Intergenerational spillovers of Collective Bargaining: a case study for Uruguay

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Abstract

In Uruguay, collective bargaining defines the formal income for the largest part of the workforce. The effects of collective bargaining on employment, income inequality and mobility have been for a long time a central topic of study in the economics literature. However, little is known on the spillovers for the next generation. This study aims to shed light on the intergenerational effects of collective bargaining for Uruguay in the last 20 years exploiting variations in wages induced by the second round of the *Consejos de Salarios*, the country's institution for centralized three-way wage negotiation. Using a treatment at the sector-level we find descriptive evidence that children with parents who work in sectors with strong collective bargaining units occupy a higher place in the income distribution, are more likely to enroll in university and inherit a parent's employer and - though with a weaker effect - more likely to anticipate their entry to the labor market. We try to identify the causal impact of collective bargaining on intergenerational mobility using a treatment at the individual level and controlling for parent sector. We find some positive noisy but non-robust results in the same direction for the place children occupy in the income distribution and entry to the labor market, but these shift when observing variables such as educational decisions and inheritance of employers.

Keywords: Collective Bargaining, Intergenerational Mobility, Spillovers, Uruguay, Minimum Wages

JEL Classification: C13, C21, E24, J52, J62

1 Introduction

The relationship between labor institutions and workers' outcomes has been broadly studied in the economics literature (Blau and Kahn, 1999, Nickell and Layard, 1999). One of these institutions is collective bargaining, the scheme under which workers, companies and other actors negotiate wages, working conditions and many other aspects of the formal employment relation. While the literature has focused on the effects that collective bargaining has had on workers, their spillovers[2], in particular to the next generation, still remain an underexplored question.

In this work we propose to study the intergenerational spillovers of the *Consejos de Salarios*[3], Uruguay's centralized wage setting institution that covers the majority of the formal workforce. This institution has existed under different forms since the first half of the twentieth century, and it remains to this day the biggest labor market institution in the country. Since its reestablishment in 2005, workers, firms and the government engage in these negotiations and set agreements for 24 different activity groups in the private sector. Negotiations take place, on average, every two years. From 2005 to the present day, 10 rounds have been held, one roughly every two years (Instituto Cuesta Duarte, 2025).

Collective bargaining has been a broadly studied topic in the labor economics literature. Its effects on employment, wages, the income distribution and ties to unionization have been studied both in developed and developing countries (Jäger et al., 2024, Fanfani, 2023, Bhuller et al., 2022, Carrasco et al., 2022, Brum and Perazzo, 2020, Perazzo, 2012, Bucheli and Amarante, 2011, Plasman et al., 2007, Holden, 1998, Card, 1996). In many shapes, this institution is present across broadly different labor markets

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[2] Understood here as the unanticipated effect of these policies.

[3] *Consejos de Salarios* and Wage Councils will be treated as interchangeable expressions.

around the world. While its coverage has been on the decline in decentralized establishment-based schemes, it's still a prominent feature of centralized systems (Jäger et al., 2024). Uruguay is not foreign to this, having a nationwide centralized scheme with universal coverage since 2007 (Casacuberta and Gandelman, 2023).

On the other hand, intergenerational mobility has long been a subject of interest in the economics literature, studied from a variety of approaches over several decades (Solon, 1999, Black and Devereux, 2011). Topics vary from health status all the way to earnings. Theoretical models all the way from Becker and Tomes (1979) to Solon (2004) have focused on the transmission channels of mobility. This last one aims to develop a theoretical framework to interpret the channels that could be operating behind the empirical results being produced around the turn of the millennia.

The empirical evidence has put its focus on quantifying the persistence of these outcomes. One of the main studies of this literature is Corak and Heisz (1999) which aims to quantify the intergenerational income association for Canada around the turn of the millennia, finding the magnitude of this association to be around 0.2. Since then, Corak (2013) and Chetty et al. (2014, 2017) among others, have continued the development of this research agenda, with recent trends showing a decline in intergenerational mobility for developed countries. For Latin America Behrman et al. (2001) provides one of the first comparisons for Intergenerational Mobility versus the United States using survey data. Their results show how mobility rates are significantly lower in Latin America, with barriers to opportunities such as borrowing constraints, discrimination and spatial segregation being at play. More recent studies show how opportunities of upward social mobility have not changed much over time and that there's still a high persistence in the inheritance of inequality throughout the continent (Neidhöfer et al., 2022, Brunori et al., 2023).

Delving into the transmission channels, while education seems to be the most studied one (Bingley et al., 2009, Pekkarinen et al., 2009, Ichino et al., 2010), there are many others covered by this research. For example Arrondel (2009) has focused on the transmission of values - such as attitudes towards risk and time - from one generation to the other. Over time, even some debates have risen about the importance of some transmission channels. For instance while Bowles and Gintis (2002) originally argue that the genetic transmission of cognitive skills - measured by the correlation in IQ scores - is relatively unimportant, Anger and Heineck (2009), Anders et al. (2010) and Adda et al. (2011) have given more importance to the correlations of these abilities and the potential impact that they have in labor market outcomes.

Intergenerational mobility is a more recent subject in the Uruguayan economics literature with the works from Sanromán (2010), González and Sanromán (2010), Araya (2018), Leites et al. (2022a) and Leites et al. (2022b) leading this research agenda. Despite the recent growth of this literature in Uruguay and the new evidence documenting the decline in income inequality over recent decades (De Rosa and Vilá, 2020, Carrasco, 2025) that can be partly attributed to the role played by wage councils (Carrasco et al., 2022), there remains little to no evidence on the potential spillover effects of collective bargaining on intergenerational mobility. This leads to the question on whether collective bargaining had an effect on the offspring of the workers affected by it and which are the possible transmission channels - found in models such as Solon (2004) - that come into play.

This is important since parents could allocate the income increases generated by collective bargaining into investments in their children's human capital - since it reduces the constraint under which these families operate. These new investments could modify their decisions like delaying their entry to the labor market and accessing higher paying jobs. However, wage bargaining could also affect the overall returns to education (in the case that workers serially invest more in their children's education) by increasing the supply of skilled workers. In Latin America, Acosta et al. (2019) show how the wage premia for workers having completed secondary education rose in the nineties but, as inequality fell in the 2000's and 2010's this was followed by a decrease in the returns to education. Studies such as Yapor García (2018) and Amarante et al. (2025) have focused solely on Uruguay and looked at the evolution of the returns to education in the period where the new scheme of collective bargaining has come back into play. Taking as a reference group workers with complete primary school Yapor García (2018) concludes that over the period 2005-2010 - with a increasing minimum wages and progressive tax reforms - returns to education have decreased for Uruguay. Using administrative data, Amarante et al. (2025) study the returns to university education in Uruguay for the period 1997-2022 and they show how the university-wage premia has grown until 2012 and remained stable ever since.

A second way in which collective bargaining could influence intergenerational outcomes is through the changes it makes in the labor market institutions. The agreements from this new scheme could make it more costly for firms to turn over jobs and tighten up the labor market. In this case, workers that were inside the formal labor market could be serially benefited in detriment of those who are not part of it, reinforcing the insider-outsider logic as presented in Lindbeck and Snower (2002). Looking into the next generation, children in families with workers inside the labor market could benefit from this in many ways. For example, they could benefit of the social network a parent creates inside the workplace when it's their time to enter the labor market, making it easier to find a job or even more, a better paying job from earlier on in their lives.

In addition, we could consider how this institution could affect the transmission of preferences for work. Parents positively affected by collective bargaining could value more their occupation and workplace, and would thus put more effort into passing on their abilities or values to their children, having repercussions on the decisions these make in terms of career paths and their human capital accumulation.

Our study aims to be a first approach to make a bridge in between the intergenerational mobility and collective bargaining literature using rich administrative data from Uruguay's social security records. We link formal workers to their collective bargaining agreement[4] and use the 2006 negotiation round for workers born between 1950 and 1966 and their children born between 1988 and 1996. We leverage these administrative records to study income, educational and (formal) labor market outcomes for children by combining the most common measures of intergenerational mobility found in the literature (Corak and Heisz, 1999, Corak and Piraino, 2011, Leites et al., 2022b, Araya, 2018) with methods from the minimum wages and collective bargaining literature (Dustmann et al., 2021, Carrasco et al., 2022).

We propose to study this on two fronts. First, to give a general assessment on the topic, we want to see if there's descriptive evidence linking parents who are in strong collective bargaining units with children occupying higher places in the income distribution. For this, we take a sector level approach where we link parents to strong sectors, defined as those who negotiate salary increases above the evolution of the country's average salary index in the spirit that strong unions tend to reduce wage inequality by setting higher salaries (Brändle, 2024)[5]. We find evidence that indicates that children of parents who belong to

[4] Available in the *Ministerio de Trabajo y Seguridad Social* public records and processed by the Labor Economics Group at the IECON-UDELAR.

[5] Albeit this effect being stronger in countries with weaker collective bargaining institutions.

a strong sector are more likely to occupy a higher place in the income distribution, enroll in university and work at a firm where their parent has worked. Although noisy, we also find evidence that these children enter the labor market earlier with respect to those whose parents do not work in these sectors. Articulating this with our previous results, we do not see evidence that suggests a trade-off between enrollment in university and entry to the labor market. Though our results are robust, our identification strategy presents some issues that deter us from giving them a causal interpretation and only present this evidence as descriptive.

To address some of the problems found in our previous framework we propose an individual-level approach that draws on the methods of the minimum wage literature found in Dustmann et al. (2021). Taking into account the noncompliance of the agreements by some employers, we propose to see what happens with the sons of workers who, before the adjustment were close to the next sectoral minimum wage and receive the adjustment, and compare them to those who did not get it. Since before the adjustment both groups are expecting to receive it and with the assumption that receiving it or not is random, our setting is similar that of an experiment. Since we'll be focusing on workers near the sectoral minimum wage, our results will only allow us to draw inference on this subpopulation. In this framework we find some positive albeit noisy evidence pointing in the same direction for the impact of collective wage bargaining on the place a child occupies in the income distribution. However, we do find some robust evidence that these children enroll slightly less in higher education and enter the workforce a little earlier, suggesting a tradeoff between the decision of working and studying.

Previous studies like Leites et al. (2022a) have tackled the negative intergenerational effects of a macroeconomic shock in children's outcomes in the Uruguayan context, although in general, there is little known about the causes that drive this issue. In the same fashion Carrasco et al. (2022) among others have studied the earnings and unemployment effects that collective bargaining has had in Uruguay in the recent decades. These studies show the magnitude that this institution has had in reducing inequality from an intragenerational perspective. This study contributes to the literature by exploring how collective bargaining induced wage increases influence intergenerational mobility through mechanisms affecting earnings, educational choices, labor market outcomes, and their interaction with labor market institutions. By doing so, it lays the groundwork for the development and improvement of this research agenda, one that is important to uncover the potential long-term consequences that these policies can have across generations.

The rest of the document is structured as follows. Section 2 reviews the existing literature on the topics of collective bargaining and intergenerational mobility both domestically and internationally. Section 3 provides a brief history on the development of the *Consejos de Salarios*, Uruguay's longstanding collective bargaining institution. Section 4 describes our data sources, variables, and samples to be used. Section 5 presents our identification strategies, their assumptions and limitations. Section 6 presents the main results for both specifications. Section 7 provides a heterogeneity analysis and also delves into the robustness of our main results when cutting the sample by different characteristics. Finally, section 8 concludes.

2 Literature Review

2.1 Intergenerational Income Mobility

2.1.1 Theoretical Background

On the theoretical front, we draw upon the models of intergenerational mobility proposed by Becker and Tomes (1979, 1986) and Solon (2004) which explain the main income transmission channels from one generation to the next one. Becker and Tomes (1979, 1986) start from a family utility function where a parent values their own consumption, the characteristics of their children, and the number of children. This utility function has three arguments and is written as follows:

$$U_t = U_t\left(C_t, \varphi_{t+1}, k\right)$$

φ_{t+1} represents a vector of relevant characteristics of a person's k children. In this model, parents are assumed to be interested only in consumption at time t and in the investment dedicated to all their children.

$$\varphi_{t+1} \cdot k = I_{t+1}$$

In this scheme, I_{t+1} represents the aggregated wealth of a family's children. This wealth can be influenced by parents through investment in both human and non-human capital, through the inheritance passed down to children, and by luck in the labor market. Becker and Tomes (1979, 1986) emphasize the importance of parental decisions to invest in their children's human capital and their expectations regarding their children's luck in the labor market.

The models proposed by Piketty (2000) and Solon (2004) attribute more importance to the exogenous effects and constraints that families face, which may influence their decisions on investing in their children's human capital. Solon (2004) presents a modification to the canonical models of Becker and Tomes (1979), which allows for (1) bridging the gap between theoretical models and the empirical specifications used in the literature, (2) describing the dynamics of intergenerational mobility with respect to parameters of interest such as human capital investment, the inheritance of abilities, and the return rate on education investment, and (3) understanding what factors affect the intergenerational income elasticity.

This starts from a problem analogous to the previous model, where a parent must decide where to allocate their disposable income, having two alternatives: personal consumption $(C_{i,t-1})$ and investment in their child's human capital $(I_{i,t-1})$. In this framework, the human capital accumulated by each child (h_{it}) is a function of the parent's investment, government spending on the child's human capital $(G_{i,t-1})$, and a parameter denoting the natural ability of each person $(e_{i,t})$.

$$h_{i,t} = \theta \log\left(I_{i,t-1} + G_{i,t-1}\right) + e_{i,t}$$

Where $\theta > 0$ represents the marginal product of each unit of investment in human capital.

Solon argues that $e_{i,t}$ is correlated with the parent's ability $(e_{i,t-1})$. It is assumed that $e_{i,t}$ follows an AR(1) process of the form:

$$e_{i,t} = \vartheta + \lambda \cdot e_{i,t-1} + \eta_{i,t}$$

Where λ represents a coefficient of inherited ability from the previous generation, and $\eta_{i,t}$ is the innate ability of the person, assumed to be distributed as white noise. When deciding on the investment $I_{i,t-1}$, the family maximizes a utility similar to the model of Becker and Tomes (1979).

The child's income function, on the other hand, is given by the expression:

$$\log y_{i,t} = \mu + \rho \cdot h_{i,t}$$

Where ρ is the return rate on human capital. Solon argues that parents internalize all these parameters in their utility function[6], and human capital investment is then determined exogenously by the following equation:

$$I^*i,t-1 = \frac{\alpha\theta p}{1-\alpha(1-\theta p)}\, y_{i,t-1} - \left[\frac{1-\alpha}{1-\alpha(1-\theta p)}\right] G_{i,t-1}$$

Solon argues that parents invest in their child's education but also pass through their social capital (e.g, skills, behavior and connections). In this case, a higher parent income translates to better investment opportunities for the son's human capital, which leads to the development of skills valued by the market and thus can translate to a higher income.

This study proposes to examine various components that affect intergenerational income elasticity. The most direct of these is income, y. The reintroduction of wage bargaining agreements significantly raised the income level of the population, with targeted effects on improving the lower end of the distribution Brum and Perazzo (2020), Carrasco et al. (2022). As a result, throughout the theoretical channels shown here, higher income implies a relaxation of the constraints parents face and thus potentially approach them to the optimal level of investment in their child's human capital. Gaviria (2002) shows that these constraints play a role in retarding social mobility through the aforementioned channel.

It also lies the possibility that the agreements affect the parameters that the model takes as exogenous. Take in for example the returns to human capital signaled by ρ. Two forces could be at play here. First, collective bargaining can compress the earnings distribution by significantly increasing wages at the left tail of the distribution. This narrower distribution means that, for children in low income families, the returns to human capital diminish since their starting point is higher than previously. On the other hand, over time, if the wage increases induced by collective bargaining are highly invested into the next generation's human capital, then the supply of more educated workers would increase significantly and firms would not have to pay a wage premia to hire more educated workers, also meaning a decline in ρ. There is also an argument to be held on the transmission of abilities portrayed by λ. Workers inside sectors - and firms - with considerable wage increases could develop stronger preferences for a firm or a sector and be more prone to pass their skills to their children.

An additional feature that is not contemplated in Solon is how labor market institutions can affect the intergenerational transmission of income from channels other than the investment in (or returns to) human capital accumulation or the transmission of abilities.

[6]Following a Cobb-Douglas specification such as: $U_i = (1-\alpha),\log,C_{i,t-1} + \alpha,\log,y_{i,t}$ where α represents the relative preferences between their current consumption $C_{i,t-1}$ and their child's income $y_{i,t}$.

2.1.2 Empirical Literature

There is a vast body of empirical literature on intergenerational income mobility in the global context. In general, there is a positive association between parents' income and their children's economic mobility (Corak and Heisz 1999, Aaronson and Mazumder 2008, Celhay et al. 2009, Corak and Piraino 2011, Corak 2013), with more rigid mobility at the tails of the distribution. Additionally, recent studies reveal how intergenerational mobility has been declining in developed countries since the last decades of the past century (Chetty et al., 2014, 2017, Manduca et al., 2024, Davis and Mazumder, 2022).

The estimations made by Corak and Heisz (1999) provide a good starting point to explore the modern literature. Using administrative data for around 400,000 people in Canada, they find that on average, the intergenerational rankikng association (IRA) is around 0.2. That is, for each percentile the father goes up in the income distribution of his cohort, the sons go up by 0.2. Similar estimates have been found for Latin American countries such as Chile, with data from the *Encuesta de Caracterización Socioeconómica Nacional* for 1996-2006 Celhay et al. (2009) show that 20% of children share the same income decile as the household they grew up in when they become a household head. Non-linearities are also found in this case, finding the most persistent effects in the tails of the distribution (29.1% for the poorest decile and 39.9% for the richest one).

For Uruguay, intergenerational income mobility has been studied in Leites et al. (2022b), Leites et al. (2022a), Gandelman and Robano (2014), Rodríguez Ingold (2022) and Araya (2018). Leites et al. (2022b) use administrative data to estimate the Intergenerational Ranking Association for sons belonging to the 1970-1996 birth cohorts. In line with those estimates obtained by Corak and Heisz (1999) and Celhay et al. (2009), the IRA for parents and sons in Uruguay is around 0.23 for formal sector workers, showing on average a slightly higher persistence level in comparison to Canada or Chile. Additionally, Leites et al. (2022a) exploits the impact of job loss on intergenerational mobility. Exploiting the mass layoffs induced by the 2002 financial crisis they show how this event has a significant negative impact on the next generation's permanent income. Araya also presents estimates for intergenerational social mobility (i.e. the association between parents education and income towards their children's human capital formation). Similar to the results seen for the US, these studies find that mobility has been in decline in Uruguay since the beginning of the 80's until the 2010's.

2.2 Evidence on Collective Bargaining

There is a substantial body of theoretical and empirical literature that investigates the effects and stylized facts of collective bargaining institutions. Collective wage bargaining — particularly under centralized schemes — has been directly linked to lower wage inequality (Jäger et al., 2024). This outcome is often attributed to mechanisms such as upward pressure on wages at the lower end of the income distribution, the stabilization of wage floors, and constraints on firms' ability to rapidly adjust employment. However, despite these insights, there remains a lack of evidence on how collective bargaining affects income mobility.

In the international context, the focus has been on the macroeconomic effects of centralized and decentralized collective bargaining schemes and the role of trade unions in setting wage floors (Calmfors et al., 1988, Card, 1996, Holden, 1998, Cardoso and Portugal, 2005, Plasman et al., 2007). However, these studies focus on the results from developed countries with bipartite bargaining systems, meaning between workers and firms, with regulation but without active participation from government authorities.

Meanwhile, Bhuller et al. (2022) provide a taxonomy of the forms that collective bargaining schemes take in OECD countries, documenting their evolution from 1980 to 2018, the spectrum between centralized and decentralized bargaining, and the effects on wages and employment.

Hermo (2024) represents the most recent study on the effects of collective bargaining on key labor market variables. Additionally, this work focuses on the collective bargaining scheme in Argentina during the period 2009-2013. Using exogenous economic shocks, it finds that an increase equivalent to a 10% rise in firm revenues results in a 1.2% wage increase. When studying performing this analysis with collective bargaining units, an average 10% increase in firm revenues leads to a 4.2% increase in the wage level for firms under the CB agreement. This study also finds that the wage increase is driven by a rise in the wage floor, which is set in collective agreements, suggesting that collective bargaining extends the impact of positive economic shocks from firms to workers who otherwise would not be affected.

Fanfani (2023) studies the employment effects of collective bargaining induced wage growth. Similar to the Uruguayan context, it takes a setting where pay schemes are defined at the sector-national level. The paper exploits the similar indexation of the wages defined under bargaining agreements to the evolution of the national minimum wage to analyze the effect from of wage growths on employment in the 2006-2016 period.

These papers are linked to a vast body of evidence and methods drawn from the minimum wage literature, an institution that plays a key role in wage inequality and the income distribution around the world, with these policies having strong positive effects on workers earnings but limited effects on employment in some contexts (Dube and Lindner, 2024). Studies like that of Lee (1999) propose methods to estimate the impact of the federal minimum wage in the income distribution. His findings state that the erosion of many state minimum wages in the 80's account for 70 to 100% of the increasing inequality at the lower tail of the earnings distribution for women and 70% for men.

More recent studies like Dustmann et al. (2021) delve into the reallocation effects of the introduction of a nationwide minimum wage in Germany around 2015. When taking workers below the minimum wage before its implementation and comparing them to workers who earn significantly more, they find that the policy increased wages for workers at the left of the wage distribution and at a bigger pace than those further to the right. Additionally, this policy did not lead to a decline in the employment prospects of those workers earning below this threshold.

In the national context, the period since the re-establishment of tripartite wage councils has been accompanied by the fall in income inequality, in particular, during the first two decades of this century. Carrasco (2025) shows how, from 2005 there is a noticeable decrease in permanent income inequality joined by a rise in intragenerational income mobility in particular of younger workers. On the other hand, De Rosa and Vilá (2020) articulate evidence from surveys and administrative data to show how inequality has fallen in Uruguay in the 2009-2010 period with the rise in the participation of the bottom 90% in the share of total income and reductions from the top 10% and top 1% (though moderate) share.

In the period 1997-2015, Carrasco (2021) observes differences in the labor trajectories of individuals depending on the time window in which they engage in their work activities. His work distinguishes between two sub-periods: 1997-2004 and 2005-2015, with the first characterized by a higher prevalence of informality, growing inequality, and low wage levels, and the second marked by changes in labor market institutions, economic growth, and the increased relevance of the national minimum wage and collective bargaining. Although this study does not investigate the direct effects of collective bargaining, it sheds light on the main changes in the labor market in the period immediately following the reinstatement of the wage councils.

On the other hand, Perazzo (2012) and Cabrera et al. (2013) study the non-compliance with the payment of minimum wage rulings for the period 2007-2011 using microdata from the Continuous Household Survey to test the effectiveness of the *Consejos de Salarios* in setting minimum wages.

In Brum and Perazzo (2020), Perazzo and Brum study the effect of wage councils on workers' incomes during the period 2005-2015. They find that "a 1% increase in the minimum wages negotiated in the wage councils during the period 2005-2015 is associated with increases of around 0.2% in real wages" (p.126). A novel aspect of this study is that this growth is not observed uniformly across the income distribution. The strongest effects are found for "the 10% of workers with the lowest incomes, while the effects are positive and decreasing up to the 4th decile of the distribution" (p.127).

In line with the above, Carrasco et al. (2022) study the redistributive and labor displacement effects generated by a change in national and sectoral wage policies. Focusing on the period 2004-2014, they find that the implemented policy had a redistributive impact, reducing wage dispersion in both tails of the distribution (pp.5-7). However, these effects show heterogeneities by gender and the considered period. Between 2005-2009, the reduction in wage dispersion became more pronounced, while for women, there were almost no significant changes, and no effect was found in the lower tail of the distribution. Regarding the effects of these policies on employment, workers are considered "treated" if their hourly wage is above the sectoral minimum wage but below the next one (*in t+1*). The found effect is that treated workers have a lower probability of keeping the same job in the 6 months following the wage adjustment by 1 percentage point. These estimates show similar heterogeneities as those mentioned previously, by period and gender, in the lower tail of the wage distribution.

Finally, Urruty Rodríguez (2024) studies the gender impacts of collective wage bargaining taking into account the effects of gender-specific clauses in the first five rounds of negotiation, finding null effects on women's wages and the wage gap with respect to men. However, this study finds that these clauses contribute to increase the gender wage gap by 1 percentage point within firms, but it also raises the share of employments occupied by women in these firms by the same magnitude.

In this section we have begun by presenting a brief review of the existing literature on intergenerational mobility in the theoretical front pointing out the transmission channels through which collective wage bargaining could affect intergenerational mobility, both through parental decisions or changing the magnitude of some parameters taken as exogenous. Then we have proposed an overview of the empirical literature on intergenerational mobility, both internationally and in the context of Uruguay, the country that is also our unit of interest. Finally, we have performed a selected review of the collective bargaining and minimum wages literature. Evidence from around the world shows how collective bargaining is correlated both with higher wages and a lower income inequality, although sometimes at the expense of displacement effects. This evidence holds for Uruguay, in a context of falling inequality and increasing wages, collective bargaining has contributed to increase real wages and redistribute income across workers and owners.

3 Institutional Background - *Consejos de Salarios* in Uruguay

Tripartite Collective bargaining has existed in Uruguay since the first half of the twentieth century. Law 10,449 sanctioned in 1943 established the first rules for wages to be set between business federations, unions and the State. This way the government could enforce some control and actively participate in the negotiations between employers and employees (Zunino Cánepa, 2009). This new tool organized occupations into groups classified by economic activity, with each group including 3 representatives from the Executive Branch (EB), 2 from the employers, and 2 from the workers. The main function of each council was to classify workers and set each minimum wage (*laudo*[7]) (Bucheli and Amarante, 2011).

This first phase of the CCSS extended from 1948 to 1968, during which, through the *Decreto de Congelación de Precios y Salarios*[8], the government of the time suspended the convening of the CCSS and froze wages to combat the economic stagflation that prevailed during that period (Zunino Cánepa, 2009).

With the return to democracy, the wage councils resumed, adding groups to the collective bargaining framework and dividing each group into subgroups. Occupations were divided into 48 groups and 242 subgroups. In this second instance, the State was involved until 1992, after which it abstained from participating in negotiations until 2005 (with some exceptions for the construction, public enterprises, health, and transport sectors)[9].

At this time Uruguay was starting its way to recovery from 5 years of economic decline. Around the turn of the millennia the country's economic activity was contracting steadily. This decline reached its peak around the 2002 crisis, where by that point the GDP had contracted 30% from 1999 (Pellegrino and Vigorito, 2004). The shocks for workers were twofold. First, prices rose substantially, with inflation reaching a peak of 28.1% in February 2003. Second, the labor market became very unstable, with workers from all activity sectors being laid off by their employers. The unemployment rate at the time of the crisis was around 17%, the highest since the return of the democracy (Brum and Perazzo, 2020). These factors made private sector real wages decrease around 12.9% in 2003, and the hourly earnings Gini index for full time workers rise to 0.442 (Brum and Perazzo, 2020, Pellegrino and Vigorito, 2004).

Recovery began in the second semester of 2003, though wages stayed stagnant until 2005. In this year, a new government rose to power for the first time in their history, and it implemented strong minimum wages policies. Along them came a reform to collective bargaining, with the State taking a proactive role in the negotiations (Zunino Cánepa, 2009). The following decade was one of sustained economic growth and decline of inequality, with the nation's GDP increasing around 60% in the 2005-2016 period. This time out workers earnings followed this trend, with real wages having grown 59.2% in the same period (Brum and Perazzo, 2020). In these years inequality fell, with the Gini index being around 0.37 in 2012, its lowest since 1986 (De Rosa and Vilá, 2020). Figure 1 represents the evolution of the economic activity and real wages, as well as the Gini coefficient from 2001 to 2023.

This return added new features to the negotiations. First, the group structure was changed again, with new actors in place. This new negotiation model is integrated by 24 activity groups, adding new branches like rural, domestic and public sector workers. In the rounds that would come after 2005,

[7] Which represents the minimum wage floor set for each category inside a bargaining sector for a given negotiation round and its adjustments throughout its duration.

[8] *Decreto N° 420/968 - REGIMEN DE ESTABILIZACION DE PRECIOS Y SALARIOS.*

[9] *Decreto N°105/005 - CONSEJOS DE SALARIOS. CLASIFICACIÓN POR GRUPOS DE ACTIVIDAD.*

Figure 1: GDP, Real Wage Index, and Gini Coefficient in the 2001-2023 period

new aspects of the employment regimes, like paid time off, trigger clauses or out of wage endowments begin to be regulated (Brum and Perazzo, 2020). In this new scheme, the government sets the guidelines and deadlines for the signature of each agreement. Workers and firms then proceed to determine the adjustment to each *laudo* and other work conditions (Carrasco, 2021). These agreements are subject to government approval before each one can take effect. 3 negotiation rounds were held between 2005 and 2010. In the first two, agreements were short and adjustments were made two times per year. In the following rounds, the agreements would begin to be set for a longer period of time.

Our focus will be around the second round of negotiations that took place between July 2006 and July 2007. This is because it's early on in the negotiations, where big adjustments were being made, the ones that could have the higher impact on children. Secondly, it's by the time the agreements came to cover the entirety of the workforce. Third, it sets a standard for the shape of the future negotiations and it will make us able to identify which sectors have strong bargaining power from early on.

In recent times, it has been shown that following the reinstatement of this tripartite scheme, the gap between the marginal labor income value of the firm and the level of remuneration for labor (i.e., wage markdown) has decreased. Casacuberta and Gandelman (2023) suggest that this institutional change helped to boost the negotiating power of unions on wages. This, in part, suggests the importance of studying the bargaining power from a sectoral perspective, as the available evidence points to the fact that this is manifested through the resolutions adopted in collective bargaining rounds.

4 Data, sample and variables

4.1 Data

4.1.1 Social Security Records

Our main dataset consists of administrative records (*Historias Laborales*) from the *Banco de Previsión Social (BPS)*[10], Uruguay's main social security institution[11]. The data contains the work histories for the universe of Uruguayan formal workers between 1996 and 2022. We have data on earnings, hours worked, firm (firms), type of employment and activity sector for each person working in the formal sector on a monthly basis. These records also hold each person's sex, date of birth and date of entry to each company. From this we can draw on variables like firm size, cohort of birth, year and age of entry to the labor market, worked months per year, and wage distributions for all the formal sector employees, and it is representative for the country as a whole, since the informality level of the labor market stands around 25% (Carrasco et al., 2023). Additionally, we know if the person enrolled and/or completed a university degree from the *Universidad de la República* (UdelaR), Uruguay's largest tertiary education institution. Nationwide, it accounts for approximately 66% of university attendees (MEC, 2021)[12] [13]. These sources are complete and exhaustive since they come from administrative records. This allows for reconstructing individuals' income trajectories without concern for under-reporting or collection errors, problems that are common when using more typical sources like household surveys for Uruguay (Burdin et al., 2014). In total, we hold records for 2,937,222 individuals working among 1,203,136 firms with an average tenure of about 6.9 years inside a firm.

4.1.2 Wage council data

Records[14] from the wage councils resolutions hold information for minimum wages and weekly hours for each category of occupation[15]. Moreover, for each subgroup there is a sectoral minimum wage that is binding. We match these records with our worker history data using 4-digit ISIC (fourth version) identifiers for each industry class. We take as sectoral minimum wage the lowest wage in each subgroup.

We have wage adjustments for a total of 25 groups, 224 subgroups and 7675 categories negotiating across 153 year-round combinations for the period 2005 - 2020. These account for full time workers (monthly wages) and day workers (daily wages). We will focus on the second round which took place between 2006 and 2007.

[10]These were obtained via an agreement between the *Ministerio de Trabajo y Seguridad Social* (MTSS) and the *Instituto de Economía de la Facultad de Ciencias Económicas y de Administración*. The MTSS has given us the permission to use these records for this specific document via a formal agreement between the former and the IECON-FCEA.

[11]*Banco de Previsión Social - BPS*.

[12]Data for the year 2020 published by Uruguay's *Ministerio de Educación y Cultura*.

[13]The remaining third of the enrollment shared between private universities, a newly born technological university and teaching formation institutions.

[14]These are public records available via the *Ministerio de Trabajo y Seguridad Social*. We thank the Labor Economics group at the *Instituto de Economía de la Facultad de Ciencias Económicas y de Administración* for systematizing and sharing this data with us.

[15]Wage councils encase each worker inside a given category, subgroup and group.

4.1.3 Parent-Child identifier data

To identify fathers (mothers) with their sons, we use administrative records from the BPS that allows us to link families via household identifiers. Parents and sons are identified thanks to information from a range of records of social programs that the BPS regulates. This way we can construct household identifiers for about 3 million people. We identify parent-child couples by linking individuals who ever lived in the same household in the same fashion as Leites et al. (2022a). The previous work also documents how these records are powerful enough for constructing a representative sample of the income distribution of each generation. We do not observe whether these are or not biological parent-child couples.

Although we are not able to identify people working in the informal sector, this is not a major problem since its presence in Uruguay is not as high as in other Latin American countries. We link this to our main data source via unique individual identifiers[16].

These records account for a total of 3,040,671 individuals inside 1,064,386 unique households. We have a total of 647,650 (18.2%) fathers, 883,502 (24.83%) mothers and 2,027,345 (56.97%) sons[17].

As additional sources, we get data from the Uruguayan National Institute of Statistics (INE)[18] regarding the evolution of average and real salary indexes (*IMS & ISR*)[19], the consumer price index (*IPC*[20]) and the Gini Coefficient. Lastly, we draw upon data from Uruguay's Central bank for the evolution of the economic activity[21].

4.2 Variables

To construct our measures of intergenerational income mobility we'll use both the parent's and children's place in the formal earnings distribution of their birth cohort constructed by their permanent income over our years of interest. As we want to reflect each person's place in the population, we draw upon the entirety of our social security records database. This means for the parents, taking the whole sample of formal workers with positive earnings between 2006 and 2010 who were born between 1950 and 1966 and for children, earnings of people born between 1988 and 1996 for the years 2018 through 2022. We take as a definition of permanent income (both for parents and children) the average formal earnings of a person in 5 years. When we see a person reporting null formal wages for a year used to construct this measure, we include those zeros in our definition. Following Leites et al. (2022b), this criterion represents a lower bound of the permanent income[22]. Then, we create an income distribution for each cohort and locate each person in our sample in their respective percentile. The *Rank* variable represents the percentile that a person occupies in the income distribution of their respective birth cohort. Earnings distributions are mostly similar among parents cohorts and the same goes for children cohorts. These distributions can be found in Figure A.3 for children and Figure A.4 for parents. The income distributions

[16] These are constructed by anonymizing the Uruguayan National ID *Cédula de Identidad*. We do not hold anyone's National ID in order to protect the individuals' privacy.

[17] The data spans across multiple generations, therefore it is possible to have the same person both as a mother (father) and daughter (son). We have around 29.1% of our individuals occupying more than a role in this data (same person across generations).

[18] *Instituto Nacional de Estadística*.

[19] *Índice Medio de Salarios*.

[20] *Índice de Precios del Consumo*.

[21] Banco Central del Uruguay

[22] This is because in this criterion, when observing a person with no formal annual income it assumes that they were either unemployed or inactive and not receiving any other type of informal income. Since they could be having positive unreported earnings that we do not observe, we assume this measure of permanent income as a lower bound.

are more skewed to the right as the children get older, indicating that older cohorts are, on average, richer than their younger counterparts. We build our income distributions for each cohort because otherwise we would be comparing children at different stages in the labor market and the distribution would be filled with younger children at the left (mechanically less experienced) and older at the right (with more years in the labor market which could lead to higher salaries). When observing parents, this pattern seems to dissipate, providing evidence in support of our assumption that earnings stabilize once a parent reaches a certain age[23].

We have data upon enrollment and completion of a degree in the *Universidad de la República*. One of our outcomes of interest is the child's enrollment in university, we define this as a dummy variable equal to one if we observe them ever attending university. A limitation of our data is that we do not have the exact moment in which they enroll, so we cannot account if this happens either before or during the years we take to construct their permanent income. Around 21% of the children in our sample ever enroll in university[24], this is consistent with the numbers given by Uruguay's Ministry of Education and Culture for the year 2019 (MEC (2021) p.71).

In regards to labor market outcomes, we focus on two fronts. First, we define the age of entry to the labor market as the age (in years) a person was when they got their first job. This variable has the main limitation that we only count on data for formal employment, so we may be catching people that previously worked in the informal sector. The mean entry age to the labor market is at 20 and a half years.

Secondly we inquire into the inheritance of employers. For this, we want to see if a child ever works at a firm where at least one of their parents has ever worked. For this, we take companies in where we observe a parent and we do the same for their children. If they ever overlap, we define this dummy as one[25]. This overlap can happen simultaneously - they both work there at the same time - or separate through time - parent works there first and child follows.

4.3 Samples

In this subsection we will present our overall sample drawn from our Social Security Records and the years we will consider for observing both child and parental income on a more detailed manner. This paper will contain two different empirical strategies that make for two different samples. Our first strategy will be able to capture more individuals, being more representative of the overall population. Meanwhile, our second strategy aims to find a more precise effect, and for this, we take a subpopulation that focuses on a specific part of the income distribution.

4.3.1 General Sample

Our overall sample is defined as pairs of matched children with their respective parent by household identifier. We take parents from the 1950 to 1966 cohorts who have at least one month of paid[26] formal employment between the years 2006-2010, which implies an age range of 40 to 60 years old at the time

[23]Figures A.5, A.6, A.7 and A.8, A.9, A.10 provide some additional information on the expected rank for a child given their parent's rank and the probability of passing their parent in the income distribution. It appears that these trends are similar for both sons and daughters, with the former being more disperse in terms of expected ranking.

[24]Table A.1.

[25]Meaning they overlap in more than one company.

[26]With a reported salary higher than 0.

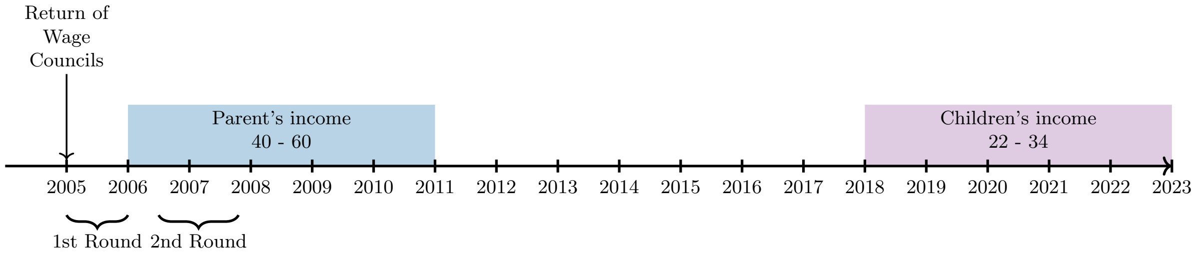

Figure 2: Timeline of the sample and the wage council adjustments

of observing them in our administrative registry data. We do this following the literature both nationally (Leites et al., 2022a) and internationally (Chetty et al., 2014) where they take parental income at the 45-65 age window. The argument is that parents' place in the income distribution is already set and that it's between these ages that they have children living with them in the household. We move this window five years earlier, as this allows us to increase the size of our sample and, at the same time, we consider that this shift does not represent a significant difference in terms of the period during which parents live with and influence their children.

Considered children are those born between 1988 and 1996 and we observe their formal employment history between 2018 and 2022, being between 22 and 34 years old at the time of observing them in our records. We have a total of 189,041 children and 169,700 parents. When merging these datasets we have a total of 237,277 parent-child pairs. Our average cohort size is 21,005 children, and as shown in Table A.1 about 51% of our children are females, with a mean of 0.68 siblings per child. We do not observe our children and parents in each year of our records. This can be due to them not being present in the formal labor market for a period of time. Here, we will take every person that shows up at least one time in a year. However, as seen in Figures A.1 and A.2 we see that most of our children and parents are present in all five years that we measure their outcomes.

In Figure 2 we can visually show the periods for which we will construct our permanent income measure and also measure our outcomes for the considered children. Here too we can see how the first two rounds of the new collective bargaining scheme took place one after the other. Moreover, the agreements for the second round are made within the first two years that we observe to construct parental earnings.

Two distinct empirical strategies will be used for our analysis. The first follows a sector-level approach, while the second focuses on the individual level. The sector-level sample is discussed in Section 4.3.2, and the individual-level sample is presented in Section 4.3.3.

4.3.2 Sample for the Sector-level strategy

For our first design we will match each parent in this sample with our collective bargaining data using the 4-digit ISIC classification corresponding to the company they were employed at during the time of the negotiations (July 2006 - July 2007). We drop parents who are employed at a sole proprietorship and we impose for the parent to be working at least 6 months in the sector before the adjustment takes place to account for people who work temporarily during select periods of the year. Following Carrasco et al. (2022), we drop groups 21 through 24 whose collective bargaining agreements start after the first round[27] and public sector workers. This leaves us with our final data consisting of 88,037 parent-child combinations.

Alternatively, we'll define different samples depending on the amount of years a worker is present in the sample with a positive formal salary. Our main specification will use the entirety of our data, but we'll construct two additional samples, one using workers who are present for at least three years (intermediate) and another one using those who report positive earnings for every year of interest (06-10 for parents and 18-22 for children).

4.3.3 Sample for the Individual-level strategy

Our individual-level strategy will try to pinpoint the effect of collective bargaining for workers who are near the sectoral minimum wage around the adjustment negotiated in the second round.

At baseline - before the adjustment - we will have parents that, within a sector, had positive formal earnings but these did not surpass the SMW that would enter to place next month. We construct two groups. The first one looking at their earnings immediately after the adjustment kicks in, that is, at month t. The second one will define a sample with the same characteristics but looking at parental earnings 6 months after the sectoral adjustments kick in ($t+6$).

In addition, we would like to consider parents with earnings close to the sectoral minimum wage after the adjustment kicks in and that they stay in the same company and same activity sector[28] to try and assign the increment mainly to the results of the sectoral CB agreements. For this, we'll take a radius of 20% around the SMW at time t or $t+6$ and we'll draw our treated and control parents from these people. In this case, we will be looking for effects for a subpopulation of our sample defined in section 4.3.2, where parents will be overly represented at middle and lower parts of the income distribution since their earnings are close to the sectoral minimum wage.

Considering our cohorts and the restrictions to form part of the treatment and control group, for the group defined when we observe if they get the adjustment at time t we end up with 3519 parents and 4804 children making for a sample size of 4876 parent-child pairs. For the group that will consider workers' earnings at time $t+6$ to construct our treatment definition we have 3817 parents and 5094 children. In this group, our parent-child pairings amount to 5186.

5 Empirical Strategy

5.1 Treatment at the sector level

Our first approach will attempt to assess the correlation between having a parent in a so-called *strong* sector and the position their child occupies in the income distribution of their birth cohort when observing their wages for the 2018-2022 period. A sector (subgroup) here is determined as strong if the relative increase in the sectoral minimum wage is higher than that of the average salary index between the first and second round of negotiations (2006 to 2007). Strong unions are characterized by having more market power and ability to increase the salaries of the workers covered by their influence (Brändle, 2024).

We match the evolution of the salary index (IMS) with the dates in which the sectoral minimum wages are updated and if in that given period the relative increase of a sector is bigger than that of the

[27] These correspond to: (21) domestic service, (22) livestock rural work, (23) agricultural work and (24) forestation.

[28] Since a parent can move from sector to sector within a company.

IMS, we define the former as *Strong*. This translates to[29]:

$$Strong = 1 \iff \frac{SMW_{s,m2}}{SMW_{s,m1}} \geq \frac{IMS_{s,m2}}{IMS_{s,m1}} \tag{1}$$

Summary statistics for this sample can be found at Table A.3, while the composition of strong sectors (and other characteristics) by negotiation branch are at Table B.1. First stage estimates found on Tables C.1 and C.2 indicate that being in strong sectors is positively correlated with the parent's permanent income and a higher place in the distribution. A possible explanation which goes in line with the literature that studies the effect of wage councils in the income distribution is that these wage increases compressed the income distribution. That said, another possible explanation is that these are better jobs occupied by workers with a higher productivity and thus with average higher salaries.

Our first approach aims to see the association between having parents working in strong sectors and position in the income distribution, entry to university and labor market outcomes. For this, we take the following equation:

$$Y_i^{c_h} = \beta_0 + \beta_1 Strong_i + \beta_2 R_{i,p}^{c_p} + \beta_3 R_{i,p}^{c_p} \cdot Strong_i + \Gamma' X + \phi_{ch} + \mu_{cp} + \varepsilon_i \tag{2}$$

The outcomes defined in $Y_i^{c_h}$ are the rank of son i in cohort c_h defined by their permanent income, the child's entry age to the labor market and a dummy equal to one indicating if the child was ever enrolled in university or if he ever worked at a company where their parent works or used to work.

On the right hand side of the equation, $R_i^{c_p}$ corresponds to the rank of parent defined by their permanent income[30] of child i born in cohort c. $Strong_{i,p}$ is a dummy equal to one if parent p of child i was treated following our definition (or any of the other 3 alternatives). X contains a vector of control variables such as parent p sex and child i sex. ϕ_{c_h} and μ_{c_p} represent parent and child cohort fixed effects. Finally, standard errors ε_i are clustered at the child level. We will also run these estimates for our samples that require the parents having earnings in at least 3 different years of in every year that we consider to build their permanent income.

β_1 represents the average incidence of having a parent in a strong bargaining sector and the position a child occupies in their birth cohort income distribution. Referring back to Leites et al. (2022b), β_2 is the estimator of intergenerational persistence in the income ranking. This coefficient represents the association between the rank of the father/mother in the income distribution and the rank of their child, both within their respective cohorts. β_3 allows for the treatment coefficient to vary depending on the position a parent occupies in the income distribution [31]. This is, in the measure that a parent climbs up in the income distribution, we see on average how the treatment effect changes.

Since there are a lot of potential unobservable factors to this strategy (selection into sectors, differential composition of workers between sectors among others) this strategy lacks power to be deemed causal and is merely associative.

[29] We also use specifications for 1, 1.5 and 2 times the adjustment. The composition of strong sectors according to each treatment can be found in Table B.1.

[30] A child can have either one or both parents present in this estimation.

[31] This could be interpreted as a difference in difference estimator where we have, for two parents in a given percentile, the additional effect of the treatment with respect to β_2. β_3, computing the average association would reflect how this changes once you go up the income distribution.

Our identification strategy relies on some key assumptions that may also have their limitations. First and foremost, incorporating $R_i^{c_p}$ into our estimations allows us to control indirectly for the parent's income and how it locates itself in the general population. We consider this to be key since there could be differences of the average income of parent's between sectors that need to be accounted for. However this has a limitation that may be biasing our estimates downwards. We define permanent income using five years of formal earnings from 2006 to 2010. Our treatment comes into effect also during these years, affecting their income and - ultimately - their position in the earnings distribution. For this reason, the negotiation effect may also be manifesting itself by the income effect it has on parents which can be captured by the rank variable.

A way around this could be defining the permanent income before or after our treatment happens. However, doing it for previous years would imply incorporating - indirectly - the effects of the 2002 crisis in Uruguay, and in contrast, doing it after (from 2008 on) would mean taking the permanent income for years where our children are older and not as prone to being affected by the treatment. We think that this round sets the tone for how these sectors would negotiate in the future, and, that it is early on in the construction of the parent's permanent income, so its effects would be present for most of the years where we define this variable.

Other key identification assumptions behind our strategy are that before tripartite councils were re-established, sectors held a similar bargaining power, and that before the CB agreements were re-established, the distribution of workers inside strong and non-strong sectors was random. This is, however, highly unlikely since sectors comprised of higher paying jobs could have better funding capabilities for their representatives and an overall bigger bargaining power. Finally, we have to assume that before the agreements were set workers were not able to anticipate and select themselves into a sector on the basis that this would be strong in the way we define it.

We would expect that the treatment effect loses its power for our rank outcome as we move up the income distribution. The main reason for this is that when we take parents that are in the upper tails of the distribution, they will already have the means necessary to invest in their children regardless of the adjustments that are made in their work sector. Once we are in the upper tails of the distribution, the treatment would not be expected to statistically influence the ranking that the children will occupy, nor their educational decisions. Additionally, we define a sector as strong based on the increment of the minimum wage, but these parents could be receiving a significantly different adjustment since they're far away from the wage level that determines our treatment status. In terms of labor market outcomes, we could be having parents who are firm owners (controls) and their children working in the same firms in the future. In this case, we would expect for the outcome to slightly shift in a negative direction. This also goes in hand with the age of entry to the labor market, since those children could start in their parents companies from early on, so we would expect that as one goes up the income distribution, children with treated parents enter the labor market at an older age. On the other hand, parents in strong sectors but in the lower tails of the distribution could pass on their preferences for work (in those particular sectors) to their children, making them enter the labor market early on in their lives.

As noted previously, our empirical strategy presents some challenges that prevent us from giving our results a causal interpretation. In addition, an important limitation to our treatment definition is that we do not observe if each individual gets an adjustment. That is, the fact that someone belongs to a treated subgroup is not a one-to-one match with that person getting a wage increase, in particular for parents that within a sector occupy the higher paying jobs. In the following subsection we'll refine our treatment specification in order to develop an empirical strategy that accounts for a causal mechanism between wage bargaining and intergenerational mobility.

5.2 Individual-level treatment

Considering the limitations in the section above, we now turn into a strategy to define the treatment at an individual level. An ideal setting to find a causal effect of collective bargaining on intergenerational mobility would require for us to have, in each sector, parents randomly distributed between two groups: some covered by collective bargaining agreements and others not. We would then compare the outcomes of the children that are born into each one of these groups[32]. Since this is not possible due to the universal coverage of Uruguay's collective bargaining framework, we'll look for a comparable set of parents within each sector and to identify the parents for whom collective bargaining caused a greater change in their income.

For this, we'll follow a strategy similar to that adopted in Carrasco et al. (2022). This takes Dustmann et al. (2021) framework where the authors use bins centered around the minimum wage in order to create comparable groups of workers in terms of earnings and study the employment, earnings and reallocation effects of a nationwide minimum wage policy.

We will exploit the fact that in our data we find some parents that should have gotten a raise but do not seem to get it by the time the adjustment kicks in. These parents will be our control group, because they should have gotten over the minimum wage threshold for the sector that they work in (given that they fit the restrictions we impose) but their reported labor income is below the sectoral minimum wage after the adjustment kicks in. The main assumption is that whether a parent receives this increase or not is random, so there would be no selection into the treatment or control groups, making these comparable among each other.

There is evidence that firms paid below the sectoral minimum wage during the rounds held between 2006 and 2011. To estimate the extent of this noncompliance (defined as the share of workers whose reported earnings fall below the statutory minimum wage), Perazzo (2012) uses data from the Continuous Household Survey. According to her findings, in 2006, the noncompliance rate ranged between 12.1% and 17.4%. Although noncompliance declined in the subsequent years, it remained present. As shown by Cabrera et al. (2013), the noncompliance rate dropped from 16.6% in 2007 to 14% in 2011. This noncompliance allows us to build a control group comprised by those workers who were affected by it.

We would like to point out that this does not mean that they are not given additional compensation, as there is the possibility of compensation outside the formal spectrum or other types of non-pecuniary payments or clauses as seen in Brum and Perazzo (2020) & Urruty Rodríguez (2024).

Given the similarities in earnings of these groups, we are focusing on people on a specific part of the income distribution. Therefore, including differential effects like those of β_3 from our previous strategy no longer makes sense. This can be seen with the density estimates in Figure A.12. We'll adopt three treatment definitions[33]. The first one consists of taking as treated those parents who, at time t when the

[32] With the additional assumption that children were not earning wages that were affected by this round of negotiations at the time of the adjustment.

[33] For all these treatments, we'll also use a treatment where we observe them 6 months after the adjustment kicks in $t+6$.

adjustment kicks in, see a wage increase that puts their earning above the new SMW. This results in:

$$Treated_{i,s,t} = \begin{cases} 1, & \text{if } w_{i,s,t-1} < w_{s,t}^{min} \;\&\; w_{s,t}^{min} \leq w_{i,s,t} \leq 1.2 \cdot w_{s,t}^{min} \\ 0, & \text{if } w_{i,s,t-1} < w_{s,t}^{min} \;\&\; 0.8 \cdot w_{s,t}^{min} \leq w_{i,s,t} < w_{s,t}^{min} \end{cases} \tag{3}$$

Identity (3) shows how we take at $t-1$ workers with positive formal earnings below the minimum wage in t, no matter how far from it they were. However, at t once the adjustment kicks in, we take as treated those that earn equal or up to 20% above the minimum wage and controls as those who earn more than 80% of the new SMW but less than it.

The second treatment will take into consideration the percentage that the parent makes of the SMW at time of the adjustment. This takes our treatment as continuous, which will let us see the effect of a 1 p.p. increase - relative to the parent's sector's minimum wage - in children's outcomes.

Similar to our first specification, our estimations will be as follow:

$$Y_i^{C_h} = \gamma_0 + \gamma_1 Treated_{i,s,t} + \gamma_2 R_i^{C_p} + \Gamma' X + \phi_{ch} + \mu_{cp} + \zeta_s + \omega_i \tag{4}$$

$$Y_i^{C_h} = \gamma_0 + \gamma_1 Pct.SMW_{i,s,t} + \gamma_2 R_i^{C_p} + \Gamma' X + \phi_{ch} + \mu_{cp} + \zeta_s + \omega_i \tag{5}$$

Here, all the components from (2) stay in place and we add sector fixed effects ζ_s [34], with our treatment variable being a dummy equal to one if the parent was below the next sectoral minimum wage the month before the adjustment kicks in and above it after comes into effect. We'll also explore when the parent is above the SMW 6 months after the adjustment kicks in. As in our previous section, γ_2 may still incorporate some indirect effects on child ranking via the effect of the negotiation on parental income. To sort this, we will perform our regressions on child's ranking excluding γ_2 and see if we notice any important changes in γ_1.

Finally, we'll adopt a bin-based approach, where we will try to pinpoint the effects of being inside a specific wage bin. As our control group we will have those parents in the [80%; 100%) bin, and as treatment dummies for the $[100\%;110\%)$ and $[110\%;120\%]$ bins. This will also allow us to get more information as to the magnitude of the adjustment that is necessary to have an effect on children's outcomes without taking unitary wage bins as in (5). Although we split the treatment group into two, we do it under the same identification assumption that for 4, which is that these workers are comparable at $t-1$.

The estimation goes as follows:

$$Y_i^{C_h} = \gamma_0 + \sum_{k=1}^{k=2} \gamma_k Treated_{i,s,t}^{g_k} + \gamma_4 R_i^{C_p} + \Gamma' X + \phi_{ch} + \mu_{cp} + \zeta_s + \omega_i \tag{6}$$

Where $g_k \in \{[1,1.1)_{k=1}, [1.1,1.2]_{k=2}\}$ wage bins. We want to see if the wage increase induced by these adjustments carries a spillover to the next generation when compared to children in the control group. As with equations 4 and 5 we'll also explore these outcomes by switching our treatment definition for when the parent is above the SMW 6 months after the adjustment kicks in. An increase in income would push the budget constraint under which the family operates, this could affect the moment that a child enters the labor market or his educational decisions. Delaying the entry to the labor market for

[34] Using ISIC-4 digits.

human capital acquisition reasons could lead into better jobs that make children occupy a higher place in the income distribution. On the other hand, an earlier entry to the labor market could imply more experience and the generation of more income from earlier on in a person's life.

6 Results

6.1 Sector-level treatment

This strategy is our first approach to see whether there is a correlation between parents working in what we define as *Strong* sectors and the place their children occupy in the income distribution. Although we cannot make causal claims for the results below, the sample we use here considers workers across the entirety of the income distribution and across all activity sectors[35], making this the closest we can get to the correlation for the entire population.

Table 1 shows the aggregate estimates for the full sample of sector-level treated parent and son pairs. The estimated IRA for formally employed children and parents is of 0.2, in line with the recent international literature and somewhat less persistent than the estimates shown in Leites et al. (2022a,b) and Araya (2018). These results are mainly driven by the younger cohorts, where their place in the income distribution is less established than older children. When we observe these results for older children (e.g, those born in the 1988-1991 cohorts) the IRA point estimate is of about 0.23 (Table C.5) consistent with the previous findings for Uruguay. This may be partly due to the fact that we count with a more precise measure of permanent income since we take children at older ages when their earnings trajectory has already stabilized.

Concerning our other outcomes, for each place a parent climbs up the income distribution of their cohort, their children are 0.4 and 0.1 percentage points more likely to enroll in college and work at a firm where the parent has worked (columns (2) and (4)). Children whose parents are higher in the income distribution tend to enter the labor market later. A 1 percentile increase in parental ranking is correlated with a child delaying their entry to the labor market by 0.007 years. Unlike the results from the IRA, this pattern holds consistently across both older and younger cohorts.

For a child to have a parent working at a strong sector at the moment of the second round adjustments is positively correlated with their children occupying a higher position in the income distribution of their cohort. The aggregate point estimate is of about 1.3 places. This positive result is consistent when restricting the sample to parents and children who were in our administrative records for 3 or more years or 5 years, although these last ones are noisy. The coefficient that incorporates the interaction of parent rank with treatment status (β_3 from equation (2)) seems to indicate that the sector level effect fades once we move upwards the income distribution, although it's not powerful enough to make this claim. When we open up these results by age we notice that, even if the positive effect is present for both groups, older cohorts are the ones benefiting most from the adjustments.

A potential limitation of this approach is that we may be incorporating the effect of the negotiation in more than one way. Ideally, we would like to isolate the strong sector correlation only in β_1, however, we may be incorporating some of it when we incorporate the parent's ranking in the regression. Since we take negotiation rounds in 2006-2007 and we define permanent income from 2006 to 2010, being in a strong sector could have possibly helped a parent to occupy in a higher place of their cohort's income

[35] Bar the ones we excluded for methodological issues.

distribution. In this way, the correlation would be present both in β_1 and β_2 since the definition of permanent income that we use could be incorporating the improvements of parents induced by collective bargaining.

Children whose parents are working in strong sectors are 2.5 p.p. more likely to attend university. Although virtually zero, our estimates point out that this association becomes negative once we go upward the income distribution. In some ways, this could mean that the treatment status had a differential effect on attending university for children born in lower income households. When opening up these results by age, we see similar patterns with a slightly stronger impact for older children. A possible factor might be that for a portion of the 1992-1996 cohorts educational decisions were not fully made yet. We see, however, how younger children are more likely to be enrolled in college overall (control means of 0.21 for the 92-96 cohorts vs 0.17 for 88-91).

There's a noisy signal when it comes to analyzing the correlation between a parent working in a strong sector and their child's age of entry to the labor market. On aggregate, it seems that treatment status anticipates the entry by 0.078 years. However, the interaction between treatment status and parental ranking estimates from column (3) of Table 1 show that children with treated parents are more likely to delay their entry to the labor market by 0.002 years conditional on the parent's place in the income distribution. Concerned by this, we compute an estimation without the interaction of β_3 to observe the overall treatment effect and its direction[36]. Results found in Table C.4 allow us to see that the overall correlation when looking at the age of entry to the labor market becomes positive, although quite small. Having a parent working in a strong sector is associated with delaying the entry to the labor market by 0.03 years. Contrasting it to our main specification, this could be evidence in favor of the β associated with the age of entry to the labor market being driven by children with parents in the left tail of the income distribution[37].

Some opposing channels may explain this change in direction. First, the relaxation of the family constraint can give space for better investments in education. A possible outcome could be that the child stays longer in the educational system, keeping them off the labor market for more time. One counterbalancing channel could be that the parent develops stronger networks which could help children when entering the labor market. Comparing both the main and pooled estimates for the inheritance of employers, we see how the one found in our main specification is bigger (columns (4) of Tables 1 and C.4). This can be taken as evidence of the correlation between working in a strong sector and the inheritance of employers being stronger for families with parents at the left of the income distribution. If we combine these two outcomes, we could argue that children born in these households could use their parent's network to enter the labor market at younger ages.

Wage bargaining may also enhance the parent's views for the sector they work in, contributing to the intergenerational transmission of preferences for work. If a parent observes that they are in a strong sector, they may want to remain in that job and want their children to do as such and be more active in developing the child's positive views for the job and passing the required abilities for it. Comparing the results in our main specification and our pooled treatment, the evidence points that these last two channels could be the ones driving our point estimate.

[36]This translates to: $Y_i^{c_h} = \beta_0 + \beta_1\, Strong_i + \beta_2 R_{i,p}^{c_p} + \Gamma' X + \phi_{ch} + \mu_{cp} + \varepsilon_i$

[37]Figure A.11 shows how the distribution of age of entry to the labor market is similar across all children cohorts.

Table 1: Main Estimates: Rank, education, entry to the labor market and same firm

	(1) Rank Child	(2) Enters College	(3) Age Entry LM	(4) Same firm
Rank Parent	0.204***	0.004***	0.007***	0.001***
	(0.005)	(0.000)	(0.000)	(0.000)
Strong	1.292***	0.025***	-0.078*	0.062***
	(0.455)	(0.006)	(0.040)	(0.005)
Strong ×	-0.011	-0.000**	0.002***	-0.001***
Rank Parent	(0.007)	(0.000)	(0.001)	(0.000)
Control Mean	55.042	0.189	19.928	0.111
N	88,037	88,037	88,037	88,037

Notes: Rank Child represents the ranking (percentile) of the child in the income distribution of its cohort. Enters College and Same Firm are a dummies =1 if the child ever enrolls in public university or works at a firm where one of their parents has ever worked. Age Entry LM represents the age (in years) in which the child gets their first job. Strong is a dummy =1 if the parent is in a subgroup that negotiates above the average salary index increase. Control variables are parent and children sex. Fixed Effects are for parents (1950-1966) and children's birth cohorts (1988-1996). Standard errors in parentheses and Clustered at the child level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Like in Staiger (2022) this can also be the reason why the β associated with the place a child occupies in the income distribution is stronger for children with parents at the left tail (we observe how this effect dims in column (1) of Table C.4). A parent being in a strong sector develops networks or preferences that they pass on to their children, this can make it easier for them to get their first job at a parent's firm and quicker than the remaining people in their cohort. In turn, these children would have higher earnings because they could gain experience faster or have an easier access to higher paying jobs via their parent's network, thus making them occupy higher places in the income distribution of their respective cohort.

We see that the treatment is associated with a higher likelihood of employer inheritance from parent to child. Children with treated parents are 6.2 p.p. more likely to have ever worked in the same firm as one of their parents. Similar to the higher education story, this effect seems to be concentrated in the lower ends of the distribution, which can lead to think for a transmission of social capital or preferences for a specific sector once the household identifies the strength of it *a la* Corak (2013). Another potential channel is that collective bargaining fosters a stronger attachment of the parent to their job, which would lead him to remain in it longer and build stronger social networks inside the sector or even the company itself. In turn, their children could benefit from these strong networks to obtain employment, increasing the likelihood of employer inheritance. This could also mean a reinforcement of the insider–outsider logic, creating on the long run a difference between the children of parents in strong sectors and those who are not (Lindbeck and Snower, 2002). Ultimately, strong sectors may differ in their informal institutions. These could foster an environment in which it is more favorable for a person to hire close people such as family members, which could explain why the inheritance of employers is stronger in the lower ends of the income distribution.

When it comes to the most commonly used estimates of intergenerational rankings associations, we find similar IRA magnitudes for Uruguayan formal sector employees to those existing in the literature. In regards to higher education, entry to the labor market and inheritance of employers, our results go in line with the evidence that as a parent moves up the income distribution, their children are more likely to attend university, delay their entry to the labor market and share at least one employer with their parents in their lifetime[38]. Overall, having a parent working in a strong sector is positively correlated with a higher place in the income distribution. These results are consistent across different samplings, though noisy and seem to be driven by older cohorts. These children are also more likely to attend university and inherit one of their parent's employers at some time in their lives. Contrary to our intuitions, it seems that these children also enter the workforce at a younger age, however, we do not have enough statistical power to make this claim (only being significant at the 10% level in our aggregate estimations and losing even more power when we open up by age).

Although this approach sheds some light into the possible spillover effects of wage bargaining on the next generation, it has some limitations that constrain its explanatory power. First and foremost, not all workers inside of a treated sector get a wage increase and the ones who get it do not get it in the same magnitude. Then, selection into sectors is not random. A parent may select him or herself into a sector due to the labor market conditions of it. Third - and following this line - the strength of a sector can also be due to the resources that their workers have from baseline. We perform this exercise and notice imbalances across all our *Strong* sector definitions in terms of earnings in Figure A.13. Here, we take the log average monthly income at 2005 for the parents in our sample that are in strong sectors in the second round. Although we find similar distributions, treated parents (i.e. those in strong sectors) were earning on average more than those that would form the control group since there's a higher density of them at the right of the earnings distribution.

Due to this, there is a high chance of imbalances between the types of populations in each sector that can make our empirical strategy unfit for a causal explanation. In the following subsection, we shift our focus to a strategy that is more suitable to make comparisons between children. As stated in section 4.3, we'll be defining our treatment status for each individual with respect to the minimum wage of the sector that each parent is in at the time of the adjustments.

6.2 Individual-level treatment

We will now turn into our individual-level treatment which will try to identify the effect of collective wage bargaining on intergenerational mobility for comparable workers. The interpretation of the following results will allude to a subsample of the one we used in the previous section since we're using workers that are close to the minimum wage at the time of the adjustments of the second round.

In this new strategy, children with treated parents are those whose parents were earning less than the next sectoral minimum wage the month before the wage adjustment kicked in and were earning more than that once the adjustment kicks in[39].

6.2.1 Balance at $t-1$

Before presenting our results, we will see how comparable are our treatment and control groups. Descriptive evidence in the left panel of Figure A.14 shows that - based on the income distributions constructed by our measure of permanent income from 2006 to 2010 - although treated parents occupy slightly higher

[38] β_1 for columns 2, 3 and 4 of Table 1.

[39] In the next section we'll discuss results imposing that the parents earn more than the SMW the 6 months after the adjustment kicked in.

rankings, both distributions are similar, with both treated and control parents present across the entire distribution. However, when we turn into the left panel of Figure D.1 and our Table D.1 to see the differences in earnings pre-treatment, we notice no statistical difference in earnings between the treatment and control group in the month before the adjustment comes into play. The sample is balanced also in terms of gender composition of the parents, and while on average control parents are older than treated ones, this is only by 0.37 years and only significative at the 10% level.[40].

6.2.2 Main results - Rank in the Income Distribution

In terms of spillovers to the next generation, we mostly observe a positive association between treatment and place occupied by children in the income distribution of their cohort. However, these treatment effects are noisy. On average, those children with treated parents are 1.6 places higher than their counterparts in the income distribution, although the treatment effect is not significative. There is also associative evidence that when a parent moves up one percentage point of the SMW that he wins, his children are better off (around 0.2 places per percentage point as seen in column (2) of Table 2) but still this estimate remains insignificant.

However, when using specification number 6 (column (3) of Table 2), there is some statistically significant evidence that the treatment had a positive effect on the child's position in the income distribution when the parent is part of the $[1.1;1.2]$ bin, being on average 4.7 places above those in the control group. Our first bin does not indicate that CB had an impact when the parent's new salary - at the month of the adjustment - is above the new SMW but not more than 10%. This could indicate that the magnitude of the increase is highly important to have an effect on children.

As noted before, the concern to about the effect being accounted for both in the treatment variable and in parental income here is also present. The point we're trying to make here is the fact that the timing and the shape of the way they get their income matters. Since we take a parent's permanent income observing their wages from 2006 to 2010 and the adjustments are mainly made around 2006 and 2007, parents who got the bigger increase at an earlier stage of that time period had the chance to allocate those additional earnings to the household and - potentially - their children from earlier on. In columns (4) through (6) of Table 2 we compute the treatment effect without controlling for parental ranking. Across our treatment definitions, we get similar results to the ones we observe in our main specification. Perhaps the most important is that we observe almost exactly the same outcome for the $[1.1;1.2]$ wage bin while maintaining our significance level (column (3) vs column (6) of Table 2).

[40]For the 6 month treatment, these are the right panel of Figures A.14 and D.1 and Table D.2. Qualitatively, these are different to our immediate treatment results mainly due to the earnings of the treatment group being slightly higher than those in the control. There is no statistical difference in gender composition, but treated parents are on average almost half a year older than control ones.

Table 2: Main Estimates - Rank in the income distribution Conditional and Unconditional on Parent Rank - Immediate Treatment

	(1) Rank Child	(2) Rank Child	(3) Rank Child	(4) Rank Child	(5) Rank Child	(6) Rank Child
Rank Parent	0.166^{***}	0.165^{***}	0.166^{***}			
	(0.024)	(0.024)	(0.024)			
Treated	1.596			1.479		
	(1.145)			(1.148)		
Treat Cont.		0.021			0.027	
		(0.052)			(0.052)	
100 - 110 % Bin			0.478			0.326
			(1.274)			(1.280)
110 - 120 % Bin			4.684^{**}			4.685^{**}
			(2.089)			(2.081)
Control Mean	56.948	56.894	56.948	56.948	56.894	56.948
Controls	YES	YES	YES	YES	YES	YES
FE	YES	YES	YES	YES	YES	YES
Treatment	Discrete	Continuous	Bins	Discrete	Continuous	Bins
N	4,876	4,876	4,876	4,876	4,876	4,876

Notes: Rank Child represents the ranking (percentile) of the child in the income distribution of its cohort. Treated is a dummy =1 if the parent is below the next SMW at month $t-1$ before the adjustment and above it at month t when the adjustment kicked in. Treated Cont. is % of the new SMW the parent won at time t. Bins variables indicate which bin the parent was in with his relative adjustment to the SMW. Control variables are parent and child sex. Fixed Effects are for parents (1950-1966), children's birth cohorts (1988-1996) and parents' industry sector at 4 digits (ISIC - 4). Standard errors in parentheses and clustered at the child level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

6.2.3 Main Results - Education and Labor Market Outcomes

With respect to the remaining outcomes found in 3, these children are less likely to attend university (around 6.8 p.p.), they enter the labor force at a younger age (around 0.3 years) and a noisy effect in regards to inheritance of employers of around 2.5 p.p. less likely to be at a parent's firm. When we use our second treatment definition we find that a 1 p.p. increase of the SMW earned has a negative impact of around 0.4 and 0.1 percentage points in the likelihood of a child enrolling in university and inheriting a parent's employer. In terms of entry to the labor market, an increase of the same magnitude anticipates the entry to the labor market by around 0.015 years.

The direction of these results can be interpreted in a bunch of different ways. First, we could argue that the small adjustments were not enough for parents to transmit their advantages to their children. When comparing to similar children, the monetary gains from the adjustments might not have been allocated to more resources for the children. A possible mechanism could be that the adjustment is too low and it is mainly received by households with a tight financial constraint. The gains from the increase in wages would be thus versed into more immediate necessities and not invested into children's education. While both can have an effect on later outcomes (e.g., better living conditions can create a more favorable environment for the development of children) the literature points that the latter would have the highest returns in terms of expected future earnings (Becker and Tomes, 1986).

Table 3: Main Estimates - Enters College, Age of Entry to the Labor Market and Inheritance of Employers - Immediate Treatment

	(1) Enters College	(2) Age Entry LM	(3) Same firm	(4) Enters College	(5) Age Entry LM	(6) Same firm	(7) Enters College	(8) Age Entry LM	(9) Same firm
Rank Parent	0.003***	0.005***	0.001***	0.003***	0.006***	0.001***	0.003***	0.005***	0.001***
	(0.000)	(0.002)	(0.000)	(0.000)	(0.002)	(0.000)	(0.000)	(0.002)	(0.000)
Treated	-0.068***	-0.295***	-0.025*						
	(0.015)	(0.101)	(0.014)						
Treat Cont.				-0.004***	-0.015***	-0.002**			
				(0.001)	(0.005)	(0.001)			
100 - 110 % Bin							-0.067***	-0.280**	-0.039**
							(0.017)	(0.113)	(0.015)
110 - 120 % Bin							-0.063**	-0.327*	-0.001
							(0.027)	(0.199)	(0.027)
Control Mean	0.235	20.035	0.147	0.220	19.981	0.141	0.235	20.034	0.147
Controls	YES	YES	YES	YES	YES	YES	YES	YES	YES
FE	YES	YES	YES	YES	YES	YES	YES	YES	YES
Treatment	Discrete	Discrete	Discrete	Continuous	Continuous	Continuous	Bins	Bins	Bins
N	4,876	4,876	4,876	4,876	4,876	4,876	4,876	4,876	4,876

Notes: Rank Child represents the ranking (percentile) of the child in the income distribution of its cohort. Treated is a dummy =1 if the parent is below the next SMW at month $t-1$ before the adjustment and above it at month t when the adjustment kicked in. Treated Cont. is % of the new SMW the parent won at time t. Bins variables indicate which bin the parent was in with his relative adjustment to the SMW. Control variables are parent and child sex. Fixed Effects are for parents (1950-1966), children's birth cohorts (1988-1996) and parents' industry sector at 4 digits (ISIC - 4). Standard errors in parentheses and clustered at the child level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Another possible explanation is that, although treated and control families had similar economic needs prior to the intervention, the income shock received by treated families was not large enough to substantially improve their living conditions with respect to the children in the control group. As a result, it may not have delayed children's entry into the labor market compared to the control group. When we take into consideration results from our fourth specification, we see that adjustments of a higher magnitude are likely to have an effect which is also very large in relative terms with respect to those in the other bins.

Three additional mechanisms are the possibility of the treatment making parents transfer their preferences for their employer, their abilities or their social capital to children (Anger and Heineck, 2009, Arrondel, 2009, Corak and Piraino, 2011, Staiger, 2022). This could have led the children of treated parents to enter the labor market earlier than those in the control group. Entering the labor market earlier on can have a positive impact in terms of the place these children occupy in the income distribution since they gather experience from an earlier age, which could lead for them to get better paid jobs at the same age than that of a person who delays their entry to the workforce. The counterbalancing factor could be the substitution effect between work and study, which could be the reason why those children enroll less than their counterparts in university. An interesting result is that we have no evidence - on aggregate terms - suggesting that children in the treatment group inherit employers more than controls do. A way of making sense of these results as a whole is that, when observing the positive income shock, parents develop stronger preferences and build a strong social network in those firms. Children could benefit from those social networks entering the labor market at a younger age and get better paying jobs from early on as shown in Staiger (2022). However, these networks can be "bad" since they would imply a trade-off between earnings and education and prevent them from getting better paying jobs in the future.

7 Robustness and Heterogeneity analysis

7.1 Sector-level

We now turn into analyzing the heterogeneous impacts of our strategy. This is shown throughout Tables C.5, C.6, C.7, C.8, and C.9.

For our sector level approach, older children (Table C.5) seem to be more affected by the treatment when it comes to explaining their placement in the income distribution. In this case, children would be around the age of entry to university when their parent becomes treated. This could be seen indirectly as an investment in human capital since the possible shock in parental income could mean that children do not have to go into the labor force and can thus invest more in their education, choosing to enroll in university. Though slim, treatment association with college enrollment is higher for older cohort than young ones, with 2.9 p.p. for the former and 2.3 p.p. for the latter (Tables C.5, C.6). Looking at Tables C.8 & C.9 we see that the treatment mainly impacts younger girls (both significantly and intensively) even when that control group is on average composed by children at the rightmost part of the income distribution (in comparison to the other three)[41].

College attendance seems to increase both for sons and daughters when the parent occupies a higher place in the income distribution. When considering the treatment status, the effect is similar for boys and girls (3 and 2.2 p.p. respectively). Older sons and younger daughters are on average 4.5 and 3 percentage

[41] When looking at the IRA division by cohort and sex, we see that this is similar for sons and daughters in both cases.

points more likely to enroll in university if their parent belongs to a strong sector. This is equal to the parent moving up to fifteen steps in the income distribution for the former and 8 in the latter even after controlling for each of the parent's university enrollment and completion status.

Young sons tend to enter the labor market earlier upon having a parent working in a strong sector (0.23 years before) and do not see their college attendance particularly affected. This said, we need to keep in mind that we take children who have somewhat a formal work history, so we cannot observe the full effect for people who have never entered the labor market. Thus, we do not observe an effect for the entire population. A possible mechanism can be that the treatment delays the entry to the labor market due to higher education decisions to an extent that we are not catching these sons because they have not yet entered the workforce. Despite this, we get a consistent result when we see that it's also the group with the higher share of employer inheritance, pointing out the mechanism for the possible transmission of preferences realizing for sons at a younger age.

When taking stricter samples as shown in Table C.3 we see how the incidence of the treatment points in the same direction as our main specification. However, these estimates lose power and become more noisy when taking these samples[42].

Finally, we run these sector-level results on the log of permanent income for children. The main result here is that treatment status increased the permanent income of the children with respect to the control group. This is consistent across sex and age. The overall effect is mainly driven by daughters on the aggregate level, but it tends to fade once we move up the income distribution. As with our rank outcome, this effect is also driven by older cohorts.

7.2 Individual-level

In the same fashion we perform robustness checks and observe if there exist heterogeneous effects by performing our analysis dividing our sample by age and sex. Additionally, we present evidence for our treatment effect when we define the treatment as a parent earning more than the SMW 6 months later the adjustment kicks in.

For our original treatment specification, when opening up these results by age, the main difference we find is that children from younger cohorts seem to be more affected by the treatment than older ones. The IRA for sons born between 1992 and 1996 is almost half of what it is for those born in 1988-1991 (Tables D.5 & D.6). This goes in line with the literature that states that the correlation is more sound once children occupy a stable place in the income distribution, which starts to happen around their 30's.

In terms of our treatment effect we still observe the strong effect for children with parents that are in the $[110\%;120\%]$ bin. The treatment effect is even stronger, with its magnitude being of about 6.6 places in the income distribution. When considering labor market and university results, although both (for older and younger children) go in the same direction as in our main specification, these are much stronger for the 1992 and 1996 cohorts. For children with parents in the $[110\%;120\%]$ bin, the previous can be an indication that the same mechanism as in the previous section could be at work: younger entry to the labor market makes for more work experience and the access to better salaries at a younger age, though it is detrimental when considering higher education decisions since there is a clear tradeoff between working and studying. The fact that the effect is stronger for younger children could point out

[42] This can be due in part to a composition effect, since the share of younger children is higher in our stricter samples. In turn, these children are the ones that were less affected by their parent's treatment status, so the smaller and less significant effect could be explained - partly - due to this channel.

that for older cohorts, the returns to higher education are playing a more important role and these people start to catch up with the ones that have been in the labor market for longer. The confounding factor here can be that educational decisions are not entirely made in the younger cohorts, or, as mentioned before, that the treatment can delay the entry to the labor market to the extent that we do not yet observe some of the younger children in our formal earnings records.

Boys that belong in the treatment group are around 1.5 times more prone to not enter university than girls in the same status (the effect being -0.079 for the former and -0.049 for the latter), however, with very different control means, where we see that in our sample boys are significantly more likely to enroll in university than girls.

We get null results across the board when taking our alternate treatment definition (Tables D.7 and D.8) [43], in that the treatment had no effect on children when examining their place in the income distribution of their cohort. However, in terms of our other results we still find a negative effect on university enrollment and the treatment anticipating a child's age of entry to the labor market.

The composition of the university effect still stands for the distinction between sons and daughters. Taking the treatment when observing wages 6 months after the adjustment still shows that sons are less likely (around 10 p.p.) to enroll in university. This effect is stronger than the one found in table D.10. When opening up results in our bins specification, the effect for sons is evenly distributed across the $[100-110\%)$ and $[110-120\%]$ bins with around 10.5 percentage points for each, while for daughters, it is mainly focused on the latter.

Lastly, neither young nor old cohorts' place in the income distribution seems to be affected by the parent's treatment status when taking this alternate definition. They share similar features when considering university enrollment and age of entry to the labor market. Older children's educational trajectories seem to be more affected by the treatment, but their younger counterparts enter the labor force at a younger age (both in treatment effect and considering the control mean). For both groups, the effect on university enrollment seems to be evenly distributed by treatment bins. Finally, across both groups, sons of richer parents seem to inherit employers at a slightly higher rate, however, when considering our treatment there is no evidence on the inheritance of employers. This combined with our previous results on this variable makes up for an argument that wage bargaining does not appear to have made a child more likely to enter a company where his parents have worked.

8 Conclusion and further expansions

In this document we try to shed some light into the impact of the early stages of tripartite collective wage bargaining on intergenerational mobility for children born around the turn of the millennia. This represents the first attempt of linking these subjects in the Uruguayan context.

We have proposed two strategies to tackle this question. Our sectoral approach shows robust evidence that children who are born with parents working in a sector with a strong collective bargaining unit - defined as negotiating wages above the national average increase around a time where these increases were being big - are benefited in terms of the place they'll occupy in the income distribution and their likelihood to enroll in university. Although evidence points to the fact that they enter the labor force at an earlier age, we do not have enough statistical power to make this claim for the entirety of our

[43] That is, parents who earn more than the sectoral minimum wage at month $t+6$ after the adjustment kicks in.

sample. This previous result is driven by sons, especially those born between 1992 - 1996. Both sons and daughters across both of our cohort divisions are on average more likely to ever work in a firm where one of their parents has worked.

This first approach is helpful to see the association between mobility and the characteristics of parents' labor environments, although it has some limitations, the main one being that not all parents who are in a strong sector get the same adjustment (and the opposite for parents in the control group). Moreover, the possible role played by the transmission of preferences to children - among others - do not let us claim causality for this first strategy.

In our second strategy, we address this issue by defining a treatment at the individual level, focusing on a subsample from our initial analysis group. Exploiting the fact that each sector has a different minimum wage, we take workers that are below that threshold before the adjustment comes to play and see their increase the month after. We leverage them with some workers that do not get the adjustment right away and see the effect on the same outcomes for their children. We find some noisy though non-robust evidence that collective wage bargaining has a positive impact on children's upward intergenerational mobility. This effect seems to be driven by the cohorts born between 1992 and 1996, which constitute our younger part of the sample. It makes sense for these spillovers to be concentrated in this part of the sample since that is when children are more dependent on their household income and are not yet in the age of contributing to it at the time of the negotiations.

This strategy has some limitations. The first one being power. We do not have enough observations and variation to draw more statistically robust effects. The second one is time. Ideally we would like to observe children at a young age and catch them later in life when their place in the income distribution and their educational choices are made once and for all. We cannot have both at the same time. The literature in intergenerational mobility usually states that a person's place in the income distribution is set around the ages of 30 to 39 years of age, meanwhile the literature on spillovers on children says that these effects are more important for children around their first decade of life. We do not have these children at the desired ages. For the younger cohort (1992-1996) we can observe them at this early stage but we observe their earnings at a moment where they're not set in the labor market. On the opposing side, the older cohorts were already at an age where they could enter the labor market at the time of the adjustments. Third, our strategy relies on some assumptions we cannot fully account for with our available data and research design. Finally, we may be incorporating the effect of the negotiations via two channels since we measure parent's permanent income in the same period during which we define treatment status.

Studying the channels through which labor institutions influence intergenerational mobility could have some key implications for public policy design, giving policymakers a more complete set of tools to foresee the long term effects of their policies. As a possible example, if there are firms that fail to comply with the agreements established through collective bargaining, this could have negative effects on the workers who do not receive those raises. This could provide an additional incentive for policymakers to strengthen their oversight and enforcement of the agreements.

Future editions of this work may help to enhance our understanding of the impact of collective bargaining on future generations. These could strengthen the aspects where this study falls short. Over time, it will be possible to take more accurate measures of the income of the children who were most affected by the adjustments made to their parents, whether because their position in the income distribution became more settled or because they completed their educational decisions, among other factors. In more recent rounds the agreements have established non-wage related clauses directed to different populations. This line of study would benefit substantially from taking these aspects into account. Our exercise represents an initial effort to contribute evidence and combine two branches of literature already studied in Uruguayan economics.

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Appendix

A Descriptives

Table A.1: Descriptives Children by birth cohort

	% Fem.	Mean Siblings	P25 Siblings	P50 Siblings	P75 Siblings	Cohort Size
1988	0.511	0.650	0	0	1	21,005
1989	0.504	0.638	0	0	1	20,854
1990	0.511	0.657	0	0	1	22,965
1991	0.514	0.666	0	1	1	23,145
1992	0.517	0.695	0	1	1	22,101
1993	0.518	0.707	0	1	1	21,834
1994	0.512	0.702	0	1	1	20,567
1995	0.517	0.688	0	1	1	19,252
1996	0.519	0.732	0	1	1	17,318
Total	0.514	0.680	0	1	1	189,041

Figure A.1: Group Composition Children

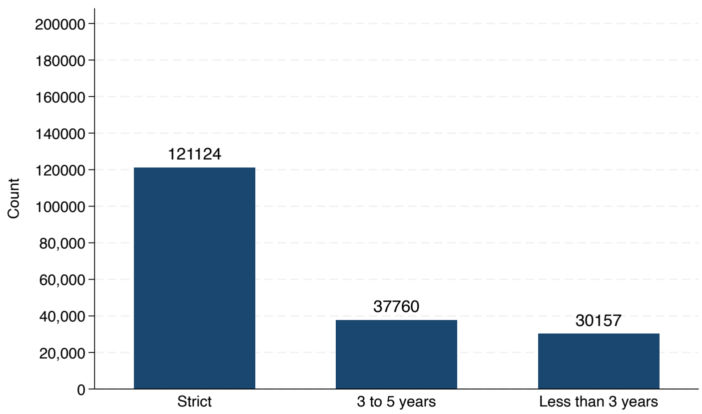


Figure A.2: Group Composition Parents

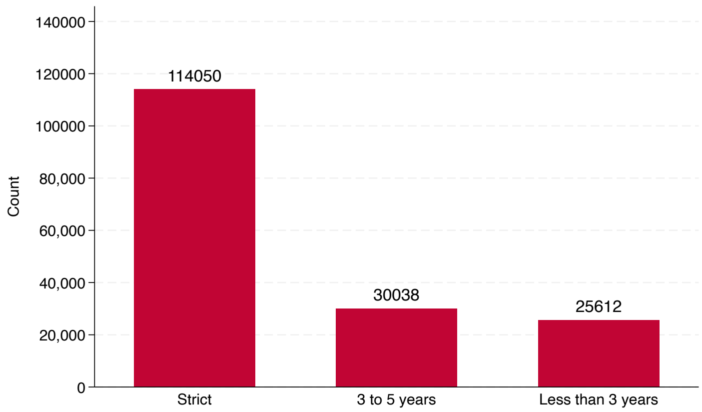


Figure A.3: Earnings distribution for the Children's cohorts

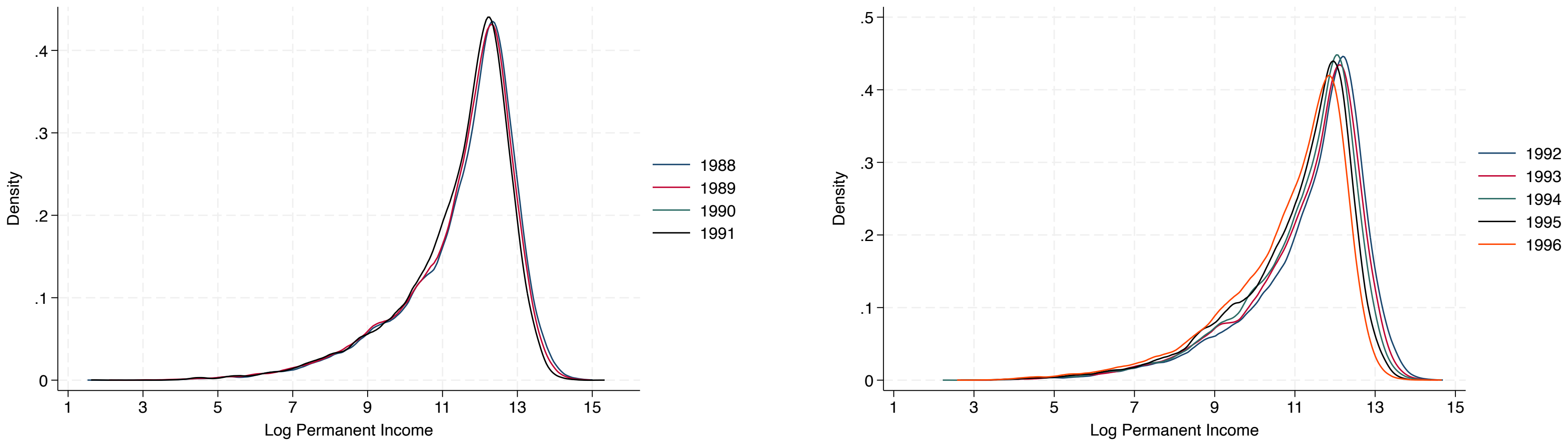


Figure A.4: Earnings distribution for the Parent's cohorts

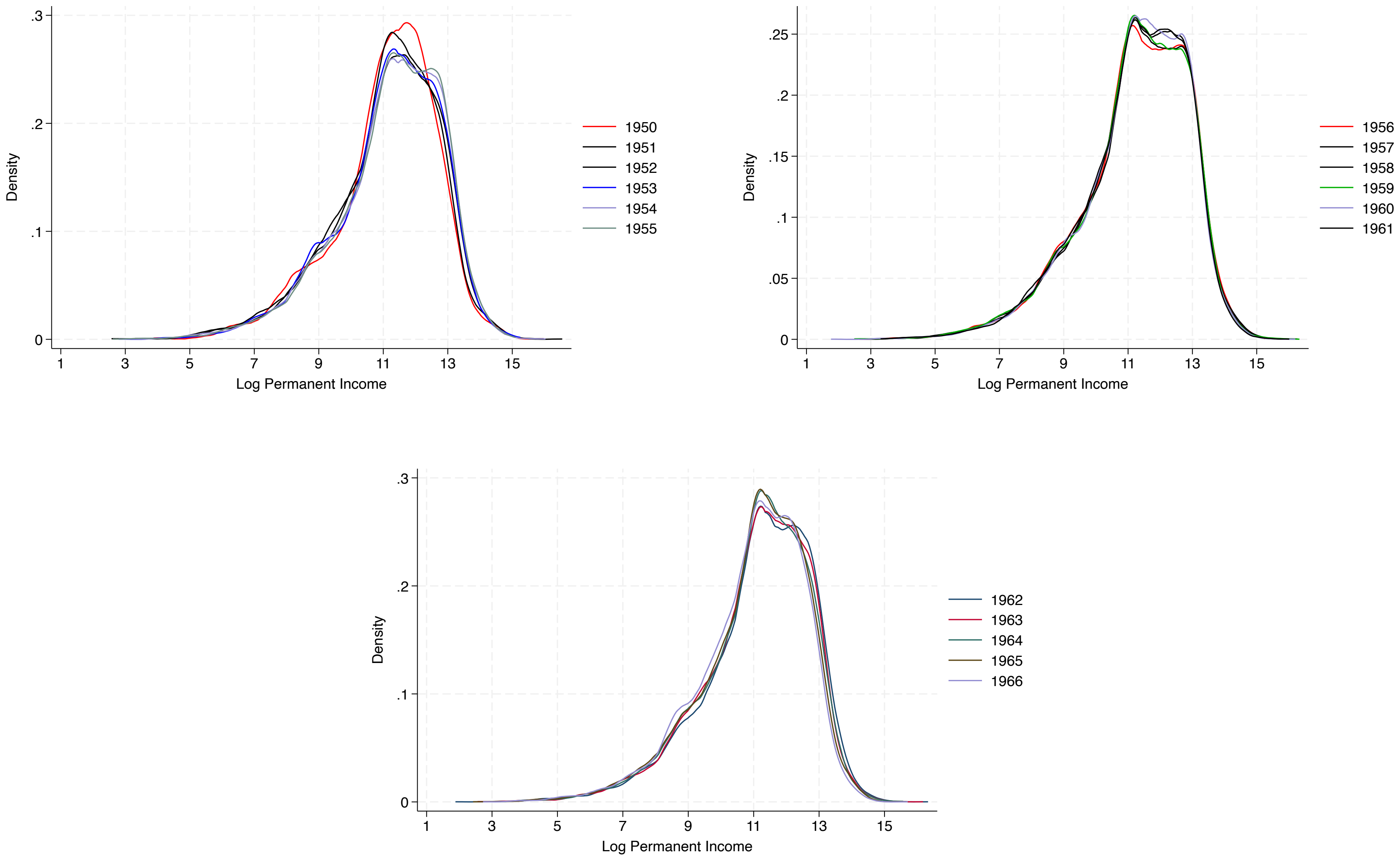


Figure A.5: Expected Child Rank given Parent Rank

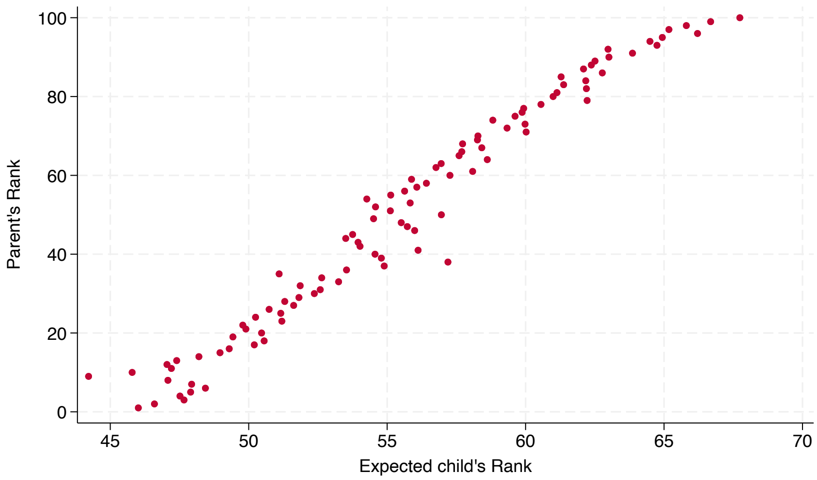


Figure A.6: Expected Son Rank given Parent Rank

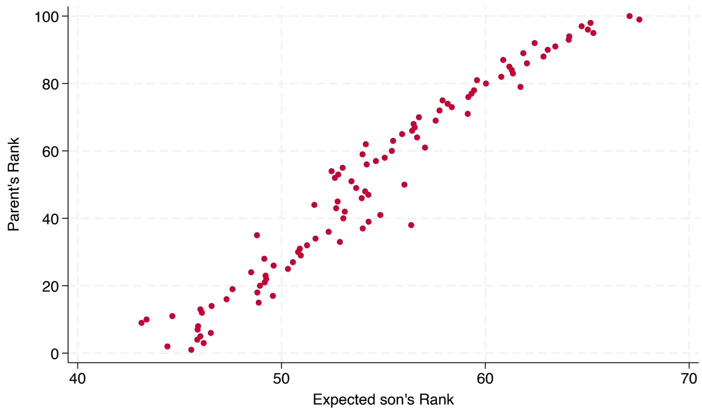


Figure A.7: Expected Daughter Rank given Parent Rank

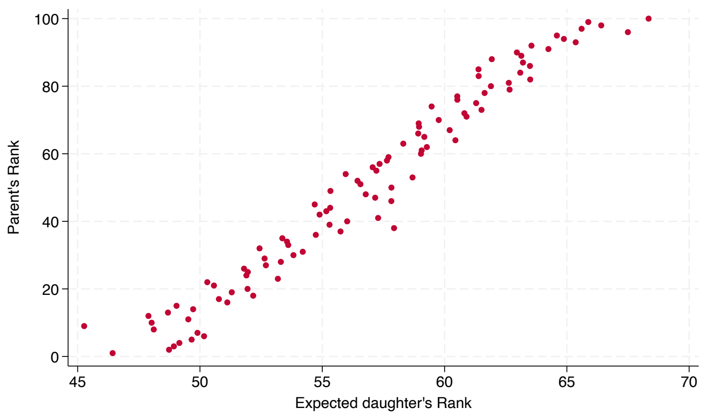


Figure A.8: Probability of Child passing Parent's rank given percentile of birth

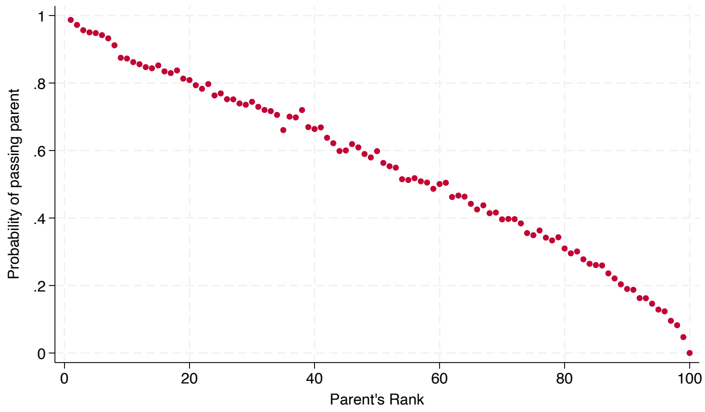


Figure A.9: Probability of Son passing Parent's rank given percentile of birth

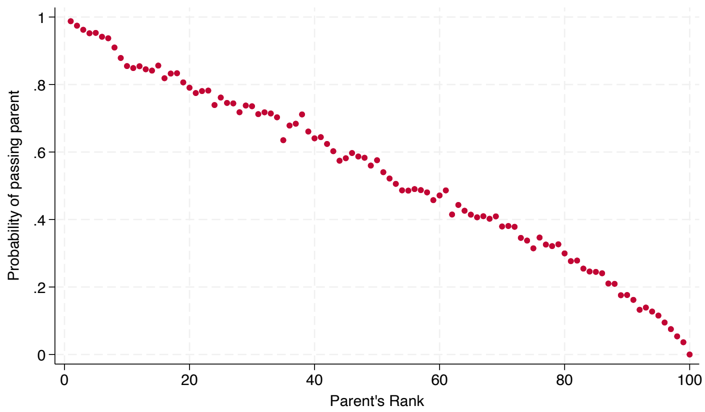


Figure A.10: Probability of Daughter passing Parent's rank given percentile of birth

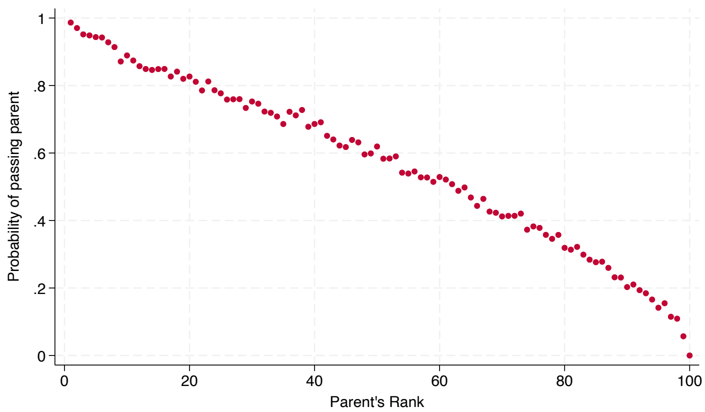


Figure A.11: Age of Entry to the Labor Market by Cohort

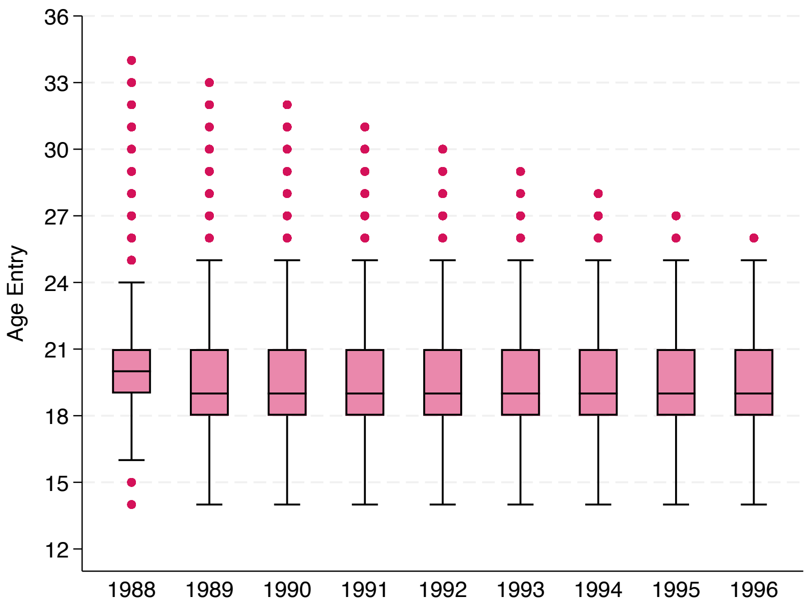


Table A.2: Descriptives of Children outcomes by birth cohort

	Mean log(perm inc)	P25 log(perm inc)	P50 log(perm inc)	P75 log(perm inc)	Mean Entry Age	P25 Entry Age	P50 Entry Age	P75 Entry Age	Mean Enter Univ.	Mean Same Company
1988	11.534	10.871	11.967	12.549	20.320	19	20	21	0.185	0.099
1989	11.462	10.799	11.897	12.487	20.179	18	19	21	0.188	0.101
1990	11.487	10.864	11.889	12.469	20.123	18	19	21	0.202	0.102
1991	11.406	10.779	11.830	12.414	20.042	18	19	21	0.212	0.104
1992	11.307	10.658	11.740	12.332	19.970	18	19	21	0.210	0.107
1993	11.193	10.512	11.621	12.235	19.941	18	19	21	0.213	0.104
1994	11.111	10.426	11.539	12.142	19.929	18	19	21	0.219	0.107
1995	10.978	10.288	11.412	12.031	19.961	18	19	21	0.228	0.112
1996	10.796	10.062	11.219	11.887	20.020	18	19	21	0.242	0.111
Total	11.269	10.586	11.684	12.305	20.055	18	19	21	0.210	0.105

Table A.3: Summary Statistics for Sector-level treatment sample

Variable	Mean	Std. Dev.	Min.	Max.	N
Panel A: Children					
Graduates College	0.212	0.409	0	1	79527
Enters College	0.009	0.092	0	1	79527
Age Entry Labor Market	19.993	2.446	14	34	79527
Same firm	0.115	0.319	0	1	79527
log(perminc child)	11.343	1.488	-6.789	15.332	79527
Father Strong	0.216	0.412	0	1	79527
Mother Strong	0.29	0.454	0	1	79527
Both Parents Strong	0.033	0.178	0	1	79527
Sex	0.512	0.5	0	1	79527
Strict	0.658	0.474	0	1	79527
3 to 5 years	0.195	0.397	0	1	79527
Less than 3 years	0.146	0.354	0	1	79527
Panel B: Parents					
Parent Enters College	0.056	0.23	0	1	63389
Parent Graduates College	0.024	0.154	0	1	63389
log(perminc parent)	11.296	1.478	1.762	16.55	63389
Sex Parent	0.516	0.5	0	1	63389
Strict	0.773	0.419	0	1	63389
3 to 5 years	0.161	0.368	0	1	63389
Less than 3 years	0.065	0.247	0	1	63389
Strong	0.46	0.498	0	1	63389
Strong 1.25x	0.262	0.44	0	1	63389
Strong 1.5x	0.228	0.42	0	1	63389
Strong 2x	0.088	0.284	0	1	63389

Figure A.12: Parent Ranking density in each sample

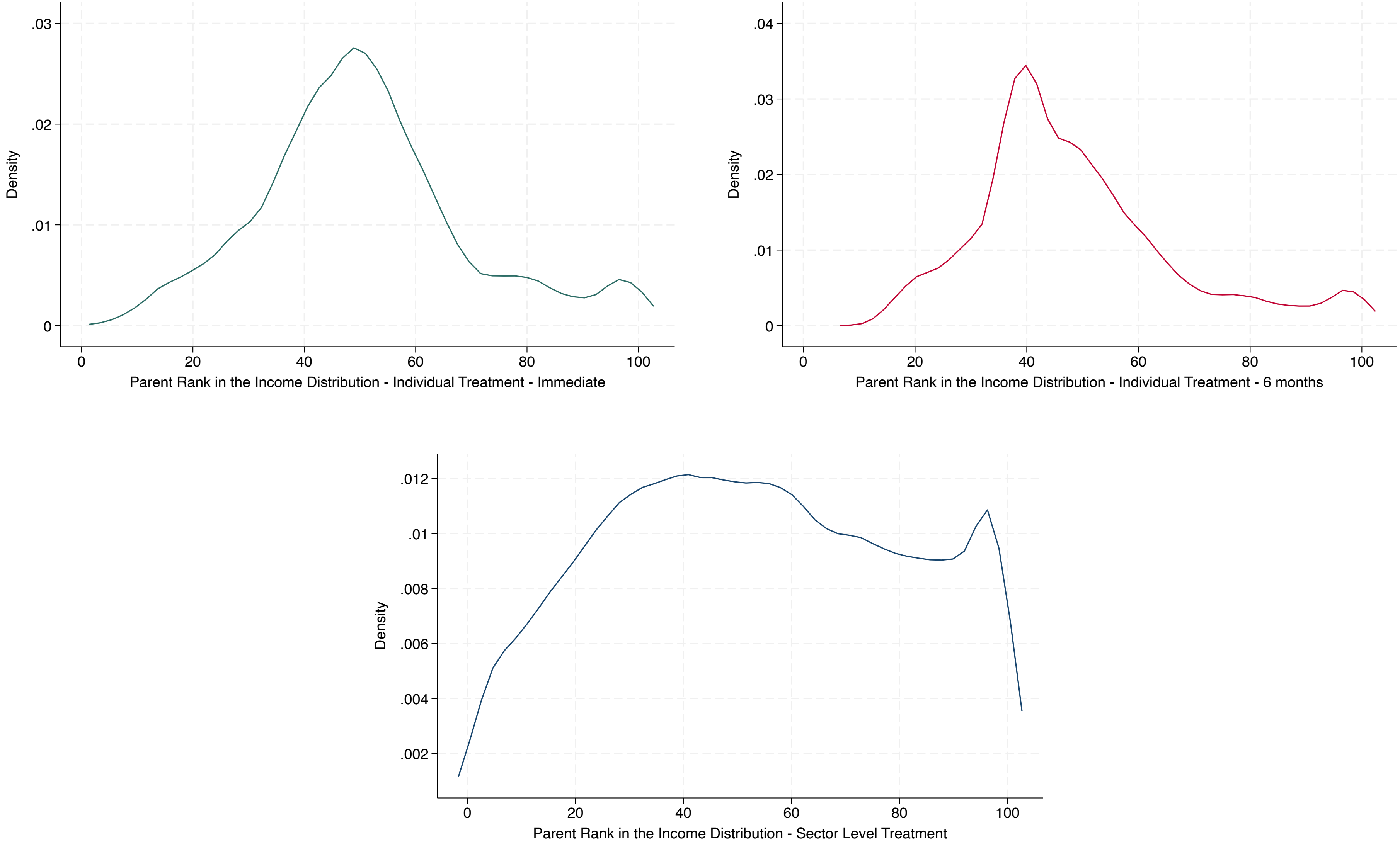


Figure A.13: Log average income in 2005 for parents in Strong vs Non-Strong Sectors

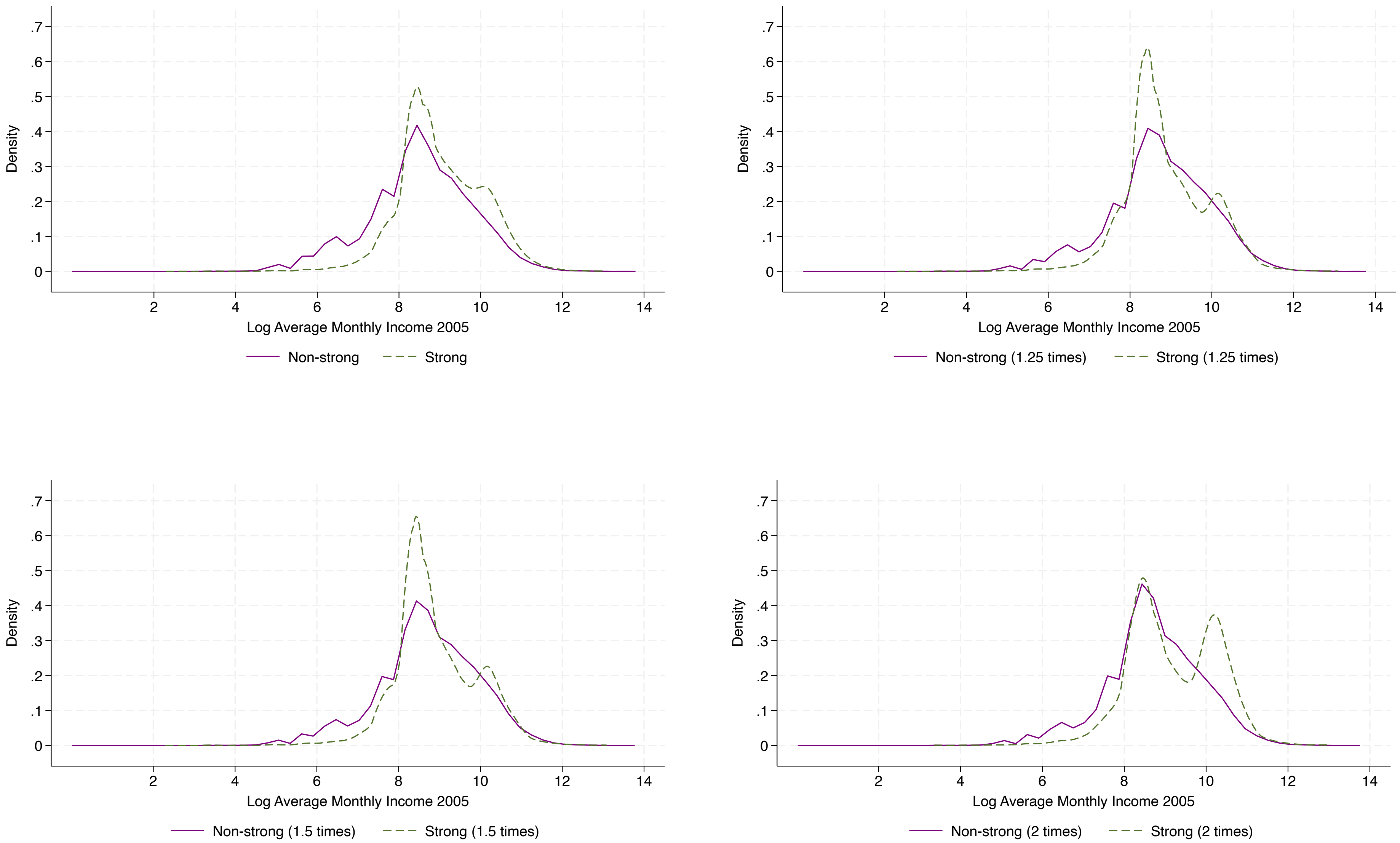
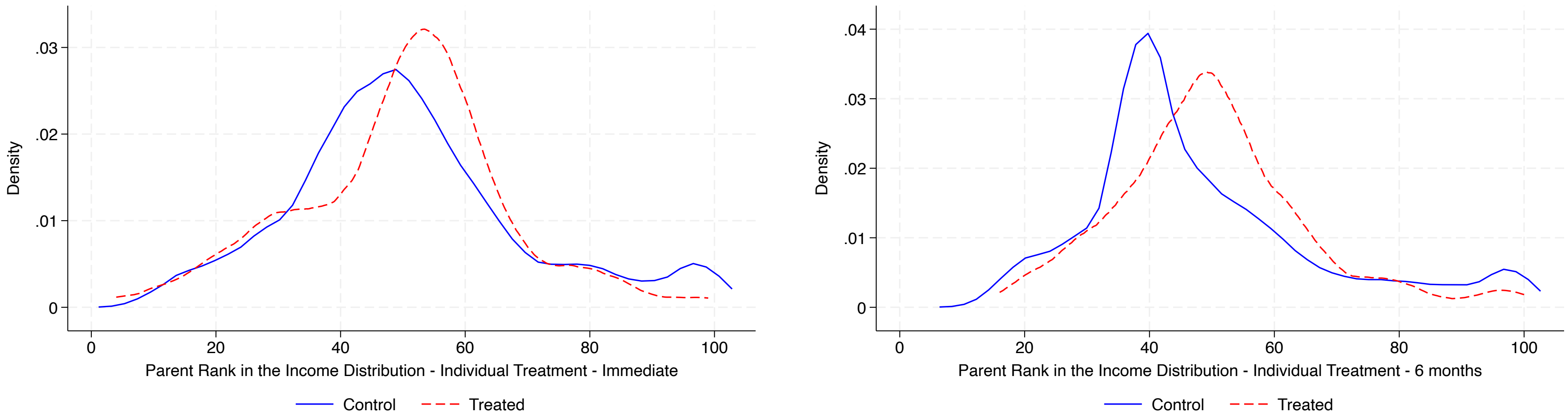


Figure A.14: Parent Ranking density by treatment status



B Treatment Status Composition of Wage Bargaining Groups

Table B.1: Treatment status by Wage Council Group

	N Subgroups	Median Adj.	Mean Adj.	Prop Trat	Prop Trat 1.25	Prop Trat 1.5	Prop Trat 2
1 Procesamiento y conservación de alimentos, bebidas y tabaco	14	11.25	13.52	0.57	0.21	0.21	0.07
2 Industria frigorífica	3	11.57	11.33	0.67	0.00	0.00	0.00
3 Pesca	1	11.63	11.63	1.00	0.00	0.00	0.00
4 Industria Textil	2	9.48	9.48	0.00	0.00	0.00	0.00
5 Industrias del Cuero, Vestimenta y Calzado	4	20.00	21.80	0.75	0.75	0.50	0.00
6 Industria de la madera, celulosa y papel	3	10.00	10.33	0.00	0.00	0.00	0.00
7 Industria química, del medicamento, farmacéutica, de combustibles y anexos	10	13.22	12.71	0.90	0.00	0.00	0.00
8 Industria de productos metálicos, maquinarias y equipo	38	10.61	10.76	0.18	0.00	0.00	0.00
9 Industria de la construcción y actividades complementarias	8	23.92	17.42	0.75	0.75	0.75	0.62
10 Comercio en general	23	13.44	15.31	0.74	0.48	0.48	0.00
11 Comercio minorista de alimentación	1	16.98	16.98	1.00	1.00	0.00	0.00
12 Hoteles, restoranes y bares	6	10.81	12.01	0.17	0.17	0.17	0.00
13 Transporte y Almacemiento	10	10.64	10.68	0.30	0.20	0.20	0.00
14 Intermediación finciera, seguros y pensiones	7	11.04	21.45	0.43	0.43	0.43	0.43
15 Servicios de salud y anexos	3	12.98	12.81	1.00	0.00	0.00	0.00
16 Servicios de enseñanza	1	6.37	6.37	0.00	0.00	0.00	0.00
17 Industria Gráfica	6	11.02	11.71	0.17	0.17	0.00	0.00
18 Servicios culturales, de esparcimiento y comunicaciones	3	11.03	539.49	0.33	0.33	0.33	0.33
19 Servicios profesionales, Técnicos Especializados	25	12.57	16.12	0.80	0.56	0.32	0.24
20 Entidades gremiales, sociales y deportivas	1	2.33	2.33	0.00	0.00	0.00	0.00
22 ganadería, Agricultura y actividades conexas	2	5.44	5.44	0.50	0.00	0.00	0.00
23 Viñedos, fruticultura, horticultura, floricultura, criaderos de aves, suinos, apicultura y otras	1	9.13	9.13	0.00	0.00	0.00	0.00

C Sector-level Treatment Estimates

Table C.1: Wage Bargaining effects on Parents Rank - Results by Sample Cutoff

	(1) All	(2) 3+ years	(3) 5 years	(4) log(perm inc)
Strong	12.590***	11.780***	9.290***	0.716***
	(0.194)	(0.217)	(0.251)	(0.010)
Sex Parent	15.301***	16.962***	17.209***	0.792***
	(0.195)	(0.218)	(0.253)	(0.011)
Parent Enter College	20.472***	22.347***	22.052***	1.024***
	(0.541)	(0.600)	(0.672)	(0.028)
Parent Graduates College	9.172***	10.307***	10.812***	0.582***
	(0.817)	(0.908)	(1.007)	(0.044)
Control Mean	46.645	41.958	41.511	10.929
N	63,389	59,238	49,009	63,389

Notes: Strong is a dummy= 1 if the parent is in a subgroup that negotiates above the average salary index increase. Rank Parent all, 3 and 5 years refer to the parent's position in the income distribution, calculated for those individuals who have positive income for at least a year, 3 or more years and 5 years in their work history, respectively. *log(perminc)* represents the logarithm of the parent's permanent income. Standard errors in parentheses and clustered at the parent level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table C.2: Wage Bargaining effects on Parents Rank - Results by Treatment Cutoff

	(1) Rank Parent	(2) Rank Parent	(3) Rank Parent	(4) Rank Parent	(5) log(perm inc)	(6) log(perm inc)	(7) log(perm inc)	(8) log(perm inc)
Strong	12.640***				0.719***			
	(0.196)				(0.011)			
Sex Parent	16.434***	17.428***	17.375***	17.345***	0.850***	0.904***	0.901***	0.902***
	(0.198)	(0.203)	(0.203)	(0.202)	(0.011)	(0.011)	(0.011)	(0.011)
Father Enter College	21.174***	22.091***	22.004***	21.683***	1.046***	1.098***	1.091***	1.076***
	(0.703)	(0.701)	(0.701)	(0.695)	(0.035)	(0.035)	(0.035)	(0.035)
Father Graduates College	10.393***	10.517***	10.508***	9.959***	0.563***	0.571***	0.570***	0.540***
	(1.092)	(1.077)	(1.078)	(1.078)	(0.056)	(0.055)	(0.055)	(0.055)
Strong 1.25x		2.413***				0.215***		
		(0.217)				(0.011)		
Strong 1.5x			3.454***				0.264***	
			(0.227)				(0.011)	
Strong 2x				9.401***				0.504***
				(0.348)				(0.017)
Control Mean	46.645	52.367	52.156	52.193	10.929	11.232	11.225	11.245
N	63389	63389	63389	63389	63389	63389	63389	63389

Notes: Strong is a dummy =1 if the parent is in a subgroup that negotiates above IMS increase. Strong 1.25, 1.5 and 2 are a dummies =1 if the parent is in a subgroup that negotiates at least 25%, 50% or 100% above respectively. Standard errors in parentheses clustered at the parent level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table C.3: Son Rank - Estimations by Sample

	(1) Rank Child All	(2) Rank Child 3 years	(3) Rank Child 5 years
Rank Parent	0.204***		
	(0.005)		
Strong	1.292***	1.011**	0.875*
	(0.455)	(0.446)	(0.520)
Strong × Rank Parent	-0.011		
	(0.007)		
Rank Parent 3+		0.173***	
		(0.005)	
Strong × Rank Parent 3+		-0.004	
		(0.008)	
Rank Parent 5			0.162***
			(0.007)
Strong × Rank Parent 5			-0.005
			(0.009)
Control Mean	55.042	53.654	53.550
N	88,037	70,670	46,147

Notes: Strong is a dummy =1 if the parent is in a subgroup that negotiates above IMS increase. Rank Parent (Child) all, 3 and 5 years refer to the parent's position in the income distribution, calculated for those individuals who have positive income for at least a year, 3 or more years and 5 years in their work history, respectively. Standard errors in parentheses clustered at the child level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table C.4: Pooled Main Estimates: Rank, education, entry to the labor market and same firm

	(1) Rank Child	(2) Enters College	(3) Age Entry LM	(4) Same firm
Rank Parent	0.200***	0.004***	0.008***	0.001***
	(0.004)	(0.000)	(0.000)	(0.000)
Strong	0.702***	0.010***	0.033*	0.013***
	(0.197)	(0.003)	(0.017)	(0.002)
Control Mean	55.042	0.189	19.928	0.111
N	88,037	88,037	88,037	88,037

Notes: Rank Child represents the ranking (percentile) of the child in the income distribution of its cohort. Enters College and Same Firm are a dummies =1 if the child ever enrolls in public university or works at a firm where one of their parents has ever worked. Age Entry LM represents the age (in years) in which the child gets their first job. Strong is a dummy =1 if the parent is in a subgroup that negotiates above the average salary index increase. Control variables are parent and children sex. Fixed Effects are for parents (1950-1966) and children's birth cohorts (1988-1996). Standard errors in parentheses and Clustered at the child level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table C.5: Rank, education, entry to the labor market and same firm - Children born between 1988 - 1991

	(1) Rank Child	(2) Enters College	(3) Age Entry LM	(4) Same firm
Rank Parent	0.230***	0.003***	0.005***	0.001***
	(0.007)	(0.000)	(0.001)	(0.000)
Strong	1.297**	0.029***	-0.043	0.046***
	(0.653)	(0.009)	(0.061)	(0.007)
Strong ×	-0.008	-0.000*	0.002*	-0.001***
Rank Parent	(0.011)	(0.000)	(0.001)	(0.000)
Control Mean	54.467	0.170	20.021	0.105
N	41,928	41,928	41,928	41,928

Notes: Rank Child represents the ranking (percentile) of the child in the income distribution of its cohort. Enters College and Same Firm are a dummies =1 if the child ever enrolls in public university or works at a firm where one of their parents has ever worked. Age Entry LM represents the age (in years) in which the child gets their first job. Strong is a dummy =1 if the parent is in a subgroup that negotiates above the average salary index increase. Control variables are parent and children sex. Fixed Effects are for parents (1950-1966) and children's birth cohorts (1988-1991). Standard errors in parentheses and clustered at the child level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table C.6: Rank, education, entry to the labor market and same firm - Children born between 1992 - 1996

	(1) Rank Child	(2) Enters College	(3) Age Entry LM	(4) Same firm
Rank Parent	0.181^{***}	0.004^{***}	0.008^{***}	0.001^{***}
	(0.007)	(0.000)	(0.001)	(0.000)
Strong	1.125^{*}	0.023^{***}	-0.102^{*}	0.077^{***}
	(0.634)	(0.009)	(0.054)	(0.007)
Strong ×	-0.011	-0.000^{*}	0.002^{**}	-0.001^{***}
Rank Parent	(0.010)	(0.000)	(0.001)	(0.000)
Control Mean	55.576	0.207	19.841	0.116
N	46,109	46,109	46,109	46,109

Notes: Rank Child represents the ranking (percentile) of the child in the income distribution of its cohort. Enters College and Same Firm are a dummies =1 if the child ever enrolls in public university or works at a firm where one of their parents has ever worked. Age Entry LM represents the age (in years) in which the child gets their first job. Strong is a dummy =1 if the parent is in a subgroup that negotiates above the average salary index increase. Control variables are parent and children sex. Fixed Effects are for parents (1950-1966) and children's birth cohorts (1992-1996). Standard errors in parentheses and Clustered at the child level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table C.7: Rank, education, entry to the labor market and same firm - By gender

	(1) Rank Child	(2) Enters College	(3) Age Entry LM	(4) Same firm	(5) Rank Child	(6) Enters College	(7) Age Entry LM	(8) Same firm
Rank Parent	0.207^{***}	0.004^{***}	0.004^{***}	0.001^{***}	0.202^{***}	0.003^{***}	0.010^{***}	0.001^{***}
	(0.007)	(0.000)	(0.001)	(0.000)	(0.007)	(0.000)	(0.001)	(0.000)
Strong	0.802	0.030^{***}	-0.189^{***}	0.064^{***}	1.770^{***}	0.022^{***}	0.029	0.061^{***}
	(0.638)	(0.010)	(0.061)	(0.007)	(0.648)	(0.008)	(0.053)	(0.008)
Strong ×	0.001	-0.000	0.003^{***}	-0.001^{***}	-0.022^{**}	-0.000^{**}	0.001	-0.001^{***}
Rank Parent	(0.010)	(0.000)	(0.001)	(0.000)	(0.011)	(0.000)	(0.001)	(0.000)
Control Mean	53.682	0.239	20.307	0.089	56.328	0.142	19.569	0.132
Gender	Sons	Sons	Sons	Sons	Daughters	Daughters	Daughters	Daughters
N	42,920	42,920	42,920	42,920	45,117	45,117	45,117	45,117

Notes: Rank Child represents the ranking (percentile) of the child in the income distribution of its cohort. Enters College and Same Firm are a dummies =1 if the child ever enrolls in public university or works at a firm where one of their parents has ever worked. Age Entry LM represents the age (in years) in which the child gets their first job. Strong is a dummy =1 if the parent is in a subgroup that negotiates above the average salary index increase. Control variables are parent sex. Fixed Effects are for parents (1950-1966) and children's birth cohorts (1988-1996). Standard errors in parentheses and Clustered at the child level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table C.8: Rank, education, entry to the labor market and same firm - Cohorts 1988 - 1991 By gender

	(1) Rank Child	(2) Enters College	(3) Age Entry LM	(4) Same firm	(5) Rank Child	(6) Enters College	(7) Age Entry LM	(8) Same firm
Rank Parent	0.230***	0.004***	0.003***	0.001***	0.230***	0.003***	0.008***	0.001***
	(0.010)	(0.000)	(0.001)	(0.000)	(0.010)	(0.000)	(0.001)	(0.000)
Strong	1.031	0.045***	-0.127	0.036***	1.558*	0.013	0.046	0.055***
	(0.909)	(0.014)	(0.092)	(0.010)	(0.938)	(0.011)	(0.079)	(0.011)
Strong ×	0.007	-0.000	0.002	-0.000**	-0.022	-0.000	0.001	-0.001***
Rank Parent	(0.015)	(0.000)	(0.001)	(0.000)	(0.016)	(0.000)	(0.001)	(0.000)
Control Mean	53.284	0.218	20.456	0.084	55.615	0.123	19.599	0.126
Gender	Sons	Sons	Sons	Sons	Daughters	Daughters	Daughters	Daughters
N	20,564	20,564	20,564	20,564	21,364	21,364	21,364	21,364

Notes: Rank Child represents the ranking (percentile) of the child in the income distribution of its cohort. Enters College and Same Firm are a dummies =1 if the child ever enrolls in public university or works at a firm where one of their parents has ever worked. Age Entry LM represents the age (in years) in which the child gets their first job. Strong is a dummy =1 if the parent is in a subgroup that negotiates above the average salary index increase. Control variables are parent sex. Fixed Effects are for parents (1950-1966) and children's birth cohorts (1988-1991). Standard errors in parentheses and Clustered at the child level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table C.9: Rank, education, entry to the labor market and same firm - Cohorts 1992 - 1996 By gender

	(1) Rank Child	(2) Enters College	(3) Age Entry LM	(4) Same firm	(5) Rank Child	(6) Enters College	(7) Age Entry LM	(8) Same firm
Rank Parent	0.184***	0.004***	0.005***	0.001***	0.177***	0.004***	0.011***	0.001***
	(0.009)	(0.000)	(0.001)	(0.000)	(0.009)	(0.000)	(0.001)	(0.000)
Strong	0.362	0.016	-0.234***	0.090***	1.873**	0.030***	0.015	0.065***
	(0.896)	(0.014)	(0.081)	(0.010)	(0.897)	(0.011)	(0.073)	(0.011)
Strong ×	0.000	-0.000	0.004***	-0.001***	-0.021	-0.000**	0.001	-0.001***
Rank Parent	(0.014)	(0.000)	(0.001)	(0.000)	(0.014)	(0.000)	(0.001)	(0.000)
Control Mean	54.060	0.259	20.166	0.093	56.973	0.158	19.542	0.138
Gender	Sons	Sons	Sons	Sons	Daughters	Daughters	Daughters	Daughters
N	22,356	22,356	22,356	22,356	23,753	23,753	23,753	23,753

Notes: Rank Child represents the ranking (percentile) of the child in the income distribution of its cohort. Enters College and Same Firm are a dummies =1 if the child ever enrolls in public university or works at a firm where one of their parents has ever worked. Age Entry LM represents the age (in years) in which the child gets their first job. Strong is a dummy =1 if the parent is in a subgroup that negotiates above the average salary index increase. Control variables are parent sex. Fixed Effects are for parents (1950-1966) and children's birth cohorts (1992-1996). Standard errors in parentheses and Clustered at the child level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table C.10: Permanent Income Sons on Parent's Rank

	(1) log(perminc)	(2) log(perminc)	(3) log(perminc)	(4) log(perminc)	(5) log(perminc)
Rank Parent	0.010***	0.010***	0.010***	0.011***	0.008***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Strong	0.094***	0.072**	0.116***	0.106***	0.075**
	(0.024)	(0.035)	(0.035)	(0.035)	(0.034)
Strong ×	-0.001***	-0.000	-0.002***	-0.001*	-0.001
Rank Parent	(0.000)	(0.001)	(0.001)	(0.001)	(0.001)
Control Mean	11.286	11.229	11.339	11.448	11.135
Sample	All	Sons	Daughters	88-91	92-96
N	88,037	42,920	45,117	41,928	46,109

Notes: log(perminc) represents the log of permanent income for children. Strong is a dummy =1 if the parent is in a subgroup that negotiates above the average salary index increase. Control variables are parent and child sex. Fixed Effects are for parents (1950-1966) and children's birth cohorts (1988-1996). Standard errors in parentheses and Clustered at the child level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

D Individual-level Treatment Estimates

Table D.1: t-tests for parent characteristics - Immediate Treatment

	Control	Treatment	Difference	Std. Error	N
Earnings Pre-treat	3513.68	3484.38	29.30	(66.04)	3519
Sex Parent	0.52	0.53	-0.01	(0.02)	3519
Cohort Parent	1960.24	1959.86	0.37*	(0.21)	3519

Notes: Treatment status is defined upon a dummy =1 if the parent is below the next SMW at month $t-1$ before the adjustment and above it at month t when the adjustment kicked in. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table D.2: t-tests for parent characteristics - 6 Month Treatment

	Control	Treatment	Difference	Std. Error	N
Earnings Pre-treat	3314.28	3517.80	-203.53***	(40.69)	3817
Sex Parent	0.50	0.47	0.03	(0.02)	3817
Cohort Parent	1960.03	1960.47	-0.44***	(0.15)	3817

Notes: Treatment status is defined upon a dummy =1 if the parent is below the next SMW at month $t-1$ before the adjustment and above it at month $t+6$ when the adjustment kicked in. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

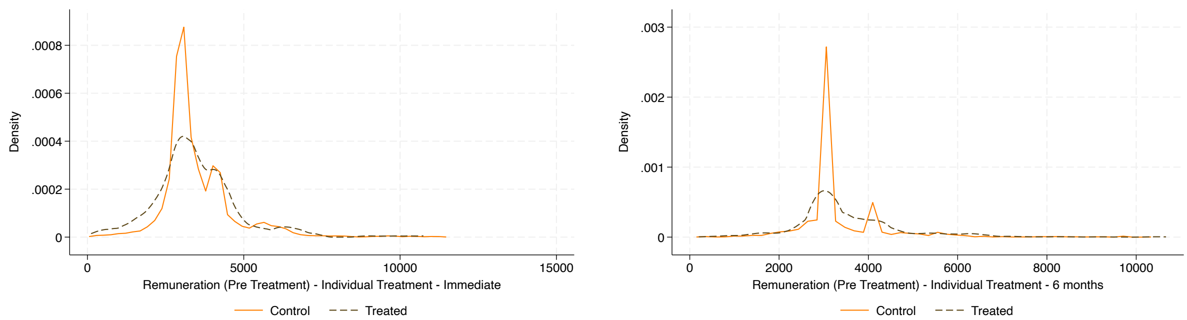


Figure D.1: Density for pre-treatment earnings by treatment status

Table D.3: Main Estimates - Immediate Treatment for Daughters

	(1) Rank Child	(2) Enters College	(3) Age Entry LM	(4) Same firm	(5) Rank Child	(6) Enters College	(7) Age Entry LM	(8) Same firm	(9) Rank Child	(10) Enters College	(11) Age Entry LM	(12) Same firm
Rank Parent	0.159***	0.003***	0.004	0.001*	0.160***	0.003***	0.004	0.001*	0.159***	0.003***	0.004	0.001*
	(0.035)	(0.001)	(0.003)	(0.000)	(0.035)	(0.001)	(0.003)	(0.000)	(0.035)	(0.001)	(0.003)	(0.000)
Treated	1.971	-0.049**	-0.355***	-0.014								
	(1.686)	(0.020)	(0.128)	(0.022)								
Treat Cont.					-0.006	-0.003***	-0.018***	-0.001				
					(0.077)	(0.001)	(0.006)	(0.001)				
100 - 110 % Bin									0.835	-0.047**	-0.328**	-0.026
									(1.860)	(0.022)	(0.140)	(0.025)
110 - 120 % Bin									5.449*	-0.062*	-0.400	0.006
									(3.144)	(0.035)	(0.281)	(0.042)
Control Mean	58.283	0.198	19.766	0.159	58.232	0.185	19.693	0.157	58.291	0.198	19.764	0.160
Controls	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
FE	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Treatment	Discrete	Discrete	Discrete	Discrete	Continuous	Continuous	Continuous	Continuous	Bins	Bins	Bins	Bins
N	2,429	2,429	2,429	2,429	2,429	2,429	2,429	2,429	2,429	2,429	2,429	2,429

Notes: Rank Child represents the ranking (percentile) of the child in the income distribution of its cohort. Enters College and Same Firm are a dummies =1 if the child ever enrolls in public university or works at a firm where one of their parents has ever worked. Age Entry LM represents the age (in years) in which the child gets their first job. Treated is a dummy =1 if the parent is below the next SMW at month $t-1$ before the adjustment and above it at month t when the adjustment kicked in. Treated Cont. is % of the new SMW the parent won at time t. Bins variables indicate which bin the parent was in with his relative adjustment to the SMW. Control variables are parent sex. Fixed Effects are for parents (1950-1966), children's birth cohorts (1988-1996) and parents' industry sector at 4 digits (ISIC - 4). Standard errors in parentheses and clustered at the child level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table D.4: Main Estimates - Immediate Treatment for Sons

	(1) Rank Child	(2) Enters College	(3) Age Entry LM	(4) Same firm	(5) Rank Child	(6) Enters College	(7) Age Entry LM	(8) Same firm	(9) Rank Child	(10) Enters College	(11) Age Entry LM	(12) Same firm
Rank Parent	0.189***	0.003***	0.008**	0.001**	0.188***	0.003***	0.008**	0.001**	0.189***	0.003***	0.008**	0.001**
	(0.034)	(0.001)	(0.003)	(0.000)	(0.034)	(0.001)	(0.003)	(0.000)	(0.034)	(0.001)	(0.003)	(0.000)
Treated	1.609	-0.079***	-0.221	-0.044**								
	(1.604)	(0.023)	(0.159)	(0.017)								
Treat Cont.					0.061	-0.004***	-0.012*	-0.002**				
					(0.073)	(0.001)	(0.007)	(0.001)				
100 - 110 % Bin									0.358	-0.081***	-0.194	-0.058***
									(1.796)	(0.026)	(0.178)	(0.018)
110 - 120 % Bin									4.524	-0.052	-0.299	-0.016
									(2.899)	(0.041)	(0.293)	(0.036)
Control Mean	55.617	0.272	20.303	0.134	55.566	0.255	20.266	0.125	55.608	0.272	20.304	0.135
Controls	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
FE	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Treatment	Discrete	Discrete	Discrete	Discrete	Continuous	Continuous	Continuous	Continuous	Bins	Bins	Bins	Bins
N	2,447	2,447	2,447	2,447	2,447	2,447	2,447	2,447	2,447	2,447	2,447	2,447

Notes: Rank Child represents the ranking (percentile) of the child in the income distribution of its cohort. Enters College and Same Firm are a dummies =1 if the child ever enrolls in public university or works at a firm where one of their parents has ever worked. Age Entry LM represents the age (in years) in which the child gets their first job. Treated is a dummy =1 if the parent is below the next SMW at month $t-1$ before the adjustment and above it at month t when the adjustment kicked in. Treated Cont. is % of the new SMW the parent won at time t. Bins variables indicate which bin the parent was in with his relative adjustment to the SMW. Control variables are parent sex. Fixed Effects are for parents (1950-1966), children's birth cohorts (1988-1996) and parents' industry sector at 4 digits (ISIC - 4). Standard errors in parentheses and clustered at the child level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table D.5: Main Estimates - Immediate Treatment for Cohorts 1988-1991

	(1) Rank Child	(2) Enters College	(3) Age Entry LM	(4) Same firm	(5) Rank Child	(6) Enters College	(7) Age Entry LM	(8) Same firm	(9) Rank Child	(10) Enters College	(11) Age Entry LM	(12) Same firm
Rank Parent	0.215***	0.002***	0.003	0.001**	0.215***	0.002***	0.003	0.001**	0.215***	0.002***	0.003	0.001**
	(0.034)	(0.001)	(0.003)	(0.000)	(0.035)	(0.001)	(0.003)	(0.000)	(0.034)	(0.001)	(0.003)	(0.000)
Treated	0.738	-0.039*	-0.215	-0.042**								
	(1.616)	(0.021)	(0.144)	(0.019)								
Treat Cont.					0.002	-0.002*	-0.011*	-0.002***				
					(0.074)	(0.001)	(0.007)	(0.001)				
100 - 110 % Bin									-0.017	-0.044*	-0.184	-0.040*
									(1.803)	(0.023)	(0.163)	(0.022)
110 - 120 % Bin									2.688	-0.016	-0.329	-0.066**
									(2.986)	(0.041)	(0.273)	(0.034)
Control Mean	56.082	0.211	20.124	0.150	56.034	0.200	20.070	0.141	56.075	0.211	20.125	0.150
Controls	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
FE	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Treatment	Discrete	Discrete	Discrete	Discrete	Continuous	Continuous	Continuous	Continuous	Bins	Bins	Bins	Bins
N	2,356	2,356	2,356	2,356	2,356	2,356	2,356	2,356	2,356	2,356	2,356	2,356

Notes: Rank Child represents the ranking (percentile) of the child in the income distribution of its cohort. Enters College and Same Firm are a dummies =1 if the child ever enrolls in public university or works at a firm where one of their parents has ever worked. Age Entry LM represents the age (in years) in which the child gets their first job. Treated is a dummy =1 if the parent is below the next SMW at month $t-1$ before the adjustment and above it at month t when the adjustment kicked in. Treated Cont. is % of the new SMW the parent won at time t. Bins variables indicate which bin the parent was in with his relative adjustment to the SMW. Control variables are parent and child sex. Fixed Effects are for parents (1950-1966), children's birth cohorts (1988-1991) and parents' industry sector at 4 digits (ISIC - 4). Standard errors in parentheses and clustered at the child level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table D.6: Main Estimates - Immediate Treatment for Cohorts 1992-1996

	(1) Rank Child	(2) Enters College	(3) Age Entry LM	(4) Same firm	(5) Rank Child	(6) Enters College	(7) Age Entry LM	(8) Same firm	(9) Rank Child	(10) Enters College	(11) Age Entry LM	(12) Same firm
Rank Parent	0.118^{***}	0.004^{***}	0.008^{***}	0.001^{*}	0.117^{***}	0.004^{***}	0.008^{***}	0.001^{*}	0.117^{***}	0.004^{***}	0.008^{***}	0.001^{*}
	(0.035)	(0.001)	(0.003)	(0.000)	(0.035)	(0.001)	(0.003)	(0.000)	(0.035)	(0.001)	(0.003)	(0.000)
Treated	2.493	-0.086^{***}	-0.373^{**}	-0.003								
	(1.672)	(0.022)	(0.146)	(0.020)								
Treat Cont.					0.056	-0.005^{***}	-0.020^{***}	-0.000				
					(0.075)	(0.001)	(0.007)	(0.001)				
100 - 110 % Bin									0.991	-0.076^{***}	-0.345^{**}	-0.031
									(1.865)	(0.026)	(0.159)	(0.022)
110 - 120 % Bin									6.588^{**}	-0.106^{***}	-0.428	0.068
									(3.001)	(0.034)	(0.299)	(0.042)
Control Mean	57.750	0.257	19.952	0.144	57.698	0.239	19.897	0.141	57.757	0.257	19.950	0.145
Controls	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
FE	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Treatment	Discrete	Discrete	Discrete	Discrete	Continuous	Continuous	Continuous	Continuous	Bins	Bins	Bins	Bins
N	2,520	2,520	2,520	2,520	2,520	2,520	2,520	2,520	2,520	2,520	2,520	2,520

Notes: Rank Child represents the ranking (percentile) of the child in the income distribution of its cohort. Enters College and Same Firm are a dummies =1 if the child ever enrolls in public university or works at a firm where one of their parents has ever worked. Age Entry LM represents the age (in years) in which the child gets their first job. Treated is a dummy =1 if the parent is below the next SMW at month $t-1$ before the adjustment and above it at month t when the adjustment kicked in. Treated Cont. is % of the new SMW the parent won at time t. Bins variables indicate which bin the parent was in with his relative adjustment to the SMW. Control variables are parent and child sex. Fixed Effects are for parents (1950-1966), children's birth cohorts (1992-1996) and parents' industry sector at 4 digits (ISIC - 4). Standard errors in parentheses and clustered at the child level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table D.7: Main Estimates - Rank in the Income Distribution Conditional and Unconditional on Parent Rank - 6 Month Treatment

	(1) Rank Child	(2) Rank Child	(3) Rank Child	(4) Rank Child	(5) Rank Child	(6) Rank Child
Rank Parent	0.159***	0.160***	0.160***			
	(0.026)	(0.027)	(0.026)			
Treated	0.249			0.526		
	(0.934)			(0.939)		
Treat Cont.		-0.006			0.029	
		(0.044)			(0.044)	
100 - 110 % Bin			0.154			0.297
			(1.059)			(1.067)
110 - 120 % Bin			0.055			0.647
			(1.351)			(1.349)
Control Mean	58.392	58.081	58.424	58.392	58.081	58.424
Controls	YES	YES	YES	YES	YES	YES
FE	YES	YES	YES	YES	YES	YES
Treatment	Discrete	Continuous	Bins	Discrete	Continuous	Bins
N	5,186	5,186	5,186	5,186	5,186	5,186

Notes: Rank Child represents the ranking (percentile) of the child in the income distribution of its cohort. Treated is a dummy =1 if the parent is below the next SMW at month $t-1$ before the adjustment and above it at month $t+6$ when the adjustment kicked in. Treated Cont. is % of the new SMW the parent won at time $t+6$. Bins variables indicate which bin the parent was in with his relative adjustment to the SMW. Control variables are parent and child sex. Fixed Effects are for parents (1950-1966), children's birth cohorts (1988-1996) and parents' industry sector at 4 digits (ISIC - 4). Standard errors in parentheses and clustered at the child level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table D.8: Main Estimates - Enters College, Age of Entry to the Labor Market and Inheritance of Employers - 6 Month Treatment

	(1) Enters College	(2) Age Entry LM	(3) Same firm	(4) Enters College	(5) Age Entry LM	(6) Same firm	(7) Enters College	(8) Age Entry LM	(9) Same firm
Rank Parent	0.003***	0.005**	0.001***	0.004***	0.006***	0.001***	0.003***	0.006**	0.001***
	(0.000)	(0.002)	(0.000)	(0.000)	(0.002)	(0.000)	(0.000)	(0.002)	(0.000)
Treated	-0.071***	-0.283***	-0.015						
	(0.014)	(0.081)	(0.012)						
Treat Cont.				-0.003***	-0.012***	-0.001*			
				(0.001)	(0.004)	(0.001)			
100 - 110 % Bin							-0.067***	-0.240**	-0.001
							(0.016)	(0.095)	(0.014)
110 - 120 % Bin							-0.075***	-0.334***	-0.037**
							(0.019)	(0.113)	(0.016)
Control Mean	0.279	20.283	0.152	0.254	20.166	0.148	0.279	20.281	0.152
Controls	YES	YES	YES	YES	YES	YES	YES	YES	YES
FE	YES	YES	YES	YES	YES	YES	YES	YES	YES
Treatment	Discrete	Discrete	Discrete	Continuous	Continuous	Continuous	Bins	Bins	Bins
N	5,186	5,186	5,186	5,186	5,186	5,186	5,186	5,186	5,186

Notes: Enters College and Same Firm are a dummies =1 if the child ever enrolls in public university or works at a firm where one of their parents has ever worked. Age Entry LM represents the age (in years) in which the child gets their first job. Treated is a dummy =1 if the parent is below the next SMW at month $t-1$ before the adjustment and above it at month $t+6$ when the adjustment kicked in. Treated Cont. is % of the new SMW the parent won at time $t+6$. Bins variables indicate which bin the parent was in with his relative adjustment to the SMW. Control variables are parent and child sex. Fixed Effects are for parents (1950-1966), children's birth cohorts (1988-1996) and parents' industry sector at 4 digits (ISIC - 4). Standard errors in parentheses and clustered at the child level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table D.9: Main Estimates - 6 Month Treatment for Daughters

	(1) Rank Child	(2) Enters College	(3) Age Entry LM	(4) Same firm	(5) Rank Child	(6) Enters College	(7) Age Entry LM	(8) Same firm	(9) Rank Child	(10) Enters College	(11) Age Entry LM	(12) Same firm
Rank Parent	0.091**	0.004***	0.006*	0.001***	0.095**	0.004***	0.006**	0.001***	0.092**	0.004***	0.006*	0.001***
	(0.038)	(0.001)	(0.003)	(0.000)	(0.039)	(0.001)	(0.003)	(0.000)	(0.038)	(0.001)	(0.003)	(0.000)
Treated	-1.310	-0.031*	-0.242**	-0.022								
	(1.338)	(0.018)	(0.108)	(0.017)								
Treat Cont.					-0.068	-0.002***	-0.012**	-0.001				
					(0.062)	(0.001)	(0.005)	(0.001)				
100 - 110 % Bin									-1.375	-0.019	-0.238*	-0.001
									(1.569)	(0.022)	(0.129)	(0.021)
110 - 120 % Bin									-1.808	-0.053**	-0.220	-0.055**
									(1.872)	(0.024)	(0.143)	(0.022)
Control Mean	59.735	0.208	20.005	0.165	58.940	0.195	19.887	0.159	59.800	0.208	20.003	0.165
Controls	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
FE	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Treatment	Discrete	Discrete	Discrete	Discrete	Continuous	Continuous	Continuous	Continuous	Bins	Bins	Bins	Bins
N	2,582	2,582	2,582	2,582	2,582	2,582	2,582	2,582	2,582	2,582	2,582	2,582

Notes: Rank Child represents the ranking (percentile) of the child in the income distribution of its cohort. Enters College and Same Firm are a dummies =1 if the child ever enrolls in public university or works at a firm where one of their parents has ever worked. Age Entry LM represents the age (in years) in which the child gets their first job. Treated is a dummy =1 if the parent is below the next SMW at month $t-1$ before the adjustment and above it at month $t+6$ when the adjustment kicked in. Treated Cont. is % of the new SMW the parent won at time $t+6$. Bins variables indicate which bin the parent was in with his relative adjustment to the SMW. Control variables are parent sex. Fixed Effects are for parents (1950-1966), children's birth cohorts (1988-1996) and parents' industry sector at 4 digits (ISIC - 4). Standard errors in parentheses and clustered at the child level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table D.10: Main Estimates - 6 Month Treatment for Sons

	(1) Rank Child	(2) Enters College	(3) Age Entry LM	(4) Same firm	(5) Rank Child	(6) Enters College	(7) Age Entry LM	(8) Same firm	(9) Rank Child	(10) Enters College	(11) Age Entry LM	(12) Same firm
Rank Parent	0.237***	0.003***	0.006*	0.001**	0.235***	0.003***	0.006*	0.001***	0.237***	0.003***	0.006*	0.001**
	(0.037)	(0.001)	(0.003)	(0.000)	(0.038)	(0.001)	(0.003)	(0.000)	(0.037)	(0.001)	(0.003)	(0.000)
Treated	1.562	-0.109***	-0.316***	-0.005								
	(1.326)	(0.021)	(0.121)	(0.017)								
Treat Cont.					0.052	-0.005***	-0.013**	-0.001				
					(0.063)	(0.001)	(0.006)	(0.001)				
100 - 110 % Bin									1.463	-0.106***	-0.247*	-0.001
									(1.471)	(0.023)	(0.139)	(0.019)
110 - 120 % Bin									1.667	-0.102***	-0.433**	-0.010
									(1.990)	(0.031)	(0.179)	(0.024)
Control Mean	57.033	0.351	20.564	0.140	57.228	0.311	20.444	0.138	57.031	0.351	20.563	0.140
Controls	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
FE	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Treatment	Discrete	Discrete	Discrete	Discrete	Continuous	Continuous	Continuous	Continuous	Bins	Bins	Bins	Bins
N	2,604	2,604	2,604	2,604	2,604	2,604	2,604	2,604	2,604	2,604	2,604	2,604

Notes: Rank Child represents the ranking (percentile) of the child in the income distribution of its cohort. Enters College and Same Firm are a dummies =1 if the child ever enrolls in public university or works at a firm where one of their parents has ever worked. Age Entry LM represents the age (in years) in which the child gets their first job. Treated is a dummy =1 if the parent is below the next SMW at month $t-1$ before the adjustment and above it at month $t+6$ when the adjustment kicked in. Treated Cont. is % of the new SMW the parent won at time $t+6$. Bins variables indicate which bin the parent was in with his relative adjustment to the SMW. Control variables are parent sex. Fixed Effects are for parents (1950-1966), children's birth cohorts (1988-1996) and parents' industry sector at 4 digits (ISIC - 4). Standard errors in parentheses and clustered at the child level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table D.11: Main Estimates - 6 Month Treatment for Cohorts 1988-1991

	(1) Rank Child	(2) Enters College	(3) Age Entry LM	(4) Same firm	(5) Rank Child	(6) Enters College	(7) Age Entry LM	(8) Same firm	(9) Rank Child	(10) Enters College	(11) Age Entry LM	(12) Same firm
Rank Parent	0.223***	0.003***	0.007*	0.001**	0.226***	0.003***	0.007**	0.001**	0.224***	0.003***	0.007*	0.001**
	(0.039)	(0.001)	(0.004)	(0.001)	(0.040)	(0.001)	(0.004)	(0.001)	(0.039)	(0.001)	(0.004)	(0.001)
Treated	0.481	-0.064***	-0.353***	-0.016								
	(1.374)	(0.019)	(0.121)	(0.017)								
Treat Cont.					-0.012	-0.003***	-0.017***	-0.001				
					(0.066)	(0.001)	(0.005)	(0.001)				
100 - 110 % Bin									0.768	-0.082***	-0.279*	-0.008
									(1.542)	(0.021)	(0.145)	(0.020)
110 - 120 % Bin									-0.124	-0.032	-0.457***	-0.036
									(2.029)	(0.028)	(0.160)	(0.023)
Control Mean	57.548	0.257	20.411	0.153	57.518	0.232	20.259	0.146	57.563	0.257	20.410	0.154
Controls	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
FE	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Treatment	Discrete	Discrete	Discrete	Discrete	Continuous	Continuous	Continuous	Continuous	Bins	Bins	Bins	Bins
N	2,496	2,496	2,496	2,496	2,496	2,496	2,496	2,496	2,496	2,496	2,496	2,496

Notes: Rank Child represents the ranking (percentile) of the child in the income distribution of its cohort. Enters College and Same Firm are a dummies =1 if the child ever enrolls in public university or works at a firm where one of their parents has ever worked. Age Entry LM represents the age (in years) in which the child gets their first job. Treated is a dummy =1 if the parent is below the next SMW at month $t-1$ before the adjustment and above it at month $t+6$ when the adjustment kicked in. Treated Cont. is % of the new SMW the parent won at time $t+6$. Bins variables indicate which bin the parent was in with his relative adjustment to the SMW. Control variables are parent and child sex. Fixed Effects are for parents (1950-1966), children's birth cohorts (1988-1991) and parents' industry sector at 4 digits (ISIC - 4). Standard errors in parentheses and clustered at the child level. * $p<0.10$, ** $p<0.05$, *** $p<0.01$.

Table D.12: Main Estimates - 6 Month Treatment for Cohorts 1992-1996

	(1) Rank Child	(2) Enters College	(3) Age Entry LM	(4) Same firm	(5) Rank Child	(6) Enters College	(7) Age Entry LM	(8) Same firm	(9) Rank Child	(10) Enters College	(11) Age Entry LM	(12) Same firm
Rank Parent	0.107***	0.004***	0.007**	0.001***	0.108***	0.004***	0.007**	0.001***	0.105***	0.004***	0.007**	0.001***
	(0.036)	(0.001)	(0.003)	(0.000)	(0.037)	(0.001)	(0.003)	(0.000)	(0.037)	(0.001)	(0.003)	(0.000)
Treated	-0.775	-0.086***	-0.240**	-0.009								
	(1.301)	(0.020)	(0.111)	(0.017)								
Treat Cont.					-0.038	-0.004***	-0.010*	-0.000				
					(0.061)	(0.001)	(0.005)	(0.001)				
100 - 110 % Bin									-1.378	-0.065***	-0.213*	0.007
									(1.480)	(0.023)	(0.128)	(0.020)
110 - 120 % Bin									-0.130	-0.119***	-0.267	-0.033
									(1.875)	(0.027)	(0.166)	(0.024)
Control Mean	59.181	0.300	20.163	0.151	58.603	0.273	20.081	0.151	59.231	0.300	20.161	0.151
Controls	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
FE	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Treatment	Discrete	Discrete	Discrete	Discrete	Continuous	Continuous	Continuous	Continuous	Bins	Bins	Bins	Bins
N	2,690	2,690	2,690	2,690	2,690	2,690	2,690	2,690	2,690	2,690	2,690	2,690

Notes: Rank Child represents the ranking (percentile) of the child in the income distribution of its cohort. Enters College and Same Firm are a dummies =1 if the child ever enrolls in public university or works at a firm where one of their parents has ever worked. Age Entry LM represents the age (in years) in which the child gets their first job. Treated is a dummy =1 if the parent is below the next SMW at month $t-1$ before the adjustment and above it at month $t+6$ when the adjustment kicked in. Treated Cont. is % of the new SMW the parent won at time $t+6$. Bins variables indicate which bin the parent was in with his relative adjustment to the SMW. Control variables are parent and child sex. Fixed Effects are for parents (1950-1966), children's birth cohorts (1992-1996) and parents' industry sector at 4 digits (ISIC - 4). Standard errors in parentheses and clustered at the child level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.