The Long(er)-Term Impacts of *Chile Solidario* on Human Capital and Labor Income

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Conditional cash transfer (CCT) programs have become one of the most prominent antipoverty policy innovations over the last two decades. In the Latin American region alone, 18 countries have adopted CCTs to cover approximately 130 million people living in poverty (Stampini and Tornarolli 2012). CCTs vary in terms of their scale, scope, and design features, but overall they provide income support to poor households, on the condition that school-age children go 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 examining the short-term effects of CCTs has shown that, under certain conditions, these programs can successfully improve the outcomes of beneficiary children in many dimensions of well-being, including schooling (Akresh, De Walque, and Kazianga 2013; Behrman, Parker, and Todd 2009; Dammert 2009; Filmer and Schady 2008; Lincove and Parker 2016; Maluccio and Flores 2005; Skoufias et al. 2001), health and nutrition (Attanasio, Oppedisano, and Vera-Hernández 2015; Behrman and Parker 2013; Buser et al. 2017; Fernald, Gertler, and Neufeld 2008; Gertler 2004; Manley, Gitter, and Slavchevska 2013; Perova and Vakis 2012), and, to a lesser degree, cognitive abilities and learning (Baird, De Hoop, and Özler 2013; Fernald and Gunnar 2009; Macours, Schady, and Vakis 2012; Paxson and Schady 2007, 2010) and child work (Behrman et al. 2012; Dammert 2009; Edmonds and Schady 2012; Ferro, Kassouf, and Levison 2010; Canelas and Niño-Zarazúa 2019; 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 such programs on, for instance, income and labor market outcomes of young adults who received treatment as children remains scarce and is mostly restricted to the early stages of labor market entry (Molina-Millan et al. 2016; Saavedra 2016). Some have expressed concern that the observed short-term effects of CCTs on human capital may not be strong enough to address the structural

factors that keep people in poverty across generations (Levy and Schady 2013).

This article contributes to the literature on CCTs by examining the long(er)-term effects of *Chile Solidario*, an innovative cash transfer program that was started in 2002 in Chile with the specific objective of tackling extreme poverty. One of the main instruments to achieve that goal has been the provision of information to increase awareness of the program's conditions, benefits, and eligibility criteria. Past studies assessing the short- and medium-term effects of *Chile Solidario* showed that the program was successful in increasing uptake among eligible households (Carneiro, Galasso, and Ginja 2019; Galasso 2011).

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

As our identification strategy, we exploited the fact that by design and program rules, only children aged under 18 are eligible to receive SUF. Hence, we compared individuals born in 1985 or afterward and who spent their childhood in households that were eligible to receive *Chile Solidario* at the beginning of the program in 2002 with individuals born before 1985, who were not eligible to receive SUF benefits due to their age.

We adopted a difference-in-differences approach—which we then extended to include propensity score matching estimators and a regression discontinuity design—to nationally representative household survey data that allowed us to collect retrospective information on the characteristics of the households in which individuals grew up. In this way, we were able to measure the long-term effects of the increased uptake of SUF as a result of *Chile Solidario* on educational achievement and labor income at the ages of 25–28. To the best of our knowledge, this is the first study to examine 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.

Context and intervention

A period of sustained income growth and proactive social policies in the 1990s helped Chile to reduce the percentage of the population living below the national poverty line by almost half, from 38.6 percent of the total population in 1990 to 20.2 percent in 2000. Despite this progress, extreme poverty remained stubbornly unaffected, at 6 percent over the same period; this 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 program explicitly designed to tackle extreme deprivation.

Chile Solidario provides a combination of policy interventions to support poor households to exit poverty. During the first 24 months of program treatment, the so-called *Programa Puente* ("bridge program") provides psychological support to family members via one-hour home visits by social workers, to help them overcome barriers that prevent them from fully utilizing the social services and subsidies available to them. This is important, as recent scholarly work shows that poverty leads to risk aversion and detrimental behaviors that contribute to the perpetuation of poverty (Haushofer and Fehr 2014). During this initial phase, households also receive *Bono Solidario*, a monthly cash transfer of US$8–$21, which decreases over time and is called *Bono de Egreso* in the 36 subsequent months.

In addition, *Chile Solidario* provides preferential access to (1) SUF, which aims to increase poor households' investment in children's human capital (Fiszbein and Schady 2009, Hoces de la Guardia, Hojman, and Larrañaga 2011); (2) *Pension Asistencial* (PASIS), an old-age and disability pension; (3) *Subsidio de Agua Potable* (PAS), a water subsidy for up to 15 cubic meters a month; (4) housing facilities; (5) health care and education services; and (6) employment and training programs.

In 2002–2006, the government gradually incorporated poor families into *Chile Solidario*, adding an average of about 50,000 households each year, to reach a level of approximately 264,000 households in 2011. The program targets the extremely poor through proxy means tests based on a basic needs approach. Participation among eligible households is notably high, with around 95 percent of households receiving treatment, and with very low dropout rates, at around 3 percent of all treated households (Galasso 2011).

Chile Solidario differs from other CCT programs in Latin America in several respects. First, its integrated approach combines income support with nonmonetary interventions, including 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 program, but instead acts as an incentive to encourage households to undertake investment decisions that are beneficial to their children's well-being.

Earlier evaluations of *Chile Solidario* showed that the program was successful in linking the poor with the social protection system (Galasso 2011; Hoces de la Guardia, Hojman, and Larrañaga 2011; Martorano and Sanfilippo 2012). More recently, Carneiro, Galasso, and Ginja (2019) found that *Chile Solidario* increased the uptake of SUF by 22–32 percent (contingent upon the period of enrollment), as well as uptake of employment programs that target the extremely poor—especially spouses of household heads—by 20 percent.

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 percent of the income distribution. The monthly payment, which is about US$6 in 2003 prices and represents less than 10 percent of the total household income of poorest families, is delivered to the mother on the condition that (1) her children aged 6–18 attend school regularly, and/or (2) her children under age 6 attend regular medical check-ups.[2]

Past studies have focused on the short-term effects of *Chile Solidario*. For example, Martorano and Sanfilippo (2012) showed that the program reduced poverty and increased school enrollment and utilization of public health services among children of participating households. Galasso (2011), Hoces de la Guardia, Hojman, and Larrañaga (2011), and Carneiro, Galasso, and Ginja (2019) also found a significant increase in the uptake of social subsidies, with particularly strong effects among families who had no previous access to the social protection system. Furthermore, previous studies have shown no evidence of negative effects on employment choices, leisure, or welfare dependency.

To the best of our knowledge, this is the first study examining the long-term effects of *Chile Solidario*. Taking a longer-term perspective is particularly important because it allows us to examine the more structural and transformative impacts of the program on the poorest members of the Chilean society. Indeed, only a limited number of studies have investigated the long-term impacts of CCTs; they primarily come from Latin America—e.g., Baez and Camacho (2011) and Barrera-Osorio, Linden, and Saavedra (2015) on Colombia's Familias en Acción; Barham, Macours, and Maluccio (2013) on Nicaragua's Red de Protección Social; and Behrman, Parker, and Todd (2011) on Mexico's *Progresa-Oportunidades* program—and have often provided contrasting results.[3]

Data and identification strategy

The data used in this study came from the Caracterización Socioeconómica Nacional (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 were obtained from the 2013 CASEN survey, while additional data were obtained from the 2003 CASEN survey, the round that immediately followed implementation of *Chile Solidario* in 2002.[4] The CASEN surveys were particularly suitable for the purposes of our study: their richness in retrospective information enabled us to reconstruct the socioeconomic circumstances that individuals experienced in childhood while controlling for individual characteristics.

Furthermore, since *Chile Solidario* is a nationwide program, a nationally representative household survey is the most appropriate tool with which to examine the long-term effects on former beneficiary children. For our analysis, we restricted our sample to adult individuals born in 1973 to 1988, and we measured the impacts of the program with available information on education, income, and parental educational background.[5]

Treatment and control groups

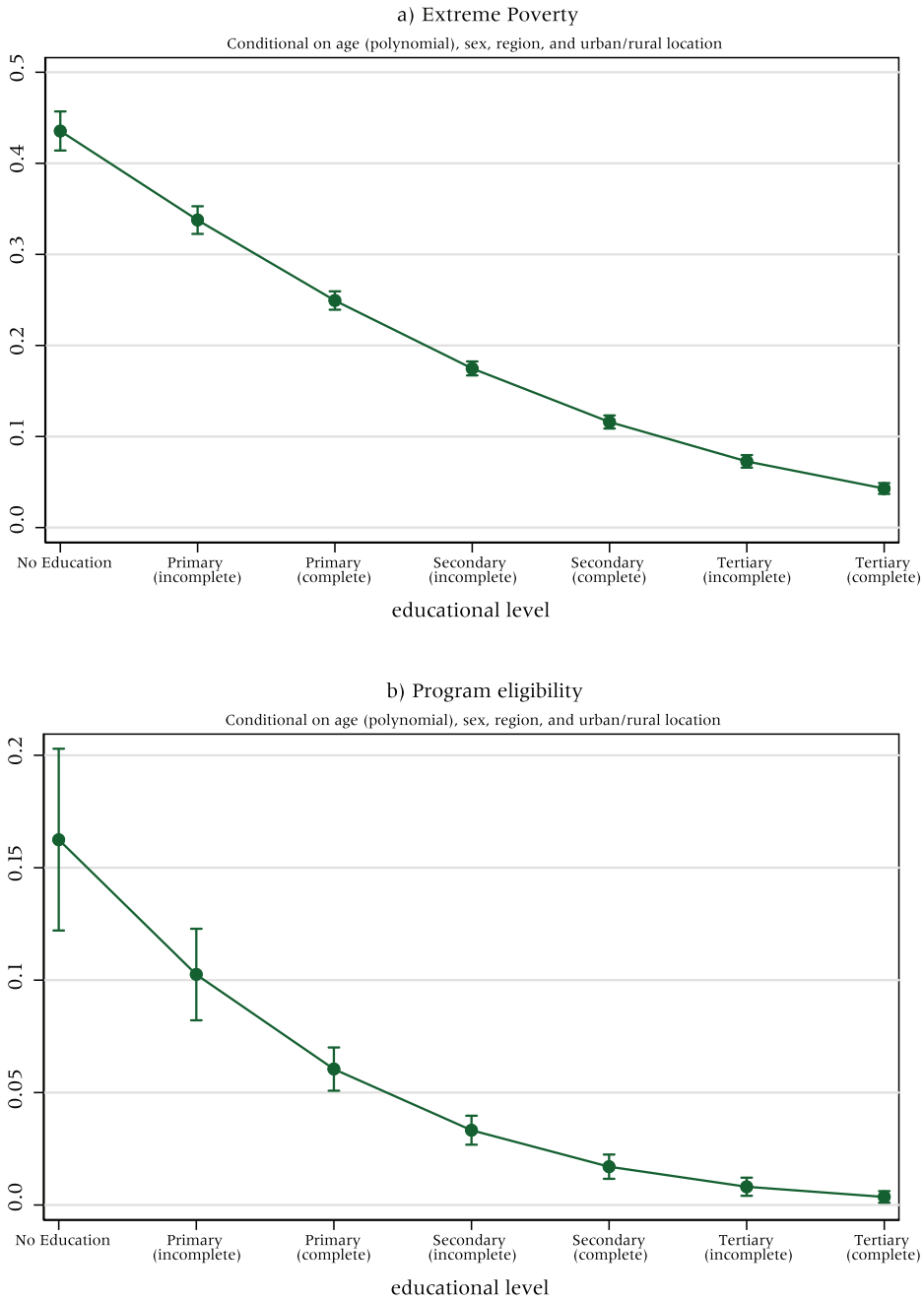
In an ideal setting, our treatment group would have consisted of individuals who spent their childhood in extreme poverty and therefore were eligible to receive *Chile Solidario* (i.e., households whose members had a high pre-treatment likelihood of being unaware of their entitlement to receive SUF and other social protection benefits). Similarly, the control group would be individuals who spent their childhood in households eligible to receive SUF but were not eligible to receive *Chile Solidario* due to program exclusionary rules, such as not being in extreme poverty. Consequently, the uptake of SUF should have risen drastically only in the treated group after the introduction of *Chile Solidario* in 2002, as shown by Carneiro, Galasso, and Ginja (2019).

Unfortunately, we lacked income data from the households in which individuals grew up that would have enabled us to identify eligibility to participate in *Chile Solidario*. Instead, we used a proxy measure for household income and program eligibility. In the CASEN survey, we were able to identify the circumstances that individuals faced in childhood through retrospective information about their parents' educational levels. An important advantage of this approach is that we could identify adult individuals even if they had left their household of origin. The procedure enabled us to measure the program's long-term outcomes, while reducing the bias arising from coresidency and sample attrition (Emran, Greene, and Shilpi 2018).

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 a primary education. Our examination of the 2003 CASEN survey showed that the conditional probabilities of being extremely poor and eligible for *Chile Solidario* were higher for individuals with no formal education (see Figure 1).

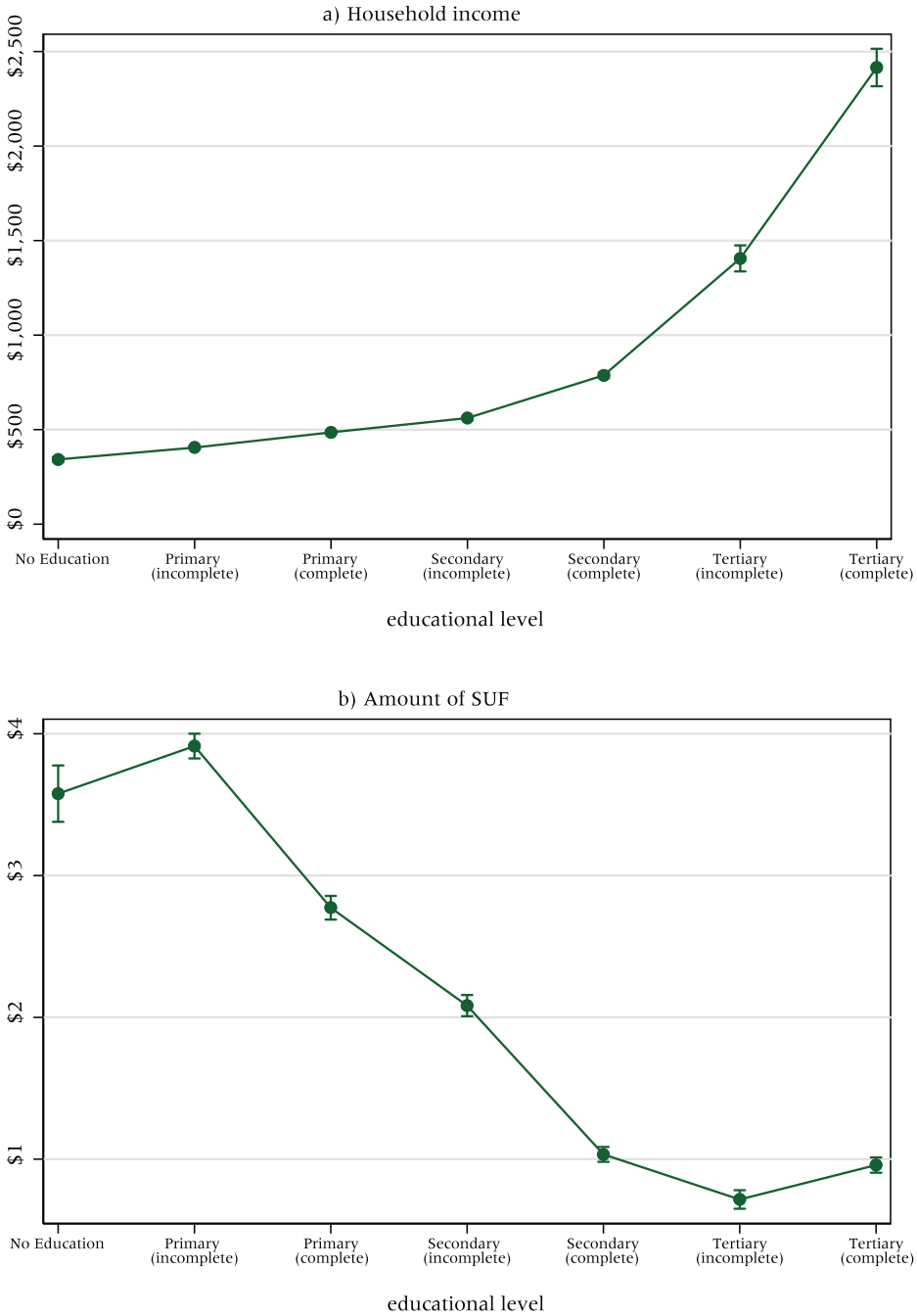
Furthermore, Figure 2 shows the predicted household income and amount of SUF received for each level of education. Household income was low for household heads with no or only a primary education and became substantially higher with increasing levels of education. The amount of SUF received was relatively close in the first educational categories but was lower among household heads with no formal education than among those with

FIGURE 1 Predicted probability of a) eligibility for *Chile Solidario* and b) extreme poverty, by educational level



NOTES: Probabilities are conditional on age, sex, region, and rural or urban area; they include only individuals in the age interval 30–60. Bootstrap confidence intervals.
SOURCE: Authors' estimations based on CASEN 2003 (Ministerio de Desarrollo Social 2003).

FIGURE 2 Predicted total household income and amount of SUF (both in US dollars), by educational level



NOTES: Probabilities are conditional on age, sex, region, and rural or urban area; they include only individuals in the age interval 30–60. Bootstrap confidence intervals.
SOURCE: Authors' estimations, based on CASEN 2003 (Ministerio de Desarrollo Social 2003).

an 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 chose treatment and control groups conservatively. The treatment group was made up of individuals whose parents had no education, while the control group comprised individuals whose parents had some years of schooling or who had completed a primary education. We excluded from the analysis individuals whose parents had educational levels above that threshold. Given the limitations of our identification strategy, however, our results likely picked up a differential in the likelihood of program treatment and therefore should be treated as lower-bound estimates of the "true" program effects.

Time dimension

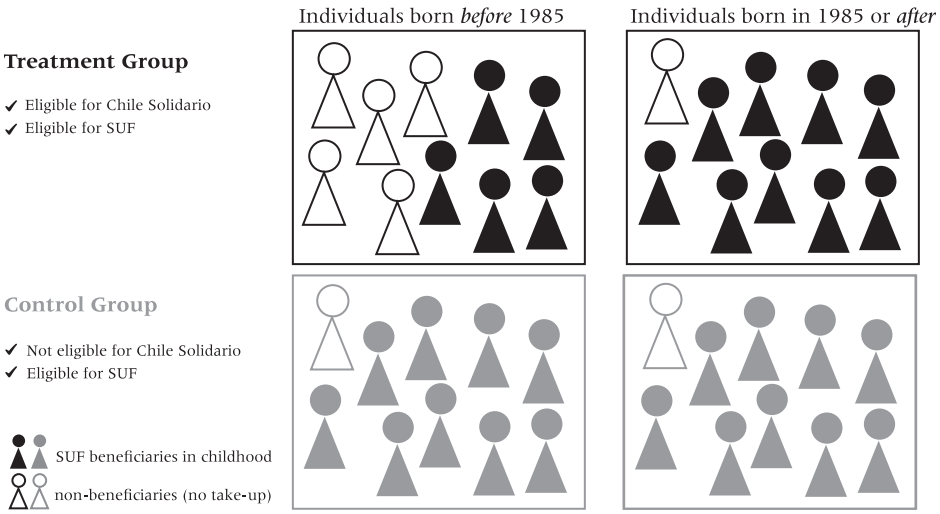
Our identification strategy also exploited an age restriction imposed exogenously by the program. While the poorest households eligible to receive *Chile Solidario* were identified using a proxy means test, only families with children aged 18 and younger were eligible to receive SUF.[6] Since *Chile Solidario* was implemented in 2002, individuals born in 1985 or later were younger than 18 when the program started and were therefore eligible for SUF, while people born before 1985 were aged 18 or older and therefore not eligible to receive SUF.[7] 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 program eligibility to receive *Chile Solidario* should be the same for both groups. Figure 3 illustrates the key aspects of our identification strategy.

We restricted 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 coresidency bias and to reduce age effects and bias deriving from individuals who did not finish their educational career or who had recently entered the labor market.[8] 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.

Model and methods

We adopted a difference-in-differences (DD) approach based on demographic groups with different access to the CCT program (Card and Krueger 1994). This methodology is particularly useful for comparing heterogeneous individuals (Meyer 1995). Intuitively, our estimates gauged 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 was adult individuals whose parents had no

FIGURE 3 Representation of the study's identification strategy



NOTES: Variation in time (cohorts before and after 1985) is on uptake of SUF. Variation between treatment and control group is in eligibility status for *Chile Solidario*.

formal education, while the time dimension was defined by individuals' year of birth. We restricted the control group to individuals with parents who had very low levels of education (some years of schooling or a completed primary education).[9]

We opted for a linear model of the following form:

$$y_{ijt} = \eta_j + \lambda_t + \gamma X_{ijt} + \delta S_{jt} + \varepsilon_{ijt} \tag{1}$$

where y was 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 captured group and cohort fixed effects, X_{ijt} was a vector of control variables expected to influence the outcomes of interest, and S_{jt} was a binary variable indicating the treatment status for group j in cohort t.

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

Table 1 shows the descriptive statistics of the covariates.[10] 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 were qualitatively similar and mostly did not differ

TABLE 1 Pre- and posttreatment sample averages (weighted)

	1973–1984 (t = 0)		1985–1988 (t = 1)	
Cohorts 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
		(0.027)**		(0.067)**
Age	36.094	−0.963	26.615	0.022
		(0.187)***		(0.138)
Rural	0.285	−0.096	0.155	0.029
		(0.022)***		(0.038)
Household members	4.385	−0.156	4.390	−0.177
		(0.159)		(0.267)
Indigenous	0.194	−0.064	0.164	−0.018
		(0.018)***		(0.048)
Migrant	0.005	0.000	0.002	0.001
		(0.003)		(0.002)
Self-reported health	5.804	−0.043	6.059	−0.080
		(0.062)		(0.122)

*$p < 0.10$, **$p < 0.05$, ***$p < 0.01$.
NOTES: There were 1,399 observations in the treated group and 10,442 in control group. (0/1) indicates that this was a dummy variable. Self-reported health ranged from (1) very bad to (7) very good. Migrants (individuals born outside Chile) were only included if they had migrated to Chile before 2002, the starting year of *Chile Solidario*. Bootstrapped standard errors of the difference in averages between treated and control group are shown in parentheses.
SOURCE: Authors' calculations, based on CASEN 2013 (Ministerio de Desarrollo Social 2013).

significantly from zero. The difference in the group differences in means between the two cohorts is:

$$DD = (y_{T1} - y_{T0}) - (y_{C1} - y_{C0}) \tag{2}$$

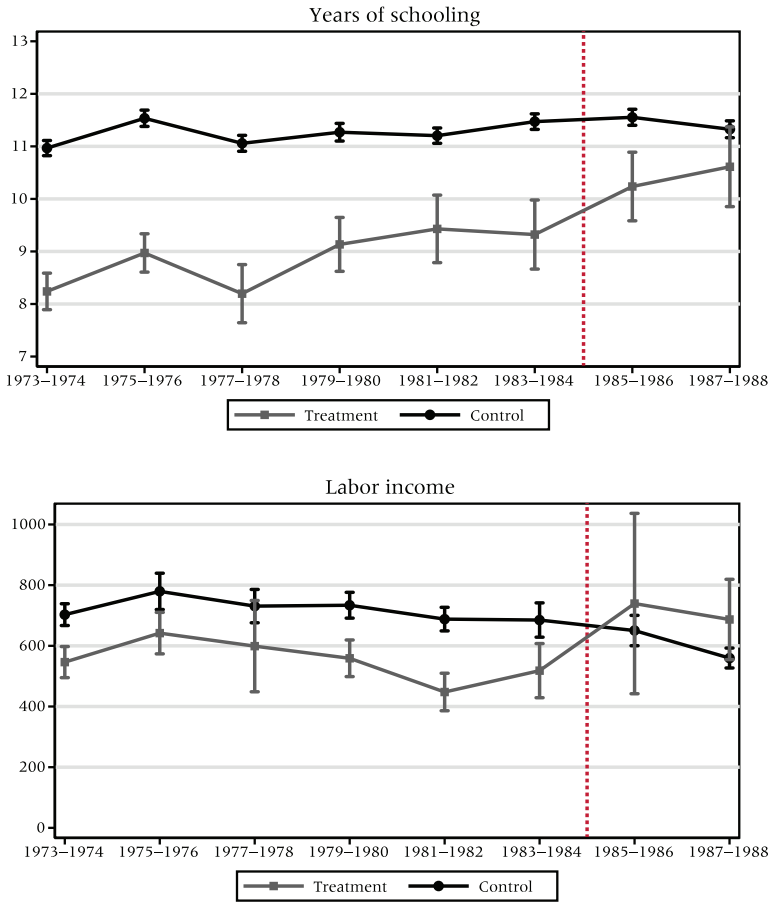
and it is straightforward to show that:

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

If the difference between the last two terms is zero, δ consistently estimates the effect of the program. 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 was not random, this condition was crucial to the interpretation of the causal effects of the program (Garganta and Gasparini 2015).[11]

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, the DD parameters can also be interpreted as the intention-to-treat effect of SUF. To avoid potential bias in the presence of serial autocorrelation in the outcomes, we applied the correction to the standard errors suggested by Bertrand, Duflo, and

FIGURE 4 Parallel trends by cohort



SOURCE: Authors' calculations, based on CASEN 2013 (Ministerio de Desarrollo Social 2013).

Mullainathan (2004) and aggregated the data into one period before and one period after the implementation.[12] Furthermore, since municipalities are responsible for the proxy means tests that identify households that are in extreme poverty and thus are eligible to participate in *Chile Solidario*, we suspected that the error terms may be correlated within these geographic units. Therefore, we computed standard errors clustering observations at the municipality level.[13]

Results

Baseline

Figure 4 illustrates our unconditional baseline results.[14] In the treatment group, average years of schooling and labor income increased sharply for

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***
	(0.397)	(0.355)	(0.406)	(0.556)
Treated	−2.521***	−2.287***	−2.250***	−2.366***
	(0.220)	(0.202)	(0.196)	(0.336)
Born after 1984	0.211**	0.017	−0.217	0.542
	(0.097)	(0.246)	(0.249)	(0.394)
Observations	11,821	11,690	7,661	4,029

(b) Labor income	(1) Unconditional	(2) Conditional	(3) Only women	(4) Only men
DD	261.566***	268.752***	51.903	335.881**
	(95.269)	(98.600)	(69.325)	(134.404)
Treated	−156.702***	−133.698***	−152.067***	−159.264***
	(33.321)	(29.235)	(28.753)	(41.235)
Born after 1984	−110.948***	−20.301	−47.974	−56.744
	(30.544)	(88.485)	(74.136)	(163.768)
Observations	8,244	8,149	4,330	3,819

*$p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.
NOTES: DD is the coefficient of the interaction term. Control variables included age, age squared, number of household members, rural or urban location, dummies for region of residency, ethnic background (indigenous or not), a dummy for migrants, and self-reported health. Cluster robust standard errors at municipality level in parentheses.
SOURCE: Authors' estimations, based on CASEN 2013 (Ministerio de Desarrollo Social 2013).

eligible cohorts, which was not observed 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 number of years of schooling increased to more than 10 for eligible cohorts in the treatment group, nearly catching up with the control group. For labor income, before treatment the average labor 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 uptake of SUF by poor families as a consequence of receiving *Chile Solidario* seems to have had a positive, sustained effect on the human capital and income of young adults who received support from the program as children.

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 labor income. To put this into perspective, the latter represented about 14 percent of the Chilean average

monthly wage in 2013.[15] Columns (3) and (4) present the results separately for men and women. It is evident that the effect on schooling was similar for both sexes, while the effect on labor income was largely driven by men. This is not surprising: although female labor participation in Chile improved over the last decades, it was still low, at around 60 percent, and was among the lowest in Latin America (Gasparini and Marchionni 2015). For men, the effect on labor income was about US$335, or approximately 17 percent of the average monthly wage.[16]

We suspected that one possible mechanism underpinning the effect of SUF on labor income was the additional years of schooling obtained by the treated cohorts. At first glance, 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 for 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 noncognitive abilities. Second, since individuals in our treatment cohort were at a critical age—between 14 and 18 years of age—when the program started in 2002, the additional years of schooling could have led to the completion of the school certificates needed to enter the formal labor market or to access better-paid occupations.

We tested these two possible routes with the available data in the CASEN surveys. The first route was 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?"[17] However, we did not find significant differences in these domains between treatment and control cohorts. We tested the second route by looking at the probabilities of attaining a secondary school certificate. We found that the treatment cohort was twice as likely to complete secondary schooling (60 percent) as the control cohort (30 percent).

Thus, the evidence seems to suggest that it was not simply the additional years of schooling that lay behind the significant increases in labor income among young male members of the treatment cohort. More importantly, the program facilitated the *completion* of secondary education, and it was this that most likely helped these individuals to access better-paid occupations.[18]

It is worth pointing out that the psychological support provided by social workers may itself also have had 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.[19] Earlier studies found that nonmonetary interventions can also have positive effects on child development: e.g., Dahl and Lochner (2012), in the context of the United States, and Paxson and Schady (2010), in the context of Ecuador. However, we suspect that these potential factors played a very minor role in our results, since all

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

We expected some possible sources of measurement error in our estimates. One may have come from the fact that treatment status in childhood was not directly observed, but instead was approximated with retrospective information on parental educational levels. The control group might have included individuals who grew up in eligible households, while the opposite might also have been observed in the treatment group. If that were the case, our estimates would have been biased downward.

Another potential source of bias could have arisen from sibling spillover effects—i.e., if noneligible individuals with younger siblings who received treatment were also positively affected by the program via the monetary subsidy and changes in behavior. Similarly, we might suspect that noneligible individuals would still have benefited from *Chile Solidario* if they lived with an elderly person eligible to receive an old-age pension.

To investigate these potential sources of measurement error, we estimated the probability among individuals eligible to receive *Chile Solidario* of living with older siblings or an elderly person in the household. The estimated probabilities were relatively low—less than 10 percent—and statistically nonsignificant when compared with the probabilities of noneligible individuals. Consequently, while we cannot disregard the possibility of sibling spillover effects and other potential sources of bias, the evidence suggests that these were not a cause for concern.

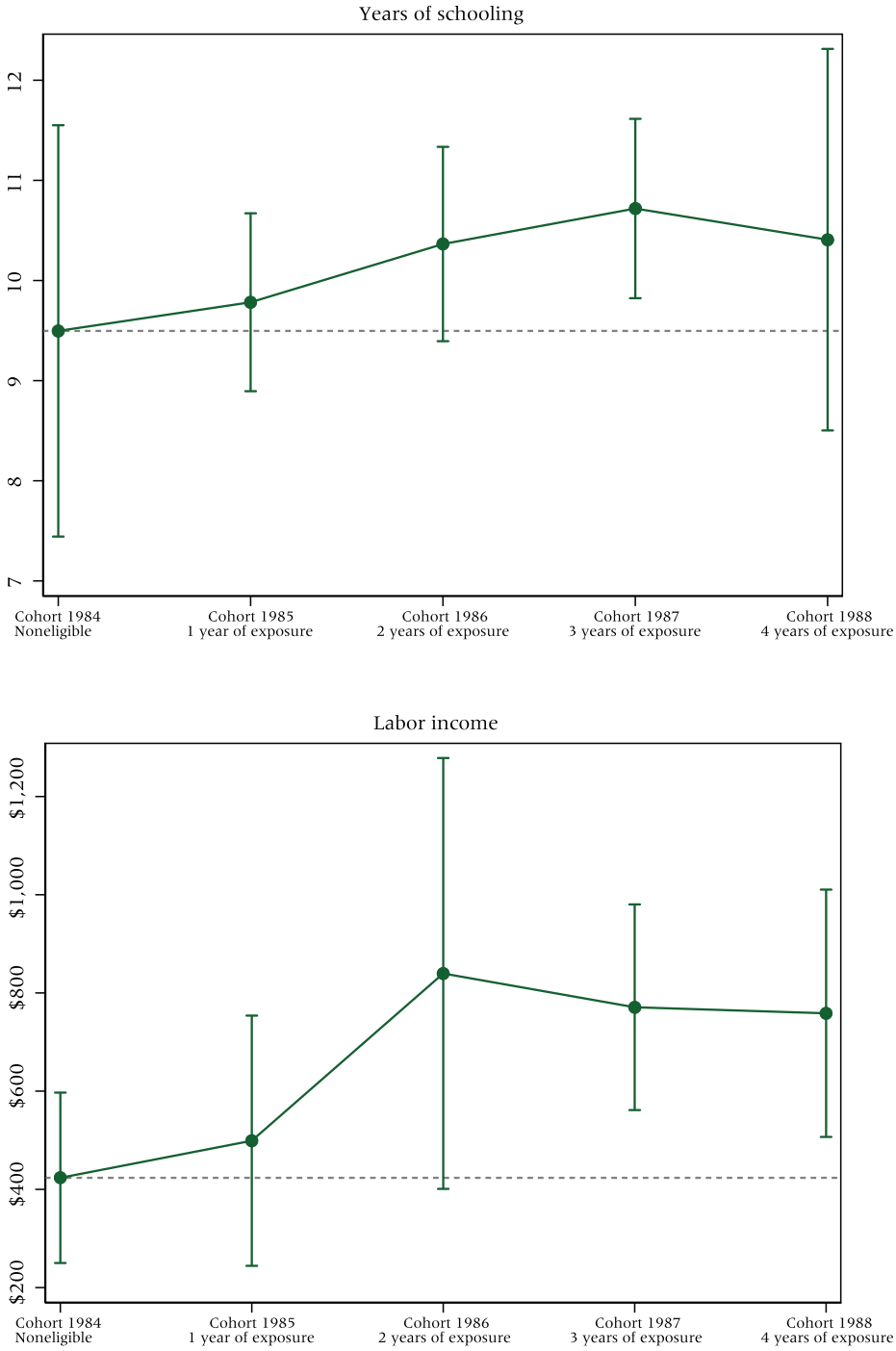
Intensity of treatment

Figure 5 shows the findings on the intensity of the treatment effect. As expected, the treatment effect varied with the time exposure of individuals to the social transfer. However, we observed a measurable effect, especially for labor 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, as well as the fact that *Chile Solidario* was implemented gradually in the first years until it addressed all eligible families.

Impact heterogeneity

We analyzed impact heterogeneity by computing the DD estimators separately for different population subgroups. We first divided the analysis by rural and urban areas, as well as by indigenous and nonindigenous groups. Then we restricted the analysis to women and tested for impact variation between married and single women and between women with and without children.

FIGURE 5 Intensity of treatment effect



SOURCE: Authors' estimations, based on CASEN 2013 (Ministerio de Desarrollo Social 2013).

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

(a) Years of education	(1) Rural	(2) Urban	(3) Indigenous	(4) Nonindigenous
DD	−0.037	1.661***	1.377***	1.237***
	(0.642)	(0.469)	(0.484)	(0.476)
Treated	−1.644***	−2.469***	−1.916***	−2.332***
	(0.183)	(0.285)	(0.345)	(0.242)
Born after 1984	−0.096	−0.053	0.162	−0.083
	(0.333)	(0.258)	(0.454)	(0.223)
Observations	3,108	8,582	2,076	9,614

(b) Labor income	(1) Rural	(2) Urban	(3) Indigenous	(4) Nonindigenous
DD	338.716	305.373**	120.088	338.291**
	(234.829)	(140.981)	(108.381)	(154.937)
Treated	−96.814**	−127.556***	−36.015	−149.415***
	(44.167)	(36.399)	(57.601)	(32.053)
Born after 1984	−67.116	19.628	−7.417	12.406
	(74.009)	(93.335)	(79.145)	(88.173)
Observations	2,001	6,148	1,441	6,708

*$p < 0.10$, **$p < 0.05$, ***$p < 0.01$.
NOTES: DD is the coefficient of the interaction term. Control variables included age, age squared, number of household members, rural or urban location, dummies for region of residency, ethnic background (indigenous or not), a dummy for migrants, and self-reported health. Cluster robust standard errors at municipality level in parentheses.
SOURCE: Authors' estimates, based on CASEN 2013 (Ministerio de Desarrollo Social 2013).

Interestingly, we found that the effect of *Chile Solidario* on schooling and labor income was significant in urban areas but nonsignificant in rural areas (see panel 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, Galasso, and Ginja (2019) argued that these heterogeneous effects may be due to the remoteness of rural communities in Chile and the associated transaction and opportunity costs of program membership.

Furthermore, we found significant treatment effects on schooling for both indigenous and nonindigenous groups, but the effects became nonsignificant for indigenous groups when we measured the treatment effects on labor income (see panel b of Table 3). This may reflect the existence of discriminatory norms against indigenous groups that prevail in the labor market in Chile, and which ethnographic and anthropological research has long emphasized (e.g., Merino et al. 2009).

When we disentangled the analysis by different groups of women, we found an interesting degree of heterogeneity (Table 4). First, we observed no sizable effects of the program on the labor income of married women and women with children.[20] In contrast, for women without children, the program treatment effects were large and statistically significant. These

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

(a) Years of education	(1) Married or in relationship	(2) Single	(3) No children	(4) With children
DD	1.156*	1.640**	3.606**	0.959**
	(0.615)	(0.741)	(1.489)	(0.426)
Treated	−2.288***	−2.315***	−4.868***	−2.134***
	(0.230)	(0.478)	(1.340)	(0.181)
Born after 1984	−0.651**	0.161	−0.840	−0.274
	(0.311)	(0.405)	(0.657)	(0.275)
Observations	4,861	2,038	368	7,277

(b) Labor income	(1) Married or in relationship	(2) Single	(3) No children	(4) With children
DD	29.924	69.393	441.021***	35.739
	(104.444)	(109.484)	(163.648)	(72.5578)
Treated	−159.937***	−124.488**	−466.370***	−127.336***
	(36.160)	(49.486)	(144.554)	(25.582)
Born after 1984	−75.075	−19.149	−415.544	−35.201
	(84.727)	(92.209)	(307.236)	(54.533)
Observations	2,249	1,502	271	4,049

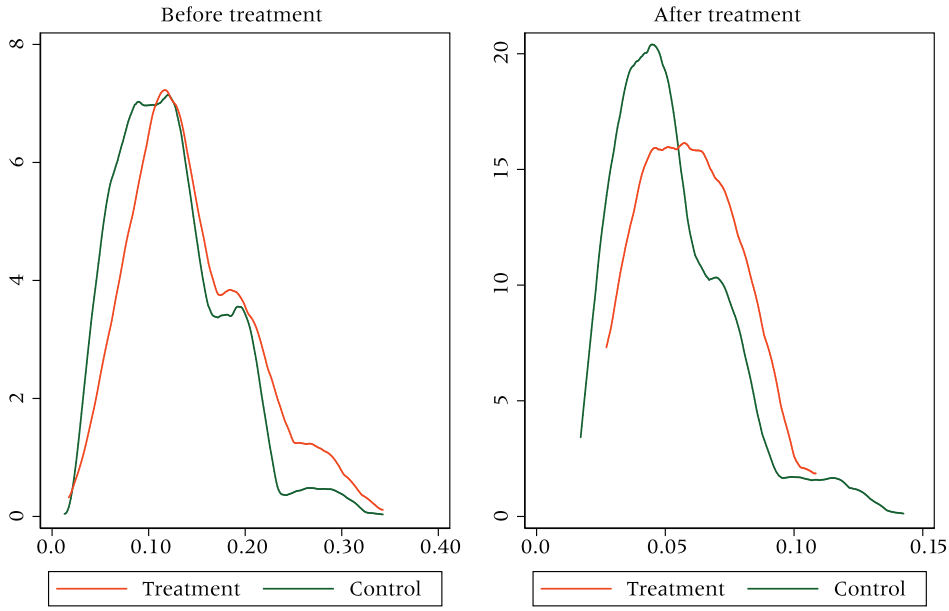
*$p < 0.10$, **$p < 0.05$, ***$p < 0.01$.
NOTES: DD is the coefficient of the interaction term. Control variables included age, age squared, number of household members, rural or urban location, dummies for region of residency, ethnic background (indigenous or not), a dummy for migrants, and self-reported health. Cluster robust standard errors at municipality level in parentheses.
SOURCE: Authors' estimates, based on CASEN 2013 (Ministerio de Desarrollo Social 2013).

results are consistent with economic theory about labor supply within the household (Becker 1985).[21] Second, for the case of schooling, our estimates showed significant effects for all women, although the treatment effects were strongest for women without children, followed by single women.

DD with matching

As discussed earlier, because of the nonrandom nature of assignment to treatment, the treatment and control groups may have differed in observable characteristics. To rule out the possibility that these differences systematically influenced the outcomes of interest, in addition to our baseline estimates, we computed DD with propensity score matching estimations (Heckman, Ichimura, and Todd 1997). In particular, we applied a kernel matching estimator that maximized the use of nearly all observations in the control group, while weighting them by the distance of the propensity score.[22] This methodology relied on an additional identifying assumption of common support (i.e., enough individuals in the control group who have a probability of treatment similar to the individuals in the treatment group).[23]

FIGURE 6 Estimated propensity scores, before and after treatment



SOURCE: Authors' estimations, based on CASEN 2013 (Ministerio de Desarrollo Social 2013).

To avoid incurring biased estimates due to selection, we only used time-invariant pretreatment 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 scores for the four groups: individuals in the control and treatment groups before the intervention (cohorts born in 1973–1984), and individuals in the control and treatment groups after the intervention (cohorts born in 1985–1988). The graphic representations confirm that both groups shared a large area of common support before and after treatment.

Table 5 shows the results of the DD propensity score matching procedure. The treatment effect estimates did not deviate substantially from the baseline results. The effect of *Chile Solidario* on the schooling of men was very similar, at around 1.5 additional years, while the effect on labor income was slightly lower, at around US$290. Again, the effect on women's labor income was not statistically significant. All in all, the DD with matching estimators confirmed our findings indicating that *Chile Solidario*, by facilitating access to SUF, had important long-term effects on young adults.

Regression discontinuity

As an additional step to validate the DD estimates, we adopted a regression discontinuity (RD) design by restricting the analysis around the year of birth

TABLE 5 The long(er)-term effects of *Chile Solidario*: DD matching estimators

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

*p < 0.10, **p < 0.05, ***p < 0.01.
NOTES: Displayed values are coefficients of the interaction term between the dummies for time and treatment status. Variables used to estimate the propensity score were year of birth, sex, place of birth, indigenous group, and migration background. Type of kernel function was Epanechnikov. Cluster robust standard errors at municipality level in parentheses.
SOURCE: Authors' estimations, based on CASEN 2013 (Ministerio de Desarrollo Social 2013).

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 who were younger than 18 and therefore eligible for treatment.

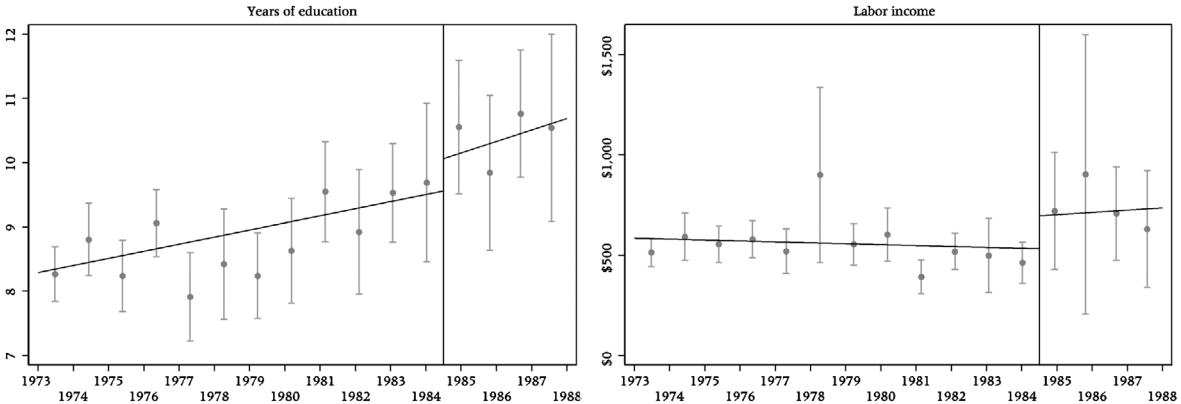
Because of the smaller number of observations, especially within each cohort (bin) and around the cut-off, the RD in this case was less powerful than the DD approach. However, it served as a complementary method to validate our baseline results. Because of the program's high uptake rates, we assumed a sharp design and opted for a parametric approach using all observations at both sides of the cut-off point to obtain the RD estimates (see, e.g., Hahn, Todd, and Van der Klaauw 2001).[24]

Figure 7 graphically shows a clear discontinuity in both outcome measures—years of education and labor income—at the cut-off point. Table 6 presents the treatment effects based on the RD estimates. For men, the treatment effects were qualitatively similar to the results from the DD estimators. Furthermore, the RD estimates confirmed that there was no program effect on women's labor income. In contrast to the DD estimators, the RD results showed no sizable program impacts on women's schooling. Nevertheless, the RD results should be evaluated with caution, given their limited precision due to sample constraints.

Robustness checks

To test for the validity of our preferred DD baseline estimates, we performed a series of additional robustness checks.[25] First, we restricted our sample to the cohorts of individuals born in 1981–1988, to avoid age effects affecting coresidence in the household of origin. Second, we set missing values in labor income to zero when information on employment status was available but when the individual was indicated as unemployed or inactive. Third, we split the control group between individuals whose parents had incomplete and complete primary education and ran a model including fixed effects at

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	−0.504
	(0.661)	(0.673)	(1.067)	(1.039)	(0.917)	(0.930)
N	1394	1371	567	554	827	817
Labor income (relative to cohort average)	144.43	110.77	311.83	319.57	−103.46	−134.61
	(141.320)	(141.400)	(234.030)	228.360)	(87.280)	(87.870)
N	970	950	532	519	438	431

NOTES: Covariates included to obtain conditional estimates were rural or urban location, number of household members, indigenous group, migration background, and self-reported health. Cluster robust standard errors at municipality level in parntheses.
SOURCE: Authors' estimations, based on CASEN 2013 (Ministerio de Desarrollo Social 2013).

those group levels. Fourth, we ran all estimations without weighting by the inverse probability of selection provided by the survey.[26]

Fifth, although we tested exhaustively for the existence of common trends prior to the treatment, the assumption of common trends posttreatment in the absence of the intervention could not be verified, because it relied on a counterfactual scenario. As a further examination, we ran a placebo test by restricting the sample to pretreatment 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 an incomplete secondary education, and another that assessed the effect of *Chile Solidario* on nonlabor income, which was an outcome that we expected not to have been affected by the program.

The presence of outliers could have been a source of measurement error in our treatment effect estimates on labor income. An exploration of our data revealed that there were very few observations in the treatment group above US$5,000 in monthly labor income, and among these, there was only one after the treatment. Excluding these observations yielded lower but consistent estimates of the effect on labor income, at around US$230 for men.

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

Conclusions

In this study, we examined the long(er)-term effects of *Chile Solidario*, a social program that provides the poorest households with preferential access to SUF, a CCT with the explicit objective of improving children's human capital. We focused on the educational attainment and labor 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 DD approach and regression discontinuity design to measure the impact of the program.

Our findings show that *Chile Solidario*, and particularly the uptake of SUF, had a positive and long-lasting impact among the extremely poor in Chile. Individuals who spent their childhood in poverty and were eligible to receive the program attained higher educational and labor income levels as adults than did individuals from poor families who were not eligible to receive the transfer. The average treatment effects are in the order of about 1.2 years of schooling and an additional US$200–$250 in labor income per month, which represents about 15 percent of the Chilean average monthly labor income.

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

Thus, the positive short-term effects reported by earlier impact studies of the program seem to have persistently improved the human capital of children from poor households, which in the longer term translated into better educational achievements and higher labor incomes for program beneficiaries. Our study contributes to the scant literature on long(er)-term effects of CCTs and provides evidence that under certain conditions such programs can contribute to positive changes in the future living conditions of beneficiary children.

The findings that average treatment effects on labor income among women were nonsignificant and that the completion of secondary education was the most likely mediating channel through which school attainment led to increases in labor income among men both indicate that the impact of *Chile Solidario* most likely depends on societal norms and structural factors that underpin the functioning of labor markets in Chile.

Further research is needed to better understand the incentive mechanisms and societal norms that seem to inhibit women from taking full advantage of the future opportunities that CCTs are meant to generate.

Notes

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

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

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

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

5 All estimates are weighted by the inverse probability of selection, which are provided by the survey design. However, as Solon, Haider, and Wooldridge (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.

6 Generally, the conditionality of *Chile Solidario* is tied to the willingness and effort of the household to fulfill 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.

7 Since the program 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 uptake of SUF.

8 We performed robustness checks with a shorter age interval.

9 We always refer to the parent with the highest educational level within the family or to the parent with available information, if information for one of the two is missing. Since we relied on the retrospective questions in the survey on the father's and mother's education, we did not need individuals to be residing with their parents in the same household to retrieve this information.

10 Table A1 in the Appendix illustrates the Chilean educational system; it 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.

11 A violation of the second assumption might derive from the possibility that the likelihood of leaving home rises with age. Therefore, people born in 1973–1984 (i.e., those aged 18–29 when the program started in 2002) might have already left their household of origin, in contrast with people born in 1985–1988 (i.e., those 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 percent in 2014, and 62 percent 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 expected that the bias resulting from leaving home should not affect our estimates significantly. Nevertheless, we performed robustness checks restricting the time window of analysis, with qualitatively equivalent results. The results of this alternative specification are presented in the Appendix.

12 To evaluate the intensity of the treatment effect with program exposure and to test the parallel trends assumption, we also ran our estimations including a full set of cohort dummies.

13 As a robustness check, we clustered standard errors at the regional level, applying the bootstrap-based procedure to get significance levels with few clusters, as proposed by

Cameron, Gelbach, and Miller (2008). Also following this methodology, the estimates for the treatment effect of the social program, on both years of education and labor income, differed significantly from zero.

14 Figure 4 serves as a first justification for the validity of the common trends assumption. Cohorts were 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 verified the validity of the assumption through a model that included a full set of dummies for cohorts and the respective interactions with the treatment status. We jointly tested the coefficients of the interaction terms of the pre- and post-treatment cohorts against the null hypothesis of equality to zero. The null could not be rejected for pre-treatment cohorts (F=1.11, Prob>F=0.3520) and was rejected after the treatment (F=4.93, Prob>F=0.0007).

15 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).

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

17 The other questions were: "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?"

18 These results can be found in the Appendix.

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

20 The group of married women includes women cohabiting with a partner.

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

22 The type of kernel function adopted here is Epanechnikov.

23 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).

24 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.

25 The results of the robustness checks are reported in sections a–g of Table A4 in the Appendix.

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

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Appendix

TABLE A1 Chilean educational system and median monthly income in US dollars (cohorts 1977–1988)

Years	Type			Median total income (weighted)	Median labor 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	%	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 1973–1988)

Parental educational level	Weighted frequency	%	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***
	(0.301)	(0.281)	(0.397)	(0.382)
Treated	−2.276***	−1.982***	−2.003***	−1.954***
	(0.109)	(0.108)	(0.128)	(0.168)
Born after 1984	0.466***	−0.045	−0.210	0.270
	(0.063)	(0.138)	(0.171)	(0.247)
Observations	11,821	11,690	7,661	4,029

Labor income (in U.S. dollars)	(1) Unconditional	(2) Conditional	(3) Only women	(4) Only men
DD	290.668***	281.291***	22.186	373.473***
	(105.174)	(100.739)	(68.947)	(140.6176)
Treated	−115.975***	−106.991***	−107.458***	−142.176***
	(28.630)	(17.761)	(19.251)	(28.176)
Born after 1984	−106.401***	−2.381	−15.887	−26.501
	(15.774)	(34.968)	(42.567)	(53.260)
Observations	8,244	8,149	4,330	3,819

TABLE A4 (Continued)

(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		Labor income	
DD	1.187***	1.482**	323.496**	307.772**
	(0.416)	(0.699)	(154.629)	(124.834)
Treated	−2.188***	−2.366***	−137.634***	−156.134***
	(0.194)	(0.312)	(26.706)	(48.437)
Born after 1984	−0.356	0.295	−114.717	57.158
	(0.250)	(0.474)	(83.979)	(165.576)
Observations	7,101	4,433	4,863	3,175

(c) Symmetric time window: cohorts 1981–1988

Years of education	(1) Unconditional	(2) Conditional	(3) Only female	(4) Only male
DD	0.806**	0.683*	0.543	1.047*
	(0.378)	(0.369)	(0.592)	(0.582)
Treated	−1.765***	−1.616***	−1.689***	−1.873***
	(0.232)	(0.236)	(0.460)	(0.435)
Born after 1984	0.179**	0.026	−0.098	0.024
	(0.075)	(0.164)	(0.287)	(0.363)
Observations	4,725	4,668	3,190	1,478

Labor income	(1) Unconditional	(2) Conditional	(3) Only female	(4) Only male
DD	361.675***	341.367***	23.795	378.425***
	(103.704)	(102.642)	(75.032)	(142.177)
Treated	−186.982***	−165.003***	−114.749***	−219.162***
	(32.662)	(31.456)	(39.635)	(62.164)
Born after 1984	−71.869***	−45.365	−46.271	−215.213
	(18.826)	(36.907)	(79.391)	(202.848)
Observations	3,180	3,137	1,738	1,399

TABLE A4 (Continued)

(d) Full set of dummies for cohort and group

	(1)	(2)	(1)	(2)
	All		Men	
	Education	Labor income	Education	Labor income
DD	1.318***	274.576***	1.612***	346.659***
	(0.362)	(97.872)	(0.614)	(129.312)
Cohort = 1974	−0.218	−0.557	0.591	51.332
	(0.307)	(55.355)	(0.432)	(78.794)
Cohort = 1975	0.150	111.644	0.992***	292.081**
	(0.266)	(76.922)	(0.201)	(143.514)
Cohort = 1976	0.681*	62.439	2.024***	144.424*
	(0.413)	(58.472)	(0.549)	(85.197)
Cohort = 1977	−0.072	5.961	0.676**	79.188
	(0.256)	(63.655)	(0.303)	(102.195)
Cohort = 1978	0.008	49.109	1.026***	61.418
	(0.304)	(65.718)	(0.340)	(72.941)
Cohort = 1979	−0.090	7.352	1.250***	45.755
	(0.296)	(68.512)	(0.328)	(80.886)
Cohort = 1980	0.389	9.110	1.096***	71.307
	(0.293)	(58.090)	(0.278)	(69.007)
Cohort = 1981	0.128	−82.697*	1.058***	−107.628
	(0.257)	(49.452)	(0.284)	(72.697)
Cohort = 1982	0.148	−17.525	1.034***	−33.064
	(0.265)	(63.054)	(0.270)	(62.724)
Cohort = 1983	0.385	−77.509	1.022***	−23.130
	(0.273)	(54.226)	(0.292)	(82.544)
Cohort = 1984	0.221	5.516	0.842**	135.885
	(0.303)	(105.082)	(0.355)	(202.253)
Cohort = 1985	0.307	−97.861	1.039***	−86.457
	(0.290)	(94.005)	(0.265)	(134.785)
Cohort = 1986	0.226	−118.243*	0.750**	−135.008*
	(0.277)	(64.079)	(0.346)	(71.702)
Cohort = 1987	0.037	−205.729***	0.932***	−225.521***
	(0.286)	(50.312)	(0.301)	(68.848)
Cohort = 1988	0.289	−152.415***	0.866**	−108.170
	(0.281)	(55.822)	(0.359)	(74.626)
Incomplete primary	1.879***	88.329***	2.072***	145.732***
	(0.192)	(27.618)	(0.334)	(45.125)
Complete primary	2.817***	189.980***	2.900***	186.067***
	(0.224)	(41.013)	(0.390)	(57.229)
Observations	11,690	8,149	4,029	3,819

TABLE A4 (Continued)

(e) Labor 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***
	(81.966)	(79.145)	(51.824)	(127.889)
Treated	−128.104***	−119.386***	−103.361***	−238.256***
	(34.564)	(26.973)	(26.973)	(53.909)
Born after 1984	−79.109***	−12.545	−25.239	−139.609
	(23.540)	(67.036)	(56.694)	(175.552)
Observations	7,737	7,642	5,084	2,558

(f) Labor income (excluding outliers)

	(1) Unconditional	(2) Conditional	(3) Only female	(4) Only male
DD	198.048***	197.265***	51.903	232.738***
	(62.293)	(65.234)	(69.324)	(82.214)
Treated	−163.350***	−133.877***	−152.067***	−159.591***
	(32.483)	(29.243)	(28.753)	(41.141)
Born after 1984	−110.948***	−22.264	−47.974	−62.458
	(30.543)	(88.447)	(74.136)	(163.684)
Observations	8,241	8,148	4,330	3,818

(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
	(0.208)	(0.207)	(0.216)	(0.499)
Treated	1.012***	0.721***	0.682***	0.817***
	(0.127)	(0.123)	(0.140)	(0.289)
Born after 1984	0.211**	0.081	−0.017	0.314
	(0.097)	(0.238)	(0.255)	(0.388)
Observations	13,643	13,495	9,016	4,479

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

	(1) Unconditional	(2) Conditional	(3) Only female	(4) Only male
DD	−79.305	−37.607	−47.787	−28.172
	(87.074)	(93.334)	(54.407)	(186.338)
Treated	128.717***	71.446	72.158	107.962
	(40.824)	(47.629)	(47.172)	(90.669)
Born after 1984	−110.948***	−4.514	29.831	−144.018
	(30.542)	(72.148)	(69.213)	(146.974)
Observations	9,622	9,520	5,273	4,247

TABLE A4 (Continued)

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
	(0.435)	(0.432)	(0.508)	(0.727)
Treated	−2.500***	−2.283***	−2.252***	−2.372***
	(0.242)	(0.221)	(0.211)	(0.367)
Born after 1984	0.277***	0.188	−0.126	0.751*
	(0.105)	(0.293)	(0.320)	(0.446)
Observations	9,713	9,607	6,240	3,367

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

	(1) Unconditional	(2) Conditional	(3) Only female	(4) Only male
DD	−36.016	18.610	94.707	−65.625
	(60.528)	(60.579)	(58.830)	(101.490)
Treated	−154.559***	−135.490***	−164.824***	−150.229***
	(34.333)	(32.157)	(30.965)	(45.327)
Born after 1984	−19.678	70.512	56.413	126.103
	(37.731)	(68.433)	(89.974)	(91.068)
Observations	6,856	6,780	3,583	3,197

Placebo test III (outcome = nonlabor income)

	(1) Unconditional	(2) Conditional	(3) Only female	(4) Only male
DD	−1.212	−0.841	−10.043	15.425
	(11.084)	(11.551)	(25.581)	(9.499)
Treated	−6.390	−7.157	4.743	−11.452***
	(8.130)	(8.067)	(16.613)	(3.664)
Born after 1984	−9.330**	−6.905	−7.323	−0.631
	(3.887)	(7.166)	(13.400)	(8.348)
Observations	8,244	8,149	4,330	3,819

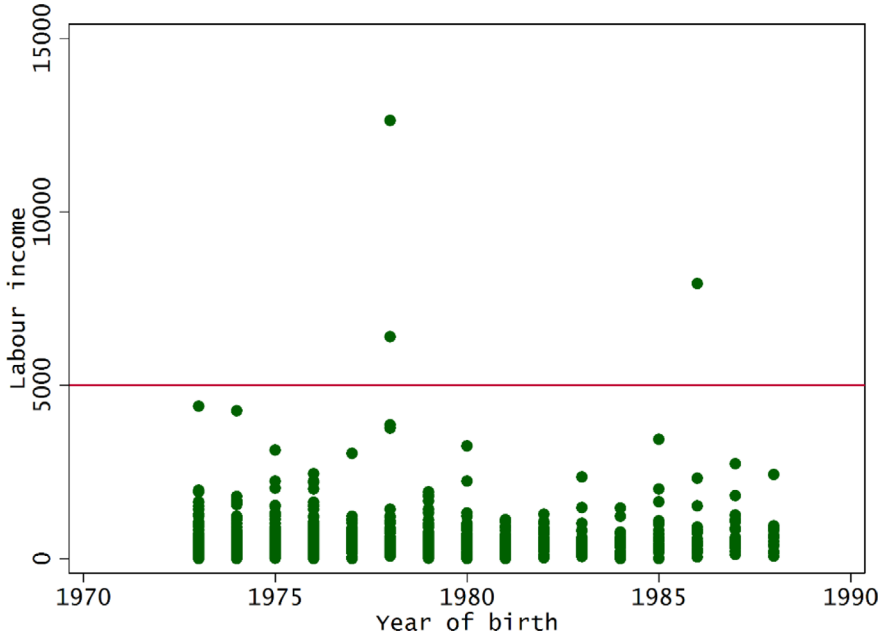
*$p < 0.10$, **$p < 0.05$, ***$p < 0.01$.
NOTES: DD is the coefficient of the interaction term. Control variables include age, age squared, number of household members, rural or urban location, dummies for region of residency, ethnic background (indigenous or not), a dummy for migrants, and self-reported health. Cluster robust standard errors at municipality level in parentheses.
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*
	(0.072)	(0.068)	(0.073)	(0.105)
Treated	−0.296***	−0.254***	−0.269***	−0.244***
	(0.026)	(0.024)	(0.027)	(0.038)
Born after 1984	0.062***	−0.008	−0.051	0.081
	(0.017)	(0.041)	(0.046)	(0.087)
Observations	11,818	11,687	7,659	4,028

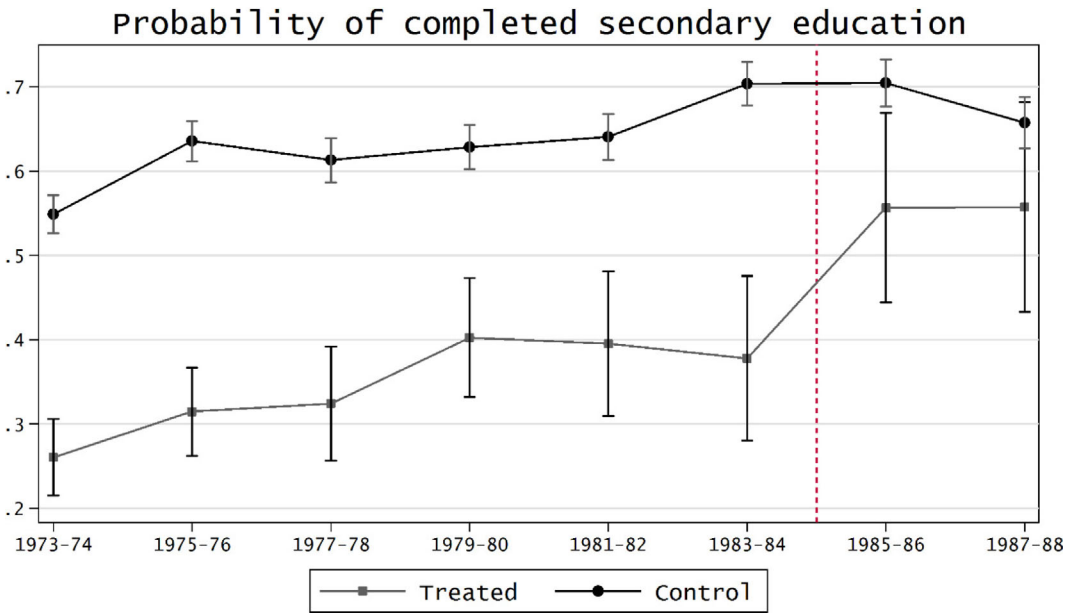
*$p < 0.10$, **$p < 0.05$, ***$p < 0.01$.
NOTES: DD is the coefficient of the interaction term. Control variables include age, age squared, number of household members, rural or urban location, dummies for region of residency, ethnic background (indigenous or not), a dummy for migrants, and self-reported health. Cluster robust standard errors at municipality level in parentheses.
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).