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Cash Transfers, Behavioral Changes, and Cognitive Development in Early Childhood: Evidence from a Randomized Experiment†

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Cash transfer programs have become extremely popular in the developing world. A large literature analyzes their effects on schooling, health and nutrition, but relatively little is known about possible impacts on child development. This paper analyzes the impact of a cash transfer program on early childhood cognitive development. Children in households randomly assigned to receive benefits had significantly higher levels of development nine months after the program began. There is no fade-out of program effects two years after the program ended. Additional random variation shows that these impacts are unlikely to result from the cash component of the program alone. (*JEL* H23, I15, J13, O15)

Development in early childhood is an important predictor of success throughout life. In developed countries, children with low levels of cognitive development before they enter school have lower school achievement and earn lower wages (Currie and Thomas 2001; Case and Paxson 2008). In developing countries, low levels of cognitive development have been tied to poor performance in school in a number of settings (see Grantham-McGregor et al. 2007 for a review).

Evidence from the medical and economic literature suggests that outcomes in early childhood are malleable (Heckman 2006; Knudsen et al. 2006). Randomized

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trials in the United States show that children who benefited from intensive preschool interventions have higher school attainment, better test scores, lower rates of criminality, and earn higher wages in adulthood (Currie 2001; Schweinhart 2005), although the impacts appear to be concentrated among girls (Anderson 2008). A well-known study from Jamaica shows that children randomly assigned to receive home-based early stimulation have substantial improvements in cognitive development and subsequent school performance (Grantham-McGregor et al. 1991, 1997; Walker et al. 2000; Powell et al. 2004). Nonexperimental evidence suggests that preschool attendance is associated with better school performance in Argentina (Berlinski, Galiani, and Gertler 2009) and Uruguay (Berlinski, Galiani, and Manacorda 2008). There is also a large literature documenting the impacts of nutritional supplementation programs, including substantial evidence from randomized control trials (see Walker et al. 2007 for a review). In Guatemala, children exposed to a nutritional intervention have better reading comprehension and perform better on tests of cognitive development in adulthood, and earn higher wages (Maluccio et al. 2009; Hoddinott et al. 2008).

A reasonable amount of evidence is therefore available on how the cognitive development of young children responds to supply-side interventions, including access to preschool, or food supplementation programs. Much less is known about interventions that attempt to directly affect the investments parents make in child development—either by relieving financial constraints or by changing how resources are allocated within households.

This paper analyzes the impact of a cash transfer program on development in early childhood. The program, known as *Atención a Crisis*, made sizeable payments to poor households in rural areas in Nicaragua. There are a variety of reasons why one might expect a program like *Atención a Crisis* to improve development in early childhood. Children in better-off households generally have higher levels of development than those in poorer households in developing countries.[1] These associations may not be causal—rather, they may reflect a correlation between child development and parental wealth, parental behavior, or genetic endowments. However, if cash transfers, such as those made by *Atención a Crisis*, allow households to spend more on nutritious foods, early stimulation, or health care, this may result in improvements in child development.

There are other features of the *Atención a Crisis* program that could result in improvements in child development. Beneficiaries were told that transfers were intended to improve the diversity and nutrient content of children's diets and to buy school material. The social marketing of the program may have transmitted knowledge about child-rearing practices. It may also have affected how transfer income was used through a flypaper or labeling effect.[2] Such changes in behaviors could be further enhanced through social interactions with other program beneficiaries and

[1] References include Paxson and Schady (2007) and Schady (2011) on Ecuador; Halpern et al. (1996) on Brazil; Ghuman et al. (2005) on the Philippines. See also Schady (2006) for a discussion.

[2] See Thaler (1999) for a general discussion. Fraker, Martini, and Ohls (1995) presents evidence for the United States, although these results have been challenged by Hoynes and Schanzenbach (2009). See also Kooreman (2000) for the Netherlands, Jacoby (2002) for the Philippines, and Islam and Hoddinott (2008) for Guatemala. Edmonds (2002) finds no evidence of labeling effects for child benefit income in Slovenia.

peer pressure (Macours and Vakis 2009). Finally, *Atención a Crisis* transfers were made to women, and income controlled by women may be spent in a way that benefits children more than income that is controlled by men.[3]

A large number of studies have assessed the impact of cash transfers, conditional and unconditional, on health status, nutrition, and education.[4] In contrast, we are aware of only two earlier papers on the impact of cash transfers on child development in developing countries. Fernald et al. (2008) suggest that larger transfers made by the PROGRESA program in Mexico resulted in better nutritional status, improved motor skills, and higher levels of cognitive development. However, the variation in the amount of cash that is used to identify these effects may be endogenous (Attanasio, Meghir, and Schady 2010). Paxson and Schady (2010) use random assignment in the roll-out of the *Bono de Desarrollo Humano* (BDH) cash transfer program in Ecuador to analyze the effects on health and development of children between three and six years of age. They show that cash transfers resulted in an improvement of about 0.18 standard deviations in development among the poorest quartile of children in their sample, with no effects among somewhat less poor children.

Our analysis adds to the existing literature in a number of important ways. To the best of our knowledge, this is the first paper on the impact of cash transfers on child development in a developing country that uses data spanning the period before, during, and after the program ended. We show that children in households that were randomized into the *Atención a Crisis* program had significantly higher levels of development nine months after households started receiving transfers. Program effects of a similar magnitude are still apparent two years after *Atención a Crisis* had been discontinued and transfers had ended. Thus, there appears to be no fade-out of treatment effects among beneficiaries of the *Atención a Crisis* program, at least over the period covered in our study. This stands in contrast with the results from evaluations of a number of preschool programs in the United States (see Currie and Thomas 2000 and Garces, Thomas, and Currie 2002 on Head Start, and Heckman et al. 2010 on the Perry Preschool Program), the results of a randomized evaluation of a food supplementation program in Jamaica (Walker et al. 2000, 2005), and the results of the evaluation of PROGRESA on child height (Neufeld et al. 2005, and the discussion in Fiszbein and Schady 2009). On the other hand, a parenting program in Jamaica appeared to sustainably change behaviors, and there was no fade-out of program effects on child development (Walker et al. 2000, 2005).

Another important contribution of this paper is that it analyzes the extent to which changes in child development can be explained solely by the cash component of the *Atención a Crisis* program. We provide two pieces of evidence that strongly suggest that this is unlikely. First, the *Atención a Crisis* program randomly assigned a group

[3] For example, Thomas (1994), Hoddinott and Haddad (1995), Doss (2006), and Schady and Rosero (2008) show that income controlled by women is associated with higher expenditures on food. Macours and Vakis (2010) show nonexperimental evidence on the positive impact of mother's seasonal migration on children's cognitive development that is consistent with this hypothesis. Lundberg, Pollack, and Wales (1997) and Ward-Batts (2008) present quasi-experimental evidence from the United Kingdom to argue that income controlled by women is more likely to be spent on clothing for women and children than income controlled by men.

[4] The literature is extensive—see Fiszbein and Schady (2009) for a review. Maluccio and Flores (2005) look at the effects of an earlier cash transfer program in Nicaragua.

of households to a variant of the basic treatment that included a substantially larger cash transfer. Relative to households in the basic treatment group, households that received the larger cash transfer had higher expenditure levels during and (in particular) after the program, but they did not have better child development outcomes.

Second, we analyze changes in a number of intermediate inputs into the production of child development, including the consumption of food, early stimulation, and the utilization of preventive health services. The changes in the use of these inputs among treated households, which persisted even after the program had ended, are inconsistent with a simple story of higher overall expenditure levels among *Atención a Crisis* beneficiaries. Hence, other program features, such as the social marketing that accompanied the transfers, or the fact that transfers were made to women, or both, are likely to be important in explaining the changes in child development we observe. In sum, then, our paper goes beyond Fernald et al. (2008) and Paxson and Schady (2010) in analyzing impacts during and after the intervention, in showing that the impact is due not just to the cash transfer, and in establishing impact on intermediate inputs, indicating the plausible underlying mechanisms.

The rest of the paper proceeds as follows. In Section I, we describe the *Atención a Crisis* pilot program and the data, in particular the measures of cognitive development. Section II discusses methods. We present results in Section III. Specifically, Section IIIA presents the main results, IIIB considers differences between variations of the treatment received by different households, and IIIC presents evidence on the change in the use of various inputs into child development by *Atención a Crisis* beneficiaries. Section IV concludes.

I. Program Design, Data, Identification, and Early Childhood Development Outcomes

A. *The Atención a Crisis Pilot Program*

The *Atención a Crisis* pilot program was implemented between November 2005 and December 2006 by the Ministry of the Family in six municipalities in rural Nicaragua. We provide a detailed description of the program in online Appendix 1. The program included a careful evaluation based on random assignment. Randomization was conducted as follows. First, among all communities in the six municipalities, 56 intervention and 50 control communities were randomly selected through a lottery. Second, baseline data were collected in both treatment and control communities. These data were used to define program eligibility based on a proxy means test. Around 10 percent of households (and only 5 percent of households with children under 6 years of age) in treatment and control communities were ineligible for the program because their estimated baseline expenditures, as determined by the proxy means, was above the predefined threshold. This process resulted in the identification of 3,002 households to participate in the program. A further 3.7 percent of households that had originally been deemed eligible by the proxy means were reclassified as ineligible after a process of consultation with community leaders, and a corresponding 3.7 percent that had originally been deemed ineligible were

reclassified as eligible. To avoid any possibility of selection bias from these choices, we use the *original* eligibility as the intent-to-treat.

In communities randomly selected to participate in the *Atención a Crisis* program, the primary child caregiver (known as the "titular"), who in the vast majority of cases was a woman, was invited to a registration assembly where the program objectives and various components were explained. At the end of the assembly, a lottery took place in each community. Participation in the assemblies and lotteries was close to 100 percent. On the basis of this lottery, all eligible households within each community were assigned to one of three treatments.

Households in Group 1 were offered a cash transfer, paid to the "titular" every two months. For households with children ages 0–5, this transfer was in principle conditional on regular preventive health check-ups. However, in practice, this conditionality was not monitored, and households were not penalized for noncompliance. Households with children between 7 and 15 years old who had not finished primary school received an additional educational transfer, conditional on the school enrollment and regular attendance of those children. The education conditionality was monitored in practice. The basic *Atención a Crisis* intervention was modeled after an earlier CCT program in Nicaragua, the *Red de Protección Social* (RPS).[5] On average, transfers made to this group represented 15 percent of per capita expenditures of the average recipient household in our sample over the year in which it was implemented.[6] We refer to this treatment as the basic treatment.

Households in Group 2 received a cash transfer that was identical to that received by households in Group 1. In addition, they were offered a scholarship that allowed one of the household members to choose among a number of vocational training courses offered at the municipal headquarters. These household members also participated in labor market and business-skill training workshops organized in their own communities. We refer to this treatment as the training package.

Households in Group 3 received a cash transfer that was identical to that received by households in Group 1. In addition, they were offered a lump-sum payment to start a small nonagricultural activity. This lump sum was conditional on the household developing a business development plan. It was paid out between the end of May and September 2006.[7] The value of the lump-sum payment represented approximately 11 percent of per capita expenditures of the average recipient household over the year in which it was implemented. A household in Group 3 therefore was eligible for transfers equivalent to approximately 26 percent of annual expenditures. We refer to this treatment as the lump-sum payment package.

In addition, all beneficiaries of the *Atención a Crisis* program, regardless of the treatment they were assigned to, were exposed to repeated information and communication efforts by program staff during enrollment and paydays. These stressed the importance of varied diets, health, and education, and were meant to change

[5] See Maluccio and Flores (2005) for the impacts on education, health, and nutrition of the RPS program.

[6] Households received a transfer of US $145 if they had no children or only children younger than 7. In addition, households with children between 7 and 15 years old enrolled in primary school received US $90 per household, and a further US $25 per child.

[7] Households received US $175 at the end of May, and an additional US $25 in September, conditional on having started the nonagricultural activity that was planned.

household investment and consumption patterns. Beneficiaries were also expected to attend regular meetings with local program promoters to talk about the objectives and conditionalities of the program.

Program take-up was high. More than 95 percent of all households randomized into the three treatment groups signed up for the program and took up the basic cash transfer.[8] A small fraction of those households, less than 5 percent, did not collect the full amount of the transfer they were eligible for because they had not complied with the school enrollment and attendance requirements. Take-up of the additional benefits offered to groups 2 and 3 was also high—89 percent for the vocational training courses, and close to 100 percent for the lump-sum payment.[9] Contamination of the control group was negligible (one household).

B. *Data*

Baseline data for the evaluation were collected in April–May 2005. A first follow-up survey was collected in July–August 2006, nine months after the households had started receiving payments. The sample includes the 3,002 eligible households in the treatment group, and a random sample of 1,019 eligible households in the communities that were assigned to the control group. A second follow-up survey, covering the same households as those included in the first follow-up, was collected between August 2008 and May 2009 (henceforth referred to as 2008). At this point, households had stopped receiving transfers for an average of two years.

Attrition over the study period was minimal, less than 1.3 percent in 2006 and 2.4 percent in 2008. Attrition is uncorrelated with treatment status, and does not differ across treatment packages. The baseline characteristics of the full sample of households and those that could be located at follow-up are very similar. We further discuss possible concerns regarding attrition and missing test data in online Appendix 2.

All three surveys included comprehensive information on household socioeconomic status, including detailed expenditure modules,[10] extensive information on child health and nutrition, including child height and weight, and one measure of child cognitive development, the TVIP. The TVIP is the Spanish-speaking version of the Peabody Picture Vocabulary Test (PPVT), a test of receptive vocabulary that can be applied to children 36 months and older (Dunn et al. 1986).

Both follow-up surveys included a large number of tests to assess child development. Social-personal, language, fine motor, and gross motor skills for all children were assessed using the four sub-scales of the Denver Developmental Screening

[8] The main reason households did not take up the program was the fact that some originally eligible households were deemed ineligible by local leaders after the initial assignment—see above. A small number of households had also migrated out of the communities after baseline. In order to avoid any selection bias, we treat all of these households as *eligible*.

[9] About 10 percent of the business development plans were initially turned down by the Ministry of the Family, which oversaw the program. These proposals were sent back to the households and virtually all of them developed a new plan, with the help of technical assistance (the few exceptions being households that had migrated out).

[10] These modules were taken from the 2001 Nicaragua Living Standards Measurement Study (LSMS) survey. The expenditure module includes detailed information on various expenditure categories. For example, food expenditures include questions about 63 food items, and include actual expenditures, home production, and food consumed outside the home.

Test (Frankenberg and Dodds 1996). The Denver can be applied to children as young as one month of age. A slightly modified version of the Denver is used for child monitoring by the national early childhood stimulation program in Nicaragua, which suggests that the test is appropriate for the population we study.

For children age 36 months and older, we applied five additional tests. The first of these is the TVIP. We also use a short-term memory test from the McCarthy test battery, and a test of associative memory drawn from the Woodcock-Johnson-Muñoz battery of cognitive abilities (Woodcock and Muñoz 1996; Schrank 2006; Schrank et al. 2005); the test of associative memory was only applied in the second follow-up survey. In both the first and second follow-up surveys, we included a test of leg motor development from the McCarthy test battery (Boivin et al. 1995). The final test we use is the Behavior Problem Index (BPI), which is based on the caregiver's report of the frequency that a child displays each of 29 problematic behaviors, with responses coded as "never," "sometimes," and "often" (Baker and Mott 1989). We use the number of behavioral problems for which a caregiver answers "often."[11]

All of the tests were carefully piloted in the field, and adjustments were made, as necessary. Many of these tests have been applied in similar populations in Latin America, including in the evaluations of cash transfer programs in Ecuador and Mexico (see Paxson and Schady 2010 and Fernald et al. 2008, respectively). An important advantage of the tests we use, with the exception of the BPI and a subset of items in the Denver, is that they provide observed, as opposed to parent-reported, measures of child development.[12] This substantially reduces concerns about reporting biases. Details of all of the tests we use are provided in online Appendix 3.

The two follow-up surveys also include information on stimulation, birthweight, preventive health care, and caregivers' mental health. Mental health was measured using the Center for Epidemiological Studies Depression scale (CESD), a widely used measure of depression which consists of 20 questions on self-reported depression (Radloff 1977). Finally, caregivers' observed parenting behavior was registered through a shortened version of the HOME score, an index of 11 positive and negative behaviors that the enumerator observes during interviewing and testing (Bradley 1993; Paxson and Schady 2007, 2010).

Table 1 summarizes the baseline characteristics of households in our sample, focusing on socioeconomic status and child health. It shows that households and children are disadvantaged in a number of important ways. Expenditure levels are very low. Turning the local currency units (*Córdobas*) into US dollars shows that 81 percent of households in our sample have per capita expenditures that are below $1 per capita per day. The mean years of schooling of mothers is 4 years, and 66 percent have not completed primary school. The mean years of schooling of fathers is equally low, and 72 percent have not completed primary school. Children in this sample have substantial health problems—27 percent are stunted (have height for their age that is more than two standard deviations below that of a reference

[11] Unlike the other outcomes we study, behavioral problems do not necessarily indicate a delay, as there are no benchmarks or established ages at which they are predicted to decrease.

[12] For the Denver subtests, there are no significant differences between children in the treatment and control groups in the likelihood that items were administered by direct observation rather than caregivers' report (see online Appendix 3).

TABLE 1—BASELINE CHARACTERISTICS AND RANDOMIZATION CHECKS

	N	Control	Treatment	*P*-value diff. T − C	*P*-value diff. T1 = T2 = T3	*P*-value diff. T1 = T3
Child-specific characteristics						
All children						
Male	4,245	0.49	0.50	0.376	0.912	0.727
Age in months when transfers started	4,245	22	21	0.194	0.465	0.488
Mother lived in household at baseline	4,245	0.95	0.97	0.183	0.699	0.632
# years education mother	4,005	4.21	4.05	0.557	0.075*	0.025**
# years education father	4,007	3.88	3.81	0.773	0.572	0.877
Children age 3–6 at baseline						
TVIP (vocabulary recognition) test score	1,066	5.37	6.23	0.207	0.396	0.290
Children age 0-5 at baseline						
Weight-for-Age z-score	2,377	−0.88	−1.06	0.094*	0.510	0.466
Height-for-Age z-score	2,368	−1.08	−1.27	0.109	0.081*	0.673
Weight-for-Height z-score	2,383	−0.16	−0.18	0.799	0.829	0.724
Birth weight	2,415	6.76	6.75	0.947	0.340	0.193
Weighed in last 6 months	2,503	0.93	0.90	0.178	0.698	0.817
Received vitamins in last 6 months	2,503	0.75	0.68	0.070*	0.541	0.276
Received deworming drugs in last 6 months	2,503	0.59	0.51	0.036**	0.578	0.319
Household-level characteristics						
Male household head	2,407	0.84	0.85	0.539	0.397	0.215
Household size	2,407	6.05	5.90	0.344	0.732	0.446
# hh members 0–5 years old	2,407	1.06	1.04	0.705	0.686	0.655
# hh members 5–14 years old	2,407	1.69	1.70	0.954	0.382	0.627
# hh members 15–24 years old	2,407	1.21	1.17	0.515	0.601	0.853
# hh members 25–64 years old	2,407	1.88	1.84	0.473	0.423	0.205
# hh members more than 65 years old	2,407	0.18	0.13	0.061*	0.757	0.625
Number of rooms in the house	2,407	1.63	1.57	0.498	0.040**	0.387
Time to school (minutes)	2,407	0.31	0.26	0.149	0.062*	0.683
Time to health center (minutes)	2,407	1.28	1.17	0.493	0.968	0.802
Time to municipal headquarters (minutes)	2,407	1.69	1.58	0.523	0.940	0.763
Owns toilet/latrine	2,407	0.75	0.72	0.461	0.827	0.887
Access to water	2,407	0.11	0.13	0.646	0.104	0.151
Access to electricity	2,407	0.36	0.38	0.790	0.633	0.780
Own land	2,407	0.64	0.63	0.731	0.829	0.543
Total consumption per capita (córdobas)	2,407	4,723	4,635	0.809	0.841	0.586
Food consumption per capita (córdobas)	2,407	3,333	3,110	0.408	0.632	0.364
Proportion of food in total expenditures	2,407	0.70	0.68	0.132	0.376	0.165
Proportion of staples in all food exp.	2,399	0.59	0.58	0.871	0.389	0.172
Proportion of animal proteins in all food exp.	2,399	0.16	0.16	0.868	0.372	0.479
Proportion of fruit and vegetables in all food exp.	2,399	0.05	0.05	0.573	0.375	0.168

Notes: *P*-values based on standard errors clustered by community. Data for all children and household-level characteristics are based on all children in 2008 sample (with at least 1 of the 11 outcomes in Table 3 available) that was either younger than 6 years when transfers started or born to baseline household members since the baseline. Calculations do not include data on children born after the baseline.

***Significant at the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.

population). Weight-for-height is not particularly low. The composition of food expenditures shows that a very high proportion of consumption consists of staples (59 percent), in particular tortillas, rice, and beans. Much smaller proportions of food consumption are animal products (16 percent) and, in particular, fruits and vegetables (5 percent). This suggests that lack of balance in diets, rather than insufficient overall caloric intake, may be part of the explanation for the nutritional deficiencies in this population.

Table 2 focuses on our measures of child development. It reports the fraction of children in the control group who are in the bottom 25 percent and, separately,

TABLE 2—FREQUENCY OF DELAY IN CONTROL COMMUNITIES COMPARED TO INTERNATIONAL NORM IN 2006

	Children 0–83 months old					
	Denver					
	Social-personal	Language	Fine motor	Gross motor	Weight	Height
Child is in lowest 25 percent of international distribution						
All	0.65	0.82	0.60	0.46	0.56	0.64
Child is in lowest 10 percent of international distribution						
All	0.47	0.60	0.39	0.29	0.37	0.47
Boys	0.49	0.63	0.41	0.30	0.38	0.49
Girls	0.44	0.57	0.37	0.29	0.36	0.45
0–35 months	0.30	0.48	0.28	0.41	0.34	0.41
36–59 months	0.48	0.59	0.53	0.27	0.37	0.54
60–83 months	0.68	0.77	0.41	0.14	0.41	0.49

	Children 36–83 months old			
	TVIP Receptive language	WJ Associative memory	Mccarthy Short memory	Mccarthy Leg motor
Child is in lowest 25 percent of international distribution				
All	0.96	0.87	0.84	0.40
Child is in lowest 10 percent of international distribution				
All	0.84	0.75	0.58	0.23
Boys	0.83	0.72	0.57	0.23
Girls	0.85	0.77	0.59	0.24
36–59 months	0.70	0.78	0.56	0.21
60–83 months	0.98	0.75	0.61	0.25

Notes: All tests are from 2006, except WJ associative memory from 2008. To calculate delays, international standardized scores were calculated for each test. For the Denver, which consists of various tasks, each of which is age standardized, children are categorized in the lowest 25 percent (resp. 10 percent) if they are in the lowest 25 percent (10 percent) for at least one of the tasks.

the bottom 10 percent of the international distribution that was used to standardize a given test.[13] The table shows that a very large fraction of children in our sample is delayed, although this varies considerably by outcome. The fraction of children who are behind for their age is largest for the measures of language—96 percent of children in our sample are in the lowest quartile of the distribution of the TVIP, and 84 percent have a score that places them in the lowest decile. Comparable numbers for the measure of language in the Denver test place 82 percent of children in the lowest quartile, and 60 percent in the lowest decile. A very large fraction of children in our sample is also delayed in memory—84 percent place in the lowest quartile of the test of short-term memory and 58 percent in the lowest decile of the distribution used to standardize the test. In the case of the test of associative memory, 87 percent of children place in the lowest quartile and 75 percent in the lowest decile.

[13] For this purpose we use data from the first follow-up survey for all tests except for the test of associative memory, which was only collected in the second follow-up survey. Results are very similar if we use the second follow-up survey for all of these calculations.

These delays in language and memory are severe. For instance, the numbers for the TVIP imply that 85 percent of the children in our sample are at least 21 months delayed in receptive vocabulary. However, the implied delays are reasonably consistent with those observed among other populations with high poverty levels and low education in Latin America.[14]

Turning to other domains of child development, Table 2 shows that outcomes are somewhat better on the social-personal scale of the Denver—47 percent of children in the sample place in the lowest decile—and for fine motor skills—39 percent place in the lowest decile for this outcome. Children in our sample perform even better in terms of gross motor skills. A much smaller fraction of children, 29 percent, place in the lowest decile of the distribution of the Denver, and 23 percent place in the lowest decile of the McCarthy leg motor scale. In addition to documenting the large fractions of children in our sample that are delayed, Table 2 shows that there are no obvious differences in delays between boys and girls. However, delays increase with child age for some outcomes.

It is more likely that cash transfers like those made by *Atención a Crisis* will result in improvements in cognitive development if there are socioeconomic gradients in these outcomes. Figure 1 presents nonparametric (Fan) regressions of each standardized outcome on log per capita expenditures among children in control communities (Fan and Gijbels 1996). The figure shows positive socioeconomic gradients in most measures of child development. Gradients appear to be steepest for language (in particular, for the TVIP), height-for-age, and weight-for-age.

Table 1 checks for balance between households randomly assigned to receive *Atención a Crisis* transfers and the control group (fourth column) and between households randomly assigned to the three treatment groups (basic treatment, training, lump-sum transfer—last two columns). The table shows that, by and large, random assignment equated the characteristics of households and children randomly assigned to different groups. Only one of 35 characteristics, whether the child received deworming drugs, is significantly different between treatment and control groups at the 5 percent level. For only one characteristic, the number of rooms in a house, can we reject the null of equal baseline means across the three treatment groups at the 5 percent level. And, for only one characteristic, mother's education, can we reject the null of no differences between the basic treatment and lump-sum payment, which is the focus of the results we present on differences across treatment groups.

Although random assignment was successful, there are some small differences at baseline between households that were assigned to treatment and control groups. For example, children in the treatment group have somewhat lower height and weight than

[14] Our analysis shares two tests with the results reported in Paxson and Schady (2007, 2010) and Schady (2011), namely the TVIP and the Woodcock-Johnson measure of associative memory. The average child in the sample from Ecuador places in the eleventh percentile of the distribution of the TVIP, and in the thirteenth percentile of the test of associative memory. In our sample of children from Nicaragua, the average child places in the sixth percentile of the distribution of the TVIP and the tenth percentile of the test of associative memory. We note that the sample of children from Ecuador is considerably better off. Thirty-four percent of households in the Ecuador study have consumption levels that are below US$1 per capita per day, compared to 82 percent of households in this study. There are also marked differences in parental education, which is very robustly associated with performance on the cognitive tests—the average education of mothers in the Ecuador sample is 6.7, compared to 4.2 for the sample used in our paper.

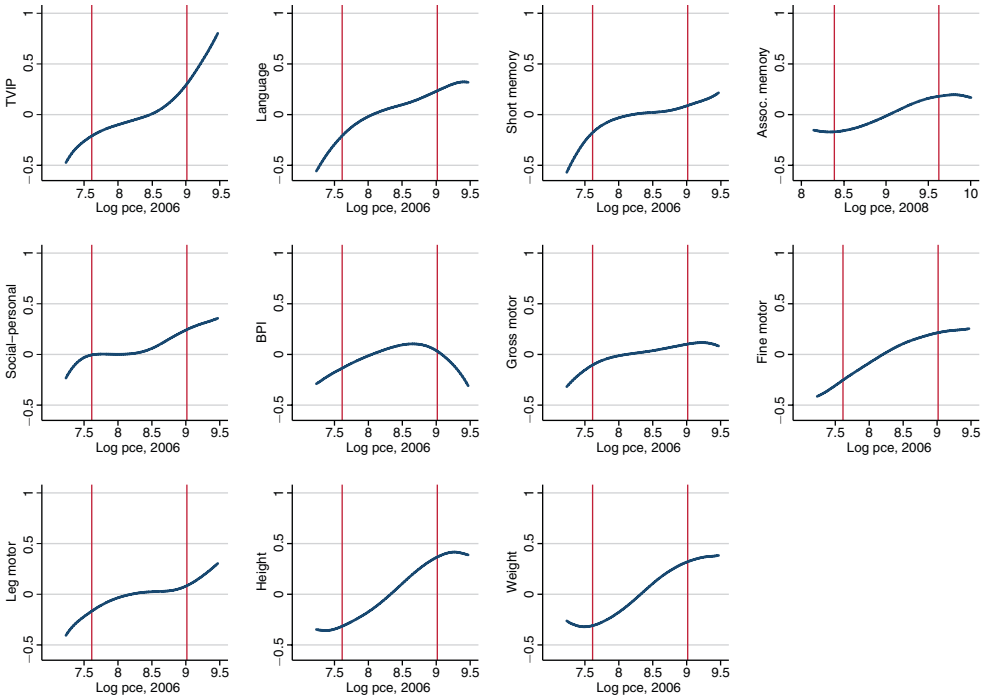

FIGURE 1. SOCIO-ECONOMIC GRADIENTS IN CHILD OUTCOMES IN CONTROL COMMUNITIES

Notes: Outcomes for 2006, except associative memory, which is for 2008. All outcomes are standardized by subtracting the mean and dividing by the standard deviation of the control group. Sample includes children under 6 years old when the transfers started and all children born in sample households since. For the Denver (social-personal, language, fine motor, gross motor), the sample includes children up to 83 months. For the TVIP (receptive language), McCarthy (memory, leg motor), WJ (associative memory) and BPI, the sample includes children 36–83 months. Height-for-age and weight-for-age is for all children. For the Denver test, calculations are based on the number of delays. For TVIP, McCarthy, and WJ, calculations are based on raw test scores. Vertical lines are included at tenth and ninetieth percentiles of log per capita expenditures in control communities. Fan regressions with bandwidth of 0.99. 2.5 percent highest and lowest outliers of log(pce) trimmed from graph.

those in the control group. They are also less likely to have been weighed, and to have received vitamins or deworming drugs in the six months prior to the baseline survey. These differences suggest that it may be important to control for the baseline characteristics of households and children when estimating *Atención a Crisis* program effects on child development. We return to this point below.

II. Methods

We estimate child-level intent-to-treat regressions of the following form:

$$Y_k = \alpha_k T + \beta_k \mathbf{X} + \varepsilon_k, \quad k = 1 \ldots K, \tag{1}$$

where Y_k is the kth outcome (out of 10 in the first follow-up survey, 11 in the second follow-up survey); T is a treatment indicator, which takes on the value of one for children in communities that were randomly assigned to receive *Atención a Crisis* benefits; and $\mathbf{X}$ is a set of controls (including an intercept). To make it easier to draw

comparisons across outcomes, we first convert each outcome into a within-sample z-score by subtracting the sample mean and dividing by the standard deviation of the control group.[15] Also, we reverse the signs on the BPI, so that higher values correspond to "better" outcomes (as with the other outcomes). The coefficients on the treatment indicator therefore measure effect sizes in standard deviation units.

In one set of specifications, $\mathbf{X}$ includes only controls for the child's age when the transfers started, in single-month intervals, and an indicator for the child's gender. In another set of specifications, $\mathbf{X}$ also includes a number of baseline characteristics: age and gender of the household head, the years of schooling of the mother, the number of household members, the fraction of members in five age categories, birth weight, height-for-age, weight-for-age, TVIP score, whether a child has been weighed, received deworming medicine, and vitamin A in the last six months, baseline community averages of height-for-age, weight-for-age, and TVIP score, and municipal fixed effects.[16] Including these controls helps adjust for small baseline differences between treated and control groups, and may also make the estimated program effects more precise. Standard errors adjust for clustering at the community level.

In addition to estimating the effect for individual outcomes, we estimate the average treatment effect, across all outcome measures, and separately for the subsets of six cognitive and behavioral outcomes and five health and motor outcomes:

$$\bar{\alpha} = \frac{1}{K}\sum_{k=1}^{K}\hat{\alpha}_k. \tag{2}$$

We estimate (1) or (2) by running seemingly unrelated regressions (SUR) for all (or a subset) of the outcomes, and use the estimated variance-covariance matrix of the estimates to calculate the standard error of $\bar{\alpha}$ (see Kling, Liebman, and Katz 2007; Duflo et al. 2008).

We also estimate intent-to-treat regressions that allow for separate effects for households that were randomly assigned to the three *Atención a Crisis* treatment packages:

$$Y_k = \gamma_k T_1 + \eta_k T_2 + \lambda_k T_3 + \beta_k \mathbf{X} + \varepsilon_k, \quad k = 1 \ldots K, \tag{3}$$

where T_1, T_2, and T_3 correspond to the basic treatment, the training package, and the lump-sum payment package, respectively. Finally, to tease out the role of higher expenditures on child development, we limit the sample to households assigned to either $T1$ or $T3$, and run regressions of the following form:

$$Y_k = \theta_k T_3 + \beta_k \mathbf{X} + \varepsilon_k, \quad k = 1 \ldots K, \tag{4}$$

[15] We use the standard deviation of the control group in 2006 for both years in order to be able to compare magnitudes across years.

[16] In those cases where there are missing values for the covariates, we include the sample mean. However, our results are robust to including only covariates with very few missing values.

TABLE 3—IMPACTS ON INDIVIDUAL TESTS IN 2006 AND 2008

	Cognitive and socio-emotional outcomes					
	TVIP	Language	Short memory	Assoc. memory	Social-personal	BPI
2006: All children						
Age & gender controls	0.201***	0.108*	0.087		0.114**	−0.007
	(0.075)	(0.055)	(0.056)		(0.050)	(0.088)
Extended controls	0.228***	0.139***	0.156***		0.130***	−0.048
	(0.062)	(0.050)	(0.044)		(0.047)	(0.084)
N	1,817	3,287	1,827		3,307	1,620
2008: All children						
Age & gender controls	0.104	0.060	0.0789	0.073	0.056	0.016
	(0.100)	(0.056)	(0.050)	(0.062)	(0.052)	(0.060)
Extended controls	0.094	0.093**	0.086*	0.105**	0.098**	0.021
	(0.078)	(0.045)	(0.044)	(0.046)	(0.046)	(0.063)
N	2,990	3,095	3,011	3,015	3,097	2,863

	Health and motor development outcomes				
	Gross motor	Fine motor	Leg motor	Height-for-age	Weight-for-age
2006: All children					
Age & gender controls	−0.031	0.024	0.023	−0.063	−0.061
	(0.058)	(0.064)	(0.092)	(0.091)	(0.081)
Extended controls	−0.006	0.038	0.130*	0.072**	0.036
	(0.046)	(0.063)	(0.076)	(0.034)	(0.037)
N	3,253	3,265	1,838	3,082	3,082
2008: All children					
Age & gender controls	0.056	0.099*	−0.036	−0.096	−0.065
	(0.064)	(0.051)	(0.046)	(0.094)	(0.082)
Extended controls	0.102	0.156***	0.006	0.045	0.029
	(0.064)	(0.039)	(0.034)	(0.031)	(0.043)
N	3,080	3,085	1,881	4,185	4,185

Notes: Standard errors (in parentheses) adjust for clustering at the community level. Controls include individual-level controls (dummies for child gender and month dummies for child age, the years of schooling of the mother, baseline height-for-age, weight-for-age, TVIP score, and birthweight), household-level controls (age and gender of the household head, the number of household members, the fraction of members in five age categories), and community-level controls (baseline community averages of the height-for-age, weight-for-age, TVIP score, participation in growth monitoring, and vitamin and deworming intake, and municipal fixed effects). Variations in sample size across tests are mainly driven by the fact that different tests apply to different age groups. Within age groups, it is also due to a limited number of missing observations (see online Appendix 2 for details).

***Significant at the 1 percent level.

**Significant at the 5 percent level.

*Significant at the 10 percent level.

In this case, the coefficients θ_k are an estimate of the difference in outcomes between children in households assigned to the basic treatment and those that in addition were assigned to receive the lump-sum payment.

III. Results

A. *Overall Program Effects*

Our main results on the effect of the *Atención a Crisis* program on child health and development are reported in Tables 3 and 4. Table 3 focuses on program effects

on individual outcomes in 2006 (upper panel) and 2008 (lower panel). In each case we include specifications that include controls for age and gender only (first row), and the extended set of controls described above (second row). All regressions are limited to children younger than six years of age at the time the transfers started (November 2005), as well as children born into these households since then.

The results in Table 3 are generally consistent with positive *Atención a Crisis* effects on child health and development. More than three-quarters (33 out of 42) of the coefficients are positive, and almost one-half of those that are positive (15 out of 33) are significant at the 10 percent level or higher. There are no significant negative coefficients. The evidence in favor of positive program effects is stronger in those specifications that include the extended set of controls than in those that only include controls for child age and gender. This likely reflects a small degree of imbalance between treatment and control at baseline, as seen in Table 1. In the case of the regressions of child height and weight, where the baseline imbalance was apparent, all of the coefficients are negative with the basic set of controls, but positive with the extended set of controls.

Table 4 reports the average effect across all outcomes, and separately for cognitive and socio-emotional development (the two language tests, the two memory tests, the two behavioral tests) and health and motor development (the measures of gross motor, leg motor, fine motor, height, and weight). The upper panel reports the mean effect sizes in 2006 and the lower panel in 2008, as before.

The first two rows in each panel correspond to the specifications in Table 3. In the specification with extended controls, households randomized into the *Atención a Crisis* program had outcomes that were 0.09 standard deviations higher than households randomized into the control group in 2006, and 0.08 standard deviations higher in 2008. In both years, the *p*-values for the mean effect sizes are below 0.01. For the cognitive and socio-emotional outcomes, the program effects are 0.12 standard deviations in 2006 and 0.08 standard deviations in 2008. For the health and motor outcomes, the program effects are 0.05 standard deviations in 2006 and 0.07 standard deviations in 2008.

Other rows in the table provide three important robustness checks on our main results. The Denver and the BPI tests are based, in part, on parents' reports about their children's development. It is conceivable that parents randomly assigned into the *Atención a Crisis* program were more likely to over-report the development of their children because they thought that this is what enumerators expected to hear (although it is unclear why this would affect the results for 2008, two years after the program had ended). It is also possible that the program made parents better able to detect delays in child development, in which case the treatment effects we estimate could be biased down. To check for these kinds of effects, we recalculated the averages but excluded the Denver and BPI. Excluding tests that are partly parent-reported does not have a substantive effect on our results—the mean effect size for the remaining outcomes is 0.12 standard deviations in 2006 and 0.06 standard deviations in 2008, both of which are highly significant. Thus, it does not appear that the positive program effects we estimate are a result of systematic misreporting by *Atención a Crisis* beneficiaries.

TABLE 4—IMPACT ON EARLY CHILDHOOD DEVELOPMENT OUTCOMES: MEAN EFFECT SIZE BY FAMILY OF OUTCOME

	All outcomes	Cognitive and socio-emotional outcomes	Health and motor development	Observations
2006				
Age and gender controls only	0.0395	0.1007**	−0.0217	$N = 3,326$
	(0.046)	(0.040)	(0.058)	
Extended controls	0.0876***	0.1211***	0.0541	$N = 3,326$
	(0.028)	(0.028)	(0.035)	
Excluding caregiver-reported tests	0.1246***	0.1921***	0.0795**	$N = 3,305$
	(0.026)	(0.035)	(0.032)	
Sample same tests 2006–2008	0.0706**	0.0978***	0.0434	$N = 3,149$
	(0.030)	(0.031)	(0.040)	
Mother is titular	0.0697**	0.1019***	0.0375	$N = 2,423$
	(0.032)	(0.030)	(0.041)	
2008				
Age and gender controls only	0.0314	0.0646	−0.0085	$N = 4,245$
	(0.043)	(0.043)	(0.049)	
Extended controls	0.0758***	0.0827***	0.0674***	$N = 4,245$
	(0.025)	(0.029)	(0.026)	
Excluding caregiver-reported tests	0.0607**	0.0949**	0.0265	$N = 4,228$
	(0.026)	(0.042)	(0.026)	
Sample same tests 2006–2008	0.0755***	0.0964**	0.0546*	$N = 3,149$
	(0.029)	(0.043)	(0.028)	
Mother is titular	0.0651**	0.0736**	0.0548*	$N = 2,917$
	(0.026)	(0.030)	(0.030)	

Notes: Coefficients for index of family of outcomes (estimated with SUR following Kling, Liebman, and Katz 2007); standard errors (in parentheses) adjust for clustering at the community level. See Table 3 for information on controls. "Excluding caregiver reported tests" excludes all tests that in part are reported by the caregiver (Denver and BPI). "Sample same tests 2006–2008" only includes children for whom a given outcome is available in both years. "Mother is titular" restricts sample to children whose mother was the recepient of the cash in the household.

***Significant at the 1 percent level.

**Significant at the 5 percent level.

*Significant at the 10 percent level.

One difficulty in comparing the magnitude of the effects in 2006 and 2008 is that new children are born into the sample. Also, baseline children can age into tests that can only be applied to those 36 months or older, or age out of the Denver when they turn seven years of age or older. Therefore, the composition of the sample changes between 2006 and 2008. Moreover, the 2008 survey included an additional memory test. To see how this could affect our results, we report results that exclude the associative memory test, and are estimated over a "restricted" sample of children who took a given test in both years.[17] The results for this smaller sample are very similar to those for the full sample. They suggest average *Atención a Crisis* program effects of 0.07 standard deviations in 2006 and 0.08 standard deviations in 2008.

[17] This also implies that the duration of exposure to the program is the same for all the children in this restricted sample, including the youngest children, if one includes the time *in utero* and given that the 2006 follow-up was conducted nine months after the start of the transfers.

Our main specification includes all children of relevant ages who were living in the sample of households randomized into a particular group at baseline, plus all additional children that were born to baseline household members. This implies that the person who received the cash transfer (the "titular") was not always the mother of the child in our sample. Moreover, in a small number of cases, households split between baseline and the first or second follow-up, so the titular might no longer be living with the children we study. As a final robustness check, we restrict the sample to include only children of the titular at baseline (excluding children of other household members) and still living with the titular at the time of the follow-up surveys. Again, these results are similar to those from the larger sample—the mean effect size across all outcomes is 0.07 standard deviations in 2006 and 2008. This suggests that the program effects we estimate are not primarily a result of any possible effects of the *Atención a Crisis* program on household formation or dissolution.[18]

In sum, the results in Table 4 make clear that the *Atención a Crisis* program improved the health and development of children in beneficiary households. There is no evidence that the positive program effects we estimate are a result of systematic misreporting by parents. There is no apparent fade-out of program effects two years after the program ended, and the persistence of program effects cannot be explained by compositional changes in the sample.

B. *Disaggregated Effects by Treatment Package*

An important question is whether the changes in child outcomes we observe can plausibly be explained by the income effect of the transfer alone. To answer this question, we first estimate the impact of the *Atención a Crisis* program on the log of total per capita expenditures. These results are in Table 5. In the first column of the table, we report the results from a specification for the program as a whole, without differentiating by treatment package. The second through fourth columns separately estimate the effect of the basic treatment, the basic treatment plus training grant, and the basic treatment plus lump-sum transfer.

The results in Table 5 make clear that the *Atención a Crisis* program had large effects on household per capita expenditures in 2006. The specification for the full sample, including the extended set of controls, shows that households randomly assigned to the basic treatment increased their expenditures by 28 log points.[19] The coefficient on households that received the basic treatment plus the lump-sum payment implies an increase in per capita expenditures of 33 log points. The relatively small difference in total expenditures between households assigned to receive only

[18] The *Atención a Crisis* program effects we estimate are also robust to accounting in alternative ways for the relationship between the child and the titular, the main caregiver and the mother, to removing outliers, and to different ways of coding the tests. Results for families of outcomes are also similar when estimating the impact on the average of the standardized test scores, instead of using SUR (see Kling, Liebman, and Katz 2007). We also tested for heterogeneity by child age and gender. Program effects are generally somewhat larger for children who were older at baseline and for girls. These results are available from the authors upon request.

[19] This increase in expenditures is substantially larger than the magnitude of the transfer. On average, households in this group received a transfer of US $20 per month, but increased their expenditures (a large share of which is food expenditures, with recall of the last two weeks) by almost US $35. The transfers were made somewhat irregularly, and the two transfers prior to the survey had occurred in a period of six weeks (instead of two months), including one just prior to the survey, which could explain the large effect on per capita consumption.

TABLE 5—IMPACT ON HOUSEHOLD-LEVEL PER CAPITA CONSUMPTION, BY TREATMENT

	3 treatment packages (1)	Basic (2)	Training (3)	Lump-sum payment (4)	*F*-test equality 3 packages *p*-value	*t*-test basic versus grant *p*-value
2006						
No controls	0.293***	0.287***	0.278***	0.314***	0.315	0.391
	(0.051)	(0.057)	(0.052)	(0.050)		
Extended controls	0.299***	0.281***	0.285***	0.331***	0.061	0.083
	(0.028)	(0.032)	(0.031)	(0.031)		
2008						
No controls	0.030	0.008	0.024	0.054	0.235	0.109
	(0.037)	(0.043)	(0.041)	(0.037)		
Extended controls	0.054**	0.022	0.048	0.088***	0.044	0.012
	(0.023)	(0.028)	(0.030)	(0.026)		

Notes: Standard errors (in parentheses) are adjusted for clustering at the community level. Controls include baseline log per capita consumption and household and community-level controls as defined in Table 3. The number of households is 2,212 for 2006 and 2,561 for 2008. This is higher than in Table 1, as split-off households with sample children are included.

***Significant at the 1 percent level.

**Significant at the 5 percent level.

*Significant at the 10 percent level.

the basic treatment and those assigned to also receive the lump-sum payment can be explained by the timing of the payment. The largest share of the lump-sum payment was made at the end of May, and the first follow-up survey was collected between July and August of that year.[20] The small increase in expenditures in households that received the lump-sum payment is also consistent with households investing (part of) the additional transfer in income-generating activities, as was intended.

Results for 2008, in the second row of the table, show that households that received the lump-sum payment continue to have higher per capita expenditures than those in the control group, about 8.8 log points. In contrast, the effect of the basic treatment on per capita expenditures is very small, about 2.2 log points, and is not significantly different from zero. This is not surprising given that the program had ended, and transfers had been discontinued, for approximately two years. An *F*-test rejects the null of equal coefficients for the basic treatment and the lump-sum payment in both 2006 and 2008.

Table 5 shows that households randomly assigned to the lump-sum payment had significantly higher consumption levels than those assigned to the basic treatment, most clearly in 2008. We therefore next compare child development outcomes for these two groups of households by estimating equation (4). These results are reported in Table 6. They show no evidence of better child development outcomes among households that received the lump-sum payment, relative to those that only received the basic treatment.

On the basis of the values in the table, we conducted a simple back of the envelope calculation. Households that received the lump-sum payment had per capita

[20] This timing also implies that all groups likely had similar levels of consumption for the first seven months of the transfers.

TABLE 6—DIFFERENCES IN EARLY CHILDHOOD DEVELOPMENT OUTCOMES WITH LUMP-SUM PAYMENT PACKAGE VERSUS BASIC PACKAGE

	All outcomes	Cognitive and socio-emotional outcomes	Health and motor development	Observations
2006				
Age and gender controls only	−0.0047	−0.0127	0.0034	1,625
	(0.026)	(0.030)	(0.029)	
Extended controls	0.0183	0.0160	0.0205	1,625
	(0.023)	(0.028)	(0.023)	
2008				
Age and gender controls only	−0.0242	−0.0385	−0.0070	2,114
	(0.026)	(0.026)	(0.033)	
Extended controls	0.0072	−0.0079	0.0253	2,114
	(0.024)	(0.026)	(0.029)	

Notes: Coefficients for index of family of outcomes (estimated with SUR following Kling, Liebman, and Katz 2007); standard errors (in parentheses) adjust for clustering at the community level. Controls and categories as defined in Table 3.

***Significant at the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.

expenditures that were 5 log points higher than those that received only the basic treatment in 2006, and 6.6 log points higher in 2008. If the program effects on expenditures for 2007, when there was no survey, are reasonably similar to those for 2006 and 2008, then households assigned to receive lump-sum payments had cumulative per capita expenditures roughly 17 log points higher than those assigned to the basic treatment over the three-year period between 2006 and 2008. In 2006, households assigned to receive the basic treatment had per capita expenditure levels that were 28 log points higher than those assigned to the control group, and child development outcomes that were 0.088 standard deviations higher. Conservatively, we would therefore expect that children in households assigned to the lump-sum payment would have child development outcomes that are 0.053 standard deviations $[(17/28) \times 0.088]$ higher than those assigned to receive the basic treatment. In fact, this value falls outside the 90 percent confidence interval $(-0.032$ to $0.046)$ for the effect of the lump-sum payment, relative to the basic treatment. Similarly, for the family of cognitive development outcomes, we would expect that children in households assigned to the lump-sum payment would have child development outcomes that are 0.072 standard deviations $[(17/28) \times 0.118]$ higher than those assigned to receive the basic treatment. This value falls outside the 99 percent confidence interval $(-0.074$ to $0.059)$ for the effect of the lump-sum payment relative to the basic treatment.

In sum, the higher expenditure levels of households that randomly received the lump-sum treatment do not appear to have resulted in better child development outcomes, especially in terms of cognitive development. It is possible that this is a result of convexity in the relationship between outcomes and expenditures—although Figure 1 shows no evidence of such nonlinearities for most outcomes. More likely, perhaps, the results suggest that something other than (or in addition to) the cash explains the *Atención a Crisis* treatment effects on child development we observe.

One limitation of the comparison between households randomly assigned to the basic treatment and the lump-sum payment is the fact that the latter were expected

TABLE 7—DIFFERENCES BETWEEN MOTHERS IN HOUSEHOLDS WITH LUMP-SUM PAYMENT PACKAGE VERSUS BASIC PACKAGE

	2006			2008		
	Mean basic	Coef	SE	Mean basic	Coef	SE
Economic activity mother:						
Number of days in year work in:						
Agricultural wage work	3.41	1.638	(2.073)	8.92	−3.875**	(1.817)
Nonagricultural wage work	17.04	−5.394	(4.345)	16.26	−8.038**	(3.217)
Nonagricultural self employment	31.86	41.02***	(6.923)	55.11	25.83***	(7.171)
Professional wage job	14.10	−3.399	(3.504)	12.92	−2.172	(3.129)
Of which days in seasonal migration	6.26	0.412	(1.726)	20.61	−3.932*	(2.281)
Total days	70.77	32.57***	(9.295)	107.20	9.883	(8.137)
Time mother allocates to child						
Tells stories to child	0.65	0.031	(0.030)	0.68	0.007	(0.022)
Read stories to child	0.13	0.033	(0.024)	0.08	0.002	(0.015)
Number of hours reading per week	0.30	0.114	(0.110)	0.18	0.046	(0.035)
Total number of hours caregiving per day		NA		6.31	0.170	(0.147)
Number of hours uniquely caregiving per day		NA		2.87	−0.009	(0.100)
Number of hours caregiving while working per day		NA		3.45	0.179*	(0.099)
Environment						
CESD depression scale	10.96	0.324	(0.672)	13.75	−0.345	(0.601)
Home scale	3.76	0.103	(0.182)	3.83	0.151	(0.142)

Notes: Standard errors adjusted for clustering at the community level. All regressions include controls for mother's education, household-level, and community-level baseline characteristics as defined in Table 3. Estimations for time mother allocates to child are estimated at the child level, and also include control for child age and gender. The sample only includes mothers in household eligible for the basic treatment and the lump-sum payment package. The sample for estimations for economic activity are mothers that were household members at baseline and follow-up: 1,073 mothers in 2006 (of 1,527 sample children) and 1,260 mothers in 2008 (of 1,994 sample children). Information on time allocated to specific child is only available if mother is main caregiver: available for 1,019 mothers of 1,458 children in 2006 and 1,163 mothers of 1,854 children in 2008. Depression and HOME scale are available for mothers who are main caregivers: available for 937 mothers in 2006 and 1,151 mothers in 2008.

***Significant at the 1 percent level.

**Significant at the 5 percent level.

*Significant at the 10 percent level.

to start a small business. In particular, one concern is that starting a small business may itself have an effect on child development. The lump-sum payment is therefore not a clean measure of the possible effects of the additional cash. To assess the extent to which these concerns are important, Table 7 compares the economic activity of mothers, patterns of work and time use, maternal mental health, and the home environment between households randomly assigned to the basic treatment and the lump-sum payment.

As expected, Table 7 shows that mothers assigned to the lump-sum payment spent fewer days in wage work than those assigned to the basic treatment, and more days in self-employment. In total, mothers assigned to the lump-sum payment worked 33 more days in 2006 (from a control group mean of 71 days), but there is no significant difference in the total number of days worked in 2008. There is no evidence that mothers assigned to the lump-sum payment spent fewer hours taking care of their children than those that received the basic treatment, no matter whether we consider hours that were devoted only to caregiving or also hours of caregiving

while working. Mothers assigned to the lump-sum payment were as likely to read or tell stories to their children. Finally, there is no evidence that the lump-sum transfer had an effect on the mental health of mothers or on the quality of the home environment. In sum, the lump-sum payment does not appear to have had any obvious, negative effects on the amount or quality of the time that mothers spent with their children. Thus, the absence of better child development outcomes for households in the lump-sum transfer group, in spite of the larger transfers they received and the higher overall levels of expenditures, cannot easily be explained by other changes that could have had a deleterious effect on child development.

C. *Changes in the Use of Intermediate Inputs*

We next analyze *Atención a Crisis* program effects on a number of "risk factors" that have been identified as important determinants of child development in the literature—namely, expenditures on food, availability of micronutrients, inadequate stimulation, exposure to infectious disease, and caregivers' mental health (see the review by Walker et al. 2007).

Table 8 reports the effects of the *Atención a Crisis* program on various measures of these risk factors. We include both estimates of changes in individual outcomes and averages across families of inputs (the latter, in standard deviation units, as before). The top panel of the table reports results for 2006, and the bottom panel for 2008. The first two columns focus on the impact of the *Atención a Crisis* program, without distinguishing between treatment packages, while the last column focuses only on the effects of the basic treatment, relative to the control group.

The first column in Table 8 shows that the *Atención a Crisis* program had a substantial effect on the use of various inputs into child development. In 2006, households randomly assigned to the program changed the composition of food expenditures, spending a lower fraction on staples and higher fractions on animal proteins, fruits, and vegetables.[21] Treated households had substantial increases in various measures of child stimulation. They were more likely to tell stories, sing to, or read to their children, and to have pen, paper, and toys for children in the house; children in households randomly assigned to the *Atención a Crisis* program were also more likely to have been weighed, received iron, vitamins, or deworming medicine, and they spent fewer days in bed. The magnitude of the changes is substantial. For example, the mean increase in stimulation is 0.26 standard deviations, and the mean increase in health inputs is 0.13 standard deviations.

In Nicaragua, as elsewhere, wealthier households generally spend more on relatively expensive sources of calories (animal proteins and fresh fruits and vegetables, rather than staples), provide more inputs for child stimulation (books, toys), and make more use of preventive health services. At first blush, then, the overall program effects for 2006 may not be surprising, given that the *Atención a Crisis* program made

[21] We also investigated whether treated households report a higher number of days that children consumed specific food items, including tortillas, milk, meat, eggs, fruits, and vegetables in the last week. These results are consistent with those in Table 8 for 2006, but differences between treated and control households are no longer significant in 2008. We note, however, that there is considerably less variability in these measures of reported intake than in the measures of household expenditures used for Table 8.

TABLE 8—IMPACT ON INTERMEDIATE INPUTS

		3 treatment packages together		Basic package
		Ind, hh, and com controls	Add. control for log(pce) and log(pce)^2	
2006	Mean control	Coef (1)	Coef (2)	Coef (3)
Nutrition:				
Percent food in total expenditures	0.707	0.009	0.010	0.005
		(0.008)	(0.008)	(0.009)
Percent staples in total food expenditures	0.567	−0.093***	−0.066***	−0.087***
		(0.011)	(0.011)	(0.013)
Percent animal proteins in total food exp.	0.155	0.087***	0.060***	0.082***
		(0.008)	(0.008)	(0.009)
Percent fruit and vegetables in total food exp.	0.073	0.029***	0.020***	0.028***
		(0.005)	(0.005)	(0.006)
Index (of standardized outcomes)		0.422***	0.299***	0.393***
		(0.036)	(0.036)	(0.043)
Stimulus				
Has pen and paper in house	0.682	0.113***	0.102***	0.107***
		(0.026)	(0.026)	(0.028)
Somebody tells stories/sings to child	0.527	0.124***	0.080***	0.088***
		(0.029)	(0.030)	(0.033)
Number of hours read to per week	0.134	0.295***	0.266***	0.145**
		(0.061)	(0.062)	(0.059)
Has toy in house	0.271	0.068***	0.038	0.059*
		(0.023)	(0.026)	(0.030)
Index (of standardized outcomes)		0.258***	0.204***	0.183***
		(0.037)	(0.039)	(0.040)
Health				
Weighed	0.735	0.044**	0.035**	0.050***
		(0.018)	(0.018)	(0.019)
Got vitamins or iron	0.750	0.082***	0.072***	0.097***
		(0.017)	(0.018)	(0.020)
Got deworming drugs	0.567	0.059***	0.036	0.043*
		(0.021)	(0.023)	(0.026)
Number of days sick in bed (last month)	0.623	−0.327***	−0.425***	−0.357***
		(0.123)	(0.144)	(0.133)
Index (of standardized outcomes)		0.131***	0.117***	0.138***
		(0.022)	(0.024)	(0.024)
Environment				
CESD depression scale	11.88	−0.605	−0.328	−0.480
		(0.749)	(0.779)	(0.696)
HOME scale	4.018	−0.265	−0.088	−0.204
		(0.291)	(0.284)	(0.308)
Index (of standardized outcomes)		0.079	0.032	0.061
		(0.075)	(0.075)	(0.072)
All risk factors: index (standardized outcomes)		0.222***	0.163***	0.194***
		(0.021)	(0.022)	(0.024)

(*Continued*)

TABLE 8—IMPACT ON INTERMEDIATE INPUTS (*Continued*)

		3 treatment packages together		Basic package
		Ind, hh, and com controls	Add. control for log(pce) and log(pce)^2	
2008	Mean control	(1) coefficient	(2) coefficient	(3) coefficient
Nutrition:				
% food in total expenditures	0.719	−0.008	−0.007	−0.008
		(0.009)	(0.008)	(0.009)
% staples in total food expenditures	0.589	−0.025***	−0.019**	−0.026***
		(0.009)	(0.008)	(0.009)
% animal proteins in total food exp.	0.161	0.022***	0.017**	0.024***
		(0.008)	(0.007)	(0.009)
% fruit and vegetables in total food exp.	0.064	0.009*	0.007	0.009*
		(0.005)	(0.005)	(0.005)
Index (of standardized outcomes)		0.096***	0.074**	0.101***
		(0.035)	(0.035)	(0.037)
Stimulus				
Has pen and paper in house	0.824	0.039**	0.037**	0.044*
		(0.017)	(0.017)	(0.025)
Somebody tells stories/sings to child	0.600	0.066***	0.062***	0.067**
		(0.023)	(0.022)	(0.027)
Number of hours read to per week	0.191	0.039	0.032	0.006
		(0.035)	(0.035)	(0.040)
Has toy in house	0.849	0.081***	0.078***	0.079**
		(0.028)	(0.028)	(0.033)
Index (of standardized outcomes)		0.129***	0.121***	0.120***
		(0.034)	(0.033)	(0.043)
Health				
Weighed	0.646	0.007	0.005	0.003
		(0.025)	(0.025)	(0.027)
Got vitamins or iron	0.558	0.079***	0.076***	0.064**
		(0.025)	(0.025)	(0.029)
Got deworming drugs	0.547	0.070***	0.069***	0.066***
		(0.022)	(0.022)	(0.024)
Number of days sick in bed (last month)	0.669	−0.047	−0.053	−0.101
		(0.107)	(0.107)	(0.116)
Index (of standardized outcomes)		0.084***	0.082***	0.078***
		(0.024)	(0.024)	(0.025)
Environment				
CESD depression scale	14.00	0.027	0.036	−0.039
		(0.640)	(0.640)	(0.785)
Home scale	4.072	−0.081	−0.078	−0.128
		(0.120)	(0.119)	(0.135)
Index (of standardized outcomes)		0.017	0.016	0.031
		(0.042)	(0.041)	(0.047)
All risk factors: index (standardized outcomes)		0.081***	0.073***	0.083***
		(0.018)	(0.017)	(0.021)

Notes: Standard errors (in parentheses) adjust for clustering at the community level. Negative sign on CESD and Home scale indicate improvement. Coefficients for standardized indices of families of nutrition, stimulus, health, and environment inputs calculated following Kling, Liebman, and Katz (2007). Coefficient for all risk factors gives equal weight to indices of families of nutrition, stimulus, health, and environment inputs. Individual, household, and community-level controls as defined in Table 3. Results for full sample of children with at least one test. $N = 3{,}326$ for 2006; $N = 4{,}245$ for 2008.

***Significant at the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.

substantial cash transfers. The remaining results in Table 8 investigate whether the effects of the program on the use of various inputs into child development are consistent with an explanation that focuses on the cash transfer alone.

The second column of the table includes controls for the log of total per capita expenditures, and its square. Controlling for the higher total expenditures of the *Atención a Crisis* beneficiaries has only a modest effect on the estimated coefficients. For example, the mean increase in stimulation among treated households from these regressions for 2006 is 0.20 standard deviations (rather than 0.26 standard deviations in the regressions that do not control for total expenditure levels), while the increase in health inputs is 0.12 standard deviations (rather than 0.13 standard deviations). It does not seem that the higher use of inputs into child development by *Atención a Crisis* households can easily be explained by their higher overall expenditure levels alone.[22]

An important caveat for these estimates is that total expenditures are themselves determined by the *Atención a Crisis* program, which could bias the regression coefficients. The remaining results in the table attempt to deal with this concern. Recall from Table 5 that households assigned to receive only the basic treatment did not have higher expenditures than those in the control group in 2008—the coefficient in a regression of the log of total per capita expenditures for these households is 0.022 (with a standard error of 0.028). Nevertheless, these households continue to show significant differences in the use of inputs into child development. On average, households that were randomly assigned the basic treatment had a 0.12 standard deviation increase in stimulation, and a 0.08 standard deviation increase in health inputs, relative to those in the control group, in 2008. Households assigned to the basic treatment also continued to devote a higher fraction of food expenditures to animal proteins and a lower fraction to staples. These effects cannot easily be explained by any contemporaneous income effect of the transfer. Rather, they suggest that the *Atención a Crisis* program had an effect on behavior, and that some of these behavioral changes were still apparent two years after the program had been discontinued.

IV. Conclusion

In many developing countries, young children suffer from profound delays in cognitive development. These delays have serious implications for the success of these children as adults. A variety of theories of skill formation suggest that investments in schooling and other dimensions of human capital will have low returns if children do not have adequate levels of cognitive and social skills at early ages (for example, Cunha et al. 2006). Understanding the causes of deficits in early childhood and identifying interventions that can help address them are important priorities for research.

[22] We also conducted a similar analysis non-parametrically by running Fan regressions of the nutrition, stimulation, health and environment inputs as a function of the log of total per capita expenditures, separately for households in the Atención a Crisis treatment and control groups. These results are very similar in character to those in Table 8, and are available from the authors upon request.

This paper uses a randomized evaluation to assess the impact of a cash transfer program on a large set of measures of child development in Nicaragua, a low-income country. The identification is straightforward. It is based on random assignment, with almost perfect compliance, and remarkably low levels of attrition over three survey waves. We show that a program that transferred cash to women improved child development. Remarkably, there was no fade-out of impacts two years after the program was ended and transfers discontinued. This stands in contrast with evaluations of a number of interventions in both developed and developing countries.

The magnitude of the effects we estimate is modest, but not trivial. One way of putting the magnitude in context is by comparing it with differences in outcomes between children of mothers with more or less schooling. In the control group, every year of maternal schooling is associated with 0.05 standard deviations better child development, on average. The program effects we estimate are therefore equivalent to comparing children with mothers with one-and-a-half more or less years of schooling—a substantial amount, given the control group average of four years of schooling. Another way of putting the magnitude in context is by comparing it with the impacts of interventions on child development estimated elsewhere. Paxson and Schady (2010) estimate that the BDH unconditional transfer program in Ecuador improved child development by 0.18 standard deviations among the poorest quartile of children in the sample, with no effects among less poor children. Berlinski, Galiani, and Gertler (2009) report an effect size of 0.23 standard deviations for the impact of one year of preschool for children 3–5 years of age on learning outcomes in Argentina. Behrman, Cheng, and Todd (2004) report an impact of 3–4 percent of the mean for a preschool program in Bolivia. All of these estimates refer to cognitive outcomes, and to children 36 months and older. *Atención a Crisis* program effects on cognitive outcomes (language and memory) for these older children are 0.19 standard deviations in 2006, and 0.20 in 2008. Our estimates are therefore very close in magnitude to those that have been reported from other settings in Latin America.

Households who benefited from transfers increased expenditures on critical inputs into child development. They spent more on nutrient-rich foods, provided more early stimulation to their children, and made more use of preventive health care. Changes in the use of these inputs are larger than what one would expect to see if the program were simply moving children along the curves that relate inputs to overall expenditures. Thus, the program appears to have resulted in behavioral changes. Some of these behavioral changes persisted after the program ended, although the differences in input use between the *Atención a Crisis* treatment and control groups are generally smaller than when the program was still operating. It is therefore not clear whether the persistence of better child development outcomes among *Atención a Crisis* beneficiaries is a result of the one-time jump in outcomes that took place while the program was operating, or behavioral changes that continued after the program ended. We note, however, that the fact that fade-out of impacts appears to occur for many different early childhood programs suggests that the behavioral changes among *Atención a Crisis* beneficiaries are likely to be important.

The *Atención a Crisis* program randomized three treatment variations. One of the treatment groups had significantly higher per capita expenditure both during the program and after the program ended. We find no evidence that child development

outcomes are better for these households. Thus, in Nicaragua, a dollar is not always a dollar (or, rather, a *Córdoba* is not always a *Córdoba*). Something other than, or in addition to, the cash appears to be important. The social marketing that accompanied the transfers, or the fact that transfers were made to women, or both, could be part of the explanation. A better understanding of what features of this and other cash transfer programs account for improvements in child development is an important priority for future research.

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