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The long lasting effects of a conditional cash transfer on children's human capital

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Abstract

A social program that started 2002 in Chile increased the take-up of a conditional cash transfer (CCT) among poor families. We use the introduction of this program as a natural experiment to evaluate the long lasting impacts of the CCT. To identify causal effects, we exploit the exogenous variation in the eligibility of children from different age groups for the CCT when the program started. Our results show that the short run achievements of the program in linking the poor to the social protection scheme had a persistent effect on children's human capital, as measured by educational attainments and labor income at age 25-28.

Keywords: Social transfers, Conditional Cash Transfers (CCTs), long-term effects, child development, human capital investment, Latin America, Chile Solidario, Single Family Subsidy (Subsidio Unitario Familiar).

JEL classification: I21, I38, J24, O15

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1. Introduction

Short-run evaluations of conditional cash transfer programs show that these programs can successfully improve the outcomes of beneficiary children in many dimensions such as schooling, health, and cognitive abilities (Almond & Currie, 2011; Fiszbein & Schady, 2009). However, scholars have argued that despite the existence of these short-run impacts, the essential effects on the human capital of poor children might be modest and not able to reduce the intergenerational transmission of poverty (Levy & Schady, 2013). To verify these concerns and clearly understand the more intricate capacity of social transfer programs to support equality of opportunity through the persistent improvement of children's human capital, a longer analytical perspective is crucial. Nevertheless, while short run effects of social transfer programs have been extensively evaluated, evidence on long-term impacts, for instance on labor market outcomes of children from poor households, remains scarce and mostly restricted to early stages of the labor market insertion (as pointed out in recent reviews by Molina-Millan et al. 2016 and Saavedra 2016).

Our main contribution is to evaluate these long lasting effects in the framework of *Chile Solidario*; an innovative social program that started in 2002 and served as an example for social programs in several other countries. One of the main tools of Chile Solidario to help families out of poverty is to increase their awareness about being eligible for yet existing social transfers. Recent evaluations of the short and medium-run effects of this program shows that the program was indeed very successful fulfilling this objective, with a substantially higher take up of transfers among participating households (Carneiro, Galasso, & Ginja, 2015; Galasso, 2011). We focus on one particular conditional cash transfer (CCT) intended to support the investment in children's human capital. Our identification strategy exploits the exogenous variation given by the fact that only children in a particular age interval are eligible for the CCT. Hence, among individuals that spent their childhood in households that participated to Chile Solidario, certain cohorts of individuals should have benefited from the CCT while others not. This occurrence should constitute an interesting natural experiment in order to measure the causal effects of the CCT.

We adopt a difference in difference methodology, later extended by propensity score matching, on nationally representative household survey data that allow us to gather information on the household were the individual grew up through retrospective questions. Therefore, we are able to measure the impacts of the program on the long run outcomes at the age of 25-28 of beneficiary children, even if these children left their household of origin. To the best of our knowledge, this study is the first long-term evaluation of this particular program, and contributes to the scant literature on long-run impacts of social transfers on the human capital of poor children.

Our results show that the CCT, and consequently the capacity of the social program to rise the take-up of this transfer among extremely poor households, had a positive and long lasting impact. Individuals that spent their childhood in extreme poor households and were in an eligible age to receive the CCT have significantly higher schooling and labor income as adults than individuals from poor families that were not eligible for the transfer. Our average estimates of the effect are almost two years of schooling and about 25 % of the Chilean mean wage.

There is, however, also an interesting amount of heterogeneity in our estimates. The effects on men and women are rather similar in schooling, but completely driven by men in the case of labor income. Further analyses show, that the impact is not significantly different from zero for women with children, while positive and significant for the childless. The effects seem furthermore to be concentrated in urban areas, confirming past findings on short-run impacts. Summing up, our study shows that the positive effects found by short run evaluations of the program in past, for instance on schooling and health outcomes, seem to translate into higher educational achievements and labor outcomes in the long-run. Hence, the intervention persistently improved the human capital of children from poor households.

The paper is organized as follows: Section 2 gives a short overview of the institutional background and describes the social program. Section 3 presents the data, our identification strategy and the employed method for the baseline analysis. In Section 4, all our results are presented and discussed: First, the baseline results on the impact of the social program (4.1) and the intensity of the effect (4.2). Then, heterogeneous effects by different subgroups (4.3). Furthermore, to proof the consistency of the estimates we perform a series of robustness checks (4.4) and run our estimations following an alternative methodology (4.5). Section 5 concludes.

2. Background

In Chile, a period of sustained income growth and proactive social policies in the 1990s helped to decrease overall poverty substantially, while in the same period extreme poverty rates stayed on a stable level of 6% of the population. This controversial development was argued to depend mainly on the lack of information of extremely poor families about the social protection scheme and the application process to access it (Galasso, 2011). In response, the Chilean government started Chile Solidario in May 2002 with the objective to address the program gradually to about 260,000 poor households in the country.[1] The program is since then intended to address families in extreme poverty – identified through a proxy means test score defining the satisfaction of basic needs – and provides a battery of measures to sustain their way out of poverty.[2] Participation of eligible households is notably high around 95%, with very low drop-out rates (around 3% of all invited households; Galasso, 2011).

[1] From 2002 to 2006, each year about 50,000 households were invited to participate.

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

In particular, the program provides, especially in the first part of the intervention (24 month, the so-called 'bridge program' *Programa Puente*), psychosocial support to the family, a small cash transfer at a decreasing rate over time, and, in parallel, preferential access to existing monetary subsidies and other social programs. One of its main purposes is, indeed, to link poor households to the Chilean social policy scheme (Fiszbein & Schady, 2009).

In this sense, Chile Solidario differs from other cash transfer programs in Latin America. First, it combines income support with non-monetary interventions, including social workers who are actively involved in deciding the type of supportive measures that households may need to get out of extreme poverty. Second, the cash transfer itself is not the main feature of the program, but it acts as an incentive device to motivate households to commit to certain actions to improve the wellbeing of their children and their economic conditions.

One of the main aims of the program is to raise the awareness of the poor about the existing social transfers they are eligible for; in particular, those transfers intended to improve investment in children's human capital, like the Single Family Subsidy (*Subsidio Unico Familiar*, SUF). Indeed, the program was designed with the specific objective of supporting families that systematically underinvest in children's human capital (Fiszbein & Schady, 2009). Evaluations of the program in the short run show that the program fulfilled this objective, linking the poor to the social system. Recently, Carneiro et al. (2015) show that one of the most important accomplishments was the take up of the SUF by extremely poor families with children in eligible age.

SUF itself, counts as one of the oldest conditional cash transfers, having started in 1981. As in the case of Chile Solidario, a proxy-mean score identifies program eligibility. However, the threshold for SUF is higher, being the transfer targeted to the bottom 40% of the distribution. The monthly payment (approximately 6 USD in 2003, less than 10 % of total household income of poor families) is delivered to the mother under the conditionality of, i) having children under the age of 18, ii) the children attending school regularly if 6 to 18 years old, iii) the children attending medical controls regularly if under the age of 6.[3] Additional transfers are available to young mothers and disabled persons.

Past studies have primarily focused on the short term effects of Chile Solidario and agree in the main accomplishments of the program. Galasso (2011), Guardia et al. (2011) and Carneiro et al. (2015) find a significant increase in the take up of social subsidies, particularly strong among families who had little access to the welfare system. Furthermore, these studies find no negative employment effects. The latter is confirmed by Martorano and Sanfilippo (2012) who also find that the program alleviated poverty and increased school participation and enrolment in public health services among children of participating households. To the best of our knowledge, this is the first study evaluating the long-term effects of Chile Solidario.[4] This perspective is particularly important because it allow us to complement the existing evidence on short and medium-run effects and examine the more structural and transformative impacts that the program may have achieved.

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

[4] Generally, the number of studies measuring the long-run impacts of social programs is still quite limited. Among the few, recent evaluations for Latin America include Baez & Camacho (2011) and Barrera-Osorio, Linden, & Saavedra (2015) for Colombia, Barham, Macours, & Maluccio (2013) for Nicaragua, and Behrman, Parker, & Todd (2011) for Mexico. For recent reviews on the topic, see Molina-Millan et al. 2016 and Saavedra 2016.

3. Data and Method

3.1. Data and Identification Strategy

The data used in this study derives from the CASEN survey, a nationally representative cross-sectional household survey implemented since 1985 by the Chilean Ministry of Planning (MINDEPLAN). The main evaluations on adult outcomes are performed with the 2013 wave of CASEN. Some additional analyses are performed with the 2003 wave, which is the first CASEN survey after implementation of Chile Solidario in 2002.[5] The CASEN survey is particularly suitable for our analysis because of its rich content in retrospective information that enable to reconstruct the circumstances experienced in childhood by adult respondents and control for individual characteristics. Furthermore, since Chile Solidario is a nation-wide program, a nationally representative household survey allows us to evaluate whether it had a sizeable overall effect on certain demographic groups. We restrict our sample to adult individuals aged 25-36 with available information on education, income, and parental educational background.[6]

Treatment and Control Group

The ideal treatment group for our evaluation is formed by individuals who spent their childhood in extremely poor households that were eligible for Chile Solidario; i.e. households that had a higher pre-treatment likelihood to be unaware of their eligibility for SUF and other social transfers. Accordingly, the control group should

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[5] For further information, see the methodological report of the CASEN survey (MINDEPLAN 2013).

[6] All estimates are obtained weighting by the inverse probability of selection, provided by the survey design. However, as Solon et al. (2015) point out, under certain conditions weighting might not be necessary and even harmful to obtain causal parameters. Therefore, we run also unweighted regressions to obtain our estimates. Results do not change significantly.

comprise individuals who spent their childhood in eligible households for SUF (bottom 40 percent of the income distribution) but not in state of extreme poverty and therefore not eligible for Chile Solidario. Consequently, the take up of SUF should have risen drastically only in the treated group after introduction of Chile Solidario in 2002, as shown by Carneiro et al. (2015).

Unfortunately, we do not have information on whether the household where the individual grew up was actually eligible, and participated, to the social program. Thus, our identification of treatment and control group rely on a proxy measure for the income level of the household where the individual spent his childhood. In the CASEN survey, we can identify the circumstances faced in childhood by individuals through retrospective questions about their parental educational background and chose our treatment and control group relying on these information.[7] The intuition behind this approach is that households with lower levels of education face a higher risk of poverty and thus eligibility for the social transfer program. Indeed, as shown by Galasso (2011) 2/3 of the participating household heads and spouses in the first years of Chile Solidario had not completed primary education. Our examination of the 2003 wave of CASEN confirms this. Figure 1 shows that the conditional probabilities to be extremely poor and eligible for Chile Solidario are higher for individuals with no formal education.

At the same time, Figure 2 shows predicted total household income and amount of SUF received for each level of education. Household income is low for individuals with no or only primary education and turns substantially higher with increasing lev-

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[7] An advantage of this approach is that we can identify adult individuals even if they left their household of origin. The procedure enables furthermore to measure the long run outcomes of the program reducing substantially the bias deriving from co-residency and sample attrition (Emran, Greene, & Shilpi, 2016).

els of education. The amount of SUF received is relatively close in the first educational categories. It is conspicuous that among individuals with no formal education the amount is lower than would be expected, measured in comparison to the group of people with incomplete primary education. This finding could be a suggestive evidence confirming lower take up of this subsidy among extremely poor households in 2003. Hence, all evidence suggests that parental education is a useful (imperfect) proxy for treatment status of the household where the individual presumably spent his childhood.

To avoid possible sources of upward bias in our estimates, we chose treatment and control group conservatively. The treated group comprises only individuals whose parents have no education, the control group only individuals with at least some primary education but no secondary education. We exclude individuals with parental educational background above that threshold from the analysis. Figure 3 shows the sample averages of the outcomes under evaluation – years of education and labor income – for each of the seven categories displaying the composition of treatment and control group.

Figure 1. Predicted probability of: a) eligibility for Chile Solidario, and b) extreme poverty by educational level (Conditional on age, sex, region and rural or urban area; Only individuals in age interval 30-60). Bootstrap confidence intervals. Source: CASEN 2003, own estimations.

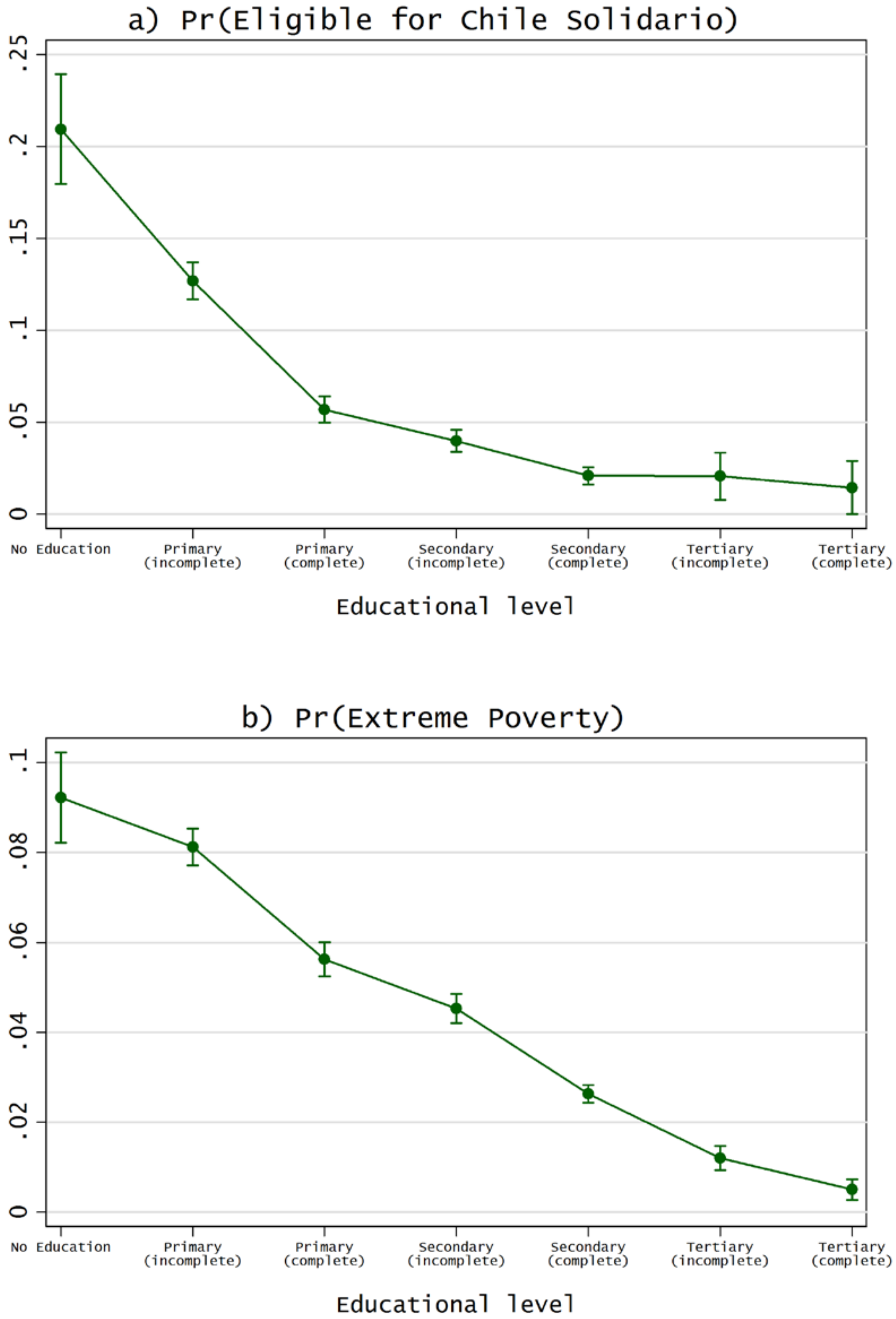
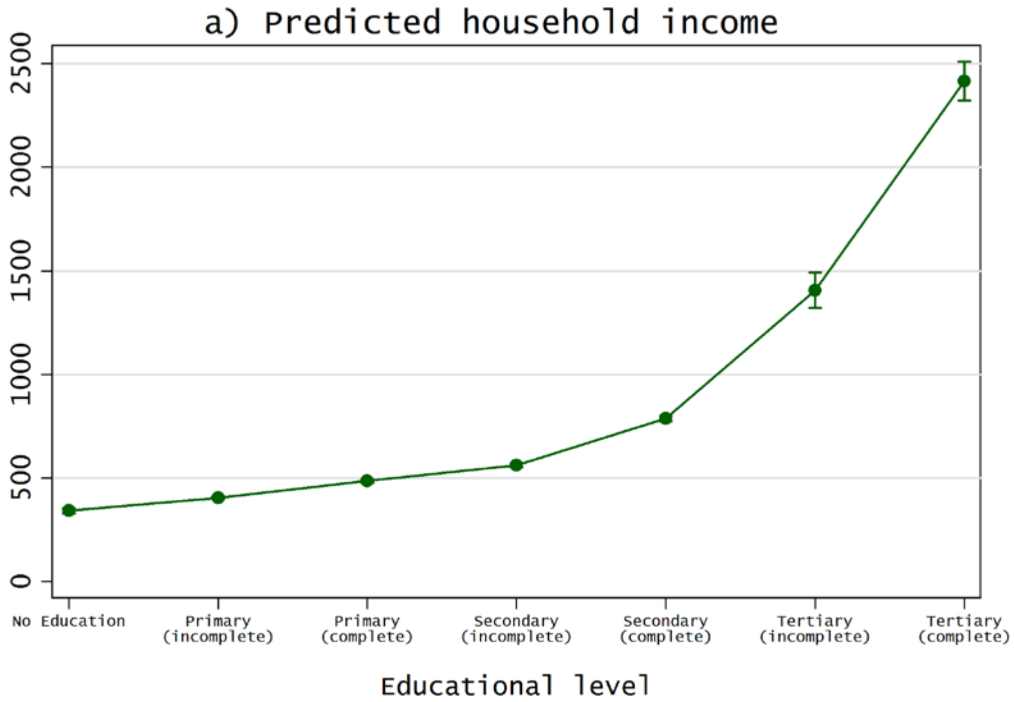


Figure 2. Predicted a) total household income, and b) amount of SUF (both in USD) by educational level (Conditional on age, sex, region and rural or urban area; Only individuals in age interval 30-60). Bootstrap confidence intervals. Source: CASEN 2003, own estimations.



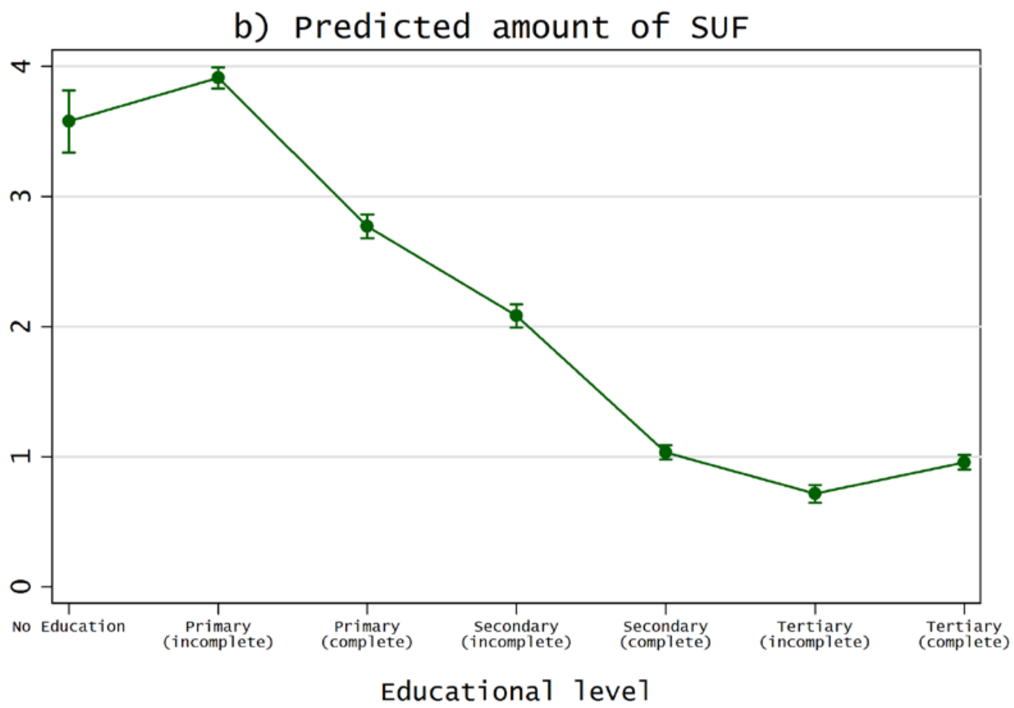
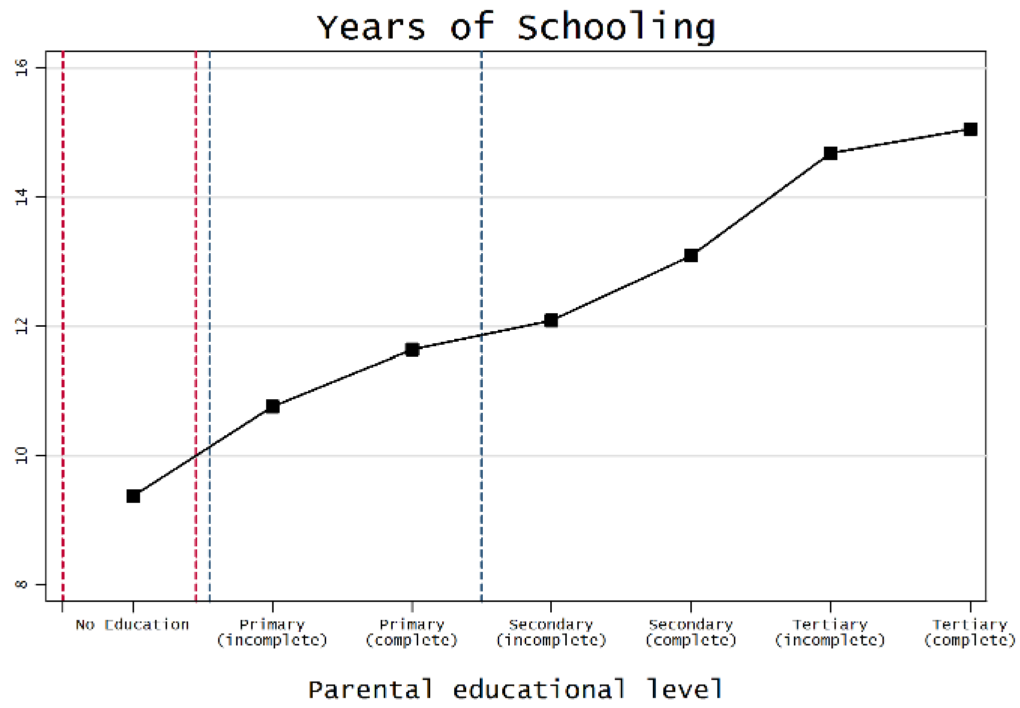
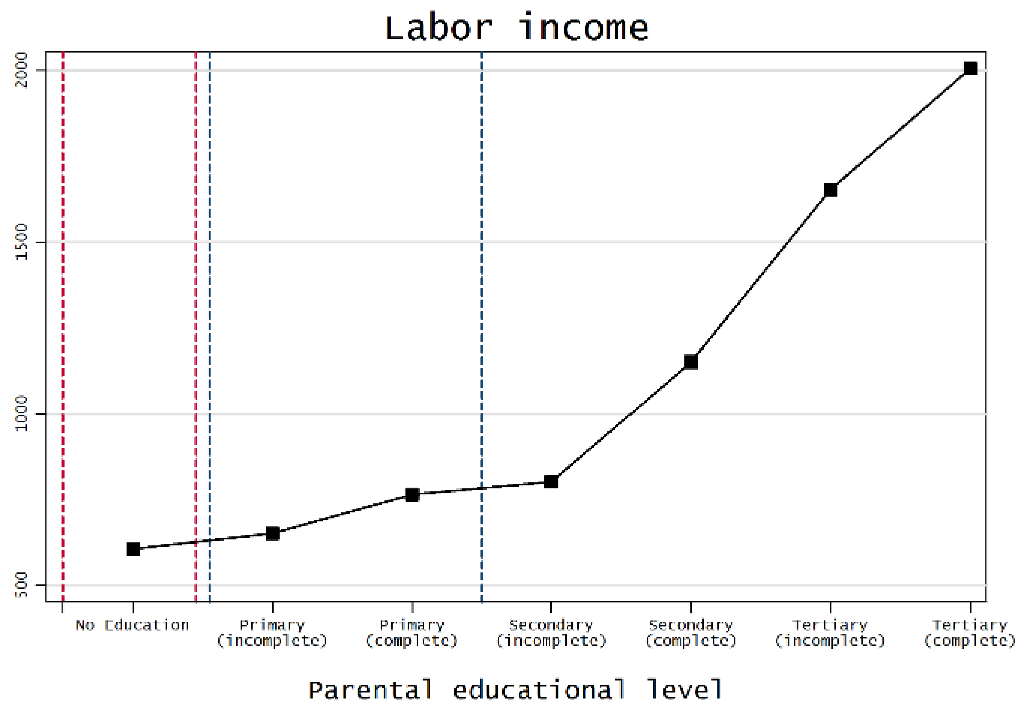


Figure 3. Average outcomes by parental background (Cohorts 1977-1988). Source: CASEN 2013, own estimations.





Time Dimension

The time dimension used in our analysis is the year of birth of individuals. Hereby, our identification strategy to identify the time when the treatment started, exploits the age restriction imposed to SUF. While extremely poor households under a certain proxy mean score are unconditionally eligible for Chile Solidario, only families with children under 18 years of age are eligible for this particular subsidy.[8] Since Chile Solidario was implemented in 2002, individuals born 1985 or later were younger than 18 when the program started, and therefore eligible for SUF, while people born before 1985 were 18 or older and therefore not eligible.[9] The only variation between individuals from extremely poor households born before and after 1985 should be the eligibility status for SUF, while the eligibility for Chile Solidario of their household of origin should be the same. Figure 4 illustrates the key aspects of our identification strategy.

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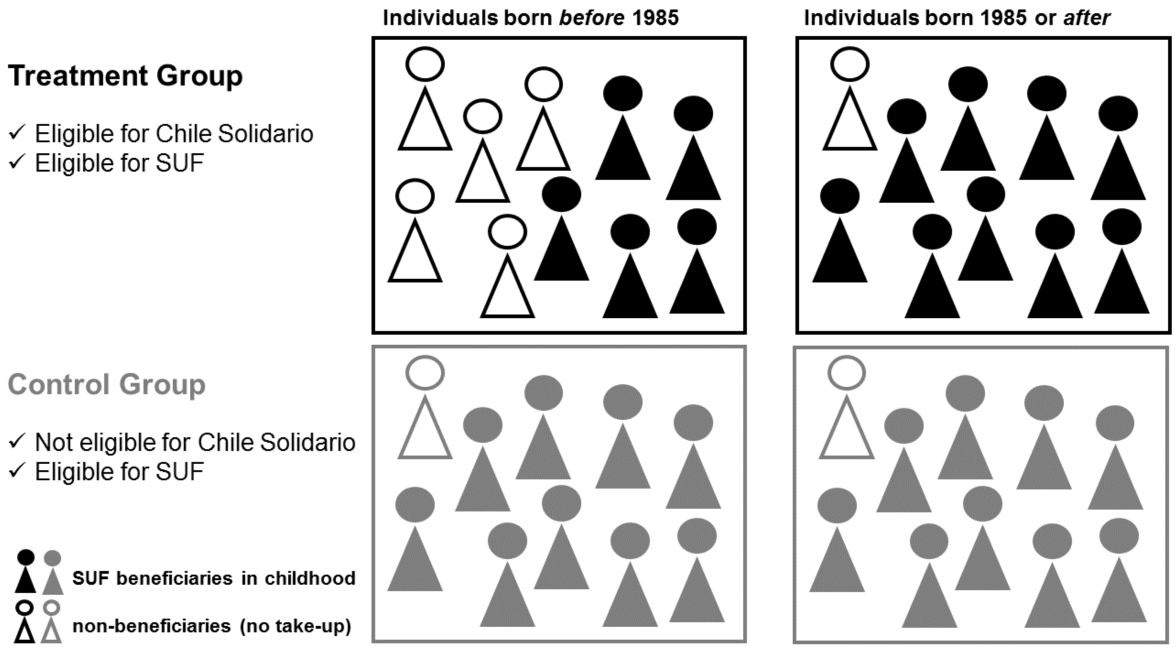
[8] Generally, the conditionality of Chile Solidario is bounded to the willingness and effort of the household to fulfil the stipulated contract with the social worker. Therefore, the protection and exit grant, as well as some other more specific transfers like the save water allowance, are guaranteed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

[9] Since the program was implemented in May 1984, we might think that people born from June to December 1984 might be eligible as well. However, only after a first working period of 6 to 8 month the social worker actively activates the demand for the social services by the family. Therefore, we expect the 1985 cohort to be the first effectively affected by the take up of SUF.

Figure 4. Illustration of the identification strategy. Variation in time (cohorts before and after 1985) is on the take up of SUF. Variation between treatment and control group is in eligibility status for Chile Solidario.



We restrict the time window of our analysis to the cohorts 1977 to 1988 to reduce age effects and bias deriving from individuals who did not finish their educational career or recently entered the labor market. Thus, treated individuals are at least 25 years old and have a maximum exposure to the program of four years within the age interval from the ages of 14 to 18.

Descriptive Statistics

Table 1 shows descriptive statistics, Figure 4 differences between treatment and control group in the geographical distribution of our sample in both cohorts. Since the intervention was not random, differences in observable (and unobservable) characteristics might be expected. Indeed, the two groups differ significantly and substantially in the share of men, indigenous people and individuals in rural areas. In contrast, the averages of the two groups are qualitatively similar in age, number of household members, self-reported health and share of migrants.[10] Furthermore, the two groups have a similar geographical distribution over the country.[11]

Table 1. Pre and Post-Treatment sample averages (weighted). Source: CASEN 2013, own calculations.

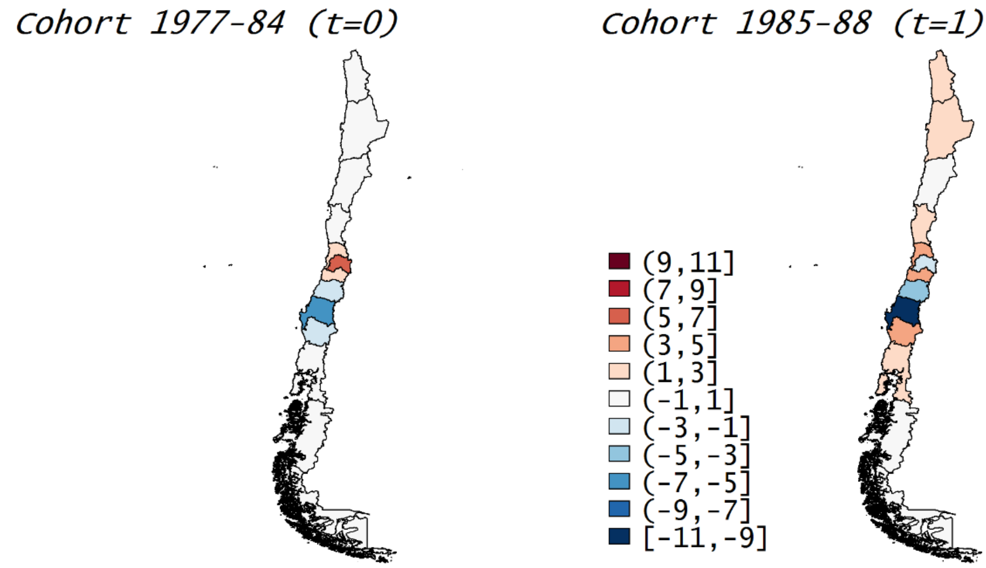
Cohorts	*1977-1984 (t=0)*			*1985-1988 (t=1)*		
Individual characteristics	*Control*	*Treated*	*Diff.*	*Control*	*Treated*	*Diff.*
Male (0/1)	0.333	0.419	-0.086	0.329	0.514	-0.185
	0.0106	*0.0338*	*0.0333*	*0.0270*	*0.0618*	*0.0675*
Age	32.612	33.273	-0.661	26.637	26.615	0.022
	0.0634	*0.1219*	*0.1301*	*0.0581*	*0.1021*	*0.1161*
Rural (0/1)	0.194	0.290	-0.096	0.184	0.155	0.029
	0.0078	*0.0267*	*0.0276*	*0.0119*	*0.0340*	*0.0347*
Number of household members	4.260	4.158	0.102	4.213	4.390	-0.177
	0.0564	*0.1295*	*0.1390*	*0.0732*	*0.2141*	*0.2331*
Indigenous (0/1)	0.124	0.175	-0.051	0.146	0.164	-0.018
	0.0061	*0.0185*	*0.0203*	*0.0123*	*0.0450*	*0.0480*
Migrant (0/1)	0.005	0.007	-0.003	0.003	0.002	0.001
	0.0016	*0.0053*	*0.0056*	*0.0009*	*0.0016*	*0.0018*
Self-reported Health	5.817	5.796	0.022	5.978	6.059	-0.080
	0.0340	*0.0741*	*0.0814*	*0.0352*	*0.1048*	*0.1089*

Notes: 734 observations in treated and 7,003 in control group. (0/1) indicates dummy variable. Self-reported Health: (1) "very bad"- (7) "very good". Migrants only included if migrated to Chile before 2002. Bootstrapped standard errors reported in italics below the estimates.

[10] We define migrants as people born outside of Chile. People migrated to Chile before 2002, the starting year of Chile Solidario, are excluded from the sample.

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

Figure 5. Geographical distribution. Difference in percentage points between control and treatment group. Source: CASEN 2013, own estimations.



3.2. Method

We adopt a simple Difference in Difference approach (Card & Krueger, 1994) based on demographic groups with different accessibility to the social transfer. This methodology is particularly useful to compare 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 the program. Hereby, our treatment group are adult individuals whose parents have extremely low levels of education (i.e. no formal education) while the time dimension is defined by the year of birth of individuals. We restrict the control group to individuals with low level of parental education (some years of schooling or completed primary education).[12]

[12] We always refer to the parent with the highest educational degree within the family or the parent with available information if one of the two has missing information. Since we rely on the retrospective questions

We opt for a linear specification of the following form:

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

where y is the outcome of individual i belonging to group $j\epsilon(T, C)$ and cohort $t\epsilon(0,1)$ being $t = 0$ 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 matrix of individual control variables, and S_{jt} 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 program. The control variables include age, age-squared, household size, and self-reported health status, as well as dummies for the geographical region, urban or rural area, and ethnic background (indigenous or not).

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 social transfer. The two key assumptions of our identification strategy are that the two groups would follow the same trends in the outcomes of interest in absence of the treatment, and the absence of other events with differential effects on both groups, contemporaneous to the social program. Since the evaluation mechanism of

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of the survey on father and mother's education, we do not need individuals to reside with their parents in the same household to retrieve this information.

the social program evaluated here is not random, this last condition is crucial for a causal interpretation of our estimates (Garganta & Gasparini, 2015).[13]

The interpretation of the estimated parameter is twofold. On the one side, because of the high participation rates to Chile Solidario, DD yields the Average Treatment Effect on the Treated (ATT) of the capacity of the social program to link poor families to the social transfer system, in particular to the cash transfers intended to increase the investment in children's human capital. On the other side, we can interpret DD as Intention to Treat Effect (ITT) of SUF. Hence, we can complement our estimates with the findings of Carneiro et al. (2015) on the increase in the take-up of SUF as causal impact of Chile Solidario, and estimate the long lasting, causal effect of this conditional cash transfers on the human capital of poor children. Finally, because participation to the social program is measured by a proxy, we have to account for measurement error in our estimates.

[13] A possible violation of the second assumption could derive from the possibility that the likelihood of leaving home rises with age. Therefore, people born 1977 to 1984, 25 to 18 years old when the program started in 2002, might have already left their household of origin in contrast to people born 1985 to 1988 that were 17 to 14 years old. In this case, the 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 % in 2014 and 62 % in 2007 (see OECD, 2016). Furthermore, the primary reason to leave 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 this issue should not affect our estimates seriously. Nevertheless, we perform robustness checks restricting the time window of analysis with qualitatively similar results.

To avoid potential bias in the standard errors in presence of serial autocorrelation in the outcomes, we apply the suggested correction by Bertrand et al. (2004) and aggregate the data into one period before and one after the implementation.[14] Furthermore, since municipalities are in charge of the assignment of the social program, error terms might be correlated within these units. Therefore, we compute standard errors clustering observations at the municipality level.[15]

4. Results

4.1. Baseline

Figure 6 shows our unconditional baseline results graphically.[16] We see a sharp rise in years of schooling and labor income for eligible cohorts in the treated group (i.e. individuals affected by the social program) that we do not observe in the control group. Hence, the higher take-up of SUF by poor families in consequence of Chile

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

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

[16] Figure 6 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 the visual inspection may leave some doubts, we verify the validity of the assumption also 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 null cannot be rejected for pre-treatment cohorts (F=0.54, Prob>F=0.8054) and is rejected after the treatment (F=2.03, Prob>F=0.0901).

Solidario seems to have a positive and sustained impact on the human capital children, measured by these two outcomes. Table 2 quantifies the impact in four different model specifications. Column (1) shows the unconditional results, column (2) the estimates including control variables for demographic characteristics and health status. The conditional impact of the transfer is around one year of education and 291 USD labor income. The latter is about 15 % of the Chilean average monthly wage in 2013.[17]

Column (3) and (4) show the results separately for men and women. It is evident that the effect on schooling is similar around one year for both sexes, while the effect on labor income is completely driven by men. This is an expected finding for two reasons. First, although female labor participation in Chile has been constantly rising in the last decades, it is still around 60 % and among the lowest in Latin America (Gasparini & Marchionni, 2015). Second, besides the conditional cash transfer for school participation, other subsidies were available for women older than 18 years old in Chile in 2002 (Cecchini & Madariaga, 2011). For men, the effect on labor income is about 386 USD, approximately 20 % of the average monthly wage.

If we take into account that the take-up of SUF was not universal in the treatment group, these results have to be interpreted as intention to treat effects. We can approximate the average treatment effect on the treated of the conditional cash transfer under evaluation (SUF) using the recent estimates of Carneiro et al. (2015) on rising take-up rates among Chile Solidario beneficiaries. Following their findings, between 50 and 65 % of eligible households for Chile Solidario were already SUF beneficiaries before the start of the social program in 2002. Among the families that did not take

[17] The Chilean monthly mean wage in 2013 was about 1918.25 USD (Yearly mean wage 23,019 USD; Source: OECD Data), while minimum wage in 2013 was about 425 USD (Source: WageIndicator.org).

up SUF before 2002, their estimates show that the take-up of this subsidy rose between 18 and 32 percentage points. This translates into a higher take-up of SUF between 36 and 67 % among the group intended to be treated (i.e. poor families that were unaware of their eligibility or about the existence of this transfer). Following these take-up rates, the effects of receiving SUF in childhood on adult outcomes range between 1.6 and 3 years of education, and an average return in labor income between 22 % and 42 % of average monthly wages. These amounts seem plausible, since some individuals in our sample might have benefit from the conditional cash transfer for up to four years in an age that is crucial for the completion of formal education.[18]

[18] These estimates are computed on the sample of individuals with available information about their labor income. In a robustness check, we impute a zero to unemployed and inactive individuals with missing information on labor income. The conditional estimates of the effect are slightly lower for the whole sample, because of the relatively high number of inactive women, and do not change significantly for men.

Figure 6. Trends by cohorts. Source: CASEN 2013, own estimations.

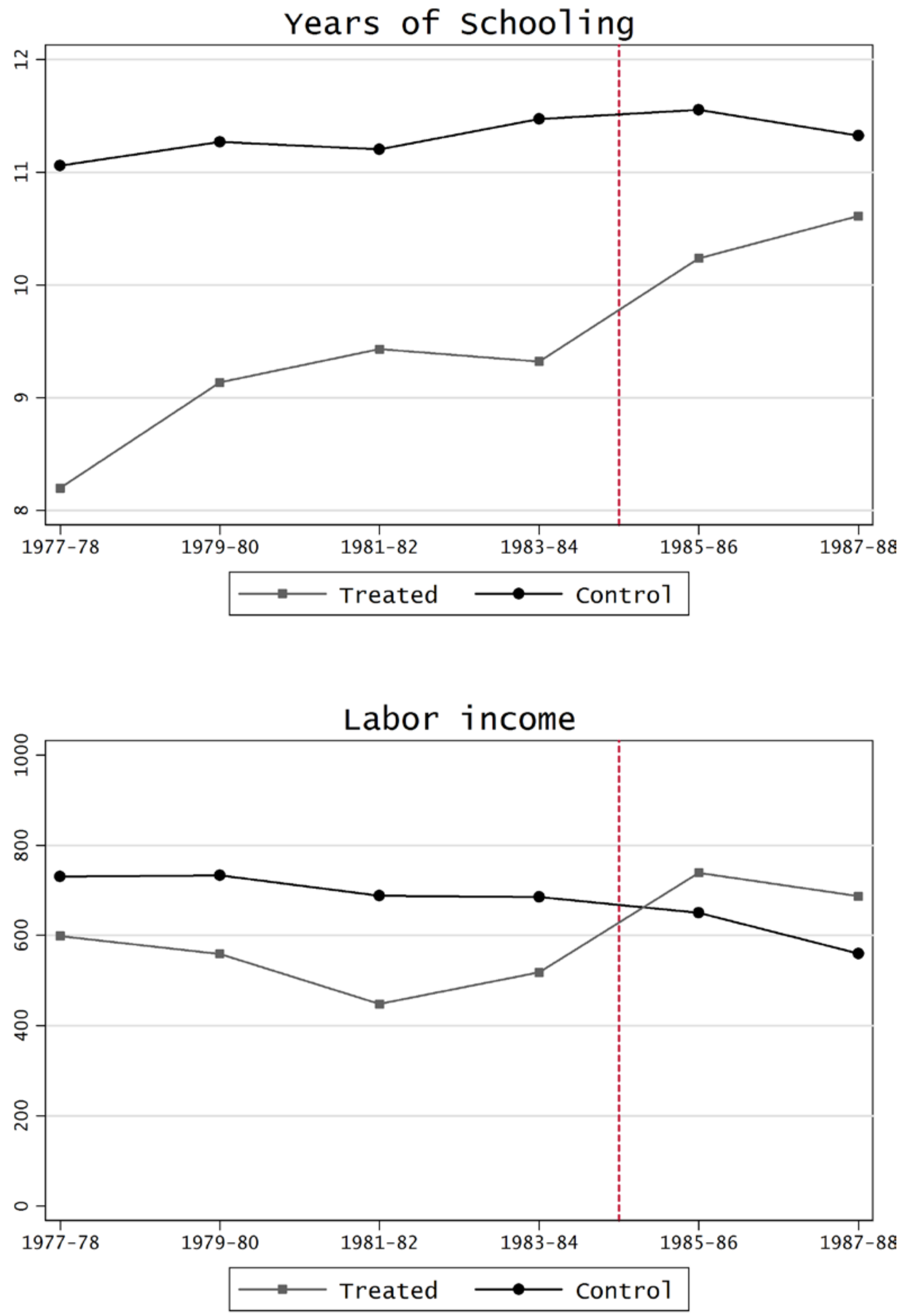


Table 2. The long lasting effects of the social transfer. Source: CASEN 2013, own estimations.

Years of education	(1) Unconditional	(2) Conditional	(3) Only Women	(4) Only Man
DD	1.271***	1.075***	0.913*	1.374**
	(0.4434)	(0.4073)	(0.4720)	(0.6391)
Treated	-2.330***	-2.129***	-2.059***	-2.232***
	(0.2504)	(0.2276)	(0.2935)	(0.3678)
Born after 1984	0.206**	-0.037	-0.043	0.062
	(0.1008)	(0.2106)	(0.2819)	(0.3279)
Observations	7722	7627	5075	2552

Labor income (in USD)	(1) Unconditional	(2) Conditional	(3) Only Women	(4) Only Man
DD	274.328***	290.987***	51.483	385.861***
	(98.3727)	(95.5945)	(75.7571)	(132.2370)
Treated	-169.464***	-157.258***	-142.350***	-223.765***
	(45.1214)	(33.0931)	(36.1156)	(54.6378)
Born after 1984	-99.557***	-59.414	-52.434	-142.624
	(31.1357)	(93.3812)	(76.6662)	(191.2097)
Observations	5298	5229	2815	2414

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. Data: CASEN 2013, own estimations. $^{*}\ p < 0.1$, $^{**}\ p < 0.05$, $^{***}\ p < 0.01$

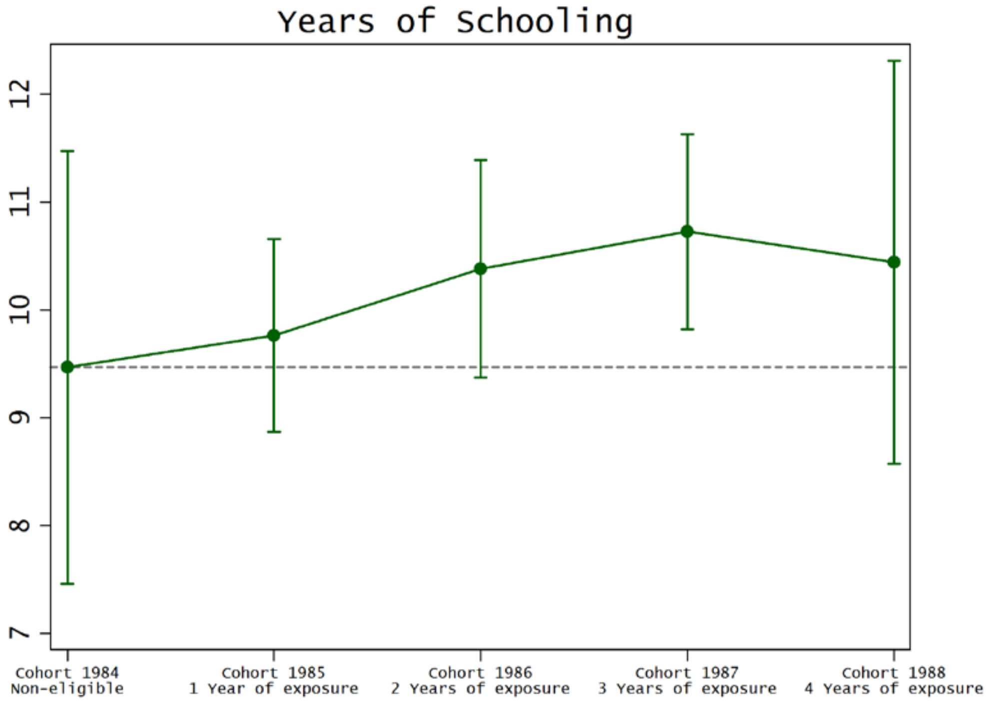
Measurement Error

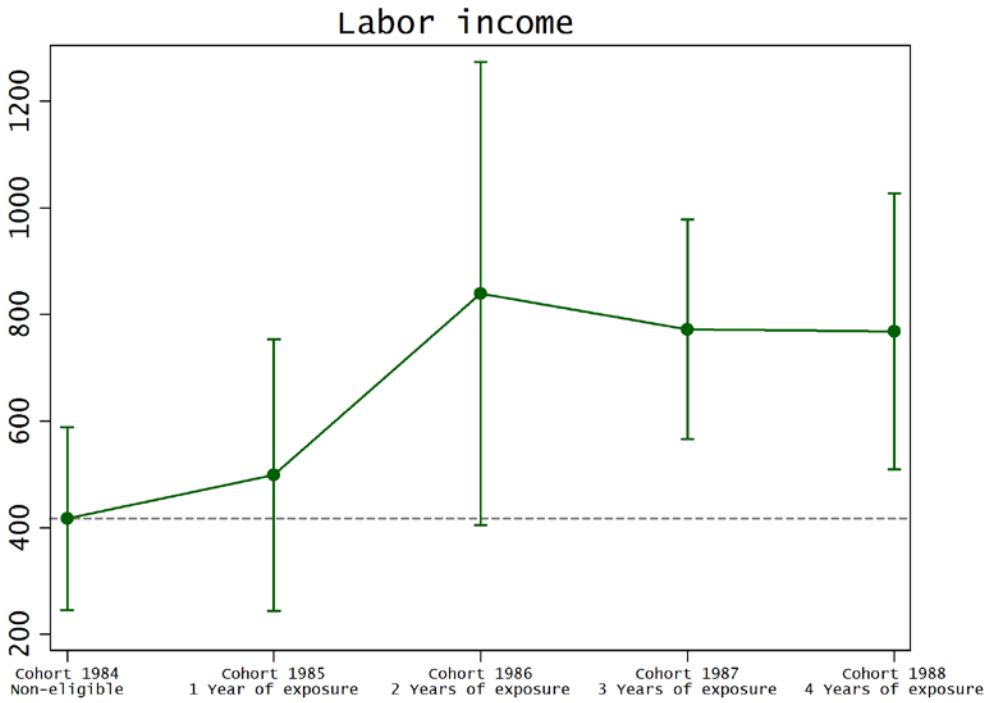
Two possible sources of measurement error in the estimates challenge the validity of our findings. First, the fact that treatment status in childhood is not directly observed, but approximated with the available retrospective information on parental educational background. In Section 3, we exhaustively discuss this issue and show through a battery of sensitivity analyses that our proxy should be a useful indicator for eligibility status in childhood. Nevertheless, the control group might encompass individuals that grew up in eligible households for Chile Solidario and the opposite might be true for the treated group. If this is the case, we might expect a downward bias in our estimates. Another source of bias arises if non-eligible individuals for SUF with younger siblings in an eligible age when Chile Solidario started in 2002 were also positively affected by the transfer. Under two assumptions, this situation would lead to lower estimates: i) sibling spillovers, ii) externalities within the households, both caused by the monetary subsidy and the increased schooling of the younger sibling. Because of these possible sources of downward bias, our estimates might be understand as a lower bound for the effect of the social program.

4.2. Intensity of Treatment

Figure 7 shows the intensity of the treatment effect. As expected, the treatment effect varies with the exposure of individuals to the social transfer. However, we observe a measurable effect, especially for labor income, only from the 1985 to the 1986 cohort, i.e. from one to two years of exposure. Among the other cohorts, the intensity of the effect is similar. The reasons might be the relatively short time window of our analysis and the fact that the social program was implemented gradually in the first years until addressing all eligible families.

Figure 7. Intensity of treatment effect. Source: CASEN 2013, own estimations.





4.3. Heterogeneous Effects

Next, we perform the analysis separately for different population groups, to test for heterogeneous effects of the social program. First, we analyze the overall results, divided by rural and urban areas, as well as indigenous and non-indigenous people. Then, we restrict the analysis to women, and test for heterogeneity among the married, single, childless and with children.

We see that the effect on schooling and labor income is significant in urban areas, while non-significant in rural areas. Indeed, the findings of one of the earliest short run evaluation of Chile Solidario by Galasso (2011) suggest that particularly households affected by the program in urban areas were more likely to have received the SUF.[19] Among indigenous people, we find the effects to be stronger in schooling, but weaker in labor income. However, the differences in the coefficients between indigenous and non-indigenous people are not statistically significant from each other.

When disentangling the analysis by different subgroups of women, we find an important and interesting amount of heterogeneity in our estimates. We do not find any significant effect of the transfer on schooling and labor income for married women and women with children.[20] In contrast, for women without children the coefficient of the interaction term is high and statistically significant. These findings are in line with economic theory on the division of labor supply within the household (Becker, 1985).[21]

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[19] Carneiro et al. (2015) argue that there might be heterogeneous effects due to the degree of remoteness of the local of residence and the associated opportunity costs, but do not find any significant differences.

[20] The group of married includes women cohabitating with their partner.

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

Table 3. The long lasting effects of the social transfer. Heterogeneous effects on the complete sample. Source: CASEN 2013, own estimations.

Years of education	(1) Rural	(2) Urban	(3) Indigenous	(4) Not indigenous
DD	0.092	1.418**	1.696***	0.995*
	(0.6529)	(0.5497)	(0.6276)	(0.5233)
Treated	-1.763***	-2.229***	-2.055***	-2.091***
	(0.2895)	(0.3274)	(0.5440)	(0.2576)
Born after 1984	-0.068	-0.053	0.064	-0.080
	(0.3835)	(0.2511)	(0.4430)	(0.2325)
Observations	2028	5599	1358	6269

Labor income (in USD)	(1) Rural	(2) Urban	(3) Indigenous	(4) Not indigenous
DD	353.989	348.778**	237.576**	344.719**
	(238.1678)	(137.5073)	(101.5546)	(151.4162)
Treated	-127.391**	-167.103***	-116.807***	-156.588***
	(56.9420)	(34.2540)	(39.5679)	(33.2205)
Born after 1984	-93.665	-4.732	-48.269	-17.103
	(59.4417)	(101.5928)	(67.3129)	(98.0642)
Observations	1280	3949	924	4305

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. Data: CASEN 2013, own estimations. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$

Table 4. The long lasting effects of the social transfer. Heterogeneous effects for sub-groups of women. Source: CASEN 2013, own estimations.

Years of education	(1) Married or in relationship	(2) Single	(3) No children	(4) With children
DD	0.873	1.537*	2.889**	0.685
	(0.6418)	(0.9078)	(1.3132)	(0.4951)
Treated	-2.095***	-2.213***	-4.108***	-1.905***
	(0.3567)	(0.6284)	(1.0742)	(0.2950)
Born after 1984	-0.397	0.036	-0.977	-0.076
	(0.3198)	(0.4081)	(0.6550)	(0.3103)
Observations	3199	1506	294	4769

Labor income (in USD)	(1) Married or in relationship	(2) Single	(3) No children	(4) With children
DD	27.140	73.352	498.960***	19.583
	(109.8463)	(112.8655)	(145.7410)	(75.8758)
Treated	-148.812***	-123.352*	-535.030***	-111.804***
	(55.6985)	(63.8100)	(130.2771)	(35.0163)
Born after 1984	-93.470	-78.566	-555.340**	-27.812
	(88.0495)	(79.6613)	(261.1240)	(52.6239)
Observations	1440	1098	223	2584

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. Data: CASEN 2013, own estimations. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$

4.4. Robustness

To proof the consistency of our estimates we perform a series of robustness checks.[22] First, we restrict our sample to the cohorts 1982 to 1988 to avoid age effects affecting co-residence in the household of origin, as explained above. Second, we set missing values in labor income to zero when information on employment status is available and indicates that the individual is unemployed or inactive. Third, we split the control group between individuals whose parents have incomplete and complete primary education and run a model including fixed effects for these multiple groups. Fourth, we run all estimations without weighting by the inverse probability of selection provided by the survey data design variables.[23]

Fifth, although we tested exhaustively for the existence of common trends prior to the treatment, the assumption of post treatment common trends in absence of the intervention cannot be certainly verified, because it relies on a counterfactual scenario. As further examination, we run a Placebo-Test restricting the sample to pre-treatment cohorts and set individuals 1982 as the first cohort affected by the social program. In this analysis, the coefficient of the interaction term between the dummies for time and treated is not significantly different from zero. The same applies to two further Placebo-Test that we perform: i) Changing the treated group to individuals whose parents have incomplete secondary education. ii) Evaluating the effect of the social program on non-labor income; an outcome that should not have been affected by the intervention.

[22] The results of the robustness checks are available from the authors upon request.

[23] As pointed out by Solon et al. (2015), under certain conditions weighting might not be necessary and even harmful to obtain causal parameters.

Last, 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 be migrated from rural areas, where they spend their childhood, to urban areas in consequence of the intervention that provided them the monetary resources to move. If this would be the case, the effects of the reform would still maintain their internal validity, since the migration decision might also be understand as an investment in human capital, but loose external validity. To account for this issue, we perform the analysis separately for individuals living in the same municipality in which they were born and individuals living in a different location. A test of the two coefficients yields no significant differences (F=0.14, Prob>F=0.7101 for schooling, and F=0.02, Prob>F=0.8949 for labor income).

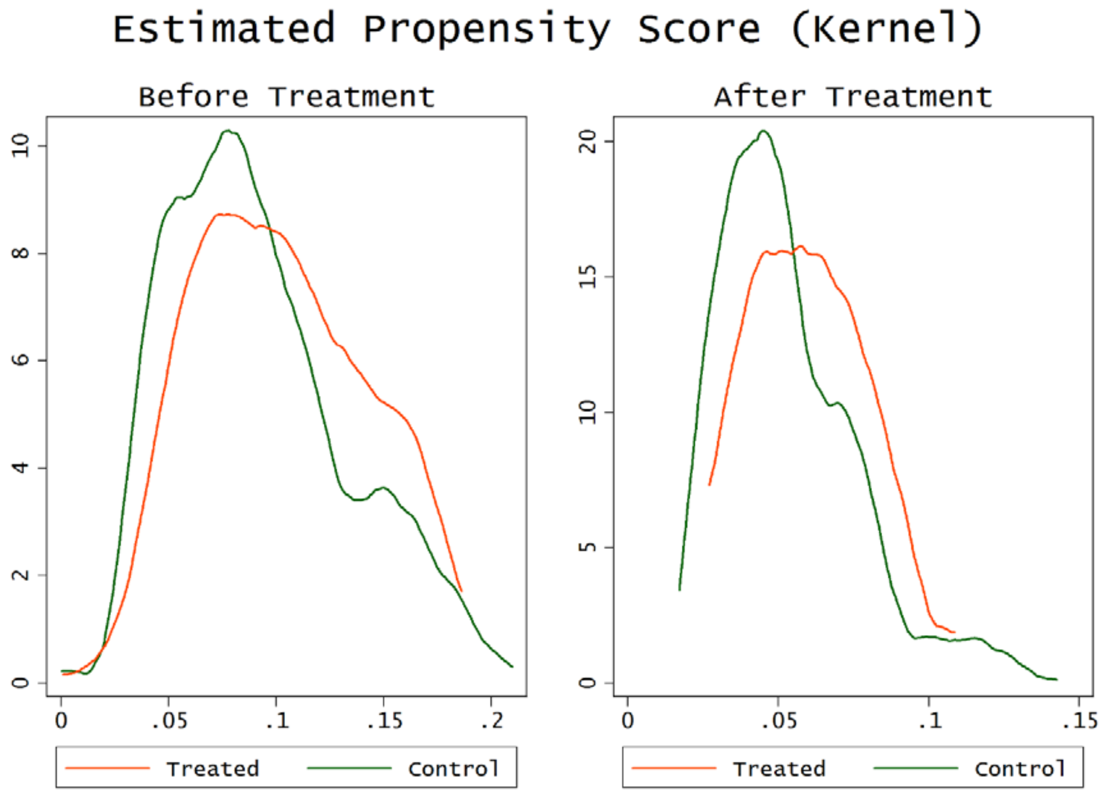
4.5. Difference in Difference Matching

As discussed in Section 3, because of the non-randomness of the intervention, treatment and control group differ in observable characteristics. To rule out the possibility that these differences affect also the outcome variables systematically, we further adopt a method that combines Propensity Score Matching with Difference in Difference estimations (Heckman, Ichimura, & Todd, 1997). Particularly, we apply a Kernel-Matching estimator that makes use of nearly all observations in the control group, weighting them by the distance of the propensity score.[24] This methodology rests 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.[25]

[24] The type of the Kernel-function adopted here is Epanechnikov.

[25] For an exhaustive discussion about the application of Difference in Difference Matching to repeated cross-sectional data, as performed in our study across cohorts, see Blundell & Dias (2009).

Figure 8. Common support. Source: CASEN 2013, own estimations.



We use only variables that do not change in time to estimate the propensity score: year of birth, sex, place of birth, indigenous group and migration background. Figure 8 shows the distribution of the Propensity Score for the four groups: Individuals in the control and treated group before the intervention (i.e. cohorts 1977-1984) and individuals in both groups after the intervention (i.e. cohorts 1985-1988). The graphical representation confirms that both groups share a rather large area of common support before and after the treatment.

Table 5 shows the results from this application. The estimates of the treatment effect do not deviate substantially from the baseline. The effect on the schooling of men is almost one year higher, while the effect on labor income slightly lower. The effect on schooling and labor income of women is not statistically significant. In conclusion, the adoption of Difference in Difference Matching confirms the results obtained so far on the long lasting effects of SUF and Chile Solidario.

Table 5. The long lasting effects of the social transfer. Method: Difference in Difference Matching. Source: CASEN 2013, own estimations.

	Women	Men
Years of Schooling	0.772	2.159***
	(0.535)	(0.574)
Labor income (in USD)	52.898	345.308**
	(82.285)	(158.429)

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. Data: CASEN 2013, own estimations. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

5. Conclusions

In this study, we evaluated the long run effects of a conditional cash transfer on children's human capital. We exploited hereby the introduction of a social program that increased drastically the take-up of this transfer among poor families as sort of natural experiment. Our identification strategy relies on exogenous variation given by the fact that some cohorts of individuals were eligible in terms of age when the program started, while others not. Our findings point at rather strong and persistent effects of the transfer on the human capital of children from poor households, quantified around two years of schooling and 25 % of average monthly labor income.

Our results add suggestive evidence to the scant literature on the effects of social transfers in the long run and confirm that these forms of social policies might be an effective tool to increase children's human capital persistently. The resulting effects on equality of opportunity certainly depend on the institutional background and the structural characteristics of the labor market. For Chile, we find that together with an increase in schooling among beneficiaries, a substantial effect on labor income can be observed as well.

Although clearly beyond the scope of this work, the question remains whether the effect on labor income is completely driven by the increase in schooling or whether other factors influenced the human capital and earnings ability of beneficiary children. Past research found, for instance, that parental income and unconditional monetary subsidies might have a positive effect on child development in the context of developed and developing countries (e.g. Dahl & Lochner, 2012, for the US and Paxson & Schady, 2010, for Ecuador; see Almond & Currie, 2011, for an overview). Furthermore, the support provided to the families by the social worker might have an effect in itself, as well as the take-up of the transfer, for example reducing informational barriers and transaction costs for poor families to benefit from the social protection system.[26]

The latter issue should pertain less to our results, since all individuals in the treatment group were eligible for the social program that provided support by social workers and increased the take-up of subsidies, Chile Solidario, but only the younger group was also eligible for the conditional cash transfer, SUF. The former is a stimulating topic leaving space for further research.

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[26] 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 at detrimental effects in the short and long run, that could however, be biased by selection of certain parents into welfare support (Mayer, 2002).

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APPENDIX

Table A 1. Chilean Educational System and Median Income (Cohorts 1977-1988). Source: CASEN 2013, own elaboration.

Years	Type			Median Total Income (Weighted)	Median Labor Income (Weighted)
	Tertiary				
22	PhD			2426.4	2426.4
21				2022	2047.277
20	Master			3038.055	3922.68
19				1722.071	1819.58
18	Bachelor			1834.601	1834.601
17				1273.86	1307.561
16		Professional		958.7657	913.944
15				788.58	781.3493
14			Technical	721.1807	709.075
13				633.5593	626.82
	Secondary				
12	Academic track	Vocational track		525.72	512.2393
11				444.84	427.9907
10	General secondary education			444.84	424.62
9				444.84	424.62
	Primary				
8	General primary education			427.9907	424.62
7				404.4	303.3
6				404.4	250.6451
5				424.62	343.74
4				424.62	374.07
3				424.62	390.246
2				494.9229	474.1772
1				404.4	214.4493
0	No formal educational degree			404.4	165.9213

Table A 2. Population by level of education in 2003 (weighted frequency). Only individuals in age interval 30-60. Source: CASEN 2003, own elaboration.

Educational Level	Weighted Frequency	Percent	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	

Table A 3. Population by level of parental education in 2013 (weighted frequency). Only individuals in sample (born 1977-1988). Source: CASEN 2013, own elaboration.

Parental Educational Level	Weighted Frequency	Percent	Cumulative
No Education	47,314	3.26	3.26
Primary (incomplete)	266,678	18.36	21.62
Primary (complete)	237,262	16.33	37.95
Secondary (incomplete)	202,579	13.95	51.90
Secondary (complete)	457,717	31.51	83.41
Tertiary (incomplete)	66,626	4.59	88.00
Tertiary (complete)	174,369	12.00	100.00
Total	1,452,545	100.00	

Table A 4. Unweighted estimates. Source: CASEN 2013, own estimations.

Years of education	(1) Unconditional	(2) Conditional	(3) Only Women	(4) Only Man
DD	1.377***	1.172***	0.899**	1.604***
	(0.3264)	(0.2986)	(0.4130)	(0.4083)
Treated	-2.336***	-2.087***	-2.032***	-2.155***
	(0.1747)	(0.1662)	(0.1949)	(0.2347)
Born after 1984	0.325***	-0.065	-0.182	0.194
	(0.0659)	(0.1521)	(0.1850)	(0.2509)
Observations	7722	7627	5075	2552

Labor income (in USD)	(1) Unconditional	(2) Conditional	(3) Only Women	(4) Only Man
DD	266.588**	280.413***	25.592	381.080***
	(115.1908)	(102.5672)	(75.8539)	(143.2504)
Treated	-91.895*	-104.689***	-111.900***	-152.238***
	(51.6731)	(23.2939)	(25.9868)	(37.2355)
Born after 1984	-92.170***	-31.603	-18.896	-75.504
	(16.4925)	(35.8477)	(43.5700)	(51.6547)
Observations	5298	5229	2815	2414

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. Data: CASEN 2013, own estimations. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$