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School Costs, Short-Run Participation, and Long-Run Outcomes

Evidence from Kenya

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Abstract

Access to school has risen dramatically in recent decades, with large gains from reducing costs. Few studies report long-term impacts, however. This paper reports the impact of an educational intervention that reduced out-of-pocket schooling costs for children in poor communities in Kenya by providing school uniforms. The program used a lottery to determine who would receive a school uniform. Receiving a uniform reduced school absenteeism by 37 percent for the average student (7 percentage points) and by 55 percent for children who initially had no uniform (15 percentage points). Eight years after the program began, there is no evidence of sustained impact of the program on highest grade completed or primary school completion rates. A bounding exercise suggests no substantive positive, long-term impacts. These results contribute to a small literature that demonstrates the risk of fade-out of initial impacts of education investments.

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School Costs, Short-Run Participation, and Long-Run Outcomes: Evidence from Kenya

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1. Introduction

This paper takes advantage of a free uniform distribution program in Kenya in order to examine the impact of reducing the out-of-pocket cost of education on short run school attendance and longer run educational outcomes. We study a child sponsorship program that used a lottery to assign sponsorships to students in 12 study schools. Although not all winners of the lottery received uniforms, winning the lottery is highly correlated with receipt of a uniform and thus serves as an effective instrumental variable (IV). We find that receiving a uniform increases school attendance by 7.0 percentage points, which amounts to a 37 percent reduction in absenteeism. For children who did not already own a uniform, distributing a uniform reduces absenteeism by 55 percent. We present results of the program eight years after its inception and find no consistent pattern of long-term impacts, although the standard errors on long-term estimates are large, so the estimates are imprecise. However, a bounding exercise suggests that we can rule out large long-term positive impacts.

These results provide direct evidence of the impact of eliminating non-tuition user fees. User fees are a common feature of education in developing countries. Formal fees have now been abolished in many countries, but other significant costs remain, such as those associated with providing school uniforms and textbooks. These costs can be considerable. In Sierra Leone, for instance, the price of uniforms is as much as levied school fees (World Bank 2007). In Nigeria, where formal fees are no longer levied, books and uniforms cost 2.5 times more than official fees did before their elimination (Lincove, 2009). These non-tuition user fees are reported to have significant negative effects on enrollment. In Malawi and Uganda, where fees were abolished over a decade ago, half the households with children who have dropped out still cite lack of money as the main problem (UNESCO 2010).

Existing evidence suggests that reducing the cost of schooling can increase school participation. Abolishing official school fees has been associated with huge surges in enrollment in Uganda (Deininger

2003), Kenya (Lucas & Mbiti 2012), Malawi (Al-Samarrai & Zaman 2007), and Nigeria (Lincove, 2009). Kremer et al. (2003) evaluate a previous child sponsorship program in which a non-governmental organization provided a package including uniforms, textbooks, and classroom construction to a set of randomly selected schools. In the treatment schools, dropout rates fell dramatically, and after five years pupils had completed 15 percent more schooling; at the same time, class sizes increased greatly, between 14 percent and 30 percent, depending on when the effect is measured. The authors hypothesize that the bulk of the impacts come from the uniforms, as another evaluation in the same area provided textbooks with no impact on dropout rates (Glewwe et al. 2009), and dropout rates fell significantly before classroom construction took place. In a narrower intervention, providing two school uniforms over the course of 18 months, Duflo et al. (2015) find that providing uniforms to school girls reduces dropout rates. The pupils studied in Duflo et al. (2015) are in grade 6, on average 3 years older than the pupils in our study.

Wydick et al. (2013) study the impact of child sponsorship and find large, statistically significant impacts on years of schooling; primary, secondary, and tertiary school completion; and the probability and quality of employment. The authors attribute these impacts – in part – to increases in children's aspirations. In a follow-up study, Wydick et al (2016) estimating the impact of child sponsorship on adult income and wealth of formerly sponsored children find that child sponsorship increased monthly income by 13–17 percentage points over an untreated baseline of 75, principally from inducing higher future labor market participation.

In the limited literature on reducing costs via uniform distribution, Hidalgo et al. (2013) find that a free school uniform program had a negative impact on attendance in Ecuador – a higher income environment than Kenya – using school-level randomized assignment of free uniforms. In that context, uniforms failed to materialize in more than one-third of schools where those had been announced, so

some parents may have been disaffected by the unfulfilled promise of a free uniform. Alternatively, a quasi-experimental analysis of a government school uniform distribution program in India found heterogeneous impacts, with positive effects for girls and historically-disadvantaged castes but negative effects for wealthier students (Adukia 2014). This further supports the idea that free uniforms may improve outcomes for the most disadvantaged students.

These results also contribute to a wider literature demonstrating fade-out of educational intervention impacts. A recent review of randomized evaluations of educational interventions demonstrated that most evaluations examine impacts after one year or less (McEwan 2015). Among evaluations that have examined longer term impacts, some show fade-out. Andrabi et al. (2011) find in Pakistan that only a fifth to a half of learning persists between grades, and Jacob et al. (2010) similarly find that certain kinds of learning gains largely dissipate within a year or so in the United States. While the program we study examines the early years of primary education, the same has been observed in early child education programs, such as the Perry Pre-school Program and Head Start – have demonstrated short-run cognitive impacts that may diminish in the long run (Schweinhart et al., 2005; Puma et al., 2010). Despite evidence of fade-out of some cognitive outcomes with those programs, other positive benefits persist: participants in these programs show improvement in a range of other outcomes, including high school completion, college attendance, reduced incarceration, and increased earnings (Garces, Thomas, and Currie 2002; Heckman et al. 2010). In another example of persistent effects, Chetty et al. (2011) find that children in smaller classes outperform others on a range of adult outcomes, including college performance.

In our study, to identify children for sponsorship, the implementing NGO first prioritized orphans. If there were remaining sponsorships, it used a lottery to determine which non-orphans would receive sponsorships and consequently a free uniform. Although not all winners of the lottery received

uniforms, winning the lottery is highly correlated with receipt of a uniform and thus serves as an effective instrumental variable (IV). Since schools in Kenya already use uniforms, we are not measuring the effect of a student uniform policy per se, but rather the impact of paying for the uniforms. Our results are thus less relevant to the debate over school uniforms in developed country contexts, which focuses on the impact of uniform policies on reducing aggressive behaviors.[2]

The rest of the paper is organized as follows. Following a description of the context of the experiment in Section 2, Section 3 describes the data and the empirical strategy. Section 4 presents our results, and finally Section 5 discusses some policy implications of our results and concludes.

2. Context

2.1 Costs of schooling

In a range of countries, parents face many costs of education such as school fees and provision of uniforms. In Kenya, students were required to pay school fees to attend primary school through 2002. In January 2003, a new government policy eliminated fees and provided free textbooks and notebooks to schools, which led to dramatic increases in school participation. However, uniforms remained a major cost of schooling. Kenyan schools each have their own uniform.

Historically, students who did not pay their school fees or those who did not wear uniforms could be sent away from school. Whether they were sent away and for how long varied greatly at the discretion of the school's headmaster. Students would often not pay full fees at the beginning of the year and would fulfill these obligations as the year progressed. Over the last 15 years, several prominent officials in the

[2] Brunsma & Rockquemore (1998) examine cross sectional tenth-grade students from the United States, controlling for observable characteristics, and find no impact on behavioral problems or attendance. In other work, Brunsma (2006) uses similar methods to look at school climate, academic achievement, and attendance using national U.S. data for kindergarten, first grade, and eighth grade, and finds no significant impacts. Other work finds positive relationships between uniforms and school climate in two South Carolina middle schools (Murray 1997) and on academic achievement (but without controlling for confounding factors) (Bodine 2003). Recent work in the U.S. finds some evidence for positive impacts on student attendance and teacher retention (Gentile & Imberman 2012).

Kenyan government have voiced that head schoolteachers should not dismiss children who fail to wear a school uniform. However, it is difficult to find a clear expression of the official policy. Anecdotal evidence suggests that students were less likely to be sent away from school for failure to wear a uniform after 2002 than previously, but that students still feel stigmatized by the failure to wear a uniform and may be reprimanded by teachers. For example, recent news items have highlighted that parents are unable to send students to school for lack of funds to pay for uniforms (Kapchanga 2014) or that school uniforms in Kenya are "de rigueur" (Kantai 2014).

The school uniforms provided in the project currently being studied cost between 325 and 550 Kenyan shillings (US$4.15 to $7.03) for girls and between 405 and 550 (US$5.18 to $7.03) for boys.[3] The variation in prices is because uniforms for each school require different materials and also because tailors local to each school were contracted to sew that school's uniforms.

2.2 The Project

This analysis was possible in the context of an existing development project not designed by the authors. International Christelijk Steunfonds Africa (ICS), a Dutch non-governmental organization, operated a child sponsorship program (CSP). Like many such programs, it provided benefits to the school and the community at large, rather than focusing resources only on sponsored children. The principal benefit targeted specifically to sponsored children was a free school uniform each year. In 2012, new schools entered this program. Within each school, a randomly selected sub-set of children received the benefits of individual sponsorship. This paper evaluates the impacts of those individually-targeted benefits.

[3] The exchange rate is from January 2002, when uniforms were purchased, and is in 2002 dollars.

School-level benefits: The schools in which CSP operated benefited from the program by receiving visits from nurses, agricultural extension officers and grants for classroom construction, desks, and books. Specifically, a pair of ICS nurses visited each school several times a year and provided basic first aid to any child (sponsored or not) or local adult who requested it. An agricultural representative organized student clubs to grow crops on the school grounds. Upon inception into the program, each school received a sizeable grant for classroom construction, for desks, and for books. These benefits were school-wide and not restricted to individual students.

Child-level benefits: Certain children within schools were selected for sponsorship. (The selection process is described below.) The standard individual benefit that sponsored children received was a school uniform; in June 2002, uniforms were distributed to all sponsored children who were still in school. 932 uniforms were distributed. Sponsored children went on to receive a uniform in June/July of 2003 and 2004 as well.

In addition, sponsored children had their photo taken and sent to sponsors upon enrollment. Some sponsored children received additional letters or gifts from sponsors, but this was a small fraction. During the first two years of the program, only 6.5 percent of sponsored children received any communication from their sponsors; and only three percent of sponsored children received gifts.[4] The infrequency of these gifts suggests that the bulk of this program's impact stems from the uniform provision and not from these idiosyncratic gifts, although we cannot rule out the possible psychological impact of simply being identified for sponsorship by a donor organization.

[4] The most common items sent were cards or letters (45 percent of children who received anything received only a letter, card, photograph, or drawing), and only a handful of children received anything larger than a pencil set or some exercise books (e.g., six children received a mattress, and seven children received a lantern). Letters, when received, commonly inquired about the child's school progress and other pleasantries. Again, only a small fraction of sponsored children received any communication from their sponsors in the first two years of the program. While one popular account of letter exchange in Zimbabwe indicates large impacts, but these were accompanied by major financial support and – ultimately – an invitation to emigrate to the United States (Alifirenka, Ganda, and Welch 2015).

2.3 Selection of schools and program participants

In fall 2001, ICS selected 12 primary schools in Western Kenya to participate in the CSP. The 12 primary schools are all in Busia district, the westernmost district of Kenya's Western province. Busia district borders Uganda and is located just north of Lake Victoria. Through a qualitative process of local consultation, the NGO identified schools of particular vulnerability within the area.

In January 2002, ICS organized a census of children in standards one through four of the 12 selected schools. Based on that census, ICS selected all children who had experienced one or both parent deaths (i.e., orphans) to automatically receive sponsorships.[5] Therefore out of the 876 sponsorships ICS had to allocate, 309 were assigned to orphans after the initial census. ICS then used a lottery to randomly select the remaining beneficiaries. All the remaining (i.e., non-orphan) students were automatically enrolled in the lottery. Since there were only a fixed number of sponsorships available, ICS determined that a lottery would be the fairest way to distribute the remaining sponsorships among equally needy children. The result was that 550 non-orphans were randomly chosen for sponsorship and 602 non-orphans were not chosen for sponsorship. Since sponsorships were not randomly allocated to the orphans, the effects of those sponsorships are not included in this analysis. Thus, the 550 non-orphans chosen were the group intended for treatment; and the 602 not chosen were intended as a comparison group. This is summarized in Figure 1 (Stage 1).

Next, a field representative from ICS went to the 12 schools to enroll those children selected for sponsorship into the program. For enrollment in the program, a child had to be present for a photograph to be taken and a small information card to be filled in, which would then be sent to the sponsor (with some basic information about the child, such as her preferred pastimes). ICS made efforts to ensure that

[5] Children are referred to as "orphans" if they have either experienced one parent death or both parent deaths.

sponsored children were in attendance on the day of enrollment. Schools and parents varied in their compliance. If a child intended for treatment was not present, then a replacement was selected from a list. If that replacement was not present, another was chosen. Because of that, some children who won the lottery for child sponsorship were ultimately assigned to the comparison group and vice-versa. ICS registered 809 children into the program. As a result, we use treatment assignment as an instrumental variable to estimate a local average treatment effect (LATE). The LATE parameter measures the average effect of treatment on compliers, i.e., those students who received a uniform in 2002 because they were present on the day of the lottery and were randomized into the program.

Table 1 presents the first-stage results: what is the probability that a child, having been randomized into the project, was actually registered or went on to receive a uniform? Table 1 indicates that 77 percent of children who won the lottery were actually registered as part of the child sponsorship program. Seventy-four percent of children who won the lottery went on to receive uniforms in June of that year. Being randomized into the program thus acts as an appropriate instrumental variable for actual enrollment and later uniform receipt.

3. Data and Empirical strategy

The final data set we use comprises six smaller survey and administrative data sets with information about (1) students who won the sponsorship lottery in 2002, (2) students who were present in school on the date of enrollment in 2002, (3) a pupil questionnaire administered in 2002, (4) students who received uniforms in 2002, (5) pupil attendance from 2002 to 2006, and (6) a tracking questionnaire administered in 2010. Figure 1 demonstrates the relationship between the Pupil Attendance data set and the other data sets. We have attendance information for each of the 12 schools from 2002 through the end of 2005. Attendance was gathered as field officers made unannounced visits to each school multiple

times each year and recorded whether each child was present. From these multiple visits, an annual per-child attendance average is collected. The 2002 Pupil Questionnaire was carried out in mid-January 2002 before uniforms were distributed, and socioeconomic data were gathered on all children who were present at school that day.

The Pupil Attendance data set has uniquely defined pupil identification numbers, and the 2002 Pupil Questionnaire and the 2010 follow-up survey use those same numbers. However, the data sets with information about the students who won the lottery, those who were present on the day of the lottery and those who received uniforms do not include those pupil identification numbers. Therefore, names were matched manually and by computer. Some pupils in the various other data sets did not match any pupil in the Pupil Attendance data set. This may stem from the fact that sometimes children have several names and may only give a subset of those in one data gathering exercise and a non-overlapping subset in another exercise. It is also common for students to spell their names differently in different contexts and to change their names, further challenging the matching process. In total, we matched attendance data for 805 pupils out of the 932 pupils who had been fitted for uniforms (86 percent). In 2010, eight years after initial uniform distribution, we collected follow-up information on the students in our sample. Field officers were sent to the 12 program schools with a roster of students from the start of the program. They asked head teachers, teachers and older students whether or not the student had finished primary school, whether they were still in school and the highest grade completed. Field Officers also examined official Ministry of Education Records at the school to get results from the Kenya Certificate of Primary Education (KCPE) exam, which students take at the end of 8^{th} grade.

Overall, the tracking rate was high; we were able to collect information for about 95 percent of the students in our sample. Since the students usually live within close proximity of the school, in many cases teachers and older students knew what had happened to the students. School officials reported that

371 students (32 percent of the original sample) had completed Standard 8 and thus taken the KCPE Exam. KCPE results from 2005 through 2009 were copied from official statistics from the Ministry of Education. We matched 328 out of these 371 students (88 percent) of pupils with KCPE results from the MoE. We examined KCPE scores with the same school officials – at the school – who had helped us track the students, to minimize any errors due to a failure to match children's names.

Our principal regression sample consists of 1,152 children who are identified in the initial Randomization data set, the Recipient 2002 data set, and the Pupil Attendance data set. Table 2 compares children who won the lottery to those who did not across a set of basic characteristics – gender, age, school dummies, and age dummies, for which data are available for virtually all the children. There are no significant differences across any of these characteristics. For a smaller sample, the subset of children who were present when enumerators arrived at school for the baseline census, we have a wide array of variables. Table 3 Panel A compares children who won the lottery to children who did not win the lottery for the 780 children in school on the day of the 2002 Pupil Questionnaire. Even in this smaller, more selected sample, we observe few significant differences (e.g., winners have slightly fewer meals at home per day, have fewer pens, were slightly more likely to have breathing trouble, and were less likely to cook regularly at home or work on the farm). Table 3 Panel B compares children who received uniforms in June 2002 to those who did not, again for the 780 children in school on the day of the questionnaire. We see few significant differences – uniform recipients are slightly less likely to be female, have slightly higher pre-program attendance, and eat slightly fewer meals at home per day than non-recipients. There are no discernable patterns of difference between groups. Once we include school-grade-gender fixed effects, treatment and comparison groups (both intended and actual) are balanced in advance of the program (results not reported). Eighty percent of children registered in the program received uniforms; 78 percent of children who received uniforms had initially won the lottery.

In our main identification strategy, we use random assignment into the treatment group as an instrument for treatment (or receiving a sponsorship), measuring the local average treatment effect (LATE). Since there was some crossover from the comparison to the treatment group, the intent-to-treat strategy – in which groups are analyzed purely based on their initial assignment to treatment or comparison – would likely understate the true impacts. The initial randomization serves as an ideal instrument since it was randomly determined, thereby giving no reason to expect it would impact the outcome except through affecting the likelihood of treatment. Also, since the majority of children who were chosen for treatment actually received uniforms, the first stage is strong.

We estimate the equation below:

$$Outcome_{ij} = \alpha + \beta_1 Uniform_{ij} + \beta_2 No\_Uniform\_Before_{ij} + \rho\ School\text{-}Grade\text{-}Gender_j + \varepsilon_{ij}$$

where $outcome_{ij}$ is attendance (in the short-run) or other outcomes (in the longer run) for student i in school-grade-gender cell j. $Uniform_{ij}$ is an indicator for whether student i in school-grade-gender cell j received a uniform, and this is instrumented with random assignment into the sponsorship program. We control for whether or not the student had a uniform at the baseline ($No\_Uniform\_Before_{ij}$), and school-grade-gender fixed effects to account for any differences across groups of students not driven by the program.

Although we sometimes refer to the impact of sponsorship as the impact of receiving a uniform, sponsorship could potentially work through two other mechanisms. First, sponsored children had their picture taken and were singled out, and that kind of attention could conceivably have self-esteem impacts that could affect school attendance.[6] This could work together with the aforementioned moral support offered by sponsors to a few students. Our current estimates cannot differentiate between these effects.

[6] This fits with a small literature on how the framing of education benefits can affect their take-up. In Cambodia, when similar children received a scholarship labeled either as "poverty-based" or "merit-based," those receiving the "merit-based" scholarship went on to exert more effort (Barrera-Osorio and Filmer 2015).

However, those effects would be expected to be largest immediately after initiation of sponsorship, and we do estimate separately the impacts of sponsorship in the six months between registration and initial uniform receipt and subsequent impacts. Significant effects only appear after uniform receipt, suggesting that sponsorship alone is not driving these effects.

4. Results

This section presents our empirical results of the effects of uniform provision on school attendance, test scores and long-run outcomes for the students in the program.

4.1 School Attendance

In Table 4 Regression 1, we examine whether initial registration into the program had any effect on children's school attendance in the six months before distribution of uniforms. We see an insignificant positive impact on attendance. This mildly positive but insignificant impact of being registered in the program before the students receive uniforms might be a measure of the impact of the students' expectations of program benefits. While this anticipatory effect of the program is positive, the effect of the program is only statistically significant after uniforms are distributed in June 2002 as shown in Regressions 2-4. In Regression 2, we provide the intent-to-treat regression, measuring the simple impact of being randomized into the project on attendance after uniform distribution, and see an effect of 3.8 percentage points on school attendance. The IV estimates of actually receiving a uniform in 2002 (Regression 3) on attendance after uniform distribution is 7.0 percentage points (standard error 2.5 percentage points). This constitutes a reduction in absenteeism of about 37 percent from a baseline level of 18.8 percent. (In Table 4 Regression 4, we provide an OLS estimate of receiving a uniform on school attendance, which is of the same order of magnitude as the IV estimate.)

The impact of the program may be larger for children who are traditionally at higher risk of dropping out – young children, girls, and poorer children. Regression 1 of Table 5 shows an impact of 4.1 percentage points for boys and a larger (but not significantly different) effect of 10.9 percentage points for girls. Table 5 Regression 2 examines the effects for younger children (aged 5-9) versus older children (aged 10-14). We observe an effect of 11 percentage points for younger children who received a uniform in 2002 (but again, not significantly different) relative to 4.7 percentage points for older children.

We would expect receiving a uniform to be most important to children who do not already have one. We include an interaction for students who both receive a uniform but did not have a uniform at baseline (Table 5 Regression 3), and we find that the attendance of students who receive a uniform and did not already have a uniform is estimated at 9.8 percentage points higher than those who already had a uniform (i.e., the total effect for those students is 14.6 percentage points, significant with 95 percent confidence), but that this difference is not statistically significant.[7] While these higher point estimates for more vulnerable students are suggestive, the relatively small sample means that we cannot rule out equality of impacts.

We examine the effect of receiving a uniform on test scores in the first three years of the program. The data were obtained by collecting test scores from all the study schools for these three years and matching those records to our sample. While we observe a pattern of insignificant positive effects, our match rate is low (between 30 percent and 50 percent, depending on the year). We also test for program impacts on student transfer – in case the program keeps participants from transferring to better schools, for example – and find statistically insignificant impacts.[8] The positive impact on attendance together

[7] We also examine the effect of the program on grade progression and find no significant effect (results not reported).
[8] The intent-to-treat estimate is -0.035 (s.e. 0.024), and the instrumental variables estimate is -0.063 (s.e. 0.045).

with an insignificant impact on learning is consistent with other work in Kenya (Miguel and Kremer 2004) as well as work elsewhere in the world (Filmer and Schady 2014).

4.2 Long-run Effects

We collected follow-up data from the students in the sample in early 2010, eight years after the start of the program, to see whether the program had longer-term effects on the students' education: whether or not they had completed primary school, their highest level of education, whether or not they were still in school, and their performance on the KCPE.

Table 6 demonstrates that those students who were randomized into treatment are slightly more likely to have school officials identify the highest level of education attained by the student in 2010, whether the student is still in school in 2010, and whether the student participated in the Kenya Certificate of Primary Education exam. This biased attrition is itself an outcome, suggesting that the program led the children to be more known to school officials. This unbalanced attrition in long-term follow-up could affect the results; we seek to address this using Lee bounds.

Table 7 Panel A displays three estimates on long-term outcomes: the intent-to-treat regression – measuring the simple impact of being randomized into the project, the IV estimate of actually receiving a uniform in 2002, and the simple OLS estimate of uniform receipt on long term outcomes. It includes Lee bounds on the intent-to-treat estimates for those outcomes with biased attrition (Lee 2009; Tauchmann 2014). The bounding exercise assumes that attrition is monotonic: in other words, any subject who would not attrit if assigned to the control group, would also not attrit if assigned to the treatment group. We think that this is a reasonable assumption since students who received uniforms attended more school and thus may be more memorable to the school officials interviewed. The results show that on aggregate there was no significant program impact on any of the long-term outcomes. The

ITT (and IV) estimates (where the units are probabilities) are -0.036 (-0.066) for primary school completion, -0.069 (-0.126) for highest level of education, 0.005 (0.008) for still in school, and -0.014 (-0.025) for participation in the KCPE exam, and 0.024 (0.039) for KCPE score. While a 95 percent confidence interval includes zero, three of the five point estimates are negative and the other two are small. For primary school completion, the upper bound is still relatively low: 0.026 for the IV estimate of primary school completion. In fact, the Lee bounds rule out sizeable positive impacts in the long-run. Our results are consistent with the fade-out of cognitive gains observed in some other studies (Puma et al. 2010; Schweinhart et al. 2005), including others that – like this one – take place after the earliest years of childhood have passed (Andrabi et al. 2011; Jacob et al. 2010).

Note also that the OLS estimates are biased upwards for four out of five outcomes. (The one exception has a very small sample size.) This serves as a further reminder that observational associations between school inputs – such as uniforms – are likely positively correlated with unmeasured parental investments that positively impact educational outcomes, leading to unreliably optimistic OLS estimates.

Panel B displays estimates for students who did not have a uniform at baseline – those with the largest point estimates in the short run – and similarly do not reveal any significant program impact. Furthermore, the point estimates are not systematically larger or smaller than those for the full sample. Larger standard errors for this smaller sample make it more difficult to rule out large positive or negative impacts.

Hierarchical Linear Models allow us to examine whether impacts vary across schools within the sample (Hamilton 2012); specifically, HLMs allow random effects where intercepts and slopes can vary across subgroups of the sample. While the results reveal statistically significant school-to-school variation in coefficients, the magnitude of these differences is small, at zero in the hundredths place for

all long-run outcomes (Table 8). This suggests that the lack of program impacts is consistent across the sample.

5. Discussion

In this paper, we report the impact of a program that reduces out-of-pocket costs of schooling through the distribution of free uniforms. As almost all schools globally have turned to a policy of officially free primary education, incidental expenses – including uniforms in Kenya and many other countries in Sub-Saharan Africa – become the primary out-of-pocket expense. Indeed, in Kenya, the cost of a uniform is the highest monetary outlay for primary school with the abolition of general school fees. We find that distributing uniforms results in a 37 percent reduction in student absenteeism (7 percentage points). For children who did not already own a uniform, distributing a uniform reduces absenteeism by 55 percent. Yet we observe no long-term effects on the likelihood of completing primary school or on the number of years of education completed.

In terms of cost-effectiveness, the treatment effect of the program on the treated is an increase in school participation of 0.070 years per treated child (standard error 0.026 – Table 4 Column 3). The average cost of a school uniform was 436.86 Kenyan shillings (US$5.82). Thus, the cost of increasing school by one year is $5.82 / 0.065, or US$89.54.[9] This is still considerably more than the cost of an additional year of schooling through providing deworming medication (US$2.92), an intervention also carried out in the same geographic area (Miguel and Kremer 2004; Miguel, Kremer, and Hamory Hicks 2015). However, since uniform provision constitutes a transfer to the household, a more appropriate

[9] The marginal cost of labor associated with providing an additional uniform is very small. Field officers for the non-government organization in Kenya make approximately US$350 per month, but the fraction of that time required for distributing uniforms (one day per school for measuring, one day per school for distributing) is very low if one imagines a program that distributes uniforms annually.

comparison of the cost effectiveness of uniform provision might be to cash transfer programs.[10] The per pupil cost of inducing an additional year of schooling with the conditional cash transfer program PROGRESA (subsequently *Oportunidades* and *Prospera*) in Mexico was $3,333, and the cost was $1,429 for a conditional cash transfer program in Malawi (Dhaliwal et al. 2013). Both of these are significantly higher than the cost of an additional year of schooling through uniform provision. Thus, in some contexts, uniforms may be an effective way to boost short-term educational participation. But these results suggest that such efforts may have limited impact over the long run.

To place our results in the context of other results of uniform distribution programs, Hidalgo et al. (2013) examine the impact of providing uniforms on attendance in Ecuador using school-level randomized assignment of free uniforms. They find that announcing a free school uniform program had a negative impact on attendance. School uniforms were distributed in only 63 percent of the schools that were told that they would get them, thus the authors hypothesize that this negative impact could have been generated by creating false expectations of free distribution, or also by a sunk cost effect – that is, those in the control group, who pay for their children's school uniforms, may feel more committed to the school than parents whose children get the uniforms for free (the treated) and therefore do not allow their children to miss classes easily. Significant differences in institutional environments between Kenya and Ecuador might explain the divergence in results. Ecuador is wealthier. The net enrollment of children in primary school in Ecuador is about 95 percent, while in Kenya it is about 82 percent (World Bank 2014).[11] The value of a free uniform on a marginal parent's decision to send the child to school might be weightier in Kenya. Two additional potential explanations could be either negative peer effects from

[10] Since children need clothing, one potential spill-over from a uniform transfer program might be to younger siblings, as the initial recipients grow out of their donated uniform. We find no significant impacts on school attendance for younger siblings of beneficiaries (results not reported).

[11] This uses the most recent year of available data for each country, 2009 for Kenya and 2012 for Ecuador. (Ecuador's net enrollment rate in 2009 was very similar to its rate in 2012.)

having worse peers drawn into (and staying in) school or capable students having new incentives to remain in low-quality schools. A quasi-experimental analysis of a government school uniform distribution program in India found heterogeneous impacts, with positive effects for girls and historically-disadvantaged castes but negative effects for wealthier students (Adukia 2014). This further supports the idea that free uniforms may improve outcomes for the most disadvantaged students.

These results reinforce the importance of collecting long-term follow-up data. This is particularly important given that the average impact evaluation of an instructional intervention gathers its follow-up data after 13 months of treatment and less than a month after treatment has concluded (McEwan 2015): In other words, very few evaluations explore the long-term impacts, but this is crucial to understanding the true return on educational interventions.

Future systematic reviews should take care when interpreting short-term results, taking into account these results and others which demonstrate that long-term impacts may vary – sometimes dramatically – from initial effects. Gathering long-term data is costly, but without it, the trajectory of impacts resulting from the wide range of interventions currently being implemented remains a mystery. Most education interventions have the ultimate objective of improved long-term well-being – such as more learning, more completed schooling, or better labor market outcomes – and short-term educational improvements are merely a hopeful proxy. As more education studies gather long-term data, an essential understanding of the relationship between short-term and long-term gains may illuminate which investments truly change children's life trajectories.

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Figure 1: Relationship of various datasets to the attendance data

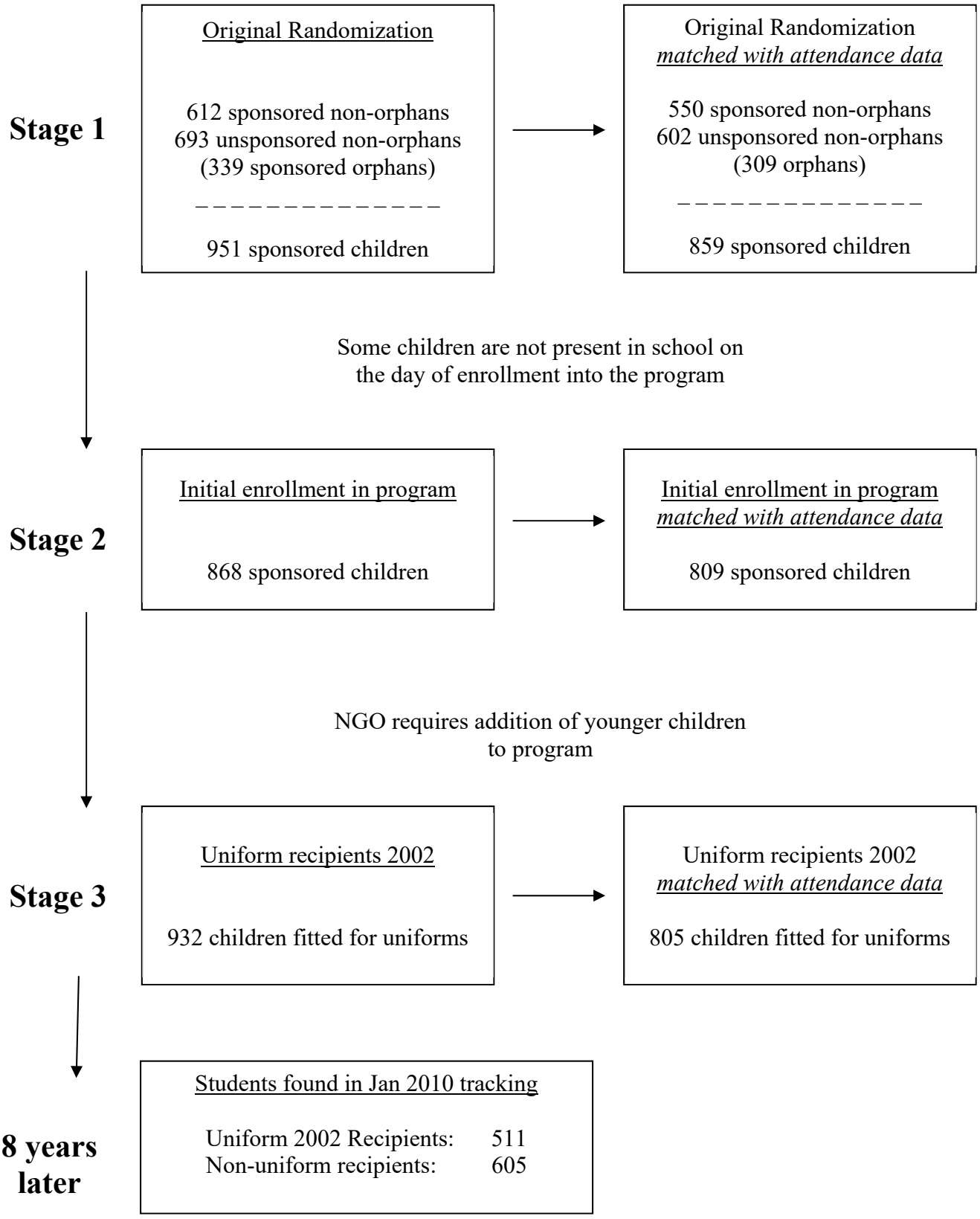

Table 1: First stage regressions

	Dependent variable:	
	Initially registered in program	Received uniform 2002
	(1)	(2)
Randomized into program	0.565***	0.541***
	(0.024)	(0.024)
Mean among Controls	0.193	0.184
Observations	1,152	1,152
R-squared	0.417	0.398
F-Statistic		49.76

Notes: All regressions include a full set of gender-school-standard fixed effects and controls for whether or not the child had a uniform previous to uniform distribution. * indicates significant with 90% confidence, ** 95%, *** 99%.

Table 2: Pre-treatment Comparison of Winners and Losers of the Lottery

	Winners	Losers	Difference (W-L)	Std Error For Diff
Female	0.462	0.477	-0.015	0.029
Age	9.559	9.669	-0.11	0.127
School 1	0.087	0.081	0.006	0.016
School 2	0.076	0.076	0	0.016
School 3	0.033	0.037	-0.004	0.011
School 4	0.127	0.13	-0.002	0.02
School 5	0.096	0.091	0.005	0.017
School 6	0.091	0.088	0.003	0.017
School 7	0.08	0.081	-0.001	0.016
School 8	0.105	0.101	0.004	0.018
School 9	0.078	0.073	0.005	0.016
School 10	0.084	0.08	0.004	0.016
School 11	0.065	0.065	0.001	0.015
School 12	0.076	0.096	-0.02	0.017
Grade 1	0.335	0.307	0.027	0.028
Grade 2	0.291	0.272	0.018	0.027
Grade 3	0.236	0.251	-0.014	0.025
Grade 4	0.138	0.169	-0.031	0.021

Note: These summary statistics are for the full sample of 1209 students

Table 3 Panel A: Pre-treatment Comparison of Winners and Losers of the Lottery

	Winners	Losers	Difference (W-L)	Std Error For Diff
Demographics and Home Characteristics				
Female	0.47	0.47	0.00	0.04
Age	9.6	9.73	-0.13	0.14
School attendance (Jan - June '02)	0.86	0.85	0.01	0.02
Kids in Same Compound	2.31	2.02	0.29	0.28
Iron roof	0.4	0.4	-0.01	0.04
Toilet	0.85	0.86	-0.01	0.03
Meals per Day	2.22	2.29	-0.07*	0.04
School Requirements				
Pens	0.53	0.65	-0.12**	0.06
Pencils	1.23	1.26	-0.03	0.07
Books	0.21	0.21	0.00	0.05
Exercise books	4.92	5.04	-0.12	0.18
Bag	1.02	1.06	-0.04	0.05
Uniform shirts	0.77	0.8	-0.03	0.06
Uniform dresses	0.8	0.71	0.09	0.06
Self-Reported Health				
Headache	0.31	0.27	0.03	0.03
Flu	0.24	0.21	0.03	0.03
Coughing	0.17	0.17	0.00	0.03
Breathing	0.05	0.03	0.03**	0.01
Stomachache	0.25	0.21	0.04	0.03
Vomiting	0.09	0.07	0.02	0.02
Diarrhea	0.07	0.06	0.01	0.02
Work at Home				
Cook	0.56	0.62	-0.06*	0.04
Fetch water	0.95	0.94	0.00	0.02
Wash clothes	0.75	0.78	-0.03	0.03
Fetch wood	0.78	0.8	-0.02	0.03
Clean house	0.84	0.83	0.02	0.03
Works on the Farm	0.66	0.74	-0.08**	0.03
Shoes	0.05	0.04	0.01	0.01
Uniform	0.66	0.66	0.00	0.03
Days absent	0.94	1.08	-0.14	0.28
Received uniform from ICS in 2002	0.81	0.22	0.59***	0.03

Note: These summary statistics are for the 780 children (out of the full sample of 1,209) who were present when enumerators arrived at school for the baseline census in Jan 2002, before the start of program. As such, they likely represent – on average – children slightly more likely to be in school.

Table 3 Panel B: Pre-treatment Comparison of Uniform Recipients and Non Recipients

	Received	Didn't	Difference (R-D)	Std Error For Diff
Demographics and Home Characteristics				
Female	0.44	0.5	-0.06*	0.04
Age	9.63	9.69	-0.06	0.14
School attendance (Jan - June '02)	0.87	0.84	0.03*	0.02
Kids in Same Compound	2.41	2.15	0.27	0.36
Iron roof	0.38	0.43	-0.05	0.04
Toilet	0.87	0.84	0.03	0.03
Meals per Day	2.19	2.32	-0.14***	0.04
School Requirements				
Pens	0.52	0.67	-0.15***	0.06
Pencils	1.24	1.28	-0.04	0.07
Books	0.2	0.24	-0.05	0.05
Exercise books	4.73	5.26	-0.53***	0.18
Bag	1	1.08	-0.08**	0.04
Uniform shirts	0.78	0.79	0	0.06
Uniform dresses	0.77	0.75	0.02	0.06
Self Reported Health				
Headache	0.28	0.29	-0.01	0.03
Flu	0.24	0.21	0.02	0.03
Coughing	0.18	0.17	0.01	0.03
Breathing	0.04	0.04	0	0.01
Stomachache	0.23	0.22	0.01	0.03
Vomiting	0.08	0.08	0	0.02
Diarrhea	0.06	0.06	0	0.02
Work at Home				
Cook	0.58	0.6	-0.02	0.04
Fetch water	0.94	0.95	-0.02	0.02
Wash clothes	0.75	0.77	-0.02	0.03
Fetch wood	0.78	0.8	-0.02	0.03
Clean house	0.82	0.85	-0.03	0.03
Works on the Farm	0.69	0.72	-0.02	0.03
Shoes	0.04	0.04	0.01	0.01
Uniform	0.68	0.64	0.04	0.03
Days absent	0.9	1.14	-0.24	0.27

Note: These summary statistics are for the 780 children (out of the full sample of 1,209) who were present when enumerators arrived at school for the baseline census in Jan 2002, before the start of the program. As such, they likely represent – on average – children slightly more likely to be in school.

Table 4: IV and OLS regressions (using randomization into the program as an IV for uniform receipt)

	Attendance Jan '02 – June '02	Dependent variable: Attendance - Jun '02 – Nov '05		
	IV	OLS	IV	OLS
	(1)	(2)	(3)	4
Initially registered in program	0.039			
	(0.026)			
Randomized into program		0.038***		
		(0.014)		
Received uniform in 2002			0.070***	0.055***
			(0.026)	(0.014)
Didn't have uniform at baseline	-0.046**	-0.038**	-0.033**	-0.034**
	(0.019)	(0.017)	(0.017)	(0.017)
Mean among Controls	0.825		0.812	
Observations	1,152	1,152	1,152	1,152
R-squared	0.226	0.109	0.114	0.115

Notes: All regressions include a full set of gender-school-standard fixed effects, controls for whether or not the child had a uniform previous to uniform distribution. F-Statistic for regression (1) 585.08. F-Statistic for regression (3) 520.20. Mean attendance among comparison students is 0.813 (s.e. 0.246). * indicates significant with 90% confidence, ** 95%, *** 99%.

Table 5: Effect of Free Uniforms on School Attendance by Gender and Age (using randomization into the program as an IV for uniform receipt)

	Dependent Variable Attendance June '02 - Nov '05			
	(1)	(2)	(3)	(4)
Received Uniform in 2002	0.041	0.047	0.048*	-0.087
	(0.032)	(0.032)	(0.028)	(0.293)
Female * Received Uniform in 2002	0.068			
	(0.052)			
Young * Received Uniform in 2002		0.063		
		(0.053)		
Young		0.004		
		(0.033)		
Received Uniform 2002 * Didn't have uniform at baseline			0.098	
			(0.067)	
Received Uniform in 2002 * Proportion of school who had Uniform				0.222
				(0.397)
Didn't have Uniform at Baseline	-0.033**	-0.032*	-0.078**	-0.034**
	(0.017)	(0.017)	(0.037)	(0.017)
Mean Attendance Among Controls	0.812			
Observations	1,152	1,152	1,152	1,152
R-squared	0.111	0.112	0.107	0.111

Notes: All regressions include a full set of gender-school-standard fixed effects. Mean attendance among comparison students is 0.813 (s.e. 0.246). * indicates significant with 90% confidence, ** 95%, *** 99%. A student is considered "young" if she is age 5-9; older children are age 10-14. The "female" dummy is excluded because it is collinear with the gender-school-standard fixed effects.

Table 6: Correlates with Attrition in Follow-up Data from 2010

	Missing Completed Primary School	Missing Highest level of Education	Missing Still in School	Missing Participated in KCPE exam	Missing Standard Deviations from mean KCPE Score
Randomized into treatment	-0.011	-0.049***	-0.031**	-0.031**	0.013
	(0.010)	(0.018)	(0.015)	(0.015)	(0.024)
Mean Missing Rate among Controls	0.037	0.125	0.083	0.083	0.698
Observations	1,152	1,152	1,152	1,152	1,152
R-squared	0.122	0.132	0.108	0.108	0.287

Notes: All regressions include a full set of gender-school-standard fixed effects.
* indicates significant with 90% confidence, ** 95%, *** 99%.

Table 7: Long-term impacts (8 years after program began)

		(1) Completed Primary School	(2) Highest Level of Education	(3) Still in School	(4) Participated in KCPE Exam	(5) Std Deviations from Mean KCPE Score
Panel A: Full Sample						
ITT	Randomized into the program	-0.036	-0.069	0.005	-0.014	0.024
		(0.026)	(0.104)	(0.028)	(0.025)	(0.129)
Lee Bounds on ITT	Lower		-0.187	-0.010	-0.039	
	Upper		0.037	0.023	-0.017	
	Trimming Proportion		*0.049*	*0.030*	*0.030*	
IV	Received uniform in 2002	-0.066	-0.126	0.008	-0.025	0.039
		(0.047)	(0.190)	(0.050)	(0.046)	(0.205)
OLS	Received uniform in 2002	-0.009	0.06	0.013	0.015	-0.04
		(0.027)	(0.106)	(0.029)	(0.026)	(0.141)
	Mean among Controls	0.362	7.486	0.656	0.301	0.009
	Observations	1,116	1,033	1,072	1,072	328
Panel B: Restricted to students who didn't have a uniform before the start of the program						
ITT	Randomized into the program &	-0.042	-0.082	0.018	0.002	0.027
	didn't have uniform at baseline	(0.036)	(0.142)	(0.040)	(0.034)	(0.283)
IV	Received uniform in 2002 &	-0.090	-0.181	0.040	0.005	0.053
	didn't have uniform at baseline	(0.077)	(0.313)	(0.089)	(0.076)	(0.551)
OLS	Received uniform in 2002 &	0.031	0.180	0.008	0.049	0.299
	didn't have uniform at baseline	(0.038)	(0.146)	(0.043)	(0.036)	(0.364)
	Mean among Controls	0.288	7.206	0.654	0.224	0.080
	Observations	562	522	544	544	120

Notes: All regressions include a full set of gender-school-standard fixed effects. Column 5 shows standard deviations from mean KCPE test scores by school, conditional of having taken the KCPE exam. * indicates significant with 90% confidence, ** 95%, *** 99%

Table 8: Hierarchical Linear Models of Long-term impacts (8 years after program began)

	(1)	(2)	(3)	(4)	(5)
	Completed Primary School	Highest Level of Education	Still in School	Participated in KCPE Exam	Std Deviations from Mean KCPE Score
Randomized into treatment	-0.070*	-0.157	0.017	-0.045	-0.015
	(0.027)	(0.108)	(0.031)	(0.027)	(0.109)
Variance (Randomized into treatment)	0.000	0.000**	0.002**	0.000***	0.000
	(0.000)	(0.000)	(0.004)	(0.000)	(.)
Variance (Constant)	0.003***	0.066***	0.010***	0.004***	0.000
	(0.002)	(0.039)	(0.005)	(0.002)	(.)
Observations	1,116	1,033	1,072	1,072	328

Notes: Variance in "Randomized into treatment" gives the variance in the ITT treatment coefficient (the slope), whereas variance in the Constant reveals variation in the intercept. The sample size across schools in Column (5) is too small to estimate a standard error for the variance.