# USING EXPERIMENTAL ECONOMICS TO MEASURE THE EFFECTS OF A NATURAL EDUCATIONAL EXPERIMENT ON ALTRUISM*

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**Abstract:** Economic research examining how educational intervention programs affect primary and secondary schooling primarily focuses on test scores although schools can affect many other outcomes. This paper examines how an educational intervention, a voucher program, affected students' altruism. The voucher program used a lottery to allocate scholarships among low-income applicant families with children in K-8$^{th}$ grade. By exploiting the lottery to identify the voucher effects, and using experimental economic methods, we measure the effects of the intervention on children's altruism. We also measure the voucher program's effects on several other student outcomes including test scores and on parents' altruism. We find that the educational intervention positively affects students' altruism towards charitable organizations but not towards their peers. We fail to find statistically significant effects of the vouchers on student test scores or parents' altruism.

# I. Introduction

Educational interventions can affect both academic and non-academic outcomes [Figlio and Ludwig 2000, Greene 2000, Angrist, Bettinger, Bloom, King, and Kremer 2002, Jacob and Lefgren 2003]. Test scores are certainly one of the more important measures of the success of an educational intervention since they reflect academic achievement and potential future earnings [Levy, Murnane and Willet 1995]. Yet, test scores are just as certainly not the only measure of an educational intervention [Angrist et al. 2002].[1] Bowles, Gintis and Osborne [2001, pp. 158] argue that test scores are an imperfect measure of the effects of schooling and that economists "need broader indicators of school success, including measures based on the contribution of schooling to behavioral and personality traits." Heckman [2000, pp. 4] further argues that "[t]he preoccupation with cognition and academic 'smarts' as measured by test scores to the exclusion of social adaptability and motivation causes a serious bias in the evaluation of human capital formation. Educational programs may benefit students and society independent of their effects on test scores.

To understand the possible non-academic effects of educational interventions, this paper studies the effect of a voucher program on altruism. We study the effect of an educational intervention on altruism for several reasons. First, economists are increasingly interested in the welfare implications of altruism and its possible effect on other outcomes. For example, altruism can generate positive externalities [Bergstrom, Blume and Varian, 1986; Andreoni 1988] and can positively influence capital accumulation, intergenerational transfers, marriage, and the environment [e.g. Jouvet, Michal and Pestieau 2000, Dynan, Skinner and Zeldes 2002, Andreoni, Brown and Rischall 2003, MacDonald and Koh 2003]. Second, schools (and hence educational interventions)

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[1] For example, Angrist et al. [2002] evaluate the effect of educational vouchers on both test scores and additional outcomes such as teen pregnancy and marriage rates; Figlio and Ludwig [2000] examine the effects of private schooling on teenage sexual activity and drug use; Greene [2000] looks at the effects of private schooling on future voting; and Jacob and Lefgren [2003] examine the effects of school holidays on juvenile crime. In addition, Becker [1993] noted that empirical evidence shows that more education, among other outcomes, improves health, reduces smoking, increases voting and increases knowledge about birth control.

may affect altruism. A school's role in socialization may increase altruism by teaching pupils to share and help others in their classroom, school and community. Further, schools, particularly religious schools, even claim to teach charity or altruism as part of their curriculum.[2] Third, research by economists [e.g. Harbaugh and Krause 2000] and psychologists [e.g. Bryan and Walbek 1968, Froming 1985, Bizman et al. 1978] suggest that individuals develop altruism while young and that even school-age children are developing altruism.

Even though educational interventions can affect the development of altruism (and other non-academic outcomes), economists continue to use test scores as the central (and often sole) metric of their success. This is especially the case for educational voucher programs where research has largely focused on whether voucher winners' test scores improve relative to some control group [Rouse 1998, Myers, Peterson, Mayer, Chou, and Howell 2000, Angrist et al. 2002, Krueger and Zhu 2003]. In the case of vouchers, this focus on test scores is easy to understand. Test scores are relatively inexpensive to collect and, with the advent of high-stakes testing, they are the primary metric by which federal and state governments judge schools. However, the focus on test scores seems at odds with voucher proponents' claims that greater choice is justifiable by the fact that non-academic outcomes and programs (e.g. safety, discipline, peers or curricula that promote health or a specific moral code) influence parents' educational decisions for their children [Harrison and Kennison 1993]. Thus, given the economic importance of altruism and its development and given the claims that vouchers may affect non-academic behaviors including altruism [Greene 1998], we investigate the effects of vouchers on altruism.

Measuring children's altruism is not without difficulty, however, since economists typically measure altruism through gifts that are observable in adults, e.g., through household surveys or tax

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[2] Almost all of the private schools in our sample are Catholic, and in our pilot study interviews, private school principals claimed that their schools teach children to be "charitable."

records, but that are typically unobservable in children. Fortunately, laboratory methods exist to help measure and quantify altruism. Specifically, economists developed "dictator" games [Hoffman, McCabe and Smith 1996] to present subjects with situations in which they can exhibit altruism [Eckel and Grossman 1996, 1998]. Besides testing theory and quantifying altruism, these experiments study the determinants of altruism such as gender, race, age, deservingness of recipients, social distance and the relative costs of altruism [e.g., Andreoni and Vesterlund 2001 Eckel and Grossman 1998, Harbaugh and Krause 2000].[3] Using similar methods, we measure the effect of vouchers on altruism. Thus, this paper measures the effect of an educational intervention on altruism and presents a novel use for the experimental economic *laboratory* to assess policy intervention.[4]

Our use of *laboratory* experiments in a field setting extends recent efforts by economists. For example, experimental laboratory methods have been used in the field to study discrimination [Fershtman and Gneezy 2001 and List 2004], discount rates [Eckel, Johnson and Montmarquette 2003 and Harrison, Lau and Williams 2002], social capital [Karlan 2003], auction theory [List and Lucking-Reiley 2000 and Lucking-Reiley 1999] and charitable giving [List and Lucking-Reiley 2002]. We extend this line of laboratory field research to test for the effects an educational intervention.

Data for this research come from the Children's Scholarship Fund (CSF) of Toledo, Ohio. CSF offers 4-year renewable, private school scholarships to K-8th grade students in Northwest Ohio To be eligible, students must qualify for federal reduced/free lunch programs. In 1998, almost 2,500

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[3] Studying altruism using decision-making experiments is also potentially beneficial compared to survey methods since research shows that respondents tend to overestimate their charitable contributions [Fehr et al. 2003]. Additionally, psychologists have studied reciprocity and generosity in children for decades, with experimental studies demonstrating that children share with "charities" without receiving external rewards for their generosity [e.g. Midlarsky and Bryan 1967].

[4] Of course, economists frequently use natural and randomized field experiments, such as negative income tax and audit studies, to examine economic phenomena. The distinction made here is the *laboratory* setting. The laboratory setting not only allows us to measure outcomes of interest in populations in which the outcomes would otherwise be hard to observe, but also allows us to measure the outcomes quickly, which is perhaps a relatively more important concern for policy makers than for researchers.

families applied for CSF scholarships, and CSF awarded more than ,500 scholarships by lotery.[5]

Within this population, we use experimental economic methods to compare levels of altruism between voucher lottery winners and losers. We also use similar methods to measure altruism of parents whose children applied for scholarships. Gathering the same experimental data for both parents and children, as well as gathering family demographics, allows us to better control for the extent to which family background affects the children's altruism. Further, the extent to which children's behavior is correlated with their parents' behavior is of interest itself.

We find that voucher lottery winners were more altruistic towards charitable organizations than unsuccessful applicants. Voucher winners gave about 25 percent (or about $1) more than losers to charitable organizations This increased altruism, however, does not extend to greater generosity to peers. We also find that parent's altruism does not explain children's altruism. Similar to some past studies [e.g. Krueger and Zhu 2003], we find little evidence that voucher winners have significantly higher test scores although the estimated effects on test scores are noisy. Finally, we find that test scores and altruism are not correlated. These latter two findings (that vouchers significantly affect altruism and that test scores are uncorrelated with altruism) suggest that studies focusing exclusively on test scores may understate the overall impact of an educational intervention program.

## II.  Experimental Protocols and Data

We focus on the sample of applicants to the Children's Scholarship Fund of Toledo (CSF) in 1998. CSF offers 4-year, renewable, private school scholarships to low-income families in Northwest Ohio. To be eligible, students had to qualify for federal reduced/free lunch programs and either be entering or attending elementary or junior high school. Scholarships are renewable so long

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[5] The national office of CSF controlled the application process. In Northwest Ohio, the scholarships were advertised through radio, newspaper, and television advertisements.

as students attend private school. We focus on the 2,424 families who applied for scholarships in 1998.[6] CSF partitioned this group into two parts: those who had self-reported that at least one child had previously attended private school (1,265) and those who had not (1,159). CSF held separate lotteries for each group. We refer to these lotteries as the "private" and "public" school lottery. If a family won the lottery, all children were eligible for a voucher. The random assignment of scholarships facilitates identification by allowing us to use unsuccessful applicants as a comparison/control group for the scholarship winners.

We attempted to contact a random sample of 438 families representing almost 900 children. We attempted to survey and invite this sample to attend an "evaluation event" where we conducted our experiments, administered a standardized test (California Achievement Test) and administered an additional survey to parents.[7] Appendix 1 describes the data collection procedures in more detail.

A. Experimental Protocols for Altruism

To measure altruism, we had all subjects make decisions in six "dictator" games.[8] For the first three, we matched subjects to different non-profit organizations: The American Red Cross, The Make-A-Wish Foundation and The Children's Scholarship Fund. We used three organizations to control for the possibility that different degrees of personal association can influence generosity (for instance, greater personal association may cause greater reciprocity). In each decision, children were endowed with $10 (in Toys-R-Us gift certificates) and parents were endowed with $50 in cash. After reading instructions aloud and reviewing a brief written description of each organization, including a simplified version for children, subjects chose how much of their endowment to keep for

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[6] The program was small enough relative to the size of Toledo's public schools that it likely did not affect the neighboring public school system. In years before and after 1998 the number of scholarships CSF offered was much less than in 1998, thus we focus on the 1998 sample.

[7] Angrist et al. [2002] and Myers et al. [2000] gathered test score data at similar events. We also gathered data on time-preferences and self-confidence. These other measures are discussed in other papers. The order of the data collection was identical across all sessions and is presented in Appendix 1.

[8] The decision-making protocols for the generosity measures were adapted from Harbaugh and Krause [2000].

themselves and how much to give to the organization.[9] Appendix 2 shows the decision sheet for the American Red Cross. Eckel and Grossman [1996 and 2003] and Eckel, Grossman and Johnson [2003] use a similar pairing of college students with charitable organizations to measure other determinants of altruism.[10]

In the last three dictator games, we again endowed children with $10 in gift certificates and parents with $50 in cash. In each of these decisions, each subject chose how much of the endowment to keep and how much to give to another person. Any amount given was multiplied by an exchange rate before being given to the other person. The exchange rate varied from 50 percent (so each dollar given became $0.50 to the other person) to 100 percent and 150 percent. The recipient of the amount given away (i.e., the other person) was another child at the event for the children and another parent at the event for the parents. At the end of the session we *randomly* and *anonymously* selected who these recipients would be. Subjects did not know who they would be paired with at the time of the decision, nor did they ever find out. We did not collect this generosity-to-other-children measure in sessions with fewer than 5 children. To control for wealth effects, at the end of each session we randomly selected approximately one parent and one child per every five attendees and paid them for one of the decisions they made.[11] To avoid experimenter bias, during

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[9] To maximize the likelihood that the subjects believed we would send the organization the money, we had envelopes prepared. We also gave subjects the option to receive a "thank you" letter from the organization.

[10] We verified that parents felt that benefits from the charitable organizations went to deserving people. At the completion of the decision-making exercises we had parents (in the survey) respond on a scale from 1 (strongly disagree) to 5 (strongly agree) to the statement "(t)he recipients of the [charitable organization name] are deserving of support." For the Red Cross, Make-A-Wish and Children's Scholarship Fund, the average responses were 4.72, 4.80 and 4.79, respectively and only 3.4 percent of respondents assessed the deservingness of these organizations less than 4.

[11] Wealth effects in an experimental context occur when payments made to a given subject in an early decision impact their behavior in subsequent decisions. The experimental economics literature extensively discusses the use of a random selection mechanism to control for wealth effects [e.g., Davis and Holt, 1993]. The idea is that by being paid for a random subset of decisions, or just one, subjects anticipated wealth for a given decision, based on all other decisions, is essentially identical, thus placing them at the same point on their utility curves.

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the sessions the experimenters did not know whether subjects were voucher lottery winners or losers.[12]

We examine the amounts given to the three specific organizations to potentially capture different aspects of altruism. Generosity to charities often comes from an appreciation for non-profit organizations' goals and especially the neediness of the charity's potential beneficiaries. However, direct and/or indirect reciprocity can also be a motivation to give to non-profit organizations. Thus, among the three charities that we included in the experiment, we included the Children's Scholarship Fund – the same organization that gives the scholarships. Comparison of the amounts students give to each organization will allow us to partially distinguish between gifts motivated by the organizations' goals and gifts motivated by reciprocity.

We further examine the amounts students give to charities and to their peers to capture additional dimensions of altruism. Economists recognize that individuals may show different levels of altruism to charities than they show to peers: for instance, Eckel and Grossman [1996] show that college students give more to a non-profit organization than to peers in anonymous dictator games. While generosity to charities often comes from an appreciation for non-profit organizations' goals, generosity to peers often comes from concern for fairness [e.g., Fehr and Schmidt 1999, Bolton and Ockenfels 2000]. Giving to peers therefore captures an element of altruism motivated by concern for others and/or for fairness whereas giving to charities captures an element of altruism motivated by concern for non-profit organizations' goals. Thus, vouchers lottery winners may demonstrate greater altruism differentially across the amounts given to each charity and/or give different amounts to the charities than to peers depending on the relative importance of the voucher programs' influence on each dimension of altruism.

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12 All data was collected single blind (subjects did not know the research objectives) and single anonymous (subjects did not know any other subject's choices).

## B. Descriptive Statistics

Table shows descriptive statistics for the 2,424 families who applied to CSF in 1998. In the public school lottery, successful applicants (lottery winners) had statistically similar household sizes and proportions of children previously attending a private school, but averaged about $2,200 less income. While the income difference at the means is significant, the difference between the medians is not significant and is only about $600. A few outlier observations cause the difference in means. Although the public school lottery was supposed to include families with no private school experience, about 10 percent of families in the public lottery actually had at least one child who had attended private school prior to the lottery. Further, although the public school lottery was supposed to include families with at least one child with private school experience, in the private school lottery about 4 percent of the families had no child who previously attended private school. Since we are primarily interested in estimating the effects of the voucher on students who may have changed from public to private schools, our attempted survey and experimental sample focused largely on families from the public school lottery (390 of the 438).

For the preliminary survey, we contacted a majority of families by phone, mail, and household visits between March and October 2001.[13] Our survey response rate in the public school lottery was 61 and 67 percent for unsuccessful and successful applicants, respectively. This was much higher than anticipated since our contact information was 3 to 4 years old and was especially higher than anticipated given that more than 25 percent of the sample had moved during these 3 to 4 years. Moreover, the response rate is similar (if not better) than other voucher studies: for instance, Angrist et al. [2002] report a response rate of 52 percent for students contacted after 3 years and Myers et al. [2000] reports a 65 percent response rate after 2 years. The 67 and 61 percent response

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[13] The complete protocol (attempts, contact methods, resources for locating addresses) is available upon request from the authors.

rates across voucher winners and losers are symmetric (i.e., statistically identical), thus increasing

the likelihood of the internal consistency of the estimates of the voucher effect [Angrist 1997]

Further, we find no significant differences between winners who responded and winners who did not

and we find no significant differences between losers who responded and losers who did not

Table shows that 39 percent of unsuccessful families were single parent homes in the

public school lottery, 23 percent had one parent who graduated from college and that African-

Americans made up 57 percent of the sample. Race is the only variable in Table that suggests

differences between lottery winners and losers There were a lower proportion of blacks among

winners who responded than among losers who responded. This cannot be the result of non-random

voucher assignment, however, since CSF did not collect information on race prior to the lottery.[14]

The difference is possibly due to some non-response pattern in our survey. We control for race

throughout our empirical results to control for this difference.

Table 2 shows student characteristics among the surveyed families. We surveyed the parents

of 471 students of which 218 had not won the voucher lottery. On average, these students were 10

years old, in the fourth grade, and just over half were female. Column 2 compares the differences

between lottery winners and losers. There are no significant differences between lottery winners and

losers in age, gender, or grade, but consistent with the data in Table 1, African-American voucher

winners were less likely to respond than African-American lottery losers

Columns 3 and 4 of Table 2 compare students who attended our "evaluation events.

Students who attended were insignificantly younger than those not attending, and African-

Americans were more likely to attend. Column 5 reports the differences among winners and losers

who attended. As before, African-American lottery winners were less likely than African-American

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[14] Since CSF observed names, they may have been able to discriminate by observing distinctively African-American names; however, the magnitude of CSF's operation makes this less likely. CSF had over 1.25 million applicants nationwide in 1998 and only one person who managed the lists and conducted the selection lotteries for each of the sites.

lottery losers to attend the event, and we had more representation from winners than losers in the private school lottery

### III. Voucher Effects on Academic Outcomes

Before reporting the voucher effects on altruism, we show estimates of the voucher effects on educational outcomes and the correlations between educational outcomes and altruism. The effect on educational outcomes from the intervention provides comparison to previous studies. The correlation between educational outcomes and altruism provides an indicator of whether test scores are a sufficient statistic for the voucher program's overall effect.

Throughout the paper, we estimate the effect of the "intention to treat" rather than the effect of the "treatment on the treated" (i.e. the effect of using the voucher) or the effect of private schooling. The "intention to treat" is often referred to as the effect of being offered the voucher, and to measure the effect, we compared outcomes of lottery winners and losers, even if the winners declined the scholarship. Besides being perhaps the parameter of most interest to policymakers [Rouse 1998], the "intention to treat" is one effect for which we can produce an unbiased estimate.[15] Because of randomization, simple t-test comparisons of voucher winners and losers can identify the effects of the voucher [Angrist and Krueger 1999]. We also use the following regression to assess the effects of the voucher:

(1) $\qquad y_i = a - \beta(WonVoucher)_i - \gamma X_i - \varepsilon_i$

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[15] The "intention to treat" parameter is not the only parameter of interest to policy-makers or researchers. The effect of the "treatment on the treated" estimates the effect of voucher *usage* on student outcomes. Unbiased estimation of the effect of the "treatment on the treated" requires that we compare winners who took up the voucher to losers who would have taken up the voucher had they been offered. Since unobservable characteristics may determine take-up, creating an appropriate control group may be implausible, and any estimates that we may generate of this effect would not be defensible without additional information. Economists have also long been interested in estimating the effects of private school [e.g. Neal 1997]. Some have debated whether vouchers may facilitate identification of the effect of private schools [e.g. Rouse 1998, Angrist et. al 2002]. While vouchers may affect the likelihood of attending private schooling, it still may not be a suitable instrument since winning a voucher lottery may affect students' outcomes for reasons other than the voucher (e.g. income effects, reciprocity to charities).

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where $y_i$ is an outcome for student $i$ and $X_i$ includes controls for race, gender, age, family income, house size, and whether the family was part of the private school lottery. Throughout the tables, we report standard errors that correct for correlation across siblings.

Table 3 reports estimates of Equation 1 on academic outcomes. The first two rows show the effects that the voucher had on the type of school that students attended. At the time of our survey, 21 percent of unsuccessful voucher applicants were attending private school. The voucher intervention increased private school attendance by almost 23 percent. This effect is highly significant. We also find significantly less voucher winners than losers were suspended. voucher winners being suspended may be a positive outcome but may also be due to differences in public/private school disciplinary policies and thus may not be a "true" change in behavior. We also find no evidence that voucher winners were more or less likely to have repeated a grade.

Table 3 also shows results for the mathematics exams administered to most of the event participants.[16] We administered exams and computed the normal curve equivalents for each student on each exam. Because we have a small sample size, we "stack" the test scores creating two observations for most of our sample. Students scored on average in the 48th percentile with a standard deviation of 23 percentage points. When we compare test scores without controlling for student covariates, we find that voucher winners scored about 0.97 percentage points higher. covariates, students who won the voucher lottery score an insignificant 0. percentage points higher than unsuccessful applicants. The standard error bands are generous on these estimates because of the small sample size and we cannot rule out test score effects that may be positive or negative and as large as 1/3 of a standard deviation in magnitude. The test score results are qualitatively consistent to those found in voucher studies from New York City [Mayer, Peterson,

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[16] There were 212 students who actually attended the "evaluation events." These events lasted up to 2.5 hours and several families left before the test administration. In the initial events, we also did not anticipate a need for tests for students above 7th grade. We were thus unable to test a few students who came to these initial events.

Myers, Tuttle, and Howell 2002, Myers et. al 2000, Krueger and Zhu 2003] where no significant test score effects were also detected.

A typical voucher study might stop at this point. We found no significant effects on test scores. Thus, if test scores are a sufficient statistic for all outcomes of interest, then we need not proceed further. However, this is not the case. If test scores were a sufficient statistic for other outcomes, then we should see test scores correlate with outcomes even if there is a "true" test score effect that is not detectable because of our small sample size. When we compare student test scores to our measures of altruism, we find small correlations. The correlation between students' math scores and the amount they give to the Red Cross, the Make-a-Wish Foundation, and the Children's Scholarship Fund are only .06, .06, and .09 respectively. The correlations with the three measures of giving to their peers are all slightly negative at -.02, -.01, and -.13. Thus, test scores do not appear to be a sufficient statistic for other outcomes of interest, and the next section explores the extent to which vouchers may affect students' altruism.

## IV. Voucher Effects on Altruism

Table 4a presents OLS regressions that show the determinants of the amount children give to charitable organizations and to each other. The dependent variable is the amount that a student gives to the respective entity. The first three columns report estimates of Equation 1 where the dependent variable is the amount given to the respective charity. Columns 4 and 5 report the effects of vouchers when we "stack" the regressions across the charities.[17] Column 6 reports the estimated amount given to peers. We report the means for the voucher lottery losers in the first row.

On average, voucher lottery winners give 40 percent ($1.19/$3.00) more to the Red Cross than voucher lottery losers, and likewise 14 percent ($0.52/$3.73) and 22 percent ($0.71/$3.27) more

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[17] In the stacked regressions, we include dummy variables for the respective charities. As before, we control for correlation across siblings.

to Make-A-Wish and CSF, respectively. Averaging across all three charities, voucher winners give just over 25 percent ($.84/$3.33) more than voucher losers. Controlling for the covariates, we find that winning the voucher significantly increases giving to the Red Cross (t=2.66, p=.009). The voucher also has a positive though insignificant effect on giving to Make-A-Wish and the Children's Scholarship Fund (CSF: t=1.43, p=.157; Make-A-Wish: t=0.99, p=.322). Column 4 shows that in the "stacked" regressions voucher lottery winners give a marginally significant amount more than voucher lottery losers (t=1.77, p=.076).

When we control for the size of the child's parental contribution to the same charity in the stacked regression (Column 5), we find that the estimated voucher effect slightly increases from 0.75 to 0.78 and is slightly more significant (t=1.87, p=.061). While we do find a positive but insignificant relationship between parental and child giving in the stacked regression, this relationship is marginally significant for the amount that children give to the Red Cross and is directionally positive for the amount children give to Make-A-Wish and CSF.

If children's altruism is motivated by direct and/or indirect reciprocity to the non-profit organizations, then we would expect the largest increase in the amount given by lottery winners than lottery losers to occur for CSF. However, the estimated mean additional amount that lottery winners gave to the Red Cross and Make-A-Wish is $0.85 [= ($1.19+$0.51)/2] whereas the additional amount lottery winners gave to CSF is $0.71. These estimates suggest that the motivation that lottery winners gave more to charities is not motivated by reciprocity.

The final column of Table 4a shows the determinants of how much children gave to other children. In this regression, we fail to find a significant effect of the voucher. The point estimate is close to zero and statistically insignificant. These results suggest that the reason lottery winners

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gave more to charities is not motivated solely by a concern for fairness. The regression results also show that the exchange rate has no significant affect on the amount children give.[18]

To validate our measures, we compare coefficients presented in Table 4a with those examined by other researchers in similar experiments. Our age and gender estimates on giving are consistent with results reported in Harbaugh et al. [2000]; older children give more (significantly) to the charities) and boys give less (significantly) to their peers.[19] The current data thus show that Harbaugh et al.'s results, which focus on a relatively suburban white population, are robust to our sample consisting of a majority of urban African-American children. Also, similar to the behavior of college students reported in Eckel and Grossman [1996], we find that children in first through eleventh grade, as well as their parents, give more to charities than to peers. For instance, aggregating across voucher winners and losers, we find that children on average gave $3.77 (38 percent) of their $10 endowment to the charities but only $2.61 (26 percent) to other children. Regressions (not shown) indicate that these differences are highly significant. These comparisons indicate that our measures are externally valid with, and provide robustness to, the previous experimental evidences on the determinants of altruism.

In Table 4b, we test alternative specifications for the robustness of these results. These estimates of the stacked regressions are based on ordered probits and tobits. These approaches are more parametric in nature and thus may have advantages over the OLS estimates. The ordered probit model may capture the fact that increasing one's contribution by $1 may have a different

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[18] We expected children would give more if they had other regarding preferences [see e.g., Bolton and Ockefels 2000 or Fehr and Schmid 2000] since the higher the exchange rate the more the same dollar given would be worth to the recipient. However, one reason there may not be a significant relationship between the exchange rate and the amount given is that some children may have a preference for equal outcomes; for these children, giving less than the higher exchange rate will maximize utility (i.e., give $7 or $6 when the exchange rate is 50% so each child receives $3.50, give $5 when the exchange rate is 100% so each child receives $5, and give $4 when the exchange rate is 150% so each child receives $6.00). Additional evidence consistent with this behavior is presented in Andreoni and Vesterlund [2001] using college students and theoretical developments are in Charness and Rabin [2002].

[19] Harbaugh et al. [2002] report regressions that do and do not control for height. The lower generosity of boys generally disappears when they control for height. However, since we did not measure height, we compare our results to those in which the previous study also did not control for height.

significance when, for instance, the base contribution was $0 as opposed to $9. The tobit model may capture the fact that students could not give more than $10 or less than $0: in students' overall generosity, there may be censoring relative to the range we assign in the experiment.

In all specifications, OLS, ordered probits and tobits, we find similar results. We find significant effects of the voucher on students' gifts to charities. The marginal effects in the tobit model are even larger than the marginal effects reported in Table 4a.

Although voucher lottery winners gave more to charities than lottery losers, the estimated effect does not directly test whether the observed voucher effect in children is the result of private schooling or possibly reciprocity for being the recipient of a charity (the voucher). One potential way to indirectly test the latter possibility of reciprocity is to compare students giving across charities. If the reciprocity is directed at CSF, then this may be the case; however, the largest voucher effect is on the Red Cross. Moreover, among voucher winners, the average donation to CSF was nominally smaller but not statistically different from their other donations.

If reciprocity is aimed at any charity rather than just CSF, then another way to test for reciprocity may be to look at the effect of the voucher on parental giving since parents were not exposed to the schooling but yet were beneficiaries of the charity. Inherently, this type of strategy assumes that parents and children have a similar psychology of giving. Table 5 reports stacked regressions on the effects of the voucher on parental choices. In this case, we find no significant effects of the voucher on the amount parents gave to charities or peers. The point estimates on the effects on amount given to the charities is in fact negative in the OLS and ordered probit specifications, but insignificant (p > .20). If the psychology of giving is similar for parents and children, then the fact that voucher parents were beneficiaries of a charity and yet did not give more money back to charities suggests that the voucher effect in children is more likely the result of

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private schooling or some other voucher mechanism rather than reciprocation for being the voucher lottery winners.

## V. Conclusion

Education research largely focuses on the effects of interventions on student test scores. However, educational interventions may affect other academic and non-academic outcomes. Using experimental economic methods, the main contribution of this paper is that we document significant effects of an educational intervention on the amount children give to charities. This result, in conjunction with virtually no correlation between test scores and altruism, suggests that studies that primarily focus on test scores may understate the overall effect of educational intervention.

We do not find voucher effects on the amount children give to other children, suggesting that the voucher effect on altruism does not extend to all forms of giving. We also find that voucher lottery winners on average gave the same additional amount to each charity than voucher losers and no significant voucher effects on parent's generosity to charities, both suggesting that children's increased altruism towards charities may not be driven by reciprocity but rather by other mechanisms.

While the experimental methods and protocols used in the current study were developed primarily to test theory, the current paper presents an alternative use for these methods. The paper combines these methods with modern program evaluation techniques and exploits a natural experiment to demonstrate the possible effects of an educational program on altruism. Moreover, the use of experimental methods allowed us to measure generosity in a more timely and reliable way than we would have been able to measure it had we used traditional measures of charitable giving (e.g. charitable donations on tax records or through household expenditure surveys).

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In addition to our main contributions, we report several results regarding children's behavior that extend this growing literature [e.g., Harbaugh et al. 2000, 2001, 2002, and 2004]. We confirm several previous findings regarding children's behavior using an ethnically diverse urban group of children. We also find results previously only observed in the literature for adults: children give more to charitable organizations than they give their peers. Finally, we find that age and gender are important covariates for altruism: girls gave more than boys to their peers, while older children gave more to charities. Thus, behaviors observed in experimental economic studies of adults are observed with children who are in their $1^{st}$-$11^{th}$ grade years.

Appendix 1. Description of data collection.

*Applicant Data and Lottery*

The national headquarters of the Children's Scholarship Fund collected the applicant list and conducted the randomization. They assigned families random numbers and then released winners' names in three separate waves. The first wave was the largest and included most winners. Applicants received notification of the award by mail and phone. Applicants were required to submit tax records to verify income eligibility. After seeing that many scholarships would not be claimed, the national headquarters released a second wave of names and eventually a small third wave. There were 39 winners who did not appear in the applicant list yet all received the scholarship. A majority of these families had children previously attending private school. It is not clear when these people were added and whether these people were added at the national or local levels. Because of potential non-randomness, we excluded these people in our analyses. Our estimates of the effects of the voucher on generosity do not change when we include these people.

*Experimental Procedures*

We collected all outcomes single blind (subjects are unaware of what choices other subjects make) and single anonymous (subjects are unaware of the objective of the experiment). To avoid bias, the experimenters were unaware of the subjects' voucher status during the experiments. We also simultaneously and separately collected similar experimental measures for parents.

We compensated each parent $15 in cash for attending and each child $5 in Toys-R-Us gift certificates. Towards the end of our data collection procedures, we increased the show-up fee to $50. We include controls in our regression for any differences that this change in show-up fee may have generated. Other researchers have found that compensating children with money may not hold children's attention as effectively as compensating them with toys. Not only do some children, especially young ones, not fully comprehend the value of money, but children may also fear that their parents will confiscate their earnings or at least partially influence how the money will be spent. All of the children in our sample were familiar with Toys-R-Us store. The use of Toys' R Us gift certificates may not fully eliminate the concern that parents will ultimately influence the children's purchases, but it mitigates it.

In addition to the show up fee, we compensated parents and students for decisions they made during the session. Each decision had tangible, financial consequences. Since we examine several measures, and since the subjects' incomes are low, we reduced possible wealth effects by using a random selection payment mechanism across most of the tasks, including all generosity choices. The random selection payment mechanism is a common experimental economics procedure to control for wealth effects (see Davis and Holt 1993).

Most events were conducted within groups (101 families), although a few sessions were conducted privately with individual families (26 families). In our regression analysis, we control for the type of session that students attended to account for any differences in behavior that may result from these different types of sessions. In the group sessions, we randomly and anonymously chose one or more participants at the end of each session to compensate for each decision. We selected the specific number of participants to compensate for every decision so that within each session a subject's decision for each set of tasks had approximately the same likelihood (about 1 in 5 chance) of being selected for compensation. When conducting individual sessions and at the end of all tasks, we randomly selected one task to compensate the subjects. We informed all subjects of these procedures before they made any decisions.

In the parent room, we administered the generosity tasks followed by a manipulation check and attitudinal survey which is available upon request. In the manipulation check, we asked parents to assess on a scale from 1 (strongly disagree) to 5 (strongly agree) whether they felt that the procedures had preserved anonymity, had confidence that we would pay the money as promised, and whether the instructions were clear and easy to follow. The average responses were 4.63, 4.63, and 4.78 respectively. Only three parents (2.4 percent) gave a response less than 3 for any of these statements and over 90 percent of all responses were 4 or 5.

The specific schedule and order of tasks we ran for each session were identical and were as follows

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Registration
a. Parents and children randomly given identification tags
b. Consent and Accent Forms Provided, Read and Signed

2. Everyone gathered in "central" room
a. Refreshments (fruit, drinks, cookies) available
b. Informal description of where each family member would be located

3. Subjects separated into different rooms where decision-making data were collected

| Decision-Making Events | 1$^{st}$ – 2$^{nd}$ Graders | Higher Grades | Parents | Time (in minutes) |
|---|---|---|---|---|
| 1. Penny jar guessing game (ice-breaker event) | Yes | Yes | Yes | 15 |
| 2. Generosity to Non-profit Organizations | Yes | Yes | Yes | 15 |
| 3. Generosity to Peers | Yes | Yes | Yes | 10 |
| 4. Other experimental data collected for related projects | Yes | Yes | Yes | 30 |
| 5. Standardized achievement Test | Yes | Yes | No | 40 |
| 6. Survey: manipulation checks and attitudinal indicators. Plus informal discussion | No | No | Yes | 15 |

4. Everyone returns to central room
a. Pizza, fruit, cookies and beverages provided
b. Parents and children called one at a time for private payments

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**Appendix 2.** Children's Decision-making sheet for the American Red Cross dictator game.

The Organization you are put together with for this decision is:

**The American Red Cross**

The American Red Cross tries to make people's lives better. It tries to make people's lives better by helping people be prepared for disasters, such as fires, earthquakes and flooding. The American Red Cross also helps people survive after a major disaster by providing food and a place to sleep.

I want to divide the $10 in Toys-R-Us gift certificates as follows (mark one choice only):

| My Choice | KEEP For Myself | SEND To Organization |
|---|---|---|
| | $10 | $0 |
| | $9 | $1 |
| | $8 | $2 |
| | $7 | $3 |
| | $6 | $4 |
| | $5 | $5 |
| | $4 | $6 |
| | $3 | $7 |
| | $2 | $8 |
| | $1 | $9 |
| | $0 | $10 |

Do you want the organization to send you a thank you letter?   No   Yes

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## Table 1
### Personal Characteristics and Voucher Status. CSF 1998 Applicants

| Dependent Variable | Public School Lottery | | Private School Lottery | |
|---|---|---|---|---|
| | Loser's Mean (1) | Diff for Winners (2) | Loser's Mean (3) | Diff for Winners (4) |
| **A. Applicant List** | | | | |
| Income | 21782 (14738) | -2179*** (885) | 25740 (14032) | 2665*** (1168) |
| Household Size | 3.7 (1.4) | -.066 (.093) | 3.7 (1.6) | .284** (.127) |
| Proportion w/ Any Child Previously Attending Private School | .103 (.304) | .013 (.021) | .966 (.182) | .011 (.015) |
| N | 331 | 1126 | 1085 | 1259 |
| **B. Attempted Survey Sample** | | | | |
| Income | 20944 (15094) | 169 (1460) | 25652 (16115) | 1297 (4481) |
| Household Size | 3.7 (1.3) | 0.18 (0.14) | 3.6 (1.2) | 0.01 (0.32) |
| Proportion w/ Any Child Previously Attending Private School | 0.10 (.30) | .037 (.033) | | |
| Survey Response Rate | .61 | .06 (.05) | .72 | -.15 (.14) |
| N | 200 | 390 | 25 | 48 |
| **C. Survey Sample** | | | | |
| Income | 21379 (15025) | 679 (1786) | 24335 (15320) | 961 (4927) |
| Household Size | 3.7 (1.4) | .26 (.18) | 3.6 (1.3) | 0.37 (0.41) |
| Proportion w/ Any Child Previously Attending Private School | .123 (.330) | .025 (.043) | | |
| Single Parent Household | .393 (.491) | .036 (.062) | .500 (.514) | -.115 (.185) |
| One parent is College Grad | .230 (.422) | .067 (.056) | .333 (.485) | -.103 (.167) |
| Oldest Child Attended Private (if Applied) | .400 (.492) | .105 (.070) | .875 (.342) | .125 (.086) |
| African-American | .574 (.045) | -.144** (.063) | .278 (.461) | -.047 (.163) |
| N | 122 | 250 | 18 | 31 |

Standard deviations appear in Columns 1 and 3. Standard errors appear in Columns 2 and 4. Unit of observation is a family applying for the scholarship program. The sample size in Columns 1 and 3 is for the population of unsuccessful lottery applicants. The sample in Columns 2 and 4 is the sample of both winners and losers.

**Table 2**
Children's Characteristics, Event Attendance and Voucher Status

| | Survey Sample Means | | Event No-Show (3) | Diff for Event Attenders (4) | Diff for Winners (w/i Attenders) (5) |
|---|---|---|---|---|---|
| | Loser's Mean (1) | Diff For Winners (2) | | | |
| Age | 10.1 (2.7) | -.11 (.26) | 10.2 (2.9) | -.342 (.263) | .013 (.385) |
| Male | .447 (.498) | .049 (.047) | .446 (.498) | .064 (.047) | .123* (.072) |
| Grade | 4.4 (2.6) | -.02 (.25) | 4.5 (2.6) | -.278 (.251) | .139 (.374) |
| Black | .546 (.499) | -.170*** (.046) | .411 (.493) | .099** (.046) | -.155** (.068) |
| Private Lottery | .110 (.314) | .005 (.029) | .106 (.309) | .014 (.030) | .112** (.044) |
| N | 218 | 471 | 263 | 471 | 208 |

Standard deviations appear in Columns 1 and 3. White standard errors appear in Columns 2, 4 and 5 in parentheses. The unit of observation is the child applying for the scholarship program. (*, **, ***: p<.10, p<.05, p<.01, respectively.)

**Table 3**
Children's Characteristics and Voucher Status. CSF 1998 Applicants

| Dependent Variable | Loser's Mean (1) | Diff for Winners With No Covar (2) | Winners' Diff With Basic Covs (3) |
|---|---|---|---|
| Private School at time of Survey | .208 (.407) | .227*** (.042) | .208*** (.040) |
| Attended Private Since 1997 | .389 (.489) | .261*** (.046) | .243*** (.043) |
| Ever Repeated | .218 (.414) | -.058 (.039) | -.052 (.039) |
| Suspensions | .184 (.389) | -.080** (.034) | -.063** (.031) |
| Math Scores (N=349) | 48.5 (22.7) | .970 (3.37) | .111 (3.53) |
| N | 216 | 466 | 466 |

Standard deviations appear in Column 1. White standard errors appear in Columns 2 and 3. Unit of observation is a child applying for the scholarship program. Covariates include age, gender, race, family income, private school lottery dummy, and session dummies. The math scores include two observations for 163 students who took both parts of a test and 23 observations for students who took only one part of the test in our pilot. (*, **, ***: $p < .10$, $p < .05$, $p < .01$, respectively.)

| | Amount Given to Each Charity | | | Amount Given to Charities (Stacked Regressions) Amount Given To Peers (Stacked Regressions) | | Amount Given To Peers (Stacked Regressions) |
|---|---|---|---|---|---|---|
| | Red Cross | Make-Wish | CSF | | | |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Losers' Mean (SD) | $3.00 (3.00) | $3.73 (3.42) | $3.27 (3.07) | $3.33 (3.17) | | 2.74 (2.79) |
| Voucher Effect | 1.19** (.448) | .515 (.518) | .708 (.497) | .750* (.418) | .779* (.411) | -.112 (.440) |
| Exchange Rate | | | | | | -.221 (.235) |
| Red Cross | | | | -.079 (.200) | -.042 (.207) | |
| Make-A-Wish | | | | .318* (.173) | .381* (.174) | |
| Family Income (In thousands) | .027 (.018) | .005 (.019) | .016 (.017) | .018 (.015) | .017 (.014) | -.007 (.015) |
| Number Children Living at Home | -.202 (.201) | -.474* (.280) | -.439 (.257) | -.422** (.202) | -.378* (.205) | -.100 (.202) |
| Private Lottery | 1.30** (.645) | 1.27* (.667) | .270 (.594) | .942* (.510) | .913* (.512) | .890 (.633) |
| Male | -1.07** (.467) | -.259 (.529) | -.182 (.458) | -.533 (.425) | -.513 (.418) | -.788** (.385) |
| African-American | -.723 (.440) | -.950 (.578) | -.280 (.495) | -.769 (.406) | -.663 (.422) | -.655 (.454) |
| Age | .426*** (.095) | .409*** (.112) | .289*** (.098) | .369*** (.099) | .374*** (.089) | -.033 (.088) |
| Amount Parents Gave (On same decision) | .034* (.018) | .014 (.022) | .008 (.017) | | .018 (.014) | .015 (.019) |
| N | 191 | 190 | 191 | 572 | 572 | 443 |

Notes: Standard errors clustered within families reported in parentheses, session effects not reported.
(*, **, ***: p<.10, p< .05, p<.01, respectively.)

## Table 4b
### Alternate Specification of Voucher Effects on Children's Generosity

| | Amount Given to Charities (Stacked Regressions) | | Amount Given To Peers (Stacked Regressions) | |
|---|---|---|---|---|
| | (1) O. Probit | (2) Tobit | (3) O. Probit | (4) Tobit |
| Losers' Mean (SD) | $3.33 (3.17) | | 2.74 (2.79) | |
| Voucher Effect | .272* (.157) | 1.16** (.492) | .013 (.184) | .207 (.573) |
| Exchange Rate | | | -.034 (.100) | -.275 (.275) |
| Red Cross | -.039 (.078) | -.078 (.228) | | |
| Make-A-Wish | .110* (.064) | .516** (.227) | | |
| Family Income (In thousands) | .004 (.005) | .010 (.017) | -.003 (.006) | -.005 (.020) |
| Number Children Living at Home | -.101 (.073) | -.055 (.267) | -.034 (.086) | .113 (.293) |
| Private Lottery | .360** (.177) | .212 (.586) | .310 (.270) | 1.15 (.941) |
| Male | -.161 (.156) | -1.36** (.588) | -.324** (.154) | -1.58** (.687) |
| African-American | -.219 (.154) | -1.26*** (.422) | -.316 (.208) | -1.47** (.715) |
| Age | .144*** (.032) | .477*** (.113) | .001 (.033) | -.058 (.093) |
| Amount Parents Give (On same decision) | .007 (.005) | .054 (.054) | .026 (.039) | -.051 (.102) |
| N | 572 | 572 | 443 | 443 |

Notes: Ordered Probit controls for correlation with families. Tobit includes effects for children, session effects not reported. Tobit regressions censored at giving $0 and $10. (*, **, ***: p<.10, p< .05, p<.01, respectively.)

## Table 5
## Voucher Effects on Parents' Generosity

| | Amount Given to Charities (Stacked Regressions) | | | Amount Given To Peers (Stacked Regressions) | | |
|---|---|---|---|---|---|---|
| | (1) OLS | (2) O. Probit | (3) Tobit | (4) OLS | (5) O. Probit | (6) Tobit |
| Losers' Mean (SD) | $32.37 (16.49) | | | $20.93 (7.86) | | |
| Voucher Effect | -1.71 (2.83) | -.116 (.127) | .950 (1.85) | .458 (1.52) | .077 (.123) | .847 (.727) |
| Exchange Rate | | | | -6.11*** (.553) | -1.20 (.148) | -6.20*** (.527) |
| Red Cross | -2.09** (1.01) | -.182 (.141) | -3.31** (1.34) | | | |
| Make-A-Wish | .323 (1.01) | .008 (.143) | .609 (1.36) | | | |
| Family Income (In thousands) | .094* (.134) | .007 (.006) | .021 (.083) | .087 (.072) | .014 (.006) | .132*** (.032) |
| Number Children Living at Home | -2.70 (1.57) | -.193*** (.070) | -3.88*** (.847) | .572 (.845) | .036 (.068) | .976** (.408) |
| Private Lottery | -3.45 (4.83) | -.303 (.213) | -4.80* (2.60) | 2.61 (2.59) | .418 (.209) | .536 (1.03) |
| Male | -3.62 (3.90) | -.230 (.174) | -10.48*** (2.77) | -1.13 (2.09) | -.137 (.168) | -1.35 (.956) |
| African-American | -4.71 (3.05) | -.376*** (.138) | -11.11*** (2.18) | 3.29** (1.64) | .389 (.132) | 5.69*** (.817) |
| Religion | -1.78 (5.09) | -.130 (.233) | 5.12 (4.43) | -.038 (2.73) | -.045 (.219) | .105 (1.12) |
| N | 333 | 333 | 333 | 333 | 333 | 333 |

Notes: Heteroskedasticity corrected standard errors in parentheses, session effects not reported. Tobit regressions censored at giving $0 and $50. (*, **, ***: $p < .10$, $p < .05$, $p < .01$, respectively.)