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The long(er)-term impacts of Chile Solidario on human capital and labour income

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Abstract: This paper examines Chile Solidario, a social protection programme that provides poor households in Chile with preferential access to a conditional cash transfer programme designed to facilitate investments in children's health and education. We assess the programme's longer-term impact on educational attainment and labour income at ages 25–28. Overall, Chile Solidario has a positive and long-lasting effect, albeit with significant impact heterogeneity. The effects on educational attainment are similar for women and men, and for indigenous and non-indigenous people, but the effects on labour income are driven by men and non-indigenous people. The impact on labour income is not significantly different from zero for women with children, but is positive and significant for women without children. The effects on both education and labour income are concentrated in urban areas. Our results indicate that the impact of Chile Solidario depends on societal and structural factors underpinning labour markets in Chile.

Keywords: Conditional cash transfers, human capital, labour income, children, Latin America, Chile Solidario

JEL classification: I21, I38, J24, O15

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1 Introduction

Conditional cash transfer programmes (CCTs) have become one of the most prominent anti-poverty policy innovations over the last two decades. In the Latin American region alone, CCTs have been adopted in 18 countries to cover approximately 130 million people living in poverty (Stampini and Tornarolli 2012). CCTs vary in terms of scale, scope, and design features, but overall they provide income support to poor households on condition that they send school-age children to school, and that family members attend regular health check-ups. The underlying idea is that by providing monetary incentives to poor households that underinvest in the human capital of their children, CCTs help to break the intergenerational transmission of poverty.

A growing literature that examines the short-term effects of CCTs has shown that these programmes can, under certain conditions, successfully improve the outcomes of beneficiary children in many well-being dimensions, including schooling (Akresh et al. 2013; Behrman et al. 2009; Dammert 2009; Filmer and Schady 2008; Lincove and Parker 2016; Maluccio and Flores 2005; Skoufias and Parker 2001), health and nutrition (Attanasio et al. 2015; Behrman and Parker 2013; Buser et al. 2017; Fernald et al. 2008; Gertler 2004; Manley et al. 2013; Perova and Vakis 2012), and to a lesser degree, cognitive abilities and learning (Baird et al. 2013; Fernald and Gunnar 2009; Macours et al. 2012; Paxson and Schady 2007, 2010) and child work (Behrman et al. 2012; Canelas and Niño-Zarazúa 2016; Dammert 2009; Edmonds and Schady 2012; Ferro et al. 2010; Skoufias et al. 2001).[1]

While short-term, and more recently, medium-term effects of CCTs have been studied, evidence of the long(er)-term effects of programmes on, for instance, income and labour market outcomes of young adults that receive treatment as children remain scarce, and are mostly restricted to the early stages of labour market entry (Molina-Millan et al. 2016; Saavedra 2016). There are concerns that the observed short-term effects of CCTs on human capital may not be sufficiently strong to address the structural factors that keep people in poverty across generations (Levy and Schady 2013).

This paper contributes to the literature on CCTs by examining the long(er)-term effects of Chile Solidario, an innovative cash transfer programme that started in 2002 with the specific objective of tackling extreme poverty in Chile. One of the main instruments to achieve that goal has been the provision of information to increase awareness of the programme's conditions, benefits, and eligibility criteria. Past studies that assessed the short- and medium-term effects of Chile Solidario showed that the programme was successful in increasing take-up among eligible households (Carneiro et al. 2015; Galasso 2011).

A key component of Chile Solidario is to provide poor households with preferential access to Subsidio Unico Familiar (SUF), a conditional cash transfer programme designed as an incentive device to facilitate higher investment in children's health and education.

As our identification strategy, we exploit the fact that by design and programme rules, only children aged under 18 are eligible to receive SUF. Hence, we compare individuals born in 1985 or afterwards, and who spent their childhood in households that were eligible to receive Chile

[1] For reviews of the literature, see Barrientos and Niño-Zarazúa (2010), Baird et al. (2013), and Bastagli et al. (2016).

Solidario at the beginning of the programme in 2002, with individuals born before 1985, who were not eligible to receive SUF benefits due to their age.

We adopt a difference-in-differences approach—which we then extend to include propensity score matching estimators and a regression discontinuity design—to nationally representative household survey data that allows us to collect information on the characteristics of the households where individuals grew up, using retrospective information. In this way, we are able to measure the long-term effects of the increased take-up of SUF as a result of Chile Solidario on educational achievements and labour income at the ages of 25–28. To the best of our knowledge, this is the first study that examines the long-term effects of Chile Solidario, and it contributes to the scant literature on long-run impacts of social transfers on human capital and occupational choices.

Our findings show that Chile Solidario, and particularly the take-up of SUF, has a positive and long-lasting impact among the extremely poor in Chile. Individuals that spent their childhood in poverty and were eligible to receive the programme obtain higher educational and labour income levels as adults than individuals from poor families that were not eligible to receive the transfer. The average treatment effects are in the order of more than one year of schooling and an additional US$200–250 per month in labour income.

We also find a degree of impact heterogeneity in our estimates. The effect of the programme on schooling is similar among women and men, but it is largely driven by men in the case of labour income. Further analysis shows that the impact on labour income is not significantly different from zero for women with children, while it is positive and significant for women with no children. Furthermore, the effects seem to be concentrated in urban areas, which confirms earlier findings on short-term impacts.

Our results indicate that the positive short-term effects reported by earlier impact studies on the programme seem to have persistently improved the human capital of children from poor households, and that this ultimately translates into better educational achievements and higher labour incomes for programme beneficiaries in the longer term.

The remainder of the paper is organized as follows. Section 2 provides a brief overview of the context in which Chile Solidario was introduced. Section 3 describes the data used and the identification strategy. Section 4 presents the model and estimation methods, while Section 5 presents the baseline results and an analysis of the intensity of treatment and impact heterogeneity. Section 6 performs a series of robustness checks. Section 7 concludes with reflections on the policy implications of our findings.

2 Context and intervention

A period of sustained income growth and proactive social policies in the 1990s helped Chile to reduce the poverty headcount rate—which measures the percentage of the population living below the national poverty line—by almost half, from 38.6 per cent of the total population in 1990 to 20.2 per cent in 2000. Despite this progress, extreme poverty remained stubbornly unaffected at six per cent over the same period, which was partly attributed to limited information among the extremely poor about SUF, its eligibility conditions, and how they could access their entitlements (Galasso 2011). In response to this constraint, in 2002 the Chilean government introduced Chile Solidario, a programme explicitly designed to tackle extreme deprivation.

The programme provides a combination of policy interventions to support poor households to exit poverty. In particular, the first 24 months of the treatment—the so-called Programa Puente ('bridge programme')—provides psychosocial support to the families, a cash transfer at a decreasing rate over time, and in parallel, preferential access to SUF, a cash transfer programme that aims to increase poor households' investment in children's human capital (Fiszbein and Schady 2009).[2]

In 2002–06, the government began to gradually incorporate poor families into Chile Solidario, with an average of about 50,000 additional households each year, to the level of approximately 264,000 households in 2011. The programme targets the extremely poor through proxy means tests based on a basic needs approach. Participation among eligible households is notably high, with around 95 per cent of households receiving treatment, and with very low dropout rates at around three per cent of all treated households (Galasso 2011).

Chile Solidario differs from other cash transfer programmes in Latin America in several respects. First, it adopts an integral approach by combining income support with non-monetary interventions, including psychological support and advice from social workers who are actively involved in deciding the type of supportive measures that households need. Second, the cash transfer per se is not the main feature of the programme, but instead acts as an incentive device to encourage households to undertake investment decisions that are beneficial to the well-being of their children.

Earlier evaluations of Chile Solidario showed that the programme was successful in linking the poor with the social protection system (Galasso 2011; Guardia et al. 2011; Martorano and Sanfilippo 2012). More recently, Carneiro et al. (2015) have shown that one of the most important achievements of the programme is the high take-up of SUF by extremely poor families with children.

SUF is one of the oldest CCTs worldwide, having started in 1981. As in the case of Chile Solidario, it relies on proxy means tests to identify eligible households. However, the threshold for SUF is higher, as the transfer is targeted at the bottom 40 per cent of income distribution. The monthly payment, which is about US$6 in 2003 prices and represents less than 10 per cent of the total household income of poorest families, is delivered to the mother on the condition that she has a) children aged between six and 18 years attending school regularly, and/or b) children under the age of six attending regular medical check-ups.[3] Additional transfers are made available to young mothers and disabled persons.

Past studies have focused on the short-term effects of Chile Solidario. For example, Martorano and Sanfilippo (2012) find that the programme reduces poverty, and increases school enrolment and utilization of public health services among children of participating households. Galasso (2011), Guardia et al. (2011), and Carneiro et al. (2015) also find a significant increase in the take-up of social subsidies, with particularly strong effects among families who had no previous access to the social protection system. Furthermore, previous studies find no evidence of negative effects on employment choices, leisure, or welfare dependency.

To the best of our knowledge, this is the first study that examines the long-term effects of Chile Solidario. Taking a longer-term perspective is particularly important because it allow us to examine the more structural and transformative impacts of the programme on the poorest members of

[2] For a detailed description of the programme, especially its admission mechanism, see Guardia et al. (2011).

[3] For further information on SUF, see Fiszbein and Schady (2009) and Cecchini and Madariaga (2011).

Chilean society. Indeed, the number of studies that have investigated the long-term impacts of CCTs is still very limited; they primarily come from Latin America (see e.g., Baez and Camacho (2011) and Barrera-Osorio et al. (2015) on Colombia's Familias en Acción; Barham et al. (2013) on Nicaragua's Red de Protección Social; and Behrman et al. (2011) on Mexico's Progresa-Oportunidades programme), and often provide contrasting results.[4]

3 Data and identification strategy

The data used in this study comes from CASEN, a nationally representative cross-sectional household survey conducted since 1985 by the Chilean Ministry of Planning. Data on adult outcomes of former beneficiary children was obtained from the 2013 CASEN survey, while additional data was obtained from the 2003 CASEN survey, which was the round that immediately followed the implementation of Chile Solidario in 2002.[5] The CASEN surveys are particularly suitable for the purposes of our study: their richness in retrospective information enables us to reconstruct the socio-economic circumstances that individuals experienced in childhood while controlling for individual characteristics.

Furthermore, since Chile Solidario is a nationwide programme, a nationally representative household survey is the most appropriate informational tool to examine the long-term effects on former beneficiary children. For our analysis, we restrict our sample to adult individuals born in 1973–88, and measure the impacts of the programme with available information on education, income, and parental educational background.[6]

3.1 Treatment and control groups

In an ideal setting, our treatment group would be formed of individuals who spent their childhood in extreme poverty and therefore were eligible to receive Chile Solidario, i.e. households that had a high pre-treatment likelihood of being unaware of their entitlements to receive SUF and other social protection benefits. Similarly, the control group would be formed of individuals who spent their childhood in households eligible to receive SUF but were not eligible to receive Chile Solidario due to programme exclusionary rules, such as not being in extreme poverty. Consequently, the take-up of SUF should have risen drastically only in the treated group after the introduction of Chile Solidario in 2002, as shown by Carneiro et al. (2015).

Unfortunately, we do not observe income data from the households where individuals grew up that would enable us to identify eligibility to participate in Chile Solidario. Our identification strategy relies instead on a proxy measure for household income. In the CASEN survey, we can identify the circumstances individuals faced in childhood through retrospective information about their parents' educational levels. An important advantage of this approach is that we can identify adult individuals even if they have left their household of origin. The procedure enables us to measure the long-term outcomes of the programme, while reducing the bias arising from co-residency and sample attrition (Emran et al. 2017).

[4] For recent reviews on the topic, see Molina-Millan et al. (2016) and Saavedra (2016).

[5] For further information, see the methodological report of the CASEN survey (Ministerio de Desarrollo Social 2015).

[6] All estimates are weighted by the inverse probability of selection, which are provided by the survey design. However, as Solon et al. (2015) point out, under certain conditions weighting might be unnecessary, and even harmful, for obtaining causal parameters. Therefore, we also ran unweighted regressions to obtain our estimates. The results did not change significantly.

The intuition behind our strategy is that households with very low levels of education face higher risks of poverty, and therefore are more likely to be eligible to receive treatment from Chile Solidario. Indeed, Galasso (2011) has shown that in the first years of Chile Solidario, two thirds of the beneficiary households' heads and their spouses had not completed primary education. Our examination of the 2003 CASEN survey shows that the conditional probabilities of being extremely poor and eligible for Chile Solidario are higher for individuals with no formal education (see Figure 1).

Furthermore, Figure 2 shows the predicted household income and amount of SUF received for each level of education. Household income is low for household heads with no or only primary education, and becomes substantially higher with increasing levels of education. The amount of SUF received is relatively close in the first educational categories. It is conspicuous that among household heads with no formal education, income levels are lower than among household heads with incomplete primary education. Thus, the evidence suggests that parental education is a good (although arguably imperfect) proxy for treatment status. To delimit possible sources of bias in our estimates, we have chosen treatment and control groups conservatively. The treatment group is made up of individuals whose parents had no education, while the control group is comprised by individuals whose parents had some years of schooling or completed primary education. We exclude from the analysis individuals who have parents with educational levels above that threshold.

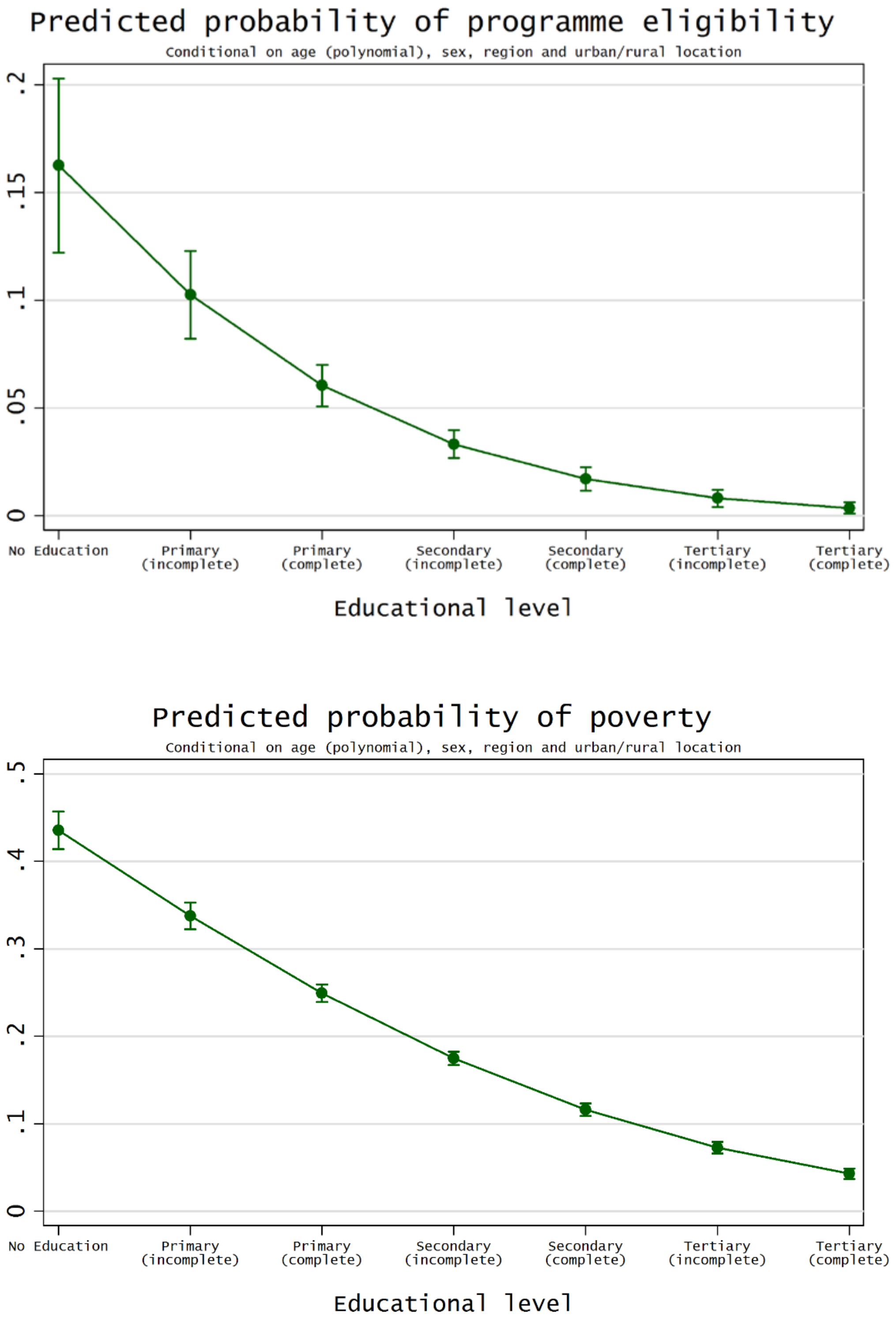
3.2 Time dimension

Our identification strategy also exploits an age restriction imposed exogenously by the programme. While the poorest households eligible to receive Chile Solidario are identified using a proxy means test, only families with children aged 18 and younger are eligible to receive SUF.[7] Since Chile Solidario was implemented in 2002, individuals born in 1985 or later were younger than 18 when the programme started, and were therefore eligible for SUF, while people born before 1985 were aged 18 or older and therefore not eligible to receive SUF.[8] This exogenous rule allowed us to adopt an age cohort approach in our analysis based on individuals' year of birth. A variation in outcomes between individuals from extremely poor households born before and after 1985 could thus be attributed to SUF, while programme eligibility to receive Chile Solidario should be the same for both groups. Figure 3 illustrates the key aspects of our identification strategy.

[7] Generally, the conditionality of Chile Solidario is bonded to the willingness and effort of the household to fulfil the stipulated contract with the social worker. Therefore, the protection and exit grant, as well as some other more specific transfers such as the save water allowance, are guaranteed to all participating families. However, other eligibility criteria are valid for some particular transfers, such as SUF and certain allowances for the elderly or disabled.

[8] Since the programme was implemented in May 1984, one might think that people born between June and December 1984 would be eligible as well. However, it was only after a first working period of six to eight months that social workers began to activate demands for social services to support the households. Therefore, we expect the 1985 cohort to be the first effectively affected by the take-up of SUF.

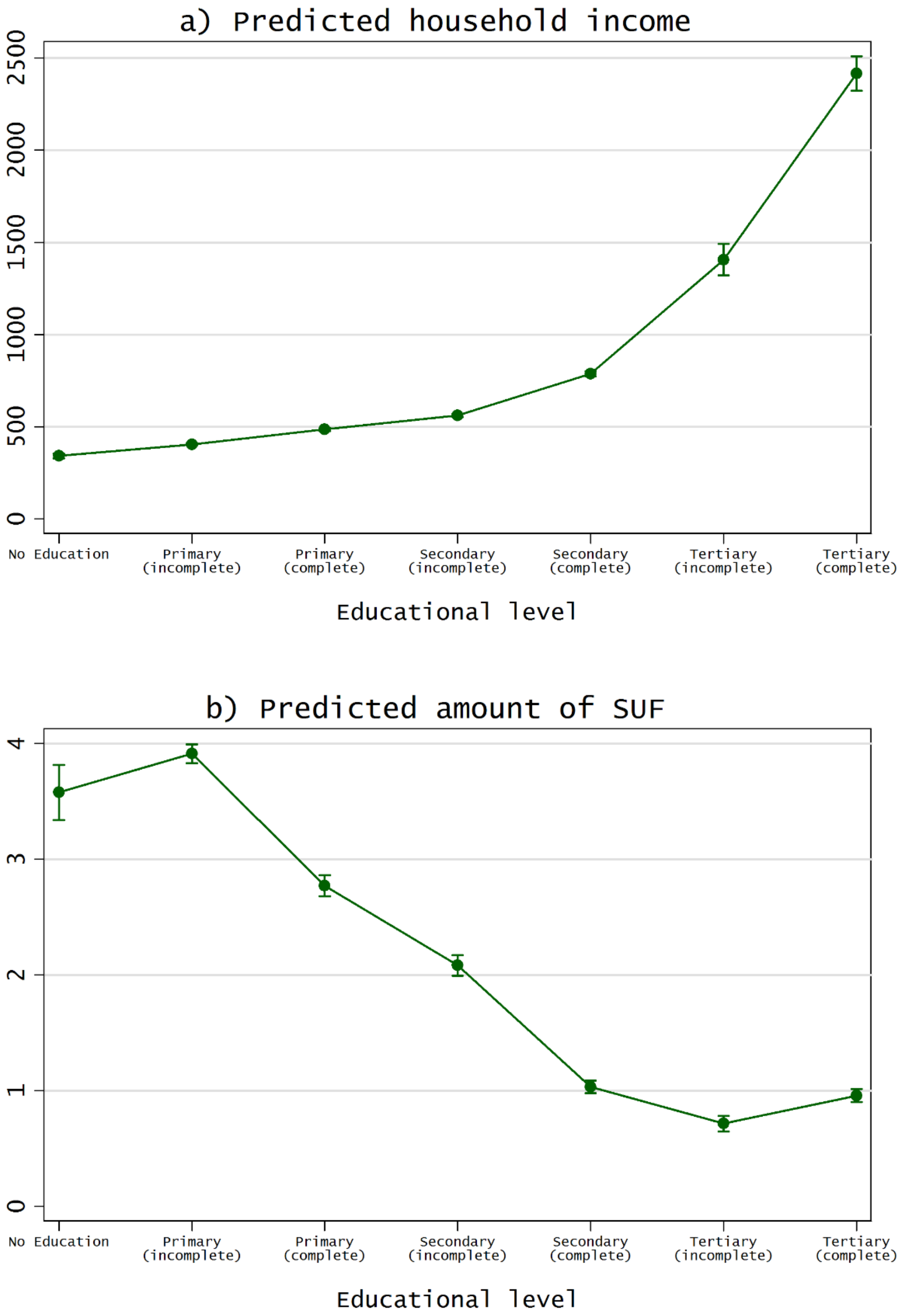
Figure 1: Predicted probability of a) eligibility for Chile Solidario and b) extreme poverty by educational level



Notes: Conditional on age, sex, region, and rural or urban area; only individuals in age interval 30–60. Bootstrap confidence intervals.

Source: authors' estimations based on CASEN 2003 (Ministerio de Desarrollo Social 2003).

Figure 2: Predicted a) total household income and b) amount of SUF (both in US dollars) by educational level

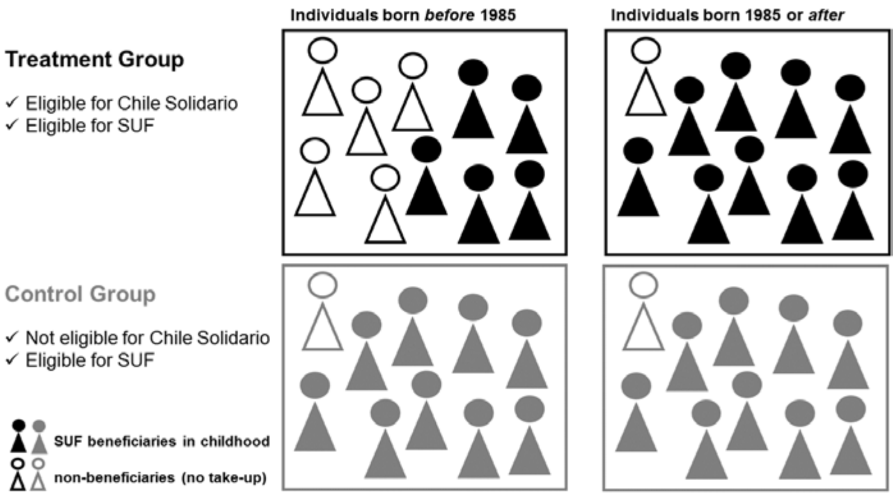


Notes: Conditional on age, sex, region, and rural or urban area; only individuals in age interval 30–60. Bootstrap confidence intervals.

Source: authors' estimations based on CASEN 2003 (Ministerio de Desarrollo Social 2003).

We restrict the time window of our analysis to the age interval 25–40—i.e. individuals who were at most 29 years old when the intervention started in 2002—to avoid co-residency bias, and to reduce age effects and bias deriving from individuals who did not finish their educational career or who recently entered the labour market.[9] Correspondingly, treated individuals were at least 25 years old in 2013 and had a maximum exposure to the programme of four years within the interval 14–18 years of age.

Figure 3: Identification strategy



Notes: Variation in time (cohorts before and after 1985) is on take-up of SUF. Variation between treatment and control group is in eligibility status for Chile Solidario.

Source: authors' illustration.

4 Model and methods

We adopt a difference-in-differences (DD) approach based on demographic groups with different access to the cash transfer programme (Card and Krueger 1994). This methodology is particularly useful for comparing heterogeneous individuals (Meyer 1995). Intuitively, our estimates measure the changes in average outcomes of individuals facing similar circumstances—measured by parental background—before and after the introduction of Chile Solidario. Thus, our treatment group is adult individuals whose parents had no formal education, while the time dimension is defined by individuals' year of birth. We restrict the control group to individuals with parents who had very low levels of education (some years of schooling or completed primary education).[10]

We opt for a linear model of the following form:

$$y_{ijt} = \eta_j + \lambda_t + \gamma X_{ijt} + \delta \mathrm{S}_{\mathrm{jt}} + \varepsilon_{ijt} \qquad [1]$$

[9] We perform robustness checks with a shorter age interval.

[10] We always refer to the parent with the highest educational level within the family, or the parent with available information if one of the two has missing information. Since we rely on the retrospective questions in the survey on the father's and mother's education, we do not need individuals to reside with their parents in the same household to retrieve this information.

where y is the outcome of individual i belonging to group $j\epsilon(T,C)$ and cohort $t\epsilon(0,1)$, with $t = 0$ measuring if the individual was born before 1985, and $t = 1$ otherwise. η_j and λ_t capture group and cohort fixed effects, X_{ijt} is a vector of control variables that are expected to influence the outcomes of interest, and S_{jt} is a binary variable that indicates the treatment status for group j in cohort t.

The estimated coefficient δ of the model in equation [1], without including control variables, measures the unconditional differences in average outcomes at the group level before and after the implementation of the programme. The control variables in X_{ijt} include age, age-squared, household size, and self-reported health status, as well as dummies for the geographical region, the rural-urban divide, and the ethnic background (indigenous or not) of individual i.

Table 1 shows the descriptive statistics of the covariates.[11] Since the intervention was not random, differences in observable (and unobservable) characteristics might be expected. However, the averages of the covariates for the two groups are qualitatively similar and mostly not significantly different from zero. The difference in the group differences in means between the two cohorts is

$$DD = (y_{T1} - y_{T0}) - (y_{C1} - y_{C0}) \qquad [2]$$

and it is straightforward to show that

$$DD = \delta + (\varepsilon_{T1} - \varepsilon_{T0}) - (\varepsilon_{C1} - \varepsilon_{C0}) \qquad [3]$$

If the difference of the last two terms is zero, δ consistently estimates the effect of the programme. The key assumption of our identification strategy is that the two groups will follow a parallel trend in outcomes of interest in the absence of the treatment and other events contemporaneous with Chile Solidario. Since the assignment to treatment is not random, this condition is crucial for the interpretation of the causal effects of the programme (Garganta and Gasparini 2015).[12]

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[11] Table A1 in the Appendix illustrates the Chilean education system, and shows the estimated median income for each level of education. Tables A2 and A3 show the weighted population shares by educational level and level of parental education.

[12] A violation of the second assumption might derive from the possibility that the likelihood of leaving home rises with age. Therefore, people born in 1973–84, i.e. aged 18–29 years when the programme started in 2002, might have already left their household of origin, in contrast with people born in 1985–88, i.e. aged 14–17. In this case, eligibility for SUF would not be the only source of variation in our time dimension. However, in Chile the share of young people aged 15–29 living with their parents is relatively high: 61 per cent in 2014, and 62 per cent in 2007 (see OECD 2016). Furthermore, the primary reason for leaving home is marriage, and the mean age at first marriage is 28.5 years for women and 30.4 for men. For these reasons, we expect that the bias resulting from leaving home should not affect our estimates significantly. Nevertheless, we perform robustness checks restricting the time window of analysis, with qualitatively equivalent results. The results of this alternative specification are presented in the Appendix.

Table 1: Pre- and post-treatment sample averages (weighted)

Cohorts	1973–84 (t=0)		1985–88 (t=1)	
Covariates	Average of treatment group	Control group (difference to treatment group)	Average of treatment group	Control group (difference to treatment group)
Male	0.422	-0.074	0.514	-0.185
		*0.0271***		*0.0667***
Age	36.094	-0.963	26.615	0.022
		*0.1871****		*0.1375*
Rural	0.285	-0.096	0.155	0.029
		*0.0218****		*0.0375*
Household members	4.385	-0.156	4.390	-0.177
		0.1591		*0.2668*
Indigenous	0.194	-0.064	0.164	-0.018
		*0.0178****		*0.0476*
Migrant	0.005	0.000	0.002	0.001
		0.0028		*0.0021*
Self-reported health	5.804	-0.043	6.059	-0.080
		0.0616		*0.1223*

Notes: 1,399 observations in treated and 10,442 in control group. (0/1) indicates dummy variable. Self-reported health: from (1) 'very bad' to (7) 'very good'. Migrants (individuals born outside Chile) only included if migrated to Chile before 2002, the starting year of Chile Solidario. Bootstrapped standard errors of the difference in averages between treated and control group reported in italics below the estimates. Statistical significance reported at conventional levels, * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Source: authors' calculations based on CASEN 2013 (Ministerio de Desarrollo Social 2013).

The interpretation of the estimated parameter can be twofold. First, because of the high participation rates in Chile Solidario, the estimated DD coefficient yields the average treatment effect on the treated of the capacity of Chile Solidario to link poor families to SUF. Second, we can also interpret the DD parameters as the intention-to-treat effect of SUF. To avoid potential bias in the presence of serial autocorrelation in the outcomes, we apply the correction to the standard errors suggested by Bertrand et al. (2004), and aggregate the data into one period before and one period after the implementation.[13] Furthermore, since municipalities are responsible for the proxy means tests that identify households in extreme poverty and thus eligible to participate in Chile Solidario, we suspect that the error terms may be correlated within these geographical units. Therefore, we also compute standard errors clustering observations at the municipality level.[14]

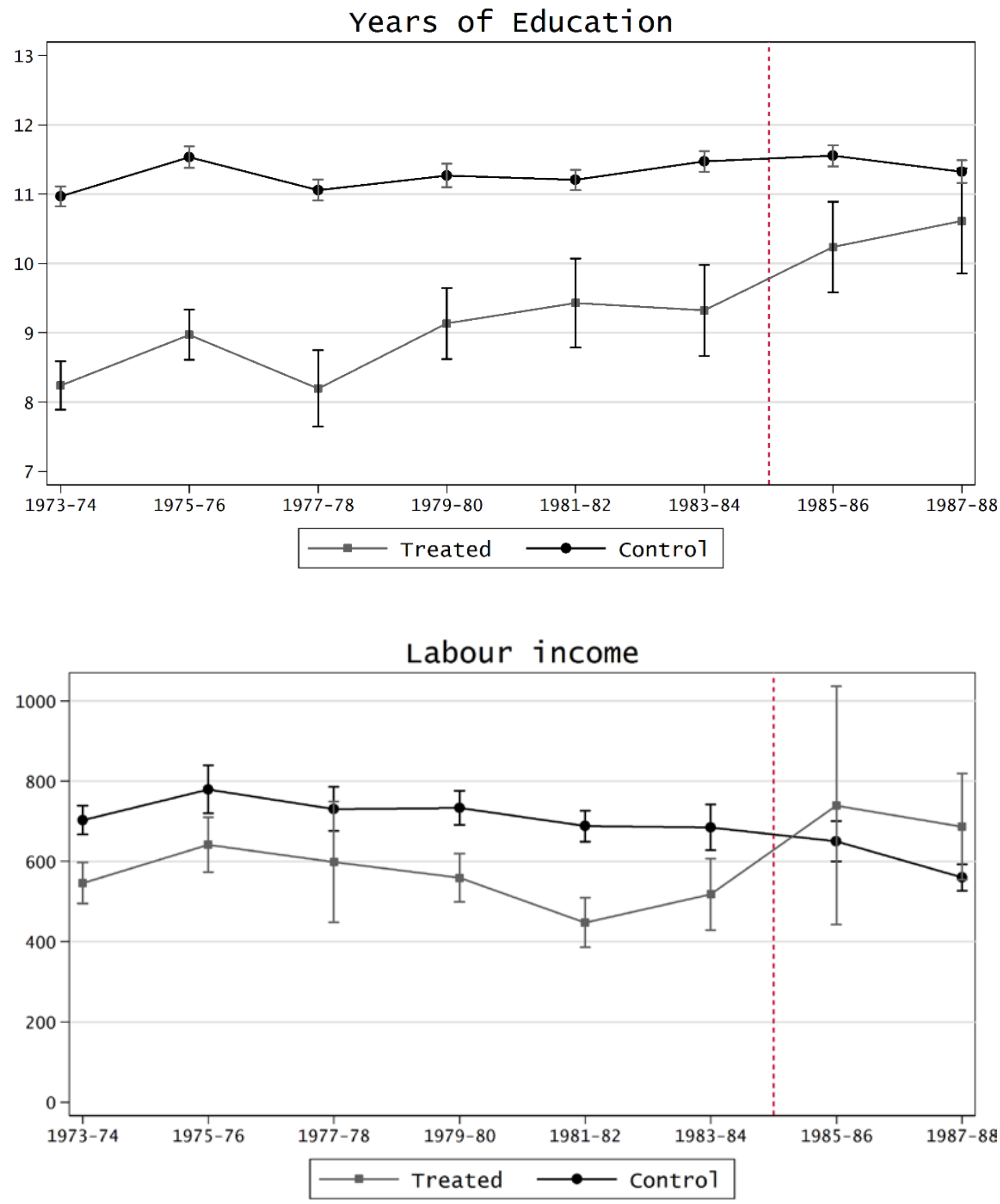
[13] To evaluate the intensity of the treatment effect with programme exposure and test the parallel trends assumption, we also run our estimations including a full set of cohort dummies.

[14] As a robustness check, we cluster standard errors at the regional level, applying the bootstrap-based procedure to get significance levels with few clusters, as proposed by Cameron et al. (2008). Also following this methodology, the estimates for the treatment effect of the social programme, on both years of education and labour income, are significantly different from zero.

5 Results

5.1 Baseline

Figure 4: Parallel trends by cohort



Source: authors' calculations based on CASEN 2013 (Ministerio de Desarrollo Social 2013).

Figure 4 illustrates our unconditional baseline results.[15] We see a sharp increase in average years of schooling and labour income for eligible cohorts in the treatment group, which we do not observe

[15] Figure 4 serves furthermore as a first justification for the validity of the common trends assumption. Cohorts are displayed pairwise to yield more precise estimates because of the number of observations for each single cohort. Since visual inspection may leave some doubt, we also verify the validity of the assumption through a model that includes a full set of dummies for cohorts and the respective interactions with the treatment status. We jointly test the coefficients of the interaction terms of the pre- and post-treatment cohorts against the null hypothesis of equality to zero. The

among members of the control group. For educational attainment, the average years of schooling of treatment and control groups were around nine and 11 years respectively, and rose very slowly before the treatment. After the intervention, the average years of schooling increased to more than 10 years for eligible cohorts in the treatment group, nearly catching up with the control group. For labour income, before treatment the average labour income of individuals in the treatment group was significantly lower than that of the average income of the control group (around US$600 and US$750 respectively). After the intervention, the average income of the treatment group increased by about one third, overtaking the average income of the control group. The higher take-up of SUF by poor families as a consequence of receiving Chile Solidario seems to have had a positive and sustained effect on the human capital and income of young adults who received support from the programme as children.

Table 2: The long(er)-term effects of Chile Solidario

(a) Years of education	(1) Unconditional	(2) Conditional	(3) Only women	(4) Only men
DD	1.463***	1.243***	1.138***	1.534***
	(0.3968)	(0.3550)	(0.4062)	(0.5558)
Treated	-2.521***	-2.287***	-2.250***	-2.366***
	(0.2201)	(0.2022)	(0.1964)	(0.3359)
Born after 1984	0.211**	0.017	-0.217	0.542
	(0.0972)	(0.2464)	(0.2494)	(0.3937)
Observations	11821	11690	7661	4029

(b) Labour income	(1) Unconditional	(2) Conditional	(3) Only women	(4) Only men
DD	261.566***	268.752***	51.903	335.881**
	(95.2696)	(98.6000)	(69.3246)	(134.4035)
Treated	-156.702***	-133.698***	-152.067***	-159.264***
	(33.3210)	(29.2349)	(28.7532)	(41.2353)
Born after 1984	-110.948***	-20.301	-47.974	-56.744
	(30.5436)	(88.4852)	(74.1361)	(163.7679)
Observations	8244	8149	4330	3819

Notes: DD is the coefficient of the interaction term. Control variables include age, age-squared, number of household members, rural or urban location, region of residency dummies, ethnic background (indigenous or not), a dummy for migrants, and self-reported health. Statistical significance reported at conventional levels, * p < 0.1, ** p < 0.05, *** p < 0.01.

Source: authors' estimations based on CASEN 2013 (Ministerio de Desarrollo Social 2013).

Table 2 quantifies the impact in four different model specifications. Column (1) shows the unconditional impact results; column (2) presents the DD estimates including control variables for demographic characteristics and health status. The conditional impact estimates are around 1.2 additional years of schooling and US$270 in labour income. To put this into perspective, the latter

null cannot be rejected for pre-treatment cohorts (F=1.11, Prob>F=0.3520) and is rejected after the treatment (F=4.93, Prob>F=0.0007).

represented about 14 per cent of the Chilean average monthly wage in 2013.[16] Columns (3) and (4) present the results separately for men and women. It is evident that the effect on schooling is similar for both sexes, while the effect on labour income is largely driven by men. This is not surprising: although female labour participation in Chile has improved over the last decades, it is still low at around 60 per cent, and among the lowest in Latin America (Gasparini and Marchionni 2015). For men, the effect on labour income is about US$335, or approximately 17 per cent of the average monthly wage.[17]

We suspect that one of the possible mechanisms underpinning the effect of SUF on labour income is the additional years of schooling obtained by the treated cohorts. At first sight, the increase in income might appear too large to be caused by an additional 1.5 years of schooling among men. However, there are two possible explanations of the apparently large impact size. First, there may be important treatment effects on skills and aptitudes, for instance through improvements in children's cognitive or non-cognitive abilities. Second, since individuals in our treatment cohort were at a critical age—between 14 and 18 years—when the programme started in 2002, the additional years of schooling could have led to the completion of the school certificates that are needed to enter the formal labour market or access better-paid occupations.

We test these two possible routes with the available data in the CASEN surveys. The first route is tested through a battery of questions on socio-emotional stability, such as 'do you have difficulties concentrating and remembering things?' or 'do you have difficulties learning new tasks?'[18] However, we do not find significant differences in these domains between treatment and control cohorts. We test the second route by looking at the probabilities of attaining a secondary-school certificate. We find that treatment cohorts are twice as likely to complete secondary schooling (60 per cent) vis-à-vis the control cohort (30 per cent).

Thus, the evidence seems to suggest that it is not simply the additional years of schooling that lie behind the significant increases in labour income among young male members of the treatment cohort. More importantly, the programme facilitated the completion of secondary education, and it was this that most likely helped these individuals to access better-paid occupations.[19]

It is worth pointing out that the psychological support provided by social workers may itself also have an effect, as well as better information about the policy tools available to the poor through Chile Solidario to help them cope with economic adversity.[20] Earlier studies have found that non-monetary interventions can also have positive effects on child development (see e.g., Dahl and Lochner (2012) in the context of the USA, and Paxson and Schady (2010) for a study in the context of Ecuador). However, we suspect that these potential factors play a very minor role in our results,

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[16] The Chilean monthly mean wage in 2013 was about US$1,918.25 (yearly mean wage US$23,019, according to OECD (2017)), while the minimum wage in 2013 was about US$425 (WageIndicator 2017).

[17] These estimates are computed on the sample of individuals with available information about their labour income. In a robustness check, we impute a zero to unemployed and inactive individuals with missing information on labour income. The conditional estimates of the effect are slightly lower for the whole sample because of the relatively high number of inactive women, and slightly higher for men.

[18] The other questions are: 'do you have difficulties …establishing and maintaining personal relationships? …relating to people you don't know? …putting effort into your studies or profession? …participating in recreational activities? …moving because of physical obstacles?'

[19] These results can be found in the Appendix.

[20] It is still an open question whether the income from welfare is beneficial for the outcomes of children. The existing evidence seems rather to point to detrimental effects in the short and long run; however, these could be biased by the selection of certain parents for welfare support (Mayer 2002).

since all individuals in our sample received these services, although only the younger group was eligible to receive SUF.

5.1.1 Potential sources of measurement error

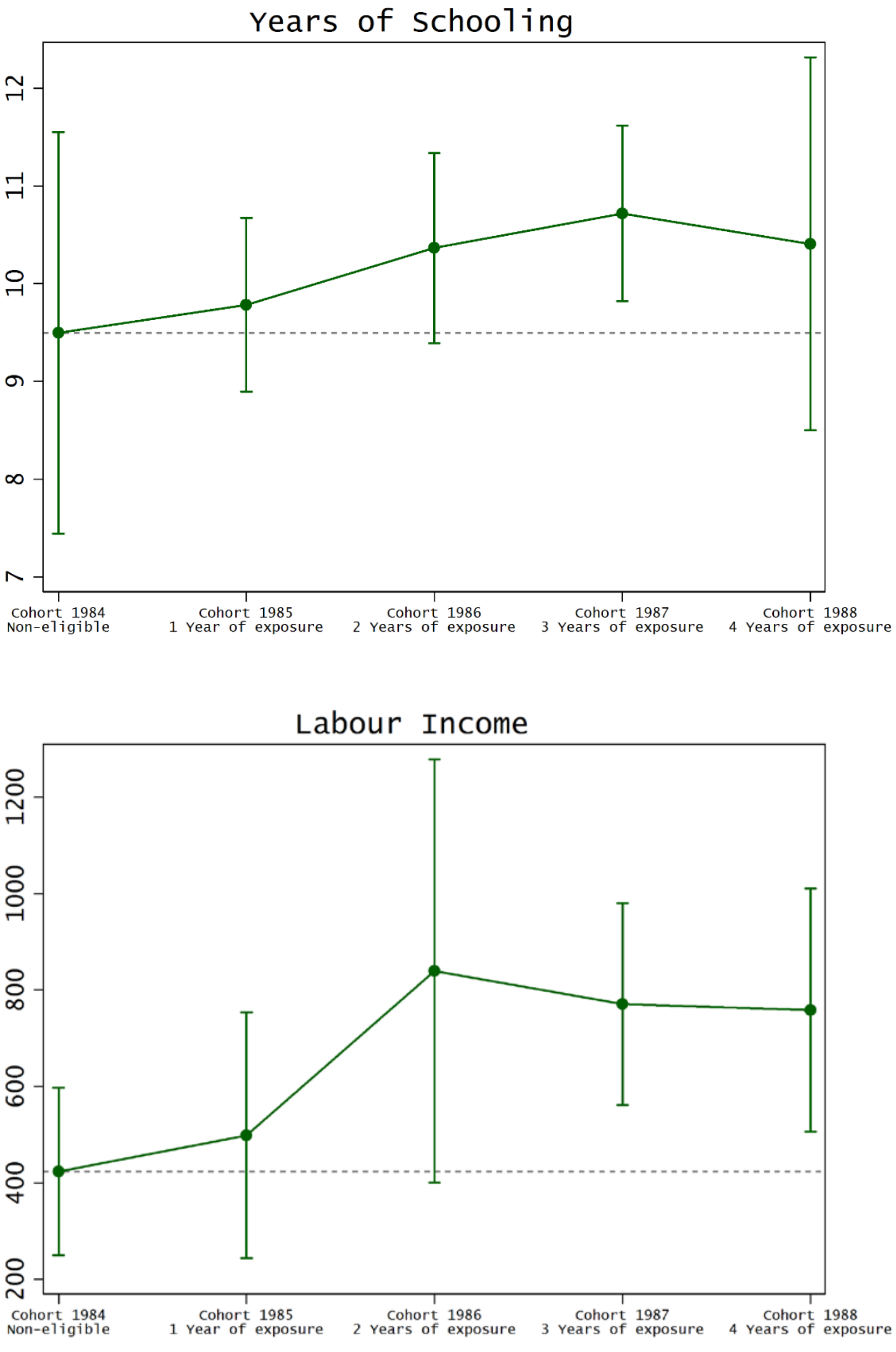
We expect some possible sources of measurement error in our estimates. One may come from the fact that treatment status in childhood was not directly observed, but was approximated with retrospective information on parental educational levels. The control group might include individuals that grew up in eligible households, while the opposite might also be observed among the treatment group. If that were the case, we would get downward-biased estimates. Another potential source of bias could arise from sibling spillover effects, i.e. if non-eligible individuals with younger siblings that received treatment were also positively affected by the programme via the monetary subsidy and changes in behaviour. Similarly, we might suspect that non-eligible individuals would still benefit from Chile Solidario if they lived with an elderly person eligible to receive an old age pension.

In order to investigate these potential sources of measurement error, we estimate the probability among individuals eligible to receive Chile Solidario of living with older siblings or an elderly person in the household. The estimated probabilities are relatively low—less than 10 per cent—and statistically insignificant when compared with the probabilities of non-eligible individuals. Consequently, while we cannot disregard the possibility of sibling spillover effects and other potential sources of bias, the evidence suggests that these are not cause for concern.

5.2 Intensity of treatment

Figure 5 shows the intensity of the treatment effect. As expected, the treatment effect varies with the time exposure of individuals to the social transfer. However, we observe a measurable effect, especially for labour income, only for the 1985 and 1986 cohorts, i.e. from one to two years of exposure. Among the other cohorts, the intensity of effect is similar. The reasons might be the relatively short time window of our analysis, and the fact that Chile Solidario was implemented gradually in the first years until it addressed all eligible families.

Figure 5: Intensity of treatment effect



Source: authors' estimations based on CASEN 2013 (Ministerio de Desarrollo Social 2013).

5.3 Impact heterogeneity

In this section we present an analysis of impact heterogeneity by computing the DD estimators separately for different population subgroups. We first divide the analysis by rural and urban areas, as well as by indigenous and non-indigenous groups. Then we restrict the analysis to women, and test for impact variation between married and single women, and between women with and without children.

Table 3: The long(er)-term effects of Chile Solidario: heterogeneous effects on the full sample

(a) Years of education	(1)	(2)	(3)	(4)
	Rural	Urban	Indigenous	Not indigenous
DD	-0.037	1.661***	1.377***	1.237***
	(0.6418)	(0.4693)	(0.4840)	(0.4764)
Treated	-1.644***	-2.469***	-1.916***	-2.332***
	(0.1832)	(0.2847)	(0.3448)	(0.2421)
Born after 1984	-0.096	-0.053	0.162	-0.083
	(0.3329)	(0.2580)	(0.4537)	(0.2231)
Observations	3108	8582	2076	9614
(b) Labour income	**(1)**	**(2)**	**(3)**	**(4)**
	Rural	Urban	Indigenous	Not indigenous
DD	338.716	305.373**	120.088	338.291**
	(234.8286)	(140.9813)	(108.3813)	(154.9368)
Treated	-96.814**	-127.556***	-36.015	-149.415***
	(44.1670)	(36.3999)	(57.6013)	(32.0532)
Born after 1984	-67.116	19.628	-7.417	12.406
	(74.0089)	(93.3345)	(79.1451)	(88.1729)
Observations	2001	6148	1441	6708

Notes: DD is the coefficient of the interaction term. Control variables include age, age-squared, number of household members, rural or urban location, region of residency dummies, ethnic background (indigenous or not), a dummy for migrants, and self-reported health. Statistical significance reported at conventional levels, * p < 0.1, ** p < 0.05, *** p < 0.01.

Source: authors' estimates based on CASEN 2013 (Ministerio de Desarrollo Social 2013).

Interestingly, we find that the effect of Chile Solidario on schooling and labour income is significant in urban areas, but insignificant in rural areas (see part (a) of Table 3). This seems to confirm the findings reported by Galasso (2011), who found that urban households were more likely to receive SUF via Chile Solidario than their rural counterparts. Carneiro et al. (2015) argue that these heterogeneous effects may be due to the remoteness of rural communities in Chile, and the associated transaction and opportunity costs of programme membership.

Furthermore, we also find significant treatment effects on schooling for both indigenous and non-indigenous groups, but the effects become insignificant for indigenous groups when we measure the treatment effects on labour income (see part (b) of Table 3). This may reflect the existence of discriminatory norms against indigenous groups that prevail in the labour market in Chile, and which ethnographic and anthropological research has long emphasized (e.g., Merino et al. 2009).

When we disentangle the analysis by different groups of women, we find an interesting degree of heterogeneity (Table 4). First, we do not find any sizeable effect of the programme on the labour income of married women and women with children.[21] In contrast, for women without children, the programme treatment effects are found to be large and statistically significant. Our results are consistent with economic theory about labour supply within the household (Becker 1985).[22] Second, for the case of schooling, our estimates show significant effects for all women, although the treatment effects were strongest for women without children, followed by single women.

Table 4: The long(er)-term effects of Chile Solidario: heterogeneous effects for subgroups of women

(a) Years of education	(1)	(2)	(3)	(4)
	Married or in relationship	Single	No children	With children
DD	1.156*	1.640**	3.606**	0.959**
	(0.6148)	(0.7415)	(1.4892)	(0.4263)
Treated	-2.288***	-2.315***	-4.868***	-2.134***
	(0.2303)	(0.4781)	(1.3404)	(0.1814)
Born after 1984	-0.651**	0.161	-0.840	-0.274
	(0.3113)	(0.4047)	(0.6566)	(0.2749)
Observations	4861	2038	368	7277
(b) Labour income	**(1)**	**(2)**	**(3)**	**(4)**
	Married or in relationship	Single	No children	With children
DD	29.924	69.393	441.021***	35.739
	(104.4440)	(109.4839)	(163.6477)	(72.5578)
Treated	-159.937***	-124.488**	-466.370***	-127.336***
	(36.1602)	(49.4862)	(144.5543)	(25.5823)
Born after 1984	-75.075	-19.149	-415.544	-35.201
	(84.7266)	(92.2086)	(307.2363)	(54.5334)
Observations	2249	1502	271	4049

Notes: DD is the coefficient of the interaction term. Control variables include age, age-squared, number of household members, rural or urban location, region of residency dummies, ethnic background (indigenous or not), a dummy for migrants, and self-reported health. Statistical significance reported at conventional levels, * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Source: authors' estimates based on CASEN 2013 (Ministerio de Desarrollo Social 2013).

6 Difference-in-differences with matching

As discussed in Section 3, because of the non-random nature of assignment to treatment, the treatment and control groups may differ in observable characteristics. To rule out the possibility that these differences systematically influence the outcomes of interest, in addition to our baseline estimates we compute difference-in-differences with propensity score matching estimations

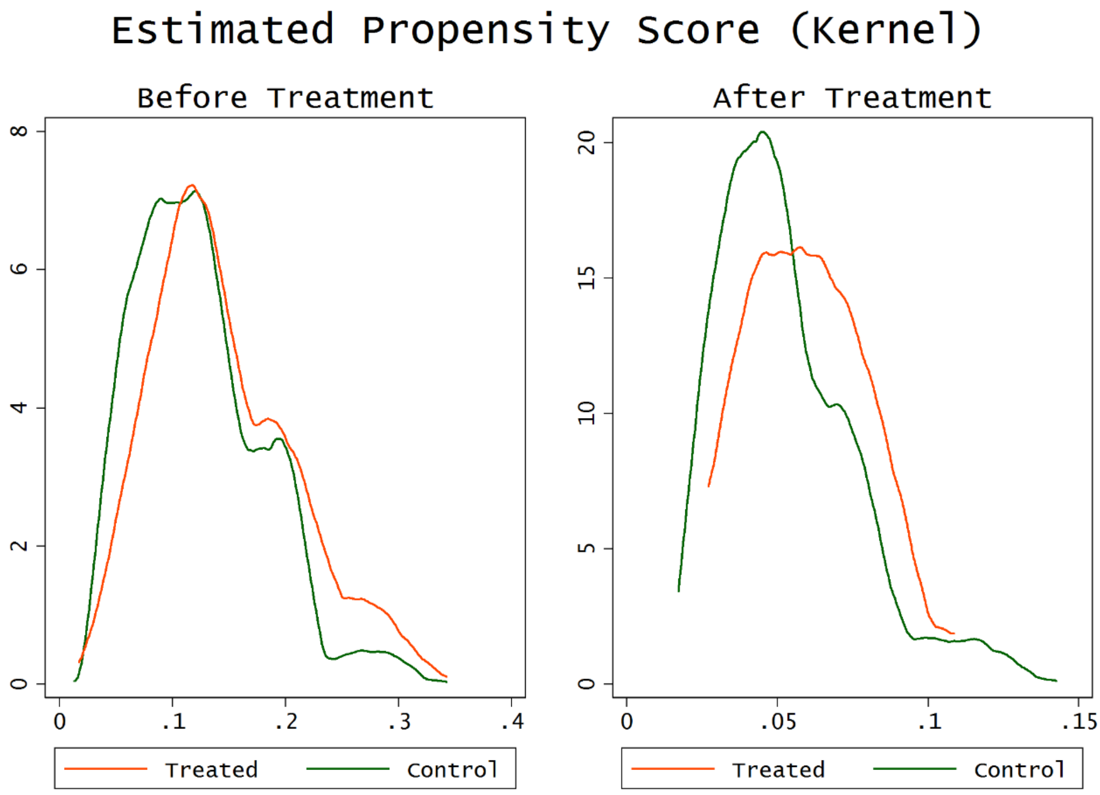
[21] The group of married women includes women cohabiting with a partner.

[22] For a review of empirical findings on the topic, see Altonji and Blank (1999).

(Heckman et al. 1997). In particularly, we apply a kernel matching estimator that maximizes the use of nearly all observations in the control group while weighting them by the distance of the propensity score.[23] This methodology relies on an additional identifying assumption of common support, i.e. enough individuals in the control group have a probability of treatment similar to the individuals in the treatment group.[24]

In order to avoid incurring biased estimates due to selection, we only use time-invariant pre-treatment variables to estimate the propensity score: year of birth, sex, place of birth, whether the individual belongs to an indigenous group, and migration background. Figure 6 shows the distribution of the propensity score for the four groups: individuals in the control and treatment groups before the intervention (cohorts born in 1973–84), and individuals in the control and treatment groups after the intervention (cohorts born in 1985–88). The graphical representation confirms that both groups share a large area of common support before and after treatment.

Figure 6: Common support



Source: authors' estimations based on CASEN 2013 (Ministerio de Desarrollo Social 2013).

Table 5 shows the results of the DD propensity score matching procedure. The treatment effect estimates do not deviate substantially from the baseline results. The effect of Chile Solidario on the schooling of men is very similar at around 1.5 additional years, while the effect on labour income is slightly lower at around US$290. The effect on the labour income of women is, again, not statistically significant. All in all, the DD with matching estimators confirms our findings that

[23] The type of kernel function adopted here is Epanechnikov.

[24] For an exhaustive discussion of the application of difference-in-differences matching to repeated cross-sectional data, as performed in our study across cohorts, see Blundell and Dias (2009).

indicate that Chile Solidario, by facilitating access to SUF, has had important long-term effects on young adults.

Table 5: The long(er)-term effects of Chile Solidario: difference-in-differences estimators with matching

Outcomes	(1) All	(2) Only women	(3) Only men
Years of schooling	1.169***	0.796*	1.544***
	(0.345)	(0.458)	(0.499)
Labour income (in US dollars)	263.290**	17.402	285.750*
	(108.768)	(83.117)	(155.061)

Notes: Displayed values are coefficients of the interaction term between the dummies for time and treatment status. Variables used to estimate the propensity score are year of birth, sex, place of birth, indigenous group, and migration background. Type of kernel function is Epanechnikov. Statistical significance reported at conventional levels, * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Source: authors' estimations based on CASEN 2013 (Ministerio de Desarrollo Social 2013).

7 Regression discontinuity

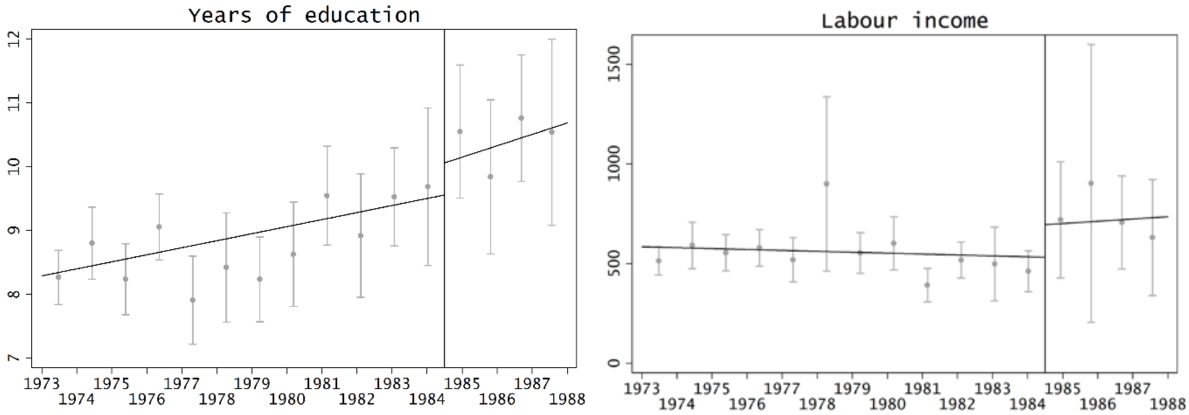
As an additional step to validate the DD estimates, we adopt a regression discontinuity design (RD) by restricting the analysis around the year of birth of individuals whose parents had no formal education. The cut-off point where the discontinuity takes place is between individuals who were 18 or older when Chile Solidario started operations in 2002, and who therefore were not eligible to receive SUF, and individuals that were younger than 18 and therefore eligible for treatment.

Because of the smaller number of observations, especially within each cohort (bin) and around the cut-off, the RD in this case is less powerful than the DD approach. However, it serves as a complementary method to validate our baseline results. Because of the programme's high take-up rates, we assume a sharp design and opt for a parametric approach using all observations at both sides of the cut-off point to obtain the RD estimates (see e.g., Hahn et al. 2001).[25]

Figure 7 graphically shows a clear discontinuity in both outcome measures—years of education and labour income—at the cut-off point. Table 6 presents the treatment effects based on the RD estimates. For men, the treatment effects are qualitatively similar to the results obtained by the DD estimators. Furthermore, the RD estimates confirm that there is no programme effect on the labour income of women. In contrast to the DD estimators, the RD results show no sizeable programme impacts on the schooling of women. Nevertheless, the RD results should be taken with caution, given their limited precision due to sample constraints.

[25] The optimal bandwidth computed with the method proposed by Imbens and Kalyanaraman (2012) is around four. When this bandwidth is chosen, the results do not change significantly.

Figure 7: Discontinuity in weighted average outcomes at cut-off



Source: authors' estimations.

Table 6: Regression discontinuity estimators: weighted sample

	(1)	(2)	(3)	(4)	(5)	(6)
	All		Men		Women	
	Unconditional	Conditional	Unconditional	Conditional	Unconditional	Conditional
Years of education	0.364	0.213	1.492	1.204	-0.436	-.0504
	0.661	0.673	1.067	1.039	0.917	0.930
N	1394	1371	567	554	827	817
Labour income (relative to cohort average)	144.43	110.77	311.83	319.57	-103.46	-134.61
	141.32	141.40	234.03	228.36	87.28	87.87
N	970	950	532	519	438	431

Notes: Covariates included to obtain conditional estimates are rural or urban location, number of household members, indigenous group, migration background, and self-reported health. Cluster robust standard errors at municipality level.

Source: authors' estimations based on CASEN 2013 (Ministerio de Desarrollo Social 2013).

8 Robustness checks

To test for the validity of our preferred DD baseline estimates, we performed a series of additional robustness checks.[26] First, we restricted our sample to the cohorts of individuals born in 1981–88 to avoid age effects affecting co-residence in the household of origin. The results are presented in section (c) of Table A4 in the Appendix. Second, we set missing values in labour income to zero when information on employment status was available but the individual was indicated as unemployed or inactive. The results are presented in section (e) of Table A4. Third, we split the control group between individuals whose parents had incomplete and complete primary education, and ran a model including fixed effects at those groups levels. The results are presented in section

[26] The results of the robustness checks are reported in sections (a–g) of Table A4 in the Appendix.

(g) of Table A4. Fourth, we ran all estimations without weighting by the inverse probability of selection provided by the survey. The results are presented in section (a) of Table A4.[27]

Fifth, although we tested exhaustively for the existence of common trends prior to the treatment, the assumption of common trends post-treatment in the absence of the intervention cannot be verified, because it relies on a counterfactual scenario. As further examination, we ran a placebo test by restricting the sample to pre-treatment cohorts and setting individuals born in 1982 as the first cohort affected by Chile Solidario. In this analysis, the coefficient of the interaction term between the dummies for time and treated was not significantly different from zero. The same applied to two further placebo tests that we performed: one that replaced the treated group with individuals whose parents had incomplete secondary education, and another that assessed the effect of Chile Solidario on non-labour income, which was an outcome that we expected not to have been affected by the programme. All these placebo tests are presented in section (g) of Table A4.

A possible source of measurement error in our treatment effect estimates on labour income could be the presence of outliers. An exploration of our data reveals that there are very few observations in the treatment group above US$5,000 in monthly labour income, and among these there is only one after the treatment. Excluding these observations yields lower but consistent estimates of the effect on labour income, at around US$230 for men (see section (f) of Table A4).

Finally, the migration of individuals affected by the reform could be a source of upward bias in our estimates, driven by selectivity. For instance, some individuals might have migrated from rural areas, where they spent their childhood, to urban areas, partly facilitated by the monetary resources that the programme provided. If that was the case, and migration decisions had led to higher incomes, the effects of the programme would still be positive, but at the cost of yielding downward-biased estimates. To account for this potential source of bias, we ran additional models by separating individuals that still lived in the municipality where they were born from those that lived in different locations from their place of birth. A test of the two coefficients yielded no significant differences (see section (b) in Table A4). Overall, our results remained consistent in terms of direction and statistical significance after these robustness checks were performed.

9 Conclusions

In this study, we have examined the long(er)-term effects of Chile Solidario, a social programme that provides the poorest households with preferential access to Subsidio Unico Familiar, a conditional cash transfer with the explicit objective of improving children's human capital. We focused on the educational attainment and labour income of cohorts of adults who were eligible as children to receive the cash transfer at the time Chile Solidario was launched, relative to cohorts of individuals who were not eligible to receive the cash transfer due to their slightly older age. We adopted a difference-in-differences approach and regression discontinuity design to measure the impact of the programme.

Overall, we find strong and persistent treatment effects that quantitatively are in the order of 1.2 additional years of schooling and approximately US$200–250 per month, which represents about 15 per cent of the Chilean average monthly labour income. Our study contributes to the scant

[27] As pointed out by Solon et al. (2015), under certain conditions weighting might be unnecessary, and even harmful, for obtaining causal parameters.

literature on long(er)-term effects of CCTs, and provides evidence that cash transfer programmes can, under certain conditions, contribute to positive changes in the future living conditions of beneficiary children.

The facts that we find insignificant average treatment effects on labour income among women, and that the completion of secondary education is the most likely mediating channel through which school attainment leads to increases in labour income among men, indicate that the impact of Chile Solidario certainly depends on societal norms and structural factors that underpin the functioning of labour markets in Chile.

Further research is needed to better understand the incentive mechanisms and societal norms that seem to be inhibiting women from taking full advantage of the opportunities that CCTs generate for their future.

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Appendix

Table A1: Chilean educational system and median income in US dollars (cohorts 1977–88)

Years	Type			Median total income (weighted)	Median labour income (weighted)
	Tertiary				
22	PhD			2,426.40	2,426.40
21				2,022.00	2,047.28
20	Master			3,038.06	3,922.68
19				1,722.07	1,819.58
18	Bachelor			1,834.60	1,834.60
17				1,273.86	1,307.56
16		Professional		958.77	913.94
15				788.58	781.35
14			Technical	721.18	709.08
13				633.56	626.82
	Secondary				
12	Academic track	Vocational track		525.72	512.24
11				444.84	427.99
10	General secondary education			444.84	424.62
9				444.84	424.62
	Primary				
8	General primary education			427.99	424.62
7				404.40	303.30
6				404.40	250.65
5				424.62	343.74
4				424.62	374.07
3				424.62	390.25
2				494.92	474.18
1				404.40	214.45
0	No formal educational degree			404.40	165.92

Source: authors' elaboration based on CASEN 2013 (Ministerio de Desarrollo Social 2013).

Table A2: Population by level of education in 2003 (weighted frequency): only individuals in age interval 30–60

Educational level	Weighted frequency	Per cent	Cumulative
No education	123,699	2.03	2.03
Primary (incomplete)	1,085,745	17.80	19.82
Primary (complete)	777,081	12.74	32.56
Secondary (incomplete)	1,102,511	18.07	50.63
Secondary (complete)	1,836,578	30.10	80.74
Tertiary (incomplete)	569,016	9.33	90.06
Tertiary (complete)	606,163	9.94	100.00
Total	6,100,793	100.00	

Source: authors' calculations based on CASEN 2003 (Ministerio de Desarrollo Social 2003).

Table A3: Population by level of parental education in 2013 (weighted frequency): only individuals in sample (born 1973–88)

Parental educational level	Weighted frequency	Per cent	Cumulative
No education	89,915	4.37	4.37
Primary (incomplete)	421,830	20.52	24.89
Primary (complete)	347,223	16.89	41.78
Secondary (incomplete)	272,944	13.28	55.05
Secondary (complete)	599,218	29.14	84.20
Tertiary (incomplete)	82,506	4.01	88.21
Tertiary (complete)	242,374	11.79	100.00
Total	2,056,010	100.00	

Source: authors' calculations based on CASEN 2003 (Ministerio de Desarrollo Social 2003).

Table A4: Sensitivity analysis

(a) Unweighted estimates

Years of education	(1) Unconditional	(2) Conditional	(3) Only women	(4) Only men
DD	1.318***	1.079***	0.890**	1.392***
	(0.3011)	(0.2813)	(0.3965)	(0.3815)
Treated	-2.276***	-1.982***	-2.003***	-1.954***
	(0.1097)	(0.1075)	(0.1284)	(0.1679)
Born after 1984	0.466***	-0.045	-0.210	0.270
	(0.0633)	(0.1378)	(0.1711)	(0.2473)
Observations	11821	11690	7661	4029

Labour income (in US dollars)	(1) Unconditional	(2) Conditional	(3) Only women	(4) Only men
DD	290.668***	281.291***	22.186	373.473***
	(105.1736)	(100.7388)	(68.9467)	(140.6176)
Treated	-115.975***	-106.991***	-107.458***	-142.176***
	(28.6302)	(17.7610)	(19.2513)	(28.1761)
Born after 1984	-106.401***	-2.381	-15.887	-26.501
	(15.7742)	(34.9676)	(42.5673)	(53.2606)
Observations	8244	8149	4330	3819

(b) Estimates by migration status

When individual was born, mother lived in…	(1) Same municipality	(2) Other municipality	(3) Same municipality	(4) Other municipality
	Years of education		Labour income	
DD	1.187***	1.482**	323.496**	307.772**
	(0.4163)	(0.6992)	(154.6290)	(124.8344)
Treated	-2.188***	-2.366***	-137.634***	-156.134***
	(0.1936)	(0.3125)	(26.7058)	(48.4374)
Born after 1984	-0.356	0.295	-114.717	57.158
	(0.2505)	(0.4739)	(83.9791)	(165.5762)
Observations	7101	4433	4863	3175

(c) Symmetric time window: cohorts 1981–88

Years of education	(1) Unconditional	(2) Conditional	(3) Only female	(4) Only male
DD	0.806**	0.683*	0.543	1.047*
	(0.3781)	(0.3692)	(0.5915)	(0.5822)
Treated	-1.765***	-1.616***	-1.689***	-1.873***
	(0.2318)	(0.2357)	(0.4600)	(0.4351)
Born after 1984	0.179**	0.026	-0.098	0.024
	(0.0749)	(0.1641)	(0.2866)	(0.3634)
Observations	4725	4668	3190	1478

Labour income	(1) Unconditional	(2) Conditional	(3) Only female	(4) Only male
DD	361.675***	341.367***	23.795	378.425***
	(103.7038)	(102.6415)	(75.0317)	(142.1769)
Treated	-186.982***	-165.003***	-114.749***	-219.162***
	(32.6622)	(31.4563)	(39.6354)	(62.1639)
Born after 1984	-71.869***	-45.365	-46.271	-215.213
	(18.8256)	(36.9070)	(79.3911)	(202.8477)
Observations	3180	3137	1738	1399

(d) Full set of dummies for cohort and group

	(1)	(2)	(1)	(2)
	All		Men	
	education	Labour income	education	Labour income
DD	1.318***	274.576***	1.612***	346.659***
	(0.3615)	(97.8724)	(0.6144)	(129.3125)
Cohort=1974	-0.218	-0.557	0.591	51.332
	(0.3070)	(55.3554)	(0.4320)	(78.7949)
Cohort=1975	0.150	111.644	0.992***	292.081**
	(0.2662)	(76.9224)	(0.2011)	(143.5143)
Cohort=1976	0.681*	62.439	2.024***	144.424*
	(0.4130)	(58.4727)	(0.5499)	(85.1970)
Cohort=1977	-0.072	5.961	0.676**	79.188
	(0.2566)	(63.6553)	(0.3034)	(102.1953)
Cohort=1978	0.008	49.109	1.026***	61.418
	(0.3043)	(65.7189)	(0.3400)	(72.9416)
Cohort=1979	-0.090	7.352	1.250***	45.755
	(0.2961)	(68.5127)	(0.3288)	(80.8862)
Cohort=1980	0.389	9.110	1.096***	71.307
	(0.2934)	(58.0909)	(0.2785)	(69.0072)
Cohort=1981	0.128	-82.697*	1.058***	-107.628
	(0.2571)	(49.4522)	(0.2849)	(72.6972)
Cohort=1982	0.148	-17.525	1.034***	-33.064
	(0.2658)	(63.0549)	(0.2705)	(62.7243)
Cohort=1983	0.385	-77.509	1.022***	-23.130
	(0.2733)	(54.2269)	(0.2924)	(82.5443)
Cohort=1984	0.221	5.516	0.842**	135.885
	(0.3033)	(105.0822)	(0.3555)	(202.2534)
Cohort=1985	0.307	-97.861	1.039***	-86.457
	(0.2905)	(94.0053)	(0.2654)	(134.7856)
Cohort=1986	0.226	-118.243*	0.750**	-135.008*
	(0.2771)	(64.0791)	(0.3461)	(71.7027)
Cohort=1987	0.037	-205.729***	0.932***	-225.521***

	(0.2864)	(50.3126)	(0.3013)	(68.8483)
Cohort=1988	0.289	-152.415***	0.866**	-108.170
	(0.2811)	(55.8227)	(0.3592)	(74.6269)
Incomplete primary	1.879***	88.329***	2.072***	145.732***
	(0.1924)	(27.6184)	(0.3348)	(45.1256)
Complete primary	2.817***	189.980***	2.900***	186.067***
	(0.2241)	(41.0139)	(0.3905)	(57.2291)
Observations	11690	8149	4029	3819

(e) Labour income (including missing values coded to zero)

	(1) Unconditional	(2) Conditional	(3) Only female	(4) Only male
DD	224.471***	243.459***	29.407	402.630***
	(81.9665)	(79.1451)	(51.8247)	(127.8891)
Treated	-128.104***	-119.386***	-103.361***	-238.256***
	(34.5647)	(26.9736)	(26.9738)	(53.9097)
Born after 1984	-79.109***	-12.545	-25.239	-139.609
	(23.5408)	(67.0366)	(56.6941)	(175.5527)
Observations	7737	7642	5084	2558

(f) Labour income (excluding outliers)

	(1) Unconditional	(2) Conditional	(3) Only female	(4) Only male
DD	198.048***	197.265***	51.903	232.738***
	(62.2935)	(65.2343)	(69.3246)	(82.2140)
Treated	-163.350***	-133.877***	-152.067***	-159.591***
	(32.4837)	(29.2437)	(28.7532)	(41.1410)
Born after 1984	-110.948***	-22.264	-47.974	-62.458
	(30.5436)	(88.4478)	(74.1361)	(163.6848)
Observations	8241	8148	4330	3818

(g) Placebo tests

Placebo test I (treated group = parental educational background is incomplete secondary education): years of schooling

	(1) Unconditional	(2) Conditional	(3) Only female	(4) Only male
DD	-0.354*	-0.220	-0.391*	0.175
	(0.2087)	(0.2076)	(0.2166)	(0.4993)
Treated	1.012***	0.721***	0.682***	0.817***
	(0.1277)	(0.1233)	(0.1408)	(0.2893)
Born after 1984	0.211**	0.081	-0.017	0.314
	(0.0972)	(0.2387)	(0.2553)	(0.3889)
Observations	13643	13495	9016	4479

Placebo test I (treated group = parental educational background is incomplete secondary education): labour income

	(1) Unconditional	(2) Conditional	(3) Only female	(4) Only male
DD	-79.305	-37.607	-47.787	-28.172
	(87.0749)	(93.3341)	(54.4070)	(186.3380)
Treated	128.717***	71.446	72.158	107.962
	(40.8246)	(47.6297)	(47.1728)	(90.6698)
Born after 1984	-110.948***	-4.514	29.831	-144.018
	(30.5429)	(72.1483)	(69.2136)	(146.9744)
Observations	9622	9520	5273	4247

Placebo test II (cohort 1982 set as first eligible cohort): years of schooling

	(1) Unconditional	(2) Conditional	(3) Only female	(4) Only male
DD	0.050	0.133	0.184	0.094
	(0.4356)	(0.4322)	(0.5082)	(0.7272)
Treated	-2.500***	-2.283***	-2.252***	-2.372***
	(0.2422)	(0.2216)	(0.2112)	(0.3675)
Born after 1984	0.277***	0.188	-0.126	0.751*
	(0.1054)	(0.2932)	(0.3207)	(0.4468)
Observations	9713	9607	6240	3367

Placebo test II (cohort 1982 set as first eligible cohort): relative labour income

	(1) Unconditional	(2) Conditional	(3) Only female	(4) Only male
DD	-36.016	18.610	94.707	-65.625
	(60.5287)	(60.5793)	(58.8306)	(101.4904)
Treated	-154.559***	-135.490***	-164.824***	-150.229***
	(34.3338)	(32.1578)	(30.9652)	(45.3278)
Born after 1984	-19.678	70.512	56.413	126.103
	(37.7315)	(68.4334)	(89.9746)	(91.0683)
Observations	6856	6780	3583	3197

Placebo test III (outcome = non-labour income)

	(1) Unconditional	(2) Conditional	(3) Only female	(4) Only male
DD	-1.212	-0.841	-10.043	15.425
	(11.0848)	(11.5517)	(25.5814)	(9.4991)
Treated	-6.390	-7.157	4.743	-11.452***
	(8.1304)	(8.0673)	(16.6131)	(3.6644)
Born after 1984	-9.330**	-6.905	-7.323	-0.631
	(3.8870)	(7.1667)	(13.4007)	(8.3482)
Observations	8244	8149	4330	3819

Notes: DD is the coefficient of the interaction term. Control variables include age, age-squared, number of household members, rural or urban location, region of residency dummies, ethnic background (indigenous or not), a dummy for migrants, and self-reported health. Statistical significance reported at conventional levels, * p < 0.1, ** p < 0.05, *** p < 0.01.

Source: authors estimations based on CASEN 2013 (Ministerio de Desarrollo Social 2013).

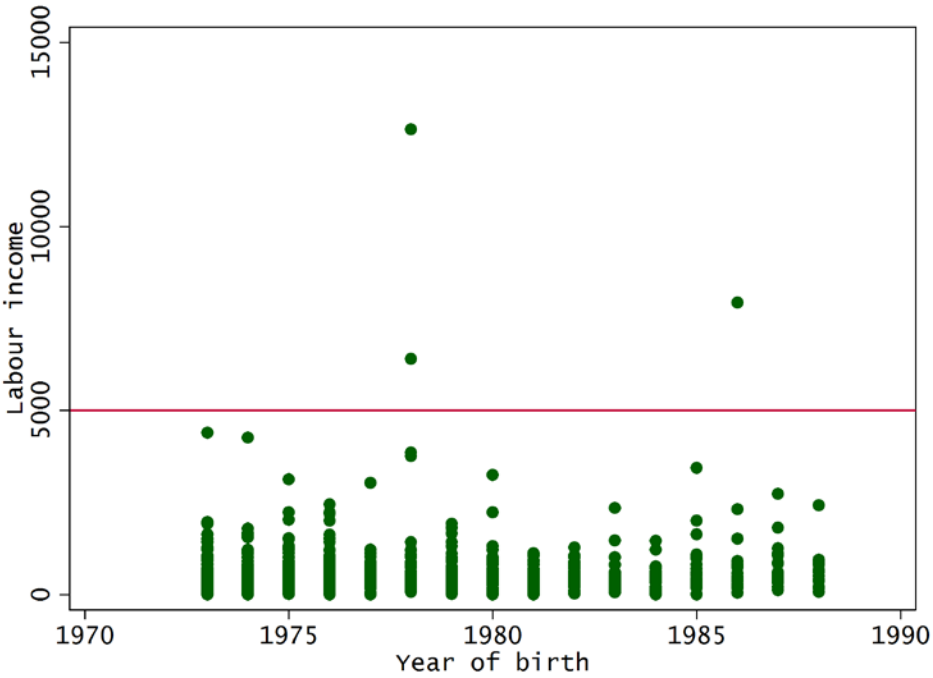
Table A5: Probability of completed secondary education (linear probability model)

	(1) Unconditional	(2) Conditional	(3) Only female	(4) Only male
DD	0.169**	0.124*	0.104	0.184*
	(0.0727)	(0.0689)	(0.0731)	(0.1058)
Treated	-0.296***	-0.254***	-0.269***	-0.244***
	(0.0264)	(0.0241)	(0.0271)	(0.0385)
Born after 1984	0.062***	-0.008	-0.051	0.081
	(0.0177)	(0.0411)	(0.0460)	(0.0870)
Observations	11818	11687	7659	4028

Notes: DD is the coefficient of the interaction term. Control variables include age, age-squared, number of household members, rural or urban location, region of residency dummies, ethnic background (indigenous or not), a dummy for migrants, and self-reported health. Statistical significance reported at conventional levels, * p < 0.1, ** p < 0.05, *** p < 0.01.

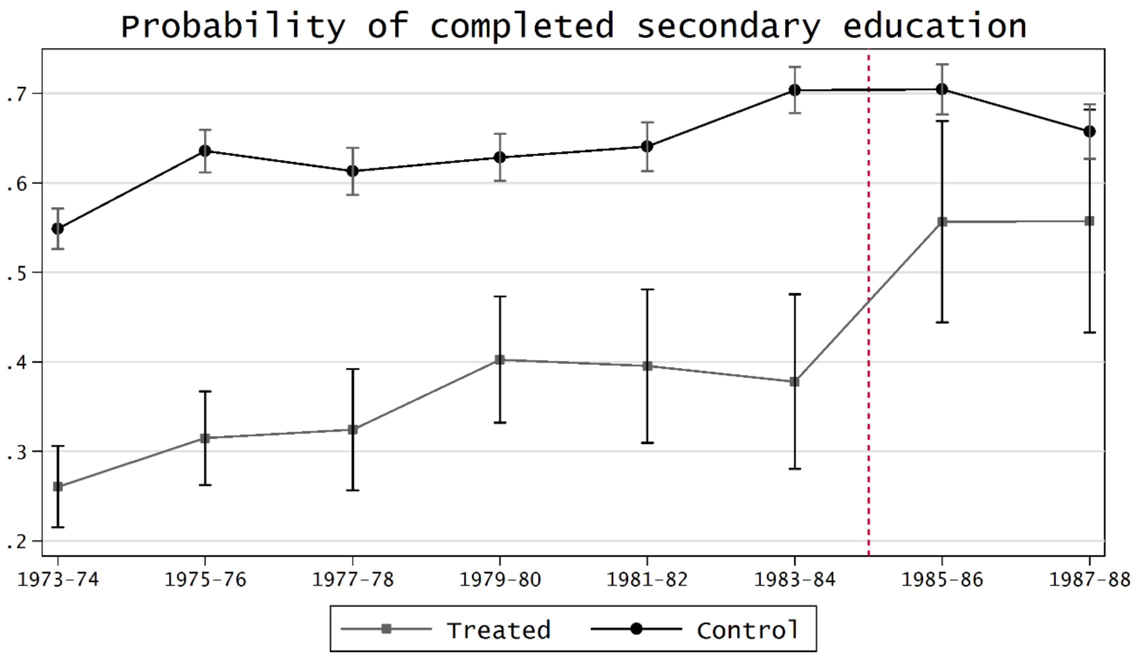
Source: authors' estimations based on CASEN 2013 (Ministerio de Desarrollo Social 2013).

Figure A1: Outliers in treatment group by year of birth



Source: authors' estimations based on CASEN 2013 (Ministerio de Desarrollo Social 2013).

Figure A2: Probability of completed secondary education



Source: authors' estimations based on CASEN 2013 (Ministerio de Desarrollo Social 2013).