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Wealthy, healthy, and wise: does money compensate for being born into difficult conditions?

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Wealthy, healthy, and wise: does money compensate for being born into difficult conditions?[1]

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Abstract

Recent studies have linked transfers from Mexican conditional cash transfer program Oportunidades (formerly PROGRESA) to improvements in child development (Fernald, Gertler, and Neufeld 2008, 2009) but this work has been criticized as failing to account for endogeneity of the transfers. We create an exogenous instrument for the amount of transfers and use it to test program and transfer effects. Applying the new instrument confirms that improvements in child development are more linked to the transfers themselves than to other portions of the program, which involve medical checkups as well as educational sessions for mothers. We also find evidence that the program facilitates catch-up growth, a phenomenon of disputed importance in the health literature.

JEL codes: I12, O12, I38

Keywords: PROGRESA, Oportunidades, conditional cash transfers, instrumental variables, child development, child health, Mexico

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Introduction

In a far-ranging survey, Fiszbein and Schady (2009) describe a paucity of evidence when it comes to examining the effects of conditional cash transfer (CCT) programs on households in developing countries. CCT programs have increased consumption, cut poverty headcount and severity as well as inequality, improved participation in education and decreased child labor. However, "the evidence of CCT impacts on *final* outcomes in health and education—achievement and cognitive development rather than school enrollment, child height for age rather than growth monitoring—is more mixed." [p. xii, emphasis in original]

The question of CCT effectiveness on final outcomes remains open. Do CCTs increase child nutritional status, cognitive development, or emotional growth? If so, do they succeed through their transfers or through their conditionality (i.e. by improving the likelihood that children will participate in education and health care)?

Mexico's *Oportunidades* (formerly known as PROGRESA) involves monetary transfers to poor households conditional on household members coming in for medical checkups, sending children to school, and/or attending educational discussions with care providers (Rivera et al. 2004). In the context of the program, information comes in the form of clinic visits and "pláticas," talks that women in the household are required to attend as a condition of receiving transfers. Transfers are awarded based on a number of criteria, including the time that the household was brought into the program (randomly assigned), household size, demographic structure, and school attendance. Since information and income are provided differentially, we can evaluate the importance of each and thereby open a window into some of the complex interactions between income and child development. Below we review the literature investigating this relationship in the context of other cash transfer programs.

Previous work has found a link between short-term program participation in CCTs and improvements in human capital accumulation (cf. summaries in the systematic reviews written by Gaarder, Glassman and Todd (2010) and Leroy, Ruel, and Verhofstadt (2009)) but much of this is in the health literature and attempts to show no more than correlation. In this paper we introduce an instrument that uses exogenous household and program characteristics to rigorously estimate program impacts. We find that in the context of *Oportunidades*, for most outcomes, the amount of additional money given to households is associated with improved human capital outcomes while program participation alone is not. Results from our alternate estimation method identify effects similar to Fernald, Gertler, and Neufeld (2008), and we conclude that we have more confidence than ever that the results are causal results of the program.

We also use this method to address two open issues in the health and development literature. First, to what degree does early deprivation determine future growth, and to what degree is catch-up growth possible? Second, if CCTs make a difference for recipient households, is the difference attributable to the cash transfer component or to the conditionality, i.e. to the other requirements imposed upon the household? Since the randomization determined the length of time households were exposed to the conditions but had only a moderate impact on total transfers received, the exogenous instrument for transfers received enables us to compare and contrast the effects of each separately.

Theoretical Background

It is a simple matter to advocate for transfer payments to the poor on equity grounds, but Becker and Tomes (1986) contend that under the right conditions such transfers can be efficient as well. Market imperfections such as binding constraints on households' access to capital may imply an inability to optimally invest in children, as working household members may take precedence. They argue that under these conditions, "[A] redistribution of investments toward less advantaged children is equivalent to an improvement in the efficiency of capital markets" (p. S16). At the same time, they note that evaluators often find no impact of programs such as [the U.S.'s child assistance program] Head Start since families receiving assistance may redistribute resources that would otherwise have gone to the child. Returns to investing in children may be high, but those from investing in working household members may be higher. Recent research, however, affirms Becker and Tomes' original point: engaging children early can have long term impacts. Effective programs can improve executive function (Diamond and Lee 2011) and protect and stimulate young children's developing brains (Shonkoff 2011).

For programs to be effective they need to change values (i.e. inform parents that investments in children provide higher returns than they might expect), improve potential returns to investments in children, or provide enough additional income that households can invest in all of their members. One unanswered question relating to cash transfer programs in developing countries is whether the binding constraint is information or income.

Since the Mexican PROGRESA/ Oportunidades program provided both in varying measure, we can test this difference. As noted by Fernald, Gertler, and Neufeld (2008), the randomized portion of the program meant that early treatment communities had access to an additional 18 months of exposure to health talks, called “pláticas.” The amount of transfers received varies not only by randomization into the early or late treatment groups but also by the demographics of the household and by progress in school. The below chart, taken from Fernald, Gertler, and Neufeld (2008) shows how the total amount of transfers received depends on family size as well as the group into which the community was randomized.

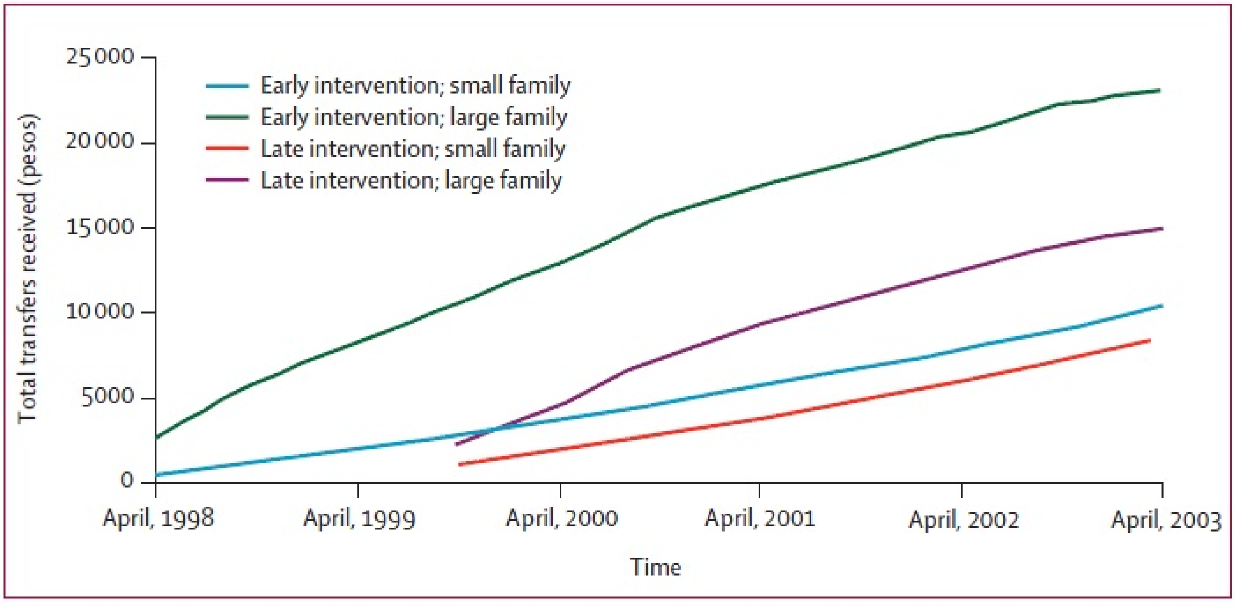

Figure 2: **Simulation of a scenario specifying projected *Oportunidades* cumulative cash transfers for large and small families**
*Small family is defined as having only one child who turned 6 years old in 1997, and large family as one in which there are three children, including two boys aged 10 and 12 and one girl aged 8 (as of 1997). The early intervention group began receiving cash transfers in April, 1998, and the late intervention group began receiving transfers in November, 1999. The survey described here took place from September to December, 2003.

Thus, comparing separately the effects of time on the program with the effects of total transfers received will allow us to identify whether the cash and/ or the informational constraints are inhibiting child development.

The Program allows us to test some theories of child nutrition as well. A recent study in the Lancet (Victora et al. 2008) reviews data from five longitudinal studies in different parts of the world and concludes that damage suffered early in life leads to permanent impairment. Others note that while adverse health events are known to affect growth in the short term, most people are able to recover once they return to health (Tanner 1986, p. 168). However, repeated insult may limit the capacity for "catch-up growth" (Tanner 1986, p. 176). Data from Gambia to Guatemala have shown that height deficits are established early in life and often persist into adulthood, with adolescence providing the only window of opportunity in which limited catch-up may be possible (Coly et al. 2006).

Additional studies show evidence of the possibility of catch-up growth. In a study of Swedish adoptees of Indian origin, the majority (34/47) had catch-up growth of over 1 standard deviation, including 35/40 who arrived in Sweden after they reached age 3 (Proos et al. 1991). A study comparing the adolescent development of well-nourished and malnourished children in Kenya observed catch-up gains such that the groups converged over the age period studied (Kulin et al. 1982).

We are able to use the randomization to test whether catch-up growth makes a difference in this group. Since the first few years of life are very important, we focus on children born around the time the program started. This enables us to contrast children randomized to early treatment against later treatment children. If effects on the youngest children are absolute, then early treatment should have long-lasting effects; if catch-up growth compensates for early deprivation, then we should not see longer term differences between the two groups. If catch-up growth is largely unable to mitigate early deprivation, then nutritionally constrained children in households participating earliest should show improvements in nutritional status, while the extent of program participation over a longer period should matter less. On the other hand if the total transfer amount over time matters the most, then catch-up growth must be potentially sufficient to compensate for early malnutrition.

Literature Review

I. Prior Analyses of Cash Transfer Programs

Papers on other programs use a variety of techniques to estimate the effects of cash transfers on child nutritional status. Leroy et al. (2008) and Baulch (2010) estimate programme effects with difference-in-difference propensity score matching (PSM). Duflo (2003) instruments for the

South African Old Age Pension program using dummy variables indicating the presence of eligible men and women. León and Younger (2007) instrument for participation in Ecuador's *Bono Solidario* program by modelling transfer amounts as a function of the means testing done by the government. Later work, however, casts doubt on this approach by noting that a weakness of the *Bono Solidario* is that it was not sufficiently means-tested (Paxson and Schady 2010). Attanasio, Gomez et al. (2005) use matching at the community level, but they provide little information on the matching process or the counterfactual. The quality of that study is compromised by the simultaneous existence of a large-scale nurseries programme, *Hogares Comunitarios*, which also targets child nutrition. Sinha and Yoong (2009) use a difference-in-difference approach since they have panel data. However, the programme was not randomized at implementation, so they are forced to estimate the intent-to-treat effect because they do not observe actual participation. Also, the counterfactual that they use is not similar eligible households who are not offered the transfer, but rather ineligible households. Agüero et al. (2009) analyze the impact of the Child Support Grant in South Africa. The slow roll out of program benefits created variations in coverage period and treatment dosage was used for identification.

The method we use allows us to instrument for transfer amounts using a combination of exogenous program and household characteristics. Before we detail it, we must describe some salient features of the program in question.

II. PROGRESA/ Oportunidades Program Description

In 1997 the Mexican government began a new welfare program through which income transfers were awarded to poor households in such a way as to provide incentives for children to remain in school, for all members of the household and particularly pregnant mothers to receive regular health care, and for nutrition to be improved by improved food consumption and nutritional supplementation. Called PROGRESA, an acronym for "program for education, health, and nutrition" and the Spanish word for "progress," the program sought to maximize returns to investments in human capital by targeting the poor (Skoufias, Davis, and Behrman 1999). Initially 506 communities were randomly selected for participation; 320 of these were chosen to receive benefits immediately while 186 were brought on later when funds became available. The current study uses a set of surveys of both types of communities carried out from 1997 through 2007. (See the Data section for more details.) The program changed its name to *Oportunidades* in 2000 when a new political party came to power in Mexico, but the implementation was unchanged.

An issue of particular interest to this paper is the algorithm used to determine the amount of transfers a household receives. The transfer consists of three parts: a grant for consumption of food, awarded conditional on attendance at scheduled visits to health centers; a per child grant for school materials, awarded yearly, and a per child grant awarded monthly. The amount of the last portion of the transfer varies according to the grade the child is in. Finally, there is a cap on the total amount of transfers a household can receive in a given month (Skoufias 2005 p. 4).

III. Previous Analyses of the Program

A number of earlier studies have examined the impacts of *Oportunidades*- Gaarder, Glassman, and Todd (2010) list over 40. Several studies link the program to improved child growth in the short run (i.e. follow-up after 2-5 years of enrollment) in rural (Behrmann and Hoddinott 2005, Gertler 2004, Rivera et al. 2004) and urban areas (Leroy et al. 2008). A study looking at the amount of money transferred rather than simple program participation identifies a decrease in the prevalence of stunting, obesity, and sick days in the medium term of six years (Fernald, Gertler, and Neufeld 2008). Other authors cast doubt on the early findings of enhanced growth (Fiszbein and Schady 2009; Attanasio, Meghir, and Schady 2010). Two papers evaluate the program after ten years: one finds no effect of being in the (randomized) early treatment sample relative to the later treatment sample (Behrman et al. 2008) while the other, assessing effects in terms of total transfers received by households, finds that though transfers make a difference, effects of the program are not apparent over the longer term of ten years though certain subgroups, such as households with uneducated mothers, show significant impacts (Fernald, Gertler, and Neufeld 2009).

In addition, earlier studies have found evidence linking the program to decreased incidence of alcohol abuse & domestic violence in the short run (Angelucci 2008) but less in the longer run (Bobonis and Castro 2010), improved birth outcomes (Barber & Gertler 2010), keeping children in school in spite of economic shocks to the household

(Skoufias & Parker 2001, de Janvry et al. 2006), improved BMI, blood pressure, and self-reported health in adults (Fernald, Hou, and Gertler 2008)[5], and increased quality and quantity of food consumed (Hoddinott and Skoufias 2004).

This paper is most closely linked to two previous works by the same research team (Fernald, Gertler and Neufeld 2008; Fernald, Gertler and Neufeld 2009). The 2008 paper analyzes data from a six-year follow-up of the program (data from 2003) and found that examining separate program effects (i.e. separating the effects of program participation from cash received) required separate analysis of the time on the program and the amount of payments received. As they put it, "The number of years on the programme was not associated with any of the outcomes, nor did its exclusion from the multivariate model modify the relation between transfers and outcomes" (833). Increased receipt of transfers was associated with decreased prevalence of stunting, overweight, BMI-for-age and increased height for age z-scores as well as positive impacts on four measures of cognitive development and on motor development. Similarly in 2009 they published a 10-year follow-up identifying positive effects of transfer amounts on height for age and verbal and cognitive performance scores. Effects of transfers on BMI for age were no longer statistically significant and on one outcome, child behavior, effects were now apparent of having been on the program for a longer duration. Table 2 replicates their results.

Attansio, Meghir and Schady (2010, hereafter AMS) question these findings. They contend that associations between transfer size and child cognitive outcomes reflect the structure of the cash transfer program. Children who are successful in school go on to higher grades and thereby obtain higher payments for their families. Thus, households with children who are healthier or otherwise disposed to do better in school (e.g. gifted academically or behaviorally) will garner larger payments, so an association between a child's physical or cognitive development and a household's payments is mechanical.

[5] The program as a whole had these effects, while the transfers were associated only with increased BMI.

This is true, to a point: households receive higher payments when their children continue on to higher grades. However, the payment mechanism depends on a variety of additional factors.

1. AMS note that transfers depend upon reaching higher grades, which in turn depend on a variety of potentially unobservable characteristics. Our instrument obviates this level of variation by assuming that reaching higher grades is a matter of course.
2. At a more basic level the amount of transfers depends on the exact demographic structure of the household. Part of a household's demographics were determined before the program began, which would make them exogenous, and Stecklov et al. (2007) show that the program did not influence household demographic structure. Further, we control for demographics using rough groupings on the right hand side[6], but our control variables group all school-aged children together. This leaves considerable heterogeneity in payment amounts within our groupings. For example, in 2003 a child enrolled in fifth grade earned her household 185 pesos per month, while a child in sixth grade was awarded 250 pesos, a 35% increase based on a difference in age of a month. This difference only gets larger as we examine children separated by more years. A 12th grade student gets almost six times as much for her family as a third grader. None of this heterogeneity is explained by our control variables.
3. AMS next call attention to the fact that households with more children receive higher transfers. In a separate work, one of them notes that "it is not clear that the identification strategy is robust to the presence of economies of scale or 'quantity-quality' trade-offs in child outcomes…." (Fiszbein and Schady 2009, p. 324). This effect should go in the opposite direction to that which we show. Becker and Lewis (1973) argue that quantity and quality of children are substitutes, and that families with more children tend to invest less in each. Thus, higher transfers go to families with more children, which should be associated with decreased child development. Any effects we find have overcome that initial bias.

[6] We include household size and the share of the household in each of eight groups: males and females aged 0-5, 6-17, 18-49, and 50 and up.

4. At the beginning of the program, communities were randomized into two groups, one of which began receiving transfers 18 months earlier than the other. Total accumulated transfers depends critically upon the group into which the household was randomized.

The amount received by participating households is therefore dependent upon a number of characteristics not subsumed by the AMS critique, leaving us heterogeneity we exploit to explain child development outcomes. In the next section below we describe in detail how we accomplish this.

Data

We identified children born between March 1, 1997 and October 31, 1998 whose households participated in rural PROGRESA/ *Oportunidades* surveys in 1997, 2003, and 2007. These children were born into households receiving the program just after the time the program began, and reached the age of 3 on or about the time that the late treatment group came into the program. Three years is identified by some as a critical age cutoff, beyond which nutritional status is to a large degree set and unalterable (Bhutta et al. 2008). Child heights and weights were measured in 2003 and 2007, and were converted to height for age and BMI for age z-scores using international norms coded into free software available from the World Health Organization (WHO 2010). Cognitive development and language ability were assessed using the Wechsler Abbreviated Scale of Intelligence (WASI), a shorter version of the WISC-III (Wechsler Intelligence Scale for Children). These variables are scaled close to traditional IQ scores, with a mean near 100 and a standard deviation of about 15. Child behaviors were assessed by administering the Strengths and Difficulties Questionnaire to children’s mothers. A variety of control variables were merged in from other surveys collected on the same households.

Table 1 lists the control variables included in our analysis. The later treatment group contains significantly more firstborns (13% vs. 9% of the respective sample) and access to piped water on a household’s land is slightly more common in the later group (the difference is significant at the 10% level) but for the most part the groups do not differ in statistically significant ways.

Methods

A first pass consists of an OLS regression of the various outcomes on the total amount of transfers, an indicator for early vs. late treatment, a vector of pre-program household and community level variables, and community random effects. Early vs. late treatment was chosen randomly and the controls pre-date the program. However, as noted by AMS transfer amounts may be correlated with the error term, since receipt of the transfers is conditional on the household's decisions such as whether to send children to school. To address this problem we use an instrumental variables approach.

To instrument for transfers received by the household we create a variable we call "Potential Transfers." Similar to the approach used by Albarran and Attanasio (2003), we simulate the grants based on exogenous characteristics. In each household we look at the number of children and their specific ages, assuming that each child enrolled in school will continue to attend school. This ends up being a maximum amount that households can receive. We start by looking at the 1997 data and use that to describe the first few years of the program. In 2000 households were recertified, so we re-examine their demographics and use those to project 2001-2003 transfer amounts. Finally we reevaluate the demographics in 2003 and project those through the 2007.

We are confident that this is exogenous for a number of reasons. First is the fact that early or late treatment status is a factor, and it was randomized. Second, the number and timing of children is exogenous in many cases as family planning is not commonly practiced. A previous study by Stecklov et al. (2007) found that the program "had no net effects on fertility." This is consistent with our observation that in March of 1998 a survey shows that 75% of the households in our data aren't actively using contraception, and 69% of them say they never have. Third, the nonlinearities imposed by the cap constrain the link between household size and school enrollment on the one hand and transfers on the other. Finally, the exact ages of the children in the household cannot be planned, and even a few months' difference affects transfer sizes.

The instrument works well. T-scores on the potential transfers variable in the first stage are well over 20, and the R-squared for the regression is between 0.45 and 0.5. Thus, after running the random effects analysis to replicate Fernald, Neufeld, and Gertler

(2009) we estimate effects using an IV approach with results shown on the right side of Table 2.

Results

Table 2 shows our results. On the left are the results using a random effects regression with actual transfers included directly as an explanatory variable. These results are presented to show comparability with the findings of Fernald, Neufeld, and Gertler (2009); while point estimates vary slightly, all signs and significance levels are identical. All coefficients but one are within 10% of the previous estimates, and the last is a decrease in magnitude from -0.03 to -0.02.

The instrumental variable results are similar. Transfers have a positive and significant effect upon height for age and upon verbal skills. The link to cognitive development is less robust, dropping from significant at the 1% level to just the 10% level, but the point estimate is similar. With and without the instrumental estimation, the effect of randomization to the early treatment group is insignificant for all variables but emotional development, for which a decrease in problems is observed. When we use the 2SLS, the estimated effect of transfers ceases to be significant, though the point estimate is nearly unchanged.

Conclusion

The effects of transfer amounts are stronger than the effects of 18 additional months on the program for all outcomes but the Strengths and Difficulties Questionnaire, for which only program duration is significant. Higher transfer amounts are associated with both physical and cognitive development, being linked to improvements in height for age and on the verbal WASI scores. Effects are not statistically significant but point estimates are positive for the effects of additional transfers on BMI for age and for the cognitive performance WASI scale.

Improving our methodology through use of the potential transfers instrument and adding new interaction terms brings out many useful pieces of information. First, estimates of the program and transfer effects in Fernald, Neufeld, and Gertler (2009) are largely robust. Point estimates are quite similar across specifications. The effects of transfer amounts are stronger than the effects of 18 additional months on the program for all outcomes but the Strengths and Difficulties Questionnaire. Higher transfer amounts are associated with both physical and cognitive development, being linked to improvements in height for age and on the verbal WASI scores. Effects are not statistically significant but point estimates are positive for the effects of additional transfers on BMI for age and for the cognitive performance WASI scale.

We conclude that income, not information, is the key to achieving improvements in child development outcomes. This is in line with Becker and Tomes' (1986) predictions that efficiency gains may come from providing capital to poor families. Also, this paper shows that height for age can be improved over a longer time period than some nutritionists have assumed. While younger children may be more strongly affected by a lack of nutrition than older children, an increase in income can enable at least some catch-up growth.

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Table 1. Comparison of treatment and control groups

	Early treatment Mean (se) or %	Late treatment Mean (se) or %	Test for significant difference†
Actual transfer amount (10000s of pesos)	4.81 (0.08)	4.37 (0.09)	t = 2.17 **
Potential transfer amount (IV for actual transfers)	6.96 (0.13)	5.96 (0.17)	t = 4.81***
Household size in 1997	6.34	6.33	t = 0.04
Asset Index (principal components)	-0.41 (0.02)	-0.35 (0.02)	t = 0.99
Hectares of land	1.50 (0.07)	1.56 (0.10)	t = 0.26
Child age in months	116.9	117.0	t = 0.11
Female	49%	49%	χ^2 = 0.00
Household head speaks indigenous language	42%	42%	χ^2 = 0.00
Land has improved water access	23%	32%	χ^2 = 2.76*
Electricity access in 1997	66%	62%	χ^2 = 0.58
Household owned draft animal(s) in 1997	32%	32%	χ^2 = 0.00
Household owned small animals in 1997	79%	80%	χ^2 = 0.11
Child's father lives in same household	82%	81%	χ^2 = 0.21
Child's father attended primary school	80%	82%	χ^2 = 0.41
Child's mother attended primary school	81%	79%	χ^2 = 0.64

†Test statistic is t for continuous variables and χ^2 for dichotomous variables, both clustered at the community level.

*significant at 10% level ** significant at 5% level *** significant at 1% level

Table 2. Effects of program enrollment & transfers, with and without IV's

	Treatment	Transfers	N	R^2		Treatment	Transfers (IV)	N	R^2
	β (se)	β (se)				β (se)	β (se)		
Height for age z	0.06 (0.05)	0.03*** (0.01)	1710	0.14		0.04 (0.06)	0.05*** (0.02)	1710	0.14
BMI for age z	-0.03 (0.06)	-0.00 (0.01)	1705	0.04		-0.04 (0.07)	0.01 (0.02)	1705	0.04
Verbal WASI	1.21 (1.12)	0.67*** (0.12)	1661	0.19		1.21 (1.17)	0.75*** (0.23)	1661	0.19
Cognitive WASI	-1.15 (1.07)	0.42*** (0.13)	1661	0.09		-1.14 (1.05)	0.37* (0.25)	1661	0.09
SDQ	-0.14** (0.07)	-0.02*** (0.01)	1751	0.04		-0.14** (0.07)	-0.02 (0.02)	1751	0.04

* significant at 10% level ** significant at 5% level *** significant at 1% level

Results from five OLS and five iv regressions, with community level fixed (non-iv) or random effects (iv). Other explanatory variables in the regressions include child sex, indicators for 6-month birth cohorts, indicators for water and electricity access in 1997, hectares of land owned, whether the household speaks an indigenous language, whether the household owns farm animals, an asset index created using principal components analysis, whether the child's father lives in the household, indicators for the child's mother and father having attended primary school, and dummy variables indicating state of residence.