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Can provision of free school uniforms harm attendance? Evidence from Ecuador☆

Diana Hidalgo [a], Mercedes Onofa [b], Hessel Oosterbeek [a,b,*], Juan Ponce [b]

[a] University of Amsterdam, TIER, The Netherlands
[b] FLACSO-Ecuador, Ecuador

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ABSTRACT

To raise school participation, many programs in developing countries eliminate or reduce private contributions to education. Using data from a randomized experiment in Ecuador, we ironically find that announcing a free school uniform program had a negative impact on attendance. The school uniforms were distributed in only 63% of the schools that were told that they would get them, thus this negative impact could have been generated by creating false expectations of free distribution, or also by a sunk cost effect on those who did actually receive the uniforms.

1. Introduction

Governments of developing countries and NGO's alike consider it a priority to increase school participation. The government of Ecuador is no exception. Because financial constraints are regarded a barrier for children from poor families to go to school, the Ecuadorian government eliminated school fees for children from poor families and provides free school meals and textbooks. Recently it also started to provide free school uniforms to children from poor families in rural areas with the plan to expand this program to poor urban areas.[1]

We examine to what extent the provision of free school uniforms to children in primary schools contributes to an increase in their school attendance. Free uniforms may increase attendance if children more often have clean clothes to wear to go to school, or if they have to spend less time earning money to cover the expenses of a uniform. For this study we took advantage of the further expansion of the program into poor urban areas in Ecuador. One hundred and one schools – randomly chosen from a sample of 201 schools – were scheduled to receive free uniforms one year ahead of the official schedule (in 2009 instead of 2010). Information about school and teacher characteristics was collected through a baseline survey in the beginning of the school year (in May 2009). Through three unannounced visits (in July, September and November) to the schools, we registered attendance of children enrolled in fifth and sixth grade.

The study took place in five provinces in the coastal part of the country. In three provinces, the program was executed as intended. The uniforms were produced and delivered to the schools on time. Compliance with the assigned treatment status was high but not perfect. Some schools received free uniforms while they shouldn't and some other schools did not receive free uniforms although they should. In the two remaining provinces, however, the local offices of the Ministry of Education failed to arrange the procurement of the uniforms and just two out of 52 schools received the free uniforms. Since assignment to treatment status was stratified by province, results can be presented separately for the group of three provinces where the schools were delivered on time (to which we will refer as "served provinces") and for the group of two provinces where the schools were not delivered on time ("unserved provinces").

Somewhat unexpectedly, we find that assignment to the free uniforms program reduces attendance. Averaged over the three unannounced visits, attendance is 2 percentage points lower in the schools assigned to treatment than in the control schools. This should

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* Corresponding author at: University of Amsterdam, TIER, The Netherlands.
E-mail address: h.oosterbeek@uva.nl (H. Oosterbeek).

[1] Ecuador is among the countries where school uniforms are compulsory. Other countries where uniforms are compulsory or common include many countries from the Commonwealth, as well as Brazil, Cambodia, Chile, China, Honduras, Indonesia, Israel, Japan, Philippines, South Korea and Turkey. Arguments in favor of (compulsory) school uniforms include: (1) increasing students' safety, (2) increase of student learning and positive attitudes towards school, (3) decreased behavior problems by increased attendance rates, lowering suspension rates and decreasing substance use, and (4) increased self-esteem (Brunsma and Rockquemore, 1998).

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be compared to attendance among controls of 92 percent, implying a 25% increase in absenteeism.

We explore several explanations for this finding. We first test whether the result can be attributed to selection effects, where treatment schools attract additional (perhaps less motivated) students. By not publicly announcing the assignment of schools to the free uniforms treatment and by concentrating on pupils in fifth and sixth grades who are less likely to switch schools, it was attempted to avoid such selection effects. We also formally examine whether assignment to treatment affects school enrollment and find that it does not.

A second explanation is related to disappointment. Our results suggest that there is also a negative impact of assignment to treatment in the two unserved provinces. This may be explained by parents in the schools where free uniforms were announced but not delivered being disappointed with the school and therefore more likely to keep their children at home. Alternatively it may be that these parents were unable to (immediately) buy the uniforms when it turned out that the free uniforms were not delivered. The fact that the negative impact estimate in the two unserved provinces is only significant at the first unannounced visit (if no control variables are included) is consistent with this. In the three served provinces, 14 school of the 76 schools that were assigned to treatment did not receive the free uniforms that were promised to them. This means that 18% of the disappointment effect found in the unserved provinces may also be present in the served provinces. Since the negative effect in the unserved provinces is typically smaller (in absolute terms) than the negative effect in the served provinces, the disappointment effect can explain a part but not the entire negative effect in the served provinces.

We also discuss other explanations, including the sunk cost fallacy, anticipation of free uniforms in the near future among the controls, stigmatization and poor quality of government-provided uniforms. Unfortunately, we do not have the data necessary to assess the importance of these other explanations.

Our findings stand in contrast with the results reported by Evans et al. (2012), who evaluate the impact of a very similar intervention in which free uniforms were provided to primary school children in western Kenya. They find that receiving a uniform increased attendance by 7 percentage points, where baseline attendance is 78 percent. There are some differences in the setting and the design of their study and ours that may explain the different results. We elaborate on this in Section 1.[2]

The rest of the paper is organized as follows. The next section describes the context of the education system in Ecuador and provides further details of the free uniforms program. Section 3 describes the experimental design and the data. Section 4 presents the main results and discusses these. The final section summarizes and concludes.

2. Context and program

Ecuador is a low-middle income country with a large share of poor families and high inequality. Compulsory schooling in Ecuador starts at the age of 5 and ends at the age of 14. This covers one year of pre-school, six years of primary school and three years of basic secondary school. Enrollment at the primary level is almost universal, but drops sharply at the transition from primary to secondary school. [3]This drop is mainly concentrated among children from poor families. Official statistics on school attendance are not available. Based on the data used in this paper, absenteeism is close to 10 percent. Typically, school-aged girls who do not go to school help their mothers with domestic work, whereas boys usually help with farm work or work as street vendors.

Wearing a school uniform is compulsory in Ecuador and children can in principle not attend school without one. Normally, parents buy uniforms in specialized shops before the beginning of the school year. Each city or town has some of these shops, which make uniforms for several schools. In some cases, parents' associations have an agreement with artisans to make uniforms. Schools do not play any role in this process. Extremely poor parents may instead of buying a uniform, buy the raw materials and make the uniforms themselves.

In 2007 the government of Ecuador launched the free uniforms program. The program has two main objectives. First, it intends to increase school participation among children in poor areas. Secondly, it aims to improve local economic conditions by contracting small and medium-sized local artisans for the production of the uniforms. Artisans could only be contracted for the government's free uniform program if they satisfied certain quality standards, to ensure that free uniforms were of at least the same quality as regular uniforms. In the first phase of the program only children enrolled in public schools in rural areas were served. In 2008 the government decided to expand the program to public schools in poor urban areas by 2010. The free uniforms are paid for by the government; the expenses are not deducted from the budget of the schools that receive the free uniforms.

Since school uniforms are compulsory, exposure to the free uniforms program implies that parents do not have to pay for a uniform on which they would otherwise have spend between 20 and 25 US$.[4]This amount can be compared to average monthly household expenditures in the two lowest quintiles of the wealth distribution of around 100 US$ (cf. Oosterbeek et al., 2008). A school uniform (self-bought or government-provided) typically consists of: pants or skirt, blouse, shirt, round neck shirt or polo shirt, shorts and sweat suit.

3. Design and data

3.1. Design

The program of free school uniforms is part of a larger program called "Eliminating barriers to access to school". The primary objective of that larger program is to increase enrollment. The government of Ecuador invited us to evaluate the impact of the provision of free uniforms on school enrollment. In meetings with people from the Ministries of Education and of Planning that took place before we implemented the design, we learned that they also expected that the provision of free uniforms would increase attendance (they were aware of the 2005 version of Evans et al., 2012). During these meetings we had various discussions with the ministries about possible research designs. Designs that would allow us to evaluate the impact on enrollment turned out to be infeasible and therefore we chose to implement a design that would provide relatively clean evidence about the impact of free uniforms on attendance.

Given the planned expansion of the program, we proposed to provide the free uniforms to some schools one year ahead of the official schedule, in 2009 instead of 2010. From a list of over 5000 schools, 201 schools in five coastal provinces were randomly drawn to participate in the experiment. We then randomly assigned 101 of these 201 schools to the provision of free school uniforms one year ahead of the official schedule. The other 100 schools were assigned to the control group and were thus supposed not to receive free school uniforms in 2009. Randomization was stratified by province.[5]

[2] Duflo et al. (2011) manipulate the cost of schooling by providing free school uniforms to school children in Kenya. This intervention reduces the dropout rate, teen pregnancy and teen marriage, but not the risk of sexually transmitted infection. The study does not look at the impact on school attendance.

[3] In a sample drawn from the bottom two quintiles of the wealth distribution, the enrollment rate among children aged 10 or 11 – the ages when children are in grades 5 and 6 – is equal to 0.97. For children aged 12 or 13 the enrollment rate in this sample drops to 0.79. (This is based on the data used in Oosterbeek et al., 2008).

[4] Based on the 2006 Survey of Life Conditions in Ecuador. When we restrict this sample to children aged 10–12, living in the provinces included in our experiment and from families in the lowest two quintiles of the wealth distribution, average expenditures are 24 US$.

[5] The actual randomization was conducted through a small computer program that assigned a random number between zero and one to each school. Schools were than sorted on province and random number. The first half of the schools of each province was assigned to the treatment group, the others to the control group.

To identify an uncontaminated effect of the program on school attendance, it is important that the student composition of the schools in the experiment is not affected by the program. For this reason there was no public announcement of the free provision of uniforms in treatment schools. Also for this reason we restricted the data collection to students in the last two years of primary school (fifth and sixth grade). Children in these grades are supposedly less likely to switch schools. In the next section we present evidence showing that the program indeed had no impact on school enrollment. Other reasons to restrict the data collection to fifth and sixth grade students were cost considerations and the fact that older children can more easily get paid work and thus have more reason to be absent from school.

At the end of the school year 2008, schools in the treatment group were informed that by next school year they would receive free uniforms. At this time, parents were informed that they did not have to buy a uniform for the coming year. Treated schools had to submit a list with names, gender, grades and uniform size of their pupils, to the provincial offices of the Ministry of Education ("Direcciones Provinciales de Educación"). These offices contracted local artisans to make the uniforms before the start of the new school year. The uniforms were handed out during the first days of the new school year.

3.2. Data

The data that we use in this study were collected at the school level through a baseline survey in the beginning of the school year (in May 2009). In addition, school attendance was measured for all pupils in grades five and six through three unannounced visits in July, September and November. The school year runs from April to December, so that July is relatively early in the school year. The lists with student names used for this were supplied to the enumerators by the class teachers at the occasion of the first unannounced visit.

A school level questionnaire was administered to collect information regarding school infrastructure, the number of teachers, availability of books, computer labs, and other school inputs. The school questionnaire was addressed to the school principals. A special section in the questionnaire asked about the number of students enrolled in each grade at the start of the current and past years. This is used to construct enrollment variables. Teachers were interviewed separately and from that information we construct the share of female teachers and the average age and experience of teachers in the school. These variables are used as control variables.

In two of the five provinces artisans were contracted too late because of administrative issues. As a result only one of the 24 schools in these provinces that were supposed to get the free uniforms, actually received them. (Also one school assigned to the control group received the free uniforms.) The fact that the program was poorly implemented in two provinces has little to do with the implementation of the program in the other three provinces. Execution of the program is in the hands of the provincial offices. These offices operate independently from each other, with some clearly better and more professional than others. Problems in two provinces are therefore not suggestive that there are also problems in other provinces. Since randomization was stratified by province, we will analyze the data separately for the two unserved provinces (El Oro and Esmeraldas) and the other three provinces (Guayas, Los Ríos and Manabí).[6]

Two of the 201 schools turn out to specialize in adult education and two schools have missing data on school attendance. For the full sample of five provinces we are therefore left with 197 schools, 100 of which were supposed to receive free uniforms and 97 not. In these schools we have attendance records of 9851 pupils in fifth and sixth grade, giving an average number of observed pupils per school of 50.

Table 1 shows descriptive statistics at the school level, separately for schools assigned to treatment and for schools assigned to the control group. The first five columns report these statistics for all five provinces together, the next five columns pertain to the three served provinces and the last five columns to the two unserved provinces. Columns (5), (10) and (15) show p-values that test for differences in means between treated and controls. None of the p-values is below 0.10, indicating that the randomization worked properly. The descriptives also show that the average school is rather small, with only four to five teachers and around 170 pupils. Eighty percent of the primary school teachers in our sample of schools are female. The average level of education is between secondary school (level 4) and professional college (level 5), and average experience is just over 20 years. Although most schools own their premises, the scores on the indexes for infrastructure and pedagogical equipment are rather low. This is probably the clearest indication that the schools included in this study belong to the poor segment of the country.

4. Results

The presentation of the results is divided into four subsections. Subsection 1 shows the relationship between assigned treatment status and actual treatment status. Subsection 3 presents the main findings, and subsection 4 discusses several explanations for these findings. Finally subsection 5 compares our results to those of the study of (Evans et al., 2012).

4.1. The relation between actual and assigned treatment

Table 2 reports how schools (and pupils) are distributed across the assigned and the actual treatment status. The top panel shows this for all five provinces together, while the middle and bottom panels report this separately for the groups of served and unserved provinces. In bold are the numbers of schools and in normal font the numbers of pupils. Percentages are reported in parentheses. The table shows that in the two unserved provinces just two schools (with together only eight pupils) received free uniforms. One of these schools was assigned to treatment, the other not.

In the other three provinces, 24 out of 145 schools have an actual treatment status that differs from their assigned treatment. The 14 schools that should have received free uniforms but did not, were informed before the school year started that the artisans that were contracted to produce their uniforms, could not deliver. The 10 schools that received uniforms while they should not, were mainly smaller schools that combine pre-school and primary school. Free uniforms are provided to all preschool children in Ecuador. Some small schools that combine preschool and primary school and that were assigned to the control group, caused confusion at the provincial offices of the Ministry of Education. When staff in these offices saw the names of these combined schools they mistakenly thought that all pupils should receive free school uniforms instead of only the children in preschool. For the other 121 schools the assigned treatment status corresponds to the actual treatment status.

Table 3 shows the estimates of regressions of the actual treatment status on the assigned treatment status, again separately per group of provinces and for all five provinces together. Regressions are presented both at the pupil level and at the school level, and from specifications without and with control variables. For the three served provinces all coefficients are around 0.7 and are highly significant. Inclusion of control variables has almost no effect on the estimates. For the two unserved provinces the estimates are basically identical to zero; assigned treatment status does not predict actual treatment status. Consequently, the estimates for the five provinces combined are between 0.4 and 0.5.

With partial compliance, the assigned treatment status can be used as an instrumental variable for actual treatment status to obtain estimates of the (local) average treatment effect (Duflo et al., 2007).

[6] Ecuador has 24 provinces, many of them sparsely populated. The provinces included in this study are located in the coastal part of the country (as opposed to the mountains or the jungle). Together the five provinces have a population of around 7 million people; the total population of Ecuador amounts to 13 million people.

Table 1
Balancing of treatment and control schools, by group of provinces.

	5 Provinces					3 Served provinces					2 Unserved provinces				
	Controls		Treated			Controls		Treated			Controls		Treated		
	Mean	SD	Mean	SD	p-value	Mean	SD	Mean	SD	p-value	Mean	SD	Mean	SD	p-value
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)
Average age pupils	10.77	(0.49)	10.88	(0.63)	0.201	10.71	(0.42)	10.82	(0.62)	0.202	10.94	(0.63)	11.06	(0.66)	0.486
Share of girls	0.490	(0.191)	0.472	(0.151)	0.452	0.492	(0.180)	0.477	(0.159)	0.602	0.487	(0.220)	0.456	(0.123)	0.523
Infrastructure index (0-7)	2.948	(1.577)	3.150	(1.888)	0.417	2.522	(1.378)	2.763	(1.896)	0.379	4.000	(1.563)	4.375	(1.245)	0.341
Pedagogical equipment index (0-4)	1.227	(1.005)	1.270	(1.118)	0.776	1.145	(0.944)	1.158	(1.033)	0.937	1.429	(1.136)	1.625	(1.313)	0.570
At least one class per grade	0.557	(0.499)	0.550	(0.500)	0.925	0.435	(0.499)	0.474	(0.503)	0.641	0.857	(0.356)	0.792	(0.415)	0.548
School owns building	0.876	(0.331)	0.880	(0.327)	0.937	0.899	(0.304)	0.921	(0.271)	0.640	0.821	(0.390)	0.750	(0.442)	0.543
Share of female teachers	0.832	(0.227)	0.806	(0.265)	0.467	0.817	(0.256)	0.793	(0.291)	0.593	0.869	(0.128)	0.850	(0.155)	0.634
Number of teachers	5.749	(3.539)	5.942	(4.307)	0.731	4.692	(2.890)	5.125	(4.217)	0.469	8.353	(3.689)	8.528	(3.556)	0.863
Mean education level teachers	4.295	(0.498)	4.291	(0.693)	0.963	4.255	(0.563)	4.249	(0.767)	0.956	4.392	(0.264)	4.423	(0.353)	0.727
Mean experience teachers (yrs)	20.77	(7.65)	20.08	(8.07)	0.540	19.20	(8.37)	18.42	(8.32)	0.574	24.46	(4.59)	24.72	(5.19)	0.849
School size pre-treatment (pupils)	172.2	(141.1)	171.2	(138.8)	0.958	138.2	(109.2)	146.8	(133.6)	0.671	256.0	(174.7)	248.3	(129.0)	0.856
N	97		100			69		76			28		24		

Note: The infrastructure index is the sum of dummies for presence of: boarding for teachers, potable water, electricity, sewage, bathroom, telephone, a teachers' room, a health clinic, a playground and Internet access. The pedagogical equipment index is the sum of the presence of special instructions room, science lab, computer lab, classrooms in good condition and library.

Because of the lack of a first stage effect in two provinces, this is not a very useful approach here. In the next subsection, we will therefore only report estimates of the intention-to-treat effects. For the three served provinces these estimates should be multiplied by $\frac{1}{0.7}$ to obtain average treatment effects for the compliers in these provinces.

4.2. Main findings

Table 4 reports least squares estimates of the effect of assigned treatment status on school attendance, separately for the three times that attendance is measured, with and without the inclusion of control variables, separately for the two groups of provinces and for the five provinces together. The top panel reports the results for the five provinces together. All point estimates are negative and for the first and third visit the effects are significantly different from zero. Also the effect on the average measure of attendance is significantly negative. These findings do not depend on the inclusion of control variables. The size of the effect on the average attendance measure is around 2 percentage points. This should be compared to an average attendance level among the controls of 0.91. A decrease of attendance by 2 percentage points is then equivalent to an increase in absenteeism of 22% ($=\frac{0.02}{1-0.91}$), which can be considered as quite substantial.

The middle and bottom panels report estimates of the ITT effects separately for the two groups of provinces. The results for the three served provinces are very similar to the results of the five provinces together. Surprisingly, however, also in the two unserved provinces all point estimates are negative and in one case even significantly so. The pattern of findings for the two provinces in which the program was not successfully implemented is suggestive that assignment to treatment (without actually receiving treatment) adversely affects school attendance. Some of the estimates for the two unserved provinces are even larger (in absolute terms) than those for the three served provinces.

Table 2
Numbers of schools (in bold) and pupils by assigned versus actual treatment status.

			Assigned treatment		
			$Z=0$	$Z=1$	Total
5 provinces	Actual treatment	$T=0$	**86 (43.7%)**	**37 (18.8%)**	**123 (62.4%)**
			4595 (46.7%)	2505 (25.4%)	7100 (72.1%)
		$T=1$	**11 (5.6%)**	**63 (32.0%)**	**74 (37.6%)**
			218 (2.2%)	2533 (25.7%)	2751 (27.9%)
		Total	**97 (49.2%)**	**100 (50.8%)**	**197 (100%)**
			4813 (48.9%)	3371 (51.1%)	9851 (100%)
3 served provinces	Actual treatment	$T=0$	**59 (40.7%)**	**14 (9.7%)**	**73 (50.3%)**
			2569 (41.8%)	841 (13.7%)	3410 (55.4%)
		$T=1$	**10 (6.9%)**	**62 (42.8%)**	**72 (49.7%)**
			213 (3.5%)	2530 (41.1%)	2743 (44.6%)
		Total	**69 (47.6%)**	**76 (52.4%)**	**145 (100%)**
			2782 (45.2%)	3371 (54.8%)	6153 (100%)
2 unserved provinces	Actual treatment	$T=0$	**27 (51.9%)**	**23 (44.2%)**	**50 (96.2%)**
			2026 (54.8%)	1664 (45.0%)	3690 (99.8%)
		$T=1$	**1 (1.9%)**	**1 (1.9%)**	**2 (3.8%)**
			5 (0.1%)	3 (0.1)	8 (0.2%)
		Total	**28 (53.9%)**	**24 (46.2)**	**52 (100%)**
			2031 (54.9%)	1667 (45.0%)	3698 (100%)

Note: Cells report numbers of schools in bold and numbers of pupils (in parentheses as a percentage of the total). *Z* refers to the assigned treatment, *T* refers to the actual treatment.

Table 3
The impact of the assigned treatment on the actual treatment.

	Pupil level		School level	
5 Provinces (9851 pupils; 197 schools)				
Assigned to treatment	0.406***	0.450***	0.491***	0.491***
	(0.059)	(0.052)	(0.053)	(0.053)
Mean dep. var. control group	0.022	0.022	0.056	0.056
N pupils	9851	9851	9851	9851
N schools	197	197	197	197
3 Served provinces				
Assigned to treatment	0.658***	0.718***	0.663***	0.665***
	(0.081)	(0.064)	(0.062)	(0.063)
Mean dep. var. control group	0.035	0.035	0.069	0.069
N pupils	6153	6153	6153	6153
N schools	145	145	145	145
2 Unserved provinces				
Assigned to treatment	−0.001	−0.006	0.009	−0.009
	(0.003)	(0.006)	(0.055)	(0.053)
Mean dep. var. control group	0.001	0.001	0.019	0.019
N pupils	3698	3698	3698	3698
N schools	52	52	52	52
Additional control variables	No	Yes	No	Yes

Note: Controls at the school level include indicators for infrastructure and pedagogical equipment, a dummy for the school having at least one class per grade, a dummy for the school owning the building, percentage of female teachers, number of teachers, average level of teachers' education, teachers' experience in the teaching profession, enrollment in the previous year, average age of pupils in grades 5 and 6, share of girls in grades 5 and 6 and dummies for the province. Additional controls at the pupil level are dummies for pupils' sex and grade, pupils' age, class-size, and share of girls in the class. At school level, robust standard errors are in parentheses. At pupil level, standard errors are clustered at school level in parentheses.
*** Indicates significance at the 1%-level.

We also looked at heterogenous effects by gender and grade. Results are reported in Tables A1 and A2 in the Appendix. In the three served provinces, effects are in most cases more negative (and sometimes significantly so) for boys than for girls, while there is no difference between the effects for fifth and sixth graders. Also in the two unserved provinces, the estimates are in most cases more negative for boys than for girls and the same for fifth and sixth graders.

4.3. Discussion

This subsection discusses several explanations for the negative effect of assignment to the free school uniforms program on pupils' attendance.

4.3.1. Selection effects

Since randomization was conducted at the school level rather than at the pupil level, and since parents of pupils in schools assigned to treatment were informed at the end of the previous school year that their children would get a free uniform at the start of the next school year, it might be that treatment schools attracted additional pupils from other schools. If this is the case and if the additional students differ from the average (e.g. being less motivated) then the effects that we reported may be attributed to selection or composition effects.

To examine this possible mechanism, we collected data on school enrollment. In the baseline questionnaire, principals were asked to report the numbers of enrolled pupils in their school at the start of the current year (2009) and the previous year (2008), by grade. We also constructed a measure of the current (2009) enrollment from the lists that the teachers supplied at the occasion of the first visit. The correlation of the two measures of the current enrollment equals 0.97.

Table 5 presents the least squares estimates of the impact of assignment to treatment on enrollment, first for the five provinces together and then separately for the three served provinces and the two unserved provinces. Columns (1) to (5) are based on current enrollment reported by the school principal, columns (6) to (10) on current enrollment from the lists supplied by the teachers. In columns (1) to (3) and (6) to (8) the dependent variable is measured in levels, in the other columns in relative changes.

None of the estimates in Table 5 is significantly different from zero, nor is there a consistent pattern in the signs of the estimates. This is

Table 4
Estimates of the impact of assignment to the free uniform program on the probability of attendance.

	Attendance July		Attendance September		Attendance November		Attendance average	
5 Provinces								
Assigned to treatment	−0.032***	−0.033***	−0.011	−0.007	−0.025*	−0.027***	−0.023*	−0.022**
	(0.011)	(0.010)	(0.014)	(0.014)	(0.012)	(0.011)	(0.010)	(0.009)
Mean dep. var. control group	0.915	0.915	0.892	0.892	0.921	0.921	0.909	0.909
N pupils	9851	9851	9851	9851	9851	9851	9851	9851
N schools	197	197	197	197	197	197	197	197
3 Served provinces								
Assigned to treatment	−0.022**	−0.027**	−0.007	−0.006	−0.028*	−0.037**	−0.019*	−0.023**
	(0.011)	(0.011)	(0.015)	(0.015)	(0.015)	(0.015)	(0.011)	(0.011)
Mean dep. var. control group	0.926	0.926	0.900	0.900	0.930	0.930	0.918	0.918
N pupils	6153	6153	6153	6153	6153	6153	6153	6153
N schools	145	145	145	145	145	145	145	145
2 Unsaved provinces								
Assigned to treatment	−0.049**	−0.025	−0.015	−0.005	−0.020	−0.002	−0.028	−0.010
	(0.023)	(0.019)	(0.028)	(0.024)	(0.022)	(0.017)	(0.021)	(0.015)
Mean dep. var. control group	0.900	0.900	0.881	0.881	0.910	0.910	0.897	0.897
N pupils	3698	3698	3698	3698	3698	3698	3698	3698
N schools	52	52	52	52	52	52	52	52
Additional control variables	No	Yes	No	Yes	No	Yes	No	Yes

Note: All regressions include controls for province dummies. For specifications with interaction terms of free uniform and girl/grade, main effects for girl/grade are also included. Additional control variables are dummies for pupils' sex and grade, pupils' age, class-size, share of girls in the class, indicators for the school's infrastructure and pedagogical equipment, a dummy for the school having at least one class per grade, a dummy for the school owning the building, percentage of female teachers, number of teachers, average level of teachers' education, teachers' experience in the teaching profession and lagged enrollment in grades 5 and 6. Standard errors in parentheses are clustered at the school level.
*** Indicates significance at the 1%-level.
** Indicates significance at the 5%-level.
* Indicates significance at the 10%-level.

Table 5
Estimates of the impact of assigned treatment status on school enrollment in grades 5 and 6.

	Administrative					Attendance list				
	Levels			Rel. change		Levels			Rel. change	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
5 Provinces										
Assigned to treatment	−0.395	0.596	−0.013	−0.012	−0.035	0.654	1.668	0.803	0.023	−0.004
	(5.075)	(1.489)	(1.489)	(0.060)	(0.057)	(5.264)	(1.783)	(1.681)	(0.069)	(0.069)
Mean dep. var. control group	51.72	51.72	51.72	0.065	0.065	50.94	50.94	50.94	0.028	0.028
N schools	197	197	197	197	197	197	197	197	197	197
3 Served provinces										
Assigned to treatment	1.552	0.954	0.055	−0.068	−0.093	2.921	2.303	1.136	−0.015	−0.053
	(5.466)	(1.621)	(1.591)	(0.066)	(0.066)	(5.755)	(2.100)	(2.029)	(0.087)	(0.087)
Mean dep. var. control group	41.14	41.14	41.14	0.083	0.083	41.04	41.04	41.04	0.074	0.074
N schools	145	145	145	145	145	145	145	145	145	145
2 Unserved provinces										
Assigned to treatment	−5.841	−0.631	1.140	0.137	0.153	−5.685	−0.441	−0.099	0.125	0.121
	(11.834)	(3.390)	(3.709)	(0.131)	(0.116)	(11.944)	(3.293)	(3.023)	(0.103)	(0.097)
Mean dep. var. control group	77.79	77.79	77.79	0.021	0.021	75.32	75.32	75.32	−0.081	−0.081
N schools	52	52	52	52	52	52	52	52	52	52
Lagged enrollment	No	Yes	Yes	No	No	No	Yes	Yes	No	No
Additional control variables	No	No	Yes	No	Yes	No	No	yes	No	Yes

Note: In columns (1) to (3) the dependent variable is enrollment into grades 5 and 6 taken from the school administration. In columns (6) to (8) the dependent variable is based on information from the attendance lists used during the unannounced visits. In columns (4) and (5) the dependent variable is the relative change in enrollment in grades 5 and 6, where the current enrollment is based on data from the school administration, in columns (9) and (10) the dependent variable is the relative change in enrollment in grades 5 and 6, where the current enrollment is based on information from the attendance lists used during the unannounced visits. All regressions include province dummies. Additional control variables in columns (3), (5), (8) and (10) are indicators for infrastructure and pedagogical equipment, a dummy for the school having at least one class per grade, a dummy for the school owning the building, percentage of female teachers, number of teachers, average level of teachers' education, teachers' experience in the teaching profession, share of girls and average age of pupils.

evidence that assignment to treatment did not have an impact on school enrollment and thus that the estimates of the effect of the intervention on attendance are not biased by changes in composition. This also rules out that pupils in treated schools are exposed to negative peer effects from having worse peers drawn into or staying in school.

4.3.2. Disappointment effects

The negative estimates that we find in the two provinces where the program was not properly executed, suggest that being promised a free uniform but not getting one harms attendance. This might be due to not having bought a uniform and therefore not owning one, when the school starts. And, since wearing a uniform is mandatory, therefore not being able to attend. Alternatively, the negative effect might be due to disappointment or bitterness with the school. Both explanations are consistent with the fact that in the two unserved provinces the negative effect seems to be most prominent early in the school year.

These factors may also play a role in the three served provinces. Almost 20% of the schools that were supposed to receive the free uniforms in these provinces, didn't receive these.

4.3.3. Sunk cost fallacy

The negative effect of (assignment to) treatment on attendance in the three served provinces can potentially be caused by a sunk cost effect. According to this effect, higher expenses can stimulate product use through loss aversion, regret avoidance or more commitment (Ashraf et al., 2010).[7] Especially the commitment channel may operate in the context of education expenditures.

Two recent studies (Ashraf et al., 2010; Cohen and Dupas, 2010) assess the importance of sunk cost effects in the context of health products in developing countries. (Ashraf et al., 2010) look at a water purification product in Zambia, (Cohen and Dupas, 2010) at insecticide-treated bed nets in Kenya. To disentangle selection effects from sunk cost effects, both papers use an ingenious two-stage pricing design, where the first stage manipulates the offer price to be able to condition on willingness-to-pay, and the second stage manipulates the transaction price. Our design omits the first stage, but conditions on willingness-to-pay by ensuring that there are no effects on enrollment. In the second stage discounts of 0% and 100% are randomly assigned to people that by virtue of the randomization have on average the same willingness-to-pay.

Whereas our results are consistent with a sunk-cost effect, both studies that use the two-stage pricing design report results that are not consistent with such an effect. One reason why these studies may reject the role of a sunk cost effect is that the price variation they use in their experiments was too small (0.60 US$ or less) to trigger it.

4.3.4. Control group anticipates future treatment

If students in the control group anticipate that they may receive uniforms in the next year they may have an incentive to stay in school. Knowing that school will be cheaper in the future, they may study harder so as not to repeat or be advised to quit.

Evidence against this explanation is that the largest effect is found for sixth graders who are in their final year of primary school and will move to a secondary school in the next year. There is no provision of free uniforms in secondary schools.

4.3.5. Government-provided uniforms lead to stigmatization

Forcing students to wear a uniform where formerly regular clothes were allowed could lead to a stigmatization effect.

Since the unit of randomization is the school, all students in control schools are wearing a school uniform purchased by their families, and all students in treatment schools are wearing the government-provided uniform. This leaves little scope for stigmatization, which could have occurred with within-school randomization. Moreover, regular clothes are nowhere allowed.

[7] The sunk cost effect or sunk cost fallacy is a deviation from rational economic behavior where sunk should not influence current or future decisions. The classical empirical study reporting evidence of a sunk cost effect is (Arkes and Blumer, 1985), who present evidence that customers who had initially paid more for a season subscription to a theater series attended more plays during the next 6 months.

4.3.6. Government-provided uniforms are of inferior quality

Government-provided uniforms may be of lesser quality than the uniforms parents would have bought themselves and this may keep students out of school.

The artisans that produce government-provided uniforms are certified and the certification depends on the quality they deliver. They have an incentive to produce good quality to increase the chance to be contracted again for the next year. The price the government pays for the uniforms is high enough to be attractive for the artisans. Finally, the schools in our sample are from poor areas in the country. Although we have no hard evidence, these reasons make it unlikely that the self-bought uniforms are of higher quality than the government-provided uniforms.

4.3.7. Treated parents increase other school-related expenses

In the context of a feeding program targeted towards children in the Philippines, (Jacoby, 2002) finds evidence that in the poorest families children's gains from the program are "taxed" within the household. In the context of the program in Ecuador, parents of students that receive the free uniforms may spend the same cost for their children's education by buying other school supplies.

Given that the experiment was conducted in relatively poor neighborhoods we think that this is not a very likely scenario. One reason for this is that the government of Ecuador also supplies book and meals for free to students in poor areas and that public schools are not allowed to charge tuition fees. Unfortunately, data to support this claim have not been collected. Moreover, intra-household compensation can at best explain why the impact of free uniforms on attendance is non-positive, not why it is negative.

Related to this is that receipt of a free uniform may generate an income effect. This would bias the results against a negative effect unless school attendance is an inferior good. One might also worry that recipients of free uniforms resell their uniforms. Given that the colors of uniforms are school specific and all schoolmates of pupils that received free uniforms also received free uniforms, this seems an unlikely scenario.

4.3.8. Treated parents use extra cash for activity that requires child's involvement

The extra cash that parents in the treatment group have available, may be used for activities that require the involvement of the child. For example, the freed up cash can be used to buy merchandise to sell in the streets, where people are more likely to buy from children or when children are around. Unfortunately, we lack the data to assess the importance of this channel.

4.4. Comparison with results from Kenya

(Evans et al., 2012) were the first to study the impact of the provision of free school uniforms on attendance. In 2002, the Dutch NGO with which they work, selected twelve schools for the intervention. Randomization took place within schools; 612 children were assigned to treatment and 693 others to the control group. Upon enrollment in the treatment group in January 2002, a child had to be present for a photograph to be taken and to fill in a small card with some basic information about the child, which would then be sent to the sponsor who donates money to the program. The uniforms were handed out in June of 2002.

The results from this study indicate that receiving a uniform increased attendance by 7 percentage points, where baseline attendance is 78 percent. There are no significant differences in impact across girls and boys, younger and older students, and children that did or did not have a uniform at baseline. Eight years after the intervention, in 2010, the authors collected data on students' educational attainment and test scores. Their results show that there is no significant impact of the provision of free uniforms on these long-run outcomes.

There are some differences in the setting and design that may explain why the findings from (Evans et al., 2012) do not travel from Kenya to Ecuador. First, there is probably a difference in absolute poverty between rural Kenya and urban Ecuador. Due to this it may be the case that the free uniforms in Kenya are lifting a binding credit constraint, while the free uniforms in Ecuador are merely replacing the uniforms parents of targeted pupils would have bought anyhow.

Second, the within-school randomization in Kenya (as opposed to the across school randomization in Ecuador) may cause that parents of pupils who are assigned to the control group feel treated unfairly. To the extent that they attribute the unfair treatment to the school rather than the experimenters, this may lead to a reduced motivation to send their children to school. There may then be a negative spillover of the treatment to the controls.[8](Evans et al., 2012) report that among the pupils in the control group, attendance went up from 85% to 88%, suggesting that the controls did not experience such negative spillovers.

Third, as the authors of the Kenyan study acknowledge, they cannot rule out that there is possibly a psychological impact on the children in the treatment group of being identified by a donor organization. This may explain part of the effect from the free uniforms. An indication of this being the case is that before treated children receive their uniforms but after they have been photographed and filled in the information card for their sponsor, attendance among the treated is already higher (although not significantly so) than among the controls.

5. Conclusions

This paper evaluates the impact of the provision of free school uniforms on school attendance in Ecuador. In contrast to a previous study for Kenya that finds a positive impact of free uniforms on attendance, we find a significantly negative effect on attendance in Ecuador. While the programs in both countries are targeted at poor households, poor people in urban Ecuador are less poor than poor people in rural Kenya. Due to this it may be the case that the free uniforms in Kenya are lifting a binding credit constraint, while the free uniforms in Ecuador are merely replacing the uniforms parents of targeted pupils would have bought anyhow.

We consider several explanations for the unexpected negative effect. A part of it is likely to be due to some students who were promised a uniform but didn't receive it (on time) not having a uniform when the school starts or being disappointed with school. The results from the two provinces in which the implementation of the program failed and where no uniforms were delivered, point to that. This, however, cannot explain the entire negative effect in the three provinces where the program was properly implemented. A possible explanation for the finding in these provinces is that parents who pay for their children's school uniforms (those in the control group) 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 too easily. We acknowledge, however, that the sunk cost explanation is speculative and that more research is needed to understand when and why it manifests itself and when and why it does not.

The design of the experiment only allows us to assess the impact of a free uniforms program in the first year of its introduction. Whether long-run effects are larger or smaller than short-run effects is hard to say. This depends on the mechanism that drives the short-run effects. If commitment through sunk cost is the main channel, short-run effects are probably underestimating the long-run impacts. The reason is that

[8] A related example where the level of randomization matters is documented in (Angrist and Lavy, 2002) (see also Angrist and Lavy, 2009) in which they study the impact of financial incentives for students on their performance. In an across school randomization design they find a positive effect, while they find no effect in a within school randomization design, supposedly due to spillovers where control students benefit from the extra effort of treated students.

parents in the treatment group of the experiment may still be primed with the idea that education is worthwhile because they have been spending money on uniforms in previous years. But if negative short-run effects are due to other reasons, long-run effects may go in another direction.

While our study doesn't allow us to offer a clear explanation for the negative effect that we find, the results are intriguing. They show that a policy motivated by good intentions, even when there is evidence of a positive impact in another setting, may not always have the desired or expected effects (e.g. Karlan and Appel, 2011).

Appendix A

Table A1
Estimates of the impact of treatment assignment on the probability of attendance, by gender.

	Attendance July		Attendance September		Attendance November		Attendance average	
5 Provinces (9851 pupils; 197 schools)								
Assigned to treatment	−0.037*	−0.041***	−0.015	−0.014	−0.033**	−0.041***	−0.028**	−0.032***
	(0.014)	(0.012)	(0.018)	(0.017)	(0.015)	(0.014)	(0.013)	(0.012)
Assigned to treatment × Pupil is girl	0.012	0.017	0.010	0.016	0.019	0.028**	0.014	0.020**
	(0.016)	(0.015)	(0.014)	(0.013)	(0.013)	(0.012)	(0.010)	(0.009)
p-Value (impact on girl = 0)	0.057	0.049	0.727	0.899	0.259	0.298	0.123	0.186
3 Served provinces (6153 pupils; 145 schools)								
Assigned to treatment	−0.019	−0.026*	−0.012	−0.015	−0.040**	−0.053***	−0.023*	−0.031**
	(0.015)	(0.014)	(0.018)	(0.018)	(0.018)	(0.017)	(0.014)	(0.013)
Assigned to treatment × Pupil is girl	−0.005	−0.003	0.011	0.019	0.024*	0.034**	0.010	0.017
	(0.018)	(0.017)	(0.016)	(0.016)	(0.014)	(0.014)	(0.011)	(0.010)
p-Value (impact on girl = 0)	0.076	0.035	0.951	0.796	0.312	0.222	0.203	0.166
2 Unserved provinces (3698 pupils; 52 schools)								
Assigned to treatment	−0.070**	−0.046*	−0.019	−0.004	−0.026	−0.010	−0.038	−0.020
	(0.028)	(0.024)	(0.038)	(0.033)	(0.027)	(0.021)	(0.027)	(0.020)
Assigned to treatment × Pupil is girl	0.044	0.044	0.008	−0.001	0.015	0.018	0.022	0.020
	(0.030)	(0.027)	(0.026)	(0.026)	(0.025)	(0.022)	(0.019)	(0.017)
p-Value (impact on girl = 0)	0.327	0.939	0.642	0.801	0.654	0.703	0.383	0.991
Additional controls	No	Yes	No	Yes	No	Yes	No	Yes

Note: All regressions include controls for province dummies. For specifications with interaction terms of free uniform and girl/grade, main effects for girl/grade are also included. Additional controls are dummies for pupils' sex and grade, pupils' age, class-size, share of girls in the class, indicators for the school's infrastructure and pedagogical equipment, a dummy for the school having at least one class per grade, a dummy for the school owning the building, percentage of female teachers, number of teachers, average level of teachers' education, teachers' experience in the teaching profession and lagged enrollment in grades 5 and 6. Standard errors in parentheses are clustered at the school level. ***/**/* indicates significance at the 1%/5%/10%-level.

Table A2
Estimates of the impact of treatment assignment on the probability of attendance, by grade.

	Attendance July		Attendance September		Attendance November		Attendance average	
5 Provinces (9851 pupils; 197 schools)								
Assigned to treatment	−0.033*	−0.033**	−0.012	−0.007	−0.028**	−0.030***	−0.024**	−0.023**
	(0.013)	(0.013)	(0.016)	(0.016)	(0.013)	(0.012)	(0.011)	(0.010)
Assigned to treatment × Fifth grade	0.001	0.000	0.003	0.001	0.006	0.004	0.003	0.002
	(0.014)	(0.014)	(0.018)	(0.017)	(0.013)	(0.013)	(0.011)	(0.010)
p-Value (impact on fifth grade = 0)	0.111	0.019	0.758	0.609	0.162	0.032	0.206	0.050
3 Served provinces (6153 pupils; 145 schools)								
Assigned to treatment	−0.023*	−0.028**	−0.011	−0.007	−0.034**	−0.040***	−0.023**	−0.025**
	(0.013)	(0.014)	(0.014)	(0.015)	(0.014)	(0.015)	(0.009)	(0.010)
Assigned to treatment × Fifth grade	0.002	0.001	0.007	0.002	0.012	0.007	0.007	0.004
	(0.017)	(0.017)	(0.021)	(0.018)	(0.018)	(0.018)	(0.013)	(0.013)
p-Value (impact on fifth grade = 0)	0.148	0.057	0.849	0.818	0.250	0.084	0.298	0.129
2 Unserved provinces (3698 pupils; 52 schools)								
Assigned to treatment	−0.047*	−0.025	−0.016	−0.007	−0.020	−0.005	−0.028	−0.012
	(0.027)	(0.023)	(0.037)	(0.034)	(0.025)	(0.020)	(0.023)	(0.019)
Assigned to treatment × Fifth grade	−0.004	−0.000	0.002	0.005	0.001	0.006	−0.000	0.004
	(0.024)	(0.023)	(0.033)	(0.035)	(0.019)	(0.019)	(0.018)	(0.018)
p-Value (impact on fifth grade = 0)	0.045	0.254	0.612	0.921	0.405	0.956	0.192	0.592
Additional controls	no	yes	no	yes	no	yes	no	yes

Note: All regressions include controls for province dummies. For specifications with interaction terms of free uniform and girl/grade, main effects for girl/grade are also included. Additional controls are dummies for pupils' sex and grade, pupils' age, class-size, share of girls in the class, indicators for the school's infrastructure and pedagogical equipment, a dummy for the school having at least one class per grade, a dummy for the school owning the building, percentage of female teachers, number of teachers, average level of teachers' education, teachers' experience in the teaching profession and lagged enrollment in grades 5 and 6. Standard errors in parentheses are clustered at the school level. ***/**/* indicates significance at the 1%/5%/10%-level.

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