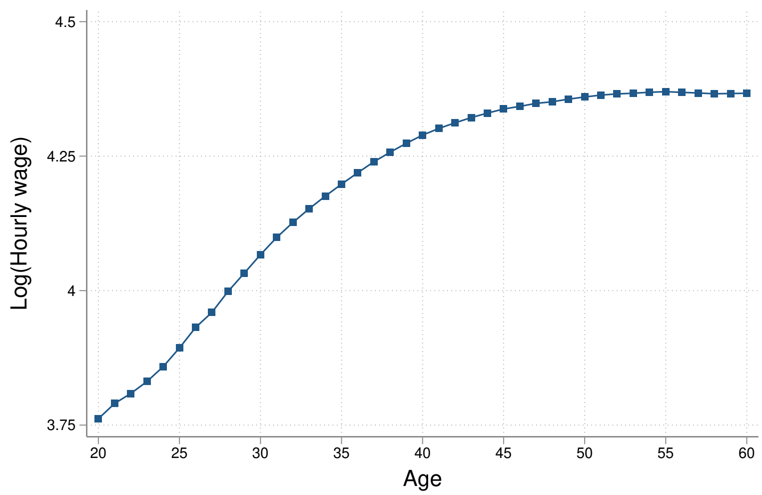


(a) Lower tail ratios

(b) Upper tail ratios

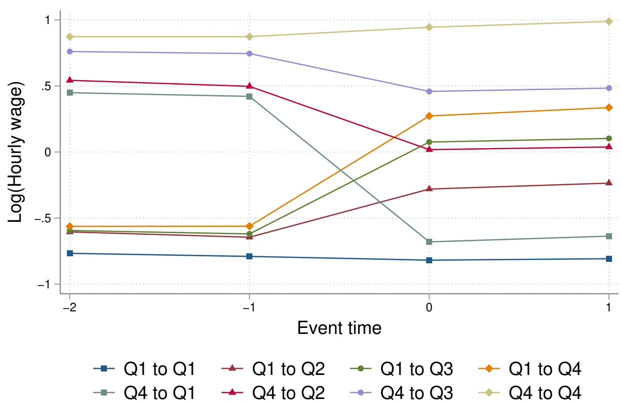
Notes: This figure uses the processed yearly panel described in Section 3 (Panel (c) of Table 1) to extend Panel (b) of Figure 2. Panel (a) shows lower tail ratios of percentiles. The blue series plots the p50/p5 ratio. The red series plots the p50/p10 ratio. The green series plots the p50/p25 ratio. Panel (b) shows upper tail ratios of percentiles. The blue series plots the p95/p50 ratio. The red series plots the p90/p50 ratio. The green series plots the p75/p50 ratio.

Figure D.3: Age-Wage Pattern

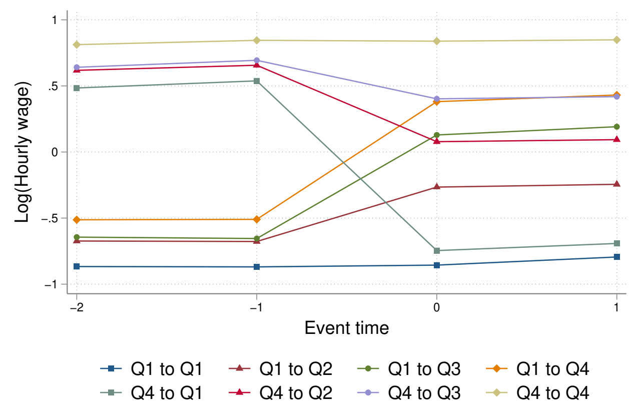


Notes: This figure plots the age-wage pattern observed in the processed yearly panel described in Section 3 (Panel (c) of Table 1). This figure is computed as follows. We first average log-hourly wages by age and cohort and then residualize the average log-hourly wages against cohort fixed effects. The residual is then plotted against age. The coefficients that emerge from a regression on the residual against age and age squared reveal that the curve flattens when age is equal to 53.

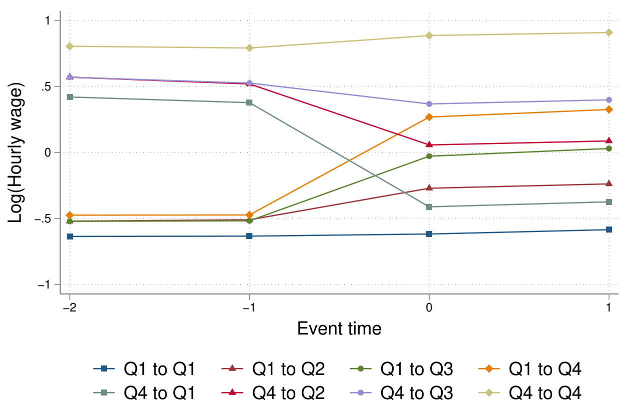
Figure D.4: AKM Event Studies for Job Switchers



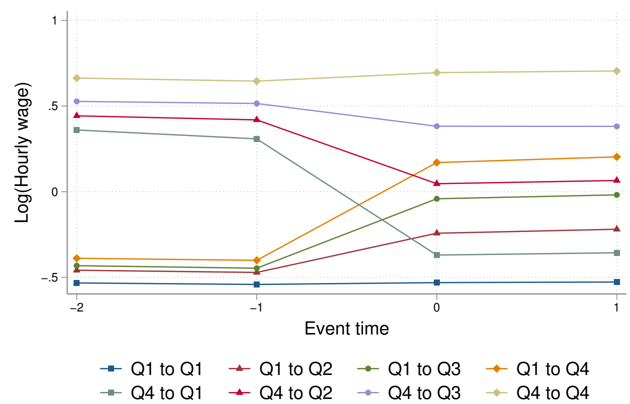
(a) 1997-2001



(b) 2001-2005



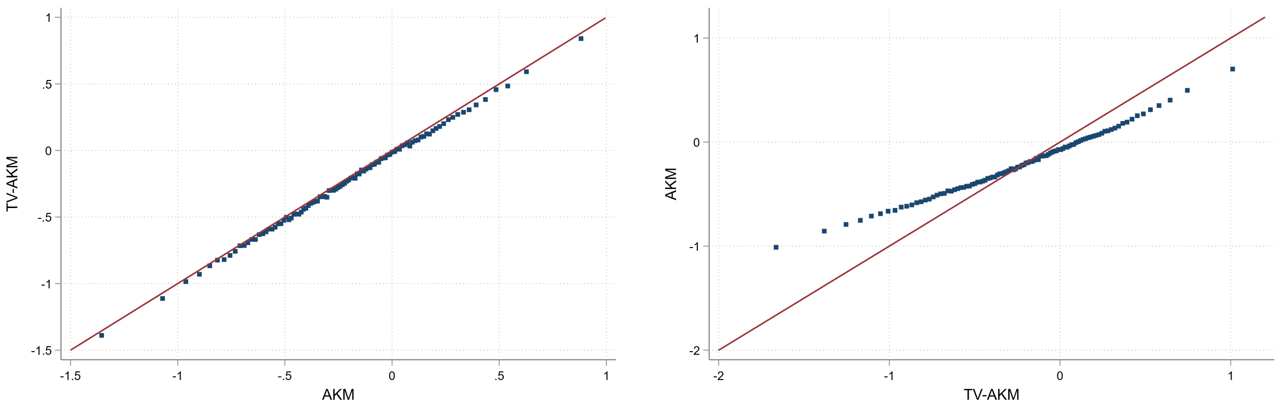
(c) 2005-2009



(d) 2009-2013

Notes: This figure plots "event studies" of log hourly wages of switchers around job transitions as in Card et al. (2013) and Card et al. (2018). We define a switcher within a 5-year period as a worker who stays for (at least) 2 consecutive years in a firm and switches to a different firm and stays for (at least) 2 consecutive years. We compute employment-weighted quartiles of firms based on the hourly wages of switchers' coworkers and plot the residualized (against year fixed effects and age controls) log hourly wage of switchers that experience transitions between firms in different quartiles.

Figure D.5: Projections Between AKM F-FEs and TV-AKM F-FEs

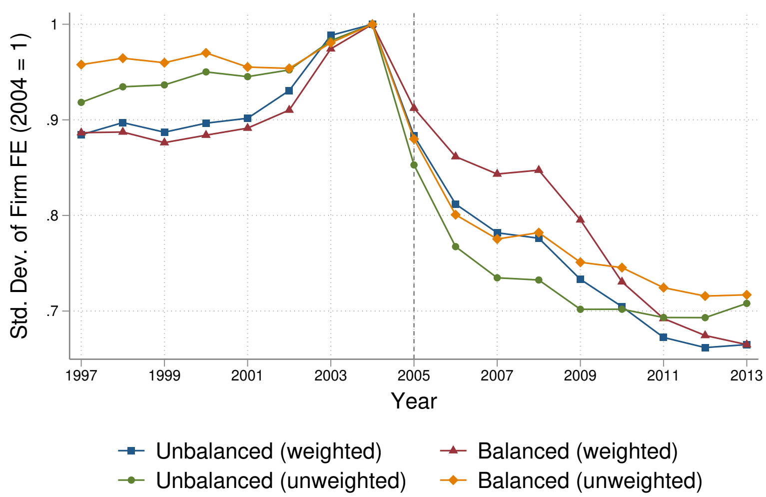


(a) TV-AKM into AKM

(b) AKM into TV-AKM

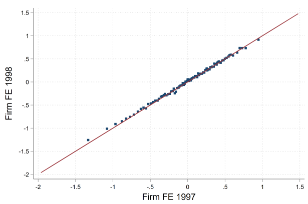
Notes: This figure plots binned scatter plots of the firm fixed effects estimated with the TV-AKM model against against the firm fixed effects estimated with the AKM model (Panel (a)) and vice-versa (Panel (b)).

Figure D.6: Standard Deviation of TV-AKM F-FEs

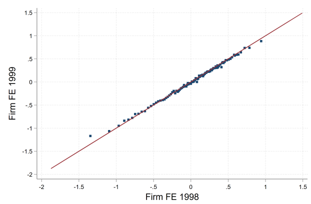


Notes: This figure plots the year-by-year standard deviation of the firm fixed effects estimated with the TV-AKM model. All four series are normalized, so the standard deviation in 2004 is equal to one. The blue series plots the employment-weighted annual standard deviation considering the full (unbalanced) sample of firms. The red series plots the employment-weighted annual standard deviation considering the balanced sample of firms that are observed in each of the 17 years. The green series plots the unweighted annual standard deviation considering the full (unbalanced) sample of firms. The yellow series plots the unweighted annual standard deviation considering the balanced sample of firms that are observed in each of the 17 years.

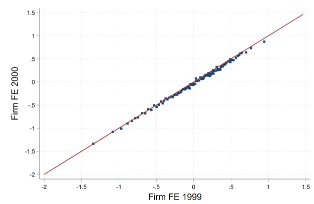
Figure D.7: Within-Firm Short-Run Persistence in F-FEs: All Years



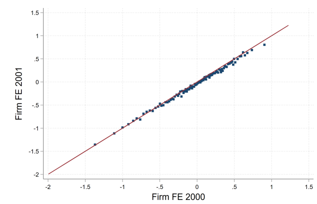
(a) 1997-1998



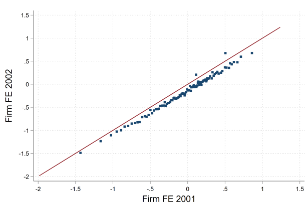
(b) 1998-1999



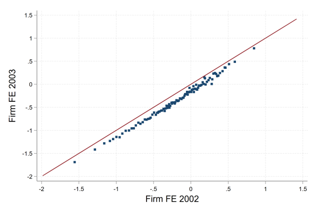
(c) 1999-2000



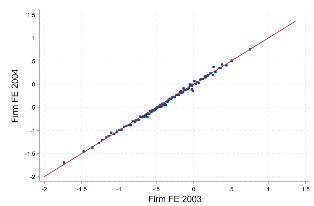
(d) 2000-2001



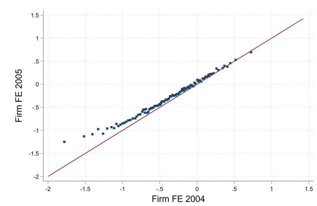
(e) 2001-2002



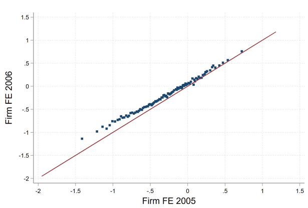
(f) 2002-2003



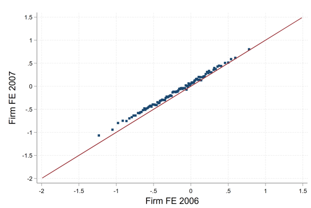
(g) 2003-2004



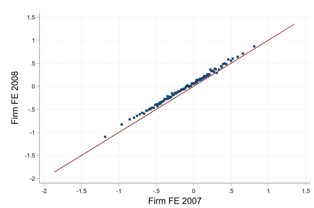
(h) 2004-2005



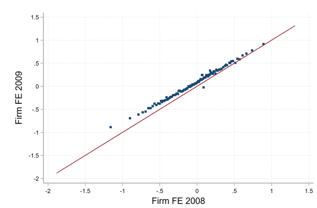
(i) 2005-2006



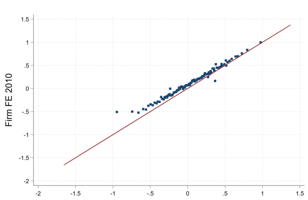
(j) 2006-2007



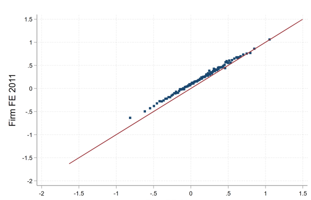
(k) 2007-2008



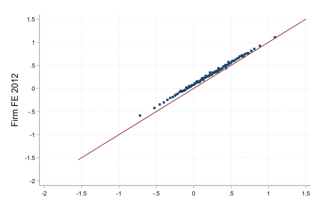
(l) 2008-2009



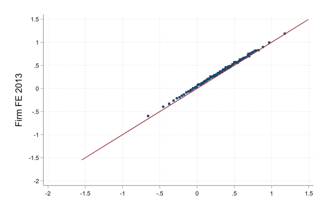
(m) 2009-2010



(n) 2010-2011



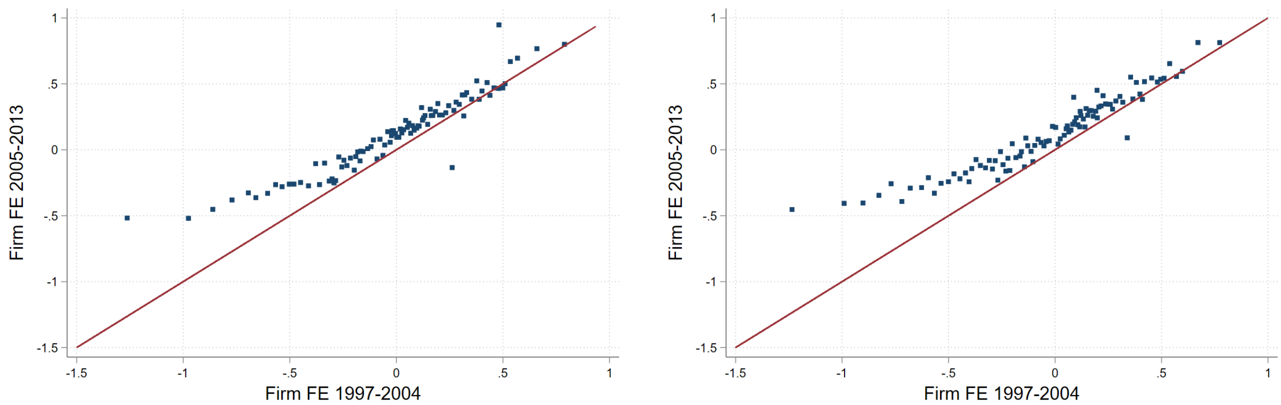
(o) 2011-2012



(p) 2012-2013

Notes: This figure presents binned scatter plots of the firm fixed effects (F-FEs) estimated with the TV-AKM model in year t against the F-FEs estimates in year $t-1$ for all pairs of consecutive years between 1997 and 2013. Observations are weighted by employment in year $t-1$.

Figure D.8: Long-Run Persistence in F-FEs: 1997-2004 vs 2005-2013, Robustness

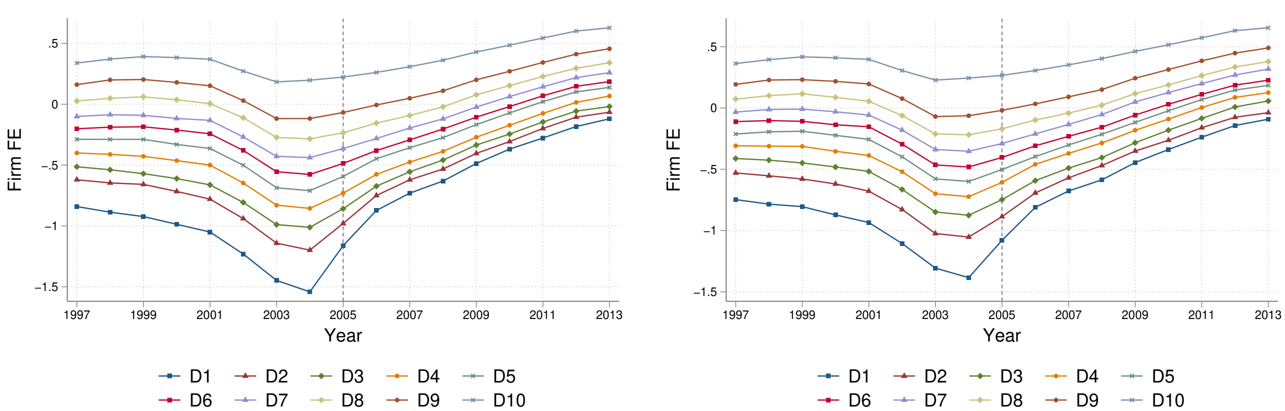


(a) No imputed hours

(b) Different firm size restriction

Notes: This figure presents a binned scatter plot of the average firm fixed effect (F-FEs) estimated with the TV-AKM model in years 2005-2013 against the average F-FEs estimates in years 1997-2004. Panel (a) uses the panel that excludes observations for which weekly hours are imputed. Panel (b) uses the panel that imposes a minimum firm size of 3 instead of 4. This figure only considers firms in the balanced panel (i.e., observed in our data in all 17 years between 1997 and 2013). Observations are weighted by the average employment in the years 1997-2004.

Figure D.9: Evolution of Average F-FE by Decile of F-FE in 2004

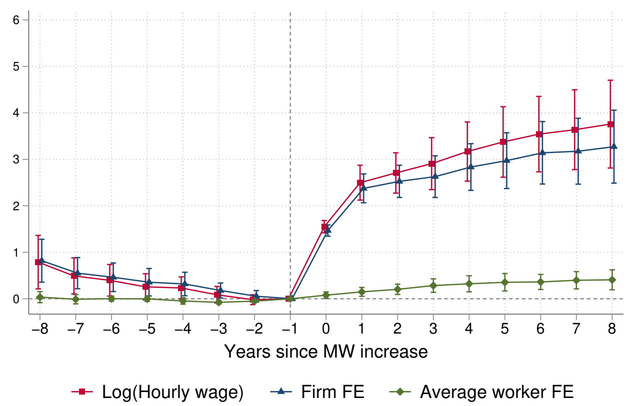


(a) Unbalanced panel

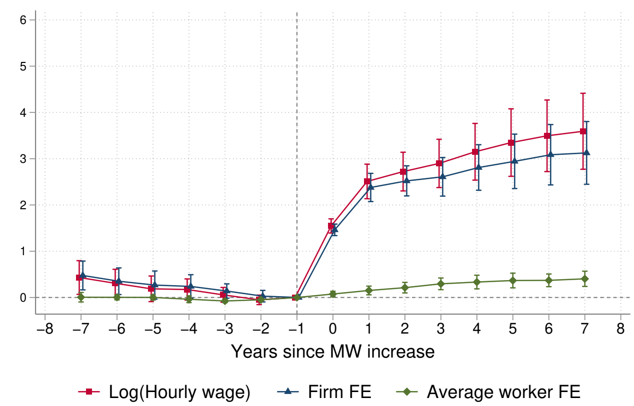
(b) Balanced panel

Notes: This figure plots the evolution of the firm fixed effect (F-FEs) estimated with the TV-AKM model by decile of F-FE in 2004. Deciles are determined based on the F-FE level in 2004 and are fixed for the rest of the periods. Panel (a) considers an unbalanced panel, meaning that firms are required to exist in 2004 but not in the other years. Panel (b) considers a balanced panel, meaning that firms are observed in our data in all 17 years between 1997 and 2013.

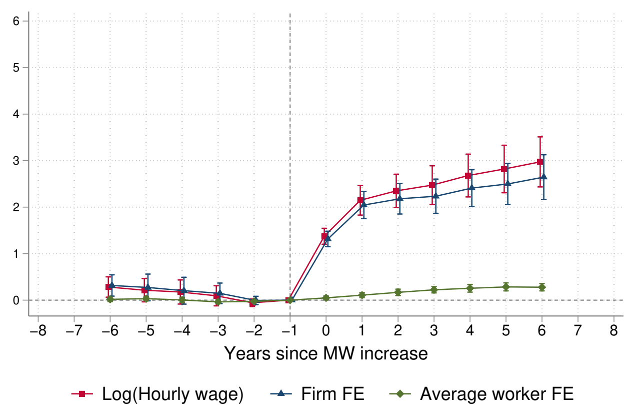
Figure D.10: Firm-Level Design: Different Balanced Samples



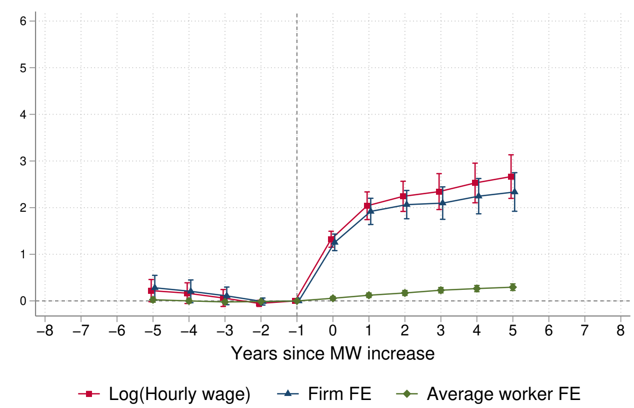
(a) Balanced panel 1997-2013



(b) Balanced panel 1998-2012



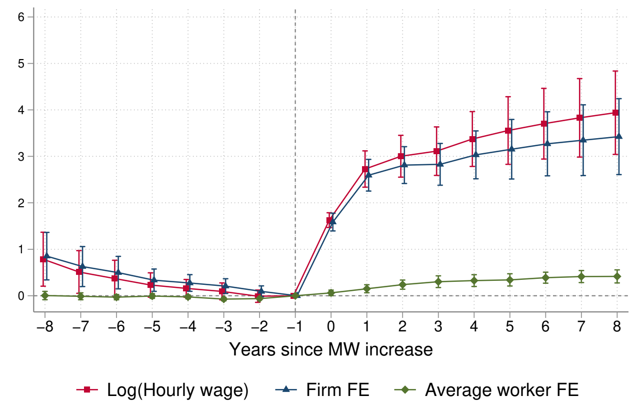
(c) Balanced panel 1999-2011



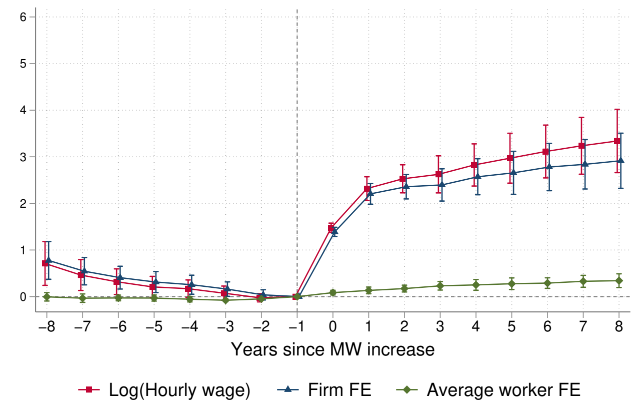
(d) Balanced panel 2000-2010

Notes: This figure plots the estimated event study coefficients β_t of the firm-level equation (8) with their corresponding 95% confidence intervals, for different balanced panels. Panel (a) considers a balanced panel of firms observed in our data in all 17 years between 1997 and 2013. Panel (b) considers a balanced panel of firms observed in our data in all 15 years between 1998 and 2012. Panel (c) considers a balanced panel of firms observed in our data in all 13 years between 1999 and 2011. Panel (d) considers a balanced panel of firms observed in our data in all 11 years between 2000 and 2010. Within each panel, each series corresponds to a different regression with a different dependent variable. The red series uses the hours-weighted average log real hourly wage at the firm level. The blue series uses the firm fixed effect (F-FEs) estimated with the TV-AKM model. The green series uses the hours-weighted firm-level average worker fixed effect estimated with the TV-AKM model. Standard errors are clustered at the 2-digit industry level.

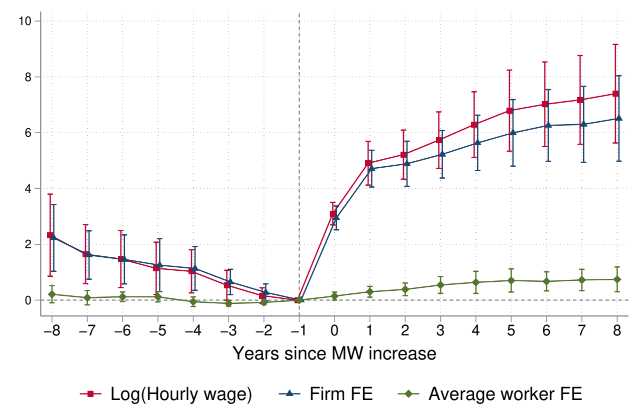
Figure D.11: Firm-Level Design: Additional Robustness Checks



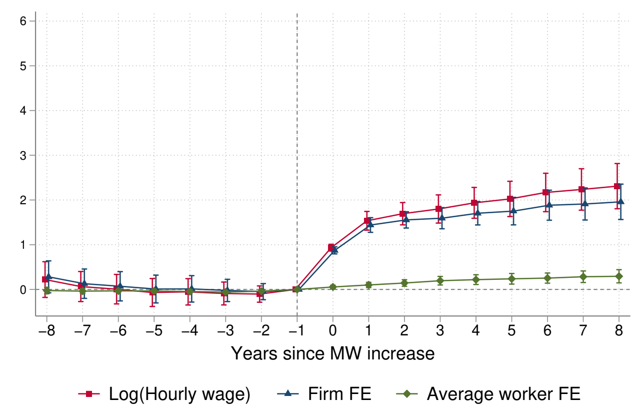
(a) Panel with no imputed hours



(b) Different firm size restriction



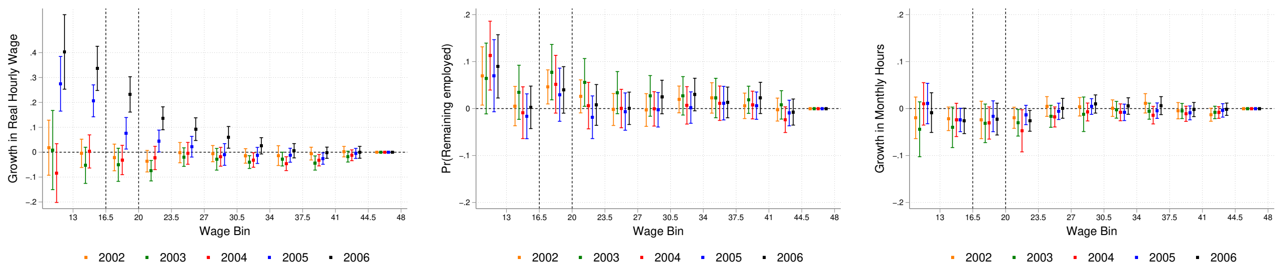
(c) GAP defined relative to NMW_{2005}



(d) GAP defined relative to $1.5{\cdot}NMW_{2005}$

Notes: This figure plots the estimated event study coefficients β_t of the firm-level equation (8) with their corresponding 95% confidence intervals, for different specifications. Panel (a) considers the yearly worker-level panel that drops observations for which weekly hours are imputed. Panel (b) considers the yearly worker-level panel that imposes a minimum firm size of 3 instead of 4. Panel (c) defines the GAP measure relative to NWM_{2005}. Panel (d) defines the GAP measure relative to $1.5 \cdot NWM_{2005}$. Within each panel, each series corresponds to a different regression with a different dependent variable. The red series uses the hours-weighted average log real hourly wage at the firm level. The blue series uses the firm fixed effect (F-FEs) estimated with the TV-AKM model. The green series uses the hours-weighted firm-level average worker fixed effect estimated with the TV-AKM model. Standard errors are clustered at the 2-digit industry level.

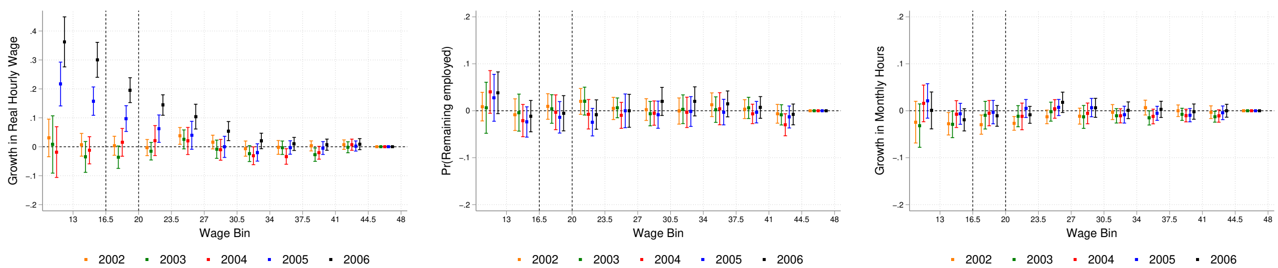
Figure D.12: Worker-Level Design: Hourly Wages, Employment, and Monthly Hours, Robustness Checks



(a) Hourly wage growth (panel with no imputed hours)

(b) Employment probability (panel with no imputed hours)

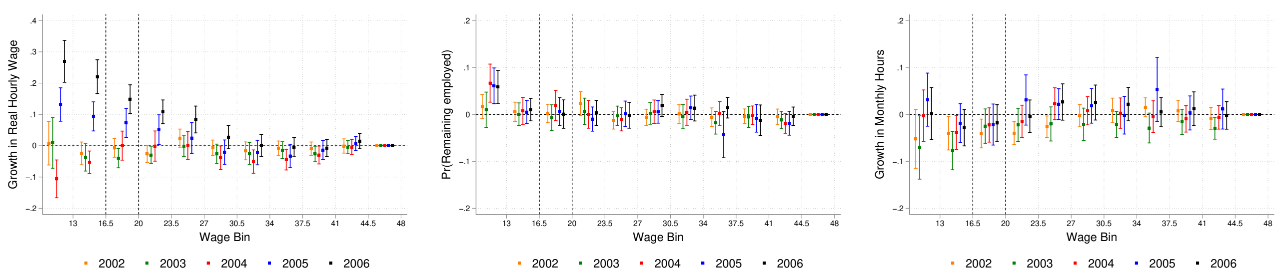
(c) Monthly hours (panel with no imputed hours)



(d) Hourly wage growth (different firm size restriction)

(e) Employment probability (different firm size restriction)

(f) Monthly hours (different firm size restriction)



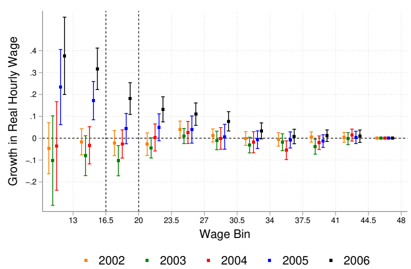
(g) Hourly wage growth (no AKM restrictions)

(h) Employment probability (no AKM restrictions)

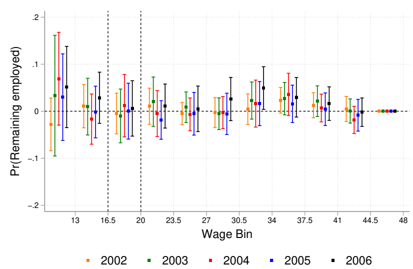
(i) Monthly hours (no AKM restrictions)

Notes: This figure plots the estimated event study coefficients β_{kt} of the worker-level equation (10) with their corresponding 95% confidence intervals. Panels (a), (b) and (c) consider the yearly worker-level panel that drops observations for which weekly hours are imputed. Panels (d), (e), and (f) consider the yearly worker-level panel that imposes a minimum firm size of 3 instead of 4. Panels (g), (h), and (i) consider the yearly worker-level panel before imposing the AKM restrictions. Within each panel, the horizontal axis accounts for different wage bins (k), and each series represents a different year (t). Each panel uses a different dependent variable. Panels (a), (d), and (g) use the 2-year hourly wage growth. Panels (b), (e), and (h) use the 2-year change in employment status. Panels (c), (f), and (i) use the 2-year change in growth in monthly hours. All regressions control for a vector of baseline characteristics, including gender, age, tenure, firm size, and firm industry (5-digit ISIC codes). Standard errors are clustered at the (lagged) industry level.

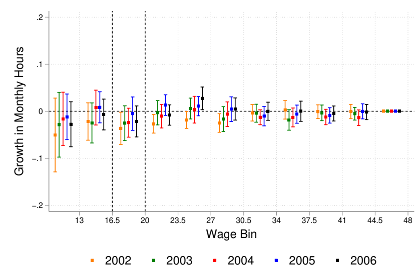
Figure D.13: Worker-Level Design: Hourly Wages, Employment, and Monthly Hours, Gender Heterogeneity



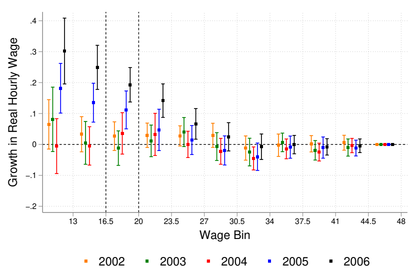
(a) Hourly wage growth (males)



(b) Employment probability (males)

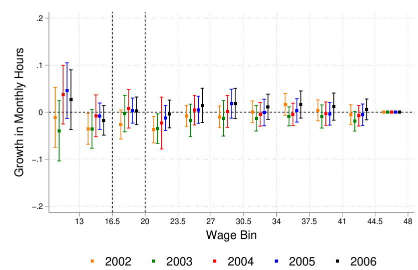


(c) Monthly hours (males)



(d) Hourly wage growth (females)

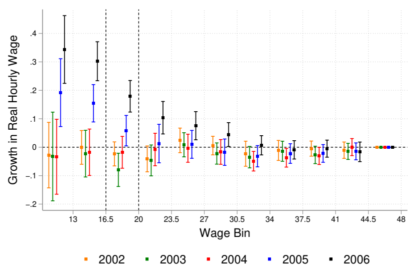
(e) Employment probability (females)



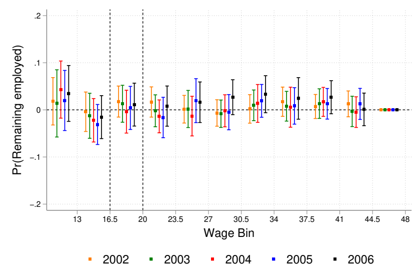
(f) Monthly hours (females)

Notes: This figure plots the estimated event study coefficients β_{kt} of the worker-level equation (10) with their corresponding 95% confidence intervals. Panels (a), (b) and (c) restrict the sample to male workers. Panels (d), (e), and (f) restrict the sample to female workers. Within each panel, the horizontal axis accounts for different wage bins (k), and each series represents a different year (t). Each panel uses a different dependent variable. Panels (a) and (d) use the 2-year hourly wage growth. Panels (b) and (e) use the 2-year change in employment status. Panels (c) and (f) use the 2-year change in growth in monthly hours. All regressions control for a vector of baseline characteristics, including gender, age, tenure, firm size, and firm industry (5-digit ISIC codes). Standard errors are clustered at the (lagged) industry level.

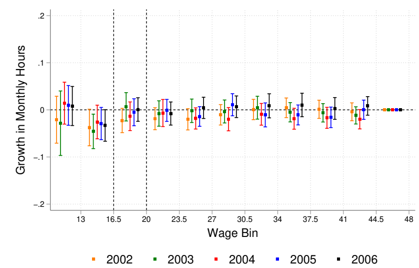
Figure D.14: Worker-Level Design: Hourly Wages, Employment, and Monthly Hours, Age Heterogeneity



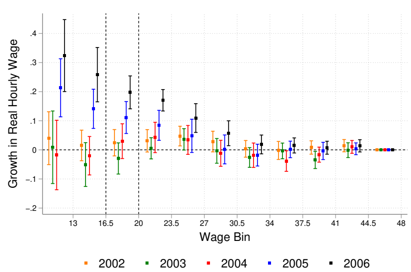
(a) Hourly wage growth (young)



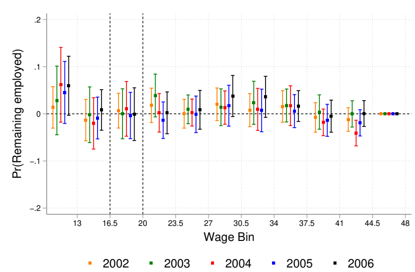
(b) Employment probability (young)



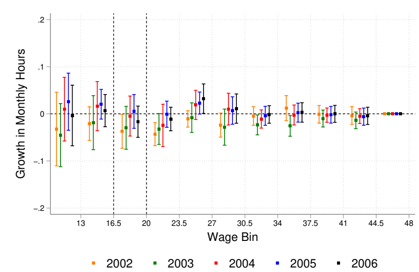
(c) Monthly hours (young)



(d) Hourly wage growth (old)



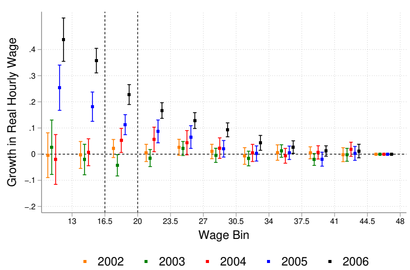
(e) Employment probability (old)



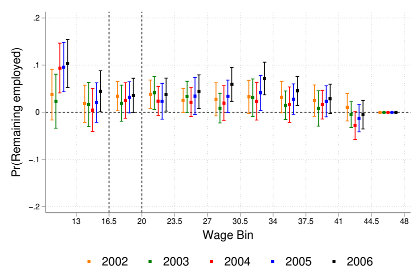
(f) Monthly hours (old)

Notes: This figure plots the estimated event study coefficients β_{kt} of the worker-level equation (10) with their corresponding 95% confidence intervals. Panels (a), (b) and (c) restrict the sample to "young workers" (aged 30 or less in $t-2$). Panels (d), (e), and (f) restrict the sample to "old workers" (aged 30 or more in $t-2$). Within each panel, the horizontal axis accounts for different wage bins (k), and each series represents a different year (t). Each panel uses a different dependent variable. Panels (a) and (d) use the 2-year hourly wage growth. Panels (b) and (e) use the 2-year change in employment status. Panels (c) and (f) use the 2-year change in growth in monthly hours. All regressions control for a vector of baseline characteristics, including gender, age, tenure, firm size, and firm industry (5-digit ISIC codes). Standard errors are clustered at the (lagged) industry level.

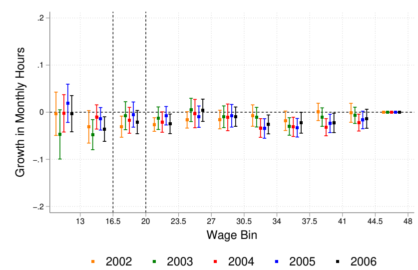
Figure D.15: Worker-Level Design: Hourly Wages, Employment, and Monthly Hours, Firm Size Heterogeneity



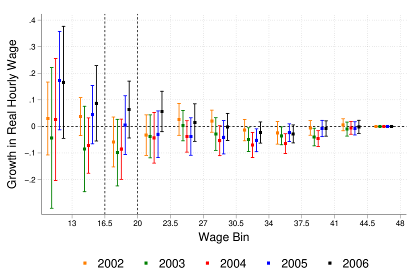
(a) Hourly wage growth (small firm)



(b) Employment probability (small firm)

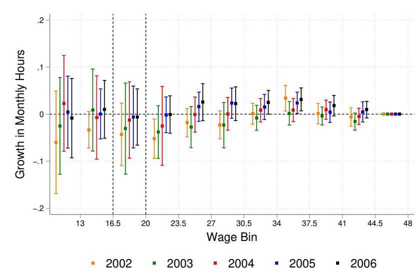


(c) Monthly hours (small firm)



(d) Hourly wage growth (large firm)

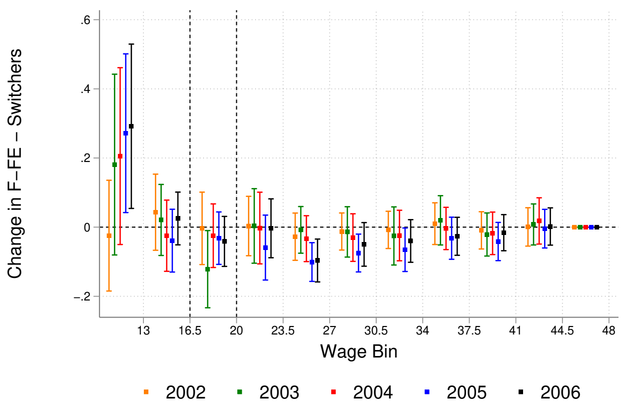
(e) Employment probability (large firm)



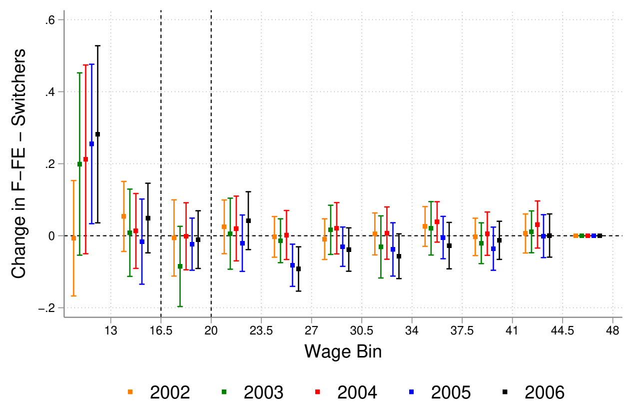
(f) Monthly hours (large firm)

Notes: This figure plots the estimated event study coefficients β_{kt} of the worker-level equation (10) with their corresponding 95% confidence intervals. Panels (a), (b) and (c) restrict the sample to workers in "small firms" (working in firms with 25 or less valid worker observations in $t-2$). Panels (d), (e), and (f) restrict the sample to workers in "large firms" (working in firms with 26 or more valid worker observations in $t-2$). Within each panel, the horizontal axis accounts for different wage bins (k), and each series represents a different year (t). Each panel uses a different dependent variable. Panels (a) and (d) use the 2-year hourly wage growth. Panels (b) and (e) use the 2-year change in employment status. Panels (c) and (f) use the 2-year change in growth in monthly hours. All regressions control for a vector of baseline characteristics, including gender, age, tenure, firm size, and firm industry (5-digit ISIC codes). Standard errors are clustered at the (lagged) industry level.

Figure D.16: Reallocation Effects: Different Definitions



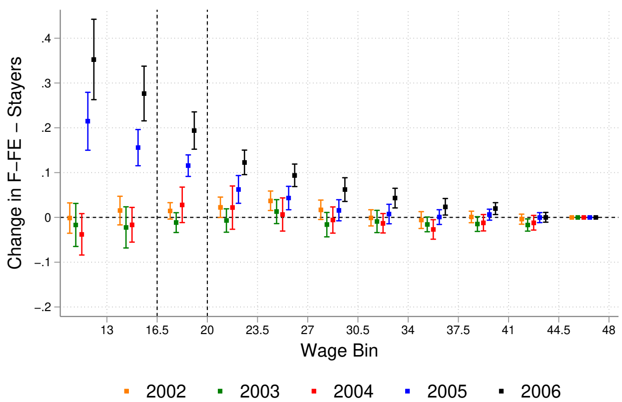
(a) Change in contemporaneous TV-AKM F-FE (switchers)



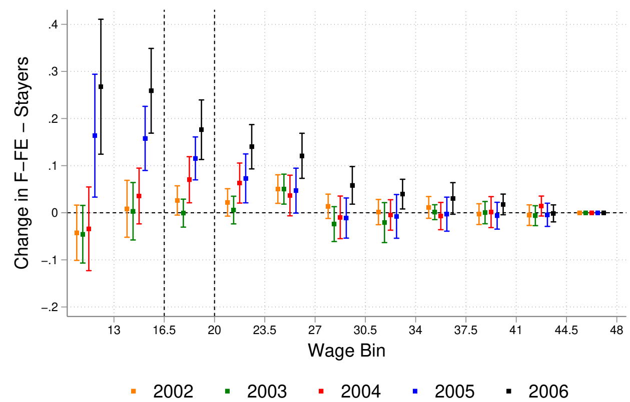
(b) Change in average TV-AKM F-FE of the period 1997-2004 (switchers)

Notes: This figure plots the estimated event study coefficients β_{kt} of the worker-level equation (10) with their corresponding 95% confidence intervals. Panel (a) uses the change in the firm fixed effect (F-FE) estimated with the TV-AKM model for populations of job switchers. Panel (b) uses the change in the average F-FE estimated with the TV-AKM model for the period 1997-2004 for populations of job switchers. All regressions control for a vector of baseline characteristics, including gender, age, tenure, firm size, and firm industry (5-digit ISIC codes). Standard errors are clustered at the (lagged) industry level.

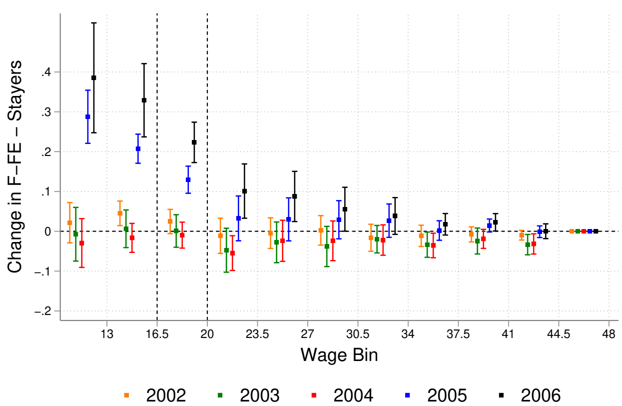
Figure D.17: Worker-Level Design: Change in TV-AKM F-FE (stayers), Different Exposure to CBAs



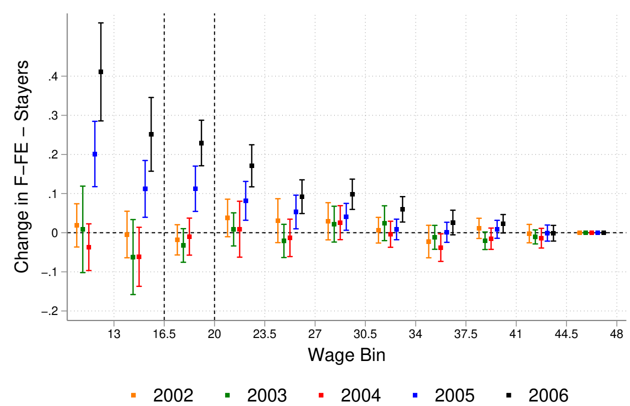
(a) All industries



(b) Tercile 1 (lowest exposure)



(c) Tercile 2 (medium exposure)



(d) Tercile 3 (highest exposure)

Notes: This figure plots the estimated event study coefficients β_{kt} of the worker-level equation (10) with their corresponding 95% confidence intervals using the processed yearly panel described in Section 3 (Panel (c) of Table 1). Within each panel, the horizontal axis accounts for different wage bins (k), and each series represents a different year (t). All panels use the change in the firm fixed effect (F-FEs) estimated with the TV-AKM model for populations of job stayers as a dependent variable. Each panel estimates the main regression for a different group of observations, based on firm-level exposure to CBAs, which is proxied by industry codes as explained in footnote 19. Panel (a) uses the whole sample of workers for which we can build the exposure measure (92% of the baseline sample of job switchers). Panels (b), (c), and (d) consider the bottom, middle, and top terciles of workers based on the CBA exposure of the firms they work on. All regressions control for a vector of baseline characteristics, including gender, age, tenure, firm size, and firm industry (5-digit ISIC codes). Standard errors are clustered at the (lagged) industry level.

Table D.1: Descriptive Statistics: Panel With No Imputed Hours

	Observations	Workers	Firms	log Wage Mean	log Wage SD	Age Mean	Age SD	Male Mean
Panel (a) Yearly Panel								
1997-2001	1,983,795	679,405	39,922	4.28	0.83	35.63	12.34	0.60
2001-2005	1,974,993	710,796	43,312	4.02	0.86	36.85	12.69	0.60
2005-2009	2,723,771	950,873	55,263	4.15	0.76	36.26	12.70	0.59
2009-2013	3,491,196	1,119,165	57,815	4.43	0.74	36.24	12.68	0.56
All years	8,688,600	1,565,048	100,437	4.26	0.80	36.22	12.62	0.58
Panel (b) Yearly Panel + Restrictions								
1997-2001	1,266,563	432,323	15,864	4.42	0.75	36.12	10.45	0.58
2001-2005	1,240,011	435,275	16,043	4.15	0.79	36.80	10.44	0.58
2005-2009	1,639,824	579,495	19,698	4.24	0.70	36.45	10.52	0.58
2009-2013	2,195,092	716,641	22,214	4.51	0.65	36.48	10.57	0.55
All years	4,528,185	922,691	26,707	4.39	0.72	36.21	10.17	0.56
Panel (c) Yearly Panel + Restrictions + LCS								
1997-2001	1,203,885	405,799	12,539	4.44	0.75	36.04	10.44	0.58
2001-2005	1,174,365	410,725	12,912	4.17	0.79	36.70	10.44	0.58
2005-2009	1,603,963	563,662	17,482	4.25	0.70	36.39	10.52	0.58
2009-2013	2,168,876	706,384	20,677	4.51	0.65	36.44	10.57	0.55
All years	4,521,221	919,642	26,151	4.39	0.72	36.20	10.17	0.56

Notes: This table presents descriptive statistics of the yearly worker-level panel built from the raw monthly administrative records of the SSA, excluding the observations for which weekly hours are imputed. Panel (a) displays statistics for the baseline yearly panel, where each observation reports annual outcomes for the primary employer, defined as the employer that had larger earnings. Panel (b) displays statistics for the yearly panel after imposing restrictions on age, labor market attachment, outliers, and firm size. Panel (c) further restricts the sample to firms belonging to the largest connected set (LCS). SD accounts for Standard Deviation. See Section 3 and Appendix A.3 for additional details on the panel construction.

Table D.2: Descriptive Statistics: Different Firm Size Restriction

	Observations	Workers	Firms	log Wage Mean	log Wage SD	Age Mean	Age SD	Male Mean
Panel (a) Yearly Panel								
1997-2001	2,581,830	828,988	48,252	4.15	0.84	35.94	13.10	0.61
2001-2005	2,424,552	821,661	48,853	3.94	0.85	36.92	12.90	0.60
2005-2009	3,238,379	1,036,156	60,957	4.09	0.74	36.39	12.74	0.60
2009-2013	3,953,293	1,216,746	66,686	4.42	0.71	36.35	12.64	0.59
All years	10,434,988	1,690,771	114,817	4.19	0.80	36.37	12.82	0.60
Panel (b) Yearly Panel + Restrictions								
1997-2001	1,762,974	569,359	25,197	4.27	0.79	36.16	10.50	0.60
2001-2005	1,677,334	561,606	25,635	4.04	0.81	36.75	10.48	0.58
2005-2009	2,155,478	707,990	30,209	4.17	0.70	36.53	10.55	0.59
2009-2013	2,634,815	842,224	33,163	4.48	0.63	36.61	10.58	0.58
All years	6,191,948	1,127,595	45,576	4.30	0.73	36.22	10.18	0.58
Panel (c) Yearly Panel + Restrictions + LCS								
1997-2001	1,700,445	547,841	21,484	4.29	0.79	36.11	10.50	0.60
2001-2005	1,609,901	536,358	21,238	4.06	0.81	36.68	10.48	0.58
2005-2009	2,114,221	691,197	27,130	4.18	0.70	36.47	10.54	0.59
2009-2013	2,602,112	829,145	30,562	4.48	0.63	36.56	10.58	0.58
All years	6,182,824	1,123,816	44,670	4.30	0.73	36.22	10.18	0.58

Notes: This table presents descriptive statistics of the yearly worker-level panel built from the raw monthly administrative records of the SSA, but imposing a minimum firm size of 3 instead of 4. Panel (a) displays statistics for the baseline yearly panel, where each observation reports annual outcomes for the primary employer, defined as the employer that had larger earnings. Panel (b) displays statistics for the yearly panel after imposing restrictions on age, labor market attachment, outliers, and firm size. Panel (c) further restricts the sample to firms belonging to the largest connected set (LCS). SD accounts for Standard Deviation. See Section 3 and Appendix A.3 for additional details on the panel construction.

Table D.3: Comparison of the Largest Connected Set and the Leave-One-Out Largest Connected Set

	Observations	Workers	Firms	log Wage Mean	log Wage SD	Age Mean	Age SD	Male Mean
Panel (a) Yearly Panel + Restrictions + LCS								
1997-2001	1,641,416	529,508	17,457	4.32	0.78	36.15	10.49	0.60
2001-2005	1,553,646	518,191	17,295	4.09	0.80	36.71	10.47	0.58
2005-2009	2,042,785	669,955	21,905	4.19	0.70	36.48	10.54	0.59
2009-2013	2,524,680	807,331	24,821	4.49	0.63	36.57	10.57	0.58
Panel (b) Yearly Panel + Restrictions + Leave-one-out LCS								
1997-2001	1,450,632	384,169	14,706	4.38	0.78	36.42	10.40	0.60
2001-2005	1,349,578	359,441	14,322	4.14	0.80	37.05	10.36	0.58
2005-2009	1,840,496	496,438	19,560	4.23	0.70	36.75	10.43	0.59
2009-2013	2,297,166	607,934	22,549	4.52	0.63	36.79	10.45	0.58

Notes: This table presents descriptive statistics of the yearly worker-level panel built from the raw monthly administrative records of the SSA. Panel (a) displays statistics for the baseline yearly panel, where each observation reports annual outcomes for the primary employer of the worker, defined as the employer that had larger earnings, after imposing restrictions on age, labor market attachment, outliers, firm size, and further restricting the sample to firms belonging to the largest connected set (LCS). Panel (b) furthermore restricts the sample to firms belonging to the leave-one-out connected set as defined in Kline et al. (2020). SD accounts for Standard Deviation. See Section 3 and Appendix A.3 for additional details on the panel construction.

Table D.4: Bias-Corrected Variance Decomposition: Monthly Earnings

	1997-2001		2001-2005		2005-2009		2009-2013	
	Level	%	Level	%	Level	%	Level	%
Variance of Log Monthly Earnings	0.625	100	0.679	100	0.538	100	0.442	100
Std. Dev.	0.790		0.824		0.733		0.665	
Panel (a) KSS (Bias corrected)								
$\mathbb{V}(\alpha_i)$	0.206	32.9	0.231	34.0	0.190	35.3	0.179	40.4
Std. Dev.	0.454		0.481		0.436		0.423	
$\mathbb{V}(\psi_j)$	0.141	22.7	0.184	27.1	0.100	18.5	0.062	14.1
Std. Dev.	0.376		0.429		0.316		0.250	
$2 \cdot \mathbb{C}(\alpha_i, \psi_j)$	0.176	28.1	0.138	20.3	0.131	24.3	0.099	22.3
Share explained		83.7		81.4		78.2		76.7
Panel (b) Plug-in (Naive)								
$\mathbb{V}(\alpha_i)$	0.291	46.6	0.293	43.1	0.258	48.0	0.234	52.9
Std. Dev.	0.539		0.541		0.508		0.484	
$\mathbb{V}(\psi_j)$	0.199	31.9	0.222	32.6	0.135	25.1	0.086	19.5
Std. Dev.	0.446		0.471		0.367		0.294	
$2 \cdot \mathbb{C}(\alpha_i, \psi_j)$	0.066	10.6	0.067	9.8	0.066	12.2	0.055	12.4
Share explained		89.1		85.5		85.3		84.8
N Movers	106,364		97,629		168,667		217,186	
N Firms	14,706		14,322		19,560		22,549	
Movers/Firms	7.23		6.82		8.62		9.63	

Notes: This table presents AKM variance decompositions of log monthly earnings. The AKM model is estimated separately for 5-year overlapping intervals –1997-2001, 2001-2005, 2005-2009, and 2009-2013– using the processed yearly panel described in Section 3 (Panel (c) of Table 1). Panel (a) shows bias-corrected variance decompositions using the leave-one-out method of Kline et al. (2020). Panel (b) shows naive plug-in decompositions that are contaminated by limited mobility bias. Each panel shows results in terms of variances, standard deviations, and share explained. N Movers is the number of movers used to identify the firm fixed effects. N Firms is the number of firms. Movers/Firms is the ratio between the two.

Table D.5: Bias-Corrected Variance Decomposition: Log Hourly Wages, Panel With No Imputed Hours

	1997-2001		2001-2005		2005-2009		2009-2013	
	Level	%	Level	%	Level	%	Level	%
Variance of Log Hourly Wages	0.540	100	0.623	100	0.497	100	0.420	100
Std. Dev.	0.735		0.789		0.705		0.648	
Panel (a) KSS (Bias corrected)								
$\mathbb{V}(\alpha_i)$	0.193	35.8	0.211	33.9	0.198	39.8	0.190	45.2
Std. Dev	0.439		0.460		0.445		0.436	
$\mathbb{V}(\psi_j)$	0.144	26.7	0.175	28.1	0.105	21.2	0.076	18.0
Std. Dev.	0.379		0.418		0.325		0.275	
$2 \cdot \mathbb{C}(\alpha_i, \psi_j)$	0.110	20.4	0.119	19.1	0.093	18.8	0.067	16.0
Share explained		82.9		81.1		79.7		79.2
Panel (b) Plug-in (Naive)								
$\mathbb{V}(\alpha_i)$	0.250	46.3	0.260	41.8	0.239	48.1	0.222	53.0
Std. Dev.	0.500		0.510		0.489		0.472	
$\mathbb{V}(\psi_j)$	0.183	34.0	0.208	33.4	0.128	25.8	0.090	21.5
Std. Dev.	0.428		0.456		0.358		0.301	
$2 \cdot \mathbb{C}(\alpha_i, \psi_j)$	0.035	6.4	0.055	8.9	0.050	10.1	0.040	9.6
Share explained		86.7		84.1		84.1		84.1
N Movers	66,644		62,799		115,150		160,382	
N Firms	9,943		10,148		14,927		18,405	
Movers/Firms	6.70		6.19		7.71		8.71	

Notes: This table presents AKM variance decompositions of log hourly wages. The AKM model is estimated separately for 5-year overlapping intervals –1997-2001, 2001-2005, 2005-2009, and 2009-2013– using the processed yearly panel described in Section 3 that excludes the observations for which weekly hours are imputed (Panel (c) of Table D.1). Panel (a) shows bias-corrected variance decompositions using the leave-one-out method of Kline et al. (2020). Panel (b) shows naive plug-in decompositions that are contaminated by limited mobility bias. Each panel shows results in terms of variances, standard deviations, and share explained. N Movers is the number of movers used to identify the firm fixed effects. N Firms is the number of firms. Movers/Firms is the ratio between the two.

Table D.6: Bias-Corrected Variance Decomposition: Log Hourly Wages, Different Firm Size Restriction

	1997-2001		2001-2005		2005-2009		2009-2013	
	Level	%	Level	%	Level	%	Level	%
Variance of Log Hourly Wages	0.608	100	0.652	100	0.495	100	0.395	100
Std. Dev.	0.780		0.807		0.703		0.628	
Panel (a) KSS (Bias corrected)								
$\mathbb{V}(\alpha_i)$	0.216	35.5	0.226	34.7	0.192	38.8	0.172	43.5
Std. Dev	0.465		0.475		0.438		0.415	
$\mathbb{V}(\psi_j)$	0.184	30.2	0.198	30.3	0.112	22.7	0.075	18.9
Std. Dev.	0.429		0.445		0.335		0.273	
$2 \cdot \mathbb{C}(\alpha_i, \psi_j)$	0.115	18.9	0.105	16.1	0.086	17.3	0.061	15.4
Share explained		84.7		81.0		78.8		77.8
Panel (b) Plug-in (Naive)								
$\mathbb{V}(\alpha_i)$	0.272	44.7	0.275	42.2	0.236	47.8	0.205	51.9
Std. Dev.	0.522		0.524		0.486		0.453	
$\mathbb{V}(\psi_j)$	0.218	35.8	0.226	34.7	0.133	26.9	0.088	22.2
Std. Dev.	0.467		0.475		0.365		0.296	
$2 \cdot \mathbb{C}(\alpha_i, \psi_j)$	0.051	8.4	0.052	8.0	0.047	9.6	0.037	9.4
Share explained		88.9		84.8		84.3		83.5
N Movers	114,873		105,254		180,217		230,179	
N Firms	17,409		16,821		23,363		26,727	
Movers/Firms	6.60		6.26		7.71		8.61	

Notes: This table presents AKM variance decompositions of log hourly wages. The AKM model is estimated separately for 5-year overlapping intervals –1997-2001, 2001-2005, 2005-2009, and 2009-2013– using the processed yearly panel described in Section 3 that imposes a minimum firm size of 3 instead of 4. (Panel (c) of Table D.2). Panel (a) shows bias-corrected variance decompositions using the leave-one-out method of Kline et al. (2020). Panel (b) shows naive plug-in decompositions that are contaminated by limited mobility bias. Each panel shows results in terms of variances, standard deviations, and share explained. N Movers is the number of movers used to identify the firm fixed effects. N Firms is the number of firms. Movers/Firms is the ratio between the two.

Table D.7: Bias-Corrected Variance Decomposition: Monthly Hours

	1997-2001		2001-2005		2005-2009		2009-2013	
	Level	%	Level	%	Level	%	Level	%
Variance of Log Monthly Hours	0.073	100	0.077	100	0.074	100	0.079	100
Std. Dev.	0.271		0.277		0.272		0.282	
Panel (a) KSS (Bias corrected)								
$\mathbb{V}(\alpha_i)$	0.023	30.9	0.027	34.8	0.022	30.3	0.025	31.7
Std. Dev.	0.151		0.164		0.150		0.158	
$\mathbb{V}(\psi_j)$	0.022	30.3	0.025	32.8	0.022	30.0	0.025	31.7
Std. Dev.	0.149		0.159		0.149		0.158	
$2 \cdot \mathbb{C}(\alpha_i, \psi_j)$	0.008	11.1	0.004	4.8	0.005	6.4	0.005	6.2
Share explained		72.4		72.4		66.7		69.6
Panel (b) Plug-in (Naive)								
$\mathbb{V}(\alpha_i)$	0.043	58.5	0.046	59.9	0.041	56.1	0.043	54.0
Std. Dev.	0.207		0.215		0.203		0.207	
$\mathbb{V}(\psi_j)$	0.035	48.0	0.037	48.2	0.032	43.2	0.033	41.8
Std. Dev.	0.188		0.192		0.178		0.182	
$2 \cdot \mathbb{C}(\alpha_i, \psi_j)$	-0.016	-22.2	-0.018	-23.9	-0.013	-17.6	-0.009	-11.8
Share explained		84.4		84.2		81.7		84.1
N Movers	106,364		97,629		168,667		217,186	
N Firms	14,706		14,322		19,560		22,549	
Movers/Firms	7.23		6.82		8.62		9.63	

Notes: This table presents AKM variance decompositions of log monthly hours. The AKM model is estimated separately for 5-year overlapping intervals –1997-2001, 2001-2005, 2005-2009, and 2009-2013– using the processed yearly panel described in Section 3 (Panel (c) of Table 1). Panel (a) shows bias-corrected variance decompositions using the leave-one-out method of Kline et al. (2020). Panel (b) shows naive plug-in decompositions that are contaminated by limited mobility bias. Each panel shows results in terms of variances, standard deviations, and share explained. N Movers is the number of movers used to identify the firm fixed effects. N Firms is the number of firms. Movers/Firms is the ratio between the two.

Table D.8: Bias-Corrected Variance Decomposition: Distributional AKM

	1997-2001		2001-2005		2005-2009		2009-2013	
	Level	%	Level	%	Level	%	Level	%
Panel (a) $1\{\log w \geq p_{90}\}$								
$\mathbb{V}(\alpha_i)$	0.064	65.2	0.061	62.0	0.058	60.1	0.060	63.1
$\mathbb{V}(\psi_j)$	0.005	5.6	0.006	6.4	0.007	6.8	0.005	5.3
$2 \cdot \mathbb{C}(\alpha_i, \psi_j)$	0.004	4.3	0.005	4.8	0.005	5.0	0.004	4.4
Share explained		75.1		73.2		71.8		72.8
Panel (b) $1\{\log w \geq p_{70}\}$								
$\mathbb{V}(\alpha_i)$	0.102	46.6	0.090	40.8	0.092	42.0	0.096	44.5
$\mathbb{V}(\psi_j)$	0.035	15.7	0.033	15.2	0.034	15.7	0.028	12.8
$2 \cdot \mathbb{C}(\alpha_i, \psi_j)$	0.027	12.5	0.027	12.2	0.020	9.4	0.020	9.1
Share explained		74.8		68.2		67.1		66.4
Panel (c) $1\{\log w \geq p_{50}\}$								
$\mathbb{V}(\alpha_i)$	0.085	34.1	0.072	28.8	0.086	34.4	0.090	36.0
$\mathbb{V}(\psi_j)$	0.068	27.4	0.075	30.3	0.066	26.3	0.058	23.1
$2 \cdot \mathbb{C}(\alpha_i, \psi_j)$	0.031	12.3	0.029	11.7	0.020	8.1	0.017	6.7
Share explained		73.9		70.7		68.7		65.8
Panel (d) $1\{\log w \geq p_{30}\}$								
$\mathbb{V}(\alpha_i)$	0.051	25.7	0.047	23.9	0.052	25.6	0.053	26.0
$\mathbb{V}(\psi_j)$	0.093	47.1	0.094	47.6	0.082	40.5	0.072	35.6
$2 \cdot \mathbb{C}(\alpha_i, \psi_j)$	0.007	3.6	0.005	2.3	-0.003	-1.5	-0.002	-1.1
Share explained		76.4		73.8		64.6		60.6
Panel (e) $1\{\log w \geq p_{10}\}$								
$\mathbb{V}(\alpha_i)$	0.019	25.0	0.019	23.7	0.020	24.3	0.020	24.2
$\mathbb{V}(\psi_j)$	0.043	55.5	0.033	41.5	0.027	33.2	0.031	37.3
$2 \cdot \mathbb{C}(\alpha_i, \psi_j)$	-0.006	-7.2	-0.004	-5.4	-0.007	-8.9	-0.010	-12.3
Share explained		73.3		59.9		48.6		49.3
N Movers	106,364		97,629		168,667		217,186	
N Firms	14,706		14,322		19,560		22,549	
Movers/Firms	7.23		6.82		8.62		9.63	

Notes: This table presents distributional AKM variance decompositions following equation (4) using different percentiles of the wage distribution. Each percentile is computed within the 5-year period. The distributional AKM model is estimated separately for 5-year overlapping intervals –1997-2001, 2001-2005, 2005-2009, and 2009-2013– using the processed yearly panel described in Section 3 (Panel (c) of Table 1). Variance decompositions are bias-corrected using the leave-one-out method of Kline et al. (2020). Panel (b) shows naive plug-in decompositions that are contaminated by limited mobility bias. Each panel shows results in terms of variances and share explained. N Movers is the number of movers used to identify the firm fixed effects. N Firms is the number of firms. Movers/Firms is the ratio between the two. Total variances and standard deviations are not reported as they are mechanically determined by the Bernoulli variance formula.

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