# The Role of Cash in Illegal Labor Market Practices: Evidence from Uruguay*

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**PRELIMINARY AND INCOMPLETE**

**Abstract**

This paper studies the effect of prohibiting the use of cash for wage disbursements on labor markets in developing countries. We study a reform in Uruguay that mandated wage payments to be disbursed using only electronic methods. Using a difference-in-differences approach based on sector-level cash intensity prior to the reform, our results indicate that firms in high cash intensity sectors are significantly more likely to discontinue formal activities post-reform. Active firms show a slight reduction in the number of employees and an increase in reported wages. These results are driven by low productivity firms. Complementary results using survey data indicate an increase in informal employment and a decrease in collusive underreporting of earnings partially explain these results. Overall, results suggest that, while eliminating cash for wage payments enhances tax compliance among formal workers, it may also shift some economic activity into full informality, offsetting the revenue gains from improved payroll tax compliance.

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# 1. Introduction

Developing countries often have low levels of financial inclusion, with a majority of the population relying on cash for most transactions and saving. In low- and middle-income countries, typically less than half of the population over 15 years of age owned a bank account by the year 2014 (Demirgüç-Kunt et al., 2022). The lower traceability of cash relative to electronic transactions has raised concerns in academic and policymaking circles alike, such as reduced transparency and the potential for fostering illegal activities such as tax evasion and organized crime (e.g. Rogoff, 2017; Gupta et al., 2017).

These concerns have lead to significant efforts from governments in developing countries to reduce the reliance on cash. Such efforts include, for example, giving access to free-of-charge bank accounts, debit cards, tax breaks for electronic purchases, and reducing cash circulation. The literature has analyzed effects on the take-up of electronic payment technology (Higgins, 2024; Crouzet et al., 2023), savings (Bachas et al., 2021), overall economic activity (Chodorow-Reich et al., 2020), and compliance with value-added taxes (Brockmeyer and Sáenz Somarriba, 2022; Das et al., 2023).[1] However, there is little evidence on how these measures specifically affect the functioning of labor markets, which usually display several forms of non-compliance with taxes and regulations that could be affected by the use of cash. For instance, informal employment could potentially thrive in an environment with widespread use of hard-to-trace cash transactions, and recent research has found cash to be a primary method for disbursing unreported earnings for formal workers (Feinmann et al., 2024). However, the effect of cash use on labor markets is unclear *a priori*: while an increased use of electronic transactions can improve traceability, a large share of economic activity operates informally beyond regulatory oversight.

In this paper, we study the labor market effects of prohibiting the use of cash for wage disbursements. Specifically, we study a 2015 reform in Uruguay in which the government mandated all disbursements of wage payments to be made using any form of electronic payment (e.g. bank transfer, electronic wallets, pre-paid cards, etc). This was part of a broader effort from the Uruguayan government to increase the levels of digitization and formalization, especially since Uruguay's levels of financial inclusion were significantly lower than those of countries of similar levels of income.[2]

[1] We indeed document in the data that sectors with higher shares of workers paid in cash have more informality and more formal workers who admit to underreporting their labor earnings for tax purposes. See appendix figure A.III.

[2] The reform also incorporated other measures to boost financial inclusion, such as VAT rebates for debit card purchases and a mandate to disburse pensions and other social assistance programs with electronic methods, among others. We describe the institutional context in more detail in section 2. See the official government website for a discussion of the several goals of the reform, https://inclusionfinanciera.uy/porque/, accesed on April 2024.

Using rich administrative tax records and labor force household survey data, we leverage pre-reform degrees of cash use for wage disbursements at the sector level as a measure of exposure using a difference-in-differences approach, comparing the evolution over time of sectors with higher pre-reform cash intensity to those with lower cash intensity. The intuition behind our identification strategy is that sectors that were unlikely to rely on cash for wage disbursements prior to the reform are less affected by the no-cash mandate relative to sectors in which cash was a primary method for wage payments.

We find that the mandate was successful in reducing cash use for wage payments, with the gap in cash use across sectors significantly shrinking in the quarters following the reform. For each percentage point higher cash intensity in the pre-reform period, we find a reduction of 0.63 percentage points in the gap of cash use for wage payments among formal workers 3 years after the reform, almost completely closing the gap between high and low cash intensity sectors. This is mirrored almost perfectly by an increase in the use of bank transfers for wage disbursements.

Regarding firm behavior and the labor market, we use firm-level administrative data and find that firms in more cash-intensive sectors are signifcantly more likely to cease operating formally after the reform. Specifically, firms in sectors 50 percentage points more cash intensive are about 6 percentage points less likely to be active 3 years after the reform. Conditional on remaining formally active after the reform, firms in high cash intensity sectors show a significant reduction in the number of employees on payroll, of about 6 percent for sectors 50 percentage points higher in pre-reform cash intensity. In addition, firms that remain active show a significant increase in average reported wages: firms in sectors with 50 percentage points higher cash intensity show an increase of about 4 percent in wages 3 years after the reform.

The firm-level effects are driven mostly by smaller firms and those that reported lower reported wages prior to the reform, both of which we interpret as indicators of firm productivity (e.g. Berlingieri et al., 2018).[3] Large firms in high cash intensity sectors appear to be unaffected and, if anything, are slightly more likely to remain active after the reform. Even though small firms account for most informality (Ulyssea, 2018) and often have significantly higher levels of collusive underreporting of earnings (Feinmann et al., 2024), our findings do not immediately imply that firms that disappear switch to operate informally and that the increase in reported earnings is due to a reduction of tax evasion (in a similar logic to Brockmeyer and Sáenz Somarriba, 2022 and Das et al., 2023). It could be that firms that cease to operate formally actually cease operations altogether, and that differential trends in unobservable productivity trajectories among firms that remain active drive increases in real

[3] Small firms are also more likely to pay their workers in cash, see Appendix A.

wages.

To further understand the degree to which informality and collusive underreporting of earnings play a role in explaining our results, we complement our previous findings using household survey data that covers informality and wage underreporting. We find that formal workers in high cash intensity sectors show no significant break in survey-reported earnings but are signficantly less likely to state that they underreport their labor earnings for tax purposes, which suggests that a reduction in collusive underreporting drives part of the increase in reported earnings we observe in administrative data. In addition, we find that workers in high pre-reform cash intensity sectors are significantly more likely to be informally employed after the reform, suggesting that transitions into informal employment could be driving part of the effect on the share of firms that are active after the reform and the number of workers on payroll after the reform. We also do not find an effect on the overall probability of being employed across sectors, which suggests that transitions into informality are the main driver behind the reduction of formal employment observed in administrative data.

We conclude with some simple back-of-the-envelope calculations on the revenue implications of the policy. Empirical results suggest two counteracting effects: reducing collusive underreporting of wages increases revenue from formal workers, while the shift to informality shrinks the tax base. The overall impact on government revenues depends on which effect dominates. Using our event study coefficients, we extrapolate a 23.6 percentage point reduction in overall cash intensity due to the reform, which would increase reported wages by 1.88% but decrease formal employment by 4.96%. Assuming a tax schedule that is linear with respect to the wage mass, the reform would ultimately result in a 3.08% decrease in revenues, with the reduction of formal employment more-than-compensating the improvement in payroll tax compliance.

Our paper contributes to several branches of the literature. First, we contribute to the literature studying the effects of reducing the use of cash and boosting financial inclusion. Some recent papers have focused on the financial implications of such policies, such as how the disbursement of debit cards for the poor affects savings (Bachas et al., 2021) and the adoption of electronic payment technology (Higgins, 2024; Crouzet et al., 2023). More related papers have analyzed the effects of reduced cash use and tax compliance. For instance, Brockmeyer and Sáenz Somarriba (2022) study the effects of value added tax rebates for debit card purchases in Uruguay and find no effect on tax compliance. Das et al. (2023) study the demonetization policy in India and find that it led to a significant increase in reported tax sales. Our findings contribute to this literature by showing that cash use for wage payments is related to illegal labor market practices such as informal employment and collusive

underreporting of earnings. However, the effect is not a straightforward reduction of evasion after the reduction in cash use, but rather a reduction in evasion among employment that remains formal after the reform but combined with an increase in fully informal employment. This presents a cautionary tale regarding policy recommendations to reduce the use of cash: even if doing so can improve tax compliance among firms that remain formal, it can also have detrimental effects on overall revenue if transitions into fully off-the-books operations are possible.

Our paper also contributes to the literature studying labor markets in developing countries, specifically regarding informality and collusive underreporting of labor earnings. A vast body of research has studied the spread and consequences of informal employment (e.g. Williams and Lansky, 2013; Bitran, 2014). A significant fraction of this literature has focused on how taxes and registration costs affect informality (see La Porta and Shleifer, 2014 and Ulyssea, 2020 for reviews of this literature). Our paper indicates that the use of cash is a significant factor driving informal hiring of employees: forbidding the use of cash for wage disbursements shifts some employment into full informality. This suggests that some features of informal employment as a form of tax non-compliance could be different to those of, for instance, evasion of value-added taxes.

Regarding collusive underreporting of earnings, recent research has challenged the notion that third-party reporting eliminates the possibility of collusive underreporting from Kleven et al. (2011). For instance, Kumler et al. (2020) infer sizeable unreported labor earnings from area-by-industry-by-firm size reported wages in household surveys compared to administrative data in Mexico. Bíró et al. (2022) study a reform to the establishment audits rule in Hungary and document that bunching at the minimum wage is partly driven by collusive underreporting of earnings. Feinmann et al. (2024) find sizeable and widespread engagement in collusive underreporting of labor earnings in Brazil, with cash payments referenced as one of the most common methods for the disbursement of payments under the table. Our paper indicates that reducing the use of cash for wage disbursements can reduce the spread of collusive underreporting of earnings in a similar fashion as demonetization led to a reduction of value-added tax evasion in India (Das et al., 2023).

More broadly, our paper also contributes to the vast literature studying tax enforcement (e.g. Slemrod and Yitzhaki, 2002, Andreoni et al., 1998), particularly in developing countries (e.g. Besley and Persson, 2013). Some papers have found that third-party reporting can improve tax compliance (e.g. Pomeranz, 2015; Naritomi, 2019), although evasion can shift to the cost margin (e.g. Carrillo et al., 2017; Slemrod et al., 2017). Several papers document how the challenges of tax enforcement in developing countries affect countries' choices of tax instruments (e.g. Best et al., 2015; Gordon and Li, 2009). Our paper indicates that

widespread use of cash for wage disbursements can contribute to the concealing of labor earnings, complicating tax collection on wages. In addition, our paper highlights that a policy that could be conjectured to reduce tax evasion according to previous research (e.g. Das et al., 2023) can do so on the intensive margin, but can also increase extensive margin evasion in the form of fully informal economic activity.

The rest of the paper is structured as follows. Section 2 describes the context and institutional setting. Section 3 describes the data and the econometric strategy. Section 4 presents the results. Section 5 discusses the revenue implications of our findings. Section 6 concludes.

# 2. Context

**The Uruguayan context** Uruguay is an upper-middle-income country in South America, with a population of around 3.5 million and a GDP per capita of about $18,000 dollars in 2018 according to data from the World Bank. The country has an established contributory social security system for formal workers, including retirement benefits, unemployment insurance, workers' compensation, disability insurance, health insurance, and parental leave, all of which are handled by the Social Security Agency (SSA) called *Banco de Previsión Social*. The funding for this system comes from social security contributions levied on formal workers and employers on a monthly basis, for which employers are required to immediately report the employees they hire to the SSA.

As with many other Latin American countries, Uruguay faces significant challenges with tax evasion, and the financial inclusion reform intended to tackle some of these (Brockmeyer and Sáenz Somarriba, 2022). The tax administration estimates that at least 20 percent of potential VAT revenues were evaded in 2012, an equivalent to 2.5 percent of GDP (Dirección General Impositiva, 2019). The country also has a substantial fraction of employment that is non-registered (about 26% of employees are informally employed throughout the 2014-2019 period) and there is widespread non-compliance with payroll taxes, although both of these measures of informality have been falling in recent years.

Regarding levels of financial inclusion, by 2011 the country was significantly lagging behind other countries of similar income: less than 25% of the population over 15 years of age owned a bank account, while other countries of similar GDP per capita were closer to 50% (see panel A of Figure 1). Panel (b) presents the evolution of account ownership over time across several Latin American countries, indicating that Uruguay significantly increased its share of the population with access to a bank account in the years following the financial inclusion reform.[4]

[4] The countries used in panel (b) include Uruguay, Argentina, Brazil, Panama, Guatemala, Mexico, and

**The reform** In 2014, the government passed a law intended to increase the levels of bancarization and financial inclusion. The intent was to incorporate segments of the population that were not included in the financial system, reduce the degree to which Uruguay lagged behind countries of similar income in the degree of use of electronic payment methods, and to incentivize formalization and reduce tax evasion.[5] The reform included many components, such as VAT rebates for debit card purchases that Brockmeyer and Sáenz Somarriba (2022) study. In addition, it also defined a specific type of bank account (called "wage accounts") that workers use to receive their wage payments, which it also heavily regulated. Specifically, these accounts do not have any cost for opening, closing, maintaining, nor minimum balance requirements. Workers were also allowed to take some loans whose payments would be automatically deducted from their wage accounts.

Specifically regarding wage disbursements, one of the components of the law forbid wage payments to be made with cash and mandated the disbursements to be made using an electronic money instrument (such as bank account transfers, electronic apps, pre-paid cards, etc).[6] The schedule for implementation stipulated that, starting in October 2015, workers should voluntarily arrange with their employers which electronic payment method to use for receiving their wage payments. By May 2017, wage payments for all workers were mandated to receive their wage payments with an electronic money instrument. We refer to the period between October 2015 and May 2017 as the phase-in period and the period starting on May 2017 as the mandate period.

The enforcement of this mandate is similar to that of other labor market regulations (such as minimum wages, pension contributions, vacation time, and the sort), and it relies on a combination of establishment audits and workers' complaints filed to the Ministry of Labor or the Internal Revenue Service. Employers who are found to be paying wages in cash are to be fined the maximum between 25% of the sum paid in cash and twice the minimum wage. As in most developing country contexts, however, enforcement of labor market regulations is imperfect and there is significant non-compliance. Thus, we will empirically assess the degree to which the mandate effectively translated into lower use of cash in wage disbursements.

Costa Rica.

[5] See the official government website https://inclusionfinanciera.uy/por-que/, accesed on April 2024.

[6] The reform also stipulated that pensions and other social assistance programs would have to be disbursed electronically.

# 3. Data and empirical strategy

## 3.1 Data

**Administrative records** Our first source of data comes from firm-level administrative records from the Social Security Administration, which are constructed from the payroll tax forms that employers have to file monthly to submit contributions to the social security system. These records consist of a quarterly panel of the universe of formally registered firms for the years 2014-2019. For each firm we observe all their employees with their corresponding wages, the 2-digit level ISIC code for the sector of activity, and the legal status of the firm. As with all administrative records, these data cover only formal workers who work in formally registered firms.

We construct an indicator of whether a firm is active in a given quarter that equals 1 if the firm has any reported dependent employees in that period and zero otherwise. Then, for each firm in a given time period we calculate the number of dependent employees on payroll and the average wage. We keep firms that were active at any point between January of 2014 and September of 2015 and that had at least 3 employees on average. Our final administrative dataset consists of a quarterly panel of firms from the first quarter of 2014 to the last quarter of 2019. Summary statistics are presented in panel A of table 1.

**Household surveys** Our second source of data is individual-level data from the Continuous Household Survey (*Encuesta Continua de Hogares*, ECH), collected by the National Institute of Statistics since 1990. The ECH is a nationally representative survey carried out in accordance with international standards on a quarterly basis. It collects information on living standards, labor force participation, earnings, social security benefits, and sector of employment at the 4-digit ISIC code.

The survey asks employed workers whether their wage is paid in cash or using some other method. We use this question to construct an indicator equal to 1 if the worker is paid in cash and zero otherwise. We use this indicator not only as a dependent variable is some econometric specifications, but also to construct a pre-reform sector-level degree of cash intensity at the 2-digit ISIC code that we calculate as the proportion of workers who were paid in cash in the pre-reform periods in the sample (the first quarter of 2014 and the third quarter of 2015). We then merge this sector-level indicator of cash intensity to the same 2-digit ISIC code sector in the administrative data.

The ECH also asks workers whether their employer deducts their pension contributions from their paycheck, which we use to construct an indicator of the worker being formally employed if their employer does deduct pension contributions from their paycheck and zero if

not (as is standard in the literature, e.g. Tornarolli et al., 2014). In addition, the survey asks a question aimed at detecting underreporting of wages: formal workers are asked whether their social security contributions are based on the total earnings of their job or only part of them. The explicit aim of this question is to capture if collusive tax evasion is taking place (Bergolo and Cruces, 2014). We use this question to construct an indicator equal to 1 if the worker underreports their earnings and zero if not. Our final survey dataset consists of quarterly repeated cross sections of workers from the first quarter of 2014 to the last quarter of 2019. All calculations using survey data are done using the survey's sampling weights. Summary statistics are presented in table 1.[7]

## 3.2 Econometric strategy

We leverage the differential pre-reform cash intensiveness at the sector level as a measure of exposure to the reform with a difference-in-differences with treatment intensity approach. Inuitively, we presume that sectors that used little cash in wage disbursements prior to the reform are unlikely to be affected by the non-cash mandate compared to sectors in which cash was the main method of wage disbursement. Panel (a) of figure 2 shows a histogram of the sector-level proportion of workers who are paid in cash, showing significant variation in exposure to the reform.[8] Specifically, we estimate the following equation:

$$Y_{ist} = \sum_{k \neq -1} \beta_k \mathbb{1}\{t = e + k\} \times \text{PreCashUse}_s + \alpha_s + \mu_{I(s)t} + \varepsilon_{ist} \tag{1}$$

where $Y_{ist}$ denotes any of our outcomes of interest for unit $i$ in sector $s$ at time $t$, $\text{PreCashUse}_s$ denotes the proportion of workers of sector $s$ who were paid in cash prior to the reform, $\alpha_s$ is a sector fixed effect, $\mu_{I(s)t}$ is a time-by-industry (broader than sector) fixed effect, and $\varepsilon_{ist}$ is the error term. We define sectors at the 2-digit ISIC code level and industries at the 1-digit ISIC code level.

The $\beta_k$ coefficients are the parameters of interest, which measure the differential evolution over time among sectors with higher pre-reform cash intensity compared to sectors with lower cash intensity. For negative values of $k$, the coefficients allow us to test for the similarities in the pre-reform trends across sectors with different cash intensities, while coefficients associated with positive values of $k$ allow us to capture the dynamics of the effects. In all

[7] To avoid using sectors for which we have too few workers to reliably estimate the degree pre-reform cash intensity, we drop from the analysis the bottom 10% of sectors with the lowest number of observations (9 sectors), which leaves us with a total of 75 sectors. Results remain virtually unchanged when including the omitted sectors in the analysis.

[8] Given that we include industry-by-time fixed effects in our empirical specifications, we also present a version of this histogram but residualizing cash intensity from industry fixed effects in Appendix figure A.II, which also shows substantial variation in within-industry cash intensity.

specifications, we cluster standard errors at the sector level, which is the dimension in which cash intensity varies.

# 4. Results

## 4.1 Main results

**Cash use for wage payments** We begin by exploring whether the reform effectively reduced the use of cash in wage payments. To do so, we first estimate two versions of equation 1, first with the dependent variable being an indicator equal to 1 if the worker reports being paid with cash and second with the dependent variable being an indicator equal to 1 if the worker reports being paid with a bank transfer. For this analysis, we use the sample of formal workers from the household survey in which workers were asked about the disbursement method for their wage payments.[9]

Figure 3 reports OLS estimates and 95% confidence intervals of the $\beta_k$ coefficients for this exercise. We find stable trends prior to the reform, with a significant reduction in the probability of receiving cash for wage payments during the phase-in period, and a more pronounced reduction during the mandate period. This is further reflected in panel (b) of figure 2, which shows a comparison between cash intensity in the pre-reform and the post-reform periods. Notably, the post-reform distribution is significantly shifted towards lower cash intensity.

Given the continuous definition of our exposure variable, the event-study coefficients should be interpreted as the reduction in the probability of being paid in cash for workers in a sector in which all workers were paid with cash compared to a sector in which no workers were paid in cash. As shown in figure 2, cash intensity prior to the reform is more evenly distributed within the 0-1 interval, so the actual effects should be interpreted as more modest. Notably, the reduction in cash use for wage payments seems to be completely mirrored by an increase in bank transfer payments.

**Firm behavior** We then turn to analyzing the effects of the reform on firm behavior using administrative firm-level records, starting with firm survival. Panel (a) of Figure 4 reports OLS estimates and 95% confidence intervals of the $\beta_k$ coefficients from equation 1, with the dependent variable being an indicator equal to 1 if the firm is active and 0 otherwise. Once again, we observe stable pre-reform trends across sectors with varying levels of cash intensity. Once the reform starts, we observe a significant reduction in the probability of the firm being

[9] In appendix figure A.I, we show the raw time series for workers paid in cash and in bank transfer, showing a significant reduction of cash use in favor of bank transfers.

active: two years after the reform, firms in sectors with 50 percentage points higher cash intensity are around 6.5 percentage points less likely to be active. This is reflected in the pooled event study coefficients in column 1 of table 2, indicating a highly significant reduction in the share of active firms.

We then explore the effects of the reform on firm behavior conditional on the firms remaining active. Given that firms that are inactive have no employees on payroll by definition, for this exercise we keep a balanced panel of firms that remained active in all periods in the sample, so as to prevent event-study coefficients from being estimated with different samples in different periods.[10] Panel (b) of figure 4 reports OLS estimates of the $\beta_k$ coefficients with the dependent variable being the natural logarithm of the number of workers employed by the firm. There is a significant reduction in the number of workers employed by the firm in more cash intensive sectors after the reform: firms in sectors with 50 percent higher cash intensity reduce their number of workers on payroll by about 5.5 percent in the long-run. In panel (c), we report OLS estimates of the event study coefficients for the average wage. We find a significant increase in reported average wages: firms in sectors with 50 percent higher cash intensity report wages about 4 percent higher in the long-run. These event-study coefficients are summarized in pooled coefficients in table 2.

We then analyze the heterogeneous effects of the reform according to firm size and pre-reform wages, both of which we interpret as measures of firm-level productivity (e.g. Berlingieri et al., 2018).[11] The literature has also established that informality is more common in small firms (e.g. Ulyssea, 2018), and recent research has found that collusive underreporting of earnings is also more frequent in smaller firms (Feinmann et al., 2024). We define an indicator for small firm if the firm had less than 20 employees on average during the pre-reform period and an indicator for large firm if the firm had 20 or more, and then we interact the event-study coefficients with the indicators of small and large firm.[12]

Figure 5 reports the results from this exercise. Importantly, panel (a) indicates that the reduction in the share of active firms is driven exclusively by small firms, with large firms being unaffected in the short run and, if anything, slightly more likely to be active in the long run. This is reflected in the first column of Panel A of table 3. Panel (b) of figure 5 shows that the reduction in the number of workers is also more pronounced in smaller firms, although effects for large firms are economically significant despite statistical insignificance (see Column 2 of Panel A in table 3). Finally, panel (c) of figure 5 shows that the increase

[10] In appendix figure A.VIII we show that our results remain similar when using the unbalanced full sample of firms.

[11] The use of cash for wage disbursements is also significantly more widespread for small firms, which makes them more affected by the reform, see Appendix figure A.IV.

[12] We choose 20 as a cutoff because it matches one of the categories for firm sized used in the labor force household survey.

in wages comes exclusively from smaller firms.

We then turn to analyzing the heterogeneous effects of the reform based on pre-reform wages. We first residualize wages during the pre-reform periods from time fixed effects and sector fixed effects, and then categorize firms as high wages firms if their residualized average wage is above-median and low wages if their average wage is below median. Figure 6 shows the results from this exercise. Panel (a) of figure 6 shows that the reduction in the probability of the firm being active is more pronounced among firms that paid low wages prior to the reform, although it is still significant for firms paying high wages. Panel (b) indicates that the reduction in the number of workers is more pronounced among firms that paid low wages prior to the reform. Finally, panel (c) indicates that the increase in wages comes exclusively from firms paying low wages prior to the reform. These results are summarized in Panel B of table 3.

Overall, results using administrative data suggest that a ban on cash use for wage payments reduces the probability of a firm being active, particularly among smaller firms and those that paid lower wages prior to the reform. Conditional on remaining active after the reform, firms in higher cash intensity sectors show a signficant reduction in the number of workers that they employ and a significant increase in their reported wages, both also driven primarily by smaller firms and those that paid lower wages prior to the reform.

Although the fact that the effects on firm survival, number of workers, and reported wages come mostly from smaller firms in which tax evasion and informality are more widespread, our findings so far are insufficient to conclusively determine the degree to which informality and tax evasion play a role. While it could be that wages increase because the reduced use of cash allows for less tax evasion, it could also be the case that higher productivity firms that pay higher wages are the ones that remain active after the reform. It is also hard to disentangle the degree to which firms ceasing activity in administrative data reflects actual ceasing operations relative to transition into pure informality. In future versions of the paper, we will explore effects on the wages for incumbent workers and, more broadly, the effects of the reform at the worker level.

**Survey data results** To further disentangle the degree to which informality and collusive underreporting of earnings drive the results, we complement the previous analysis with household survey data that covers informality and collusive underreporting of earnings. We begin by studying whether the reform affected wages reported in survey data. Panel (a) of figure 7 reports OLS estimates of the event study coefficients with the dependent variable being the natural logarithm of the wage. Importantly, we do not find any significant increase in wages reported in survey data, which is also reflected in column 2 of table 4. If anything,

earnings reported in survey data show a minor reduction in higher cash-intensive sectors.

Panel (c) of figure 7 shows that the share of workers that underreport their earnings falls significantly in more cash-intensive sectors after the reform: workers in sectors 50 percentage points more cash intensive prior to the reform are almost 2 percentage points less likely to state that they underreport their labor earnings for tax purposes in the long run. This is reflected in column 4 of table 4 Although this measure of collusive underreporting of earnings is discrete, which renders us unable to quantify magnitudes on reported earnings, it suggests that a reduction of tax evasion explains in part the increase in earnings reported in administrative data.

Panel (b) of figure 7 shows that workers in more cash intensive sectors show a significant increase in the probability of being informal in the long run, which is also reflected in column 3 of table 4. Specifically, workers in sectors 50 percentage points more cash intensive prior to the reform are about 3.5 percentage points more likely to report being informally employed in the long run. This suggests that part of the reduction in active firms and the number of workers among firms that remain active could potentially be driven by transitions into informality. It is complicated to assess how much of the reduction of formal workers is driven purely by informality, so we approximate the effect on overall employment by leveraging the fact that unemployed workers are asked in which sector they were employed before. Panel (d) of figure 7 shows that there seems to be no effect on the overall probability of a worker being employed, suggesting that transitions into full informality are the main driver.

Table 5 shows heterogeneity of the effects on earnings, informality, and evasion according to firm size using household survey data, where small firm is defined as an establishment with less than 20 employees.[13] Notably, earnings among formal workers seem to be unaffected by the reform in firms of any size (Column 1), while the degree of earnings underreporting significantly falls in small firms. This can explain the increase in wages observed in administrative data in small firms. Inforamlity, on the other hand, seems to increase in both small and large firms, which is consistent with the drop in employees observed for both types of firms in administrative data.

Overall, results in this section indicate that the elimination of cash for wage payments produces a significant reduction in the probability of firms remaining formally active. Among those that remain active after the reform, we observe a minor reduction in the number of workers they employ and a significant increase in reported wages. These effects are driven by smaller firms and those that paid lower wages prior to the reform. Results using survey data suggest that these results are partially driven by transitions into full informality and a reduction in collusive underreporting of earnings among formal workers.

[13] Appendix figure A.V shows the event study coefficients for large and small firms.

### 4.2 Robustness checks

**Leave-one-out robustness checks** Given that we leverage within-industry levels of cash intensity at the sector level, we assess the degree to which individual industries influence the results using a series of leave-one-out robustness checks. For each of our dependent variables, we estimate the event-study coefficients by sequentially excluding one of the industries at a time and compare it with our baseline estimates. Appendix figures A.VI and A.VII show the results of this exercise using administrative and survey data, respectively. For succintness, we report the long-run effect coefficient (i.e. $k \in [12, 16]$). In all panels, we observe very similar coefficients across specifications, indicating that our results are not driven by one specific industry.

**Unbalanced sample robustness checks** Given that the reform induced a reduction in the share of firms that are active, to estimate the effects of the reform conditional on remaining active we rely on the balanced sample of firms that remained active throughout the whole sample period. This ensures that event-study coefficients are estimated using the same sample for all time periods. To assess the degree to which constraining the sample to the balanced panel of firms affects the results, we also estimate the event-study coefficients with the full unbalanced sample. Results of this exercise can be found in Appendix figure A.VIII. In both panels, results remain virtually identical to estimates using the balanced sample.

**Unweighted regression robustness checks** Our baseline results using survey data use the survey sampling weights to approximate representativeness of the population. To assess the degree to which the use of sampling weights affects our estimated coefficients, we re-estimate our main survey data specifications using unweighted regressions. The results of this exercise can be found in Appendix table B.I, which shows results remain virtually unchanged when not using sampling weights.

## 5. Revenue implications

In this section we present some back-of-the-envelope calculations regarding the effects of the virtual payment mandate on overall government revenues. Empirical results from section 4 suggest that there are two counteracting effects. On one hand, the reduction of collusive underreporting of wages mechanically increases revenue from formal workers. On the other hand, the transition of workers and businesses into informality shrinks the tax base of workers. Which of these effects dominates determines the overall impact on government revenues.

Assuming a simplified tax system in which taxes on wages are collected by applying a flat tax rate $\tau$ to formal wages $w$ over formal workers $L$ (as is the case for contributions for social security in Uruguay), we can define government revenues as $T = \tau wL$. Thus, in this simplified setting, total revenue has an elasticity of 1 relative to both formal wages and the number of formal workers. We can then match the changes in formal wages and employment estimated from the empirical analysis.

Starting from a baseline cash intensity among formal workers of 37% (the average in the pre-reform period) and assuming proportionality in the reduction in cash intensity, we can use our pooled long-run event study coefficient of 0.638 (Column 1 of table 4) to attribute a 23.6 percentage point reduction in economy-wide cash intensity to the policy. Given this 23.6 percentage point reduction in cash intensity, given our pooled long-run event study coefficients, we would expect reported wages to increase by 1.88% as a result of the reform. Combining the extensive- and intensive-margin responses on overall formal employment (see Appendix figure A.IX), we would expect overall formal employment to drop by 4.96%. This yields a negative effect of the policy on revenues of 3.08%, with the drop in registered employment more than compensating revenue gains from improved payroll tax compliance.

# 6. Conclusion

In this paper, we studied the effects of a reform that mandated wage payments to be made using electronic payments instead of cash in Uruguay. Leveraging pre-reform degrees of cash intensity at the sector level, we study the effects of the reform using a difference-in-differences approach, comparing the evolution over time of sectors with higher cash intensity relative to those with lower cash intensity using rich administrative and survey data.

We find that the reform induced a significant reduction in the share of firms that are active in high cash intensity sectors. Among those that remain active, we find a minor reduction in the number of workers that they employ and a significant increase in reported wages. These effects are driven by smaller firms and those that paid lower wages prior to the reform. Complementary results using survey data suggest that the increase in reported wages is driven in part by a reduction in underreporting of earnings, while transitions into full informality can explain part of the reduction in formally active firms and the number of employees conditional on remaining active.

Our findings provide the first empirical evidence on the effects of cash use for wage disbursements on labor market outcomes and, specifically, illegal labor market practices. Part of our findings are consistent with recent research that has documented how recent efforts to foster the use of electronic payment methods have led to significant reductions in

tax evasion (Das et al., 2023). However, our findings also highlight a potential increase in other types of evasion: even though reported wages increase among firms that remain active after the reform, the reform induced a significant reduction of formal employment and firms, which offsets the revenue gains from better payroll tax compliance.

It is worth noting that the financial inclusion reform had many objectives beyond that of boosting formalization and reducing tax evasion, with the main goal being to increase overall access to financial services. In that sense, despite potential detrimental effects on informality and tax compliance, we do find that the reform increased the use of bank accounts for wage disbursements, which can have benefits beyond that of effects on labor markets.

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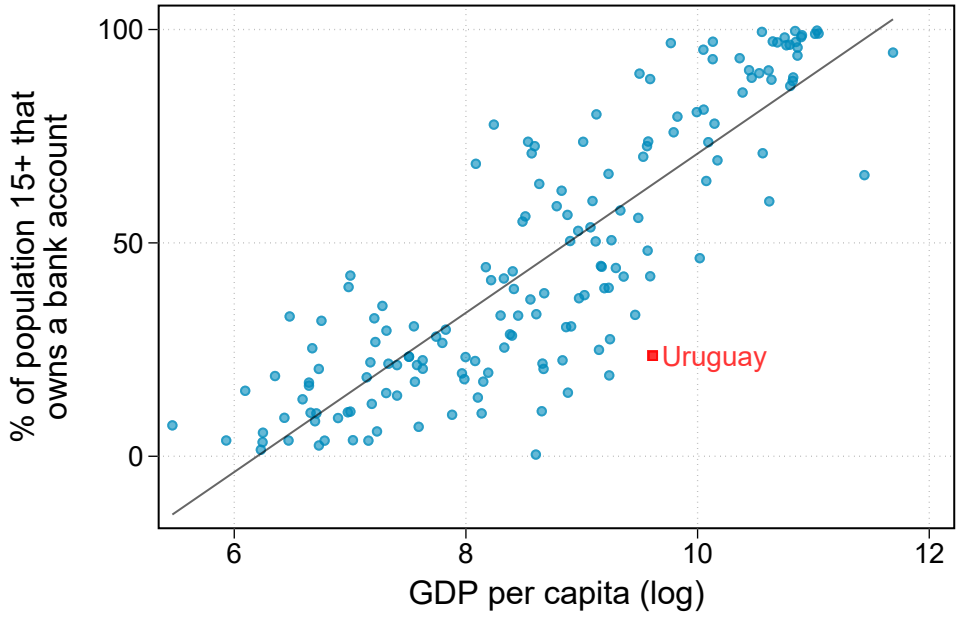
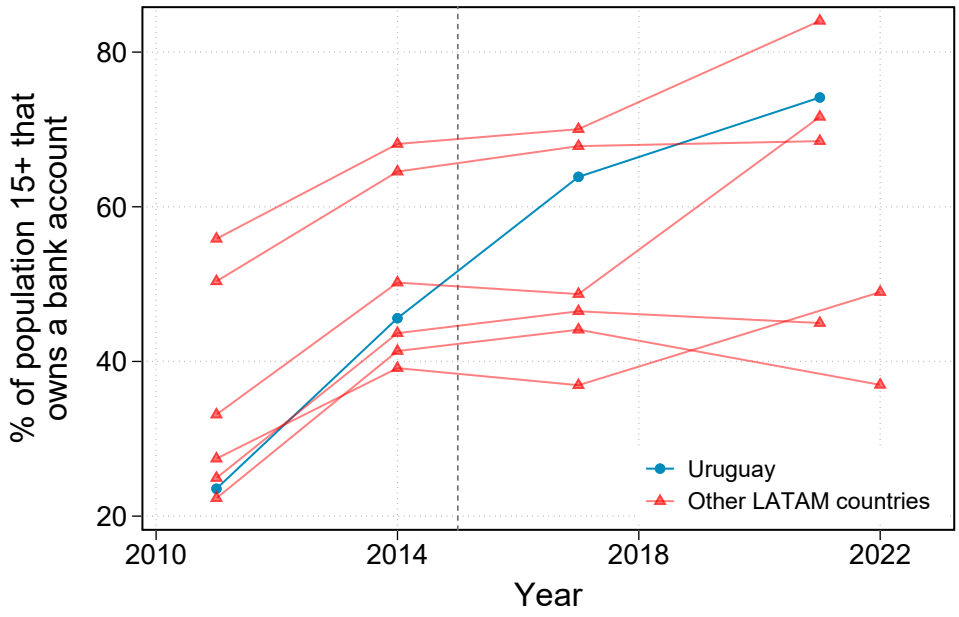
# A. Figures

**Figure 1:** Account ownership across countries

**(a)** Cross-country account ownership (2011)



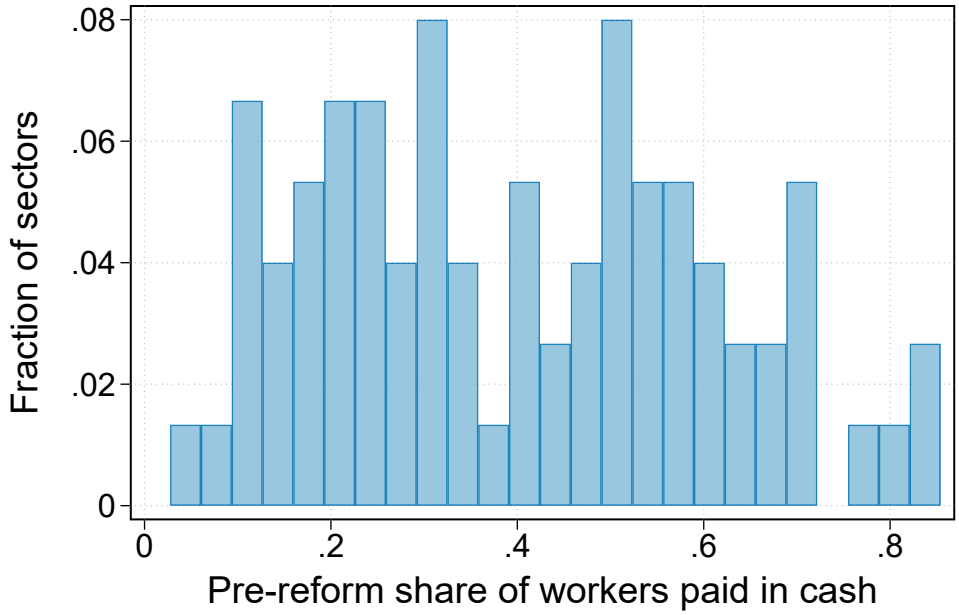

**(b)** Evolution of account ownership for selected Latin-American countries



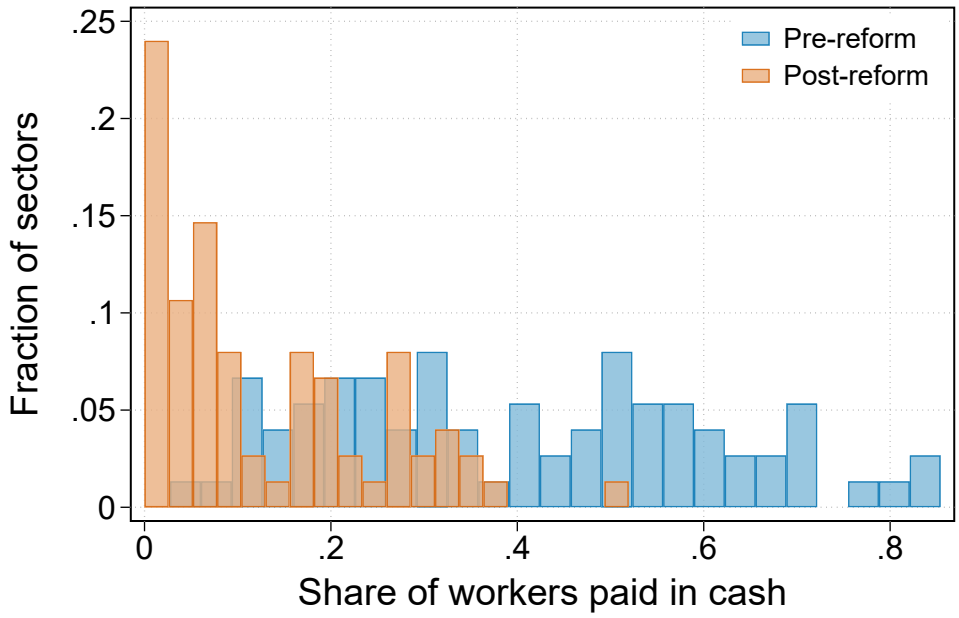

*Notes*: This figure presents cross-country comparisons of account ownership. Panel (a) presents a scatter plot of the percentage of the population aged 15 and higher that owns a bank account at the country level and the corresponding GDP per capita in current US dollars, both measured in the year 2011. Uruguay, the country studied in this paper, is highlighted in red. Panel (b) shows the evolution over time for selected countries in Latin America, with Uruguay highlighted in blue. The dashed line indicates the year of the reform mandating wage payments to be made with electronic methods.

**Figure 2:** Sector-level cash intensity histogram

**(a)** Pre-reform

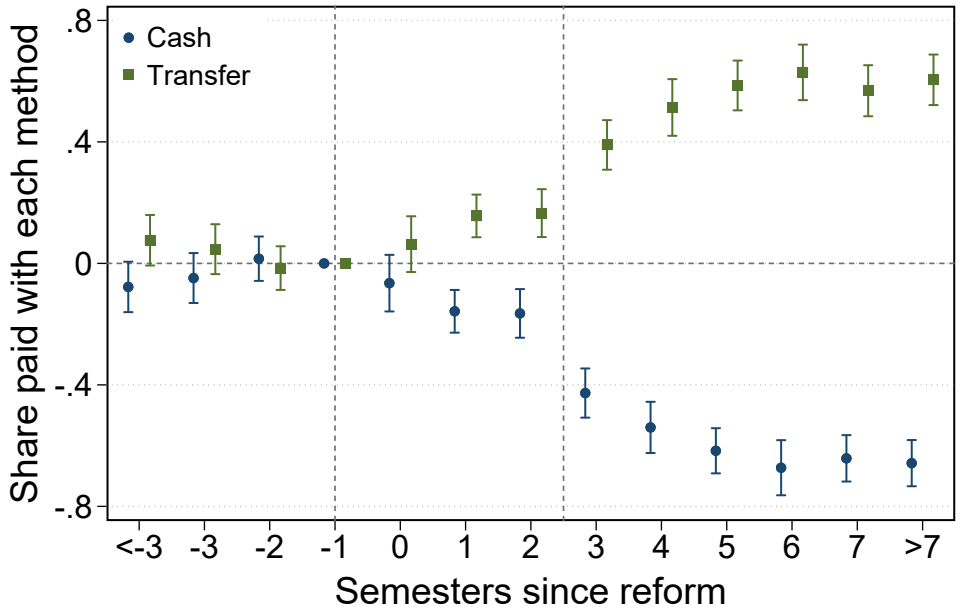



**(b)** Pre- and post-reform comparison



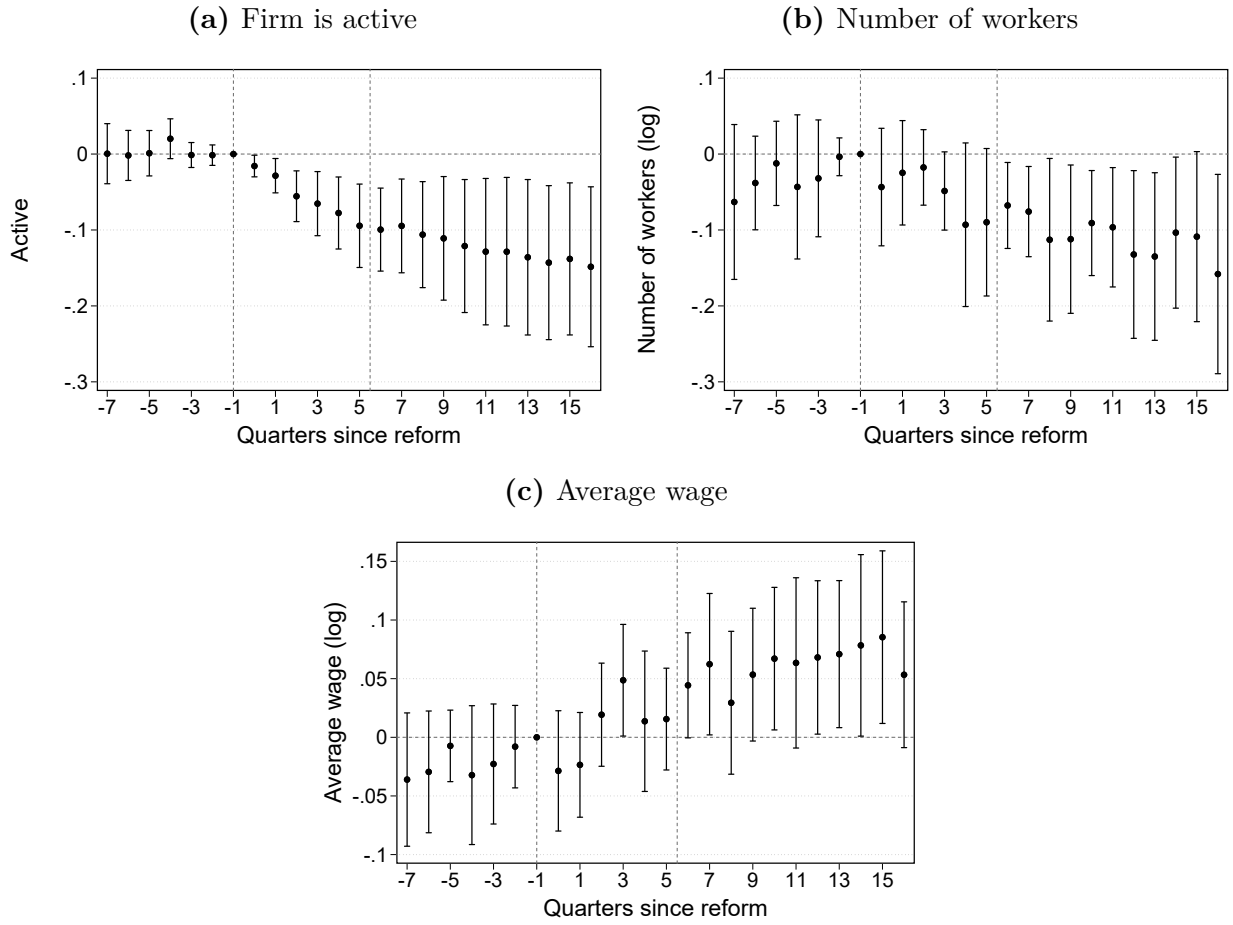

*Notes*: This figure shows a histogram of the sector-level share of workers who are paid in cash. The sector is defined at the 2-digit ISIC code level. Panel (a) shows a histogram only for the pre-reform period and panel (b) shows a comparison with the post-reform period. The pre-reform period in blue corresponds to the year 2014 and the post-reform period in orange corresponds to the year 2019.

**Figure 3:** Payment method event study




*Notes*: This figure shows OLS estimates of the $\beta_k$ coefficients from equation 1 pooled by semesters. The dependent variable for blue coefficients is an indicator equal to 1 if the worker is paid in cash. The dependent variable for green coefficients is an indicator equal to 1 if the worker is paid in with a bank transfer. The first dashed line indicates the beginning of the phase-in period and the second dashed line indicates the beggining of the mandate period. Capped vertical bars represent 95% confidence interval from standard errors clustered at the sector level.

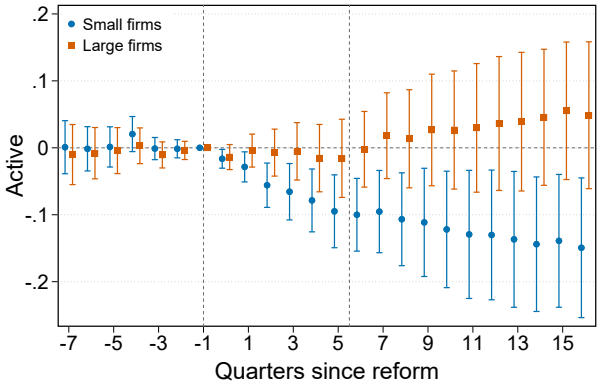
**Figure 4:** Main effects of the reform (administrative data)



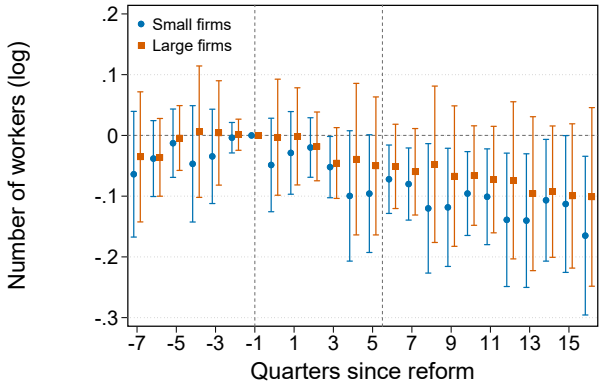

*Notes*: This figure shows OLS estimates of the $\beta_k$ coefficients from equation 1. In panel (a) the dependent variable is an indicator equal to 1 if the firm is active. In panel (b) the dependent variable is the natural logarithm of the number of employees on payroll. In panel (c) the dependent variable is the natural logarithm of the average wage of the firm. The first dashed line indicates the beginning of the phase-in period and the second dashed line indicates the beggining of the mandate period. Capped vertical bars represent 95% confidence interval from standard errors clustered at the sector level.

**Figure 5:** Main effects of the reform - heterogeneity by firm size (administrative data)

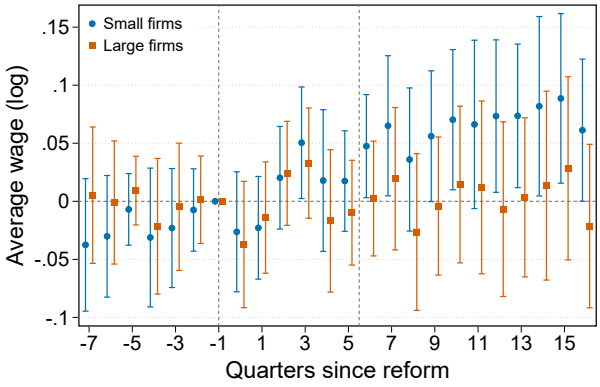
**(a)** Firm is active




**(b)** Number of workers

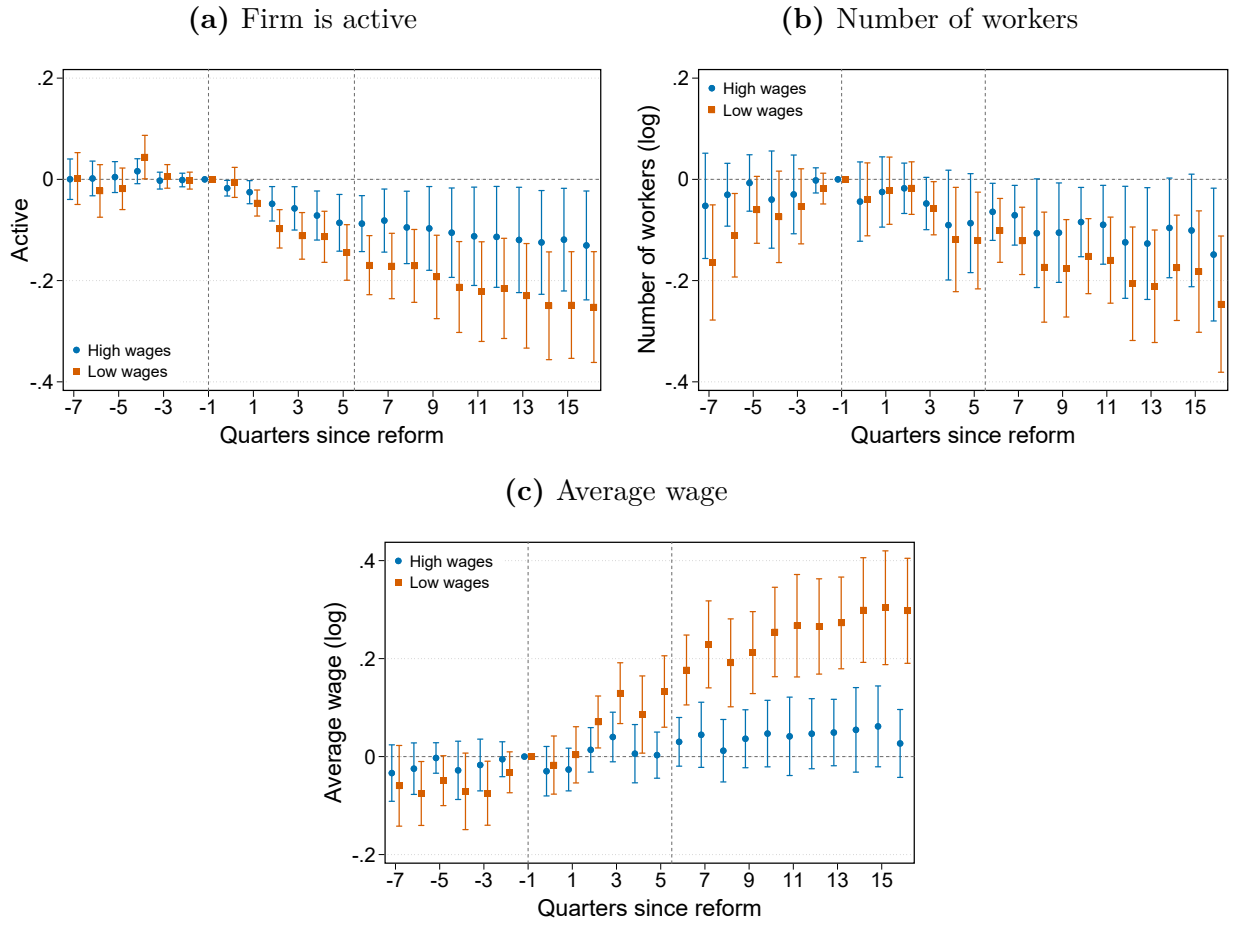



**(c)** Average wage



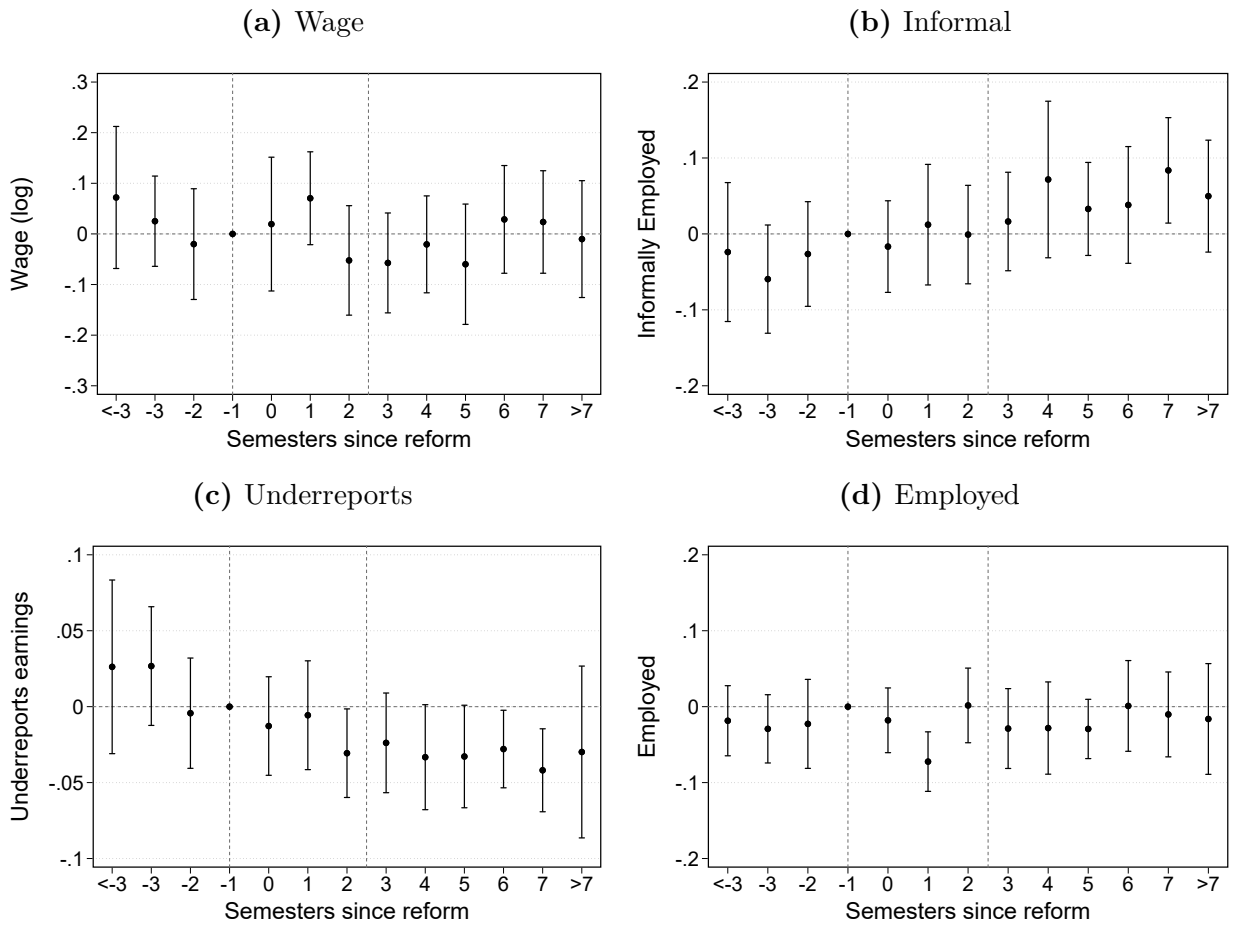

*Notes*: This figure shows OLS estimates of the $\beta_k$ coefficients from equation 1 interacted with indicators for small firm and large firm. Blue corresponds to coefficients interacted with an indicator for small firm and orange corresponds to coefficients interacted with an indicator for large firm. Small firm is defined as having less than 20 employees on average during the pre-reform period. In panel (a) the dependent variable is an indicator equal to 1 if the firm is active. In panel (b) the dependent variable is the natural logarithm of the number of employees on payroll. In panel (c) the dependent variable is the natural logarithm of the average wage of the firm. The first dashed line indicates the beginning of the phase-in period and the second dashed line indicates the beggining of the mandate period. Capped vertical bars represent 95% confidence interval from standard errors clustered at the sector level.

**Figure 6:** Main effects of the reform - heterogeneity by pre-reform wages (administrative data)



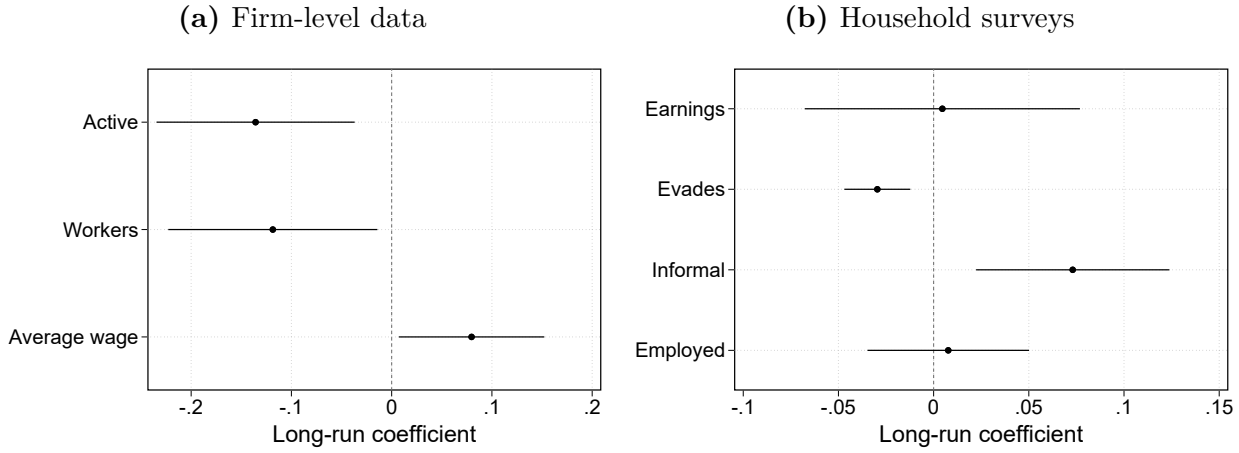

*Notes*: This figure shows OLS estimates of the $\beta_k$ coefficients from equation 1 interacted with indicators of the firm having high or low pre-reform wages. Blue corresponds to coefficients interacted with an indicator equal to 1 if the firm has above median pre-reform wages and orange corresponds to interactions with an indicator equal to 1 if the firm has below median pre-reform wages. Pre-reform wages are constructed by residualizing average wages at the firm level during the pre-reform period by time fixed effects and sector fixed effects. In panel (a) the dependent variable is an indicator equal to 1 if the firm is active. In panel (b) the dependent variable is the natural logarithm of the number of employees on payroll. In panel (c) the dependent variable is the natural logarithm of the average wage of the firm. The first dashed line indicates the beginning of the phase-in period and the second dashed line indicates the beggining of the mandate period. Capped vertical bars represent 95% confidence interval from standard errors clustered at the sector level.

**Figure 7:** Main effects of the reform (survey data)




*Notes*: This figure shows OLS estimates of the $\beta_k$ coefficients from equation 1 pooled by semesters. In panel (a) the dependent variable is the natural logarithm of the wage. In panel (b) the dependent variable is an indicator equal to 1 if the worker is informally employed. In panel (c) the dependent variable is an indicator equal to 1 if the worker underreports their labor earnings for tax purposes. In panel (d) the dependent varialbe is an indicator equal to 1 if the worker is employed and 0 if unemployed. The first dashed line indicates the beginning of the phase-in period and the second dashed line indicates the beggining of the mandate period. Capped vertical bars represent 95% confidence interval from standard errors clustered at the sector level.

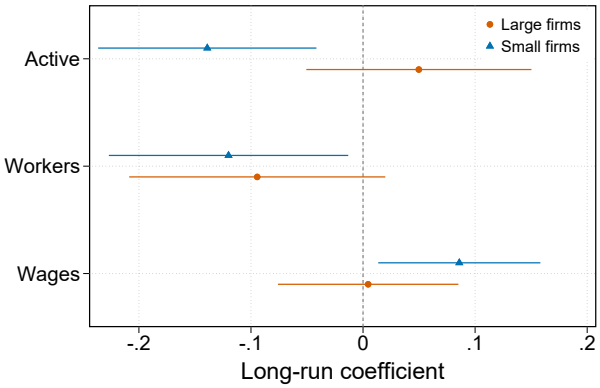
**Figure 8:** Difference-in-differences estimates (long-run)



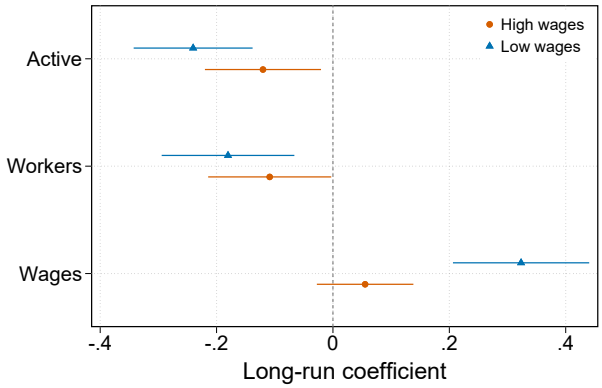

*Notes*: This figure shows OLS estimates of the $\beta_k$ coefficients from equation 1 pooled for the last 5 quarters of the mandate period. Panel (a) corresponds to estimates using administrative data and panel (b) corresponds to estimates using household survey data. Capped vertical bars represent 95% confidence interval from standard errors clustered at the sector level.

**Figure 9:** Difference-in-differences heterogeneity estimates (long-run)

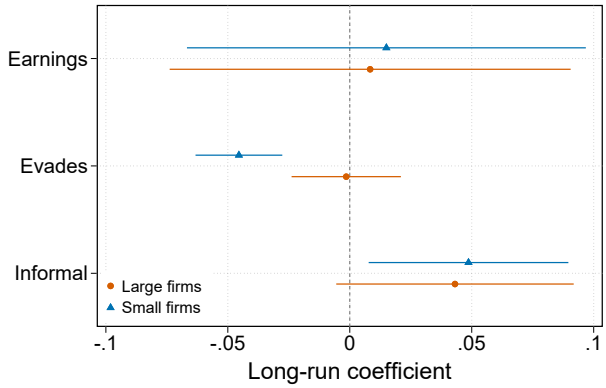
**(a)** Firm size (administrative data)




**(b)** Pre-reform wages (administrative data)




**(c)** Firm size (household surveys)




*Notes*: This figure shows OLS estimates of the $\beta_k$ coefficients from equation 1 pooled for the last 5 quarters of the mandate period. Panel (a) corresponds to estimates using administrative data and panel (b) corresponds to estimates using household survey data. Capped vertical bars represent 95% confidence interval from standard errors clustered at the sector level.

# B. Tables

**Table 1:** Summary statistics

| | Observations | Mean | Standard Deviation | Median |
|---|---|---|---|---|
| *Panel A. Administrative data* | | | | |
| Active | 805,656 | 0.856 | 0.352 | 1.000 |
| Number of workers | 689,278 | 17.513 | 96.613 | 6.000 |
| Average wage | 689,278 | 33968.850 | 33575.948 | 25692.000 |
| Small firm | 804,336 | 0.870 | 0.336 | 1.000 |
| *Panel B. Household survey data* | | | | |
| Paid in cash | 189,992 | .305 | .46 | 0 |
| Female | 605,766 | .498 | .5 | 0 |
| Age | 605,766 | 36.7 | 23.5 | 35 |
| Informally employed | 280,742 | .258 | .438 | 0 |
| Underreports earnings | 164,697 | .0353 | .185 | 0 |
| Wage | 280,742 | 17232 | 18691 | 14500 |
| Small firm | 283,065 | .582 | .493 | 1 |

*Notes*: This table presents summary statistics from our two datasets. Panel A reports summary statistics for administrative data. Active is an indicator equal to 1 if the firm is active in a given period and zero otherwise. Number of workers is the total number of dependent workers employed by a firm in a given period. Average wage is the average wage of the firm in a quarter. Small firm is an indicator equal to 1 if the firm had less than 20 employees on average during the pre-reform period. Panel B reports summary statistics for household survey data. Paid in cash is an indicator equal to 1 if the worker reports being paid in cash. Female is a dummy variable equal to 1 if the respondent is female. Age is the age of the respondent in years. Informally employed is a dummy variable equal to 1 if the worker is informally employed (does not make pension contributions). Underreports earnings is a dummy variable equal to 1 if the respondend states that they underreport their labor earnigns for tax purposes. Wage is the workers' earnings in current Uruguayan pesos. Small firm is an indicator equal to 1 if the firm has less than 20 employees.

**Table 2:** Main results (firm-level)

| | (1) Active | (2) Workers (log) | (3) Average wage (log) |
|---|---|---|---|
| Cash intensity × Pre period $[k \in [-7, -4]]$ | 0.00589 | -0.0273 | -0.0160 |
| | (0.0137) | (0.0241) | (0.0138) |
| Cash intensity × Phase-in $[k \in [0, 5]]$ | -0.0553*** | -0.0410 | 0.0177 |
| | (0.0166) | (0.0271) | (0.0173) |
| Cash intensity × Short-run $[k \in [6, 11]]$ | -0.109*** | -0.0808** | 0.0636* |
| | (0.0362) | (0.0338) | (0.0320) |
| Cash intensity × Long-run $[k \in [12, 16]]$ | -0.138*** | -0.116** | 0.0815** |
| | (0.0493) | (0.0536) | (0.0365) |
| Observations | 805656 | 557184 | 555902 |
| Clusters | 75 | 75 | 75 |

*Notes*: This table shows OLS estimates from pooling several $\beta_k$ coefficients from equation 1. Cash intensity is the pre-reform share of workers paid in cash. Phase-in pools the $\beta_k$ coefficients for the phase in period ($k \in [0, 5]$). Short-run pools the $\beta_k$ coefficients for the first 6 quarters in the mandate period ($k \in [6, 11]$). Long-run pools the $\beta_k$ coefficients for the last 5 quarters in the mandate period ($k \in [12, 16]$). In column 1, the dependent variable is an indicator equal to 1 if the firm is active and 0 otherwise. In column 2, the dependent variable is the natural logarithm of the number of workers employed in the firm. In column 3, the dependent variable is the natural logarithm of the average wage of the firm. All specifications include sector fixed effects and industry-by-time fixed effects. Standard errors are clustered at the sector level * Significant at the 10% level ** Significant at the 5% level *** Significant at the 1% level.

**Table 3:** Main results heterogeneity (firm-level)

| | (1) Active | (2) Workers (log) | (3) Average wage (log) |
|---|---|---|---|
| *Panel A. Heterogeneity by firm size* | | | |
| Cash intensity × Pre period [$k \in [-7,-4]$] × Large firm | 0.0000271 | -0.0191 | -0.000864 |
| | (0.0150) | (0.0245) | (0.0142) |
| Cash intensity × Phase-in [$k \in [0,5]$] × Large firm | -0.00543 | -0.0280 | -0.00236 |
| | (0.0177) | (0.0306) | (0.0182) |
| Cash intensity × Short-run [$k \in [6,11]$] × Large firm | 0.0236 | -0.0624 | 0.00408 |
| | (0.0370) | (0.0385) | (0.0349) |
| Cash intensity × Long-run [$k \in [12,16]$] × Large firm | 0.0498 | -0.0944 | 0.00457 |
| | (0.0504) | (0.0573) | (0.0404) |
| Cash intensity × Pre period [$k \in [-7,-4]$] × Small firm | 0.00618 | -0.0277 | -0.0163 |
| | (0.0137) | (0.0244) | (0.0139) |
| Cash intensity × Phase-in [$k \in [0,5]$] × Small firm | -0.0558*** | -0.0447* | 0.0197 |
| | (0.0165) | (0.0264) | (0.0175) |
| Cash intensity × Short-run [$k \in [6,11]$] × Small firm | -0.110*** | -0.0851** | 0.0670** |
| | (0.0360) | (0.0336) | (0.0320) |
| Cash intensity × Long-run [$k \in [12,16]$] × Small firm | -0.139*** | -0.120** | 0.0859** |
| | (0.0489) | (0.0536) | (0.0363) |
| Observations | 804336 | 555912 | 554630 |
| Clusters | 75 | 75 | 75 |
| *Panel B. Heterogeneity by pre-reform wages* | | | |
| Cash intensity × Pre period [$k \in [-7,-4]$] × Below median wages | -0.000107 | -0.0784*** | -0.0282 |
| | (0.0163) | (0.0283) | (0.0204) |
| Cash intensity × Phase-in [$k \in [0,5]$] × Below median wages | -0.0878*** | -0.0388 | 0.103*** |
| | (0.0182) | (0.0282) | (0.0272) |
| Cash intensity × Short-run [$k \in [6,11]$] × Below median wages | -0.191*** | -0.123*** | 0.257*** |
| | (0.0375) | (0.0379) | (0.0510) |
| Cash intensity × Long-run [$k \in [12,16]$] × Below median wages | -0.240*** | -0.180*** | 0.323*** |
| | (0.0513) | (0.0572) | (0.0588) |
| Cash intensity × Pre period [$k \in [-7,-4]$] × Above median wages | 0.00693 | -0.0219 | -0.0148 |
| | (0.0138) | (0.0244) | (0.0138) |
| Cash intensity × Phase-in [$k \in [0,5]$] × Above median wages | -0.0497*** | -0.0412 | 0.00858 |
| | (0.0169) | (0.0271) | (0.0187) |
| Cash intensity × Short-run [$k \in [6,11]$] × Above median wages | -0.0952** | -0.0762** | 0.0427 |
| | (0.0366) | (0.0334) | (0.0359) |
| Cash intensity × Long-run [$k \in [12,16]$] × Above median wages | -0.120** | -0.109** | 0.0553 |
| | (0.0501) | (0.0531) | (0.0416) |
| Observations | 805656 | 557184 | 555902 |
| Clusters | 75 | 75 | 75 |

*Notes*: This table shows OLS estimates of interactions from pooling several $\beta_k$ coefficients from equation 1. Cash intensity is the pre-reform share of workers paid in cash. Panel A interacts the event study coefficients with indicators of whether the firm is small or large. Panel B interacts the event study coefficients with indicators of whether the firm has below or above average pre-reform wages. Phase-in pools the $\beta_k$ coefficients for the phase in period ($k \in [0,5]$). Short-run pools the $\beta_k$ coefficients for the first 6 quarters in the mandate period ($k \in [6,11]$). Long-run pools the $\beta_k$ coefficients for the last 5 quarters in the mandate period ($k \in [12,16]$). Pre-reform wages are constructed from the residuals of regressing average wages at the firm level on time fixed effects and sector fixed effects in the pre-reform period. Small firm is a dummy variable equal to 1 if the firm had less than 20 employees on average during the pre-reform period and zero otherwise. Above median wages is an indicator equal to 1 if the firm had above median wages during the pre-reform period and below median wages is an indicator equal to 1 if the firm had below median wages during the pre-reform period. In column 1, the dependent variable is an indicator equal to 1 if the firm is active and 0 otherwise. In column 2, the dependent variable is the natural logarithm of the number of workers employed in the firm. In column 3, the dependent variable is the natural logarithm of the average wage of the firm. All specifications include sector fixed effects and industry-by-time fixed effects. Standard errors are clustered at the sector level * Significant at the 10% level ** Significant at the 5% level *** Significant at the 1% level.

**Table 4:** Main results (survey data)

| | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| | Paid in cash | Earnings (log) | Underreports | Informal | Employed |
| Cash intensity × Pre period [$k \in [-7, -4]$] | -0.00221 | -0.00347 | 0.0261 | -0.0253 | -0.00422 |
| | (0.0263) | (0.0401) | (0.0162) | (0.0262) | (0.0161) |
| Cash intensity × Phase-in [$k \in [0, 5]$] | -0.109*** | -0.00201 | -0.0119 | 0.0120 | -0.0145 |
| | (0.0274) | (0.0356) | (0.0109) | (0.0215) | (0.0125) |
| Cash intensity × Short-run [$k \in [6, 11]$] | -0.507*** | -0.0599* | -0.0254* | 0.0538** | -0.0139 |
| | (0.0332) | (0.0347) | (0.0141) | (0.0249) | (0.0172) |
| Cash intensity × Long-run [$k \in [12, 16]$] | -0.637*** | 0.00459 | -0.0296*** | 0.0731*** | 0.00769 |
| | (0.0344) | (0.0363) | (0.00869) | (0.0255) | (0.0213) |
| Observations | 165498 | 165530 | 164695 | 280738 | 410757 |
| Clusters | 75 | 75 | 75 | 75 | 75 |

*Notes*: This table shows OLS estimates from pooling several $\beta_k$ coefficients from equation 1. Cash intensity is the pre-reform share of workers paid in cash. Phase-in pools the $\beta_k$ coefficients for the phase in period ($k \in [0, 5]$). Short-run pools the $\beta_k$ coefficients for the first 6 quarters in the mandate period ($k \in [6, 11]$). Long-run pools the $\beta_k$ coefficients for the last 5 quarters in the mandate period ($k \in [12, 16]$). In column 1, the dependent variable is an indicator equal to 1 if the reports receiving their wage payment in cash. In column 2, the dependent variable is the natural logarithm of labor earnings. In column 3, the dependent variable is a dummy variable equal to 1 if the worker is informally employed. In column 4, the dependent variable is a dummy variable equal to 1 if the worker underreports their earnings for tax purposes. All calculations are done using the survey sampling weights. All specifications include sector fixed effects, industry-by-time fixed effects, and region-by-time fixed effects. Standard errors are clustered at the sector level * Significant at the 10% level ** Significant at the 5% level *** Significant at the 1% level.

**Table 5:** Main results heterogeneity (survey data)

| | (1) Earnings (log) | (2) Informal | (3) Underreports |
|---|---|---|---|
| Small firm | -0.424*** | 0.190*** | 0.0277*** |
| | (0.0478) | (0.0566) | (0.00707) |
| Small firm × Cash intensity | 0.311*** | 0.327** | 0.0572*** |
| | (0.0867) | (0.149) | (0.0118) |
| Small firm × Cash intensity × Pre period [$k \in [-7, -4]$] | 0.0126 | -0.0305 | 0.0201 |
| | (0.0418) | (0.0229) | (0.0149) |
| Small firm × Cash intensity × Phase-in [$k \in [0, 5]$] | 0.00233 | 0.00682 | -0.0153 |
| | (0.0385) | (0.0195) | (0.0123) |
| Small firm × Cash intensity × Short-run [$k \in [6, 11]$] | -0.0414 | 0.0268 | -0.0371** |
| | (0.0385) | (0.0207) | (0.0152) |
| Small firm × Cash intensity × Long-run [$k \in [12, 16]$] | 0.0150 | 0.0487** | -0.0455*** |
| | (0.0410) | (0.0205) | (0.00893) |
| Large firm × Cash intensity × Pre period [$k \in [-7, -4]$] | 0.00176 | -0.0272 | 0.0210 |
| | (0.0410) | (0.0236) | (0.0158) |
| Large firm × Cash intensity × Phase-in [$k \in [0, 5]$] | -0.0110 | -0.00290 | -0.00577 |
| | (0.0402) | (0.0204) | (0.0120) |
| Large firm × Cash intensity × Short-run [$k \in [6, 11]$] | -0.0305 | 0.0332 | -0.0128 |
| | (0.0423) | (0.0222) | (0.0137) |
| Large firm × Cash intensity × Long-run [$k \in [12, 16]$] | 0.00835 | 0.0431* | -0.00145 |
| | (0.0413) | (0.0244) | (0.0112) |
| Observations | 165530 | 280738 | 164695 |
| Clusters | 75 | 75 | 75 |

*Notes*: This table shows OLS estimates from pooling several $\beta_k$ coefficients from equation 1. Cash intensity is the pre-reform share of workers paid in cash. Phase-in effect pools the $\beta_k$ coefficients for the phase in period ($k \in [0, 5]$). Short-run effect pools the $\beta_k$ coefficients for the first 6 quarters in the mandate period ($k \in [6, 11]$). Long-run effect pools the $\beta_k$ coefficients for the last 5 quarters in the mandate period ($k \in [12, 16]$). In column 1, the dependent variable is an indicator equal to 1 if the reports receiving their wage payment in cash. In column 2, the dependent variable is the natural logarithm of labor earnings. In column 3, the dependent variable is a dummy variable equal to 1 if the worker is informally employed. In column 4, the dependent variable is a dummy variable equal to 1 if the worker underreports their earnings for tax purposes. All calculations are done using the survey sampling weights. All specifications include sector fixed effects, industry-by-time fixed effects, and region-by-time fixed effects. Standard errors are clustered at the sector level * Significant at the 10% level ** Significant at the 5% level *** Significant at the 1% level.

# The Role of Cash in Illegal Labor Market Practices: Evidence from Uruguay

## Appendix

Marcelo Bérgolo[1], Javier Feinmann[2], Maximiliano Lauletta[3]

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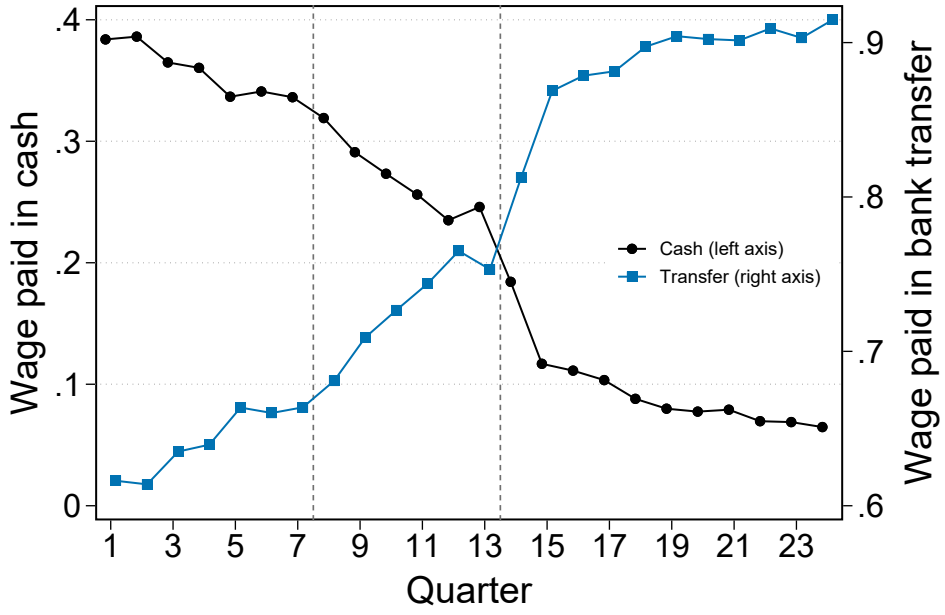
# A. Additional figures

**Figure A.I:** Share of workers paid in cash and bank transfer over time




*Notes*: This figure shows a the proportion of formal workers that are paid in cash (in black corresponding to the left axis) and the proportion that are paid with a bank transfer (in blue corresponding to the right axis) in each quarter. The first dashed line indicates the beginning of the phase-in period and the second dashed line indicates the beggining of the mandate period.

**Figure A.II:** Cash-intensiveness pre-reform (residualized from industry fixed effects)


*Notes*: This figure shows a histogram of the pre-reform sector-level share of workers who are paid in cash, residualized from industry fixed effects. The sector is defined at the 2-digit ISIC code level and the industry is defined at the 1-digit ISIC code level.

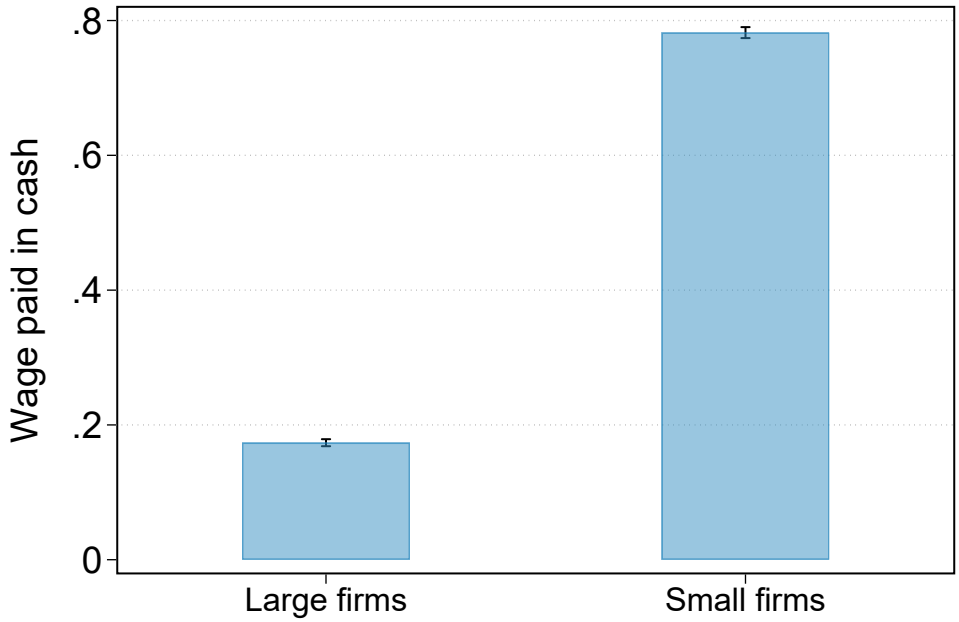
**Figure A.III:** Correlations between cash use and illegal labor market practices

**(a)** Informal employment


**(b)** Wage evasion

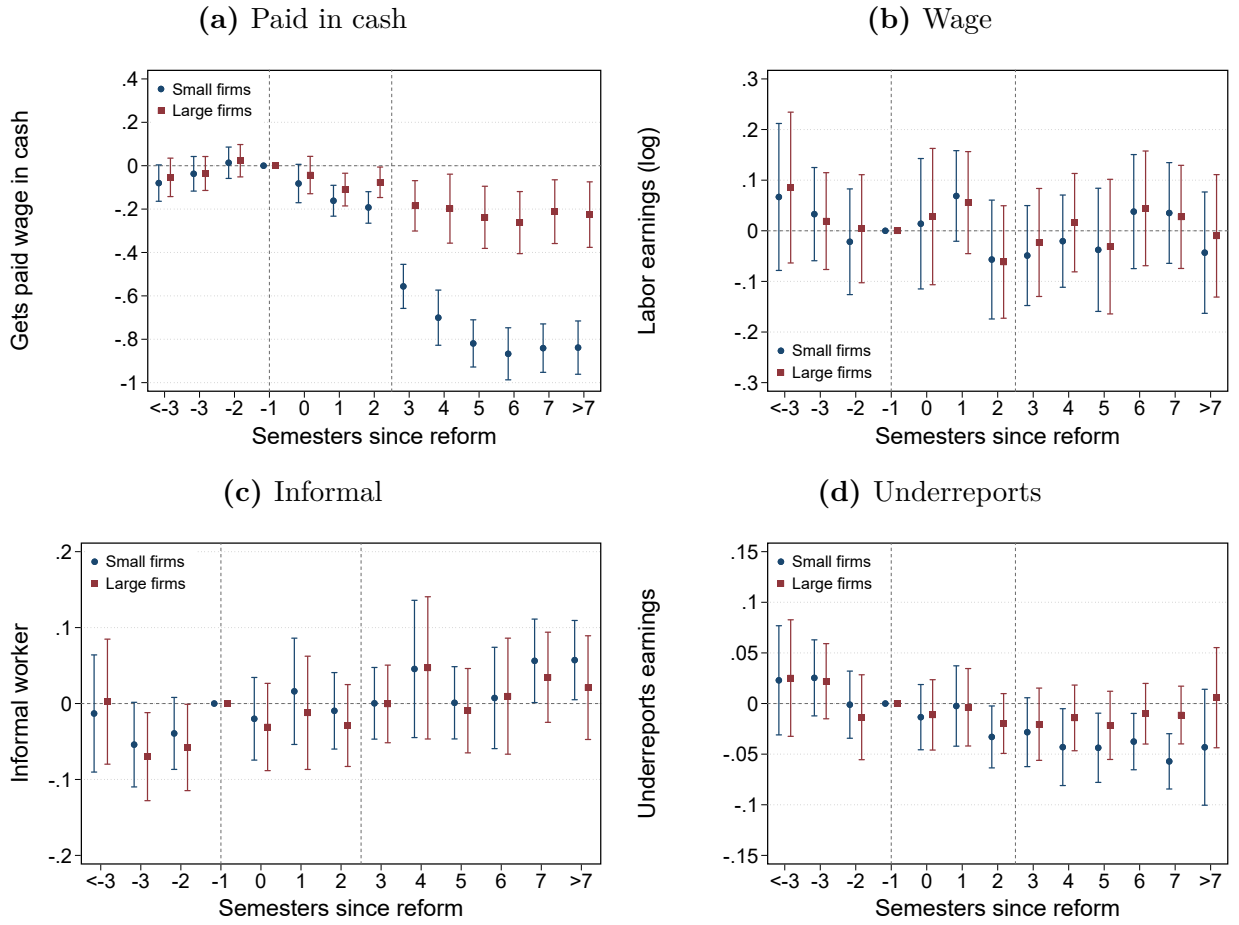

*Notes*: This figure shows binned scatterplots of the sector-level spread of illegal labor market practices and the share of formal workers that reports being paid in cash in the year prior to the reform. Panel (a) shows the share of workers who are informally employed and panel (b) shows the share of formal employees who admit to underreporting their labor earnings for tax purposes.

**Figure A.IV:** Proportion of workers paid in cash by firm size




*Notes*: This figure shows the proportion of workers that report being paid in cash for each corresponding firm size in the pre-reform year. Small firm corresponds to firms with fewer than 20 workers and large firm corresponds to 20 or more. Vertical bars represent 95% confidence intervals from robust standard errors.

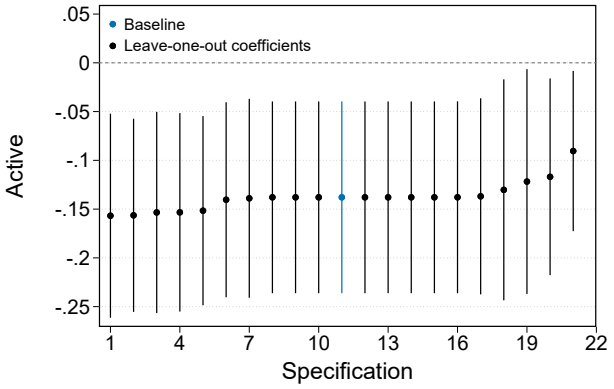
**Figure A.V:** Heterogeneous effects of the reform - firm size (survey data)



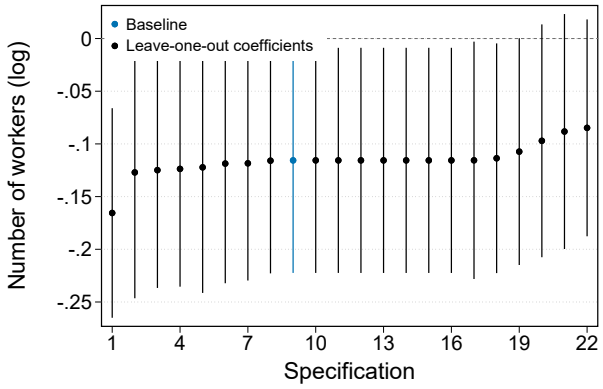

*Notes*: This figure shows OLS estimates of the $\beta_k$ coefficients from equation 1 pooled by semesters interacted with an indicator for small firm and an indicator for large firm. Blue coefficients correspond to small firms (less than 20 employees) and red coefficients correspond to large firms (20 or more employees). In panel (a) the dependent varialbe is an indicator equal to 1 if the worker is paid in cash. In panel (b) the dependent variable is the natural logarithm of the wage. In panel (c) the dependent variable is an indicator equal to 1 if the worker is informally employed. In panel (d) the dependent variable is an indicator equal to 1 if the worker underreports their labor earnings for tax purposes. The first dashed line indicates the beginning of the phase-in period and the second dashed line indicates the beggining of the mandate period. Capped vertical bars represent 95% confidence interval from standard errors clustered at the sector level.

**Figure A.VI:** Leave-one-out robustness checks - Administrative data

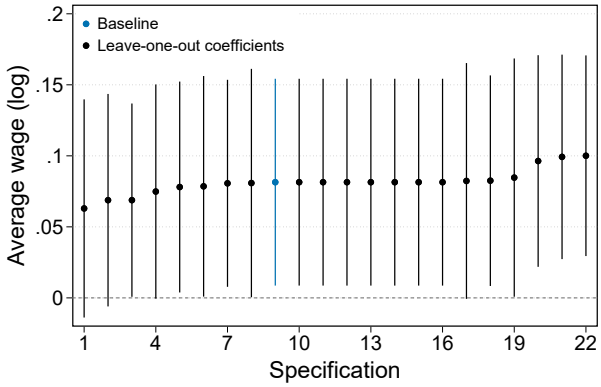
**(a)** Firm is active




**(b)** Number of workers



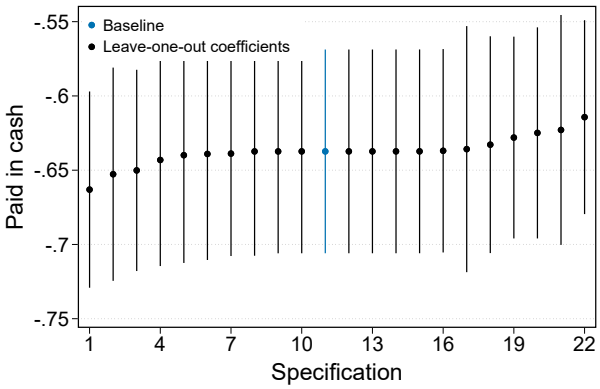

**(c)** Average wage



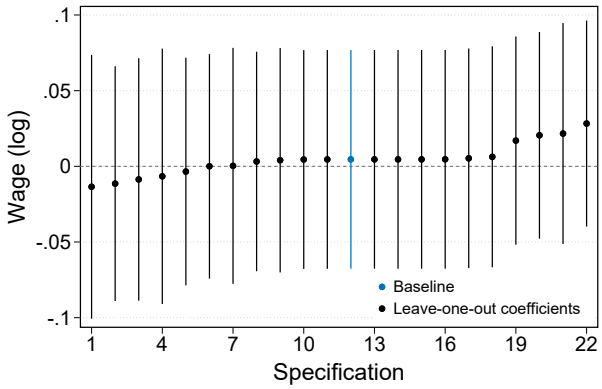

*Notes*: This figure reports OLS estimates of the pooled $\beta_k$ coefficients from equation 1 for $k \in [12, 16]$ sequentially dropping each industry using administrative data. Baseline coefficients are reported in blue and leave-one-out coefficients are reported in black. In panel (a) the dependent variable is an indicator equal to 1 if the firm is active. In panel (b) the dependent variable is the natural logarithm of the number of employees on payroll. In panel (c) the dependent variable is the natural logarithm of the average wage of the firm. Specifications are sorted by coefficient size in each panel and numbers need not correspond to the same specification across panels. Vertical bars represent 95% confidence interval from standard errors clustered at the sector level.

**Figure A.VII:** Leave-one-out robustness checks - Survey data

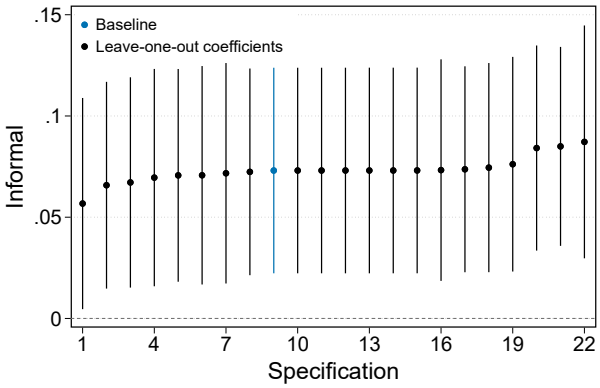
**(a)** Paid in cash



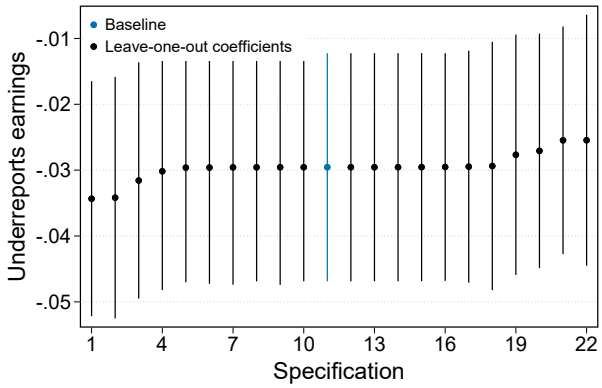

**(b)** Wages




**(c)** Informal



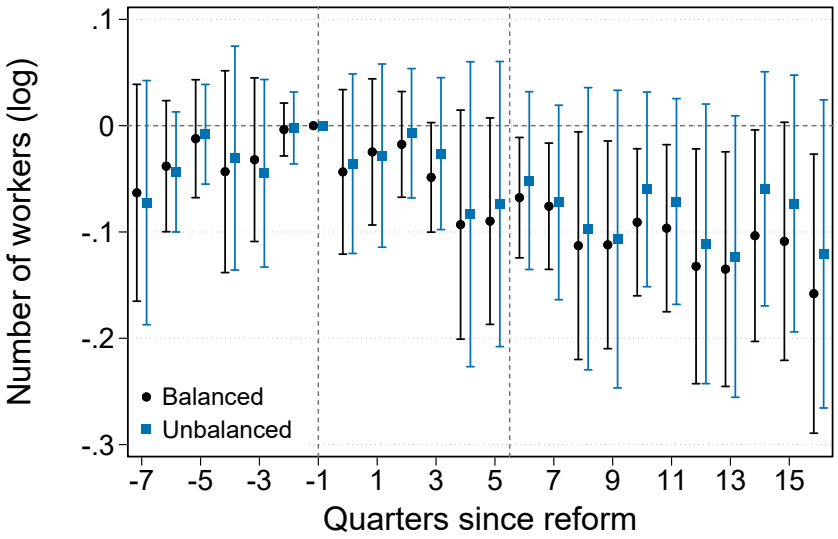

**(d)** Underreports



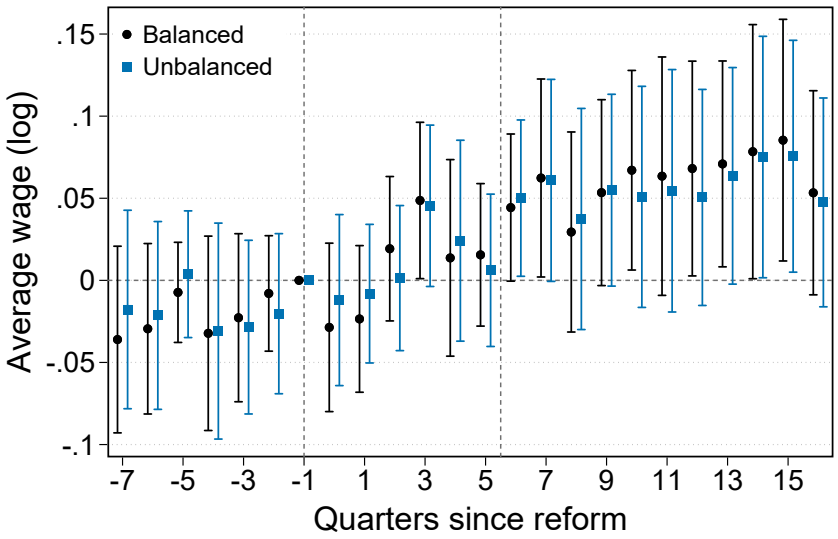

*Notes*: This figure reports OLS estimates of the pooled $\beta_k$ coefficients from equation 1 for $k \in [12, 16]$ sequentially dropping each industry using survey data. Baseline coefficients are reported in blue and leave-one-out coefficients are reported in black. In panel (a) the dependent variable is an indicator equal to 1 if the worker's wage is paid with cash. In panel (b) the dependent variable is the natural logarithm of the wage. In panel (c) the dependent variable is an indicator equal to 1 if the worker is informal. In panel (d) the dependent variableis an indicator equal to 1 if the worker underreports their earnings for tax purposes. Specifications are sorted by coefficient size in each panel and numbers need not correspond to the same specification across panels. Vertical bars represent 95% confidence interval from standard errors clustered at the sector level.

**Figure A.VIII:** Balanced panel of firms robustness check

**(a)** Number of workers

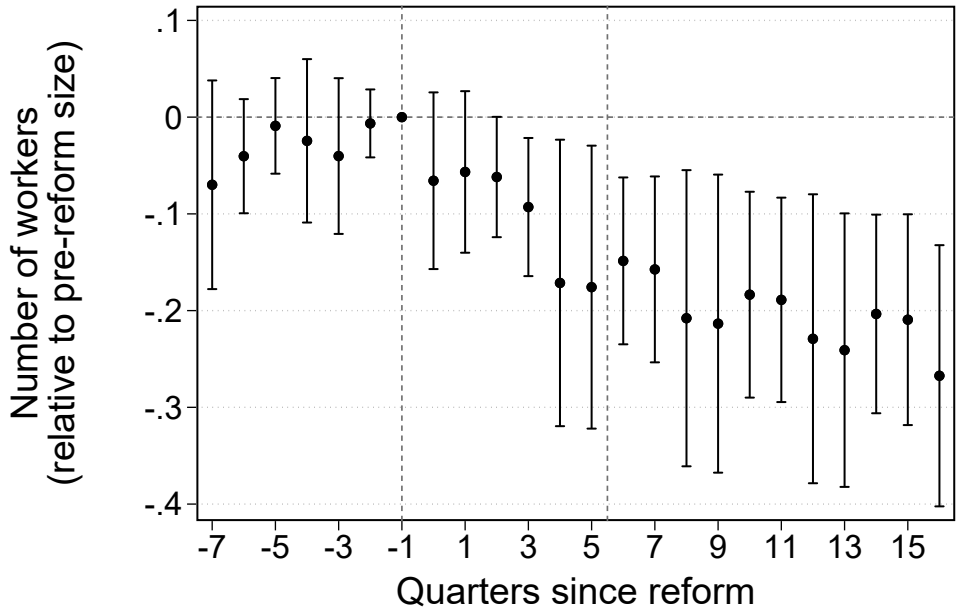



**(b)** Average wage




*Notes*: This figure shows OLS estimates of the $\beta_k$ coefficients from equation 1 using the full sample of firms and restricting to the balanced panel of always-active firms. In panel (a) the dependent variable is the natural logarithm of the number of employees on payroll. In panel (b) the dependent variable is the natural logarithm of the average wage of the firm. Coefficients in black correspond to baseline estimates using the balanced panel of always active firms. Blue corresponds to estimates using the full sample of firms. The first dashed line indicates the beginning of the phase-in period and the second dashed line indicates the beggining of the mandate period. Capped vertical bars represent 95% confidence interval from standard errors clustered at the sector level.

**Figure A.IX:** Number of workers (extensive and intensive margin combined)




*Notes*: This figure shows OLS estimates of the $\beta_k$ coefficients from equation 1. The dependent variable is the number of workers employed in the firm relative to the average number of workers employed during the pre-reform period. Inactive firms are coded as having zero workers. The first dashed line indicates the beginning of the phase-in period and the second dashed line indicates the beggining of the mandate period. Capped vertical bars represent 95% confidence interval from standard errors clustered at the sector level.

# B. Additional tables

**Table B.I:** Unweighted regression robustness check (survey data)

| | (1) Paid in cash | (2) Earnings (log) | (3) Underreports | (4) Informal | (5) Employed |
|---|---|---|---|---|---|
| Cash intensity × Pre period [$k \in [-7, -4]$] | -0.000596 | 0.00714 | 0.0237 | -0.0301 | -0.00471 |
| | (0.0277) | (0.0404) | (0.0168) | (0.0242) | (0.0161) |
| Cash intensity × Phase-in [$k \in [0, 5]$] | -0.116*** | 0.000598 | -0.0131 | 0.00849 | -0.0156 |
| | (0.0264) | (0.0365) | (0.00959) | (0.0214) | (0.0127) |
| Cash intensity × Short-run [$k \in [6, 11]$] | -0.510*** | -0.0676* | -0.0264* | 0.0445* | -0.0138 |
| | (0.0337) | (0.0354) | (0.0143) | (0.0239) | (0.0173) |
| Cash intensity × Long-run [$k \in [12, 16]$] | -0.647*** | 0.00925 | -0.0295*** | 0.0733*** | 0.0110 |
| | (0.0344) | (0.0375) | (0.00879) | (0.0276) | (0.0222) |
| Observations | 165498 | 165530 | 164695 | 280738 | 410757 |
| Clusters | 75 | 75 | 75 | 75 | 75 |

*Notes*: This table shows OLS estimates from pooling several $\beta_k$ coefficients from equation 1. Cash intensity is the pre-reform share of workers paid in cash. Phase-in pools the $\beta_k$ coefficients for the phase in period ($k \in [0, 5]$). Short-run pools the $\beta_k$ coefficients for the first 6 quarters in the mandate period ($k \in [6, 11]$). Long-run pools the $\beta_k$ coefficients for the last 5 quarters in the mandate period ($k \in [12, 16]$). In column 1, the dependent variable is an indicator equal to 1 if the reports receiving their wage payment in cash. In column 2, the dependent variable is the natural logarithm of labor earnings. In column 3, the dependent variable is a dummy variable equal to 1 if the worker is informally employed. In column 4, the dependent variable is a dummy variable equal to 1 if the worker underreports their earnings for tax purposes. All specifications include sector fixed effects, industry-by-time fixed effects, and region-by-time fixed effects. Standard errors are clustered at the sector level * Significant at the 10% level ** Significant at the 5% level *** Significant at the 1% level.