# Wage Effects of Means-Tested Transfers: Incidence Implications of Using Firms as Intermediaries[*]

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## Abstract

We show that the way 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 capture 6 to 14 percent of the transfers through lower wages when they mediate the payments. We argue that transfers were likely understood as part of the compensation package and employers exploited this confusion to extract rents. Our findings suggest that relying on firms as mediators in the tax-benefit system could have unintended consequences.

JEL CODES: H23, H31, H71, I38, J31, J32, J33

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

# 1 Introduction

Most countries provide some type of financial aid to families with children. In developed countries, this type of social assistance was established after World War II and, in developing economies, towards the end of the 20th century. 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 embedded in these programs, such as the timing of payments, the role of conditionalities, or the way transfers are disbursed. Among these, the empirical question of who bears the economic incidence of work subsidies and family allowances remains poorly understood (Nichols & Rothstein, 2015). The general assumption is that individuals get the entire benefit. In practice, however, benefits could be incident on employers, meaning that workers receiving benefits are paid a lower wage.[1] In this paper, we bring new evidence to the discussion and study whether the way family allowances (tax credits) are disbursed affects the gross wage of workers.[2] We exploit an unusual reform in Argentina that shifted the *disbursement responsibility* of family allowances from employers to the social security administration (SSA) in a staggered fashion, while keeping other features of the program unchanged.

In Argentina, registered wage earners with children less than 18 years old are entitled to a family allowance that they receive every month (*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] This transfer was historically disbursed by employers who could net these payments out from employer social security contributions (SSC) before remitting SSC to the tax authority. In 2003, for transparency reasons, the government decided to discontinue the intermediary role played by firms and to start depositing the transfer directly into workers' bank accounts. Because of the administrative burden associated with such a change, the government gradually transitioned from the old to the new system over the course of eight years, from 2003 to 2010. Importantly, for identification, the switching dates were determined by the SSA rather than chosen by firms.[4]

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[1]A similar idea applies to Food Stamps in which grocery stores could increase prices to capture part of these transfers (Goldin *et al.* , 2022, Hastings & Washington, 2010, Jaravel, 2018). Similarly, in the case of health insurance, there is evidence of incomplete pass-through of government subsidies into lower premiums (Cabral *et al.* , 2018).

[2]We use the term 'wage' throughout the paper to refer to monthly wage earnings, not the wage rate.

[3]In that sense, this transfer is similar in spirit to the EITC in the U.S. but presents *notches* instead of kinks and is paid monthly instead of yearly.

[4]In the body of the paper, we explain that the transition was made through memos and decrees published during the eight years, which included the list of firms that had to switch at different dates. Therefore, from the firm's point of view, this was a plausibly exogenous event.

The gradual roll-out of the new payment system and the change in the saliency of the transfer provide ideal variation and a rare opportunity to cast light on the labor market consequences that derive from the way tax credits are paid. Under the old payment system, named *Sistema de Fondo Compensador* (SFC), the transfer was very salient to employers and, therefore, provided incentives to integrate the benefit into the compensation package of eligible workers with children, potentially shifting part of the incidence of the transfer in the form of lower wages. Moreover, workers might have thought that the firm funded the transfer because the credit appeared as an extra line on pay slips. In contrast, under the new system, named *Sistema Único de Asignaciones Familiares* (SUAF), the employer could no longer tag beneficiaries or see the amount of the transfer—especially for new hires. Naturally, given the staggered roll-out, we identify wage effects using an event-study approach that aligns firms at the switching date and compares, within firms, the monthly wages (pre tax and transfer) of workers with and without children before and after that date. The null hypothesis of interest is whether the way transfers are distributed affects wages or not. If it is neutral, we should observe no effects on monthly wages after the transition. On the contrary, if employers were fully shifting the incidence of the transfer under the old scheme, then monthly wages should increase 1-to-1 by the transfer amount after the event.

We use rich population-wide administrative data for the universe of private and public wage earners registered in the social security of Argentina for the period 2003-2010. Employers report these data to the tax authority monthly, and thus provide high-frequency variation with firms switching to the new system for 96 consecutive months. The data contain monthly information on total wage earnings (before taxes and transfers), social security contributions, zip codes, and some demographic and firm characteristics. Importantly, we also observe the exact amount of the monthly transfer each worker received before the firm switched to the new payment system. This is because when a firm was part of the SFC, it had to report the number of workers receiving the subsidy and the amount paid to each to deduct the transfer from payroll taxes. We combine this employer-employee panel with another dataset of family relationships that allows us to link workers with their spouses and children. In this database, we also observe each child's exact date of birth, allowing us to accurately flag workers with eligible and non-eligible children.

Our results can be summarized as follows. In the first part of the paper, we provide compelling graphical evidence that the way tax credits are paid out affects gross wages. Before firms transition to the new government-based system, workers' wages with and without children evolve similarly. However, when firms stop delivering the transfer, the average monthly wage of workers with children increases by approximately 5 pesos relative to workers without children. This wage effect occurs instantly

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and increases over time, reaching an average of 13 pesos two years after the event. This result is robust to a set of validation exercises. In addition, this effect declines as we move up in the income distribution where the amount and salience of the transfer are smaller: the percentile 25 wage differential of workers with and without children presents a sizable jump after the event, while percentile 75 is relatively stable. In terms of magnitude or the pass-through rate, our estimates imply that employers were capturing about 6-14 percent of the transfer by paying lower wages when they mediated the disbursement in the old firm-based system.[5]

Our preferred explanation of this wage effect is that, in the old system, the transfer was likely understood as part of the compensation package, and employers exploited this confusion by capturing part of the transfer in the form of lower wages. In the second part of the paper, we explore heterogeneous effects and potential mechanisms—along with anecdotal evidence—that are consistent with this story.[6] The critical piece of evidence is that the effect is more prominent for newly hired workers and appears more muted for incumbent workers. Intuitively, when firms are no longer in charge of delivering the transfer, they can no longer integrate it into the wage package, and thus, the market wage of new hires with children goes up. For incumbent workers, wage rigidities prevent such adjustments. In addition, the wage effect is stronger in small firms, which aligns with the idea that rent-seeking behavior is prevalent in places where employers are closer to their employees. In addition, it is also likely that small firms are less monitored by labor unions. In fact, we show that the effect is stronger in firms with a low share of unionized workers and is more muted in firms where more than 50 percent of their employees are unionized. This result strikes us as remarkable and suggests that unions could prevent employers from lowering wages to capture part of tax credits (as conceptually discussed by Lee & Saez, 2012).

Finally, we argue that the effect is unlikely to be driven by a pay-equity-concern channel, where workers with children start to bargain more aggressively after the reform. Under this alternative explanation, one would expect the effect to build up slowly over time and affect incumbent workers. Yet, we find that the effect appears immediately in the first month after the event and features more prominently for new hires. In addition, one would expect pay equity concerns to operate more strongly in firms with a mix of eligible and non-eligible workers. We show, however, that wage effects are stronger the higher the firm's exposure to family allowances.

Our findings therefore suggest that the way governments set up tax credit pro-

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[5]The pass-through rate scales the reduced-form effect by the average amount of child benefits right before the event, which was about 90 pesos per month (10 percent of average wages).

[6]We also rationalize wage effects in a setting with a misperception of benefits by adding a perception parameter to the standard Gruber (1997) incidence model. Under complete confusion, we show that employers capture part of the transfers through lower wages (see Appendix E).

grams, like the EITC in the U.S., matters and influences the final economic incidence, contrary to what the standard incidence model predicts. We find that wages adjust to how transfers are disbursed, rejecting the null hypothesis that transfers are *entirely* captured dollar for dollar by workers. This important result sheds light on an understudied topic that is common in other countries. For instance, employers currently disburse child benefits in Brazil, Chile, Paraguay, Peru, Greece, Italy, and Switzerland.[7] Our results highlight a potential economic cost of using employers as intermediaries to disburse fiscal benefits, a point ignored by previous literature. More generally, this paper suggests that relying on firms as mediators in the tax-benefit system could have unintended consequences.

This paper contributes to the literature on incidence, in general, and the incidence of tax credits, in particular. The basic idea behind an incidence analysis is to determine how the burden of a particular tax or subsidy is allocated among different agents. The standard partial-equilibrium model predicts that the burden of a tax depends on the relative elasticity of supply and demand, where the more elastic side can shift the burden to the more inelastic one. This framework is largely based on classic references such as Atkinson & Stiglitz (2015), Fullerton & Metcalf (2002), Kotlikoff & Summers (1987), Summers (1989), and Musgrave (1959).

A number of recent studies have extended standard tax incidence models by incorporating salience effects, remittance and compliance costs, market rigidities and imperfect competition, and asymmetric effects, among others.[8] For instance, recent evidence in other settings shows that who remits the tax to the tax authority affects the final incidence (Kopczuk *et al.* , 2016, Slemrod, 2008). Another strand of the literature that focuses on payroll taxes casts doubt on the standard prediction in public finance that statutory incidence is irrelevant in determining final incidence. For example, Saez *et al.* (2012) show that, in Greece, the economic incidence matches the statutory incidence (i.e., full incidence of employer SSCs on employers and full incidence of employee SSCs on workers). Similarly, Saez *et al.* (2019) exploit a reduction of employer SSCs in Sweden and find that posted wages of treated workers did not change, implying a full incidence on employers.[9] Unlike the existing tax studies, our project focuses on transfers for which minimal evidence exists. The most intriguing aspect is that we look at a change in the payment system (the *disbursement responsibility*), keeping other features of the benefit schedule constant.

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[7]In Appendix C we document other experiences of firm-mediated transfers around the world.

[8]For instance, Chetty *et al.* (2009) show that consumers underreact to sales taxes that are not included in posted prices. Benzarti *et al.* (2020) show asymmetric price responses to VAT increases and decreases, suggesting that the direction of a tax change matters for incidence. Recent advances also analyze tax incidence considering tax salience and imperfect competition (Kroft *et al.* , 2020).

[9]In a parallel policy experiment, Cahuc *et al.* (2019) exploited a reduction in employer SSC for new hires in France, and found positive employment effects but no effects on wages.

To the best of our knowledge, there are three papers closely related to ours that make valuable efforts to estimate the incidence of in-work subsidies. Rothstein (2010) and Leigh (2010) focus on the EITC in the U.S. and Azmat (2019) analyzes the Working Family Tax Credit (WFTC) in the U.K. Leigh (2010) exploits the introduction of state-level EITCs in a difference-in-differences framework. He finds that a 10 percent increase in the EITC leads to a 5 percent reduction in pre-tax wages for high-school dropouts (2 percent for high-school graduates). Nichols & Rothstein (2015) argue that although his approach is reasonable, the results are somewhat unrealistic and cannot reject the null hypothesis that wages remained constant. Azmat (2019) exploits an economy-wide and simultaneous increase in the generosity of the WFTC and the shift to an employer-based payment system. Wage effects are identified with survey data by comparing the effective wage of a worker relative to a counterfactual wage predicted using pre-reform worker characteristics. She finds that firms cut the wage of claimant male workers relative to similarly skilled nonclaimants by 30 percent of the tax credit. In the absence of clean identification strategies for transfer effects on wages, Rothstein (2010) relies on external estimates of the labor demand elasticity to calibrate the distributional effects of the EITC. His simulations suggest that employers capture about $0.36 of each dollar spent on EITC through reduced wages. Overall, the consensus from scholars is that identifying wage effects is a tall order and that the evidence remains inconclusive (Nichols & Rothstein, 2015). We build on these papers and help advance the understanding of this topic by combining rich administrative data and a novel staggered design that allow us to identify wage effects and shed light on some of the underlying mechanisms. Most notably, we provide compelling graphical evidence, arguably representing the first non-parametrically identified evidence of wage effects in the context of tax credits.

We also contribute to the tax saliency literature following Chetty *et al.* (2009) pioneering work. More recent studies analyzing behavioral responses to tax visibility or transparency include both quasi-experimental evidence (e.g., Bradley & Feldman, 2020, who exploit the display of tax-inclusive prices in the airline industry) and lab experiments (e.g., Feldman & Ruffle, 2015). Generally, most papers exploit increases in salience, understood as greater visibility of the tax. An exception is a paper by Finkelstein (2009) that analyzed the introduction of electronic tolls which decreased its salience. More closely related to our work, Azmat (2019) shows that the wage effects of the WFTC are mostly explained by the increased visibility to employers rather than the increase in the transfer amount. In addition, our explanation of wage effects based on an imperfect understanding of the benefits system also connects with the study by Feldman *et al.* (2016), who show that tax complexity can cause confusion and lead to unintended behavioral responses.

Finally, our paper adds to a growing literature on the design of welfare programs and social protection policies. Some examples in this domain are Jones (2010) on the Advanced Earned Income Tax Credit (AEITC) in the U.S., and Doornik *et al.* (2018) on Unemployment Insurance (UI) in Brazil. The latter shows an extreme form of collusion with layoff and rehiring patterns between firms and workers seeking to extract rents from the UI system. Our results help to inform policy debates on the consequences of decentralizing sensitive tasks prone to confusion, such as the disbursement of tax credits.

The paper is organized as follows. In Section 2 we describe the institutional setting and the change in the remittance system. In Section 3 we revise the standard incidence terminology and conceptual framework. Section 4 introduces the data sources. The empirical strategy and main results are presented in Section 5. In Section 6 we explore some of the mechanisms and drivers of the wage effects. Finally, Section 7 ends with some concluding remarks.

# 2   Institutional setting

## 2.1   Family Allowances in Argentina (AAFF)

The Argentine family allowance program, *Asignaciones Familiares* (AAFF), is the largest means-tested transfer in Argentina. It was introduced in 1996 and is funded by social security contributions (SSC).[10] It consists of a monthly child benefit paid to private sector employees with children under 18 years old and monthly wages below a certain threshold. The benefit amount varies by the number of children and decreases across three discontinuous monthly wage brackets.[11]

The parameters that characterize the AAFF transfer scheme for the early years of our data are presented in Figure 1a.[12] There are three brackets and the transfer per child decreases as we move to the right (AR$40, AR$30, and AR$20 per child). The figure shows the average tax rate for a worker with one child (blue line), two children (red line), and three children (green line). For instance, in the case of a worker with two children at the upper end of the first bracket, the transfer represents 16 percent of the monthly wage and, if she earns a bit more, it decreases to 12 percent (she receives

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[10]See Law 24,714/1996. Prior to this program, there were some limited schemes (either firms designing their own system, or local government initiatives). For more details, see Appendix A.

[11]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.

[12]Table A1 provides a complete picture of this scheme by year, including the evolution of the brackets and the exact benefit amount per child.

AR\$30 instead of AR\$40 per child).[13] This discontinuous way of phasing the transfer out as wages increase is known in the tax literature as a *notched* scheme.[14]

The AAFF 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.

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 capture 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.[15] Figure 1b describes the evolution of the upper bracket thresholds from 2003 to 2011, jointly with the evolution of the 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 the paper comes 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 2 and describe them below. Importantly, the only thing that changed was how workers received the transfer, i.e., the benefit schedule and other features remained unchanged.

**The old system (SFC):**   In the original scheme of the AAFF program, child benefits were disbursed indirectly by employers to eligible employees with children (upper panel of Figure 2). Under this payment system, called *Sistema de Fondo Compensador* (SFC), employers could debit the transfer from the employer's portion of SSCs before

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[13]In Figure I.29, we present the distribution of monthly wages and show no bunching at these discontinuities. This constitutes suggestive evidence of no labor supply responses and no strategic collusion between employers and employees.

[14]This differs from the EITC schedule, which includes *kinks* instead of *notches* (i.e., discontinuities in the marginal tax rate rather than in the average tax rate).

[15]Appendix 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.

remitting the money to the tax authority. The firm could claim a refund if the amount paid was greater than the tax bill. In this setting the employer was merely an intermediary in charge of disbursing funds from the public sector. This implied that firms could tag eligible and non-eligible workers and also know precisely how much subsidy each worker received from the government. A key feature of this system is that it was mandatory to include the transfer as an extra item on workers' pay slips (see Figure 3 panel (a)). This is relevant for the incidence analysis because it enhances the saliency of the transfer, and it could also induce workers to believe that the transfer was part of their employment compensation and, thus, funded by the firm. In Section 6, we provide some anecdotal evidence consistent with this interpretation.

**The new system (SUAF):**   The new payment system, named *Sistema Unico de Asignaciones Familiares* (SUAF), was launched in June 2003 with the goal of replacing the existing SFC payment system (Memo 641/2003 ANSES). Under this system, the Argentine SSA (ANSES, for its acronym in Spanish) removed the intermediary role of firms and began to deposit the subsidy directly into workers' bank accounts, creating a new centralized payment system. In that way, the firm's only responsibility was to remit monthly SSC to the tax authority. This implied that employers could no longer tag beneficiaries or see the benefit amount—especially for newly hired workers. In addition, the subsidy was no longer reported on workers' pay slips, reducing the salience of the transfer to employers (see Figure 3 panel (b)). The official reasons for the reform were: to make the system more transparent, to make sure beneficiaries effectively receive the transfer, to control fraud, and to provide administrative relief to firms. Importantly for our empirical strategy, the SSA had to gradually incorporate firms into the new system because it lacked sufficient operational capacity to do it in one step. Employers were switched from the old to the new system every month from June 2003 to June 2010, when the transition process finished. Importantly, workers continued receiving the transfer, but it was paid through a different channel, and all of a sudden, it became less salient to employers.[16]

**Incorporation process and empirical roll-out:**   The SSA determined the incorporation of firms into the new system through a series of memos posted online. Hence, because employers could not choose when to switch, the switching date works as a plausibly exogenous event from the firm's point of view. In a nutshell, the whole process can be described as follows (see Figure 4). The incorporation started with the SSA setting an *internal* incorporation schedule where the SSA issued a list of targeted

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[16]In Figures I.1 and I.2 we show that AAFF spending and the number of beneficiaries did not decrease during the transition. This in fact implies that in the aggregate workers kept receiving the transfers.

firms that would be gradually incorporated up to a certain point in time. Firms were then contacted by an officer and informed to present specific documentation (payroll, beneficiaries, bank accounts). After checking that everything was correct, the last step consisted of formal approval and incorporation of the firm into the new system. Employers were required to notify their employees via an individual form to be signed by both parties within ten days of the transition.[17]

In the data, we observe when the firm stops disbursing the transfer under the old system. After this date, the payment variable is reported with missing values. Hence, we define the *event* as the month-year when the firm stops paying family allowances so that the last payment observed in the micro-data occurs at month $t - 1$. Figure 5 illustrates the empirical roll out that emerges from our micro-data. We plot the share of firms paying the transfer under the old system (SFC) and the share of workers receiving the transfer through that system. As seen in panel (a), the transition was gradual.[18] In panel (b), we further show that large firms were the first to switch to the new system. Although the SSA determined the switching date, it seems that they initially targeted large firms. Nevertheless, our empirical strategy exploits within-firm variation to address any potential selection issues.

To sum up, the staggered change in the remittance obligation and the salience of the transfer provide ideal variation and a unique opportunity to elucidate the labor market consequences that derive from the way tax credits are disbursed. In particular, the reform allows us to study whether employers captured part of the transfers by lowering wages.

# 3   Conceptual framework

In this section, we briefly describe a way to rationalize the mechanisms that could explain who bears the final 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.

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[17]More details on the roll-out and the incorporation process can be found in Appendix D.

[18]In Figure I.3 we use aggregate official budget information to show the gradual decrease in the share of family allowances paid through the SFC (old system) as a proportion of total spending. We also calculated the total sum disbursed through the SFC using the micro-data and compared it to the macro totals. Both values are very similar, confirming that the quality of family allowance payments in the micro-data is high (see Figure I.1).

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 1 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 3.[19] The figure presents a real case from an anonymous worker right before and right after the firm where he works switched from the SFC to the SUAF system. In the old system (panel a), the 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.[20] Hence, the transfer became less visible to employers.[21]

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 statu-

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[19] Article 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.

[20] Interestingly, 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.

[21] Put it differently, at least it is less salient for employers regarding newly hired workers. Moreover, as far as we know, employers have no information about workers' family structure aside from what this program provides. Note that the salience of the transfer may also increase for employees when it is deposited directly into their bank account.

tory, 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.[22]

To rationalize the presence of wage effects in a setting with partial perception of benefits, in Appendix E, we set up a simple model based on Gruber (1997) and Bozio *et al.* (2023) that aligns closely with our findings. Formally, workers perceive a monthly wage $\tilde{w} = w + (1 - q) \cdot \tau^e$, which is a function of the true monthly wage $w$, the monthly transfer disbursed by employers $\tau^e$, and a perception parameter $q \in [0, 1]$. When $q = 1$, workers fully understand how the transfer works and how it is funded, and when $q = 0$, there is complete confusion. 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 capture part of the benefits by lowering wages.[23]

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.

## 4 Administrative data

**Wage earnings data (SIPA)**. The main source of information used in our analysis is an employer-employee panel compiled by the Ministry of Labor and known as SIPA. Employers report these administrative data monthly through Form 931 (the equivalent of Form 941 in the U.S.). All firms must use the same online processing software,

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[22]Per 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.

[23]We discuss two other alternative mechanisms in Appendix E.1, featuring shifts in the labor demand curve due to changes in the administrative burden for employers and/or rent-seeking opportunities. Our evidence, however, does not align with the corresponding predictions.

SICOSS, with a simple interface that makes it a reliable data 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.[24]

Table 2 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 AAFF 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, respectively. Note also that the average number of children is two across the earnings distribution.

Recall that under the old SFC system, family allowances were paid by employers to their employees, who could then net these payments out from payroll taxes. Employers thus had to report this information to the tax authority. The richness of our data allows us to observe how many employees received the transfer and the exact benefit amount each received. Once a firm is incorporated into the new SUAF system, this variable is automatically filled with zeros. This variable is of great importance since it allows us to properly estimate the first stage, i.e., the drop in the benefit disbursed by employers, and identify the switching date. Another interesting feature of this dataset is that it is reported monthly, which is only sometimes the case in most countries. This is appealing because it enables us to look at wage variation across the year and because the treatment timing variation we observe, i.e., the switching date, occurs every month.

**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.

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[24]This 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).

# 5   Empirical strategy and results

## 5.1   Event-study approach

We implement an event-study approach that leverages the staggered transition of firms into the new payment system. We define the *event* as the month-year $t$ at which a given firm is incorporated into SUAF and stops disbursing family allowances. Hence, the last payment observed in the micro-data occurs at month $t-1$. To accurately identify the event date using the employer-employee micro-data we focus on firms that: (i) were paying family allowances for at least six months before the event, (ii) existed at least six months before and six months after the event $[-6;5]$, (iii) have more than one worker receiving the transfer before the transition $(t-1)$, and (iv) have workers both with and without children during the entire event-window. Our estimation sample comprises an unbalanced panel of firms observed between January 2003 and December 2010. In particular, we include firms for which we observe an event and that transitioned before the beginning of the 2008 crisis.[25]

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 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).[26] Our setting is rich because we have large variation and heterogeneity to analyze and explore. We observe thousands of firms experiencing this event (a large number of treated units), events occurring during 96 consecutive months (time variation), and heterogeneity in firm size (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 are receiving the transfer (intensity of treatment at the worker level).

We exploit within-firm variation before and after the transition into the new system

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[25] The post-August 2008 roll-out period is not ideal for an event-study approach for several reasons: (i) bunching of events with a large number of companies simultaneously switching to the new system in August 2008, (ii) the beginning of the 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, (v) in 2009 the family allowance program was expanded to include informal workers (AUH). See Appendix B for more details about the macroeconomic context of Argentina during 2003-2010.

[26] Workers 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.

as follows. For each firm $f$, group $g = C, T$, and month $t$, 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:

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

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.[27]

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:

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

To compute the reduced-form point estimates in our tables we can simply pool all the coefficients before and after the switching date and then take the difference. We do so in a regression framework so that we also get the standard errors:

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

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.

To construct the wage gap in the data, we consider a monthly wage variable (before

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[27]In Section F we explain the econometric specification in greater detail. In Table A2 we present baseline characteristics for eligible and ineligible workers at $j = -1$.

taxes and transfers) that is used to calculate employers' social security contributions. As in most countries, this variable is right-censored, as a cap on SSC exists. This cap, however, is above the 95th percentile for all the months that we analyze; thus, it does not threaten our results. In addition to computing the average wage, we compute other moments such as the 25th, 50th, and 75th percentiles. We look at an event-time window of two years around the event, bin up the end points as standard in this literature (Schmidheiny & Siegloch, 2019), and we cluster standard errors at the firm level.

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.

Our baseline estimations focus on wage effects during a two-year window centered on the switching date (one year before and one year after). The rationale for such a specification is that this time frame allows us to look at a whole calendar year, which naturally considers wage dynamics due to seasonalities and wage- renegotiations or labor agreements usually updated once a year. As a robustness check, we vary the time window and show very stable results.

## 5.2   First stage, reduced form, and pass-through

We start the analysis by estimating the first-stage change in the remittance of family allowances before and after the event. We run specification (1) using the difference in average transfers each employer paid to workers with and without children as the dependent variable. We plot the $\gamma's$ of the referenced equation in Figure 6. It shows that, when firms transition to the new system, they immediately stop disbursing the transfer and hand this task over to the government. On average, workers with children received approximately 90 pesos more in transfers, disbursed by employers, before the event than workers without children (about 10% of average monthly wages). It is worth remembering that workers do not lose the transfer after the switch; it just starts being paid directly by the government. But, notably for the present analysis, it is no longer managed by employers.[28]

The null hypothesis of interest is whether the way transfers are delivered affects

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[28]Unfortunately, we do not observe payments at the individual level under the new system. Hence, we implicitly assume that workers continued to receive their benefits. This assumption is consistent with aggregate statistics reported in Figures I.1 and I.2, where total child benefit spending and the number of beneficiaries do not decline over time.

gross wages or not. If it is neutral, we should observe no effects on monthly wages after the transition; employees benefit entirely regardless of how the transfer is paid. By contrast, if employers were fully shifting the incidence of the transfer under the old scheme, then monthly wages should increase peso for peso by 90 pesos after the change.[29] In practice, wage effects could arise if employers offer the transfer within the compensation package by saying they could pay a certain amount, including the family allowance. Intuitively, if employers were capturing part of the transfers, we should observe a wage increase for eligible workers relative to non-eligible workers as soon as the firm enters into the SUAF system. An interesting fact of our setting is that inflation was high during the whole period (about 15 percent annually) and, thus, monthly wages were frequently renegotiated.

In Figure 7, we look at the reduced-form effect on monthly wages (before taxes and 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. 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 contrast, the monthly wage of workers with children increases by approximately 5 pesos relative to workers without children when firms stop disbursing the transfer.[30] Hence, both figures present clear evidence that the wages of eligible workers adjust to changes in the payment system.

The effect dynamics show an initial bump in the wage gap after the firm stops disbursing the transfer, a gap that tends to increase over time. Figure 8 examines wage levels and differentials up to two years after firms enter the new system. If we focus on the last six months, we observe an increase in the wage of workers with children relative to workers without children of about 12 Argentinean pesos.

In Figure 9, we analyze other moments of the distribution beyond the average wage for each group ($\bar{w}_{f,t}^T$ and $\bar{w}_{f,t}^C$). The figure shows that the increase in wages is mostly driven by workers at the lower end of the monthly wage distribution. The $p25$ wage differential of workers with and without children exhibits a sizable jump after the

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[29] An important caveat, is that we do not know the incidence prior to the reform, i.e., the incidence in levels. We estimate the change in incidence due to a change in the remitter. In Appendix G.3, we elaborate further on what happens with the baseline incidence by looking at wage effects after a worker becomes ineligible due to children reaching adulthood.

[30] The increase in the wage gap is driven by an increase in eligible workers' wages as opposed to a decrease in the wages of non-eligible workers. The latter is rather uncommon in nominal terms. However, real wages could decrease in contexts of high inflation, as Argentina experienced.

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event, while relatively little happens with the $p75$. This result is reassuring as family allowances represent a higher share of wages for $p25$.[31]

In Table 3, 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, i.e., the difference between the 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 exercise summarizes previous figures and shows that the monthly wage of eligible workers increases by 5 pesos relative to those ineligible after the firm switches to the new regime. This effect declines as we move up in the earnings distribution, where the transfer's average rate and salience are lower. Moreover, in terms of the pass-through, our estimates show that, for a 1 peso decrease in the transfer paid by employers (holding the total transfer constant), the wage increases by 5 cents, implying an incidence of around 5 percent for those at the average of the monthly wage distribution. Our longer-run effect of 12 pesos, displayed in Figure 8, yields a pass-through rate of approximately 14 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 A3). Point estimates are fairly stable with no fixed effects, with firm and time fixed effects, or with firm-specific linear trends. Second, results do not change when we use a balanced panel of firms present in the 96 months of data (Figure I.5), when we change the number of consecutive months the firm was paying transfers right before the event (Figure I.6), or when we vary the length of the event-time window (Figure I.7). Third, the results are preserved when we consider workers that are fully treated during the period 2003-2010, namely, those with children under 18 years of age during the entire roll-out period (Figure I.8). In addition, our results are robust to controlling for firm structure (size and composition) in the main specification (Figure I.9), and to different estimation samples of firms (Figure I.10). Moreover, we show that our results barely change when we include never-treated firms as additional controls in the event study (Figure I.11).[32] Reassuringly, our results are also very similar to those estimated with the recently proposed alternatives to TWFE regressions that do not restrict treatment effect heterogeneity between groups and over time (see Figure I.12 and Appendix G.2). Finally, we include a placebo exercise where we assign a fake event date to each firm

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[31]Intuitively, this exercise estimates the wage gap of two line workers ($p25$) and two executives ($p75$) with and without children within each firm. One potential interpretation of the wage effect, is that it could be driven by a discriminatory behavior against workers with children. This figure somehow shuts down this channel, as the wage effect is predominantly observed among workers situated at the bottom of the earnings distribution (likely program recipients), rather than those at the upper end (less likely to qualify for the allowance).

[32]Note that there is a share of firms that are never treated, i.e., for whom we never observe a payment under the old system. We discuss this in Figure 5 and its corresponding footnote.

and then we re-estimate the reduced-form effects. We replicated this exercise 1,000 times to have a distribution of simulated reduced-form estimates. As can be seen in Figure I.13, our baseline estimate of the wage effect is outside the suggested 99 percent confidence interval.

**Worker ineligible (child turning 18 years old)**: Given the nature of the variation that we exploit—a change in the way the transfer is paid—we cannot make a statement about the baseline incidence of the transfer, i.e., who benefits from it regardless of how it is disbursed. However, the richness of the dataset we have access to allows us to exploit another quasi-experimental variation. Specifically, we investigate what happens to a worker's wage when her child turns 18 and loses eligibility.[33] In a nutshell, we find a clear drop in the transfer amount as the child turns 18 but no effect on wages (see Figure I.25). We have three potential explanations for this null effect. First, this is an individual rather than a firm-level shock, which could have differential effects, e.g., how workers negotiate could be different. Second, it is likely that workers that become ineligible because of child aging are closer to the $p75$ than to the $p25$ and, therefore, have a weaker first-stage change in the amount of the transfer. Third, if rent extraction occurs when the labor relationship and contract are set, as with new hires, then it is harder to detect changes in wages for contracts that are already set.

**Who ultimately bears the increase in wages?** A comprehensive incidence analysis would require exploiting firm-level data such as profits, sales, prices, etc., to disentangle whether workers or firms bear the increased wages. For example, the increase in wages received by eligible workers might come at the expense of other (control) workers or result in reduced profits. Unfortunately, we lack access to data related to the latter point. However, we 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 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.

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[33]This exercise includes firms under the old system because, during that period, we observe the transfer's amount and thus we are able to compute a first stage. In addition, we could eventually do the inverse exercise and analyze what happens to gross wages when a worker starts receiving the transfer after their child is born. Note, however, that many other things can change after a child birth (e.g., labor supply) making the identification harder.

# 6 Heterogeneity and potential mechanisms

Our results thus far show that the way family allowances are disbursed is not neutral and affects gross wages. On average, employers capture about 5-13 percent of the transfers when they mediate these payments. The incidence literature would benefit from having a story that explains the drivers of this finding, which usually remains unexplained. We would like to know the circumstances that made this rent capture possible and how it was subsequently reduced. In this section, we discuss some competing channels that could explain this response, one driven by employers and another driven by employees. We show evidence in favor of the former and somewhat against the latter.

On the one hand, it could be that employers were exploiting workers' confusion under the old regime and integrating the transfer into the compensation package when the contract was set to capture part of the transfer. After the firm switched to the new system, they could no longer engage in this practice (*employer-driven* channel). Alternatively, it could be that eligible wage earners were confused and, after the event, when they started getting the transfer directly from the government, they realized that their paycheck went down compared to their co-workers and thus started bargaining more aggressively (*employee-driven* channel).[34]

Both of these stories require an imperfect understanding of how family allowances work and how they are funded. This confusion, indeed, is what was happening on the ground before and during the reform. The situation before the reform is clearly illustrated in a book compiled by the social security administration: "...*the old system (SFC) blurred the image of the State as responsible for it. (...) The roles were confused. People considered that these benefits were part of their salary and that employers were responsible for them. They even ignored that it is the State that pays for them*..." (Marasco, 2007). Before the reform it seems that benefits were poorly understood and that there was only a partial awareness of them. Furthermore, a survey conducted by the SSA in 2018 confirms that workers are still confused about how family allowances are currently funded. More than 50 percent of respondents replied that they do not know, 35 percent correctly said that the government pays them, and 8.6 percent still think that it is funded by employers (see Table 5).

The key piece of evidence for the *employer-driven* channel is that wage effects seem to be driven by new hires rather than incumbent workers. This is reported in Figure 11 where we run two different regressions: the one that considers all the workers (blue

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[34]Intuitively, it could be that people were upset because some experienced a fall in their paycheck but their childless peers did not. Therefore, workers with children complained to the employer and, in response to repeated pestering, the employer gives them some compensation.

line) and another one for a balanced panel of employees present at the firm during the entire window (red line). The difference between these two lines thus captures the response of new hires.[35] The figure shows no effect for incumbents. This group of workers already has a written contract, and their payment schedule is somewhat predetermined, at least in the short run. For new hires, in contrast, the contract is set when they are hired. When the firm is no longer in charge of paying the transfer, they can no longer offer it as part of the starting wage, and thus, the gross wage (before taxes and transfers) of new hires goes up.[36, 37]

In Figure 12, we further break down the aggregate wage effect by firm size and type of business. The effect is stronger in small firms with 10 or fewer employees. The results are also summarized in Table 4. Note that in the case of small incorporated businesses, the pass-through rate is -0.092, meaning employers were capturing about 10 percent of the transfer when they were in charge of its payment. This result aligns with the idea that such rent-seeking behavior is stronger in places where employers are closer to their employees. Large firms usually have a human resources department in charge of hiring; thus, it is presumably harder for managers to engage in such behavior. There are two other plausible reasons for differential effects by firm size. First, large firms generally give their workers other types of payoffs and in-kind benefits. Second, it is also likely that small firms are less monitored by labor unions, the SSA, and the tax agency.[38]

Further to the last point about contract enforcement of labor unions, we present evidence on the potential role of unions in limiting the economic incidence of tax credits.

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[35]Note that our empirical strategy requires firms to have both workers with and without children during the whole event window of the figure. This is why we have to infer the behavior of new hires indirectly. Otherwise, we would be asking too much from the data, especially for small firms, i.e., firms hiring at least two workers every month, one with and another without children.

[36]The new hires story shut down an alternative interpretation linked to employers partially insuring their workers in a hypothetical scenario where they do not receive the transfer under the new regime (or take some time to be effective). This also shuts down the potential left-digit bias type of adjustment common in the behavioral literature. This adjustment would have suggested that employers increase the wages of those workers such that the first digit of their take-home pay remains unchanged.

[37]We also explore whether the composition of workers between groups changed after the firm joined the new payment system. Naturally, it could be the case that firms reacted to the reform by hiring different types of workers, e.g., younger, more educated, etc. We then focus on some observable characteristics that can be inferred from the data we access. In particular, we look at the following outcome variables: the share of unionized, full-time and female workers, and a proxy of age (extracting and using information from the individual identifier). Overall, we do not find statistically significant effects, the only exception being a small increase in full-time workers (see Table A4). Unfortunately, some other interesting characteristics are not available to us e.g., education, type of occupation, etc.

[38]In some cases, there may have been a systematic violation of collective bargaining agreements (CBA). That is, employers were agreeing on wages plus transfers before the event to match the CBA wage schedule. Although the CBA refers to wages, in practice they might have been implemented or understood by employers as wages plus transfers (predominantly in small firms). This story of companies violating union contracts became less feasible after the reform because employers lacked accurate information on beneficiaries and transfer amounts.

We estimate 2SLS pass-through rates by different bins of firms' exposure to union regulations. We measure exposure as the percentage of unionized workers within each firm. The results are summarized in Figure 13 and show that rent capture is greater in firms with a low share of unionized workers and is more muted in firms where more than 50 percent of their employees are unionized. This result strikes us as remarkable and suggests that unions could prevent employers from lowering wages to capture some of the family allowances (as conceptually noted by Lee & Saez, 2012).

Taken together, the results from Figures 11 to 13 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.[39]

Finally, we discuss three reasons why pay equity concerns do not seem to be the dominant channel at play. First, if this was a bargaining story, one would expect the effect to arise slowly over time. However, we find an immediate effect at $t = 0$, which is more consistent with an *employer-driven* response. Second, fairness concerns would operate mainly for incumbent workers at the time of the event. Yet, we find a null effect for this group and a larger effect on new hires. Third, one would expect pay equity concerns to operate more strongly when there is a mix of eligible and non-eligible workers. Hence, the pass-through effect should exhibit a U-shaped relationship with a firm's exposure to family allowances. That is, it should be stronger at firms where 50 percent of the workforce have children and smaller at the extremes. We test this hypothesis in Figure 14. Panel (a) shows the distribution of firms based on their exposure to family allowances, and panel (b) shows the wage effects for different bins of this exposure measure. The analysis shows an increasing—rather than U-shaped—relationship. That is, rent capture of child benefits is higher in firms with a high share of eligible workers with children. Considering that rent-capture rates are relatively low, this result could imply that firms find it worth engaging in such rent-seeking behavior when there is a large share of workers to extract rents from.

**Is this a firm-level or a market-level response?** 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

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[39]For completeness, we also analyzed heterogeneities across sectors, but found rather similar point estimates across the different sub-samples of firms (see Figure I.14).

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*.[40]

To this end, we track the dynamics of the wage effect by looking at how the treatment outcome evolves as a larger number of firms enter into the new payment system. Figure 15 presents the 2SLS pass-through rates over time, where each dot includes a different sample of firms that changes over time. We focus on firms that changed regimes within a 30-month window, and then we move forward one month following a rolling window of events. For instance, the first point includes firms that switched to SUAF 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. Overall, we find a relatively stable (or slight U-shaped) effect over time, which somehow rules out both the *learning* story and the market-level effect.[41]

**Composition and employment**: The interpretation of our wage effects as causal would require no instant changes in employment and the composition of workers within treated firms. The composition of firms in terms of workers with and without children, as well as its size, could have reacted to the reform. In Figure 16, we look at these two margins using the same specification as in equation (2). As we can see, there is no immediate effect on the gap between eligible and non-eligible workers (panel a and b) or on the total number of employees within the firm (panel c). In the long run, two years after the event, it seems that firms start to hire more workers without children than with children. This is what we would expect given the change in the relative price of these two groups, as our wage effects indicate that workers with children become relatively more expensive as time goes by after the event.

**Other responses**: We also look at whether the reform affected the delinquency rate of firms. To that end, we used an additional source of information containing employers' monthly financial situation. The Central Bank of Argentina compiles this information into the *Central de Deudores del Sistema Financiero* (CENDEU) database. Though somewhat limited due to the time frame to which we had access, it seems that the reform

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[40]A simple search of the word "*SUAF*" in Google trends, as a proxy of general awareness of the new system, from the beginning of 2004 (the first year where information is available) to the end of 2010, shows no spike at the beginning and a rather constant number of searches. In fact, the peak occurred in June 2010, the final due date for enrollment in the new system.

[41]In Figure 15, we also see that the effect fades away as soon as we start including firms that switched after the beginning of the crisis (dashed red vertical line). As a reminder, in footnote (25), we list the reasons why we exclude firms that transitioned after the start of the crisis.

had almost no effect on the delinquency rates of firms (see Figure I.28).[42]

Finally, the family allowance scheme contains *notches* that could eventually lead to collusion between employers and employees, which would show up as bunching behavior right before the threshold. Moreover, before the reform, employers were more able to strategically keep the salary below the notch to benefit from the transfer; therefore, there was space for collusion because the employer was relatively more aware of the transfer. However, when we examine the charts in the appendix, we see no clear bunching in the different notches of the transfer scheme (see Figure I.29).

# 7   Conclusion

In this paper, we study whether the way family allowances are disbursed affects who bears the final incidence of the transfer. To test the standard neutrality hypothesis from the neoclassical model, we exploit a change in Argentina's remittance system of family allowances. Under the old system, employers were in charge of disbursing the transfer to their eligible employees and had the right to net these payments out of employer SSCs. Under the new payment system, the government eliminated the firm's intermediary role, and started disbursing the transfer directly into workers' bank accounts. As a result, the transfer became less salient to employers, who used to know who received it and how much each received. Firms were gradually incorporated into the new scheme over the course of eight years due to administrative and capacity constraints. We combine this gradual roll out, using an event-study design, and employer-employee administrative data to identify wage effects of means-tested transfers that employers mediated.

Our results show that how family allowances are disbursed is not neutral and affects gross wages. We estimate that the average monthly wages of workers with children increase by 5 to 12 pesos relative to workers without children when firms stop disbursing the transfer. Wage effects are larger for those located at the lower end of the income distribution, where the average tax credit rate and salience of the transfer are larger, and decline as income increases. In terms of the pass-through rate, i.e., when we scale the previous increase by the size of the transfer, we find that employers were capturing 6 to 14 percent of the transfer through lower wages when they mediated the disbursement.

The wage increase is mostly driven by new hires rather than by incumbent workers, eliminating the pay-equity-concern channel. As soon as firms switch to the new

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[42]This result somehow shuts down the hypothesis that firms started paying higher wages to eligible workers with children due to the administrative burden relief (as a result of the new system).

regime and stop delivering the transfer, they lose the ability to integrate the transfer into the wage package, and thus, the market wage of new hires with children increases. The fact that the effect appears immediately in the first month post event, and that it is driven by new hires, argues against a pay-equity-concern channel, because one would expect the effect to build up slowly and also to affect incumbents. This finding makes sense as incumbents have already-written contracts; thus, it is harder to observe wage effects; firms have greater freedom to negotiate in the case of new hires. Moreover, we find that the effect is larger in small firms and in less unionized firms, which probably have greater flexibility to adjust wages. Finally, we document that wage effects are stronger the higher the exposure of a firm to family allowances.

Taken together, our findings suggest that the way governments set up tax credit programs, like the EITC in the U.S., matters and influences the final economic incidence, contrary to what the standard incidence model would have predicted. We find that wages adjust to how transfers are disbursed, rejecting the null hypothesis that transfers are *entirely* captured dollar for dollar by workers. Our results have practical significance in light of the increasing importance of social protection systems worldwide. Moreover, there exist both developing (e.g., Brazil) and developed (e.g., Switzerland) countries that mediate transfers through employers, despite the lack of evidence or awareness of potential economic costs associated with this practice. More generally, this paper suggests that relying on firms as mediators in the tax-benefit system could have unintended consequences; where less salient schemes may lead to rent capture.

Finally, Blanchet *et al.* (2022), argue that what drives the difference in inequality between European countries and the US, is not explained by a successful redistributive scheme (via taxes and transfers), as commonly assumed. Instead, it is influenced by a more equitable pre-distribution (i.e., market earnings). Our paper extends this view by emphasizing that the implementation of the redistributive mechanism can also have a notable impact on pre-distribution income.

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# Figures and Tables

Figure 1: Family allowance schedule

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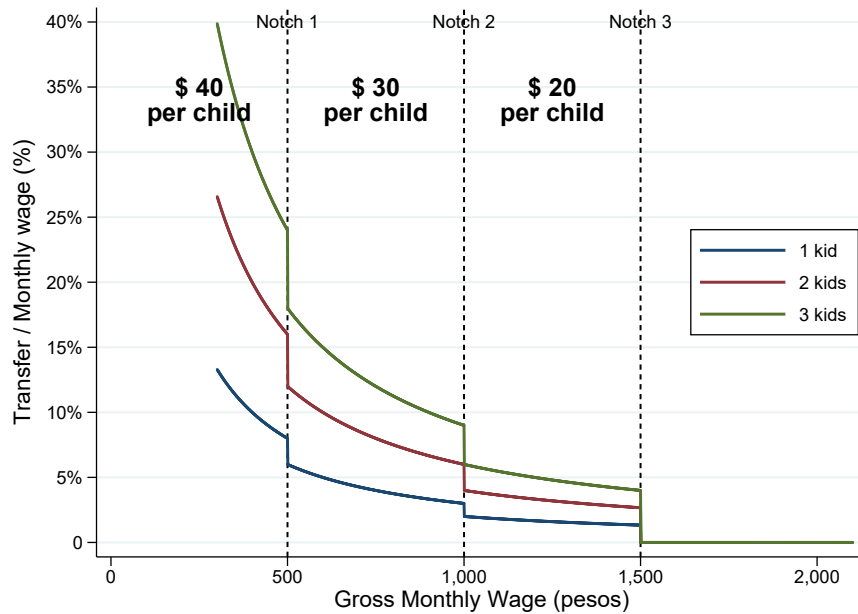


(b) Bracket thresholds (2003-2011)
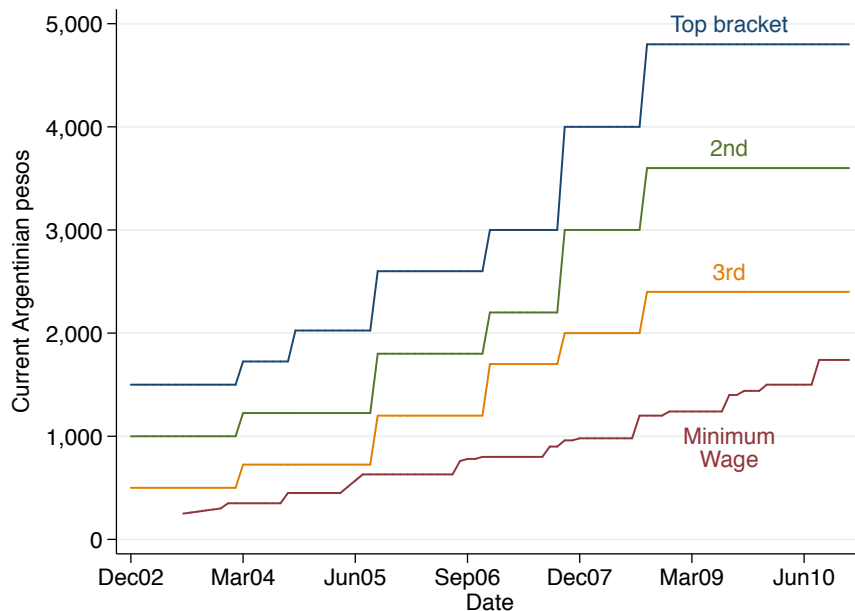


*Source*: Author's elaboration based on official documentation.
*Notes*: Panel (a) shows the transfer as a proportion of wage (i.e., the ratio of transfer to earnings) over monthly wages. Each line corresponds to a different number of children below 18 years old. Panel (b) presents the three upper monthly thresholds of each bracket; the series at the bottom represents the evolution of the minimum wage. All series are expressed in current Argentinian pesos.

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

**Old system (SFC)**

$$\textbf{Wage}_0 + Transfer(\tau^e) \qquad\qquad SSC - Transfer(\tau^e)$$

| Employees | Employers | Government |

**New system (SUAF)**

| Employees | Employers | Government |

$$\textbf{Wage}_1 \qquad\qquad SSC$$

$$Transfer(\tau^g)$$
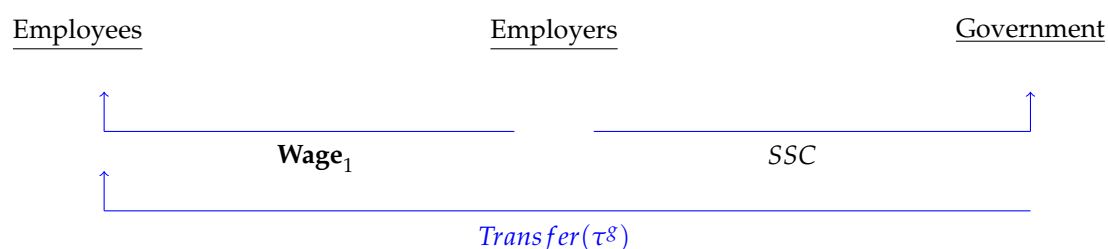
*Notes*: This diagram illustrates the change in the payment system of family allowances. Under the old system (SFC), employers had responsibility for delivering child benefits together with the monthly wage. For transparency purposes, the government replaced the intermediary role of firms and started 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 $\textbf{Wage}_0$ and $\textbf{Wage}_1$ are the same for eligible workers with children before and after the change.

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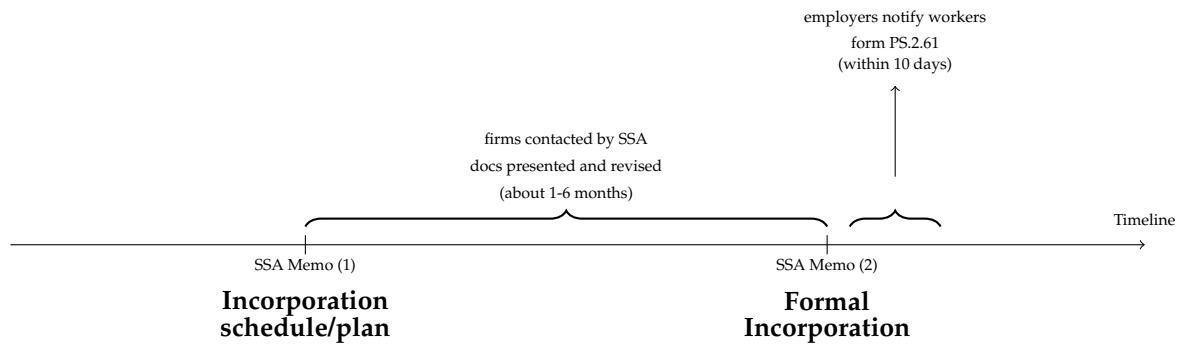


*Notes*: This figure shows the timeline of the incorporation steps to the new payment system (SUAF). This process was determined by the SSA through official memos posted online. The incorporation started with the SSA setting an *internal* incorporation schedule, where the SSA issued a list of targeted 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 5: Gradual roll out from the old to the new system

### (a) Micro roll out (employer-employee microdata)



### (b) Roll out by firm size
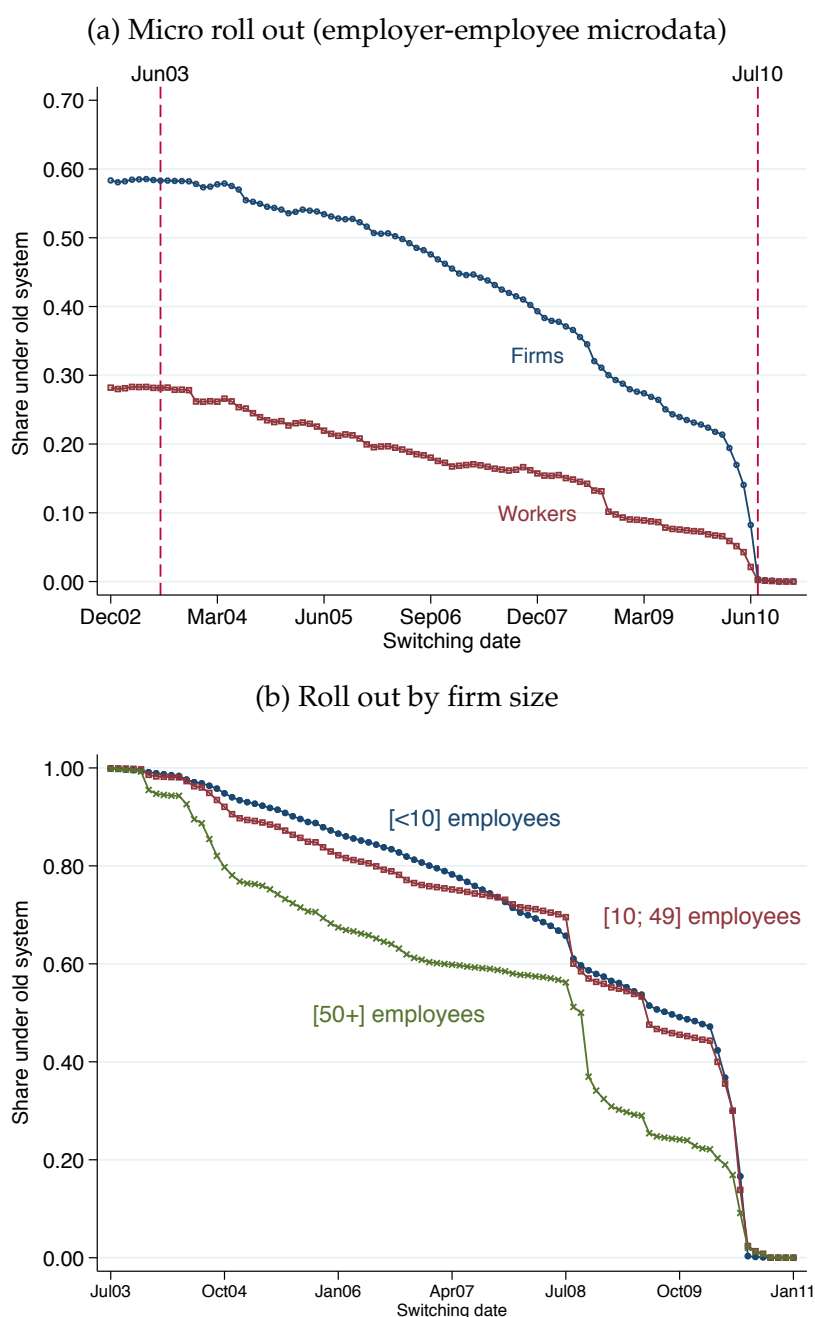


*Notes*: This figure shows the gradual transition of firms and workers from the employer-based to the government-based payment system. Panel (a) focuses on all firms and workers in the micro-data. In the first month of the chart i.e., January 2003, 40 percent of firms were not paying family allowances (a similar share emerges if we look at, for instance, the corresponding figures from 1998, a dataset to which we also have access). This share can be decomposed into the following types of employers: [i] micro firms i.e., with one or two employees; these compose approximately 20 percent; [ii] employers that never pay family allowances jointly with wages, notably those in rural areas and with seasonal activities; [iii] some public firms or dependencies, and education-related institutions. Those employees who used to work for employers of type [ii], received the transfer directly from the government (under a system named (*pago directo*). The rationale for these employers not being part of the SFC (old system) is that, as they typically have seasonal income flows, and low-wage workers with a rather large number of children, their corresponding transfer was rather high, and thus the government did not want these firms to have to make monthly payments. This *pago directo* system also helps explain why 20 percent do not paid through the SFC mechanism at the beginning of the period according to official budget information (see figure I.3.) 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 6: First-stage change in the remittance of child benefits
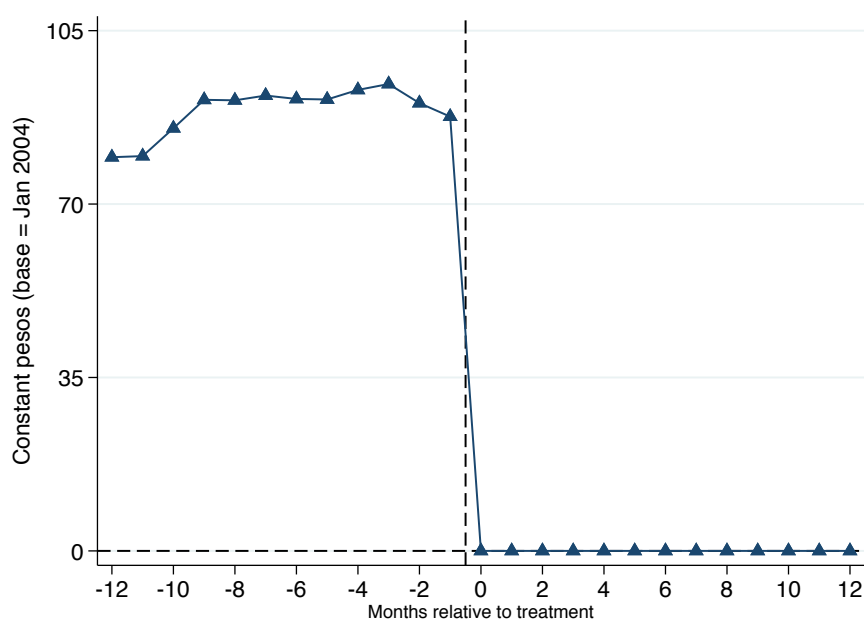
*Notes*: This figure shows the first-stage change in the remittance of child benefits by firms. It presents event-study estimates of the parameter $\gamma$ of equation (1). The dependent variable is the within-firm difference in average family allowance of workers with and without children. The figure shows that when firms transition to the new system, they immediately stop disbursing the transfer and hand this task over to the government. On average, workers with children were receiving approximately 90 pesos more in transfers per month, disbursed by employers, than workers without children.

## Figure 7: Reduced-form wage effects

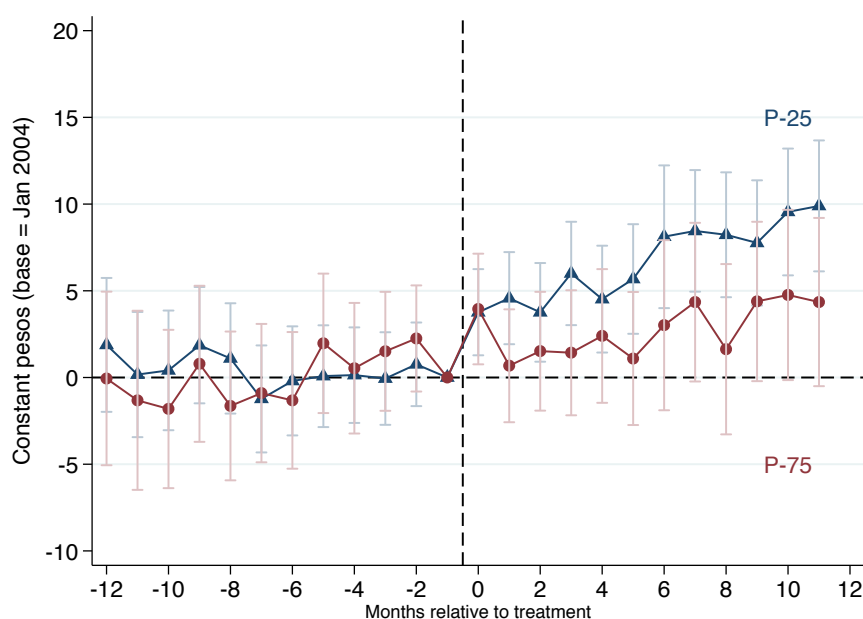### (a) Average wage levels
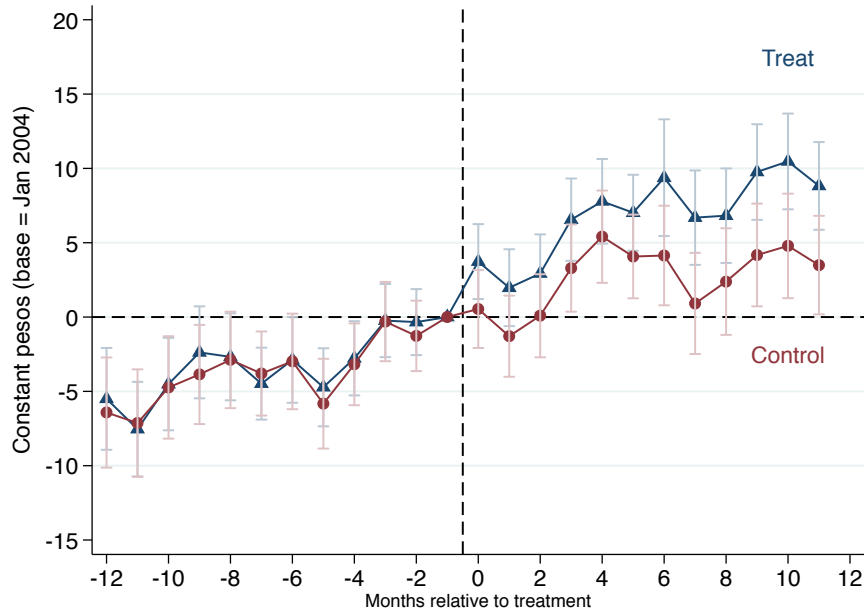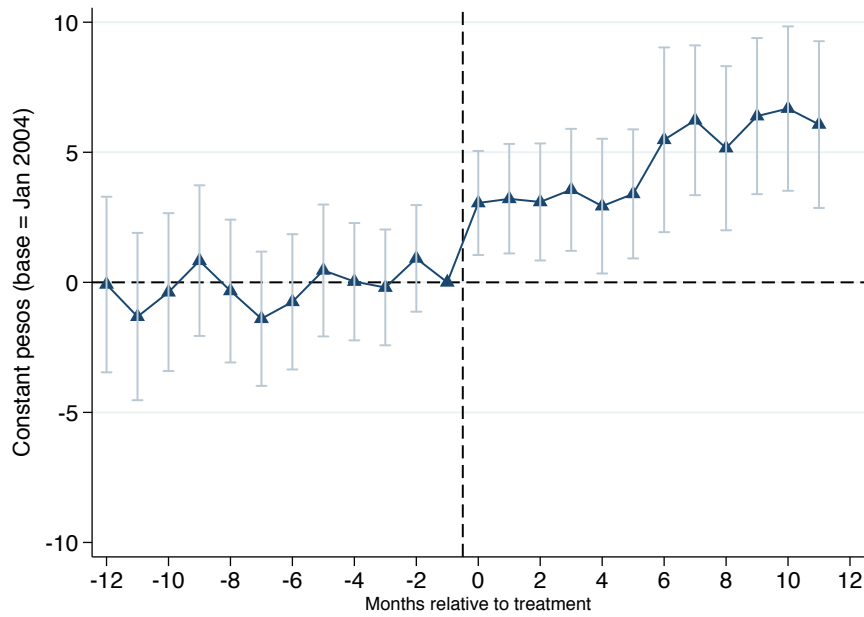


### (b) Average wage gap



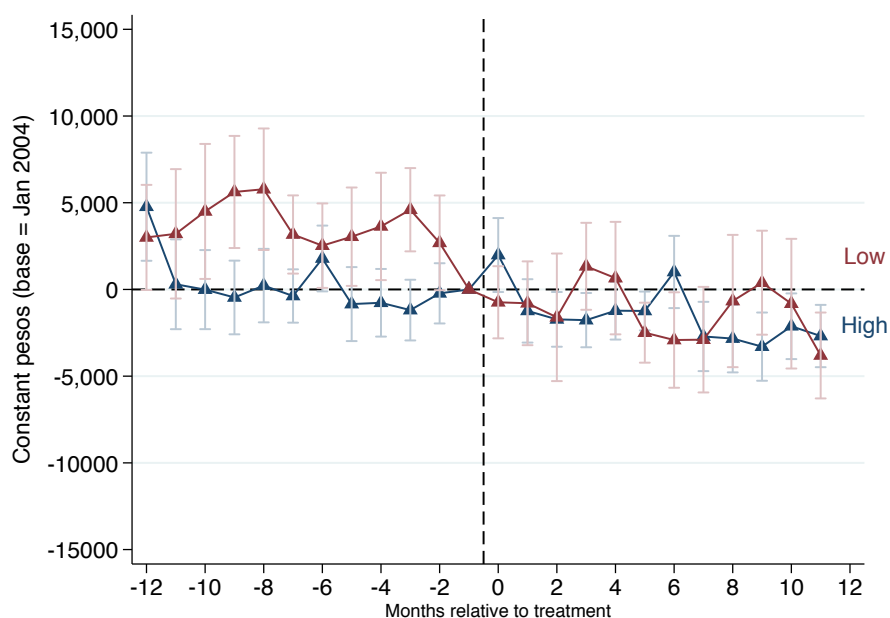*Notes*: These figures plot the event-study estimates of parameter $\gamma$ and its corresponding 95-percent confidence intervals of equation (2). Panel (a) shows the wage levels when we estimate this equation separately for workers with children (treatment) and without children (control). In Panel (b), 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 8: Reduced-form wage effects (longer run)

### (a) Average wage levels
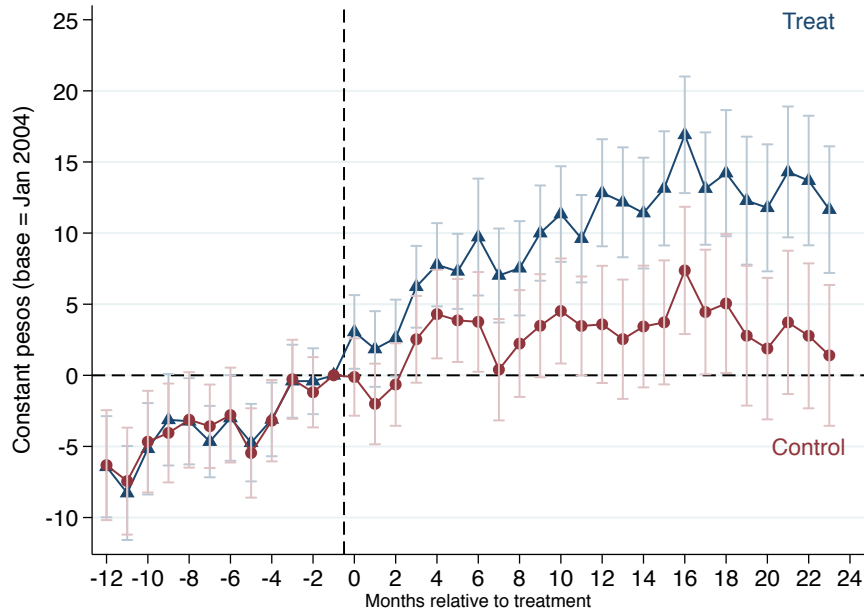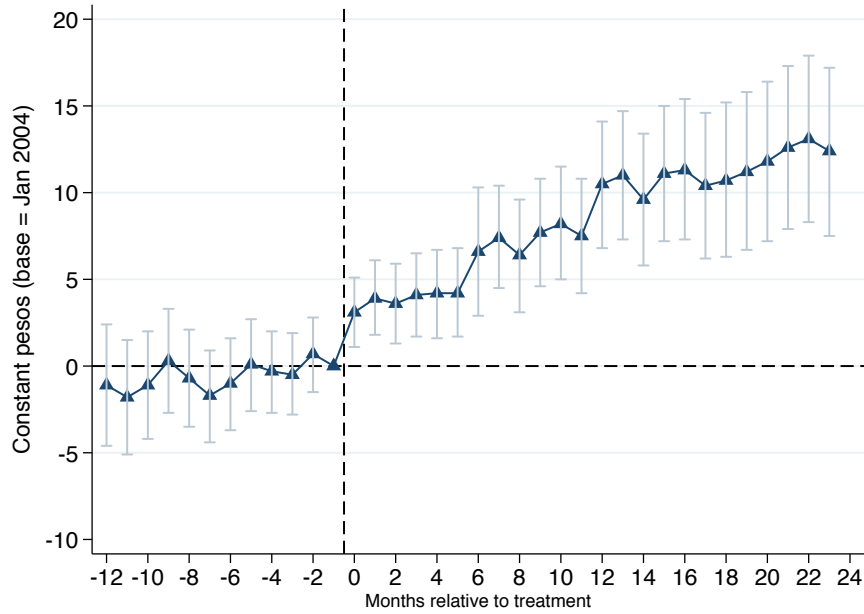


### (b) Average wage gap



*Notes*: These figures plot the event-study estimates of parameter $\gamma$ 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 9: Reduced-form wage effects: $p25$ vs $p75$



*Notes*: This figure plots the event-study estimates of parameter $\gamma$ 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.
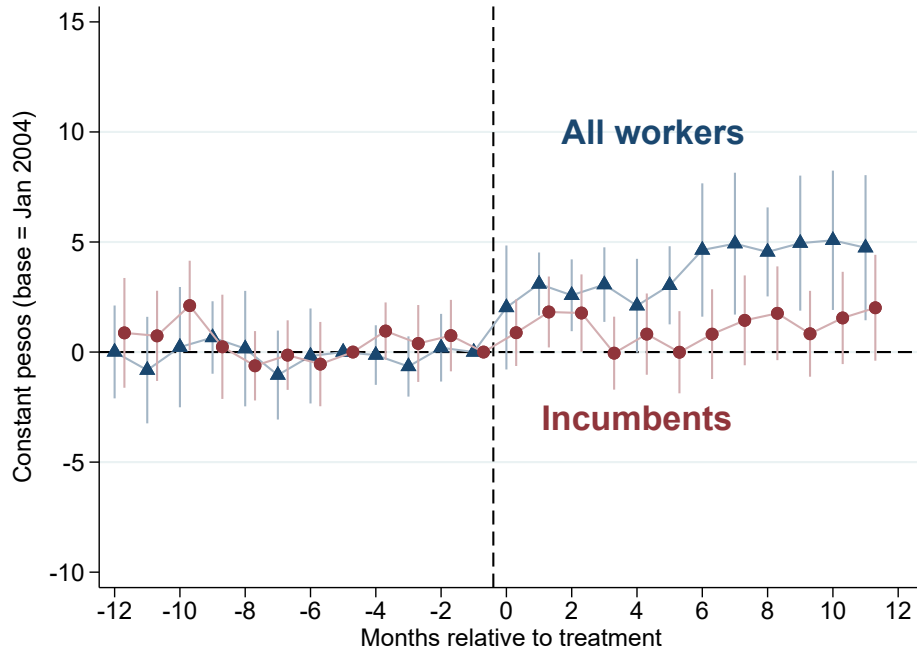
## Figure 10: Evolution of total wage bill by treatment intensity



*Notes*: This figure plots the event-study estimates of parameter $\gamma$ 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).
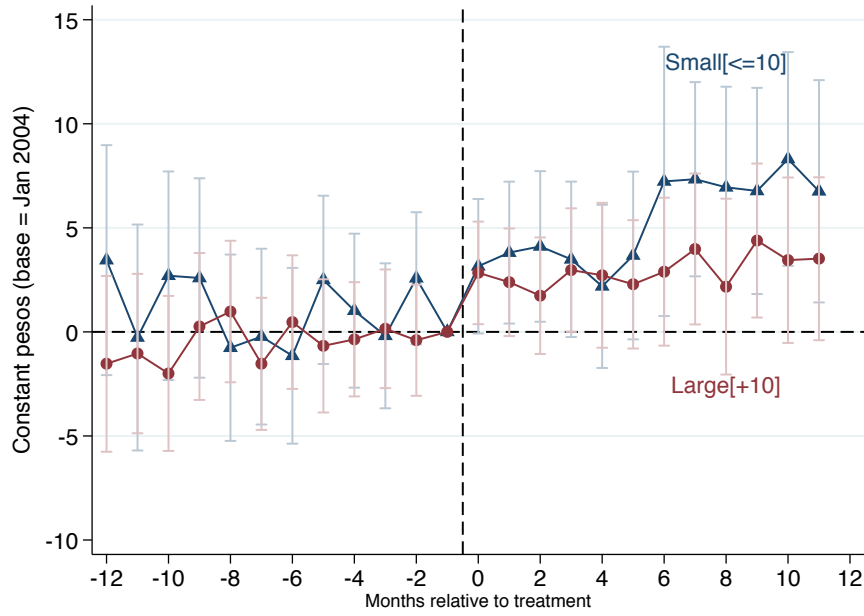
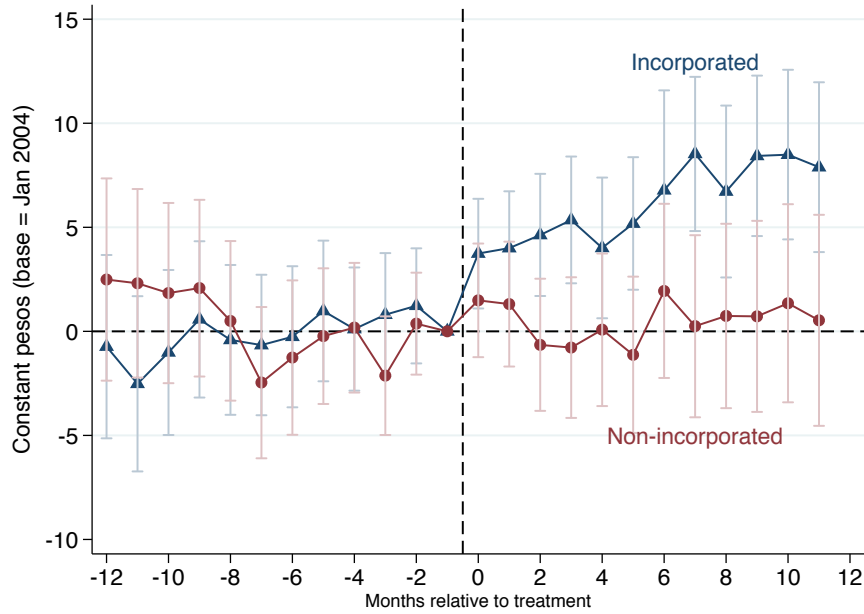Figure 11: Wage effects: New hires and incumbents



*Notes*: This figure plots the event-study estimates of parameter $\gamma$ 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 12: Firm size and type of business

## (a) Small vs. large firms



## (b) Incorporated vs. unincorporated



*Notes*: Panel (a) presents event-study estimates of parameter $\gamma$ 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.
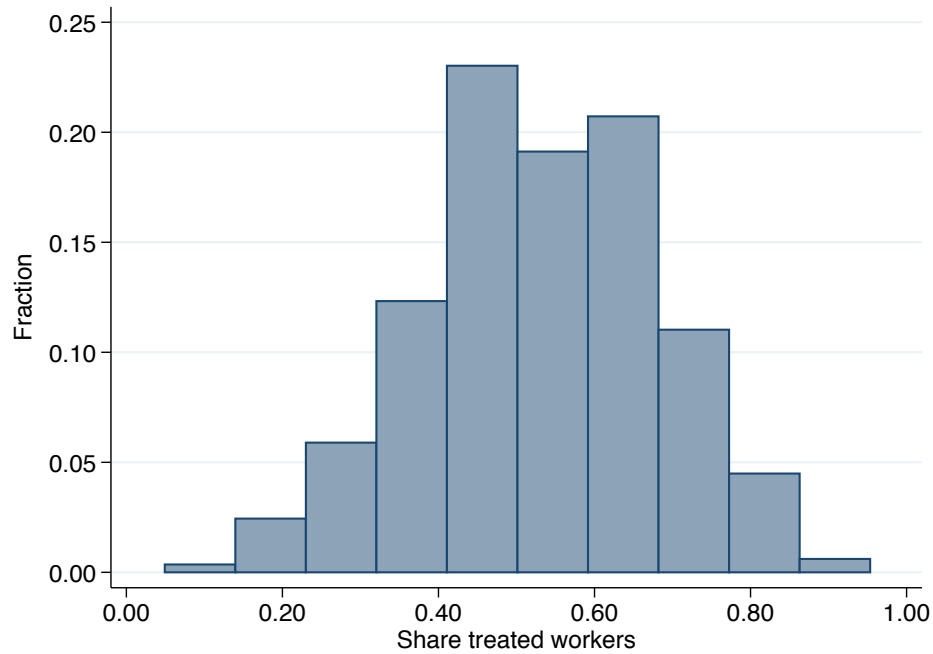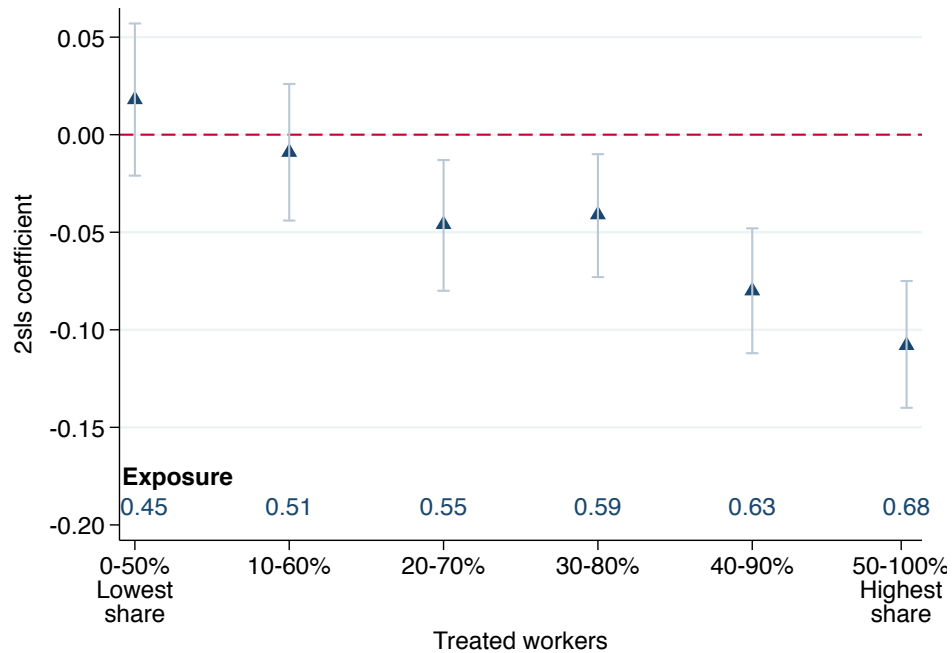
Figure 13: Pass-through by unionization rates
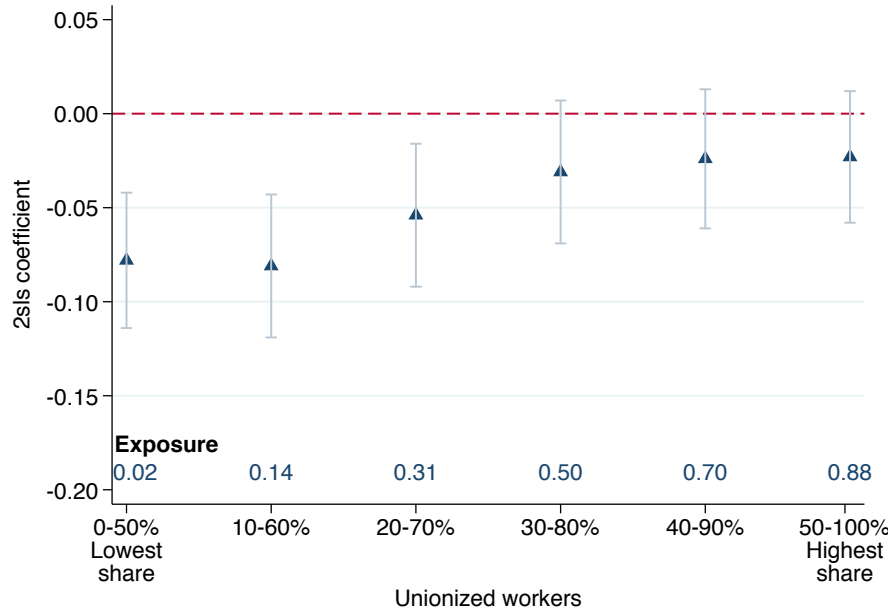
*Notes*: This figure plots the reduced-form estimates of equation (3) scaled by the first-stage change in the remittance of benefits. On the vertical axis we put the 2SLS pass-through coefficients. On the horizontal axis we put different exposure groups, defined as the share of unionized employees within 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 14: 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 15: Dynamic of wage effects over time (rolling window of events)
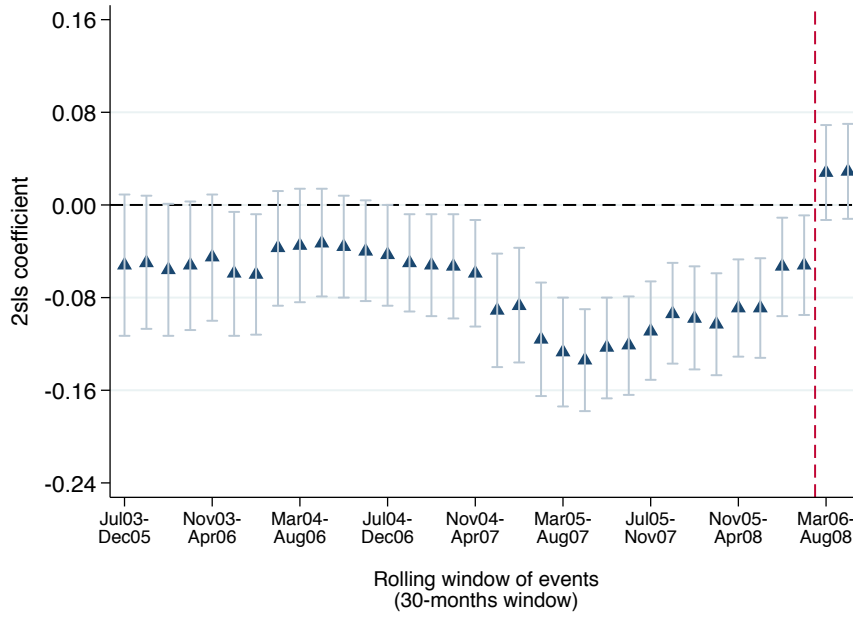


*Notes*: This figure plots the reduced-form point estimates of equation (3) scaled by the first-stage change in the remittance of benefits. It considers firms that switched to the new system in different time spans. In particular, we focus on firms that changed regime within a 30-month window and then 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 vertical dashed line indicates the moment when we start including firms that switched after the 2008 crisis began. The effect is more muted in the post crisis period. The last point includes firms that switched after the crisis, and the effect disappears.

## Figure 16: Composition of workers and firm size (24 months after the event)
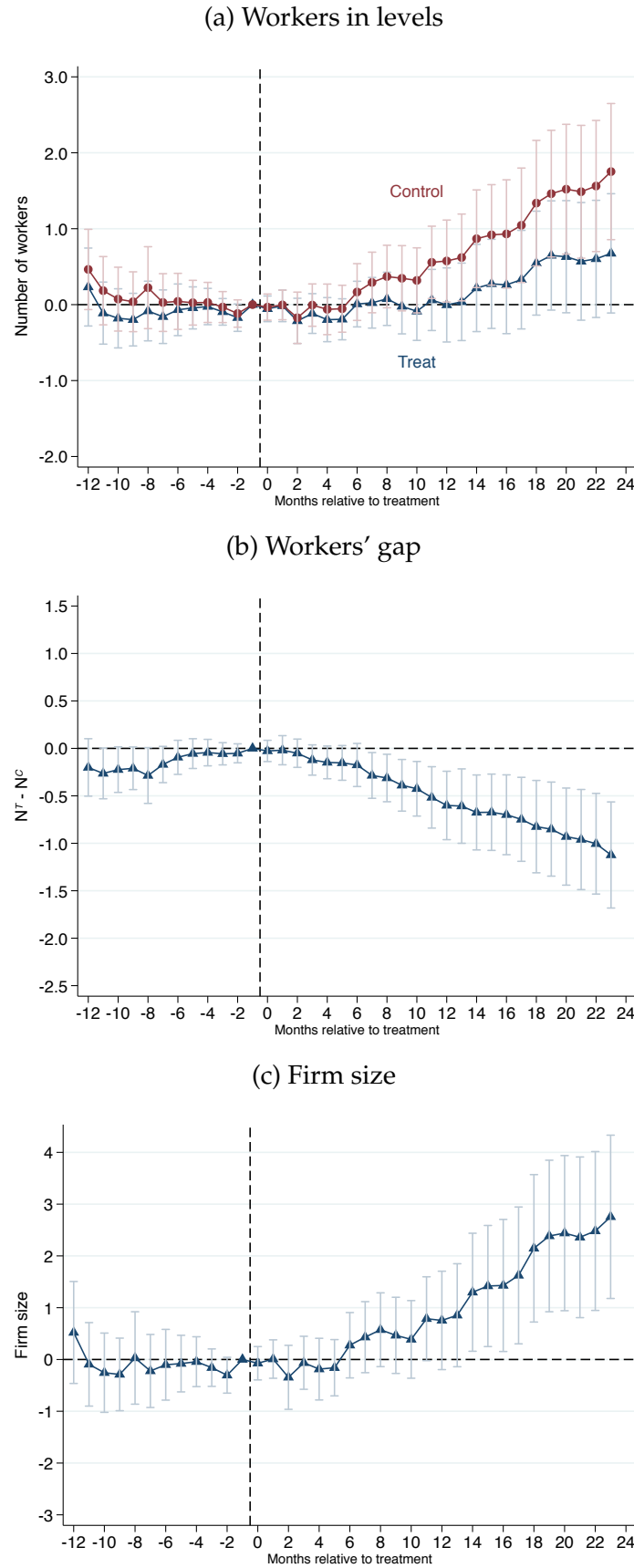
### (a) Workers in levels



### (b) Workers' gap



### (c) Firm size



*Notes*: These figures plot the event-study estimates of parameter $\gamma$ 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.

## Table 1: Key dimensions under the two payment systems

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

*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 2: Summary statistics for registered wage earners in Argentina, 2004

|  | 1st Bracket (1) | 2nd Bracket (2) | 3rd Bracket (3) | Universe (4) |
|---|---|---|---|---|
| Wage earners | 2,154,722 | 1,426,404 | 550,571 | 4,787,496 |
| Beneficiaries AAFF | 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 3: Wage effects and pass-through of a change in the remittance system

| | All post periods [t = 0-11] (1) | Last 6 periods [t = 6-11] (2) | Last period [t = 11] (3) |
|---|---|---|---|
| **Reduced form** | | | |
| Δ monthly wage | 4.69*** | 5.93*** | 5.73*** |
| (in pesos) | (1.21) | (1.52) | (1.88) |
| | | | |
| **First stage** | | | |
| Δ transfer $(\tau^e)$ | -94.13*** | -95.28*** | -93.94*** |
| (in pesos) | (0.35) | (0.37) | (0.38) |
| | | | |
| **2SLS** | | | |
| $\frac{\Delta wage}{\Delta transfer(\tau^e)}$ | -0.05*** | -0.06*** | -0.06*** |
| | (0.01) | (0.02) | (0.02) |
| | | | |
| Number of firms | 26,226 | 26,226 | 26,226 |
| Observations | 2,285,705 | 2,128,349 | 1,998,351 |
| 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 7 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 column (2), we pool the coefficients for the last 6 months post event. In column (3), we take the coefficients for the last month post event. Standard errors clustered at the firm level are reported in parentheses. *** significant at 1%, ** significant at 5%; * significant at 10%.

Table 4: Wage effects and pass-through by firm size and type of business

| | Small [<= 10] | Large [+10] | Non Incorpo | Incorpo | Incorporated Small [<= 10] | Large [+10] |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| **Reduced form** | | | | | | |
| Δ monthly wage | 4.21** | 3.36** | 0.26 | 6.15*** | 9.35*** | 3.10* |
| (in pesos) | (2.01) | (1.51) | (1.92) | (1.54) | (3.07) | (3.07) |
| | | | | | | |
| **First stage** | | | | | | |
| Δ transfer | -102.22*** | -86.62*** | -99.50*** | -91.66*** | -101.67*** | -85.76*** |
| (in pesos) | (0.61) | (0.37) | (0.71) | (0.40) | (0.82) | (0.40) |
| | | | | | | |
| **2SLS** | | | | | | |
| $\frac{\Delta wage}{\Delta transfer(\tau^e)}$ | -0.041** | -0.039** | -0.003 | -0.067*** | -0.092*** | -0.036* |
| | (0.020) | (0.017) | (0.019) | (0.017) | (0.030) | (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 7 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%.

Table 5: 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).

# 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]

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[1]Workers 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.

[2]To 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.

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

[4]When 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.

[5]See Decree 1667/2012.

[6]In 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

Besides the AAFF 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 AAFF 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.

# 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 the most severe depressions in its history including institutional, political,

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

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

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

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 lootings, 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 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.16 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.15 for an example of a CBA). In principle, these agreements could take place at any moment within the year, as shown in Figure I.17.

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.18, where we document the

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[9]An intriguing aspect of inflation, is its tendency to prompt more frequent updates of prices and wages, as illustrated in Figure 7a, 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.

monthly evolution of economic activity. In a similar vein, private employment grew steadily until mid-2008, and stabilized afterwards (see Figure I.19).[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.20, 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.[11]

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

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

[11]This brief classification is largely based on Roddis & Tzannatos (1999).

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 A5, we present a non-exhaustive list of similar schemes, several of which continue to operate.[12]

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.[13] 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 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).

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[12]Importantly, note that here we are not referring to the well-known conditional cash transfers, CCT, (e.g., *Bolsa Familia, Chile Solidario, Juntos and Tekopora*, for the Latin American countries listed in the table.
[13]There are also other common features in the region such as the contributory scheme and the *notched* structure.

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 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, i.e., 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).[14] Figure I.21 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.[15] The 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.[16]

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[14]There were nearly 300 UDAIs located throughout the country.

[15]Both duration references were extracted from an audit of the SUAF incorporation made by the AGN (*Auditoría General de la Nación*).

[16]The 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

Figure I.22 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).[17]

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.23. 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*).[18]

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.[19] 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 commited 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, 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.24 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 beneficia-

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the SFC and under the SUAF, for a given month.

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

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

[19]We found that only 0.001 percent of the employers appeared in more that one resolution.

ries 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.[20] 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 memo, they cannot compensate the money of the transfers they disburse.[21]

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[20]Nevertheless, we test the sensitivity of our wage effects using different estimation samples as shown in Figure I.10.

[21]For instance, see article 3 in Figure I.22 panel (a) (ARTICULO 3: *...employers will not be able to compensate the family allowances paid to their workers, as of the period...*).

# E   Incidence model with misperception of benefits

In this section we discuss the derivation of the simple model that we propose. As already mentioned, the best way to reconcile the setting with the results that we document is to incorporate a partial perception of benefits. Based on Gruber (1997), we put forward the following specification:

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

where $\tilde{w}$ represents the perceived wage as a function of the wage ($w$), a perception parameter ($q$) and the transfer rate disbursed by employers ($\tau^e$). In addition, we define $\tau^e = \bar{\tau} - \tau^g$, where $\tau^g$ 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 is equal to 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 $\tilde{w}_0 = w(1 + \tau^e)$.

We then express the labor supply function as follows:

$$L_i^s = L_i^s(\tilde{w}_i) = L_i^s(w_i \cdot (1 + (1 - q) \cdot \tau_i^e)) \text{ with } i = 1, ... n \tag{2}$$

and the corresponding labor supply elasticity as:

$$\eta_i^s = \frac{dln(L_i^s)}{dln(\tilde{w}_i)} = \frac{l_i^s}{L_i^s} \cdot w_i \cdot (1 + (1 - q) \cdot \tau_i^e) \tag{3}$$

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:

$$L_i^d = L_i^d(w) \text{ with } i = 1, ... n \tag{4}$$

and labor demand elasticity as:

$$\eta_i^d = \frac{dln(L_i^d)}{dln(w_i)} = \frac{l_i^d}{L_i^d} \cdot w_i \tag{5}$$

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

$$dln(L_i^s) = \eta_i^s \cdot [dln(w_i) + dln(1 + (1-q) \cdot \tau_i^e)] \tag{6}$$

and

$$dln(L_i^d) = \eta_i^d \cdot [dln(w_i)] \tag{7}$$

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

$$\frac{dln(w_i)}{dln(1 + \tau_i^e)}\bigg|_{\bar{\tau} = \tau^e + \tau^g, \, \bar{q} = q} = \frac{\eta_i^s \cdot (1-q) \cdot \left[\frac{(1+\tau_i^e)}{(1+(1-q)\cdot\tau_i^e)}\right]}{\eta_i^d - \eta_i^s} \tag{8}$$

**Incidence predictions**: To keep things simple, we can reduce the previous equation and focus on two polar cases. Subsequently, we identify the following situations:

- $q = 1 \longrightarrow$ perfect knowledge, then we have $\frac{dln(w_i)}{dln(1+\tau_i^e)} = 0$ 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.

- $q = 0 \longrightarrow$ situation with no knowledge or complete confusion about the scheme. In this case we have $\frac{dln(w_i)}{dln(1+\tau_i^e)} = \frac{\eta_i^s}{\eta_i^d - \eta_i^s} < 0$ 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 imposing the new payment system eligible workers indirectly update their beliefs or perception about the overall AAFF scheme. Consequently, we repeated the derivation but allowing $q$ to change as a result of the change in the transfer disbursed by employers ($\tau^e$). We obtain the following expression:

$$\frac{dln(w_i)}{dln(1 + \tau_i^e)}\bigg|_{\bar{\tau} = \tau^e + \tau^g} = \frac{(1 + \eta_i^{(1-q)}) \cdot \eta_i^s \cdot (1-q) \cdot \left[\frac{(1+\tau_i^e)}{(1+(1-q)\cdot\tau_i^e)}\right]}{\eta_i^d - \eta_i^s} \tag{9}$$

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 to say, $\eta_i^{(1-q)}$ 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 ($\tau^e$), there is an increase in confusion which means decreases in ($q$) and, naturally, an increase in $(1-q)$. This positive elasticity reinforces the main effect that we previously derived.

## E.1 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 5). 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.

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

(a) Perfect awareness and knowledge (**q=1**)   (b) Situation with no knowledge (**q=0**)
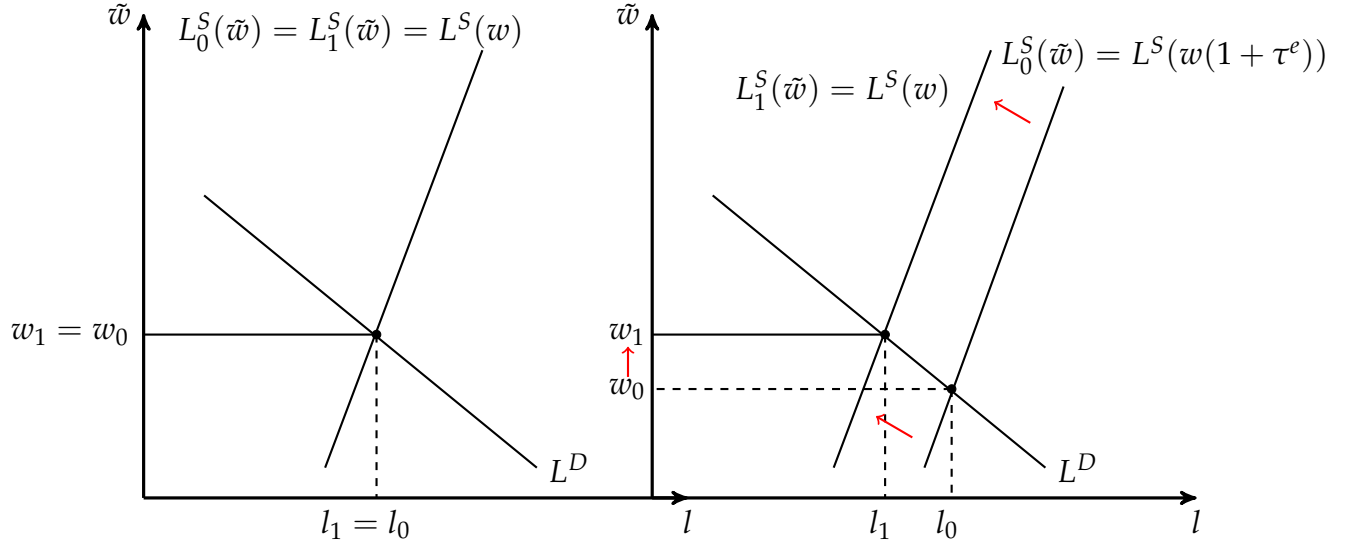


**Under the new regime, employers stop disbursing the transfer $\Rightarrow$ labor demand curve may shift upward or inward.**

1. 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 $L_0^D$ to $L_1^D$. 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.28).

2. 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
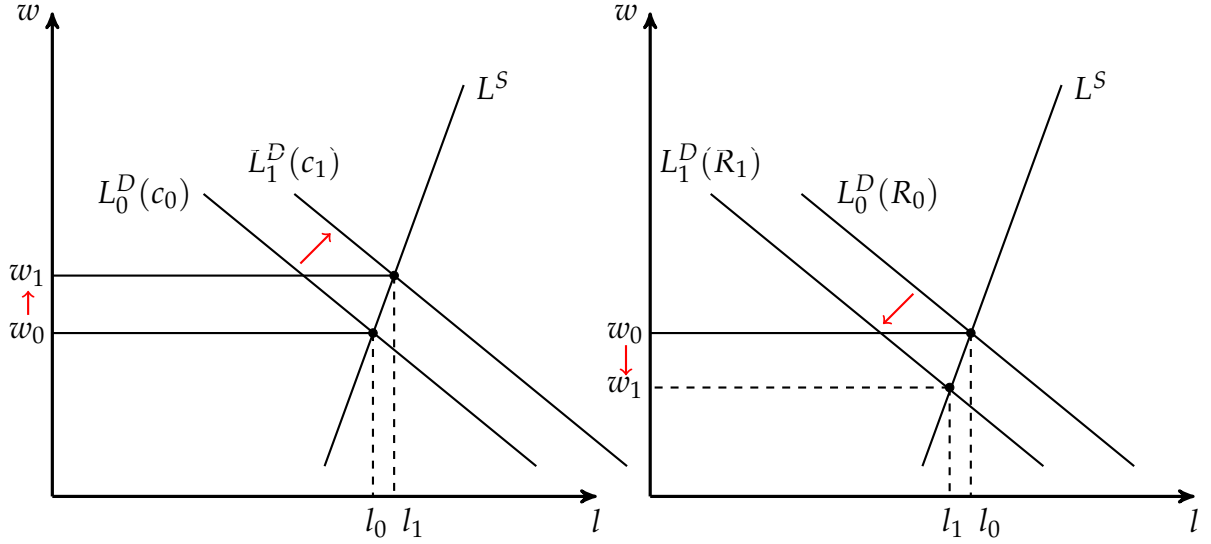


# F   Econometric specification

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:

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

where $T$ refers to workers belonging to the treatment group, $Post$ to the period after the event, and $\mu_t$ 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

$$w_{i,f,t} = \beta_0 \cdot T_{i,f,t} + \beta_1 \cdot T_{i,f,t} \cdot Post_{i,f,t} + \mu_{ft} + \epsilon_{i,f,t} \tag{2}$$

where $\mu_{ft}$ 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:

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

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).

$$\bar{w}_{g,f,t} = \beta \cdot T_{g,f,t} + \sum_{j=-13}^{12} \gamma_j \cdot d_{f,t}^j \cdot T_{g,f,t} + \mu_{f,t} + \epsilon_{g,f,t}{}^{22} \tag{4}$$

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).

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

The $\gamma$'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.[23]

In order to compute the reduced-form point estimates and sum up our results, we simply pool all the gamma coefficients before ($G_{before}^{\bar{w}} = (\gamma_{-12} + \gamma_{-11} + ... + \gamma_{-3} + \gamma_{-2} + 0)/12$) and after ($G_{after}^{\bar{w}} = (\gamma_0 + \gamma_1 + \gamma_2 + ... + \gamma_{10} + \gamma_{11})/12$) 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:

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

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[22]Alternatively, 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.

[23]The 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 scenarious:

- $E(G^{\bar{w}} / Window = 0, post = 0) = \alpha$

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

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

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

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:
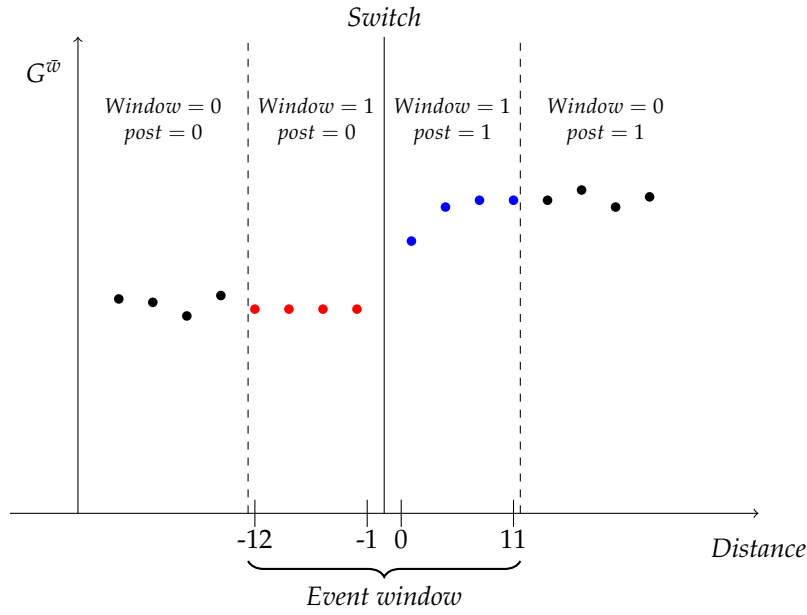


Similarly, the first-stage point estimate will be recovered as follows

$$G_{f,t}^{\overline{Transfer}} = \alpha + \delta_1 \cdot Window_{f,t} + \delta_2 \cdot Window_{f,t} \cdot Post_{f,t}$$

$$+\delta_3 \cdot (1 - Window_{f,t}) \cdot Post_{f,t} + \epsilon_{f,t}$$

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

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changing over time and to control for time-specific trends, respectively.

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

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

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

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

# 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.10 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 where 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 the 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'Haultfœuille, 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'Haultfœuille, 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'Haultfœuille (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.[24] 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.12. 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.

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[24]The 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.[25]

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.[26] 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 I.25 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 one hundred percent on workers. Below, we discuss why this result does not contradict the main findings of our 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

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

[26]We 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 $\gamma_j$ 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 $\eta_{ft}$ 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.26 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 $\gamma_j$, 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 $\eta_{ft}$.

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 9 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.26 summarizes the FHS strategy. It presents event-study estimates of the coefficients $\gamma_j$ 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 $\eta_{ft}$ in panel (a).

The geometry of these plots suggests an instrumental variables setup, in which panel (a) of Figure I.26 plots the reduced form for the outcome and panel (b) plots the first stage. Indeed, FHS show that $\gamma_j$ 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 $\gamma_j$ for an additional $j$. In the figures, we set $\gamma_{-5} = 0$.

Panel (d) of Figure I.26 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 $\gamma_j$ 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.27 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.27 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.26 and I.27) are displayed in Figure 11 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,[27] 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.[28]

We present the event study result in Figure I.28. The figure shows no clear effect of switching on financial distress. We get the same results if instead we re-define risky debtors as those with payment delay of more than 180 days. If we were to break down the analysis by firm size, we would 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 3). This salience could, in principle, affect the collusion between employers and workers to extract rents from the transfer scheme, in the spirit of Doornik *et al.* (2018) (they identify strategic behavior in claiming unemployment

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[27]The 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.

[28]The 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.

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.29 (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.29 (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.30, 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: Macro and micro aggregates comparison
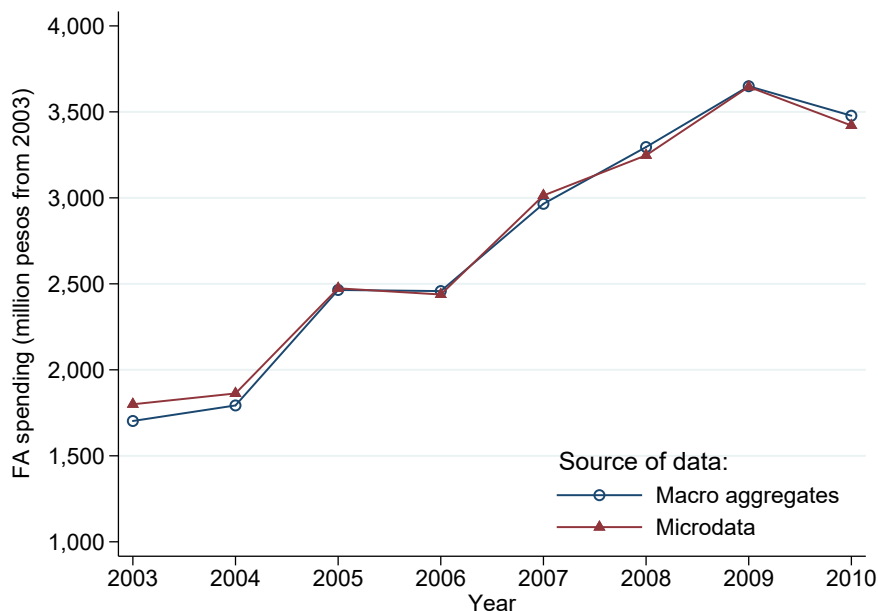


*Notes*: This figure shows the total expenditure on family allowances in real terms (old and new system). The blue connected dots present the macro total available in official budget information (data extracted from *Cuenta de Inversion, Contaduría General de la Nacíon* and *Informe Gerencial* (AFIP)) 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.2: Beneficiaries (number of children)
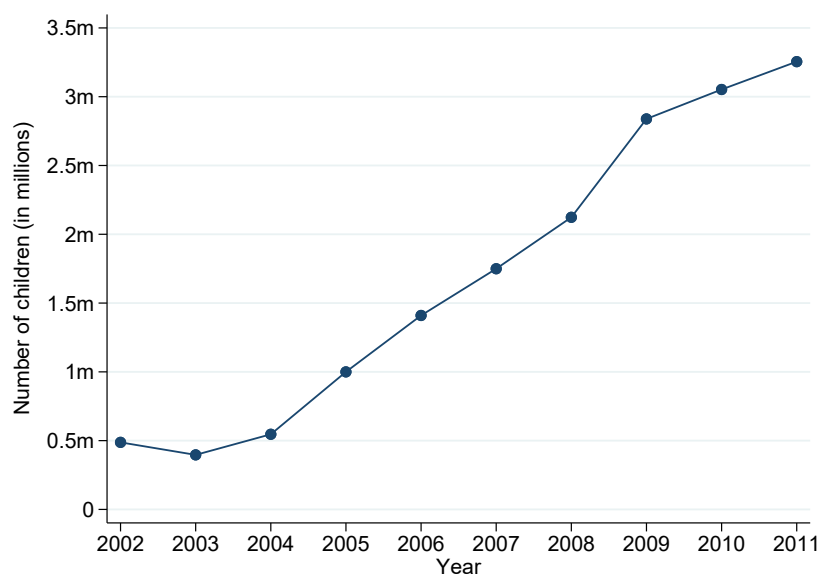


*Notes*: This figure shows the number of children receiving the child benefit between 2002 and 2011. Reassuringly, the number does not decrease during the transition from the old to the new system. The sharp increase could be due to the fact that the economy was booming and there was a formalization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

## Figure I.3: Macro roll-out (official budget information)
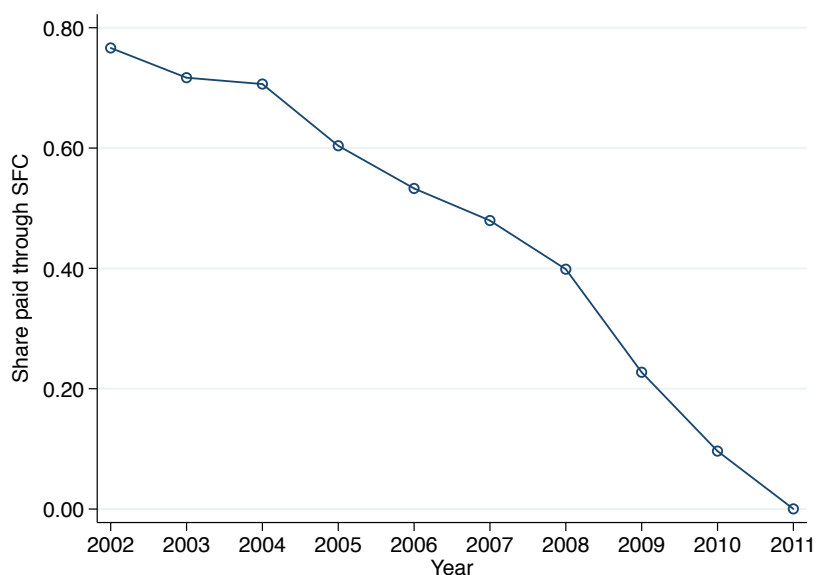


*Notes*: This figure shows the share of family allowances paid under the old system (SFC). The aggregate expenditure on family allowances is taken from official budget information (*Cuenta de Inversion, Contaduría General de la Nacíon* and *Informe Gerencial* (AFIP)). The gradual decline in this share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

## Figure I.4: Event frequencies per month-year (number of firms)

### (a) Full period 2003-2010
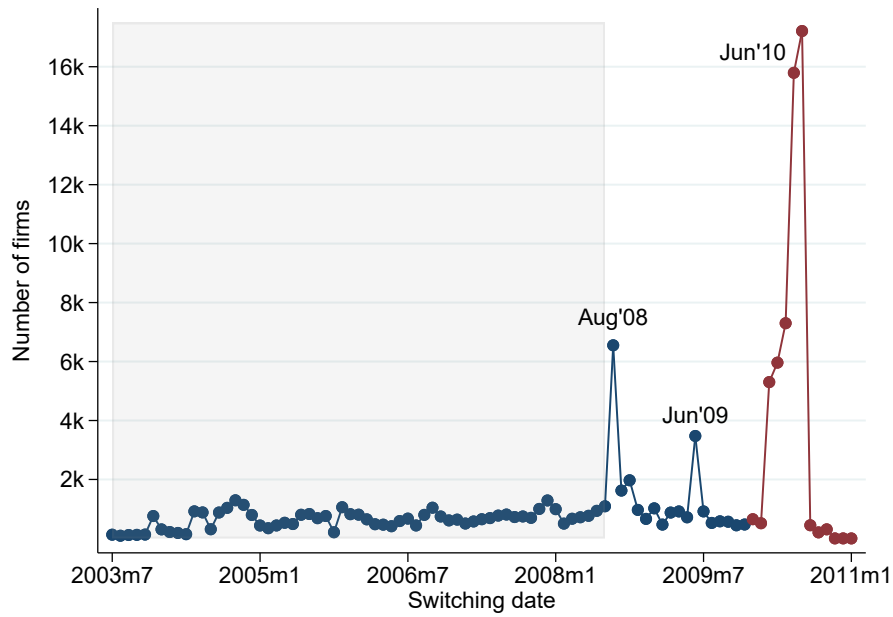


### (b) Zoom in before 2010
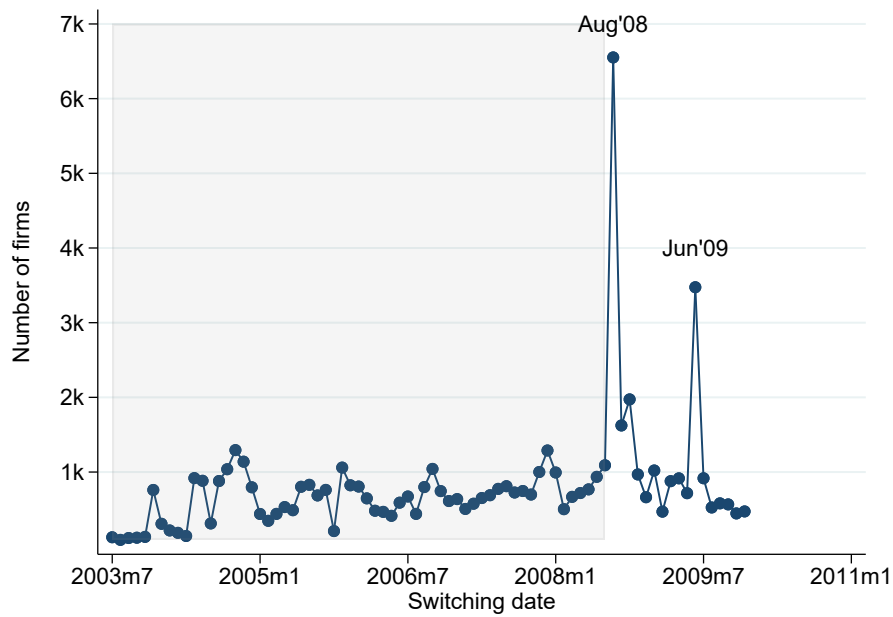


*Notes*: These figures show the number of firms switching to the new system at each month-year of our micro-data. Panel (a) shows the full period from 2003 to 2010 and panel (b) restricts the graph to pre-2010 data to provide a clearer picture. The spikes correspond to three massive incorporation dates: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

Figure I.5: Balanced panel of firms present in the 96 months of data
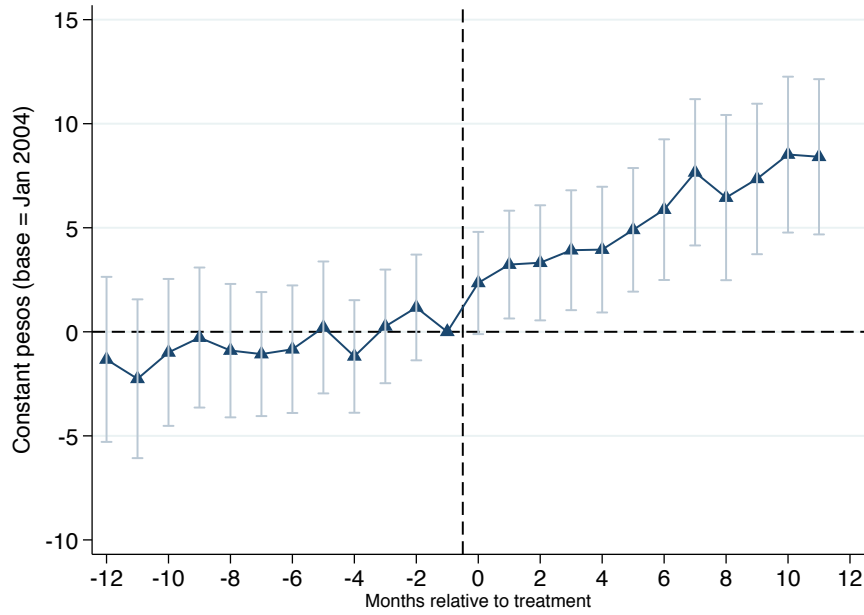


*Notes*: This figure plots the event-study coefficients and 95-percent confidence intervals of equation (2). It shows that results remain unchanged when considering a balanced panel of firms present in the 96 months of data.

Figure I.6: Sensitivity to months of transfer payments before the event
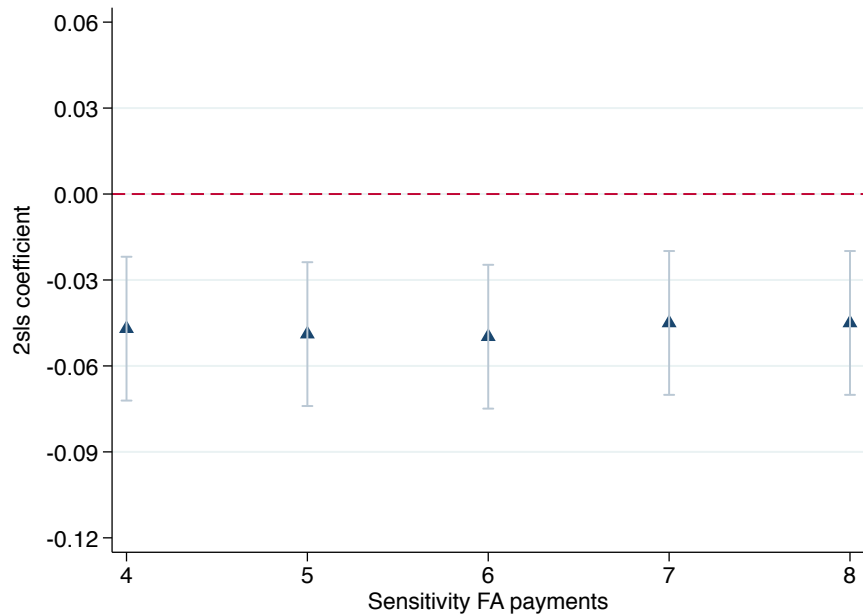


*Notes*: Each dot in this figure corresponds to a different reduced-form coefficient of equation (3) scaled by the first-stage change in the remittance of benefits, where we vary the sample of firms according to the number of months that each firm was paying family allowances (FA) right before the event.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

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Figure I.7: Sensitivity to the length of the event window
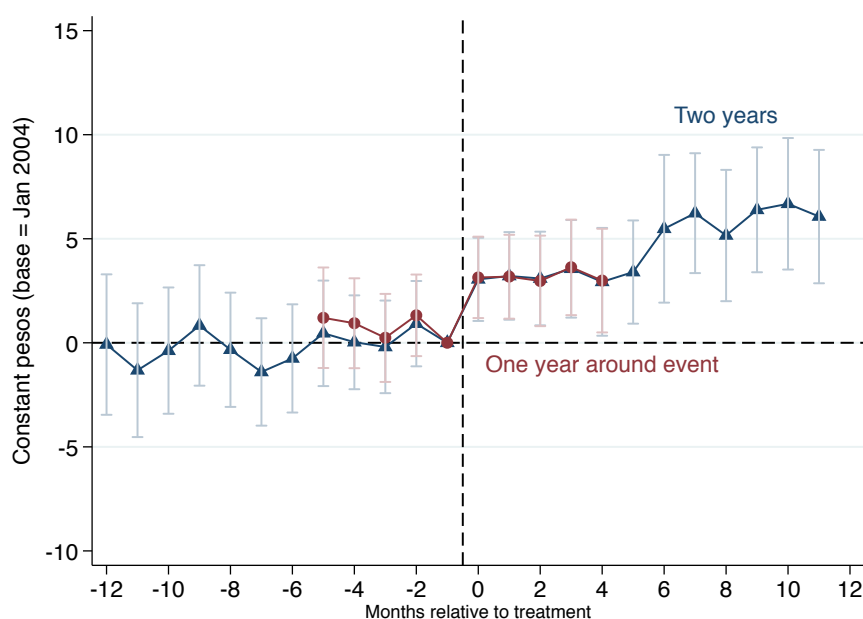


*Notes*: This figure plots the event-study coefficients and 95-percent confidence intervals of equation (2). The figure shows that results remain unchanged when considering a time window of 6 months before and after the event (red line) instead of 12 months (blue line).

Figure I.8: Alternative treatment group definition (always treated workers)
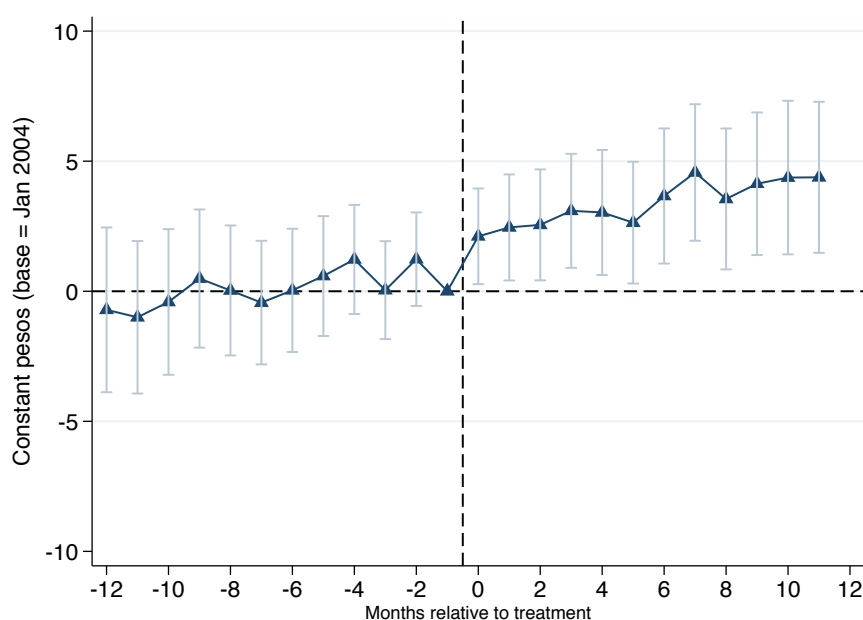


*Notes*: This figure plots the event-study coefficients and 95-percent confidence intervals of equation (2). It shows that results remain unchanged when using an alternative definition of the treatment group that considers workers who are fully treated during the period 2003-2010, i.e., those with children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

Figure I.9: Wage effects under alternative specifications with controls
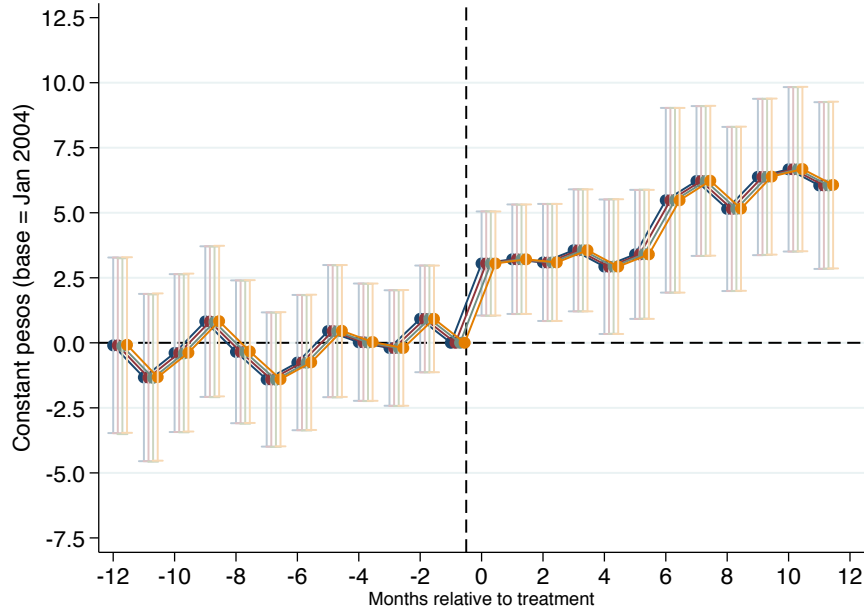


*Notes*: This figure plots the event-study estimates of parameter $\gamma$ and its corresponding 95-percent confidence intervals for different specifications with and without controls. The blue line corresponds to our baseline estimate of equation (2). In the other series, we include controls such as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

Figure I.10: Wage effects using alternative samples
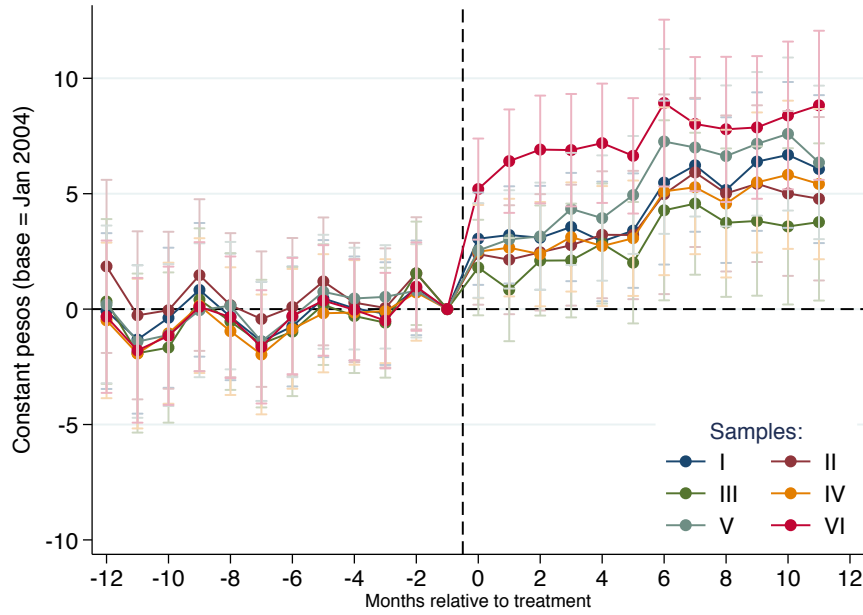


*Notes*: This figure plots the event-study estimates of parameter $\gamma$ and its corresponding 95-percent confidence intervals of equation (2) for different subsamples: (I) baseline sample (includes firms with more than one worker receiving the transfer at $t-1$), (II) adds to (I) the restriction of 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.11: Wage effects including never-treated firms
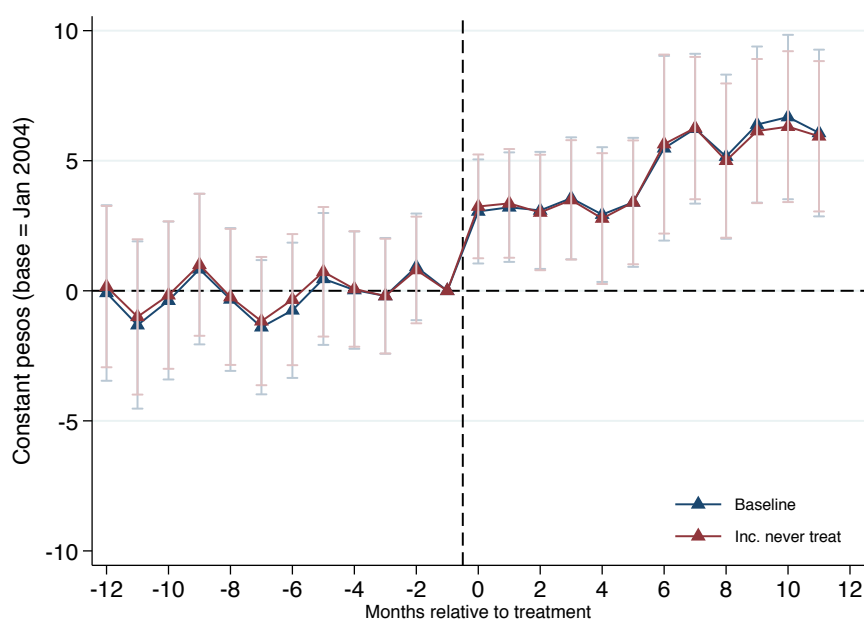


*Notes*: This figure plots the event-study estimates of parameter $\gamma$ and its corresponding 95-percent confidence intervals of equation (2) for two different subsamples. The blue series refers to our baseline specification while the red series adds never-treated firms. In our setting, untreated 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 5 and its corresponding footnote.

Figure I.12: Wage effects using heterogeneity-robust methods
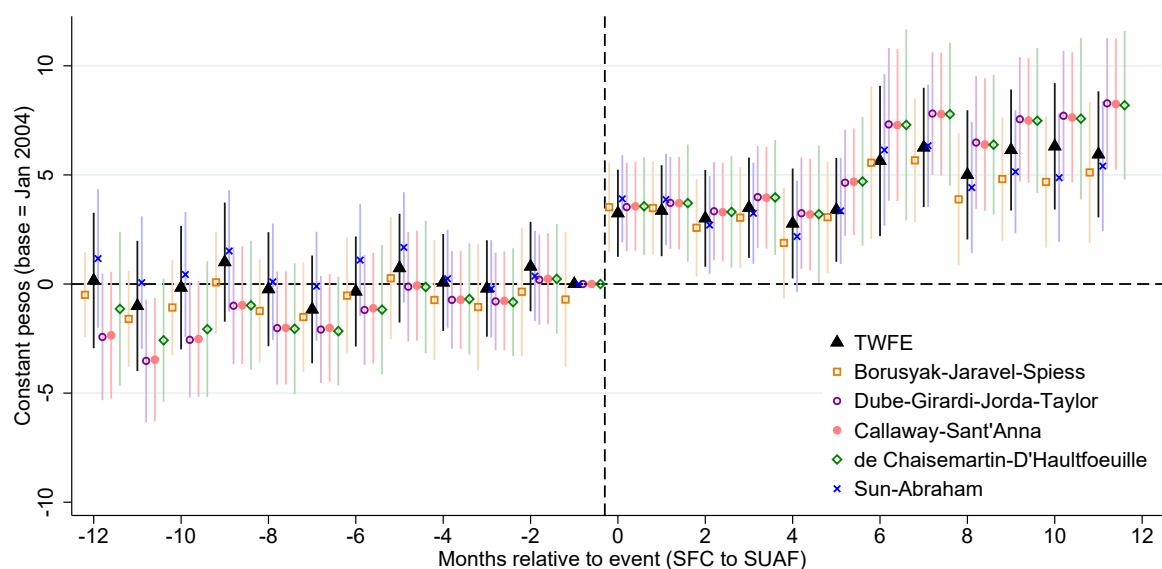
*Notes*: This figure plots the event-study coefficients estimated with five heterogeneity-robust difference-in-differences methods and the conventional TWFE regression. The dependent variable is the within-firm average wage gap between workers with children (treatment) and without children (control). 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'Haultfœuille (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.13: Placebo test using fake event dates (wage effects)
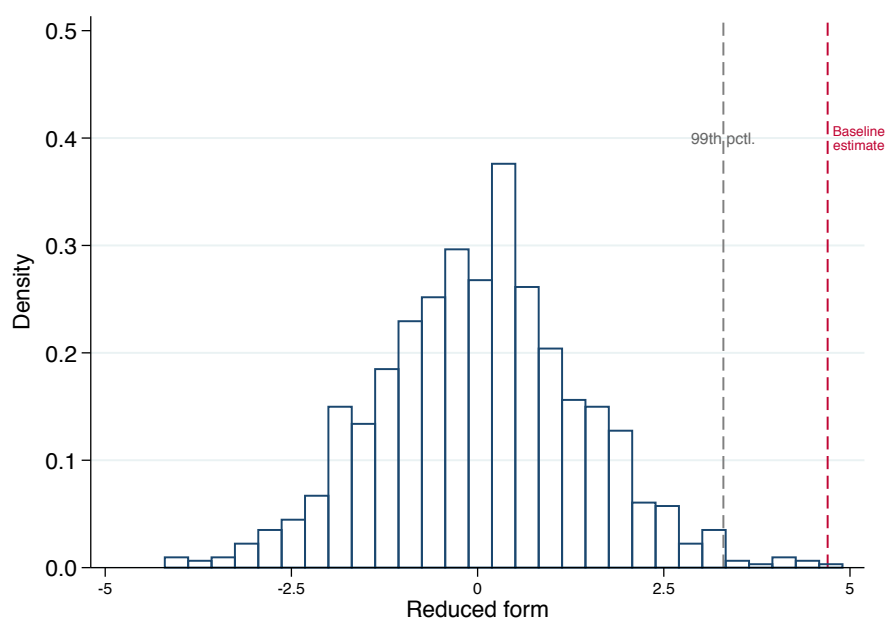


*Notes*: This figure plots the distribution of reduced-form coefficients of equation (3), where each of these coefficients is the result of assigning a fake event date to each firm and then re-estimating the wage-effects. For this figure, we focus on firms that have both types of workers throughout the 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.14: Pass-through across sectors
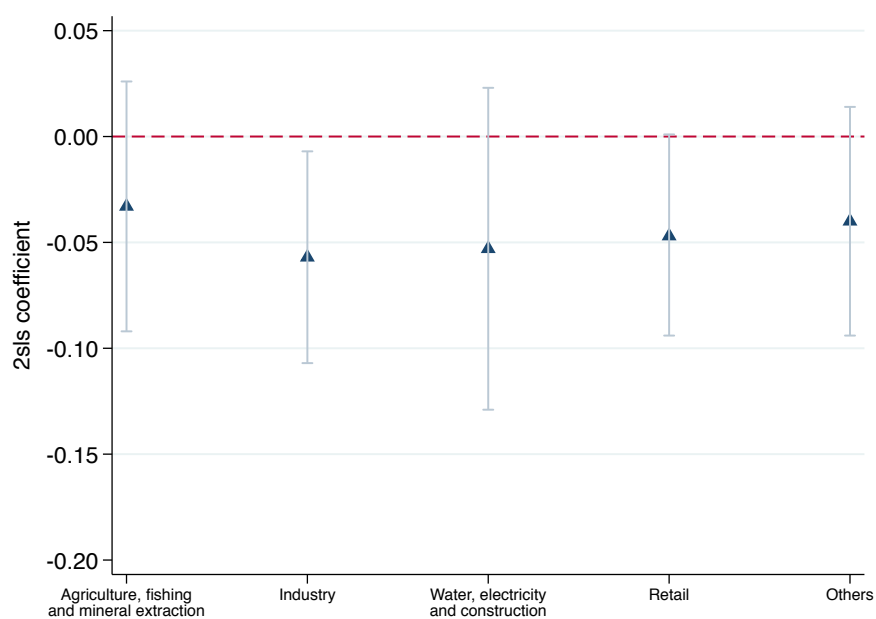


*Notes*: Each dot in this figure corresponds to a different reduced-form coefficient of equation (3) scaled by the first-stage change in the remittance of benefits; each dot corresponds to a separate regression of a given sector. We identify the following sectors: [a] Agriculture, fishing and mineral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

Figure I.15: Collective agreement

(a) **Example of a collective agreement**
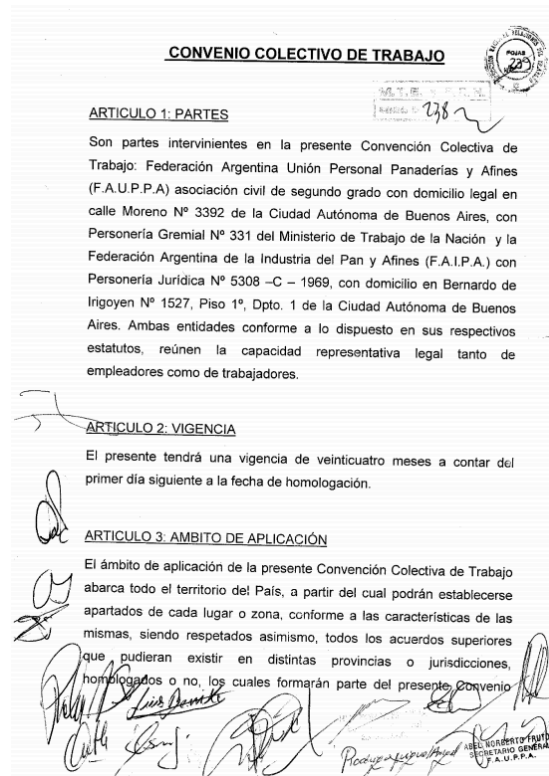


(b) **Summary of a collective agreement**



*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 $29^{th}$ 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.).
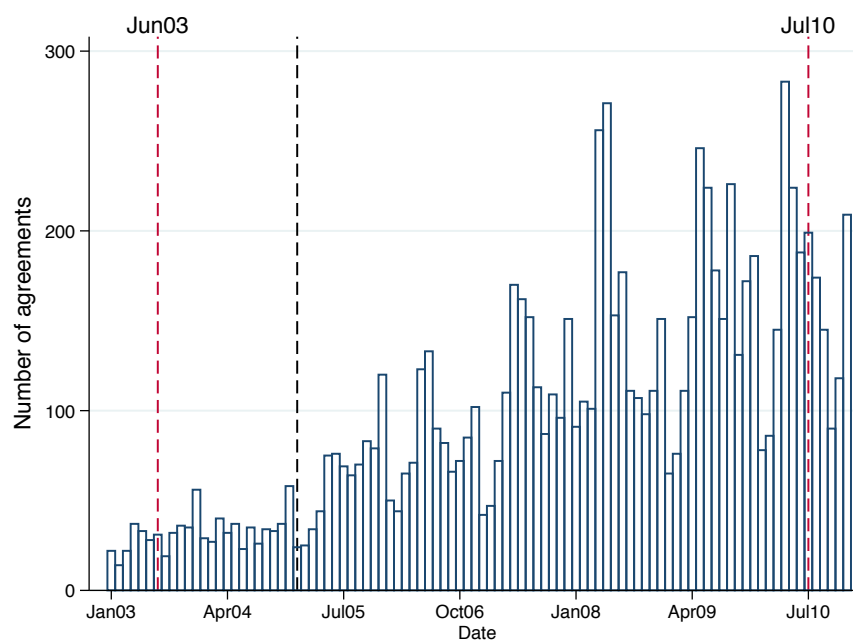
Figure I.16: Monthly evolution of inflation and nominal wages (2003-2010)
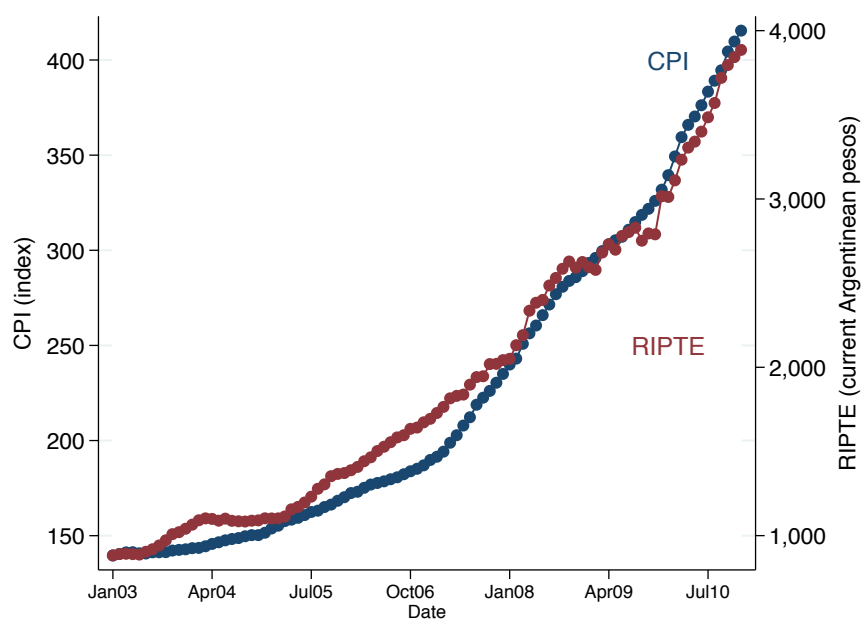


*Notes*: CPI denotes consumer price index while RIPTE denotes the average salary of registered workers (in current pesos).
*Source*: Ministry of Labor, Argentina.

Figure I.17: 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.18: Monthly evolution of the economic activity estimator (2004-2010)
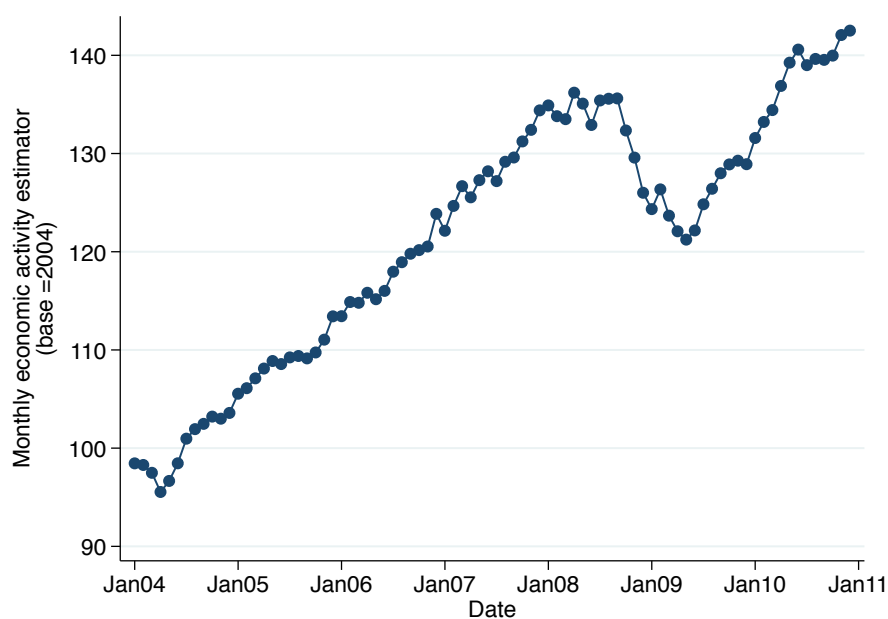


*Notes*: The vertical axis measures the monthly economic activity indicator as a function of time. We observe a large drop in economic activity from August 2008 onwards.
*Source*: National Statistical Office of Argentina, *Instituto Nacional de Estadíistica y Censos* (INDEC).

Figure I.19: Quarterly evolution of private employment (2003-2010)
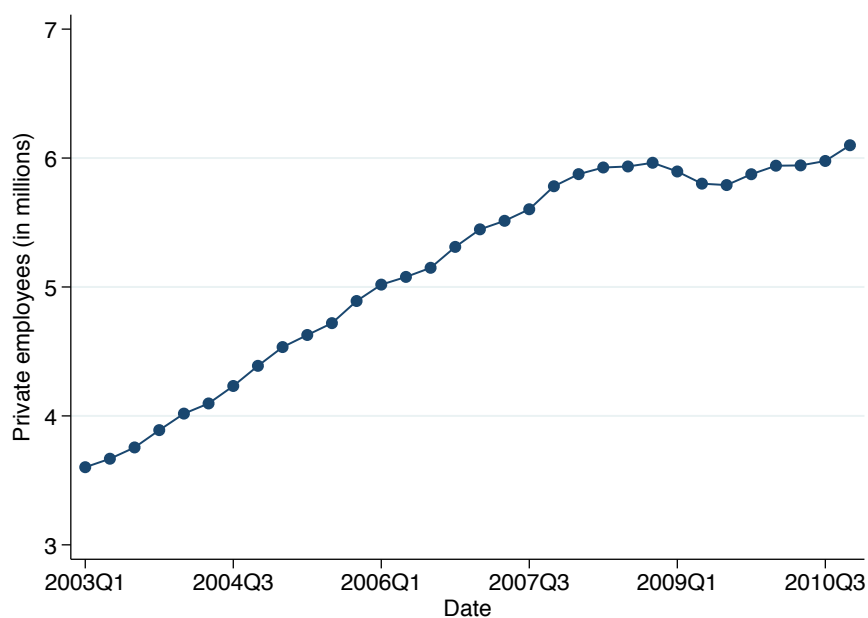


*Notes*: The figure presents the evolution of registered private wage employees for the years 2003-2010. The period is characterized by a steady increase in the number of registered workers followed by stabilization of employment since the third quarter of 2008.
*Source*: Ministry of Labor, Argentina.

Figure I.20: Evolution of the average tax rate of the family allowance (2003-2010)
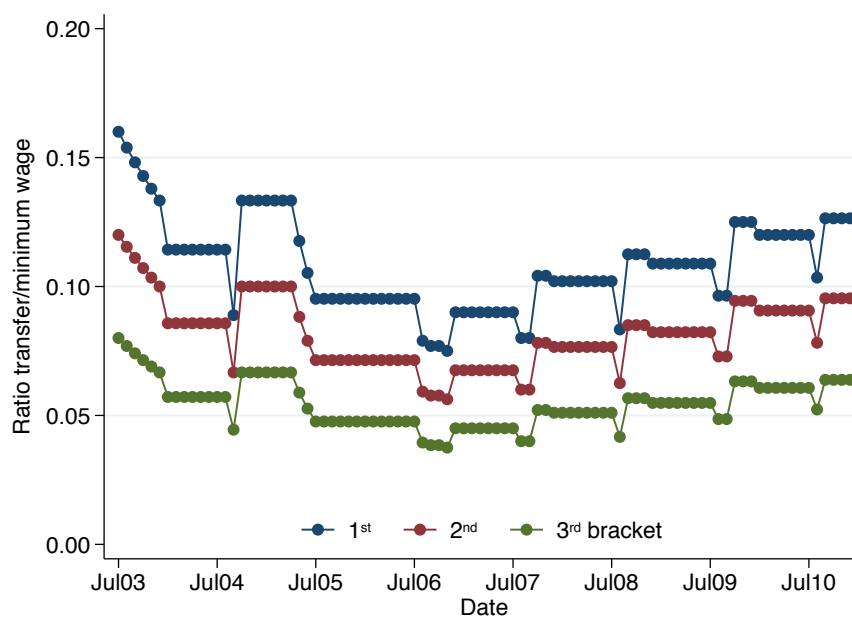


*Notes*: The vertical axis presents a proxy for the average tax rate (ATR), i.e., the ratio of the transfer normalized by the minimum wage, for three transfer amounts. The figure shows that the ATR remains roughly constant during the period of analysis.
Source: Author's elaboration based on official documentation.

Figure I.21: Incorporation schedule memo

(a) Resolution (body text)

*Resolución Nº 333/2005*[1]

*Cronograma de inclusión de empleadores al Sistema Único de Asignaciones Familiares. A.N.Se.S.*

**Artículo 1°—** Apruébase el cronograma de inclusión al Sistema Único de Asignaciones Familiares (S.U.A.F.) respecto de los empleadores que se encuentran detallados en el Anexo que forma parte integrante de la presente, y que obligatoriamente serán incluidos formalmente al Sistema Único de Asignaciones Familiares en forma paulatina hasta el mes devengado diciembre de 2005.

**Artículo 2°—** La Gerencia de Prestaciones notificará fehacientemente a los empleadores referenciados en el artículo 1° de la presente sobre los requisitos que deberán cumplir y la documentación que deberán presentar ante la Unidad de Atención Integral/Área Central de esta Administración Nacional de la Seguridad Social, a los efectos de quedar incluidos formalmente en el Sistema Único de Asignaciones Familiares.

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

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S ANSES
Ministerio de Trabajo, Empleo y Seguridad Social

"2005 – AÑO DE HOMENAJE A ANTONIO BERNI"

ANEXO

| CUIT | RAZON SOCIAL |
|---|---|
| 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.22: Incorporation memo

### (a) Resolution (body text)

Resolución Nº 456/2006[1]

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

**Artículo 1°—** Incorpóranse 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

| CUIT | Razón Social | U.D.A.I. |
|---|---|---|
| 30–963840333–8 | Agroexport S.A. | U.D.A.I. San Juan |

### (b) Resolution appendix (with employer identifiers)

2005 – AÑO DE HOMENAJE A ANTONIO BERNI

ANSES 000647

Ministerio de Trabajo, Empleo y Seguridad Social

ANEXO

| CUIT | RAZON SOCIAL | UDAI |
|---|---|---|
| 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 |
| 30515923329 | 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 |
| 30608964076 | 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 | GOYAIKE S A A C I Y F | OFICINA ESCOBAR |
| 30653466931 | GNC ESCOBAR SA | OFICINA ESCOBAR |
| 30580736528 | BEST PAINT S A | OFICINA ESCOBAR |
| 30559721502 | PRENSADORA MURO 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.23: Website query



*Notes*: This is a screenshot of a public query where it is possible to check whether a given employer (CUIT) is already in the SUAF. After introducing the CUIT and the security code, the site reports the firm's name (*Razón Social*), whether the firm 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*). The official website can be accessed at ANSES website.

## Figure I.24: Event accuracy and formal incorporation date

### (a) All firms



### (b) By number of beneficiaries at $t_{-1}$
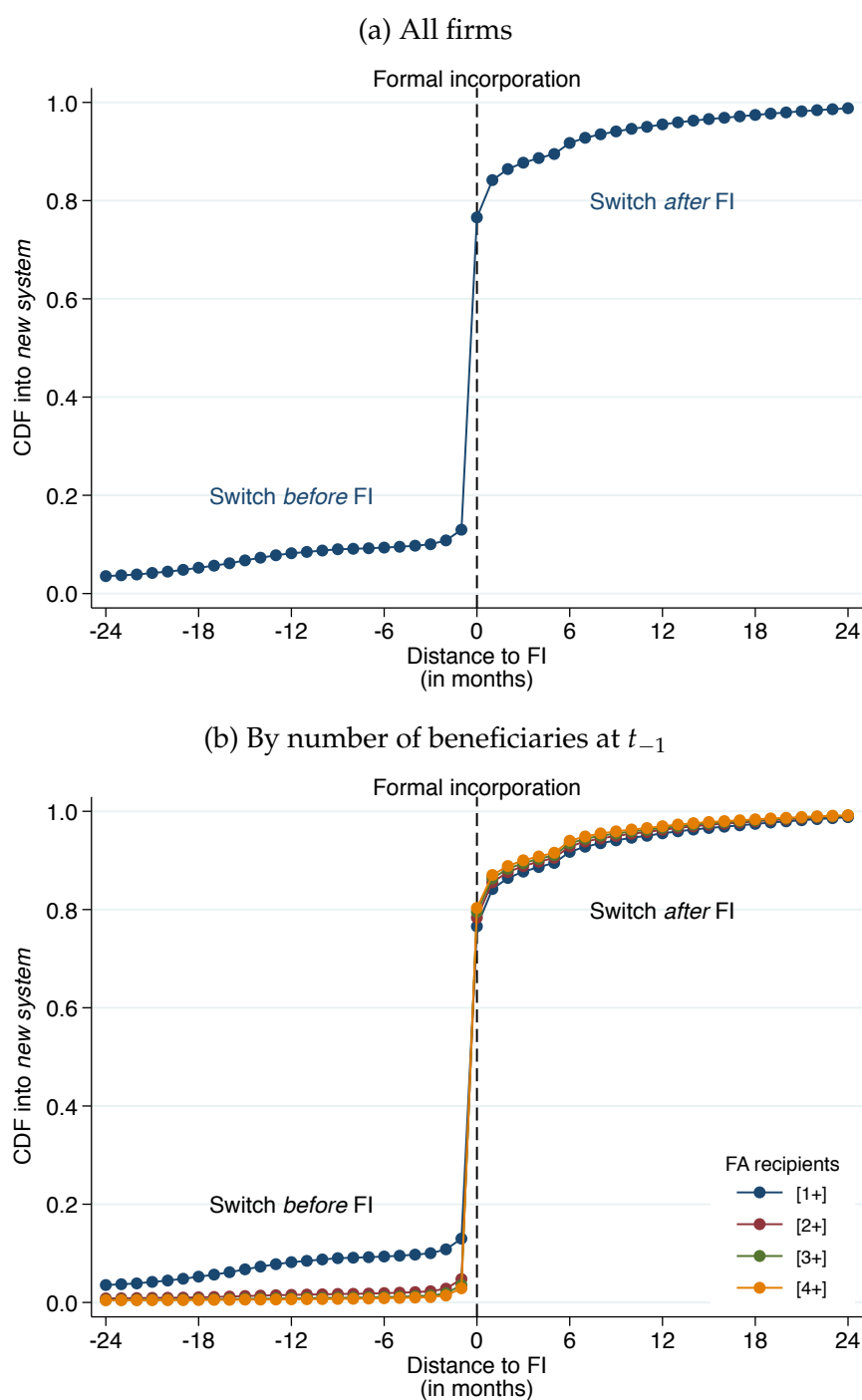


*Notes*: The vertical axis of these figures contains the cumulative density function (*CDF*) of firms incorporated into the new system as a function to the distance (in months) to the formal incorporation date (FI). Panel (a) includes all firms while in panel (b) we break down the *CDF* by the number 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.25: Turning 18, becoming ineligible (individual-level shock)
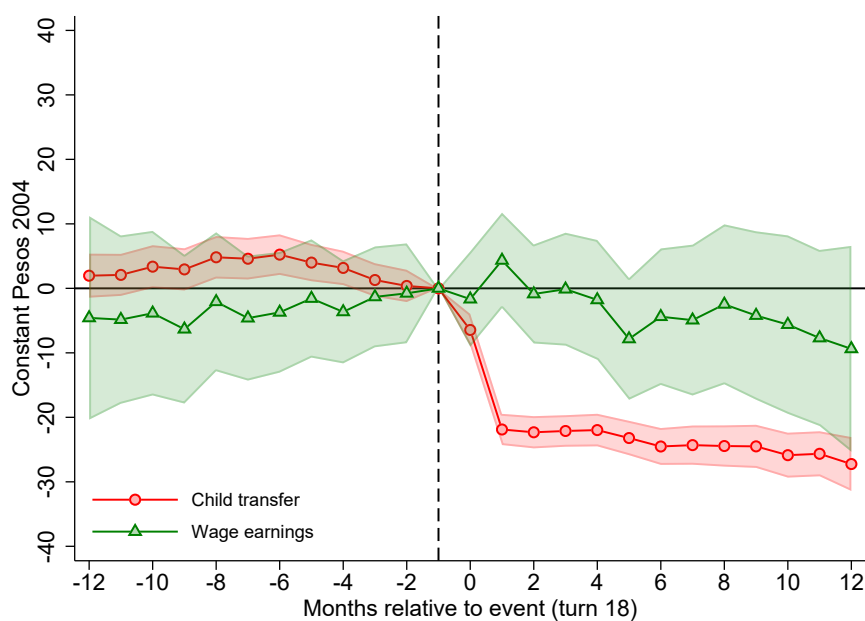
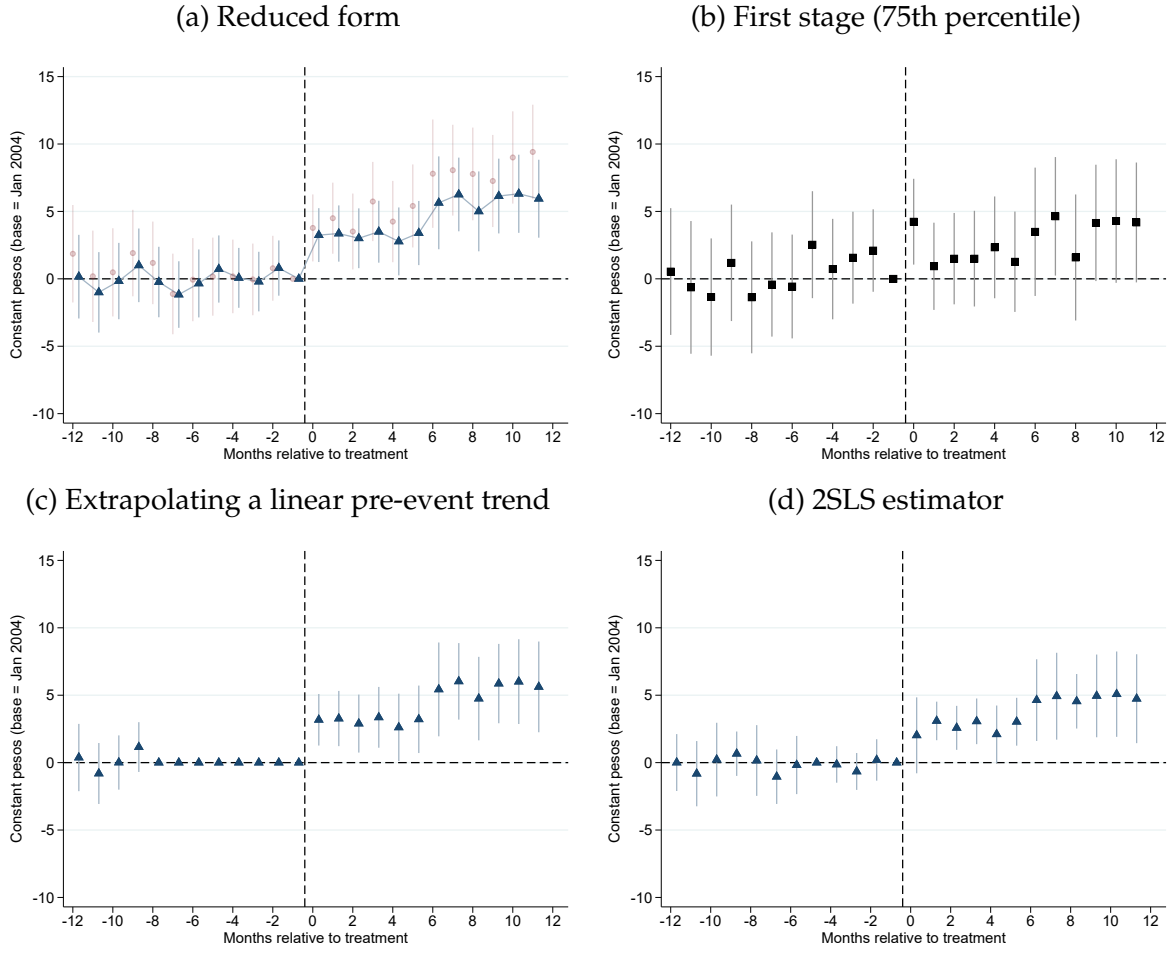*Notes*: This figure plots the event-study estimates of parameter $\gamma$ and its corresponding 95-percent confidence intervals of equation (2). The event, in this exercise, refers to having a child that turns 18 years old in a given month. In one series, we plot the gap in transfer (first stage), while in the other we plot the evolution of the wage gap (reduced form) around the event.

# Figure I.26: Wage effects for incumbent workers
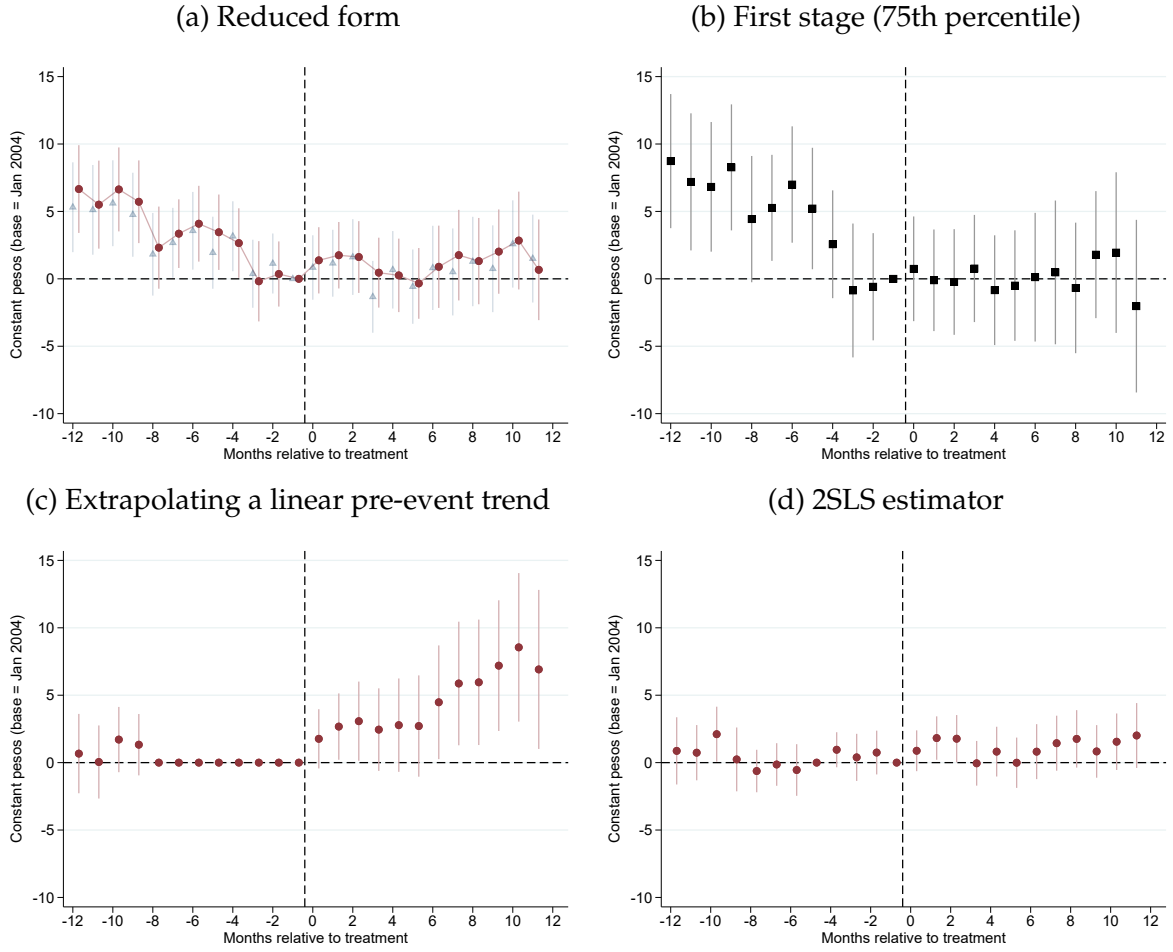
### (a) Reduced form



### (b) First stage (75th percentile)



### (c) Extrapolating a linear pre-event trend



### (d) 2SLS estimator



*Notes*: This figure plots the event-study estimates of parameter $\gamma$ 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 $\eta_{ft}$ 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 $\gamma_j$ 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.27: Wage effects pooling incumbents and new hires together

(a) Reduced form

(b) First stage (75th percentile)

(c) Extrapolating a linear pre-event trend
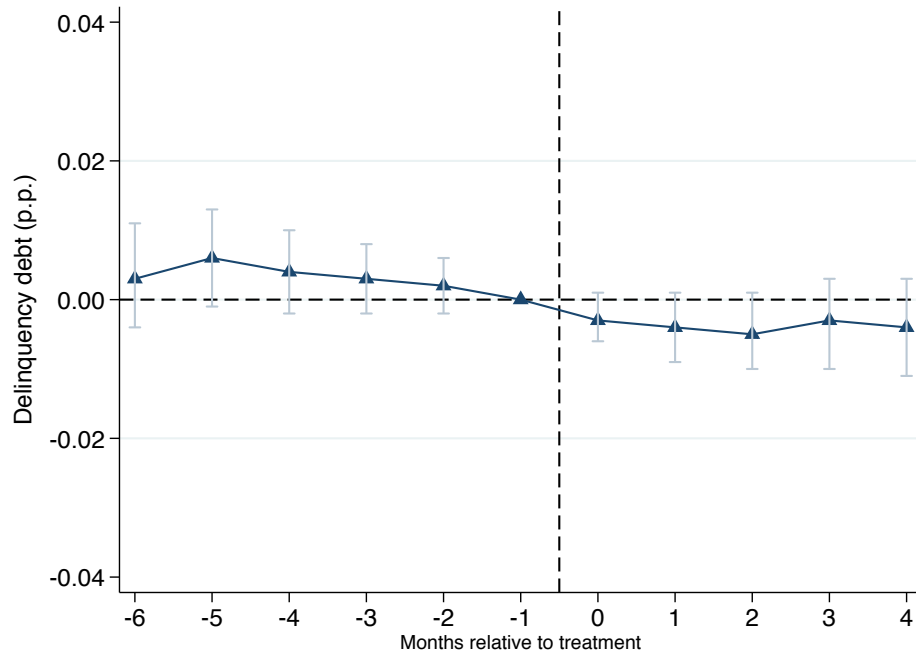
(d) 2SLS estimator



*Notes*: This figure plots the event-study estimates of parameter $\gamma$ 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 $\eta_{ft}$ in panel (a). Panel (d) corresponds to a 2SLS regression of the average wage gap on the policy (event) indicators $d^j_{f,t}$ and $x_{ft}$, using the closest lead of the event $d^{-1}_{f,t}$ as an excluded instrument for $x_{ft}$. Using $d^{-1}_{f,t}$ as an instrument means that we need to normalize $\gamma_j$ 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.28: Delinquency rates



*Notes*: This figure plots the event-study estimates of parameter $\gamma$ and its corresponding 95-percent confidence intervals of equation (2) considering a one-year period. 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.

Figure I.29: Bunching as a collusion response

(a) Gross wage and average tax rate
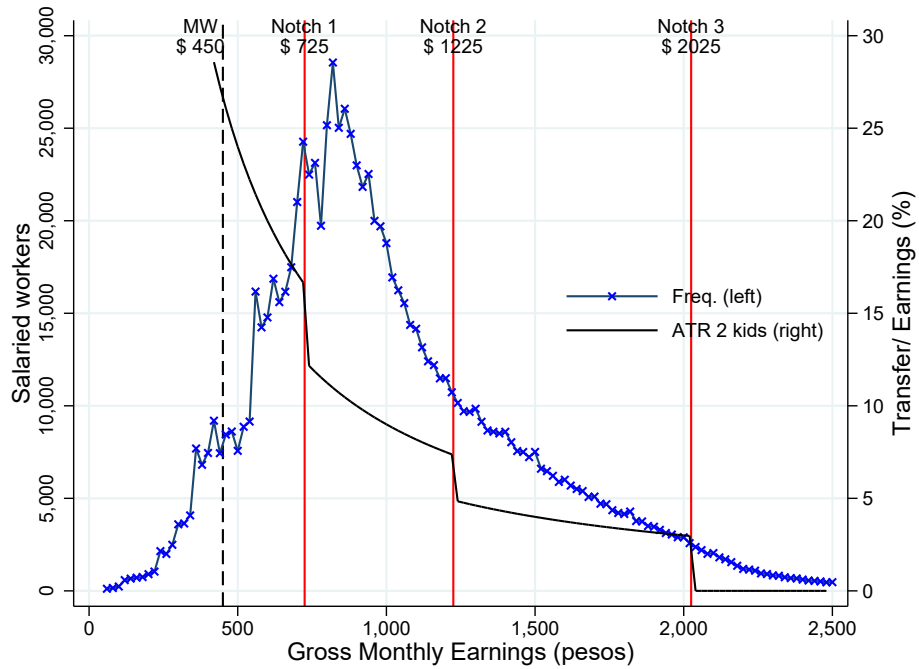


(b) Distribution by number of kids
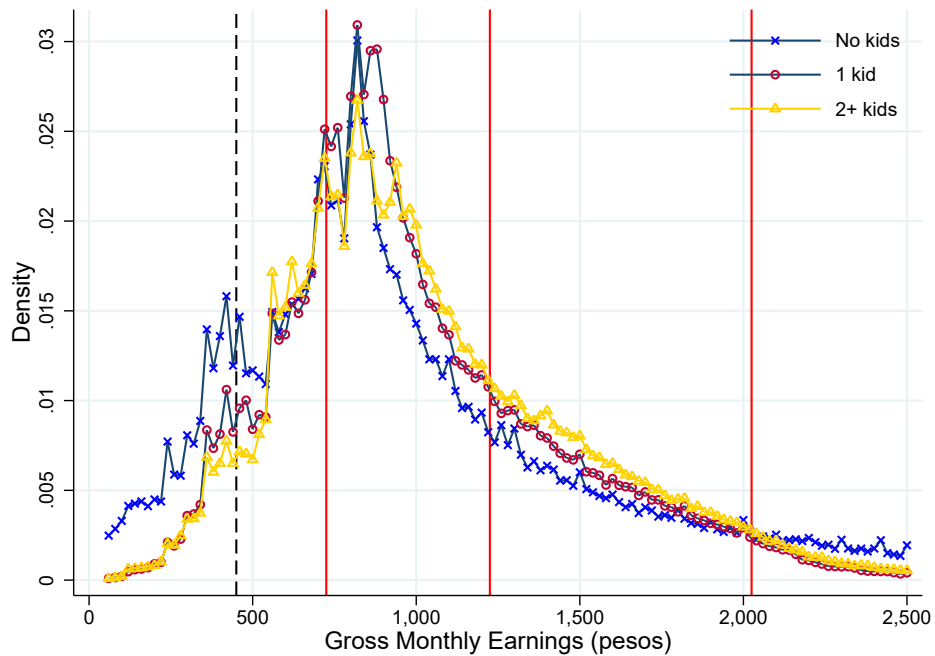


*Notes*: These figures show the bunching response of wage employees to the presence of *notches* embedded in the transfer scheme. Panel (a) presents the distribution of wage-employees grouped in bins of 20 Argentinean pesos together with the theoretical average tax rate for a worker with 2 children.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

Figure I.30: Empirical discontinuities in transfer amount at *notches*

(a) Median transfer
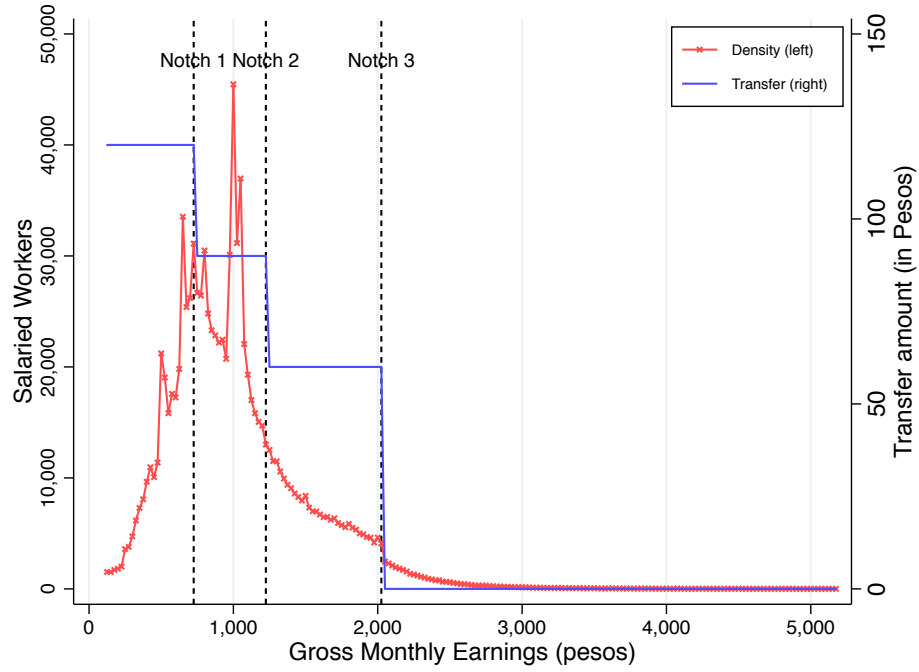


(b) Mean transfer
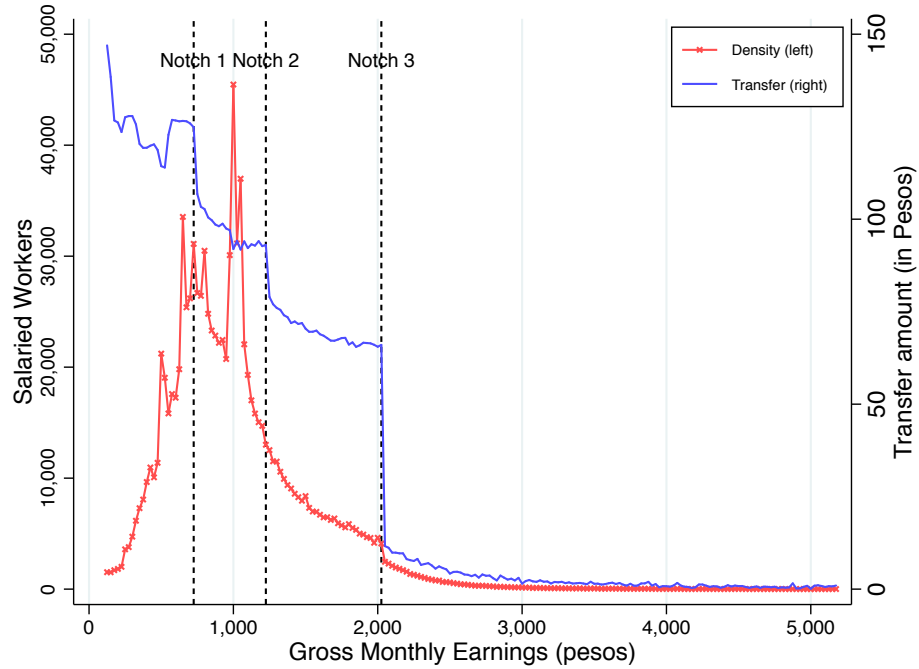


*Notes*: These figures show the bunching response of wage employees to the presence of *notches* embedded in the transfer scheme. Panel (a) presents the distribution of transfer recipients grouped in bins of 20 Argentinean pesos together with the empirical median ATR. In panel (b) we repeat the analysis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

Table A2: 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 A3: Robustness exercises - alternative specifications

|  | (1) | (2) | (3) |
|---|---|---|---|
| **Reduced Form** | | | |
| Δ monthly wage | 4.44*** | 4.69*** | 4.33*** |
| (in pesos) | (0.85) | (1.21) | (1.23) |
| | | | |
| **2SLS** | | | |
| $\frac{\Delta wage}{\Delta transfer(\tau^e)}$ | -0.05*** | -0.05*** | -0.05*** |
| | (0.01) | (0.01) | (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 A4: 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 A5: Employer-mediated child benefits around the globe

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