Online Appendices

Child Control in Education Decisions: An Evaluation of Targeted Incentives to Learn in India

James Berry

1 Appendix: Choice Treatments

1.A Description

As described in Section II.A, the experiment included two treatment groups that tested whether parents want to reward their children for positive outcomes but cannot commit to doing so. For example, parents might be unable to credibly commit to reward their children for good performance on a test because they are unable to put the resources aside to purchase the reward.[1] To test this hypothesis, these two additional treatments offered the parents a choice between money for themselves and a toy for their children. In ex ante choice treatment, the parent made her choice when the program was announced and was committed to her choice. In the ex post choice treatment, the parent made her choice after the child had reached the goal.

The ex post choice treatment was included to confirm that the salience or the convenience of the choice itself does not cause a positive impact of the ex ante choice. If the choice itself drives the results, one would expect a positive impact of either choice treatment on outcomes. On the other hand, if the results are driven by an actual desire to commit, only the ex ante choice treatment will positively affect outcomes.

Note that the effectiveness of commitment in this context depends on whether parents have commitment problems as well as their awareness of these problems. If parents are unaware of commitment problems, they will not desire commitment, and the ex ante commitment treatment will not be effective.

1.B Results

The choices of the parents in the two choice treatments can provide preliminary evidence of a commitment problem on the part of parents. If parents cannot commit and are aware of this

problem, they will be willing to reward their children with a toy ex ante but will decide to keep the money for themselves ex post. Therefore, if the choice of a toy is primarily driven by a desire for commitment, one would expect more parents to choose the toy in the ex ante treatment than in the ex post treatment.

However, the results show that substantially *more* parents in the ex post treatment chose to reward their children with toys. Appendix Table 1 tabulates the choices in both treatments. Thirty-three percent of parents in the ex ante treatment and 51 percent of parents in the ex post treatment chose toys. The percentage of parents in the ex ante treatment choosing the toy remains virtually unchanged (32 percent) when restricting the sample to those who achieved the goal. Because more parents chose the toy in the ex post treatment, these results provide suggestive evidence against a commitment problem on the part of parents. Instead, it is possible that parents initially chose money because they were uncertain of their needs for cash at the end of the program. Once the uncertainty was resolved, they were willing to choose the toy.

Panel A of Appendix Table 2 compares mean attendance and achievement outcomes of the choice treatments with the parent money treatment. In the attendance regressions, the coefficient on the ex ante choice treatment is positive, but it is significant in only one of the two specifications. However, the effect is negative and insignificant for achievement. The estimated effects for the ex post treatment are small and insignificant across both outcomes. On balance, there is weak evidence that the opportunity to commit did improve attendance in the after-school classes, but there is no evidence that it affected achievement.

Panel B of Appendix Table 2 presents interactions of the choice treatments with the child's baseline test score. The omitted category is again the parent money treatment. The interactions of ex ante choice treatments and initial test score are negative, but the magnitudes are small and

none of the coefficients are statistically significant. In addition, the sign and magnitude of the coefficients on the ex post interactions are similar to those of the ex ante interactions, suggesting that the opportunity to commit ex ante did not differentially affect outcomes relative to the ex post choice.

Overall, this analysis provides no consistent evidence that offering parents the opportunity to commit to rewarding their children with toys improved outcomes relative to rewarding parents with money, and there is no evidence that this effect differs by pretest score. As noted above, however, these results would be both consistent with a lack of commitment problems or a lack of awareness on the part of parents that they face commitment problems.

2 Appendix: Attrition

This section analyzes attrition in the study sample. Out of 925 students who were reached for the baseline survey and program announcement, 25 (2.7 percent) did not take the post-test.

Appendix Table 3 displays attrition patterns in each treatment group and compares the means of variables for the attrited group with the group that remained in the sample. Importantly, attrition is not significantly related to assignment to any particular treatment group. In addition, attriters and stayers have comparable characteristics across the majority of variables examined. Attriters do have significantly higher relative pretest scores, have fewer children in the household, have lower levels of durables ownership, and have lower self-reports of transfers of toys, clothes, and school supplies at the baseline.

In order to examine how the attrition patterns across treatment groups could affect the achievement results, Appendix Table 4 computes bounds for the treatment effects found in Table 4, using Lee's (2009) trimming method. Because attrition was very low in the sample, and

because attrition did not substantively differ across treatment groups, the bounds are tight: all bounds are within 1 percentage point of the point estimate in Table 4.

Appendix Table 5 explores how attrition could affect the heterogeneity in treatment effects of the toy treatments compared with the money treatments by computing similar bounds on treatment effects at each baseline test score level. Again, the trimming procedure produces relatively tight bounds: bounds on each treatment effect are within three percentage points of the point estimate. Because the interval for each test score level lies strictly above the interval for the next level, the bounded estimates are consistent with monotonically decreasing treatment effects by test score.

3 Appendix: Overall Effects of Incentives Program

The overall effects of incentives programs are important from a policy perspective, and it is useful to show that the incentives used in this study did influence test scores overall. In order to gain support of the local school committee, the study was designed to treat all eligible children and therefore did not include a pure control group. However, features of the implementation of the program allow me to construct a quasi-experimental control group to estimate the overall effects of the incentives schemes on test scores.

As shown in Table 1, a number of children and their mothers in the randomized sample were not reached at the time of the baseline survey. Some children were not in school when addresses were initially collected, but they were included in the randomization in case they could be found at a later date. In other households, the children and their mothers were not available during the surveyors' two to three daytime visits when the baseline was conducted. There were 161 students included in the randomization but who were not reached for the program announcement. Out of

these 161 students, 152 were in school when the post-test was administered. This group (the "no program" group) will serve as a control group for the analysis of this section.

Appendix Figure 1 presents the distributions of raw pretest and post-test scores of the program group and the no-program group. As shown in the top panel, the pretest scores are remarkably similar between the two groups. A Pearson χ^2 test fails to reject the equality of the two distributions (p-value = 0.88). The bottom panel presents the post-test scores of the two groups. The program group now has a much lower proportion of test scores of zero and higher proportions of test scores of 2 and 4. There is a lower proportion of scores of 3, but this result is not surprising given that this score category was not one of the goals given to students in the program group. A Pearson χ^2 test now strongly rejects equality of the two distributions (p-value < 0.01). Appendix Figure 2 repeats this exercise using relative test scores. Again, while the distributions are remarkably similar at pretest, the distribution of the program group has shifted strongly to the right at post-test.

Appendix Table 6 presents the results of regressions of a dummy variable that indicates if the mother and child were reached at the baseline on the child's relative pretest score and the set of characteristics that was observable for both the program and no-program groups. Column 1 indicates that being reached at the baseline is not significantly related to the pretest score, grade or gender of the child. Columns 2 and 3 add controls for classroom and surveyor dummies.[2] Of the three specifications presented in this table, the only significant difference in test scores between the program and no program group is the specification with both sets of dummies, displayed in Column 3. The point estimate in this specification indicates that an increase in pretest score by one standard deviation is associated with a 2.6 percent lower likelihood of being

reached at the baseline. Overall, however, the results of this table imply few differences between the program and no program groups.

The results in Appendix Figures 1 and 2 and Appendix Table 6 suggest that although the no program group was not randomly assigned, children in this group may serve as a plausible control group for the purposes of determining the overall impact of the program. In order to verify that the no-program group represents a reasonable counterfactual for the program group, I also compare the increase in test scores for the no-program group to the cross-sectional differences in test scores between grades one, two, and three. On average, the no-program group increased by 0.21 points during the two months between the pretest and post-test. On average, a first grader is 1.01 points below a second grader at the pretest, and a second grader is 1.18 points below a third grader. Gurgaon schools are in session 11 months out of the year, and if a student improves by an equal amount each month, one would expect a first-grade student to improve 2/11*1.01 = 0.18 points on average, and a second-grade student to improve 2/11*1.18 = 0.21 points. These estimated improvements are very close to the 0.21-point increase observed among the no-program group.

Appendix Table 7 presents estimates of the effects of participation in the program on test scores. Column 1 displays the estimated program effect on achievement of the goal. Controlling for pretest, classroom and surveyor, the program group was 27 percent more likely to achieve the goal. Columns 3 and 5 display the estimates using raw and relative pretest scores, respectively. The estimated effects of the program are 0.59 points using raw test scores and 0.61 standard deviations using relative test scores. Each estimate is significant at the 1 percent level.

One caveat is in order with respect to interpretation of the results presented in this section. While the after-school classes were open to any child who wished to attend, children in the

incentives program were notified individually when the program was announced. In practice, children in both the program and no-program groups were often reminded of the classes during school time by their teachers, but this was not controlled as part of the experiment. Therefore, the program effects estimated in this section are the combined effects of receiving an incentive treatment in addition to individual notification of the classes.

However, several pieces of evidence suggest that individual notification of the classes does not drive the program effects found in this section. First, estimating the program effects controlling for attendance in the after-school classes leaves the impact estimates virtually unchanged. Columns 2, 4, and 6 of Appendix Table 7 re-run the analysis of program effects controlling for attendance. The estimated effect size is nearly identical in each case. Second, the difference in attendance between the program and no-program group is relatively small. While 20 percent of children in the incentive treatments attended the classes, 9 percent of children in the no-program group attended. Based on this 11 percent difference in attendance, the classes would have to have been responsible for an improvement of one standard deviation in order to account for just 18 percent of the estimated program effect. It is therefore unlikely that differential class attendance between the program and no-program groups could be driving a substantial fraction of the estimated program effect.

4 Appendix: Self-reported Transfers from Parents to Children

This section analyzes survey data on transfers from parents to children in the money and toy treatments. The goal of this analysis is twofold. First, expanding on the analysis of self-reported spending of the prize money in Section III, it examines the extent to which parents in the money treatment were mimicking the toy treatments by buying toys with the money. Second, it analyzes whether parents were providing other transfers to their children before or after the post-test.

Appendix Table 8 presents estimates of the effects of the combined toy and voucher treatment groups, relative to the money treatment groups, on transfers from parents to children either before or after the post-test. This analysis provides evidence on the overall pattern of transfers across treatment groups, and whether those transfers primarily occurred before or after the post-test. The behaviors examined are the amount of money given to the child over the past week, and whether the parent gave the child a toy, sweets, clothes, or school supplies over the past week. I also include an aggregate measure of the 5 types of transfers by averaging the z-scores of the measures.

Panel A uses data from the first follow-up survey, taken just before the post-test. Using the aggregate measure of transfers, parents in the toy treatments provided 0.09 standard deviations fewer transfers, a result that is statistically significant at the 10 percent level. This could imply that some parents in the money treatments were rewarding children based on effort. Alternatively, some parents may have known their children's levels before the post-test and were providing transfers in anticipation of the reward.

Panel B of Appendix Table 8 repeats the exercise using data from the second follow-up survey, taken just after the post-test. Using the aggregate measure of transfers, the coefficient on the toy treatments is more than five times as large as the coefficient using the first follow-up survey and is highly statistically significant, indicating that parents in the money treatments provided substantially more transfers after the outcome of the test had been realized. Across the individual categories, parents in the toy treatments were 20 percentage points less likely to give their children clothes after the post-test, 8 percentage points less likely to give school supplies, and 8 percent less likely to give toys. Consistent with reported spending of the prize money, these results show that while some parents in the money treatments did buy toys for their

children at the end of the program, a much larger percentage bought clothes or school supplies. Again, however, it is not known whether these items were used to motivate the children or because the parents viewed the money as earmarked for child goods as a result of the program structure. Nonetheless, these results show that children received different transfers across the money and toy treatment groups as a result of the experiment.

5 Appendix: Additional Analysis on Difficulty in Goal Achievement

In this section I extend the discussion of Section III.B by examining whether difficulty in goal achievement varies by pretest score.

Appendix Table 9 presents regressions of achievement on raw pretest score, inclusion in the program group, and the interaction of these two variables. The regressions are run with and without controls for surveyor and classroom. The coefficient on the pretest score provides evidence of whether achievement for the no-program group varies by pretest score. In both specifications the estimates are small, negative, and statistically indistinguishable from zero. Thus, in the absence of incentives, progression across test score levels does not vary by baseline level. The coefficient on the interaction term tests whether the treatment effects of inclusion in the program group varies by pretest score, and again this estimate is small and statistically insignificant. Taken together, these results imply that difficulty in achieving program goals did not differ monotonically by pretest score.

6 Appendix: Proofs

6.A Proposition 1: First-period Probability of Success is Increasing in β_p and β_c

This proof shows that the first-period probability of success is strictly increasing in β_p and is weakly increasing in β_c.

Maximizing equations (3) and (4) yields the incentive-compatibility constraints $p = \frac{\beta_p(1-\gamma)}{k_p}$ and $c = \frac{\beta_c \gamma}{k_c}$. Substituting these equations into the parent's optimization for γ (equation (5)) yields

$$(12) \quad \max_{\gamma}(1-\gamma)\left\{\frac{\beta_p^2(1-\gamma)}{k_p} + \frac{\beta_c^2\gamma}{k_c}\right\} - \frac{\beta_p^2(1-\gamma)^2}{2k_p}$$

The first-order condition for γ is given by

$$(13) \quad \frac{\beta_c^2(1-\gamma)}{k_c} - \frac{\beta_c^2\gamma}{k_c} - \frac{\beta_p^2(1-\gamma)}{k_p} = 0$$

Solving this equation for γ, and noting that $0 \leq \gamma \leq 1$, yields

$$(14) \quad \gamma^* = \frac{k_p\beta_c^2 - k_c\beta_p^2}{2k_p\beta_c^2 - k_c\beta_p^2}$$

When $\beta_p^2 < \beta_c^2 \frac{k_p}{k_c}$, and 0 otherwise.

When $\gamma^* = 0$, then the probability of success equals $\frac{\beta_p^2}{k_p}$, which is increasing in β_p and is invariant to β_c.

To prove that the probability of success is increasing in β_p or β_c on the interior (that is, when $\beta_p^2 < \beta_c^2 \frac{k_p}{k_c}$), it is sufficient to show that $\frac{(1-\gamma^*)\beta_c^2}{k_c}$ is also increasing in β_p or β_c.[3] Because $\frac{(1-\gamma^*)\beta_c^2}{k_c}$

is decreasing in γ^*, I can show that γ^* is decreasing in β_p to show that the probability of success is increasing in β_p. I do this by differentiating (14) with respect to β_p:

$$(15) \quad \frac{\partial \gamma^*}{\partial \beta_p} = -\frac{2k_c\beta_p}{2k_p\beta_c^2 - k_c\beta_p^2} - \frac{2k_c\beta_p\left(k_p\beta_c^2 - k_c\beta_p^2\right)}{\left(2k_p\beta_c^2 - k_c\beta_p^2\right)^2}$$

This quantity will be negative when γ^* is on the interior.

To prove that $\frac{(1-\gamma^*)\beta_c^2}{k_c}$ is increasing in β_c, differentiate with respect to β_c:

$$(16) \quad \frac{\partial}{\partial \beta_c}\frac{(1-\gamma^*)\beta_c^2}{k_c} = -\frac{\partial \gamma^*}{\partial \beta_c}\frac{\beta_c^2}{k_c} + (1-\gamma^*)\frac{2\beta_c}{k_c}$$

where

$$(17) \quad \frac{\partial \gamma^*}{\partial \beta_c} = \frac{2k_ck_p\beta_c\beta_p^2}{\left(2k_p\beta_c^2 - k_c\beta_p^2\right)^2}$$

After substituting (14) and (17) into (16) and simplifying, I have

$$(18) \quad \frac{\partial}{\partial \beta_c}\frac{(1-\gamma^*)\beta_c^2}{k_c} = \frac{4k_p\beta_c^3\left(k_p\beta_c^2 - k_c\beta_p^2\right)}{k_c\left(2k_p\beta_c^2 - k_c\beta_p^2\right)^2}$$

This quantity will be positive when γ^* is on the interior.■

F.2 Proposition 2: Incentives to Parents Are Relatively More Effective when $\beta_p\sqrt{k_c} > \beta_c\sqrt{k_p}$, and Incentives to Children Are More Effective when $\beta_p\sqrt{k_c} < \beta_c\sqrt{k_p}$

In this proof I show that when parental productivity is high relative to child productivity, incentives to parents are more effective, and when parental productivity is low relative to child productivity, incentives to children are more effective.

I first solve for the parent's choice of γ_e if she is given the incentive directly. In the experimental period, the incentive-compatibility constraints and are given by $p_e =$

$(\pi + 1)\frac{\beta_p(1-\gamma_e)}{k_p}$ and $c_e = (\pi + 1)\frac{\beta_c\gamma_e}{k_c}$. Performing the parent's maximization over γ subject to these incentive-compatibility constraints yields

(19) $$\gamma_e^* = \frac{k_p\beta_c^2 - k_c\beta_p^2}{2k_p\beta_c^2 - k_c\beta_p^2}$$

when $\beta_p^2 < \beta_c^2\frac{k_p}{k_c}$, and 0 otherwise.

Note that $\gamma_e^* = \gamma^*$, that is, the optimal γ does not depend on the size of the reward. This occurs because the parent's and child's optimal input allocation, conditional on γ, are both proportional to the reward size. As a result, the entire maximand for the parent's optimization over γ is multiplicatively separable in the reward size, and thus the optimal γ does not depend on reward size.

Now I can use the solution for γ_e^* to examine the how equation (11) varies with β_p and β_c. Substituting the incentive-compatibility constraints into (11), and noting that $\gamma_{child} = \frac{\pi}{\pi+1}$, equation (11) can be written as

(20) $$\beta_p^2\frac{(\pi + 1)(1 - \gamma_e^*)}{k_p} + \beta_c^2\frac{(\pi + 1)\gamma_e^*}{k_c} - \beta_p^2\frac{1}{k_p} - \beta_c^2\frac{\pi}{k_c}$$

Equation (20) will be positive when $\frac{\beta_p}{\beta_c} > \sqrt{\frac{k_p}{k_c}}$ and negative when $\frac{\beta_p}{\beta_c} < \sqrt{\frac{k_p}{k_c}}$. Equation (20) equals zero only when $\frac{\beta_p}{\beta_c} = \sqrt{\frac{k_p}{k_c}}$. At this crossing point, the partial derivative with respect to $\frac{\beta_p}{\beta_c}$ is positive, implying that parent incentives are more effective when parental productivity is relatively higher, and vice versa. ■

7 Appendix: Additional Robustness Checks of Parental Productivity Index

This section presents several additional robustness checks of the parental productivity analysis in Section V.

7.A Interaction Including Pretest Scores

Appendix Table 10 examines the extent to which the interaction between the productivity index and the toy treatments affects the interaction with relative test scores. For comparison, Columns 1 and 3 present the results from Table 5, in which only the interaction with pretest scores is included. Columns 2 and 4 show that including the interaction with the index dampens the interaction with pretest scores. Using attendance as the outcome, the coefficient on the interaction with pretest score increases from -0.10 to -0.08. Using achievement as the outcome, the coefficient on pretest score increases from -0.07 to -0.05. Thus, the index accounts for a portion of the negative interaction between pretest scores and the toy treatments. The interaction still remains negative, suggesting that there may be variation in pretest scores that reflect unobserved parental productivity.

7.B Components of Productivity Index

In this section I explore which of the components of the parental productivity index are most influential in driving the interactions between the index and toy treatments in Table 7.

I examine the individual components through two sets of regressions. First, I drop one variable at a time from the first-stage regressions in Column 2 of Table 6 and re-run the regressions in Columns 2 and 4 of Table 7. The results are presented in Appendix Table 11.[4] Overall, dropping one variable from the index does not change the estimated coefficients

substantially. For both outcome measures, the largest decreases in the interaction coefficients occur after dropping the variable indicating the number of children in the household.

Second, I re-run the regressions in Columns 2 and 4 of Table 10, interacting the toy treatments with each component of the index. The results are presented in Appendix Table 12. In the attendance regression, the only variable that is significant when interacted with the toy treatment is the tutoring variable (significant at the 5 percent level). In the achievement regression, the only significant variable is the number of children in the household (significant at the 5 percent level).

This analysis suggests that while no single variable drives the interactions between the productivity index and the toy treatment, the number of children in the household and the baseline amount spent on tutoring are most influential in these interactions. The tutoring variable may be especially influential in the attendance regression because it reflects the parent's ability to send her children to classes outside of school time. It should be noted, however, that parents in the money treatments did not use the classes provided as part of the program as a substitute for prior tutoring classes. There is no significant difference in average spending on tutoring between the money and toy treatments in the month before the post-test (results not shown).

Finally, I classify the components of the index into three groups and form separate sub-indices based on regressions of relative test scores on the variables of each group. The first group, "demographics," includes the two variables reflecting the number of children and adults in the household, as well as the mother's employment status. The second group, "education," includes the mother's and father's education level. The third group, "helping," includes the variable indicating whether anyone helped the child with studies in the past day and the amount

spent on tutoring in the past month. Durables ownership would form its own separate group, and is therefore excluded in this analysis.

Appendix Table 13 displays the results of interacting sub-indices constructed from each group of variables with the toy treatments. In all specifications, the coefficients on the interactions are negative. When attendance is used as the outcome, the interactions with the demographics and helping groups are large in magnitude and statistically significant. When achievement is used as the outcome, the strongest interaction is with the demographics group, although none are statistically significant.

7.C Altruism

This subsection explores whether the productivity index could reflect different levels of altruism on the part of the parent. The theory developed in Section IV can be extended to incorporate altruism, where the parent places a value of α (where $0 < \alpha \leq 1$) on the child's utility. Doing so increases the fraction γ given to the child. This occurs because the parent now values a contribution to the child both for its motivational effect as well as for its effect on the child's consumption. In case of perfect altruism, when $\alpha = 1$, the parent transfers all of the benefits to the child, that is, $\gamma = 1$.

When parent and child productivity are fixed, and altruism varies across households, altruism could be positively related to the relative effect of parent incentives. This occurs because altruism raises transfers to the child and thereby lowers the extent to which the toy incentive puts a constraint on the parent. Suppose that for low-altruism households the toy treatments constrain transfers and cause child incentives to be relatively more effective. As altruism rises, the child incentive will become less of a constraint, and parent and child incentives will produce

equivalent results. Thus, if the productivity index reflects altruism, this altruism could result in a negative interaction of the index and the toy treatments.

Note that the results presented in Section V do not conform to this theory in that this theory predicts that toys will be more effective for low-altruism households and that toys and money will be equivalent for high-altruism households. The results from Table 7 imply that the toy treatments are more effective for households with low values of the productivity index and less effective for households with high values of the index.

I can also test whether the productivity index is related to altruistic behavior. The theory predicts that an altruistic parent will provide more transfers to her child in the absence of external incentives. As shown in Panel B of Table 8, however, the productivity index is not significantly related to baseline transfers.

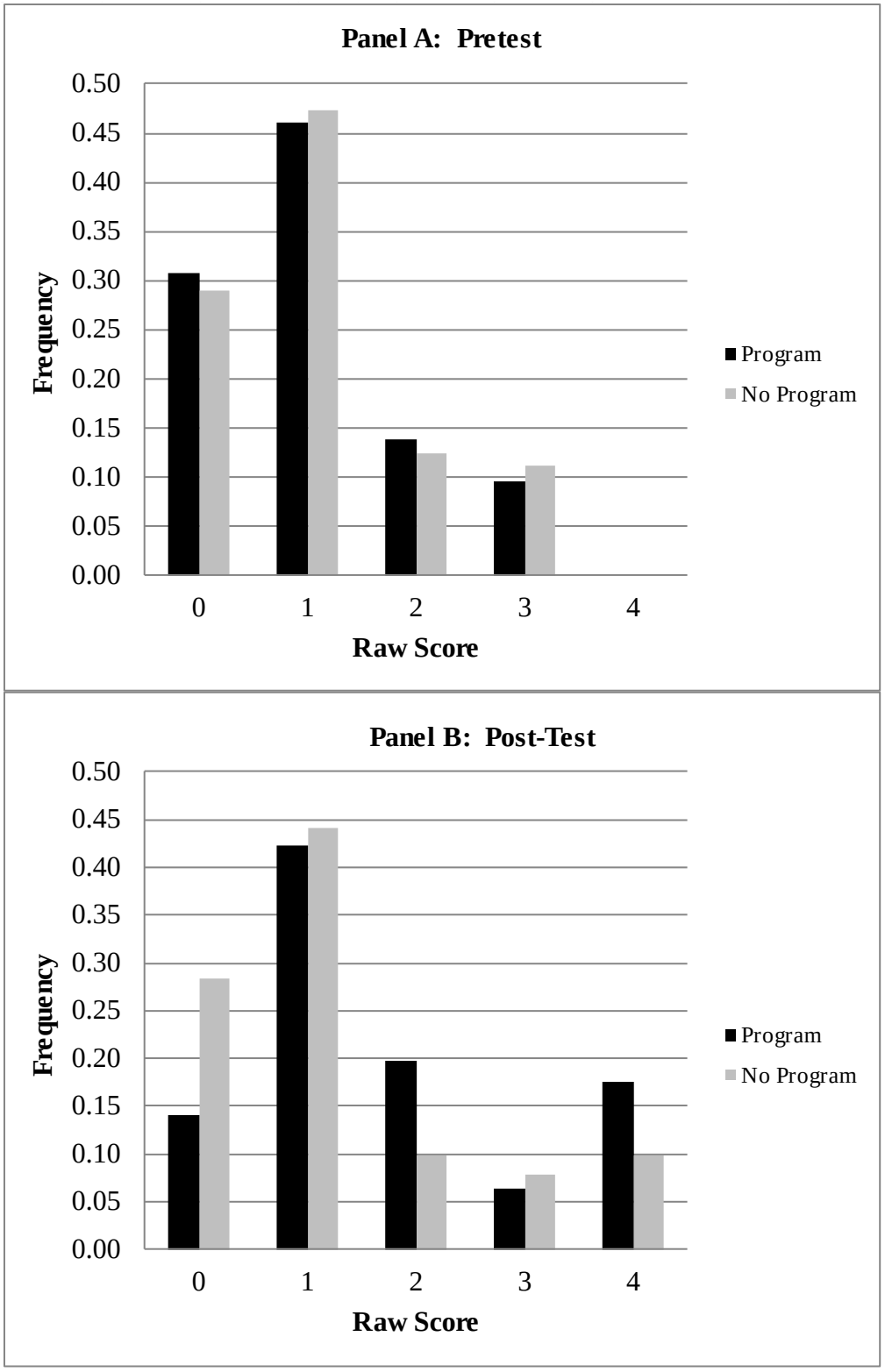
---

[1] This hypothesis was formed based on focus group discussions conducted during pilot activities.

[2] The female dummy is not included in Columns 2 and 3 of Table 3 because the majority of classes are organized by gender.

[3] This follows because on the interior, the first-order condition (13) can be rewritten as $\frac{(1-\gamma^*)\beta_c^2}{k_c} = \beta_p p + \beta_c c$.

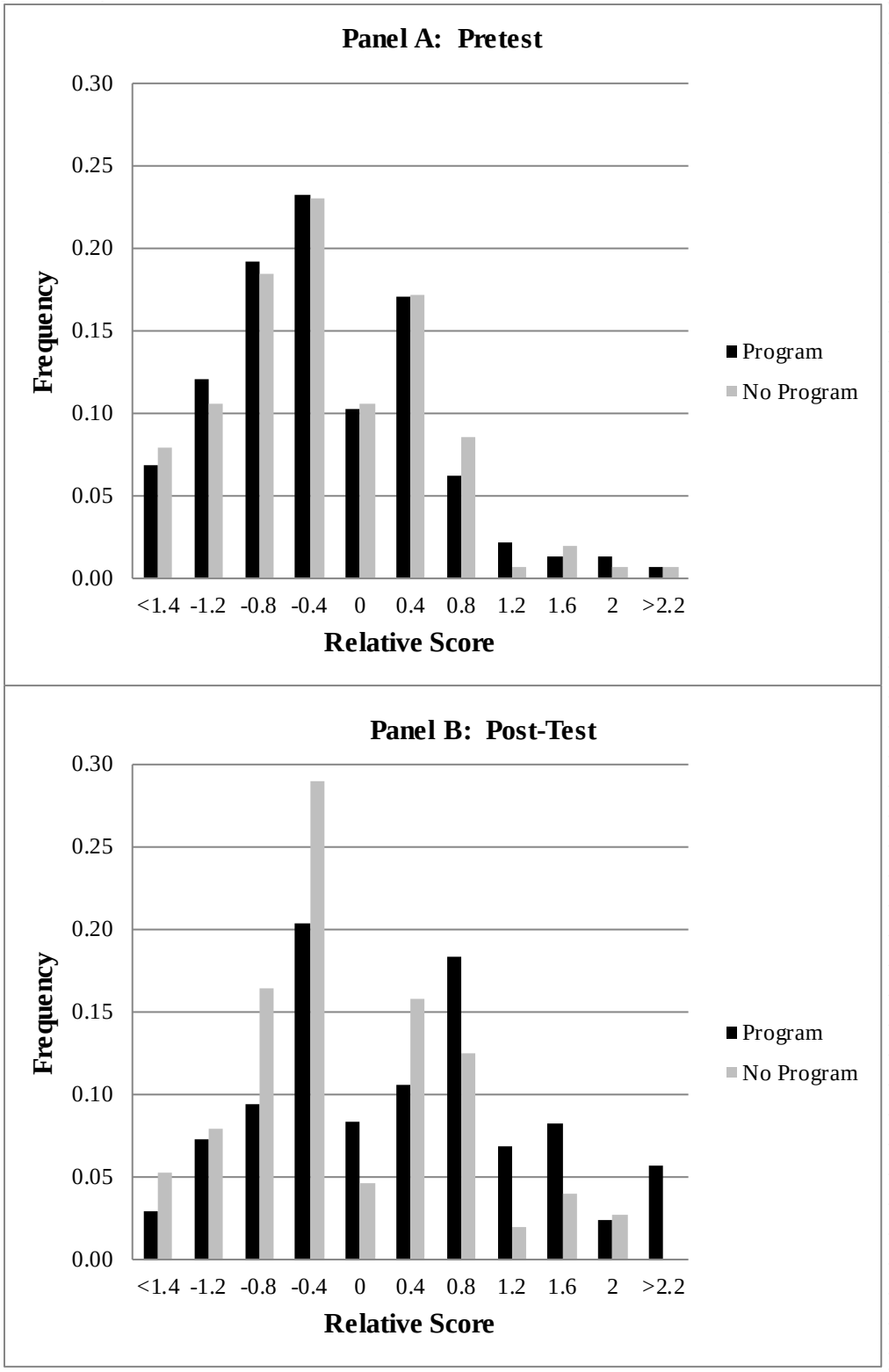
[4] Repeating the regressions in Appendix Table 11 using the productivity index generated from Column 1 of Table 9 produces similar results.



Appendix Figure 1

Distributions of Raw Test Scores, Program and No-Program Groups

Note: Sample includes all children in the randomized sample who took the post-test.



Appendix Figure 2

Distributions of Relative Test Scores, Program and No-Program Groups

Note: Sample includes all children in the randomized sample who took the post-test.

Appendix Table 1

Choice Between Toy and Money

	Sample		
	All	Achievers Only	
	Ex Ante Treatment	Ex Ante Treatment	Ex Post Treatment
	(1)	(2)	(3)
Chose Toy	0.327	0.316	0.513
Chose Money	0.673	0.684	0.487
Observations	153	76	78

Notes: This table displays the choices of the parents in the exante and expost choice treatments. Columns 2 and 3 include only parents of children who reached the program goal.

Appendix Table 2

Treatment Effects of Choice Treatments

	Dependent Variable: Attendance in After-School Classes		Dependent Variable: Achievement of Literacy Goal	
	(1)	(2)	(3)	(4)
Panel A: Average Effects				
Ex Ante Choice	0.093*	0.077	-0.050	-0.041
	(0.054)	(0.053)	(0.069)	(0.070)
Ex Post Choice	0.000	-0.004	0.016	0.024
	(0.044)	(0.039)	(0.057)	(0.062)
Pretest Dummies	NO	YES	NO	YES
Classroom Dummies	NO	YES	NO	YES
Observations	755	755	900	900
R-squared	0.006	0.135	0.003	0.172
Mean of Dep. Var.	0.210	0.210	0.553	0.553
Panel B: Interactions with Pretest Score				
	Categorical Score	Relative Score	Categorical Score	Relative Score
Ex Ante Choice * Pretest	-0.045	-0.069	-0.026	-0.053
	(0.060)	(0.073)	(0.064)	(0.058)
Ex Post Choice * Pretest	-0.061	-0.065	-0.011	-0.043
	(0.059)	(0.067)	(0.064)	(0.074)
Pretest Dummies	YES	YES	YES	YES
Classroom Dummies	YES	YES	YES	YES
Observations	755	755	900	900
R-squared	0.148	0.150	0.177	0.196

Notes: In Columns 1 and 2, the dependent variable is a dummy which equals one if the child attended the after-school classes on at least one day. In Columns 3 and 4, the dependent variable is a dummy which equals 1 if the child reached the literacy goal. The omitted treatment group is parent money. Panel A includes dummies for child money, toy, and voucher treatment groups. Panel B includes dummies for all treatment groups, other than parent money, and their interactions with pretest score. Standard errors are clustered at the classroom level. * significant at 10 percent; ** significant at 5 percent; *** significant at 1 percent

Appendix Table 3
Sample Attrition

Variable	Mean Attriters (1)	Mean Stayers (2)	Difference (3)	S.E. (4)	P-value (5)
Parent Money	0.240	0.167	0.073	0.090	0.420
Child Money	0.160	0.169	-0.009	0.068	0.896
Voucher	0.200	0.168	0.032	0.065	0.624
Toy	0.160	0.161	-0.001	0.068	0.987
Ex Ante Choice	0.080	0.168	-0.088	0.060	0.151
Ex Post Choice	0.160	0.168	-0.008	0.078	0.921
Raw Pretest Score	1.080	1.022	0.058	0.176	0.744
Relative Pretest Score	-0.063	-0.317	0.253	0.138	0.073
Female	0.560	0.571	-0.011	0.086	0.898
Number of Children 0-14	2.560	2.925	-0.365	0.167	0.034
Number of Adults 15+	2.360	2.444	-0.084	0.161	0.605
Mother Employed	0.320	0.346	-0.026	0.087	0.767
Mother Education	3.500	3.171	0.329	0.904	0.717
Father Education	6.875	6.412	0.463	0.825	0.578
Durables	-0.439	0.012	-0.451	0.111	0.000
Helped with Studies	0.280	0.360	-0.080	0.083	0.342
Tutoring Fees Paid	38.000	26.264	11.736	15.410	0.450
Money Given	10.560	13.135	-2.575	1.685	0.133
Gave Toys	0.000	0.038	-0.038	0.006	0.000
Gave Sweets	0.160	0.190	-0.030	0.072	0.675
Gave Clothes	0.000	0.016	-0.016	0.005	0.004
Gave School Supplies	0.000	0.065	-0.065	0.009	0.000

Notes: See Table 2 notes for variable definitions. Standard errors are clustered at the classroom level.

Appendix Table 4

Bounds on Treatment Effects Using Lee (2009) Trimming Method

	Dependent Variable: Achievement of Literacy Goal			
	Estimate	Lower Bound	Upper Bound	Trimming Portion
	(1)	(2)	(3)	(4)
Panel A: 6 Main Treatment Groups				
Child Money	-0.040	-0.047	-0.033	0.013
	(0.054)	(0.059)	(0.056)	
Voucher	-0.037	-0.040	-0.033	0.007
	(0.060)	(0.065)	(0.066)	
Child Toy	0.012	0.007	0.019	0.012
	(0.063)	(0.062)	(0.060)	
Panel B: Money vs. Toy				
Toy	0.007	0.006	0.009	0.003
	(0.049)	(0.049)	(0.048)	

Notes: Each cell represents the mean difference in outcome between the indicated category and the parent money treatment (Panel A) or the parent and child money treatments (Panel B). Comparisons are raw means (no controls). "Toy" represents the aggregated child toy and voucher treatments. Standard errors are constructed based on 500 bootstrap draws, sampling by classroom. * significant at 10 percent; ** significant at 5 percent; *** significant at 1 percent

Appendix Table 5

Bounds on Treatment Effects Using Lee (2009) Trimming Method, by Categorical Pretest Score

	Dependent Variable: Achievement of Literacy Goal			
	Estimate	Lower Bound	Upper Bound	Trimming Portion
	(1)	(2)	(3)	(4)
Money vs. Toy				
Pretest = 0	0.072	0.056	0.100	0.043
	(0.071)	(0.075)	(0.079)	
Pretest = 1	0.031	0.021	0.044	0.022
	(0.072)	(0.070)	(0.066)	
Pretest = 2	-0.108	--	--	0.000
	(0.094)	--	--	
Pretest = 3	-0.153	-0.161	-0.151	0.010
	(0.113)	(0.129)	(0.125)	

Notes: Estimates represent the mean difference in achievement between the aggregated toy treatments and the money treatments. Comparisons are raw means (no controls). Standard errors are constructed based on 500 bootstrap draws, sampling by classroom. * significant at 10 percent; ** significant at 5 percent; *** significant at 1 percent

Appendix Table 6

Determinants of Being Reached At Baseline

	Dependent Variable: Reached At Baseline (Dummy)		
	(1)	(2)	(3)
Relative Pretest Score	-0.001	-0.014	-0.025**
	(0.015)	(0.015)	(0.011)
Grade 2	-0.005		
	(0.042)		
Grade 3	0.001		
	(0.049)		
Female	0.029		
	(0.035)		
Classroom Dummies	NO	YES	YES
Surveyor Dummies	NO	NO	YES
Observations	1052	1052	1052
R-squared	0.002	0.098	0.389

Notes: The sample includes all children in the randomized sample who took both the pretest and post-test. In Column 3 a separate dummy is included for each surveyor in addition to a dummy indicating whether the household was surveyed by a team of two surveyors. Standard errors are clustered at the classroom level. * significant at 10 percent; ** significant at 5 percent; *** significant at 1 percent

Appendix Table 7

Overall Effects of Incentives

	Dependent Variable:					
	Reached Goal		Raw Score		Relative Score	
	(1)	(2)	(3)	(4)	(5)	(6)
Program Group	0.269***	0.261***	0.590***	0.577***	0.609***	0.596***
	(0.044)	(0.044)	(0.111)	(0.112)	(0.112)	(0.111)
Attendance in After-School Classes		0.156**		0.252*		0.252**
		(0.057)		(0.097)		(0.085)
Pretest Dummies	YES	YES	YES	YES	YES	YES
Classroom Dummies	YES	YES	YES	YES	YES	YES
Surveyor Dummies	YES	YES	YES	YES	YES	YES
Dep. Var. Mean, No-Program Group	0.309	0.309	1.270	1.270	-0.142	-0.142
Observations	1052	1052	1052	1052	1052	1052
R-squared	0.200	0.212	0.548	0.553	0.524	0.529

Notes: The sample includes all children in the randomized sample who took both the pretest and post-test. In each regression a separate dummy is included for each surveyor in addition to a dummy indicating whether the household was surveyed by a team of two surveyors. Columns 5 and 6 control for baseline relative pretest score. Standard errors are clustered at the classroom level. * significant at 10 percent; ** significant at 5 percent; *** significant at 1 percent

Appendix Table 8

Treatment Effects on Self-Reported Transfers

	Average Transfers	Money given	Gave Toys	Gave Sweets	Gave Clothes	Gave School Supplies
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. First Follow-up vs. Baseline						
Toy	-0.090*	-0.010	0.013	-0.016	-0.043***	-0.034
	(0.050)	(0.687)	(0.023)	(0.042)	(0.014)	(0.023)
Baseline	0.091**	0.027**	0.078	0.014	-0.063***	0.016
	(0.045)	(0.012)	(0.073)	(0.049)	(0.023)	(0.069)
Observations	576	596	589	590	576	576
R-squared	0.147	0.223	0.115	0.113	0.103	0.116
Panel B. Posttest Follow-up vs. Baseline						
Toy	-0.487***	-0.709	-0.076**	-0.031	-0.200***	-0.078**
	(0.081)	(0.970)	(0.036)	(0.041)	(0.037)	(0.032)
Baseline	0.025	0.023	-0.102	-0.022	0.037	0.161*
	(0.065)	(0.020)	(0.062)	(0.049)	(0.125)	(0.090)
Observations	581	595	591	592	582	582
R-squared	0.261	0.295	0.2	0.158	0.181	0.139

Notes: In Panel A the sample is restricted to the money and toy treatments. The omitted treatment categories are parent and child money. "Toy" represents the aggregated child toy and voucher treatments. "Average Transfers" is an average of the z-scores of the five individual transfer categories. Baseline means and standard deviations were used in computing the z-scores. "Baseline" represents the baseline value of the transfer variable. All regressions include classroom and pretest dummies. See Table 2 notes for variable definitions. Standard errors are clustered at the classroom level. * significant at 10 percent; ** significant at 5 percent; *** significant at 1 percent

Appendix Table 9

Heterogeneity in Program Effects

	Dependent Variable: Achieved Goal	
	(1)	(2)
Program Group	0.173*	0.265***
	(0.072)	(0.068)
Raw Pretest Score	-0.029	-0.058
	(0.042)	(0.051)
Pretest*Program Group	0.052	0.007
	(0.048)	(0.052)
Classroom Dummies	NO	YES
Surveyor Dummies	NO	YES
Observations	1052	1052
R-squared	0.028	0.160

Notes: The sample includes all children in the randomized sample who took both the pretest and post-test. In each regression a separate dummy is included for each surveyor in addition to a dummy indicating whether the household was surveyed by a team of two surveyors. Standard errors are clustered at the classroom level. * significant at 10 percent; ** significant at 5 percent; *** significant at 1 percent

Appendix Table 10:

Interactions of Toy Treatment and Productivity Index, Including Interactions with Test Scores

	Dependent Variable: Attendance in After-School Classes		Dependent Variable: Achievement of Literacy Goal	
	(1)	(2)	(3)	(4)
Toy	-0.021	-0.169***	-0.013	-0.112
	(0.037)	(0.057)	(0.057)	(0.076)
Relative Pretest Score	0.164	0.154*	0.308***	0.306***
	(0.098)	(0.092)	(0.056)	(0.056)
Toy*Relative Pretest Score	-0.099**	-0.077*	-0.069	-0.054
	(0.042)	(0.039)	(0.046)	(0.049)
Productivity Index		0.249***		0.124
		(0.087)		(0.135)
Toy * Productivity		-0.508***		-0.329
		(0.135)		(0.204)
Pretest Dummies	YES	YES	YES	YES
Classroom Dummies	YES	YES	YES	YES
Observations	502	502	598	598
R-squared	0.171	0.178	0.218	0.220

Notes: In Columns 1 and 2, the dependent variable is a dummy which equals one if the child attended the after-school classes on at least one day. In Columns 3 and 4, the dependent variable is a dummy which equals one if the child reached the literacy goal. The omitted treatment categories are parent and child money. "Toy" represents the aggregated child toy and voucher treatments. The productivity index represents the predicted values of the regression in Column 2 of Table 6, using the average effect of the classroom dummies. In Columns 2 and 4 standard errors are constructed based on 500 bootstrap draws, sampling by classroom. * significant at 10 percent; ** significant at 5 percent; *** significant at 1 percent

Appendix Table 11

Importance of Components of the Productivity Index: Toy-Productivity Interactions, Dropping One Variable at a Time from Productivity Index

	Variable Dropped							
	Number of Children 0-14	Number of Adults 15+	Durables	Mother Employed	Mother Education	Father Education	Helped with Studies	Tutoring Fees Paid
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Outcome: Attendance								
Toy * Productivity	-0.502***	-0.588***	-0.666***	-0.648***	-0.710***	-0.728***	-0.624***	-0.584**
	(0.134)	(0.149)	(0.177)	(0.147)	(0.132)	(0.154)	(0.134)	(0.236)
Observations	502	502	502	502	502	502	502	502
R-squared	0.162	0.165	0.165	0.166	0.167	0.169	0.166	0.161
Panel B: Outcome: Achievement								
Toy * Productivity	-0.212	-0.348*	-0.383**	-0.451**	-0.456**	-0.230	-0.357*	-0.486*
	(0.197)	(0.187)	(0.180)	(0.202)	(0.197)	(0.232)	(0.189)	(0.264)
Observations	598	598	598	598	598	598	598	598
R-squared	0.191	0.192	0.193	0.194	0.194	0.192	0.193	0.193

Notes: Within each panel, each column replicates the regressions from Columns 2 and 4 of Table 7, where one variable at a time is dropped from the productivity index. * significant at 10 percent; ** significant at 5 percent; *** significant at 1 percent

Appendix Table 12

Importance of Components of the Productivity Index: Individual Interactions with Toy Treatment

	Interacted Variable							
	Number of Children 0-14	Number of Adults 15+	Durables	Mother Employed	Mother Education	Father Education	Helped with Studies	Tutoring Fees Paid
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Outcome: Attendance								
Toy * Variable	0.056	-0.050	-0.002	0.010	-0.009	-0.006	-0.080	-0.160**
	(0.037)	(0.043)	(0.017)	(0.079)	(0.011)	(0.011)	(0.082)	(0.072)
Observations	500	487	495	501	488	478	502	496
R-squared	0.170	0.165	0.158	0.163	0.152	0.155	0.156	0.163
Panel B: Outcome: Achievement								
Toy * Variable	0.063**	-0.061	0.001	-0.049	0.006	0.007	-0.030	-0.022
	(0.029)	(0.039)	(0.026)	(0.092)	(0.015)	(0.012)	(0.070)	(0.095)
Observations	594	577	590	597	590	579	598	591
R-squared	0.198	0.200	0.193	0.190	0.194	0.202	0.191	0.190

Notes: Within each panel, each column replicates the regressions from Columns 2 and 4 of Table 7, where the productivity index is replaced by a single variable. Standard errors are clustered at the classroom level. * significant at 10 percent; ** significant at 5 percent; *** significant at 1 percent

Appendix Table 13

Importance of Components of the Productivity Index: Groups of Variables

	Interacted Variable		
	Demographics	Education	Helping
	(1)	(2)	(3)
Panel A: Outcome: Attendance			
Toy * Variable	-1.273**	-0.164	-0.803***
	(0.579)	(0.550)	(0.271)
Observations	502	502	502
R-squared	0.176	0.156	0.161
Panel B: Outcome: Achievement			
Toy * Variable	-0.756	-0.329	-0.025
	(0.470)	(0.459)	(0.347)
Observations	598	598	598
R-squared	0.195	0.196	0.190

Notes: Within each panel, each column replicates the regressions from Columns 2 and 4 of Table 7, where different variables are used in the productivity index. In Column 1, the number of household members under 14, the number of members above 14, and the mother's employment status are used as the index. In Column 2, the mother's and father's education levels are used in the index. In Column 3, the variables indicating help with homework and the amount spent on tutoring are used in the index. * significant at 10 percent; ** significant at 5 percent; *** significant at 1 percent