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CONDITIONAL CASH TRANSFERS FOR EDUCATION

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ABSTRACT

This chapter reviews the extensive literature to date on CCTs for education. Section 2 provides background on the origins and expansion of CCTs globally, and describes basic design features and variation in characteristics across programs. Section 3 presents a theory of change and an economic household decision-making model highlighting key comparative statics and empirical predictions for the introduction of an education CCT program. Section 4 discusses key methodological challenges in evaluating the impacts of education CCTs. Section 5 integrates and updates the extensive evidence to date on the impacts of education CCTs on various outcome domains over the life cycle, and provides the most comprehensive view to date on learning impacts by meta-analyzing new evidence from more than 30 studies—substantially more than prior reviews of the literature. Section 6 reviews the evidence on indirect and general equilibrium effects. Section 7 presents a simple model of costs commonly used in the literature, which we extend to analyze cost-effectiveness for a subset of programs. Section 8 concludes and highlights open questions for future research.

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A data appendix is available at http://www.nber.org/data-appendix/w29758

1. Introduction

Conditional cash transfers (CCT) are one of the most prevalent social assistance programs in low and middle-income countries today. In Latin America and the Caribbean, for example, there are currently 26 conditional cash transfer programs in operation (Garcia and Saavedra 2017), benefiting more than 135 million people (Stampini and Tornarolli 2012). Also, the use of CCTs is expanding quickly, as since 2008 the number of countries administering them has better than doubled and now exceeds 60.

In CCT programs, target households receive cash payments on the condition that they invest in the human capital of children. Health and nutrition conditions typically require medical visits and vaccinations for children less than 5 years of age, and attendance by mothers at periodic health information talks. Schooling conditions usually require enrollment, continued attendance, and, occasionally, some measure of performance (Fiszbein et al. 2009).

This chapter summarizes the extensive literature to date on CCTs for education, which began 20 years ago with landmark evaluations of Mexico's *Progresa* (later renamed *Oportunidades*, then *Prospera*), Bangladesh's *Female Secondary School Stipend Programme*, and *Bolsa Escola* in Brazil. The chapter begins by providing, in Section 2, background on the origins and expansion of this form of social assistance around the world and describing the program's basic design as well as variation in characteristics across countries and programs. CCTs are complex programs, and many decisions—typically made by in-country officials—determine their final shape and size. Some of these decisions include how much to pay families, how and when to pay them, how stringent the conditions for compliance should be, and who is eligible to participate. This context is important because prior reviews (e.g., Garcia and Saavedra 2017; Baird et al. 2014; Fiszbein and Schady 2009) argue that some aspect of the heterogeneity in program characteristics are associated with heterogeneity in educational program impacts, a question we examine closely later in the chapter.

The chapter continues with Section 3, which presents a theory of change and an economic household decision-making model highlighting key comparative statics and empirical predictions for the introduction of a CCT-for-education program (Skoufias 2005). Traditional approaches to poverty alleviation aim at eliminating—or at least mitigating—constraints faced by individuals and households, such as schooling costs, credit constraints, and access to formal insurance. A rather counterintuitive design aspect of CCTs in general—and for education, specifically—is their imposition of schooling conditions on beneficiaries. The economic argument for imposing conditions on recipients is threefold. First, households might underinvest in the human capital of their children because they do not internalize positive externalities associated with these investments. In the case of education, these externalities could include increased civic engagement, improved aggregate productivity, or reduced crime. Second, households may face informational constraints or principal-agent problems (Fiszbein and Schady 2009). For example, parents and children may hold biased beliefs about the returns and costs of schooling (Jensen 2010; Nguyen 2008; Attanasio and Kaufmann 2014). Parents also may act impatiently, discounting more heavily than they should the future returns on investing in their children. This may be particularly true for investments in girls' schooling because parents might overestimate the costs of raising girls in terms of dowries and help at home, or overestimate the benefits of boys providing long-term elderly care under the assumption that they are more likely to remain in close proximity in the future. Third, as the conceptual framework shows, conditions create substitution effects that alter the relative price of schooling vis-a-vis child work. These substitution effects reinforce income effects of the cash transfer itself, potentially strengthening schooling impacts relative to an unconditional transfer. The evidence shows substitution effects do seem to play an important role in strengthening the magnitude of educational impacts of CCT programs. However, the imposition of conditions on beneficiaries remains a controversial topic. Conditions may increase program administrative costs and complexity (Caldés, Coady, and Maluccio 2006), impose disproportionate costs on some household members—particularly women (Molyneux 2007), or be perceived as demeaning to the poor (Duflo 2012).

Section 4 discusses key methodological challenges in evaluating the impacts of CCTs for education. We focus, in particular, on five challenges. First, identification of CCT impacts on achievement, because compositional changes induced by increased school enrollment and attendance create endogeneity in standard test-score comparisons between treatment and comparison groups. Second, anticipation effects arising from the staggered rollout of CCTs, whereby comparison households alter their behavior in anticipation of future program receipt. Third, the loss of control groups over time in phase-in evaluation designs, which hinder identification of long-term effects. Fourth, the identification of income and, in particular, substitution effects as a justification of imposing conditions on beneficiaries. Fifth, general equilibrium effects on prices, wages, and government finances.

Section 5 reviews the extensive evidence to date on the impacts of CCTs for education. One novel contribution of this review is integrating and updating the evidence on various outcome domains, which previous systematic reviews have tended to analyze separately. We begin with the evidence on school enrollment and attendance (e.g., Rawlings and Rubio 2005; Fiszbein and Schady 2009; Baird et al. 2014; Snilstveit et al. 2015; Garcia and Saavedra 2017), which unanimously indicate that CCT programs, on average, improve these upstream educational outcomes, particularly in secondary schooling. We continue with a comprehensive review of the impacts of CCTs on achievement.

An additional novel contribution of this chapter is that it provides the most comprehensive view to date of the impact of education CCT programs on learning, as we are able to meta-analyze new evidence from more than 30 studies—substantially more than prior reviews of the literature (e.g., Baird et al. 2014; Snilstveit et al. 2015). Three main results emerge from this new evidence. First, there is substantial statistical heterogeneity in CCT program impacts on both math and language. This finding corroborates and strengthens one the strongest empirical regularities in the CCT literature: the substantial heterogeneity in impact estimates across all schooling outcomes. The second result—consistent with earlier reviews—is that we cannot reject that the overall meta-analytic impact estimate in this expanded sample of studies is statistically zero for both math and language. Third, particularly for math, we find positive correlations—although not statistically significant—between learning impact estimates and enrollment and attendance estimates. This provides support, albeit weak, for a key assumption in the conceptual framework that human capital accumulation is, in part, a function of time spent in school. Looking at schooling impacts of CCTs over the life cycle, as well as at long-term impacts on employment and earnings, our review of the evidence indicates CCTs, on the whole, improve educational attainment, including some evidence of increased access to tertiary education, despite heterogeneity across programs. The evidence on employment and earnings is more mixed, in part because most studies to date focus on employment and earnings outcomes when participants are in their late teens and twenties, a time when many haven't fully transitioned into the labor market (Molina-Millan et al. 2019). Future research over longer lifespans ultimately will shed light on whether these programs improve inter-generational mobility. We then summarize the evidence for CCTs' impacts on child labor, drawing on an extensive earlier review by De Hoop and Rosati (2014). As is the case with school enrollment and attendance, there is substantial heterogeneity in child labor impacts across CCT programs. De Hoop and Rosati estimate a strong and statistically significant correlation between school enrollment and child labor impact estimates, providing support for the existence of opportunity costs in children's time and showing how CCTs—by changing the relative price of schooling and child labor—reduce child labor. Among studies providing evidence for the effects of CCTs on other factors associated with long-term human capital accumulation, we find robust support that CCTs substantially reduce teen pregnancy, although they do not seem to affect the probability of early marriage.

The evidence on indirect and general equilibrium effects of CCTs provides mixed support for the existence of indirect schooling effects of CCTs within the household. On the other hand, the very limited evidence from *Progresa*, suggests strong indirect peer and neighborhood effects on secondary school enrollment of children, likely stemming from social interactions and information sharing. Available evidence on general equilibrium effects of CCTs suggests programs operating at scale may raise village-level consumption, although that increase may have negative unintended consequences by hiking the relative price of perishable protein sources and, as a result, children in those households experiencing increased stunting At the same time, available evidence seems to unambiguously suggest strong local labor market child wage effects as a result of child labor supply shock from the transfers increasing school enrollment. However, these child wage effects appear to have negligible feedback effects on the overall impact of CCT programs on school enrollment. Lastly, to the extent that CCT programs are financed by eliminating distortionary transfer programs such as agricultural subsidies, these programs can have a positive fiscal externality. One limitation of the available evidence on indirect and general equilibrium effects is that it is overwhelmingly drawn from Mexico's *Progresa* program. An important area of open future research is the existence of those types of general equilibrium effects in other contexts.

One important empirical question in the CCT literature is the extent to which program design matters (e.g., Barrera-Osorio et al. 2008; Barrerra-Osorio, Linden, and Saavedra 2019). On the whole, the available meta-analytic evidence (e.g., Garcia and Saavedra 2017; Snilstveit et al. 2015; Baird et al. 2014; Fiszbein and Schady 2009) finds mixed support for the predictions derived from the conceptual framework, seen in Section 3, regarding the association between program design characteristics and educational impact estimates.

The last outcome domain in our systematic review of the literature on CCTs for education concerns cost-effectiveness. To guide this analysis, we present in Section 7 a simple model of costs commonly used in the literature (e.g., Caldés, Coady, and Maluccio 2006; Caldés and Maluccio 2005), which permits computation of cost-efficiency estimates. We extend this basic model to allow for estimation of cost-effectiveness as in Garcia and Saavedra (2017). The evidence indicates substantial cost-effectiveness heterogeneity across CCT programs, which stems from heterogeneity in educational program impacts as well as from heterogeneity in program costs. Comparative cost-effectiveness across various educational interventions (e.g., Dhaliwal et al. 2013; Angrist et al. 2020) paint a rather grim picture of educational CCTs' cost-effectiveness in terms of improving schooling per dollar spent. Our review of the evidence suggests this conclusion is potentially misleading, given the wide heterogeneity in program and cost-effectiveness estimates across CCTs, and that some programs achieve important educational long-term impacts—such as increased tertiary enrollment, which will pay off well into the future. Therefore, we highlight another promising area for future research: cost-benefit analysis of CCTs for education that incorporate costs and benefits over the life cycle.

The chapter concludes in Section 8. From the perspective of CCTs as educational policy, the key assumption underlying these social assistance programs is that they improve human capital acquisition because they help households overcome demand-side constraints such as educational externalities, informational constraints, and opportunity costs of children's time. The evidence on the long-term impacts of CCTs does seem to provide some support for the role of CCTs in relaxing demand-side constraints faced by low-income households, although the lack of impacts on achievement points to possible supply-side constraints stemming from schools, resources, and teachers. To date, we know very little about what actually happens in schools with the introduction of a CCT program. This is one big open question and a promising area of future research to inform the debate on the relative merits of CCTs for poverty alleviation or as a potentially promising lever for education policy.

2. Conditional Cash Transfers as Education Policy

2.1 Background, precursors and expansion around the world

Conditional cash transfer programs emerged in the late 1990s, pioneered by the *Female Secondary School Stipend Programme* in Bangladesh, *Bolsa Escola* in Brazil, and *Progresa* in Mexico. CCTs were created with two main purposes: in the short term, alleviating poverty; and, over the long run, promoting human capital accumulation of the next generation, thereby contributing to break the intergenerational transmission of poverty (Grosh et al. 2008; Fiszbein and Schady 2009; Ibarrarán et al. 2017).

In many countries—particularly developing ones—CCTs are a key component of the social protection system. They provide non-contributory cash assistance and help integrate households with children into national social protection systems (Cecchini and Madariaga 2011). Although CCTs are mainly considered anti-poverty programs, they also can be viewed through the lens of education policy. In particular, CCTs require that school-aged children enroll in school, attend regularly, and, in some cases, reach standards of educational achievement.[1] As we elaborate in

[1] Many countries—particularly high-income countries—have conditional subsidy programs as part of their social protection system. In contrast to CCTs, most of these programs incorporate incentives that are negative rather than positive—typically because of families not meeting conditions (Medgyesi and Temesváry 2013). National programs using this approach include America's *Temporary Allowance for Needy Families* (*TANF*), Hungary's *Családi Pótlék* (*Family Support*), and Romania's *Money for High School*. Some authors label these types of programs "conditional cash transfers" because they provide a monetary incentive, conditioning payments to certain behavior related to human capital (Friedman et al. 2009; Medgyesi and Temesváry 2013). We exclude these programs from our review

Section 3, transfers and their conditionality on schooling together increase unearned income and reduce the marginal cost of child time—effects that reinforce each other and lead to increased child schooling.

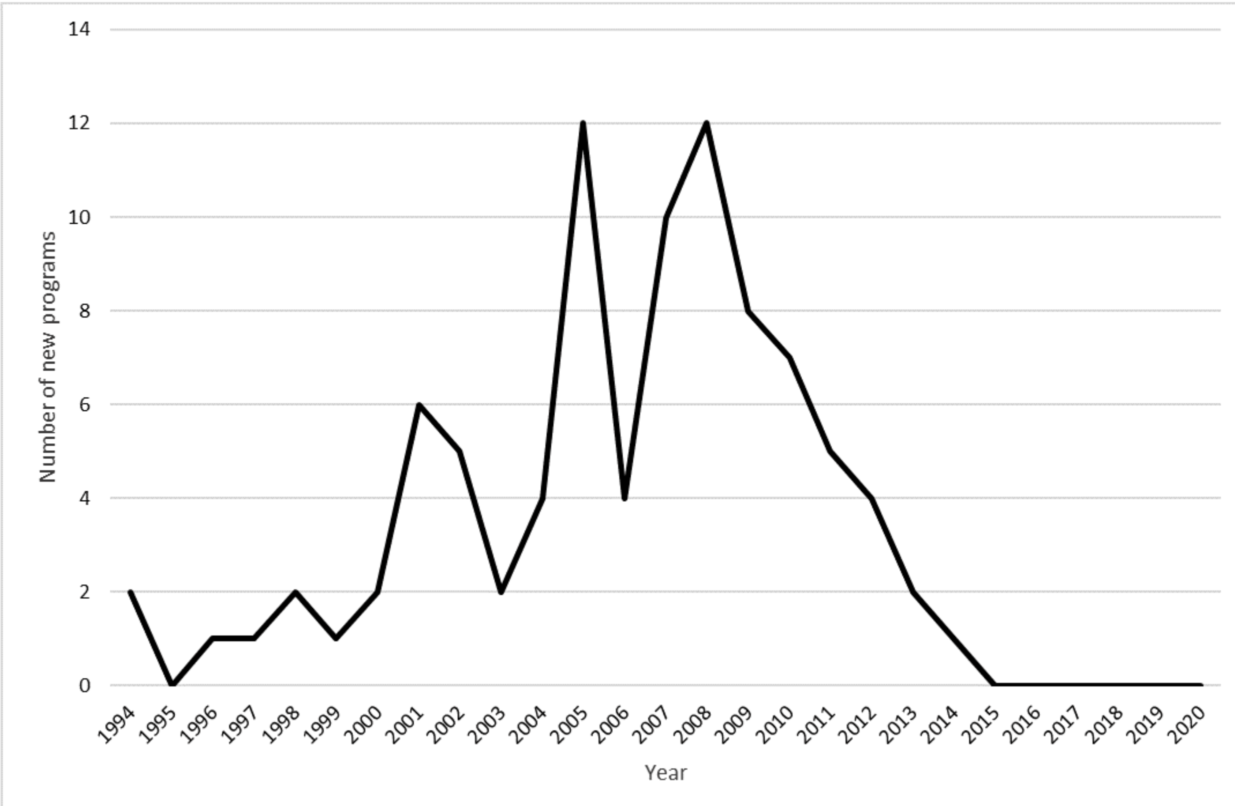
Throughout the chapter, we focus on CCTs that follow the basic structure of pioneer programs *Progresa* in Mexico and *Bolsa Familia* in Brazil. These programs satisfy the following criteria: 1) providing a cash subsidy; 2) targeted to poor households with children; 3) condition payments on children's schooling; and 4) conditions are framed as a positive incentive (a reward for meeting minimum schooling conditions) as opposed to a penalty or sanction.

CCT programs have expanded rapidly across the world over the last two decades: from three CCTs in 1997, to 32 in 2008 (Fiszbein and Schady 2009) and 50 in 2016 (Garcia and Saavedra 2017). Between 2004 and 2009, there was a major expansion of CCTs, but no new programs have been put in place since 2015 (see Figure 2.1).[2] As of 2020, we have identified 87 CCT programs in 50 countries. Latin American programs make up nearly half (44%), while 24% are from Asia, 17% from Africa, and 8% from North America (see Table 2.1). Most CCT programs (59%) are nationwide programs implemented at scale. These include Colombia's *Familias en Accion*, Brazil's *Bolsa Escola*, Nigeria's *Care for the Poor*, Mongolia's *Child Money Programme*, and Bangladesh's *Female Secondary School Stipend Programme*. A small number of CCTs in our sample (13%) are programs run by states or cities, such as New York City's *Opportunity NYC* in the United States, the Nigerian state of Kano's *Conditional Cash Transfer for Girls' Education*, and Buenos Aires' *Ciudadania Porteña Con Todo Derecho* program in Argentina. Meanwhile, 28% of CCTs (24 programs) are small-scale pilot demonstration projects, such as Colombia's *Subsidios Condicionados a la Asistencia Escolar*, Cambodia's -pilot *Primary School Scholarship Program (CSP)*, and Morocco's *Tayssir* (see Appendix Table A1 for detailed information).

because they condition behavior on a penalty or sanction, and oftentimes are not primarily focused on schooling of children.

[2] In fact, the pioneer PROGRESA (lately renamed Prospera) was abolished in 2019 (https://www.developmentpathways.co.uk/blog/the-demise-of-mexicos-prospera-programme-a-tragedy-foretold/ retrieved September 12, 2021).

Figure 2.1. Staggered expansion of CCT programs around the world



Notes: Figure shows the umber of new CCT programs per year for the period 1994-2020 according to the start year of the program. For details see Appendix Table A1.

2.2 Program characteristics: Basic program design and variation across programs

While the basic design of educational CCTs consists of a targeted cash subsidy attached to conditions for school-aged children's schooling behaviors, in general CCTs are complex programs and many decisions—typically made by in-country officials—determine their final shape and size. These decisions include how much to pay families, how and when to pay them, how stringent the conditions for compliance should be, and who is eligible to participate (Garcia and Saavedra 2017). In practice, there is substantial heterogeneity in program design (see Appendix Table A1 for details on every program included in our review).

Table 2.1 Characteristics of CCT programs

	n	%
Region		
Latin America and the Caribbean	39	42.4
Africa	12	13.0
Middle East and North Africa	3	3.3
East Asia and Pacific	12	13.0
South Asia	9	9.8
North America	12	13.0
Easter Europe and Central Asia	5	5.4
Scale		
Pilot	30	32.6
National	52	56.5
State/City/Region	10	10.9
CCT Type		
Primary only	14	15.2
Secondary only	22	23.9
Primary and secondary	56	60.9
Education conditionality requirements		
Enrollment	7	7.6
Attendance	21	22.8
Enrollment and attendance	30	32.6
Grade promotion or achievement	34	37.0
Household school subsidy varies by		
Number of eligible children	28	30.4
Age or grade and number of eligible	17	18.5
Gender & age/grade and number of eligible children	5	5.4
Achievement/completion and number of eligible children	17	18.5
Flat transfer per hh	21	22.8
Other	4	
Payment frequency		
Monthly	32	34.8
Bimonthly	22	23.9
Quarterly	13	14.1
Semesterly	5	5.4
Annually	5	5.4
Other	15	16.3

Supply incentives for education		
Yes	6	6.5
No	86	93.5
Number of programs	92	

Notes: Supply incentives refers to programs that are accompanied by a supply-side intervention such as school grants. See Appendix Table A1 for details on each program.

As Table 2.1 indicates, 60% of educational CCTs target school-aged children in both primary and secondary school, 17% of programs target primary-schoolers, and 23% target only children in secondary school. This categorization is important in many contexts because primary school enrollment is nearly universal, in which case transfers are effectively unconditional and thus do not affect the opportunity cost of children's time. In contrast, program conditions for secondary schooling are likely to affect the opportunity cost of children's time to the extent that secondary enrollment is not nearly as universal.

The basic type of schooling condition is minimum school attendance requirements—about 60% of programs condition payments on school attendance. However, there is substantial variation in minimum school attendance requirements. For example, in Bangladesh's *Female Secondary School Stipend Programme* and Malawi's *Zomba Cash Transfer Program*, students are required to attend at least 75% of school days during the reference payment period (e.g., quarterly), whereas in Cambodia's *Education Sector Support Project Scholarship Program (CESSP)* and *Japan Fund for Poverty Reduction (JFPR)*, students are required to attend 95% of school days during the reference payment period. More stringent school attendance conditions are likely to affect the opportunity cost of time for a greater proportion of targeted children, presumably resulting in stronger overall behavioral changes among beneficiaries.

Some CCT programs condition transfers on downstream educational outcomes such as satisfactory grade progression or graduation. In Cambodia's *CESSP* and *JFPR*, for example, in addition to enrollment and regular attendance, students must maintain a passing grade for households to receive payments. Similarly, Turkey's *Social Risk Mitigation Project* and Mexico's *Progresa* programs condition payments on regular attendance and on not repeating a grade more than once. Argentina's *Programa Nacional de Becas Estudiantiles (PNBE)* conditions payments on on-time grade progression. In Colombia's *Subsidios Condicionados a la Asistencia Escolar*, which targets secondary school children, one experimental variation incentivizes immediate tertiary enrollment after graduation.

Programs such as Bangladesh's *Female Secondary School Stipend Programme* and *Primary Education Stipend Project* require students to obtain minimum scores on annual examinations for households to receive payments. All but one of the seven CCT programs in North America require for payments that students obtain minimum achievement standards in the form of grades, test scores, or school completion.

Subsidy amounts vary significantly across programs. In principle, heterogeneity in subsidy amounts could map onto heterogeneity in outcomes, because if schooling is a normal good a bigger transfer will allow households to acquire more of it, all else constant. In programs such as the Philippines' *Pantawid Pamilyang Pilipino Program*, a compliant household is expected to receive about USD$2, whereas in Uruguay's *Plan de Atención Nacional a la Emergencia Social (PANES)* program, households receive, on average, USD$53. As Garcia and Saavedra (2017) noted, in 2015 the monthly average subsidy amount for a fully compliant family across CCT programs was about USD$19 per child enrolled in primary school and close to USD$23 per child enrolled in secondary school[3].

Payment structure also varies substantially across programs. In 24% of educational CCT programs, households receive a flat payment, regardless of the number of children. In 16% of programs, families receive payments for each participating child, so the only variation between households is due to their sizes. In 22% of programs, there is an attempt to vary payments according to children's opportunity cost of time, with greater transfer amounts for older children and children in more advanced grades where school dropout is more common, as well as greater amounts for girls relative to boys. For example, in the cases of Burkina Faso's *Orphans and Vulnerable Children* (also called *Nahouri Cash Transfer*) and Morocco's *Tayssir*, transfer amounts increase with age. In Indonesia's *Jaring Pengamanan Social (JPS)* and *Program Keluarga Harapan (Hopeful Family Programme)*, and Costa Rica's *Avancemos*, transfer amounts increase as a function of grade. In Turkey's *Social Risk Mitigation Project*, as well as Pakistan's *Benazir Income Support Program*, and Mexico's *Oportunidades* and *Progresa* programs, girls receive greater transfer amounts than boys at every age and grade. Furthermore, in education CCT programs imposing conditions on achievement, payments often are tied to reaching certain educational milestones. This is the case in India's *Apni Beti Apna Dhan (Our Daughter, Our Wealth)*, which transfers greater amounts if students remain unmarried, with bonus transfers for reaching grades 5 and 8. In Slovakia's *Motivation Allowance* and Chicago's pilot, transfer amounts vary by grade, and in New York City's *Opportunity NYC* program, students can receive additional monetary bonuses for achievement on standardized tests.

In the design of education CCTs, there also is variation in payment frequency. Some argue that if educational expenditures are lumpy and households face savings constraints, then larger although less-frequent payments can lead to greater schooling investments (e.g., Barrera-Osorio et al. 2011; Barrera-Osorio, Linden, and Saavedra 2019). While in 55% of education CCT programs households receive monthly payments, households receive quarterly payments in 14% of programs, and in 10% households receive only one or two payments per year. For example, in Yemen's *Basic Education Development Project*, and Bolivia's *Bono Juancito Pinto*, beneficiary households receive only one annual payment. In Argentina's *PNBE*, Bangladesh's *Female Secondary School Stipend Programme*, and Cambodia's *JFPR* and *CSP*, households receive two payments per year.

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[3] Average of a sample of 46 CCT programs that had an impact evaluation reporting effects on schooling outcomes. Average subsidy amounts reported in Garcia and Saavedra (2017) were in U.S. dollars of 2015. For comparability, they converted subsidy amounts from original currency to nominal dollars in the base year (the year the program began) using base-year exchange rates, then inflated them to dollars in the year of analysis (2015) using the U.S. Consumer Price Index.

A final important dimension of program design heterogeneity concerns the extent to which transfers are complemented with a supply-side subsidy or intervention. As we highlight in the next section, a key assumption behind the theory of change of CCTs for education is that relaxing demand-side constraints such as children's opportunity cost of time will result in increased human capital acquisition. However, in many contexts the supply of schooling—schools, teachers, textbooks—is insufficient. The idea behind introducing a supply-side component is to help schools accommodate the greater number of students expected to enroll because of transfers. In this context, six education CCT programs in our review complement the demand-side subsidy with a supply-side subsidy or intervention. For example, in Indonesia's *JPS* and Chile's *Chile Solidario*, schools receive block grants. In Nicaragua's *Red de Proteccion Social* (*RPS)*, cash transfers are given to the schools themselves, as well as to the teachers of beneficiary children. In Honduras' *Programa de Asignacion Familiar II (PRAF-II)*, parent-teacher associations (PTAs) receive block grants with the goal of using the resources to design and implement bottom-up school improvement plans. In Bangladesh's Female Secondary School Stipend, schools receive instructional materials, teacher training, and investments in school infrastructure (see Appendix Table A1 for details).

One important question in the literature concerns the extent to which variation in program characteristics maps onto heterogeneity in educational impact and cost-effectiveness estimates. In the following section, we present a theory of change for education CCT programs and a conceptual household decision-making framework. The framework, in particular, highlights how variation in program characteristics (e.g., minimum schooling attendance requirement, subsidy amount) predicts variation in program impact. In Section 5, we empirically validate these theoretically-derived predictions for the relationship between heterogeneity in program characteristics and heterogeneity in impact and cost-effectiveness estimates, following our earlier work (Garcia and Saavedra 2017).

3. Theory of change, a conceptual framework and empirical predictions

3.1 Theory of change

Conditional cash transfers make payments to target households on the condition that those households comply with schooling and health conditions aimed at incentivizing human capital accumulation of children. The notion behind this approach is that to alleviate poverty, transfer programs must blend short- and long-term objectives. Transfers supplement household income, enabling households to buy food and other necessities, improving short-term choices. With short-term needs met, conditions act as incentives for poor households to invest in children's human capital—namely, their education and health. In the long term, improved human capital of children helps break the intergenerational transmission of poverty (Reimers, DeShano da Silva, and Trevino 2006).

Attaching a condition to use educational services for receipt of payments changes the price, or opportunity cost, of using the service. In particular, by imposing schooling conditions based on enrollment and attendance, conditional transfer programs aim to reduce the opportunity cost of school enrollment and attendance relative to, for example, child labor. Because income and price effects operate in the same direction, economic theory predicts that imposing conditions should raise usage of educational services more than would an unconditional transfer, given that an unconditional transfer only has an income effect (Grosh et al. 2008; Fiszbein and Schady 2009; Garcia and Saavedra 2017).

The economic argument for imposing conditions on recipients is twofold. First, households might underinvest in the human capital of their children because they do not internalize positive externalities associated with these investments. In the case of education, these externalities could include increased civic engagement, improved aggregate productivity, and reduced crime.

Second, households may face informational constraints or principal-agent problems (Fiszbein and Schady 2009). For example, parents and children may hold biased beliefs about the returns and costs of schooling (Jensen 2010; Nguyen 2008; Attanasio and Kaufmann 2014). Parents may also act impatiently, discounting more heavily than they should the future returns on investing in their children. This may be particularly true for investments in girls' schooling, because parents might overestimate the costs of raising girls in terms of dowries and help at home or overestimate the benefits of boys providing long-term elderly care under the assumption they are more likely to live nearby in the future.

In addition to the economic rationale for imposing conditions on recipients, there are also political economy reasons. Conditions may facilitate political support for redistribution by incentivized socially desirable behaviors among the deserving poor, as opposed to unconditional transfers that may be seen as pure handouts (Grosh et al. 2008; Fiszbein and Schady 2009).

However, imposing conditions on beneficiaries remains controversial. For example, De Brauw and Hoddinott (2011) argued there are a number of concerns regarding the imposition of conditions on program participants. As the authors noted, conditions often increase program administrative costs and the complexity of running a cash transfer program. Meeting conditions also imposes costs on beneficiaries that are oftentimes unevenly shared within the household, falling disproportionately on women (Molyneux 2007). Additionally, imposing conditions and tracking compliance creates opportunities for corruption (Kidd 2019), and is demeaning to the poor as it implies they lack self-agency (Duflo 2012).

Abstracting from the controversy around conditionalities, from the perspective of CCTs as educational policy, the key assumption in the programs' theory of change is that these programs improve human capital acquisition because they help households overcome demand-side constraints: educational externalities, informational constraints, and within-household principal agent problems. To the extent this assumption is true, CCTs can improve educational achievement and attainment over the long run in ways that make the next generation less likely to be poor and, therefore, less dependent on government assistance. However, if underinvestment in education stems from supply-side factors, such as lack of school infrastructure, poorly trained teachers, or inadequate curriculum and instructional materials, then providing demand-side monetary incentives is unlikely to result in improved educational attainment and achievement (Reimers, DeShano da Silva, and Trevino 2006).

3.2 A conceptual framework

This section formalizes, into a simple economic framework, key elements of the theory of change behind CCTs for education. The framework is drawn from Skoufias (2005). Providing similar formulations are Skoufias and Parker (2001), and Fiszbein and Schady (2009).

Setup
Skoufias (2005) assumed households have full information and collapse all decisions into one period. The model also assumes unitary preferences within the household, so that principal-agent problems between parents and children are non-existent. These are strong assumptions, no doubt, but imposing them helps shed light on crucial mechanisms through which CCTs influence household behavior and investments in children's education.

Household's production of education for each child (H) depends on time inputs of family members (child time t^c and parental time t^m), as well as on other educational goods X purchased from the market (e.g., books). Skoufias (2005) allows the production of H to depend exogenously on child characteristics such as ability, health endowments, and birth order, plus characteristics of the parents and community. Without loss of generality, in our presentation of the model, we abstract away from such exogenous factors influencing the production of H:

$$H = h\ (t^c, t^m, X) \qquad (3.1)$$

With $\partial h / \partial t^c, \partial h / \partial t^m, \partial h / \partial X > 0; \partial^2 h / \partial t^{c\,2}, \partial^2 h / \partial t^{m\,2}, \partial^2 h / \partial X^2 < 0$. Income of an adult child (E) is the stock of human capital a child accumulates, rewarded at the market return on accumulated human capital (β):

$$E = \beta H = \beta h\ (t^c, t^m, X) \qquad (3.2)$$

The household's budget constraint allows the possibility of unearned income, children's contribution to household income when not engaged in school, and parents receiving a portion of children's adult earnings:

$$V + N\,w^c\,(\Omega - t^c) + w^m\,(\Omega - Nt^m) + \theta\,NE = NpX + Y \qquad (3.3)$$

Where V is unearned income; N is number of children in the household; w^c and w^m are, respectively, the wage rates of children and parents; Ω is time available; θ is the portion of children's adult earnings that parents receive; p is the price of other market goods X; and Y is household consumption, excluding the purchased goods for human capital acquisition, and is assumed to be the numeraire good.

Parents care about the earnings of adult children and consumption:

$$U = u\ (E, Y) \qquad (3.4)$$

With $\partial u / \partial E, \partial u / \partial Y > 0$ and $\partial^2 u / \partial E^2, \partial^2 u / \partial Y^2 < 0$

Household optimization problem and optimality conditions
Household's optimization problem is:

$$\max_{t^c,\, t^m,\, X} \; u\,(E, Y)$$

subject to: $V + N\, w^c\, (\Omega - t^c) + w^m\, (\Omega - Nt^m) + \theta\, N\, \beta h\, (t^c, t^m, X) = NpX + Y$

The first-order conditions of the household's optimization problem are:

$$MRS_{EY} \equiv \frac{u_E}{u_Y} = N\left\{\frac{w^c}{\beta h_{t^c}} - \theta\right\} \equiv MC_{t^c} \qquad (3.5)$$

$$MRS_{EY} \equiv \frac{u_E}{u_Y} = N\left\{\frac{w^m}{\beta h_{t^m}} - \theta\right\} \equiv MC_{t^m} \qquad (3.6)$$

$$MRS_{EY} \equiv \frac{u_E}{u_Y} = N\left\{\frac{p}{\beta h_x} - \theta\right\} \equiv MC_x \qquad (3.7)$$

Where $u. = \partial u/\partial.; h. = \partial h/\partial.$

These first-order conditions indicate that, at the optimum, households equate the marginal rate of substitution between the earnings of children as adults and household consumption with the marginal cost (or shadow price) of investments in a child's human capital. At the same time, they indicate households will allocate child time, parental time, and consumption of other goods that influence human capital so as to equalize the marginal costs associated with each.

Condition (3.5) indicates the marginal cost of children's time (MC_{t^c}) depends positively on the opportunity cost of schooling, given by the market wage of child labor when outside school (w^c). Similarly, the marginal cost of investing in child human capital increases with the number of children in the household, all else equal. Condition (3.6) further indicates that the marginal cost of parental time in human capital production of a child increases with the opportunity cost of parental time, given by their market wage, and decreases with their productivity in the production of child human capital.

Changes in unearned income (V)—e.g., an unconditional transfer—do not affect the shadow price of child time, parental time, or other goods used in the production of human capital (as V does not enter directly into the first-order conditions). Therefore, as long as E and Y are normal good, increases in unearned income result in pure income effects that increase both the production of child human capital as well as overall household consumption.

Comparative statics for the introduction of a conditional cash transfer
Unlike an unconditional cash transfer, a conditional cash transfer changes the shadow price of resources that go into the production of child human capital (assuming eligible households are better off participating than not, and, thus, choose to take part in the program). Conditions

requiring school enrollment and minimum levels of attendance have a cost in terms of potential lost child wages (w^c), so what matters for household decision-making in equilibrium is how conditions affect the relative price of present versus future earnings of the child ($w^c / \beta h_{t^c}$). Given that $\partial h / \partial t^c > 0$, if school attendance conditions are binding, they will reduce the marginal cost of child time and lead to increased time devoted by the child to school. Presumably, this effect is further enhanced if conditions also increase maternal time devoted to children's schooling, and to households demanding more educational goods (X).

Figure 3.1, reproduced from Skoufias (2005), illustrates graphically these comparative statics. The vertical axis of the graph represents the quantity of other goods available for consumption in the household (Y), and the horizontal axis represents a child's time devoted to school (t^c). Excluding leisure time, which for simplicity is assumed to be fixed, a child can devote up to T units of time to schooling ($S=T$) or to work ($S=0$). In the absence of a conditional cash transfer program, if a child spends all of her time in school, a household can consume V units of other goods (as the price of other goods is the numeraire and V is unearned income) and, thus, household's choice set in the absence of the program is given by TVA. The slope of the budget constraint is the market wage of child labor.

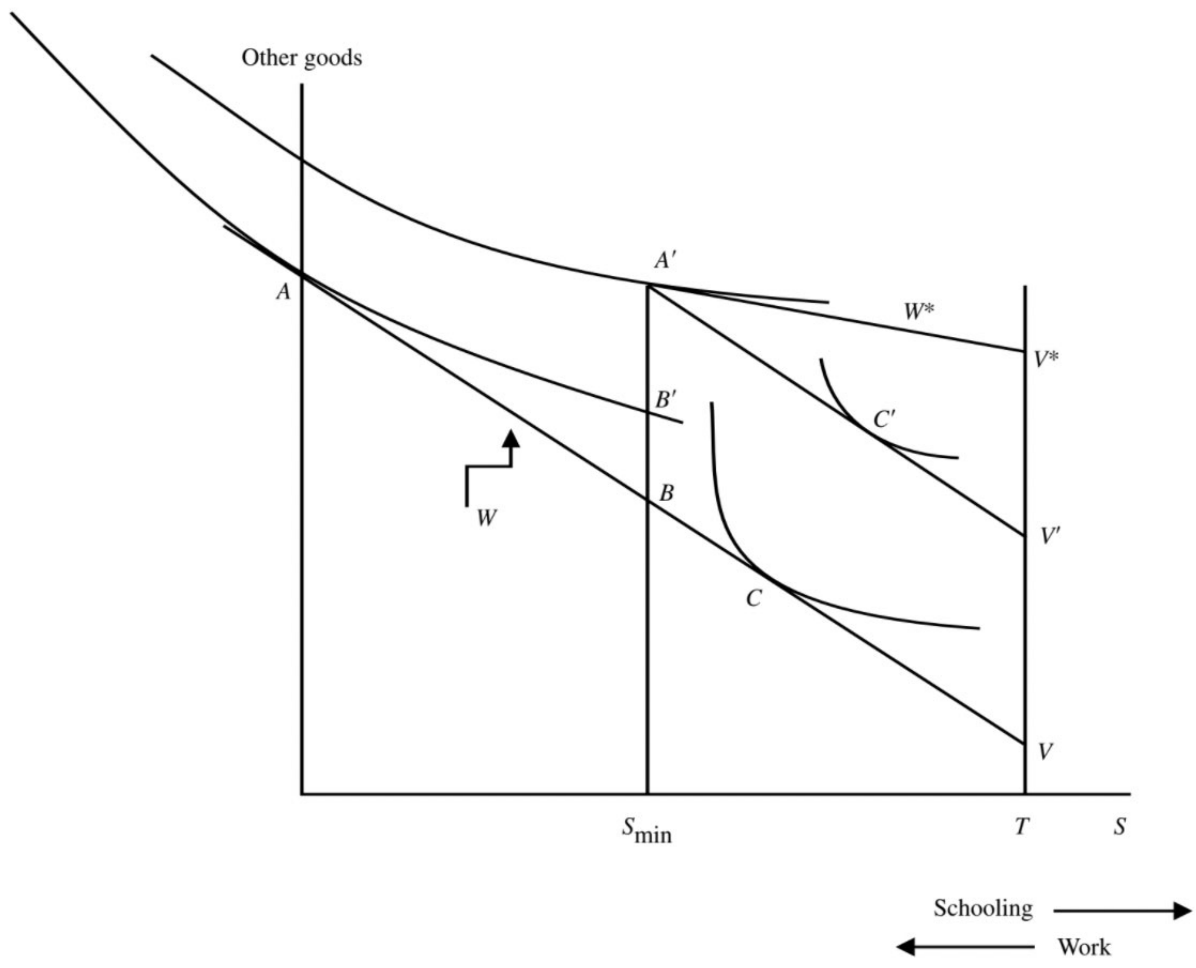
Let S_{min} denote the program's minimum attendance requirement. Program eligibility causes a parallel shift in the budget line between points S_{min} and T, as it increases unearned income to V'; then, $V' - V$ is the maximum amount of program benefits. The new choice set is $TV'A'BA$ that is discontinuous at the point S_{min}.

For a household like C, in which a child already is attending school beyond the minimum requirement, S_{min} program conditions are not binding, and the program likely will only have an income effect.

Now consider a household like A. For simplicity, Skoufias (2005) assumes a tangency point between the indifference curve of the household and budget constraint. For household A, the vertical difference of $B' - B$ represents the minimum cash transfer that will make household A just indifferent between complying with the S_{min} attendance requirement and keeping their child out of school. The figure assumes that the total amount of transfer ($V' - V$) exceeds $B' - B$ so that household A participates in the program—but, empirically, this need not be so for a subset of households. Participation in the program moves household A to A'. Skoufias (2005) demonstrates how income and substitution effects are at play for household A by linearizing the budget constraint, which, in effect, transforms point A' into a tangency point by drawing a line tangent to the indifference curve at A' (i.e., finding the shadow wage W^*) and finding the corresponding level of non-earned income (or shadow income) V^* that corresponds to the shadow wage W^*. Linearizing the budget constraint for A shows how participation in the program results in both substitution and income effects that tend to reinforce each other. The cash-transfer component of the program creates an income effect that increases schooling. At point A', the lower shadow wage W^* $(<W)$ implies a lower opportunity cost of schooling: Households have to give up less consumption of other goods for every additional unit of time the child spends in school. Thus, the substitution effect reinforces the income effect of the transfer.

Note the crucial implicit assumption in this framework is given by equation (3.2), which implies human capital production is only a function of household inputs. In other words, the income of an adult child can be enhanced simply by devoting more time in school and by purchasing more educational goods. However, as the theory of change notes, if underinvestment in education is the result of supply-side factors (lack of school infrastructure, poorly trained teachers, or inadequate curriculum and instructional materials), then providing demand-side monetary incentives will increase short-term school attendance but is unlikely to result in improved long-term human capital acquisition (Reimers, DeShano da Silva, and Trevino). Ultimately, this is an empirical question, the evidence on which we examine in Section 5.

Figure 3.1. The theoretical effect of a conditional cash transfer on children's school attendance and work



Note: *A*, Initially not attending; *C*, initially attending full time; *T*, maximum amount of time available excluding leisure; S_{min}, program's required school attendance.

Figure reproduced from Skoufias (2005).

Empirical predictions

Garcia and Saavedra (2017) derived various empirical predictions based on Skoufias' (2005) basic model of educational CCTs (as outlined previously). First, CCTs, in the short run, should increase school attendance and reduce child labor. However, baseline differences in non-earned income (V) and market opportunities for children (w^c) across households could explain why, in the absence of a CCT program, some children are enrolled in school while others are not. For a household like C, with children with very high school attendance and very little work time, the conditionalities will not be binding. For this household, the program does not produce a substitution effect—only an income effect—so while impacts may result in increased time devoted to schooling, they likely will not affect enrollment or attendance. Such a household may be representative of those in settings with very high enrollment rates in the absence of the program; such is the case with primary schooling in many countries. This income-schooling gradient prediction also may stem from regional differences in participant households' income. Therefore, the model predicts that educational program impacts—particularly on school enrollment, attendance, and reductions in child labor—should be stronger in settings with low baseline enrollment levels, low levels of participants households' income, and excess school capacity in which supply constraints are not binding.

As noted earlier, compliance with CCT program conditions is likely to change the marginal cost of household investment in human capital. The condition of minimum school attendance, for example, affects the marginal cost of children's time in terms of lost wages, as well as their parents'. This substitution effect reinforces the income effect of the transfer in incentivizing greater production of human capital. Our second empirical prediction is, therefore, that more stringent attendance and achievement conditions lead to, all else constant, steeper reductions in the marginal cost of children and parent's time, resulting in extra time devoted to schooling activities (e.g., greater attendance or effort).

Third, under the key model assumption that human capital depends only on time devoted to schooling, increased school attendance should increase human capital acquisition—educational achievement and attainment. So, the model predicts stronger impacts on achievement and attainment for programs with stronger impacts on school enrollment and attendance. Similarly, because the model assumes child earnings as adults is a linear function of human capital, also predicted are greater long-term impacts on earnings for programs with stronger impacts on enrollment and attendance.

Fourth, in a single period model like Skoufias (2005), there is no distinction between increases in permanent income or transitory income. However, in a model with multiple time periods, standard consumption theory distinguishes between the expected behavioral effects from a rise in permanent income versus a rise in current income. Some national, well-established programs likely raise permanent income, while other programs in a pilot stage do not. Therefore, all else equal, programs raising permanent income should have larger educational impacts in the short and long run than those raising only transitory income.

Fifth, the basic framework thus far outlined assumes a household with unitary preferences. This implies the household is assumed to maximize a single welfare function, without accounting for potentially divergent preferences between the adult male and mother of the household, or between parents and children. Most evidence rejects the unitary household model in favor of an alternative collective bargaining household model (e.g., Doss 2013; Haddad, Hoddinott, and Alderman 1997). When the preferences of individual household members are allowed to differ, conflicts of interest may arise because, for instance, parents may value more than children's consumption over human capital investment (e.g., through higher discount rates). Alternatively, the mother's objectives may be more closely aligned with those of her children. This potential conflict of interest within household members serves as the justification for delivering transfers to mothers, as is common in many CCT programs. Therefore, the model under its unitary assumption predicts similar educational impacts regardless of which member of the household receives the transfer. To the extent that targeted households behave according to a bargaining model, and the mother's objectives are more closely aligned with those of children in the household, distributing transfers to mothers should lead, all else equal, to greater human capital investments in children.

The basic framework collapses household decision-making into a single period. Thus, it cannot shed light on intertemporal choices stemming from constraints in liquidity, credit, or savings. In the single-period model, for example, it makes no difference whether households receive a dollar-equivalent payment in monthly or bimonthly installments. In practice, though, households may face financial constraints that limit the optimal allocation of resources. For example, if households face savings constraints yet expenditures on goods and services contributing to human capital production are "lumpy," then the timing and frequency of payments may make a difference. Therefore, our sixth and final prediction is, all else being constant, if households face credit or savings constraints, payment structure and frequency will be related to CCT program impacts in investments in human capital production.

Section 4. Methodological issues in evaluating CCT programs

4.1 Identification of direct program effects on achievement

A major challenge in estimating CCTs' achievement impacts is selection bias due to changes in the composition of the schooling population as a result of the program. As seen through the lens of the theoretical model in the previous section, children in households like *C* in Figure 1 already are enrolled in school and meet minimum attendance conditions despite the absence of a CCT program. Yet for households like *A*, whose children would not attend school in the absence of a CCT program, the program—through a combination of income and substitution effects—induces school attendance. To the extent there are many households like *A* in any given context, a CCT program substantially changes school composition. Students attending school in response to being eligible and receiving transfers will likely differ from those who do not, both in observable and unobservable ways. For example, based on the model, they might differ in the availability of household resources such as unearned income (V), educational resources (such as maternal time t^m or educational inputs X), and/or in the portion of child earnings as adults that parents could expect to receive (captured in the model by introducing heterogeneity in θ). Some characteristics might be observable to researchers while others will be unobservable.

Randomization of CCT program benefits will ensure that, on average, beneficiaries and non-beneficiaries will be similar along observable and unobservable dimensions. But if a program induces inframarginal children to attend school, and students are only tested through schools as is customary, then comparing endline achievement outcomes between randomized beneficiaries and non-beneficiaries will produce a biased estimate of the program's true impact achievement, even if randomization has not been compromised. Assuming students are only tested in school, then the test-score difference among program beneficiaries and non-beneficiaries can be written as (individual subscripts omitted):

$$E[Y|T = 1, D = 1] - E[Y|T = 1, D = 0] = E[Y_1|T_1 = 1] - E[Y_0|T_0 = 1]$$

(4.1)

Where Y is observed test scores, T is test-taking status and D is randomization status. test scores Y are related to potential scores Y_j, $j = \{0, 1\}$ through the traditional potential outcomes equation (Angrist and Pischke 2009): $Y = Y_1 D + Y_0(1 - D)$. In other words, for those randomly selected to receive CCT benefits, we observe only their potential test scores under benefits condition, while for those randomly selected to not receive CCT benefits, we only observe their potential scores under no benefits condition—but we never see both potential outcomes for any single child.

Similarly, observed test-taking status T is related to potential test-taking status through $T = T_1 D + T_0(1 - D)$. For program beneficiaries, only Y_1 and T_1 are observed; for non-beneficiaries, only Y_0 and T_0 are observed. Adding and subtracting the counterfactual expectation $E[Y_1|T_0 = 1]$ to equation (4.1), we can rewrite it as:

$$E[Y|T = 1, D = 1] - E[Y|T = 1, D = 0] =$$
$$E[Y_1 - Y_0|T_0 = 1] + \{ E[Y_1|T_1 = 1] - E[Y_1|T_0 = 1] \} \qquad (4.2)$$

The first expectation in the right-hand side of the equality in (4.2) represents the causal effect of the CCT program on test scores for children who would take the achievement test regardless of whether they receive program benefits or not—which essentially means all those children who would attend school in the absence of the program. The expression inside {} is a bias term that stems from the fact that whether or not a student takes the test is affected by the program, because children are only tested in schools and the program induces some children to attend school who would not in its absence.

One approach in the literature to deal with selection bias in test-taking (e.g., Baez and Camacho 2011) is to (implicitly) assume that both taking the test (i.e., attending school) and potential test scores are positively affected by applicants' native talent (μ), part of which might be unobserved:

$$T = 1\{- \gamma + \alpha D + \lambda\mu \geq 0\} \qquad (4.3)$$
$$Y_1 = \beta_0 + \beta_1\mu + \varepsilon \qquad (4.4)$$

Where ε is a random shock, and $\alpha, \gamma, \lambda, \beta_1 > 0$. It follows that:

$$E[Y_1|T_1 = 1] = E[Y_1|Y_1 > \beta_1\left(\frac{\gamma-\alpha}{\lambda}\right) + \beta_0 + \varepsilon] \qquad (4.5)$$
$$E[Y_1|T_0 = 1] = E[Y_1|Y_1 > \beta_1\left(\frac{\gamma}{\lambda}\right) + \beta_0 + \varepsilon] \qquad (4.6)$$

The conditional expectations [(4.5) and (4.6)] state that children choose to take the test (i.e., enroll in school) if their potential scores are above a random threshold, which is higher for non-beneficiaries not receiving CCT program benefits. Given (4.5) and (4.6), $E[Y_1|T_1 = 1]$-$E[Y_1|T_0 = 1] \leq 0$. Under the assumption that native talent positively influences test scores and school enrollment, this result implies that a comparison of endline test scores between CCT beneficiaries and non-beneficiaries—even with uncompromised randomization of program benefits—would produce a lower-bound estimate of the true impact. To obtain an upper bound, some studies (e.g., Baez and Camacho 2011) use bounding techniques developed by Lee (2009) and Angrist, Bettinger, and Kremer (2006) that essentially trim the test-score distribution of beneficiaries by removing η of lowest-scoring beneficiaries, where η is the percent increase in school enrollment as a consequence of the CCT program.

Alternative approaches to addressing selection bias in testing aim to balance the distribution of observable characteristics of beneficiary and non-beneficiary children in school at endline. These approaches are akin to assuming, in the selection model shown previously, that test-taking and test scores are influenced only by observable variables (X), rather than the unobservable (μ). For example, Behrman, Sengupta and Todd (2000) reweighted observations to impose identical age and sex distributions of beneficiaries and non-beneficiaries tested in school. Heinrich (2007) extended this idea by matching beneficiaries and non-beneficiaries on a broad set of baseline covariates. Then, García and Hill (2010) used propensity-score methods to predict the probability of school attendance, and match beneficiaries and non-beneficiaries who would have been likely to be enrolled in school in the absence of the program.

The most convincing approaches to deal with selection bias in test-taking aim to eliminate altogether the conditioning of test-taking on school enrollment, typically by testing children at home (e.g., Baird, McIntosh, and Özler 2011).

4.2. Sequential phase-in: Anticipation effects

Logistical constraints, limits on resources, and administrative feasibility often imply that CCT programs implemented at scale (e.g., Mexico's *Progresa*, Colombia's *Familias en Accion*) are implemented through a sequential phase-in. For example, *Familias en Accion (Rural Version)* was announced as a national program in 2001 and began operations in some municipalities that year, while others had to wait until 2002. The announcement of the program could trigger a behavioral response ("anticipation effect") among eligible households in the late-entry group. Therefore, late-entry beneficiaries' baseline levels of school enrollment might reflect true enrollment in the absence of the program plus an anticipation effect in response to the program. Anticipation effects imply that, in the absence of the program, both enrollment levels and trends among beneficiaries and non-beneficiaries differ—creating a methodological challenge to identify program impacts akin to Ashenfelter's Dip (Ashenfelter 1978) in the job training literature.

One way to address—and assess—the presence of anticipation effects is to collect enrollment data for beneficiaries and non-beneficiaries *before* the program was announced, then construct a pre-announcement baseline. With such data, for example, Attanasio et al. (2010) estimate the following difference-in-differences model:

$$Y_{it} = \alpha_0 + \sum_{j=1}^{2} \alpha_{1j} 1(t = j) + \alpha_2 P + \alpha_3 A + \alpha_4 T + \Gamma' Z_{it} + \epsilon_{it} \quad (4.7)$$

For $t = 0, 1, 2$. The baseline period is $t = 0$, $t = 1$ is the first survey (preprogram for late-entry group and post-program for early-entry group), and $t = 2$ is the endline (postprogram for all treated areas). $1(t = j)$ denotes indicator variables for each time period, so that α_{1j} captures enrollment trends in the absence of the program. Y_{it} takes the value of 1 if child i is enrolled in school in period t and 0 otherwise; P equals 1 for late-treat = 1 or early-treat = 1, and 0 otherwise; A equals 1 for late-treat =1 and $t = 1$, and 0 otherwise; T equals 1 for ($P=1$ and $t=2$) or (early-treat = 1 and $t = 1$), and 0 otherwise; and Z_{it} is a set of preprogram characteristics for individuals, households, and areas. The coefficient α_3 measures the school enrollment anticipation effect for the late-treat group in period 1. The coefficient α_4 measures the post-program school enrollment effect. Here, we reproduce Table 5 from Attanasio et al. (2010), which shows anticipation effects and treatment effects on school enrollment in various subsamples.

Table 4.1. Estimates of program effects and anticipation effects from Colombia's *Familias en Accion* (*Rural Version)*

MARGINAL EFFECT OF PROGRAM ON SCHOOL ENROLLMENT AND ANTICIPATION EFFECTS, PROBIT MODEL

Probit Model	Rural 14–17	Rural 8–13	Urban 14–17	Urban 8–13
Treated (α_4)	.0662	.0282	.0470	.0140
	(.0232)**	(.0111)**	(.0123)**	(.0066)*
Anticipation (α_3)	.0631	.0149	.0300	.0242
	(.0291)**	(.0144)	(.0193)	(.0057)**
N	1,873	3,648	1,439	2,579

Table reproduced from Attanasio et al. (2010), Table 5.

As estimates in Table 4.1 indicate, anticipation effects can be quite important. For example, in the rural sample of children ages 14-17, anticipation effects are about 6 percentage points, or 11-12% from the early-treat group's base rate of 55% (not reported here, taken from Table 1 in Attanasio et al. 2010) and the late-treat group's 53%, and comparable in magnitude to program effects on enrollment. For children 8-13 residing in urban areas of small municipalities, anticipation effects are 2.4 percentage points, or about 3% from a base of 95% in the early-treat group and of 90% in the late-treat group—and point estimates are larger than the estimated program impact on school enrollment of 1.4 percentage points. For the other two groups shown in the table, anticipation effects also appear to be qualitatively important even though the estimates are not statistically significant.

Anticipation effects also could arise from *ex-ante* changes in behavior among households assigned to the control group, as Schady and Araujo (2008) documented for Ecuador's *Bono de Desarrollo Humano (BDH)*. This program selected beneficiaries using lotteries, and some lottery losers may have concluded that they were eligible for transfers on the basis of their score on a means-test used to pre-screen eligibility. If consumption is smoothed over time, these households may have altered schooling decisions in anticipation of future transfers. Insofar as this is the case, schooling effects from the treatment-control contrast would likely be lower-bound estimates of *BDH*'s impact.

4.3 Sequential phase-in: Loss of control group over time and estimation of long-term impacts

The sequential phase-in of beneficiaries and communities in many CCT programs often means that, even with initial randomized assignment of benefits, experimental control groups eventually receive benefits. To investigate long-term impacts of those programs, researchers often exploit the original randomization together with differences in timing and age-specific conditionality of program components to estimate differential program impacts of "early" versus "late" treatment (Molina-Millan et al. 2019). The experimental approach estimates the impact of differential exposure to CCTs. For example, as Molina-Millan et al. (2019) noted in their review of long-term impacts of CCTs, studies such as Behrman, Parker, and Todd (2009, 2011) examined long-term impacts of Mexico's *Progresa* by taking advantage of randomization and differential exposure to the program given that the 506 initially-eligible rural communities were assigned randomly to treatment (320) and delayed-entry control (186) groups. Eligible households in original treatment communities started receiving cash transfers in 1998, while those in the original control communities started receiving them approximately 18 months later. This difference in the length of exposure between early-entry and late-entry control groups was the cornerstone of their experimental evaluation, which provides differential, rather than absolute, program impacts.

Non-experimental evaluations of long-term impacts use a variety of methods, such as matching, regression discontinuity and difference-in-differences. Depending on the nature of the control group (i.e., sequential phase-in or never treated), non-experimental methods may identify differential exposure or absolute program impacts.

One methodological concern regarding the estimation and validity of long-term impacts is selective attrition. For example, original evaluation survey sample protocols in Mexico and other countries do not follow migrants. As Molina-Millan et al. (2019) noted, 40% of individuals ages 9–15 in *Progresa*'s 1997 baseline had attrited by 2003. Reweighting methods to replicate the baseline distribution of observable characteristics assume that attrition is uncorrelated with unobservable individual and household characteristics—which often is not the case. Sometimes, the use of administrative data can help overcome attrition concerns (e.g., Parker and Vogl 2021; Barrera-Osorio, Linden, and Saavedra 2019).

A second concern of non-experimental evaluations of CCTs' long-term impacts, as highlighted by Molina-Millan et al. (2019), is because these evaluations are retrospective in nature, they often lack pre-intervention baseline data for the non-experimental comparison group. To address this issue, some researchers rely on recall survey questions, which possibly introduces measurement error due to recall bias (e.g., Behrman, Parker, and Todd 2011). In addition, for CCT programs with purposive program placement, non-program communities will be different *a priori*, creating additional challenges for achieving balanced characteristics across treatment and comparison groups, particularly along unobservable dimensions.

A third concern with non-experimental evaluations of long-term effects of CCTs (which is also relevant for measuring outcomes in the short-term) relates specifically to the use of regression discontinuity designs. For example, Cambodia's *CESSP* and Colombia's *Familias en Accion (Urban Expansion)* initially targeted community eligibility using a single-index means-test of socioeconomic disadvantage (beneficiaries and comparison groups then are chosen within eligible communities/households). At first glance, this creates plausibly exogenous variation in absolute program eligibility; thus, potentially helping circumvent the issue of sequential phase-in in the long run. However, a key identifying assumption of the RDD framework is that only eligibility for the program at hand varies discontinuously at the cutoff. This condition is likely to be violated if other social programs use the exact cutoff value or a cutoff value close to that of the CCT, potentially confounding CCT program impacts with those of other social programs (e.g., Baez and Camacho 2011). Moreover, the use of a single cutoff or near-single cutoff to determine eligibility for social programs might create incentives to manipulate eligibility—further invalidating the key assumption in RDD designs of continuity in potential outcomes near the cutoff (e.g., Camacho and Conover 2011).

Molina-Millan et al. (2019) conclude by offering some words of caution about the use of administrative data for long-term evaluations of CCTs. While administrative data can help circumvent some concerns stemming from migration, attrition, and lack of baseline data for comparison groups, they present their own challenges. For instance, using administrative data requires having detailed individual identifiers at baseline that can be tracked across various data sources. Related, it is often difficult to ascertain, retrospectively through administrative data, location of residence at the program's start. Therefore, studies using spatial variation in program intensity might introduce selection bias if there is non-random migration across areas of varying program intensity (e.g., Parker and Vogl 2021). The outcomes that can be examined through administrative data—thus, the possibility for uncovering mechanisms at play—often is limited. For evaluations of CCTs that focus on learning outcomes, administrative test-score data might not be available at the student level (e.g., Garcia and Hill 2010) or not administered in all grades (e.g., Baez and Camacho 2011). These features create three challenges. First, test-score results might not generalize to students in other grades. Then, there may be different patterns of selection into different grade levels, exacerbating the issues stemming from selection into test-taking as noted previously. Lastly, with a cross-section of test scores, often it is impossible to distinguish the program's heterogeneous effects at different ages from differential effects by length of exposure. Consider the case of scores on a high-school graduation test (e.g., Baez and Camacho 2011). As Molina-Millan et al. (2019) suggested, beneficiaries enrolling in the program at older ages have fewer years of potential schooling before they graduate from high school, compared to younger beneficiaries. Therefore, in a cross-section of administrative high-school graduation tests, older beneficiaries will be more likely to take the test than younger beneficiaries. If this is the case, shorter exposure to the program could be incorrectly associated with greater secondary-school completion and greater test-taking rates.

4.4 Estimation of income and substitution effects

Leaving aside the political economy argument about conditions making redistribution more politically palatable to voters, the economic model in Section 3 highlights how CCTs have both income effects stemming from the cash component, as well as substitution effects stemming from the way conditions change the relative price of schooling when compared against child labor. The empirical prediction from the model is that income and substitution effects reinforce each other, increasing school attendance (and, thus, human capital) more than would be expected from an unconditional transfer of the same amount, all else equal. Therefore, an important question in the literature concerns the existence of such substitution effects.

One approach to estimate substitution effects—thus, the economic merit of conditions—is to take advantage of accidental pitfalls in program implementation (Baird, McIntosh, and Özler 2011). Schady and Araujo (2008) employed this approach to study the role of conditions in Ecuador's *Bono de Desarollo Humano*, a program implemented at scale. There, beneficiaries were selected randomly among a group of eligible households—but unlike most other cash transfer programs in Latin America, *BDH* did not explicitly make transfers conditional on schooling. Rather, program administrators informally stressed the importance of school enrollment when signing up households for transfers, and, for a brief period, *BDH* television spots explicitly discussed how parents were responsible for their children's schooling and health. However, some households believed there was an enrollment requirement associated with the program, even though no requirement was ever monitored or enforced. Exploiting this quirk in the administration of the program to assess the importance of conditions attached to cash transfers, Schady and Araujo (2008) compared outcomes of beneficiaries who believed transfers were conditional to those of beneficiaries who believed transfers were effectively unconditional. Eighteen months after program rollout, school enrollment of children in households who believed transfers were conditional increased by between 7-13 statistically-significant percentage points from a base rate of 74%—whereas for those who believed transfers were unconditional, enrollment increased by only 1-2 percentage points, a difference which was not statistically significant. These results suggest important substitution effects stemming from the conditionality. While the authors validated there were no baseline enrollment "effects" relative to control among beneficiaries who believed transfers were conditional and those who did not believe such, one plausible concern of this approach is that beliefs about conditions correlate with schooling investments.

De Brauw and Hoddinott (2011) employed a similar approach to investigate the role of conditions for the effect on school enrollment of Mexico's *Progresa*. Specifically, some beneficiaries who received transfers did not receive the forms for school attendance monitoring while others did. For beneficiaries who did not receive these forms, transfers could not have been conditioned—because teachers would not have had reason to monitor the attendance of children in these families. For beneficiaries who did receive the forms, though, transfers are conditioned. De Brauw and Hoddinott (2011) argued that failure to receive school-monitoring forms is uncorrelated with households or community-level unobservables. However, as the authors noted, it is possible that household members who understood the conditions might assume that the program was somehow monitoring them, rendering forms unnecessary—thus, implying the comparison of beneficiaries who did and did not receive forms would not be a true test of conditionality. To address this concern, the authors developed a complementary test based on asking beneficiary households to list the conditions they were required to fulfill for receiving the transfer. Using this information, the authors created an additional comparison group based on households who both did not receive the form and did not know they were required to send their children to school to receive the transfer. In this group, transfers clearly were unconditional. With these various comparison groups, as well as a variety of econometric techniques including nearest-neighbor matching, and household-fixed effects regressions, De Brauw and Hoddinott (2011) show that, relative to households who did not receive school attendance forms (i.e., unconditioned transfers), those receiving these forms (i.e., conditioned transfers) were about 4-7 percentage points more likely to enroll children grades 3-8 in school, from an average base enrollment rate grades 3-8 for the unconditioned group of 84%. Moreover, the authors suggested the effect of conditions is most pronounced when children are transitioning into lower secondary school.

A second approach to estimate substitution effects stemming from schooling conditions employs structural estimation methods. Bourguignon, Ferreira, and Leite (2003) used structural estimation to simulate the impact of conditions (i.e., substitution effects) in Brazil's *Bolsa Escola* program. They began with a simple unitary household model, where S_i is a qualitative variable representing the occupational choice of a child in household i. This variable takes the value 0 if the child does not attend school, 1 if the child goes to school and works outside the household, and 2 if the child goes to school but does not work outside the household. The authors modeled S_i using standard utility-maximizing interpretations of the multinomial logit framework:

$$S_i = k \; iff \; S_k(A_i, X_i, H_i; Y_{-i} + y_{ik}) + v_{ik} > S_j(A_i, X_i, H_i; Y_{-i} + y_{ij}) + v_{ij} \; for \; j \neq k \quad (4.8)$$

where $S_k()$ is a latent function reflecting the net utility of choosing alternative $k = 0, 1 \; or \; 2$; A_i is the age of child i; X_i are child characteristics; H_i are household characteristics (such as size, age of parents, education of parents, presence of other children at school age, distance from school); Y_{-i} is total household income net of the contribution of child i; and y_{ij} is the contribution of child i towards household income, depending on the child's occupational choice j. v_{ij} is a random shock capturing unobserved heterogeneity of observed schooling/labor behavior. If all non-income explanatory variables are collapsed into a single vector (Z_i) and linearized, and child i's earnings contribution assumed to be δw_i, Bourguignon, Ferreira and Leite (2003) showed that (4.8) can be written as:

$$U_i(j) = S_j(A_i, X_i, H_i; Y_{-i} + y_{ij}) + v_{ij} = Z_i\gamma_j + Y_{-i}\alpha_j + \beta_j w_i + v_{ij} \quad (4.9)$$

Where $\beta_j = \alpha_j \delta$.

Introducing a *Bolsa Escola* (*BE)* conditional schooling transfer, (4.9) becomes:

$$U_i(j) = Z_i\gamma_j + (Y_{-i} + BE_{ij})\alpha_j + \beta_j w_i + v_{ij} \; with \; BE_{i0} = 0, \; BE_{i1} = BE_{i2} = T \quad (4.10)$$

The fact that BE_{ij} is available only for schooling choices 1 and 2 makes it conditional. Bourguignon, Ferreira and Leite (2003) used reduced-form equation (4.10) to simulate counterfactual policy experiments, including the role of conditions (i.e., substitution effects). Their results indicate the conditional requirement to enroll in school for receiving the benefit—rather than the pure income effect from the transfer—is the primary cause of the extra demand for schooling among beneficiaries.

Todd and Wolpin (2006) estimated and validated a dynamic behavioral model of parental decision about fertility and schooling, to evaluate the effects of *Progresa* under a variety of counterfactual conditions. While their full model is richer and more complex than that of

Bourguignon, Ferreira and Leite (2003), the intuition and set-up is similar. To illustrate the basic intuition, Todd and Wolpin (2006) considered a household with one child making a single-period decision about whether to send the child to school or to work. Utility of the household is separable in consumption (C) and school attendance (S), $u = C + (\alpha + \epsilon)S$, where $S=1$ if the child attends school and 0 otherwise, and ϵ is a preference shock normally distributed with mean zero and variance σ^2. The family's income is $y + w(1 - S)$, where y is the parents' income and w is the child's earnings, if working. Under utility maximization, the family chooses to have the child attend school if and only if $\epsilon > w - \alpha$. The unknown parameters of the model are, thus, α and σ. In this model, as Todd and Wolpin (2006) explained, the probability that family i's child attends school is $1 - F_{\epsilon}((w_i - \alpha)/\sigma)$. Identification of α and σ requires observable variation in child wages among families.

A CCT program in this model is equivalent to the introduction of a subsidy to parents, of amount T, if they send their child to school. Under such a program, household income is now $y + w(1 - S) + TS$, while the effect of the program on the probability that a household sends the child to school is $F_{\epsilon}((w_i - \alpha)/\sigma) - F_{\epsilon}((w_i - \alpha - b)/\sigma)$. As this expression indicates, knowledge of α and σ, estimated as shown without the program (i.e., using data from the control group), is required to forecast the impact of the program under various counterfactual scenarios. Thus, serving as a substitute for direct variation in the tuition cost of schooling (the negative of b) is variation in the opportunity cost of attending school—that is, the child market wage. As the authors noted, the magnitude of the effect of the subsidy depends on the size of the subsidy, the child wage level, and the strength and variability in parental preferences about child schooling. The full version of the model recasts this basic idea in a dynamic programming framework and requires a numerical solution, which the authors obtained by backwards recursion. Todd and Wolpin's (2006) model captures well various features of the data, including nearly-universal primary school attendance, declining attendance in secondary, children's gender-specific choices of work for pay versus staying at home, and, importantly, predicted effects of the subsidy (by comparing to actual experimental estimates), particularly among girls.

Armed with the parameter estimates from this model, the authors estimated the impact of an unconditional transfer program (i.e., only income effects) awarding families an amount close to the maximum they can receive under the conditional program. While this unconditional subsidy represents a roughly 50% increase in household income, an unconditional transfer increases schooling by an amount that is only about 20% as large as the conditioned subsidy, suggesting sizable substitution effects.

Similar in spirit to that of Todd and Wolpin (2006), Attanasio, Meghir, and Santiago (2012) estimated a dynamic choice model with *Progresa* data. While their explicit goal was not to model the role of conditions, they provided an indirect test for the role of conditionalities—and, thus, for the presence of substitution effects. Transfers for elementary school children in *Progresa* are effectively unconditional, as below grade 6 almost all children go to school. Both experimental and structural estimates for this age group suggest negligible impacts on school enrollment, consistent with small income effects. For secondary school-aged children, impacts are substantially larger—a finding replicated across the literature (Garcia and Saavedra 2017)—which is consistent with large substitution effects on this age group induced by conditions. Consistent with the presence of large substitution effects for secondary school children, Attanasio, Meghir, and Santiago (2012) thus suggested that a revenue-neutral change in program design—increasing transfer size for secondary school children while eliminating it for primary school children—would have substantially larger effects on enrollment rates for secondary school children yet only minor detrimental effects among primary school children.

A third approach to estimate income and substitution effects uses experimental variation in the presence of conditions. Baird, McIntosh, and Özler (2011) directly assess the role of conditions in cash transfer programs using experimental variation in a pilot transfer program targeting adolescent girls in Malawi. The *Zomba Cash Transfer Program* featured two distinct interventions: unconditional transfers (UCT arm) and transfers conditional on school attendance (CCT arm). Two years into the program, the authors found a modest decline in dropout rates in the unconditioned arm relative to control, which is consistent with modest income effects. However, the dropout impact in the conditioned arm was 2.5 times larger, consistent with large substitution effects. Moreover, in tests of reading comprehension, girls in the conditioned arm also outperformed, on average, those in the unconditioned arm.

A final approach to estimate income and substitution effects in cash transfer programs uses meta-analytic techniques leveraging a large number of studies across the world. Baird et al. (2014) conducted a systematic review to help inform the debate about the role of conditions. Using data from 75 reports covering 35 different CCT programs, the authors found that, in the aggregate, effect sizes for enrollment and attendance are always larger for conditional than unconditional transfers, even though the difference across the two is not statistically significant. However, when programs are categorized—no schooling conditions, some conditions with minimal monitoring and enforcement, and explicit conditions that are monitored and enforced—a much clearer pattern emerges. Programs that are explicitly conditional, monitor compliance, and penalize non-compliance have substantively larger effects (60% improvement in odds of enrollment relative to explicitly unconditioned programs). This meta-analytic evidence is consistent with evidence from other analytic approaches, highlighting the role of conditions and, thus, of substitution effects (as in the model in Section 3) in reinforcing pure income effects from the transfer.

The weight of the evidence for the importance of conditions in CCTs, notwithstanding, sometimes conditions in these programs have unintended consequences. Barrera-Osorio, Linden, and Saavedra (2019) analyzed the long-term impacts of three CCTs for secondary schooling in Bogota, Colombia, and found that conditions directly incentivizing on-time tertiary enrollment increases enrollment only in low-quality colleges.

4.5 Estimation of general equilibrium effects of CCTs

If a CCT program increases household income for a large share of beneficiaries within a local market, the program could have a general equilibrium effect in the local market by raising prices for normal goods via greater demand. Cunha, de Giorgi, and Jayachandran (2018) and Filmer et al. (2021) developed a simple framework for this type of general equilibrium effects of cash transfers. The key intuition is that in a large integrated economy, a demand shift in either one or a handful of local areas should not significantly affect prices, as the local demand increase is too small to influence aggregate demand. However, if markets are not integrated, then the local market structure determines whether local demand changes affect local prices. In village economies, as these studies noted, supply may be constrained due to transport costs for imported goods or oligopolistic production markets. Filmer et al. (2021), for example, considered a Cournot model with N producers of a homogenous good. Total quantity demanded is:

$$Q = Q(p, X) \qquad (4.11)$$

Where p is price and X is a demand shifter, such as village-level income. Filmer et al. (2021) showed that if demand is additive in X, such as: $Q=g(p)+X$, then the Cournot-Nash solution for an exogenous change in X is:

$$\frac{\partial p}{\partial X} = \frac{1}{N(\frac{\partial g(.)}{\partial p})} > 0 \qquad (4.12)$$

So for any normal good, under the assumption of an additive shifter, a demand shift will raise the price. The magnitude of the increase depends on the shape of the demand curve and the number of producers in the market (which, in turn, is partly a function of the fixed cost of entry for potential competitors). To test this prediction empirically, Filmer et al. (2021) used village-level randomization of the Philippines' *Pantawid Pamilyang Pilipino Program* to estimate its impact on beneficiary households' food budget shares, child nutrition, education, and food intake. Because the program's experimental design targets some of the poorest and most remote parts of the country, the setting allowed the authors to analyze the impact of cash on local markets largely not integrated with the national or regional production base. After establishing a demand increase for nutritious foods among beneficiary households, the authors compared prices of various goods in treated and control villages.

A similar logic applies to general equilibrium effects of CCTs in local labor markets. An exogenous decrease in the supply of labor—because of increased school enrollment and attendance of children otherwise laboring—shifts the labor supply curve inward, resulting in higher equilibrium wages (assuming a downward-sloping labor demand curve). If there are barriers to migration across local labor markets, these wage increases might be long-lasting. Attanasio, Meghir, and Santiago (2012) implemented this idea using a dynamic education choice model and experimental data from Mexico's *Progresa*. The authors assumed production involves the use of adult and child labor, as well as other inputs, and that the elasticity of substitution between the two types of labor is given by ρ ($\rho > 0$). Crucially, the authors also assumed the price of labor is determined in the local labor market. In equilibrium, the price of a unit of child labor in a locality is:

$$log\ w_{child} = \frac{\rho+\gamma_{adult}}{\rho+\gamma_{child}} log\ w_{adult} - \left[\frac{1}{\rho+\gamma_{child}} log(\frac{L_{child}}{L_{adult}}) + \kappa\right] \qquad (4.13)$$

where, $\gamma_k > 0$ ($k = adult, child$) are adult and child labor supply elasticities, respectively, and L_k ($k = adult, child$) represent the level of labor supply of each group in the village; the child labor supply elasticity is assumed to be larger than the adult one. Attanasio, Meghir, and Santiago (2012) further assumed that adult agricultural wage levels are a sufficient statistic for the local area's overall level of demand for goods. As these correlate with the price of human capital, they provide the necessary exclusion restriction to identify the wage effect in the education choice model. The term in square brackets in equation (4.13) is unobserved and reflects preferences for labor supply and technology (ρ and κ). These will be correlated with the agricultural wage through the determination of local equilibrium. To identify the model, the authors assumed that in the absence of a CCT program L_{child}/L_{adult}, as well as technological parameters, are constant across localities. A CCT program increases school enrollment, which shifts L_{child} inward, resulting in a general equilibrium effect of increased child wages.

Other authors use a macro approach to estimate general equilibrium effects of CCTs. For example, Cespedes (2014) embedded a *Progresa*-style CCT program in a dynamic general equilibrium neoclassical growth model with dynastic overlapping generations and heterogeneous agents, then modeled parent and child labor supply decisions. As is the case in previous models discussed, a cash transfer expands households' budget sets, and the income effect may affect the allocation of resources within the household. Similar to other structural approaches, Cespedes (2014) also modeled schooling choice, as well as human capital accumulation over the life cycle of the household members. In Cespedes' (2014) model economy, the government has incentives to promote schooling of the population, as schooling has a positive externality that affects workers' productivity. Finally, Cespedes (2014) assumed flexible wages and interest rates to capture price changes induced by the conditional transfers described earlier.

Coady and Harris (2001, 2004) used similar computable general equilibrium models to analyze the general equilibrium effects of CCTs, with a particular focus on fiscal externalities, given that these programs are often financed through domestic taxation. As the authors noted, part of the domestic financing for *Progresa* came from the elimination of inefficient and distortionary food subsidies. Coady and Harris (2004) simulated an economy made up of households, firms, and the government. They assumed firms maximize profits, subject to constant returns to scale production functions, so that supply is demand determined, profits are zero, and producer prices are fixed. The authors framed the problem in terms of the standard trade-off between consumer welfare and government revenue, showing that such formulation has a general equilibrium interpretation. Specifically, when commodity and factor markets are balanced (i.e., supply equals demand), then so too is the government budget and vice versa. Therefore, the authors noted, policy reforms that balance the budget also will balance commodity and factor markets. Under this framework, the authors showed that the welfare impact of a CCT financed by indirect (e.g., commodity) taxes is the sum of a redistribution impact that is expected to be positive if those receiving transfers benefit and are on average more deserving (i.e., have higher welfare weights) than those financing the program; a reallocation effect arising from the income effects of the tax change, and a distortion effect stemming from using distortionary taxes to finance the transfers. The final two terms capture the general equilibrium efficiency implications of taxes used to finance the program. As the authors noted, if the efficiency effects are negative (for example, through transfers resulting in a switch in consumption toward subsidized commodities and/or the government introducing distortionary taxes to balance the budget), then a CCT program generates a negative fiscal externality because the taxes to finance the program must be higher than in the absence of the program.

5. Empirical evidence on the impacts of CCT programs on human capital over the life cycle

This section describes the evidence for CCT programs' impacts on human capital over the life cycle. Subsection 5.1 discusses the impacts on school enrollment, attendance, and dropout. Subsection 5.2 presents a detailed description of the impacts on learning with some new meta-analytic results. Subsection 5.3 summarizes the impacts on long-term educational outcomes: school attainment, school completion, and advancement to tertiary education. Subsection, 5.4 discusses impacts on child labor. Subsection 5.5 discusses evidence on the association between program characteristics and schooling impacts of CCTs. Subsection 5.6 summarizes evidence of CCT impacts on long-term human capital outcomes related to employment and earnings. Subsection 5.7 summarizes the evidence on teen pregnancy and early marriage.

5.1 Impacts of CCTs on school enrollment, attendance and dropout

a. Enrollment

CCTs consistently have demonstrated positive and significant effects on enrollment, particularly for secondary school (Fiszbein and Schady 2009; Baird et al. 2014). Garcia and Saavedra (2017) meta-analyzed 48 studies that estimated the impact of CCTs on school enrollment in low- and middle-income countries, and found that, on average, the meta-analytic random-effects primary school enrollment impact estimate is 3 percentage points, which represents an enrollment increase of 3.4% relative to the average primary enrollment rate in the sample of 87%. The average impact on secondary enrollment is substantially higher: 7.1 percentage points, representing a 14% increase relative to average baseline secondary enrollment of about 50%. This result is one of the most robust findings in the CCT literature. One interpretation consistent with this finding is that in many contexts, primary school enrollment is nearly universal, implying that primary school transfers effectively are unconditional. As noted earlier, an unconditional transfer only has an income effect on a household's budget. In contrast, secondary school enrollment is nowhere near universal—particularly in the developing world. Transfers for secondary schooling, therefore, also change the opportunity cost of children's time, which creates a substitution effect that reinforces the income effect of the transfer alone—thus, leading to greater changes in schooling behavior.

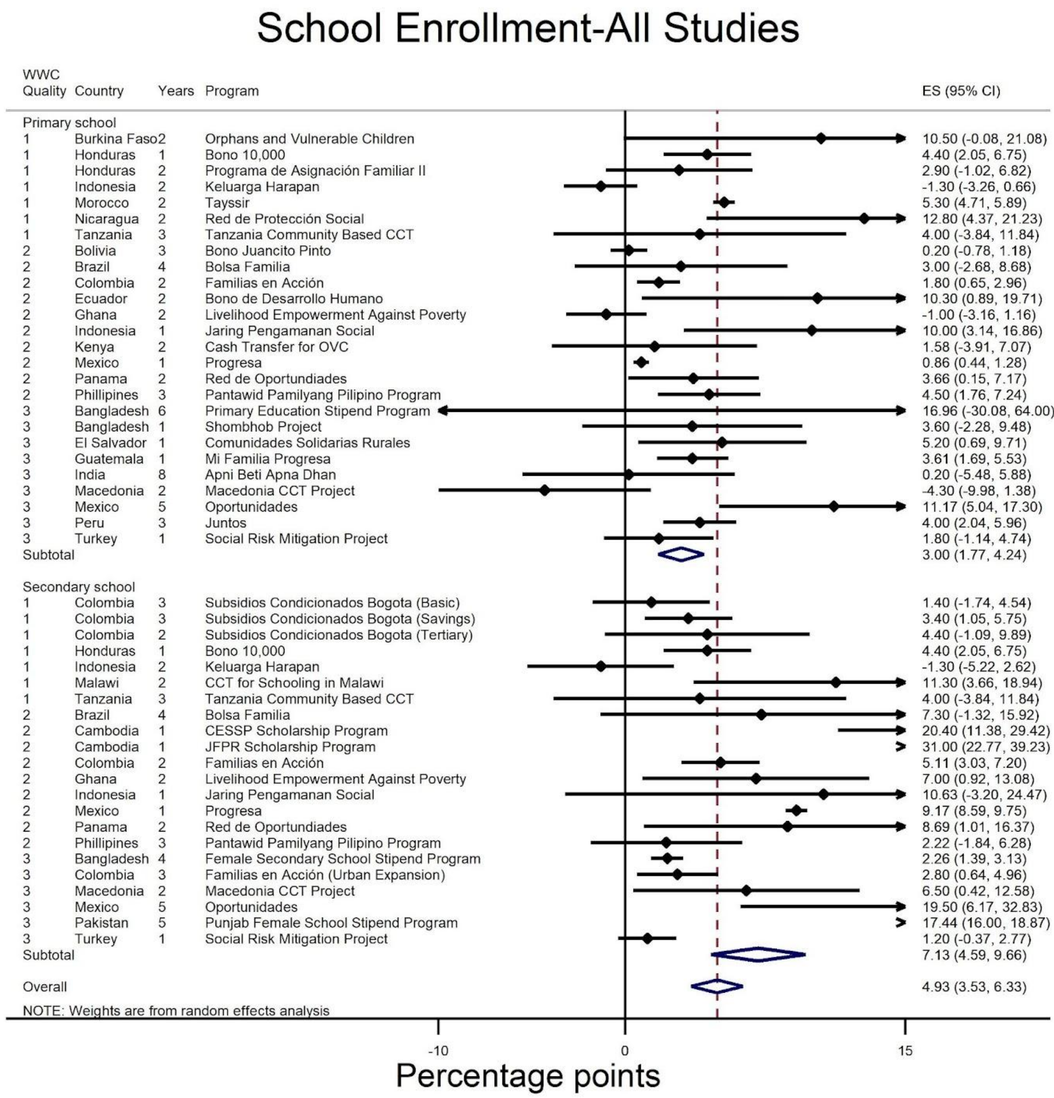
Another robust finding in the literature is that there is substantial heterogeneity in enrollment effect sizes across programs, even conditional on schooling level (Garcia and Saavedra 2017; Snilstveit et al. 2015; Baird et al. 2014). Figure 5.1 presents average effect sizes for primary and secondary enrollment of the CCTs in low- and middle-income countries included in Garcia and Saavedra (2017). Of 26 studies estimating effects on primary school enrollment, half reported positive and statistically significant effects, 10 reported positive effects that are not statistically significant, and only three studies found negative, although not-statistically significant, effects (Honduras's *PRAF-II*, Ghana's *Livelihood Empowerment Against Poverty,* and Macedonia's *Conditional Cash Transfer for Secondary School Education*). Four programs stand out as they had impact estimates on primary enrollment of 10 percentage points or more: Nicaragua's *RPS* (12.8 percentage points after two years of program exposure from a base enrollment rate of

78%); Mexico's *Oportunidades* (11.2 percentage points after five years of program exposure from a base enrollment rate of 96.5%); Ecuador's *BDH* (10.3 percentage points after two years of program exposure from a baseline enrollment rate of 97%); and Indonesia's *JPS* (10 percentage points after one year from a baseline enrollment rate of 94%). Here it is interesting to note that both Nicaragua's *RPS* and Indonesia's *JPS* provide a supply-side incentive through monetary transfers to schools.

Out of 22 studies measuring effects on secondary enrollment included in the meta-analysis by Garcia and Saavedra (2017), 14 studies (64%) reported positive and statistically significant effect estimates, seven reported positive but not-statistically significant effect estimates, and only one study (on Indonesia's *Keluarga Harapan*) reported a negative, yet not-statistically significant, effect estimate. The largest effects come from Cambodia's *JFPR* and *CESSP*, with impact estimates on secondary school enrollment of, respectively, 31 and 20.4 percentage points (from a base enrollment rate of 24%), followed by Macedonia's *Conditional Cash Transfer for Secondary School Education* (19.5 percentage points from a base enrollment rate of 78%) and Mexico's *Oportunidades* (17.4 percentage points from a base enrollment rate of 59%). Three of these condition payments on school achievement in addition to attendance: The Cambodian programs condition payments on maintaining a passing grade, while *Oportunidades* conditions payments on completion of middle school and not repeating a grade more than twice.

As Garcia and Saavedra (2017) suggested, whether an evaluation of an educational CCT uses a randomized controlled trial research design or an observational treatment-comparison approach is, all else constant, unrelated to the magnitude of school enrollment impact estimates. This finding highlights the fact that, among the set of studies the authors considered, existing evaluations of education CCT programs using treatment-control contrasts are, in general, methodologically rigorous.

Figure 5.1. Forest plot of impact estimates for primary and secondary enrollment of education CCTs in low- and middle-income countries



Notes. Figure reproduced from García and Saavedra (2017). For each program, the authors plotted the effect size estimate and 95% confidence interval for the lengthiest time of program exposure reported. The overall mean effect size is from a standard intercept-only random-effects model. Years is the time of program exposure. WWC quality is study quality according to Institute for Educational Sciences' What Works Clearinghouse (WWC) version 2.1 Evidence Standards Protocol (Institute of Educational Sciences 2011) [1=meets evidence standards, 2= meets evidence standards with reservations, 3=does not meet evidence standards]. See details in García and Saavedra (2017)

As part of the review for this chapter, we updated the literature search and found 17 additional studies examining impacts of CCT's programs on school enrollment that were not included in Garcia and Saavedra (2017), either because they were published after 2015—cut date for their meta-analysis—or because they reported effects on CCTs in high-income countries, which Garcia and Saavedra (2017) explicitly excluded (see Appendix Table A2 for details). Nine papers reported medium- or long-run effects on school enrollment of various education CCT programs, with six also reporting positive and statistically significant effects 4-10 years after program implementation: Colombia's *Subsidios Condicionados a la Asistencia Escolar*, savings treatment modality[4] (Barrera-Osorio et al. 2008); Mexico's *Prospera* (Behrman et al. 2019); Indonesia's *Keluarga Harapan* (Cahyadi et al. 2020); the Philippines' *Pantawid Pamilyang Pilipino Program* (Catubig and Villano 2017); Morocco's *Tayssir* (Gazeaud and Ricard 2021); and Bolivia's *Bono Juancito Pinto* (Niño-Zarazúa 2019).

The largest medium-term effect on enrollment is reported for Mexico's *Prospera*. Behrman et al. (2019) used matching techniques and administrative data to estimate the effect of *Prospera* on secondary enrollment after two to six years of program exposure. The authors find that the program increased secondary enrollment by 7.6 percentage points, 9.1 percentage points, and 4.6 percentage points after two, four and six years respectively[5].

In this recent set of papers, most medium-term impact estimates on secondary school enrollment range between 3.5 and 5 percentage points. Barrera-Osorio, Linden, and Saavedra (2019), for example, combined administrative data with data for the program *Subsidios Condicionados a la Asistencia Escolar* in Bogota, Colombia. The authors found that 8-10 years after program exposure, only the savings treatment—in which families had to save a portion of the transfers until the next school year—had a positive and significant effect on secondary enrollment of 3.5 percentage points (from a base enrollment rate of 65%). The other two versions of transfer structure—one with a traditional bimonthly payment and another incentivizing tertiary education enrollment—had no long-term effects on school enrollment. Barham et al. (2018) used the randomized phase-in design of Nicaragua's *RPS* to estimate effects on enrollment among boys aged 9-12 when the program started. Consistent with earlier findings for this program from Maluccio and Flores (2005), they found a positive and significant absolute effect on school enrollment of 18 percentage points from a base of 79% after two years of program implementation. Going on to estimate differential effects (comparing early versus late treatment groups), they then found a non-statistically significant effect on school enrollment after four years of program implementation, but a statistically significant differential effect of 4.5 percentage points after 10 years of program implementation. Also using a randomized controlled trial research design, Cahyadi et al. (2020), found a 4 percentage point increase in secondary school enrollment after six years in Indonesia's *Keluarga Harapan* (from a base enrollment rate of 90%). For Bolivia's *Bono Juancito Pinto*, Niño-Zarazúa (2019) used a methodology of difference-in-differences and found a positive effect on secondary school enrollment of 5.2 percentage points after seven years of program implementation (from a base enrollment rate of 92%). For Morocco's *Tayssir*, Gazeaud and Ricard (2021) used a regression discontinuity design to find a 4.5 percentage points increase in secondary school enrollment 5-10 years after program

[4] In the savings treatment modality, households of secondary school children in Bogota, Colombia are forced to save a third of the transfer amount until they enroll in the following school year.
[5] No information is provided on the time of exposure to the program before 6th grade.

exposure (from a base enrollment rate of 64%). Ikira and Ezzrari (2021) used a matching approach to find a statistically significant 7.5 percentage points increase in school enrollment of *Tayssir* among children aged 6-15 years old two to five years into the program (from a base enrollment rate of 87%).

The smallest medium-term effect for enrollment is for the Philippines' *Pantawid Pamilyang Pilipino Program*. Catubig and Villano (2017) used difference-in-differences to find a statistically significant 1.1 percentage point increase in combined primary and secondary school enrollment four years after program exposure (authors do not disaggregate impacts by schooling level). For Ecuador's *Bono de Desarollo Humano*, Araujo, Bosch and Schady (2019) used a regression discontinuity design to measure the effect on school enrollment 10 years after program exposure, reporting a small and not-statistically significant effect estimate of 0.5 percentage points[6]. Similarly, for El Salvador's *Comunidades Solidarias Rurales*, Sanchez-Chico et al. (2018) used a regression discontinuity design and found a statistically insignificant 1.4 percentage point increase in primary school enrollment for six-year-old children 5 to 6 years after program exposure. They find, however, a statistically significant 12.3 percentage point increase in preschool enrollment of five-year-old children relative to a base rate of 42%. According to the authors, this stark difference in effect sizes among both age groups is explained by a substantial increase of national enrollment rates for this age group over time, leaving less room for improvement.

There are five new education CCT programs previously not included in Garcia and Saavedra (2017): India's *Kanyashree Prakalpa*; Chile's *Chile Solidario*; Buenos Aires, Argentina's *Ciudadania Porteña*, China's CCT pilot project[7], and the United States' *Opportunity NYC* in New York City. Evidence for two (India's and Chile's) demonstrated positive and statistically significant effects, while for two (US and China) estimates are negative, not-statistically significant. In India, Das and Sarkhel (2020) used a difference-in-differences strategy to estimate a statistically significant 3.4 percentage point increase in secondary school enrollment (from a base enrollment rate of 80%) after five years of implementation. Galasso (2006) used a regression-discontinuity design to estimate the effect of *Chile Solidario*, finding a statistically significant 7.6 percentage points increase in school enrollment in a combined sample of primary and secondary students (children aged 7 to 15) after two years of program exposure (from a base enrollment rate of 58%).

In the case of *Opportunity NYC*, Riccio et al. (2010) used an RCT design to measure one- and two-year impacts on school outcomes, finding no statistically significant effects on enrollment in elementary or middle school. Similarly, for China's CCT pilot project, Li et al. (2017) used a randomized design to measure three-year impacts on secondary school enrollment, finding negative, not-statistically significant, effects.

In sum, consistent with previous findings, the new evidence on the impacts of CCTs on school enrollment confirms three very robust findings of the education CCTs literature. First, across all programs and school levels, CCTs systematically increase school enrollment. Second, school enrollment impact estimates are systematically greater for secondary school—consistent with an

[6] Baseline enrollment data not provided. Average school enrollment rate for ineligible group is 34%.
[7] China's CCT pilot project was included in Garcia and Saavedra (2017) for impacts on dropout but not on school enrollment, as the enrollment study was published in 2016.

additional substitution effect operating through opportunity cost of time among secondary school children, a large fraction of whom would not enroll otherwise and for whom conditions effectively change the relative price of schooling *vis-à-vis* leisure or labor. Third, there is substantial heterogeneity across programs. A new finding from our review of recent papers that analyzed longer-term impacts on enrollment is that in most cases, the positive impact estimates on school enrollment reported in the short term—particularly in secondary—persist in the medium term.

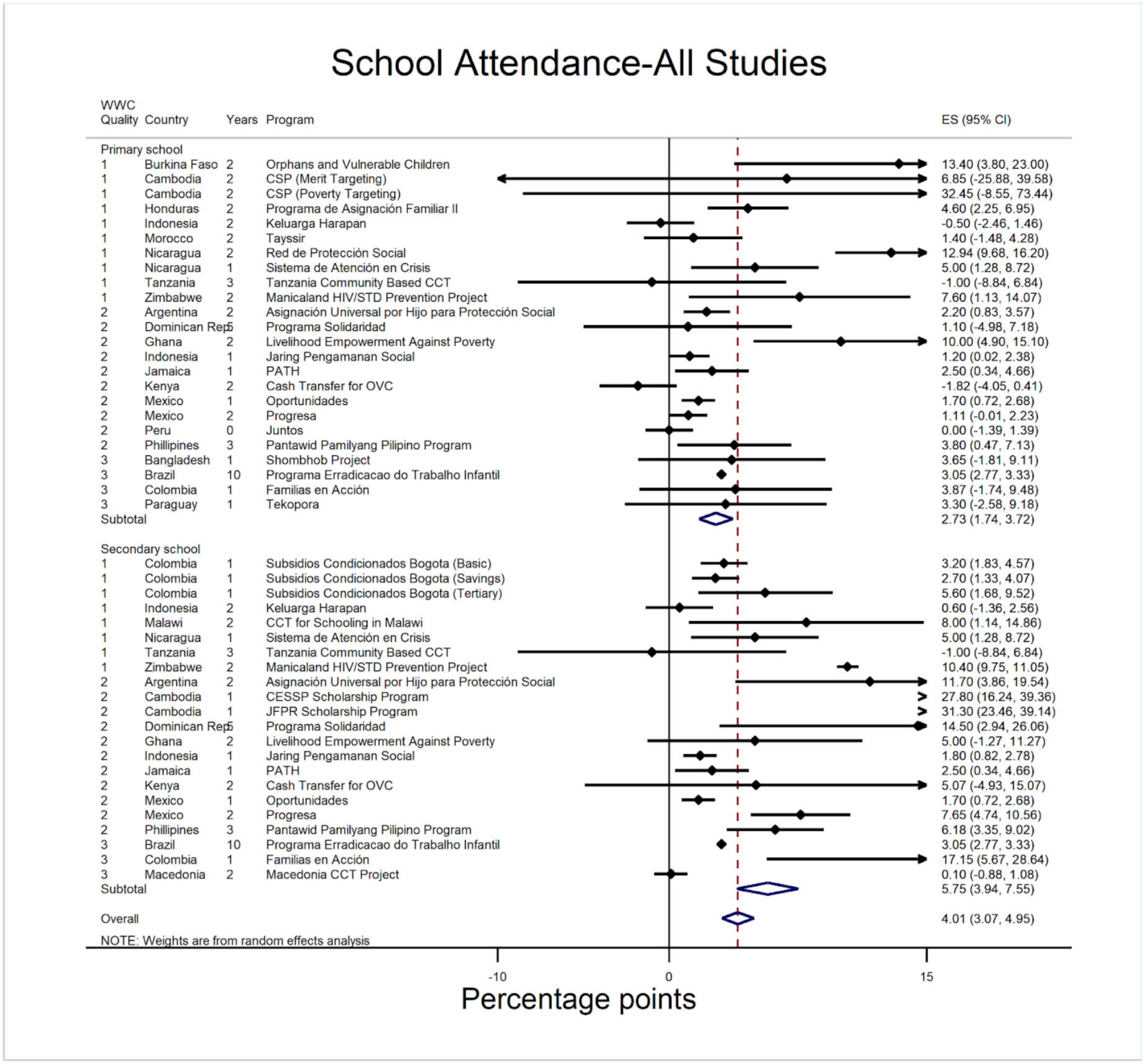
b. Attendance

As is the case for school enrollment, CCTs also have consistently demonstrated positive and significant effects on attendance, (Fiszbein and Schady 2009; Baird et al. 2014). Garcia and Saavedra (2017) meta-analyzed 46 studies estimating the impact of CCTs on school attendance in low- and middle-income countries, and found the programs, on average, increase school attendance by 4 percentage points. As is also the case with CCTs' impacts on school enrollment, the meta-analytic random-effects average effect size for secondary attendance is more than twice as large as that of primary school attendance (5.7 and 2.7 percentage points, respectively). Another robust finding in the literature is that, even conditional on schooling level, there is substantial heterogeneity in attendance effect size estimates across education CCT programs (Garcia and Saavedra 2017).

Figure 5.2 presents average effect sizes for primary and secondary attendance of CCTs in the low- and middle-income countries included by Garcia and Saavedra (2017). Of the studies, 54% found positive and statistically significant effects on school attendance, 29% found positive but not-statistically significant effects, and only three studies found effects that were negative but not statistically significant. (Those three studies looked at Indonesia's *Keluarga Harapan*, Tanzania's *Community-Based Conditional Cash Transfer*, and Kenya's *Cash Transfer for Orphans and Vulnerable Children*). The programs packing the largest impact sizes for primary school attendance are Burkina Faso's *Orphans and Vulnerable Children* (13.4 percentage points), Nicaragua's *RPS* (12.9 percentage points), and Ghana's *Livelihoods Empowerment Against Poverty* (10 percentage points). Note that *RPS* also had the largest reported effect size estimate for primary enrollment.

The pattern of CCTs' positive effects on secondary attendance is even more compelling than those for primary attendance. This is consistent with the notion of substitution effect lowering the opportunity cost of secondary school children's time spent in school that is likely not present among primary school students, most of whom already attend school. Out of 22 studies in Garcia and Saavedra (2017) reporting impacts on secondary school attendance, 77% found positive and statistically significant effect estimates, and only one reported a negative and not-statistically significant effect (for Tanzania's program). As is the case for school enrollment, the largest effects on secondary school attendance are for Cambodia's *JFPR* and *CESSP* (31.3 and 27.8 percentage points, respectively). Two other programs standing out for large reported impact estimates on secondary school attendance are Brazil's *PETI* (17 percentage points) and Argentina's *Asignacion Universal por Hijo para la Proteccion Social* (11.7 percentage points).

Figure 5.2. Forest plot of impact estimates for primary and secondary attendance of education CCTs in low- and middle-income countries



Notes. Figure reproduced from García and Saavedra (2017). For each program, the authors plotted the effect size estimate and 95% confidence interval for the lengthiest time of program exposure reported. The overall mean effect size is from a standard intercept-only random-effects model. Years is the time of program exposure. WWC quality is study quality according to Institute for Educational Sciences' What Works Clearinghouse (WWC) version 2.1 Evidence Standards Protocol (Institute of Educational Sciences 2011) [1=meets evidence standards, 2= meets evidence standards with reservations, 3=does not meet evidence standards]. See details in García and Saavedra (2017)

For this chapter, we retrieved 10 new references reporting education CCT estimates on school attendance not previously included in the 2017 paper by Garcia and Saavedra (see Appendix Table A3). Three references reported on medium-term effects of two programs that were included in Garcia and Saavedra (2017): Argentina's *Asignacion Universal por Hijo para Proteccion Social* (*AUH*), and Indonesia's *Keluarga Harapan*. For *AUH*, Edo and Marchionni (2019) used a difference-in-differences strategy to find a positive and statistically significant effect of 3.9 percentage points on secondary school attendance after four years of program exposure. Serio and Herrera (2021) used matching techniques and found that, after seven years of implementation, *AUH* still had a small but statistically significant impact on school attendance of 1.3 percentage points for upper secondary school, but not-significant effects on school attendance for lower secondary.

Cahyadi et al. (2020) reported effects for Indonesia's *Keluarga Harapan.* The authors used a randomized design to report a statistically significant increase of four percentage points in primary school attendance, and a 13 percentage points increase, also statistically significant, in secondary school attendance after two years of program exposure.[8] Moreover, Cahyadi et al. (2020) found effects on attendance are sustained over time, with a 3.4 statistically significant percentage points increase in primary school attendance and a 10 statistically significant percentage points increase in secondary school attendance after 6 years of program exposure.

Seven of these 10 new references reported attendance effects of programs not included in Garcia and Saavedra (2017),[9] most from high-income countries. Among them, only two reported positive and statistically significant impact on school attendance: Costa Rica's *Avancemos*. Meza-Cordero et al. (2015) used difference-in-differences matching to find a statistically significant 28 percentage point effect on secondary school attendance of *Avancemos* (from a base enrollment rate of 65%). For Argentina's *Ciudadania Porteña*, Hatrick (2015) used propensity score matching and estimated short-term effects, finding statistically significant impacts in school attendance, one and two years into the program of 7.5 percentage points and 3.2 percentage points. Two studies reported small negative statistically significant effects. Corrales-Herrero et al. (2021) employed matching techniques to report Panama's *Red de Oportunidades*, after two years of program implementation, had a not-statistically significant 5.7 percentage points increase in primary school attendance but a statistically significant 4.4 percentage points decrease on secondary school attendance. Fryer (2011), using a randomization design, found a statistically significant negative effect (0.087SD) on the seventh-grade attendance rate for New York City's pilot program (a program different from *Opportunity NYC*).

The remaining three new references, all from high-income countries, report primarily null effects on school attendance. In the case of Uruguay's *PANES*, Borraz and González (2009) used propensity score matching to find null effects for primary school attendance, yet a statistically significant 1.8 percentage points increase in secondary school attendance one to two years into the program. Also for *PANES,* Ferrando (2012) used a regression discontinuity design and found a positive, not-statistically significant 16.6 percentage points increase on secondary school attendance one to two years into the program, and a negative, not-statistically significant impact of 6 percentage points three years into the program. Three references reporting school attendance effects for programs in the United States found null effects: using a randomization design, Fryer (2011) found a non-statistically significant reduction of -0.04SD in the attendance rate of

[8] This impact estimate differs from Alatas (2011), included in Garcia and Saavedra (2017), which reported null effects on primary school attendance for two years of program exposure.

[9] Panama's *Red de Oportunidades* was included in Garcia and Saavedra (2017) for enrollment effect sizes but not for school attendance.

secondary school students in Dallas' pilot program, and non-statistically significant increase of 0.15SD in the attendance rate for students in Chicago's pilot program. In Opportunity NYC, Riccio et al. (2010) found null attendance effects using a randomization design among primary and secondary school students one and two years into the program.

Taken together, evidence for the impacts of education CCTs on school attendance highlight four robust findings in the literature, all closely aligned with those regarding school enrollment outcomes. First, most education CCT programs in low- and middle-income countries have systematically demonstrated positive and significant effects on school attendance. These impacts are, on average, substantially larger for secondary schooling than for primary schooling, which is consistent with a stronger substitution effect in secondary school operating through lowered opportunity cost of children's time. Second, evidence, although limited, from longer-term impact evaluations suggest effects on attendance are sustained over time. Third, there is substantial heterogeneity across programs in school attendance impact estimates, even conditional on schooling level. Fourth, among programs in high-income countries, the majority of reported positive school attendance impact estimates are not statistically significant or are small and negative. One possible explanation of the differential pattern of impact estimates between low- and middle-income countries versus high-income countries is that the latter have very high baseline school attendance rates, even in secondary school.

c. Dropout

Perhaps it is unsurprising that most CCT programs analyzed—particularly those in developing countries—increase school enrollment and attendance. After all, these two schooling dimensions are precisely the conditions that families must meet to receive the transfers. Another downstream outcome—typically not directly incentivized and closer to the process of human capital accumulation implicit in the conceptual framework—is school dropout. Families lose the subsidy if children drop out of school, of course, but increasing the process of human capital accumulation begins with keeping students in school for longer. Garcia and Saavedra (2017) meta-analyzed 18 studies measuring the impact of CCTs on school dropout decisions. They concluded education CCT programs in low- and middle-income countries, on average, reduce dropout, particularly for secondary students. For secondary schooling, the meta-analytic random effects size estimate is that CCTs, on average, reduce secondary dropout by close to 3 percentage points, which is almost three times the meta-analytic average estimate for primary school dropout reduction (1.2 percentage points). The authors also showed there is substantial heterogeneity in secondary school dropout estimates across studies and programs, and that all reported effect sizes are statistically significantly different from zero. However, the authors did not find significant heterogeneity in primary school dropout estimates. The programs with the smallest impacts on secondary school dropout reductions are Brazil's *Bolsa Familia* and Colombia's *Familias en Accion* (*Urban Expansion*), which after four years and three years of program exposure, respectively, only reduce secondary school dropout by 0.5 percentage points. At the other end of the spectrum, China's pilot program, after one year of exposure, reduced secondary school dropout by 7 percentage points.

For this review, we included eight additional studies not previously included in Garcia and Saavedra (2017—Appendix Table A4 for details). Six of these provide new evidence on dropout effects for five programs not included in Garcia and Saavedra (2017): Colombia's *Familias en*

Accion (Rural Version),[10] Costa Rica's *Avancemos*, Argentina's *AUH*, South Africa's *Swa Koteka,* and China's CCT pilot in Shaanxi and Hebei provinces (*PSH*). For Colombia's *Familias en Accion*, García et al. (2012), combined a difference-in-differences strategy with survey data collected on treatment and control groups from the original impact evaluation. The authors did not find evidence that the program, 10 years into implementation, reduced dropout. Duque et al. (2019) combined a regression discontinuity design based on the Sisben score that determines program eligibility with administrative enrollment data 10 years. They found early exposure reduced dropout by 7 percentage points from a base of about 32%. One reason for the divergent results in these two studies for *Familias en Accion (Rural Version)* is that there likely are heterogeneous treatment effects, and each approach estimates local effects for different complier groups. The discontinuity-based estimator of Duque et al. (2019) is local to children in households close to the eligibility cutoff, whereas Garcia et al. (2012) is more akin to an average treatment effect.

For the other three new programs not included in Garcia and Saavedra (2017), evidence also is mixed regarding the impact of CCTs on dropout. To estimate the effect of Argentina's *AUH* on schooling outcomes, Edo and Marchionni (2019) employed a difference-in-differences approach with household survey data. They found that the program reduced secondary school dropout by 4.7 statistically significant percentage points six years after program implementation. Similarly, for Costa Rica's *Avancemos*, Mata and Hernández (2015) used household survey data and a matching difference-in-differences strategy to learn the program reduced secondary dropout by 14 percentage points. In contrast, for South Africa's *Swa Koteka* program, Kilburn et al. (2020), employed a randomization design and found no evidence that after four years the program affected secondary school dropout rates.[11] For China's CCT pilot in Shaanxi and Hebei provinces, Li et al. (2017) used a randomization design to find a not-statistically significant increase of 1 percentage point in the probability of secondary school dropout. The design of this pilot in Shaanxi and Hebei provinces differs from the other Chinese CCT pilot first studied by Mo et al. (2013) and later included in Garcia and Saavedra (2017). The latter pilot is a traditional CCT providing two annual payments to households, conditional on regular school attendance. The pilot in Shaanxi and Hebei provinces, however, offers a voucher consisting of a contract promising the cash payment after three years if the student meets conditions of staying enrolled in a three-year secondary school program.

Two additional studies reported dropout impacts over longer time horizons for programs previously included in Garcia and Saavedra (2017). Gazeaud and Ricard (2021) investigated the impact of the national expansion of Morocco's *Tayssir*, through a regression discontinuity design based on municipality-level eligibility from an assignment means-test score. The authors found that the expansion of *Tayssir* reduced primary school dropout by 1.3 percentage points 10 years after program implementation (from a base of about 4%). This evidence is consistent with short-term evidence from *Tayssir's* pilot phase (Benhassine et al. 2015). Attanasio et al. (2021)

[10] *Familias en Accion (Rural Version)* refers to the original version of the program, implemented between 2001 and 2004 in small (100,000 inhabitants or less), mainly rural, municipalities. In 2007, the program expanded to urban areas (*Urban Expansion*).

[11] The point estimate is -1 percentage point, not statistically significant, for an outcome that is actually a "schooling deprivation" indicator that combines secondary school dropout, grade repetition at any time between baseline and follow-up, or not attending school by the time of follow-up. The authors provided no direct estimate on dropout rates.

investigated the long-term impacts on schooling Colombia's *Familias en Accion (Urban Expansion)* in the city of Medellin. The authors used a regression discontinuity design based on the Sisben score eligibility cutoff rule used to assign beneficiaries to the program. The authors found that eight years into the program, the transfers reduced secondary school dropout by 6 percentage points from a base of about 55%.

Taken together, the evidence on the impacts of education CCTs on school dropout highlight three main findings, and they are broadly consistent with the body of evidence regarding impacts on school enrollment. First, most education CCT programs in low- and middle-income countries have systematically demonstrated robust, significant reductions in school dropout, particularly in secondary schooling. Second, school dropout effects appear to persist over time for most programs. Third, there is substantial heterogeneity in long-term dropout estimates across programs, consistent with heterogeneity in the short term.

5.2 Impacts of education CCTs on learning

Previous systematic reviews have found weak evidence on the question of whether education CCTs improve learning. One limitation of such reviews is the few studies considered. For example, Baird et al. (2014) meta-analyzed five studies measuring effects on learning; these likely were not plagued by selection concerns into test-taking as a result of the program because the research teams across all five studies tested children at home. Baird et al. (2014) found that these programs did not substantially improve test scores. The pooled random-effects effect size was 0.08SD, not-statistically significant. In their meta-analysis, only two studies reported positive and statistically significant effects on test scores: Malawi's *Zomba Cash Transfer Program* and Nicaragua's *RPS*. Two other studies found positive and statistically not-significant effects on test scores: Morocco's *Tayssir* and Burkina Faso's *Nahouri Cash Transfers*. The fifth study reported a negative and not-statistically significant effect on test scores for Cambodia's *CESSP*.

Snilstveit et al. (2015) extended the meta-analysis of Baird et al. (2014) to achievement outcomes, adding 14 studies reporting impact of cash transfers (both UCT and CCTs) on learning. Although the authors did not distinguish between UCTs and CCTs when estimating average effect sizes, their meta-analytic results were consistent with those reported for CCTs by Baird et al. (2014). Specifically, Snilstveit et al. (2015) found across all 14 studies a pooled effect size estimate of 0.01SD on math test scores and, on language test scores, of 0.00SD.

Further systematic review evidence from Molina-Millan et al. (2019) on CCTs' effects on learning over longer time horizons finds a more mixed picture, with some studies reporting positive and significant effects on achievement while others reported null effects. The authors were interested primarily in long-term outcomes, so they focused their review on learning in the more mature CCT programs: Mexico's *Progresa*, Colombia's *Familias en Accion (Rural Version)*, Nicaragua's *RPS*, Cambodia's *CESSP*, and Malawi's *Zomba Cash Transfer Program*. They found mixed evidence: some studies reported positive and significant effects, but at the same time many studies found null results.

Molina-Millan et al. (2019) noted, for example, that Behrman, Parker, and Todd (2009, 2011) found no effects on learning after six years of participation in *Progresa*. A strength of these two papers is that, to deal with the issue of selection into test-taking, the authors administered tests at home to children in both treatment and control municipalities. However, one weakness is that the authors' reported estimate is for differential exposure to the program after six years, since at that point children in the delayed-entry control group had already received benefits for some time. Barham, Macours and Maluccio (2018a, 2018b) compared early- and late-treatment groups to evaluate the impact of differential exposure, for boys and girls, to Nicaragua's *RPS* after 10 years. To address the issue of selection into test-taking, the authors administered language and math tests at home to young adults aged 15-22. The authors reported that the program had a positive and statistically significant effect of 0.16SD on math and 0.2SD on language learning for boys, but no effect among girls.

Among other studies included by Molina-Millan et al. (2019), García et al. (2012) used a difference-in-differences approach from the original non-randomized evaluation sample of *Familias en Accion (Rural Version)* to analyze impacts on learning after 10 years of exposure for children exposed before age 7. Garcia et al. (2012) addressed the issue of selection into test-taking by administering cognitive tests at home. For younger children (exposed earlier and tested ages 9-11), the authors found no significant effect of *Familias en Accion* on students' TVIP cognitive test scores. For older children (exposed later and tested ages 12-17), the authors found a substantial, although only marginally statistically significant, effect of 1.07SD on math test scores, and a positive, but not statistically significant, effect, on Raven's progressive matrices test of 0.16SD. One limitation of Garcia et al. (2012) is that, as Molina-Millan et al. (2019) noted, using the early age-group means that reported effect—even if accurately addressing the issue of selection—is a combination of exposure to *Familias en Accion (Rural Version)* in early childhood (and, thus, to only the program's nutrition and health component) and during early schooling ages (then, to the educational component), so it is not possible to single out the educational subsidy's contribution to achievement.

Baez and Camacho (2011) estimated long-term learning impacts after nine years exposure to Colombia's *Familias en Accion (Rural Version)* among students primarily exposed to the program through secondary school. The authors looked at scores on a national secondary school exit exam which must be taken to graduate from secondary school. To address the issue of selection into taking the test, the authors used two approaches: a bounding approach that, as explained in Section 4, trims a proportion of the bottom-scorers in the winner sample to balance the proportion of test-takers in treatment and comparison groups; and matching on observables. Their research design drew on a means-test discontinuity in program eligibility. The authors found null effects on test scores across a number of specifications. Duque et al. (2019) followed a similar approach of regression discontinuity as did Baez and Camacho (2011), but over a longer time. They found a positive impact on scores in Colombia's secondary exit exam of 0.13SD—marginally statistically significant. These evaluations—of *Familias en Accion (Rural Version)* in particular, and other CCT programs more generally—that rely on administrative data are somewhat difficult to interpret for reasons explained in Section 4. Because administrative test-score data is not administered in all grades (e.g., Baez and Camacho 2011; Duque et al. 2019) test-score results might not generalize to students in other grades. There also could be different patterns of selection into different grade levels, exacerbating issues stemming from selection into test-taking, as highlighted previously. Finally, with only a cross-section of test scores, as is the case with the Colombian secondary school exit exam, often it is not possible to distinguish heterogeneous effects of the program at different ages from differential effects by length of exposure.

Molina-Millan et al. (2019) included in their review two more studies, both of which followed long-standing programs outside Latin America, reporting null impacts on learning. Filmer and Schady (2014) used a regression discontinuity research design and test-score data collected at home 18 months into the program to estimate the impact of Cambodia's *CESSP* on achievement. They found no evidence of impacts on test scores in either math (0.01SD) or language (0.02SD). Analyzing achievement data on a test administered at home by the researchers four years into the program, Baird, McIntosh, and Özler (2020), used the original experimental design of Malawi's *Zomba Cash Transfer Program* to show no effect on test scores, which suggests fade-out as the authors earlier had found short-term learning gains (Baird, McIntosh, and Özler 2011).

One additional substantive contribution of this chapter is the inclusion of many new studies looking at the effects of education CCTs on student achievement. This expanded sample provides a more complete picture, and we can also produce new meta-analytic results that shed light on average effect sizes on achievement and heterogeneity for the broadest set of studies in the literature to date. In total, we retrieved 37 studies[12] that had not been previously included in earlier reviews of the literature of the effects of CCTs on achievement[13] (see Appendix Table A5 for details). Among these new studies, in general, there is substantial variation in both the time frames and the methodological approaches the authors used to address issues of selection into test-taking. For example, out of these new studies, 12 (32%) conducted testing at home, two (5%) used bounding approaches, two matched on observables test-takers in treatment and comparison groups, one conditioned on prior school attendance, and one reweighted the sample to balance the distribution of observables among test-takers in treatment and comparison groups. The remaining 19 studies (51%) made no adjustment to account for the changes in composition that likely result in selection bias. As explained in Section 4, if programs induce infra-marginal, low-scoring applicants into school, then estimation strategies not accounting for compositional effects are likely to identify a lower-bound estimate on learning.

a. New studies that test children at home to deal with selection into test-taking

Among the 12 studies that tested children at home to deal with selection into test-taking, only four found positive and statistically significant effects: one study with estimates for Tanzania's *Community-Based Conditional Cash Transfer*, and two with estimates for Cambodia's *CSP (Merit Targeting)* and *CSP (Poverty Targeting)* over two different time periods. To measure the effect of Tanzania's CCT on language test scores, Evans et al. (2014) used a randomization-based design and testing at home, finding a positive and statistically significant effect of 0.04SD two years into the program, but no effects after three years. For Cambodia's *CSP*, Barrera-Osorio and Filmer (2016) used a randomization-based design and tested at home to measure impact of the program under two different targeting mechanisms: one based on students' economic need (*Poverty Targeting*) and the other based on students' test scores at baseline (*Merit Targeting*).

[12] As in Garcia and Saavedra (2017), we define a study as an impact evaluation reporting effect estimates for a given CCT program, outcome, and point in time.
[13] In this count of new studies, we are including nine reviewed by Snilstveit et al. (2015). We include them here as "new" because Snilstveit et al. (2015), in their analysis, did not differentiate between UCT and CCTs. Also, they did not detail their methodology used to deal with selection into test-taking, which is core in this section.

They found, for the merit arm after three years of program implementation, a marginally statistically significant effect of 0.195SD on math test scores. However, for the poverty arm, they found a non-statistically significant negative effect of -0.02SD. Barrera-Osorio, de Ramos and Filmer (2018) analyzed achievement impacts of *CSP* after nine years, again testing children at home. *Merit Targeting* had a statistically significant effect of 0.16SD on Raven's progressive matrices test, and a positive but non-statistically significant effect on math scores of 0.07SD. The authors did not find evidence of long-term impacts on learning among those in the *Poverty Targeting* mechanism.

Of the 12 studies using testing at home, eight found no statistically significant effects on learning (including the aforementioned two studies on *CSP Poverty Targeting* and Tanzania's after three years of program implementation). Paxson and Schady (2010) analyzed the impact on learning after three years of Ecuador's *BDH*, and found a non-statistically significant effect (0.13SD) on language test scores. Araujo, Bosch and Schady (2019) estimated the impact of *BDH* on learning after 10 years of program implementation by using a regression discontinuity design, finding close to null effects for both math and language scores (respectively, -0.125SD and -0.001SD; neither statistically significant). For Peru's *Juntos* program, Gaentzsch (2020) used a difference-in-differences matching strategy 5-7 years into the program and found, among children 7-8 years old at baseline, negative, non-statistically significant effects on math scores of -0.10 test score points,[14] and on language scores of -0.23SD. For children exposed in early childhood, 6-18 months at baseline, the author found non-statistically significant effects on language scores of 0.23SD, and a negative, statistically significant, effect on math scores of -0.61 test score points. Sanchez, Melendez and Behrman (2020) analyzed learning impacts of *Juntos* eight years into the program and found, for children exposed during early childhood (0-4 years old), a non-statistically significant effect of 0.21SD on language scores, and, for children exposed during early school years (5-8 years old), a non-statistically significant effect of -0.02SD.

Finally, among the new studies which tested students at home, in addition to Gaentzsch's (2020) finding for *Juntos* for math scores among the younger cohort, one found negative effects on learning outcomes. Das and Sarkhel (2020) used difference-in-differences to measure the effect of India's *Kanyashree Prakalpa*, which targets adolescent girls 13 to 18 years old. Five years after program implementation, the authors found a marginally statistically significant negative effect of -5.9 percentage points on the probability of solving a 3-by-1 division, and a marginally statistically significant negative effect on the probability of reading a story. When looking at heterogeneous effects, the authors found the program has a positive effect on learning outcomes in schools, with low teacher absenteeism and better school infrastructure, suggesting the important role of supply-side components for translating school attendance into learning.

b. New studies that estimate bounds or conduct covariate adjustment to deal with selection into test-taking

Six new studies conducted statistical adjustment to account for compositional effects and possible bias into test-taking. Evidence from this group of studies is mixed. Two found positive and statistically significant effects, for short-term effects in Mexico's *Progresa* and *Prospera*;

[14] The author did not provide enough information to compute effects in SD units.

two found null effects, for medium-term effects in Mexico's *Oportunidades* and short-term in Colombia's *Familias en Accion (Rural Version)*; and two found statistically significant negative effects, for long-term effects in Argentina's *AUH* and Morocco's *Tayssir*.

Behrman, Sengupta and Todd (2000) used the original randomized rollout design of *Progresa's* impact evaluation to estimate two-year effects on learning. The authors employed bounding techniques to account for bias into test-taking, and found, among fourth-grade students, lower-bound estimates of 0.11 test score points[15] statistically significant effect on math test scores and 0.62 test score points statistically significant effect on language test scores. The authors found non-statistically significant effects for fifth- and sixth-grade students: estimates for fifth-graders are 0.79 points for math and 0.20 points for language; for sixth-graders they are -0.56 points for math and 0.28 points for language. Later, Behrman, Parker, and Todd (2009) estimated medium-term effects of *Oportunidades* on schooling outcomes six years into the program. To account for selection into test-taking, the authors used reweighting techniques to balance the covariate distribution of test-takers in treatment and control groups. The authors found non-statistically significant effects on math ranging from -1.1 points among males 11-12 to 0.08 points among females 13-15.[16] For language, they found non-statistically significant effects ranging from -1.2 points among females 9-10, to 0.54 points among males 11-12. Recently, Behrman, Parker, and Todd (2019) analyzed long-term impacts of *Prospera*, using matching estimators on administrative achievement test data, and conditioning on baseline attendance to account for selectivity into test-taking. For their main achievement analyses, the authors conditioned their sample on sixth-grade students taking administrative achievement tests in 2007-08, which was 10 years into the program. As the study noted, to the extent that these students had been exposed to the program in primary school, using sixth-grade baseline underestimates the length of program exposure, and potentially achievement impacts, if length of exposure is associated with achievement. They tracked student achievement through a panel dataset of administrative tests in seventh grade (2009-2010), ninth grade (2011-2012), and twelfth grade (2014-2015), although in twelfth grade they could only measure achievement by whether students increased their level of proficiency (e.g., from basic to proficient). The authors found in seventh grade a non-statistically significant effect on math scores of about 0.02SD, and a statistically significant effect on language of about 0.03SD. In ninth grade, they found statistically significant effects on math and language scores of about 0.05SD. For twelfth grade, they found a significant reduction in the probability of advancing categories in mathematics, and no change in language proficiency. The authors also found suggestive evidence of larger achievement impact estimates for females.

Garcia and Hill (2010), to measure the short-term effect of *Familias en Accion (Rural Version)* on learning, used household data from the program's original impact evaluation merged to national standardized tests. To address the issue of selection into test-taking, they matched test-takers on an estimated propensity score across treatment and control groups. The authors found, after one year of program implementation, a non-statistically significant effect of 0.15SD on math, and a non-statistically significant effect 0.19SD on language.

[15] No information was provided to translate estimates into SD units.
[16] No information was provided to translate estimates into SD units.

Serio and Herrera (2021), to analyze medium-term effects of Argentina's AUH program while accounting for bias into test-taking, used matching estimators similar to Garcia and Hill (2010). They found, seven years after program implementation, a negative and marginally statistically significant effect on math (ranging from -0.22SD for eighth and ninth grade students to -0.11SD for sixth grade students), and a marginally statistically significant effect on language (ranging from -0.18SD for eighth and ninth grade students to -0.06SD for eleventh and twelfth grade students). Gazeaud and Ricard (2021) used a regression discontinuity design to estimate learning impacts of Morocco's *Tayssir* 5-10 years into the program. The authors deployed bounding techniques to account for bias into test-taking, and found a lower-bound negative and statistically significant effect of -0.14SD on a composite test score (primary school graduation test).

c. New studies without adjustments for selection into test-taking

Nineteen new studies (not included in Molina-Millan et al. 2019 or Baird et al. 2014) conducted no adjustments for potential selection into test-taking. There is high variation in effect sizes among these studies. Only four reported positive and statistically significant effects (three field experiments in Chicago [*Chicago Heights low financial incentive* and *high incentive*, and *Bloom Township high financial incentive*], and Nepal's *Schooling Incentives Project*). Two studies reported positive and marginally statistically significant effects (*Chicago Public Schools [CPS] - high financial incentive*, and Indonesia's *Poor Student Assistance* [*BSM*]). Four studies reported positive but non-statistically significant effects on learning (Jamaica's *Programme of Advancement Through Health and Education* [*PATH]*, Ecuador's *BDH;* one of Chicago's field experiments [*Bloom Township - low financial incentive*], and the pilot program of Dallas). Three studies reported negative and non-statistically significant effects (China's CCT pilot, and Chile's *Chile Solidario* for 1- and 2-year impacts), and one reported negative and statistically significant effects (*CPS - low financial incentive*). Five studies reported non-statistically significant mixed results (pilot programs of Chicago and New York City, *Opportunity NYC* for 1- and 2-year impacts, and Mexico's *Prepa Sí*).

In the case of Nepal's *Schooling Incentives Project*, Edmonds and Shrestha (2014) used a randomization design to find a statistically significant effect, two years into the program, on a composite achievement test of 0.18SD. For the United States, the only studies with a positive and statistically significant effect on learning are Levitt et al. (2011) for three out of six Chicago's field experiments: *Chicago Heights low and high incentives*, and *Bloom Township - high incentive*. The authors used a randomization design at the class or school-grade level and students were offered financial incentives for test score improvement just before taking the test. They found a positive, statistically significant effect of 0.38SD and 0.24SD on a composite test score for *Chicago Heights high financial* and *low financial incentive*, respectively, and a statistically significant effect of 0.17SD for *Bloom Township - high financial incentive,* but no effect for *Bloom Township - low financial incentive*. Also, Levitt et al. (2011), for Chicago's field experiment *CPS- high incentives*, found a marginally statistically significant effect of 0.19SD on math scores, and a non-statistically significant effect of 0.03SD on language scores. For Indonesia, Purba (2018) used propensity score matching to estimate the impact of *Poor Student Assistance (BSM)*, and found a marginally statistically significant 5.6%[17] higher score on a composite score among program recipients compared to non-recipients.

[17] No information was provided to translate estimates into SD units.

In the case of Jamaica's *PATH*, Stampini et al. (2018) used a regression discontinuity design to measure learning effects six years into the program. Without any adjustment for compositional effects of the test-taking sample, they found a marginally statistically significant effect of 0.06SD on language scores, and a positive but not significant effect (0.10SD) on math scores. For Ecuador's *BDH*, Ponce and Bedi (2010) used a regression discontinuity design to find, after two years of program implementation, a non-statistically significant effect of 0.10SD on math scores and a non-statistically significant effect of 0.01SD on language scores.

Mo et al., (2013) used a randomization-based design to measure 1-year impacts of China's CCT pilot in Northwest China. Without any adjustment for compositional effects into test-taking, the authors found a negative non-statistically significant effect of -0.04SD on math scores. For Chile's *Chile Solidario*, Galasso (2006) used a regression discontinuity design and no adjustment for compositional effects for selection into test-taking., finding a negative non-statistically effect of -0.10SD on a composite test one year after program implementation, and a negative non-statistically significant effect of -0.003SD two years after program implementation. In the case of Chicago's field experiments, Levitt et al. (2011) found a negative statistically significant effect of *CPS - low financial incentive*.

Dustan (2020) used a differences-in-difference estimation comparing multiple cohorts to estimate the impact of Mexico City's *Prepa Sí*. For students fully exposed to the program since their first year of high school, the author found a positive but non-statistically significant effect of about 0.01SD both in math and language scores after 3-4 years of program exposure. For those partially exposed to the program since their second year of high school, the author found a negative and non-statistically significant effect of about -0.02SD in both math and language scores.

Other studies for programs in the United States found null effects on learning. Fryer (2011) used a randomization design to analyze learning impacts of transfer pilot programs in Dallas, New York City, and Chicago. One year into Dallas' program, the author found positive but non-statistically significant effects for math (0.01SD) and language scores (0.08SD). After one year, estimates for New York City's pilot program are 0.06SD on math and -0.03SD in language for fourth grade students (neither statistically significant), and -0.03SD on math and 0.004SD in language for seventh grade students (neither statistically significant). Also after one year, Chicago's pilot program had a positive but not statistically significant effect of 0.01SD on math and a negative but not statistically significant effect of -0.01SD on language. For *Opportunity NYC*, Riccio et al. (2010) used a randomization design to estimate learning effects, finding positive and non-statistically significant effects for K-5 students after 1-2 years of program implementation (about 0.03SD in math and language scores after one year, and 0.02SD on math and 0.03SD on language after two years). The authors found small, negative, non-statistically significant effects for sixth-eighth grade students (-0.003SD on math and -0.04SD on language after one year of program implementation, and -0.05SD in math and -0.01SD in language after two years of program implementation).

Summary of effect size estimates on student learning

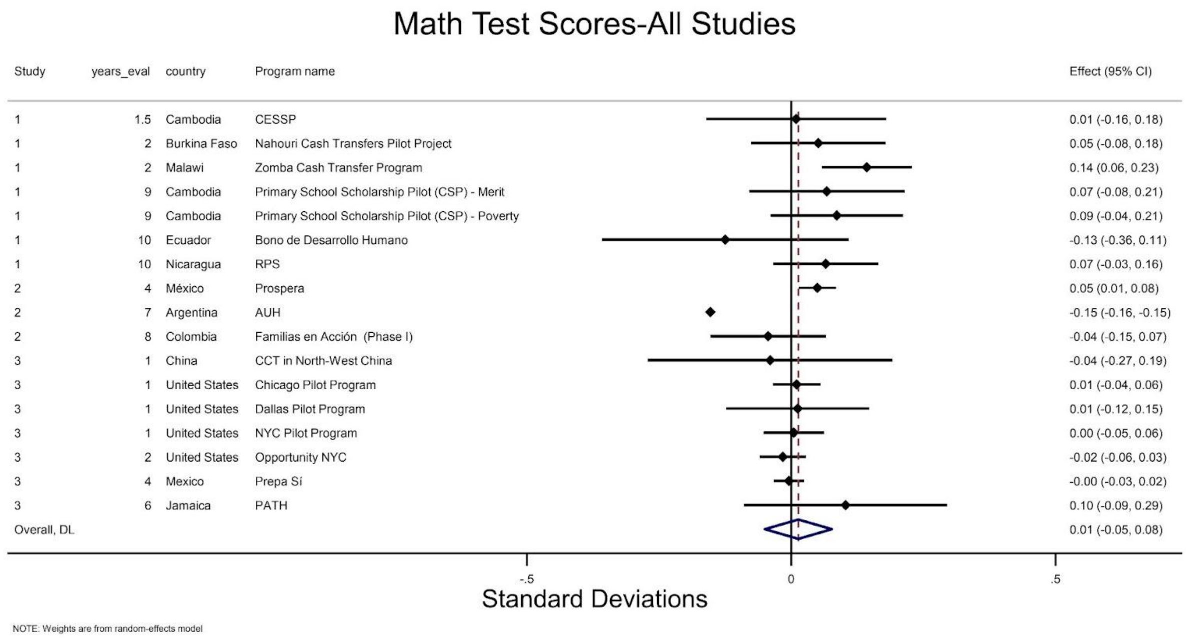
In this section we present two new sets of empirical results. The first are meta-analytic results estimating random-effects average effect sizes separately for math and language impact estimates for the various studies reviewed. We also estimate meta-analytic regressions to statistically test whether there are systematic differences in effect sizes that test students at home versus all others that do not. The second set of new results presents scatter plots and correlations for learning and school enrollment and attendance impact estimates. One key assumption in the conceptual framework presented in Section 3 concerns the human capital production function, $H = h(t^c, t^m, X)$, which primarily depends on time devoted to schooling activities by child and mother/parents. An empirical prediction stemming from this assumption is, all else being constant, we should expect to see greater human capital accumulation—as measured by program impacts on test scores—in programs that induce the greatest increases in student's time spent in school, as measured by program impacts on school enrollment and attendance. The second set of new results aims to shed light on this prediction.

Figure 5.3 presents a forest-plot summary of effect size estimates for the impacts of CCTs on math scores among the 17 studies described earlier reporting math impact estimates[18]. Two results stand out. First, the overall random-effects impact estimate for math learning is 0.013SD and is non-statistically significant (the 95% confidence interval is -0.05SD, 0.077SD). The magnitude and statistical significance of this new estimate of average math effect size is consistent with previous estimates based on smaller samples of studies (Baird et al. 2014; Snilstveit et al. 2015). The second result is that we strongly reject homogeneity in math learning impact estimates. (The Cochran's Q statistic for the null hypothesis of homogeneity in impact estimates in the random effects model is 398, p-value=0.000).

Figure 5.4 presents a forest-plot summary of effect size estimates for the impacts of CCTs on language scores among 18 studies with reported learning estimates reviewed. The overall random-effects CCT impact on language learning is 0.029SD—not statistically significant (95% confidence interval is -0.027, 0.084). This estimate is consistent with previous meta-analytic effect size estimates from earlier reviews with smaller study samples (Baird et al. 2014; Snilstveit et al. 2015). For language learning impact estimates, we also reject homogeneity. (The Cochran's Q statistic for the null hypothesis of homogeneity in impact estimates in the random effects model is 320, p-value=0.000). The finding of heterogeneity in math and learning impact estimates corroborates and strengthens one the strongest empirical regularities in the education CCT literature—namely, the substantial statistical heterogeneity in impact estimates across programs along the spectrum of schooling outcomes.

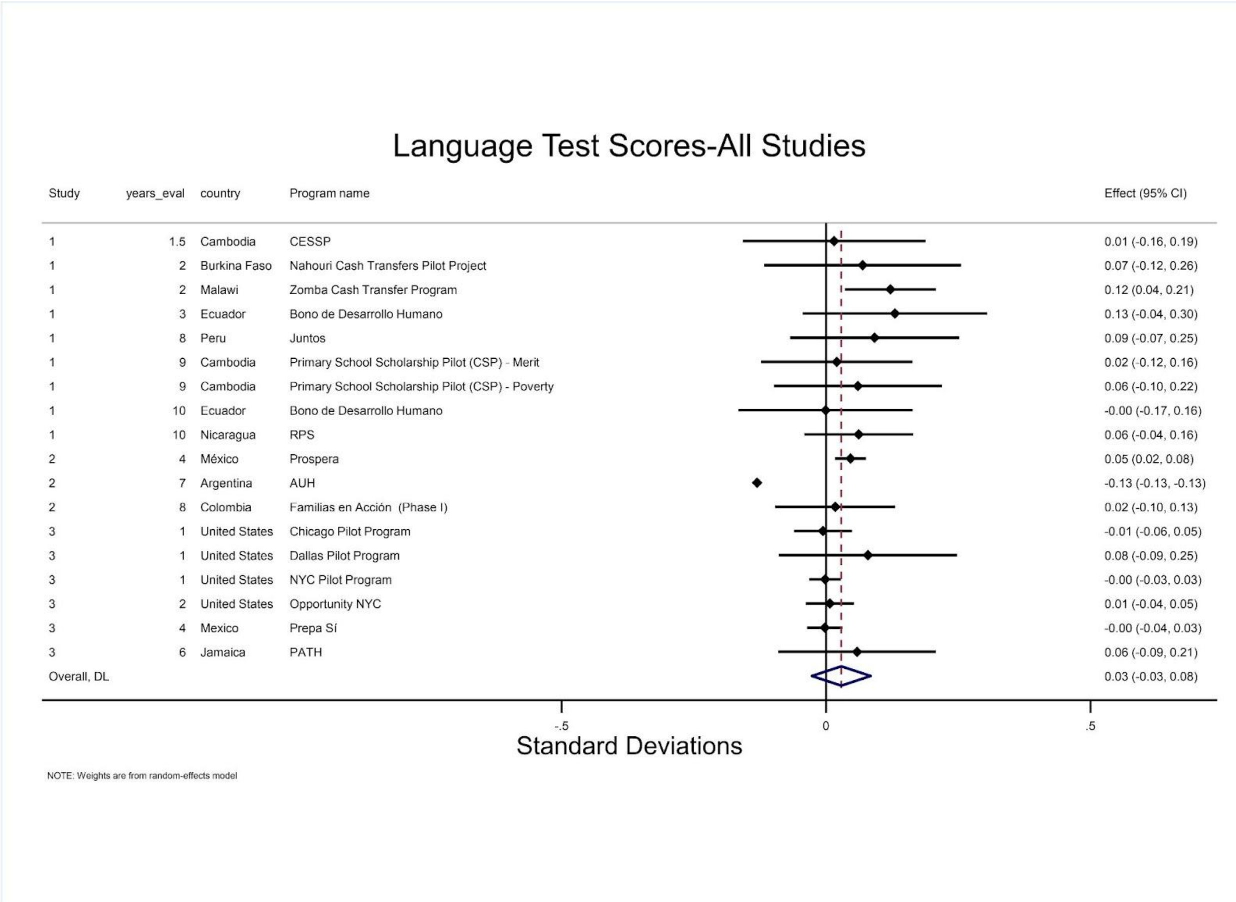
[18] For each program, we plot the effect size estimate for the lengthiest time of program exposure reported. If more than one effect size is reported for the same year (for example gender or age groups), we estimated average effect sizes using Hedges, Tipton, and Johnson (2010) approach, also used in Garcia and Saavedra (2017). We include in the meta-analysis studies that reported effects in Math and Language scores in SD or that provided sufficient information to translate effects into SD units.

Figure 5.3. Forest plot of CCT impact estimates on math achievement



Notes. For each program, we plot the effect size estimate for the lengthiest time of program exposure reported. If more than one effect size is reported for the same year (for example, different gender or age groups), we estimated average effect sizes using Hedges, Tipton, and Johnson (2010) approach, also used in Garcia and Saavedra (2017). The overall mean effect size is from a standard intercept-only random-effects model. The Cochran's Q statistic for the null hypothesis of homogeneity in impact estimates in the random effects model is 398, p-value=0.000. Column 1 shows method to address selection into test taking (1=testing at home; 2=statistical adjustment (reweighting, bounds, matching or conditioning on prior attendance); 3=no adjustment)

Figure 5.4. Forest plot of CCT impact estimates on language achievement



Notes. For each program, we plot the effect size estimate for the lengthiest time of program exposure reported. If more than one effect size is reported for the same year (for example, different gender or age groups), we estimated average effect sizes using Hedges, Tipton, and Johnson (2010) approach, also used in Garcia and Saavedra (2017). The overall mean effect size is from a standard intercept-only random-effects model. The Cochran's Q statistic for the null hypothesis of homogeneity in impact estimates in the random effects model is 320, p-value=0.000. Column 1 shows method to address selection into test taking (1=testing at home; 2=statistical adjustment (reweighting, bounds, matching or conditioning on prior attendance); 3=no adjustment)

The other new meta-analytic result we report is a random-effects meta-regression model of achievement impact estimates with whether students were tested at home as a predictor variable. As noted, CCT effect estimates on test scores can be biased from selection into test-taking. If CCTs incentivize infra-marginal, potentially low-scoring students to enroll in school, conditional on test-taking would produce lower-bound estimates of true achievement impacts. Therefore, studies that test children at home should, on average, report greater achievement impact estimates than those that do not. We estimate a random-effects model with an indicator variable that takes the value of 1 if the study conducted testing at home and 0 otherwise, controlling for years of program exposure. Impact estimates are weighted by their estimation precision. Table 5.1 shows these estimation results. The estimated coefficient on whether students tested at home is positive and statistically significant for both math and language, consistent with the selection model's prediction.

Table 5.1 Meta-regression regression results of testing children at home as a possible mediator for learning effect size estimates in the CCT literature.

	Math (SE)	Language (SE)
Students tested at home (1=Yes)	0.107 (0.040)**	0.099 (0.036)**
Years of program exposure	-0.010 (0.006)	-0.009 (0.005)
Observations	17	18

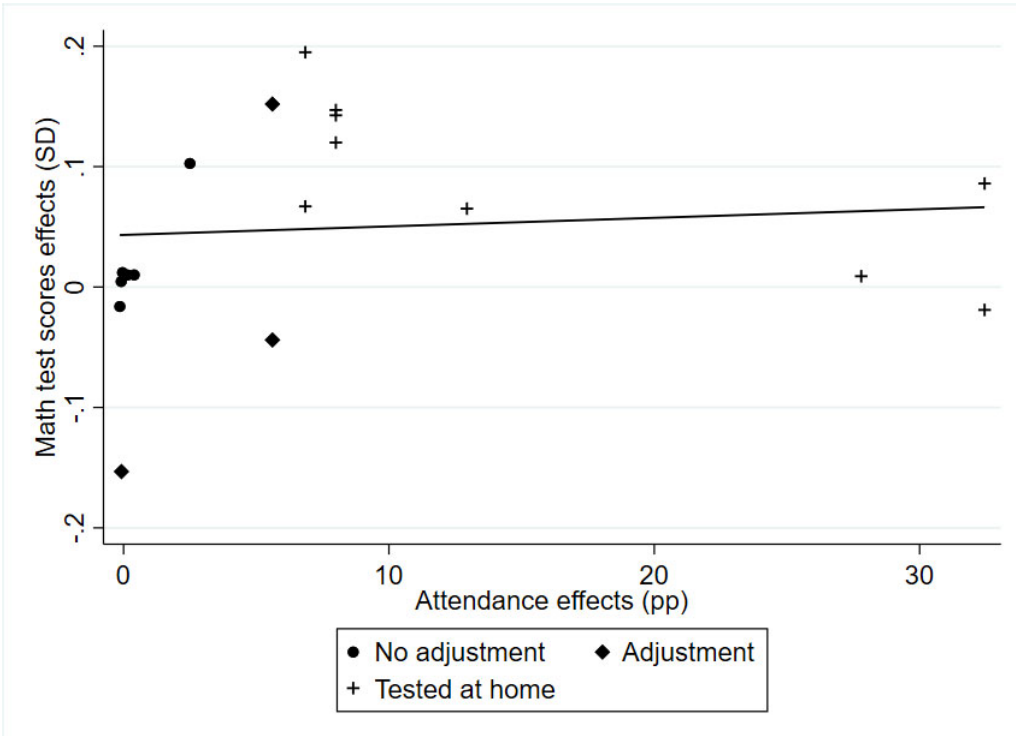
Note: Students tested at home is a binary predictor taking the value of 1 if students were tested at home and 0 if not. Zero includes estimates from bounds, conditioning on attendance, matching, weighting or no adjustment for composition in test-taking sample. Models control for years of program exposure corresponding to each estimate. Estimates are from random-effects meta-regression models with only one time period effect size per program, the one lengthiest available). Standard errors are in parentheses.

An assumption in the conceptual framework presented in Section 3 concerns the human capital production function, $H = h\ (t^c,\ t^{m,}\ X)$, which primarily depends on time devoted to schooling activities by child and mother/parents. An empirical prediction stemming from this assumption is, all else being constant, we should expect to see greater human capital accumulation—as measured by program impacts on test scores—in programs inducing the greatest increases in student's time spent in school, as measured by program impacts on school enrollment and attendance. In the remainder of this subsection, we test this prediction for the association between achievement impacts of education CCT programs and impacts on school enrollment and attendance. Throughout, we attempt to distinguish the various ways in which authors addressed selection into test-taking. Figures 5.5 and 5.6 are scatter plots of standardized math effect sizes (for studies that reported impacts on math scores) against the corresponding attendance and enrollment effects, respectively.[19] As Figures 5.5 and 5.6 indicate, there is substantial variation in both math and language impact estimates and estimates for school attendance and enrollment. For both math and language, we find positive correlations between achievement impact estimates and enrollment and attendance impact estimates, which is consistent with the theoretical prediction. Specifically, the correlation coefficient between math impact estimates and school attendance estimates is 0.09, not statistically significant (Figure 5.5). For math impact estimates and enrollment estimates, the correlation coefficient is 0.42, although not statistically significant.

[19] We assign learning effects and school enrollment and attendance effects to roughly coincide in terms of the schooling level and time periods of exposure.

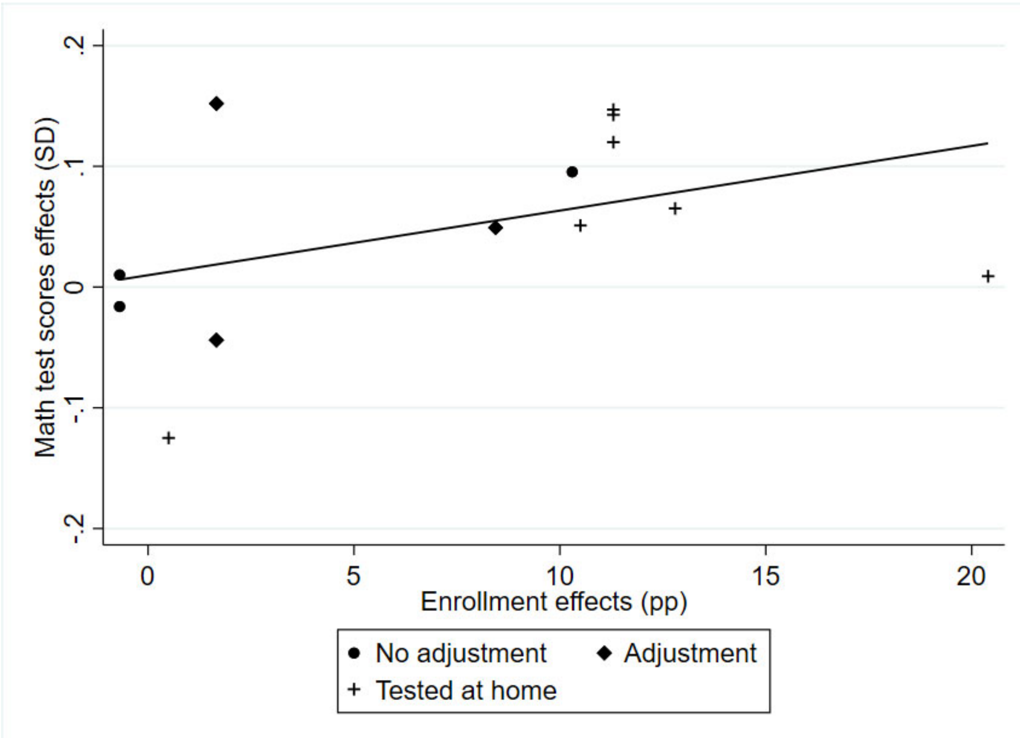
Also consistent with the prediction, although it is not statistically significant, the correlation between language learning and both enrollment and attendance estimates is close to 0.16—both not statistically significant.

Figure 5.5 Scatter plot of math achievement impact estimates and school attendance impact estimates for selected CCT programs



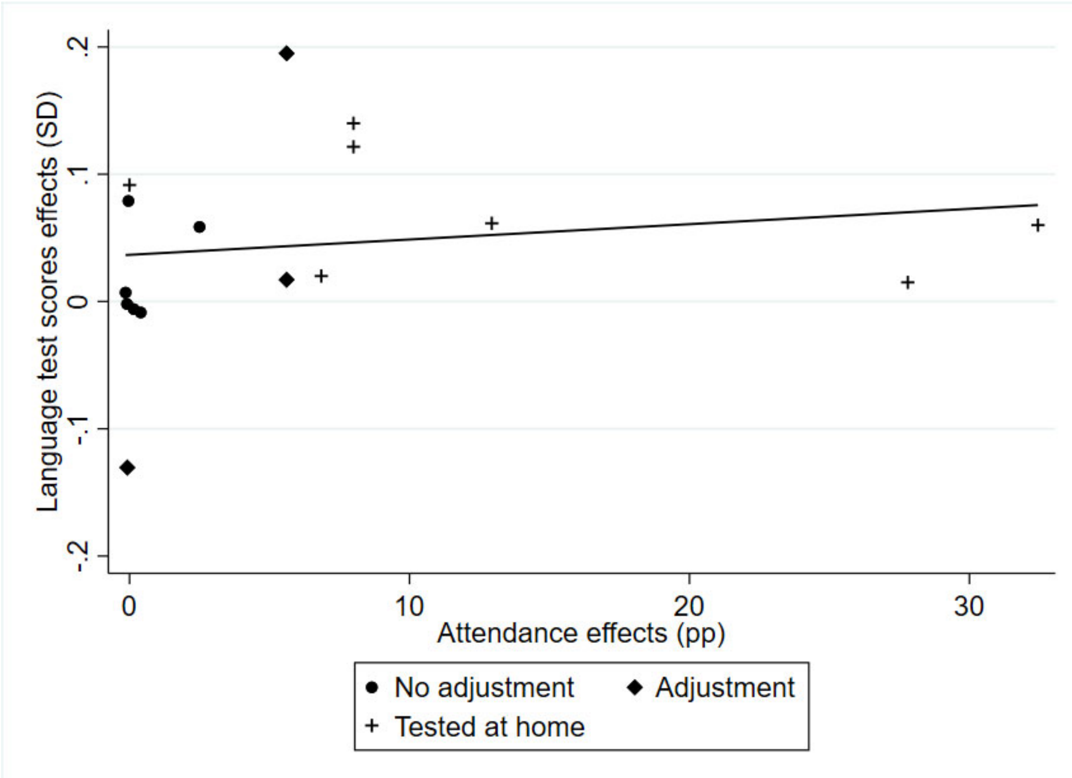
Notes: Math achievement estimates of studies that reported effects on SD (or provided sufficient information to translate estimates into SD units). Attendance effects were matched to roughly coincide in terms of the schooling level and time periods of exposure. The correlation coefficient between between math achievement and attendance estimates is 0.09 (p-value =.72) in the full sample and -0.73 (p-value =.02) among studies testing students at home.

Figure 5.6 Scatter plot of math achievement impact estimates and school enrollment impact estimates for selected CCT programs



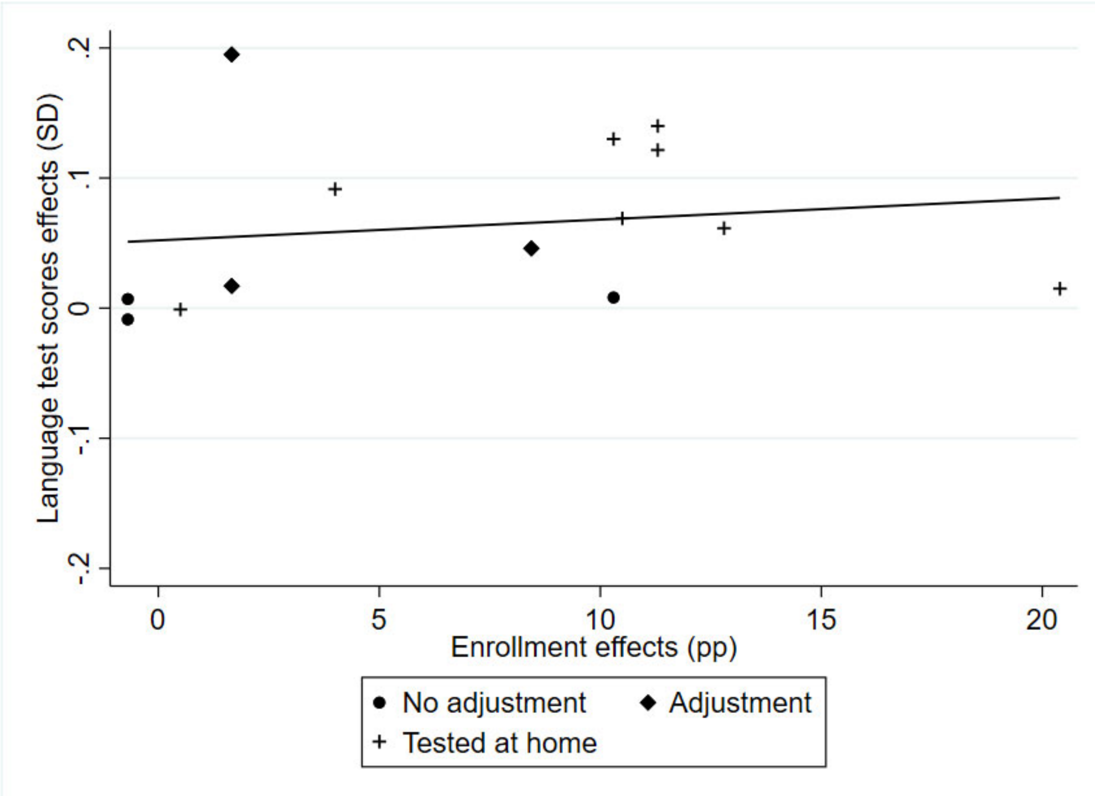
Notes: Math achievement estimates of studies that reported effects on SD (or provided sufficient information to translate estimates into SD units). Enrollment effects were matched to roughly coincide in terms of the schooling level and time periods of exposure. The correlation coefficient between math achievement and enrollment estimates is 0.42 (p-value =.15) in the full sample and 0.46 (p-value =.29) among studies testing students at home.

Figure 5.7 Scatter plot of language achievement impact estimates and school attendance impact estimates for selected CCT programs



Notes: Language achievement estimates of studies that reported effects on SD (or provided sufficient information to translate estimates into SD units). Attendance effects were matched to roughly coincide in terms of the schooling level and time periods of exposure. The correlation coefficient between language achievement and attendance estimates is 0.16 (p-value =.55) in the full sample and -0.47 (p-value =.29) among studies testing students at home.

Figure 5.8 Scatter plot of language achievement impact estimates and school enrollment impact estimates for selected CCT programs



Notes: Language achievement estimates of studies that reported effects on SD (or provided sufficient information to translate estimates into SD units). Enrollment effects were matched to roughly coincide in terms of the schooling level and time periods of exposure.The correlation coefficient between language achievement and enrollment estimates is 0.16 (p-value =.59) in the full sample and 0.05 (p-value =.90) among studies testing students at home.

Taken together, the collection of evidence over two decades of studies on the effects of education CCTs on school enrollment, attendance, dropout, and learning outcomes suggest three main findings. First, in contrast to the substantial effects of these programs on school enrollment, attendance and dropout—particularly for secondary schooling—we cannot conclude that CCTs systematically improve student learning. Our new meta-regression results confirm that overall effect sizes for math and language test score impact estimates are small and not statistically significant. However, we should also caveat the substantial heterogeneity in achievement impacts across studies and programs. A sizable number of studies find null effects, and few even find negative effects on learning. Yet, close to one-third of programs have demonstrated positive effects on learning—and in many of these cases, positive effects emerge after four or five years of program implementation, or even further. Therefore, it may be too early in some CCTs' cycle of implementation to definitively conclude they do not have a positive impact on learning.

Second, there is substantial statistical heterogeneity in math and language effect size estimates across programs. Future research could explore how program characteristics mediate (part of) this heterogeneity. For example, some programs reviewed in this chapter suggest that the type of conditions or the framing of the programs can reinforce student effort and learning results. Similarly, results from individual studies suggest that ensuring minimum supply-side inputs (teachers' attendance, proper infrastructure, adequate class sizes, and teachers' support) can contribute to translating more enrollment and attendance gains into learning. Related to this point, while we find some support for the theoretical prediction based on the conceptual model's assumption that learning is stronger when enrollment and attendance are stronger (particularly for math), estimated correlations in all cases are not statistically significant.

Finally, a new result in the literature stemming from the achievement meta-analysis suggests there is some empirical foundation for greater impact estimates among studies testing children at home. For both math and language, we find greater average effect sizes, on average, for studies testing children at home—consistent with the prediction from the selection model in Section 4.

5.3 Long-term impacts on schooling

In this section, we summarize the accumulated evidence regarding long-term impacts of CCTs on three main schooling outcomes: school attainment (years or grades completed), school completion, and enrollment in tertiary education.

a. School attainment (years or grades of schooling)

Molina-Millan et al. (2019) reviewed the long-term effects of CCTs on two life-cycle transitions: children exposed to nutrition/health CCTs in utero or during their early-childhood, transitioning into school ages; and children exposed during school ages to education CCTs who transition into young adulthood. They included 10 CCT programs from nine countries: Cambodia, Colombia, Ecuador, El Salvador, Honduras, Malawi, Mexico, Nicaragua, Pakistan (some of these are summarized in the learning section). We briefly summarize the evidence for these countries, then add some recent additional studies.

In the case of Mexico's *Progresa*, Behrman, Parker, and Todd (2009, 2011) compared early- and late-treatment groups from the randomized phase-in of *Progresa* of children aged 9-15 years at the start of the program. Authors found a statistically significant differential exposure impact, six years into the program, of 0.2 grades completed (ages 15-21). Adhvaryu et al. (2018) used the same empirical strategy to estimate heterogenous long-term effects, finding that children exposed to drought in the first year of life benefited disproportionately in terms of school attainment from exposure to *Progresa,* highlighting the program's role in offsetting economic disadvantage. Behrman, Parker, and Todd (2011) analyzed attainment impacts in rural areas using difference-in-differences matching and comparing communities from the original impact evaluation with a non-experimental control group of communities not participating in the original randomized impact evaluation. The authors found, on average, an increase of 0.8 additional grades completed (statistically significant), and larger effects with increased length of exposure. Arenas et al. (2015) used data from a nationally-representative survey panel and difference-in-differences matching estimators comparing individuals in communities that received *Progresa* in the early years of the program with those living in communities where the program started later after 2004. The authors found, for individuals aged 18-22 years, differential impacts of 0.5 additional grades attained after seven years of program exposure. Furthermore, Parker and Vogl (2021) used census data and difference-in-differences estimation comparing exposed versus non-exposed cohorts to analyze absolute impacts of *Progresa* after 13 years of program exposure. They found, on average, a statistically significant increase of 1.4 grades attained for those who were 7-11 at the start of the program, with effects larger for males than females.

Molina-Millan et al. (2019) described how García et al. (2012) analyzed long-term impacts on school attainment for Colombia's *Familias en Accion (Rural Version)*. The authors used difference-in-differences to estimate differential impacts on school attainment for children 8-16 years old when exposed to the program. They compared children living in municipalities targeted in the early years of the program (2002) with children living in municipalities where the program started in 2007. The authors found a positive, statistically significant differential exposure effect of 0.6 grades completed after 2-5 years among students in rural areas, but no effect for students residing in the more urban areas of targeted small municipalities.

Barham, Macours and Maluccio (2018a, 2018b) compared early- and late-treatment groups in Nicaragua's *RPS* using the original experimental evaluation design and examined the 10-year impact of for children who were 9-12 years old in 2000 (18-21 years in 2010). They found a positive and statistically significant differential exposure impact of 0.28 additional grades attained for boys (Barham et al. 2018a) but not for girls (Barham et al. 2018b).

Molina-Millan et al. (2020) used the original experimental design in Honduras' *PRAF-II* program to estimate schooling impacts 13 years into the program for individuals exposed at ages 6-13 (then observed at 19-26 years old). In this case, the estimates are absolute program effects because the control group of *PRAF-II* was not phased into the program. The authors found positive and statistically significant effects on grade attainment of close to 0.4 additional grades. Ham and Michelson (2018) also used the experimental design of *PRAF-II* and municipal-level data to examine the impact, comparing children aged 6-12 in 2001, aging to 18-24 in 2013. The authors found a positive and statistically significant effect of 0.3 additional schooling years on individuals who, in addition to the transfers, benefited from living in communities that also received supply components. In communities without the supply component, the authors found a non-statistically significant impact of 0.1 additional schooling years.

For Cambodia, Filmer and Schady (2014) used a regression discontinuity design to measure schooling impacts after five years of program exposure. They found a statistically significant increase in grade attainment of 0.6 years. Similarly, Baird, McIntosh and Özler (2019) used the experimental design of Malawi's *Zomba Cash Transfer Program* to find a statistically significant increase of 0.6 grades completed two years after the program ended for girls not enrolled in school at baseline, but smaller effects for girls already enrolled at baseline. In this case, the control group did not receive transfers, therefore, effects are absolute program impacts.

Sanchez-Chico et al. (2018), included in Molina-Millan et al. (2019), used a regression discontinuity design to estimate schooling impacts of El Salvador's *Comunidades Solidarias Rurales*. The authors found, after six years of program exposure (including in utero exposure for the younger cohorts), a positive impact of 9 percentage points on the probability of completing at least one year of school.

We include in this review three studies not included in Molina-Millan et al. (2019) analyzing long-term impacts of CCTs on school attainment for three different programs: Costa Rica's *Avancemos*, Peru's *Juntos*, and the Philippines' *Pantawid Pamilyang Pilipino Program* (see Appendix Table A6). Meza-Cordero et al. (2015) for *Avancemos* used household survey data and matching difference-in-differences estimators comparing schooling trends among observationally similar children in treated households relative to those in non-treated households. The authors found that four years of exposure to *Avancemos* led to a statistically significant increase of 0.5 years of schooling. After nine years of exposure, the author reported a statistically significant increase in schooling of 0.77 years among beneficiary children.

In *Juntos*, Gaentzsch (2020) used panel survey data, taking advantage of the program's staggered geographic phase-in. The author used difference-in-differences matching and found a statistically significant impact of 0.32 additional years of schooling after four years among children initially exposed in secondary school, but no impact for those initially exposed in primary school.

For the Philippines's *Pantawid Pamilyang Pilipino Program*, Orbeta, Melad, and Araos (2021) used the original experimental design to compare early- versus late-treatment groups six years after program implementation. The authors found a differential impact of 0.07 additional grades of schooling, not statistically significant.

In sum, evidence of CCTs' long-term impacts on school attainment is positive for all except among primary school children in Peru's *Juntos*. Most studies providing evidence on the effect of CCTs on years of schooling to date found long-term positive and statistically significant effects—suggesting a systematic positive pattern for the effects of CCTs on school attainment. As with other schooling outcomes, there is considerable variation in estimates across programs.

b. *School completion*

Garcia and Saavedra (2017) meta-analyzed 10 studies estimating the impact of CCTs on school completion. Cambodia's *CSP* pilot (merit and poverty targeting treatments), Colombia's *Subsidios Condicionados Bogota* (basic, savings, and tertiary treatments) and *Familias en Accion (Rural Version)*, Tanzania's *Community-Based Conditional Cash Transfer* pilot program, Argentina's *PNBE*, and Pakistan's *Punjab Female School Stipend Program*. They found an overall statistically significant effect size on school completion of 3.18 percentage points, with considerable variation in effect sizes, ranging from 0.7 percentage points (not statistically significant) for Bogota's *Subsidios* program after seven years of program implementation. to a statistically significant effect of 12 percentage points for Cambodia's *CSP* (merit targeting) after three years of program implementation. Only four of the 11 effect sizes included were statistically significant: Cambodia's *CSP* merit and poverty treatments after three years of program implementation; Colombia's *Subsidios* basic treatment and *Familias en Accion (Rural Version)*.

Molina-Millan et al. (2019) reviewed additional, more recent, studies for Colombia's *Familias en Accion (Rural Version)*, Honduras' *PRAF-II,* and Ecuador's *BDH*. Garcia et al. (2012) saw a 0.9

percentage points increase, not statistically significant, in the probability of completing secondary school graduation 10 years into *Familias en Accion (Rural Version)*. In contrast, Duque et al. (2019) found for this program after 14 years a statistically significant effect on secondary school completion of 17%. Ham and Michelson (2018), among individuals exposed to *PRAF-II* combined transfers and supply-side intervention, found a 2.9 percentage points increase in secondary school completion after 10 years. Among beneficiaries who only received transfers, they found a non-statistically significant effect of 1.6 percentage points increase in secondary school completion. Araujo, Bosch and Schady (2019) found that Ecuador's *BDH* increased the probability of secondary school completion by 1.5 percentage points, statistically significant, after 10 years of program implementation, but found no effect for primary school completion.

There are three additional studies for programs not included in Garcia and Saavedra (2017) or Molina-Millan et al. (2019) that analyzed long-term effects of CCTs on school completion: Peru's *Juntos* (Gaentzsch 2020), Argentina's *AUH* (Edo and Marchionni 2019), and the Philippines' *Pantawid Pamilyang Pilipino Program* (Orbeta, Melad, and Araos 2021). These studies are described earlier in this section for other outcomes, so here we limit to highlighting their results on school completion.

Gaentzsch (2020) found a statistically significant seven percentage point increase in primary school completion for *Juntos* after four years of program implementation, and a marginally statistically significant increase of nine percentage points in the probability of transitioning to secondary school. For Argentina's *AUH* after six years of program implementation, Edo and Marchionni (2019) reported among children fully exposed to transfers through primary school (12-14 at endline) a statistically significant increase in the probability of secondary school completion of 1.3 percentage points among boys and of 2.9 percentage points among girls. For children only partially exposed to transfers through primary school (15-17 at endline), they found for boys a statistically significant increase in primary school completion of 1.9 percentage points, and a non-statistically significant effect of 0.5 percentage points for girls. For the Philippines' *Pantawid Pamilyang Pilipino Program*, Orbeta et al. (2021) found a small and non-statistically significant effect for primary school completion of 0.7 percentage points after six years of implementation.

Overall, evidence of CCTs on school completion indicates consistent positive effects on primary and secondary school completion. However, in many cases, the effect sizes are small and not statistically significant. It may be too early to observe, however, completed secondary school transitions in many programs.

c. *Tertiary enrollment*

Precisely because beneficiaries in many programs have not fully completed their transition through secondary school, there are few studies reporting CCT impacts on tertiary enrollment. Most studies to date are included in the Molina-Millan et al. (2019) review (see Appendix Table A8).

Only three of 11 studies found positive and statistically significant effects on tertiary enrollment: Colombia's *Subsidios a la Asistencia Escolar*, savings and tertiary treatments; and Honduras' *PRAF-II*. For Colombia's *Subsidios*, Barrera-Osorio, Linden, and Saavedra (2019) used the program's randomization design combined with administrative data on tertiary enrollment eight and 12 years after the start of the program (for those enrolled in upper secondary and lower secondary education, respectively). The authors found both the savings and tertiary treatments have a positive significant impact on tertiary enrollment: 5.7 percentage points increase for tertiary treatment (20% relative to base) and 1.5 percentage points increase for savings treatment (10% relative to base), individually marginally significant, jointly significant at conventional levels. In the case of Honduras's *PRAF-II* program, Molina-Millan et al. (2020) used the original randomization design of the program's impact evaluation to find a significant increase in the probability of enrollment in tertiary education of more than 50% relative to base after 13 years of program implementation.

The other studies—on Colombia's *Familias en Accion (Urban Expansion)* (Attanasio et al. 2021; Garcia et al. 2012) and *Subsidios*, basic treatment (Barrera-Osorio, Linden, and Saavedra 2019); Mexico's *Progresa* (Parker and Vogl 2021); and Israel's *Achievement Awards* (Angrist and Lavy 2009)—found positive but mostly non-statistically significant effects on tertiary enrollment. Attanasio et al. (2021) found a non-statistically significant effect of 0.9 percentage points in tertiary enrollment after eight years of exposure to *Familias en Accion (Urban Expansion)* among students in the city of Medellin, Colombia. They also found heterogeneous effects by gender: a marginally statistically significant effect of 1.7 percentage points for males and close to 0 for females. Barrera-Osorio, Linden, and Saavedra (2019) found a non-statistically significant effect of 1 percentage point for Colombia's *Subsidios*, basic treatment, after eight years of program implementation. Parker and Vogl (2021) found a non-statistically significant effect of 1.7 percentage points in the probability of attaining some tertiary education after 13 years of Mexico's *Progresa* implementation. Angrist and Lavy (2009) found a non-statistically significant effect of 5.5 percentage points in tertiary education enrollment after two years of implementation of Israel's *Achievement Awards*.

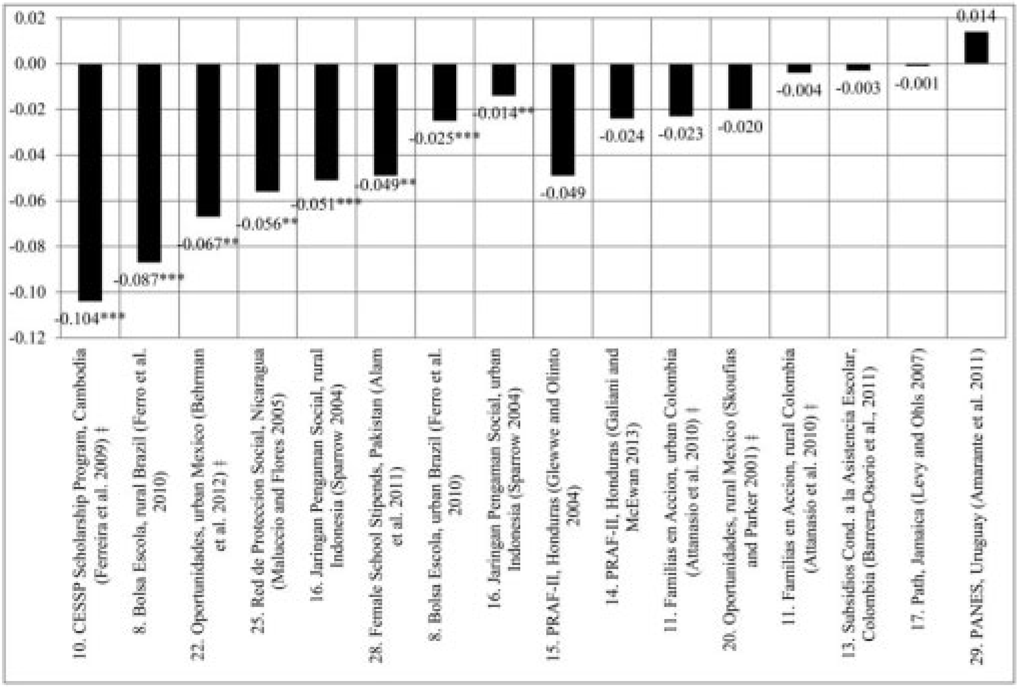
Overall, evidence accumulated on the long-term educational impacts of CCTs suggests these programs likely increase school attainment and completion. Most programs with available evidence demonstrate having a positive effect on years of schooling and the probability of school completion. However, there is a large variation in effects across programs, and some have no effect on long-term schooling attainment or completion. Evidence on the effects of CCTs on tertiary enrollment is less compelling. Fewer studies are available for impacts on this outcome, and most show positive effects, although many of these are not statistically significant. There are three, not mutually exclusive, likely explanations for these findings. First, CCTs increase years of schooling and school completion but the lack of learning improvements might preclude students from effectively transitioning into tertiary education. Second, it still may be too early to see the full cycle of effects on schooling for the children who benefited from CCTs early in their schooling trajectories, many of whom have not still graduated from secondary school. Third, heterogeneity in effect sizes on tertiary enrollment could be explained by variation in program characteristics. Specifically, the three programs demonstrating impact on tertiary enrollment so far deviate from the standard CCT design in the type of conditionality, payment structure or supply-side supplementation. For example, the tertiary treatment of Colombia's *Subsidios* has an explicit monetary incentive and conditionality attached to tertiary enrollment; *Subsidios*' savings treatment has a lump-sum payment upon enrollment in the following school year; and *PRAF-II* provides a supply-side transfer to schools. Given the small number of studies available, it is hard to draw robust conclusions as to whether design characteristics explain variation in longer-term educational attainment impacts of CCTs. Future research can help inform this conjecture.

5.4. Evidence on the role of opportunity costs as a barrier to schooling: child labor

As mentioned in Section 3, CCT programs reduce the opportunity cost of schooling relative to child labor not only through an income effect but also through a substitution effect by imposing schooling conditions based on enrollment and attendance. Accordingly, one direct empirical prediction is that, in the short run, CCTs should reduce child labor. In this section, we summarize the empirical evidence for the effects of CCTs on child labor.

In their review of CCT literature, Fiszbein and Schady (2009) found these programs consistently reduced child labor. More recently, Hoop and Rosati (2014) systematically reviewed the empirical evidence on impacts of unconditional and conditional cash transfers, including those in Fiszbein and Schady (2009). They found that across the sample of studies reviewed, CCTs lower both the extensive and intensive margin of child labor. Figure 5.9 replicates De Hoop and Rosati's (2014) summary figure for the effects of CCTs on child labor participation. Out of the 16 studies estimating CCT effects on child labor participation included in their review, half found statistically significant reductions in child labor, seven found non-statistically significant reductions, and only one reported a non-statistically significant increase in child labor, for Uruguay's *PANES* (Amarante et al. 2011). De Hoop and Rosati's (2014) emphasized, as is the case for all other outcomes reviewed in this chapter, there is substantial variation in child labor impact estimates across programs. Among the programs with statistically significant effects, impact estimates ranged from a 10.4 percentage point reduction in child labor for Cambodia's *CESSP* (Ferreira, Filmer, and Schady 2017) to a 1.4 percentage point reduction for Indonesia's *JPS* (Sparrow 2004). Among programs reporting not statistically significant reductions, impact estimates ranged from a 4.9 percentage point reduction for Honduras' *PRAF-II* (Glewwe and Olinto 2004), to a 0.1 percentage point reduction for Jamaica's *PATH* (Levy and Ohls 2007).

Figure 5.9. Impact estimates for child labor



Source: Authors' own work.

Note: Change in the probability of working as a result of the conditional cash transfer programs displayed on the horizontal axis. ‡ indicates that the estimate is a weighted average of multiple age and gender groups. To minimize the text on the horizontal axis, we only display the first author of the study if the study has more than 2 authors. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Figure reproduced from De Hoop and Rosati (2014)

De Hoop and Rosati (2014) classified Ecuador's *BDH* as unconditional because conditions in that program were loosely informed and enforced, which is why its estimate for child labor does not appear in Figure 5.9. We consider *BDH* a CCT because it does require schooling conditions for payments, even though conditions are not strictly enforced. *BDH* substantially reduced child labor by 17 statistically significant percentage points—considerably larger than the largest reduction in child labor reported in De Hoop and Rosati's (2014) figure for Cambodia's *CESSP*. Among children ages 11-16 in *BDH*, the reduction in child labor was even stronger, of 25 statistically significant percentage points (Edmonds and Schady 2012).

De Hoop and Rosati (2014) also summarized evidence from six studies reporting CCT impacts on children's weekly hours worked. However, as the authors noted, all these studies set weekly hours worked to zero for children not working, which means reported estimates from these studies are impact estimates on a combination of extensive and intensive labor force participation. Out of the six programs, four led to substantial reductions in children's weekly work hours: Nicaragua's *RPS* and *Sistema de Atencion a Crisis*, Colombia's *Familias en Accion (Urban Expansion)*, and Cambodia's *CESSP*. There is substantial variation in estimates for children's hours worked, ranging from a statistically significant reduction of 3.6 hours/week for

Nicaragua's *RPS* (Gee 2010) to a non-statistically significant reduction of 0.91 hours/week for *Oportunidades* (Skoufias and Parker 2001) and a non-statistically significant increase of 0.07 working hours/week for Colombia's *Familias en Accion (Rural Version)* (Attanasio et al. 2010).

Impact estimates in Figure 5.9 are from studies that analyzed basic CCTs schemes (cash transfers offering regular payments conditional on school enrollment or attendance). De Hoop and Rosati (2014) noted, however, that some variations in program design have demonstrated differential child labor effects. For example, Barrera-Osorio et al. (2011) found the basic treatment and savings treatment of Colombia's *Subsidios* had similar reductions in child labor, but the tertiary treatment led to significantly larger reductions in child labor than the other two treatments.

In another example of a CCT program with design deviations from the basic structure, Brazil's *PETI* explicitly aims to reduce child labor by conditioning cash payments on a child's participation in after-school programs. As Hoop and Rosati (2014) noted, this program reduced child labor overall, although with substantial variation across regions. In some areas, the program reduced the extensive margin of child labor by 5 percentage points, while in others it reduced it by 25 percentage points (Yap, Sedlacek, and Orazem 2009).

Gender is another dimension of impact effect heterogeneity in child labor estimates. De Hoop and Rosati's (2014) review argued most studies of the impact of CCTs on child labor reported statistically significant impacts only for boys. As the authors discussed, this null effect on girls' child labor may be masking effects on unpaid labor. Consistent with this hypothesis, De Hoop and Rosati (2014) found evidence from three studies that disaggregated impacts on child labor across work activities, suggesting that among boys, child labor reductions are mainly the result of reductions in work for pay, while reductions in child labor among girls are primarily for unpaid work such as household chores, work at home, and other unpaid economic activities. The review also argued that child labor impact estimates do not differ systematically across age groups.

Finally, De Hoop and Rosati (2014) investigated the association between child labor impact estimates and those for school enrollment. De Hoop and Rosati (2014) noted that among the eight studies finding a significant reduction in child labor, six also found a significant increase in school participation (enrollment or attendance). In addition, of the seven studies finding no significant reduction in child labor, only two also found a significant increase in school participation. The authors estimated that a 1 percentage point increase in school participation is associated with a 0.31 percentage point reduction in child labor. These findings are consistent with the conceptual framework's prediction that CCTs reduce the opportunity cost of school enrollment and attendance relative to child labor. However, that the reduction is less than one-for-one suggests leisure might be another margin of adjustment in terms of children's time allocation.

In sum, there are three main findings from the consolidated evidence of the impacts of CCTs on child labor. First, consistent with substantial effects of CCTs on increasing school enrollment and attendance, there is robust evidence CCTs contribute to reducing child labor. Second, and also consistent with effects of CCTs on schooling, there is substantial variation in effect size estimates across programs. Further research is needed to better understand the extent program characteristics explain such variation. Third, as predicted by the conceptual framework, reductions in child labor are statistically associated with increases in school participation, highlighting how CCTs contribute to reducing the opportunity cost of schooling.

5.5 Does variation in program design mediate impact estimates of education CCT programs?

Two of the strongest empirical regularities in the education CCT literature are heterogeneity in schooling impact estimates and in program design. To what extent do design characteristics mediate impact estimates across programs and studies?

The conceptual framework predicts an association across design aspects and schooling impacts. For instance, a greater transfer amount should lead to greater increases in human capital production through increased school enrollment and attendance, all else constant.[20] The meta-analytic evidence from Garcia and Saavedra (2017) does not support this prediction. All else constant, transfer amounts are not statistically correlated to effect sizes for any outcome analyzed (which, due to data limitations, includes only primary and secondary enrollment and attendance). As the authors noted, this finding, while inconsistent with the theoretical prediction, is consistent with other systematic review evidence on CCTs from Baird et al. (2014) and Snilstveit et al. (2015), and with single-CCT program evidence from Cambodia and Malawi. In Cambodia's *CESSP*, increases in school enrollment are not related to transfer size (Filmer and Schady 2014). In Malawi's CCT program, impacts on educational outcomes, such as dropout, do not vary with transfer amounts (Baird, McIntosh and Özler 2009).

The second key prediction from the conceptual framework is that educational program impacts, particularly on school enrollment and attendance, should be stronger in settings with low levels of enrollment at baseline, low levels of participant's household income, and in settings with excess school capacity in which supply constraints are not binding. Garcia and Saavedra (2017) found strong evidence in support of this prediction, particularly for school attendance. Enrollment and attendance impact estimates are smaller in settings with high baseline enrollment. As the authors noted, for both primary and secondary attendance impact estimates, the (negative) association with baseline enrollment is statistically significant. This is consistent with prior systematic review evidence on CCTs (Fiszbein and Schady 2009). It also is consistent with evidence from Mexico's *Oportunidades*, in which school enrollment impacts are larger in areas with better school infrastructure and lower pupil–teacher ratios (Behrman, Parker, and Todd 2008). Further reinforcing that point, Garcia and Saavedra (2017) found that impact estimates for primary enrollment and attendance are larger in programs that, all else constant, complement cash transfers to families with supply-side interventions such as school grants, or direct payments to parent–teacher associations or teachers. More recent evidence from long-term impact effects of Honduras' *PRAF-II* further supports the importance of supply-side incentives. In Ham and Michelson's (2018) long-term evaluation of Honduras' *PRAF-II* exposure to supply-side incentives in addition to transfers had significant and sustained effects on school attainment (both years of schooling and secondary school completion) compared to only transfers. However, both Garcia and Saavedra (2017), and Baird et al. (2014) suggested levels of school enrollment at baseline are not associated with enrollment effect sizes.

[20] Study characteristics included in Garcia and Saavedra include: study quality and publication year; program characteristics include whether the program is a pilot or national-scale program; school conditionality; baseline enrollment; whether the program offers a supply-side supplement; whether the payment is provided to mother; payment frequency; years of program exposure; and whether the program is located in Latin America.

The third empirical prediction from the conceptual framework is that more stringent attendance and achievement conditions lead to, all else constant, steeper reductions in the marginal cost of children's and parents' time, leading to extra time devoted to schooling activities (e.g., greater attendance or effort). Therefore, more stringent schooling conditions should be associated positively with enrollment and attendance impact estimates. Garcia and Saavedra (2017) found some evidence consistent with this prediction. Specifically, they showed that all else constant, effect sizes for primary school attendance are statistically significantly larger when, in addition to school enrollment, other conditions are imposed on beneficiaries such as grade promotion or test scores. However, for primary and secondary enrollment, and for secondary attendance, the authors did not find a consistent pattern of association between estimates and programs imposing additional schooling conditions. Supporting this finding is earlier meta-analytic evidence from Baird et al. (2014). Their examination whether school enrollment effect sizes varied according to the intensity of schooling conditions (from an unconditional cash transfer to a CCT with monitoring and enforcement) found that effect sizes increase as the intensity of the conditionality increases. In particular, CCTs with the strongest intensity—in which conditions are monitored and enforced—have, on average, larger effects on enrollment than programs in which schooling is not monitored.

Fourth, in a single-period model like the one presented in Section 3, there is no distinction between increases in permanent income or transitory income. However, in a model with multiple time periods, standard consumption theory distinguishes between the expected behavioral effects from a rise in permanent income versus that from a rise in current income. Some national, well-established programs likely raise permanent income, while other programs in a pilot stage do not. Therefore, all else equal, programs raising permanent income should have larger educational impacts in the short and long run than those raising only transitory income. However, Garcia and Saavedra (2017) did not find evidence consistent with this prediction. As the authors noted, educational effect size estimates from national CCT programs are statistically indistinguishable from those of pilot CCT programs, all else constant. This result is consistent with prior CCT review evidence from Baird et al. (2014).

Fifth, the basic framework assumed a household with unitary preferences. This implies the household is assumed to maximize a single welfare function, without accounting for potentially divergent preferences between the adult male and mother of the household, or between parents and children. Therefore, the model, under its unitary assumption, predicts similar educational impacts regardless of which member of the household receives the transfer. To the extent that targeted households instead behave according to a bargaining model, and the mother's objectives are more closely aligned with those of children in the household, distributing transfers to mothers should lead, all else equal, to greater human capital investments in children. But Garcia and Saavedra (2017) did not find evidence in support of this prediction. The lack of association between transfer recipient and educational impact estimates reported in Garcia and Saavedra (2017) is consistent with earlier meta-analytic evidence from Baird et al. (2014), as well as with single-program experimental evidence from Morocco's *Tayssir* program (Benhassine et al. 2015). However, it is inconsistent with the meta-analytic evidence reported in Snilstveit et al. (2015), who found a statistical association between effect size estimates and transfer recipient.

Sixth, the basic framework collapses household decision-making into a single period. Thus, it cannot shed light on intertemporal choices stemming from constraints in liquidity, credit, or savings. In the single-period model, for example, whether households receive a dollar-equivalent payment in monthly or bimonthly installments makes no difference. In practice, though, households may face money problems that limit the optimal allocation of resources. For example, if households face savings constraints while expenditures on goods and services that contribute to human capital production are "lumpy," then the timing and frequency of payments may make a difference. Therefore, Garcia and Saavedra (2017) predicted that, all else constant, payment structure and frequency would be related to CCT program impacts if households face credit or savings constraints for investments in human capital production. However, both meta-analyses by Garcia and Saavedra (2017) and by Baird et al. (2014) found that frequency of payments is not statistically associated with educational effect size estimates.

One additional prediction not explored in Garcia and Saavedra (2017) is that, under the key model assumption that human capital only depends on time devoted to schooling, increased school attendance should increase human capital acquisition—achievement and attainment. So, the model predicts stronger impacts on achievement and attainment for programs with stronger impacts on school enrollment and attendance. Those authors did not include learning outcomes in their review. However, in this chapter, we can include the broadest available set of studies reporting achievement impacts of education CCTs. But as discussed in subsection 5.3, we find evidence, particularly for math, that is consistent with the direction of the prediction, although not statistically significant for effects on either math or language.

On the whole, the available meta-analytic evidence (e.g., Garcia and Saavedra 2017; Snilstveit et al. 2015; Baird et al. 2014, Fiszbein and Schady 2009) gives mixed support for the predictions derived from the conceptual framework in Section 3.

5.6 Long-term impacts of CCTs on employment and earnings

This section draws primarily on Molina-Millan's et al. (2019) review of long-term impacts of CCTs. We organize the evidence according to whether studies estimate differential or absolute program impacts.

a. *Impacts of differential exposure*

Molina-Millan et al. (2019) began by describing labor market impacts of *Progresa* based on evidence by Behrman, Parker, and Todd (2009, 2011). Differential exposure, in this case, is an 18-month differential exposure for youth who are 15-21 at endline. Combining a baseline survey conducted in 1997 with follow-ups of the evaluation panel rounds of the linked household panel evaluation survey through 2003, the authors reported that differential exposure to *Progresa* decreased labor force participation among males by 2.7 statistically significant percentage points (from a base participation rate of about 63%)—but did not significantly affect the labor force participation of females. Molina-Millan et al. (2019) argued that males' delayed labor market entry is consistent with their increased educational attainment. Meanwhile, for females, the lack of program impact on labor force participation is likely the result of low reported base rates of labor force participation (around 25%).

In another study reviewed by Molina-Millan et al. (2019), Arenas et al. (2015) used nationally-representative panel survey data and the program's national, non-random geographic phase-in to look at labor market outcomes in 2005 when participants were 18-22 years of age. The authors compared participant outcomes in the initial treatment regions, which first received transfers in 1997, to those in regions receiving transfers starting in 2004, corresponding to a differential program exposure of seven years. The authors found this differential exposure significantly increased labor force participation by about 8 percentage points (from a participation base of about 50%), although it did not affect hours worked or hourly labor earnings. Molina-Millan et al. (2019) argued this pattern of results possibly reflects lower effective labor market experience because of increased educational attainment among participants in the early-entry group combined with the fact some participants in the early-entry group were still enrolled in tertiary education.

Barham, Macours and Maluccio (2018a, 2018b), also included in the Molina-Millan et al. (2019) review, estimated labor market impacts of differential exposure to Nicaragua's *RPS*. Recall that in this CCT program, randomly-selected early-treatment groups received transfers starting in 2000 for three years, while late-treatment groups entered in 2003 and received transfers through 2005, when the program ended. Barham, Macours and Maluccio (2018a) focused on males who were 9–12 years old in 2000 (18–21 years old at endline). Their estimate of impact, therefore, rather than representing differential exposure to program duration as in the case of *Progresa* studies, represented the impact of initial exposure, for a fixed three-year period, at different ages: 9-12 years of age for the early-treatment group versus 12-15 years of age for the late-treatment group. Using data from a 2010 endline survey, the authors found that those exposed early are less likely to work in agriculture and have non-agricultural earnings that are up to 10% larger, likely as a result of increased educational attainment and learning. Barham, Macours and Maluccio (2018b) conducted a similar analysis for females, finding that by the age of 18-21, females in the early treatment group have higher rates of labor force participation and higher income, on average, than those in the late-treatment group. Unlike the findings for males, however, early-treatment females did not score higher on achievement tests than those in the late-treatment group. Instead, the authors attributed improvements in labor market outcomes to delayed sexual activity and lower teen fertility among early-treatment females.

Ham and Michelson (2018), included in Molina-Millan et al. (2019), combined randomization data with municipal-level population data up to 10 years after the start of Honduras' *PRAF-II,* by the time participants were 18-24 years of age. They found that differential exposure of two years to the transfers alone did not affect labor force participation of males or females. However, two years of transfers plus three years of supply side incentives increased labor force participation, particularly females.

b. *Impacts of absolute exposure*

Behrman, Parker, and Todd (2011), included in the Molina-Millan et al. (2019) review, reported absolute employment and earnings impacts of *Progresa* by comparing beneficiaries in the original experimental sample communities with observationally matched non-participants in communities that, six years into the program, had not been selected to participate. At endline in 2003, males 15-16 years old were 14 percentage points (from a base of about 45%) less likely to be in the labor force because males still were enrolled in school. For older males, ages 19-21, there was no change in overall labor force participation, although the authors observed a substitution away from agricultural work. In contrast, for older females, ages 19-21—a group for whom there was no increase, on average, in educational attainment—the authors observed a 6 percentage point increase in labor force participation (from a base of about 30%).

Parker and Vogl (2021, an earlier version of which was included in the Molina-Millan et al. 2019 review) estimated absolute program impacts 13 years into *Progresa* using Mexico's 2010 population census. The authors combined geographic variation stemming from the non-random municipal-level program rollout across the country with cohort level variation in exposure, as children ages 7–11 at the start of the program in 1997 fully benefited from the schooling transfers whereas those ages 15–19 were too old to receive them. At the time of the census, earlier cohorts fully exposed were aged 20-24 years old. The authors found that males who were fully exposed increased their formal labor sector participation and reduced their participation in agriculture—but, overall, saw no increases in total monthly earnings. In contrast, the authors found a substantial increase in the labor force participation of females by about 5-9 percentage points (from a base of about 25%) and in their earnings by about 40%, which they attribute to pecuniary benefits accruing from increased educational attainment. However, the authors suggested caution in the interpretation of the gender differences due to differential sample selectivity between males and females. Males, in particular, were 16% less likely than women to appear in the analysis sample. While the authors warned that this gender imbalance is uncorrelated with program exposure and with cohort size, it could be the result of differential migration and homicide risk.

Molina-Millan et al. (2020) combined municipal-level randomization in Honduras' *PRAF-II* program with repeated cross-sections of national household surveys collected long after the program's 2000 start date to measure labor market impacts when participants were 16-23 years of age (10 years into the program) and up to 22-29 years of age (16 years into the program). The authors found no evidence that the program affected labor force participation or earnings among males. Among females, the authors found that those in primarily non-indigenous villages saw a decrease in labor force participation of 12 percentage points (from a base 40%) and lower total earnings, consistent with a greater proportion of them still in school.

Araujo, Bosch, and Schady (2019) estimated absolute program impacts of Ecuador's *BDH* 10 years into the program, when participants were 19-25 years of age. Employing a regression discontinuity research design, the authors found no evidence that the program increased labor force participation of males or females, despite modest increases in secondary school completion. Using a similar research design to analyze the impacts of Cambodia's *CESSP* five years into the program, when participants were 19 years of age on average, Filmer and Schady (2014) found no significant impacts on labor force participation or earnings.

Baird, McIntosh and Özler (2019) reported the effects of Malawi's pilot program that targeted adolescent girls. Combining the original experimental design with survey endline data when participants were between 18-27 years old, the authors showed that the two-year CCT had no discernible impact on labor force participation or earnings despite the program's positive effect on educational attainment.

In sum, the evidence to date on long-term labor market outcomes is mixed. There appears to be, across a variety of programs, a gender pattern in the effects on labor force participation and transitions out of agricultural work, but it is unclear whether this is the result of differential effects on educational attainment or other factors. At the same time, the evidence focuses on labor market outcomes of very young adults—in their late teens and early twenties, typically, many of whom have not yet completed their schooling and thus have not fully transitioned into the labor market. Therefore, fully assessing the labor market impacts of CCT programs and the implications for intergenerational mobility will require an even longer view in future research.

5.7. Impact of CCTs on other factors associated with long-term human capital: teen pregnancy and early marriage

In this section, we summarize the evidence of CCT impacts on teen pregnancy and early marriage. Teen pregnancy and early marriage are directly linked to increased risk of school dropout and, therefore, less human capital accumulation in the long run. At the same time, impacts in school completion can be the result of changes in attitudes towards schooling and behaviors such as pregnancy or early marriage.

a. *CCT impacts on teen pregnancy*

Compared to the accumulated evidence of CCT's impacts on schooling or child labor, there is less empirical evidence regarding impacts on teen pregnancy. However, the limited evidence suggests that CCTs reduce pregnancy in adolescents.

Molina-Millan et al. (2019) identified three studies that included impacts on adolescents' fertility, in addition to reporting long-term schooling impacts. Barham, Macours and Maluccio (2018b) found differential effects of Nicaragua's *RPS* on fertility. Girls who received transfers earlier had lower fertility than those receiving them later in their life cycle. Baird, McIntosh and et al. (2018) found that for females not enrolled in school at baseline, participation in Malawi's pilot program reduced by 4 percentage points the likelihood of teen pregnancy, but among females enrolled in school at baseline there was no such effect on teen fertility. Filmer and Schady (2014) found no effect on female teen fertility in Cambodia's *CESSP*.

Olson, Clark and Reynolds (2019) used a difference-in-differences estimation approach to comparing treated and non-treated cohorts of Brazil's *Bolsa Escola*. They found the program reduced female teen pregnancy by 3 percentage points after five years of program implementation, representing a 10% reduction. Darney et al. (2013) used nationally-representative data and matching estimators to analyze the impact of Mexico's *Oportunidades* on adolescent and young female fertility, finding a non-statistically significant reduction in fertility among them of 26%.

Cortés et al. (2016) used survey data on adolescents' sexual behavior in the city of Bogota, Colombia and difference-in-differences estimators to estimate impacts on teen fertility of two CCT programs: *Familias en Accion (Urban Expansion)* and *Subsidios* (Barrera et al. 2011). For *Subsidios*, the authors found a statistically significant reduction in teen fertility of 2 percentage points. In contrast, for *Familias en Accion (Urban Expansion),* they did not find evidence the program reduces teen pregnancy. The authors argued that these differential effects are driven by differences in program characteristics: *Subsidios* conditions payments on school progression in addition to school attendance, and students who do not comply with conditions lose the subsidy from then on; *Familias en Accion (Urban Expansion)*, meanwhile, has conditions only related to school attendance, and students who do not comply with them only lose payments for one year.

Attanasio et al. (2021) used regression discontinuity design and rich administrative data to estimate the impact of *Familias en Accion (Urban Expansion)* in the city of Medellin. The authors found that the program reduces teen pregnancy by 2.3 percentage points, representing an 8.5% reduction. One explanation for the difference in findings with Cortés et al. (2016) is that there is substantial impact effect heterogeneity across contexts, and the city of Bogota may differ from the city of Medellin in ways related to program impact on teen fertility.

In sum, the limited evidence available suggests CCTs reduce female adolescent fertility. Most studies reported a statistically significant reduction of teen pregnancy, but some non-statistically significant impacts. Given the small number of studies, it is hard to draw robust conclusions. Also, effect sizes vary across programs and contexts, and some program characteristics, such as conditioning on school progression, appear to mediate variation in teen pregnancy effect sizes. Future research can shed light on this issue.

b. *Early marriage*

Molina-Millan et al. (2019) described three studies reporting effects of CCT's on early marriage. Filmer and Schady (2014) found no effect on marriage in Cambodia's *CESSP,* and Alam, et al. (2011) found no evidence of Pakistan's *Punjab Female School Stipend Program* on the probability of marriage. In contrast, Baird, McIntosh and Özler (2019) found that in Malawi's pilot program, participating females who were not enrolled in school at baseline saw a statistically significant reduction in early marriage of 11 percentage points—but for females enrolled in school at baseline, there was no effect.

For Mexico's *Oportunidades*, Behrman, Parker, and Todd (2008) estimated impacts on early marriage by comparing early- and late-entry groups, resulting in estimates of differential exposure to the program. The authors did not find evidence across the females' age distribution of the program leading to a significant reduction in female adolescent marriage, although for some specific age groups they found the program reduced early marriage.

Cahyad et al. (2020) reported short- and medium-term impacts on early marriage for Indonesia´s *Keluarga Harapan*. The authors used instrumental variables estimation to find a non-statistically significant reduction of 2.6 percentage points and 1.2 percentage points, respectively, in the probability of early marriage after two and six years of program implementation for females 16-17 at endline.

Finally, for India's *Kanyashree Prakalpa*, Dey and Ghosal (2021) used difference-in-differences estimation, exploiting geographic variation in eligibility and comparing treated and non-treated cohorts, to analyze the program's impact on early marriage. The authors found that, after three years, the program led to a statistically significant reduction of 7 percentage points in the probability of females marrying early. Importantly, this program imposed a condition on beneficiaries of remaining unmarried for the duration of exposure, in addition to the standard school attendance conditions.

Taken together, the scarce evidence accumulated suggests these programs do not have a direct impact on early female marriage. However, one program that explicitly conditions payment on marital status (India's *Kanyashree Prakalpa*) finds that imposing this explicit condition leads to substantial reduction in early marriage.

6. Indirect and General Equilibrium Effects

Indirect and general equilibrium effects of CCTs for education usually fall into three categories: indirect schooling effects on other household and family members; indirect effects on peers, neighborhoods, and schools; and general equilibrium effects on local markets, governments' budgets, inequality, and the overall economy. In this section, we summarize the evidence in each.

6.1 Indirect schooling effects on other household and family members

In theory, schooling effects on other ineligible household members are ambiguous. On the one hand, all children in the household, regardless of whether they are eligible for transfers (e.g., due to age or gender restrictions), benefit from the transfer's income effect. To the extent that schooling for all of them is a normal good, this income effect predicts increased schooling. For ineligible children, however, there is the possibility of a "displacement effect" working in the opposite direction: Household resources may shift in favor of schooling investments for eligible children at the expense of ineligible children. Ferreira, Filmer, and Schady (2017) analyzed Cambodia's *CESSP* using a regression discontinuity research design. The authors showed that school enrollment of eligible children increased by 20 percentage points while their work for pay decreased by 10 percentage points. However, the authors also found that the school enrollment and work of ineligible siblings largely was unaffected by the program, suggesting minimal indirect effects. Galiani and McEwan (2013) reached a similar conclusion for the case of Honduras' *PRAF-II* program.

In contrast, Barrera-Osorio et al. (2008) analyzed indirect effects in Colombia's *Subsidios*. Barrera-Osorio et al. (2008) compared families in which two, one, or no children were selected into the program. Students who did not receive transfers, but whose siblings did, are 3 percentage points less likely to attend school and 7.3 percentage points less likely to re-enroll than similar students with untreated siblings. For girls, the differences are 5.3 and 10.4 percentage points, respectively. They concluded that the program positively affected the school enrollment of recipients, but that this came, in part, at the expense of their siblings—particularly sisters—who were more likely to drop out of school and enter the labor market. This result is consistent with a strong displacement effect working in opposite direction as the income effect of the transfer, and suggests that, at least in the context of Bogota, Colombia, families appear to substantially reallocate educational opportunities within the household in response to the transfers.

Angelucci et al. (2010) extended the previous within-household analyses, observing that households are typically embedded within extended family networks. Extended families may shape objectives and constraints of households within them. As the authors noted, extended families may be particularly relevant in shaping household behavior in contexts where informal insurance networks play a major role, as in the case of low-income families in rural communities—the target population of many CCT programs. Angelucci et al. (2010) investigated this hypothesis in the context of the initial rural, randomized, phase-in of *Progresa*. Using panel data and the Spanish surname convention (father's last name, mother's last name), the authors identified extended family links of each household within villages. Then, they investigated heterogeneity in secondary school enrollment impacts by the presence and characteristics of extended families, taking advantage of the original randomized rollout design of *Progresa*. The authors found that *Progresa* only increases secondary enrollment among households embedded in a family network; this contrasts with eligible but isolated households, among whom the authors do not find positive impacts. The authors argued that the mechanism through which the extended family influences household schooling choices is the redistribution of resources within the family network from those with infra-marginal eligible children (i.e., those who are enrolled in primary school for whom the transfer is effectively unconditional) towards eligible children within the family network who are on the margin of enrolling children into secondary school. Consistent with the evidence from Barrera-Osorio et al. (2008) from *Subsidios*, Angelucci et al. (2010) found an important sibling link in the pattern of network resource sharing for *Progresa*. However, rather than a displacement effect, as in the case of *Subsidios*, the results suggested a reinforcement effect in which households' response to *Progresa*, in terms of secondary school enrollment, is strongest when there are other eligible siblings in the village who receive effectively unconditional transfers because they only have primary school-aged children and can shift resources to students on the margin of enrolling in secondary school.

6.2 Indirect effects on peers, neighborhoods and schools

Bobonis and Finan (2009) investigated peer effects in school enrollment in the context of *Progresa*. The authors used *Progresa's* initial randomized rollout in rural communities to estimate peer effects in secondary school enrollment by comparing enrollment outcomes of ineligible children in treatment villages with those of ineligible children in control villages. Bobonis and Finan (2009) showed that secondary school enrollment among children from ineligible households residing in treatment villages increased by 5 percentage points relative to ineligible households in control villages. The authors presented two pieces of evidence consistent with a social interactions channel. First, the indirect program effect is strongest among the village's most socioeconomically-disadvantaged ineligible households, which are the ones most likely to interact with eligible households (who are even more disadvantaged). Second, indirect secondary enrollment effects among ineligibles are strongest in villages with strongest direct secondary enrollment effects among eligible beneficiaries. However, as the authors acknowledged, the evidence is consistent with at least three other potential channels. First, it is possible that *Progresa* improved teacher effort as more children become more interested in school, leading to increased school enrollment among ineligible children. Second, it is possible that the impact on non-eligible children is an anticipation effect, in which non-eligible children had enrolled in secondary school with the expectation that doing so would affect their future program participation. Third, availability of the program in a village may have made more salient the benefits of schooling among non-eligible households, leading them to enroll.

Lalive and Cattaneo (2009) also investigated peer effects in the context of *Progresa*. Similar to Bobonis and Finan (2009), they found evidence of strong indirect effects on peers' schooling—but there are three important differences. First, Lalive and Cattaneo (2009) focused on school attendance, arguing this margin potentially is more susceptible than enrollment to the presence of social interactions because ineligible children might want to spend more time in the classroom when their program-eligible peers attend school more frequently. In addition, changes in school attendance patterns are arguably less susceptible to result from anticipation effects of future program participation. Second, Lalive and Cattaneo (2009) employed a more fine-grained instrument for peer's schooling. Whereas Bobonis and Finan (2009) used the fraction of eligible children in a village, Lalive and Cattaneo (2009) used the fraction of a child's classroom peers eligible for *Progresa*, which enabled the authors to isolate peer influences from village-level schooling shocks (such as availability of the program in a village, making more salient the benefits of schooling to everyone, including non-eligible households). Third, Lalive and Cattaneo (2009) derived from a structural model reduced-form equations to decompose the overall program effect into a direct effect due to the subsidy and an indirect effect due to social interactions. Consistent with Bobonis and Finan (2009), they found that the indirect effect on ineligibles' schooling is as strong as the direct program effect on those eligible.

Bobba and Gignoux (2019) focused on estimating indirect effects stemming from information-sharing within networks of *Progresa's* potential beneficiaries. The authors noted that *Progresa* and other CCTs may enhance, among groups of beneficiaries, existing interactions and knowledge-sharing about the program. As in the studies referenced previously, the authors combined data from the experimental evaluation of the program with geo-referenced locations of the villages benefitting from it. The geo-referenced data showed substantial variation in the density of neighborhood treatment villages: Some treatment villages had a large cluster of treated neighboring villages while other treatment villages had few treated among neighboring villages. This is unsurprising given that poverty is often geographically concentrated. But the authors argued that, due to the means test used initially to target eligible villages, as well as village-level random assignment of the program among those eligible villages, conditional on the density of neighboring program-eligible villages (i.e., the concentration of poverty), the density of neighboring treatment villages is random because of randomization of treatment within the set of eligible villages. The authors showed, as evidence of the validity of this conditional independence assumption, tests suggesting that neighboring treatment density is orthogonal to village characteristics conditional on the density of program-eligible villages. Thus, these indirect effects are identified from comparing outcomes in treatment villages with lots of neighboring treatment villages to those in treatment villages with few neighboring treatment villages. Based on this research design, the authors estimated that if a treatment village has another neighboring treatment village, secondary school enrollment increases by 6.1% and if it has two or more, secondary school enrollment increases by 8% over and above the direct program effect. The authors found these neighborhood externalities seem to only affect children from beneficiary households, and there is no evidence of neighborhood effects for children in the control group and for those in treated villages who are ineligible for the program—which appears to be inconsistent with the evidence from Bobonis and Finan (2009), and Lalive and Cattaneo

(2009). However, the authors argued that it is precisely this pattern of heterogeneity that highlights the importance of interactions within networks of potential beneficiaries spanning across villages as a possible channel, because although interactions with preexisting social networks, in principle, affect all households that share local resources, it is only social interactions among program beneficiaries that are likely to be associated with improved knowledge and attitudes toward the program. Consistent with this hypothesis, Bobba and Gignoux (2019) found that variation in neighboring treatment density is associated with increased knowledge among eligible households about the program's different components, particularly schooling subsidies.

6.3 General equilibrium effects on local markets, governments' budgets, inequality, and the overall economy

Angelucci and De Giorgi (2009) studied how *Progresa* transfers affect consumption of non-eligible households in a village. The authors began with the observation that in many village economies in developing countries there is little income diversification—most households rely on agriculture—and access to formal insurance. In addition, village economies are vulnerable to natural disasters such as water shortages, floods, and drought. Despite this, mobility is low and, as a result, there are strong informal insurance mechanisms. Examples include reciprocal engagement in communal chores, community assemblies, and parent associations. Taken together, income risk, absence of formal insurance, and the strength of communal ties suggest that villagers engage in informal risk-sharing activities. Therefore, the authors hypothesized that through risk-sharing mechanisms, *Progresa* transfers may affect economic opportunities of ineligible households in the village. Comparing consumption of ineligible households in randomly-selected treatment villages to consumption of ineligible households in control villages, Angelucci and De Giorgi (2009) found that two years into the program, food consumption for the ineligibles in treated villages increases by about 10% per month per adult equivalent. This is the result of them borrowing more money from family and friends, receiving more transfers from eligible households, and reducing their precautionary savings (i.e., stock of grains and animals) at the start of the program. The authors provided evidence ruling out alternative hypotheses such as changes in labor earnings, or increases in income and the price of goods.

If local markets are not integrated, however, it is possible that CCTs affect prices, because in isolated village economies supply may be constrained due to transport costs for imported goods or oligopolistic production markets. Filmer et al. (2021) used village-level randomization of the Philippines' *Pantawid Pamilyang Pilipino* to estimate its impact on beneficiary households' food budget shares, as well as child nutrition, education, and food intake. As the experimental design targeted some of the poorest and most remote parts of the country, the setting allowed the authors to analyze the impact of cash on local markets largely not integrated with the national or regional production base. The authors showed that almost three years into the program, transfers increased demand for nutritious foods among beneficiary households. Comparing prices of various goods in treated and control villages, the authors found that transfers increased aggregate income in the treatment villages by 9%. The increase in income, however, only raised local prices for protein-rich perishable foods, such as eggs and fresh fish, by 6-8%. The authors noted that this rise in relative prices of perishable protein decreases non-beneficiaries' real income and leads them to substitute away from protein-rich foods, creating significant unintended negative effects on the nutritional status of non-beneficiary children, who experience a 0.4SD decrease in height-for-age z-scores (from a control base rate of -1.1SD), and a 12 percentage point increase in stunting (from a base of 32%). The authors noted that children in beneficiary households, in contrast, exhibit a gain in nutritional status, because the transfers compensated for the relative price change of these nutritional goods.

In unintegrated village economies, CCTs can have similar general equilibrium effects in local labor markets, because an exogenous decrease in the supply of child labor as a result of increased school enrollment and attendance may shift the labor supply curve inward, resulting in higher equilibrium wages (assuming a downward sloping labor demand curve). If there are barriers to migration across local labor markets, these wage increases might be long-lasting. Attanasio, Meghir, and Santiago (2012) tested for this type of general equilibrium effects in local labor markets in the context of *Progresa*. Combining the initial randomized rollout of the program with a dynamic education choice model, the authors showed that *Progresa* has a substantial impact of increasing child wages in treatment villages (accounting for the fact that child wages are only observed for a selected subset of children who actually work). However, as the authors noted, the child wage effects only marginally affect the effectiveness of the *Progresa* program at increasing school enrollment, particularly of secondary school children, because they do not substantially counterweight the combination of income and substitution effects that transfers and conditions have—and that makes schooling relatively more attractive than work. Consistent with this finding, Buddelmeyer and Skoufias (2004) found no evidence of significant village-level spillover effects of *Progresa* on children's participation in economic activities.

Cespedes (2014) considered wage effects of *Progresa* using an overlapping generations dynamic general equilibrium model of an economy that introduces a CCT program. Consistent with Attanasio, Meghir, and Santiago (2012), Cespedes showed that in the initial 5-6 years of the program, child wages increase because of the shocks to child labor supply from increased school enrollment. Adult wages, however, decrease as a result of assuming imperfect substitution between child and adult labor inputs.

Given that CCT programs often are financed through domestic taxes, Coady and Harris (2004, 2001) investigated general equilibrium effects of *Progresa* arising from fiscal externalities. The authors showed that a CCT program like *Progresa*, which was partially funded by the elimination of across-the-board food subsidies, potentially has large fiscal externalities because its financing reduces the distortionary effects of taxation while its fine-grained targeting improves the efficiency and vertical equity of redistribution. Despite these positive aggregate welfare gains, the authors' simulations highlighted noteworthy regional differences. Specifically, in the poorest regions targeted by the program, the elimination of food subsidies in favor of more targeted cash transfers increases average income and reduces inequality—but in richer areas that do not receive transfers and stop receiving food subsidies, average income falls and inequality increases. The authors argued that the welfare gains from reducing inequality within the poorest regions because of *Progresa's* direct effect on poor beneficiaries outweigh the welfare losses from increased inequality in richer areas as a result of eliminating food subsidies.

On the whole, the available evidence provides mixed evidence of the existence of indirect schooling effects of CCTs within the household. No indirect schooling effects were found in Cambodia's *CESSP*, possibly because of income and displacement effects of similar magnitude operating in opposite directions. There were negative indirect effects in Bogota's CCT programs, possibly as a result of strong displacement effects. Positive effects were found within family networks because of resource reallocation from infra-marginal primary school students for whom the transfer is effectively unconditional to students on the margin of enrolling in secondary school.

The (limited) evidence from *Progresa* suggests strong indirect peer and neighborhood effects on secondary school enrollment of children, likely stemming from social interactions and information sharing.

Finally, the available evidence on general equilibrium effects of CCT programs suggests that programs operating at scale may increase village-level consumption. In the case of the Philippines' *Pantawid Pamilyang Pilipino*, however, the increase in consumption led to negative unintended consequences by increasing the relative price of perishable protein sources, pushing non-beneficiary households to substitute away from protein-rich foods, with children in those households experiencing increased stunting as a result. At the same time, available evidence seems to unambiguously suggest strong local labor market child wage effects as a result child labor supply shock from the transfers increasing school enrollment. However, these child wage effects appear to have negligible feedback effects on the overall impact of CCT programs on school enrollment. Lastly, to the extent that CCT programs are financed by eliminating distortionary transfer programs such as agricultural subsidies, these programs can have a positive fiscal externality. One limitation of the available evidence on indirect and general equilibrium effects is that it is overwhelmingly drawn from Mexico's *Progresa* program. To the extent that village economies in other countries with CCTs for education are more or less integrated into the national economy, that social and cultural norms play less of a role than in Mexico, and that the financing of these programs differs from the way in which *Progresa* was financed (by eliminating across-the-board food subsidies), it is conceivable that general equilibrium effects in those contexts might also differ from those found for *Progresa*. Future research can shed light on these issues.

7. Cost effectiveness of CCTs

To frame the discussion of cost-effectiveness of CCT programs, it is useful to begin by outlining a simple model of costs commonly used in the literature (e.g., Caldés, Coady, and Maluccio 2006; Caldés and Maluccio 2005).

In the model, there are three sources of costs associated with CCT program implementation: administrative costs, transfer costs, and private costs. Administrative costs arise from aspects related to program design and implementation, and include operations, personnel, equipment, targeting of beneficiaries, and program auditing. Transfer costs are the sum of demand-side transfers to families (i.e., the subsidies themselves) and supply-side in-kind transfers (e.g., infrastructure improvements and materials). Private costs are those incurred by users as a result of program conditions. For example, recipients in many contexts incur costs related to transportation and waiting time to cash payments made through the banking system.

Under some simplifying assumptions, Caldés, Coady, and Maluccio (2006) showed the welfare effect of a CCT program can be written as:

$$dW = \alpha(T + WTP \times E) \qquad (7.1)$$

Where T is the value of cash transfers, E is the value of in-kind transfers (e.g., additional educational services and input provided), WTP is household's willingness to pay for those in-kind transfers, and α measures the progressivity of transfers. The total cost to the government of providing benefits *B* is composed of the cost of cash transfers *T,* the value of in-kind transfers *E*, and total program operational costs *C.* Multiplying and dividing (7.1) by *B*, Caldés, Coady, and Maluccio (2006) showed:

$$dW = \frac{\alpha(T+WTP\times E)}{T+E+C}B \qquad (7.2)$$

Which is the welfare impact per unit of expenditure (i.e., the cost-benefit ratio) multiplied by the size of the program *B.* As the authors noted, a full cost-benefit evaluation of the program would require an evaluation of the targeting effectiveness (α) and of household's willingness to pay for in-kind services (*WTP*).

To focus on cost-efficiency, Caldés, Coady, and Maluccio (2006) made two additional simplifying assumptions. First, they assumed perfect targeting of program benefits, thus setting $\alpha = 1$. Second, they assumed households value in-kind at cost, so that the total welfare impact of the program is the sum of cash and in-kind transfers. Under these simplifying assumptions, the authors wrote the cost-benefit ratio as:

$$\frac{B}{dW} = \frac{T+E+C}{T+E} = 1 + \frac{C}{T+E} = 1 + CTR \qquad (7.3)$$

Where *CTR* is the cost-transfer ratio, a measure of cost-efficiency. One way to think about a program's *CTR* is that it captures the administrative and private dollar cost associated with a one-dollar transfer to a beneficiary in a given time period (Caldés, Coady, and Maluccio 2006; Lindert, Skoufias, and Shapiro 2006). In other words, *CTR* is the ratio of non-transfer program costs to total program transfers.

Caldés, Coady, and Maluccio (2006) applied this approach to estimate cost-efficiency for three CCT programs: Mexico's *Progresa*, Honduras' *PRAF-II*, and Nicaragua's *Red de Proteccion Social*. Garcia and Saavedra (2017) used data on program administrative costs from Grosh et al. (2008) to compute additional CTR for Brazil's *Bolsa Escola*, Colombia's *Familias en Accion (Rural Version)*, Ecuador's *BDH*, Jamaica's *PATH*, Peru's *Juntos*, and Bangladesh's *Primary Education Stipend Project*. Specifically, Grosh et al. (2008) reported for these six additional CCT programs data on administrative costs as a percent of total (administrative + transfers). With data on administrative costs as a percent of total costs, and assuming no private costs, Garcia and Saavedra (2017) followed Lindert, Skoufias, and Shapiro (2006) and estimated *CTR* as:

$$CTR = \frac{\%Administrative\ Costs}{100-\%Administrative\ Costs} \qquad (7.4)$$

In addition to estimating *CTR's* for additional CCT programs, Garcia and Saavedra (2017) extended the cost-efficiency approach to evaluate the educational cost-effectiveness of various CCT programs. Because not all CCT programs offer in-kind transfers*,* for comparability Garcia and Saavedra (2017) made the additional simplifying assumption that *E=0,* and expressed *CTR* in per-beneficiary terms:

$$CTR = \frac{C}{T} \qquad (7.5)$$

Garcia and Saavedra (2017) then defined educational cost effectiveness, *CE*, as the ratio of average educational effect size per beneficiary, *ES*, to non-transfer program costs per beneficiary, *C* (Dhaliwal et al. 2013):

$$C = \frac{ES}{C} \qquad (7.6)$$

Through simple algebraic manipulations, Garcia and Saavedra (2017) showed:

$$CE \equiv \frac{ES}{T} \times \frac{T}{C} = TE \times \frac{1}{CTR} \qquad (7.7)$$

where *TE* ≡ *ES/T* is transfer-effectiveness; in other words, the ratio of educational effect size per beneficiary to dollar of transfer costs. As equation (7.7) shows, educational cost-effectiveness can be expressed as transfer-effectiveness, *TE*, multiplied by the reciprocal of *CTR*.

Garcia and Saavedra (2017) noted that as *ES* is estimated at the margin, ideally so should *T*. From an empirical perspective, this observation implies that for an Intent-to-Treat estimate of *ES*, *T* should be the difference in the mean transfer payment given to the treatment group and the mean transfer in the control group. This includes actual transfer amounts to individuals in the treatment group already attending school, as well as actual transfer amounts in the control group, which may be nontrivial in programs with substantial benefit crossover. Due to data limitations, Garcia and Saavedra (2017) were unable to implement this ideal measure of transfers at the margin as they knew only from each CCT program's description the nominal transfer amount per child. While this nominal figure reflects amounts given to individuals already attending school, there was a lack of program-level information about the difference, at the margin, between actual transfer amounts received by a participating household and actual amounts received by nonparticipants in programs in which there is benefit crossover. Therefore, given these data limitations, Garcia and Saavedra's (2017) empirical measure of transfer amounts at the margin was a more accurate approximation to the conceptual ideal for CCT programs with perfect compliance, in the sense of having almost perfect program take-up among eligible households and no crossovers to non-eligible households. With estimates of *CTR* from Caldés, Coady, and Maluccio (2006) for three programs and from Equation (4) for six others, as well as with estimates of educational effect size impacts, and transfer amounts per beneficiary, Garcia and Saavedra (2017) estimated cost-effectiveness measures for a total of nine CCT programs. Table 7.1 reproduces their cost-effectiveness estimates.

Table 7.1. Cost-effectiveness estimates of selected CCT programs

TABLE 5

Cost-effectiveness estimates of selected CCT programs

				Cost effectiveness estimates (CE)						
				Primary			Secondary			
Country	Program	Cost data year	Annual cost-transfer ratio (CTR)	Enr.	Att.	Do.	Enr.	Att.	Do.	Grad.
Mexico	PROGRESA	1997/2000	0.106	0.044	0.063		0.211	0.143		
Honduras	PRAF II	1999/2002	0.499	0.153	0.088	0.243				
Nicaragua	Red de Protección Social	2000/2002	0.629	0.175	0.139	−0.055				
Brazil	Bolsa Familia	2003	0.14	0.049		−0.005	0.042		−0.004	
Colombia	Familias en Accion	2000/2004	0.117	0.137	0.293		0.115	0.387		0.104
Ecuador	Bono de Desarrollo Humano	2005	0.043	1.326		0.184				
Jamaica	PATH	2004/2005	0.149		0.177			0.177		
Peru	Juntos	2006	0.131	0.063	0.000	0.000				
Bangladesh	Primary Education Stipend Program	2002	0.042	13.002						

Note. CCT = conditional cash transfer; Enr. = enrollment; Att. = attendance; Do. = dropout; Grad. = graduation. For PROGRESA, PRAF II, and Red de Protección Social programs, cost-transfer ratio (CTR) data are from Caldés et al. (2006, Table 2 column total). For all other programs, administrative cost data is from Grosh et al. (2008, compiled from various sources). CTR for these programs are obtained from administrative cost data using Equation 4 in text. Cost-effectiveness estimates (CE) are annual transfer-effectiveness (TE) divided by the annual CTR, see Equation (3). Annual TE is educational impact in percentage points per annual transfer amount. For PROGRESA, educational impacts are measured after 1 year. For PRAF II, primary enrollment impact is measured after 1 year and primary attendance impact is measured after 2 years. For Red de Protección Social, impacts are measured after 1 year. For Bolsa Familia, impacts are measured after 4 years. For Familias en Acción, enrollment impacts are measured after 2 years; attendance impacts after 1 year. For Bono de Desarrollo Humano, impacts are measured after 2 years. For PATH, impacts are measured after 1 year. For Juntos, enrollment impacts are measured after 3 years; attendance and dropout impacts are measured after 1 year. For Primary Education Stipend Program, impacts are measures after 6 years.

Table 7.1 reproduced from Table 5 in Garcia and Saavedra (2017).

Garcia and Saavedra (2017) noted that among the nine CCT programs considered, there is considerable cost-efficiency heterogeneity, as measured by *CTR*. In Ecuador's *BDH* and Bangladesh's *Primary Education Stipend Project*, for instance, it takes about 4 cents of administrative costs to deliver 1 dollar of transfers to beneficiaries—well below the (arithmetic) average of 21 cents of administrative costs/year for each dollar of transfer in the sample of programs considered. At the other extreme, in Honduras' *PRAF-II* and Nicaragua's *Red de Proteccion Social*, it takes, respectively, 50 cents and 63 cents to deliver 1 dollar of transfers to beneficiaries.

Garcia and Saavedra (2017) also documented considerable heterogeneity in program impact and transfer-effectiveness estimates. It is, therefore, not surprising to find considerable heterogeneity in cost-effectiveness estimates. If one were to focus solely on primary enrollment—the outcome for which the authors had the most transfer-effectiveness estimates—one would conclude that Ecuador's *BDH* and Bangladesh's *Primary Education Stipend Project* are the most cost-effective CCT programs, as they deliver, respectively, 1.3 and 13 percentage points of enrollment impact per dollar of administrative cost/year. Programs like Mexico's *Progresa* and Brazil's *Bolsa Familia*, on the other hand, deliver between 0.4 and 0.5 percentage points of enrollment impact per dollar of administrative cost/year, putting them in the least cost-effective end of this (albeit limited) sample.

With the caveat of having data on transfer-effectiveness for secondary enrollment in only three of these nine CCT programs, Garcia and Saavedra (2017) noted that it appears that Mexico's

Progresa and Colombia's *Familias en Accion* are more cost-effective than Brazil's *Bolsa Familia*. Mexico's *Progresa* delivers a 0.21 percentage point increase in secondary enrollment per dollar of administrative cost/year, while Colombia's *Familias en Accion (Rural Version)* delivers a 0.12 percentage point increase. For the most downstream outcome of secondary school graduation, the authors only had data on *CTR* and transfer-effectiveness for Colombia's *Familias en Accion*, which delivers a 0.10 percentage point increase in secondary graduation per dollar of administrative cost/year.

The authors also noted that with the caveat of a limited sample size, it is unclear whether the most cost-efficient programs (i.e., those with low *CTR*) are also the most transfer-effective (i.e., producing the greatest educational impact per dollar of transfer). For instance, the authors found a correlation of −0.13 between a program's *CTR* and its primary enrollment transfer-effectiveness estimate, suggesting that more cost-efficient programs (weakly) produce greater primary enrollment impacts per dollar of transfer (N = 8). In contrast, they found a correlation of 0.87 between a program's *CTR* and its primary attendance transfer-effectiveness estimate, suggesting cost-efficient programs (strongly) produce smaller primary attendance impacts per dollar of transfer (N = 6).

A complementary approach to estimating cost-effectiveness of CCT programs is to compare CCTs relative to other educational interventions, in terms of educational outcomes achieved per dollar spent. One limitation of this approach is that it typically takes on educational estimates from only one sample CCT program—typically, Mexico's *Progresa* or Colombia's *Familias en Accion*—and as estimates from Garcia and Saavedra (2017) indicated, there is considerable variation in cost-effectiveness estimates within CCT programs, and even across the different educational outcomes. Therefore, comparisons with other programs can be misleading and depend on which specific program and outcome are chosen.

With this caveat in mind, Dhaliwal et al. (2013) proposed a framework to conduct comparative cost-effectiveness analyses across various educational interventions to inform policy in developing countries. We refer the reader to their study for methodological details. Here, we simply highlight that the authors were careful in estimating benefits and costs, and in ensuring the use of common units across programs for comparability. In this comparative cost-effectiveness framework, Dhaliwal et al. (2013) considered the increase in average years of schooling per \$100 spent for 11 educational programs: information sessions on returns to education to parents in Madagascar; deworming through primary schools in Kenya; free primary school uniforms in Kenya; merit scholarships in Kenya; iron fortification and deworming in India; camera monitoring of teacher attendance in India; computer-assisted learning curriculum in India; remedial tutoring by volunteers in India; menstrual cups for teenage females in Nepal; information on returns to schooling to children in Dominican Republic; and *Progresa* in Mexico for primary school attendance.

The most comparatively cost-effective programs under this lens are information sessions on returns to education to parents in Madagascar, deworming through primary schools in Kenya, and India's iron fortification and deworming, which respectively, produce, per \$100 spent, 19.5 years, 14 years, and 2.7 years. Mexico's *Progresa* for primary attendance is roughly middle of the pack, producing 0.24 years per \$100 spent, comparable to uniforms (0.71 years) and merit scholarships for girls (0.27), and comparatively superior to camera monitoring of teacher attendance in India, computer-assisted learning curriculum in India, remedial tutoring by volunteers in India, and menstrual cups for teenage girls in Nepal.

Filmer et al. (2020) and Angrist et al. (2020) extended the comparative cost-effectiveness approach to account for quality of learning. The authors applied this new metric of learning-adjusted years of schooling (LAYS)[21] to compare the cost-effectiveness of multiple educational interventions, including two CCTs: Mexico's *Progresa* and Malawi's pilot program (Baird, McIntosh, and Özler 2011). Their calculations indicated that *Progresa* delivers 0.00 LAYS per $100 spent and Malawi's pilot delivers 0.01 LAYS per $100 spent, putting them at the bottom of cost-effectiveness comparisons by this metric. Meanwhile near the top are Madagascar providing information on earnings and deworming in Kenya, with 140.99 and 5.68 LAYS per $100 spent, respectively. However, the authors acknowledged LAYS-based cost-effectiveness has limitations, including limited data on costs and strong assumptions about the distribution of learning levels to translate learning effects in standard deviations into LAYS. From the perspective of CCTs, an additional limitation of the methodology is that it includes only two programs. As Garcia and Saavedra (2017) and Baird et al. (2014) documented, there is substantial heterogeneity in effect sizes of CCT programs. Further, as Garcia and Saavedra (2017) suggested, there also is substantial heterogeneity in estimated *CTRs* and cost-effectiveness. Therefore, comparative cost-effectiveness studies across a broad range of educational interventions including only one or two sample CCT programs are unlikely to capture the full scope of such heterogeneity. Future research could investigate how the recent LAYS approach can be applied to a much broader sample of CCT programs, such as those included in the meta-analytic results of learning we presented in Section 5.2.

8. Conclusions

From the perspective of CCTs as educational policy, the key assumption underlying these social assistance programs is that they improve human capital acquisition by helping households overcome demand-side constraints such as educational externalities, informational constraints, and opportunity costs of children's time. The evidence to date supports some of these arguments. The literature on educational impacts of CCTs unambiguously supports the notion that these programs increase school enrollment, attendance and attainment, and reduce dropout. However, one of the most robust empirical regularities in the education CCT literature is the substantial statistical heterogeneity in program impacts across various schooling outcomes. While some heterogeneity is explained by variation in program characteristics—as predicted by standard models of household decision-making that incorporate a conditional transfer—a substantial portion of it is not. Therefore, a promising area of future research concerns evidence to help explain further the sources of such heterogeneity.

The evidence on the long-term impacts of CCTs does seem to provide some support for their role in relaxing demand-side constraints faced by low-income households, although the lack of impacts on achievement does point to possible supply-side constraints stemming from schools, resources, and teachers. In this chapter, we provide new meta-analytic evidence on CCT

[21] We refer the reader to their report for methodological details and sample of educational programs included.

achievement impacts with a substantially larger sample of studies than used in prior reviews. The evidence indicates both substantial heterogeneity in achievement impacts across programs, as well as statistically zero overall impact impacts on both math and language. To date, we know very little about what actually happens in schools following the introduction of a CCT program. This is one big open question and a promising area of future research to inform the debate on the relative merits of CCTs as poverty alleviation or potentially promising education policy lever.

Finally, based on our holistic review of the literature, it seems premature to conclude, as others did, that education CCTs are potentially bad social investments, in terms of how much schooling and learning is achieved for every dollar spent relative to other educational innovations. It is true CCT programs are costly. However, there is too much heterogeneity across CCT programs in costs, effectiveness, and cost-effectiveness to definitively reach this conclusion. Even in the pioneer, more established programs, students have not fully transitioned into the labor market, precluding conclusive statements about welfare impacts over the life cycle. Therefore, another promising area of future research will be conducting more comprehensive welfare analysis of these programs with more realistic assumptions about the actual employment and earnings trajectories of participants in adulthood.

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