It takes two to *tango*: labor responses to an income tax holiday in Argentina *

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Abstract

We exploit a large and quasi-randomized 2.5 year-long income tax holiday to identify intertemporal labor responses of high-wage earners to net wage changes. In August 2013, the Argentine government exempted a group of wage earners from the income tax and left the remaining group taxed until February 2016, when the tax break was repealed. Eligibility was based on whether *past* wage earnings were below a fixed threshold, creating a discontinuity that treated workers who coexist in the same labor market (even in the same firm) with sharply different marginal and average tax rates—effectively 0% for workers below the threshold. Using rich population-wide administrative data and a regression discontinuity design, we estimate a precise and very small wage earnings elasticity of 0.017 to this large, salient, and temporary income tax change. 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 the fixed threshold. The strategic entry below the threshold to dodge taxes required coordination with employers. Our evidence challenges the standard model of labor supply and demand, and points toward rigidities in the labor market which requires employer-employee cooperation for wage earners to respond to tax changes.

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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 et al., 2012b).[1] In part this is because tax changes tend not to be that large or that easy to exploit for identification (Chetty et al., 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 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

[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.

[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. Indepen-

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. 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 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.

dent 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.

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 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

[5]This is computed over an average base of 25 monthly overtime hours. Although larger than the wage earnings elasticity of 0.017, it is still small if we include (unobserved) *straight-time* hours in the computation of the percentage change.

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 et al., 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 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

[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.

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 et al. (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 (2019b) and Tazhitdinova (2019a) 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 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

[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 et al. (2001) but with better data and an improved empirical strategy.

labor supply and reported income responses to taxation (Best (2014), Kreiner et al. (2016), Tazhitdinova (2019a)).

The article is organized as follows. Section 2 describes the Argentine income tax and the variation we use. Section 3 discusses the expected responses to the tax change. Section 4 introduces the administrative data and summary statistics. Section 5 presents the empirical strategy. Section 6 shows the first stage change in tax rates and evidence on saliency. The main results are presented in Section 7. Section 8 shows the avoidance behavior of new entrants. Section 9 concludes.

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.

2.1 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.[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), 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

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 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.

2.2 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

AR$ 107,783 (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.

in a context where inflation was high and nothing was indexed. In particular, the following four facts explain the evolution of the PIT 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 A2 panel a); (2) nominal wage earnings were adjusted semi-annually to preserve the purchasing power of workers (red line of Figure A2 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 A3 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 A3 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 A3 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 A3 panels b and d).

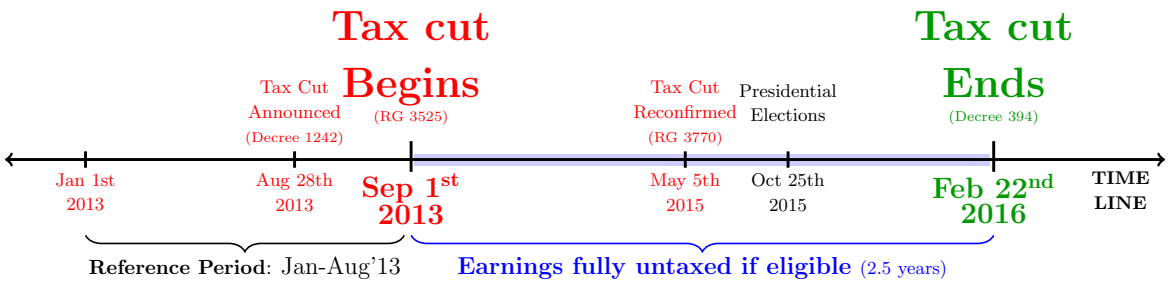
An income tax holiday on high earners. To alleviate the increasing tax burden on high-wage earners, in August 2013 the President of Argentina implemented a targeted income tax cut that lasted 2.5 years and affected differentially what would otherwise be comparable workers.[14] Eligibility was based on 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 a due date. What is certain is that workers knew that it was

[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).

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$2,000 million from the private sector. The first one was a 15% tax on capital gains from shares and securities not listed on the stock market. The second one was a 10% tax on the dividends paid to shareholders.[17] In practice, the reform reduced the share of wage earners affected by the tax by approximately 50%, benefiting workers between the 70th and 85th percentiles of the wage distribution (see Figure 1). Workers earning about AR$ 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.

2.3 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

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

the industry-wide level (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]

[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 A4 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 A5 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.

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, 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.

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 A6 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). 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

[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).

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 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.

[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.

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 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:

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

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

$$R_i \equiv \max\{gross\ monthly\ wage_i \mid \text{January to August 2013}\} \quad (2)$$

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:

$$Y_{it} = \alpha_i + \gamma_t + \sum_{t \neq Aug'13} \beta_t^k (I_t \times T_i^k) + u_{it} \tag{3}$$

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

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.

5.1 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 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

[26]This result is consistent with empirical findings in other countries such as the U.S. where Saez (2010)

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 et al. (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.

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 4, we perform the RDD Manipulation Test based on discontinuity in density using local polynomial techniques (see Cattaneo et al. (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 A9 we show, however, that there is no visible discontinuity in the age, gender, marital status, and number of children of wage earners at 15k.

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.

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.

6 First stage and salience

6.1 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 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 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]

[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.

[30] This is the reason why the tax schedule does not present the standard piecewise linear shape. See Figure A8 for a simulation using current monthly wages on the horizontal axis.

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.

6.2 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 A10 or newspaper coverage as shown in Figure A11), the tax change analyzed in this paper was unique in that it was widely covered by nationwide TV 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 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 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

[31] Hoopes et al. (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.

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 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.

7 Results

7.1 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 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 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 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 super-

[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.

[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

impose 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 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 8, and we plot their evolution over time in Figure 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 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 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

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 et al. (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.

[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.

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 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 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 A13, 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.

7.2 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

[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.

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 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 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 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

[39] See section 2.3 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 A14 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%.

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 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 et al. (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.

7.3 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 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 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 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 fixed part and variable part.

[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.

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 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 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 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 unrespon-

[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 A15.

[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.

sive wage earners, managers seem to be quite responsive to income taxation. This result can be linked to recent work by Piketty et al. (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 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.

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

[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.

the pre-reform period with wage earners that were not present in our data between January and August 2010 and appear 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 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 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 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,

[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.

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 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 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 A16 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 A17 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 7.3. 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.

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 6.2, 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 A27 and A30).

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 A18 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 A19, 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 (2011), 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 A20 and A21, 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, the strategic behavior from new entrants to dodge taxes would not be possible without coordination with employers. Taken together,

[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.

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 et al. (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 et al., 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 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

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

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.

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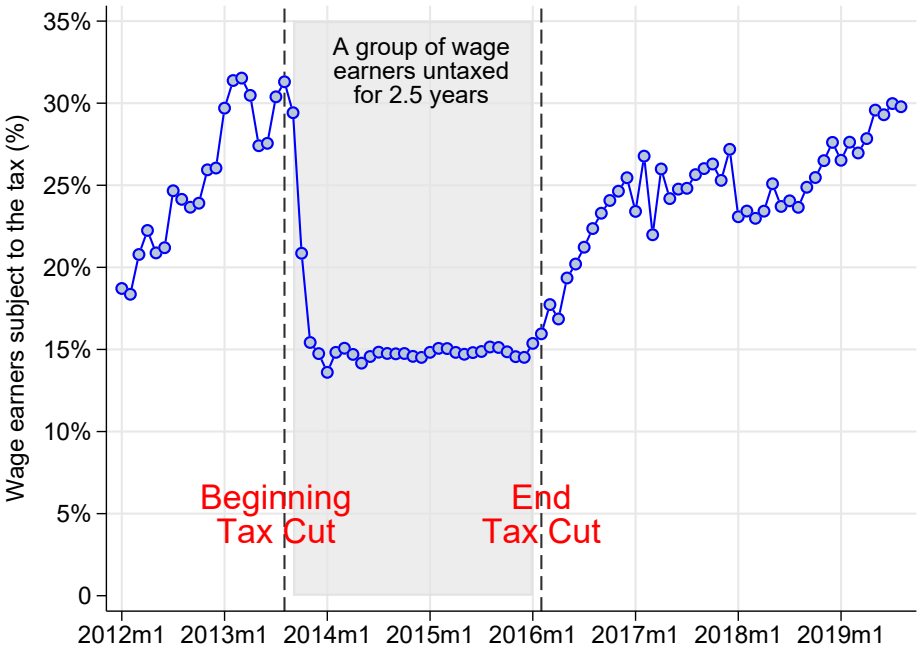
Figures and Tables

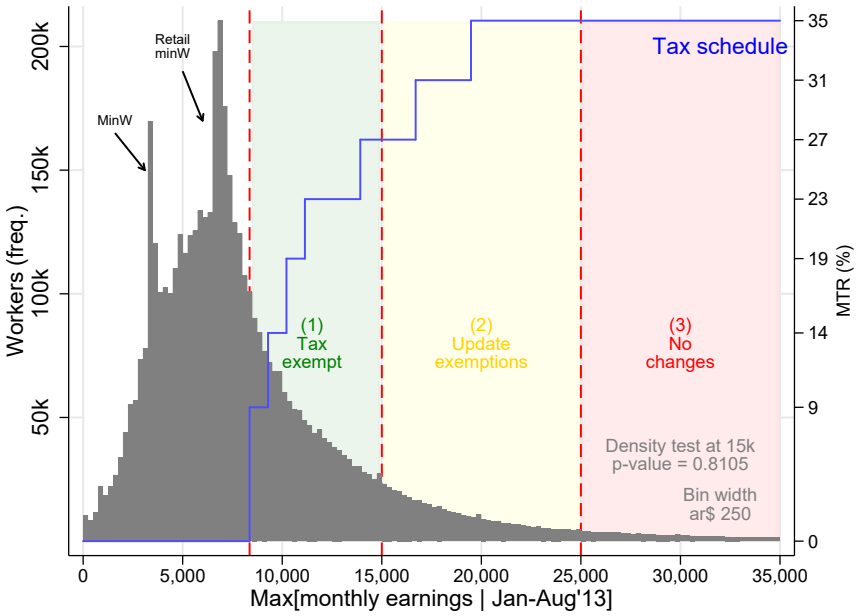
Figure 1: Wage earners subject to the income tax (%)



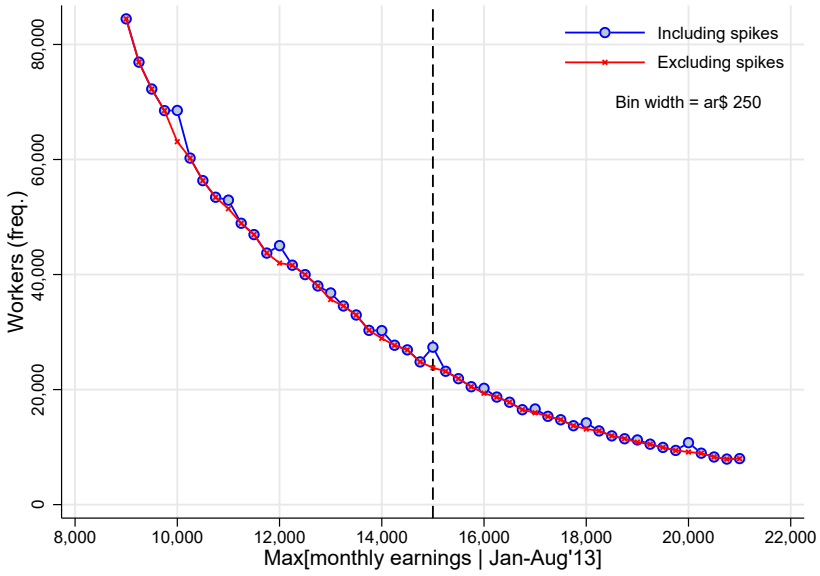
Notes: this figure plots the fraction of registered private and public wage earners with income tax withheld at source by their employer. Vertical dashed lines denote the beginning (September 2013) and the end (February 2016) of the tax holiday (Executive Order 1242/2013). Immediately after the tax change 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 2: Distribution of the running variable

(a) Full distribution



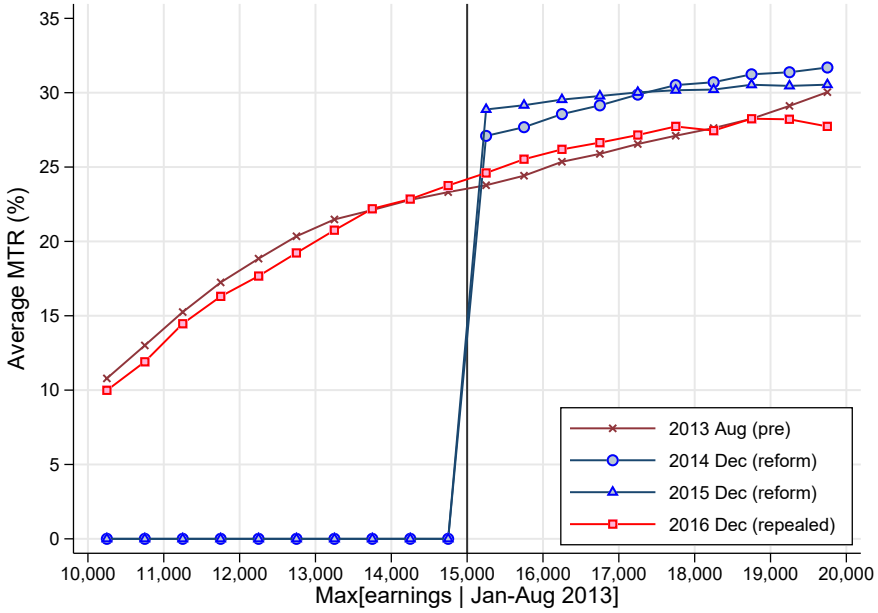
(b) Zoom around 15k



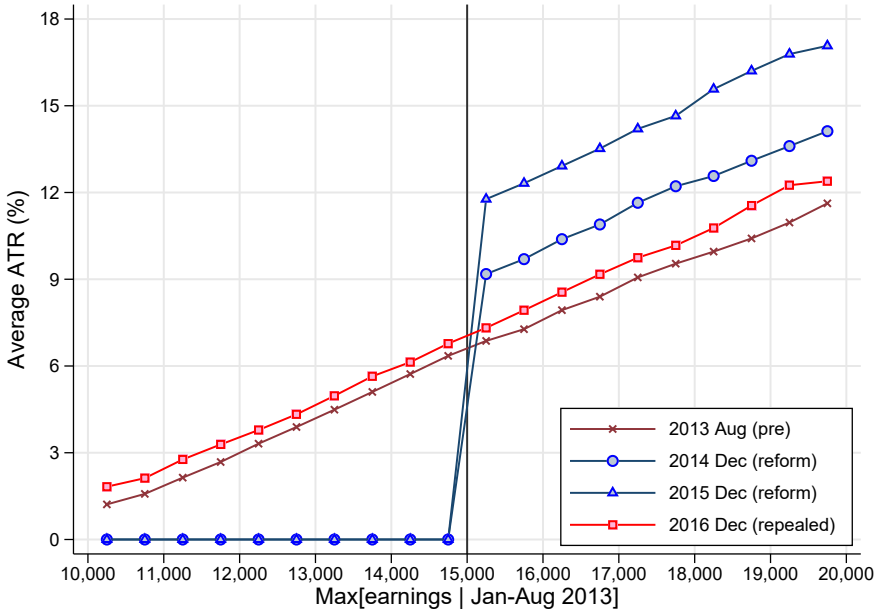
Notes: this figure displays the distribution of the highest gross monthly salary between January and August 2013 (the running variable in the RDD). Panel (a) shows the full distribution up to 35k. The vertical lines from left to right denote the the first kink of the income tax for single workers at 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 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 4: Live announcement, interviews, and TV newscasts

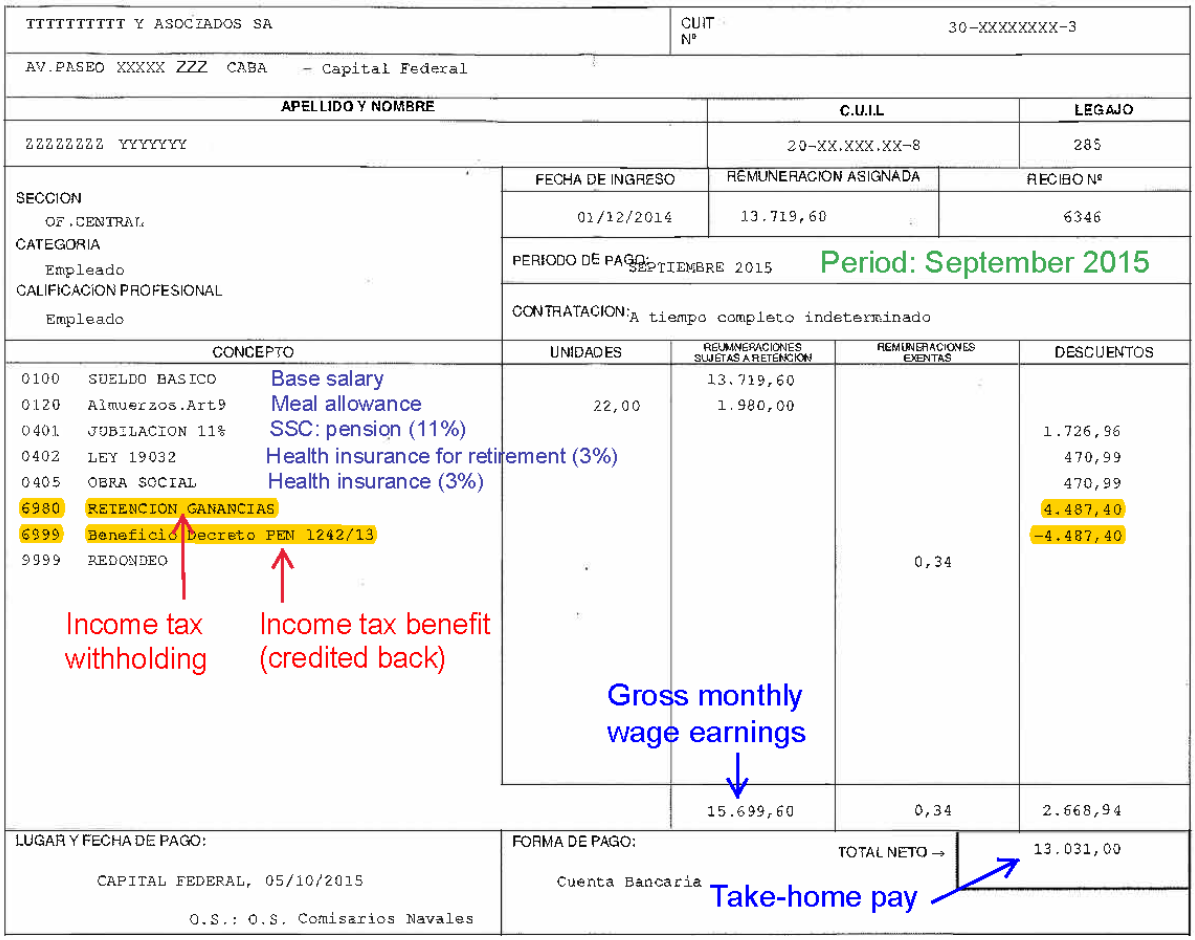
(a) August 27th, 2013 (b) August 27th, 2013

(c) August 28th, 2013 (d) August 28th, 2013

(e) August 30th, 2013 (f) August 30th, 2013

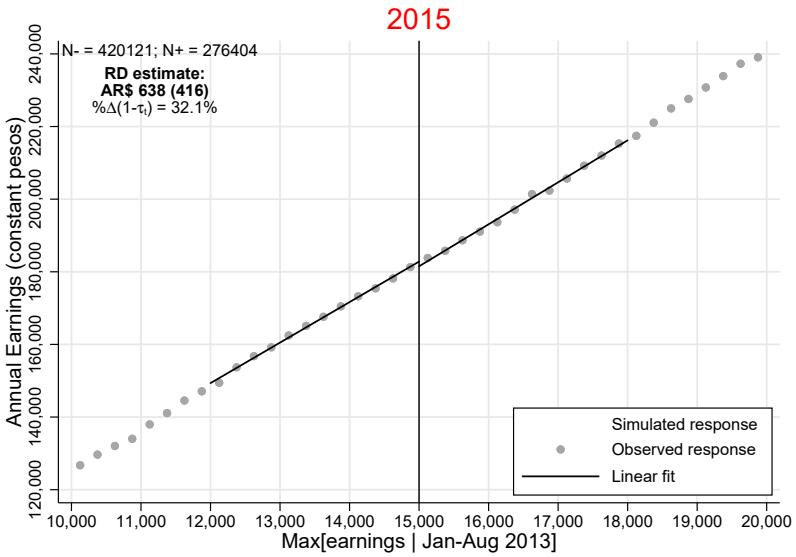
Notes: this picture shows the repercussion that the income tax change ("*ganancias*") had in the Argentine television. Panel (a) shows a photo of the official meeting where the announcement was made with the participation of the President, the head of the IRS, and representatives of employers' organizations 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 5: Pay stub of a wage earner benefited by the tax holiday (September 2015)



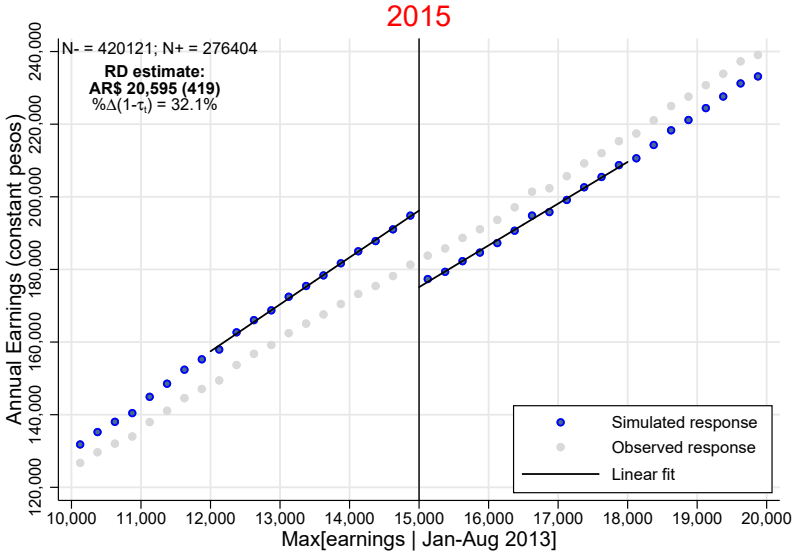
Notes: This figure displays the pay slip of an anonymous wage earner benefited by the reform (i.e., below the discontinuity) in September 2015. The presidential decree contained an article requesting employers to include a line with the income tax withholding of the corresponding month, '`Income Tax 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 6: Observed earnings response after 2 years



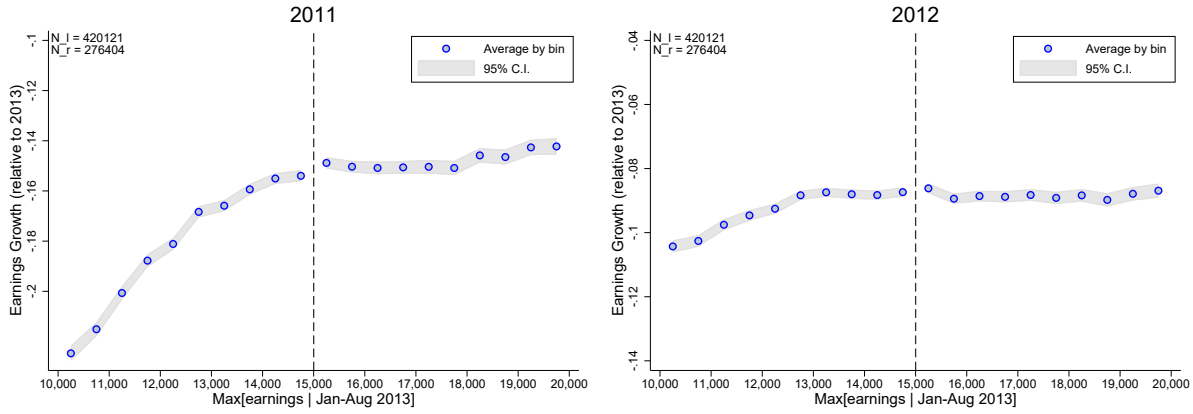
Note: this graph plots average gross annual wage earnings for 40 equally spaced bins of the running variable (width AR$ 250). The figure includes a linear fit on each side of the discontinuity, computed with `rdrobust` routine from Calonico et al. (2017) using a triangular kernel and a AR$3,000 bandwidth. 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 7: Thought experiment: observed vs simulated response



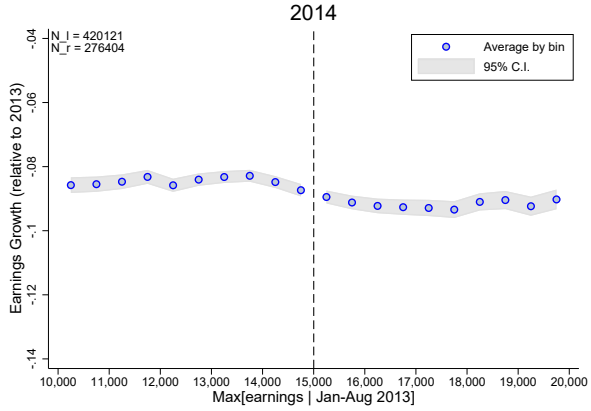
Note: this figures reproduces the observed response from the previous figure (gray dots) and superposes a simulated response in a frictionless world with $e = 0.3$ and no income effects (blue dots). Earnings are shifted by $0.3 \times \%\Delta(1 - \tau_{it})$, where τ_{it} is the individual empirical 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 8: RD for excess annual earnings growth relative to 2013

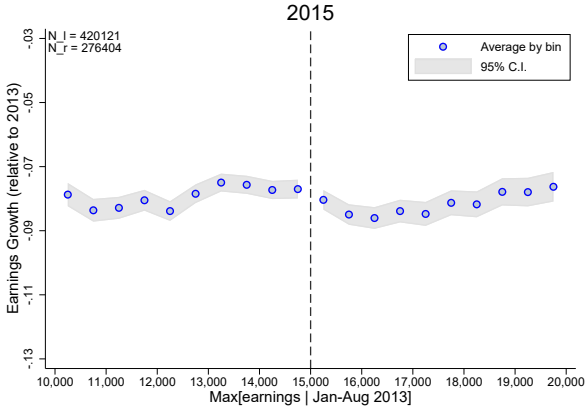


(a) 2011 (pre-reform)

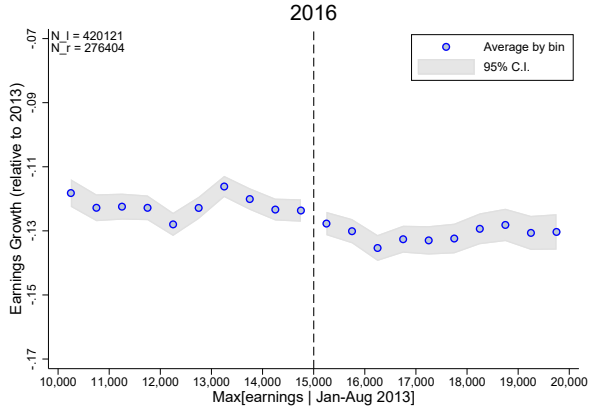
(b) 2012 (pre-reform)



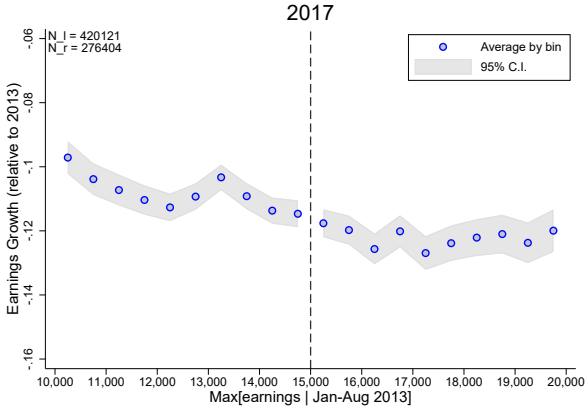
(c) 2014 (reform)



(d) 2015 (reform)



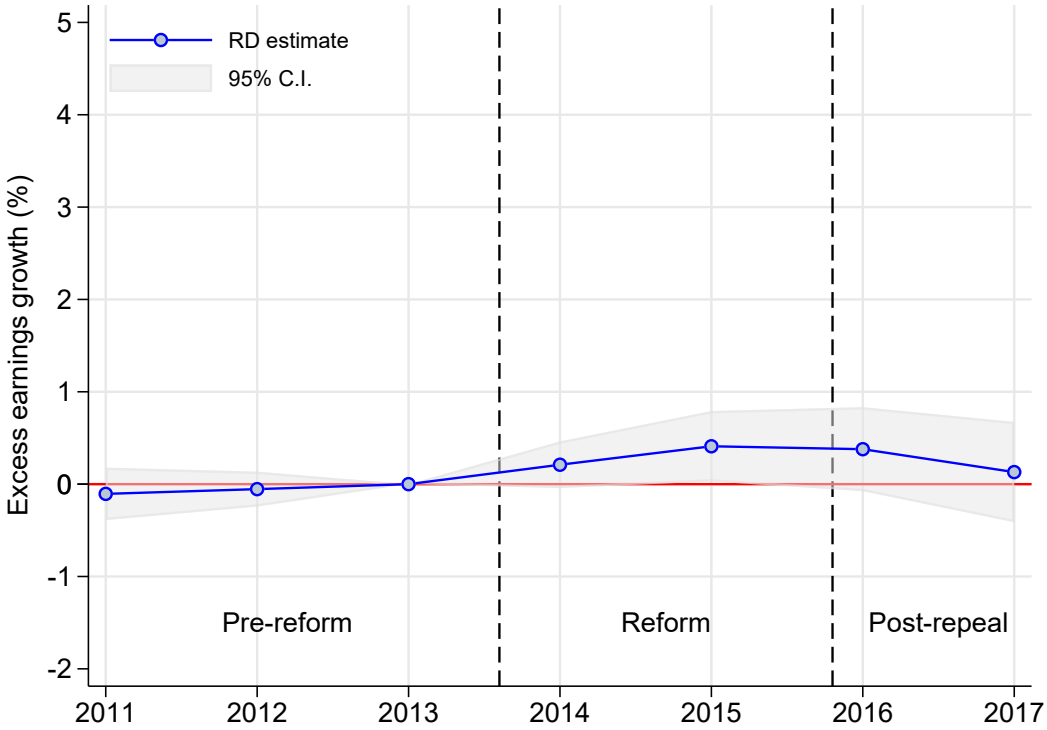
(e) 2016 (post-repeal)



(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.

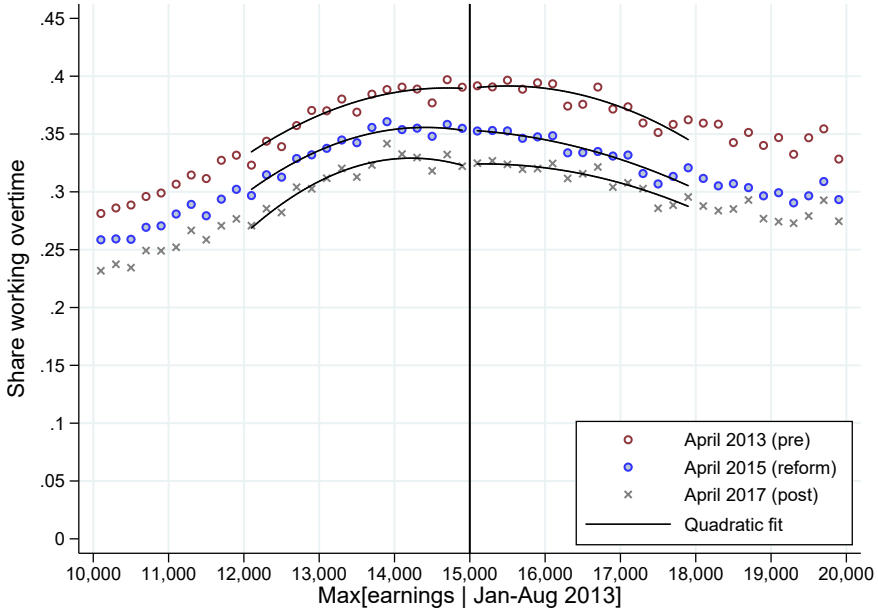
Figure 9: Evolution of RD estimates for wage earnings growth, 2011-2017



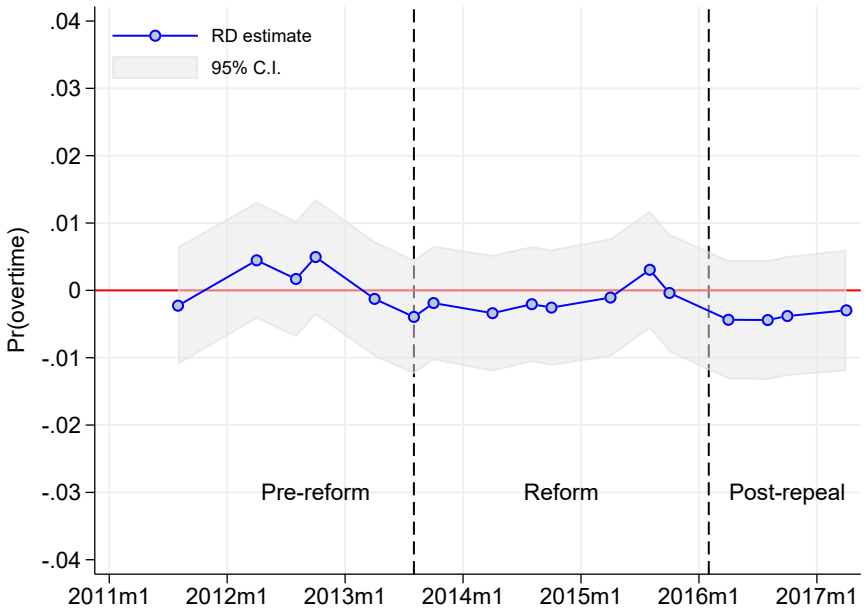
Notes: this graph plots the evolution of the RD estimates computed in each panel of Figure 8. Each dot corresponds to a separate standard RD regression using a linear fit on each side of the discontinuity, a triangular kernel, and a AR$3,000 bandwidth. We use the `rdrobust` routine from Calonico et al. (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 5.

Figure 10: RD estimates for overtime likelihood (extensive margin)

(a) RD plot for overtime likelihood



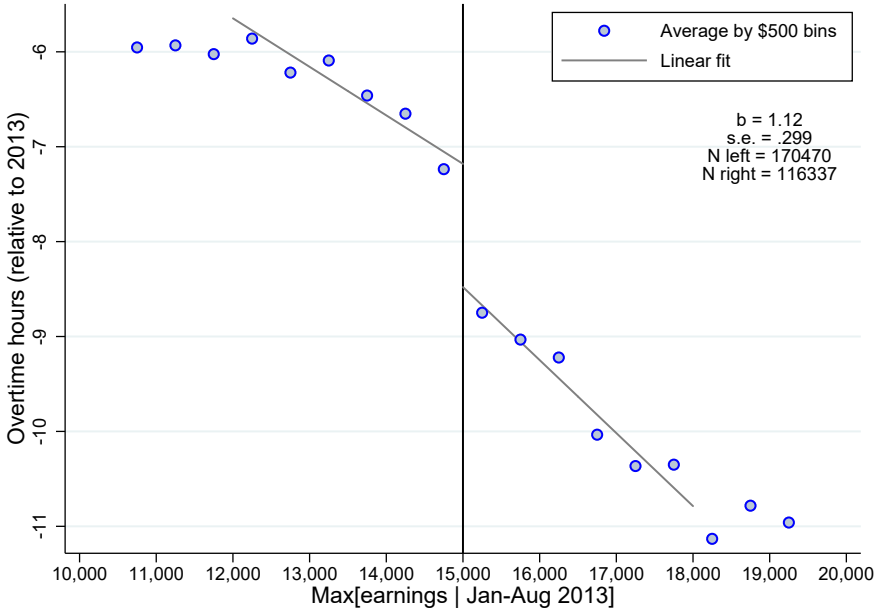
(b) Evolution of RD estimates



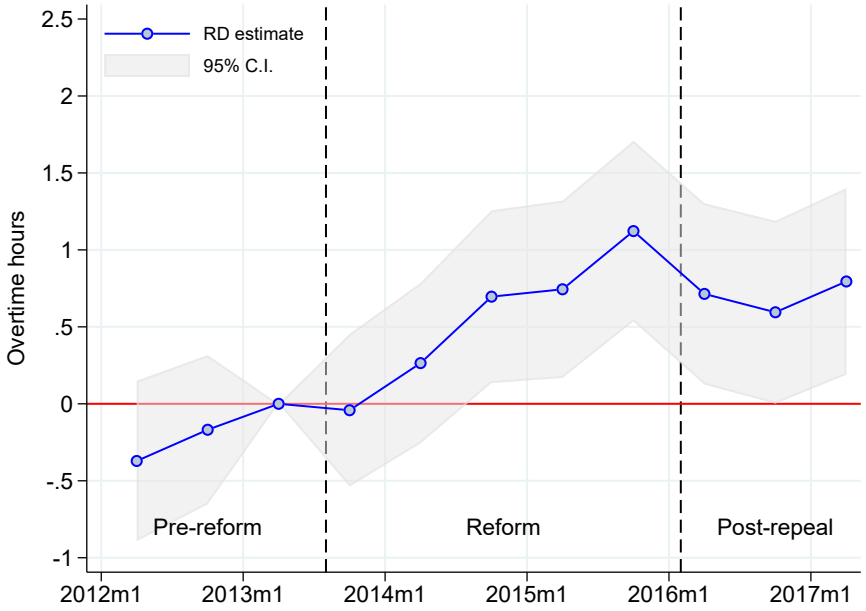
Notes: these figures present the RD analysis for the probability of working overtime using data from April, August, and October of each year. The dependent variable in the RD is an indicator for whether the worker has positive overtime payments. Panel (a) shows the fraction of workers doing overtime 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 11: RD estimates for overtime hours (intensive margin)

(a) RD plot for overtime hours in October 2015



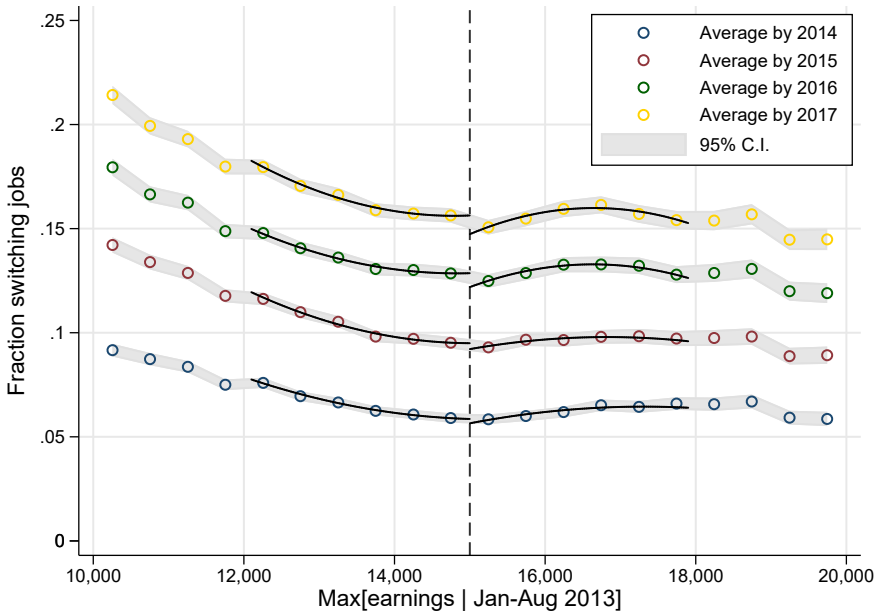
(b) Evolution of RD estimates



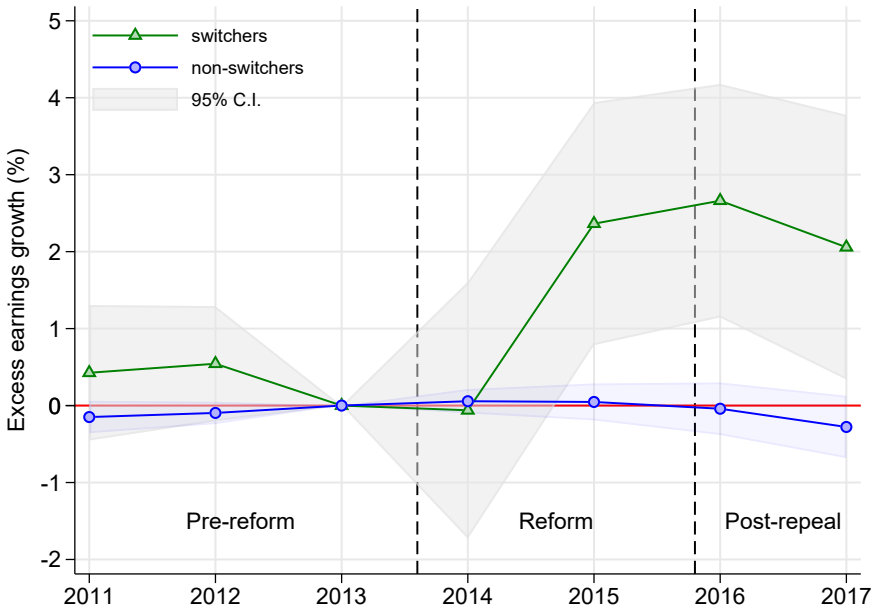
Notes: these figures report the results for overtime hours and are computed using data from April and October of each year. The dependent variable in the RD is the difference between overtime hours in each month-year and overtime hours in April 2013. Panel (a) shows the comparison between October 2015 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 12: RD estimates for job switchers (extensive and intensive margin)

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



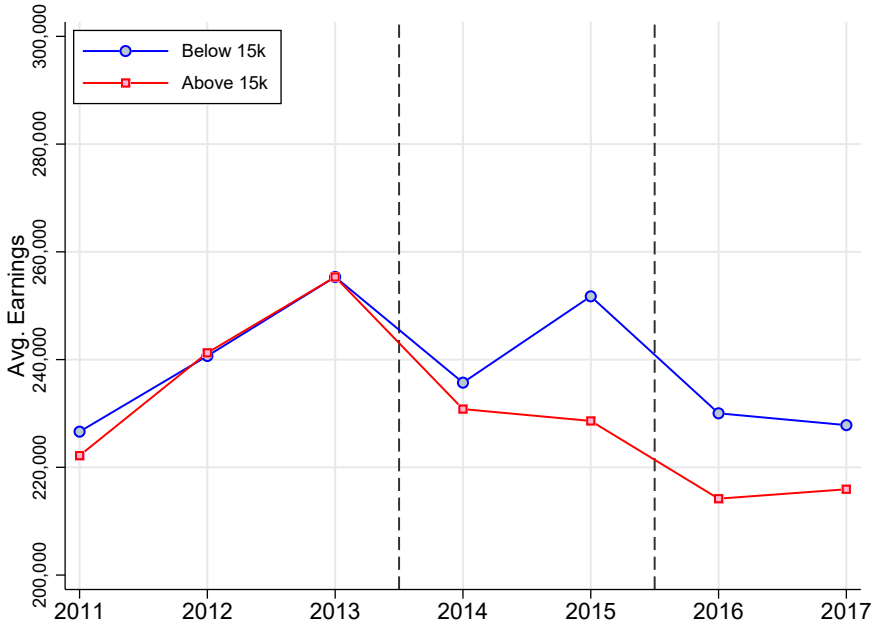
(b) Excess wage earnings growth for switchers



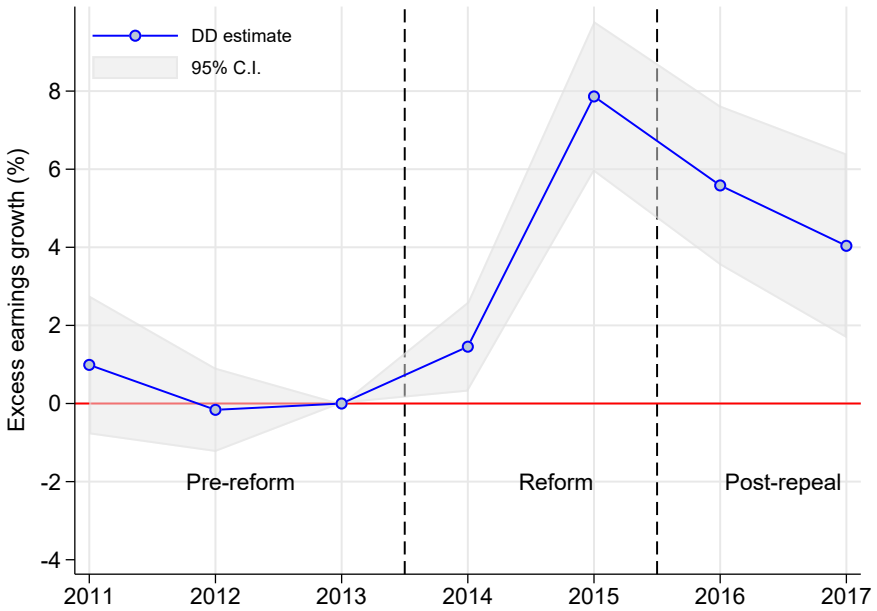
Notes: these figures present the RD analysis for job switchers. We define switchers as workers with a different firm identifier by December of each year relative to the firm identifier they had when the reform took place in 2013. Panel (a) plots the fraction of switchers by bins of the running variable (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 13: Wage earnings response of managers

(a) Annual earnings in constant pesos



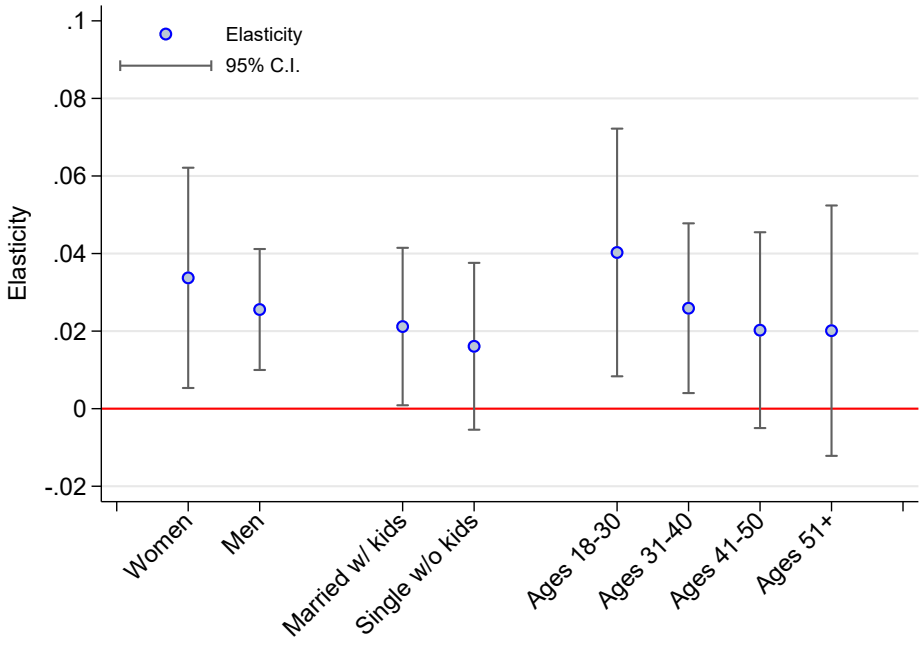
(b) Excess earnings growth (diff-in-diff estimates)



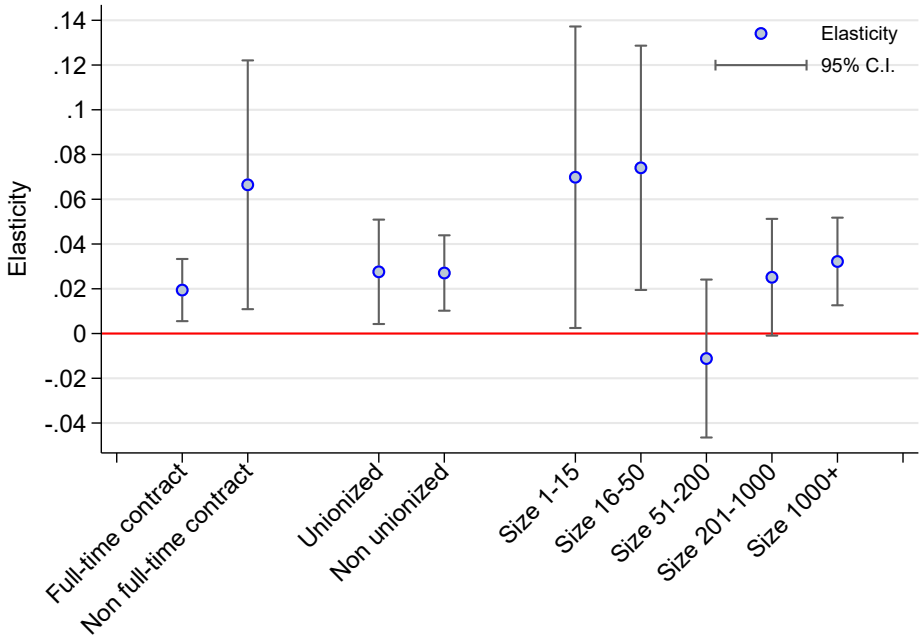
Notes: this figure shows wage earnings responses of managers to the income tax holiday. The treatment group contains managers with a running variable between AR$ 10,000 and AR$ 15,000 (untaxed), and the control group contains managers with a running variable between AR$ 15,000 and AR$ 25,000 (taxed). 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 5 column 4. The vertical dashed lines indicate the beginning and the end of the tax holiday.

Figure 14: Elasticities by subgroups (in 2015)

(a) **By demographic characteristics**



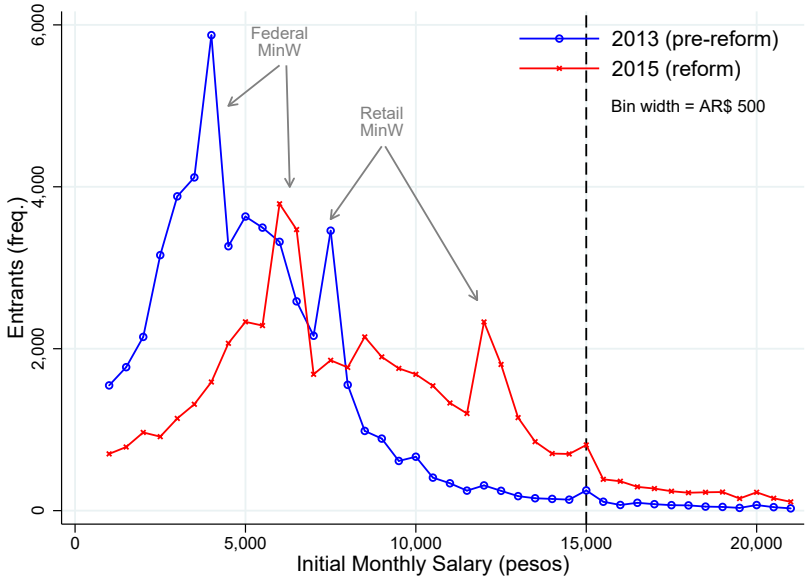
(b) **By employment characteristics**



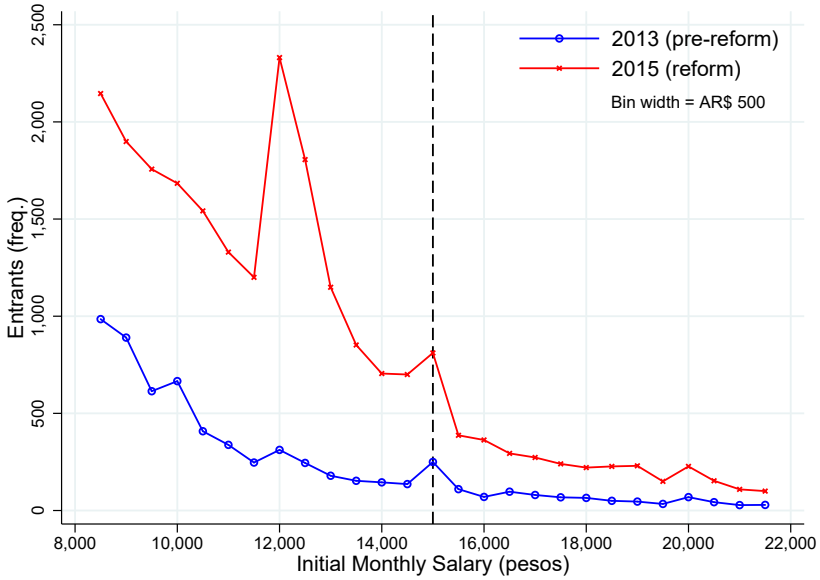
Note: this figure plots wage earnings elasticities for different subgroups in the year 2015 (the last year of the tax holiday). Each dot correspond to a separate RD regression where the dependent variable is annual earnings growth relative to 2013. To compute the elasticities we scale the reduced-form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

Figure 15: Distribution of initial monthly wages for new entrants

(a) Full distribution



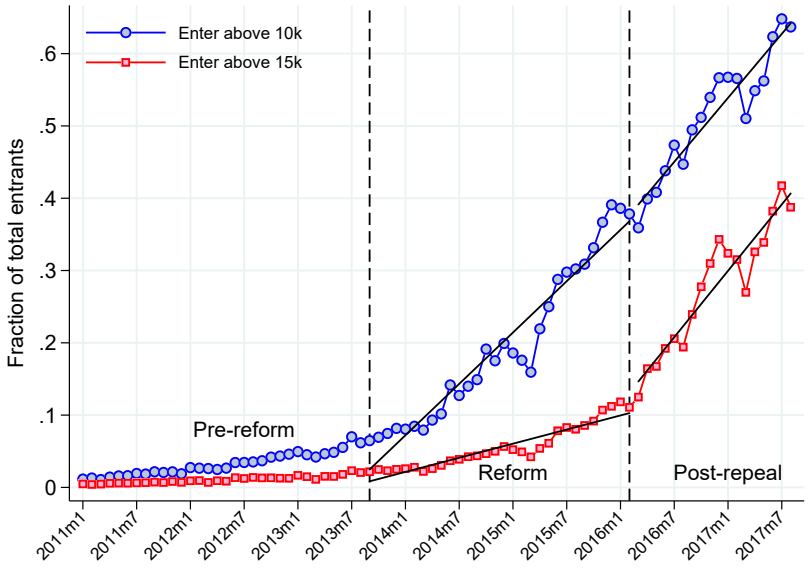
(b) Zoom in on the range 8k-22k



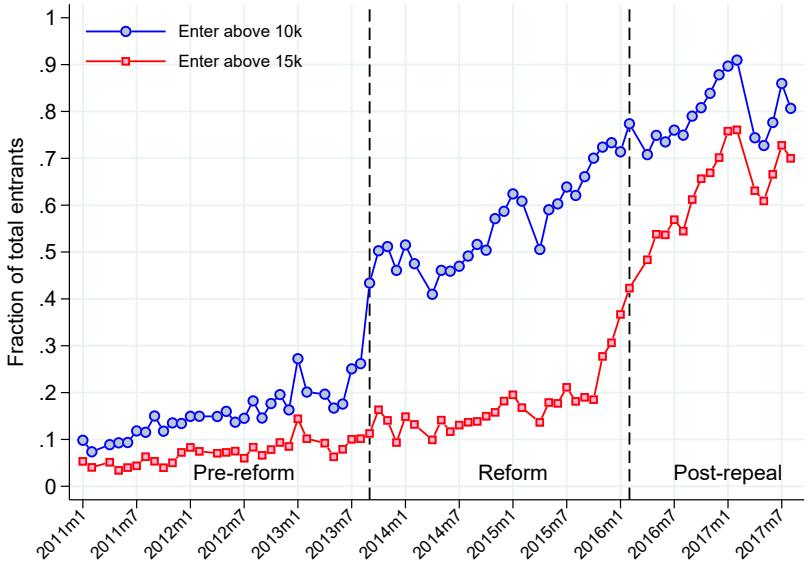
Notes: this graph plots the distribution of the first monthly salary for wage earners that did not have a job in January-August 2013. Panel (a) plots the full distribution up to AR$ 22,000, and panel (b) zooms in on the range AR$ 8,000-22,000. The blue line corresponds to the year 2013 and the red line 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 16: Share of entrants with initial monthly salary above 10k, 15k

(a) Pool of entrants



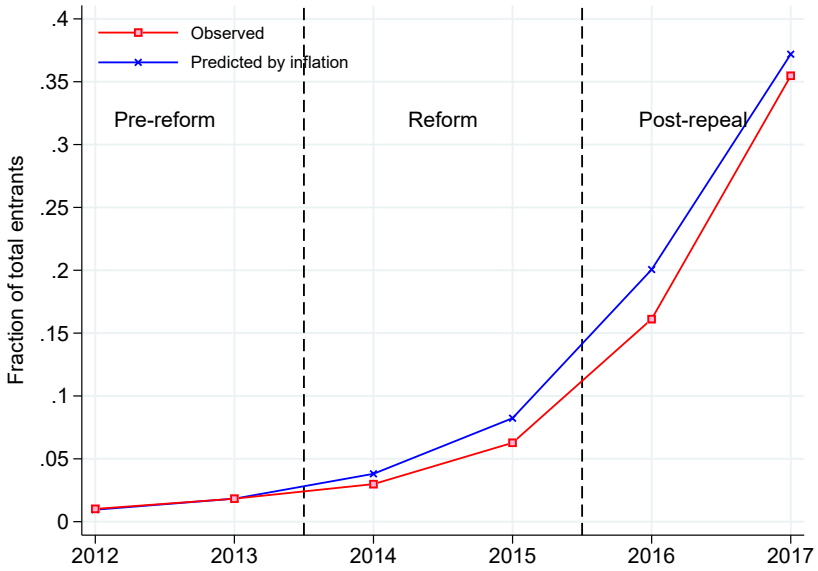
(b) Managers and executives



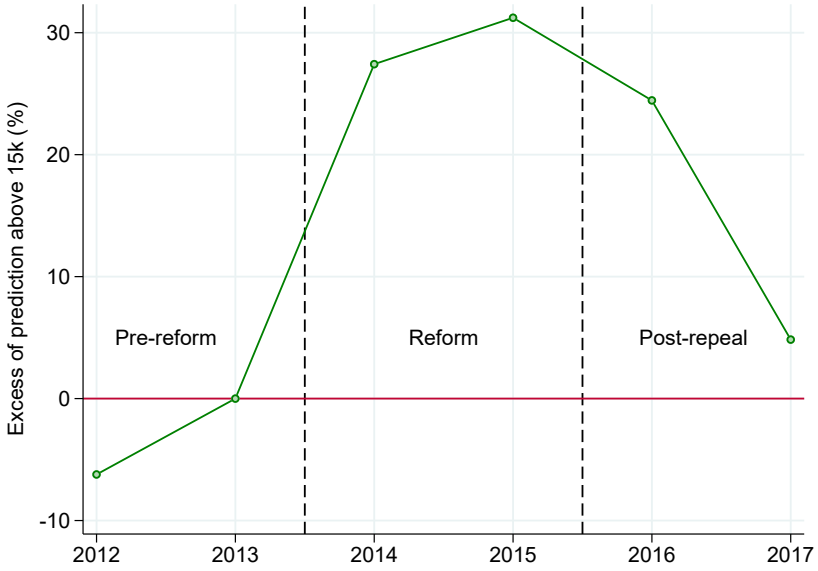
Notes: this graph plots the fraction of wage earners entering the labor market with their first monthly wage above AR$15,000 (red line) and above AR$10,000 (blue line). This fraction is computed at every month-year of our data. The sample of entrants consists of workers not present in the data between 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 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: 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 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. 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 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 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.

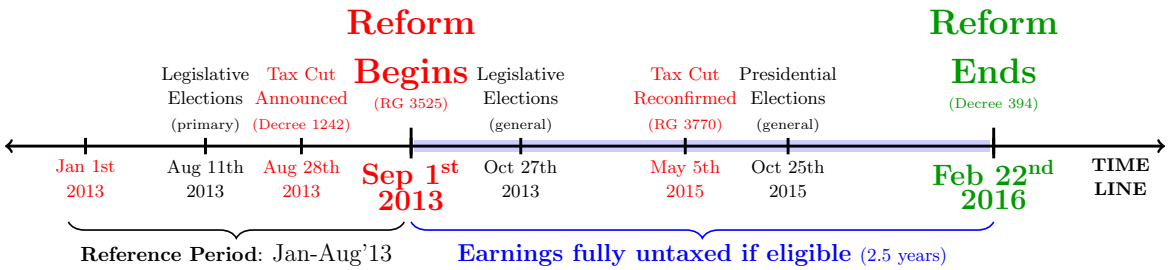
Table 5: Reduced forms, first stages, and elasticities (year 2015)

	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%.

A Additional Figures and Tables

Figure A1: 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.

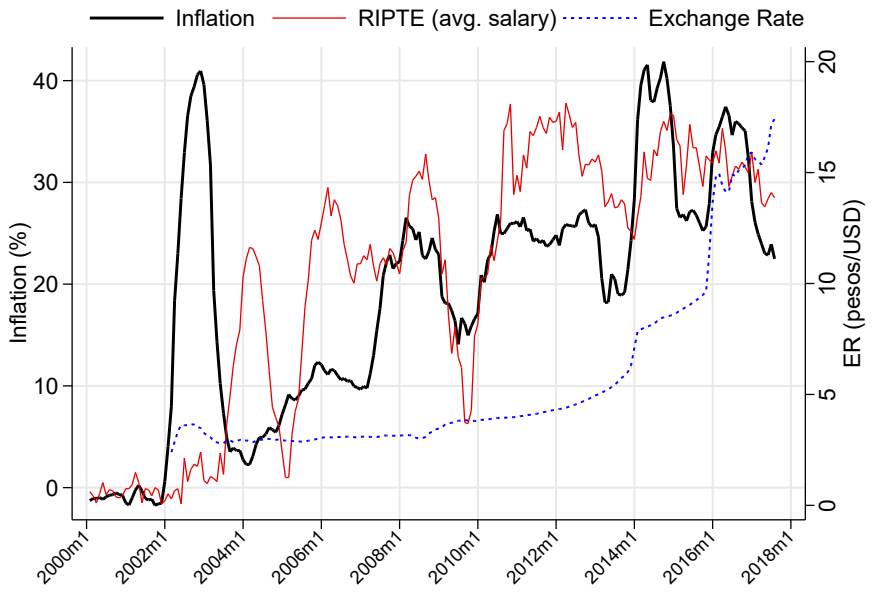
Figure: The two eligibility rules (incumbents and new entrants)

	Assignment Variable	Subsequent earnings untaxed if	Type of rule
EMPLOYED (in Jan-Aug'13)	$W_{max} \equiv$ Highest monthly salary between January-August 2013	$W_{max} \leq \$\ 15,000$	Backward-looking; Based on pre-reform earnings
NON EMPLOYED (in Jan-Aug'13)	$W_{1,t} \equiv$ First monthly salary	$W_{1,t} \leq \$\ 15,000$	Contemporaneous; Based on post-reform earnings

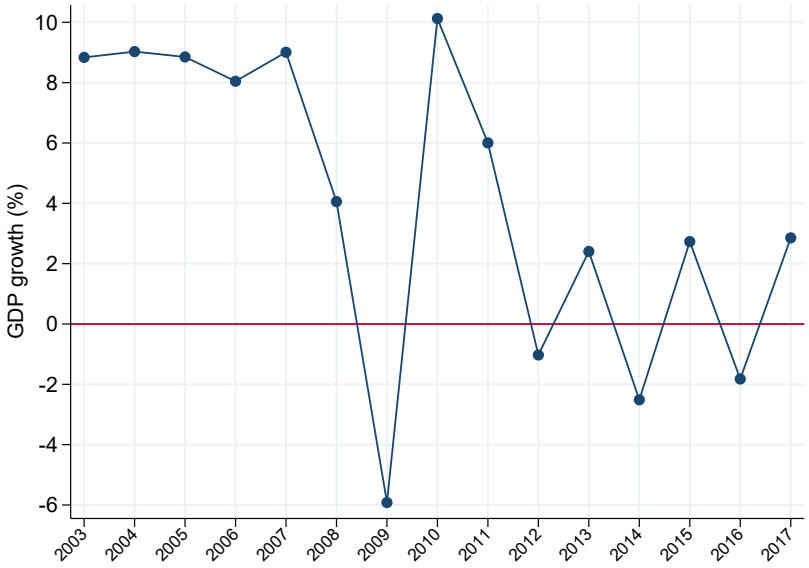
Notes: this figure summarizes the two eligibility rules that determine whether wage earners qualified for the tax holiday. The first row corresponds to workers with wage employment history between January and August 2013. These *incumbent workers* were tax-exempt if their *highest monthly wage between January and August 2013* was less than or equal to AR$ 15,000. The second row corresponds to workers without wage employment history between January and August 2013 that entered the labor force thereafter. These *new entrants* were tax-exempt if their *first monthly wage* was less than or equal to AR$ 15,000. In both cases, workers above the threshold continued to pay taxes normally. So, for the first group, the rule was based on *past* wage earnings; and for the second group, the rule was based on the wage paid in the *first* month of employment. That is, in both cases workers did not lose the benefit if subsequent monthly wages crossed AR$ 15,000 after August 2013. The tax exemption applied to their entire salary (i.e., zero marginal and average tax rates). The tax break only applied to wage earners. Independent workers are taxed under a different regime and did not benefit from the policy change. With an exchange rate of $\sim$5 in August 2013, 15k pesos correspond to 3k dollars.

Figure A2: Stylized Facts in Argentina 2000-2016

(a) **Inflation and Exchange Rate**

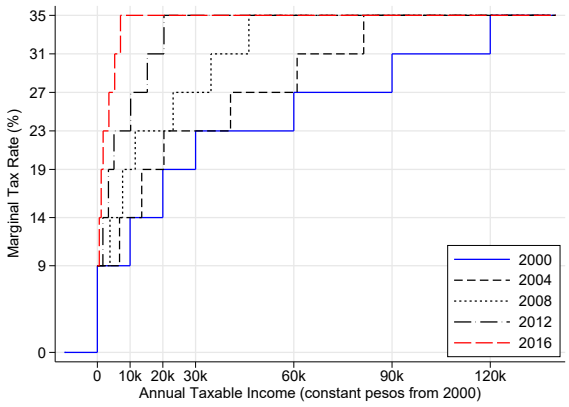


(b) **Business Cycle, 2003-2017**

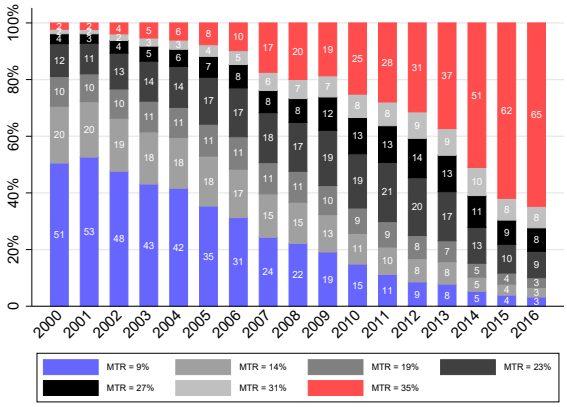


Notes: Panel (a) displays the annual inflation rate from The Billion Prices Project at MIT (Cavallo and Bertolotto, 2016), the average salary of registered workers (RIPTE, Remuneraciones Imponibles Promedio de los Trabajadores Estables) from the Minstry of Labor, and the exchange rate peso-dollar from 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.

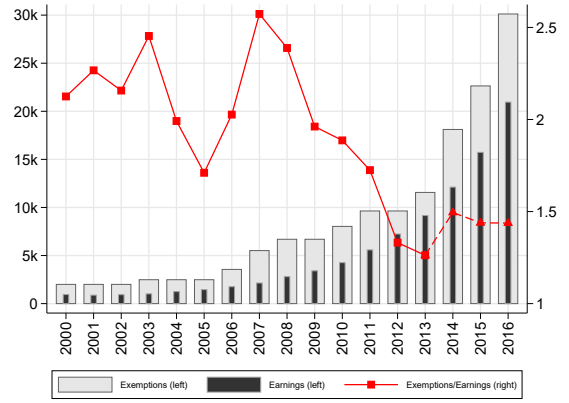
Figure A3: Stylized Facts in Argentina 2000-2016



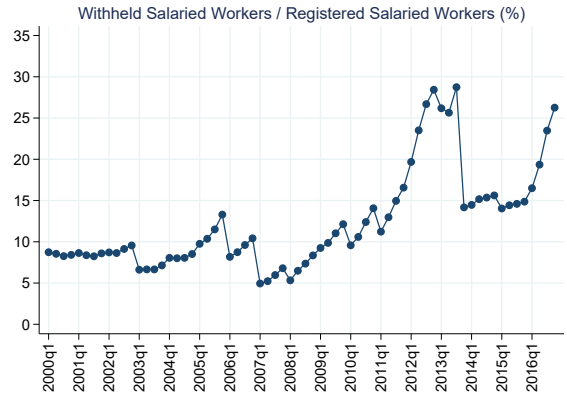
(a) **Annual Taxable Income and MTRs**



(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: taken from Tortarolo (2018).

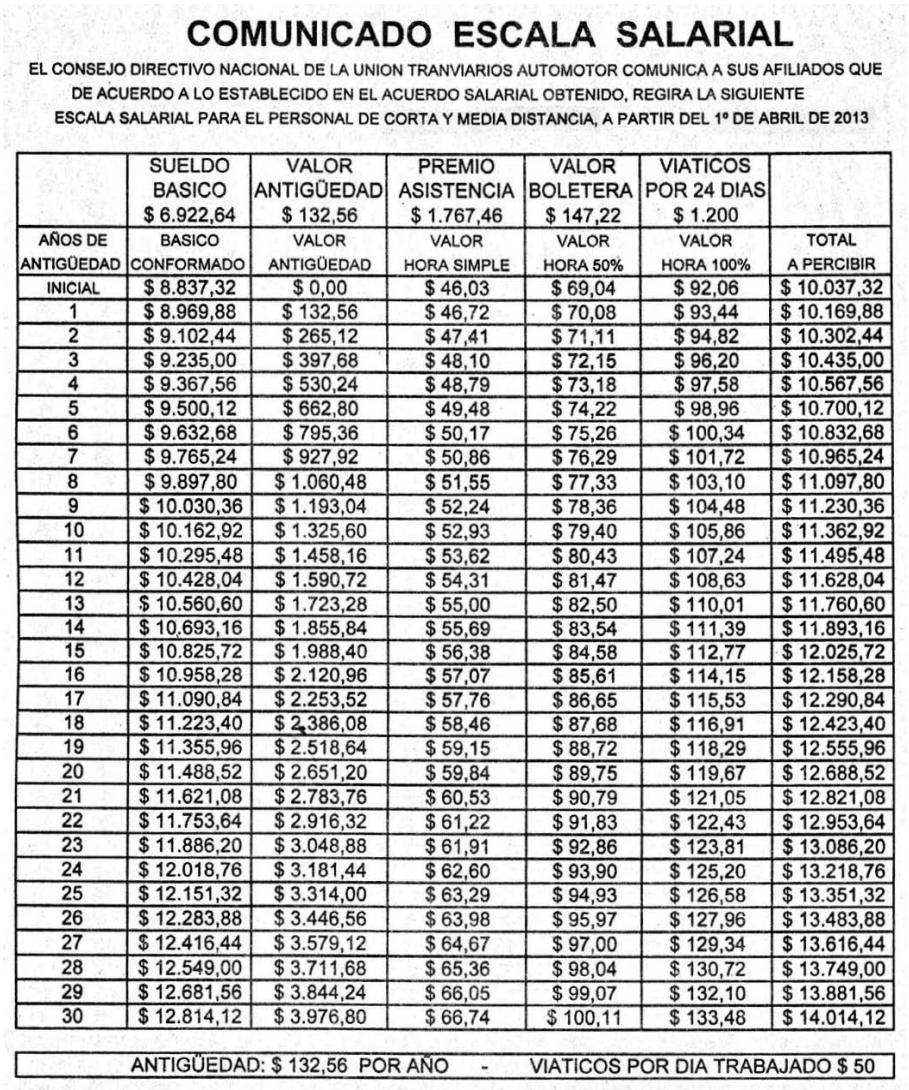
Figure A4: Pay scale for wage earners in the banking sector

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 A5: Pay scale for city bus drivers



COMUNICADO ESCALA SALARIAL

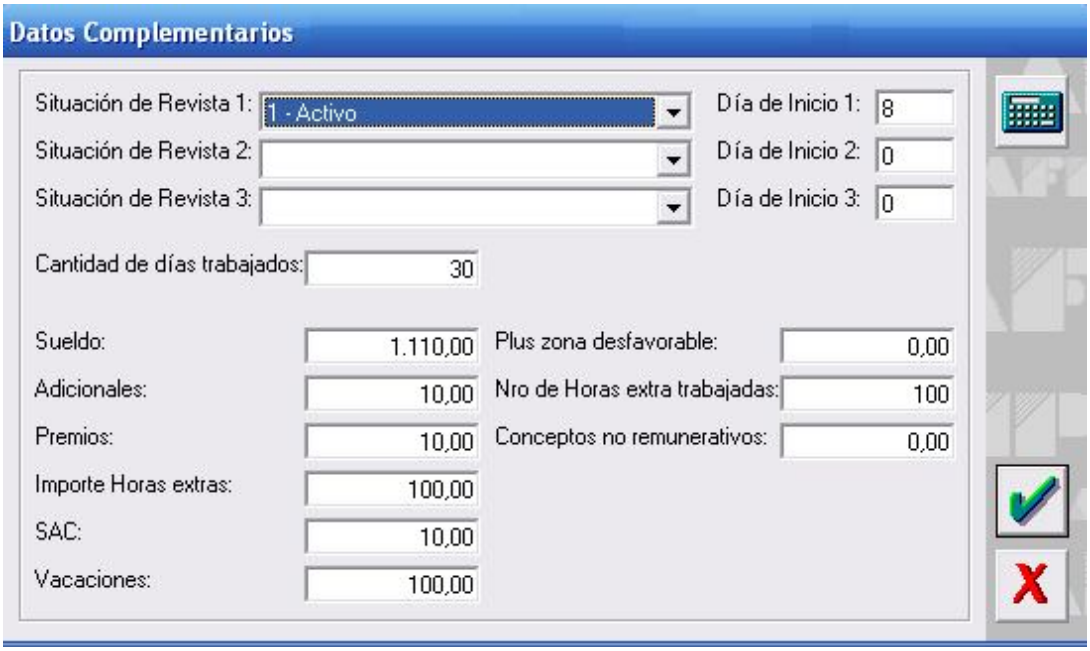
EL CONSEJO DIRECTIVO NACIONAL DE LA UNION TRANVIARIOS AUTOMOTOR COMUNICA A SUS AFILIADOS QUE DE ACUERDO A LO ESTABLECIDO EN EL ACUERDO SALARIAL OBTENIDO, REGIRA LA SIGUIENTE ESCALA SALARIAL PARA EL PERSONAL DE CORTA Y MEDIA DISTANCIA, A PARTIR DEL 1º DE ABRIL DE 2013

	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.

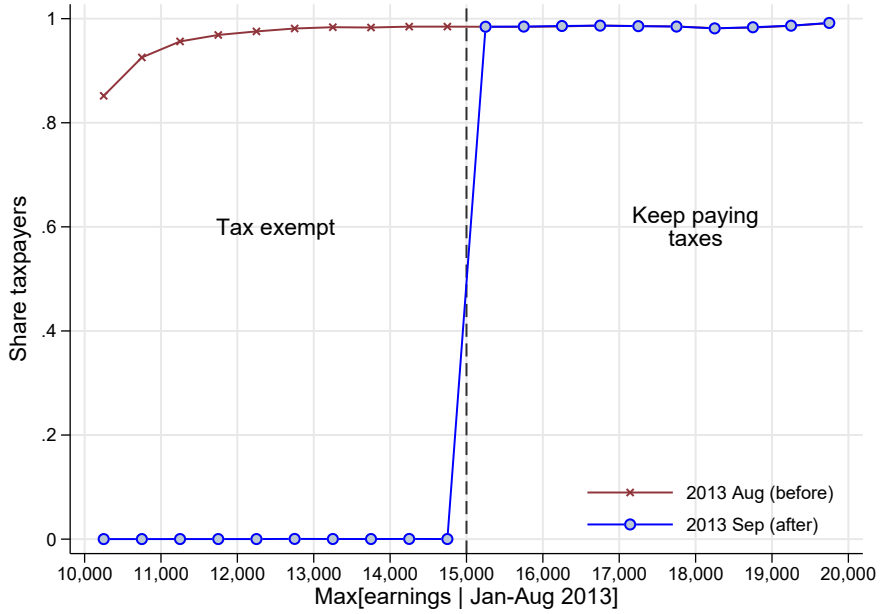
Figure A6: SSC filing software (Aplicativo SICOSS)



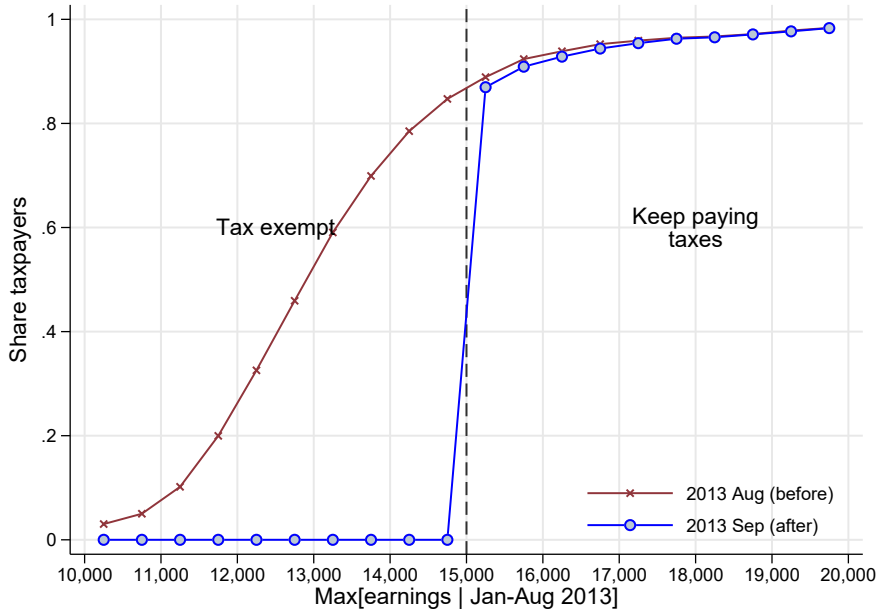
Notes: this figure provides a snapshot of the managing software used by employers to report monthly wage earnings of every worker in their payroll and to pay the associated social security contributions. This simple interface is the source of the core data used in the paper. The figure contains two panels. 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 A7: Fraction of salaried workers subject to the income tax

(a) Single workers without children



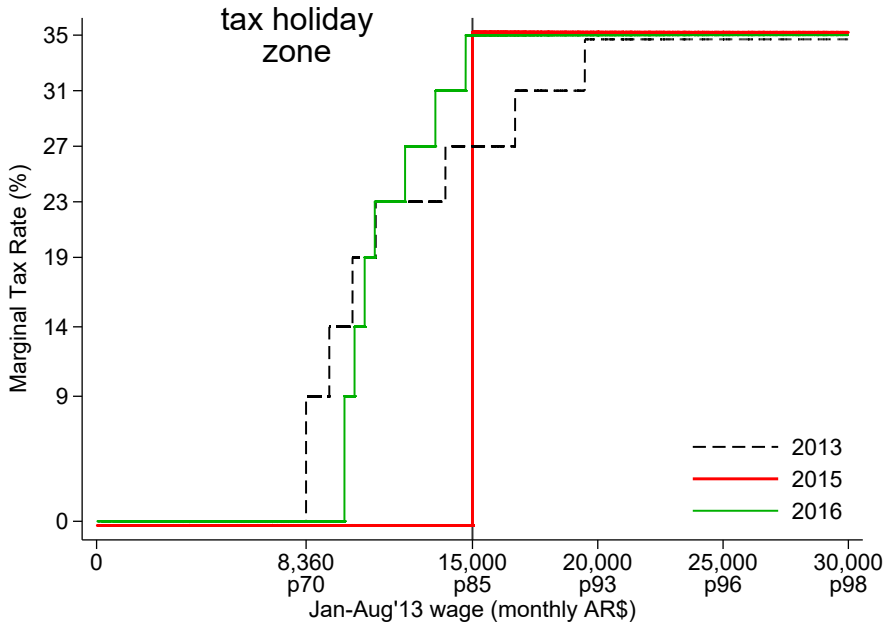
(b) Married workers with two children



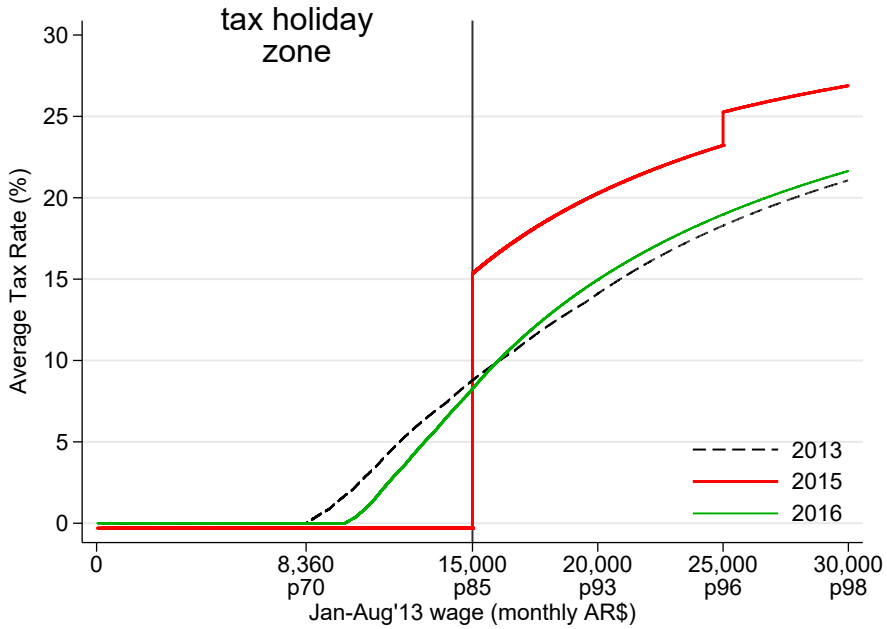
Notes: this figure plots the fraction of salaried workers subject to the income tax before and after the reform against the the running variable in the RDD for single workers without children (panel a) and married workers with two children (panel b). The vertical dashed line denotes the discontinuity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

Figure A8: First stage change in MTR and ATR - Single workers without children

(a) Marginal tax rates

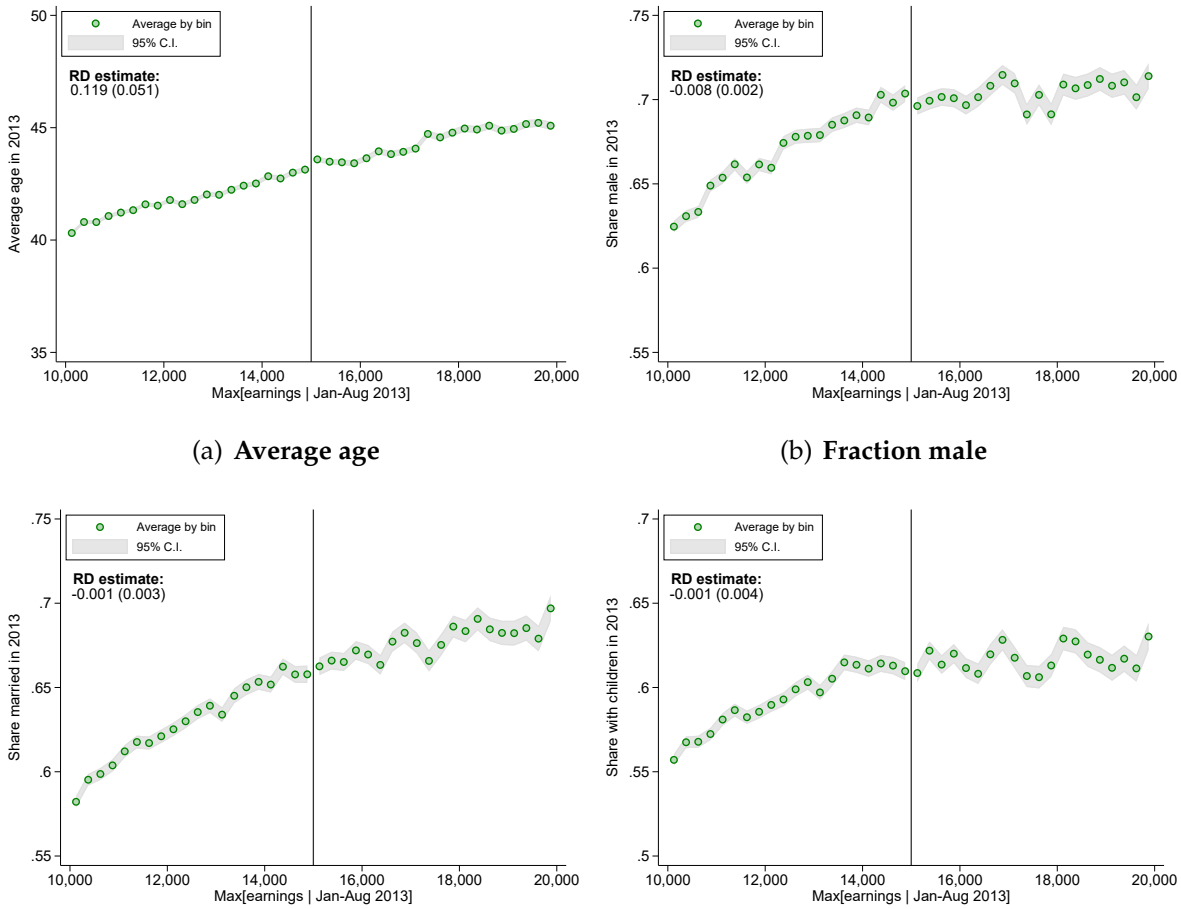


(b) Average tax rates



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.

Figure A9: Covariate balance around the discontinuity



(a) **Average age**

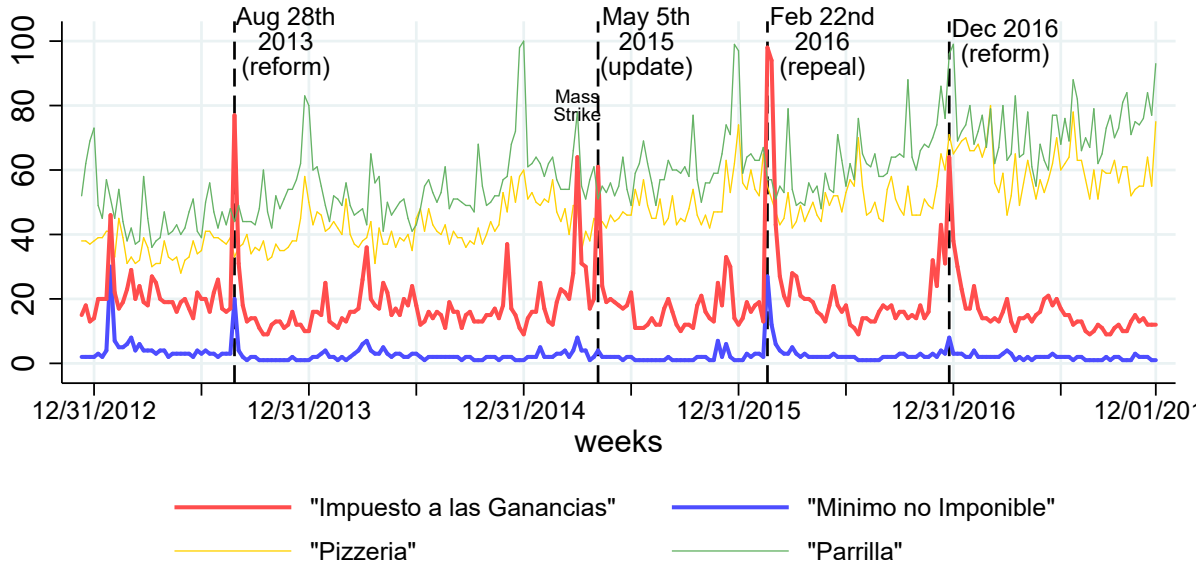
(b) **Fraction male**

(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 fours 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 A10: 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 A11: The reform covered by centre-right newspapers

POLÍTICA

MODIFICACIONES PRINCIPALES CON EL NUEVO RÉGIMEN

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

(a) August 28th, 2013

Clarín

Historia Time del siglo XX

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.

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.

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

Miércoles 1.04.2015

Rodríguez Larreta "Es un orgullo que Macri me apoye"

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

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.

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

Escriben Van der Kooy, Carpena y Bermúdez

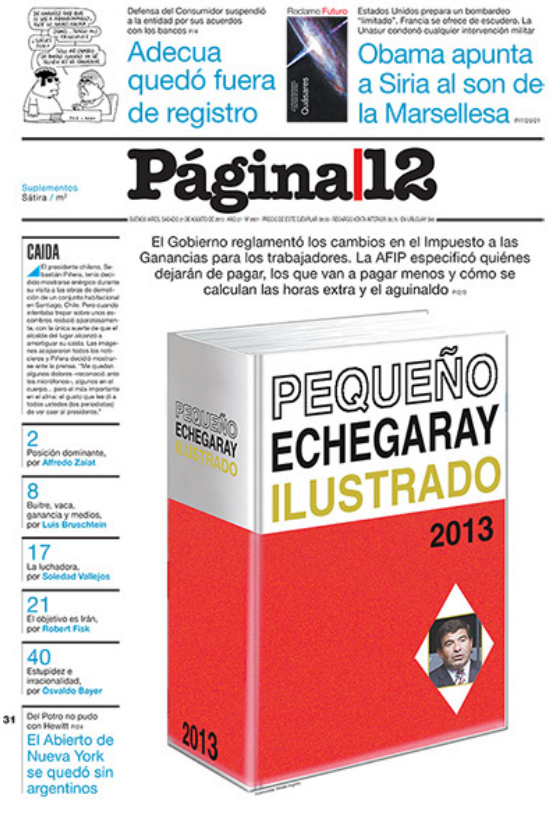
Sospechas de corrupción

(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 A11 (cont.): The reform covered by centre-left newspapers

(e) August 28th, 2013

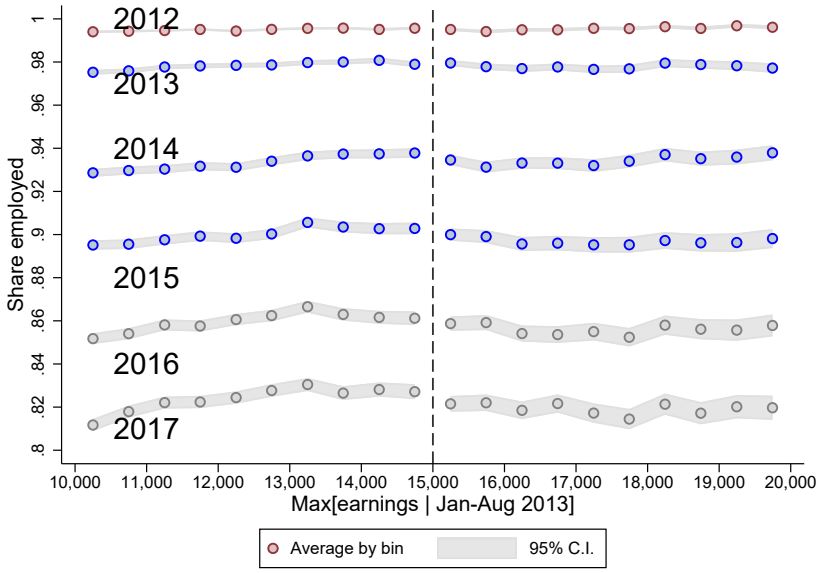


(f) August 31st, 2013

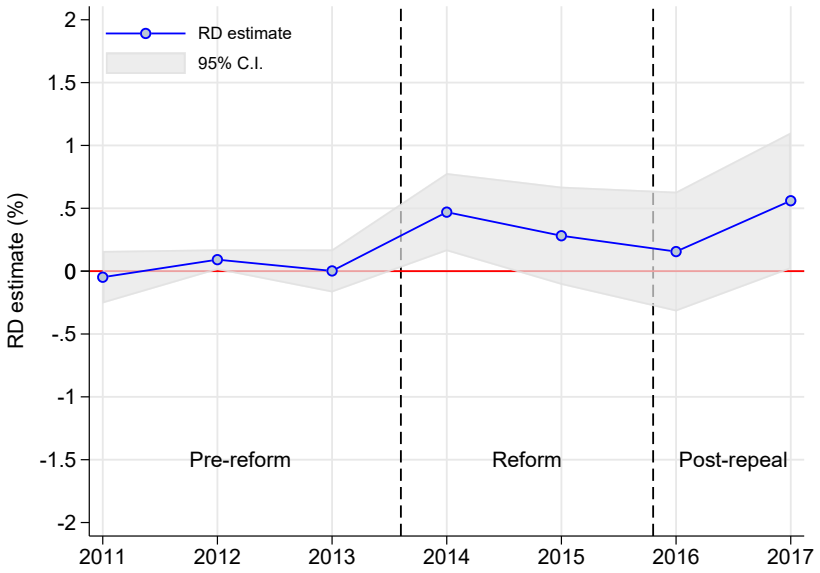
Notes: this picture shows the repercussion that the income tax change had in the main centre-left newspaper of Argentina, *Diario Página|12*. Panel (e) reads: "up to AR$ 15,000 you don't pay", and explains that the announcement made by the President takes effect immediately starting on September 1st 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 A12: Fraction of wage earners that remain employed

(a) **Averages by bin**



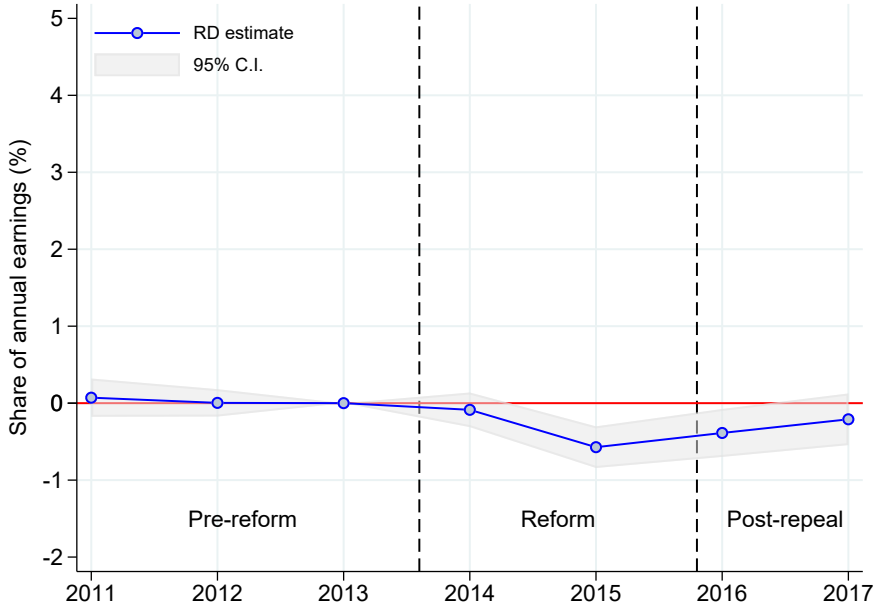
(b) **RD estimates**



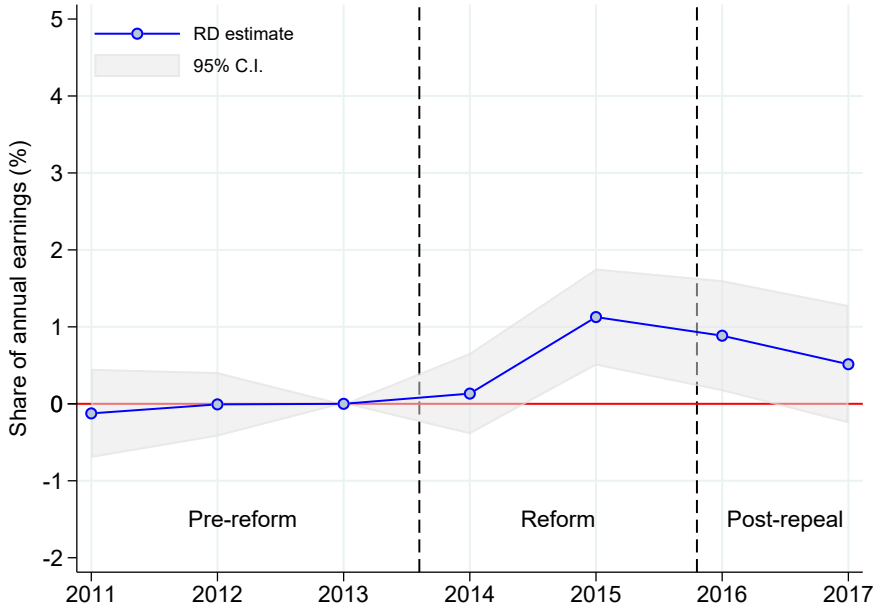
Notes: this figure plots the fraction of wage earners that remain employed by bins of the running variable (panel a) and the evolution of the RD estimates (panel b). The dependent variable is an indicator for whether the worker has positive wage earnings by December of each year. Averages are computed 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 A13: Evolution of RD estimates for base salary and residual compensation

(a) Base salary



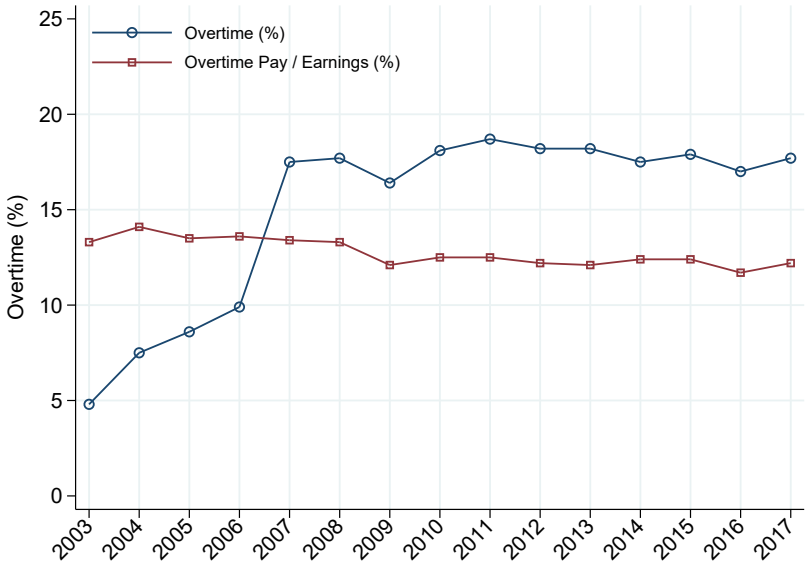
(b) Residual (total wage earnings - base salary)



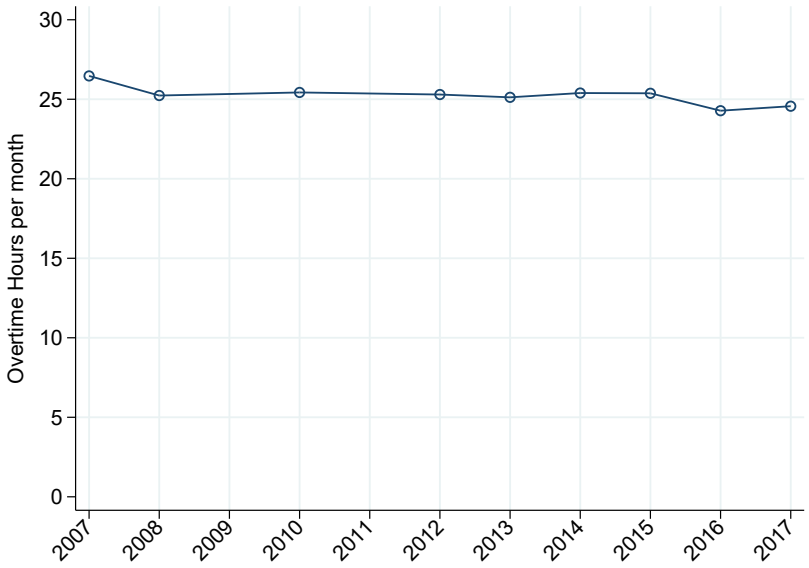
Notes: this graph plots the evolution of the RD estimates for base salary and residual compensation as a share of total compensation. Each dot corresponds to a separate RD regression using a linear fit on each side of the discontinuity, a triangular kernel, and a AR$3,000 bandwidth. The dependent variable 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 A14: Descriptives statistics for overtime work

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



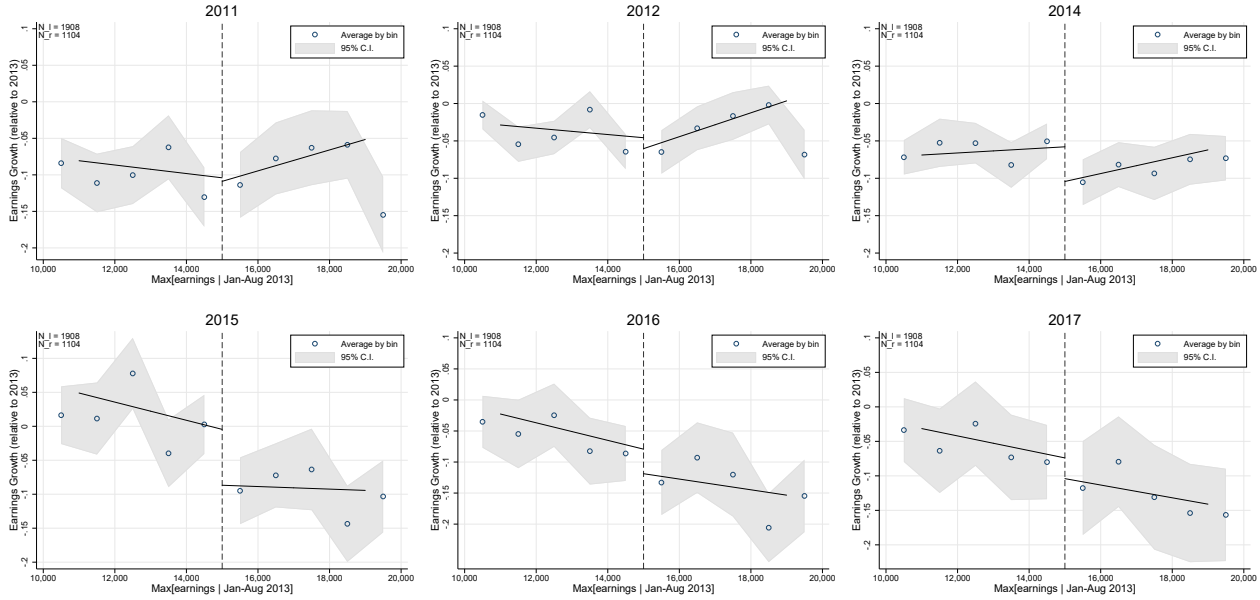
(b) **Overtime hours per month**



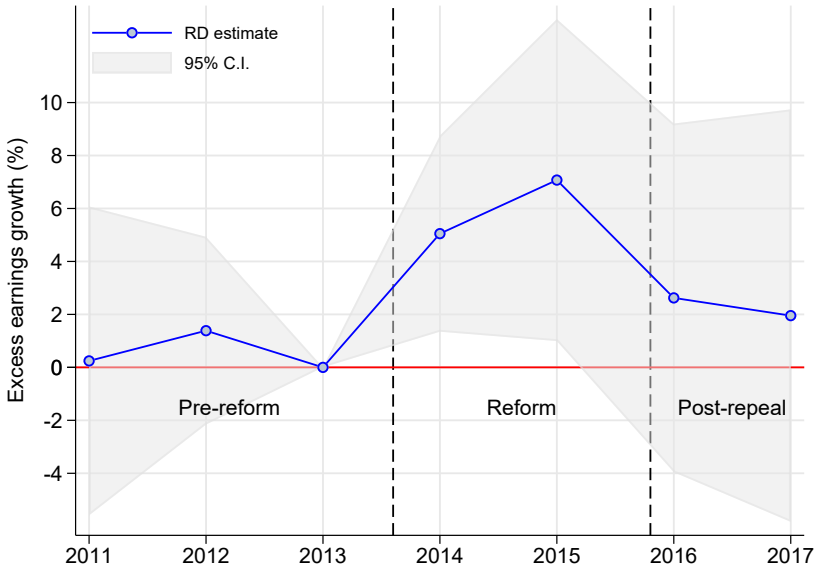
Notes: these figures show time series of three overtime outcomes for the universe of wage earners in Argentina during the period 2003-2017. Panel (a) reports the fraction of workers doing overtime (blue line) and the participation of overtime pay in total wage earnings (red line). Panel (b) shows average 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 A15: RD estimates for executives and managers

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

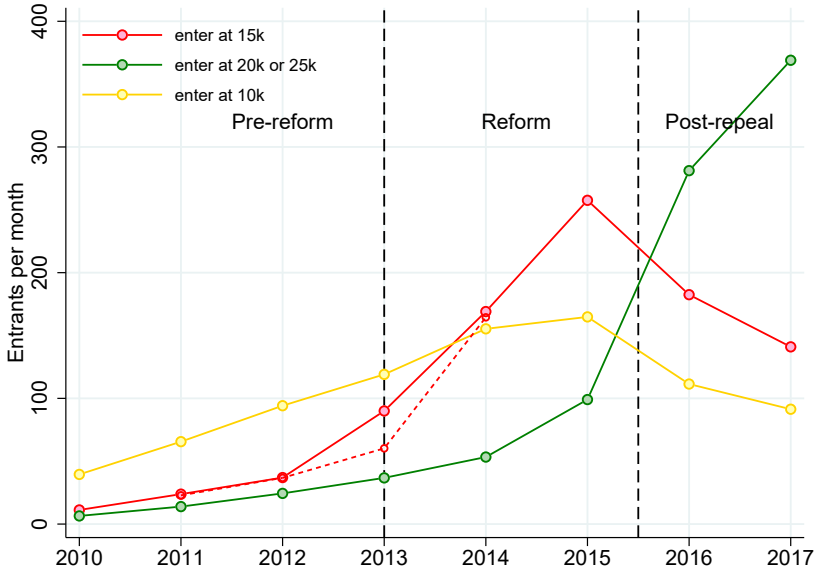


(b) **Evolution of RD estimates**



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.

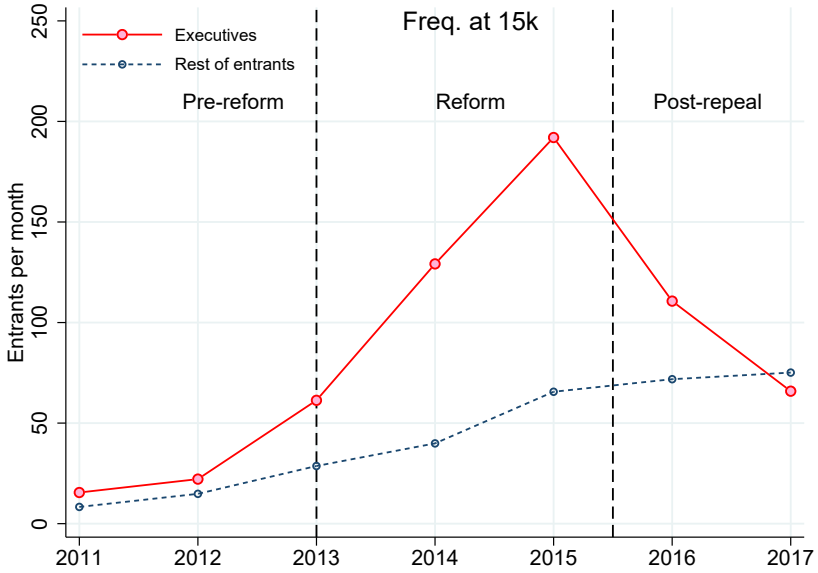
Figure A16: Entrants with initial monthly salary exactly at 10k, 15k, 20k, and 25k



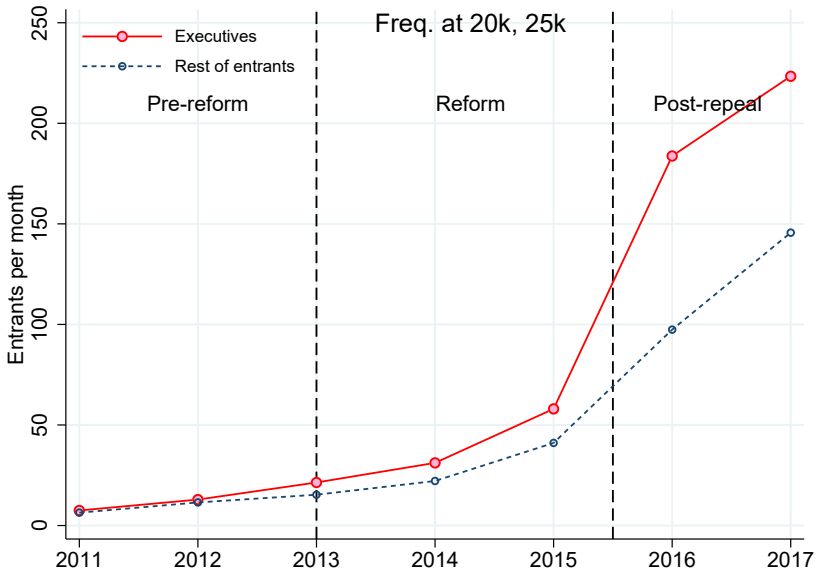
Notes: this graph plots the number of wage earners entering exactly at 10k (yellow line), 15k (red line), 20k or 25k (green line) for three years pre-reform, two years during the reform, and two years post-repeal. We first count the number of wage earners entering exactly at a focal point in each month 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 A17: Entrance at focal points: executives vs rest of entrants

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

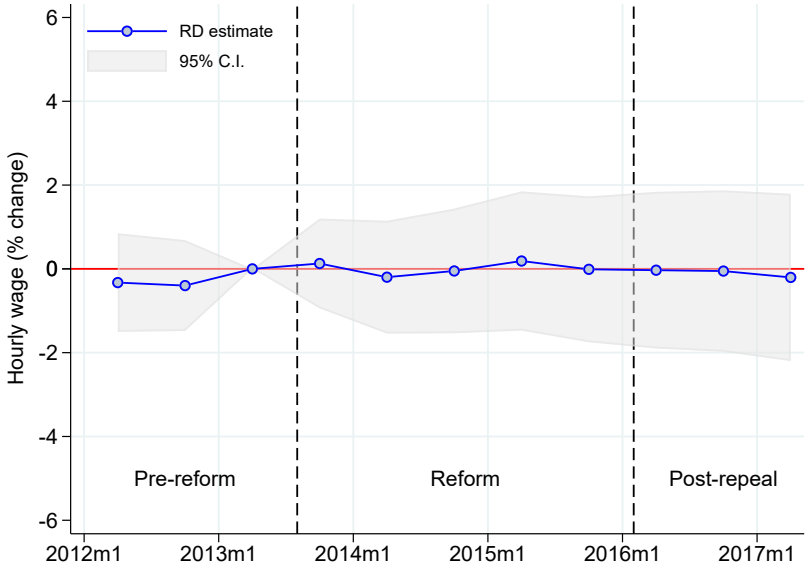


(b) Entering at 20k or 25k (tax zone)



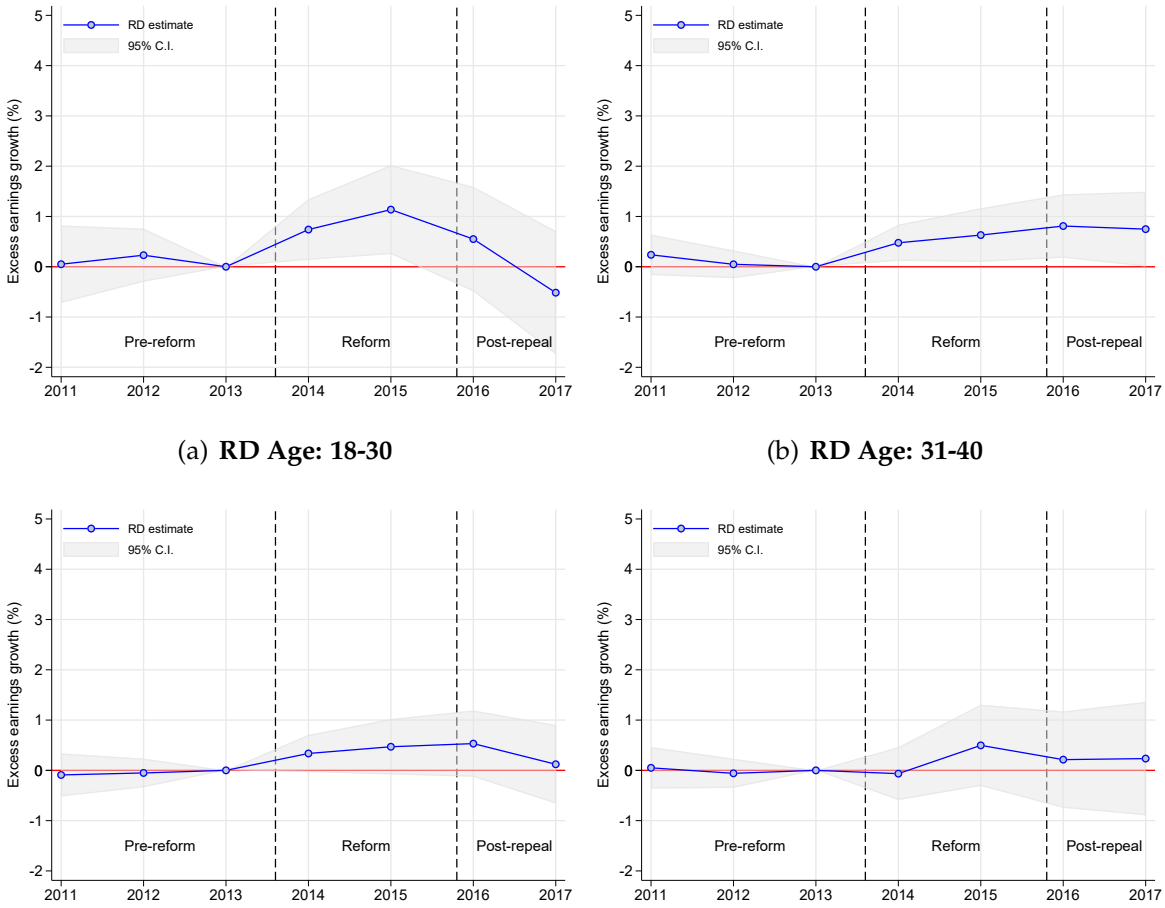
Notes: this figure shows counts of wage earners entering at focal points broken by executive workers (red line) versus rest of entrants (dashed blue line). Panel (a) corresponds to entry at 15k (tax exempt during the reform years) and panel (b) corresponds to entry at 20k or 25k (tax liable). The vertical dashed lines indicate the beginning and the end of the tax holiday. For more details see Figure A16.

Figure A18: 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 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 A19: A test for income and substitution effects that cancel out



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

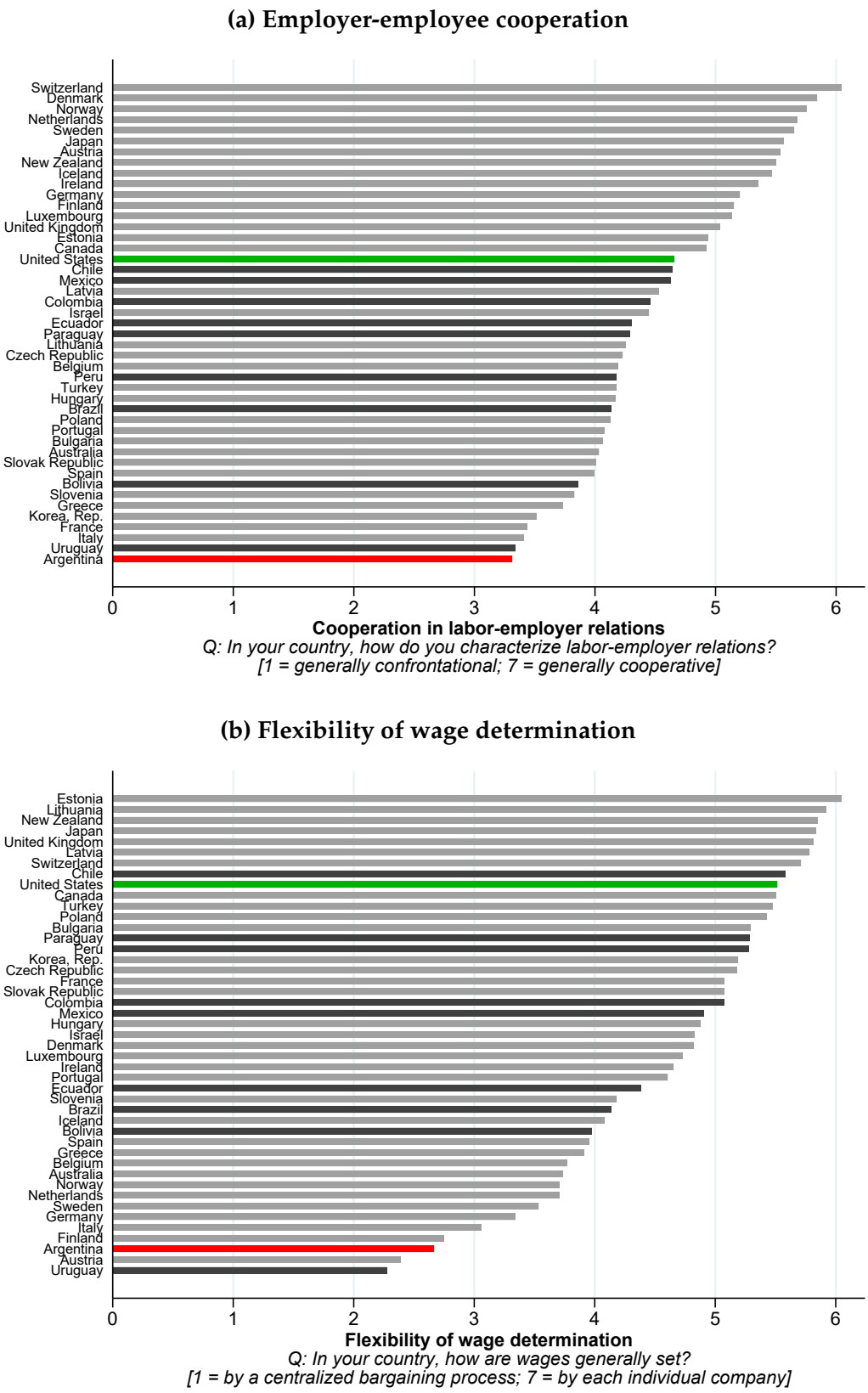
(b) **RD Age: 31-40**

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

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

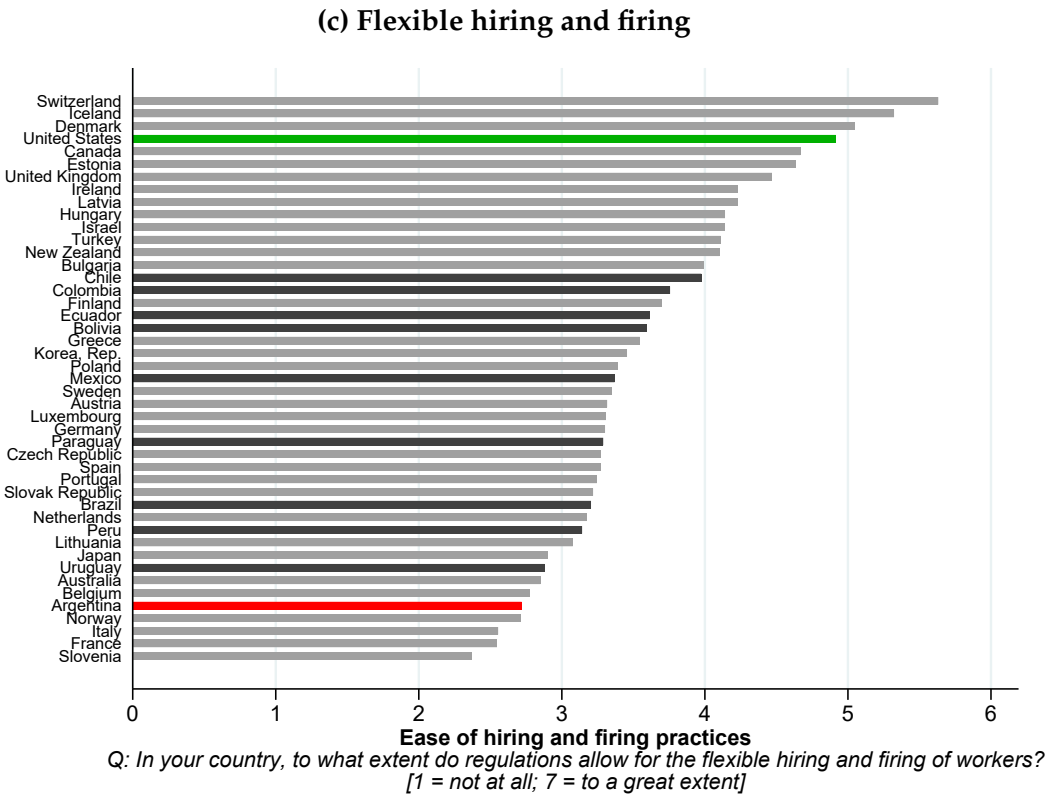
Notes: this graph plots the time series of RD estimates computed as in Figure 6. Panel (a) corresponds to workers ages 18-30; Panel (b) corresponds to workers ages 31-40; Panel (c) corresponds to workers ages 41-50; Panel (d) corresponds to workers older than 50. The vertical dashed lines indicate the 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).

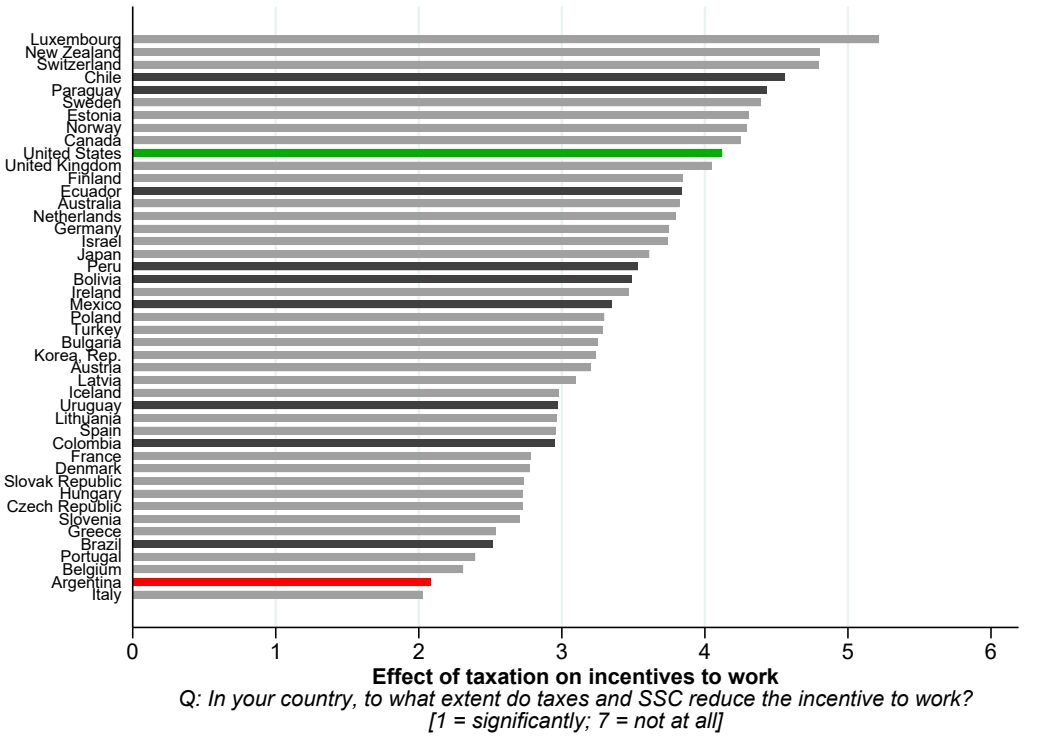
Figure A20: Labor market rigidities across the globe



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.

Figure A21: 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.

B 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.[57]

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.[58]

B.1 Non-classical measurement error in the assignment variable

We formalize this potential issue by adapting the framework developed by Battistin et al (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 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

[57] We are very grateful to Zhuan Pei for helpful feedback on this section.

[58] 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.

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).[59] 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}).[60]

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:

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

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

[59] The nature of our measurement error is related to the treatment given by Card et al (2015) for the fuzzy RKD case. This is also known as the *contaminated sampling model* (Horowitz and Manski, 1995).

[60] The fact that our assignment variable does not suffer from classical measurement error is critical, as Pei and Shen (2016) 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.

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: $\beta \times Pr[T = 0|\bar{c}_-]$.

B.2 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 A22 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.[61] 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.

B.3 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.[62]

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.[63] 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.

[61] 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.

[62] 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.

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

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 A23, 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 A24 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 A25 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.[64]

Figure A27 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.[65]

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).[66] In addition, we restrict the analysis to workers that were withheld at

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

[65] The reason why some dots below the threshold are not affected by the tax before the reform is because they have a higher family size and thus can subtract more personal exemptions from the tax base.

[66] 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

least one month before the reform. With this sensible conditions, our sample goes from 700 to 115 workers. In Figure A28 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.[67]

In Figure A29 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 A30 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 A27, 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 A31 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-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.

References

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Card, D. , Lee, D. S., Pei, Z. and Weber, A. (2015), "Inference on Causal Effects in a Generalized Regression Kink Design." *Econometrica*, 83: 2453-2483. doi:10.3982/ECTA11224

Horowitz, J. L., and C. F. Manski (1995). "Identification and Robustness with Contaminated

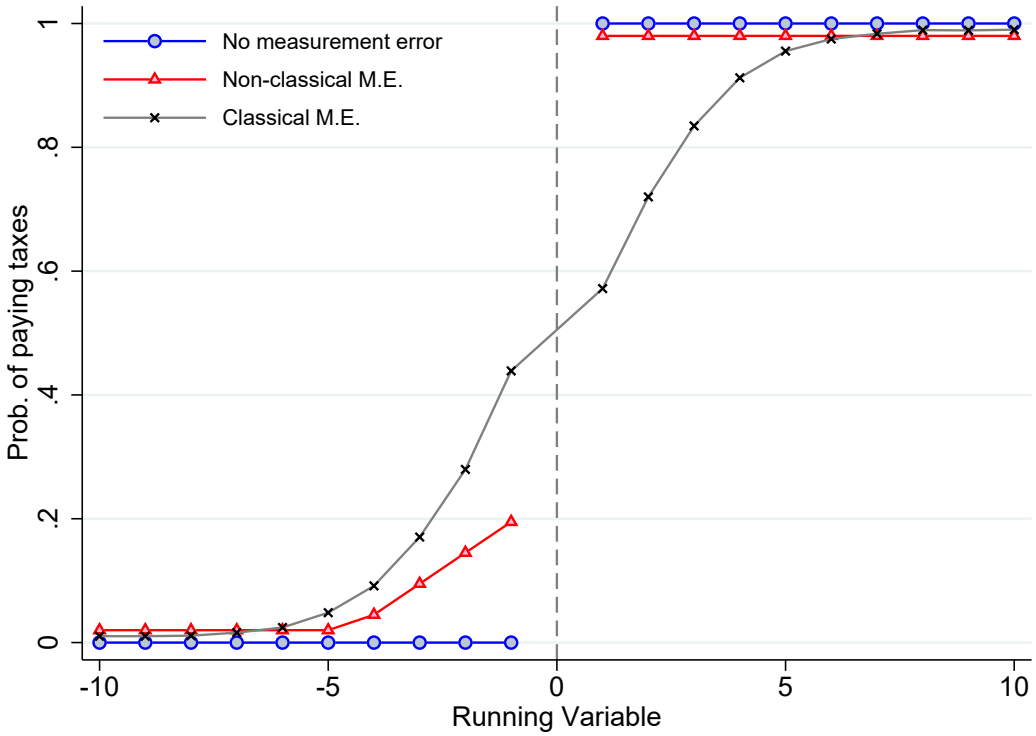
mostly include faculty and administrative employees (52 and 38 percent, respectively).

[67] 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.

and Corrupted Data." *Econometrica*, 63, 281-302.

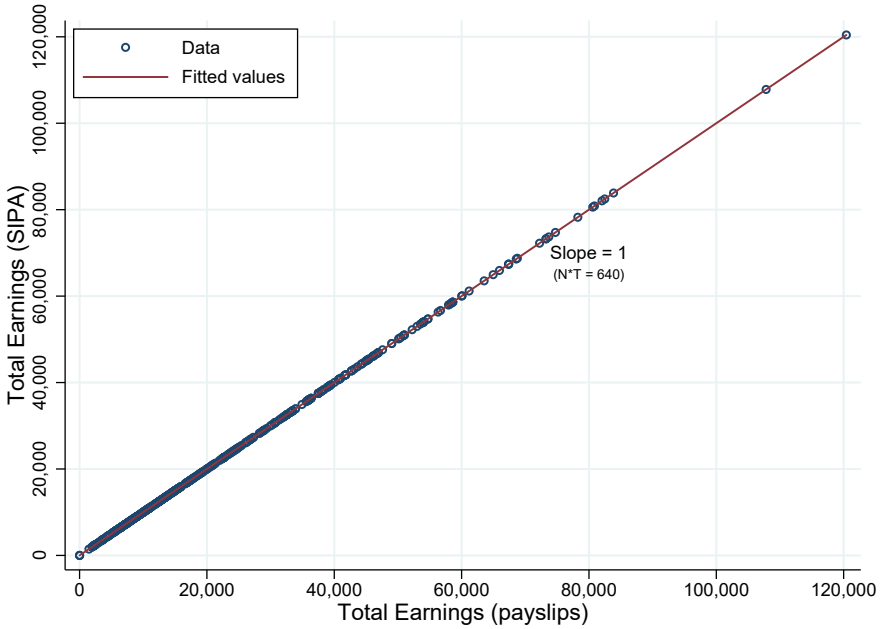
Pei, Z. and Y. Shen (2017). "The Devil is in the Tails: Regression Discontinuity Design with Measurement Error in the Assignment Variable." *Advances in Econometrics*, vol 38 (Regression Discontinuity Designs: Theory and Applications) edited by M. D. Cattaneo and J. C. Escanciano: 455-502.

Figure A22: Simulation: measurement error in the assignment variable



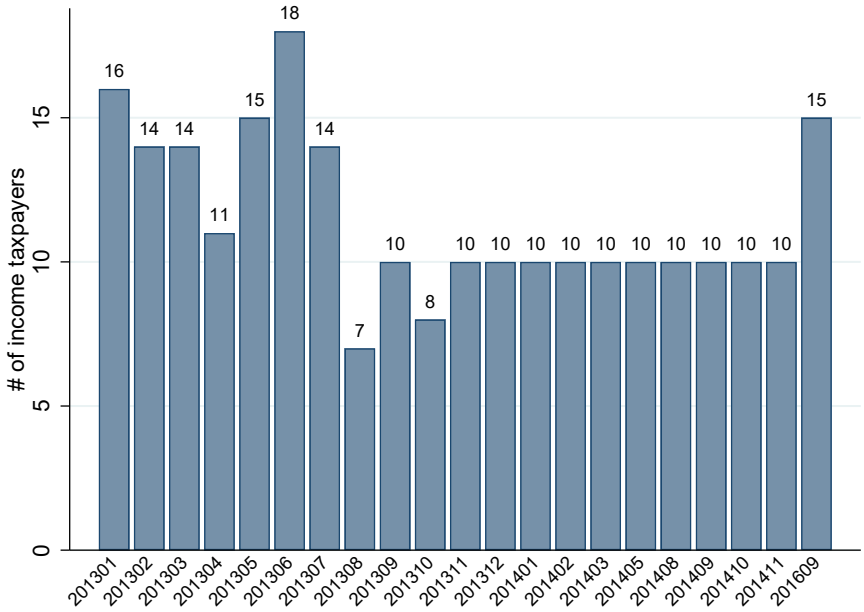
Notes: this simulation provides a visual illustration of how measurement error in the running variable affects the first stage of the RDD. The figure displays three cases: (1) under *no measurement error* in the assignment variable (blue line), workers above the threshold keep paying the tax normally 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 A23: Quality of the data: IRS vs *Firm #1*



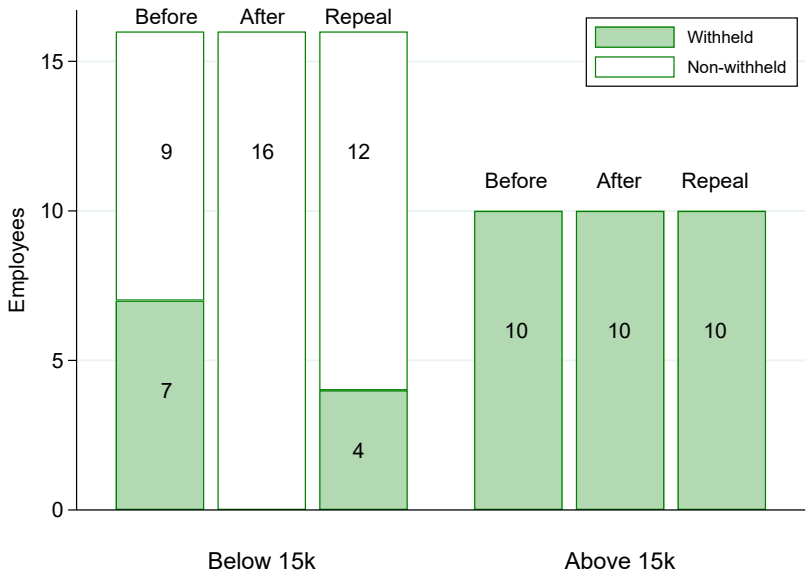
Notes: this figure plots total earnings reported by *Firm #1* to the IRS (vertical axis) against the sum of earnings from the payslips provided by the firm (horizontal axis). Each dot corresponds to an worker-year-month observation. The graph shows that the information from both sources is perfectly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

Figure A24: Withheld workers per month (*Firm #1*)



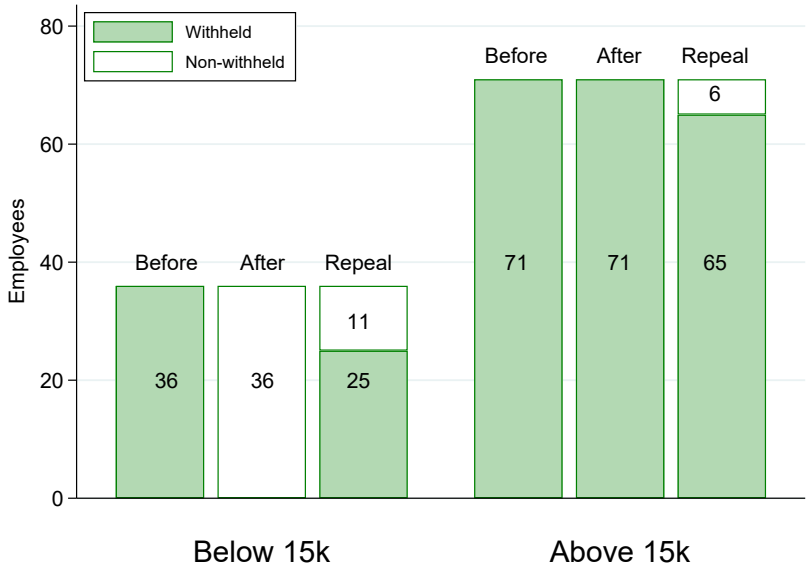
Notes: this figure presents the number of employees that are withheld at source every month. It spans the period before the reform, some months during the tax holiday, and one month after the repeal.

Figure A25: 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.

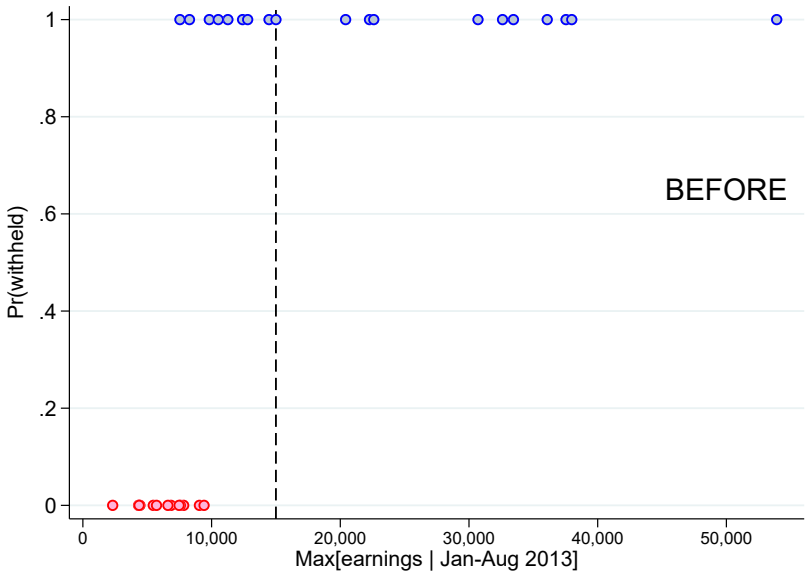
Figure A26: Withheld workers pre/post/repeal (*Firm #2*)



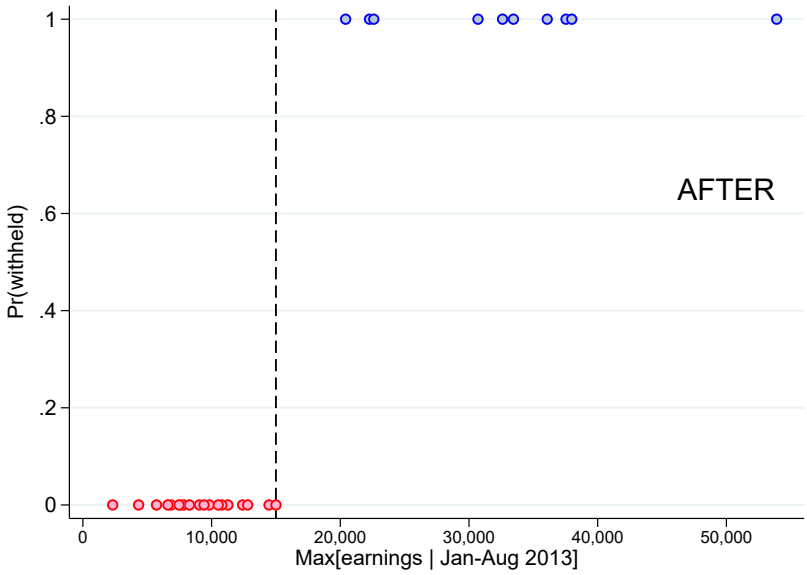
Notes: this figure is the equivalent to Figure A25 but for *Firm* #2. 36 workers below the discontinuity became untaxed in 2013 and 25 of them were hit by the tax when it was repealed in 2016. The 71 workers above the discontinuity kept paying the tax normally during the whole period.

Figure A27: Withheld workers pre/post reform (*Firm #1*)

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

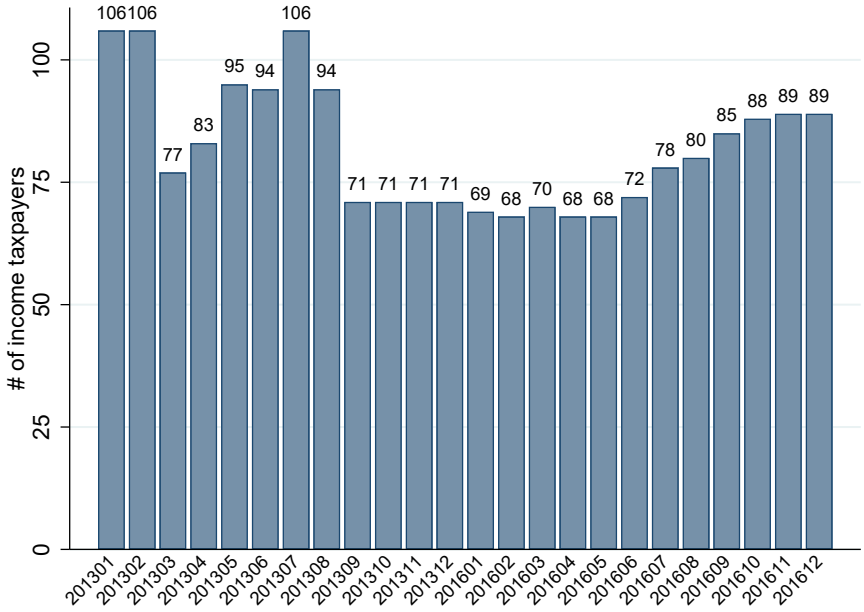


(b) $Prob(withheld = 1|post)$



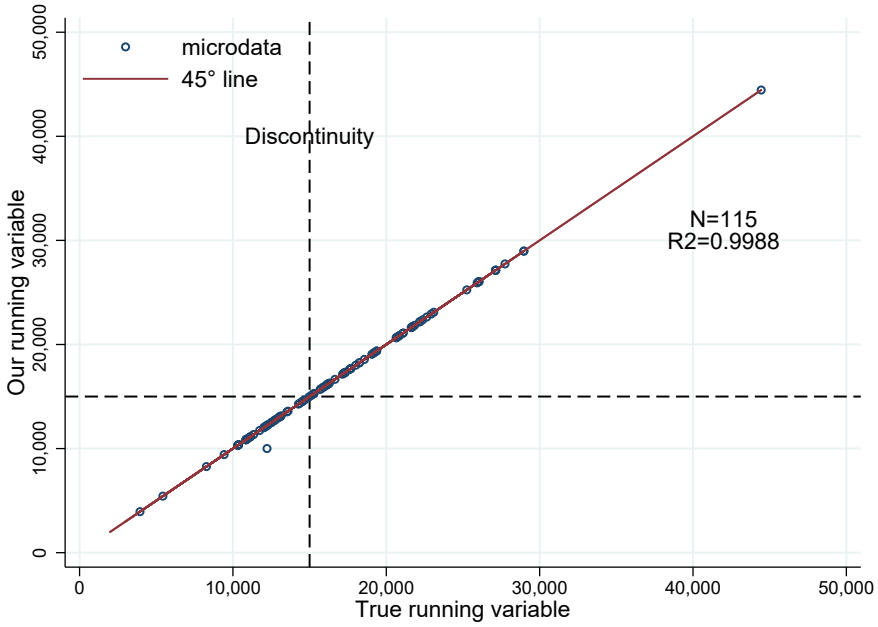
Notes: this figure shows the position of every individual on the running variable (horizontal axis) and the probability of paying the income tax (vertical axis) before and after the reform. Panel (a) considers any month between January and August 2013, and panel (b) considers any month between September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

Figure A28: 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.

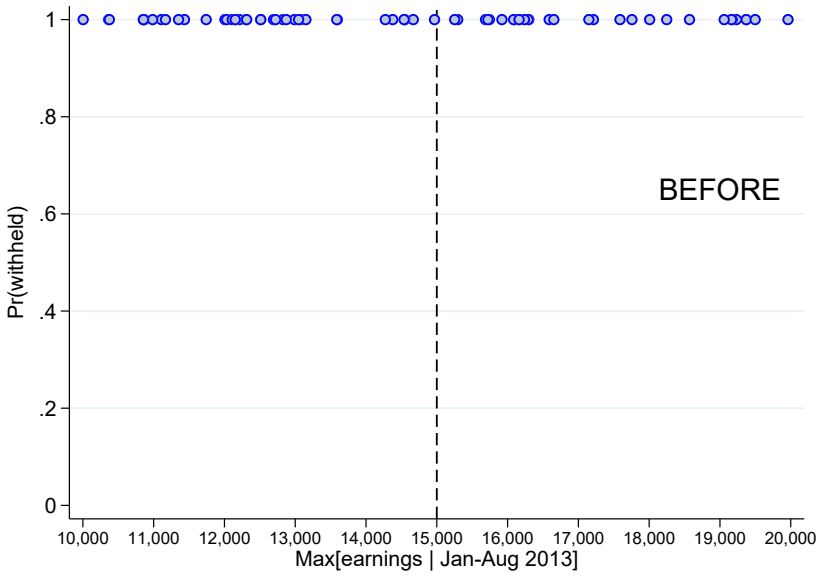
Figure A29: OWN vs TRUE running variable (*Firm* #2)



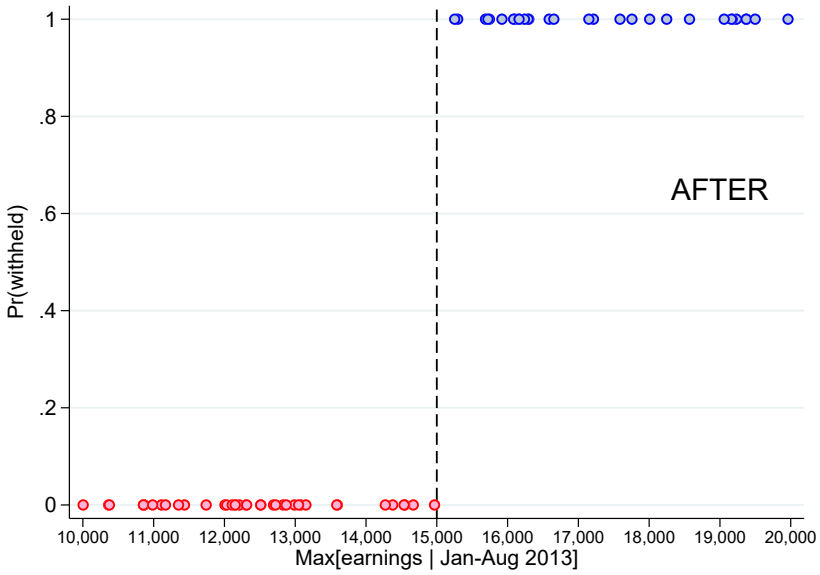
Notes: this figure compares our *measured* running variable (vertical axis) and the *true* running variable used by *Firm* #2 (horizontal axis). The sample includes 115 workers that were subject to the income tax during 2013. This graph is important as it states that almost all the observations fall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

Figure A30: Withheld workers pre/post reform (*Firm #2*)

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



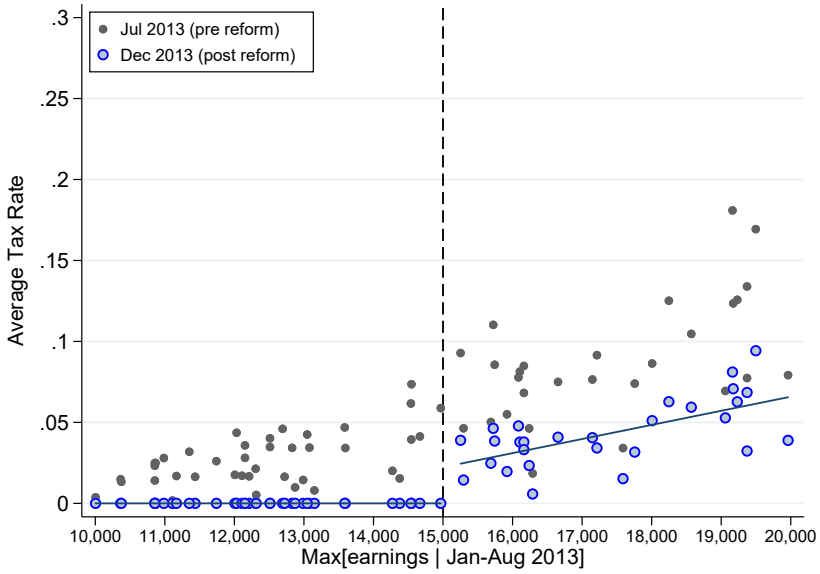
(b) $Prob(withheld = 1|September, October)$



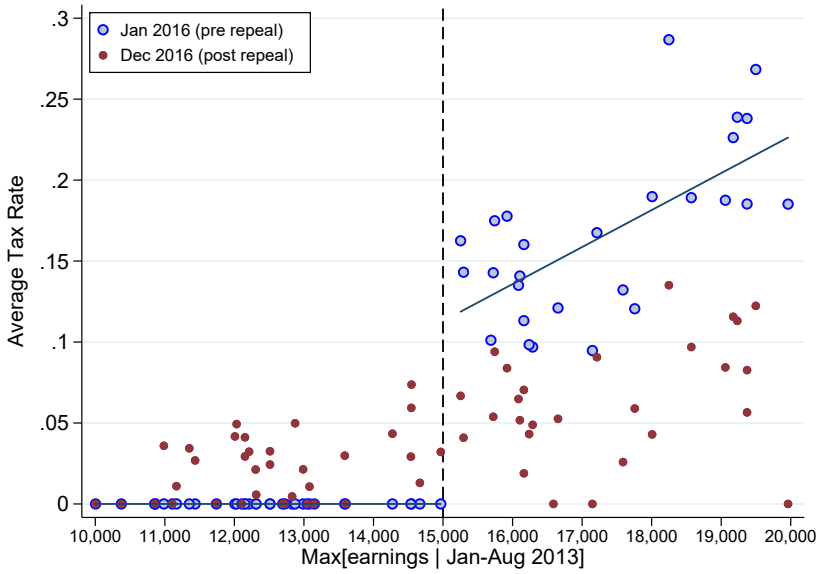
Notes: this figure shows the position of every individual on the running variable (horizontal axis) and the probability of paying the income tax (vertical axis) before and after the reform. It uses our *measured* running variable and the income tax status as reported by the firm. Panel (a) is computed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

Figure A31: 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.

C 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 A32 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.[68] Before the reform, a worker with these characteristics and gross monthly earnings greater than AR$ 8,360 was subject to the income tax.[69] 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.[70]

Figure A32 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 bunching at the first kink (if any) to decrease substantially.

Figure A32 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.[71]

[68] 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.

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

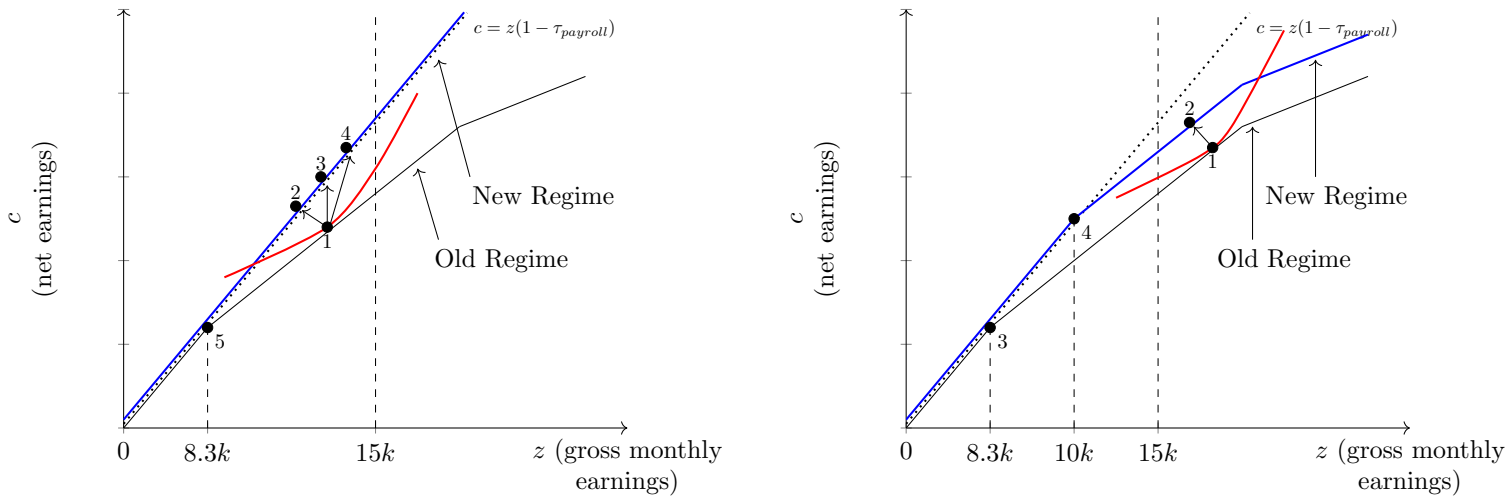
[70] In Figure A8 below we overlap the tax schedule and corresponding marginal tax rates to the distribution of gross earnings in 2013.

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

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.

Figure A32: Conceptual Framework. Single worker without children



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

(b) **Case 2**: $15k <$ max{*earnings*|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.