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Essays on Public and Development Economics

by

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Professor Emmanuel Saez, Chair
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Abstract

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This dissertation studies the effect of several policy interventions on employment, informality, tax evasion, and well-being in developing countries. Specifically, it studies the effects of pension privatization, regulations limiting lawsuits due to workplace accidents, and changes in payroll tax rates. To provide causal evidence, it collects rich microdata and leverages several sources of quasi-experimental variation.

The first chapter, coauthored with Marcelo Bérgolo, studies the effects of the privatization of the pension system on workers' reported earnings, employment and retirement behavior, and income in old age. We analyze a reform to the pension system in Uruguay that transitioned from a pay-as-you-go system with defined benefits into a mixed system, in which a fraction of social security contributions is used to fund the pay-as-you-go system and the remaining fraction is allocated to individual retirement accounts. For identification, we leverage a cohort-based discontinuity in the introduction of the new mixed system with regression discontinuity analyses, using rich administrative and census data. We find significant labor supply responses to the privatization on multiple dimensions. First, workers in the system with private retirement accounts are significantly more likely to be employed in their fifties. This effect is driven partially by lower rates of early retirement, with effects concentrated among individuals of low wealth and those who have mild disabilities. Second, workers in the system with retirement accounts report significantly higher earnings early-on in their careers, and we find suggestive evidence that this is due to a reduction of tax evasion. Regarding income in old age, we find little differences on income and poverty rates across the two systems in early old age. However, two decades after the privatization the government gave workers the option to reverse back to the non-privatized system, and we find that a significant share chose to, especially among those who did not choose the most profitable retirement savings option and those with career profiles that favor defined benefits formulas. Overall, our evidence suggests that pension privatization can boost labor supply in old age and have the unexpected benefit of increasing tax compliance, but it can have detrimental effects on the pension income of some workers, which can partially explain some of the push to roll back privatizations in several countries over the last two decades.

In the second chapter, coauthored with Damián Vergara, we study a reform to the workers' compensation system in Argentina that, after a workplace accident, mandated workers to go through a government medical commission that determines the degree of disability, whether the injury happened in the workplace, and the corresponding compensation, before additional legal actions can be taken. Leveraging the staggered implementation of the reform across provinces, we find that the reform substantially reduced workplace lawsuits with no effects on reported accidents. Employment increased by more than 5% one year after the reform in highly exposed industries, with no effects on average earnings or the number of active firms.

In the third chapter, I study the impact of payroll tax rate changes on labor markets with informality leveraging tax changes in Argentina. I find that tax changes produce modest shifts of informality in expected directions. Payroll tax cuts mainly reduce informality in large firms, while tax hikes shift employment to small firms and increase their share of informality. Wages are unaffected by changes in payroll tax rates. The study provides new insights into the effects of payroll tax rate changes on labor markets with an informal sector, highlighting the role of firm size and worker tenure in mediating the effects on informality.

To my parents.

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Chapter 1

Pension Privatization, Behavioral Responses, and Income in Old Age: Evidence from a Cohort-Based Reform

1.1 Introduction

Pension systems constitute an essential component of modern social insurance schemes, and there has been substantial debate regarding their design. Typical points of discussion include whether the system should be funded or unfunded, government-run or privately-run, use defined benefits or defined contributions formulas, among others. Concerns in these debates often revolve around the financial sustainability of the system and the effects that pension rules can have on, for example, individuals' savings, their labor supply, and economic efficiency more broadly (Lindbeck and Persson, 2003).

A frequent policy proposal has been to *privatize* pension systems, switching from government-run "pay-as-you-go" or "unfunded" systems to privately-run "capitalization" or "funded" systems with retirement accounts.[1] This recommendation typically arises with the intention of improving the financial sustainability of the system, since unfunded systems have to rely on taxes on young workers to provide benefits to an increasingly aging population, whereas the funding for a system with retirement accounts comes solely from workers' own accumulated contributions. It has also been argued that funded systems can boost labor supply and improve economic efficiency by reducing the distortion of social security contributions being perceived purely as a tax (e.g. Auerbach and Kotlikoff, 1985; Kotlikoff, 1996). This follows the fact that funded systems generally rely on defined contributions (DC) formulas that create a tighter link between contributions and subsequent pension benefits than the

[1] Throughout the paper, we use the term "unfunded" interchangeably with "pay-as-you-go" and the term "funded" interchangeably with "capitalization".

defined benefits (DB) formulas often used in unfunded systems.[2] In addition, proponents have pointed out that privatization has the potential to increase aggregate savings, improve pension benefits, and foster the development of capital markets, among other benefits. These arguments have led many countries to privatize their social security systems. However, empirical evidence on the effects of this type of pension system reform is scarce.

In this paper, we study the effects of the partial privatization of the pension system on workers' reported earnings, employment and retirement behavior, and income in old age. We leverage a reform in 1996 in Uruguay that, starting from an exclusively unfunded DB public system, introduced an individual capitalization component with retirement accounts. Specifically, the reform introduced a *two-pillar* or *mixed* system, in which a fraction of workers' contributions is used to fund pensions for retired workers in the public DB system, while the remaining fraction is allocated to individual retirement accounts managed by pension funds.[3] Retirement pensions have two components: (i) a government-provided pension determined as a replacement rate over the average earnings of the last 10 years of employment and (ii) an annuity based on the amount accumulated in the retirement account and actuarial calculations of how much time the worker will live in retirement. To gradually roll the new system in, the government assigned workers younger than 40 by the time of the reform to the new system, while those aged 40 or more remained by default in a transition system that retained the pay-as-you-go DB nature of the original system, with pension benefits being determined by a replacement rate over the average earnings of the last 10 years of employment.[4] This cohort-based discontinuity meant that workers born just a few days apart were exposed to drastically different pension systems, and it provides the basis for our identification strategy.

Using rich administrative and census records, we leverage the cohort-based discontinuity in the introduction of the mixed system with a Regression Discontinuity Design (RDD), comparing the trajectories of individuals born within days of the cutoff over the course of 20 years. The RDD methodology has the advantage of having a high degree of internal validity, while the availability of administrative records over a long trajectory allows us to analyze responses at various points over the life cycle, even far away from retirement. In addition, the availability of the universe of workers that contribute to the social security system allows us to conduct placebo tests using the same cutoff date of birth for years in which workers

[2]Intuitively, social security contributions in funded DC systems constitute a form of forced savings, therefore creating a direct link between current contributions and pension benefits in the future. In contrast, in unfunded DB systems, social security contributions are used to fund pensions for current retirees, and often only a subset of years in workers' labor history is used to determine pension benefits, which creates a weaker link between current earnings and subsequent pension benefits.

[3]The share of the contributions that is allocated to the retirement fund varies depending on the level of earnings and choices that workers can make within the system. We describe the system in more detail in section 1.2.

[4]These short "windows" of pre-retirement earnings to calculate pension benefits are common in low- and middle-income countries and some specific systems in high-income countries (such as some public-sector workers). In Latin America, several countries use a 10-year window for benefits calculation (such as Argentina, Colombia, and Uruguay), while others use shorter windows (such as Peru and Paraguay). Pension systems for civil servants in some African countries use for reference the very last salary (Stewart and Yermo, 2009).

were not switched between pension systems.

In the first part of the paper, we analyze how workers respond to the privatization incentives with their labor supply and earnings trajectories. Regarding employment rates trajectories, we find little differences in employment rates across the two systems early on, but workers in the system with retirement accounts are significantly more likely to be employed closer to retirement. Specifically, we find that employment rates of workers in the new mixed system are similar to those of workers in the unfunded DB system during the first 15 years after the reform (when workers are in their forties and early fifties), but workers in the new mixed system are significantly more likely to be formally employed closer to the age of retirement (when workers are in their late fifties). By the time workers are 57, those in the new mixed system are about 5 percentage points more likely to be formally employed than those who remained in the unfunded DB system.

Using census and income tax data, we find that this increase in the probability of being employed is driven in large part by a lower probability of having retired early. This is consistent with the incentives often associated with capitalization systems relative to DB systems, since the annuity from the retirement fund increases substantially if the worker postpones retirement.[5] Heterogeneity analysis indicates that this employment and retirement effect is driven by individuals with low socioeconomic status and is significantly stronger for workers who report experiencing some mild disability, both of which are significant predictors of early retirement.[6] Given that the disabilities listed in the census are unlikely to qualify for permanent retirement due to disability, this result potentially reflects choices regarding early retirement under regular pension rules and special schemes for early retirement.[7] Thus, we interpret this finding as suggesting that the workers who respond to a privatization by postponing their retirement are those who often tend to retire earlier.

We then turn to analyzing earnings trajectories, finding that workers in the new mixed system report significantly higher earnings in the years immediately after the reform, and this difference fades over time as workers get closer to the age of retirement. Specifically, workers in the new system report earnings around 20% higher than those that remained in the pay-as-you-go DB system in the year immediately after the reform, and this difference persists for about 10 to 12 years until it starts shrinking as workers enter their mid-to-late fifties. This is consistent with the intuition that pension privatization can create incentives to increase labor supply due to a tighter link between contributions and pension benefits (e.g.

[5]Simulation exercises indicate that these incentives are indeed strong given the pension calculation formulas for the Uruguayan pension system (Forteza and Rossi, 2018).

[6]The literature has found that health issues, including negative self-assessments of own health, are significantly predictive of early retirement decisions (e.g. Leijten et al., 2015; Van Rijn et al., 2014).

[7]For a worker to retire and receive a pension due to permanent disability, they must be deemed to be incapable of performing any job, which is unlikely to be the case for the disabilities listed in the census (such as moderate difficulties with eyesight, hearing, and movement). Retirement due to permanent disability in the mixed system is covered in part by the government unfunded DB system and in part by insurance policies pension funds are mandated to purchase, with workers being able to choose to completely withdraw their pension fund or to have it converted into an annuity. In any case, this implies cutting short the pension fund accumulation, which can induce lower incentives to retire under this modality.

Auerbach and Kotlikoff, 1985; Kotlikoff, 1996), although the magnitude of the effect implies a potentially unrealistically large elasticity of pre-tax income.

Motivated by the large effect on reported earnings and complementary survey evidence on widespread non-compliance with labor income reporting, we then conduct several heterogeneity analyses to understand whether the effect is a real labor supply response or a reduction of earnings underreporting, finding several pieces of suggestive evidence that the effect is driven by lower underreporting of earnings. First, we find no effect on reported days or hours worked, which we interpret as measures of real labor supply. Second, we find no effect in reported earnings for workers in the public sector, where income underreporting is less prevalent. Third, we find a substantially larger effect for firm owners and self-employed workers, who are more able to underreport their labor earnings. Fourth, we use household survey data to construct measures of informality and underreporting at the sector level, and find that the effect is driven completely by sectors where informal employment and income underreporting are more widespread. We interpret these findings as indicating that the increase in reported earnings is driven to a large extent by a reduction of underreporting of earnings rather than a real labor supply response.

We interpret our findings on workers' responses through a simple model of retirement decisions in which workers can conceal part of their labor earnings. Based on our model, the mixed system creates incentives for workers to postpone retirement because remaining employed increases the amount accumulated in the pension fund, while the loss of some periods of government DB pension is less significant than in the exclusively unfunded DB system. Regarding earnings reporting and tax evasion, the unfunded DB system incentivizes evasion early on since the benefit calculation formula only uses the last 10 years of employment, which creates a large number of years in workers' careers during which social security contributions are purely a tax and do not have any connection to subsequent pension benefits. The mixed system ameliorates this distortion because throughout workers' careers a fraction of contributions is deposited into workers' retirement accounts, thus creating a stronger link between contributions and subsequent pension benefits that incentivizes a reduction in tax evasion. As workers enter their windows for DB pension calculation, those that remained in the unfunded DB system have incentives to reduce tax evasion, which reduces the gap in reported earnings between the two groups.

In the second part of the paper, we analyze how the reform affected income in early old age and workers' preferences between the two systems. This analysis presents a series of challenges to bear in mind. First, ideally more time would have passed, since workers in the key privatization cohort are 66 as of 2022, being still relatively young and likely to be employed. Second, the implementation of compensation policies since 2014 can confound the effects, especially considering a reversal option sanctioned in 2017 that allowed workers in the mixed system to switch back to the pay-as-you-go DB system. For instance, if the reform created winners and losers, with losers being eventually compensated, the "privatization side" of the discontinuity could show better outcomes, but this would not be the consequence of the privatization.[8] Finally, the fact that we document differences in labor supply in old age across the two systems could drive potential differences in income.

We begin by using income tax data to analyze how total income and poverty rates vary across systems for workers in early old age, until 60 years of age in the year 2016, prior to the reversal option being sanctioned in 2017. We measure total income as the sum of any labor earnings and any pension income, which means that this measure of total income captures differences in labor supply and potential differences in pension income.[9] We find that total income and poverty rates are similar across both systems, although minor differences in labor supply persist. Although this suggests that workers in the system with retirement accounts are more likely to be working while receiving an income similar to workers in the unfunded DB system, we do not document stark patterns that lead to strong conclusions regarding the effects on income in old age.

We then turn to analyzing workers' decisions to switch to the unfunded DB system when given the chance, to get a measure of "revealed preference" for the non-privatized system. In 2017, the government implemented a reversal policy, in which workers born up until April 1st 1966 were allowed to switch to the exclusively pay-as-you-go DB system, transferring their pension fund to the social security agency and getting a pension at 90% of the benefits of the unfunded DB "transition" system.[10] We leverage this cohort based discontinuity with another regression discontinuity design, using data from all the pension funds in the country and comparing the closing of retirement accounts across workers born within days of the cutoff date of birth.[11]

We find that workers allowed to switch to the unfunded DB system are significantly less likely to remain in the retirement accounts system. Specifically, workers allowed to reverse are 9.3 percentage points less likely to have an active retirement account after the reversal option is implemented, which represents an 11% reduction in active account rates. This reversal decision is significantly stronger for workers who did not choose the most profitable retirement savings option within the mixed system, indicating that choosing favorable options in retirement accounts systems is crucial for workers to benefit from a privatization. In addition, we find significantly higher reversal take-up among public-sector workers, who typically have steeper earnings profiles with respect to age, which will often imply a significant gain in pension income with a defined benefits formula relative to a defined contributions formula from a retirement accounts system.[12] Finally, we also document significantly more

[8] In addition, the 2014 policies could also induce responses by allowing a reversal in the retirement savings choice in the mixed system or by anticipation of future compensation policies.

[9] Although the ages we consider involve mostly early retirement, by the time workers are 60 we observe about 45% of them receiving some pension income.

[10] Workers who made additional voluntary contributions would get those back, in addition to any returns associated with such contributions.

[11] In practice, workers were sequentially allowed to reverse by cohorts. Those born up until April 1st 1960 were the first to be able to reverse starting in 2018. We analyze the reversal decisions for this cohort because we do not have the data for the following cohorts.

[12] We indeed document a much steeper age-earnings profile for public-sector workers relative to private-sector workers in the data.

reversal for workers who did not make consistent contributions during the early years of the privatization, which is crucial not only to save early-on for retirement, but also because the real interest rates on pension funds were at the highest. We interpret these findings as indicative of who the potential losers from a privatization are, suggesting that this depends on career profiles and the choices of workers within the system. Given the fact that the reversal was for 90% of the benefits of the unfunded DB system and the existence of potential default effects (e.g. Madrian and Shea, 2001; Carroll et al., 2009; Chetty et al., 2014), we do not interpret workers who did not reverse as strictly winners from the reform, but rather we interpret switchers as people who are highly likely to have experienced detrimental effects from the privatization.

Overall, our findings suggest that privatizing the pension system can boost labor supply in early old age and have the unexpected benefit of increasing tax compliance with labor earnings reporting, although there are important considerations regarding who these responses come from and how incomes in old age are affected. Although workers remaining employed later in life is often seen as positive because it improves the sustainability of pension systems, the fact that the retirement postponing comes mostly from workers of low socioeconomic status and who have mild disabilities can raise concerns about regressivity. In addition, although we observe similar incomes and poverty rates in early old age, the fact that a significant share of workers choose to reverse to the non-privatized system indicates that some workers are negatively affected. This depends crucially on workers' choices within the retirement savings system that, given the complexities behind in retirement savings options, can raise concerns regarding detrimental effects of privatizations on the less financially literate. In addition, workers with steep earnings profiles or those who were exposed to worse market returns also potentially stand to lose with a privatization.

Our paper contributes to several branches of the literature. Mainly, we contribute to the literature that studies the effects of privatizing the social security system. This literature has mostly focused on theoretical general equilibrium models to simulate the economy-wide effects of a privatization, in which labor supply and income in old age are often one component of the analysis (e.g. Auerbach and Kotlikoff, 1985; Feldstein, 1995; Kotlikoff, 1996; Nishiyama and Smetters, 2007; Hosseini and Shourideh, 2019). The logic embedded in these models motivated several countries to privatize their pension systems (Orenstein, 2013), in addition to sparking serious discussion about privatizing in countries where it was ultimately not enacted, such as the United States and Brazil. However, these reforms have not yet created a compelling empirical literature exploring their effects. This can potentially be attributed to the difficulty of finding reliable quasi-experimental variation in existing privatizations, since these often involved country-wide reforms (e.g. Chile) or coexisting public and partially private systems between which workers could freely choose (e.g. Colombia and Argentina). The Uruguayan case offers a unique setting of partial privatization with a cohort-based discontinuity that, combined with rich administrative and census records, provides an ideal experiment for analyzing workers' responses and their subsequent income in old age, in addition to a reversal option with another cohort-based discontinuity that allows us to analyze a revealed preference measure for the non-privatized system. We contribute the first empirical evidence on how workers respond to the privatization of social security, finding significant responses in ways consistent with theoretical models (e.g. labor supply), although the evasion margin seems to be the most relevant early on. In addition, we contribute the first empirical evidence on subsequent incomes in old age and revealed preference measures for a non-privatized unfunded DB system, shedding light on the distributional consequences of a privatization, which have often been relegated to a secondary role relative to sustainability and efficiency concerns in the economics literature.

We also contribute to the growing empirical literature that studies labor supply responses to pension incentives. This literature has seen substantial growth in recent years, with papers using various sources of quasi-experimental variation to analyze the effects of changes in the benefit generosity on employment participation and earnings in old age (Gelber, Isen, and Song, 2016; Liebman, Luttmer, and Seif, 2009; Manoli and Weber, 2016; Fetter and Lockwood, 2018; Brown, 2013).[13] More recently, French et al., 2022 study the effects of switching from an unfunded DB system to an unfunded DC system of Notional Defined Contributions in Poland, finding significant increases in employment rates several years before the standard age of retirement. Our main contribution to this literature is providing evidence on how workers respond to the *privatization* of the pension system, while existing research has focused on *public* pay-as-you-go systems. We also contribute to this literature by studying a long trajectory of responses, even when workers are decades away from retirement, which has often been a key component of the argument by proponents of pension reform. For instance, proponents of privatization have often argued that a privatization can incentivize labor supply among younger workers due to a tighter link between contributions and subsequent pension benefits (Kotlikoff, 1996). However, the existence of potential behavioral biases, such as exponential growth bias or present focus (e.g. Goda et al., 2019), or imperfect understanding of pension incentives (e.g. Liebman and Luttmer, 2012) could lead workers not to respond to such incentives. Our findings provide compelling empirical evidence that a privatization can indeed affect reporting decisions from workers even far away from retirement, in a manner consistent with the incentives of funded DC systems.[14]

[13]A number of initial papers studied how changes in social security benefits can affect saving decisions (Attanasio and Rohwedder, 2003; Attanasio and Brugiavini, 2003) and, more recently, expenditure in retirement (Lachowska and Myck, 2018).

[14]In this regard, our paper is tangentially related to the literature studying labor supply and taxable earnings responses to taxation, especially considering how a weak benefit-contribution link can lead to contributions being perceived purely as a tax (Kotlikoff, 1996). A growing literature has studied responses to income taxation, often finding small elasticities (Saez, Slemrod, and Giertz, 2012), although some of these findings are disputed due to concerns regarding identification strategies (Keane, 2011). Recent papers have exploited various sources of quasi-experimental variation in income tax rates to analyze responses of taxable earnings and labor supply (Martinez, Saez, and Siegenthaler, 2021; Sigurdsson, 2019; Tortarolo, Cruces, and Castillo, 2020; Kleven and Schultz, 2014; Tazhitdinova, 2020; Bergolo et al., 2022), in addition to the take-up of secondary jobs (Tazhitdinova, 2021). Our findings contribute to this literature by studying labor supply and earnings responses to partially privatizing social security, thus changing the use of workers' social security contributions, finding significant responses on the trajectories of both employment rates and reported earnings.

Our final contribution to both the theoretical literature on pension privatization and the empirical literature on labor supply and pension incentives is that, while almost all the existing discussion is focused on high-income countries, we provide evidence on the effects in a middle-income country, where other margins of response are more relevant, such as informal employment and tax evasion, and pension reform has typically been a more pressing issue. Low- and middle-income countries have featured prominently among those that have privatized at least partially their pension systems since the 1980s (Orenstein, 2013), often as a response to the perceived unsustainability of their exclusively pay-as-you-go DB systems.[15] However, the discussion has focused mostly on theoretical work regarding privatization in the United States (with some exceptions, e.g. McKiernan, 2021; Moreno, 2022), and empirical evidence on labor supply and pension incentives coming mostly from the United States and Europe (with some exceptions, e.g. Troncoso, 2022). Our paper contributes to our understanding of the effects of privatizing social security in a middle-income country and how this interacts with a context of widespread informal employment and tax evasion.

Given our evidence on how income underreporting plays a role in our results, our paper also contributes to the literature studying tax evasion. This growing literature has studied the effects of tax design on compliance at the firm-level (e.g. Pomeranz, 2015; Naritomi, 2019; Bachas and Soto, 2021) and individual-level (e.g. Londoño-Vélez and Ávila-Mahecha, 2021). More specifically, we contribute to the study of underreporting of labor earnings, which has received much less attention so far. Recent research has found that underreporting of labor earnings to evade taxes is widespread and sizable (e.g. Feinmann, Lauletta, and Hsu, 2022; Bergolo and Cruces, 2014).[16] Specifically regarding pension systems, recent evidence has linked income underreporting to pension regulations in Uruguay (Dean, Fleitas, and Zerpa, 2022) and Mexico (Kumler, Verhoogen, and Frías, 2020). Our paper contributes to this literature by providing compelling evidence that workers' retirement savings incentives are closely linked to underreporting of labor earnings, both by self-employed workers and dependent employees, even decades before retirement. This suggests that employees' incentives play a significant role in the underreporting of labor earnings.

1.2 Context

Uruguay is an upper-middle-income country in South America, with a population of around 3.5 million and a GDP per capita of about $18,000 dollars in 2018 according to data from the World Bank. As with most Latin American countries, a substantial fraction of employment is non-registered and there is widespread non-compliance with payroll taxes, al-

[15]Prominent examples of privatization in Latin America include Chile, Argentina, Uruguay, Mexico, and Colombia, while cases from other continents include Kazakhstan, Romania, Malawi, and Nigeria, among others (Orenstein, 2013).

[16]Income underreporting is not constrained to low- and middle-income countries, there is some evidence of underreporting in high-income countries for self-employed workers in the United States (Saez, 2010) and employees in Norway (Bjørneby, Alstadsæter, and Telle, 2021).

though both of these measures of informality have been falling in recent years. The country has an established contributory social security system for formal workers, including retirement benefits, unemployment insurance, workers' compensation, disability insurance, health insurance, and parental leave, all of which are handled by the Social Security Agency (SSA) called *Banco de Previsión Social*.

Regarding the pension system specifically, the system that was in place before the reform we study was unfunded with defined benefits (DB). Pensions for retired workers were funded exclusively by payroll taxes on active workers, while the pension benefits were determined by a replacement rate over the average earnings of workers' final 5 years of contributions. The minimum retirement age for men was 60 and for women it was 55. As with many Latin American countries, during the late 1980s and early 1990s concerns arose regarding the financial sustainability of the government's DB system, which led to a partial privatization of the pension system in 1996.

The original social security system was reformed by Law 16,713, which was passed in September 1995 and entered into effect on April 1st of 1996. This law created a *two-pillar* or *mixed* system, which is part a government unfunded DB system and part a privately-run funded DC system.[17] Workers' contributions represent 15% of the salary, a fraction of which goes to the SSA to fund the DB part of pensions while the remaining fraction goes to individual retirement accounts managed by pension funds.[18] Workers choose one of two options of how to distribute their contributions between the two pillars, which depend on three earnings thresholds. These options are represented in appendix figure A.1. The default option is to contribute exclusively to the pay-as-you-go DB system until the first income threshold (around the 70th percentile of the salary distribution), while workers above that threshold have their contributions on income below the threshold go to the DB system and contributions on income above the threshold go to their retirement accounts. The alternative option (known as Article 8) is to evenly divide contributions between the DB system and individual accounts below the aforementioned income threshold, after which contributions go to the DB system until a second threshold, while workers whose income surpasses the second threshold revert to the default option. Contributions on earnings beyond the third threshold, which is around the 98th percentile of the wage distribution, are voluntary. About 75% of workers choose the alternative Article 8 option (CESS, 2021). In addition, workers can freely choose one of several pension funds that manage workers' contributions to the funded DC pillar.

Pensions in the new system have two components: (i) a government-provided DB pension and (ii) an annuity from the funds accumulated in the retirement account. The government part of the pension is determined as a replacement rate over a "salary for pension calculation" or "contributory salary", which is calculated from the average earnings of the last 10 years of employment.[19] This contributory salary comprises workers' labor earnings up until the

[17]The law also introduced some changes to the DB system, such as increasing the window of earnings by which DB pension benefits are calculated from 5 years to 10.

[18]The employer payroll taxes (7.5% of the salary) go entirely to the unfunded government pension system.

[19]The average of the last 10 years is used unless this is lower than the average 20 best years of earnings,

income threshold after which they start contributing to the private system. If an individual in the mixed system chose the alternative "Article 8" option, their contributory salary is computed as 75% of the contributory salary under the default option.[20] The minimum statutory replacement rate is 45%, with increases for higher retirement ages and years of contributory history.[21] There is a maximum and a minimum pension amount for the DB part of the pension.

The capitalization part of the pension is determined by the amount accumulated in the retirement account and actuarial calculations regarding how long the worker is expected to live in retirement. Upon retirement, the pension fund chosen by the worker transfers the funds contained in the retirement account to a government-run insurance company, which conducts the actuarial calculations and provides the funds to the worker in the form of an annuity. The reform also gradually increased the minimum retirement age for women to match that of men at 60 years old, although there are ways for retiring early in some specific sectors (e.g. education and risky occupations) and due to disabilities. In the case of retirement due to permanent disability, the replacement rate for the government DB pension is 65% over the contributory salary. In addition, pension funds are mandated to purchase insurance policies that add a 45% replacement rate to the earnings over which they contributed to the pension fund over the last 10 years, and workers' pension fund is transferred to the insurance company as part of payment with the exception of additional voluntary contributions and the returns associated to these.[22]

To gradually roll the new pension system in, workers aged less than 40 at the time the law entered into effect would be switched to the new mixed system, while those aged 40 or more would remain in a *transition system* that retained the unfunded DB nature of the original system. For workers left in the unfunded DB "transition" system, their pension is determined under the same rules as the DB part of the workers in the new system, with the difference that they contribute only to the public pay-as-you-go system with defined benefits based on all of their labor earnings, and the maximum pension is capped at a higher level to compensate for the fact that they do not receive a private DC pension. This discontinuity implied that individuals born up until April 1st 1956 remained by default in an unfunded DB system, while individuals born after were assigned the two-pillar system with individual capitalization. This implied that people born only a few hours away were exposed to radically

in which case the latter is used. However, this is only done for individuals who have 20 full years of earnings history registered with the SSA.

[20]Note that this implies a bonus for the Article 8 option: under this option workers' contributions towards the unfunded DB system fall by 50%, but the salary for their DB part of the pension only falls by 25%.

[21]Specifically, the statutory replacement rate applied to the contributory salary is 45% for an individual who has 30 years of contributory history (the minimum required) and retires at 60 (the minimum retirement age), with an increase of 1 percentage point for each additional year of contributory history until 35 years, and an additional 0.5 percentage points for each additional year until a maximum of 40 years of contributory history. Further increases are given for each additional year of contributory history after turning 60, with the maximum replacement rate being 82%.

[22]For the case of workers who retire due to disabilities and do not have 10 years of contributory history, the available contributory history is used.

different pension systems. There were some exceptions for workers aged 40 or more that were assigned the mixed system as well: (i) if workers had never had a formal contract registered with the SSA before the law entered into effect, and (ii) if they voluntarily chose the new system within a 6-month window after the law entered into effect.

The mixed system remains in place to this day with some minor changes. However, starting in 2014 the government announced a series of policies that could confound effects regarding workers' responses. For instance, they allowed for reversals in the Article 8 choice once individuals turn 40 and reversals back into the transition system for workers who voluntarily chose the mixed system despite being born before the cutoff. Later on, the government also announced a plan to allow for reversals for workers assigned the new mixed system, allowing them to retire under rules similar to the transition system. This could create incentives to remain employed not due to the incentives inherent to a privatization with retirement accounts, but due to individuals waiting to see if it is more convenient to retire under the reversal, or switching from Article 8 into the default option for low-income workers would essentially result in almost a "de-privatziation" for them. Thus, we analyze workers' responses until the year 2013 to truly capture responses to the privatization, and we analyze the reversal policy of 2017 to understand the effects on income in old age.

The "reversal" reform of 2017 allowed for workers assigned the new mixed system to retire under the unfunded DB rules of the transition system, as long as they were born up until April 1st 1966. This law arose as a response to concerns that the privatization could have had a detrimental effect on pension incomes among some workers in the cohorts around the original privatization discontinuity, and came to be known as the "Fifty-Somethings Law" (*Ley de Cincuentones*). This was part of a broader debate in Latin America about the consequences of pension privatizations that took place in the 80s and 90s.[23]

The procedure for "reversing" to the unfunded DB transition system consisted on an information campaign to encourage workers to analyze their situation regarding their retirement income. Workers would then have a one year period to schedule a consultation with the Social Security Agency, which would estimate the subsequent pension income upon retirement in both systems. With this information, workers could choose to remain in the mixed system or to switch to the transition unfunded DB system at 90% of the benefits, a decision that is definitive. This involved transferring their retirement fund to the government, except for any additional voluntary contributions they may have made, and subsequent social security contributions being destined only to the unfunded DB government system, with workers receiving an unfunded DB pension upon retirement. This process was gradually rolled out, with people who were 56 years old or more by April 1st 2016 being able to go first, being able to choose to reverse between March 2018 and March 2019. Then, this was followed by aged 53 to 55, and then by those 50 to 52. People younger than 50 by April 1st 2016 were not able to reverse to the transition system rules.

[23]This re-evaluation of privatizations included full-on de-privatizations in Latin American countries like Argentina and Venezuela, in addition to Eastern European countries such as Romania and Hungary.

1.3 A model of retirement decisions with tax evasion

In this section, we develop a simple model to understand workers' responses to transitioning from a public pay-as-you-go system with defined benefits to a partially private system with retirement accounts. We take a simple static model of retirement decisions in which workers decide their career length (as in Seibold, 2021), and we augment it by including the possibility for workers to conceal a fraction of their labor earnings at a cost in each period.[24] Pension benefits depend on the type of system the worker is in. In the exclusively unfunded DB system, the pension benefits are calculated by a replacement rate over the average earnings of the last few years of employment. In the partially private system, one part of the pension benefits consists of a replacement rate over the average earnings of the last few years of employment, while the other part is an annuity derived from the amount accrued in the pension fund. Using this model, we derive a series of intuitive predictions for workers' responses to the privatization of the social security system.

Set up

The basic model consists of agents that live for T periods and have to choose a career length R and a proportion of concealed earnings $\theta(t)$ for each period t. Workers earn labor earnings w in each period. Concealing a proportion $\theta(t)$ of income has an instantaneous convex cost $\sigma(\theta(t))$ (we assume $\sigma(0) = 0$, $\sigma' > 0$, and $\sigma'' > 0$), which can be rationalized with an Allingham and Sandmo, 1972-style cost of evasion. Remaining employed for R years has a convex cost $V(R)$ (we assume $V(0) = 0$, $V' > 0$, and $V'' > 0$), which can represent not only the disutility of working in old age but also the need to remain employed for longer (for example, if individuals have a high marginal utility of consumption). Upon retirement, workers receive pension benefits $B(S)$ in each time period, the formula of which depends on the pension system $S \in \{DB, M\}$ (DB stands for unfunded with defined benefits and M stands for mixed system). Assuming an interest rate of 0 and no discounting, workers choose R and $\theta(t)$ to maximize their lifetime utility, given by:

$$U = \int_0^T u(c(t))dt - \int_0^R \sigma(\theta(t))dt - V(R)$$

Subject to the lifetime budget constraint:

$$\int_0^T c(t)dt = \int_0^R (1-\tau)w(1-\theta(t))dt + \int_0^R w\theta(t)dt + \int_R^T B(S)dt$$

The budget constraint reflects that lifetime consumption has to be equal to the sum of lifetime post-tax reported earnings, untaxed concealed earnings, and pension benefits received during retirement. The pension benefits received during retirement $B(S)$ depend

[24] We present the model with reporting of labor earnings because the empirical evidence indicates tax evasion plays a key role in the effect on earnings, but it can be easily modified to capture real labor supply with "production" of earnings.

on the system the worker is in. For workers in the unfunded DB system ($S = DB$), their benefits are determined by a replacement rate ρ^{DB} over the reported labor earnings over the last L periods of employment. For workers in the mixed system ($S = M$), their pension has two components: (i) a government part of the pension determined by a replacement rate of ρ^{M} over the reported labor earnings over the last L periods of employment (we assume this to be lower than that of the unfunded DB system, consistent with the real formulas), and (ii) an annuity from the total retirement fund accumulated over the career length of R based on the share γ of contributions τ that go to their retirement account, evenly divided throughout life in retirement ($T - R$ periods).[25] The formulas are then given by:[26]

$$B(S) = \begin{cases} \rho^{DB} \frac{1}{L} \int_{R-L}^{R} w(1-\theta(t))dt & \text{if } S = DB \\ \rho^{M} \frac{1}{L} \int_{R-L}^{R} w(1-\theta(t))dt + \frac{1}{T-R}\tau\gamma \int_{0}^{R} w(1-\theta(t)) & \text{if } S = M \end{cases}$$

Assuming linear utility of consumption, some simple algebra yields the following utilities for workers in each system (U^{DB} and U^{M}):

$$U^{DB} = w\left[(1-\tau)R + \tau\int_{0}^{R}\theta(t)dt\right] + \frac{(T-R)\rho^{DB}w}{L}\int_{R-L}^{R}(1-\theta(t))dt - \int_{0}^{R}\sigma(\theta(t)) - V(R)$$

$$U^{M} = w\left[(1-\tau)R + \tau\int_{0}^{R}\theta(t)dt\right] + \frac{(T-R)\rho^{M}w}{L}\int_{R-L}^{R}(1-\theta(t))dt + \gamma\tau w\int_{0}^{R}(1-\theta(t)) - \int_{0}^{R}\sigma(\theta(t)) - V(R)$$

Our model set-up makes several simplifying assumptions to keep the model tractable and build intuition. First, there is no dynamic uncertainty, which implies that the retirement decision can be made at $t = 0$. Second, the worker fully smooths consumption, being able to freely lend and borrow at an interest rate of zero to maximize lifetime utility, with no time discounting. Regarding the pension formulas, we assume a constant replacement rate with respect to the retirement age and an interest rate of zero for the accumulation of the pension fund. In addition, we ignore the fact that the government DB pensions are capped and that the fraction of contributions that is allocated to the pension fund depends on the income level and the option that workers choose.

[25] For simplicity, we ignore the fact that the government DB pensions are capped and assume a constant replacement rate with respect to the retirement age. We also assume an interest rate of zero for the accumulation of the pension fund.

[26] Although we model the mixed system as partly funded DC and partly unfunded DB, the model can readily accommodate a purely private system if we assume $\gamma = 1$ and $\rho^{M} = 0$.

With this basic set-up, we first solve for workers' choices in two special cases of the model. First, a version of the model for the decision of the retirement age with no tax evasion, which can elucidate on the differential incentives for postponing retirement between systems. Second, a model for the decision for the concealing of earnings over time given a fixed retirement age, which can elucidate on the differential incentives for tax evasion under each system. We then solve for the general version of the model that allows workers to choose both the retirement age and concealing trajectories.

The choice of retirement age with no concealing of earnings

In this subsection, we study a simplified version of the model in which there is no concealing of earnings. This simplified model will allow us to understand the incentives for the decision of the career length under each system. Workers have to choose a retirement age in order to balance gains in lifetime consumption from postponing retirement with the disutility of working in old age.

If there is no concealing of earnings, then $\theta(t) = 0$ for all t. This simplifies the utilities under each system to be the following:

$$
\begin{aligned}
U^{DB} &= w(1-\tau)R + (T-R)\rho^{DB}w - V(R) \\
U^{M} &= w(1-\tau)R + (T-R)\rho^{M}w + \gamma\tau wR - V(R)
\end{aligned}
$$

The optimality conditions for the choice of the retirement age R^S in each system S are straightforward:

$$
\begin{aligned}
V'(R^{DB}) &= w(1-\tau-\rho^{DB}) \\
V'(R^{M}) &= w(1-(1-\gamma)\tau-\rho^{M})
\end{aligned}
$$

These conditions have straightforward interpretations: workers' retirement age decision balances out the gains in lifetime consumption with the disutility of postponing retirement in old age.[27] In the unfunded DB system, working for an additional period implies an increase in lifetime consumption in the amount of the net-of-tax earnings minus the loss of one year of retirement pension income. In the mixed system, working for an additional period increases lifetime consumption in the net-of-tax earnings minus the loss of one year of the DB part of the retirement pension income, with the addition that a fraction of the contributions will go to the pension fund and, therefore, increase lifetime income.

From these two conditions, it is evident that the optimal retirement age will be higher in the mixed system than in the unfunded DB system. This follows from two effects that increase lifetime consumption in the mixed system: (i) an additional period of earnings increases the

[27] Without loss of generality, we assume $1 > \tau + \rho^{DB}$. Otherwise, workers would not want to work even one period.

pension fund because a fraction of the contributions is saved, and (ii) the loss of the DB part of the pension for one period is smaller because the replacement rate for the mixed system is lower. The fact that a fraction of contributions are accumulated in the pension fund attenuates the effect of the tax rate on the retirement decision: a higher fraction of contributions assigned to the pension fund implies a higher retirement age, since this lowers the opportunity cost of working. These conditions can also readily rationalize findings from recent papers regarding benefit generosity and retirement behavior: higher replacement rates imply earlier retirement in both systems.

The choice of concealing trajectories given a fixed retirement age

In this subsection, we present the solution for concealing trajectories in each system, given a fixed retirement age. Workers have to decide what share of earnings to conceal in each period taking into account how this will affect their lifetime consumption and the costs associated to a given level of evasion. The optimality conditions for the fraction of earnings concealed in each period $\theta^S(t)$ in each system S are given by:

$$\sigma'(\theta^{DB}(t)) = \begin{cases} \tau w & \text{if } t \leq R-L \\ \tau w - \rho^{DB} w \frac{T-R}{L} & \text{if } t > R-L \end{cases}$$

$$\sigma'(\theta^{M}(t)) = \begin{cases} \tau w(1-\gamma) & \text{if } t \leq R-L \\ \tau w(1-\gamma) - \rho^{M} w \frac{T-R}{L} & \text{if } t > R-L \end{cases}$$

These conditions indicate that there are two levels of income concealing for each system. First, a high level of income concealing for periods outside the L-period window during which reported earnings bear no relation with the calculation of the government DB pension. Second, a lower level of evasion for the final L-periods of employment, in which the reported earnings are used to calculate the DB pension benefits. Note that the right-hand side for the low level of evasion could be negative, implying that increasing evasion has negative marginal utility. We assume that workers cannot overreport earnings (i.e. $\theta(t) \in [0,1]$), so in such cases evasion would be zero. In fact, plugging in realistic values of the parameters would yield that workers do not evade at all for periods within the window.[28] In contrast, the high level of evasion will always be positive (except in a fully private system with $\gamma = 1$).

Comparing the optimality conditions for the high level of evasion across systems, it is evident that evasion outside the window for DB pension calculation will be lower in the mixed system. This follows from the fact that, throughout workers' active lifetime, a fraction of contributions is saved in their pension account. By contrast, social security contributions for workers in the unfunded DB system represent purely a tax and reported earnings bear no relationship to subsequent pension benefits outside the window for DB pension calculation.

[28] For example, assuming a contribution rate $\tau = 0.15$, a fraction $\gamma = 0.5$ going to the pension fund, a replacement rate of $\rho^M = 0.35$, a window for DB calculation of $L = 10$, and that workers live $T - R = 15$ years in retirement yields a negative utility of evading within the window for workers in the mixed system.

The degree of attenuation in the mixed system depends on the share of contributions that go to the retirement fund (γ).

When comparing the optimality conditions for the low level of evasion within the window, which level of evasion is higher depends on parameters. Intuitively, there are opposing forces that drive evasion upwards and downwards in the mixed system relative to the unfunded DB system. First, the fact that a fraction of the contributions goes to the worker's pension fund drives evasion down, but the fact that the replacement rate for the DB part of the pension is lower drives evasion up. The final effect on differences in evasion rates depends on which effect dominates. Note, however, that in both systems evasion rates will shrink towards zero once workers enter the window for DB pension calculation, and that these evasion rates within the window could be zero (as discussed above). Thus, it is likely for evasion rates to converge once workers enter the window for DB pension calculation.

Once we move to the empirical analysis, these conditions indicate that we should observe higher reported earnings for workers in the mixed system in the first few years after the reform, while workers are outside the window for DB pension calculation. As workers approach the window for DB pension calculation, we should be more likely to observe similar reported earnings, since workers left in the unfunded DB system have incentives to increase their reported earnings.

When it comes to the rest of the parameters, given a fixed retirement age, parameters such as the replacement rates (ρ), lifespan (T), and length of the window of time periods to calculate DB pension benefits (L) only matter for evasion in the L-period window before retirement. Intuitively enough, higher replacement rates and longer lifespans imply lower rates of evasion within the L-year window. Higher ages of retirement will also increase evasion within the window, the intuition being that the worker will live less time in retirement, so the pension is less relevant. Longer windows for calculating DB pension benefits have two effects: (i) increase the number of periods of low evasion (since more periods are used to calculate DB pension benefits) and (ii) increase evasion within the window (since each individual period within the window matters less for the calculation of the DB benefits). Thus, increasing the window of years to calculate pension benefits in an unfunded DB system to emulate the lower evasion of funded DC systems could induce lower evasion for more years, but increase this lower level of evasion.

The solution for the choice of earnings concealing and retirement age

In this subsection, we solve the model for the case where workers choose both the retirement age and the earnings concealing trajectory. This version is slightly more complicated but, as we discuss below, under realistic assumptions, the implications from this model boil down to a combination of the implications from the two simplified versions discussed before. Workers in the mixed system will still retire later and evade less early on, while the differences in the evasion rates within the L-year window are still undetermined but likely to converge.

Given the solution from the previous section, workers have two different levels of earnings concealing: a high level θ_h^S for periods outside the L-period window for the DB benefits calculation and a low level θ_l^S within the window. Workers' choices of the career length and both earnings concealing levels can be solved in a two-step process, first solving for the concealing trajectories given a career length R (which is shown in the previous section), and then using those optimality conditions for the choice of the career length. The conditions for the optimal retirement age are:[29]

$$V'(R^{DB}) = w\left[1-(1-\theta_h^{DB})\tau\right] - \sigma(\theta_h^{DB}) - \rho^{DB}w(1-\theta_l^{DB})$$
$$V'(R^{M}) = w\left[1-(1-\theta_h^{M})(1-\gamma)\tau\right] - \sigma(\theta_h^{M}) - \rho^{M}w(1-\theta_l^{M})$$

Intuitively, these conditions indicate that the decision for the retirement age balances out the increase in lifetime consumption from one additional period of high evasion and the marginal cost of postponing retirement. The relevant margin on evasion is the one additional period of high evasion because the worker optimally only evades less within the L-year window, so if the worker postpones retirement they still only have low evasion for L periods.[30]

Given that different evasion trajectories are allowed, which retirement age is higher depends on parameters. Intuitively, there are two distinct forces at play that push the retirement age in different directions for the mixed system relative to the DB system. First, in the mixed system, postponing retirement increases the amount accumulated in the pension fund and, since the worker evades less outside the window for DB pension calculation, there is a lower cost of working one more period. Both of these effects push the retirement age upwards in the mixed system relative to the unfunded DB system. However, since the worker evades less outside the window for DB pension calculation, they pay more taxes for each year they postpone retirement, which pushes the retirement age downwards. The necessary condition for the retirement age in the mixed system than in the unfunded DB system is:

$$\sigma(\theta_h^{DB}) - \sigma(\theta_h^{M}) + w\gamma\tau(1-\theta_h^{M}) + \rho^{DB}w(1-\theta_l^{DB}) - \rho^{M}w(1-\theta_l^{M}) > w\tau(\theta_h^{DB}-\theta_h^{M})$$

Note that the evasion levels within the L-year window depend on the retirement age. Although the last two terms in the left-hand side depend on the retirement age, it is realistic to assume that $\rho^M(1-\theta_l^M) < \rho^{DB}(1-\theta_l^{DB})$, which basically means that the DB part of the pension in the mixed system will be lower than the full pension of workers in the exclusively unfunded DB system for any retirement age. This is realistic in the sense that the DB part of

[29]Note that, for the existence of such an equilibrium, we are assuming that $V''(R) > \rho^{DB}w/\sigma''(\theta_l^{DB})$ and $V''(R) > \rho^{M}w/\sigma''(\theta_l^{M})$. This basically means that the marginal cost of postponing retirement increases faster than the marginal utility of postponing retirement. Without this condition workers would never want to retire.

[30]Intuitively, if the worker retires at 60 and the window is 10 years, they have low evasion between 50 and 60 years of age, while if they retire at 65, they will have low evasion between 55 and 65.

the pension in the mixed system only represents a part of the total pension, and is therefore likely to be lower than the full pension of the exclusively unfunded DB system. In addition, given that evasion rates within the window are likely to be similar (and even zero), this condition is likely to hold since the replacement rate for the unfunded DB system is higher than that of the mixed system ($\rho^{DB} > \rho^{M}$).

Then, a sufficient condition for the retirement age in the mixed system to be higher than in the unfunded DB system is $\sigma(\theta_h^{DB}) - \sigma(\theta_h^{M}) + w\gamma\tau(1-\theta_h^{M}) > w\tau(\theta_h^{DB} - \theta_h^{M})$, which basically means that the incentive to postpone retirement due to increases in the pension fund persists in the presence of evasion. Intuitively, this condition means that the gain in the pension fund and cost saving due to lower evasion early on more than compensate the higher taxes that the worker has to pay because they evade less outside the window for DB pension calculation.

With these two conditions being satisfied, the optimal retirement age for workers in the mixed system will be higher than that of workers in the unfunded DB system. With the optimal retirement age, we can obtain the optimal low level of evasion within the L-period window for each system. Once again, whether evasion within the L-year window will be higher or lower in the mixed system relative to the DB system is undetermined and depends on parameter values, with the addition that workers in the mixed system will retire later, which will push their low level of evasion upwards relative to the unfunded DB system. However, bear in mind that evasion levels for both systems will shrink towards zero once workers enter the window for DB pension calculation, and that these evasion levels could possibly be zero in a corner solution.

The model then generates the following predictions: (i) workers in the mixed system will retire later; (ii) workers in the mixed system will report higher earnings early on; and (iii) reported earnings once workers enter the window for DB pension calculation are likely to be similar.

1.4 Data

In this section we describe the data sources that we use for our analysis. We combine five main sources of data: (1) administrative social security records from the SSA, (2) individual-level micro-data from the 2011 census, (3) administrative income tax records from the Internal Revenue Service, (4) administrative records from workers' retirement accounts, and (5) data from the main labor-force household survey in the country.

Social security records

Our first main source of data is administrative records from the SSA (*Banco de Previsión Social*). These records are matched employer-employee labor histories data constructed from the payroll tax forms that businesses have to file monthly to submit social security contributions to the SSA. They cover the universe of formal workers that reported some positive earnings to the SSA at least for one month from the year 1997 until 2013.[31] These records contain monthly information on workers' gross earnings, hours worked, days worked in the month, firm identifiers, the sector of employment of the firm, whether the workers are firm owners, among others.[32] In addition, these records contain data on workers' date of birth at the daily level, which is our running variable that determined the pension system workers were assigned to. In addition, we have access to another administrative dataset that contains a random subset of 80,000 observations with information on the date of birth and the pension system corresponding to each person. Although these data contain different IDs, rendering us unable to merge this with the labor histories, we are able to use this information to estimate a first stage.

We use these labor histories data to construct our main labor market variables. We define a dummy variable for employment if the worker has positive earnings for a period, and zero otherwise. Note that this indicator takes the value of zero if the worker is not formally employed for any reason (for example, if the worker is unemployed, inactive, retired, or informally employed). We define the total labor earnings as the sum of all income related to labor for the corresponding month, which includes the regular salary and the 13th salary (paid half in July and half in December) and additional payments made to the worker after the cessation of the labor relationship.[33] Our final dataset consists of a panel of workers from the year 1997 to the year 2013.

Panel A of table 1.1 presents summary statistics from the administrative data for workers born between 1955 and 1957. This encompasses workers born in the year affected by the reform, as well as those born in the year immediately before and immediately after the cohort after the reform. The indicator for being employed is an indicator equal to 1 if the worker reported positive earnings in the given period. The average labor earnings are measured in current Uruguayan Pesos and winsorized at the 1% and 99% levels to reduce the influence of outliers. Hours worked are the average monthly hours worked. Days worked in the month are the number of days worked in the month. Public sector is a dummy variable equal to 1 if the worker is employed in the public administration. High inf. sector is an indicator equal to 1 if the firm's sector is categorized as a high informality sector, which we define in section 1.4.

[31] These records do not cover some minor independent pension systems, such as the Military and the Police. However, these independent systems are marginal, accounting for less than 7% of the workers.

[32] The variable of ownership is constructed from a field that indicates whether the worker is an owner, partner, director, or administrator of the firm. The owner indicator takes the value of one in any of these cases and zero otherwise.

[33] We average earnings, hours worked, and days worked over the last six months of the year to reduce the influence of occasional noise in reporting and limit the influence of events close to people's birthdays (such as birthday salary bonuses or retirement immediately after turning a certain age). Results remain qualitatively and quantitatively very similar when considering individual months or taking averages over other groups of months. We do not consider the total labor earnings made in the year because that number would mix extensive margin and intensive margin labor supply.

Census data

Our second main source of data is individual-level records from the 2011 Population and Household census. The institution in charge of conducting the censuses in Uruguay is the National Institute of Statistics (*Instituto Nacional de Estadística*, INE), which is the agency that produces most statistical information in the country. The census consisted of in-person surveys for all households in Uruguay, and was conducted between September 1st and December 30th of 2011 (INE, 2012).

The questionnaire contained standard socio-demographic questions, such as age, gender, family relationships, ethnicity, literacy, educational attainment, and whether the individual is affected by some disabilities, among others. In addition, the census data collected some information on labor market participation, including whether the individual is currently employed and, if not, whether the individual is currently retired. Unfortunately, the census surveys did not collect any information regarding earnings or whether employment is formally registered. Finally, the census data also contains information on the individual's date of birth at the monthly level, which is our main running variable for the analysis using this data. Our final dataset consists of a cross-section of individuals surveyed during 2011.

Panel B of table 1.1 presents summary statistics from the census data for individuals born between 1955 and 1957. Again, this encompasses workers born in the year affected by the reform, as well as those born in the year immediately before and immediately after the cohort affected by the reform. About 67.6% of individuals report being employed while 16.2% report being retired. About 5.7% report experiencing some disability (defined as having at least moderate difficulties with eyesight, hearing, motor functions, or cognitive ability). We also report summary statistics for an indicator of being married, having completed college, being female, having at least one child, and an index of socioeconomic status (normalized to have mean zero and standard deviation of one). We construct the index of socioeconomic status using principal component analysis on several characteristics, such as whether the individual owns their home, has completed a college degree, and owns several appliances (television sets, a mobile phone, a personal computer, cars, a clothes drying machine), and has access to an internet connection (see appendix section A.6 for details).

Income tax data

Our third main source of data is individual-level administrative records from the Internal Revenue Service (*Dirección General Impositiva*, IRS). These records consist of income tax returns for the entire population for the period 2009 to 2016. This dataset includes all income from the main formal sources, including any labor earnings and pension income. However, these records do not contain the exact dates of birth, only the year of birth. To obtain a date of birth at the daily level, we merge this dataset with an auxiliary dataset from the SSA that merges the identifiers from the income tax data with the ones from the SSA for a subset of the observations.[34] This yields a match for 53% of the income tax filers born in the years 1955 to 1957. Although this is not a random sample of income tax returns, the matched observations seem to bear no relation to the reform, since we do not observe any difference in densities around the cutoff (see figure A.13).

We construct an indicator of being employed in a given year if the worker reports any positive labor earnings in a given year. We then create an indicator of being retired if the worker reports any positive pension income in a given year. We winsorize all variables at the 1 and 99 percent to reduce the influence of outliers and deflate monetary values to 2009 Uruguayan pesos using the yearly CPI. We construct total income as the sum of labor earnings and pension income, including zeroes. In addition, we construct an indicator of whether the person's total income is below the national poverty line for Montevideo, calculated as twelve times the monthly poverty line in December for each year.

Panel C of table 1.1 reports summary statistics for individuals born between 1955 and 1957. Again, this comprises workers born in the year affected by the reform, as well as those born in the year after and the year before. In a given year, about 69% of workers were employed and about 21% were retired. Total labor earnings are UR$302,116 on average, and the average pension income is about 31,537. On average, total yearly income is below the poverty line for 38.7% of the individuals.

Retirement accounts data

Our fourth main source of data is individual-level administrative records from workers' retirement accounts, obtained from the four pension funds in the country. These records consist on monthly retirement accounts reports for the period 1997 to 2022, for workers born in 1960 and 1961 (the first cohort allowed to reverse to the unfunded DB system, as well as those born on the year after). These records are maintained by the pension funds to keep track of workers' monthly balance, contributions made, and the opening and closing of accounts. These records are not merged to the administrative social security or tax records, since they are proprietary data from the pension funds and have different identification numbers.

For each worker, we observe the opening and closing date of the retirement account, which we use to construct an indicator equal to 1 if the account was active by March of 2019 (the last month in which the 1960 cohort was allowed to reverse back to the unfunded DB system). In addition, we also observe some basic demographics, such as whether the worker is female and foreign born. We also observe whether the worker opted in for the Article 8 option and, if so, in which year they did. Panel D of table 1.1 presents summary statistics for the retirement accounts data. About 72.9% of accounts were active by March of 2019, with 53.6% of the sample being female and about 5.8% being foreign born. About 91% of

[34] To appear in this auxiliary SSA dataset, an individual must have created the right to a dependent to access some social security benefit. The most common case is the provision of health insurance from formal workers to other members of the household, typically their children. However, other programs are also included, such as conditional cash transfers.

workers in the sample opted for Article 8, and the average worker who opted for Article 8 did so in the year 1998 and the median in 1996.

Labor force household survey

Our final main source of data is individual-level records from the main household survey in Uruguay, the Continuous Household Survey (*Encuesta Continua de Hogares*, ECH). This survey is also conducted by the National Institute of Statistics, and constitutes the main source of information regarding the labor market, education, and health in the country. The ECH is a nationally representative household survey conducted in accordance with international standards, and it consists of repeated cross-sections at the quarterly level.

Although the ECH collects important labor market information that would be interesting to analyze, such as whether the individual is informally employed, it does not collect information regarding the respondent's date of birth and the sample size would be too small to feasibly conduct Regression Discontinuity analyses. Thus, we use the ECH to construct complementary measures of informality at the sector level that we then relate to the administrative data. The ECH contains a standard question used in Latin American household surveys to determine whether the worker is informally employed, which is whether the worker is contributing to a pension system. Workers who report they do not contribute to any pension system are considered informal. In addition, starting in 2006 the ECH introduced a novel follow-up question to determine whether workers underreport their salaried earnings to the tax and social security authorities.[35] This question was included specifically to capture non-compliance with taxes and social security contributions. We categorize workers who report they do not contribute based on their total earnings as underreporting. We use the survey wave closest to the 1996 reform that contained these informality questions, which is the 2006 wave.

For each sector, we calculate the proportion of workers that are informal and the proportion of formal workers that underreport earnings for social security contributions and taxes. We then construct an index of informality by conducting a principal component analysis of both proportions. The results from this exercise can be found in table A.1. We categorize as "high informality sectors" as those that have an above median index of informality (agriculture, commerce, administrative support services, hotels and restaurants, construction, other services, and home services). In addition, we use the household survey to calculate several auxiliary measures included in the appendix that will help us understand some of the empirical analysis that uses administrative records.

[35] The question can be translated into "Do you contribute to your pension based on the total amount of earnings from this job?".

1.5 Econometric strategy

The cohort-based nature of the reforms provide an ideal setting for a Regression Discontinuity Design (RDD). Intuitively, this method estimates the effect of being assigned the new mixed system by comparing people born a few days after April 1st 1956 to those born a few days before. For the reversal policy of 2017, we compare individuals born a few days after April 1st 1960 to those born a few days before. We present standard RDD plots estimating regressions of the form:

$$Y_i = \alpha + \beta \mathbb{1}\{DOB_i > c\} + f(DOB_i) + \varepsilon_i \tag{1.1}$$

where Y_i represents any of our outcomes of interest (employment, earnings, days worked, hours worked, etc) for individual i, DOB_i is the individual's date of birth (at the daily level), c is the cutoff date of birth (April 1st 1956 for the original privatization and April 1st 1960 for the reversal policy), and $f(DOB_i)$ is a polynomial of the date of birth. Given that individuals born after the cutoff date of birth were assigned the new mixed system, the coefficient β measures the Intention-to-Treat (ITT) effect of being switched from the unfunded DB government system to the mixed system that includes a capitalization element. In our baseline specifications, we estimate several RD specifications pooling years together for additional power and to make the plots more tractable, and we present individual plots for each year as a robustness check in the appendix.[36]

Given that our running variable is discrete, usual extrapolation methods involving polynomials in RDDs are problematic because the standard smoothness assumptions do not hold, creating problems for interpreting the coefficient and conducting inference (Kolesár and Rothe, 2018). Thus, we use the Local Randomization approach (as recommended by Cattaneo, Idrobo, and Titiunik, 2019) as a baseline and use the continuity-based approach as a robustness check. Intuitively, this method assumes that units whose value of the running variable lies within a small window around the cutoff can be analyzed as if they had been randomly assigned to treatment and control, instead of relying on extrapolation techniques based on estimated polynomials. The estimated effect consists of a simple difference in means between units above the cutoff and units below the cutoff, restricting to observations within a window of the cutoff. We present baseline estimations with SSA data using a window from March 22 to April 12 (11-day window around the cutoff), which guarantees about 2,000 observations around the cutoff, and show that our results are robust to several alternative windows and to standard continuity-based approach. When using income tax data, given that we only have about 50% of the sample, we double the baseline window to 22 days around the cutoff, and show results are robust to other windows and a continuity-based approach. When pooling years together, we include year fixed effects and cluster standard errors at the worker level. When using census data, since the date of birth is at the month level, we calculate coefficients using the smallest window possible, which is one month. Thus, coeffi-

[36] The yearly individual RD plots for the main results are shown in appendix section A.5.

cients calculated using census data compare individuals born on April to individuals born on March.[37]

Figure 1.1 shows an empirical first stage of being in the new mixed system using the random subset of data for which we have information about their pension system. There is a substantial discontinuity at the cutoff date of birth, with an RD coefficient of 0.6541, indicating perfect compliance among individuals born after the cutoff date of birth.[38] Among the individuals that were left by default in the unfunded DB system, there is about a 20% of them that are in the new mixed system. This is primarily driven by individuals who chose it voluntarily within the 6-month period after the reform, while a lower share was assigned the new system because they had not had a formal contract registered with the SSA prior to the reform. Since we cannot merge the subsample of pension variables to our main sample of labor market variables, we present ITT estimates and use this first stage as informative of the degree to which the cohort-based discontinuity actually affected the pension system for workers.

1.6 Employment and earnings trajectories

In this section, we analyze workers' responses to the pension reform using the Regression Discontinuity methodology described in section 1.5. We begin by analyzing the effect of being assigned the new system on the probability of being employed in a given period. Then, we analyze the effect of being assigned the new pension system on the total reported earnings for a given period.

Employment responses

In this section, we analyze employment responses by estimating equation 1.1 with the dependent variable being an indicator of whether the worker is employed. To analyze the dynamics of the effect over time, we estimate a different RD coefficient for each group of years in our sample, which goes from 1997 to 2013.

Figure 1.2 shows the RD plots for the probability of being employed for a 60-day window around the cutoff date of birth. Each panel represents a different group of years. RD coefficients and p-values are calculated using an 11-day window around the cutoff. A summary of this exercise is shown in a time series plot of RD coefficients and confidence intervals in panel (a) of figure 1.5. There is no significant difference for observations around the cutoff for the first 15 years, while significant differences arise as workers draw closer to retirement.

[37] Note that the cutoff date of birth for being assigned the new system was the second day of April. This implies that comparisons of individuals born on April to those born on March imply some units that we count as being in the mixed system actually remained by default in the unfunded DB system (those born on April 1st). Thus, although the influence of only one day is likely to be minimal, our results using census data should be interpreted as a lower bound of the true ITT.

[38] This number is remarkably similar to the share of "passive savers" from Chetty et al., 2014, also being suggestive of the effect of default options.

Specifically, workers in the new mixed system are almost 5 percentage points more likely to be formally employed in the 2012 and 2013 (when they are 57 years old). Thus, it seems that the introduction of capitalization induced workers to remain employed closer to the retirement age.

We complement these findings that use administrative data using complementary information from the 2011 census. Importantly, the census data has the advantages of actively asking workers whether they are retired and of covering any type of employment, including informality (although individuals were not asked whether they were formally or informally employed). Since the date of birth information from the census is at the monthly level, we show RD plots within a 6-month window around the cutoff and calculate the RD coefficient as the average difference between individuals born on April and those born on March of 1956.[39] Panel (a) of figure 1.3 shows the results from this exercise for the probability of being employed in 2011, indicating that individuals in the new system are 2.2 percentage points more likely to be employed.[40] Panel (b) indicates that this is in large part due to a lower probability of having retired. Specifically, workers in the new system are 2 percentage points less likely to report being retired. This is consistent with the simple conceptual framework from section 1.3, given that capitalization systems tend to create incentives to postpone retirement (since this implies an increase in the accumulated funds and these are spread over less time periods).

We then explore heterogeneity of the effect using characteristics available in census data. The results from this exercise can be found in table 1.2. In columns 1 through 4 the dependent variable is an indicator of being employed and in columns 5 through 8 the dependent variable is an indicator of being retired. Columns 1 and 5 present the baseline RD coefficients presented in figure 1.3. Columns 2 and 6 interact the indicator of the mixed system with a dummy variable for having an above median socioeconomic status index. In both cases, the effect of being assigned the mixed system is canceled out for individuals with higher socioeconomic status, indicating that the reform induced individuals with lower socioeconomic status to remain employed and postpone retirement.

The largest heterogeneity is of the effect is driven by whether the individual experiences some mild disability, reported in columns 3 and 7. The census survey asks individuals if they are experiencing any degree of difficulty along four categories: (i) eyesight, (ii) hearing, (iii) physical movement, and (iv) cognitive. Respondents can answer in four degrees: no difficulties whatsoever, minor difficulties, moderate difficulties, and complete inability. We categorize individuals as experiencing some disability if they report having at least moderate

[39]Results using census records should be interpreted as lower bounds of the true effects of interest, since some workers that appear in census data may belong to smaller independent pension systems that were not privatized. For instance, the Military and the Police have their own pension systems that were not subject to the privatization. However, these alternative systems are small relative to the main system run by the SSA (accounting for less than 5 percent of workers), and if workers in one of these alternative systems ever worked occupations covered by the SSA, then they would be affected by the cohort-based discontinuity.

[40]Note that the census data covers both formal and informal employment, thus the effects found on census data and administrative data could be slightly different.

difficulties in at least one category. The effect is much larger for individuals experiencing some disability, who are an additional 10 percentage points more likely to be employed and almost 11 percentage points less likely to be retired. Panels (c) and (d) of Figure 1.3 presents the results separating individuals with some disability (in red) and those with no disabilities (in blue), while panels (e) and (f) do so separating individuals by above median SES (in red) and below median SES (in blue).

It is worth noting that the disabilities listed in the census are unlikely to qualify for permanent retirement due to disability. In order to qualify, the law in Uruguay requires individuals to be deemed completely unable to perform any job.[41] Thus, this result on early retirement potentially reflects choices of early retirement under *regular* pension rules. In such cases, early retirement in the mixed system can imply a substantial drop in pension wealth, since it implies cutting short the accumulation in the pension fund and dividing its amount over a larger number of years. Thus, these results are indicative of the "marginal" worker that responds to the incentives to postpone retirement inherent to a pension system that involves capitalization. Given how low socioeconomic status and experiencing some disability are significant predictors of early retirement, our takeaway from the employment and retirement response is that the privatization induced a postponing of retirement among workers who often tend to retire earlier.

A potential concern for interpreting heterogeneity in the results in the census is that some individuals born before the cutoff are in the new mixed system (if they never had a formal contract before or if they chose it voluntarily within 6 months of the reform). If being in the new mixed system is correlated with socioeconomic status or having a disability, then it could bias the heterogeneity coefficient. For example, if a large share of high SES workers born before the cutoff voluntarily chose the new system, then the interaction coefficient between the ITT and high SES would be lower simply because the high SES workers in the control group are actually in the new system. However, note that this effect is bounded. Given the empirical first stage that we document in figure 1.1, about 18% of workers born in the month before the cutoff are in the mixed system. Thus, assuming the worst case scenario in which everyone who is in the new system before the cutoff is high SES, this can only bring down the coefficient by about 36% (18 divided by 50). A similar logic applies to the coefficient on having a disability: the extent to which the effect can be amplified or attenuated is limited.

Earnings responses

In this section we analyze earnings responses by estimating equation 1.1 with the dependent variable being the log of total labor earnings reported in the period. Again, to analyze

[41]For the cases of retirement due to complete disability, workers in the unfunded DB system have their benefits being funded through payroll taxes on active workers. For the case of workers in the new mixed system, the DB part of the pension is determined as in the transition system, while the DC private part of the pension is covered in part by mandatory insurance the pension funds have to purchase and the capitalized funds can be provided either as an annuity or be completely withdrawn from the account upon retirement.

the dynamics of the effect over time, we estimate a different RD coefficient for each group of years in our sample, which goes from 1997 to 2013.

Figure 1.4 shows the RD plots for the natural logarithm of the monthly salary for a 60-day window around the cutoff date of birth. Each panel represents a different group of years. Again, RD coefficients and p-values are calculated using an 11-day window around the cutoff. A summary of this exercise is shown in a time series plot of RD coefficients and confidence intervals in panel (b) of figure 1.5. Notably, workers in the new mixed system report significantly higher earnings in the first few years after the reform, and this difference shrinks over time as years go by. Specifically, workers in the new system report salaries about 20% higher in the year immediately after the reform entered into effect, a difference that persists with for several years until it is no longer significant by the years 2012 and 2013. This is reflected in Panel A of table 1.3.

This increase in earnings is consistent with our simple conceptual framework from section 1.3, since capitalization creates incentives to increase labor earnings early on compared to an unfunded DB system. For the workers in the new system a fraction of their contributions are deposited into their retirement accounts, whereas for the workers in the unfunded DB system the pension is determined only by their last 10 years of labor earnings. Thus, at the time of the reform, when workers are 40 years old, contributions for workers in the unfunded DB system are potentially perceived purely as a tax, whereas the link between contributions and eventual pension benefits is tighter for workers in the new mixed system.

We then turn to analyze whether this increase in earnings is a real response or a reduction of underreporting of income. Income underreporting in Uruguay is widespread: data from household surveys indicate that over 10% of formal workers admit to underreporting their income for tax purposes (Bergolo and Cruces, 2014). Given that the new system created a stronger link between contributions and eventual pension benefits, it is plausible that it could have affected income reporting decisions among workers. Although it is challenging to evaluate effects on income underreporting using administrative data (given that underreporting is, by definition, unobserved), we explore several complementary pieces of evidence to attempt to understand the degree to which less underreporting of income is driving the increase in earnings.

We begin by analyzing the effect of being assigned the new mixed system on the number of days and hours worked, which we interpret as a measure of "real" effort. If workers are reporting higher wages because they are effectively working more, we should expect them to also be working more days and hours.[42] Panel (a) of figure 1.6 shows the time series plot of RD coefficients for the days worked per month and panel (b) does so for monthly hours worked. Notably, there is no significant difference in days or hours worked across both workers. Thus, during the years that workers in the new system are reporting significantly higher earnings they report similar days and hours worked. This is consistent with lower underreporting of earnings driving the increase in earnings for workers in the new system.

[42] In appendix section A.7 we show that days and hours worked are significant predictors of earnings, exploiting variation both across and within workers.

We then analyze the effect on reported earnings for workers in the public sector, where income underreporting is virtually non-existent, compared to the effect for private-sector workers discussed above. Panel (c) of figure 1.6 compares the time-series plot of RD coefficients for reported earnings in the private sector (in black) to the coefficients for public-sector workers (in orange). Notably, there is no significant effect in any time period for public-sector workers, who are unlikely to be underreporting their labor earnings. This is reflected in Panel B of table 1.3. Thus, this is also consistent with lower underreporting of earnings driving the increase in earnings for workers in the new system.

We then analyze the effect on reported earnings depending on the level of informality at the sector-level. Using the main household survey in the country, we construct two measures of informality at the sector level: (i) the proportion of workers that report not contributing to the pension system (which is a standard indicator of unregistered employment in Latin America, see Tornarolli et al., 2014), and (ii) the proportion of workers who admit to underreporting income in their contributions (reported in columns 1 and 2 of table A.1). We conduct a Principal Component Analysis of these two measures to construct an index of informality at the sector level, which is reported in column 3 of table A.1. We report the time series of RD coefficients in panel (d) of figure 1.6, where red coefficients correspond to high informality sectors and blue coefficients correspond to low informality sectors. Notably, the effect on reported earnings comes almost exclusively from sectors with higher levels of informality. This is reflected in Panel C of table 1.3.[43] This finding is also consistent with lower underreporting of earnings driving the higher earnings for workers in the new system.

We then analyze the effect for firm owners and self-employed workers compared to employees. The literature has established that self-employed workers are more responsive to incentives in tax schedules, and these responses are presumed to be due to underreporting of earnings (Saez, 2010). We exploit that the administrative records indicate whether the worker is an owner or an employee, and we present the time-series plot of RD coefficients separately for each group in panel (e) figure 1.6. Red coefficients correspond to firm owners and the self-employed while blue coefficients correspond to employees. Even though there are power limitations in this analysis, it is notable that the increase in reported labor earnings is substantially higher for firm-owners and self-employed workers. This is reflected in Panel D of table 1.3. Given that owners and self-employed workers are typically more able to conceal their income, this is also consistent with lower underreporting of earnings driving the increase in earnings for workers in the new system.

Finally, we provide some exploratory discussion as to why the increase on reported earnings seemingly fades over time. The first factor could be that, given that normal retirement ages range between late fifties and mid sixties, as workers enter their fifties, the ones that remained in the unfunded DB system enter their 10-year window during which their earnings history determines their pension benefits, creating incentives to increase reported earnings. Indeed, Dean, Fleitas, and Zerpa, 2022 use a random subset of administrative data and find

[43]Appendix figure A.8 shows the time series plot for high and low informality sectors using employees only, indicating that the increase in reported earnings from employees is also driven by high informality sectors.

that once self-employed workers and employees in small firms enter their fifties there is an increase in reported earnings, consistent with the fact that workers that enter their 10-year window reduce the amount they underreport for social security contributions. Thus, it seems plausible that one of the factors that drive the fade-out of the increase in reported earnings.

Second, starting in 2005, the government reintroduced collective bargaining in wage-setting, which implied less flexibility to set individual wages. Collective bargaining had been a staple of wage-setting in Uruguay until the military dictatorship of 1973-1985 eliminated it, and the democratic governments in the 1990s and early 2000s did not reinstate it. In 2005, the newly-elected center-left coalition gradually reintroduced collective bargaining for workers in various sectors (Mazzuchi, 2009). This, in turn, implied less flexibility for individual workers to be able to negotiate for their own wages, which potentially explains part of the fade-out in the increase in reported earnings.

Third, the spread of income underreporting for social security contributions measured through household surveys has seen a steady decline across-the-board during the sample period. Figure A.11 shows a time series plot of the proportion of survey respondents that admit to underreporting their income for the purposes of social security contributions. Notably, the proportion of workers that admit their income is underreported falls from around 10% to about 6.9%. This aggregate trend could be due to a variety of factors, in part related to the privatization of the pension system, but also the introduction of the income tax and an increase in enforcement efforts (Bergolo et al., 2021), in addition to the country having experienced a period of strong sustained growth and reduction of overall informality.

Finally, there is non-random selection among those who remain employed or retire early. As discussed in the previous section, individuals with lower socioeconomic status and with mild disabilities are more likely to remain employed and not retire early due to the reform. Thus, it is plausible that some of the individuals in the mixed system who remain employed later on are actually producing lower earnings.

Robustness checks

In this section, we present several robustness checks to validate our empirical strategy. The first concern that typically arises with Regression Discontinuity Designs is whether individuals are able to manipulate the running variable, since that could potentially induce sample selection bias. In our specific setting, this manipulation would involve workers born after the cutoff date of birth (and therefore assigned to the new mixed system) changing their date of birth to an earlier date before the cutoff.[44] If this selection was not random, it could introduce a bias that would drive our results. For example, if low-earnings individuals assigned the new mixed system decided to switch their date of birth so as to remain in the unfunded-DB system, that could create patterns of earnings like the ones we observe.

[44]Changing the date of birth for individuals born up until the cutoff would not be necessary, since these individuals were free to choose the new mixed system voluntarily in the 6 months following the reform.

Fortunately for our empirical strategy, workers were not able to modify their dates of birth. Dates of birth in the data are taken from social security records based on workers' birth certificates. Panel (a) of figure 1.7 shows a frequency histogram of observations within a 60-day window of the cutoff date of birth. There are no significant visual indications of bunching at dates of birth below the cutoff. This is further reflected in panel (b), which shows the results of a manipulation test based on local polynomials from Cattaneo, Jansson, and Ma, 2020. The p-value for the null hypothesis of no manipulation is 0.55, well above conventional significance levels. Thus, it seems implausible that sample selection via manipulation of the running variable is driving our results.

Another potential concern for our analysis is whether the date of birth chosen as cutoff by the government was set at a specific value where individuals who differ significantly in unobservable characteristics were left on each side, particularly considering that individuals born at different points within the year often have different outcomes later in life (Buckles and Hungerman, 2013). In our specific setting, this could drive our results if, for instance, the cutoff date of birth was set at a value such that higher-earning individuals were assigned the new mixed system. However, that was not the case: the cohort-based discontinuity for the social security system was introduced for the first time in the 1996 reform and it was not related to any characteristics of the individuals born around that time (Forteza and Rossi, 2018).

Unfortunately, we do not have information on labor earnings and employment rates prior to 1996 so as to be able to show pre-reform balance between the two groups. This is because the labor histories records were constructed specifically as part of the 1995 law that privatized the system. However, we can conduct placebo checks that individuals born at the same date but in different years show different employment and earnings. We conduct such placebo tests for individuals born in the year before (1955) and the year after (1957) the cohort that was affected by the reform.[45] We find no significant patterns of differential behavior for these placebo cohorts in employment rates from administrative data labor earnings (panels (a) and (b) of figure 1.8, respectively), or employment rates and retirement rates from census data (panels (c) and (d) of figure 1.8, respectively).[46] In addition, we find a perfect balance of observable characteristics using census data across several variables, such as being married, having completed a college education, having any children, having some disability, socioeconomic status, and being female (table 1.5). We interpret this as additional evidence that the RD coefficients estimated for the treated cohort reflect the effect of the reform and not some underlying characteristics inherent to individuals born around the cutoff date of birth set by the government.

We also show that our results are robust to alternative windows around the cutoff and to standard continuity-based regression discontinuity. Intuitively, the standard way for selecting a window when using the local randomization approach is to select it in a way such that pre-

[45]Note that individuals born in the year before the treated cohort were all left in the unfunded-DB only system by default, while individuals born in the year after the treated cohort were all assigned the new mixed system.

[46]The individual RD plots for each of the placebos can be found in appendix section A.4.

determined observables remain balanced across both groups around the cutoff, which would suggest balance on unobservables. However, the lack of pre-reform data precludes us from optimally determining the window in our case. When we perform this process using the `rdwinselect` routine from Cattaneo, Titiunik, and Vazquez-Bare, 2016 using indicators for gender and foreign-born, which are likely to be unaffected by the reform, we find balance around all windows around the cutoff. However, we choose to focus on narrow windows out of concerns that individuals born in different times of the year tend to show different outcomes (e.g. Buckles and Hungerman, 2013).[47] Thus, we show that our results do not change when considering slightly different windows around the cutoff. Panel (a) of figure A.9 compares the baseline estimates using alternative windows for the probability of being employed and panel (b) does so for reported labor earnings. To keep the plots tractable, we present two alternative windows: estimates using an 8-day window (in blue) and using a 14-day window (in green), but results remain similar when considering several alternative windows. In both cases, the baseline estimates are very similar to estimates using alternative specifications. We also conduct a standard continuity-based regression discontinuity fitting a quadratic polynomial, the estimates of which are shown in purple, and find very similar results.

Another potential concern is that contributions for the workers left in the default transition system comprise all of their labor earnings, whereas for workers in the mixed system contributions beyond a ceiling are voluntary. Thus, an increase in wages for workers in the mixed system could arise simply as a response to a ceiling in the schedule of contributions. This concern is minor for several reasons: (i) the ceiling for mandatory contributions is very high, located at about the 98th percentile of the wage distribution; (ii) we top code earnings at the 99 percent, which reduces the influence that some high values can have in our estimation; and (iii) some workers get deductions on earnings beyond the third threshold, that they can then choose to either leave in their retirement fund or get reimbursed for (for example, workers with multiple jobs and total earnings above the ceiling). To further alleviate this concern, we estimate the main effect on earnings while dropping all workers with earnings above the ceiling. Note that this is an overly-conservative estimate, since workers can contribute voluntarily above the ceiling. The results from this exercise can be found in appendix figure figure A.10, which shows similar results using the full sample or dropping workers with earnings above the ceiling.

1.7 Income in old age

In this section we analyze the effect of the reform on income and poverty in early old age, in addition to the decision to reverse to the unfunded DB system, applying the regression discontinuity methodologies described in section 1.5. The analysis on income in old age

[47]Indeed, figure A.12 shows that college completion rates vary significantly by month of birth, being significantly lower for individuals born during the winter, which resonates with findings in the United States (Buckles and Hungerman, 2013) and can potentially explain some of the slopes we find in the RD plots.

presents a series of challenges that are important to bear in mind. First, ideally more time would have passed since the reform. As of 2022, workers in the key privatization cohort are 66 years old, which implies that potentially many of them remain active in the labor market and not living off a pension. Adding to this is the fact that additional data that could be particularly useful, such as a census, is unavailable aside income tax returns up until 2016. Second, compensation policies started in 2014, and particularly the reversal options sanctioned in 2017, can confound the effects. Intuitively, if the reform created winners and losers, and the losers were compensated, this might bias the "reform" side of the discontinuity towards better outcomes, but this would not be due to the beneficial effects of the reform but rather due to losers being compensated. In addition, allowing reversals in the Article 8 choice that the 2014 law allowed and the communication of a future compensation policy could also induce differential responses from workers. Third, there are differences in labor supply across groups that we documented in the previous section and can drive differences in incomes in early old age.

With these caveats in mind, we first study the total income in old age across the original privatization discontinuity, that is, people born within a few days from the April 1st 1956 cutoff. This analysis can shed light on how income in old early old age varies across the two systems until workers are 60, prior to the reversals, accounting for potential differences in labor supply and pension income. Then we analyze the decision to reverse to the unfunded DB system, we analyze the closing of retirement accounts around the cutoff for the first cohort allowed to reverse, that is, people born within a few days from the April 1st 1960 cutoff. This analysis can shed light as a measure of "revealed preference" for the non-privatized unfunded DB system.

Income and poverty in early old age

In this section, we analyze the effect of the reform on income and poverty rates in early old age, from the ages of 53 to 60. This encompasses the years 2009 through 2016, prior to the reversal policy sanctioned in 2017. We begin by showing that retirement rates post-2013 converge to more similar levels. Figure 1.9 shows the RD plots for the probability of being employed and figure 1.10 for the probability of being retired. Panel (a) of figure 1.13 shows the time series plot of RD coefficients for being employed and panel (b) for the probability of being retired, indicating that workers assigned the mixed system are significantly less likely to be retired in the early years from 2009 to 2013, consistent with our findings from the previous section. By the year 2014, these differences are smaller and we cannot reject equality of retirement rates.

We then proceed to analyze how total income in old age differs across workers in the two systems. We calculate the total income as any pension income plus any labor earnings income, including zeroes. This measure of income represents the total income that workers have in early old age, which can reflect differences in both labor supply behavior and potential differences in pension income. Figure 1.11 shows several RD plots for the total income in early old age, none of them showing significant discontinuities around the cutoff. Panel (b) of figure 1.13 shows a time series plot of RD coefficients with confidence intervals. Overall, we do not find significant differences in total income in early old age: RD coefficients are small, and despite power limitations we are able to reject changes in total yearly income of a few thousand Uruguayan pesos, relative to averages of over two hundred thousand pesos.

We then analyze how poverty rates differ across workers in the two systems. For this exercise, we annualize the monthly individual national poverty line for the city of Montevideo (the capital in the country), multiplying the December value by twelve. We then create an indicator equal to 1 if the workers total income, measured as any pension income and labor earnings, is below this annualized poverty line. Figure 1.12 shows several RD plots for the probability of total income being below the annualized poverty line, with none indicating significant discontinuities around the threshold. Panel (d) of figure 1.13 shows a time series plot of RD coefficients with confidence intervals. Although coefficients are typically negative, we do not find statistically significant differences in the likelihood of total income in early old age being below the poverty line.

Results in this section indicate that workers across both systems have similar total incomes and are similarly likely to be below the poverty line, although minor differences the probability of being retired persist. This could imply that workers in the mixed system are slightly more likely to be working for a similar income that workers in the unfunded DB system are able to get through a pension. However, even though retirement rates are more similar towards the end of the sample period, retirement rates are below 50% by 2016, and compositional effects regarding labor supply could drive similarities in total income. For instance, it could be that workers who lost pension income with the reform keep on postponing retirement while those who gained pension income are able to retire earlier. It could also be that some workers who potentially gained pension income with the reform could be postponing retirement due to the incentives that increasing the annuity from the pension fund generates. Thus, we interpret findings in this section as not documenting stark patterns that lead to strong conclusions about the incomes of workers in early old age.[48]

Revealed preferences from 2017 reversal policy

In this section, we analyze the reversal policy of 2017 to analyze a "revealed preference" measure for the new mixed system using the regression discontinuity methodology described in section 1.5. We leverage the fact that individuals born up until April 1st of 1960 were allowed to reverse to the unfunded DB system during the period of March 2018 to March 2019. If they choose to do so, they transfer their pension fund to the government and contribute only to the public unfunded system until they retire, upon which they receive a DB pension from the government. This implies that their retirement account has to be closed after choosing to reverse.

[48]For instance, if we saw workers in the new system being more likely to be under poverty, this could be indicative of detrimental effects of the privatization, since workers would be more likely to be working and to be below the poverty line.

We begin by analyzing whether people took up this reversal option, measured by whether their retirement account was still active by March of 2019, which is the last month this cohort had to choose whether to reverse or not. Panel (a) of figure 1.14 presents the RD plot for the probability of the retirement account being active for a window of 120 days around the cutoff date of birth.[49] Notably, there is a significant reduction in the probability of the account being active: workers allowed to reverse are 9.3 percentage points less likely to have an open retirement account, and this effect is highly statistically significant. Given that about 80% of retirement accounts in the control group are active by March 2019, this 9.3 percentage point drop implies an 11.62% reduction in the probability of remaining in the mixed system.

Even though the majority of workers do not reverse back to the unfunded DB system, stayers should not be interpreted as strictly winners from the reform. For instance, the literature has established that default effects play a significant role in individuals' choices, in the sense that people are likely to remain within default options assigned to them and not actively choose alternatives (Madrian and Shea, 2001; Carroll et al., 2009). This could imply that some workers who lost pension income from the reform do not switch, even if it could be profitable for them. In addition, reversing to the unfunded DB system did not imply getting a pension exactly at the level of the transition system, but rather at 90% of the benefits from it. Thus, workers with small estimated losses from the privatization may not switch simply because these losses do not amount to a reduction of over 10% relative to the transition system. Thus, our interpretation is not of switchers as losers from the reform and stayers as winners, but rather as switchers representing workers whose pension income was significantly reduced by the privatization.

Although the pension fund records are not merged with the labor histories data, which precludes from pinpointing exactly the people who lost pension income, we are able to conduct a series of additional analyses to understand who the switchers are. The first source of heterogeneity that we analyze is the choice of the Article 8 option, which allows workers to contribute to their retirement account even below the initial threshold. Under this option, workers evenly divide their contributions between their retirement fund and the public unfunded DB system on earnings below the threshold. This reduces the subsequent government pension and increases the retirement savings component. However, the salary for the government pension calculation drops by 25% and not by 50%, which implies a subsidy for this option. Given the formulas and the evolution of returns over time, the choice of Article 8 can result significantly profitable for workers with low and middle earnings, with gains in pension income of around 10% depending on the case (Forteza and Rossi, 2018).

Panel (a) of figure 1.15 shows the RD plot separately by individuals who chose Article 8 (in red) and those who did not (in blue). It is evident that the choice to reverse is significantly greater among workers who did not choose the Article 8 option, who are 23 percentage points less likely to have an open account if they are allowed to reverse. However, individuals who did choose Article 8 are still significantly likely to reverse, at a reduction of about 7.5 percentage points in active account rates. This finding suggests that individuals who do not optimally

[49] In appendix table A.2, we show that observable characteristics are balanced around the cutoff.

choose the most profitable retirement savings options within a funded pension system are likely to face detrimental effects on their pension income. This, in turn, can raise issues regarding how differences in degrees of financial literacy can determine winners and losers from a privatization.

We then separately analyze the effect for public sector and private sector workers, a distinction that does appear in the retirement fund records. Panel (b) of figure 1.15 shows the RD plot separately for private sector workers (in black) and public sector workers (in orange). Notably, the reduction in the probability of having an active account among those allowed to reverse is significantly greater for public sector workers, who are 12.7 percentage points less likely to remain in the mixed system, while the reduction for private sector workers is of about 7.9 percentage points. Although public and private sector workers differ in many regards (e.g. public sector wages are typically significantly larger), this difference in reversal rates can potentially be explained by the stark differences age-earnings profiles across the two sectors. Appendix figure A.15 shows age-earnings profiles for public and private sector workers separately using household survey data. These estimates show that, while private sector workers' earnings often peak in their forties, public sector workers often face much steeper age-earnings profiles, with significant peaks in their fifties that can persist into their sixties. Thus, even though public sector workers have higher salaries that can allow them to achieve significant retirement savings, the steepness of their age-earnings profile with a peak in their fifties can imply that the DB formulas that apply a replacement rate to the last few years of earnings history from the purely unfunded system can yield significantly higher pension incomes. Thus, we interpret this finding as indicative that workers with steep age-earnings profiles can face detrimental consequences in their pension income from a privatization.

We then analyze the effect depending on the profile of contributions throughout workers' careers in relation to the returns to the pension funds' investments. The evolution of the interest rate on the pension funds over time can be found in appendix figure A.17. The privatized system initially experienced significantly high returns during its early years, with real interest rates consistently around 10% a year. These returns spiked during the 2002 financial crisis, and eventually stabilized at a significantly lower level during the 2004-2007 period. During the Great Recession, interest rates briefly hit negative values, after which returns briefly recovered. This implies that workers who managed to contribute consistently during the early years of the system were exposed to better returns on their retirement savings, in addition to being able to save for retirement early-on. We take this into our empirical analysis by categorizing workers according to the share of their total periods of contributions made to the system prior to the fall of returns in 2005. We then separate workers into terciles of the share of their contributions being made during the early high-returns years and analyze the closing of their accounts separately. The results of this exercise can be found in panel (c) of figure 1.15. Notably, the closing of accounts is not as significant among workers in the upper-tercile of the share of contributions having been made during the early years of high interest rates. Thus, we interpret this finding as indicative that workers who contributed less consistently during the early years of high interest rates can face detrimental consequences in their pension income from a privatization.

Robustness checks

In this section, we present several robustness checks to validate our empirical strategy, starting with the results on income and poverty rates in old age. Although we already covered in section 1.6 that the date of birth was not manipulable, we conduct an additional manipulation test in the income tax data. This can alleviate concerns that the income tax returns sample, although not randomly selected, is not selected in a way related to the social security discontinuity exploited for our analysis, which could potentially bias our results. The density around the cutoff and manipulation test can be found in figure A.13, showing no signs of differential density at any side of the cutoff, and the p-value of the manipulation test exceeding any conventional significance threshold.

Similarly to the robustness exercises from section 1.6, we once again conduct placebo exercises comparing workers born at the same cutoff date of birth but on the years 1955 and 1957 (the year before and the year after the cohort affected by the reform, respectively). Figure 1.16 shows the results from these exercises. In all panels, we do not observe any of the patterns for the actually treated cohort, with placebo coefficients being mostly small and non-statistically significant for the probability of being employed or retired (panels a and b, respectively), and for the total income and the probability of being below the poverty line (panels c and d, respectively).[50] Thus, we interpret this as suggesting it is unlikely that the results are driven by a date of birth effect unrelated to the reform.

We also analyze different windows for the RD design to assess whether results are driven by the specific window we implemented. In our baseline estimation, we use a 22-day window, doubling our baseline window of 11 days from the previous section to compensate for the fact that we only have 50% the sample in the income tax data. We then compare our baseline estimates to alternative estimates using 19-day and 25-day windows. The results from this exercise can be found in A.14. In all panels, the patterns found with the baseline window are very similar to the those that use alternative windows around the cutoff for employment rates, retirement rates, total income in early old age, and the probability of total income being below the poverty line. We also estimate a continuity-based regression discontinuity by fitting a quadratic polynomial, the estimates of which are shown in purple, and find similar results.

We then move on to robustness checks for the analysis on the reversal policy. As with our previous analyses, a prime concern could be whether workers were able to manipulate their dates of birth in order to be allowed to reverse back to the unfunded DB system. This could induce sample selection bias if, for example, workers who especially need to reverse changed their dates of birth to earlier values in order to be able to reverse. This would imply that we would mechanically see more accounts being closed on one side of the cutoff. To assess this concern, we conduct another manipulation test around the de-affiliation cutoff, the results

[50]The individual placebo RD plots for each group of years can be found in appendix section A.4.

of which are reported in figure A.18, with panel (a) reporting the frequency of observations and panel (b) showing the manipulation test. We find no evidence of manipulation of the running variable, with the p-value well exceeding conventional significance thresholds.

Another potential concern is whether the government chose the new cutoff at a special date, for example, at a threshold such that more people had left the mixed system on the "reversal" side of the discontinuity. An advantage of the reversal policy is the existence of outcomes data prior to the policy being implemented, which allows us to conduct across-time placebos to analyze whether individuals at both sides of the discontinuity were similarly likely to be in the mixed system prior to the reform. Panel (b) of figure 1.14 shows a time series plot of RD coefficients for whether the account is active in March and October of each year. Notably, the evolution of open accounts is similar across both groups in the pre-reversals period, and we see a stark reduction in the probability of the account being active during the period that allowed reversals for the first cohort, which catches up with the control group once they are allowed to reverse as well. This suggests that the threshold was not chosen as a response to different pre-policy evolution of trends across the two groups.

Finally, we once again show that our result is robust to alternative windows around the cutoff and continuity-based specifications. Our baseline window is 11 days, the same as for the employment and earnings responses using SSA data. Similarly as for the analysis in section 1.6, we present two alternative estimates using an 8-day window and a 14-day window. The results from this exercise can be found in panel (b) of figure A.19. In both cases, the alternative windows yield similar results to the baseline window, indicating that our result is not driven by the specific window choice. We also estimate a continuity-based regression discontinuity by fitting a quadratic polynomial, the estimate of which is shown in purple, and find similar results.

1.8 Conclusion and discussion

This paper studies the effects of privatizing the pension system on workers' reported earnings, employment and retirement behavior, and income in old age. We analyze a pension reform in Uruguay that switched from a pay-as-you-go system with defined benefits into a mixed system that is part unfunded with defined benefits and part a funded system with defined contributions with individual retirement accounts. For identification, we leverage a cohort-based discontinuity in the introduction of the new system, which allows for clean identification with a RDD approach, while the availability of high-quality administrative data allows us to study the long-run trajectories of workers' responses and their well-being in old age.

In the first part of the paper, we study workers' responses to the privatization incentives. We find significant responses on the trajectories of employment rates and reported earnings. Regarding employment rates, we find that workers in the new system with retirement accounts show similar employment rates early on (when workers are in their 40s and early 50s), while they are significantly more likely to be formally employed closer to the retirement age, particularly among those with lower socioeconomic status and who experience some disability. Regarding earnings responses, we find that workers in the new system with retirement accounts report significantly higher earnings early on (when workers are in their 40s), and this difference shrinks as workers approach the age of retirement. We find several pieces of suggestive evidence that indicate that this increase in earnings is not a real labor supply response, but rather a reduction in tax evasion.

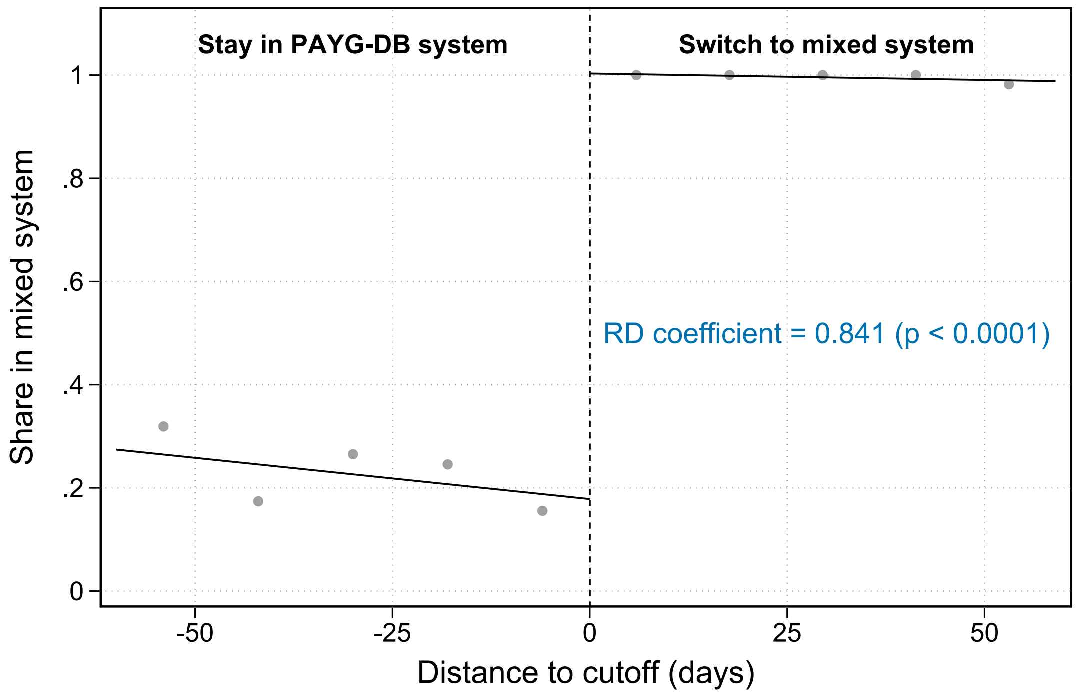
In the second part of the paper, we study the effects of the reform on workers' income in early old age and their preferences between the two systems. We find that total income, measured as pension plus labor earnings, and poverty rates are similar across the two systems, although minor differences in labor supply persist. Although this suggests that workers in the mixed system are more likely to be working for a similar level of income, we do not document stark patterns that lead to strong conclusions regarding winners and losers from the privatization. However, we document significant take-up of a reversal option that allowed workers to switch from the privatized system into the unfunded DB system, which we interpret as a measure of revealed preference for the non-privatized system. We find stronger reversals for people who did not choose the most profitable retirement savings option, which raises issues about inequities in privatizations depending on the degree of financial literacy. We also document stronger reversals for public-sector workers, who generally have steeper earnings profiles that can benefit from defined benefits formulas typically used in unfunded systems, and among those who did not consistently contribute during the early years where market returns on the pension funds were at their highest.

Given how privatizing social security is such a frequent policy proposal that has been implemented in numerous countries and is under discussion in several others, our findings bear important policy implications. Workers significantly respond to the transition from an unfunded system with defined benefits to a mixed system that includes individual capitalization. These responses are in expected directions, even as workers are decades away from retirement. This is especially important considering the existence of behavioral biases that could lead workers to underestimate the effects that their actions could have on their retirement benefits far away in the future, such as present bias or exponential growth bias (Goda et al., 2019), and the fact that workers often do not understand the complexities of pension benefits formulas (Liebman and Luttmer, 2012). Introducing a retirement accounts component can ameliorate distortions associated to short "windows" of years from which pension benefits are calculated, inducing workers to conceal less income, which potentially has important revenue implications that can help offset the large fiscal costs associated with privatizing social security. Our findings suggest that these short windows for calculating pension benefits that are customary in developing countries can contribute to widespread underreporting of labor earnings. In addition, introducing a funded DC component can induce workers to remain formally employed later in life, effectively postponing retirement. This is often seen as a positive aspect of funded DC systems, since workers postponing retirement can improve the financial sustainability of the system. However, the fact that the effect on remaining employed later in life is stronger for individuals of low socioeconomic status and those who experience some mild disability that is unlikely to qualify for permanent disability-related retirement can raise concerns about regressivity.

Regarding income in old age, our findings highlight some of the distributional consequences of pension privatization, which have often occupied a secondary role relative to efficiency concerns in the economics literature. Our measure of revealed preference indicates that a significant share of workers faced detrimental consequences due to the privatization. For instance, the fact workers who did not choose the most profitable retirement savings option in the new system are significantly more likely to reverse to the pay-as-you-go system can raise concerns that non-optimal choices within retirement accounts systems can affect workers' subsequent pension income. This is especially important since retirement savings choices are often complex and require a certain degree of financial literacy, suggesting that privatizations can be detrimental for less financially literate workers. In addition, workers who did not manage to contribute consistently during the early high-interest rate years of the system are also more likely to reverse back to the unfunded DB system, which illustrates how privatizations can be detrimental to workers who are exposed to worse market-returns on their pension funds. A similar logic applies to workers with steep age-earnings profiles who achieve peak years towards their fifties and early sixties, who likely would benefit from a defined benefits formula that replaces earnings close to the end of their career relative to relying on retirement savings they accumulated early-on while their earnings were relatively low.

1.9 Figures

Figure 1.1: First stage

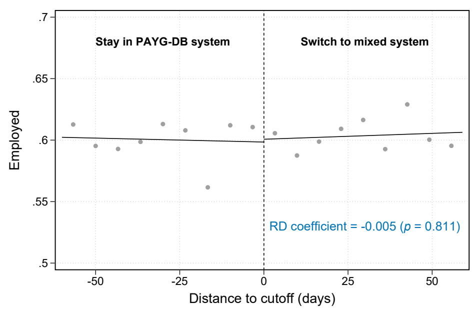


Notes: This figure plots the share of individuals that are in the new mixed pension system by equal-sized bins of distance to the cutoff date of birth. Individuals born before the cutoff were left by default in the pay-as-you-go system with defined benefits and individuals born at the cutoff or after were switched to the mixed system with retirement accounts. The dependent variable is an indicator that takes the value of 1 if the worker's pension system is the new mixed system (and 0 otherwise, such as being in the unfunded-DB only transition system or having scheduled retirement under a system in place prior to the 1996 reform). RD coefficients are estimated using a 11-day window around the cutoff date of birth and p denotes the p-value of the null hypothesis of no difference in the outcome variable across the two groups.

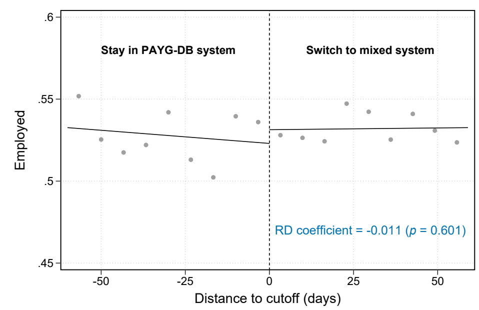
1.10 Tables

Figure 1.2: Effect of the reform on employment rates - SSA data

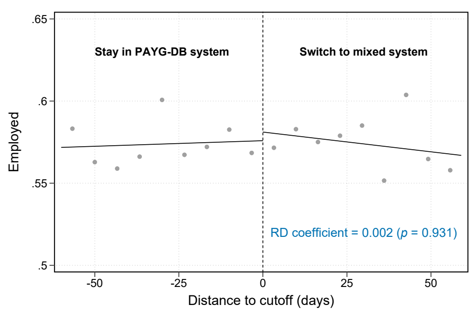
(a) 1997-2000



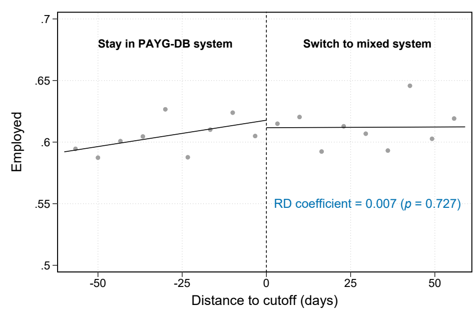
(b) 2001-2004



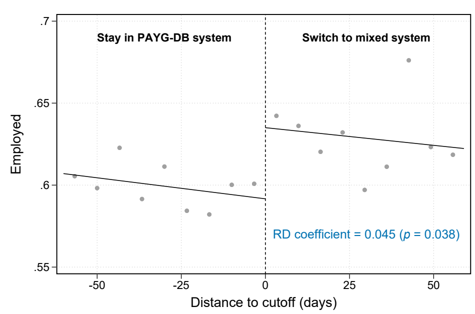
(c) 2005-2008



(d) 2009-2011



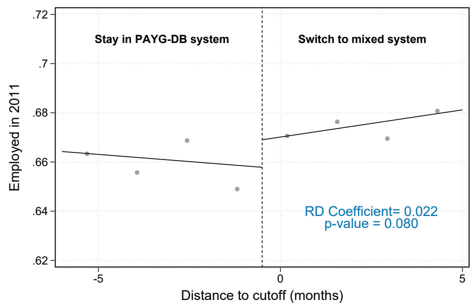
(e) 2012-2013



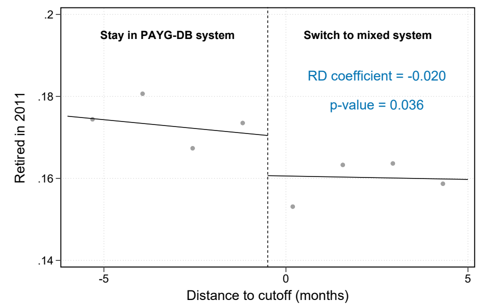
Notes: This figure plots the share of individuals who are employed by equal-sized bins of distance to the cutoff date of birth using social security data. Individuals born before the cutoff were left by default in the pay-as-you-go system with defined benefits and individuals born at the cutoff or after were switched to the mixed system with retirement accounts. Each panel corresponds to a different group of years. In all panels, the dependent variable is an indicator of whether the worker was employed (defined as reporting positive labor earnings). RD coefficients are estimated using a 11-day window around the cutoff date of birth and p denotes the p-value of the null hypothesis of no difference in the outcome variable across the two groups, calculated using worker-level cluster-robust inference. The dependent variable is residualized from year fixed effects and evaluated at the mean. The complete set of plots for all years can be found in appendix figure A.34.

Figure 1.3: Effect of the reform on employment and retirement - census data (inc. heterogeneity)

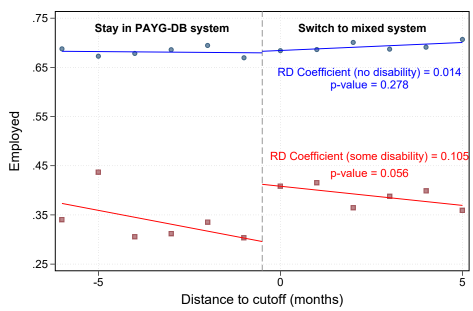
(a) Employment



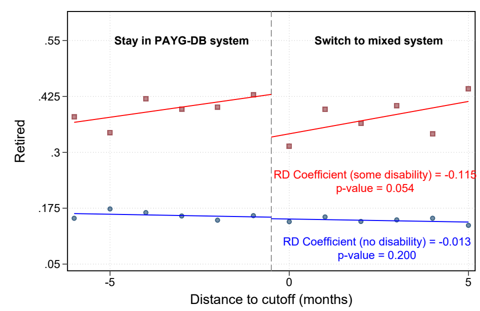
(b) Retirement



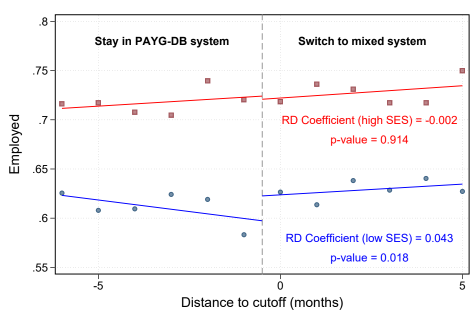
(c) Employment (by disability)



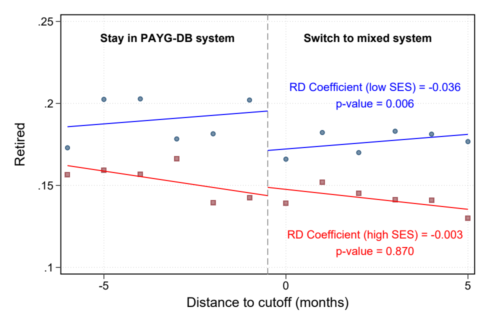
(d) Retirement (by disability)



(e) Employment (by SES)



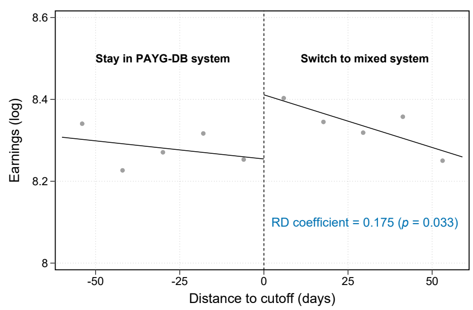
(f) Retirement (by SES)



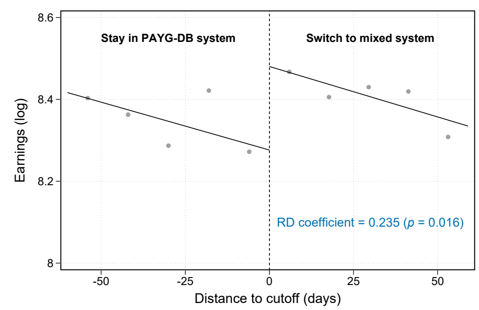
Notes: This figure plots the share of workers that are employed by bins of distance to the cutoff date of birth using census data. Panel (a) reports the effects for the probability of being employed. Individuals born before the cutoff were left by default in the pay-as-you-go system with defined benefits and individuals born at the cutoff or after were switched to the mixed system with retirement accounts. Panel (b) reports effects for the probability of being retired. Panels (c) and (e) shows the RD-plot heterogeneity for the probability of being employed and panels (d) and (f) for the probability of being retired. In panels (c) and (d) the color blue corresponds to individuals with no mild disabilities and the color red corresponds to individuals with some disability. In panels (d) and (f) blue corresponds to individuals with a below-median socioeconomic status index and red corresponds to individuals with an above median socioeconomic status index. RD coefficients are estimated by calculating average differences for individuals born on April and individuals born on March.

Figure 1.4: Effect of the reform on labor earnings - SSA data

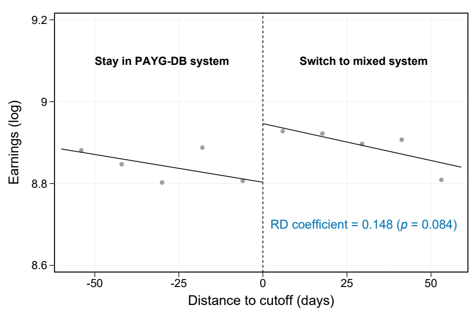
(a) 1997-2000



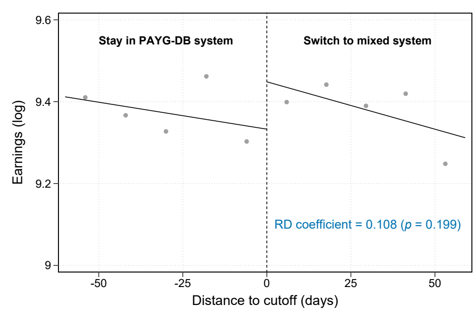
(b) 2001-2004



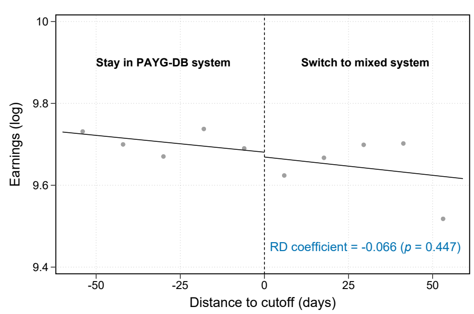
(c) 2005-2008



(d) 2009-2011



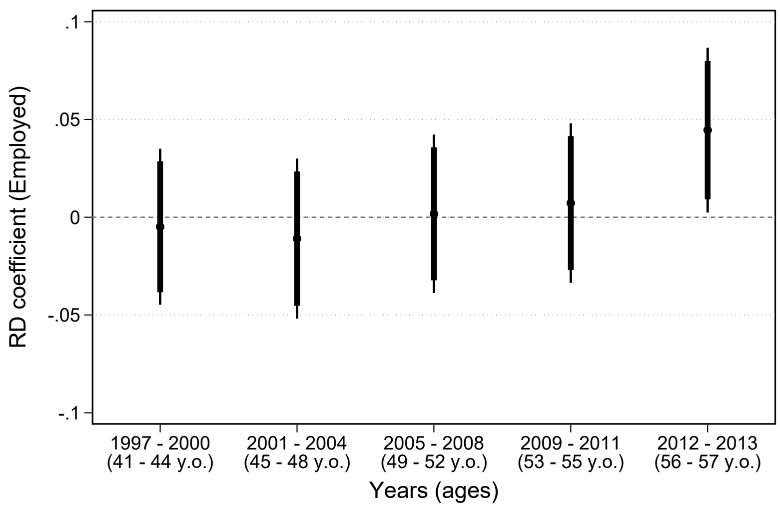
(e) 2012-2013



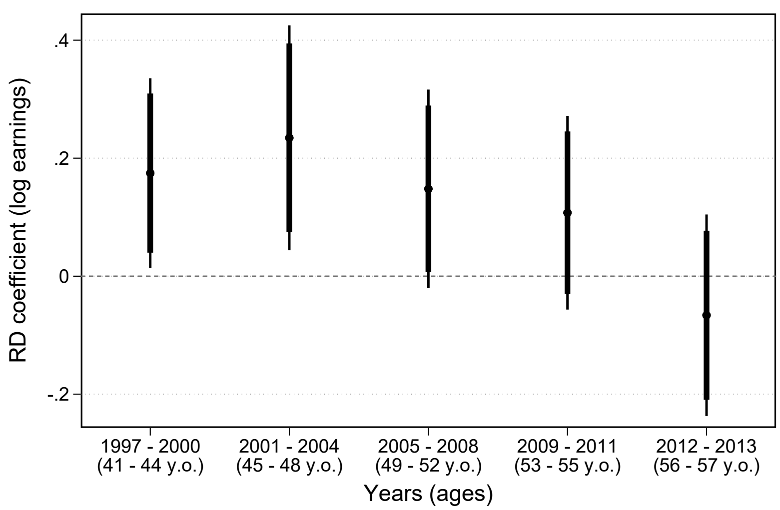
Notes: This figure shows the average log of labor earnings by equal-sized bins of distance to the cutoff date of birth using social security data. Individuals born before the cutoff were left by default in the pay-as-you-go system with defined benefits and individuals born at the cutoff or after were switched to the mixed system with retirement accounts. Each panel corresponds to a different group of years. In all panels, the dependent variable is the natural logarithm of total reported labor earnings. RD coefficients are estimated using a 11-day window around the cutoff date of birth and p denotes the p-value of the null hypothesis of no difference in the outcome variable across the two groups, calculated using worker-level cluster-robust inference. The dependent variable is residualized from year fixed effects and evaluated at the mean. The complete set of plots for all years can be found in appendix figure A.35.

Figure 1.5: Time series plots of RD coefficients - Employment rates and Earnings (SSA data)

(a) Employed



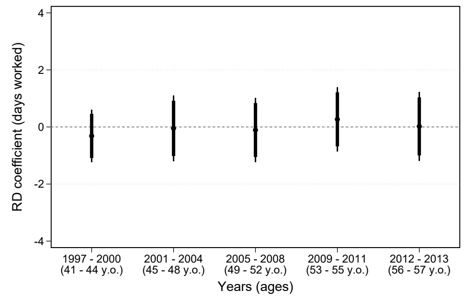
(b) Earnings



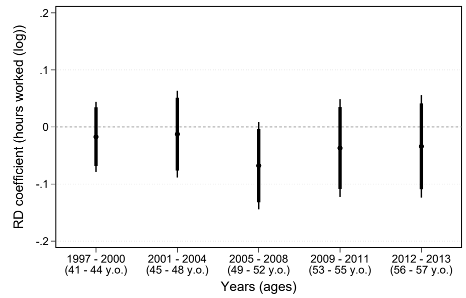
Notes: This figure shows two time series plots of RD coefficients using social security data. Panel (a) shows the RD coefficients for the probability of being employed and panel (b) shows the coefficients for the natural logarithm of total labor earnings. The numbers underneath the years indicate the ages of workers in those years. Thick vertical bars correspond to 90% confidence intervals and thin vertical bars correspond to 95% confidence intervals, both from worker-level cluster-robust inference.

Figure 1.6: Time series plot RD coefficients (labor supply and earnings heterogeneity - SSA data)

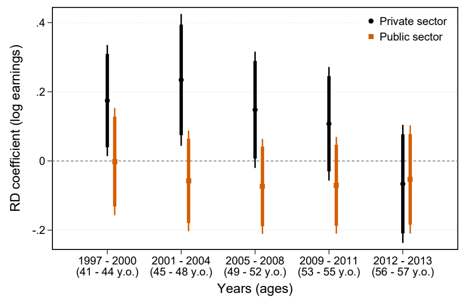
(a) Days worked



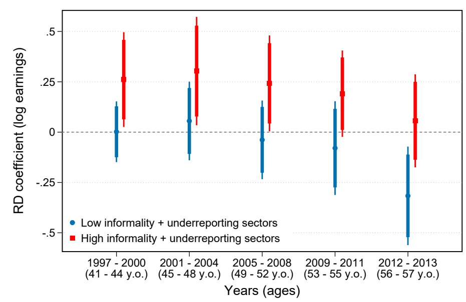
(b) Hours worked



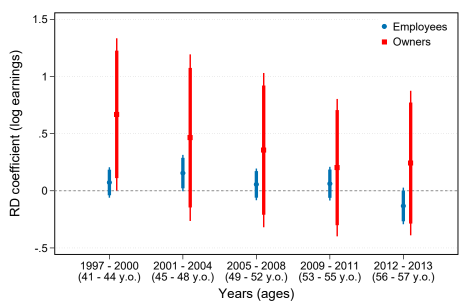
(c) Earnings - public vs private sector



(d) Earnings - high vs low sector-level informality



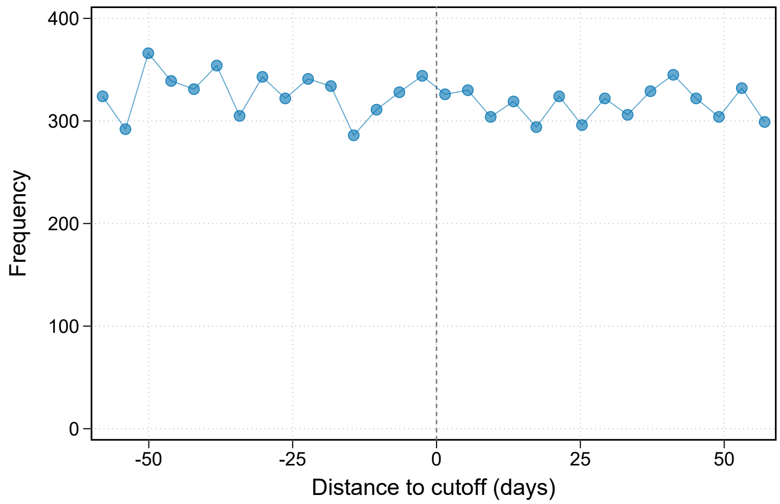
(e) Earnings - owner vs employees



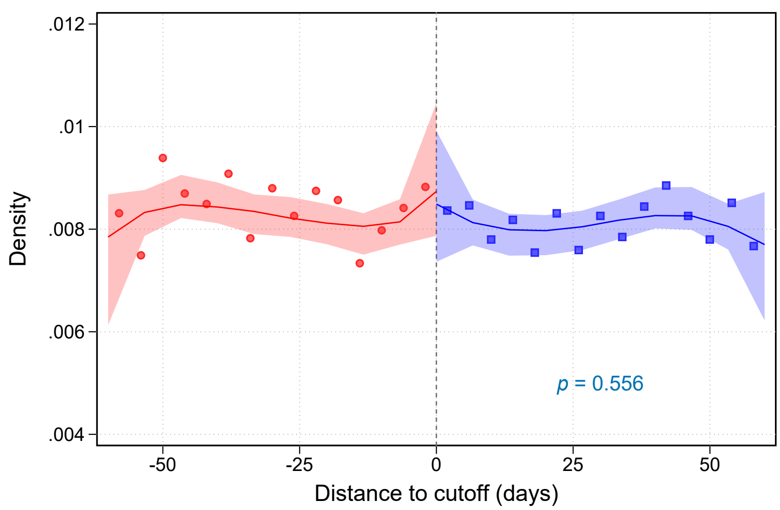
Notes: This figure shows several time series plots for the RD coefficients from equation 1.1 using census data. Panel (a) shows the effect on days worked in the month. Panel (b) shows the effect on the natural logarithm of monthly hours worked. Panels (c) through (d) show effects on earnings heterogeneity. In panel (c) black coefficients correspond to private-sector workers and orange coefficients correspond to public-sector workers. In panel (d) red coefficients correspond to sectors with high levels of informality and income underreporting and blue coefficients correspond to sectors with low informality and income underreporting. In panel (e) red coefficients correspond to firm owners and blue coefficients correspond to firm employees. The numbers underneath the years indicate the ages of workers in those years. Thick vertical bars correspond to 90% confidence intervals and thin vertical bars correspond to 95% confidence intervals, both from worker-level cluster-robust inference. The individual RD plots can be found in appendix figures A.3 through A.7.

Figure 1.7: Density around the cutoff and manipulation test (SSA data)

(a) Density around the cutoff



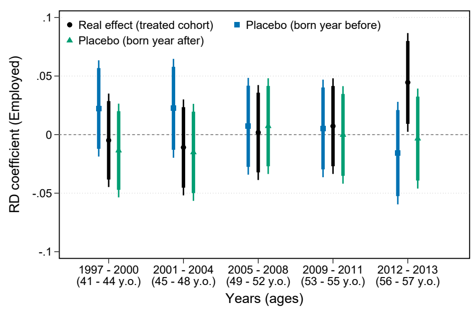
(b) Manipulation test



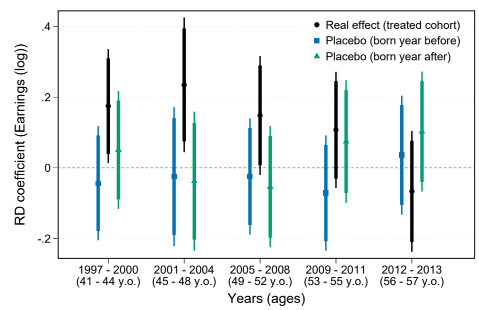
Notes: This figure shows the density of observations around the cutoff and a manipulation test for the running variable using social security data. Individuals born before the cutoff were left by default in the pay-as-you-go system with defined benefits and individuals born at the cutoff or after were switched to the mixed system with retirement accounts. Panel (a) shows a frequency histogram of the number of observations in 30 equally-spaced bins. Panel (b) shows a manipulation testing plot and a p-value for manipulation of the running variable based on local polynomials from (Cattaneo, Jansson, and Ma, 2020) using the `rddensity` routine from (Cattaneo, Jansson, and Ma, 2018).

Figure 1.8: RD coefficients - comparison with placebos (SSA and census data)

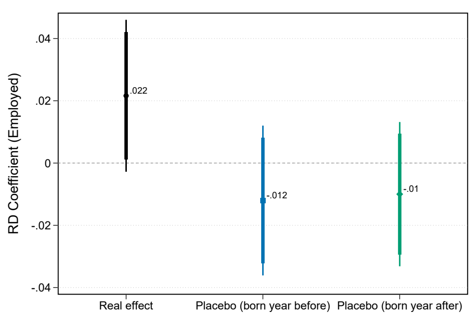
(a) Employment (SSA data)



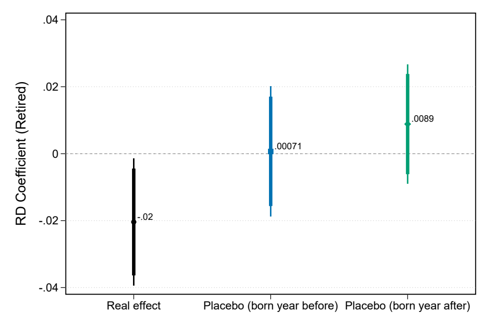
(b) Earnings (SSA data)



(c) Employment (Census data)



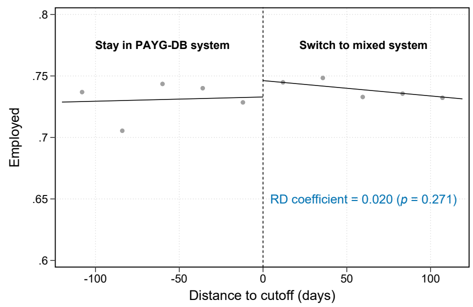
(d) Retirement (Census data)



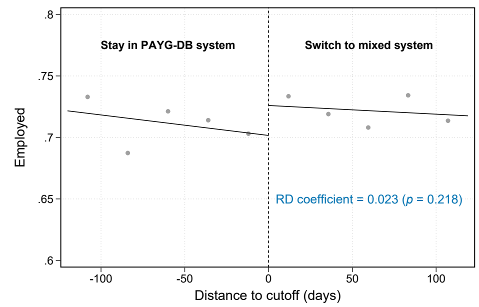
Notes: This figure shows a comparison of the main RD coefficients with placebos estimated using cohorts born on the year before and the year after the cohort affected by the reform. Panel (a) shows coefficients for the effect on the probability of being employed and panel (b) shows coefficients for the natural logarithm of labor earnings, both using SSA data. The numbers underneath the years indicate the ages of workers in those years. Panel (c) shows coefficients for the probability of being employed and panel (d) for the probability of being retired, both using census data. Thick vertical bars correspond to 90% confidence intervals and thin vertical bars correspond to 95% confidence intervals, both from worker-level cluster-robust inference. Black corresponds to estimates for the cohort affected by the reform. Green corresponds to estimates for the cohort born on the year after the cohort affected by the reform. Blue corresponds to estimates for the cohort born on the year before the cohort affected by the reform.

Figure 1.9: Effect of the reform on employment rates (IRS data)

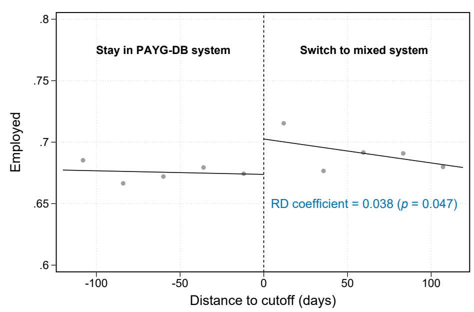
(a) 2009-2010



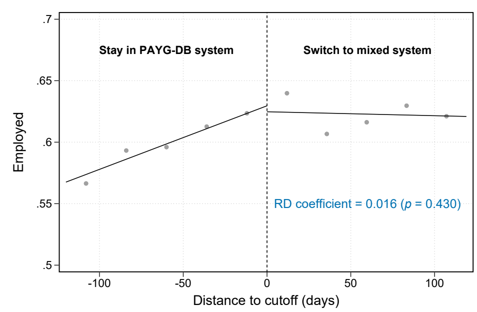
(b) 2011-2012



(c) 2013-2014

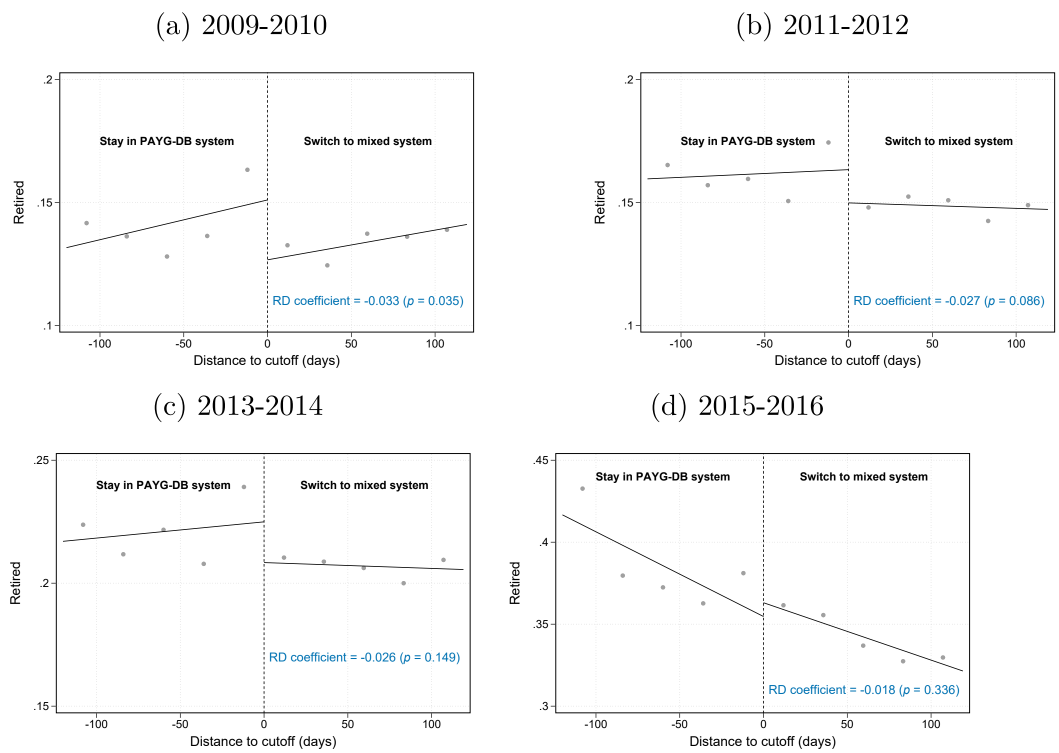


(d) 2015-2016



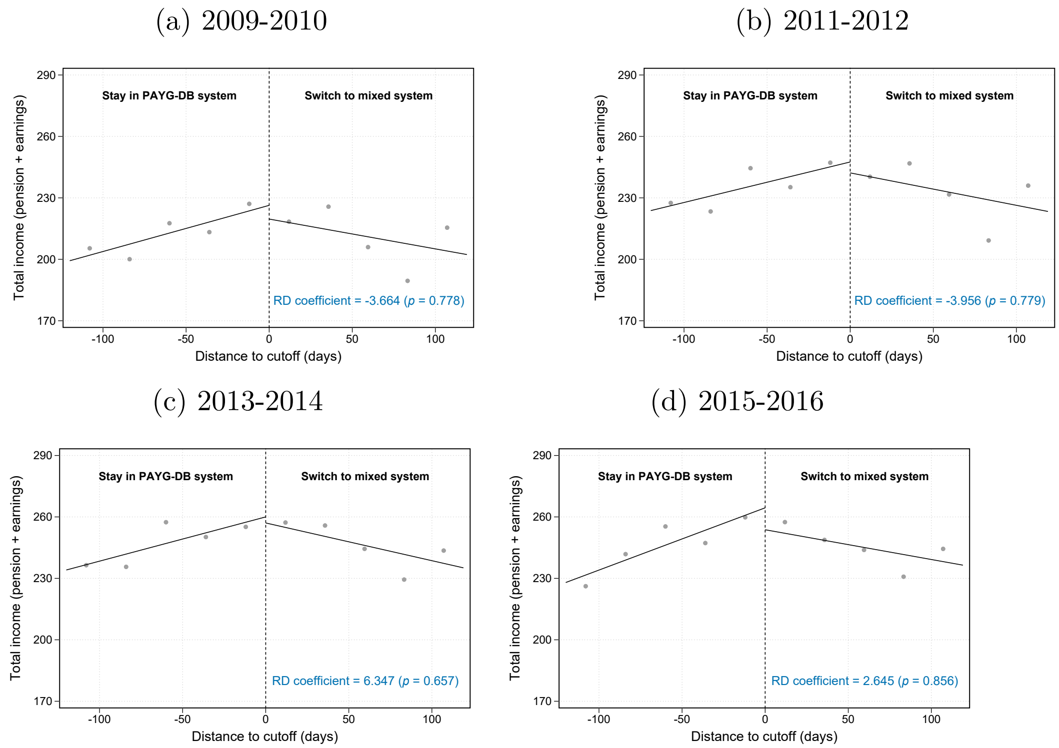
Notes: This figure shows the share of individuals who are employed by equal-sized bins of distance to the cutoff date of birth, using IRS data. Individuals born before the cutoff were left by default in the pay-as-you-go system with defined benefits and individuals born at the cutoff or after were switched to the mixed system with retirement accounts. Each panel corresponds to a different group of year. In all panels, the dependent variable is an indicator of whether the worker was employed (defined as reporting positive labor earnings). The dependent variable is residualized from year fixed effects and evaluated at the mean. RD coefficients are estimated using a 22-day window around the cutoff date of birth and p denotes the p-value of the null hypothesis of no difference in the outcome variable across the two groups, calculated using worker-level cluster-robust inference. The complete set of plots for all years can be found in appendix figure A.36.

Figure 1.10: Effect of the reform on retirement rates (IRS data)



Notes: This figure shows the share of individuals who are retired by equal-sized bins of distance to the cutoff date of birth, using IRS data. Individuals born before the cutoff were left by default in the pay-as-you-go system with defined benefits and individuals born at the cutoff or after were switched to the mixed system with retirement accounts. Each panel corresponds to a different group of year. In all panels, the dependent variable is an indicator of whether the worker was retired (defined as reporting positive pension income). The dependent variable is residualized from year fixed effects and evaluated at the mean. RD coefficients are estimated using a 22-day window around the cutoff date of birth and p denotes the p-value of the null hypothesis of no difference in the outcome variable across the two groups, calculated using worker-level cluster-robust inference. The complete set of plots for all years can be found in appendix figure A.36.

Figure 1.11: Effect of the reform on total income in old age (IRS data)

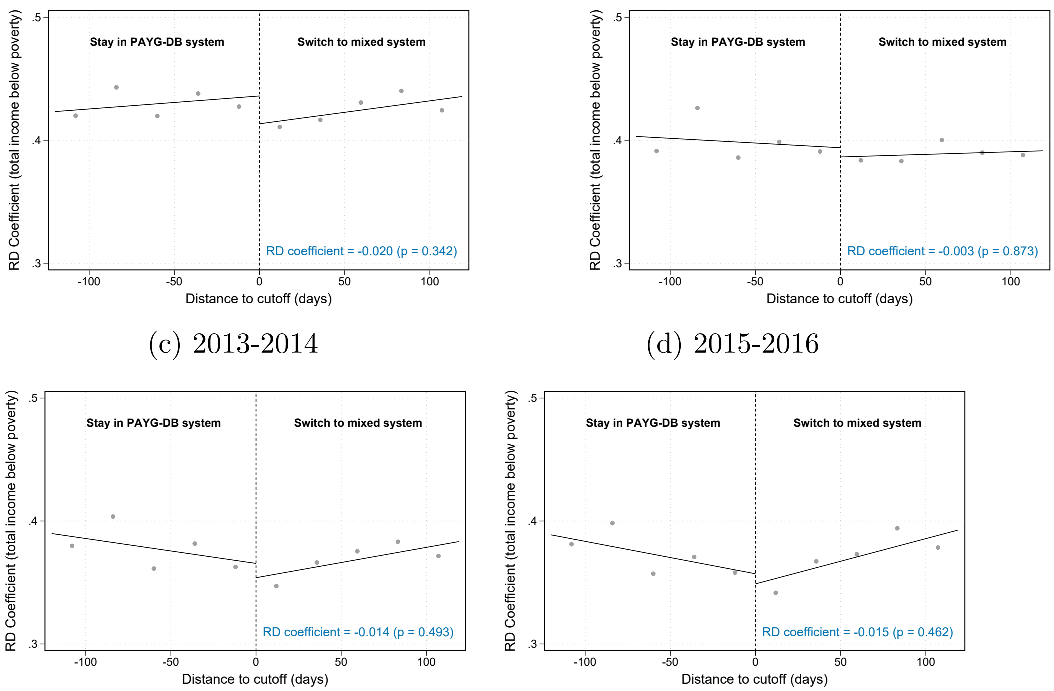


Notes: This figure shows the average total income of individuals by equal-sized bins of distance to the cutoff date of birth, using IRS data. Individuals born before the cutoff were left by default in the pay-as-you-go system with defined benefits and individuals born at the cutoff or after were switched to the mixed system with retirement accounts. Each panel corresponds to a different group of years. In all panels, the dependent variable is the sum of any pension income and any labor earnings, including zeroes, measured in thousand of 2009 Uruguayan pesos. The dependent variable is residualized from year fixed effects and evaluated at the mean. RD coefficients are estimated using a 22-day window around the cutoff date of birth and p denotes the p-value of the null hypothesis of no difference in the outcome variable across the two groups, calculated using worker-level cluster-robust inference. The complete set of plots for all years can be found in appendix figure A.37.

Figure 1.12: Effect of the reform on total income below poverty line (IRS data)

(a) 2009-2010 (b) 2011-2012

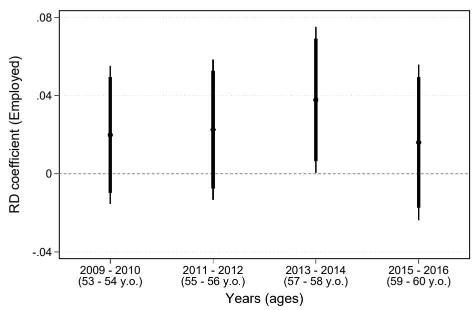
(c) 2013-2014 (d) 2015-2016



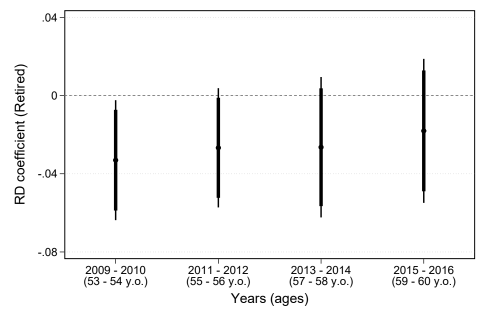
Notes: This figure shows the share of individuals whose total income is below the national poverty line of Montevideo from equation by equal-sized bins of distance to the cutoff date of birth, using IRS data. Individuals born before the cutoff were left by default in the pay-as-you-go system with defined benefits and individuals born at the cutoff or after were switched to the mixed system with retirement accounts. Each panel corresponds to a different group of years. In all panels, the dependent variable is an indicator equal to 1 if the total income is below the poverty line. The dependent variable is residualized from year fixed effects and evaluated at the mean. RD coefficients are estimated using a 22-day window around the cutoff date of birth and p denotes the p-value of the null hypothesis of no difference in the outcome variable across the two groups, calculated using worker-level cluster-robust inference. The complete set of plots for all years can be found in appendix figure A.38.

Figure 1.13: Time series plot of RD coefficients (IRS data)

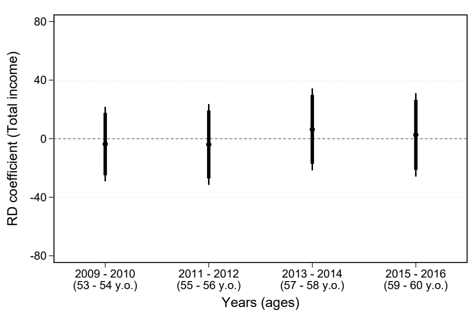
(a) Employed



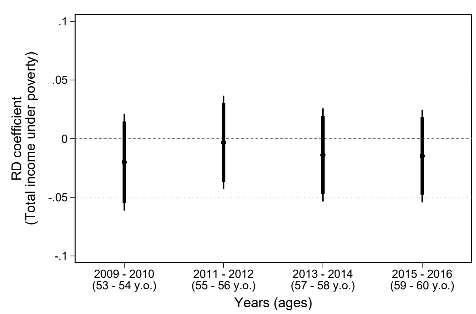
(b) Retired



(c) Total income



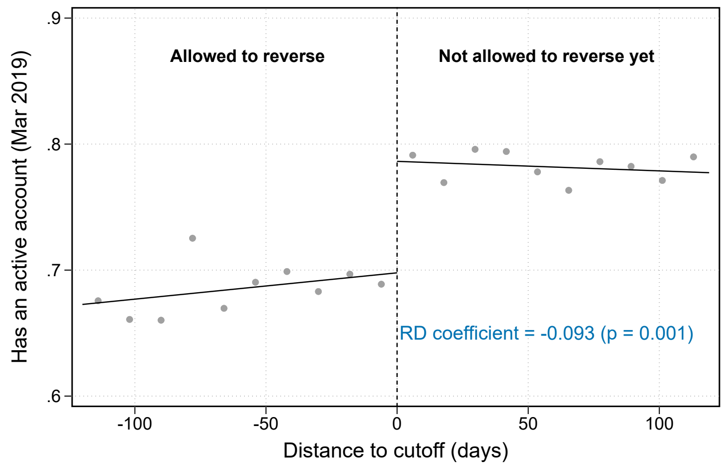
(d) Below poverty line



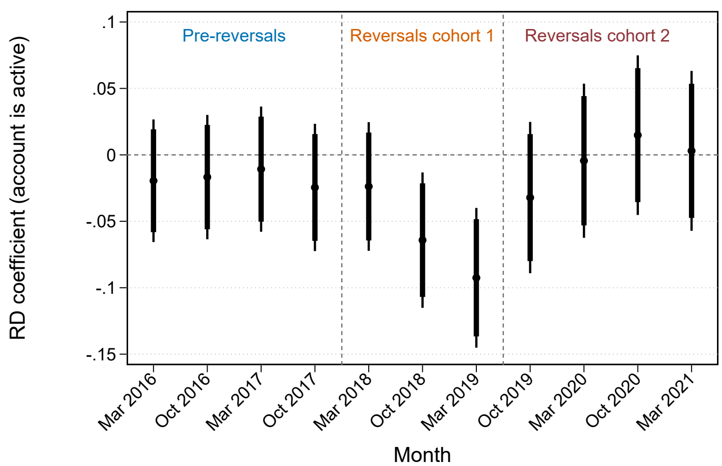
Notes: This figure shows several time series plot for the RD coefficients for each group of years, using IRS data. Panel (a) shows coefficients for the effect on the probability of being employed. Panel (b) shows coefficients for the effect on the probability of being retired. Panel (c) shows coefficients for the effect on total income, measured as pension income plus labor earnings. Panel (d) shows coefficients for the effect on the probability of the total income being below the poverty line. The numbers underneath the years indicate the ages of workers in those years. Thick vertical bars correspond to 90% confidence intervals and thin vertical bars correspond to 95% confidence intervals, both from worker-level cluster-robust inference.

Figure 1.14: RD plot and coefficient (account is active - retirement accounts data)

(a) Main RD plot (Active by Mar 2019)



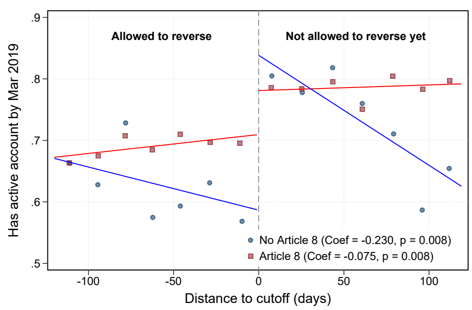
(b) Time series plot of RD coefficients



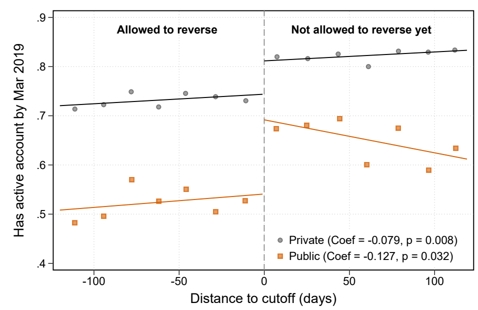
Notes: This figure shows the RD plot and a time series plot of RD coefficients for whether the account was active in a given period, using retirement accounts data. Panel (a) plots the share of workers whose retirement account is active by March of 2019 by equal-sized bins of distance to the cutoff date of birth. Individuals born before the cutoff were allowed to reverse to the pay-as-you-go system with defined benefits and individuals born at the cutoff or after were not allowed to reverse yet. Panel (b) shows a time series plot for the RD coefficients for an indicator equal to 1 if the retirement account was active in a given month. The time before the first dashed line corresponds to the months prior to the reversal policy being implemented. The area between the dashed lines corresponds to the period in which the first cohort was allowed to reverse. The area after the second dashed line indicates the period during which the second cohort was allowed to reverse. RD coefficients are estimated using an 11-day window around the cutoff date of birth and p denotes the p-value of the null hypothesis of no difference in the outcome variable across the two groups, calculated using worker-level robust inference. Thick vertical bars correspond to 90% confidence intervals and thin vertical bars correspond to 95% confidence intervals.

Figure 1.15: RD Heterogeneity plots - Active retirement account by March 2019

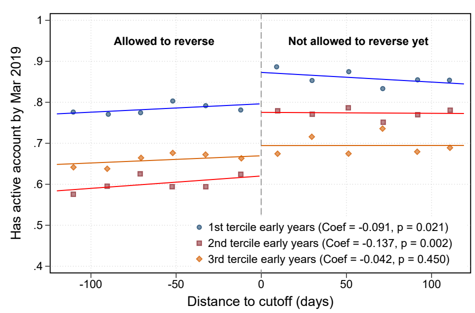
(a) By Article 8 choice



(b) By public versus private sector

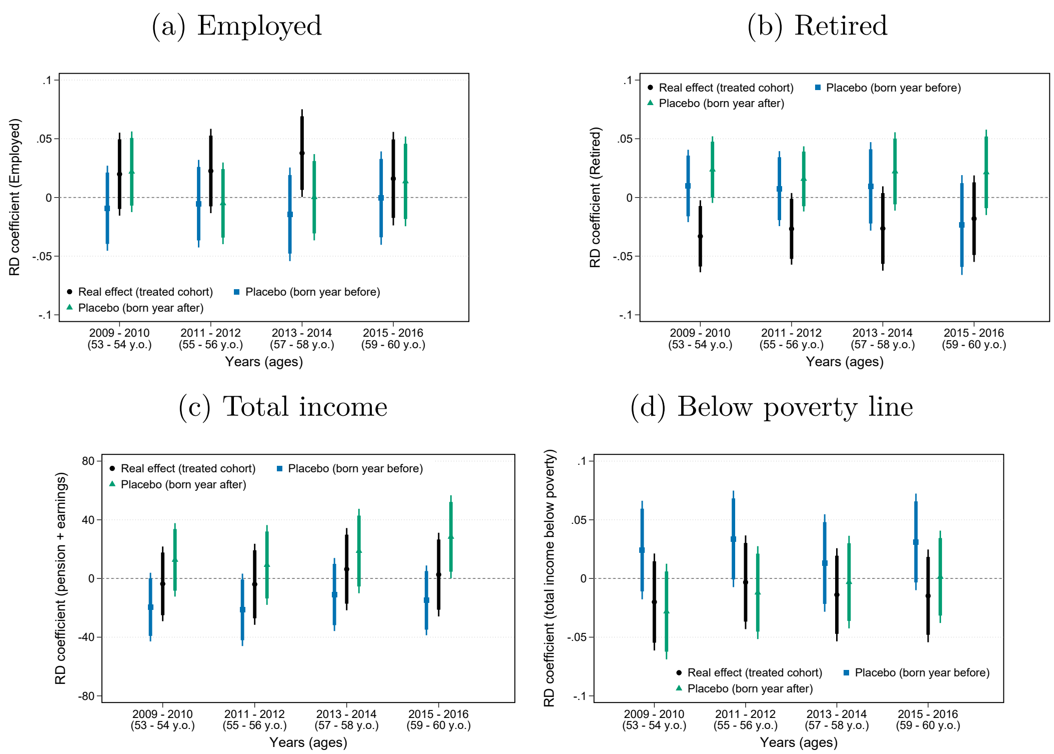


(c) By degree of contributions during early high-returns years



Notes: This figure shows heterogeneity plots of the share of workers whose retirement account is active by March of 2019 by equal-sized bins of distance to the cutoff date of birth, using retirement accounts data. Individuals born before the cutoff were allowed to reverse to the pay-as-you-go system with defined benefits and individuals born at the cutoff or after were not allowed to reverse yet. RD coefficients are estimated using an 11-day window around the cutoff date of birth and p denotes the p-value of the null hypothesis of no difference in the outcome variable across the two groups, calculated using worker-level robust inference. Panel (a) shows the heterogeneity by whether te worker chose the Article 8 option. Panel (b) shows heterogeneity by public-sector and private-sector workers. Panel (c) shows heterogeneity by terciles of contributions made during the early years of high interest rates (until December of 2004).

Figure 1.16: Time series plot of RD coefficients - comparison with placebos (IRS data)



Notes: This figure shows several time series plot for the RD coefficients for each group of years, using IRS data. Panel (a) shows coefficients for the effect on the probability of being employed. Panel (b) shows coefficients for the effect on the probability of being retired. Panel (c) shows coefficients for the effect on total income, measured as pension income plus labor earnings. Panel (d) shows coefficients for the effect on the probability of the total income being below the poverty line. The numbers underneath the years indicate the ages of workers in those years. Thick vertical bars correspond to 90% confidence intervals and thin vertical bars correspond to 95% confidence intervals, both from worker-level cluster-robust inference. Black corresponds to estimates for the cohort affected by the reform. Green corresponds to estimates for the cohort born on the year after the cohort affected by the reform. Blue corresponds to estimates for the cohort born on the year before the cohort affected by the reform.

Table 1.1: Summary statistics

	Observations	Mean	Standard Deviation	Median
Panel A. Social Security data				
Employed	1552882	0.579	0.494	1.000
Total labor earnings	929,373	14858.658	19262.985	8728.500
Monthly hours worked	902,771	163.922	54.419	171.429
Days worked in the month	929,153	24.642	8.985	30.000
Public sector	893,059	0.288	0.453	0.000
Owner	922,403	0.121	0.327	0.000
High inf. sector	840,859	0.387	0.487	0.000
Panel B. Census data				
Employed	109,583	0.676	0.468	1.000
Retired	109,583	0.162	0.368	0.000
Disability	109,575	0.057	0.231	0.000
SES Index	109,354	0.007	1.001	0.108
Married	109,584	0.674	0.469	1.000
College complete	109,828	0.224	0.417	0.000
Female	109,828	0.524	0.499	1.000
Has children	109,828	0.468	0.499	0.000
Panel C. Income tax data				
Employed	408,544	.691	.462	1
Retired	408,544	.211	.408	0
Total labor earnings	408,544	302116	588324	136928
Pension income	408,544	31537	99020	0
Income under poverty line	408,544	.387	.487	0
Panel D. Retirement accounts data				
Active March 2019	20,013	.756	.429	1
Female	20,013	.536	.499	1
Foreign born	20,013	.0616	.24	0
Article 8	20,013	.913	.283	1
Year of adoption of Article 8	18,262	1998	4.46	1996
Public sector	19,461	.262	.44	0

Notes: This table reports summary statistics from our main datasets. Panels A, B, and C correspond to workers born between 1955 and 1957. Panel D corresponds to workers born in 1960. Panel A shows summary statistics from Social Security data. Panel B shows summary statistics from census data. Panel C reports summary statistics from income tax data. Panel D reports summary statistics from the retirement accounts.

Table 1.2: Effect on employment and retirement heterogeneity - Census Data

	=1 if employed				=1 if retired			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Mixed system	0.0216*	0.0424**	0.0134	0.0301*	-0.0204**	-0.0351**	-0.0118	-0.0235*
	(0.0124)	(0.0178)	(0.0127)	(0.0181)	(0.00970)	(0.0141)	(0.00972)	(0.0142)
High SES		0.138***		0.118***		-0.0598***		-0.0449***
		(0.0173)		(0.0172)		(0.0138)		(0.0137)
Mixed system × High SES		-0.0448*		-0.0326		0.0338*		0.0235
		(0.0248)		(0.0247)		(0.0194)		(0.0193)
Disability			-0.383***	-0.356***			0.282***	0.272***
			(0.0364)	(0.0368)			(0.0394)	(0.0399)
Mixed system × Disability			0.100*	0.0860			-0.109*	-0.103*
			(0.0565)	(0.0568)			(0.0566)	(0.0572)
Constant	0.649***	0.583***	0.670***	0.611***	0.174***	0.202***	0.157***	0.180***
	(0.00872)	(0.0125)	(0.00887)	(0.0128)	(0.00692)	(0.0102)	(0.00687)	(0.0102)
Observations	5799	5743	5749	5742	5799	5743	5749	5742

Notes: This table reports estimates of the RD coefficient of the effect on the probability of being employed and of being retired using census data. In columns 1 through 4 the dependent variable is an indicator equal to 1 if the individual is employed. In columns 5 through 9 the dependent variable is an indicator equal to 1 if the individual is retired. Mixed system is a dummy variable equal to 1 if the worker was born after the cutoff (in April) and zero otherwise (in March). High SES is an indicator equal to 1 if the individual has an above median socioeconomic status index. Disability is an indicator equal to 1 if the individual reports having some mild disability. Robust standard errors are shown in parentheses. * Significant at the 10% level ** Significant at the 5% level *** Significant at the 1% level.

Table 1.3: Effect on earnings and heterogeneity

	Total labor earnings (log)				
	(1) 1997 to 2000	(2) 2001 to 2004	(3) 2005 to 2008	(4) 2009 to 2011	(5) 2012 and 2013
Panel A. Overall effect					
Mixed system	0.175**	0.235**	0.148*	0.108	-0.0662
	(0.0819)	(0.0971)	(0.0857)	(0.0837)	(0.0870)
Number of workers	1056	902	985	952	867
Panel B. Heterogeneity including public sector					
Mixed system	0.175**	0.234**	0.149*	0.107	-0.0662
	(0.0819)	(0.0971)	(0.0857)	(0.0837)	(0.0870)
Public sector	0.794***	1.035***	0.878***	0.799***	0.732***
	(0.0818)	(0.0863)	(0.0763)	(0.0728)	(0.0805)
Mixed system × Public sector	-0.177	-0.294**	-0.222**	-0.178*	0.0132
	(0.112)	(0.121)	(0.108)	(0.108)	(0.117)
Number of workers	1294	1168	1243	1258	1167
Panel C. Heterogeneity by sector-level informality					
Mixed system	0.00135	0.0549	-0.0372	-0.0793	-0.316**
	(0.0767)	(0.0993)	(0.0993)	(0.118)	(0.124)
High inf. sector	-0.740***	-0.796***	-0.619***	-0.513***	-0.516***
	(0.102)	(0.119)	(0.107)	(0.102)	(0.105)
Mixed system × High inf. sector	0.258*	0.247	0.280*	0.270*	0.373**
	(0.142)	(0.168)	(0.155)	(0.160)	(0.171)
Number of workers	988	844	931	892	816
Panel D. Heterogeneity by ownership					
Mixed system	0.0724	0.155*	0.0558	0.0620	-0.133
	(0.0679)	(0.0810)	(0.0707)	(0.0752)	(0.0816)
Owner	-1.261***	-1.285***	-1.242***	-1.041***	-0.898***
	(0.258)	(0.258)	(0.223)	(0.207)	(0.228)
Mixed system × Owner	0.595*	0.309	0.295	0.142	0.375
	(0.341)	(0.375)	(0.346)	(0.310)	(0.328)
Number of workers	1056	902	985	952	867

Notes: This table reports estimates of the RD coefficient and heterogeneity for the effect on reported labor earnings using social security data. The RD coefficient is estimated using a window of 11 days around the cutoff date of birth. In all columns the dependent variable is the log of the total labor earnings reported. Column 1 corresponds to estimates calculated using the years 1997 to 2000. Column 2 corresponds to estimates calculated using the years 2001 to 2004. Column 3 corresponds to estimates calculated using the years 2005 to 2008. Column 4 corresponds to estimates calculated using the years 2009 to 2011. Column 5 corresponds to estimates calculated using the years 2012 and 2013. Mixed system is a dummy variable equal to 1 if the worker was born after the cutoff date of birth. Public sector is a dummy variable equal to 1 if the worker is employed in the public sector. High inf. sector is an indicator if the firm's sector of employment corresponds to a high informality sector, as defined in table A.1. Owner is a dummy variable equal to 1 if the worker is listed as some type of owner of the firm (includes self-employed workers as firms). Panel B includes public sector workers, all other panels include only private sector workers. All specifications include year fixed effects. Standard errors clustered at the worker level are shown in parentheses. * Significant at the 10% level ** Significant at the 5% level *** Significant at the 1% level.

Table 1.4: Effect on employment, days worked, and hours worked

	=1 if employed				
	(1) 1997 to 2000	(2) 2001 to 2004	(3) 2005 to 2008	(4) 2009 to 2011	(5) 2012 and 2013
Panel A. Effect on probability of being employed					
Mixed system	-0.00486	-0.0109	0.00179	0.00726	0.0446**
	(0.0203)	(0.0209)	(0.0207)	(0.0208)	(0.0215)
Number of workers	1804	1804	1804	1804	1804
	Days worked				
Panel B. Effect on days worked in the month					
Mixed system	-0.334	-0.000872	-0.283	-0.0919	-0.362
	(0.432)	(0.544)	(0.547)	(0.546)	(0.580)
Number of workers	1056	902	985	952	867
	Monthly hours worked (log)				
Panel C. Effect on weekly hours worked					
Mixed system	-0.0173	-0.0100	-0.0655*	-0.0340	-0.0412
	(0.0316)	(0.0401)	(0.0396)	(0.0443)	(0.0470)
Number of workers	1048	888	977	943	854

Notes: This table reports estimates of the RD coefficient for three measures of labor supply using social security data. The RD coefficient is estimated using a window of 11 days around the cutoff date of birth. In Panel A the dependent variable is a dummy variable equal to 1 if the worker is employed. In Panel B the dependent variable is the number of days worked per month. In Panel C the dependent variable is the natural logarithm of hours worked. Column 1 corresponds to estimates calculated using the years 1997 to 2000. Column 2 corresponds to estimates calculated using the years 2001 to 2004. Column 3 corresponds to estimates calculated using the years 2005 to 2008. Column 4 corresponds to estimates calculated using the years 2009 to 2011. Column 5 corresponds to estimates calculated using the years 2012 and 2013. Mixed system is a dummy variable equal to 1 if the worker was born after the cutoff date of birth. All specifications include year fixed effects. Standard errors clustered at the worker level are shown in parentheses. * Significant at the 10% level ** Significant at the 5% level *** Significant at the 1% level.

Table 1.5: Balance - Census data

Variable	(1) Unfunded DB system	(2) Mixed system	(3) Difference
Married	0.692	0.706	0.014
	(0.462)	(0.456)	(0.012)
College complete	0.238	0.238	-0.000
	(0.426)	(0.426)	(0.011)
Has children	0.481	0.474	-0.007
	(0.500)	(0.499)	(0.013)
Disability	0.056	0.049	-0.007
	(0.230)	(0.216)	(0.006)
SES Index	0.012	0.025	0.012
	(1.014)	(1.014)	(0.027)
Female	0.540	0.528	-0.012
	(0.499)	(0.499)	(0.013)
Observations	3,004	2,810	5,814

Notes: This table shows the balance on demographic characteristics across the originally treated and control groups using census data. Married is a dummy variable equal to 1 if the respondent is married. College complete is a dummy variable equal to 1 if the respondent has completed some college education. Has children is a dummy variable equal to 1 if the respondent reports having any children. Disability is an indicator equal to 1 if the individual reported experiencing at least some moderate difficulty related to eyesight, hearing, mobility, or cognitive ability. SES index is the socioeconomic status index (see section A.6 for details). Female is an indicator for equal to 1 if the individual reported being female. * Significant at the 10% level ** Significant at the 5% level *** Significant at the 1% level.

Table 1.6: Main effects - IRS data

	(1) 2009 to 2010	(2) 2011 to 2012	(3) 2013 to 2014	(4) 2015 2016
	= 1 if employed			
Panel A. Effect on employment				
treat2	0.0199	0.0226	0.0378**	0.0160
	(0.0180)	(0.0183)	(0.0190)	(0.0203)
Number of workers	2005	2005	2005	2005
	= 1 if retired			
Panel B. Effect on retirement				
treat2	-0.0331**	-0.0267*	-0.0264	-0.0181
	(0.0156)	(0.0156)	(0.0183)	(0.0188)
Number of workers	2005	2005	2005	2005
	Total income			
Panel C. Effect on total income				
treat2	-3.664	-3.956	6.347	2.645
	(12.98)	(14.08)	(14.29)	(14.54)
Number of workers	2005	2005	2005	2005
	= 1 if total income is below poverty			
Panel D. Effect on total income below poverty				
treat2	-0.0200	-0.00324	-0.0139	-0.0148
	(0.0211)	(0.0204)	(0.0202)	(0.0201)
Number of workers	2005	2005	2005	2005

Notes: This table reports estimates of the RD coefficient for the four main outcomes using IRS data. In Panel A the dependent variable is a dummy variable equal to 1 if the worker is employed. In Panel B the dependent variable is a dummy variable equal to 1 if the worker is retired. In Panel C the dependent variable is the total yearly income (measured as any labor earnings plus any pension income) in 2009 UR$. In Panel D the dependent variable is a dummy variable equal to 1 if the total income is below the national poverty line from Montevideo. Column 1 corresponds to estimates calculated using the years 2009 and 2010. Column 2 corresponds to estimates calculated using the years 2011 and 2012. Column 3 corresponds to estimates calculated using the years 2013 and 2014. Column 4 corresponds to estimates calculated using the years 2015 to 2016. All specifications include year fixed effects. Standard errors clustered at the worker level are shown in parentheses. * Significant at the 10% level ** Significant at the 5% level *** Significant at the 1% level.

Chapter 2

Workplace Litigiousness and Labor Market Outcomes: Evidence from a Workers' Compensation Reform

2.1 Introduction

Addressing the consequences of work-related accidents and illnesses is an important policy challenge. According to the Bureau of Labor Statistics, around 2.8 million workplace injuries and illnesses – including more than 5,000 fatal injuries – were reported in the United States in 2019. Since most of these accidents may result in job absenteeism or other work-related restrictions, they can affect earnings for both workers and employers. Hence, in the absence of insurance or regulation, accidents may lead to workplace conflicts to determine who should pay for their costs, which can result in costly lawsuits between both parties.

Workers' compensation (WC) schemes – mandated insurance programs that pay for health expenses and a wage replacement for injured workers – can help to solve these conflicts by establishing guidelines on how to proceed after workplace accidents. Importantly, reducing work-related litigation costs is an explicit objective of WC schemes (Fishback and Kantor, 1998; Fishback and Kantor, 2007), in part due to the efficiency gains from reduced litigiousness that can positively affect labor market outcomes. If WC schemes reduce the litigation costs of workplace accidents, the rents of labor market matches increase, especially in industries where workplace accidents are commonplace. Larger rents may encourage employers to post more vacancies and attract more applicants, eventually affecting employment. The relative bargaining positions may also induce changes in wages, depending on how the additional rents are split between workers and employers.

To the best of our knowledge, however, there is no evidence of how effective WC schemes are for reducing litigation costs in the workplace. Empirical evidence on the effects of reducing workplace lawsuits on labor market outcomes is also missing. The answers to both questions are important inputs for thinking about the optimal design of WC schemes and, more generally, the effects that litigiousness can have on the performance of the labor market.

To contribute to this discussion, this paper studies a WC reform in Argentina that sought to reduce workplace lawsuits between workers and employers. Argentina established a WC system similar to the United States system in 1996, where employers were mandated to provide no-fault insurance for workers. In exchange, workers waived their right to sue employers and insurance companies. In the mid-2000s, several Supreme Court rulings opened up room for suing employers and insurance companies (Galiani, 2017). As a result, litigiousness escalated, generating large costs for both employers and workers. To address this problem, the system was reformed in February 2017. The new law required injured workers to go through a local government medical commission as a mandatory step before any further action can be taken. This commission determined the degree of disability, whether the injury was related to the worker's occupation, and the corresponding compensation according to the Law. The decision could be appealed to a higher-order commission, and eventually to labor courts, although this possibility was deemed unlikely: the reform tried to appeal to employees by providing quicker compensation and to employers by reducing the large and unpredictable costs from litigiousness.

We leverage the staggered introduction of the law across provinces to estimate the effect of the reform using an event study design. The new system was sanctioned at the federal level in February 2017, but each provincial legislature had to sanction its own law to adhere to the federal law.[1] Upon approval of the law, each provincial government had to set up the medical commissions, which then had to be approved by the federal agency in charge of the WC system. Only after the approval of the medical commissions, the law entered into effect at the province level. Provinces were heterogeneous in how they carried out these steps, leading to a staggered adoption of the policy. We cover the period January 2015 to July 2019, when the law entered into effect in 5 out of 24 provinces.[2] We study the effects of the reform on workplace litigiousness and labor market outcomes using quarterly province- and sector-by-province-level aggregates built from administrative records. For each unit of analysis, we observe the number of accidents, lawsuits, and amounts claimed by workers, in addition to equilibrium outcomes of the formal labor market such as employment counts, average wages, and the number of firms.

We find that the reform was very effective at reducing workplace litigiousness and its associated costs, with no effect on reported accidents. The number of lawsuits fell by about 0.7 log-points after the reform. The costs of litigiousness –measured as the amount of money claimed in lawsuits as a share of the wage bill– dropped by about 0.4 percentage points after the reform. The effect is twice as large in sectors most affected by litigiousness (measured

[1] The exception was the City of Buenos Aires where the law automatically entered into effect in February 2017.

[2] We omit the months after July 2019 because of an unanticipated result in the primary election of August 2019 that led to a stock market crash and a substantial overnight depreciation of the currency. These events, in turn, led to significant changes in economic institutions, such as reinstating capital controls and taxes on agricultural exports. This negative economic shock had a differential effect across provinces and sectors, potentially affecting our identification strategy.

as the sectors with larger shares of employers that had lawsuits before the reform), namely construction, mining, and manufacturing. We find no significant effect on the number of accidents reported, suggesting that the drop in litigiousness was not due to lower accident reporting or higher safety standards in the workplace. These results suggest that the reform increased the efficiency of the labor market by reducing the costs of managing workplace accidents.

We then explore the effects of the reform on the labor market. Province-level employment increased by about 1.8% after the reform, although the effect is not precisely estimated. The number of active firms was not affected by the reform, suggesting that the employment effect was driven by existing firms increasing their employment levels. Average wages were also unaffected by the reform, suggesting that employed workers did not capture the gains of the smaller litigation costs. The employment effects become larger and more precise when zooming at the sector-by-province level: sector-level employment experienced a significant increase of 2.8% one year after the reform. The total effect is almost exclusively driven by the sectors most affected by litigiousness, whose employment level one year after the reform was more than 5% larger. Wage effects continue to be negligible when using province-by-sector-level data.

We end the paper by proposing a simple model of the labor market to rationalize the results. We extend the basic matching model of Pissarides, 1985; Pissarides, 2000 to allow for workplace accidents. The model can rationalize positive employment effects when litigation costs decrease through an increase in posted vacancies. The wage effects are ambiguous since the reduction in the expected costs of litigation increases the rents of labor market matches, eventually pushing wages up, but also induces a compensating differential force that pushes wages down. The relative bargaining power between workers and employers mediates how these two forces balance in equilibrium.

This paper contributes to the literature on WC by providing, to our knowledge, the first analysis of the effects of the policy on workplace litigiousness and aggregate labor market outcomes. The literature has mostly focused on moral hazard questions by estimating worker-level behavioral responses on accidents, claims, or private health expenditures (Krueger, 1990; Dionne and St-Michel, 1991; Meyer, Viscusi, and Durbin, 1995; Kantor and Fishback, 1996; Dillender, 2015; Hansen, Nguyen, and Waddell, 2017; Powell and Seabury, 2018; Huet-Vaughn and Benzarti, 2020; Cabral and Dillender, 2021). Cabral, Cui, and Dworsky, 2021 discuss the role of WC schemes for dealing with other market failures such as adverse selection and market power in private insurance markets and externalities on workers' health. With the exception of the early evidence on wage incidence provided by Fishback and Kantor, 1995, there is no evidence on the labor market effects of WC schemes. We show that WC schemes can significantly reduce labor market litigation, which in turn positively affects aggregate employment. The lack of effects on earnings also makes explicit the distributional impact of the policy. The fact that external government commissions can effectively reduce workplace lawsuits could eventually inform policy-making in other contexts where workplace conflicts could lead to costly litigation as, for example, workplace discrimination (Darity and Mason, 1998; Bohren, Hull, and Imas, 2022; Kline, Rose, and Walters, 2022) or sexual harassment

(Folke and Rickne, 2022).

More generally, the labor market effects of different labor market institutions have been extensively studied. A large literature studies the labor market effects of unemployment insurance policies, both at the individual (Schmieder, Wachter, and Bender, 2016; Nekoei and Weber, 2017; Lindner and Reizer, 2020) and aggregate (Hagedorn, Manovskii, and Mitman, 2017; Marinescu, 2017; Johnston and Mas, 2018; Chodorow-Reich, Coglianese, and Karabarbounis, 2019; Boone et al., 2021) levels. Similar analyses exist regarding health insurance (Gruber, 1994; Baicker and Chandra, 2006; Baicker et al., 2014; Kucko, Rinz, and Solow, 2018; Duggan, Goda, and Jackson, 2019; Fang, Aizawa, et al., 2020; Heim et al., 2021), family policies (Rossin-Slater, Ruhm, and Waldfogel, 2013; Schönberg and Ludsteck, 2014; Givord and Marbot, 2015; Dahl et al., 2016; Olivetti and Petrongolo, 2017; Tamm, 2019), the EITC (Kleven, 2020), the minimum wage (Manning, 2021), and universal basic income policies (Hoynes and Rothstein, 2019). We add to this literature by providing evidence on the labor market effects of WC policies.

This paper also contributes to the literature on compensating differentials that emphasizes the importance of non-wage job amenities for workers' choices and outcomes (Bonhomme and Jolivet, 2009; Mas and Pallais, 2017; Lavetti and Schmutte, 2018a; Lavetti and Schmutte, 2018b; Maestas et al., 2018; Sorkin, 2018; Lavetti, 2020; Taber and Vejlin, 2020; Anelli and Koenig, 2021; Jäger et al., 2021; Le Barbanchon, Rathelot, and Roulet, 2021; Lindenlaub and Postel-Vinay, 2021; Marinescu, Qiu, and Sojourner, 2021; Sockin, 2021; Lamadon, Mogstad, and Setzler, 2022; Roussille and Scuderi, 2022). One particular (dis)amenity that enters the bundle of job characteristics is the likelihood of workplace accidents. The evidence provided in this paper can be thought of as measuring the effect of reducing the cost of this disamenity on labor market outcomes. While the proposed model suggests that employers may use this rationale to push wages down, the increase in labor market rents pushes the wage in the opposite direction, to the extent that workers are able to capture some of these rents. Then, our analysis contributes to the understanding of compensating differential wage effects in contexts where bargaining matters and changes in amenities also affect the value of the job for the employer.

The rest of the paper is structured as follows. Section 2.2 provides an overview of WC schemes and the institutional setting and reform studied in this paper. Section 2.3 describes the data. Section 2.4 describes the empirical strategy and presents the main empirical results. Section 2.5 presents a simple theoretical framework of labor markets with litigiousness and workers' compensation. Finally, Section 2.6 concludes.

2.2 Workers' compensation schemes and institutional setting

Defining WC

WC schemes provide some type of insurance for workers who experience accidents or illnesses related to their job. The insurance usually covers the health expenses related to the treatment and provides wage replacement for the duration of the injury, and in some cases they also provide compensation to the families of workers who have fatal injuries. Also, these systems typically incorporate mechanisms to limit the need to resort to lawsuits (or forbid them altogether) with the intention of avoiding large and unpredictable costs for both workers and employers (Fishback and Kantor, 2007). Some countries, such as many in Western Europe, implement a "social insurance" system, where the benefits are delivered through a government program and funded through payroll taxes. Other countries, like the United States and Argentina, use an "employer liability" system, where employers are mandated to provide no-fault insurance for their employees and workers cannot sue their employers for negligence.

WC in Argentina before the reform

Argentina established its first WC system in 1915. This system was changed multiple times and frequently experienced issues with litigiousness (Galiani, 2017). In 1995, a new law was passed, which established a WC scheme similar to the United States' system. Under this new law, employers were mandated to provide no-fault insurance for injured workers. This was typically purchased from insurance companies, called Work Hazards Insurers (*Aseguradoras de Riesgos del Trabajo*), while a few employers chose to self-insure. On the other hand, workers waived their right to sue employers and insurance companies. The system achieved the goal of limiting litigiousness for about a decade. However, between 2004 and 2007, several Supreme Court rulings gradually allowed workers to sue both employers and insurance companies (Galiani, 2017).[3] This resulted in a massive escalation of the number of lawsuits, imposing a large burden on the WC system by increasing bureaucracy and waiting times, and leading to concerns about excessive and unpredictable costs due to litigiousness. Panel (a) of Figure 2.1 shows the number of newly reported lawsuits for each quarter since the system started reporting in January 2010 until the second quarter of 2017. The number of new quarterly lawsuits more than tripled between 2010 and 2017. Panel (b) of Figure 2.1 shows the share of firms in each sector that had lawsuits during 2016. The incidence of litigiousness was substantial: in the most affected sectors –construction, mining, and manufacturing– almost one in five firms faced at least one lawsuit in 2016.

The reform In February 2017, a reform was introduced (Law 27,348). The new law established a mandatory first step after work-related accidents: injured workers' claims have to be processed by a Jurisdictional Medical Commission that determines the degree of disability, whether the injury is related to the worker's occupation, and the corresponding compensation as determined by the law passed in 1995, before any further legal action can be taken. This decision could be appealed by any party involved to a higher-level commission and, eventually, to labor courts, although few cases end up doing so. The intention behind the reform

[3] In September 2004, the *Astudillo* and *Aquino* rulings established that provincial labor courts (instead of federal courts) were responsible for handling workplace accidents and established that employers could be liable for workplace accidents. The *Llosco* ruling of June of 2007 confirmed employees' possibility of civil action against employers and insurance companies, while still receiving the wage replacement payments from insurance companies.

was to appeal to workers by streamlining the process and ensuring a quick compensation, and to employers and insurance companies by reducing the large and unpredictable costs due to litigiousness. The law was passed at the national level, but provinces were free to adhere to it by sanctioning their own adherence laws at the provincial level. Most provinces adhered in the years that followed. Upon adherence to the law, the provincial government has to set up its medical commissions, which then have to be approved by the Superintendence of Work Hazards. Once this approval takes place, the law enters into effect in that province, which happened in 5 out of 24 provinces during the sample period we cover (January 2015 to July 2019).[4]

2.3 Data

To estimate the effects of the reform, we combine administrative data from two different sources. The first source informs about labor market outcomes, while the second source contains information about the WC system. For the labor market data, we collect administrative records from the Ministry of Employment and Social Security (*Ministerio de Trabajo, Empleo, y Seguridad Social*). These records are constructed from the payroll tax forms that firms have to file monthly to submit their payroll taxes to the Social Security Agency. We have access to quarterly province-level and 1-digit sector-by-province-level aggregates of the number of workers, number of active firms, and average monthly wages.[5]

We combine the labor market data with information from the government agency in charge of the WC system, the Superintendence of Work Hazards (*Superintendencia de Riesgos del Trabajo*). These records are constructed from insurance companies' reports that are submitted each month to the Superintendence of Work Hazards. The Superintendence then constructs comprehensive monthly information on the number and type of accidents reported, the number of lawsuits started, and the amounts claimed in lawsuits in each sector-by-province cell. We have access to quarterly province-level and 1-digit sector-by-province level aggregates of the number of lawsuits, the number of accidents, and the average amount claimed in lawsuits as a share of total labor costs.[6]

Our final dataset consists of a quarterly panel of employment counts, firm counts, average monthly wages, number of lawsuits, number of accidents, and amounts claimed in

[4]The first instance of law adoption is from the City of Buenos Aires in February of 2017. This was followed by Córdoba in September 2017, Mendoza in February 2018, Buenos Aires in October 2018, and Río Negro in December 2018.

[5]Some of the information is produced with quarterly frequency (e.g. employment) and some with monthly frequency (e.g. wages). We construct quarterly values of the monthly variables by computing quarterly averages. Since we don't observe hours, we indistinctly refer to earnings and monthly wages.

[6]The universe of workers in the Superintendence of Work Hazards data is not exactly the same as the one in the Ministry of Employment and Social Security data, since the former also includes public sector workers and autonomous workers who choose to self insure. Since we are interested in the effects on private-sector employment, we conduct the labor market analysis using the Ministry of Employment and Social Security data.

lawsuits as a share of labor costs at the province and sector-by-province-level. The sample period is January 2015 (two years before the first province adopts the law) through July 2019. Summary statistics are shown in Table 2.1. Panel A presents variables aggregated at the province-level and Panel B presents variables aggregated at the sector-by-province-level. There are, on average, 5,767 accidents and 1,051 new lawsuits reported each quarter in each province. However, there is substantial heterogeneity across provinces. On average, the amount claimed in lawsuits represents 0.4% of total labor costs. The degree of heterogeneity increases when zooming at the sector-by-province level, which is consistent with the sector-level heterogeneity documented in Figure 2.1.

2.4 Results

This section presents our main results. We first present event study analyses using the data aggregated at the province level, which are more likely to inform about the aggregate effects of the policy. We then present event-study analyses using the data aggregated at the sector-by-province-level which inform about the sector-level effect of the reform.

Empirical strategy We leverage the staggered introduction of the law across provinces to estimate the effect of the reform using an event study design. For the province-level event studies, we estimate the following equation by Ordinary Least Squares (OLS):

$$Y_{pt} = \alpha_p + \mu_{r(p)t} + \sum_{k \neq -1} \beta_k \cdot 1\{t = e_p + k\} \cdot \text{Treated}_p + \varepsilon_{pt}, \tag{2.1}$$

where Y_{pt} is an outcome of interest in province p at quarter t, α_p is a province fixed effect, $\mu_{r(p)t}$ is a region-by-quarter fixed effect with $r(p)$ the region of province p, Treated_p is a dummy variable equal to 1 if province p is ever treated, $1\{t = e_p + k\}$ is a dummy variable equal to 1 if province p was treated k quarters ago at quarter t with e_p the calendar quarter in which the province is treated, and ε_{pt} is the error term. The coefficients of interest are $\{\beta_k\}$, which measure the differences in trends between treated and untreated provinces within a window of quarters around the adoption of the law. We normalize $\beta_{-1} = 0$ and cluster the standard errors at the province level. We fully saturate the regression including all time and treatment interactions and report the coefficients for a balanced window of 8 quarters prior and 5 quarters after the reform. For the sector-by-province-level analysis, we estimate the same event-study equation, but include sector-by-province fixed effects (instead of province-level fixed effects).

We also estimate difference-in-differences regressions that summarize the post-reform effect:

$$Y_{pt} = \alpha_p + \mu_{r(p)t} + \beta \cdot \text{Treated}_p \cdot \text{Post}_{pt} + \varepsilon_{pt}, \tag{2.2}$$

where $\text{Post}_{pt} = \mathbb{1}\{t \geq e_p\}$ is a dummy variable that takes value 1 if province p was already treated at quarter t, and all other variables are defined as in equation (2.1). In this regression, β summarizes the aggregate post-reform treatment effect. While we continue to cluster the standard errors at the province level, given the small number of provinces, we also report

Wild Bootstrap p-values (Cameron, Gelbach, and Miller, 2008) for the main coefficient of interest. Noting that the length of the post-period differs by treated province, tables report the average effect on the 5 quarters after the reform.

Province-level results Figure 2.2 plots the $\{\beta_k\}$ coefficients from equation (2.1) with their corresponding confidence intervals. Panel (a) uses the inverse hyperbolic sine transformation of the number of lawsuits reported in a given quarter as a dependent variable.[7] Trends in litigiousness before the adoption of the law are stable but there is a significant negative break in trends after the adoption of the law. Panel (c) shows that the total amount of money claimed in lawsuits as a percentage of total labor costs also falls significantly after the adoption of the law, suggesting that the decrease in lawsuits generates substantial monetary gains. Panel (b) shows that these results are not due to lower accident reporting or higher safety standards in the workplace: while there seems to exist a mild negative trend, there is no significant drop in reported accidents after the implementation of the law.

Regarding the labor market effects, Panel (d) reports coefficients on the inverse hyperbolic sine of the total number of workers at the province level as dependent variable, and shows a positive albeit imprecise increase in the total number of workers.[8] Panels (e) and (f) show that there is no effect on the total number of active firms and on the average monthly wage at the province level.

Panel A of Table 2.2 presents the difference-in-differences estimates of the β coefficient from equation (2.2) of the effect of the law adoption, with the corresponding clustered standard errors and the Wild Bootstrap p-value. Results indicate a substantial decrease in the number of lawsuits (0.77 log points) and amounts claimed in lawsuits (0.4 percentage points of the wage bill), with a noisy increase in employment of 1.8%. Table 2.2 also corroborates the small and non-statistically significant effects on accidents, average monthly wages, and the number of active firms.

Sector-by-province-level results The province-level results inform about the aggregate effects of the policy. We complement these results with sector-by-province level regressions to both increase the statistical power and estimate the sector-level impact of the reform. Figure B.3 of Appendix B.2 plots the $\{\beta_k\}$ coefficients from equation (2.1) with their corresponding confidence intervals for the lawsuits and accidents outcomes. Results essentially mirror the province-level results. This is confirmed in Panel B of Table 2.2: when zooming at the sector-by-province level, results also indicate a drop in lawsuits after the implementation of the reform with no corresponding change in reported accidents. Figure 2.3 plots the $\{\beta_k\}$ coefficients from equation (2.1) with their corresponding confidence intervals for the labor market outcomes. The employment effect is larger and more precisely estimated when using the sector-by-province data. Panel B of Table 2.2 shows that the estimated employment

[7]We use the inverse hyperbolic sine transformation because for one quarter there were zero reported lawsuits, but we get equivalent results when using the natural logarithm.

[8]We use the inverse hyperbolic sine even though there are no instances of zero reported workers to stay consistent with the sector-by-province-level analysis, in which there are occasional instances of zero reported workers for some sector-province pairs. Results are the same when using the natural logarithm of the number of workers.

effect is 2.8% and is significant at the 5% level. Again, the effects on average monthly wages and the number of firms are negligible.

Sector-level heterogeneity To further understand the sector-level effect of the reform, we classify sectors based on the degree of litigiousness they experienced in 2016, defined as the share of employers that had lawsuits during the year (see Panel (b) of Figure 2.1). We classify construction, mining, and manufacturing as sectors with "high litigiousness" and estimate separate event studies for this group and the residual sectors. Panel (b) of Figure 2.2 shows the results for the employment count, indicating that the increase in employment is driven by the high-litigiousness sectors. Panel C of Table 2.1 shows that the reform increased employment by about 5% in these sectors. Panel (d) shows that high-litigiousness sectors experienced a modest wage increase, although the estimated effect is small, noisy, and partially confounded by differential trends. Finally, these results confirm the null effect on the number of firms.[9]

Robustness checks Staggered event studies estimated using two-way fixed effects models may be biased when treatment effects are heterogeneous (Chaisemartin and D'Haultfœuille, 2022; Roth et al., 2022). This potential bias comes from "forbidden comparisons" between treated units, that is, when already treated units integrate the control group of units treated in later periods. In these cases, the estimated treatment effect may not be a convex combination of the heterogeneous treatment effects since the forbidden comparisons may induce negative weights. We perform two exercises that suggest that this source of bias is negligible in our setting. First, we implement the decomposition suggested by Goodman-Bacon, 2021 that shows the relative importance that different pairwise comparisons play when computing the aggregate estimate. As shown in Appendix B.1, all regressions are almost exclusively estimated using comparisons between treated and never treated units. This is not surprising given the small number of treated provinces relative to the never treated ones. These results suggest that the scope for negative weighting is negligible. To further address this concern, we estimate stacked event study specifications (Cengiz et al., 2019; Cengiz et al., 2022; Gardner, 2021; Baker, Larcker, and Wang, 2022) where we force the event-specific control groups to be exclusively composed of never-treated provinces. As we show in Appendix B.4, results remain virtually unchanged under this alternative specification.

Another concern is the small number of treated provinces, given that the law entered into effect in only five provinces in the period considered. This could be a concern if the estimated difference-in-differences effects capture some differential trend for some treated provinces and not the inherent effect of the reform. Alternatively, the main results could be driven by specific provinces which could compromise the external validity of the result. To assess whether this concern bears some relevance for our results, we replicate our main results with several "leave-one-out" estimations in which we sequentially drop one of the treated provinces and compare these results to our baseline estimates using all of the provinces. The results

[9]Panels (b), (d), and (f) of Figure B.3 of Appendix B.2, and Panel C of Table 2.2 show the heterogeneities for the lawsuits and accidents outcomes. The main difference between sectors relates to the amount claimed as a share of labor costs, which is twice as large for the more exposed sectors.

from these exercises can be found in Appendix B.3. All of our results remain very similar to our baseline estimates in all of the leave-one-out estimations, suggesting that results are not driven by some differential trend of a particular treated province.

2.5 Model

To rationalize the estimated employment and wage effects after a decrease in litigation costs, this section extends the standard Pissarides, 1985; Pissarides, 2000 matching model to incorporate workplace accidents. In the model, reduced litigation costs generate employment increases. Wage effects are ambiguous, with the relative bargaining power determining the balance of two competing forces: compensating differentials and larger labor market rents.

Preliminaries Labor supply L is exogenous. Let u be the unemployment rate and v the vacancies per worker rate, both endogenous. The number of matches is given by the matching function $M = M(uL, vL)$, which is assumed to be increasing and concave and to have constant returns to scale. Define labor market tightness as $\theta = v/u$. Constant returns to scale in M implies that the job filling rate, $M(uL, vL)/vL$, is given by $q(\theta)$, with $q_\theta := \partial q(\theta)/\partial\theta < 0$. Likewise, the job finding rate, $M(uL, vL)/uL$, is given by $p(\theta) = \theta q(\theta)$, with $p_\theta := \partial p(\theta)/\partial\theta > 0$. The exogenous job destruction rate is given by δ. The unemployment law of motion is given by $\dot{u} = \delta(1 - u) - \theta q(\theta)u$. In steady state, $\dot{u} = 0$, which implies that

$$u = \frac{\delta}{\delta + \theta q(\theta)}. \tag{2.3}$$

(2.3) is called the Beveridge curve, and establishes an equilibrium relationship between u and θ.

Value functions Firms are atomistic and decide whether to post a vacancy at cost c. If the vacancy is filled, it produces ϕ and pays wage w. Filled vacancies have a probability a of having a workplace accident. When occurring, accidents induce a cost for the firm, k_F. Let V and J be the value for the firm of a vacant job and a filled vacancy, respectively. Then, if r is the discount rate, the value functions can be written as

$$rV = -c + q(\theta)(J - V), \tag{2.4}$$
$$rJ = \phi - w - ak_F + \delta(V - J). \tag{2.5}$$

Free entry implies $V = 0$, so (2.4) is reduced to $J = c/q(\theta)$. Replacing in (2.5) yields

$$\phi - w - ak_F = \frac{(r + \delta)c}{q(\theta)}, \tag{2.6}$$

which is called the job-creation curve.

Define by b the workers' reservation value and by k_W the cost of a workplace accident for the worker. Let U and W be the value for the worker of being unemployed and employed, respectively. Then

$$rU = b + \theta q(\theta)(W - U), \tag{2.7}$$

$$rW = w - ak_W + \delta(U - W), \tag{2.8}$$

We assume that $k_F + k_W > 0$, that is, the process of a workplace injury is not a zero-sum game where employers just compensate workers. The potential presence of, for example, lawsuits implies that there is a deadweight loss associated with accidents.

Wage setting There is Nash bargaining over the total match surplus, with β the workers' bargaining power, so $w =_w (W - U)^{\beta}(J - V)^{1-\beta}$. Solving the problem yields

$$w = (1-\beta)(b + ak_W) + \beta(\phi + c\theta - ak_F). \tag{2.9}$$

Note that (2.9) coincides with the standard solution of the basic DMP model when $k_F = k_W = 0$. The fact that, in partial equilibrium, w depends positively on k_W, suggests that compensating differentials play a role in wage determination.

Equilibrium We interpret a reform that reduces workplace litigation costs as a reduction in k_F and, possibly, k_W. To explore the equilibrium effects of such a reform, we replace (2.9) in (2.6) and differentiate, which yields

$$\frac{d\theta}{dk_F} = \frac{q(\theta)(1-\beta)a\left(\frac{dk_W}{dk_F} + 1\right)}{q_\theta(\phi - w - ak_F) - q(\theta)\beta c}, \tag{2.10}$$

which is unambiguously negative provided that $J \geq 0$ and $dk_W/dk_F \geq 0$. The former is a standard assumption that implies that there is value for employers to create vacancies, and the latter implies that the hypothetical reform that lowers the costs of accidents for employers do so for workers as well. That assumption holds in the reform we study since the reduction in lawsuits implies lower costs for both workers and employers. Equation (2.10) implies that higher (lower) costs for firms of workplace injuries decrease (increase) the vacancies to applicants ratio. Together with equation (2.3), this implies that higher (lower) costs of workplace injuries induce higher (lower) equilibrium unemployment rates. Then, this simple model rationalizes how a reform that reduces k_W and k_F can induce positive employment effects.

A number of things are worth discussing about equation (2.10). First, the magnitude of $d\theta/dk_F$ depends positively on a: the employment effect is larger when workplace accidents are more likely. This is consistent with the heterogeneous results presented in Section 2.4. Second, the magnitude of $d\theta/dk_F$ depends negatively on β: the employment effect is larger when workers' bargaining power is low. This is due to the fact that when β is large, employers anticipate that workers capture a large share of the increase in rents. Therefore, the incentives for creating more vacancies are attenuated. Third, the magnitude of $d\theta/dk_F$ depends positively on dk_W/dk_F, that is, the employment effect is larger when workers' costs are also reduced with the reform. This comes from the fact that the value workers put on the reform induces a compensating differential force that employers can use to push wages down and, therefore, capture more rents from the labor market matches, thus increasing the incentives of posting more vacancies.

Using the fact that $p(\theta) = \theta q(\theta)$, we can replace (2.6) in (2.9) and then differentiate to explore the equilibrium change in w. This yields the following expression

$$\frac{dw}{dk_F} = \frac{\left[(r+\delta)(1-\beta)a\frac{dk_W}{dk_F} + \beta p_\theta \frac{d\theta}{dk_F}(p - w - ak_F) - \beta(r+\delta+p(\theta))a\right]}{r+\delta+\beta p(\theta)}. \quad (2.11)$$

The sign of the expression is ambiguous. The first term in the numerator is positive and reflects the compensating differential force that pushes wages downward when k_F decreases. The second and third terms are negative, implying that they push the wage upwards when k_F decreases. The second term measures the increase in rents in the labor market given by the change in θ because of the larger amount of vacancies, and the third term measures the direct benefits on employers given by the reduction of k_F. The parameter that mediates the sign of the wage effect is β. When β is small, workers are unlikely to capture the additional rents, thus the compensating differential force dominates pushing wages downwards. As β increases, workers gradually capture additional rents, making the wage effect eventually positive. As in the employment analysis, the magnitude of the effect is proportional to a.

While simple, this model helps to rationalize why a reduction in litigiousness may have a positive employment effect with no change in average wages.

2.6 Conclusion

WC schemes may be beneficial to workers and employers if they streamline the process of compensation for workplace accidents and limit the need to resort to costly and inefficient litigation. This paper shows that a reform in Argentina that imposed a government medical intermediary to mediate between parties was successful at reducing lawsuits, implying a substantial reduction in litigation costs. We find that this efficiency gain had effects on the labor market equilibrium: the reform increased aggregate employment with no aggregate effect on the number of active firms or average monthly wages. In the most affected sectors –construction, mining, and manufacturing– the employment effect is especially pronounced.

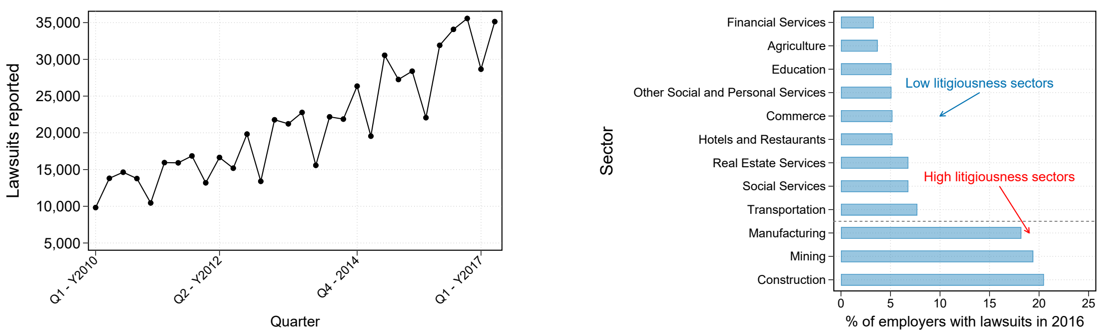
Our results suggest that the efficiency-enhancing potential of WC schemes depends on their ability to limit litigation and costly lawsuits. WC policies, however, should not be uniquely analyzed from this angle since they also affect job quality (ILO, 2017) and may have distributional effects. Our analysis shows that the positive employment effects are not tied to significant changes in wages, suggesting that employers are capturing the incremental job surplus derived from the decrease in litigation. The heterogeneous effects by economic sector also suggest that the benefits of the policy are not evenly distributed in the labor market.

More research is needed to have a more comprehensive picture of the winners and losers of the policy. Private insurance companies are also likely to be affected by the implementation of government medical intermediaries. Knowing if the profits of firms and insurers were affected by the reform would shed light on the conjectured redistributive consequences of WC policies. Other policy tools, such as income and corporate taxes or sector specific-minimum wages, could help to balance asymmetric rent-sharing when efficiency gains are not translated to higher wages.

2.7 Figures

Figure 2.1: Workplace litigiousness before the reform

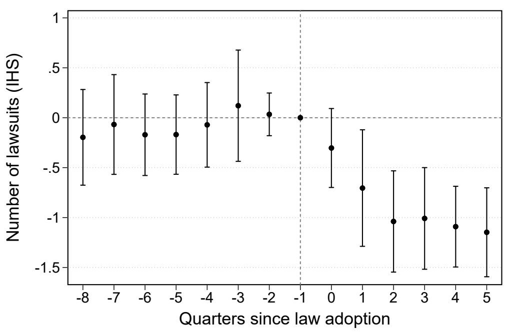
(a) New lawsuits reported by quarter (b) Number of accidents (IHS)



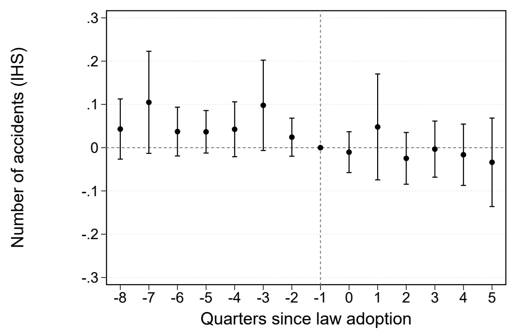
Notes: Panel (a) shows the total number of new lawsuits reported in the country in each quarter, from the first period in which the system for reporting lawsuits entered into effect (January of 2010) to the quarter in which the reform we study was sanctioned at the Federal level (February of 2017). Panel (b) shows the share of employers in each sector that had lawsuits during the 2016. Manufacturing, Mining, and Construction are indicated as sectors highly affected by litigiousness.

Figure 2.2: Province-level results

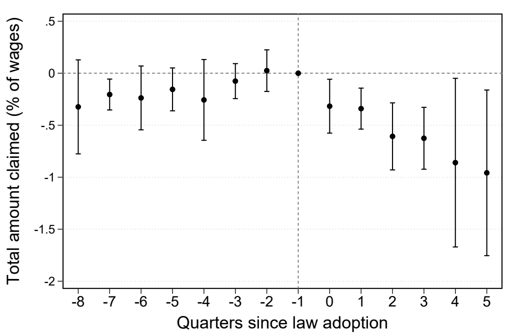
(a) Number of lawsuits (IHS)



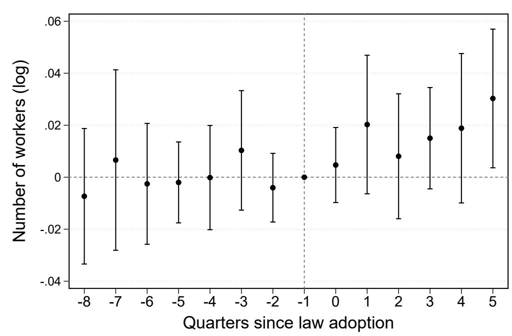
(b) Number of accidents (IHS)



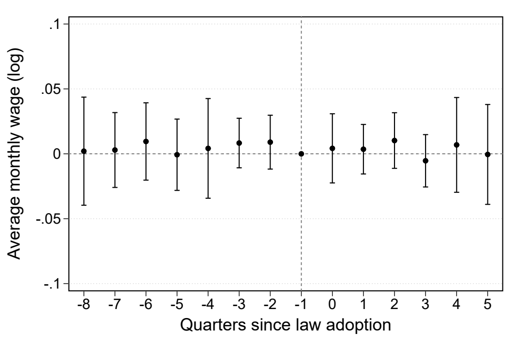
(c) Amount claimed (% of labor costs)



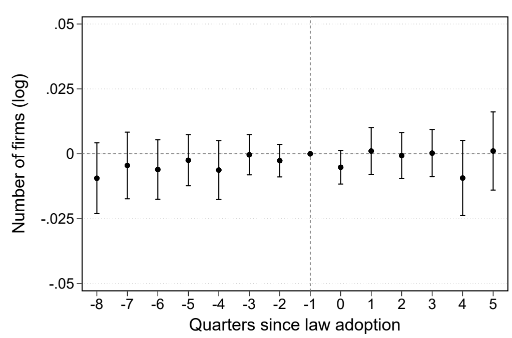
(d) Employment (IHS)



(e) Average monthly wage (log)



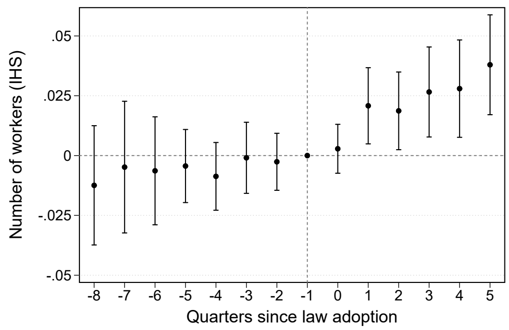
(f) Number of firms (log)



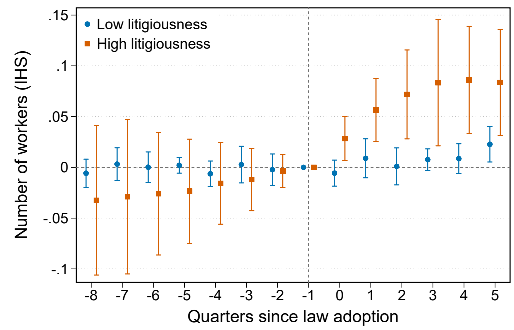
Notes: This figure plots the β_k coefficients from equation (2.1) using different dependent variables. The unit of observation is a province-by-quarter. Standard errors are clustered at the province level. Vertical bars represent 95% confidence intervals. The dependent variable in Panel (a) is the inverse hyperbolic sine transformation of the total number of lawsuits reported. The dependent variable in Panel (b) is the inverse hyperbolic sine of the total number of accidents reported. The dependent variable in Panel (c) is the amount claimed in lawsuits as a share of labor costs (total employment times average monthly wage). The dependent variable in Panel (d) is the natural logarithm of the total number of workers. The dependent variable in Panel (e) is the natural logarithm of the average monthly wage. The dependent variable in Panel (f) is the inverse hyperbolic sine of the total number of firms.

Figure 2.3: Sector-by-province-level results

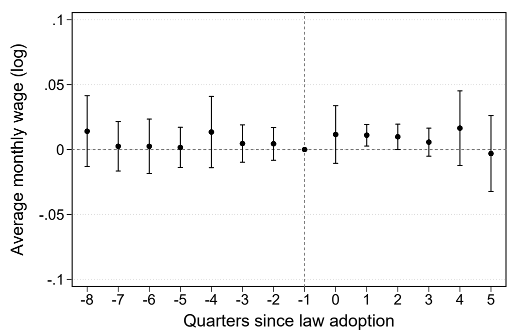
(a) Employment (log)



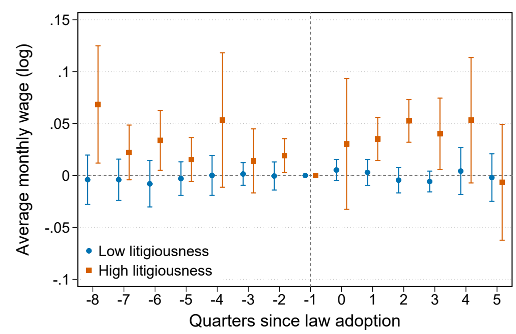
(b) Employment (log) - Heterogeneity



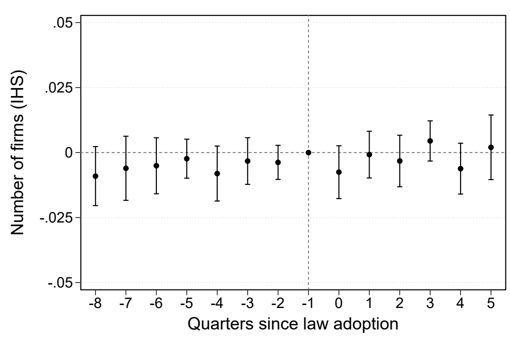
(c) Average monthly wage (log)



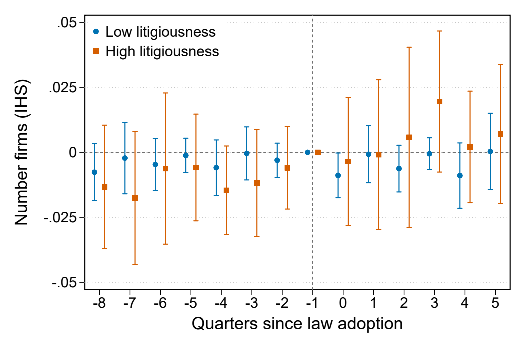
(d) Average monthly wage (log) - Heterogeneity



(e) Number of firms (log)



(f) Number of firms (log) - Heterogeneity



Notes: This figure plots the β_k coefficients from equation (2.1) using different dependent variables. The unit of observation is a province-by-quarter. Standard errors are clustered at the province level. Vertical bars represent 95% confidence intervals. The dependent variable in Panel (a) is the inverse hyperbolic sine transformation of the total number of lawsuits reported. The dependent variable in Panel (b) is the inverse hyperbolic sine of the total number of accidents reported. The dependent variable in Panel (c) is the amount claimed in lawsuits as a share of labor costs (total employment times average monthly wage). The dependent variable in Panel (d) is the natural logarithm of the total number of workers. The dependent variable in Panel (e) is the natural logarithm of the average monthly wage. The dependent variable in Panel (f) is the inverse hyperbolic sine of the total number of firms.

2.8 Tables

Table 2.1: Summary statistics

	Observations	Mean	Standard Deviation	Median
Panel A. Province level				
Number of lawsuits	432	1051	2531	115
Amount claimed in lawsuits (as % of wages)	432	.386	.496	.229
Number of accidents	432	5767	11493	1867
Number of workers	432	271499	500124	90448
Average salary	432	24109	12714	21263
Number of firms	432	23430	42262	7789
Panel B. Sector-by-province level				
Number of lawsuits	5,184	77.8	288	4
Amount claimed in lawsuits (as % of wages)	5,182	.47	.985	.112
Number of accidents	5,184	130	365	29.3
Number of workers	5,184	22362	55879	6085
Average salary	5,158	25066	18404	20076
Number of firms	5,184	1946	5271	421

Notes: Panel A shows summary statistics of variables aggregated at the province-by-quarter level and Panel B shows summary statistics of variables aggregated at the sector-by-province-by-quarter. Number of lawsuits is the total number of lawsuits reported during the quarter. Amount claimed in lawsuits (as % of labor costs) is the total amount claimed in lawsuits as a share of total labor costs in a given quarter. Number of accidents is the total number of accidents reported during the quarter. Number of workers is the average number of workers employed during a quarter. Average monthly wage is the average monthly wage during the quarter. Number of firms is the average number of active firms during a quarter.

Table 2.2: Main results

	(1) Lawsuits	(2) Amount claimed	(3) Accidents	(4) Employment	(5) Average salary	(6) Active firms
Panel A. Province-level results						
Treated	-0.771***	-0.406***	-0.0445	0.0179	0.000267	0.00150
	(0.193)	(0.132)	(0.0305)	(0.0129)	(0.00997)	(0.00592)
Wild bootstrap p	0.0000	0.0110	0.2462	0.1381	0.9820	0.7778
Observations	432	432	432	432	432	432
Number of provinces	24	24	24	24	24	24
Panel B. Sector-by-province analysis: overall effect						
Treated	-0.710***	-0.455***	-0.0412	0.0275**	0.00341	0.00176
	(0.177)	(0.0985)	(0.0453)	(0.0113)	(0.00838)	(0.00538)
Wild bootstrap p	0.0000	0.0000	0.5776	0.0380	0.7357	0.7688
Observations	5184	5182	5184	5184	5158	5184
Number of provinces	24	24	24	24	24	24
Panel C. Sector-by-province analysis: heterogeneity by litigiousness						
Treated × Low Litigiousness	-0.704***	-0.341***	-0.0276	0.0187	-0.000828	0.00113
	(0.176)	(0.0743)	(0.0313)	(0.0118)	(0.00791)	(0.00596)
Treated × High Litigiousness	-0.727***	-0.800***	-0.0819	0.0537**	0.0161	0.00366
	(0.188)	(0.254)	(0.0956)	(0.0196)	(0.0131)	(0.00841)
Wild bootstrap p (low)	0.0000	0.0020	0.5445	0.1962	0.9610	0.8599
Wild bootstrap p (high)	0.0240	0.0511	0.6777	0.0480	0.2993	0.6847
Observations	5184	5182	5184	5184	5158	5184
Number of provinces	24	24	24	24	24	24

Notes: This table reports OLS estimates of difference-in-differences coefficients from equation (2.2). Panel A reports results for regressions at the province level, with all specifications including province fixed effects and region-by-time fixed effects. Panels B and C report results for regressions at the sector-by-province level, with all specifications including sector-by-province fixed effects and region-by-time fixed effects. In column 1 the dependent variable is the inverse hyperbolic sine transformation of the total number of lawsuits reported during the quarter. In column 2 the dependent variable is the total amount of money claimed in lawsuits as a percentage of the total wage bill. In column 3 the dependent variable is the inverse hyperbolic sine of total number of accidents reported during the quarter. In column 4 the dependent variable is the inverse hyperbolic sine of the average number of workers reported during a quarter. In column 5 the dependent variable is the natural logarithm of the average monthly wage. In column 6 the dependent variable is the inverse hyperbolic sine of the total number of firms. Treated is a dummy variable equal to 1 for treated provinces in the 6 quarters after the law entered into effect. High Litigiousness is a dummy variable equal to 1 for high litigiousness sectors as defined in panel b of figure 2.1. Wild bootstrap p is the p-value for the statistical significance of the difference-in-differences coefficient using the Wild Bootstrap that imposes the null from Cameron, Gelbach, and Miller, 2008 with 1000 replications. Wild bootstrap p (interaction) is the p-value for the statistical significance of the interaction term between Treated and High Litigiousness coefficient using the Wild Bootstrap that imposes the null from Cameron, Gelbach, and Miller, 2008 with 1000 replications. In all specifications standard errors are clustered at the province level. * Significant at the 10% level ** Significant at the 5% level *** Significant at the 1% level.

Chapter 3

Payroll Taxes and Informality: Evidence from Argentina

3.1 Introduction

Informal employment accounts for a large share of total employment in low- and middle-income countries. Informality poses important challenges, since it complicates tax collection and the allocation of welfare expenditure, in addition to informal jobs being typically associated with lower wages, benefits, and job security for workers (Camacho, Conover, and Hoyos, 2014; Gerard and Gonzaga, 2016). Informality could also imply a degree of misallocation of resources, since lower productivity informal firms face lower *de facto* costs, which allows them to compete with higher productivity formal firms within the same industries (Ulyssea, 2018; Meghir, Narita, and Robin, 2015).

Employer-borne payroll taxes have frequently been referred to as a contributing factor to informality by affecting the costs firms face for operating formally.[1] The argument is that high payroll taxes imply a high cost of hiring formal workers, leading firms to substitute towards informal workers or to operate informally altogether, thus contributing to high levels of informality. This basic intuition can be extended to various models of informality where payroll taxes constitute a cost of operating formally (e.g. Ulyssea, 2018; Haanwinckel and Soares, 2020), and it has led to policy recommendations of reducing payroll taxes to reduce labor market informality (Pagés et al., 2017). In addition, the existence of widespread informality also bears implications for the effects of payroll tax rate changes on wages, since payroll taxes are levied only on formal workers and informal jobs are typically associated with lower wages. However, empirical evidence on the effects of payroll tax rates on labor markets with widespread informality is scarce.

In this paper, I study how labor markets with widespread informality respond to employer-borne payroll tax rate cuts and hikes. I exploit a series of payroll tax changes implemented in

[1] Throughout the paper, when I refer to "payroll taxes" I am referring to employer-borne payroll taxes unless otherwise noted.

Argentina in the 1990s. These changes varied by geographic area and economic sector, and ended with the adoption of a uniform tax rate across areas in 2001. Starting from an almost uniform tax rate across areas, the federal government began a process of payroll tax cuts in 1993, where the basic premise was to give larger cuts to areas farther away from the City of Buenos Aires. The first tax cut reduced the national rate of 33% to values ranging between 6.6% and 23.1%, and applied only to some sectors until mid-1995, when it was extended to all sectors.[2] A new process of additional minor tax cuts started in 1998, but it was halted in 1999 due to concerns regarding the government budget deficit, leaving different tax rates by area. Finally, all payroll tax cuts were repealed with the adoption of a 23% rate across areas in mid-2001.

I combine detailed data on all of these payroll tax rate changes with large-scale labor-force survey data that, importantly, contains information for both formal and informal workers.[3] The availability of information for both formal and informal workers allows me to explicitly analyze the effects of payroll taxes on the share of informally-employed workers and on salaries for both formal and informal workers, as opposed to analyzing administrative data –that only covers formal workers by definition– and attempting to infer what happens to informal employment and salaries.

I begin by analyzing the labor-market responses to a payroll tax cut. I leverage the fact that, starting from an almost uniform tax rate of 33% across areas, the initial tax cut in 1993 reduced this rate to values ranging between 6.6% and 23.1% depending on the area, and it applied only to some economic sectors until mid-1995. I use a difference-in-differences approach comparing the evolution of workers in sectors affected by the tax cut to that of workers in unaffected sectors. The identification assumption is that, in the absence of the tax cut, outcomes of workers in affected sectors would have followed the same trend as those in unaffected sectors, which is supported by the similar evolution of both groups across various outcomes prior to the reform.

Results show that the payroll tax cut induced a modest reduction of informality, with little-to-no effect on wages. Regarding informality, workers in affected sectors are significantly less likely to be informal after the tax cut: a 10 percentage point reduction in the payroll tax rate reduces the probability of a worker being informal by about 1.5 percentage points on average. Although consistent with intuition and simple models, this estimate indicates a modest effect: taken at face value, reducing informality by 3 percentage points would require a payroll tax cut of over 20 percentage points, which is higher than the actual payroll tax rate in many countries. This mirrors findings from other recent research that has found limited effects of policies that attempt to encourage reductions in informality (see Ulyssea, 2020).

Firm size plays a key role in the effect on informality: the tax cut reduced informality

[2] The sectors for which the initial tax cut applied were Primary Production, Manufacturing, Construction, Tourism, and R&D, leaving sectors like Commerce, Transportation, Financial Services, Real Estate, and several others unaffected. Taxes were temporarily increased in mid-1995 due to fiscal concerns during the Mexican currency crisis. I describe the process of cuts in more detail in Section 2.

[3] I define informality as an indicator of the worker reporting not having access to work-related social security benefits mandated by law (pension contributions, paid medical leave, etc).

only in larger firms, which account for little informality to begin with, without shifting workers away from smaller firms, which account for most informality. In addition, the tax cut reduced the reliance on recently hired workers, who are more likely to be informal, and the overall reduction in informality is driven primarily by lower informality among these recently hired workers. These factors explain the limited effect of the payroll tax cut on reducing informality: the tax cut was not effective for reducing informality in firms that account for most informality (small firms) and its effect on overall informality is through the flow of employment rather than the stock.

Regarding salaries, I find no significant effect of the payroll tax cut on hourly earnings. This contradicts predictions from standard models with only a formal labor market, since these would predict that a tax cut should increase (post-tax) wages as a response to an outward shift in the labor demand. Interestingly, I find no substantial effect on wages for either formal or informal workers after the tax cut, save for a minor and imprecisely estimated reduction for informal workers.[4]

I then turn to the labor-market responses to a payroll tax hike. I leverage the fact that, starting from different tax rates across areas ranging from 9.2% to 19.7% in 1999, a uniform tax rate of 23% was adopted for all areas in mid-2001. I use this convergence of different tax rates into a uniform level with a difference-in-differences approach, comparing the evolution of workers in areas with large tax increases relative to workers in areas with smaller tax increases. An important caveat for this analysis, though, is that this was a time of severe economic instability and recession, which involves an overall increase in unemployment and a substantial exchange rate depreciation.[5] The difference-in-differences approach allows to control for aggregate shocks that affect all areas in the same way at each point in time but, as discussed below, the recessionary context plays a role in explaining the findings.

Results show that workers in areas with larger payroll tax increases are significantly more likely to be informal in the medium term, with little effect on overall employment and wages. Specifically, a 10 percentage point increase in the payroll tax rate increases the rate of informality by about 3 percentage points. Importantly, the response does not kick in immediately, but rather differences in informality arise over a year after the reform. Further evidence indicates that the increase in informality is mirrored by a reduction in formal jobs, with no effect on overall employment, indicating that the payroll tax increase induced a crowd-out of formality in favor of informality. As discussed before, the context at the time probably plays a role for explaining this finding: this was a time of severe economic recession with unemployment increasing uniformly across the country. The difference in informality rates arises once the economy starts to recover in late 2002, which suggests that higher payroll

[4]Interpretation of the evolution of salaries for formal and informal workers should be cautious, however, since informality falls after the tax cut. Comparing wage changes for formal and informal workers separately implies a sample split based on post-treatment behavior and differences could arise due to composition effects after the treatment.

[5]Although not entirely obvious how an exchange rate depreciation should differentially affect informality trends across areas, it could potentially affect salaries. However, as I discuss below, I find no significant effects of the payroll tax increase on salaries.

taxes are crowding out formal jobs in favor of informal jobs during the recovery.

Once again, firm size plays a key role for the effects on informality: the increase in the payroll tax rate reallocated workers to smaller firms –that account for most informality– and reduced the already smaller share of formal employment in such firms. This dynamic is different from the effect of the tax cut, and potentially explains part of the difference in point estimates between the two cases. In addition, and similarly to the effects of the tax cut, the tax hike increased the reliance on short-tenure recently-hired workers, and the increase in informality is primarily driven by higher informality among recently hired workers. Finally, I find no immediate effects on salaries, either for formal or informal workers, albeit there seems to be a minor increase in wages of informal workers, which could potentially be driven by a composition effect (for example, if workers who become informal have higher wages than incumbent informal workers).

This paper contributes to several branches of the literature. First, it contributes to the Public Finance literature studying the effects of payroll taxes on labor markets, which has found important effects on employment, wages, and firm behavior. However, much of this literature has mostly focused on high-income countries where labor informality is low (Saez, Schoefer, and Seim, 2019; Ku, Schönberg, and Schreiner, 2020; Bennmarker, Mellander, and Öckert, 2009; Murphy, 2007; Anderson and Meyer, 1997) or has mostly studied the formal labor market in developing countries (Gruber, 1997; Kugler and Kugler, 2009; Cruces, Galiani, and Kidyba, 2010), leaving policy recommendations for reducing payroll taxes to reduce informality without solid empirical foundations.[6] In this regard, this paper contributes by explicitly studying the effects of payroll taxes on labor markets with widespread informality, uncovering novel facts about the dynamics of how payroll tax rate changes affect informal employment and wages.

This paper also contributes to the literature studying informality in labor markets in developing countries. This literature has found that informal work arrangements account for a substantial fraction of total employment in developing countries (La Porta and Shleifer, 2014), and informal jobs are associated to lower wages, benefits, and job security (Gerard and Gonzaga, 2016). Despite the existence of some debate as to the exact nature and dynamics of the informal sector (e.g. Ulyssea, 2018; Günther and Launov, 2012; Pratap and Quintin, 2006; Maloney, 2004; La Porta and Shleifer, 2014), there is substantial interest in understanding how public policies can affect the level and dynamics of informality (Djankov et al., 2002; Fajnzylber, Maloney, and Montes-Rojas, 2011). Among the policies studied are changes in the minimum wage (Dinkelman and Ranchhod, 2012), firing costs (Adhvaryu, Chari, and Sharma, 2013), openness to trade (Ponczek and Ulyssea, 2021), enforcement of labor standards (Feld, 2022), investments in transit infrastructure (Zárate, 2021), and establishment inspections

[6] Cruces, Galiani, and Kidyba, 2010 is especially related to this paper, since they also analyze the process of payroll tax cuts that took place in the 1990s in Argentina, but using administrative data on employment counts and wages at the area level. However, they explore different periods of tax cuts: the drop in late 1995 after having temporarily raised taxes and the minor tax cuts that took place in 1998 and 1999. In this paper, I explore the initial tax cut of 1993 that lowered tax rate across the country for selected industries, which they do not explore due to the unavailability of administrative records for that period.

(Parra and Bujanda, 2020). Regarding evidence on the effectiveness of policies that attempt to encourage reductions in informality, such as increasing enforcement or reducing the costs of operating formally, recent research shows limited results (see Ulyssea, 2020 for a recent survey).

Regarding payroll taxes specifically, Rocha, Ulyssea, and Rachter, 2018 find that a reduction of registration costs and social security contributions for micro-entrepreneurs in Brazil led to higher business registration, and De Farias and Hsu Rocha, 2021 find this effect to be substantially larger when using more comprehensive data. Some papers have studied the 2012 tax reform in Colombia (which included a payroll tax cut for workers earning less than 10 times the minimum wage) and found some increases in formality (Kugler, Kugler, and Herrera-Prada, 2017; Fernández and Villar, 2017; Morales and Medina, 2017), although other elements of the reform could confound the effect, such as a minimum wage increase and the introduction of a new profit tax. This paper contributes to this literature by also studying the effects of a payroll tax cut on worker-level informality, in addition to a broader set of labor-market outcomes (such as wages and new hiring), while also studying the effects of a payroll tax hike. To the best of my knowledge, all previous research has studied exclusively bundle policies that included payroll tax cuts among its features, while the effects of a payroll tax increase have not been explored before. I uncover novel facts about the effects of payroll taxes in labor markets with widespread informal employment, documenting similarities and differences between the effects of a tax increase and a tax cut.

My findings are consistent with the recent literature studying informality in developing countries, which has found that policies that reduce the cost of formality can be effective for reducing informality but their effects can be too modest for these to be considered cost-effective (Ulyssea, 2020). In this particular case, my findings indicate that large payroll tax cuts are needed to significantly reduce informality, which potentially renders the policy not cost-effective. However, the variation leveraged in this paper only allows for medium term analysis of the tax cut and it is plausible that informality could potentially have fallen further in the longer term. Regarding the effect of a payroll tax increase, the fact that informality does not increase in the short term but it does in the medium term raises dynamic concerns: for instance, an attempt to increase revenue by increasing payroll taxes may have the intended effect in the short term, but it may end up reducing future revenue by increasing informality in the medium to long term. In addition, I find no effect of payroll tax changes on overall new hiring and unemployment, suggesting that payroll tax changes are unlikely to affect employment levels and are more likely to induce shifts in the share of informal employment relative to formal employment.

The rest of the paper is structured as follows. Section 2 describes the context and the data. Section 3 describes the econometric strategy and main results for the response to a tax cut. Section 4 describes the econometric strategy and main results for the response to a tax hike. Section 5 concludes.

3.2 Context and data

Institutional context

Argentina is a middle-income country in South America, with GDP per capita of around US$11,683 as of 2018, according to data from the World Bank. Employers are required by law to register all of their wage-earning employees with the tax authority and to pay monthly payroll taxes. However, as is common in low and middle-income countries in Latin America, a substantial proportion of employment is informal and therefore not subject to taxation and labor regulations, such as the minimum wage, limits to hours worked, paid medical leave, and so on. Recent estimates of the percentage of informal workers are typically around 40% to 50%.[7]

Argentina is a federal country that collects taxes at the federal, provincial, and municipal levels. Payroll taxes are levied at the Federal level and are used to fund the welfare and social security systems (such as pensions and healthcare).[8] In the aftermath of the economic collapse and hyperinflation in the late 1980s, the government started a series of structural market-oriented reforms. After consolidating the payroll tax rate at a flat 33% for employers and 16% for employees in 1991, a law in 1993 gave the Executive power instruments for reducing the tax incidence on labor costs. The main instrument was to allow the Executive power to determine reductions on payroll taxes for employers.

In December 1993, the Federal Government started a process of payroll tax cuts. Based on the assumption that payroll tax cuts would have positive effects on labor markets, areas farther away from the City of Buenos Aires and with higher poverty rates in the 1991 census received larger reductions in payroll tax rates. The initial system consisted on assigning reduction coefficients c by area (such that the tax rate in a given area was $0.33(1-c)$).[9] These coefficients ranged from 0.3 to 0.8, which brought the national tax rate of 33% to values ranging between 6.6% and 23.1%.[10] Initially, the tax cut only applied to Primary Production, Manufacturing, Construction, Tourism, and R&D, leaving other sectors unaffected (e.g. Transportation, Commerce, Financial Services, Real Estate, etc). Thus, define T as the set of targeted sectors that received the tax cut, the payroll tax rate τ in sector s in area a is given by $\tau_{sa} = 0.33 \times (1 - c_a \mathbb{1}\{s \in T\})$.

[7]Figure C.1 shows ILO estimates for the percentage of employment that is informal for various Latin-American countries. These vary between 25% for countries like Chile and Uruguay, to over 70% for countries like Honduras and Guatemala. The estimate for Argentina, the country studied in this paper, is about 45%.

[8]Specifically, payroll taxes on employers consisted of 5 components: (i) retirement contributions, (ii) unemployment insurance, (iii) family subsidies, (iv) healthcare for active workers, (v) healthcare for retired workers. Payroll taxes on employees consisted of 3 components: (i) retirement contributions, (ii) healthcare for active workers, (iii) healthcare for retired workers. Employers were required to make only one payment comprising the full amount of all of these components.

[9]The process was done completely by the Federal government and was fairly transparent with no room for manipulation from local authorities (Cruces, Galiani, and Kidyba, 2010).

[10]Tax rates before the tax cut were not completely uniform since southern provinces had a minor tax benefit for payroll taxes for family subsidies that was eliminated in 1996.

In March of 1995, taxes were increased temporarily due to fiscal concerns during the Mexican currency crisis, and they were later reduced again and applied to all sectors.[11] A new process of additional minor tax cuts started in 1998 but it was halted before reaching its final stage in 1999 due to government budget deficit concerns (Cetrángolo and Grushka, 2004). This left different tax rates by area, ranging from 9.2% to 19.7%. Finally, in an effort to control the government budget deficit, all tax cuts were repealed halfway through 2001 with the adoption of a uniform tax rate across areas of 23%.[12] Figure C.2 shows a stylized timeline with the payroll tax rate variation leveraged for the analysis. Cruces, Galiani, and Kidyba, 2010 use administrative data on taxes effectively collected that shows that the tax changes were effectively implemented.

Data

Data on the tax rate changes were transcribed and reconstructed from the relevant executive orders and ordinances from the tax authority. Tax rates at the time of the initial tax cut were reconstructed by applying the corresponding reduction coefficients to the tax rate prevailing in each area for the economic sectors that the tax cut applied to. Tax rates at the time of the tax hike period were transcribed from several executive orders and ordinances from the tax authority that stated the corresponding rates for each area.[13]

I combine these data on tax rates with labor market household surveys at the area level (called the "Permanent Household Survey" or *Encuesta Permanente de Hogares*).[14] These surveys are the main source of labor market information in Argentina and, at the time, consisted of repeated cross sections at the area level carried out twice a year (first wave in April-May and second wave in October-November). I match the area on the surveys to the areas specified in the executive orders to assign the tax rate corresponding to each area. I have data for 17 areas for the tax cut period and 30 areas for the tax hike period.[15] I

[11]The government introduced a minor change at this point: the component for contributions for healthcare for active workers was not allowed to go below 5%. This effectively meant a lower tax cut than initially implemented.

[12]Tax rates were not completely uniform at this point since very large businesses whose main activity was services were subject to a slightly higher tax rate of 27%. For the analysis, I simply assume the payroll tax rate to be 23% at this point.

[13]Figure C.3 in the appendix shows an example of a table from an ordinance from the tax authority, detailing the rates applicable for each component of payroll taxes depending on the percentage of reduction assigned to each area. All of these tables are publicly available in the website for the Argentinean Revenue Authority: `http://biblioteca.afip.gob.ar/`.

[14]All the micro-data used for the analysis is publicly available at the website for Argentina's National Institute of Statistics and Censuses: `https://www.indec.gob.ar/indec/web/Institucional-Indec-BasesDeDatos`.

[15]This discrepancy in the number of areas across time is due to the fact that when the Census Institute started releasing the survey data, it initially released temporary datasets that contain less detailed information for some areas, with the intent of eventually updating these to permanent datasets with all the information, which did not end up happening (Cattaneo, 2001). These temporary datasets contain less detailed information in general and, specifically, they do not contain data on whether the worker is formal or informal, so I cannot

begin the tax cut period sample in the first semester of 1992, which is the earliest pre-tax cut period for which there is data for multiple areas, and end it in the first semester of 1995, which is the last period before taxes were temporarily increased and the tax cut was extended to all sectors. For the tax hike period, I begin the sample in the second semester of 1999, which is the first wave after which the second minor tax cut process was halted, and end it in the first semester of 2003, when the survey was temporarily interrupted and the new administration implemented several new policy changes for different areas. The Permanent Household Survey contains standard labor market questions such as employment status, education, hourly earnings, age, and family composition. In addition, they also collect information on the sector of employment, which I match to the sectors in the executive order for the tax cut.[16]

Regarding the definition of informality, respondents are asked whether their employer provides them with the work-related social security benefits to which they are entitled to by law (pension contributions, unemployment insurance, paid medical leave, etc). Following the literature, I use a version of the "social protection" definition (see Tornarolli et al., 2014) and define a worker as informal if they report having no access to any work-related social security benefits whatsoever. I then define a dummy variable for informal employment equal to 1 if the worker reports having no access to any work-related social security benefits and zero otherwise.[17] Finally, self-employed individuals pose a challenge since they are not asked questions to determine their formal or informal status, which is expected given that they do not have an employer to make their pension contributions or pay them during medical leave. However, this does not mean that they are informal, since they could still be filing their taxes as independent contractors. One approach in the literature is to consider low-skill self-employed individuals as informal since they are unlikely to be filing taxes (Tornarolli et al., 2014), so I consider an alternative definition of informality that counts self-employed workers who have not completed high school as informal, and present results using this definition as a robustness check.

Table 3.1 presents summary statistics. Panel A presents statistics for the tax cut period sample and Panel B for the tax hike period sample.[18] The variable "Informal worker" is a

use them for the analysis.

[16]The economic sectors are categorized as defined by the Census Institute: primary production; manufacturing of foods, drinks, and tobacco; manufacturing of textiles, clothing, and shoe-wear; manufacturing of chemical products, oil, and nuclear fuel; manufacturing of metallic products and machinery; other manufacturing industries; supply of electricity, gas, and water; construction; wholesale commerce; retail commerce; restaurants and hotels; transportation; services linked to transportation and communication; financial intermediation; real estate services; public administration and defense; teaching; social and health services; other social and community services activities; repair services; domestic services at private homes; other personal services. The treated sectors are primary production, the manufacturing sectors, construction, and restaurants and hotels.

[17]A standard way to categorize employees as informal is if they report not contributing to the pension system (e.g. Tornarolli et al., 2014). Unfortunately, this specific answer was not coded in the datasets at the time, so I rely on a similar definition but for not having any social security benefits whatsoever.

[18]One production sector was excluded from the analysis for the tax cut period: "industrial production

dummy variable equal to 1 if the worker is an employee whose employer does not give them any of the social security benefits that they should by law, which is about 23% of wage earners. The average worker makes AR$3.21 per hour, which at the time was equal to US$3.21 due to a fixed parity between the Argentine peso and the US dollar.[19] Finally, the data also contains demographic controls such as gender, age, and indicators for education, in addition to the payroll tax rate corresponding to the sector-area level.[20] Panel B presents summary statistics for the tax hike period sample. The share of wage earners that are informal is about 40%. The average worker makes AR$2.79 per hour, which is no longer equal to the same amount in US dollars since the fixed exchange rate was repealed in 2002. The data contains the same demographic variables as before, which indicates a slightly higher presence of females and slightly higher indicators of education relative to the sample in the tax cut period.

3.3 Response to a tax cut

Econometric strategy

Leveraging the fact that the initial payroll tax cut only affected some economic sectors, I begin my analysis with a standard event study approach, comparing the evolution of workers in sectors that received the tax cut to workers in sectors that did not. The equation to estimate is given by:

$$Y_{isat} = \sum_{k=-4}^{k=2} \beta_k d_{tk} \times TargetedSector_s + X'_{isat}\delta + \alpha_a + \theta_s + \mu_t + \varepsilon_{isat} \tag{3.1}$$

where Y_{isat} is any of our outcomes of interest for worker i working in sector s in area a at time t, X'_{isat} is a vector of controls, α_a is a fixed effect for geographic area, θ_s is a fixed effect for sector, μ_t is a fixed effect for time, $TargetedSector_s$ is a dummy variable equal to 1 if sector s received the tax cut, and ε_{isat} is the error term. The coefficients of interest here are the β_k, which measure the difference in trends between workers in sectors that received the tax cut and workers in sectors that did not. For negative values of k, the β_k coefficients allow us to assess the evolution of the trends before the tax cut, and for positive values of k the coefficients indicate the dynamics of the effect.

I also estimate a variation of equation 3.1 by including the numerical value of the tax rate instead of the difference-in-differences coefficients:

$$Y_{isat} = \beta TaxRate_{sat} + X'_{isat}\delta + \alpha_a + \theta_s + \mu_t + \varepsilon_{isat} \tag{3.2}$$

of tobacco, food, and beverages", because of a law that passed around the same time that created several regulations about the production and taxation of tobacco (*Ley 24,291*, passed on December of 1993).

[19] This fixed parity between the peso and the dollar was implemented as a way to stabilize inflation in 1991, which was in the single digits throughout the decade.

[20] Table 3.2 shows a comparison between the affected and unaffected sectors, showing a slightly higher rate of informality, lower education levels, and a significantly lower proportion of women in the sectors that received the tax cut. Thus, I control for these factors in several specifications to account for these differences.

where $TaxRate_{sat}$ is the payroll tax rate for sector s in area a at time t, measured from 0 to 100. Estimating this equation exploits variation in the tax rate by sector, area, and time to identify the effect of payroll taxes. The coefficient of interest is β, which captures the effect of increasing the payroll tax rate by one percentage point on the outcome of interest Y_{isat}. The main outcomes of interest are an indicator of being informally employed and the hourly salary from the main occupation. In addition, I will analyze the effects of the tax rate on an indicator of having been recently hired and the firm size for additional results. In all specifications, I report standard errors clustered at the sector-by-area level, which is the dimension in which the payroll tax rate varies.

Results

Figure 3.1 reports OLS estimates for the event study coefficients from equation 3.1 for several dependent variables. In panel (a), the dependent variable is an indicator of the worker being informally employed. The pattern indicates stable trends between affected and non-affected sectors before the tax cut, which diverge with a minor reduction in informality after the tax cut takes place. On average, the probability of a worker being informal falls by about 3 percentage points after the tax cut compared to workers in sectors unaffected by the policy. This is reflected in columns 1 and 2 of table 3.3, which reports OLS estimates of several variations of equation 3.2, regressing the dummy variable for being informal on the payroll tax rate corresponding to the sector-area-time. The estimate is stable across specifications and it indicates that a 1 percentage point increase in the payroll tax rate increases the probability of being informal by about 0.13 percentage points on average. Conversely, a reduction of the payroll tax rate of 10 percentage points implies a reduction in the probability of being informal by about 1.3 percentage points on average. Across specifications, the coefficient on the payroll tax rate is statistically significant at the 5% level.

Overall, these results indicate that payroll tax cuts reduce informality, although the effect is modest. A 10 percentage point reduction in the payroll tax rate represents a substantial tax reduction (payroll tax rates typically oscillate around 10% to 35% across countries), while a 1.5 percentage point reduction in informality is a minor effect relative to how widespread informality is (about 27% of workers in the sample are informal in this period). This is consistent with recent research in informality that finds that, although reducing the costs of formality can be effective at reducing informality, the effects can be too modest for these policies to be considered cost-effective (Ulyssea, 2020). Admittedly, however, the variation exploited in this paper only allows for medium-term analysis of the policy (about 18 months), so it could be possible that informality would have been reduced even further in the long term.

To further understand the effect on informality, I analyze the role of firm size. As is common in developing countries, most informal employment is concentrated in small firms (Ulyssea, 2018). In the survey, wage earners are asked how many employees work in the establishment they work in, I define a firm as small if the firm has up to 25 employees.[21] Panel (b) of figure 3.1 presents OLS estimates of the event study coefficients from equation 3.1 where the dependent variable is a dummy variable equal to 1 if the worker works in a firm of up to 25 employees. Notably, there is no significant break in trends after the tax cut, indicating that the tax cut did not induce a shift of employment either into or away from small firms. This is reflected in the OLS estimates of equation 3.2 reported in columns 1 and 2 of panel D of table 3.3, indicating small and non-statistically significant coefficients. Panel B of table 3.3 presents OLS estimates of equation 3.2 interacting the payroll tax rate with the dummy variable of small firm, indicating that the payroll tax rate significantly reduced informality in non-small firms, while this effect is completely canceled out for small firms. Thus, the payroll tax cut only reduced informality in large firms (which account for little informality to begin with), while not affecting informality in the firms that account for most of it (small firms).

I then analyze whether workers in sectors that received the tax cut are more likely to have been recently hired, since informality is more widespread among recently hired workers. Panel (c) of Figure 3.1 presents OLS estimates of the event study coefficients from equation 3.1 where the dependent variable is a dummy variable equal to 1 if the worker started working on their current job at most one year ago. There is a minor reduction in the share of workers that have been recently hired after the tax cut. This is reflected in the OLS estimates of equation 3.2 reported in panel D of table 3.3, which shows small and non-significant coefficients. Panel C presents OLS estimates of equation 3.2 interacting the payroll tax rate with the dummy variable of recently hired, indicating that the effect is entirely driven by reductions in informality of recent hires. This suggests that payroll tax cuts affect the flow more so than the stock of informality.

I then turn to the effects of payroll taxes on salaries. Panel (d) of figure 3.1 shows OLS estimates of event-study coefficients from equation 3.1, where the dependent variable is the natural logarithm of the hourly income from the main occupation. Notably, there is no significant pattern of effects on wages. This is reflected in the OLS estimates of several variations of equation 3.2 reported in columns 3 and 4 of table 3.3, where the dependent variable is the natural logarithm of the hourly wage from the main occupation. The estimate is stable across specifications, indicating a small and non-statistically significant effect: a 1 percentage point increase in payroll taxes implies a 0.13% increase in hourly salaries. Panel B indicates that a tax cut induces an increase in wages for workers in large firms, and that this effect is canceled out for workers in small firms. Panel C indicates that a tax cut reduces salaries among new hires, with little effect on workers with longer tenure on the job.[22]

These findings on the effects on salaries contradict predictions from standard models with only one (formal) labor market. In such models, a reduction in payroll taxes produces an

[21] This exercise results in a drop in the number of observations because workers can report not knowing how many people work in their establishment.

[22] This could potentially be driven by a composition effect given that informality falls among new hires. For instance, if "higher productivity informal workers" are the ones who transition into formal employment, then the workers who remain informal are those with lower productivity and, potentially, lower wages.

outward shift in the labor demand, which should result in an increase in (post-tax) wages, while the estimates shown above indicate a negligible effect of the tax cut. Notably, running the event studies for formal and informal workers separately indicates that the evolution of wages for formal and informal workers is similar after the tax cut, as shown in panel (e) of Figure 3.1, although statistical power is low after splitting the samples. Interpretation should be cautious here, however, since this analysis relies on a post-treatment split of the sample.

To sum up, evidence in this section indicates that the payroll tax cut reduced informality in the labor market, although the effect is modest. The tax cut reduced informality only in larger firms (which account for little informality), without shifting workers away from smaller firms (which account for most informality). This reduction in informality is driven primarily by lower informality among recently hired workers in affected sectors, with a reduction in the total share of newly hired workers. Taken at face value, these estimates indicate that large tax cuts would be needed to significantly reduce informality, although the variation only allows for medium-term analysis. Regarding effects on salaries, there is no evidence of strong effects of the tax cut on salaries, either for formal or informal workers.

Robustness checks

In this subsection, I present several robustness checks for the main empirical analysis. First, recall that the main analysis does not include self-employed workers, since they are not asked the questions to determine informal status. However, this does not mean that they are informal, since they could be paying taxes as independent contractors. One approach in the literature has been to include self-employed individuals who are low-skilled as informal, since they are unlikely to be registered (e.g. Tornarolli et al., 2014). I follow this approach and construct an indicator of informality that includes self-employed individuals who have not completed high school as informal and those with higher educational attainment as formal, in addition to the definition for wage earners previously used. Panel A of table C.1 reports the OLS coefficients of the effect of the payroll tax rate on the probability of being informal and the hourly wage from equation 3.2 while including self-employed individuals in the sample. Across specifications, the main results remain very similar to the baseline specification when including the self-employed in the estimation.

Second, recall that some individuals have missing data regarding the firm size and how long ago they started their current job. This is because individuals can report not being sure how many workers are employed in their establishment or how long ago they started working in their current job. Although this is plausibly unrelated to the payroll tax cuts, I conduct an additional robustness check including in the sample only individuals who have no missing data for the indicator of informality, firm size, and how long ago they started their current job. The results from this exercise can be found in panel B, which shows qualitatively similar results to the baseline specification, albeit the coefficients for the effect on earnings become statistically significant in the expected direction (lower taxes increase post-tax wages).

Third, the main specification exploits sector-by-area-by-time variation in the payroll tax rate to identify the effect on informality and wages. A potential concern is that the results could be driven by differential trends at the area level that happen to be correlated with the sizes of the tax cuts. To assess whether potential differential trends across areas are driving the results, I control for area-by-time fixed effects, exploiting only the sector-by-time variation in the payroll tax rate. Results from this exercise can be found in panel C, which shows similar results to the baseline specification.

Fourth, given the limited number of areas available for analysis, one could be concerned that the results are driven by the evolution of some specific area. To assess this concern, I conduct a series of leave-one-out robustness checks, where I estimate the effect of the payroll tax rate on informality and hourly wages while sequentially dropping one of the areas from the analysis. The results from this exercise can be found in figure C.5, where panel (a) shows the coefficients for the effect on the probability of being informal and panel (b) shows the effect on the natural logarithm of the hourly wage. In both panels, the coefficient of interest remains stable across specifications, indicating that the effect is not driven by some specific area.

3.4 Response to a tax hike

Econometric strategy

I now turn to the effects of a tax increase. Exploiting that, starting from different tax rates across areas in 1999, a higher uniform tax rate for all areas was adopted in mid-2001, I estimate a modified event-study specification interacting the event study dummy variables with the change in the payroll tax rate:

$$Y_{iat} = \sum_{k=-4}^{k=3} \beta_k d_{tk} \times \Delta TaxRate_a + X'_{iat}\delta + \alpha_a + \mu_t + \varepsilon_{iat} \tag{3.3}$$

where $\Delta TaxRate_a$ is the increase in the tax rate at the area level (normalized by 5 percentage points for easier interpretation), X_{iat} is a vector of individual controls, α_a is a fixed effect for area, μ_t is a fixed effect for time, and ε_{iat} is the error term. The coefficients of interest here are the β_k, which measure the difference in trends between workers in areas that received a larger tax increase compared to workers in areas that received a smaller tax increase. For negative values of k, the β_k coefficients allow us to assess the evolution of the trends before the tax hike, and for positive values of k the coefficients indicate the dynamics of the effect.[23]

As for the analysis for the tax cut, I also estimate a variation of equation 3.3 by including the numerical value of the tax rate instead of the difference-in-differences coefficients:

$$Y_{iat} = \beta TaxRate_{at} + X'_{iat}\delta + \alpha_a + \mu_t + \varepsilon_{iat} \tag{3.4}$$

[23] A potential concern for the tax increase analysis is that the results could be driven by differential trends related to potential long-run effects of the original tax cut years earlier. The assumption for this analysis is that, whatever the long-run effects of the tax cuts were, a new steady state state has been reached by the time of the tax increase, which seems plausible given the lack of significant pre-treatment trends.

where $TaxRate_{at}$ is the payroll tax in area a at time t, measured from 0 to 100. Estimating this equation exploits variation in the tax rate by area and time to identify the effect of payroll taxes. The coefficient of interest is β, which captures the effect of increasing the payroll tax rate by one percentage point on the outcome of interest Y_{iat}. Again, the outcomes of interest are: (i) a dummy variable equal to 1 if a worker is informal relative to formal (using both definitions of informality), (ii) the hourly salary from the main occupation, and (iii) a dummy variable if the worker was recently hired. I report standard errors clustered at the area level, which is the dimension in which the payroll tax rate varies.

Results

Figure 3.2 reports OLS estimates of the event study coefficients from equation 3.3 for several dependent variables. In panel (a), the dependent variable is an indicator equal to 1 if the worker reports being informal and zero otherwise. Notably, there is no significant break in trends immediately after the tax increase, and a significant increase in informality is noticeable 1.5 years after the tax hike. Given the normalization of the change in the tax rate by 5 percentage points for this figure, these coefficients indicate that increasing the payroll tax rate by 5 percentage points has little effect in the short run, but implies an increase in the probability of a worker being informal by about 2.5 percentage points in the medium run.

Panel A of table 3.4 reports OLS estimates of equation 3.4. In columns 1 and 2, I normalize the dependent variable to be equal to 100 if the worker is informal and zero if not to simplify the interpretation of the coefficient. The coefficient is not statistically significant at conventional levels, which reflects the fact that the effect is not apparent immediately after the tax hike. These coefficients indicate that a 1 percentage point increase in the tax rate increases the probability of a worker being informal by about 0.2 percentage points. Panel B separates between the short-run and long-run effect of payroll taxes, indicating a significant increase in informality in the long-run across specifications.

Overall, these results indicate that increasing payroll taxes increases the proportion of workers who are informal. Importantly, this increase in informality is not evident immediately after the tax increase, but rather becomes significant in the medium term. The economic context at the time can potentially explain this: this was a time of severe economic recession and political instability, with unemployment increasing across the board, as shown in the evolution of the probability of being unemployed reported in panel (a) of figure 3.3.[24] This increase in unemployment is not differential across tax areas, as shown in panel (b), where areas with larger tax increases have a similar evolution of unemployment relative to areas with lower tax increases. The differences in informality after the tax increase arise as economic recovery starts to kick in and unemployment falls, which suggests that the increase in payroll taxes could be crowding out formal jobs in the recovery. Panel (c) investigates

[24]For this exercise, we include self-employed individuals in the sample (not just wage earners), to account for the full labor force.

this by reporting the event study coefficients of equation 3.3 by separating respondents into three categories: (i) unemployed, (ii) informal, and (iii) formal, finding that the increase in informal employment is mirrored by a reduction of formal employment, with little effect on unemployment.[25]

To further understand the effect on informality, I analyze the role of firm size. Similarly as for the tax cut analysis, I define a firm as small if the firm has up to 25 employees. Panel (b) of figure 3.2 presents OLS estimates of the event study coefficients from equation 3.3 where the dependent variable is a dummy variable equal to 1 if the worker works in a firm of up to 25 employees. Notably, and contrary to the tax cut analysis, there is a significant increase in the share of workers employed in small firms after the tax hike. This is reflected in the OLS estimates of equation 3.4 reported in panel E of table 3.4, which indicate that higher payroll taxes increase the share of workers employed in small firms. Specifically, a 10 percentage point increase in the payroll tax rate increases the probability of working in a small firm by about 2 to 3 percentage points. Panel B of Table 3.4 presents OLS estimates of equation 3.2 interacting the payroll tax rate with the dummy variable of small firm, indicating that the payroll tax rate significantly increases informality in small firms (which already have more informality), with no effect on larger firms. Thus, the payroll tax increase shifted workers towards smaller firms, which have more informality, and reduced their already smaller share of formal workers.

I then analyze whether workers in areas that received a larger tax rate hike are more likely to have been recently hired, since informality is more prevalent among recently hired workers. Panel (c) of figure 3.2 presents OLS estimates of the event study coefficients from equation 3.1 where the dependent variable is a dummy variable equal to 1 if the worker started working on their current job at most one year ago. Although confidence intervals are wide, there is a significant increase in the share of workers that have been hired recently, who are more likely to be informal. This is reflected in the OLS estimates of equation 3.4 reported in panel E of table 3.4, which that the tax hike increased the share of workers who have been recently hired. Panel D of table 3.4 presents OLS estimates of equation 3.2 interacting the payroll tax rate with the dummy variable of recently hired, indicating that the effect is entirely driven by increases in informality among recent hires.

I now turn to the effects of payroll taxes on salaries. Panel (d) of figure 3.2 presents OLS estimates of the event study coefficients from equation 3.1, where the dependent variable is the natural logarithm of the hourly income from the main occupation. Similarly as was the case for the tax cut, there is no substantial effect on the evolution of salaries after the tax hike. Panel (e) shows the event study coefficients separately for formal and informal workers, indicating a flat evolution for formal workers while there is an increase in wages. This could potentially be driven by a composition effect, for instance, if workers who become informal have higher wages than incumbent informal workers. However, the interpretation

[25]This type of analysis on crowd out of formal jobs due to changes in payroll taxes is not feasible for the tax cut period, since the variation exploited for that analysis is by sector and unemployment is not a sector-specific variable.

here should be cautious, not only for the sample split based on post-shock behavior (as was the case for the tax cut analysis), but also because statistical power is low and confidence intervals include large effects.[26]

These results are reflected in OLS estimates of equation 3.4 reported in columns 3 and 4 of table 3.4. In both specifications, the estimated coefficient is small and not statistically significant at conventional levels. Panel B interacts the payroll tax rate with the indicator of being employed in a small firm, indicating no significant effect of payroll taxes on wages, either for small or large firms. Panel D interacts the payroll tax rate with an indicator of having been recently hired, showing no effect of payroll taxes on wages for workers with a long tenure on their job, and a minor increase among recent hires, albeit with inconsistent significance across specifications.

Results on the effects on salaries reveal a similar pattern to the one found for the effect of a tax cut: negligible effects and a similar evolution between formal and informal workers. Again, this contradicts findings from a standard only-formal market model, which would predict a reduction in (post-tax) wages following a tax increase, due to a reduction in the labor demand. Interestingly, the evolution of wages for formal and informal workers is similar immediately after the tax increase, while there is an increase in the wages of informal workers.

Summing up, evidence in this section indicates that the payroll tax hike increased informality in the labor market, with little effect on salaries and overall new hiring. Although the effect is not evident in the short term, a 10 percentage point increase in the payroll taxes increases informality by about 4 percentage points in the medium term. The recessionary context potentially plays a role in explaining this fact, since employment is falling across the board, and the differences in informality rates across tax areas become evident as the economic recovery begins. This increase in informality is mirrored by a reduction of formal jobs, with no significant effects on unemployment, suggesting that the higher payroll taxes are crowding out formal jobs during the economic recovery. Firm size plays a key role: after the tax cut, workers are more likely to work in small firms (which account for more informality) and, reduced their already smaller share of formal employment. Similarly to the tax cut, the increase in informality is primarily driven by increased informality among recent hires, suggesting that payroll taxes affect the flow of informality more so than the stock. Regarding effects on salaries, there is no evidence of strong effects of the tax hike on wages, save for an increase among informal workers in the medium term.

Robustness checks

In this subsection, I present several robustness checks for the main empirical analysis. First, recall once again that the main analysis does not include self-employed workers, since they are not asked the questions to determine informal status even though they could be paying taxes as independent contractors. To include self-employed workers, I construct an

[26]For instance, although the point estimates for the effect on salaries of formal workers are close to zero, some of the confidence intervals include hypothetical pass-through values of substantial magnitude (for example 40%).

indicator of informality that includes self-employed individuals who have not completed high school as informal and those with higher educational attainment as formal, in addition to the definition for wage earners used in the main specification. Panel A1 of table C.2 reports the OLS coefficients of the effect of the payroll tax rate on the probability of being informal and the hourly wage from equation 3.4 while including self-employed individuals in the sample. Panel A2 separates between the short-run and the long-run effect. Across specifications, the main results remain very similar to the baseline specification when including the self-employed in the estimation.

Second, recall once again that some individuals have missing data regarding the firm size and how long ago they started their current job, since respondents can report being unsure as to how many workers are employed in their establishment or how long ago they started working in their current job. Although this is plausibly unrelated to the payroll tax increase, I conduct an additional robustness check including in the sample only individuals who have no missing data for the indicator of informality, firm size, and how long ago they started their current job. The results from this exercise can be found in panels B1 and B2, both of which show qualitatively similar results to the baseline specification, albeit the coefficients for the effect on earnings become statistically significant in the expected direction (lower taxes increase post-tax wages).

Third, given the limited number of areas available for analysis, concerns could arise that the results are driven by the evolution of some specific area. To assess this concern, I conduct a series of leave-one-out robustness checks, where I estimate the effect of the payroll tax rate on informality and hourly wages while dropping one of the areas from the analysis. The results from this exercise can be found in figure C.6, where panel (a) shows the coefficients for the effect on the probability of being informal and panel (b) shows the effect on the natural logarithm of the hourly wage. In both panels, the coefficient of interest remains stable across specifications, indicating that the effect is not driven by the evolution of some specific area.

3.5 Conclusion and discussion

In this paper, I analyze how labor markets with high informality respond to reductions and increases in employer-borne payroll tax rates. I leverage a process of area-varying payroll tax cuts that took place in Argentina in the 1990s, which culminated with the adoption of a uniform tax rate in 2001. Results show modest effects on informality in expected directions: tax cuts reduce the share of informally-employed workers while tax hikes increase it. Firm size plays a key role in mediating these effects: the tax cut reduced informality only in larger firms without any effect on smaller firms (which account for most informality to begin with), while the tax increase shifted employment towards smaller firms (where most informal employment takes place) and reduced their already smaller share of formal employment. In addition, higher payroll taxes increase reliance on recently hired workers, who are more likely to be informal, while tax cuts reduce it, and the changes in informal employment are driven by recently hired workers, indicating that payroll taxes affect the flow more so than the stock of informality. I find no significant effects on salaries for the tax cut or the tax hike, for either formal or informal workers.

Results regarding the effect of a payroll tax cut are consistent with recent research regarding policies for reducing informality in developing countries, which has found that policies that reduce the cost of formality can be effective for reducing informality, but their effects can be too modest for such policies to be cost-effective (Ulyssea, 2020). Taken at face value, my results indicate that unrealistically large tax cuts are necessary to obtain substantial reductions in informality: for instance, reducing informality by 3 percentage points would require a reduction in the payroll tax rate of about 20 percentage points, which is higher than the actual payroll tax rate in many countries. This modest effect is explained by the fact that the tax cut seems to have reduced informality only in large firms, which account for little informality to begin with.

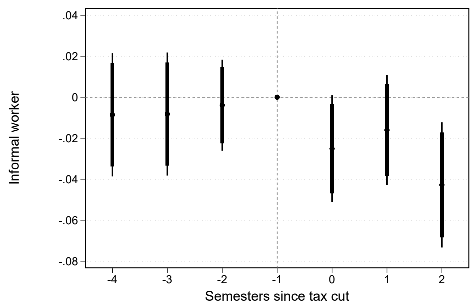
The analysis of the effect of a payroll tax increase indicates no significant effect in the short term, but a significant increase in informality in the medium term. This creates dynamic concerns, since an attempt to increase revenue by increasing payroll taxes can have the intended effect in the short term, but it can also reduce future tax revenue by increasing informality in the medium term. In addition, the increase in informal employment is due to a crowd-out of formal employment, with little effect on overall employment. This suggests that discussions on the effects of increasing payroll taxes in developing countries should focus less on effects on overall employment and more on potential crowding-out effects of formal employment in favor of informal employment.

Finally, the negligible effects on salaries found for both the tax cut and the tax hike could potentially be explained by the existence of informal payments for formal workers. Recent literature has found that paying formal workers a fraction of the salary "off the books" to avoid taxes is commonplace in developing countries (Bergolo and Cruces, 2014; Kumler, Verhoogen, and Frías, 2020), and it is plausible that workers include these payments in the income reported in surveys, since the salary question is interpreted as asking about post-tax "take-home" pay. Therefore, the negligible effects on salaries could arise if employers and employees arrange to substitute between reported and unreported payments in response to payroll tax changes in a manner that leaves the "take-home" income unchanged. This presents potential avenue for future research.

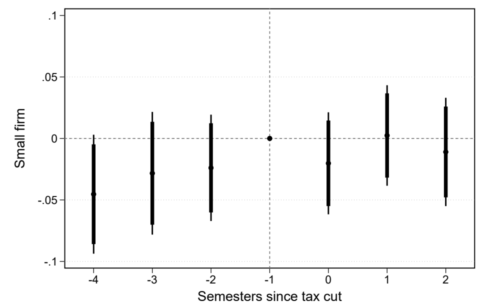
3.6 Figures

Figure 3.1: Main effects of the tax cut - event study coefficients

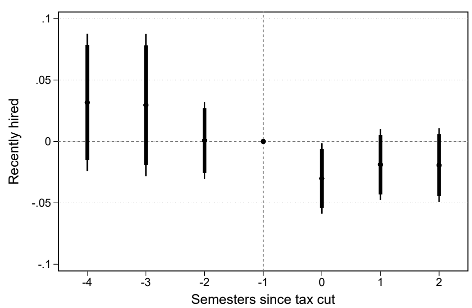
(a) Informal



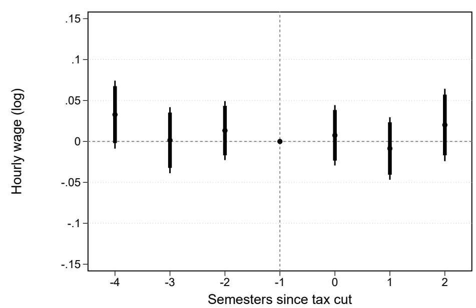
(b) Small firm



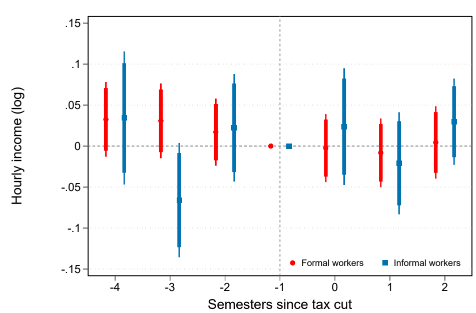
(c) Recenlty hired



(d) Hourly wage



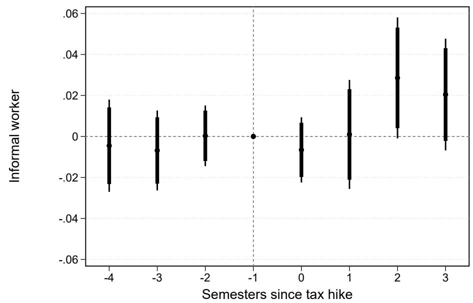
(e) Hourly wage (sep. by informality)



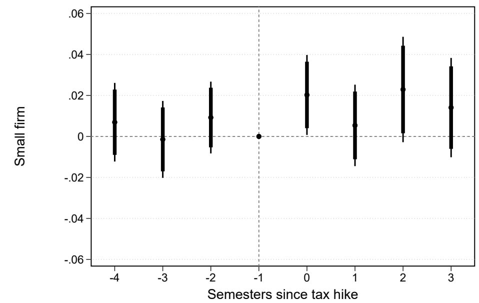
Notes: This figure shows OLS estimates for the event-study coefficients from several variations of equation 3.1. In panel (a) the dependent variable is an indicator of whether the worker is informal. In panel (b) the dependent variable is an indicator equal to 1 if the worker is employed at at a small firm (up to 25 employees). In panel (c) the dependent variable is an indicator of whether the worker started their job up to one year ago. In panels (d) and (e) the dependent variable is the natural logarithm of the hourly wage. Panel (e) separately estimates the event-study coefficients for formal workers (in red) and informal workers (in blue). Standard errors are clustered at the sector-by-area level. Thick vertical bars represent 90% confidence intervals and thin vertical bars represent 95% confidence intervals.

Figure 3.2: Main effects of the tax hike - event study coefficients

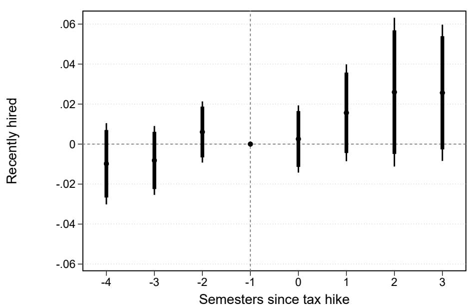
(a) Informal



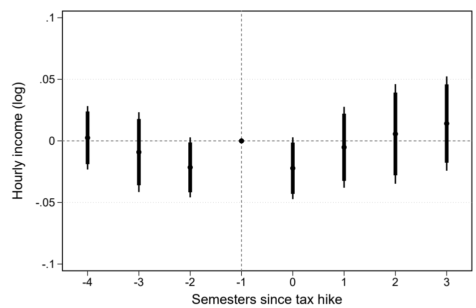
(b) Small firm



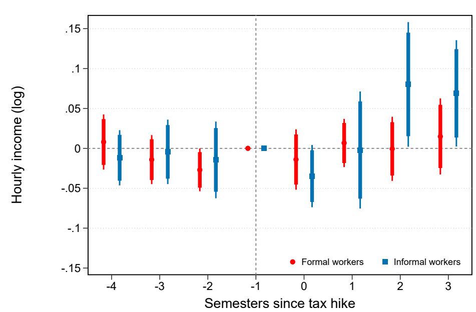
(c) Recenlty hired



(d) Hourly wage



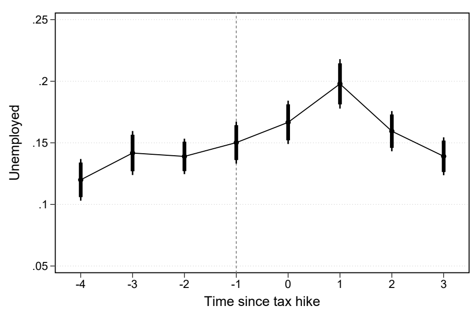
(e) Hourly wage (sep. by informality)



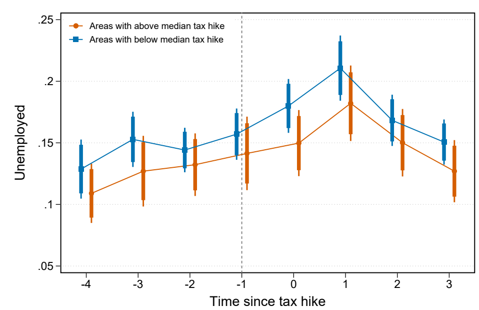
Notes: This figure shows OLS estimates for the event-study coefficients from several variations of equation 3.3. In panel (a) the dependent variable is an indicator of whether the worker is informal. In panel (b) the dependent variable is an indicator equal to 1 if the worker is employed at at a small firm (up to 25 employees). In panel (c) the dependent variable is an indicator of whether the worker started their job up to one year ago. In panels (d) and (e) the dependent variable is the natural logarithm of the hourly wage. Panel (e) separately estimates the event-study coefficients for formal workers (in red) and informal workers (in blue). Standard errors are clustered at the sector-by-area level. Thick vertical bars represent 90% confidence intervals and thin vertical bars represent 95% confidence intervals.

Figure 3.3: Tax hike, unemployment, and formality crowd-out

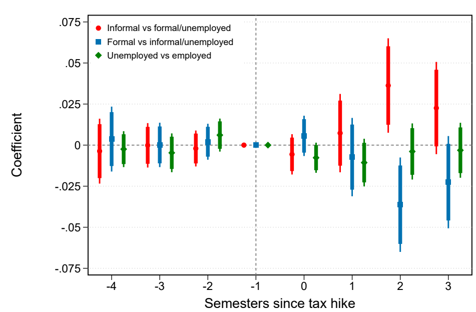
(a) Unemployment level



(b) Unemployment level (by tax hike size)



(c) Crowd-out event-study



Notes: This figure shows the evolution of the probability of being unemployed over time and OLS estimates for the event-study coefficients from several variations of equation 3.3. Panel (a) shows the probability of being unemployed over time for the whole sample. Panel (b) shows the probability of being unemployed over time separating by the size of the tax hike: areas that received below-median tax hikes are in blue and areas that received above-median tax hikes are shown in orange. Panel (c) shows OLS coefficients from several variations of equation 3.3. Coefficients in red correspond to the event study of the probability of being informal relative to being formal or unemployed. Coefficients in blue correspond to the event study of the probability of being formal relative to being informal or unemployed. Coefficients in green correspond to the event study of the probability of being unemployed relative to being formal or informal. Standard errors are clustered at the area level. Thick vertical bars represent 90% confidence intervals and thin vertical bars represent 95% confidence intervals.

3.7 Tables

Table 3.1: Summary statistics

	Observations	Mean	Standard Deviation	Median
Panel A. Tax cut period				
Informal worker	97,897	0.270	0.444	0.000
Hourly wage	94,081	3.305	2.921	2.500
Hours worked	83,902	40.640	15.520	43.000
Small firm	76,587	0.664	0.472	1.000
Recently hired	85,476	0.316	0.465	0.000
Payroll tax rate	97,897	30.473	5.549	33.000
Age	97,897	35.123	12.138	34.000
Female	97,897	0.420	0.494	0.000
Primary school incomplete	97,202	0.095	0.294	0.000
Primary school complete	97,202	0.490	0.500	0.000
Secondary school complete	97,202	0.289	0.453	0.000
College complete	97,202	0.125	0.331	0.000
	Observations	Mean	Standard Deviation	Median
Panel B. Tax hike period				
Informal worker	129,444	0.400	0.490	0.000
Hourly wage	162,477	3.292	3.686	2.310
Small firm	173,660	0.783	0.412	1.000
Unemployed	225,101	0.166	0.372	0.000
Recently hired	187,764	0.324	0.468	0.000
Payroll tax rate	225,101	18.865	4.459	19.700
Age	225,101	37.386	13.149	36.000
Female	225,101	0.408	0.491	0.000
Primary school incomplete	225,101	0.096	0.295	0.000
Primary school complete	225,101	0.461	0.498	0.000
Secondary school complete	225,101	0.311	0.463	0.000
College complete	225,101	0.132	0.339	0.000

Notes: Informal worker is a dummy variable equal to 1 if the worker is an informal wage earner and zero if formal. Self-employed is a dummy variable equal to 1 if the worker reports being self-employed. Recently hired is a dummy variable equal to 1 if the worker has a tenure of one year or less at their current job. Income per hour is the hourly income from the main occupation in AR$. Small firm is a dummy variable equal to 1 if the respondent works in a firm of up to 25 workers. Payroll tax rate is the payroll tax rate at the sector by area level. Age is the age in years. Female is a dummy variable equal to 1 if the respondent is female. Primary school incomplete is a dummy variable equal to 1 if the respondent has not completed primary school. Primary school complete is a dummy variable equal to 1 if the respondent has completed primary school. Secondary school complete is a dummy variable equal to 1 if the respondent has completed secondary school. College complete is a dummy variable equal to 1 if the respondent has completed any college degree.

Table 3.2: Pre-cut comparison between targeted and non-targeted sectors

Variable	(1) Targeted	(2) Non-targeted	(3) Difference
Informal worker	0.283	0.263	-0.020***
	(0.450)	(0.440)	(0.003)
Hourly wage	3.039	3.419	0.380***
	(2.874)	(2.940)	(0.021)
Recently hired	0.373	0.294	-0.079***
	(0.484)	(0.456)	(0.004)
Primary school incomplete	0.133	0.080	-0.053***
	(0.339)	(0.271)	(0.002)
Primary school complete	0.608	0.443	-0.165***
	(0.488)	(0.497)	(0.004)
Secondary school complete	0.219	0.318	0.099***
	(0.413)	(0.466)	(0.003)
College complete	0.041	0.159	0.118***
	(0.198)	(0.366)	(0.002)
Female	0.176	0.519	0.342***
	(0.381)	(0.500)	(0.003)
Age	34.532	35.379	0.846***
	(12.095)	(12.146)	(0.086)
Observations	27,894	69,415	97,897

Notes: Informal (wage earner) is a dummy variable equal to 1 if the worker is an informal wage earner and zero if formal. Self-employed is a dummy variable equal to 1 if the worker reports being self-employed. Recently hired is a dummy variable equal to 1 if the worker has a tenure of one year or less at their current job. Income per hour is the hourly income from the main occupation in AR$. Primary school incomplete is a dummy variable equal to 1 if the respondent has not completed primary school. Primary school complete is a dummy variable equal to 1 if the respondent has completed primary school. Secondary school complete is a dummy variable equal to 1 if the respondent has completed secondary school. College complete is a dummy variable equal to 1 if the respondent has completed any college degree.

Table 3.3: Main effects of the tax cut

	=100 if informal		Hourly wage (log)	
	(1)	(2)	(3)	(4)
Panel A. Overall effect				
Payroll tax rate	0.163**	0.131**	0.00104	0.00137
	(0.0643)	(0.0584)	(0.00111)	(0.000936)
Controls		✓		✓
Observations	97309	96623	93509	92859
R Squared	0.185	0.239	0.286	0.415
Panel B. Heterogeneity by firm size				
Payroll tax rate	0.365***	0.307***	-0.00322**	-0.00216**
	(0.109)	(0.104)	(0.00130)	(0.00104)
Small firm	31.90***	27.14***	-0.392***	-0.272***
	(4.681)	(4.300)	(0.0444)	(0.0365)
Payroll tax rate × Small firm	-0.383***	-0.322**	0.00673***	0.00506***
	(0.144)	(0.136)	(0.00145)	(0.00120)
Controls		✓		✓
Observations	76053	75498	73051	72524
R Squared	0.237	0.281	0.339	0.457
Panel C. Heterogeneity by new worker				
Payroll tax rate	-0.0322	-0.0336	-0.000457	-0.000538
	(0.0706)	(0.0693)	(0.00104)	(0.000885)
Recently hired	17.87***	14.30***	-0.352***	-0.268***
	(3.678)	(3.547)	(0.0348)	(0.0321)
Payroll tax rate × Recently hired	0.338***	0.317***	0.00437***	0.00468***
	(0.113)	(0.110)	(0.00112)	(0.00104)
Controls		✓		✓
Observations	84890	84295	81356	80789
R Squared	0.274	0.299	0.337	0.450
	=100 if small firm		=100 if recently hired	
Panel D. Small firm and new hiring				
Payroll tax rate	0.0250	0.0120	0.193*	0.149*
	(0.0815)	(0.0794)	(0.0984)	(0.0856)
Controls		✓		✓
Observations	76053	75498	84890	84295
R Squared	0.233	0.247	0.0693	0.168

Notes: Standard errors clustered at the sector-by-area level are reported in parentheses. All specifications include sector fixed effects, area fixed effects, and time fixed effects. * $p < 0.1$ ** $p < 0.05$ *** $p < 0.01$.

Table 3.4: Main effects of the tax hike

	=100 if informal		Hourly wage (log)	
	(1)	(2)	(3)	(4)
Panel A. Overall effect				
Payroll tax rate	0.177	0.229	0.00255	0.00252
	(0.161)	(0.198)	(0.00290)	(0.00292)
Controls		✓		✓
Observations	156176	156173	140736	140736
R Squared	0.0273	0.291	0.0978	0.438
Panel B. Separating short-run and long-run				
Payroll tax rate (short-run effect)	-0.0367	-0.00731	0.0000535	0.000523
	(0.135)	(0.146)	(0.00218)	(0.00225)
Payroll tax rate (long-run effect)	0.456**	0.537*	0.00576	0.00508
	(0.214)	(0.278)	(0.00410)	(0.00406)
Controls		✓		✓
Observations	156176	156173	140736	140736
R Squared	0.0275	0.291	0.0979	0.438
Panel C. Heterogeneity by firm size				
Payroll tax rate	-0.0636	-0.0462	0.00201	0.00191
	(0.198)	(0.216)	(0.00240)	(0.00262)
Small firm	36.78***	17.26***	-0.523***	-0.179***
	(3.146)	(2.079)	(0.0420)	(0.0253)
Payroll tax rate × Small firm	0.219	0.350***	0.00242	0.000871
	(0.138)	(0.122)	(0.00172)	(0.00114)
Controls		✓		✓
Observations	138113	138113	125825	125825
R Squared	0.191	0.347	0.201	0.456
Panel D. Heterogeneity by new worker				
Payroll tax rate	-0.130	-0.103	0.00335	0.00311
	(0.125)	(0.161)	(0.00290)	(0.00285)
Recently hired	34.08***	17.61***	-0.527***	-0.214***
	(2.925)	(2.368)	(0.0262)	(0.0227)
Payroll tax rate × Recently hired	0.527***	0.732***	0.00326***	0.000618
	(0.129)	(0.117)	(0.00111)	(0.00109)
Controls		✓		✓
Observations	156176	156173	140736	140736
R Squared	0.211	0.369	0.187	0.452
	=100 if small firm		=100 if recently hired	
Panel E. Small firm and new hiring				
Payroll tax rate	0.254*	0.362**	0.315**	0.310*
	(0.145)	(0.136)	(0.145)	(0.168)
Controls		✓		✓
Observations	138113	138113	156176	156173
R Squared	0.0276	0.238	0.00830	0.181

Notes: Standard errors clustered at the sector-by-area level are reported in parentheses. All specifications include area fixed effects and time fixed effects. * $p < 0.1$ ** $p < 0.05$ *** $p < 0.01$.

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Appendix A

Appendix of Pension Privatization, Behavioral Responses, and Income in Old Age: Evidence from a Cohort-Based Reform

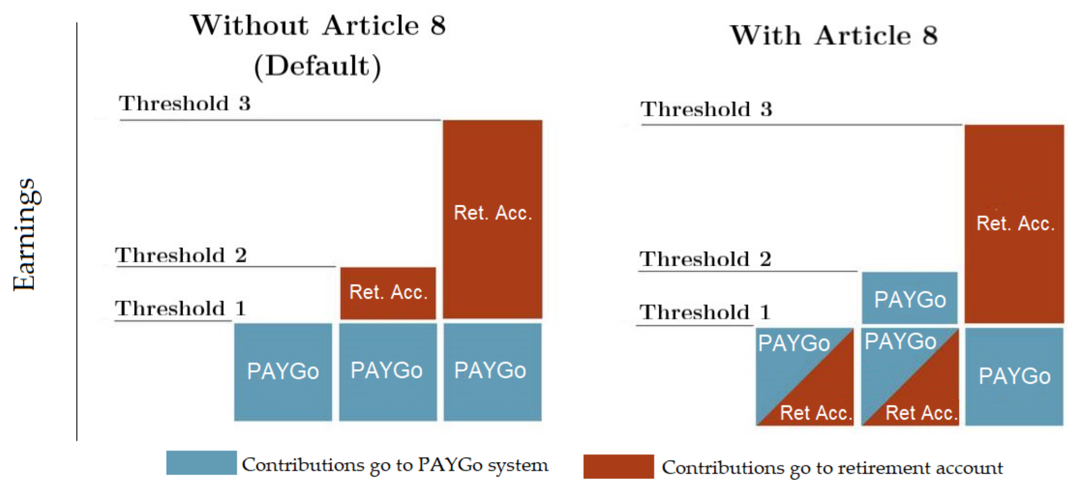
A.1 Additional pension system context

Figure A.1 shows the options on how to distribute contributions in the mixed system. The default option (without Article 8) is the option that workers are assigned by default. In it, contributions on earnings up until an earnings threshold (around the 70th percentile of the wage distribution) go entirely to the pay-as-you-go DB government system, while contributions on earnings above that threshold go entirely to the retirement account.

The alternative option (known as Article 8) allows workers whose earnings lie below the first threshold to contribute to their retirement account. For workers with earnings below threshold 1, their contributions are evenly divided between the unfunded DB system and the funded DC system. Workers whose earnings lie between thresholds 1 and 2 evenly divide contributions between the public and the private systems until threshold 1, while contributions on earnings above threshold 1 go entirely to the unfunded DB system. Finally, workers whose earnings exceed threshold 2 face the same contribution schedule as in the default option. No mandatory contributions are made on earnings above threshold 3, but workers can arrange with their employer to make those deductions and transfer them to their pension fund. The contribution rate is 15% of the pre-tax wage in all cases.

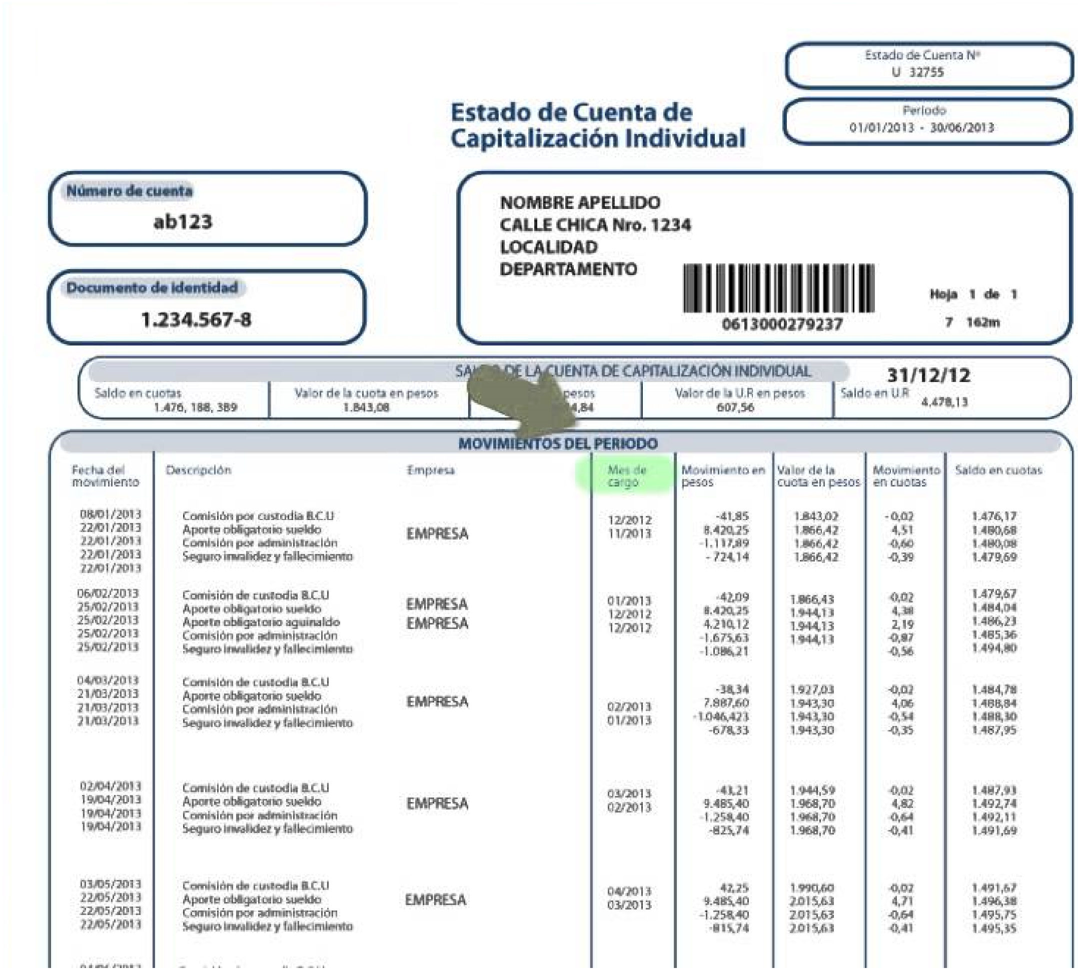
The Article 8 option implies a reduction in the government unfunded DB pension that workers will receive. This is implemented by reducing the "contributory salary" to which the replacement rate is applied by 25%. Note that this implies a subsidy for the Article 8 option: contributions to the public unfunded DB system fall by 50% but the pension received falls by 25%. This subsidy is phased-out such that the maximum government pension that a worker who chooses Article 8 can receive is the replacement rate applied to threshold 1. This is implemented by threshold 2 being set up such that a worker who chose Article 8 with earnings at threshold 2 makes the same government pension as a worker in the default option with earnings at threshold 1 and above.

Figure A.1: Options in two-pillar system



Notes: This figure shows the options on how to distribute social security contributions in the two-pillar system. Contributions on earnings indicated in blue go entirely to the unfunded DB government system. Contributions on earnings indicated in red go entirely to the worker's retirement account. The default option (without Article 8) is the option that workers are assigned by default. The alternative option (with Article 8) has to be actively chosen by the workers.

Figure A.2: Example of account summary



Estado de Cuenta N°
U 32755

Periodo
01/01/2013 - 30/06/2013

Estado de Cuenta de Capitalización Individual

Número de cuenta
ab123

Documento de identidad
1.234.567-8

NOMBRE APELLIDO
CALLE CHICA Nro. 1234
LOCALIDAD
DEPARTAMENTO

0613000279237

Hoja 1 de 1
7 162m

SALDO DE LA CUENTA DE CAPITALIZACIÓN INDIVIDUAL **31/12/12**

Saldo en cuotas	Valor de la cuota en pesos	pesos	Valor de la U.R en pesos	Saldo en U.R
1.476, 188, 389	1.843,08	4,84	607,56	4.478,13

MOVIMIENTOS DEL PERIODO

Fecha del movimiento	Descripción	Empresa	Mes de cargo	Movimiento en pesos	Valor de la cuota en pesos	Movimiento en cuotas	Saldo en cuotas
08/01/2013 22/01/2013 22/01/2013 22/01/2013 22/01/2013	Comisión por custodia B.C.U Aporte obligatorio sueldo Comisión por administración Seguro invalidez y fallecimiento	EMPRESA	12/2012 11/2013	-41,85 8.420,25 -1.117,89 - 724,14	1.843,02 1.866,42 1.866,42 1.866,42	- 0,02 4,51 -0,60 -0,39	1.476,17 1.480,68 1.480,08 1.479,69
06/02/2013 25/02/2013 25/02/2013 25/02/2013 25/02/2013	Comisión de custodia B.C.U Aporte obligatorio sueldo Aporte obligatorio aguinaldo Comisión por administración Seguro invalidez y fallecimiento	EMPRESA EMPRESA	01/2013 12/2012 12/2012	-42,09 8.420,25 4.210,12 -1.675,63 -1.086,21	1.866,43 1.944,13 1.944,13 1.944,13	-0,02 4,38 2,19 -0,87 -0,56	1.479,67 1.484,04 1.486,23 1.485,36 1.494,80
04/03/2013 21/03/2013 21/03/2013 21/03/2013	Comisión de custodia B.C.U Aporte obligatorio sueldo Comisión por administración Seguro invalidez y fallecimiento	EMPRESA	02/2013 01/2013	-38,34 7.887,60 -1.046,423 -678,33	1.927,03 1.943,30 1.943,30 1.943,30	-0,02 4,06 -0,54 -0,35	1.484,78 1.488,84 1.488,30 1.487,95
02/04/2013 19/04/2013 19/04/2013 19/04/2013	Comisión de custodia B.C.U Aporte obligatorio sueldo Comisión por administración Seguro invalidez y fallecimiento	EMPRESA	03/2013 02/2013	-43,21 9.485,40 -1.258,40 -825,74	1.944,59 1.968,70 1.968,70 1.968,70	-0,02 4,82 -0,64 -0,41	1.487,93 1.492,74 1.492,11 1.491,69
03/05/2013 22/05/2013 22/05/2013 22/05/2013	Comisión de custodia B.C.U Aporte obligatorio sueldo Comisión por administración Seguro invalidez y fallecimiento	EMPRESA	04/2013 03/2013	42,25 9.485,40 -1.258,40 -815,74	1.990,60 2.015,63 2.015,63 2.015,63	-0,02 4,71 -0,64 -0,41	1.491,67 1.496,38 1.495,75 1.495,35

Notes: This figure shows an example of the retirement account summary that workers in the mixed system receive periodically. The document indicates the type of activity and the date in which it occurred. The types of account activities displayed are: (i) the mandatory contributions for a given month, (ii) the commission charged by the pension fund, (iii) a commission charged by the Central Bank (who regulates the pension funds), and (iv) a fee for disability and death insurance pension funds have to purchase for all workers.

A.2 Additional tables

Table A.1: Informality by sector

	(1) Proportion informal	(2) Proportion underreports	(3) Informality index
Panel A. Low informality sectors			
Education	0.0975	0.0430	-1.629
Financial services	0.0605	0.0591	-1.453
Social and Health services	0.137	0.0492	-1.369
Professional services	0.224	0.0557	-0.935
Water and sewage	0.0296	0.105	-0.678
Information and communication	0.189	0.0790	-0.613
Real Estate	0.233	0.0807	-0.426
Arts and entertainment	0.372	0.0571	-0.385
Mining	0.343	0.0672	-0.293
Electricity and gas	0.0303	0.126	-0.270
Panel B. High informality sectors			
Agriculture	0.312	0.124	0.686
Commerce	0.504	0.0961	0.831
Administrative support services	0.417	0.113	0.845
Hotels and Restaurants	0.416	0.135	1.259
Construction	0.607	0.106	1.395
Other services	0.678	0.120	1.909
Home services	0.635	0.199	3.265

Notes: This table reports measures of informality for each sector constructed using household surveys. Column 1 reports the proportion of workers that report being informal. Column 2 reports the proportion of formal workers that admit to underreporting their income for their contributions. Column 3 reports an index constructed as the first component of a principal component analysis of the proportion of informal workers and the proportion of workers who underreport their earnings by sector. The sample corresponds to surveys conducted in the year 2006 (the first year in which the underreporting question was included in the questionnaire).

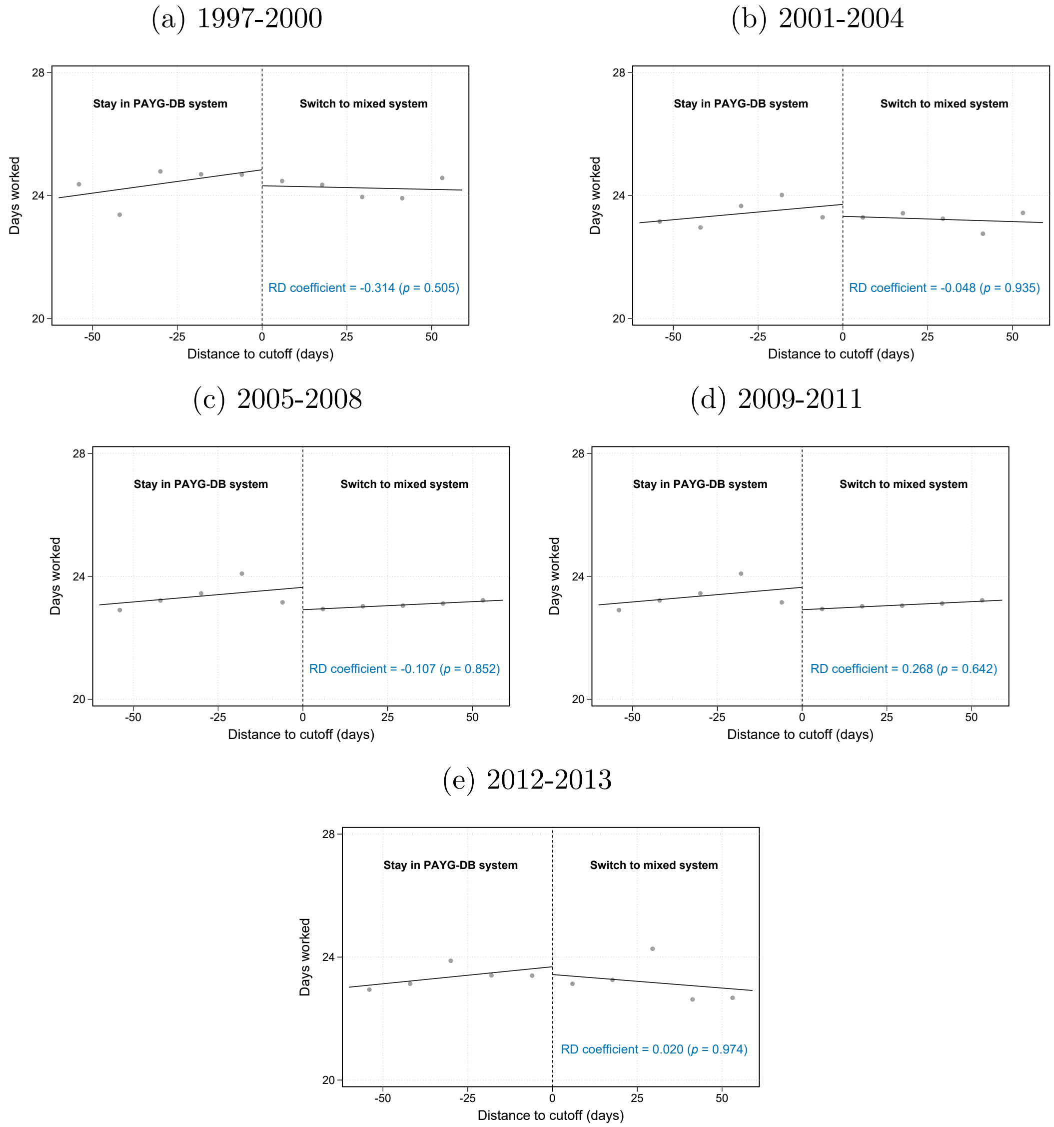
Table A.2: Balance - Retirement accounts data

Variable	(1) Not allowed to reverse	(2) Allowed to reverse	(3) Difference
Female	0.560	0.541	-0.019
	(0.497)	(0.499)	(0.026)
Foreign born	0.048	0.055	0.007
	(0.214)	(0.229)	(0.011)
Public sector	0.244	0.247	0.003
	(0.430)	(0.432)	(0.022)
Article 8	0.909	0.891	-0.019
	(0.287)	(0.312)	(0.015)
Active March 2017	0.829	0.802	-0.027
	(0.377)	(0.399)	(0.020)
Retirement fund (Dec 2017)	776.451	817.060	40.609
	(1,129.525)	(1,168.207)	(59.279)
Observations	729	777	1,506

Notes: This table shows the balance on pre-reform characteristics from equation across the reversal groups. Female is an indicator for equal to 1 if the individual reported being female. Foreign born is an indicator equal to 1 if the worker was born outside of Uruguay. Public sector is a dummy variable equal to 1 if the worker primarily contributed as a public-sector worker. Article 8 is a dummy variable equal to 1 if the worker opted for the Article 8 option. Active March 2017 is a dummy variable equal to 1 if the retirement account was not closed by March of 2017. Retirement fund (Dec 2017) is the total retirement fund accumulated by December of 2017 in thousands of current Uruguayan Pesos. * Significant at the 10% level ** Significant at the 5% level *** Significant at the 1% level.

A.3 Additional figures

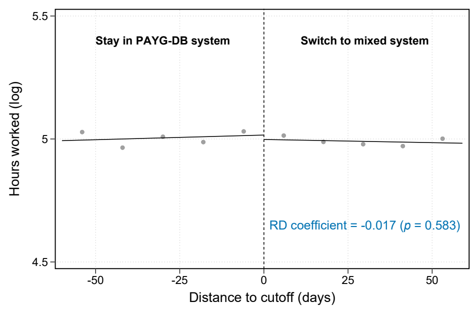
Figure A.3: Effect of the reform on days worked - SSA data



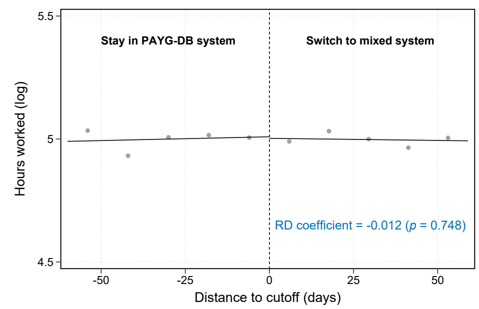
Notes: This figure shows RD-plots for the average days worked from equation 1.1. Individuals born before the cutoff were left by default in the pay-as-you-go system with defined benefits and individuals born at the cutoff or after were switched to the mixed system with retirement accounts. Each panel corresponds to a different group of years. In all panels, the dependent variable is number of days worked. RD coefficients are estimated using a 11-day window around the cutoff date of birth and p-values are calculated using clustered standard errors at the worker level. The dependent variable is residualized from year fixed effects and evaluated at the mean.

Figure A.4: Effect of the reform on hours worked - SSA data

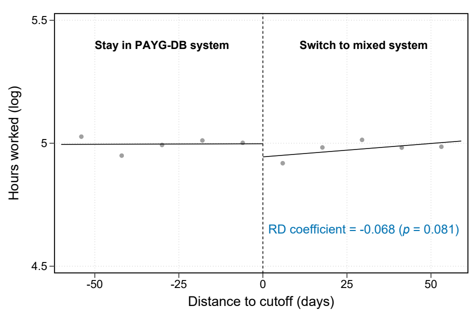
(a) 1997-2000



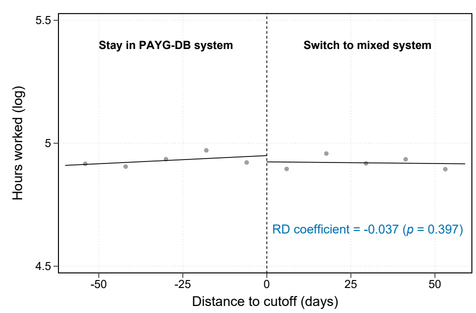
(b) 2001-2004



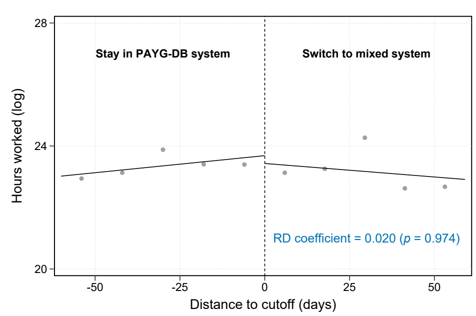
(c) 2005-2008



(d) 2009-2011



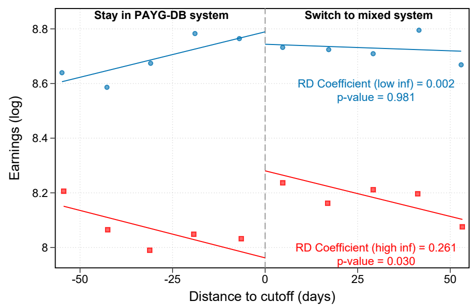
(e) 2012-2013



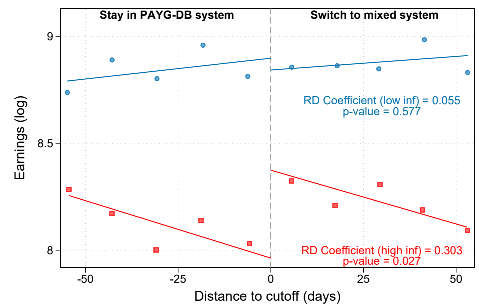
Notes: This figure shows RD-plots for the average monthly hours worked from equation 1.1. Individuals born before the cutoff were left by default in the pay-as-you-go system with defined benefits and individuals born at the cutoff or after were switched to the mixed system with retirement accounts. Each panel corresponds to a different group of years. In all panels, the dependent variable is natural logarithm of the total monthly hours worked. RD coefficients are estimated using a 11-day window around the cutoff date of birth and p-values are calculated using clustered standard errors at the worker level. The dependent variable is residualized from year fixed effects and evaluated at the mean.

Figure A.5: Effect of the reform on earnings (heterogeneity by sector-level informality and evasion) - SSA data

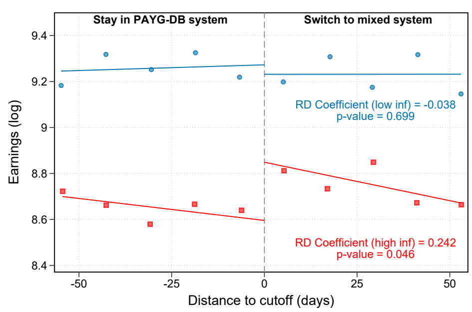
(a) 1997-2000



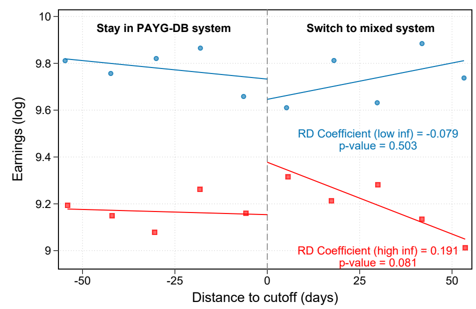
(b) 2001-2004



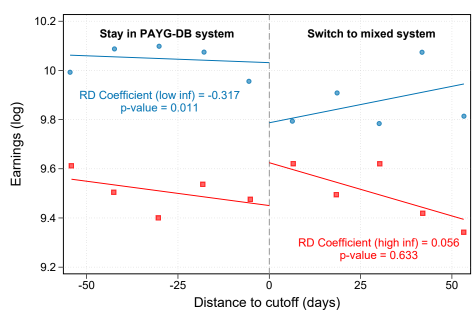
(c) 2005-2008



(d) 2009-2011



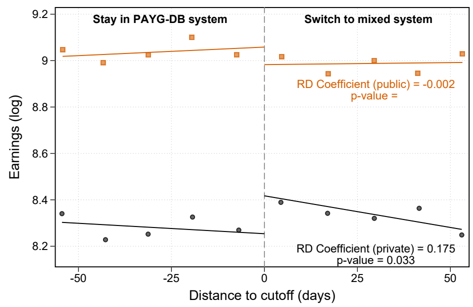
(e) 2012-2013



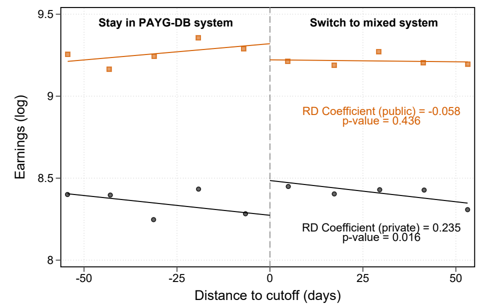
Notes: This figure shows RD-plots for the total labor earnings from equation 1.1. Individuals born before the cutoff were left by default in the pay-as-you-go system with defined benefits and individuals born at the cutoff or after were switched to the mixed system with retirement accounts. Each panel corresponds to a different group of years. In all panels, the dependent variable is natural logarithm of total reported labor earnings. RD coefficients are estimated using a 11-day window around the cutoff date of birth and p-values are calculated using clustered standard errors at the worker level. Red indicates high informality and underreporting sectors and blue indicates low informality and underreporting sectors. The dependent variable is residualized from year fixed effects and evaluated at the mean.

Figure A.6: Effect of the reform on earnings (heterogeneity by public and private sector) - SSA data

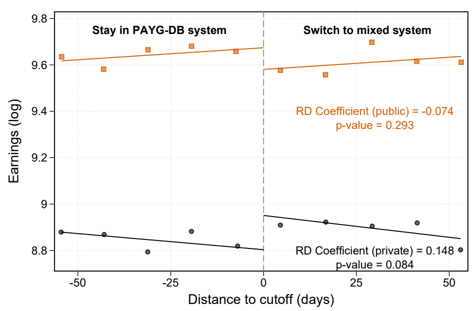
(a) 1997-2000



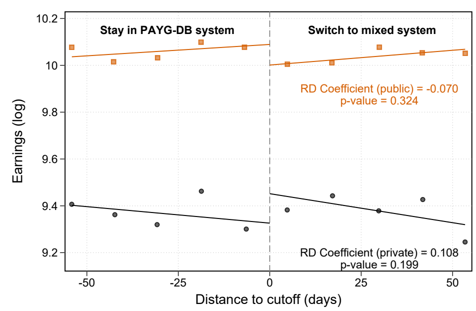
(b) 2001-2004



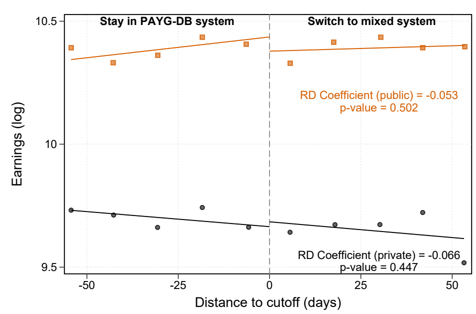
(c) 2005-2008



(d) 2009-2011



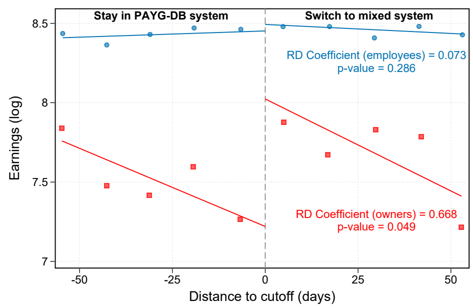
(e) 2012-2013



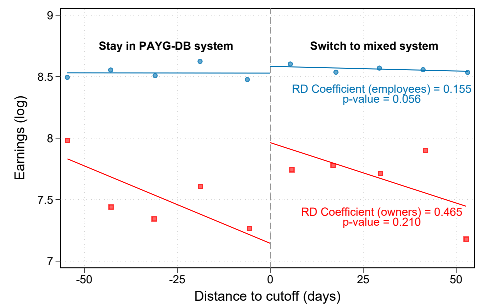
Notes: This figure shows RD-plots for the total labor earnings from equation 1.1. Individuals born before the cutoff were left by default in the pay-as-you-go system with defined benefits and individuals born at the cutoff or after were switched to the mixed system with retirement accounts. Each panel corresponds to a different group of years. In all panels, the dependent variable is natural logarithm of total reported labor earnings. RD coefficients are estimated using a 11-day window around the cutoff date of birth and p-values are calculated using clustered standard errors at the worker level. Red indicates high informality and underreporting sectors and blue indicates low informality and underreporting sectors. The dependent variable is residualized from year fixed effects and evaluated at the mean.

Figure A.7: Effect of the reform on earnings (heterogeneity by ownership) - SSA data

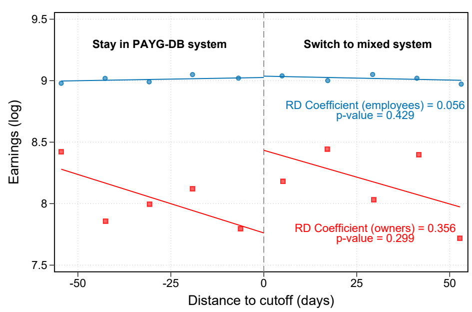
(a) 1997-2000



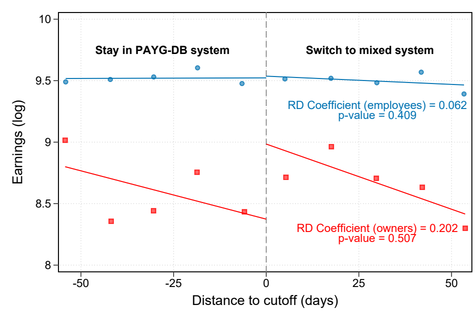
(b) 2001-2004



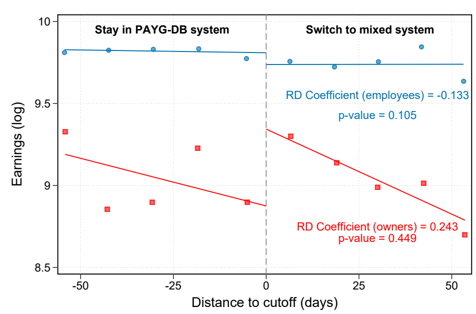
(c) 2005-2008



(d) 2009-2011

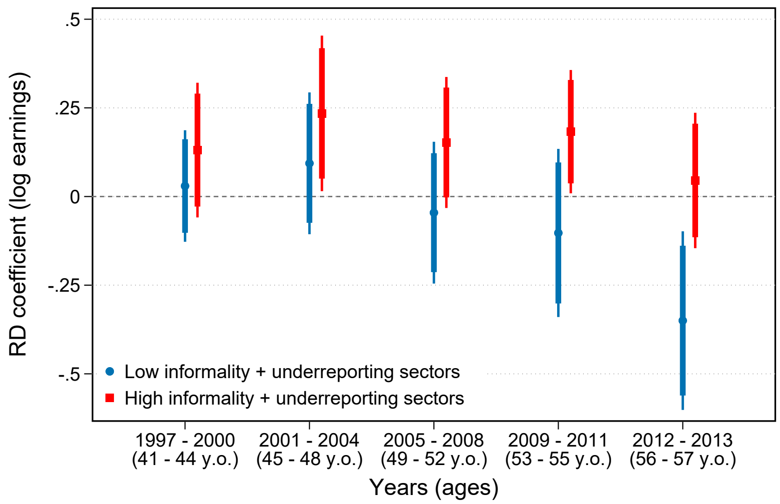


(e) 2012-2013



Notes: This figure shows RD-plots for the total labor earnings from equation 1.1. Individuals born before the cutoff were left by default in the pay-as-you-go system with defined benefits and individuals born at the cutoff or after were switched to the mixed system with retirement accounts. Each panel corresponds to a different group of years. In all panels, the dependent variable is natural logarithm of total reported labor earnings. RD coefficients are estimated using a 11-day window around the cutoff date of birth and p-values are calculated using clustered standard errors at the worker level. Red indicates high informality and underreporting sectors and blue indicates low informality and underreporting sectors. The dependent variable is residualized from year fixed effects and evaluated at the mean.

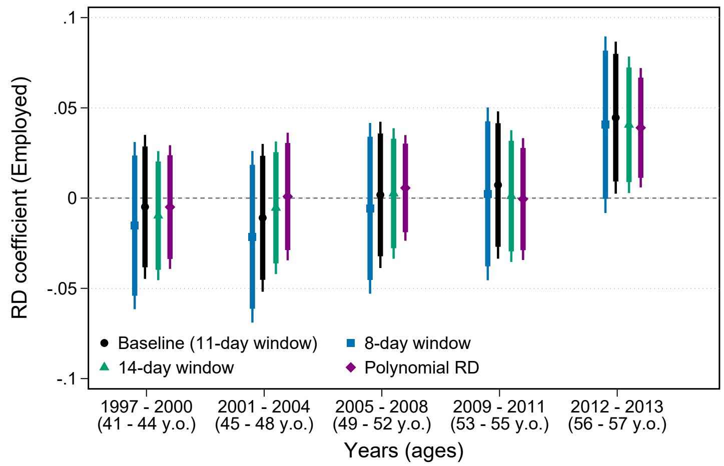
Figure A.8: Time series plot of RD coefficients - Earnings by sector level informality (SSA data - employees only)



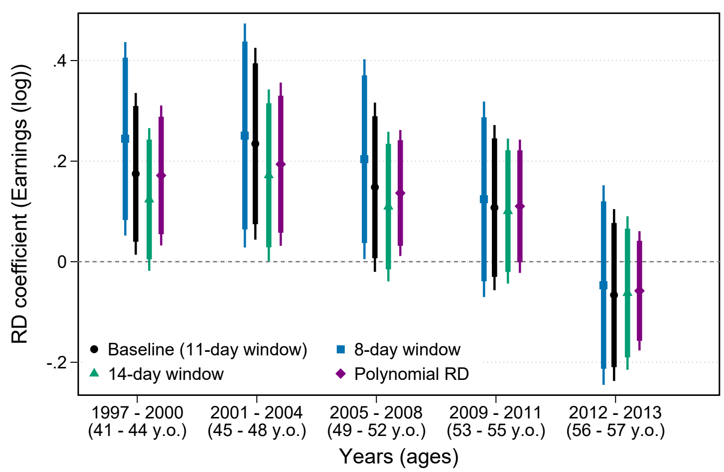
Notes: This figure shows a time series plot for the RD coefficients for each group of years. The dependent variable is the natural logarithm of labor earnings. The numbers underneath the years indicate the ages of workers in those years. Thick vertical bars correspond to 90% confidence intervals and thin vertical bars correspond to 95% confidence intervals, both from worker-level cluster-robust inference. Blue corresponds to estimates for employees in low informality and underreporting sectors. Red corresponds to estimates for employees in high informality and underreporting sectors. The sample includes only employees.

Figure A.9: Time series plot of RD coefficients - different specifications (SSA data)

(a) Employment rates

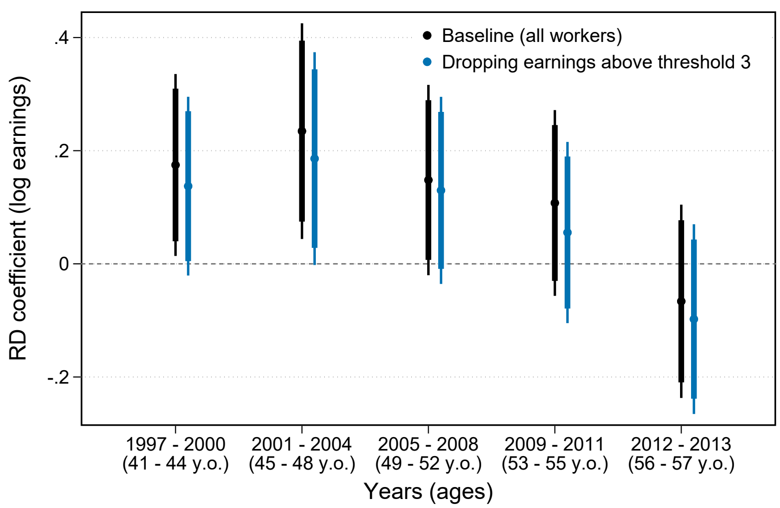


(b) Earnings



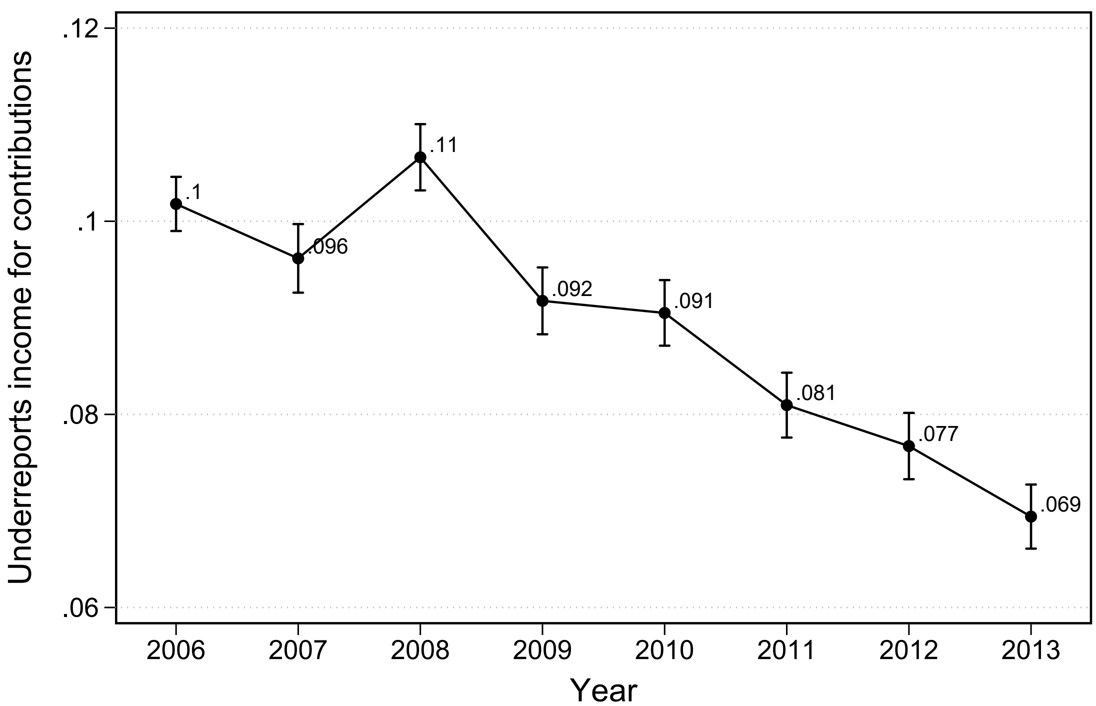
Notes: This figure shows a time series plot for the RD coefficients for different specifications. Panel (a) shows coefficients for the effect on the probability of being employed and panel (b) for the effect on the natural logarithm of monthly earnings. The numbers underneath the years indicate the ages of workers in those years. Black corresponds to estimates using our baseline window of 11 days around the cutoff. Green corresponds to estimates calculated using an alternative window of 14 days around the cutoff. Blue corresponds to estimates calculated using an alternative window of 8 days around the cutoff. Purple corresponds to estimates calculated fitting a quadratic polynomial with the continuity-based approach, using a triangular kernel, and an optimal bandwidth following Calonico, Cattaneo, and Titiunik, 2014. Thick vertical bars correspond to 90% confidence intervals and thin vertical bars correspond to 95% confidence intervals, both from worker-level cluster-robust inference.

Figure A.10: Time series plot of RD coefficients - Earnings dropping earnings above ceiling (SSA data)



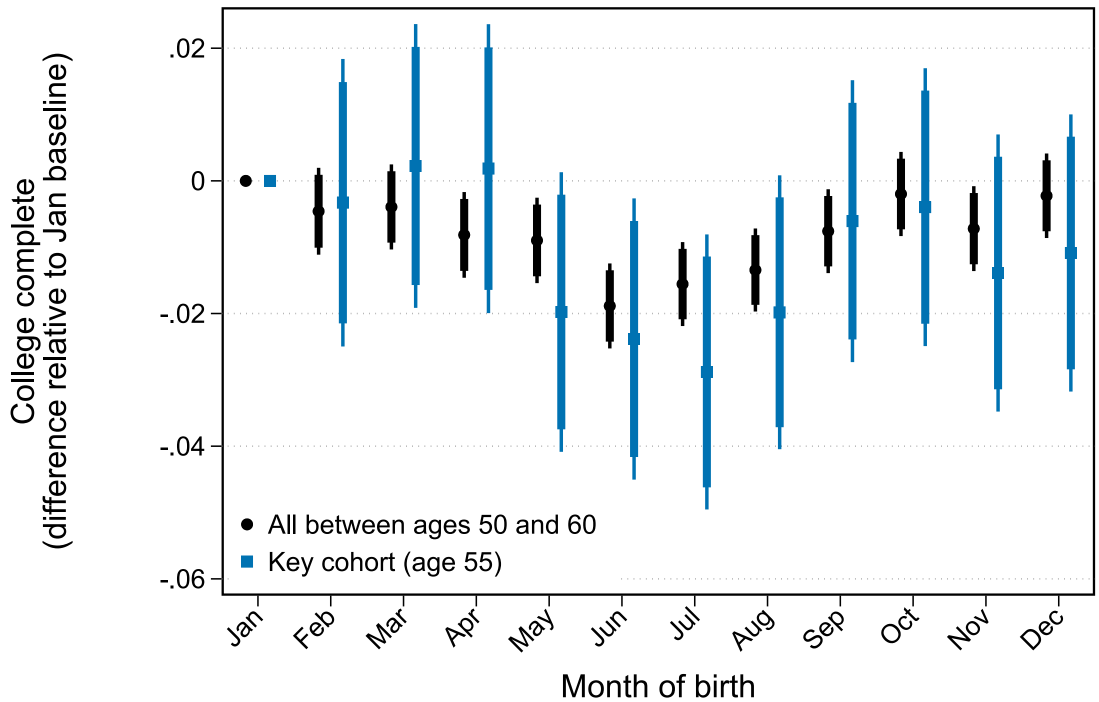
Notes: This figure shows a time series plot for the RD coefficients for each group of years. The dependent variable is the natural logarithm of labor earnings. The numbers underneath the years indicate the ages of workers in those years. Thick vertical bars correspond to 90% confidence intervals and thin vertical bars correspond to 95% confidence intervals, both from worker-level cluster-robust inference. Black corresponds to estimates for the cohort using all workers. Blue corresponds to estimates using only workers with earnings below the ceiling.

Figure A.11: Earnings underreporting over time (HH Surveys)



Notes: This figure shows the proportion of formal workers that admit to underreporting their labor earnings for their social security contributions in labor-market household surveys for each year. We define as underreporting workers who answer “no” to the following question “do you contribute to social security based on the totality of your labor earnings?”. Vertical bars represent 95% confidence intervals.

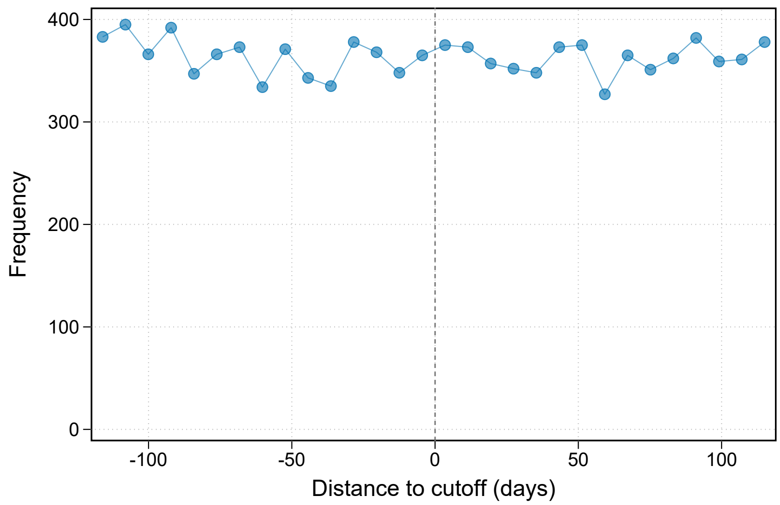
Figure A.12: College completion rates by month of birth



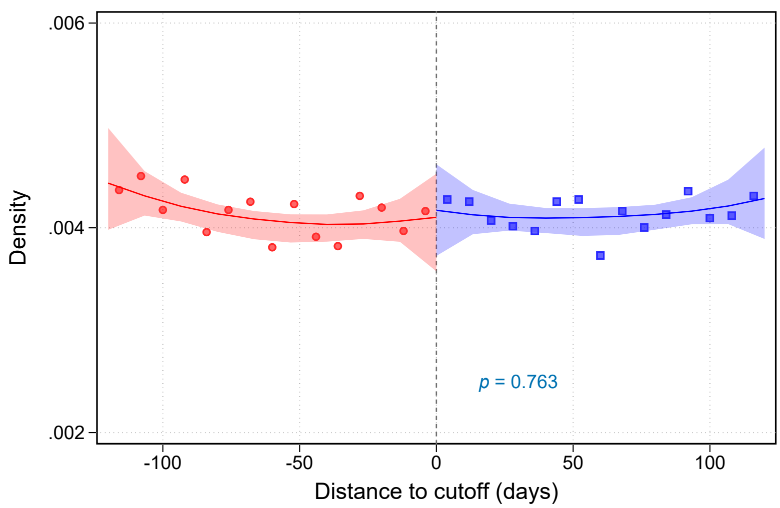
Notes: This figure shows the differences in college completion rates in the 2011 census according to the month of birth, relative to the baseline of a January date of birth. Black corresponds to coefficients estimated using all individuals between the ages of 50 and 60. Blue corresponds to estimates calculated using only workers who are 55 years of age at the time of the census (that means that they were born in 1956, the year of the cohort-based discontinuity). Thick vertical bars represent 90% confidence intervals and thin vertical bars represent 95% confidence intervals.

Figure A.13: Density around the cutoff and manipulation test (IRS data)

(a) Density around the cutoff



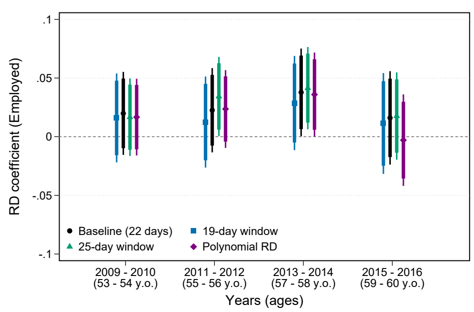
(b) Manipulation test



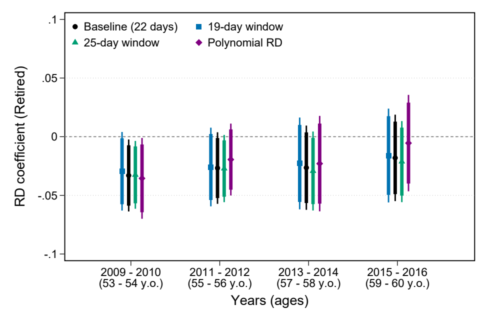
Notes: This figure shows the density of observations around the cutoff and a manipulation test for the running variable using IRS data. Individuals born before the cutoff were left by default in the pay-as-you-go system with defined benefits and individuals born at the cutoff or after were switched to the mixed system with retirement accounts. Panel (a) shows a frequency histogram of the number of observations in 30 equally-spaced bins. Panel (b) shows a manipulation testing plot and a *p*-value for manipulation of the running variable based on local polynomials from (Cattaneo, Jansson, and Ma, 2020) using the `rddensity` routine from (Cattaneo, Jansson, and Ma, 2018).

Figure A.14: Time series plot of RD coefficients - comparison with other specifications (IRS data)

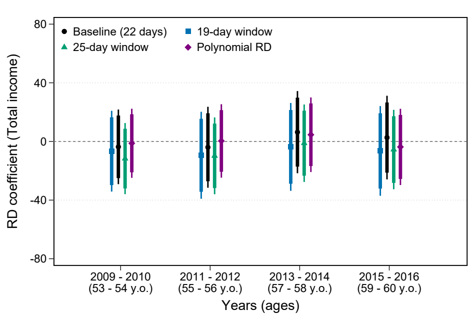
(a) Employed



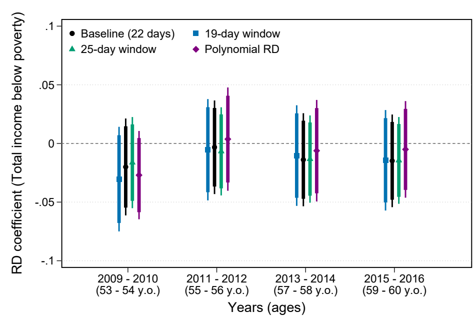
(b) Retired



(c) Total income

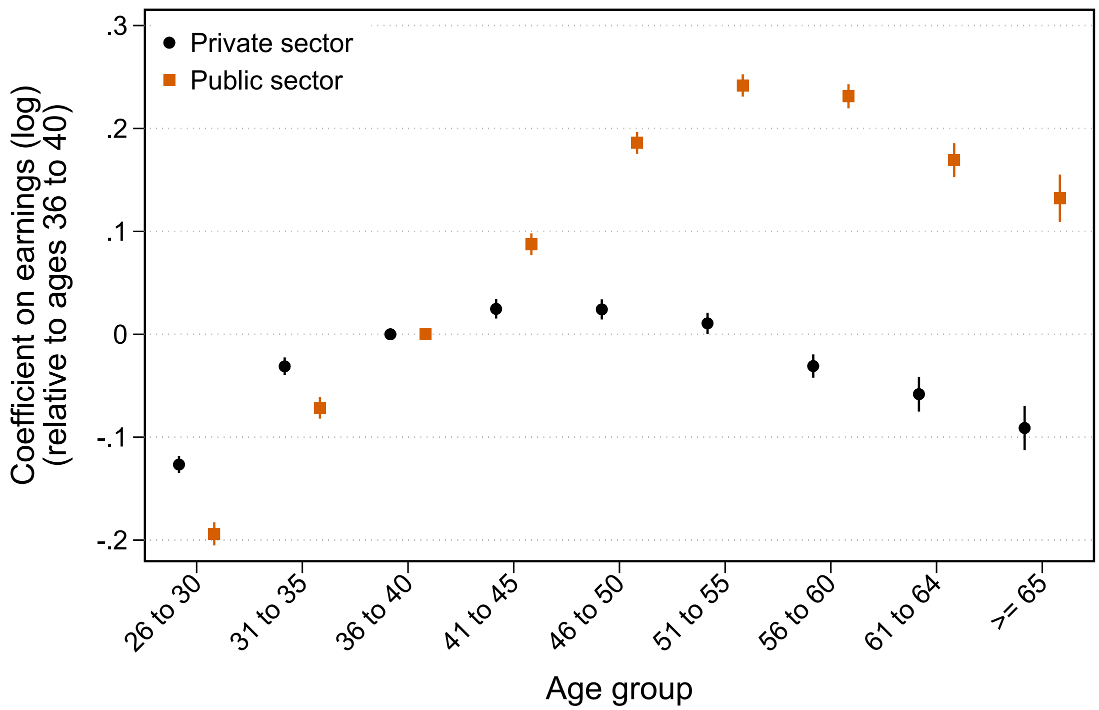


(d) Below poverty line



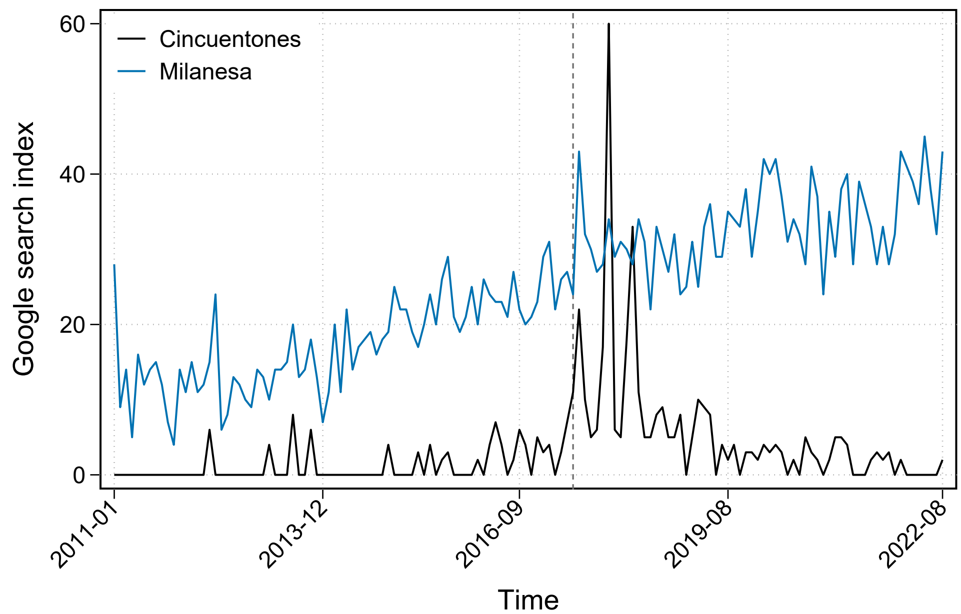
Notes: This figure shows a time series plot for the RD coefficients for each group of years using different specifications. Panel (a) shows coefficients for the effect on the probability of being employed. Panel (b) shows coefficients for the effect on the probability of being retired. Panel (c) shows coefficients for the effect on total income, measured as pension income plus labor earnings. Panel (d) shows coefficients for the effect on the probability of the total income being below the poverty line. The numbers underneath the years indicate the ages of workers in those years. Black corresponds to estimates using the baseline window of 22 days. Blue corresponds to estimates using a window of 19 days. Green corresponds to estimates using a window of 25 days. Purple corresponds to estimates calculated fitting a quadratic polynomial with the continuity-based approach, using a triangular kernel, and an optimal bandwidth following Calonico, Cattaneo, and Titiunik, 2014. Thick vertical bars correspond to 90% confidence intervals and thin vertical bars correspond to 95% confidence intervals, both from worker-level cluster-robust inference.

Figure A.15: Age-earnings profiles by public and private sector (HH Surveys)



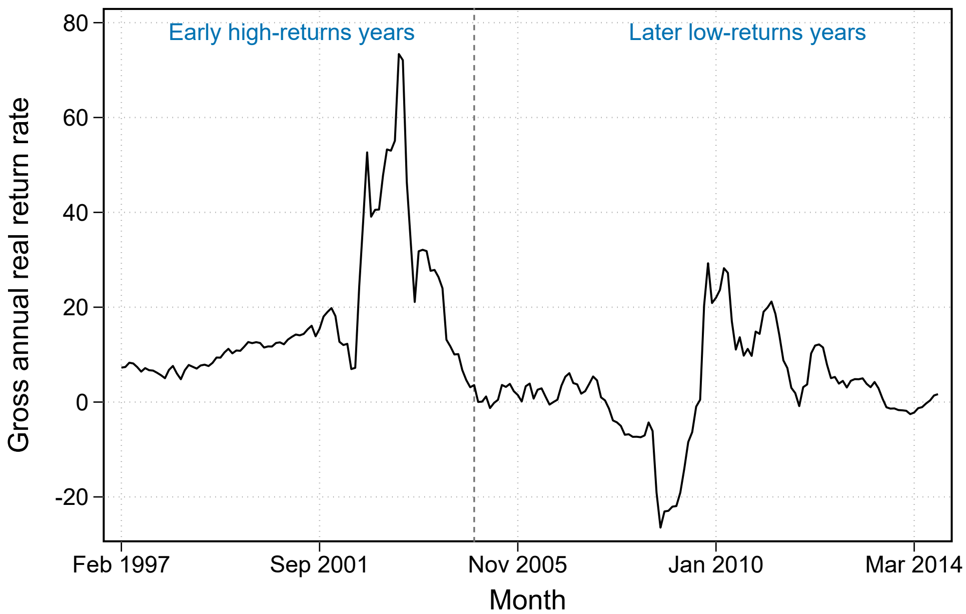
Notes: This figure shows the estimated age-earnings profiles for workers in the private sector and in the public sector for workers of at least 26 years of age. Coefficients for the private sector are shown in black and for the public sector are shown in orange. The dependent variable is the natural logarithm of total labor earnings. Each point represents the OLS coefficient of each age group dummy variable, relative to the omitted category of 36 to 40 years old. Estimates are calculated using household surveys from 2006 to 2019 and include year fixed effects. Vertical bars represent 95% confidence intervals.

Figure A.16: Google trends - "Cincuentones" and "Milanesa"



Notes: This figure shows Google Trends search indices for the terms "Cincuentones" (how the law came to be known) and "Milanesa" (which is a popular traditional food in Uruguay). The dashed line indicates the moment the reform started being debated in Congress.

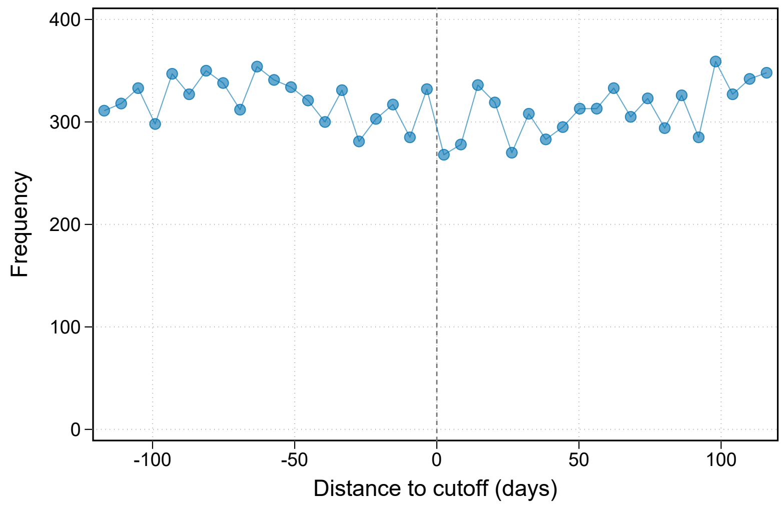
Figure A.17: Gross real annual interest rate on pension funds



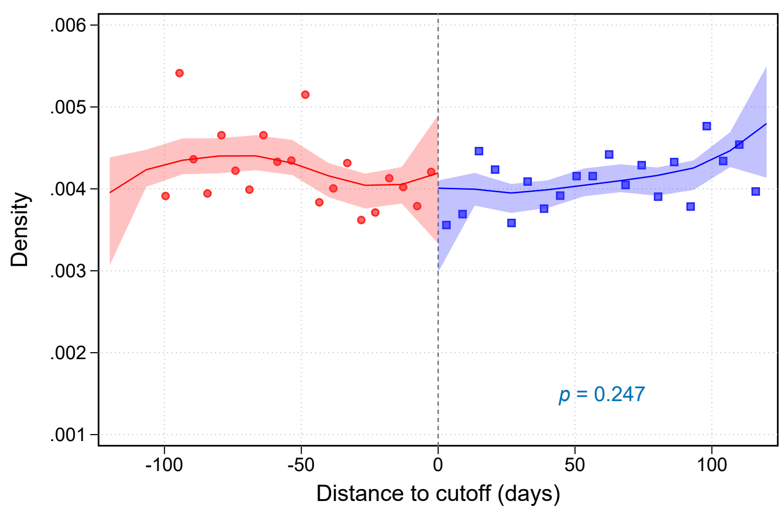
Notes: This figure shows the average gross annual rate of return on the pension funds over time. Time periods prior to late 2004 are indicated as early years with high interest rates. Time periods after are indicated as later low return years. The spike in the 2002-2003 period reflects the 2002 financial crisis.

Figure A.18: Density around the cutoff and manipulation test (retirement accounts data)

(a) Density around the cutoff

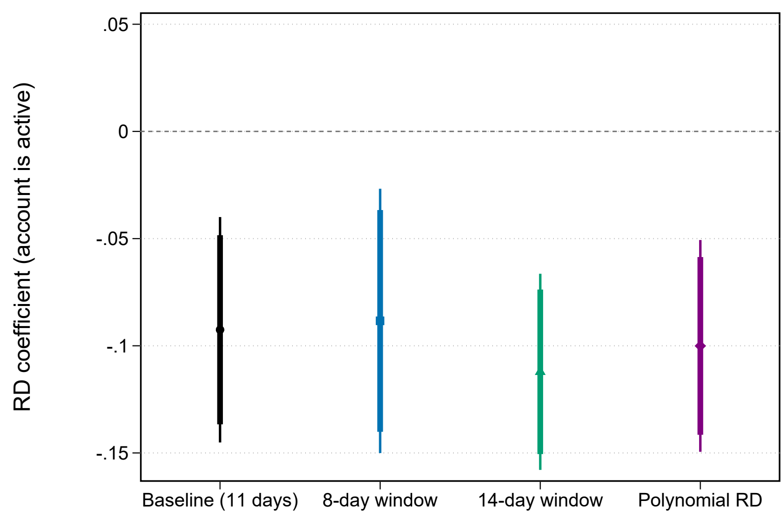


(b) Manipulation test



Notes: This figure shows the density of observations around the cutoff and a manipulation test for the running variable using the retirement accounts data. Individuals born before the cutoff were allowed to reverse to the pay-as-you-go system with defined benefits and individuals born at the cutoff or after were not allowed to reverse. Panel (a) shows a frequency histogram of the number of observations in 40 equally-spaced bins. Panel (b) shows a manipulation testing plot and a p-value for manipulation of the running variable based on local polynomials from (Cattaneo, Jansson, and Ma, 2020) using the `rddensity` routine from (Cattaneo, Jansson, and Ma, 2018).

Figure A.19: RD coefficients from several specifications (account is active - retirement accounts data)



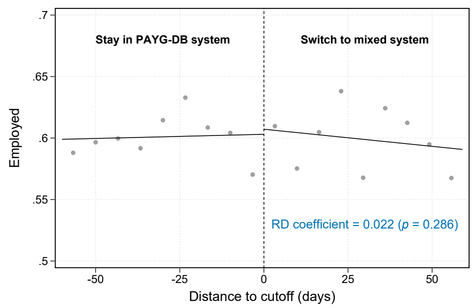
Notes: This figure shows the RD coefficient for the effect of the reversal policy on whether the account is active by March of 2019 using different specifications. The baseline coefficient is calculated using an 11-day window around the cutoff. Blue corresponds to an alternative estimate using an 8-day window and green corresponds to a 14-day window. Purple corresponds to estimates calculated fitting a quadratic polynomial with the continuity-based approach, using a triangular kernel, and an optimal bandwidth following Calonico, Cattaneo, and Titiunik, 2014. Thick vertical bars correspond to 90% confidence intervals and thin vertical bars correspond to 95% confidence intervals.

A.4 Placebo RD plots

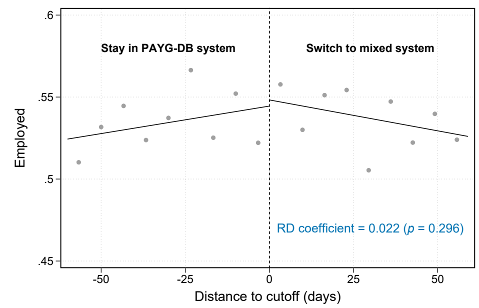
Born on year before

Figure A.20: Placebo RD plots for employment rates (year before) - SSA data

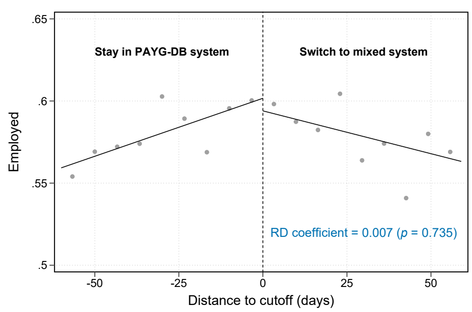
(a) 1997-2000



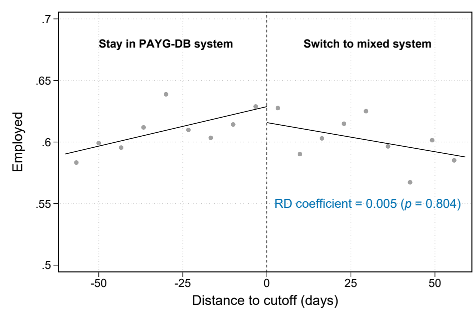
(b) 2001-2004



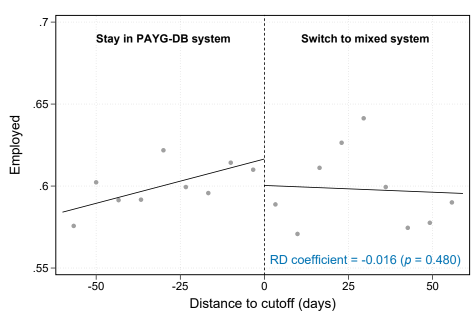
(c) 2005-2008



(d) 2009-2011



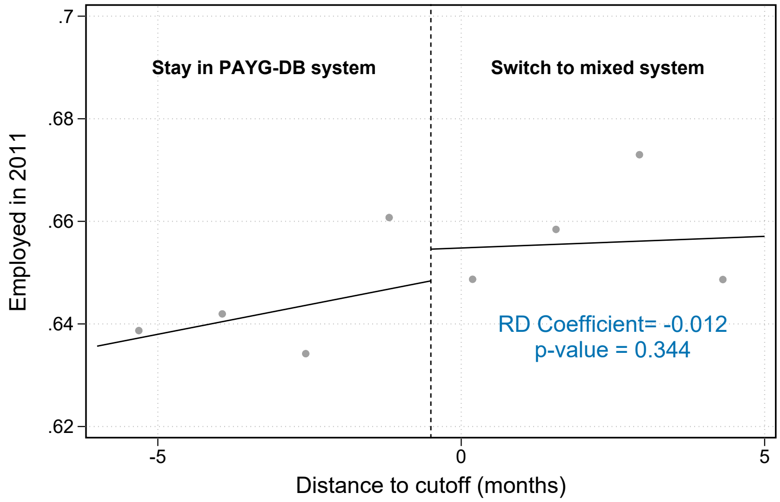
(e) 2012-2013



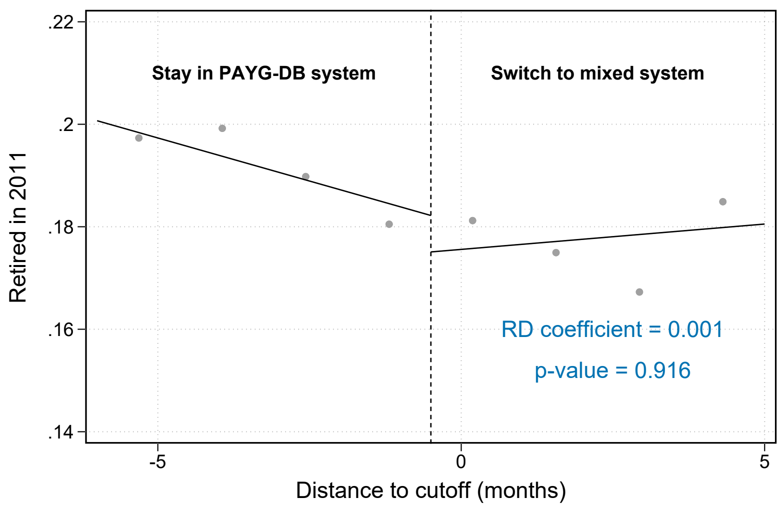
Notes: This figure shows placebo RD-plots for the probability of being employed from equation 1.1, using social security data for workers born in the year before the cohort affected by the reform. Each panel corresponds to a different group of years. In all panels, the dependent variable is an indicator of whether the worker was employed (defined as reporting positive earnings). RD coefficients are estimated using a 11-day window around the cutoff date of birth and *p*-values are calculated using clustered standard errors at the worker level. The dependent variable is residualized from year fixed effects and evaluated at the mean.

Figure A.21: Placebo RD plots (year before) - Census data

(a) Employment rates



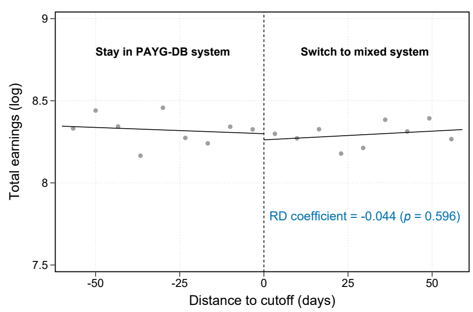
(b) Retirement rates



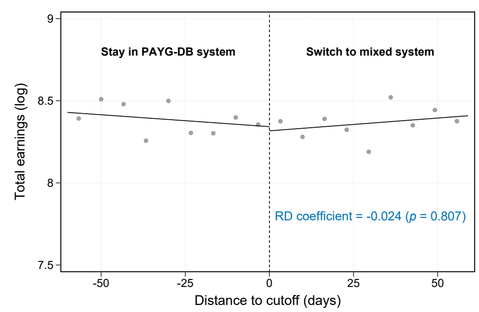
Notes: This figure shows the placebo RD plots for the probability of being employed and the probability of being retired using census data for individuals born on the year before the cohort affected by the reform. Panel (a) reports the effect for the probability of being employed. Panel (b) for the probability of being retired.

Figure A.22: Placebo RD plots for earnings (year before) - SSA data

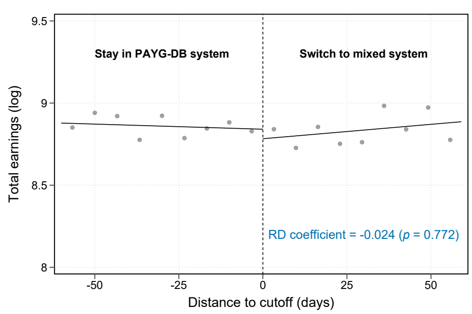
(a) 1997-2000



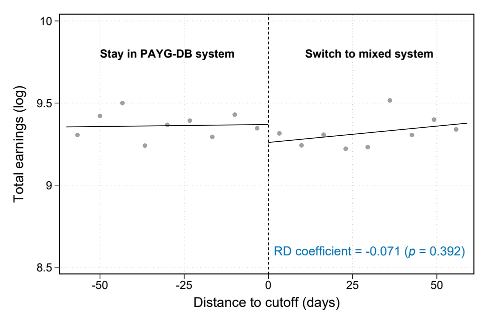
(b) 2001-2004



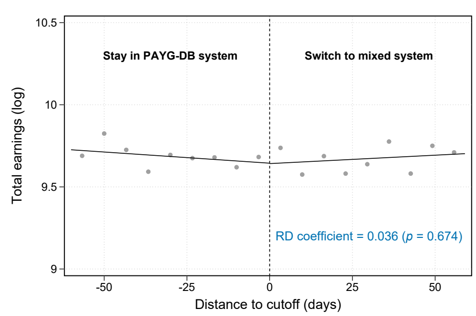
(c) 2005-2008



(d) 2009-2011



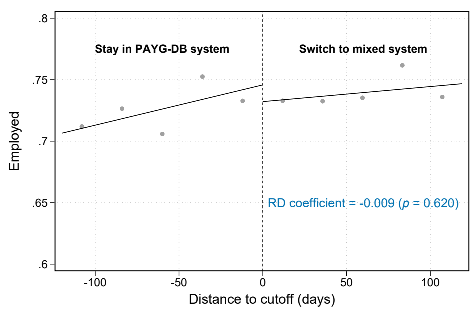
(e) 2012-2013



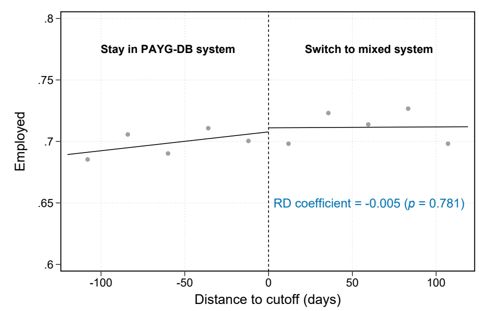
Notes: This figure shows placebo RD-plots for labor earnings from equation 1.1, using social security data for workers born in the year before the cohort affected by the reform. Each panel corresponds to a different group of years. In all panels, the dependent variable is the natural logarithm of total labor earnings. RD coefficients are estimated using a 11-day window around the cutoff date of birth and p-values are calculated using clustered standard errors at the worker level. The dependent variable is residualized from year fixed effects and evaluated at the mean.

Figure A.23: Placebo RD plots for employment rates (year before) - IRS data

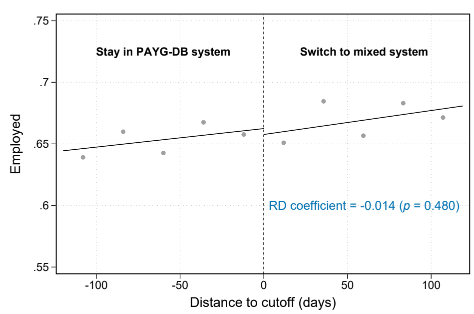
(a) 2009-2010



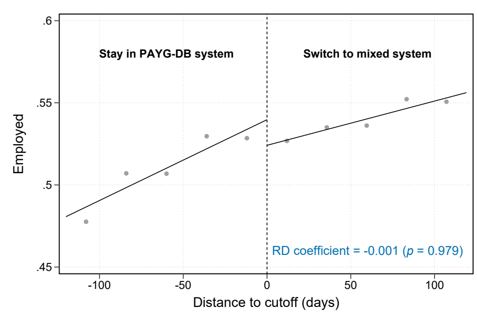
(b) 2011-2012



(c) 2013-2014



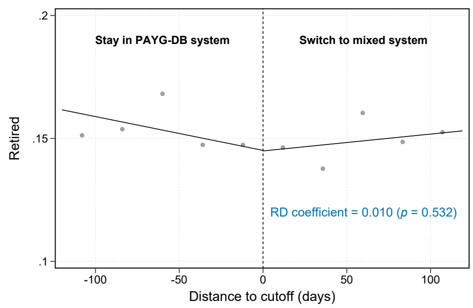
(d) 2015-2016



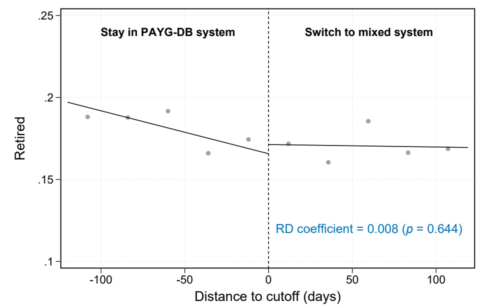
Notes: This figure shows placebo RD-plots for the probability of being employed from equation 1.1, using IRS data for workers born on the year before the cohort affected by the reform. Each panel corresponds to a different group of year. In all panels, the dependent variable is an indicator of whether the worker was employed (defined as reporting positive labor earnings). The dependent variable is residualized from year fixed effects and evaluated at the mean. RD coefficients are estimated using a 22-day window around the cutoff date of birth and p-values are calculated using clustered standard errors at the worker level.

Figure A.24: Placebo RD plots for retirement rates (year before) - IRS data

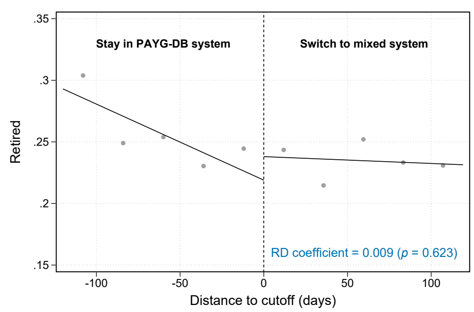
(a) 2009-2010



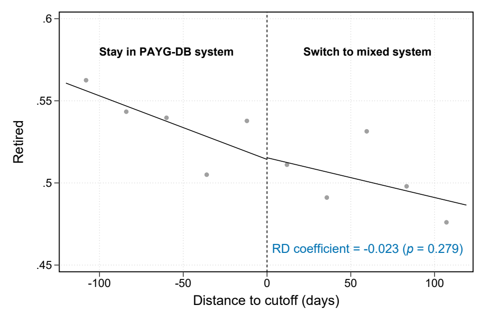
(b) 2011-2012



(c) 2013-2014



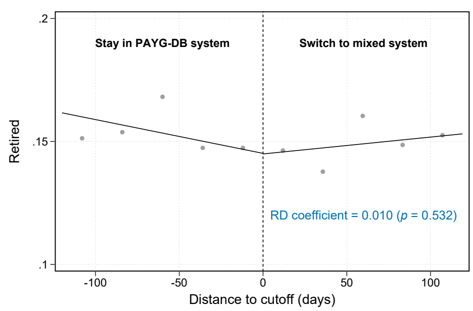
(d) 2015-2016



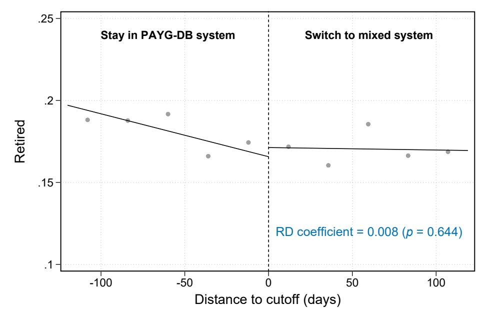
Notes: This figure shows placebo RD-plots for the probability of being retired from equation 1.1, using IRS data for workers born on the year before the cohort affected by the reform. Each panel corresponds to a different group of year. In all panels, the dependent variable is an indicator of whether the worker was retired (defined as reporting positive pension income). The dependent variable is residualized from year fixed effects and evaluated at the mean. RD coefficients are estimated using a 22-day window around the cutoff date of birth and p-values are calculated using clustered standard errors at the worker level.

Figure A.25: Placebo RD plots for total income (year before) - IRS data

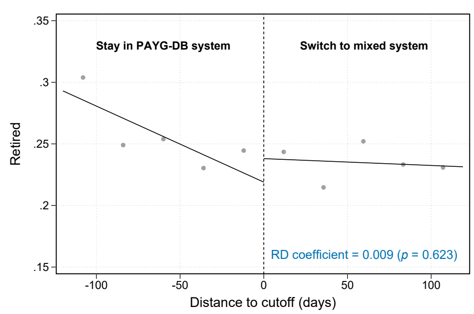
(a) 2009-2010



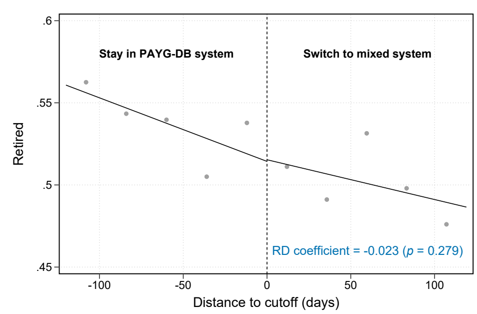
(b) 2011-2012



(c) 2013-2014



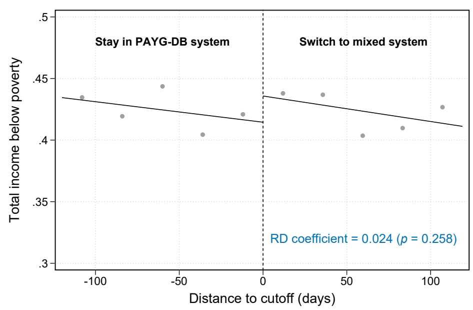
(d) 2015-2016



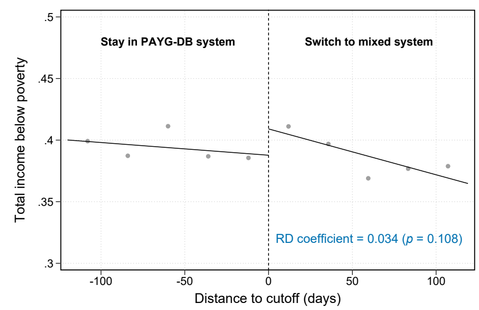
Notes: This figure shows placebo RD-plots for the total income from equation 1.1, using IRS data for workers born on the year before the cohort affected by the reform. Each panel corresponds to a different group of year. In all panels, the dependent variable is the total income (defined as pension plus earnings) measured in 2009 Uruguayan pesos. The dependent variable is residualized from year fixed effects and evaluated at the mean. RD coefficients are estimated using a 22-day window around the cutoff date of birth and p-values are calculated using clustered standard errors at the worker level.

Figure A.26: Placebo RD plots for total income below poverty line (year before) - IRS data

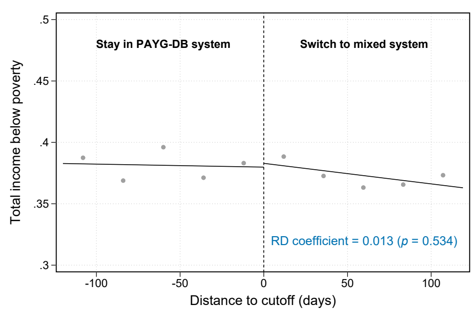
(a) 2009-2010



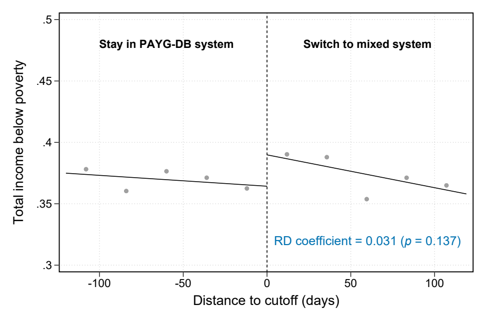
(b) 2011-2012



(c) 2013-2014



(d) 2015-2016

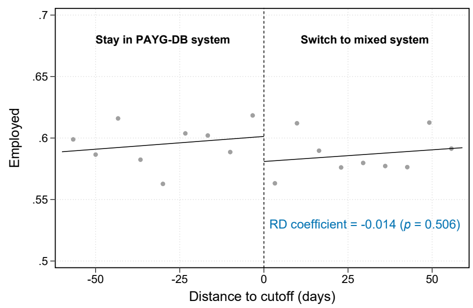


Notes: This figure shows placebo RD-plots for the probability of total income being below the poverty line from equation 1.1, using IRS data for workers born on the year before the cohort affected by the reform. Each panel corresponds to a different group of year. In all panels, the dependent variable is an indicator of whether the total income (defined as pension plus earnings) is below the National poverty line from Montevideo. The dependent variable is residualized from year fixed effects and evaluated at the mean. RD coefficients are estimated using a 22-day window around the cutoff date of birth and p-values are calculated using clustered standard errors at the worker level.

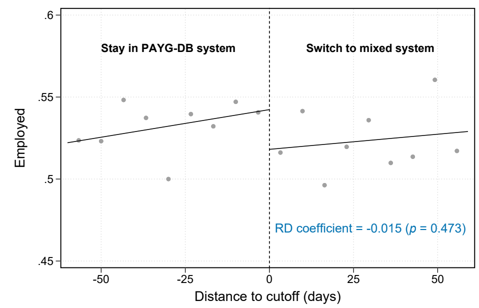
Born on year after

Figure A.27: Placebo RD plots for employment rates (year after) - SSA data

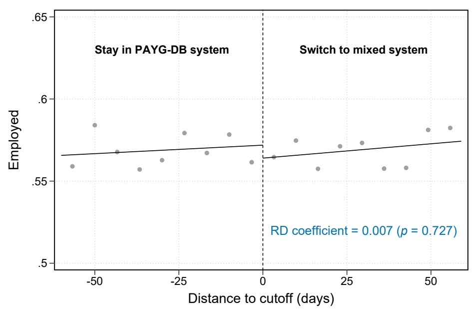
(a) 1997-2000



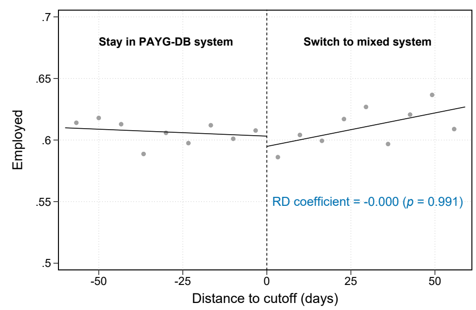
(b) 2001-2004



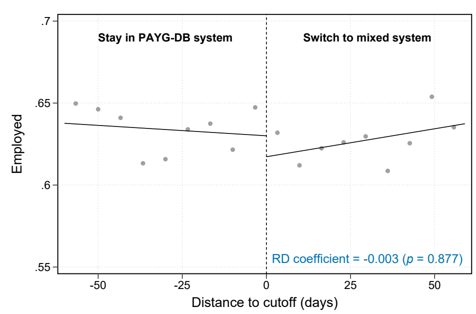
(c) 2005-2008



(d) 2009-2011



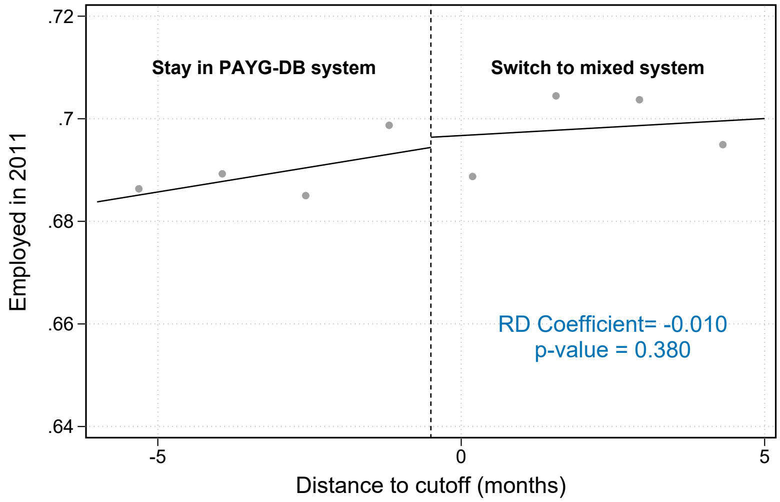
(e) 2012-2013



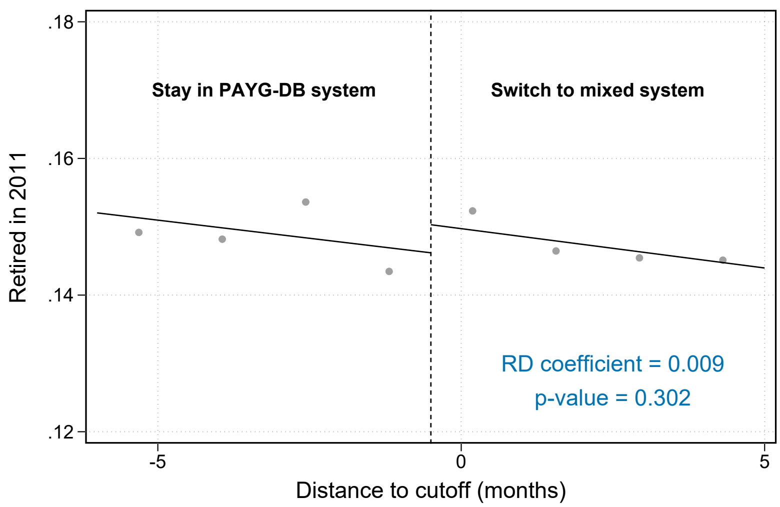
Notes: This figure shows placebo RD-plots for the probability of being employed from equation 1.1, using social security data for workers born in the year after the cohort affected by the reform. Each panel corresponds to a different group of years. In all panels, the dependent variable is an indicator of whether the worker was employed (defined as reporting positive earnings). RD coefficients are estimated using a 11-day window around the cutoff date of birth and p-values are calculated using clustered standard errors at the worker level. The dependent variable is residualized from year fixed effects and evaluated at the mean.

Figure A.28: Placebo RD plots (year after) - Census data

(a) Employment rates



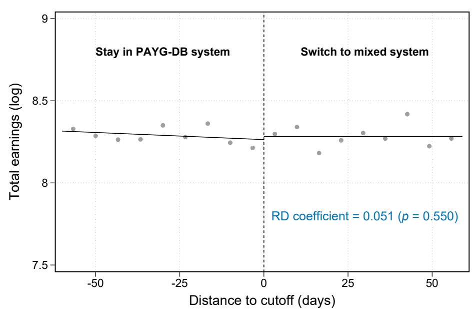
(b) Retirement rates



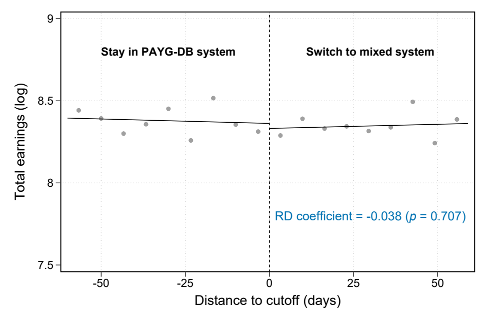
Notes: This figure shows the placebo RD plots for the probability of being employed and the probability of being retired using census data for individuals born on the year after the cohort affected by the reform. Panel (a) reports the effect for the probability of being employed. Panel (b) for the probability of being retired.

Figure A.29: Placebo RD plots for earnings (year after) - SSA data

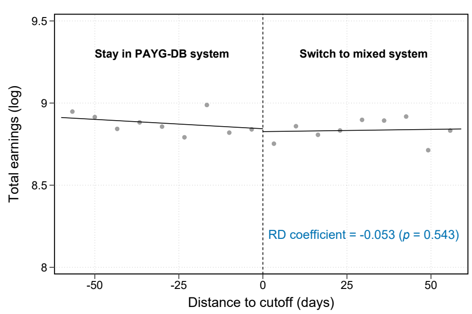
(a) 1997-2000



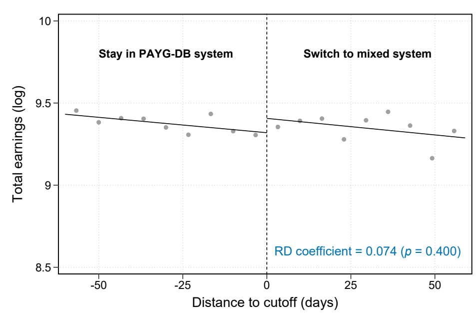
(b) 2001-2004



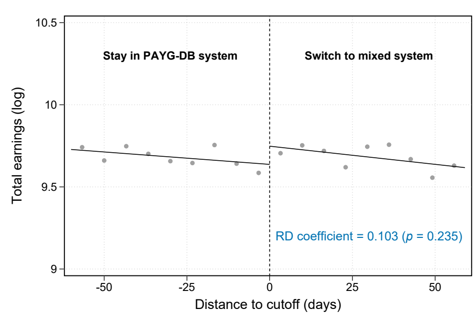
(c) 2005-2008



(d) 2009-2011



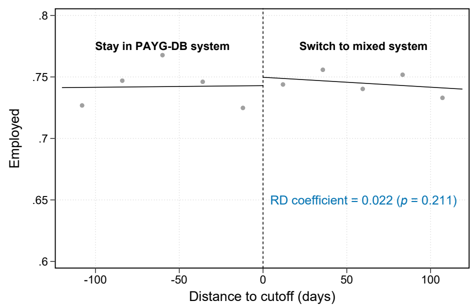
(e) 2012-2013



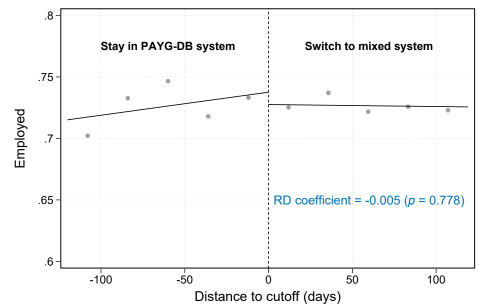
Notes: This figure shows placebo RD-plots for labor earnings from equation 1.1, using social security data for workers born in the year after the cohort affected by the reform. Each panel corresponds to a different group of years. In all panels, the dependent variable is the natural logarithm of total labor earnings. RD coefficients are estimated using a 11-day window around the cutoff date of birth and *p*-values are calculated using clustered standard errors at the worker level. The dependent variable is residualized from year fixed effects and evaluated at the mean.

Figure A.30: Placebo RD plots for employment rates (year after) - IRS data

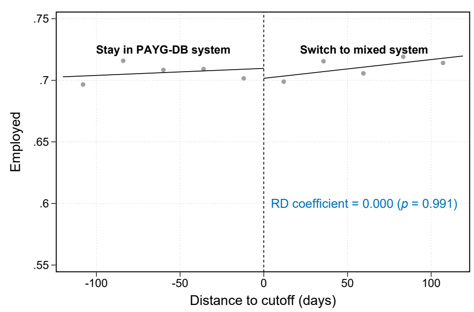
(a) 2009-2010



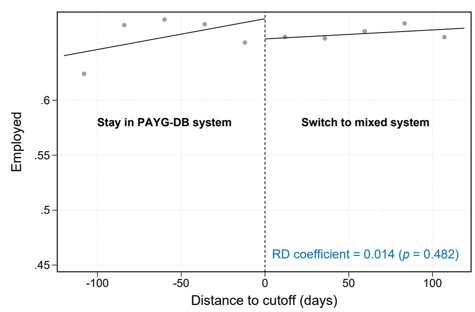
(b) 2011-2012



(c) 2013-2014



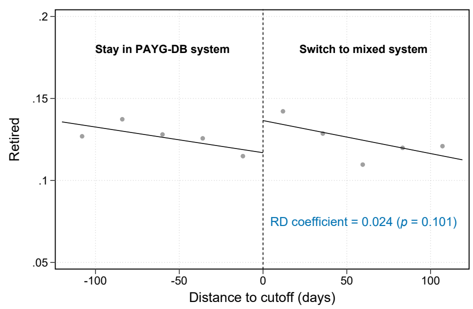
(d) 2015-2016



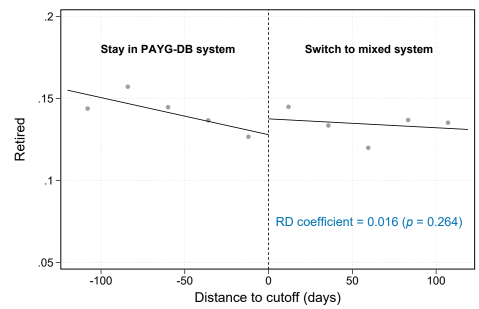
Notes: This figure shows placebo RD-plots for the probability of being employed from equation 1.1, using IRS data for workers born on the year after the cohort affected by the reform. Each panel corresponds to a different group of year. In all panels, the dependent variable is an indicator of whether the worker was employed (defined as reporting positive labor earnings). The dependent variable is residualized from year fixed effects and evaluated at the mean. RD coefficients are estimated using a 22-day window around the cutoff date of birth and p-values are calculated using clustered standard errors at the worker level.

Figure A.31: Placebo RD plots for retirement rates (year after) - IRS data

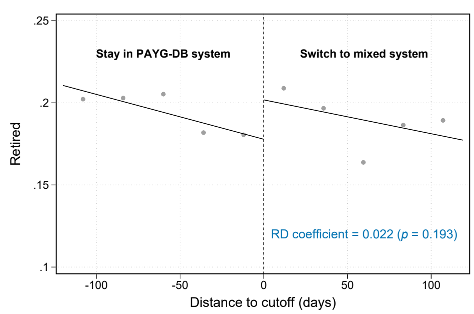
(a) 2009-2010



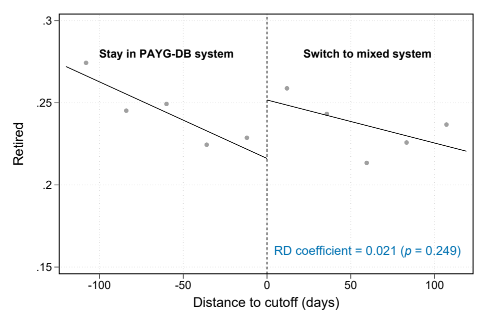
(b) 2011-2012



(c) 2013-2014

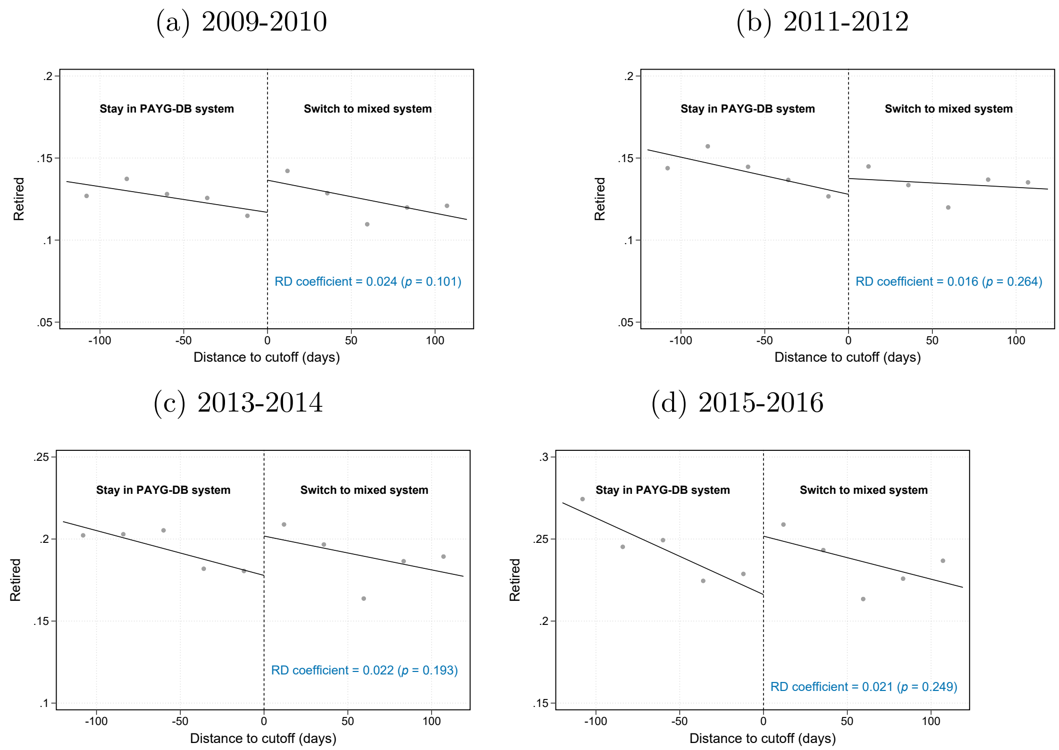


(d) 2015-2016



Notes: This figure shows placebo RD-plots for the probability of being retired from equation 1.1, using IRS data for workers born on the year after the cohort affected by the reform. Each panel corresponds to a different group of year. In all panels, the dependent variable is an indicator of whether the worker was retired (defined as reporting positive pension income). The dependent variable is residualized from year fixed effects and evaluated at the mean. RD coefficients are estimated using a 22-day window around the cutoff date of birth and p-values are calculated using clustered standard errors at the worker level.

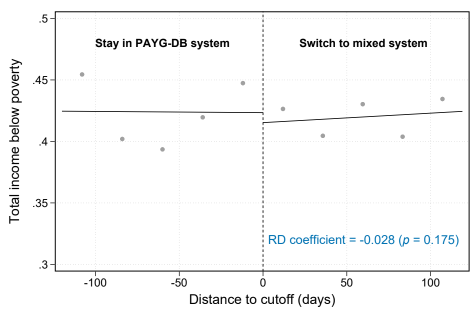
Figure A.32: Placebo RD plots for total income (year after) - IRS data



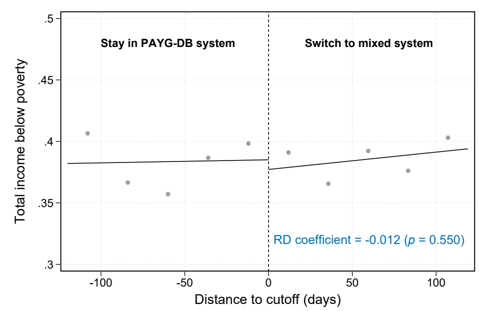
Notes: This figure shows placebo RD-plots for the total income from equation 1.1, using IRS data for workers born on the year after the cohort affected by the reform. Each panel corresponds to a different group of year. In all panels, the dependent variable is the total income (defined as pension plus earnings) measured in 2009 Uruguayan pesos. The dependent variable is residualized from year fixed effects and evaluated at the mean. RD coefficients are estimated using a 22-day window around the cutoff date of birth and p-values are calculated using clustered standard errors at the worker level.

Figure A.33: Placebo RD plots for total income below poverty line (year after) - IRS data

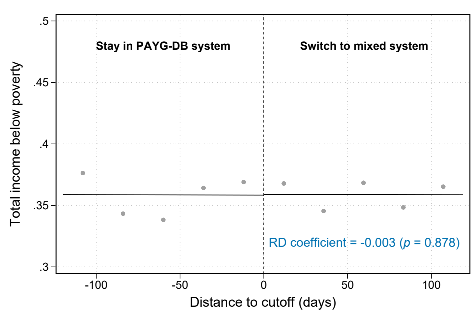
(a) 2009-2010



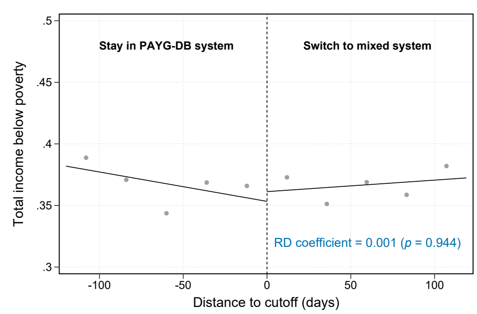
(b) 2011-2012



(c) 2013-2014



(d) 2015-2016



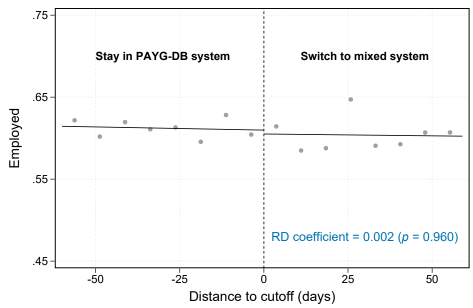
Notes: This figure shows placebo RD-plots for the probability of total income being below the poverty line from equation 1.1, using IRS data for workers born on the year after the cohort affected by the reform. Each panel corresponds to a different group of year. In all panels, the dependent variable is an indicator of whether the total income (defined as pension plus earnings) is below the National poverty line from Montevideo. The dependent variable is residualized from year fixed effects and evaluated at the mean. RD coefficients are estimated using a 22-day window around the cutoff date of birth and *p*-values are calculated using clustered standard errors at the worker level.

A.5 Individual RD plots for all years

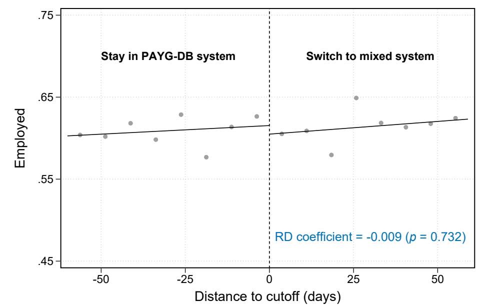
In this section, we present individual RDD plots and coefficients for all years in the data.

Figure A.34: Effect of the reform on employment rates - SSA data

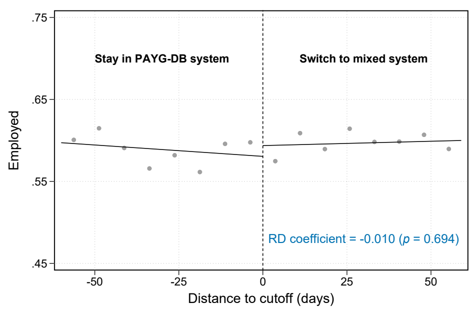
(a) In 1997



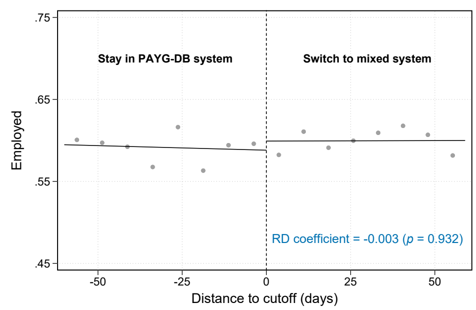
(b) In 1998



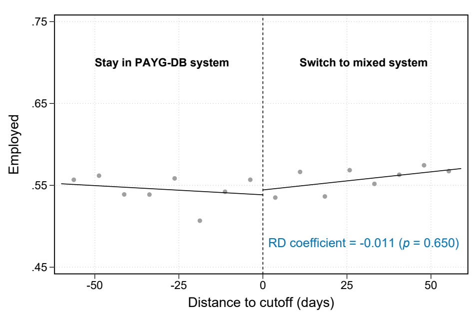
(c) In 1999



(d) In 2000



(e) In 2001



(f) In 2002

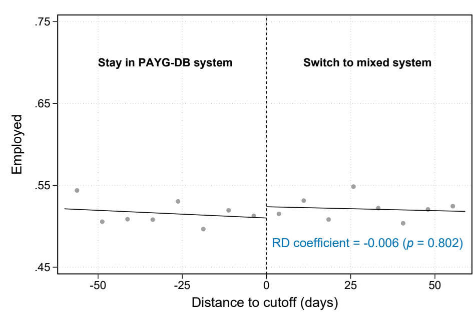
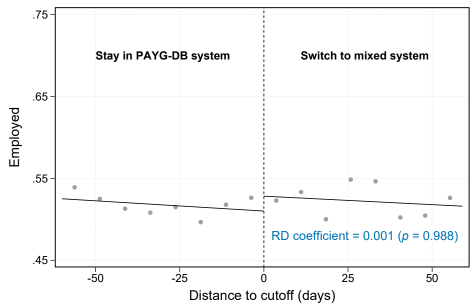
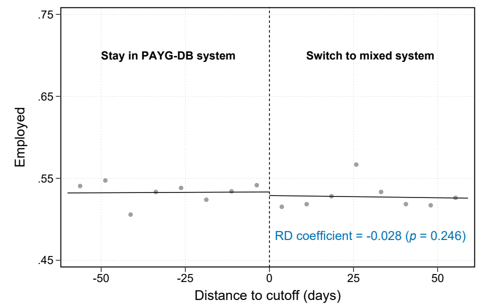


Figure A.34: Effect of the reform on employment rates - SSA data (continued)

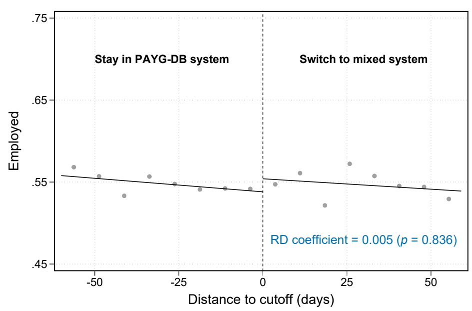
(g) In 2003



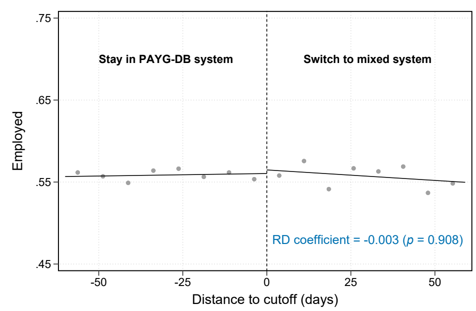
(h) In 2004



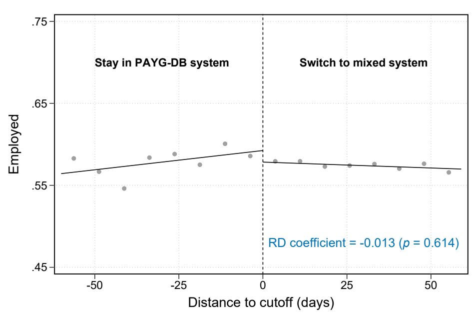
(i) In 2005



(j) In 2006



(k) In 2007



(l) In 2008

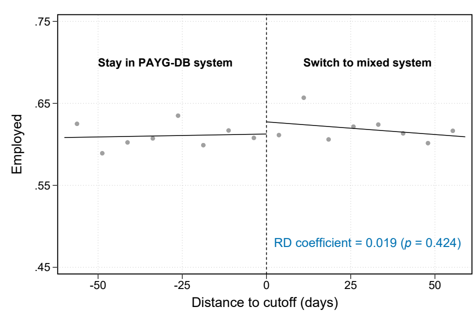
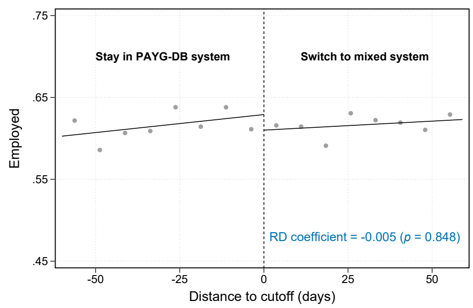
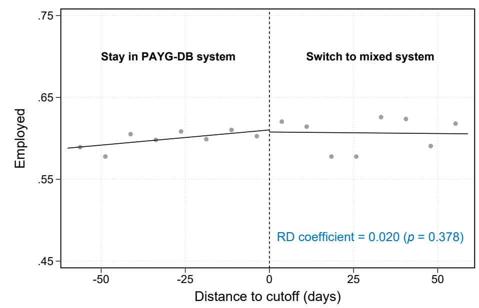


Figure A.34: Effect of the reform on employment rates - SSA data (continued)

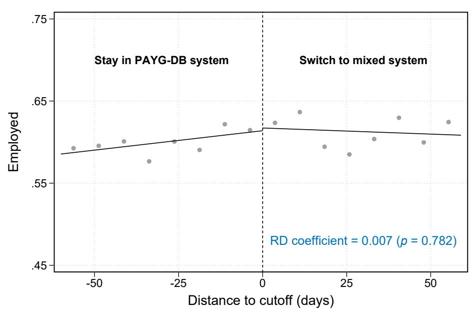
(m) In 2009



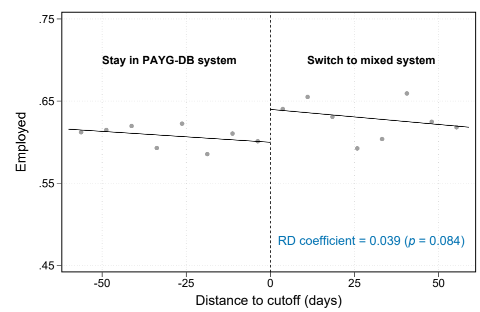
(n) In 2010



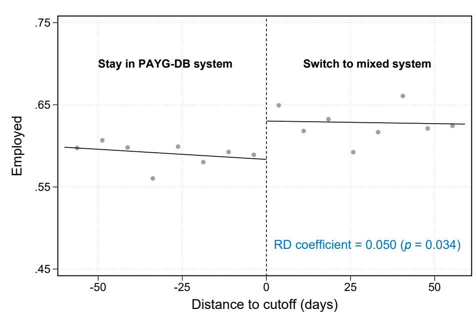
(o) In 2011



(p) In 2012



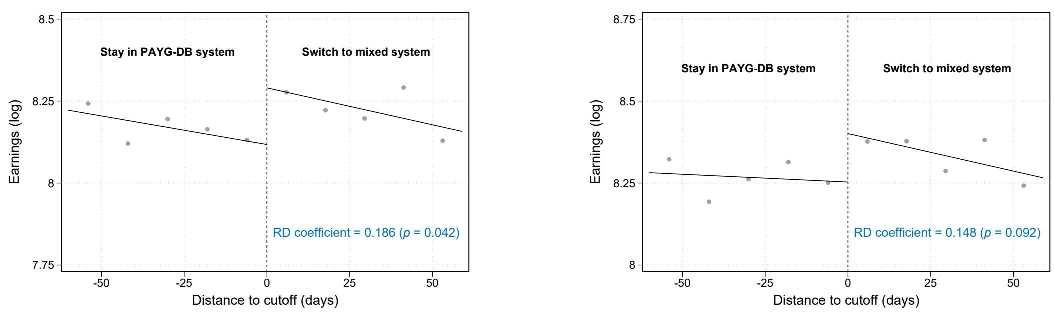
(q) In 2013



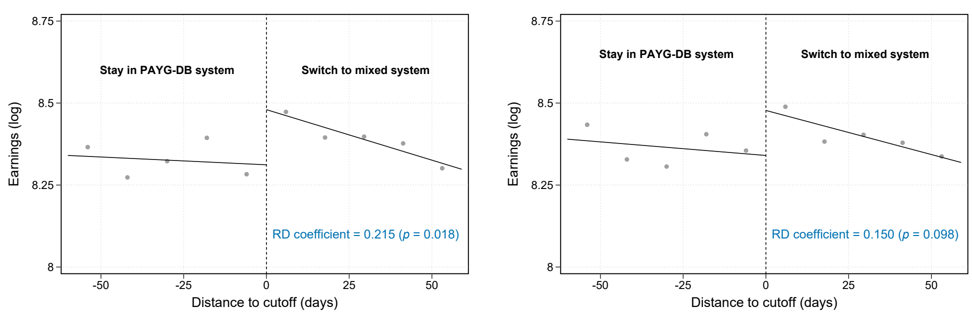
Notes: This figure shows RD-plots for the probability of being employed from equation 1.1. Each panel corresponds to a different year. In all panels, the dependent variable is an indicator of whether the worker was employed (defined as reporting positive earnings). RD coefficients are estimated using a 11-day window around the cutoff date of birth and p-values are calculated using randomization inference techniques from Cattaneo, Titiunik, and Vazquez-Bare, 2016 with 1,000 replications.

Figure A.35: Effect of the reform on labor earnings - SSA data

(a) In 1997 (b) In 1998



(c) In 1999 (d) In 2000



(e) In 2001 (f) In 2002

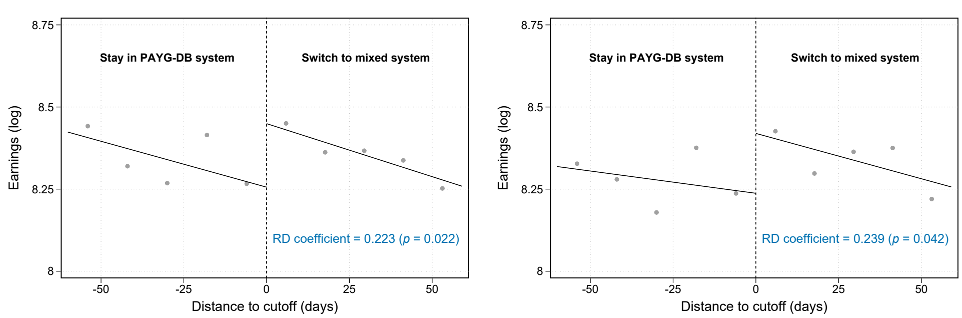
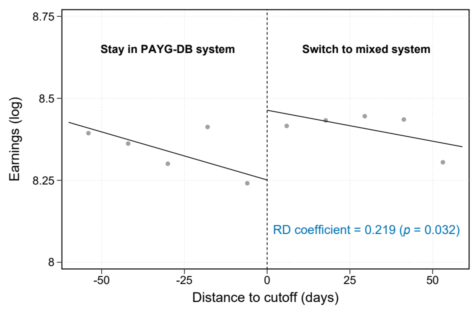
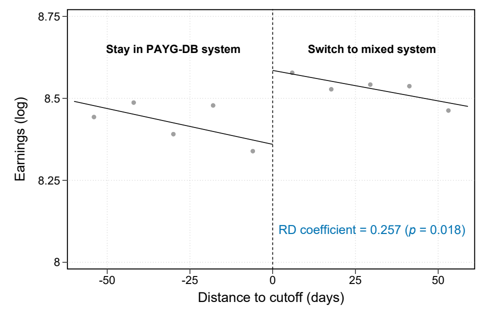


Figure A.35: Effect of the reform on labor earnings - SSA data (continued)

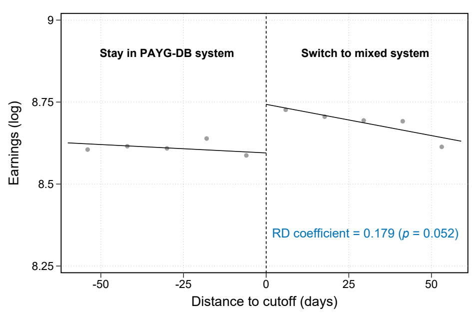
(g) In 2003



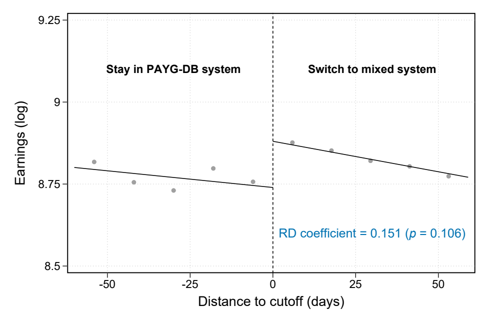
(h) In 2004



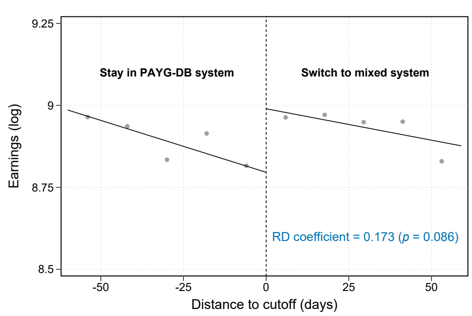
(i) In 2005



(j) In 2006



(k) In 2007



(l) In 2008

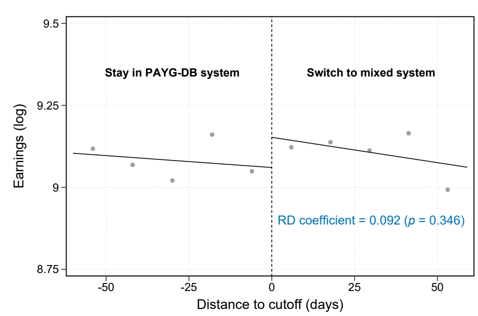
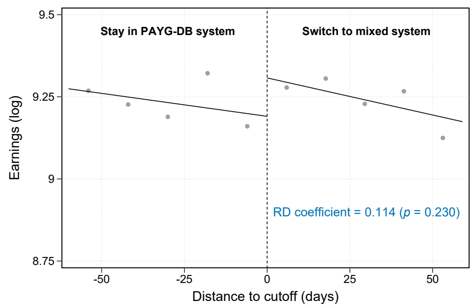
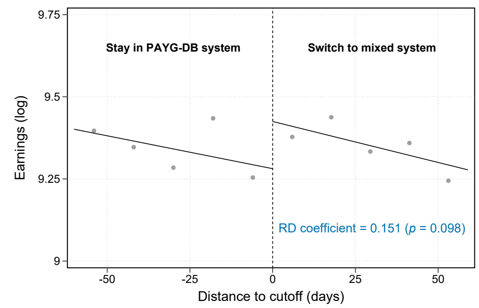


Figure A.35: Effect of the reform on labor earnings - SSA data (continued)

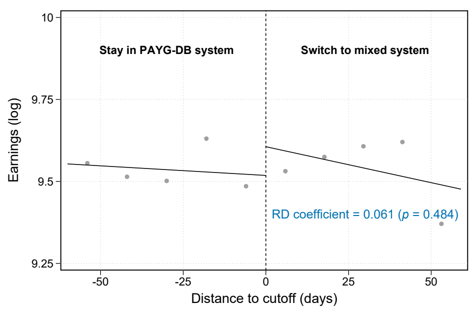
(m) In 2009



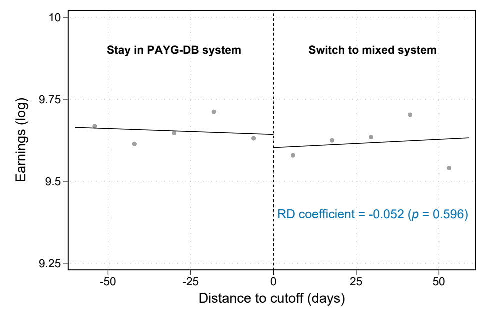
(n) In 2010



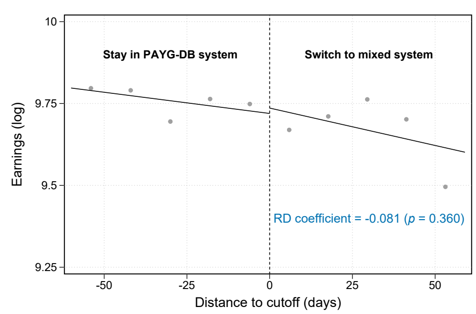
(o) In 2011



(p) In 2012



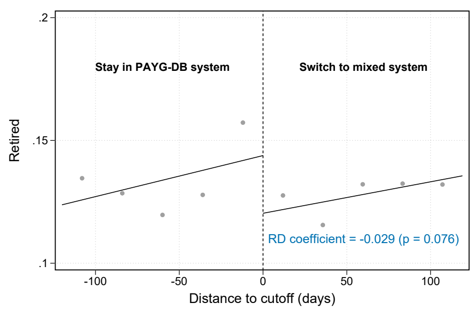
(q) In 2013



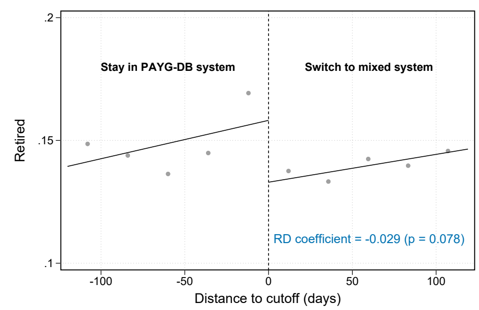
Notes: This figure shows yearly RD-plots for total labor earnings from equation 1.1. Each panel corresponds to a different year. In all panels, the dependent variable is an indicator of whether the worker was employed (defined as reporting positive earning). RD coefficients are estimated using a 11-day window around the cutoff date of birth and p-values are calculated using randomization inference techniques from Cattaneo, Titiunik, and Vazquez-Bare, 2016 with 1,000 replications.

Figure A.36: Effect of the reform on retirement rates - IRS data

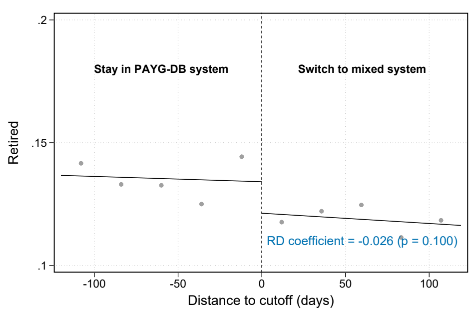
(a) In 2009



(b) In 2010



(c) In 2011



(d) In 2012

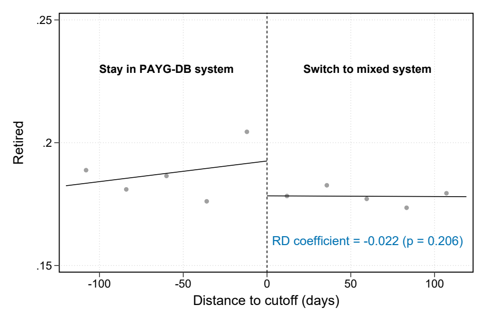
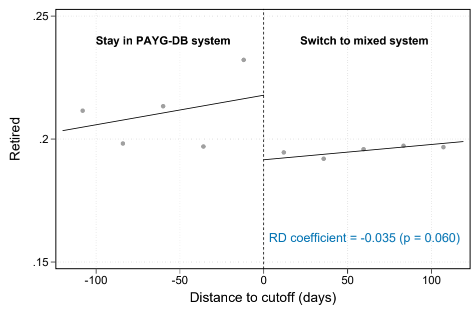
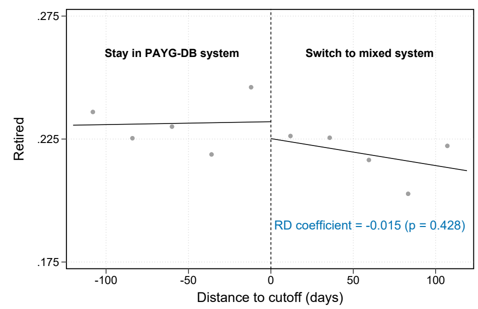


Figure A.36: Effect of the reform on retirement rates - IRS data

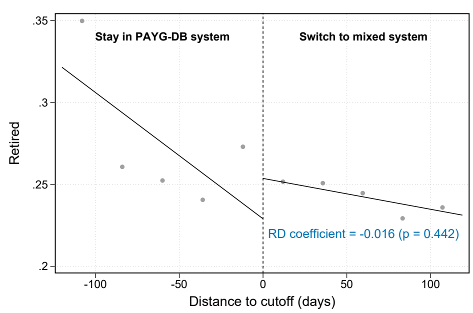
(e) In 2013



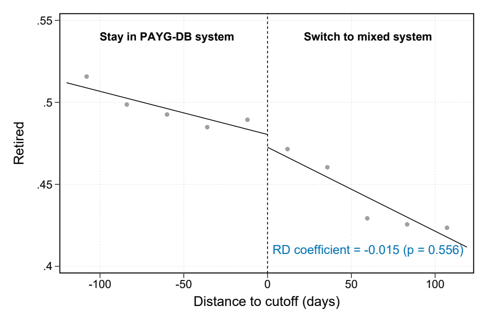
(f) In 2014



(g) In 2015

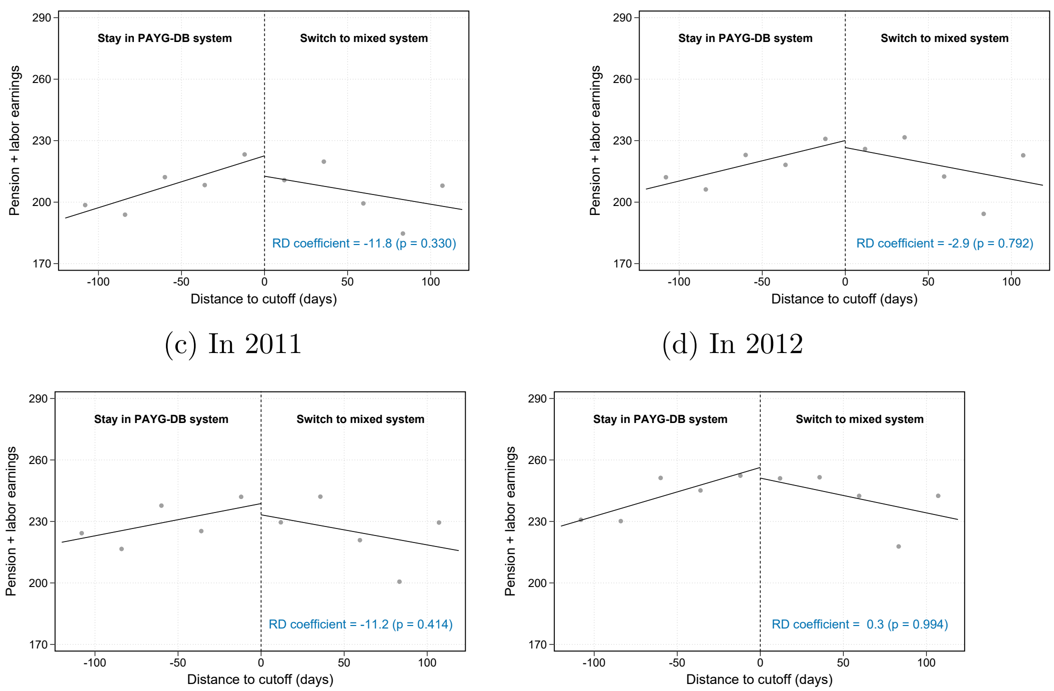


(h) In 2016



Notes: This figure shows yearly RD-plots for the probability of being retired from equation 1.1. Each panel corresponds to a different year. In all panels, the dependent variable is an indicator equal to 1 if the worker is retired. RD coefficients are estimated using a 22-day window around the cutoff date of birth and *p*-values are calculated using randomization inference techniques from Cattaneo, Titiunik, and Vazquez-Bare, 2016 with 1,000 replications.

Figure A.37: Effect of the reform on total income in old age - IRS data


(a) In 2009

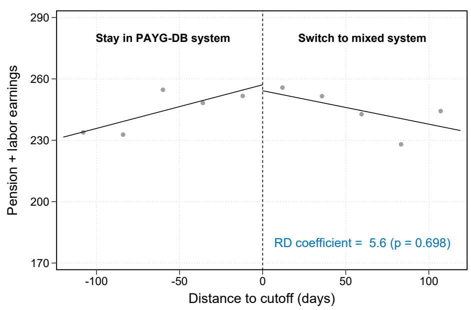
(b) In 2010

(c) In 2011

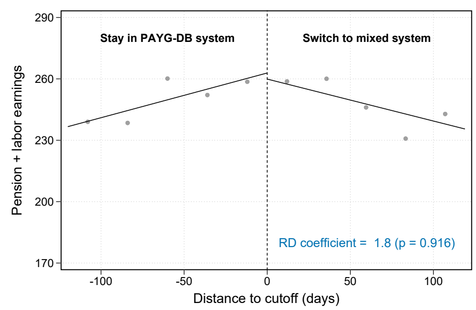
(d) In 2012

Figure A.37: Effect of the reform on total income in old age - IRS data (continued)

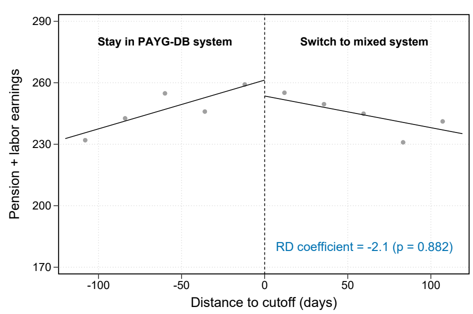
(e) In 2013



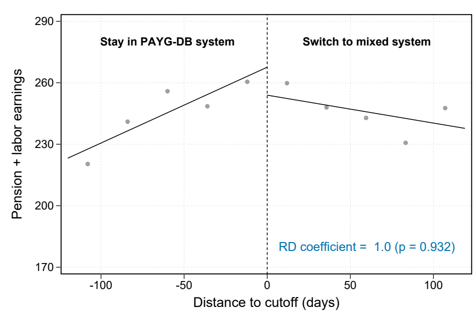
(f) In 2014



(g) In 2015



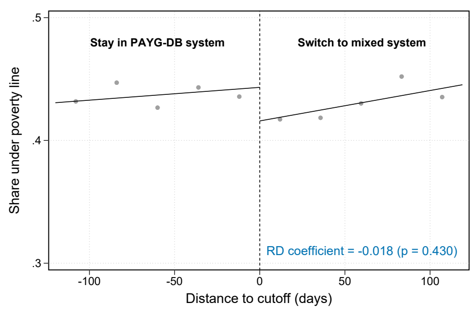
(h) In 2016



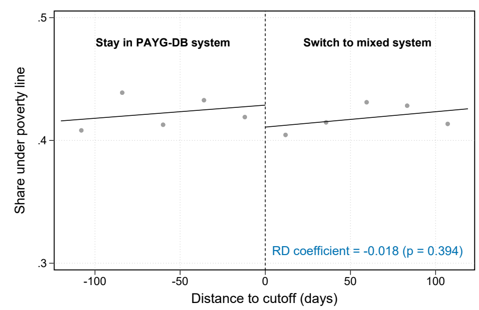
Notes: This figure shows yearly RD-plots for the total income in old age (pension income plus labor earnings) 1.1 for selected years. Each panel corresponds to a different year. In all panels, the dependent variable is the sum of any pension income and any labor earnings, including zeroes, measured in thousand of 2009 Uruguayan pesos. RD coefficients are estimated using a 22-day window around the cutoff date of birth and *p*-values are calculated using randomization inference techniques from Cattaneo, Titiunik, and Vazquez-Bare, 2016 with 1,000 replications.

Figure A.38: Effect of the reform on total income below poverty line (IRS data)

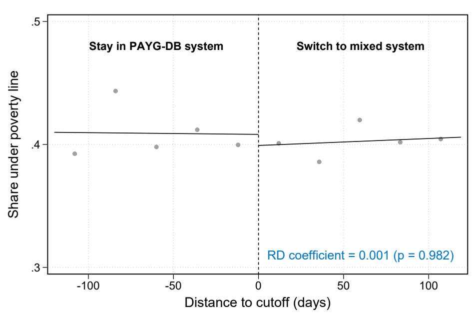
(a) In 2009



(b) In 2010



(c) In 2011



(d) In 2012

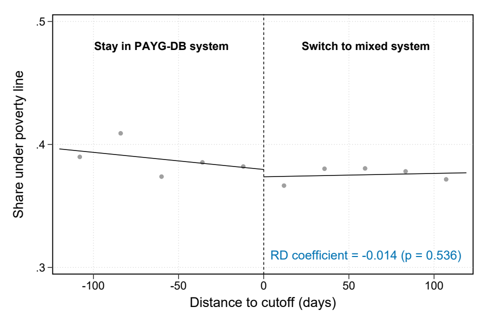
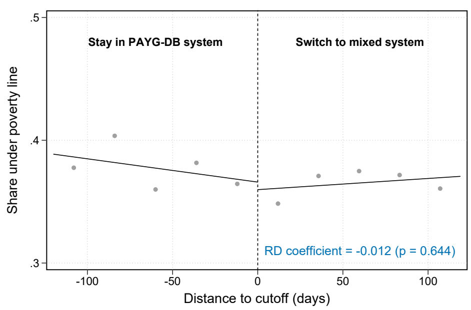
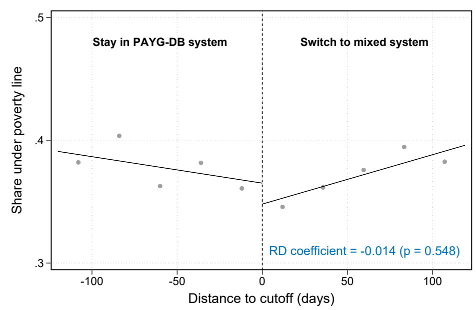


Figure A.38: Effect of the reform on total income below poverty line (IRS data)

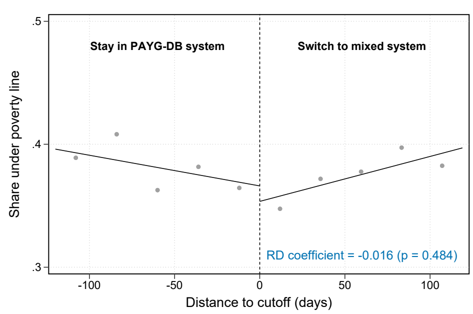
(e) In 2013



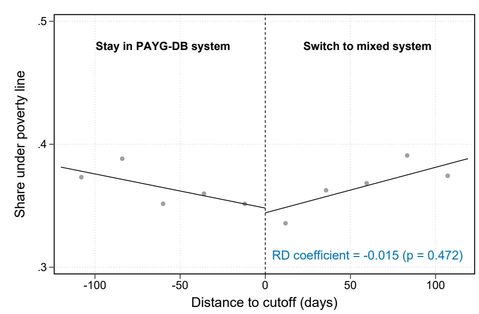
(f) In 2014



(g) In 2015



(h) In 2016



Notes: This figure shows yearly RD-plots for the probability of the total income (pension income plus labor earnings) being below the national poverty line of Montevideo from equation 1.1 for selected years. Each panel corresponds to a different year. In all panels, the dependent variable is an indicator equal to 1 if the total income is below the poverty line. RD coefficients are estimated using a 22-day window around the cutoff date of birth and *p*-values are calculated using randomization inference techniques from Cattaneo, Titiunik, and Vazquez-Bare, 2016 with 1,000 replications.

A.6 Construction of the SES index

In this section we describe the procedure to construct the socioeconomic status (SES) index. We proceed in two steps, first we select several characteristics indicative of SES (such as whether the individual owns their dwelling, has completed college, and owns several durable goods). Then we compute the index as a weighted sum of these characteristics, the weights of which we obtain via Principal Component Analysis (PCA).

We have information on several characteristics indicative of socioeconomic status. We use an indicator of whether the individual has completed a college degree, an indicator of being a home owner, an indicator for having a clothes drying machine, the number of television sets owned, an indicator for owning a mobile phone, an indicator of owning a computer, the number of cars owned, and an indicator of having an internet connection.[1] Table A.3 presents summary statistics of the variables we use to construct the SES index.

Table A.3: Summary statistics for variables used to construct the SES index

	Observations	Mean	Standard Deviation	Median
College complete	109,828	0.224	0.417	0.000
Home owner	109,354	0.676	0.468	1.000
Has clothes dryer	109,354	0.109	0.312	0.000
Number of TVs	109,828	1.848	0.981	2.000
Has mobile phone	109,354	0.930	0.255	1.000
Has computer	109,828	0.556	0.497	1.000
Number of cars	109,828	0.542	0.677	0.000
Has internet	109,354	0.506	0.500	1.000

Notes: This table reports summary statistics for the variables used to construct the socioeconomic status index. College complete is a dummy variable equal to 1 if the individual has completed any college degree and zero otherwise. Home owner is a dummy variable equal to 1 if the individual owns their home and zero otherwise. Has clothes dryer is a dummy variable equal to 1 if the household owns a clothes drying machine. Number of TVs is the number of television sets owned in the household. Has mobile phone is a dummy variable equal to 1 if the individual owns a mobile phone and zero otherwise. Has computer is a dummy variable equal to 1 if the household owns at least one computer and zero otherwise. Number of cars is the total number of cars owned by the household. Has internet is a dummy variable equal to 1 if the household has an internet connection and zero otherwise.

Table A.4 reports the results for the PCA, where we report the main 3 components. Panel A shows the variable weights and Panel B shows the statistics associated to each component.

[1] The census data also contains other variables that are frequently used to infer socioeconomic status, such as having a bathroom or having electricity. However, these have little variation, since most households in the sample have access to such amenities. Thus, we exclude them for the derivation of the socioeconomic status index.

The typical approach in the SES literature is to retain only the first component, based on the fact that it tends to provide a good estimation of the SES of the household (Filmer and Pritchett, 2001; McKenzie, 2005). The first component in our case positively correlates with all the variables, and has an eigenvalue of almost 2.8 while explaining almost 35% of the variance. We normalize this first component to have mean zero and standard deviation of one, and use it as our SES index.

Table A.4: Principal component analysis for SES index

	Component 1	Component 2	Component 3
Panel A. Variable loadings			
College complete	.2957395	.0577124	-.3226414
Home owner	.1573287	.6922537	.5696147
Has clothes dryer	.2262674	.3437633	-.6546112
Number of TVs	.3858493	.060771	.0300353
Has mobile phone	.2058759	-.3836007	.3673276
Has computer	.5070501	-.2670452	.0579324
Number of cars	.3636602	.3293137	.0448591
Has internet	.5059405	-.2620643	.0415409
Panel B. Component statistics			
Eigenvalue	2.784	1.055	0.934
Proportion explained	0.348	0.132	0.117

Notes: This table reports the results from the principal component analysis. We keep the 3 main components. Panel A reports the variable weights for each component and Panel B reports the component statistics. College complete is a dummy variable equal to 1 if the individual has completed any college degree and zero otherwise. Home owner is a dummy variable equal to 1 if the individual owns their home and zero otherwise. Has clothes dryer is a dummy variable equal to 1 if the household owns a clothes drying machine. Number of TVs is the number of television sets owned in the household. Has mobile phone is a dummy variable equal to 1 if the individual owns a mobile phone and zero otherwise. Has computer is a dummy variable equal to 1 if the household owns at least one computer and zero otherwise. Number of cars is the total number of cars owned by the household. Has internet is a dummy variable equal to 1 if the household has an internet connection and zero otherwise. Eigenvalue is the eigenvalue associated to each component. Proportion of the variance explained is the proportion of the variance explained by each component.

A.7 Correlation of earnings with days and hours worked

In this section we assess the relationship of labor earnings with real measures of labor supply. Specifically, we correlate our measures of real labor supply (days and hours worked) with labor earnings. We estimate equations of the form:

$$Y_{it} = \alpha + \beta \text{LaborSupply}_{it} + u_{it} \tag{A.1}$$

Where Y_{it} represents the earnings of worker i at time t. LaborSupply_{it} is a measure of real labor supply (days worked in the month or the natural logarithm of hours worked). In different specifications we include time fixed effects and worker fixed effects. We use the full sample of individuals born between 1955 and 1957 and cluster standard errors at the worker level.

Table A.5 presents OLS estimates of equation A.1. Panel A includes the monthly days worked as the measure of labor supply. Panel B includes the natural logarithm of the total monthly hours worked. Panel C includes both days and hours worked. Column 1 includes no additional controls, column 2 includes year fixed effects, column 3 includes worker-fixed effects, and column 4 includes worker and year fixed effects.

Across specifications, both measures of labor supply positively correlate with labor earnings. An additional day worked is associated to an increase in labor earnings between 3 and 4 percent. Similarly, monthly hours worked also positively correlate with earnings: a 10 percent increase in monthly hours worked is associated to a 2.6 percent increase in earnings. Both correlations are robust to estimating the coefficients using within-person variation by including worker fixed effects (columns 3 and 4) or while including both measures of labor supply (in Panel C).

Table A.5: Regressions of earnings on hours and days worked

	Total labor earnings (log)			
	(1)	(2)	(3)	(4)
Panel A. Days worked				
Days worked in the month	0.0366***	0.0392***	0.0228***	0.0258***
	(0.000406)	(0.000400)	(0.000287)	(0.000235)
Year fixed effects		✓		✓
Worker fixed effects			✓	✓
Number of workers	121356	121356	108831	108831
Panel B. Hours worked				
Monthly hours worked (log)	0.315***	0.343***	0.266***	0.272***
	(0.00626)	(0.00602)	(0.00446)	(0.00335)
Year fixed effects		✓		✓
Worker fixed effects			✓	✓
Number of workers	120728	120728	107957	107957
Panel C. Days and hours worked				
Days worked in the month	0.0447***	0.0463***	0.0236***	0.0260***
	(0.000445)	(0.000434)	(0.000328)	(0.000271)
Monthly hours worked (log)	-0.00512	0.0120*	0.123***	0.114***
	(0.00645)	(0.00615)	(0.00470)	(0.00345)
Year fixed effects		✓		✓
Worker fixed effects			✓	✓
Number of workers	120728	120728	107957	107957

Notes: this table reports OLS estimates of equation A.1. In all specifications the dependent variable is the natural logarithm of total labor earnings. Standard errors are clustered at the worker level. Days worked in the month is the total number of days worked in the month. Monthly hours worked (log) is the natural logarithm of total monthly hours worked. Column 2 includes year fixed effects. Column 3 includes worker fixed effects. Column 4 includes year fixed effects and worker fixed effects. * Significant at the 10% level ** Significant at the 5% level *** Significant at the 1% level.

Appendix B

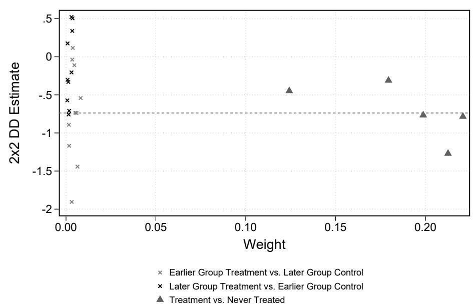
Appendix of Workplace Litigiousness and Labor Market Outcomes: Evidence from a Workers' Compensation Reform

B.1 Goodman-Bacon, 2021 decompositions

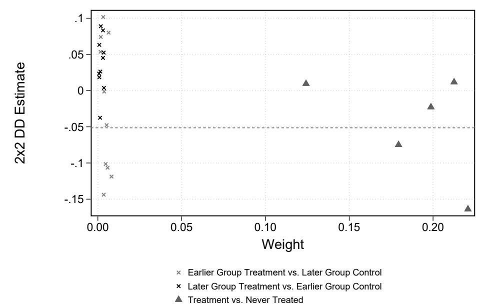
This section presents decompositions based on Goodman-Bacon, 2021. Intuitively, with staggered implementation, the difference-in-differences coefficient constitutes a weighted average of post-pre comparisons between early treated units and never treated units and not-yet-treated units, but also "forbidden comparisons" using early treated units as control for late treated units. The decomposition from Goodman-Bacon, 2021 assesses the degree to which each type of comparison drives the results. Reassuringly, in our case, the estimation for the difference-in-differences coefficient relies almost exclusively on comparisons between treated units and never-treated units.

Figure B.1: Goodman-Bacon, 2021 decomposition of province-level results

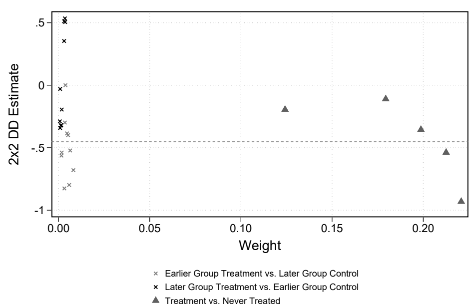
(a) Number of lawsuits (IHS)



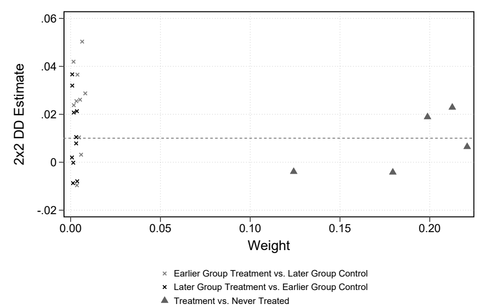
(b) Number of accidents (IHS)



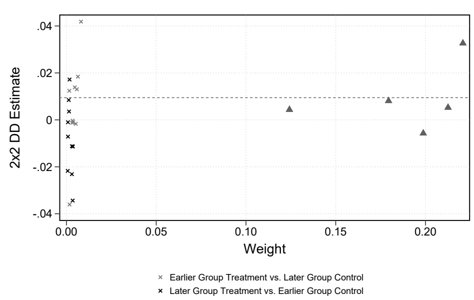
(c) Amount claimed (% of labor costs)



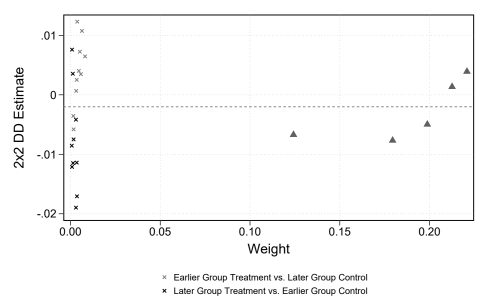
(d) Employment (IHS)



(e) Average monthly wage (log)



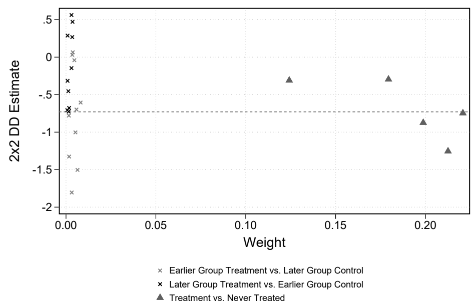
(f) Number of firms (IHS)



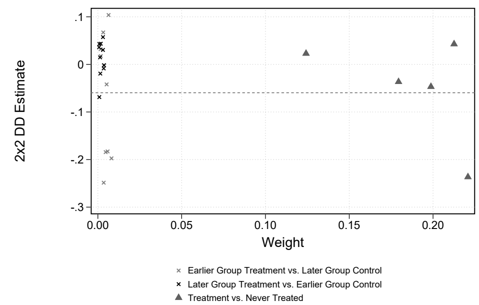
Notes: This figure shows the 2x2 difference-in-difference coefficients and weights assigned by the Goodman-Bacon, 2021 decomposition for the estimation of equation (2.2) including time and province fixed effects using different dependent variables. The unit of observation is a province-by-quarter. The dependent variable in Panel (a) is the inverse hyperbolic sine transformation of the total number of lawsuits reported. The dependent variable in Panel (b) is the natural logarithm of the total number of accidents reported. The dependent variable in Panel (c) is the amount claimed in lawsuits as a share of labor costs (total employment times average monthly wage). The dependent variable in Panel (d) is the inverse hyperbolic sine of the total number of workers. The dependent variable in Panel (e) is the natural logarithm of the average monthly wage. The dependent variable in Panel (f) is the inverse hyperbolic sine of the total number of firms.

Figure B.2: Goodman-Bacon, 2021 decomposition of sector-by-province level results

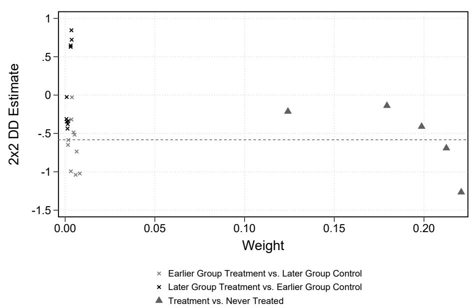
(a) Number of lawsuits (IHS)



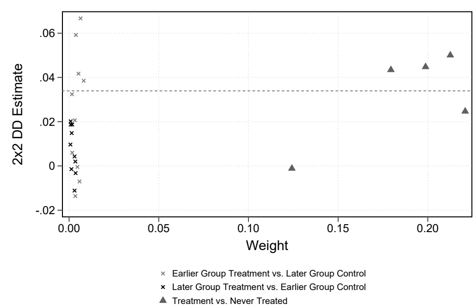
(b) Number of accidents (IHS)



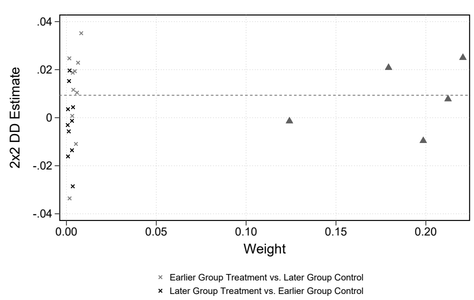
(c) Amount claimed (% of labor costs)



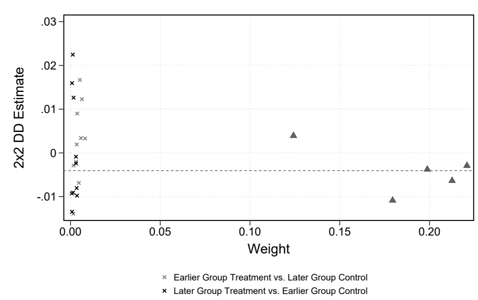
(d) Employment (IHS)



(e) Average monthly wage (log)



(f) Number of firms (IHS)

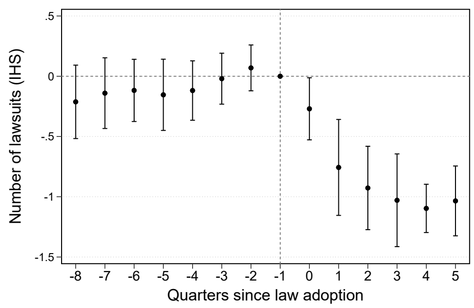


Notes: This figure shows the 2x2 difference-in-difference coefficients and weights assigned by the Goodman-Bacon, 2021 decomposition for the estimation of equation (2.2) including time and province fixed effects using different dependent variables. The unit of observation is a sector-by-province-by-quarter. The dependent variable in Panel (a) is the inverse hyperbolic sine transformation of the total number of lawsuits reported. The dependent variable in Panel (b) is the natural logarithm of the total number of accidents reported. The dependent variable in Panel (c) is the amount claimed in lawsuits as a share of labor costs (total employment times average monthly wage). The dependent variable in Panel (d) is the inverse hyperbolic sine of the total number of workers. The dependent variable in Panel (e) is the natural logarithm of the average monthly wage. The dependent variable in Panel (f) is the inverse hyperbolic sine of the total number of firms.

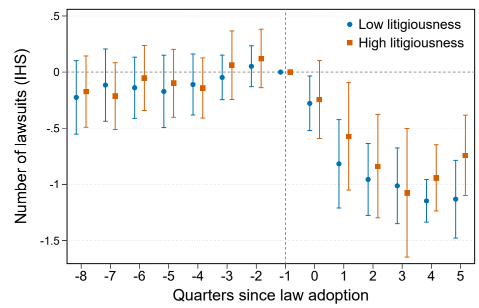
B.2 Additional results

Figure B.3: Sector-by-province level results: Lawsuits and accidents

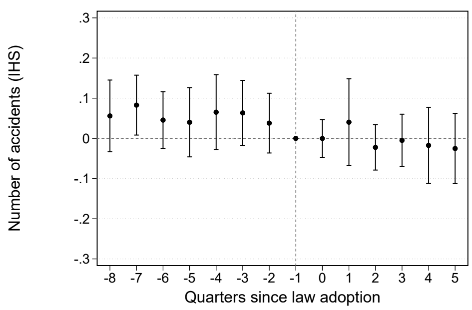
(a) Number of lawsuits (IHS)]



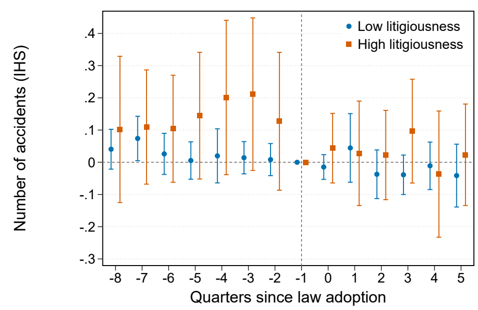
(b) Number of lawsuits (IHS) - Heterogeneity



(c) Number of accidents (IHS)

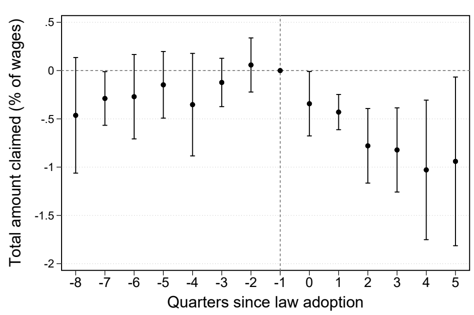


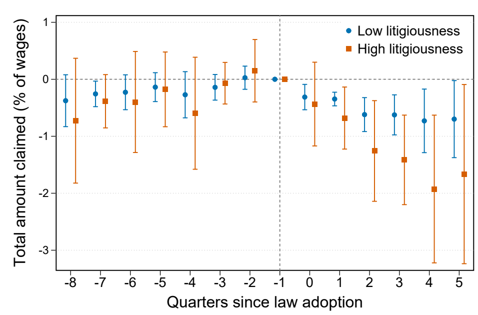
(d) Number of accidents (IHS) - Heterogeneity



(f) Amount claimed (% of labor costs)

(e) Amount claimed (% of labor costs)- Heterogeneity





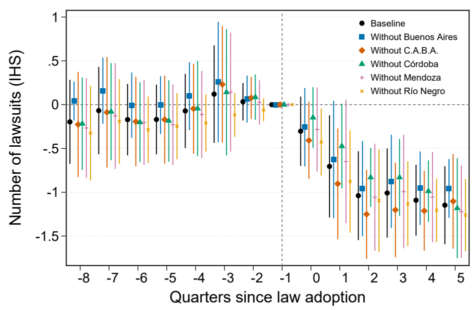
Notes: This figure plots the β_k coefficients from equation (2.1) at the sector-by-province-by-quarter level using different dependent variables. The unit of observation is a sector-by-province-by-quarter. Standard errors are clustered at the province level. Coefficients in orange correspond to the event study for sectors indicated as "high litigiousness" in figure 2.1: construction, mining, and manufacturing. Coefficients in blue correspond to the event study for the rest of the sectors. Vertical bars represent 95% confidence intervals. The dependent variable in Panels (a) and (b) is the inverse hyperbolic sine transformation of the total number of lawsuits reported. The dependent variable in Panels (c) and (d) is the inverse hyperbolic sine of the total number of accidents reported. The dependent variable in Panels (e) and (f) is the amount claimed in lawsuits as a share of labor costs (total employment times average monthly wage).

B.3 Leave-one-out regressions

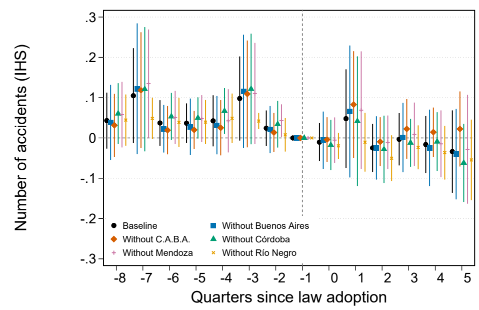
This appendix compares the baseline estimates to leave-one-out alternative specifications, where we sequentially drop one of the 5 treated provinces from the sample and run the event study using the remaining 23 provinces. We first present leave-one-out comparisons for province-level results and then for sector-by-province-level results.

Figure B.4: Leave-one-out regressions: province-level results

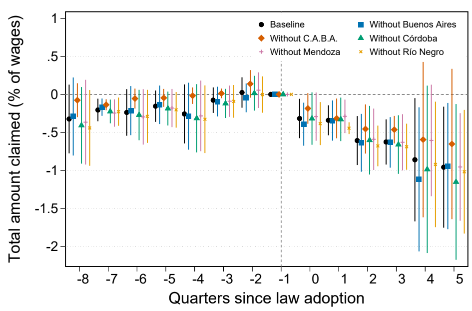
(a) Number of lawsuits (IHS)



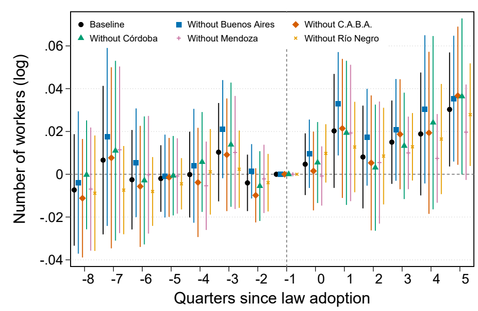
(b) Number of accidents (IHS)



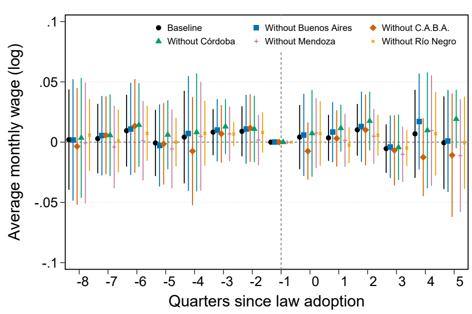
(c) Amount claimed (% of labor costs)



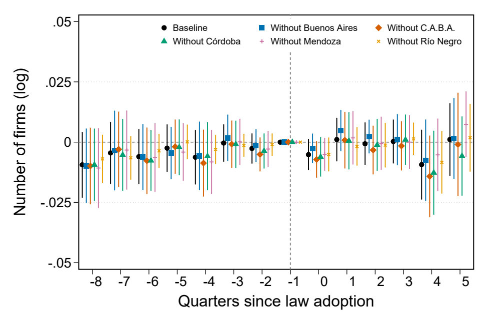
(d) Employment (IHS)



(e) Average monthly wage (log)



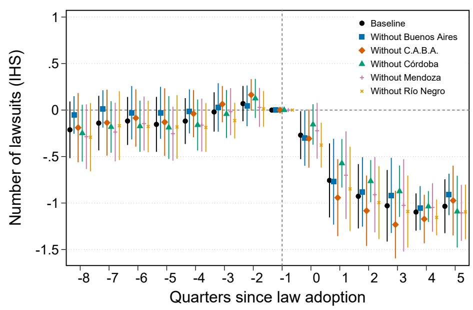
(f) Number of firms (IHS)



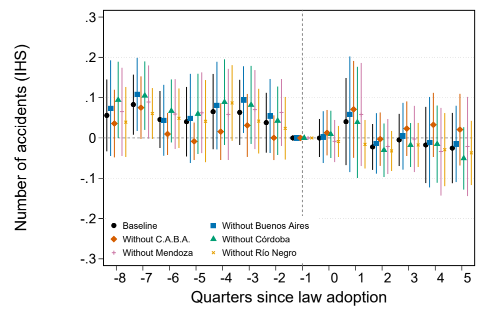
Notes: This figure plots the β_k coefficients from equation (2.1) using different dependent variables. The unit of observation is a province-by-quarter. Standard errors are clustered at the province level. Vertical bars represent 95% confidence intervals. The dependent variable in Panel (a) is the inverse hyperbolic sine transformation of the total number of lawsuits reported. The dependent variable in Panel (b) is the inverse hyperbolic sine of the total number of accidents reported. The dependent variable in Panel (c) is the amount claimed in lawsuits as a share of labor costs (total employment times average monthly wage). The dependent variable in Panel (d) is the inverse hyperbolic sine of the total number of workers. The dependent variable in Panel (e) is the natural logarithm of the average monthly wage. The dependent variable in Panel (f) is the inverse hyperbolic sine of the total number of firms.

Figure B.5: Leave-one-out regressions: sector-by-province-level results

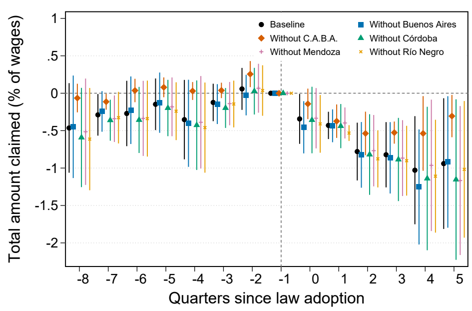
(a) Number of lawsuits (IHS)



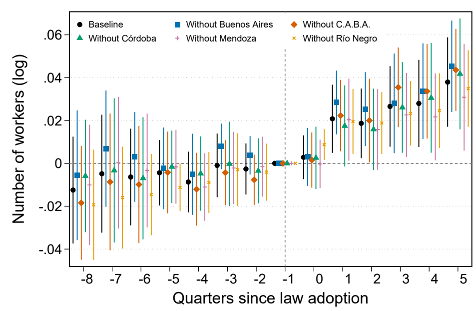
(b) Number of accidents (IHS)



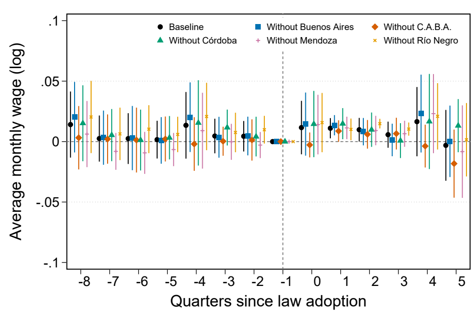
(c) Amount claimed (% of labor costs)



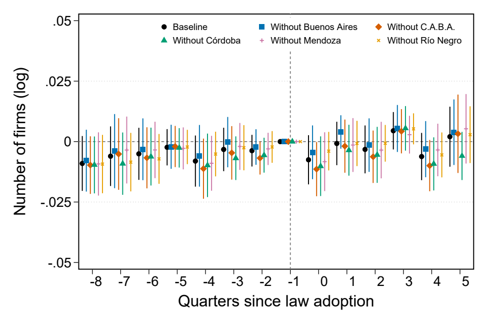
(d) Employment (IHS)



(e) Average monthly wage (log)



(f) Number of firms (IHS)



Notes: This figure plots the β_k coefficients from equation (2.1) using different dependent variables. The unit of observation is a sector-by-province-by-quarter. Standard errors are clustered at the province level. Vertical bars represent 95% confidence intervals. The dependent variable in Panel (a) is the inverse hyperbolic sine transformation of the total number of lawsuits reported. The dependent variable in Panel (b) is the inverse hyperbolic sine of the total number of accidents reported. The dependent variable in Panel (c) is the amount claimed in lawsuits as a share of labor costs (total employment times average monthly wage). The dependent variable in Panel (d) is the inverse hyperbolic sine of the total number of workers. The dependent variable in Panel (e) is the natural logarithm of the average monthly wage. The dependent variable in Panel (f) is the inverse hyperbolic sine of the total number of firms.

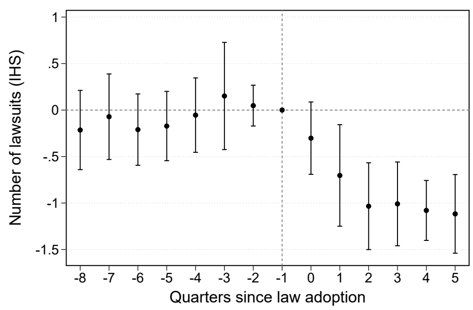
B.4 Stacked event studies

In this subsection we estimate the main event studies of interest using a stacked event study approach (Baker, Larcker, and Wang, 2022). For each treated province, we define a window of 14 quarters, 8 before the reform and 6 after. We then define an event-specific control group for that province consisting of never treated provinces. This creates a data-set for each specific event. We then stack all the event-specific data-set and estimate event-study regressions quarter-by-region-by-event fixed effects. We include province-by-event fixed effects for the province-level analysis and sector-by-province-by-event fixed effects for the sector-by-province-level analysis. The equation we estimate is given by:

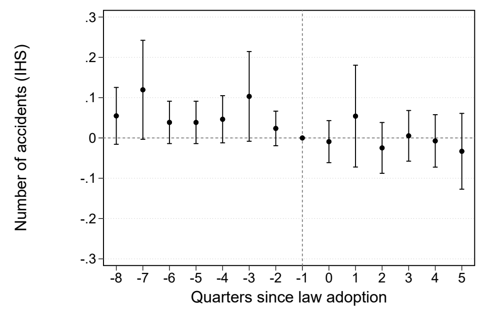
$$Y_{pt} = \alpha_{pe} + \mu_{r(p)te} + \sum_{k=-8}^{5} \beta_k \mathbb{1}\{t = e_p + k\} \times \text{Treated}_p + \varepsilon_{ept}, \tag{B.1}$$

Figure B.6: Stacked event studies: province-level results

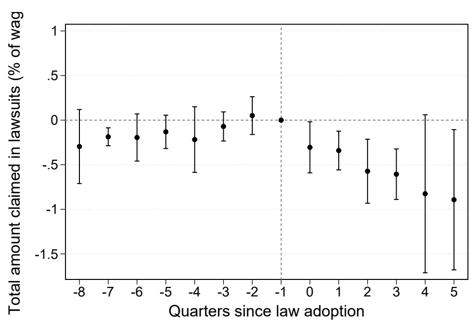
(a) Number of lawsuits (IHS)



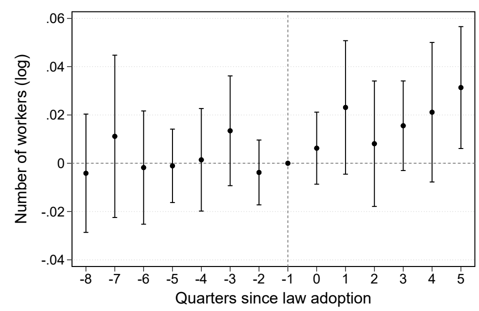
(b) Number of accidents (IHS)



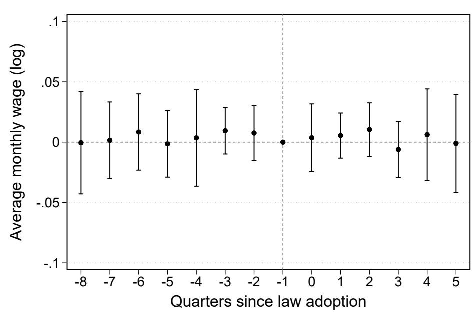
(c) Amount claimed (% of labor costs)



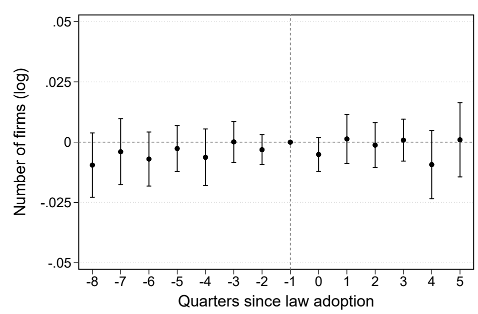
(d) Employment (IHS)



(e) Average monthly wage (log)



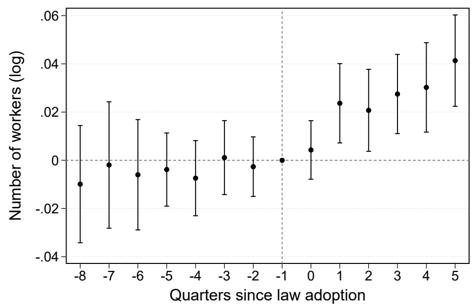
(f) Number of firms (IHS)



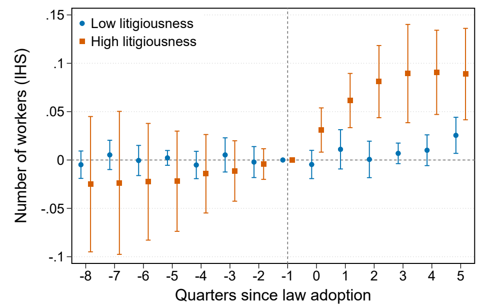
Notes: This figure plots the β_k coefficients from equation (B.1) using different dependent variables. The unit of observation is a province-by-quarter. Standard errors are clustered at the province level. Vertical bars represent 95% confidence intervals. The dependent variable in Panel (a) is the inverse hyperbolic sine transformation of the total number of lawsuits reported. The dependent variable in Panel (b) is the inverse hyperbolic sine of the total number of accidents reported. The dependent variable in Panel (c) is the amount claimed in lawsuits as a share of labor costs (total employment times average monthly wage). The dependent variable in Panel (d) is the inverse hyperbolic sine of the total number of workers. The dependent variable in Panel (e) is the natural logarithm of the average monthly wage. The dependent variable in Panel (f) is the inverse hyperbolic sine of the total number of firms.

Figure B.7: Stacked event studies: sector-by-province level results - labor market outcomes

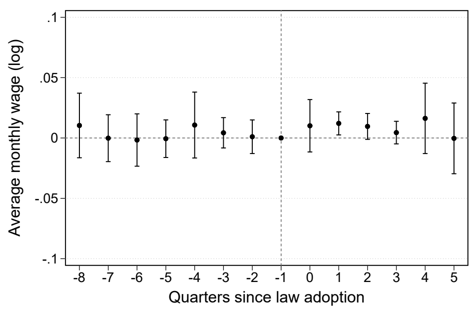
(a) Employment (IHS)



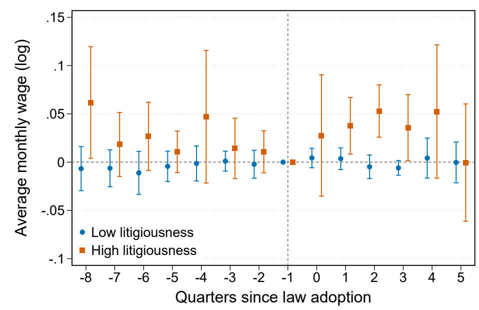
(b) Employment (IHS) - Heterogeneity



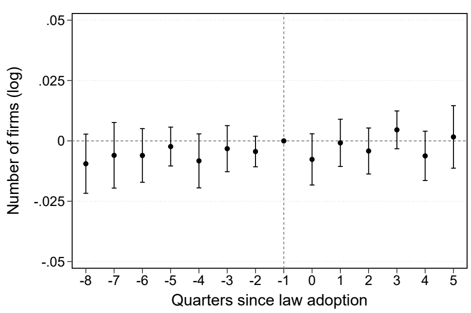
(c) Average monthly wage (log)



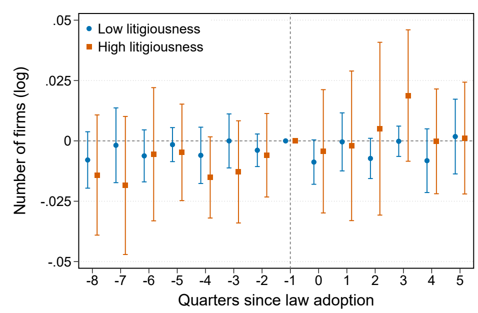
(d) Average monthly wage (log) - Heterogeneity



(e) Number of firms (log)



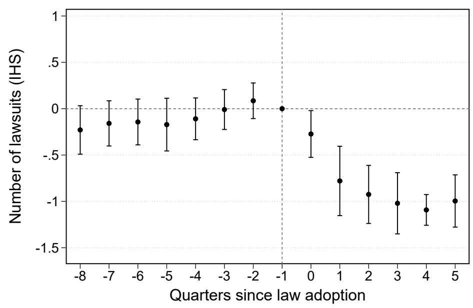
(f) Number of firms (log) - Heterogeneity



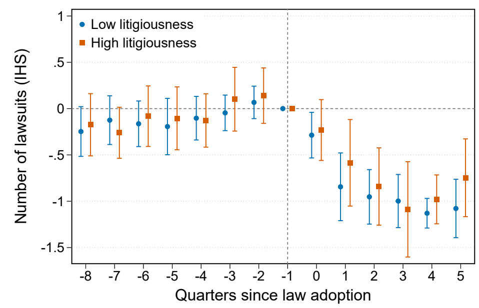
Notes: This figure plots the β_k coefficients from equation (B.1) at the sector-by-province-by-quarter level using different dependent variables. The unit of observation is a sector-by-province-by-quarter. Standard errors are clustered at the province level. Coefficients in orange correspond to the event study for sectors indicated as "high litigiousness" in figure 2.1: construction, mining, and manufacturing. Coefficients in blue correspond to the event study for the rest of the sectors. Vertical bars represent 95% confidence intervals. The dependent variable in Panels (a) and (b) is the inverse hyperbolic sine of the total number of workers. The dependent variable in Panels (c) and (d) is the natural logarithm of the average monthly wage. The dependent variable in Panels (e) and (f) is the inverse hyperbolic sine of the total number of firms.

Figure B.8: Stacked event studies: sector-by-province level results - lawsuits and accidents

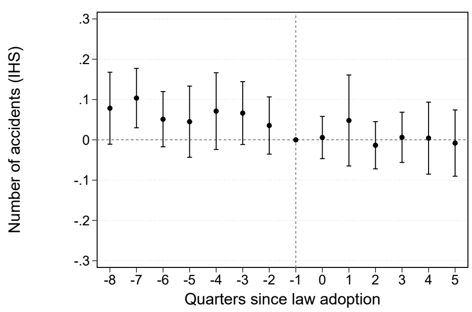
(a) Number of lawsuits (IHS)]



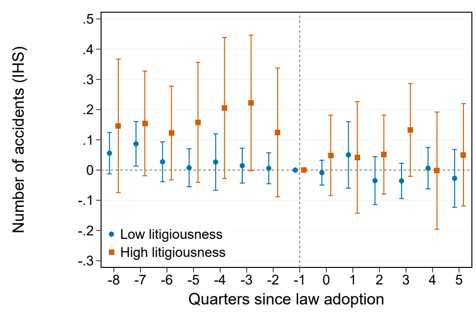
(b) Number of lawsuits (IHS) - Heterogeneity



(c) Number of accidents (IHS)

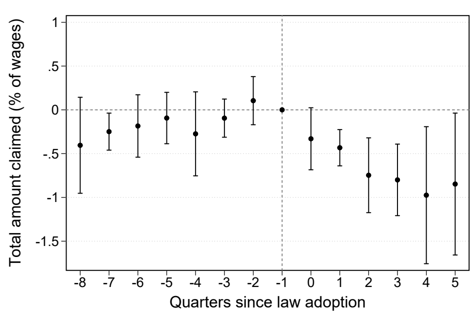


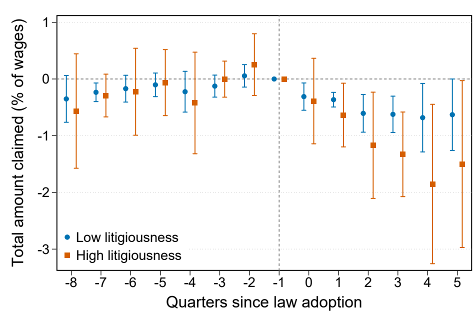
(d) Number of accidents (IHS) - Heterogeneity



(f) Amount claimed (% of labor costs)

(e) Amount claimed (% of labor costs)- Heterogeneity





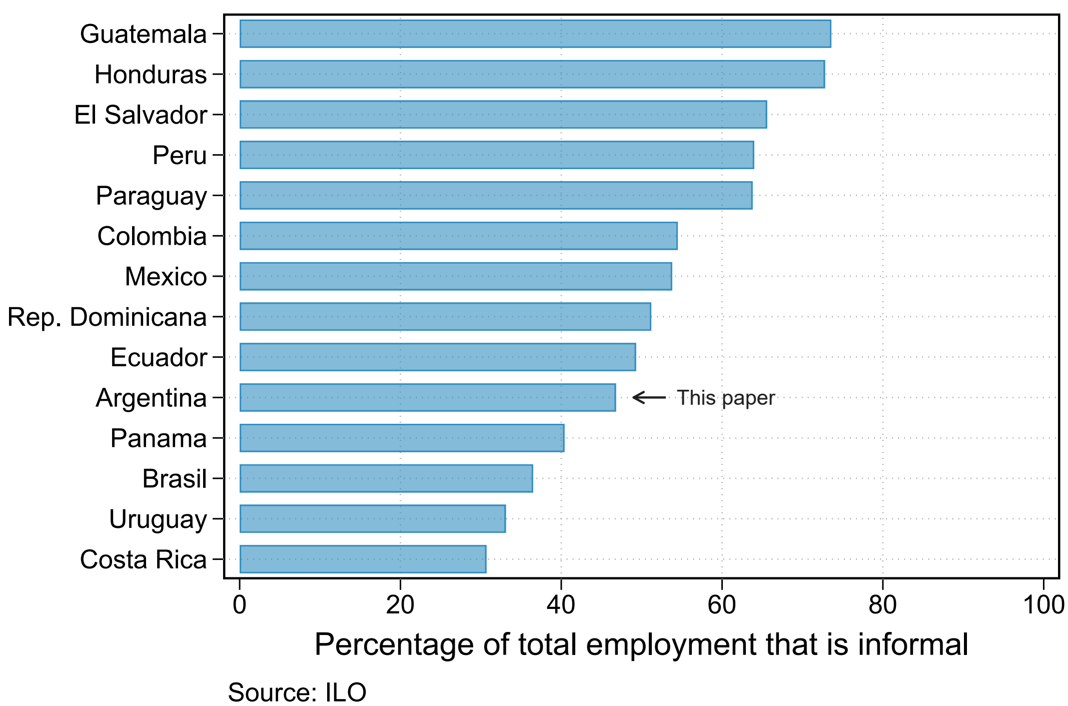
Notes: This figure plots the β_k coefficients from equation (B.1) at the sector-by-province-by-quarter level using different dependent variables. The unit of observation is a sector-by-province-by-quarter. Standard errors are clustered at the province level. Coefficients in orange correspond to the event study for sectors indicated as "high litigiousness" in figure 2.1: construction, mining, and manufacturing. Coefficients in blue correspond to the event study for the rest of the sectors. Vertical bars represent 95% confidence intervals. The dependent variable in Panels (a) and (b) is the inverse hyperbolic sine transformation of the total number of lawsuits reported. The dependent variable in Panels (c) and (d) is the inverse hyperbolic sine of the total number of accidents reported. The dependent variable in Panels (e) and (f) is the amount claimed in lawsuits as a share of labor costs (total employment times average monthly wage).

Appendix C

Appendix of Payroll Taxes and Informality: Evidence from Argentina

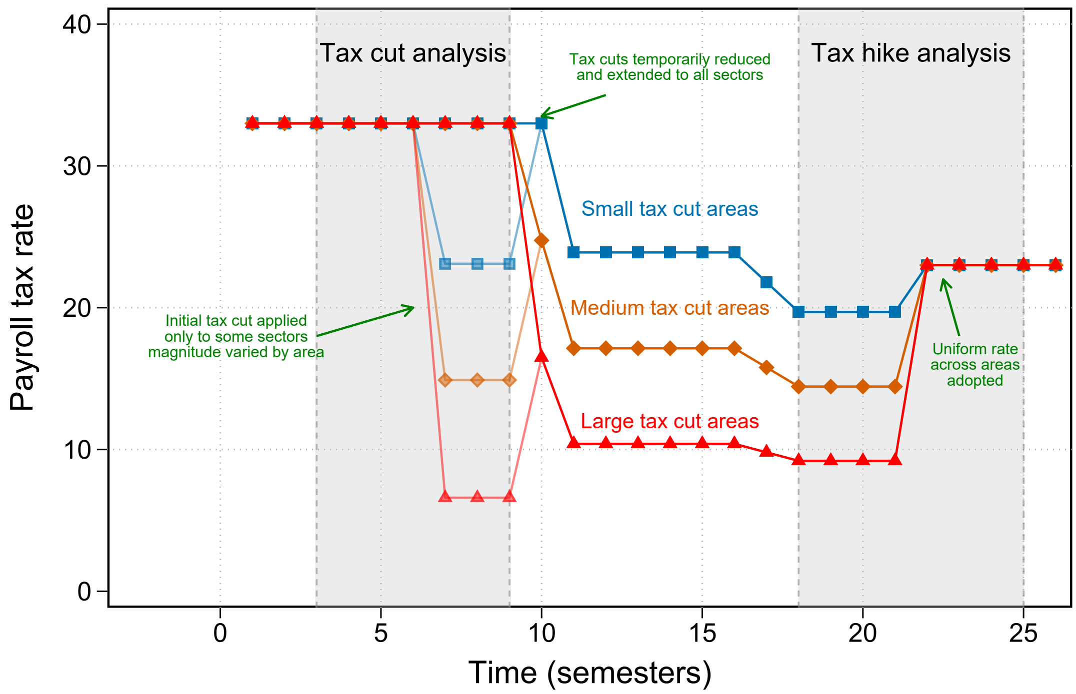
C.1 Additional figures

Figure C.1: Informality in Latin America



Notes: This figure shows estimates from the International Labor Organization for the percentage of total employment that is informal for several countries in Latin America. The country studied in this paper (Argentina) is highlighted.

Figure C.2: Timeline and variation example



Notes: This figure shows a stylized visualization of the variation of the payroll tax rate over time across different areas and sectors. In practice, there were 11 categories of payroll tax rate reduction, this figure shows three categories for illustrative purposes: (i) areas that received the smallest tax cuts (in blue), (ii) areas that received a medium-sized tax cut (in orange), and (iii) areas that received the largest tax cut (in red). The shaded areas indicate the sample periods used for the tax cut and tax hike analysis.

C.2 Snapshots of tax rates assignment and tax filing software

Figure C.3: Rates by category example

Resolución General D.G.I. 3.834 - Anexo IV - T04

TABLA DE ALICUOTAS DE CONTRIBUCIONES GENERALES Y REDUCIDAS SEGUN DTO. 2.609/93 Y SUS MODIFICACIONES

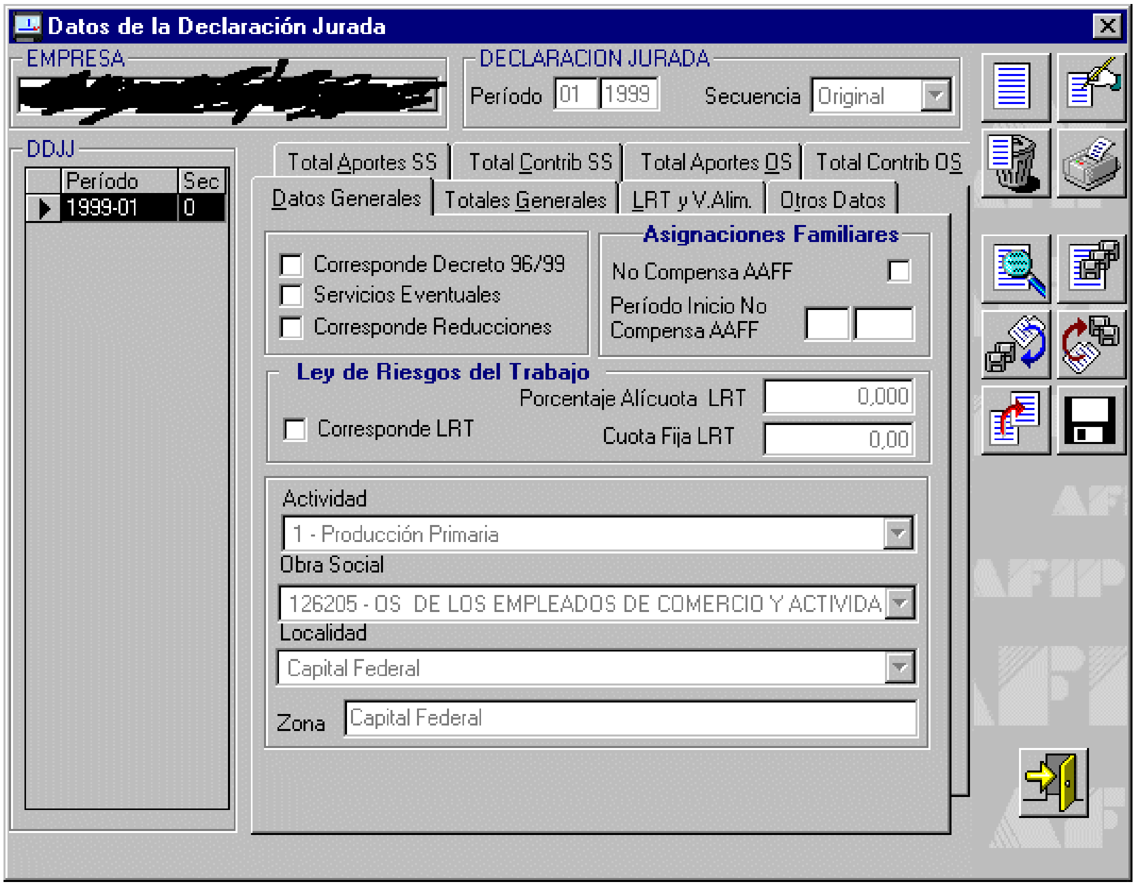
(Dto. 2.609/93: desde 7/94 hasta 2/95)

T04

% de reducc.	Contrib. de Seg. Soc.	Asig. famil.	Asig. fam. Zona Sur	F.N.E.	I.N.S.S.J.P.	Obra social
	A	b0	b1	c0	d	e0
0	16.00	7.50	3.00	1.50	2.00	6.00
30	11.20	5.25	2.10	1.05	1.40	4.20
35	10.40	4.87	1.95	0.97	1.30	3.90
40	9.60	4.50	1.80	0.90	1.20	3.60
45	8.80	4.12	1.65	0.82	1.10	3.30
50	8.00	3.75	1.50	0.75	1.00	3.00
55	7.20	3.37	1.35	0.67	0.90	2.70
60	6.40	3.00	1.20	0.60	0.80	2.40
65	5.60	2.62	1.05	0.52	0.70	2.10
70	4.80	2.25	0.90	0.45	0.60	1.80
75	4.00	1.87	0.75	0.37	0.50	1.50
80	3.20	1.50	0.60	0.30	0.40	1.20

Notes: This figure shows a snapshot of one of the tables in ordinances from the Tax Authority from which the payroll tax rates were digitized. The first column indicates the reduction coefficient for each category. The second column indicates the rate for social security contributions. The third and fourth columns indicate the rate for family allowances. The fifth column indicates the rate for unemployment insurance. The sixth column indicates the rate for contributions to healthcare for retired workers. The seventh column indicates the rate for contributions to healthcare for active workers.

Figure C.4: Tax filing software snapshot



Notes: This figure shows a snapshot of the software for filing payroll taxes that was used at the time. Note that the entry boxes for area and economic activity are fixed and tax filers could not change them using the software.

C.3 Robustness checks tables and figures

Table C.1: Robustness checks - tax cut

	=100 if informal		Hourly wage (log)	
	(1)	(2)	(3)	(4)
Panel A. Including self-employed				
Payroll tax rate	0.122**	0.124**	0.00156	0.00162*
	(0.0537)	(0.0509)	(0.000973)	(0.000842)
Controls		✓		✓
Observations	137059	135763	130780	129552
R Squared	0.234	0.323	0.255	0.370
Panel B. Including only wage earners with no missings				
Payroll tax rate	0.137**	0.302***	-0.00322**	-0.00219**
	(0.0673)	(0.103)	(0.00130)	(0.00104)
Controls		✓		✓
Observations	75702	75156	72862	72340
R Squared	0.199	0.281	0.340	0.458
Panel C. Area-by-time fixed effects				
Payroll tax rate	0.181***	0.151***	0.00121	0.00149
	(0.0597)	(0.0537)	(0.00113)	(0.000952)
Controls		✓		✓
Observations	97309	96623	93509	92859
R Squared	0.188	0.242	0.289	0.417

Notes: Standard errors clustered at the sector-by-area level are reported in parentheses. In columns 1 and 2 the dependent variable is a binary variable equal to 100 if the worker is informal and 0 if formal. In columns 3 and 4 the dependent variable is natural logarithm of the hourly wage. Payroll tax rate is the payroll tax rate at the sector by area level, measured from 0 to 100. Controls include an indicator for gender, age, age squared, and indicators for the highest degree of education achieved. All specifications include sector fixed effects, area fixed effects, and time fixed effects. Panel A includes self-employed workers in the sample, considering self-employed workers who have not completed high-school as informal. Panel B uses only observations from wage earners that have no missing values in informality, small firm, and recently hired. Panel C includes area-by-time fixed effects instead of area fixed effects and time fixed effects. * $p < 0.1$ ** $p < 0.05$ *** $p < 0.01$.

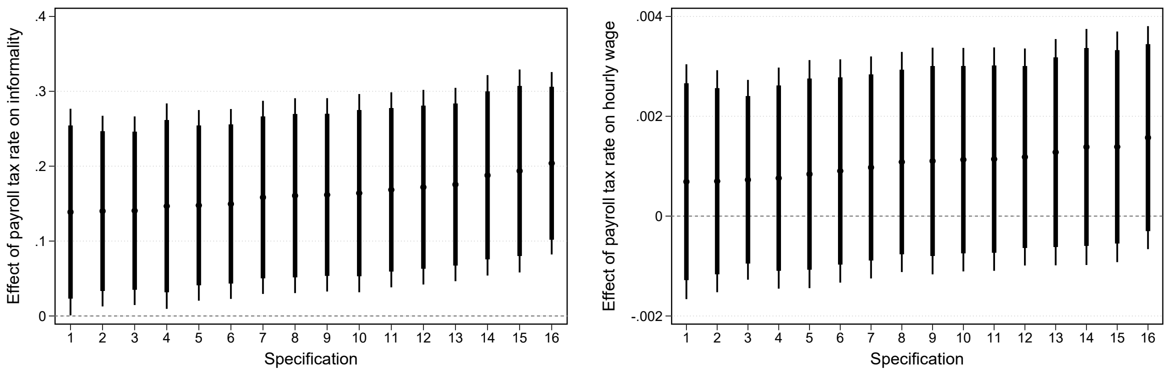
Table C.2: Robustness checks - tax hike

	=100 if informal		Hourly wage (log)	
	(1)	(2)	(3)	(4)
Panel A1. Including self-employed				
Payroll tax rate	0.0966	0.108	0.00286	0.00268
	(0.145)	(0.158)	(0.00323)	(0.00336)
Controls		✓		✓
Observations	203362	203362	180223	180223
R Squared	0.0236	0.274	0.100	0.337
Panel A2. Including self-employed (short-run and long-run)				
Payroll tax rate (short-run effect)	-0.0934	-0.0229	0.000530	0.000413
	(0.128)	(0.136)	(0.00241)	(0.00244)
Payroll tax rate (long-run effect)	0.346*	0.472**	0.00586	0.00396
	(0.182)	(0.227)	(0.00455)	(0.00462)
Controls		✓		✓
Observations	203362	203359	180223	180223
R Squared	0.0237	0.351	0.100	0.403
Panel B1. Including only wage earners with no missings				
Payroll tax rate	0.185	0.172	0.00238	0.00283
	(0.133)	(0.151)	(0.00303)	(0.00303)
Controls		✓		✓
Observations	138113	138113	125825	125825
R Squared	0.0295	0.211	0.105	0.391
Panel B2. Including only wage earners with no missings (short-run and long-run)				
Payroll tax rate (short-run effect)	0.00754	0.00788	-0.000480	-0.000299
	(0.118)	(0.124)	(0.00232)	(0.00234)
Payroll tax rate (long-run effect)	0.415**	0.386*	0.00605	0.00683
	(0.200)	(0.212)	(0.00422)	(0.00422)
Controls		✓		✓
Observations	138113	138113	125825	125825
R Squared	0.0296	0.211	0.105	0.391

Notes: Standard errors clustered at the sector-by-area level are reported in parentheses. In columns 1 and 2 the dependent variable is a binary variable equal to 100 if the worker is informal and 0 if formal. In columns 3 and 4 the dependent variable is natural logarithm of the hourly wage. Payroll tax rate is the payroll tax rate at the sector by area level, measured from 0 to 100. Payroll tax rate (short-run effect) is the interaction of the change in the payroll tax rate before and after the tax hike interacted with an indicator of the time period being 1 or 2 survey waves after the tax increase. Payroll tax rate (long-run effect) is the interaction of the change in the payroll tax rate before and after the tax hike interacted with an indicator of the time period being 3 or 4 survey waves after the tax increase. Controls include an indicator for gender, age, age squared, indicators for the highest degree of education achieved, and sector fixed effects. Panels A1 and A2 include self-employed workers in the sample, considering self-employed workers who have not completed high-school as informal. Panels B1 and B2 use only observations from wage earners that have no missing values in informality, small firm, and recently hired. * $p < 0.1$ ** $p < 0.05$ *** $p < 0.01$.

Figure C.5: Leave-one-out robustness check - tax cut

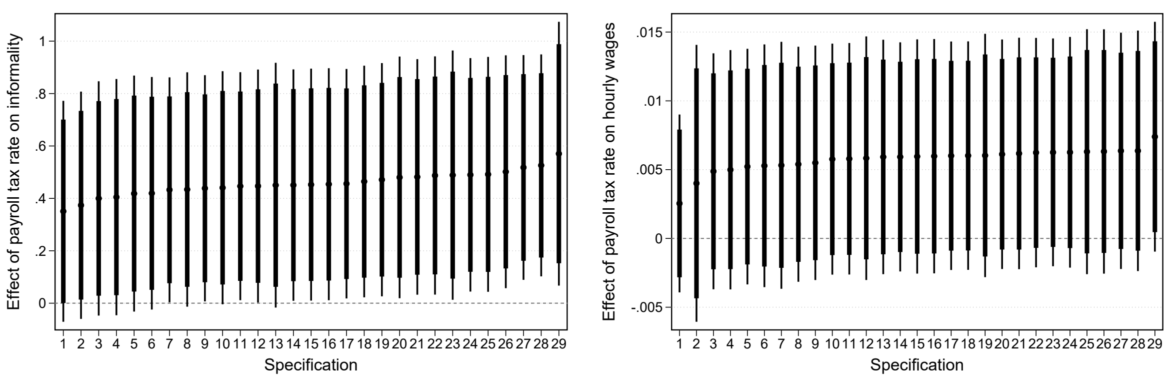
(a) Informality (b) Hourly wage



Notes: This figure shows several OLS estimates of the main coefficient from equation 3.2 for the tax cut period. Each coefficient corresponds to an estimate calculated by dropping one of the areas from the sample. Panel (a) reports the effect of the payroll tax rate on the probability of being informal. Panel (b) reports the effect of the payroll tax rate on the natural logarithm of the hourly wage. Standard errors are clustered at the sector-by-area level. Thick vertical bars represent 90% confidence intervals and thin vertical bars represent 95% confidence intervals.

Figure C.6: Leave-one-out robustness check - tax hike

(a) Informality (b) Hourly wage



Notes: This figure shows several OLS estimates of the coefficient for the long-run effect of the payroll tax rate from equation 3.2 for the tax hike period. Each coefficient corresponds to an estimate calculated by dropping one of the areas from the sample. Panel (a) reports the effect of the payroll tax rate on the probability of being informal. Panel (b) reports the effect of the payroll tax rate on the natural logarithm of the hourly wage. Standard errors are clustered at the area level. Thick vertical bars represent 90% confidence intervals and thin vertical bars represent 95% confidence intervals.

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