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**Wage effects of
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Wage Effects of Means-Tested Transfers: Incidence Implications of Using Firms as Intermediaries*

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Abstract

We show that how countries disburse tax credits matters for economic incidence. We exploit a reform in Argentina that shifted the disbursement of child benefits from employers to the government in a staggered fashion. Using administrative data and an event-study approach, we find that employers receive 5 to 13 percent of the transfers through reduced wages when they mediate the payments. This wage effect is more pronounced for low-income workers, particularly new hires, and in smaller and less unionized firms. We argue that workers likely misperceived firm-disbursed transfers as part of their work compensation, leading to incidence-sharing effects. Our findings suggest that relying on firms as intermediaries in the tax-benefit system can have unexpected labor market consequences.

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KEYWORDS: tax credits, means-tested transfers, incidence, wage effects, event study

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1 Introduction

Most countries provide some type of financial aid to families with children. An extensive literature has analyzed the work disincentive effects of tax credits and family allowances, as well as the impact on children's outcomes such as education and health (e.g., see [Moffitt, 2016](#)). However, less is known about the effects of other features of these programs, such as payment timing, conditionalities, or disbursement methods. In particular, the empirical question of who bears the economic incidence of work subsidies and family allowances remains poorly understood ([Nichols & Rothstein, 2015](#)). While it is generally assumed that individuals receive the entire benefit, in practice, employers might capture part of it through reduced wages.¹ In this paper, we bring new evidence to the discussion by examining whether the disbursement method of family allowances (tax credits) affects workers' gross wages.² We exploit a unique reform in Argentina that gradually shifted the *disbursement responsibility* of family allowances from employers to the social security administration (SSA), while keeping other program design features unchanged.

In Argentina, registered wage earners with children under 18 years old are entitled to a monthly family allowance (*Asignaciones Familiares*). This in-work, means-tested program for low-income workers provides a fixed transfer per child, which decreases through a wage earnings-based *notched* schedule with three brackets.³ Historically, employers disbursed these transfers, netting payments from their social security contributions (SSC) before remitting to the tax authority. In 2003, to improve transparency, the government decided to phase out firms' intermediary role and begin depositing transfers directly into workers' bank accounts. Due to the administrative complexity, this transition occurred gradually over eight years, from 2003 to 2010. Crucially for identification, the SSA determined the firm-specific switching dates through a series of public memos posted online, leaving employers with no control over their transition timing.

The staggered roll-out of the new payment system, along with changes in transfer information and salience for both workers and firms, provides ideal variation to cast light on the labor market consequences of tax credit disbursement methods. We identify wage effects using an event-study approach that aligns firms at their switching date and compares, within firms, the monthly pre-tax and pre-transfer wages of

¹This concept is analogous to grocery stores potentially increasing prices to capture part of Food Stamp transfers ([Goldin et al. , 2022](#), [Hastings & Washington, 2010](#), [Jaravel, 2018](#)), or the incomplete pass-through of government subsidies into lower health insurance premiums ([Cabral et al. , 2018](#)).

²Throughout this paper, 'wage' refers to monthly wage earnings, not the wage rate.

³This transfer is similar to the U.S. EITC but features *notches* instead of *kinks* and is paid monthly rather than annually. The old firm-based system resembles the Advance EITC, established in 1975 and repealed in 2010 (see [Jones, 2010](#)).

workers with and without children before and after that date. Our null hypothesis examines whether benefit disbursement methods affect wages or not. If the remitter is irrelevant, as in [Slemrod \(2008\)](#), we would expect no changes in monthly wages after the transition. Conversely, if employers were capturing part of the child benefits via incidence channels, then monthly wages should rise once firms stop disbursing the transfer.

We utilize rich, population-wide administrative data encompassing the universe of private and public wage earners registered in Argentina's social security system from 2003 to 2010. Employers report these data monthly to the tax authority, providing high-frequency variation with firms switching to the new system over 96 consecutive months. The data contain monthly information on total wage earnings (before taxes and transfers), social security contributions, zip codes, and various demographic and firm characteristics. Crucially, we observe the exact amount of the monthly transfer each worker received before the firm switched to the new payment system. This is because under the SFC, firms had to report the number of workers receiving the benefit and the amount paid to each in order to deduct the transfer from payroll taxes. We combine this employer-employee panel with another dataset of family relationships, allowing us to link workers with their spouses and children. The latter provides each child's exact date of birth, enabling us to accurately flag workers with eligible and ineligible children.

Our main findings reveal that the way cash transfers are paid out affects gross wages, challenging conventional wisdom and canonical incidence models. The graphical evidence compellingly shows that before firms transition to the new government-based system, the monthly wages of workers with and without children follow similar trends. However, once firms stop administering the transfers, the average monthly wage of workers with children increases by approximately 5 pesos relative to those without children. This wage effect occurs instantly and continues to grow over time, reaching an average of 13 pesos two years after the event. This result holds across various robustness checks. Additionally, the wage effect diminishes as we move up the income distribution, where the transfer's amount and salience are smaller. Specifically, at the 25th percentile, the wage differential between workers with and without children presents a sizable jump after the event, while the 75th percentile remains roughly unaffected. Regarding magnitude, our estimates suggest that employers were capturing about 5-13 percent of the transfers through lower monthly wages when they mediated the disbursement in the *old* firm-based system.⁴

We argue that benefit misperception plays a central role in the observed effects.

⁴This pass-through rate is calculated by scaling the reduced-form effect by the average child benefit amount of 90 pesos per month (roughly 10 percent of average wages) just before the transition.

In the old firm-based system, child benefits were “bundled” with monthly wage payments and itemized on paper pay slips, resulting in higher overall paychecks. This setup may have led workers to mistakenly perceive the State benefit as part of their salary, as suggested by anecdotal evidence, prompting them to accept lower monthly *pre-transfer* wages.⁵ In contrast, the State-based payment system did not include the benefit on pay slips, and the SSA deposited it directly into employees’ bank accounts. Employers were also required to notify employees and new hires about the new system via a signed form, which likely clarified that child benefits are a State program, distinct from work compensation. As workers stopped conflating the benefit with their salary and recognized it as a State-funded transfer, they began to demand higher pre-transfer salaries.

A key piece of evidence supporting our interpretation is that wage effects are more prominent for newly hired workers and muted for incumbents. Intuitively, new hires’ contracts are set at the time of hiring, so once firms stop disbursing the transfer, there is far less room for confusion, leading to higher wages for new hires with children. In contrast, contract rigidities may limit wage adjustments for incumbent workers (Pissarides, 2009). We also find smaller wage effects in large firms, where hiring processes are more structured and transparent due to human resources management. In contrast, small firms tend to be less organized and face less oversight from labor unions and the SSA. Indeed, wage effects are more pronounced in firms with low unionization rates and are insignificant in firms with over 50 percent unionization. This remarkable finding suggests that unions may help limit the incidence sharing of in-work tax credits and other benefits (as theorized by Lee & Saez, 2012).

In the final part of the paper, we discuss several alternative mechanisms, including deliberate rent-seeking by employers, bargaining due to pay equity concerns, administrative costs associated with disbursement, discrimination against workers with children, and take-up costs. Although these channels are hypothetically plausible, they do not fully align with our empirical evidence.

Our findings suggest that the design of tax credit programs, like the EITC in the U.S., matters and influences the final economic incidence. We demonstrate that wages respond to how transfers are disbursed, rejecting the null hypothesis that workers fully capture the transfers dollar for dollar. This important result brings attention to a common yet understudied issue. For instance, employers currently disburse child benefits in countries like Brazil, Chile, Paraguay, Peru, Greece, Italy, and Switzerland.⁶ Our results point to a potential economic cost of decentralizing sensitive tasks prone

⁵We rationalize this wage effect result by adding a benefit misperception parameter to the standard Gruber (1997) incidence model (Section 2.3 and Appendix E.1).

⁶In Appendix C, we document other global experiences of firm-mediated transfers.

to confusion, such as the disbursement of fiscal benefits—an aspect previously overlooked in the literature. More broadly, this study suggests that involving firms as intermediaries in the tax-benefit system may lead to unintended consequences.

This paper contributes to the literature on incidence, broadly, and specifically on the incidence of tax credits (Fullerton & Metcalf, 2002, Kotlikoff & Summers, 1987, Summers, 1989). While the traditional partial-equilibrium incidence model suggests that tax burdens are determined solely by supply and demand elasticities, a growing body of research has highlighted the importance of other factors, such as salience, remittance and compliance costs, market rigidities and imperfect competition, and the direction of tax changes (Benzarti, 2024, Benzarti *et al.*, 2020, Chetty *et al.*, 2009, Kroft *et al.*, 2023, Slemrod, 2008, Weyl & Fabinger, 2013). For instance, Kopczuk *et al.* (2016) find that the identity of the tax remitter influences tax incidence in the U.S. diesel fuel market. Relatedly, research on payroll taxes has questioned the traditional view that statutory incidence does not affect final incidence (Cahuc *et al.*, 2019, Saez *et al.*, 2012, 2019), with recent studies also examining the role of tax-benefit linkages on pass-through (Bozio *et al.*, 2023). Unlike existing studies, our paper focuses on transfers for which minimal evidence exists, particularly in developing countries. The most intriguing aspect of our study lies in examining a change in the payment system (specifically, the *disbursement responsibility*) while keeping other features of the benefit schedule unchanged.

To our knowledge, four papers closely relate to ours, each making valuable efforts to estimate the incidence of in-work subsidies in developed economies. Rothstein (2010) and Leigh (2010) examine the EITC in the U.S., while Azmat (2019) analyzes the Working Family Tax Credit (WFTC) in the U.K., and Zurla (2024) studies the Italian *80 Euros Bonus*. These studies suggest that employers capture about 30-36% of the transfers through reduced wages. However, despite these efforts, identifying wage effects remains a significant challenge, and the evidence is still inconclusive (Nichols & Rothstein, 2015). We build on this body of work and help advance the understanding of this topic by combining rich administrative data and a novel staggered design, enabling us to identify wage effects more precisely and shed light on underlying mechanisms. Most notably, we provide compelling graphical evidence, arguably representing the first non-parametric causal evidence of wage effects in the context of tax credits.

We also contribute to the tax salience literature (Chetty *et al.*, 2009). Recent studies have analyzed behavioral responses to tax visibility or transparency using quasi-experimental settings (e.g., Bradley & Feldman, 2020, on tax-inclusive airline pricing) and lab experiments (e.g., Feldman & Ruffle, 2015). Most research focuses on increased salience, understood as greater tax visibility. An exception is Finkelstein (2009), who analyzed the introduction of electronic tolls, which decreased tax salience.

More closely related to our work, [Azmat \(2019\)](#) finds that wage effects from the WFTC are mainly driven by increased visibility to employers rather than the transfer amount itself. Additionally, our explanation for wage effects based on imperfect understanding of the benefits system aligns with [Feldman *et al.* \(2016\)](#), who show that tax complexity can cause confusion and lead to unintended behavioral responses.

The paper is structured as follows. Section 2 describes the child benefit program, the staggered change in the remittance system, and outlines the conceptual framework. Section 3 introduces the data sources employed in our analysis. The empirical strategy and main findings are presented in Section 4. Section 5 examines the mechanisms underlying the observed wage effects. Finally, Section 6 provides concluding remarks.

2 Institutional setting, reform, and framework

2.1 Family allowances in Argentina (AAFF)

The Argentine family allowance program, *Asignaciones Familiares* (AAFF), is the country's largest means-tested cash transfer. Established in 1996 under Law 24,714, it provides a monthly child benefit to private sector employees with children under 18, whose monthly wages fall below a specified threshold. The benefit amount depends on the number of children and is phased out discontinuously across three monthly wage brackets (i.e., a *notched* scheme). For instance, for a worker with two children at the upper end of the first bracket, the transfer represents 16 percent of the monthly wage, but if she earns slightly more, it drops to 12 percent (she would receive AR\$30 per child instead of AR\$40).⁷ The AAFF program is financed through employer social security contributions (SSC), which are set at 7.5 percent of monthly wages.

The context of economic growth and high inflation that Argentina experienced from 2004 onwards makes our setting particularly interesting, because it makes it easier for employers to get some rents in real terms. This is because, while nominal wages are typically downward rigid, real wages might not be. Moreover, wages are renegotiated more often because of persistent inflation.⁸ Figure I.1b describes the evolution of the upper bracket thresholds from 2003 to 2011, jointly with the evolution of the

⁷Figure I.1a presents the parameters defining the AAFF transfer scheme for the early years of our data. In Figure I.38, we display the distribution of monthly wages and find no evidence of bunching around these discontinuities, suggesting no labor supply responses or strategic collusion between employers and employees. For more details, see Appendix A and H.2.

⁸Appendix B presents a more detailed discussion of Argentina's macroeconomic context during our analysis period. In a nutshell, the economy was booming during this period, and wages were adjusted regularly to keep up with inflation.

minimum wage, which serves as a reference point. Note that the nominal increase in the thresholds is a consequence of inflation. In addition, the minimum wage always lies below the lowest bracket, leaving some space for the eventual shift of part of the incidence (as noted by [Lee & Saez, 2012](#)).

2.2 The reform: A staggered change in the payment system

The policy variation we exploit in this paper arises from a reform that changed how transfers are disbursed, which was gradually rolled out between 2003 and 2010. We summarize the old and new systems in [Figure I.2](#) and describe them below. Importantly, this reform solely changed the delivery mechanism of the transfers without affecting the total amount received by workers. The benefit schedule, frequency and time of payments, funding, and other features remained unchanged.

The old system (SFC). In the original scheme of the AAFP program, child benefits were disbursed directly by employers to eligible employees with children. Under this payment system, called *Sistema de Fondo Compensador* (SFC), employers could net out the transfer's amount from the employer's SSCs before remitting the remaining balance to the tax authority. If the transfer exceeded the firm's SSC bill, the employer could request a refund. In this setup, employers acted as intermediaries, simply disbursing funds from the public sector. A notable feature of this system is that it was mandatory to include the transfer as an item on workers' pay slips (as shown in [Figure I.3\(a\)](#)).

The new system (SUAF). The *Sistema Unico de Asignaciones Familiares* (SUAF), introduced in June 2003 (Memo 641/2003 ANSES), replaced the SFC system by having the Argentine SSA (ANSES, for its acronym in Spanish) directly deposit child benefits into workers' bank accounts, removing firms as intermediaries. Employers' only remaining duty was to remit monthly SSC to the tax authority, and they lost access to effective beneficiaries and benefit amounts, especially for new hires. The subsidy also disappeared from pay slips, reducing its visibility to employers ([Figure I.3\(b\)](#)). The reform aimed to increase transparency, ensure payments reached beneficiaries, prevent fraud, and ease firms' administrative burden ([Marasco, 2007](#)). Crucially for our empirical strategy, firms were gradually incorporated into the new SUAF system from June 2003 to June 2010 due to SSA's limited operational capacity, which facilitates an event-study design (as detailed in [Section 4](#)).

Incorporation process. The incorporation of firms into the new system was determined by the SSA through a series of memos posted online, leaving employers with no control over when the switch would occur. This makes the switching date a plausibly exogenous event from both the firm’s and the worker’s perspective. The process unfolded as follows.⁹ The SSA first established a transition schedule, listing the firms set to be incorporated by a specific date (Figure I.28). Firms were then contacted by an SSA officer and asked to submit relevant documentation (e.g., payroll records, beneficiaries, workers’ bank account numbers). After verification, the final step involved the formal approval and incorporation of the firm into the new system (Figure I.29). Workers could verify the new disbursement method online (e.g., direct deposit or in person at a nearby bank branch) and adjust it if needed. Both employers and employees could also track the firm’s incorporation status on the SSA website (Figure I.30).

What changed for employers and employees? First, information about the family allowance program was significantly improved. Employers were required to inform all employees about the company’s incorporation into SUAF and explain how the system worked within ten business days. This process had to be repeated for new hires. Both employers and workers—regardless of their eligibility—had to sign an official form, with each party keeping a copy (see Figure I.31). By signing, employees acknowledged the new system and committed to updating their employer about any changes in family composition, which the firm transmits monthly to the tax authority (Figure I.32). As a result, the reform did not affect the availability of information about family composition for either current employees or new hires.

Another major practical change was that employers no longer handed the transfer to employees, and the benefit was no longer itemized on pay slips (Figure I.3). Under the previous system, firms had precise information about how much subsidy each worker received, and it was mandatory to list the transfer on pay slips. This is relevant for the incidence analysis, as it may have led workers to mistakenly believe that the benefit was part of their work compensation, and thus funded by the firm. In Section 2.3, we provide anecdotal evidence supporting this interpretation.

Importantly, incumbent workers did not have to take any additional action to continue receiving the transfer, as it was automatically processed. This addresses concerns about incomplete take-up. In Figures I.5 and I.6 we further show that AAFF spending and the number of beneficiaries did not decline during the transition, suggesting that, on the whole, workers continued to receive the transfers without any negative impact on take-up.

⁹See Appendix D for further details on the incorporation process.

Overall, the reform and required paperwork reinforced that the AAFF family allowance was a government program, separate from employee compensation.

2.3 Conceptual framework: Incidence under misperceptions of benefits

We aim to test whether the delivery method of tax credits affects pre-tax-and-transfer wages. Conceptually, if the remitter is irrelevant, as in [Slemrod \(2008\)](#), we would expect no changes in monthly wages after the reform. Conversely, if employers were—purposely or inadvertently—capturing part of the child benefits via incidence channels, then monthly wages should rise once firms stop disbursing the transfer.¹⁰ But how can this happen?

In practice, wage effects could arise when benefits are “bundled” with monthly wage payments, leading workers to perceive them as part of their overall compensation package. We formalize this idea in [Section E.1](#) using a simple incidence model with benefit misperceptions, building on [Gruber \(1997\)](#) and [Bozio *et al.* \(2023\)](#), which closely aligns with our findings. We assume that workers perceive a monthly wage $\tilde{w} = w_q \cdot (1 + (1 - q) \cdot \tau^e)$, which is a function of the true monthly wage w_q , the transfer rate disbursed by employers τ^e , and a perception parameter $q \in [0, 1]$. We define $\tau^e = \bar{\tau} - \tau^g$, where τ^g is the transfer disbursed by the SSA (the government), and $\bar{\tau}$ is the total transfer. Thus, under the old system $\bar{\tau} = \tau^e$ and in the new system $\bar{\tau} = \tau^g$. When $q = 1$, workers fully recognize that child benefits are government-funded and separate from their compensation, regardless of the disbursement system; when $q = 0$, workers completely confuse wages and benefits.

What are the potential wage effects in these two extreme scenarios? Under perfect awareness ($q = 1$), workers understand that the transfer is indeed not part of their compensation, viewing employers as intermediaries, and so the perceived wage equals the true wage $\tilde{w} = w_1$. In this case, the standard incidence model holds, meaning the entity responsible for disbursing the benefit does not affect who bears the final burden. Conversely, with benefit misperception ($q = 0$), workers view their compensation as a combo that includes the transfer disbursed by the firm $\tilde{w} = w_0 \cdot (1 + \tau^e)$, where $w_0 < w_1$. We argue that $q = 0$ under the firm-based disbursement system, and thus, employers shift part of the benefits through lower wages.¹¹

A key prerequisite of our proposed framework is the limited understanding of how family allowances work and how they are funded. This confusion was indeed

¹⁰Note that we do not know the baseline incidence levels of the transfer. We estimate the change in incidence due to the shift in the remitter.

¹¹[Section 5.2](#) discusses other plausible mechanisms that do not align well with our evidence.

present both before and during the reform. The situation before the reform is well-documented in a book compiled by the SSA: “...the old system (SFC) blurred the State’s role as the entity responsible for the benefits. (...) The roles were confused. People believed these benefits were part of their salary and that employers were responsible for them. They even ignored that the State funds them...” (Marasco, 2007). Before the reform, benefits seemed poorly understood, with only partial awareness of their true nature. Furthermore, a 2018 survey conducted by the SSA suggests that this misperception has not been fully eliminated. Over 50 percent of respondents stated they do not know who is responsible for paying the benefit, 35 percent correctly identified the government, and 8.6 percent still believed employers were funding it (Table A5).

What are the implications for our empirical analysis? If part of the transfer was shifted to employers under the old SFC system, we would expect to see an increase in the monthly wages of workers with children relative to ineligible workers once firms stop disbursing the allowance. This could, in turn, affect the relative employment ratios between these two groups. Although we do not explicitly model it, we anticipate that such wage effects would be more prevalent in small firms and those with low unionization rates. In larger firms, where human resources departments typically handle hiring, compensation is usually more transparent. Furthermore, unions may limit the economic incidence of tax credits in highly unionized firms (as conceptually noted by Lee & Saez, 2012). Lastly, under wage rigidity, these effects could be more pronounced for new hires, as incumbent workers are likely to have existing contracts by the time the change occurs (e.g., see Hazell & Taska, 2024). We empirically explore this heterogeneity in Section 5.

3 Administrative data

Wage earnings data (SIPA). Our main data source is the employer-employee panel compiled by the Ministry of Labor and known as SIPA. Employers report these data monthly through Form 931 (the equivalent of Form 941 in the U.S.). All firms must use the same online processing software, SICOSS, with a simple interface that makes it a reliable source. It contains social security records for the universe of registered wage earners in Argentina. It has a comprehensive set of variables, including pre-tax and transfer monthly wages, employee social security contributions, sector, region, zip code, age, and gender among other characteristics. Our dataset spans the period 2003-2010.¹²

¹²This version of SIPA is processed by *Observatorio de Empleo y Dinámica Empresarial* (OEDE-MTEySS). All the records were de-identified so that workers and firms remain anonymous. We accessed the databases at the Argentine Ministry of Labor (MTEySS).

Notably, our rich data allow us to track which employees received transfers and the exact amounts up to the month a firm transitions to the new SUAF system. Employers were required to report this information monthly to offset transfers against payroll taxes, but once a firm transitions to SUAF, these fields are filled with missing values. This is key for estimating the first stage, i.e., the decline in employer-disbursed benefits, and for identifying the switching date. Additionally, the dataset’s monthly reporting, uncommon in many countries, is particularly useful for capturing wage variation throughout the year and tracking the monthly variation in treatment timing (i.e., the event).

Family links (ADP). We combine the SIPA data with another database that contains family relationships. These data allow us to accurately link workers to their dependents (spouse and children) since the 1970s. In Argentina, applicants must register and report their family composition to claim social benefits or deduct dependents from one’s income tax. Using workers’ identifiers, we can merge these data with SIPA and determine each worker’s marital status and number of dependents. The workers appearing in SIPA but not ADP are considered single with no children. Importantly, for our estimation strategy and the definition of the treatment group, we observe each offspring’s exact date of birth.

Table A2 presents some descriptive statistics for the year 2004. In 2004, Argentina had approximately 5 million private wage earners and about 400,000 firms. About 25 percent of the registered workers received AAFP child benefits. Most of these beneficiaries were in the lower and middle brackets, with an average tax credit rate of 13 percent and 7 percent of the salary, respectively. Additionally, the average number of children is two across the earnings distribution.

4 Empirical strategy and results

4.1 Event definition and empirical roll-out

We implement an event-study approach that leverages the staggered transition of firms into the SUAF payment system. We define the *event* as the month-year t when a firm is incorporated into SUAF and stops disbursing family allowances, with the last observed payment in the micro-data occurring at month $t - 1$. To accurately identify the event date using the employer-employee micro-data we focus on firms that meet the following criteria: (i) they were paying family allowances for at least six months before the event, (ii) they existed at least six months before and six months after the event $[-6;5]$, (iii) had more than one worker that was receiving the transfer at

$t - 1$, and (iv) they had workers both with and without children throughout the event-window. Our estimation sample comprises an unbalanced panel of firms observed between January 2003 and December 2010, including only those that experienced an event and transitioned before the 2008 crisis.¹³

Figure 1 illustrates the empirical roll-out based on our micro-data. We plot the share of firms disbursing transfers under the old SFC system and the share of workers receiving transfers through that system. As depicted in panel (a), the transition was gradual.¹⁴ Panel (b) further shows that large firms were the first to switch to the new system. Although the SSA determined the switching date, they initially seemed to prioritize larger firms. Nevertheless, as we explain next, our empirical strategy exploits within-firm variation to mitigate any potential selection issues.

Our setting is rich, offering substantial variation and heterogeneity for analysis. We observe thousands of firms experiencing the event (a large number of treated units), spanning 96 consecutive months (time variation). Additionally, we observe heterogeneity in firm size (ranging from micro to large firms), the share of beneficiaries per employer (intensity of treatment at the firm level), and the number of children for whom workers receive transfers (treatment intensity at the worker level).

4.2 Estimation strategy

Our estimation strategy consists of comparing, within each firm, workers with and without children before and after the event. The control group C consists of workers without children who are therefore not eligible for child benefits, and the treatment group T consists of workers with at least one child less than 18 years old (although not all of them are eligible; eligibility depended on workers' wages and the working status of the spouse).¹⁵

¹³The post-August 2008 roll-out period is less suitable for an event study due to several factors: (i) bunching of events with a large number of companies simultaneously switching to the new system in August 2008, (ii) the onset of the financial crisis, which makes the transfer and the change in the payment system less operative, (iii) a period marked by fewer hires with potential differential impacts on small and large firms, (iv) in 2009, the family allowance program was expanded to include informal workers (AUH for its acronym in Spanish). See Appendix B for more details on the macroeconomic context during 2003-2010.

¹⁴In Figure I.7 we use aggregate official budget information to show the gradual decline in the share of family allowances paid through the SFC (old system) as a proportion of total spending. We also calculated the total disbursed through the SFC using the micro-data and compared it to the macro-level totals. Both values align closely, confirming the high quality of family allowance payments micro-data (see Figure I.5).

¹⁵Workers can change treatment status over time either as (a) their youngest child turns 18 or (b) there is a newborn. To avoid workers switching treatment status, in one of the robustness checks, we identify treated workers as those with at least one child born between 1992 and 2002. These workers were fully treated from 2003 to 2010 because their children will be [1-11] in 2002 and [8-18] in 2010. The rest of the workers belong to the control group, that is, they are either never treated or partially treated.

We exploit within-firm variation before and after the transition into the new system as follows. For each firm f , group $g = C, T$, and month-year t , we compute the average wage ($\bar{w}_{f,t}^g$) and, to keep things simple, we take the difference across groups within a given firm and month ($G_{f,t}^{\bar{w}} = \bar{w}_{f,t}^T - \bar{w}_{f,t}^C$). This specification allows us to control for time-varying unobserved firm heterogeneity, which is not generally possible in many designs, i.e., it is quite hard to have a setting where treated and control workers coexist within a firm. Consequently, for each firm, we end up with one time series of wage gaps between workers with and without children, allowing us to run the following standard event-study specification:

$$G_{f,t}^{\bar{w}} = \alpha + \sum_{j=-13}^{12} \gamma_j \cdot d_{f,t}^j + \epsilon_{f,t} \quad (1)$$

where $d_{f,t}^j$ are event-time indicators that the event happened j months away. Note that $j = 0$ is the first month in which the firm no longer disburses the transfer and, as is generally done, we take $j = -1$ as the omitted category in our estimations and figures. Importantly, this specification is numerically equivalent to having two observations per firm (the average wage for workers with and without children) and including firm-by-time fixed effects, because the coefficients are identified by differencing them out.¹⁶

Moreover, in our estimations, we also add firm and month-year fixed effects to account for the change in the composition of firms over time and to control for time-specific shocks. Thus, we propose the following final specification:

$$G_{f,t}^{\bar{w}} = \sum_{j=-13}^{12} \gamma_j \cdot d_{f,t}^j + \mu_f + \mu_t + \epsilon_{f,t} \quad (2)$$

To compute the reduced-form point estimates in our tables, we pool all the coefficients before and after the switching date and then take the difference. We do so in a regression framework, allowing us to obtain standard errors:

$$\begin{aligned} G_{f,t}^{\bar{w}} = & \beta_1 \cdot Window_{f,t} + \beta_2 \cdot Window_{f,t} \cdot Post_{f,t} \\ & + \beta_3 \cdot (1 - Window_{f,t}) \cdot Post_{f,t} + \mu_f + \mu_t + \epsilon_{f,t} \end{aligned} \quad (3)$$

where $Window_{f,t}$ is an indicator equal to one for the *event window* and zero for the binned end points, and $Post_{f,t}$ is an indicator equal to one for the months after the event.

¹⁶In Section F we explain the econometric specification in greater detail. In Table A4 we present baseline characteristics for eligible and ineligible workers at $j = -1$.

We examine a two-year window around the switching date (one year before and one after), binning the end points as is standard in the literature (Schmidheiny & Siegloch, 2019), and clustering standard errors at the firm level. This time frame captures a full calendar year, allowing us to account for wage dynamics related to seasonality, as well as wage negotiations or labor agreements, typically updated annually. As a robustness check, we vary the time window and show very stable results.

Dependent variables. To construct the wage gap, we use a monthly wage variable (before taxes and transfers), which is used to calculate employers' social security contributions. As in most countries, this variable is right-censored due to a cap on SSC, but since the cap is above the 95th percentile in all months analyzed, it does not compromise our results. In addition to the average wage, we compute other moments such as the 25th, 50th, and 75th percentiles.

We use the same framework to compute the first-stage change in the transfer, using the monthly transfer gap of workers with and without children as the dependent variable. Finally, to compute the pass-through rate, we use the Wald estimator to scale the reduced-form relative to the first-stage effect. We estimate this pass-through with a two-stage least squares method (2SLS) to get the correct standard errors.

4.3 First stage, reduced form, and pass-through

We begin by estimating the first-stage change in family allowance remittances before and after the event. We run specification (1) using the difference in average transfers paid by employers to workers with and without children as the dependent variable. The estimated γ 's, represented by the blue-triangle series in Figure 2, show that when firms transition to the new system, they immediately stop disbursing the transfer, which is then taken over by the government. Before the event, workers with children received, on average, 90 pesos more in employer-disbursed transfers than those without children—approximately 10% of average monthly wages. It is worth emphasizing that workers do not lose the transfer after the switch; it is simply paid directly by the government. Crucially, however, it is no longer managed by employers. For clarity, the red-dot series simulates how government-paid transfers under the new scheme would look.¹⁷

In Figure 3, we look at the reduced-form effect on monthly wages (before taxes and

¹⁷To our knowledge, the SUAF data with government-disbursed transfers remains inaccessible to researchers. However, our assumption that workers continued receiving child benefits aligns with aggregate statistics reported in Figures I.5 and I.6, which show no decline in total child benefit spending and the number of beneficiaries over time.

transfers) relative to period $t - 1$. Panel (a) shows the average wage levels when we estimate equation (2) separately for workers with children (treatment) and without children (control), $\bar{w}_{f,t}^T$ and $\bar{w}_{f,t}^C$, respectively. In Panel (b) the dependent variable is the within-firm average wage gap of these two groups, $G_{f,t}^{\bar{w}}$. The first panel, in levels, constitutes a simple plot of the raw wages of both groups before and after the reform, while the second panel, the wage gap, nets out all potential confounders within each firm.

The reduced-form results in Figure 3 provide compelling evidence that the pre-tax-and-transfer monthly wage of eligible workers responds to changes in the transfer payment system. The monthly wage of workers with children increases relative to those without children immediately after their employer stops disbursing the transfer, with the gap growing over time. Reassuringly, the average wage does not differ between treated and control workers before the transition (relative to the last month in the old system).

In Figure 4, we analyze moments of the wage distribution beyond the average for each group ($\bar{w}_{f,t}^T$ and $\bar{w}_{f,t}^C$). The figure shows that wage increases are primarily driven by workers at the lower end of the monthly wage distribution ($p25$), with a more muted effect for upper-middle-wage workers ($p75$), where the transfer's share of the total wage and its salience are lower. The $p25$ wage differential between workers with and without children exhibits a sizable jump after the event, while the $p75$ wage gap remains relatively unchanged.¹⁸

In Table 1, we report the reduced-form and pass-through estimates from the event study. The reduced-form and first-stage point estimates correspond to difference-in-differences coefficients, reflecting the difference between pooled coefficients pre- and post-event. The 2SLS is the Wald estimate, where we scale the reduced-form by the first-stage change in the transfer. This summary of previous figures shows that, after a firm switches to the new regime, the monthly wage of eligible workers increases by 5 pesos relative to those ineligible. In terms of pass-through, our estimates indicate that a 1 peso decrease in the transfer disbursed by employers (holding the total transfer constant) results in a 5-cent increase in the monthly wage, implying an incidence of around 5 percent for those at the average of the monthly wage distribution. The last column of Table 1 examines wage effects up to two years after firms transition to the new system (also shown in Figure I.9). The longer-run effect of 12.7 pesos results in a pass-through rate of about 13 percent.

Robustness checks. Our results are robust to a set of validation exercises. First, we show that they are not affected by modeling choices (Table A6). The point estimates are

¹⁸Intuitively, this exercise estimates the wage gap of two line workers ($p25$) and two executives ($p75$) with and without children within each firm.

fairly stable with no fixed effects, with firm and time fixed effects, or with firm-specific linear trends. Second, the results hold when using a balanced panel of firms present throughout the 96-month period (Figure I.11), adjusting the number of consecutive months firms were paying transfers prior to the event (Figure I.12), or varying the length of the event-time window (Figure I.13). Third, the results are preserved when focusing on workers who were fully treated during the 2003-2010 period, specifically those with children under 18 throughout the roll-out (Figure I.14). Additionally, our results are robust to controlling for firm structure (size and composition) in the main specification (Figure I.15), and to different estimation samples of firms (Figure I.16). Moreover, including never-treated firms in the event study does not alter the results (Figure I.17). Reassuringly, our findings align with estimates derived from five newer alternatives to TWFE regressions, which do not impose restrictions on treatment effect heterogeneity across groups and time (Figure I.18 and Appendix G.2). Lastly, we perform a placebo exercise by assigning a fake event date to each firm and re-estimating the reduced-form effects. After replicating this exercise 1,000 times, we find that our baseline estimate of the wage effect lies outside the 99 percent confidence interval of the simulated reduced-form estimates distribution (Figure I.19).

Composition and employment: For our wage effects to be interpreted causally, there must be no immediate changes in employment levels or the composition of workers with and without children within treated firms following the reform.¹⁹ Figure 5 examines these two margins using the same specification as in equation (2). The results reveal no immediate effect on the gap between eligible and non-eligible workers (panels a and b), nor on the total number of employees within the firm (panel c). Over time, however, firms tend to hire more workers without children compared to those with children, leading to an average reduction of one worker with children two years post-reform. This pattern is interpreted as a response to the change in the relative cost of these groups, as our wage effects suggest that workers with children become relatively more expensive over time after the reform.

Who ultimately bears the increase in wages? A thorough incidence analysis would require firm-level data containing information on profits, sales, prices, etc., to determine whether the increased wages are borne by workers, firms, or consumers. For instance, the increase in wages for eligible workers could come at the expense of other ineligible workers, capital owners through reduced profits, or even consumers via higher prices. Unfortunately, we lack access to the data needed to evaluate these pos-

¹⁹Firms could also react by hiring different types of workers, such as younger or more educated individuals. However, based on available data—specifically, the share of unionized, full-time, and female workers, along with a proxy for age—we find no meaningful changes in workers' composition post-reform (Table A7).

sibilities directly, and we leave this question for future research.²⁰

5 Potential mechanisms

Our main findings indicate that the way cash transfers are disbursed influences the final economic incidence, contrary to conventional wisdom and canonical incidence models. On average, employers capture about 5-13 percent of the transfers through lower monthly wages when they mediate transfer payments. The broader incidence literature often overlooks the mechanisms behind these outcomes, highlighting the importance of understanding what may drive the wage effects. In this section, we explore several potential channels. To preview our results, our preferred explanation centers on an imperfect understanding of how child benefits operate and are funded (confusion), though we cannot completely rule out some degree of rent-seeking by employers.

5.1 Confusion channel

Wage effects may arise when workers mistakenly perceive firm-disbursed State transfers as part of their total compensation, leading them to accept lower pre-transfer salaries (see Section 2.3). This benefit misperception is indeed supported by anecdotal evidence and a survey conducted by the SSA, as shown in Section 2.3 (e.g., the transfer was itemized on paper pay slips). Conversely, the shift in responsibility for remitting child benefits to the government could have enhanced employees' overall understanding of the benefit scheme. As some workers with children stop conflating the benefit with their compensation and recognize it as a government-funded transfer, they begin to demand higher salaries. This effect is especially likely for new hires, when employment contracts are initially negotiated (e.g., see Tortarolo *et al.*, 2020, for wage responses of new hires and incumbents to the income tax).

A central piece of evidence supporting our interpretation is that wage effects are mostly driven by new hires rather than incumbent workers. This is shown in Figure 6, where we run two different regressions: one including all workers (blue line) and another focusing on a balanced panel of employees present throughout the entire event

²⁰We provide indirect evidence by examining the total wage bill in firms with high and low treatment intensity, based on whether the share of workers with children at $t = -1$ is above or below the median, respectively. An upswing in the wage bill would indicate that employers bear the incidence, while a relatively steady trend would suggest a shift of the incidence to workers without children. In Figure I.10, we plot the event-study coefficients using the wage bill as the dependent variable and observe a relatively smooth evolution. This offers suggestive—albeit imperfect—evidence that co-workers may have ultimately paid the portion of the transfer previously captured by employers.

window (red line). The difference between these two lines captures the response of new hires.²¹ The figure shows no effect for incumbents, whose contracts and payment schedules are usually predetermined in the short term (Hazell & Taska, 2024, Pissarides, 2009). For new hires, however, contracts are set at the time of hiring, and once firms stop disbursing the transfer, there is far less room for confusion. Consequently, the monthly wages (before taxes and transfers) of new hires increase after the reform.

In Table 2, we further break down the average wage effect by firm size and business type (see Figure I.21 for dynamic estimates). The effect is larger in small firms with 10 or fewer employees. Specifically, for small incorporated businesses, the pass-through rate is -0.092, indicating that firms were getting about 9 percent of the transfer when they were responsible for disbursing it. Wage effects may be less prevalent in larger firms where hiring processes are typically managed by human resources departments, which tend to be more transparent. In contrast, small firms are often more disorganized and face less oversight from labor unions and the SSA.

Building on the point about labor union contract enforcement, we present evidence on unions' potential role in limiting the economic incidence of tax credits. We estimate 2SLS pass-through rates across different levels of firms' exposure to union regulations, measured by the percentage of unionized workers within each firm. Figure 7 summarizes the results, showing greater wage effects in firms with a low share of unionized workers and more muted effects in firms with over 50 percent unionization. This remarkable finding suggests that unions may help limit the incidence sharing of in-work tax credits and other benefits (e.g., see Lee & Saez, 2012).²²

Taken together, the results from Figures 6, 7, and Table 2 indicate that settings where firms have more flexibility to adjust new contracts (e.g., with new hires), settings with less rigid structures (e.g., in small firms), and those with less union monitoring (e.g., with low unionization rates) drive most of the observed wage effects.²³

²¹Our empirical strategy requires firms to have both workers with and without children during the full event window, which is why we infer new hires' behavior indirectly. Otherwise, we would place too much demand on the data, especially for small firms that would need to hire at least two workers every month—one with children and one without.

²²In some instances, there may have been discrepancies with respect to what collective bargaining agreements (CBAs) establish. Although CBAs typically specify monthly wages, in practice employers might have understood or implemented these agreements as wages plus transfers, especially in small firms. After the reform, this practice became less feasible because employers no longer disbursed the transfer.

²³For completeness, we also analyzed heterogeneities across sectors, but found rather similar point estimates across the different sub-samples of firms (see Figure I.20).

5.2 Potential alternative explanations

We explore several alternative interpretations that, while hypothetically plausible, do not fully align with our empirical evidence.

Rent seeking. Under the firm-based regime, employers may have *deliberately* exploited workers' limited understanding of child benefits by *purposely* offering eligible workers a compensation package that combined the transfer with a reduced monthly wage, compared to workers without children. In the new regime, where firms no longer handled the transfer, this rent-seeking opportunity vanished. This explanation, however, would predict a decline in labor demand for workers with children, with both employment and wages falling after the transition to SUAF (as depicted in Appendix E.2). While this prediction aligns with the employment evidence (Figure 5), it contradicts the observed wage effects (Figure 3).

The distinction between this channel and the one discussed in Section 5.1 is subtle. Both rely on an imperfect understanding of how family allowances function and are funded. However, under the rent-seeking (or labor demand) channel, employers deliberately extract rents, whereas in the confusion channel from Section 5.1, eligible workers inadvertently accept lower wages. Once workers recognize that child benefits are funded and disbursed by the State, they start demanding higher wages upon hiring. While we think both channels may be operating simultaneously, the evidence suggests that the explanation from Section 5.1 plays a more prominent role, as indicated by the increase in pre-transfer monthly wages.

Pay equity concerns and bargaining. Wage effects could arise at $t = 0$ if eligible workers, upon receiving direct government transfers, notice that their paychecks (now net of transfers) have decreased compared to their co-workers. This may lead them to complain to the employer and bargain more aggressively for higher compensation.

We discuss three reasons why pay equity concerns are unlikely to be the dominant channel. First, if this was purely an equity-bargaining story, we would expect the effect to arise gradually over time. However, we find an immediate effect at $t = 0$. Second, fairness concerns would mainly impact incumbent workers at the time of the event. Yet, we find no effect for this group and a larger effect on new hires. Third, one would expect pay equity concerns to be more pronounced in firms with a mix of eligible and ineligible workers. This would imply a U-shaped relationship between wage pass-through and a firm's exposure to family allowances, with stronger effects where around 50 percent of the workforce have children and weaker effects at the extremes. We test this hypothesis in Figure 8. Panel (a) shows the distribution of firms by their exposure to family allowances, and panel (b) shows the wage effects across different exposure bins. This analysis reveals an increasing—rather than U-shaped—

relationship, suggesting that incidence sharing is greater in firms with a high proportion of eligible workers with children. Given the relatively low pass-through rates, this result could imply that firms find it worthwhile to engage in rent-seeking—as outlined in the previous channel—when there is a large share of workers from whom to extract rents.

Administrative costs. Tasking firms with the disbursement of child benefits could impose administrative burdens, which may have contributed to lower monthly wages for eligible workers through standard incidence forces. This burden was alleviated when employers were no longer responsible for these payments, which was one of the motivations behind the reform (Marasco, 2007). However, if this were the primary channel, we would expect to see increases in both wages and employment (as shown in Appendix E.2). While we find evidence of wage increases, there is no corresponding rise in employment. Furthermore, in Appendix H.1, we provide suggestive evidence that the old payment system did not substantially impair firms’ finances. Specifically, using firm-level financial debt data, we observe no significant impact of the reform on firms’ delinquency rates (measured as overdue debt exceeding 90 days). This holds true even for small firms, where the wage effect is strongest (Figure I.37). In all, the ‘administrative relief’ channel is unlikely to be the main driver of the observed wage dynamics.

Discrimination against workers with children. Firms may discriminate against workers with children by paying them lower monthly wages if they perceive them as more fatigued, less productive, more likely to arrive late, or miss work, among other reasons. If employers only learn about a worker’s parental status when disbursing child transfer payments, any post-reform increase in monthly wages could indicate discrimination. However, we argue that this channel is unlikely for two reasons. First, as explained in Section 2.2, employees must report their family composition for tax purposes when hired, regardless of who disburses the transfer. Thus, employers retain the ability to flag workers with children (see Figure I.32). Second, Figure 4 shows that wage effects are concentrated among workers with children at the bottom of the earnings distribution (likely program recipients) rather than those at the upper end, who would also be subject to the discrimination channel if it existed.

Take-up costs. Wage effects under the new regime could arise if there is imperfect take-up of child benefits, perhaps due to transaction costs, lack of information, or stigma (Currie, 2006, Finkelstein & Notowidigdo, 2019). For instance, employers might decide to partially compensate workers who stop receiving the transfer (or face delays) by offering a higher monthly wage. However, this scenario is unlikely for two reasons. First, firms were required to inform both current employees and new hires about the new system, and workers did not need to take any additional steps to continue receiv-

ing the transfer, as it was automatically processed (see Section 2.2). Workers could also submit claims online on a dedicated interface, mitigating concerns about stigma and transaction costs. Second, our macro-level evidence indicates a steady rise in both the number of beneficiaries and total spending throughout the transition (Figures I.5 and I.6). Thus, take-up does not appear to be a major issue.

Market-level response and learning. A valid concern is whether our results are more related to a general equilibrium story, namely market-level effects, or to a firm-specific shock. For instance, if we observe that as more firms enter the new system, the treatment effect grows larger, then the wage effect is probably driven more by a market-level response. Alternatively, the way information about the new payment system was disseminated over time might also help to explain our results. For instance, firms that switched first, when there was little or no information available, might have been scared of potential retaliation, and therefore they compensated workers. Firms that switched afterward already had adjusted wages long before they switched because they learned about the new system. If so, then we should see a large effect at the beginning that declines over time. As time passes, the effect should gradually go to zero as everybody learns about the new system. This observation would be consistent with a *learning story*.²⁴

To this end, we track the dynamics of the wage effect as more firms transition into the new payment system. Figure 9 presents the 2SLS pass-through rates over time, with each dot representing a sample of firms that changes over time. We focus on firms that switched regimes within a 30-month window, advancing the window by one month at a time. For instance, the first point includes firms that switched to SUAF between July 2003 and December 2005 (the first 30 months of our panel). The next point shifts one month forward, excluding firms that switched in July 2003 and including those that transitioned in January 2006, and so on. Overall, we observe a relatively stable (or slight U-shaped) effect over time, which mitigates both the *learning story* and the market-level effect.²⁵

6 Conclusion

What happens when employers act as intermediaries between the government and tax credit recipients by directly paying benefits with paychecks? Our evidence shows

²⁴A Google Trends search for “SUAF” as a proxy for awareness of the new system, reveals no spike from early 2004 to the end of 2010, with searches remaining steady until a peak in June 2010, the final enrollment deadline.

²⁵In Appendix G.3, we also show that individual-level shocks do not affect the incidence of cash transfers. We estimate wage effects when workers become ineligible as their children reach adulthood and find no significant effects.

that the disbursement method of tax credits significantly affects economic incidence. We analyze a reform in Argentina that gradually shifted the disbursement responsibility of in-work cash transfers from employers to the SSA over eight years. Utilizing employer-employee administrative data and an event-study approach, we find that employers obtain 5 to 13 percent of the transfers through reduced *pre-transfer* monthly wages when they mediate the payments. This wage effect is more pronounced among low-income workers, particularly new hires, and in smaller and less unionized firms that can more easily adjust wages. We argue that workers mistakenly perceived these transfers as part of their salaries, leading to an incidence-sharing effect between workers and firms manifested as lower monthly wages.

Taken together, our findings indicate that the design of tax credit programs, such as the EITC in the U.S., can influence the final economic incidence, challenging conventional wisdom. We demonstrate that wage responses to transfer disbursement methods contradict the assumption that workers capture the full value of transfers dollar for dollar.

These results have practical implications, particularly given the increasing relevance of social protection systems globally. Numerous countries, both developing (e.g., Brazil) and developed (e.g., Switzerland), mediate transfers through employers without sufficient evidence or awareness of the potential economic costs. More broadly, this study highlights that involving firms as intermediaries in the tax-benefit system may lead to unexpected consequences, where less transparent schemes result in confusion and non-standard incidence effects.

Finally, [Blanchet *et al.* \(2022\)](#) argue that the disparity in inequality between European countries and the U.S. cannot be solely attributed to successful redistributive schemes (via taxes and transfers), as is often assumed. Instead, it is influenced by a more equitable pre-distribution of income (i.e., market earnings). Our paper extends this perspective by emphasizing that the implementation and design of redistributive tools, such as child benefits, can also significantly impact income pre-distribution.

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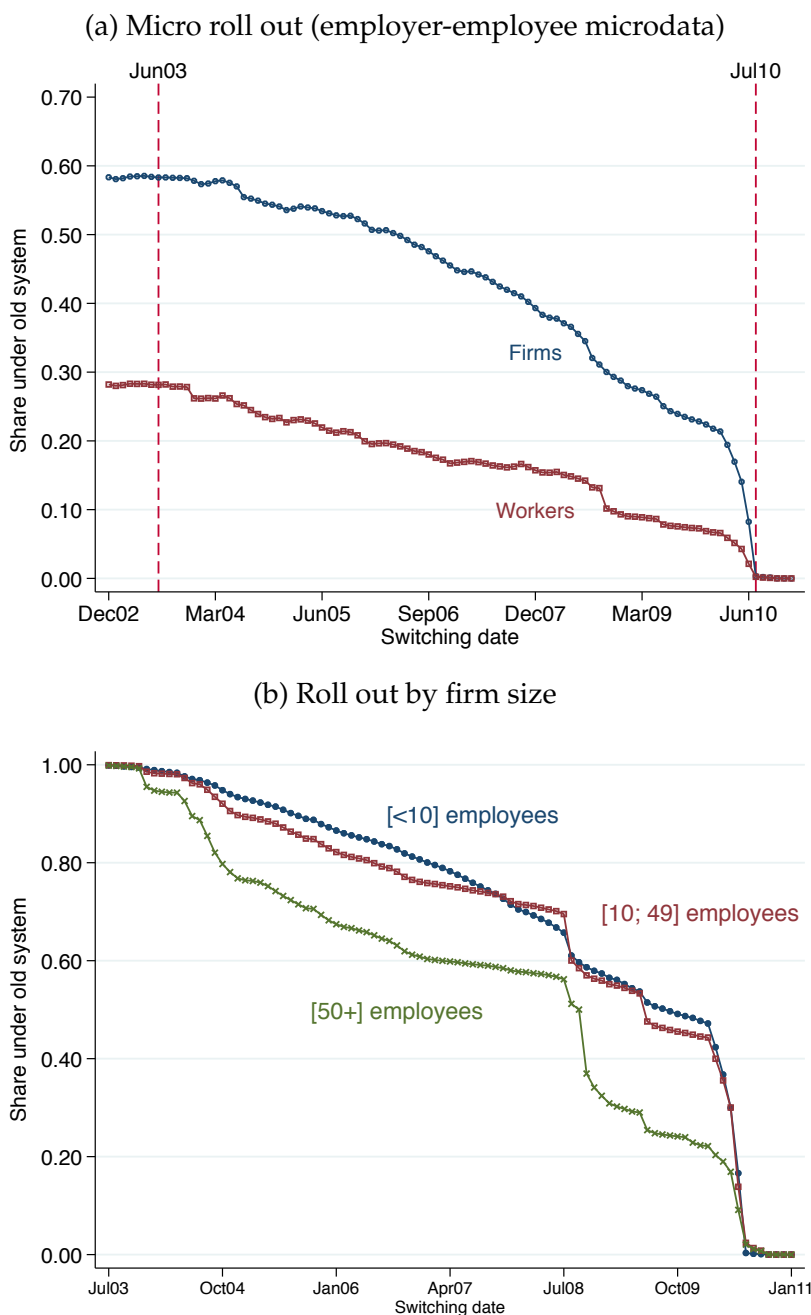
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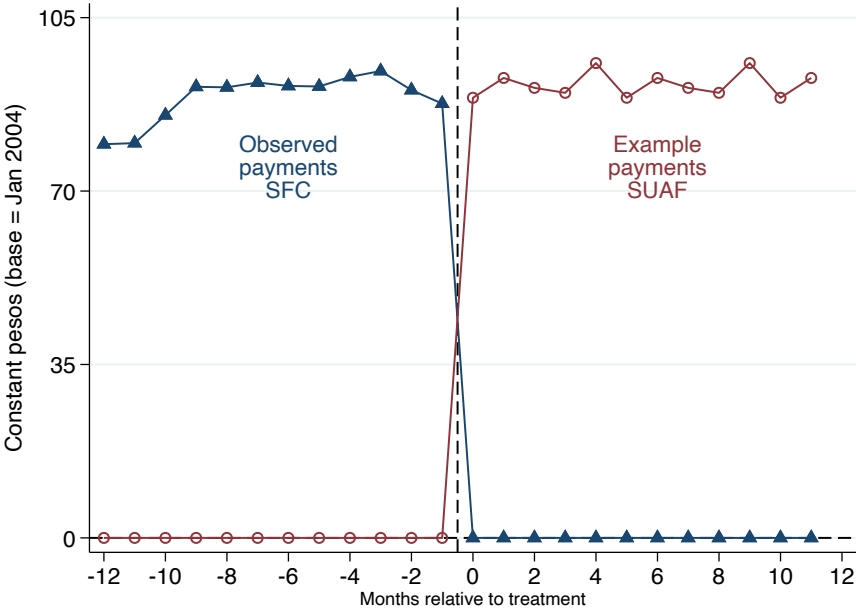
Figures and Tables

Figure 1: Gradual roll out from the old to the new system



Notes: This figure shows the gradual transition of firms and workers from the employer-based to the government-based payment system. Panel (a) focuses on all firms and workers in the micro-data. In January 2003, 40 percent of firms were not paying family allowances (a similar share is observed in 1998, based on an additional dataset we have access to). This share is divided into several categories: [i] micro firms with one or two employees, comprising approximately 20 percent; [ii] employers who never paid family allowances alongside wages, such as those in rural areas or with seasonal activities; [iii] some public firms or dependencies, and education-related institutions. Employees working for employers in category [ii] received the transfer directly from the government under the *pago directo* system. The rationale for excluding these employers from the old SFC system is that, due to their seasonal income flows and high transfer amounts for low-wage workers with many children, the government preferred not to require them to make monthly payments. This *pago directo* system also explains why 20 percent of payments were not processed through the SFC scheme at the beginning of the period, as shown in official budget information (see figure 1.7.) Panel (b) is restricted to our estimating sample and breaks down the roll out by firm size based on the number of employees in 2003.

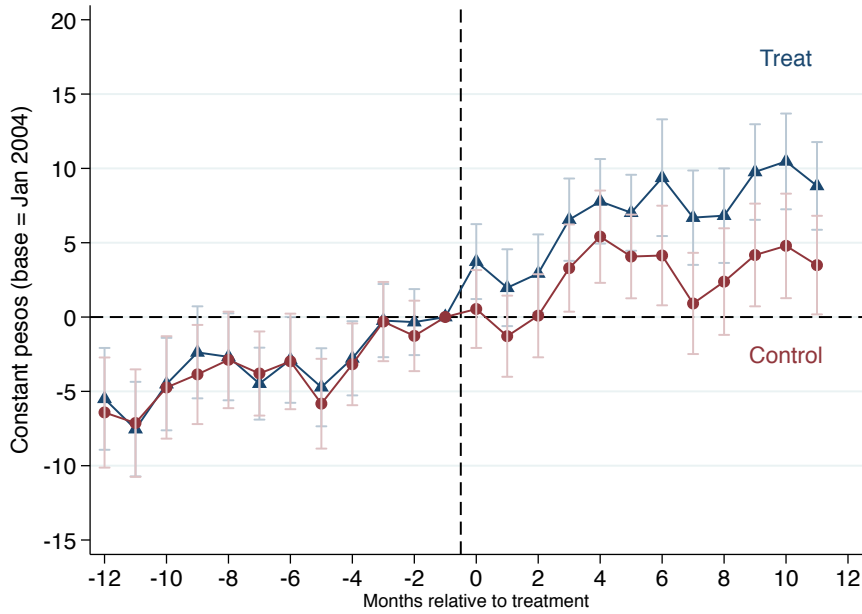
Figure 2: First-stage change in firms' remittance of child benefits



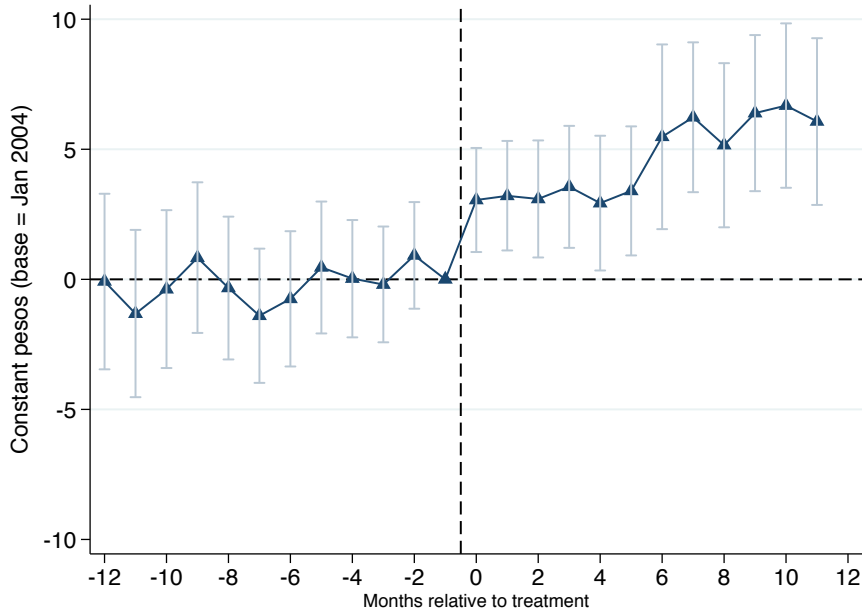
Notes: This figure shows the first-stage change in firms' remittance of child benefits. The blue-triangle series presents the event-study estimates of the parameter γ from equation (1), where the dependent variable is the within-firm difference in average family allowances between workers with and without children. The figure shows that when firms transition to the new system, they immediately stop disbursing the transfer, which is then taken over by the government. On average, workers with children were receiving approximately 90 pesos more in monthly employer-paid transfers than those without children. Additionally, the red-dot series simulates how payments under the new SUAF system would look.

Figure 3: Reduced-form wage effects

(a) Average wage levels

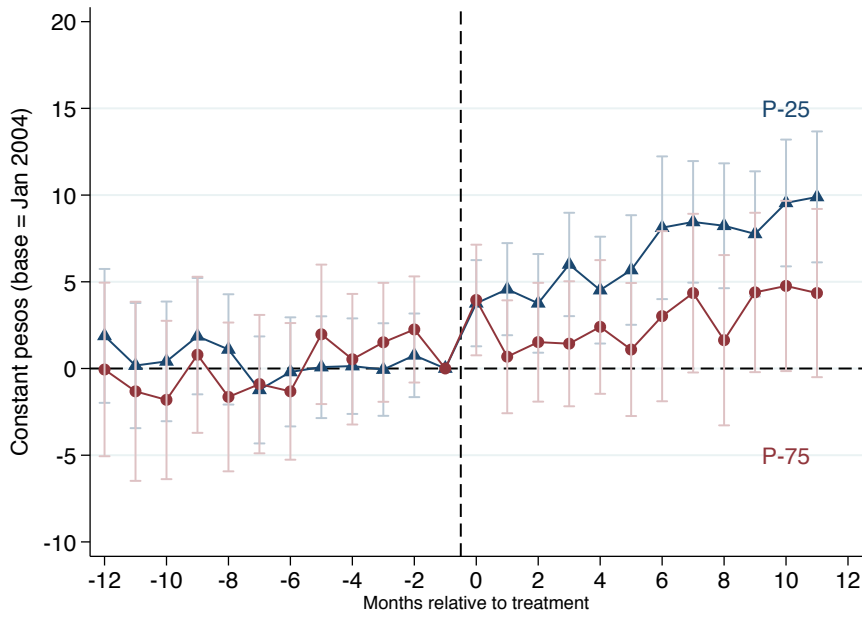


(b) Average wage gap



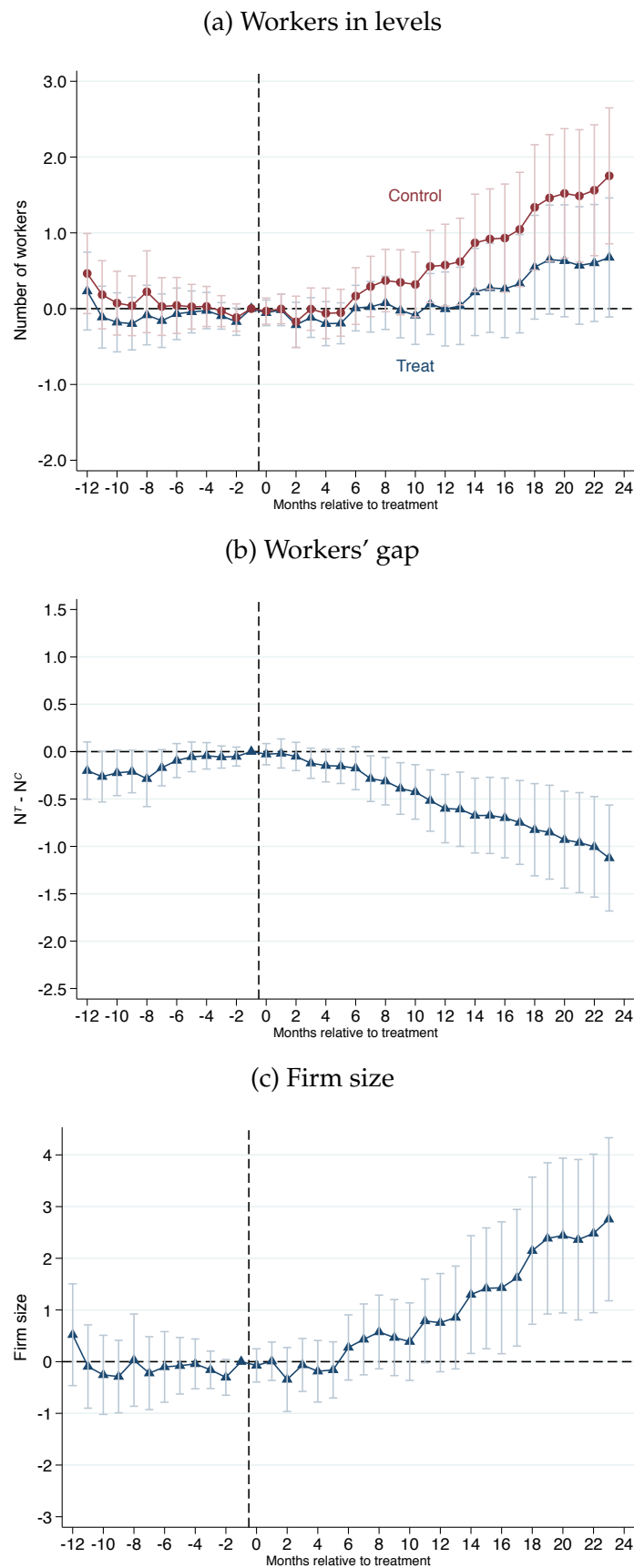
Notes: These figures plot the event-study estimates of parameter γ and its corresponding 95-percent confidence intervals of equation (2). Panel (a) shows the wage levels when we estimate this equation separately for workers with children (treatment) and without children (control). In Panel (b), the dependent variable is the within-firm average wage gap of these two groups. It shows that monthly wages increase by approximately 5 pesos when firms stop disbursing the transfer to eligible workers.

Figure 4: Reduced-form wage effects: p_{25} vs p_{75}



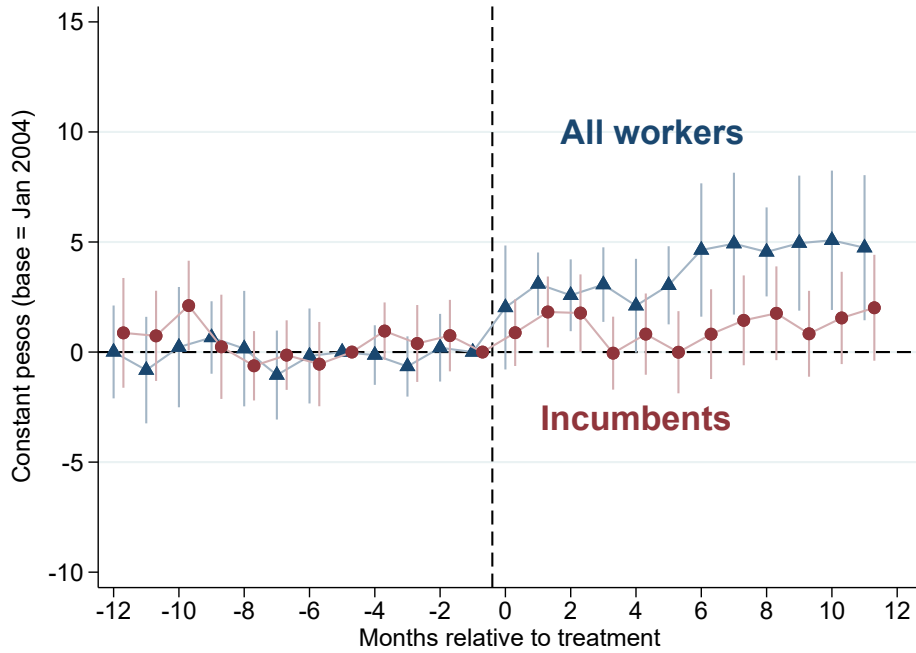
Notes: This figure plots the event-study estimates of parameter γ and its corresponding 95-percent confidence intervals of equation (2). We run two different regressions where the dependent variable is either the 25th or 75th percentile within each firm. It shows that monthly wages increase mostly at the lower end of the distribution and rather less in the upper part.

Figure 5: Composition of workers and firm size (24 months after the event)



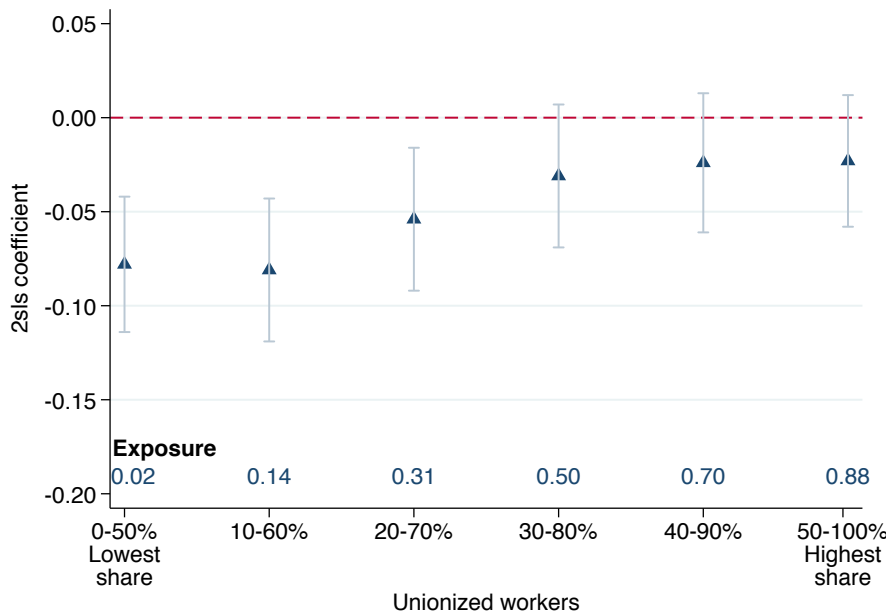
Notes: These figures plot the event-study estimates of parameter γ and its corresponding 95-percent confidence intervals of equation (2) considering 24 months after the event. Panel (a) shows the number of workers with and without eligible children in levels (treat and control, respectively); panel (b) considers the within-firm difference between treat and control workers as the outcome variable; panel (c) shows the firm size defined as the total number of workers within firms.

Figure 6: Wage effects: New hires and incumbents



Notes: This figure plots the event-study estimates of parameter γ and its corresponding 95-percent confidence intervals of equation (2). The dependent variable is the monthly wage gap between workers with and without children. We run two different regressions: the blue line includes all the workers while the red line only considers a balanced panel of employees present at the firm for the entire window (two years). The difference between these two lines captures the wage effect for new hires. We use the 2SLS estimator proposed by Freyaldenhoven *et al.* (2019) to account for potential confounders leading to a pre-event trend in the wage effect of incumbent workers. For more details see Appendix G.4.

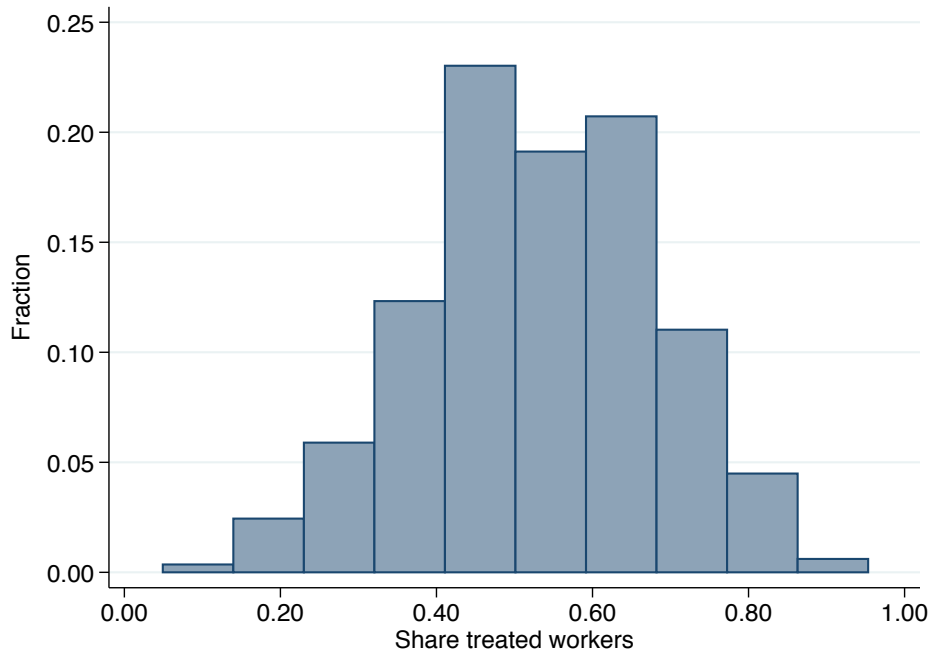
Figure 7: Pass-through by unionization rates



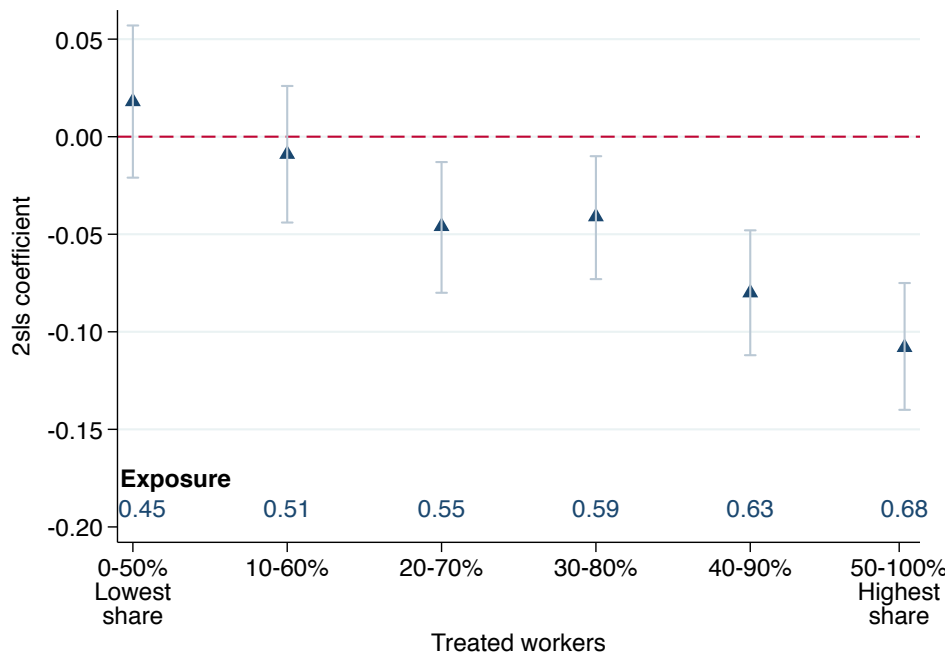
Notes: This figure plots the reduced-form estimates of equation (3) scaled by the first-stage change in the remittance of benefits. On the vertical axis we put the 2SLS pass-through coefficients. On the horizontal axis we put different exposure groups, defined as the share of unionized employees within each firm. Each dot corresponds to a separate regression where we consider rolling groups of exposure. For example, the first dot corresponds to our baseline regression estimated on a subsample of firms with 0-50% unionized employees; the second dot considers firms with 10-60% unionized employees, and so on. This means that a firm can participate in more than one regression. We consider non-mutually exclusive groups to get more observations and power in our regressions. We report the average unionization rate of each exposure group above the horizontal axis.

Figure 8: Horizontal equity

(a) Firm exposure to family allowances

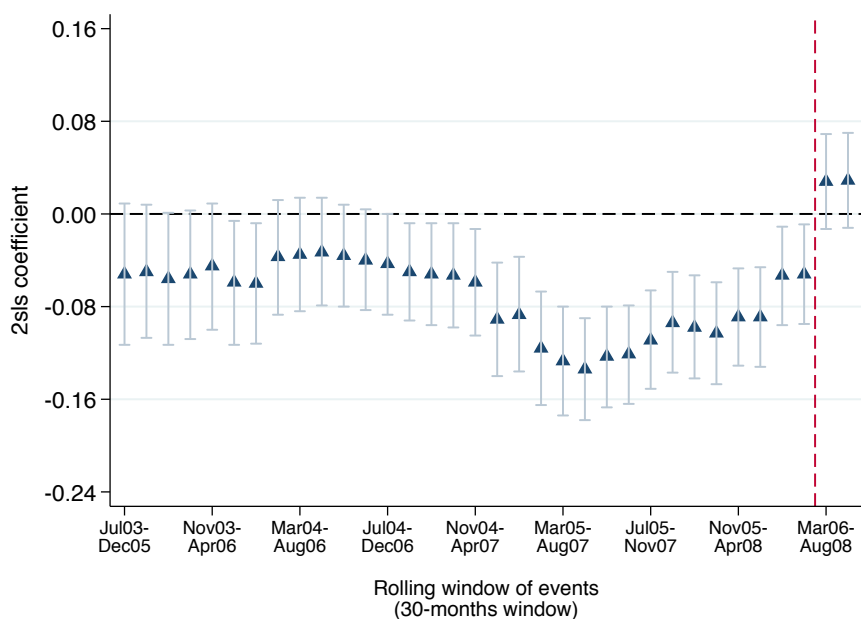


(b) Pass-through by firm exposure



Notes: Panel (a) shows the density of firm exposure to the reform. Exposure is defined as the within-firm share of workers with children. Panel (b) plots the reduced-form point estimates of equation (3) scaled by the first-stage change in the remittance of benefits, for different breaks of firm exposure where each dot corresponds to a separate regression. The bottom part of the figure also shows the mean exposure of each group of firms considered in every single regression.

Figure 9: Dynamic of wage effects over time (rolling window of events)



Notes: This figure plots the reduced-form point estimates of equation (3) scaled by the first-stage change in the remittance of benefits. It considers firms that switched to the new system in different time spans. In particular, we focus on firms that changed regime within a 30-month window and then we move forward following a rolling window of events. For instance, the first point includes those firms that switched between July 2003 and December 2005 (the first 30 months of our panel data), then we move one month forward (i.e., exclude firms that switched in July 2003 and include firms that switched in January 2006), and so on. The effect fades away as soon as we start including firms that switched after the beginning of the financial crisis (dashed red vertical line). Footnote 13 lists the reasons why we exclude these firms from the main analysis.

Table 1: Wage effects and pass-through of a change in the remittance system

	Short-run		Long-run
	All post periods [t = 0-11] (1)	Last period [t = 11] (2)	Last period [t = 23] (3)
Reduced form			
Δ monthly wage (in pesos)	4.69*** (1.21)	5.73*** (1.88)	12.72*** (2.76)
First stage			
Δ transfer (τ^e) (in pesos)	-94.13*** (0.35)	-93.93*** (0.38)	-97.35*** (0.48)
2SLS			
$\frac{\Delta wage}{\Delta transfer(\tau^e)}$	-0.05*** (0.01)	-0.06*** (0.02)	-0.13*** (0.03)
Number of firms	26,226	26,226	24,634
Observations	2,285,705	1,998,351	1,603,885
Avg wage at t_{-1}	871	871	871

Notes: This table reports the reduced-form and 2SLS estimates from the event study. In the first panel, we pool the coefficients from Figure 3 before and after the switching date and we take the difference. In the second panel, we do the same for the change in transfers paid by employers. In the third panel, we run a 2SLS regression to scale the reduced-form coefficient by the first-stage change in the transfer. In column (1), we pool the coefficients for the 12 months post event. In columns (2) and (3), we take the coefficients for the last month post event (t=11 and t=23 for the short and long-run respectively). Standard errors clustered at the firm level are reported in parentheses. *** significant at 1%, ** significant at 5%; * significant at 10%.

Table 2: Wage effects and pass-through by firm size and type of business

	Small		Non		Incorporated	
	[≤ 10]	[$+10$]	Incorpo	Incorpo	[≤ 10]	[$+10$]
	(1)	(2)	(3)	(4)	(5)	(6)
Reduced form						
Δ monthly wage (in pesos)	4.21** (2.01)	3.36** (1.51)	0.26 (1.92)	6.15*** (1.54)	9.35*** (3.07)	3.10* (3.07)
First stage						
Δ transfer (in pesos)	-102.22*** (0.61)	-86.62*** (0.37)	-99.50*** (0.71)	-91.66*** (0.40)	-101.67*** (0.82)	-85.76*** (0.40)
2SLS						
$\frac{\Delta wage}{\Delta transfer(\tau^e)}$	-0.041** (0.020)	-0.039** (0.017)	-0.003 (0.019)	-0.067*** (0.017)	-0.092*** (0.030)	-0.036* (0.02)
Number of firms	12,278	13,948	8,133	18,093	6,451	11,642
Observations	1,050,424	1,235,281	688,451	1,597,254	556,922	1,040,332

Notes: This table reports the reduced-form and 2SLS estimates from the event study. In the first panel, we pool the coefficients from Figure 3 before and after the switching date and we then take the difference. In the second panel, we do the same for the change in transfers paid by employers. In the third panel, we run a 2SLS regression to scale the reduced-form coefficient by the first-stage change in the transfer. In columns (1) and (2), we break the result for small firms (10 or fewer employees) and large firms (more than 10 employees). In columns (3) and (4), we break the result for incorporated and unincorporated businesses. In columns (5) and (6), we combine size and type of business. Standard errors clustered at the firm level are reported in parentheses. *** significant at 1%, ** significant at 5%; * significant at 10%.

Supplementary Materials for Online Publication: “Wage Effects of Means-Tested Transfers”

A Family allowances in Argentina

The AAFF program benefits low and middle-income families. For example, a worker who earns the minimum wage typically falls in the lowest bracket and is eligible for the highest allowance. More generally, between 2001 and 2008 the upper earnings limit, where the worker loses eligibility, was approximately equal to the average monthly wage of registered workers.^{1, 2} Note that the typical FA recipient is located at the low-middle end of the *formal* wage distribution. The existence of unregistered or *informal* wage employees, who generally have lower earnings, indicates that the AAFF is not targeted towards the poorest families in the country.

Table A1 provides a complete picture of the scheme including the evolution of the brackets and the exact transfer amount per child. The amounts are adjusted semi-annually. The average tax credit rate for the lowest category is, on average, 7 percent,³ and in the micro-data, we observe that, on average, each claimant claims for two children (therefore the final ATR is double). In 2010, roughly 1.5 million registered workers received a total of AR\$10 billion in AAFF payments.

The AAFF is an “individually-based” scheme meaning that individual earnings are considered to determine the bracket and transfer amount (as opposed to nuclear family earnings). Only one of the parents or guardians, conditional on being formally employed, is entitled to receive this benefit, but not both of them at the same time. This implies that if one of the spouses earns more than the upper gross earnings threshold, he/she is not entitled to receive the benefit but the other parent can (conditional on being a formal employee and with gross wage earnings below the upper threshold).⁴ Since 2012, the tax credit went from being individually-based with 3 progressive brackets to family-based with 4 progressive brackets.⁵ The family-based component means that to be entitled to receive the allowance, none of the child’s parents can earn more than the upper threshold.⁶

¹Workers are also entitled to one-time benefits upon marriage; pregnancy, birth, or adoption of a child; for maternity leave or prenatal care; and for a disability of a child or spouse.

²To avoid any potential gaming behavior in the system, the worker has to earn more than 100 pesos to be eligible to receive the transfer. This floor remained constant from March 2004 to September 2012.

³Calculated using the upper threshold e.g., in the first row we took the ratio 40 over 500.

⁴When a certain worker has more than one job, she is entitled to receive the family allowances benefits in only one of them, the one with the highest seniority.

⁵See Decree 1667/2012.

⁶In principle, this change could improve the targeting of the scheme. However, it may also impose some costs to secondary earners within the household, typically female, given that they face a higher

The AAFP program is based on a contributory system financed by employers' SSC, which rise to 7.5 percent of wages. In general, employer payroll taxes have the following components: family allowances (7.5 percent), retirement (16 percent), health insurance (5 percent), health insurance for the elderly (2 percent), unemployment insurance (1.5 percent), life insurance (0.03 percent), and contributions to a worker compensation fund for individuals who suffer work-related accidents. The percentage of the latter contribution varies by type of job.

Besides the AAFP program mentioned above, the Argentinian government currently transfers money to households with children in two other different schemes. First, middle- and high-income workers subject to the income tax are entitled to personal exemptions in the form of a fixed deduction per spouse and per child (this is technically a tax credit conditional on having children). As in many countries, taxpayers below a given threshold are exempt from the personal income tax. In general, this threshold coincides with the upper threshold where workers lose the AAFP transfer but this is not always the case. The unification of both thresholds is a way to assure that every child receives at least a certain amount of aid from the government. Second, Argentina introduced a universal child credit (the Universal Childhood Allowance, AUH for its acronym in Spanish) in 2009, extending in this way the coverage to unemployed and informal workers (Decree 1602/2009). Payments are conditional on enrolling children into schools, health check-ups, and vaccinations.⁷

The ongoing Argentinian scheme, including the three systems mentioned above, is plagued with inconsistencies and inequities. For instance, while transfers received through AUH are conditional on some requirements e.g., school enrollment, the child tax credit embedded in the personal income tax exemptions does not impose any conditionality. Moreover, family allowances to formal employees are paid on a monthly basis and the full transfer is paid each month. AUH recipients, by contrast, receive 80 percent of the transfer each month and the rest is disbursed at the end of the year when conditionalities are checked. In a context of high inflation, where the purchasing power of money is quickly eroded, this can make a big difference. Finally, if both spouses file personal income tax, they can both take deductions for the children they have in common duplicating the amount of the tax credit.⁸ It is then likely that the effective final transfer received by a rich household is indeed higher than the transfer received by a poorer one.

marginal tax rate with a potential concern regarding labor supply. This is an interesting reform for future research.

⁷This type of program is known as a Conditional Cash Transfer (CCT) and has been gradually introduced throughout Latin America following Mexico's famous experience (*Progres*a).

⁸This has been recently removed (Resolution 4283/2018), and only one spouse is allowed to deduct the children they have in common.

B Macroeconomic and historical context

Argentina went through a severe economic crisis during the period 2001/2002 with a sizable drop in the per capita GDP of around 12 percent. The crisis established the end of the *convertibilidad* that tied the Argentinean peso to the US dollar, and led the country into one of the most severe depressions in its history including institutional, political, and social unrest. For example, during this period, the country experienced the highest poverty and inequality levels ever documented and had five different presidents in only one week. Moreover, the period was characterized by some difficult episodes, including looting, and the introduction of quasi-currencies that were gradually removed afterwards. Naturally, as the Argentinean peso was uncoupled from the US dollar, there was a jump in the exchange rate and fear of a return of inflation.

Starting in 2003, Argentina experienced a steady and continuous recovery with an (average) annual GDP growth of approximately 7 percent. Throughout the period, we observe an increase in employment, production, and formalization rates, among other indicators. As a caveat, inflation, which had been almost nonexistent during the nineties, started rising after the depreciation of the peso. Moreover, during the 2003-2010 period, yearly inflation averaged 15 percent. For the purpose of this paper, i.e., to estimate wage effects, the presence of inflation is a very interesting feature. During periods of high inflation, wages are frequently renegotiated. In other settings, wage renegotiation occurs less often and thus the identification of wage effects is more challenging; here we think is cleaner for this reason. Figure I.23 illustrates this point; it presents the consumer price index (CPI) and the average salary of registered workers in nominal terms, both on a monthly basis.⁹

During the 96-month period that we analyzed, the minimum wage was updated every 4 months (23 changes). During these years, there were also several changes in the minimum pension allowance and, as shown in Table A1, the thresholds and amounts of family allowances were updated roughly once per year. Overall, this suggests that prices (wages) were quite flexible during the time span of interest.

In terms of wage setting institutions and regulations, approximately half of the workers are, one way or another, covered by unions. Collective bargaining agreements occur either at firm level (70%) or activity level (30%) (see Figure I.22 for an example of a CBA). In principle, these agreements could take place at any moment within the

⁹An intriguing aspect of inflation, is its tendency to prompt more frequent updates of prices and wages, as illustrated in Figure 3a, thereby diminishing the signaling capacity of prices. This introduces a dynamic element of confusion for employees, potentially working in favor of employers who can capitalize on the situation. If this holds true, we may observe larger wage effects in countries with frequent price updates compared to those with minimal movement, such as Switzerland (where conditions are more stable). If indeed the case, our setting emerges as well-suited for investigating the specific questions we aim to address.

year, as shown in Figure I.24.

The sustained growth and fast recovery that the Argentinean economy exhibited after the crisis was interrupted in mid-2008 by the great recession. Argentina, like other countries, was hit by the greatest worldwide crisis since the 1930s. The impact on local economic activity can be summarized in Figure I.25, where we document the monthly evolution of economic activity. In a similar vein, private employment grew steadily until mid-2008, and stabilized afterwards (see Figure I.26).¹⁰ There was a satisfactory recovery in 2010/2011, and since then the country has fluctuated between years of positive and negative growth.

As a way to deal with the 2008 crisis, the family allowance program was expanded in 2009 to include informal workers i.e., workers who are not registered and who therefore were not initially covered by the FA program. This extension gave place to a new (sub) program called *Asignación Universal por Hijo* (AUH), that imposes some conditionalities on its beneficiaries linked to health and education investments. The spirit of the AUH is very much aligned with the standard conditional cash transfer type of program. Nonetheless, the general regime, devoted to registered wage earners, retained its original scheme with periodic updates due to inflation. In Figure I.27, we plot the ratio of the transfer to the minimum wage for a worker with one child and for three different income brackets. The figure shows that the average tax rate remains roughly constant throughout the period.

C Employer-mediated schemes around the world

Family allowance schemes vary substantially across countries. The main difference consists on whether a given country adopts a universal approach or not, where universality simply refers to the employment status of children's parents. While some countries pay out allowances to all families regardless of parents' labor condition (e.g., France) others do so based on the parents' employment status, such that workers receive an extra payment that is linked to the family composition (e.g., Argentina). Differences appear also when looking at sources of funding (general revenues or employer/employee contributions), benefit level (percentage of the minimum wage), progressivity, and administration, among others. Most of the countries set the children's age eligibility threshold at the minimum working age, generally determined somewhere between 14 and 18 years old (see Roddis & Tzannatos, 1999).

In modern economies, governments quite often rely on firms as intermediaries

¹⁰A priori, this seems interesting, especially if the wage effect that we document comes from, or is driven by, new hires.

in the tax-benefit system. This could include different types of interactions such as employer-based health insurance, withholding of the payroll and income taxes, or even disbursing child benefits. There are, of course, pros and cons associated with such intermediation. For instance, one of the great advances in contemporary tax schemes is the use of firms to withhold taxes. Together with improved technology, third-party reported information derived from tax withholding is one of the key drivers of better enforcement and lower evasion (Kleven, 2014, Kleven *et al.* , 2016). However, sensitive information could be revealed to the firm during the process and rent opportunities arise (e.g., wage effects). In the case of employer-mediated transfers, employers could become aware of who is receiving the transfer, the amount of the benefit, the structure and family composition, among others.

There is scant evidence regarding the economic incidence of means-tested transfers (Nichols & Rothstein, 2015). In addition, very little has been documented concerning the role of firms as intermediaries of family allowances / transfers / tax credits. Overall, employer-mediated transfers are more widespread than publicly known. This is true both in developed as well as in developing countries (mostly middle-income countries, because a certain level of development is necessary to set up this type of schemes). In Table A8, we present a non-exhaustive list of similar schemes, several of which continue to operate.¹¹

There are various Latin American countries that have schemes that are almost identical to the Argentinean one. Generally, it seems that this type of family allowance scheme has been introduced on top of the existing social security systems, which is why they use employers as intermediators.¹² The Brazilian program, *Salário Família*, is a means-tested program based on individual income, targeted to workers in the formal sector, and funded by a contributory system. Similar to the SFC, the transfer is paid to employees by their employer and then the payments are deducted from social security contributions. The *Salário Família* is a bit less generous, as compared to the Argentinean transfer program, as the transfer covers children only up to 14 years old.

The Earned Income Tax Credit (EITC) is currently the largest cash transfer program in the United States and, probably, one of the most famous programs around the world. It consists of a refundable tax credit which is a function of household earnings and number of children. Interestingly, it contains a phase-in scheme that creates positive incentives to work. The transfer schedule also contains a plateau and a phase-out range where benefits are taxed away. The *Advance* option, the AEITC, allowed taxpayers to receive the transfer in their paychecks rather than when filing their year-end

¹¹Importantly, note that here we are not referring to the well-known conditional cash transfers, CCT, (e.g., *Bolsa Família*, *Chile Solidario*, *Juntos* and *Tekopora*, for the Latin American countries listed in the table.

¹²There are also other common features in the region such as the contributory scheme and the *notched* structure.

tax return. The advance option was eliminated in 2010 by President Obama due to a very low take-up rate. Similarly, the United Kingdom had, between 1999 and 2003, the Working Family Tax Credit (WFTC) a welfare program that disbursed the payments through the employers rather than directly to workers. This program was then replaced by the Working Tax Credit, which is the reform analyzed by [Azmat \(2019\)](#).

There are also more recent experiences, some of which are still in place. For instance, in 2014 Italy introduced the *Bonus Renzi 80 Euro*, a welfare transfer program targeted to employees with certain level of income. The bonus was paid through the employers who acted as the withholding agent. In Switzerland, the *Familienzulagen* is a child benefit regulated on a cantonal basis (26 cantons) that is financed by a compensation fund and paid by employers every month along with the corresponding salary. Finally, Greece has a family allowance scheme for each child less than 18 years old that is paid together with the salary by the worker's employer.

D Incorporation process

Firms were gradually incorporated into the SUAF as follows (see [Figure I.4](#)). The first step consisted of the social security administration (ANSES, for its name in Spanish) publishing various resolutions that established that firms will be gradually incorporated into the system before a certain month (e.g., December 2005). It published more than fifty resolutions between 2003 and 2008 with the different incorporation schedules. Each firm was notified regarding the different documents that they had to submit. Specifically, the formalization process required that each employer had to submit a set of specific documents and paperwork including the form F.560. These documents were supposed to be presented either at the ANSES headquarters office or at a subsidiary office, *Unidad de Atención Integral* (UDAI).¹³ [Figure I.28](#) presents an example of such a memo. The top panel contains the body of the resolution including the first two articles. Note that some of the key words are: *cronograma* (schedule), *paulatina* (gradual) and *obligatoriamente* (mandatory); while the bottom panel presents the corresponding appendix that includes firm identifiers.

As noted above, the second step consisted of the different firms submitting the required documentation. In general, it took three and a half months from the moment the firm was notified to the submission of the documentation. The third, and last, step consisted of the final approval or formal incorporation of the firm into the system, which in most cases took approximately 50 days after step two was completed.¹⁴ The

¹³There were nearly 300 UDAs located throughout the country.

¹⁴Both duration references were extracted from an audit of the SUAF incorporation made by the AGN

approval was documented in another memo in which ANSES established the date on which each firm would be formally included in SUAF and the date until which it could *compensate* the family allowances paid under the old system.¹⁵

Figure I.29 shows an example of an incorporation memo. The top panel presents the whole memo where it is possible to see the key components such as *incorpórase formalmente* (formal incorporation), *agosto 2006* (incorporation date) as well as the firm identifier. When the memo involves several employers, it contains an appendix listing them (as seen in the bottom panel).¹⁶

Employers were also able to search a public website for whether a given firm was in fact under the new scheme and, if so, the starting date. This is shown in Figure I.30. To do a query on this website the user had to enter the firm's CUIT (employer identifier) and a security code; afterwards, the site reports the firm's name (*Razón Social*), whether it is allowed to be in the new system (*Estado*), and the corresponding legal memo as well as the date (month and year) of incorporation into SUAF (*Detalle*).¹⁷

This last point refers to firms' observed responses in the micro-data i.e., the first month in which we observe an interruption of family allowance payments under SFC. As explained in the body of the paper, we define an event date as the moment in which we identify in the micro-data that a given firm stops disbursing funds under the old payment mechanism. We then check whether the different administrative dates (schedule and formal incorporation) align with what we observe at the micro level.

To that end, we digitized all internal schedules that we were able to find on the ANSES webpage (more than the 50 appendixes). We ended up with approximately 63,000 firm identifiers with the corresponding final schedule deadline for each firm.¹⁸ As far as we know, the date functioned as an internal due date to commit to the gradual incorporation process rather than a deadline imposed on firms. We combined these dates with the event dates constructed from the micro-data. We found that 80 percent of the employers were incorporated before the *internal* deadline, which shows that ANSES committed to its internal planning for gradual incorporation.

We then looked at the formal incorporation date (listed in the incorporation resolution) and its correspondence with the micro-data. In contrast to the schedule memo,

(Auditoría General de la Nación).

¹⁵The term *compensate* refers to firms' ability to deduct the transfer from employer SSC. The last month to *compensate* a payment, i.e., to deduct it from workers' SSC liabilities, was the month before the formal incorporation date. The idea behind this was to avoid duplicate payments i.e., both, a payment under the SFC and under the SUAF, for a given month.

¹⁶Note that the third column contains that name of the UDAI, i.e., where the documentation was submitted.

¹⁷We manually checked whether the date that appeared in the memo matches that on the website and in nearly all the cases they do match.

¹⁸We found that only 0.001 percent of the employers appeared in more than one resolution.

it is quite hard to track the incorporation memo because there were hundreds of them and they lacked any organization. However, we used the public website to recover the formal incorporation date for a random sample of firms. Figure I.33 presents the correlation between the formal incorporation date and the one derived from the micro-data. In panel (a) we present the correlation for all the firms in our estimation sample whereas in panel (b) we separate firms into groups based on the number of beneficiaries in the last month we observe a payment in the micro-data, i.e., in t_{-1} . The vertical axis shows the cumulative distribution function (*CDF*) of firms that entered into the new payment system. On the horizontal axis we show the distance (in months) from the event (identified in the micro data) to the formalization date. Those on the left side have an event (switch) before the formalization date, while in contrast those on the right had an event afterwards.

Overall we observe a high correlation between the event, identified using the micro-data, and the formalization date which suggests that we are correctly determining the exact moment when each employer switched to the new regime. Note, however, that in the top panel we see that some firms entered into the SUAF before the formalization date. Although this was possible, it was quite unlikely. To further understand this observation, in panel (b) we broke the *CDFs* down by number of beneficiaries receiving FA the last month before switching, i.e., we separately considered firms with 1 or more FA recipients, 2 or more, and so on.

We find that the mass on the left side of the figure is entirely driven by firms with only one beneficiary before the switch. Therefore, it is likely that we have a measurement error in the event definition when we consider employers with only one FA recipient. For instance, it could be the case that the unique beneficiary left the firm or was fired, which would explain why we observe that such firms stop disbursing the transfer and afterwards, by mistake, we identify such an event as the switch when it was not so. Narrowing the sample to FA $t_{-1} > 1$ shows that switching before the FI was not possible (which is consistent with the anecdotal evidence that we have).

We exclude the potentially erroneous cases by restricting our estimation sample to those firms that had at least two beneficiaries before the switch.¹⁹ We observe that, in most of the cases, the event date coincides with the formalization date (roughly 80 percent of the cases) and, six months after the FI, 95 percent of firms were already incorporated into the new system. This rather fuzzy correlation could reflect some inattention or miss communication between employers and the SSA. In principle, firms have no incentive to delay their incorporation after the formalization because, according to the

¹⁹Nevertheless, we test the sensitivity of our wage effects using different estimation samples as shown in Figure I.16.

memo, they cannot compensate the money of the transfers they disburse.²⁰

E Conceptual framework

In this section, we briefly describe the way to rationalize the mechanisms that could explain who bears the incidence of a transfer. The economic incidence under the standard partial equilibrium model depends on the relative size of the elasticity of supply and demand of the good being taxed; where the more elastic side can shift the burden to the more inelastic one. Under perfect information, perfect competition, and no compliance cost, the statutory incidence is irrelevant in determining the final incidence.

We begin by defining some key concepts following the terminology proposed by Slemrod (2008). We interpret *legal liability* as what the law says about who should pay the tax or, in other words, on whom the tax is levied (generally called statutory or formal incidence). For example, employees should pay employees' SSCs. There is also the *remittance responsibility*, which determines who is responsible for remitting the amount of the tax to the tax authorities. For example, employees' SSCs are typically remitted by employers. And, finally, *economic incidence* refers to who actually bears the tax burden—i.e., who is worse off as a result of it.

To further understand the institutional setting, we adapt the above concepts to our case and analyze how the different elements change between the two payment systems. Table A3 lists some key dimensions that may affect the final economic incidence. Columns (1) and (2) correspond to the old and the new system, respectively. The table shows that the main change between the two systems lies in the *remittance responsibility*. While employers disburse the transfer with the monthly wage in the old system, under the new system, the government pays the benefit directly to eligible workers. Note, however, that the *legal liability* is the same in both systems: the ultimate transfer recipient is the employee.

Another important dimension is the *salience* of the transfer. As has been documented, the visibility of a tax influences its economic incidence (Chetty *et al.*, 2009), but it is uncertain whether and how this effect operates in the case of transfers. In principle, under the old system, the transfer was more visible to both employers and employees; in fact, it was reported in workers' pay slips as shown in Figure I.3.²¹ The figure presents a real case from an anonymous worker right before and right after the firm switched from the SFC to the SUAF system. In the old system (panel a), the

²⁰For instance, see article 3 in Figure I.29 panel (a) (ARTICULO 3: ...employers will not be able to compensate the family allowances paid to their workers, as of the period...).

²¹Article 140 of the Labor Contract Law, established that it is mandatory to list in pay slips all the items that employers pay and deduct from wages.

pay slip contains a line with the benefit amount of AR\$ 720 received in that month (about 25 percent of total wage earnings). Under the new system (panel b), the transfer disappears from the pay slip, and the worker starts to receive the transfer from the government directly in his checking account.²² Hence, the transfer became less visible to employers.²³

In addition, the introduction of a new disbursement system could have prompted a reassessment of the tax-benefit linkage. In particular, while the theoretical, or statutory, tax-benefit linkage remained unchanged, workers' perception of this linkage may have increased after firms could no longer integrate the transfer into the compensation package. That is, people's understanding of what the transfer is and how it is financed could have improved, potentially shifting bargaining conditions.²⁴

The claiming procedure for new workers also changed, as they must claim the benefits themselves rather than through their employers. Many other dimensions may induce behavioral responses but remain unchanged under the two payment systems. For instance, the timing of the payments maintained its monthly frequency, and we are unaware of any delays or complaints after firms switched to the new system. In addition, the reform induced no change in the way the transfer is funded; it continued to be financed by a specific component of employer SSCs. Finally, the benefit schedule also stayed the same. Unlike most of the incidence literature that exploits changes in marginal or average tax rates, the transfer amount remains unchanged in our setting. Therefore, it is a pure change in how the money is delivered but not in the total amount that workers receive.

E.1 Incidence model with misperception of benefits

To rationalize the presence of wage effects in a setting with partial perception of benefits, we set up a simple model based on Gruber (1997) and Bozio *et al.* (2023) that aligns closely with our findings. Workers perceive wages according to the following specification:

$$\tilde{w} = w \cdot (1 + (1 - q) \cdot \tau^e) \quad (4)$$

where \tilde{w} represents the perceived wage as a function of the wage (w), a perception

²²Interestingly, note that the take-home pay, highlighted by the red square on the bottom-right side of the pay slip, went down in nominal terms between the two months; also this occurred in a context with inflation which makes everything even more unique.

²³Put it differently, at least it is less salient for employers regarding newly hired workers. Note that the salience of the transfer may also increase for employees when it is deposited directly into their bank account.

²⁴Per legal provisions, employers were mandated to promptly notify their employees about the newly implemented payment system for the family allowance program within ten days after the transition.

parameter (q) and the transfer rate disbursed by employers (τ^e). In addition, we define $\tau^e = \bar{\tau} - \tau^g$, where τ^g is the transfer disbursed by SSA (the government), and $\bar{\tau}$ is the total disbursed transfer. In a situation with perfect awareness and knowledge ($q = 1$), the perceived wage equals the true wage $\tilde{w}_1 = w$. This means that there is an accurate understanding of the way family allowances work and how they are funded. Conversely, in a situation with no knowledge ($q = 0$), workers perceive that the transfer is part of their wage $\tilde{w}_0 = w(1 + \tau^e)$. What are the potential wage effects in these two extreme scenarios? In a context of perfect information ($q = 1$), the standard model holds and the transfer remitter does not affect who bears the final burden of the benefit. In a context of misinformation ($q = 0$), however, we show that employers shift part of the benefits by lowering wages.

We then express the labor supply function as follows:

$$L_i^s = L_i^s(\tilde{w}_i) = L_i^s(w_i \cdot (1 + (1 - q) \cdot \tau_i^e)) \text{ with } i = 1, \dots, n \quad (5)$$

and the corresponding labor supply elasticity as:

$$\eta_i^s = \frac{d \ln(L_i^s)}{d \ln(\tilde{w}_i)} = \frac{l_i^s}{L_i^s} \cdot w_i \cdot (1 + (1 - q) \cdot \tau_i^e) \quad (6)$$

where $l_i^s = \partial L_i^s / \partial \tilde{w}_i$ is the partial derivative of the labor supply with respect to the perceived wage.

Similarly, labor demand is expressed as follows:

$$L_i^d = L_i^d(w) \text{ with } i = 1, \dots, n \quad (7)$$

and labor demand elasticity as:

$$\eta_i^d = \frac{d \ln(L_i^d)}{d \ln(w_i)} = \frac{l_i^d}{L_i^d} \cdot w_i \quad (8)$$

totally differentiating supply and demand equations (2) and (4), we have

$$d \ln(L_i^s) = \eta_i^s \cdot [d \ln(w_i) + d \ln(1 + (1 - q) \cdot \tau_i^e)] \quad (9)$$

and

$$d \ln(L_i^d) = \eta_i^d \cdot [d \ln(w_i)] \quad (10)$$

Equating (6) and (7) and rearranging terms we get

$$\left. \frac{d \ln(w_i)}{d \ln(1 + \tau_i^e)} \right|_{\bar{\tau} = \tau^e + \tau^g, \bar{q} = q} = \frac{\eta_i^s \cdot (1 - q) \cdot \left[\frac{(1 + \tau_i^e)}{(1 + (1 - q) \cdot \tau_i^e)} \right]}{\eta_i^d - \eta_i^s} \quad (11)$$

Incidence predictions: To keep things simple, we can simplify equation (8) by focusing on two polar cases:

- $q = 1 \rightarrow$ perfect knowledge, then we have $\frac{d \ln(w_i)}{d \ln(1 + \tau_i^e)} = 0$ and, thus, we recover the standard incidence result. The way the money is disbursed does not have an effect on wages and, therefore, the remittance responsibility does not determine the economic incidence.
- $q = 0 \rightarrow$ situation with no knowledge or complete confusion about the scheme. In this case we have $\frac{d \ln(w_i)}{d \ln(1 + \tau_i^e)} = \frac{\eta_i^s}{\eta_i^d - \eta_i^s} < 0$ and hence the change in the remitter does have an effect on wages.

The anecdotal evidence that we were able to find suggests that employees did not really understand the way the old payment system worked. As it was mentioned above, the characteristics of the old system included: confusion of roles, people perceived that benefits were integrated with their wage package and even ignored the fact that the state was the one paying the benefit.

Change in perception: the shift in the remittance responsibility from employers to the government could have led to a change in employees' knowledge, and this could have altered employees' perception (q) of the scheme. This is a sort of information treatment, such that after launching the new payment system, eligible workers indirectly update their beliefs or perception about the overall transfer scheme. Consequently, we repeat the derivation but allowing q to change following the change in the transfer disbursed by employers (τ^e):

$$\left. \frac{d \ln(w_i)}{d \ln(1 + \tau_i^e)} \right|_{\bar{\tau} = \tau^e + \tau^g} = \frac{(1 + \eta_i^{(1-q)}) \cdot \eta_i^s \cdot (1 - q) \cdot \left[\frac{(1 + \tau_i^e)}{(1 + (1 - q) \cdot \tau_i^e)} \right]}{\eta_i^d - \eta_i^s} \quad (12)$$

where we define $\eta_i^{(1-q)} = \frac{\partial(1-q)}{\partial \tau_i^e} \cdot \frac{\tau_i^e}{(1-q)}$ as a *misperception elasticity*. That is, $\eta_i^{(1-q)}$ measures how much $(1 - q)$ changes as the benefits disbursed by employers increase. This elasticity is positive meaning that, as there are more transfers disbursed by employers (τ^e), there is an increase in confusion (q decreases and $1 - q$ increases). This positive elasticity reinforces the main effect that we previously derived.

E.2 Comparing competing channels

Conceptually, various channels could be at play. In practice, the labor market transitions from an existing equilibrium (under the old scheme, featuring specific wages and employment), to a new one dictated by the government disbursement system (with a different level of wages and employment). We discuss three alternative mechanisms below, with the first one aligning more closely with our empirical evidence.

New disbursement system \Rightarrow employees' labor supply curve shifts upwards.

1. Under the old regime, employees may have incorrectly perceived the transfer as part of their compensation package, a concept we refer to in the paper as '*partial perception of benefits*'. This notion is supported by anecdotal evidence (Marasco, 2007) and a survey conducted by the SSA (Table A5). The two polar cases from the modified version of Gruber (1997) model outlined above, can be reproduced in the standard labor supply and demand graph as follows.

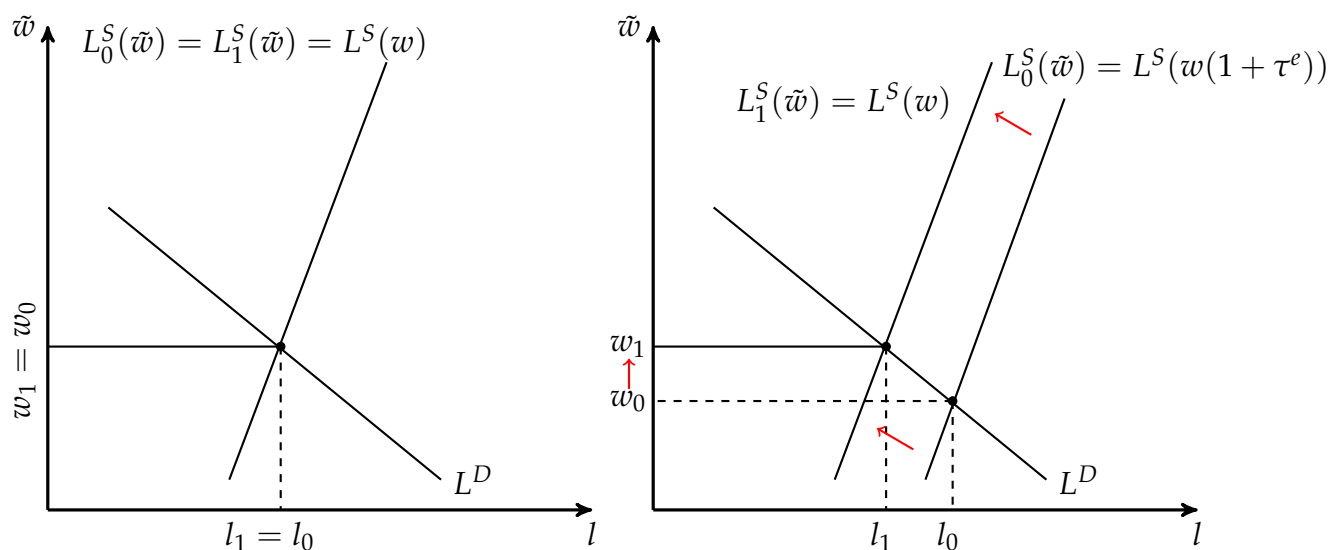
The left panel of the figure below shows the standard situation where workers are fully aware of the disbursement system; in this perfect knowledge case ($q = 1$), the perceived wage is equal to the true wage and, therefore, there should be no movement whatsoever of the labor supply. The initial equilibrium (L_0^S), in which firms disburse the transfer, remains unchanged after the new payment system is introduced (L_1^S). The standard incidence model dictates that who disburses the transfer, is orthogonal to determination of who bears its burden.

The right panel illustrates the opposite case, in which workers have little understanding ($q = 0$) and believe that the transfer is part of their wage. As firms no longer disburse the transfer ($\tau^e = 0$), workers realize it is provided by the State, and labor supply shifts leftwards from L_0^S to L_1^S . Therefore, if our setting is characterized by imperfect knowledge ($q \neq 1$), and if this is the main channel at play; we should observe an increase in wages and a reduction of employment. Indeed, both effects align with the results documented in the paper.

Graphical analysis: shift in labor supply $L^S(\tilde{w})$

(a) Perfect awareness and knowledge ($q=1$)

(b) Situation with no knowledge ($q=0$)



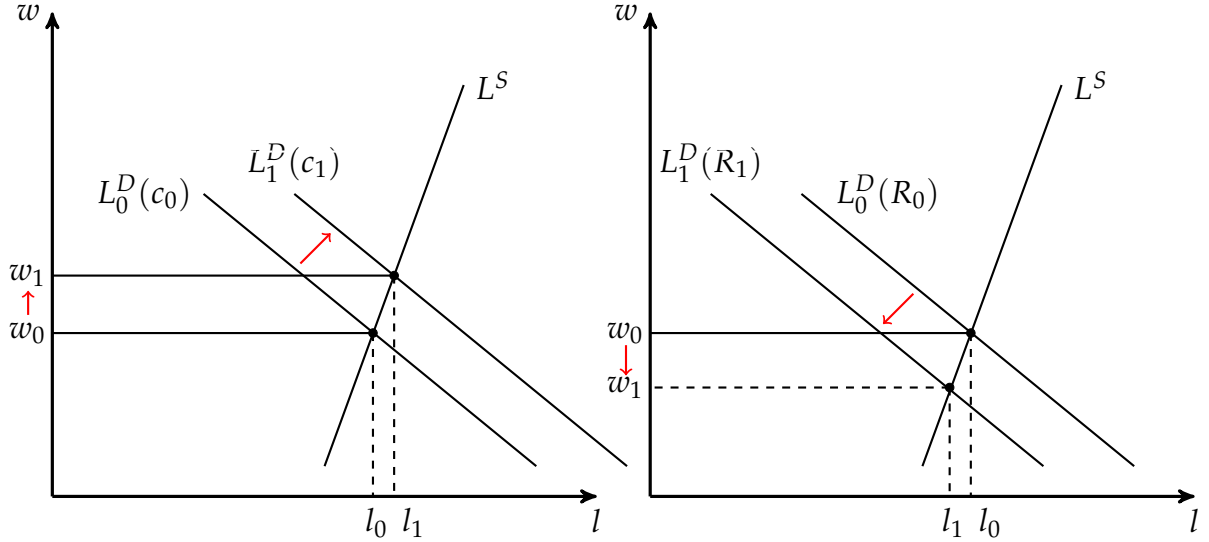
Under the new regime, employers stop disbursing the transfer \Rightarrow labor demand curve may shift upward or inward.

- Let's assume that under the old system there were some associated costs (c_0) of delivering the transfer (e.g., some processing or paperwork cost). Then, these costs should disappear as soon as the government starts disbursing the child allowance ($c_1 = 0 < c_0$). In the left panel of the figure below, the labor demand would shift upward from L^D_0 to L^D_1 . This means that, for a given wage, firms are willing to hire more workers. If this is the channel that prevails, then we should observe an increase in wages and employment. We find evidence of the former but not of the latter. In addition, in sub-section H.1, we further explore whether tasking firms with the disbursement of child allowances may have hindered their financial situation. We analyze firms' delinquency rates on financial debt before and after the event and find a null effect (Figure I.37).
- If under the old system there was a rent-seeking space (R_0) for employers (e.g., the possibility for them to offer a package that includes the actual wage plus the transfer), this opportunity vanishes under the new regime (with $R_1 < R_0$). The explanation is linked to the fact that the transfer becomes less salient for employers when the government is in charge; it becomes harder to flag transfer recipients and the amount involved. It is even less clear for newly hired workers (where the information component is muted after the firm switches to the new system). In this case labor demand shifts inwards, because the rent-seeking space

disappears (moves from L_0^D to L_1^D in the right panel of the figure below). The prediction is that wages and employment should decrease under this channel; which is at odds with the evidence we find.

Graphical analysis: shift in labor demand (L^D)

(a) Reduction in administrative costs (c) (b) Reduction in rent-seeking (R) opportunity



F Econometric specification

Intuitively, our identification strategy can be summarized as follows. Assume that there is only one firm and, thus one treatment date. Then, the natural within-firm variation to be exploited can be specified as follows:

$$w_{i,t} = \alpha + \beta_0 \cdot T_{i,t} + \beta_1 \cdot T_{i,t} \cdot Post_{i,t} + \mu_t + \epsilon_{i,t} \quad (1)$$

where T refers to workers belonging to the treatment group, $Post$ to the period after the event, and μ_t to month-year fixed effects. Finally, the outcome variable w denotes the monthly wage used as the base for employers' SSC.

If, however, it happens that there are N firms, all with the same treatment date, then we would have

$$w_{i,f,t} = \beta_0 \cdot T_{i,f,t} + \beta_1 \cdot T_{i,f,t} \cdot Post_{i,f,t} + \mu_{f,t} + \epsilon_{i,f,t} \quad (2)$$

where $\mu_{f,t}$ refers to firm-specific month-year fixed effects.

If we then allow the N firms to have different treatment dates, we could write the

following:

$$w_{i,f,t} = \beta \cdot T_{i,f,t} + \sum_{j=-13}^{12} \gamma_j \cdot T_{i,f,t} \cdot d_{f,t}^j + \mu_{f,t} + \epsilon_{i,f,t} \quad (3)$$

Afterwards, we could obtain the mean wage for each firm-group-month ($\bar{w}_{g,f,t}$) and thus present the following reduced-form specification in levels (note that here we have two observations by firm-month).

$$\bar{w}_{g,f,t} = \beta \cdot T_{g,f,t} + \sum_{j=-13}^{12} \gamma_j \cdot d_{f,t}^j \cdot T_{g,f,t} + \mu_{f,t} + \epsilon_{g,f,t}^{25} \quad (4)$$

To keep things simple, afterwards we take the difference across groups and thus define the (mean) wage gap between treatment and control workers.

$$G_{f,t}^{\bar{w}} = \bar{w}_{f,t}^T - \bar{w}_{f,t}^C$$

This means that, for each firm, we have a time series of first differences. Thus, we specify a first difference model and run a regular event study specification (note that in this case we will have one observation by firm-month).

$$G_{f,t}^{\bar{w}} = \alpha + \sum_{j=-13}^{12} \gamma_j \cdot d_{f,t}^j + \epsilon_{f,t} \quad (5)$$

The γ 's in equation (5) should be numerically the same as those estimated in equation (4). That is to say, we get the same result as when having two observations per firm-month and including firm-by-time fixed effects because gammas are identified in equation (4) by differentiating.²⁶

In order to compute the reduced-form point estimates and sum up our results, we simply pool all the gamma coefficients before ($G_{before}^{\bar{w}} = (\gamma_{-12} + \gamma_{-11} + \dots + \gamma_{-3} + \gamma_{-2} + 0)/12$) and after ($G_{after}^{\bar{w}} = (\gamma_0 + \gamma_1 + \gamma_2 + \dots + \gamma_{10} + \gamma_{11})/12$) the switching date and then take the difference ($G_{average}^{\bar{w}} = G_{after}^{\bar{w}} - G_{before}^{\bar{w}}$). Getting previous coefficient ($G_{average}^{\bar{w}}$) in a regression framework would imply estimating the following specification, which, in turn, will allow us to estimate the standard errors:

$$G_{f,t}^{\bar{w}} = \alpha + \beta_1 \cdot Window_{f,t} + \beta_2 \cdot Window_{f,t} \cdot Post_{f,t}$$

²⁵Alternatively, we can run either (a) $\mu_f + \mu_t$ i.e., firm and time, separately, fixed effects, or (b) $\mu_f + \mu_t + \mu_f \cdot t$ plus firm linear trends. Nevertheless, our preferred alternative is the less parametric one, which is the one included in the main specification.

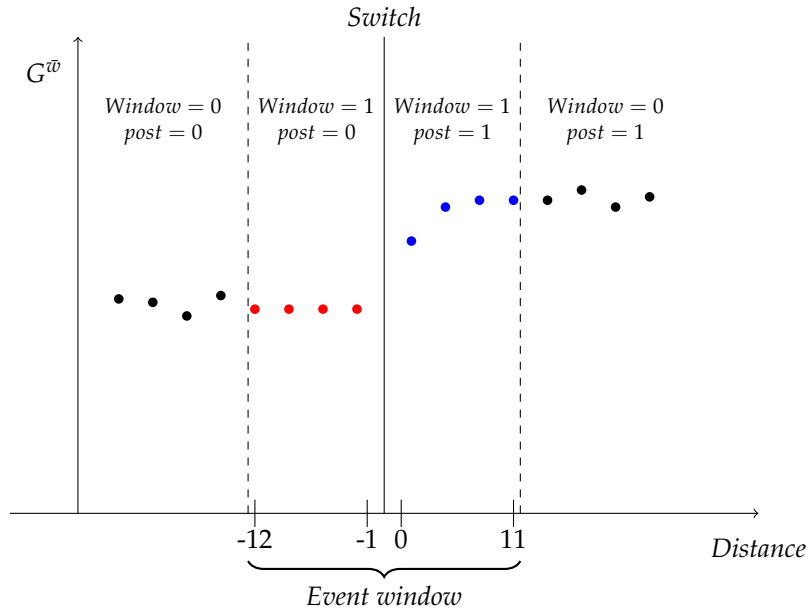
²⁶The standard errors, clustered at firm level, are also the same under both specification. This is true because both specifications use the same estimator so they must have the same true variability.

$$+\beta_3 \cdot (1 - Window_{f,t}) \cdot Post_{f,t} + \epsilon_{f,t}$$

where *Window* equals one in those months that belong to the time span [-12;11]. If we then apply the expected value operator to the previous equation we would have the following scenarios:

- $E(G^{\bar{w}} / Window = 0, post = 0) = \alpha$
- $E(G^{\bar{w}} / Window = 1, post = 0) = \alpha + \beta_1$
- $E(G^{\bar{w}} / Window = 1, post = 1) = \alpha + \beta_1 + \beta_2$
- $E(G^{\bar{w}} / Window = 0, post = 1) = \alpha + \beta_3$

A difference in differences (DID) coefficient could be approximated by taking $[3] - [2] = \beta_2$. Graphically, we will have a situation that can be illustrated as follows:



Similarly, the first-stage point estimate will be recovered as follows

$$G_{f,t}^{Transfer} = \alpha + \delta_1 \cdot Window_{f,t} + \delta_2 \cdot Window_{f,t} \cdot Post_{f,t} + \delta_3 \cdot (1 - Window_{f,t}) \cdot Post_{f,t} + \epsilon_{f,t}$$

Thus, the 2SLS Wald estimator will be given by the following ratio $\Theta = \frac{\beta_2}{\delta_2}$. Moreover, to the first difference model specified in equation (5) we could add firm and time fixed effects to account for the fact that the composition of the panel of firms is changing over time and to control for time-specific trends, respectively.

$$G_{f,t}^{\bar{w}} = \sum_{j=-13}^{12} \gamma_j \cdot d_{f,t}^j + \mu_f + \mu_t + \epsilon_{f,t} \quad (6)$$

Finally, to get the point estimate we run the following specification:

$$\begin{aligned} G_{f,t}^w &= \beta_1 \cdot Window_{f,t} + \beta_2 \cdot Window_{f,t} \cdot Post_{f,t} \\ &+ \beta_3 \cdot (1 - Window_{f,t}) \cdot Post_{f,t} + \mu_f + \mu_t + \epsilon_{f,t} \end{aligned}$$

G Extensions

G.1 Other sub-samples

Our main estimation sample considers firms that have more than one worker receiving the transfer in the last month (t_{-1}) before the switch to the new regime. This restriction allows us to correctly identify the event date and therefore avoid potential fake events that could confound the estimated effects. If we do not introduce this restriction we could have a situation where the only worker that was receiving the transfer left the firm e.g., because he was fired which would lead us to observe that the firm stops paying the transfer. Consequently, we would incorrectly identify the date on which the worker was fired as the date of the switch.

Nevertheless, we also show that our results are robust to different sample sets. First, we do not impose previously mentioned restriction and thus we include firms that have only one worker receiving the transfer in t_{-1} . Second, at the other extreme, we impose a tighter restriction that requires firms to have more than one recipient in each of the six months before the switch (in $t_{-6;-1}$). Third, we keep firms that have more than one worker in the last month (t_{-1}) and also where the event date and the formalization date coincide. Fourth, we retain firms with the same date but with no restriction regardless of the number of beneficiaries in (t_{-1}). Fifth, we restrict the sample to firms that have at least three workers receiving family allowances before the event date (t_{-1}).

Figure I.16 plots the coefficients of estimating equation (2) using as dependent variable the within-firm average wage gap of the two groups ($G_{f,t}^{\bar{w}}$) for different sub samples (namely those that were introduced in previous paragraph). Several interesting facts arise from this figure. First, regardless of which sample of firms we consider, the point estimates are roughly stable and, more importantly, they remain economically and statistically significant. Second, if we do not impose the restriction of having

more than one worker (sample VI in the graph) we observe a higher bump in the wage gap after the switch. We recognize that this effect could be a mechanical result of firing the only transfer recipient (which in principle is a treated worker likely to have a rather low wage).

G.2 Heterogeneity-robust diff-in-diffs methods

The conventional TWFE specification has recently been criticized in settings with staggered treatment timing for making “forbidden comparisons” between already-treated units (de Chaisemartin & D’Haultfoeuille, 2020, Goodman-Bacon, 2021). In our setting, earlier switchers are used as controls for firms that switch later on. In this context, problems may arise if, for example, the average treatment effect in the first year after transitioning is different for firms switching to SUAF in 2005 and those switching in 2007. Such heterogeneous dynamic treatment effects across adoption cohorts would compromise the interpretation of the coefficients estimated by TWFE regressions.

Many recent papers have proposed alternative estimators that more sensibly aggregate heterogeneous treatment in settings like ours (see the surveys by de Chaisemartin & D’Haultfoeuille, 2022, Roth *et al.*, 2023). Choosing among the various heterogeneity-robust methods is not straightforward. The estimators differ in who they use as the comparison group (e.g., not-yet-treated versus never-treated) as well as the pre-treatment time periods used in the comparisons (e.g. the whole pre-treatment period versus the final untreated period). In practice, however, these estimators typically (although not always) produce similar results (Roth *et al.*, 2023).

For completeness and transparency, we implemented five of the recently-proposed alternatives to TWFE regressions that do not restrict treatment effect heterogeneity between groups and over time. Intuitively, all the estimators carefully choose valid control groups to avoid making the ‘forbidden comparisons’ that render TWFE invalid.

We use the imputation approach proposed by Borusyak *et al.* (2021), the local projection approach proposed by Dube *et al.* (2023), the switchers approach of de Chaisemartin & D’Haultfoeuille (2023), the interaction weighted estimator of Sun & Abraham (2021), and the approach proposed by Callaway & Sant’Anna (2021). We estimate the latter indirectly using the re-weighting trick from Dube *et al.* (2023) which recovers an equally-weighted ATT and is numerically equivalent.²⁷ In all the cases, we use ‘not-yet-treated’ firms (including the never-treated) as the control group. We also reproduce the TWFE specification that includes never-treated firms.

We summarize the results in Figure I.18. Overall, our exercise suggests that the TWFE estimate is robust to the alternative estimators considered. All the approaches exhibit similar results, especially six months before and after the event. The similarity could either be due to the fact that wage effects are not very heterogeneous or to the fact that the event-study regression is fairly robust to heterogeneous treatment effects.

²⁷The direct approach of Callaway & Sant’Anna (2021) attempts to estimate all 2x2 Diff-in-Diff estimates for all groups across all periods. With our large dataset, the implementation rapidly consumed the memory resources.

G.3 Children turn 18: becoming ineligible

The richness of the dataset that we have access to enables us to take advantage of another source of variation. In particular, we tried to shed some light on the baseline incidence, i.e., who benefits from the transfer regardless of the way it is delivered, by taking advantage of an individual-level shock. Specifically, we analyze what happens when a certain worker loses eligibility due to the worker's child reaching 18 years old. A priori, this is a very interesting event to look at because, from the parents' point of view, a child going from 17 to 18 years is a rather smooth event. The opposite case, becoming eligible due the birth of a child, is also very interesting but is a more drastic type of event because several things could change at the time of the birth.²⁸

We consider workers with kids who reach 18 years old between January and December 2005, i.e., born in 1987. We focus on a balanced panel of workers with one job during the 36 months of 2004, 2005 and 2006.²⁹ The treatment group comprises workers with a child turning 18 in 2005 (who may or may not receive FA) and the control group comprises workers without kids turning 18 in 2005 (who may or may not be receiving FA). We retain firms with "treated" workers that switch to SUAF in 2006 or later, that have at most one event, and that have other workers with children but who experience no event. Afterwards, we collapse everything at the firm level and do a within-firm-level analysis.

Figure L.34 plots the coefficients of the first and reduced-form specifications. We document a very clean first stage result with a drop in the transfer amount as soon as the child turns 18. This finding is reassuring about the overall functioning of the FA program, particularly under the SFC when the transfer was disbursed by employers. Age-eligibility thresholds appeared to be working properly even when the disbursement of the transfer was decentralized. Furthermore, as a reduced-form result, we estimate a rather precise null effect on wage earnings which, in principle, does not imply that the economic incidence of the transfer falls entirely on workers. Below, we discuss why this result does not contradict the main findings of the paper.

First, it could be that the worker does complain, and tries to bargain, but the employer explains to them that it's actually a transfer from the government and thus not part of their compensation package, and now they are no longer eligible. Second, our main finding, i.e., exploiting the switch from SFC to SUAF is mostly driven by new employees or hires, is still consistent with a null effect of "child turns 18." It could mean that the incidence or rent-extraction takes place when the worker is hired, at the

²⁸For this reason, we focus only on those cases where the child reaches 18 years old.

²⁹We selected those born in 1987 for two reasons. First, in order to maximize the number of firms that had not yet switched and, second, to use 2004 and 2006 as pre-post years.

beginning of the labor relationship, when the contract is set. If the child turns 18 in the middle of the contract, when the wage and other obligations are already written, then it is hard to observe a wage response given that there is little freedom to adjust.

Third, this is an individual-level shock while the core of the paper is about a firm-level shock. Responses could be very different when only one worker is affected as opposed to a situation where many co-workers are involved. Indeed, we show and discuss in the mechanisms section that the wage effect is stronger in those firms where the share of workers with children is larger. Fourth, workers exposed to the “child turns 18” type of event, are more likely to be closer to $p75$ and thus have a weaker saliency and average tax rate. Fifth, the composition of firms that we use for both exercises is not exactly the same and it could be the case that they differ in size and/or union coverage.

G.4 Addressing pre-event trends

This section provides details of the approach developed by [Freyaldenhoven *et al.* \(2019\)](#) (henceforth denoted FHS) that we use to correct the pre-event trends when estimating the event study on the sample of incumbent workers.

FHS propose a 2SLS estimation strategy that allows for parallel trends to be violated when there exists a covariate assumed to be affected by the same confounds as the outcome but not by the treatment itself. In our paper, we are interested in estimating the event-study coefficients γ_j from our equation (2):

$$G_{f,t}^{\bar{w}} = \sum_{j=-13}^{12} \gamma_j \cdot d_{f,t}^j + \mu_f + \mu_t + \epsilon_{f,t}$$

where $d_{f,t}^j$ are event-time indicators for the change in disbursement happening j months away, and $G_{f,t}^{\bar{w}} = \bar{w}_{f,t}^T - \bar{w}_{f,t}^C$ is the wage gap at firm f between workers with and without children. We use the normalization that $\gamma_{-1} = 0$ and bin up the end points.

We are concerned that the strict exogeneity of $d_{f,t}^j$ may fail due to the presence of a time-varying latent unobserved factor η_{ft} in the error term that is correlated with both $d_{f,t}^j$ and $G_{f,t}^{\bar{w}}$. This may be leading the pre-event trends in the regression when using incumbent workers (Figure [I.35](#) panel (a)). The key question that FHS ask is: given some pre-trend in the outcome, how much of the apparent effect of the policy is due to confounds, and how much to the causal effect of the policy? The paper argues that one can still conduct valid inference on γ_j , by looking at the dynamics of a covariate x_{ft} (unaffected by the policy) around the event, and using these to correct for the role of the confound η_{ft} .

In our context, we propose to use as variable x_{ft} the wage gap of workers with and without children located at the 75th percentile (ineligible for child transfers). Intuitively, Figure [4](#) shows that the reform had a bite for low-income workers with children at the 25th percentile, but not for upper-income workers at the 75th percentile. In addition, we show below that this variable exhibits similar dynamics than the average wage gap. An analogous strategy is used in the minimum wage and youth employment literature, in which it's possible to proxy for labor market conditions using the employment of prime-age workers as measure x_{ft} , for which the effect of the minimum wage is plausibly small ([Brown, 1999](#)), lending credibility to the exclusion restriction ([Freyaldenhoven *et al.*, 2019](#)).

Figure [I.35](#) summarizes the FHS strategy. It presents event-study estimates of the coefficients γ_j for incumbent workers under various specifications. Panel (a) corresponds to our standard event study using the average wage gap (red circles), and the

wage gap at the 25th percentile (light blue triangles). The main concern is the clear pre-trend in the outcome of interest. Panel (b) shows that the wage gap of workers at the 75th percentile exhibits a pre-trend similar to that of the outcome. This is the key covariate x_{ft} that FHS leverage in their method. Intuitively, the method uses the dynamics of covariate x_{ft} in panel (b) to correct for the role of the confound η_{ft} in panel (a).

The geometry of these plots suggests an instrumental variables setup, in which panel (a) of Figure I.35 plots the reduced form for the outcome and panel (b) plots the first stage. Indeed, FHS show that γ_j can be estimated by a 2SLS regression of the outcome $G_{f,t}^{\bar{w}}$ on the policy (event) indicators $d_{f,t}^j$ and covariate x_{ft} , using the closest lead of the event $d_{f,t}^{-1}$ as an excluded instrument for x_{ft} . Using $d_{f,t}^{-1}$ as an instrument means that we need to normalize γ_j for an additional j . In the figures, we set $\gamma_{-5} = 0$.

Panel (d) of Figure I.35 uses the proposed estimator. The adjusted plot removes the estimated effect of the pre-trend from panel (a), revealing the dynamics of the outcome net of the confound, and hence γ_j in our equation (2), the causal effect of interest. The estimator proposed by FHS delivers sensible estimates of pre-trends and policy effects. In particular, we estimate a precise null wage effect on incumbent workers.

In addition, panel (c) attempts to account for the confound by extrapolating a linear trend from the eight periods immediately preceding the event. The results from extrapolating from the four (or other) periods immediately preceding the event deliver similar results. Importantly, FHS show that their 2SLS estimator outperforms the linear trend extrapolation approach.

Lastly, to validate our approach, Figure I.36 repeats this exercise using *all* the workers within the firm (i.e., incumbents and new hires). Reassuringly, in this case, the proposed adjustment makes a small difference to the point estimates (blue triangles). In the case of all workers, panel (d) of Figure I.36 shows that taking the confound into account does not alter the conclusions from the uncorrected plot in panel (a).

The results from the FHS estimator for incumbents and all workers (panel (d) of Figures I.35 and I.36) are displayed in Figure 6 in the main body of the paper.

H Other responses

H.1 Delinquency rates

The goal of this exercise is to ask whether early switchers differ from late ones and to test for the existence of financial stress experienced by firms before they enrolled in

the new system. It is an empirical question, whether the old payment system imposed a burden on firms' financial situation. We tried, with rather limited information, to approach this question by using a complementary administrative database.

To do this, we combined our set of events with the monthly financial situation of employers. This information is compiled by the Central Bank of Argentina (BCRA) into what is called the *Central de Deudores del Sistema Financiero* (CENDEU). The CENDEU records, for each taxpayer, the debts incurred with financial entities within the Argentine Financial System. The dataset is the result of information that financial institutions send to the BCRA every month; they report the following information for each debtor: the situation, amount of debt, reporting entity and date. The central bank groups taxpayers into six different categories based on the probability of default and,³⁰ following this classification, we identify high-risk debtors as those that have payment delays of more than 90 days.

Specifically, we have access to a dataset that contains the financial situation of every firm on a monthly basis for the period from April 2003 to November 2004 (20 consecutive months). Put differently, we have a time series of trends in delinquency rates for firms switching between that period. We then run a standard event-study design where we use the delinquency rate as the dependent variable.³¹

We present the event study result in Figure I.37. The left panel shows no effect of switching on financial distress. We obtain the same results if instead we re-define risky debtors as those with payment delay of more than 180 days. When we break the analysis by firm size on the right panel, we observe a precisely zero effect for small firms, i.e., those that drive the wage effects, and a decreasing trend, that we are not able to remove with the specification that we propose, for large firms (this decreasing trend in delinquency rates over time is likely the result of an economy that was recovering from the 2001/2002 crisis).

H.2 Bunching at *notches*

Under the old payment system, the transfer was very salient to both employers and employees (see Figure I.3). This salience could, in principle, affect the collusion between employers and workers to extract rents from the transfer scheme, in the spirit of [Van Doornik et al. \(2023\)](#) (they identify strategic behavior in claiming unemploy-

³⁰The categories are the following: [1.] Normal: delay in payments less than 31 days, [2.] Low risk: delay between 31 to 90 days since maturity, [3.] Medium risk: delay in payments between 91 and 180 days, [4.] High risk: delay great than 180 days but less then a year, [5.] Irretrievable: delay greater than a year and [6.] Irretrievable for technical reasons: debt with an ex-entity.

³¹The dummy variable takes a value of one if we are referring to a risky debtor i.e., more than 90 days overdue, or zero in any other case.

ment benefits). Before the reform, the greater visibility of the transfer made it easier for employers to keep workers' salaries below the notch so that they could benefit from the transfer; therefore, there was space for collusion because the employer was relatively more aware of the transfer.

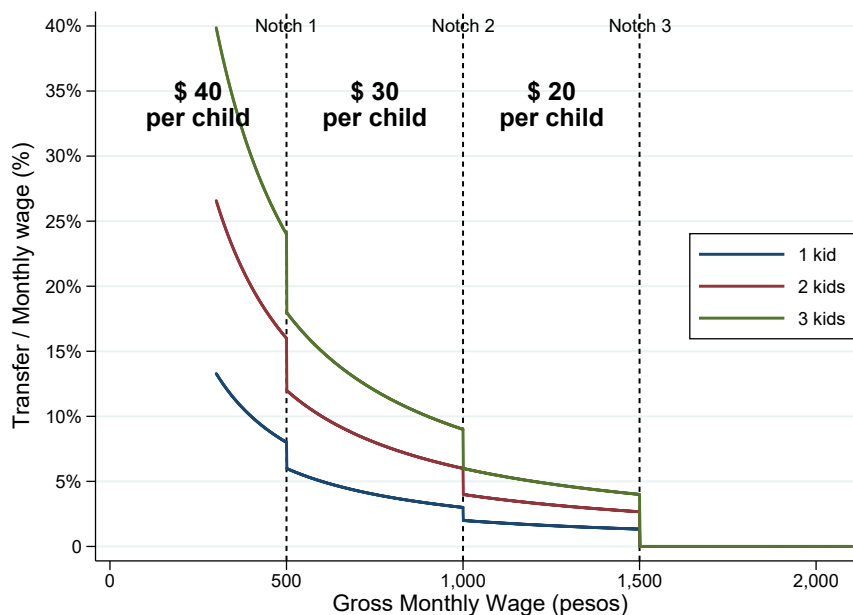
Empirically, if this is what occurred, we should observe bunching to the left of the thresholds for eligible workers with children compared to the distribution of those without children. The gradual roll out should then be translated into a gradual decrease of the bunching behavior. Intuitively, under the new system, the situation is more opaque to employers about where the notches are and who is currently receiving the transfer. Figure I.38 (a) presents the distribution of employees grouped in bins of 20 pesos and the three transfer notches, with the minimum wage added as a reference point for August 2004. To illustrate the discontinuity induced by the transfer scheme, we also plot the theoretical average tax credit (i.e., the ratio of transfer to earnings) for workers with two children, as a way of identifying the location of the bunching and the strength of the incentives to bunch. As opposed to *kinks*, notches imply that there exist dominated areas, and therefore large incentives to remain below the threshold. In our setting, moving above the notch means that the firm pays more and, at the same time, workers receive less income (including the transfer). We observe in the figure that there is no clear bunching in the last two notches (even when zooming into neighboring areas). There seems to be something in the first notch, but we show later that it is confounded by something else.

In Figure I.38 (b), we break down the previous figure by number of children because, given a particular earnings level, incentives operate more strongly for workers with many children. Again, there is no visible bunching at any *notch*. Although there are some spikes, there is no clear pattern between those workers with and without children. Reasons for the lack of a pattern may include the following. First, there are no incentives to bunch if there is low enforcement in general and at the *notches* in particular. To rule out this alternative, we look at the empirical first stage, that is, what happens with the transfer at the *notches*. In Figure I.39, we plot the median and average transfer, grouping workers in bins of 20 pesos. We confirm that the *notches* are properly enforced because there is a discontinuity in the transfer paid right at the threshold. Second, there are frictions and labor market regulations that make it difficult to collude and therefore to bunch at the *notch*.

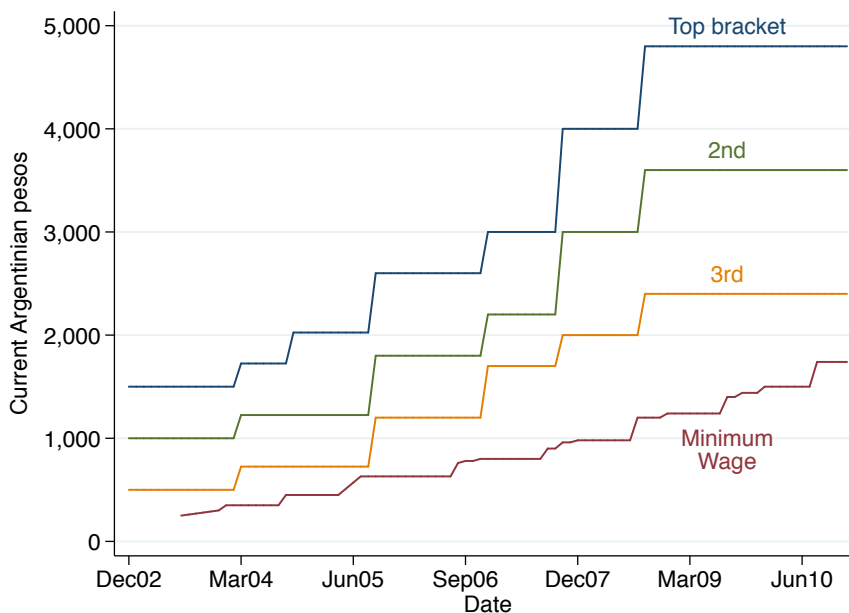
I Appendix Figures and Tables

Figure I.1: Family allowance schedule

(a) Transfer as a proportion of wage (1996-2004)



(b) Bracket thresholds (2003-2011)

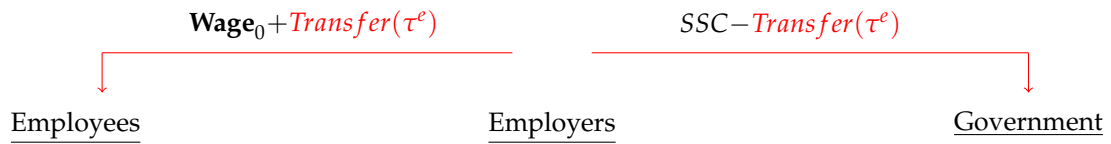


Source: Author's elaboration based on official documentation.

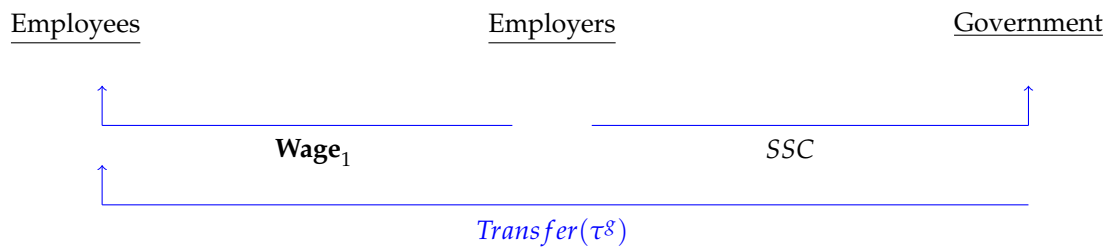
Notes: Panel (a) shows the transfer as a proportion of monthly wages. Each line corresponds to a different number of children below 18 years old. Panel (b) presents the three upper monthly thresholds of each bracket; the series at the bottom represents the evolution of the minimum wage. All series are expressed in current Argentinian pesos. Table A1 provides a complete picture of this scheme by year, including the evolution of the brackets and the exact benefit amount per child.

Figure I.2: The reform: A change in the payment system

Old system (SFC)

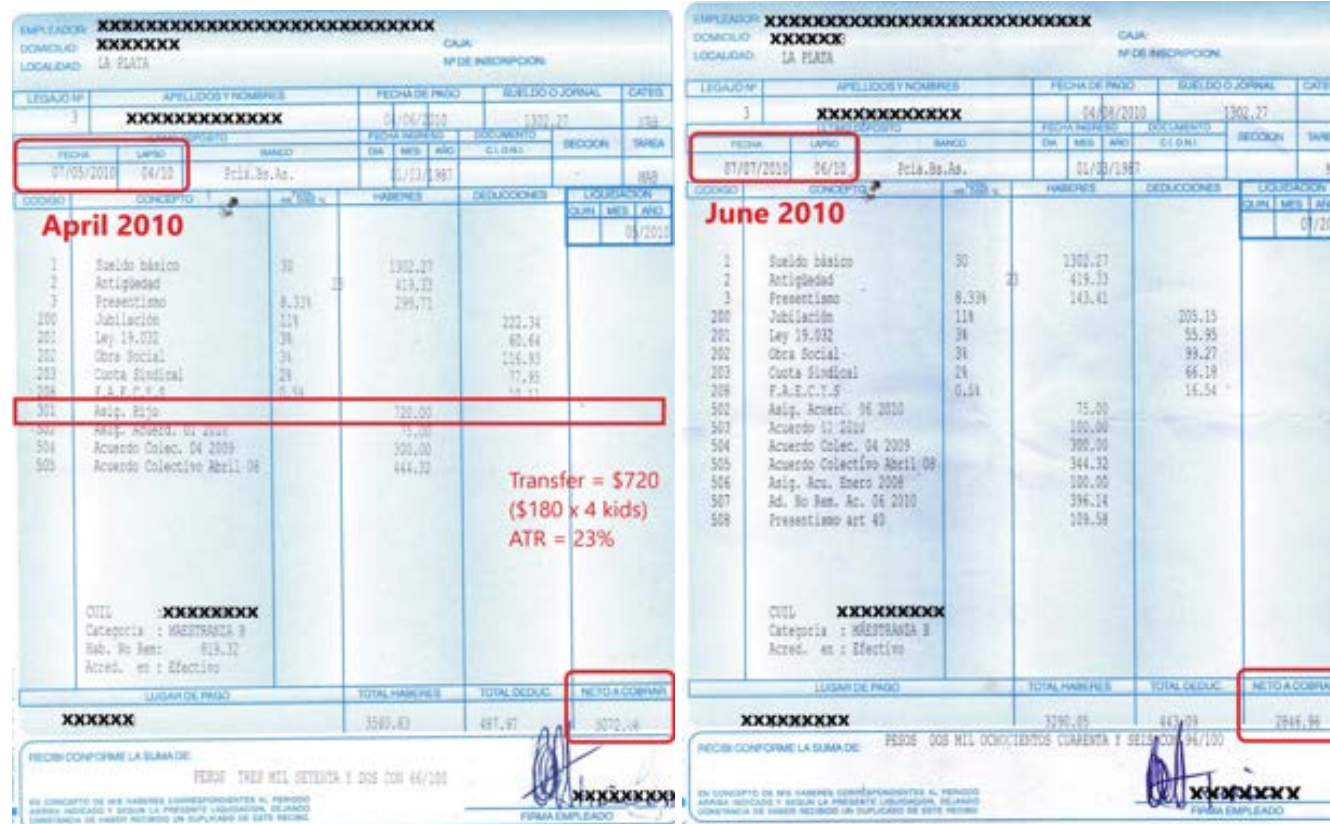


New system (SUAF)



Notes: This diagram illustrates the change in the payment system of family allowances. Under the old system (SFC), employers had responsibility for delivering child benefits together with the monthly wage. For transparency purposes, the government replaced the intermediary role of firms and started depositing the transfer directly into workers' bank accounts. In the new system (SUAF), firms only had to remit payroll taxes (SSC) to the tax authority. The question our paper asks is whether $Wage_0$ and $Wage_1$ are the same for eligible workers with children before and after the change.

Figure I.3: Saliency of the transfer on a worker's pay slip



(a) Before the firm switched

(b) After the firm switched

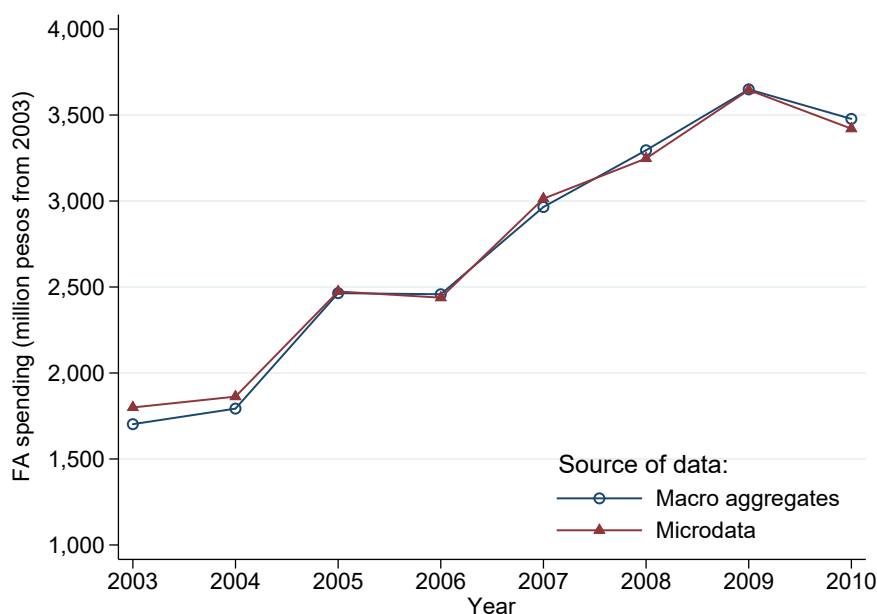
Notes: This figure shows the pay slip of a worker right before and right after the firm switched from the old payment system (SFC) to the new payment system (SUAF). In the old system (panel a), the pay slip contains a line for the transfer, "Asig. Hijo", making it very salient to both the employer and the employee. The amount that this worker receives is 720 Argentinian pesos corresponding to an average tax credit of 23 percent of total wage earnings. In the new system (panel b), that line disappears and the worker receives the transfer directly from the government in his checking account. As a result, the take-home pay of this worker decreases from 3072.66 to 2846.96, but the worker is still receiving the transfer in his bank account.

Figure I.4: Firms' incorporation steps into the new payment system



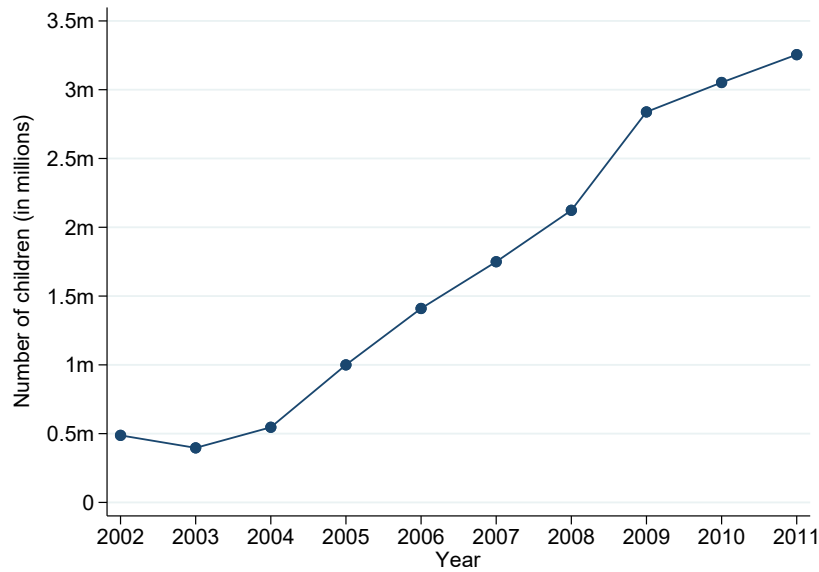
Notes: This figure shows the timeline of the incorporation steps to the new payment system (SUAF). This process was determined by the SSA through official memos posted online. The incorporation started with the SSA setting an *internal* incorporation schedule, where the SSA issued a list of targeted firms that would be gradually incorporated up to a certain point in time. Firms were then contacted by an officer and notified to present certain documentation to be incorporated into the system (payroll, beneficiaries, bank accounts). The last step consisted of the formal approval and incorporation into the new system. Employers were required to notify their employees via an individual form to be signed by both parties (affidavit).

Figure I.5: Macro and micro aggregates comparison



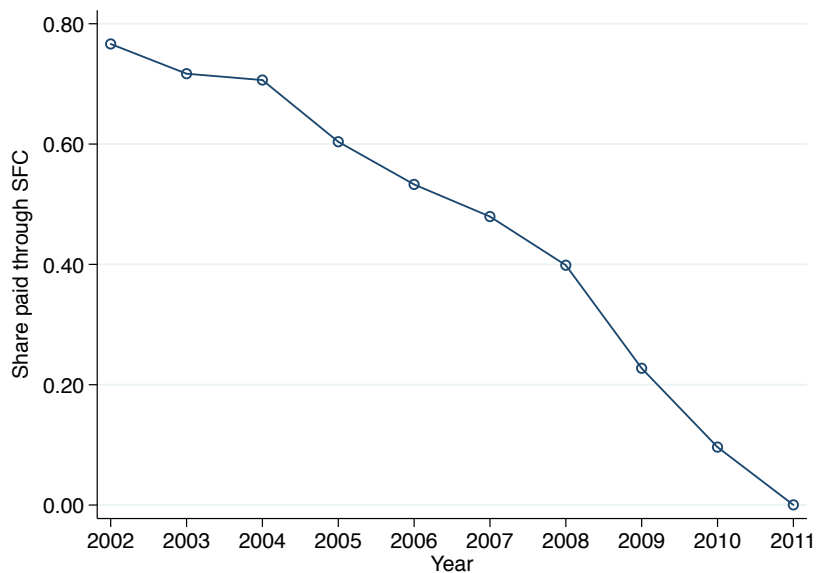
Notes: This figure shows the total expenditure on family allowances in real terms (old and new system). The blue connected dots present the macro total available in official budget information (data extracted from *Cuenta de Inversion, Contaduría General de la Nación* and *Informe Gerencial (AFIP)*) while the red triangles indicate the total estimated spending using the employer-employee micro-data adding up the transfer amounts reported by employers.

Figure I.6: Beneficiaries (number of children)



Notes: This figure shows the number of children receiving the child benefit between 2002 and 2011. Reassuringly, the number does not decrease during the transition from the old to the new system. The sharp increase could be due to the fact that the economy was booming and there was a formalization process carried out by the tax authority.

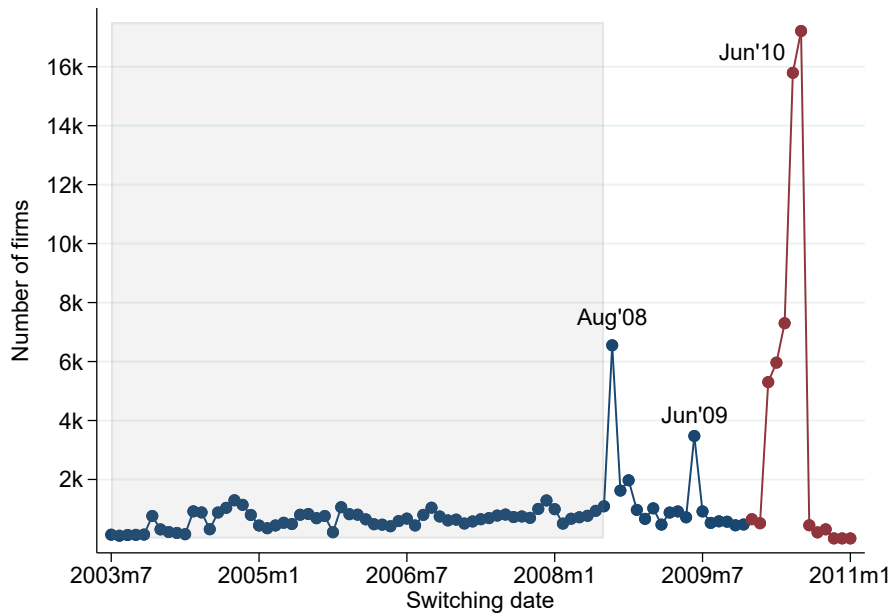
Figure I.7: Macro roll-out (official budget information)



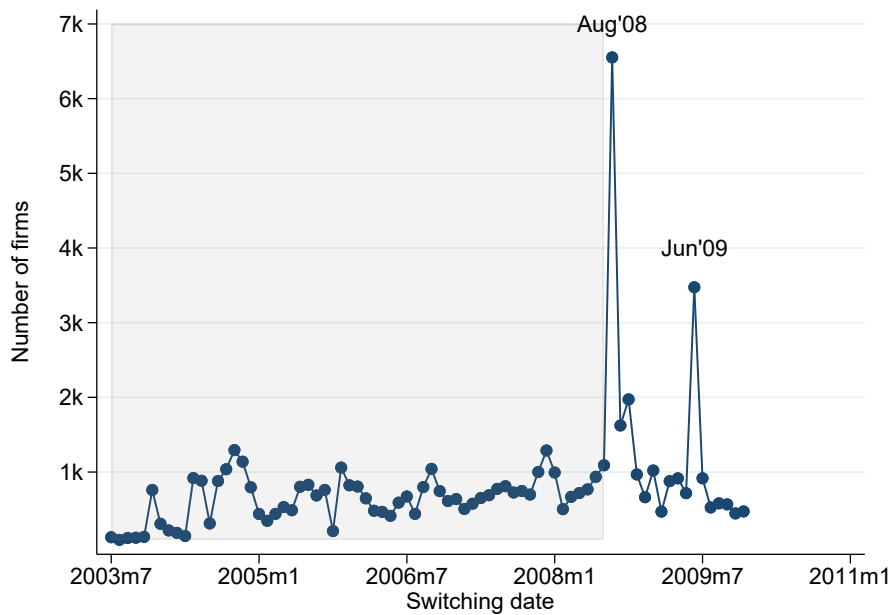
Notes: This figure shows the share of family allowances paid under the old system (SFC). The aggregate expenditure on family allowances is taken from official budget information (*Cuenta de Inversion, Contaduría General de la Nación* and *Informe Gerencial* (AFIP)). The gradual decline in this share illustrates the staggered transition to the new payment system.

Figure I.8: Event frequencies per month-year (number of firms)

(a) Full period 2003-2010



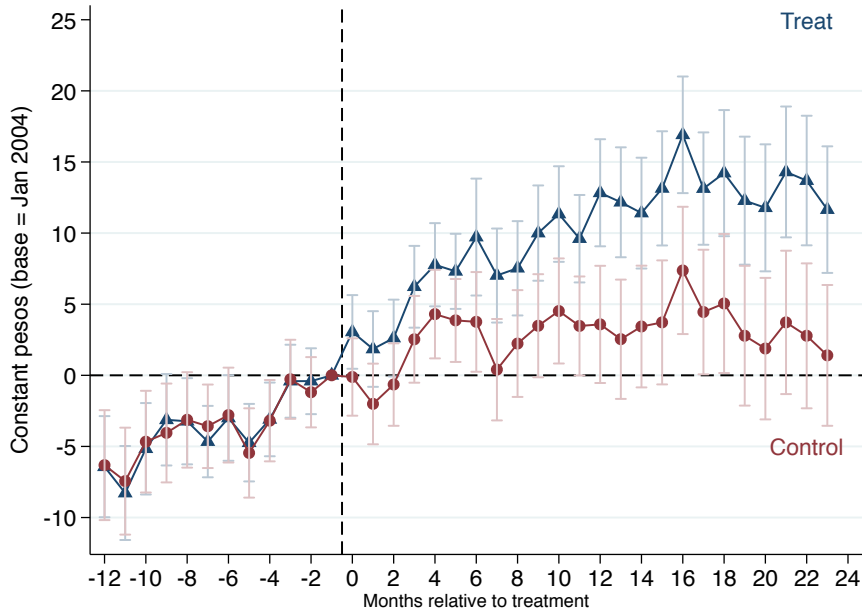
(b) Zoom in before 2010



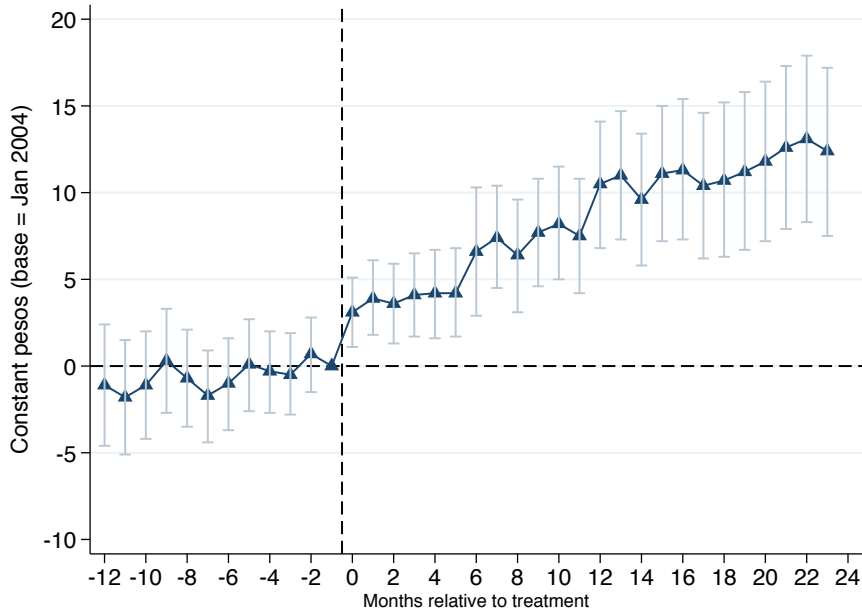
Notes: These figures show the number of firms switching to the new system at each month-year of our micro-data. Panel (a) shows the full period from 2003 to 2010 and panel (b) restricts the graph to pre-2010 data to provide a clearer picture. The spikes correspond to three massive incorporation dates: August 2008 (Great Recession), June 2009, and March-July 2010. Source: Author's elaboration based on employer-employee micro-data.

Figure I.9: Reduced-form wage effects (longer run)

(a) Average wage levels

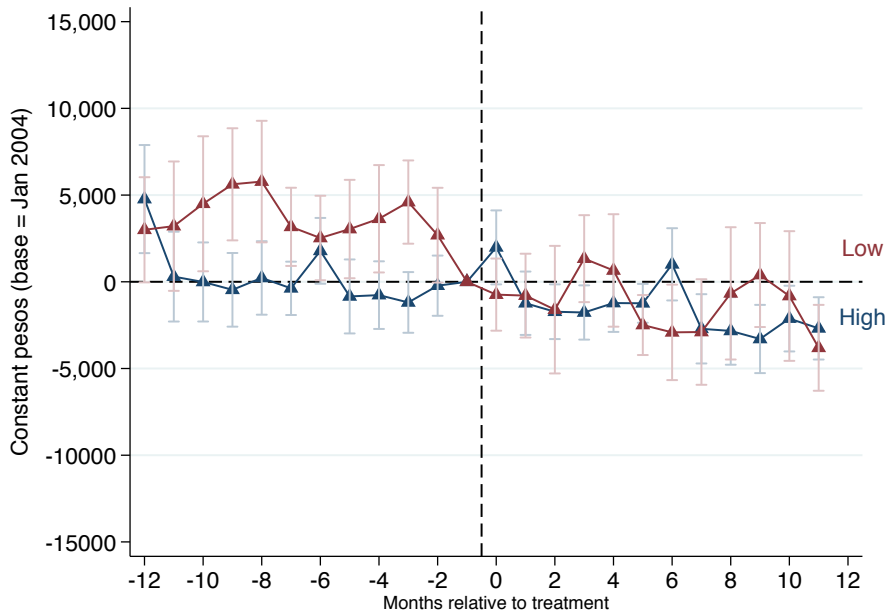


(b) Average wage gap



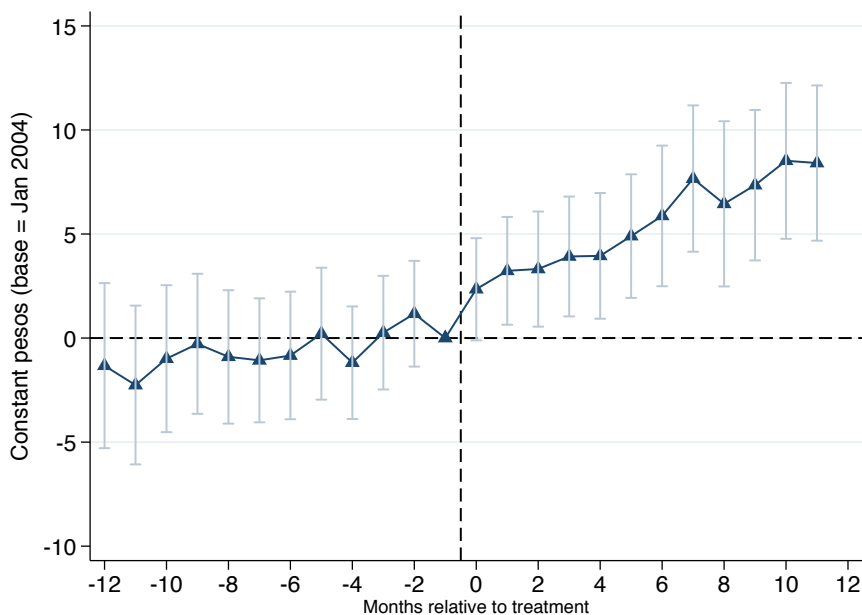
Notes: These figures plot the event-study estimates of parameter γ and its corresponding 95-percent confidence intervals of equation (2) focusing on 12 months before and 24 months after the switch. Panel (a) shows the wage levels when we estimate this equation separately for workers with children (treatment) and without children (control). In Panel (b), the dependent variable is the within-firm average wage gap of these two groups. Both figures provide clear evidence of the effect on wages as a result of the change in the payment system.

Figure I.10: Evolution of total wage bill by treatment intensity



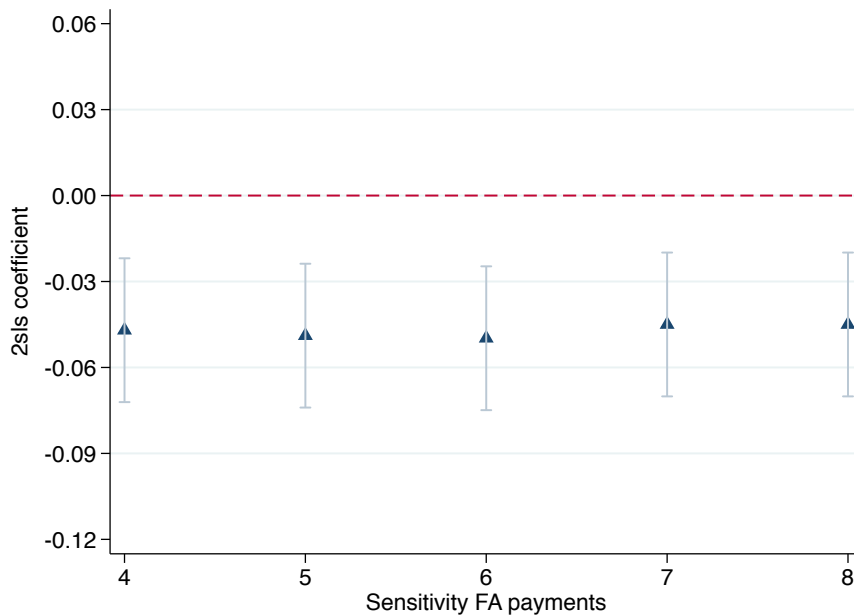
Notes: This figure plots the event-study estimates of parameter γ and its corresponding 95-percent confidence intervals using as the dependent variable the total wage bill. The blue series correspond to firms with a share of workers with children at $t = -1$ above the median (high treatment intensity). The red series correspond to firms with a share of workers with children at $t = -1$ below the median (low treatment intensity).

Figure I.11: Balanced panel of firms present in the 96 months of data



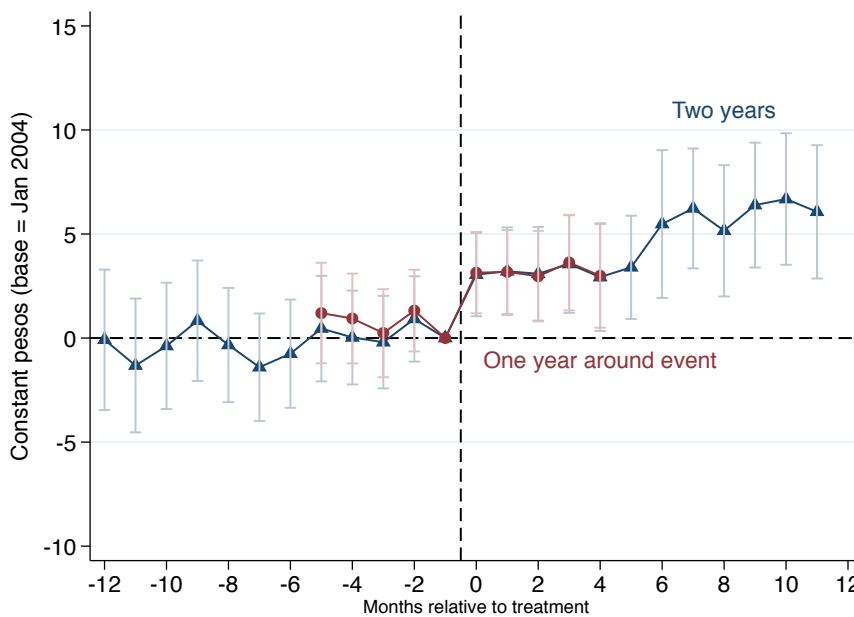
Notes: This figure plots the event-study coefficients and 95-percent confidence intervals of equation (2). It shows that results remain unchanged when considering a balanced panel of firms present in the 96 months of data.

Figure I.12: Sensitivity to months of transfer payments before the event



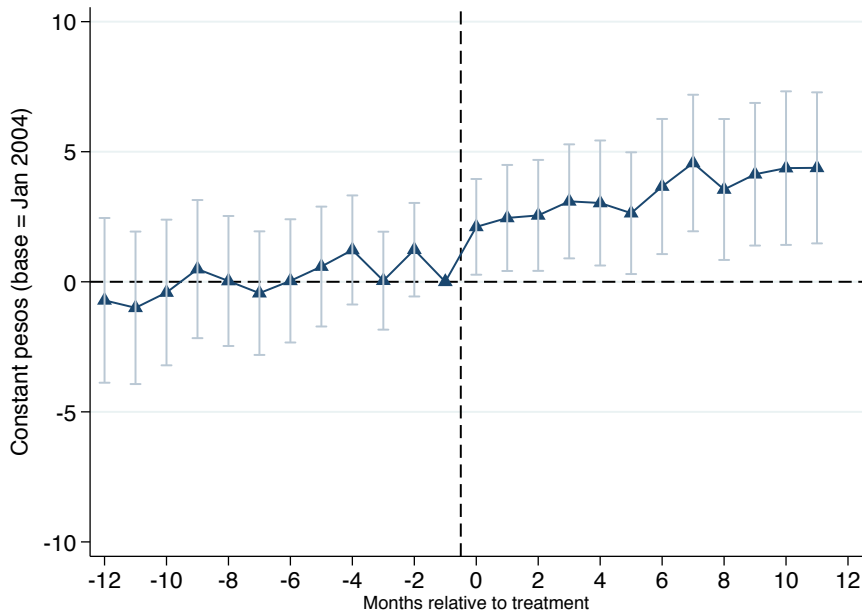
Notes: Each dot in this figure corresponds to a different reduced-form coefficient of equation (3) scaled by the first-stage change in the remittance of benefits, where we vary the sample of firms according to the number of months that each firm was paying family allowances (FA) right before the event. We consider firms paying at least 4, 5, 6, 7, and 8 months respectively. The result is very stable across specifications.

Figure I.13: Sensitivity to the length of the event window



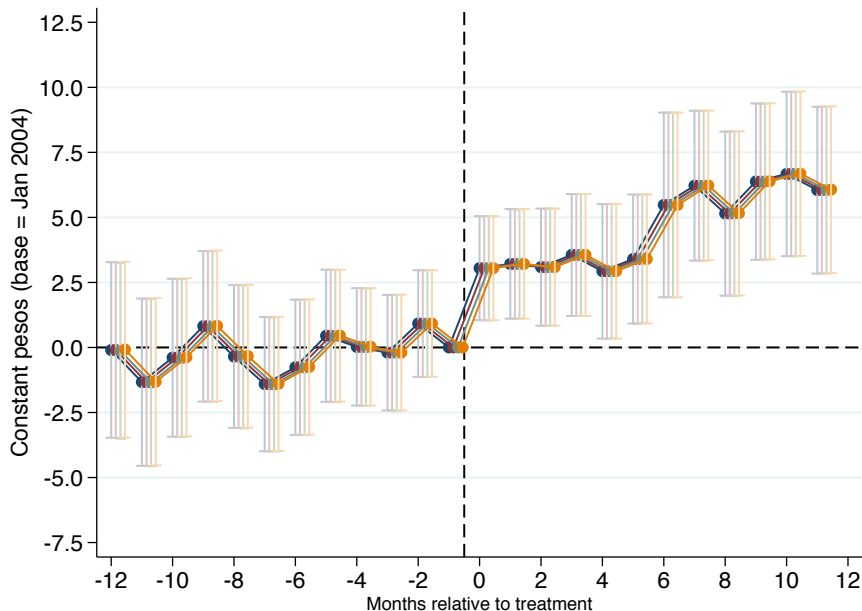
Notes: This figure plots the event-study coefficients and 95-percent confidence intervals of equation (2). The figure shows that results remain unchanged when considering a time window of 6 months before and after the event (red line) instead of 12 months (blue line).

Figure I.14: Alternative treatment group definition (always treated workers)



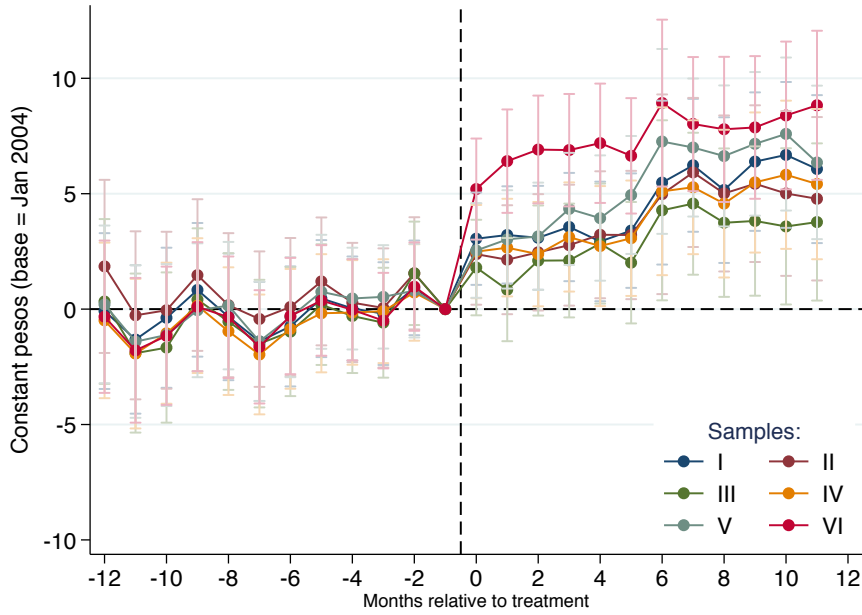
Notes: This figure plots the event-study coefficients and 95-percent confidence intervals of equation (2). It shows that results remain unchanged when using an alternative definition of the treatment group that considers workers who are fully treated during the period 2003-2010, i.e., those with children less than 18 years old during the entire roll-out period.

Figure I.15: Wage effects under alternative specifications with controls



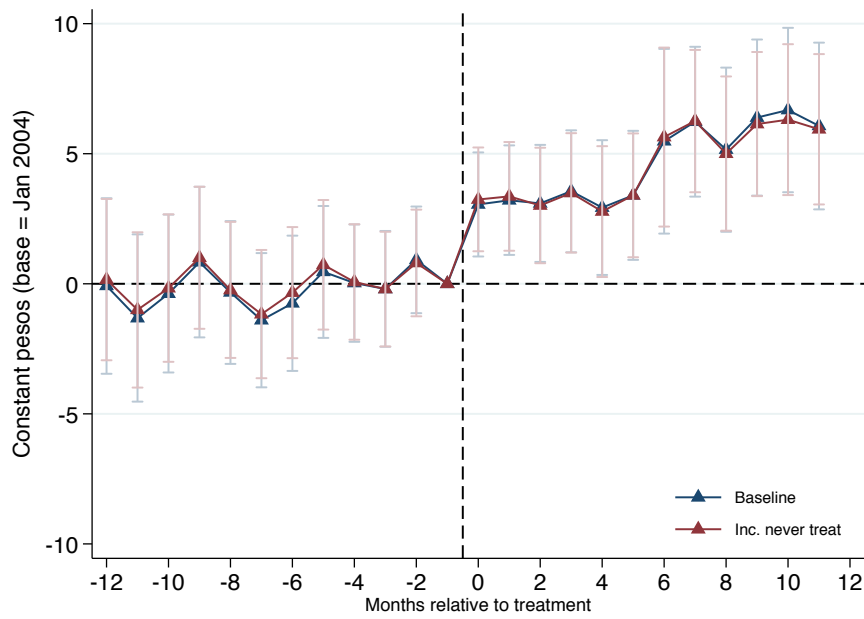
Notes: This figure plots the event-study estimates of parameter γ and its corresponding 95-percent confidence intervals for different specifications with and without controls. The blue line corresponds to our baseline estimate of equation (2). In the other series, we include controls such as firm size (maroon), the gap in the number of treated and control workers (teal), and its square (yellow). The point estimate and the standard errors remain unchanged after adding this set of controls.

Figure I.16: Wage effects using alternative samples



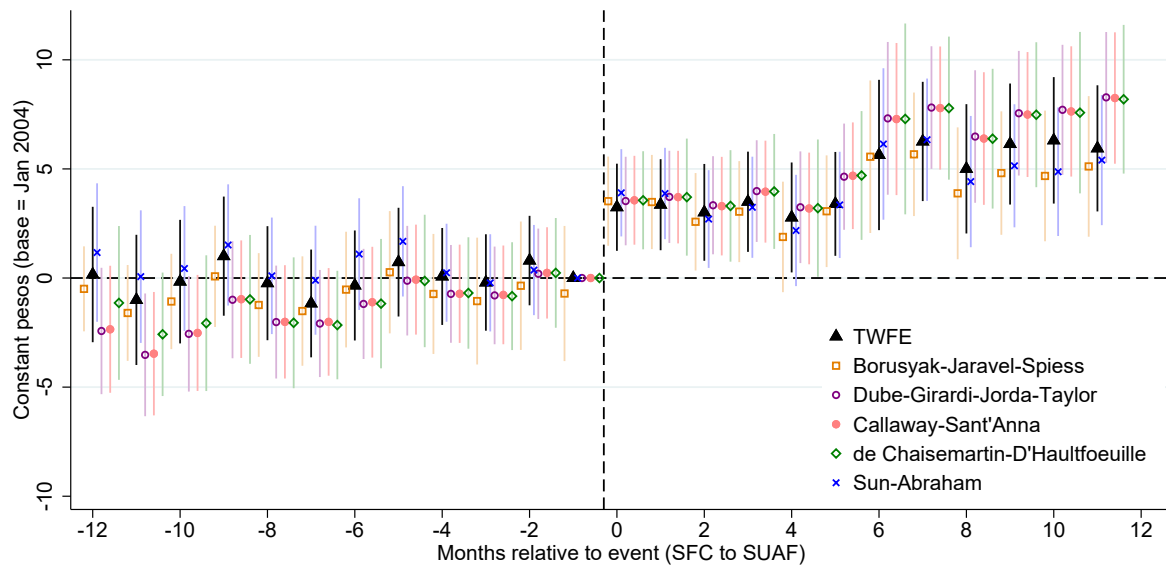
Notes: This figure plots the event-study estimates of parameter γ and its corresponding 95-percent confidence intervals of equation (2) for different subsamples: (I) baseline sample (includes firms with more than one worker receiving the transfer at $t - 1$), (II) adds to (I) the restriction of having the same event date and formalization date in the memo, (III) firms having the same date and one worker receiving the benefit at $t - 1$; (IV) firms with more than one worker receiving the benefit in each month for the period $[-6; -1]$; (V) firms with at least three workers receiving the allowance before the event date ($t - 1$) and (VI) no restriction.

Figure I.17: Wage effects including never-treated firms



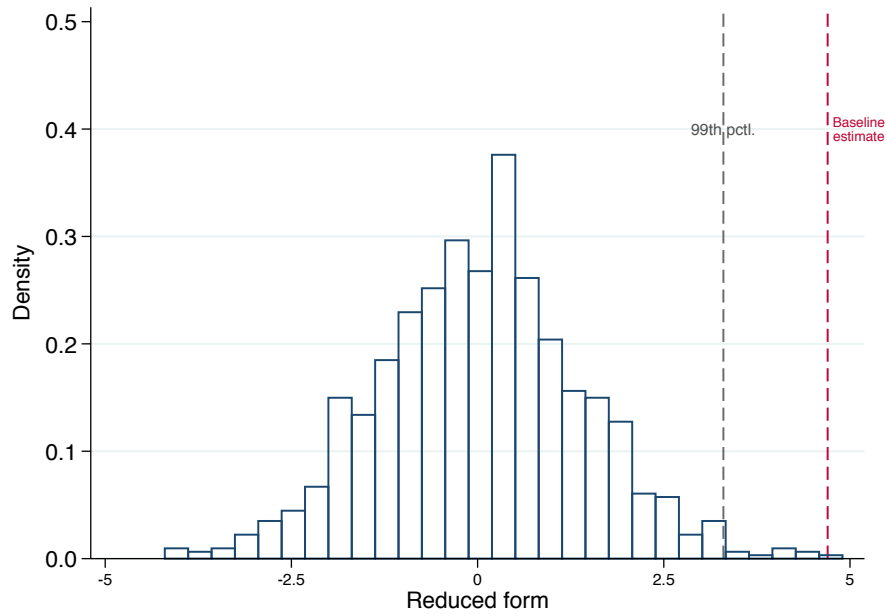
Notes: This figure plots the event-study estimates of parameter γ and its corresponding 95-percent confidence intervals of equation (2) for two different subsamples. The blue series refers to our baseline specification while the red series adds never-treated firms. In our setting, untreated firms are those that 1) made no payment under the old system in any of the years included in the data to which we have access; and 2) have both treated and control workers. For more details see Figure 1 and its corresponding footnote.

Figure I.18: Wage effects using heterogeneity-robust methods



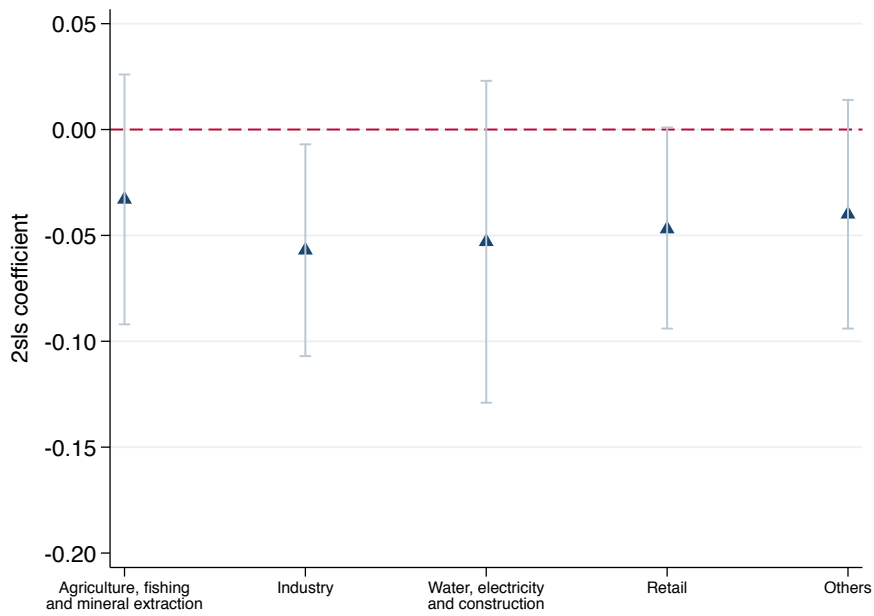
Notes: This figure plots the event-study coefficients estimated with five heterogeneity-robust difference-in-differences methods and the conventional TWFE regression. The dependent variable is the within-firm average wage gap between workers with children (treatment) and without children (control). Black triangles denote the TWFE specification that includes never-treated firms. Orange squares show the imputation approach proposed by [Borusyak et al. \(2021\)](#). Purple circles show the local projection estimates of [Dube et al. \(2023\)](#). Pink circles correspond to the estimates from the [Callaway & Sant'Anna \(2021\)](#) method. Green diamonds correspond to the switchers approach of [de Chaisemartin & D'Haultfoeuille \(2023\)](#). Blue crosses show the interaction weighted estimates of [Sun & Abraham \(2021\)](#). In all the cases, we use 'not-yet-treated' firms (including the never-treated) as the control group. Vertical bands denote 95-percent confidence intervals based on standard errors clustered by firm.

Figure I.19: Placebo test using fake event dates (wage effects)



Notes: This figure plots the distribution of reduced-form coefficients of equation (3), where each of these coefficients is the result of assigning a fake event date to each firm and then re-estimating the wage-effects. For this figure, we focus on firms that have both types of workers throughout the period so that we can estimate the effect regardless of the event date that we assign. We replicate this exercise i.e., assign an alternative date and re-estimate, 1,000 times so that we end up with a distribution of simulated reduced-form estimates. We highlight the location of the 99-percent confidence interval (dashed grey vertical line) as well as our baseline reduced-form coefficient (red vertical line).

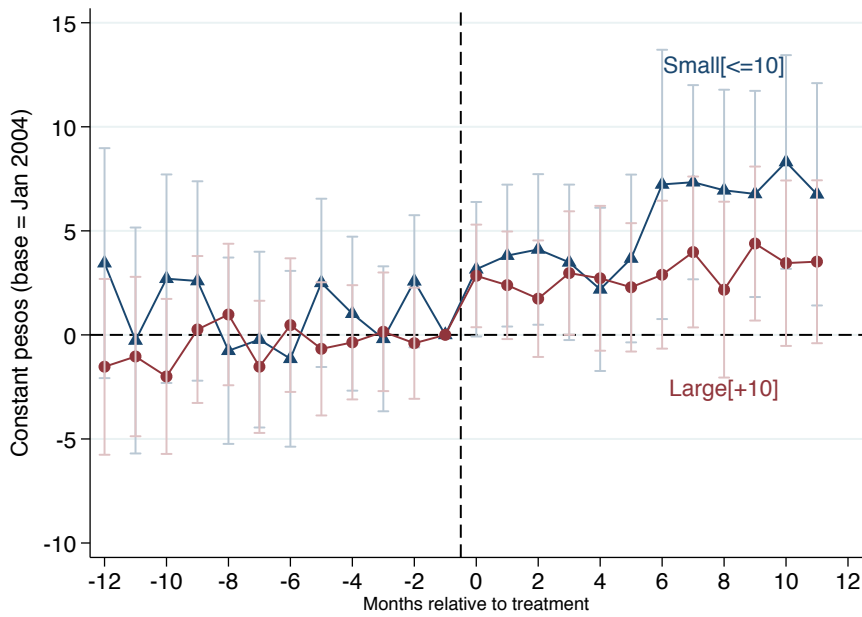
Figure I.20: Pass-through across sectors



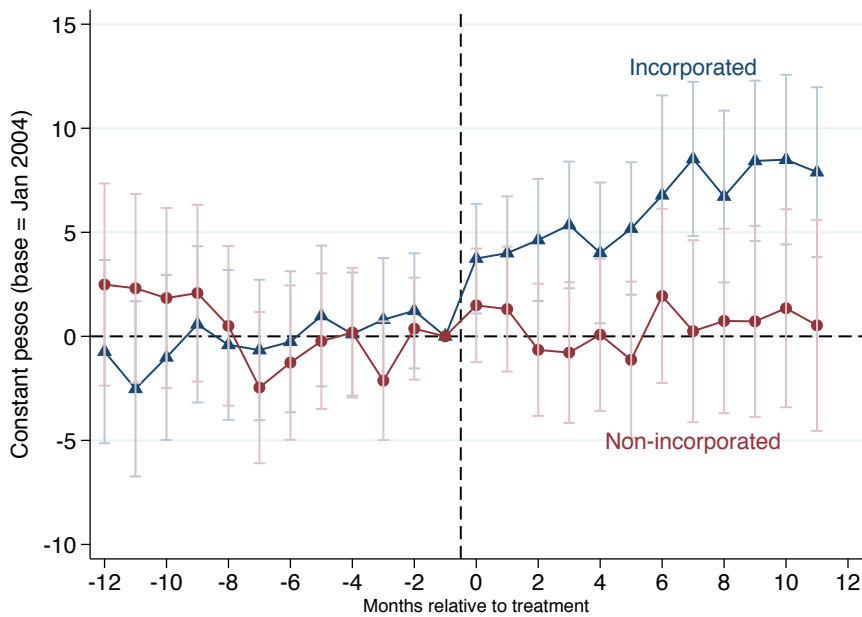
Notes: Each dot in this figure corresponds to a different reduced-form coefficient of equation (3) scaled by the first-stage change in the remittance of benefits; each dot corresponds to a separate regression of a given sector. We identify the following sectors: [a] Agriculture, fishing and mineral extraction, [b] Industry, [c] Water, electricity and construction, [d] Retail and [e] Others.

Figure I.21: Firm size and type of business

(a) Small vs. large firms



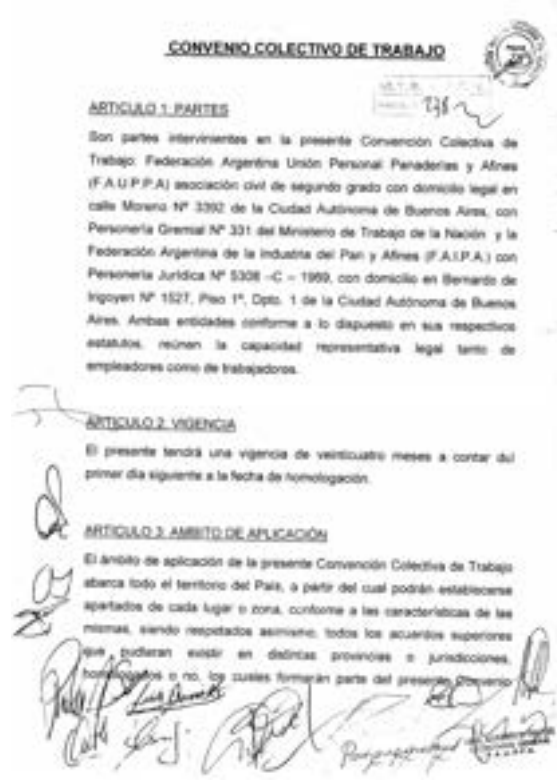
(b) Incorporated vs. unincorporated



Notes: Panel (a) presents event-study estimates of parameter γ and its corresponding 95-percent confidence intervals of equation (2) for small and large firms (with ten or fewer employees, and more than ten employees, respectively). Panel (b) plots the event-study estimates for incorporated and unincorporated businesses.

Figure I.22: Collective agreement

(a) Example of a collective agreement

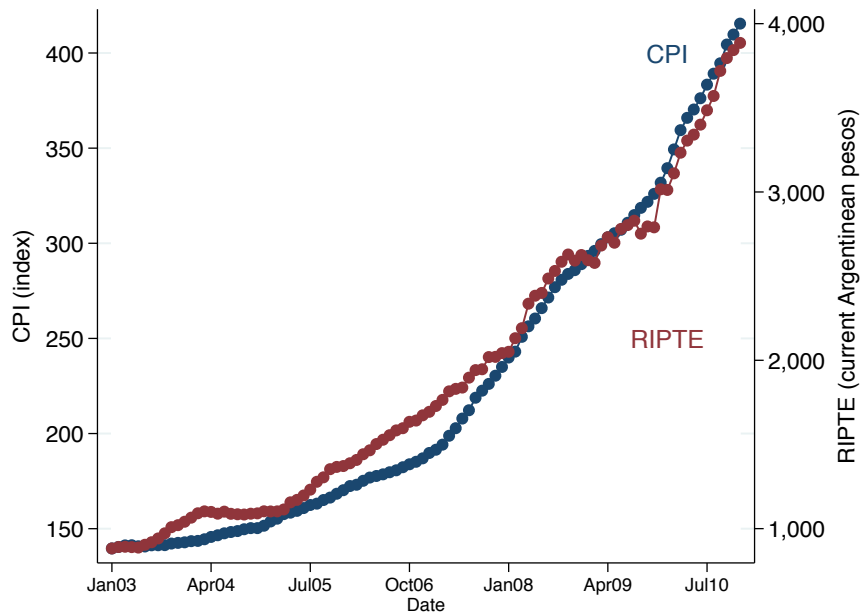


(b) Summary of a collective agreement

CONVENIO COLECTIVO DE TRABAJO 1523/2016 [CCT-1523-2016-E]					
Nivel : EMPRESA	SubNivel : Empresa Única	Celebración 29-09-2015	Publicación en BO	No Informada	Actividad PETROLEROS
Partes	Sindicato/s Empleador/s	SUPEH - FEDERACION SINDICATOS UNIDOS PETROLEROS E HIDROCARBURIFEROS YEL INFORMATICA S.A.			
Vigencia	Salarial General	Desde 1/9/2015 hasta 31/8/2016 Desde 1/9/2015 hasta 31/8/2018			
Ambito Territorial de Aplicación	Ambito Territorial	No abarca ni siquiera una zona de tipo provincia			
	Territorio Congregado	AMBITO DE LA EMPRESA - REFINERIA BARADERO 777 - LA PLATA, SEDE CENTRAL TUCUMAN 744 - CABA Y PETROQUIMICA AV. GOBERNADOR VERGARA KM 27 - LA PLATA			
Ambito Personal de Aplicación	Personal Incluido	TRABAJADORES DE LA EMPRESA QUE SE DESEMPEÑEN EN TODA ACTIVIDAD QUE SE DESARROLLE EN LOS AMBITOS DE EXPLORACION, PRODUCCION, INDUSTRIALIZACION, COMERCIALIZACION, TRANSPORTE Y ADMINISTRACION DEL PETROLEO Y SUS DERIVADOS EN CONTINENTE Y COSTA AFUERA			
	Personal Excluido	NO ESPECIFICA			
Contenidos discutidos		ADICIONAL TAREAS DE TURNO; ANTIGUEDAD; APORTE SOLIDARIO; COMPENSACION BRIGADA CONTRA INCENDIOS; CONTRIBUCION EMPRESARIA; CUOTA DE AFILIACION; DIA DEL TRABAJADOR PETROLERO; FORMAS DE CONTRATACION; HORAS EXTRAS; INFORMACION Y CONSULTA; JORNADA LABORAL; TAREAS INSALUBRES; FRANCS NO APLICADOS; LICENCIA ORDINARIA; LICENCIAS ESPECIALES REMUNERADAS; NACIMIENTO DE HIJO; MATRIMONIO; FALLECIMIENTO DE FAMILIARES; EXAMEN, ADOPCION, RAZONES DE SALUD; SITUACIONES DE FUERZA MAYOR; FENOMENOS METEOROLOGICOS; DONACION DE SANGRE; PERIODO DE PRUEBA; POLIVALENCIA LABORAL; PRESENTISMO; RECONOCIMIENTO MUTUO; REDUCCION DE JORNADA POR MOTIVOS FAMILIARES; REGIMEN DE OBRA SOCIAL; SEGURIDAD E HIGIENE INDUSTRIAL; SUELDO ANUAL COMPLEMENTARIO; SUELDO BASICO; TELETRABAJO; VANDA AYUDA ALIMENTARIA			
Convenio, Acuerdo o Lavado		CCT-1523-2016-E			
Norma Homologatoria		Norma Homologatoria			
Información Relacionada al Convenio, Acuerdo o Lavado		ANEXO - ESCALA SALARIAL			
		ANEXO - RES. TOPE INDEMNIZATORIO			

Notes: Panel (a) contains a screenshot of the first page of a collective agreement. This is a standard type of agreement where the different articles (ARTICULO) describe what has been discussed and/or negotiated. Panel (b) presents a summary of the information extracted from a given collective agreement (CCT – 1523 – 2016 – E). This agreement is at firm level (Nivel: EMPRESA), was ratified in September 29th 2015 (Celebración: 29-09-2015) and it affected workers in the oil sector (Actividad: PETROLEROS). Moreover, the main provisions of the agreement are also enumerated (Contenidos discutidos: ADICIONAL TAREAS DE TURNO; ANTIGUEDAD; APORTE SOLIDARIO, etc). In addition, firm’s name is available within the extracted information (Empleador/s: YEL INFORMATICA S.A.).

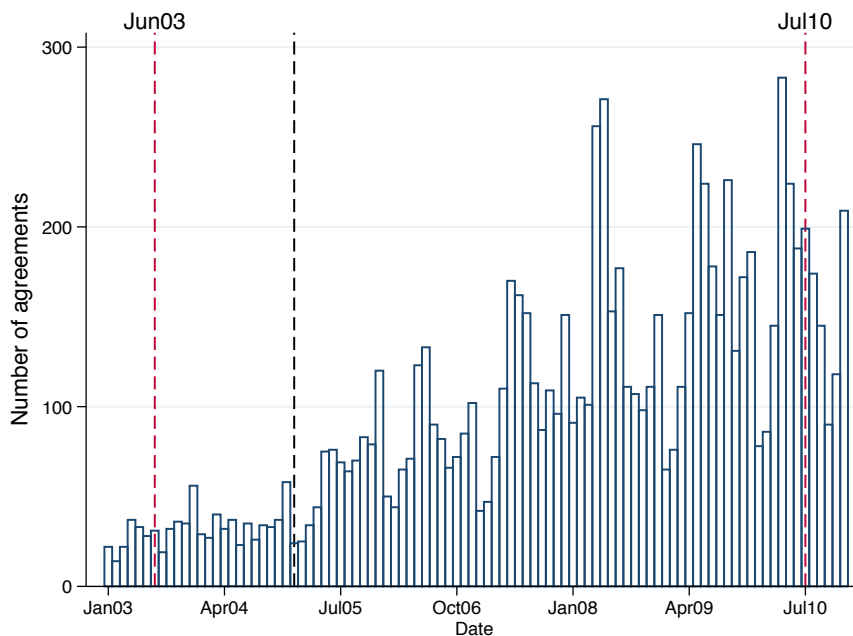
Figure I.23: Monthly evolution of inflation and nominal wages (2003-2010)



Notes: CPI denotes consumer price index while RIPTE denotes the average salary of registered workers (in current pesos).

Source: Ministry of Labor, Argentina.

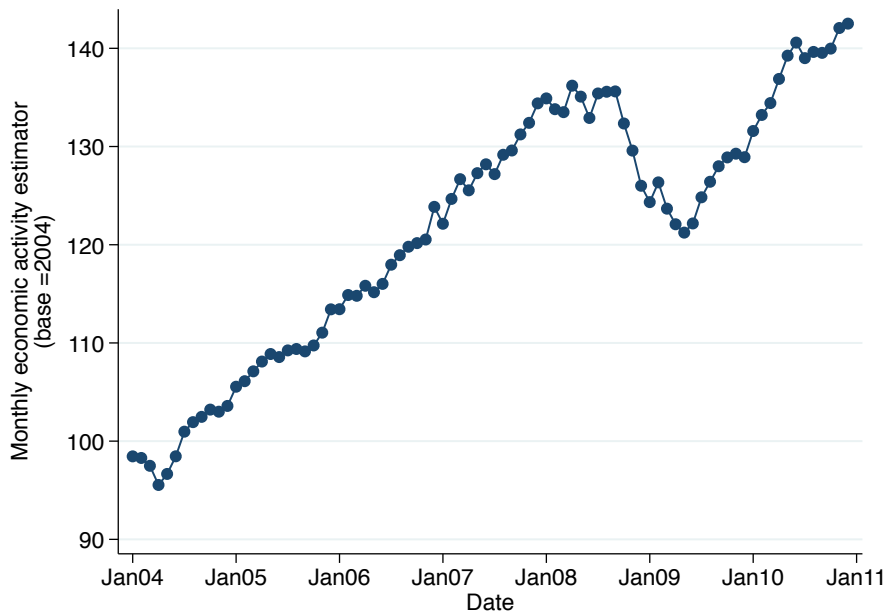
Figure I.24: Issuance of collective bargaining agreements (2003-2010)



Notes: Each bar on the vertical axis measures the number of collective agreement by month of issuance. Approximately, two-thirds of them are firm-level agreements.

Source: Author's elaboration based on a dataset containing the universe of collective agreements in Argentina.

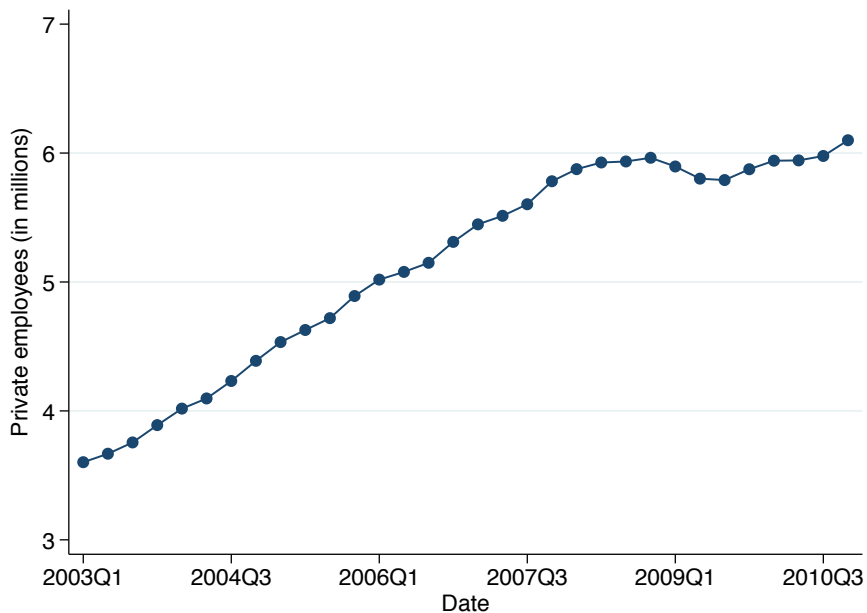
Figure I.25: Monthly evolution of the economic activity estimator (2004-2010)



Notes: The vertical axis measures the monthly economic activity indicator as a function of time. We observe a large drop in economic activity from August 2008 onwards.

Source: National Statistical Office of Argentina, *Instituto Nacional de Estadística y Censos* (INDEC).

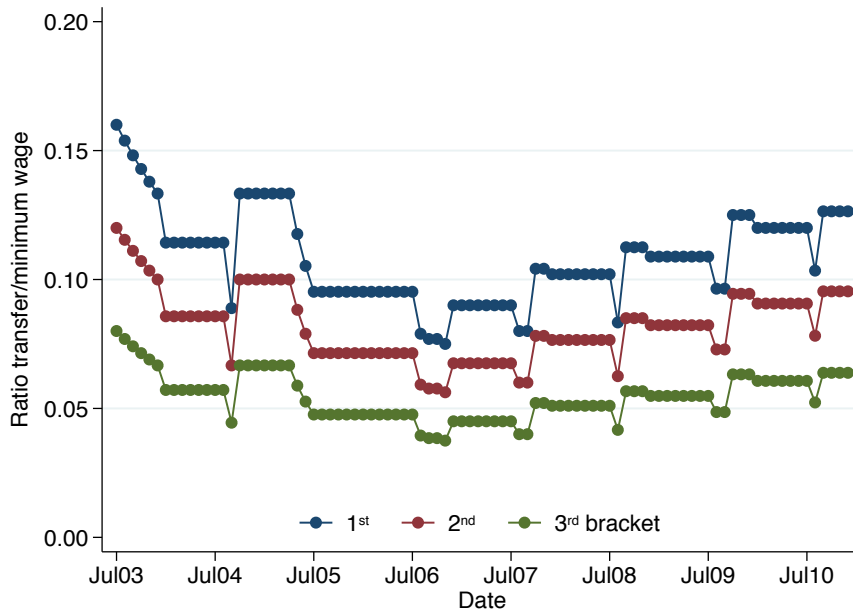
Figure I.26: Quarterly evolution of private employment (2003-2010)



Notes: The figure presents the evolution of registered private wage employees for the years 2003-2010. The period is characterized by a steady increase in the number of registered workers followed by stabilization of employment since the third quarter of 2008.

Source: Ministry of Labor, Argentina.

Figure I.27: Evolution of the average tax rate of the family allowance (2003-2010)

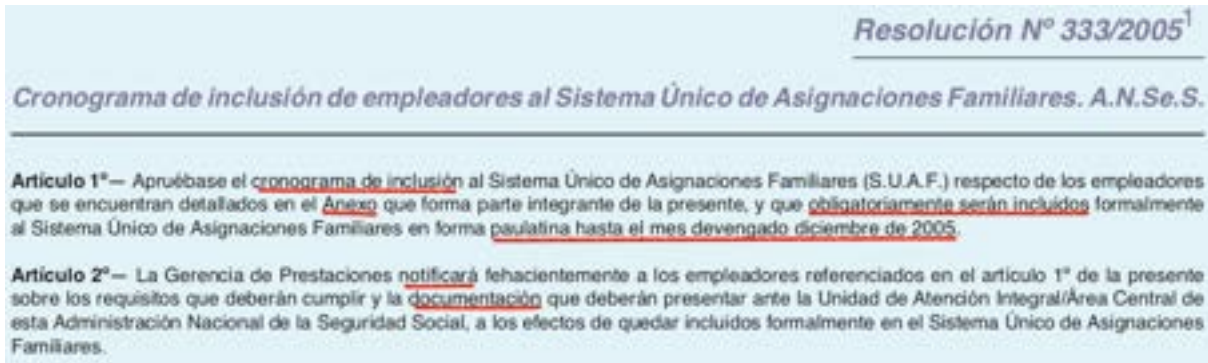


Notes: The vertical axis presents a proxy for the average tax rate (ATR), i.e., the ratio of the transfer normalized by the minimum wage, for three transfer amounts. The figure shows that the ATR remains roughly constant during the period of analysis.

Source: Author's elaboration based on official documentation.

Figure I.28: Incorporation schedule memo

(a) Resolution (body text)



(b) Resolution appendix (with employer's identifiers)

ANSES **333** 2005 - AÑO DE HOMENAJE A ANTONIO BERNABÉ

Ministerio de Trabajo, Empleo y Seguridad Social

ANEXO

20-05047024-6	JUAN NESTOR NARCISO
20-07924169-6	BORDA PAULINO APARICIO
20-14131275-9	WINGEYER HUGO DANIEL
20-17071721-0	BALLARIO JORGE ALBERTO
20-17639159-7	FRIGERIO FERNANDO DANILO
20-20195515-8	CIPOLLONE RAUL ALBERTO
20-22126363-5	RUIZ DIAZ EULOGIO ANTONIO
23-05243056-9	MOROSI RICARDO EDER
23-14940864-9	ROJAS RICARDO ALFONSO
27-13881818-2	HULZANQUI PATRICIA CARMEN
27-22127177-2	GONZALEZ MARIELA ALEJANDRA
30-57189536-2	LINEA 22 SOCIEDAD ANONIMA
30-63872707-9	GREEN S A
30-65464085-4	SE NE MI SRL
30-66760328-1	ASOCIACION COOPERADORA HOSPITAL MUNICIPAL

Notes: Panel (a) presents the first two articles of the the incorporation schedule published in resolution N°333/2005. The first article states that all employers listed in the appendix will be gradually incorporated into the SUAF until December 2005. The inclusion into the new system is mandatory. Afterwards, the second article states that the government agency will notify each of the employers to let them know what documentation they need to submit. Panel (b) shows the appendix of resolution N°333/2005. The left column of the resolution lists the taxpayer identifier, while the second column lists the name of the employer/firm.

Figure I.29: Incorporation memo

(a) Resolution (body text)

Resolución N° 456/2006¹

Incorporación de empleadores al Sistema Único de Asignaciones Familiares. A.N.Se.S.

Artículo 1°— Incorporánse formalmente al Sistema Único de Asignaciones Familiares a los empleadores que se encuentran detallados en el Anexo que forma parte integrante de la presente a partir del período mensual devengado correspondiente a agosto de 2006.

Artículo 2°— Los empleadores deberán continuar abonando las asignaciones familiares a sus trabajadores a través del Sistema de Fondo Compensador hasta el período mensual devengado correspondiente a julio de 2006.

Artículo 3°— Los empleadores referenciados en el artículo 1° de la presente, no podrán compensar las asignaciones familiares abonadas a sus trabajadores, a partir del período devengado agosto de 2006.

Artículo 4°— Dése cuenta a la Administración Federal de Ingresos Públicos (A.F.I.P.).

Artículo 5°— De forma.

ANEXO

C.U.I.T.	Razón Social	U.D.A.I.
30-963842333-8	Agrosport S.A.	U.D.A.I. San Juan

(b) Resolution appendix (with employer identifiers)



ANEXO

CUIT	RAZON SOCIAL	UDAJ
30575438772	CONFRAVE S A I C	GERENCIA UCA
30651778170	CONFRAVE INDUMENTARIA S R L E	GERENCIA UCA
30505426661	TEJEDURIAS NAIBERGER SAICI Y F	GERENCIA UCA
30515772746	TRIUMPH INTERNATIONAL	GERENCIA UCA
30515923320	FAMOFEL FABRICA MODELO DE	GERENCIA UCA
30516142452	EPIFANIO VELASCO E HIJOS S A I C I	GERENCIA UCA
30500834087	VIDRIERIA ARGENTINA SOCIEDAD	GERENCIA UCA
30626831660	INDUSTRIAS 9 DE JULIO S A	OFICINA 9 DE JULIO
30666501396	COOP DE COOPERATIVAS DE	OFICINA 9 DE JULIO
30545724819	COOPERATIVA ELECTRICA Y DE	OFICINA 9 DE JULIO
30545744569	COOPERATIVA DE ELECTRICIDAD	OFICINA BALCARCE
30593302462	MHOR INDUSTRIAL S A	OFICINA ESCOBAR
30610738369	ASOCIACION CIVIL NAUTICO	OFICINA ESCOBAR
30608964078	MARTIN BARROCAS Y CIA SRL	OFICINA ESCOBAR
30520473501	MANDOLA MATEO LORENZO Y	OFICINA ESCOBAR
30662051868	ABRANTES SA	OFICINA ESCOBAR
20101633331	SZYKULA MIGUEL ANGEL	OFICINA ESCOBAR
20121737052	KANDRACHOFF NESTOR PABLO	OFICINA ESCOBAR
30578380015	GÓYAIKE S A A C I Y F	OFICINA ESCOBAR
30653466931	GNC ESCOBAR SA	OFICINA ESCOBAR
30580736528	BEST PAINT S A	OFICINA ESCOBAR
30559721502	PRENSADORA MUÑO SOCIEDAD DE	OFICINA ESCOBAR
30522601264	COOP ELECT CONS Y SERVICIOS	OFICINA GENERAL ALVEAR

Notes: Panel (a) presents an example of an incorporation resolution. The first red box on the upper-left side, states that the firm(s) listed below will be formally incorporated into the SUAF. The second red box on the upper-right side refers to the specific month this enrollment will occur i.e., August 2006. The last red box contains the taxpayer identifier (CUIT) to which the resolution refers. Panel (b) contains the list of employers listed in the appendix.

Figure I.30: Website query interface


The image displays two screenshots of the ANSES website interface. The top screenshot is titled "Consulta de Habilitación de Empresas en SUAF". It features a header with the ANSES logo and "autopista de Servicios". The main content area contains a form with a "CUIT:" field, a note "(Ingrese sólo dígitos)", and a captcha image showing the number "955865". Below the form is a "Consultar" button. A blue box below the form displays "Datos de la Empresa" with the following information: Razón Social: TERRA CITRUS SRL; Estado: HABILITADO; Detalle: DESDE EL DEVENGADO 08/2004 POR RESOLUCION. D.E. ANSES Nº 641/03 DEL 29-05-03.

The bottom screenshot is titled "¿Dónde Cobro?". It features a header with the ANSES logo and "Autopista de Servicios". The main content area contains a form with two tabs: "1. Selección de CUIB, a Nº de Beneficio" and "2. Constancia". The form has a "Nº de Beneficio / CUIL" field, a note "Ingrese el Código que se muestra en la imagen", and a captcha image showing the number "017957". Below the form are "Consultar por" buttons for "BENEFICIO" and "CUIB". A footer box contains the service availability information: "Horario de disponibilidad del servicio Lunes a viernes de 00 a 20:30 y de 22:30 a 24. Sábados, domingos y feriados de 8 a 20."

Notes: The top panel shows a screenshot of a public query interface where employers could verify whether their firm (via its CUIT number) had been incorporated into the SUAF payment system. After entering the CUIT and captcha, the site displays the firm's name (*Razón Social*), its eligibility for incorporation into the new system (*Estado*), the corresponding legal memo, and the date (month and year) of the firm's incorporation into SUAF (*Detalle*). The bottom panel shows a screenshot of the public query where employees could check the method of receiving their child benefits, either directly into their bank account or in person at a specified bank branch. The bottom panel shows a screenshot of the public query interface where employees could check the method of receiving their child benefits, either directly into their bank account or in person at a specified bank branch.

Figure I.31: Form PS.2.61

Versión 1.3



Form.
PS.2.61

Notificación del Régimen de Asignaciones Familiares Sistema Único de Asignaciones Familiares

Frente 1

Este Formulario reviste carácter de Declaración Jurada y se debe completar en letra de imprenta, sin tachaduras ni enmiendas
All workers must complete this form

RUBRO I – DATOS DEL TRABAJADOR (a completar por todos los trabajadores con o sin carga de familia)

Apellido y Nombre Completo		Fecha de Nacimiento	Nacionalidad	
CUIL	Tipo y N° Doc /CUIL	Sexo	Estado Civil	
Domicilio - Calle - Número				
Piso	Depto.	Código Postal	Localidad	Provincia
Teléfono		Dirección de Correo Electrónico		

RUBRO II – DATOS DEL EMPLEADOR

Razón Social		CUIT		
Domicilio - Calle - Número				
Piso	Depto.	Código Postal	Localidad	Provincia
Teléfono		Dirección de Correo Electrónico		

Knowledge of family allowance system

Dejo constancia, por medio de la presente, que en el día de la fecha, me he notificado de las normas básicas y principales derechos que me asisten con relación al Régimen de Asignaciones Familiares y que surgen del cuadro existente al dorso de la presente, recibiendo copia, en este acto, de la Ley N° 24.714, sus normas reglamentarias y de la Resolución ANSES N° 292/08 y sus modificatorias.

Request the family allowance personally

Asimismo, me notifico que los trámites para solicitar la liquidación y pago de las Asignaciones Familiares que me correspondan deberá realizarlos personalmente o a través de un "Representante" designado por mí para tal fin, dentro de los plazos que surgen del cuadro existente al dorso de la presente, en cualquiera de las Unidades de Atención de ANSES, presentando -cuando corresponda-, debidamente confeccionados, los Formularios respectivos y la documentación que en cada caso se detalla, además de la que adicionalmente me pudiera ser requerida. Tomo conocimiento, además, que cualquier reclamo deberá formularlo personalmente ante ANSES dentro de los plazos de caducidad establecidos por la normativa vigente, presentando el Formulario PS.2.72 "Reclamos Generales para los Sistemas SUAF y UVHI", debidamente cumplimentado.

Notify his/her employer of any update

Dejo constancia también, que asumo el compromiso de notificar a mi empleador toda novedad/modificación que se produzca con relación a mis cargas y relaciones de familia, acompañando la documentación que las acredite, a efectos de que éste las informe a ANSES a través del Programa de Simplificación Registral.

Me comprometo a informar a ANSES el medio de pago a través del cual deseo percibir las Asignaciones Familiares. Finalmente me notifico que todos los datos que aporte a ANSES personalmente, a través de un "Representante" o de mi Empleador, para la percepción de las Asignaciones Familiares, tendrán carácter de Declaración Jurada, reconociendo el derecho de ANSES a reclamarme su restitución o compensar automáticamente los importes con otras asignaciones en caso de percepción indebida de mi parte, sin necesidad de notificación previa por parte del citado Organismo.

Localidad, de de

Signature employee

Firma/Aclaración de Firma del Trabajador

Signature employer

Firma/Aclaración de Firma y Sello del Empleador

Notes: This is a copy of the form that both employers and employees were require to sign upon transitioning to the new SUAF system. Key sections, highlighted in blue, include: A statement that all workers, regardless of whether they receive child benefits, must complete the form by providing personal details, their address, and employer information. A declaration where workers acknowledge their awareness of the SUAF system and the requirement that all future requests related to child benefits must be handled directly through the SSA (ANSES). Workers also agree to inform their employer of any changes in their family status, which the employer must report to the tax authority via the *Programa de Simplificación Registral* (PSR). Both the employee and the employer must sign at the bottom of the form.

Figure I.32: Form 931

AFIP ADMINISTRACIÓN FEDERAL

Declaración en línea de Seguridad Social

Usuario: GUILLERMO RAMOS
 CUIT: 20-88888888-9
 Contribuyente: GUILLERMO RAMOS

INGRESE...

Cerrar Sesión
 Ir a la pantalla de Login

vienes, 09 de julio de 2010

Inicio » Selección de DJ » Ingreso de Datos » Resultado del Cálculo » F931 » Envío de DJ

Declaración Jurada para Período 10/2010

CUIL: 20-18677732-9
 Nombre y Apellido: CUIL inexistente en AFIP

seleccionar CUIL: 20-18677732-9

Datos generales:

Cónyuge: **Spouse**
 Hijos: 00 **N of children**

Trabajador en CCT: Con cobertura S.C.V.D.:

Perfil de trabajador:

Situación: 01 - Activo
 Condición: 01 - SERVICIOS COMUNES Mayor de 18 años
 Actividad: 049 - Actividades no clasificadas
 Modalidad Contratación: 201 - Puesto nuevo art 16 Ley 26476 beneficio primeros 12 meses
 Código de Siniestrado: 00 - No Incapacitado
 Localidad: 02 - Buenos Aires - Almt. Brown

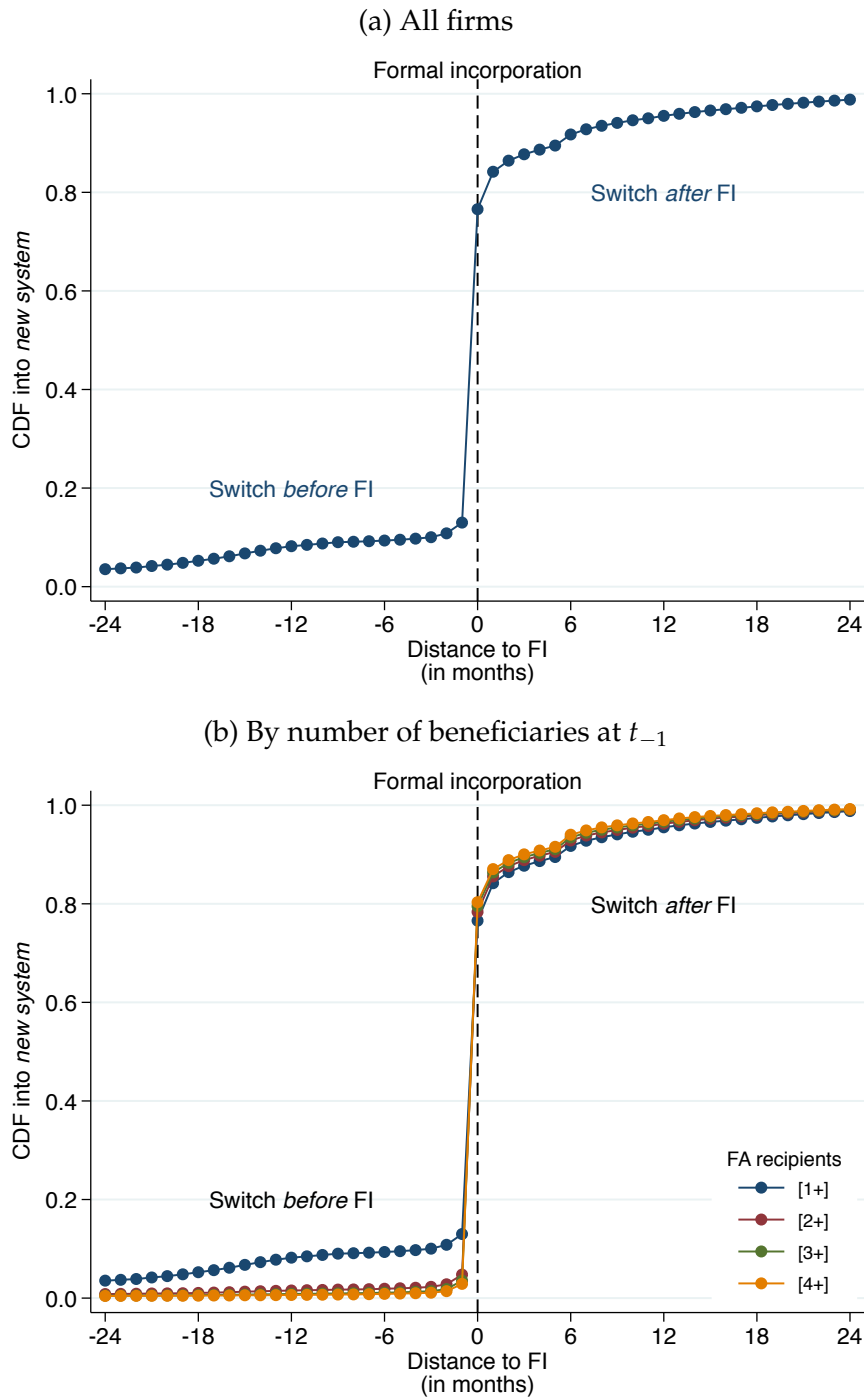
Remuneraciones:

Rem. Total:	120000,00
Rem. Imponible 1:	10119,08
Rem. Imponible 2:	120000,00
Rem. Imponible 3:	120000,00
Rem. Imponible 4:	10119,08
Rem. Imponible 5:	10119,08

Aportes Previsionales
 Contribuciones Previsionales y PAMI
 Contribuciones de Fondo Nacional de Empleo, Asig. Familiares y RENATRE
 Aportes de Obra Social y ANSSAL
 Aportes PAMI

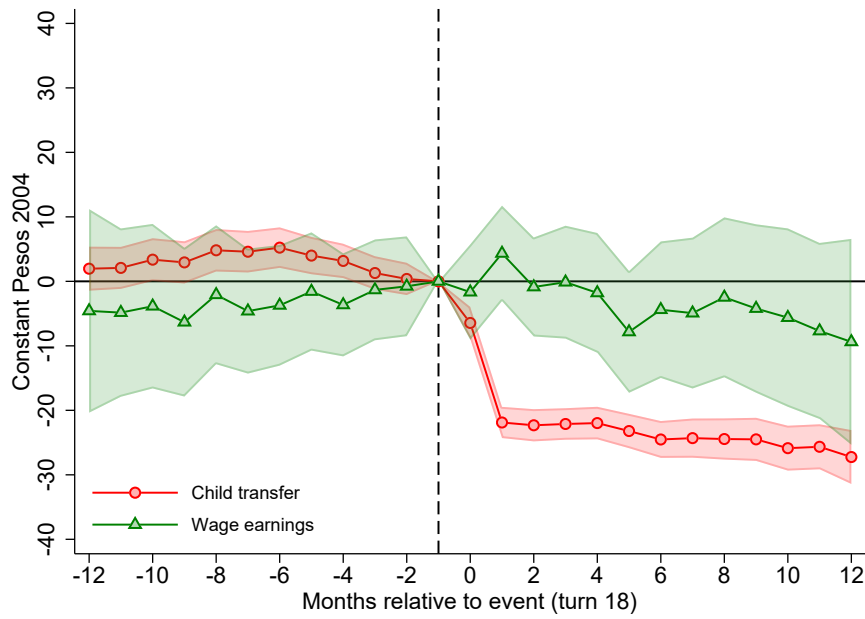
Notes: This image shows a copy of Form 931 that employers file monthly to the tax authority (AFIP) to report wage earnings and social security contributions. The red rectangle highlights a section where employers must report details about the employee's spouse and number of children. The underlying data of this form is the one used to construct the employer-employee database used in the paper.

Figure I.33: Event accuracy and formal incorporation date



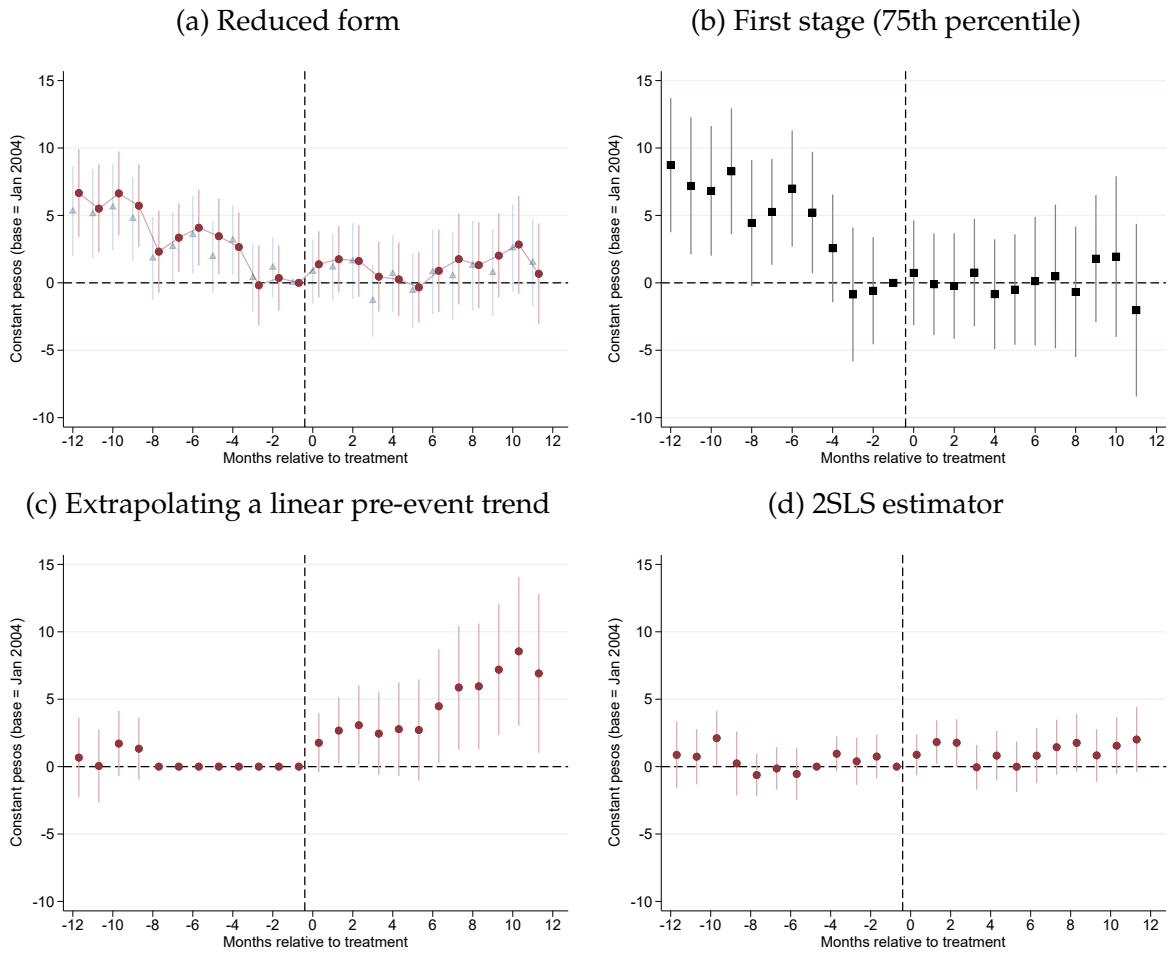
Notes: The vertical axis of these figures contains the cumulative density function (CDF) of firms incorporated into the new system as a function to the distance (in months) to the formal incorporation date (FI). Panel (a) includes all firms while in panel (b) we break down the CDF by the number of transfer recipients within each firm in the last month before the switch (t_{-1}). We consider firms with 1 or more FA recipients, 2 or more, 3 or more and 4 or more.

Figure I.34: Turning 18, becoming ineligible (individual-level shock)



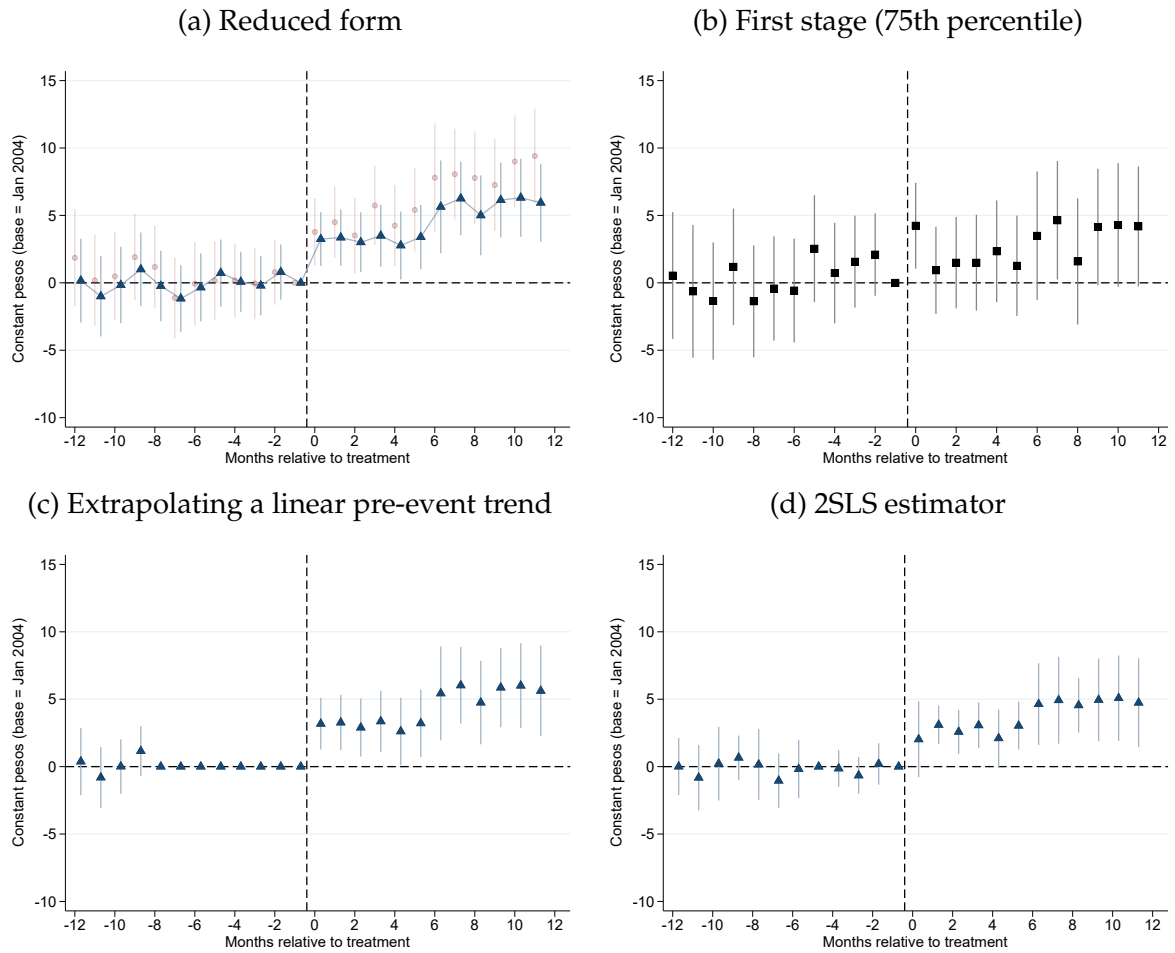
Notes: This figure plots the event-study estimates of parameter γ and its corresponding 95-percent confidence intervals of equation (2). The event, in this exercise, refers to having a child that turns 18 years old in a given month. In one series, we plot the gap in transfer (first stage), while in the other we plot the evolution of the wage gap (reduced form) around the event.

Figure I.35: Wage effects for incumbent workers



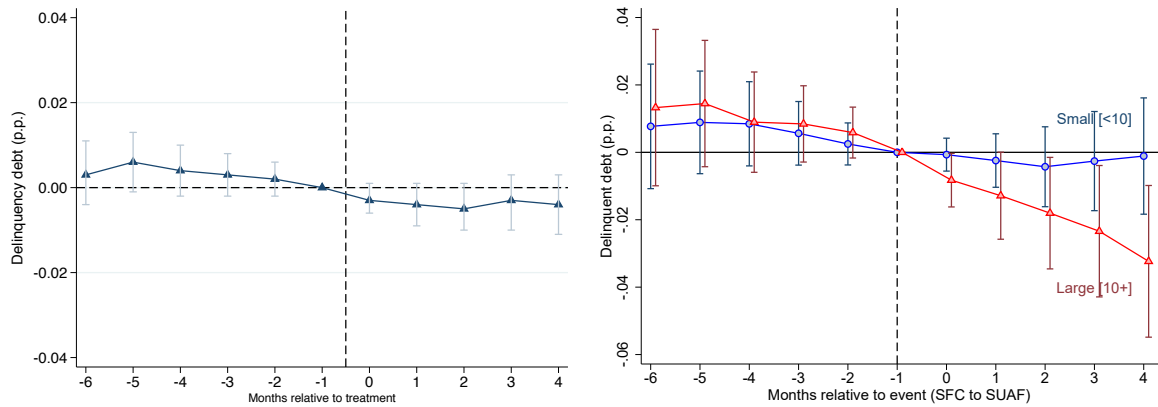
Notes: This figure plots the event-study estimates of parameter γ and its corresponding 95-percent confidence intervals under various specifications. The sample corresponds to incumbent workers. Panel (a) shows the reduced-form effect on the average wage gap (red circles), and the wage gap at the 25th percentile (light blue triangles). Panel (b) shows the first-stage effect on the wage gap of workers at the 75th percentile (black squares). This is the key covariate x_{ft} that Freyaldenhoven *et al.* (2019) use to correct for the role of a confound η_{ft} in panel (a). Panel (d) corresponds to a 2SLS regression of the average wage gap on the policy (event) indicators $d_{f,t}^j$ and x_{ft} , using the closest lead of the event $d_{f,t}^{-1}$ as an excluded instrument for x_{ft} . Using $d_{f,t}^{-1}$ as an instrument means that we need to normalize γ_j for an additional j . We have set $\gamma_{-5} = 0$. Panel (c) attempts to account for the confound by extrapolating a linear trend from the eight periods immediately preceding the event.

Figure I.36: Wage effects pooling incumbents and new hires together



Notes: This figure plots the event-study estimates of parameter γ and its corresponding 95-percent confidence intervals under various specifications. Panel (a) shows the reduced-form effect on the average wage gap (red circles), and the wage gap at the 25th percentile (light blue triangles). Panel (b) shows the first-stage effect on the wage gap of workers at the 75th percentile (black squares). This is the key covariate x_{ft} that Freyaldenhoven *et al.* (2019) use to correct for the role of a confound η_{ft} in panel (a). Panel (d) corresponds to a 2SLS regression of the average wage gap on the policy (event) indicators $d_{f,t}^j$ and x_{ft} , using the closest lead of the event $d_{f,t}^{-1}$ as an excluded instrument for x_{ft} . Using $d_{f,t}^{-1}$ as an instrument means that we need to normalize γ_j for an additional j . We have set $\gamma_{-5} = 0$. Panel (c) attempts to account for the confound by extrapolating a linear trend from the eight periods immediately preceding the event.

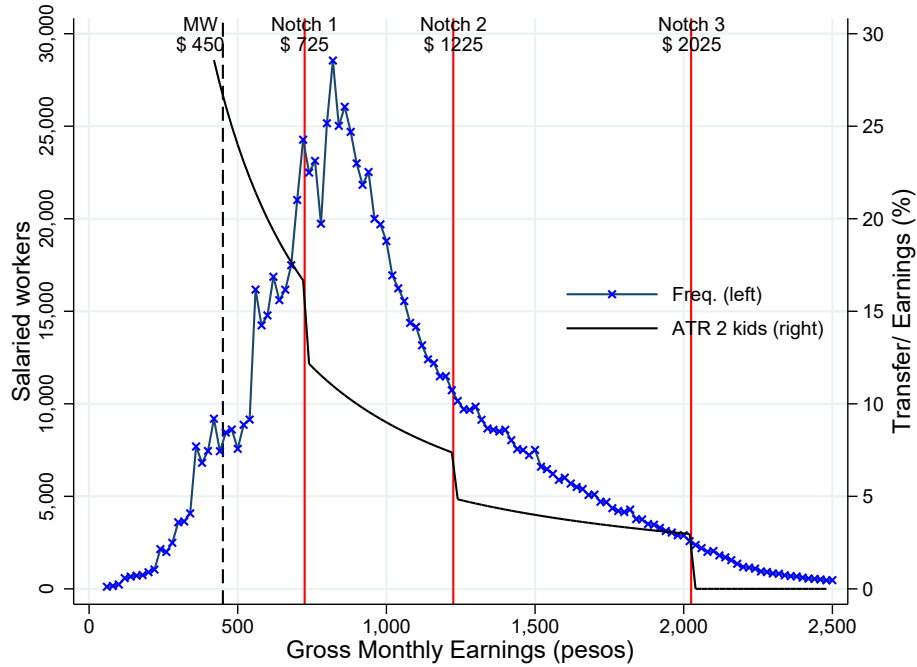
Figure I.37: Delinquency rates on financial debt



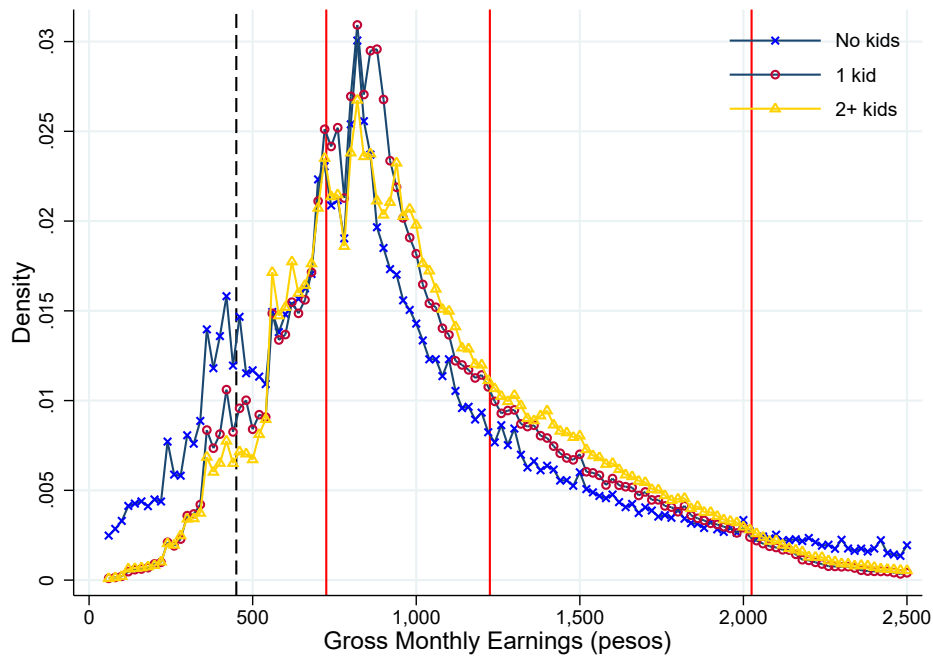
Notes: This figure plots the event-study estimates of parameter γ and its corresponding 95-percent confidence intervals of equation (2) considering a one-year window. The dependent variable is an indicator for whether a firm has overdue debt for more than 90 days. We include untreated firms, i.e., those that switched during 2005, in the regression and re-center the time variable as being t_{-1} for this set of firms. The left panel uses the full sample of firms. The right panel splits this result into small and large firms, with less or more than ten employees, respectively. We use firm-level financial debt data from CENDEU.

Figure I.38: Bunching as a collusion response

(a) Gross wage and average tax rate



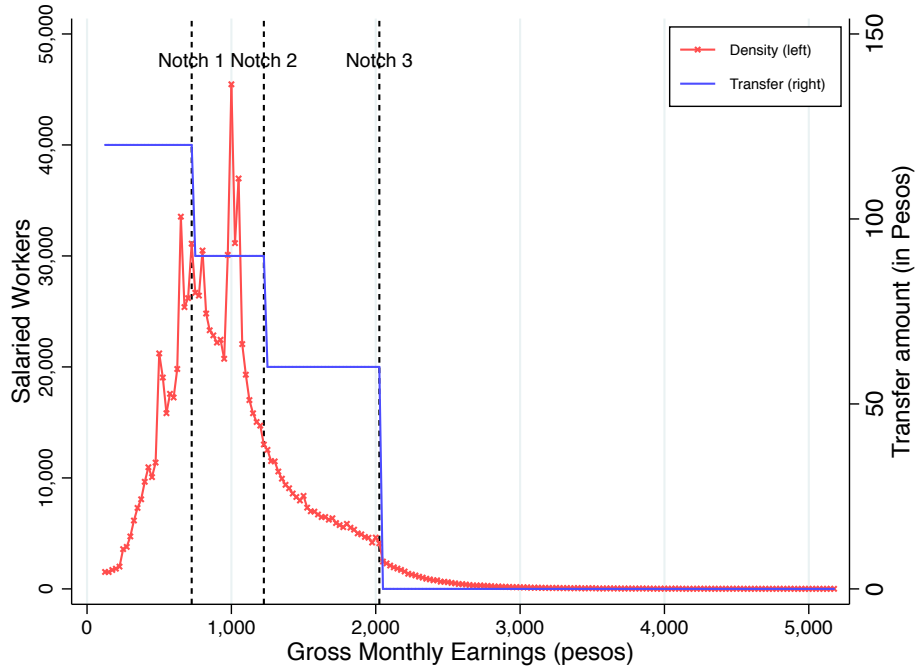
(b) Distribution by number of kids



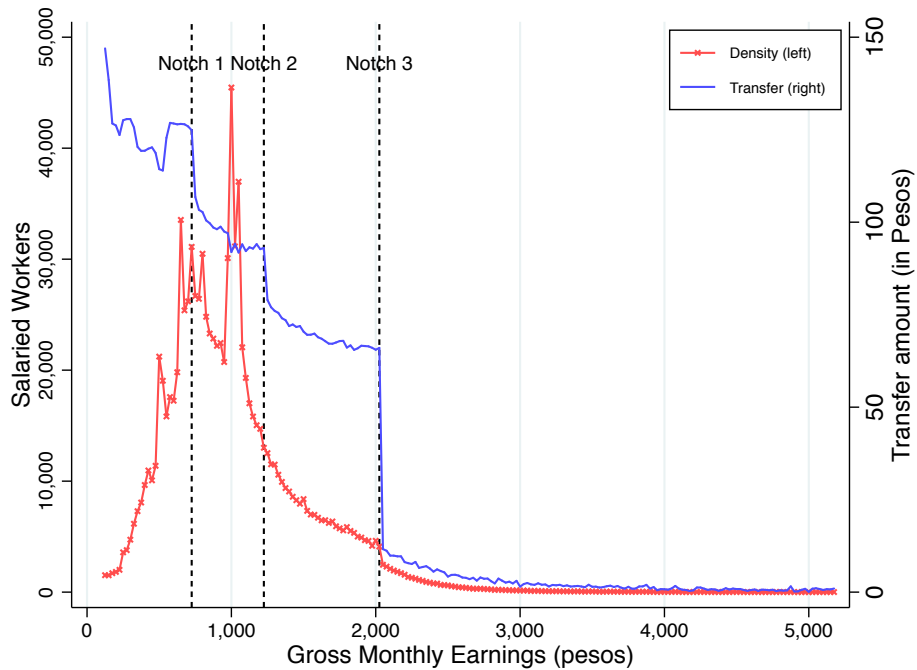
Notes: These figures show the bunching response of wage employees to the presence of *notches* embedded in the transfer scheme. Panel (a) presents the distribution of wage-employees grouped in bins of 20 Argentinean pesos together with the theoretical average tax rate for a worker with 2 children. In panel (b), we repeat the analysis separately for groups varying by number of children.

Figure I.39: Empirical discontinuities in transfer amount at *notches*

(a) Median transfer



(b) Mean transfer



Notes: These figures show the bunching response of wage employees to the presence of *notches* embedded in the transfer scheme. Panel (a) presents the distribution of transfer recipients grouped in bins of 20 Argentinean pesos together with the empirical median ATR. In panel (b) we repeat the analysis using mean ATR for each bin.

Table A1: Monthly transfer by income bracket (1996-2010)

Year	Effective date M/D/Y		Law	Monthly Gross E.		Child Transfer
	Start	End		\geq	\leq	
1996	10/16/96	03/01/04	Law 24714/1996	-	500	40
			Dto. 1245/1996	500	1,000	30
			Res. 112/1996	1,000	1,500	20
2004	03/01/04	10/01/04	Dto. 0368/2004	100	725	40
				725	1,225	30
				1,225	1,725	20
2004	10/01/04	09/01/05	Dto. 1691/2004	100	725	60
				725	1,225	45
				1,225	2,025	30
2005	09/01/05	12/01/06	Dto. 1134/2005	100	1,200	60
				1,200	1,800	45
				1,800	2,600	30
2007	12/01/06	10/01/07	Dto. 0033/2007	100	1,700	72
				1,700	2,200	54
				2,200	3,000	36
2007	10/01/07	09/01/08	Dto. 1345/2007	100	2,000	100
				2,000	3,000	75
				3,000	4,000	50
2008	09/01/08	10/01/09	Dto. 1591/2008	100	2,400	135
				2,400	3,600	102
				3,600	4,800	68
2009	10/01/09	09/01/10	Dto. 1729/2009	100	2,400	180
				2,400	3,600	136
				3,600	4,800	91
2010	09/01/10	10/01/11	Dto. 1388/2010	100	2,400	220
				2,400	3,600	166
				3,600	4,800	111

Notes: Author's elaboration based on official documents. The last three columns are expressed in current Argentinian pesos. There is also a supplemental transfer for workers living in less favorable areas; there are 4 zones under this classification. These areas are not very dense in terms of population, so not many workers receive this supplement.

Table A2: Summary statistics for registered wage earners in Argentina, 2004

	1st Bracket	2nd Bracket	3rd Bracket	Universe
	(1)	(2)	(3)	(4)
Wage earners	2,154,722	1,426,404	550,571	4,787,496
Beneficiaries AAFP	480,185	488,414	188,979	1,226,459
Number of children	2.0	2.0	2.0	2.0
Female (%)	21.4	19.5	13.6	33.8
Average earnings	555	941	1,486	1,148
Transfer/Earnings (%)	13.1	6.8	3.6	7.7

Notes: This table displays summary statistics for private formal wage earners in April 2004.

Table A3: Key dimensions under the two payment systems

	SFC (1)	SUAF (2)
Legal liability	<i>Employee</i>	<i>Employee</i>
Remittance responsibility	<i>Employer</i>	<i>Government</i>
Information reporting	<i>Form 931</i>	<i>Form 931</i>
Tax-benefit linkage	<i>Low</i>	<i>Higher</i>
Source of funding	<i>Contributory Employer SSC</i>	<i>Contributory Employer SSC</i>
Transfer' claiming procedure	<i>Employer</i>	<i>Employee</i>

Notes: Column (1) refers to the *Sistema de Fondo Compensador* (SFC) while column (2) to the *Sistema Único de Asignaciones Familiares* (SUAF), the old and the new payment systems, respectively.

Table A4: Baseline characteristics of treated and control workers

	Treatment w/children (1)	Control wo/children (2)	Difference (3)
Monthly wage t_{-1}	879.6 (2.89)	861.9 (2.91)	17.7*** (4.10)
% female	0.21 (0.002)	0.26 (0.002)	-0.05*** (0.003)
% full-time	0.65 (0.002)	0.61 (0.002)	0.04*** (0.003)
% unionized	0.47 (0.003)	0.46 (0.003)	0.00*** (0.004)
Proxy for age	22,182,123 (43,339)	21,725,828 (59,905)	456,295*** (73,938)
Proxy for tenure	11.1 (0.01)	10.8 (0.01)	0.02*** (0.02)

Notes: This table shows the baseline characteristics for treated and control workers, as well as the corresponding difference. We analyze the following characteristics (all measured in the month before their firm switches to the new system): [a] baseline wage, [b] share of female workers, [c] share of full-time workers, [d] share of unionized workers, [e] proxy for age (we use the first two digits of the individual identifier, the greater the number the younger a certain worker is) and [f] a proxy for tenure (for those that were in the firm at t_{-1} , we calculate how many months the worker has been in the firm during the last year before the switch). Standard errors are reported in parentheses.

Table A5: Survey evidence about the understanding of family allowances

Who is the responsible of paying family allowances?	
Responses:	
A. Government	35.4%
B. Employer	8.6%
C. Other	4.0%
D. Don't know	52.0%

Notes: This table shows the results from a survey carried out by the social security administration (ANSES) in 2018 where they asked people whether they knew who was responsible of paying family allowances in Argentina. Option C includes: N/A; the call got interrupted, or the bank. Source: based on [Cruces \(2019\)](#).

Table A6: Robustness exercises - alternative specifications

	(1)	(2)	(3)
Reduced Form			
Δ monthly wage (in pesos)	4.44*** (0.85)	4.69*** (1.21)	4.33*** (1.23)
2SLS			
$\frac{\Delta wage}{\Delta transfer(\tau^e)}$	-0.05*** (0.01)	-0.05*** (0.01)	-0.05*** (0.01)
Simple mean difference	✓		
Firm and time FE		✓	✓
Firm linear trend			✓
Observations	2,285,705	2,285,705	2,285,705

Notes: This table presents the reduced-form and 2SLS point estimates of equation (3) in column (2). In column (1) we run equation (3) without firm and time fixed effects, while column (3) refers to equation (3) plus firm linear trends. Standard errors clustered at the firm level are reported in parentheses.

Table A7: Composition of workers after the switch to the new system

	(1)
Reduced form	
% unionized	0.0009 (0.0006)
% female	-0.0002 (0.0007)
% full-time	0.0036** (0.0011)
Proxy for age	31,184 (29,528)
Number of firms	26,226
Observations	673,295

Notes: This table reports the reduced-form estimates using alternative outcomes variables. From top to bottom, we consider the following left-hand side variables (all of them expressed in differences): [a] share of unionized workers, [b] share of female workers, [c] share of full-time workers [d] proxy for age (we use the number embedded in the anonymized individual identifier, the greater the number the younger a certain worker is). Standard errors clustered at the firm level are reported in parentheses. *** significant at 1%, ** significant at 5%; * significant at 10%.

Table A8: Employer-mediated child benefits around the globe

	Country	Program's name
Latin American countries	Argentina	<i>Asignaciones Familiares</i> (1)
	Brazil	<i>Salário Família</i>
	Chile	<i>Asignación Familiar</i>
	Paraguay	<i>Asignación Familiar</i>
	Perú	<i>Asignación Familiar</i>
Developed countries	Greece	<i>Βοήθημα Τοκετού</i>
	Italy	<i>Bonus Renzi 80 Euro</i>
	Switzerland	<i>Familienzulagen</i>
	United Kingdom	<i>Working Family Tax Credit</i> (2)
	United States	<i>Advance Earned Income Tax Credit</i> (3)

Notes: Author's elaboration. This table contains a non-exhaustive list of countries that have, or had at some point, employer-mediated transfers. (1) In place during 1995-2010; (2) In place during 1999-2003; (3) In place during 1979-2010.