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Behavioral Responses of Workers and Businesses to Tax and Transfer Policies

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Behavioral Responses of Workers and Businesses to Tax and Transfer Policies

by

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Committee in charge:

Professor Emmanuel Saez, Chair
Professor Alan Auerbach
Professor Danny Yagan

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Abstract

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Professor Emmanuel Saez, Chair

This dissertation studies behavioral responses of workers and businesses to tax and transfer policies. It examines how these policies affect the incentives of economic agents, whether they foment rent-seeking behavior, and how frictions might limit the set of choices. To address these questions, it exploits plausibly exogenous policy changes using quasi-experimental techniques from labor and public economics as well as large administrative datasets.

The proper taxation of high earners is front and center in the policy debate. While higher progressive income taxes may mechanically reduce income disparities, critics argue that they can backfire by lowering work effort and hours. The first chapter of my dissertation studies intertemporal labor responses of high-wage earners to income tax changes. This chapter is co-authored with Guillermo Cruces and Victoria Castillo. We exploit a large and quasi-randomized income tax holiday in Argentina that exempted a group of high-wage earners from the income tax for 2.5 years and left another comparable group of workers taxed. We provide one of the cleanest evidence to date that the labor supply of high-wage earners responds very little to temporary income tax changes. We report a precise and very small wage-earnings elasticity of 0.02. Responses are larger for more flexible outcomes (overtime hours) and more elastic groups (job switchers and managers). We also find avoidance responses from new entrants who faced no tax if their first monthly wage was below a fixed threshold. Our evidence suggests that low responses might be driven by labor demand constraints (eg. fixed contractual hours) and labor market rigidities, and that employer-employee cooperation is required for wage earners to be able to respond to tax changes.

Most countries also provide some sort of financial aid at the low end of the income distribution (e.g., the EITC in the U.S.). An understudied question in Economics is whether employers capture part of those transfers by lowering wages. The second chapter, co-authored with Santiago Garriga, studies whether the way family allowances (tax credits) are disbursed affects the wage of workers. We exploit an unusual reform in Argentina that was gradually rolled out and shifted the disbursement responsibility from firms to the government, reducing the saliency to employers. Our event study estimates show that employers capture about 10-20 percent of the transfer when they mediate its disbursement. In terms of the mechanisms,

the increase in monthly wages after the event is more consistent with a labor demand story rather than pay equity concerns. For example, the effect is explained by new hires rather than incumbent workers. Our evidence suggests that wages do adjust to the way transfers are disbursed, rejecting the null hypothesis that transfers are captured dollar for dollar by workers.

In addition to income taxes, many countries tax small and large businesses under different regimes. The third chapter, co-authored with Santiago Garriga and Jorge Puig, estimates the response of self-employed and firms to two revenue taxes—*monotributo* and the gross receipts tax—across the revenue distribution. We exploit several revenue-dependent discontinuities (*notches*) in Argentina that provide incentives to underreport sales. For self-employed workers and small firms, we find sizeable responses that are stronger for higher tax incentives and in sectors with more space for manipulation such as service-based activities. In the case of medium and large firms, bunching is less striking but it suggests that even large firms are able to underreport their gross sales to avoid facing higher tax rates. Firms also seem to find more costly the indirect administrative cost of becoming a collection agent than the direct fiscal cost of the gross receipts tax. Our results also suggest that some entities face substantial adjustment frictions or inattention to tax discontinuities. We cannot rule out, however, that large firms adjust other margins to compensate for the higher tax pressure.

To my parents, siblings, wife and son.

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Dissertation Introduction

This dissertation studies behavioral responses of workers and businesses to tax and transfer policies. A well established proposition in Economics is that tax systems distort relative prices and create disincentives to work, to sell, to produce, and to invest. Yet, the evidence on whether the real world responds to economic incentives qualitatively and quantitatively as the theory predicts is still inconclusive. This is essentially due to the difficulty of finding compelling research designs and the intricacy of accessing large administrative data. In this dissertation, I make progress on these two fronts using Argentina as my laboratory. The three chapters combine rich population-wide administrative data with unprecedented features of the tax system that allow me to analyze real and avoidance responses of wage earners, independent workers, and businesses.

Argentina constitutes an interesting setting for several reasons. The first and most important one is that Argentina experienced drastic changes in its tax system during the last two decades that offer a unique opportunity to provide fresh evidence on classic questions in Public Finance. Second, Argentina experienced high and persistent inflation for almost 15 years which makes tax avoidance responses easier to accommodate. This is because nominal income and sales go up mechanically and thus it is harder for the IRS to detect avoidance responses such as underreporting. Moreover, although wages are nominally rigid, they might not be in real terms and thus rent-seeking behavior can arise. Third, Argentina has high evasion, informality, and relatively weak enforcement. Fourth, Argentina has high-quality and high-frequency data that has been barely used by researchers. Finally, it is a high-middle income country with a tax system and labor market institutions that are similar to other Latin American countries and some OECD countries, such as France, Italy, and Portugal, and thus it could offer important lessons to them as well.

In the first Chapter, I focus on wage earners and analyze real labor supply responses to the personal income tax. Wage earners are an interesting group because their actions are tied to a contract with their employer, and therefore cannot choose hours of work freely. Hence they are deemed as less responsive to taxation. Some scholars argue that low responses from this group might be due to small tax changes exploited in the literature that do not make it worth to readjust hours. To test this, in the first chapter I leverage a large, salient, temporary, and quasi-randomized tax holiday that exempted a group of high-wage earners from the income tax for 2.5 years and left a comparable group taxed. This variation, which I came across by reading the law, comes close to an ideal experiment where policy makers

randomly assign tax rates across similar workers. By comparing workers below and above an eligibility cutoff, I show that the labor supply of high-wage earners responds very little to temporary income tax changes. I also argue that this low response does not imply that structural elasticities are small for wage earners, but rather that it requires more than their will to make changes in their labor supply. This is because employers need to be onboard with their decision as well. As stated in the title of the chapter, it takes two to *tango*. To the best of my knowledge, this constitutes one of the cleanest contributions to date on this topic.

The second Chapter explores the relationship between employers and employees further, but in the context of tax credits. In this case, however, I turn the attention to rent-seeking behavior from employers in the mediation of family allowances. In particular, the Chapter studies whether the way family allowances (tax credits) are disbursed affects the wage of workers. In this case, I exploit an unusual reform in Argentina that shifted the disbursement responsibility of family allowances from employers to the government. This was gradually rolled out over the course of eight years allowing for an event-study approach. I find that employers capture about 10-20 percent of tax credits by lowering wages when they mediate the disbursement. Intuitively, when firms are in charge of delivering the transfer, they integrate it in the total compensation package and thus capture a fraction of the wage from eligible workers. The Chapter explores some of the mechanisms and shows evidence consistent with this story. The economic incidence of tax credits has been largely ignored in the literature because the identification of wage effects is challenging. As such, the reform at hand and the findings constitute a clear contribution to the field.

Finally, in the third Chapter I turn the attention to independent workers and businesses who are taxed under different regimes than wage earners. The goal of this Chapter is to analyze whether the low responses of wage earners reported in Chapter 1 are a general feature shared by other economic agents of the economy. Other papers have shown that self-employed workers and firms are more responsive to taxation. I exploit revenue-dependent discontinuities (*notches*) to show that this is also the case in Argentina. The distinctive feature of my setting is that it applies to a gross sales tax (i.e., with no deductions) with a broad tax base and presumably less space for responses. Yet I find that entities underreport sales to lower their tax liabilities. This behavior is stronger for small taxpayers in the service sector. These findings complement the results from Chapter 1, by showing that other agents do respond to other form of income taxes. Importantly, these agents do not depend on other people to change their behavior, as is the case for wage earners.

All in all, this dissertation offers a complete picture of how workers and firms respond to the tax system and how they interact with each other in that process: it shows that wage earners barely adjust their hours of work to large and temporary tax changes; it also sheds light on the role of employers in those low responses; in the case of tax credits it shows that when it is convenient for employers, they do extract rents from their employees; and unlike wage earners, the dissertation shows that self-employed workers and businesses do adjust their income in response to taxes, but the evidence points to avoidance rather than real behavior.

Chapter 1

It Takes Two to *Tango*: Labor Responses to an Income Tax Holiday in Argentina

1.1 Introduction

The proper taxation of high earners is front and center in the policy debate. While higher progressive income taxes may mechanically reduce income disparities, critics argue they can backfire by triggering behavioral responses—real, avoidance and/or retiming (Slemrod, 1995)—that increase efficiency costs. For high-income earners, there is convincing evidence on tax avoidance responses, but it has proven much more difficult to assess whether they change their real behavior in response to taxation (Saez, Slemrod, and Giertz, 2012b).[1] In part this is because tax changes tend not to be that large or that easy to exploit for identification (Chetty, Friedman, Olsen, and Pistaferri, 2011). For instance, research designs based on kink points perform poorly at the high-income end, and variation from tax reforms is rarely large enough to trigger real responses (Chetty, 2012). In fact, most of the literature is typically based on small changes to provisions of the tax code, or comprehensive tax reforms with many moving pieces, that hinder clean identification of real work behavior. Oftentimes, such policy variation does not provide a good counterfactual or comparable control group. Another limitation is that measuring the actual work effort of high-income earners is particularly difficult given data availability (Saez, 2017). For example, most studies use annual tax return data, which are good to estimate the elasticity of taxable income and to uncover avoidance responses, but are not well-suited to analyze labor supply responses (e.g., datasets lack information on hours or days worked). Convincing identification of real labor responses to taxation thus requires granular data coupled with large, salient, and exogenous

[1] Saez (2017) argues that in the literature on reported income responses, it is difficult to compellingly uncover real responses, especially at the high-income end.

variation in tax rates that affect differentially a comparable subset of the labor force.[2]

In this paper, we break new ground on this important topic by leveraging high-quality data and by exploiting an unprecedented quasi-randomized income tax holiday for high-wage earners, that meet the aforementioned ideal features for identification. In August 2013, the President of Argentina passed an Executive Order that exempted a group of high-wage earners from the income tax for 2.5 years and left the remaining group taxed. Eligibility was based on two simple rules: (i) workers with wage employment history between January and August 2013 were tax-exempt if their *highest monthly wage during those eight months* was less than or equal to a fixed threshold of AR$ 15,000 (about US$ 3,000); (ii) workers without wage employment history from January to August 2013 entering the labor force were tax-exempt if their *first monthly wage* was less than or equal to AR$ 15,000. In contrast, wage earners above the threshold continued to pay taxes normally.[3] So, for the first group, the rule was based on *past* wage earnings but it applied to *subsequent* wage earnings. For the second group, the rule was based on the wage paid in the *first* month of employment irrespective of subsequent earnings. That is, in both cases workers did not lose the benefit if monthly wages crossed the AR$ 15,000 threshold after August 2013. Moreover, the tax exemption applied to their entire salary (i.e., zero marginal and average tax rates).

This policy emerged as an immediate tax relief and a temporary fix to a deteriorated progressive income tax schedule whose parameters were not indexed for inflation. Between 2000 and 2016, Argentina suffered an average annual inflation of 25% that reduced the significance of nominal taxable thresholds and laid the foundations for such a sharp change. The consequence of this targeted tax cut, in terms of the share of wage earners affected by the income tax, is illustrated in Figure 1.1. In September 2013, about 50% of wage earners subject to the tax suddenly stopped being liable and remained untaxed for 2.5 years. This amounted to 1.2 million of upper wage earners, between percentiles 70 and 85. In contrast, the top 15% experienced a tax increase due to a "bracket creep" effect.[4] A new administration took office in December 2015, and in February 2016 it reversed the tax change and increased the nontaxable income floor to avoid a discrete jump in the number of taxpayers. Nonetheless, with almost 40% of inflation in 2016, most of the exempt workers were hit again by the tax in less than a year. All in all, this tax break effectively treated wage earners who coexist in the same labor market (even in the same firm) with sharply different tax rates.

The different scope for manipulation of the two assignment rules naturally leads to two

[2]The ideal yet unfeasible experiment would be to randomize tax rates across workers and then compare working hours between those facing higher tax rates and those facing lower or zero tax rates (e.g., see Ashenfelter and Plant (1990) for cash transfers).

[3]The AR$ 15,000 threshold is located at the 85th percentile of the monthly wage distribution. Independent workers are taxed under a different regime and did not benefit from the policy. They could serve as a control group, but their income is reported in another database unavailable to us.

[4]The idea of bracket creep is that a taxpayer near the top-end of a bracket is likely to "creep" to the next bracket due to inflation and wage negotiations, even if her income does not change in real terms. In a related paper we use the bracket creep design from Saez (2003) to estimate the labor supply elasticity.

research designs. In the case of rule *(i)-incumbents*, its backward-looking nature precluded manipulation as the current tax treatment was based on past wage earnings from the *reference period* of January-August 2013, before the policy was even announced. Hence, comparing workers below and above AR$ 15,000 using a regression discontinuity design (RDD) offers a unique opportunity to estimate the causal effect of a large, salient, and sharp tax cut on the labor supply of high-wage earners. In contrast, with rule *(ii)-new entrants*, there was space for manipulation because it was based on the first monthly wage, and the information was available to workers before entering the labor market. Hence, workers had incentives to collude with employers to enter strategically below AR$ 15,000 and escape from the income tax. It was also advantageous to employers who could recruit more cheaply. Accordingly, we implement a density analysis of the starting monthly wage around this *notch*. We also leverage the context of high inflation to predict a counterfactual mass of new entrants above the notch, which we compare to the observed mass.

The tax break was announced on August 28th, 2013, applied to monthly wages earned after September 1st, 2013, and was repealed on February 22nd, 2016. Although the policy was perceived as temporary, both the beginning and the end were unanticipated and thus created income effects for the group that was employed. Hence, our RDD strategy allows to identify uncompensated intertemporal elasticities for the intensive margin. For both strategies, we use rich employer-employee social security data for the universe of private and public wage earners. These records are reported by employers on a monthly basis and contain not only monthly wage earnings but also some of its components such as base pay, bonuses, overtime pay (and hours), and other supplemental pay—all outcomes that are rarely available in other countries and are well-suited for studying labor responses to taxation.

Our first-stage analysis documents sizable changes in tax rates for upper wage earners above and below the tax holiday threshold, much larger than most studies (see Table 1 in Chetty (2012)). Five days after the announcement, the marginal tax rate for workers slightly below the discontinuity went from about 25% to 0%, and the average tax rate decreased from about 7% to 0%. Both marginal and average tax rates converged quickly to their pre-reform levels after the decree was repealed, and remained stable thereafter. In the case of new entrants, the marginal tax rate could hit 31-35% if they entered slightly above the notch, while the average tax rate would be about 10%.

The four major findings are the following. First, the RDD analysis delivers a precisely estimated and very small response of wage earners to the large, salient, and temporary income tax change. The evolution of the RD estimates shows a small increase in wage earnings in 2014 and 2015, that fades away in 2016 and 2017 when the tax holiday was repealed. In 2015, tax-exempt workers below the discontinuity present an excess wage earnings growth of 0.4% relative to non-exempt workers, which translates into an elasticity of 0.017. Second, this small aggregate effect is primarily driven by relatively flexible components of workers' pay. In particular, among these components we find an intensive margin elasticity of overtime hours to taxation of 0.184.[5] Third, we also find larger effects when we zoom in on small

[5]This is computed over an average base of 25 monthly overtime hours. Although larger than the wage earnings

subgroups likely to be more elastic in their responses. For example, tax-exempt workers switching jobs seem to negotiate their new contracts more favorably. The implied wage earnings elasticity for jobs switchers is 0.096 in 2015. Another responsive group is given by managers and executives. In this case we find a large increase in the wage earnings of those located in the tax-benefit zone relative to those that kept paying taxes normally. The wage earnings elasticity for managers is 0.311 in 2015. Fourth, for high-wage earners entering the labor market, we find that the reform induced some of them to enter strategically below the AR$ 15,000 threshold to avoid the income tax. Again, this behavior is more pronounced for managers and executives.

We complement these findings with evidence that goes against four alternative explanations of the aggregate near-zero result. First, we argue this is not driven by lack of *saliency*, since the tax shock was highly publicized and discussed. The announcement was made by the President with live nationwide coverage on the main news channels. It also appeared on the front page of the main newspapers. The IRS issued a memo explaining who was benefited and how to compute the assignment variables, and this was amply discussed on TV newscasts.[6] In addition, the Executive Order mandated the inclusion of two items in the pay stubs of tax-exempt workers, one with the amount that should be withheld and another one with the same amount credited back. Second, we argue that the null result is not explained by lack of *enforcement*. Employers and their accountants, who calculate and file monthly withholdings on behalf of workers, were in charge of computing the running variable, and could face high penalties from the Argentine IRS for placing workers on the wrong side of the discontinuity. Anecdotal evidence suggests that accountants followed the eligibility rules closely to avoid such penalties.[7] Third, we show evidence that rules out an *incidence* story where employees work longer hours but employers lower the wage rate. We use a sample of overtime workers, for whom we observe monthly hours, to back out wage rates and we find a very precise zero effect in the RDD. Fourth, we also argue that the near-null result is not a combination of *substitution effects* and *income effects* that cancel out. Under the assumption that the income effect decreases with age (Cesarini, Lindqvist, Notowidigdo, and Östling, 2017), the uncompensated response should be higher for older workers. Yet our empirical analysis broken by age groups does not support this argument.

Overall, this paper provides one of the cleanest evidence to date that, in the aggregate, high-wage earners do not adjust their labor supply in response to temporary tax changes. This result strikes us as remarkable given the size, saliency, and length of the tax break. The larger effect for job switchers might imply that wage earners are demand constrained. That is, workers are basically stuck in a job in which the contract states how much they

elasticity of 0.017, it is still small if we include (unobserved) *straight-time* hours in the computation of the percentage change.

[6]Unlike typical tax reforms, these features were simple to understand from the point of view of the worker. They did not require an understanding of the tax code whatsoever, just that if a worker was lucky to be below the threshold, she did not have to worry about the income tax anymore.

[7]In the appendix we present evidence from two anonymous firms (medium- and large-sized) that shared detailed payroll data with us, and we find 100% compliance around the discontinuity.

work and how much they earn, restraining the choice of hours of work over the course of the year. Likewise, overtime is a margin that allows for some discretion in hours of work and yet we find relatively small effects. This could mean that labor demand restrictions are at play such that workers are not free to vary overtime hours (e.g., many facilities require some fixed level of overtime to run operations continuously). The larger response of managers and executives could be rationalized by their proximity to firm owners and by a broader compensation mix that let them adjust reported wages and hours more easily than the typical employee. Finally, the strategic behavior from new entrants to dodge taxes would not be possible without coordination with employers. Taken together, our results point toward rigidities in the labor market which requires employer-employee cooperation for wage earners to respond to tax changes.

This paper complements the empirical literature estimating labor supply responses to income taxation. The most reliable evidence up to the late 2000s is summarized in two contemporaneous surveys that reach opposite conclusions: while Saez et al. (2012b) argue that the profession settled on a fairly small compensated elasticity of labor supply with respect to taxes, Keane (2011) casts doubt on the existence of such a broad consensus.[8] The controversies in this literature usually revolve around identification issues and data limitations that our setting circumvents. We contribute to this debate by documenting that in contrast to numerous other tax reforms studied, a large and salient tax change had no detectable near-term impact on the labor supply of high-wage earners.

Our paper also contributes to very limited quasi-experimental evidence on intertemporal real labor responses to taxation. In two contemporaneous papers close to ours, Martinez, Saez, and Siegenthaler (2018) and Sigurdsson (2018) estimate the Frisch elasticity by exploiting tax-free years in Switzerland and Iceland, respectively, that arose from the transition to pay-as-you-earn tax systems.[9] Unlike these labor market-wide tax holidays, an advantage of our setting is that the tax cut only affected a subgroup of high-wage earners and therefore general equilibrium effects are less of a concern. In addition, our local RDD has higher internal validity and provides a clean comparable control group that lets us average out recurring threats such as mean reversion and measurement error.

This article is also related to recent work on real labor responses to taxation. Tazhitdinova (2019) and Tazhitdinova (2020) analyze part-time and secondary jobs in Germany, and find large responses at the lower end of the wage distribution. These two margins of response are less common at the upper end that is focus of our study. Kleven and Schultz (2014) provide compelling small labor income elasticities for wage earners by exploiting large variation in Denmark. We also add to a scant literature that uses survey data to study overtime hours (e.g., Cahuc and Carcillo (2014) in France). This is an important yet relatively unexplored

[8]Keane (2011) meta analysis points to an average compensated elasticity of 0.31, which he considers sufficient to induce large efficiency losses from progressive income taxation. Saez et al. (2012b) focus on a narrower base, taxable income, which captures real and avoidance behavior. Elasticities range from no effect to sizable responses. See also Blundell and Macurdy (1999) for an older major survey.

[9]The study by Sigurdsson (2018) uses the same variation as Bianchi, Gudmundsson, and Zoega (2001) but with better data and an improved empirical strategy.

margin of response to income taxation due to lack of data and sharp identifying variation that we manage to leverage. Finally, our findings are connected to recent work that argues that firm preferences matter for labor supply and reported income responses to taxation (Best (2014), Kreiner, Leth-Petersen, and Skov (2016), Tazhitdinova (2020)).

The article is organized as follows. Section 1.2 describes the Argentine income tax and the variation we use. Section 1.3 discusses the expected responses to the tax change. Section 1.4 introduces the administrative data and summary statistics. Section 1.5 presents the empirical strategy. Section 1.6 shows the first stage change in tax rates and evidence on saliency. The main results are presented in Section 1.7. Section 1.8 shows the avoidance behavior of new entrants. Finally, Section 1.9 concludes.

1.2 Setting and a *local* income tax holiday

Argentina is one of the countries with the highest tax pressure in the world, in line with the average of OECD countries. It is a federal country that levies taxes at the federal, provincial, and municipal level. Taken together, the total revenue-to-GDP ratio from these three levels of government went from 20% in 2001 to 34% in 2015.[10] National taxes contributed to this dynamic by going from 13.4% to 17.6% of GDP during this period. In particular, the income tax has played a central role by increasing its participation in national tax revenue from 27.4% to 37.1%. It is the most progressive tax and the second most important source of tax revenue after the VAT. We next explain how the personal income tax works in Argentina and we describe in detail the tax change exploited in this paper.

The personal income tax in Argentina

Argentina has an individually-based personal income tax (PIT) with seven brackets and progressive marginal tax rates ranging from 9% to 35%. The schedule for workers with positive taxable income is depicted in Table 1.1.[11] Contrary to more developed economies, the system is characterized by a large exemption floor with two components: a fixed universal amount and another portion that varies with the number of dependents (spouse, children, and other relatives). In 2016 the personal exemptions for married wage earners with two children were 50% higher than the average wage of the economy (10% higher for single workers with no children). Consequently, relatively high-income workers are reached by this tax (the top 20-30% according to Figure 1.1), which is a common feature in many developing countries (Jensen, 2019).[12]

[10] This is mainly explained, in order, by social security contributions, income tax, gross receipts tax, VAT, export duties, and financial transactions tax.

[11] This schedule applies to wage earners and retirees. The self-employed are taxed under a simplified regime called *Monotributo*. Since 2017, the PIT has 9 brackets and MTRs ranging from 5 to 35%.

[12] In August 2013, single workers with no children and gross annual earnings below AR$ 108,676 (USD 19,406) were not subject to the income tax. The average gross annual wage for private workers was AR$ 107,783

Employers use a PAYE (Pay-as-You-Earn) system to withhold income taxes from monthly wages (similar to the U.K. system). Withheld amounts are treated as advance payments of annual income tax. Each employee receives a withholding summary from their employer at the end of the year (form F.649) and, if the amount withheld exceeds an annual tax assessment, the worker is entitled to a refund in January's paycheck. The employer is responsible for remitting the income tax to the IRS each month (form F.744), along with social security contributions (form F.931). At the beginning of the year or upon hire, employees must inform their employers about exemptions, deductions, and other jobs through online form F.572 (equivalent to W-4 in the U.S.).[13]

In practice, Argentina uses the Cumulative Wage Withholding Method. Every month, employers have to compute the cumulated taxable income up to the corresponding month (cumulated wage earnings net of cumulated deductions and allowances), then use Table 1.2 (a monthly version of the tax schedule) to calculate the cumulated tax liability up that month, and finally subtract withholdings from previous months, resulting in the amount to be withheld. Contrary to the U.S. Percentage Method, under the Argentine system the tax burden varies according to the seasonal nature of a job (i.e., higher withholding during months with supplemental pay). This provides space for instant responses to changes in the income tax. For example, an individual working overtime in one month could decide to work fewer hours in the next month because the income tax erodes part of the overtime premium.

Importantly, in Argentina the law does not require wage earners to file a tax return at the end of the fiscal year, unless they exceed an annual income threshold determined by law (e.g. USD 30,000 in 2016). If a person crosses this threshold, or earns honoraria, dividends from a company, capital gains, rental income, or have some other complicating tax factor, then she is required to file the annual tax return F.711, similar to form 1040 in the U.S. Hence, it is really the employer and firm accountants that play a key role in computing and enforcing the tax, while employees only have to check out their pay stub every month to see how much taxes they pay.

Macro context and the income tax holiday

Inflation and bracket creep

The policy exploited in this paper emerged as an immediate tax relief and a temporary fix to a deteriorated progressive income tax schedule in a context where inflation was high and nothing was indexed. In particular, the following four facts explain the evolution of the PIT

(USD 19,247), and the monthly minimum wage was set at AR$ 3,300 (about USD 7,661 annually), well below the first kink.

[13] Exemptions include spouse and dependents allowance, a minimum non-taxable income, and a large special deduction for wage earners. Deductions include SSC and other minor deductions such as prepaid medical care fees, life insurance, medical expenses, mortgage interests, donations, funeral expenses, domestic services. Contrary to developed countries, itemized deductions are typically capped and represent on average only 2-3% of gross earnings (Tortarolo, 2018). In case of multiple employers, the one that pays the highest monthly wage acts as the withholding agent on total earnings.

during the period 2000-2016 and, ultimately, motivated the tax break on high earners: (1) starting in 2007, Argentina experienced an average annual inflation rate of 25% and peaks of up to 40% (black line of Figure A.2 panel a); (2) nominal wage earnings were adjusted semi-annually to preserve the purchasing power of workers (red line of Figure A.2 panel a); (3) the tax schedule remained fixed in nominal terms from 2000 to 2016; (4) the exemption floor was partially adjusted in some years and usually behind the average increase of wage earnings (Figure A.3 panel c).

Taken together, these stylized facts had two direct implications: (i) that more workers started to pay the income tax since inflation reduced the real value of personal exemptions—a parameter that determines the floor of the first bracket (Figure A.3 panel c); and (ii) for those paying the tax, their taxable income was quickly taxed at the top marginal tax rate because inflation reduced the significance of taxable thresholds (Figure A.3 panels a and b). This phenomenon is known in the literature as "bracket creep" (Saez, 2003). Hence, the PIT lost progressivity since workers with very different earnings could be taxed at the top marginal tax rate. But at the same time, the system gained some progressivity by reaching more workers in the upper tail of the earnings distribution, and by increasing the tax burden on those that were already subject to the tax (Figure A.3 panels b and d).

An income tax holiday on high earners

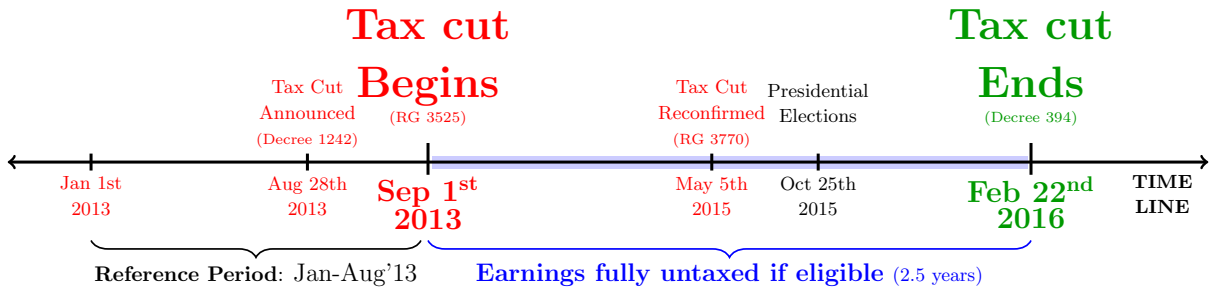
To alleviate the increasing tax burden on high-wage earners, in August 2013 the President of Argentina implemented a targeted income tax cut that lasted 2.5 years and affected differentially what would otherwise be comparable workers.[14] Eligibility was based on two simple rules:

- **Rule (i) - Incumbents**: workers with wage employment history between January and August 2013 were tax-exempt if their *highest gross monthly wage accrued between January and August 2013* was less than or equal to a fixed threshold of AR$ 15,000 (percentiles 70th through 85th; AR$ 15,000 $\approx$ US$ 3,000 in 2013);
- **Rule (ii) - New entrants**: workers without wage employment history from January to August 2013 entering the labor force were tax-exempt if their *first gross monthly wage* was less than or equal to AR$ 15,000.

[14] The official reason for the tax break was that "*it is a permanent policy of the executive branch to implement countercyclical measures that strengthen the purchasing power of workers and their families and, with it, the consolidation of the demand and the domestic market*" and that "*the implementation of these measures responds to strict justice and equity*" (Decree 1242/2013). However, the opposition claimed that it was a political strategy of the government who lost midterm legislative elections on August 11th, 2013, and thus used the tax cut to improve the public image before the general elections held on October 27th, 2013. Moreover, a hike in the exemption floor was a key request of labor unions representing upper wage earners (e.g., Hugo Moyano, leader of the General Confederation of Labor and the Truckers' Union).

In contrast, wage earners above the threshold continued to pay taxes normally.[15] Independent workers also did not benefit from the policy, because they are taxed under a different regime that remained unchanged. This group could serve as an alternative control group, but their income is reported in another database unavailable to us.[16]

The tax holiday applied to monthly wages earned after September 1st, 2013, regardless of whether they crossed AR$ 15,000 after that date. Moreover, it applied to the entire wage (i.e., zero marginal and average tax rates). The key difference between the two rules is that the first one was based on *past* wage earnings, while the second rule was based on the wage paid in the *first* (and only the first) month of employment. As explained later on, the first rule allows for a clean regression discontinuity design.



The timeline of the tax holiday and other relevant events are summarized above. The policy was announced by the President, with live TV coverage, in the evening of August 27, 2013. On August 28, 2013, the government published Executive Order 1242/2013 where it formalized the tax change and groups affected. On August 29, 2013, the Argentine IRS issued a 2-pages memo (RG 3525/2013) explaining in detail the way to implement the tax cut in practice (e.g., how to compute the threshold, what type of income should be included, etc.). The tax cut entered into force on September 1, 2013, and was repealed in February 2016 through Executive Order 394/2016 by a new administration that took office in December 2015.

In terms of expectations, the policy was perceived as a temporary relief and it was expected to be in place at least until the end of 2015. In addition, both the beginning and the end of the tax break were unanticipated and thus created income effects for the group that was employed. Although the public generally do not expect such sharp policies to become permanent, at the beginning workers and firm accountants had some uncertainty on whether it was going to continue in 2014 because the Executive Order did not include

[15] In practice, wage earners whose highest gross monthly wage between January and August 2013 was between AR$ 15,001 and AR$ 25,000 were partially benefited by a 20% increase in personal exemptions (30% for workers living in the Patagonia region). But this was quickly eroded by inflation and can be ignored in the analysis.

[16] In Argentina, 76% of the workers are wage earners, 20% are self-employed, and 4% are entrepreneurs. The share of informal wage earners was about 30% in the period of analysis. Source: SEDLAC (CEDLAS and The World Bank).

a due date. What is certain is that workers knew that it was not going to be reversed in 2015 for two reasons. First, Argentina had presidential elections in 2015 making unlikely for the government to reverse the policy before then. Second, the IRS issue another memo (RG 3770/2015) in May 2015 that reconfirmed the tax cut for workers with earnings from 2013 below the threshold.

During the official announcement of the tax cut, the head of the Argentine IRS reported that 1,497,368 workers and retirees would no longer be subject to the income tax and that the implied fiscal cost would be AR$4,495 million for the rest of 2013. To partially fund this loss, the president announced that two new bills were going to be sent to the Congress which would raise AR$2,000 million from the private sector. The first one was a 15% tax on capital gains from shares and securities not listed on the stock market. The second one was a 10% tax on the dividends paid to shareholders.[17] In practice, the reform reduced the share of wage earners affected by the tax by approximately 50%, benefiting workers between the 70th and 85th percentiles of the wage distribution (see Figure 1.1). Workers earning about AR$ 15,000 per month (US$ 3,000) went from a marginal tax rate of 23-27% to 0%.

Enforcement

In terms of the enforcement, the income tax law states that if the employer does not withhold the income tax at source properly they are subject to a 100% fine of the tax owed. Moreover, if they hide information and cheat, the tax penalty could be between 2 and 10 times the evaded tax liability (Law 11683 article 45). So we believe that employers (and accountants) had incentives to comply with the law and determine precisely whether the payroll were above or below the thresholds. Or at best, they did not have anything to gain by colluding and helping their employees whatsoever.

To sum up, the income tax holiday made that relatively similar workers ended up facing sharply different tax rates, depending on whether their wage earnings from January to August 2013 were higher or lower than AR$15,000. This is in fact the running variable that we use later in the RDD analysis. Note also, that with an annual inflation of 38% in 2014 and 27% in 2015, workers above AR$15,000 ended up experiencing a tax hike due to the "bracket creep" effect. Therefore, the comparison of workers below and above this fixed cutoff constitutes a unique opportunity to estimate the impact of large, salient, and temporary tax cut on the labor supply of high earners.

Argentine labor market and wage setting

Argentina has a highly regulated labor market with strong labor unions and a wage setting mechanism fairly centralized. Collective wage agreements are signed every year between the major labor unions, employers' associations, and the government at the industry-wide level

[17] This official information can be checked in two articles from Pagina|12 (`https://goo.gl/iZUFtF`) and La Nación (`https://goo.gl/x8bCzv`).

(tripartite negotiations).[18] Each agreement regulates the contractual base pay (monthly or hourly) as a function of seniority, qualifications, degree of responsibility, etc., and sets other specific clauses such as non-contributory one-time payments if inflation is higher than expected, overtime premiums, meal allowances, vacations, etc.[19] Due to high and persistent inflation, the contracts are typically negotiated every year (known as *paritarias*). These industry-wide agreements are de-facto binding for all employers and all workers, irrespective of union membership. In some cases, firm-level agreements might be signed to distribute top-up wage components (e.g., related to indicators of profitability or productivity).

In terms of the working schedule, the majority of wage earners are employed under a standard fixed contract (indeterminate full-time or part-time) and in some sectors it is common practice to work overtime. A normal working day has a maximum of 8 hours and 48 hours a week. In the case of night work between 9pm and 6am, the working day cannot exceed 7 hours and 42 hours a week. In the case of jobs considered unhealthy or risky, the workday cannot last more than 6 hours and 36 hours a week. Any working time beyond these statutory limits is considered supplementary and must be paid as overtime. Employers must pay a 50% overtime premium during weekdays and 100% premium on Saturdays after 1pm, Sundays, and national holidays. The legislation also establishes a limit of 30 overtime hours per month and 200 overtime hours per year. Nonetheless, employers can request a specific authorization from the Ministry of Labor to increase that quota. Likewise, collective bargaining agreements might set a cap better suited for the sector's specific needs (e.g., oil workers are exempt from the time limits established in the law). In practice, to compute the base hourly wage, employers usually divide the monthly salary by the number of hours worked per month (e.g., 8 hours $\times$ 25 days = 200 monthly hours).[20]

The rigid bargaining structure and working schedule described in this subsection already suggest that it might be hard for wage earners to adjust their labor supply freely in response to net wage changes.[21]

1.3 Predictions from the theory

Individuals can respond to income taxation through many margins such as working hours, work effort, career choices, form and timing of compensation, tax avoidance, tax evasion, etc. Our sharp design and rich data offer a rare opportunity to test for real responses and,

[18] Agreements become operative upon approval from the Ministry of Labor (a process called "*homologacion*"). Once approved, it is legally binding on all employers and workers included in the sector, within its territorial scope.

[19] Contrary to the U.S., in Argentina most employees are paid by the month and not by the hour.

[20] All these regulations are contemplated in the Labor Contract Law Art. 201 and Decree 484/2000.

[21] In the appendix we provide two examples of pay scales from two labor unions. Figure A.4 shows the pay scale for wage earners in the banking sector in 2015. The pay scale is pretty much predetermined as it is based on hierarchy and seniority, which limits the space for real labor supply adjustments. Figure A.5 shows the pay scale for city bus drivers in 2013. Although the scale is also predetermined, in this case there is more space for adjustments as, for example, workers can choose overtime hours.

for some groups, avoidance behavior. We explain the predicted responses in two blocks depending on whether workers had a wage employment history in the reference period of January-August 2013 (incumbents) or entered thereafter (new entrants).

Incumbent wage earners

The dynamic labor supply model from MaCurdy (1981) provides the bedrock for understanding labor supply responses to temporary changes in the net-of-tax wage rate (e.g., a temporary tax cut in period 1 reversed in period 2). Under this model, workers might find rational to work more in period 1, save part of the earnings, and work less in period 2. When tax changes are anticipated, income effects are muted, and thus the strength of this reaction is measured by the Frisch elasticity of substitution. In our setting, however, the tax cut came as a surprise thus creating income effects. Hence, in principle, our estimates capture a mix of substitution and income effects. The former pushes wage earners to work more and the latter pushes them to work less. When substitution effects dominate, as suggested by previous work, then we should expect higher hours and wage earnings for tax-exempt workers during 2014 and 2015 that decrease in 2016 when the tax cut was revoked.

It is worth noting that in practice wage earners typically do not have a lot of flexibility to adjust their work schedule (e.g., due to demand-side constraints on hours). Hence, behavioral responses are likely restricted to certain discrete choices (e.g., full-time or part-time jobs, job switching, secondary jobs) or flexible portions of the pay (overtime, commissions, bonuses). Likewise, there could be some groups with higher resiliency than others. For example, managers and executives, private workers versus public servants, workers close to retirement, non-unionized workers, single women, and workers in specific sectors such as manufacturing, transportation, or professional services. Our design and data allow to look at these margins and subgroups.

Among more flexible outcomes, overtime work deserves especial attention. This is an important yet relatively unexplored margin of response to taxation that is less constrained than *straight-time* hours. In a world with taxes, the additional income effectively received from working longer hours is lower than workers might expect. This is because every extra hour worked is taxed at the worker's highest marginal tax rate. Moreover, working overtime could bump workers into higher tax brackets. So, in countries where overtime work is common, this is indeed the key margin of interest to understand the efficiency effects of income taxes. In our setting, and during the tax holiday, tax-exempt wage earners could find that overtime hours on the job were more worthy because they could keep the full dollar out of them. Moreover, the substitution effect is more likely to dominate in this case because overtime pay only represents a small portion of total compensation making income effects less operative. In contrast, during the period of analysis non exempt employees became more discouraged to work overtime because of the "bracket creep" effect. Our data allow us to study monthly overtime hours to test these predictions.

Finally, our setting also allows to study an extensive margin decision: the probability of dropping out. Intuitively, tax-exempt workers on the margin of exiting (e.g., close to

retirement) could find optimal to remain employed to take advantage of the tax break. In contrast, the unequal horizontal treatment of workers around the discontinuity, could induce the annoyed and discouraged non exempt workers to drop out to an informal job to escape from the tax or perhaps become self-employed. Hence, we could expect a lower dropout rate of incumbent workers to the left of 15k during the tax holiday.

New entrants

For this group of workers, the tax change induced entry effects and avoidance behavior. Recall that the eligibility rule was based on the *first* monthly wage, and the information was available to workers in advance of entering the labor market. Hence, there was space for manipulation. On the intensive margin, the new entrants had incentives to collude with employers to enter strategically below AR$ 15,000 and escape from the income tax. On the extensive margin, workers with potential monthly wage earnings around AR$ 15,000 that were on the margin of entering the labor force, had incentives to do so (below the threshold) during the tax holiday to take advantage of this zero-tax period. Nevertheless, we will show that this threshold was too high for an entry salary and thus it is unlikely to trigger employment effects.

1.4 Data and Summary Statistics

In the analysis we combine three administrative databases: (1) Wage earnings data: monthly earnings reported to the Social Security Administration (known as SIPA); (2) Registry of employees (known as *Simplificacion Registral*); (3) Family relationships: a database that links family members (known as ADP).

Wage earnings data (SIPA)

The core data source used in the analysis is the SIPA database. It contains social security records for the universe of registered wage earners in Argentina from January 1995 to December 2018. These administrative data are third-party reported by employers on a monthly basis through form F.931 (the equivalent of Form 941 in the U.S.). All the firms have to use the same online processing software, SICOSS, with a simple interface that makes it a reliable source (see Figure A.6 in the appendix). We use a particular version of the SIPA database, which follows the full working history of workers, in every firm, month by month. This employer-employee panel allows us to generate variables related to the jobs before and after the tax holiday, and to identify job switches. We focus on the period 2011-2017.[22]

In 2013, the year of the reform, the data included about 450 thousands private employers and about 7 million private wage earners (10 million when we include public employees).

[22]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. The administrative databases were accessed at the Argentine Ministry of Labor (MTEySS).

The data have two types of (scrambled) identifiers: CUIL, which identifies workers, and CUIT, which identifies the firm(s) where people work. Other variables contained in the data are: gross monthly wage earnings, date of birth, gender, tenure, indicator for private sector, 4-digit ISIC sector code of the firm, labor union status, type of contract (permanent, temporary, full-time, part-time, manager). Importantly, we do not observe take-home pay. We observe posted earnings before employee social security contributions and income taxes in each month of the data. This is indeed the variable that firms had to use to determine whether a worker qualified for the tax break or not.

The Ministry of Labor also provided access to some of the raw files that are used to create the SIPA database. In particular, we have access to every April, August, and October in the period of 2011-2016. The advantage of these monthly files is that they contain very rich information that is rarely available in standard employer-employee databases. For instance, we can decompose monthly wage earnings into 5 categories: base pay, overtime pay, 13th salary (50% in June and 50% in December), special concepts (seniority, plus for college degree), bonuses (productivity, commissions, presenteeism), vacation pay, and non-contributory payments negotiated by labor unions (e.g., lump sum bonuses to compensate inflation). In the case of overtime pay, employers also have to report the corresponding number of overtime hours worked in that month, allowing us to back out the hourly wage. Employers also report the number of days worked per month. Although this variable does not have much variability in the aggregate it does vary for special work arrangements such as night shifts or hazardous jobs in which employees are required to work every other week/day and have to rest in the remainders.

Registry of employees (*Simplificacion Registral*)

Every time that employers register or unregister an employee they must do it online on the IRS website through the centralized system *Simplificacion Registral*. During this process, they also have to report a firm-branch identifier, the address of the worker and of the firm-branch, the initial occupation of the employee using 4-digit ISCO codes, a code for the labor union that represents the worker, and in some cases the educational level. These are important variables that are rarely available in typical administrative datasets and let us shed light on the mechanisms of workers responses through a set of exercises that we will explain later on.

Family links (ADP)

We also combine the social security data with another database that contains family relationships. These data allow us to link workers to their dependents (spouse and children) accurately since 1970s. In Argentina, to claim social benefits or deduct dependents from the income tax, applicants have to register and report their family composition. Using worker's identifiers we are able to merge these data with SIPA and determine marital status and

number of dependents of each worker. The workers that appear in SIPA but not in ADP are considered single with no children.

It is important to clarify that the earnings data used in this paper do not contain income tax variables and, thus, we do not observe withheld income taxes. Note, however, that social security data include both workers paying the income tax and not paying the income tax. This feature is crucial for the analysis which requires to follow workers that were fully exempt after the reform, and this is the reason why SIPA data are better suited for the empirical analysis than any other source. To alleviate the missing tax information, we use the tax calculator developed by Tortarolo (2018) which allows to identify workers subject to the income tax, marginal tax rates, and monthly withholdings.[23]

Table 1.3 reports some summary statistics for all private and public wage earners, and for three groups of workers defined based on earnings between January and August 2013: (1) workers between $10k$ and $15k$;[24] (2) workers between $15k$ and $25k$; and (3) workers between $14k$ and $16k$. About 14% of employees went from the income tax paying zone to the non-paying zone in September 2013 (column 2), and about 9% of total wage earners qualified for a partial tax cut due to the 20% increase in personal exemptions right after the reform (column 3).[25] These two groups of workers belong to the 7th-9th deciles of the earnings distribution. Hence, the reform studied mainly affected upper earning workers.

Narrowing the attention to the group of workers located around 15k, which is the main discontinuity introduced by the reform, we can see that they are prime-age workers, 43% work in the public sector, half of them are covered by a collective bargaining agreement, 38% are female workers, and around 7% have multiple jobs. It is worth noting that in August 2013 average earnings for group 4 were AR$13,203, well below the cutoff that determined who was exempt from that point onwards.

1.5 Empirical Strategy

To study the response of individuals to the income tax, one could run a regression of the change in reported income on the change in the net-of-tax rate. However, the regression coefficient would be biased because marginal tax rates are a function of taxable income. Hence, the literature has typically relied on exogenous variation provided by tax reforms and a variety of estimation techniques to identify the elasticity of taxable income to taxation (see Saez et al. (2012b) for a recent survey). In this paper, we use a regression discontinuity design (RDD), which overcomes identification difficulties that affected previous work (e.g., mean reversion and heterogenous income trends) and which is known to have a higher internal

[23]The calculator uses income from SIPA, family links from ADP, and the parameters of the income tax. It is analogous to the NBER's Tax Simulator in the U.S. For more details see Tortarolo (2018).

[24]In August 2013, the monthly minimum non taxable income for a single worker without children was AR$ 8,360 gross and for a married worker with two kids was AR$ 11,563 gross.

[25]Note that the percentage of exempt workers in column 2 basically coincides with the official numbers reported in Figure 1.1.

validity than other methods. We also complement the analysis with a difference-in-difference approach to study the response of workers farther away from the discontinuity or when we are underpowered to run the RDD in small subgroups.

Recall that the policy created a sharp discontinuity on tax rates depending on whether the assignment variable—*the highest gross monthly wage accrued between January and August of 2013*—was below or above AR$ 15,000. This feature naturally leads to a regression discontinuity design. The basic idea is to compare wage earners just above and just below the threshold to infer the causal effect of the tax change. This design is appealing because it is relatively simple and transparent. Therefore, we will identify tax effects by running regressions of the form:

$$Y_i = \alpha + \beta \cdot 1(R_i \leq c) + \sum_{k=1}^{K} \gamma_{0k} \cdot (R_i - c)^k + \sum_{k=1}^{K} \gamma_{1k} \cdot 1(R_i \leq c)(R_i - c)^k + e_i \tag{1.1}$$

where Y_i denotes any outcome of interest for worker i in any month or year before, during, and after the tax holiday, $c = 15k$ is the cutoff of interest, and R_i is the running variable defined as

$$R_i \equiv \max\{gross\ monthly\ wage_i \mid \text{January to August 2013}\} \tag{1.2}$$

The coefficient of interest capturing the effect of the discontinuity at c is β. A simple way to illustrate the RDD is to plot average outcome Y_i by disjoint bins of the running variable R_i and draw a polynomial fit below and above the cutoffs. We follow this procedure before, during, and after the tax holiday is implemented. Intuitively, the treatment may be as good as randomly assigned for individuals in the neighborhood of $R_i = c$, so comparing treated and non-treated workers reveals a treatment effect (i.e. the effect of the tax cut/hike on labor supply).

The labor outcomes considered in the analysis below include: annual and monthly wage earnings, overtime pay, overtime hours, base pay, percentiles of wage earnings, fraction of workers with multiple jobs, fraction of workers dropping out, percentage change in gross earnings. In the first stage, we show that the MTR and ATR change sharply around the $15k$ cutoff. In the second stage, we ask whether workers adjust their labor supply in response to this change.

To complement this strategy, we also implement a standard difference-in-differences (DD) analysis with the goal of studying the response of workers farther away from the discontinuity. In this case, we run regressions of the following form:

$$Y_{it} = \alpha_i + \gamma_t + \sum_{t \neq Aug'13} \beta_t^k (I_t \times T_i^k) + u_{it} \tag{1.3}$$

$$T_i^1 = \begin{cases} 1, & \text{if } R_i \in (10k, 15k] \\ 0, & \text{if } R_i \in (15k, 25k] \end{cases}$$

where Y_{it} is the same as before, I_t are indicators for time, and T_i^k is an indicator for whether i is affected by the reform. We normalize $\beta_{Aug'13} = 0$ so that these estimates can be interpreted as the change in earnings relative to August 2013 when the tax holiday was implemented. The identification assumption is that the outcomes of workers in different buckets would have trended similarly in the absence of the tax cut. We present the results graphically because it is more transparent and it is an easy way to test for parallel pre-trends.

Identification Checks

A fundamental identifying assumption for the RDD is that R_i must be as good as randomly assigned in the neighborhood of $R_i = c$. This may be violated if individuals can exactly control the value of R_i and therefore the location relative to the threshold. If individuals are strategically locating above or below the threshold to benefit from the tax cut, we would expect bunching on whichever side of the discontinuity is preferable (in this case the left side to escape the tax entirely).

Figure 1.2 plots the distribution of the running variable to visually test for this threat. Reassuringly, wage earners did not sort in the neighborhood of the thresholds as there is no bunching in the number of wage earners just below 15k. Another important observation is that wage earners do not seem to bunch at the first kink point of the income tax where tax liability starts, denoted by the first red vertical line in the figure. The absence of bunching at the first kink already suggests that the overall response of wage earners to the income tax ought to be small.[26] In the figure, one can also see that relatively high-income workers are subject to the income tax (those to the right of the first kink). The mass of workers between the first kink and 15k are the ones that go tax exempt for 2.5 years, and the mass of workers above 15k end up paying more taxes after 2.5 years due to inflation and the bracket creep.

Although not visible, the data present spikes at some round numbers that serve as focal points (e.g., base pay pre-determined in the pay scales of labor unions). This is a standard feature of administrative wage earnings data (see for example Dube, Manning, and Naidu (2018)).[27] We argue that this is a fluke of the data and does not pose a threat to our empirical strategy for the following reasons. First, the data we use to construct the running variable were reported before the reform was put in place, so it is virtually impossible to manipulate a firm's payroll tax return to game the system. Second, firms face no economic incentives to cheat as the statutory burden of the income tax falls completely on the worker.[28] Third, the firm is subject to high penalties by the IRS if audited and discovered misreporting. Fourth, there is no visible missing mass to the right of the spike meaning that bunchers could potentially be located to the left or the right.

[26] This result is consistent with empirical findings in other countries such as the U.S. where Saez (2010) finds evidence of bunching at the first kink of the income tax for self-employed workers but not for wage earners.

[27] Dube et al. (2018) interpret the bunching at round numbers and symmetry in the missing mass as a combination of labor market power and employer mis-optimization.

[28] In fact, informal conversations with accountants suggest that manipulation was not in the interest of the firm.

A formal way to convince the reader that the spikes are unrelated to the reform is to perform a manipulation test at other focal points close to 15k, and show that we pass the test when we exclude the spike. As a result, we do not think these spikes reflect sorting to escape the income tax. The group of workers reporting data are observationally different than other workers around: more likely to have a contract as executives or managers, less likely to work overtime, and less variability in their earnings over the year. In Table 1.4, we perform the RDD Manipulation Test based on discontinuity in density using local polynomial techniques (see Cattaneo, Jansson, and Ma (2018)). The null hypothesis is that there is no manipulation of the density at the cutoff. We cannot reject the null hypothesis of no manipulation, and thus offers support in favor of the RDD (p-value is 0.8105). Our results suggest that there is no statistical evidence of systematic manipulation of the running variable

Another requirement for identification is for workers just below and just above the discontinuity to be comparable. If people are not sorting in the neighborhood of the threshold, we would expect the distribution of pre-determined characteristics X_i to be smooth around 15k. This motivates a test for whether a discontinuity in average X_i exists at $R_i = 15k$. In Figure A.9 we show, however, that there is no visible discontinuity in the age, gender, marital status, and number of children of wage earners at 15k.

The graphical evidence presented suggests that incumbent workers are comparable around the cutoff and that they could not game the policy by modifying past wage earnings to take advantage of the tax cut after it was announced. This finding is crucial for the validity of the subsequent RDD analysis.

1.6 First stage and salience

First stage change in marginal and average tax rates

For our research design to work, we must first show that the tax burden changes sharply around the discontinuity. Recall that since we are not using income tax data, it is not possible to formally test for this. However, we can use our tax calculator to impute tax liabilities at the worker level and empirically show how large the first-stage effects are.[29] We believe our exercise is a good approximation (if not perfect) of the true tax withheld, before and after the reform, for various reasons. First, the earnings variable that we have in the data is the one that employers actually use to calculate tax withholdings. Second, we observe whether the worker has spouse and dependents to compute their personal exemptions. Third, we already showed that workers were no able to manipulate the running variable. Fourth, all the workers below 15k had incentives to enforce and claim the income tax exemption after the reform, which implies that their tax liability will drop to zero. Fourth, the policy rule was transparent and based on prior wage earnings, and salaries are third-party reported by employers. Hence, the tax agency could easily cross-check whether workers to the right of

[29] To go from pre-tax gross earnings to taxable earnings, we subtract 17 percent for social security contributions, personal exemptions, other minor deductions using the values reported in the law.

15k were cheating to pay lower taxes. Moreover, cheating would be something hard to do and coordinate because it would require some sort of collusion with employers, who are in charge of withholding and filing taxes on their behalf.

Figure 1.3 shows marginal tax rates (panel a) and average tax rates (panel b) before, during, and after the tax change. This is done for single workers with no children, but a similar pattern emerges for other groups. The brown line shows the tax rates in August 2013 before the tax holiday began, the blue lines correspond to December 2014 and 2015 when the tax change was in place, and the red line corresponds to the tax rates by December 2016 after the tax break was repealed. We compute the mean of the tax rates by bins of the running variable on the x-axis. Since the running variable is constructed using the highest monthly wage in the first eight months of 2013, there is a distribution of taxes and earnings for each value of the running variable.[30]

Workers below 15k experienced a temporary tax cut on their entire income, and workers above 15k experienced a tax hike due to inflation and the "bracket creep". Panel (a) shows that the marginal tax rate of single workers earning slightly less than 15k pesos went from about 25% to 0%, and panel (b) shows that the ratio of withheld taxes to gross earnings dropped from about 7% to 0% immediately after the tax break was put in place. On the contrary, two years after the reform, the marginal tax rate for workers slightly to the right of 15k increased from 25% to 30%, and the average tax rate went from 7% to 12%. Interestingly, both marginal and average tax rates converged to their pre-reform levels once the tax holiday was reversed. Hence, this graph illustrates how the reform (and lack of indexation) effectively created groups of workers that coexist in the same labor market but face sharply different tax rates. We next show that, in addition to this large first stage, the tax change was also highly salient and well advertised.

A simple and salient tax change

A necessary requirement for a tax change to affect work incentives is to be simple and salient to the worker. Otherwise, one could argue that workers do not react because they are not aware of the variation or it is too complicated to understand. However, we believe this story is unlikely for various reasons.

First, unlike other comprehensive reforms, this one was easier to understand: if the worker was lucky to be below the discontinuity she did not have to worry about the income tax anymore. So no complicated calculations or changes in the tax base were involved. In fact, firm accountants were in charge of computing the assignment variable and enforcing the targeted tax holiday.

Second, in addition to the standard saliency checks reported in other tax studies (e.g., Google searches as shown in Figure A.10 or newspaper coverage as shown in Figure A.11), the tax change analyzed in this paper was unique in that it was widely covered by nationwide TV

[30]This is the reason why the tax schedule does not present the standard piecewise linear shape. See Figure A.8 for a simulation using current monthly wages on the horizontal axis.

channels.[31] The President made a public announcement with live TV coverage in a meeting where the head of the IRS, main labor union leaders, and business associations were present (panels a and b of Figure 1.4). Moreover, a day after the announcement, the IRS issued a memo explaining who was benefited and how to compute the assignment variable. The details of this memo were amply discussed on TV newscasts by journalists and the head of the IRS (panels c, d, e, and f of Figure 1.4).

Third, in Argentina workers can see in their monthly pay stub if they hit the income tax and the amount withheld. In particular, the Executive Order mandated the inclusion of two lines in the pay stub of tax-exempt workers, one line with the amount that should be withheld had the tax holiday not existed, and another line with the same amount credited back. So workers slightly below the discontinuity experienced an immediate increase in their take-home pay between August and September 2013 that was very visible in their pay stub and bank account. This is an important difference to other European tax holidays where workers typically keep paying taxes from the previous year, potentially blurring the incentives to work more during the tax-free period.

Figure 1.5 shows and example of a pay stub from a wage earner working in the private sector who was benefited by the reform. The pay stub corresponds to September 2015, two years after the tax holiday began. Gross wage earnings before taxes and social security contributions were AR$ 15,699.6. This is the number that we observe in the data. We also highlight in yellow the two lines related to the income tax. The first line shows that this worker should have paid an income tax of AR$ 4,487.4, but this amount is exactly offset in the following line due to Decree 1242/2013. With such a tax liability, the marginal tax rate for this worker should have been 31% instead of 0%. Tax savings amount to 28.6% of this worker's gross monthly wage.

1.7 Results

Aggregate response of wage earners

We now turn to the main empirical findings of the paper and analyze labor supply responses of wage earners to the temporary tax holiday. Figure 1.6 displays average annual wage earnings in the year 2015 by bins of the running variable for the pool of wage earners around the discontinuity.[32] From this figure it can be seen that, two years after the tax cut was put in place and right before it was reversed, there is no visible discontinuity in annual earnings around the 15k cutoff. This result suggests that upper-wage earners did not respond neither

[31] Hoopes, Reck, and Slemrod (2015) use Google and Wikipedia searches about the U.S. income tax to show that policy changes and exogenous shocks to tax salience drive taxpayer information search.

[32] Although the data are reported at the month level, the reason why we aggregate earnings at the annual level is because it captures all the concepts received throughout the year beyond regular payments that might respond differently for workers below and above the cutoff (e.g., annual bonuses), and it also absorbs idiosyncratic seasonalities from jobs.

to the tax cut (those to the left of 15k) nor to the tax hike due to bracket creep (those to the right of 15k).

To get a sense on the magnitude of such a small response, in Figure 1.7 we present a thought experiment on what the observed earnings change should have been with a labor supply elasticity of 0.3, which is in the ballpark of what other papers have estimated (e.g., see the meta analysis by Keane (2011)).[33] For comparison, we superimpose the simulated response (blue dots) to the observed response (gray dots). The figure clear shows that if the elasticity were 0.3, it would deliver a large visible discontinuity in annual earnings. Moreover, the reduced-form estimate computed by comparing workers to the left and to the right of the discontinuity would be AR$ 20,595 additional annual earnings (about 3,500 dollars), which is significantly higher than the observed response of AR$ 638.[34]

The span of our data allows to run the analysis for some years before (2011 and 2012), during (2014 and 2015), and after (2016 and 2017) the reform. The two years before the reform serve as a placebo test, and the two years after the repeal allow to test for asymmetric responses when the tax holiday is gone and workers below the discontinuity start paying taxes again. A convenient way to visually detect small changes is to use earnings growth instead of earnings levels as the dependent variable. In Figure 1.8 we present average growth of annual earnings relative to 2013 within equally spaced bins of AR$ 500.[35] For comparison, we keep the scale of the vertical axis fixed with a range of 10 percentage points. From each panel, corresponding to a separate year, we can see that responses around the discontinuity are close to zero.[36]

For completeness, we compute the RD estimates and 95% confidence intervals in each panel of Figure 1.8, and we plot their evolution over time in Figure 1.9. Reassuringly, the RD estimates are statistically zero before the reform came into force, which reinforces our research design. More importantly, the time series shows a very precisely estimated small increase in earnings in 2014 and 2015, that fades away in 2016 and 2017 when the tax holiday was repealed.

In column 1 of Table 1.5, we report the RD estimates and standard errors for the year 2015. Panel A shows the reduced-form percentage change in earnings around the discontinuity, panel B shows the percentage change in the net of marginal tax rates, and panel

[33]Earnings are shifted by $0.3 \times \%\Delta(1 - \tau_{it})$, where τ_{it} is the individual empirical marginal tax rate before (August 2013) and after (December 2015) the reform. Note that predicted earnings above the discontinuity decrease because of the bracket creep effect. We assume no income effects.

[34]RD estimates throughout the paper are computed with `rdrobust` routine from Calonico, Cattaneo, Farrell, and Titiunik (2017).

[35]Earnings growth is computed at the individual level and the averaged within bin. Note that the denominator, annual earnings in 2013, is positive and relatively large because by construction we consider wage earners paying taxes in 2013. Hence the growth rate does not have large outliers over time but, for precaution, we winsorize it at the 99th percentile.

[36]The reason why annual earnings growth is negative in the figures is because we include workers with zero earnings. That is, the base year 2013 contains workers with positive wage earnings in the reference period of January-August 2013 and we replace pre-reform and post-reform wage earnings with zeroes if the worker is out of the labor force. In that way, the analysis captures the intensive as well as the extensive margin.

C presents the elasticity which essentially scales the reduced-form by its first stage. The elasticity is calculated using a two-stage fuzzy RD procedure.[37] From panel A, we can see that workers benefited by the tax holiday, present an excess earnings growth of 0.4%, which translates into a precise small elasticity of 0.017. For comparison, the elasticity of 0.3 that we used in the thought experiment, would deliver a reduced-form excess of earnings growth of 7.5% in Figure 1.9.

To close this subsection, we analyze whether the small response is driven by relatively rigid or flexible components of a worker's compensation. Recall that employers report total wage earnings and they also break it up into some subcomponents such as base salary, overtime pay, productivity bonuses, vacation pay, etc. The data that we have access to, allow us to look at each of this subcomponents but only in April and October of every year. At the annual level, however, we have access to total wage earnings and base salary. The difference between these two measures thus captures any compensation that the worker gets beyond the base salary during the year. This "residual" is an outcome that the worker could presumably adjust more flexibly relative to the base salary, which is typically predetermined by labor unions. Thus, we break the aggregate result from Figure 1.9, based on total wage earnings, into two subcomponents: the base salary and the residual. To avoid dealing with zeroes, we first compute the share of these two outcomes in total wage earnings at the individual level, and then estimate the RD coefficients.[38] The evolution of the RD estimates reported in Figure A.14, suggests that the small aggregate response is explained by relatively flexible components of wage earnings. The residual gains participation in total compensation relative to 2013 (panel b) and the base salary loses participation (panel a).

To sum up, the evidence presented in this subsection confirms a precisely measured tiny response of wage earners to a large, salient, and temporary income tax change. This effect is driven by flexible components of workers' compensation. Note that since the aggregate response is small, it is virtually impossible to be masking heterogeneous responses from large subgroups, otherwise it would show up in the RD. Nonetheless, the aggregate response could still be masking large responses from small groups or pay components that represent a low share of total wage earnings. In the following subsections we zoom in on outcomes and subgroups where responses could be larger.

[37] For the change in the net of tax rate, we adopt a conservative approach and use individual-level marginal tax rates in August 2013 (pre) and December 2013 (post). Using marginal tax rates in 2015 could potentially capture behavioral responses of workers above the threshold because the bracket creep makes them face higher taxes during the period of analysis. In any case, using marginal tax rates in 2015 would make the first stage even larger and the elasticity even smaller.

[38] About 5% of the sample in the RD had zero residual (i.e., total wage earnings equal to base salary). In addition, some workers have low residual values and therefore interannual growth can become extremely large. That is why we find more natural to work with shares in this exercise.

A more flexible real margin: overtime hours

Overtime work is an important yet relatively unexplored margin of response to taxation. In our setting, overtime is a particularly interesting outcome because it is subject to more discretion from the point of view of the worker. One could argue that even if an employee works under a 8-hours/day rigid contract, there could be some space to choose how many overtime hours to supply. In that sense, it is expected to be more flexible than straight-time hours and therefore more likely to respond to tax changes. Note, however, that labor demand restrictions could still be at play as overtime hours are costly for employers who must pay a premium of 100-150%.[39] Although administrative data typically do not contain information on working hours, in our data we do observe *overtime hours* and *overtime pay*, as employers are required to report them every month when they file social security contributions.[40] The availability of such rare outcomes provide a unique opportunity to learn about their response to taxation using our clean design. In addition, it is the only "pure" real labor supply measure that we have at hand and thus it plays a key role in the analysis.

We start by showing a precise zero effect of the tax holiday on the likelihood of working overtime, and then proceed to the intensive margin where we find a positive, albeit small effect on hours. For the first outcome, we use an indicator for whether workers have positive overtime pay which is available in April, August, and October of every year. Figure 1.10 panel (a) plots the fraction of workers doing overtime by bins of the running variable and fits a quadratic line on each side of the discontinuity for one month-year before the reform (April 2013), one month-year during the reform (April 2015), and one month-year after the repeal (April 2017). This is a very important figure as it shows that overtime is a very common practice among workers affected by the reform (40% overtime in April 2013), but the tax holiday does not seem to have any effect on the extensive margin as no visible discontinuity emerges at 15k.[41] Panel (b) plots the evolution of the RD estimates and 95% confidence interval computed at every month for which overtime pay is available. The precise zero result from this graph implies that the tax holiday did not induce wage earners below the discontinuity to start working overtime.

We next turn to the intensive margin response of overtime hours to taxation where we find positive but small effects (Figure 1.11). In this case we only have access to overtime hours for every April and October of each year. The dependent variable in the RD is the difference between overtime hours in each month-year and overtime hours in April 2013. Panel (a) shows the comparison between October 2015 and April 2013 by bins of the running

[39] See section 1.2 for more details on the regulation of overtime work.

[40] With these variables at hand one can also construct the overtime premium and back out hourly wages for overtime workers. These outcomes allow to study the standard economic incidence channel: whether the tax change depressed hourly wages in response to an increase in hours. We address this threat later in the paper.

[41] Note that the profiles shift down across years due to the business cycle, but do not shift differentially for workers below and above the discontinuity. In appendix Figure A.15 we report summary statistics for the universe of wage earners. About 17% work overtime with an average of 25 overtime hours per month and a participation in total wage earnings of 13%.

variable and fits a linear regression on each side of the discontinuity. This is the month-year in which we get the highest effect and shows that tax-exempt wage earners worked 1.12 more overtime hours in October 2015 relative to workers that remained taxed.[42] Panel (b) plots the evolution of the RD estimates computed at every month for which overtime hours are available. We can see that before the reform, overtime hours do not differ around the discontinuity, but a positive effect slowly emerges as soon as the tax holiday is put in place, and it decreases smoothly after the reform is reversed.

In Table 1.5 column 2, we report the RD estimates and standard errors for 2015. We scale the reduced-form change in hours by average overtime hours in April 2013 (26.3 hours at 15k), and then compute the elasticity by scaling it again with the percentage change in the net-of-MTR. The elasticity of overtime hours to taxation is 0.184. This is much larger than the wage earnings elasticity of 0.017 reported in the previous subsection. Nonetheless, it would still be small if we included (unobserved) *straight-time* hours in the computation of the percentage change.

To sum up, the previous exercise showed that overtime is common, 40% work overtime, so in principle the standard labor supply model would say that wage earners have room to increase hours in response to tax changes, and they do, but just a little bit. This could mean that labor demand restrictions are at play such that workers are not free to vary the number of hours that they work (e.g., many facilities require some fixed level of overtime from employees in order to run operations continuously).

It is striking how little evidence there is on overtime responses to taxation. Two reasons for this are that it is an outcome rarely reported in administrative data and it is hard to find good identifying variation.[43] The scant economic literature that has analyzed overtime is entirely based on labor surveys. To the best of our knowledge, the only empirical paper on this topic is the one by Cahuc and Carcillo (2014) that studies labor supply responses to the detaxation of overtime pay introduced in France in 2007 using survey data and a diff-in-diff design.[44] In that sense, the results from this subsection are itself interesting and provide important lessons for other countries that view overtime hours as an effective way of increasing the number of hours worked (e.g., Austria, Belgium, and France).

[42] The reason why the dependent variable is negative is because we include the zeroes and by construction the sample at the baseline, April 2013, has positive wage earnings.

[43] Brown and Hamermesh (2019) argue that U.S. overtime laws do not provide as fertile a field for evaluating policy as the regulation of wages and, therefore, it is not surprising that very little research on overtime has been produced in the U.S. in the last decade.

[44] A theoretical treatment of overtime decisions to tax reforms can be found in Frederiksen, Graversen, and Smith (2008). Overtime responses to net wage changes have also been explored earlier in the 1990s by Trejo (1991) and other work recently summarized by Brown and Hamermesh (2019). However, these papers are mainly focused on the effect of overtime pay provisions on total hours and the incidence on straight-time hourly wage through demand-side forces.

Two responsive subgroups: job switchers and managers

In this subsection we focus on two subgroups that are expected to be more responsive to taxation: jobs switchers and managers.

Job switchers

This is an interesting group because the tax status of workers moving to a new firm was still tied to monthly wages earned in the previous job (the highest monthly wage between January and August 2013). Hence, it could be the case that workers could not get paid more on the current job because the contract was already written, but perhaps they could switch to another job that paid better (and may require more work).

Figure 1.12 presents the RD analysis for the likelihood of switching (extensive margin) and the excess of earnings growth (intensive margin) around the discontinuity. We define switchers as workers with a different firm identifier by December of a year relative to the firm identifier they had when the reform took place in 2013. Panel (a) plots the fraction of switchers by bins of the running variable for two years during the tax holiday (2014 and 2015) and two years after (2016 and 2017).[45] It also includes a quadratic fit on each side of the discontinuity. Panel (b) plots the evolution of the RD estimates for wage earnings growth relative to 2013. The green line corresponds to wage earners that switched firms by December 2015 and the blue line corresponds to wage earners that stayed in the same firm by December 2015.

From Figure 1.12 we have that the tax change did not affect the likelihood of switching jobs, but conditional on switching, it seems that tax-exempt workers negotiated monthly wages more favorably. Table 1.5 column 3 reports the point estimates and wage earnings elasticity for the year 2015. The elasticity for this subgroup is 0.096, an order of magnitude larger than 0.017 estimated for the pool of wage earners, but still quite small when compared to other estimates in the literature. Our evidence thus suggests that employees switching jobs seem to negotiate new contracts differently based on their income tax status. These findings for switchers versus stayers might imply that wage earners are constrained. That is, they do not get to choose how many hours they work over the course of the year. Workers are basically stuck in a job in which the contract states how much they work and how much they earn, making it hard to adjust their labor supply in response to net wage changes.

Managers and executives

Another interesting case study is given by executives and managers because they have a broader income base to respond to tax changes than the typical employee, and they are closer to the board of directors who set their pay. In most organizations, the compensation mix for executives usually differs from other workers' pay. The package typically includes a

[45] By construction, the measure in 2015 includes workers switching in 2014 and 2015, the measure in 2016 includes workers switching in 2014, 2015, and 2016, etc.

fixed part and variable part. The fixed part consists of a regular monthly salary (accounting for 50-70% of total income) and the variable part can include honoraria, annual bonuses, profit shares, equity shares, etc.[46] For the fixed part, employers must withhold income taxes at source and, for the variable part, managers must file an annual tax return as independent workers.[47] Key to our analysis, if the portion paid as wage earnings was below the discontinuity then executives qualified for the tax holiday on anything paid as wage earnings. Hence, this group had incentives to shift their compensation mix toward wages because they remained untaxed during 2.5 years. Furthermore, this practice did not entail a higher labor cost for employers, because executives usually make social security contributions as independent workers and thus anything paid as wage earnings is exempt from employer and employee payroll taxes. So, for this peculiar subgroup, wage earnings adjustments were easier to accommodate.

In Figure 1.13 we analyze wage earnings responses from managers and executives. In our data, employees performing managerial duties are reported under a different type of contract than the rest of wage earners and thus are easy to flag. This is because their wage earnings are not subject to payroll taxes and thus the IRS uses a separate category for this group. We use this flag to identify them and then track their annual wage earnings for the period 2011-2017. In this case we run a difference-in-differences analysis because the RDD is somewhat underpowered due to a small sample size.[48] In the treatment group we include managers with a running variable between AR$ 10,000 and AR$ 15,000 (untaxed), and in the control group we put managers with a running variable between AR$ 15,000 and AR$ 25,000 (taxed). Panel (a) shows average annual earnings for both groups and panel (b) reports the evolution of the diff-in-diff estimates using wage earnings growth relative to 2013 as the dependent variable.

Figure 1.13 shows parallel trends before the tax holiday was put in place and, more importantly, a sharp increase in the wage earnings of managers located in the tax benefit zone relative to those that kept paying taxes normally. The reduced-from increase in wage earnings builds up slowly, reaching almost 8% in 2015, and fades away smoothly when the tax change is reversed.[49] The point estimates for the year 2015 are reported in Table 1.5 column 4. The wage earnings elasticity for managers is 0.311, which is significantly higher than the 0.017 estimated for the pool of wage earners.

This finding is important, as it shows that among the group of overall unresponsive wage earners, managers seem to be quite responsive to income taxation. This result can be linked

[46] For instance, a common practice in Argentina is for managers to receive honorarium payments in advance during the year, that the firm formally recognizes as an expense in April of the following year before the fiscal calendar ends. These payments are treated as self-employment income and thus taxed under a different regime.

[47] Capital gains of non-publicly (privately) traded stocks were taxed at 15% and capital gains of publicly traded stocks faced no tax. In addition, dividends faced a temporary 10% tax between September 2013 and July 2016.

[48] The RD results are similar but noisier and can be found in Appendix Figure A.16.

[49] The reason why such large effect is not visible when using the full sample, is because managers represent a small share of total wage earners affected by the tax change.

to recent work by Piketty, Saez, and Stantcheva (2014) who decompose the taxable income elasticity into real responses, bargaining effects, and avoidance behavior. These authors argue that the negative relationship between marginal tax rates and CEO pay is likely due to bargaining effects (i.e., stronger bargaining of top earners when top rates are low). Whether our large response is due to bargaining/reporting effects rather than productive effort remains an open question. Nonetheless, the closer ties with the board of directors, the broader compensation package, and the favorable tax treatment of wage earnings make the avoidance story more plausible.

Other subgroups

The richness of the data allows us to zoom in on other subpopulations that are typically considered to have more flexibility in their labor supply choices. For example, public servants usually face a relatively fixed working schedule than private sector workers (e.g., in some ministries it is not possible to work overtime). Similarly, the literature typically finds larger elasticities for women than men (Keane, 2011).[50] Workers not covered by labor unions could adjust their working hours more easily. The elderly close to retirement could delay that decision to take advantage of the tax holiday. The coordination between workers and employers could be easier in small- or medium-sized firms. Workers with non full-time contracts might have more space to respond, etc. Nonetheless, as pointed out before, since the aggregate response in the RD analysis is small, it is virtually impossible to find a large subgroup responding to the tax holiday.

We run the RD analysis for these subgroups of workers for the year 2015 and summarize the wage earnings elasticities and confidence intervals in Figure 1.14. Panel (a) displays the elasticities for different demographic groups and panel (b) breaks the aggregate result by employment characteristics. From panel (a) we have that women, married employees with children, and the youth are more elastic. Panel (b) shows a higher elasticity for workers with non full-time contracts (e.g., this group includes managers and executives) and workers at small- and medium-sized firms where coordination might be easier. Nonetheless, all these magnitudes are pretty small overall.

1.8 Entry effects

We now show that the tax change induced some high-wage earners entering the labor market to do so strategically below the eligibility threshold to qualify for the tax holiday. To that end we construct the sample of "new entrants" as those that were not present in our data between January and August 2013 and appear afterwards (i.e., non-wage earners in the reference period). We also construct a "placebo sample" for the pre-reform period with wage earners that were not present in our data between January and August 2010 and appear

[50] Chetty et al. (2011) show that bunching at kinks is larger for married women than for single men. Gelber (2014) also finds higher elasticities for married women than men.

afterwards.[51] It is worth noting that, in principle, these samples not only include entrants without an employment history, but also those that for some reason were unemployed during the reference period, or those that were employed under a different regime (e.g., the simplified regime for independent workers, general regime for firm owners).

One natural way to show the entry effects would be to use the bunching to notches approach (Kleven and Waseem, 2013), where one compares the excess of mass below AR$ 15,000 and the missing mass above AR$ 15,000.[52] There are two factors, however, that complicate its use. Both factors are depicted in Figure 1.15 where we plot the distribution of the starting monthly wage for all the entrants in the pre-reform year 2013 (blue line) and the reform year 2015 (red line). The first factor is that 15k was a very high monthly entry wage in 2013, leaving very little mass around the notch when the reform was implemented (only 2% of entrants above 15k).[53] Hence, in practice, this threshold was only binding for top wage earners with the potential to enter in that zone. The second one is that, although the notch is nominally fixed at 15k, and we do observe some bunching at the notch (panel b), it is hard to apply static bunching techniques in an inflationary context because the distribution and its composition shift constantly to the right as labor unions and firms renegotiate nominal wages to keep up with inflation.[54]

An alternative way to show the entry effects, which circumvents low frequency at 15k and dynamic adjustments in the distribution, is to work with the cumulative distribution function instead of the probability density function. At every month-year of our data, we compute the share of workers entering above 15k and then plot the time series in Figure 1.16. This strategy provides a visual test of the speed at which the mass accumulates above 15k over time. Of particular interest is the repeal of the tax holiday in February 2016, because by that date the 15k threshold had become more operative and, therefore, one would expect a trend break in the fraction of wage earners entering above 15k before and after the policy was reversed.

Three elements are worth noting in Figure 1.16 panel (a). First, it was indeed a rare event to enter the labor market with a monthly wage higher than AR$ 15,000 when the reform was implemented. Second, between September 2013 and February 2016, the blue line takes off and the red line remains relatively stable, meaning that wage earners were mostly entering in the 10k-15k zone. Third, the red line shows a trend break when the notch was removed in February 2016, which means that all of a sudden workers started entering above that threshold (now irrelevant for tax purposes). Panel (b) repeats this exercise for managers

[51] We restrict the sample to workers that stay at least 5 months in the new job and work in at most 3 firms during the period of analysis. This is a sensible condition that lets us exclude seasonal workers entering the labor market for a few months and that are not affected by the income tax.

[52] Recall that new entrants were fully exempt from the income tax if their monthly wage at the starting month was lower than AR$ 15,000, regardless of subsequent income. Hence, the region above 15k was strictly dominated.

[53] In August 2013, the average entry salary was AR$ 5,200 (AR$ 8,850 in 2015; AR$ 11,200 in 2016) and the overall average monthly wage was AR$ 8,200.

[54] According to the official wage index, RIPTE, the average nominal increase of wage earnings was 32% in 2013-2014, 33% in 2014-2015, 32% in 2015-2016, and 29% in 2016-2017.

and executives, a peculiar group that presented the largest responses on the intensive margin analysis. Strategic entrance in this case is even more striking with sharp changes in the CDF at the two key dates.

In Figure 1.17 we refine the previous analysis by constructing a counterfactual CDF (blue line), computed based on annual inflation, that is superimposed to the observed CDF (red line). For this predicted share, we take the distribution of initial monthly wage earnings in 2013, we shift it backward and forward in time using the Argentine wage index (RIPTE), and then we compute the share of wage earners that fall above AR$ 15,000. Panel (a) shows the shares in levels and panel (b) reports the excess in the number of predicted workers above 15k relative to the observed number of workers above the notch. The estimates reported in panel (b) mean, for example, that in 2015 the predicted number of workers entering above 15k is 30% larger than the observed fraction. The figure shows that the gap between the prediction and the observed entrance increases during the tax holiday and it decreases when the tax change is reversed. We interpret this result as evidence that workers entered strategically below the notch to avoid the income tax.

We close this section by zooming in on some reference entry points that help to make the wage manipulation story more compelling. In this case, to smooth noise out we calculate the average number of entrants per month. That is, we first count the number of wage earners in each month entering exactly at a focal point and then compute the monthly average for different years. We focus on four reference points: two that qualify for the tax holiday, 10k and 15k, and two that do not qualify, 20k and 25k. Figure A.17 shows a clear response to the tax notch as the mass at 15k increases during 2014 and 2015 relative to the other focal points, and decreases in 2016 and 2017 when the tax change was reversed. In contrast, the mass at 20k and 25k remains relatively stable until 2015 and then increases sharply when entrance below 15k is not advantageous anymore. In Figure A.18 we break the previous figure by executives versus the rest of entrants to show that this manipulation is mostly driven by executives. For instance, in 2015 about 50% of the executives entering in the range of 10k-15k are piled up exactly at 15k, while for the rest of the entrants, the share in that zone entering exactly at 15k is less than 1%. So manipulation of starting monthly wages is higher for executives, a result that is in line with the elastic wage earnings response documented in section 1.7. The responsiveness of this group could be rationalized by non-standard contracts that let them coordinate labor responses more easily with their employers.

To sum up, notwithstanding the low frequency of entrants around 15k when the reform was implemented, the evidence from this section reinforces the idea that high-wage earners were aware of the reform and some were able to manipulate their first monthly wage to enter below the discontinuity and escape from the income tax. This behavior is more pronounced for executives and managers.

1.9 Competing explanations and discussion

The analysis has shown so far negligible responses of high-wage earners to a large tax change in the aggregate, but somewhat larger effects for some flexible outcomes and subgroups. The next natural question is what is driving these results. We discuss and explore some competing explanations.

Lack of saliency

One could argue that workers did not react because the reform was not very salient or well-understood. We believe that the aggregate null result is probably not because of lack of saliency. In section 1.6, we showed that the tax change was highly publicized and the nitty-gritty was amply discussed by the IRS and journalists. Moreover, the unusually large change in marginal and average tax rates, and the mandatory inclusion of the tax credit on pay stubs, made this tax change much more visible than standard tax reforms. Unlike typical tax reforms, our tax variation was simple to understand for workers. It did not require an understanding of the tax code whatsoever, just that if a worker was lucky to be below the discontinuity, she did not have to worry about taxes anymore (at least until the presidential election in December 2015).

Poor enforcement/compliance

We argue that the aggregate null result is not explained by poor enforcement of the eligibility rules. Employers and their accountants, who calculate and file monthly withholdings on behalf of workers, were in charge of computing the running variable, and could face high penalties from the Argentine IRS for placing workers on the wrong side of the discontinuity. Anecdotal evidence suggests that accountants followed the eligibility rules closely to avoid such penalties. In the appendix we present evidence from two anonymous large firms that shared detailed payroll data, and we find 100% compliance around the discontinuity (see Figures A.28 and A.31).

An incidence story

The idea here is that employees are indeed working longer hours but employers lower their wage rate leaving monthly wage earnings unchanged. Figure A.19 presents evidence against this labor demand channel. We compute RD estimates using hourly wages as the dependent variable and plot the evolution for the period 2012-2017. We use a sample of overtime workers, for whom we observe monthly hours and pay, to back out wage rates. The figure shows a very precise zero effect suggesting that the null labor response is hardly explained by an incidence story.

Substitution and income effects cancel out

From the theory we know that income tax changes create substitution effects (SE) and income effect (IE) on work effort that move in opposite direction. Some people believe the SE to be small relative to the IE and others believe the converse. Such differences arise to a considerable extent because of the difficulty of obtaining reliable evidence (Giupponi, 2019). Cesarini et al. (2017) argue that income effects are smaller at old ages.[55] So, do we see that old workers are more responsive, with a large substitution effect only partially masked by a small income effect? In Figure A.20, we break the aggregate RD analysis into four age subgroups, and find evidence against this argument. If anything, the figure shows a slightly higher effect for young workers (panel a).

Frictions and rigidities

The modern economic literature acknowledges that several factors might attenuate observed responses to taxation, such as restricted contractual hours choices and fixed adjustment costs (see Rogerson, Rogerson and Wallenius (2013), Chetty et al. (2011), and Chetty (2012)). Large adjustment costs to changing labor supply (e.g., search costs or adjusting hours of work.) can create slow dynamic responses from wage earners to the tax holiday.

Importantly, Argentina has a highly rigid labor market compared to OECD and other South American countries. In Figures A.21 and A.22, we present cross-country evidence to shed some light on labor market rigidities using comparable data from the World Economic Forum. The Argentine labor market is comparable to central European countries such as France and Italy that are highly unionized. The high rigidities could be one of the factors that limit the response of wage earners to net wage changes.

Labor demand constraints

Labor choices do not occur in a vacuum and usually require some coordination between employers and employees, further limiting a worker's choice set (see for instance Kreiner et al. (2016)). Our larger effects for job switchers, overtime hours, and new entrants might imply that wage earners are labor-demand constrained. In the case of jobs switchers, workers are basically stuck in a job in which the contract states how much they work and how much they earn, restraining the choice of hours of work over the course of the year. Likewise, overtime is a margin that allows for some discretion in hours of work and yet we find relatively small effects. This could mean that labor demand restrictions are at play such that workers are not free to vary overtime hours (e.g., many facilities require some fixed level of overtime to run operations continuously). The larger response of managers and executives could be rationalized by their proximity to firm owners and by a broader compensation mix that let them adjust reported wages and hours more easily than the typical employee. Finally,

[55] In Panel C of Table 5 the authors show that the income effect varies with age and is higher for young workers. The table shows that the income effect is cut in half by age 50.

the strategic behavior from new entrants to dodge taxes would not be possible without coordination with employers. Taken together, our results point toward rigidities in the labor market in which employee-employer cooperation is needed for wage earners to respond to tax changes.

Real low responses

Another potential explanation could be that upper wage earners may indeed have a very low intensive wage earnings elasticity with respect to marginal tax rates. In fact, the results of this paper are consistent with other papers. Saez (2010) finds that labor supply responses in the U.S. are mostly concentrated among self-employed workers but not among wage earners, for which the implied elasticity is zero and precisely estimated. The result is also consistent with the paper by Saez, Matsaganis, and Tsakloglou (2012a) for payroll taxes in Greece and Bastani and Selin (2014) for the income tax in Sweden. Chetty et al. (2011) also estimate very low elasticities for wage earners in Denmark. This is also related to the findings by Zidar (2019) who shows that lower-income groups respond more to tax cuts and that the effect of tax cuts on employment growth for the top 10 percent is small. Another recent literature has looked at earnings responses to thresholds in the social security contributions (SSCs). By exploiting concave kinks in the budget set of workers due to SSC ceilings, it finds no evidence of dips in the distribution of earnings (Alvaredo, Breda, Roantree, and Saez, 2017). This result suggests that taxable earnings for high-income workers are inelastic, at least for those located near the threshold.[56] However, in this cases it could be explained by relatively small changes in marginal rates. However, recent work by Chetty et al. (2013) estimate significant impact of EITC on the intensive margin of low-income employees using differential knowledge across regions of the U.S. They find an earnings elasticity of 0.31 in the phase-in region of the EITC schedule and 0.14 in the phase-out region.

Concluding remarks

Argentina implemented a large and salient income tax cut for wage earners in 2013 that lasted 2.5 years. This paper used a regression discontinuity design and administrative data to analyze labor supply responses of upper-wage earners. Notwithstanding the large and salient change in marginal and average tax rates, we find a precisely measured and very small effect of the tax cut and hike on wage earnings and other labor supply measures (e.g., overtime hours). This finding suggests that upper wage earners were not responsive to a large, salient, and temporary tax change. Our findings could imply that the costs of raising income taxes in economies with more rigid labor markets are not large, at least for the intensive margin and high-wage earners. Nevertheless, this depends crucially on the nature of labor market frictions. If they are permanent, then the statement is correct. But if there

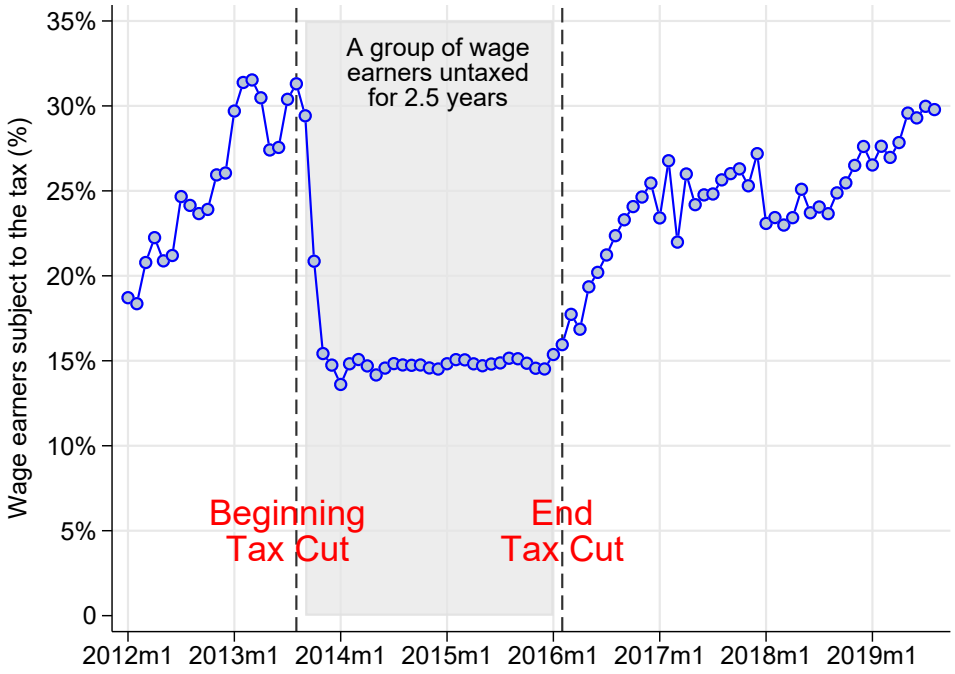
[56]This result holds in the Netherlands (Bosch and Micevska-Scharf, 2017), France (Bozio, Breda, and Grenet, 2017), Germany (Müller and Neumann, 2017), and the United Kingdom (Adam, Roantree, and Phillips, 2017).

are adjustment costs that would be overcome for a permanent change in the tax system, then one cannot use the small short-run responses as a guide for permanent tax policy.

In future research, we would like to explore more rigorously the mechanisms behind this result, study what workers did with such a large windfall (e.g., financial consequences), and analyze aggregate effects in the cities that were more benefited by the tax holiday. We also intend to run a large scale survey on wage earners to learn more about rigidities at the workplace and attitudes toward the income tax.

Figures and Tables

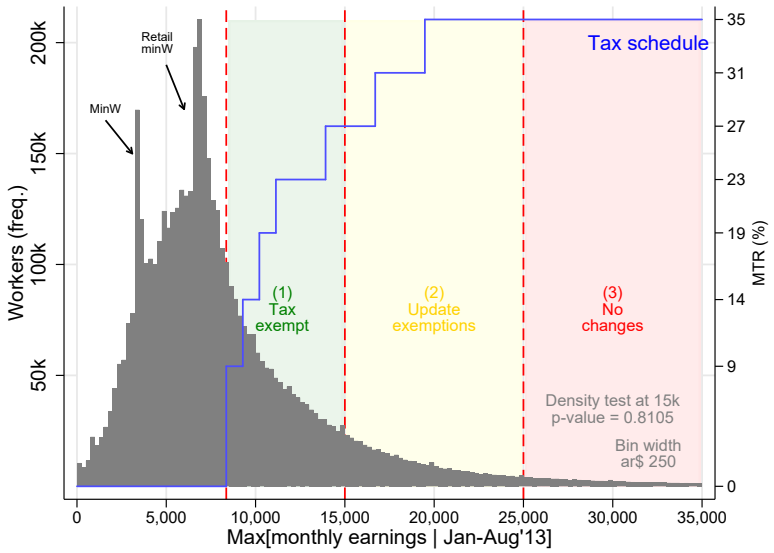
Figure 1.1: Wage earners subject to the income tax (%)



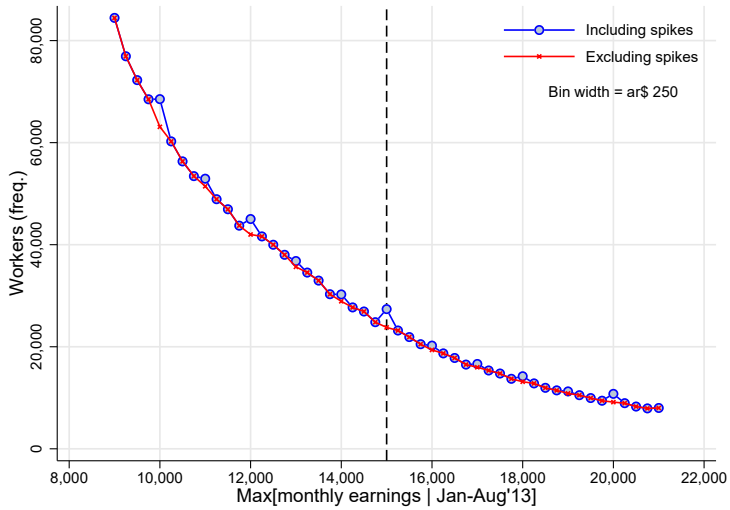
Notes: this figure plots the fraction of registered private and public wage earners with income tax withheld at source by their employer. Vertical dashed lines denote the beginning (September 2013) and the end (February 2016) of the tax holiday (Executive Order 1242/2013). Immediately after the tax change was put in place, the number of wage earners paying the income tax fell from 2.3m to 1.1m. In February 2016, the new administration repealed the tax break and increased the nontaxable income floor to prevent a discrete jump in the number of taxpayers. But with 40% inflation, many workers smoothly crossed the exemption floor and the number of taxpayers reverted to about 2.2m by 2017. Source: official numbers reported in "*Informe del Jefe de Gabinete de Ministros (HCDN)*".

Figure 1.2: Distribution of the running variable

(a) Full distribution



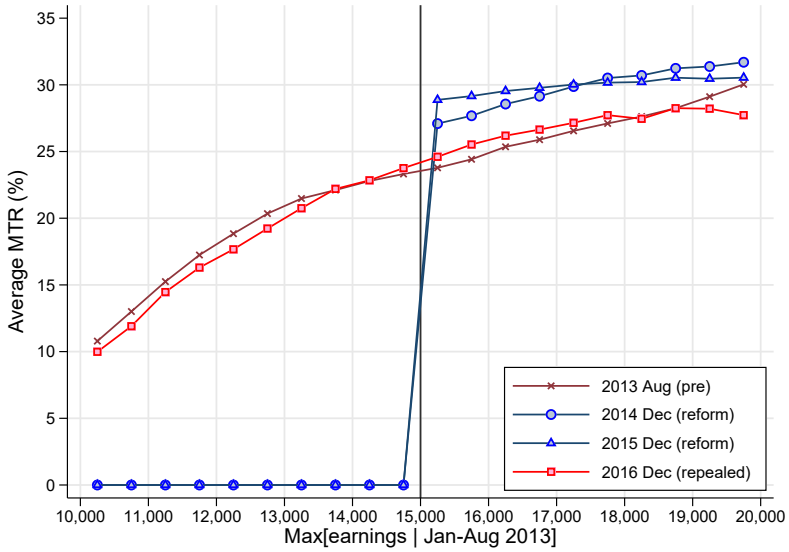
(b) Zoom around 15k



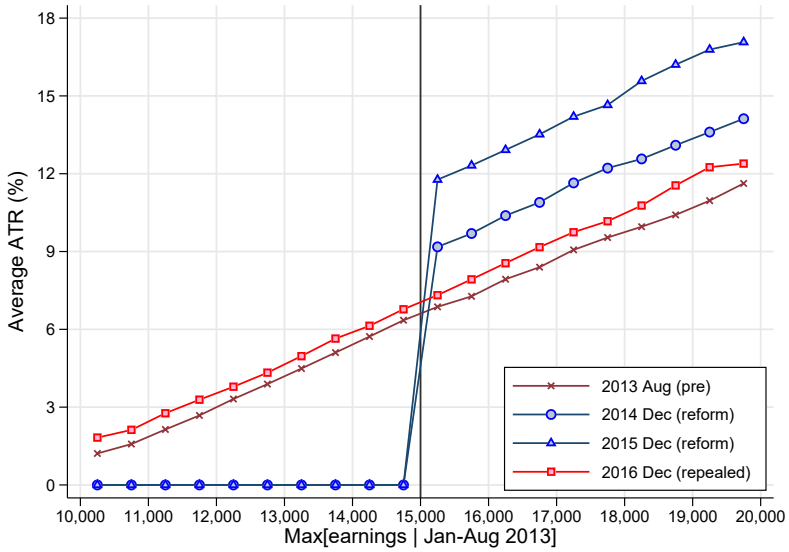
Notes: this figure displays the distribution of the highest gross monthly salary between January and August 2013 (the running variable in the RDD). Panel (a) shows the full distribution up to 35k. The vertical lines from left to right denote the the first kink of the income tax for single workers at ar$8,360 and the two key thresholds introduced by the reform at ar$15,000 and ar$25,000. The fraction of salaried workers the became tax exempt is highlighted in green and the fraction the kept paying the tax are highlighted in yellow and red. The stepwise blue line denotes the tax schedule for single workers without children. Panel (b) shows the distribution of the running variable in the range 9k-21k. The blue line includes the spikes at focal points 10k, 11k, ..., 20k, and the red line excludes these spikes. In both panels the bin width is ar$250.

Figure 1.3: First stage change in tax rates (single workers without children)

(a) **Marginal tax rates**



(b) **Average tax rates**



Notes: this figure shows the empirical first stage change in marginal tax rates (panel a) and average tax rates (panel b) by bins of the running variable (the highest gross monthly wage between January and August 2013). The brown line shows the tax rates in August 2013 before the tax holiday began, the blue lines correspond to December 2014 and 2015 when the tax change was in place, and the red line corresponds to the tax rates by December 2016 after the tax break was repealed. Tax rates are computed using our own tax calculator (similar to the TAXSIM in the U.S.). Workers below 15k experienced a temporary tax cut, and workers above 15k experienced a tax hike due to the bracket creep.

Figure 1.4: Live announcement, interviews, and TV newscasts

(a) August 27th, 2013

(b) August 27th, 2013

(c) August 28th, 2013

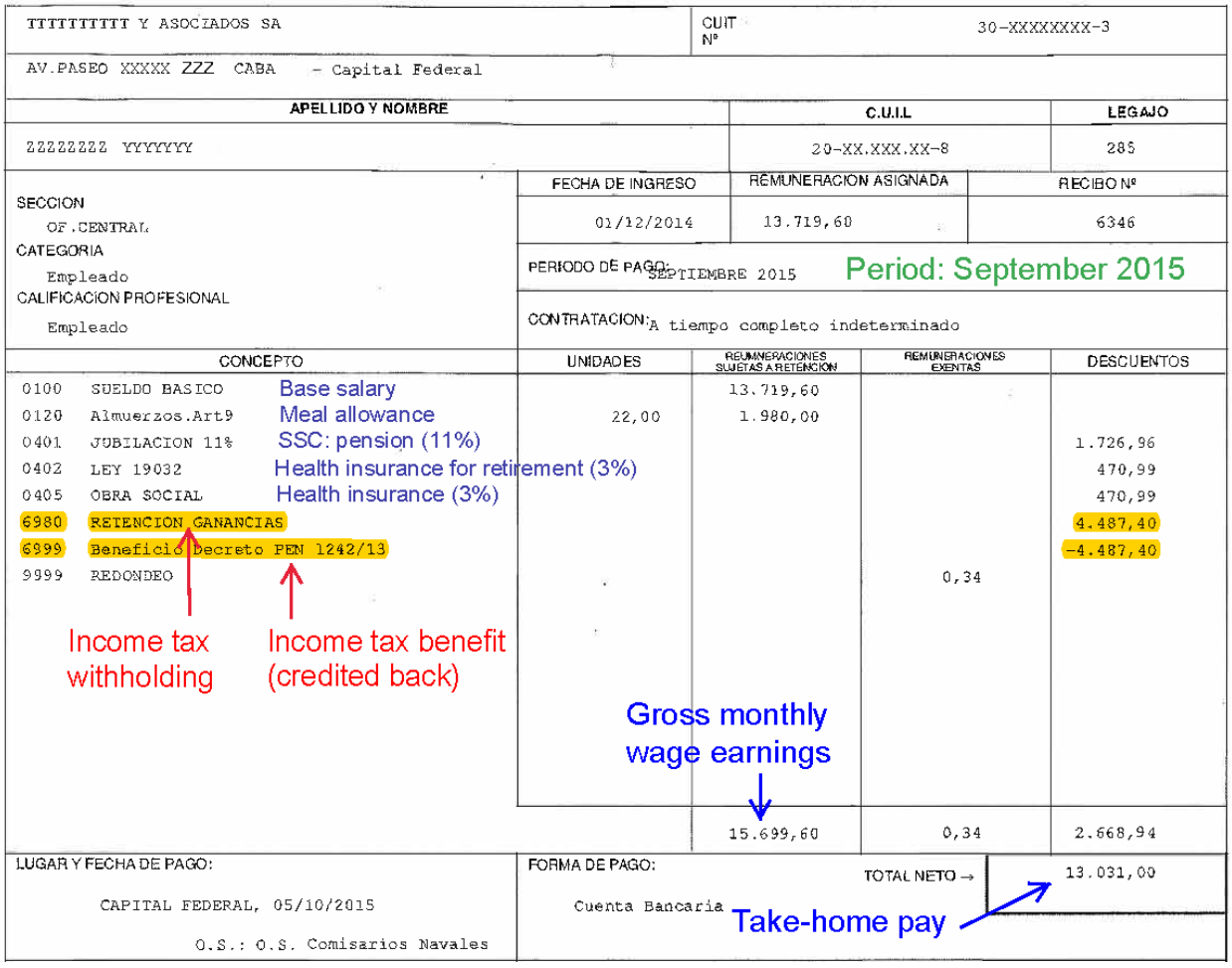
(d) August 28th, 2013

(e) August 30th, 2013

(f) August 30th, 2013

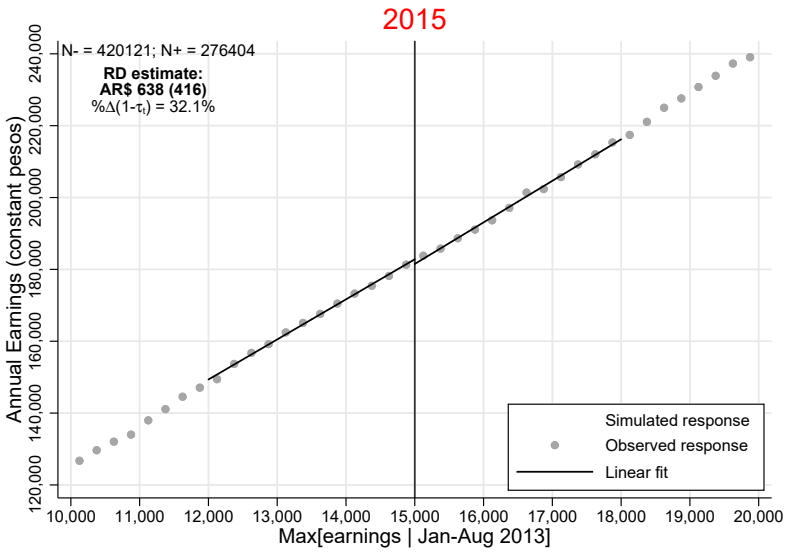
Notes: this picture shows the repercussion that the income tax change ("*ganancias*") had in the Argentine television. Panel (a) shows a photo of the official meeting where the announcement was made with the participation of the President, the head of the IRS, and representatives of employers' organizations and labor unions. This announcement had live nationwide coverage on the main news channels, as shown in panel (b) where the President is explaining the tax change. Panels (c) and (d) show the head of the IRS in a live interview a day after the announcement where he answered questions from the audience and provided some clarifications (e.g., that *"those below 15k are not liable"* -panel c- and that *"it would become operative on September 1st"* -panel d-). In panels (e) and (f) a journalist is explaining the details of the tax holiday. The screen in panel (e) reads "how to compute the AR$ 15,000 threshold" in reference to the assignment variable. Panel (f) explains that annual bonuses are unusual payments and should not be included in the running variable. Source: screenshots from public YouTube videos.

Figure 1.5: Pay stub of a wage earner benefited by the tax holiday (September 2015)



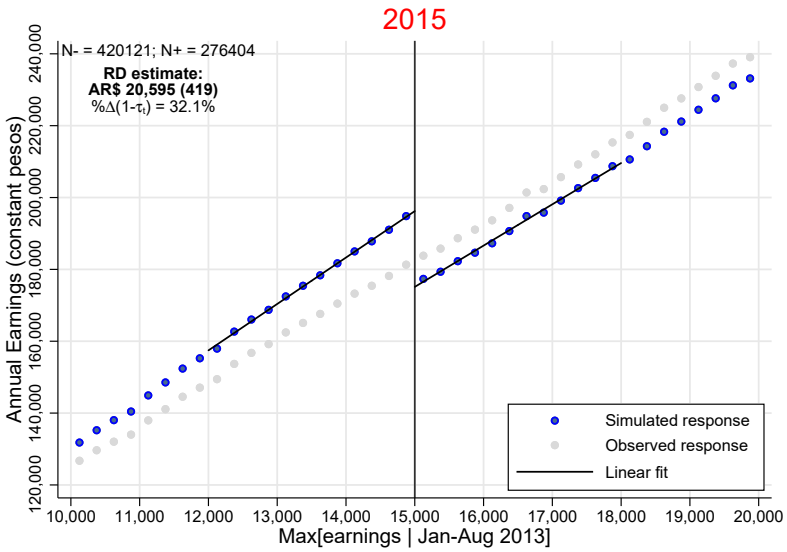
Notes: This figure displays the pay slip of an anonymous wage earner benefited by the reform (i.e., below the discontinuity) in September 2015. The presidential decree contained an article requesting employers to include a line with the income tax withholding of the corresponding month, 'Income Tax Withholding' AR$ 4,487.4, and another line exactly offsetting that amount, 'Benefit Decree PEN 1242/2013' - AR$ 4,487.4, as highlighted in yellow in the figure (see Article 3, Executive Order 1242/2013). This amount represents a large fraction of this worker's gross monthly wage. The exchange rate peso-dollar in September 2015 was 9.24.

Figure 1.6: Observed earnings response after 2 years



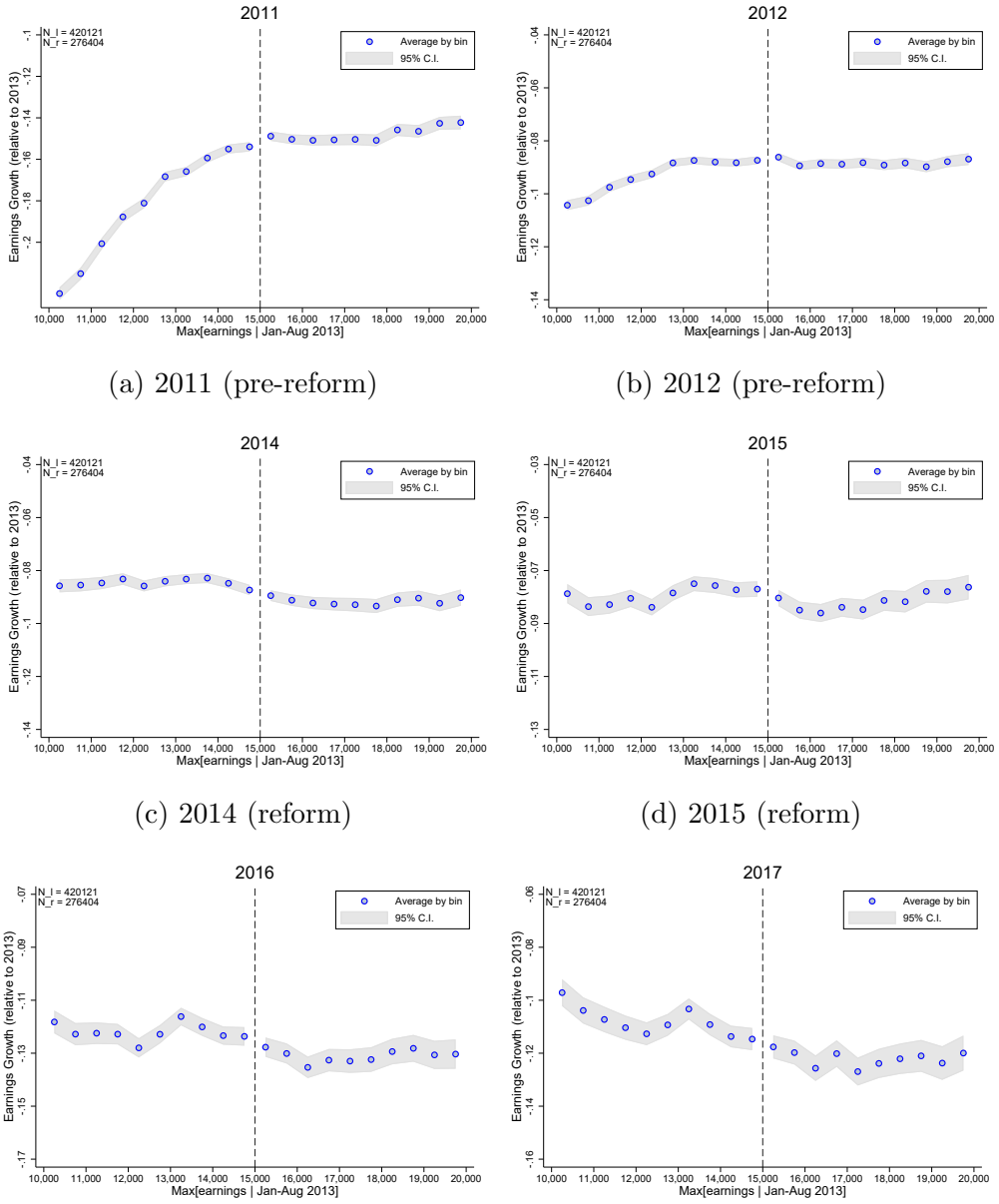
Note: this graph plots average gross annual wage earnings for 40 equally spaced bins of the running variable (width AR$ 250). The figure includes a linear fit on each side of the discontinuity, computed with `rdrobust` routine from Calonico et al. (2017) using a triangular kernel and a AR$3,000 bandwidth. The top left corner reports the number of observations, the first stage change in the net-of-tax rate, and the reduced-form estimate (standard error in parentheses). Wage earnings are expressed in constant pesos from August 2013. The exchange rate peso-dollar was 5.5 in August 2013.

Figure 1.7: Thought experiment: observed vs simulated response



Note: this figures reproduces the observed response from the previous figure (gray dots) and superposes a simulated response in a frictionless world with $e = 0.3$ and no income effects (blue dots). Earnings are shifted by $0.3 \times \%\Delta(1 - \tau_{it})$, where τ_{it} is the individual empirical marginal tax rate before (August 2013) and after (December 2015) the reform. The top left corner reports the observations, the change in the net-of-tax rate, and the reduced-form estimate (standard error in parentheses). RD estimates computed with the `rdrobust` routine from Calonico et al. (2017).

Figure 1.8: RD for excess annual earnings growth relative to 2013



(a) 2011 (pre-reform)

(b) 2012 (pre-reform)

(c) 2014 (reform)

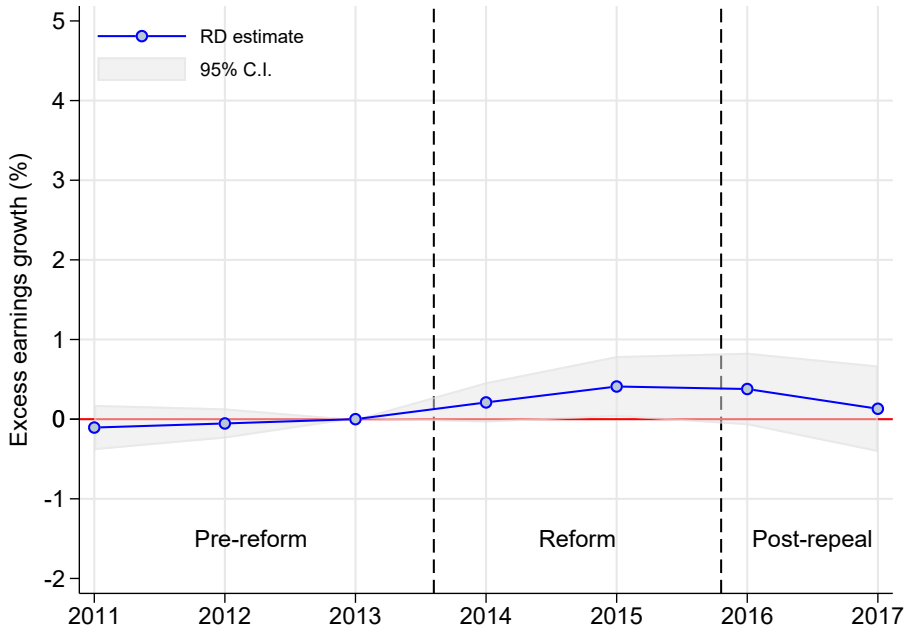
(d) 2015 (reform)

(e) 2016 (post-repeal)

(f) 2017 (post-repeal)

Notes: this graph plots average annual wage earnings growth relative to 2013 for 20 equally spaced bins (width AR$ 500) of the running variable: the highest gross monthly wage between January and August 2013. Panels (a) and (b) correspond to two years pre-reform as a placebo test, panels (c) and (d) correspond to the two years in which the tax holiday was fully in place, and panels (e) and (f) correspond to two years after the reform was repealed. The sample contains private workers with positive wage earnings in the reference period of January-August 2013. All the figures include workers with no wage earnings either before or after the reform was put in place.

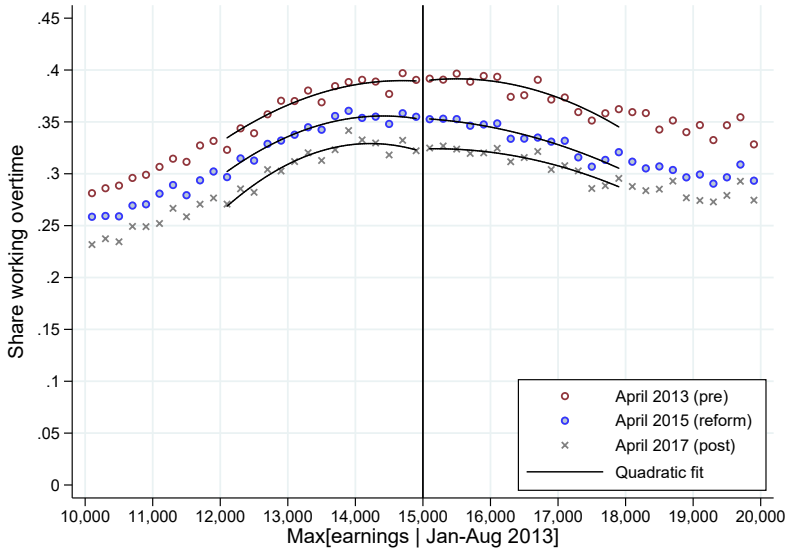
Figure 1.9: Evolution of RD estimates for wage earnings growth, 2011-2017



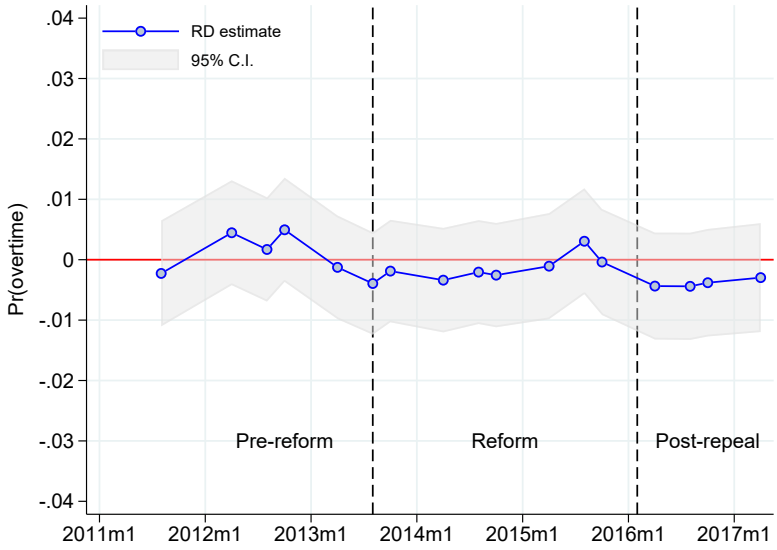
Notes: this graph plots the evolution of the RD estimates computed in each panel of Figure 1.8. Each dot corresponds to a separate standard RD regression using a linear fit on each side of the discontinuity, a triangular kernel, and a AR$3,000 bandwidth. We use the `rdrobust` routine from Calonico et al. (2017). The dependent variable in the RD is annual earnings growth relative to 2013. The point estimate thus measures the excess earnings growth between workers below and above the discontinuity. The vertical dashed lines indicate the beginning and the end of the targeted tax holiday. Note that with an elasticity of $e = 0.3$ (thought experiment), the reduced-form point estimate would be 7.5%. The implied elasticity for the year 2015 is reported in Table 1.5.

Figure 1.10: RD estimates for overtime likelihood (extensive margin)

(a) RD plot for overtime likelihood



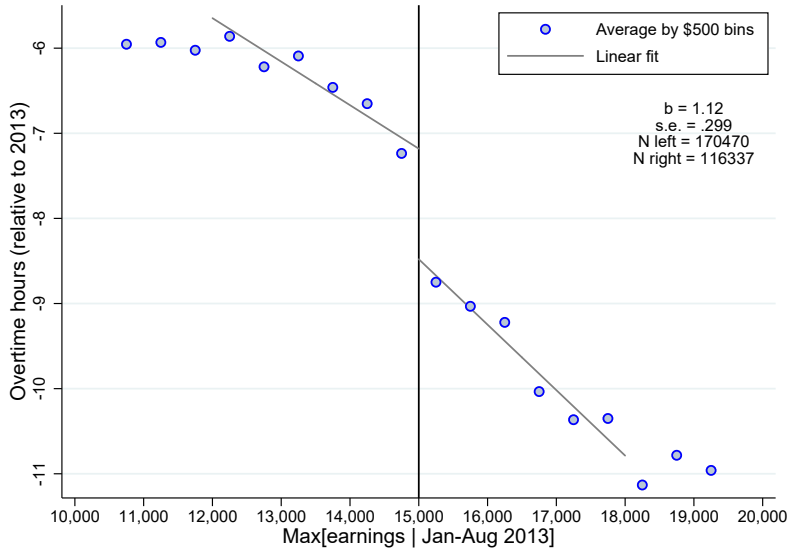
(b) Evolution of RD estimates



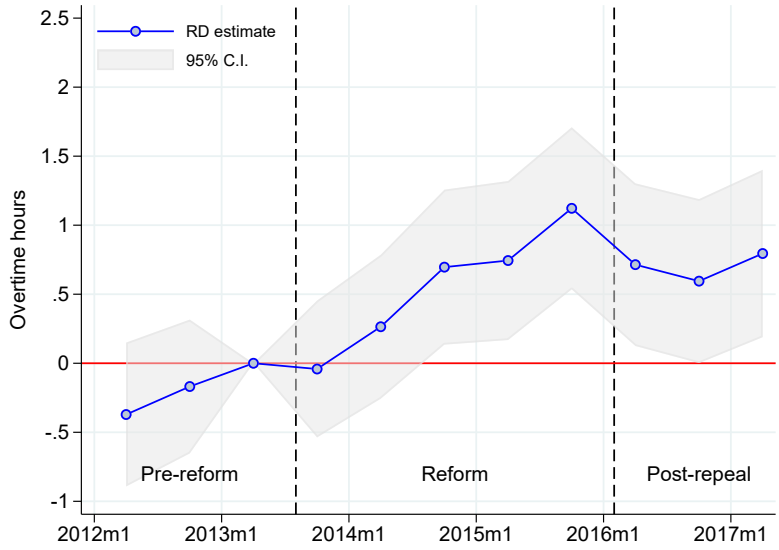
Notes: these figures present the RD analysis for the probability of working overtime using data from April, August, and October of each year. The dependent variable in the RD is an indicator for whether the worker has positive overtime payments. Panel (a) shows the fraction of workers doing overtime for 50 equally spaced bins of the running variable (width AR$ 200). It also includes a quadratic fit to each side of the discontinuity. Panel (b) plots the evolution of the RD estimates computed at every month for which overtime pay is available. Each dot corresponds to a separate RD regression using a quadratic fit on each side of the discontinuity, a triangular kernel, and a AR$3,000 bandwidth. We use the `rdrobust` routine from Calonico et al. (2017). The vertical dashed lines indicate the beginning and the end of the tax holiday.

Figure 1.11: RD estimates for overtime hours (intensive margin)

(a) RD plot for overtime hours in October 2015



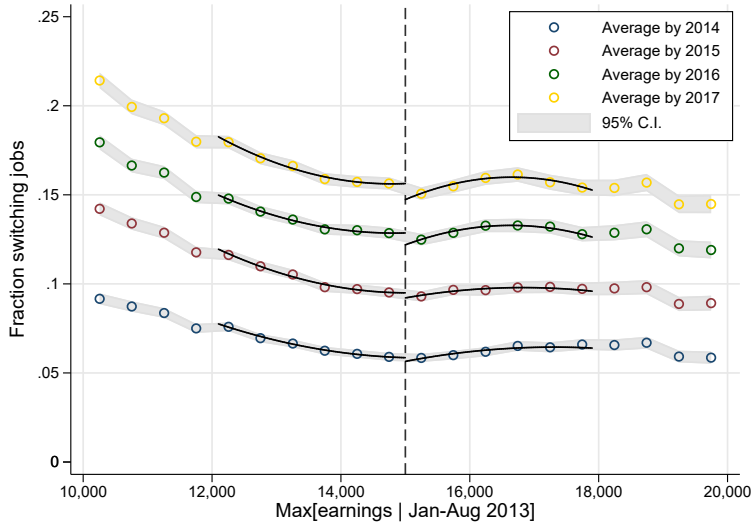
(b) Evolution of RD estimates



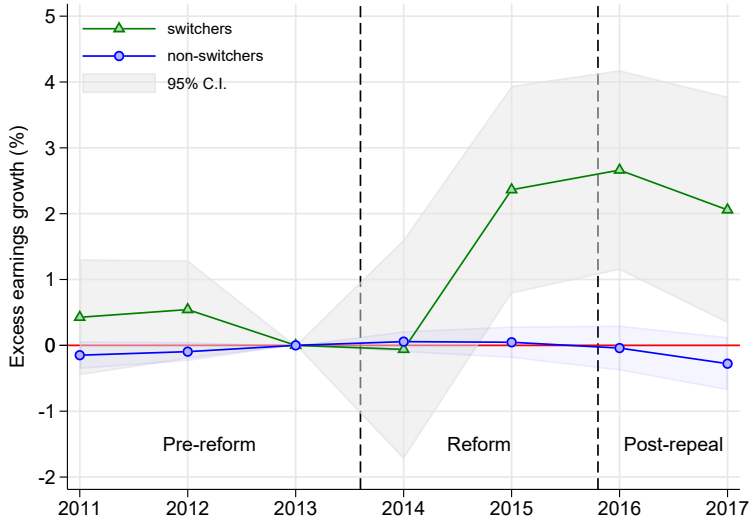
Notes: these figures report the results for overtime hours and are computed using data from April and October of each year. The dependent variable in the RD is the difference between overtime hours in each month-year and overtime hours in April 2013. Panel (a) shows the comparison between October 2015 and April 2013 where we get the highest effect. Workers to the left of the discontinuity worked 1.12 more overtime hours in October 2015. Average monthly overtime hours at the discontinuity were 26.3 in April 2013. Panel (b) plots the evolution of the RD estimates computed at every month for which overtime hours are available. Each dot corresponds to a separate RD regression using a linear fit on each side of the discontinuity, a triangular kernel, and a AR$3,000 bandwidth. We use the `rdrobust` routine from Calonico et al. (2017). The vertical dashed lines indicate the beginning and the end of the tax holiday.

Figure 1.12: RD estimates for job switchers (extensive and intensive margin)

(a) Likelihood of switching jobs by 2014, 2015, 2016, 2017



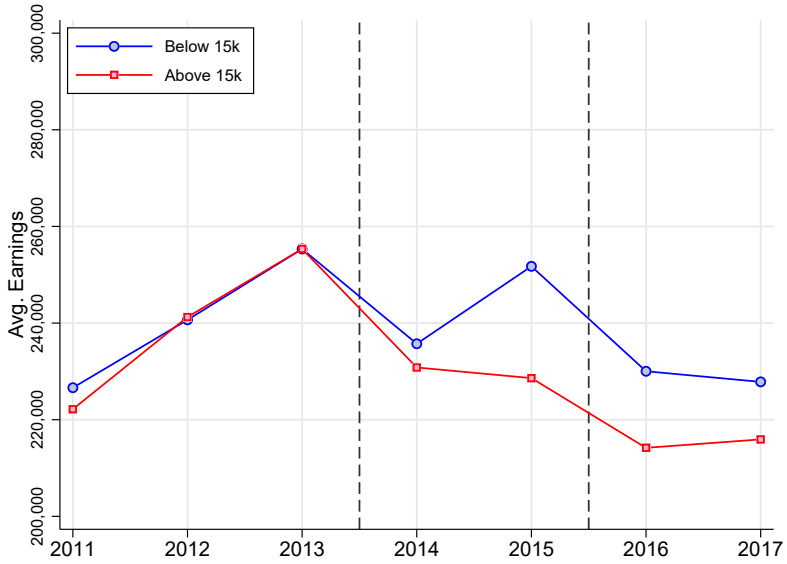
(b) Excess wage earnings growth for switchers



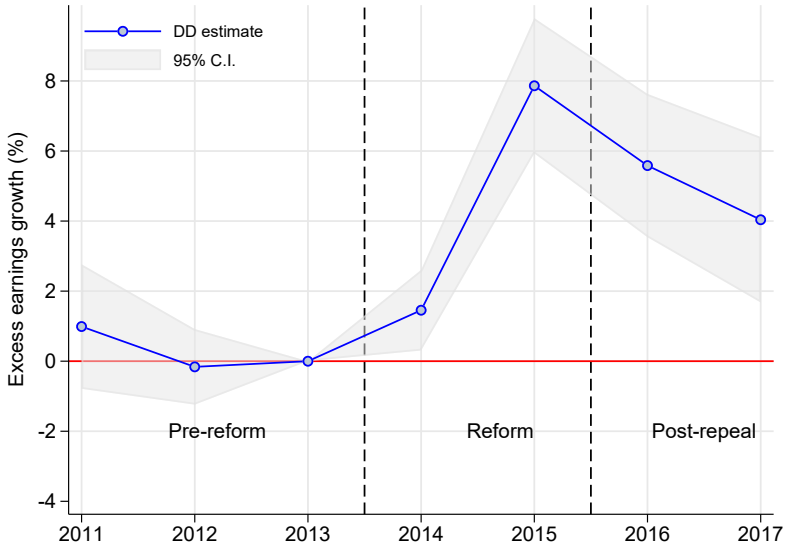
Notes: these figures present the RD analysis for job switchers. We define switchers as workers with a different firm identifier by December of each year relative to the firm identifier they had when the reform took place in 2013. Panel (a) plots the fraction of switchers by bins of the running variable (width AR$ 500) for two years during the tax holiday (2014 and 2015) and two years after (2016 and 2017). It also includes a quadratic fit on each side of the discontinuity. Panel (b) plots the evolution of the RD estimates for wage earnings growth relative to 2013 (intensive margin). We use monthly wages instead of annual wages to construct the dependent variable because switchers may undergo a period of unemployment, artificially lowering annual earnings relative to 2013. The green line corresponds to wage earners that switched firms by December 2015 (N=73,459) and the blue line corresponds to wage earners that stayed in the same firm by December 2015 (N=583,892). Each dot corresponds to a separate RD regression using a linear fit on each side of the discontinuity, a triangular kernel, and a AR$3,000 bandwidth. We use the `rdrobust` routine from Calonico et al. (2017). The vertical dashed lines indicate the beginning and the end of the tax holiday.

Figure 1.13: Wage earnings response of managers

(a) Annual earnings in constant pesos



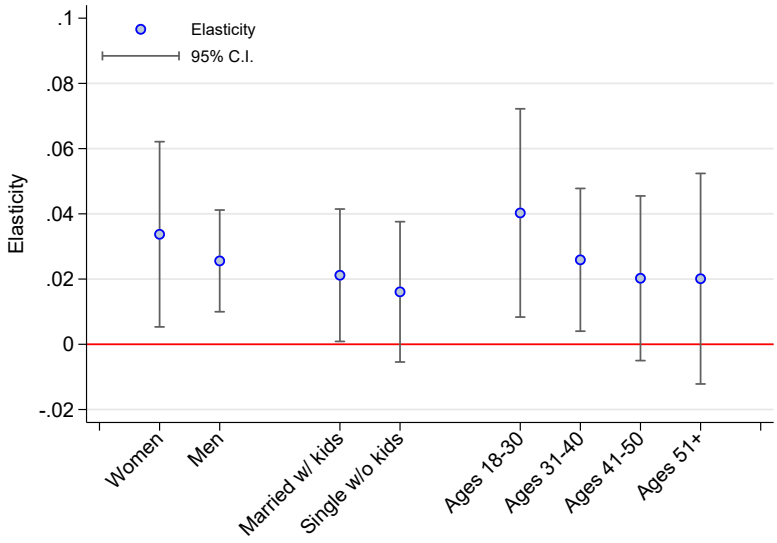
(b) Excess earnings growth (diff-in-diff estimates)



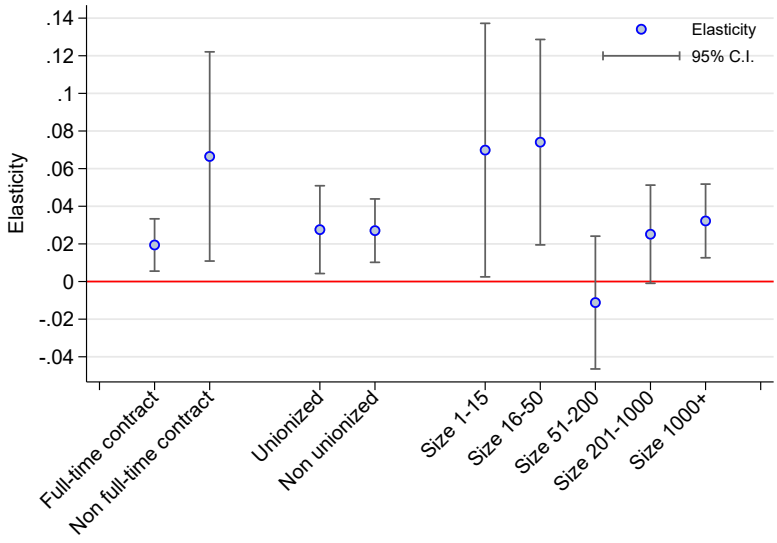
Notes: this figure shows wage earnings responses of managers to the income tax holiday. The treatment group contains managers with a running variable between AR$ 10,000 and AR$ 15,000 (untaxed), and the control group contains managers with a running variable between AR$ 15,000 and AR$ 25,000 (taxed). Panel (a) plots average annual earnings for both groups and panel (b) reports the evolution of difference-in-differences estimates using wage earnings growth relative to 2013 as the dependent variable. In panel (a) we scale the level of the treatment group so that it matches the level of the control group in 2013. The point estimates for the year 2015 are reported in Table 1.5 column 4. The vertical dashed lines indicate the beginning and the end of the tax holiday.

Figure 1.14: Elasticities by subgroups (in 2015)

(a) **By demographic characteristics**



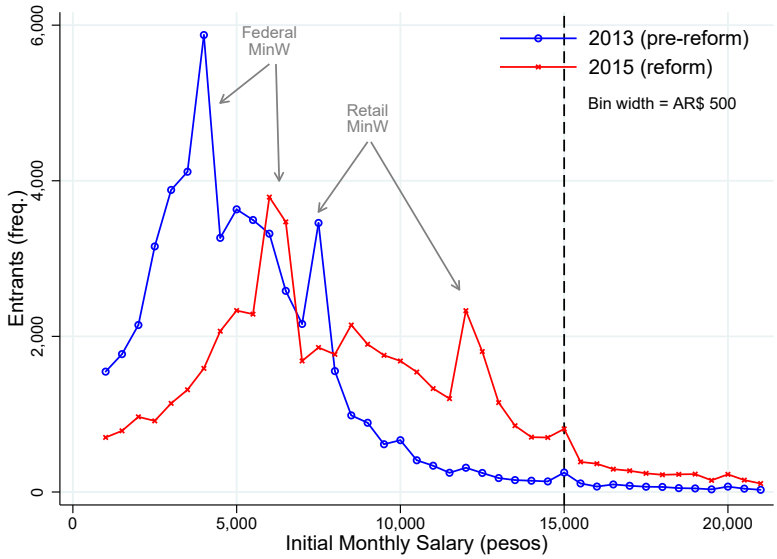
(b) **By employment characteristics**



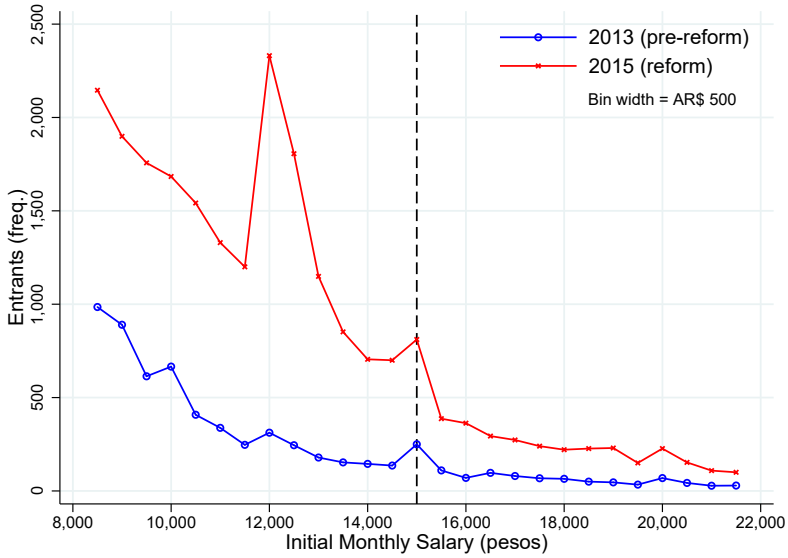
Note: this figure plots wage earnings elasticities for different subgroups in the year 2015 (the last year of the tax holiday). Each dot correspond to a separate RD regression where the dependent variable is annual earnings growth relative to 2013. To compute the elasticities we scale the reduced-form estimate by the first change in the net-of-marginal tax rate around the discontinuity using a fuzzy two-stage procedure (`rdrobust` routine).

Figure 1.15: Distribution of initial monthly wages for new entrants

(a) Full distribution



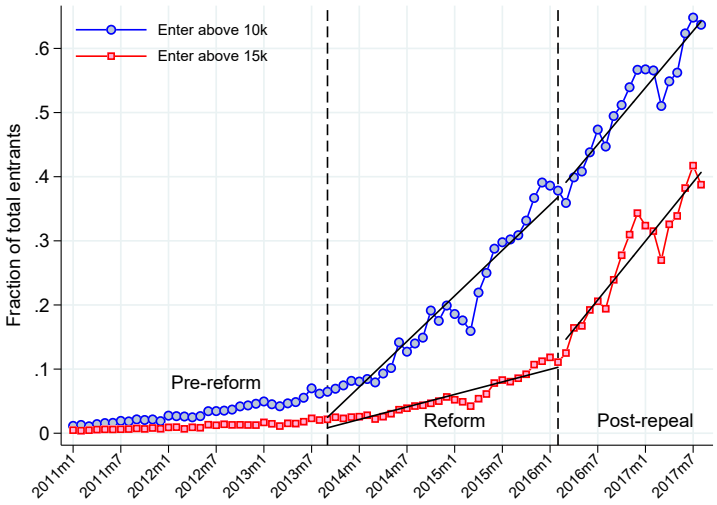
(b) Zoom in on the range 8k-22k



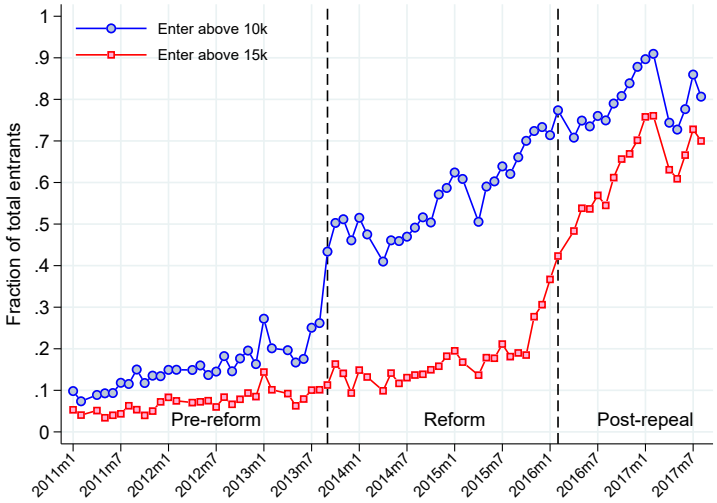
Notes: this graph plots the distribution of the first monthly salary for wage earners that did not have a job in January-August 2013. Panel (a) plots the full distribution up to AR$ 22,000, and panel (b) zooms in on the range AR$ 8,000-22,000. The blue line corresponds to the year 2013 and the red line to the year 2015. The vertical dashed line indicates the discontinuity at 15k. During the tax holiday (August 2013-February 2016), if workers enter below 15k they escape the tax and if they enter above 15k they are subject to the tax, in both cases regardless of their second monthly pay, the third, etc. The distribution shifts to the right over time due to high inflation and corresponding wage adjustments. In both years of panel (a), the first spike corresponds to the federal minimum wage and the second spike to the base salary in the retail sector.

Figure 1.16: Share of entrants with initial monthly salary above 10k, 15k

(a) Pool of entrants



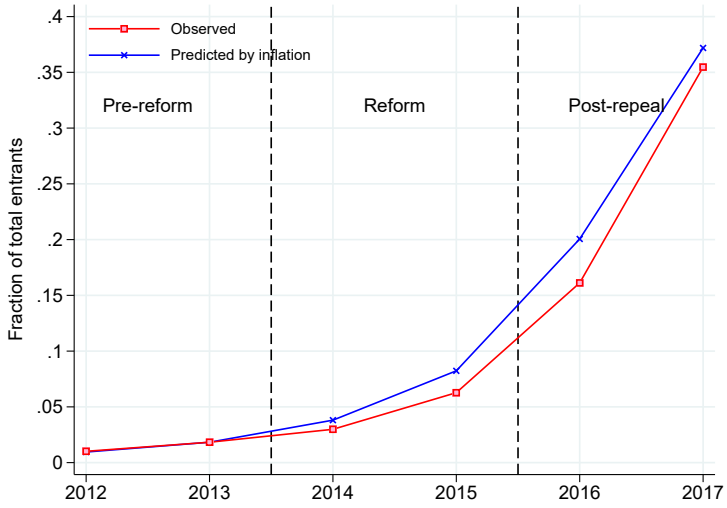
(b) Managers and executives



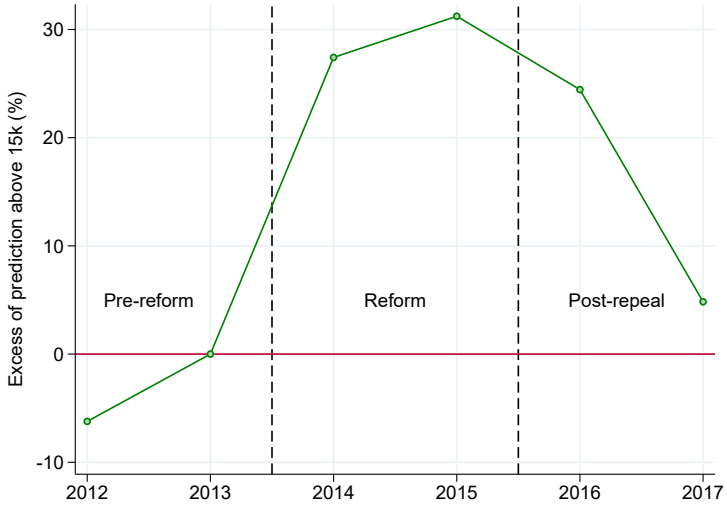
Notes: this graph plots the fraction of wage earners entering the labor market with their first monthly wage above AR$15,000 (red line) and above AR$10,000 (blue line). This fraction is computed at every month-year of our data. The sample of entrants consists of workers not present in the data between January and August 2013 that appear afterwards. For the pre-reform period we use wage earners not present in the data between January and August 2010 that appear afterwards. The blue line is a superset the includes the red line as well. So the difference between these lines contain the mass of entrants between 10k-15k. The vertical dashed lines indicate the beginning and the end of the tax holiday. The black solid lines in panel (a) denote linear fits that highlight the trend break of mass above AR$15,000 during and after the reform. For workers entering above 10k the slope goes from 0.0118 (0.0006) to 0.0147 (0.0013) and for workers entering above 15k the slopes goes from 0.0032 (0.0002) to 0.0153 (0.0013). Panel (a) includes the pool of entrants and panel (b) zooms in on managers and executives entering the labor market as wage earners.

Figure 1.17: Observed and predicted share of workers entering above 15k

(a) Predicted and observed share of entrants above 15k



(b) Gap between predicted and observed entrance (%)



Notes: this graph compares the observed share of workers entering above AR$ 15,000 against a predicted share based on annual inflation. For the predicted share, we take the distribution of initial monthly wage earnings in 2013, we shift it backward and forward in time using the RIPTE index, and then we compute the share of entrants that fall above AR$ 15,000. Panel (a) shows the shares in levels and panel (b) reports the excess in the number of predicted workers above 15k relative to the observed number of workers above 15k. The vertical dashed lines indicate the beginning and the end of the tax holiday.

Table 1.1: Personal Income Tax Schedule in Argentina (annual)

Annual Taxable Income		Annual Tax Payment		
from AR$	to AR$	AR$	+	over AR$
0	10,000	-	9%	0
10,000	20,000	900	14%	10,000
20,000	30,000	2,300	19%	20,000
30,000	60,000	4,200	23%	30,000
60,000	90,000	11,100	27%	60,000
90,000	120,000	19,200	31%	90,000
120,000		28,500	35%	120,000

Notes: this table shows the personal income tax schedule in Argentina that was in place during the period 2000-2016. Taxable income refers to adjusted gross income net of personal exemptions and general deductions. Taxable thresholds have been fixed in nominal terms since the year 2000.

Table 1.2: Schedule used by employers to compute monthly withholdings

Taxable Income at month M		Cumulated Tax at month M		
from AR$	to AR$	AR$	+	over AR$
0	$833\times M$	0	9%	0
$833\times M$	$1{,}667\times M$	$75\times M$	14%	$833\times M$
$1{,}667\times M$	$2{,}500\times M$	$191.67\times M$	19%	$1{,}667\times M$
$2{,}500\times M$	$5{,}000\times M$	$350\times M$	23%	$2{,}500\times M$
$5{,}000\times M$	$7{,}500\times M$	$925\times M$	27%	$5{,}000\times M$
$7{,}500\times M$	$10{,}000\times M$	$1600\times M$	31%	$7{,}500\times M$
$10{,}000\times M$		$2375\times M$	35%	$10{,}000\times M$

Notes: this table shows the personal income tax schedule that employers use to compute monthly withholdings. It is a monthly version of the annual schedule presented in Table 1.1. Under the Cumulative Withholding Method, employers compute Taxable Income at month M based on cumulated earnings (z_{it}) and cumulated deductions and allowances: $TI_{iM} = \sum_{t=1}^{M} z_{it} - \sum_{t=1}^{M} SSC_{it} - \frac{deductions}{12} \times M - \frac{exemptions}{12} \times M$. Then they take TI_{iM} to this table, calculate the cumulated tax up to month M, and subtract withholdings from previous months: $Withholding_{iM} = Cumul\_tax_{iM} - Cumul\_tax_{iM-1}$.

Table 1.3: Summary statistics for Argentine wage earners and estimation sample, 2013

	All	10k-15k	15k-25k	**14k-16k**
	(1)	(2)	(3)	**(4)**
Fraction of total wage earners	1	0.142	0.089	**0.036**
Decile earnings Jan-Aug'13	1-10	8	9	**8-9**
Age	40.4	43.1	45.7	**44.6**
Public worker (%)	0.310	0.410	0.454	**0.421**
Unionized (%)	0.492	0.452	0.459	**0.460**
Female (%)	0.398	0.385	0.334	**0.354**
Number of jobs	0.92	1.05	1.10	**1.07**
Multiple jobs (%)	0.049	0.070	0.110	**0.087**
Average monthly wage Aug'13	8,052	10,816	16,292	**13,203**
Number of workers	9,936,088	1,413,204	881,104	**357,775**

Notes: This table displays summary statistics for private and public registered wage earners in Argentina in the year 2013. Groups 1 through 3 are defined based on the highest gross monthly wage between January and August 2013 (the running variable in the RD analysis). Column 4 includes the universe of wage earners. Table entries are means unless otherwise noted. Monetary values are in Argentine pesos.

Table 1.4: Density test at potential reference points

	Density test at:		
	AR$ 10,000	AR$ 15,000	AR$ 20,000
p-value	0.0000	0.0001	0.0000
p-value removing spike	0.2833	0.8105	0.8520

Notes: this table shows the p-values of the RD manipulation test based on discontinuity in density using local polynomial (Cattaneo et al., 2018). The null hypothesis is that there is no manipulation of the density at the cutoff. We run the test at the income tax discontinuity of 15k, and two other reference points unrelated to the income tax, 10k and 20k. We report the p-value including and excluding the mass (spike) right at each threshold.

Table 1.5: Reduced forms, first stages, and elasticities (year 2015)

	Total	**Overtime hours**	**Switchers**	**Managers**
	(1)	(2)	(3)	(4)
Panel A: reduced-form				
% Δy	0.41**	4.27***	2.36***	7.86***
	(0.19)	(1.14)	(0.805)	(0.985)
Panel B: first-stage				
% $\Delta[1-\tau]$	24.7***	23.1***	24.6***	25.2***
	(0.04)	(0.06)	(0.14)	(0.28)
Panel C: (A)/(B)				
Elasticity e	**0.017****	**0.184*****	**0.096*****	**0.311*****
	(0.008)	(0.049)	(0.033)	(0.041)
Observations	466,721	200,939	53,637	7,802

Notes: this tables reports point estimates and standard errors for the year 2015. Panel A shows the reduced-form percentage change in labor supply measures around the discontinuity, panel B shows the percentage change in the net of marginal tax rates, and panel C presents the elasticity which essentially scales the reduced-form by its first stage. The elasticity, $e = \%\Delta y/\%\Delta[1-\tau]$, is computed using a two-stage fuzzy RD procedure at 15k. Standard errors reported in parentheses. Dependent variables: annual earnings growth relative to 2013 (columns 1, 3, and 4), difference in overtime hours relative to 2013 (column 2). For overtime hours we scale the reduced-form (1.12 hours per month) by average overtime hours around 15k (26.3 hours per month) and apply the Delta Method to get the standard errors. For the change in the net of tax rate, we adopt a conservative approach and use individual-level marginal tax rates in August 2013 (pre) and December 2013 (post). Using marginal tax rates in 2015 could potentially capture behavioral responses of workers above the threshold because the bracket creep makes them face higher taxes during the period of analysis. In any case, using marginal tax rates in 2015 would make the first stage even larger and the elasticity even smaller. The last row denotes the effective number of observations that participate in each regression. RD estimates are computed with the `rdrobust` routine from Calonico et al. (2017). *** significant at 1%, ** significant at 5%; * significant at 10%.

1.10 Transitional Section to Chapter 2

A central prediction in public finance is that the action of governments (e.g., through taxes, subsidies, and welfare programs) distorts the behavior of workers and businesses, and therefore creates deadweight loss. In this Chapter, we focused on high-wage earners and showed that in fact they respond very little to large but temporary tax changes. This implies that the revenue leakage might be small due to real responses from high earners. We also presented evidence to unbundle the near-zero effect that suggests that low responses might be driven by labor demand constraints (e.g., fixed contractual hours) and labor market rigidities. It seems that employer-employee cooperation is required for wage earners to be able to respond to tax changes.

The reform that we exploited affected workers at the upper end of the wage distribution. A natural question is whether labor responses are also small at the lower end. Moreover, tax systems typically provide credits (negative taxes) for low income workers and therefore it could create rent-reeking behavior when these transfers are mediated by employers. In the following Chapter we address these questions. In particular, we dig deeper on the role that employers could have in the tax system when the tax authority requests them to disburse family allowances.

Chapter 2

Wage Effects of Means-tested Transfers

2.1 Introduction

Most countries provide some sort of financial aid to families with children. This type of social assistance was established in developed countries after World War II and in developing economies towards the end of the 20th century. The existing literature on this type of policies has mainly analyzed extensive and intensive labor supply responses to transfers itself, as well as the effects on children's outcomes such as education and health. However, very little is known about the effect of other features embedded in these programs like the timing of payments, the role of conditionalities, or the way the transfer is disbursed. Among these, the empirical question of who bears the economic incidence of work subsidies and family allowances is still poorly understood. In this article, we break new ground on these important issues and study whether the way family allowances (tax credits) are disbursed affects the wage of workers. We exploit an unusual reform in Argentina that shifted the *disbursement responsibility* of family allowances from employers to the social security administration (SSA).

In Argentina, registered wage earners with children less than 18 years old are entitled to a family allowance (tax credit) that they receive on a monthly basis (*asignaciones familiares*). This is a means-tested program for low-income workers that provides a fixed transfer per child that decreases as workers earn more through a wage earnings-based *notched* schedule with three brackets.[1] This transfer was historically disbursed by employers who could net these payments out from social security contributions (SSC) before remitting SSC to the tax authority. In 2003, for transparency reasons the government decided to replace the intermediary role played by firms and to start depositing the transfer directly into workers' bank accounts. Because of the administrative burden of such a change, the government had

[1] In that sense, this transfer is similar in spirit to the EITC in the U.S. but presents *notches* instead of kinks, and it is paid every month instead of at the end of the year.

to switch firms from the old to the new system gradually over the course of eight years (from 2003 to 2010).[2]

The gradual roll-out of the new payment system and the change in saliency of the transfer provide ideal variation and a unique opportunity to cast light on the labor market consequences derived 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 providing incentives to integrate the family allowance into the salary package of eligible workers, potentially shifting part of the incidence of the transfer in the form of lower wages. Moreover, since the credit appeared as an extra line on pay slips, it could make workers believe that the transfer was actually funded by the firm. In contrast, under the new system, named *Sistema Único de Asignaciones Familiares* (SUAF), the employer was not able to tag beneficiaries or see the amount of the transfer anymore. Naturally, given this setting, we use an event study design where we align firms at the switching date and compare monthly wages of eligible and non-eligible workers before and after that date to identify wage effects.

To carry out our study, we use employer-employee administrative data containing the universe of wage earners registered in the social security of Argentina for the period 2003-2010. These data are reported by employers to the tax authority every month, and thus provides high-frequency variation with firms switching to the new system during 96 consecutive months. It contains monthly information on total wage earnings, social security contributions, firm's sector, zip codes, and some demographic variables. Importantly, in this dataset we observe the exact amount of the monthly transfer received by each worker before firms switch 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 of them, so that the transfer could be deducted from payroll taxes. We also have access to another dataset of family relationships that allows us to link workers with their spouse and children. In this dataset, we also observe the exact date of birth for each child allowing us to flag eligible and non-eligible workers accurately.

Our results can be summarized as follows. In the first part of the paper we show that the way family allowances are disbursed is not neutral and affects gross wages. The monthly wage of workers with children increases by 9 pesos relative to workers without children when firms stop disbursing the transfer (and it is instead delivered by the SSA). This effect declines as we move up in the income distribution where the amount and salience of the transfer are smaller. In terms of the pass-through rate, our estimates imply that employers were capturing about 10-20 percent of the transfer by paying lower wages when they mediated the disbursement.

In the second part of the paper we explore some of the mechanisms and argue that the increase in monthly wages after the event is more consistent with a labor demand story rather than pay equity concerns. The key piece of evidence is that the effect is driven by new hires

[2]In the body of the paper, and also in more detail in the appendix, we explain that the transition was made through a set of memos and decrees published over the course of the eight years that included annexes with the list of firms that had to switch at different dates. Critical for the identification, from the point of view of the firm this was a plausibly exogenous event.

rather than incumbent workers. Intuitively, when firms are no longer in charge of delivering the transfer, they cannot integrate it in the wage package anymore and thus the market wage of new hires with children goes up. The fact that the effect appears immediately in the first month post event, and that it is explained by new hires, go against a pay equity concern channel, since one would expect the effect to build up slowly and also to affect incumbents. In addition, we show that wage effects are stronger the higher the exposure of a firm to family allowances.

These findings therefore suggest that the way governments set up tax credit programs, like the EITC in the U.S., influences the final economic incidence. We find that wages do adjust to the way transfers are disbursed, rejecting the null hypothesis from the standard model that transfers are *all* captured dollar for dollar by workers. This is an important result that sheds light on a topic still understudied but that is common in other countries. For instance, in Brazil, Chile, Paraguay, Italy, and Switzerland family allowances for employees in the formal sector are disbursed by employers. Our results suggest that letting firms operate as mediators could be a bad idea.

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 model predicts that, in a partial equilibrium framework, 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 and Stiglitz (2015), Fullerton and Metcalf (2002), Kotlikoff and Summers (1987) and Musgrave (1959). Modern approaches have extended the standard model by incorporating salience effects (Chetty, Looney, and Kroft, 2009), remittance and compliance costs, as well as market rigidities and imperfect competition. For instance, recent evidence in other settings show that who remits the tax to the tax authority matters to explain the final incidence (Slemrod, 2008; Kopczuk, Marion, Muehlegger, and Slemrod, 2016).

We also contribute to recent research that casts doubts of the standard prediction in public finance that statutory incidence is totally irrelevant in determining final incidence. For payroll taxes, Saez et al. (2012a) 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, Schoefer, and Seim (2019) exploit a reduction of employer SSCs in Sweden and find that posted wages of treated workers did not change implying full incidence on employers. Our project looks at a slightly different question focusing on transfers rather than on taxes as most of the existing literature has done.[3] The most interesting aspect is that we look at a change in the payment system (the *disbursement responsibility*) keeping other features constant.

To the best of our knowledge there are three papers closest to ours that evaluate the incidence of in-work subsidies, but with weaker research designs and poorer data. Rothstein

[3]For instance, Benzarti, Carloni, Harju, and Kosonen (2017) show that prices respond asymmetrically to VAT increases and decreases, suggesting that the direction of a tax change does matter for incidence.

(2010) and Leigh (2010) estimate the incidence of the EITC in the U.S. and Azmat (2019) analyzes a change in the payment system of the Working Family Tax Credit (WFTC) in the U.K.. The evidence is still not conclusive and there is room for more work on this area. The reason is that identification of wage effects is challenging, so our paper is a clear contribution on this end.[4]

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

The paper is organized as follows. In Section 2.2 we describe the institutional setting and the change in the remittance system. In Section 2.3 we revise the standard incidence terminology and conceptual framework. Section 2.4 introduces the data sources. The empirical strategy and main results are presented in Section 2.5. In Section 2.6 we explore the mechanisms. Finally, section 2.7 concludes.

2.2 Institutional setting

Family Allowances (AAFF)

The family allowance program *Asignaciones Familiares* (AAFF) is the largest means-tested transfer in Argentina. It was introduced in 1996 and it is funded by social security contributions (SSC).[5] It consists of a monthly child benefit paid to private employees with monthly wages below a certain threshold and with children less than 18 years old. The benefit amount varies by the number of children and decreases discretely by monthly wage bins.[6]

The parameters that characterize the AAFF transfer scheme for the early years of our data are presented in Figure 2.1a.[7] There are 3 brackets and the transfer per child decreases as we move to the right ($40, $30, and $20 per child). The figure shows the average tax rate for a worker with one children (blue line), 2 children (red line), and 3 children (green line). So, for example, for a worker with two kids right at the end of the first bracket, the transfer

[4]There are also various recent contributions on different incidence analysis including Suárez Serrato and Zidar (2016) Fuest, Peichl, and Siegloch (2018), Bozio, Breda, and Grenet (2018) and Benzarti and Carloni (2019).

[5]See Law 24,714/1996. Prior to this program, there were some limited schemes (either firms designing their own system, or local government initiatives). See Section B.2 for more details.

[6]There is also a supplement transfer for workers living in less favorable areas (there are 4 zones under this classification).

[7]Table B.1 provides a complete picture of this scheme by year including the evolution of the brackets and the exact transfer amount per child.

represents 16% of the monthly wage and if she earns a bit more, she starts to receive $30 per child instead of $40.[8]

The context of high and persistent inflation that Argentina experienced from 2004 onwards makes our setting very interesting because it makes it easier for employers to capture rents in real terms. This is because although nominal wages are typically downward rigid, real wages might not be. Moreover, because of this persistent inflation wages are renegotiated more often. Figure 2.1b describes the evolution of the upper bracket thresholds from 2003 to 2011, jointly with the evolution of the minimum wage that 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 meaning that there is room to eventually shift part of the incidence (Lee and Saez, 2012).

The reform: a staggered change in the payment system

The policy variation that we exploit in the paper comes from a reform that changed the way transfers are disbursed, and that was gradually rolled out between 2003 and 2010. The old and new systems are summarized in Figure 2.3.

The old system (SFC)

The original scheme of the AAFF program was such that transfers were disbursed indirectly by employers to eligible employees. This system was called at that time *Sistema de Fondo Compensador* (SFC), and the name comes from the fact that employers could *compensate* the transfer from the employer portion of social security contributions before remitting the money to the IRS. If the amount paid was greater than the tax bill, the firm could claim a refund. In this setting the employer was merely an intermediary in charge of disbursing funds from the public sector. This implied that firms had the ability to distinguish between eligible and non-eligible workers and also to know precisely how much subsidy they were getting from the government. A key fact of this system is that the transfer was included as an extra concept on the worker's pay slip (see Figure 2.2 panel (a)). This is relevant for the incidence analysis because it enhances the saliency of the transfer and it could also make workers believe that the transfer was part of the compensation and thus funded by the firm.

The new system (SUAF)

The new payment system, named *Sistema Único 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 scheme, the Social Security Administration (SSA or ANSES, for its acronym in Spanish) removed the intermediary role of firms and started depositing

[8] In Figure B.1 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.

the subsidy directly into workers' bank accounts giving place to a new centralized payment system. In that way, the only duty of the firm was to remit monthly SSC to the IRS. This implied that employers were not able to tag beneficiaries or see the benefit amount anymore. In addition, the subsidy stopped being reported on workers' pay slip reducing the saliency of the transfer (see Figure 2.2 panel (b)). The reasons that motivated this reform, as stated by the government, were to make the system more transparent, to make sure beneficiaries effectively get the transfer, to control fraud, and to provide administrative relief to the firms. Since the SSA did not have enough operational capacity, they gradually incorporated firms into the new system instead of doing it in one step. Employers were switched from the old to the new system on a monthly basis from June 2003 to June 2010, when the whole transition process finished. Importantly, workers kept receiving the transfer, but it is paid through a different window and all of a sudden it becomes less salient to employers.[9]

Incorporation process and empirical roll-out

The incorporation of firms into the new system was determined by the SSA through official memos posted online. Hence, since employers could not choose when to switch, the switching date works as a plausibly exogenous event from the point of view of the firm. In a nutshell, the whole process can be described as follows (see Figure 2.4). The incorporation started with the SSA setting an *internal* incorporation schedule, where basically the SSA issues a list of targeted firms that will be gradually incorporated up to a certain point in time. Firms were then contacted by an officer and informed to present certain documentation to be incorporated into the system (payroll, beneficiaries, bank accounts). The last step, after checking everything was correct, consisted on the formal approval and incorporation into the new system. Employers were required to notify their employees through an individual form to be signed by both parties (affidavit).[10]

In the data we observe the date when the firm stops disbursing the transfer under the old system. After this date, the payment variable becomes missing. Hence, we define the *event* as the month-year when the firm stops paying family allowances so that the last payment observed in micro-data will be at month $t-1$. Figures 2.5 illustrates the empirical roll out that emerges from our microdata. 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.[11] As can be seen in panel (a), the transition was gradual.[12] In panel (b), we further show that large firms switched first into the new system. Although the SSA was the one that determined the

[9] In Figures B.2 and B.3 we show that spending and beneficiaries did not decrease during this transition.

[10] More details can be found in Appendix B.3.

[11] Some firms never paid family allowances through the SFC system, which explains why the initial share is not 100% at the beginning of the reform.

[12] In Figure B.4 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 of very good quality (see Figure B.2).

switching date, it seems that they targeted large firms initially. Nevertheless, our empirical strategy exploits within-firm variation to address any potential selection issue.

To sum up, the staggered change in the remittance duty and the saliency of the transfer provide ideal variation and a unique opportunity to cast light on the labor market consequences derived from the way tax credits are disbursed. In particular, it allows to study whether employers were capturing part of the transfers by lowering wages.

2.3 Conceptual framework

In this section we briefly describe a way to rationalize the mechanisms that could explain who bears the final incidence of a subsidy. 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. Assuming that there is perfect information and competition, and that there is no compliance cost, this implies that the statutory incidence is totally irrelevant in determining final incidence.

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

It is important to analyze how the different elements change under the two different payments systems to further understand the institutional setting but also as a preview of what could drive behavioral responses. In Table 2.1 we list some key dimensions that may affect the final economic incidence. Column (1) corresponds to the old system and column (2) to the new system. The main change between the two systems is in the *remittance responsibility*. Under the old system, employers disburse the transfer together with the monthly wage. Under the new system, the government is the one that pays the benefit directly to eligible workers. Note, however, that the *legal liability* is exactly the same in both systems: the ultimate recipient of the transfer is the employee.

Another important component is the *salience* of the transfer. As it has already been documented, the visibility of a tax matters to determine the economic incidence of taxes (Chetty et al., 2009) but it is uncertain whether and how this effect operates in the case of transfers. In principle, the transfer is fully visible and salient to both employers and employees under the old scheme; indeed, it appeared in the worker's pay slip as it is possible to see in Figure 2.2.[13] In that figure we present a real pay slip from an anonymous worker right before and right after the firm switches from SFC to SUAF. In the old system (panel

[13] Article 140 of the Labor Contract Law, established that it is mandatory to list in the pay slip all the items that the employer is paying and deducting from the employee.

a) the pay slip contains a line where the transfer is reported 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 approximately 25% of total wage earnings. Under the new system (panel b), the transfer disappears from the pay slip and the worker starts getting the transfer directly from the government in his checking account. Hence, the transfer becomes less salient, at least to employers.

Furthermore, the tax-benefit linkage may have changed after a firm switches systems. The perception of what the transfer is and how it is financed could have changed and, as a result of this, the bargaining conditions as well.[14] The understanding of the whole system may have changed after the new disbursement mechanism is put into place. It may be likely that employers and employees interpret the sum of wages and the transfer as a combo under the old scheme.[15] The claiming procedure for new workers changed as well, as they have to claim the benefits by themselves rather than through their employers.

The transfer is financed by a specific component of employer SSCs making it salient to employers. In general, the payroll tax has different components: family allowances which is 7.5 percent of employers SSCs, pension component (16 percent), health insurance for the elderly (2 percent), health insurance (5 percent), unemployment insurance (1.5 percent), life insurance (0.03 percent), and a worker compensation, against accidents, a percentage that varies by type of job. Nevertheless, there is no change induced by the reform on the way the transfer is funded.

There are many other dimensions that may induce a behavioral reaction, but that remain the same under the two payment mechanisms. For instance, the timing of the payments does not change, it keeps the monthly frequency and we are not aware of delays or complaints on this regard after the firm switches to the new system. Unlike most of the literature on incidence that exploits either changes in the marginal or average tax rate, in our setting the amount of the transfer remains unchanged. It is a pure change in the way the money is delivered but not in the total amount the worker receives.

2.4 Administrative data

Wage earnings data (SIPA). The main source of information that we use in this paper is an employer-employee database compiled by the Ministry of Labor and known as SIPA. This dataset is the result of employer reported information about their employees gathered in form F.931, which is somehow equivalent to Form 941 in the U.S.. This comprehensive dataset contains monthly information for the universe of private formal wage earners in Argentina. Therefore, we are able to observe every single employee in the country conditional on being

[14] Formally, workers perceive that their wage is $\tilde{w} = w + (1 - q) \cdot t$, where w is the monthly wage, t is the monthly transfer, and q $[0; 1]$ is a perception parameter. When $q = 1$ workers fully understand the way the transfer works and how it is funded, and when $q = 0$ there is complete confusion.

[15] Within ten days after the switch, employers had to inform their workers about the new payment mechanism and about the overall scheme of the family allowances system.

formally employed. This dataset has a wide set of variables including pre-tax monthly wages, employee social security contributions, sector, region, zip code, age, and gender among other characteristics. Our dataset spans the period 2003-2010.

Table 2.2 presents some descriptive statistics for the year 2004. In 2004 Argentina had approximately 5 million private wage earners and about 400 thousand firms. About 25 percent of the registered workers received AAFF transfers. The majority of these beneficiaries were in the lower and middle bracket with an average tax credit rate of 13 percent and 7 percent, respectively. Note also that average number of children is 2 across the earnings distribution.

Recall that under the SFC scheme, family allowances were paid by employers to their employees who could then net these payments out from payroll taxes. So, employers had to report this information to the IRS. The richness of our data allows to observe how many employees received the transfer and also the exact amount each of them received. Once a firm is incorporated into the new scheme, the SUAF, this variable is automatically filled with zeros.

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

We are also in the process of getting access to two complementary databases. The first one contains the payments under the current SUAF system. Somehow, this database is a continuation of the information that used to be reported in the employer-employee database, in the sense that one could observe payments under the new system. This could let us check whether workers keep receiving the transfer after the transition. The second one contains the monthly financial situation of all employers. This is compiled by the Central Bank of Argentina into what is called *Central de Deudores del Sistema Financiero* (CENDEU). This source will allow to look at the indebtedness level of each firm before and after moving into the new transfer scheme. The idea here is to rule out the potential threat that firms switching first were the ones in financial hardship.

2.5 Empirical strategy and results

Empirical strategy: event study

The staggered roll out of firms into the new scheme naturally leads to an event study design. 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 will be at month $t-1$. Our setting is pretty rich in the sense that we have large variation and heterogeneity to analyze and explore. We observe thousands of firms experiencing this event (large number of treated units), events occurring during 96 consecutive months (time variation), heterogeneity in firm size (from micro firms to super big firms), share of workers receiving family allowances paid by their employer (intensity of treatment at the firm level), number of kids for whom workers are receiving the transfer (intensity of treatment at the worker level).

Our estimation sample is comprised by an unbalanced panel of firms that we observe during the period of January 2003 to December 2010 (96 consecutive months). We include firms for which we observe an event (switch from SFC to SUAF).[16] Our strategy consist of comparing within each firm, eligible and non-eligible workers to receive the transfer. The control group C consists of workers without children i.e., not eligible for the 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, this depended on their wage level and working status of their spouse).[17]

We exploit within-firm variation before and after the transition into the new system as follows. For each firm f, group $g = C, T$, and month t, we compute the average wage ($\bar{w}^g_{f,t}$) and, to keep things simple, we take the difference across groups within a given firm and month ($G^{\bar{w}}_{f,t} = \bar{w}^T_{f,t} - \bar{w}^C_{f,t}$). 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:

$$G^{\bar{w}}_{f,t} = \alpha + \sum_{j=-13}^{12} \gamma_j \cdot d^j_{f,t} + \epsilon_{f,t} \tag{2.1}$$

where $d^j_{f,t}$ are event-time dummies measuring that the event happened j periods away. Note the $j = 0$ is the first month in which the firm does not disburse the transfer anymore and, as it is generally done, we take $j = -1$ as the omitted category in our estimations and figures. Note that 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 way that those are identified is by differencing them out.[18] To

[16] There are two types of employers that for the moment we are not using but we could eventually use to run a sort of triple DID: (a) placebo group: firms affected the same way before and after the reform: these are firms for which we cannot identify the event date in the micro-data because they were paying through a direct-method and therefore they did not report they payments in the data; (b) firms that were not paying under the old system but never incorporated into the new one e.g., schools, some public agencies.

[17] Workers can change treatment status over time as (a) either their youngest child turns 18 (b) a newborn. To avoid workers switching treatment status, in one of the robustness checks, we identify as treated workers those who have at least one child born in [1992-2002]. This means that these workers are fully treated during the period 2003-2010 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 to say, they are either never treated or partially treated.

[18] In Section B.4 we elaborate on this equivalence.

construct the wage gap in the data, we consider a monthly wage variable that is used to calculate employers' social security contributions. As in most countries, this variable is right censored, as it exists a cap on social security contributions.[19] Besides the average wage, we also compute other moments such as percentiles 25, 50, and 75. We look at an event-time window of twelve months before and twelve after the change, and we cluster standard errors at the firm level.[20]

Although not necessary in our setting, in our estimations we also add firm and month-year fixed effects as it is standard in the literature. So the specification is the following:

$$G^{\bar{w}}_{f,t} = \sum_{j=-13}^{12} \gamma_j \cdot d^j_{f,t} + \mu_f + \mu_t + \epsilon_{f,t} \tag{2.2}$$

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:

$$G^{\bar{w}}_{f,t} = \beta_1 Window_{f,t} + \beta_2 \cdot Window_{f,t} \cdot Post_{f,t} + \beta_3 \cdot (1 - Window_{f,t}) \cdot Post_{f,t} + \mu_f + \mu_t + \epsilon_{f,t} \tag{2.3}$$

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.

We use the same framework to compute the first-stage change in the transfer where we use 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. To get the right standard errors, we estimate this pass-through with a two-stage least squares method.

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 simply run specification (2.2) but using as the dependent variable the difference in average transfers that each employer paid to workers with and without children. We plot the $\gamma's$ of the referred equation in Figure 2.6. 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, before the event, workers with children were receiving approximately 90 pesos more in transfers, disbursed by employers, than the workers without children. It is worth remembering that workers do not lose the transfer but it rather starts being paid directly by the government. But, importantly for the question of this paper, it is not managed by employers anymore.

[19] This cap is above the 95th percentile for all the months that we analyze and thus it does not pose a threat to our results.

[20] In the robustness checks we play around with the time window and results are very stable. We also bin up the end points as it is common in this literature (Schmidheiny and Siegloch, 2019).

The null hypothesis of interest is whether the way payments are disbursed is neutral in terms of who ends up benefiting from the transfer. If it is neutral, then we should observe no effects on monthly wages after the transition; there is full shifting to employees regardless of how the transfer is paid. On the contrary, if employers where fully shifting the incidence of the transfer under the old scheme, then the monthly wage should increase peso for peso by 90 pesos after the change. One possible way to do so, is to think that employers were offering the transfer within the wage package saying that 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. An interesting fact of our setting is that inflation was high during the whole period (15% on average) and thus monthly wages were renegotiated quite often.[21]

In Figure B.10 we look at the reduced-form effect on monthly wages relative to period $t-1$. Panel (a) shows the wage levels when we estimate equation (2.2) 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. 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 9 pesos relative to workers without children when firms stop disbursing the transfer. In Figure 2.8 we show that this result is entirely driven by workers at the lower end of the monthly wage distribution. The $p25$ wage differential of workers with and without children presents a sizable jump after the event while nothing happens with $p75$. This result is reassuring as $p25$ is where family allowances present a higher bite.

In Table 2.3 we report the reduced-form and pass-through estimates from the event study. The reduced-form and first stage point estimates constitute diff-in-diff coefficients (i.e., we pool the coefficients before and after the event and take the difference). The 2SLS is the Wald estimate where we essentially scale the reduced-form by the first stage change in the transfer. This exercise shows that the monthly wage of eligible workers increases by 9 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 average tax rate and salience of the transfer is smaller. Moreover, in terms of the pass-through, what our estimates show is that for a 1 peso decrease in the transfer paid by employers (holding the total transfer constant) the wage increases by 9 cents, implying an incidence of around 10 percent for those at the average of the monthly wage distribution.

Robustness checks. Our results are robust to a battery of checks. First, they are not affected by modeling choices (Table B.2). 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 B.6), when we change the number of consecutive months the firm was paying transfers right

[21] As an illustration of this, it is worth mentioning that the minimum wage had 23 changes during the 96-month period that we are analyzing i.e., they were updated every 4 months.

before the event (Figure B.7), or when we vary the length of the event-time window (Figure B.8). Third, the results are preserved when we consider workers that are fully treated during the period 2003-2010, namely, those with children ages less than 18 years old during the whole roll-out period (Figure B.9).

2.6 Mechanisms: labor demand vs. labor supply

Our results thus far show that the way family allowances are disbursed is not neutral and that employers capture about 10 percent of the transfer when they mediate these payments. In this section we discuss two competing channels that could explain this response. One driven by employers (labor demand) and another one driven by employees (labor supply). We show evidence in favor of the former and against the latter.

On the one hand, it could be that employers were exploiting the confusion of workers 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 not do this practice anymore. 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 coworkers and thus started bargaining more aggressively.

Both of these stories require an imperfect understanding of the way family allowances work and how they are funded. This confusion is 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 integrated their salary and that employers were responsible for them. They even ignored that it is the State that pays for them...*" (Marasco, 2007). Furthermore, a survey ran by the SSA in 2018 confirms that workers are still confused about how family allowances are currently funded. More than 50% replied that they do not know, 35% correctly said that they are paid by the government, and 8.6% still think that it is funded by employers (Table 2.5).

Our analysis suggests that the increase in monthly wages after the event is more consistent with a labor demand story rather than pay equity concerns.

The key piece of evidence for the labor demand channel is that the result seems to be driven by new hires rather than incumbent workers. This is reported in Figure 2.9 where we run two different regressions: the one we ran thus far that considers all the workers (blue line), and another one for a balanced panel of employees present at the firm in the whole window (red line). The difference between these two lines thus captures the response of new hires.[22] The figure shows no effect for incumbents. This is a group of workers that already have a written contract and their payment schedule is somewhat predetermined, at least in

[22]Note that our empirical strategy requires, for each firm, 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 every month at least two workers, one with and another without children.

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 cannot offer it as part of the wage anymore and thus the market wage of new hires goes up.

In Figure 2.10 we further break the aggregate wage effect by firm size and type of business. The effect is stronger in small firms with less than 10 employees. This result is in line 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 that is in charge of hiring and thus it is presumably harder for managers to engage in such behavior. The results are also summarized in Table 2.4. Note that in the case of small incorporated businesses, the pass-through rate is -0.2 which means that employers were capturing about 20% of the transfer when they were in charge of its payment.

Finally, we discuss three reasons why the horizontal equity channel does not seem to be at play. First, if this was a bargaining story, then one would expect the effect to arise slowly over time. However, we find an immediate effect at $t = 0$ which is more consistent with response from the labor demand. Second, fairness concerns would operate mostly for incumbent workers at the time of the event. Yet, we find a null effect for this group and large 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 present a U-shaped relationship with the exposure of a firm to family allowances. That is, it should be stronger at firms with 50% of their workforce with children and smaller at the extremes. We test this hypothesis in Figure 2.11. 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. The analysis shows an increasing rather than U-shaped relationship (the effect increases with firm exposure).

In future work we would like to test whether there was 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 getting away with cheating the union contracts was harder to do after the reform because employers lacked accurate information on beneficiaries and transfer amounts.

2.7 Conclusion

We study whether the way family allowances are disbursed matters for the final incidence of the transfer. To test the standard neutrality hypothesis from the neoclassical model, we exploit a change in the remittance system of family allowances in Argentina. Under the old system, employers were in charge of disbursing the transfer to eligible employees and had the right to net these payments out of employer SSCs. Under the new payment system, the SSA eliminated the intermediary role of firms and started disbursing the transfer directly into workers' bank accounts. Hence, the transfer became less salient to employers.

Due to capacity constraints, firms were gradually incorporated into the new scheme over the course of eight years. We combine this gradual roll out, an event study design, and employer-employee administrative data to identify wage effects of means-tested transfers.

In the first part of the paper we showed that the way family allowances are disbursed is not neutral. The monthly wage of workers with children increases by 9 pesos relative to workers without children when firms stop disbursing the transfer (and the SSA delivers them instead). This effect declines as we move up in the income distribution where the average tax rate and salience of the transfer are smaller. In terms of the pass-through rate, our estimates imply that employers were capturing about 10-20 percent of the transfer when they mediated its disbursement.

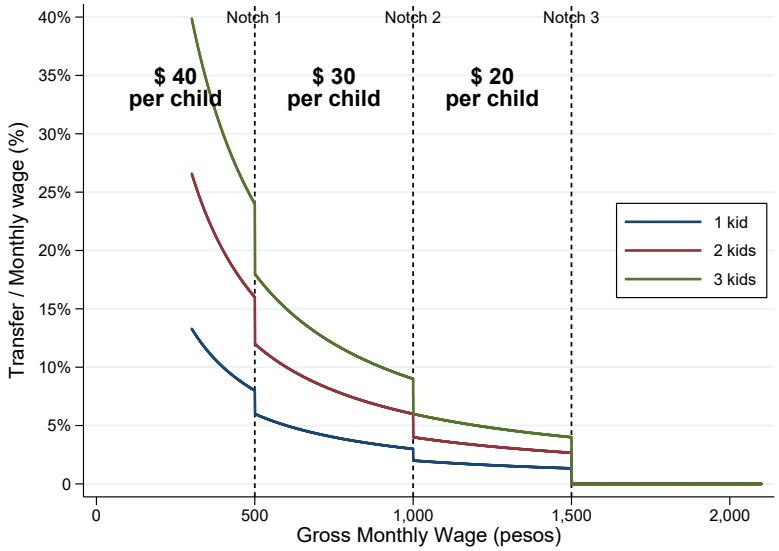
In the second part of the paper we explore some of the mechanisms and argue that the increase in monthly wages after the event is more consistent with a labor demand story rather than pay equity concerns. The key piece of evidence is that the effect is explained by new hires rather than incumbent workers. Intuitively, when firms are no longer in charge of delivering the transfer, they cannot integrate it in the wage package anymore and thus the market wage of new hires with children goes up. The fact that the effect appears immediately in the first month post event, and that it is driven by new hires, go against a pay equity concern channel, since one would expect the effect to build up slowly and also to affect incumbents. In addition, we show that wage effects are stronger the higher the exposure of a firm to family allowances.

These findings therefore suggest that the way governments set up tax credit programs, like the EITC in the U.S., influences the final economic incidence. We find that wages do adjust to the way transfers are disbursed, rejecting the null hypothesis that transfers are *all* captured dollar for dollar by workers. Our results exhibit a great promise of informing policy debates on some of the consequences of decentralizing sensitive tasks prone to irregularities, such as the disbursement of tax credits. This is an important aspect of welfare programs that is relevant for many countries and is still understudied. For instance, in Brazil, Chile, Paraguay, Italy, and Switzerland family transfers for employees in the formal sector are disbursed through employers. Our results suggest that such schemes designed to help workers are prone to fraud and irregularities when firms operate as mediators.

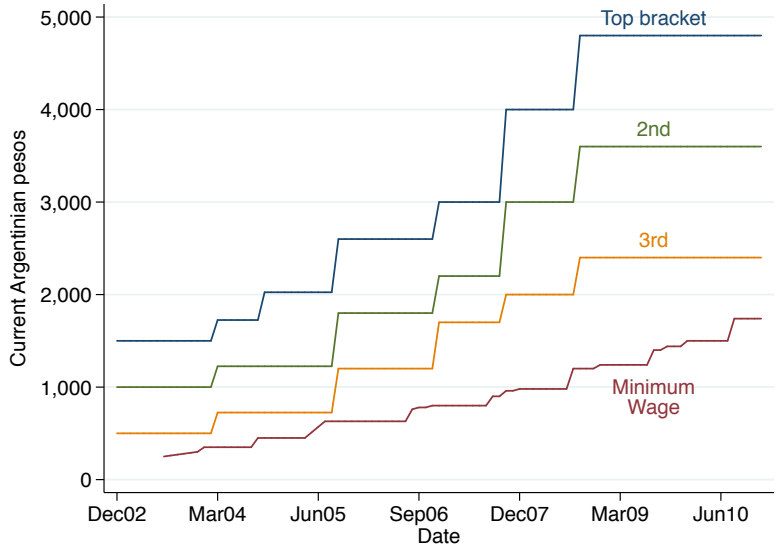
Figures and Tables

Figure 2.1: Family allowance schedule

(a) Average tax rate (1996-2004)

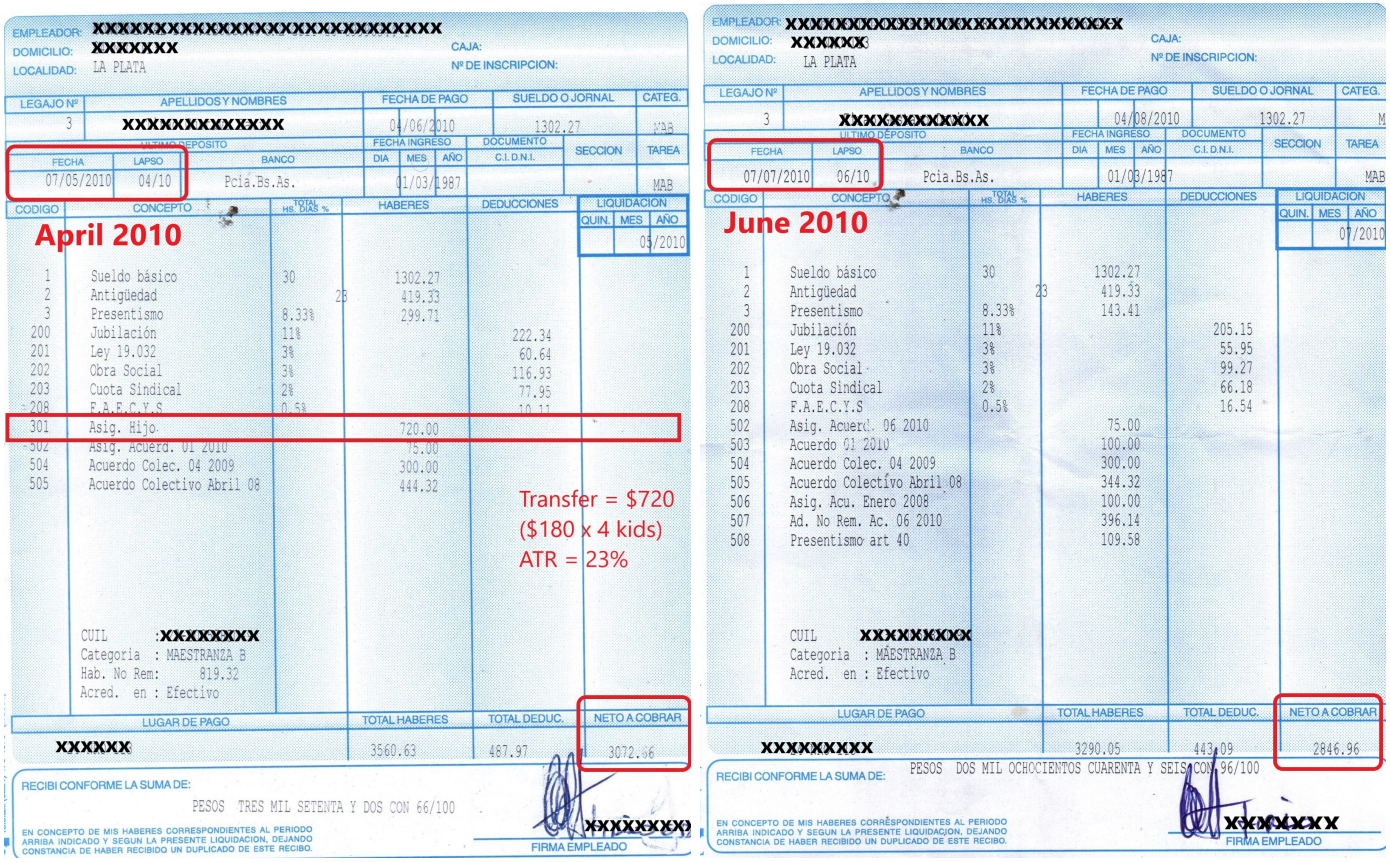


(b) Bracket thresholds (2003-2011)



Source: own elaboration based on official documentation.
Notes: Panel (a) shows the average tax rate (the ratio of transfer to gross earnings) over monthly gross 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 2.2: Saliency of the transfer on a worker's pay slip

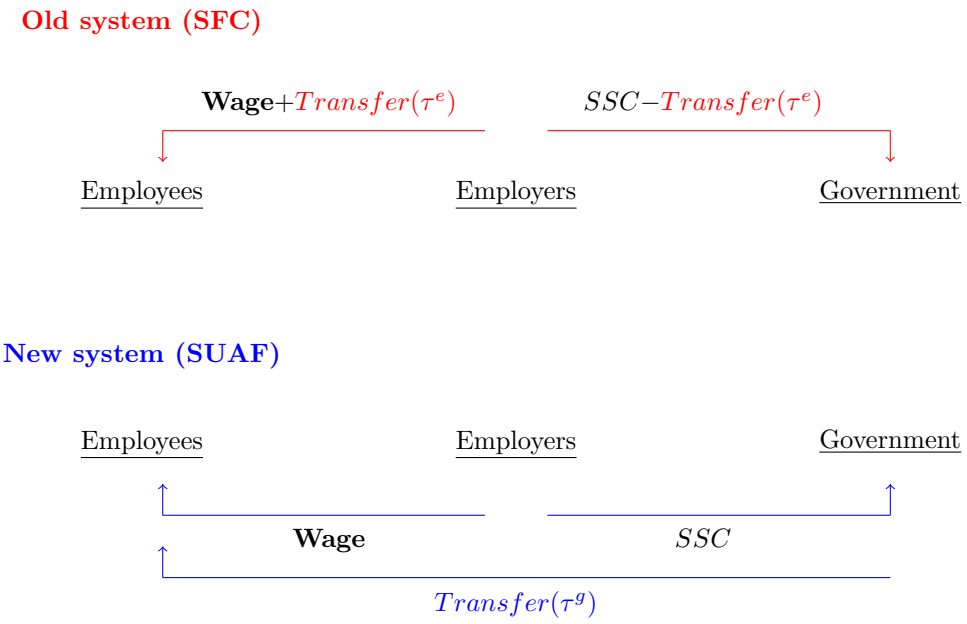


(a) Before the firm switches

(b) After the firm switches

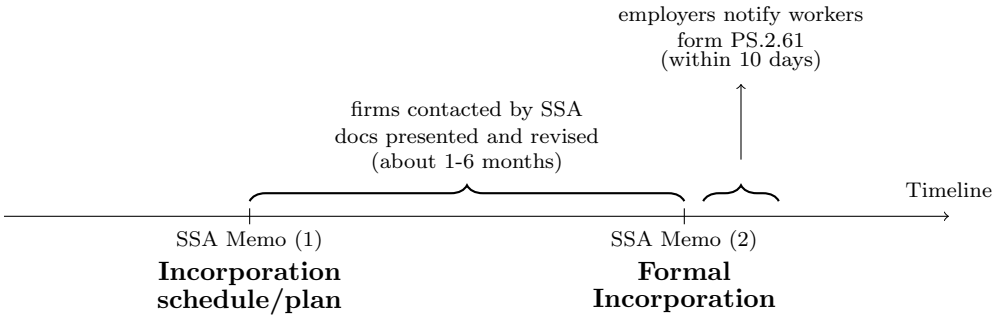
Notes: This figure shows the pay slip of a worker right before and right after the firm switches from the old payment system (SFC) to the new payment system (SUAF). In the old system (panel a) the pay slip contains a line for the transfer making it very salient to both the employer and the employee. The amount that this worker receives is 720 Argentinian pesos corresponding to an average tax credit of approximately 25% of total earnings. In the new system (panel b) that line disappears and the worker gets the transfer directly from the government in his checking account.

Figure 2.3: The reform: a change in the payment system



Notes: This figure illustrates the change in the payment system of family allowances. Under the old system (SFC), employers were in charge of 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 SCC to the IRS.

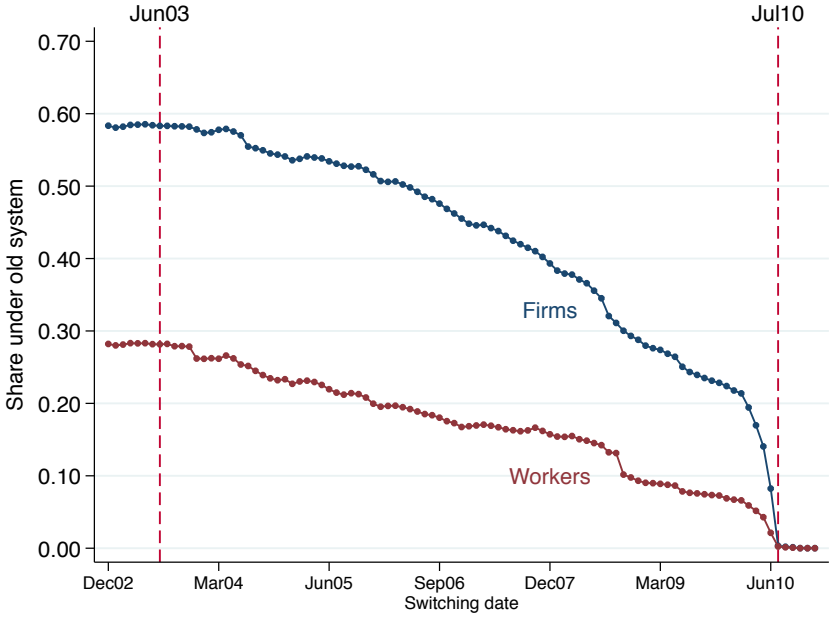
Figure 2.4: Firms' incorporation steps into the new payment system



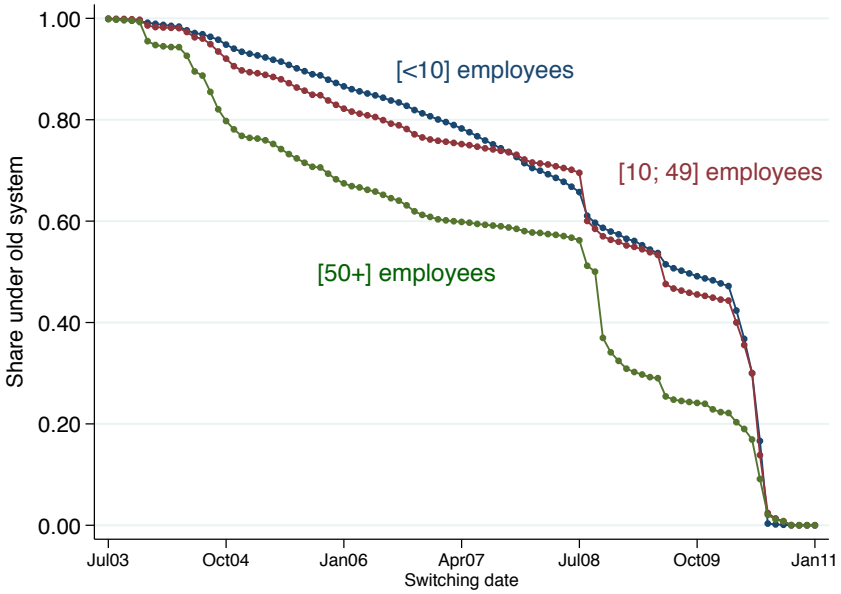
Note: this figure shows the timeline of the incorporation steps to the new payment system (SUAF). This process was determined by the SSA through official memos posted online. The incorporation started with the SSA setting an *internal* incorporation schedule, where basically the SSA issues a list of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

Figure 2.5: Gradual roll out from the old to the new system

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

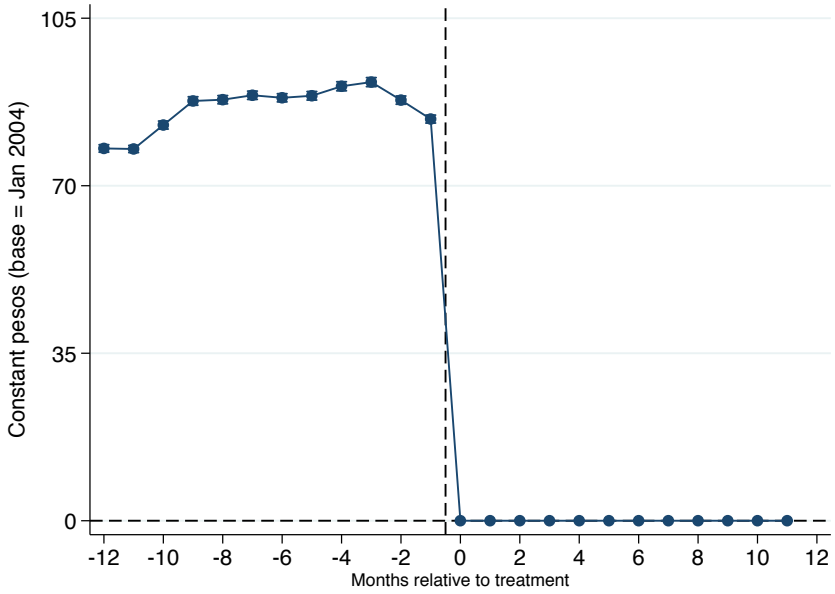


(b) **Roll out by firm size**



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 microdata. Panel (b) is restricted to our estimating sample and breaks the roll out by firm size proxied by number of employees in 2003. Source: own elaboration based on employer-employee micro-data.

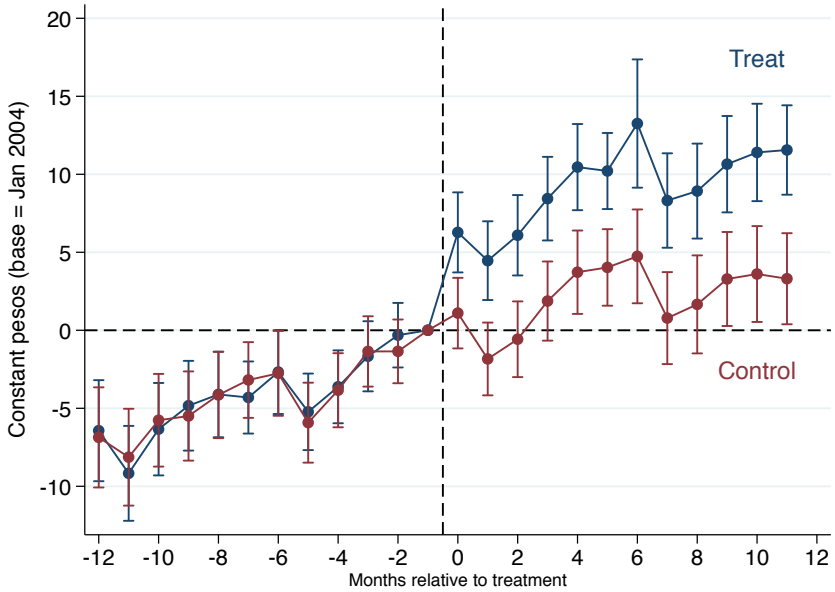
Figure 2.6: First stage change in the remittance at the event



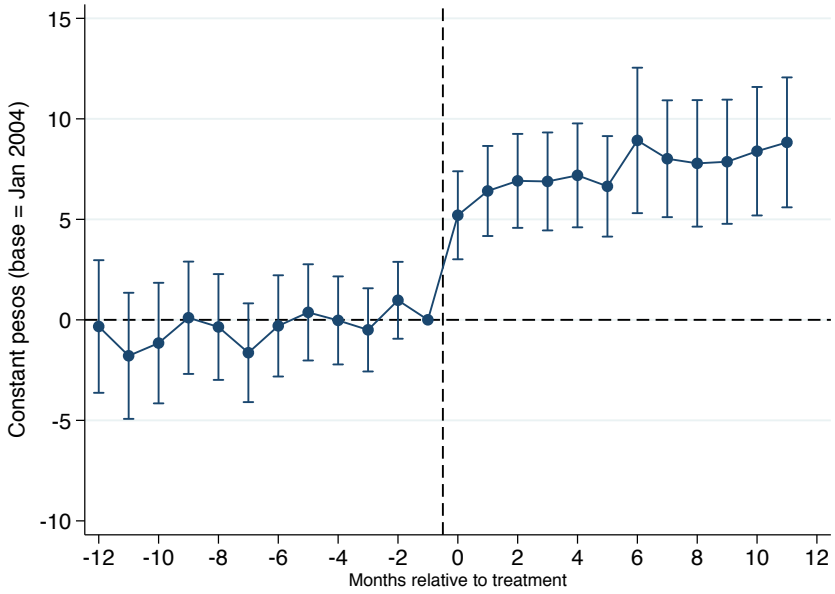
Notes: This figure presents event-study estimates of the parameter γ and its corresponding 95 percent confidence intervals of equation (2.1). The dependent variable is the within-firm difference in average family allowance of workers with and without children. It shows that when firms transition to the new system they immediately stop disbursing the transfer and hand this task over to the government. On average, workers with children were receiving ~ 90 pesos more in transfers per month, disbursed by employers, than workers without children.

Figure 2.7: Reduced-form wage effects

(a) Average wage levels

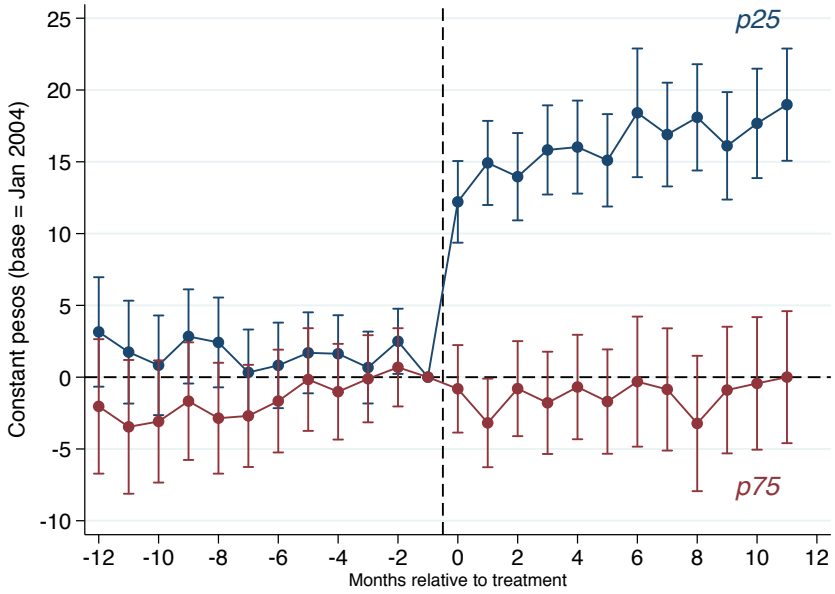


(b) Average wage gap



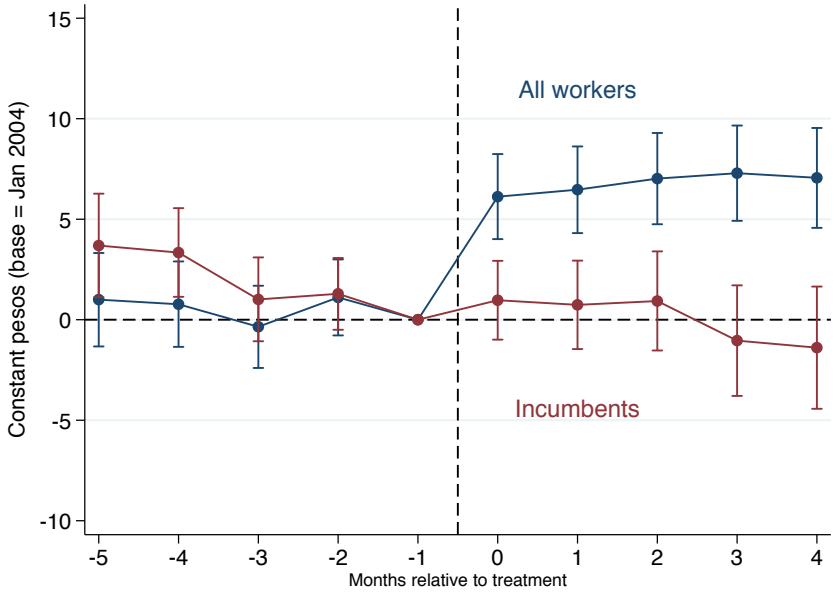
Notes: these figures plot the event-study estimates of the parameter γ and its corresponding 95 percent confidence intervals of equation (2.2). Panel (a) shows the wage levels when we estimate this equation separately for workers with children (treat) and without children (control). In Panel (b) the dependent variable is the within-firm average wage gap of these two groups. It shows that monthly wages increase by approximately 10 pesos when firms stop disbursing the transfer to eligible workers.

Figure 2.8: Reduced-form wage effects: $p25$ vs $p75$



Notes: these figures plot the event-study estimates of the parameter γ and its corresponding 95 percent confidence intervals of equation (2.2). 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 at the lower end of the distribution but not in the upper part.

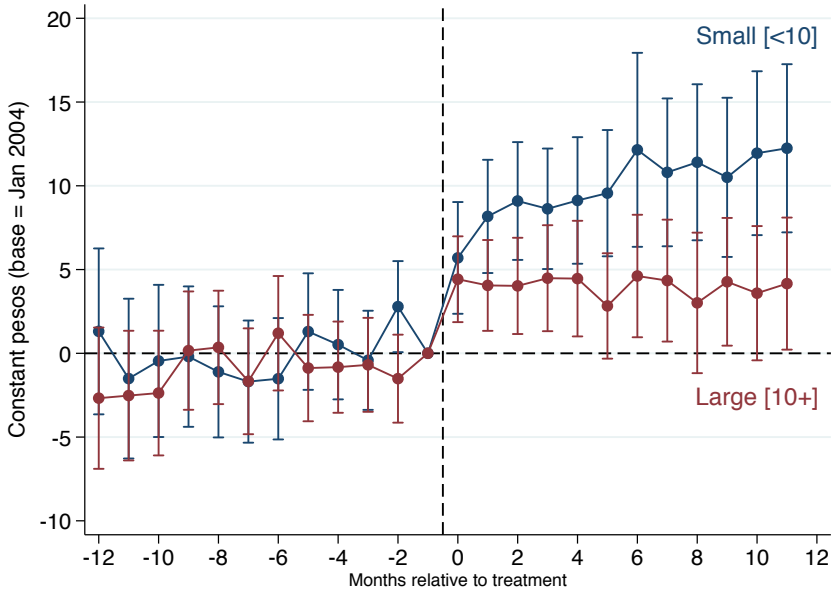
Figure 2.9: Wage effects: new hires and incumbents



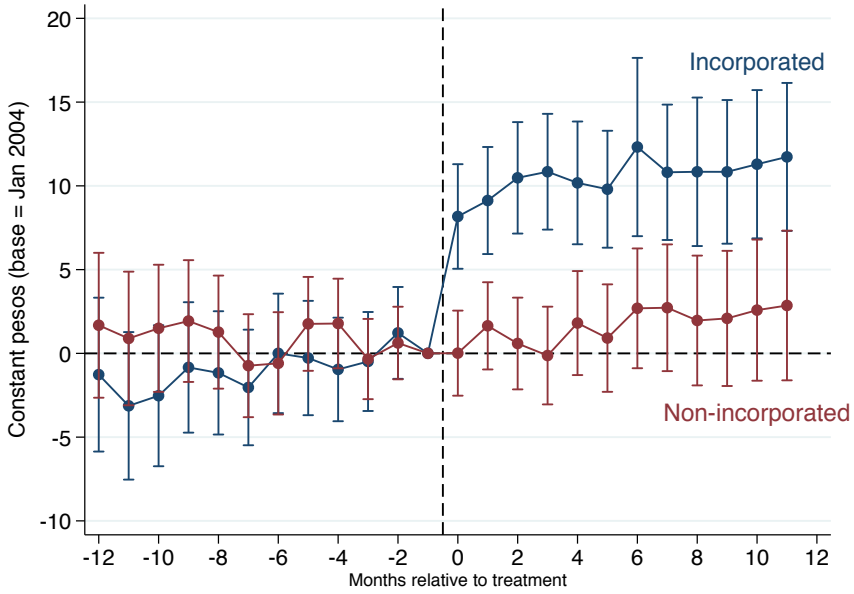
Notes: this figure plots the event-study coefficients and 95 percent confidence intervals of equation (2.2). The dependent variable is the gross monthly wage gap between workers with and without children. We run two different regressions: the blue line includes all the workers and the red line only considers a balanced panel of employees present at the firm in the whole window. The difference between these two lines captures new hires.

Figure 2.10: Heterogeneities: firm size and type of business

(a) Small vs. large firms



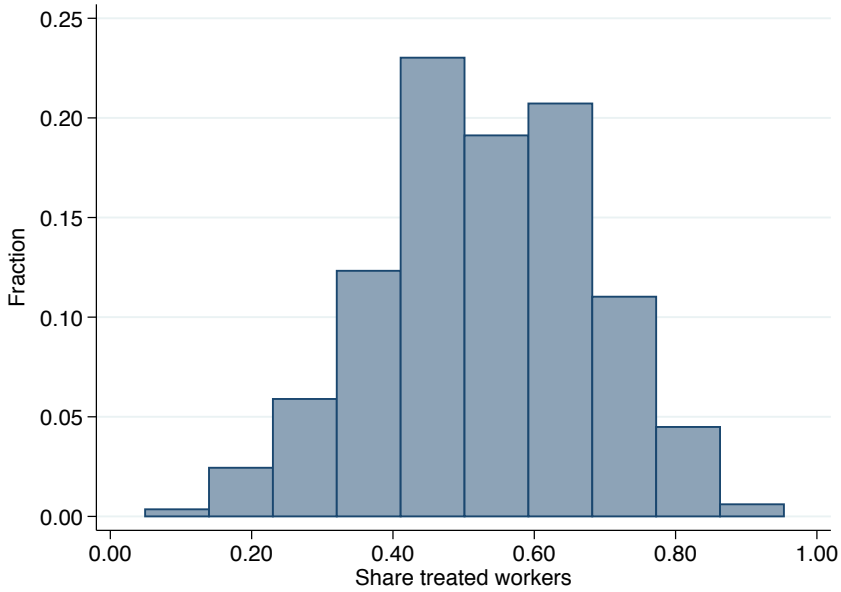
(b) Incorporated vs. unincorporated



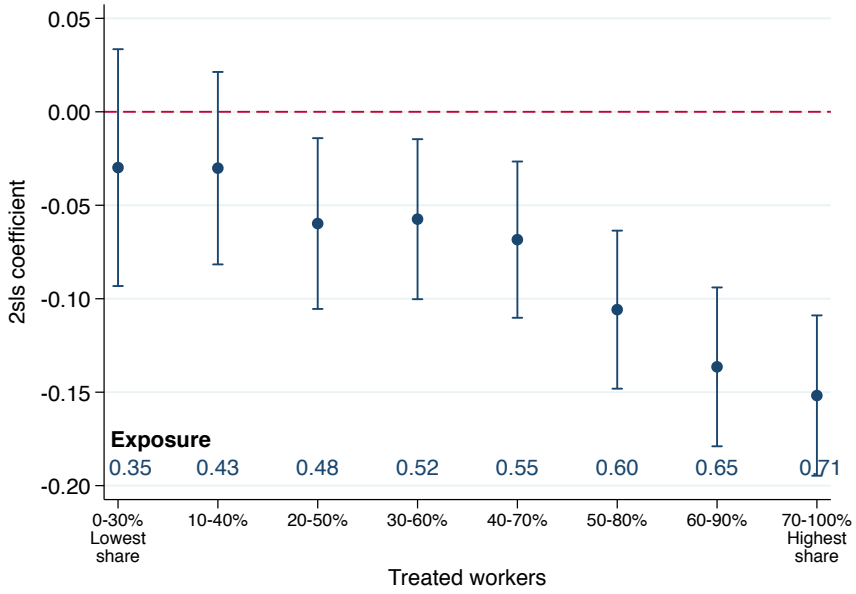
Notes: Panel (a) presents event study estimates of the parameter γ and its corresponding 95 percent confidence intervals of equation (2.2) for small firms and large firms. Panel (b) plots the event study estimates for incorporated and unincorporated businesses.

Figure 2.11: 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 event study estimates for different breaks of firm exposure. Each dot corresponds to a separate regression.

Table 2.1: 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*
Salience	*Pay slip*	*Less salient to employers and employees*
Tax-benefit linkage	*Low (?)*	*Higher (?)*
Source of funding	*Contributory Employer SSC*	*Contributory Employer SSC*
Transfer's claiming procedure	*Employer (?)*	*Employee (?)*

Note: The first column refers to the *Sistema de Fondo Compensador* (SFC) while the second to the *Sistema Único de Asignaciones Familiares* (SUAF), the old and new payment scheme respectively.

Table 2.2: Summary statistics for registered salaried workers in Argentina, 2004

	1st Bracket	**2nd Bracket**	**3rd Bracket**	**Universe**
	(1)	(2)	(3)	(4)
Salaried workers	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

Note: This table displays summary statistics for private formal salaried workers in April 2004. The number of employers is 396,334.

Table 2.3: Wage effects and pass-through of a change in the remittance system

	All post periods [0;11] (1)	**Last 6 periods** [6;11] (2)	**Last period** [11] (3)
Reduced form			
Δ monthly wage	7.71***	8.74***	9.23***
(in pesos)	(1.25)	(1.55)	(1.87)
First stage			
Δ transfer (τ^e)	-90.98***	-92.17***	-91.44***
(in pesos)	(0.35)	(0.37)	(0.37)
2sls			
$\frac{\Delta wage}{\Delta transfer(\tau^e)}$	-0.08***	-0.09***	-0.10***
	(0.01)	(0.02)	(0.02)
Number of firms	35,787	35,787	35,787
Observations	3,061,870	2,847,148	2,670,757
Avg wage at t-1	868	868	868

Note: in this table we report the reduced-form and 2SLS estimates from the event study. In the first panel, we pool the coefficients from Figure B.10 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 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 are clustered at the firm level are reported in parentheses. *** significant at 1%, ** significant at 5%; * significant at 10%.

Table 2.4: Wage effects and pass-through by firm size and type of bussines

					Incorporated	
	Small [< 10] (1)	**Large** [10+] (2)	**Non Incorpo** (3)	**Incorpo** (4)	**Small** [< 10] (5)	**Large** [10+] (6)
Reduced form						
Δ monthly wage	9.86***	4.94***	0.81	11.37***	19.27***	5.73***
(in pesos)	(1.96)	(1.55)	(1.74)	(1.72)	(3.30)	(1.81)
First stage						
Δ transfer	-97.02***	-82.62***	-96.86***	-87.52**	-94.32***	81.97***
(in pesos)	(0.54)	(1.55)	(0.65)	(0.41)	(0.75)	(0.40)
2sls						
$\frac{\Delta wage}{\Delta transfer(\tau^e)}$	-0.10***	-0.06***	-0.01	-0.13***	-0.20***	-0.07***
	(0.02)	(0.02)	(0.02)	(0.02)	(0.04)	(0.02)
Number of firms	20,253	15,534	13,029	22,758	9,843	12,915
Observations	1,694,509	1,367,361	1,080,767	1,981,103	833,347	1,080,767

Note: in this table we report the reduced-form and 2SLS estimates from the event study. In the first panel, we pool the coefficients from Figure B.10 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 (less than 10 employees) and large firms (10 or more 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 are clustered at the firm level are reported in parentheses. *** significant at 1%, ** significant at 5%; * significant at 10%.

Table 2.5: Survey evidence about the understanding of family allowances (2018)

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

Note: this table shows the results from a survey carried out by the social security administration (Anses) where they asked people if they knew who was responsible of paying family allowances in Argentina. Option C includes: N/A; the call got interrupted, the bank. Source: based on a SSA report (Cruces, 2019).

2.8 Transitional Section to Chapter 3

The evidence presented so far shows that wage earners barely respond to large, salient, and temporary tax changes (Chapter 1) and also to discontinuities in the transfer schedule at the lower end (Chapter 2). A series of exercises provide suggestive evidence that employers seem to mediate tax and transfer responses. Moreover, when it is in the interest of employers, they take advantage of the tax system to extract rents by lowering wages (Chapter 2).

Unlike wage earners, self-employed workers and businesses do not depend directly and vertically on other parties. Hence one would expect them to be more responsive to taxation. In Argentina, independent workers and firms face special tax regimes characterized by revenue-based discontinuities that induce them to underreport income. In the next Chapter, we turn the attention to these two groups to study whether they are more responsive than wage earners. Our setting allows to measure taxable income responses across the revenue distribution. This allows to test whether responses decrease as we move up in the distribution. One would expect larger firms to find it more difficult to evade because they are more in the public eye of the tax authority.

Chapter 3

Taxpayers' Responses to Tax and Administrative *Notches* across the Revenue Distribution

3.1 Introduction

A central question in public and labor economics is the way workers and businesses respond to the action of governments in the economy (e.g., to taxes, subsidies, and welfare programs). Pioneer studies showed simple correlations ignoring some of the endogeneity problems. The marginal revolution and the availability of large administrative datasets, have led to a boom of research that studies behavioral responses to government policies. Nevertheless, the evidence is still scarce and inconclusive, particularly in developing countries where governments are more reluctant to share information.

The vast majority of the literature has documented labor supply responses to taxes considering the standard margins; the extensive (decision to work or not) and the intensive (how much to work). However, individuals may react in many dimensions including job effort, career choice, form and timing of compensation, and tax avoidance and evasion, among other dimensions. Indeed, Feldstein (1999) stated that observed responses in taxable income provide a broader concept of behavior as compared to the traditional labor supply analysis. Therefore, if the taxable income elasticity is greater than the labor supply elasticity, the resulting deadweight loss induced by taxation will also be greater. The study of the elasticity of taxable income is relatively new as compared to the standard labor supply elasticity (Saez et al., 2012b).

In this paper we study the response of firms to two different revenue taxes, *Monotributo* and the Gross Receipts Tax.[1] Our laboratory is Argentina, which offers very rich administrative data and quasi-experimental variation provided by discontinuities in the tax schedules

[1]The Gross Receipts Tax is also known as Turnover Tax (in spanish, *Impuesto sobre los Ingresos Brutos*). We use these terms interchangeably.

(known as *notches*). *Monotributo* is characterized by 11 income categories with different tax liabilities. This federal tax is a simplified regime for small entities that replaces the income tax, the value added tax, and social security contributions. The Gross Receipts Tax is characterized by a 3-bracket schedule with tax rates and thresholds that vary by sector and are increasing in income. This is a sub-national tax levied on gross receipts in every stage of the supply chain. Importantly, the discontinuities of *Monotributo* affect relatively small businesses and the discontinuities of the Gross Receipts Tax affect relatively large firms. The number of discontinuities in the tax liability, jointly with the existence of two taxes that share the same tax base, provide an ideal setting to estimate revenue responses for taxpayers located at very different parts of the gross income distribution. We look at small and large taxpayers, as well as self-employed workers and firms that vary in size. Besides the experiment, an interesting feature of our setting is that it takes place in a context of high evasion, informality, inflation, and somehow, weak enforcement.

Our research design exploits the presence of *notches* along the revenue distribution to estimate behavioral responses to taxes using the bunching approach developed by Kleven and Waseem (2013).[2] This technique approximates individuals' responses to a given policy by comparing the observed distribution with a counterfactual one. Under some conditions, it can be shown that the excess of mass of the observed distribution around a given *notch*, can be understood as the individual response to the tax, and therefore be interpreted as the elasticity of taxable income.[3]

Our findings show substantial bunching of small taxpayers below the thresholds of the first 8 categories in the *Monotributo* regime. The absence of *missing mass* above the discontinuities suggests that some entities face substantial adjustment frictions or inattention. The intensity of bunching is stronger for higher discrete jumps in average tax rates. In the remaining 3 notches, however, bunching is less evident, which can be justified by a higher composition of taxpayers with less flexibility to adjust their reported revenue. We also document that taxpayers in services react the most, followed by agriculture and manufacturing, and finally wholesale and retail. We interpret these behavioral responses as suggestive evidence that a significant number of taxpayers attempt to avoid higher tax liabilities by keeping their reported revenue below the thresholds.

In the case of medium and large taxpayers ($\sim$ above percentile 75 of the revenue distribution), although bunching to turnover tax notches is not as striking as in the first 8 notches of *Monotributo*, the evidence suggests that some large firms are able to manipulate their reported sales to avoid facing higher tax rates. We also find that bunching is stronger for an administrative discontinuity that forces firms to work as collection agents if they cross a revenue threshold. This could imply that firms find more costly this indirect administrative

[2]Saez (2010) initiated this literature by exploiting kinks at which *marginal* tax rates jump to estimate taxable income elasticities. Kleven and Waseem (2013) extended this methodology to notches where *average* tax rates change discretely. The survey by Kleven (2016) summarizes the growing literature using bunching estimation techniques.

[3]Indeed, Kleven and Waseem (2013) show that the compensated elasticity is a function of the change in the tax liability and of the change in reported taxable income.

cost than the direct fiscal cost of the turnover tax. When we split the analysis by sector we find that the manipulation is mainly driven by service and manufacturing firms. Our analysis cannot identify, however, general equilibrium effects (e.g., cascading) or rule out that taxpayers react in other dimensions (e.g., fiscal externalities to other tax bases).

Our paper contributes to a recent but growing literature on firm responses to tax and administrative regulations in developing countries. A strand of literature exploited kink points in firms' budget sets provided by minimum tax schemes where firms pay the largest tax liability between a tax on profits and a tax on turnover (e.g., Best, Brockmeyer, Kleven, Spinnewijn, and Waseem (2015) in Pakistan and Alejos (2018) in Guatemala). This policy is motivated by the idea that the broader turnover base is harder to evade. In Argentina, however, firms face both national corporate taxes and subnational turnover taxes. In the same vein, Bachas and Soto (2018) use notches in the tax schedule to estimate profits' responses to corporate taxes in Costa Rica. They estimate a very high elasticity of reported profits, which is entirely driven by evasion and cost-deductibility, providing support in favor of taxing turnover.

More generally, our article is related to recent research on firm responses to revenue-dependent regulations that provide incentives to underreport revenue, such as administrative notches and VAT registration thresholds where firms are required to register and charge VAT on all sales (e.g., Onji (2009) in Japan, Boonzaaier, Harju, Matikka, and Pirttilä (2017) in South Africa, Rauhanen, Harju, and Matikka (2016) in Finland, Liu, Lockwood, and Almunia (2017) in the U.K., and Asatryan and Peichl (2017) in Armenia). Almunia and Lopez-Rodriguez (2018) also show that in a more developed economy such as Spain, firms underreport revenue to avoid stricter monitoring from the tax authority. Overall, this literature provides suggestive evidence that income under-reporting drives the bunching response of firms. Note, however, that a common feature of all these papers is that the regulations allow to study either small or large firms. Our setting instead applies to small, medium, and very large firms, and thus lets us measure the responses across the revenue distribution in a comparable way.

The magnitude of firm responses to features of the tax code is of utmost importance in the formulation of tax policy and the assessment of efficiency costs. From a policy perspective, this article pins down policy-relevant elasticities that enable a better understanding of two taxes for which there is very limited causal evidence. Evaluating tax-filers' behavioral responses across the turnover distribution is important for several reasons. First, there are still inconclusive estimates of the magnitude of this elasticity, especially for developing countries where the institutional setting differs to the one present in developed economies. For instance, the possibility to shift income due to lower enforcement capacities is higher. Second, unlike previous studies, our setting and data allow us to study the response of large firms, small firms, and self-employed workers (with presumably different elasticities) in a comparable way. Namely, by exploiting discontinuities in average tax rates and using the same source of administrative data which contains the reported tax base that applies to both taxes. The self-employed typically have more flexibility, and therefore the reaction to the policy could be greater. Third, the results from this study will be of great value for national

and sub-national governments in developing countries with similar tax systems. Moreover, our paper could also be of interest to policymakers in more developed federal countries, such as the United States, where some states currently have a gross receipts tax (e.g., Washington State) and some others are considering its implementation (e.g., Oregon). As such, the results from this research exhibit great promise of informing policy debates.

The paper is structured as follows. Section 3.2 describes the institutional details of the two taxes that are analyzed in the empirical section. It also briefly describes the administrative data. Section 3.3 presents the analytical framework and behavioral responses at the lower end of the gross income distribution. Section 3.4 focuses instead on the responses at the upper end of the distribution. Finally, Section 3.5 concludes.

3.2 Institutional setting and data

Argentina is a federal country where the activity of businesses is taxed both at the national and the sub-national level. At the national level, medium and large firms face the Value Added Tax (VAT) and the Corporate Income Tax (CIT), and self-employed and small businesses face a simplified tax regime called *Monotributo*. In addition, at the sub-national level firms and self-employed workers are taxed by the Gross Receipts Tax (GRT). Taxes at the national level are collected by the Federal Administration of Public Revenue (AFIP, in Spanish), and taxes at the sub-national level are collected by provincial (state) tax agencies.

The *Monotributo* and GRT are both levied on gross income before costs and taxes are deducted, which make them more distortive than other standard taxes. In addition, their differential tax schemes provide quasi-experimental variation at different parts of the revenue distribution and different incentives to respond to the tax. In this paper, we will exploit such variation to analyze the response of firms to these two taxes. For data availability, we focus the attention to the Province of Buenos Aires (PBA), which is the largest province in Argentina in terms of population (16 million), area (11% of the country), and GDP (the largest gross state product, almost 40% of the national total). This implies that if PBA were a country, it would be the 6th economy in Latin America, very close to Chile. In the rest of this section, we describe the features of each tax and how they can affect firm's behavior.

Monotributo

Monotributo is a simplified tax regime for small taxpayers that was created in 1998 with the goal of inducing self-employed and small firms to enroll into the formal system by the simplification of tax duties as well as social security contributions (pension and health coverage) (Law 24.977).[4]

[4] As of September 2017, there were about 1.5 million registered self-employed and small firms. Monteiro and Assunção (2012) and Fajnzylber, Maloney, and Montes-Rojas (2011) analyzed the effects of a similar program in Brazil called SIMPLES. Both papers found that the program leads to an increase in the probability of registering a business and becoming formal.

This regime has three main components, one that refers to taxes and the other two to social security contributions (SSC). First, the regime simplifies the tax duties of taxpayers by the unification of two pre-existing taxes, personal income tax and value added tax. The result of this is an integrated tax, *impuesto integrado*, that gave also the name to those registered into this system i.e., *monotributistas*, and also to the tax i.e., *monotributo*. Secondly, it contains a component linked to contributions for retirement pension. These are then accounted in the Argentinean pension system (SIPA, *Sistema Integrado Previsional Argentino* in Spanish). Finally, the last component comprises health contributions, generally known as *obra social*, and provides access to health care.

Taxpayers are subject to a combined monthly fee that varies by gross income level accrued in the last twelve months based on 11 categories. Table C.1 presents the 11 brackets and corresponding upper thresholds since January 2010. The monthly fee comprises a tax that varies by bracket (e.g., see Table C.2) and a flat SSC component for retirement and health that do not vary by income level (e.g., see Table C.3). Taken together, this means that if a taxpayer makes 1 cent more than the upper threshold, then the tax fee increases discretely while the SSC remains constant. Hence, the structure of the regime is characterized by 11 discontinuities or *notches*, as depicted in panel (a) of Figure 3.1. Two points are worth noting. First, the average tax rate is overall increasing across brackets and decreasing within brackets (this is due to the structure of the tax i.e., a fixed fee for those in a given bracket). Second, the tax schedule is such that the average tax rate increases from 1% to 6% with 11 *notches* that create space for behavioral responses. These discontinuities provide strong incentives to locate to the left of the thresholds to pay lower taxes.

Gross Receipts Tax

Each of the 24 provinces into which Argentina is divided imposes a tax on gross revenues from the sale of goods and services, the so called *Impuesto sobre los Ingresos Brutos*. This Gross Receipts Tax (GRT) is the main source of own revenue in all of these jurisdictions. In the Province of Buenos Aires (PBA), the laboratory of this article, it represents about 75% of tax revenue and currently raises 4% of the national GDP (Figure C.1).[5]

The GRT is determined on the basis of gross income accrued by any firm or independent worker selling goods or providing services inside a particular province. Tax rates vary according to the taxpayer activity, annual turnover from the previous year, and the place where the transaction takes place (inside or outside the province). Taxpayers are classified as *contribuyentes locales* when performing the activity inside the province or as *contribuyentes de convenio* when they operate both inside and outside the province (this set of firms is part of the so-called *Multilateral Agreement*). For firms operating in multiple provinces, there exists an apportionment regime to distribute the tax base. The formula is based on a sales factor and an expenditure factor determined at year t-1 (*coeficiente unificado*). In particular, 50% of total revenue is distributed according to the ratio of a taxpayer's sales in the taxing

[5]The other taxes levied at the provincial level are: the stamp tax, property tax, car tax, and inheritance tax.

province to its overall sales; and the other 50% is distributed according to the ratio of a taxpayer's expenditures in the taxing province to its overall expenditure. Importantly, firms selling in foreign provinces might be taxed at differential tax rates (as if there were internal customs). This type of firms are called "Foreign Jurisdiction" firms.

In the province of Buenos Aires, economic activities are classified into 791 sectors that can be taxed either under a general regime or a differential regime. In the *general regime*, there are 647 activities that face a three-bracket progressive tax schedule based on annual turnover from the previous year (or the first two months when the firm is new). That is, a firm's tax rate at year t depends on whether annual turnover at year $t-1$ is above or below a threshold. If a firm crosses the threshold, then the tax rate jumps and is applied to all the tax base.[6] Hence, this revenue threshold represents a *notch* because the average tax rate changes discretely. In the *differential regime*, the remaining 144 activities are taxed at differential flat tax rates.

The provincial Tax Act from Buenos Aires defines three salient features of the GRT that are critical for the empirical strategy. Without loss of generality, in Figure 3.1 panels (b)-(d), we summarize these features for the year 2015. The law establishes progressive tax rates based on annual gross revenue from the previous year, y_{t-1}, for three broad groups of activities classified in the law as follows:[7]

- **Type A (wholesale and retail)**: the main article sets a tax rate of 5%; another article reduces the rate from 5% to 3.5% when $y_{t-1} \leq 40$ million pesos; and another article reduces the rate from 3.5% to 3% when $y_{t-1} \leq 1$ million pesos. "Foreign Jurisdiction" firms are always taxed at 5%.

- **Type B (services)**: the main article sets a tax rate of 3.5%; another article increases the rate from 3.5% to 4% when $y_{t-1} > 500$ thousand pesos; and another article increases the rate from 4% to 5% when $y_{t-1} > 30$ million pesos.

- **Type C (agriculture and manufacturing)**: the main article sets a tax rate of 4%; another article sets a rate of 1.75% for firms based in PBA with $y_{t-1} > 60$ million pesos; another article sets the rate to 0.5% for firms based in PBA with $y_{t-1} > 40$ million pesos; the remaining firms based in PBA with $y_{t-1} \leq 40$ million pesos face a 0% tax rate. Hence, in practice "Foreign Jurisdiction" firms are the only ones with a statutory tax of 4%.

Each firm has to discriminate its taxable base by the type of activities they carry out and apply the corresponding tax rate in each case. For example, a large manufacturing firm with gross sales of 65 million in 2014 that also sells to final consumers would be taxed at 1.75% for manufacturing sales and 5% for retail sales. Importantly, every province passes a

[6]Note that the tax base coincides with annual turnover for *contribuyentes locales* but not for *contribuyentes de convenio* where the apportionment formula must be applied first.

[7]The tax was initially designed such that tax rates increase as the product or service approach the final consumer to alleviate inefficiencies in the supply chain.

new law every year where they either rectify and/or revise tax rates and revenue thresholds. In fact, these parameters have been modified several times during the period of analysis, as shown in Table C.4.

There is also a regulatory provision that forces firms with y_{t-1} greater than a certain cutoff to work as *collection agents* for the tax authority. These firms are required to withhold the GRT at source from suppliers and non-final customers as payments on account. While this regulation improves the enforcement and revenue potential of the tax, it also increases significantly the accounting costs of the affected companies. Between 2004 and 2009, the threshold that determines whether a firm must act as a collection agent remained fixed at 5 million pesos. In 2010, it was updated to 10 million pesos and remained fixed until 2016 when it was updated to 20 million pesos. The lack of update of the amounts that determine this duty during 2004-2009 and 2010-2015, and the context of high inflation led many SMEs to join the system, causing additional costs due to the lack of administrative structure to meet this fiscal requirement. It is also said that the duplication of the threshold in 2016 affected 30% of the collection agents, most of them SMEs, which benefited from a significant cost reduction in their administrative structure.

All these features and the variation (or lack of variation) of some parameters over time provide an ideal setting for estimating the response of firms to tax and administrative *notches*, as explained in the following section. In this paper we will focus on the activities affected by the tax *notches* of the general regime and the administrative *notch* to become a collection agent. We leave the analysis of specific activities taxed at differential tax rates for future research.[8]

From a theoretical point of view, these taxes on gross income are usually criticized due to the introduction of multiple distortions. The main point is that the tax is levied at every stage of the production process, including intermediate business-to-business purchases of supplies, raw materials, and machinery. Besides the distortion of input prices, cascading effect represents a distinctive feature, since the sale price of a good includes the tax accrued in all the previous stages. As a result, cascading encourages vertical integration since firms may seek to avoid taxes on their inputs by producing them in-house. It also affects the competitiveness of goods that are locally produced and traded, either because in the export they cannot fully recover the locally imposed taxes or because in the import they face goods that come from countries with neutral tax schemes, free from indirect tax burden to exportable products. Finally, cascading could increase the cost of capital when capital inputs are taxed, affecting productive efficiency (Libonatti (1998), Moskovits and Susmel (2006) and Keen (2014)).

[8]These tax rates vary widely across activities and over time. The number of tax rates has been increasing over time. While in 2007 there were 7 tax rates that vary between 0.1% and 6%, in 2016 there were 18 tax rates that vary between 0% and 12%. For instance, between 2012 and 2013 tax rates went from 8% to 12% for bingo rooms and slot machines but remained constant for the other recreational activities. This represents an interesting setting to perform a difference-in-difference analysis.

Administrative Data

To estimate the response of taxpayers to the different discontinuities (i.e., *notches*) we use administrative tax records from the Province of Buenos Aires, Argentina, for the period 2011-2016. The data cover the universe of firms and self-employed workers who must file their GRT returns electronically on a monthly basis. It contains about 750,000 taxpayers per month and includes standard information such as scrambled identifiers, type of activity (around 438 different codes), type of business (LLC, Inc, Partnerships, etc.), monthly revenue, monthly tax liability, tax rates, an indicator for exempt activities, tax withholdings, etc. Importantly, all the records were de-identified and all the computations were performed at the Ministry of Economy of the Province of Buenos Aires.[9]

A key feature of this dataset is that it contains the tax base of the two taxes that serve as a quasi-experiment. Gross revenue, is the variable that determines in which category taxpayers fall within the *Monotributo* scheme, and the same variable is used to determine the tax rate and tax liability of the GRT. To the best of our knowledge, this is the first study that exploits this rich administrative database for academic purposes.

3.3 Behavioral responses at the lower end

In this section we analyze the behavioral responses to revenue taxation of self-employed workers and small businesses located at the lower end of the income distribution. To this end, we focus on the *Monotributo* simplified tax regime and use quasi-experimental variation provided by the discrete increases in average tax rates.

Conceptual framework

This regime is structured into 11 brackets (with 11 *notches*) as shown in Figure 3.1 panel (a) and Table C.1. The tax schedule can be written in the following way:

$$T_b(z) = F_b + [\Delta F_b] * \mathbb{1}(z > z_b^*) \tag{3.1}$$

where F_b is a fixed tax liability that should be paid in bracket b when revenue is below the *notch* z_b^*, ΔF_b represents a discrete increase in the tax liability above the threshold, and $\mathbb{1}(.)$ is a indicator function for being above this cutoff. This type of *notch* is generally known as a *pure notch*.[10]

Tax *notches*, as opposed to kinks (where marginal tax rates change discretely), create a strictly dominated area under which it is possible to increase both, consumption and leisure

[9]The Ministry of Economy of Buenos Aires is in charge of designing the provincial tax policy and passes a new tax law every year.

[10]The marginal tax rate on either side of z_b^* remains the same, but individuals with before-tax revenue greater than z_b^* incur a discrete increase in their tax liability.

at the same time.[11] [12] As a consequence, it generates strong incentives to *move* from a region above the threshold (z^*) to a point below. This happens due to the existence of an implicit marginal tax rate of more than 100% over a window of earnings imposed by the *notch*. Therefore, in a frictionless world, the empirical distribution of turnover should present a big mass below the threshold, where the tax liability is lower, and a missing mass above it. This gives rise to *excess bunching* and a *hole* in the earnings distribution.

Under the *Monotributo* scheme, individuals with practically the same gross income can face very different tax liabilities (and therefore their after-tax income will be different). Analytically, individuals face a budget constraint with *notches* at which they have incentives to *bunch*. Figure 3.2 presents graphically the analytical framework for the marginal "buncher". Without the tax, this agent would reach the indifference curve U_0. With the tax, the new budget constraint will present a *notch* at z^* and the person reaches the indifference curve U_1. In this case, she will be indifferent between the interior and the corner solution. Two things are worth noting; first, the figure clearly shows that two individuals with practically the same income before taxes, end up with different after-tax income. Second, the tax incentivizes individuals to the right of z^* to reduce their reported income (or work less) and thus pay a lower tax. Hence, bunching is created by individuals coming from above the *notch*. A key lesson from the bunching literature is that it allows to estimate the elasticity of revenue to tax rates.

Counterfactuals and mass. The identification of bunching and missing mass require comparing the empirical distribution with an estimated counterfactual as shown by Kleven and Waseem (2013). The latter is estimated by fitting a flexible polynomial to the empirical density in the following form:

$$c_j = \sum_{i=0}^{p} \beta_i (z_j)^i + \sum_{i=z_L}^{z_U} \gamma_i [z_j = i] + v_j \tag{3.2}$$

where c_j is the number of taxpayers and z_j the income level in a given bin j, p is the order of the polynomial, Z_L and Z_U are the lower and upper bounds of the *excluded range* area (this is generally the area affected by bunching responses). The counterfactual distribution is the prediction that results from the previous regression, with the only exception that those in the excluded area are fitted without including the second term of the regression (dummy variables for each bin in the excluded range). Excess bunching is estimated as the difference between the observed distribution of taxpayers and the counterfactual for all those located within the range between the lower bound (Z_L) and the *notch* (z^*). Likewise, the missing mass is estimated in the same way but focusing on the window between the *notch* (z^*) and the upper bound (Z_U).[13] Kleven and Waseem (2013) also define the share of unresponsive

[11] This holds only for *downward notches*, that is to say either in situations where there is an increase in the tax liability or a decrease in the transfer's amount.

[12] Technically, the dominated area is defined such that net earnings in z^* (with tax liability F_b) are equal to net earnings in z^d (with tax liability $F_b + \Delta F_b$).

[13] Formally, excess bunching is defined as $\sum_{j=z_L}^{z^*} (c_j - \hat{c})$ while missing mass as $\sum_{j>z^*}^{z_U} (\hat{c} - c_j)$.

individuals, those that suffer from some optimization frictions and thus, are unable to locate below the *notch*, as the share of individuals located in the dominated region over the number predicted by the counterfactual distribution.

The natural question one may impose to this methodology is how to determine the excluded range. We are agnostic on this and follow Kleven and Waseem (2013), and we then try to do several sensitivity analysis to check the robustness of our results. As they claim in their pioneer work, the lower bound is visually determined as the point where the excess bunching starts to be notorious. The upper bound is somehow more ambiguous to determine. They propose that this threshold should be determined such that the excess bunching (below the *notch*) equals the missing mass (above the *notch*).

Empirical evidence: sharp bunching

To test the predictions from the previous model, we start by simply plotting the empirical distribution of earnings pooling taxpayers in fiscal years 2014, 2015, and 2016 (Figure 3.3). For clarity, we split the figure into two panels: panel (a) presents the first four *notches* and panel (b) the following seven *notches*. Taxpayers are grouped in bins of $1,000 pesos, and red dotted vertical lines denote the *Monotributo* tax *notches*. There are various interesting facts to highlight from this figure. First, we observe sharp bunching in every *notch*, except the three highest located at the right tail of the distribution. Second, we do not observe, at least in this figure, a hole above each *notch* suggesting that some taxpayers' may be inattentive or unable to move below the cutoff. Third, there is also some bunching in other parts of the distribution (e.g., at 60k, 120k and 180k).

Therefore, in Figure 3.4 we present, once again, the empirical distribution of earnings for a non-rounder sample where we remove taxpayers whose reported taxable income is a multiple of $1,000$. We observe the same bunching as in the previous figure although somehow attenuated and, at the same time, a smoother distribution and no bunching at non-notches points. Hence, this suggests that the spurious bunching is mostly driven by rounding behavior of taxpayers when reporting their taxable income.

To compute bunching, missing mass, and counterfactuals, we zoom in on a specific discontinuity, the *notch* at 400k pesos (Figure 3.5). We first group and count taxpayers by mutually exclusive bins and plot this as a function of the value of the bin (blue line with crosses). The solid green line is the result of the predicted values of regression (3.2) with the caveat within the excluded range that we already mentioned. The red vertical line indicates the *notch* and the dashed black line indicates the income value that makes a taxpayer indifferent between this point and the *notch*. The area between these two lines represents the dominated range. The remaining dashed gray lines correspond to the lower and upper bound.

The figure confirms various interesting facts. First, there is a significant excess of bunching below the *notch* of around 2.6 times the height of the counterfactual distribution. This means that there is 2.6 times the density that should be expected. In the absence of the notch, the marginal "buncher" would have an income of 421,000 pesos, 5% higher than the

threshold. The bunching estimate of $b = 2.6$ implies that on average "bunchers" reduce taxable income by 2.6 bins or 2,600 pesos. Second, although there is some evidence of missing mass above the notch, a substantial amount of taxpayers suffer from some sort of friction or inattention (approximately 80%). Third, the start of the bunching behavior is somehow clear and identified by z_L. Fourth, although not perfect, the upper bound is such that the excess bunching and the missing mass are very similar.

For completeness, in Figure 3.6, we repeat the analysis for the first eight *notches*. The main message of this set of figures is roughly the same as above. There is significant bunching right below of each *notch* and little missing mass above. As a placebo test or alternative counterfactual, we can use the fact that the four notches located at 192K, 240K, 288K and 400K did not exist before 2014 to confirm that there is no bunching at those thresholds during the pre-2014 fiscal years (Figure 3.7).

Robustness. The previous conclusions remain unchanged when exploring additional robustness checks: (1) when we use either the full sample including multiples of $\$1,000$ (Figure C.2); (2) for a balanced panel of taxpayers present in 2014, 2015, and 2016 (Figure C.3); (3) when we break the bunching analysis by year (Figure C.4); (4) by grouping taxpayers into different bin sizes (see Figure C.5 for three alternative bin sizes).

Discussion, puzzles, and next steps. In the previous analysis, we documented substantial bunching right below the *Monotributo* thresholds. A natural question is why some notches exhibit more bunching than others. This result can be justified by the first stage reported in panel (a) of Figure 3.1. In this figure we can see that the average tax rate increases the most at 288k and 400k, which are precisely the two notches that display the sharpest bunching in Figure 3.4. Hence, reassuringly the differential bunching intensity is consistent with tax incentives.

Another puzzling result is given by the absence of bunching at the top three notches of the regime at 470k, 540k, and 600k (Figure C.6). One explanation that could rationalize this result is the type of activity that each category of *Monotributo* encompasses (i.e., a composition effect). While the first eight notches span both, services and the sale of goods, the last three notches only consider the sale of goods (see Table C.2). Therefore, by definition tax incentives in the last three notches are not binding for individuals in the service sector, a group that is typically more responsive to taxes and transfers (e.g., Saez (2010)). Hence, the absence of bunching in the top three notches could be explained by a higher composition of taxpayers selling goods, who presumably face more indivisibilities and frictions making bunching at the thresholds more difficult. Note also that taxpayers providing services and making more than 400k are excluded from the *Monotributo* regime and must register in the VAT and pay the income tax. This implies that the burden for taxpayers changes discontinuously when their revenue hits this eligibility threshold, providing further incentives to bunch at 400k.

Finally, the previous two observations lead to the question of whether the response to this simplified regime varies across sectors. To this end, we focus on the discontinuity at 400k where there is a mix of taxpayers across sectors. In Figure 3.8 we break the sample into services, wholesale and retail, agriculture and manufacturing. Interestingly, we find

that taxpayers in the services sector react the most ($b = 4.57$), followed by agriculture and manufacturing ($b = 1.53$), and finally wholesale and retail ($b = 0.97$).

There is some consensus in the field that any evidence of sharp bunching in earnings is likely due to tax evasion or tax avoidance rather than real responses (Kleven, 2016). We adhere to this view and interpret the behavioral responses from this section as suggestive evidence that a significant number of taxpayers attempt to avoid higher tax liabilities by keeping their reported revenue below the thresholds.[14] The next logical step of this section consists of translating the behavioral responses into elasticities. Since our data are remitted on a monthly basis, we can also analyze the timing of the bunching and the probability of filing after the due date. We leave these exercises for future work.[15]

3.4 Behavioral responses at the upper end

In this section we turn the attention to the gross receipts tax (GRT) whose schedule allows us to study the middle and upper part of the revenue distribution.

Conceptual framework

The GRT notched tax schedule can be written as:

$$T(z) = t * z + [\Delta t * z] * \mathbb{1}(z > z^*) \tag{3.3}$$

where t is the tax rate that should be paid when sales are below the threshold z^*, Δt corresponds to the increase in the tax rate above the notch and $\mathbb{1}(.)$ is a indicator function for being above the cutoff. Unlike *Monotributo*, the notch considered here takes the form of a discontinuity in a proportional tax rate, and thus the threshold represents a discontinuity in both the average and the marginal tax rate. This is shown in Figure 3.9, where the budget constraint not only shifts down at z^* but also becomes flatter. This type of notch is generally known as *proportional tax notch*. Nonetheless, incentives operate in the same way as in *Monotributo*, since firms have incentives to bunch right below the discontinuities to avoid a discrete jump in the tax burden.

It is also worth noting that the bunching approach are related to the regression discontinuity (RD) design. The latter essentially exploits notched incentives, but in situations where the assignment variable that determines whether the firm is above or below the relevant threshold is not subject to manipulation. Given the large size of firms around these

[14] Unfortunately, in this article we cannot study the benefit side (and perhaps the main purpose) of this simplified regime, i.e. whether it effectively serves as a formalization instrument for small taxpayers. This is an extensive-margin question that cannot be addressed with administrative data because one needs to observe both registered and non-registered individuals.

[15] Figure C.7 presents some preliminary figures for the notch at 400K. In these figures we look at: (a) the evolution of total earnings for those just above and just below the notch across the different months of the year, (b) date of latest declaration (c) number of declarations within a year.

notches, one could expect frictions and indivisibilities to operate more strongly and, thus, bunching to be weaker or nonexistent (i.e., gross revenue is harder to manipulate). In such cases, one could exploit an RD design to measure the revenue elasticity to the GRT.

Empirical evidence: little bunching

Recall from panels (b)-(d) of Figure 3.1 that the tax scheme of the general regime varies by three broad groups: retail and wholesale, services, agriculture and manufacturing. Hence, in this subsection we analyze each of these groups separately. We start the empirical analysis by plotting the revenue distribution as we did in the previous section for the simplified regime. We do so by pooling data from years 2013 to 2015, a period in which the tax rates and revenue thresholds remained unchanged (see Table C.4). Since the determination of tax rates is based on annual income from year t-1, the densities consider revenue from years 2012-2014.[16]

Figure 3.10 presents the results for the two notches in retail and wholesale. In the first notch at 1 million pesos, there is no visible bunching. This is consistent with the absence of bunching in the top 3 notches of *Monotributo*, a result that perhaps can be explained by the relatively small first-stage change in average tax rates at these thresholds. In the second notch at 40 million pesos, however, there is a spike to the left of the threshold and a hole to the right. Although bunching is not as striking as in the first 8 notches of *Monotributo*, this is suggestive evidence that some large firms are able to manipulate their gross revenue to avoid a higher tax burden in the following year. Unlike the first notch, note that at 40m pesos the average tax rate goes from 3.5% to 5%. This stronger first stage applied to a broad gross revenue tax base can indeed be translated into very high rates on profits. For example, assuming a profit margin of 10% (which is in the ballpark of empirical estimates), a turnover tax of 5% corresponds to a 50% tax on profits (i.e., the tax on profits is increasing by 15 percentage points).[17]

Figure 3.11 presents the results for the two notches in the service-based sector. Note that in this sector revenue is closer to profits than the other two sectors because it is less intensive in intermediate inputs and, thus, the first stage tax variation and bunching incentives operate less strongly. In the first notch at 500k pesos, there is a spike to the left of the threshold, albeit not as sharp as in the 400k notch of *Monotributo* which is very close to it. Recall that self-employed workers providing services are excluded *de facto* from the simplified regime if their annual income is higher than 400k pesos and must register in the VAT. Hence,

[16] Figure C.8 presents a diagram with an example of the standard timing of fiscal year, tax code approval, and deadline to file.

[17] Profit margin is defined as reported profits over revenue. In practice, margins vary greatly by sector and industry. For example, average profit margin ranges from 5% to 15% in Costa Rica (Bachas and Soto, 2018). Similarly, Almunia and Lopez-Rodriguez (2018) report stable profit margins between 5% and 6% for Spanish firms. An example of low-margin product-based sectors are grocery stores and supermarkets with net profit margins ranging from 1% to 3%. An example of a high-margin sector is pharmaceutical with net profit margins of about 18%. In service-based sectors, bookkeeping and payroll services firms can reach margins of 20%.

when we analyze the notch at 500k we are mostly dealing with registered firms instead of independent workers which could have less flexibility to manipulate reported sales, or they could compensate the additional tax burden by adjusting other margins (e.g., inflating costs to pay less income tax). In the second notch at 30 million pesos, there is no visible bunching.

Finally, in Figure 3.12, we present the results for the two notches in manufacturing. Note that this is the sector that faces the lowest tax rates. In fact, from Table C.4 on can see that before 2008 this sector was fully exempted from the turnover tax to alleviate inefficiencies from the cascading effect. In this case, the two notches operate up in the revenue distribution and therefore we plot them together. From the figure we can see that there is some manipulation of annual sales right below the taxable thresholds. Again, although we do not observe sharp bunching, this result is striking as this figure includes "super firms" in the top 0.1% of the sales distribution.

Administrative notch. Recall from the institutional setting, that the GRT also has a regulatory provision that obliges firms with annual sales above a threshold to work as *collection agents* for the tax authority. Since this regulation increases significantly the accounting costs of companies, they have incentives to bunch strictly below the threshold. Between 2010 and 2016, the threshold that determines whether a firm must act as a collection agent remained fixed at 10 million pesos.

In Figure 3.13 we plot the distribution of firms around 10 million pesos. From the figure we can see a clear mass to the left and a missing mass to the right of the threshold.[18] Note that in this case, the degree of bunching, albeit small, is stronger than in the tax notches. This result strikes us as remarkable given that these are very large firms and it is more difficult to manipulate broad tax bases such as income from sales. Hence, it is very likely that this is capturing underreporting behavior rather than real production effects. We interpret this result as suggestive evidence that firms find more costly the indirect administrative cost of working as a collection agent than the direct fiscal cost of the GRT tax. When we split the analysis by sector we find that the manipulation is mainly driven by service and manufacturing firms (Figure 3.14).

First stage and compliance. Unlike *Monotributo*, note that for the GRT we can also explore the empirical first stage because the data we use is generated from turnover tax returns. The goal here is to confirm whether tax rates jump (and to what extent) around the different cutoffs for the different sectors. Note also that, if the distribution of revenue is smooth at the discontinuity, in a second stage, one could test whether the tax variation from the first stage affects the production/sales of firms in the following year.

In Figures 3.15 through 3.17, we plot the fraction of firms paying the statutory tax rate mandated to the right of the threshold. The vertical line indicates the *notch* at which the statutory tax rate changes discretely. Under perfect compliance, the line should be flat at zero and jump to one at the threshold. The sample, bins, and x-axis are the same as in the density figures.

[18] Note that in the case of administrative *notches* it is less straightforward to determine the dominated area given the difficulty to assign a specific value, or cost, to administrative tasks.

The figures show that, although firms comply with the tax law, compliance is far from perfect making our research design fuzzy. Moreover, one can see that firms make two type of mistakes: to the left of the notch we have some firms paying a higher tax rate than mandated (overrate) and to the right of the notch we have firms paying a lower tax rate than mandated by law (underrate). The intensity of these behavioral mistakes also vary by the way the law is written for each sector.[19] Hence, we conclude that the way tax laws are written also matters for tax compliance. This is a novel result in public finance that has important implications for revenue collection and vertical equity considerations.

Related to this point, we also show that non-compliance falls over time. In Figure 3.18, we split the previous analysis by year for the first notch in services, retail and wholesale. The figure shows that while "overrate" mistakes to the left are fairly constant over time, "underrate" mistakes to the right are reduced across years. This is possibly explained by asymmetric enforcement actions of the tax authority.

Discussion. In this section we documented behavioral responses for medium and large firms to the notched schedule of the turnover tax. Although less sharp that in *Monotributo*, the importance of this group of firms in terms of GDP and tax revenue raises concerns about the efficiency (direct) cost of taxes on gross sales. The differential response to the notches from the two taxes we study could be explained by firm-size characteristics, such as the number of employees or the level of fixed assets, that might prevent large firms from engaging in evasion actions such as the underreporting of their gross revenue. Kleven, Kreiner, and Saez (2016) make this point theoretically, arguing that larger and more complex firms are less likely to reach a colluding agreement to evade taxes, as there is a higher chance that one of the employees may act as a "whistleblower".

Note also that the low degree of bunching by large firms does not necessarily imply that the efficiency costs is low and firms are absorbing the additional burden of the tax. In fact, lowering revenue is only one of the possible responses to a higher tax rate. For instance, we cannot rule out responses in other margins or even fiscal externalities to other tax bases. For example, this could lead firms to inflate costs in order to pay a lower profit tax or value added tax.

We leave for future work the computation of revenue elasticities. These elasticities ought to be small when scaled by the first-stage tax change because of the broad tax base and because thresholds are way up in the distribution. From a technical view, this indeed makes sense as notches generate sizable changes in implicit marginal tax rates and even large bunching is consistent with moderate elasticities (Bachas and Soto, 2018). Nonetheless, on a small profit base a modest change in revenue can generate a large profit elasticity.[20]

[19] In section 3.2, where we described the GRT, we listed the tax rates for the different sectors following the exact order as they appear written in the official tax code.

[20] Another important feature of the turnover tax that was not studied in this paper is the differential and high tax rates imposed on "foreign jurisdiction" firms, which works as an internal custom. In future work we would like to analyze cross-border effects of the turnover tax on "foreign jurisdiction" firms.

3.5 Conclusions

In this paper we analyzed the response of self-employed workers and firms to two different revenue taxes, *Monotributo* and the Gross Receipts Tax. We used administrative tax return data from Argentina and quasi-experimental variation provided by several *notches* in the tax schedules.

In the case of small taxpayers, our findings show substantial bunching right below the thresholds of the first 8 categories in the *Monotributo* regime. The intensity of this clustering is stronger for higher discrete jumps in average tax rates. In the remaining 3 notches, however, bunching is non existent, which can be justified by a higher composition of taxpayers selling goods instead of services (i.e., indivisibilities). We also document that taxpayers in services react the most, followed by agriculture and manufacturing, and finally wholesale and retail. We interpret these behavioral responses as suggestive evidence that a significant number of taxpayers attempt to avoid higher tax liabilities by keeping their reported revenue below the thresholds.

In the case of medium and large taxpayers, although bunching to turnover tax notches is not as striking as in the first 8 discontinuities of *Monotributo*, it suggests that some very large firms are able to manipulate their gross sales to avoid facing higher tax rates. We also find that bunching is stronger for the administrative notch that obliges firms to work as collection agents. This could imply that firms find more costly this indirect administrative cost than the direct fiscal cost of the turnover tax. When we split the analysis by sector we find that the manipulation is mainly driven by service and manufacturing firms.

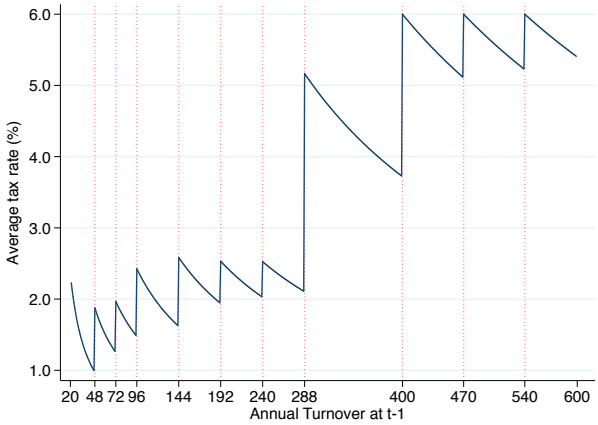
Overall, these results suggest that taxes imposed on broader tax bases are prone to avoidance for small firms, but are harder to avoid for larger firms since they are in the public eye of tax authorities. Although less sharp that in *Monotributo*, the importance of large firms in terms of GDP and tax revenue raises concerns about the efficiency (direct) cost of taxes on gross sales. The differential response to the notches from the two taxes we study could be explained by firm-size characteristics, such as the number of employees or the level of fixed assets, that might prevent large firms from engaging in evasion actions, such as the underreporting of their gross sales.

Importantly, despite a relatively low response of medium and large firms, we cannot rule out that companies react on other margins. Furthermore, our identification strategy does not let us identify or measure the cascading effect of the turnover tax, which is presumably the most relevant distortion introduced by this tax. This type of question could be eventually addressed if researchers were given access to other administrative databases that allow to follow business-to-business transactions. One example of this type of data is the one generated from the withholding regime of collection agents or from the value added tax.

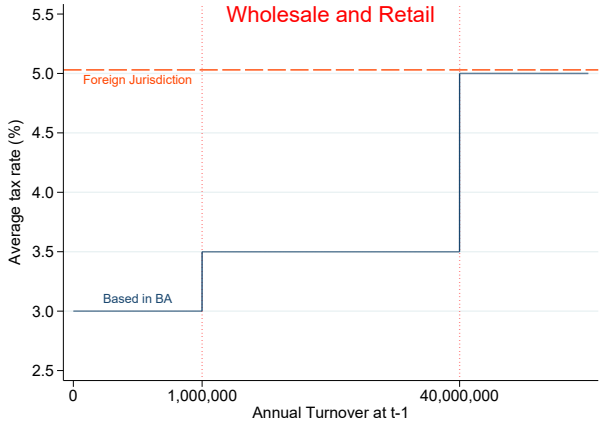
Figures and Tables

Figure 3.1: The tax schemes: *Monotributo* and *Turnover Tax*

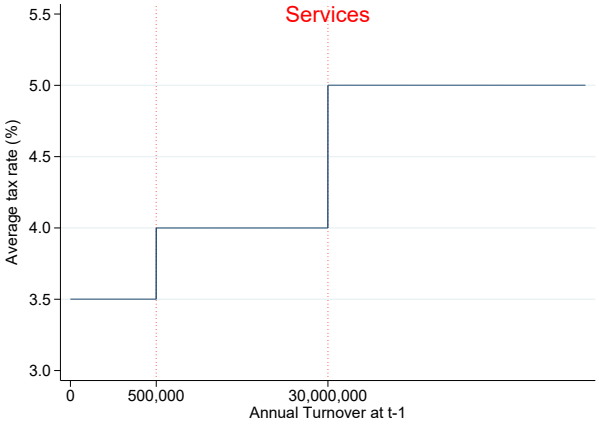
(a) *Monotributo*



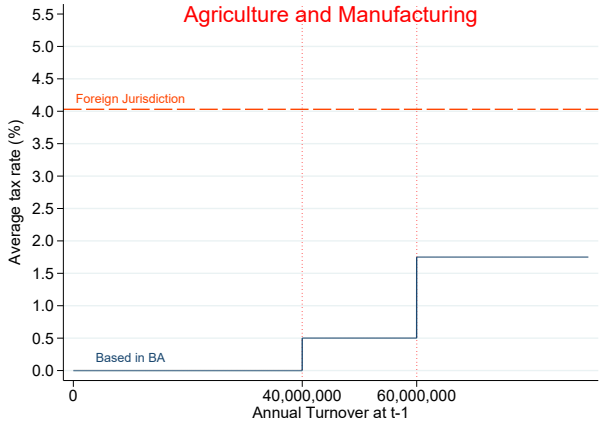
(b) IIBB (A)



(c) IIBB (B)

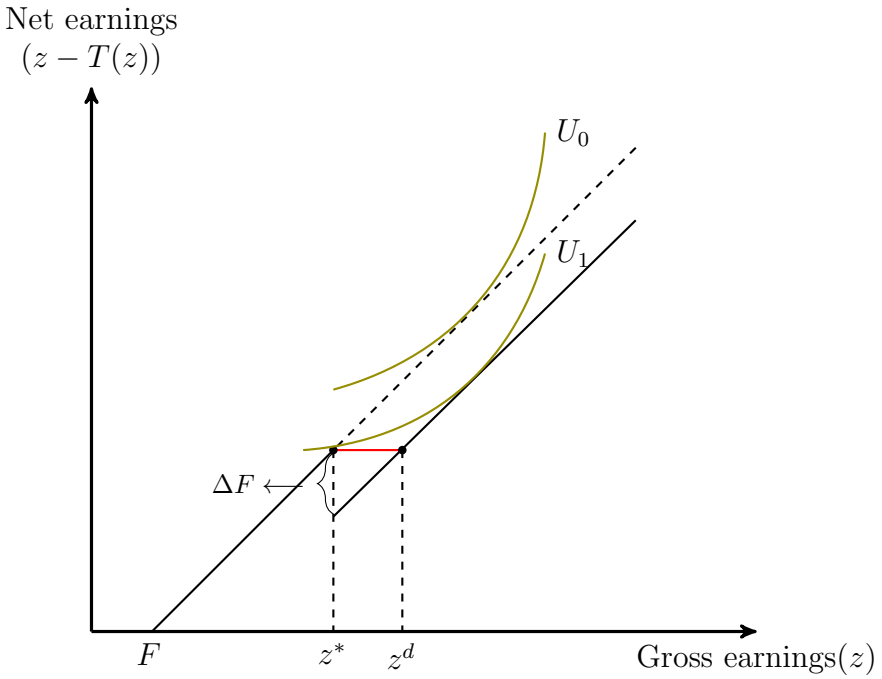


(d) IIBB (C)



Source: Own elaboration based on official documents and tax laws.
Notes: Panel (a) presents the average tax rate i.e., tax liability as a proportion of turnover, for *monotributo* tax schedule for retail and wholesale. Panels (b), (c) and (d) present the average tax rate for the GRT for different sectors for the period 2013-2015. These three panels are not drawn to scale. Each dotted red vertical line in the different panels, refers to a different tax *notch*. It is worth highlighting that while the horizontal axis in panel (a) is measured in thousands of pesos, in the other panels it is so in millions. Then, the discontinuities in each of these two taxes occurs at very different parts of the turnover distribution.

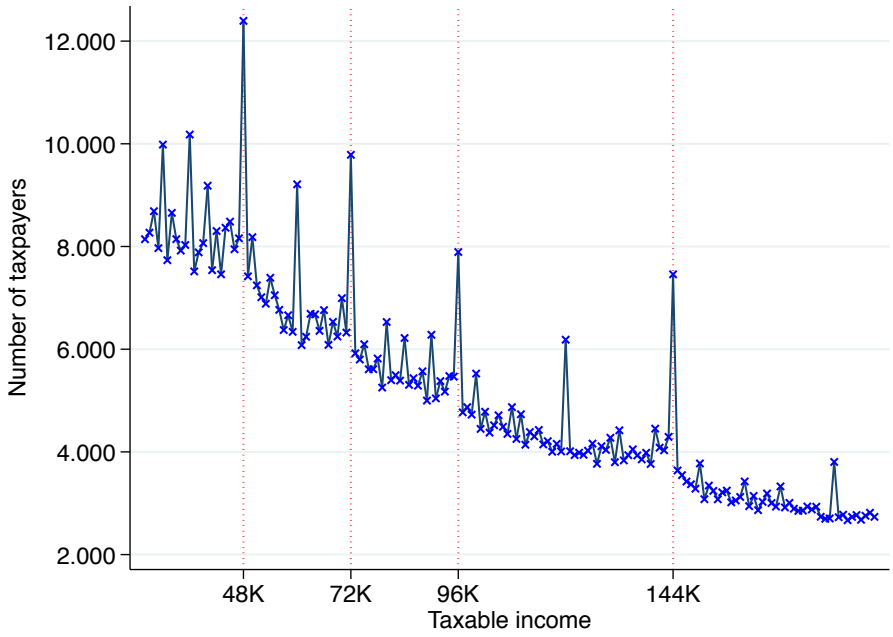
Figure 3.2: *Monotributo* scheme: $T_b(z) = F_b + [\Delta F_b] * \mathbb{1}(z > z_b^*)$



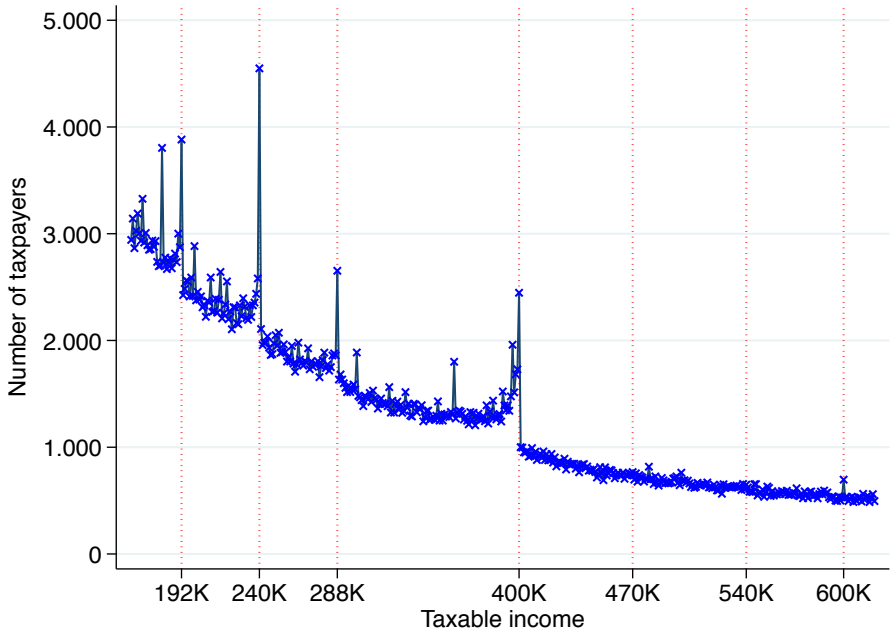
Notes: In this figure we present the standard framework to show the incentives that a rational agent should have to bunch. On the vertical axis we measure after tax income while on the horizontal before tax earnings. Note that the *notch* that is introduced in this example refers to *pure notch* i.e., there is a downward shift in the budget choice set, but the *slope* remains the same before and after the notch (choice sets are parallel). Before the introduction of the notch the *marginal buncher* enjoys utility U_0. After the tax change she will be in U_1, where she will be indifferent between enjoying consumption at z^* or at the point where the new utility curve is tangent to the new choice set. Taxpayers to the left of the *marginal buncher* will have incentives to bunch at the notch. Importantly, no rational taxpayers should be located in the segment given by the red line. This segment corresponds to the *dominated area*. At the *notch*, taxpayers enjoy more consumption and leisure than in the dominated area.

Figure 3.3: Full sample

(a) First four *notches*



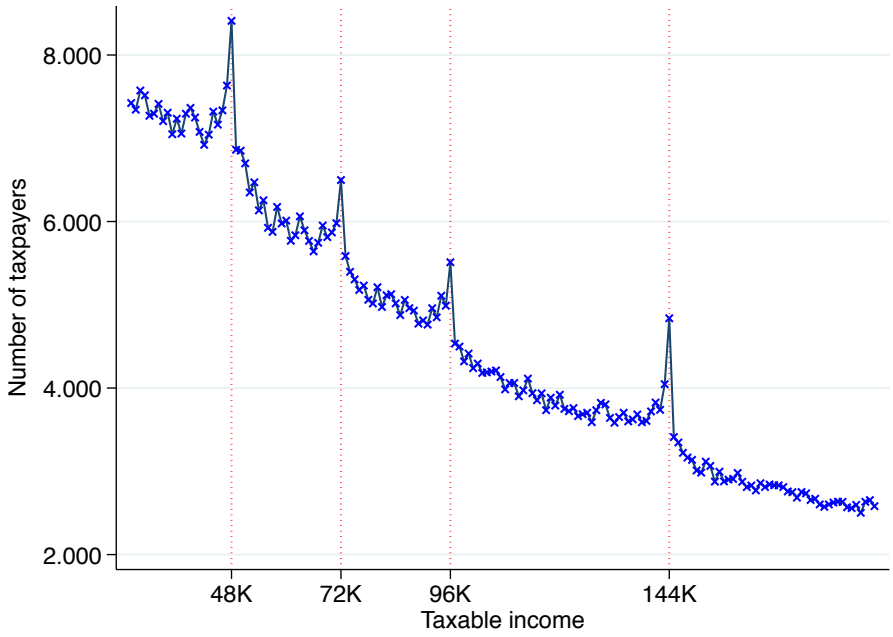
(b) Next seven *notches*



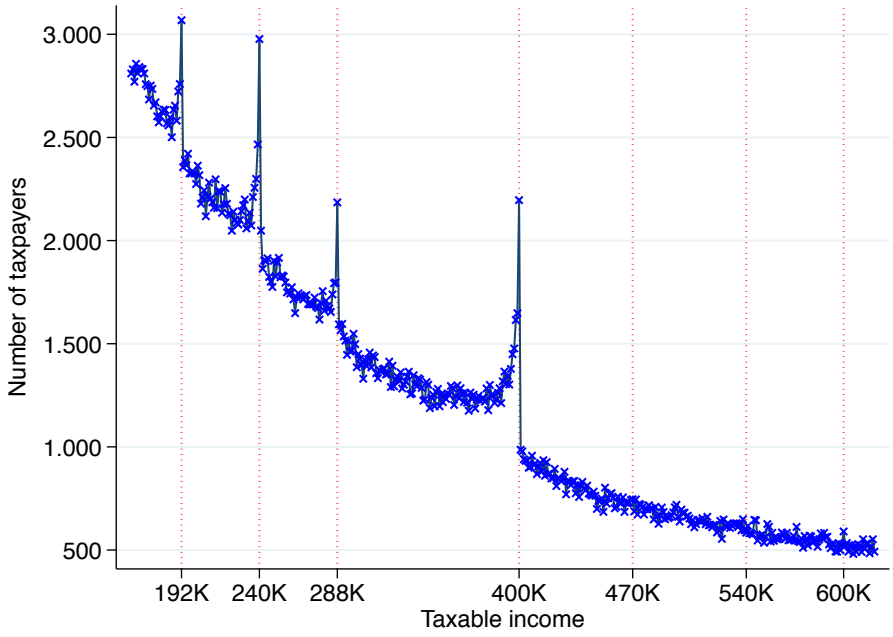
Source: Own elaboration.
Notes: Taxpayers of the years 2014, 2015 and 2016 are grouped in bins of 1,000$. Each dotted red vertical line refers to a different tax *notch*. On the horizontal axis we measure taxable income which is equal to annual turnover. Full sample.

Figure 3.4: Non-rounder sample

(a) First four *notches*

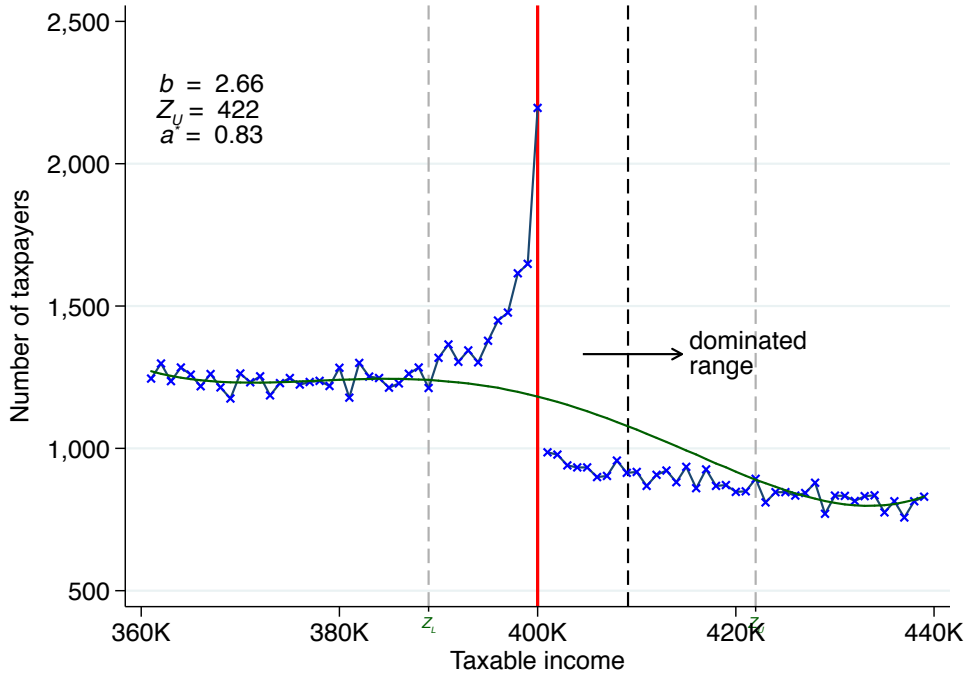


(b) Next seven *notches*



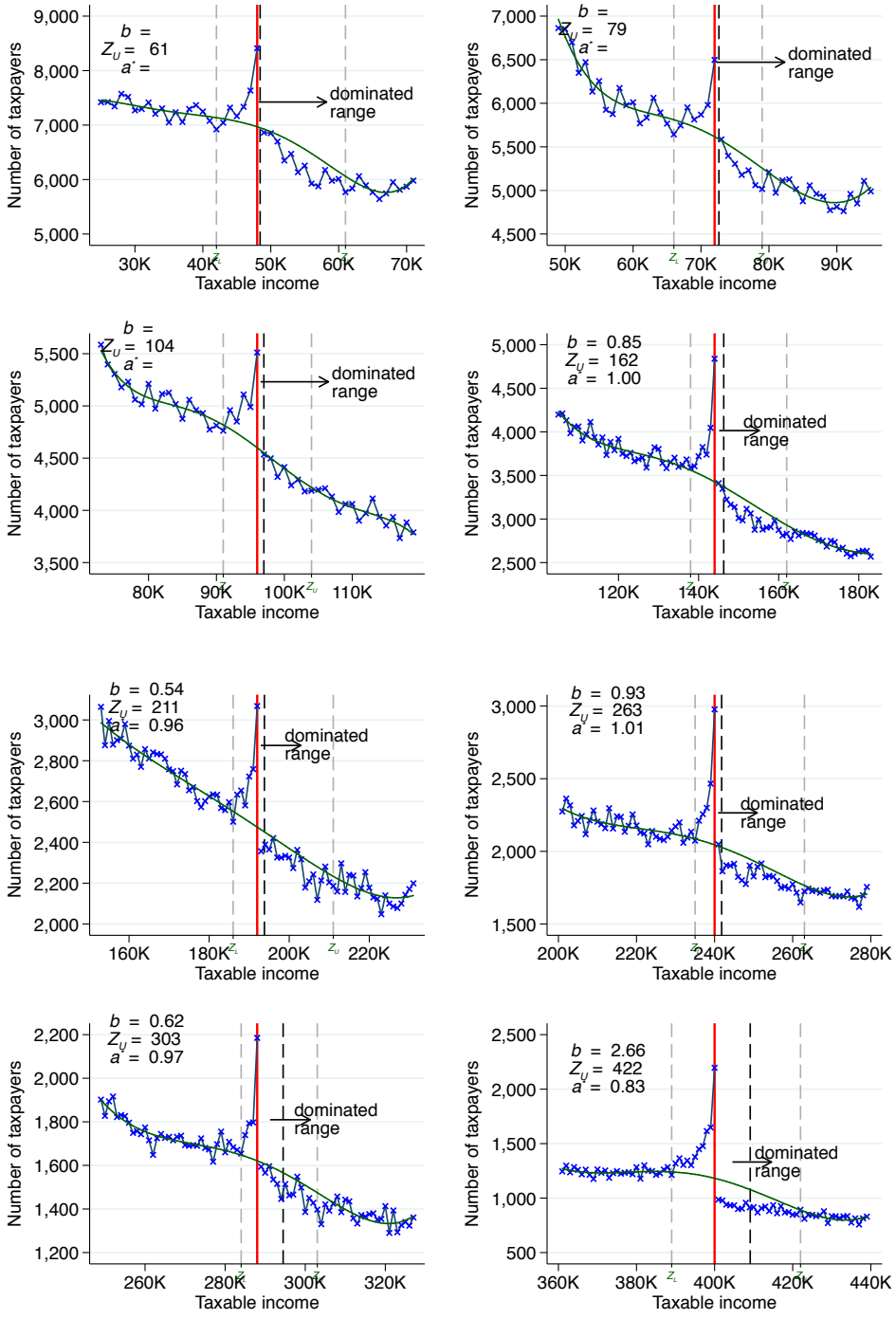
Source: Own elaboration.
Notes: Taxpayers of the years 2014, 2015 and 2016 are grouped in bins of 1,000$. Each dotted red vertical line refers to a different tax *notch*. On the horizontal axis we measure taxable income which is equal to annual turnover. We remove those taxpayers whose reported taxable income is a multiple of 1,000.

Figure 3.5: Bunching and missing mass at *monotributo notch* ($400K$)



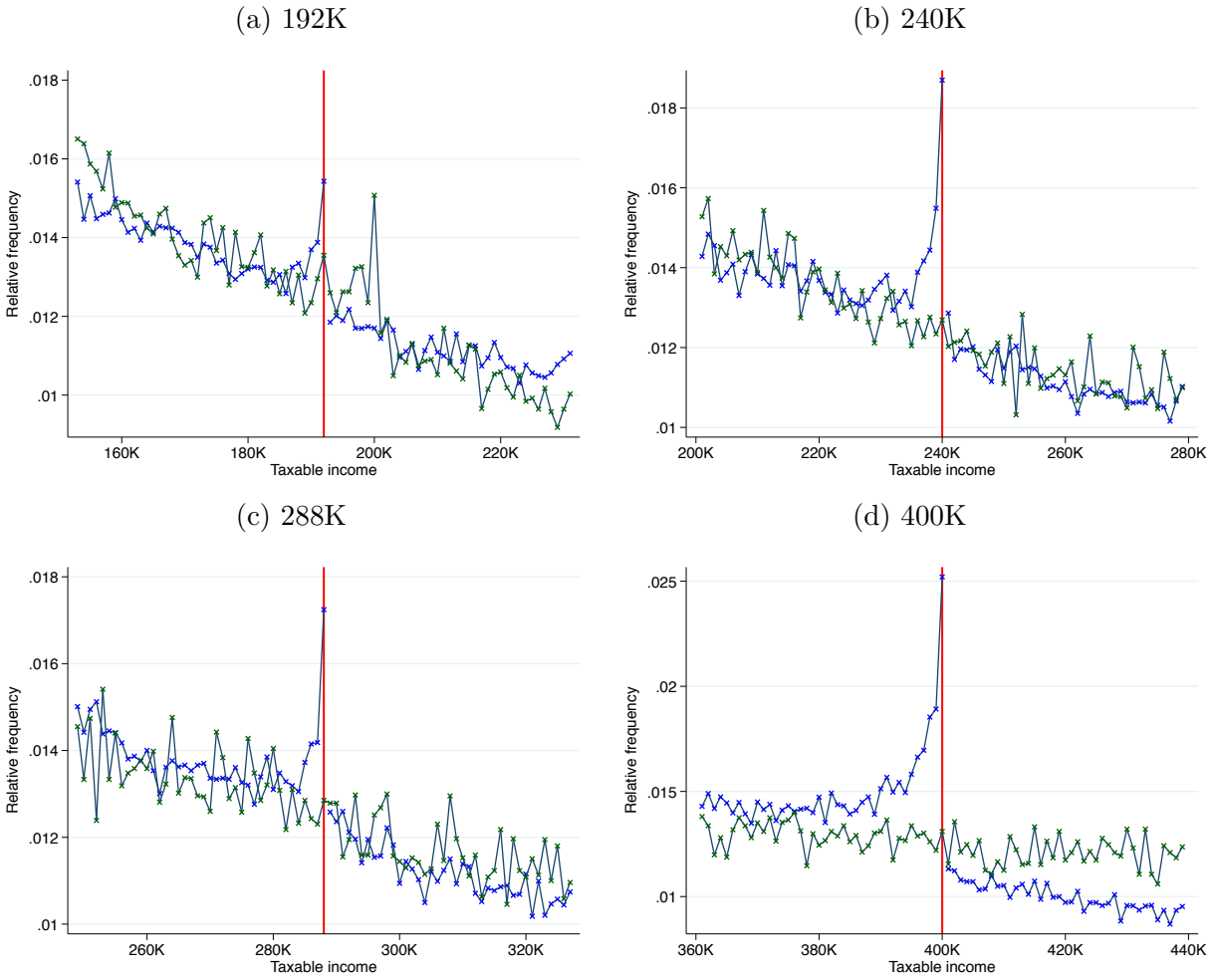
Notes: Taxpayers of the years 2014, 2015 and 2016 are grouped in bins of $1,000$\$. The solid red vertical line refers to the existing *notch* at $400K$. The green solid line is the prediction of regressing equation (1) with the caveat of the dummies in the excluded range. On the horizontal axis we measure taxable income which is equal to the annual turnover. We remove those taxpayers whose reported taxable income is a multiple of $1,000$.

Figure 3.6: Bunching at *monotributo notches*



Notes: Taxpayers of the years 2014, 2015 and 2016 are grouped in bins of 1,000 pesos. The solid red vertical lines refer to the different *notches*. The green solid line is the prediction of regressing equation (1) with the caveat of the dummies in the excluded range. On the horizontal axis we measure taxable income which is equal to the annual turnover. We remove those taxpayers whose reported taxable income is a multiple of 1,000.

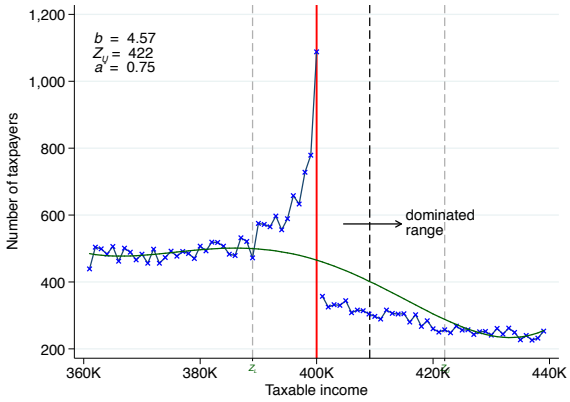
Figure 3.7: Bunching at *monotributo notches* and *control years*



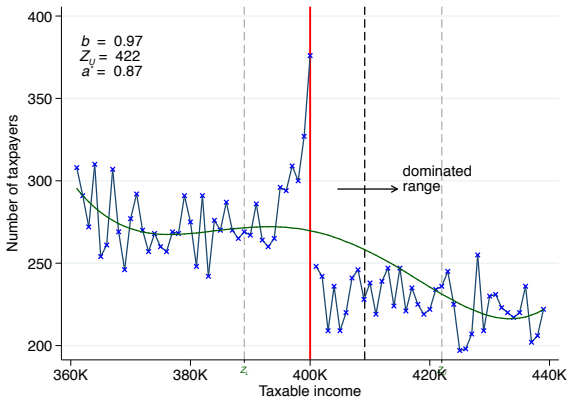
Notes: This figure shows that there is no bunching when we use pre-2014 data and the notches located at 192K, 240K, 288K, and 400K do not exist yet (green line). The blue lines correspond to taxpayers in fiscal years 2014 to 2016. The vertical axis measures relative frequency. The horizontal axis measures taxable income which is equal to the annual gross income. We remove taxpayers whose reported taxable income is a multiple of $1,000$.

Figure 3.8: Bunching at 400K by sector

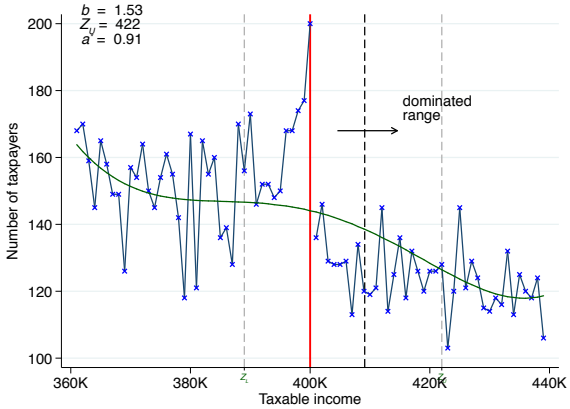
(a) **Services**



(b) **Wholesale and Retail**

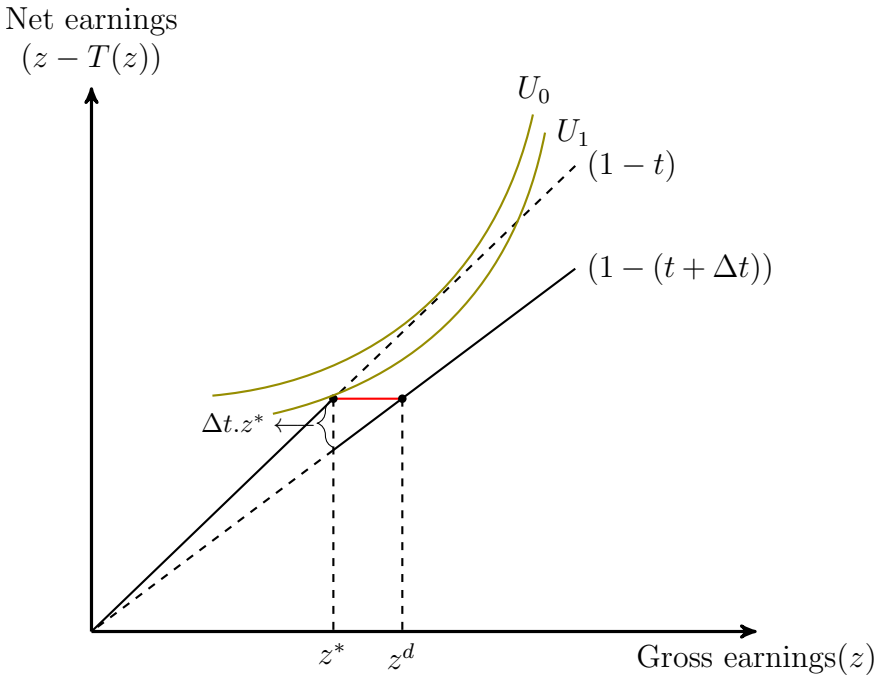


(c) **Agriculture and Manufacturing**



Notes: Taxpayers of the years 2014, 2015 and 2016 are grouped in bins of 1,000 pesos. This figure splits the sample into services, wholesale and retail, and agriculture and manufacturing. We remove taxpayers whose reported taxable income is a multiple of 1,000.

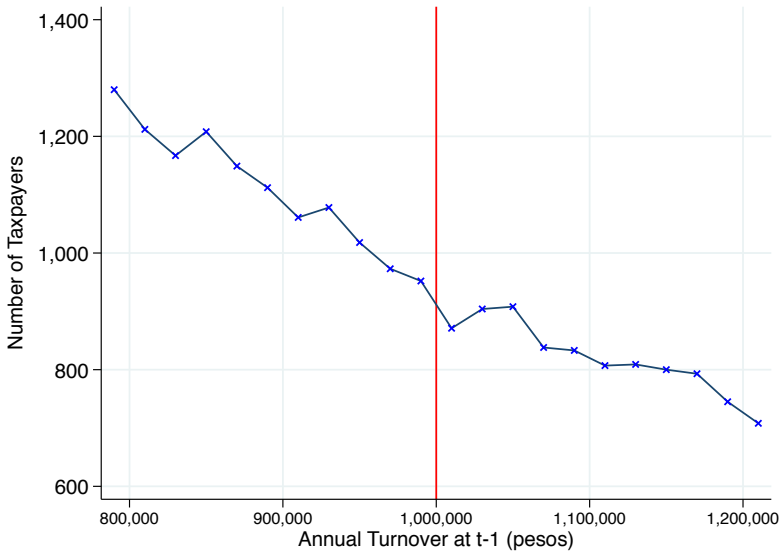
Figure 3.9: GRT: $T(z) = t * z + [\Delta t * z] * \mathbb{1}(z > z^*)$



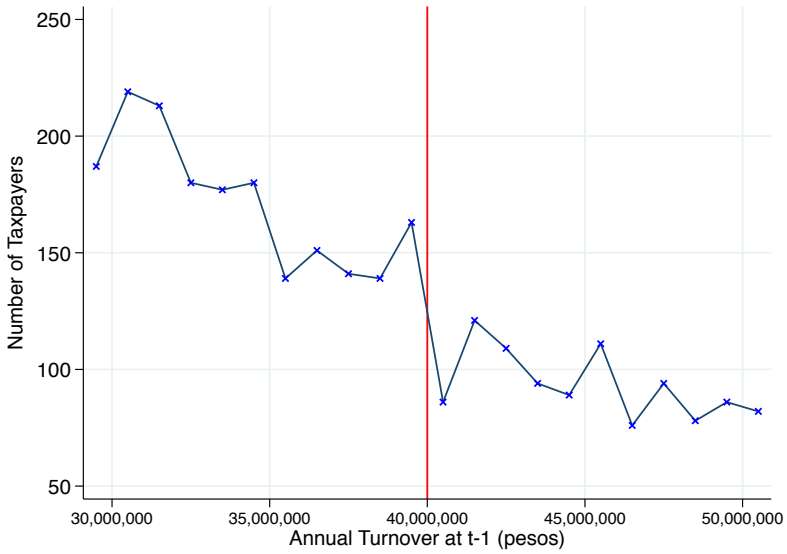
Notes: In this figure we present the standard framework to show the incentives that a rational agent should have to bunch. On the vertical axis we measure after tax income while on the horizontal before tax earnings. Note that the *notch* that is introduced in this example refers to *proportional notch* i.e., there is a downward shift in the budget choice set but also a change in the *slope*; therefore a change in the marginal tax rate either (marginal tax rate was t and then is $t + \Delta t$). Before the introduction of the notch the *marginal buncher* enjoys utility U_0; after the tax change she will be in U_1. Importantly, no rational taxpayers should be located in the segment given by the red line. This segment corresponds to the *dominated area*. At the *notch*, taxpayers enjoy more consumption and leisure than in the dominated area.

Figure 3.10: Bunching at *GRT notches*: Retail and wholesale

(a) **Notch 1:** tax rate goes from 3% to 3.5%



(b) **Notch 2:** tax rate goes from 3.5% to 5%

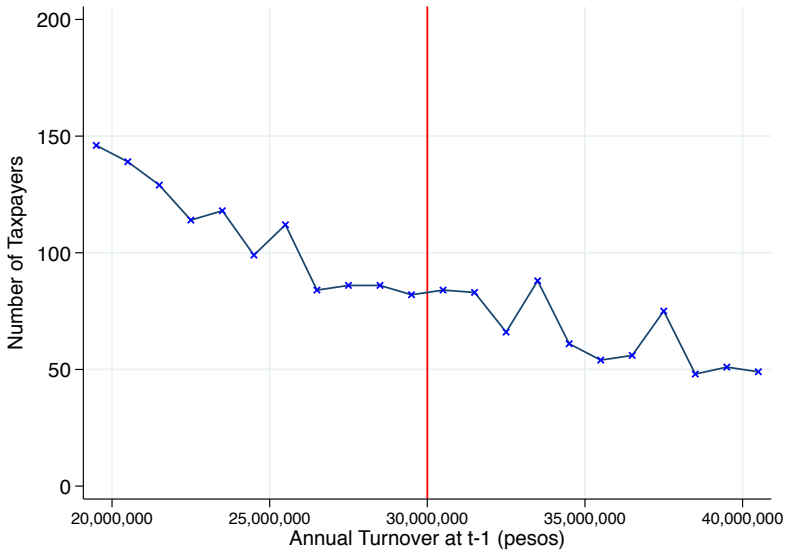


Notes: This graph shows the distribution of reported annual revenue from year $t-1$ using pooled data for the years 2012-2014. The vertical line indicates the *notch* at which the average tax rate changes discretely. The bins are 20k pesos wide in panel (a) (N=21226) and 1m pesos in panel (b) (N=2915), delimited such that no bin contains data both to the left and to the right of the threshold. These figures do not consider *foreign jurisdiction* firms. For those multi-sector firms, we identify their main sector.

Figure 3.11: Bunching at *GRT notches*: Services

(a) **Notch 1:** tax rate goes from 3.5% to 4%

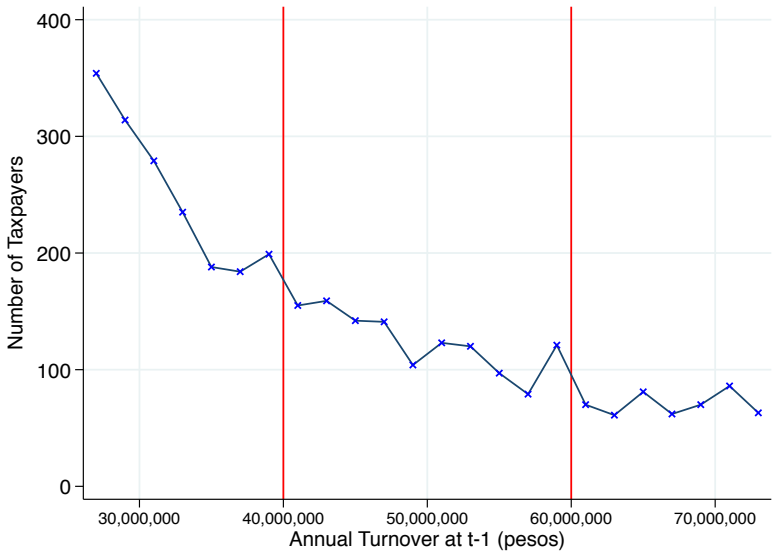
(b) **Notch 2:** tax rate goes from 4% to 5%



Notes: This graph shows the distribution of reported annual revenue from year $t-1$ using pooled data for the years 2012-2014. The vertical line indicates the *notch* at which the average tax rate changes discretely. The bins are 10k pesos wide in panel (a) (N=20104) and 1m pesos in panel (b) (N=1910), delimited such that no bin contains data both to the left and to the right of the threshold. These figures do not consider *foreign jurisdiction* firms. For those multi-sector firms, we identify their main sector.

Figure 3.12: Bunching at *GRT notches*: Manufacturing

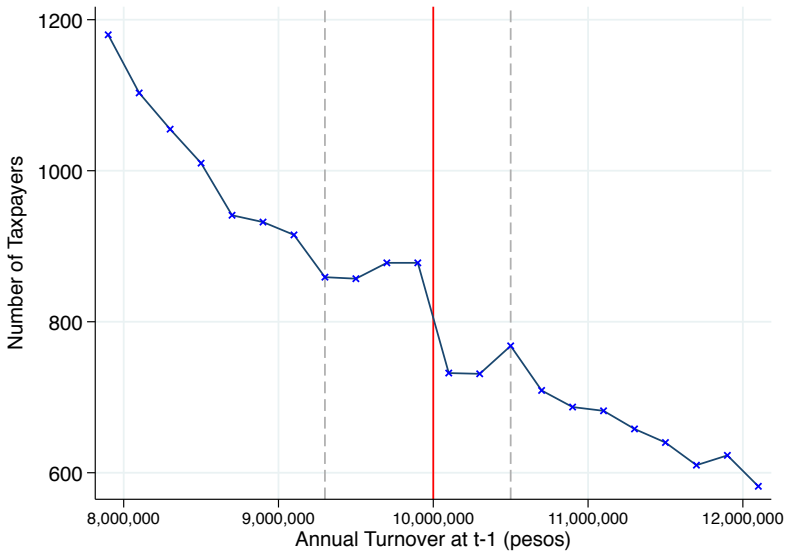
(a) **Notch 1 and 2:** tax rate goes from 0% to 0.5% and 0.5% to 1.75%



Notes: This graph shows the distribution of reported annual revenue from year $t-1$ using pooled data for the years 2012-2014. The vertical line indicates the *notch* at which the average tax rate changes discretely. The bins are 1m pesos wide in panel (a) (N=3487) and 1m pesos in panel (b) (N=18030), delimited such that no bin contains data both to the left and to the right of the thresholds. These figures do not consider *foreign jurisdiction* firms. For those multi-sector firms, we identify their main sector.

Figure 3.13: Bunching at *administrative notches*

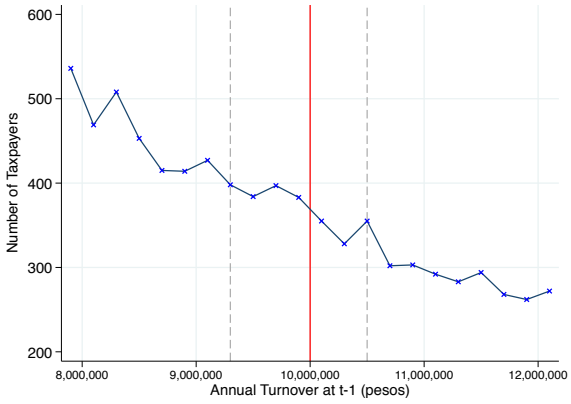
(a) **Administrative Notch:** firms have to work as collection agents



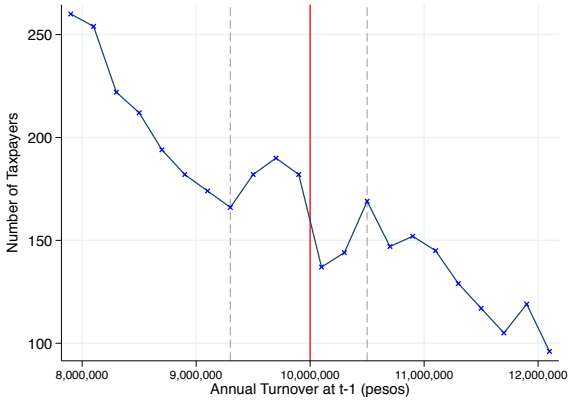
Notes: This graph shows the distribution of reported annual revenue from year $t-1$ using pooled data for the years 2012-2014. The red vertical line indicates the administrative *notch* at which firms become collection agents. The bins are 200k pesos wide (N=18030).

Figure 3.14: Bunching at *administrative notches* by sector

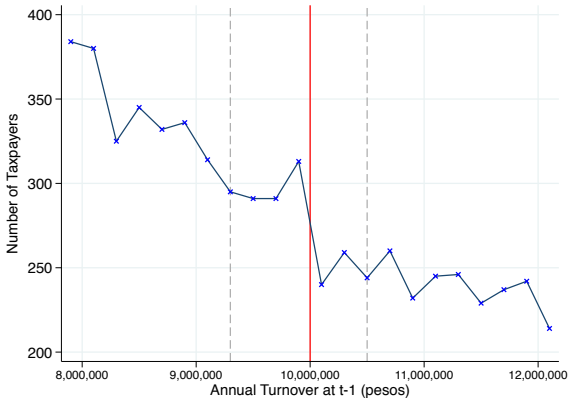
(a) **Retail and Wholesale**



(b) **Services**



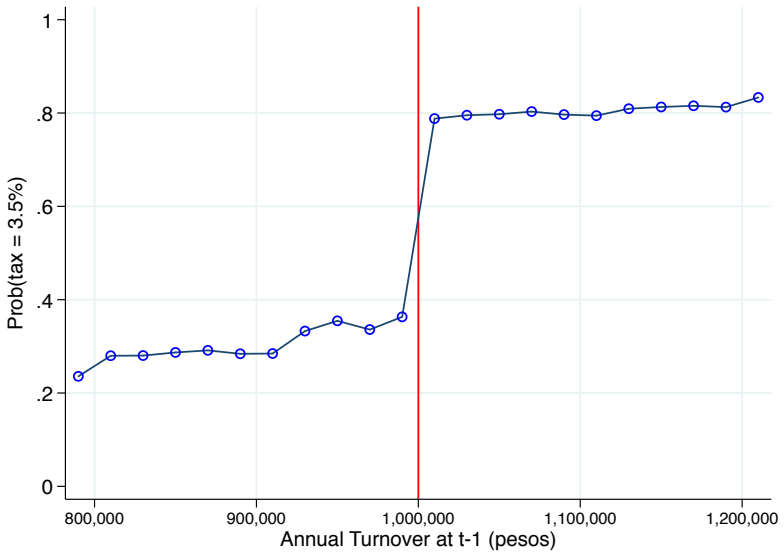
(c) **Manufacturing**



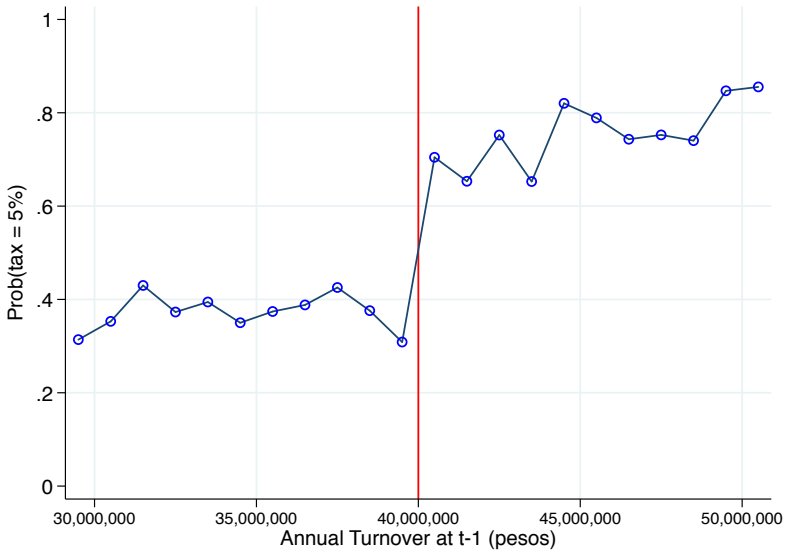
Notes: This graph separates the frequencies from Figure 3.13 into the main sector of the firm. The red vertical line indicates the administrative *notch* at which firms become collection agents. The bins are 200k pesos wide. N=6254 in panel (a), N=3678 in panel (b), and N=8098 in panel (c).

Figure 3.15: First stage tax variation and compliance: Retail and wholesale

(a) **Notch 1:** statutory tax rate goes from 3% to 3.5%



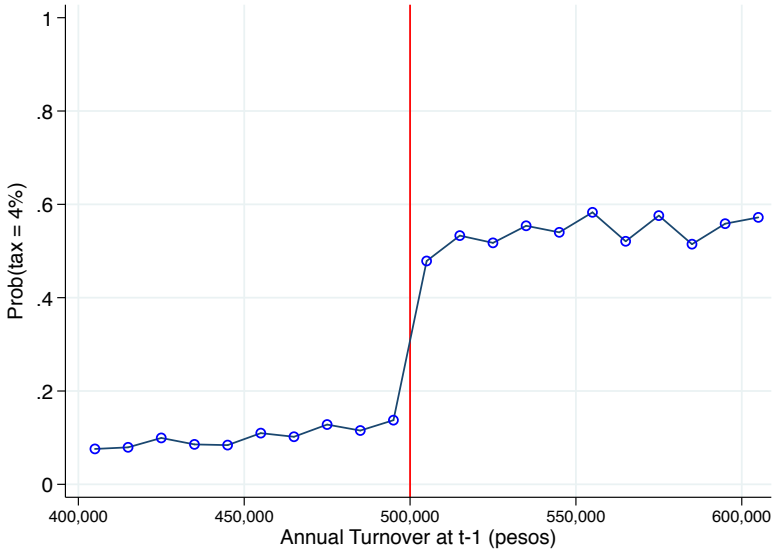
(b) **Notch 2:** statutory tax rate goes from 3.5% to 5%



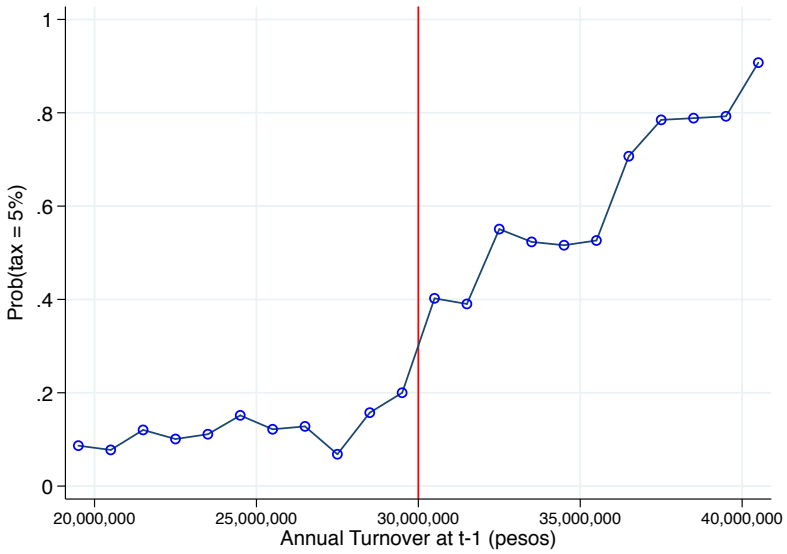
Notes: This graph shows the fraction of firms paying the tax rate that applies to the right of the threshold. The vertical line indicates the *notch* at which the statutory tax rate changes discretely. Under perfect compliance, the line should go from zero to one. The sample, bins, and x-axis are the same as in the density figures.

Figure 3.16: First stage tax variation and compliance: Services

(a) **Notch 1:** statutory tax rate goes from 3.5% to 4%



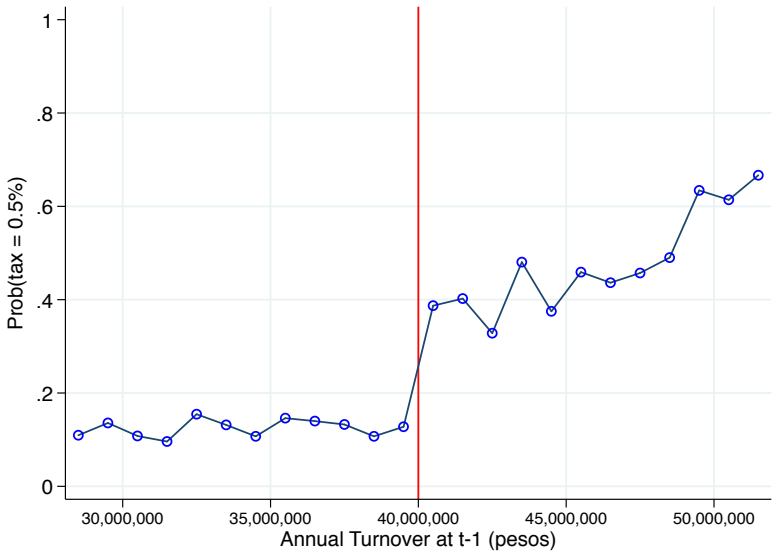
(b) **Notch 2:** statutory tax rate goes from 4% to 5%



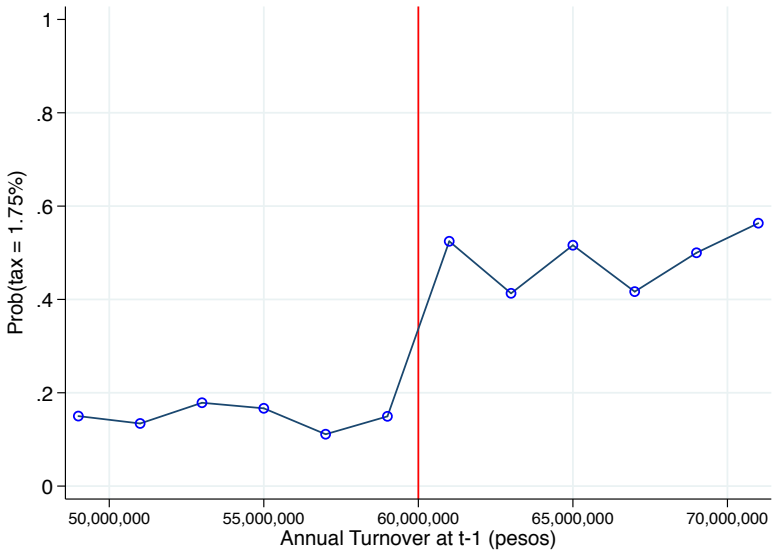
Notes: This graph shows the fraction of firms paying the tax rate that applies to the right of the threshold. The vertical line indicates the *notch* at which the statutory tax rate changes discretely. Under perfect compliance, the line should go from zero to one. The sample, bins, and x-axis are the same as in the density figures.

Figure 3.17: First stage tax variation and compliance: Manufacturing

(a) **Notch 1:** statutory tax rate goes from 0% to 0.5%



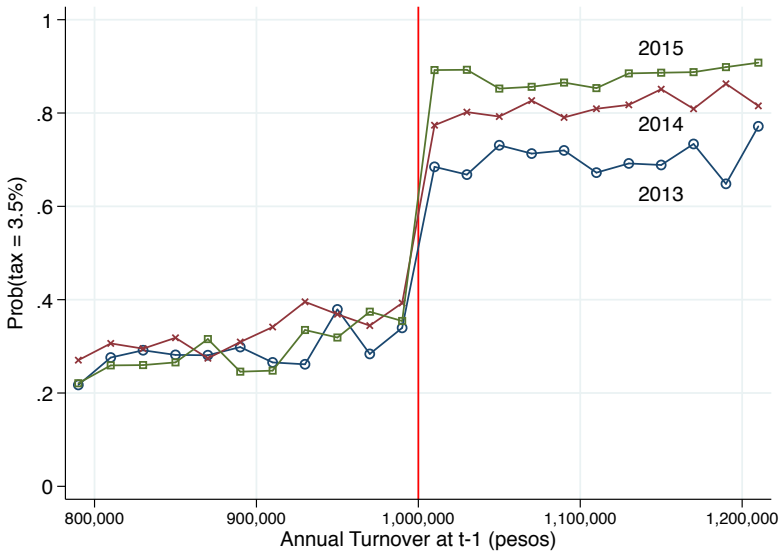
(b) **Notch 2:** statutory tax rate goes from 0.5% to 1.75%



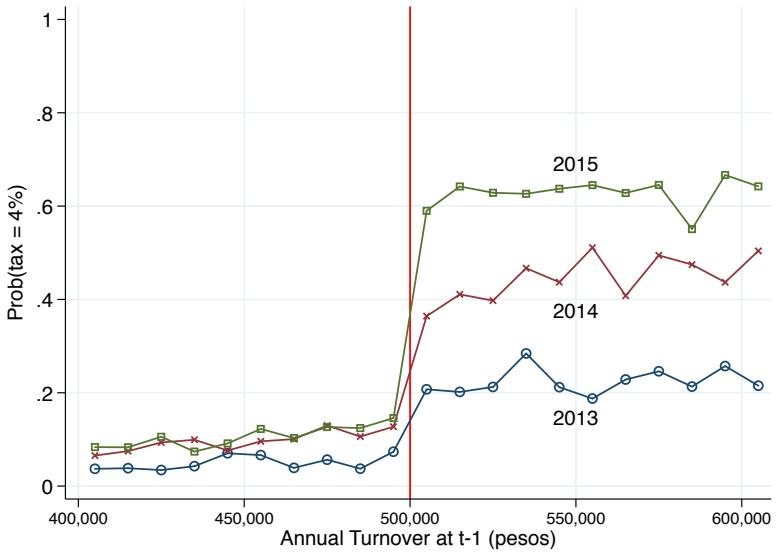
Notes: This graph shows the fraction of firms paying the tax rate that applies to the right of the threshold. The vertical line indicates the *notch* at which the statutory tax rate changes discretely. Under perfect compliance, the line should go from zero to one. The sample, bins, and x-axis are the same as in the density figures.

Figure 3.18: First stage tax variation and compliance, by years

(a) **Notch 1 - Retail and Wholesale:** statutory tax rate goes from 3% to 3.5%



(b) **Notch 2 - Services:** statutory tax rate goes from 3.5% to 4%



Notes: This graph shows the fraction of firms paying the tax rate that applies to the right of the threshold. The vertical line indicates the *notch* at which the statutory tax rate changes discretely. Under perfect compliance, the line should go from zero to one. The sample, bins, and x-axis are the same as in the density figures.

Dissertation Conclusion

This dissertation analyzed real and avoidance responses of wage earners, independent workers, and businesses to tax and transfer policies. It advanced the current knowledge on this topic by leveraging unprecedented variation in the Argentine tax-benefit system and large administrative datasets. This work also made important contributions on the mechanisms driving these responses (or the lack of them).

Taken together, this dissertation offers a complete picture of how workers and firms respond to the tax system and how they interact with each other in that process: it shows that wage earners barely adjust their hours of work to large and temporary tax changes, and also sheds light on the role of employers in those low responses (Chapter 1); in the case of tax credits it shows that when it is convenient for employers, they do extract rents from their employees (Chapter 2); and unlike wage earners, the dissertation shows that self-employed workers and businesses do adjust their income in response to taxes, but the evidence points to avoidance rather than real behavior (Chapter 3). Chapters 1 and 2 deserve special recognition because they provide one of the cleanest evidence to date on intertemporal labor responses of wage earners to income taxes and on wage effects of means-tested transfers.

As a concluding remark, I would like to acknowledge that it remains an open question on whether the evidence presented in this dissertation can be extrapolated to other settings and other countries. My goal is to keep advancing our knowledge towards this end. In particular, I would like to explore more rigorously the relationship between labor market rigidities and the responsiveness of workers to the tax system. I would also like to improve our understanding on the wage effects that might arise from other features of the tax system. For example, Italy introduced in 2014 a means-tested transfer similar to the EITC in the U.S. that is disbursed by employers. This is an economy-wide reform that offers a unique opportunity to study local and general equilibrium effects of means-tested tax credits.

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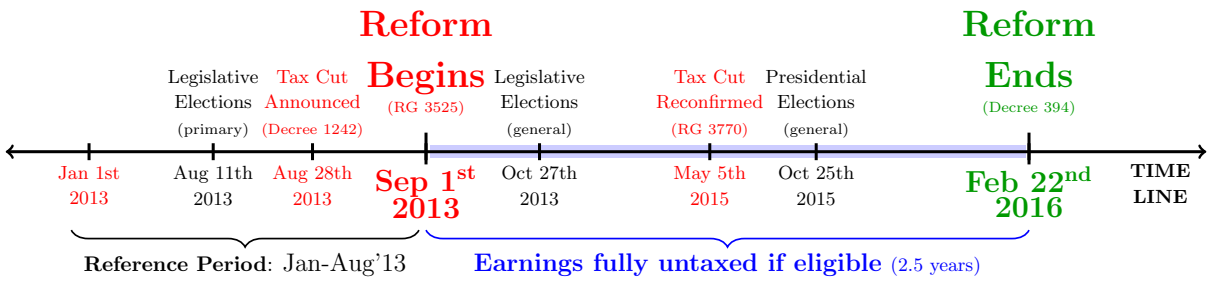
Appendix A

A.1 Appendix Figures and Tables from Chapter 1

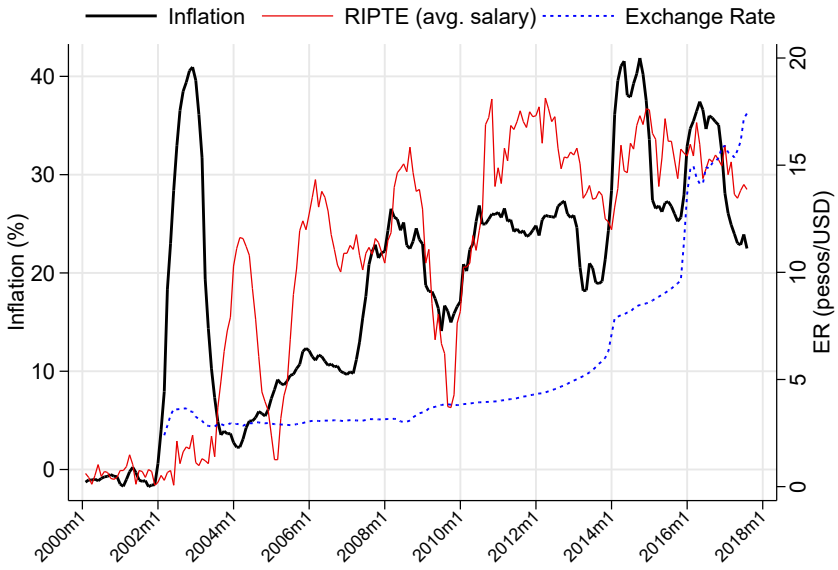
Figure A.1: Timeline of the tax holiday



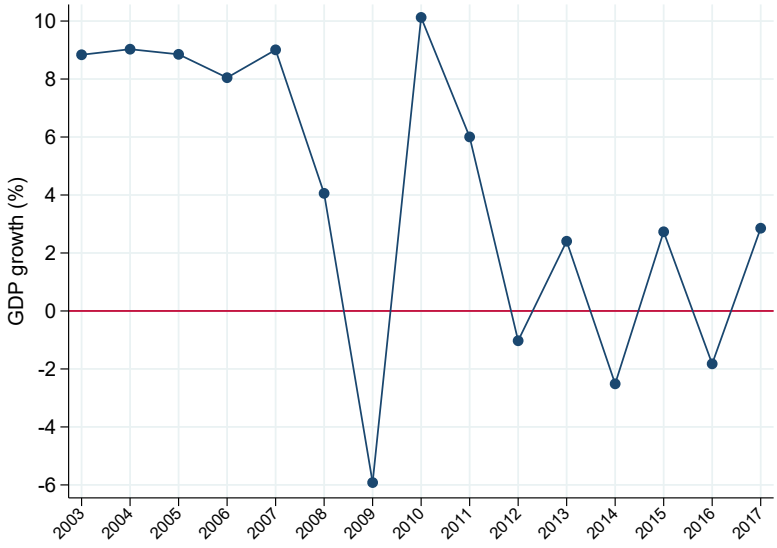
Notes: This figure displays the chronology of the events. The tax break was announced on August 28th, 2013, and entered into force on September 1st, 2013. On August 29th, the Argentine IRS issued a memo (RG 3525/2013) explaining in detail who was affected and how to compute the threshold. On May 5th 2015, the IRS reconfirmed the tax cut with another memo (RG 3770/2015). The policy was repealed on February 22nd, 2016 by the new administration that took office in December 2015. The beginning and end of the tax holiday were unanticipated and thus created income effects. The policy was perceived as a temporary fix to a deteriorated income tax schedule where inflation was high and tax parameters were not indexed. The tax cut was expected to be in place at least until the end of 2015 when Argentina held presidential elections.

Figure A.2: Stylized Facts in Argentina 2000-2016

(a) **Inflation and Exchange Rate**

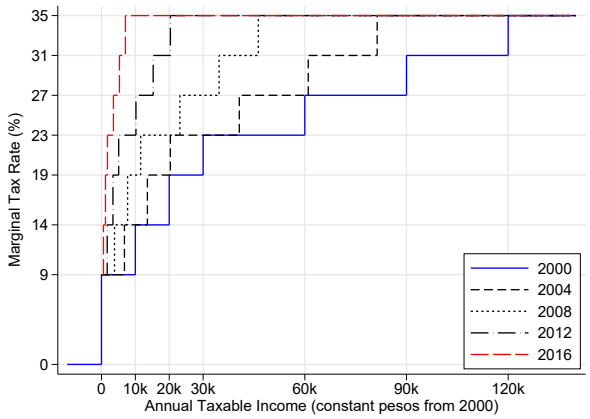


(b) **Business Cycle, 2003-2017**

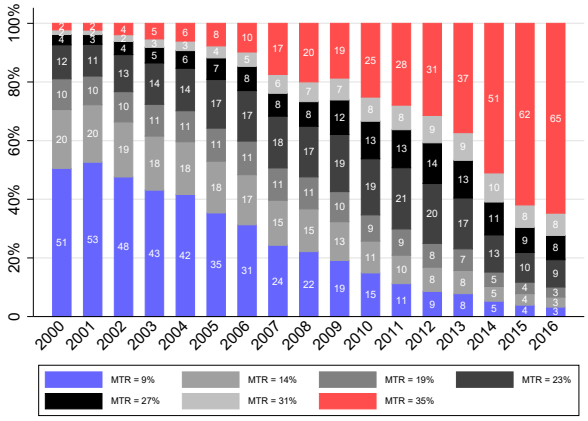


Notes: Panel (a) displays the annual inflation rate from The Billion Prices Project at MIT (Cavallo and Bertolotto, 2016), the average salary of registered workers (RIPTE, Remuneraciones Imponibles Promedio de los Trabajadores Estables) from the Minstry of Labor, and the exchange rate peso-dollar from the Central Bank. Panel (b) shows the GDP growth from WDI-World Bank as a proxy for the business cycle. It can be seen that, after some years of persistent growth excluding the U.S. recession, the economy was cooling down during the period of analysis 2011-2017 as GDP growth oscillates around zero.

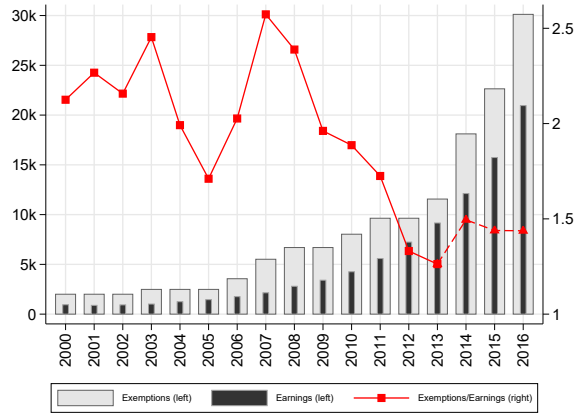
Figure A.3: Stylized Facts in Argentina 2000-2016



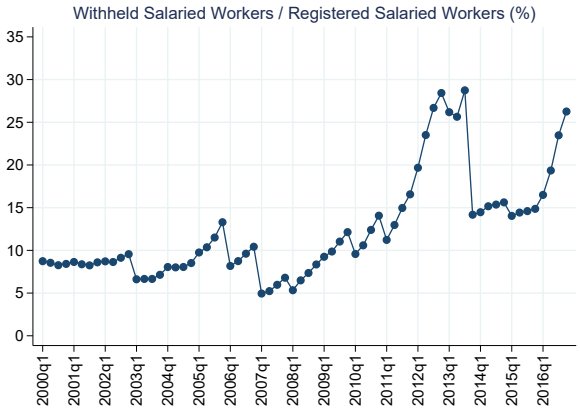
(a) **Annual Taxable Income and MTRs**



(b) **Bracket Creep**



(c) **Exemptions and Avg. Wage Earnings**



(d) **Wage earners subject to the income tax (%)**

Notes: Panel (a) presents the income tax schedule and illustrates how inflation reduced the significance of taxable thresholds. Panel (b) shows the fraction of taxpayers in each tax bracket and illustrates the "bracket creep" phenomenon: in the early 2000s the first bracket had the highest frequency and by 2016 the top bracket became the most popular. Panel (c) shows the evolution of personal exemptions for a married worker with two children (gray bar), average nominal earnings for registered workers (black bar), and the ratio between both variables (red line). Panel (d) reports the share of wage earners affected by the income tax. Source: these figures are from Tortarolo (2018).

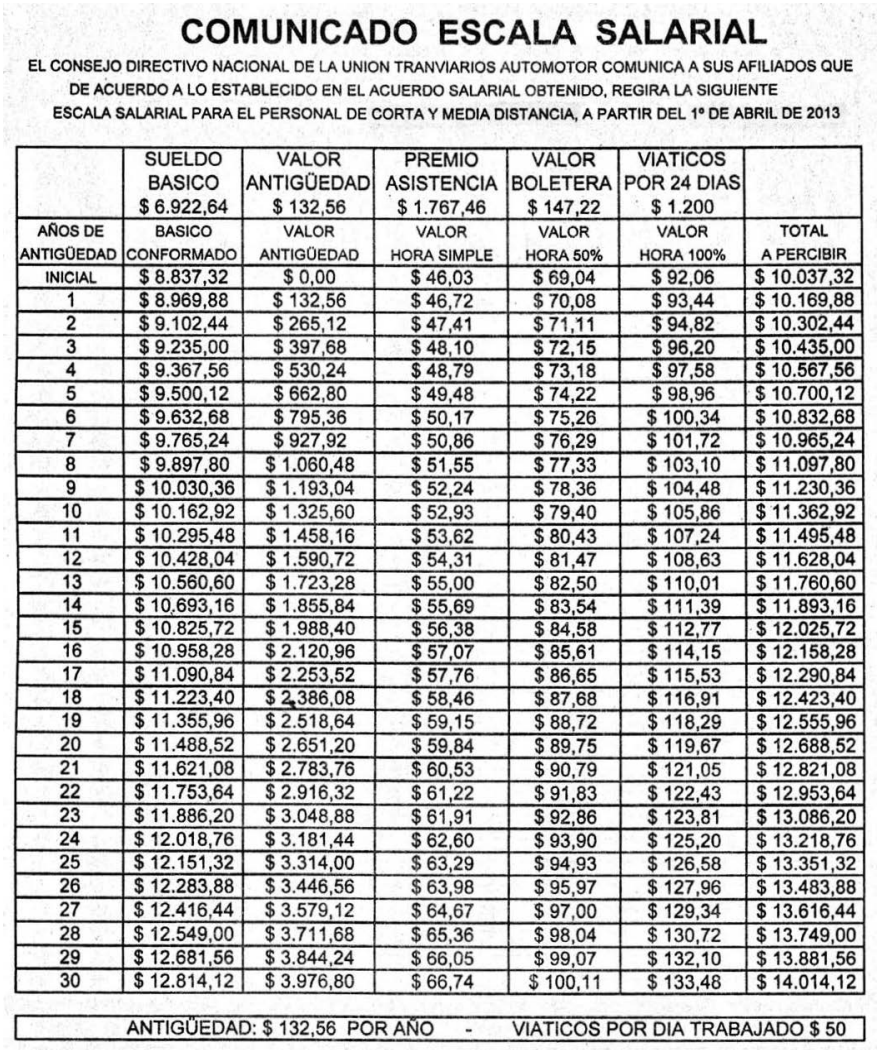
Figure A.4: Pay scale for wage earners in the banking sector

ANEXO (BASICOS 2015))

RAMA ADMINISTRATIVA			Conformado2015
CATEGORIA	Coef		
INICIAL	1	10.750,47	17.400,00
1 AÑO	1,04	10.750,47	17.400,00
2 AÑOS	1,08	10.750,47	17.400,00
3 AÑOS	1,11	10.848,19	17.400,00
4 AÑOS	1,13	11.043,67	17.425,53
5 AÑOS	1,17	11.434,59	17.457,48
6 AÑOS	1,2	11.727,79	17.489,43
7 AÑOS	1,24	12.118,71	17.521,38
8 AÑOS	1,26	12.314,18	17.553,33
9 AÑOS	1,3	12.705,10	17.585,28
10 AÑOS	1,35	13.193,78	17.617,23
11 AÑOS	1,39	13.584,70	17.649,18
12 AÑOS	1,43	13.975,62	17.681,13
13 AÑOS	1,48	14.464,26	17.713,08
14 AÑOS	1,52	14.855,21	17.745,03
15 AÑOS	1,63	15.930,25	17.832,84
20 AÑOS	1,74	17.005,29	
25 AÑOS	1,96	19.155,40	
30 AÑOS	2,07	20.230,43	
35 AÑOS	2,17	21.207,75	
2DO JEFE DE DIVISION DE 3RA	1,96	19.155,40	
2DO JEFE DE DIVISION DE 2DA	2,07	20.230,43	
2DO JEFE DE DIVISION DE 1RA	2,17	21.207,75	
JEFE DE DIVISION DE 3RA	2,28	22.282,79	
JEFE DE DIVISION DE 2DA	2,5	24.432,89	
JEFE DE DIVISION DE 1RA	2,61	25.507,93	
2DO JEFE DE DEPARTAMENTO DE 3RA	2,83	27.658,04	
2DO JEFE DE DEPARTAMENTO DE 2DA	2,93	28.635,35	
2DO JEFE DE DEPARTAMENTO DE 1RA	3,04	29.710,39	
JEFE DE DEPARTAMENTO DE 3RA	3,26	31.860,50	
JEFE DE DEPARTAMENTO DE 2DA	3,48	34.010,57	
JEFE DE DEPARTAMENTO DE 1RA	3,7	36.160,68	
JEFE PRINCIPAL DE DEPARTAMENTO	4,02	39.288,11	
SUBGERENTE DEPARTAMENTAL DE 3A	4,35	42.627,74	
SUBGERENTE DEPARTAMENTAL DE 2A	4,65	45.445,13	
SUBGERENTE DEPARTAMENTAL DE 1A	4,89	47.790,70	

Notes: this picture shows the pay scale negotiated by the labor union representing wage earners in the banking sector (*bancarios*) in the year 2015. This is a sector highly affected by the income tax, and they always participate in mass strikes to complain about it. The table shows the base salary that every bank has to pay to their employees depending on the seniority and hierarchy in the company (e.g., administrative with 1 to 35 years of tenure, chief of division, main chief, submanagers, etc.).

Figure A.5: Pay scale for city bus drivers



COMUNICADO ESCALA SALARIAL

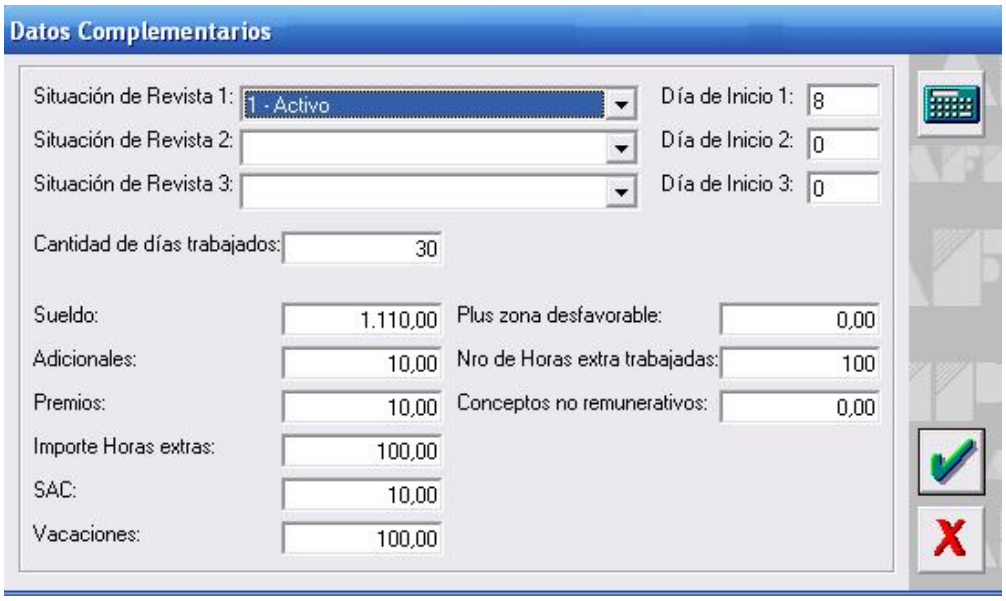
EL CONSEJO DIRECTIVO NACIONAL DE LA UNION TRANVIARIOS AUTOMOTOR COMUNICA A SUS AFILIADOS QUE DE ACUERDO A LO ESTABLECIDO EN EL ACUERDO SALARIAL OBTENIDO, REGIRA LA SIGUIENTE ESCALA SALARIAL PARA EL PERSONAL DE CORTA Y MEDIA DISTANCIA, A PARTIR DEL 1º DE ABRIL DE 2013

	SUELDO BASICO $ 6.922,64	VALOR ANTIGÜEDAD $ 132,56	PREMIO ASISTENCIA $ 1.767,46	VALOR BOLETERA $ 147,22	VIATICOS POR 24 DIAS $ 1.200	
AÑOS DE ANTIGÜEDAD	BASICO CONFORMADO	VALOR ANTIGÜEDAD	VALOR HORA SIMPLE	VALOR HORA 50%	VALOR HORA 100%	TOTAL A PERCIBIR
INICIAL	$ 8.837,32	$ 0,00	$ 46,03	$ 69,04	$ 92,06	$ 10.037,32
1	$ 8.969,88	$ 132,56	$ 46,72	$ 70,08	$ 93,44	$ 10.169,88
2	$ 9.102,44	$ 265,12	$ 47,41	$ 71,11	$ 94,82	$ 10.302,44
3	$ 9.235,00	$ 397,68	$ 48,10	$ 72,15	$ 96,20	$ 10.435,00
4	$ 9.367,56	$ 530,24	$ 48,79	$ 73,18	$ 97,58	$ 10.567,56
5	$ 9.500,12	$ 662,80	$ 49,48	$ 74,22	$ 98,96	$ 10.700,12
6	$ 9.632,68	$ 795,36	$ 50,17	$ 75,26	$ 100,34	$ 10.832,68
7	$ 9.765,24	$ 927,92	$ 50,86	$ 76,29	$ 101,72	$ 10.965,24
8	$ 9.897,80	$ 1.060,48	$ 51,55	$ 77,33	$ 103,10	$ 11.097,80
9	$ 10.030,36	$ 1.193,04	$ 52,24	$ 78,36	$ 104,48	$ 11.230,36
10	$ 10.162,92	$ 1.325,60	$ 52,93	$ 79,40	$ 105,86	$ 11.362,92
11	$ 10.295,48	$ 1.458,16	$ 53,62	$ 80,43	$ 107,24	$ 11.495,48
12	$ 10.428,04	$ 1.590,72	$ 54,31	$ 81,47	$ 108,63	$ 11.628,04
13	$ 10.560,60	$ 1.723,28	$ 55,00	$ 82,50	$ 110,01	$ 11.760,60
14	$ 10.693,16	$ 1.855,84	$ 55,69	$ 83,54	$ 111,39	$ 11.893,16
15	$ 10.825,72	$ 1.988,40	$ 56,38	$ 84,58	$ 112,77	$ 12.025,72
16	$ 10.958,28	$ 2.120,96	$ 57,07	$ 85,61	$ 114,15	$ 12.158,28
17	$ 11.090,84	$ 2.253,52	$ 57,76	$ 86,65	$ 115,53	$ 12.290,84
18	$ 11.223,40	$ 2.386,08	$ 58,46	$ 87,68	$ 116,91	$ 12.423,40
19	$ 11.355,96	$ 2.518,64	$ 59,15	$ 88,72	$ 118,29	$ 12.555,96
20	$ 11.488,52	$ 2.651,20	$ 59,84	$ 89,75	$ 119,67	$ 12.688,52
21	$ 11.621,08	$ 2.783,76	$ 60,53	$ 90,79	$ 121,05	$ 12.821,08
22	$ 11.753,64	$ 2.916,32	$ 61,22	$ 91,83	$ 122,43	$ 12.953,64
23	$ 11.886,20	$ 3.048,88	$ 61,91	$ 92,86	$ 123,81	$ 13.086,20
24	$ 12.018,76	$ 3.181,44	$ 62,60	$ 93,90	$ 125,20	$ 13.218,76
25	$ 12.151,32	$ 3.314,00	$ 63,29	$ 94,93	$ 126,58	$ 13.351,32
26	$ 12.283,88	$ 3.446,56	$ 63,98	$ 95,97	$ 127,96	$ 13.483,88
27	$ 12.416,44	$ 3.579,12	$ 64,67	$ 97,00	$ 129,34	$ 13.616,44
28	$ 12.549,00	$ 3.711,68	$ 65,36	$ 98,04	$ 130,72	$ 13.749,00
29	$ 12.681,56	$ 3.844,24	$ 66,05	$ 99,07	$ 132,10	$ 13.881,56
30	$ 12.814,12	$ 3.976,80	$ 66,74	$ 100,11	$ 133,48	$ 14.014,12

ANTIGÜEDAD: $ 132,56 POR AÑO - VIATICOS POR DIA TRABAJADO $ 50

Notes: this picture shows the pay scale negotiated by the labor union UTA representing city bus drivers (*colectiveros de corta y media distancia*) in the year 2013. The table shows different pay concepts (in columns) that vary by years of tenure from 0 to 30 (in rows): base salary in column 1, additional pay per year of tenure in column 2, a plus for presenteeism in column 3, overtime pay premiums in columns 4 and 5, and total monthly wage in column 6.

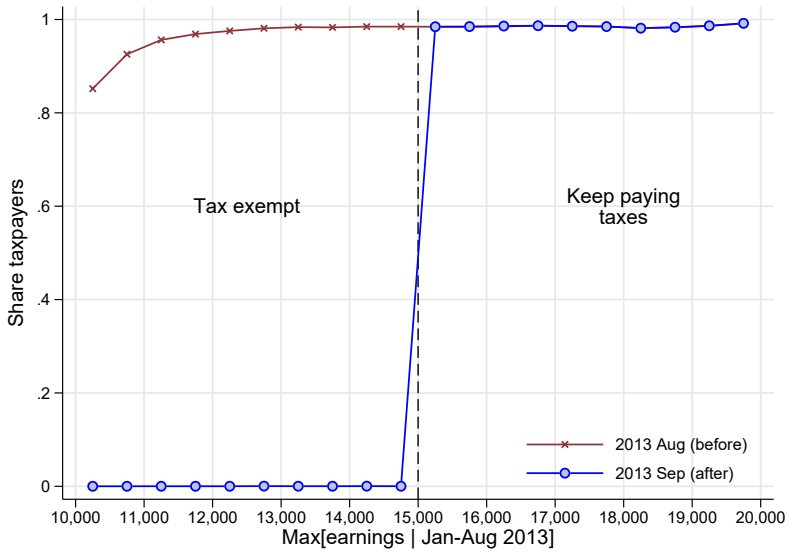
Figure A.6: SSC filing software (Aplicativo SICOSS)



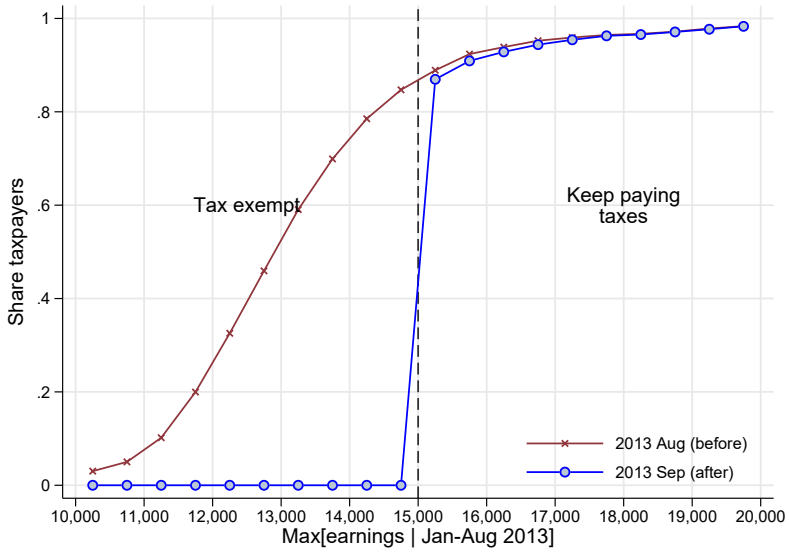
Notes: this figure provides a snapshot of the managing software used by employers to report monthly wage earnings of every worker in their payroll and to pay the associated social security contributions. This simple interface is the source of the core data used in the paper. The figure contains two panels. The bottom panel is the one where employers report earnings and some subcomponents. *Sueldo* contains monthly wage earnings. *Adicionales* contains other payments such as presenteeism, college degree, seniority. *Premios* contains bonuses (productivity, commissions). *Importe Horas extras* contains monthly overtime pay. *SAC* contains the 13th salary. *Vacaciones* contains vacation plus. *Plus zona desfavorable* contains a payment for people living in the south of the country. *Nro de Horas extra trabajadas* contains monthly overtime hours. *Conceptos no remunerativos* contains non-contributory payments negotiated by labor unions that are exempt from payroll taxes.

Figure A.7: Fraction of salaried workers subject to the income tax

(a) Single workers without children



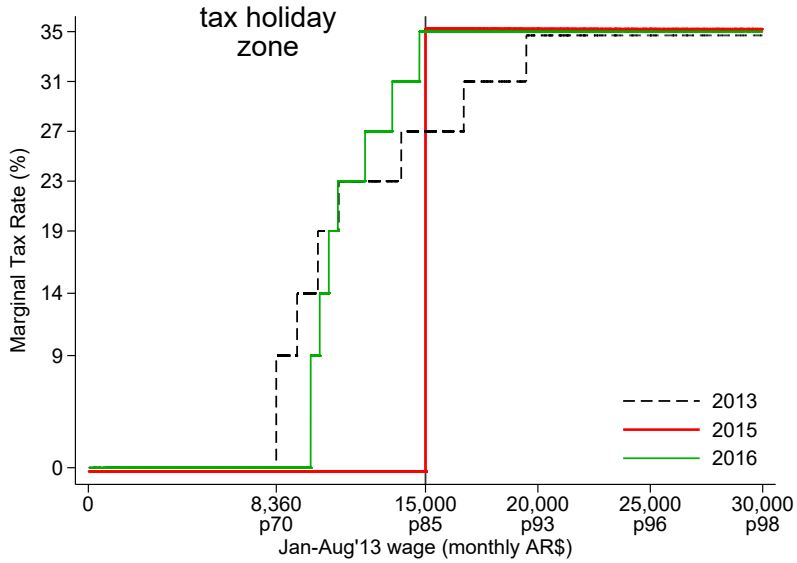
(b) Married workers with two children



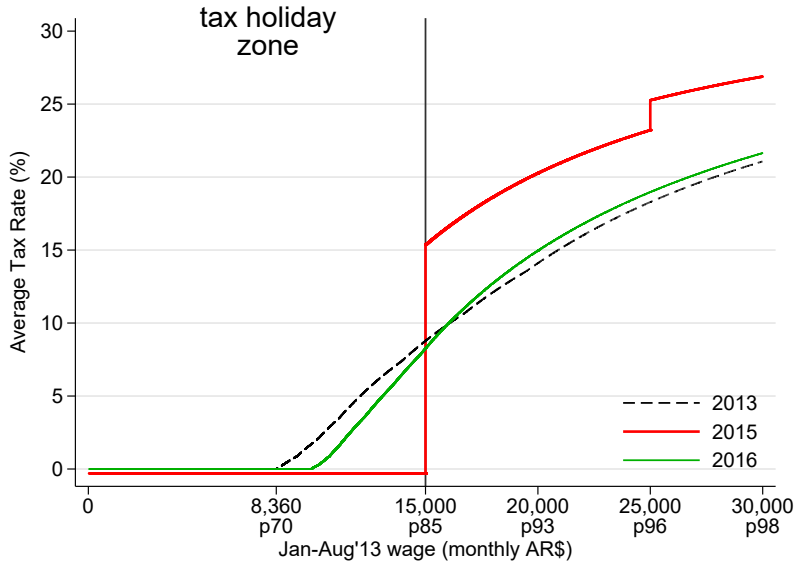
Notes: this figure plots the fraction of salaried workers subject to the income tax before and after the reform against the the running variable in the RDD for single workers without children (panel a) and married workers with two children (panel b). The vertical dashed line denotes the discontinuity introduced by the reform at AR$15,000. In both panels the bin width is AR$500.

Figure A.8: First stage change in MTR and ATR - Single workers without children

(a) Marginal tax rates

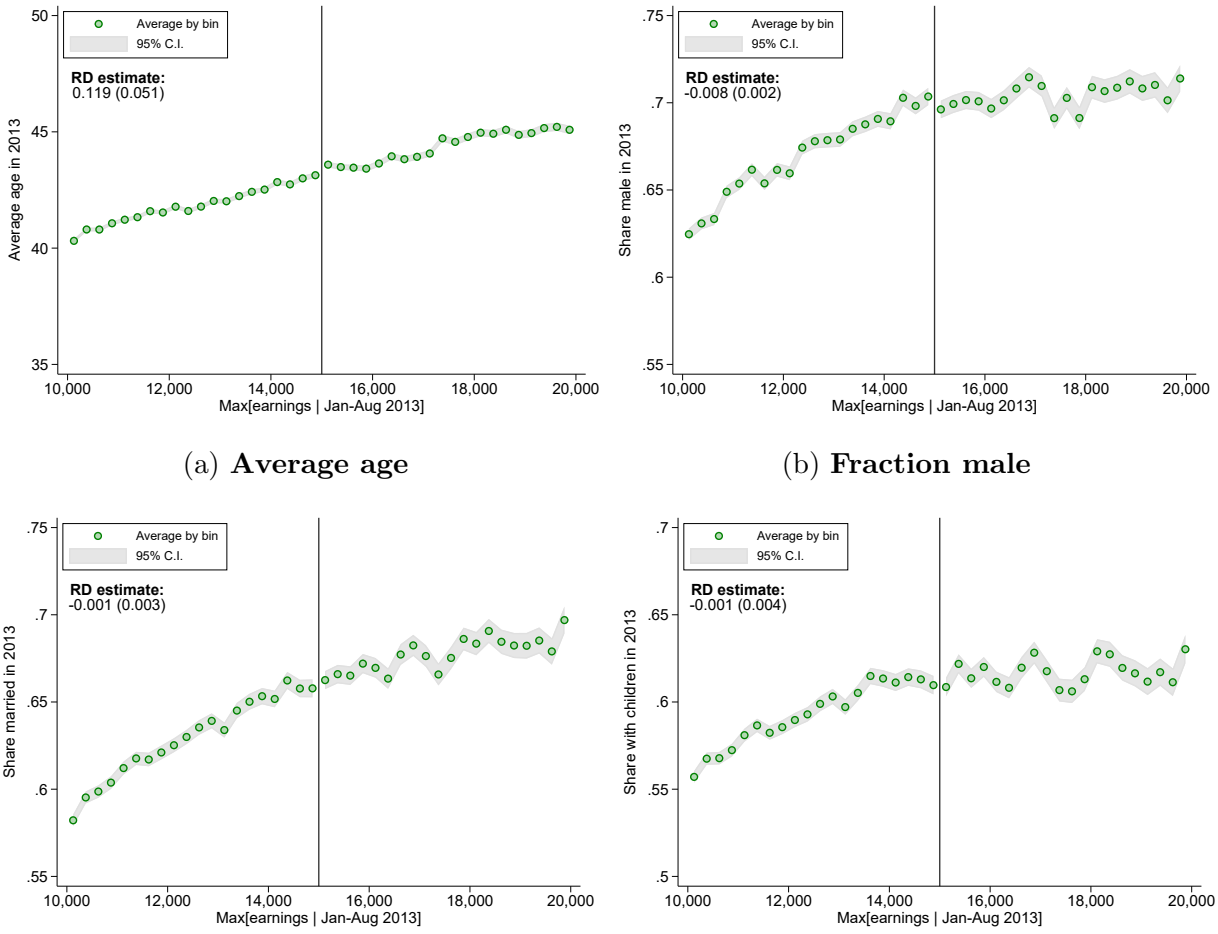


(b) Average tax rates



Notes: These figures plot marginal and average tax rates before, during, and after the tax holiday. The rates are computed for a single worker without children and assuming that she earners a constant monthly wage in the first eight months of 2013. Since the running variable takes the highest monthly wage, the numbers from this figure constitute an upper bound for the empirical first stage. Taxable income is computed by subtracting payroll taxes of $\tau_{payroll} = 17\%$ and personal exemptions of AR$8,360 from gross wage earnings. Personal exemptions for married workers with two children are AR$11,563 and therefore the change in tax rates would look smaller. The MTRs and tax liability are calculated using the schedule in Table 1.1.

Figure A.9: Covariate balance around the discontinuity



(a) **Average age**

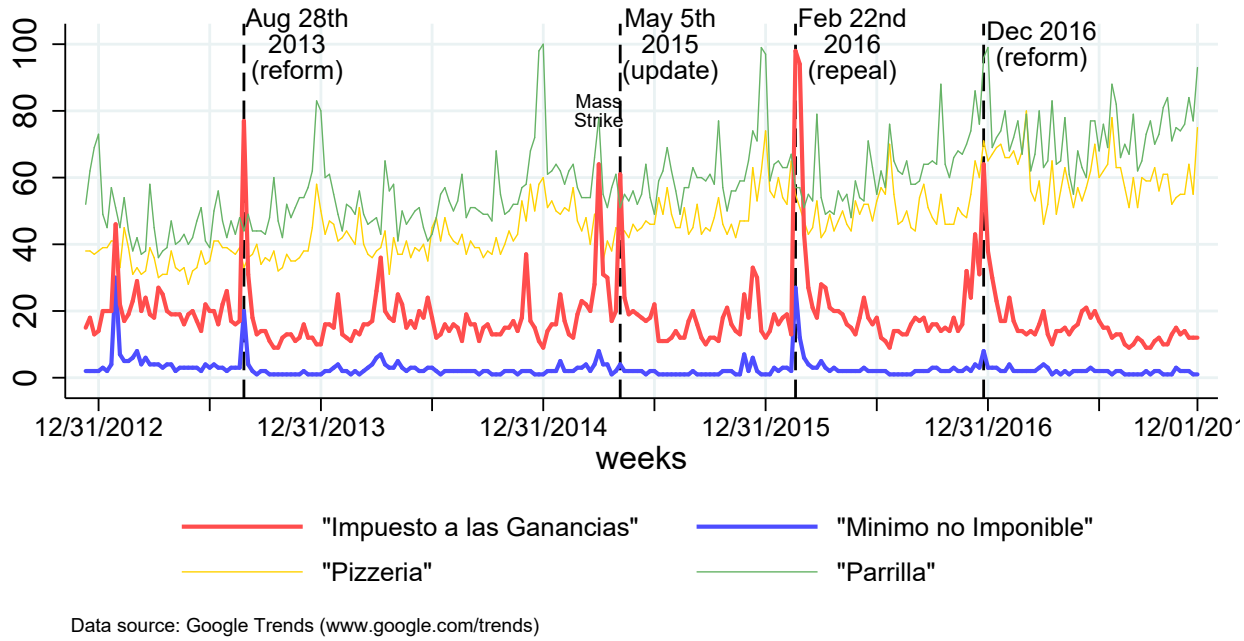
(b) **Fraction male**

(c) **Fraction married**

(d) **Fraction with children**

Notes: these figures show demographic characteristics of the sample around the discontinuity as of August 2013 by bins of the running variable (width AR$ 250). Panel (a) displays the average age of wage earners, panel (b) displays the fraction of male workers, panel (c) displays the fraction of married workers, panel (d) displays the fraction of wage earners with children. RD estimates are reported in each graph using a triangular kernel, linear fit, and bandwidth of AR$ 3,000. We use the `rdrobust` routine from Calonico et al. (2017). The four panels show that there is no systematic difference in observable variables between wage earners just above and just below the cutoff, a requirement for the RDD to be valid.

Figure A.10: Google Trends queries for income-tax related terms in Argentina 2012-2017



Data source: Google Trends (www.google.com/trends)

Notes: This figure displays Google Trends queries for income-tax related terms in Argentina during 2012-2017: *impuesto a las ganancias* (income tax) and *minimo no imponible* (non-taxable income floor). It also shows vertical markers for policy events and two other popular searches as a benchmark: *parrillas* (steak restaurant) and *pizzeria* (pizza restaurant). The numbers represent the popularity of each term in Argentina, during this period of time, relative to the highest point on the chart (*parrillas* in December 2014). A value of 50 means that the term is half as popular as the peak. The first red spike corresponds to March 2013 when the government updated the annual value of personal exemptions after 2 years with no adjustments, the second red spike coincides with the announcement of the tax holiday, the third and fourth red spikes correspond to a mass national strike organized by labor unions that were partially benefitted by the holiday, the fifth red spike coincides with the repeal of the holiday, and the last red spike coincides with a comprehensive reform of the income tax (a new law voted in Congress). The figure shows that people actively searched for key words related to the income tax on the internet around the time the reform was passed, updated, and repealed. Although the search level of income tax terms is lower than the level of more popular terms like *parrillas* and *pizzeria*, the red line displays sharp spikes exactly at the key dates.

Figure A.11: The reform covered by centre-right newspapers

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MODIFICACIONES PRINCIPALES CON EL NUEVO RÉGIMEN

Cómo impactará en el bolsillo de los trabajadores las modificaciones en el cobro del impuesto a las ganancias y cuál será el universo afectado

TOPE DEL MONTO MÍNIMO NO IMPONIBLE — Antes del anuncio — Ahora

TOPE COMÚN — Soltero y casado con hijos — Casado con hijos — Soltero — $ 11.563 — $ 9360 — 15000 — AUMENTO EN EL TRAMO — + 33% — + 60%

CAMBIO DE DEDUCCIONES

El monto mínimo para trabajadores con sueldo bruto de entre $ 15.000 y 25.000, aumentará 20%

Ejemplo de sueldo bruto de $ 30.000 — Pagaba — Pagará

Casado con hijos — $ 750 — $ 304 — $ 450

Soltero — $ 1486 — $ 1092 — $ 395

En la región patagónica

El monto mínimo para trabajadores con sueldo bruto de entre $ 15.000 y 25.000, aumentará 30%

LOS QUE PAGARÁN Y LOS QUE NO

Empleados — 89,8% no pagarán — 10,2% pagarán

Jubilados — 99,3% no pagarán — 0,7% pagarán

POLÍTICA

Edición de hoy a cargo de Jorge Liotti | www.lanacion.com/politica | @politica_LN | Facebook.com/lanacion | LNpolitica@lanacion.com.ar

CAMBIOS IMPOSITIVOS | UN RECLAMO GENERALIZADO

Anuncian que no pagarán Ganancias los salarios inferiores a los 15.000 pesos

El Gobierno busca una mejora del clima social de cara a las elecciones de octubre; entre los 15.000 y los 25.000 pesos habrá una reducción del 20 por ciento; también subirán las asignaciones familiares; se crea un impuesto para títulos y dividendos

Mariano Obarrio
LA NACION

(a) August 28th, 2013

Clarín

UN TOQUE DE ATENCION PARA LA SOLUCION ARGENTINA DE LOS PROBLEMAS ARGENTINOS

MIERCOLES 28 AGOSTO DE 2013

BUENOS AIRES REPUBLICA ARGENTINA AÑO LXIX Nº 24.321

Historia Time del siglo XX

HOY, ENTREGAN Nº 2: LOS FASCISMOS. COMPRA OPCIONAL

Luisana Lopilato es mamá

Nació Noah, el hijo de la actriz argentina y el canadiense Michael Bublé. "Nos llenó de luz su llegada", dijo Luisana. P.62

Tema del día Un anuncio del Gobierno que reclamaban los gremios y la oposición

Pagarán Ganancias sólo los que ganen más de $ 15.000

Eximen así a un millón y medio de trabajadores y jubilados. También se reducirá ese impuesto a los que ganen un sueldo bruto de $ 15.000 a $ 25.000. Y mejoran las asignaciones familiares. Rige desde septiembre. Para compensar en parte esta reducción de ingresos del Estado, crearán dos gravámenes a empresas. P.3

Tiempo electoral

Una medida para seducir a la clase media y la Patagonia

(b) August 28th, 2013

Ganancias: el decreto 1242 provocó un trato desigual entre asalariados

Hace casi un año, la norma liberó del pago de Ganancias a los asalariados y jubilados que entre enero y agosto de 2013 habían percibido salarios o haberes brutos no mayores a $ 15.000

SEGUIR Silvia Stang LA NACION MARTES 22 DE JULIO DE 2014 • 12:24

Hace casi un año, el decreto 1242 liberó del pago de Ganancias a los asalariados y jubilados que, entre enero y agosto de 2013, habían percibido salarios o haberes brutos no mayores a $ 15.000. Tal como se había advertido en su momento, esa medida provocó un trato

(c) July 22nd, 2014

Clarín

Un toque de atención para la solución argentina de los problemas argentinos

Miércoles 1.04.2015

BUENOS AIRES, ARGENTINA · PRECIO: $ 11,00

Rodríguez Larreta "Es un orgullo que Macri me apoye" El jefe de Gabinete porteño, en el ciclo "Un café en Clarín". P.20

Argentina Venció 2-1 a Ecuador en Nueva Jersey y mejoró su nivel. P.62

AGÜERO FESTEJA SU GOL, EL PRIMERO.

Tema del día Los gremios ya discuten nuevas protestas contra el Gobierno

El paro por Ganancias tuvo un fuerte impacto

No hubo colectivos, trenes ni subtes. Y además la izquierda cortó rutas y accesos en las principales ciudades del país. Según aseguraron los gremialistas, pararon el 94% de los trabajadores. La huelga se sintió tanto en Capital como en el GBA y en el interior. P.3

Cristina: "Con trenes y subtes, no hubiese habido paro general"

Escriben • Van der Kooy, Carpena y Bermúdez

TAMARA FLOWN

Sospechas de corrupción

(d) April 1st, 2015

Notes: this picture shows the repercussion that the income tax change had in the main newspapers of Argentina. Panels (a) and (c) correspond to *Diario La Nación* and panels (b) and (d) to *Diario Clarín*. Panel (a) reads: "the government announced that monthly wage earnings lower than AR$ 15,000 are exempt from the income tax"; panel (b) reads: "only workers earning more than AR$ 15,000 will be subject to the income tax"; panel (c) reads: "income tax: decree 1242 provoked an unequal treatment between wage earners"; panel (d) reads: "the national mass strike against the income tax had a strong impact". Panels (a) and (b) correspond to the day the tax holiday was announced, and panels (c) and (d) correspond to two dates in the middle of the tax holiday. See also Diario La Nación (`http://servicios.lanacion.com.ar/archivo/2013/08/28/005/DT`) and Diario Clarín (`http://tapas.clarin.com/tapa.html#20130828`).

Figure A.11 (cont.): The reform covered by centre-left newspapers

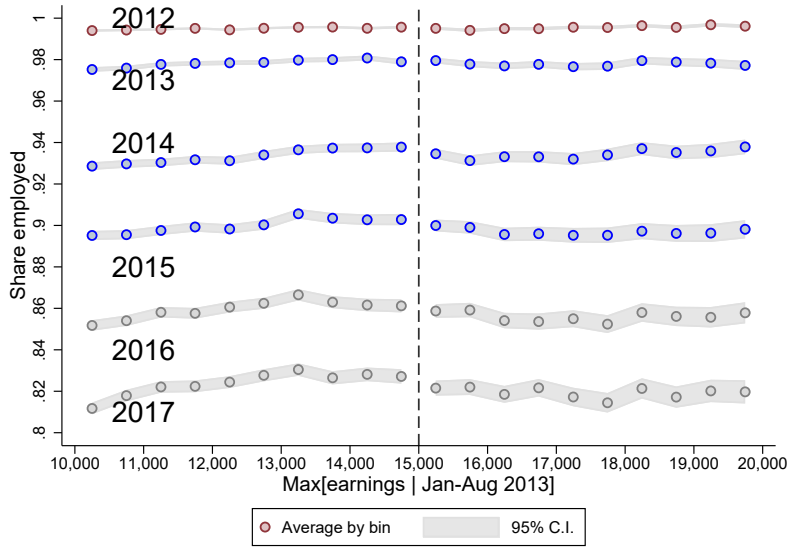
(a) August 28th, 2013

(b) August 31st, 2013

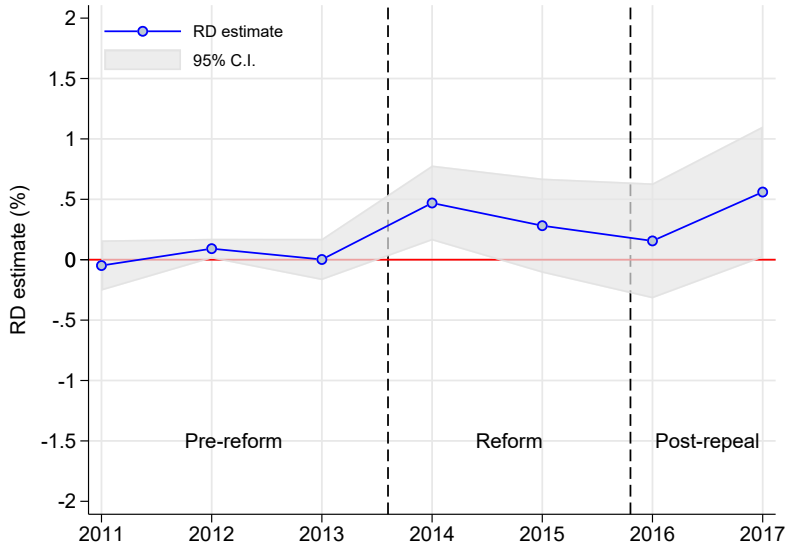
Notes: this picture shows the repercussion that the income tax change had in the main centre-left newspaper of Argentina, *Diario Página|12*. Panel (e) reads: "up to AR$ 15,000 you don't pay", and explains that the announcement made by the President takes effect immediately starting on September 1st 2013; the front page from panel (f) says that the government and the Argentine IRS issued a resolution explaining in detail who is benefited and who is not, and the way to compute the assignment variable.

Figure A.13: Fraction of wage earners that remain employed

(a) **Averages by bin**



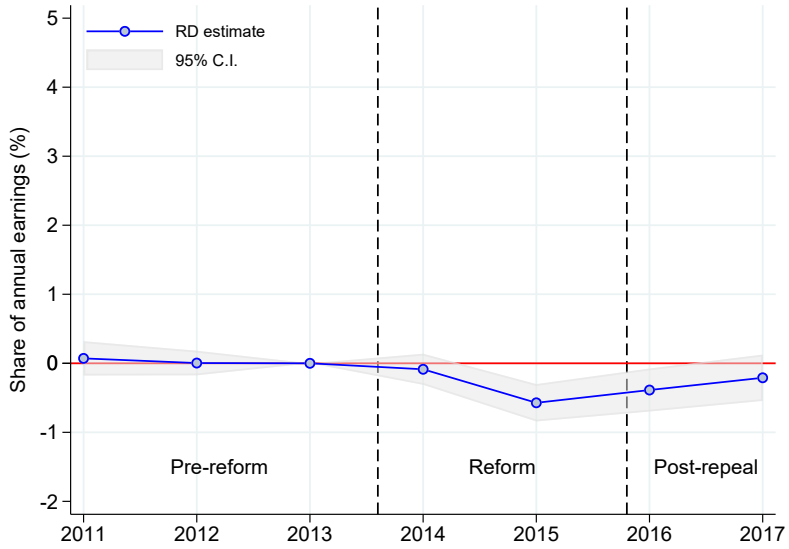
(b) **RD estimates**



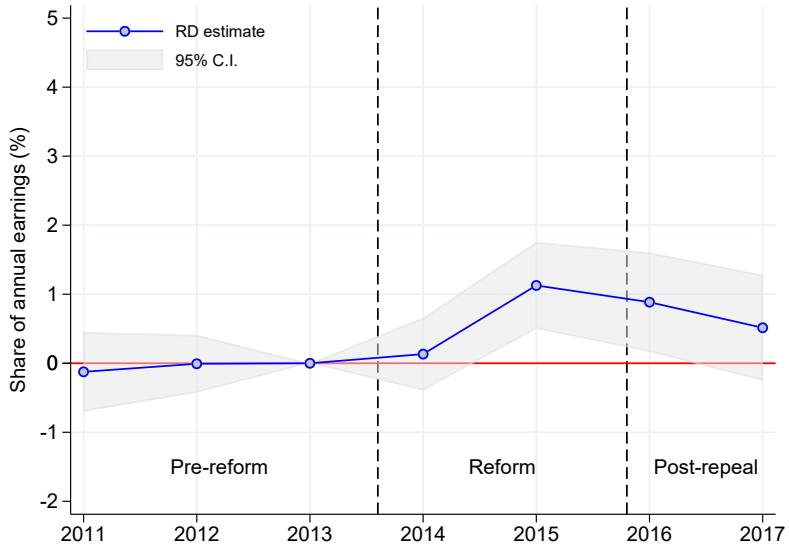
Notes: this figure plots the fraction of wage earners that remain employed by bins of the running variable (panel a) and the evolution of the RD estimates (panel b). The dependent variable is an indicator for whether the worker has positive wage earnings by December of each year. Averages are computed for 10 equally spaced bins of AR$ 500 on each side. In panel (a) we use blue dots to denote the years (December) in which the tax holiday was in place. The figure captures an extensive margin responses and shows that workers did not dropout out of the labor force differentially above and below the discontinuity.

Figure A.14: Evolution of RD estimates for base salary and residual compensation

(a) Base salary



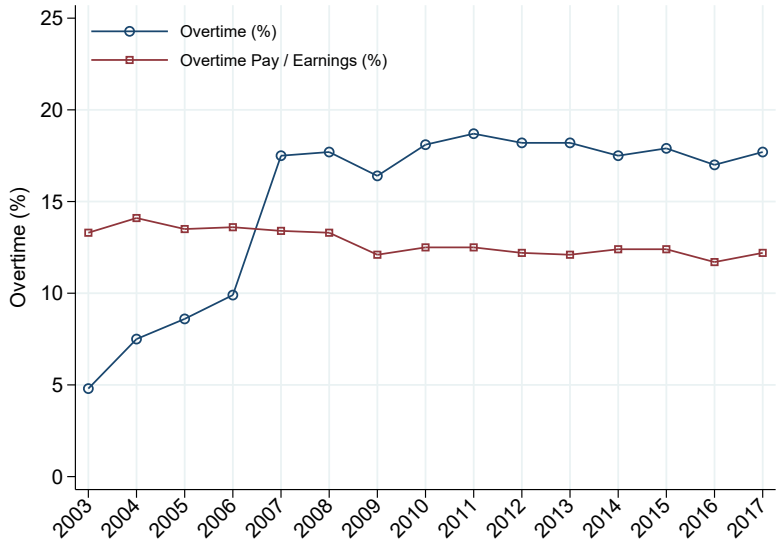
(b) Residual (total wage earnings - base salary)



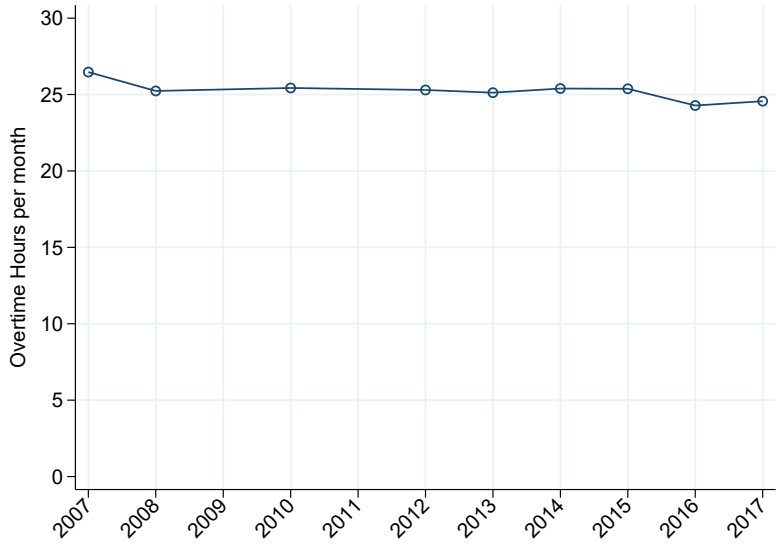
Notes: this graph plots the evolution of the RD estimates for base salary and residual compensation as a share of total compensation. Each dot corresponds to a separate RD regression using a linear fit on each side of the discontinuity, a triangular kernel, and a AR$3,000 bandwidth. The dependent variable in the RD is the share of base salary in total wage earnings (panel a) and the share of residual compensation in total wage earnings (panel b), both relative to their share in 2013. The residual is computed as the difference between total wage earnings and base salary. The vertical dashed lines indicate the beginning and the end of the tax holiday.

Figure A.15: Descriptives statistics for overtime work

(a) **Overtime likelihood (%) and participation in wage earnings (%)**



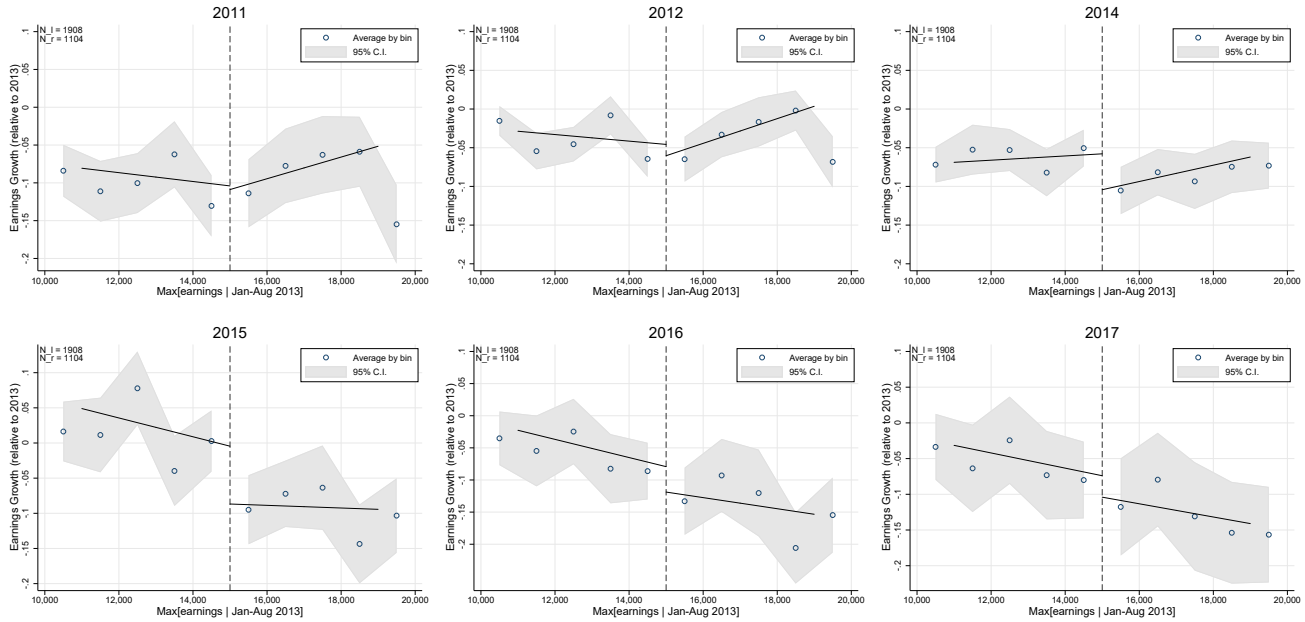
(b) **Overtime hours per month**



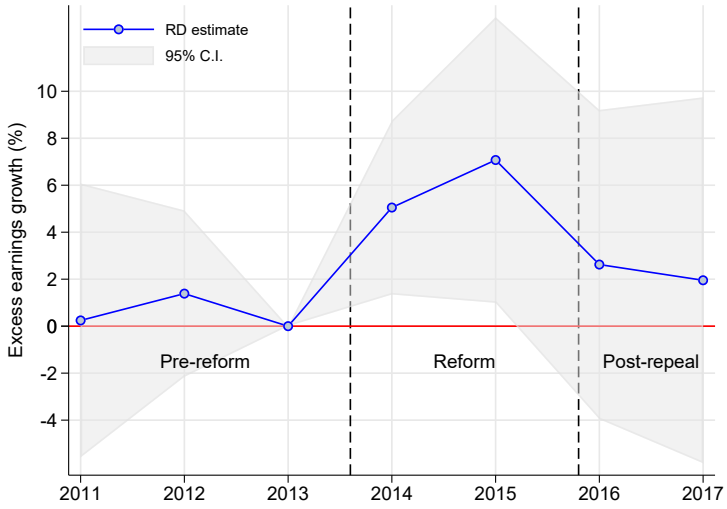
Notes: these figures show time series of three overtime outcomes for the universe of wage earners in Argentina during the period 2003-2017. Panel (a) reports the fraction of workers doing overtime (blue line) and the participation of overtime pay in total wage earnings (red line). Panel (b) shows average overtime hours per month. Overtime hours started being reported in the data in 2007. About 17% of wage earners work overtime with a participation in total wage earnings of 13%. Conditional on working overtime, average monthly hours are 25. Source: own calculation based on SIPA microdata.

Figure A.16: RD estimates for executives and managers

(a) **RD plots for executives and managers, 2011-2017**

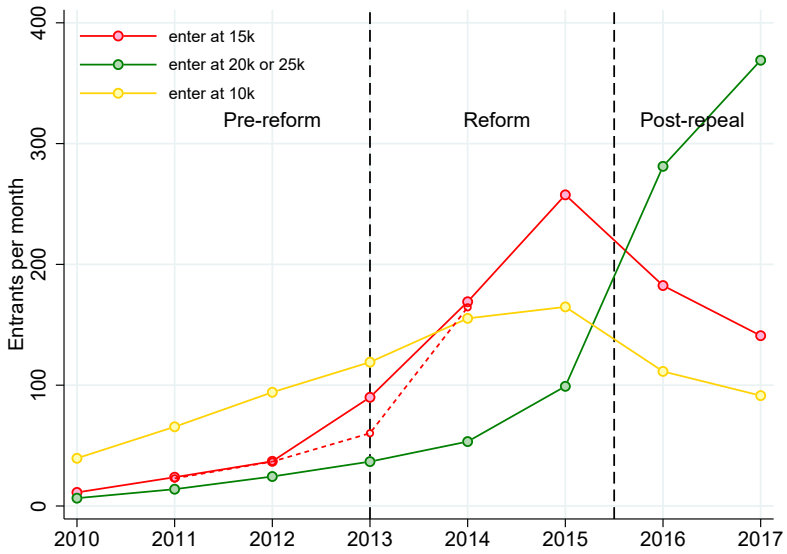


(b) **Evolution of RD estimates**



Notes: this figure plots wage earnings growth by bins of the running variable (panel a) and the evolution of the RD estimates (panel b) for executives paid as wage earners in the period 2011-2017. The dependent variable in the RD is annual earnings growth relative to 2013. The averages in panel (a) are computed for 10 equally spaced bins of AR$ 1,000. Each dot from panel (b) corresponds to a separate standard RD regression using a linear fit on each side of the discontinuity, a triangular kernel, and a AR$4,000 bandwidth. The point estimate thus measures the excess earnings growth between managers below and above the discontinuity. The vertical dashed lines indicate the beginning and the end of the reform.

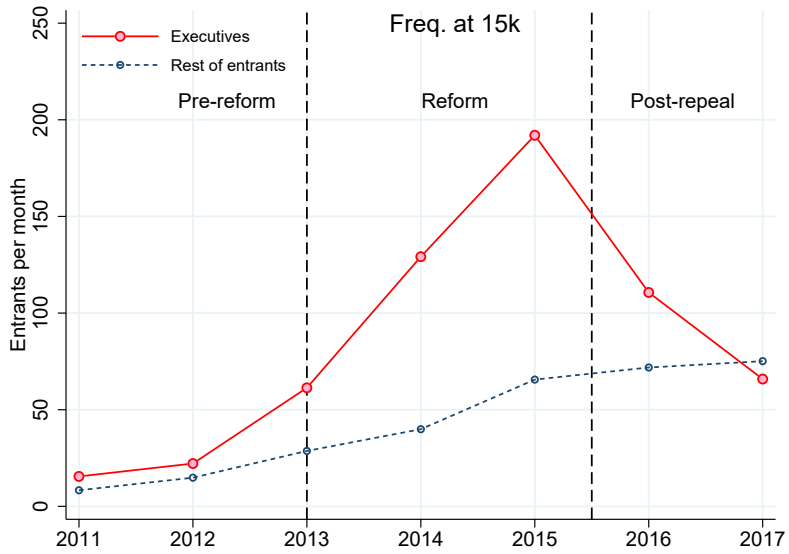
Figure A.17: Entrants with initial monthly salary exactly at 10k, 15k, 20k, and 25k



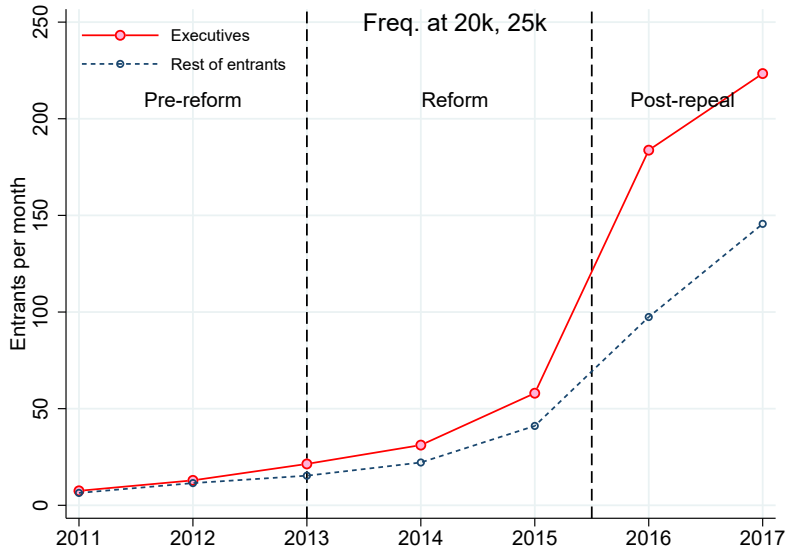
Notes: this graph plots the number of wage earners entering exactly at 10k (yellow line), 15k (red line), 20k or 25k (green line) for three years pre-reform, two years during the reform, and two years post-repeal. We first count the number of wage earners entering exactly at a focal point in each month and then compute the monthly average for different years. The mass at 10k and 15k qualifies for the tax holiday, and the mass at 20k and 25k does not qualify. The vertical dashed lines indicate the beginning and the end of the tax holiday.

Figure A.18: Entrance at focal points: executives vs rest of entrants

(a) Entering at 15k (tax benefit zone)

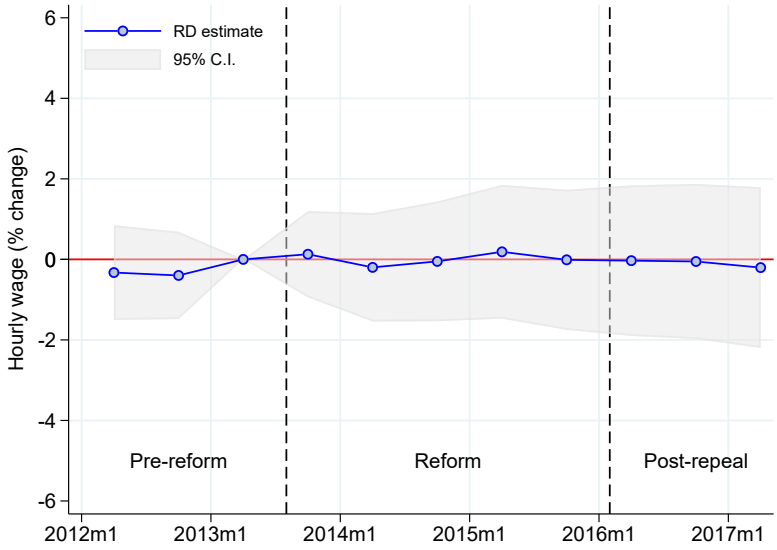


(b) Entering at 20k or 25k (tax zone)



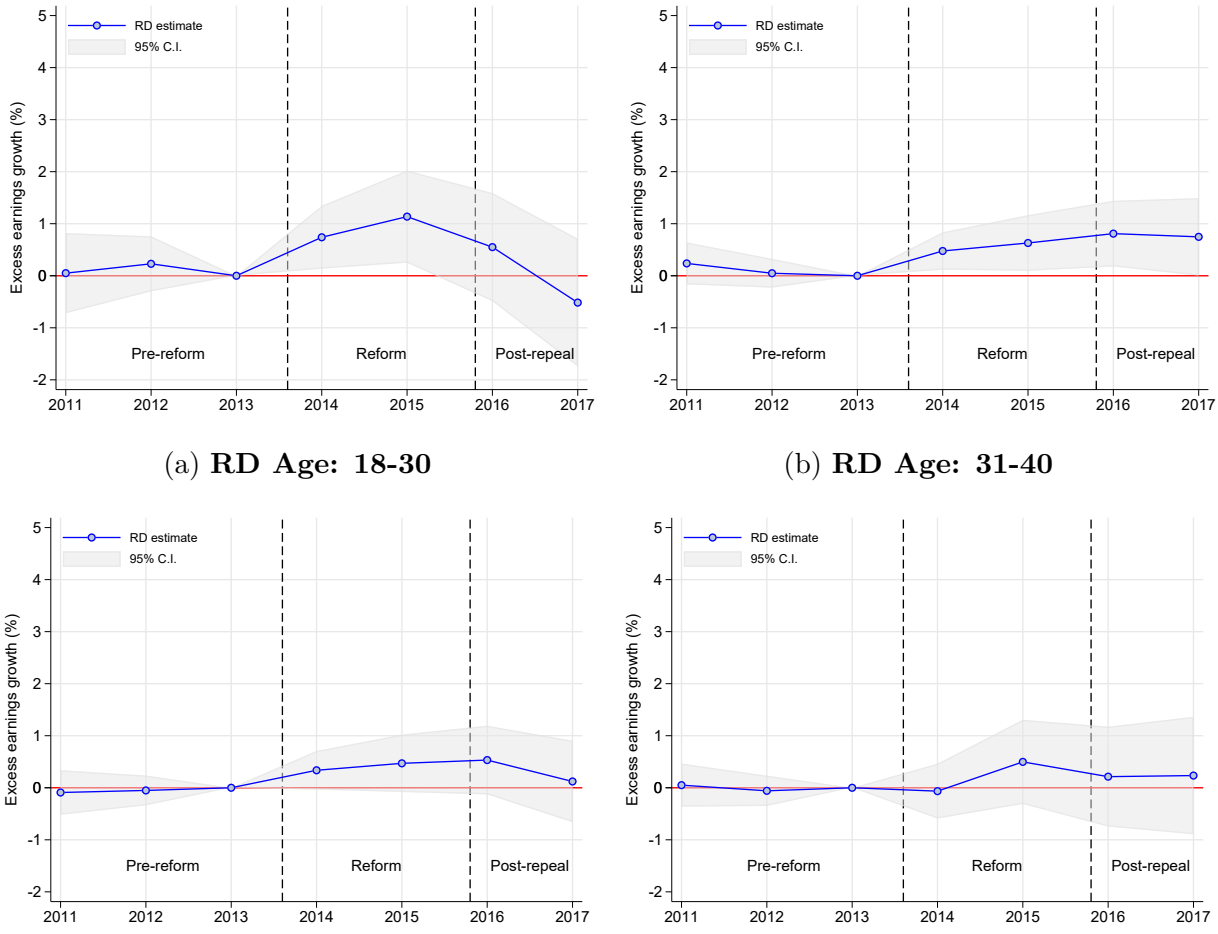
Notes: this figure shows counts of wage earners entering at focal points broken by executive workers (red line) versus rest of entrants (dashed blue line). Panel (a) corresponds to entry at 15k (tax exempt during the reform years) and panel (b) corresponds to entry at 20k or 25k (tax liable). The vertical dashed lines indicate the beginning and the end of the tax holiday. For more details see Figure A.17.

Figure A.19: RD estimates for hourly wages to rule out an incidence story



Notes: this graph plots the time series of RD estimates computed as in Figure 1.6. The vertical blue lines indicate the beginning of the reform, August 2013, and the date it was repealed, February 2016. Each dot corresponds to a separate regression. The dependent variable is the hourly wage. We use the sample of overtime workers for whom we observe monthly hours, and back out the hourly wage from the ratio of overtime pay to overtime hours. This precise zero effect from this figure shows that the null aggregate elasticity is hardly explained by an incidence story in which workers are indeed working longer hours but employers reduce their wage rate.

Figure A.20: A test for income and substitution effects that cancel out



(a) **RD Age: 18-30**

(b) **RD Age: 31-40**

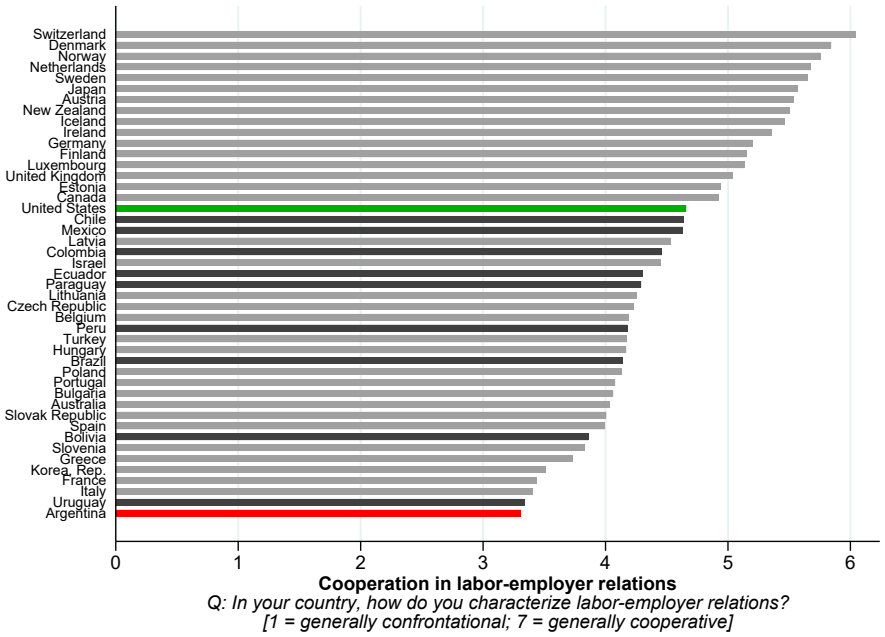
(c) **RD Age: 41-50**

(d) **RD Age: 51+**

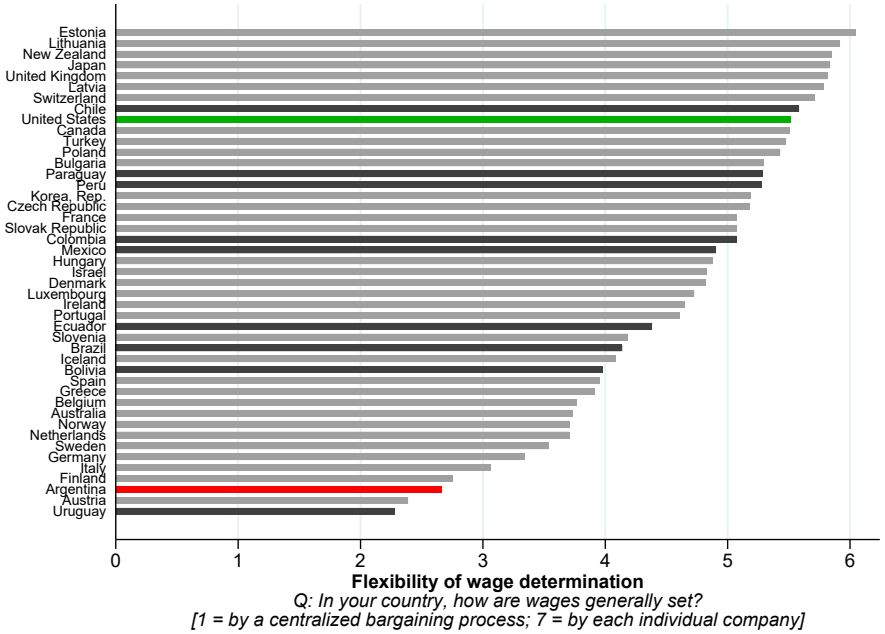
Notes: this graph plots the time series of RD estimates computed as in Figure 1.6. Panel (a) corresponds to workers ages 18-30; Panel (b) corresponds to workers ages 31-40; Panel (c) corresponds to workers ages 41-50; Panel (d) corresponds to workers older than 50. The vertical dashed lines indicate the beginning of the tax holiday, August 2013, and the date it was repealed, February 2016. Each dot corresponds to a separate regression. The RD estimates are computed by comparing annual wage earnings growth relative to 2013 for workers with a running variable slightly below and above AR$ 15,000 (i.e., the excess earnings growth at the threshold).

Figure A.21: Labor market rigidities across the globe

(a) Employer-employee cooperation



(b) Flexibility of wage determination



Notes: this figure presents measures on labor market rigidities using comparable data from 45 countries (OECD and South America). The statistics come from an Executive Opinion Survey of a representative sample of business leaders in their respective countries (In 2014: 14,000 leaders in 148 economies; Median = 87 overall, 122 in Argentina). **Source**: World Economic Forum, the Global Competitiveness Index Dataset 2013-2014.

Figure A.22: Labor market rigidities across the globe

Notes: this figure presents measures on labor market rigidities using comparable data from 45 countries (OECD and South America). The statistics come from an Executive Opinion Survey of a representative sample of business leaders in their respective countries (In 2014: 14,000 leaders in 148 economies; Median = 87 overall, 122 in Argentina). **Source**: World Economic Forum, the Global Competitiveness Index Dataset 2013-2014.

A.2 The quality of the running variable

The identification of causal effects in RDDs is potentially undermined by measurement error in the assignment variable. In this section we take this threat very seriously and acknowledge that our assignment could in principle suffer from *non-classical* measurement error to the left of the discontinuity, but we argue that in practice it ought to be a small issue. To that end, we first formalize the argument and then present granular evidence from two anonymous firms that suggests that attenuation bias appears small and it does not pose a threat to our empirical findings.[1]

Recall that the running variable is given by the **highest gross monthly wage** accrued in the first eight months of 2013. The day after the decree was passed, the Argentine IRS issued a circular (RG 3525/2013) clarifying the way employers should compute this variable. In particular, it stated that accountants should only consider **monthly**, **normal**, and **habitual** concepts perceived for at least 6 of the 8-months reference period. This implied that they had to exclude unusual one-time payments such as the 13th salary paid in June, annual bonuses, vacation plus, non-regular overtime pay or commissions, etc.

Although firms and accountants are familiarized with the definition of the running variable (e.g., it is the same earnings base used to calculate severance payments), there could be some ambiguous cases where our judgement of "unusual payments" differs from the (unobserved) decision taken by the firm. In particular, the monthly frequency and detail of our data allow us to be very cautious and to subtract *any* unusual payment from the running variable. Thus, it could happen that a firm places a worker to the right of the discontinuity if it misses an unusual payment when computing the assignment variable (e.g., the firm includes an annual bonus paid in May that we exclude). Hence, we argue that our *measured* running variable is lower than or equal to the *true* running variable, meaning that *some* workers could be misplaced to the left of the discontinuity (i.e., the error is not symmetric) introducing some fuzziness to our design.[2]

Non-classical measurement error in the assignment variable

We formalize this potential issue by adapting the framework developed by Battistin, Brugiavini, Rettore, and Weber (2009). Using the potential outcomes framework, the outcome of interest can be written as:

$$Y = Y_0 + T(W_{max})\beta \tag{B.1}$$

where Y_0 is the outcome absent the reform, $W_{max} \equiv \max\{Monthly\ Salary|\text{Jan-Aug'13}\}$ is the true running variable, $T = 1$ if $W_{max} > 15k$ so that the worker keeps paying the income

[1] We are very grateful to Zhuan Pei for helpful feedback in this section.

[2] Nonetheless, the richness of our data allow us to be "good accountants" and minimize this source of error. Moreover, the data we use are in fact reported by accountants themselves to the IRS based on workers' payslips.

tax normally, $T = 0$ if $W_{max} \leq 15k$ so that the worker becomes tax exempt, and $\beta = Y_1 - Y_0$ is the causal effect (the change in earnings corresponding to a change in the income tax). Hence, our design is sharp by construction. We make the following assumptions:

Assumption 1: $E[Y_0|W_{max}]$ is a continuous function of W_{max} at $\bar{c} = 15k$. That is, in the absence of the policy no discontinuity would be observed in outcome Y around $15k$.
Assumption 2: our *measured* assignment variable presents *non-classical* measurement error with the following form:

$$\hat{W}_{max} = W_{max} \cdot Z + \widetilde{W}_{max} \cdot (1 - Z), \quad \text{with } \widetilde{W}_{max} < W_{max} \tag{B.2}$$

$$\varepsilon = \hat{W}_{max} - W_{max} = (\widetilde{W}_{max} - W_{max}) \cdot (1 - Z) \leq 0 \tag{B.3}$$

where $\hat{W}_{max}$ is our *measured* running variable, W_{max} is the one calculated by employers, and Z is an indicator for exact matches that we assume is *iid*. So our measure $\hat{W}_{max}$ captures a mixture of accountants constructing the running variable exactly as we do and accountants doing it slightly different (e.g., by not excluding unusual one-time payments from the running variable).[3] Note that measurement error ε is non-classical because it depends on the true running variable W_{max} (while under classical measurement error, ε is assumed to be independent of W_{max}).[4]

This formulation says that regardless of the value of W_{max}, there is some probability that our *measured* running variable is smaller than the true (unobserved) one, i.e. there is some probability that the firm misses some unusual payments that the econometrician properly excludes. As a result, the misclassification of $T(W_{max})$ is only one-sided, meaning that to the left of the cutoff there are some people for which we get it right and some other people for which we get it wrong. While to the right of the cutoff, workers are not misclassified. Empirically, the relevant question is whether the fraction $Pr[Z = 1|\hat{W}_{max} = \bar{c}_-]$ is large or small.

Proposition 1: Under Assumption 1, $E[Y_0|\hat{W}_{max}]$ is a continuous function of $\hat{W}_{max}$ at $\bar{c} = 15k$.
Proof. Noting from (B.3) that $\hat{W}_{max} = W_{max} + \varepsilon$, we can write:

$$\begin{aligned} E[Y_0|\hat{W}_{max} = w] = E[Y_0|W_{max} + \varepsilon = w] &= \int E[Y_0|W_{max} = w - \varepsilon, \varepsilon] \cdot dF(\varepsilon|w) \\ &= \int E[Y_0|W_{max} = w - \varepsilon] \cdot dF(\varepsilon) \end{aligned}$$

[3]The nature of our measurement error is related to the treatment given by Card, Lee, Pei, and Weber (2015) for the fuzzy RKD case. This is also known as the *contaminated sampling model* (Horowitz and Manski, 1995).
[4]The fact that our assignment variable does not suffer from classical measurement error is critical, as Pei and Shen (2017) show that under classical measurement error (i.e., mean-zero white noise) even if $E[T|W_{max}]$ were discontinuous at $\bar{c}$, such discontinuity would be smoothed out by the measurement error and as a result $E[T|\hat{W}_{max}]$ would be smooth at $\bar{c}$ killing the identification of the causal effect at the discontinuity.

where the second equality follows from the LIE and the third equality follows from the standard independence assumption that measurement error does not affect Y directly. Then, since $E[Y_0|W_{max}]$ is continuous, integrating over different values of ε is also continuous. □

Using equation (B.1), Assumption 1, Assumption 2, and Proposition 1, we can write the difference in mean outcomes for workers slightly above and below $\bar{c} = 15k$ as:

$$\begin{aligned} E[Y|\hat{W}_{max} = \bar{c}_+] - E[Y|\hat{W}_{max} = \bar{c}_-] &= E[Y_0|\hat{W}_{max} = \bar{c}_+] - E[Y_0|\hat{W}_{max} = \bar{c}_-] \\ &+ E[T(W_{max})\beta|\hat{W}_{max} = \bar{c}_+] - E[T(W_{max})\beta|\hat{W}_{max} = \bar{c}_-] \end{aligned} \tag{B.4}$$

The first two terms on the RHS cancel out by Proposition 1. From Assumption 2 we have that $Pr[T = 1|\hat{W}_{max} = \bar{c}_+] = 1$. Then, using the LIE, the RHS of equation (B.4) can be written as:

$$\begin{aligned} E[T(W_{max})\beta|\hat{W}_{max} = \bar{c}_+] &= E[1\beta|T = 1, \hat{W}_{max} = \bar{c}_+] \cdot Pr[T = 1|\hat{W}_{max} = \bar{c}_+] \\ &+ E[0\beta|T = 0, \hat{W}_{max} = \bar{c}_+] \cdot Pr[T = 0|\hat{W}_{max} = \bar{c}_+] \\ &= \beta \\ E[T(W_{max})\beta|\hat{W}_{max} = \bar{c}_-] &= E[1\beta|T = 1, \hat{W}_{max} = \bar{c}_-] \cdot Pr[T = 1|\hat{W}_{max} = \bar{c}_-] \\ &+ E[0\beta|T = 0, \hat{W}_{max} = \bar{c}_-] \cdot Pr[T = 0|\hat{W}_{max} = \bar{c}_-] \\ &= \beta \cdot Pr[T = 1|\hat{W}_{max} = \bar{c}_-] \end{aligned} \tag{B.5}$$

Hence, expression (B.4) simplifies to:

$$E[Y|\hat{W}_{max} = \bar{c}_+] - E[Y|\hat{W}_{max} = \bar{c}_-] = \beta \cdot (1 - Pr[T = 1|\hat{W}_{max} = \bar{c}_-]) \tag{B.6}$$

Rearranging yields:

$$\beta = \frac{E[Y|\hat{W}_{max} = \bar{c}_+] - E[Y|\hat{W}_{max} = \bar{c}_-]}{Pr[T = 0|\hat{W}_{max} = \bar{c}_-]} \tag{B.7}$$

Hence, equation (B.7) suggests that by estimating the numerator there is a potential attenuation bias due to the form of our measurement error. Two points are worth noting. First, the only way to get sharp compliance where $Pr[T = 0|\hat{W}_{max} = \bar{c}_-] = 1$ is by requesting monthly payroll data from every firm, in which case we would observe the true running variable $\hat{W}_{max} = W_{max}$. Second, if we had data from the IRS of withheld and non-withheld workers after the reform we would be able to compute $Pr[T = 0|\hat{W}_{max} = \bar{c}_-]$ allowing us to scale the reduced-form estimate. Unfortunately, the administrative data at hand only allow to estimate the numerator and therefore, if $Pr[T = 0|\hat{W}_{max} = \bar{c}_-] < 1$, we identify an attenuated version of the true causal effect: $\beta \times Pr[T = 0|\bar{c}_-]$.

Discussion

The previous formal derivation suggests that the discontinuity in the probability of paying taxes observed around the cutoff understates the true sharp jump from 0 to 1 by a factor of

$Pr[T = 0|\hat{W}_{max} = \bar{c}_-]$. Intuitively this is the fraction of workers below the threshold that are not misclassified. This is illustrated in Figure A.23 where we simulate the consequences of having non-classical vs classical measurement error in the running variable.

To estimate this bias term we would need to know whether workers are withheld or not after the policy change, which is infeasible due to tax data limitations. Nonetheless, we argue that in practice this issue ought to be small or, in other words, that $Pr[Z = 1|\hat{W}_{max} = \bar{c}_-]$ is likely close to 1 for two reasons. First, our running variable is carefully constructed using monthly data from SICOSS reported by the same accountants that file income tax withholdings through SICORE.[5] Moreover, earnings are reported with some detail allowing us to net out unusual payments such as the 13th salary paid in June, annual bonuses, etc. Second, the results from our case study in which we observe the true running variable reassuringly show that at least for these two firms we are getting the measure 100% right.

The administrative data at hand only allow to estimate the numerator and therefore, if $Pr[T = 0|\bar{c}_-] < 1$, we are identifying an attenuated version of the true causal effect: $\beta \times Pr[T = 0|\bar{c}_-]$. For example, with an estimated reduced-form of 0.02 and $Pr[T = 0|\bar{c}_-] = 0.5$ we would get $\beta = 0.04$ which is still a tiny effect. Conversely, how problematic does measurement error have to be to get to higher values reported in some studies? For example, assuming $\beta = 0.5$ and three different reduced-form estimates (1%, 5%, and 10%) we get:

- $0.5 = 0.01/\text{Pr}(.) \Rightarrow Pr(T = 0|\bar{c}_-) = 2\%$
- $0.5 = 0.05/\text{Pr}(.) \Rightarrow Pr(T = 0|\bar{c}_-) = 10\%$
- $0.5 = 0.10/\text{Pr}(.) \Rightarrow Pr(T = 0|\bar{c}_-) = 20\%$

So, it would require to get the assignment below 15k correctly for only 2%, 10%, 20% of the workers. Our careful construction of the running variable, the two case studies, and anecdotal evidence from accountants suggest that these low values are highly unrealistic. Thus, without loss of generality, it is safe to ignore this source of attenuation bias since it will not change the conclusions from the empirical analysis.

Empirical evidence from two firms

In this subsection we present evidence from two anonymous firms for which we observe the *true running variable* and income tax concepts. By comparing their assignment relative to our *own-derived measure* we confirm that, in practice, the attenuation bias derived from *non-classical* measurement error in the running variable is a second-order issue.[6]

[5] Firms must file a monthly tax return to remit social security contributions through a centralized processing software called SICOSS. In this return they report monthly gross earnings and other related concepts. These are the data that we have at hand. For workers affected by the income tax, firms must also remit income tax withholdings (or refunds) every month through another centralized processing software called SICORE.

[6] We are aware that this conclusion is drawn based on two non-random cases. But the fact that these data come from a medium and large firm, and that their records and processing perfectly match our criteria is indeed reassuring.

The confidential information was provided by two employers in the form of monthly payslips that we digitized with a Python script and were then merged to the SSA administrative data.[7] These granular data contain every positive and negative concept reported in workers' payslips (including income tax withholding) allowing us not only to exactly replicate the running variable used by the firm's accountant but also to observe whether the worker continued paying the income tax or became tax exempt after August 2013.

One of the firms, hereinafter *Firm #1*, is medium-sized with approximately 30 workers and belongs to the wholesale food sector. The other firm, hereinafter *Firm #2*, is large with approximately 700 workers and belongs to the educational services sector. *Firm #1* provided data for the 12 months of 2013, 8 months of 2014, and 1 month of 2016. *Firm #2* provided data for the 12 months of 2013 and 2016. We first present some graphical evidence for *Firm #1* and then proceed to a similar analysis for *Firm #2*.

In Figure A.24, we plot total earnings reported by *Firm #1* to the IRS against the earnings variable that the accountant provided directly to us. Each dot corresponds to an individual-year-month observation. The graph shows that the information from both sources is perfectly aligned with a slope equal to 1. Although reassuring, this is not surprising as earnings and social security contributions reported in payslips is the information that the firm actually files every month to the IRS through form 931, which is the source of the administrative data that we use in the main analysis.

The key advantage of getting access to granular private data is that it contains income tax-related concepts. Figure A.25 presents the number of employees at *Firm #1* that are withheld at source every month. It spans the period before the reform, some months during the tax holiday, and one month after the reform was reversed. From these raw data we can see that the number of workers affected by the tax decreases when the decree is passed, it stabilizes during the tax break, and jumps up again after the new administration repealed the decree. With these data at hand, we proceed to compare the *true* tax status of workers versus our *measured* running variable. In Figure A.26 we report the number of withheld and non-withheld workers before, during, and after the reform for a balanced sample present in the whole period. The three bars to the left correspond to workers below 15k and the three bars to the right correspond to workers above 15k (always using our running variable). On the one hand, seven workers below the discontinuity were positively affected by the reform and four of them were negatively affected when it was repealed. On the other hand, the ten workers above the discontinuity kept paying the tax normally during the whole period.[8]

Figure A.28 is even more transparent as it shows the position of every employee from *Firm #1* along the running variable and whether they were affected by the income tax, both before and after the reform (panels a and b, respectively). By comparing both panels we can see that all the dots below the discontinuity stopped paying the income tax and all the dots above the discontinuity kept paying the tax.[9]

[7]The merge was done by personnel at the Ministry of Labor to preserve the statistical secrecy.

[8]In fact, the tax burden increased for these workers due to a lack of adjustment in nominal exemptions, inflation, and the bracket creep.

[9]The reason why some dots below the threshold are not affected by the tax before the reform is because they

A concern that the reader might have is that there is not enough mass around the discontinuity to judge the likelihood of mismeasurement in the running variable. This is why we went for a "bigger fish" and requested data from a larger firm like *Firm #2*. Since educational services is a sector where workers typically have more than one employer (e.g., a part-time teaching position and a full-time position in another institution) we restrict the analysis to workers with only one job (i.e., permanent workers at the firm).[10] In addition, we restrict the analysis to workers that were withheld at least one month before the reform. With this sensible conditions, our sample goes from 700 to 115 workers. In Figure A.29 we report the number of employees subject to the income tax from January to December 2013 and 2016. About 35 workers stopped paying the income tax after August 2013 and about 20 workers are hit by the tax again when the reform is reversed.[11]

In Figure A.30 we compare the *measured* running variable constructed from our data and the *true* running variable used by *Firm #2*. The graph was done for the 115 workers that were subject to the income tax during 2013. This graph is important as it states that almost all the observations fall on the 45-degree line. There are only 2 cases out of 115 ($\sim 2\%$) in which the value of our running variable is lower than the one used by the firm, but since both values are below 15k it does not lead to a misclassification of workers in terms of their tax status.

In Figure A.31 we zoom in on our *measured* running variable at 10k-20k and, using tax data from the firm, we compare what happened to workers below and above 15k before and after the decree was passed (panels a and b, respectively). Similar to Figure A.28, all the workers below the discontinuity became tax exempt and all the workers above the discontinuity kept paying the tax normally. This figure is more convincing because there is sufficient mass around the threshold.

Finally, the larger sample size of *Firm #2* also allow to gauge the magnitude of the first stage change in tax withholdings. In Figure A.32 we plot the average tax rate (ATR) against our *measured* running variable right before and right after the reform is put in place (panel a) and right before and right after the reform is repealed (panel b). While the ATR goes from about 6% to 0% for workers right below the cutoff, it increases substantially to about 12% for workers above the cutoff due to the lack of adjustment in nominal exemptions, inflation, and the bracket creep. When the reform is reversed, the ATR increases (decreases) to the pre-reform level for workers below (above) the threshold.

Taken together, the evidence presented in this subsection suggests that, in practice, *non-*

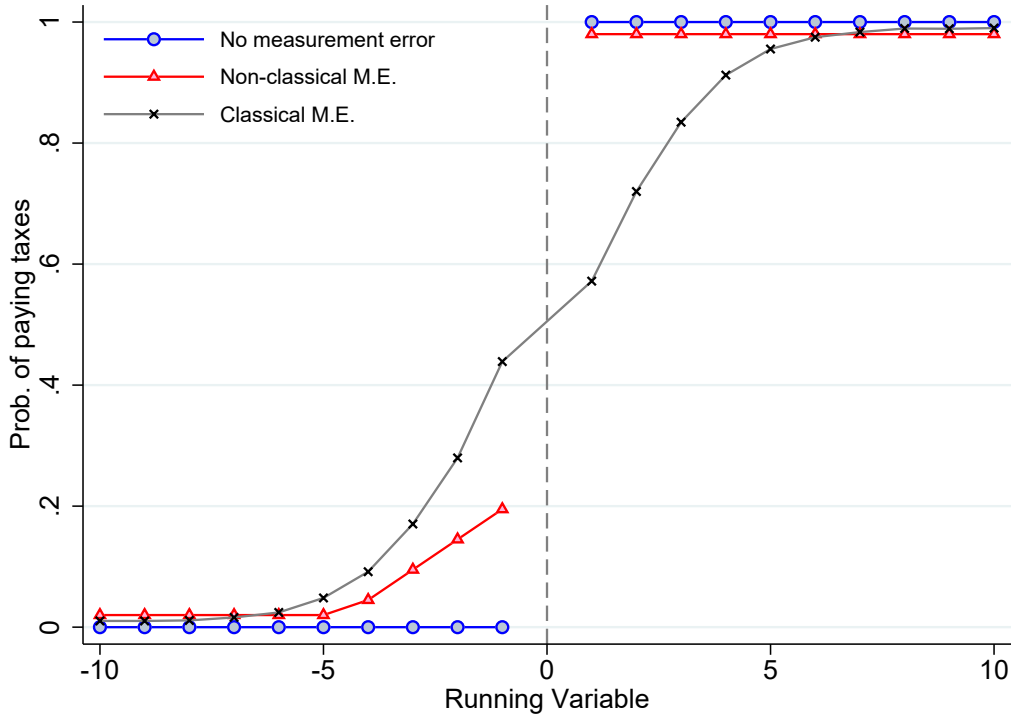
have a higher family size and thus can subtract more personal exemptions from the tax base.

[10] Recall that in the case of workers with multiple jobs, the employer in charge of withholding the income tax for the income earned in all the jobs, is the one paying the highest salary. Hence, for the purpose of the exercise it is necessary to restrict to single-job workers (about 40 percent of the firm). These mostly include faculty and administrative employees (52 and 38 percent, respectively).

[11] The drop in March 2013 is explained by a another decree passed by the president that updated the nominal value of personal exemptions. This was the standard tool used by the government every other year to avoid a massive bracket creep. The time series quickly goes up again due to inflation and wage negotiations celebrated during March-May.

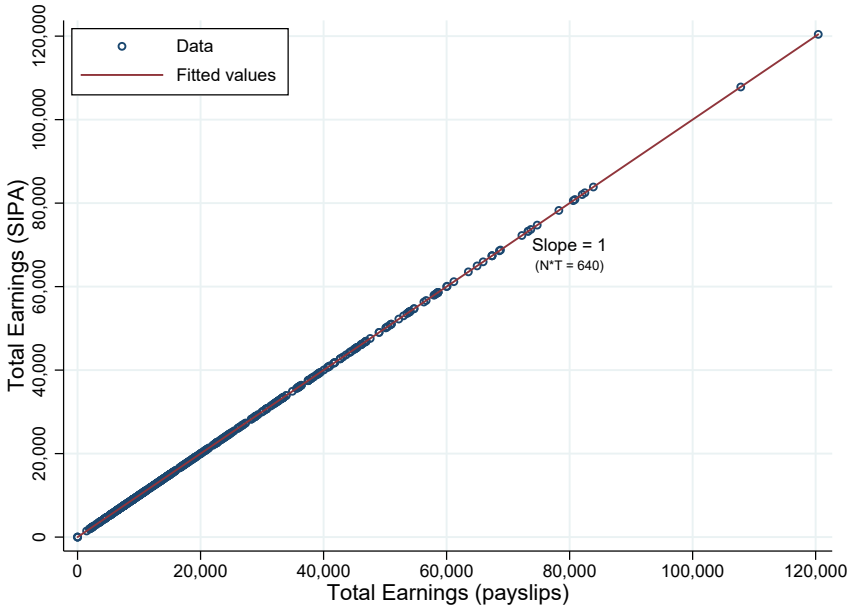
classical measurement error in the running variable appears small. Thus, we believe that any attenuation bias arising from it is presumably a second-order issue and does not pose a threat to the main empirical findings of the paper.

Figure A.23: Simulation: measurement error in the assignment variable



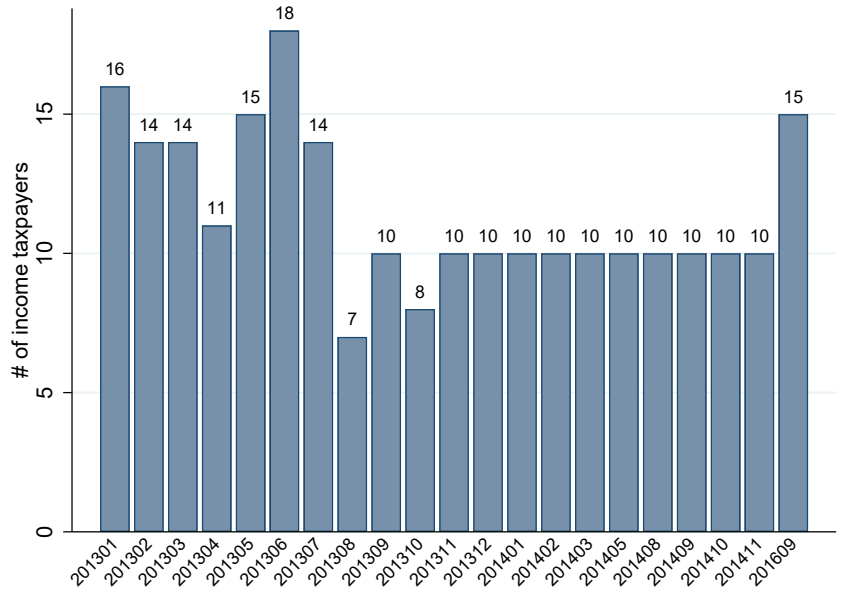
Notes: this simulation provides a visual illustration of how measurement error in the running variable affects the first stage of the RDD. The figure displays three cases: (1) under *no measurement error* in the assignment variable (blue line), workers above the threshold keep paying the tax normally and workers below the threshold become tax exempt. Therefore, there is a sharp jump from 0 to 1 at the discontinuity; (2) under *non-classical measurement error* (red line), workers whose highest salary was below the threshold are very likely to become tax exempt, but some workers receiving unusual payments could be put incorrectly above the threshold introducing some fuzziness in the design. The first stage jump still exists but is attenuated; (3) under *classical measurement error* (black line), both workers to the left and to the right of the threshold could be incorrectly misclassified killing the first stage. The sample is set to 100,000 observations. For classical measurement error, the running variable is $X = X^* + u$ where $X^* \sim U[-10, 10]$ and $u \sim N(0, 3)$. The true sharp treatment is defined as $T = 1[X^* > 0]$. The treatment for the non-classical case is defined as $T = (0.2 + 0.05X^*)$ if $X^* \leq 0$ and $T = 1$ if $X^* > 0$ so that the size of the first-stage jump is 0.8.

Figure A.24: Quality of the data: IRS vs *Firm #1*



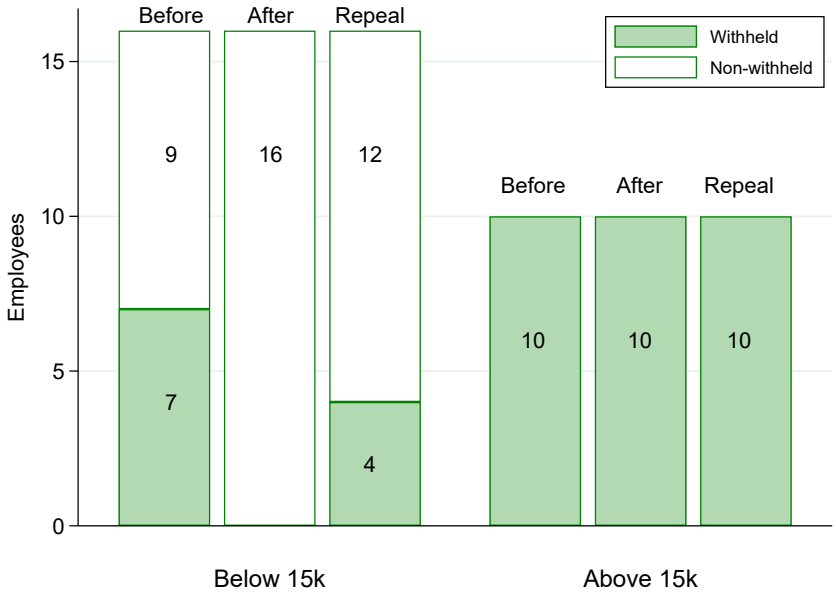
Notes: this figure plots total earnings reported by *Firm #1* to the IRS (vertical axis) against the sum of earnings from the payslips provided by the firm (horizontal axis). Each dot corresponds to an worker-year-month observation. The graph shows that the information from both sources is perfectly aligned.

Figure A.25: Withheld workers per month (*Firm #1*)



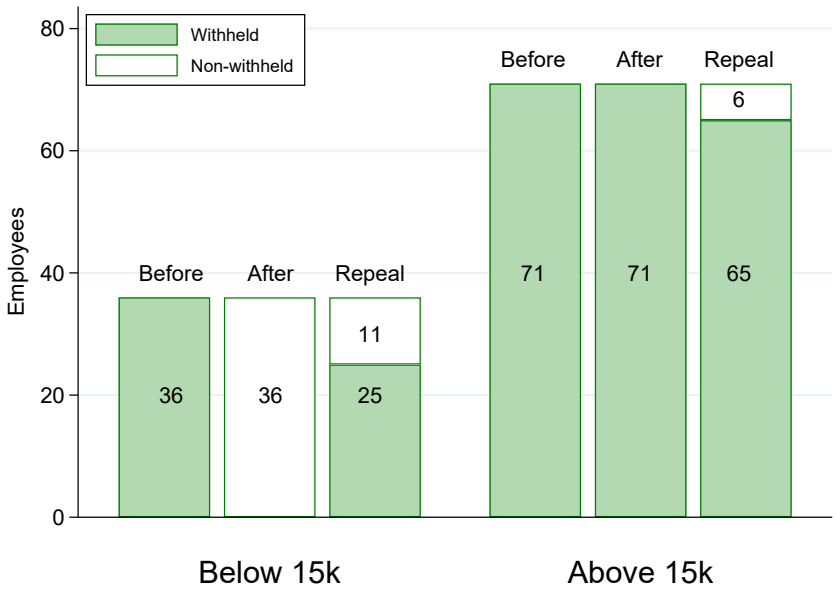
Notes: this figure presents the number of employees that are withheld at source every month. It spans the period before the reform, some months during the tax holiday, and one month after the repeal.

Figure A.26: Withheld workers pre/post/repeal (*Firm #1*)



Notes: this figure reports the number of withheld and non-withheld workers before, during, and after the reform for a balanced panel of workers present in the whole period. The 3 bars to the left correspond to workers below 15k and the 3 bars to the right correspond to workers above 15k. In both cases we use our *measured* running variable based on IRS data and the tax status reported by *Firm #1*. 7 workers below the discontinuity became tax exempt and 4 of them were hit by the tax again when it was repealed. The 10 workers above the discontinuity kept paying the tax normally during the whole period.

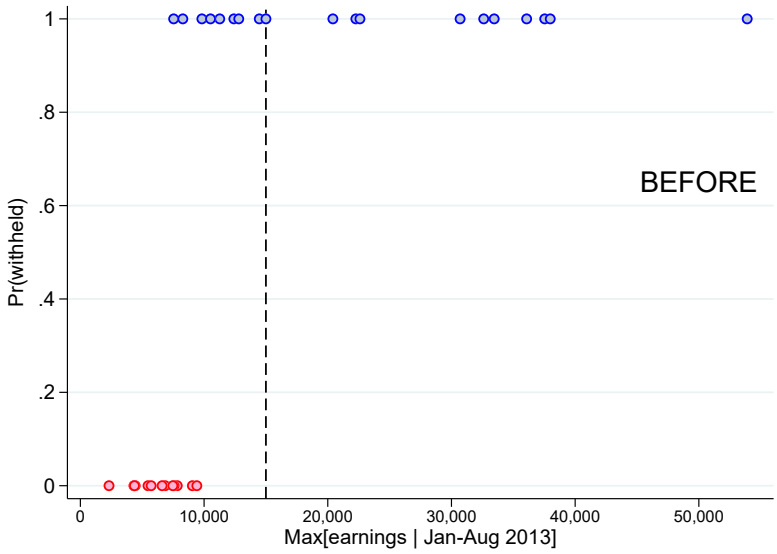
Figure A.27: Withheld workers pre/post/repeal (*Firm #2*)



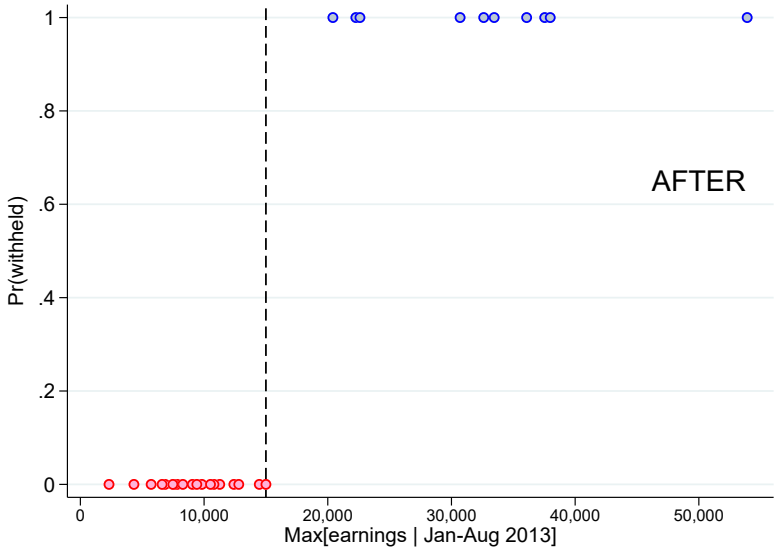
Notes: this figure is the equivalent to Figure A.26 but for *Firm #2*. 36 workers below the discontinuity became untaxed in 2013 and 25 of them were hit by the tax when it was repealed in 2016. The 71 workers above the discontinuity kept paying the tax normally during the whole period.

Figure A.28: Withheld workers pre/post reform (*Firm #1*)

(a) $Prob(withheld = 1|pre)$

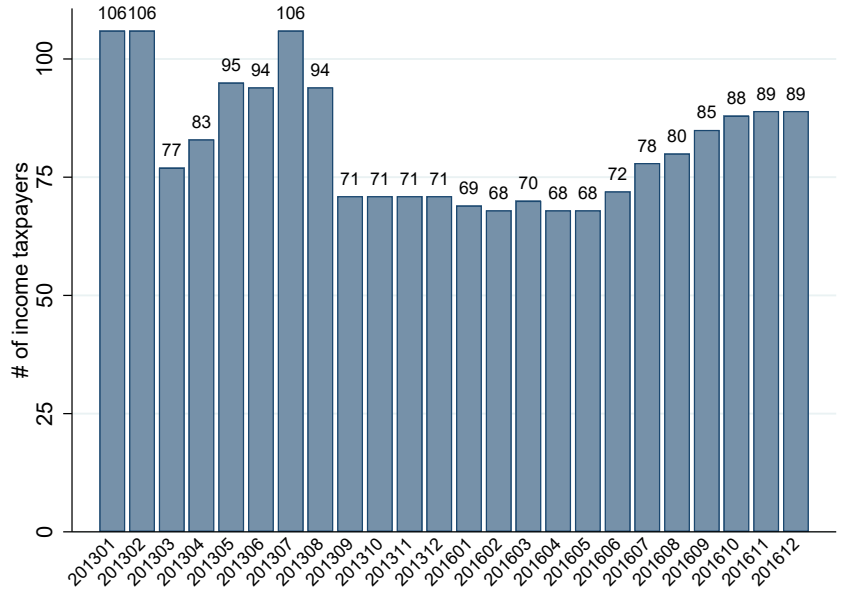


(b) $Prob(withheld = 1|post)$



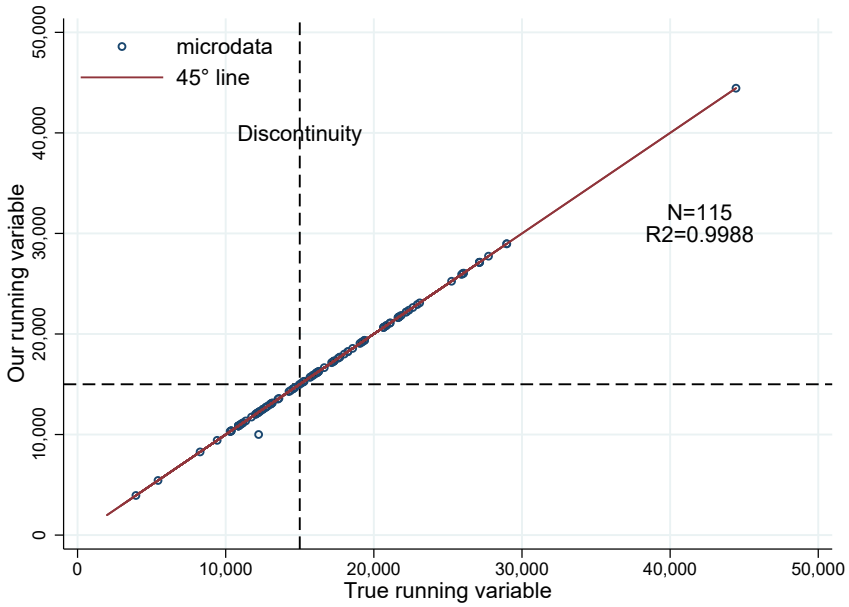
Notes: this figure shows the position of every individual on the running variable (horizontal axis) and the probability of paying the income tax (vertical axis) before and after the reform. Panel (a) considers any month between January and August 2013, and panel (b) considers any month between September 2013 and December 2014. To maximize the mass the graph was done for the unbalanced sample of workers.

Figure A.29: Withheld workers per month (*Firm #2*)



Notes: this figure presents the number of employees that are withheld at source from January to December 2013 and from January to December 2016. The drop in March 2013 is explained by a another decree passed by the president that updated the nominal value of personal exemptions.

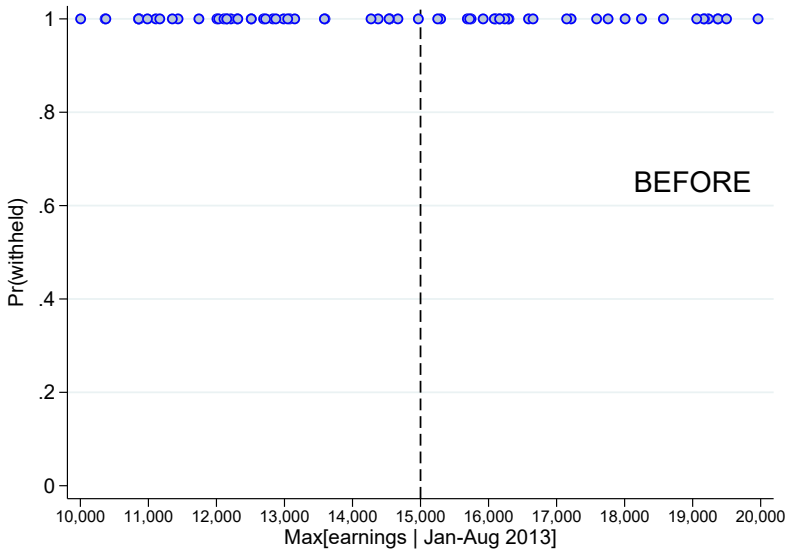
Figure A.30: OWN vs TRUE running variable (*Firm #2*)



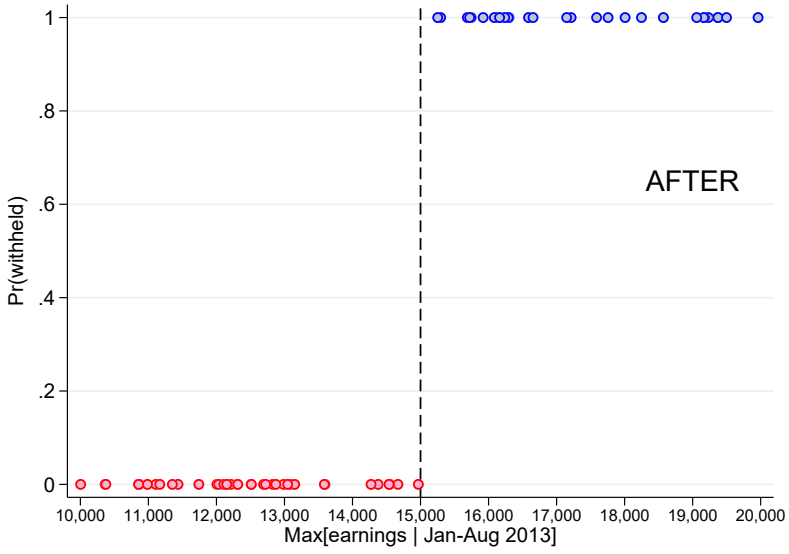
Notes: this figure compares our *measured* running variable (vertical axis) and the *true* running variable used by *Firm #2* (horizontal axis). The sample includes 115 workers that were subject to the income tax during 2013. This graph is important as it states that almost all the observations fall on the 45-degree line, so that measurement error is close to zero.

Figure A.31: Withheld workers pre/post reform (*Firm #2*)

(a) $Prob(withheld = 1|July, August)$



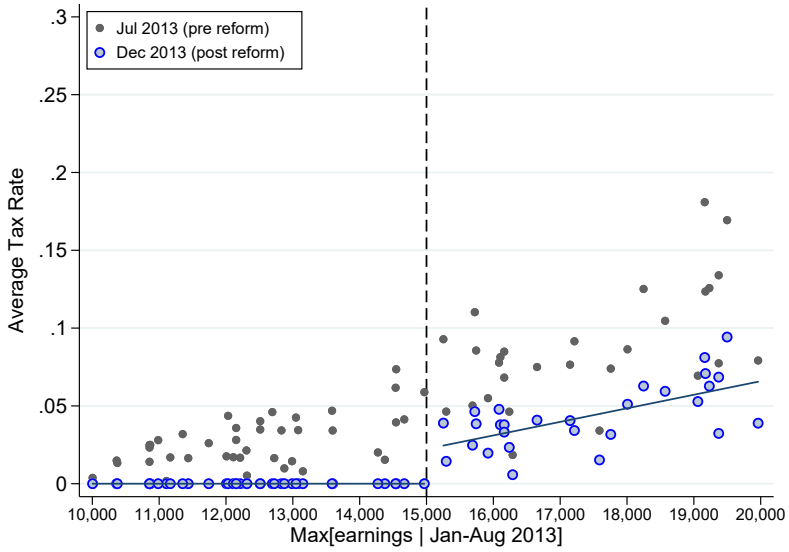
(b) $Prob(withheld = 1|September, October)$



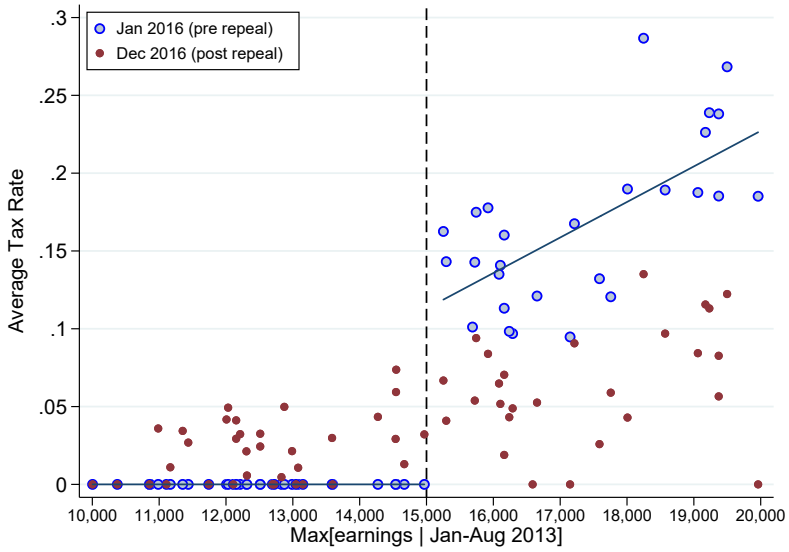
Notes: this figure shows the position of every individual on the running variable (horizontal axis) and the probability of paying the income tax (vertical axis) before and after the reform. It uses our *measured* running variable and the income tax status as reported by the firm. Panel (a) is computed for the months of July and August 2013, and panel (b) is computed for the months of September and October 2013.

Figure A.32: Evolution of the Average Tax Rate (*Firm #2*)

(a) ATR before and after August 2013 (reform)



(b) ATR before and after February 2016 (repeal)



Notes: this figure plots the average tax rate (vertical axis) against the running variable (horizontal axis) before, during, and after the reform. Each dot corresponds to a worker. It uses our *measured* running variable and the income tax withholding as reported by the firm. Panel (a) is computed for the months of July and December 2013, and panel (b) is computed for the months of January and December 2016. The gray dots correspond to pre-reform period, the blue dots show data during the reform, and the red dots correspond to post-repeal period.

A.3 A graphic representation in the static model

The static model of labor supply can be viewed as a special case of a dynamic model where all intertemporal linkages do exist, but where workers are myopic and ignore them when deciding on current labor supply. We present a simple graphical framework to understand the predictions that the reform has on the labor supply of workers. Monthly wage earnings z are defined as posted earnings before employee's payroll and income taxes. Net earnings c are defined as earnings after taxes (i.e., take-home pay). Earnings include several concepts such as base pay, overtime pay, seniority, bonuses, vacation pay, 13th-month salary, etc.

In Figure A.33 we depict the effect of the reform on the individual budget set and utility maximizing choices in the consumption-earnings space for a frictionless labor market. Utility increases with disposable income c (as disposable income funds consumption) and decreases with z (as labor supply is costly). To simplify the analysis, we focus on a single worker with no children.[12] Before the reform, a worker with these characteristics and gross monthly earnings greater than AR$ 8,360 was subject to the income tax.[13] This first kink is shown in the figure at $8.3k$. Without loss of generality, we also assume that the first tax bracket goes beyond the $15k$ cutoff.[14]

Figure A.33 panel (a) shows the predicted effects of the reform for individuals whose highest gross monthly salary accrued between January and August 2013 was less than AR$15,000 (group 1). These wage earners were fully exempt from the income tax from September 2013 onwards, regardless of subsequent earnings. Along the intensive margin, workers below $8.3k$ were not paying income taxes before the reform and thus are unaffected. Workers with pre-reform earnings between $8.3k$ and $15k$ experience a decrease in marginal income tax rates from $\tau > 0$ to $\tau = 0$ so that their net-of-income-tax rate increases from $1 - \tau$ to 1. Their budget set shifts upwards from the black solid line to the blue solid line. This shift creates a substitution and an income effect.

The substitution effect pushes individuals to work more hours increasing wage earnings. Intuitively, individuals have incentives to work more hours, accept promotions, or switch to higher paying jobs, because they can keep the full pay (net of payroll taxes). However, holding everything else constant, workers maximizing utility in $z \in (8.3k, 15k]$ will get a higher take-home pay now and, therefore, the income effect will push them to work less hours reducing wage earnings. In this case, a worker maximizing utility at point 1 could end up in points like 2, 3, or 4. Thus, the effect of the tax break on earnings for this group of workers is ambiguous. Finally, note that workers bunching at the first kink $8.3k$ (i.e. maximizing at point 5) experience a substitution effect that will push them to work more hours (or report higher earnings). This implies that after the reform we should expect

[12] In section 4.2 below we show that is the group that faced the largest incentive to adjust their labor supply when the reform entered into force.

[13] The minimum non taxable income for a married worker with two children was AR$ 11,563 right before the reform.

[14] In Figure A.8 below we overlap the tax schedule and corresponding marginal tax rates to the distribution of gross earnings in 2013.

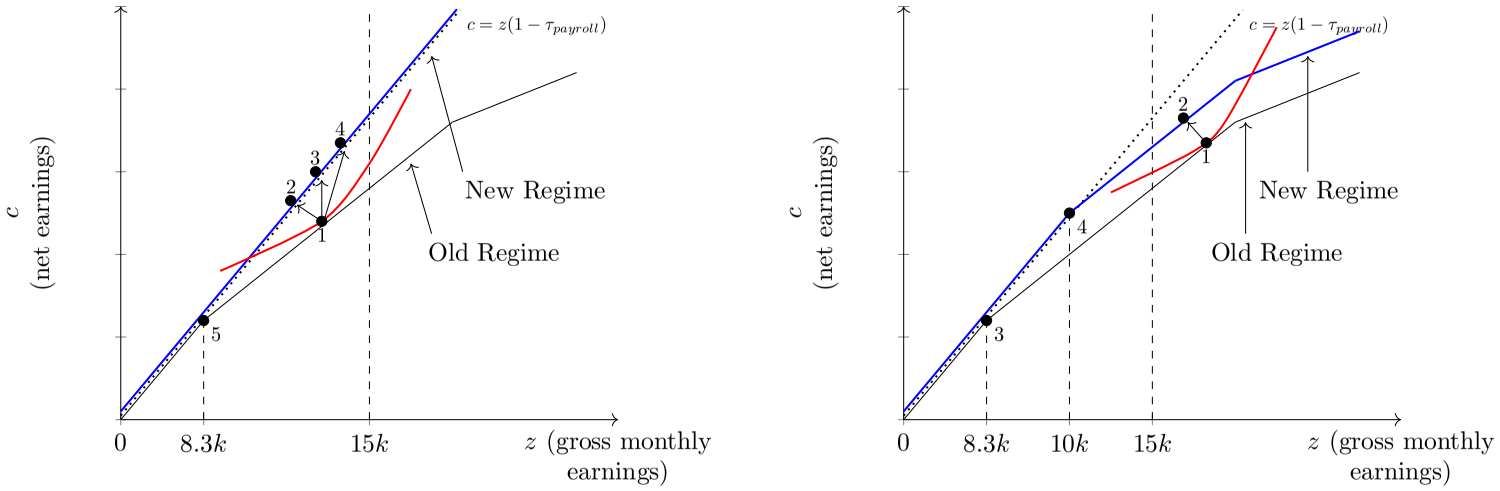
bunching at the first kink (if any) to decrease substantially.

Figure A.33 panel (b) shows the predicted effects of the reform for individuals whose highest gross monthly wage accrued between January and August 2013 was between AR$15,001 and AR$25,000 (group 2). In this case, the reform increased the minimum non taxable income 20 percent from $8.3k$ to $10k$, hence shifting outward the first kink point in the budget set.[15] Workers with pre-reform earnings between $15k$ and $25k$ experience no changes in marginal income tax rates and therefore the substitution effect is zero. However, holding everything else constant, workers maximizing utility in $z \in (15k, 25k]$ will get a higher take-home pay now. Thus, the income effect predicts a reduction in hours of work and hence gross earnings. For example, a worker maximizing utility at point 1 would go to a point like 2. Finally, note that the first kink moved from $8.3k$ to $10k$ (point 3 to 4). However, this change should not matter for the analysis as, by definition, these workers were already making more than $15k$ before the reform.

Finally, workers whose highest gross monthly wage accrued between January and August 2013 was greater than AR$25,000 continued paying taxes based on the black solid line (group 3). In practice, however, group 2 and 3 experienced an increase in marginal and average tax rates due to inflation and the "bracket creep". In this case, the substitution effect will reduce hours of work and hence gross earnings. But income effect will make them work more hours. In the case of group 3, most of these workers were already facing the top 35% marginal tax rate and, thus, experience a pure income effect.

[15] The 20 percent increase in personal exemptions corresponds to deductions for spouse, children, non-taxable income, and a special deduction for wage earners.

Figure A.33: Conceptual Framework. Single worker without children



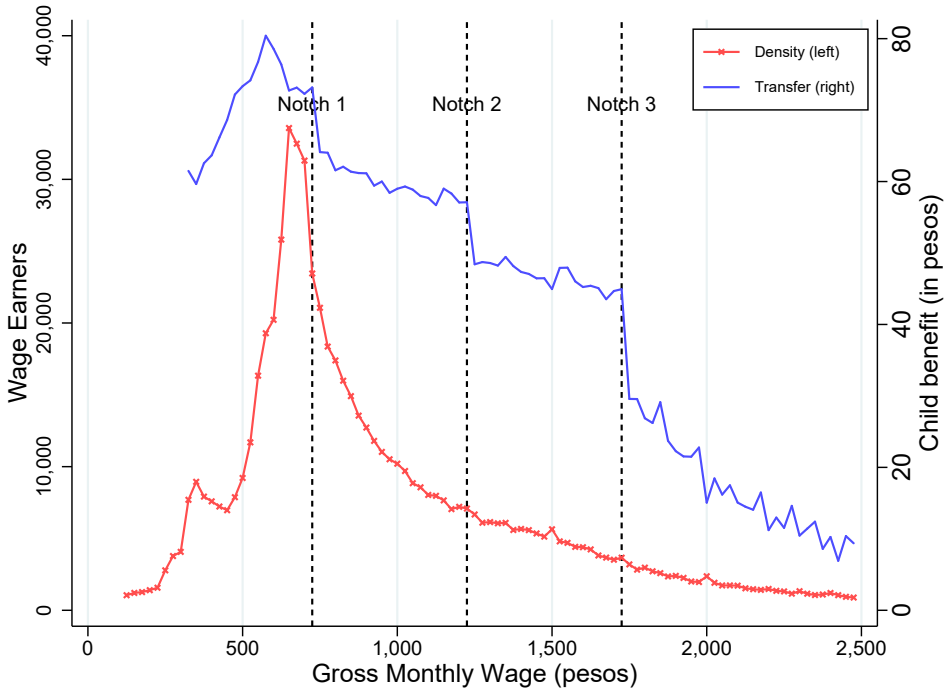
(a) **Case 1**: $\max\{earnings|\text{Jan to Aug 2013}\} \leq 15k$ (b) **Case 2**: $15k < \max\{earnings|\text{Jan to Aug 2013}\} \leq 25k$

Notes: The figure displays the effects of the 2013 income tax change on the monthly budget constraint of single workers with no children. This static framework can be viewed as a special case of the dynamic model, where all intertemporal linkages exist, but workers are myopic and ignore them when deciding on current labor supply. The x-axis represents gross monthly earnings (including employee's payroll taxes). The y-axis represents net monthly earnings (earnings net of both payroll and income taxes). The black solid line is the old regime budget and the blue solid line is the new regime budget. Panel (a) shows the effect of the reform for workers that were fully exempt. Panel (b) shows the effect of the reform for workers that received a 20% increase in personal exemptions. The first kink denoted by $8.3k$ corresponds to AR$ 8,360 as of August 2013. The kink denoted by $10k$ corresponds to AR$ 10,032 starting in September 2013.

Appendix B

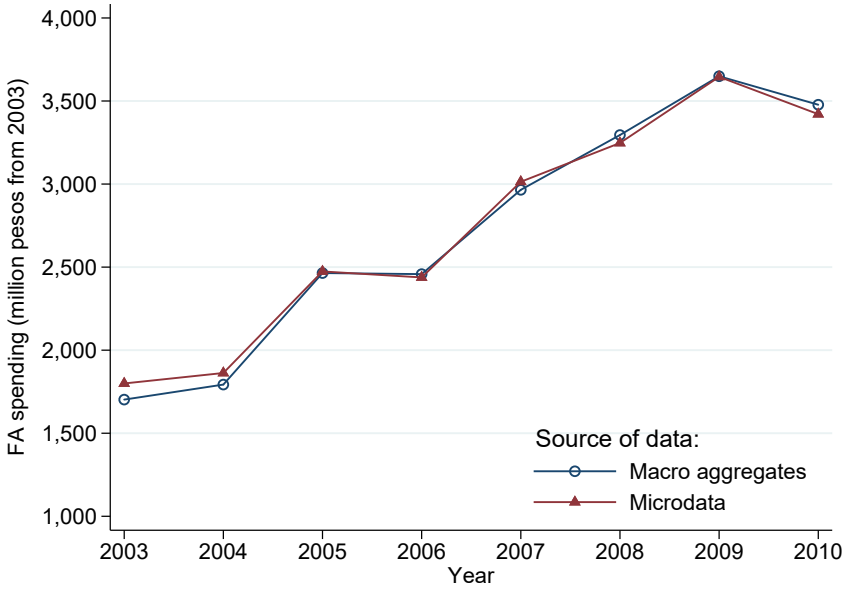
B.1 Appendix Figures and Tables from Chapter 2

Figure B.1: Distribution of monthly wages (May 2004)



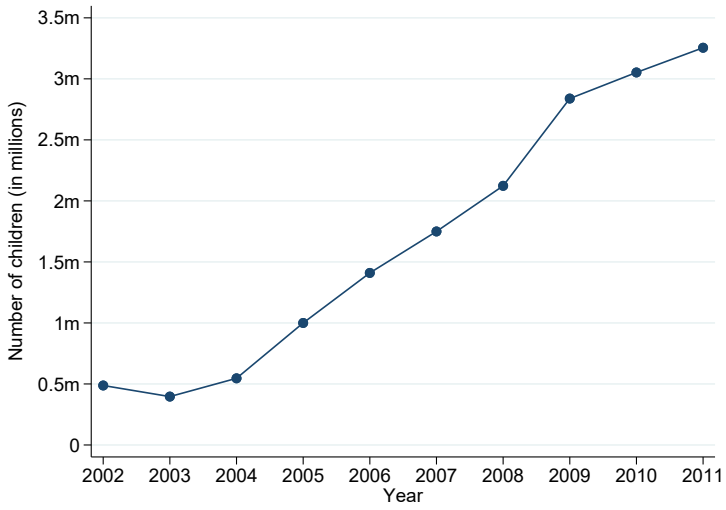
Notes: this figure shows the distribution of monthly wages (orange line) and the average transfer (blue line) for a balanced panel of employees with children working during the twelve months of 2004. Notch 1 is located at percentile 40; Notch 2 is located at percentile 70; and Notch 3 is located at percentile 80. The monthly minimum wage was 350 Argentine pesos in May 2004. The figure suggests that there is not bunching of workers at these notches which rules out labor supply responses or strategic collusion between employers and employees.

Figure B.2: Macro and micro aggregates comparison



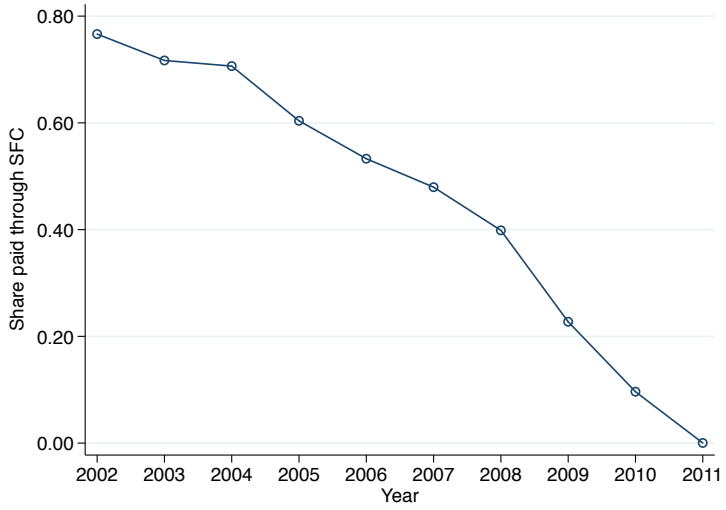
Notes: this figure shows the total expenditure on family allowances in real terms (old and new system). The blue connected dots present the macro total available in official budget information (data extracted from *Cuenta de Inversion, Contaduría General de la Nacíon* and *Informe Gerencial* (AFIP))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

Figure B.3: 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 IRS.

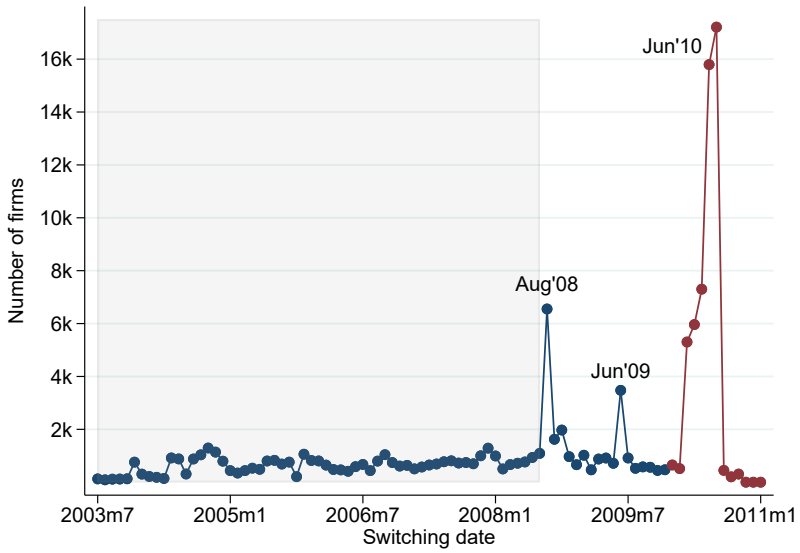
Figure B.4: Macro roll-out (official budget information)



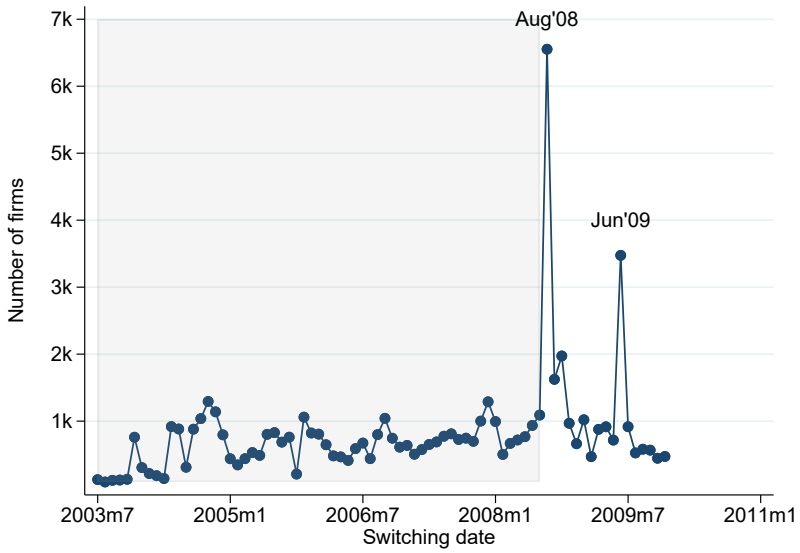
Notes: this figure shows the share of family allowances paid under the old system (SFC). The aggregate expenditure on family allowances is take from official budget information (*Cuenta de Inversion, Contaduría General de la Nacíon* and *Informe Gerencial* (AFIP)). The gradual decline in this share illustrates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

Figure B.5: Event frequencies per month-year (number of firms)

(a) **Full period 2003-2010**

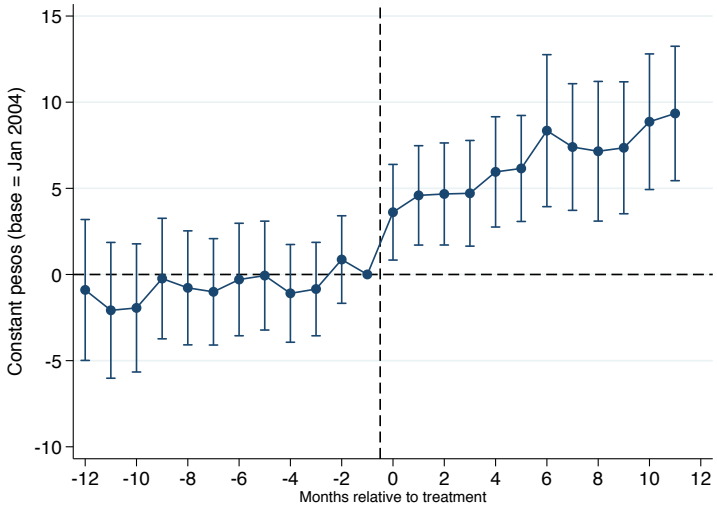


(b) **Zoom in before 2010**



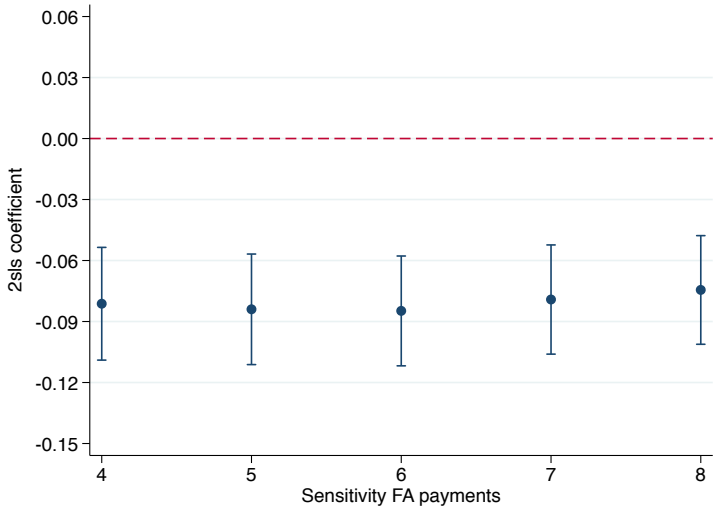
Notes: this figure shows the number of firms switching to the new system at each month-year of our micro-data. Panel (a) shows the full period from 2003 to 2010 and panel (b) restricts the graph to pre-2010 data to get a clearer picture. The spikes correspond to three massive incorporation dates: August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

Figure B.6: Balanced panel



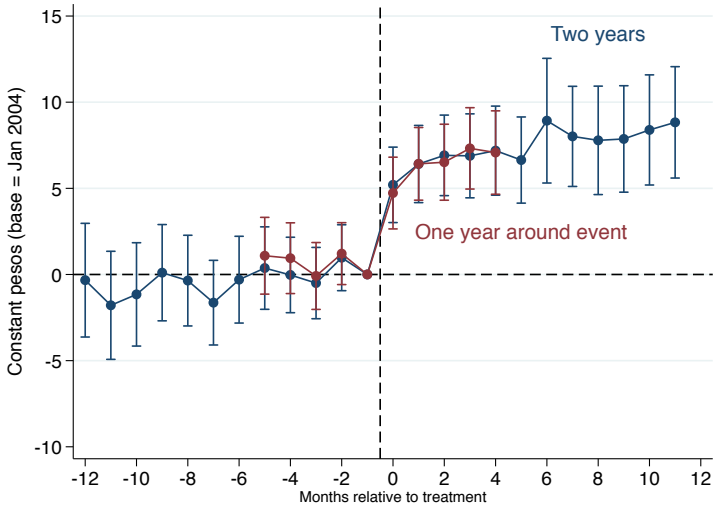
Notes: see the details in Figure B.10. This figure shows that results remain unchanged when considering a balanced panel of firms present in the 96 months of data.

Figure B.7: Sensitivity to months of transfer payments (2sls)



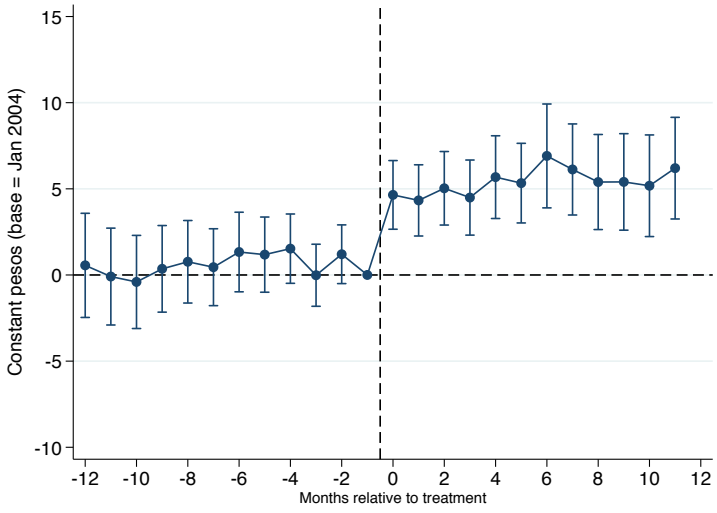
Notes: each dot of this regression corresponds to a different 2sls regression where we vary the sample of firms according to the number of months that each firm was paying family allowances (FA) right before the event. We consider 4, 5, 6, 7, and 8 months. The result is very stable across specifications.

Figure B.8: Sensitivity to event window range



Notes: see the details in Figure B.10. This figure shows that results remain unchanged when considering a time window of 6 months before/after instead of 12 months.

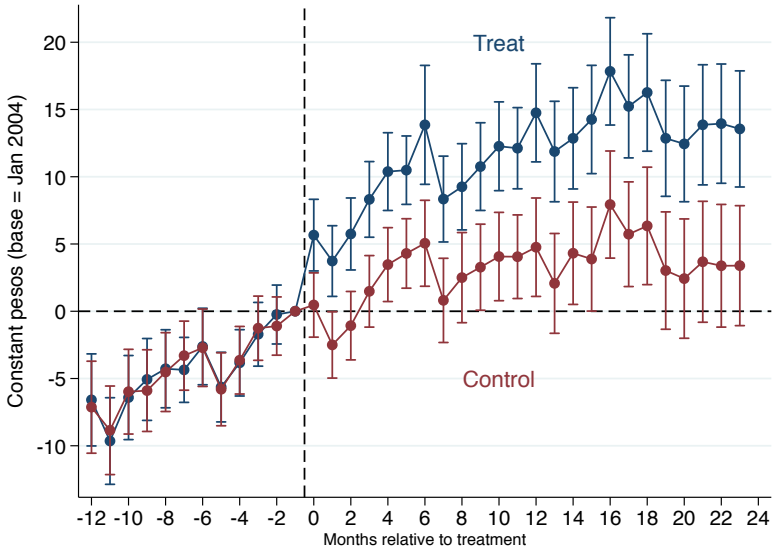
Figure B.9: Alternative treatment group definition



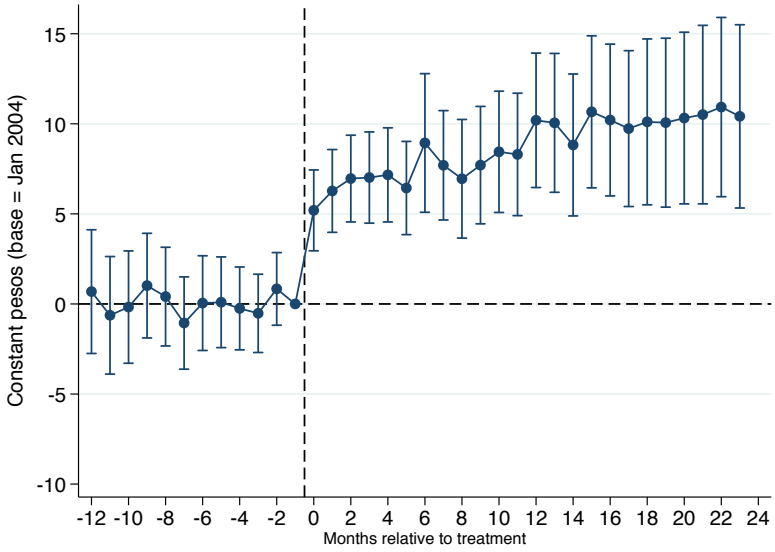
Notes: see the details in Figure B.10. This figure shows that results remain unchanged when using an alternative definition of the treatment group that considers workers that are fully treated during the period 2003-2010 (i.e., those with children ages less than 18 years old during the whole roll-out period).

Figure B.10: Reduced-form wage effects (long run)

(a) Average wage levels



(b) Average wage gap



Notes: these figures plot the event-study estimates of the parameter γ and its corresponding 95 percent confidence intervals of equation (2.2). Panel (a) shows the wage levels when we estimate this equation separately for workers with children (treat) 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 10 pesos when firms stop disbursing the transfer to eligible workers and it stabilizes after 12 months.

Table B.1: Monthly transfer by income bracket (1996-2010)

Year	Effective date M/D/Y		Law	Monthly Gross E.		Child Transfer
	Start	End		≥	≤	
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

Note: Own elaboration based on official documents. The last three columns are expressed in current Argentinian pesos.

Table B.2: Robustness exercises

	(1)	(2)	(3)
Reduced Form			
Δ monthly wage	6.94***	7.71***	5.40***
(in pesos)	(0.90)	(1.25)	(1.27)
2sls			
$\frac{\Delta wage}{\Delta transfer(\tau^e)}$	-0.08***	-0.08***	-0.06***
	(0.01)	(0.01)	(0.01)
Simple mean difference	✓		
Firm and time FE		✓	✓
Firm linear trend			✓
Observations	3,061,870	3,061,870	3,061,870

Note: Standard errors clustered at the firm level in parentheses.

B.2 Family Allowances in Argentina

Table B.1 provides a complete picture of this scheme including the evolution of the brackets and the exact transfer amount per child. As it is possible to see from the table, the amounts are adjusted semi-annually. The average tax credit rate for the lowest category is on average 7%,[1] and in the micro-data presented later in this paper, we observe that on average each claimant does so for two children (therefore final ATR is double). In 2010, roughly 1.5 million registered workers received a total of $10 billion in AAFF payments. The 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.[2] [3]

The AAFF is an "individually-based" scheme that considers individual earnings to determine the bracket and transfer amount. Only one of the parents or guardian, 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 can (conditional on being a formal employee and with gross wage earnings below the upper threshold).[4] Since 2012, the

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

[2] 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.

[3] To avoid any potential gaming behavior to 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.

[4] When a certain worker has more than one job, she is entitled to receive the family allowances benefits in

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]

Besides the AAFF program mentioned above, the Argentinian government also 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 of 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 receive at least a certain amount of aid from the government. Second, Argentina also introduced a universal child credit (the Universal Childhood Allowance, AUH for its acronym in Spanish) in November 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 current 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 implicit 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 recipient's, on the contrary, receive 80% of the transfer each month and the rest is disbursed at the end of the year when conditionalities are confirmed. In a context of high inflation, where the purchasing power of money is eroded quite fast, this can make a big difference. Final remark, if both spouses file personal income tax, they can both deduct 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 a poorer one.

B.3 Incorporation process

The way firms were gradually incorporated into the SUAF can be summarized as follows; we illustrate this process in chart B.11 below. The first step consisted on ANSES publishing various resolutions that established that firms will be gradually incorporated and, mandatory

only one of them, the one with the highest seniority.

[5] See Decree 1667/2012.

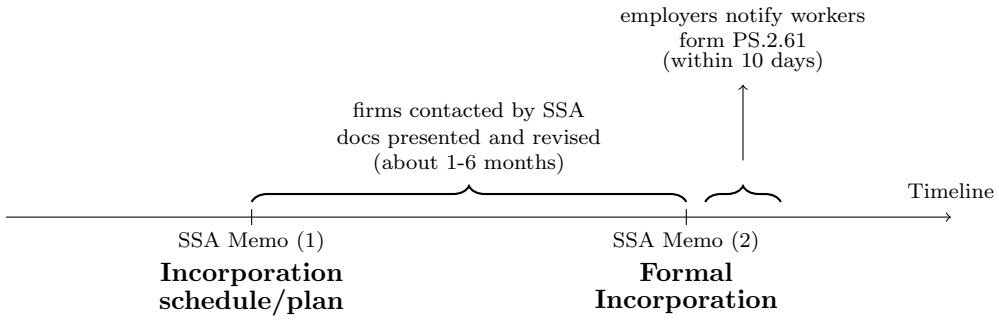
[6] In principle, this change could improve the targeting of the scheme. However, it may also impose some costs to secondary earners within the household, typically female, given that they face a higher marginal tax rate with a potential concern on labor supply. This is an interesting reform for future research.

[7] This type of programs are known as Conditional Cash Transfer (CCT) and have been gradually introduced across all Latin American countries after the famous Mexican 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.

included, 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 accordingly about the different documents that they had to present. Concretely, the formalization process required that each employer had to hand in a set of specific documents and paperwork including the form F.560. These documents were supposed to be presented either at ANSES headquarter's office or in a subsidiary, *Unidad de Atención Integral* (UDAI).[9] Figure B.12 presents an example of a memo. The top panel contains the body of the memo 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 annex (with firm identifiers).

Figure B.11: Firms' incorporation steps into the new payment system



Source: Own elaboration based on official documentation.

The second step consisted on, as stated above, the different firms presenting the whole documentation. In general, it took three months and a half between the moment where the firm was notified and the presentation of the documentation. The third, and the last one, consisted on the final approval or *effective* incorporation into the system, which in most cases took approximately 50 days after step two was completed.[10] The approval was materialized in another memo where ANSES established the date in which each firm would be formally included in SUAF and up to when it can *compensate* the family allowances paid under the old system.[11]

Figure B.13 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

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

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

[11]The term *compensate* refers to the possibility that firms had where they could deduct the transfer from employer SSC. The last month to *compensate* a payment was the month before the formal incorporation date. The idea behind this was to avoid duplicates payments i.e., both, a payment under the SFC and under the SUAF, for a given month.

the memo involves several employers, it contains an annex with the list of them (as it can be seen in the bottom panel).[12]

Employers were also able to search through a public website whether the firm was in fact under the new scheme and, if so, the starting date. This is shown in Figure B.14. To do a query it is necessary to introduce 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*).[13]

The last point in the chart B.11 refers to firms' observed responses in the micro-data i.e., the first month where we observe an interruption of family allowance payments under SFC. As it was already explained in the body of the paper, we define an event date as the moment where we identify that in the micro-data a given firm stops paying under the old payment mechanism. We then check whether the different administrative dates (schedule and formal incorporation) correlates with what we observe at the micro level.

[12]Note that the third column contains that name of the UDAI i.e., where the documentation was handed in.

[13]We manually checked whether the date that appeared in the memo coincides with that one in the website and in nearly all the cases it coincides.

Figure B.12: 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 1º— 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 2º— La Gerencia de Prestaciones notificará fehacientemente a los empleadores referenciados en el artículo 1º 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 annex (with employer's identifiers)

ANSES 333 "2005 – AÑO DE HOMENAJE A ANTONIO BERNI"

FOLIO 5

Ministerio de Trabajo, Empleo y Seguridad Social

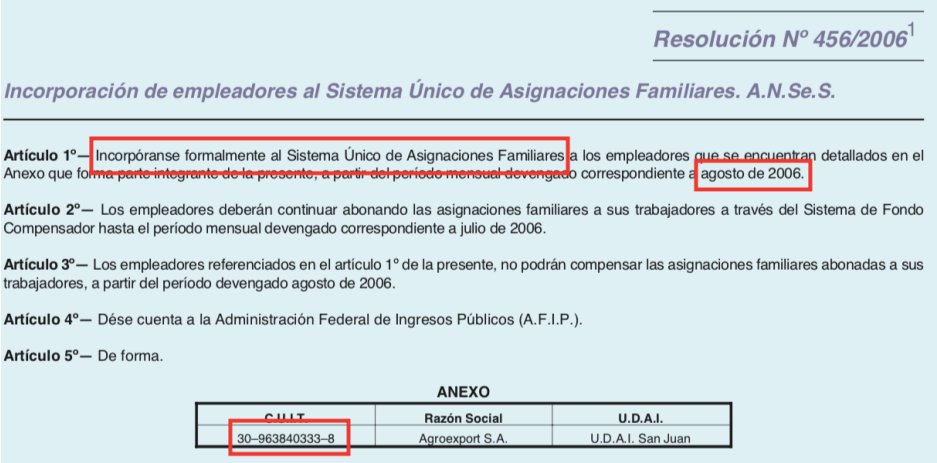
ANEXO

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 annex 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 the documentation that they have to hand in. Panel (b) shows the annex of resolution $N°333/2005$. As it is possible to see, on the left column the resolution lists the taxpayer identifier, while on the second column the name of the employer/firm.

Figure B.13: 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 1º— 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 2º— 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 3º— Los empleadores referenciados en el artículo 1º de la presente, no podrán compensar las asignaciones familiares abonadas a sus trabajadores, a partir del período devengado agosto de 2006.

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

Artículo 5º— De forma.

ANEXO

C.U.I.T.	Razón Social	U.D.A.I.
30-963840333-8	Agroexport S.A.	U.D.A.I. San Juan

(b) Resolution annex (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 is referring to. Panel (b) contains the list of employers contains the annex (for those cases where several employers are referred in the body of the resolution.

Figure B.14: 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 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*). The official website can be accessed at ANSES website.

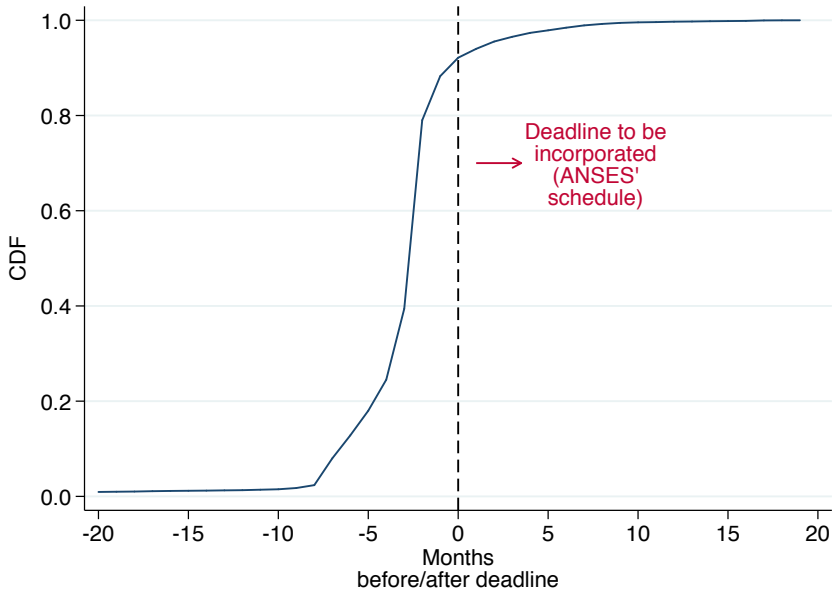
To do that we digitized all internal schedules that we were able to find in ANSES webpage (more than the 50 annexes). We end up with approximately 63,000 firm identifiers with its corresponding final schedule deadline.[14] As far as we know, the date worked as an internal due date to commit to the gradual incorporation process rather than a deadline imposed to firms. We combine these dates with the event dates constructed from micro-data. Figure B.15 panel (a) presents the cumulative density function of firms incorporated into the new system as a function to the distance (in months) to the deadline established in the schedule memo. More than 90 percent of the employers were incorporated before the deadline.

We then looked at the effective incorporation date (listed in the incorporation resolution) and its correspondence with the micro-data. As opposed to the schedule memo, it is quite hard to track the incorporation memo basically because there were various hundreds of them and they are not systematized at all. However, we exploit the public website and recover the formal incorporation date for a random sample of firms. Panel (b) of B.15, presents the correlation between the formal incorporation date and the one derived from the micro-data. We find that in more than 80 percent of the cases both *event* dates coincide.

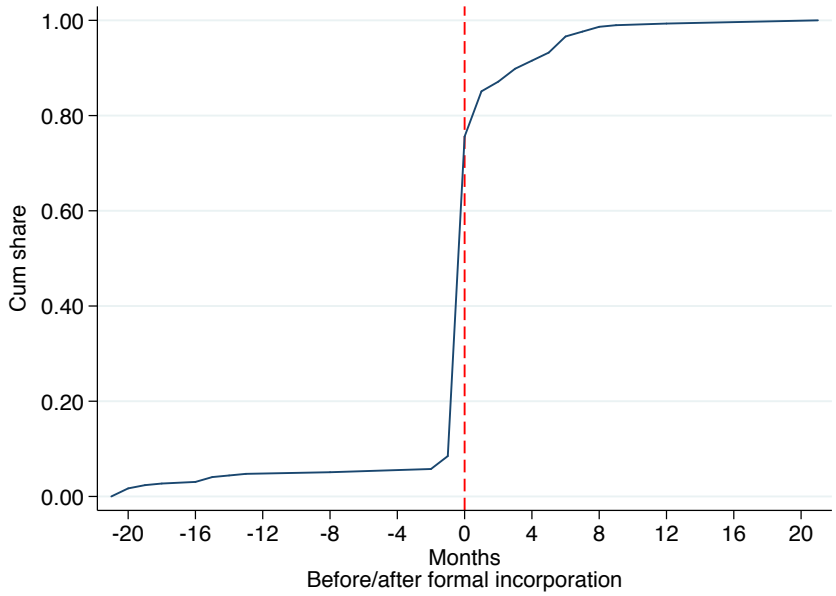
[14] We found that only 0.001 percent of the employers appeared in more that one resolution.

Figure B.15: Dates correspondence

(a) **Schedule vs observed incorporation date**



(b) **Formal vs observed incorporation date**



Notes: Panel (a) contains the CDF of firms incorporated into the new system as a function to the distance (in months) to the deadline established in the schedule memo. (b) Presents the correlation between the *event* date defined using the micro-data and the *event* date that results from public queries (effective incorporation into the new system). It is based on a random sample of 300 CUITs. The vertical axis measures cumulative share of firms that moved from the SFC to the SUAF based on the website query, and on the horizontal one each month for the period that goes from January 2003 to December 2010.

B.4 Econometric specification

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

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

where T refers to workers belonging to the treatment group, *Post* to the period after the event, and μ_t to month-year fixed effects.

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

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

where μ_{ft} refers to firm-specific month-year fixed effects.

Finally, the case with N firms with different treatment dates can be written as:

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

[15]

It is also possible to pool all the γ's as in a difference-in-differences approach and run the following specification:

$$w_{i,f,t} = \beta_0 T_{i,f,t} + \beta_1 W_{i,f,t} \cdot T_{i,f,t} \cdot Post_{i,f,t} + \beta_2 W_{i,f,t} \cdot T_{i,f,t} + \mu_{f,t} + \epsilon_{i,f,t} \tag{B.4}$$

where W refers to the *event window*.

In the analysis, for each firm-group-month we constructed a cell where we calculated the average wage and transfer, as well as percentiles 25, 50, and 75. Note that we need firms with workers in the two groups (with and without children) to perform the analysis. Wages are right-censored at a ceiling due to social security contributions so that is the reason why we use percentiles.

[15] 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, the one included in the main specification.

Appendix C

C.1 Appendix Figures and Tables from Chapter 3

Table C.1: Categories *Monotributo*

Categories	Upper threshold - Gross Earnings (annual)	
Monotributo	Jan10-Sep13	Sep13-Jan17
B	24,000	48,000
C	36,000	72,000
D	48,000	96,000
E	72,000	144,000
F	96,000	192,000
G	120,000	240,000
H	144,000	288,000
I	200,000	400,000
J	235,000	470,000
K	270,000	540,000
L	300,000	600,000
Law/Resolution	Law 26.565	Res. General 3.529

Source: Own elaboration based on official documentation i.e., laws and general resolutions.
Notes: The thresholds are expressed in current Argentinean pesos and are not adjusted for inflation.

Table C.2: Monthly tax

Period	Categories	*Impuesto integrado*	
		Services	Retail/Wholesale
Jan10-Jan17 Law 26.565	B	39	39
	C	75	75
	D	128	118
	E	210	194
	F	400	310
	G	550	405
	H	700	505
	I	1,600	1,240
	J		2,000
	K		2,350
	L		2,700

Source: Own elaboration based on official documentation i.e., laws and general resolutions.
Notes: The differential rate between the two different columns varies depending on the activity each self-employed works in. These tax duties should be paid on a monthly basis.

Table C.3: Social security contributions (subcomponents of the *monotributo*)

Retirement	SIPA
Jan10-Jul12 Law 26.565	110
Jul12-Jan17 Res. General 3.334	157
Health	**Obra Social**
Jan10-Jul12 Law 26.565	70
Jul12-Nov13 Res. General 3.334	100
Nov13-Sep14 Res. General 3.533	146
Sep14-Jul15 Res. General 3.653	233
Jul15-Jun-16 Res. General 3.775	323
Jun16-act Res. General 3.845	419

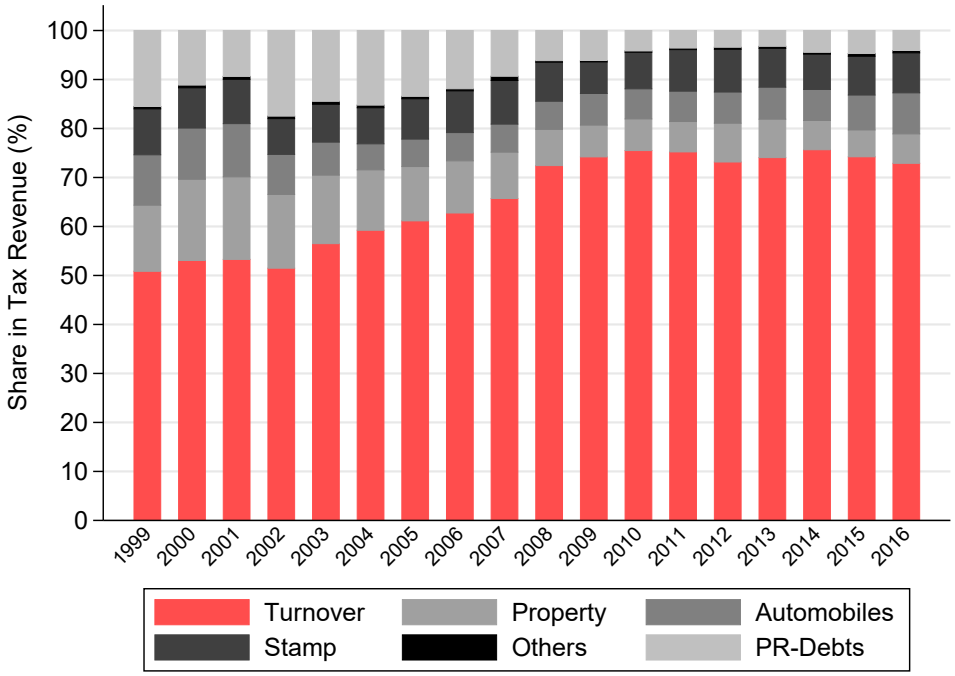
Source: Own elaboration based on official documentation i.e., laws and general resolutions.
Notes: These contribution duties should be paid on a monthly basis.

Table C.4: Variation of the Turnover Tax in the Province of Buenos Aires, 2006-2017

Year	Type of Activities									
	Wholesale and Retail			Services			Agriculture (*) and Manufacturing			
	(A)			(B)			(C)			
2017	2.50% <1.3M	3.50% <52M	**5.00%** F.J. or >52M	**3.50%** <650K	4.00% >650K	5.00% >39M	0.00% <52M	0.50% >52M	1.75% >78M	**4.00%** F.J.
2016	3.00% <1.3M	3.50% <52M	**5.00%** F.J. or >52M	**3.50%** <650K	4.00% >650K	5.00% >39M	0.00% <52M	0.50% >52M	1.75% >78M	**4.00%** F.J.
2015	3.00% <1M	3.50% <40M	**5.00%** F.J. or >40M	**3.50%** <500K	4.00% >500K	5.00% >30M	0.00% <40M	0.50% >40M	1.75% >60M	**4.00%** F.J.
2014	3.00% <1M	3.50% <40M	**5.00%** F.J. or >40M	**3.50%** <500K	4.00% >500K	5.00% >30M	0.00% <40M	0.50% >40M	1.75% >60M	**4.00%** F.J.
2013	3.00% <1M	3.50% <40M	**5.00%** F.J. or >40M	**3.50%** <500K	4.00% >500K	5.00% >30M	0.00% <40M	0.50% >40M	1.75% >60M	**4.00%** F.J.
2012 S2	3.00% <1M	3.50% <40M	**5.00%** F.J. or >40M	**3.50%** <30M		5.00% >30M	0.00% <60M		1.00% >60M	**3.00%** F.J.
2012 S1	3.00% <30M		**4.50%** F.J. or >30M	**3.50%** <30M		4.50% >30M	0.00% <60M		1.00% >60M	**3.00%** F.J.
2011	3.00% <30M		**4.50%** F.J. or >30M	**3.50%** <30M		4.50% >30M	0.00% <60M		1.00% >60M	**3.00%** F.J.
2010	3.00% <30M		**4.50%** F.J. or >30M	**3.50%** <30M		4.50% >30M	0.00% <60M		1.00% >60M	**3.00%** F.J.
2009	3.00% <30M		**4.50%** F.J. or >30M	**3.50%**			0.00% <60M		1.00% >60M	**3.00%** F.J.
2008	3.00% <30M		**4.50%** F.J. or >30M	**3.50%**			0.00% <60M		1.00% >60M	**3.00%** F.J.
2007	**3.00%**			**3.50%**			0.00% based in BA		**1.00%** F.J. (Agro)	**1.50%** F.J. (Manuf)
2006	**3.00%**			**3.50%**			0.00% based in BA		**1.00%** F.J. (Agro)	**1.50%** F.J. (Manuf)
2005	**3.00%**			**3.50%**			0.00% based in BA		**1.00%** F.J. (Agro)	**1.50%** F.J. (Manuf)

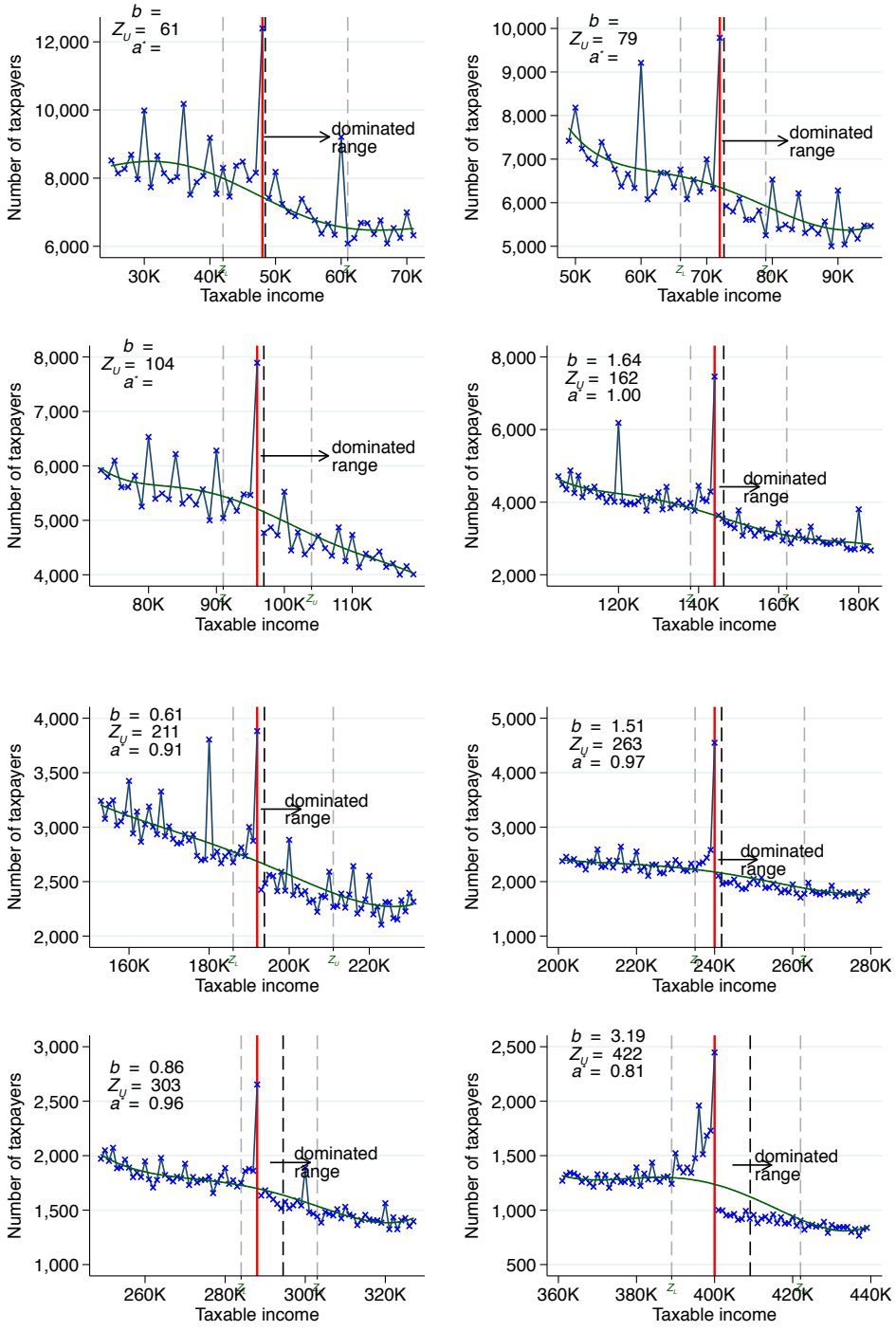
Source: Own elaboration based on the Tax Law of the Province of Buenos Aires 2006-2017.
Note: this table shows the variation of the turnover general tax rates for (A) wholesale and retail, (B) services, and (C) agriculture and manufacturing. In the top row of each year we report the tax rates and in the bottom row we report the condition the determines each firm's tax rate. Gross income thresholds denote annual sales at t-1. F.J. denotes foreign jurisdiction (firms based outside the province and selling inside the province). (*) In agriculture, a tax rate of 1% applies to "Cultivation of cereals, oilseeds, and forage crops" and "Livestock breeding" (2% for sowing pools with annual turnover at year t-1 greater than 10m).

Figure C.1: Composition of own revenue (Buenos Aires, 1999-2016)



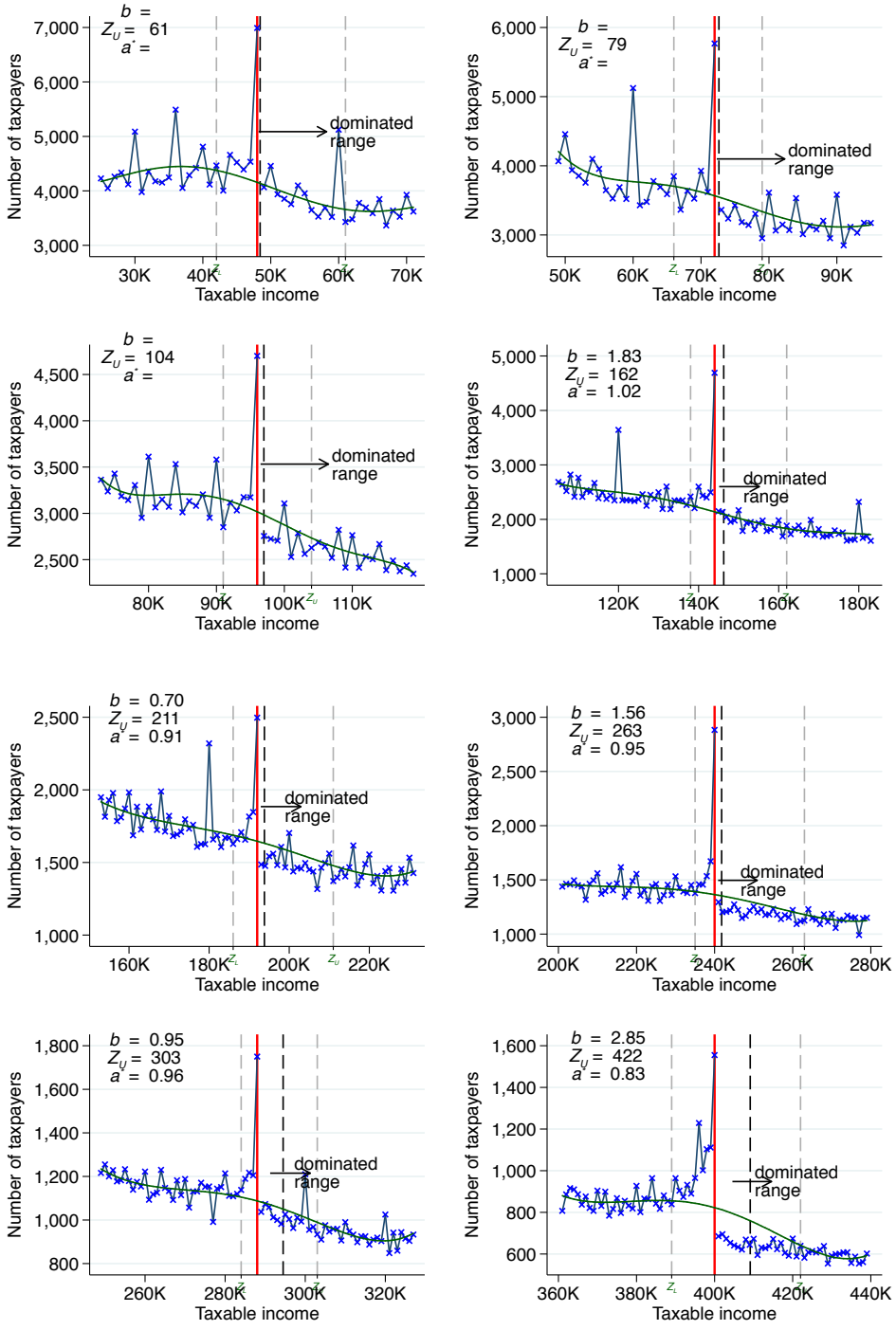
Source: Tax Revenue is taken from 'Dirección Provincial de Política Tributaria'. Provincial GDP is taken from 'Dirección Provincial de Estadística'.

Figure C.2: Bunching at *monotributo notch* (full sample)



Notes: Taxpayers of the years 2014, 2015 and 2016 are grouped in bins of $1,000$ pesos. The solid red vertical lines refer to the different *notches*. The green solid line is the prediction of regressing equation (1) with the caveat of the dummies in the excluded range. On the horizontal axis we measure taxable income which is equal to the annual turnover. These figures consider the full sample i.e., includes multiple of $\$1,000$.

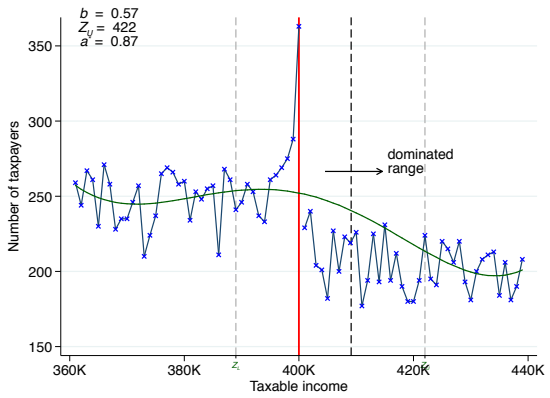
Figure C.3: Bunching at *monotributo notch* (panel sample)



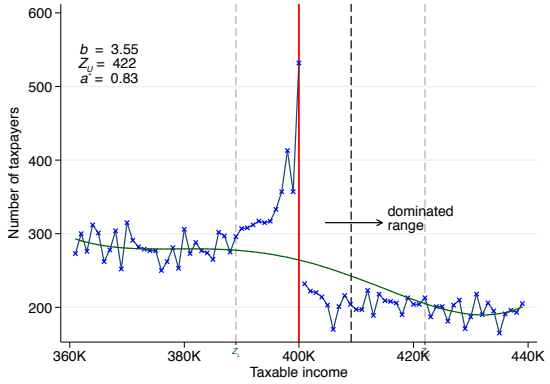
Notes: Taxpayers of the years 2014, 2015 and 2016 are grouped in bins of $1,000$ pesos. The solid red vertical lines refer to the different *notches*. The green solid line is the prediction of regressing equation (1) with the caveat of the dummies in the excluded range. On the horizontal axis we measure taxable income which is equal to the annual turnover. These figures consider taxpayers that have filed taxes during the three consecutive years mentioned before and includes multiple of $\$1,000$.

Figure C.4: Bunching at 400K by year

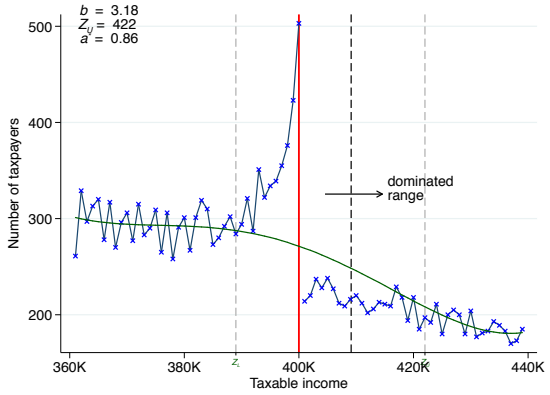
(a) 2014



(b) 2015

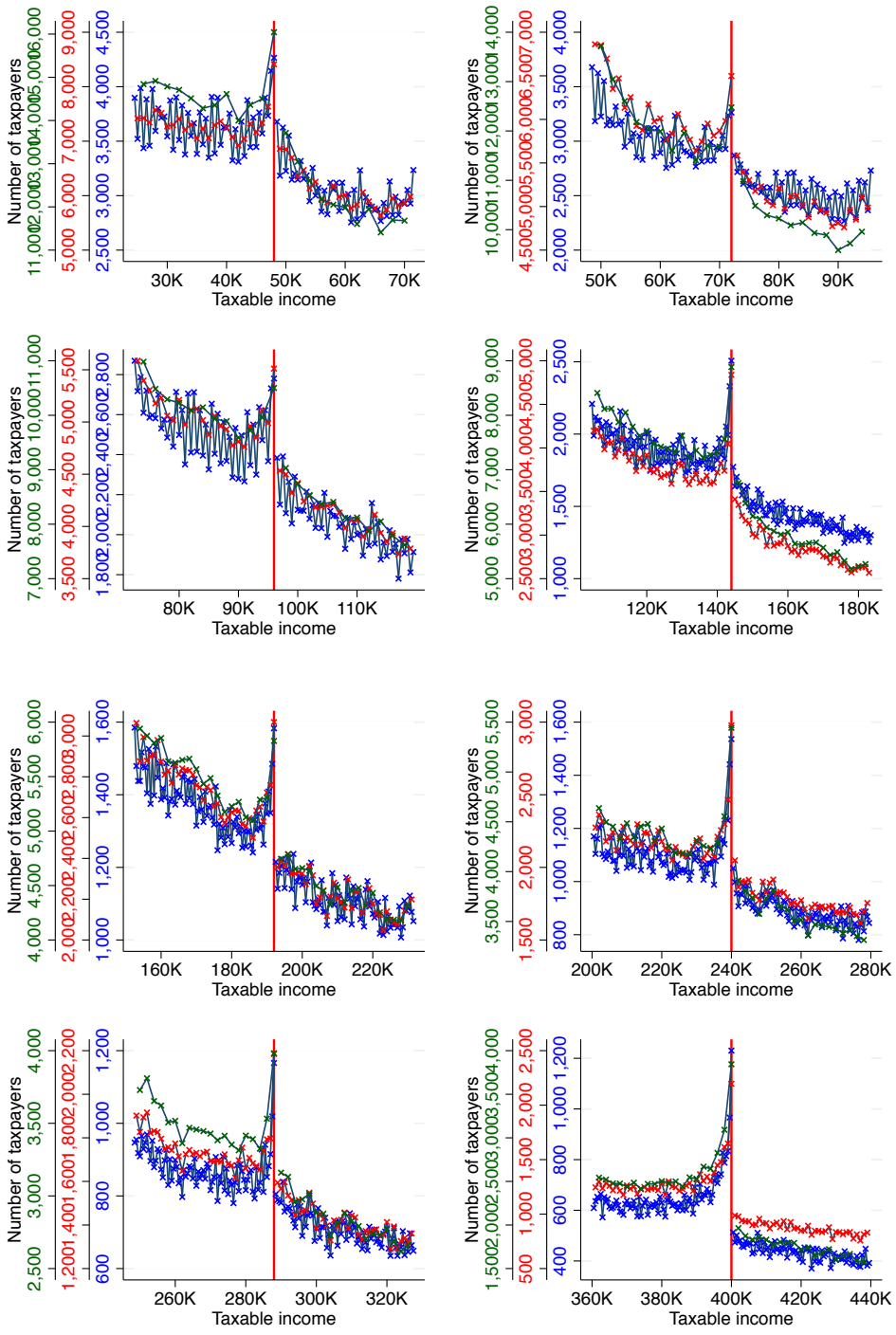


(c) 2016



Notes: In this set of figures we focus on taxpayers that have filed taxes in the three years. The solid red vertical lines refer to the different *notches*. The green solid line is the prediction of regressing equation (1) with the caveat of the dummies in the excluded range. On the horizontal axis we measure taxable income which is equal to the annual turnover.

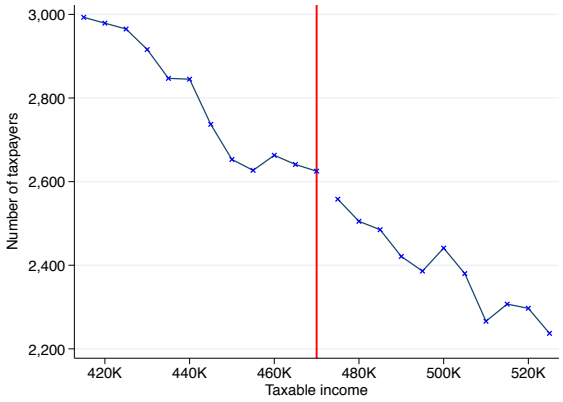
Figure C.5: Bunching at *monotributo notches* and bin sizes



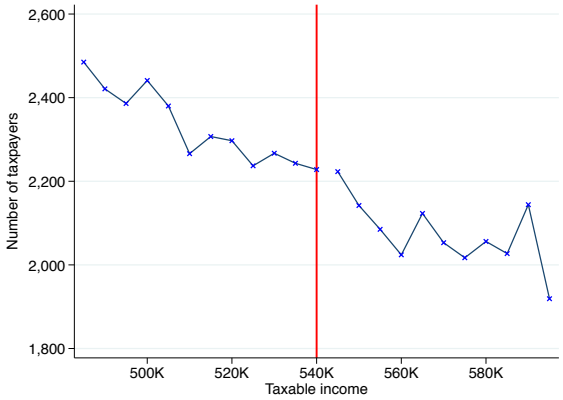
Notes: Bunching remains when using different bin sizes. In each series, we group taxpayers of the years 2014, 2015 and 2016 using three different bins of 500 (blue), 1,000 (red) and 2,000 (green) pesos respectively. Note that the vertical axis has a different scale for each series, where the color of the series corresponds to the color of the axis label. The horizontal axis we measure taxable income which is equal to the annual turnover. We remove taxpayers whose reported taxable income is a multiple of 1,000.

Figure C.6: Bunching at top *monotributo notches*

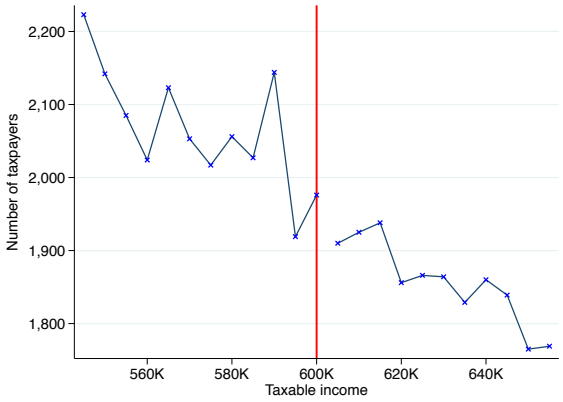
(a) *470K*



(b) *540K*



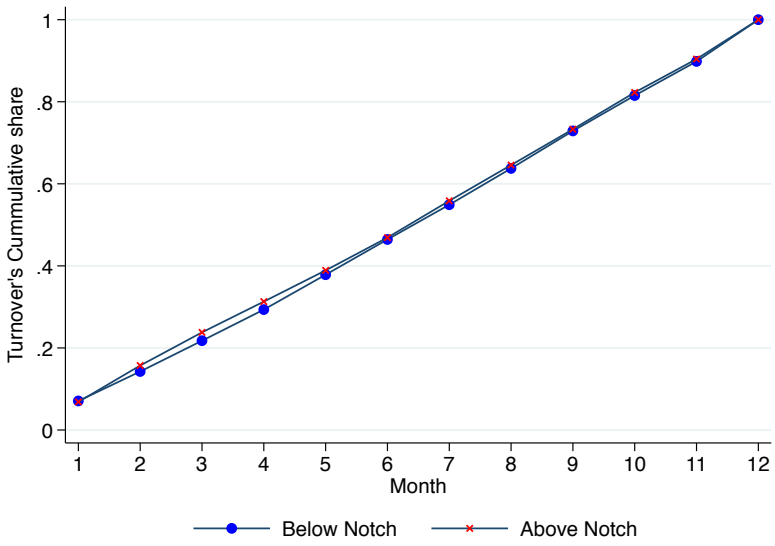
(c) *600K*



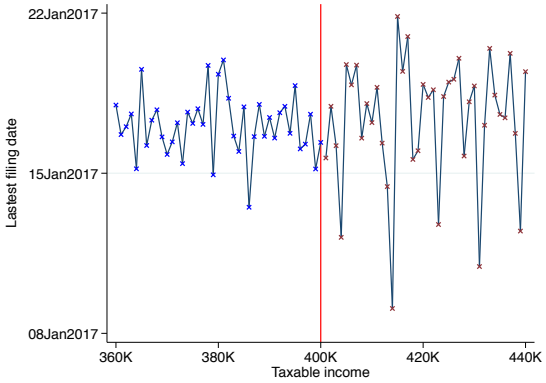
Notes: taxpayers of the years 2014, 2015 and 2016 are grouped in bins of 1,000 pesos. The solid red vertical lines refer to the different *notches*. On the horizontal axis we measure taxable income which is equal to the annual turnover. We remove taxpayers whose reported taxable income is a multiple of 1,000.

Figure C.7: Bunchers' characteristics (at 400K)

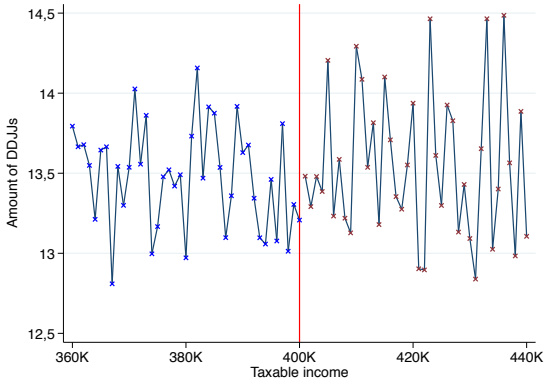
(a) **Cumulative share**



(b) **Latest filing date**



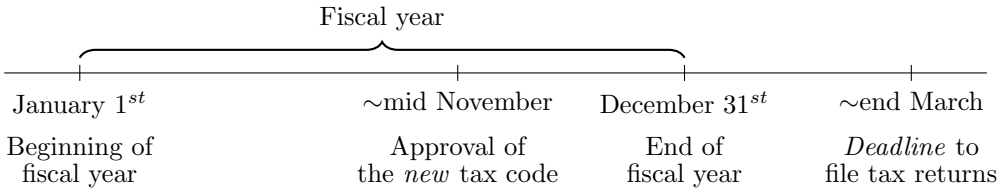
(c) **Number of tax returns**



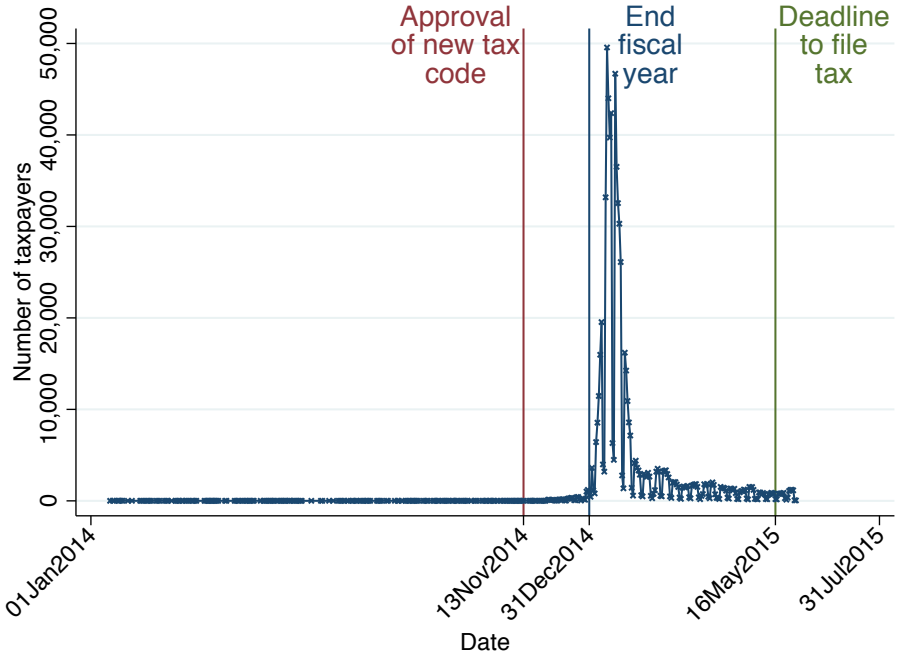
Notes: Panel (a) presents the evolution of the turnover's cumulative share across the different months (year 2016) for those taxpayers just right below the *notch* (bins 399 and 400K) and those just right above (bins 401 and 402K). Panel (b) contains the last filing date for each bin (year 2016). Panel (c) presents the number of declarations each taxpayer files. Note that taxpayers can adjust or correct filing from previous months.

Figure C.8: GRT filing diagram

(a) **Filing timeline**



(b) **Last filing date (2014)**



Notes: In panel (a) we present the filing timeline of the gross receipts tax. The fiscal year goes from January 1^{st} to December 31^{st} each year. Normally, the tax code for the following fiscal year is approved by mid/end of November. Finally, the last data to file is generally three months after the fiscal year has finished.Simply, this means that a given firm will know the tax code that will be valid starting next year sometime in November and will have time to file the latest declaration three months after the fiscal year ends. In November the firm becomes aware of the *notches* that will be in place, and thus has time to *manipulate* the reported revenue to lower its tax liability. In panel (b) we present an example using data from 2014.