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Can poverty alleviation programs crowd-in private support? Short- and Middle-Run Effects of a Conditional Cash Transfer Program on Inter-Household Transfers

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Abstract

Conditional cash transfer (CCT) programs have become an important component of social assistance in developing countries. CCTs, as well as other cash subsidies, have been criticized for allegedly crowding out private transfers. Whether social programs crowd out private transfers is an important question with worrisome implications, as private support represents an important fraction of households' income and works as a risk sharing mechanism in developing countries. Furthermore, empirical evidence on the effect of public transfers on private transfers is mixed. This paper contributes to the literature by using a unique dataset from the quasi-experimental evaluation of a CCT in Colombia and an empirical strategy that allows us to correct for pre-existing differences between treated and control groups. Our results suggest that the public transfer did not crowd out private transfers, neither in the short-run nor in the middle-run. Instead, it increased the probability of receiving support in cash, in kind, and in non-paid labor from different private sources by approximately 10 percentage points. Moreover, we find that the monetary value of private transfers increased by 32-38% for treated households.

Keywords: conditional cash transfer; public transfers; private transfers; inter-household transfers; crowding-out

Introduction

Conditional cash transfer (CCT) programs have become an important component of social assistance in developing countries over the last two decades. CCTs are social assistance programs aimed at alleviating poverty in the short run and breaking the intergenerational transmission of poverty in the long run (Ibarrarán *et al.*, 2017). These programs are targeted to low-income households with children, and comprise monetary transfers that are conditioned upon behaviors related to children's education, health and nutrition, such as school enrollment and attendance and assisting to regular health check-ups. In 2017, all Latin American countries but Cuba, Nicaragua[1] and Venezuela, and over 20 countries in Asia and Africa had a CCT program in operation (García and Saavedra, 2017; Cecchini and Atuesta, 2017). The impressive growth in the prevalence of CCTs has been accompanied by evidence about its effects on reducing poverty and improving consumption and schooling (Fiszbein *et al.*, 2009; García and Saavedra, 2017). Nevertheless, some studies have raised the concern that CCTs can have unexpected negative effects by crowding out private transfers (Albarran and Attanasio, 2003). As with other public cash transfers, CCTs may displace private support received by beneficiary households from other people outside the household like family or friends.

Economic theory predicts that, in the presence of altruism, public transfers will displace private transfers (Barro, 1974; Becker, 1974). From a policy perspective, if public transfers have a large effect on reducing private transfers, then beneficiary households would not necessarily be better off because households may end up with no gain or, even worse, a reduction in their total income. This potential risk is particularly important in economically developing countries, where market imperfections and the pervasive risks faced by poor households render private transfers a common safety net and risk-sharing mechanism. Indeed, in the developing world private transfers represent an important fraction of households' income (Cox and Jimenez, 1990; Cox *et al.*, 1998; Fafchamps, 2011).

Previous studies have shown crowding-out effects of social protection programs like old age pensions (Gerardi and Tsai, 2014; Jensen, 2003; Jung *et al.*, 2016), social assistance programs such as unconditional cash transfers (Rosenzweig and Wolping, 1994; Strobbe and Miller, 2011) or public transfers in general (Kang and Sawada, 2003; Kananurak and Sirisankanan, 2016). However, evidence on the displacement effects of Conditional Cash Transfers on private support is more scarce and mixed. For instance, Albarran and Attanasio (2003) examined the effects of the Mexican CCT program *Progresa* and found a negative effect on private support in cash or in-kind after eight months of exposure to the program. However, Angelucci *et al.* (2012), found that two years after the program started, there is a crowding out effect of *Progresa* on in-kind transfers[2] but not on cash transfers. Similarly, Teruel and Davis (2000) found no crowding-out effect of *Progresa* 19 months after exposure to the program. In the context of Nicaragua and Honduras, Nielsen and Olinto (2007), found a crowding out effect of CCTs for food private transfers, but not for cash remittances.

This paper builds on previous literature on CCTs and private transfers by examining whether the Colombian CCT Families in Action (FA) program (in Spanish, *Familias en Acción*) crowded out inter-household transfers in cash, in-kind, and in unpaid labor. We use quasi-experimental data collected from the impact evaluation of the program. These data are unique for two main reasons: 1) baseline and follow-up data were collected from households in municipalities where the program was implemented (treatment group) as well as from comparable households in municipalities where the program was not implemented (control group), allowing the implementation of a strategy to assess causal effects, and 2) the survey included a rich set of questions related to contributions received by households from several sources, allowing us to identify inter-household private transfers with a high level of specificity in terms of the type of each transfer (in-cash, in-kind, or in unpaid labor) and its source (family, friend, neighbor). We use baseline, first follow-up (approximately 2 years afterward), and second follow-up (approximately 5 years after baseline) surveys to estimate short term and middle term impacts of the CCT through a difference-in-differences (DD) strategy. Our methodology allows us to account for unobserved pre-existing differences, under the assumption that there are no differences in time-variant characteristics, which may be plausible given that treated and control groups were matched previously to be as comparable as possible (Gómez *et al.*, 2004). Furthermore, we perform additional robustness checks using a matched DD approach, which additionally allows us to control for observed heterogeneity at baseline between treatment and control groups.

Our results show that the CCT did not crowd out private in-cash or in-kind transfers, nor did it crowd out receiving support in unpaid labor between households. On the contrary, the CCT actually increased the likelihood that beneficiaries received support from neighbors by 3.4 percentage points at first follow-up. At second follow-up, the program increased the probability of receiving support from any private source by 10.1 percentage points: from neighbors by 6.3 percentage points, and from relatives by 3.7 percentage points. Overall, the CCT increased the total average value of private transfers received by households by 32% at first follow-up and 38% at second follow-up compared to baseline levels. We also find larger effects in rural areas compared to urban areas. Data about participation in program meetings, as well as beneficiaries' knowledge of program materials, suggest that these program features play an important role in explaining the observed effects.

Our contribution to the literature on the relationship between public and private transfers is threefold. First, we extend our analysis to a wider range of households and individuals relative to previous studies on crowding-out effects of public transfers. Most of the literature in developing countries examines inter-household transfers among parents and children (e.g., Cox and Jimenez, 1992; Jensen, 2003; Schoeni, 2002). In this paper, we examine transfers among friends, neighbors, and relatives beyond parents and children. Second, we consider a more comprehensive definition of private support, examining not only cash transfers, but also in-kind transfers and unpaid labor. Third, we contribute to the literature on the effect of CCTs on private transfers, where evidence is still mixed (Albarran and Attanasio, 2003; Nielsen and Olinto, 2007; Teruel and Davis, 2000).

The remainder of the paper proceeds as follows. Section 2 discusses related literature, presenting empirical evidence on the relationship between public and private transfers. Section 3 describes the CCT and the particular features planned by program staff to build social capital and foster collaboration. Section 4 presents the data and summarizes descriptive statistics. Section 5 presents the identification strategy. Section 6 summarizes the main results, including the overall effect of the program on the likelihood of receiving private support and the value of the support, and heterogeneous treatment effects. The final section concludes and presents policy implications.

Public transfers and private support

In the context of economically developing countries, most research on the effects of public transfers on private support focuses on the relationship between social security or pension income and private transfers from younger to older generations. Cox and Jimenez (1992) examined this question in Peru and found that receiving social security reduced the probability of receiving private transfers by 8 percentage points. However, they found no relationship between the amount of social security income received and receipt of private transfers. Jensen (2003) looked at the effect of state old-age income in South Africa on remittances sent from migrant children and found that public pension income reduced private transfers from children living away from home (by 0.25 to 0.30 rands for each rand of public pension[3]). Moreover, using data from a Mexican income allowance program for senior citizens, Juarez (2009) found that the public subsidy received by the elderly had a large crowding-out effect on private transfers. That is also the case for Taiwan's old-age allowance program, where Lai and Orsuwan (2009) found a crowding-out effect on transfers from adult children to parents receiving the public subsidy.

Evidence on the effects of other types of public subsidies on inter-household transfers is much more limited, and findings are mixed, depending on the context or type of program. A study from a randomized experiment of a food-for-training program in Southern Sudan found no evidence of a crowding-out effect on private transfers (Sulaiman, 2010). In the case of Burkina Faso, Kazianga (2006) found no crowding-out effects among low-income households, but did find crowding-out effects among middle-income households. Finally, in the case of Bangladesh, Mozumder *et al.* (2009) found crowding-out effects for a short-term intervention after a devastating flood, but no crowding-out effects for a means-tested longer-term intervention.

In the context of CCTs, evidence is inconclusive on the effect of the transfers on private support. In the case of Mexico, Albarran and Attanasio (2003) found crowding-out effects of the CCT program *Progresa* on private transfers in cash or in kind, and found that the size of the effect decreases as income variance increases. However, Teruel and Davis (2000), using more than one wave of follow-up data, found no crowding-out effects of the same program after 19 months of exposure. In addition, Angelucci *et al.* (2012) found no crowding-out effect of *Progresa* on cash transfers after two years of exposure to the program but did find that the CCT program decreased in-kind transfers. In a different study, Nielsen and Olinto (2007) estimated the effects of CCTs in Nicaragua and Honduras and found no evidence of crowding-out effects on remittances in either country, but a negative effect on food transfers in the case of Nicaragua. One possible explanation suggested by the authors is that the amount of the subsidy in Nicaragua is much larger than in Honduras.

These mixed results may hide differences in the design of the public transfers in general, and heterogeneity in the characteristics of CCTs in particular. For instance, providing a cash subsidy electronically, with no interaction among participants or among program staff and participants, is different from providing a cash subsidy that also enhances interactions among individuals. As explained in the next section, the CCT program we analyze in this paper had its own particular dynamics in terms of potential impact on crowding-out (or crowding-in), as it offered additional components that could have strengthened social bonds and fostered collaboration among households.

The conditional cash transfer program

FA is Colombia's flagship CCT program, aimed at fostering human capital accumulation and reducing extreme poverty. The program targets families with children living in extreme poverty, and has two main components: an education subsidy and a health and nutrition subsidy. The education subsidy is provided to households with children between seven and 17 years old on the condition that the children are enrolled in school and their attendance is at least 80%. In 2002, when the program started, the subsidy was 16.4 USD PPP[4], per month per child attending elementary school, and 32.8 USD PPP[5]) per month per child attending secondary school. The health and nutrition component is delivered to households with children under six and is conditional upon regular medical check-ups and participation in vaccination programs. In 2002, the nutrition subsidy was (54.4 USD PPP[6]) per month per family. On average, between 2002 and 2006 each household received transfers of approximately 117 USD PPP[7]) per month, which represented a 37.4% increase in average family monthly income (DNP, 2006).

FA's first phase (2000-2006) included complementary strategies to promote education and health, and to foster social capital and collaboration among beneficiaries[8]. One of the strategy's main components was Caregiving Meetings (in

Spanish, *Encuentros de Cuidado*). These meetings offered beneficiary mothers from the same neighborhood (or municipality[9], in cases where these were small) a space to talk about their concerns related to their families, and to discuss strategies to improve their families' and their own well-being.

Caregiving Meetings had several planned features to build fellowship among the beneficiaries, and to strengthen social bonds between neighbors, friends, and family. Each meeting began with an activity referred to as the '*ritual*', in which every mother had to offer food, music, or another good to the other beneficiaries. According to printed material from the first phase of the program, "*the ritual recovers collective feelings related to the sacred and the collective experience of unity. (…) The offering of food, music, and play must be present in every* Caregiving Meeting *(…) It is a symbolic way to share and to build a proper place to meet (…) and to find collective well-being*" (Presidencia de la República de Colombia, 2004).

Each meeting was led by a Leader Mother (in Spanish, *Madre Líder*), who was responsible for organizing and facilitating the meetings, strengthening the relationships among beneficiaries in the neighborhood or municipality, supporting initiatives related to the improvement of beneficiaries' well-being through collective work, and managing aid from private sources (Acción Social, 2010; Presidencia de la República de Colombia, 2004). Beneficiary mothers elected the Leader Mother democratically: any mother could be a candidate, as long as she was a program beneficiary, was literate, and had good relationships with the community. The labor of each leader was voluntary and non-paid (Acción Social and DNP, 2010).

At Caregiving Meetings participants had access to printed material for all to read and discuss aloud, such as information booklets, decks of informative cards that were used for educational exercises and games with other beneficiaries, and a bi-monthly instructional journal. These materials had a strong focus on fostering social capital, and explicitly addressed the idea that beneficiaries should support each other in hard times (see Presidencia de la República, 2002; 2005).

Previous evidence suggests that the program had an impact on social capital: games in a field experiment revealed that beneficiaries were more likely to cooperate, to participate in neighborhood decisions and meetings, and to have higher trust levels compared to individuals in the control group (Attanasio *et al.*, 2015). As such, it is likely that FA could have crowding-in instead of crowding-out effects.

Methods

Data

We use data collected as part of the impact evaluation of FA that was supervised by the Colombian National Planning Department[10]. The program phase-in was not random across municipalities, but rather was targeted to households in situation of poverty[11] living in small municipalities with fewer than 100,000 inhabitants and with a minimum level of educational, health, and financial infrastructure. FA did not target district capitals nor municipalities in the coffee region (which received special social assistance after a natural disaster in 1995). Subsequently the program evaluators followed a quasi-experimental approach to evaluate the program, selecting 57 treatment and 65 control municipalities (Gómez *et al.*, 2004). Evaluators selected a random sample of beneficiary municipalities and matched them to control municipalities based on characteristics such as geography, urbanization (size of the population living in the municipality's urban area), number of eligible families, a quality of life index score, and education and health infrastructure.

After sampling municipalities, a random sample of eligible households was selected from each municipality (IFS-Econometria-SEI, 2003). Due to political pressures, the program started before evaluators collected baseline information in some municipalities, leaving 31 treatment municipalities with full baseline information. Since we do not have retrospective data on our outcome of interest (private help), we limit the sample to the municipalities with baseline information (31 treated and 65 control municipalities).

We use baseline (collected between June and October 2002), first follow-up (July and November 2003), and second follow-up (November 2005 and April 2006) surveys to identify short term and middle term effects of the CCT. Our sample consists of 5,781 households (2,341 in treated municipalities) that have complete data on outcomes and covariates at both baseline and follow-up. Data includes socioeconomic and demographic characteristics of treated and control households, such as household composition, monthly income, head of household's educational attainment and marital status, and access to basic services, among others.

Additionally, the data includes information about whether the household received any transfer in cash, in kind (e.g., food, clothes), or in unpaid labor in 12 months preceding the survey, as well as information on who provided the transfer (family, friend, or neighbor in the municipality or outside the municipality). The data also allows us to identify the total monetary value of the transfers. Households reported the monetary value of cash received from each source, as well as the value of in-kind help received, answering the question: "*how much would you have to pay for the in-kind help you received from each source.*" For unpaid labor, households reported the amount of *jornales* (i.e., working days) they received from each source. In order to estimate the monetary value of labor received, we multiplied the number of working days by the current minimum daily wage in Colombia (12.1 USD PPP[12]) for baseline and both follow-ups. Finally, we converted all monetary sums to 2002 Colombian Pesos (COP), considering annual inflation for the analysis.

TABLE 1. Sample characteristics by treatment group at baseline

	Treatment (T)	Control (C)	Difference (T-C)	SE for difference (T-C)
Municipality (N)	31	65	96	96
Quality of life index	53.92	56.20	−2.28	2.33
Population (urban)	13,749	12,660	1,089	3,497
Population (rural)	12,274	10,084	2,189	2,157
Number of banks	0.08	0.04	0.04*	0.02
Number of hospitals	0.94	0.65	0.29**	0.10
Region				
Atlantic	0.32	0.29	0.03	0.10
Eastern	0.23	0.31	−0.08	0.10
Central	0.32	0.29	0.03	0.10
Pacific	0.13	0.11	0.02	0.07
Taxes collected (millions of COP)	2.71	2.59	1.27	1.53
Household (N)	2,341	3,440	5,781	5,781
Number of people in the household	6.06	5.95	0.11	0.06
Number of adults with earnings	1.5	1.6	−0.10***	0.02
Household monthly income (COP)	256,607	278,152	−21,545**	7,904
Household head age	42.92	44.09	−1.16***	0.33
Household head education				
None	0.44	0.45	−0.01	0.01
Elementary	0.16	0.15	0.01	0.01
Secondary	0.04	0.04	0.00	0.01
Household head marital status				
Married	0.34	0.33	0.01	0.01
Single	0.02	0.02	0.00	0.00
Household basic services				
Water	0.67	0.64	0.03*	0.01
Gas	0.06	0.08	−0.02**	0.01
Electricity	0.85	0.89	−0.04***	0.01
Sewage	0.30	0.26	0.04**	0.01
Toilet with connection	0.51	0.54	−0.02	0.01

Notes:

1. Results reported: number of household and municipalities in treatment and control group; mean of treatment and control groups at baseline; and standard errors for difference between treatment and control group.
2. Results for analytical sample for estimation (excluding program dropouts at follow-up and missing values).
3. Average annual exchange rate of $1 USD= $2,275 COP.
4. * Significant at 10%, ** Significant at 5%, *** Significant at 1%.

As Table 1 shows, no statistical differences emerged between treatment and control municipalities in different characteristics, except for the number of banks and hospitals. Nonetheless, there are some differences in household characteristics. On average, treated households had fewer adults with earnings and thus a smaller amount of monthly income. Moreover, heads of households in the treatment group were younger compared to those in the control group and households in the treatment group were less likely to have access to electricity

TABLE 2. Private support by treatment group at baseline

	Treatment (T)	Control (C)	Difference (T-C)	SE for difference (T-C)
Cash				
From any private source	0.19	0.20	−0.01	0.01
From neighbors	0.06	0.07	−0.01	0.01
From family or friends living in municipality	0.06	0.05	0.01	0.01
In-kind				
From any private source	0.34	0.43	−0.10***	0.01
From neighbors	0.15	0.23	−0.08***	0.01
From family or friends living in municipality	0.12	0.13	−0.01	0.01
Labor				
From any private source	0.07	0.07	0.00	0.01
From neighbors	0.04	0.04	0.00	0.01
From family or friends living in municipality	0.02	0.02	0.00	0.00
Summary: any help				
From any private source	0.45	0.53	−0.08***	0.01
From neighbors	0.21	0.29	−0.08***	0.01
From family or friends living in municipality	0.17	0.18	−0.01	0.01
Value of the transfers (COP)				
From neighbors	36,899	73,715	−36,816***	6,448
From family or friends living in municipality	52,580	31,633	−20,946***	6,237
Total help received	107,259	179,456	−72,196***	10,809
Number of households	2,341	3,440	5,781	5,781

Notes:

1. Results reported: number of households and municipalities in treatment and control group; mean of treatment and control groups at baseline; and standard errors for difference between treatment and control group.
2. Results for analytical sample for estimation (excluding program dropouts at follow-up and missing values).
3. Average annual exchange rate of $1 USD= $2,275 COP.
4. * Significant at 10%, ** Significant at 5%, *** Significant at 1%.

or gas than households in the control group. Overall, our sample is composed of households living in extreme poverty at baseline: on average their monthly household income was 270,000 Colombian Pesos (COP) (315.9 USD PPP), and taking into account that six persons lived in each household on average, their monthly per capita income was less than 50,000 COP (58.5 USD PPP).

Table 2 presents household receipt of private support at baseline by treatment group. There are no significant differences in private support in the form of cash or labor between treatment and control groups. On average, 19% of households received cash transfers from private sources and 7% received unpaid

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labor. There is a difference, however, in the receipt of in-kind support: households in the treatment group were less likely to receive in-kind support from a private source (34%) than households in the control group (43%). It is important to note that most of this aid came from neighbors, friends, and family living in the municipality, suggesting the existence of strong social networks in the municipalities where these households reside.

In terms of the monetary value of help received, households received, on average, $143,400 COP (167.8 USD PPP) in one year. This is not a negligible amount considering that the minimum monthly wage in 2002 was $309,000 COP (361.6 USD PPP). Moreover, private transfers represented on average almost 5% of household's monthly income at baseline, showing the importance of private support for the households in our sample.

In sum, we do not find statistically significant differences in cash or labor support between treatment and control groups. We do find, however, that households in the treatment group were less likely to receive in-kind support (from any source) than households in the control group and received a smaller amount. A mean difference between treated and control individuals, thus, may produce biased estimates, possibly in the direction of finding larger crowding-out effects given that there is a difference at baseline favoring control individuals. Even though we carefully control for observed household and municipality characteristics throughout our analysis, pre-existing differences in our outcomes and control variables motivate the use of an identification strategy that allows us to clean unobserved heterogeneity in order to identify unbiased estimates.

Statistical analysis

Our outcomes of interest are whether the household received private support and the amount of private support received from each source. As explained above, FA was not randomly assigned, therefore a simple difference in means would be biased if there were differences between treatment and control groups before the implementation of the program. Moreover, even after controlling for household and municipality characteristics we would have an omitted variable bias problem due to unobserved heterogeneity.

We take advantage of two features of the data to identify a causal effect. First, our sample consists of treated and control municipalities that were matched according to socioeconomic and demographic characteristics, which eases some concerns about the comparability between both groups (see Table 1, Panel A). Second, having baseline data allows us to follow a difference-in-differences (DD) approach, which controls for unobserved pre-existing differences (Imbens and Wooldridge, 2007). DD allows us to identify a causal effect under the assumption that there are no differences in time-variant characteristics (i.e., parallel trend assumption), which may be plausible given that municipalities were matched to be comparable in socioeconomic and demographic characteristics, and individuals for the evaluation were selected randomly within each municipality. Yet, we perform additional checks to reduce concerns about potential biases using matching techniques.

We begin by estimating probabilistic models by maximum likelihood to identify the effect of the program on the probability of receiving different types of private help from different sources. In Equation 1, $Help_{i,t}$ is the outcome of interest (which will vary across analyses) for household i in period t. The outcome of interest equals one if household i received τ help (τ=cash, in-kind, or labor) from s source (s=familiar or friend, or neighbor). We estimate a system of 12 equations for receiving help in each τ from each s and from receiving any help. Equation 1 presents our basic model, where FA_i is an indicator for being a program beneficiary, and T_t an indicator for follow-up. The coefficient of interest is β_3, which estimates the average impact of the program on receiving τ help from s source.

$$P(Help)_{\tau,s,i,t} = \beta_0 + \beta_1 FA_i + \beta_2 T_t + \beta_3 FA_i * T_t + X_{i,t}\gamma + M_{i,t}\varphi + \mu_i \quad (1)$$

Although the internal validity of DD estimators depends on the parallel trends assumption, we are not able to test it for lacking data on periods previous to the treatment[13]. Nonetheless, we also include a vector of baseline household characteristics ($X_{i,t}$), and municipality characteristics ($M_{i,t}$) to increase the plausibility of the identifying assumption by ensuring comparisons between households with similar pre-treatment characteristics, and to improve the efficiency of the model (Bernal and Peña, 2011). We include exclusively baseline characteristics, given that some post-treatment characteristics may be affected by treatment itself and thus the exogeneity condition may be violated. At the household level, we include monthly income, household head age, marital status, and education level, number of people living in the household (adults, and children between 0-6, 7-11, and 12-17 years old), a binary variable that equals one if a household member owns the house, number of bedrooms, main material of floors, walls, and roofs, ownership of assets, and access to basic services, such as electricity, water, and toilet. At the municipality level, we consider the quality of life index; population; presence of health, educational, and financial infrastructure; and fixed effects for urban/rural and for Colombia's main regions (Atlantic, Eastern, Central, and Pacific).

Furthermore, we employ a matched DD methodology that allows us to control for fixed heterogeneity and time-variant differences in observable characteristics. In doing so, we are better able to account for observed heterogeneity that may threaten the parallel trends assumption and the internal validity of our estimates. Particularly, we matched treated and control individuals based on baseline characteristics using entropy balancing (Hainmueller, 2012) and in order to test the sensitivity of results we used a more restrictive approach through Coarsened Exact Matching – CEM (Iacus *et al.*, 2011). We used exclusively baseline information, given that post-treatment characteristics may be endogenous (i.e., could be influenced by treatment itself).

Subsequently, we estimate different models by Ordinary Least Squares (OLS) to identify the effect of the program on the monetary value of different types of help received from different sources, and, particularly, the total value of transfers.[14] Equation 2 presents the basic model, where β_3 is our coefficient of interest.

$$Value_{\tau,s,i,t} = \beta_0 + \beta_1 FA_i + \beta_2 T_t + \beta_3 FA_i * T_t + X_{i,t}\gamma + M_{i,t}\varphi + \mu_i \qquad (2)$$

Additionally, we use a difference-in-difference-in-differences methodology (DDD) to identify heterogeneous treatment effects across sub-populations or, in other words, whether observed (crowding-in or crowding-out) effects differ according to beneficiaries' socioeconomic characteristics (Imbens and Wooldridge, 2007). Particularly, we assess whether the program had differential effects across geographic location (rural and urban areas), and across income quintiles. Let us denote H_i as the sub-populations of interest. Equation 3 presents the DDD probabilistic model, where β_7 captures heterogeneous treatment effects for the sub-population of interest.

$$\begin{aligned} P(Help)_{\tau,s,i,t} = \beta_0 + \beta_1 FA_i + \beta_2 T_t + \beta_3 H_i + \beta_4 FA_i * T_t + \beta_5 FA_i * H_i + \beta_6 T_t \\ * H_i + \beta_7 FA_i * T_t * H_i + X_{i,t}\gamma + M_{i,t}\varphi + \mu_i \end{aligned} \qquad (3)$$

Since we aim to identify short-term and middle-term impacts, we estimate our models comparing baseline with first follow-up data and baseline with second-follow-up data independently.

Results

Overall effect on the probability of receiving private transfers

Table 3 presents results for the effect of the CCT on the probability of receiving private support. We present marginal effects from probabilistic models' maximum likelihood estimation for the first follow-up (Columns 1, 2, and 3) and second follow-up (Columns 4, 5, and 6). We also present results without including covariates (Columns 1 and 4), including household and municipality characteristics (Columns 2 and 5), and including municipality fixed effects (Columns 3 and 6) to test the sensitivity of the results. We present results for each type of transfer (cash, in-kind, and labor) and from each source (any private source, neighbors, and family/friends). In addition, on the last panel, we present the overall effect for receiving any transfer from any private source and any help from neighbors or family/friends in the municipality.

For the first follow-up, we do not find evidence that the CCT crowds out cash and in-kind transfers or labor help from private sources. Conversely, we

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TABLE 3. Difference-in-differences regression on the probability of receiving private transfers

	First Follow-up			Second Follow-up		
	(1)	(2)	(3)	(4)	(5)	(6)
Cash						
From any private source	−0,005	−0.006	−0.007	0.040***	0.042***	0.042***
	(0.014)	(0.015)	(0.015)	(0.015)	(0.016)	(0.015)
From neighbors	−0.010	−0.009	−0.008	0.004	0.004	0.004
	(0.09)	(0.08)	(0.008)	(0.009)	(0.009)	(0.009)
From family or friends living in municipality	−0.005	−0.005	−0.006	0.007	0.006	0.006
	(0.009)	(0.009)	(0.008)	(0.009)	(0.009)	(0.008)
In kind						
From any private source	0.018	0.019	0.024	0.106***	0.110***	0.115***
	(0.017)	(0.019)	(0.019)	(0.018)	(0.019)	(0.019)
From neighbors	0.039**	0.042**	0.046***	0.077***	0.079***	0.083***
	(0.017)	(0.017)	(0.018)	(0.017)	(0.017)	(0.017)
From family or friends living in municipality	−0.012	−0.012	−0.017	0.030**	0.032**	0.028**
	(0.013)	(0.014)	(0.013)	(0.014)	(0.014)	(0.014)
Labor†						
From any private source	−0.001	−0.005	−0.004	0.035***	0.029***	0.029***
	(0.009)	(0.008)	(0.008)	(0.011)	(0.010)	(0.009)
From neighbors	−0.002	−0.004	−0.003	–	–	–
	(0.007)	(0.007)	(0.007)			
From family or friends living in municipality	0.004	0.002	0.002			
	(0.006)	(0.004)	(0.005)			
Summary: any help						
From any private source	0.017	0.018	0.026	0.100***	0.104***	0.101***
	(0.017)	(0.018)	(0.018)	(0.017)	(0.017)	(0.018)

TABLE 3. Continued

	First Follow-up			Second Follow-up		
	(1)	(2)	(3)	(4)	(5)	(6)
From neighbors	0.028^{*}	0.031^{*}	0.034^{*}	0.059^{***}	0.062^{***}	0.063^{***}
	(0.017)	(0.018)	(0.018)	(0.017)	(0.017)	(0.018)
From family or friends living in municipality	−0.015	−0.015	−0.018	0.038^{**}	0.040^{**}	0.037^{**}
	(0.015)	(0.015)	(0.015)	(0.016)	(0.016)	(0.016)
Control variables	No	Yes	Yes	No	Yes	Yes
Municipality fixed effects	No	No	Yes	No	No	Yes
Number of households	5,781	5,781	5,781	5,781	5,781	5,781

Notes:

1. Marginal effects from Probit model maximum likelihood estimation. Columns 1, 2, and 3 for first follow-up effects; Columns 4, 5, and 6 for second follow-up effects.
2. * Significant at 10%, ** significant at 5%, *** significant at 1%.
3. Robust standard errors in parentheses.
4. Control variables: at household level we include monthly income, household head age, marital status, and education level, number of people living in the household per age group, an indicator for ownership of the house, number of bedrooms, main material of floors, walls, and roofs, value of assets, access to basic services (electricity, water, and toilet). At municipality level, we include quality of life index, population, presence of health, educational, and financial infrastructure, fixed effects for urban/rural and region.

[†]Second follow-up survey does not include information regarding the source of non-paid labor, thus we were only able to identify whether each household received support in labor or not, but not its provenance.

identify a positive impact of the program on the probability of receiving in-kind help from neighbors: for treated individuals, the probability of receiving in-kind support (food, clothes, or other goods) from private sources increased by 4.6 percentage points. These results are robust to the inclusion of household and municipality level control variables and to the inclusion of municipality fixed-effects.

For the second follow-up, we find positive effects of the program on private support. First, beneficiaries were 4.2 percentage points more likely to receive cash transfers from any private source, 11.5 percentage points more likely to receive in-kind support, and 2.9 percentage points more likely to receive unpaid labor from neighbors, family, or friends. Overall, households in treatment municipalities were 10.1 percentage points more likely to receive any type of support from any private source and 6.3 and 3.7 percentage points more likely to receive help from neighbors and from family or friends in the municipality respectively. These effects represent an increase of 20.6% (overall), 25.2% (neighbors), and 21.2% (familiar or friends) compared to baseline levels. Moreover, the effects of the CCT for receiving in-kind support or in any type from neighbors, which were statistically significant at first follow-up, almost doubled at second follow-up, suggesting a larger effect. These findings are robust using different matched DD specifications (see Table A1, online appendix).

Overall effect on the value of private transfers

Table 4 presents results for the effect of the CCT on the monetary value of private support received. We follow the same structure presented in Table 3. At the first follow-up, FA increased support received in cash, in kind, and in unpaid labor from family and friends by 27,503 pesos (32 USD PPP). Taking into account all help received (inside or outside the municipality), the program increased the monetary value of private support by 46,812 pesos (54.8 USD PPP), which represents an increase of 32% compared to baseline levels.

The program had stronger effects on treated municipalities at the second follow-up. Although cash transfers from private sources did not increase, in-kind transfers increased by 12,338 pesos (14.4 USD PPP), and non-paid labor support increased by 16,026 pesos (18.8 USD PPP). Overall, private transfers from neighbors increased, on average, by 22,556 pesos (26.4 USD PPP), and from family or friends in the municipality by 12,882 pesos (15 USD PPP). Considering all types of help and sources, FA increased the value of support received by 53,969 pesos (63.2 USD PPP), representing an increase of 38% compared to baseline levels. We present results for a matched DD regression using the same specification in Table A2 (online appendix). The results from the DD and matched DD models show that estimates are robust to this more conservative specification.

TABLE 4. Difference-in-differences regression for the value of private transfers in the last 12 months (COP)

	First Follow-up			Second Follow-up		
	(1)	(2)	(3)	(4)	(5)	(6)
Cash						
From neighbors	21.40	90.46	141.73	2,118	2,185	2,228
	(3,219)	(3,217)	(3,225)	(2,820)	(2,820)	(2,834)
From family or friends living in municipality	6,186*	6,269*	6,322*	2,569	2,652	2,637
	(3,707)	(3,719)	(3,747)	(3,102)	(3,109)	(3,121)
In-kind						
From neighbors	3,375	3,458	3,483	12,335***	12,350***	12,338***
	(4,515)	(4,522)	(4,542)	(3,658)	(3,664)	(3,678)
From family or friends living in municipality	11,823***	11,914***	11,996***	4,915	4,840	4,811
	(4,589)	(4,597)	(4,631)	(3,145)	(3,147)	(3,159)
Labor†						
From neighbors	5,113	5,229	5,545	15,229***	15,435***	16,026***
	(3,532)	(3,549)	(3,587)	(3,358)	(3,382)	(3,423)
From family or friends living in municipality	14,443***	14,286***	14,731***			
	(4,517)	(4,526)	(4,565)			
Any help						
From neighbors	8,511	8,778	9,171	21,840***	22,092***	22,556***
	(6,837)	(6,851)	(6,890)	(5,348)	(5,360)	(5,399)
From family or friends living in municipality	27,340***	27,240***	27,503***	12,668**	12,758**	12,882**
	(6,801)	(6,812)	(6,865)	(4,988)	(4,999)	(5,028)
Total help received	45,944***	46,002***	46,812***	52,735***	53,271***	53,869***
	(10,731)	(10,718)	(10,780)	(8,993)	(9,012)	(9.063)
Control variables	No	Yes	Yes	No	Yes	Yes
Municipality fixed effects	No	No	Yes	No	No	Yes
Number of households	5,781	5,781	5,781	5,781	5,781	5,781

Notes:
1. Results from OLS estimation. Columns 1, 2, and 3 for first follow-up effects; Columns 4, 5, and 6 for second follow-up effects.
2. * Significant at 10%, ** significant at 5%, *** significant at 1%.
3. Robust standard errors in parentheses.
4. Average annual exchange rate of $1 USD= $2,275 COP.
5. All control variables stated in Table 3 are included.

†Second follow-up survey does not include information regarding the source of non-paid labor; thus we only could identify whether each household received support in labor or not and its value, but not its provenance.

Heterogeneous treatment effects

Table 5 summarizes results for treatment heterogeneous effects. We consider two basic sub-populations for the analysis: the poorest of the poorest households (poorest quintile in our sample), and households living in rural areas. Columns 1 and 4 present the overall effect for first and second follow-up respectively. Columns 2 and 5 present the additional effect for poorest households, and columns 3 and 6 the additional effect for households in rural areas.

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TABLE 5. Difference-in-difference-in-differences estimation for heterogeneous treatment effects

	First Follow-up			Second Follow-up		
	Overall effect	Poorest households†	Rural areas	Overall effect	Poorest households†	Rural areas
Type of private help						
Cash	−0,005	0.017	0.030	0.040***	0.053	0.086*
	(0.014)	(0.049)	(0.049)	(0.015)	(0.052)	(0.055)
In-kind	0.018	−0.044	−0.016	0.106***	0.110*	0.157**
	(0.017)	(0.062)	(0.065)	(0.018)	(0.060)	(0.062)
Labor	−0.001	0.005	−0.034	0.035***	0.006	−0.006
	(0.009)	(0.029)	(0.015)	(0.011)	(0.028)	(0.026)
Any help						
From any private source	0.017	−0.056	−0.036	0.100***	0.089	0.120**
	(0.017)	(0.061)	(0.062)	(0.017)	(0.057)	(0.057)
From neighbors	0.028*	−0.002	0.085	0.059***	0.078	0.097*
	(0.017)	(0.058)	(0.068)	(0.017)	(0.059)	(0.063)
From family or friends living in municipality	−0.015	−0.025	−0.050	0.038**	0.010	0.082
	(0.015)	(0.045)	(0.044)	(0.016)	(0.047)	(0.059)
Number of households	5,781	5,781	5,781	5,781	5,781	5,781

Notes:

1. Marginal effects from Probit model maximum likelihood estimation. Heterogeneous treatment effects.
2. Coefficients reported for heterogeneous effects refer to the triple interaction. The total increase in the probability of a sub-population is the sum between that coefficient and the overall effect.
3. *Significant at 10%, ** significant at 5%, *** significant at 1%.
4. Robust standard errors in parentheses.
5. All control variables stated in Table 3 are included.

†Poorest households refers to the first group when dividing households into income quintiles, that is, the poorest of the poorest households in the program.

We find no heterogeneous effects in the first follow-up, but FA's effect on the probability of in-kind private support is 11 percentage points larger for the poorest households and 15.7 percentage points larger for households in rural areas in the second follow-up. In addition, for households living in rural areas the effect of the public subsidy on receiving cash transfers from private sources is 8.6 percentage points larger than in rural areas.

Conclusions

A common concern in the design of public subsidies is the possibility of introducing non-desirable effects that can render public investments inefficient. One such possible effect is that public transfers might crowd out private transfers. This is particularly important in the case of CCTs, as they have become the most important social protection program in many developing countries, particularly in Latin America. These programs were created with the objective of alleviating poverty in the short term and increasing human capital in the long term, but if CCTs crowd out private transfers, the potential for poverty reduction in the short term is limited.

This paper finds that the Colombian CCT Families in Action not only had no crowding-out effects on private support, but that it actually had a crowding-in effect. Almost five years after the program was implemented, FA increased the cash transfers received by program beneficiaries from private sources by 4.2 percentage points, in-kind support by 11.5 percentage points, and unpaid labor support by 2.9 percentage points. Moreover, the program increased the total average value of private transfers received by households 38% at second follow-up compared to baseline levels. While these findings cannot be generalized to other CCT programs, they provide evidence of potential synergies that can be produced by social protection programs.

There are two main hypotheses that may help explain these results. One possible explanation, relying on the reciprocity model, is that family, friends, and neighbors provide support to beneficiary households in the expectation of future compensation. Another possible explanation is that the program helps to enhance collaboration and solidarity within communities. Both qualitative and quantitative evidence suggest that FA had a positive impact on social capital. Program staff and beneficiaries affirm that FA program features offered spaces that fostered fellowship and solidarity within communities (see Acción Social, 2010). The fact that crowding-in effects are observed mainly in the second follow-up (rather than the first), and that there was a substantial increase in participation in community activities between first and second follow-up, provides strong support for the second hypothesis.

The findings presented in this article can shed some light on the design of social protection programs, particularly CCTs. Complementary activities beyond the provision of the cash subsidy can produce a multiplier effect on reducing income poverty and increasing families' well-being. For instance, community activities that foster solidarity and reciprocity have the potential to boost the effects of public transfers. Future research should further examine specific components that can maximize CCTs' potential effectiveness.

This article focuses on the effect of one CCT program on a particular set of outcomes related to private support, limiting the generalization of the results regarding the overall benefits of CCTs on poverty alleviation and human development. Although there is systematic evidence on the positive effects of CCTs on educational outcomes in the short run (García and Saavedra, 2017), there is mixed evidence on the effects of CCTs on people's well-being in the long run (Baird *et al.*, 2019; Molina-Millán *et al.*, 2019). In addition, the feature of "conditionality" of CCTs is under debate as there are concerns about the potential harm that conditions can have by limiting the autonomy of disadvantaged populations (Curchin, 2019). Thus, a comprehensive assessment of the efficacy of CCTs would require the consideration of more countries and a larger set of outcomes, as well as a thorough examination of its different components, including the conditionality, the cash transfer amounts, and complementary activities with households and communities.

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Supplementary material

To view supplementary material for this article, please visit https://doi.org/10.1017/S0047279420000240

Notes

1 Nicaragua had a CCT program in operation between 2000 and 2006 (Social Protection Network).
2 Non-cash contributions such as food or clothing.
3 Equivalent to 25 to 30 cents for each PPP USD.
4 14,000 Colombian Pesos (COP). All monetary figures in the text are converted to 2002 PPP, using 2002 exchange rate of 854.61 for Colombian Peso (LCU per international dollar, World Development Indicators: https://data.worldbank.org/indicator/PA.NUS.PPP, retrieved on February 26th, 20220).
5 28,000 COP.
6 46,500 COP.

7 100,000 COP.
8 We conducted unstructured interviews with program staff who had been working since the beginning of the program in March 2016. In those interviews, we also were able to recover some material that was delivered to beneficiaries in the first phase of the program, including informational booklets and decks of informative cards.
9 Municipalities are the smallest administrative units in Colombia.
10 The impact evaluation design of FA and corresponding data collection was conducted by a consortium hired by the Colombian national government and supervised by the Colombian National Planning Department (DNP). The consortium was formed by the Instituted for Fiscal Studies (IFS), a Colombia research institute called Econometría and a data collection firm called SEI (see Gómez *et al.*, 2004 for baseline report and Attanasio *et al.*, 2010 for published results on the impact of FA on schooling and child labour). All data used in this study (baseline and follow-up surveys), without identification numbers of individuals, were publically available at DNP's website.
11 Belonging to the lowest level of SISBEN (System for the Selection of Beneficiaries of Social Programs), the household welfare index used by the Colombian government to target social programs to poor households. The index is a function of a set of household demographic characteristics and variables related to the consumption of durable goods, human capital endowments, and current income. This index is divided into 6 strata, with SISBEN 1 corresponding to extremely poor or indigent, SISBEN 2 to poor, and SISBEN 3 to near poor.
12 In 2002, the monthly minimum wage in Colombia was COP 309,000 (362 USD PPP).
13 Several factors make the parallel trends assumption likely to hold in this case. First, as part of the impact evaluation design, municipalities were matched in order to be similar in observed pre-treatment characteristics, which reduces the possibility that treated and control households will tend to have different trajectories in time (see Attanasio *et al.*, 2010). Second, we include a comprehensive set of covariates in our analyses, which seek to control for preexisting differences that may make the assumption implausible. Third, we employ a matched-DD approach, which reduces imbalances in observed characteristics between treatment and control groups, thus making it more likely that their trajectory in time will be similar.
14 For this analysis, we removed extreme values (which reached as high as 18,000 USD), representing 1% of our sample.

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