# Pricing and Access: Lessons from Randomized Evaluations in Education and Health[1]

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**Abstract**

This paper surveys evidence from recent randomized evaluations in developing countries on the impact of price on access to health and education. Debate on user fees has been contentious, but until recently much of the evidence was anecdotal. Randomized evaluations across a variety of settings suggest prices have a large impact on take-up of education and health products and services. While the sign of this effect is consistent with standard theories of human capital investment, a more detailed examination of the data suggests that it may be important to go beyond these models. There is some evidence for peer effects, which imply that for some goods the aggregate response to price will exceed the individual response. Time inconsistent preferences could potentially help explain the apparently disproportionate effect of small short-run costs and benefits on decisions with long-run consequences.

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## I. Introduction

Over the past 10 to 15 years, randomized evaluations have gone from being a rarity to a standard part of the toolkit of academic development economics. We are now at a point where, at least for some issues, we can stand back and look beyond the results of a single evaluation to see whether certain common lessons emerge and what implications these lessons have for our models of human capital investment.

In this essay, we review the evidence from randomized evaluations on one particular issue that has been the subject of extensive and often contentious policy debate - the impact of pricing on take up of education and health services and products.[2] The idea that development projects should aim at financial sustainability has become the driving force behind much development thinking and practice. Advocates of charging argue that even the poor can (and do) pay at least some fee for important services and products; see such fees as vital to sustainability and motivating providers; note that charging may screen out those who place low value on the product or service, thus concentrating take-up on those who value it most (Oster, 1995); and argue that there is a psychological effect, known as the sunk cost fallacy, whereby paying a higher price can make someone feel committed to a product and thus use it more (Thaler, 1980). For example, Population Services International (PSI) a leading social marketing non-profit organization with activities in more than 60 countries, argues that "when products are given away free, the recipient often does not value them or even use them" (PSI, 2006). Accordingly, they have pursued an approach to condom, mosquito net, and water

[2] See Easterly (2006) and Shea (2007).

disinfectant promotion that relies primarily on charging, rather than free distribution. For many aid organizations, charging at least something is a matter of principle.

Yet the idea of charging for education and health products and services in developing countries has come under great criticism as well.[3] The World Bank has started shifting away from this position under pressure from activists, and the WHO recently, and controversially, endorsed free distribution of mosquito nets (Sachs, 2005; WHO, 2007; Lancet, 2007). PSI is also shifting to free distribution of mosquito nets for pregnant women in Kenya.

Another paper in this conference, Rodrik (2008) argues that it is hard to derive general lessons from randomized evaluations, illustrating his case with a discussion of a randomized evaluation of the impact of pricing on access to mosquito nets in Kenya (Cohen and Dupas, 2007). Cohen and Dupas (2007) find that charging for mosquito nets at antenatal clinics in Western Kenya greatly reduces take up, does not serve to target those most in need, and does not induce greater use. Rodrik argues that we cannot generalize too much from these results, because they are likely to be context dependent.

Of course, any attempt to generalize from randomized evaluations or indeed from any particular piece of evidence requires a theory. For example, the PROGRESA program in Mexico provided cash transfers conditional on children's school attendance. Randomized evaluations show it boosted primary school enrollment. Was this effect dependent on there being less than universal primary enrollment to begin with? Presumably yes. Was the impact of the program dependent on the local currency in which

[3] Morduch (1999) argues that the pursuit of sustainability by microfinance organizations has led them to move away from serving the poor. Meuwissen (2002) argues that a health cost-recovery program in Niger led to unexpectedly large drops in health care utilization.

the cash transfer was made being called the Peso? Presumably not. Generalizing from particular pieces of evidence requires an underlying theory of what is likely to be important and what is not.

If our theories are not very good, and the impact of treatment depends on context in a way that is complicated, subtle, and difficult to predict, results from one setting are unlikely to generalize in other settings that may look similar to reasonable people. If indeed it is so difficult to generalize, then this would raise questions not simply about randomized evaluations but more generally about the extent we can learn from social science. For example, if treatment effects vary across countries, then cross-country estimates of the impact of different policies or institutions will typically yield biased estimates (See Pande and Udry, 2005).

On the other hand, if our theories about the world are sufficiently accurate, then randomized evaluations would not be necessary. If we knew, for example, that decisions on school attendance were made to maximize lifetime income, and if we were confident that the correlation between wages and level of education gave us the causal impact of education on earnings (rather than confounding this with a selection effect in which richer, higher ability, or more hard working children stay at school longer and have higher earnings), then it would be possible to build a general model that could simulate the impact of arbitrary changes in school fees on education decisions, wages, and welfare. Or, if we were confident that households, schools, and clinics were distributed randomly and knew how much people valued their time, we could estimate a travel cost model based on differences in take up of education and health services with distance from

schools and clinics, and use the model to predict how changes in price would affect access.

Based on a review of the evidence on how price affects take up, an intermediate position seems warranted, at least in this case. A range of studies on price and take-up of health and education services, including Mexico's experience with PROGRESA, early randomized evaluations in Kenya, and recent studies in Zambia, find remarkably similar results in very different contexts. Imposing even small costs consistently leads to dramatic reductions in take up, both for well-known technologies like mosquito nets and for less familiar technologies like deworming medication. However, we may need to expand the standard model of human capital investment to incorporate time inconsistent preferences and peer effects if it is to fit the data. The evidence from randomized evaluations may help point the way toward better modeling of human behavior in these areas, but it seems unlikely that our existing models fit well enough for us to put a high degree of faith in the results of structural estimation of simple models of human capital investment.

It is worth noting that increasing take up of these products may increase welfare. First, there are positive externalities from some health products - for example, mosquito nets and deworming medication. Second, credit constrains may cause human capital investment to be sub-optimal. Third, time consistency problems may cause under-investment. Fourth, for new technologies there may be information externalities. Finally, many of the investments are on behalf of children, and there may be divergence between investment levels preferred by parents and those that society prefers.

The next section reviews evidence from randomized evaluations on the impact of positive prices. Section III reviews the evidence on negative prices, or incentives for participation. Section IV discusses implications and concludes.

## II. User fees

Below we summarize the evidence from a number of studies on the impact of price on take-up, first in health and then in education. Where evidence is available, we ask not only the extent to which higher prices reduce take up but whether there is evidence that higher prices had a positive impact by concentrating take up on those who most value or most need the product. Two of the studies examine whether prices also induce a psychological commitment to the product that increases usage due to a sunk cost effect.

### (i) *Deworming drugs*

Kremer and Miguel (2007) find that the introduction of a small cost-sharing component into a school-based deworming program dramatically reduced take up of deworming medication and raised little revenue relative to administrative costs. User fees did not help target treatment to the sickest students. As deworming pills are delivered directly into children's mouths, there is no gap between take up and usage, so there is no potential role for pricing to have a positive psychological impact on use because of sunk costs.

Intestinal worms are among the most widespread diseases in the developing world, with two billion people infected and many suffering from anemia and listlessness

as a result (WHO, 2005). School aged children are particularly at risk and a locus for spreading the disease. Treating school aged children for worms therefore has strong externalities—as demonstrated in an earlier evaluation (Miguel and Kremer, 2004). These externalities provide an economic rationale for subsidies. To avoid costly individual parasitological screening, the WHO recommends yearly treatment for all school children in schools where more than half the children are believed to be infected with soil transmitted helminthes (roundworm, hookworm, and whipworm) or where more than 30% of children are affected with schistosomiasis. This type of mass treatment program, however, is most cost effective when take up is high.

In the initial evaluation of an NGO deworming program in rural Kenya, deworming reduced the baseline school absence rate of 30 percent by 7 percentage points (or one-quarter), a gain in attendance that reflects both the direct effect of deworming and any within-school externalities. Including the cross-school externalities, deworming increased schooling by 0.14 years per pupil treated. Overall, it proved to be among the most cost effective ways to boost school enrollment, requiring only $3.50 per additional year of school participation.

The NGO administering this program, ICS-Africa, typically requires communities to contribute to the costs of its projects—as is common among development NGOs. Three years into the deworming program, they did so in a randomly chosen subset of schools. Parents were charged for the use of the deworming drugs, and as was often the case in Kenyan schools, fees were charged on a per-family rather than a per-child basis. The average price charged per child was $0.30, which amounted to roughly one fifth of

the per-child cost of the program if it had been delivered to all children.[4] After the introduction of cost-sharing, the take up rate was 75 percent in the free treatment schools but only 19 percent in the cost sharing schools. While it is possible that this dramatic decline in take up resulted from the fact that people were initially receiving the medication for free and somehow anchored on the price of zero or felt a sense of entitlement, Kremer and Miguel (2007) find that cost-sharing triggered a similar decline in schools exposed to free treatment for different lengths of time.

There is no evidence that charging a higher price helped target the drugs to those who most needed them. Students with helminth infections did not appear any more likely to pay for the drugs in the cost-sharing schools.

Although take up was highly sensitive to having a positive price, there is less evidence that take up was sensitive to variation within the positive price range. Since user-fees were implemented in the form of a per-family fee, the deworming price-per-child varied with the number of primary school children in a household. Kremer and Miguel (2007), however, find that take up was not sensitive to these variations in the exact (positive) price level. Given the dramatic reduction in take up at any positive price level, it may be particularly counterproductive to charge small positive prices for the treatment of infectious diseases.

Fees raised little revenue compared to administrative costs. As noted above, the fees amounted to about 20% of the cost of the program. Charging, however, dramatically increased the administrative costs per pupil because the fixed costs of visiting the school

[4] The $1.50 per-child cost for this program was relatively high because it was delivered as a small-scale pilot program with an evaluation built in and. NGO workers went to each school to provide the medication each time. The cost of a large scale program is less than $0.50 cents per child.

to deliver drugs were amortized over many fewer pupils, so charging fees would allow only about a 5% increase in the number of children given a fixed budget.

In addition to this sensitivity to any non-zero price, Kremer and Miguel (2007) find evidence of social network effects. Kremer and Miguel (2007) exploit the randomization of the school-based deworming program across schools since it created random variation in people's social links to treatment schools, conditional on their total number of social links. Unlike what the non-experimental correlations in the data would have suggested, social networks appear to have depressed take up; having more social links to parents of students in treatment schools reduced the probability that children took deworming medication by 3.1 percentage points and increased the likelihood that parents said that deworming drugs were "not effective" by 1.7 percentage points. These negative peer effects, combined with the sensitivity of take up to any positive price, suggest that temporary subsidies intended to spur imitation are unlikely to lead to a sustainable increase in this kind of technology adoption and that ongoing subsidies might be necessary. Kremer and Miguel (2007) attribute this to social learning about the technology, arguing that in other settings in which technologies proved to be more attractive than people originally believed, such social learning would likely lead to positive spillovers in adoption.

(ii) *Mosquito nets*

Cohen and Dupas (2007) similarly find that charging even a small fraction of the full cost for mosquito nets dramatically reduces take up. They find that charging has no positive effect on the probability that a net is hung in the home, either through a

screening mechanism or through a psychological impact. Hoffman (2008) similarly finds that free nets are as likely to be used as those that are paid for but that free nets are more likely to be used by those who need them most—children under five.

In 2002, the WHO estimated that malaria was responsible for a quarter of all young child deaths in Africa and for over one million African deaths a year. Pregnant women are particularly vulnerable since pregnancy reduces a woman's immunity to malaria. Maternal malaria can also have effects in utero since it increases the risk of spontaneous abortion, stillbirth, premature delivery, and low birth weight.

Insecticide treated nets are a much more powerful way of fighting malaria than untreated nets. Historically nets had to be re-treated frequently, and since many people failed to re-treat their nets, their usefulness was limited. Recently, long-lasting insecticide treated nets have been developed. Evidence suggests that these not only protect the user, but can create positive externalities by reducing transmission of disease.

In the area Cohen and Dupas studied in western Kenya, however, net usage was quite low. The 2003 Demographic and Health Survey estimated that while 19.8 percent of households had at least one mosquito net, only 6.7 percent had an insecticide treated net and only 4.8 percent of children under 5 and 3 percent of pregnant women slept under an insecticide treated net. PSI distributed nets in Kenya for a price that corresponded to a 87.5 percent subsidy. However, they did not go to entirely free distribution.

Since children and pregnant women are most vulnerable to malaria, antenatal clinics are a logical place to distribute nets. Cohen and Dupas' study incorporated a two-stage randomization designed to separate out the two potential routes through which pricing can effect use. In the first stage, patients in antenatal clinics were offered a menu

of subsidized prices for insecticide treated nets. Then, women who agreed to this initial offer price received a randomly chosen discount, generating random variation in both the initial price of the net *and* the final transaction price. The initial randomization occurred at the level of the health clinic, so every woman going to a particular clinic faced the same initially offered price, whereas discounts were randomly chosen from an envelope once a patient agreed to purchase a net. With this design, the effect of the initial price indicates how prices can change the composition of buyers, and the effect of the final transaction price (the initial price minus the amount of the discount) indicates if a higher price increases the likelihood that a given buyer uses the net.

In the clinics that offered free nets, take up was 99 percent. Relative to this rate, take up in clinics that charged for the nets declined at an increasing rate as prices moved from 10 to 20 to 40 Ksh (or US $0.15 to $0.30 to $0.60) by 7.3, 17.2, and 60.5 percentage points respectively, according clinic-based surveys conducted throughout the first six weeks of the program. The linear estimates of the effect of price on take up imply that take up drops by 75 percent when the price of a net increases from zero to $0.75, the cost-sharing price at which insecticide treated nets were sold to pregnant women in Kenya at the time of the intervention.[5] Cohen and Dupas (2007) do not literally find a discontinuity at a price of zero, but since the highest price they examine already represents a 90 percent subsidy relative to the cost of nets, and take up is very low at that level, it does appear that charging any substantial amount will radically cut take up and that the revenues

[5] This reduction in take up, however, drops to 55 percentage points when Cohen and Dupas (2007) restrict their sample to women experiencing first pregnancies in order to avoid contaminating their results with another campaign that had distributed free insecticide treated nets to families with children 9 months prior to the intervention.

generated by any price that would induce a large fraction of mothers to take up the intervention might well be modest relative to the administrative costs of charging for nets.

Cohen and Dupas (2007) find no evidence of screening or psychological "sunk cost" effects. The results are not consistent with the potential role that prices might play in targeting nets to individuals who need them the most: those who paid higher prices appeared no sicker than the prenatal clients in the comparison group in terms of measured anemia, an important indicator of malaria. This could be due to credit constraints: the sickest women may be least able to pay.

According to enumerators making house visits, women who received the free insecticide treated nets were not less likely to have hung their net above a bed than those who paid positive subsidized prices.

Another related recent field experiment in Uganda suggests that charging for a net affects the distribution of net usage within the household relative to free distribution. (Hoffman, 2008). Participants in this intervention were randomly assigned to receive either cash or insecticide treated nets with the opportunity to trade the nets for cash or the cash for nets, with the amount of cash sufficient to purchase nets that would cover every household member given their current sleeping arrangements. They were also read a statement about malaria and the relative vulnerability of young children and pregnant women to the disease. Among those offered the nets for free with the opportunity to trade the nets for cash, 99 percent took home at least one net, and the average number of nets obtained per household member was 0.42; among those who were given cash with the opportunity to trade it for nets, 85 percent took home at least one net, with the average

number of nets obtained per household member only 0.33. Conditional on buying at least one net, however, the average number of nets obtained per member was statistically the same in the free-nets and purchased-nets households.

As with the Cohen and Dupas (2007) study, nighttime checks of net usage roughly three weeks later found that usage rates were as high for those nets given out for free as those that were purchased. Free-nets and purchased-nets households had statistically indistinguishable numbers of unused nets, as well as propensities to leave at least one net unused. In fact, conditional on the number of nets per household member, the probability that an individual household member was using a net was higher for households receiving the nets for free, although insignificantly so.

Who was using the net did vary with price. In households in which the nets had been received for free, the proportion of children under five sleeping under a net conditional on the number of nets acquired was 12.2 to 14.3 percentage points higher relative to households that were offered cash, where 56 percent of children under five were sleeping under a net.[6] On the other hand, when the nets had been purchased, household members perceived to experience at least one malaria episode per year were more likely to use the nets, which were adults in this case, while young children were no more likely to be sleeping under a net than other household members. These results are suggestive of separate mental accounts for free and purchased goods, which is consistent with a growing literature in behavioral economics and psychology on separate mental accounts linked to different needs and different sources of income (Thaler, 1990; Duflo and Udry, 2004).

[6] Hoffman (2008) instruments the number of nets acquired with treatment status.

(iii) *Water disinfectant*

Prior to the Cohen and Dupas (2007) study, Ashraf, Berry, and Shapiro (2007) also used the two stage pricing randomization to test the two potential routes by which pricing could impact use in a door-to-door marketing campaign for water disinfectant in the outskirts of Lusaka. As with deworming and nets, pricing led to a rapid drop off in take up with no evidence of increased targeting to the most vulnerable. There was no statistically significant psychological effect of pricing on use. Ashraf et al (2007) argue that there was some screening effect—in that those who were willing to pay for the disinfectant were more likely to have chlorine in their water at later random checks than those who received it for free. But, since usage was measured two to six weeks after the intervention, it is not clear if pricing did screen out substantial numbers of people who would have *never* used the product, and as discussed below, unless people would repeatedly accept product they did not intend to use on a long-run basis, any wastage of product from non-usage due to free distribution would be a small fraction of the amount distributed in the long run.

In the experiment, water disinfectant was offered to households at a randomly chosen price. Then, households that agreed to this initial offer price received a randomly chosen discount, generating random variation in both the initial price of the disinfectant and the final transaction price. Two follow-up surveys two and six weeks later measured use of the water disinfectant both from households' self reports and from tests of the chemical composition of water stored in the house.

Ashraf et al (2007) document a strong relationship between the initially offered price and the share of households that agreed to purchase the disinfectant at the initial

offer price: a price increase of 100kw ($0.03) triggered a 7 percentage point reduction in the probability of purchase, which corresponds to a price elasticity of nearly -0.6 when evaluated at the mean offer price and purchase probability. When offered an initial price of 300kw (200kw less than the prevailing price at health clinics and 500-700kw less than at retail outlets), 80 percent of respondents purchased the disinfectant, while only 50 percent purchased it at 800kw.

There was no statistically significant evidence that the discounts alter the likelihood that a household used the disinfectant once it had already made its purchase decision. When the final transaction prices increased by 100Kw, households' reports of disinfectant usage increased, but only by a statistically insignificant 0.9 percentage points. Specifications that use measured chlorination rather than self-reports show an insignificant negative effect of 0.7 percentage points.

Ashraf et al (2007) also explore whether there is a discontinuity at zero in this "sunk cost" effect, to see whether just the act of paying any non-zero price influences use. Here they find positive point estimates of 5.7 percentage points for self-reported use and 3.2 percentage points for measured use, but these are still not statistically significant.[7]

The initially offered price also did not help target the disinfectant to households that could benefit from it the most. Families with young children, who are more prone to water-borne diseases, or pregnant women were not more likely to purchase the disinfectant.

[7] When they divide their sample into households that displayed a sunk-cost effect when responding to a hypothetical scenario posed to them by surveyors and those that did not, they find coefficients of much larger magnitude for the hypothetical-sunk-cost households, although these remain insignificant and cannot be statistically distinguished from the estimated effects for households that did not display this hypothetical sunk-cost effect. Ashraf et al (2007) identify hypothetical-sunk-cost households from their answers to the following question posed during the follow-up survey: *Suppose you bought a bottle of juice for 1,000 Kw. When you start to drink it, you realize you don't really like the taste. Would you finish drinking it?*

However, Ashraf et al (2007) argue that higher prices did screen out some buyers who were not planning to use the product. For a given transaction price, a 10 percent increase in the initial offer price led to purchase by a set of buyers who were 3 to 4 percentage points more likely to be using the product two weeks later—in other words, raising the price somewhat disproportionately screened out those who would not have used the product within two weeks, although it also screened out many who would have used the product during that time. This result, however, should be interpreted with caution since the follow-up surveys that measured disinfectant use occurred only two to six weeks after the marketing intervention, so we cannot interpret this as evidence that the observed non-users would never use the disinfectant. Some of the households may have been saving the product for later use.

In our view, charging a 10 percent higher price would be unlikely to cut non-use of the product by 3.6 percent on an ongoing basis, because while households might buy a single bottle of disinfectant and not use it, it is unlikely that they would indefinitely accumulate bottles of disinfectant that they did not intend to use. Therefore, the longer-term screening effect is likely to be much smaller than the short-run effect.

The danger most likely posed by ongoing programs of free distribution would not be that people would accumulate large stocks of water disinfectant or mosquito nets that they do not plan to use, but rather that there would be diversion through secondary markets to alternative uses that were not efficient. For example, people might use the chlorine solution intended to disinfect water for washing clothes or they might use mosquito nets for other purposes. The extent to which that is likely to occur and the extent to which it could be controlled administratively, for example by limiting the

number of free units distributed per person, merits further investigation although it is worth noting that Cohen and Dupas (2007) find no evidence of widespread diversion among households who had not hung up their nets three weeks after their distribution, even though these nets have quite a high resale value (94 percent of non users still had their nets in their house).

(iv) *School uniforms*

In many countries where primary school is meant to be free, there are still substantial costs of attending, a large faction of which is the cost of school uniforms. Traditionally in Kenya students were required to wear uniforms; now headmasters are not officially supposed to turn away a child for not wearing a uniform, but de facto there continues to be strong social pressure to wear uniforms. In 2002, a primary school uniform in Kenya cost nearly $6—a substantial expense in a country with an annual per capita GDP of $340 (Evans, Kremer, and Ngatia (2005)).

Three studies have looked at the responsiveness of school participation to reductions in the cost of schooling through the provision of free uniforms in Kenya—all show a high responsiveness to price at different ages.

The first intervention targeted pupils in early primary school, where uniforms were distributed to students by lottery. Student presence was then recorded from multiple unannounced visits to each school. The students randomly chosen to receive a free uniform were 6 percentage points more likely to be attending school (from a base attendance rate of 82 percent) than students who did not receive a uniform through the lottery (Evans, Kremer, and Ngatia (2005)). Students who did not own a uniform prior to

the program were 13 percentage points more likely to be attending school, which represents a 64 percent decrease in absence.

A similar intervention in the same area that targeted pupils in grade 6 yields further evidence that uniforms serve as a financial barrier to school attendance (Duflo, Dupas, Kremer, and Sinei (2006)). Children randomly chosen to receive free uniforms dropped out of primary school 13.5 percent less often than their counterparts in comparison schools. This program also led to a 1.5 percentage point decline in teenage childbearing (from a baseline rate of 15 percent), most likely because girls who become pregnant typically leave school, and the provision of uniforms made being in school more attractive relative to the alternative of getting pregnant and leaving school. In fact, providing uniforms proved to be more successful in reducing teenage pregnancy than training teachers to teach the national HIV/AIDS curriculum.

These results are consistent with an earlier randomized evaluation in 1995, in which schools in rural Kenya were randomly selected to receive the Child Sponsorship Program – a package of assistance that included free uniforms, textbooks, and classroom construction. Students in treatment schools remained enrolled an average of 0.5 years longer after five years and advanced an average of 0.3 grades further than their counterparts in comparison schools. The program not only led to greater retention of existing students, but it also attracted many students from neighboring schools. Kremer et al (2003) estimate that the average treatment class had 8.9 more students than it would have had in the absence of the intervention.

Although the intervention was implemented as a package, the financial benefit of free uniforms was probably the main reason program schools retained pupils and

attracted transfers. A program that provided textbooks alone did not reduce dropout rates (Glewwe, Kremer, and Moulin (2007)). While the new classrooms may also have had an impact, the first new classrooms were not built until the second year of the program, and dropout rates fell dramatically after the first year, prior to the construction of any new classrooms. Although this could potentially have been due to anticipation of later classroom construction, dropout rates also fell during the first year of the program in upper grades, casting doubt on this hypothesis, since students in upper grades often have good classrooms in any case, and the new classroom construction would not have been complete in time for older students to benefit from it.

## III. Incentives for participation

The previous section reviewed the impact of cutting out-of-pocket costs. This section reviews the impact of negative prices, or incentives. As the evidence above shows, moving from a small positive price to free distribution can have large effects on take up. The studies below suggest there is a similar non linearity for incentives—i.e. small incentives may have a disproportionate impact on take up. As with positive prices, there is evidence of strong peer spillovers in take up. There is also evidence that the timing of payments can be as important as the level of payments—a result found in other randomized evaluations in very different contexts.

### (i) *Conditional cash transfer programs*

Mexico's Programa de Educacion, Salud y Alimentacion (PROGRESA) provided incentives for school attendance and take up of health care services. It was implemented

in 1998 in rural Central and South Mexico and provided up to three years of cash grants for poor mothers whose children attended school 85 percent of the time. Subsidy amounts increased with grade-level to offset the increasing opportunity cost of going to school for older children and provided premia for girls enrolled in junior secondary school. The monthly grant for a ninth-grade girl corresponded to about 44 percent of the typical male day-laborer's wage in 1998 or roughly two thirds of what a child that age could earn if she worked full time. The program also disbursed cash transfers if households participated in certain health and nutrition related activities such as prenatal care, immunization, nutrition monitoring and supplementation, and educational programs about health and nutrition.

The designers of the program structured its phase in so as to allow for a rigorous evaluation. From administrative and census data, they identified approximately 500 rural areas that were considered to be the poorest and the least likely to experience economic growth and randomly allocated the program to two thirds of these areas for the first two years. The remaining third were phased into the program by the third year.

An evaluation of the education aspects of the program finds an increase in enrolment reported in household surveys averaging 3.4 to 3.6 percentage points across all students in grades 1 through 8 (Schultz, 2004). However, this masks important heterogeneity; there was not much scope for the program to affect enrollment rates in the younger grades since enrolment rates were already very high. The largest enrolment increase—11.1 percentage points from a baseline enrolment rate of 58 percent—occurred for children who had already completed sixth grade and were transitioning to junior secondary school. Girls' enrolment increased by 14.8 percentage points, significantly

more than the 6.5 percentage point gain experienced by boys. Schultz (2004) estimates that PROGRESA increased total schooling attainment by 0.66 years (from a baseline of 6.8 years) and would generate an internal rate of return of 8 percent under certain assumptions about the effect of education on earnings.

PROGRESA also led to changes in health seeking behavior and improved child health outcomes. Public health clinics in treatment areas received 2.09 more visits per day (or an 18.2 percent increase) as a result of the program (Gertler and Boyce, 2001). PROGRESA beneficiaries comprised only about one third of the number of families in a clinic's service area, so if all of this increase can be attributed to beneficiaries, then visits in the treatment group increased by 60 percent.

Children under the age of 3 who received the conditional cash transfers were 22.3 percent less likely to be reported as ill in the previous 4 weeks than the children in the comparison group. Children young enough to be exposed to the program for 24 months were 39.5 percent less likely to be reported ill, which suggests that the program generated cumulative health benefits. They were also around 1 centimeter taller and 25.5 percent less likely to display hemoglobin levels indicative of anemia (Gertler, 2004).

There is also evidence that PROGRESA program led to spillovers that increased enrolment of other children. Bobonis and Finan (2008) and Lalive and Cattaneo (2006) examine the enrolment rates of ineligible (wealthier) children in treatment villages and compare them to ineligible children in comparison villages. Bobonis and Finan (2008) find that ineligible children in the treatment villages were 5 percentage points more likely to attend secondary school (from a base of 68 percent) than their ineligible counterparts in comparison villages, with most of this increase concentrated among the poorest of the

ineligible households. Using a similar strategy, Lalive and Cattaneo (2006) find that primary school attendance among ineligibles in treatment villages increased by 2.1 percentage points (from a base of 76 percent) relative to ineligibles in comparison villages. It is not entirely clear whether these spillovers arose from peer effects, increases in school quality in the treatment villages, or an increased expectation of future treatment among ineligibles in treatment villages, but they do suggest that targeted conditional cash transfer programs may have a social multiplier effect. It is worth noting that whereas the spillovers in the case of deworming were possibly due to information transmission and social learning, in this case, since education is well known, that channel is less plausible. There may be a social norm effect, or perhaps children want to be with their friends and if their friends are in school they want to be in school as well. It is also worth noting that in this case the spillovers were positive rather than negative.

Based in part on the clear evidence of program impact provided by the randomized evaluation, the Mexican government expanded the program to cover poor rural and urban households in the rest of Mexico, and nearly 30 other countries have established similar conditional cash transfer programs (The Brookings Institution, 2007).[8] By 2006, 5 million families, or one quarter of Mexico's population, were participating in the program, now called *Oportunidades* (WHO, 2006). Similar programs have been established in many other countries, including Brazil (*Bolsa Escola*, now *Bolsa Familia*), Ecuador (*Bono de Desarrollo Humano - BDH*), Honduras (*Programa de Asignacion Familiar – PRAF*), and Nicaragua (*Red de Proteccion Social - RPS*). A number of these

[8] See Parker, Todd, and Wolpin (2006) for an evaluation of the urban Oportunidades program.

conditional cash transfer programs were subject to randomized evaluations, which found similar effects.[9]

It may be possible to make these kinds of conditional cash transfer programs more effective by altering the timing of cash disbursements if savings constraints, in addition to short-term liquidity constraints, constrain human capital investment. A similar program implemented in Bogota, Colombia (Conditional Subsidies for School Attendance Program or *Subsidios Condicionados a la Asistencia Escolar*) found that the timing of payments was as important as the level of payments, giving insights into why pricing affects take up so strongly and showing how the design of these type of programs can be adapted to have a bigger impact on participation for a given budgetary cost.

The first variant of the program was similar to the PROGRESA conditional cash transfer program and provided families with a cash grant of $15 per month conditional on school attendance. The second variant, a savings treatment, reduced the monthly grants by one third; the remaining third was saved each month and only made available to students' families during the period in which students enroll and prepare for the next school year. The third variant of the program, a graduation/matriculation treatment, was similar to the savings treatment, but it additionally offered students who graduated from secondary school and enrolled in a tertiary institution a transfer of $300, equivalent to 73 percent of the average cost of the first year in a vocational school.

While all variants of the program increased contemporaneous secondary school attendance, the savings and graduation/matriculation treatments also affected enrollment in the subsequent year (Barrera-Osorio, Bertrand, Linden, and Perez (2007)). According to attendance data collected directly from random classroom visits, students in grades 6

[9] See Maluccio and Flores (2005), Schady and Araujo (2006), and Glewwe and Olinto (2004).

through 11 receiving the basic or savings treatments attended school 2.8 to 3.3 percentage points (or 4 percent) more often than their counterparts in a comparison group. Placing the conditionality on graduation from secondary school and subsequent enrollment in a tertiary institution also increased contemporaneous school attendance by 5 percentage points (or 6 percent).

Changing the timing of the transfer with the savings incentive, however, also increased subsequent enrollment in secondary and tertiary institutions by 3.6 and 8.8 percentage points (5 and 39 percent), respectively, representing gains that were significantly different from those experienced by both the comparison group and the group assigned to the basic treatment. The tertiary treatment variant generated gains of similar magnitude in secondary school while raising enrollment in a tertiary institution by a staggering 50 percentage points (or 258 percent). Despite its effect on attendance, however, the basic treatment does not appear to have affected enrolment rates.

Thus, despite the lower monthly transfers, daily attendance rates under the savings and tertiary treatments do not suffer relative to both the comparison group and the basic treatment, while enrolment in the subsequent year significantly improves when payments are delayed until the period immediately prior to enrolment for the subsequent school year or when funding for further education is guaranteed upon graduation.

These findings suggest that in this setting longer-term saving constraints may represent more important barriers to academic participation than more short-term liquidity constraints (Barrera-Osorio et al, 2007). (If the problem were short-term liquidity constraints then the promise of funds in the future should have exerted a less powerful incentive effect, whereas if time consistency problems make savings difficult,

then people may have well found the commitment device valuable.) This is consistent with evidence from Kenya on the take up of fertilizer (Duflo, Kremer, and Robinson (2007)) and from the Philippines on demand for commitment savings products (Ashraf, Karlan, and Yin (2006)).

Barrera-Osorio et al (2007) also collected detailed data on friendship networks during the baseline survey and find evidence of strong peer effects. Since a lottery was used to assign program participation and since randomization was at the level of the student, it is possible to estimate any peer effects associated with the program because the fraction of a student's friends who were treated, conditional on their registering for the initial lottery, should also be randomly assigned. For the average participant (the participant with the average number of treated registered friends), the estimated magnitude of the effect of one treated friend on attendance equals the direct impact of treatment. Any additional treated friends, however, do not imply similar gains in attendance.

Barrera-Osorio et al (2007) also find evidence consistent with negative spillovers within the household for children that were registered but not selected for treatment in the lottery. Families appear to redistribute resources within the household to facilitate the education of treated children. When Barrera-Osorio et al (2007) compare untreated siblings within households that registered two children but only received one treatment to untreated children in households that registered two children but received no treatment, they find that the untreated children within the treated households attended school 2.9 percentage points less often in one locality and worked 1.2 hours less per week in

another. Thus, we have positive spillovers to children in other families but negative spillovers from the program to other children within the family.

(ii) *School meals*

School meals are a common, though controversial, incentive to attend school. The Indian Supreme Court, for example, has made them mandatory for schools across India. One potential advantage of school meals is that they provide automatic targeting and would not be subject to teachers' discretion in documenting attendance in programs that give rewards to families based on attendance (see Shastry and Linden (2007) for evidence of this kind of manipulation). Kremer and Vermeersch (2004) evaluate a randomized evaluation of a school feeding program in preschools in Busia and Teso districts in Kenya. In general, preschools have much lower attendance rates than regular schools. In this case, the average enrollment in a class in community run preschools (for children aged 4 to 6) was 85 according to enrolment rosters, but only 35 students showed up on a typical day. The evaluation found very strong attendance effects.

Preschools were randomly selected to receive fortified flour and money to hire a cook to make porridge for breakfast every day. In order to assess the impact of this program on the attendance rates of both children currently in school and children who had never even enrolled in school prior to the program, baseline statistics were collected for children aged 4 to 6 who at the time were either in school themselves or had siblings in the treatment or comparison schools – either in preschool or in the attached primary schools. With attendance measured by direct observation from an average of six annual surprise visits, the results suggest that after one year, the average attendance of children

in treatment schools increased by 8.5 percentage points relative to the attendance of children in comparison schools who were attending school an average of 27 percent of the time. For children not attending school prior to the intervention, this increase was 4.6 percentage points; for children who were enrolled prior to the school feeding program, it was 11 percentage points.

Attendance gains in the second year of the program were smaller. It is worth noting, however, that the introduction to the program in treatment schools seems to have induced competitive effects that affected comparison schools. After the start of the program, treatment schools increased school fee collection by 57 percent while many nearby comparison schools decreased fee collection and started feeding programs of their own.[10] Thus, these estimated differences in school participation between treatment and control schools may in fact represent a lower bound for the effect of school meals on attendance since the higher school fees in treatment schools could have deterred some children from attending and since these price hikes might not have arisen if all schools simultaneously had offered the same amenity.

This program also increased test scores on curriculum tests in treatment schools for students enrolled at baseline, although only in classrooms with experienced teachers. Anthropometric measurements and cognitive tests suggest that these gains do not derive from increased nutrition or cognitive ability. Rather, the improvement in school attendance appears to be responsible for the observed achievement gains.

(iii) *The Girls' Scholarship Program*

Results from a randomized evaluation of the Girls Scholarship Program in primary schools in western Kenya show that the incentive effect of merit scholarships can also increase attendance rates prior to scholarship receipt (Kremer, Miguel, and Thornton (2008)). In program schools, grade 6 girls who scored in the top 15 percent of the district in their annual district exam were to receive a two year award consisting of a yearly grant to cover school fees that was paid directly to the school for grades 7 and 8 (the remaining two years of primary school), a yearly grant for school supplies paid to the recipient's family, and public recognition at an awards assembly held for students, parents, teachers, and local government officials.

The first cohort of eligible grade 6 girls in program schools scored 0.18 standard deviations higher than their counterparts in comparison schools, and the gains accruing to the second cohort were statistically indistinguishable from this. Overall teacher attendance also improved in treatment schools, increasing by 4.8 percentage points or 6 percent.

The results for these and other outcomes such as student attendance or effects for boys, however, point to the possibility of heterogeneous program effects across geographic areas. ICS-Africa, the NGO administering the program, chose program schools in both Busia and Teso districts. Only schools in Busia district showed any gains in school participation, with a 3.2 percentage point increase in school attendance relative to comparison schools. Similarly, all of the increase in teacher attendance and all of the test score gains were concentrated in Busia. In this successful district, the program also appears to have had spillover effects on boys (who were ineligible for the scholarships), whose test scores increased by 0.15 standard deviations in the first cohort affected by the

program. There also seem to have been peer effects on girls with low pre-scores, who were unlikely to receive scholarships under the program. Kremer et al (2008) cannot reject the hypothesis that treatment effects were equal for all quartiles of the baseline test score distribution, so girls with little or no chance of winning the awards also benefited from the program.

(iv) *Retrieving HIV results*

It is often argued that getting people to learn their HIV status is crucial for fighting HIV/AIDS but that stigma and fear of obtaining positive results create a major barrier that prevents people from finding out their status. This evaluation of an HIV testing program in Malawi, however, found that small incentives and deadlines were sufficient to induce people to pick up their test results at designated testing centers. Distance to the center was also a key determinant of attendance at these centers. This suggests that procrastination and the inconvenience of travel, rather than deep-rooted stigma, explains much of the failure to pick up HIV test results.

In a field experiment in Malawi, nurses visited households and administered free HIV tests, randomizing the amount of vouchers (from $0 to $3) offered to participants which were redeemable upon learning their HIV results in a voluntary counseling and testing center (VCT), which would only be open for one week two to four months later. Prior to the intervention, only 18 percent of people had been tested before, and only half of those had learned their results. After the intervention, those receiving any voucher amount were twice as likely to visit a testing center as those receiving nothing, who went to learn their results 39 percent of the time (Thornton, 2005). The probability of

attendance increased by 8.9 percentage points for every additional dollar offered; even those people assigned a voucher equivalent to 1/10 of a day's wage displayed sizeable attendance gains.

There is also evidence of particularly large effects around a price of zero. A change in the voucher amount from $0 to $0.10 generates an increase in the likelihood of attendance by more than 20 percentage points, which is larger than the changes associated with any other ten cent increase between $0.10 and $3.

Since vouchers were redeemable for only a week after VCT assignment, the results are consistent with the hypothesis that deadline effects are important and that procrastination plays a large role in explaining the low rates of retrieving HIV results prior to the intervention. It may be a mistake to think of people as facing a choice between learning their status and not learning their status. The tradeoff may be between learning status today and tomorrow, with people continuously postponing learning their status.

The distance between a households and its assigned VCT center was another randomized component of the program. The average straight-line distance to a center was 2.1 kilometers, and the average time it took to reach the center was 42 minutes. Individuals assigned to a VCT center over 1 kilometer away were 5 percentage points (or 7 percent) less likely to go to the center to learn their results than those assigned to a closer location. No one visited VCT centers that were 9 kilometers away from sample households.

## V. Conclusion

Table 1 summarizes the interventions reviewed above. Prices appear to have large impacts on take up of health and education products and services, and this basic result seems to hold across a range of contexts. At least some generalization seems possible.

While the sign of this effect is consistent with standard theories of human capital investment, a more detailed examination of the data suggests that it will be important to incorporate peer effects and insights from behavioral economics into our models of take up of education and health services.

There is considerable evidence of peer effects in take up of education and health products, not just for new technologies (Kremer and Miguel, 2007; Kremer et al, 2008) but also for primary education (Bobonis and Finan, 2008; Kremer, Miguel, and Thornton, 2007). Although peer effects were negative for take up of deworming medication, they seem more generally to be positive for school participation rates. As is well understood (e.g. Miguel and Kremer, 2007), peer effects of this type have implications for generalizing from randomized evaluations, and this type of peer effect suggests that the aggregate response to price changes may actually exceed the responses found in randomized evaluations that are not designed to check for the possibility of such effects. Indeed, it is worth noting that when a number of African countries recently abolished school fees or charges in clinics, reported usage went up dramatically: Malawi's reported primary school enrollment increased by 51 percent from approximately 1.9 million pupils in 1993/94 to 3 million in 1994/95; Uganda saw its reported enrollment skyrocket to 5.3 million in 1996 from 3.1 million;[11] similar reported influxes in enrollment occurred in Cameroon in 1999, Tanzania in 2001, and Kenya in 2003. When Uganda's president

[11] Kattan, Raja Bentaoutet and Nicholas Burnett (2004), "User Fees in Primary Education", The World Bank

banned user fees in government health clinics in 2001, reported new outpatient attendance grew 83 percent.[12] (These figures, however, should be taken with a grain of salt, since local officials may have incentives to understate usage when fees are required and overstate it when fees are replaced with central government subsidies.)

In standard models of human capital investment (Becker, 1993; Ben-Porath, 1976; and Rosen, 1977), people weigh the opportunity costs of time against the discounted value of returns. Small fees should not make much difference unless people happen to be right at the margin of going to school. In fact, though, relatively small short-run costs (for example, the cost of uniforms) and subsidies (a $0.10 voucher to go to a HIV testing center) appear to generate sizeable movements in take up, consistent with models of time inconsistent preferences (Laibson, 1997). Also consistent with such models is evidence that people show a preference for committing themselves to save (Barerra-Osorio et al, 2007; Duflo, Kremer, and Robinson, 2007; Ashraf et al, 2006). Thornton's (2005) finding that people are much more likely to learn their HIV status when faced with a deadline for receiving a small reward is consistent with models of procrastination driven by time-inconsistent preferences (O'Donoghue and Rabin, 1999). Finally, there is some evidence that take up behavior is particularly sensitive to price at prices close to zero (e.g. Kremer and Miguel, 2007; Thornton, 2005).

It is worth emphasizing that these behavioral effects are not unique to developing countries. Default rules in tax deferred account retirement plans, like 401(k) plans in the U.S., have a large impact on employee participation and their choice of portfolio (see, for example, Madrian and Shea (2001)). There is also evidence of peer effects in the decision

[12] World Bank PSIA Sourcebook.

to enroll in such plans (Duflo and Saez, 2003). One explanation for mandatory education laws is as a response to these sorts of behavioral effects.

This article has focused on positive, rather than normative, issues, but some of these findings of the role of behavioral factors in take up imply that the steep decline in take up of education and health products and services with price could have serious consequences for welfare. Under a standard model of human capital investment, the welfare consequences of elimination of small fees are likely to be small or even negative, since the people whose behavior is affected by these price changes will be those with low returns from the education and health services. To the extent that these services were subsidized initially and that their associated externalities were internalized, people may have been over-consuming them and further subsidies might have a negative welfare impact. Under some behavioral models, on the other hand, many people may be under-consuming education and health products and services such as deworming medicine, and elimination of prices could potentially substantially increase welfare. There is not yet even an agreed conceptual framework for thinking about welfare in such settings, and we are far from being able to estimate the welfare consequences of price changes in these cases, but it is worth noting that there does not seem to be much evidence that charging for health services targets services to those with the most medical need.

While this article has focused on the take up of education and health products, this is a means to an end, with the ultimate goal being learning and health. In some cases (e.g. mosquito nets), simply increasing use can be assumed to lead to these ultimate objectives because there is solid evidence of the impact on health of these interventions. In other cases, such as learning one's HIV status, we have little evidence on whether take up has

positive effects. There are also cases, such as school participation, where the benefits depend strongly on the quality of services participants receive and their subsequent behavior (see Hanushek, 2008). Longer-term follow up of participants in programs such as PROGRESA could shed light on whether those attracted to education by lower fees have a low or high return to education.

Credit constraints and externalities from consumption provide two other potential rationales for subsidies in some cases. Eliminating prices for deworming medicine and mosquito nets is likely to be welfare-maximizing due to these externalities, and the same may well be true of water disinfectant. Reducing costs of education for students who do well academically may generate positive externalities within the classroom.

An important caveat is that the question of how consumer behavior varies with price is not dispositive for policy debates regarding cost sharing. Other rationales for cost sharing could be advanced. In particular, this survey has not discussed the impact of charging consumers on provider incentives or the utility of cost-sharing requirements in overcoming asymmetric information problems for donors. Given the weakness of provider incentives in the developing world (Chaudhury et al, 2006) and the asymmetric information problems between donors and aid organizations, one could probably build a stronger theoretical case for user fees based on their role in incentivizing providers and screening out aid organizations providing useless services rather than their role in motivating consumers to value products.[13] Yet if these are the problems that user fees are designed to address, it seems worth considering alternatives, such as motivating providers through voucher programs or screening out projects by requiring randomized evaluations before introducing large-scale funding.

[13] Kremer is working with Sendhil Mullainathan on a model along these lines.

Another caveat is that the randomized trials discussed here do not test the role of the background understanding people have of the value of the product and of the marketing surrounding products such as mosquito nets and water disinfectant. People may well be responding in part to the idea that they have been offered a particularly good opportunity. Marketing campaigns may be effective, and it is conceivable that it is harder to design a marketing campaign for a free product or that free distribution over long periods changes people's perceptions of the value of a product. Still, this would suggest that it may be worthwhile to explore whether this is in fact the case, and there are ongoing and planned randomized evaluations that are addressing some of these issues. It may well be possible to advertise products effectively while providing them free through certain channels (e.g. mosquito nets through antenatal clinics).

This review has focused on the impact of price on access, but evidence is also accumulating on the potential role of information in increasing access (Jensen, 2007; Dupas, 2006; and Pandey et al, 2007) as well as the more difficult problem of improving the *quality* of social service delivery. Evidence is also now accumulating on the effectiveness of certain school inputs like extra teachers and textbooks (Banerjee et al, 2005; Duflo, Dupas and Kremer, 2007; and Glewwe et al, 2007), and provider incentives (Glewwe at al, 2008; and Muralidharan and Sundaramanan, 2007), remedial education (Banerjee et al, 2007; Duflo et al, 2007; He et al, 2007), citizens' report cards, the hiring of contract teachers, or increased oversight of local school committees (Bjorkman and Svensson, 2007; and Duflo, Dupas and Kremer, 2007), school choice programs (Angrist et al, 2002, 2006; Bettinger et al, 2007), and contracting out the provision of basic health care services (Bloom et al, 2006). In order to fully capitalize on gains in access, more

experimentation in these areas will be needed so that we can begin to generalize about the most effective ways of delivering social services.

**Table 1: Summary of effects of price on access from randomized evaluations**

| Intervention | Setting | Estimated effects | Authors |
|---|---|---|---|
| **User fees** | | | |
| *Charging an average of $0.30/child for deworming medicine* | Rural Kenya | • Relative to free treatment, take-up drops by 62 percentage points (82%)<br>• Take-up drops for any non-zero price and not sensitive to the exact positive price level.<br>• No evidence that prices target medicine to sickest | Kremer and Miguel (2007) |
| *Varying offer price and final transaction price of a water disinfectant at or below market price of $0.25 in a door-to-door marketing campaign* | Peri-urban Zambia | • Estimated price elasticity of -0.6<br>• 10% increase in offer price leads to purchase by people who are 3.6% more likely to use product<br>• No significant effects of final transaction price on use<br>• Insignificant increase in use for non-zero price.<br>• No evidence that prices target the product to the most vulnerable | Ashraf, Berry, and Shapiro (2007) |
| *Varying offer price and final transaction price of insecticide treated mosquito nets in antenatal clinics from $0 to $0.75* | Rural Kenya | • Relative to free nets condition, charging prevailing cost-sharing price reduces take-up by 75%<br>• No evidence that final transaction price increases use<br>• No evidence that prices target nets to sickest women. | Cohen and Dupas (2007) |
| *Offering free mosquito nets or cash to purchase nets* | | • Probability of acquiring at least | Hoffman (2008) |

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|---|---|---|---|
| | | one net is 99% in free-nets group, 85% in purchased-nets group. Conditional on acquiring at least one net, total acquired nets per household member is the same.<br>• Net usage is statistically indistinguishable in free-nets and purchased-nets households.<br>• In free nets group, proportion of children under five sleeping under a net is 12.2 to 14.3 percentage points higher than the purchased-nets group, where the proportion is 56%. | |
| *Paying for textbooks, school construction, and uniforms* | Rural Kenya | • After 5 years, class size increased by 8.9 students from base of 29 students via increase attendance of prior students and transfers of new students.<br>• After 5 years, years of enrollment increased by 0.5 year (13%) and grade advancement increased by 0.3 grades (16%) | Kremer, Moulin, and Namunyu (2003) |
| *Provision of free uniforms with an average price of $5.82* | Rural Kenya | • For younger pupils, 6 percentage point increase (7%) in school attendance and a 13 percentage point (15%) increase for students without a uniform prior to program | Evans, Kremer, and Ngatia (2008)<br><br>and<br><br>Duflo, Dupas, |

| | | | |
|---|---|---|---|
| | | • For older pupils, 13.5% decline in absence and 10% decline in teenage childbearing | Kremer, and Sinei (2006) |
| **Incentives** | | | |
| *PROGRESA*<br>*Cash transfers conditional on school attendance and take-up of health services*<br><br>*Education grants reduce private cost of going to school by 50-75%*<br><br>*Health grants equivalent to 20-20% of household income* | Rural Mexico | *Education*<br>• 3.4-3.6 percentage point increase in attendance for all children in grades 1 to 8<br>• 11.1 percentage point increase (19%) in attendance for students who have completed 6th grade and 14.5 percentage point increase for girls who have completed 6th grade<br>• Spillovers to ineligibles in treatment villages of 5 percentage points (7%) in secondary enrollment<br>• Spillovers to ineligibles in treatment villages of 2.1 percentage points (3%)<br><br>*Health*<br>• Health clinics in treatment areas receive 2 (18%) more visits per day<br>• Children under 3 years in treatment areas 22.3% less likely to be reported ill in past month | Schultz (2004)<br><br>Bobonis and Finan (2008)<br><br>Lalive and Cattaneo (2006)<br><br>Gertler and Boyce (2001)<br><br>Gertler (2004) |

| | | | |
|---|---|---|---|
| | | • Treatment children 1cm taller<br>• Treatment children 25.5% less likely to display hemoglobin levels indicative of anemia. | |
| *3 variants of conditional cash transfers based on attendance:*<br><br>*(a) PROGRESA variant ($15/month)*<br><br>*(b) Savings treatment where 1/3 of each monthly transfer delayed until enrollment part of school year*<br><br>*(c)Graduation/matriculation treatment which was like (b) plus large transfer ($300) upon secondary school graduation and matriculation in tertiary institution* | Bogota, Colombia | • The three variants improved attendance by 2.8 to 5 percentage points (4 to 6%)<br>• Basic treatment had no effect on enrollment in subsequent year<br>• Enrollment in secondary institutions increased by 3.6 percentage points (5%) under both saving and tertiary treatments<br>• Enrollment in tertiary institutions increased by 8.8 percentage points (39%) under savings treatment and by 50 percentage points (258%) under tertiary treatment | Barerra-Osorio, Bertrand, Linden, and Perez (2007) |
| *Free school meals in preschools* | Rural Kenya | • School attendance increased by 8.5 percentage points (31%) in treatment schools<br>• Attendance gains both for current students and students who had never attended before<br>• In response, comparison also | Kremer and Vermeersch (2004) |

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|---|---|---|---|
| | | introduced by second year of program and treatment schools increase fees by 57 percent. | |
| *Merit scholarships of $19.20 for school fees and school supplies for 6$^{th}$ grade girls* | Rural Kenya | • 0.18 SD increase in girls' test scores<br>• Heterogeneous treatment effects across districts. In successful district, 5 percentage point increase in student attendance and 0.18 SD increase in boys' test scores | Kremer, Miguel, and Thornton (2008) |
| *Varying vouchers from $0 - $3 and the distance to go to a testing center to learn results of a free HIV test administered at home* | Rural Malawi | • Vouchers double likelihood of attendance from a base of 39%<br>• Likelihood of attendance increases 8.9 percentage points with every $1 increase in voucher<br>• Large discontinuity when raising voucher from $0 to $0.10.<br>• An increase in testing center distance of 1km leads to a 5 percentage point (7%) decline in likelihood of attendance | Thornton (2005) |

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