Essays in Formal Labor Supply and Intergenerational Mobility

by

Pablo A. Troncoso

(Under the Direction of Roozben Hosseini)

Abstract

In Chapter 1, I study the employment effects of a 2008 policy reform of the Chilean pension system. The reform increased pension benefits, changed the accrual rates, and relaxed the eligibility criteria for low-savings retirees. Using a difference-in-differences approach, I find that, on average, the reform increased labor force participation and hours worked for men aged 60–64 years by 15% and 4%, respectively. I find that a lack of pension knowledge is crucial in explaining the results. People with similar levels of pension wealth and monthly contributions behave differently depending on what they believe their pension assets and contributions are. I then develop a three-period model where workers supply labor. I conclude that the net impacts of the reform on labor decisions depend on pension wealth and workers' knowledge about the pension system. In Chapter 2, the paper studies the relationship between intergenerational economic persistence and preferences for the provision of public goods. We develop a simple theoretical model in which a public good is financed through proportional taxation, and that predicts a lower provision of public goods given an increase in the intergenerational earnings elasticity (IGE), which is a measure of the degree of economic persistence. We test this model using the results of the 2020 Chilean national plebiscite, which asked about the replacement of the standing constitution by a new one that would potentially expand the role of the state in the provision of public goods. Our estimates suggest the existence of a positive association between the IGE and the share of the vote against a new constitution, even after controlling for median income and income inequality. These findings are consistent with our model and suggest that sectors of society that exhibit higher degrees of economic persistence also show greater reluctance towards redistributive policies that increase public goods provision. Chapter 3 revisits the Two-Sample Two-Stage Least Squares (TSTSLS) method,

which is commonly used to estimate intergenerational mobility in the absence of parental earnings data. First, we decompose the TSTSLS intergenerational earnings elasticity (IGE) into the linked administrative data estimate, a projection bias, and a variance bias. We propose a parsimonious imputation procedure to eliminate the variance bias in the IGE and show that, under plausible conditions, the corrected TSTSLS IGE estimate provides a lower bound for the linked administrative IGE. Furthermore, we demonstrate that the uncorrected rank-rank correlation estimated through TSTSLS only exhibits projection bias, thus providing a lower bound to the linked administrative rank-rank correlation. Second, we use administrative data from a developing country to test our lower bound methodology through an Empirical Monte Carlo approach, confirming its validity.

INDEX WORDS: Administrative Data, Pension System, Pension Savings, Formal Workers, Intergenerational Mobility, Linked Administrative Data, Two-Sample Two- Stage Least Squares.

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Dedication

To my parents Patricia and Hernán, my siblings Marcelo and Claudia, my nieces Victoria and Laura and last but not least to Nicole who went through this journey with me.

A mis padres Patricia y Hernán, mis hermanos Marcelo y Claudia, mis sobrinas Victoria y Laura y finalmente a Nicole quien vivió esta hermosa experiencia conmigo.

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Chapter 1

Employment Effect of Means-Tested Program: Evidence from a Pension Reform in Chile

1.1 Introduction

In 2008, the Chilean government reformed the means-tested portion of its pension system. The reform relaxed the eligibility criteria, changed the accrual rate, and increased the pension income of its beneficiaries.[1] This paper studies the impact of this reform on labor supply. Since 1981, Chile has had a fully funded pension system. Under this system, the retirement benefits of the participants are funded through their individual retirement accounts (similar to 401(k) in the United States). However, only 64% of workers contribute to this system; many "informal workers" (e.g., self-employed workers or those with no permanent contracts) do not contribute to it. These workers rely on pension transfers from the government in retirement. Moreover, the government also makes a transfer to low-income participating workers with small pension assets at their retirement. These "low-income formal workers" and informal workers were the target of the 2008 reform.

[1] The accrual rate is the fraction at which pension benefits are built up in a fully funded pension system. The larger the percentage, the better the pension income people will receive.

In this paper, I use two data sets to study the impact of this reform on labor force participation and hours worked. The first is a representative cross-sectional survey with 12 rounds between 1992 and 2017. The second is a unique data set that combines a representative household panel survey with six rounds between 2002 and 2019, and monthly administrative records starting in 1981. These linked data allow me to observe pension wealth and the number of monthly

contributions to the pension system from two different sources: the actual values from the administrative records and the self-reported values from the panel survey. The information derived from these data sets is crucial to categorize people in different groups of workers who face alternative incentives because of the reform.

To obtain causal estimates, I adopt a difference-in-differences (DID) approach with multiple time periods using Callaway and Sant'Ana's estimator (Callaway and Sant'Anna 2021). In this setup, the average treatment effect on the treated (ATT) is well defined even when there are more than two time periods and heterogeneous impacts across time. The control group is defined as people affiliated with the Armed Forces. Military workers have their own pension system and, on average, start claiming pension income at age 45. Although some might work as civilians and contribute to the fully funded pension system, they are not eligible for a pension supplement (neither pre-reform nor post-reform). Hence, the 2008 reform does not affect these workers. This unique condition allows me to identify causal changes in the labor supply due to the reform.

On average, the pension reform caused men aged 60–64 years to increase their labor force participation and hours worked per capita by 14% and 5%, respectively. I find no positive significant effects for younger male or female workers. Furthermore, I document that the reform has heterogeneous effects on workers depending on their pension balance and contribution history. More specifically, I find zero or negative impacts on labor supply for people who would have been eligible for benefits before the reform. For them, standard theory would suggest either a positive or a negative impact on labor supply because income (benefits) and substitution (accrual rate) effects go in opposite directions. More puzzling, however, is that among male workers who would not have qualified for benefits pre-2008 but are eligible post-2008, I find positive effects on hours worked and labor force participation. These positive impacts are surprising because, contradict standard theory, where both income and substitution effects move in the same direction, decreasing labor supply.

In order to understand this result, I look at individuals' level of understanding of the pension system and its reform. To this end, I document people have lack of knowledge about the scheme that determines pension supplements mainly because of its high complexity. Later, I compare the actual pension wealth people have accumulated together with the number of years of contribution with their self-reported pension wealth and contribution history. I find that on average, people underestimate their pension assets and slightly overestimate their number of monthly contributions. I also find that people overestimate their contribution rate, which may lead to a higher self-reported pension wealth

even if workers underestimate their contribution history. Finally, I show that people with similar pension assets and contributions behave differently in terms of labor force participation and formality, depending on their beliefs. Indeed, people make labor decisions according to their perceived incentives, which do not always coincide with the actual ones.

I perform several robustness checks to show that the empirical results do not depend on particular assumptions. In that sense, I address the possible confounding effects due to the 2008–2009 financial crisis. I estimate scenarios such as calculating the impacts without 2008 data, controlling by occupation and sector fixed effects, and incorporating losses into pension savings due to the crisis. The main conclusion remains valid in all of these cases. I also estimate the labor supply responses assuming that the policy reform took place in a different year, as a placebo test. I find zero effects on labor force participation and hours worked.

Finally, I develop a three-period model to rationalize my empirical findings. In this model, workers consume, and decide non-pension assets, and labor supply in the formal or informal sector. They receive a pension income from the fully funded pension system and potentially (if they are eligible) also a pension supplement from the government. Using this simple model, I show that for those who underestimate their pension wealth, the reform can lead to an increase in labor supply because of the wealth effect. Moreover, the reform can lead to a rise in labor supply because of a dominant substitution effect for those who overestimate their pension contribution rate. Therefore, people's misestimation of the amount of their pension wealth and contribution rate can provide a mechanism to explain how a pension reform leads to an increase in labor supply among a large portion of Chilean workers.

Literature Review: This research is related to several strands of literature. The macro-labor literature shows how people change their labor and saving decisions in response to policy changes. For example, several papers have estimated the labor supply responses due to cuts in Social Security benefits and increases in the full retirement age in the United States (Behaghel and Blau 2012; Mastrobuoni 2009, 2011; Pingle 2006; Purcell 2020; Duggan et al. 2021; Blau and Goodstein 2007; Gruber and Wise 1998, 2009.). Most of the papers have focused on workers close to retirement age (Liebman et al. 2009; Diamond and Gruber 1999), and only a few articles on younger workers, 45–56 years old (French et al. 2022; Bovini 2019; Lauletta and Bérgolo 2022). I complement this literature by calculating the impacts on labor supply for younger (i.e., 40–49 years old) and older workers (i.e., 50–64 years old). They will receive benefits soon but still have time to modify their pension outcomes by

making different labor market decisions. One would expect to see higher impacts of the reform among them than among workers who are 25–40 years old (manoli2016nonparametric,song2007new,liebman2009labor).

Some empirical papers have estimated the effects of the 2008 Chilean reform on people older than 65 years. They found little evidence of crowding-out of private transfers, an increase in debt, and an increase in unemployment (Behrman et al. 2011; Encina 2013; Bucciol et al. 2020). Similarly, 65-year-old or older workers could be already retired. Under these circumstances, labor decisions seem to be more affected by alternative reasons such as pension income, replacement rate, or health status than expected benefits.[2]

[2] Erosa et al. 2012 found social security rules (e.g., retirement age, lifetime earnings, and more) are the main factors that explain the differences in labor supply for older workers across Europe and the United States. Alonso-Ortiz 2014 found that the replacement rate and the implicit tax on wages when retirees work are the most important features that explain discrepancies in labor supply for people aged over 55 years across Organization for Economic Cooperation and Development (OECD) countries.

The impacts of means-tested programs on employment and asset accumulation have been studied broadly from a theoretical perspective (Cohen and Friedman 1972; Feldstein 1987). Empirical studies have quantified the effects of means-tested pensions on savings and labor decisions for Australia, the United Kingdom, and the United States (Sefton et al. 2008; Sefton and Van De Ven 2009; O'Dea 2018; Kudrna and Woodland 2011; Tran and Woodland 2014; Dunsford and Rice 2004). As in Wong 2016, I find heterogeneous effects of the 2008 Chilean reform on employment, depending on pension accumulation and number of monthly contributions. The main difference is the chosen control group: for Wong 2016, it comprises people who are not poor, whereas my control group comprises people who are affiliated with the Armed Forces. Potentially, military workers are a better control group because they are not eligible for pension benefits no matter their current income level or previous labor decisions. Assigning these workers to the control group allows me to identify causal estimates caused by the 2008 reform. Furthermore, I have access to more extensive data to control for year-fixed effects and use a DID approach that allows heterogeneous treatment effects over time. Given the rich data, I can analyze different groups of workers that face changes in benefits and accrual rates because of the reform.

As part of the pension literature, my work extends and complements previous papers that study fully funded pension systems. Attanasio et al. (2011) and Otero (2015) estimated the 2008 Chilean reform's impacts by using a regression analysis that includes expected pension wealth and expected accrual rate (by the respondent) at retirement age as dependent variables.[3] The assumption is that people behave rationally and decide their labor supply by considering those variables that were modified because of the change in the scheme. I show in Appendix A.6 that this approach produces biased estimates because of low prediction power when computing expected pension wealth and expected ac-

[3] Becerra (2019) used a similar approach to account for the change in pension wealth and accrual rates after the reform.

crual rate. Indeed, when I use actual values for these two variables, the results are not statistically significant.

Some papers have studied the Chilean pension system using structural dynamic models (Joubert and Todd 2011; Joubert 2015; Joubert and Todd 2022; McKiernan 2021; Garcia 2015; Sabat 2022)[4]. For instance, Joubert and Todd 2011 found, on average, adverse impacts of the reform on the fraction of men older than 60 years working in the formal sector. I, however, rely on an empirical approach to estimate the causal impacts of the labor reform on labor supply. I use a similar data set but at least 10 additional years of information. Additionally, I include the monthly information, because the requirements to apply for government benefits include pension assets and contributions that vary per month.

[4] Cabezon 2022 used the 2008 reform as a quasi-experimental source of variation to identify the efficiency of the Chilean pension system and found that an increase in benefits' progressivity has a significant efficiency cost.

Finally, I conclude that pension knowledge is crucial to understanding why some workers do not behave as expected. For instance, there is evidence of suboptimal decisions of consumers in health insurance markets (Handel 2013; Handel et al. 2019; Handel and Kolstad 2015). There is also a considerable literature that has documented people having a basic understanding of pension systems and financial markets (Gustman and Steinmeier 2005; Lusardi and Mitchell 2011; Mitchell and Lusardi 2011). I find people have wrong perceptions of the pension system and the scheme that determines the pension supplements. This lack of knowledge could explain the differences in labor supply responses for structural dynamic models because of the 2008 reform. Some authors assume people behave rationally, knowing every detail in the pension supplement scheme, which implies a complete take-up rate. In contrast, I document that workers have wrong perceptions, and the take-up rate is around 50% among eligible retirees. I complement and extend those papers by showing strong evidence of a lack of pension knowledge in Chile and incorporating this fact into a theoretical model and empirical analysis.

The rest of the paper is organized as follows. Section 1.2 describes the Chilean pension system. Section 3.4.2 describes the data. Section 1.4 shows crucial patterns about the Chilean labor market. Section 1.5 shows evidence of the lack of pension knowledge among Chilean workers. Section 3.4.3 establishes the empirical approach. Section 1.7 shows the results using the cross-sectional and the administrative panel data. Section 1.8 develops a three-period model that rationalizes the empirical results. Finally, Section 1.9 presents the conclusions of this work.

1.2 The Chilean Pension System and the 2008 Reform

In 1981, the Chilean government transitioned from a pay-as-you-go (PAYG) pension system to a fully funded (FF) pension system.[5,6] This transition affected only civilians, while workers in the Armed Forces stayed in the PAYG system.[7] Military-affiliated, or all "military workers", claim and collect a pension, on average at age 45. While some may continue working in the private sector and contribute to the FF system, they are not eligible for additional government benefits.[8]

The new civilian plan was meant to establish a fully funded pension system based on personal capital accounts and managed by private companies known as Pension Fund Administrators (AFP). Simultaneously, the government provided guarantees of pension savings, and regulated and supervised the market's functioning through the Superintendency of Pension Fund Administrators. Under this new system, civilian employees could select which AFP they contributed to. The benefits they had saved under the PAYG system were accounted for using a "recognition bond" payable upon retirement (de Mesa et al. 2006; Joubert and Todd 2011).

The fully funded system has defined contributions but undefined benefits. This implies that workers know how much they have to contribute when working but are unsure of their pension income when retired. Contributions to the FF system are mandatory for formally employed workers and voluntary for informal workers (i.e., self-employed workers or people with no permanent contracts who pay taxes but do not contribute to the pension system).[9] Formal workers pay 10% of their monthly wages into a tax-deferred pension account. Contributions are capped; for example, in 2017, the monthly threshold was approximately USD 3,100, but this affected less than 5% of contributing workers. In addition to the pension contribution, formal workers must contribute towards health services, disability and survivorship insurance, and an AFP commission.[10] In 2017, these contributions represented approximately 7%, 0.8%, and 1.3% of monthly wages, respectively.

Regarding benefits, men and women can receive old-age pensions at 65- and 60-years-old, respectively. At that age, the total balance in the pension account determines the benefit they receive. People can choose either a programmed withdrawal or an annuity, but annuities are only available to those with high pension accumulations who are unlikely to receive government transfers. A programmed withdrawal provides a front-loaded draw-down of pension savings, according to a regulated schedule paid until funds run out. In the first years,

[5] In Appendix A.1.1, Table A1 and Table A2 show a list of countries with a FF pension system and some details of their government benefits.

[6] McKiernan (2021) studied the Chilean transition from a PAYG pension system to a FF pension system.

[7] People who work in the army, navy, marine, air force, or who worked as a correctional officer or police officer stayed in the PAYG pension system. I use "Armed Forces" or "military" to denote all of these groups.

[8] By the laws number 21,190 and 20,255, people who either work in, retire from, or receive survivor benefits upon the death of a family member who worked in the Armed Forces are not eligible for government transfers (?. I denote as "affiliated" with the Armed Forces a person who falls in any of these three categories.

[9] The workforce is 64% formal. Only 5% of informal workers contribute to the pension system.

[10] The AFP commission has been decreasing over time, from 4% in 1988 to 2.6% in 2008 when the pension reform took place.

retirees start receiving around 7% of their total pension account funds, and the percentage declines over time (Illanes et al. 2019).

Before 2008, people with low pensions could apply for one of two government benefits. The first was the *Assistance Pension* (Pensiones Asistenciales in Spanish, or PASIS), which was approximately a third of the monthly minimum wage (USD 100). To be eligible for this transfer, the worker had to have a pension income below the PASIS amount; for example, in 2008, around 60% of men aged 60–64 years satisfied this condition. The second benefit was the *Guaranteed Minimum Pension* (Pensión Mínima Garantizada in Spanish, or MPG), which was twice as much as PASIS. MPG recipients had to have a pension income below the MPG amount and 20 years of contributions to the pension system. To receive either or both benefits, a person had to be older than 65 years and have a total income less than that of the 40% percentile of the population.[11]

[11] If people stop working, the reduction in earnings will make most of them satisfy this requirement.

Figure 1.1a shows the benefits, thresholds, and different incentive groups of the distribution prior to the 2008 reform. The self-financed pension (b) is represented on the x-axis, and the self-financed pension plus government benefits ($b + G$) is on the y-axis. The PASIS benefit is depicted in red. It goes from zero to x_1. Hence, any individual who has a pension income lower than x_1 ends up with y_1.[12] I refer to this segment of the distribution as Group I, or non-contributory pension beneficiaries. The MPG is represented by the blue line. The main difference between the two is that workers needed at least 20 years of contributions to obtain the MPG (Group II, or contributory pension beneficiaries). Thus, workers whose old-age income was lower than x_2 and satisfied the contribution requirement had y_2 as total old-age income. The PASIS and the MPG are top-up benefits, implying that given an increase of one dollar in the self-financed pension, the government will decrease the transfer by one dollar (i.e., the accrual rate is zero).

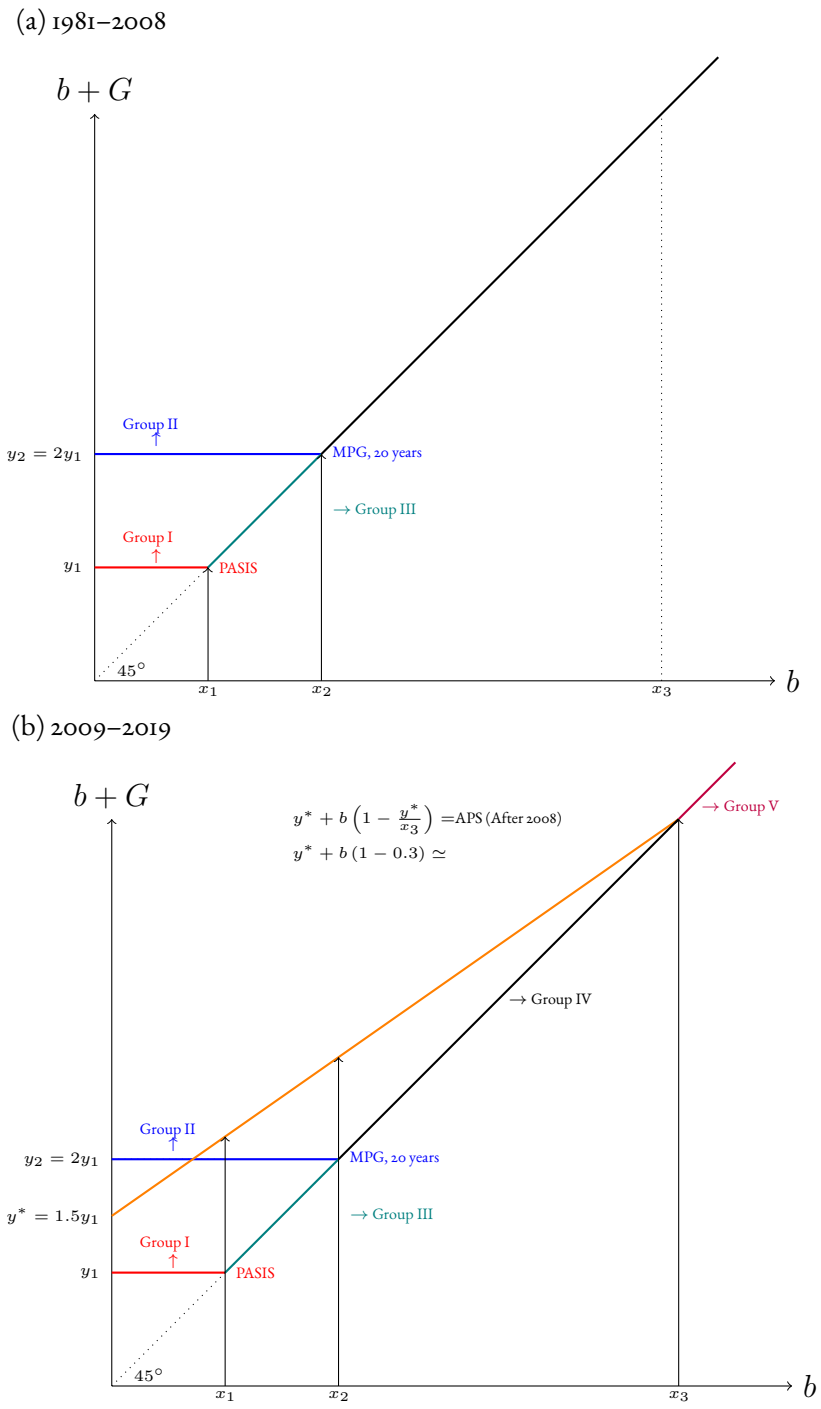
[12] This is because the person does not have enough years of contributions to receive the MPG.

Note that a worker who had 20 years of contribution received the MPG even if the worker's self-financed pension is to the left of x_1. However, retirees with that number of years of contribution usually received between x_1 and x_2 (more details in Section 1.4). On the other hand, people with pensions between x_1 and x_2 with fewer than 20 years of contributions received no government benefits. I refer to this segment of the distribution as Group III, or year-threshold workers. All retirees with a pension income greater than x_2 also received no benefit.

In 2008, after 27 years without significant pension system changes, the Chilean government introduced a critical reform. The Solidarity Pillar (Pilar Solidario) simplified the pension scheme and eliminated the contribution requirement. Furthermore, it increased the benefits for people with low pensions among retirees and expanded the coverage from the bottom 40% to 60% of the

total income distribution. Figure 1.1b illustrates the new scheme. The new level of transfers is given by the orange line, called the *Solidarity Pension Supplement* (Aporte Previsional Solidario, APS), which goes from zero pension income to the *Maximum Supplemented Pension* (Pensión Máxima con Aporte Solidario, PMAS), which is represented by x_3. Notice that the new level of benefits for people without a pension income has an intrinsic relevance. It is represented by y^* and is 50% higher than y_1. In the literature, y^* is usually called the *Basic Solidarity Pension* (Pension Básica Solidaria, PBS) because it is aimed at people who have never contributed to the system.

Figure 1.1: Government Benefits, pre- and post-2008



Note: Panel (a) shows government transfers before 2008. PASIS and MPG benefits are represented by the red and blue lines, respectively. Here b is self-financed pension, and G is government benefits, so $b + G$ is the total pension income. Panel (b) shows the new government transfer scheme (APS), in orange. PASIS and MGP are also included, to facilitate the comparison.

The final observation about the new scheme is that the APS is not a top-up benefit. Indeed, the APS can be written as follows:

$$APS(b) = \begin{cases} y^* + b \cdot \left(1 - \frac{y^*}{x_3}\right) \; if \; b \leq x_3, \\ b \qquad\quad if \; b > x_3, \end{cases} \tag{1.1}$$

where $\frac{y^*}{x_3} \simeq 0.3$. Thus, for each additional dollar accumulated in the pension account, the government reduces the transfer by 30% of a dollar. This holds for all pension incomes below x_3.

Recipients from the old scheme do not need to apply for benefits again. Hence, retirees in Group I jumped from y_1 to $APS(b)$, and those in Group II moved from y_2 to $APS(b)$ if they had more than 20 years of contributions. Both groups face a higher value of the benefit (income effect) and a higher accrual rate (substitution effect). The effects of the reform on labor supply are unclear: the income effect suggests people should work less, but the substitution effect suggests people should work more. People in Group III had to apply for the APS when the new scheme became available. For this group, both income and substitution effect move in the same direction, and according to theory, labor supply should decrease.

The 2008 reform creates two new interesting groups. The first is workers with a pension income above x_2 but below x_3. They were not eligible pre-2008 because their pension income was too high, but after the reform, they can apply for APS. I refer to this segment of the distribution as Group IV, or pension threshold. The second is Group V, which is composed of people with pension incomes above x_3 who were never eligible for government transfers (neither pre- nor post-2008). The theoretical impacts of the reform on labor supply for these two groups are different. Group IV faces similar incentives to Group III and, therefore, labor supply should decrease. Group V does not see any modifications in its budget constraint, so no changes to hours worked and labor force participation are expected. Finally, people who worked in the military might contribute and retire from the FF system at age 65 for men and 60 for women (Group VI). However, they are not eligible for government transfers no matter the level of pension income and the number of monthly contributions.

As expected, the number of recipients increased after the 2008 reform. The number tripled in 10 years, with a growth rate of 50% in the first year. Furthermore, pension payouts doubled from 0.25% to 0.54% of the GDP in 2009. The main reason for the increase was that many people had low pension amounts and were not covered by the MPG, especially women, who tend to take time

off work to look after their family. The excessive fiscal burden triggered a tax reform in 2014.

1.3 Data

This article uses two data sets. The first is a representative cross-sectional survey called the *National Socioeconomic Characterization Survey* (Encuesta de Caracterización Socioeconómica Nacional, CASEN, in Spanish). It has 12 rounds between 1992 and 2017, and each wave has information about age, education, formal sector employment, if part of the Armed Forces, and non-pension assets (specifically, housing).[13] Unfortunately, it does not have any information about pension savings or about previous labor market experience. However, CASEN allows for estimating aggregate impacts on the labor force and the fraction of formal workers, because informal workers are included in the data. The final sample contains 1.25 million observations, with 49,273 men between 60 and 64 years old.[14] Table A3, Figures A.1, A.2, and A.3 in Appendix A.1.1 show the number of observations and educational distribution of the total male population and that of men aged 60–64 years. Similarly, Table A37 in Appendix A.5 presents the final sample for the female population, with 1.3 million observations, where 56,584 women are between 60 and 64 years old.

The second data set comes from merging an extensive, nationally representative longitudinal household survey, the *Social Protection Survey* (Encuesta de Protección Social, SPS); the *Affiliate Pension History* (Historia Previsional de Afiliados, APH), a complete administrative record for workers in the formal sector after the FF system was created; and the *System of Consultations and Offers of Pension Amounts* (SCOMP),[15] an administrative record of people who are eligible for annuities. I refer to this merged data set as SPS-APH-SCOMP.

The SPS has six rounds: 2002, 2004, 2006, 2009, 2015 and 2019.[16] In each wave, household heads self-report information about age, education, formal sector employment, wages, and non-pension and pension savings.[17] Additionally, demographic information was collected for every member of the household. The APH contains information about monthly employment, number of years of contributions to the pension system, formal wages, pension savings, and old-age income for every household head in the SPS since joining the fully funded pension system. Hence, for household heads who started their working life during the FF pension system, I have data on wages, contributions, and pension savings for almost 40 years. The SCOMP has the types of annuity contracts people apply for. For example, some characteristics are the choice of a guarantee period to provide payouts after death or a deferral of payments, among others.

[13] The 12 waves are 1992, 1994, 1996, 1998, 2000, 2003, 2006, 2009, 2011, 2013, 2015, and 2017. There was an additional round, in 1990, but it does not have all the necessary variables to warrant its inclusion in the analysis. Finally, wave 2020 was released in 2021, and it has crucial differences from the previous ones because of the Covid-19 pandemic, and was excluded from the analysis.

[14] Using weights reveals the sample accounts for 98.4% of the national male population.

[15] Sistema de Consultas y Ofertas de Montos de Pensión in Spanish.

[16] There is an additional round, in 2012, but it is not a nationally representative sample.

[17] I use pension savings or pension wealth to account for the mandatory pension wealth in the system. Fewer than 5% of people also have voluntary contributions, but these resources do not play a role in qualifying for pension supplements.

Workers are eligible for annuities when their pension savings exceed a certain threshold. I observe all household heads who satisfy this constraint even if they eventually choose a programmed withdrawal. Finally, the date of birth and death are available for every household head. For more details, see Section A.2.1 in Appendix.

The SPS-APH-SCOMP sample comprises 20,867 men. Among them, 2,212 are affiliated with the old PAYG pension system for civilians. These are workers who usually retired before the 2008 reform. Because of the different rules of the old system, it is almost impossible to categorize these people into the groups mentioned earlier; therefore, I excluded them from my data set. It is important to highlight that people who moved from the civilians' PAYG pension system to the fully funded pension system are included in my data set. I have the value of their recognition bonds and the details of their monthly contributions to the new system. The final sample has 17,078 men, of which 3.8% are affiliated with the Armed Forces (see Appendix A.2, Table A14 for details). Similarly, Table A45 in Appendix A.5 presents the final SPS-APH-SCOMP sample for women.

Aggregate effects in the population are measured using CASEN, whereas detailed impacts on Groups I–IV can be observed using SPS-APH-SCOMP. The labor market outcomes are labor force participation, hours worked, formal worker (i.e., contributed to pension system) status, and pension savings. In the case of CASEN, formality is observed only once for each individual, while in SPS-APH-SCOMP, I know all formal jobs since 1981.

1.4 Formal Labor Supply in Chile

Like many developing countries, Chile has significant levels of informality in almost all occupations and industries. Understanding and quantifying these characteristics would shed light on workers' incentives and trade-offs when a reform modifies government transfers. Table 1.1 shows the labor transition matrix for men aged 20–64 years between 2000 and 2003 based on CASEN. Wave 2003 is the only round that asked about previous labor supply. Thus, it is the best proxy available to measure the Chilean labor market's flexibility between formal and informal jobs. Among people who were out of the labor force in 2000, 57.4% were still out of it three years later, and moving to formal or informal employment seemed to have similar probabilities. Among the unemployed, 17.3% stayed in the same condition, but 43.3% of them transitioned to a formal job. Among formal workers in 2000, 77.6% continued contributing to the pension system in 2003. Informal workers seemed to be the least flexible ones; 86.1% continued to be informally employed. Note that 50.4% of men aged 20–64 years

worked in the formal sector in 2003. This number represents around 58% of the total labor force participation, and 64% of workers. McKiernan 2021 found similar numbers using SPS. Table A38 in Appendix A.5 shows the transition matrix with similar numbers for women. Bracha and Burke (2016) found that 20% of workers in the United States are informal.

Table 1.1: Transition Probability Matrix, Men Aged 20–64 Years (CASEN 2003)

	Current Period: 2003			
Previous Period: 2000	Out of labor force	Unemployed	Formal	Informal
Out of labor force	57.4	10.1	18.0	14.5
Unemployed	7.8	17.3	43.3	31.6
Formal	2.8	7.0	77.6	12.6
Informal	3.0	2.9	8.0	86.1
Total	13.3	7.5	50.4	28.8

Note: The transition probability matrix is calculated using CASEN 2003 concerning current and previous observed activities: out of labor force, unemployed, formal, and informal. The sample contains men aged 20–64 years. Weights are used.

Furthermore, informal workers are employed in different occupations and industries. Table A4 and Table A5 in Appendix A.1.1 show the fraction of formal workers for men aged 20–64 years across occupations and industries, respectively. In 2003, among men aged 20–64 years, occupations such as clerical staff, and engineers and architects have the lowest informality ratio, with 11.7% and 19.53%, respectively. The two industries with the highest formality ratio are mining, and finance and real estate, where more than 78% of men aged 20–64 years are formal. Table A39 and Table A40 in Appendix A.5 show the fraction of female workers aged 20–64 years across occupations and industries, respectively. Although there are some changes in the percentages with respect to the male workers' distribution, the same occupations and industries present the highest levels of formality for female workers.

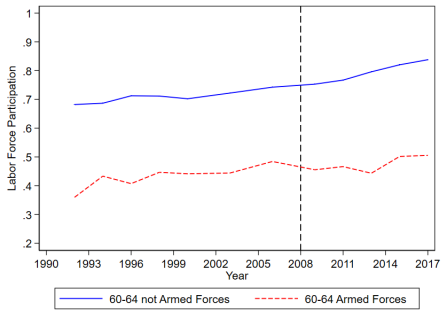
Using CASEN reveals that 4.8% of men aged 60–64 years are affiliated with the Armed Forces (See Table A6 and Figure A.4 in Appendix A.1.1). Some of them continue participating in the labor market after retiring from their PAYG pension system. I construct hours worked per capita and labor force participation for civilian men aged 60–64 years as well as for those affiliated with the Armed Forces. Figure 1.2 shows the results. As can be seen in Figure 1.2a, hours worked per capita stayed relatively flat for people not affiliated with the military, around 130 hours per month before 2008, but after the reform, they increased every year, reaching 155 monthly hours (20% increase). On the other

hand, people affiliated with the Armed Forces worked around 80–85 hours per month both before and after the reform. Figure 1.2b shows similar results for civilians' labor force participation, which increased by 21% after the 2008 reform, while for people affiliated with the military, labor force participation increased by only 3%.

Figure 1.2: Hours Worked and Labor Force Participation, Men Aged 60–64 Years (CASEN)

(a) Monthly Hours Worked per Capita

(b) Labor Force Participation



Note: Panel (a) shows the monthly hours worked per capita, which is calculated for everyone (unconditional on being employed). Panel (b) depicts labor force participation. Each panel shows a different line for civilians and people affiliated with the Armed Forces. Both indexes are calculated for each year using the corresponding weights. Vertical line represents when the Solidarity Pillar reform occurred.

Using the SPS-APH-SCOMP data, I can observe several characteristics about workers, especially about their level of attachment to the pension system. Figure 1.3 shows the number of months in the formal sector, the level of pension savings, the number of months in the labor force, and the distribution of people within the six incentive groups from Section 1.2 for men aged 60–64 years in 2008. Figure 1.3a depicts the number of monthly contributions, showing that around 45% of the sample has fewer than 12 monthly contributions in the formal sector. Furthermore, less than 25% satisfy the MPG requirement of having 240 monthly contributions. Figure 1.3b shows the distribution of pension savings. About 61% of workers have pensions low enough to qualify for PASIS, and an additional 10% have pensions low enough to qualify for MPG. Figure 1.3c exhibits the distribution of months in the labor force. Most people work at least 30 years; however, some of these workers are in the informal economy. Figure 1.3d presents Groups I–VI for men aged 60–64 years in 2008.[18] People in Group I have few contributions and pension assets, whereas Group II and Group III have a similar level of accumulation. Still, they differ in the num-

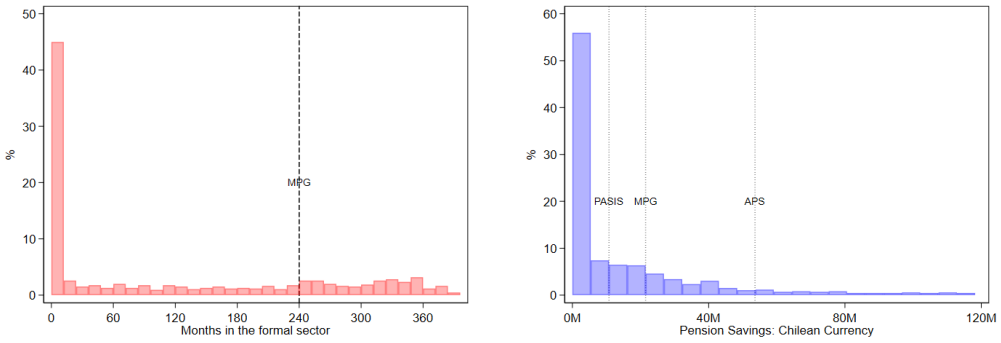
[18] I calculate the total months in the formal sector and expected pension income, to construct the groups for each period. In the case of expected pension income, I estimate the pension income that people would receive if they were 65 years old for men (60 years old for women) and had the current level of pension wealth.

ber of monthly contributions to the system. Groups IV and V both have high pension savings and monthly contributions. Although people in Group VI (people affiliated with the Armed Forces) are distributed along both axes, they are concentrated in the bottom part with few contributions and pension wealth in the fully funded pension system. In 2008, 73.2% of people affiliated with the Armed Forces would be in Group I according to their level of contributions (See Table A15).

Figure 1.3: Pension System Attachment, Men Aged 60–64 Years in 2008

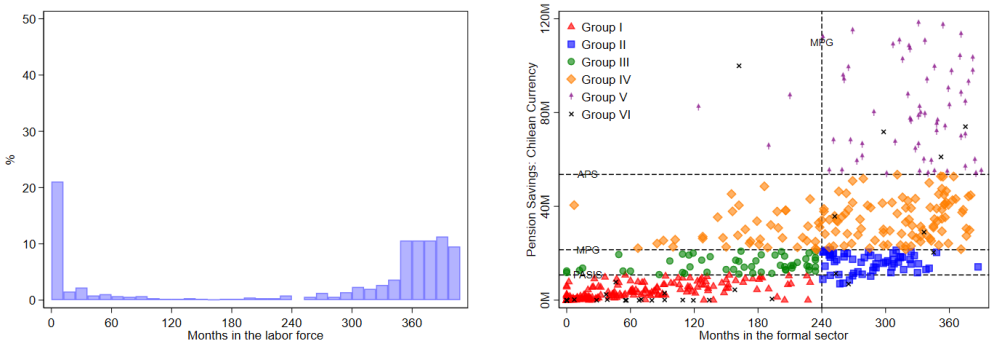
(a) Months in the Formal Sector

(b) Pension Savings



(c) Months in the Labor Force

(d) Groups I–VI



Note: The distribution of (i) months in the formal sector, (ii) pension savings, (iii) months in the labor force, and (iv) Groups I–VI is shown for men aged 60–64 years in 2008. I calculate the total number of months in the formal sector and expected pension income, to construct the groups for each period. In the case of expected pension income, I estimate the pension income that people would receive if they were 65 years old for men (60 years old for women) and had the current level of pension wealth.

Table 1.2 shows the distribution of workers in the different groups over time. Group I represents around 65.6% of all male workers aged 60–64 years in 2008 (55.2% overall). People who were eligible for the PASIS benefit are the

most populated group in the analysis. Groups II and III comprise 2.6% and 6.9% of the sample in 2008, respectively. Notice that both groups tend to have similar expected self-financed pension income, but they differ in the number of monthly contributions. Indeed, around 78% of people in Group II have an expected level of pension income that is between x_1 and x_2 (See Figure 1.3d). Group IV comprises people who became eligible for supplements just after the reform and represents 10.0% of men aged 60–64 years in 2008. Finally, Group V comprises 6.7% of the sample, and the workers affiliated with the Armed Forces represent 8.1% of men aged 60–64 years in 2008. Group II has become more populated because with time, younger cohorts have a higher likelihood to work for at least 20 years in the system that started in 1981.

Table 1.2: Groups I–VI, Men Aged 60–64 Years (SPS-APH-SCOMP)

Year	Group I	Group II	Group III	Group IV	Group V	Group VI
	Non-contributory	Contributory	Year-threshold	Income-threshold	Wealthy	Armed Forces
2008	65.6	2.6	6.9	10.0	6.7	8.1
2010	62.8	4.5	6.3	11.9	8.1	6.5
2012	59.2	5.5	5.7	14.9	8.9	5.8
2014	53.7	5.9	5.8	18.1	10.8	5.7
2016	49.8	7.2	5.7	20.4	11.8	5.1
2018	47.6	7.7	5.3	23.7	11.4	4.3
Total	55.2	5.9	5.9	17.4	9.9	5.7

Note: The distribution of men aged 60–64 years into different groups is presented over time. The categorization of each individual depends on the number of monthly contributions and the level of expected pension income.

1.5 (Lack of) Pension Knowledge

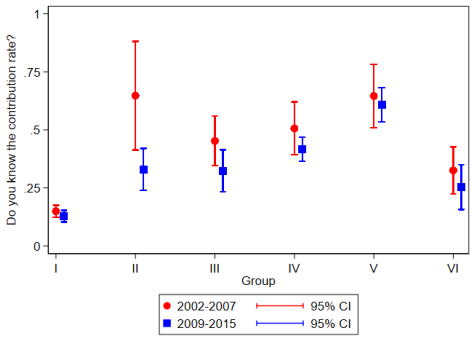
So far, I have calculated the distribution of workers assuming people fully understand the system, and their current pension assets and years of contribution. However, past literature has shown that individuals have a low understanding of pension systems and financial markets (Lusardi and Mitchell 2011; Mitchell and Lusardi 2011; Gustman and Steinmeier 2005). To analyze if it is the case for Chilean workers, I use several questions included in the SPS to measure the respondents' level of knowledge concerning the fully funded pension system and how government transfers work. I find evidence that people believe they are in a different group and, therefore, they face alternative incentives to continue working.

1.5.1 (Lack of) Pension System Knowledge

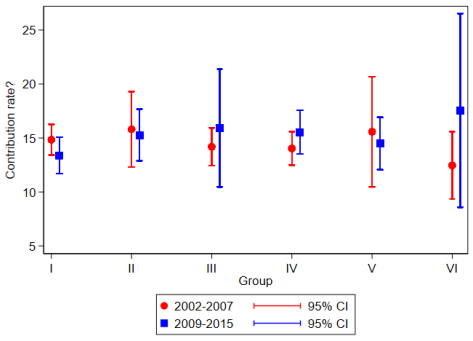
Figure 1.4 illustrates people's understanding of four of the most crucial features of the FF pension system pre- and post-reform. Figure 1.4a shows the proportion of men aged 60–64 years who know the value of their contribution rate among Groups I–VI. As can be seen, Group I (the most populated one) has the lowest knowledge; less than 25% of people in that group answered positively. Groups II–VI are more informed than Group I, especially Group V. Notice that the 2008 reform does not seem to change the understanding across groups. Only Group II has a significantly lower proportion of positive answers post-reform. Figure 1.4b shows the value of the contribution rate conditional on having answered yes to the previous question. The reported average value for the contribution rate is around 15%, which presumably includes the actual contribution rate of 10%, the disability insurance of 1.5%–2%, and the commission to the AFP of 3%–3.5%. This overestimation of the actual contribution rate has crucial implications for explaining an increase in the formal labor supply due to a more significant substitution effect, as I will discuss later in Section 1.8. Figure 1.4c shows the level of understanding of how pension income is calculated across groups. Less than 10% of Group I and 20% overall comprehend how pension savings are transformed into pension income. In fact, some people still believe that pension income represents a percentage of the last five years of labor income, as it was in the PAYG pension system. Figure 1.4d shows the proportion of men who know the level of pension assets in their individual accounts. As before, Group I has the lowest fraction of positive answers (before and after the reform). Additionally, around 80% of people in Group II know their accumulated pension before the 2008 reform. They are the people whose eligibility depends on the level of assets and number of months of contribution to the system, so it is expected they have a better understanding of these features.

Figure 1.4: Pension System Knowledge, Men Aged 60–64 Years

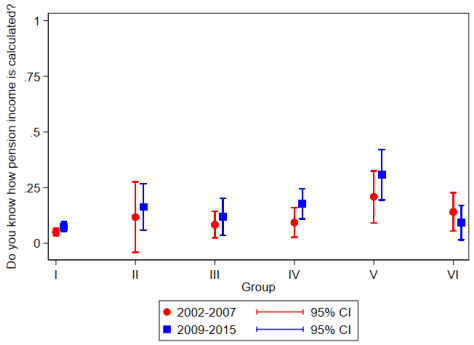
(a) Do you know which is the contribution rate? (Yes/No)



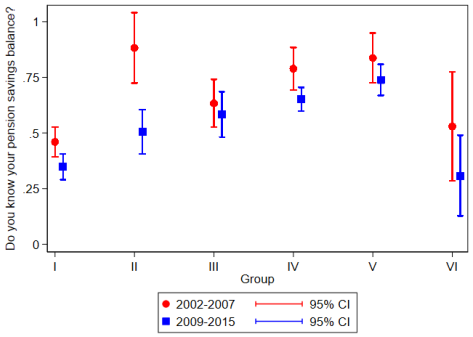
(b) What is the value of the contribution rate? (Conditional)



(c) Do you know how pension income is calculated? (Yes/No)



(d) Do you know how much you have accumulated in your pension account? (Yes/No)



Note: The figure shows the respondents' level of understanding of four important features of the FF pension system: whether they know the contribution rate, the conditional value of the contribution rate, how pension income is calculated, and the level of their pension assets. The average values are shown for Groups I–VI pre- and post-reform.

Although there are differences between groups, it seems clear many workers lack pension knowledge. Nevertheless, it may not be related to how much people understand pension supplements. I study this in the next section.

1.5.2 (Lack of) Government Transfers' Knowledge

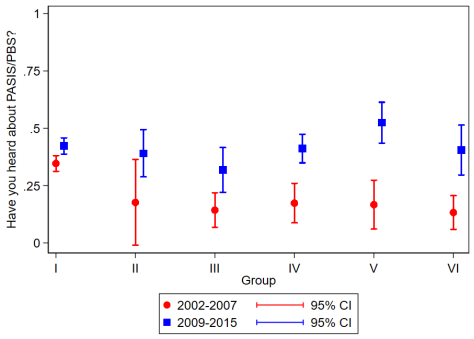
As I discussed in Section 1.2, before 2008, there were two benefits for people with zero (non-contributory pension) or low pension savings (contributory pension): the PASIS and the MPG. After the reform, people could apply for the PBS and APS. In some sense, PBS replaces PASIS for people with zero

formal contributions and, therefore, pension assets. Similarly, the APS gives benefits to people who could have been PASIS and MPG beneficiaries as well as workers who were not eligible because of too few monthly contributions (Group III) and excessive pension income (Group IV).

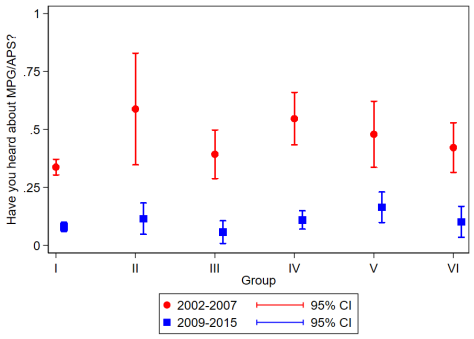
Figure 1.5 shows evidence concerning the respondents' level of knowledge about pension benefits. Figure 1.5a depicts the proportion of men aged 60–64 years who have heard about PASIS and PBS, before and after the reform, respectively. As can be seen, around 25% of men know about PASIS pre-2008, and 40% have heard about PBS after the reform. The level of knowledge does not seem to change across groups. Figure 1.5b shows the proportion of men aged 60–64 years who have heard about MPG and APS before and after the 2008 reform. Pre-reform, on average, 40% of men aged 60–64 years know about MPG, and Group II presents the highest fraction, with 53%. This makes sense because this is the group most likely to receive this benefit. Post reform, 10% of people have heard about the APS benefit. Figure 1.5c illustrates the fraction of workers who have heard about the 2008 pension reform. As can be noticed, on average, 25% of men aged 60–64 years know about the reform. Figure 1.5d shows the proportion of people who know one crucial detail about the reform: if PASIS beneficiaries would have to apply for the new benefits: PBS if they have zero formal contributions or APS if they have positive formal contributions. Around 25% of people know about this feature, but Group II and III present the lowest proportions among groups. The intuition is straightforward: these workers were closer to being eligible for MPG than for PASIS. As a final detail, MPG beneficiaries did not need to apply again, but, unfortunately, no question tested the respondents' knowledge of this fact.

Figure 1.5: Government Transfers' Knowledge, Men Aged 60–64 Years

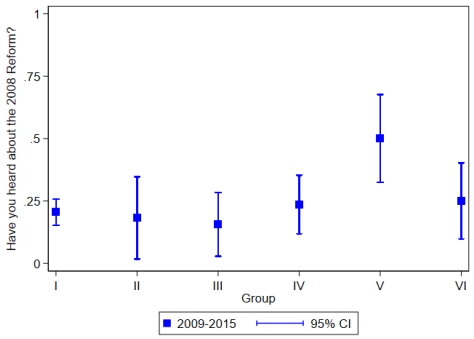
(a) Have you heard about PASIS/PBS? (Yes/No)



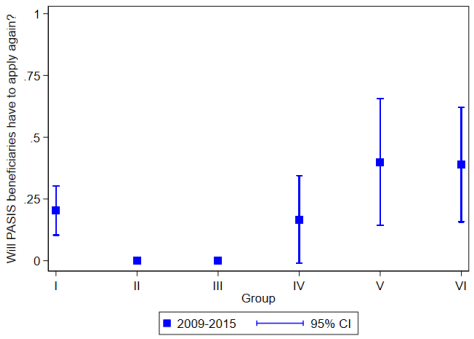
(b) Have you heard about MPG/APS? (Yes/No)



(c) Have you heard about the 2008 pension reform? (Yes/No)



(d) Will PASIS beneficiaries have to apply for the new benefits? (Yes/No)



Note: The proportion of people who have heard about pension transfers is shown. Panels (a) and (b) depict the ratio of people who know about PASIS/PBS and MPG/APS, respectively. Panels (c) and (d) illustrate the fraction of people who have heard about the 2008 reform and that PASIS beneficiaries would have to apply for new benefits. The average values are shown for Groups I–VI pre- and post-reform.

After analyzing people's lack of knowledge concerning the pension system and the transfers, one might worry that the labor supply responses due to the 2008 reform may be driven by respondents' wrong beliefs of their relative position in the joint distribution of pension savings and the number of monthly contributions. I analyze this possible explanation by using the administrative and self-reported information of the SPS-HPA-SCOMP data.

1.5.3 Perceived Groups

Actual Groups I–VI were constructed using administrative records on pension assets and monthly contributions included in the HPA. In the survey, there is information on "self-reported" pension assets and "self-reported" months in the formal sector. Concerning pension wealth, the question is direct: "How much have you accumulated in your pension account?" This information is available for each SPS wave (since 2004), and in the best case scenario, I observe self-reported pension wealth five times for each individual. Figure A.9 in the appendix shows a comparison between the self-reported and the true pension wealth across Groups I–VI. Although the median individual has a good estimate of his pension savings, the average worker underestimate his pension accumulation. For instance, the median worker in Group I underestimates his actual pension by 10%. People also self-reported if they worked in the formal sector each time they were employed; thus, I can construct the total self-reported months in the formal sector at any time.

Finally, I construct perceived groups using perceived pension wealth and perceived monthly contributions. One crucial issue to deal with is the case where people were not interviewed. I try alternative scenarios of what the individual would have responded on those periods and later I choose the case that minimizes the squared mean error and the absolute mean error between "perceived pension assets" (case with imputation) and "self-reported pension assets" (direct answer). By doing so, I choose the scenario where, on average, people align their responses about both pension assets and number of monthly contributions.[19] The scenario (Scenario 1) with the best performance is the one in which the formality is 0% in missing months. The intuition is the following: people tend to overestimate the number of months in the formal sector, so any scenario where there are additional months of contribution increases the difference between the self-reported pension wealth and the imputed value. For instance, I compare the formality between the administrative and "perceived" information. In 70% of the cases, both sources of information coincide, the case when people think they were formal but they were not represents 23%, and the final 7% is the case when the agent believes to be an informal worker when he is a formal worker.

[19] See Appendix A.2.2 for a detailed discussion of the imputation process and alternative scenarios.

The goal is to see if people who do not know to which group they belong behave differently from people who know where they are in the joint distribution of pension wealth and the number of formal contributions. Table 1.3 compares the perceived (under Scenario 1) and actual groups and reports the additional months in the formal sector and labor force after five years for men aged 60–64 years in 2009.[20] Among people in Group I, 83.8% of them perceive they are in

[20] Tables A11 and A12 in the appendix show the same analysis but using alternative scenarios for constructing the perceived groups. As can be seen, there are not huge changes in the proportion of each group.

that group. The most common mistake these workers make is to overestimate their pension income, as do the people in Groups III (6.3%) or IV (5.1%).

Men who perceive to be and are in Group I increased their number of months in the formal sector and labor force after five years by 2 and 15.1 months, respectively. On the other hand, people in Group I who perceive they belong to another group increased their number of months in the formal sector and labor force by at least 600% and 300%, respectively. Those disparities are significant at the 1% level. People in Groups III, IV, and V would not have been eligible for benefits before the 2008 reform, so these people would not have received any government transfers. According to Figure 1.1, they might be convinced that they face a transfers scheme represented by a 45-degree line that starts at the origin. Therefore, instead of experiencing an increased accrual rate (from 0% to 70%) and pension income (from y_1 to y^*), they may expect a decrease in accrual rate (from 100% to 70%) and an increase in benefits (the difference between the 45-degree line and $APS(b)$). In the real case, the substitution and income effects are moving in opposite directions, whereas in the perceived scenario, labor supply should fall because both substitution and income effects move in the same direction.

Among people who belong to Group II, 52.9% perceive they are in this group, and 35.3% think they satisfy the requirements to be in Group IV. The latter group also has the same beliefs as the people in Group I about alternative accrual rates and benefits. Around 38.7% of people in Group III perceive to be part of Group IV. There are no relevant changes in the slope or benefits between the perceived and actual groups in this case because both groups became eligible for benefits after 2008. However, 27% of people in Group III perceive that their group is either I or II. It is crucial because for them the net effect of the reform on formal labor supply can go up or down because the two effects go in opposite directions. In addition, 77.3% of people in Group IV know their actual group, but 16% think they are in Group V and are not eligible for benefits. Finally, 91.7% of people in Group V realize they are part of this group.

Table 1.3: Additional Months in the Formal Sector and Labor Force After 5 Years, Men 60–64 Years in 2008 (Scenario 1)

Actual Group		Perceived Group				
	Outcome	I	II	III	IV	V
Group I	Formal Sector	2.0	30.8***	25.2***	26.0***	12.0***
(Non-contributory)	Labor Force	15.1	45.5***	45.7***	46.9***	49.5***
	Proportion	83.0	4.8	6.3	5.1	0.8
Group II	Formal Sector	58.0***	43.0	50.2**	51.0***	
(Contributory)	Labor Force	60.0**	54.2	54.0	59.8***	
	Proportion	5.9	52.9	5.9	35.3	
Group III	Formal Sector	32.5	55.1***	33.2	29.0**	37.3**
(Year-threshold)	Labor Force	45.4**	60.0***	49.0	52.9**	54.0**
	Proportion	16.2	11.3	29.0	38.7	4.8
Group IV	Formal Sector	60.0**		26.0***	45.4	39.7**
(Income-threshold)	Labor Force	60.0**		50.7**	53.6	47.6**
	Proportion	2.7		4.0	77.3	16.0
Group V	Formal Sector				46.8***	58.0
(Wealthy)	Labor Force				49.5***	59.2
	Proportion				8.3	91.7

Note: The table shows the additional months in the formal sector and in the labor force after five years across actual and perceived groups. *** p<0.01, ** p<0.05, * p<0.1. Stars represent if average choices of misinformed workers differ from average choices for correctly informed workers. In the third row, the table shows the distribution (proportion) of people between the actual and perceived groups.

In conclusion, it is apparent that even if workers are in the same actual group, alternative perceptions make them behave differently. The same is true in all groups: misinformed workers often make significantly different labor supply choices from informed workers.

1.6 Empirical Approach

In this section, I use a difference-in-differences (DID) approach with multiple time periods to measure the impacts of the 2008 reform on labor force participation, fraction of formal workers, and hours worked.

1.6.1 Difference in Differences

In the canonical DID case, there are two time periods and two groups (treatment and control). In the first period no group is treated, whereas in the second period only the treated group is treated. The assumption is that in the absence of treatment, the average outcomes for both treated and control groups would

have followed parallel paths over time. The average treatment effect for the treated (ATT) can be estimated by comparing the average change in outcomes experienced by the treated group to the average change in outcomes experienced by the control group (Heckman et al. 1997; Abadie 2005; Athey and Imbens 2006).

In this case, D_{it} is an index variable that identifies if individual i in period t is treated,

$$D_{it} = \begin{cases} 1 \text{ if unit } i \text{ is treated at time } t, \\ 0 \qquad \text{otherwise.} \end{cases} \tag{1.2}$$

In this paper, I deviate from the simplest DID setup because I have more than two time periods. It is an important issue when having heterogeneous effects and a staggered adoption setting, because in that case, standard two-way fixed effects (TWFE) might fail (De Chaisemartin and d'Haultfoeuille 2020; Goodman-Bacon et al. 2019; Sun and Abraham 2021).

In this article, however, I do not have a staggered adoption setting. Here $D_{it} = 1$ for the men aged 60–64 years after 2008 who receive the treatment; therefore, the control group is the men in the same age group who did not receive the treatment.[21] The control group is people who are affiliated with the Chilean Armed Forces who are not eligible for these government benefits because the pension income from their PAYG pension system is not compatible with transfers for people in the FF system. In this context, the people in the Armed Forces play the role of the never-treated group.

[21] In Section A.1.2, I show the estimates for other age groups. In Section A.5, I present the results for women.

In order to estimate the interpretable causal parameters, I can use Callaway and Sant'Anna's estimator or TWFE with heterogeneous time effects to find the ATT (Callaway and Sant'Anna 2021; Sun and Abraham 2021). If the never-treated people are the control group, Callaway and Sant'Anna's estimator and Sun and Abraham's estimator are equivalent.

Thus, following Callaway and Sant'Anna (2021), the group time ATT at time t can be written as

$$ATT(t) = \mathbb{E}[Y_t(1) - Y_t(0)|X, D = 1], \tag{1.3}$$

where $Y_t(1)$ is the outcome variable (hours worked, labor force participation, fraction of formal workers) in period t for the treatment group, $Y_t(0)$ is the outcome variable in period t for the control group, and X is a vector of controls.[22] Then, I can compute $ATT(t)$ for every post-treatment period without imposing that the group time ATT values have the same magnitude. I am also

[22] Caetano et al. 2022 mention additional problems with TWFE when using predetermined control even if there is no staggered adoption.

interested in the overall ATT, which in this case is given by

$$ATT = \frac{1}{\kappa} \sum_{t=\hat{t}}^{T} ATT(t), \tag{1.4}$$

where $\kappa \equiv T - \hat{t} + 1$ is a constant that depends on the number of post-treatment periods; $\hat{t}$ is the first period after treatment, and T is the last available period. For instance, in CASEN there are five group time $ATT(t)$ after the treatment (2009, 2011, 2013, 2015, and 2017); therefore, $\kappa = 5$. In the next section, I estimate the impacts of the pension reform on employment outcomes using both the cross-sectional and the administrative panel data.

1.7 Empirical Results

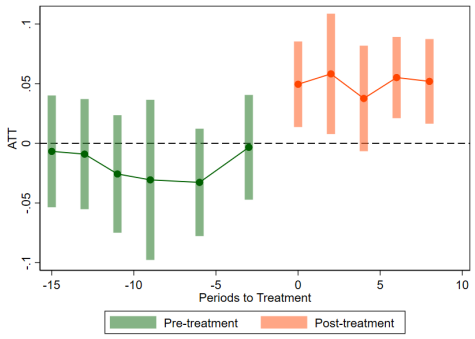
In this section, I show the empirical results using cross-sectional and administrative panel data. Furthermore, I include the perceived groups in the analysis to show how the estimates vary with the level of perception. Finally, I present a battery of robustness checks and placebo tests to confirm the main conclusions.

1.7.1 Cross-sectional Data

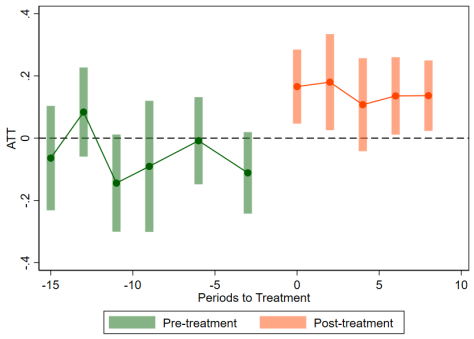
Figure 1.6 shows the results for hours worked and labor force participation. Specifically, Figure 1.6a depicts the group time ATT pre- and post-reform for hours worked. On the x-axis is represented the number of periods pre- and post-treatment, where period zero is assigned to 2009 (first wave after the reform). All group time ATT in the pre-treatment periods are statistically indistinguishable from zero. On the other hand, all group time ATT in the post-treatment periods are significant at the 10% level, and four of them, at the 5% level. Furthermore, all group time ATT have a similar magnitude, implying an increase of around 5% in the number of hours worked. The same conclusion can be obtained from observing Figure 1.6b, where the increase in labor force participation is around 14%. Figure A.5 in the appendix shows that the group time ATT for hours worked per worker and the fraction of formal workers that move around are 3.2% and 17.3%, respectively. In each regression, I control for demographic variables such as education, and I use dummies for the region.

Figure 1.6: Group Time ATT, Men Aged 60–64 Not Affiliated with the Armed Forces (CASEN)

(a) Monthly Hours Worked per Capita



(b) Labor Force Participation



Note: Panel (a) shows the group time ATT for monthly hours worked per capita, whereas panel (b) depicts the group time ATT for labor force participation using Callaway and Sant'Anna (2021). The 95% confidence intervals are also included. Treatment group is men aged 60–64 years who are not affiliated with the Armed Forces. Control group is men aged 60–64 years who are affiliated with the Armed Forces. In each regression I control for demographic variables such as education, and I use dummies for the region. Standard errors are estimated using the doubly robust DID estimator based on stabilized inverse probability weighting. Results are obtained using population weights.

After obtaining the group time ATT, I use Equation (1.4) to estimate the overall ATT. The results are shown in Table 1.4. In line with Figure 1.6, the ATT for hours worked and labor force participation are 5.0% and 14.4%, respectively. Furthermore, the impact on hours worked per worker is 3.0% and significant at the 5% level. This shows that the reform impacted both the intensive and extensive margins for labor supply. I find a positive and significant effect of 15.1% on the fraction of formal workers. This result contrasts with previous papers that found a reduction in formality using structural models (Joubert and Todd 2011, 2022; Attanasio et al. 2011). Table A7 in the appendix shows the results for alternative outcome variables and gives evidence that the increase in hours worked is due to an increase in the labor supply of formal workers.

In addition to the ATT, Table 1.4 depicts the pre-trend test where the null hypothesis assumes that all ATT in the pre-treatment periods are jointly zero. The null is rejected for hours worked per capita at 10%, but it is not rejected for labor force participation, hours worked per worker, and formality. Although the test rejects the null in the case of worked per capita, all the group time ATT pre-treatment are not individually different from zero. Moreover, the group time ATT just before the treatment is negative. This shows that there is no

significant evidence of anticipatory effects on those variables because of the policy.[23]

[23] The reform was discussed in both the Senate and House of Deputies from January 2007 to March 2008. In July 2008, the new scheme was established. The low likelihood of anticipatory effects is based on the years where the two nearest CASEN waves around the reform were conducted: 2006 and 2009. Since the former took place before any announcement of the forthcoming policy change, the latter measured outcomes a couple of months after the new scheme was available.

Table 1.4: Several Outcomes, ATT, Men Aged 60–64 Years Not Affiliated with the Armed Forces (CASEN)

	Hours Worked (everyone)	LFP (everyone)	Hours Worked (workers)	Formality (everyone)
ATT	.050***	.144***	.030**	.151**
	(0.015)	(0.046)	(0.015)	(0.065)
Pretrend Test	11.406*	9.300	8.593	4.914
p-value	[0.076]	[0.157]	[0.198]	[0.555]
Observations	48,979	48,979	34,432	48,979
Weights	3,344,245	3,344,245	2,384,454	3,344,245
Mean: Dep. Variable	138	.759	190	.438
SD: Dep. Variable	101	.428	64	.496

Note: Robust standard errors in parentheses; p<0.01, ** p<0.05, * p<0.1; p-values in square brackets. The overall ATT and the pre-trend test are shown for alternative outcome variables using Callaway and Sant'Anna (2021). Treatment group is men aged 60–64 years who are not affiliated with the Armed Forces. Control group is men aged 60–64 years who are affiliated with the Armed Forces. In each regression I control for demographic variables such as education, and I use dummies for the region. Standard errors are estimated using the doubly robust DID estimator based on stabilized inverse probability weighting. Results are obtained using population weights. LFP is labor force participation.

So far, I have calculated the average impacts of the reform on labor supply using cross-sectional data. In the next section, I estimate the employment effects including administrative information such as pension savings and number of monthly contributions to the pension system from SPS-HPA-SCOMP.

1.7.2 Administrative Panel Data

According to Figure 1.1, there are four groups who face changes in their pension benefits and accrual rates because of the 2008 reform. Notice that workers in Group I and II would have qualified for PASIS and MPG, respectively. Post-reform, they face an increase in the value of benefits and in accrual rates. On the other hand, workers in Group III and IV would not have been eligible for benefits before 2008, the former, because they made too few contributions to the pension system; the latter, because their pension income is too high. After the reform, both groups receive transfers and face a lower accrual rate.

Table 1.5 shows the ATT for men aged 60–64 years who are not affiliated with the Armed Forces and belong to one of the four groups. The control group is the same as before, men aged 60–64 years affiliated with the military.

For Group I, I find no significant results for the main outcomes. It is the most populated group in the sample for men between 60 and 64 years old. I also compute the fraction of men in the treatment group who moved from zero monthly contributions before the reform to any positive number after 2008. This proportion is around 3%, implying that this policy change had small effects in the extensive margin.[24]

[24] Kline and Tartari (2016) find similar results for the U.S. Jobs First (JF) program, which changed marginal benefit rates in 1997.

Post-reform, the workers in Group II reduced their hours worked per capita and per worker, by 6.7% and 4.7%, respectively. The fraction of formal workers fell as well, but it is not statistically significant. The intuition for this group comes from the fact that they knew they would be eligible for benefits, because they had enough contributions to receive the MPG. The reform then only increases the value of benefits but does not encourage people to continue working. Under the previous system, there was a discrete jump in pension benefits at 20 years of contributions. Some workers might have planned to work exactly 20 years to earn these benefits. I find evidence for this behavior when analyzing labor responses for people within each group.

Groups III and IV increased their hours worked by 4.8% and 3.2% respectively. Furthermore, the workers in Group IV increased their labor force participation by 9.5%. For both groups, the intuition would say that both the substitution and income effects should be negative, and therefore hours worked and labor force participation should decrease. I analyze the behavior of these two groups by comparing how much they knew about the total pension savings in their individual accounts before and after the 2008 reform. For instance, according to Table 1.3, 28% of the people in Group III underestimate their pension wealth, believing they are in Groups I–II. Their perceived group faces alternative incentives, where the positive substitution effect competes with the negative income effect. Thus, people may be encouraged to continue working and contributing to the pension system. A similar intuition applies to people in Group IV.

Table 1.5: Overall ATT by Actual Groups, Men Aged 60–64 Years (SPS-HPA-SCOMP)

Outcome	Group I Non-contributory	Group II Contributory	Group III Year-threshold	Group IV Income-threshold
Hours worked	.011	-0.068*	0.049**	0.032**
(everyone)	(0.021)	(0.038)	(0.025)	(0.014)
LFP	0.051	-0.0685	0.1325	0.0954*
(everyone)	(0.070)	(0.118)	(0.103)	(0.051)
Hours worked	-0.0065	-0.0475**	0.0050	0.0123
(workers)	(0.012)	(0.021)	(0.005)	(0.013)
Formality	0.0185	-0.1018	0.2364*	0.0159
(everyone)	(0.070)	(0.115)	(0.129)	(0.085)

Note: Robust standard errors in parentheses; p<0.01, ** p<0.05, * p<0.1. Overall ATT is shown for alternative outcome variables using Callaway and Sant'Anna (2021). Treatment group is men aged 60–64 years who are not affiliated with the Armed Forces and belong to one of the four groups. Control group is men aged 60–64 years who are affiliated with the Armed Forces. In each regression I control for demographic variables such as education, previous occupation, and industry, and I use dummies for the region. Standard errors are estimated using the doubly robust DID estimator based on stabilized inverse probability weighting. LFP is labor force participation.

While my defined groups tend to put together people with similar pension savings and contributions, there might be more than one representative agent inside each group. This is especially true for people near the threshold between groups. To analyze heterogeneous behavior, I run the DID model for alternative subgroups. Results are in Section A.2.4 in the appendix. For Group I workers who have between 1 and 239 monthly contributions, the reform has a positive impact on hours worked. I show that the negative impacts of the reform on labor supply for Group II are driven by people whose expected pension income is far from the MPG line. Those workers satisfy the contribution requirement, but their expected pension income is low. They face an increase in benefits (income effect) and an increase in accrual rate (substitution effect), and for them, the income effect seems to overcompensate the substitution effect. Workers close to the thresholds drive the results for Groups III and IV. For instance, people in Group III who have between 180 and 239 months in the formal sector increase their hours worked. At the same time, individuals in Group IV who have pension assets just above the income threshold augmented their hours worked and labor force participation. Both subgroups have income and substitution effects moving in the same direction, and for them the expected labor market effects are negative. In the next section, I show evidence that these results can be explained by people's lack of knowledge about where they are in the joint distribution of pension assets and formal monthly contributions.

Perceived Groups

Here I estimate the overall ATT when using only people who know their own group. Table 1.6 shows the results. I find positive and significant responses on labor force participation for Group I. When the same regression was run for all members of Group I (Table 1.5), I found no significant effects. The intuition for these differences is the following: Group I workers misinformed about their group decreased their labor force participation compared to earlier cohorts (See Table A10). By removing people who have a lower understanding of their location in the joint distribution, I obtain positive estimates. Notice that according to a theoretical model, Group I's predicted responses are uncertain, given that the income and substitution effects move in opposite directions. For Group II, I find a decrease in labor supply. Since in Table A10 the evidence comes from hours worked per capita and per worker in Table 1.6 force participation is the one with a marginal effect of a decrease by 4.3%. Moreover, as in the case of Group I, the theoretical predictions are not conclusive, because both effects do not move in the same direction.

I find negative responses on formality for people in Group III because people who know they are in Group III have a lower increase in the number of months in the formal sector (See Table 1.3). This result is consistent with the fact that people might stop working formally if their accrual rate decreases and their benefits increase. The impact on labor force participation is positive and with a similar magnitude that in Table 1.5. However, since only 29% of all people in that group have correct perceptions about pension wealth and monthly contribution (Table 1.3), the standard errors increased by 15%, losing the parameter its significance. Finally, I find significant negative impacts on hours worked and formality for Group IV. The positive effects of the reform on hours worked, and labor force participation when running the regression for everyone in this group seem to disappear after controlling for perceptions. One plausible explanation is that among people in Group IV, around 38% predict they belong to Group V, which does not face any change in its government benefits (workers in this group have never been eligible for transfers). Then, once I remove them to perform this analysis, the responses are not positive. Table A16 in the Appendix shows the results once I included everyone according to their perceived group without restricting people where they actually are. The main conclusion is that the positive estimates (but not significant) of the labor force for people who perceived they are in Groups III and IV tend to vanish even more while adding people in a different group. For instance, the ATT on the labor force for Groups III and IV decreased by 28% and 9.8%, respectively.

Table 1.6: Overall ATT, People Who Know Their Groups, Men Aged 60–64 Years (SPS-APH-SCOMP)

Outcome	Group I Non-contributory	Group II Contributory	Group III Year-threshold	Group IV Income-threshold
Hours worked	0.035	-0.122	-0.020	-0.028**
(everyone)	(0.019)	(0.063)	(0.040)	(0.010)
LFP	0.039**	-0.043**	0.142	0.051
(everyone)	(0.015)	(0.019)	(0.119)	(0.032)
Hours worked	-0.0047	-0.023	0.012	0.0135
(per worker)	(0.020)	(0.0209)	(0.022)	(0.020)
Formality	0.029	-0.137	-0.024**	-0.040*
(everyone)	(0.018)	(0.172)	(0.010)	(0.020)

Note: Robust standard errors in parentheses; p<0.01, ** p<0.05, * p<0.1. Overall ATT for alternative outcome variables using Callaway and Sant'Anna (2021). Treatment group is men aged 60–64 years who are not affiliated with the Armed Forces, belong to one of the four groups, and know their groups. Control group is men aged 60–64 years who are affiliated with the Armed Forces. In each regression I control for demographic variables such as education, previous occupation, and industry, and I use dummies for the region. Standard errors are estimated using the doubly robust DID estimator based on stabilized inverse probability weighting. LFP is labor force participation.

On average, when people have the correct beliefs about their relative location in the joint distributions of pension wealth and contributions, they behave as expected. A lack of knowledge and understanding of how government transfers work may lead to suboptimal responses. Knowing this fact is crucial for policymakers at the moment of approving such reforms and for researchers who want to assess the impacts of a reform on labor supply.

Although Pension Fund Administrators release information each quarter concerning the level of wealth and expected pension at retirement, people do not receive a clear explanation of how pension assets are converted to pension income. Additionally, the expected pension at retirement is estimated under strong assumptions on future labor decisions and contributions. In that sense, a scheduled pension assuming no additional contribution or the average contribution of the past five years would be a more straightforward estimate of future income. Finally, pre-2008, Pension Fund Administrators did not publish the number of contributions into the system in their reports. People had no certainty about their eligibility for government transfers. I have anecdotal evidence of people who learned they did not have enough years of contributions to qualify for MPG only after they looked for additional information. Some of these people had already quit their jobs and would need to find a new one to satisfy the requirement for receiving MPG benefits.

Additional support for the relevance of misperception can be found when calculating the take-up rate of pension supplements. I find that among people in Group I, workers with a better understanding of their group present a 10% to 17% higher probability of receiving the benefits (See Table A17 in the appendix). It shows the lack of pension knowledge is relevant for making unexpected labor decisions and for not applying for available benefits.

The results presented here are related to the results of several papers in the literature (Wong, 2016; Otero, 2015; Attanasio et al., 2011; Sabat, 2022). For instance, Wong (2016) applies a DID approach using SPS-HPA. The author uses annual data for 2006 and 2009 to estimate the impacts of the 2008 reform on the probability of working in the formal sector. Wong (2016) identifies two groups: one is people with pension savings between the PASIS and the MGP with fewer than 20 years of contribution (Group III in this paper), and the other group is people who were eligible for the MPG before the reform (Group II). The author concludes that after the reform, the first group decreased formal labor supply by 3%, and the second increased it by 8%. There are two main differences between this paper and mine: First, the outcome variable is different, taking a value of 1 when a person works in the formal sector and 0 otherwise. Second, the control group is men who do not belong to the bottom 40% of the income distribution. I show in Appendix A.7 that Wong's results are susceptible to how the treatment group is defined. Workers aged 60–64 years move easily between the bottom 40% and the bottom 60% of the income distribution because labor income shocks and, therefore, the identification are very susceptible to individual shocks. Moreover, the usage of only 2006 and 2009 waves impeded the author's ability to control for additional variables. However, in this paper, I use the people in the Armed Forces as the control group. These people are not eligible for benefits even if they do not work or they belong to the bottom 40% of the income distribution. Moreover, I also have access to more extensive data to first incorporate year-fixed effects and then use a DID approach that allows multiple time periods and heterogeneous effects across time.

Additionally, I perform a replication exercise concerning Otero (2015) and Attanasio et al. (2011). These two papers use SPS-HPA data up to 2009. The authors conclude that after the reform, men aged 56–65 years decreased formal labor supply by 3%. Following these papers, I obtain similar results (Section A.6 in the appendix). However, one driver of their results is the use of expected pension wealth and accrual rate at retirement. They strongly rely on the assumption of perfect-foresight agents who know the value of their pension wealth at 65 years old even if they are 40 years old. However, such an assumption may lead to bias, because, as I showed in the previous sections, agents have consider-

able problems comprehending the pension system. Indeed, when using their approaches and supplementing data between 2009 and 2017, I find that the results are biased for both men and women, with no significant impacts due to the 2008 reform.

1.7.3 Robustness Checks and Placebo Tests

In 2008, the financial crisis affected the Chilean economy as it did the rest of the world. Although this crisis significantly impacted employment and economic activity in several countries, in Chile it had smaller and short-term effects. In Appendix A.3, I show evidence that the impacts of the crisis on employment and economic activity lasted for at most 12 months because of active fiscal and monetary interventions from the Chilean government.

As a first robustness check, in Section A.3.1, I show the occupation and industrial composition over time for men aged 60–64 years who work. Later, I will calculate the average effects of the reform on employment using the administrative panel data. First, I follow the same setting and variables, and later I also control for previous industry and occupation to account for the fact that some jobs might be affected more than others. Hours worked and labor force participation increased by 3.8% and 10.8%, respectively. These values are close to either the ones I obtained when using CASEN or the results I obtained when using SPS-HPA-SCOMP without including previous industry and occupation. I find no significant impacts on hours worked per worker and fraction of formal workers. Finally, I estimate the heterogeneous impacts for Groups I–IV including individual fixed effects. The point estimates vary, but the negative or zero impacts for Groups I–II remain valid and so do the positive responses for Groups III and IV.

As a second robustness check, I remove the 2009 data from the analysis. The results are in Section A.3.2 in the appendix. The main conclusions from the baseline analysis hold: (i) pre-treatment periods are not statistically different from zero, (ii) post-treatment periods are positive and significant, and (iii) the group time ATT are the same. The overall ATT for hours worked per capita is 5.1%, which means it increased by 2% with respect to the value in Table 1.4. I see similar results for labor force participation, hours worked per worker, hours worked per formal worker, and hours worked per informal worker; the results vary by less than 6% from the baseline.

As a third robustness check, I estimate the impact of the 2008 crisis on pension assets. The results are in Section A.3.3. The financial crisis that started in 2008 affected pension savings through the return in the pension system. One could argue that while people in the treatment groups (Groups I–IV) have

positive pension assets, these workers would have been impacted stronger by the crisis compared to workers affiliated with the military. Therefore, the Great Recession of 2008 would have created a wealth effect that increased the labor supply. I show that because of age rules in the type of investment, people close to retirement age were highly protected from financial shocks. Indeed, because of the recession, men aged 60–64 years lost 2.3% of their pension savings in the first months. However, workers in all groups recovered their initial levels of pension savings in less than 12 months. For instance, on average, people in Groups III and IV had the same pension wealth after six months. Furthermore, only 0.7% of men aged 60–64 years changed their initial group because of the reform. In conclusion, the recession produced a reduced pension loss for men aged 60–64 years and does not seem to explain the main results.

As a fourth robustness check, I estimate the impact of the reform on workers using people in Group V (wealthy workers) as the control group. The results are in Section A.3.4. I show that people in Groups V and VI have similar non-pension assets and total assets. On average, they receive comparable pension income, and they cannot apply for pension supplements for different reasons. The estimation shows that the positive responses for Groups III and IV remain valid.

One possible problem with the identification strategy mentioned in Section 3.4.3 is that if employers preferred to hire workers without any previous military experience because of the 2008 reform, labor force participation for civilians would increase at the cost of ex-military workers' labor force participation. In Section A.3.5 in the appendix, I perform the fifth robustness check where I show that there is no strong evidence that supports this idea. It can also be easily seen in Figure 1.2 that labor force participation and hours worked increased for both treatment and control groups but at different rates. Finally, I test the idea that spouses' labor decisions would have affected the control group. I show null evidence of changes in labor supply for wives whose husbands are 60–64 years of age, especially for men affiliated with the Armed Forces.

As a placebo test, I estimate the impacts of an imaginary reform that occurs in 2003. The results are in Section A.4.1 in the appendix. I find no significant values for the ATT for hours worked, labor force participation, and hours worked per worker. For the fraction of formal workers, the ATT is positive and significant at the 1% level because of the significant effects of the last ATT, which most likely are capturing the effect of the 2008 reform. In Section A.4.2 in the appendix, I repeat the placebo test but assuming that the reform was introduced in 2006. The findings are similar, and the only significant estimates are for hours worked per capita and fraction of formal workers, mainly because

the periods after the reform are positive and significant and drive the results. In order to account for this issue, I also run a placebo test where I assume that the policy was introduced in 2003, but instead of using data until 2017, I compute the overall ATT with observations between 1992 and 2006. The results are in Section A.4.3 in the appendix, and as expected, there is no positive evidence of increasing labor supply.

1.8 A Three-period Model

In this section, I develop a simple three-period model where people decide consumption, non-pension assets, and labor supply. The novelty comes from people's lack of knowledge about the pension system. By doing so, I can rationalize the empirical results found in the previous section.

1.8.1 Agents and Decisions

Consider the following dynamic framework where individuals stay in the model for three periods, $t = 1, 2, 3$. People derive utility from consumption and leisure, where l^f is their formal labor supply and $1-l^f$ is leisure. People discount future utility at $\beta \in (0, 1)$. I assume people can borrow and lend at the same interest rate (r), which is fixed and exogenous.[25] Here w_t^f is the wage in the formal sector at period t.

[25] In Section A.8.2 in the appendix, I show similar results when analyzing borrowing-constrained agents.

The timeline of the model is displayed in Figure 1.7. In the first period, when people are 59 years old for men (54 years old for women), individuals start with some initial non-pension savings a_0 and B_0, which is the initial pension wealth. Workers decide their consumption (c_1) and non-pension savings (a_1). I assume that they work $l_1^f = \bar{l}$, but it is inelastic.[26] The after-tax labor income can be written as follows:

$$\text{After-tax formal labor income} = w_1^f \cdot \bar{l} \cdot (1 - \tau - \theta),$$

[26] Assuming that $l_1^f = \bar{l} = 0$ does not change the results. The level of non-pension assets can be redefined so that labor income in the first period can be zero or any positive number.

where τ is the labor tax rate, and θ is the contribution rate to the fully funded pension system.

In the second period, when individuals are 60–64 years old, they start with a_1 as the level of non-pension savings and $B_1 = (1 + \phi_1) \cdot \left(B_0 + w_1^f \cdot \bar{l} \cdot \theta\right)$ as pension savings, where ϕ_1 is the pension savings return at the end of period $t = 1$. Workers choose consumption (c_2), non-pension savings (a_2), and labor supply (l_2^f).

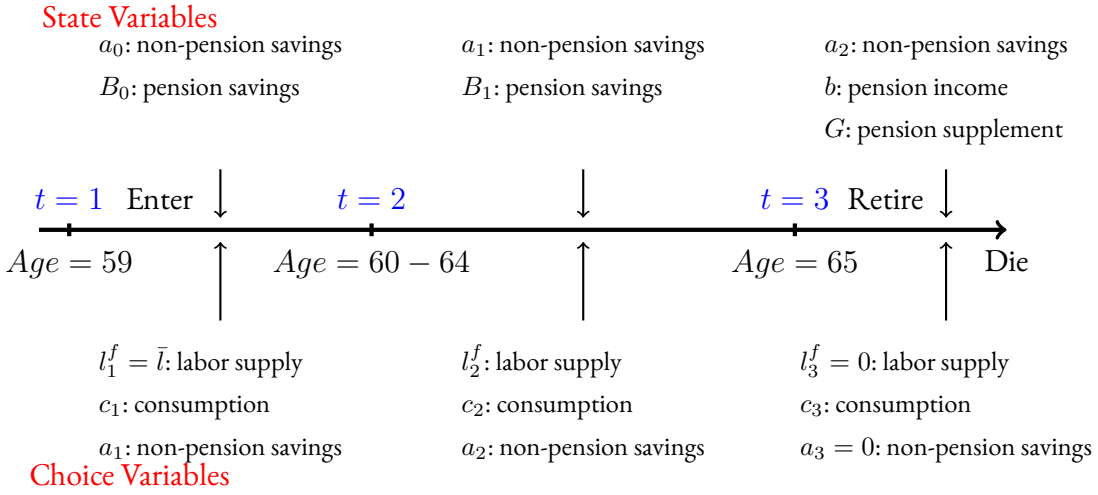


Figure 1.7: Timeline

In the last period, individuals are 65 years old and retired ($l_3^f = 0$). They choose their consumption (c_3) and die. There is no bequest motive and therefore their optimal non-pension savings is equal to zero ($a_3 = 0$). People have three sources of income: non-pension savings plus interest rate, self-financed pension income under the fully funded pension system, and government transfers, as follows:

$$c_3 = \underbrace{a_2 \cdot (1 + r)}_{\text{non-pension assets income}} + \underbrace{b}_{\text{pension income}} + \underbrace{\mathrm{G}(b).}_{\text{government transfer}}$$

Self-financed Pension Income

In a fully funded pension system, self-financed pension income is related to the contribution made into the system. In this simple model, self-financed pension income (b) is given as follows:

$$b = \underbrace{(1 + \phi_2)}_{\text{pension return at period 2}} \left[\underbrace{(1 + \phi_1)}_{\text{pension return at period 1}} \cdot \underbrace{\left(B_0 + w_1^f \cdot \bar{l} \cdot \theta \right)}_{\text{pension wealth at period 1}} + \underbrace{w_2^f \cdot l_2^f \cdot \theta}_{\text{additional contribution at period 2}} \right], \tag{1.5}$$

where ϕ_2 is the pension return at the end of $t = 2$. The first term inside the brackets corresponds to the initial pension wealth at period 1, which is multiplied by the return gained at the end of the first period. The second term in the brackets is the additional contribution made in period 2 if people work in the

formal sector. Pension income is increasing in pension savings returns, initial pension wealth, and formal labor supply.

Pension Supplement

The government uses a means-tested program to give benefits to retirees with low pension savings. The old and new schemes are given by Equation (1.6);

$$\mathrm{G}(b) = \begin{cases} b^{pasis} - b & \text{if } b \leq b^{pasis} \text{ and pre-2008,} \\ b^{pbs} \cdot \left(1 - \frac{b}{PMAS}\right) & \text{if } b \leq b^{pasis} \text{ and post-2008,} \end{cases} \tag{1.6}$$

where b^{pasis} is the level of government transfers before 2008 (i.e., the intercept of the red line in Figure 1.1a). As can be seen, the accrual rate is zero (i.e., the slope of the red line in Figure 1.1a), so the transfers are reduced by one dollar when pension income increases by a dollar. After the reform, the benefits are $b^{pbs} \cdot \left(1 - \frac{b}{PMAS}\right)$, where b^{pbs} is the value of the new guaranteed pension (i.e., the intercept of the orange line in Figure 1.1b), and $\left(1 - \frac{b}{PMAS}\right)$ plays a role in the accrual rate, which will be discussed in Section 1.8.2; $PMAS$ is the maximum value complemented for pension income (x_3 in Figure 1.1b).

Both pre- and post-reform pension supplements are strictly decreasing in self-financed pension income (b). For this simple exercise, I assume people are far from the threshold, implying that this analysis holds for values such that $b \ll b^{pasis}$.[27] This assumption allows me to study the effects of the reform on labor supply for people in Group I, which represents the most populated group, comprising 60.9% of all men aged 60–64 years in 2008.

I solve the model assuming perfect foresight, in order to estimate the labor responses for people who expect some kind of reform to the pension system in the future.[28] In $t = 1$, people anticipate the reform that will be implemented in $t = 2$. Agents maximize their discounted utility taking this fact into consideration. Notice that the level of transfers affects the optimization problem in period $t = 3$ through the budget constraint. Table 1.7 summarizes the information.

[27] In addition to this assumption, I assume that the agent cannot reach the minimum number of contributions so that the individual is no longer eligible for the MPG benefit (Group II). This inclusion would require incorporating a discrete variable that measures the number of periods where $l^f > 0$. Although one could expect some level of bunching around the level of pension income and number of periods in the formal sector, I find no evidence of it in Section 1.4.

[28] In Section A.8.3 in the appendix, I show similar results when people do not expect the reform and realize there is a reform only in the second period. I call this scenario the MIT shock case.

Table 1.7: Pre-reform and Perfect Foresight

	Period	Pre-Reform	Perfect Foresight
Group I	$t = 1$	$b^{pasis} - b$	$b^{pbs}\left(1 - \frac{b}{PMAS}\right)$
	$t = 2$	$b^{pasis} - b$	$b^{pbs}\left(1 - \frac{b}{PMAS}\right)$
	$t = 3$	$b^{pasis} - b$	$b^{pbs}\left(1 - \frac{b}{PMAS}\right)$

Note: In the pre-reform case, people believe that the old scheme stays forever. With perfect foresight, workers anticipate the reform in $t = 1$ and maximize their discounted utility accordingly.

1.8.2 Optimization Problems

I present the optimization problem pre- and post-reform. The latter is solved by assuming perfect foresight.

Pre-reform

Individuals maximize the sum of their discounted utility by solving the following problem pre-2008:

$$
\begin{aligned}
\underset{c_1,c_2,c_3,a_1,a_2,l_2^f}{\text{maximize}} \quad & u(c_1, \bar{l}) + \beta \cdot u(c_2, l_2^f) + \beta^2 \cdot u(c_3, 0) \\
\text{subject to} \quad & c_1 + a_1 = \underbrace{a_0 \cdot (1 + r),}_{\text{non-pension assets income}} + \underbrace{w_1^f \cdot \bar{l} \cdot (1 - \tau - \theta),}_{\text{after-tax labor income}} \\
& c_2 + a_2 = \underbrace{a_1 \cdot (1 + r)}_{\text{non-pension assets income}} + \underbrace{w_2^f \cdot l_2^f \cdot (1 - \tau - \theta),}_{\text{after-tax labor income}} \\
& c_3 = \underbrace{a_2 \cdot (1 + r)}_{\text{non-pension assets income}} + \underbrace{b}_{\text{pension income}} + \underbrace{\left(b^{pasis} - b\right),}_{\text{gov. transfer}} \\
& b = (1 + \phi_2)\left[(1 + \phi_1) \cdot \left(B_0 + w_1^f \cdot \bar{l} \cdot \theta\right) + w_2^f \cdot l_2^f \cdot \theta\right], \\
& a_0, B_0, \bar{l} \text{ given}, \\
& c_1, c_2, c_3 \geq 0, \\
& a_1, a_2, l_2^f \text{ unrestricted}.
\end{aligned}
$$

Individuals choose consumption and non-pension assets, and in the second period, also formal labor supply. The first three restrictions are the budget constraints for each period. In $t = 1, 2$ workers have to allocate their resources between consumption and non-pension savings. The fourth condition is Equa-

tion (1.5), which shows how pension income is calculated in a fully funded pension system. The remaining are conditions about predetermined values and decision variables. Notice that in the third period, people only consume, and one could simplify the expression because b appears twice and with different signs. Doing so yields

$$c_3 = \underbrace{a_2 \cdot (1+r)}_{\text{non-pension assets income}} + \underbrace{b^{pasis}}_{\text{guaranteed pension income}}. \tag{1.7}$$

Equation (1.7) shows that individuals receive b^{pasis} as total pension income (i.e., pension income plus government benefits) no matter the actual level of their pension wealth. Thus, the accrual rate is zero in the pre-reform scenario.

Post-reform, Perfect Foresight

The only difference with respect to the pre-reform scenario is Equation (1.7). In $t = 3$, agents know that the benefits are

$$c_3 = \underbrace{a_2 \cdot (1+r)}_{\text{non-pension assets income}} + \underbrace{b}_{\text{pension income}} + \underbrace{b^{pbs} \cdot \left(1 - \frac{b}{PMAS}\right)}_{\text{gov. transfer}}.$$

Organizing the terms results in agents facing

$$c_3 = \underbrace{a_2 \cdot (1+r)}_{\text{non-pension assets income}} + \underbrace{b^{pbs}}_{\text{guaranteed pension income}} + \underbrace{b}_{\text{pension income}} \cdot \underbrace{\left(1 - \frac{b^{pbs}}{PMAS}\right)}_{\text{accrual rate}}. \tag{1.8}$$

From Equation (1.8), I can identify both elements of the new scheme on transfers. First, the guaranteed pension income is represented by b^{pbs}. It can be understood as the intercept of the orange line in Figure 1.1b. Second, the accrual rate is $\left(1 - \frac{b^{pbs}}{PMAS}\right)$. Given that $\frac{b^{pbs}}{PMAS} \simeq 0.3$, if pension income increases by one dollar, the individual receives around 0.7 dollars. The accrual rate is represented by the slope of the orange line in Figure 1.1b.

1.8.3 Analytical Results

Utility without Income Effect

In order to obtain simple analytical results, I use the following utility function:[29]

[29] In Section A.8.4 in the appendix, I show the results for additional utility functions, with similar conclusions.

$$u(c, l^f) = c - \alpha \cdot \frac{(l^f)^{1+\frac{1}{\eta}}}{1+\frac{1}{\eta}},$$

where c is consumption, l^f is formal labor supply, α is a constant, and η is the Frisch elasticity. This utility does not have an income effect, so the formal labor supply will depend on the substitution effect. After solving the optimization problem, I find the formal labor supply for the pre-reform scenario and that for the perfect-foresight scenario are given by Equation (1.9) and Equation (1.10), respectively:

$$l_2^{f,pre} = \left(\frac{w_2^f \cdot (1-\tau-\theta)}{\alpha \cdot \beta \cdot (1+r)} \right)^{\eta}, \tag{1.9}$$

$$l_2^{f,pf} = \left(\underbrace{\frac{w_2^f \cdot (1-\tau-\theta)}{\alpha \cdot \beta \cdot (1+r)}}_{\text{income through labor market}} + \underbrace{\frac{w_2^f \cdot \theta \cdot (1+\phi_2) \cdot \left(1-\frac{b^{pbs}}{PMAS}\right)}{\alpha \cdot \beta \cdot (1+r)^2}}_{\text{income through pension system}} \right)^{\eta}. \tag{1.10}$$

Formal labor supply before the reform depends on wage, tax rate, contribution, and interest rate. Additionally, assuming agents have perfect foresight, formal labor supply depends on pension return, guaranteed pension, and the maximum pension supplemented. The intuition is the following: before the reform, one additional hour of formal work increases labor income by $\frac{w_2^f \cdot (1-\tau-\theta)}{1+r}$, while the contribution to the pension system does not increase pension income, because of the zero accrual rate. After the reform, one additional hour of formal work increases labor income by $\frac{w_2^f \cdot (1-\tau-\theta)}{1+r}$ and pension income by $\frac{w_2^f \cdot \theta \cdot (1+\phi_2) \cdot \left(1-\frac{b^{pbs}}{PMAS}\right)}{(1+r)^2}$. Since the latter term is positive, the formal labor supply in the perfect-foresight scenario is larger than pre-reform.

Including Income Effect

I analyze the case when both substitution and income effects are included:

$$u(c, l^f) = \ln(c) - \alpha \cdot \frac{(l^f)^{1+\frac{1}{\eta}}}{1+\frac{1}{\eta}},$$

to get analytical results, I need to assume either $\eta = 1$ or $\eta = \frac{1}{2}$. The labor supply pre-reform assuming $\eta = 1$ is

$$l_2^{f,pre} = \underbrace{\frac{2 \cdot (1 + \beta + \beta^2)}{\Pi_1^{f,pre} + \sqrt{\left(\Pi_1^{f,pre}\right)^2 + \frac{4 \cdot (1+\beta+\beta^2) \cdot \left(A_{pre}^f\right)^2}{\alpha \cdot \beta}}}}_{\text{income/substitution effect}} \cdot \underbrace{\frac{A_{pre}^f}{\alpha \cdot \beta}}_{\text{substitution effect}}, \tag{1.11}$$

where $A_{pre}^f \equiv \frac{w_2^f \cdot (1-\tau-\theta)}{(1+r)}$ and $\Pi_1^{f,pre} \equiv a_0 \cdot (1+r) + w_1^f \cdot \bar{l} \cdot (1-\tau-\theta) + \frac{b^{pasis}}{(1+r)^2}$. The substitution effect is the same as in Equation (1.9) when $\eta = 1$. The additional term combines the income and substitution effects. The higher the guaranteed pension (b^{pasis}), the higher $\Pi_1^{f,pre}$, and therefore, the lower the formal labor supply. Thus, the income effect decreases the formal labor supply. Notice that the initial pension wealth (B_0) does not affect labor decisions, because of the zero accrual rate.

The formal labor supply post-reform assuming $\eta = 1$ is

$$l_2^{f,pf} = \underbrace{\frac{2 \cdot (1 + \beta + \beta^2)}{\Pi_1^{f,pf} + \sqrt{\left(\Pi_1^{f,pf}\right)^2 + \frac{4 \cdot (1+\beta+\beta^2) \cdot \left(A_{pf}^f\right)^2}{\alpha \cdot \beta}}}}_{\text{income/substitution effect}} \cdot \underbrace{\frac{A_{pf}^f}{\alpha \cdot \beta}}_{\text{substitution effect}}, \tag{1.12}$$

where $A_{pf}^f \equiv \frac{w_2^f \cdot (1-\tau-\theta)}{(1+r)} + \frac{w_2^f \cdot \theta \cdot (1+\phi_2) \cdot \left(1 - \frac{b^{pbs}}{PMAS}\right)}{(1+r)^2}$ and $\Pi_1^{f,pf} \equiv a_0 \cdot (1+r) + w_1^f \cdot \bar{l} \cdot (1-\tau-\theta) + \frac{b^{pbs} + (1+\phi_2) \cdot (1+\phi_1) \cdot (B_0 + w_1^f \cdot \bar{l} \cdot \theta) \cdot \left(1 - \frac{b^{pbs}}{PMAS}\right)}{(1+r)^2}$.

Comparing the substitution effects pre- and post-reform, A_{pre}^f and A_{pf}^f, I can conclude that the substitution effect is always larger in the perfect-foresight case because of the positive accrual rate. By observing $\Pi_1^{f,pre}$ and $\Pi_1^{f,pf}$, it is clear that $\Pi_1^{f,pre} < \Pi_1^{f,pf}$ because of $b^{pasis} < b^{pbs}$. Thus, the income effect is larger in absolute terms in the perfect-foresight scenario.

The post-reform formal labor supply can decrease or decrease. Lower probabilities of increasing formal labor supply come from a larger initial pension wealth (B_0) and a higher pension return at period 1 (ϕ_1). The opposite is true when there exist both a larger contribution rate (θ) and a higher pension return at period 2 (ϕ_2). In Section A.8.1 in the appendix, I show that the informal labor supply remains the same (no income effect) or can decrease (with income effect) after the reform; therefore, the fraction of formal workers can increase.

I repeat the analysis for Groups II–IV. For Group II the intuition is practically the same as for Group I, where the net effect of the reform on formal labor

supply can be positive or negative. This is because the substitution effect and the income effects move in opposite directions. On the other hand, the impacts on Groups III and IV are typically negative because both effects move in the same direction, decreasing the formal labor supply. In sum, the three-period model produces positive or negative impacts on formal labor supply for Groups I–II. For Groups III–IV, the model generates unambiguous implications where the formal labor supply after the reform could only decrease. In the next section, I analyze the impacts of the reform on formal labor supply when pension knowledge is incorporated.

1.8.4 Incorporating Pension Knowledge

As before, agents have accumulated (B_0) pension wealth. However, they believe that $B_0 + \varphi_B$ is their pension savings, where $\varphi_B \in (-\infty, \infty)$ is an error term in their perception. Additionally, people know that their contribution rate is $\theta \equiv \theta^p + \theta^{fee}$, where θ^p and θ^{fee} are the part of the contribution that goes to the individual pension account and the commission charged by the pension administrator, respectively.[30] However, people might perceive

[30] I show empirical evidence of both sources of error in Section 1.5.

$$\theta \equiv \underbrace{(\theta^p + \varphi_\theta)}_{\text{perceived contribution}} + \underbrace{(\theta^{fee} - \varphi_\theta)}_{\text{perceived fee}}, \tag{1.13}$$

where $\varphi_\theta \in (-\infty, \infty)$. If $\varphi_\theta > 0$, workers overestimate their contribution to the pension system. People decide their labor supply at $t = 2$ perceiving a different level of pension savings and contribution rate. Later, at $t = 3$, they observe their actual pension income and realize their error, which affects consumption only in the last period.

Any overestimation (resp., underestimation) of the initial pension wealth produces an overestimation (resp., underestimation) of total wealth. Furthermore, a misunderstanding about the contribution rate affects the substitution effect. To simplify the analysis, I assume that the labor supply in the first period is zero, $\bar{l} = 0$, and therefore, the misunderstanding about the contribution rate affects the labor supply in the second period only through the additional contribution.

The labor supply pre-reform is the same as in Equation (1.11), where $A^f_{pre} \equiv \frac{w_2^f \cdot (1-\tau-(\theta^p+\theta^{fee}))}{(1+r)}$ and $\Pi_1^{f,pre} \equiv a_0 \cdot (1+r) + \frac{b^{pasis}}{(1+r)^2}$. Thus, both errors do not modify the labor supply decision, because of the zero accrual rate. Nevertheless,

the post-reform formal labor supply is

$$l_2^{f,pf} = \underbrace{\frac{2 \cdot (1+\beta+\beta^2)}{\hat{\Pi}_1^{f,pf} + \sqrt{\left(\hat{\Pi}_1^{f,pf}\right)^2 + \frac{4 \cdot (1+\beta+\beta^2) \cdot \left(\hat{A}_{pf}^f\right)^2}{\alpha \cdot \beta}}}_{\text{income/substitution effect}} \cdot \underbrace{\frac{\hat{A}_{pf}^f}{\alpha \cdot \beta}}_{\text{substitution effect}}, \qquad (1.14)$$

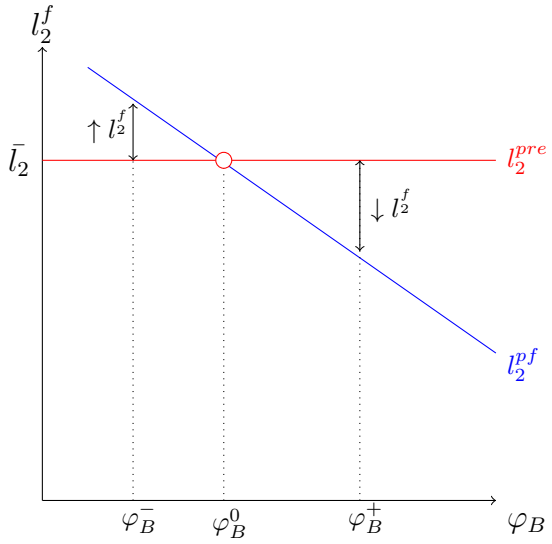
where $\hat{A}_{pf}^f \equiv \frac{w_2^f \cdot (1-\tau-(\theta^p+\theta^{fee}))}{(1+r)} + \frac{w_2^f \cdot (\theta^p+\varphi_\theta) \cdot (1+\phi_2) \cdot \left(1-\frac{b^{pbs}}{PMAS}\right)}{(1+r)^2}$ and

$$\hat{\Pi}_1^{f,pf} \equiv a_0 \cdot (1+r) + \frac{b^{pbs} + (1+\phi_2) \cdot (1+\phi_1) \cdot (B_0+\varphi_B) \cdot \left(1-\frac{b^{pbs}}{PMAS}\right)}{(1+r)^2},$$

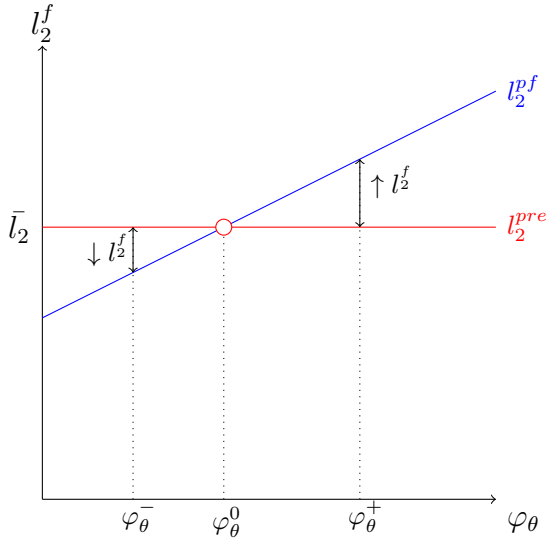
where φ_θ and φ_B are included in $\hat{A}_{pf}^f$ and $\hat{\Pi}_1^{f,pf}$, respectively. If $\varphi_B = \varphi_\theta = 0$, the results are the same as when there is no lack of knowledge. Figures 1.8a and 1.8b show how the labor supply changes for alternative values of φ_B and φ_θ, respectively. In Figure 1.8a, since the formal labor supply pre-reform (l_2^{pre}) is inelastic with respect to φ_B, the formal labor supply post-reform (l_2^{pf}) decreases in φ_B. For people with $\varphi_B \to -\infty$, a lack of knowledge about their initial pension wealth might make them to work more than they are supposed to do, because φ_B reduces the wealth effect on perfect foresight.

Figure 1.8: Formal Labor Supply and Pension System Knowledge

(a) Effects of φ_B on l_2^f



(b) Effects of φ_θ on l_2^f



Note: This figure shows the relationship between formal labor supply and two sources of errors: φ_B and φ_θ. In panel (a) I show how the pre- and post-reform formal labor supplies depend on φ_B. Panel (b) shows the pre- and post-reform formal labor supplies depend on φ_θ.

On the other hand, if people overestimate their perceived contribution rate ($\varphi_\theta > 0$), the labor supply increases through $\hat{A}_{pf}^f$ because of the accrual rate. Since an overestimation of the contribution rate does not change the pre-reform

formal labor supply, because of the zero accrual rate, it increases the labor supply in the perfect-foresight case, as shown in Figure 1.8b.

For people in Groups I and II, the conclusion remains unchanged: the net effect of the reform on labor supply can go up or down even if people know exactly their initial pension wealth and contribution rate. If people in Groups III and IV have either underestimated their initial pension assets ($\varphi_B < 0$) or overestimated their perceived contribution rate ($\varphi_\theta > 0$), the income effect may not overcompensate the substitution effect, and therefore, labor supply might go up after the reform. In conclusion, the three-period model can rationalize decreases or increases in labor supply for Groups I–IV depending on the workers' value of pension accumulation and perception of the group they belong to.

1.9 Conclusions

This paper studies a pension reform that increases government transfers by 50%, changes the accrual rates, and relaxes the eligibility criteria for low-savings retirees. Higher government benefits increase old-age income for retirees, decreasing poverty and reducing gender inequality (Joubert and Todd 2011, 2022). Most people would expect a decreased labor supply after the reform, because of its negative effect on workers' income. Indeed, this is the conclusion of several papers (Otero 2015; Attanasio et al. 2011). However, in this work, I show that men aged 60–64 years do not decrease their labor force participation or hours worked after the reform. In fact, I find that both outcomes increased on average by 11%–14% and 3%–5%, respectively, when I use CASEN and administrative data.

To explain these results, I estimate the heterogeneous effects of the reform using pension assets and the number of monthly contributions. I conclude that people who were eligible for government transfers before the reform do not increase their labor force participation or hours worked (Groups I and II) after the reform. The intuition comes from the fact that the substitution effect does not compensate the income effect. On the other hand, people who were not eligible for the MPG before the reform because of too few contributions (Group III) or excessive pension accumulation (Group IV) increased their hours worked and labor force participation after the reform.

Surprisingly, the positive impacts of the reform on hours worked and labor force participation for Groups III and IV have not been explored before. By constructing perceived groups, I find that people who fail to predict their groups drive the results. Indeed, when estimating the effects of the reform only on

people who know their groups, I find the expected results: negative or zero effects for new eligible workers (Groups III and IV). The same happens when analyzing people who predict correctly they are in Group I: they are the ones who increased their labor supply after the reform. To rationalize these results, I construct a three-period model, concluding that misperceptions of pension savings and contribution rate can generate positive changes in the labor supply.

Countries with high levels of informality under a fully funded pension system (such as Chile, Australia, Argentina, Denmark, Peru, Mexico, and Singapore) must consider unexpected responses of labor supply after massive pension reforms. Governments should realize that people may not fully understand their pension benefits and thus they can make unexpected decisions. Furthermore, the change in benefits or accrual rates should be explained to each individual to help all of them make educated decisions. In that sense, governments may want to require pension fund administrators to incorporate specific individual information, mentioning what benefits people might be eligible for in the future. In this direction, Chile implemented in 2019 a web page named Middle-Class Protection Network (Red de Protección de la Clase Media) where people can see all the available government benefits and check if they satisfy the requirements. The goal was to reduce the misinformation about benefits and application paperwork.[31]

[31] Unfortunately, there is no available data to see how this program has performed since its implementation.

Finally, I believe that future research should focus on accounting for the channels that drive differences in labor decisions among groups of people, especially among misinformed workers. Given the results in this work, I conjecture that the misunderstanding of pension wealth and the overestimation of the accrual rate might be important drivers of unexpected outcomes. Nevertheless, to account for these mechanisms, there is a possible approach that future researchers could follow. Firstly, they should pair a structural model with quasi-experimental evidence from the pre-reform period. In this case, one can think about exploiting the kinks and notches in the retirement benefits schedule pre-2008. This can be done through a bunching estimator, if households are manipulating their labor decisions to maximize benefits, or a regression discontinuity design (RDD), if there is no manipulation. By using one of them, researchers could estimate the behavioral parameters of interest, determining the income and substitution effects. Researchers may then calibrate the structural model with these parameters and then simulate the 2008 reform with the model. The model could tell us how important the information frictions mentioned earlier are.

Chapter 2

Intergenerational Earnings Persistence and the Provision of Public Goods: Evidence from Chile's Constitutional Proces with Javier Cortes-Orihuela, Juan D. Díaz and Pablo Gutiérrez Cubillos

2.1 Introduction

Economic inequality is one of the main concerns of modern societies (Atkinson 2015). In this regard, high levels of economic inequality may be the symptom of a deeper issue: the intergenerational perpetuation of socioeconomic status (Corak 2013). This phenomenon is not only concerning for the achievement of social justice; it may also reflect economic inefficiencies in labor or capital markets, implying that individuals' economic performance is unrelated to their skills, productivity, effort, or creativity, resulting in suboptimal aggregate output (Sen 1999). To alleviate these pernicious issues, societies can decide to enact

redistributive policies that can be implemented via the provision of public goods at the cost of relying on taxes. In this context, scholars have documented that pessimistic expectations of social mobility are linked to increased individual support for redistribution (Alesina et al. 2018). Similarly, low social mobility as measured through objective indicators is also associated with greater support for redistribution (Alesina and La Ferrara 2005). This paper revisits this question by looking at the relationship between intergenerational earnings persistence, as measured through an administrative database of earnings, and popular demand for redistributive policies, as indicated by the the electoral outcome of a national referendum.

In particular, we use the case of Chile, a country that has gained international recognition for its impressive economic growth during the past three decades following the end of the dictatorship led by Augusto Pinochet. Much of this success has been credited to a series of free-market-oriented neoliberal reforms promulgated during the dictatorship and enshrined by the 1980 political constitution. However, and despite being the regional leader in terms of GDP per capita, Chile is also one of the most unequal countries in the world, with a Gini coefficient of 44.4 points PNUD 2017.[32]

This high level of economic inequality, according to supporters of meritocracy, can be justified in a society if income disparities are due to differences in reward for talent, hard work, and skill, rather than luck or transmission of advantages Miller 1996.However, in October 2019, a social outbreak occurred in Chile where the lower and middle classes took to the streets to express their dissatisfaction regarding their perception of unfairness in the distribution of income and political power; the unjust transmission of advantages prevalent across generations; the inequitable social safety net of the country; and the limited presence of the state on the solution of social problems Somma et al. 2021; Barozet et al. 2021; Méndez et al. 2020.[33]

In response to the massive demonstrations, President Sebastián Piñera announced reforms to the healthcare, pension, and education systems that failed to appease the protesters. As violence escalated, the National Congress settled on an "Agreement for Social Peace and the New Constitution" which proposed a national referendum to replace the standing constitution with a new one that would potentially expand the role of the state in the provision of public goods. One year later, Chileans went to the polls, resulting in a landslide win by the approval for a new constitution, which reached 78% of the national vote with the highest total votes in Chilean history at that point in time.

In this context, many researchers have studied the theoretical association between the several dimensions of economic inequality and preferences for re-

[32] Recent literature shows that inequality in Chile is even higher than what the estimates reported by the World Bank suggest (Díaz et al. 2021; Flores et al. 2020; Larrañaga et al. 2021; Fairfield and Jorratt De Luis 2016; Gutiérrez et al. 2015; Gutiérrez Cubillos 2022).

[33] Gouzoulis and Constantine (2021) argue that the private provision of social services has led to a persistent decline in the share of wages.

distribution. For example, traditional models predict that poor and rich individuals prefer higher and lower levels of redistribution, respectively (Meltzer and Richard, 1981; Romer, 1975; Roberts, 1977; Alesina and La Ferrara, 2005). Recent developments in the literature have considered the dimension of social mobility as a way to obtain a more nuanced and dynamic view of preferences towards redistributive policies Benabou and Ok 2001; Piketty 1995; Alesina and Angeletos 2005; Bénabou and Tirole 2006. These models, in general, argue that the median voter may not necessarily prefer greater levels of redistribution, provided that their expectations of upward mobility are optimistic. For example, Piketty (1995), Alesina and Angeletos (2005), and Benabou and Ok (2001) conclude that people with optimistic beliefs about individual effort or upward mobility will prefer less redistribution.

Thus, our first contribution is to propose a theoretical model to explain the relationship between intergenerational earnings mobility and the desire for the provision of public goods. Following Hindriks and Myles (2013), we assume that the disputes over the public good are resolved through majority voting.[34] In our model, the counterpart to public goods is a proportional tax.In addition, there are two ways in which parents can secure the well-being of their children: through the transmission of private parental earnings and through the provision of public goods. We assume that both devices are substitutes. Furthermore, we model the private transmission of earnings via the intergenerational earnings elasticity (IGE), where a higher IGE is related to higher earnings persistence. Hence, we argue that an increase in the IGE is related to a lower desire for public goods (and thus lower desired levels of taxes). The mechanism is that a higher IGE increases the expected child earnings for any parental earnings level, so the marginal benefit of increasing public goods decreases due to substitutability. As far as we know, this is the first paper to model preferences towards redistribution and social mobility through the IGE.

[34] In this context, the preferred provision of public goods is the amount that is closer to the individual desired level of the public good.

Our framework differs from previous models in the sense that we focus on the interaction between public goods provision and intergenerational earnings persistence, rather than emphasizing expectations of upward mobility. In that sense, we extend and complement several papers in the literature that have studied the role of taxes using intra-generational redistribution Moffitt et al. 1998; Alesina and La Ferrara 2005; Alesina and Angeletos 2005. For example, Alesina and Angeletos (2005) find that in their "European" equilibrium people support high level of redistribution and taxes when inequality comes from luck and not effort. Our model, however, concludes that a higher level of public goods can be present when the relationship between parents' and children's labor income

is low. Therefore, parents see the public good as an insurance for the children's well-being.

Our second contribution is to test our theoretical result by using the 2020 Chilean national plebiscite as a proxy of preferences for the provision of public goods. To do so, we estimate the IGE at the municipality level using a unique and novel administrative dataset that links parents and children and their respective labor earnings in the formal private sector. In this context, we are the first paper that estimates intergenerational earnings mobility indicators for Chile at the municipality level. Hence, our paper intersects with the literature studying the geographical variation of intergenerational mobility Acciari et al. 2022; Chetty et al. 2014; Deutscher and Mazumder 2020; Cortés Orihuela et al. 2022. We then use our IGE estimates to assess the plausibility of our theoretical proposition, finding that municipalities with higher levels of IGE (and thus greater earnings persistence) are also more likely to vote against the drafting of a new constitution (and hence are less prone to support reforms that increase redistribution).[35] We highlight that our results are not causal in nature, yet the partial correlations estimated in this work are robust to model specification, to the inclusion of several socioeconomic controls such as median income or income inequality, and to the use of alternative indicators of intergenerational persistence, such as the rank-rank correlation and quintile transition probabilities.[36]

Our article is novel in the sense that we use administrative earnings data to construct objective measures of intergenerational earnings persistence at the municipality level, unlike Alesina and La Ferrara (2005) and Siedler and Sonnenberg (2012), who use proxies such as job occupation to capture intergenerational mobility or panel survey data to construct intergenerational earnings profiles, respectively.[37] Furthermore, we complement the literature that predominantly uses survey questions to capture sentiments towards redistribution Alesina and Angeletos 2005; Lai et al. 2021; Gugushvili 2019; Siedler and Sonnenberg 2012 by using actual electoral results.

Thus, our paper connects to the literature studying the link between preferences for redistribution and social mobility. This literature, which has seen great development due to the efforts of Alberto Alesina, in general, shows that beliefs about mobility are key drivers of redistributive attitudes. Concretely, Alesina and La Ferrara (2005) find that support for redistributive policies decreases as objective measures of expected future income improve. In addition, their findings confirm the link between preference towards lower redistribution and optimistic beliefs about hard work and merit. Alesina et al. (2011) build on their previous research by accentuating the role of historical experiences, cultural factors, and personal history in a way that mirrors the foundational work

[35] Corneo and Grüner (2002) and Corneo (2001) study empirically the relationship between individual demand for redistribution and social mobility using data for OECD countries, Germany and the U.S., respectively. Both papers find that *self-assessments* about having high chances of upward mobility affects attitudes towards redistribution. Meanwhile, Livert and Gainza (2018) find that public investment is mediated by political budget cycles favoring the relatively better-off areas.

[36] In this regard, our analysis is similar to Roine et al. (2009) and Vergara (2022), whose results are based on robust correlations instead of causal conclusions.

[37] Alesina et al. (2018), Gugushvili (2019) and Lai et al. (2021) focus on the association between redistribution and perceptions of mobility, rather than actual mobility.

of Piketty (1995). Finally, Alesina et al. (2018) is the most complete and robust work to date studying the relationship between intergenerational mobility and preferences for redistribution. Concretely, the authors contend that pessimistic expectations of social mobility increase the support for redistribution as suggested by a randomized treatment design.[38]

Finally, our work is also related to several papers that have studied the effect of the 2019 Chilean social outbreak on different outcome variables such as democratic preferences, labor demands in the private sector, and challenges in the health system Medel et al. 2021; Singh and Williams 2021; Méndez et al. 2020. We extend this literature by using the results of the 2020 Chilean plebiscite that emerged due to the social outbreak and by estimating intergenerational mobility measures at the municipality level through a unique administrative dataset.

The rest of the paper is structured as follows: Section 2.2 describes the constituent process, Section 2.3 develops the theoretical model, Section 2.4 performs the empirical analysis, and Section 3.5 concludes.

[38] Additionally, Mayer and Lopoo (2008) study the relationship between government spending and intergenerational economic mobility using data for the U.S. They find greater intergenerational mobility in high-spending states compared to low-spending ones.

2.2 Chile's constituent process

This section describes the constituent process, where we present the political circumstances behind it as well as the context of the 1980 constitution. In addition, we argue that the vote in support of changing the constitution can be used as a proxy for preferences in favour of a higher provision of public goods. Finally, we describe the results of the 2020 referendum and their connection with intergenerational persistence.

2.2.1 Context of the constituent process

At the outset of October 2019, the Chilean government announced an increase of 30 Chilean pesos (0.05 USD) in the fare of the Santiago subway, resulting in a final fee of 1.2 USD. Despite the fact that this policy would imply that a minimum wage worker would spend around 12% of their income in transportation, the authorities implemented the price increase, justifying it with the raising costs of providing the service.[39] Ultimately, the measure backfired as enraged citizens began protesting by massively evading the subway fare.

Protests spread throughout the country a couple of days later, and what started as demonstrations against the fare increase convened in declarations against the prevailing neoliberal economic model and the scarce state presence in the solution of social issues. As violence escalated, it then did not take long for the demands to fall on the replacement of the standing political constitution,

[39] Specifically, due to increases in the price of oil and the deteriorating exchange rate of the Chilean peso.

which was identified as the root institutional support of the Chilean socioeconomic model.[40]

The current constitution was originally written in 1980 during the military dictatorship of Pinochet.[41] Given this, the demonstrations were based on 1) that the Magna Carta had an illegitimate origin and 2) the text enshrined a "subsidiary state" that does not directly provide the benefits that have to do with health, education, or pensions but rather a state that is limited to supervising how the private sector provides those goods. This fact is documented with the significant privatizations of the Pinochet regime in its essential services such as electricity, utilities, and the pension, education, and health systems Cavallo 2008, 2012; González et al. 2020; Aldunate et al. 2021. The privatizations came together with a decrease in the collection power of the state to provide public goods and concern about the efficiency of public spending Echeverría 1980; PIIE 1984; Velasco 1994; Bautista et al. 2020. One of the protesters' demands was that the state should have greater participation and involvement in providing essential goods to reduce the significant income inequality in Chile.

2.2.2 Voting to change the constitution as a proxy of preferences for the provision of public goods

After approving the process to establish a new constitution in Chile, one of the important questions was to determine what the Chilean citizens expected of the constituent process. In this regard, various local surveys attempted to quantify public opinion on the new constitution, particularly the role of the state in the provision of public goods and services in general.

For example, according to Criteria's survey conducted at the beginning of 2020, 80% of the participants reported that it is preferable that in the new constitution the solution of social problems be mainly in the hands of the state. Moreover, 78% of the participants in this survey stated that it is preferable that the rights to health and education be guaranteed and not only recognized by the new constitution (Criteria 2020). These results support the idea that voting to change the constitution can be seen as a preference to have a stronger social state, which was claimed in the massive protests that began in October 2019.[42] In addition, survey data from Activa (2020) collected four weeks before the referendum revealed that the principal concerns of voters were healthcare, unemployment, pension plans, crime, and inequality.

[40] For more details, see Garcés (2019) and Maguire (2021).

[41] Albeit the constitution has been emended 59 times, it is still characterized by the subsidiary role given to the state. For more details, see Huneeus and Sagaris (2007) and Somma (2021).

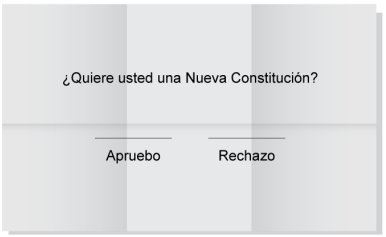
[42] Importantly, in this survey, the declared preferences are clearly in favor of approval: 67% of the participants indicate that they would approve the process to establish a new constitution.

2.2.3 Plebiscite results

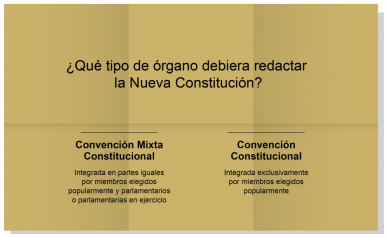
On November 10, 2019, the government approved the future application of a process to establish a new constitution. The referendum consisted of two questions asked through two separate ballots as illustrated in Figure 2.1.

Figure 2.1: Ballots for the 2020 Chilean national plebiscite[†]

(a) First ballot: "Do you want a New Constitution?"



(b) Second ballot: "What kind of body should write the New Constitution?"



Note: Ballots for the 2020 Chilean national plebiscite, written in Spanish. Both ballots were given simultaneously to citizens and the vote between both was independent, meaning that individuals could choose any option in both ballots.

Figure 2.1 shows the ballots in the Spanish language. Concretely, the questions were:

1. Do you want a New Constitution? where the options were “Approve" or “Reject", as illustrated in Figure 2.1a.

2. What type of body should draft the New Constitution? The options were, 1) “Constitutional Mixed Convention," an entity made up of 50% by directly elected constituents and 50% by members of the current Congress, or 2) “Constitutional Convention," an assembly made up of 100% elected constituents, as represented in Figure 2.1b.

Finally, one year later, on October 25, 2020, the referendum occurred. A total of 7,569,082 people participated in the referendum, making it the election with the second highest number of voters in Chilean history, reaching a 50.95% participation rate. The “Approve" and “Constitutional Convention" options were the winners by a wide margin, reaching 78% and 79% of the national vote, respectively. The "Approve" and "Constitutional Convention" options won in all regions except for five municipalities: Antartica, Colchane, Las Condes, Lo Barnechea, and Vitacura. It should be noted that Antartica and Colchane

are small towns with fewer than 600 voters, most of them military personnel, civilian employees of the army, and their relatives. Nevertheless, Las Condes, Lo Barnechea, and Vitacura are big and rich municipalities located in the northeast of the Metropolitanian region. As we show in Section 2.4, they are among the most persistent municipalities at the top in terms of intergenerational mobility, with probabilities of top-persistence, i.e., the conditional probability that a child is in the top quintile given that the parent is in the top quintile, above 67%.

2.3 A simple model of intergenerational mobility and the provision of public goods

We develop a model that links intergenerational earnings persistence to the provision of public goods. We follow and extend the political economy model of Hindriks and Myles (2013). Disagreement over the level of public goods is resolved via majority voting, where all agents prefer a provision of public good that is closest to their desired level.

Suppose that there is an odd number, $H > 2$, of parents, whose earnings ranges from y_{min} to $y_{max} < \infty$. The government provides a public good G that is financed through a proportional tax τ. Agents face preferences for the level of the public good G and the tax rate τ. In this regard, the government budget constraint is:

$$G = \tau \sum_{i=1}^{H} y_i^p = \tau \cdot H \cdot \mu, \tag{2.1}$$

where y_i^p are the earnings of parent i and μ is the average of parental earnings. Agents also care about their children's pre-tax earnings. For simplicity, assume that parental preferences are over G and τ and we assume that children will decide their own G and τ. Furthermore, the public good gives utility to the parent i directly and indirectly through its effect on their child's utility. Thus, the utility function of the parent i can be written as:

$$u(\tau, G) = \log\left((1-\tau) \cdot y_i^p\right) + B(G) + \xi \cdot \log(y_i^c + \theta \cdot G), \tag{2.2}$$

where y_i^c are the earnings of parent i's child.[43] As stated above, in this model, the public good directly affects the utility of the agent i through $B(G)$ and also indirectly trough the effect on the child's welfare. We assume that $B'(\cdot) > 0$ and $B''(\cdot) < 0$, i.e., the agent i directly values the public good but with decreasing marginal utility, and we assume that a proportion θ of the public good can

[43] See Section B.2 for an alternative utility form.

affect the utility received by the child.[44] Furthermore, $\xi > 0$ is interpreted as the intergenerational discount factor. Finally, we assume that the child's earnings are endogenously defined by the parental earnings through the following stochastic process:

[44] θ can be interpreted as the percentage of public good expenditures that improves the child's welfare.

$$\log(y_i^c) = \alpha + \beta \cdot \log(y_i^p) + \epsilon_i, \tag{2.3}$$

where ϵ_i is an independent and identically distributed continuous random variable. In this context, β can be interpreted as the intergenerational elasticity of earnings, i.e., the effect of a 1% increase in parental earnings on child earnings. This parameter represents the transmission of earnings between parents and children. On the other hand, α is a measure of the state of the economy since a higher α implies higher wages for children independent of the parental earnings. In this case, each agent votes for a value of G by solving the following optimization problem:

$$\max_G \mathbb{E}[u(\tau, G)|y_i^p] \quad \text{s.t.} \quad \log(y_i^c) = \alpha + \beta \cdot \log(y_i^p) + \epsilon_i \text{ and } \tau = \frac{G}{H \cdot \mu}. \tag{2.4}$$

Because the sum of strictly concave functions is strictly concave, the objective function is strictly concave. Similarly, the objective function is continuous as it is a sum of continuous functions. Moreover, given that the number of individuals H is finite, and that the objective function is increasing with respect to G, the choice set is compact. This implies that this problem has an unique global maximum for public goods provision for each individual i. Furthermore, notice that this value decreases as long as y_i^p increases. Hence, we can use the median voter theorem because individuals' preferences are single peaked with respect to G. This implies that the value for G and for τ are given by the earnings level of the median parent y_{median}^p. Given this setting, if we show that each individual chooses a lower value of G when the intergenerational earnings elasticity β increases, then it follows that an increase in β reduces the public good level preferred by the median voter and therefore, the provision of the public good G.

Proposition 1 *G is decreasing with respect to β when $y_{median}^p > 1$.*

Proof. See Appendix B.1. ■

The former result can be explained by the fact that in our setup, the valuation of the public good for the children is contingent on children's earnings. In fact, a higher β is implicitly replacing the intergenerational transmission of resources through G. Increasing the private earnings that can be transmitted

via β decreases the valuation of the public good, thus reducing G. Indeed, using equation (B.3), we can notice that the first element on the right-hand side represents the marginal cost of the public good, while the second and third terms represent the marginal benefits.[45] Thus, a higher value of β decreases the marginal benefit of an additional unit of G on the children but the marginal cost, which is the increase of taxation, remains constant when β increases. In the following section, we test the main prediction of our model by estimating β for municipalities and through the result of the referendum that acts as a proxy for preferences for the provision of public goods.

[45] Since the former comes from the direct benefit of a higher provision of the public good, the latter is the indirect benefit through children's utility.

2.4 Empirical analysis

In this section, we conduct an empirical analysis to assess the plausibility of the previous theoretical result. We start by describing the dataset used; then we define the empirical strategy; and finally, we present the empirical results with the respective robustness checks and heterogeneity analysis.

2.4.1 Datasets and materials

Referendum data

As we discussed in section 2.2.2, we use the referendum results as a proxy of preferences for the provision of public goods. The data on the referendum's results were gathered by the Chilean Electoral Service. To measure the popular opposition to the new constitution, we calculate the share of the vote rejecting the new constitution, considering both null and blank votes when computing this ratio.[46] The share of rejection is available at the municipality level for the whole country.

[46] The referendum also asked whether the new constitution should be written by a partially elected body or a fully elected one. As the correlation between the share of the vote in favour of a mixed convention and the share of the vote against the new constitution is high, it is reasonable to believe that both indicators capture the same latent variable. Therefore, in our analysis, we focus on the share of the vote against the new constitution.

Intergenerational earnings data

We collect information on the labor earnings of children and their parents using the Chilean government's unemployment insurance program (UIP) database. The UIP is a formerly voluntary program that became mandatory in 2002. It includes all 18-year-old employees working formally in the private sector. We have information about monthly labour earnings of children and their parents from September 2002 to December 2018.

We link children and their parents using a database provided by Chile's Civil Registry Office (CRO). The CRO keeps track of all deaths, marriages, and births that occur in the country. Births are supported by a birth certificate, which

includes unique identification numbers called RUN (Rol Unico Nacional) of the parents and child at the moment of birth. For individuals, the RUN number is the same as the individual tax ID number.

We use this identification number to associate children with their parents in the UIP database. For our baseline analysis, we obtained a sample of children composed of individuals between 28 and 33 years old in 2018. We link these observations to a sample of parents composed of individuals between 42 and 87 years old in that same year.[47]

We measure parental earnings as the five-year average of monthly gross formal labour earnings between 2003 and 2007.[48] For example, if a parent worked 32 months between 2003 and 2007, we define earnings as total labour income divided by 32. Furthermore, we only include parents who worked for at least twelve months throughout the period. If both parents worked for at least twelve months during the period, earnings are calculated as the arithmetic mean of their earnings in accordance with Chetty et al. (2014) and Corak (2020). Earnings for children are measured in the same way in the 2014-2018 period.[49]

We omit observations with no income since these individuals may work in the public sector, the informal sector, or not be covered by the UIP due to the program's non-mandatory character in its early years. This is similar to Chetty et al. (2014) and Corak (2020) with the difference that they impute missing earnings as 0.[50]

Residential data and municipal assignment

We link our baseline sample of parent-child associations with residential data in order to assess how intergenerational mobility varies with the municipality in which children grow up.

We assign children to municipalities by linking their residential addresses while they attended 12th grade in school. In this case, the child's age is between 17 and 18 years old, depending on the month that the child was born. We obtain this information from administrative records provided by the Ministry of Education of Chile. If the child's residential address while attending 12th grade is not available, we use the most recently available residential address while enrolled from 7th to 11th grade in school (when the child is 13-17 years old). We end up with 93.17% (575,633 linkages) of the children's sample linked to their residential address.

Naturally, the municipality where a child grew up does not necessarily correspond to the municipality where she lives as an adult at age 28-33 in 2018. However, in addition to the fact that our data set does not allow us to identify the residence of children after 12th grade, the analysis of the geography of

[47] Due to age differences between children and parents, our baseline sample might be affected by life-cycle bias (Engzell and Mood, 2021). To account for this possible issue, we perform a robustness check in which we control for parents' and children's age, finding that our main conclusions remain valid.

[48] Our measure of income is before personal taxes, Social Security and health-related contributions.

[49] Following Chetty et al. (2014), we choose the years to measure parental earnings when children attend high school, while we measure children's earnings nearby prime-working age.

[50] In Section 58 we perform a robustness check in which we impute these missing earnings as 0. Also, in our baseline sample, we pool fathers and mothers.

intergenerational mobility focuses on how the available resources and opportunities in neighborhoods in which children grow up shape their earnings during adulthood (Chetty et al. 2014; Chetty and Hendren 2018).

2.4.2 Empirical strategy

To study the association between the persistence of earnings between two subsequent generations and popular support towards a greater provision of public goods, we proceed in two stages. First, we estimate the IGE, which provides a measure of the intergenerational association between the log permanent earnings of children and their parents. Concretely, we characterize the IGE at the municipality-level by permanently assigning each child to their residence while attending 12th grade during high school.[51] Then, we estimate this elasticity for each municipality through simple OLS regressions using individual-level data, but restricting the sample to those municipalities with more than 250 parent-child linkages. We end up with 231 municipalities that satisfy this condition.[52] In sum, we fit equation (2.5) for each municipality of Chile:

$$\log\left(y_{i,m}^{c}\right) = \alpha_m + \beta_m \cdot \log\left(y_{i,m}^{p}\right) + \varepsilon_{i,m}, \quad \text{for } m = 1, 2, \ldots, 231, \quad (2.5)$$

where $y_{i,m}^{c}$ are the earnings of child i who lives in municipality m, $y_{i,m}^{p}$ are the earnings of the child's parents living in municipality m, β_m is the intergenerational earnings elasticity for municipality m (IGE$_m$), and $\varepsilon_{i,m}$ is the error term. Figures B.1 and B.2 in Appendix depict the variation in the IGE and its confidence intervals for Chilean municipalities.[53]

Having obtained our main measures of intergenerational earnings persistence, we proceed to the second stage, in which we empirically assess the prediction of our model by estimating the following baseline regression model via OLS:

$$\text{Reject}_m = \delta \cdot \text{IGE}_m + \lambda_p + X_m\Gamma' + u_m, \quad (2.6)$$

where Reject$_m$ is the share of the vote against a new constitution in municipality m, λ_p are province fixed effects, X_m is a matrix of control variables at the municipality-level, and u_m is the error term. The parameter of interest is the IGE coefficient δ. In particular, a positive estimate for this parameter would suggest that municipalities with higher intergenerational persistence in earnings are less likely to support the drafting of a new constitution that would potentially allow reforms which could permit a greater provision of public goods. Thus, a positive $\hat{\delta}$ would be consistent with the predictions of our model developed in Section 2.3. Our second stage bears close resemblance to the empirical approach employed in Alesina and La Ferrara (2005), with the distinction that they use

[51] Chile is divided into 16 regions, the first-level administrative division of the country. Each region is divided into provinces, the second-level administrative division. In total, there are 56 provinces, each one divided into municipalities, the third and lowest-level administrative division. There are 345 municipalities in Chile.

[52] In this, we follow Chetty et al. (2014) who restrict their sample to commuting zones with 250 or more parent-child linkages.

[53] One particular concern is the fact that the IGE exhibits low variability. Specifically, Figures B.1 and B.2 suggest that many IGEs may be statistically indistinguishable from each other. To address this issue, we perform a linear test of hypothesis in which we consider the null hypothesis $\beta_i = \beta_j \; \forall \; i \neq j$ where β_m corresponds to the IGE of municipality m. Our results indicate that we can reject the equality between all IGEs at the 99% confidence level.

individual-level data over time and a discrete dependent variable to measure the support towards redistribution.[54]

In our baseline regression model, we include municipal median income as a covariate to account for the effect of the median voter as described in our theoretical model. Similarly, we include the average age and the average number of minors within a household to account for the impact of the intergenerational discount rate ξ. We also incorporate the Gini coefficient in our matrix X_m, as levels of inequality could affect levels of support for redistributive policies as suggested by Meltzer and Richard (1981). Since the degree of observable poverty in society could impact demand for redistributive policies in ambiguous directions (Alesina and La Ferrara 2005; Luttmer 2001), we incorporate average household overcrowding levels as a covariate in order to account for the levels of housing precariousness within a municipality.[55] In addition, we include the share of immigrants within a municipality to account for the possible effects of the "compensation hypothesis", according to which concerns about labour market risks caused by increased immigration lead to a higher demand for redistribution (Alesina et al. 018a). Next, we incorporate the unemployment rate. Specifically, it has been argued that experiencing unemployment may lead to increased risk aversion and a change in attitudes toward government intervention (Alesina and La Ferrara 2005). Similarly, we include local per capita government expenditures as a way to account for the effect of opportunistic fiscal political cycles, a phenomenon that has been widely documented in the literature of political economy (Akhmedov and Zhuravskaya 2004). We also incorporate a binary indicator that equals one, provided that the major of a municipality belongs to the right-wing coalition. This is to regulate the high degree of party ideologization within Chilean politics (Acuña-Duarte and Salazar 2021). We decide to include the municipal voter turnout in the referendum as the last control variable. Concretly, it has been argued that predictions based on the median voter approach significantly improve when accounting for the level of electoral turnout, which could reflect skews in the distribution of income. This link might be particularly concerning for the lower and middle parts of the earnings distribution (Mahler, 2008).

Information on median income, income inequality, age, the average number of minors within a household, and the unemployment rate comes from the National Socioeconomic Characterization Survey (CASEN), a representative cross-sectional survey. Indeed, it is the official source to compute inequality at the municipality level in Chile. Finally, information on overcrowding and the share of immigrants can be found at the Registro Social de Hogares, a dataset constructed by the Chilean government based on administrative records. Elec-

[54] Alesina and La Ferrara (2005) use a discrete index as the main variable which measures the level of individual support to reduce income differences between the rich and the poor. The possible answer goes from 1 to 7 being 1 "no support" and 7 "extremely support."

[55] Overcrowding is defined by the Chilean government as the ratio of people living in a household to the number of rooms in a household.

toral turnout and the political alignment of local majors were obtained from the Chilean Electoral Service (SERVEL); while local municipal per capita expenditures were obtained from the National System of Municipal Information (SINIM).

Finally, to account for the uncertainty in the estimation of the IGE, we bootstrap the first — Eq. (2.5) — and second stage — Eq. (2.6) — in a single routine when computing standard errors. Table 2.1 reports summary statistics for all variables included in the baseline regression model.

Table 2.1: Summary statistics

Variable	Obs.	Mean	Std. dev.	Min	Max
Reject	231	0.221	0.089	0.092	0.667
IGE	231	0.185	0.075	0.006	0.480
Median income	231	0.235	0.125	0.123	1.284
Gini	231	0.406	0.059	0.292	0.606
Average age	231	38.159	2.740	28.950	46.117
Number of minors	231	0.716	0.148	0.356	1.366
Overcrowding	231	0.059	0.012	0.012	0.090
% Immigrants	231	0.025	0.038	0.002	0.302
Unemployment rate	231	0.078	0.033	0.004	0.188
Municipal per capita expenditures	231	0.290	0.140	0.028	1.151
Right-wing major dummy	231	0.39	0.49	0.00	1.00
Voter turnout	231	0.48	0.08	0.28	0.68

Note: We use the CASEN survey to obtain median income, income inequality, average age, the average number of minors within a household, and the unemployment rate. These statistics are computed using the survey weights provided by the Chilean government. Overcrowding and the share of immigrants were obtained through the "Registro Social de Hogares" dataset. Reject, electoral turnout and the right wing major dummy rely on data from the Chilean Electoral Service. Municipal per capita expenditures were obtained from the National System of Municipal Information (SINIM). Municipal per capita expenditures are measured in million Chilean pesos, while median income is measured in thousand Chilean pesos.

A lower IGE is interpreted as a more intergenerationally mobile society because a marginal increase in parental earnings correlates with a small increase of children's income. We find that the average IGE across municipalities is 0.185, which is similar to what other papers have found when using administrative data

for several countries such as the Italy, U.S, Canada, and Australia (Deutscher and Mazumder 2020; Acciari et al. 2022; Chetty et al. 2014).

2.4.3 Empirical results

In this section we present the main empirical results. The baseline empirical estimates are reported in Table 2.2 from Section 2.4.3, while we explore a battery of sensitivity test consisting of alternative specifications and estimation strategies in Section 58. Finally, we explore heterogeneities in our results in Section 61.

Baseline estimates

Table 2.2 reports the OLS estimates for equation (2.6). Specifically, column (1) shows the results using a simple unadjusted regression model; column (2) adds controls related to the distribution of income in addition to province fixed effects; column (3) incorporates variables that account for the effect of ξ, i.e., the intergenerational discount factor, from the theoretical model as depicted in Equation (2.2); columns (4) to (6) progressively include more controls that account for confounding variables known to affect preferences for redistribution.

Column (1) suggests that the IGE and the share of the vote against a new constitution have a positive and significant association, as predicted by our model. Specifically, we find that a one percentage point increase in the IGE is related to a 0.5355 percentage point increase in the share of the vote against a new constitution.

In column (2), we account for the effect of the median income and Gini coefficient of each municipality. This leads to a change in the IGE coefficient, which decreases to 0.297. However, this effect is still significant at the 99% confidence level. Furthermore, we find that both median income and the Gini coefficient have a positive association with the share of the vote against the new constitution. The former finding is consistent with the standard median voter model (Meltzer and Richard 1981), while the latter could be explained by the fact that inequality in Chile is mainly driven by high incomes, leading to higher rejection levels.[56]

[56] See, for example, Fairfield and Jorratt De Luis (2016) and Flores et al. (2020).

Column (3) reports the IGE coefficient after adjusting for the effects of the average age and the mean number of minors in a household at the municipality level. It can be seen that the magnitude of the IGE coefficient stabilizes while both the average age and the number of minors are positively associated with the share of votes against a new constitution, a fact consistent with our model, since

an increase in the intergenerational discount factor leads to a higher demand for public goods.

Column (4) shows the results once overcrowding, the share of immigrants, and the unemployment rate are incorporated into the set of control variables. Again, the main coefficient for the IGE remains positive and significant. Moreover, it can be seen that overcrowding and the share of votes in rejection of the new constitution are negatively associated, suggesting that municipalities facing precarious housing conditions might be more inclined to support the constitutional change. We also find that the share of immigrants is negatively related to the share of the vote against a new constitution, which is consistent with the above mentioned "compensation hypothesis" (Alesina et al. 018a). Furthermore, we see a negative but statistically insignificant relationship between the unemployment rate and the share of the vote against the new constitution.

Finally, column (5) reports estimates of the complete regression model. We find a positive and significant association between fiscal expenditures executed by local governments and the share of votes against a new constitution.[57] On the contrary, both the presence of right-wing incumbent mayors and voter turnout are negatively correlated with the share of rejection votes. The former might indicate that anti-incumbent attitudes at the local level could have a slight effect on rejection votes, while the latter is consistent with the fact that high turnout suggests more low-income voters at the polls who prefer greater redistribution.

In column (5), a one percentage point increase in the IGE is related to a 0.228 percentage point increase in the share of the vote against a new constitution. In sum, our baseline estimates suggest that areas with low social mobility, and therefore high income persistence, are less supportive of reforms that increase redistribution. This finding stands in contrast with the literature, which recognizes that pessimistic expectations of social mobility increase support for redistribution (Alesina et al. 2018). Similarly, these results contradict the literature studying the link between objective mobility and demand for redistribution, which finds a negative association between increased mobility and support for redistribution (Alesina and La Ferrara 2005). In Section 61 we explore how intergenerational earnings persistence interacts with median income and economic inequality in an attempt to reconcile our findings with the literature.

[57] This could be explained by the fact that there is a proportion of municipal expenditures that are financed through municipal taxes, such as property taxes, automobile circulation permits, and fines. Thus, government expenditures may capture the municipal socioeconomic level as well.

Table 2.2: Relationship between the IGE and the share of the vote against a new constitution

	(1)	(2)	(3)	(4)	(5)	(6)
Variables	Reject	Reject	Reject	Reject	Reject	Reject
IGE	0.535***	0.297***	0.305***	0.284***	0.228***	-1.217***
	(0.029)	(0.026)	(0.025)	(0.024)	(0.026)	(0.095)
Median income		0.392***	0.423***	0.386***	0.218***	0.241***
		(0.008)	(0.009)	(0.010)	(0.008)	(0.008)
Gini		0.152***	0.142***	0.142***	0.155***	-0.642***
		(0.018)	(0.018)	(0.017)	(0.014)	(0.049)
Average age			0.003***	0.011***	0.008***	0.008***
			(0.000)	(0.000)	(0.000)	(0.000)
% Minors			0.098***	0.240***	0.178***	0.168***
			(0.007)	(0.009)	(0.007)	(0.006)
Overcrowding				-0.417***	-0.658***	-1.057***
				(0.077)	(0.071)	(0.051)
% Immigrants				0.591***	0.477***	0.537***
				(0.025)	(0.024)	(0.021)
Unemployment rate				-0.033	0.034	0.102***
				(0.028)	(0.024)	(0.022)
Municipal per capita expenditures					0.222***	0.168***
					(0.006)	(0.008)
Right wing major					-0.003***	-0.006***
					(0.001)	(0.001)
Voter turnout					-0.009**	-0.052***
					(0.021)	(0.021)
Intercept	0.092***					
	(0.006)					
Province FE	No	Yes	Yes	Yes	Yes	Yes
Observations	231	231	231	231	231	231
Adj. R-squared	0.131	0.766	0.774	0.796	0.843	0.870

Bootstrap standard errors in parentheses.

*** p<0.01, ** p<0.05, * p<0.1

Note: We use the CASEN survey to obtain median income, income inequality, average age, the average number of minors within a household, and the unemployment rate. These statistics are computed using the survey weights provided by the Chilean government. Overcrowding and the share of immigrants were obtained through the "Registro Social de Hogares" dataset. Electoral turnout and the right wing major dummy rely on data from the Chilean Electoral Service. Municipal per capita expenditures were obtained from the National System of Municipal Information (SINIM). Municipal per capita expenditures are measured in million Chilean pesos, while median income is measured in thousand Chilean pesos. standard errors are reported in parentheses. Bootstrap percentile confidence intervals are used to determine the significance level. We employ the number of parent-child links in a municipality as weights when estimating both the model's parameters and their respective confidence intervals.

Robustness checks

Our baseline IGE estimates exclude missing earnings from the analysis, which could lead to an overestimation of permanent earnings for individuals with frequent periods of unemployment. This, in turn, could lead to biased intergenerational mobility estimates. Therefore, our first robustness check examines the sensitivity of the results once all missing earnings are imputed as 0, a procedure in line with Chetty et al. (014a), Chetty et al. (2014) and Corak (2020).[58] These results are reported in Table B1, where it can be observed that the coefficient of interest remains statistically significant and its magnitude increases once missing earnings are included.

An additional concern is the fact that parents are observed when they are substantially older than their children, which could lead to life cycle bias for intergenerational mobility measures, especially in the case of the IGE (Engzell and Mood 2021). Hence, our second robustness test is to re-estimate equation (2.5) but including year of birth dummies for children and parents. The results are reported in Table B2. As before, our conclusions remain virtually unchanged.[59]

In line with the previous analysis, we perform a third and fourth sensitivity check in which we use alternatives indicators of intergenerational persistence such as the rank-rank correlation and the circle of privilege transition probability (p_{55}) rather than the IGE (See Section B.3 for a formal definition). Since the rankings in the earnings distribution are determined at earlier ages and are difficult to change throughout the age distribution, rank-based indicators may be robust to life cycle bias (Corak 2020). The estimates for the rank-rank correlation and the p_{55} transition probabilities are reported in Table B3 and B4, respectively. The estimates suggest that our results are robust to alternative indicators of intergenerational persistence.

Our fourth robustness analysis is to remove outliers from our regression analysis. Concretely, we remove three municipalities that were the only ones within our baseline sample to reject the constitutional change. Furthermore, these municipalities exhibit remarkably high levels of intergenerational persistence in earnings. Thus, there might be a concern that the observed results are driven by these three communes. The estimates resulting from this pruning procedure are reported in Table B5, which confirm the robustness of our results.

As a fifth robustness check, we assess how our results vary when the Gini coefficient and the median income are built with the subsample of formal private sector workers from the CASEN survey, and are considered as control variables in equation (2.6). As can be seen in Table B6, our main conclusions remain valid.

[58] Unlike Chetty et al. (014a), Chetty et al. (2014) and Corak (2020), we preserve the restriction that individuals must have at least twelve months of positive earnings during the period of interest to be included in the sample.

[59] This is unsurprising, as there are only 3,391 parents born before 1938 (69 or older), representing less than 0.2% of the sample.

Furthermore, there may be concerns about potential collinearities between our control variables given their nature as measures of socioeconomic status, in addition to concerns about our choice of the set of covariates. To overcome these issues, we perform a sixth robustness analysis in which we expand the set of control variables, X_m from equation (2.6), and adjust this model using the least absolute shrinkage and selection operator (LASSO).[60] These estimates are provided in Table B7, where the main coefficient of interest, $\hat{\delta}$, survives the penalization process and is statistically significant at the 1% level.

[60] The LASSO method was first introduced by Tibshirani (1996) and it is commonly used for the estimation of coefficients and the selection of covariates in linear models with many predictors that exhibit high levels of multicollinearity.

Finally, the estimates reported so far are consistent with the model presented in Section 2.3 that predicts a lower provision of public goods given an increase in the IGE. However, an alternative theory could also explain the referendum results: citizens might be sanctioning the government's poor performance. We contend that this possibility is unlikely, as additional evidence suggests no systematic link between IGM measures and approval ratings of the president nine months before the referendum. Concretely, we perform a placebo test in which we continue with the methodology proposed in Section 2.4.2 but replacing the outcome variable with the approval rating of the president. In this exercise, finding a significant estimate for the IGE would cast doubt on our leading interpretation of the results. The coefficients for these regressions are reported in Table B8. In particular, we find no systematic, statistically significant relationship between the IGE and the approval ratings of the president. On the other hand, these results might suggest that factors other than intergenerational persistence measures play a role in the president's approval ratings.

Heterogeneities

As stated above, we explore how our results can be reconciled with the literature. We re-estimate equation (2.6), but this time we include two new interaction terms: IGE $\times$ median income and IGE $\times$ Gini, where both the Gini coefficient and the median income have been standardized by subtracting their means and dividing by their standard deviations to make the resulting estimates easier to interpret. The results are presented in Table 2.3.

Table 2.3: Role of median income and inequality in the relationship between the IGE and the share of the vote against a new constitution

Variables	(1) Reject	(2) Reject	(3) Reject
IGE	0.184***	0.152***	0.130***
	(0.024)	(0.024)	(0.024)
IGE × Median income	0.122***		0.082***
	(0.036)		(0.031)
IGE × Gini		0.225***	0.202***
		(0.014)	(0.015)
Median income	-0.008	0.027***	0.005
	(0.013)	(0.001)	(0.011)
Gini	0.010***	-0.043***	-0.038***
	(0.001)	(0.003)	(0.003)
Average age	0.007***	0.008***	0.008***
	(0.001)	(0.000)	(0.001)
% Minors	0.153***	0.168***	0.152***
	(0.025)	(0.006)	(0.021)
Overcrowding	-0.685***	-1.057***	-1.035***
	(0.079)	(0.052)	(0.062)
% Immigrants	0.473***	0.537***	0.528***
	(0.037)	(0.022)	(0.032)
Unemployment rate	0.037	0.102***	0.097***
	(0.021)	(0.022)	(0.021)
Municipal per capita expenditures	0.202***	0.168***	0.160***
	(0.019)	(0.008)	(0.015)
Right wing major	-0.003	-0.006***	-0.006**
	(0.002)	(0.001)	(0.002)
Voter turnout	0.025	-0.052***	-0.025**
	(0.022)	(0.021)	(0.022)
Province FE	Yes	Yes	Yes
Observations	231	231	231
Adj. R-squared	0.853	0.870	0.874

Bootstrap standard errors in parentheses.

*** p<0.01, ** p<0.05, * p<0.1

Note: We use the CASEN survey to obtain median income, income inequality, average age, the average number of minors within a household, and the unemployment rate. These statistics are computed using the survey weights provided by the Chilean government. Overcrowding and the share of immigrants were obtained through the "Registro Social de Hogares" dataset. Electoral turnout and the right wing major dummy rely on data from the Chilean Electoral Service. Municipal per capita expenditures were obtained from the National System of Municipal Information (SINIM). Municipal per capita expenditures are measured in million Chilean pesos, while median income is measured in thousand Chilean pesos. standard errors are reported in parentheses. Bootstrap percentile confidence intervals are used to determine the significance level. We employ the number of links in a municipality as weights when estimating both the model's parameters and their respective confidence intervals.

First, column (1) from Table 2.3 includes the interaction between the IGE and median income into the model. It suggests that the popular opposition towards redistribution increases with the IGE, but this increase varies with the level of median income of the municipality. Specifically, we find that the relationship between the IGE and the share of the vote rejecting the constitutional change increases by 12.2 percentage points given a one-standard-deviation increase in the municipality's median income. That is, while the persistently poor sectors might oppose redistributive reforms, they do not oppose them as strongly as the rich sectors, who benefit more from earnings persistence and for whom rejecting redistributive reforms means securing their socioeconomic status.

Second, column (2) from Table 2.3 reports the estimates but only includes the interaction between the IGE and the Gini coefficient. It shows that the association between the IGE and the share of the vote against a new constitution also varies with inequality levels within a municipality. We find that the IGE is positively associated with the reject vote, and that the magnitude of this association increases as inequality expands. Since inequality in Chile is largely driven by the upper tail of the earnings distribution (Gutiérrez Cubillos 2022), more unequal municipalities are more likely to reject the constitutional change in order to protect their socioeconomic status. Finally, column (3) from Table 2.3 includes both interaction terms. It can be seen that the results are consistent, since both median income and inequality mediate the effect of the IGE in the same directions as in columns (1) and (2), respectively.

In general, these estimates help to reconcile our baseline results with the findings of the related literature. Specifically, by differentiating between the support of poor municipalities and the support of rich sectors towards redistribution, we can observe that the rich benefit from income persistence, as this means securing their affluence, and therefore reject redistribution, while the poor do not benefit from this persistence as strongly, and therefore do not reject redistribution through state intervention as much.

2.5 Conclusion

There is a strong relationship between economic inequality and the intergenerational economic persistence of socioeconomic status. Higher intergenerational persistence is associated with greater inequality (Corak 2013). In contexts with higher economic inequality and higher intergenerational persistence, the provision of public goods could be an instrument to increase redistribution, which

may limit the levels of economic inequality and may reduce the levels of intergenerational persistence.

In this paper, we study the relationship between intergenerational mobility and preferences for public goods. We theoretically show that a lower intergenerational mobility is associated with a lower desire for public goods, even if the public good is financed through a proportional tax. Our empirical analysis confirms this theoretical result by using a unique case study for a developing country.

Indeed, Chile is one of the most unequal countries in the world, with a Gini coefficient of 0.44 as of 2017 (World Bank 2017). Economic inequality, intimately linked to the 1980 free-market-oriented constitution imposed by the dictatorship of Augusto Pinochet, has been widely cited in popular discourse as the leading cause of the 2019 social outburst, which led to the 2020 constitutional referendum. This paper complements this notion by arguing that the static dimension of inequality fails to explain the referendum results thoroughly. Instead, we argued that the persistence of earnings through generations is also a relevant driving force behind the social frustrations exhibited by Chilean society. Our analysis suggests that given the lower levels of intergenerational mobility in some municipalities, the higher levels of economic inequality may not necessarily be smoothed with preferences by the median voter for a higher level of public goods that may limit the levels of economic inequality. Furthermore, our research sheds light on the mechanisms shaping institutional change, preferences towards redistribution, and their relation to intergenerational economic mobility.

Future research on the relationship between intergenerational mobility and preferences for public goods could focus on two fronts: first, the relationship between intergenerational mobility and progressive taxation should be explored; second, this empirical analysis should be performed in other contexts and for other countries with different political systems and economic development stages.

Chapter 3

How Much Should We Trust TSTSLS Intergenerational Mobility Estimates?: Evidence From A Developing Country with Javier Cortes-Orihuela, Juan D. Díaz and Pablo Gutiérrez Cubillos and Gabriel I. Villarroel

3.1 Introduction

The field of intergenerational mobility — i.e., the extent to which some key characteristics and outcomes of individuals differ from those of their parents (OECD, 2008) — has experienced an increase in popularity during recent years. This subject is often studied by measuring the strength of the relationship between children's and their parents' earnings (Emran and Shilpi 2019; Black et al. 2011; Blanden 2013), which can be analyzed through the intergenerational earn-

ings elasticity (IGE) or the rank-rank correlation.[61] These indicators are of tax policy relevance, as a more progressive national tax system could be justified to restrict the heritability of the endowment of privileges.

On the basis of data linking children's and parents' earnings, both measures can be estimated with ordinary least squares (OLS) regressions. In this context, the recent literature for developed countries (Chetty et al. 2014; Corak 2020; Deutscher and Mazumder 2020; Acciari et al. 2022) is characterized by the use of detailed administrative data to compute such estimates. However, such data are still scarce in developing countries, where the literature of intergenerational mobility has mostly used survey data along with procedures to impute missing parental earnings (Narayan et al. 2018; Torche 2005, 2014; Ferreira and Veloso 2006; Jiménez 2011; Grawe 2001). In this paper, we revisit how reliable imputation-based estimates are when compared to the estimates derived from administrative data.

A commonly employed alternative for studying intergenerational mobility in the context of missing parental earnings is the Two-Sample Two-Stage Least Squares estimator (TSTSLS), proposed by Klevmarken (1982)[62]. The TSTSLS method uses information from two samples: one sample with information on children's earnings and their parents' characteristics excluding earnings (main sample), and another sample with information on the characteristics and earnings of a different group of parents (sample of pseudo parents). Then, the TSTSLS proceeds in two stages. First, the parents' earnings in the main sample are imputed via a Mincer type regression, which is adjusted with the sample of pseudo parents. Second, the IGE (or the rank-rank correlation) is estimated using the information of children's earnings in the main sample and their parents' imputed earnings (from the first stage). Despite its popularity, the literature has extensively documented that TSTSLS IGE estimates are inconsistent with respect to the true effect of parental earnings on children's earnings, as several decompositions have shown that asymptotic bias arises if either the variables included in the parents' equation have a direct effect on child's earnings or the prediction of the parental equation is not perfect; i.e., the R^2 of the estimated Mincer equation is less than one (Solon 1992; Björklund and Jäntti 1997; Nicoletti and Ermisch 2008; Jerrim et al. 2016; Bloise et al. 2021). However, none of these papers study the differences between intergenerational mobility measures estimated through TSTSLS with missing parental earnings versus OLS with administrative data linking children's and parents' earnings.

Thus, our first contribution is to perform a novel decomposition of the IGE estimated through TSTSLS that allows us to find a lower bound for the IGE estimated with administrative data under plausible conditions. Concretely, our

[61] The IGE (rank-rank correlation) is a statistical measure of the intergenerational association between the income (rank) of children and the income (rank) of their parents.

[62] This procedure is also called the Two-Stage Instrumental Variable (TSIV) strategy (Angrist and Krueger 1992).

decomposition partitions the TSTSLS IGE estimate into i) the IGE estimated using administrative data, ii) a prediction bias — which arises whenever children's earnings correlate with the difference between actual parental earnings and imputed parental earnings, and iii) a variance bias — that appears whenever the variance of imputed parental earnings and actual parental earnings differ. We propose a simple procedure to vanish the variance bias that relies on the multiple imputation literature (Little and Rubin 2019; Rubin 2004, 1996). Following this correction, we establish conditions under which the TSTSLS IGE estimate provides a lower bound for the IGE estimated with administrative data. An additional result of this decomposition is the fact that the rank-rank correlation estimated via TSTSLS only exhibits prediction bias with respect to the rank-rank correlation estimated with administrative data due to the fixed variance of imputed and actual parental earnings ranks.

Our second contribution is to measure the scope of the prediction bias, variance bias, and the validity of our lower bound TSTSLS methodology through an Empirical Monte Carlo (EMC) approach that relies on administrative data from a developing country. Concretely, we use Chilean administrative earnings records for males to estimate the IGE and the rank-rank correlation. Then, we compare our estimates through a simulated TSTSLS context, where we observe children earnings and parental characteristics, but we proceed as if parental earnings were missing.[63] In our experiment, we obtain a sample from the dataset that we split into two: the first subsample is used to estimate a Mincer-type equation for parental earnings, while the second subsample is used to impute parental earnings through the parameters estimated with the first subsample. We repeat this procedure 1,000 times and show that, on average, the TSTSLS IGE estimate overstates the administrative IGE by 73%. However, correcting the variance bias in our estimates using a technique from the multiple imputation literature leads to a TSTSLS IGE estimate which is a lower bound for the administrative IGE and that is robust to a set of alternative specifications of the Mincer equation. Furthermore, we empirically show that the uncorrected TSTSLS rank-rank correlation also provides a lower bound for the administrative rank-rank correlation, a result that is consistent with the fact that TSTSLS rank-rank correlations only exhibit prediction bias.

[63] Acciari et al. (2022) perform a similar exercise for Italy.

Our EMC exercise leads to recommend two simple practices to estimate intergenerational mobility under scarcity of administrative data and missing parental earnings: first, when the IGE is estimated through TSTSLS, the imputation procedure for parental earnings should be corrected using stochastic regression imputation or other methods from the multiple imputation literature in order to eliminate the variance bias; second, researchers should estimate

the rank-rank correlation using TSTSLS. Our empirical results show that both estimates provide a lower bound for the linked administrative data-based IGE and rank-rank correlation, respectively.

The rest of the paper is organized as follows. Section 3.2 describes the measurement of intergenerational mobility using administrative data and the TSTSLS method, taking into consideration theoretical and empirical developments. Section 3.3 presents our lower bound methodology using a novel decomposition of TSTSLS biases. Section 3.4 performs EMC exercises with administrative data to evaluate the performance of the TSTSLS estimator, and to test our lower bound methodology. Finally, section ?? concludes.

3.2 Measurement of intergenerational mobility: An overview

In this section we briefly describe the traditional regression method to estimate intergenerational mobility. First, we present the definition of the intergenerational earnings elasticity (IGE) and rank-rank correlation. Then, we outline the Two-Sample Two-Stage Least Squares estimator that is used to compute mobility measures in the absence of parental earnings data. Next, we do a short summary of the literature that uses this procedure to estimate intergenerational mobility along with the literature that relies on administrative earnings data linking parents' and children's earnings. Finally, we present an international comparison between the TSTSLS estimates and the administrative data-based metrics.

3.2.1 The Intergenerational Earnings Elasticity

The following equation gives the standard empirical specification for estimating the intergenerational earnings elasticity:

$$y_i^c = \alpha + \beta \cdot y_i^p + \varepsilon_i, \tag{3.1}$$

where y_i^c is the logarithm of permanent individual earnings for child i, y_i^p is the logarithm of his parent's permanent earnings, α is the constant, and ε_i is the error term.[64] The coefficient β is the "intergenerational elasticity" (IGE). This indicator is one of the most well-known intergenerational mobility measures, since it provides a statistical measure of the intergenerational association between the earnings of children and their parents (Corak 2013). A lower number is interpreted as a more intergenerationally mobile society.

[64] Solon (1992) discusses that equation (3.1) could not be the true income model. In particular, omitted variables could affect child earnings that are correlated with parent's income. One example of this variable is parent's education. Following Chetty et al. (2014), Connolly et al. (2019), and Acciari et al. (2022) we assume that equation (3.1) is the correct model. We also ignore any measurement error in the children's and parent's income. De Haan and Plug (2011) study the impacts of censoring, while An et al. (2020) and Durlauf et al. (2017) discuss the linearity assumption in detail.

3.2.2 The Rank-Rank correlation

Another approach to estimate intergenerational mobility is to use rank-rank correlations. The rank-based regression was first introduced by Jureckova (1971) and Jaeckel (1972). More recently, Dahl and DeLeire (2008) used the rank-rank regression in the context of relative mobility. This measure gained broad visibility following the paper by Chetty et al. (2014), who used it because of its stability and robustness to life cycle measurement (Mazumder 2016; Chetty et al. 2017, 014a).

The rank-rank correlation is estimated through the following OLS regression:

$$r_i^c = \alpha_{rr} + \beta_{rr} \cdot r_i^p + \xi_i, \tag{3.2}$$

where r_i^c is the child i's ranking over the children earnings distribution, r_i^p is the parent i's ranking on the parents distribution, α_{rr} is the constant, and ξ_i is the error term. The coefficient β_{rr} is the rank-rank correlation, which measures the strength of the correlation between a child's ranking and their parents' position in the earnings distribution. As before, a small number reflects a society with a high degree of intergenerational mobility.

3.2.3 Two-Sample Two-Stage Least Squares Estimator

The Two-Sample Two-Stage Least Squares (TSTSLS) estimator has been used to measure intergenerational mobility when matched child-parent data of earnings is unavailable. The TSTSLS estimator uses retrospective information on parents' socioeconomic background and a sample of pseudo parents to impute parental incomes via Mincer equations. Although this method has been mostly used to estimate the IGE, it can also be used to estimate the rank-rank correlation. The TSTSLS requires two samples. The main sample contains information on children's earnings and demographic information on their parents. The auxiliary sample consists of pseudo-parents, that is, earlier data from the parent's cohort with earnings and demographic information. On the basis of these two samples, the estimation proceeds in two steps. First, the auxiliary sample of pseudo-parents is used to estimate a Mincer equation by OLS:

$$y_j^{pp} = \omega' z_j^{pp} + v_j, \tag{3.3}$$

where y_j^{pp} is the earnings of pseudo-parent j, z_j^{pp} is the pseudo-parent vector of time-invariant characteristics, and v_j is the error term. Then, after estimating ω', we predict y_i^p as $\hat{y}_i^p = \hat{\omega}' z_i{}^p$ which is a deterministic imputation where z_i^p is the vector of characteristics for the actual parent i.

Having imputed the earnings for all parents in the main sample, the second stage of the TSTSLS estimator consists of quantifying the relationship between $\hat{y}_i^p$ and y_i^c, where y_i^c denotes the earnings of child i in the main sample. Specifically, to estimate the IGE via the TSTSLS, we regress the log of y_i^c on the log of $\hat{y}_i^p$ by OLS, and keep the estimate of the slope, while for the rank-rank correlation based on the TSTSLS, we regress r_i^c on $\hat{r}_i^p$ by OLS, and keep the estimate of the slope, where r_i^c is the ranking of earnings of child i in the children's earnings distribution and $\hat{r}_i^p$ is the ranking of imputed earnings of parent i in the parents' imputed earnings distribution.

Many researchers have documented the biases plaguing the TSTSLS estimator. Concretely, Solon (1992) first shows that an instrumental variable approach using fathers' education as a predictor in the first stage consistently estimates the intergenerational earnings correlation if either (i) father's education does not influence son's earnings, or (ii) father's education and earnings are perfectly collinear. Since fathers' education tend to be positively associated with both sons' and fathers' earnings, Solon (1989) argues that the IV estimator is upwardly inconsistent. Björklund and Jäntti (1997) expand this approach to the case in which sons' earnings and parental characteristics come from two separate samples. Nicoletti and Ermisch (2008) generalize the previous insights to the case of a first stage characterized by multiple predictors. Specifically, they show that the TSTSLS estimator is consistent provided that (i) the R^2 of the first stage equals 1 or (ii) the predictors of parental earnings have no effect on children's earnings. They further show that, under the assumption that all instruments are positively correlated with parental earnings and have a positive effect over children's earnings, then the TSTSLS estimator will exhibit upward bias. Recently, Jerrim et al. (2016) extend the framework of Nicoletti and Ermisch (2008) to the intergenerational earnings correlation and attempted to quantify the consistency of TSTSLS estimates when using a range of different instrumental variables. Finally, Bloise et al. (2021) test several machine learning approaches to minimize the out-of-sample prediction error of parental income, finding that the relaxed least absolute shrinkage and selection operator outperforms other methods. Their insights complement our research, as they provide an objective criterion to minimize the projection bias that characterizes the TSTSLS estimates and which we explore in Section 3.1. Albeit the drawbacks of the TSTSLS have been widely documented, this method is still commonly used for the study of intergenerational mobility in both developed and developing countries.[65]

[65] Jerrim et al. (2016) compiles a list of 30 articles that rely on this method. Since then, new articles using TSTSLS have been published, such as Barbieri et al. (2020) and Narayan et al. (2018).

3.2.4 Empirical estimation of intergenerational mobility using TSTSLS

Most intergenerational mobility estimates from developing countries use imputation procedures such as the TSTSLS methodology Narayan et al. 2018; Torche 2005, 2014; Ferreira and Veloso 2006; Jiménez 2011; Grawe 2001. Nonetheless, researchers have also applied it to study mobility in developed countries Barbieri et al. 2020; Bloise et al. 2021; OECD 2018; Narayan et al. 2018; Olivetti and Paserman 2015. A particularly relevant article in this regard is Acciari et al. (2022), who estimate mobility measures directly using administrative data linking children's and parents' earnings, but also through a simulated TSTSLS setting. Their exercise and results are similar to ours, as their findings suggest the presence of an upward bias in the TSTSLS procedure. They argue that the main challenge of using TSTSLS when estimating intergenerational mobility measures is the issue of finding a suitable instrument that predicts parental income while also having no independent effect over child's earnings beyond parental income. We complement this by arguing that the differences in variance between imputed and actual parental earnings are also a significant source of bias in the estimation of the IGE.

3.2.5 Estimation of intergenerational mobility using administrative data linking parents' and children's earnings

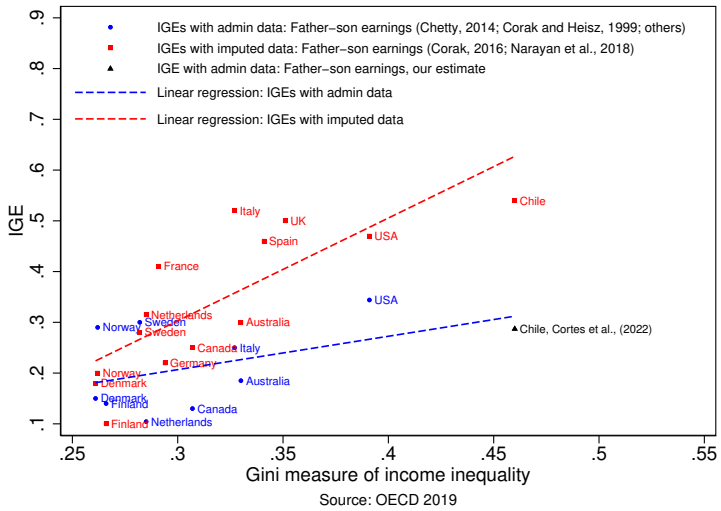
Most papers that examine intergenerational mobility through administrative data linking parents' and children's earnings focus on the case of developed economies. For example, Chetty et al. (2014) and Chetty et al. (014a) estimate intergenerational mobility measures of for the U.S. using administrative tax data. For Canada, Corak and Heisz (1999) constitutes the first paper to study intergenerational mobility using administrative records. More recently, Corak (2020) studies the geography of intergenerational mobility in Canada utilizing census data. Regarding other developed countries, Heidrich (2017) uses administrative data to investigate intergenerational mobility in Sweden, which Deutscher and Mazumder (2020) also does for Australia. Similarly, Acciari et al. (2022) compute intergenerational mobility measures for Italy using administrative data from tax returns. On the other hand, intergenerational mobility estimates based on administrative records are scarce for developing countries. Concretely, Leites et al. (022b) study recent trends in intergenerational earnings mobility in Uruguay using social security records. Similarly, Cortés-Orihuela et al. (2022) and Cortés-Orihuela et al. (2023) study nonlinearities in intergenerational earnings mobility in Chile through administrative earnings data, while

Meneses (2021) estimates intergenerational income mobility using administrative records for children and self-reported income for parents for the same country. To the best of our knowledge, these are the only papers studying intergenerational income mobility in the context of a developing economy.

3.2.6 Administrative data IGE estimates versus TSTSLS estimates: an international comparison

The estimation of intergenerational mobility can be dramatically different depending on whether researchers use administrative data linking parents' and children's earnings or survey data along with imputation methods. To understand the effects of such discrepancies, Figure 3.1 shows the Great Gatbsy curve (Corak 2013). This compares the Gini Index on the x-axis to the IGE on the y-axis for selected OECD countries. The Gini index measures the extent to which the distribution of income among individuals within an economy deviates from a perfectly equal distribution. The blue circles show the IGE estimates using administrative data linking parents' and children's earnings for the U.S., Canada, Sweden, Netherlands, Norway, Finland, Denmark, Italy, and Australia (Corak and Heisz 1999; Lindahl 2008; Carmichael et al. 2020; Jantti et al. 2006; Deutscher and Mazumder 2020; Chetty et al. 2014; Acciari et al. 2022). Meanwhile, the red squares present the TSTSLS estimates from survey data (Corak 2006; Narayan et al. 2018).

Figure 3.1: Gatsby Curve for selected OECD countries



As can be seen, Figure 3.1 shows that the TSTSLS tends to overestimate the IGE estimated with administrative data. Concretely, the U.S., Canada, and

Australia exhibit overestimation ratios of 1.36, 1.92, and 1.62, respectively (Corak and Heisz 1999; Chetty et al. 2014; Deutscher and Mazumder 2020; Corak 2006; Narayan et al. 2018). Naturally, the magnitude of the TSTSLS bias depends on each country's characteristics and the Mincer equation's predictive power. This discrepancy may affect what we know about the relationship between inequality and intergenerational mobility. In particular, this curve seems to have a higher gradient when we use TSTSLS instead of administrative data which can be explained by the bias from TSTSLS. In the next section we decompose this bias and present a simple procedure to eliminate one of its sources.

3.3 Decomposing and bounding the Two-Sample Two-Stage Least Squares Estimator

In this section we present a novel decomposition of the TSTSLS estimator. Our decomposition splits the TSTSLS IGE estimation into the administrative data IGE estimate, a projection bias, and a variance bias. On the other hand, we decompose the TSTSLS rank-rank correlation into the administrative data parameter, and a projection bias. Then, we propose a simple procedure that under general conditions delivers a lower bound for the TSTSLS IGE estimation.

3.3.1 The Sources of Bias in the TSTSLS Estimator of the IGE

Assume that we obtained a prediction for parental earnings using equation 3.3. Let us call this prediction $\hat{y}_i^p$, then we study the following relationship between y_i^c and $\hat{y}_i^p$

$$y_i^c = \alpha^{TSTSLS} + \beta^{TSTSLS} \cdot \hat{y}_i^p + \psi_i, \tag{3.4}$$

where α^{TSTSLS} is the constant, β^{TSTSLS} is the elasticity between the imputed parent's earnings and child's earnings, and ψ_i is an error term. We estimate β^{TSTSLS} as:

$$\hat{\beta}^{TSTSLS} = \frac{\hat{cov}\left(y^c, \hat{y}^p\right)}{\hat{var}\left(\hat{y}^p\right)}. \tag{3.5}$$

There are many ways to obtain $\hat{y}^p$, i.e., the imputed values of parents' earnings in the main sample. For example, it is possible to use different specifications of the Mincer equation or to use the method showcased by Bloise et al. (2021). Let us call $\Omega = \{\hat{y}^p(j)\}_{j=1}^{\infty}$ the set of imputations for y^p. Each element $\hat{y}^p(j)$ of Ω defines a different parameter, call it $\beta(j)^{TSTSLS}$ with its respective TSTSLS estimator given by $\hat{\beta}(j)^{TSTSLS}$. Importantly, we can establish a relationship

between the $\hat{\beta}$, which is the IGE estimated using administrative data linking parents' and children's earnings, and $\hat{\beta}^{TSTSLS}(j)$ in the following Proposition.

Proposition 2 *$\hat{\beta}^{TSTSLS}(j)$ is a function of $\hat{\beta}$, $\eta(j) \equiv \frac{cov(y^c, y^p - \hat{y}^p(j))}{var(y^p)}$, and $\kappa(j) \equiv \frac{var(\hat{y}^p(j))}{var(y^p)}$ $\forall j$.*

Proof. We know that

$$\hat{\beta} = \frac{cov(y^c, y^p)}{var\left(y^p\right)} = \frac{cov(y^c, y^p + \hat{y}^p(j) - \hat{y}^p(j))}{var\left(y^p\right)},$$

$$\hat{\beta} = \frac{cov(y^c, y^p - \hat{y}^p(j))}{var\left(y^p\right)} + \frac{cov(y^c, \hat{y}^p(j))}{var\left(y^p\right)} \cdot \frac{var\left(\hat{y}^p(j)\right)}{var\left(\hat{y}^p(j)\right)}.$$

Define $\eta(j) \equiv \frac{cov(y^c, y^p - \hat{y}^p(j))}{var(y^p)}$, and $\kappa(j) \equiv \frac{var(\hat{y}^p(j))}{var(y^p)}$. Using $\hat{\beta}^{TSTSLS}(j) = \frac{cov(y^c, \hat{y}^p(j))}{var(\hat{y}^p)}$ we have that

$$\hat{\beta}^{TSTSLS}(j) = \frac{\hat{\beta} - \eta(j)}{\kappa(j)}. \tag{3.6}$$

■

Equation (3.6) relates the probability limit of $\hat{\beta}^{TSTSLS}$ and $\hat{\beta}$. Given this, we can define two sources of bias for $\hat{\beta}^{TSTSLS}$ with respect to $\hat{\beta}$. We call $\kappa(j)$ the variance bias and $\eta(j)$ the projection bias. The variance bias stems from the fact that $var\left(\hat{y}^p(j)\right) \neq var\left(y^p\right)$, i.e., the variance of imputed parents' earnings is not equal to the actual variance of parents' earnings. In particular, when we use TSTSLS, we obtain $var\left(\hat{y}^p(j)\right) \leq var\left(y^p\right)$ because the variance of any OLS prediction will be at most as large as the variance of the variable to be predicted. This implies that $\kappa(j) \leq 1$ for any TSTSLS procedure.

On the other hand, the projection bias is present when $cov(y^c, y^p - \hat{y}^p(j)) \neq 0$, i.e., when the difference between predicted and actual parents' earnings correlates with children's earnings. In addition, any bias generated by $E(\varepsilon_i | y_i^p) \neq 0$ is going to be directly reflected on both probability limits.[66] Although it is often stated that $\hat{\beta}^{TSTSLS}$ is upward inconsistent (Blanden 2013), we show in equation (3.6) that it can actually be upwards or downwards biased. See Jerrim et al. (2016) who exemplify this point.

[66] Notice that when $E(\varepsilon_i | y_i^p) = 0$, $\hat{\beta}^{TSTSLS}(j) = \frac{\beta - \eta(j)}{\kappa(j)}$.

3.3.2 A procedure to find a lower bound for the administrative data IGE estimate using the TSTSLS IGE method

Equation (3.6) shows that the TSTSLS IGE bias with respect to the administrative data IGE depends on two terms: $\eta(j)$, the projection bias, and $\kappa(j)$, the

variance bias. Given the structure of the OLS procedure, $\kappa(j)$ is always less or equal than 1, even when the regression is correctly specified since the imputation will not fully reflect the uncertainty of the missing data. In consequence, the variance bias amplifies $\hat{\beta} - \eta(j)$. Thus, to find a lower bound, it is necessary to vanish the variance bias. This can be accomplished with a technique from the literature of missing data imputation (Little and Rubin 2019; Rubin 2004, 1996), where the difference in variance between parents' earnings and the imputed parents' earnings has been deeply discussed. Intuitively, we increase the variance of the imputed parents ($\hat{y}^p$). For this, we add an intermediate step in the TSTSLS procedure: when we predict $\hat{y}^p$ as $\hat{\omega}' z^p$ we add a white noise term with mean 0 and variance equal to the variance of the residuals from the pseudo-parents regression — i.e., the variance of $e^v = y^{pp} - \hat{\omega}' z^{pp}$. With this, the new imputation, called $\hat{y}^{pv}$, replicates the variance of the pseudo-parents sample y^{pp}. This procedure is usually called stochastic regression imputation. Concretely, we impute parents' earnings in the main sample using:

$$\hat{y}^{pv} = \hat{\omega}' z^p + \epsilon^v, \tag{3.7}$$

where $\epsilon^v \sim \mathcal{N}(0,\ \hat{\sigma}_v^2)$ and where $\hat{\sigma}_v^2$ is the estimated variance of the residuals from Mincer equation fitted with the sample of pseudo-parents ($y^{pp} = \omega' z^{pp} + v$). This implies that $var(\hat{y}^{pv}) = var(y^{pp})$. That is, under the assumption that the variance of the parental earnings observed in the administrative data is equal to the variance of the pseudo parents sample ($var(y^p) = var(y^{pp})$), we are able to eliminate the variance bias by adding noise to the imputed earnings of the parents in the main sample. This procedure allow us to obtain $\kappa = 1$. Then, by estimating the IGE using $\hat{y}^{pv}$ as imputed parental earnings information, Equation (3.6) converges to:

$$\hat{\beta}^{TSTSLS}(j) = \hat{\beta} - \eta(j). \tag{3.8}$$

That is, by Equation (3.8), the TSTSLS IGE estimate provides a lower bound for the linked administrative IGE if $\eta(j) > 0$; i.e., when the correlation between children earnings and the projection error of the parental earnings is positive. This means that the projection error increases (decreases) as children earnings increase (decrease). In the following section, we show numerically that η is positive through an Empirical Monte Carlo setting. This suggests that the variance-corrected TSTSLS IGE estimator yields a lower bound for the linked administrative IGE estimate.

3.3.3 The Source of Bias in the TSTSLS Estimator of the Rank-rank Correlation

To estimate the rank-rank correlation, we first construct the ranking of imputed earnings ($\hat{r}^p(j)$) by computing the percentiles from the imputed parents' earnings distribution, as in Barbieri et al. (2020) and Olivetti et al. (2018). Thus, the second stage of the TSTSLS procedure that estimates the rank-rank correlation is:

$$r_i^c = \alpha_{rr}^{TSTSLS} + \beta_{rr}^{TSTSLS} \cdot \hat{r}_i^p + \varsigma_i, \tag{3.9}$$

where α_{rr}^{TSTSLS} is the constant, β_{rr}^{TSTSLS} is the rank-rank correlation under the TSTSLS approach, and ς_i is the error term. In this model we can also establish a relationship between $\hat{\beta}_{rr}$ and $\hat{\beta}_{rr}^{TSTSLS}(j)$.

Proposition 3 *$\hat{\beta}_{rr}^{TSTSLS}(j)\ \forall j$ is a function of $\hat{\beta}_{rr}$ and $\theta(j) \equiv cov\left(r^c, r^p - \hat{r}^p(j)\right)$.*

Proof. The probability limit can be written as

$$\hat{\beta}_{rr}^{TSTSLS}(j) = \frac{cov\left(r^c, \hat{r}^p(j)\right)}{var\left(\hat{r}^p(j)\right)} = \frac{cov\left(r^c, r^p + \hat{r}^p(j) - r^p\right)}{var\left(\hat{r}^p(j)\right)}.$$

Notice that $var\left(\hat{r}^p(j)\right) = var\left(r^p\right) = \frac{1}{12}$ because both distributions are uniform between zero and one. Therefore, the rank-rank correlation does not suffer from variance bias. Thus, the problem translates to:

$$\hat{\beta}_{rr}^{TSTSLS}(j) = \frac{cov\left(r^c, r^p\right)}{var\left(r^p\right)} - \frac{cov\left(r^c, r^p - \hat{r}^p(j)\right)}{var\left(r^p\right)},$$

$$\hat{\beta}_{rr}^{TSTSLS}(j) = \hat{\beta}_{rr} - 12 \cdot \theta(j),$$

where $\hat{\beta}_{rr} = \frac{cov(r^c, r^p)}{var(r^p)}$, and $\theta(j) \equiv cov\left(r^c, r^p - \hat{r}^p(j)\right)$. ∎

That is, there is only one source of bias, which is given by the error of projection between the true parental ranking and the ranking of predicted earnings. For this result we do not require any additional methodology and/or assumption. This result is important for the recent literature that estimates intergenerational mobility indicators. Indeed, Chetty et al. (2014) recommend the use of rank-rank correlations rather that the IGE due to its stability. As we show here, this recommendation can be still valid in the TSTSLS estimation context because the rank-rank correlation estimated though TSTSLS do not suffer from variance bias.

Regarding the bias in the TSTSLS rank-rank correlation. A positive θ means that the bias from estimating parental rank increases as the child's rank

increases. In summary, in the context of TSTSLS, two relevant issues arise with the imperfect prediction of parental earnings: 1) the lack of dispersion problem — which only affects the estimation of the IGE — and 2) the prediction problem — which affects both the IGE and rank-rank correlation. The former occurs when predicted earnings have a lower variance than actual earnings. The latter occurs when the model fails to predict earnings. In the following section, we empirically assess the bias of the TSTSLS IGE and TSTSLS rank-rank estimation. Additionally, we perform an empirical test of our lower bound methodology for both the IGE and rank-rank correlation.

3.4 Evaluation, estimation and empirical lower bounds of the TSTSLS method

For our empirical analysis, we use administrative linked earnings data for father and sons in the formal labour sector for Chile. Using this information, we provide several empirical exercises that estimate the IGE and rank-rank correlation using the TSTSLS methodology. First we perform several Empirical Monte Carlo (EMC) simulations that estimate the IGE using TSTSLS to empirically assess the prediction bias (η) and the variance bias (κ). Second, we apply our methodology to generate lower bounds for the IGE. We empirically show, by using EMC simulations, that the variance bias almost disappears when we use the multiple imputation methodology and that η is positive, implying that our methodology provides empirical lower bounds for the IGE. Moreover, we perform EMC simulations to estimate the TSTSLS rank-rank correlation, showing that the TSTSLS rank-rank is a lower bound without any adjustment. Finally, our empirical results are robust to several specifications of the Mincer equation.

3.4.1 Administrative Data for Chile

We use the Chilean unemployment insurance program (UIP) to obtain information on sons and their parents' labor earnings.[67] The UIP is a benefit that involves all individuals over the age of 18 working under a fixed-term or permanent contract in the formal private sector. Participation in the scheme is mandatory for all contracts that started after September 2002, and voluntary for contracts that began before that date.

In 2003 and 2004, the coverage rate for formally contracted employees was below 50% because of the scheme's optional retroactive aspect. This coverage rate improved significantly over the following years, attaining a 65% average in 2005-2007 and 80% in 2012. The program's coverage has remained constant

[67] We focus on sons because previous works for Chile have concentrated on this group. For the U.S, Richey and Rosburg (2018) and Minicozzi (2003) study intergenerational mobility using males.

until 2019 since the remaining workers are public sector employees covered by a similar yet different system.[68]

[68] See Table C1 in Appendix for more details.

We establish parent-child linkages through administrative records provided by the Civil Registry Office (CRO). Birth certificates issued by this agency contain information on both the child and the parents at the time of birth. Therefore, these records allow us to identify and build the pairs of children and parents included in the UIP database. In our baseline analysis, the sample of children is composed of individuals that were 29-34 in 2018, while parents were 31-66 years old in 2007.

We measure parents' earnings from 2003 to 2007 and children's earnings from 2014 to 2018 by computing the five-year average of monthly private-sector earnings. In our baseline sample, we only consider parents and children that worked at least six months in the formal private sector. Additionally, we exclude observations with zero earnings as these individuals could be working either as public employees, in the informal sector, or the formal private sector but not covered by the UIP. The latter can be particularly predominant during the early years of the program. Moreover, we only include children and parents who earn more than half the minimum wage on average to reduce the potential noise from low earnings observations.[69]

[69] Half the minimum wage for children is $133,000 in 2019 Chilean pesos (measured from 2014 to 2018) and $103,000 in 2019 Chilean pesos for parents (from 2003 to 2007). Using the National Socioeconomic Characterization Survey (CASEN) in 2017, 14.1 percent of the population earn less than the minimum wage.

The baseline sample has 283,127 parent-son links. Table 3.1 shows descriptive statistics for this baseline sample. As can be seen in Table 3.1, the average log permanent income is 12.71 for parents and 13.23 for sons. The mean age for parents is 47.28, while sons are on average 32.22 years old. Moreover, 55% of parents have less than high school, 37% complete high school, 8% have some college degree, and 51% have a permanent contract.

Table 3.1: Descriptive Statistics

	Obs.	Mean	SD	p25	p50	p75
Parents, log(permanent income)	282,122	12.80	0.58	12.40	12.69	13.10
Children, log(permanent income)	282,122	13.19	0.59	12.78	13.14	13.57
Parents, Age in 2007	282,122	47.19	6.03	43.00	47.00	51.00
Children, Age in 2018	282,122	32.26	1.70	31.00	32.00	34.00
Parents, Less than High School	282,122	0.53	0.50	0.00	1.00	1.00
Parents, High School	282,122	0.38	0.49	0.00	0.00	1.00
Parents, Some College	282,122	0.09	0.28	0.00	0.00	0.00
Parents, Permanent Contract	282,122	0.44	0.39	0.00	0.39	0.85

Note: The final sample is composed of 282,122 parents-sons links. Permanent incomes are in 2018 Chilean pesos. Parents' permanent income is calculated as a 5-year average between 2003 and 2007. Sons' earnings are calculated as a 5-year average between 2013 and 2018. Parents' education is divided into three categories, and type of contract which equals the proportion of months under which the parent worked under a permanent contract.

3.4.2 Baseline TSTSLS and linked administrative data estimates

Using our unique administrative data with linked information of children's and their parents' permanent earnings, we adjust equations (3.1) and (3.2) by OLS and TSTSLS. To mimic a TSTSLS setting, we take a random subsample of 50,000 (out of 283,127 total links) parent-son links information from the baseline sample. We then randomly split this subsample into two subsubsamples, of 25,000 observations each. One of the subsubsamples plays the role of the auxiliary sample of pseudo-parents and, therefore, is used to estimate the Mincer equation (3.3) for the pseudo-parents, where in z^{pp} we include the variables age, age squared, industry, education type, and type of contract. The results are shown in Table 3.2.

Table 3.2: OLS estimates of the intergenerational earnings elasticity and rank-rank correlation for male children

	(1) IGE	(2) Rank-Rank	(3) IGE	(4) Rank-rank
Parameter	0.282	0.239	0.486	0.207
Observations	283,127	283,127	25,000	25,000
Method	OLS	OLS	TSTSLS	TSTSLS

Note: Earnings are measured as average earnings over the months where a sons-parents pair report positive earnings over the studied 5-year period. We keep individuals with average earnings greater than half of the corresponding minimum wage and with at least 6 months of positive earnings. Column (1) shows the linked administrative IGE estimate, column (2) depicts the linked administrative rank-rank correlation, column (3) shows the TSTSLS IGE estimate, and column (4) shows the TSTLS rank-rank correlation

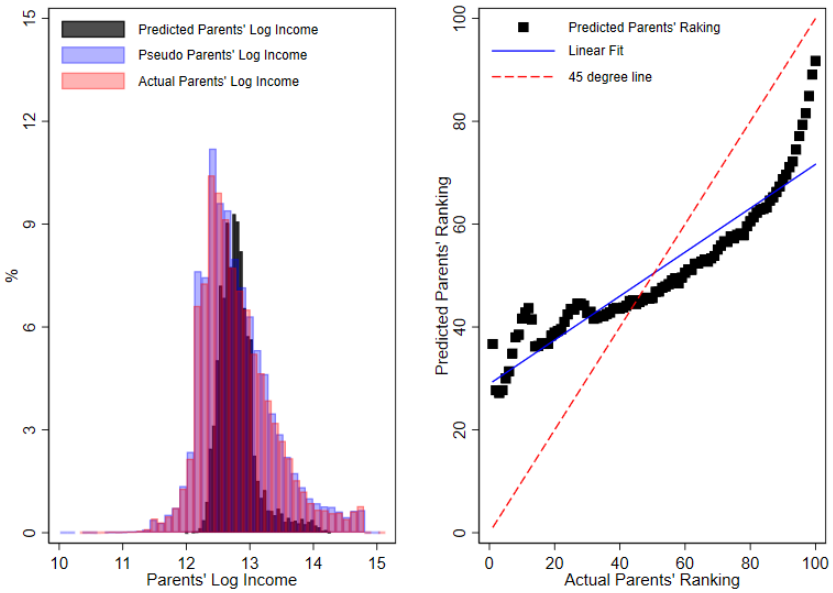
Columns (1) and (2) present the estimates of the linked administrative data-based IGE (β) and rank-rank correlation (β_{rr}), respectively via OLS. Meanwhile, columns (3) and (4) report the estimates for the same respective parameters using the TSTSLS method. The estimated linked administrative IGE is 0.282, meaning that a 1% increase in parents' earnings is associated with a 0.282% increase in their children's earnings. Importantly, our IGE estimated using administrative data is much lower than the IGE estimated through the simulated TSTSLS, which is 0.486, implying that the TSTSLS procedure overstates the IGE by 72.3%. Our results are quantitative similar to Acciari et al. (2022) that perform a similar exercise for Italy, showing that the TSTSLS procedure delivers an IGE that is overstated by 117.3%.

The linked administrative rank-rank correlation estimate is 0.239, meaning that a one percentile increase in parental earnings rank is related with an increase of the child's earnings percentile by 0.239. On the other hand, the rank-rank correlation estimated via TSTSLS is 0.207, implying a 13.4% difference between both methods. We can notice that the rank-rank correlation estimated through TSTSLS is a lower bound for the rank-rank estimated using administrative data.

To shed light on the difference between the TSTSLS and the administrative data estimates we present Figure 3.2 that shows in detail the biases of the TSTSLS estimation using our simulated exercise. The left panel shows the predicted parents' log earnings, pseudo parents' log earnings, and actual parents' log earnings. First, we can notice that the predicted (imputed) earnings distribution

presents less variance than the true log parental earnings distribution. Second, the predicted distribution is not able to accurately account for extreme values, which means that as long as the true parental earnings increases, the residual from the Mincer equation increases. This, and the fact that the correlation between log parental earnings and log sons' earnings is positive, suggests that the prediction bias (η) is positive. In addition, the right panel depicts the ranking of predicted income as a function of actual ranking. As we can see, because there is no variance bias, the prediction is less volatile.

Figure 3.2: Parents' Log Income and Ranking, TSTSLS Method



Note: Left panel shows the distribution of the predicted parents' log income, pseudo parents' log income and actual parent' log income. Right panel depicts the ranking of predicted income as a function of the actual ranking. A linear fit line and a 45 degree line are also included. Income prediction is constructed with education, age, age square, industry and type of contract as control variables.

In the following subsection, we empirically compute the variance and the prediction biases for the TSTSLS IGE using administrative data and Empirical Monte Carlo simulations.

3.4.3 Empirical Monte Carlo simulations for the TSTSLS IGE and rank rank estimates

Here we repeat the TSTSLS IGE estimation performed in Section 3.4.2, but instead of carrying out a single computation, we perform Empirical Monte Carlo exercises where we re-estimate 1,000 times each parameter to i) compute the variance and the prediction bias of the IGE and rank-rank correlation estimated through TSTSLS, and ii) evaluate the performance of the variance bias-corrected TSTSLS IGE estimate.

Empirical Monte Carlo for the IGE TSTSLS estimator

First, we empirically assess the biases that appear in the TSTSLS methodology. For this, we use our administrative data to simulate a TSTSLS context. Concretely, we adopt the following steps to evaluate the bias of the IGE and rank-rank correlation estimation via the TSTSLS:

i) We take $\hat{\beta} = 0.282$ and $\hat{\beta}_{rr} = 0.239$ from column (1) and column (2) of Table 3.2 as the estimates based on linked administrative data.

ii) We take a random subsample of 50,000 (out of 283,127 total links) parent-son links information from the baseline sample. We then randomly split this subsample into two subsubsamples, of 25,000 observations each. The first subsubsample plays the role of the auxiliary sample of pseudo-parents and the second plays the role of the main sample. Then, with the auxiliary sample of pseudo-parents, we estimate the Mincer equation for the pseudo-parents:

$$y^{pp} = \omega' z^{pp} + v, \tag{3.10}$$

where in z^{pp} we initially include the variables age, age squared, industry, education type, and type of contract. We estimate ω' by OLS. We then use the parents' information in the main sample to impute $\hat{y}^p = \hat{\omega}' z^p$. Let us denote this imputation as $\hat{y}^p(1)$.

iii) We compute $\hat{\beta}^{TSTSLS}(1)$, and $\hat{\beta}^{TSTSLS}_{rr}(1)$ by regressing y^c on $\hat{y}^p(1)$, and r^c on $\hat{r}^p(1)$ from the main sample.

iv) We repeat ii)-iii) 1,000 times.

Table 3.3 shows $\hat{\beta}$ and the results of this simulation for the IGE. As can be seen, $\hat{\beta}^{TSTSLS}(1)$ ranges from .445 to .532. On average, $\hat{\beta}^{TSTSLS}(1)$ overestimates $\hat{\beta}$ by 73%. The results in Table 3.3 reveal that both biases are present, since

$\eta(1) \neq 0$ and $\kappa(1) \neq 1$. In particular, $\eta(1) > 0$ as it is suggested by Figure 3.2. Thus, when $\kappa = 1$ we can estimate a lower bound for the administrative IGE.

Table 3.3: Results for the IGE

	(1)	(2)	(3)	(4)	(5)	(6)
Coefficient	min	p25	p50	mean	p75	max
$\hat{\beta}$	0.282	0.282	0.282	0.282	0.282	0.282
$\hat{\beta}^{TSTSLS}(1)$	0.445	0.479	0.487	0.488	0.497	0.532
$\eta(1)$	0.140	0.155	0.158	0.159	0.162	0.177
$\kappa(1)$	0.243	0.258	0.263	0.263	0.268	0.284

Note: This table shows IGE when administrative data $(\hat{\beta})$ and TSTSLS approach $(\hat{\beta}^{TSTSLS})$ are used. Prediction bias $(\eta(1))$, and variance bias $(\kappa(1))$ are included. Mincer equation contains age, age squared, occupational sector, education type, and type of contract as control variables.

Empirical Monte Carlo for the variance-corrected IGE TSTSLS estimator

The problem of lack of variance of an imputed value has been deeply discussed in the literature of missing data imputation (Little and Rubin 2019; Rubin 2004, 1996). Concretely, $\hat{y}^p(1)$ will not reflect the uncertainty behind the missing values. This implies that $\kappa < 1$ thus leading to biased estimates. Empirically, we can see this in Figure 3.2. One way to overcome this problem is through stochastic regression imputation — which is given by the procedure described in equation (3.7). To assess this procedure, we repeat the steps from the previous empirical Monte Carlo exercise for the IGE but with the addition of a stochastic error term as in equation (3.7) that inflates the variance of the imputed parental earnings. Let us denote this imputation as $\hat{y}^p(1b)$. The results of this procedure are presented in Table 3.4.

Table 3.4: Results for the IGE†.

Coefficient	(1) min	(2) p25	(3) p50	(4) mean	(5) p75	(6) max
$\hat{\beta}$	0.282	0.282	0.282	0.282	0.282	0.282
$\hat{\beta}^{TSTSLS}(1b)$	0.108	0.123	0.128	0.128	0.132	0.152
$\eta(1b)$	0.130	0.153	0.159	0.159	0.165	0.185
$\kappa(1b)$	0.948	0.991	1.002	1.002	1.013	1.059

Note: This table shows IGE when administrative data ($\hat{\beta}$) and TSTSLS approach ($\hat{\beta}^{TSTSLS}$) are used. Prediction bias ($\eta(1b)$), and variance bias ($\kappa(1b)$) are included. Mincer equation contains age, age squared, occupational sector, education type, and type of contract as control variables. Stochastic regression imputation is used to increase the variance of the imputed parents' income.

As expected, the variance bias is reduced which reflects the fact that $\kappa(1b)$ increases significantly, attaining a mean value of 1.002. Thus, this methodology eliminates the variance bias from the IGE TSTSLS estimate and that its bias only depends on the projection bias $\eta(1b)$ that in this case is positive. This means that the variance-corrected TSTSLS IGE ($\hat{\beta}^{TSTSLS}(1b)$) is a lower bound for the administrative data IGE ($\hat{\beta}$). In addition, notice that, on average, $\eta(1) = \eta(1b)$ implying that the stochastic imputation methodology only affects the variance bias while the prediction bias remains unchanged.

Empirical Monte Carlo for the rank-rank TSTSLS estimator

Additionally, we perform an empirical Monte Carlo simulation exercise for rank-rank correlations. Table 3.5 shows the results for the simulated TSTSLS setting. In particular, $\hat{\beta}_{rr}^{TSTSLS}(1)$, that is based on a Mincer equation with age, age square, education, industry, and type of contract, underestimates $\hat{\beta}_{rr}$ by 7%, on average. We highlight this result because survey data typically includes these predictors considered in the Mincer equation and, therefore, researchers can use them to estimate intergenerational mobility by calculating rank-rank correlation with a moderate bias. We can notice that the rank-rank TSTSLS estimator is a lower bound for the administrative data rank-rank estimator because $\theta(1) > 0$. Indeed, the largest $\hat{\beta}_{rr}^{TSTSLS}(1)$ obtained is lower than the administrative data estimate.

Table 3.5: Results for the rank-rank correlation

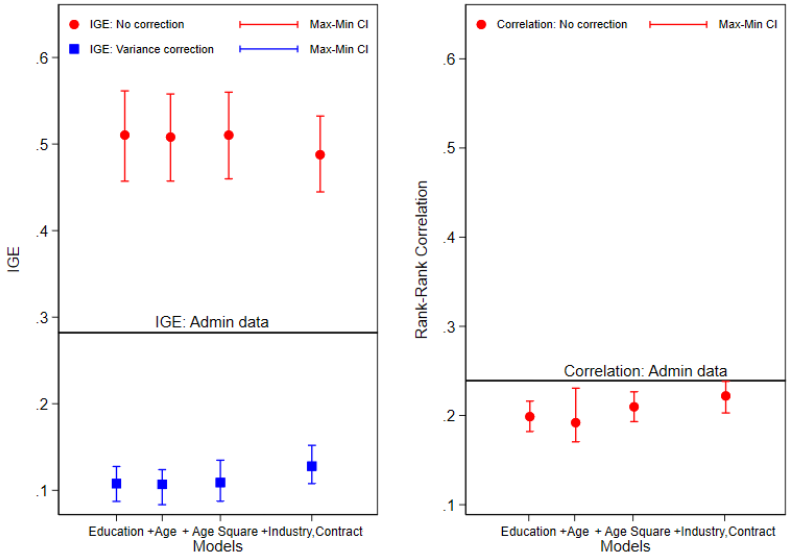
Coefficient	(1) min	(2) p25	(3) p50	(4) mean	(5) p75	(6) max
$\hat{\beta}_{rr}$	0.239	0.239	0.239	0.239	0.239	0.239
$\hat{\beta}_{rr}^{TSTSLS}(1)$	0.203	0.218	0.222	0.222	0.226	0.238
$\hat{\theta}(1)$	5.45e-05	0.00109	0.00143	0.00142	0.00175	0.00301

Note: This table shows rank-rank correlation when administrative data ($\hat{\beta}_{rr}$) and TSTSLS approach ($\hat{\beta}_{rr}^{TSTSLS}$) are used. Prediction bias (θ), is included. Mincer equation contains age, age squared, occupational sector, education type, type of contract.

Sensitivity to alternative specifications

We further test the sensitivity of our results to alternative specifications of the Mincer equation. Concretely, we first adjust this equation using only education. Then, we progressively add age, age squared, industry type and type of contract. The results for both the IGE and rank-rank correlation are depicted in Figure 3.3.

Figure 3.3: IGE and Rank-Rank Correlation: Alternative set of variables



Note: Average IGE and rank-rank correlation values are shown when alternative set of controls are included in the Mincer equation. The average is calculated taking the simple mean over the 1,000 replications. The min-max interval is also included for each alternative model. Red circles depict the average uncorrected IGE, while blue squares illustrate the average variance-corrected IGE. Black lines show the values for $\hat{\beta}$, and $\hat{\beta}_{rr}$ on Table 3.2.

As can be observed in Figure 3.3, both the variance-corrected TSTSLS IGE and rank-rank correlation establish a lower bound for our linked administrative data estimate, irrespective of the choice of predictors used in the Mincer equation of the first stage.

3.5 Conclusion and Discussion

In the absence of administrative data linking parents' and their children's earnings, measuring intergenerational mobility is a challenging task. In this paper, we show that relying on estimates from the commonly used TSTSLS estimator leads to biased measures of intergenerational mobility with respect to linked administrative data-based estimates (Acciari et al. 2022). Unfortunately, in most developing countries, such administrative data are not available. Hence, the intergenerational mobility literature studying these countries has relied on TSTSLS methods providing unreliable results.

To understand the magnitude of this problem, we estimate the IGE and rank-rank correlation using administrative data linking parents' and their children's earnings for Chile, and we compare these estimates with the results from a simulated empirical Monte Carlo TSTSLS setting. We find that the administrative data-based IGE and rank-rank correlation are 0.282 and 0.239, respectively. We then estimate these measurements through TSTSLS, showing that the TSTSLS estimate for the IGE is almost twice as large as our linked administrative data estimate, while the rank-rank estimate is slightly lower. To the best of our knowledge, this is the first paper that compares the results provided by TSTSLS and linked administrative records for a developing country.

We show that the variance bias in the TSTSLS estimation can be removed by using stochastic regression imputation to better match the spread of parental earnings. We show that this methodology delivers an empirical lower bound for the administrative data IGE estimate. In addition, the rank-rank correlation estimated through traditional TSTSLS presents a lower bound for the linked administrative rank-rank estimate. Thus, our analysis suggests that researchers should use stochastic regression imputation (or a multiple imputation approach) to generate plausible lower bounds for the TSTSLS estimates of the IGE. Also, researchers should consider the spread use of the TSTSLS rank-rank correlations to estimate intergenerational mobility when parental earnings information is not available.

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Appendix A

Employment Effect of Means-Tested Program: Evidence from a Pension Reform in Chile

A.1 CASEN 1992-2017

A.1.1 Tables and Figures

Table A1: Countries with Fully Funded Pension System

Country	Year	Country	Year
Argentina	1994	Australia	1992
Bolivia	1997	Bulgaria	2000
Chile	1981	Croatia	2001
Colombia	1993	Denmark	2002
Costa Rica	1995	Estonia	2002
Dominican Republic	2003	Hungary	1998
El Salvador	1998	Kazakhstan	1998
Mexico	1997	Kosovo	2002
Panama	2008	Latvia	2001
Peru	1993	Nigeria	2001
Uruguay	1996	Poland	1999
		Russia	2002
		Singapore	1997
		Slovakia	2005
		Sweden	1999

Note: A list of countries that have FF pension system is shown. Moreover, the corresponding year when a country moved to FF pension system is included.

Table A2: OECD countries with fully-funded pension system, details

Country	First Tier Public	Second Tier
Australia	Resource Tested	Private
Chile	Resource Tested	Private
Denmark	Resource Tested/Basic/Minimum	Private
Hungary		Private+Public
Mexico	Basic/Minimum	Private
Poland	Minimum	Private+Public

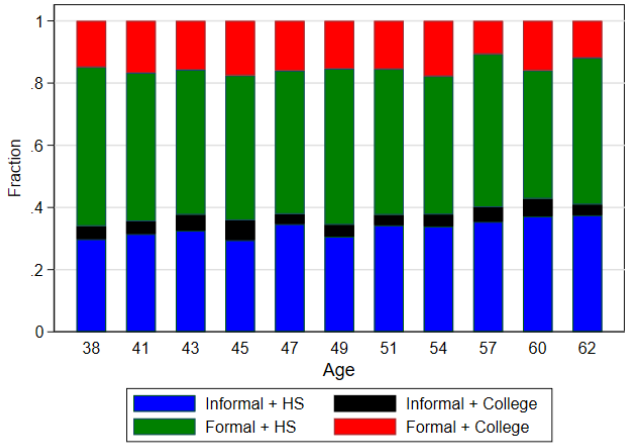
Note: A list of OECD countries that have FF pension system is shown. Moreover, the design of the government transfers (first tier) and self-finance pension (second tier) are included. Resource-tested (mean tested) is the case when the value of benefits depends on income from other sources or on both income and assets. Minimum pensions take account only of pension income and are not affected by income from other savings or assets. Basic-pension is the case when the benefit is either flat rate (the same amount) or it depends only on years of work, but not on past earnings.

Table A3: CASEN Sample, total males and males aged 60-64

	Males			Males aged 60-64		
Year	Weights	Observations	Fraction of Formal Workers	Weights	Observations	Fraction of Formal Workers
1992	6,529,225	70,845	.60	214,840	2,329	.51
1994	6,7455,88	87,704	.64	215,429	2,861	.57
1996	7,020,199	66,289	.62	198,041	1,943	.55
1998	7,196,656	93,154	.62	200,048	2,785	.53
2000	7,408,987	126,005	.62	205,935	4,214	.52
2003	7,618,753	127,427	.62	247,730	4,604	.55
2006	7,381,719	124,592	.65	256,453	4,877	.58
2009	7,998,643	121,102	.63	316,516	5,149	.57
2011	8,073,254	96,199	.69	322,082	4,019	.64
2013	8,179,592	104,255	.70	358,813	4,598	.66
2015	8,305,641	127,592	.67	377,883	6,237	.61
2017	8,469,711	103,322	.68	445,058	5,657	.64
Total	90,927,968	1,248,486	.64	3,358,828	49,273	.59

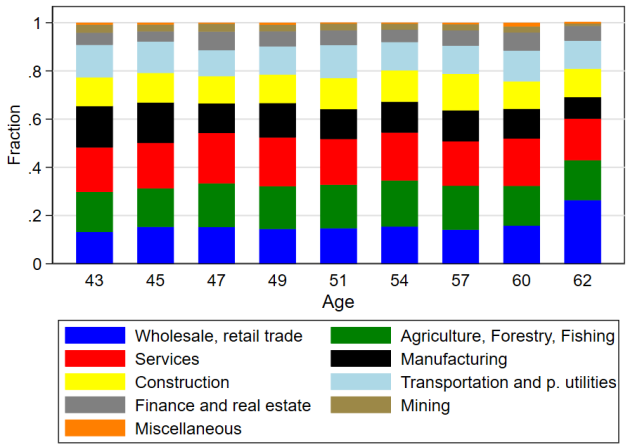
Note: Distributions of total males and males aged 60-64 are shown in the left and right column, respectively. Total weights and observations are included. The fraction of formal workers is presented for each wave by using weights.

Figure A.1: Formality and Education's distribution, males aged 60-64 in 2009



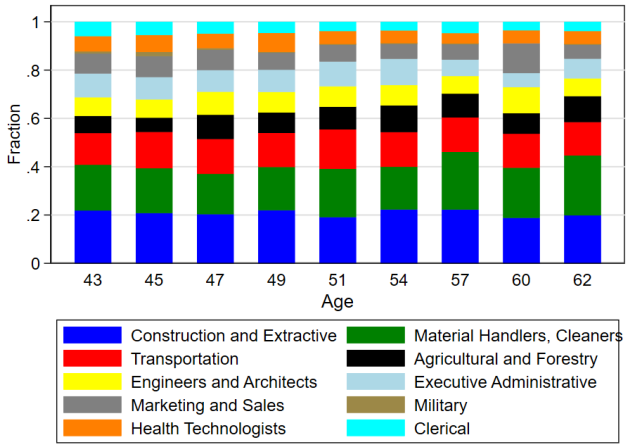
Note: The joint distribution of education (high school and college) and formality is shown for the cohort who aged 60-64 in 2009. Averages are calculated for each year using the corresponding weights.

Figure A.2: Industrial Classification, males aged 60-64 in 2009



Note: The joint distribution of industrial classification is shown for the cohort who aged 60-64 in 2009. Averages are calculated for each year using the corresponding weights.

Figure A.3: Occupation Classification, males aged 60-64 in 2009



Note: The joint distribution of occupation classification is shown for the cohort who aged 60-64 in 2009. Averages are calculated for each year using the corresponding weights.

Table A4: Formality across occupations, males (CASEN 2003)

Occupation	Males aged 20-64			Males aged 60-64		
	Formal Worker			Formal Worker		
	No	Yes	Total	No	Yes	Total
Military	18,271	1,207	19,478	314	0	314
	93.80	6.20	100.00	100.00	0.00	100.00
Executive Administrative	67,423	79,703	147,126	5,685	6,543	12,228
	45.83	54.17	100.00	46.49	53.51	100.00
Engineers and Architects	52,544	216,453	268,997	3,408	10,520	13,928
	19.53	80.47	100.00	24.47	75.53	100.00
Health Technologists	52,374	179,731	232,105	2,148	4,076	6,224
	22.56	77.44	100.00	34.51	65.49	100.00
Clerical	23,836	179,896	203,732	1,125	6,588	7,713
	11.70	88.30	100.00	14.59	85.41	100.00
Marketing and Sales	113,996	224,611	338,607	6,226	6,848	13,074
	33.67	66.33	100.00	47.62	52.38	100.00
Agricultural and Forestry	134,310	111,995	246,305	13,628	6,982	20,610
	54.53	45.47	100.00	66.12	33.88	100.00
Construction	285,939	493,292	779,231	19,382	17,907	37,289
	36.70	63.30	100.00	51.98	48.02	100.00
Transportation	131,179	384,030	515,209	8,243	16,214	24,457
	25.46	74.54	100.00	33.70	66.30	100.00
Material Handlers	248,581	498,620	747,201	15,845	29,459	45,304
	33.27	66.73	100.00	34.97	65.03	100.00
Total	1,128,453	2,369,538	3,497,991	76,004	105,137	181,141
	32.26	67.74	100.00	41.96	58.04	100.00

Note: The number and proportion of formal workers are shown by occupations. Averages are calculated using the corresponding weights.

Table A5: Formality across industries, males (CASEN 2003)

Industry	Males aged 20-64			Males aged 60-64		
	Formal Worker			Formal Worker		
	No	Yes	Total	No	Yes	Total
Agriculture, Forestry	231,211	311,708	542,919	19,991	18,186	38,177
	42.59	57.41	100.00	52.36	47.64	100.00
Mining	9,521	87,208	96,729	614	2,689	3,303
	9.84	90.16	100.00	18.59	81.41	100.00
Manufacturing	136,392	408,403	544,795	8,932	13,014	21,946
	25.04	74.96	100.00	40.70	59.30	100.00
Construction	160,234	355,211	515,445	9,881	16,188	26,069
	31.09	68.91	100.00	37.90	62.10	100.00
Wholesale and Retail	192,188	340,824	533,012	13,370	11,059	24,429
	36.06	63.94	100.00	54.73	45.27	100.00
Finance, real estate	55,942	209,456	265,398	3,461	8,905	12,366
	21.08	78.92	100.00	27.99	72.01	100.00
Services	202,239	360,353	562,592	10,430	22,092	32,522
	35.95	64.05	100.00	32.07	67.93	100.00
Transportation and p. utilities	133,741	268,656	402,397	8,756	12,230	20,986
	33.24	66.76	100.00	41.72	58.28	100.00
Miscellaneous	6,985	27,719	34,704	569	774	1,343
	20.13	79.87	100.00	42.37	57.63	100.00
Total	1,128,453	2,369,538	3,497,991	76,004	105,137	181,141
	32.26	67.74	100.00	41.96	58.04	100.00

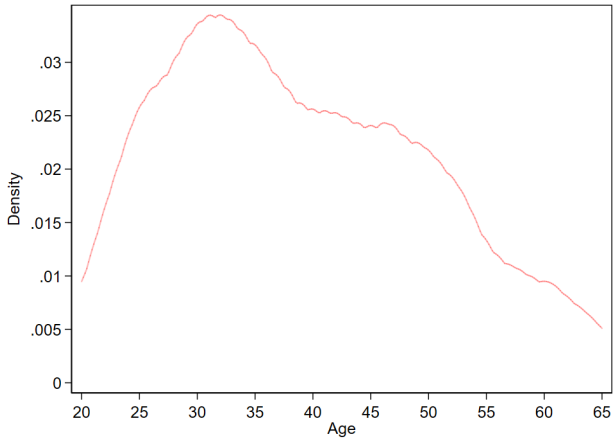
Note: The number and proportion of formal workers are shown by industries. Averages are calculated using the corresponding weights.

Table A6: Males aged 60-64, affiliated versus not affiliated with the Armed Forces (CASEN)

Year	Not Affiliated		Affiliated		Total	
	Number	%	Number	%	Number	%
1992	202,985	94.5%	11,855	5.5%	214,840	100.0%
1994	203,840	94.6%	11,589	5.4%	215,429	100.0%
1996	185,659	93.8%	12,382	6.2%	198,041	100.0%
1998	189,264	94.6%	10,784	5.4%	200,048	100.0%
2000	195,033	94.7%	10,902	5.3%	205,935	100.0%
2003	233,278	94.2%	14,452	5.8%	247,730	100.0%
2006	243,350	94.9%	13,103	5.1%	256,453	100.0%
2009	306,939	97.0%	9,577	3.0%	316,516	100.0%
2011	309,656	96.1%	12,426	3.9%	322,082	100.0%
2013	345,360	96.3%	13,453	3.7%	358,813	100.0%
2015	357,252	94.5%	20,631	5.5%	377,883	100.0%
2017	424,553	95.4%	20,505	4.6%	445,058	100.0%
Total	3,197,169	95.2%	161,659	4.8%	3,358,828	100.0%

Note: Column one shows the distributions of males aged 60-64 who are not affiliated to the Armed Forces' pension system. Column two depicts the distributions of males aged 60-64 who are affiliated to the Armed Forces' pension system. Column three shows the total distribution of males aged 60-64. The proportion is calculated using the corresponding weights.

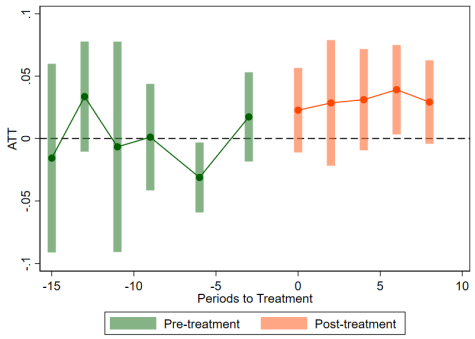
Figure A.4: Distribution of Males who are working in the Armed Forces (CASEN 2003)



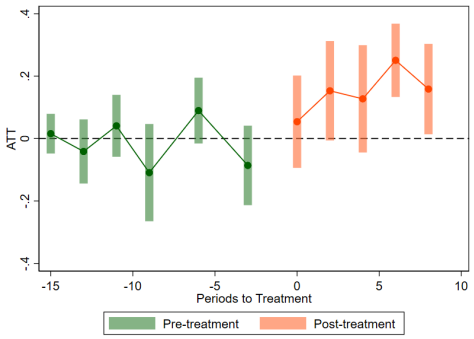
Note: The distribution of males who currently work in the Armed Forces is shown by using CASEN 2003 and the corresponding weights.

Figure A.5: Monthly Hours worked per worker and fraction of formal workers, males aged 60-64 (CASEN)

(a) Monthly Hours worked per worker



(b) Fraction of Formal Workers



Note: Panel a) shows the group time ATT for monthly hours worked per worker whereas Panel b) depicts the group time ATT for fraction of formal workers (unconditional of being employed) using Callaway and Sant'Anna (2021). 95% confidence intervals are also included. Treatment group is males aged 60-64 who are not affiliated with the Armed Forces. Control group is males 60-64 who are affiliated with the Armed Forces. Each regression controls for demographic variables such as education, housing assets, and region fixed effects. Standard errors are estimated using the doubly robust DID estimator based on stabilized inverse probability weighting. Results are obtained using population weights.

Table A7: Several outcomes, ATT, males aged 60-64 not affiliated with the Armed Forces (CASEN)

	Formality (workers)	Hours Worked (formal workers)	Hours Worked (informal workers)	Duration (workers)
ATT	.103	.067***	.022	.570
	(0.099)	(0.019)	(0.026)	(3.006)
Pretrend Test	10.455	80.424***	23.842***	11.807***
p-value	[0.107]	[0.000]	[0.000]	[0.003]
Observations	34,432	18,692	15,740	26,531
Weights	2,384,454	1,412,009	972,445	1,848,318
Mean: Dep Variable	.603	196	182	16
SD: Dep Variable	.489	52	78	14

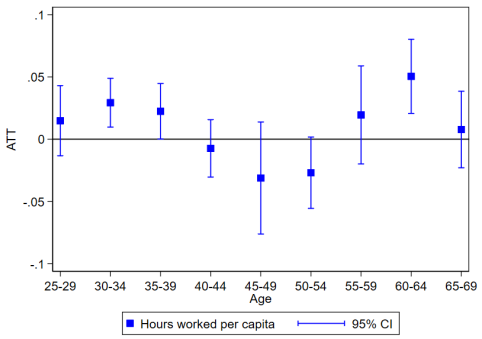
Robust standard errors in parentheses. p<0.01, ** p<0.05, * p<0.1. P-values in square brackets.

Note: This table shows the ATT and the pre-trend test for alternative outcome variables using Callaway and Sant'Anna (2021). Formality conditional of being working, monthly hours worked conditional of being a formal and an informal worker. Duration is the number of years working in the same job conditional of being working. This variable is present only in eight waves (1996, 2003, 2006, 2009, 2011, 2013, 2015, 2017). Treatment group is males aged 60-64 who are not affiliated with the Armed Forces. Control group is males aged 60-64 who are affiliated with the Armed Forces. Each regression controls by demographic variables such as education, housing assets, and dummies per region. Standard errors are estimated using the doubly robust DID estimator based on stabilized inverse probability weighting. Results are obtained using population weights.

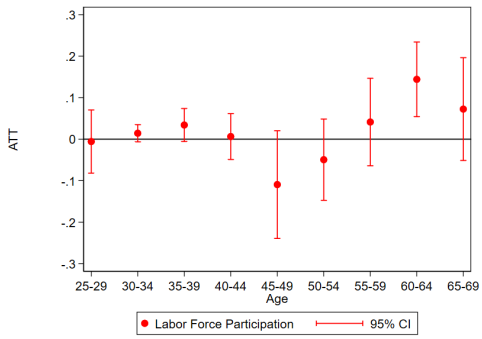
A.1.2 Different Age Groups

Figure A.6: ATT, males in different age groups (CASEN)

(a) Monthly Hours worked per capita



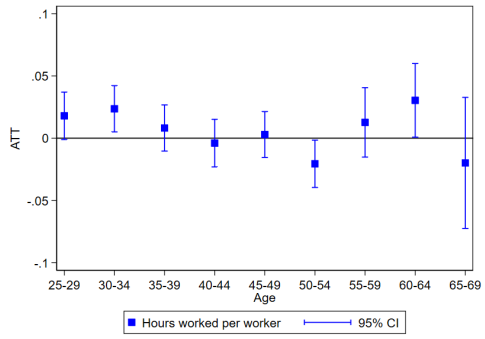
(b) Labor Force Participation



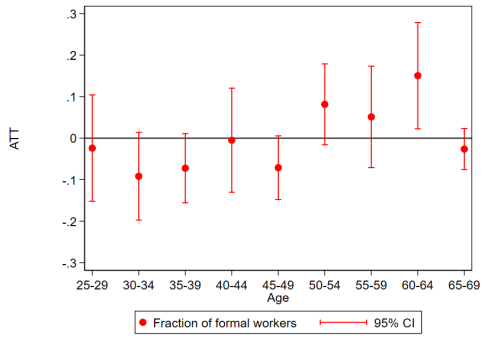
Note: Panel a) shows the ATT for monthly hours worked per capita whereas Panel b) depicts the ATT for labor force participation using Callaway and Sant'Anna (2021). 95% confidence intervals are also included. For each age group, a different DID procedure is run. For all these groups, the treatment group is males who are not affiliated with Armed Forces while the control group is males affiliated with Armed Forces. Each regression controls by demographic variables such as education, housing assets, and dummies per region. Standard errors are estimated using the doubly robust DID estimator based on stabilized inverse probability weighting. Results are obtained using weights.

Figure A.7: ATT, males in different age groups (CASEN)

(a) Monthly Hours Worked per worker



(b) Fraction of Formal Workers



Note: Panel a) shows the ATT for hours worked per worker whereas Panel b) depicts the ATT for the fraction of formal workers using Callaway and Sant'Anna (2021). 95% confidence intervals are also included. For each age group, a different DID procedure is run. For all these groups, the treatment group is males who are not affiliated with Armed Forces while the control group is males affiliated with Armed Forces. Each regression controls by demographic variables such as education, housing assets, and dummies per region. Standard errors are estimated using the doubly robust DID estimator based on stabilized inverse probability weighting. Results are obtained using weights.

A.2 SPS-HPA-SCOMP

This section details how the administrative data is constructed by merging SPS, HPA, and SCOMP. Moreover, it shows tables and figures mentioned in the main text.

A.2.1 Data Construction

The administrative data comes from a unique merge among i) an extensive, nationally representative longitudinal household survey, the Social Protection Survey (SPS), with ii) the Affiliate Pension History (APH) – a complete administrative record for workers in the formal sector after the FF system was created, and iii) the System of Consultations and Offers of Pension Amounts (SCOMP)– an administrative record for people who are eligible for annuities. I refer to this merged data as SPS-APH-SCOMP.

Figure A.8 shows the details. First, the SPS has six rounds: 2002, 2004, 2006, 2009, 2015, and 2019. In each wave, household heads self-report information about age, education, formal sector employment, wages, and non-pension and pension saving. Additionally, demographic information is asked for every member of the household. Each household head has self-reports previous labor decisions (labor force participation, formal sector, industry, occupation) in the labor decisions section since the last wave. However, in waves 2002 and 2004, people give employment details since 1981. The date of birth and death are available for every household head. I, then, construct a monthly panel between 1981 and 2019 for every household head who appears at least in one wave.

The APH contains information about monthly employment, number of contributions to the pension system, formal wages, pension savings, and old-age income for every household head in the SPS since joining the formal sector. Hence, for a household head who started his formal working life in 1981, I have data on wages, contributions, and pension savings for almost forty years.[70]

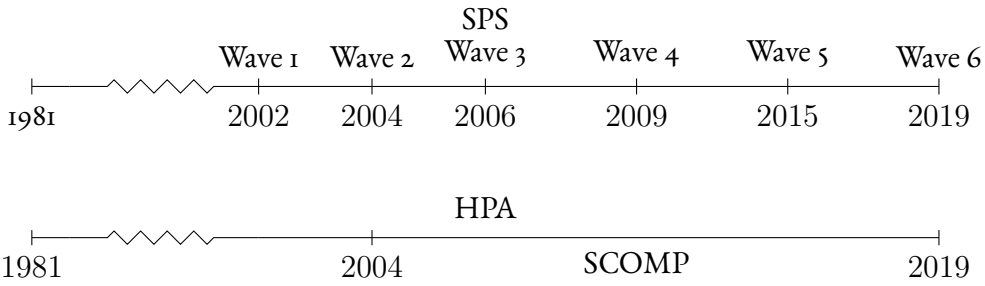
[70] The pension wealth balance has been available since January 2008. I construct pension assets before that date by using the information of monthly contributions (in the data) and the return of the pension system. The process is not super challenging because before 2004; there was a unique portfolio for each Pension Fund Administrator.

The SCOMP has the types of annuity contracts people had wanted to receive offers for since 2004 (when the system was created). Workers are eligible for annuities when their pension savings exceed a certain threshold. They usually participte in the SCOMP when they are close to the retirement age to know the values and conditions that they would face when retire. I observe all household heads who satisfy this constraint even though they finally choose a programmed withdrawal. Some included characteristics are the choice of a guarantee period to provide payouts after death or a deferral of payments, among others.

Then, the final sample has people who have been interviewed in the SPS. I can see additional labor information for them if they have contributed at least

one month in the formal sector using the APH. After 2004, I can also complement that information with the data included in the SCOMP.

Figure A.8: Data Construction



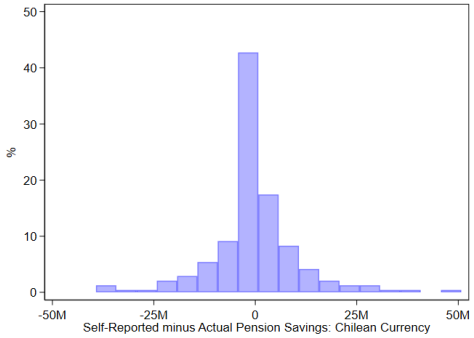
Note: I construct the sample by merging i) SPS, ii) HPA, and iii) SCOMP. SPS has six rounds in 2002, 2004, 2006, 2009, 2015 and 2019. The APH has data for formal workers between 1981 and 2019. The SCOMP has information about what kind of annuities people want to receive offers for since 2004.

A.2.2 Perceived Groups

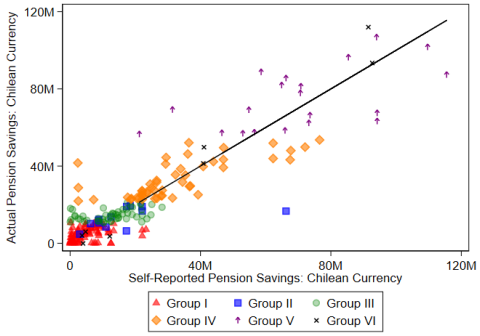
Actual groups I-VI were constructed using administrative records on pension assets and monthly contributions included in the HPA. In the survey, there is information on i) "self-reported" pension assets and ii) "self-reported" months in the formal sector. Concerning pension wealth, the question is direct; "How much have you accumulated in your pension account." This information is available for each SPS wave (since 2004) and in the best case scenario, I observe self-reported pension wealth five times for each individual. Figure A.9 shows a comparison between the self-reported and the true pension wealth across groups I-VI.

Figure A.9: Pension system knowledge, Males aged 60-64

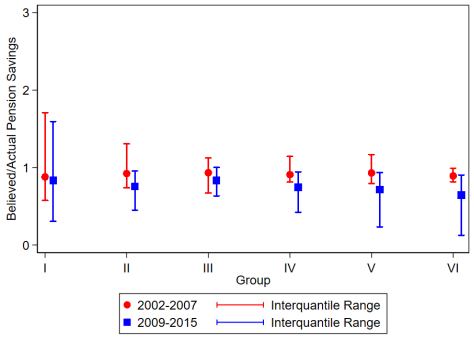
(a) Self-reported minus Actual Pension Wealth



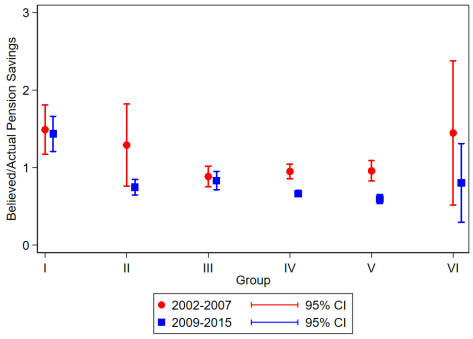
(b) Self-reported and Actual Pension Savings by groups



(c) Self-reported over Actual Pension Savings Ratio, Median



(d) Self-reported over Actual Pension Savings Ratio, Mean



Note: Panel a) shows the underestimation/overestimation between the self-reported and the actual pension savings. Panel b) details the self reported and actual pension wealth for Groups I-VI. Panel c) and d) depict the median and mean ratio between the self-reported and the actual pension wealth.

People also self-reported if they worked in the formal sector each time they were employed. Thus, I can construct the total self-reported months in the formal sector at each time. One crucial issue is to deal with the case where people were not interviewed.[71] There are several alternatives to predict those missing values. I try six scenarios of what the individual would have responded: i) the formality is 0% in missing months, ii) the formality is 25% in missing months, iii) the formality is 50% in missing months, iv) the formality is 100% in missing months, v) the administrative records for that specific month is what the person would have said if interviewed, and vi) I use the observed probability of someone saying he contributes to the pension system given he is a formal worker. Table A8 shows an example of how the six scenarios impute the missing information. The "perceived months" in the formal sector is the self-reported formality after imputing on the missing months.

[71] Missing months account for only around 30% of individual-month observations. Around 40% of males have less than 10% of individual-month observations imputed.

Table A8: Six Scenarios to construct believed formal contributions

Month	Data		Scenarios					
	Admin	Survey	1 (0%)	2 (25%)	3 (50%)	4 (100%)	5 (Real)	6 (Prob)
1	Formal	Formal	1	1	1	1	1	1
2	Formal	No Formal	0	0	0	0	0	0
3	No Formal	Formal	1	1	1	1	1	1
4	No Formal	No Formal	0	0	0	0	0	0
5	No Formal	Missing	0	0	0	1	0	$1/2 \to 0$
6	No Formal	Missing	0	0	0	1	0	$1/2 \to 1$
7	Formal	Missing	0	0	1	1	1	$1/2 \to 0$
8	Formal	Missing	0	1	1	1	1	$1/2 \to 1$
MSE	Ranking		1°	2°	3°	6°	4°	5°
MAE	Ranking		1°	2°	3°	6°	4°	5°

Note: The table shows how all the six scenarios to impute formality when I cannot observe that in the survey. Scenarios 1-4 assume that the probability of being formal is 0%, 25%, 50%, and 100%, respectively. Scenario 5 assumes that people would have answered what I observed in the administrative data. Scenario 6 imputes according to the probability the individual has said he has contributed when he is a formal worker. In this example, he contributes to the pension system in the first two months but declares himself to be a formal worker in month one. The probability, then, is 0.5 for each month with missing data. MSE and MAE stand for minimum square error and minimum absolute error.

Once I have the "perceived months" in the formal sector, I compute the "perceived contribution" and, therefore, the "perceived pension assets" for each of the six scenarios. I construct the former by multiplying the "self-reported" wage and the contribution rate which is 10%, for people who "perceived" to be a formal worker. The latter is the "accumulated perceived contribution" plus the actual return in the pension system.

I then find the scenario that minimizes the squared mean error and the absolute mean error between "perceived pension assets" and "self-reported pension assets." By doing so, I choose the scenario where, on average, the "perceived pension assets" are the closest one to the value said in the interview. Notice that the "self-reported pension assets" is the value responded directly after the question about the total balance in their pension accounts which is observed a couple of time for each person. In contrast, the "perceived pension assets" are constructed using the self-reported information in the survey which is available for almost every month since 1981.

Scenario 1, where I assume that the formality is 0% in missing months, has the lowest squared mean error and absolute mean error. In the second place is Scenario 2, where I impute 25% as formal months when having missing data. The reason that both models have the best performance is shown in Table A9, where on 50.93% of the cases, the self-reported pension assets are lower than the true pension wealth, and the latter smaller than the perceived pension wealth under Scenario 1. It implies that although I assume no additional months in the formal sector when facing missing information, the self-reported months overestimate the actual months and therefore generate a pension accumulation above both the self-reported and the true ones.

Table A9: Different Cases over self-reported pension wealth

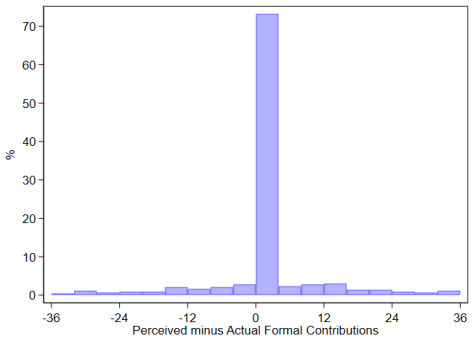
Cases							Obs.	%
Self reported	<	True	<	Scenario 1	<	Scenario 2	12,031	50.93
Self reported	<	Scenario 1	<	Scenario 2	<	True	2,766	11.71
True	<	Self reported	<	Scenario 1	<	Scenario 2	3,549	15.02
True	<	Scenario 1	<	Scenario 2	<	Self reported	2,081	8.81
Scenario 1	<	Scenario 2	<	Self reported	<	True	1,672	7.08
Scenario 1	<	Scenario 2	<	True	<	Self reported	1,319	5.58
Rest							203	0.8
Total							26,306	100.00

Note: The table compares the relative ranking of i) self-reported pension assets, ii) true pension assets, iii) pension assets under scenario 1, and iv) pension assets under Scenario 2.

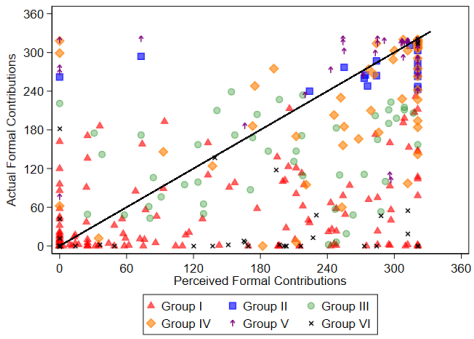
Figure A.10 shows the results for Scenario 1, on average, Groups I, III, and VI are the ones who overestimate the months in the formal sector. The median agent does not overestimate at all.

Figure A.10: Perceived Formal Contribution, Males aged 60-64 in 2008

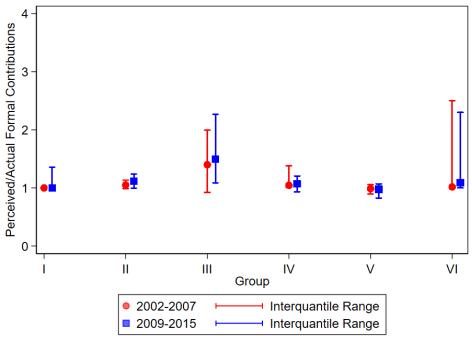
(a) Perceived minus Actual Formal Contribution



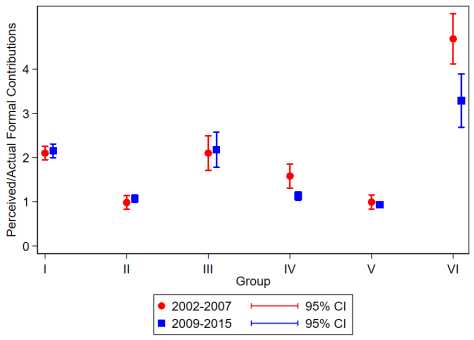
(b) Perceived and Actual Formal Contribution by groups



(c) Perceived over Actual Formal Contribution, Median



(d) Perceived over Actual Formal Contribution Ratio, Mean



Note: Panel a) shows the underestimation/overestimation between the perceived and the actual formal contribution. Panel b) details the perceived and actual formal contribution for Groups I-VI. Panel c) and d) depict the median and mean ratio between the perceived and the actual formal contribution.

In Table A10 from the Appendix, I compare the changes of months in the formal sector after 5 years between two cohorts i) males aged 60-64 in 2008, and ii) males aged 60-64 in 2002 to see if the reform modifies to attachment to the labor market. The former group is the one who faces the reform and may change its behaviour since the latter plays the role of the control group. I find four important results. Firstly, among males who predict correctly they are in Group I, workers aged 60-64 in 2008 increase their formal participation by 34.8% more than males aged 60-64 in 2002 (from .64 to .87 additional months in five years). Secondly, among people in Group I who fail to guess their group tend to increase in a smaller amount their formal contributions than males aged 60-64 in 2002. This is especially true for people who belonging to Group I, think they are in Group III and IV. Thirdly, among people in Group II who

predict they are in Group IV, males aged 60-64 in 2009 increase their months in the formal sector less than the previous cohort (from 26.20 to 19.69 additional months in five years), implying a drop of 24.8% in the formality. Fourth, among who predict correctly they are in Group IV, males aged 60-64 in 2009 increase their formal participation by 66.2% more than males aged 60-64 in 2002 (from 14.01 to 23.29 additional months in five years).

Table A10: Growth in the change of months in the formal sector after 5 years, Males 60-64 in 2009 vs Males 60-64 in 2002 (Scenario 1, 0%)

Actual	Perceived Group						
Group	I	II	III	IV	V	VI	Total
Group I	34.8%***	-40.7%***	93.0%***	-37.8%***	-52.3%***		
(proportion)	74.13	6.95	7.69	8.38	2.85		63.13
Group II	-47.7%***	11.3%*	394.5%***	-24.8%***			
(proportion)	7.42	19.10	3.57	51.80	18.10		3.43
Group III	6.1%	-38.9%***	-12.9%*	-24.5%***	-36.6%***		
(proportion)	10.86	6.19	22.29	52.37	8.29		6.90
Group IV	106.9%***	-39.8%***	68.2%***	66.2%***	-6.2%		
(proportion)	4.33	0.16	5.34	62.19	27.97		12.75
Group V	-39.4%***			111.6%***	47.8%***		
(proportion)	0.78	0.00	1.16	6.01	92.05		6.55
Group VI						-8.1%***	
(proportion)						100.00	7.24

Note: *** p<0.01, ** p<0.05, * p<0.1. The table shows the growth (or fall) of months in the formal sector after 5 years between i) males aged 60-64 in 2009, and ii) males aged 60-64 in 2002.

Finally, I show Tables A11 and A12 that calculate the distribution of people between the actual and the perceived group but assuming Scenario 2 and 5, respectively. As we can see there are small difference with respect to the baseline number shown in Table 1.3. People who miss-perceived their group tend to behave differently.

Table A11: Additional months in the formal sector and labor force after 5 years, males 60-64 in 2008 (Scenario 2, 25%)

Actual Group		Perceived Group				
	Outcome	I	II	III	IV	V
Group I	Formal Sector	2.1	30.8***	25.2***	26.0***	12.0***
	Labor Force	15.2	45.5***	45.7***	47.0***	50.0***
	Proportion	83.0	4.8	6.3	5.1	0.8
Group II	Formal Sector	58.0**	43.0	50.00**	51.0**	
	Labor Force	60.0**	54.2	54.0	59.8**	
	Proportion	5.9	52.9	5.9	35.3	
Group III	Formal Sector	32.5	54.3***	33.2	30.2*	37.3**
	Labor Force	45.4*	60.0***	49.0	53.2*	54.0*
	Proportion	16.1	9.7	29.0	40.3	4.8
Group IV	Formal Sector	60.0***		26.0***	45.4	40.0**
	Labor Force	60.0**		50.7**	53.6	47.6**
	Proportion	2.7		4.0	77.3	16.0
Group V	Formal Sector				46.8***	58.0
	Labor Force				49.5***	59.2
	Proportion				8.33	91.7

Note: *** p<0.01, ** p<0.05, * p<0.1. The table shows the additional months i) in the formal sector and ii) labor force after five years across actual and perceived groups. Stars represent if average choices of misinformed workers differ from average choices for correctly informed workers. In the third row, the table shows the distribution (proportion) of people between the actual and perceived group.

Table A12: Additional months in the formal sector and labor force after 5 years, males 60-64 in 2008 (Scenario 5, Real)

Actual Group		Perceived Group				
	Outcome	I	II	III	IV	V
Group I	Formal Sector	2.0	30.8***	25.7***	26.1***	10.3***
	Labor Force	15.1	45.9***	45.4***	48.4***	42.4***
	Proportion	82.9	4.9	6.1	5.1	0.9
Group II	Formal Sector	58.0**	43.0	51.0**		
	Labor Force	60.0**	54.2	54.0**	59.8**	
	Proportion	5.9	52.9	5.9	35.3	
Group III	Formal Sector	32.5	54.3**	33.2	31.5**	28.0
	Labor Force	45.4*	60.0**	49.0	52.9*	55.5
	Proportion	16.1	9.7	29.0	38.7	6.5
Group IV	Formal Sector	60.0*		26.0*	46.2	36.6**
	Labor Force	60.0*		50.7	54.5*	44.0**
	Proportion	2.7		4.0	76.0	17.3
Group V	Formal Sector				46.8	58.0
	Labor Force				49.5	59.2
	Proportion				8.33	91.7

Note: *** p<0.01, ** p<0.05, * p<0.1. The table shows the additional months i) in the formal sector and ii) labor force after five years across actual and perceived groups. Stars represent if average choices of misinformed workers differ from average choices for correctly informed workers. In the third row, the table shows the distribution (proportion) of people between the actual and perceived group.

A.2.3 Tables and Figures

Table A13: Pension Systems, males (SPS-APH-SCOMP)

	Freq	Percent
New System	11,265	54.0
Old System	2,212	10.6
Armed Forces	221	1.1
Old-New System	3,192	15.3
Armed Forces and New System	331	1.6
Armed Forces and Old System	60	.3
Armed Forces and Old-New System	85	.4
Informal Workers	3,501	16.8
Total	20,867	100.0

Note: The distribution of pension system is shown. New System stands for the fully funded pension system. Old System is the PAYG pension system for civilians. Armed Forces is the PAYG pension system for military workers. Informal workers represents all workers who have never contributed to any of the three pension systems.

Table A14: Final Sample, males (SPS-APH-SCOMP)

	Initial Sample (%)	Dis. + Early Retirement (%)	Final Sample (%)
New System	54.0	31.2	63.1
Old System	10.6	0	0
Armed Forces	1.1	0	1.3
Old-New System	15.3	65.9	12.6
Armed Forces and New System	1.6	1.2	1.8
Armed Forces and Old System	.3	0	.4
Armed Forces and Old-New System	.4	1.6	.3
Informal Workers	16.8	0	20.5
Total	20,867	1,577	17,078

Note: The distribution of pension system is shown. New System stands for the fully funded pension system. Old System is the PAYG pension system for civilians. Armed Forces is the PAYG pension system for military workers. Informal workers represents all workers who have never contributed to any of the three pension systems. To construct the final sample, I remove people from the Old System and workers who claim for disability or early retirement benefits. People who are affiliated with the Armed Forces are the sum of third, fifth, sixth, and seventh rows which represents 3.8%.

Table A15: Interesting Groups, males aged 60-64 who are affiliated with the Armed Forces (SPS-APH-SCOMP)

Year	Group I Non-contributory	Group II Contributory	Group III Year-threshold	Group IV Income-threshold	Group V Wealthy
2008	73.2	2.6	6.9	10.2	7.1
2010	68.7	4.6	6.3	12.2	8.2
2012	64.1	5.7	5.7	15.2	9.3
2014	58.5	6.2	5.8	18.3	11.2
2016	53.9	7.4	5.7	20.8	12.2
2018	50.9	7.8	5.3	24.1	11.9
Total	60.0	6.2	5.9	17.7	10.3

Note: The distribution of males males aged 60-64 who are affiliated with the Armed Forces into different groups is presented. The categorization of each individual depends on the level of expected pension income and the number of contributions.

Table A16: Overall ATT by perceived group, no matter the actual group, males aged 60-64

Outcome	Group I Non-Contributory	Group II Contributory	Group III Year-Threshold	Group IV Pension-Threshold
Hours worked	0.030	-0.123	-0.024	-0.031**
(everyone)	(0.017)	(0.066)	(0.038)	(0.011)
LFP	0.040**	-0.041**	0.102	0.046
(everyone)	(0.016)	(0.020)	(0.108)	(0.031)
Hours worked	-0.004	-0.026	0.010	0.0103
(workers)	(0.020)	(0.022)	(0.020)	(0.020)
Formality	0.027	-0.130	-0.026***	-0.043*
(everyone)	(0.011)	(0.096)	(0.010)	(0.019)

Note: Robust standard errors in parentheses. p<0.01, ** p<0.05, * p<0.1. Overall ATT for alternative outcome variables using Callaway and Sant'Anna (2021). Treatment group is males aged 60-64 who i) are not affiliated with the Armed Forces, and ii) perceive what they belong to one of the four groups. Control group is males aged 60-64 who are affiliated with the Armed Forces. Each regression controls for demographic variables such as education, previous occupation and industry, and dummies for region. Standard errors are estimated using the doubly robust DID estimator based on stabilized inverse probability weighting.

Table A17: Proportion of Beneficiaries for Groups, males 60-64 in 2008 (Scenario 1,0%)

Actual Group		Perceived Group				
	Outcome	I	II	III	IV	V
Group I	PBS	0.45	.11***	0.13***	0.16***	0.17***
(Non-contributory)	APS	0.08	.33***	0.36***	0.18***	0.17***
	PBS+APS	0.50	.39***	0.40***	0.32***	0.33***
	Proportion	83.0	4.8	6.3	5.1	0.8
Group II	PBS	0.00***	0.22	0.00***	0.00***	
(Contributory)	APS	1.00***	0.55	0.00***	0.50**	
	PBS+APS	1.00***	0.66	0.00***	0.50***	
	Proportion	5.9	52.9	5.9	35.3	
Group III	PBS	0.00	0.00	0.00	0.04**	0.00
(Year-threshold)	APS	0.40***	0.71***	0.61	0.58*	0.33***
	PBS+APS	0.40***	0.71***	0.61	0.63*	0.33***
	Proportion	16.1	9.7	29.0	40.3	4.8
Group IV	PBS	0.00***		0.00***	0.05	0.00***
(Income-threshold)	APS	1.00***		0.67***	0.46	0.08***
	PBS+APS	1.00***		0.67***	0.48	0.08***
	Proportion	2.7		4.0	77.3	16.0
Group V	PBS				0.00	0.00
(Wealthy)	APS				0.05*	0.00
	PBS+APS				0.05*	0.00
	Proportion				8.3	91.7

Note: *** p<0.01, ** p<0.05, * p<0.1. The table shows the proportion of actual beneficiaries across actual and perceived groups. I follow males aged 60-64 five years later to determine if they apply and receive benefits. Stars represent if average choices of misinformed workers differ from average choices for correctly informed workers. In the fourth row, the table shows the distribution (proportion) of people between the actual and perceived group.

A.2.4 Heterogeneous Effects by Subgroups

While my defined groups tend to put together people with similar pension savings and contributions, there might be more than one representative agent inside each group. This is especially true for people near the threshold between groups. To analyze heterogeneous behavior, I run the DID model for alternative subgroups. Table A18 shows the ATT for four subgroups of Group I. Subgroup A puts together all workers who have zero formal contributions. They are pure informal workers and represent around 36% of Group I. Subgroup B has individuals with formal contributions between 1 and 239 months.

As can be seen, positive impacts are found for subgroup B in terms of hours worked and labor force participation. People who have never worked in the formal sector do not increase their labor supply and formality. Additionally, I split subgroup B even further: B1 and B2 for workers who have from 1 to 179 and 180 to 239 monthly contributions, respectively. Subgroup B1 puts together workers who require more than five years of working in the formal sector to satisfy the contribution requirement for the MPG benefit.

Subgroup B1 represents 82% of people in subgroup B (53% of people in Group I). The results indicate that the positive impacts of subgroup B are mainly driven by workers who have between 1 and 179 monthly contributions. The intuition comes from the fact that they will receive government transfers after the reform and keep around 70% of each additional dollar contributed in their pension accounts. For people in subgroup B2, the necessity of contributing to be eligible for MPG no longer exists; they will receive APS even if they do not work formally any additional month.

Table A18: ATT by subgroups, males aged 60-64 (SPS-APH-SCOMP)

	Group I Non-contributory			
Subgroup	A	B	B1	B2
N° of Contributions	0	1-239	1-179	180-239
Hours worked	-0.005	0.0478**	0.0348	0.008
(everyone)	(0.0215)	(0.0237)	(0.0245)	(0.032)
LFP	-0.006	0.172**	0.129*	0.010
(everyone)	(0.071)	(0.078)	(0.080)	(0.113)
Hours worked	-0.014	0.007	0.006	0.012
(workers)	(0.014)	(0.014)	(0.015)	(0.014)
Formality	-0.0632	0.071	0.0926	0.043
(everyone)	(0.069)	(0.091)	(0.069)	(0.116)

Note: Robust standard errors in parentheses. p<0.01, ** p<0.05, * p<0.1. Overall ATT is shown for alternative outcome variables using Callaway and Sant'Anna (2021). Treatment group is males aged 60-64 who i) are not affiliated with the Armed Forces and ii) belong to each subgroup. Subgroup A has people with zero formal contributions into the pension system. People in Subgroup B have between 1 and 239 monthly formal contributions in the pension system. Subgroup B1 and B2 divide Subgroup B using 180 monthly contributions as a threshold. Control group is males aged 60-64 affiliated with the Armed Forces. Each regression controls for demographic variables such as education, previous occupation and industry, and dummies for region. Standard errors are estimated using the doubly robust DID estimator based on stabilized inverse probability weighting.

Tables A19, A20, and A21 show the results for alternative subgroups inside Groups II, III, and IV, respectively. I show that the negative impacts on labor supply for Group II are driven by people whose expected pension income are far from the MPG line. Those workers satisfy the contribution requirement, but their expected pension income is low. They face an increase in benefits (income effect) and an increase in accrual rate (substitution effect) and the income effect seems to overcompensate the substitution effect. Workers close to the thresholds drive the results for Groups III and IV. For instance, people in Group III who have between 180 and 239 months in the formal sector increase the hours worked. At the same time, individuals in Group IV who have pension assets just above the threshold augment their hours worked and labor force participation. Both subgroups have income and substitution effects moving in the same direction, and expected labor market effects are negative.

Table A19: ATT by subgroups, Group II, males aged 60-64 (SPS-APH-SCOMP)

	Group II Contributory			
Subgroup	A	B	C	D
N° of Contributions	240-252	252-		
Pension Income			0-x^*	$x^* - x_2$
Hours worked	-0.032	-0.001	-0.062*	-0.022
(everyone)	(0.0296)	(0.021)	(0.021)	(0.049)
LFP	-0.0703	-0.003	-0.0303	-0.013
(everyone)	(0.1031)	(0.069)	(0.129)	(0.0855)
Hours worked	-0.009*	0.002	-0.032	-0.001
(workers)	(0.005)	(0.0136)	(0.069)	(0.091)
Formality	-.031	-0.115	-.026	-0.075
(everyone)	(0.129)	(0.0855)	(0.059)	(0.106)

Note: Robust standard errors in parentheses. p<0.01, ** p<0.05, * p<0.1. This table shows the ATT for alternative outcome variables using Callaway and Sant'Anna (2021). Treatment group is males aged 60-64 who i) are not affiliated with the Armed Forces and ii) belong to each subgroup. Subgroup A has people between 240-252 formal contributions into the pension system. People in Subgroup B have more than 252 monthly formal contributions in the pension system. Subgroup C and D divide Group II by using x^* as threshold. It is the middle point between x_1 and x_2 in Figure 1.1. Control group is males aged 60-64 affiliated with the Armed Forces. Each regression controls for demographic variables such as education, previous occupation and industry, and dummies for region. Standard errors are estimated using the doubly robust DID estimator based on stabilized inverse probability weighting.

Table A20: ATT by subgroups, Group III, males aged 60-64 not affiliated with the Armed Forces (SPS-APH-SCOMP)

	Group III			
	Year-threshold			
Subgroup	A	B	C	D
N° of Contributions	0-179	180-239		
Pension Income			$x \in (0, x^*)$	$x \in (x^*, x_2)$
Hours worked	-0.024	0.063**	-0.013	0.034
(everyone)	(0.026)	(0.021)	(0.064)	(0.035)
LFP	0.0703	0.028	-0.056	0.103*
(everyone)	(0.096)	(0.067)	(0.106)	(0.049)
Hours worked	0.004	0.058	-0.001	-0.013
(workers)	(0.004)	(0.023)	(0.091)	(0.046)
Formality	-.029	.032	-0.075	-0.029
(everyone)	(0.129)	(0.0855)	(0.063)	(0.085)

Robust standard errors in parentheses. p<0.01, ** p<0.05, * p<0.1.

Note: This table shows the ATT for alternative outcome variables using Callaway and Sant'Anna (2021). Treatment group is males aged 60-64 who i) are not affiliated with the Armed Forces and ii) belong to each subgroup. Subgroup A has people between 0-179 formal contributions into the pension system. People in Subgroup B have between 180 and 239 monthly formal contributions in the pension system. Subgroup C and D divide Group II by using x^* as threshold. It is the middle point between x_1 and x_2 in Figure 1.1. Control group is males aged 60-64 affiliated with the Armed Forces. Each regression controls by demographic variables such as education, previous occupation and industry and dummies for region. Standard errors are estimated using the doubly robust DID estimator based on stabilized inverse probability weighting.

Table A21: ATT by subgroups, Group IV, males aged 60-64 not affiliated with the Armed Forces (SPS-APH-SCOMP)

	Group IV	
	Income-threshold	
Subgroup	A	B
Pension Income	$x \in \left(x_2, \frac{x_2+x_3}{2}\right)$	$x \in \left(\frac{x_2+x_3}{2}, x_3\right)$
Hours worked	0.0243**	0.002
(everyone)	(0.012)	(0.075)
LFP	0.054*	0.019
(everyone)	(0.035)	(0.103)
Hours worked	-0.004	-0.001
(workers)	(0.023)	(0.039)
Formality	.0103	.0209
(everyone)	(0.187)	(0.109)

Note: Robust standard errors in parentheses. p<0.01, ** p<0.05, * p<0.1. This table shows the ATT for alternative outcome variables using Callaway and Sant'Anna (2021). Treatment group is males aged 60-64 who i) are not affiliated with the Armed Forces and ii) belong to each subgroup. Subgroups A and B divide Group II by using the middle point between x_2 and x_3 in Figure 1.1 as threshold. Control group is males aged 60-64 affiliated with the Armed Forces. Each regression controls by demographic variables such as education, previous occupation and industry and dummies for region. Standard errors are estimated using the doubly robust DID estimator based on stabilized inverse probability weighting.

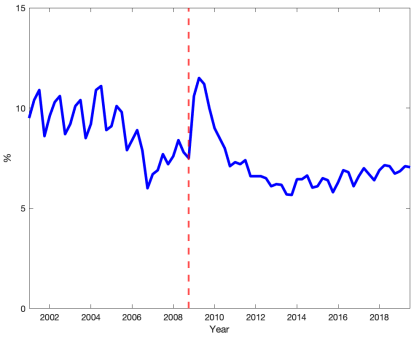
A.3 Robustness Checks

This section shows how the results change when modifying certain assumptions. In particular, one of the most critical confounding events is the 2008-2009 recession. I perform alternatives analysis accounting for the possibility of heterogeneous effects by industry and occupation, the fall in pension wealth across treatment groups, and plausible hypotheses that could affect the validity of the control groups (military workers). The main conclusions remain valid among all these cases, and the main reason is that the 2008 recession was well-managed by the Chilean government.

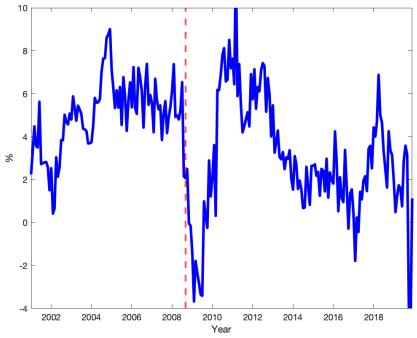
Although this crisis significantly impacted employment and economic activity in several countries, Chile had smaller and short-term effects. Figure A.11 summarizes four economic indexes that support this statement. First, panel a) depicts the unemployment rate, which was 7.5% on September 2008 and increased to 10.5% on January 2009. After twelve months of high unemployment, the index returned to values before the crisis. Panel b) shows the monthly economic activity index, which decreased from 2% to -.04% between September and October 2008. The negative growth lasted nine months until July 2009. The main reason for these short-term impacts on employment and GDP was mainly driven by active monetary and fiscal policy. The former was due to incipient inflation during the months before the crisis that allowed the Central Bank to reduce the interest rate by 775 basis points from 8.25% to 0.50% in 9 months (See panels c) and d) for details). The latter was about using the sovereign wealth funds named the Economic and Social Stabilization Fund (ESSF). The ESSF is a government-owned investment organization that manages the surplus revenues from Chile's copper exports. The ESSF reached a market value of US$20 billion in 2008 (10% of GDP), and during the recession, the government implemented a US$4 billion government fiscal stimulus plan (2% of GDP). A low-interest rate and a high level of transfers allowed the Chilean government to compensate for the negative impacts on employment and GDP due to the 2008 recession.

Figure A.11: Economic activity, unemployment, inflation and interest rate

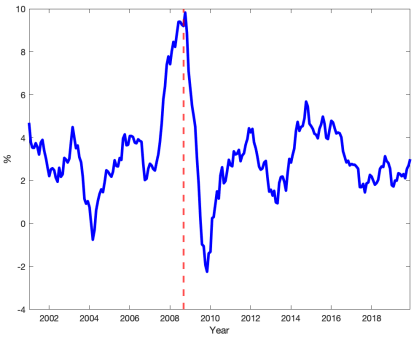
(a) Unemployment (% labor force)



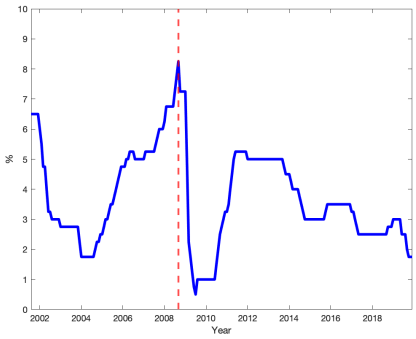
(b) Indicator of economic activity (%)



(c) Inflation (%)



(d) Interest Rate (%)

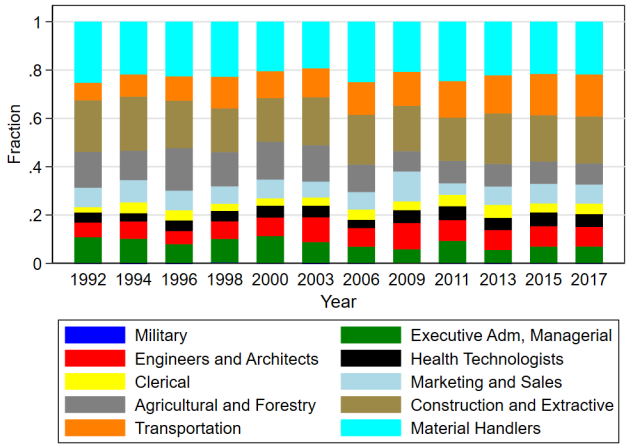


Note: Panel a) shows the unemployment rate as proportion of the labor force participation. Panel b) depicts the monthly indicator of economic activity. Panels c) and d) show the inflation and the interest rate, respectively. Data comes from the Chilean Central Bank.

A.3.1 Occupation and Industry

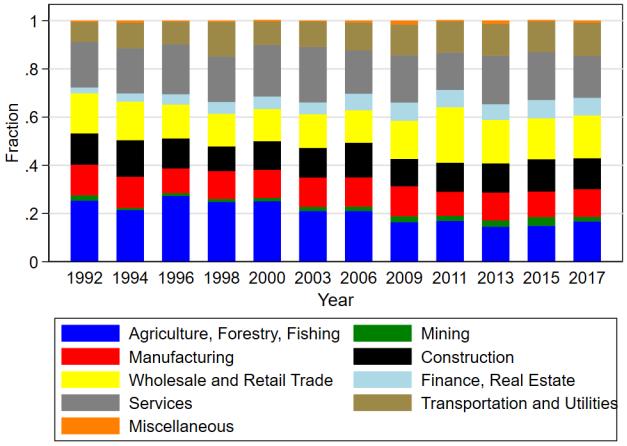
Because CASEN is a cross sectional dataset with no previous labor information, I am not able to include either individual fixed effects or previous labor labor decisions in the DID approach. I only observe industrial and occupation classification for people who work so these variables are not defined for people unemployed or out of the labor force. With these limitations in mind, I study the occupation and industrial composition over time for males aged 60-64 who work. The idea is to observe if there were radical changes in the distribution of both variables between different waves. I shows the occupation and industrial composition over time for males aged 60-64 who work. Since the population share of some occupations increased between 2006 and 2009 such as i) engineers and architects, ii) marketing and sales occupations; others such as i) material handlers, and ii) agricultural and forestry occupations decreased. Nevertheless, the composition in 2011 seems to return to 2006 levels. I see similar results for the industrial distribution.

Figure A.12: Occupation Classification, Males aged 60-64



Note: The distribution of employed males aged 60-64 by occupations is shown. Weights are used.

Figure A.13: Industrial Classification, Males aged 60-64



Note: The distribution of employed males aged 60-64 by industries is shown. Weights are used.

I then calculate the average effects on employment due to the reform using the administrative panel data. To mimick the results found in CASEN, I use i) SPS-HPA-SCOMP as a repetead cross-sectional data and ii) the same empirical approach. Table A22 concludes that the impacts on hours worked and labor force participation are 3.7% and 12.9%, respectively. These values are very similar to the ones found in Table 1.4 when using CASEN. This shows that under both datasets, there seems to be a consistent increase in employment measures as hours worked and labor force participation. One of the issues with the empirical approach is that, as stated before, the Great Recession occured. Using SPS-HPA-SCOMP, I can control for previous industry and occupation to account for the fact that some jobs might be affected more than others.

Table A22: ATT, males aged 60-64 not affiliated with the Armed Forces (SPS-APH-SCOMP)

Outcome	ATT	Pretrend Test	Pretrend
Monthly Hours worked (everyone)	0.0376** (0.0191)	3.21 [0.200]	No
LFP (everyone)	0.1290** (0.0644)	4.44 [0.108]	No
Monthly Hours worked (workers)	0.0049 (0.0108)	0.61 [0.737]	No
Frac. of Formal Workers (workers)	0.0443 (0.0744)	6.34* [0.041]	Yes

Note: Robust standard errors in parentheses. p<0.01, ** p<0.05, * p<0.1. P-values in square brackets. This table shows the ATT and the pre-trend test for alternative outcome variables using Callaway and Sant'Anna (2021). Treatment group is males aged 60-64 who are not affiliated with the Armed Forces. Control group is males aged 60-64 affiliated with the Armed Forces. Each regression controls by demographic variables such as education, housing assets, previous occupation and industry as well as dummies per region. Standard errors are estimated using the doubly robust DID estimator based on stabilized inverse probability weighting.

Table A23 shows the results for the main outcomes. Hours worked and labor force participation increased by 3.8% and 10.8%, respectively. These values are pretty close to either the ones when using CASEN or SPS-HPA-SCOMP without including previous industry and occupation (Table 1.4 and Table A22). I find no significant impacts on hours worked per worker and in the fraction of formal workers. Although the pre-trend test does not rule the existence of pre-trends, each individual point-estimate placebo ATT is statistically indistinguishable from zero.

Table A23: ATT, males aged 60-64 not affiliated with the Armed Forces controlling by occupation and industry (SPS-HPA-SCOMP)

Outcome	ATT	Pretrend Test	Pretrend
Hours worked	0.0389**	6.84**	Yes
(everyone)	(0.0153)	[0.032]	
LFP	0.1083**	10.04***	Yes
(everyone)	(0.0513)	[0.006]	
Hours worked	0.0061	0.61	No
(workers)	(0.0098)	[0.737]	
Frac. of Formal Workers	-0.0578	5.12*	Yes
(workers)	(0.0744)	[0.077]	

Note: Robust standard errors in parentheses. p<0.01, ** p<0.05, * p<0.1. P-values in square brackets. Overall ATT and pre-trend test are shown for alternative outcome variables using Callaway and Sant'Anna (2021). Treatment group is males aged 60-64 who are not affiliated with the Armed Forces. Control group is males aged 60-64 who are affiliated with the Armed Forces. Each regression controls for demographic variables such as education, housing assets, previous occupation and industry as well as region fixed effects. Standard errors are estimated using the doubly robust DID estimator based on stabilized inverse probability weighting.

The inclusion of both previous industry and occupation confirms the results found when using CASEN. Now, I use the panel features to control for any characteristics that do not vary over time. Table A24 shows the results. I find no significant effects on Group I. As before, negative and significant impacts on hours worked are found for Group II. Groups III and IV drive the positive results in hours worked, and Group IV presents a positive change in labor force participation. Although the magnitudes change with respect to Table 1.5, the main conclusions hold.

Table A24: ATT by groups using individual fixed effects, males aged 60-64 not affiliated with the Armed Forces (SPS-APH-SCOMP)

Outcome	Group I ATT	Group II ATT	Group III ATT	Group IV ATT
Hours worked	.008	-0.0534*	0.0376*	0.0297**
(everyone)	(0.034)	(0.022)	(0.020)	(0.010)
LFP	0.0413	-0.0234	0.1031	0.0705**
(everyone)	(0.070)	(0.118)	(0.1031)	(0.025)
Hours worked	-0.0055	-0.0375	0.0050	0.0114
(workers)	(0.013)	(0.025)	(0.005)	(0.023)
Frac. of Formal Workers	-0.0203	-0.1018	-0.1972	-0.0245
(everyone)	(0.089)	(0.1153)	(0.129)	(0.084)

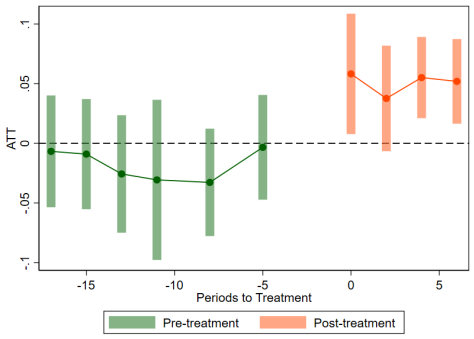
Note: Robust standard errors in parentheses. p<0.01, ** p<0.05, * p<0.1. Overall ATT is shown for alternative outcome variables using Callaway and Sant'Anna (2021). Treatment group is males aged 60-64 who are not affiliated with the Armed Forces and belong to one of the four groups. Control group is males aged 60-64 who are affiliated with the Armed Forces. Each regression controls for i) individual fixed effects and ii) time-varying demographic variables such as housing assets, previous occupation and industry, and region fixed effects. Standard errors are estimated using the doubly robust DID estimator based on stabilized inverse probability weighting.

A.3.2 CASEN: Removing data from 2009

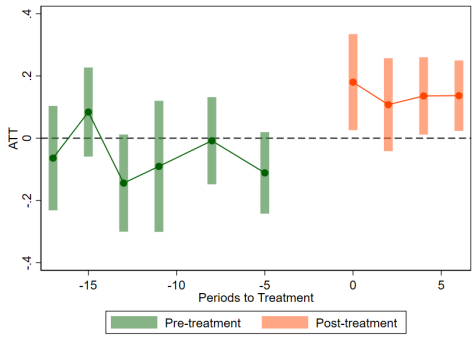
As a robustness check, I remove the 2009 data from the analysis. By doing that, 2011 is the first year where the treatment group has been treated. Figure A.14 shows the group time ATT for both hours worked per capita and labor force participation while Figure A.15 for both hours worked per worker and fraction of formal workers. The main conclusions from the baseline analysis hold: i) pre-treatment periods are not statistically different from zero, ii) post-treatment periods are positive and significant, and iii) the group time ATT are the same. Since data from 2009 does not affect the estimates for group time ATT(t) where t is different from 2009, it changes the overal ATT by removing one of the five group time ATT used in Eq. (1.4).

Figure A.14: Group time ATT, removing data from 2009, males aged 60-64 not affiliated with the Armed Forces (CASEN)

(a) Monthly Hours Worked per capita



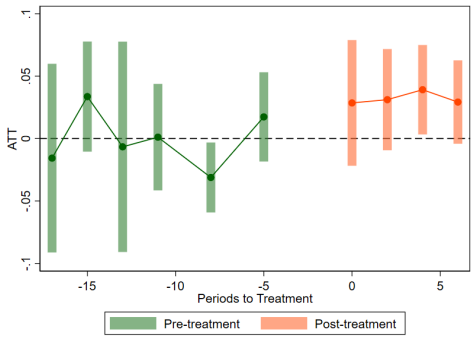
(b) Labor Force Participation



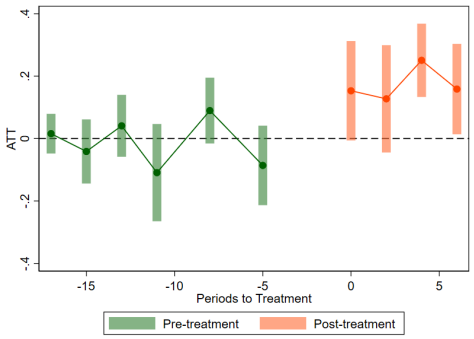
Note: Group time ATT are shown after removing the 2009 data from the analysis. Panel a) shows results for monthly hours worked per capita whereas Panel b) depicts the estimates for labor force participation using Callaway and Sant'Anna (2021). 95% confidence intervals are also included. Treatment group is males aged 60-64 who are not affiliated with the Armed Forces. Control group is males 60-64 who are affiliated with the Armed Forces. Each regression controls by demographic variables such as education, housing assets, and dummies for region. Standard errors are estimated using the doubly robust DID estimator based on stabilized inverse probability weighting. Results are obtained using population weights.

Figure A.15: Group time ATT, removing data from 2009, males aged 60-64 not affiliated with the Armed Forces (CASEN)

(a) Monthly Hours worked per worker



(b) Fraction of Formal Workers



Note: Data from 2009 is removed to estimate this robustness check. Panel a) shows the group time ATT for monthly hours worked per worker whereas Panel b) depicts the group time ATT for fraction of formal workers (unconditional of being employed) using Callaway and Sant'Anna (2021). 95% confidence intervals are also included. Treatment group is males aged 60-64 who are not affiliated with the Armed Forces. Control group is males 60-64 who are affiliated with the Armed Forces. Each regression controls by demographic variables such as education, housing assets, and dummies per region. Standard errors are estimated using the doubly robust DID estimator based on stabilized inverse probability weighting. Results are obtained using population weights.

I then calculate the overall ATT for several outcomes and the results are shown in Table A25. The overall ATT for hours worked per capita is 5.1% which increased by 2% with respect to the numbers in Table 1.4. We see similar results for labor force participation and hours worked per worker where the differences with respect to the base line results represent less than 6%. For formality, the overall ATT is 17.3% that is much larger than the baseline results. This is because the 2009 coefficient was the smallest of all the post-treatment effects: removing this estimate increases the overall ATT. Table A26 depicts the results for alternative outcome variables where the magnitudes for hours worked per formal and informal worker remain almost unchanged.

Table A25: Several outcomes, ATT, removing data from 2009, males aged 60-64 not affiliated with the Armed Forces (CASEN)

	Hours Worked (everyone)	LFP (everyone)	Hours Worked (workers)	Formality (everyone)
ATT	.051***	.139***	.032**	.173***
	(0.016)	(0.049)	(0.016)	(0.066)
Pre-trend Test	11.406*	9.300	8.594	4.915
p-value	[0.076]	[0.157]	[0.198]	[0.555]
Obs	43,830	43,830	30,928	43,830
Weights	3,027,729	3,027,729	2,158,527	3,027,729
Mean: Dep Variable	139	.759	191	.440
sd: Dep Variable	101	.427	63	.496

Note: Robust standard errors in parentheses. p<0.01, ** p<0.05, * p<0.1. P-values in square brackets. Overall ATT and pre-trend test are shown after removing the 2009 data from the analysis using Callaway and Sant'Anna (2021). Treatment group is males aged 60-64 who are not affiliated with the Armed Forces. Control group is males aged 60-64 who are affiliated with the Armed Forces. Each regression controls by demographic variables such as education, housing assets, and dummies for region. Standard errors are estimated using the doubly robust DID estimator based on stabilized inverse probability weighting. Results are obtained using population weights.

The results show that the estimate changes by removing data from 2009 are small for hours worked per capita, labor force participation, and hours worked per worker. This shows that the possible bias of the 2008 crisis is small and does not change the main conclusions. In fact, the greater overall ATT for formality when removing 2009 data seems to suggest that without the 2008 crisis, the hours worked and the fraction of formal workers could have been more significant.

Table A26: Several outcomes, ATT, removing data from 2009, males aged 60-64 not affiliated with the Armed Forces (CASEN)

	Formality (workers)	Hours Worked (formal workers)	Hours Worked (informal workers)	Duration (workers)
ATT	.125	.069***	.020	.019
	(0.100)	(0.020)	(0.026)	(3.059)
Pre-trend Test	10.455	80.425***	23.843***	11.807***
p-value	[0.107]	[0.000]	[0.001]	[0.003]
Obs	30,928	16,932	13,996	23,027
Weights	2,158,527	1,282,642	875,885	162,2391
Mean: Dep Variable	.606	196	184	16
sd: Dep Variable	.489	51	78	15

Note: Robust standard errors in parentheses. p<0.01, ** p<0.05, * p<0.1. P-values in square brackets. Data from 2009 is removed to estimate this robustness check. This table shows the ATT and the pre-trend test for alternative outcome variables using Callaway and Sant'Anna (2021). Formality conditional of being working, monthly hours worked conditional of being a formal and an informal worker. Duration is the number of years working in the same job conditional of being working. This variable is present only in eight waves (1996, 2003, 2006, 2009, 2011, 2013, 2015, 2017). Treatment group is males aged 60-64 who are not affiliated with the Armed Forces. Control group is males aged 60-64 who are affiliated with the Armed Forces. Each regression controls by demographic variables such as education, housing assets, and dummies per region. Standard errors are estimated using the doubly robust DID estimator based on stabilized inverse probability weighting. Results are obtained using population weights.

A.3.3 Financial Crisis

Workers can decide among five portfolios where the pension assets will be invested.[72] However, they cannot choose the specific stock or bond in which the savings are put. The portfolios are A, B, C, D, and E, where A has the most significant fraction of stocks (riskiest) and E has the most minor (most secure).

[72] Before 2002, the system had one portfolio. Since 2004, the system has had five options.

Workers can move between funds, but there are some constraints to protecting people close to retirement ages. Males aged 56-64 cannot have all their savings in portfolio A. They need to have a minimum level of assets outside of fund A, the amount that allows them to pay the minimum pension guaranteed (PASIS or PBS, before and after the reform). Thus, when a male is 56, savings are moved automatically from fund A to fund B if the person does not choose another fund. Suppose he has more than the minimum savings level. He could put the difference between his savings and the threshold in portfolio A. Males aged 65 and older have an additional constraint that their pension savings cannot be funded A or B. Workers could keep some of the assets in A or B only if pension savings are above the minimum pension accumulation, but few people satisfy this requirement.

Table A27 shows the distribution of males aged 60-64 among the fives funds. 67% of people in Group I do not have any portfolio because they have never worked in the formal sector. 76% and 81.25% of Group II and Group III workers are in fund D, a low-risk, low return fund. Among people in Group IV, 64% have their stocks in fund D, while 39% of males in Group V prefer portfolio C. People affiliated with the military most likely do not have any portfolio or choose fund D. In sum, because the funds are highly regulated, few people were either in portfolio A or B.

Table A27: Distribution across portfolio, Males aged 60-64 in 2008

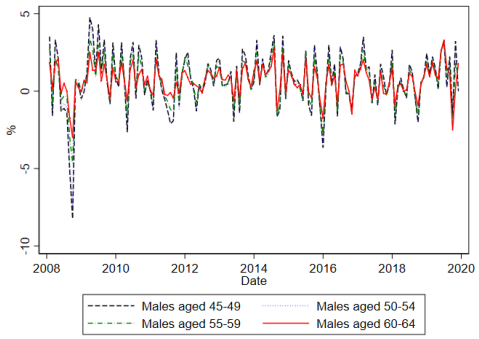
	Actual Group						
Portfolio	I	II	III	IV	V	VI	Total
	Non-contributory	Contributory	Year-threshold	Income-threshold	Wealthy	Armed Forces	
A	0.00	0.00	0.00	0.00	0.00	0.00	0.00
B	0.97	5.88	6.25	13.21	24.39	0.00	4.00
C	4.11	11.76	4.17	18.87	39.02	19.23	9.12
D	26.81	76.47	81.25	64.15	36.59	25.00	36.00
E	0.24	5.88	8.33	3.77	0.00	0.00	1.28
No	67.87	0.00	0.00	0.00	0.00	55.77	49.60

Note: The table shows the distribution of males aged 60-64 across groups and portfolios. The portfolios are A, B, C, D, and E where A has the largest fraction of stocks and E has the smallest. "No" stands for people who do not have a choice because they do not have pension wealth.

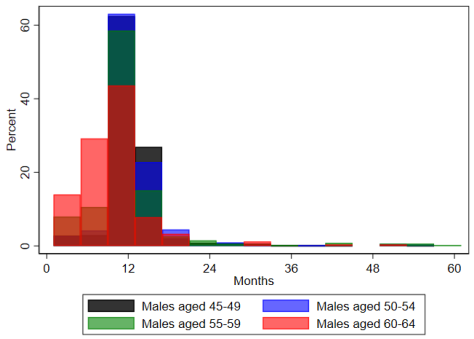
Now, I show the pension return after and the period they needed to recover the same pension savings level because of the crisis. Figure A.16a shows the monthly pension return from 2008 to 2019 for males in four age groups; 45-49, 50-54, 55-50, and 60-64. Since males aged 45-49 lost around 8% of their pension assets in October 2008, males aged 60-64 only faced a 2.3% decrease in their savings. After that, the return has been positive or zero almost all the time. Figure A.16b depicts the months that people had to wait to recover the pension assets they had in September 2008, a month before the crisis. Around 90% of males aged 60-64 had the same pension wealth in less than twelve months. For males aged 55-59, 78% recovered their pension assets in less than one year. Although, on average, males aged 45-49 and 50-54 required more time, around 60% of them had the initial pension wealth in twelve months. Finally, I repeat the same exercise but only for males aged 60-64, depending on the group they were in 2008. Figures A.16c shows that, on average, people in Groups I, II, and III have the same level of pension assets after 9.6, 10.1, and 8.0 months, respectively. Figure A.16d concludes that, on average, people in Groups IV, V, and VI have the same level of pension assets after 9.2, 10.9, and 7.4 months, respectively.

Figure A.16: Pension System

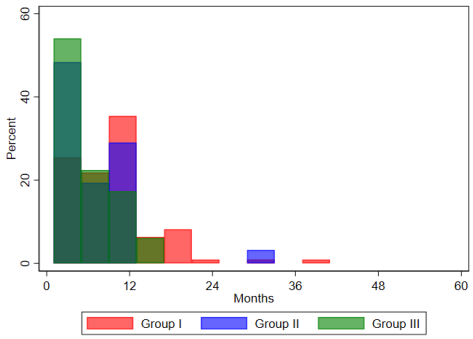
(a) Monthly Pension Return by age groups between 2008-2019



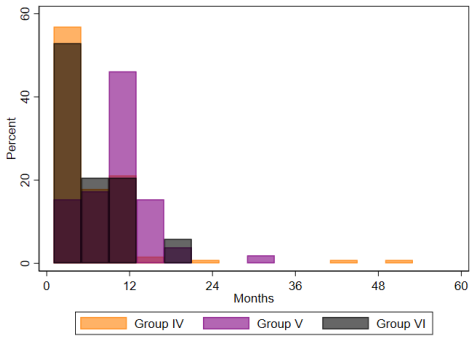
(b) Months to recover pension savings by age groups



(c) Months to recover pension savings, males aged 60-64 by groups



(d) Months to recover pension savings, males aged 60-64 by groups



Note: The figure shows the level of understanding of four important features of the FF pension system: the contribution rate, the full retirement age for males, how pension income is calculated, and the level of pension assets. The average values are shown for Groups I-VI pre- and post-reform.

To assess the impact of the negative return in October 2008, I estimate the proportion of males who moved to other groups because of the crisis. Due to the lost pension wealth, they are workers who do not qualify for the original group and therefore face alternative incentives. The first panel of Table A28 shows that less than one percent of males aged 60-64 switched to another group. Most moves are down, for instance, from Group III to Group I. On the other hand, males aged 35-39 and 40-44 changed 6.2% and 5.9% due to most of them being in portfolios A or B. One could expect that because of the reduction in pension assets, people would switch to a different portfolio to either increase the expected safety or return. The second panel of Table A28 shows the proportion of people who decided to switch to an alternative portfolio in the first twelve months after the crisis. For males aged 60-64, this proportion represents 2.5%,

the lowest among all age groups. On the other hand, the highest rate of 18.2% is among males 35-39.

Table A28: Switch in Groups and Portfolios after the 2008 recession, Males

Age Group	Switch in Groups	Switch in Portfolio
25-29	0.2%	2.9%
30-34	2.2%	3.7%
35-39	6.2%	18.2%
40-44	5.9%	4.3%
45-49	5.2%	3.2%
50-54	4.7%	2.6%
55-59	2.3%	11.9%
60-64	0.7%	2.5%
Total	3.9%	6.4%

Note: The table shows the proportion of males who switched across groups in one month between September and October 2008 because of the crisis. The second column shows the proportion of people who chose to move to another portfolio in the first twelve months after the recession.

In sum, people 60-64 lost around 2.3% of their pension wealth because of the crisis in October 2008 and less than 1% switched to another group. However, they recovered their initial level on average in 7 months. Although people in Groups III and IV might face a higher loss than workers in Group VI (control group), these three groups recovered their initial pension accumulation in less than six months.

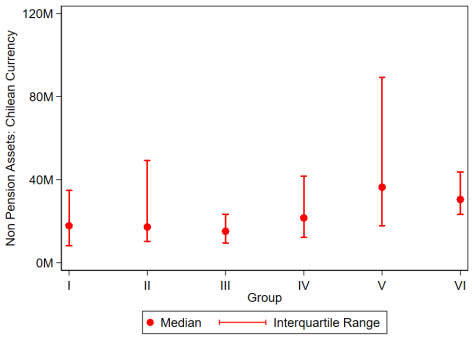
A.3.4 Using Wealthy Retirees as Control Group

The election of the Armed Forces as a control group is because they have never been eligible for pension supplements. After all, they receive benefits from the PAYG pension system. Thus, the 2008 reform does not affect them, no matter if they have contributed to the fully funded pension system. However, another group of workers has never qualified for pension benefits for a different reason. As shown in Figure 1.1b, workers to the right on x_3 belong to Group V, and because of their high pension savings, they cannot apply for transfer (pre- or post-reform).

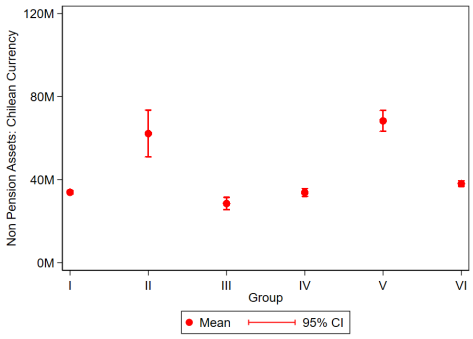
As one can expect, people in Group V tend to be wealthy with high levels of assets. To show the differences between workers in each group, Figure A.17 shows the distribution of non-pension and total assets (pension and non-pension). Non-pension assets are the sum of housing, cars, bank savings, machines, and the value of own companies. On average, the most important category is housing, representing around 88% of the total value of non-pension assets. The most important conclusion from Figure A.17b is that both Group V has the highest non-pension assets. Using Figure A.17c and Figure A.17d, I can conclude that Group V is about 2.5 times more wealthy when including pension savings than Groups I-IV. The richest groups in Group VI. One crucial detail is that I include as pension wealth the amount accumulated in the FFPS plus the entitled resources in the military pension system for Group VI. To do so, I follow Carrasco and Strajilevich (2020), who estimate that the average monthly pension income is 820,000 Chilean pesos (USD 1,285) in 2017. Thus, I estimate the average pension wealth in the fully funded pension system, such as the monthly pension income is similar to what military workers receive. I conclude that, on average, people in Groups V and VI have similar total assets.

Figure A.17: Non-pension and total pension assets, Males aged 60-64 in 2008

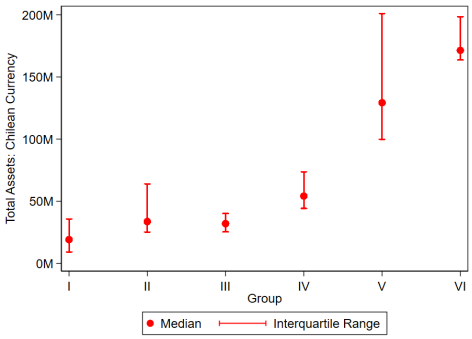
(a) Non-pension assets, Median



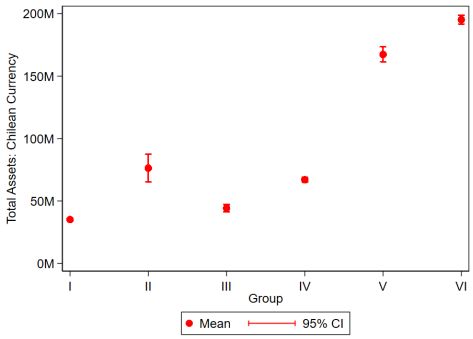
(b) Non-pension assets, Mean



(c) Total assets, Median



(d) Total assets, Mean



Note: Panel a) shows the mean of non-pension assets for Groups I-VI. Panel b) details shows the median of non-pension assets by groups. Panel c) and d) depict the median and mean of total assets (pension and non-pension) for Groups I-VI. For people in Group VI (Armed Forces), the total wealth includes the pension wealth in the fully funded pension system and the imputed pension wealth in the military pension system following the values reported in Carrasco and Strajilevich (2020).

After showing the differences and similarities between Groups V and VI, I perform the same regression as in the main text, but here, workers in Group V are the control group. The results are in Table A29. As before, I find no significant effects for Group I and a significant negative impact on hours worked for Group II. The positive impacts on working hours are still present for Groups III and IV. Additionally, I find a significant effect at 10% in labor force participation for Group IV.

Table A29: ATT by groups, People in Group V as control group, males aged 60-64

Outcome	Group I Non-Contributory	Group II Contributory	Group III Year-Threshold	Group IV Pension-Threshold
Hours worked	0.031	-0.082**	0.054**	0.053**
(everyone)	(0.030)	(0.040)	(0.025)	(0.020)
LFP	0.044	-0.098	0.113	0.154*
(everyone)	(0.071)	(0.099)	(0.090)	(0.079)
Hours worked	-0.004	-0.032	0.007	0.0135
(workers)	(0.013)	(0.022)	(0.006)	(0.016)
Formality	0.019	-0.058	0.156	0.021
(everyone)	(0.071)	(0.122)	(0.109)	(0.101)

Note: Robust standard errors in parentheses; p<0.01, ** p<0.05, * p<0.1. Overall ATT is shown for alternative outcome variables using Callaway and Sant'Anna (2021). Treatment group is men aged 60–64 years who are not affiliated with the Armed Forces and belong to one of the four groups. Control group is men aged 60–64 years who belong to Group V (wealthy workers). In each regression I control for demographic variables such as education, previous occupation, and industry, and I use dummies for the region. Standard errors are estimated using the doubly robust DID estimator based on stabilized inverse probability weighting. LFP is labor force participation.

Although there are some changes in the magnitude of the estimates, the main conclusion remains that the reform has positive impacts on employment for people in Groups III and IV while zero or negative effects for people in Groups I and II.

A.3.5 Employment of military workers and their spouses

One plausible problem with the identification strategy mentioned in Section 3.4.3 is that if enterprises preferred to hire workers without any previous experience in the military because of the 2008 reform, it would cause an increase in the labor force participation for civilians at the cost of ex-military's workers. As I showed in Figure 1.2, it does not seem to be the case because labor force participation and hours worked increase for both groups but at different rates. I, however, can show additional evidence that the demand-side effects do not drive the results.

In the SPS survey, two questions help us better comprehend if the labor supply of ex-military workers has been negatively affected by the 2008 reform. The questions say the following:

- Wave 2006: Did you complete your military service? If yes, since January of 2004, did the military service help you find a job?
- Wave 2009: Did you complete your military service? If yes, since January of 2006, did the military service help you find a job?

Table A30 shows the percentage of people who answer affirmatively to whether they have completed the military service. For all males, the proportion is close to 20% in both 2006 and 2009. Nevertheless, this proportion is twice as much among people who work or worked in the Armed Forces.

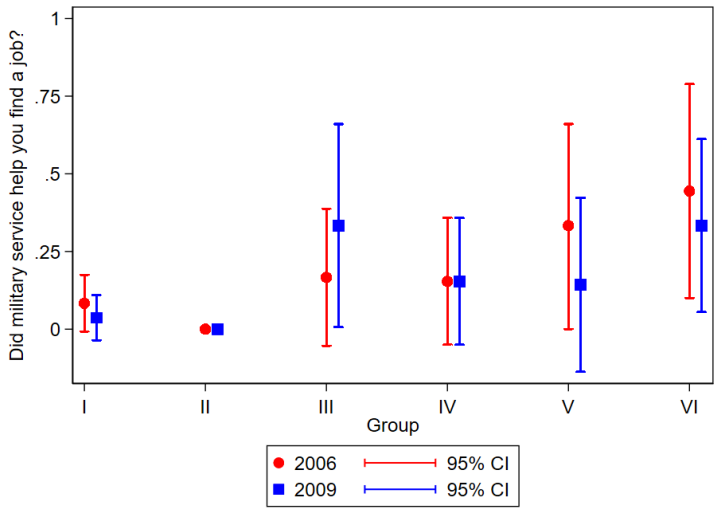
Table A30: Did you complete your military service? (SPS-HPA-SCOMP)

Age Group	Did you complete your military service? All 2006	All 2009	Armed Forces 2006	Armed Forces 2009
20-24	15.0	17.8	60.2	66.7
25-29	22.2	16.1	21.3	25.3
30-34	22.7	22.5	27.0	15.7
35-39	21.7	21.5	55.7	25.3
40-44	25.3	23.3	51.9	56.2
45-49	19.7	17.7	33.0	18.4
50-54	27.6	23.1	53.6	50.0
55-59	27.0	22.9	56.1	57.0
60-64	26.0	24.4	30.1	30.4
65-69	31.8	26.2	44.7	24.3
Total	21.8	20.9	45.9	41.9

Note: The percentage of people who completed their military service is presented among i) everyone and ii) people who are affiliated to the Armed Forces.

Figure A.18 depicts the proportion of people to whom the military service helped them to find a job conditional on having done the military service for males aged 60-64. If someone at age 20-24 answers this question it is more likely related only to the gained experience in the military service. However, if the person is 60-64 and also worked in the Armed Forces, his response sheds light on how the most recent skills have helped him find a job. As can be seen, there are no relevant (and also significant) differences in the proportion of people who consider that the military service has helped them to find a job between 2006 and 2009 when analyzing all groups. For instance, for Group VI the proportion is around 40% in 2006 and 2009. Something similar occurs when analyzing people who are in Groups I-IV.

Figure A.18: Did military service help you find a job, Males aged 60-64

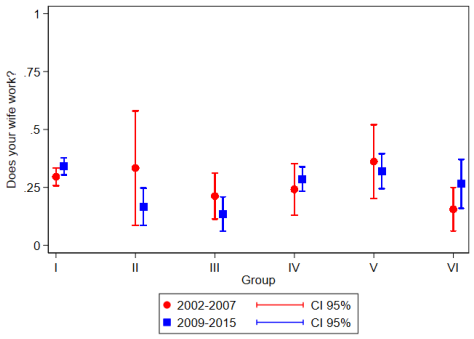


Note: The proportion of people to whom the military service helped them find a job.

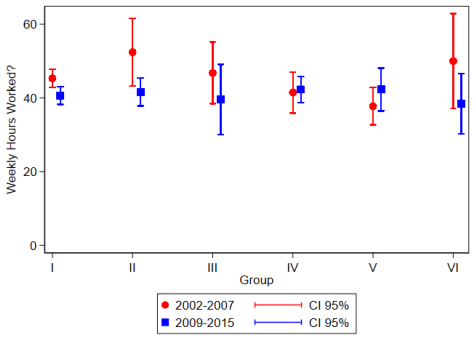
Another plausible problem with the identification strategy is that the control group would have been affected by the reform through spouses. Imagine the case of a 60-64 old male affiliated to the Armed Forces whose wife decided to work because of the 2008 reform. This behavior would affect the labor decisions of the control group and might bias the results. I test this hypothesis by constructing the participation rate, the hours worked, and the formality of wives whose husbands are 60-64 and belong to Groups I-VI. The results are shown in Figure A.19. Panel a) depicts that around 30% of wives work. Notice the low employment rate is since wives are around 60-64, and women can claim pension benefits at 60. Thus, many of them are retired. Panel b) shows the weekly hours worked for wives conditional on working. On average, they work around 40 hours per week, and there are no significant changes around 2008. In panels c) and d), I show if wives contribute and in which system. Observing panel c), I conclude that Groups I and IV present some significant jumps in their formality which might be driven by the low participation in the labor market. Finally, almost all wives contribute to the Fully Funded pension system. The exception comes from Group VI, where around 65% of spouses contribute to the Armed Forces pension system.

Figure A.19: Wives' labor decisions, Males aged 60-64

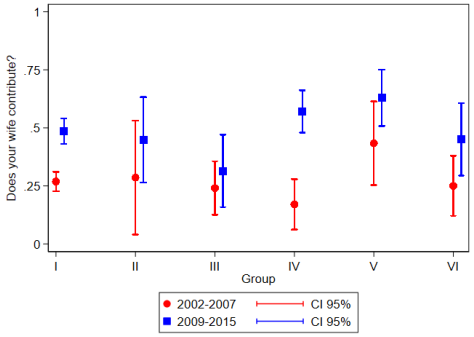
(a) Does your wife work?



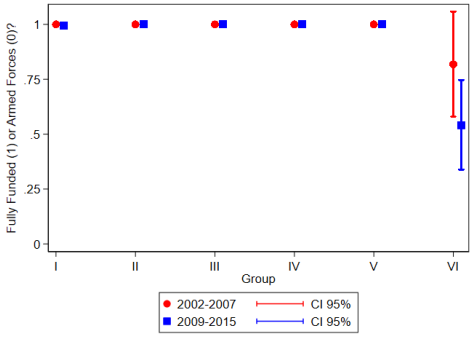
(b) Weekly hours worked? (conditional)



(c) Does your wife contribute? (conditional)



(d) Where? Fully Funded (1) or Armed Forces (0)



Note: Panel a) shows the proportion of wives who work for Groups I-VI. Panel b) details the average weekly hours worked by groups (conditional on working). Panel c) depicts the formality among wives who work for Groups I-VI. Panel d) shows if wives contribute (conditional on contributing) to the Fully Funded pension system or the Armed Forces pension system.

The null changes in the employment rates, hours worked, and small changes in formality among wives do not support the idea of indirect impacts on labor decisions for the control group through wives. Although it would be fantastic to dig deeper into the changes in spouses' labor supply, the SPS-HPA-SCOMP data does not allow us to know pension wealth and the number of contributions of spouses.

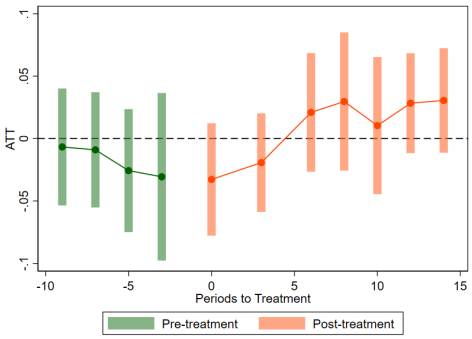
A.4 Placebo Tests

A.4.1 Placebo Test: Treatment in 2003

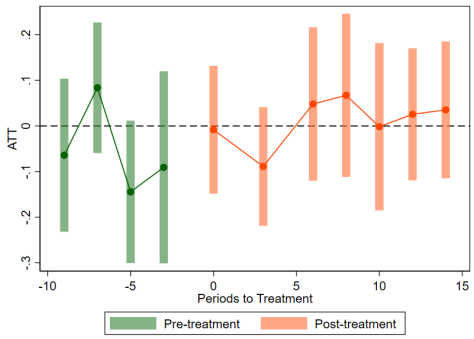
As a placebo test, I estimate the impacts of an imaginary reform that occurs in 2003. Figure A.20 shows the group time ATT for hours worked per capita and labor force participation. For both variables the group time ATT in 2003 and 2006 are negative since the rest of the post treatment ATT are positive but not significant. This is because nothing relevant for the treatment group happens in the first two time periods and the procedure obtains ATT that are not distinguishable from zero.

Figure A.20: Group time ATT, Placebo Test, Treatment in 2003 (CASEN)

(a) Monthly Hours worked per capita



(b) Labor Force Participation

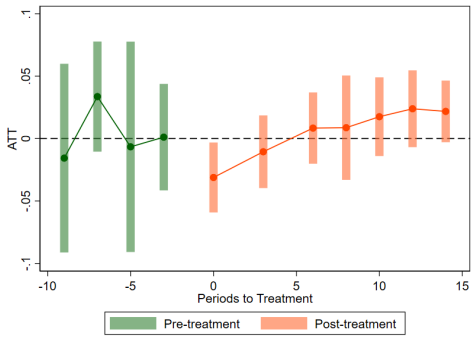


Note: Group time ATT are shown when I estimate the impacts of an imaginary reform that occurs in 2003. Panel a) shows the results for monthly hours worked per capita whereas Panel b) depicts the estimates for labor force participation using Callaway and Sant'Anna (2021). 95% confidence intervals are also included. Treatment group is males aged 60-64 who are not affiliated to Armed Forces. Control group is males aged 60-64 who are affiliated to Armed Forces. Each regression controls by demographic variables such as education, housing assets, and dummies for region. Standard errors are estimated using the doubly robust DID estimator based on stabilized inverse probability weighting. Results are obtained using population weights.

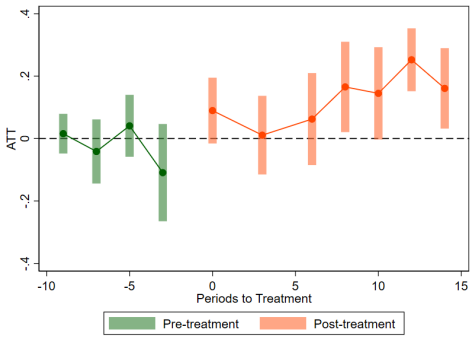
Similar results hold for hours worked per worker and fraction of formal workers are shown in Figure A.21. Nevertheless, for the fraction of formal worker the last four post treatment ATT (2011, 2013, 2015, and 2017) are positive and significant which makes the overall ATT be positive and significant.

Figure A.21: Group time ATT, Placebo Test, Treatment in 2003, males aged 60-64 not affiliated with the Armed Forces (CASEN)

(a) Monthly Hours Worked per worker



(b) Fraction of Formal Workers



Note: Placebo test assumes that the reform took place in 2003. Panel a) shows the ATT for hours worked per worker whereas Panel b) depicts the ATT for the fraction of formal workers using Callaway and Sant'Anna (2021). 95% confidence intervals are also included. Treatment group is males aged 60-64 who are not affiliated to Armed Forces. Control group is males aged 60-64 who are affiliated to Armed Forces. Each regression controls by demographic variables as education, housing assets, and dummies per region. Standard errors are estimated using the doubly robust DID estimator based on stabilized inverse probability weighting. Results are obtained using weights.

Table A31 shows the overall ATT for the main outcomes when using CASEN. I find no significant values for the ATT for both hours worked, labor force participation, hours worked per worker. For the fraction of formal workers, the ATT is positive and significant at 1% due to the significant effects of the last ATT that more likely are capturing the effect of the 2008 reform. No significant overall ATT are found once removing data after 2009. I find no evidence on hours worked per formal and informal workers in Table A32.

Table A31: Several outcomes, ATT, Placebo Test, Treatment in 2003, males aged 60-64 not affiliated with the Armed Forces (CASEN)

	Hours Worked (everyone)	LFP (everyone)	Hours Worked (workers)	Formality (everyone)
ATT	.012	.014	.008	.133***
	(0.018)	(0.063)	(0.010)	(0.052)
Pre-trend Test	4.885	6.959	3.730	2.619
p-value	[0.299]	[0.138]	[0.444]	[0.624]
Observations	48,979	48,979	34,432	48,979
Weights	3,344,245	3,344,245	2,384,454	3,344,245
Mean: Dep Variable	138	.759	190	.438
SD: Dep Variable	101	.428	64	.496

Note: Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1. P-values in square brackets. Overall ATT and pre-trend test are shown when I estimate the impacts of an imaginary reform that occurs in 2003 using Callaway and Sant'Anna (2021). Treatment group is males aged 60-64 who are not affiliated with the Armed Forces. Control group is males aged 60-64 who are affiliated with the Armed Forces. Each regression controls by demographic variables such as education, housing assets, and dummies for region. Standard errors are estimated using the doubly robust DID estimator based on stabilized inverse probability weighting. Results are obtained using population weights.

Table A32: Several outcomes, ATT, Placebo Test, Treatment in 2003, males aged 60-64 not affiliated with the Armed Forces (CASEN)

	Formality (workers)	Hours Worked (formal workers)	Hours Worked (informal workers)	Duration (workers)
ATT	.086	-.017	.012	6.594***
	(0.096)	(0.014)	(0.014)	(1.895)
Pre-trend Test	10.447	59.707***	13.956***	-
p-value	[0.034]	[0.000]	[0.007]	-
Observations	34,432	18,692	15,740	26,531
Weights	2,384,454	1,412,009	972,445	1,848,318
Mean: Dep Variable	.603	196	182	16
SD: Dep Variable	.489	52	78	14

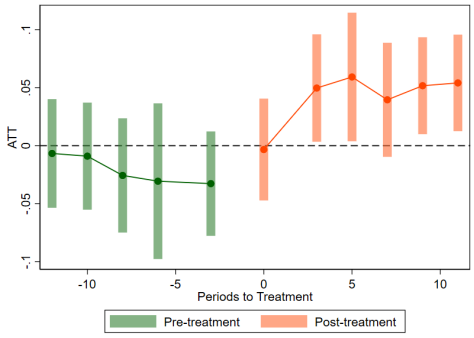
Note: Robust standard errors in parentheses. p<0.01, ** p<0.05, * p<0.1. P-values in square brackets. Placebo test assumes that the reform took place in 2003. This table shows the ATT and the pre-trend test for alternative outcome variables using Callaway and Sant'Anna (2021). Formality conditional of being working, hours worked conditional of being a formal and an informal worker. Duration is the number of years working in the same job conditional of being working. This variable is present only in eight waves (1996, 2003, 2006, 2009, 2011, 2013, 2015, 2017). Treatment group is males aged 60-64 who are not affiliated with the Armed Forces. Control group is males aged 60-64 affiliated with the Armed Forces. Each regression controls by demographic variables such as education, housing assets, and dummies per region. Standard errors are estimated using the doubly robust DID estimator based on stabilized inverse probability weighting. Results are obtained using population weights.

A.4.2 Placebo Test: Treatment in 2006

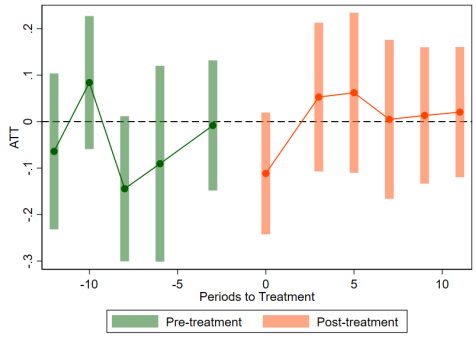
As a placebo test, I estimate the impacts of an imaginary reform that occurs in 2006. Figure A.22 shows the group time ATT for hours worked per capita and labor force participation while A.23 for hours worked per worker and fraction of formal workers.

Figure A.22: Group time ATT, Placebo Test, Treatment in 2006, males aged 60-64 not affiliated with the Armed Forces (CASEN)

(a) Monthly Hours worked per capita



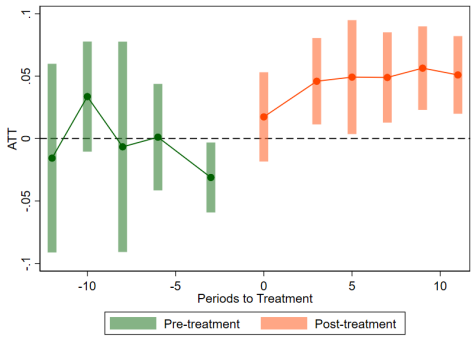
(b) Labor Force Participation



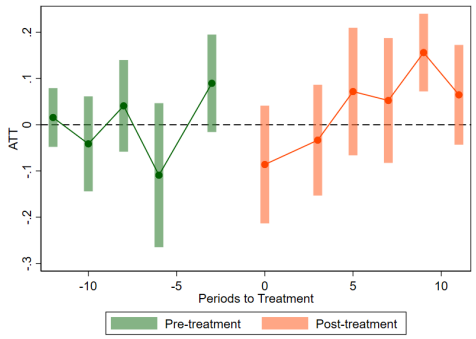
Note: Placebo test assumes that the reform took place in 2006. Panel a) shows the ATT for monthly hours worked per capita whereas Panel b) depicts the ATT for labor force participation using Callaway and Sant'Anna (2021). 95% confidence intervals are also included. Treatment group is males aged 60-64 who are not affiliated to Armed Forces pension system. Control group is males aged 60-64 who are affiliated to Armed Forces. Each regression controls by demographic variables as education, housing assets, and dummies per region. Standard errors are estimated using the doubly robust DID estimator based on stabilized inverse probability weighting. Results are obtained using weights.

Figure A.23: Group time ATT, Placebo Test, Treatment in 2006, males aged 60-64 not affiliated with the Armed Forces (CASEN)

(a) Monthly Hours Worked per worker



(b) Fraction of Formal Workers



Note: Placebo test assumes that the reform took place in 2006. Panel a) shows the ATT for Monthly hours worked per worker whereas Panel b) depicts the ATT for the fraction of formal workers using Callaway and Sant'Anna (2021). 95% confidence intervals are also included. Treatment group is males aged 60-64 who are not affiliated to Armed Forces. Control group is males aged 60-64 who are affiliated to Armed Forces. Each regression controls by demographic variables as education, housing assets, and dummies per region. Standard errors are estimated using the doubly robust DID estimator based on stabilized inverse probability weighting. Results are obtained using weights.

Table A33: Several outcomes, ATT, Placebo Test, Treatment in 2006, males aged 60-64 not affiliated with the Armed Forces (CASEN)

	Hours Worked (everyone)	LFP (everyone)	Hours Worked (workers)	Formality (everyone)
ATT	.043**	.010	.046***	.043
	(0.019)	(0.061)	(0.014)	(0.047)
Pre-trend Test	9.593*	7.568	8.528	4.114
p-value	[0.088]	[0.182]	[0.129]	[0.533]
Observations	48,979	48,979	34,432	48,979
Weights	3,344,245	3,344,245	2,384,454	3,344,245
Mean: Dep Variable	138	.759	190	.438
SD: Dep Variable	101	.428	64	.496

Note: Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1. P-values in square brackets. Placebo test assumes that the reform took place in 2006. This table shows the ATT and the pre-trend test for alternative outcome variables using Callaway and Sant'Anna (2021). Treatment group is males aged 60-64 who are not affiliated with the Armed Forces. Control group is males aged 60-64 who are affiliated with the Armed Forces. Each regression controls by demographic variables such as education, housing assets, and dummies per region. Standard errors are estimated using the doubly robust DID estimator based on stabilized inverse probability weighting. Results are obtained using population weights.

Table A34: Several outcomes, ATT, Placebo Test, Treatment in 2006, males aged 60-64 not affiliated with the Armed Forces (CASEN)

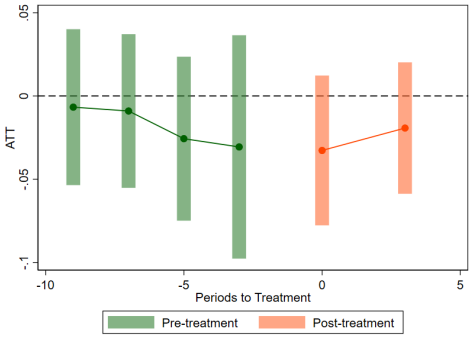
	Formality (workers)	Hours Worked (formal workers)	Hours Worked (informal workers)	Duration (workers)
ATT	.025	.003	.070***	-2.417
	(0.089)	(0.028)	(0.024)	(1.675)
Pre-trend Test	10.449*	60.793***	17.480***	11.792***
p-value	[0.063]	[0.000]	[0.004]	[0.001]
Observations	34,432	18,692	15,740	26,531
Weights	2,384,454	1,412,009	972,445	1,848,318
Mean: Dep Variable	.603	196	182	16
SD: Dep Variable	.489	52	78	14

Note: Robust standard errors in parentheses. p<0.01, ** p<0.05, * p<0.1. P-values in square brackets. Placebo test assumes that the reform took place in 2006. This table shows the ATT and the pre-trend test for alternative outcome variables using Callaway and Sant'Anna (2021). Formality conditional of being working, hours worked conditional of being a formal and an informal worker. Finally, duration is the number of years working in the same job conditional of being working. This variable is present only in eight waves (1996, 2003, 2006, 2009, 2011, 2013, 2015, 2017). Treatment group is males aged 60-64 who are not affiliated with the Armed Forces. Control group is males aged 60-64 who are affiliated with the Armed Forces. Each regression controls by demographic variables such as education, housing assets, and dummies per region. Standard errors are estimated using the doubly robust DID estimator based on stabilized inverse probability weighting. Results are obtained using population weights.

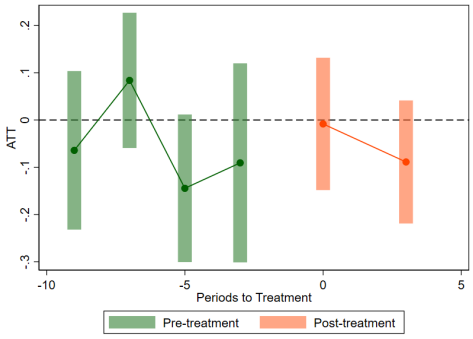
A.4.3 Placebo Test: Treatment in 2003 using data until 2006

Figure A.24: Group time ATT, Placebo Test, Treatment in 2003 using data until 2006, males aged 60-64 not affiliated with the Armed Forces (CASEN)

(a) Monthly Hours worked per capita



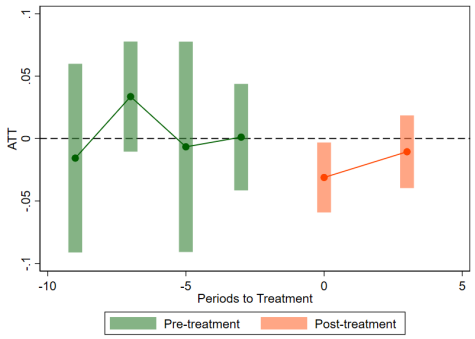
(b) Labor Force Participation



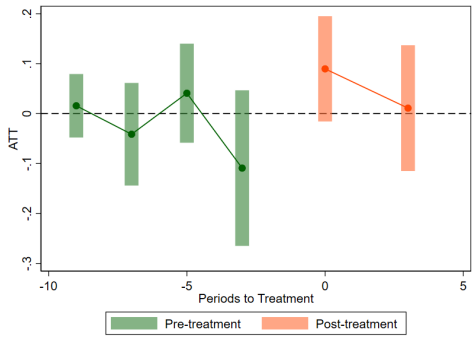
Note: Placebo test assumes that the reform took place in 2003 and I only use data until 2006. Panel a) shows the ATT for monthly hours worked per capita whereas Panel b) depicts the ATT for labor force participation using Callaway and Sant'Anna (2021). 95% confidence intervals are also included. Treatment group is males aged 60-64 who are not affiliated to Armed Forces pension system. Control group is males aged 60-64 who are affiliated to Armed Forces pension system. Each regression controls by demographic variables such as education, housing assets, and dummies per region. Standard errors are estimated using the doubly robust DID estimator based on stabilized inverse probability weighting. Results are obtained using weights.

Figure A.25: Group time ATT, Placebo Test, Treatment in 2003 using data until 2006, males aged 60-64 not affiliated with the Armed Forces (CASEN)

(a) Monthly Hours Worked per worker



(b) Fraction of Formal Workers



Note: Placebo test assumes that the reform took place in 2003 and I only use data until 2006. Panel a) shows the ATT for monthly hours worked per worker whereas Panel b) depicts the ATT for the fraction of formal workers using Callaway and Sant'Anna (2021). 95% confidence intervals are also included. Treatment group is males aged 60-64 who are not affiliated to Armed Forces. Control group is males aged 60-64 who are affiliated to Armed Forces. Each regression controls by demographic variables as education, housing assets, and dummies per region. Standard errors are estimated using the doubly robust DID estimator based on stabilized inverse probability weighting. Results are obtained using weights.

Table A35: Several outcomes, ATT, Treatment in 2003 using data until 2006, males aged 60-64 not affiliated with the Armed Forces (CASEN)

	Hours Worked (everyone)	LFP (everyone)	Hours Worked (workers)	Formality (everyone)
ATT	-.026	-.049	-.020*	.050
	(0.018)	(0.060)	(0.012)	(0.049)
Pre-trend Test	4.884	6.959	3.730	2.619
p-value	[0.299]	[0.138]	[0.444]	[0.624]
Obs	23,558	23,558	15,591	23,558
Weights	1,535,750	1,535,750	1,010,370	1,535,750
Mean: Dep Variable	131	.711	195	.374
sd: Dep Variable	107	.453	68	.484

Note: Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1. P-values in square brackets. Placebo test assumes that the reform took place in 2003 and I only use data until 2006. This table shows the ATT and the pre-trend test for alternative outcome variables using Callaway and Sant'Anna (2021). Treatment group is males aged 60-64 who are not affiliated with the Armed Forces. Control group is males aged 60-64 who are affiliated with the Armed Forces. Each regression controls by demographic variables such as education, housing assets, and dummies per region. Standard errors are estimated using the doubly robust DID estimator based on stabilized inverse probability weighting. Results are obtained using population weights.

Table A36: Several outcomes, ATT, Treatment in 2003 using data until 2006, males aged 60-64 not affiliated with the Armed Forces (CASEN)

	Formality (workers)	Hours Worked (formal workers)	Hours Worked (informal workers)	Duration (workers)
ATT	.031	-.046***	-.018	6.755***
	(0.070)	(0.012)	(0.020)	2.324
Pre-trend Test	10.447	59.706	13.956	-
p-value	[0.034]	[0.000]	[0.007]	-
Obs	15,591	7,491	8,100	7,690
Weights	1,010,370	550,646	459,724	474,234
Mean: Dep Variable	.555	200	189	16
sd: Dep Variable	.497	57	80	15

Note: Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1. P-values in square brackets. Placebo test assumes that the reform took place in 2003 and I only use data until 2006. This table shows the ATT and the pre-trend test for alternative outcome variables using Callaway and Sant'Anna (2021). Formality conditional of being working, hours worked conditional of being a formal and an informal worker. Finally, duration is the number of years working in the same job conditional of being working. This variable is present only in eight waves (1996, 2003, 2006, 2009, 2011, 2013, 2015, 2017). Treatment group is males aged 60-64 who are not affiliated with the Armed Forces. Control group is males aged 60-64 who are affiliated with the Armed Forces. Each regression controls by demographic variables such as education, housing assets, and dummies per region. Standard errors are estimated using the doubly robust DID estimator based on stabilized inverse probability weighting. Results are obtained using population weights.

A.5 Impacts for Females

A.5.1 CASEN

Table A37: CASEN Sample, total females and females aged 60-64

	Females			Females aged 60-64		
Year	Weights	Obs.	Fraction of Formal Workers	Weights	Obs.	Fraction of Formal Workers
1992	6,929,398	72,614	.60	252,444	2,506	.46
1994	7,149,043	90,353	.63	268,907	3,269	.46
1996	7,365,832	67,973	.63	238,817	2,231	.40
1998	7,568,763	95,003	.62	245,886	3,256	.46
2000	7,703,672	126,623	.61	269,378	4,651	.44
2003	8,021,032	129,502	.60	285,657	4,959	.44
2006	7,763,279	127,277	.61	313,328	5,388	.46
2009	8,608,364	125,822	.59	378,997	5,800	.42
2011	8,889,261	104,103	.65	408,330	4,936	.41
2013	9,093,525	114,236	.66	455,287	5,512	.46
2015	9,246,864	139,376	.66	479,326	7,393	.39
2017	9,337,703	113,117	.66	533,177	6,683	.45
Total	97,676,736	1,305,999	.63	4,129,534	56,584	.43

Note: Distributions of total females and females aged 60-64 are shown in the left and right column, respectively. Total weights and observations are included. The fraction of formal workers is presented for each wave.

Table A38: Transition Probability Matrix, females aged 20-64 (CASEN 2003)

	Current Period: 2003			
Previous Period: 2000	Out of labor force	Unemployed	Formal	Informal
Out of labor force	77.9	5.5	6.7	9.9
Unemployed	14.2	20.3	35.6	29.9
Formal	11.2	7.2	73.8	7.8
Informal	10.1	4.7	11.0	74.2
Total	51.2	6.3	25.9	16.6

Note: The transition probability matrix is calculated using CASEN 2003 concerning current and previous observed activities: out of labor force, unemployed, formal, and informal. The sample contains females aged 20-64. Weights are used.

Table A39: Formality across occupations, females (CASEN 2003)

Occupation	Females aged 20-64			Females aged 60-64		
	Formal Worker			Formal Worker		
	No	Yes	Total	No	Yes	Total
Military	1,824	38	1,862	-	-	-
	97.96	2.04	100.00	-	-	-
Executive Administrative	65,215	34,407	99,622	6,106	4,654	10,760
	65.46	34.54	100.00	56.75	43.25	100.00
Engineers and Architects	37,085	207,446	244,531	1,109	6,422	7,531
	15.17	84.83	100.00	14.73	85.27	100.00
Health Technologists	53,916	176,374	230,290	2,147	4,784	6,931
	23.41	76.59	100.00	30.98	69.02	100.00
Clerical	35,459	273,839	309,298	363	4,876	5,239
	11.46	88.54	100.00	6.93	93.07	100.00
Marketing and Sales	233,142	291,154	524,296	13,954	5,412	19,366
	44.47	55.53	100.00	72.05	27.95	100.00
Agricultural and Forestry	23,037	20,336	43,373	1,896	361	2,257
	53.11	46.89	100.00	84.01	15.99	100.00
Construction	87,748	50,018	137,766	6,749	1,299	8,048
	63.69	36.31	100.00	83.86	16.14	100.00
Transportation	15,376	38,700	54,076	479	925	1,404
	28.43	71.57	100.00	34.12	65.88	100.00
Material Handlers	280,017	309,244	589,261	16,009	13,015	29,024
	47.52	52.48	100.00	55.16	44.84	100.00
Total	832,819	1,401,556	2,234,375	48,812	41,748	90,560
	37.27	62.73	100.00	53.90	46.10	100.00

Note: The number and proportion of formal workers are shown by occupations. Averages are calculated using the corresponding weights.

Table A40: Formality across industries, females (CASEN 2003)

Industry	Females aged 20-64			Females aged 60-64		
	Formal Worker			Formal Worker		
	No	Yes	Total	No	Yes	Total
Agriculture, Forestry	56,231	92,400	148,631	4,032	1,333	5,365
	37.83	62.17	100.00	75.15	24.85	100.00
Mining	658	5,278	5,936	-	-	-
	11.08	88.92	100.00	-	-	-
Manufacturing	104,122	132,133	236,255	6,699	3,309	10,008
	44.07	55.93	100.00	66.94	33.06	100.00
Construction	6,626	20,261	26,887	32	463	495
	24.64	75.36	100.00	6.46	93.54	100.00
Wholesale and Retail	252,610	302,604	555,214	16,776	7,127	23,903
	45.50	54.50	100.00	70.18	29.82	100.00
Finance, real estate	32,489	123,458	155,947	1,032	1,772	2,804
	20.83	79.17	100.00	36.80	63.20	100.00
Services	351,118	643,491	994,609	19,242	26,343	45,585
	35.30	64.70	100.00	42.21	57.79	100.00
Transportation and p. utilities	20,674	62,859	83,533	438	249	687
	24.75	75.25	100.00	63.76	36.24	100.00
Miscellaneous	8,291	19,072	27,363	561	1,152	1,713
	30.30	69.70	100.00	32.75	67.25	100.00
Total	832,819	1,401,556	2,234,375	48,812	41,748	90,560
	37.27	62.73	100.00	53.90	46.10	100.00

Note: The number and proportion of formal workers are shown by industries. Averages are calculated using the corresponding weights.

Table A41: Females 60-64, affiliated versus not affiliated to Armed Forces (CASEN)

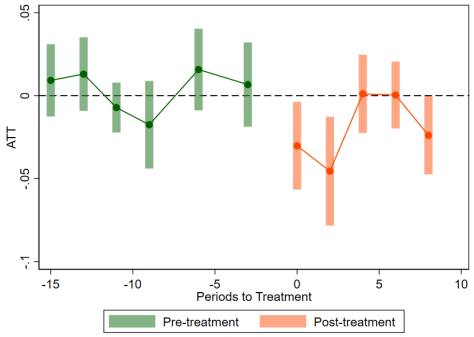
Year	Not Affiliated		Affiliated		Total	
	Number	%	Number	%	Number	%
1992	240,083	95.1%	12,361	4.9%	252,444	100.0%
1994	254,430	94.6%	14,477	5.4%	268,907	100.0%
1996	222,107	93.0%	16,710	7.0%	238,817	100.0%
1998	233,002	94.8%	12,884	5.2%	245,886	100.0%
2000	253,800	94.2%	15,578	5.8%	269,378	100.0%
2003	269,584	94.4%	16,073	5.6%	285,657	100.0%
2006	301,826	96.3%	11,502	3.7%	313,328	100.0%
2009	364,820	96.3%	14,177	3.7%	378,997	100.0%
2011	394,812	96.7%	13,518	3.3%	408,330	100.0%
2013	439,187	96.5%	16,100	3.5%	455,287	100.0%
2015	462,169	96.4%	17,157	3.6%	479,326	100.0%
2017	511,835	96.0%	21,342	4.0%	533,177	100.0%
Total	3,947,655	95.6%	181,879	4.4%	4,129,534	100.0%

Note: Column one shows the distributions of females aged 60-64 who are not affiliated to the Armed Forces' pension system. Column two depicts the distributions of females aged 60-64 who are affiliated to the Armed Forces' pension system. Column three shows the total distribution of females aged 60-64. The proportion is calculated using the corresponding weights.

Figure A.26: Group time ATT, females aged 60-64 not affiliated in Armed Forces (CASEN)

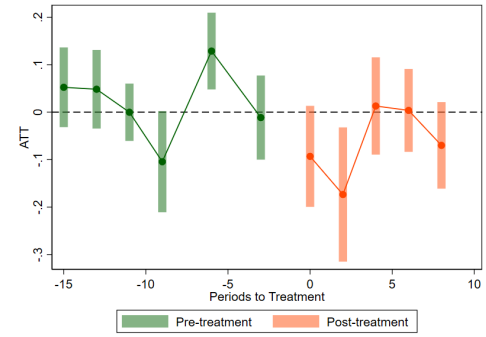
(a) Monthly Hours Worked per capita

females.png



(b) Labor Force Participation

females.png

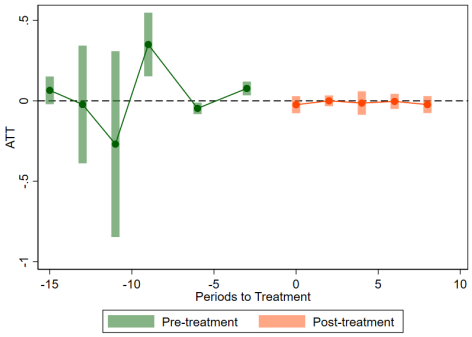


Note: Panel a) shows the ATT for monthly hours worked per capita whereas Panel b) depicts the ATT for labor force participation using Callaway and Sant'Anna (2021). 95% confidence intervals are also included. Treatment group is females aged 60-64 who are not affiliated to Armed Forces. Control group is females 60-64 who are affiliated to Armed Forces. In addition to the DID variable, each regression controls by demographic variables such as education, housing assets, previous occupation and industry as well as dummies per region. Standard errors are estimated using the doubly robust DID estimator based on stabilized inverse probability weighting. Results are obtained using weights.

Figure A.27: Group time ATT, females aged 60-64 not affiliated in Armed Forces (CASEN)

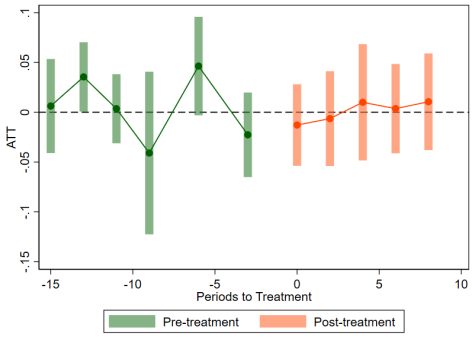
(a) Monthly Hours Worked per worker

females.png



(b) Fraction of Formal Workers

females.png



Note: Panel a) shows the ATT for monthly hours worked per capita whereas Panel b) depicts the ATT for labor force participation using Callaway and Sant'Anna (2021). 95% confidence intervals are also included. Treatment group is females aged 60-64 who are not affiliated to Armed Forces. Control group is females 60-64 who are affiliated to Armed Forces. In addition to the DID variable, each regression controls by demographic variables such as education, housing assets, previous occupation and industry as well as dummies per region. Standard errors are estimated using the doubly robust DID estimator based on stabilized inverse probability weighting. Results are obtained using weights.

Table A42: Several outcomes, ATT, Treatment in 2003 using data until 2006, males aged 60-64 not affiliated with the Armed Forces (CASEN)

	Hours Worked (everyone)	LFP (everyone)	Hours Worked (workers)	Formality (everyone)
ATT	-.019**	-.062*	-.013	.002
	(0.009)	(0.036)	(0.020)	(0.018)
Pre-trend Test	7.619	16.860***	42.081***	11.229*
p-value	[0.267]	[0.000]	[0.000]	[0.082]
Obs	56,057	56,057	13,970	56,057
Weights	4,098,835	4,098,835	1,150,791	4,098,835
Mean: Dep Variable	48	.300	167	.126
sd: Dep Variable	87	.458	80	.332

Note: Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1. P-values in square brackets. This table shows the ATT and the pre-trend test for alternative outcome variables using Callaway and Sant'Anna (2021). Treatment group is females aged 60-64 who are not affiliated with the Armed Forces. Control group is females aged 60-64 affiliated with the Armed Forces. Each regression controls by demographic variables such as education, housing assets, and dummies per region. Standard errors are estimated using the doubly robust DID estimator based on stabilized inverse probability weighting. Results are obtained using population weights.

Table A43: Several outcomes, ATT, Treatment in 2003 using data until 2006, males aged 60-64 not affiliated with the Armed Forces (CASEN)

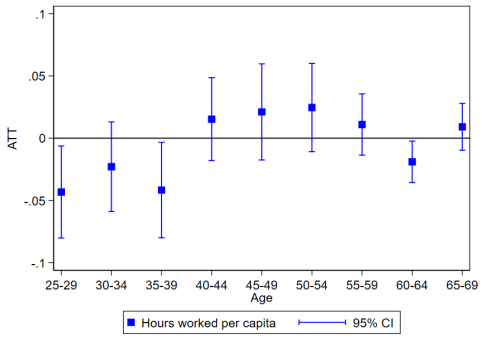
	Formality (workers)	Hours Worked (formal workers)	Hours Worked (informal workers)	Duration (workers)
ATT	.108	-.515***	-.027	-3.055
	(0.091)	(0.062)	(0.018)	(2.037)
Pre-trend Test	7.461	70.888	5.707	4.591
p-value	[0.280]	[0.000]	[0.457]	[0.101]
Obs	13,970	5,599	8,371	11,698
Weights	1,150,791	498,765	652,026	954,242
Mean: Dep Variable	.439	179	157	13
sd: Dep Variable	.496	61	90	13

Note: Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1. P-values in square brackets. This table shows the ATT and the pre-trend test for alternative outcome variables using Callaway and Sant'Anna (2021). Formality conditional of being working, hours worked conditional of being a formal and an informal worker. Finally, duration is the number of years working in the same job conditional of being working. This variable is present only in eight waves (1996, 2003, 2006, 2009, 2011, 2013, 2015, 2017). Treatment group is females aged 60-64 who are not affiliated with the Armed Forces. Control group is females aged 60-64 affiliated with the Armed Forces. Each regression controls by demographic variables such as education, housing assets, and dummies per region. Standard errors are estimated using the doubly robust DID estimator based on stabilized inverse probability weighting. Results are obtained using population weights.

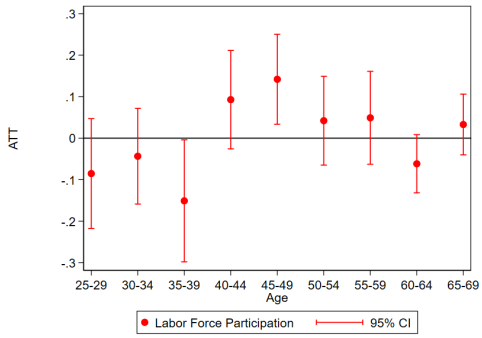
Different Age Groups

Figure A.28: ATT, females in different age groups (CASEN)

(a) Monthly Hours worked per capita



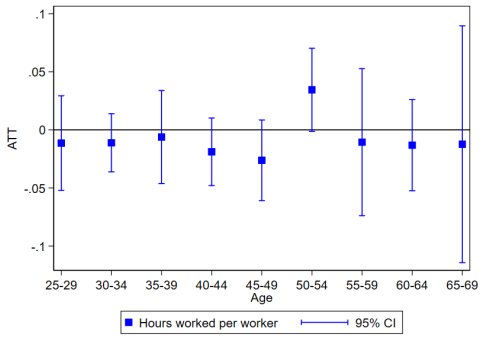
(b) Labor Force Participation



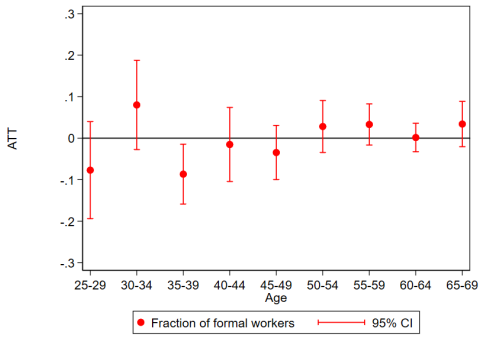
Note: Panel a) shows the ATT for monthly hours worked per capita whereas Panel b) depicts the ATT for labor force participation using Callaway and Sant'Anna (2021). 95% confidence intervals are also included. For each age group, a DID estimator is performed. For these groups, the treatment group is females who are not affiliated to Armed Forces while the control group is females affiliated to Armed Forces. Each regression controls by demographic variables such as education, housing assets as dummies per region. Standard errors are estimated using the doubly robust DID estimator based on stabilized inverse probability weighting. Results are obtained using weights.

Figure A.29: ATT, females in different age groups (CASEN)

(a) Monthly Hours worked per worker



(b) Fraction of Formal Workers



Note: Panel a) shows the ATT for hours worked per worker whereas Panel b) depicts the ATT for the fraction of formal workers using Callaway and Sant'Anna (2021). 95% confidence intervals are also included. For each age group, a DID estimator is performed. For these groups, the treatment group is females who are not affiliated to Armed Forces while the control group is females affiliated to Armed Forces. Each regression controls by demographic variables such as education, housing assets as well as dummies per region. Standard errors are estimated using the doubly robust DID estimator based on stabilized inverse probability weighting. Results are obtained using weights.

A.5.2 SPS-HPA-SCOMP

Table A44: Pension Systems, females (SPS-APH-SCOMP)

	Freq	Percent
New System	11,527	84.6
Old System	114	.8
Armed Forces	4	.0
Old-New System	1,454	10.7
Armed Forces and New System	396	2.9
Armed Forces and Old system	2	.0
Armed Forces and Old-New System	80	.6
Informal Workers	54	.4
Total	13,631	100.0

Note: The distribution of pension system is shown. New System stands for the fully funded pension system. Old System is the PAYG pension system for civilians. Armed Forces is the PAYG pension system for military workers. Informal workers represents all workers who have never contributed to any of the three pension systems.

Table A45: Final Sample, females (SPS-APH-SCOMP)

	Initial Sample (%)	Dis.+Early Retirement (%)	Final Sample (%)
New System	84.6	60.8	86.3
Old System	.8	.0	.0
Armed Forces	.0	.0	.0
Old-New System	10.7	35.9	9.7
Armed Forces and New System	2.9	2.2	3.0
Armed Forces and Old system	.0	.0	.0
Armed Forces and Old-New System	.6	1.1	.6
Informal Workers	.4	.0	.4
Total	13,631	546	12,971

Note: The distribution of pension system is shown. New System stands for the fully funded pension system. Old System is the PAYG pension system for civilians. Armed Forces is the PAYG pension system for military workers. Informal workers represents all workers who have never contributed to any of the three pension systems. To construct the final sample, I remove people from the Old System and workers who claim for disability or early retirement benefits. People who are affiliated with the Armed Forces are the sum of third, fifth, sixth, and seventh rows which represents 3.5%.

Table A46: Groups I-VI, females aged 55-59 (SPS-APH-SCOMP)

Year	Group I Non-contributory	Group II Contributory	Group III Year-threshold	Group IV Income-threshold	Group V Wealthy	Group VI Armed Forces
2008	65.8	3.2	4.5	12.0	8.4	6.1
2010	62.9	3.3	6.2	13.1	7.7	6.8
2012	62.9	6.1	5.5	13.8	6.1	5.6
2014	63.0	8.4	5.7	12.0	6.3	4.5
2016	63.2	9.7	4.8	10.9	6.3	5.2
2018	63.1	10.0	5.5	11.3	6.2	4.0
Total	63.4	7.4	5.4	12.0	6.7	5.1

Note: The distribution of females aged 55-59 into different groups is presented over time. The categorization of each individual depends on the number of contributions and the level of expected pension income.

Different Age Groups

Table A47: Groups I-VI, females aged 25-64 in 2008 (SPS-APH-SCOMP)

Year	Group I Non-contributory	Group II Contributory	Group III Year-threshold	Group IV Income-threshold	Group V Wealthy	Group VI Armed Forces
25-29	97.1	.0	.4	.0	.0	2.5
30-34	90.1	.0	5.7	.4	.0	3.8
35-39	79.6	.3	12.4	3.8	.0	3.9
40-44	72.8	4.6	10.5	8.2	.2	3.6
45-49	69.0	8.0	7.2	10.5	1.3	3.9
50-54	67.0	7.8	6.3	11.9	3.0	4.0
55-59	63.4	7.4	5.4	12.0	6.7	5.2
60-64	64.5	13.6	4.2	6.5	5.4	5.9
Total	77.7	4.4	6.3	6.2	1.6	3.8

Note: The distribution of females aged 25-64 into different groups is presented over time. The categorization of each individual depends on the number of contributions and the level of expected pension income.

A.6 Attanasio et al. (2011)

In this Appendix, I replicate Attanasio et al. (2011); Otero (2015) to understand the differences in formal labor supply results due to the SP. First, I construct the sub-sample composed only for surveyed people on all of each of the SPS rounds (2002, 2004, 2006, and 2009). People need to be older than 20 and younger than 65, not receiving an old-age pension. Table A48 shows the descriptive statistics for the sub-sample generated for 2009. The left section shows the official number published in their papers, while the right depicts the number obtained in my exercise. The number of individuals is very close comparing the left and right sections, but I have fewer males and more females under my sample than Attanasio et al. (2011). There are no critical differences in the averages for age, formality, per capita household income, and education. The sub-sample is highly educated, the rate of people who finished college is 21% and 28% for males and females, respectively. Taking simple averages for each SPS round, this rate moves between 14% and 17% for both genders. Building the sample only with people who were available for each wave tends to over-represent more educated and wealthy people in the sub-sample.

Table A48: Descriptive Statistics: SPS 2009

	Paper		Replication	
Treatment	Men	Women	Men	Women
Obs.	4,793	3,994	4,584	4,024
Age	45	44	45	44
Formality	0.54	0.39	0.59	0.42
Percapita HH income	CLP 114,559	CLP 103,427	CLP 112,309	101,799
Finished Primary	0.41	0.36	0.43	0.45
Finished Secondary	0.21	0.24	0.27	0.31
Finished College	0.21	0.28	0.18	0.24

Note: Left column shows the descriptive statistics for selected variables (Otero, 2015). The right column depicts the averages that come from the replication process.

Using data between 2002 and 2009, I estimate the system of equations where labor force participation, formality, and wage in formal and informal sectors are the dependent variables. The independent variables are age, age squared, educational dummies, married status, and the number of children. Note that pension wealth is not included as a regressor in the system of equations. Having the estimates for each control variable, I predict the dependent variables from 2010 to retirement ages for each sample person. By doing that,

I can compute the expected pension wealth at period t (E_tPW_{iR}) using the previous pension wealth balance, additional contributions to the system, and government transfers (ex and post-reform). Moreover, the expected accrual rate at period t (E_tAR_{iR}) is constructed by estimating the marginal increase in the pension wealth once each agent decides to work in the current period.

To analyze the approach's performance, I use the information between 2010 and 2017 for the same sub-sample. Then, I compare the prediction based on the method with the actual data. On average, labor force participation, formality, and wages in formal and informal sectors are pretty good. Lack of persistence in the system of equations impedes to mimic high wages in both sectors. An extended model with lags of dependent variables as controls improves model proficiency, but the main results do not change drastically.

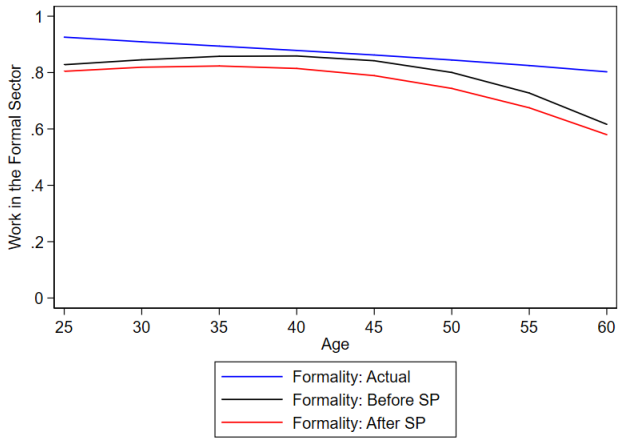
Finally, I run the following model:

$$Y_{it}^{*} = X_{it}\gamma + \beta E_tPW_{iR} + \delta E_tAR_{iR} + \tau_t + \alpha_i + \epsilon_{it}, \qquad \text{(A.1)}$$

where Y_{it}^{*} if the individual i works in the formal sector at t. X_{it} is a vector of demographic variables, τ_t is a time fixed effect and α_i is an individual fixed effect, and ϵ_{it} is the error term. The goal is to find β and δ. They represent the effect of expected pension wealth and expected accrual rate in formal labor supply. As the authors stated, a possible endogeneity could be present due to a correlation between expected pension wealth, expected accrual rate, and the error term. That is why an instrumental variables estimation is performed where age cohorts and gender are the instruments. The last step is to predict formal labor supply before and after the creation of the SP. By doing so, I can calculate the impact of the reform on formality for several populations.

Figure A.30 shows the profiles for males using data between 2010 and 2017. As can be seen, both predicted profiles pre- and post-reform tend to underestimate the actual formal labor supply. Moreover, the profile pre-Solidarity Pillar is always above the profile post the reform. The intuition is straightforward: higher government benefits reduce labor supply through an income effect. Although the differences between predicted profiles and actual ones are small, they seem larger for people close to retirement ages.

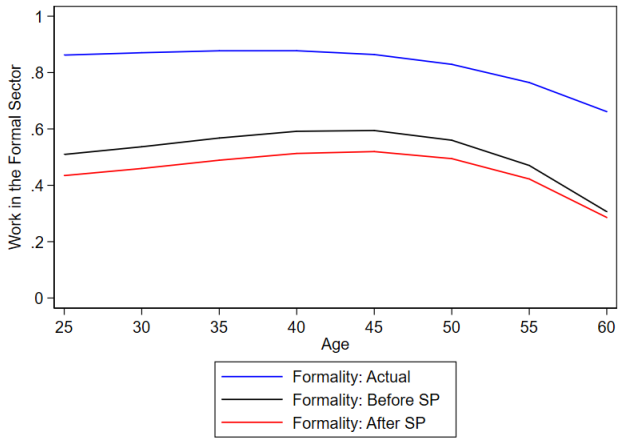
Figure A.30: Formal Labor Supply, males aged 25-64 in 2010-2017



Note: Actual, predicted pre-reform and predicted post reform formal labor supply for males age 25-64 using SPS-HPA data between 2010 and 2017.

I repeat the same analysis for females, and Figure A.31 presents the results. As for males, females' predicted profiles understate the actual profile. However, now the gap is much larger. Indeed, it moves between 40% and 50%, depending on the age group. The lack of prediction power might be explained because a typical Mincer's equation tends to fail when female participation is studied.

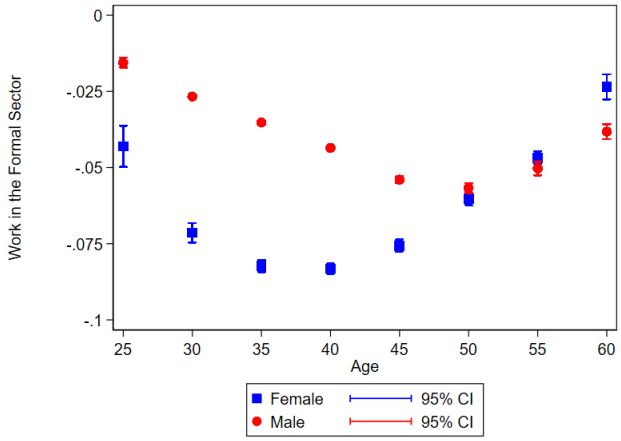
Figure A.31: Formal Labor Supply, females aged 25-64 in 2010-2017



Note: Actual, predicted pre-reform and predicted post reform formal labor supply for females age 25-64 using SPS-HPA data between 2010 and 2017.

Despite underestimating predictions, I estimate the average impact of the Solidarity Pillar for formal labor supply. The purpose is to compare my results with those enunciated in Otero (2015). Figure A.32 shows the change in formality due to the reform for females and males. The results show that the SP decreases formal labor supply for females and males, but the impact's size depends on gender and age. For females aged 35-45, the fall in formality is around 8%. On the other hand, males see a negative impact between 1.5% and 5.5%. Male workers aged 55-64 reduce their formal labor supply by 2.8% while women aged 55-64 decreased by approximately 3.5%. Those results are very similar to Otero (2015) (around 3% for women and men between 56 and 65 years old).

Figure A.32: Formal Labor Supply Impacts due to SP, females and males



Note: Formal labor supply impacts due to the SP, females and males.

Then, to verify the validity of the results, I test whether people decide that their labor supply is not correct. It means analyzing Eq. (A.1). One approach to check if people make decisions according to the paper is running the following model:

$$Y^*_{i,2010} = X_{i,2010}\bar{\gamma} + \bar{\beta}PW_{iR} + \bar{\delta}AR_{iR} + \bar{\epsilon}_i, \tag{A.2}$$

where PW_{iR} and AR_{iR} are the actual pension wealth and accrual rate at retirement ages. Then, I estimate each of those variables twice: once under the previous scheme and once after the reform. Hence, the only difference among them is government benefits. I assume people know those values precisely, and they decide their formal labor supply considering them. To give the approach a better chance, I run Eq. (A.2) in 2010 (one year ahead). People decide whether to work in the formal sector, knowing their actual pension wealth under both schemes. By doing that, the gap between both scenarios will just be related to

the policy reform. As may be expected, this regression is estimated only for people who retire between 2010 and 2017, because of data limitation. I find no significant effects on formal labor supply for females and males due to the policy. The estimate points are practically zero, and the confidence intervals include it.

A.7 Wong (2016)

The author analyzes two groups of people:

- Group A: People with less than 20 years of contribution and pension savings below x_2. The 2008 pension reform would generate a jump in their implicit marginal tax rate on pension contributions from 0 percent to 29.4 percent. This group corresponds to Group III in my analysis.
- Group B: Workers who have more than 20 years of pension contribution and who have pension amounts that are lower than the minimum pension guarantee level. They face a drop in their implicit marginal tax rate on their pension contribution from 100 percent to 29.4 percent. This group corresponds to Group II in my analysis.

The control group is defined as people who would reasonably expect that the 2008 pension reform would not affect their implicit marginal tax rate – that it would be 0 percent both before and after the 2008 reform. Table A49 shows the descriptive statistics of the final sample. The variable Poverty Score (FPS) is the actual variable to determine eligibility. SPS asked for that question, and people self-reported in 2009. Unfortunately, only 16% of people answered that question, so the author predicts the poverty score for the rest of the sample.

Table A49: Descriptive Statistics, replication

	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	N	mean	sd	min	p50	max
Age	14,463	49.90	15.23	19	48	108
Male	14,463	0.489	0.500	0	0	1
No Secondary School Diploma	14,463	0.468	0.499	0	0	1
Secondary School Graduate	14,463	0.246	0.431	0	0	1
Affiliate with Personal Account	14,463	0.786	0.410	0	1	1
Poverty Score (FPS)	2,286	6,742	3,783	1,000	6,500	30,000
Heard About Pension Reform	14,463	0.246	0.431	0	0	1
Work (Year: 2006)	14,463	0.601	0.490	0	1	1
Formal Sector (Year: 2006)	8,691	0.706	0.456	0	1	1
Work (Year: 2009)	14,463	0.573	0.495	0	1	1
Formal Sector (Year: 2009)	8,286	0.690	0.462	0	1	1
Monthly Net Income (pesos)	8,668	278,978	383,620	0	200,000	1.500e+07
Monthly Expenditure (pesos)	14,337	200,079	156,480	0	170,000	3.500e+06

Note: Descriptive Statistics of main sample following Wong (2016). It is constructed with household heads in SPS 2006, 2009.

The regression is shown in Equation (A.3)

$$FPS\ score_i = \alpha + \beta \cdot Age_i + \gamma \cdot Wages_i + \delta \cdot Consumption_i + \eta \cdot Health_i + \zeta \cdot Education_i + \xi \cdot Pension\ Account\ Balance_i + \nu_i, \tag{A.3}$$

where Age_i, $Wages_i$, $Consumption_i$, $Health_i$, $Education_i$, and $Pension\ Account\ Balance_i$ represent the age, wages, consumption, health, education and pension account balance for the household head. Once the model is run using 2,286 people, the estimates predict the FPS for the rest of the sample. Having the predicted FPS, the author creates three alternatives definitions of treatment group:

1) Person belongs to the bottom 60% of income distribution (1 or 0)

2) Person belongs to the bottom 40% of income distribution (1 or 0)

3) Continuous variable defined in terms of the percentile ranks

The intuition is that the 2008 reform increases the coverage from 40% to 60% in terms of total income. The first definition captures eligible people after the reform while some of them could have qualified before. The second will reflect impacts for people who were and are eligible before and after the reform. The final option tries to capture the same but under a continuous variable. Notice that the higher the third variable, the lower the probability of being in the treated group.

The results for Group A are shown in Table A50. The first column shows the estimates found in Wong (2016). Using all three definitions of treatment, the result is that group A (Group III in my analysis) decreases the formal labor supply by around 3.5%. The second column depicts the value of the replication process. As can be seen, the impacts on formal labor supply and the number of observations are very comparable between the first two columns. Column 3 shows the estimates when comparing people in Group A to people in Group V. Notice that Group A corresponds to Group III in my analysis, and people in Group V have a high level of pension income. Therefore they have never been eligible for benefits. By doing that, I find adverse impacts on formal labor supply by 1.7%. The first three columns use the sample in 2006 and 2009, so no fixed effect by years can be included. Column 4 repeats the analysis done in the previous column but incorporates monthly data from January 2006 to December 2009. Under this specification, no significant results are found.

Table A50: Prob of working in the formal sector, Group A, Males (SPS-HPA)

Treatment	Wong (2016) 2006, 2009	Replication 2006, 2009	Replication 2006, 2009	Replication 2006-2009
Bottom 60%	-.029***	-.032***		
Bottom 40%	-.034***	-.054***		
Percentile Rank	.115***	.05***		
Group A vs Group IV			-.017***	0.001
Year Fixed Effect	No	No	No	Yes
Month Fixed Effect	No	No	No	Yes
Observations	5,198	5,962	6,716	21,581

Note: *** p<0.01, ** p<0.05, * p<0.1. Marginal impacts on formal labor supply for Group A. Notice that Group A corresponds to Group III in my analysis. First column shows the results in Wong's paper while the rest come from a replication process. Column two uses the same approach, while column three changes the control group. Column four changes the control group and includes monthly data from January 2006 to December 2009. The first three rows represent the alternatives treatment definitions in Wong (2016). The fourth row shows the comparison between Group A and Group V.

The results for Group B are shown in Table A51. Notice that Group B corresponds to Group II in my analysis. Wong (2016) only shows results for the first treatment definition. According to the results in column 1, Group B increases its formal labor supply by 8%. In column 2, I find a positive impact of 3.3%. By using the second and third treatment definition, I find no significant and positive effects. Note that a negative estimate in the third treatment definition can be understood as positively impacting the outcome among treated. While the ranking is higher (i.e., more wealthy people), the more significant adverse effects, on average. Column 3 shows no significant impacts on formal labor supply when comparing Group B and Group V. Column 4 repeats the analysis done in the previous column but incorporates monthly data from January 2006 to December 2009. Under this specification, I find adverse impacts on formal labor supply by 0.1%. This finding is consistent with using more extensive data and a more clear control group.

Table A51: Prob of working in the formal sector, Group B, Males (SPS-HPA)

Treatment	Wong (2016) 2006, 2009	Replication 2006, 2009	Replication 2006, 2009	Replication 2006-2009
Bottom 60%	.081***	.033*		
Bottom 40%		.010		
Percentile Rank		-.0005***		
Group B vs Group V			.00001	- .001**
Year Fixed Effect	No	No	No	Yes
Month Fixed Effect	No	No	No	Yes
Observations	7,796	2,909	3,103	20,721

Note: *** p<0.01, ** p<0.05, * p<0.1. Marginal impacts on formal labor supply for Group B. Notice that Group B corresponds to Group II in my analysis. First column shows the results in Wong's paper while the rest come from a replication process. Column two uses the same approach, while column three changes the control group. Column four changes the control group and includes monthly data from January 2006 to December 2009. The first three rows represent the alternatives treatment definitions in Wong (2016). The fourth row shows the comparison between Group B and Group V.

A.8 Model: Alternatives Setup

A.8.1 Informal Labor Supply and Formality

So far, people can only receive labor income in the second period by working in the formal sector. Now I allow agents to work either in the formal or informal sector but not both at the same time. I assume that there is no complementarity between the labor supply in both sectors and therefore individuals choose the option with the highest utility. This decision depends on marginal costs and benefits. In terms of marginal cost, the new utility function is:

$$u(c, l^f, l^i) = \ln(c) - \alpha \cdot \frac{(l^f)^{1+\frac{1}{\eta}}}{1+\frac{1}{\eta}} - \alpha \cdot \rho^i \cdot \frac{(l^i)^{1+\frac{1}{\eta}}}{1+\frac{1}{\eta}},$$

where l^i is the informal labor supply and ρ^i is a parameter that measures the disutility of working in the informal sector relative to the formal sector. If $\rho^i > 1$, one hour in the informal sector generates a higher disutility. To make a fair comparison with the results found above, I assume that individuals continue working in the first period $l_1^f = \bar{l}$ in the formal sector. As I showed earlier, the labor income in the first period does not change the conclusions. In terms of benefits, the after tax informal labor income in the second period is:

$$\text{After tax informal labor income} = w_2^i \cdot l_2^i \cdot (1-\tau),$$

where w_2^i is the informal wage in the second period. Informal workers pay taxes but not contribute to the pension system. Upon solving the model, the pre-reform and perfect foresight informal labor supplies are:[73]

$$l_2^{i,pre} = \underbrace{\frac{2 \cdot (1+\beta+\beta^2)}{\Pi_1^{i,pre} + \sqrt{\left(\Pi_1^{i,pre}\right)^2 + \frac{4 \cdot (1+\beta+\beta^2) \cdot \left(A_{pre}^i\right)^2}{\alpha \cdot \beta \cdot \rho^i}}}}_{\text{income and substitution effects}} \cdot \underbrace{\frac{A_{pre}^i}{\alpha \cdot \beta \cdot \rho^i}}_{\text{substitution effect}}, \tag{A.4}$$

$$l_2^{i,pf} = \underbrace{\frac{2 \cdot (1+\beta+\beta^2)}{\Pi_1^{i,pf} + \sqrt{\left(\Pi_1^{i,pf}\right)^2 + \frac{4 \cdot (1+\beta+\beta^2) \cdot \left(A_{pf}^i\right)^2}{\alpha \cdot \beta \cdot \rho^i}}}}_{\text{income and substitution effects}} \cdot \underbrace{\frac{A_{pf}^i}{\alpha \cdot \beta \cdot \rho^i}}_{\text{substitution effect}}, \tag{A.5}$$

[73] If the utility does not have income effect, the informal labor supply is $l_2^{i,pre} = l_2^{i,pf} = \left(\frac{w_2^i \cdot (1-\tau)}{\alpha \cdot \beta \cdot \rho^i \cdot (1+r)}\right)^{\eta}$ with no changes after the reform.

where $A^i_{pre} = A^i_{pf} = \frac{w^i_2 \cdot (1-\tau)}{1+r}$, it implies the substitution effect is the same in both cases. Moreover,

$$\Pi_1^{i,pre} \equiv a_0 \cdot (1+r) + w_1^f \cdot \bar{l} \cdot (1-\tau-\theta) + \frac{b^{pasis}}{(1+r)^2},$$

$$\Pi_1^{i,pf} \equiv a_0 \cdot (1+r) + w_1^f \cdot \bar{l} \cdot (1-\tau-\theta) + \frac{b^{pbs} + (1+\phi_2)\cdot(1+\phi_1)\cdot(B_0 + w_1^f \cdot \bar{l} \cdot \theta)\cdot\left(1-\frac{b^{pbs}}{PMAS}\right)}{(1+r)^2},$$

where $\Pi_1^{i,pre} < \Pi_1^{i,pf}$, thus the income effect is larger on perfect foresight. In sum, informal labor supply may decrease after the reform.

I conclude that when analyzing utility function with no income effect, people in Group I increase the formal labor supply and they do not change the informal labor supply because of the reform. Furthermore, when including income effect in the utility function, the formal labor supply can go up or down but the informal labor supply always decrease due to the reform. To understand how the 2008 reform impacts the fraction of formal workers, I analyze the indifferent worker pre- and post-reform. Imagine the indifferent agent who obtains the same utility pre-reform working formally or informally. Comparing the marginal cost and benefit, the indifferent worker is such that:

$$\begin{aligned} \frac{w^i_2 \cdot (1-\tau)}{\rho^i} &= w^f_2 \cdot (1-\tau-\theta), \\ \bar{w}^i_2 &\equiv w^f_2 \cdot \rho^i \cdot \frac{(1-\tau-\theta)}{(1-\tau)}, \end{aligned} \tag{A.6}$$

where $\bar{w}^i_2$ is the informal wage such that any wage above it, people work in the informal sector. The threshold is increasing in w^f_2, ρ^i and decreasing in θ. For instance, if the disutility of working in the informal sector is high, the informal wage must be large enough to compensate for it. Repeating the same procedure post-reform, I obtain:

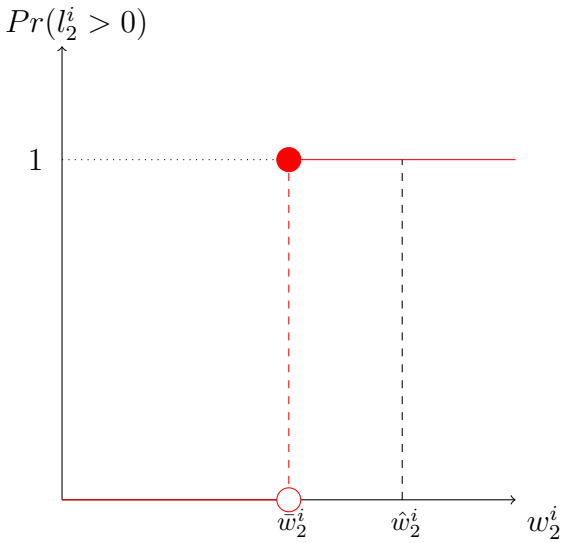
$$\begin{aligned} \frac{w^i_2 \cdot (1-\tau)}{\rho^i} &= w^f_2 \cdot (1-\tau-\theta) + \frac{(1+\phi_2)\cdot\theta\cdot w^f_2 \cdot \left(1-\frac{b^{pbs}}{PMAS}\right)}{1+r}, \\ \hat{w}^i_2 &\equiv w^f_2 \cdot \rho^i \cdot \frac{(1-\tau-\theta)}{(1-\tau)} + \frac{\rho^i \cdot (1+\phi_2)\cdot\theta\cdot w^f_2 \cdot \left(1-\frac{b^{pbs}}{PMAS}\right)}{(1+r)\cdot(1-\tau)}, \end{aligned} \tag{A.7}$$

where the second term on the right-hand side corresponds to the additional pension income that comes from the contribution in the second period. Since the accrual rate in post-reform is different from zero, any additional contribution to the system in period 2, increases pension income. Figure A.33 shows the results. Comparing, I can conclude that $\bar{w}^i_2 < \hat{w}^i_2$. This implies that the

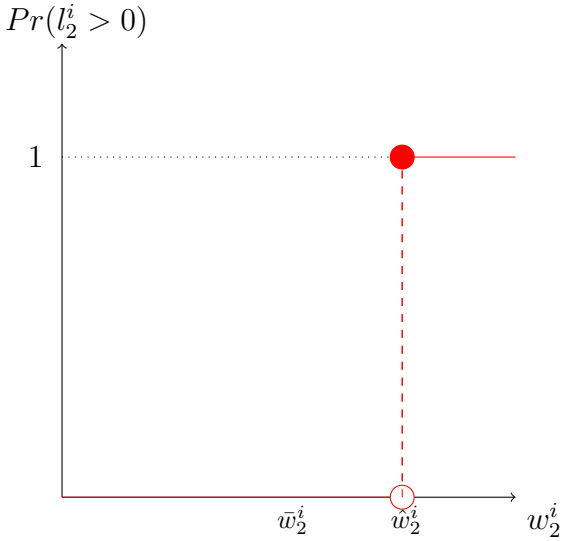
indifferent worker has to receive a higher informal wage post-reform to move to the informal sector. Assuming that the distribution of wages either does not change or the growth is the same for both sectors, the fraction of formal workers may increase.

Figure A.33: Probability of working in the informal sector

(a) Pre-reform



(b) Post-reform



Note: This figure shows the relationship between the probability of working in the informal sector and the informal wage (w_2^i). Panel a) shows the cutoff $(\bar{w}_2^i)$ on pre-reform. Panel b) depicts the threshold on post-reform $(\hat{w}_2^i)$ which is higher than the previous one.

A.8.2 Borrowing-Constrained Agents

In the case of borrowing constraint, $a_1, a_2 \geq 0$. Thus, the problem on pre-reform scenario is:

$$
\begin{aligned}
\underset{c_1,c_2,c_3,a_1,a_2,l_2^f}{\text{maximize}} \quad & u(c_1, \bar{l}) + \beta \cdot u(c_2, l_2^f) + \beta^2 \cdot u(c_3, 0) \\
\text{subject to} \quad & c_1 + a_1 = \underbrace{a_0 \cdot (1+r),}_{\text{non-pension assets income}} + \underbrace{w_1^f \cdot \bar{l} \cdot (1-\tau-\theta),}_{\text{after tax labor income}} \\
& c_2 + a_2 = \underbrace{a_1 \cdot (1+r)}_{\text{non-pension assets income}} + \underbrace{w_2^f \cdot l_2^f \cdot (1-\tau-\theta),}_{\text{after tax labor income}} \\
& c_3 = \underbrace{a_2 \cdot (1+r)}_{\text{non-pension assets income}} + \underbrace{b}_{\text{pension income}} + \underbrace{\left(b^{pasis} - b\right),}_{\text{Gov. transfers}} \\
& b = (1+\phi_2)\left[(1+\phi_1) \cdot \left(B_0 + \cdot w_1^f \cdot \bar{l} \cdot \theta\right) + w_2^f \cdot l_2^f \cdot \theta\right], \\
& a_0, B_0, \bar{l} \text{ given}, \\
& c_1, c_2, c_3, a_1, a_2 \geq 0, \\
& l_2^f \text{ unrestricted}.
\end{aligned}
$$

Replacing c_1, c_2, and c_3 in the utility function, I have

$$
\begin{aligned}
\underset{a_1,a_2,l_2^f}{\text{maximize}} \quad & u\left(a_0 \cdot (1+r) + w_1^f \cdot \bar{l} \cdot (1-\tau-\theta) - a_1, \bar{l}\right) \\
& + \beta \cdot u\left(a_1 \cdot (1+r) + w_2^f \cdot l_2^f \cdot (1-\tau-\theta) - a_2, l_2^f\right) \\
& + \beta^2 \cdot u\left(a_2 \cdot (1+r) + b + \left(b^{pasis} - b\right), 0\right) \\
\text{subject to} \quad & b = (1+\phi_2)\left[(1+\phi_1) \cdot \left(B_0 + \cdot w_1^f \cdot \bar{l} \cdot \theta\right) + w_2^f \cdot l_2^f \cdot \theta\right], \\
& a_0, B_0, \bar{l} \text{ given}, \\
& a_1, a_2 \geq 0, \\
& l_2^f \text{ unrestricted}.
\end{aligned}
$$

Utility with no income effect

Agents choose optimally to borrow in both periods but because of the borrowing constraint ($a_1^* = a_2^* = 0$). Thus, under this scenario, the formal labor supply pre- and post-reform are:

$$l_2^{f,pre} = \left(\frac{w_2^f \cdot (1 - \tau - \theta)}{\alpha} \right)^{\eta}, \tag{A.8}$$

$$l_2^{f,pf} = \left(\underbrace{\frac{w_2^f \cdot (1 - \tau - \theta)}{\alpha}}_{\text{income through labor market}} + \underbrace{\frac{\beta \cdot w_2^f \cdot \theta \cdot (1 + \phi_2) \cdot \left(1 - \frac{b^{pbs}}{PMAS}\right)}{\alpha}}_{\text{income through pension system}} \right)^{\eta}. \tag{A.9}$$

The formal labor supply pre- and post-reform under borrowing constraint are similar to Eq. (1.9) and Eq. (1.10). Given the lack of income effect, I conclude that $l_2^{f,pf} > l_2^{f,pre}$. Thus, formal labor supply should increase after the reform.

Utility with income effect

When including income effect and assuming $\eta = 1$, the pre-reform formal labor supply is

$$l_2^{f,pre} = \left(\frac{1}{\alpha} \right)^{\frac{1}{2}}, \tag{A.10}$$

which depends only on α. The larger the disutility of working, the smaller the formal labor supply. Notice that formal labor supply in this case does not depend on pension supplement b^{pasis}. The post-reform labor supply solves

$$a \cdot \left(l_2^{f,pf} \right)^3 + b \cdot \left(l_2^{f,pf} \right)^2 - d = 0.$$

This expression is a cubic equation of the form $a \cdot x^3 + b \cdot x^2 + c \cdot x + d = 0$ where

$$a = \alpha \cdot (1 + \phi_2) \cdot w_2^f \cdot \theta \cdot \left(1 - \frac{b^{pbs}}{PMAS}\right),$$

$$b = \alpha \cdot \Pi^{f,pf} - (1 - \beta) \cdot w_2^f \cdot \theta \cdot \left(1 - \frac{b^{pbs}}{PMAS}\right),$$

$$c = 0, d = -\Pi^{f,pf},$$

where

$$\Pi^{f,pf} = b^{pbs} + (1 + \phi_1) \cdot (1 + \phi_2) \cdot (B_0 + w_1^f \cdot \theta \cdot \bar{l}) \cdot \left(1 - \frac{b^{pbs}}{PMAS}\right).$$

The solution is given by:

$$l_2^{f,pf} = \sqrt[3]{\left(\frac{-b^3}{27a^3} + \frac{bc}{6a^2} - \frac{d}{2a}\right) + \sqrt{\left(\frac{-b^3}{27a^3} + \frac{bc}{6a^2} - \frac{d}{2a}\right)^2 + \left(\frac{c}{3a} - \frac{b^2}{9a^2}\right)^3}} + \sqrt[3]{\left(\frac{-b^3}{27a^3} + \frac{bc}{6a^2} - \frac{d}{2a}\right) - \sqrt{\left(\frac{-b^3}{27a^3} + \frac{bc}{6a^2} - \frac{d}{2a}\right)^2 + \left(\frac{c}{3a} - \frac{b^2}{9a^2}\right)^3}} - \frac{b}{3a}, \tag{A.11}$$

which is decreasing on initial pension wealth B_0 and pension supplement b^{pbs}. Thus, as in the main text, the formal labor supply can go up or down because of the reform. For low level of pension wealth, the income effect might not overcompensate the substitution effect.

Informal Labor Supply

If the utility does not have income effect, the pre- and post-reform labor supply is given by:

$$l_2^{i,pre} = l_2^{i,pf} = \left(\frac{w_2^i \cdot (1-\tau)}{\alpha \cdot \rho^i}\right)^{\eta}, \tag{A.12}$$

where I can conclude that there is no change in informal labor supply because the reform. When using the utility with income effect and $\eta = 1$, I get

$$l_2^{f,pre} = l_2^{i,pf} = \left(\frac{1}{\alpha \cdot \rho^i}\right)^{\frac{1}{2}}, \tag{A.13}$$

which depends only on α and ρ^i. In sum, with borrowing constraint, there is no change in informal labor supply due to the reform.

Fraction of Formal Workers

In the case of having no income effect, the intuition is direct: the fraction of formal worker should increase because Eq. (A.6) and Eq. (A.7) hold. It implies that the indifferent worker must face a higher informal wage to move to the informal sector. In the case of having income effect, the fraction of formal workers can increase or decrease. Since the formal labor supply can go up or down, the informal labor supply remains unchanged.

A.8.3 Pre-reform versus MIT shock

One could believe some workers do not expect the reform at all. In $t = 1$ the agent solves the same problem as the pre-form case. After savings a_1^{pre} and consuming c_1^{pre}, workers move to period $t = 2$. In the beginning of the period, government announces the change in the transfers' scheme. Table A52 shows the comparison between pre-reform and MIT shock.

Table A52: Rationality Assumption: Pre-reform and MIT shock

	Period	Pre-Reform	MIT Shock
Group I	$t = 1$	$b^{pasis} - b$	$b^{pasis} - b$
	$t = 2$	$b^{pasis} - b$	$b^{pbs}\left(1 - \frac{b}{PMAS}\right)$
	$t = 3$	$b^{pasis} - b$	$b^{pbs}\left(1 - \frac{b}{PMAS}\right)$

Note: In the pre-reform case, people believe that the old scheme stays forever. In the MIT shock case, workers realize about the reform in $t = 2$ taking the decisions made in $t = 1$ as given.

Individuals re-optimize the discounted utility solving the following problem;

$$
\begin{aligned}
\underset{c_2,c_3,a_2,l_2^f}{\text{maximize}} \quad & u(c_2, l^f) + \beta \cdot u(c_3, 0) \\
\text{subject to} \quad & c_2 + a_2 = a_1^{pre} \cdot (1 + r) + w_2^f \cdot l_2^f \cdot (1 - \tau - \theta), \\
& c_3 = \underbrace{a_2 \cdot (1 + r)}_{\text{non-pension assets income}} + \underbrace{b}_{\text{pension income}} + \underbrace{b^{pbs}\left(1 - \frac{b}{PMAS}\right)}_{\text{Gov. transfer}}, \\
& b = (1 + \phi_2)\left[(1 + \phi_1)\left(B_0 + \cdot w_1^f \cdot \bar{l} \cdot \theta\right) + w_2^f \cdot l_2^f \cdot \theta\right], \\
& a_1^{pre}, B_0, \bar{l} \text{ given}, \\
& c_2, c_3 \geq 0, \\
& a_2, l_2^f \text{ unrestricted}.
\end{aligned}
$$

This optimization problem is almost identical to the one solved on perfect foresight with the difference that the level of savings at the beginning of period two a_1^{pre} is a function of initial assets a_0, and the expected government transfers b^{pasis}. This fact modifies the individual's total wealth and therefore labor supply decision. Utility with no income effect

The post-reform (MIT shock) formal labor supply is:

$$l_2^{f,mit} = \left(\underbrace{\frac{w_2^f \cdot (1-\tau-\theta)}{\alpha}}_{\text{income through labor market}} + \underbrace{\frac{w_2^f \cdot \theta \cdot (1+\phi_2) \cdot \left(1 - \frac{b^{pbs}}{PMAS}\right)}{\alpha \cdot (1+r)}}_{\text{income through pension system}} \right)^{\eta}, \tag{A.14}$$

which is similar to the labor supply on perfect foresight case in Eq. (1.10). Comparing to the formal labor supply on pre-reform in Eq. (1.9), I can conclude that if $\beta \cdot (1+r) \geq 1$ implies that $l_2^{f,mit} > l_2^{f,pre}$. On the other hand, if $\beta \cdot (1+r) < 1$, the discount factor (β) has to be low enough to mitigate the additional marginal income through the pension system.

Utility with income effect

The post-reform (MIT shock) formal labor supply is:

$$l_2^{f,mit} = \underbrace{\frac{2 \cdot (1+\beta)}{\Pi_1^{f,mit} + \sqrt{\left(\Pi_1^{f,mit}\right)^2 + \frac{4 \cdot (1+\beta) \cdot \left(A_{mit}^f\right)^2}{\alpha}}}_{\text{income and substitution effect}} \cdot \underbrace{\frac{A_{mit}^f}{\alpha}}_{\text{substitution effect}}, \tag{A.15}$$

where $A_{mit}^f \equiv w_2^f \cdot (1-\tau-\theta) + \frac{w_2^f \cdot \theta \cdot (1+\phi_2) \cdot \left(1 - \frac{b^{pbs}}{PMAS}\right)}{(1+r)}$ and $\Pi_1^{f,mit} \equiv a_0 \cdot (1+r) + w_1^f \cdot \bar{l} \cdot (1-\tau-\theta) - c_1^{pre} + \frac{b^{pbs} + (1+\phi_2) \cdot (1+\phi_1)(B_0 + w_1^f \cdot \bar{l} \cdot \theta)\left(1 - \frac{b^{pbs}}{PMAS}\right)}{(1+r)}$.

The substitution effect is larger in MIT shock than in pre-reform. The income effect is larger (more negative) in MIT shock. These two elements are also present in the case of perfect foresight. Nevertheless, in the MIT shock case, we have an additional element. It is the wealth effect that comes from the term c_1^{pre} in $\Pi_1^{f,mit}$.[74] In simple words, in MIT shock scenario, the individual at the moment of re-optimizing has a lower level of total wealth, specifically non-pension wealth. The larger the consumption in the first period, the lower the non-pension wealth in the second period and the lower the total wealth. In sum, the negative impacts of income effect can be compensated by the positive impacts on the substitution effect and wealth effect bringing a higher likelihood of finding positive impacts on formal labor supply after the reform. The net effect on labor supply depends on B_0, ϕ_1, ϕ_2 and $\bar{l}$ as before.

[74] There is an additional difference that comes from having β dividing A_{mit}^f in both the substitution effect and the combined substitution-income effect of Eq. (A.15). If β is close enough to one, the impact of c_1^{pre} drives the results.

Informal Labor Supply

If the utility does not have income effect, the pre- and post-reform labor supply is given by:

$$l_2^{i,pre} = l_2^{i,mit} = \left(\frac{w_2^i \cdot (1-\tau)}{\alpha \cdot \rho^i} \right)^{\eta}, \tag{A.16}$$

where I can conclude that there is no change in informal labor supply because the reform. When using the utility with income effect and $\eta = 1$, I get

$$l_2^{i,mit} = \underbrace{\frac{2 \cdot (1+\beta)}{\Pi_1^{i,mit} + \sqrt{\left(\Pi_1^{i,pf}\right)^2 + \frac{4 \cdot (1+\beta) \cdot \left(A_{mit}^i\right)^2}{\alpha \cdot \rho^i}}}}_{\text{income and substitution effects}} \cdot \underbrace{\frac{A_{mit}^i}{\alpha \cdot \rho^i}}_{\text{substitution effect}}, \tag{A.17}$$

where $A_{pre}^i = A_{mit}^i = \frac{w_2^i \cdot (1-\tau)}{1+r}$, it implies the substitution effect is the same in both cases. Moreover, $\Pi_1^{i,mit} \equiv a_0 \cdot (1+r) + w_1^f \cdot \bar{l} \cdot (1-\tau-\theta) + \frac{b^{pbs} + (1+\phi_2) \cdot (1+\phi_1) \cdot (B_0 + w_1^f \cdot \bar{l} \cdot \theta) \cdot \left(1 - \frac{b^{pbs}}{PMAS}\right)}{(1+r)^2}$, where $\Pi_1^{i,pre} < \Pi_1^{i,mit}$, thus the income effect is larger in MIT case. Finally, the wealth effect due to the consumption in period 1, is smaller in MIT shock case. In sum, informal labor supply can increase or decrease after the reform. The lower B_0, $\bar{l}$ and the larger c_1^{pre} the more probable to have an increase in the informal labor supply.

Fraction of Formal Workers

In this case because of the wealth effect the fraction of formal workers can increase or decrease in both cases with and without income effect. The effect will depend on B_0, $\bar{l}$, and c_1^{pre}.

A.8.4 Alternative Utility Functions

1) Utility with constant Frisch Elasticity; $\eta = \frac{1}{2}$

Assume that the utility is the following:

$$u(c, l^f) = \ln(c) - \alpha \cdot \frac{(l^f)^{1+\frac{1}{\eta}}}{1+\frac{1}{\eta}},$$

where l^f is labor supply and η is the Frisch elasticity. To obtain analytical expressions, I need to assume either $\eta = 1$ (main text) or $\eta = \frac{1}{2}$.

The first order condition with respect labor supply is:

$$\alpha \cdot \beta \cdot (l_2^f)^2 = \frac{1}{c_1 \cdot A_{pre}^f},$$

$$l_2^f = \frac{1}{\sqrt{\alpha \cdot \beta \cdot c_1 \cdot A_{pre}^f}}, \tag{A.18}$$

using the positive root. Replacing this expression in the budget constraint, I get

$$c_1(1+\beta+\beta^2) = \Pi_1^{f,pre} + \frac{1}{A_{pre}^f \sqrt{\alpha \cdot \beta \cdot c_1 \cdot A_{pre}^f}},$$

$$c_1\sqrt{c_1}(1+\beta+\beta^2) = \Pi_1^{f,pre}\sqrt{c_1} + \frac{1}{A_{pre}^f \sqrt{\alpha \cdot \beta \cdot A_{pre}^f}},$$

defining $x \equiv \sqrt{c_1}$ and therefore $x^2 \equiv c_1$, it yields

$$(1+\beta+\beta^2) \cdot x^3 = \Pi_1^{f,pre} \cdot x + \frac{1}{A_{pre}^f \sqrt{\alpha \cdot \beta \cdot A_{pre}^f}},$$

$$(1+\beta+\beta^2) \cdot x^3 - \Pi_1^{f,pre} \cdot x - \frac{1}{A_{pre}^f \sqrt{\alpha \cdot \beta \cdot A_{pre}^f}} = 0,$$

this expression is a cubic equation of the form $a \cdot x^3 + b \cdot x^2 + c \cdot x + d = 0$ where $a = (1+\beta+\beta^2)$, $b = 0$, $c = -\Pi_1^{f,pre}$, and $d = -\frac{1}{A_{pre}^f \sqrt{\alpha \cdot \beta \cdot A_{pre}^f}}$. The

solution is given by:

$$x = \sqrt[3]{\left(\frac{-b^3}{27a^3} + \frac{bc}{6a^2} - \frac{d}{2a}\right) + \sqrt{\left(\frac{-b^3}{27a^3} + \frac{bc}{6a^2} - \frac{d}{2a}\right)^2 + \left(\frac{c}{3a} - \frac{b^2}{9a^2}\right)^3}}$$
$$+ \sqrt[3]{\left(\frac{-b^3}{27a^3} + \frac{bc}{6a^2} - \frac{d}{2a}\right) - \sqrt{\left(\frac{-b^3}{27a^3} + \frac{bc}{6a^2} - \frac{d}{2a}\right)^2 + \left(\frac{c}{3a} - \frac{b^2}{9a^2}\right)^3}} - \frac{b}{3a}. \tag{A.19}$$

Using Eq. (A.19), I find x, and later $x^2 = c_1$. Having c_1, I can find labor supply by using Eq. (A.18) which is decreasing on initial pension wealth B_0 and pension supplement b^{pbs}. Thus, as in the main text, the formal labor supply can go up or down because of the reform. For low level of pension wealth, the income effect might not overcompensate the substitution effect.

2) Constant Relative Risk Aversion

Assume that the utility is the following:

$$u(c, l^f) = (1-\alpha) \cdot \frac{c^{1-\sigma}}{1-\sigma} + \alpha \cdot \frac{(1-l^f)^{1-\eta\sigma}}{1-\eta\sigma}$$

where σ is the risk aversion and η is parameter. I assume $\eta = 1$, thus the formal labor supply on pre-reform and perfect foresight are:

$$l_2^{f,pre} = 1 - \left(\frac{\alpha\beta}{(1-\alpha)A_{pre}^f}\right)^{\frac{1}{\sigma}} \frac{\left[a_0(1+r) + w_1^f\bar{l}(1-\tau-\theta) + \frac{w_2^f(1-\tau-\theta)}{1+r} + \frac{b^{pasis}}{(1+r)^2}\right]}{1 + \frac{[\beta(1+r)]^{\frac{1}{\sigma}}}{1+r} + \frac{[\beta(1+r)]^{\frac{2}{\sigma}}}{(1+r)^2} + \left[\frac{\alpha\beta}{1-\alpha}\right]^{\frac{1}{\sigma}}\left[A_{pre}^f\right]^{\frac{\sigma-1}{\sigma}}}, \tag{A.20}$$

$$l_2^{f,pf} = 1 - \left(\frac{\alpha\beta}{(1-\alpha)A_{pf}^f}\right)^{\frac{1}{\sigma}} \cdot \Lambda. \tag{A.21}$$

where $\Lambda \equiv \frac{\left[a_0(1+r) + w_1^f\bar{l}(1-\tau-\theta) + \frac{w_2^f(1-\tau-\theta)}{1+r} + \frac{b^{pbs} + (1+\phi_2)\left[(1+\phi_1)(B_0 + w_1^f\theta\,\bar{l}) + \theta w_2^f\right]\left(1 - \frac{b^{pbs}}{PMAS}\right)}{(1+r)^2}\right]}{1 + \frac{[\beta(1+r)]^{\frac{1}{\sigma}}}{1+r} + \frac{[\beta(1+r)]^{\frac{2}{\sigma}}}{(1+r)^2} + \left[\frac{\alpha\beta}{1-\alpha}\right]^{\frac{1}{\sigma}}\left[A_{pf}^f\right]^{\frac{\sigma-1}{\sigma}}}$.

Given that $A_{pre}^f < A_{pf}^f$, the substitution effect is larger under perfect foresight. The income effect is larger on perfect-foresight because $b^{pbs} > b^{pasis}$. Finally, if $\sigma < 1$ the term $\left[A_{pre}^f\right]^{\frac{\sigma-1}{\sigma}}$ increases the combined income and substitution ef-

fect on perfect foresight more than the corresponding one on pre-reform. Thus, the impact of the labor supply depends on parameters.

Informal Labor Supply

Including informal labor supply to the model, the utility can be written as:

$$u(c, l^f) = (1-\alpha) \cdot \frac{c^{1-\sigma}}{1-\sigma} + \alpha \cdot \frac{(1-l^f)^{1-\sigma}}{1-\sigma} + \alpha \cdot \rho^i \cdot \frac{(1-l^i)^{1-\sigma}}{1-\sigma}$$

where l^i and ρ_i are the informal labor supply and the relative disutility of working in the informal sector relative to the formal sector. In the first period, I assume people work in the formal sector to ease the comparison to the previous case. However, in the second period, people can work either in the formal or in the informal sector but not in both. The informal labor supply on pre-reform is:

$$l_2^{i,pre} = 1 - \left(\frac{\alpha\beta\rho^i}{(1-\alpha)A_{pre}^i}\right)^{\frac{1}{\sigma}} \cdot \frac{\left[a_0(1+r) + w_1^f \bar{l}(1-\tau-\theta) + \frac{w_2^i(1-\tau)}{1+r} + \frac{b^{pasis}}{(1+r)^2}\right]}{1 + \frac{[\beta(1+r)]^{\frac{1}{\sigma}}}{1+r} + \frac{[\beta(1+r)]^{\frac{2}{\sigma}}}{(1+r)^2} + \left[\frac{\alpha\beta}{1-\alpha}\right]^{\frac{1}{\sigma}} \left[A_{pre}^i\right]^{\frac{\sigma-1}{\sigma}}}, \tag{A.22}$$

$$l_2^{i,pf} = 1 - \left(\frac{\alpha\beta\rho^i}{(1-\alpha)A_{pf}^i}\right)^{\frac{1}{\sigma}} \cdot \varsigma \tag{A.23}$$

where $\varsigma \equiv \frac{\left[a_0(1+r) + w_1^f \bar{l}(1-\tau-\theta) + \frac{w_2^i(1-\tau)}{1+r} + \frac{b^{pbs} + (1+\phi_2)\theta\left[w_1^f(1+\phi_1)(\epsilon+\bar{l})\right]\left(1-\frac{b^{pbs}}{PMAS}\right)}{(1+r)^2}\right]}{1 + \frac{[\beta(1+r)]^{\frac{1}{\sigma}}}{1+r} + \frac{[\beta(1+r)]^{\frac{2}{\sigma}}}{(1+r)^2} + \left[\frac{\alpha\beta}{1-\alpha}\right]^{\frac{1}{\sigma}} \left[A_{pf}^i\right]^{\frac{\sigma-1}{\sigma}}}$

and $A_{pre}^i = A_{pf}^i = \frac{w_2^i(1-\tau)}{1+r}$. Thus, the substitution effects are identical and because $b^{pasis} < b^{pbs}$, the income effect is larger on perfect foresight. Thus, the informal labor supply should decrease. Finally, evaluating the fraction of formal workers, Eq. (A.6) and Eq. (A.7) keep holding in this situation and therefore, workers must face a higher informal salary to be indifferent between working in the formal or informal sector. This implies that the fraction workers should increase after the reform.

3) Constant Relative Risk Aversion with $\eta \neq 1$

Assume that the utility is the following:

$$u(c, l^f) = (1-\alpha) \cdot \frac{c^{1-\sigma}}{1-\sigma} + \alpha \cdot \frac{(1-l^f)^{1-\eta\sigma}}{1-\eta\sigma}$$

the first order condition with respect labor supply is:

$$\alpha\beta(1-l_2^f)^{-\eta\sigma} = (1-\alpha)c_1^{-\sigma}A_{pre}^f,$$

$$l_2^f = 1 - \left(\frac{\alpha\beta}{(1-\alpha)A_{pre}^f}\right)^{\frac{1}{\eta\sigma}} c_1^{\frac{1}{\eta}}, \tag{A.24}$$

where the value of η determines if the formal labor supply can have analytical results. I can solve for values $\eta = \{\frac{1}{4}, \frac{1}{3}, \frac{1}{2}, 1, 2, 3, 4\}$. Assuming $\eta = 2$, the formal labor supply on pre-reform is:

$$l_2^{f,pre} = 1 - \left(\frac{\alpha\beta}{(1-\alpha)A_{pre}^f}\right)^{\frac{1}{2\sigma}} \cdot \Gamma_{pre}^f \tag{A.25}$$

where

$$\Gamma_{pre}^f = \frac{-\left(\frac{\alpha\beta}{1-\alpha}\right)^{\frac{1}{2\sigma}}\left(A_{pre}^f\right)^{\frac{2\sigma-1}{2\sigma}} + \sqrt{\left(\frac{\alpha\beta}{1-\alpha}\right)^{\frac{1}{\sigma}}\left(A_{pre}^f\right)^{\frac{2\sigma-1}{\sigma}} + 4\left(1+\frac{[\beta(1+r)]^{\frac{1}{\sigma}}}{1+r}+\frac{[\beta(1+r)]^{\frac{2}{\sigma}}}{(1+r)^2}\right)\left(\Pi_1^{f,pre}+\frac{w_2^f(1-\tau-\theta)}{1+r}+\frac{w_2^f\theta(1+\phi_2)\left(1-\frac{b^{pbs}}{PMAS}\right)}{(1+r)^2}\right)}}{2\cdot\left(1+\frac{[\beta(1+r)]^{\frac{1}{\sigma}}}{1+r}+\frac{[\beta(1+r)]^{\frac{2}{\sigma}}}{(1+r)^2}\right)}$$

and $\Pi_1^{f,pre} \equiv a_0(1+r) + w_1^f\bar{l}(1-\tau-\theta) + \frac{b^{pasis}}{(1+r)^2}$. The formal labor formal on perfect foresight

$$l_2^{f,pf} = 1 - \left(\frac{\alpha\beta}{(1-\alpha)A_{pf}^f}\right)^{\frac{1}{2\sigma}} \cdot \Gamma_{pf}^f \tag{A.26}$$

where

$$\Gamma_{pf}^f = \frac{-\left(\frac{\alpha\beta}{1-\alpha}\right)^{\frac{1}{2\sigma}}\left(A_{pf}^f\right)^{\frac{2\sigma-1}{2\sigma}} + \sqrt{\left(\frac{\alpha\beta}{1-\alpha}\right)^{\frac{1}{\sigma}}\left(A_{pf}^f\right)^{\frac{2\sigma-1}{\sigma}} + 4\left(1+\frac{[\beta(1+r)]^{\frac{1}{\sigma}}}{1+r}+\frac{[\beta(1+r)]^{\frac{2}{\sigma}}}{(1+r)^2}\right)\left(\Pi_1^{f,pre}+\frac{w_2^f(1-\tau-\theta)}{1+r}+\frac{w_2^f\theta(1+\phi_2)\left(1-\frac{b^{pbs}}{PMAS}\right)}{(1+r)^2}\right)}}{2\cdot\left(1+\frac{[\beta(1+r)]^{\frac{1}{\sigma}}}{1+r}+\frac{[\beta(1+r)]^{\frac{2}{\sigma}}}{(1+r)^2}\right)}$$

and $\Pi_1^{f,pf} \equiv a_0(1+r) + w_1^f\bar{l}(1-\tau-\theta) + \frac{b^{pbs}}{(1+r)^2}$.

The intuition is similar to the previous utility functions. The income effect is lager on perfect foresight. However, the magnitude is smaller than when I assume $\eta = 1$. The reason is that $\Pi_1^{f,pre}$ and $\Pi_1^{f,pf}$ are inside the square root. Thus, the gap between $b^{pbs} - b^{pasis}$ is reduced at having a concave function. The expressions A_{pre}^f and A_{pf}^f are three times in the labor supply. The substitution effect as I discussed in the base line scenario. The second and third term affects the total wealth, the former reduces it and the latter increases it in a quadratic fashion. Thus, the net effect on labor supply depends on parameters.

Appendix B

Intergenerational Earnings Persistence and the Provision of Public Goods: Evidence from Chile's Constitutional Proces with Javier Cortes-Orihuela, Juan D. Díaz and Pablo Gutiérrez Cubillos

B.1 Proofs

Proposition 4 *G is decreasing with respect to β when $y^p_{min} > 1$.*

Proof. Before voting for G and τ each individual solves the optimization problem given by (2.4). Additionally, note that using equation (2.3), we get:

$$y^c_i + \theta \cdot G = e^{\alpha} e^{\epsilon_i} (y^p_i)^{\beta} + \theta \cdot G. \tag{B.1}$$

Replacing equations (2.1) and (B.1) into (2.2) yields

$$\mathbb{E}[u(G)|y_i^p] = \log\left(1 - \frac{G}{H \cdot \mu}\right) + \log(y_i^p) + B(G) + \xi \int_{-\infty}^{\infty} \log(e^{\alpha} e^{\beta} e^{\epsilon_i} y_i^p + \theta \cdot G) f(\epsilon_i)\, d\epsilon_i, \tag{B.2}$$

where $f(\epsilon_i)$ is the probability density function of ϵ_i. Differentiating with respect to G gives

$$\begin{aligned} \frac{\partial \mathbb{E}[u(G)|y_i^p]}{\partial G} &= -\frac{1}{H \cdot \mu - G} + B'(G) \\ &+ \xi \cdot \theta \int_{-\infty}^{\infty} \frac{1}{e^{\alpha} e^{\epsilon_i} (y_i^p)^{\beta} + \theta \cdot G} f(\epsilon_i)\, d\epsilon_i \equiv F(\beta, G) = 0. \end{aligned} \tag{B.3}$$

The implicit function theorem states that

$$\frac{dG}{d\beta} = -\frac{\dfrac{\partial F}{\partial \beta}}{\dfrac{\partial F}{\partial G}}. \tag{B.4}$$

Direct computation yields

$$\frac{\partial F}{\partial \beta} = -\xi \cdot \theta \int_{-\infty}^{\infty} \frac{e^{\alpha} e^{\epsilon_i} (y_i^p)^{\beta} \log(y_i^p)}{\left(e^{\alpha} e^{\epsilon_i} (y_i^p)^{\beta} + \theta \cdot G\right)^2} f(\epsilon_i)\, d\epsilon_i < 0, \tag{B.5}$$

using the fact that $y_{median} > 1$. This implies that the earnings of the median voter are greater than 1. On the other hand we obtain

$$\frac{\partial F}{\partial G} = -\frac{1}{(H\mu - G)^2} + B''(G) - \xi \cdot \theta^2 \int_{-\infty}^{\infty} \frac{1}{\left(e^{\alpha} e^{\epsilon_i} (y_i^p)^{\beta} + \theta \cdot G\right)^2} f(\epsilon_i)\, d\epsilon_i < 0, \tag{B.6}$$

Hence, replacing equations (B.6) and (B.5) into equation (B.4) yields

$$\frac{dG}{d\beta} < 0. \tag{B.7}$$

Since the optimal value of G_i decreases with y_i^p and given that preferences are single peaked, we can apply the median voter theorem to state that G decreases with β. ■

B.2 Alternatives utility functions

Assume that the new utility function for parents is as follows

$$u(G,\tau) = \log\left[(1-\tau)y_i^p + B(G)\right] \tag{B.8}$$

Replacing equations (2.1) and (B.8) into (2.2) yields

$$\mathbb{E}[u(G)|y_i^p] = \log\left[\left(1-\frac{G}{H\cdot\mu}\right)y_i^p + B(G)\right] + \xi\int_{-\infty}^{\infty}\log(e^{\alpha}e^{\beta}e^{\epsilon_i}y_i^p + \theta\cdot G)f(\epsilon_i)\,d\epsilon_i, \tag{B.9}$$

where $f(\epsilon_i)$ is the probability density function of ϵ_i. Differentiating with respect to G gives

$$\begin{aligned}\frac{\partial\mathbb{E}[u(G)|y_i^p]}{\partial G} = &-\frac{\frac{-y_i^p}{H\cdot\mu} + B'(G)}{\left(1-\frac{G}{H\cdot\mu}\right)\cdot y_i^p + B(G)} + \\ &+\xi\cdot\theta\int_{-\infty}^{\infty}\frac{1}{e^{\alpha}e^{\epsilon_i}(y_i^p)^{\beta}+\theta\cdot G}f(\epsilon_i)\,d\epsilon_i \equiv F(\beta,G) = 0.\end{aligned} \tag{B.10}$$

The implicit function theorem states that

$$\frac{dG}{d\beta} = -\frac{\dfrac{\partial F}{\partial\beta}}{\dfrac{\partial F}{\partial G}}. \tag{B.11}$$

Direct computation yields

$$\frac{\partial F}{\partial\beta} = -\xi\cdot\theta\int_{-\infty}^{\infty}\frac{e^{\alpha}e^{\epsilon_i}(y_i^p)^{\beta}\log(y_i^p)}{\left(e^{\alpha}e^{\epsilon_i}(y_i^p)^{\beta}+\theta\cdot G\right)^2}f(\epsilon_i)\,d\epsilon_i < 0, \tag{B.12}$$

using the fact that $y_{median} > 1$. On the other hand we obtain

$$\begin{aligned}\frac{\partial F}{\partial G} = &\frac{[-y_i^p + B'(G)]^2}{\left[\left(1-\frac{G}{H\cdot\mu}\right)\cdot y_i^p + B(G)\right]^2} + \frac{B''(G)}{\left[\left(1-\frac{G}{H\cdot\mu}\right)\cdot y_i^p + B(G)\right]} \\ &-\xi\cdot\theta^2\int_{-\infty}^{\infty}\frac{1}{\left(e^{\alpha}e^{\epsilon_i}(y_i^p)^{\beta}+\theta\cdot G\right)^2}f(\epsilon_i)\,d\epsilon_i,\end{aligned} \tag{B.13}$$

Since the first element on the right hand side is positive, the second and the third ones are always negative. Thus, if y^p_{median} is large enough $\frac{\partial F}{\partial G}$ will be positive. Let y^p_{i*} be the value that makes Eq. (B.13) be equal to zero. Thus, if the median individual has an income lower than y^p_{i*}, $\frac{\partial F}{\partial G} < 0$ and hence an increase on β will decrease G by using Eq. (B.11).

B.3 Alternative measures of intergenerational mobility

B.3.1 Rank-rank correlation

The rank-rank correlation is a statistical measure of the intergenerational association between the income rank of children and their parents that has gained popularity over the past years (Mazumder 2016; Chetty et al. 2017, 014a). Since rankings on the earnings distribution are determined at earlier ages and are difficult to change throughout the age distribution, it is argued that this indicator is robust to the life-cycle bias (Corak, 2020). This correlation can be estimated through the following OLS regression:

$$r^c_i = \alpha + \beta^r \cdot r^p_i + \epsilon_i, \tag{B.14}$$

where r^c_i is the ranking of i-th child in the national distribution of child earnings, r^p_i is the ranking of i-th child's parent on the national distribution of parental earnings, ϵ_i is the error term, and β^r is the rank-rank correlation, where a small number reflects a society with a high degree of IGM. We compute the rank-rank correlation for each municipality of the country, i.e., we fit equation (B.14) for each municipality of Chile.

B.3.2 Transition probabilities

We complement the previous measures by estimating for each municipality the circle of privilege (p_{55}), defined as the probability that the child will belong to the top quintile given that their parents belong to the top quintile. We formally define it as in Corak (2020):

$$p_{55} = \Pr\{y^c \in Q^c_5 \mid y^p \in Q^p_5\}, \tag{B.15}$$

where y^c represents the earnings of the child, y^p are parental earnings, Q^c_j is the set of childrens' earnings belonging to the jth quintile, and Q^p_j is the set of

parental earnings that belong to the jth quintile. Following Chetty et al. (2014), we interpret p_{55} as a measure of top-persistence. We compute this transition probability for each municipality of Chile.

B.4 Tables

Table B1: Relationship between the IGE and the share of the vote against a new constitution imputing missing earnings as 0

	(1)	(2)	(3)	(4)	(5)	(6)
Variables	Reject	Reject	Reject	Reject	Reject	Reject
IGE	0.759***	0.480***	0.478***	0.467***	0.339***	-1.776***
	(0.057)	(0.035)	(0.036)	(0.034)	(0.034)	(0.162)
Median income		0.379***	0.407***	0.359***	0.217***	0.263***
		(0.005)	(0.006)	(0.008)	(0.006)	(0.009)
Gini		0.157***	0.152***	0.142***	0.165***	-0.531***
		(0.015)	(0.015)	(0.015)	(0.011)	(0.061)
Average age			0.003***	0.011***	0.008***	0.008***
			(0.000)	(0.001)	(0.000)	(0.000)
% Minors			0.086***	0.234***	0.180***	0.165***
			(0.007)	(0.009)	(0.006)	(0.008)
Overcrowding				-0.559***	-0.749***	-1.031***
				(0.085)	(0.069)	(0.068)
% Immigrants				0.622***	0.525***	0.515***
				(0.026)	(0.019)	(0.023)
Unemployment rate				-0.029	0.028	0.088**
				(0.030)	(0.021)	(0.031)
Municipal per capita expenditures					0.200***	0.134***
					(0.006)	(0.010)
Right wing major					-0.005***	-0.007***
					(0.001)	(0.001)
Voter turnout					0.007**	-0.048
					(0.015)	(0.022)
Intercept	0.110***					
	(0.007)					
Province FE	No	Yes	Yes	Yes	Yes	Yes
Observations	231	231	231	231	231	231
Adj. R-squared	0.137	0.781	0.787	0.812	0.847	0.883

Bootstrap standard errors in parentheses.

*** p<0.01, ** p<0.05, * p<0.1

Note: We use the CASEN survey to obtain median income, income inequality, average age, the average number of minors within a household, and the unemployment rate. These statistics are computed using the survey weights provided by the Chilean government. Overcrowding and the share of immigrants were obtained through the "Registro Social de Hogares" dataset. Electoral turnout and the right wing major dummy rely on data from the Chilean Electoral Service. Municipal per capita expenditures were obtained from the National System of Municipal Information (SINIM). Municipal per capita expenditures are measured in million Chilean pesos, while median income is measured in thousand Chilean pesos. standard errors are reported in parentheses. Bootstrap percentile confidence intervals are used to determine the significance level. We employ the number of parent-child links in a municipality as weights when estimating both the model's parameters and their respective confidence intervals.

Table B2: Relationship between the age-adjusted IGE correlation and the share of the vote against a new constitution

	(1)	(2)	(3)	(4)	(5)	(6)
Variables	Reject	Reject	Reject	Reject	Reject	Reject
IGE	0.529***	0.298***	0.307***	0.284***	0.216***	-1.294***
	(0.037)	(0.025)	(0.025)	(0.024)	(0.024)	(0.111)
Median income		0.398***	0.429***	0.392***	0.224***	0.249***
		(0.003)	(0.004)	(0.005)	(0.004)	(0.007)
Gini		0.154***	0.144***	0.145***	0.161***	-0.670***
		(0.013)	(0.014)	(0.013)	(0.010)	(0.071)
Average age			0.003***	0.011***	0.008***	0.009***
			(0.000)	(0.000)	(0.000)	(0.000)
% Minors			0.098***	0.240***	0.179***	0.170***
			(0.005)	(0.006)	(0.004)	(0.006)
Overcrowding				-0.410***	-0.626***	-1.033***
				(0.057)	(0.049)	(0.057)
% Immigrants				0.590***	0.480***	0.540***
				(0.017)	(0.015)	(0.019)
Unemployment rate				-0.046***	0.022	0.103***
				(0.021)	(0.016)	(0.023)
Municipal per capita expenditures					0.222***	0.165***
					(0.004)	(0.008)
Right wing major					-0.002**	-0.006***
					(0.001)	(0.001)
Voter turnout					-0.002	-0.047
					(0.014)	(0.021)
Intercept	0.095***					
	(0.007)					
Province FE	No	Yes	Yes	Yes	Yes	Yes
Observations	231	231	231	231	231	231
Adj. R-squared	0.118	0.764	0.772	0.795	0.840	0.868

Bootstrap standard errors in parentheses.

*** p<0.01, ** p<0.05, * p<0.1

Note: We use the CASEN survey to obtain median income, income inequality, average age, the average number of minors within a household, and the unemployment rate. These statistics are computed using the survey weights provided by the Chilean government. Overcrowding and the share of immigrants were obtained through the "Registro Social de Hogares" dataset. Electoral turnout and the right wing major dummy rely on data from the Chilean Electoral Service. Municipal per capita expenditures were obtained from the National System of Municipal Information (SINIM). Municipal per capita expenditures are measured in million Chilean pesos, while median income is measured in thousand Chilean pesos. standard errors are reported in parentheses. Bootstrap percentile confidence intervals are used to determine the significance level. We employ the number of parent-child links in a municipality as weights when estimating both the model's parameters and their respective confidence intervals.

Table B3: Relationship between the rank-rank correlation and the share of the vote against a new constitution

Variables	(1) Reject	(2) Reject	(3) Reject	(4) Reject	(5) Reject	(6) Reject
Rank-rank	0.655***	0.409***	0.406***	0.384***	0.339***	-1.097***
	(0.029)	(0.027)	(0.027)	(0.025)	(0.026)	(0.091)
Median income		0.334***	0.362***	0.330***	0.191***	0.230***
		(0.008)	(0.009)	(0.010)	(0.008)	(0.008)
Gini		0.071***	0.066***	0.069***	0.106***	-0.552***
		(0.018)	(0.018)	(0.017)	(0.014)	(0.047)
Average age			0.003***	0.011***	0.007***	0.008***
			(0.000)	(0.001)	(0.000)	(0.000)
% Minors			0.084***	0.222***	0.166***	0.149***
			(0.007)	(0.009)	(0.007)	(0.006)
Overcrowding				-0.387***	-0.744***	-1.103***
				(0.079)	(0.073)	(0.053)
% Immigrants				0.570***	0.428***	0.488***
				(0.026)	(0.025)	(0.021)
Unemployment rate				-0.033	0.022	0.102***
				(0.028)	(0.023)	(0.022)
Municipal per capita expenditures					0.197***	0.133***
					(0.006)	(0.008)
Right wing major					-0.006***	-0.008***
					(0.001)	(0.001)
Voter turnout					-0.097**	-0.103***
					(0.022)	(0.021)
Intercept	0.077***					
	(0.006)					
Province FE	No	Yes	Yes	Yes	Yes	Yes
Observations	231	231	231	231	231	231
Adj. R-squared	0.241	0.792	0.797	0.819	0.858	0.886

Bootstrap standard errors in parentheses.

*** p<0.01, ** p<0.05, * p<0.1

Note: We use the CASEN survey to obtain median income, income inequality, average age, the average number of minors within a household, and the unemployment rate. These statistics are computed using the survey weights provided by the Chilean government. Overcrowding and the share of immigrants were obtained through the "Registro Social de Hogares" dataset. Electoral turnout and the right wing major dummy rely on data from the Chilean Electoral Service. Municipal per capita expenditures were obtained from the National System of Municipal Information (SINIM). Municipal per capita expenditures are measured in million Chilean pesos, while median income is measured in thousand Chilean pesos. standard errors are reported in parentheses. Bootstrap percentile confidence intervals are used to determine the significance level. We employ the number of parent-child links in a municipality as weights when estimating both the model's parameters and their respective confidence intervals.

Table B4: Relationship between the circle of privilege transition probability and the share of the vote against a new constitution

Variables	(1) Reject	(2) Reject	(3) Reject	(4) Reject	(5) Reject	(6) Reject
p_{55}	0.393***	0.239***	0.240***	0.218***	0.168***	-1.102***
	(0.015)	(0.017)	(0.017)	(0.017)	(0.018)	(0.072)
Median income		0.316***	0.348***	0.343***	0.189***	0.200***
		(0.007)	(0.008)	(0.007)	(0.005)	(0.006)
Gini		0.141***	0.134***	0.147***	0.166***	-0.869***
		(0.012)	(0.013)	(0.013)	(0.010)	(0.067)
Average age			0.003***	0.010***	0.007***	0.006***
			(0.000)	(0.000)	(0.000)	(0.000)
% Minors			0.094***	0.224***	0.166***	0.127***
			(0.004)	(0.005)	(0.004)	(0.005)
Overcrowding				-0.034*	-0.384***	-0.932***
				(0.054)	(0.040)	(0.051)
% Immigrants				0.518***	0.424***	0.461***
				(0.017)	(0.017)	(0.021)
Unemployment rate				-0.050***	0.018	0.121***
				(0.020)	(0.015)	(0.022)
Municipal per capita expenditures					0.219***	0.146***
					(0.004)	(0.007)
Right wing major					-0.005***	-0.007***
					(0.001)	(0.001)
Voter turnout					-0.014	-0.077**
					(0.015)	(0.026)
Intercept	0.074***					
	(0.005)					
Province FE	No	Yes	Yes	Yes	Yes	Yes
Observations	231	231	231	231	231	231
Adj. R-squared	0.195	0.769	0.776	0.796	0.841	0.883

Bootstrap standard errors in parentheses.
*** p<0.01, ** p<0.05, * p<0.1

Note: We use the CASEN survey to obtain median income, income inequality, average age, the average number of minors within a household, and the unemployment rate. These statistics are computed using the survey weights provided by the Chilean government. Overcrowding and the share of immigrants were obtained through the "Registro Social de Hogares" dataset. Electoral turnout and the right wing major dummy rely on data from the Chilean Electoral Service. Municipal per capita expenditures were obtained from the National System of Municipal Information (SINIM). Municipal per capita expenditures are measured in million Chilean pesos, while median income is measured in thousand Chilean pesos. standard errors are reported in parentheses. Bootstrap percentile confidence intervals are used to determine the significance level. We employ the number of parent-child links in a municipality as weights when estimating both the model's parameters and their respective confidence intervals.

Table B5: Relationship between the IGE and the share of the vote against a new constitution removing outliers

Variables	(1) Reject	(2) Reject	(3) Reject	(4) Reject	(5) Reject	(6) Reject
IGE	0.269***	0.143***	0.143***	0.139***	0.174***	-0.057
	(0.031)	(0.017)	(0.018)	(0.017)	(0.019)	(0.092)
Median income		0.250***	0.248***	0.241***	0.238***	0.235***
		(0.003)	(0.003)	(0.004)	(0.004)	(0.005)
Gini		0.108***	0.107***	0.099***	0.123***	-0.004
		(0.008)	(0.008)	(0.008)	(0.007)	(0.053)
Average age			0.000	0.005***	0.004***	0.004***
			(0.000)	(0.000)	(0.000)	(0.000)
% Minors			-0.002	0.080***	0.076***	0.077***
			(0.002)	(0.003)	(0.004)	(0.004)
Overcrowding				-0.351***	-0.637***	-0.693***
				(0.029)	(0.041)	(0.045)
% Immigrants				0.329***	0.246***	0.264***
				(0.009)	(0.013)	(0.013)
Unemployment rate				0.070***	0.075***	0.081***
				(0.011)	(0.013)	(0.013)
Municipal per capita expenditures					0.032***	0.032***
					(0.004)	(0.003)
Right wing major					-0.005***	-0.005***
					(0.001)	(0.001)
Voter turnout					-0.160***	-0.160***
					(0.012)	(0.012)
Intercept	0.139***					
	(0.006)					
Province FE	No	Yes	Yes	Yes	Yes	Yes
Observations	228	228	228	228	228	228
Adj. R-squared	0.045	0.848	0.846	0.855	0.862	0.862

Bootstrap standard errors in parentheses.

*** p<0.01, ** p<0.05, * p<0.1

Note: We use the CASEN survey to obtain median income, income inequality, average age, the average number of minors within a household, and the unemployment rate. These statistics are computed using the survey weights provided by the Chilean government. Overcrowding and the share of immigrants were obtained through the "Registro Social de Hogares" dataset. Electoral turnout and the right wing major dummy rely on data from the Chilean Electoral Service. Municipal per capita expenditures were obtained from the National System of Municipal Information (SINIM). Municipal per capita expenditures are measured in million Chilean pesos, while median income is measured in thousand Chilean pesos. standard errors are reported in parentheses. Bootstrap percentile confidence intervals are used to determine the significance level. We employ the number of parent-child links in a municipality as weights when estimating both the model's parameters and their respective confidence intervals.

Table B6: Relationship between IGE correlation and the share of the vote against a new constitution. Formal private sector subsample

Variables	(1) Reject	(2) Reject	(3) Reject	(4) Reject	(5) Reject	(6) Reject
IGE	0.535***	0.370***	0.373***	0.355***	0.239***	-0.639***
	(0.033)	(0.026)	(0.026)	(0.025)	(0.023)	(0.137)
Median income		0.349***	0.375***	0.321***	0.158***	0.168***
		(0.003)	(0.004)	(0.006)	(0.004)	(0.006)
Gini		0.026***	0.038***	0.016***	0.105***	-0.467***
		(0.014)	(0.014)	(0.014)	(0.010)	(0.093)
Average age			0.005***	0.014***	0.009***	0.010***
			(0.000)	(0.000)	(0.000)	(0.000)
% Minors			0.108***	0.267***	0.185***	0.178***
			(0.006)	(0.008)	(0.005)	(0.006)
Overcrowding				-0.823***	-0.816***	-1.195***
				(0.072)	(0.054)	(0.080)
% Immigrants				0.687***	0.512***	0.602***
				(0.023)	(0.016)	(0.018)
Unemployment rate				-0.011	0.062***	0.141***
				(0.026)	(0.017)	(0.017)
Municipal per capita expenditures					0.251***	0.207***
					(0.004)	(0.010)
Right wing major					-0.003***	-0.007***
					(0.001)	(0.001)
Voter turnout					0.012***	0.025*
					(0.014)	(0.022)
Intercept	0.092***					
	(0.007)					
Province FE	No	Yes	Yes	Yes	Yes	Yes
Observations	231	231	231	231	231	231
Adj. R-squared	0.131	0.734	0.745	0.775	0.835	0.854

Bootstrap standard errors in parentheses.
*** p<0.01, ** p<0.05, * p<0.1

Note: We use the CASEN survey to obtain median income, income inequality, average age, the average number of minors within a household, and the unemployment rate but restricting the sample to those workers in the formal private sector. These statistics are computed using the survey weights provided by the Chilean government. Overcrowding and the share of immigrants were obtained through the "Registro Social de Hogares" dataset. Electoral turnout and the right wing major dummy rely on data from the Chilean Electoral Service. Municipal per capita expenditures were obtained from the National System of Municipal Information (SINIM). Municipal per capita expenditures are measured in million Chilean pesos, while median income is measured in thousand Chilean pesos. standard errors are reported in parentheses. Bootstrap percentile confidence intervals are used to determine the significance level. We employ the number of parent-child links in a municipality as weights when estimating both the model's parameters and their respective confidence intervals.

Table B7: Relationship between the IGE correlation and the share of the vote against a new constitution: LASSO regression

	(1)
Variable	Reject
IGE	0.080***
	(0.022)
Median income	0.000
	(0.002)
Gini index	0.080***
	(0.016)
% People $\geq$ 18 years of schooling	1.640***
	(0.027)
Poverty rate	0.001***
	(0.000)
% Students in publicly-funded school	-0.026***
	(0.008)
% Public health plan	0.000
	(0.036)
% Monoparental households	0.000
	(0.002)
% Indigenous	0.000
	(0.003)
Average age	0.003***
	(0.001)
% Minors	0.051***
	(0.027)
Overcrowding	-0.188***
	(0.122)
% Immigrants	0.000
	(0.069)
Unemployment rate	0.000
	(0.010)
Per capita expenditures	159.201***
	(9.031)
Right wing major	0.000
	(0.000)
Voter turnout	-0.161***
	(0.016)
Intercept	0.030
	(0.083)
Province FE	Yes
Observations	231
R-squared	0.911

Note: We use the CASEN survey to obtain median income, income inequality, average age, the average number of minors within a household, and the unemployment rate. These statistics are computed using the survey weights provided by the Chilean government. Overcrowding and the share of immigrants were obtained through the "Registro Social de Hogares" dataset. Electoral turnout and the right wing major dummy rely on data from the Chilean Electoral Service. Municipal per capita expenditures were obtained from the National System of Municipal Information (SINIM). Municipal per capita expenditures are measured in million Chilean pesos, while median income is measured in thousand Chilean pesos. standard errors are reported in parentheses. Bootstrap percentile confidence intervals are used to determine the significance level. We employ the number of links in a municipality as weights when estimating both the model's parameters and their respective confidence intervals. We select the shrinkage parameter by cross-validation, leading to a value of 0.0016.

Table B8: Relationship between the IGE and the approval ratings of the president in December 2019.

	(1)	(2)	(3)	(4)	(5)	(6)
Variables	Approval	Approval	Approval	Approval	Approval	Approval
IGE	0.140**	0.075	0.106	0.107	0.189*	-0.286
	(0.052)	(0.074)	(0.076)	(0.075)	(0.085)	(0.386)
Median income		0.157***	0.192***	0.068***	-0.019***	-0.028***
		(0.007)	(0.006)	(0.007)	(0.008)	(0.009)
Gini		-0.090**	-0.115***	-0.196***	-0.197***	-0.449**
		(0.035)	(0.037)	(0.037)	(0.034)	(0.185)
Average age			0.001***	0.008***	0.007***	0.007***
			(0.000)	(0.000)	(0.000)	(0.000)
% Minors			0.095***	0.197***	0.169***	0.165***
			(0.005)	(0.005)	(0.006)	(0.007)
Overcrowding				-2.051***	-2.973***	-3.096***
				(0.035)	(0.115)	(0.127)
% Immigrants				0.549***	0.454***	0.479***
				(0.010)	(0.029)	(0.036)
Unemployment rate				0.344***	0.411***	0.431***
				(0.018)	(0.027)	(0.031)
Municipal per capita expenditures					0.095***	0.095***
					(0.005)	(0.005)
Right wing major					-0.021***	-0.020***
					(0.002)	(0.002)
Voter turnout					-0.251***	-0.256***
					(0.049)	(0.048)
Intercept	0.022**					
	(0.011)					
Province FE	No	Yes	Yes	Yes	Yes	Yes
Observations	109	109	109	109	109	109
Adj. R-squared	-0.002	0.303	0.295	0.308	0.302	0.291

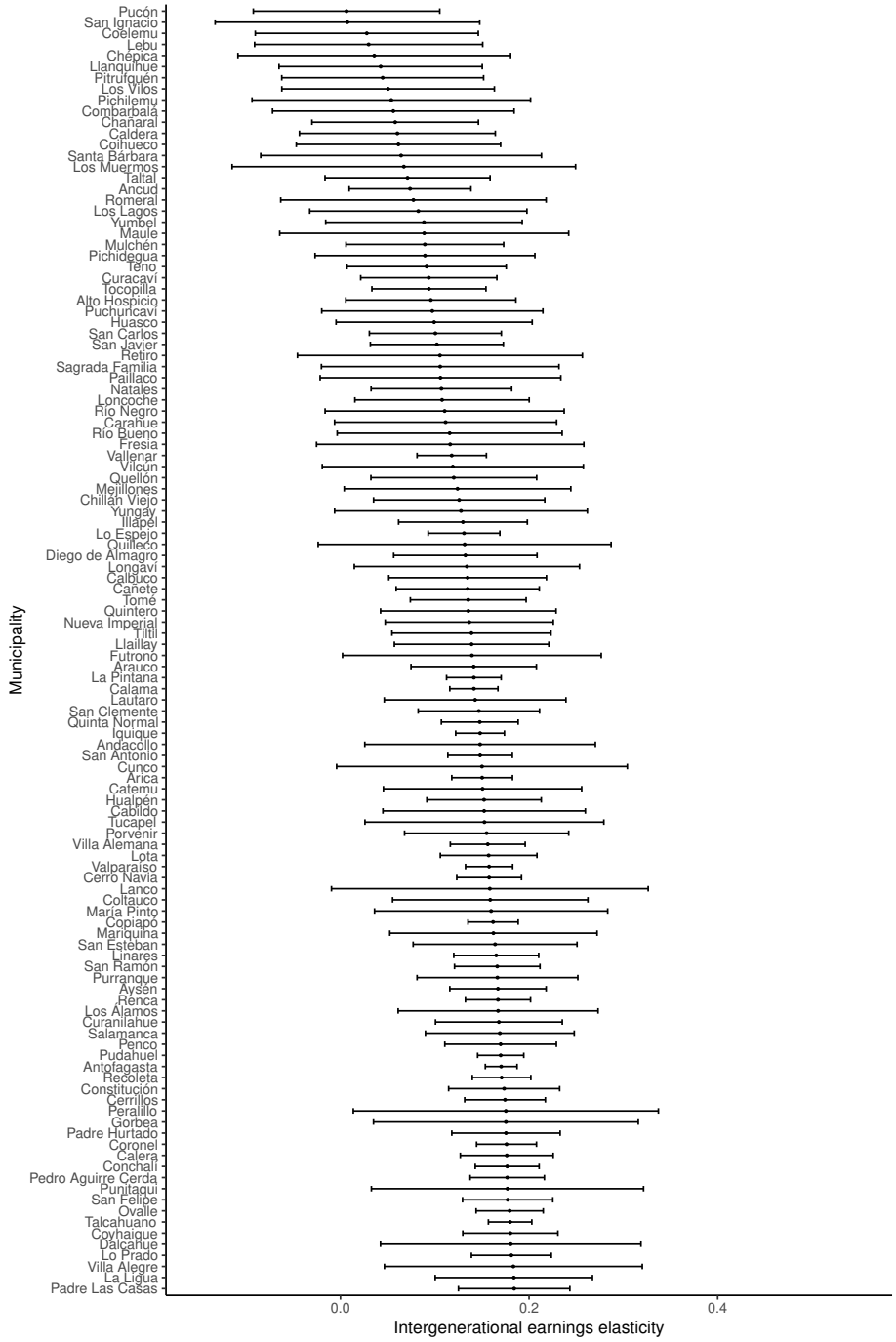
Bootstrap standard errors in parentheses.

*** p<0.01, ** p<0.05, * p<0.1

Note: We use the CASEN survey to obtain median income, income inequality, average age, the average number of minors within a household, and the unemployment rate. These statistics are computed using the survey weights provided by the Chilean government. Electoral turnout and the right wing major dummy rely on data from the Chilean Electoral Service. Municipal per capita expenditures were obtained from the National System of Municipal Information (SINIM). Additional variables were estimated using the "Registro Social de Hogares" dataset. Municipal per capita expenditures are measured in million Chilean pesos, while median income is measured in thousand Chilean pesos. Bootstrap percentile confidence intervals are used to determine the significance level. We employ the number of parent-child links in a municipality as weights when estimating both the model's parameters and their respective confidence intervals.

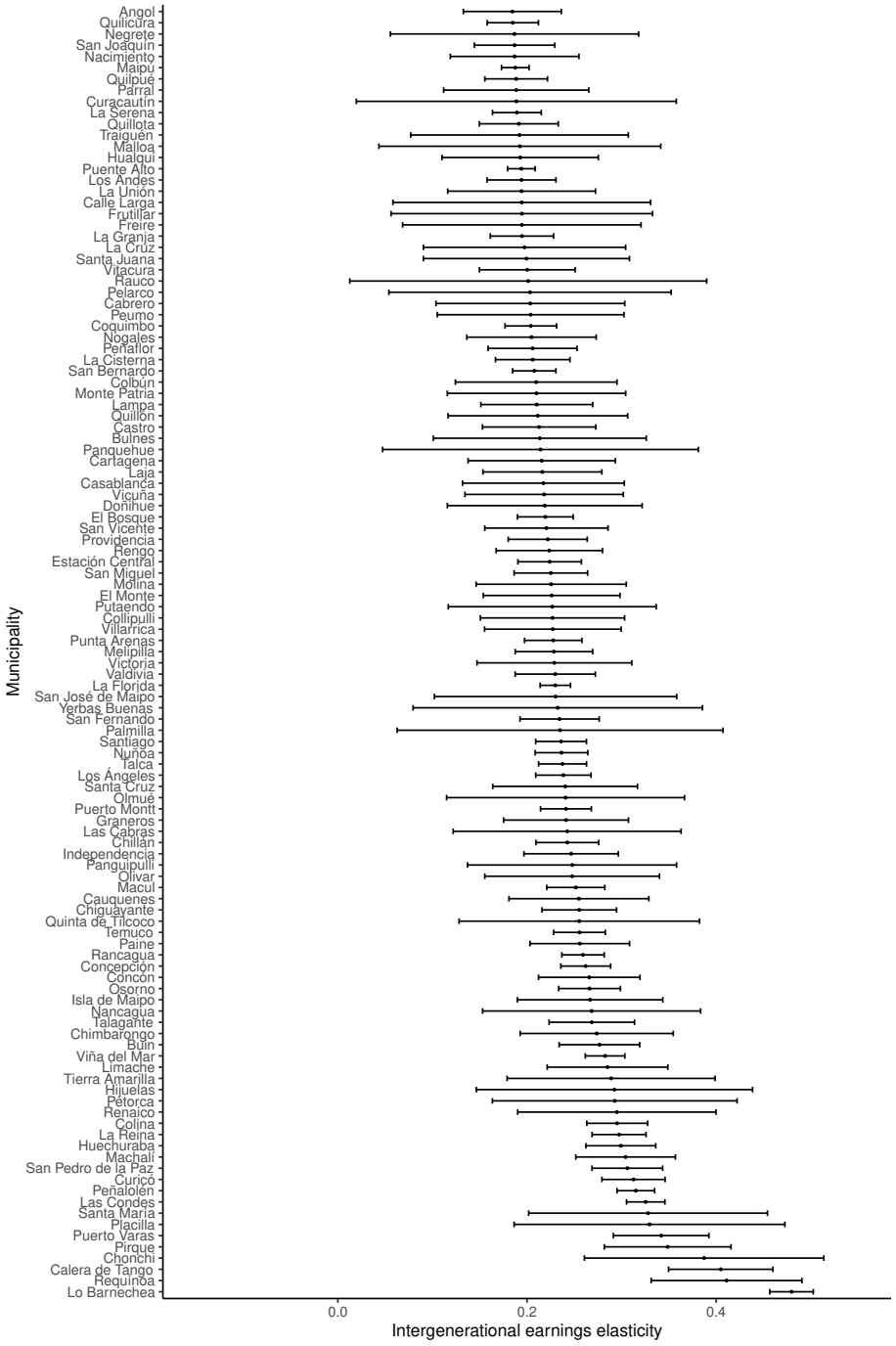
B.5 Figures

Figure B.1: Municipality-level IGE estimates



Note: Points represent point estimates while error bars represent 95% confidence intervals.

Figure B.2: Municipality-level IGE estimates - continued



Note: Points represent point estimates while error bars represent 95% confidence intervals.

Appendix C

How Much Should We Trust TSTSLS Intergenerational Mobility Estimates?: Evidence From A Developing Country with Javier Cortés, Juan Díaz, Pablo Gutiérrez , Gabriel I. Villarroel

C.1 Figures and Tables

Table C1: Representativity of the unemployment insurance program dataset

Year	Total UIPD	W ENE	Coverage W	LF ENE	Coverage LF
2003	1,349.5	3,672.7	36.7%	5,119.1	26.3%
2004	1,849.5	3,806.3	48.6%	5,286.1	34.9%
2005	2,337.8	3,987.4	58.6%	5,438.7	43.0%
2006	2,701.3	4,166.4	64.8%	5,442.2	49.6%
2007	3,103.1	4,360.3	71.1%	5,555.5	55.8%
2008	3,309.2	4,583.5	72.2%	5,762.4	57.4%
2009	3,419.8	4,500.1	76.0%	5,839.9	58.6%
2010	3,742.4	4,908.1	76.2%	6,210.1	60.2%
2011	4,050.4	5,146.7	78.7%	6,448.8	62.8%
2012	4,286.4	5,360.2	80.0%	6,520.0	65.7%

Note: This dataset is compared with the information of the ENE (Encuesta Nacional de Empleo) questionnaire administred by the goverment statistics agency in Chile (INE-Instituto Nacional de Estadisticas). W ENE refers to the total number of formal employees recorded by ENE and LF ENE is the total labour force (formal and informal) recorded by ENE. The information regarding ENE numbers come from Sehnbruch et al. (2015). Units are measured on thousands