ABSTRACT

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IN DEVELOPING COUNTRIES

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Almost all countries subsidize education. These subsidies are generally designed to account for positive social returns to education and a recognition of education as a basic human right. Without subsidies, credit constraints may preclude children from attending school. While the availability of low-cost private schooling is increasing, it is likely that governments, through these subsidy programs, will be responsible for ensuring access to a quality education for all children.

The first two papers of my dissertation examine government implemented formal education policies in Kenya designed to improve access to secondary schooling and the quality of selected secondary schools, respectively. My first paper exploits the introduction of a free secondary education program to examine the demand response to a supply side government program to improve access as well as measure the impacts of secondary schooling on demographic and labor market outcomes. My second paper evaluates a school upgrade program designed to improve school quality at selected secondary schools.

In many developing countries formal education is often insufficient, however, to ensure that individuals are able to enter the formal labor market. With this in mind, in my third paper, I examine a multifaceted labor market intervention implemented by an international NGO that was designed to improve labor market outcomes for young women who have already completed or dropped out of formal schooling.

ESSAYS ON EDUCATION IN DEVELOPING COUNTRIES

by

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Chapter 1: Introduction

1.1 Introduction

Investments in human capital and the associated development of cognitive skills have a demonstrated relationship with economic growth (Hanushek and Wößmann, 2008). However, while improving, many developing countries still lag behind developed countries in access to education and have students that substantially underachieve relative to their counterparts in developed countries (Pritchett, 2013). With the potential growth benefits in mind, ensuring and increasing access to high-quality education has become a key development goal.

Policies designed to ensure access to a quality education must address a number of questions: are supply-side policies to increase access to education sufficient to spur demand? If the policies do increase demand, is there an access-quality tradeoff where a rapid influx of students decreases the education quality? Is increasing educational attainment sufficient to improve labor market outcomes or are there other binding constraints? Finally, in many developing countries, the transition into the labor market is slow and youth underemployment is high for individuals of all education levels; if formal education is insufficient to secure a position in the labor market, can policies that promote self-employment and entrepreneurship speed up this transition and help young adults earn an income?

In this dissertation, I address these questions using a combination of government policy changes in Kenya and a randomized evaluation of a non-governmental organization's labor market intervention implemented in Nairobi, Kenya.

In my first paper, I examine the Kenyan government's 2008 abolition of tuition for public secondary schools, showing that it dramatically increased the proportion of students continuing from primary to secondary school, particularly from areas with low initial primary to secondary transition rates. Using this regional variation in exposure to the program together with birth-cohort variation, I show that post-primary education in Kenya delays childbirth and related behaviors, and shifts employment away from agriculture towards skilled work. Despite concerns over the quality impact of this rapid expansion of schooling, there is little evidence that secondary school completion examination grades deteriorated in regions more impacted by the program.

In the second chapter, I focus on a Kenyan government program that upgraded selected secondary schools to a higher-quality national tier and examine whether the program improved student educational outcomes, as measured by student secondary school completion examination results. The program impact is identified by comparing student outcomes at upgraded schools to student outcomes at schools that met the government's upgrade eligibility criteria, but were not selected for the upgrade program. To avoid potential composition changes resulting from the program, I examine only cohorts already enrolled in the schools prior to the upgrade announcements. Using this difference-in-differences approach, I find evidence of heterogeneous program impact: while the program had no measurable impact for girls, the program improved overall examination scores for boys. The improved scores for boys appear to be driven by shifting up the lower tail of the test score distribution.

Finally, recognizing that in many developing countries, education is often not sufficient to ensure that individuals are able to enter the formal labor market, in my third paper which is co-authored with Maddalena Honorati, Pamela Jakiela, and Owen Ozier, I focus on a non-governmental organization's approach to addressing high youth underemployment in a developing country context by examining the impacts of a multifaceted labor market intervention implemented in Nairobi, Kenya. We benchmark the program impact against a cash grant of comparable value to the program and find that both programs increase self-employment and income in the short run but that these impacts do not persist into the second year of the program.

Chapter 2: The Impacts of Free Secondary Education: Evidence from Kenya

2.1 Introduction

Over the past 15 years, countries throughout sub-Saharan Africa have abolished school fees for primary education (UNESCO, 2015). These policies have been shown to increase educational attainment across a variety of contexts and among the most vulnerable populations.[1] Free primary education programs also coincided with the rapid increase in the region's net primary enrollment rate from 59% in 1999 to 79% in 2012 (UNESCO, 2015).[2] A small number of countries have recently expanded their free education systems to include secondary school.[3] Whether these supply side policies are sufficient to increase educational attainment at the secondary school level remains to be seen.

There are a number of reasons why we might expect a more muted demand

[1]See, for example the analysis of programs in Kenya (Lucas and Mbiti, 2012a), Malawi (Al-Samarrai and Zaman, 2007), Tanzania (Hoogeveen and Rossi, 2013), and Uganda (Deininger, 2003; Grogan, 2009; Nishimura, Yamano, and Sasaoka, 2008).

[2]For a broad review of interventions targeting schooling access and quality, including easing financial constraints, see Murnane and Ganimian (2014) and Petrosino, Morgan, Fronius, Tanner-Smith, and Boruch (2012). The global net enrollment rate rose from 84% to 91% between 1999 and 2012.

[3]Secondary school fees were eliminated in Uganda (2007), Rwanda (2007, 2012), Tanzania (2016), for girls in The Gambia (2001-2004), and selectively for schools in relatively poorer areas in South Africa (2007).

response to free secondary education programs than has been observed for free primary education programs. First, the opportunity cost of schooling is likely to increase with child age, so that the opportunity costs of attending secondary school will typically exceed those of attending primary school.[4] Second, these opportunity costs may be particularly important in settings with low returns to secondary education, where it may be optimal for individuals to forgo secondary schooling entirely: in contexts where secondary schooling does not increase cognitive skills, the returns to education are likely to be low, and the demand response to a free secondary education policy is likely to be small.[5] Even in contexts where secondary schooling does increase cognitive skills, it may still be optimal to forgo schooling if the expected demand for secondary school graduates is low. Third, parents may be responsible for selecting the schooling level of the child but may not have incentives fully aligned with the child's long-term earnings potential (Baland and Robinson, 2000). In this case, parents may be less responsive to a free secondary education policy, opting instead to enter the child into the labor market. Finally, individuals or parents may underinvest in secondary schooling if they are misinformed about the returns to further schooling (Jensen, 2010).[6] This may be particularly important at the secondary school level in areas with low educational attainment, and where the

[4]All countries in sub-Saharan Africa, except Liberia and Somalia, have ratified the International Labour Organization's Minimum Age Convention (1973) mandating minimum ages of labor market participation between 14 and 16.

[5]While it is generally the development of cognitive skills, and not schooling attainment, that is important for individual earnings (Hanushek and Wößmann, 2008), recent evidence has found relatively low returns to additional schooling when credentials are held constant, implying a large signaling benefit (Eble and Hu, 2016).

[6]There are a number of behavioral reasons one might underinvest in education, such as present bias, overemphasis on routine, and projection bias (Lavecchia, Liu, and Oreopoulos, 2015).

community perception of the value of secondary education may be low. If access to free secondary education does increase educational attainment, then we might expect such a policy to impact a range of demographic and economic outcomes.

Increased educational attainment is likely to have broad demographic impacts; Schultz (1993) describes the negative relationship between parental education and fertility as "one of the most important discoveries in research on nonmarket returns to women's education." There are three main mechanisms through which education is likely to impact fertility (Ferré, 2009). First, secondary school students may learn about contraceptive methods leading to lower rates of unintended pregnancies. If women are getting pregnant earlier than they would like, this knowledge could help them delay pregnancy until they are ready. Second, education may shift preferences towards fewer, higher quality children (Grossman, 2006). Third, if having a child precludes the mother from continued schooling, young women may delay sexual activity to ensure that they can finish their schooling. Regardless of the mechanism, delaying age of first birth and lowering desired fertility could have long term benefits for the mother and child. Childbearing at a young age and high total fertility have been linked to deleterious impacts on both the mother and child, including higher morbidity and mortality, lower educational attainment, and lower family income (Ferré, 2009; Schultz, 2008).[7]

Additional education is also likely to impact labor market outcomes (Hanushek

[7] The longer terms impacts of women delaying marriage is more nuanced. Delaying marriage without an accompanying increase in educational attainment has been shown to lead individuals to partner with lower cognitive ability spouses. In contrast, individuals who delayed marriage while also increasing their schooling attainment have been shown to partner with more educated husbands (Baird, McIntosh, and Özler, 2016).

and Wößmann, 2008; Heckman, Lochner, and Todd, 2006; Goldberg and Smith, 2008). Potential impacts of education on occupational choice may be particularly important as labor flows from low-productivity sectors to high-productivity sectors have been shown to be a key driver of development (McMillan and Rodrik, 2011; McMillan, Rodrik, and Verduzco-Gallo, 2014). Free secondary education policies may stimulate economic growth if they provide the cognitive skills required for occupations in higher productivity sectors.

An important caveat is that lowering the cost of education may adversely impact student learning. A rapid influx of students together with an inelastic supply of education inputs may dilute per-student resources.[8] If these inputs enter positively into an education production function, a dilution is likely to decrease student achievement.[9] Additionally, lowering the cost of schooling may induce lower-ability students to attend secondary school, decreasing average peer quality. In the presence of positive peer effects, this would lower student learning. An impact on academic achievement, as measured by test scores, combines a deterioration of per-student resources with a change in the composition of the student body. An increase in test scores indicates that the price decrease enabled high performing, credit-constrained individuals to attend secondary school, overcoming the negative impact of a dilution

[8]Teacher supply has been shown to be a constraint at the primary school level in developing countries where there are relatively few secondary school graduates to teach future students (Andrabi, Das, and Khwaja, 2013). Teacher supply at the secondary school level may be particularly inelastic as a result of small tertiary education systems; countries in sub-Saharan Africa have an average tertiary education gross enrollment rate of 6% (UNESCO, 2010).

[9]The distribution of a fixed supply of teachers within a national school system contrasts with some of the U.S. research on exogenous increases in the number of students. For example, an influx 'Katrina children' had little impact on per-student resources due to displaced teachers entering the same school systems as displaced students leading to no impact on non-evacuee students' learning (Imberman, Kugler, and Sacerdote, 2012).

of resources.

This paper examines the impacts of a national free secondary education (FSE) program in Kenya on educational attainment and achievement, and uses the program as an instrument to examine the impact of education on fertility behaviors and labor market outcomes. There are three primary contributions of this paper. First, I present the first evaluation of a national secondary school fee elimination program implemented without gender or socioeconomic eligibility restrictions. Second, I present new evidence on the impact of secondary education on both labor and non-labor market outcomes. Finally, I compile and use new data on educational achievement at the individual level for all students who completed secondary school to evaluate the impact of the policy on academic performance.

My identification of causal impacts exploits region and cohort-specific variation in the treatment intensity of individuals exposed to the program. Regional variation in treatment intensity stems from heterogeneous pre-program primary to secondary school transition rates across Kenya: regions with low pre-FSE primary to secondary transition rates experienced larger increases in secondary schooling rates as a result of the program.[10] The cohort variation arises from the timing of the program: individuals above secondary schooling age at the time of the program's implementation in 2008 would have had to return to school to take advantage of FSE rather than simply continue their schooling from primary to secondary school. I use these sources of variation to measure the impact of FSE on educational attain-

[10] The transition rate is unrelated to overall county educational attainment. Rather, it measures the proportion of students who progress to start secondary school after finishing primary school. Thus, high transition rates can arise in counties where only a small fraction of a cohort completes primary school but where most of the completers then subsequently start secondary school.

ment using a difference-in-differences framework. There are two main assumptions underlying this approach. First, variation in pre-program primary to secondary transition rates should be attributable to unchanging characteristics of the counties and second, the pre-FSE time trends across high and low transition rate counties should be the same. Under these assumptions, the identification strategy differences out the structural region and cohort differences yielding a consistent measure of program impact. I present evidence indicating that these assumptions are likely to be satisfied in this setting. I demonstrate that the pre-program treatment intensity measures are highly correlated across time indicating that differences across counties in primary to secondary transition rates are due to structural rather than transitory factors. I also show long term pre-program common trends across high and low treatment intensity regions and, as a robustness test, explicitly control for potentially confounding region specific trends.

As my analysis exploits variation in primary to secondary transition rates rather than the proportion of the population with any secondary schooling, I require a further assumption that FSE did not differentially change the composition of primary school completers across treatment intensities. I present analogous difference-in-difference estimates to demonstrate that FSE intensity is uncorrelated with changes in the probability of completing primary school.[11]

My difference-in-difference estimates indicate that FSE increased educational

[11]Using the primary to secondary transition rate and focusing on the sample of primary school completers should provide additional power as it restricts attention to individuals who are likely to be affected by the program; that is, students who do not attend primary school are unlikely to change their behavior as a result of the program. I confirm that the results are robust to defining the intensity based on the proportion of the population with any secondary schooling.

attainment and, contrary to concerns expressed in the local media, had no significant detrimental impacts on the academic achievement of students. At the mean county intensity, the program is estimated to have increased schooling by 0.8 years of education, with similar results by gender, indicating that the program was successful at inducing students to continue to secondary school. There is also suggestive evidence that the program increased the proportion of students completing secondary school, although this result is not significant across all specifications.[12]

Building on the demonstrated impact of FSE on educational attainment, I then use exposure to the FSE program as an instrumental variable to measure the impact of education on a variety of fertility behaviors. This instrumental variables approach is most closely related to that of Keats (2014) and Osili and Long (2008) who examine the impact of free primary education on similar variables in Uganda and Nigeria respectively. My results suggest education causes large delays for age of first intercourse (10-20% at each age), age of first marriage (50% at each age), and age of first birth (30-50%) for each age between 16 and 20. Despite impacts on probability of first birth, I find no evidence that education decreased women's desired fertility or increased contraceptive use. This suggests that the primary mechanism through which education acted on fertility behaviors is through a confinement effect whereby women delay intercourse to ensure that they can continue their schooling.

Using the same instrumental variables approach, I also use exposure to the FSE program to examine impacts of education on labor market outcomes. My estimates

[12] I run a falsification test where I assume that the program was implemented five years before its actual implementation and demonstrate that, as expected, the hypothetical program had no significant impacts on educational attainment.

show that post-primary education shifts young women into more productive sectors: decreasing the probability of agricultural work and increasing the probability of skilled labor while potentially delaying entry into the labor force.

This paper contributes to several literatures. First, it connects with the growing literature on the impact of education on non-market outcomes. While recent empirical work in both the United States and Cambodia found little evidence that education increased the age of first birth (McCrary and Royer, 2011; Filmer and Schady, 2014), empirical work from developing countries in East Africa has found that secondary schooling has significant impacts on child bearing decisions (Baird, Chirwa, McIntosh, and Özler, 2010; Ferré, 2009; Ozier, Forthcoming). These divergent findings suggest that impacts may be conditional on high fertility levels.[13]

My results also contribute to the literature examining the impacts of formal education on labor market sector.[14] Earlier studies, focusing on outcomes for men, have found that education increases the probability of wage work (Duflo, 2004) and decreases the probability of self-employment (Ozier, Forthcoming). My results for women compliment this earlier work: while I find no impact on wage work or self-employment, I find that education shifts women across sectors, decreasing the likelihood of working in agriculture and increasing the probability of skilled work.

I also provide new evidence on the impact of a free secondary education pro-

[13]An ongoing randomized evaluation of secondary school scholarships in Ghana will examine the impacts on incomes, health, and fertility outcomes as described in Duflo, Dupas, and Kremer (2012). Preliminary data from the evaluation has been used to examine the impact of school management on academic outcomes Dupas and Johnston (2015).

[14]A related but distinct literature examines the impact of vocational education programs on labor market outcomes (Attanasio, Kugler, and Meghir, 2011; Bandiera, Buehren, Burgess, Goldstein, Gulesci, Rasul, and Sulaiman, 2014b; Card, Ibarraran, Regalia, Rosas-Shady, and Soares, 2011).

grams on academic attainment, building on recent studies in a range of contexts (Gajigo, 2012; Garlick, 2013; Barrera-Osorio, Linden, and Urquiola, 2007).[15] These studies have found that the programs are successful at enrolling additional students although the magnitude of estimated effects has varied widely with larger impacts typically stemming from lower income countries. To date, the literature has not examined a national FSE program that was offered unconditional on gender or socioeconomic status. Examining a policy that targeted both males and females might be particularly important if the price elasticity of demand varies across gender.

Finally, my results also contribute to the related but smaller literature examining the causal impacts of free education policies on educational achievement. This recent empirical work suggests an optimistic ability of countries to rapidly expand access through free education programs without negative achievement impacts. Evaluations of large scale primary education programs have been able to rule out broad negative impacts (Lucas and Mbiti, 2012a; Valente, 2015), while a smaller secondary school program in The Gambia was shown to increase achievement (Blimpo, Gajigo, and Pugatch, 2015). The literature has yet to examine the impact of a secondary school program at the scale of the Kenya FSE, or one that impacted the cost of schooling for both males and females. The absolute size of the secondary school system may be important; there were over 1.3 million students in the Kenyan secondary school system at FSE implementation, potentially limiting the ability of policy makers to target attention or resources towards mitigating quality declines.

[15] There is a related literature demonstrating the sensitivity of schooling behaviors to programs that lower either basic household costs, such as school feeding programs (Kremer and Vermeersch, 2005), or ancillary education costs, such as school uniform subsidies (Duflo, Dupas, and Kremer, 2015; Evans, Kremer, and Ngatia, 2012).

Subsequent sections of this paper detail a model of schooling, credit constraints, and fertility (section 2.2), provide a background of Kenya's education system (section 3.2), describe the data (section 2.4), examine the impact of FSE on educational attainment (section 2.5), examine the impact of FSE on fertility and occupational choice (section 2.6), present reduced form results examining the impact of FSE on student achievement (section 2.7), and conclude (section 2.8).

2.2 School Attainment, Credit, Ability, and Fertility

I motivate the analysis using a stylized model of human capital investment and child-bearing adapted from Lochner and Monge-Naranjo (2011) and Duflo, Dupas, and Kremer (2015). The model presents conditions under which the expanded access resulting from free secondary education leads to changes in academic performance, depending on credit constraints and the ability level of the students induced by the program to enroll in secondary school. Incorporating child-bearing, I illustrate that free secondary education should lead to decreased levels of risky behaviors that preclude attaining further education.

2.2.1 Basic model

Consider a two-period model where a primary school graduate can either enter the labor force in period 0, or continue to secondary school and delay entry into the labor force until period 1. Preferences are over consumption in the two periods:

$$U = u(c_0) + \delta u(c_1) \tag{2.1}$$

where $u(\cdot)$ is the period utility function with $u'(\cdot) > 0, u''(\cdot) < 0$, c_t is period t consumption, and δ is a discount factor. Individuals inelastically supply one unit of labor in each period and utility is maximized by choosing to either work or attend school in the initial period. Individuals who have not gone to school can provide unskilled labor in either period and earn a wage which is normalized to 1. Skilled labor results from attending school and earns a premium on the accumulated human capital, $h(a)$, which is increasing in individual ability, a, which itself is drawn from a distribution $F(\cdot)$ with domain $A = [a_{min}, a_{max}]$. Attending school in the first period costs $p = p_t + p_f$ which is the sum of tuition, p_t, and other fees such as uniforms, p_f, and which can be borrowed at gross interest rate $R > 1$. The utility that students obtain from attending school/working are:

$$U_s(a) = u(c_0) + \delta u(c_1) = \delta u(h(a) - R \cdot p) \tag{2.2}$$

$$U_w = u(c_0) + \delta u(c_1) = u(1) + \delta u(1) \tag{2.3}$$

where initial period consumption for students is normalized to zero. Individuals compare the utility from working, U_w, against attending school, U_s, and attend school if:

$$U_s(a) \geq U_w \tag{2.4}$$

Let $a_p^\star$ be the ability level such that individuals are indifferent, at price p, between attending school and working in the initial period so that all students with $a > a_p^\star$ attain greater utility from attending school than from working in the initial period. The mean ability of students attending school in this baseline scenario is:

$$\bar{A}_p = \frac{\int_{a_p^\star}^{a_{max}} af(a)\,da}{\int_{a_p^\star}^{a_{max}} f(a)\,da} \tag{2.5}$$

Eliminating tuition in this scenario lowers the price from p to p_f. A lower price of schooling increases the utility of attending school at any ability level and serves to lower a^* so that $a_{p_f}^* < a_p^*$. Thus, in addition to those students who would attend at the full price (for whom $a \geq a_p^*$), tuition-free schooling also induces lower-ability students (for whom $a_{p_f}^* \leq a < a_p^*$) to attend school. As the only change is that lower ability students now attend secondary school, it follows that eliminating tuition necessarily lowers the mean ability of students attending secondary school:

$$\bar{A}_{p_f} = \frac{\int_{a_{p_f}^\star}^{a_{max}} af(a)\,da}{\int_{a_{p_f}^\star}^{a_{max}} f(a)\,da} < \bar{A}_p \tag{2.6}$$

I summarize the findings of this section in the following prediction:

Prediction 1. *The introduction of free secondary education will increase educational attainment and lower the average ability of students who continue through to secondary school.*

2.2.2 Credit Constraints

I now extend the model of the prior section to introduce the possibility that some students are credit constrained. Suppose that there is a mass 1 of individuals split between a fraction, w, who come from wealthy families, while the remainder, $1-w$, come from poor families.[16] Suppose also that individuals from poor families are restricted to borrowing an amount $\bar{p}(a)$, which is increasing in ability and is such that the original price of schooling precludes poor students from attending school; that is, $\forall a \in A$, $\bar{p}(a) < p$.[17] This credit constraint has no impact on students from wealthy households who attend school subject to the same ability cutoff level as the basic model. For students from poor families, the borrowing limit serves to preclude continued schooling. The mean ability level at p depends only on the ability of wealthy students attending school and is the same as the basic model.[18]

Lowering the price of schooling from p to p_f increases access and has an ambiguous impact on average ability. As in the basic model, a decrease in price allows lower-ability students from wealthy families to attend school. For students from poor families, the price decrease lowers the cost of schooling so that, with a sufficient price drop, the cost of schooling for high-ability students falls below their borrowing constraint. This increases attendance from students who were, at the original price,

[16]This setup yields the same Prediction 1 in the absence of credit constraints. Students from both wealthy and poor families would attend subject to the same ability cutoff as above: $a^{\star}_{\{p\}} = a^{\star}_{\{p,W\}} = a^{\star}_{\{p,P\}}$. A decrease in price lowers the requisite ability level for both types of students in the same fashion and Prediction 1 would follow.

[17]The idea that the borrowing limit is increasing in ability relates to the increased return to education that high-ability students receive which, in turn, makes creditors willing to lend more.

[18]While the mean ability level is the same, access is lower as students from poor families with ability levels above the cutoff are not attending school.

precluded from schooling by the credit constraints. The mean student quality after the price drop is:

$$\hat{A}_p = \frac{w \cdot \int_{a^\star_{p_f}}^{a_{max}} af\left(a\right)da + (1-w) \cdot \int_{a^\star_{cc}}^{a_{max}} af\left(a\right)da}{w \cdot \int_{a^\star_{p_f}}^{a_{max}} f\left(a\right)da + (1-w) \cdot \int_{a^\star_{cc}}^{a_{max}} f\left(a\right)da} \tag{2.7}$$

where $a^\star_{cc}$ is the lowest ability level such that poor individuals both want to, and are able to, attend secondary school.[19] The mean student ability could be lower than the original cohort if, for example, either no students from poor families are induced to go to secondary school ($a^*_{cc} > a_{max}$) or students from poor families attend subject to the same ability threshold as those from wealthy families ($a^*_{cc} = a^*_{p_f}$). In either of these cases, the impact on the mean ability of wealthy students attending secondary school indicates what will happen to the overall mean ability. In the case where no students from poor families attend secondary school, then only students from wealthy families attend school and the new, lower ability wealthy students cause a drop in the mean ability. In the case where the lower price completely eases the credit constraints and poor students attend subject to the same ability cutoff as wealthy students, then average ability among the new poor students is the same as the average ability among the wealthy students and mean ability decreases. However, as the borrowing constraint is increasing in ability, in between these two extremes cases, average ability could increase. This could happen if, for example, the price drop only eases the credit constraint for particularly high achieving students from poor families, (when $a^*_{p_f} < a^*_{cc} < a_{max}$), and if the poor are a sufficiently large

[19]This corresponds to satisfying both $\bar{p}\left(a^\star_{cc}\right) = p_f$ and $U_s\left(a^\star_{cc}\right) \geq U_w$.

proportion of the population.[20]

I summarize this credit-constrained model in the following prediction:

Prediction 2. *In the presence of credit constraints, the introduction of free secondary education will increase educational attainment and lead to an ambiguous change in the average ability of students who continue through to secondary school.*

2.2.3 A caveat on capacity constraints

If the education system can accommodate only a certain number of students and the highest-ability students who are willing to pay are admitted, the above predictions change only slightly. Without credit constraints, lowering the price of schooling serves to lower the threshold ability level for students from all families. These new students attempting to attend school are lower ability than those already in school and, with capacity constraints, will be excluded. Thus, in this case, a price decrease yields no change in average ability.

In the presence of credit constraints, however, all individuals from poor families are initially precluded from further schooling. When the price drops, high-ability students from poor families will attend school, displacing low-ability students from wealthy families. In this case, the mean ability of students increases.

[20] While I use one density, f, for students from wealthy and poor families in expression 2.7, the same argument holds if the densities differ. That is, without loss of generality, I could instead allow the ability density to differ across the populations with f_W for students from wealthy families and a different f_P for students from poor families.

2.2.4 Child bearing

I next incorporate childbirth and sexual activity into the above credit constrained framework by assuming that children arrive as a probabilistic outcome of unprotected sex.[21] Utility now depends on both consumption and the quantity of unprotected sex which yields a benefit, absent a pregnancy, of $\mu(s)$ and is additively separable from the utility of consumption. I assume that utility is increasing in unprotected sex to a certain level, $\bar{s}$, above which utility is decreasing in s: that is, $\mu'(\cdot) > 0$ for $s < \bar{s}$, $\mu'(\cdot) < 0$ for $s \geq \bar{s}$, and $\mu''(\cdot) < 0$. I assume that pregnancy itself yields a utility benefit, $B > 0$, and occurs with a probability $v(s_i)$ which satisfies $v'(\cdot) > 0$ and $v''(\cdot) < 0$. Individuals who have a child are unable to continue their schooling, so they earn the unskilled labor wage in both periods. The timing is such that individuals select a level of initial period unprotected sex, realize the pregnancy outcome, and then in the absence of a birth, select initial period schooling or labor. Individuals choose a level of unprotected sex to maximize expected utility.

As in the baseline case, there is a threshold ability level, $a^{\star}$, such that individuals from both poor and wealthy families with ability below this threshold prefer to work in the initial period rather than go to school. For these individuals, the potential arrival of a child does not change the optimal decision as they can still work in unskilled labor in the second period. As such, for these low ability individuals,

[21] This addition is an adaptation of the model of education and sexual activity in Duflo, Dupas, and Kremer (2015).

there is no expected utility cost of unprotected sex. These individuals maximize:

$$U = \max_{s} \mu(s) + u(1) + v(s)\left[B + \delta u(1)\right] + (1 - v(s))\left[\delta u(1)\right] \tag{2.8}$$

which yields the following first order condition:

$$\mu'(s) = -v'(s) B \tag{2.9}$$

which, as both $v'(\cdot)$ and B are positive, implies that these low ability individuals choose a sufficiently high level of s, denoted s_l, so that $s_l > \bar{s}$ and the marginal utility of unprotected sex is negative. These individuals set the marginal disutility of unprotected sex equal to the expected marginal utility gain from having a child.

For individuals with ability $a > a^{\star}$, it is optimal to attend school in the first period. These individuals maximize:

$$U = \max_{s} \mu(s) + v(s)\left[B + u(1) + \delta u(1)\right] + (1 - v(s))\left[\delta u(h(a) - Rp)\right] \tag{2.10}$$

which yields the first order condition that equates marginal costs and benefits of unprotected sex:

$$\mu'(s) + v'(s)\left[B + u(1) + \delta u(1)\right] = v'(s)\left[\delta u(h(a) - Rp)\right] \tag{2.11}$$

which can be rearranged to:

$$\mu'(s) = -v'(s)\,B + v'(s)\left[\delta u\left(h(a) - Rp\right) - u(1) - \delta u(1)\right] \qquad (2.12)$$

where I denote s_h as the level of unprotected sex that satisfies this condition. Equation 2.12 is similar to the optimality condition of equation 2.9 with the addition of the second term on the right. From equation 2.4, this second term is positive for high ability individuals for whom, in the absence of childbearing, schooling is the optimal decision. This indicates that $\mu'(s_h) > \mu'(s_l)$ so that the marginal utility of unprotected sex is less negative for high ability individuals than low ability individuals. As $\mu''(\cdot) < 0$, this finding implies that $s_h < s_l$; high ability individuals select a lower level of unprotected sex than low ability individuals.

For credit constrained high ability individuals from poor families, attending secondary school is not an option. Rather, these individuals maximize utility by acting as low ability individuals and selecting a high level of unprotected sex. Lowering the cost of schooling from p to p_f allows individuals from poor families to attend school and changes their optimal behavior to incorporate the possibility of lost income resulting from a potential pregnancy. Thus, lowering the price of schooling is expected to lower the incidence of unprotected sex and decrease the rate of pregnancy by decreasing the rates for high ability individuals from poor families. This yields the following prediction:

Prediction 3. *The introduction of free secondary education will decrease risky behaviors that preclude additional schooling.*

2.2.5 Model predictions and implications for analysis

I now summarize the predictions of the above model. The introduction of free secondary education will increase educational attainment and decrease risky behaviors. It will also have an ambiguous effect on average student ability depending on the presence of credit constraints. Without credit constraints, the average ability will decrease. With credit constraints, the average ability can increase, decrease, or stay the same.

Free education is likely to impact both average ability, as modeled above, and the quality of educational resources. Without a large accompanying program to increase resource quantity or quality, free education is likely to dilute educational resources. As a result, the impact on student achievement is a combination of this negative impact on resource quality, together with an unknown impact on mean ability. A positive or null effect on mean achievement implies an increase in mean ability sufficient to overcome any negative impact of diluted resource quality and is indicative of the presence of credit constraints. I take these predictions to the data in Sections 2.5-2.6.

2.3 Kenya's Education System and Free Secondary Schooling

In 2003, the Kenyan government implemented a free primary education policy covering the 8 first years of schooling.[22] This was followed up by the passage of a

[22] Lucas and Mbiti (2012a) describes the introduction of the free primary education program in their evaluation of the short term impacts of the program.

free, 4 year, secondary education (FSE) policy in January 2008. The FSE program covered basic tuition expenses of KSh10,265 (∼USD100) annually and was aimed at increasing access to secondary schools. In conjunction with the FSE policy, the Kenyan government also implemented policies designed to increase the capacity of public secondary schools. The government sought to increase class sizes from 40 to 45 students and increase the standard number of classes per grade per school to a minimum of three (Ministry of Education, 2008a). The introduction of the FSE program coincided with a rapid expansion in the number of students attending secondary school. Figure 2.1 shows the number of students entering secondary school in each year and demonstrates the rapid growth in admissions which started following the introduction of FSE and which has continued in recent years.

FSE was implemented as a capitation grant disbursed directly to schools from the central government in three payments each year. The capitation grant was not designed to cover all costs of attendance and students were still responsible for costs of school uniforms as well as infrastructure and boarding fees.[23] The capitation grant was not available to students attending private schools which, as Figure 2.A.2 shows, are generally lower performing than public schools. Using data from the 2005 Kenya Integrated Household Budget Survey, Glennerster, Kremer, Mbiti, and Takavarasha (2011) estimate that households spent an average of KSh25,000 per secondary school student with approximately KSh10,000 going towards non-tuition expenditures. These calculations suggest that the capitation grant covered approx-

[23] Ministry of Education, Science and Technology (2014b) notes that infrastructure fees are capped at KSh2000 (∼USD25) per year. Approximately 10% of students attend premium tier public schools where the FSE policy did not completely defray the higher tuition these institutions are allowed to charge.

imately 40% of the household cost of a secondary school student.[24]

At the conclusion of both primary school and secondary school, students take a set of standardized exams: the Kenya Certificate of Primary Education (KCPE) is used to determine admission into secondary school while the Kenya Certificate of Secondary Education (KCSE) determines admission and funding for higher education and is also used as a credential on the labor market. The exams are conducted by a national testing organization and are centrally developed and graded. Admission to public secondary schools is obtained through a central mechanism that allocates students based on KCPE scores and student submitted preferences over schools.[25] The Kenyan school year follows the calendar year so that students in the first FSE cohort took the KCPE in November 2007 and decided whether or not to continue to secondary school in February 2008.

Within the context of the model presented in the prior section, without credit constraints, this expansion in access would open up additional slots for lower performing students, decreasing the average ability of students attending secondary school. Alternatively, with credit constraints, the policy would potentially allow both high and low ability, credit constrained individuals to attend school, yielding an ambiguous change in the average ability of students.

[24]In the 2016/2017 budget, FSE was allocated 1.9% of the total national budget (∼USD320 million).

[25]Students list separate preferences over schools in each of the three public school tiers: national, county, and district.

2.4 Data

This paper uses two main datasets: the 2014 Kenya Demographic and Health Survey (DHS) and an administrative dataset of secondary school completion examination results.

2.4.1 Demographic and Health Survey

The 2014 Kenya DHS comprises two survey instruments which were administered to slightly different samples: a short module was administered in all sample households and is representative of females aged 15-49 at the county level while a full module was administered to males and females in every other sample household. The short module includes questions on education, health, and child-bearing histories. The additional modules include questions on income-generating activities, spousal education and employment, desired fertility, and contraceptive usage.

To focus on individuals who were both near the first FSE cohort as well as those likely to have completed schooling by the time of the survey, I restrict attention to DHS respondents born between 1983 and 1996 and who are at least 18 years old at the time of the survey.[26] In my analysis of the impact of FSE, I focus on the 13,605 individuals who have completed primary school.[27]

[26] In Section 2.4.3, I use administrative registration data to show that students born after 1990 likely made their secondary education decision in the free secondary education regime.

[27] Focusing on individuals who completed primary school introduces the potential for selection bias if the free secondary education policy changes whether individuals choose to complete primary school. I discuss the validity of this approach and evaluate the robustness of my results to relaxing this restriction in Section 2.5.1 and Appendix 2.B.3.

Summary statistics are presented in Table 2.1.[28] As the DHS disproportionately targeted women, the sample is 71% female. The average individual has slightly more than 10.5 years of education, 65% have some secondary schooling and 42% have completed secondary school.[29] Within the sample, the average ages of first intercourse, marriage, and birth for women are all between 17 and 19. This contrasts with men, for whom there is a 6 year gap between age of first intercourse and age of first marriage. A little over one quarter of the sample reports not working. Of those who are working, the majority are in unskilled work while an equal amount report either agricultural or skilled work.

Panel B restricts the sample to individuals who have completed secondary school demonstrating, in the cross section, the later fertility behavior ages. Relative to the full sample, women are over 1 year older at age of first intercourse, and 1.75 years older at age of first birth and age of first marriage. Smaller delays are seen for men who complete secondary school among whom age of first intercourse is about 0.5 years later and age of first marriage is 0.8 years later. For employment outcomes, individuals completing secondary school are slightly less likely to report no work than the full sample. There is, however, a noticeable shift across sectors: secondary school completers are about 35% less likely to report working in agriculture relative to primary school completers and almost 60% more likely to report skilled work.

[28]Summary statistics for the full DHS are presented in Appendix Table 2.A.1.

[29]The high disparity between any secondary schooling and the secondary school completion rates may be due to younger members of the sample still being in school; while the survey does not ask whether individuals are still in school, 58% of the sample aged 20 or under have some secondary schooling but have not completed secondary school while the number drops to 25% for those over age 20. If individuals initially have a noisy signal about their own ability and can gain information by attending secondary school, FSE may lead to more students trying and quitting secondary school, as it lowers the cost of gaining more information for marginal students.

There is no change in likelihood of unskilled labor.

2.4.2 Administrative Test Scores

This paper also uses an administrative dataset of all students who took the KCSE between 2006-2015 with the exception of the 2012 cohort.[30] The KCSE is a national test administered at the conclusion of secondary school and is used as a credential in the labor market as well as for admissions decisions into tertiary education. Each student must take a minimum of 7 exams across four subject categories: three compulsory subjects (English, Kiswahili, and math), 2 sciences, 1 humanities, and 1 practical subject.[31] Each subject is graded on a 12(A)-1(F) scale with a maximum total score of 84 points.[32] Each student is assigned an aggregate grade between A and E based on their composite score.[33] Detailed subject grades are available from 2009 to 2015 while only the overall letter grades are available prior to 2009. I standardize grades within each year to account for small differences across years in the grade distributions.[34] As my identification strategy exploits variation in county level exposure, I exclude national tier schools that draw students from around the

[30]The test data are a combination of publicly available data from 2006-2010 together with data scraped from the national examination council's website for 2011 and 2013-2015. The national examination council web site did not have the 2012 results publicly available.

[31]Science options include biology, chemistry, and physics. Humanities options are history/government and geography. Practical subjects include Christian religious education, Islamic religious education, Hindu religious education, home science, art and design, agriculture, woodwork, metalwork, building construction, power mechanics, electricity, drawing and design, aviation technology, computer studies, French, German, Arabic, Kenyan sign language, music, and business studies.

[32]The grading scheme has plus and minus and decreases by one point for each grade type so that 12 is equivalent to A, 11 is equivalent to A-, and so on.

[33]Overall KCSE grades are assigned as follows: a score between 84 and 81 is an A, 80 to 74 is an A-, 73 to 67 is a B+, 66 to 60 is a B, 59 to 53 is a B-, 52 to 46 is a C+, 45 to 39 is a C, 38 to 32 is a C-, 31 to 25 is a D+, 24 to 18 is a D, 17 to 12 is a D- and below 12 is an E.

[34]Similar results are obtained with the raw test data.

country, restricting attention to schools that primarily cater to the local county population.[35]

Each student record within the dataset is identified by a 9-digit student number that is unique within each year. The first three digits of the student number indicate the student's district, the second three identify the school within a district, while the last three denote the student within the school. Additional data on school characteristics come from the Ministry of Education's Kenyan Schools Mapping Project conducted in 2007, the National Examination Council's testing center public/private categorization for 2015, and an individual level examination results panel for a single cohort who took the KCPE in 2010 and the KCSE in 2014. Table 2.2 presents selected summary statistics for the examination data.

2.4.3 Defining Treatment

Each of the two datasets contains different information about individual exposure to FSE and requires slightly different treatment definitions. The DHS includes year and month of birth so exposure can be defined based on birth cohort.[36] Conversely, the administrative examination data do not include comprehensive year of birth data and require a treatment definition based on examination cohort.

I first consider how to define treatment based on the birth cohort available in the DHS data. The first cohort able to make their secondary schooling decision after FSE was implemented completed primary school in 2007. The Kenya National

[35] There were 18 national tier schools in 2011.

[36] The DHS does not include comprehensive schooling histories which would indicate who completed primary school in the FSE period, and clearly delineate treatment.

Examinations Council (KNEC) calls for students to take the primary school completion examination between ages 13 and 14 suggesting that the first cohort was born in 1993 and 1994.[37] However, rates of grade repetition are high for primary school students in Kenya: registration data for the 2014 primary school completion examination indicates that only 40% of the students who took this exam were in the 13-14 age range, while over 40% were aged 15-16 years old, and 16% were aged 17 or older.

Figure 2.2 examines the implications of the observed age distribution for cohort level exposure to FSE. The histogram in the figure plots the age distribution of the first cohort impacted by FSE, where I assume that the age distribution of the students who complete primary school in 2007 follows that of the 2014 cohort.[38] The scatter plot depicts the fraction of each cohort exposed to FSE assuming the same distribution of cohort exposure in subsequent exam years. For example, the oldest students in the first FSE cohort were aged 19 and were the last of their cohort to complete primary school. Therefore, the remainder of their cohort completed primary school before FSE and these 19 year olds are the only ones from their cohort

[37] The official entrance age to primary school is 6. The KNEC age range assumes that students start primary school at age 6 and continue through the 8 years of primary school with no grade repetition. This yields a November exam cohort of 13-14 year-olds.

[38] Ideally, I would use a pre-period cohort to examine the age distribution but I do not have the necessary data. I do, however, have data from 2008 for 15 of the 47 counties which together account for approximately 37% of the population. While this is in the FSE period, there are a number of reasons to expect that the distribution is indicative of a pre-FSE cohort. First, students retaking the exam are given identifiable registration numbers so that I can focus only on first time test takers. Second, as students take the exam after completing primary school, only students who either dropped out after 7th grade or who completed primary school but did not take the KCPE could be endogenous first time takers in 2008 in response to the program. I expect these groups to be small as there are likely frictions to returning to school and because the KCPE results are used as a credential on the market, most students who reach the exam are likely to take it. With this in mind, Appendix Figure 2.A.1 compares the age distribution of first time test takers in these regions in 2008 and 2014. There are not substantive differences in the age distributions suggesting that the 2014 cohort provides a valid indication of cohort FSE exposure.

exposed to FSE. Conversely, the youngest students in the first FSE cohort were aged 12. These students were the first in their cohort to complete primary school and were in the FSE regime, so that all other students in their cohort were also in the FSE regime. For cohorts of students between ages 12 and 19, some students completed primary school before FSE while others completed primary school after FSE was introduced. The figure suggests three general treatment intensity periods based on student age in 2007: almost all students aged 14 or under in 2007 had access to FSE and I consider these cohorts treated (born in 1993 or after). Around half of the students aged 15 or 16 (born in 1991 or 1992) were exposed to FSE and I consider these cohorts treated in most specifications, although this may reduce power. Only a small fraction of students who were aged 17 or older (born in 1990 or before) were exposed to FSE, and I consider these students untreated.[39]

In my analysis of the impact of FSE on student achievement, I consider treatment based on examination cohort as the administrative examination data does not have the year of birth for all individuals. FSE was first available for students who entered secondary school in 2008 who would, without grade repetition, take the KCSE in 2011. Grade repetition is a potential threat to identification using this definition of treatment. If students entered secondary school in the pre-FSE 2007 cohort but then took five years to complete secondary school, I would consider them treated. Grade repetition within secondary school is, however, relatively low; KCSE registration data show that almost 80% of students proceed through secondary school in

[39]Appendix Table 2.A.4 presents a robustness check of the results where the transition cohorts are excluded.

4 years.[40] As such, I consider students who take the secondary school completion examination in 2011 or later as treated and those who take the exam in 2010 or earlier as untreated.[41] This analytical choice, in the worst case, should only slightly bias my results towards zero.

2.5 FSE and educational attainment

2.5.1 Identification strategy

I measure the impact of FSE on educational attainment by exploiting cohort-region variation in exposure to the program using a difference-in-differences approach. Cohort variation arises from the timing of the program: individuals above secondary schooling age at the time of the program's implementation in 2008 would have had to return to school to take advantage of FSE rather than simply continue their schooling from primary to secondary school. Regional variation stems from heterogeneous pre-program primary to secondary transition rates across Kenya's 47 counties. In counties with high pre-FSE primary to secondary transition rates, there were relatively few students who could be induced by the program to continue to secondary school. In contrast, in counties with low pre-FSE primary to secondary transition rates, there was a relatively large number of primary school graduates who could be

[40] Appendix Figure 2.A.3 presents a histogram of the number of years between primary school and secondary school completion examinations for students in the 2014 KCSE cohort.

[41] Students who take the exam between 2008 and 2010 are marginally exposed to FSE in that their school fees were removed which may lead to higher persistence in secondary school. I focus on treatment as inducing students to change their secondary schooling decision so I consider these years untreated as students in these cohorts entered secondary school in the pre-FSE regime.

induced by the program to continue to secondary school.[42]

Based on the program cohort exposure described in Section 2.4.3, I use the DHS data to calculate the pre-program primary to secondary transition rate for each county. The rate is calculated as the fraction of primary school completers who attend some secondary school for cohorts born in the two closest pre-FSE periods.[43] Figure 2.3 shows how the transition rates have evolved over time for counties with high/low pre-FSE transition rates. While areas with high and low pre-program transition rates moved similarly before the introduction of FSE, with a noticeable gap between the two, the rates converged following the introduction of FSE.

Figure 2.4 presents a histogram of the pre-program primary to secondary transition rates across counties showing that the rate ranges from 0.34 (Kitui county) to 0.94 (Mandera county) with a median of 0.659. Figure 2.5 maps the transition rates. The one obvious pattern in the figure is the high transition rate band across the North and North-Eastern portion of the country.[44] In Appendix 2.B.1-2.B.2, I confirm robustness of the results to excluding the smallest population counties,

[42] Using the primary to secondary transition rate and focusing on a sample of primary school completers should provide additional power relative to an alternative definition based on the proportion of each cohort with any secondary schooling. However, using the transition rate approach imposes an additional assumption on the difference-in-differences estimates: that FSE did not differentially induce students to complete primary school. This assumption ensures that there is no selection bias introduced by a changing composition of the secondary school student body. I consider the validity of this assumption later in this section.

[43] I consider the average over two years to ensure that I am not calculating the transition rate from a small number of observations. 13 counties have fewer than 10 observations in the closest pre-program birth cohort. The intensity for county k is calculated as $Frac_k = \frac{m_{k,1989}+m_{k,1990}}{n_{k,1989}+n_{k,1990}}$ where n_{kj} represents the number of individuals completing primary school in cohort j and m_{kj} is the number of individuals who have attended some secondary schooling.

[44] The transition rate measures the proportion who progress to start secondary school after finishing primary school and is not related to overall educational attainment.

which includes these high-transition rate counties and also to excluding the two major cities: Nairobi and Mombasa.[45]

To motivate my approach, I first consider the impact of FSE on educational attainment within a binary treatment difference-in-differences framework. To satisfy the difference-in-differences setup, I define a binary treatment equal to one for high intensity counties, where pre-program primary to secondary transition rates are below the median level, and equal to zero for low intensity counties, with transition rates above the median level. I then consider the change in outcomes in treated regions following the introduction of FSE relative to the change in untreated regions. This identifies the program impact under the assumption that absent the program, the outcomes of treated regions would have followed the same trajectory of low intensity regions. This corresponds to a regression of the form:

$$S_{ijk} = \alpha_0 + \beta_1 \left(\text{High}_k * \text{FSE}_j\right) + \eta_k + \gamma_j + \varepsilon_{ijk} \tag{2.13}$$

where S_{ijk} reflects the schooling of individual i in cohort j in county k, High_k is an indicator variable equal to 1 if county k has a pre-program primary to secondary transition rate below the median, FSE_j is a dummy variable equal to one for individuals born in cohorts impacted by FSE, and η_k and γ_j represent county and

[45]I rerun the analysis without Kenya's two main cities, Nairobi and Mombasa, to ensure that the results are not being driven by these cities and the potential noise arising from internal migration. In the second set, I rerun the analysis without the smallest counties where the estimation of the primary to secondary transition rates are calculated based on a particularly small sample. The small counties excluded in the robustness check are Garissa, Mandera, Marsabit, Samburu, Turkana, and Wajir. A third set of robustness results employs an alternative definition of treatment intensity that varies over years allowing for earlier cohorts to be impacted by larger younger cohorts.

cohort fixed effects, respectively.[46] The interaction coefficient, β_1 is the estimate of the effect of FSE on education.

There are two main assumptions underlying this difference-in-differences approach. First, selection bias (treatment intensity) should be attributable to unchanging characteristics of the counties and second, the pre-FSE time trends across high and low transition rate counties should be the same. In assessing the first assumption, it is notable that if capacity constraints at the secondary school level are binding, then the ratio of primary school graduates to secondary school spots determines the transition rate. Without large changes in either the number of primary school graduates or secondary school capacity, the transition rate is likely to be serially correlated over time. Indeed, the data suggest that the first assumption is likely to hold: the correlation between the treatment intensity calculated using the 2 years prior to treatment and 10 years prior to treatment is 0.8. For the second assumption, Figure 2.3 demonstrates common trends going back 8 years, suggesting that differences across counties in primary to secondary transition rates are due to structural rather than transitory factors. If the assumptions are satisfied, the identification strategy differences out these structural county and cohort differences yielding a consistent measure of treatment impact.

Even under the above assumptions, another threat to my identification is that the estimated impact may be due to other programs which are correlated with my treatment intensity measure. With this in mind, I follow Lucas and Mbiti (2012a)

[46]The usual formulation of a difference-in-differences with two periods includes two dummy variables in addition to the interaction: one for the treated group and one for the post period. In the present formulation, these are subsumed by the county and year fixed effects.

and control for county development funding levels, pre-program unemployment levels, and county specific linear trends.[47] The constituency development funding levels were calculated based on the poverty incidence in 2003 and are reported annually at the sub-county level which I aggregate to the county level.[48] If areas with higher treatment intensity also received greater development funding, I may conflate the impact of FSE with the differential funding. To address this concern, I interact the development funding with cohort dummies. Similarly, I include unemployment levels interacted with cohort dummies to account for programs that potentially targeted areas with higher unemployment. I cluster errors at the county level to account for possible serial correlation within school markets over time.

Forming a binary high intensity variable from the continuous primary to secondary transition rate entails a loss of information. To use the relative magnitudes of the transition rates across counties, I define a continuous treatment intensity measure based on the transition rate as $I_k = (1 - \text{transition rate})$ which reflects the maximum potential increase in the transition rate.[49] Thus, the intensity is higher for counties with low pre-program transition rates where there is greater ability for FSE to induce students to attend secondary school. I use this treatment intensity in an analysis analogous to equation 2.13 where the binary treatment intensity variable

[47] The basic difference-in-differences approach relies on common trends across treated and comparison groups. In the above specification with heterogeneous treatment intensities, I can not test for common trends across the counties but instead include county-specific linear trends as a control to directly account for potentially confounding trends.

[48] Constituency development fund allocation data are posted on http://www.cdf.go.ke.

[49] An alternative analysis that defined treatment intensity as 1/(transition rate) yielded similar results.

is replaced with the continuous measure:

$$S_{ijk} = \alpha_0 + \beta_1 \left(\mathrm{I}_k * \mathrm{FSE}_j\right) + \eta_k + \gamma_j + \varepsilon_{ijk} \tag{2.14}$$

where I_k is the treatment intensity for county k. The coefficient β_1 is the estimate of the impact of FSE on education. This regression uses all of the primary to secondary transition rate information and its estimates should be more precise. I include the same controls in this continuous intensity analysis as in the binary treatment analysis described above.

In examining the impact on educational attainment, I focus on the sample of primary school completers as they are the group most likely induced by free secondary education to continue their schooling.[50] Identification in this framework requires that FSE did not differentially induce students to complete primary school, thereby avoiding any potential selection bias introduced by a changing composition of the secondary school student body. Table 2.3 presents coefficients from regressions represented by equation 2.14 with primary school completion as the dependent variable. All estimated coefficients are small with two of the 15 coefficients marginally significant.[51] The coefficients for the male only sample are slightly larger in magnitude. Overall, the regressions provide little evidence in support of differential increases in the likelihood of completing primary school.

I also run a falsification test that examines the impact of a hypothetical program introduced for the 1987 birth cohort: five years before the program was actually

[50] Appendix 2.B.3 presents results relaxing the restriction on primary school completers.
[51] Appendix Table 2.A.2 presents analogous results for the binary treatment analysis.

implemented. For this analysis, I use the primary to secondary transition rate in 1985-1986 to identify high/low intensity counties and calculate the continuous treatment intensity measure. This generates estimates for the impact of a hypothetical program on education levels.[52]

2.5.2 FSE and educational attainment results

Table 2.4 column 1 presents the standard difference-in-differences estimates represented by equation 2.13, where each coefficient represents the marginal impact of being in a high intensity county in the FSE period. Results are presented for both years of education (Panel 1) and completed secondary school (Panel 2). Column 2 controls for funding made available to counties by the central government through the constituency development fund mechanism and examines whether changes in schooling were related to differential funding levels by interacting the levels with cohort dummy variables. Similarly, column 3 interacts unemployment levels with birth cohorts to account for endogenous schooling expansion in response to unemployment levels. Column 4 adds a county specific linear trend to control for potentially heterogeneous pre-program trends. Column 5 controls for both county funding and unemployment as well as the county specific linear trends. The difference in primary to secondary transition rates between the high and low intensity regions is 0.21. The results suggest that moving from the low intensity average to the high intensity average led to an average increase of 0.3-0.4 years of education. The esti-

[52]In Appendix Table 2.A.3, I also run falsification tests using the basic above/below median intensity difference-in-differences framework for each of the six years 1981-1986.

mated coefficients across both genders are similar for the first 3 columns. Including county trends reveals a potential divergence where the gains in years of education for males becomes insignificant. This suggests that the earlier estimated impact for men is explained by pre-program trends. In contrast, controlling for county trends increases the estimated impact on women's education. Despite increasing average education, there is no estimated impact on the likelihood of completing secondary school across either gender. While these estimates are informative and guide the interpretation of the results, my preferred specification exploits the full variation in primary to secondary transition rates by using the continuous intensity measure.

Table 2.5 presents the difference-in-differences estimates represented by equation 2.14 for years of education (Panel 1) and completed secondary schooling (Panel 2). Results are presented for the entire sample as well as for males and females separately.[53] The consistently positive and significant coefficients across the table illustrate that free secondary education induced students in counties with low pre-program primary to secondary transition rates to continue to secondary school. While the coefficients are much larger than those presented in Table 2.4, the intensity variable has a different interpretation. While the binary intensity measure presents the impact from going from a low treatment intensity county to a high treatment intensity county, the continuous variable presents the expected gain at a given intensity level. At the mean treatment intensity value of 0.34, the average years of education are estimated to increase by about 0.8 years which is slightly

[53]Appendix 2.B.1-2.B.3 presents a series of robustness results that exclude either main cities (Nairobi and Mombasa), the smallest counties, or including individuals who have not completed primary school. The results are similar in magnitude and significance to those presented below.

larger than an estimate based on a comparable increase in the binary treatment specification. Assuming that all students complete their secondary schooling, this corresponds to inducing about 57% of students not-transitioning prior to the program to transition. The estimated coefficients are similar across all specifications. Notably, and in contrast with the simple binary treatment specification, the results for men are significant and similar in magnitude to those of the women only sample.

Panel 2 examines whether greater exposure to FSE is associated with a greater likelihood of completing secondary school. Both pooled gender and female only specifications yield coefficients around 0.15-0.25. At the average intensity of 0.34, this corresponds to a decrease in the drop-out rate at primary school completion of approximately 20%. Estimates across columns 2-5 remain similarly significant and with similar estimated coefficients with the controls added.

The gains of around 0.8 years of education at the mean intensity estimated here are considerably lower than the 1 and 1.5 years of education increase estimated by Keats (2014) for free primary education (FPE) in Uganda and by Osili and Long (2008) for FPE in Nigeria, respectively. While smaller than the estimated impacts for primary school programs, the impacts estimated for FSE in Kenya are slightly larger than those estimated for a secondary school program in The Gambia. Gajigo (2012) estimates that the girls' scholarship program led to an increase of about 0.3-0.4 years of education for female students. The smaller coefficients obtained for secondary schooling programs relative to primary schooling may reflect the higher opportunity cost of schooling at the secondary level.

I next examine the results of the falsification test examining the impact of a hypothetical program introduced for the 1986 birth cohort. Appendix Figure 2.A.4 splits the sample based on the calculated treatment intensity, suggesting reasonable common trends across the bifurcated sample. The two rates continue to share the same trend after the introduction of the hypothetical program. Table 2.6 presents the regression estimates analogous to Table 2.5 using the falsification sample and treatment intensity. The coefficients are small and insignificant reflecting the fact that the pre-FSE transition rates are not correlated with pre-FSE schooling changes.

These results together suggest that FSE led to large and significant gains in schooling. I next use exposure to FSE as an instrument to examine the impact of secondary schooling on the fertility behaviors of young women.

2.6 Education, fertility, and occupational choice

2.6.1 Identification strategy

In this section, I use the relationship between FSE and increased educational attainment established in Section 2.5 as the first stage of an instrumental variables approach to examine the impact of educational attainment on various demographic and occupational choice variables.

Figure 2.6 presents Kaplan-Meier estimates of the distribution of age of first intercourse, age at first birth, and age at first marriage, highlighting the drastically different age of first incidence among those who attend secondary school relative to those who do not.[54] By age 18 over 28% of women who had not attended sec-

[54] Kaplan-Meier figures illustrate the probability of survival across different intervals when data are censored (Kaplan and Meier, 1958). Age at first birth and marriage are reported to the nearest

ondary school had given birth compared to just 12% for those who had attended some secondary school, while by age 20, the rates were 56% and 26% for the same groups. For age of first marriage: by age 18 almost 33% of women who had not attended secondary school were married compared to just 11% for those who had some secondary schooling. Results from OLS regressions examining the age of first incidence for these behaviors are reported in Appendix Table 2.A.5 with large estimated impacts.[55] However, relating education to these variables directly is fraught with endogeneity.[56] Using the demonstrated relationship between FSE and increased educational attainment developed in the prior section, I use exposure to the FSE program as an instrument for education in regressions examining the impact of education on women's fertility decisions. This corresponds to estimating equations of the form:

$$S_{ijk} = \alpha_1 + f\left(I_{ijk}\right) + \beta_1 X_{ijk} + \eta_{1k} + \gamma_{1j} + \varepsilon_{ijk} \tag{2.15}$$

$$P_{ijk} = \alpha_2 + \xi_2 \hat{S}_{ijk} + \beta_2 X_{ijk} + \eta_{2k} + \gamma_{2j} + \upsilon_{ijk} \tag{2.16}$$

where the endogenous level of schooling S_{ijk} is instrumented using the exposure to FSE, $f\left(I_{ijk}\right)$, which depends on county and year of birth. In running this analysis, only the interaction instruments of the first stage are excluded from the second stage. I include religion and tribe demographic variables as covariates. The identifying

month while age at first intercourse is reported at the year level.

[55] Similarly, summary statistics presented in Section 2.4.1 illustrate large differences across educational attainment in labor market sector.

[56] Omitted variables such as ability and discount rates are likely to be correlated with both education and childbearing decisions introducing bias into OLS estimates.

assumption in this instrumental variables approach is that FSE had no direct effect on the fertility variables other than through its effect on educational attainment.[57]

Figure 2.7 presents the coefficients on the interactions between county intensity and year of birth in a regression where the dependent variable is years of schooling. These coefficients are approximately equal to 0 prior to the implementation of FSE at which time it jumps to a positive and significant coefficient. All of the subsequent interaction coefficients are positive and significant. As expected, the intensity measure is correlated with attainment gains following the FSE introduction but had no measurable effect on the education of cohorts who reached secondary school age before the program was implemented. With this figure in mind, I define my instrument as the interaction between an indicator variable for the post period and the county intensity measure.[58] Thus, the instrument is defined as:

$$f\left(I_{ijk}\right) = \xi_1 \left(I_k \times post_j\right) \tag{2.17}$$

where $post_j$ is an indicator variable equal to one for cohort impacted by FSE.

I examine the impact of educational attainment on a number of key demographic variables: age of first intercourse, age of first birth, age of first marriage,

[57]For FSE exposure to serve as a valid instrument, two assumptions must hold. First, FSE must impact educational attainment. The results presented in Section 2.5 indicate that this is so. Second, the exclusion restriction must hold. This requires that conditional on covariates, FSE must only impact the fertility variables through its impact on educational attainment. The exposure of an individual to the program was determined by the individual's year and region of birth. After controlling for region of birth and cohort fixed effects, the interactions between cohort indicator variables and county intensity measures are plausibly exogenous variables, and are used as instruments in the fertility and labor market equations.

[58]Appendix tables 2.A.6-2.A.9 present the results including interactions between individual birth cohorts and the county intensity measure. The results are consistent with the main results presented below albeit with a slightly weaker instrument.

desired fertility, and contraceptive use. For each of the age variables, I examine the impact of schooling on the probability of doing each of these activities before ages 16, 17, 18, 19, and 20. I report results for both genders pooled and for women only.[59]

I use the same approach to examine the impact of education on labor market outcomes including whether individuals are working and the sector of work. As my sample includes individuals as young as 18, it is likely that some younger members of the sample have not yet entered the labor market. This would increase the proportion reporting no work and potentially underestimate the impact on working in the professional sector. With this in mind, I progressively restrict the sample to older and older cohorts to try and focus on individuals who are unlikely to still be in school. This yields three sets of results; one for individuals aged 18 and over, another for individuals 19 and over, and a final set for those aged 20 and over.

2.6.2 Impacts of education on fertility

Figure 2.8 and Table 2.7 present the coefficients from the instrumental variable estimates for the probability of first intercourse, birth, and marriage before each teenage age. The coefficients for first intercourse are all negative with the magnitude of the estimated coefficients increasing as the age cutoff increases. Each additional year of education is estimated to decrease the probability of having first intercourse at age 16 by around 3 percentage points, rising to almost 8 percentage points by age

[59]For the male only sample, which is smaller, the instrument fails to satisfy the Staiger and Stock (1997) recommendation that the F-statistic exceed 10.

18 and 18 percentage points by age 20 on base rates of 23%, 46% and 70% suggesting a decrease of between 10-25% at each age. The results appear to be driven by large effects for women as the coefficients for the pooled sample where men are included are lower than the women's only sample. Larger impacts are estimated for age of first marriage. The coefficients for women indicate a decrease of about 59% in the likelihood of being married before age 18 and of 47% for being married before age 20. The results for age of first birth are also consistent across ages with the estimated coefficients indicate a decreased likelihood of having a first child by age 16-20 of 30-50%.[60]

Taken together, the estimated coefficients suggest large and significant impacts of education on delaying these fertility behaviors. The estimated impacts on age of first birth are slightly larger in magnitude than those estimated by Ferré (2009) who, in her analysis of adding an additional year of primary schooling in Kenya, concludes that an additional year of education decreases the probability of teenage childbearing by 24-29%. In his analysis of the secondary school admissions discontinuity in Kenya, Ozier (Forthcoming) estimates that secondary schooling almost completely eliminates teen pregnancy. While my instrument is too weak to examine the impact of completing secondary school on teenage pregnancy, the estimated coefficient on each additional year of education suggests that a similar elimination of teenage pregnancy would arise from a full four years of secondary education. Using the introduction of free primary education in Uganda, Keats (2014) obtains slightly

[60] Appendix Table 2.A.5 presents coefficients from OLS estimates of the impact of education on age of first intercourse, age of first birth, and age of first marriage. The IV estimates are universally larger than the OLS estimates suggesting downward omitted variable bias due to negative correlation between family variables, such as income, and fertility behaviors.

smaller estimates: each additional year of education decreases the probability of first birth at each age between 16 and 20 by between 5-20%. By contrast, while I estimate large and significant negative impacts of education on age of first intercourse, Keats (2014) finds evidence that an additional year of education increased the likelihood of intercourse by age 18, and estimates insignificant coefficients for other ages. The apparent incongruity of the Uganda and Kenya results may be due to the different policies examined and the different ages at which they keep students in school: free primary education is likely to induce students to remain in school through their middle teenage years while free secondary education is likely to induce students to remain through their late teenage years.

Ferré (2009) details three main mechanisms through which education may delay child bearing: a "knowledge" effect where more educated individuals are better informed about contraception, an "autonomy" or empowerment effect where women shift their preferences towards fewer, higher quality children, and an "incarceration" effect whereby students either spend time in school and therefore have less time to get pregnant or they may delay childbearing to finish their schooling.[61] All of these effects are potential avenues through which education could also impact age of first intercourse and age of first birth. If an incarceration effect is the primary mechanism through which education delays childbearing then the different impact patterns on age of first birth estimated in this paper and Keats (2014) could be due to the differing sources of variation: primary education may only impact behaviors around

[61] Students who get pregnant in Kenya are often asked to leave school. While Ministry of Education guidelines stipulate that students can remain in school while pregnant and return to school post-pregnancy this does not always occur in practice.

the primary age range and impacts decreasing in the late teenage years. In Tables 2.8-2.9, I investigate whether educational attainment changed behaviors or beliefs corresponding with the first and second of the three mechanisms.

Table 2.8 examines the impact of education on reported contraceptive usage finding no evidence of increased contraceptive use or access. Table 2.9 examines the impact of education on desired fertility which is marginally significant for the pooled sample and negative and insignificant for the female sample. The pooled estimate of -0.2 desired children is similar to that estimated by Keats (2014), who found evidence of a large (-0.3) and significant impact. The lack of significant impacts for women on proxies for both the knowledge and autonomy effects suggests that the incarceration effect may be the dominant mechanism of behavior change. As FSE primarily impacted day schools where students return home in the evenings and over weekends, it seems likely that the measured impacts are not attributable to a direct confinement effect associated with being separated from individuals of the opposite gender, but rather to individuals choosing to delay intercourse to ensure that they can continue their schooling.

2.6.3 Impacts of education on occupational choice

Table 2.10, Panel A presents analogous instrumental variables estimates examining the impacts of education on women's sector of work. The results indicate an increased likelihood of skilled work and a decreased likelihood of agricultural work. The estimates of decreased agricultural work may be attributable to a delayed transition to the labor market as many of the younger women in the sample might still be in school, which would potentially inflate the proportion reporting no work. If individuals exit secondary school and enter agricultural work, the presented coefficients overestimate the negative impact on agricultural work. Panel B and Panel C restrict the sample to slightly older populations to try and decrease the proportion reporting no work due to continued schooling. As expected, the older samples are less likely to report no work. While the estimated impact of education on agricultural work decreases slightly in the older samples, the estimated coefficient remains large in magnitude and significant with no corresponding decrease in impact on skilled work. This suggests that individuals are less likely to work in agriculture and more likely to have skilled work. These findings for women complement those of Ozier (Forthcoming), who found that secondary schooling for men decreased low-skill self-employment and may have increased formal employment.

While the positive impact I find on skilled work is likely a lower bound, as it may grow stronger as the sample ages, this shift towards skilled work might not yield the growth benefits if it comes as a result of signaling rather than an increased stock of cognitive ability (Hanushek and Wößmann, 2008). However, Ozier (Forthcoming) presents evidence that secondary schooling in Kenya increases human capital. His estimates are valid for the inframarginal students that may be impacted by FSE, suggesting that the sectoral shifts are likely not purely the result of signaling, but also of increased human capital.

2.7 FSE and student achievement

2.7.1 Identification strategy

I next examine the impact that FSE had on student achievement by exploiting the differential exposure to FSE, and the associated differential expansion, across counties. As modeled in Section 2.2.1, a decrease in the cost of schooling will lead to a decrease in student achievement unless high-performing students are credit constrained prior to the program. With this in mind, my analysis of the impact of FSE on student achievement is a test of credit constraints: an increase in student achievement indicates that credit constraints precluded high-ability students from further schooling.

As described in Section 2.4.3, I consider cohorts of students who made the secondary school entrance decision after the program was announced as treated – the first cohort entered secondary school in 2008 and subsequently took the secondary school completion examination in 2011. With this in mind, I set the treatment intensity to zero for cohorts prior to 2011.[62] I first examine the impact of FSE on overall student achievement by running a regression analogous to equation 2.14 examining student performance on the secondary school completion examination:

$$T_{ijk} = \alpha_0 + \beta_1 \left(\mathrm{I}_k * \mathrm{FSE}_j\right) + \eta_k + \gamma_j + \varepsilon_{ijk} \tag{2.18}$$

[62] Appendix 2.B.4 presents the results of an alternative analysis where I assume that older cohorts are impacted by larger younger FSE cohorts and the early FSE cohorts are less impacted due to smaller cohorts ahead of them.

where T_{ijk} is the normalized test score of individual i in cohort j in county k. As described in the model, this regression will conflate a dilution of resource quality with a changing composition of the student body. If the estimated coefficient of β_1 is zero, then counties that expanded their secondary schooling levels more saw no change in their average performance. With a dilution of existing school resources, this implies that the average student ability increased which indicates that students were credit constrained.[63] A negative coefficient on average achievement confounds a dilution of school resources with new student quality and I am unable to determine the impact of FSE on average student ability. The results from this test, shown below in Section 2.7.2, are able to rule out large negative impacts.

I then follow Lucas and Mbiti (2012a) and Valente (2015) and examine the impact of the program on students who likely would have continued through to secondary school even in the absence of the program. For these students, there is no change in the composition of the student body so that any measured impact on academic achievement should be restricted to arise only from the dilution of resources.[64] While the preceding analysis suggests that FSE eased credit constraints allowing both high and low performing students to continue their schooling, for this analysis,

[63]Redistributing existing resources, such as teachers, across counties would mitigate the dilution of resources and bias my estimates down. If the program targeted a select population or region, additional resources could be diverted and offset the dilution with only limited impact on the resources available to the non-treated population. FSE was implemented at a national level and thus the estimates are appropriate in incorporating any intentional redistribution of resources across counties. Empirically, data from Ministry of Education (2008b) and Ministry of Education, Science and Technology (2014a) do not provide evidence of a redistribution of teachers: the growth in number of teachers at the regional level is negatively correlated (-0.5) with the mean regional intensity and only weakly positively correlated (0.1) when excluding three North-Eastern Kenya counties which experienced a large relative increase in teachers (Garissa, Mandera, and Wajir).

[64]Lucas and Mbiti (2012a) and Valente (2015) both assume that additional students are lower performing and use the changes-in-changes approach of Athey and Imbens (2006) to measure the impact across the upper-half of the distribution.

I assume that the highest performers would have been able to attend secondary school even in the absence of the program by raising funds through family or village networks.[65] The analysis requires that I identify a sufficiently high-performing sample, based on KCSE score, for whom the introduction of FSE was unlikely to change their schooling decisions. I use the one cohort of primary school graduates for whom I have both KCPE and KCSE data to examine the relationship between primary school and secondary school completion examination results. Figure 2.A.5 shows the proportion of students who completed secondary school in either 2014 or 2015 broken down by their score on the 2010 KCPE. In this period, 90% of the students who score over 290 points (out of 500) complete secondary school within 5 years suggesting that the remaining uniform, book, and facilities fees are deterring, at most, 10% of students from continuing to secondary school. I define my sample so that no more than 5% of students are expected to have KCPE results below this value; 95% of students who scored 64 or above on the KCSE also scored above 290 on the KCPE and constitute a little less than 10% of the test taking body. I therefore restrict attention to the highest performing 10% across counties and assume that these students are sufficiently high performing that they would have been able to raise the requisite funds for schooling in the absence of FSE.[66] The identification

[65]Ideally, I would like to examine the likelihood, by primary school completion examination grade, that students who completed primary school in 2007 continued on to secondary school. If almost all students who scored above some mark proceeded to secondary school then I could examine the impact of FSE on the relative performance of students who scored above the mark without composition effects of additional students induced to attend secondary school. Unfortunately, I do not have matched primary and secondary school completion examination results for all years and can only examine the likelihood of completing secondary school for those who sat for the primary school examination in 2010. This is in the post-FSE period which will inflate the likelihood that students of any score proceed to secondary school but which I assume is indicative of the pre-FSE period.

[66]The cutoff for funded admission to universities, which varies year-to-year, has historically been

assumption to examine the impact of resource dilution on academic outcomes within the sample is that the schooling decisions of students who scored in the top 10% of their county were unaffected by the FSE program.[67] While I can also examine the impact at lower performance levels, these estimates are more likely to conflate resource dilution together with potential composition changes.

I also examine the impact on the top 10% of the distribution using the changes-in-changes (CiC) approach of Athey and Imbens (2006). The CiC model is a generalization of the difference-in-differences estimator that estimates the entire counterfactual distribution of a treated group which is identified under the assumption that the changes in the distribution of the treated and comparison groups would, absent treatment, be the same. The standard estimator considers the impact of a binary treatment across two time periods. I consider the pre- and post-FSE periods and compare students in counties exposed to a treatment intensity above the median to those in counties below the median intensity level. The treatment effect at quantile q is calculated as:

$$\tau_q^{CiC} = F^{-1}_{Y^1,11}\left(q\right) - F^{-1}_{Y^N,11}\left(q\right) = F^{-1}_{Y^1,11}\left(q\right) - F^{-1}_{Y,01}\left(F_{Y,00}\left(F^{-1}_{Y,10}\left(q\right)\right)\right) \tag{2.19}$$

where $F_{Y^1,gt}$ is the cumulative distribution function of group g in time t. The CiC model imposes three main assumptions.[68] First, the potential test scores of

around 64 points.

[67] I am currently seeking an analogous probability of reaching the KCSE by KCPE score for the pre-FSE period.

[68] These are laid out in Athey and Imbens (2006) Assumption 3.1-3.3. An additional common support assumption (Athey and Imbens (2006) assumption 3.4) requires that outcomes of the treated group in any period be a subset of the untreated outcomes.

untreated individuals $\left(KCSE_i{}^N\right)$ should satisfy:

$$KCSE_i{}^N = h\left(A_i, T_i\right) \tag{2.20}$$

where A_i is an underlying unobserved ability and T_i is the time period in which the test was taken. Second, CiC imposes a strict monotonicity framework that the test score production function $h\left(A_i, T_i\right)$ be strictly increasing in A. Third, the underlying ability distribution within a group can not vary over time:

$$A_i \perp T_i | G_i \tag{2.21}$$

Focusing on the entire sample of students who sat for the KCSE exam in the post-FSE period would violate this assumption as FSE would likely have induced not only the credit constrained students to continue their schooling but also lower-ability students for whom FSE changes their optimal schooling decision. As described above, I try to satisfy the requirement that the underlying ability distribution within a group not vary over time by restricting my focus to the highest performing students. I control for county fixed effects and county linear trends following the parametric approach suggested by Athey and Imbens (2006).

2.7.2 FSE and student achievement results

In examining the impact of FSE on student achievement, I first confirm that the growth in secondary school students is evident in the secondary school completion examination results. Table 2.11 presents the estimates of the regression represented by equation 2.18 where the dependent variable is the number of students who sat for the secondary school completion examination relative to pre-FSE levels. The results show that more intensely treated counties saw larger increases in the number of test takers. The gains in test takers are robust to controlling for potentially confounding programs and funding levels. The estimated impacts are similar gains for males and females, and are also similar in magnitude to the statistically insignificant gains estimated above using the DHS data.

Confirming that the FSE growth is evident in the test data, I next consider the impact on average test scores. Table 2.12 Panel A presents results examining whether average test scores decreased in areas that more intensely treated. The pooled results presented in columns 1-2 indicate small impacts as the estimated coefficients of -0.007 and 0.07 standard deviations suggest that at the average intensity of 0.34 the estimated impact is about 0.02 standard deviations. To put this coefficient into perspective, I run a simulation to estimate the effect of the program without credit constraints where I assume that the program induced lower-ability students to continue their schooling. This simulation yields an estimated impact of about -0.3 standard deviations.[69] This null result is indicative of credit constraints, as the negative impact of a dilution in educational resources requires an increase in average ability to yield no overall impact.

With the near zero estimated coefficients providing evidence in support of

[69]Appendix C describes the simulation and Appendix Table 2.C.1 presents the estimated treatment coefficients.

the presence of credit constraints, I next consider the impact of the program on the scores of students at the very upper end who may have taken the exam in the absence of FSE. Panel B of Table 2.12 presents the results associated with equation 2.18 where the sample is restricted to individuals who scored above 64 on the KCSE. The results rule out large negative impacts and are suggestive of positive impacts.

Finally, Table 2.13 uses the changes-in-changes methodology to examine whether FSE differentially impacted students across the top of the score or ability distribution. One of the nine coefficients is significant at the 10% level with all estimated coefficients relatively small. The estimated impacts here and above together suggest limited impacts of school access expansion on academic achievement. This finding is in line with that of Lucas and Mbiti (2012a) who find evidence of, at most, small impacts of free primary education in Kenya. While I can not fully rule out positive impacts, the non-negative impacts are in line with those of Blimpo, Gajigo, and Pugatch (2015) who find that free secondary education for girls in The Gambia increased test scores.

2.8 Conclusion

In early 2008, the Kenyan government implemented a free secondary education program. The program increased educational attainment for primary school completers by approximately 0.8 years. This paper uses differential exposure to the program, in an instrumental variables framework, to present new evidence on the impact of education on a range of demographic and labor market outcomes.

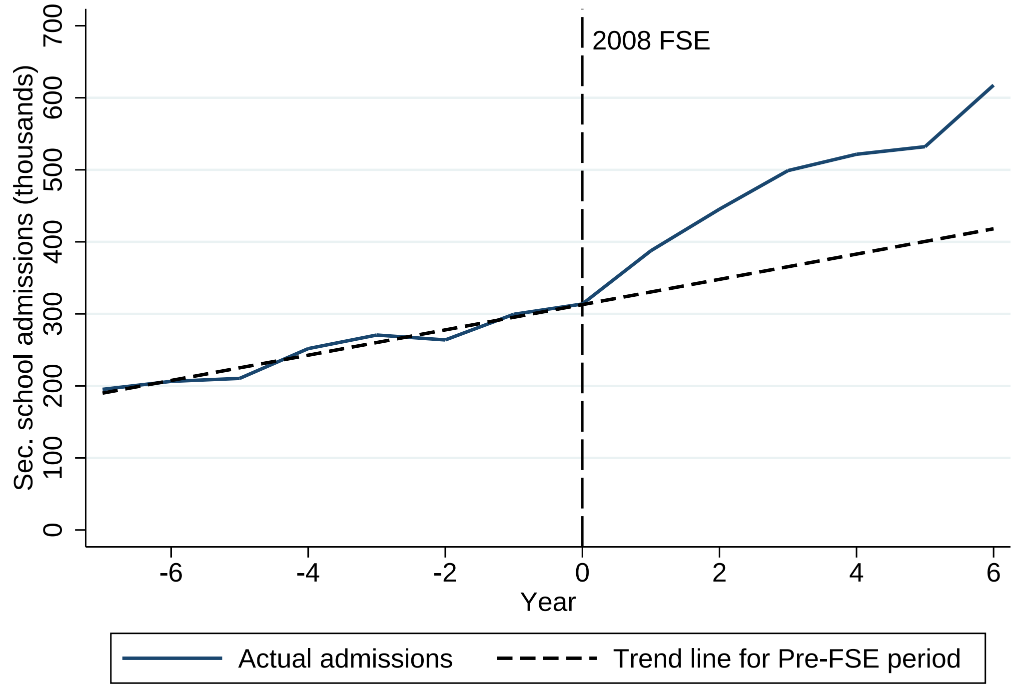
I find that secondary schooling has broad impacts on fertility behaviors. Secondary schooling decreases the probability of first intercourse at all ages between 16 and 20 by around 25%, decreases the probability of first marriage at all ages between 16 and 20 by around 50%, and decreases the probability of teenage childbearing by 37%. Despite these impacts, I find no evidence that secondary schooling decreases desired fertility or increases modern contraceptive use. This suggests that free education decreases risky behaviors that could potentially preclude continued schooling. These demographic impacts suggest a potentially large additional benefit of the program, as delayed fertility behaviors are associated with significant benefits for both the mother and child (Ferré, 2009; Schultz, 2008).

I also find that post-primary schooling shifts young women across labor market sectors. Education increases the likelihood of work in skilled labor by 28% and decreases the likelihood of working in agriculture by almost 80%. These findings for women complement similar existing findings for men (Ozier, Forthcoming). This shift towards more productive sectors is suggestive of potential growth consequences of the program.

Finally, I use new individual examination results data to demonstrate that the rapid increases in educational attainment associated with the free secondary education policy did not lead to a corresponding decrease in the educational achievement of students. Impact estimates which combine both composition changes and resource dilution are small and insignificant. With a decrease in resource quality, this implies an increase in mean student ability. I present a model showing that an offsetting increase in mean student ability is consistent with credit constraints precluding poor students from attending secondary schooling. The results on student achievement suggest that concerns over rapid expansions of schooling systems may be overstated and that countries are able to adjust to additional students without negative consequences to the quality of education.

The methodology used here could be employed with future survey data, in which individuals exposed to the program are older, to examine longer term fertility and labor market outcomes. Further, using future data may also provide evidence on the impact of education on spousal quality: if assortative matching is taking place, we may expect the education gains to have intergenerational impacts.

Figure 2.1: Secondary school admissions 2000-2013



Source: Kenya Economic Surveys (2000-2013).

Figure 2.2: Cohort exposure

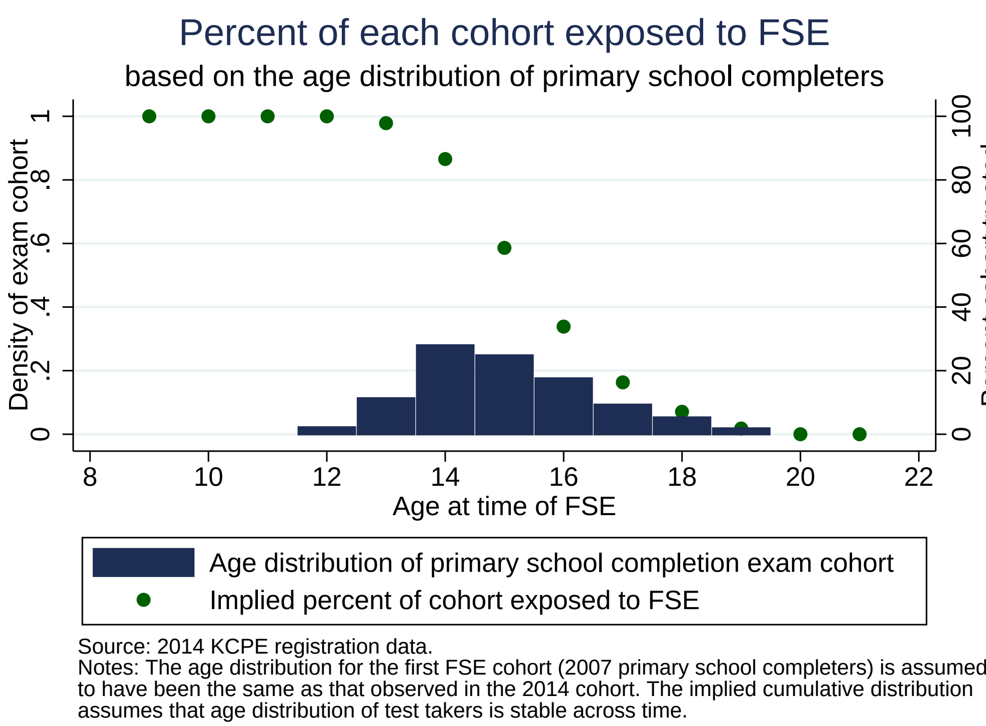
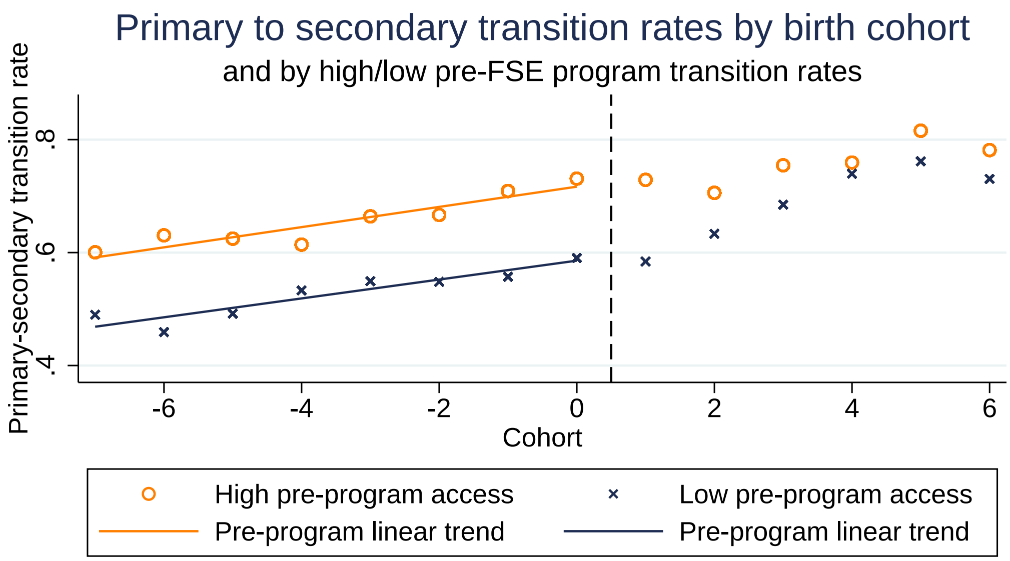


Figure 2.3: Common pre-program trends



Source: 2014 Kenya DHS.
Notes: High/low pre-program access defined as whether county average pri-sec transition rate between 1989 and 1990 was above/below the average transition rate. Pri-sec transition rate defined as share of primary school graduates with at least some secondary schooling. Free secondary education introduced in early 2008 for the 2007 KCPE cohort. 70% of KCPE students in 2014 were 14-16 years old suggesting program first impacted students born between 1991 and 1993.

Figure 2.4: Pre-program primary to secondary transition rate histogram

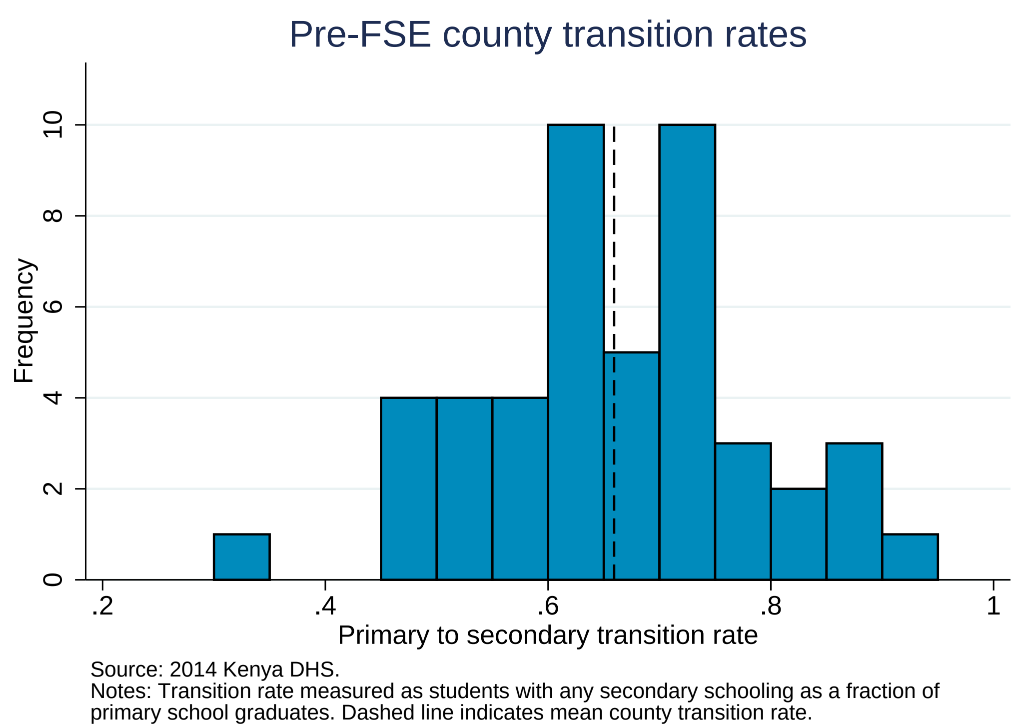


Figure 2.5: Pre-program primary to secondary transition rates by county

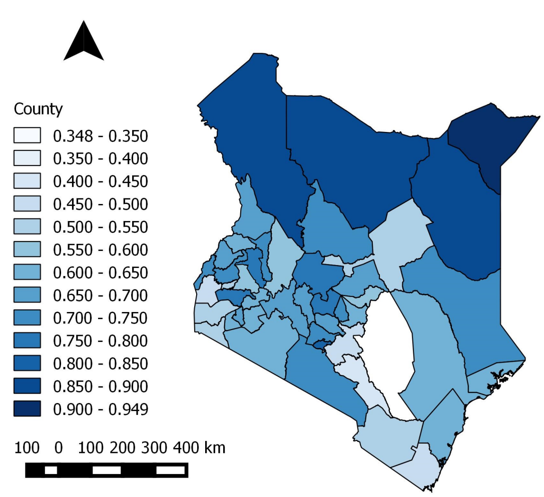
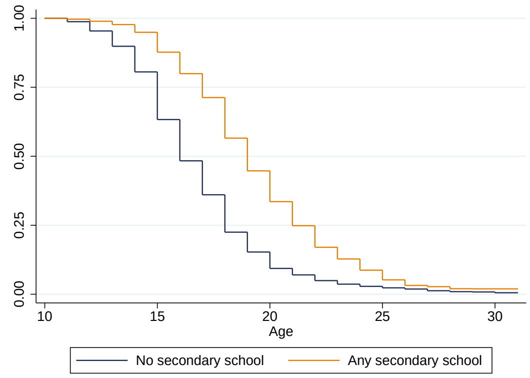
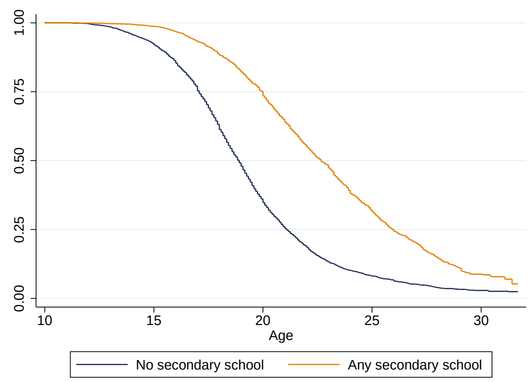


Figure 2.6: Kaplan-Meier survival estimates

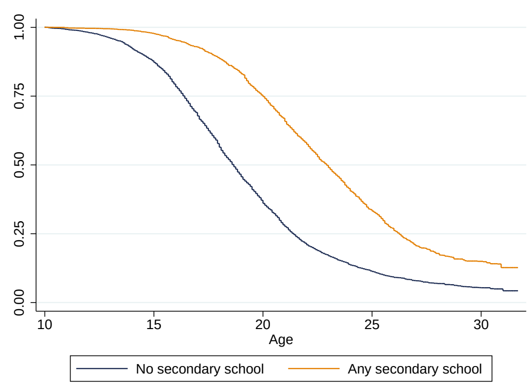
Panel A: Age of first intercourse:



Panel B: Age at first birth:

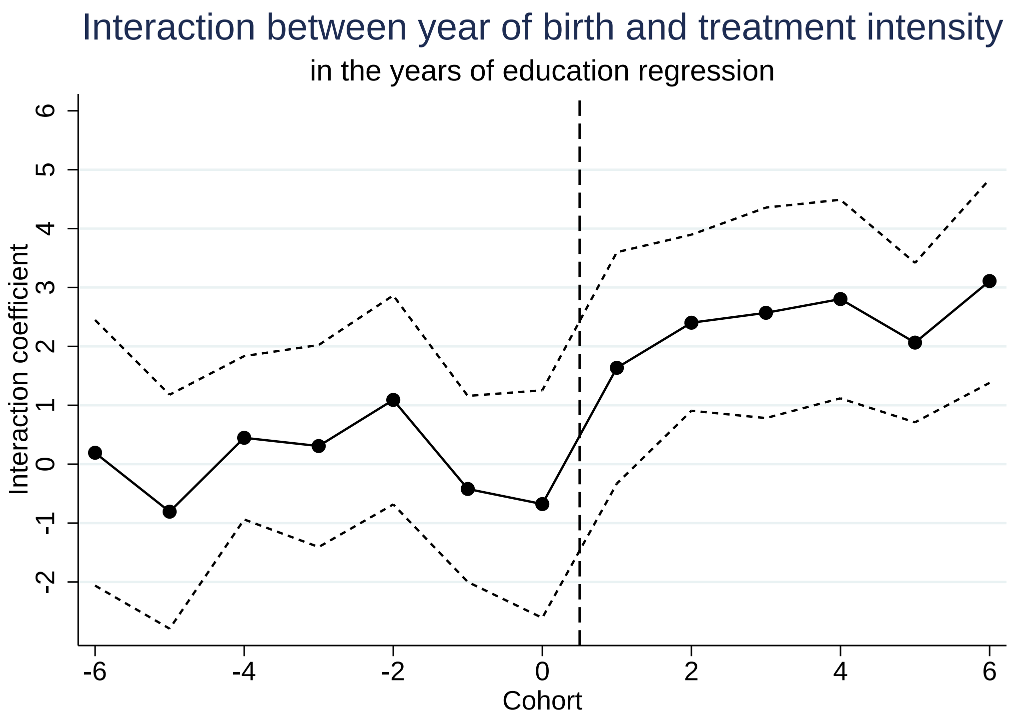


Panel C: Age of first marriage:



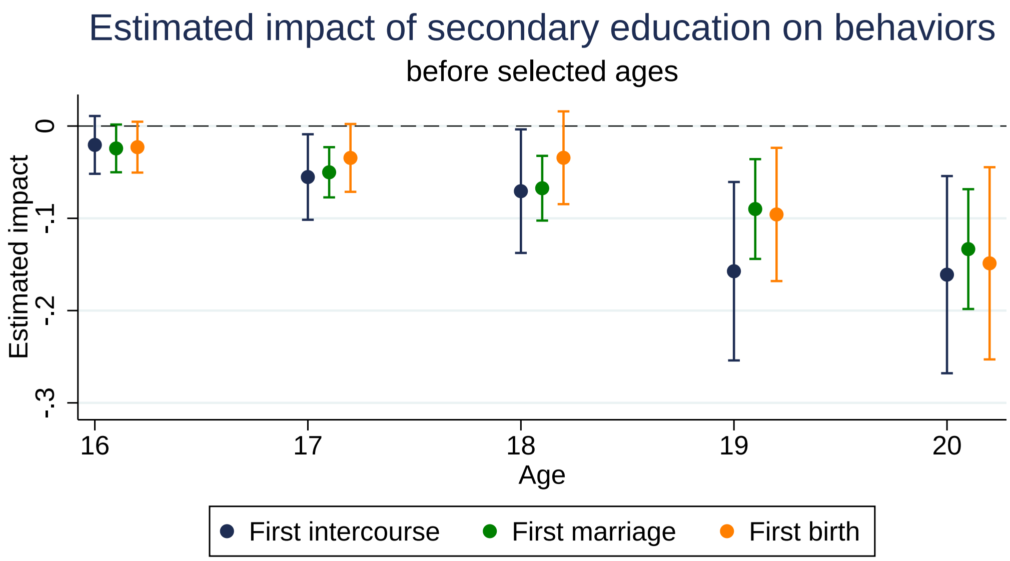
Lines depict the probability of still being in a (a) no intercourse, (b) no birth, or (c) no marriage state by schooling level. Sample restricted to women. In this context, survival refers to remaining in the initial state.

Figure 2.7: Interaction coefficients



Note: Coefficients are on the interaction between county FSE intensity and birth cohort. A joint F-test of pre-program values does not reject that all values are equal to zero.

Figure 2.8: Fertility behavior coefficients



Each point represents the coefficient on years of education from separate regressions where the dependent variables are binary indicators for whether individuals participated in each behavior before age X. Years of education is instrumented with cohort * county level exposure. The bars denote the corresponding 95% confidence intervals, with standard errors clustered by county. The F-statistics for first intercourse and first marriage are 10.46, 10.46, 10.46, 12.43, and 14.38 for age 16, 17, 18, 19, and 20, respectively. First birth F-statistics are 18.08, 18.08, 18.08, 22.76, and 13.34.

Table 2.1: DHS sample characteristics

	Obs.	Mean	S.D.	Median	Min.	Max.
A. Primary School Completers						
Female	13605	0.71	0.46	1	0	1
Age	13605	23.97	3.90	24	18	31
Years of education	13605	10.49	2.35	10	8	19
Completed primary school	13605	1.00	0.00	1	1	1
Attended some secondary school	13605	0.65	0.48	1	0	1
Completed secondary school	13605	0.42	0.49	0	0	1
Female fertility behaviors:						
Age at first intercourse	8298	17.72	2.85	18	5	30
Age at first birth	6432	19.54	3.08	19	11	31
Age at first marriage/cohabitation	6097	19.47	3.23	19	10	31
Male fertility behaviors:						
Age at first intercourse	3446	16.45	3.38	16	5	30
Age at first marriage/cohabitation	1454	22.46	3.13	23	13	30
Employment sector:						
Not working	8499	0.28	0.45	0	0	1
Agricultural work	8499	0.17	0.38	0	0	1
Unskilled work	8499	0.37	0.48	0	0	1
Skilled work	8499	0.18	0.38	0	0	1
Intensity (1-transition rate)	13605	0.35	0.12	0.34	0.05	0.66
B. Secondary School Completers						
Female	5704	0.69	0.46	1	0	1
Age	5704	24.32	3.64	24	18	31
Female fertility behaviors:						
Age at first intercourse	3389	18.95	2.81	19	8	29
Age at first birth	2231	21.24	3.10	21	11	31
Age at first marriage/cohabitation	2166	21.21	2.94	21	10	30
Male fertility behaviors:						
Age at first intercourse	1575	16.93	3.41	17	5	30
Age at first marriage/cohabitation	603	23.30	2.90	23	13	30
Employment sector:						
Not working	3615	0.24	0.43	0	0	1
Agricultural work	3615	0.11	0.32	0	0	1
Unskilled work	3615	0.35	0.48	0	0	1
Skilled work	3615	0.30	0.46	0	0	1

Source: 2014 Kenya DHS
Note: Sample restricted to individuals born between 1983 and 1996 and who are at least 18 years old at the time of the survey. Employment questions were only included in the full survey which was asked of approximately half the sample. Unskilled work comprises unskilled manual work, household work, and services work. Skilled work comprises skilled manual work or professional work.

Table 2.2: Secondary school completion examination summary statistics

	Pre-FSE (2008-2010) (1)	**Post-FSE (2011-2015) (2)**
Number of schools:	5141	7445
Public schools:	4346	6213
Private schools:	795	1232
Number of test takers per year:	300355	437049
Public schools:	262995	384756
Private schools:	37360	52294
Number of test takers per school:	88.94	92.32
Public schools:	90.23	94.76
Private schools:	79.89	74.38
Standardized KCSE score:	-0.051	-0.066
Public schools:	-0.022	-0.050
Private schools:	-0.254	-0.205

Note: Counts calculated as annual averages over stated period. 2012 data are not available. A small number of national schools that draw high performing students from across Kenya are excluded.

Table 2.3: Difference-in-differences estimates: primary schooling

	(1)	(2)	(3)	(4)	(5)
A. Pooled Gender					
(1-transition rate)*FSE period	0.055	0.06	0.086**	-0.134	-0.06
	(0.044)	(0.044)	(0.043)	(0.083)	(0.083)
Observations	20458	20458	20458	20458	20458
R^2	0.208	0.209	0.209	0.211	0.212
B. Female Only					
(1-transition rate)*FSE period	0.04	0.043	0.082	-0.129	-0.024
	(0.059)	(0.057)	(0.067)	(0.098)	(0.102)
Observations	14934	14934	14934	14934	14934
R^2	0.228	0.229	0.229	0.232	0.234
C. Male Only					
(1-transition rate)*FSE period	0.116	0.124	0.122	-0.143	-0.148
	(0.105)	(0.107)	(0.112)	(0.152)	(0.165)
Observations	5524	5524	5524	5524	5524
R^2	0.153	0.156	0.155	0.164	0.169
Control variables:					
Constituency development funds * birth year		✓			✓
2009 unemployment rate * birth year			✓		✓
County linear trends				✓	✓

Note: Dependent variable is a binary variable equal to one if an individual has completed primary school. All regressions include birth year, county, and ethnicity/religion fixed effects. Standard errors are clustered at the county level. Regressions are weighted using DHS survey weights. Transition rate defined as the percentage of primary school graduates who attend secondary school. Initial transition rate defined as the average transition rate in each county for students born in either 1989 or 1990. FSE period defined as birth cohorts after and including 1991. *** indicates significance at the 99 percent level; ** indicates significance at the 95 percent level; and * indicates significance at the 90 percent level.

Table 2.4: Binary treatment intensity difference-in-differences estimates: secondary education

	(1)	(2)	(3)	(4)	(5)
Panel 1: years of education					
A. Pooled Gender					
High Intensity*FSE period	0.348***	0.344***	0.283**	0.382**	0.387*
	(0.133)	(0.129)	(0.135)	(0.168)	(0.198)
Observations	13605	13605	13605	13605	13605
R^2	0.093	0.095	0.094	0.1	0.102
B. Female Only					
High Intensity*FSE period	0.346***	0.357***	0.279**	0.479**	0.564**
	(0.132)	(0.13)	(0.139)	(0.199)	(0.221)
Observations	9596	9596	9596	9596	9596
R^2	0.089	0.091	0.09	0.096	0.099
C. Male Only					
High Intensity*FSE period	0.367*	0.348*	0.328	0.138	0.024
	(0.199)	(0.192)	(0.205)	(0.337)	(0.39)
Observations	4009	4009	4009	4009	4009
R^2	0.124	0.128	0.127	0.139	0.146
Panel 2: completed secondary school					
A. Pooled Gender					
High Intensity*FSE period	0.001	0.005	-0.004	0.013	0.024
	(0.021)	(0.021)	(0.023)	(0.039)	(0.038)
Observations	13605	13605	13605	13605	13605
R^2	0.1	0.101	0.101	0.103	0.105
B. Female Only					
High Intensity*FSE period	-0.008	-0.002	-0.012	0.033	0.063
	(0.023)	(0.023)	(0.026)	(0.049)	(0.048)
Observations	9596	9596	9596	9596	9596
R^2	0.098	0.1	0.099	0.102	0.105
C. Male Only					
High Intensity*FSE period	0.014	0.017	0.009	-0.044	-0.062
	(0.035)	(0.035)	(0.04)	(0.072)	(0.078)
Observations	4009	4009	4009	4009	4009
R^2	0.129	0.134	0.135	0.14	0.151
Control variables:					
Constituency development funds * birth year		✓			✓
2009 unemployment rate * birth year			✓		✓
County linear trend				✓	✓

Note: Reported coefficients are the estimated interaction coefficient between a dummy variable for high treatment intensity counties and an FSE indicator variable equal to one for all individuals born in 1991 or later. Sample restricted to individuals who have completed at least primary school. All regressions include birth year, county, and ethnicity/religion fixed effects. Standard errors are clustered at the county level. Regressions are weighted using DHS survey weights. Transition rate defined as the percentage of primary school graduates who attend secondary school. Initial transition rate defined as the average transition rate in each county for students born in either 1989 or 1990. *** indicates significance at the 99 percent level; ** indicates significance at the 95 percent level; and * indicates significance at the 90 percent level.

Table 2.5: Difference-in-differences estimates: secondary education

	(1)	(2)	(3)	(4)	(5)
Panel 1: years of education					
A. Pooled Gender					
(1-transition rate)*FSE period	2.255***	2.256***	2.060***	2.059***	2.134***
	(0.31)	(0.311)	(0.356)	(0.718)	(0.677)
Observations	13605	13605	13605	13605	13605
R^2	0.099	0.101	0.1	0.104	0.106
B. Female Only					
(1-transition rate)*FSE period	2.409***	2.449***	2.221***	2.058**	2.336***
	(0.277)	(0.268)	(0.336)	(0.897)	(0.709)
Observations	9596	9596	9596	9596	9596
R^2	0.091	0.093	0.092	0.096	0.099
C. Male Only					
(1-transition rate)*FSE period	2.047***	2.035***	1.942***	2.374**	2.075
	(0.673)	(0.616)	(0.686)	(1.090)	(1.309)
Observations	4009	4009	4009	4009	4009
R^2	0.125	0.129	0.128	0.14	0.147
Panel 2: completed secondary school					
A. Pooled Gender					
(1-transition rate)*FSE period	0.129*	0.145**	0.116	0.158	0.201
	(0.071)	(0.066)	(0.087)	(0.135)	(0.126)
Observations	13605	13605	13605	13605	13605
R^2	0.104	0.105	0.105	0.107	0.109
B. Female Only					
(1-transition rate)*FSE period	0.143*	0.164**	0.146	0.213	0.321**
	(0.081)	(0.069)	(0.091)	(0.156)	(0.13)
Observations	9596	9596	9596	9596	9596
R^2	0.099	0.1	0.099	0.102	0.105
C. Male Only					
(1-transition rate)*FSE period	0.085	0.107	0.064	0.046	-0.05
	(0.111)	(0.108)	(0.129)	(0.231)	(0.267)
Observations	4009	4009	4009	4009	4009
R^2	0.129	0.134	0.135	0.14	0.151
Control variables:					
Constituency development funds * birth year		✓			✓
2009 unemployment rate * birth year			✓		✓
County linear trend				✓	✓

Note: Sample restricted to individuals who have completed at least primary school. All regressions include birth year, county, and ethnicity/religion fixed effects. Standard errors are clustered at the county level. Regressions are weighted using DHS survey weights. Transition rate defined as the percentage of primary school graduates who attend secondary school. Initial transition rate defined as the average transition rate in each county for students born in either 1989 or 1990. FSE period defined as birth cohorts after and including 1991. *** indicates significance at the 99 percent level; ** indicates significance at the 95 percent level; and * indicates significance at the 90 percent level.

Table 2.6: Falsification test difference-in-differences estimates: secondary education

	(1)	(2)	(3)	(4)	(5)
Panel 1: years of education					
A. Pooled Gender					
(1-transition rate)*FSE period	0.713	0.462	0.737	1.418	1.034
	(0.45)	(0.357)	(0.478)	(1.028)	(1.081)
Observations	7661	7661	7661	7661	7661
R^2	0.108	0.11	0.108	0.113	0.114
B. Female Only					
(1-transition rate)*FSE period	0.718	0.475	0.731	1.062	1.092
	(0.674)	(0.548)	(0.664)	(1.147)	(1.323)
Observations	5484	5484	5484	5484	5484
R^2	0.099	0.101	0.1	0.105	0.107
C. Male Only					
(1-transition rate)*FSE period	0.517	0.289	0.668	2.482*	1.193
	(0.877)	(1.037)	(0.92)	(1.484)	(1.922)
Observations	2177	2177	2177	2177	2177
R^2	0.12	0.124	0.122	0.142	0.147
Panel 2: completed secondary school					
A. Pooled Gender					
(1-transition rate)*FSE period	0.058	0.022	0.07	0.05	0.014
	(0.084)	(0.078)	(0.096)	(0.178)	(0.214)
Observations	7661	7661	7661	7661	7661
R^2	0.09	0.092	0.091	0.094	0.096
B. Female Only					
(1-transition rate)*FSE period	-0.027	-0.077	-0.003	-0.13	-0.054
	(0.118)	(0.102)	(0.123)	(0.191)	(0.238)
Observations	5484	5484	5484	5484	5484
R^2	0.088	0.09	0.089	0.095	0.097
C. Male Only					
(1-transition rate)*FSE period	0.212	0.214	0.224	0.517**	0.306
	(0.176)	(0.18)	(0.183)	(0.251)	(0.311)
Observations	2177	2177	2177	2177	2177
R^2	0.093	0.096	0.097	0.11	0.116
Control variables:					
Constituency development funds * birth year		✓			✓
2009 unemployment rate * birth year			✓		✓
County specific linear trends				✓	✓

Note: All regressions include birth year, county, and ethnicity/religion fixed effects. Standard errors are clustered at the county level. Regressions are weighted using DHS survey weights. Transition rate defined as the percentage of primary school graduates who attend secondary school. Initial transition rate defined as the average transition rate in each county for students born in either 1986 or 1985. Pre-FSE treatment period defined as birth cohorts after and including 1987.

Table 2.7: Instrumental variables estimates: fertility behaviors

	Mean dep. var		Est. treatment effect	
	Pooled	Female	Pooled	Female
	(1)	**(2)**	**(3)**	**(4)**
First intercourse before age:				
16	0.226	0.186	-0.027*	-0.050*
			(0.016)	(0.028)
17	0.341	0.302	-0.061**	-0.104***
			(0.025)	(0.040)
18	0.460	0.425	-0.078**	-0.114***
			(0.038)	(0.042)
19	0.604	0.573	-0.162***	-0.203***
			(0.056)	(0.062)
20	0.700	0.678	-0.177***	-0.224***
			(0.058)	(0.073)
First marriage before age:				
16	0.046	0.063	-0.027*	-0.042**
			(0.015)	(0.020)
17	0.080	0.109	-0.057***	-0.084***
			(0.016)	(0.023)
18	0.130	0.176	-0.073***	-0.104***
			(0.020)	(0.029)
19	0.197	0.262	-0.095***	-0.117***
			(0.032)	(0.039)
20	0.281	0.364	-0.143***	-0.171***
			(0.035)	(0.048)
First birth before age:				
16		0.052		-0.029**
				(0.014)
17		0.099		-0.050**
				(0.020)
18		0.175		-0.055**
				(0.027)
19		0.273		-0.137**
				(0.056)
20		0.384		-0.203***
				(0.074)

Note: Dependent variable is equal to one if the event (intercourse/marriage/birth) happened before the individual turned age X. Reported values are the estimated coefficients on years of education where years of education is instrumented with post * county level exposure. The F-statistics for the pooled sample are 55.52, 55.52, 55.52, 41.47, and 40.48 for age 16, 17, 18, 19, and 20, respectively. The first birth F-statistics are 75.78, 75.78, 75.78, 55.04, and 37.47. Standard errors clustered at the county level are reported in parenthesis. Sample restricted to individuals who have completed at least primary school. All regressions include birth year, county, and ethnicity/religion fixed effects. Regressions are weighted using DHS survey weights. *** indicates significance at the 99 percent level; ** indicates significance at the 95 percent level; and * indicates significance at the 90 percent level.

Table 2.8: Instrumental variables estimates: contraceptive use

	Uses any contraceptive (1)	**Uses modern method (2)**	**Uses condoms (3)**	**Can get condoms (4)**
Years of education	-0.017	-0.043	0.022	0.032
	(0.052)	(0.042)	(0.03)	(0.049)
Constant	0.675	0.83**	-0.14	0.203
	(0.492)	(0.399)	(0.282)	(0.496)
Observations	8298	8298	8298	3868
First stage F-stat:	30.311	30.311	30.311	10.419

Note: Years of education instrumented with post * county level exposure. Standard errors clustered at the county level are reported in parenthesis. Sample restricted to individuals who have completed at least primary school and have had intercourse. All regressions include birth year, county, and ethnicity/religion fixed effects. Regressions are weighted using DHS survey weights. *** indicates significance at the 99 percent level; ** indicates significance at the 95 percent level; and * indicates significance at the 90 percent level.

Table 2.9: Instrumental variables estimates: desired fertility

	Pooled (1)	**Female (2)**
Years of education	-0.228*	-0.133
	(0.133)	(0.153)
Constant	5.001***	4.849***
	(1.214)	(1.536)
Observations	8465	4502
First stage F-stat:	30.622	15.031

Note: Dependent variable is the respondent's ideal number of children. Years of education instrumented with post * county level exposure. Standard errors clustered at the county level are reported in parenthesis. Sample restricted to individuals who have completed at least primary school. All regressions include birth year, county, and ethnicity/religion fixed effects. Standard errors are clustered at the county level. Regressions are weighted using DHS survey weights. *** indicates significance at the 99 percent level; ** indicates significance at the 95 percent level; and * indicates significance at the 90 percent level.

Table 2.10: Instrumental variables estimates: sector of work

	Skilled Work (1)	**Unskilled Work (2)**	**Agricultural Work (3)**	**No Work (4)**
Panel 1. Age 18 and over				
Years of education	0.069***	-0.06	-0.18***	0.171**
	(0.022)	(0.064)	(0.039)	(0.079)
Observations	4525	4525	4525	4525
First stage F-stat:	22.909	22.909	22.909	22.909
Panel 2. Age 19 and over				
Years of education	0.074***	-0.047	-0.169***	0.142**
	(0.023)	(0.059)	(0.037)	(0.07)
Observations	4295	4295	4295	4295
First stage F-stat:	24.347	24.347	24.347	24.347
Panel 3. Age 20 and over				
Years of education	0.082***	-0.037	-0.137***	0.092
	(0.025)	(0.057)	(0.033)	(0.067)
Observations	3935	3935	3935	3935
First stage F-stat:	16.226	16.226	16.226	16.226

Note: Dependent variable is a binary variable equal to one if respondent works in sector X. Years of education instrumented with post * county level exposure. Unskilled labor aggregates household/domestic work, service jobs, and unskilled manual labor. Skilled labor aggregates professional/technical/managerial/clerical and skilled manual labor. Standard errors clustered at the county level are reported in parenthesis. Sample restricted to women who have completed at least primary school. All regressions include birth year, county, and ethnicity/religion fixed effects. Regressions are weighted using DHS survey weights. *** indicates significance at the 99 percent level; ** indicates significance at the 95 percent level; and * indicates significance at the 90 percent level.

Table 2.11: County expansion at secondary school completion

	(1)	**(2)**	**(3)**	**(4)**
A. Pooled Gender				
(1-transition rate)*FSE period	0.192**	0.196*	0.245***	0.246***
	(0.097)	(0.104)	(0.09)	(0.093)
Observations	423	423	423	423
R^2	0.969	0.971	0.977	0.977
B. Female Only				
(1-transition rate)*FSE period	0.763**	0.751**	0.405**	0.413**
	(0.297)	(0.295)	(0.16)	(0.162)
Observations	423	423	423	423
R^2	0.942	0.944	0.966	0.966
C. Male Only				
(1-transition rate)*FSE period	0.557***	0.542***	0.488***	0.483***
	(0.126)	(0.129)	(0.126)	(0.128)
Observations	423	423	423	423
R^2	0.948	0.953	0.962	0.964
Control variables:				
Constituency development funds * birth year		✓		✓
2009 unemployment rate * birth year			✓	✓

Note: Regressions are run at the county-year level. Dependent variable is the county cohort KCSE registration divided by the 2010 cohort KCSE registration. Standard errors clustered at the county level are reported in parenthesis. All columns include year fixed effects as well as county linear trends. *** indicates significance at the 99 percent level; ** indicates significance at the 95 percent level; and * indicates significance at the 90 percent level.

Table 2.12: Student achievement

	(1)	(2)	(3)	(4)
A. Full sample				
(1-transition rate)*FSE period	-0.007	0.074	.	.
	(0.02)	(0.047)		
(1-transition rate)*FSE period*Female	.	.	-0.019	0.055
			(0.02)	(0.054)
(1-transition rate)*FSE period*Male	.	.	0.012	0.112*
			(0.024)	(0.06)
Observations	3321504	3321504	3321504	3321504
R^2	0.039	0.221	0.049	0.238
B. High performers				
(1-transition rate)*FSE period	0.131	0.122*	.	.
	(0.197)	(0.074)		
(1-transition rate)*FSE period*Female	.	.	0.147	0.01
			(0.215)	(0.1)
(1-transition rate)*FSE period*Male	.	.	0.121	0.173**
			(0.197)	(0.082)
Observations	269436	269436	269436	269436
R^2	0.357	0.409	0.361	0.418
Control variables:				
Constituency development funds * birth year		✓		✓
2009 unemployment rate * birth year		✓		✓

Note: Dependent variable is standardized KCSE score. Standard errors clustered at the county level are reported in parenthesis. All columns include county fixed effects and county linear trends while Panel B also includes year fixed effects. Columns 2 and 4 also include dummies for public schools, single gender schools, and district level schools. Controls are interacted with gender for columns 3 and 4. *** indicates significance at the 99 percent level; ** indicates significance at the 95 percent level; and * indicates significance at the 90 percent level.

Table 2.13: Estimates from a changes-in-changes model

Percentile	**Overall**	**Female**	**Male**
0.8	0.037	0.063*	-0.005
0.9	0.015	0.026	-0.018
0.95	0.022	0.010	-0.021

Note: Estimates are from a changes-in-changes model. Standard errors clustered at the county level. County fixed effects and linear trends are included as described in the text above. *** indicates significance at the 99 percent level; ** indicates significance at the 95 percent level; and * indicates significance at the 90 percent level.

Appendix

2.A Additional Tables and Figures

Figure 2.A.1: Age distribution of KCPE test takers

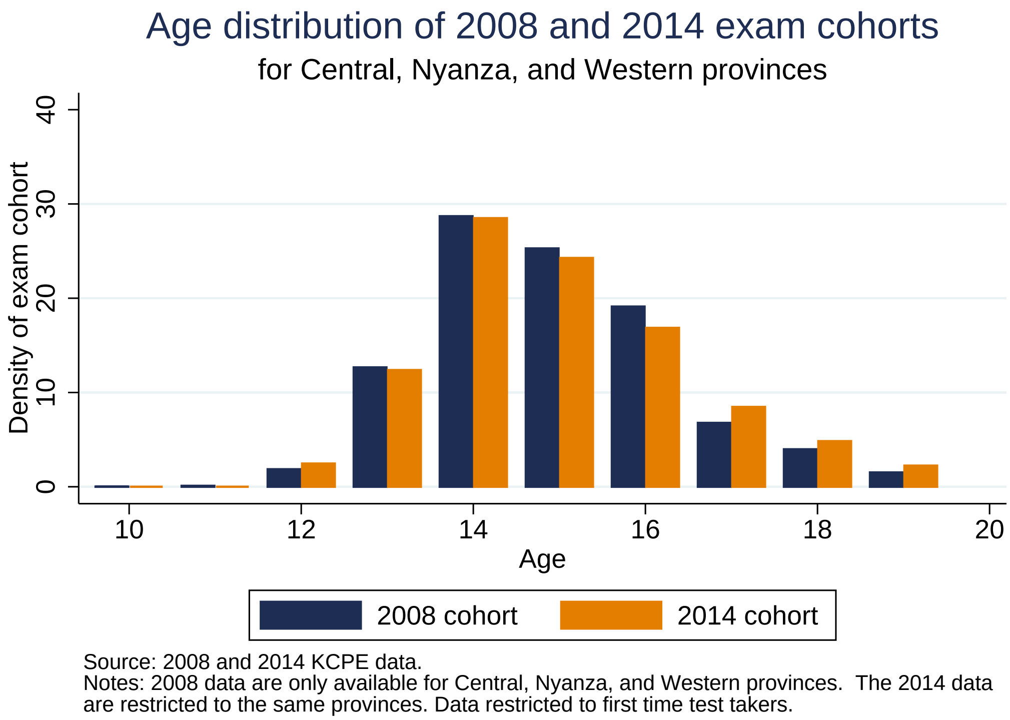
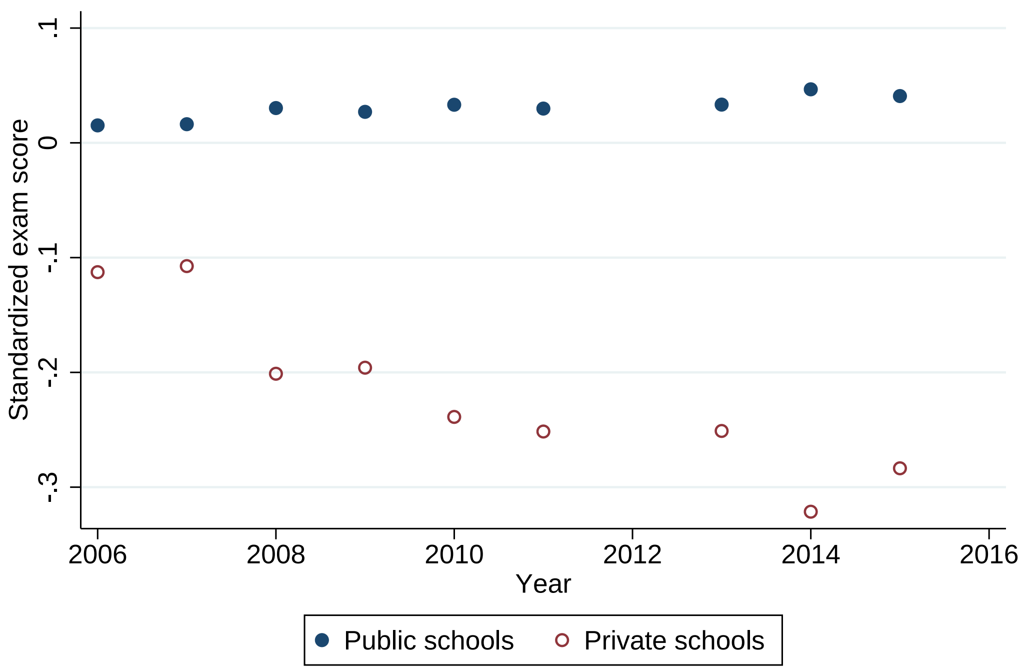
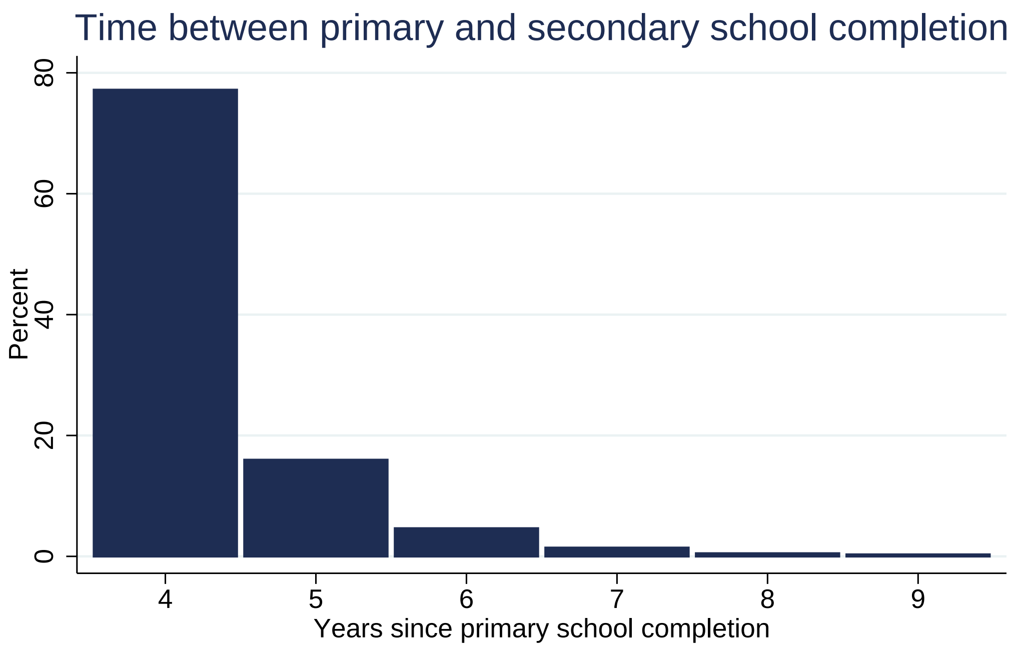


Figure 2.A.2: Mean KCSE scores (Public/Private)



Notes: Mean scores calculated from KCSE data. Each year approximately 12% of test takers attend private schools.

Figure 2.A.3: Secondary school time to completion



Source: 2014 KCSE Registration Data
Note: Fewer than 2% of test takers complete secondary school more than 7 years after primary school.

Figure 2.A.4: Falsification test: Pre-FSE sample

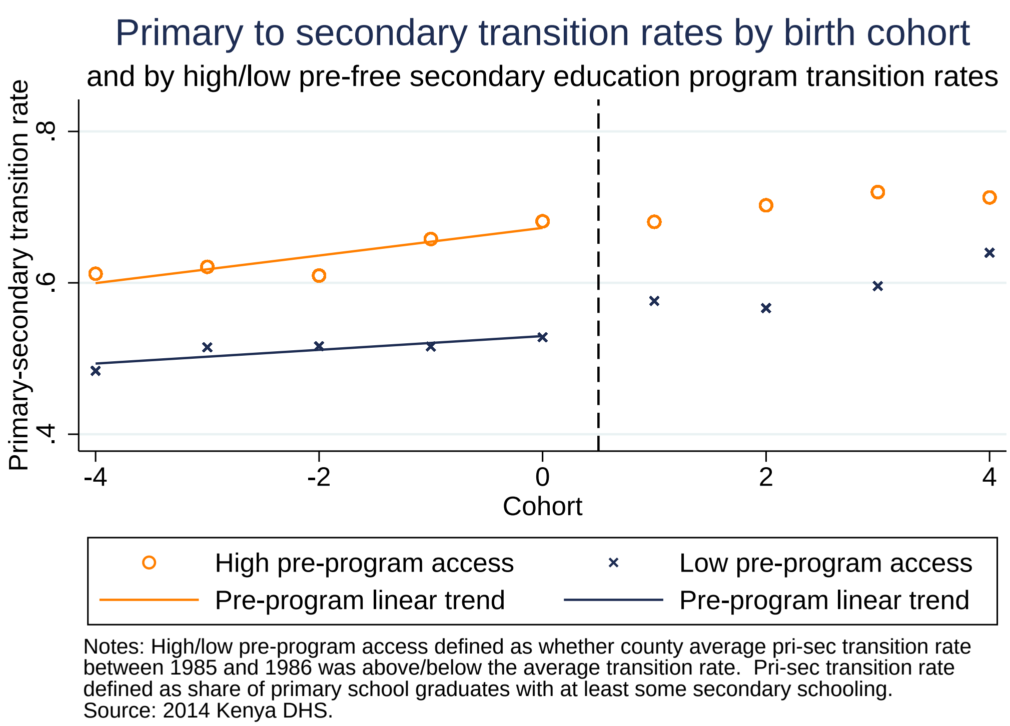
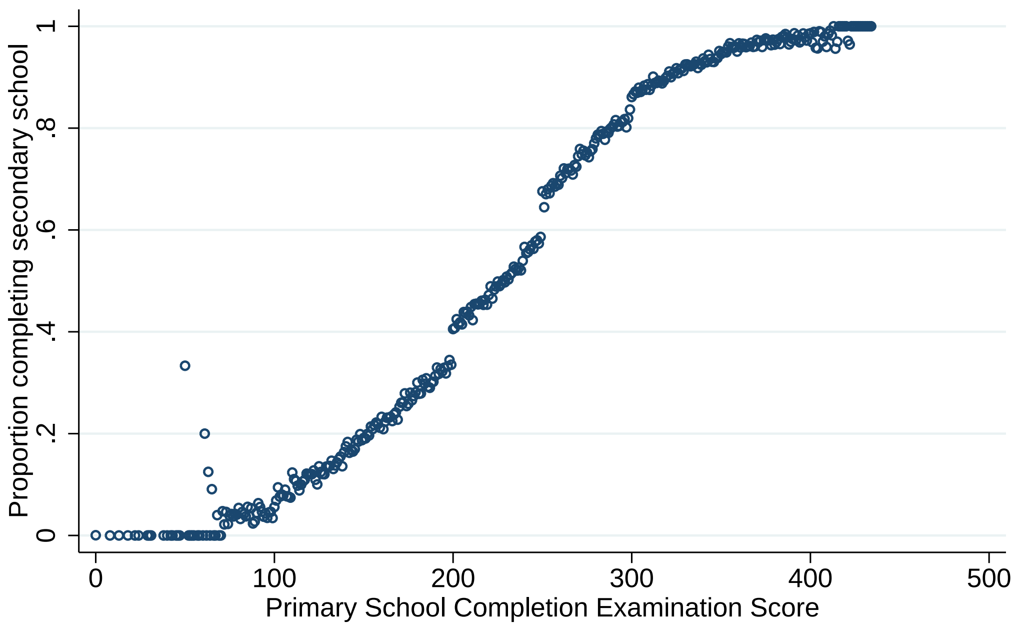


Figure 2.A.5: Probability of secondary school completion by KCPE score



Source: KCPE results data and KCSE registration data.
Note: The graph shows, by primary school completion examination score, the proportion of 2010 primary school completers who registered for the secondary school completion examination in either 2014 or 2015.

Table 2.A.1: DHS sample characteristics

	Obs.	**Mean**	**S.D.**	**Median**	**Min.**	**Max.**
DHS Sample						
Female	20458	0.73	0.44	1	0	1
Age	20458	24.24	3.97	24	18	31
Christian	20458	0.84	0.37	1	0	1
Muslim	20458	0.13	0.34	0	0	1
Kalenjin	20458	0.15	0.36	0	0	1
Kikuya	20458	0.15	0.35	0	0	1
Luhya	20458	0.12	0.33	0	0	1
Luo	20458	0.10	0.30	0	0	1
Other ethnicity	20458	0.22	0.41	0	0	1
Urban household	20458	0.41	0.49	0	0	1
Years of education	20458	8.21	4.11	8	0	19
Completed primary school	20458	0.67	0.47	1	0	1
Attended some secondary school	20458	0.43	0.50	0	0	1
Completed secondary school	20458	0.28	0.45	0	0	1
Female fertility behaviors:						
Age at first intercourse	13287	17.00	2.95	17	5	30
Age at first birth	11104	18.75	3.10	18	10	31
Age at first marriage/cohabitation	10718	18.37	3.41	18	9	31
Male fertility behaviors:						
Age at first intercourse	4734	16.31	3.38	16	5	30
Age at first marriage/cohabitation	2204	21.95	3.25	22	13	30
Intensity (1-transition rate)	20458	0.34	0.12	0.34	0.05	0.66

Source: 2014 Kenya DHS
Note: Sample restricted to individuals born between 1983 and 1996.

Table 2.A.2: Binary intensity measure difference-in-differences estimates: primary schooling

	(1)	**(2)**	**(3)**	**(4)**	**(5)**
A. Pooled Gender					
High Intensity*FSE period	-0.0005	0.00002	0.007	-0.059***	-0.044*
	(0.013)	(0.013)	(0.014)	(0.023)	(0.023)
Observations	20458	20458	20458	20458	20458
R^2	0.201	0.201	0.201	0.204	0.205
B. Female Only					
High Intensity*FSE period	0.006	0.005	0.014	-0.054*	-0.032
	(0.015)	(0.015)	(0.015)	(0.028)	(0.028)
Observations	14934	14934	14934	14934	14934
R^2	0.228	0.229	0.229	0.232	0.234
C. Male Only					
High Intensity*FSE period	-0.011	-0.006	-0.015	-0.057	-0.066
	(0.026)	(0.026)	(0.027)	(0.038)	(0.04)
Observations	5524	5524	5524	5524	5524
R^2	0.153	0.155	0.155	0.164	0.17
Control variables:					
Constituency development funds * birth year		✓			✓
2009 unemployment rate * birth year			✓		✓
County linear trends				✓	✓

Note: Dependent variable is a binary variable equal to one if an individual has completed primary school. All regressions include birth year, county, and ethnicity/religion fixed effects. Standard errors are clustered at the county level. Regressions are weighted using DHS survey weights. Transition rate defined as the percentage of primary school graduates who attend secondary school. Initial transition rate defined as the average transition rate in each county for students born in either 1989 or 1990. FSE period defined as birth cohorts after and including 1991. *** indicates significance at the 99 percent level; ** indicates significance at the 95 percent level; and * indicates significance at the 90 percent level.

Table 2.A.3: Binary treatment intensity difference-in-differences estimates: secondary education

	(1)	(2)	(3)	(4)	(5)
A. Falsification for program introduced in 1986					
High Intensity*FSE period	0.198*	0.139	0.224*	0.229	0.162
	(0.119)	(0.094)	(0.126)	(0.2)	(0.214)
Observations	10324	10324	10324	10324	10324
R^2	0.112	0.115	0.112	0.117	0.12
A. Falsification for program introduced in 1985					
High Intensity*FSE period	0.184	0.126	0.222	0.152	0.121
	(0.13)	(0.114)	(0.14)	(0.214)	(0.204)
Observations	11142	11142	11142	11142	11142
R^2	0.111	0.114	0.111	0.117	0.12
B. Falsification for program introduced in 1984					
High Intensity*FSE period	0.095	0.044	0.104	-0.047	-0.157
	(0.104)	(0.086)	(0.1)	(0.203)	(0.21)
Observations	10643	10643	10643	10643	10643
R^2	0.111	0.114	0.111	0.116	0.119
C. Falsification for program introduced in 1983					
High Intensity*FSE period	0.062	0.002	0.082	-0.03	-0.06
	(0.116)	(0.12)	(0.111)	(0.246)	(0.231)
Observations	10264	10264	10264	10264	10264
R^2	0.113	0.117	0.114	0.118	0.121
D. Falsification for program introduced in 1982					
High Intensity*FSE period	0.04	0.07	0.085	0.385*	0.504**
	(0.133)	(0.145)	(0.125)	(0.207)	(0.232)
Observations	9760	9760	9760	9760	9760
R^2	0.113	0.115	0.114	0.118	0.121
E. Falsification for program introduced in 1981					
High Intensity*FSE period	-0.158	-0.075	-0.133	0.19	0.476**
	(0.174)	(0.176)	(0.168)	(0.247)	(0.233)
Observations	9353	9353	9353	9353	9353
R^2	0.111	0.114	0.112	0.117	0.12
Control variables:					
Constituency development funds * birth year		✓			✓
2009 unemployment rate * birth year			✓		✓
County linear trend				✓	✓

Note: Reported coefficients are the estimated interaction coefficient between a dummy variable for high treatment intensity counties and an FSE indicator variable equal to one for all individuals born after the introduction of the falsified program. Sample restricted to individuals who have completed at least primary school. All regressions include birth year, county, and ethnicity/religion fixed effects. Standard errors are clustered at the county level. Regressions are weighted using DHS survey weights. Transition rate defined as the percentage of primary school graduates who attend secondary school. Initial transition rate defined as the average transition rate in each county for students born in either 1989 or 1990. *** indicates significance at the 99 percent level; ** indicates significance at the 95 percent level; and * indicates significance at the 90 percent level.

Table 2.A.4: Binary treatment diff-in-diffs excluding transition cohorts: secondary education

	(1)	**(2)**	**(3)**	**(4)**	**(5)**
Panel 1: years of education					
A. Pooled Gender					
High Intensity*FSE period	0.346**	0.39***	0.332**	0.578***	0.605***
	(0.146)	(0.147)	(0.153)	(0.192)	(0.186)
Observations	11684	11684	11684	11684	11684
R^2	0.093	0.101	0.1	0.106	0.109
B. Female Only					
High Intensity*FSE period	0.356**	0.416***	0.319**	0.725***	0.852***
	(0.15)	(0.147)	(0.155)	(0.234)	(0.204)
Observations	8246	8246	8246	8246	8246
R^2	0.089	0.095	0.095	0.102	0.104
C. Male Only					
High Intensity*FSE period	0.322*	0.389**	0.407*	0.274	0.151
	(0.194)	(0.188)	(0.208)	(0.459)	(0.473)
Observations	3438	3438	3438	3438	3438
R^2	0.117	0.136	0.135	0.147	0.155
Panel 2: completed secondary school					
A. Pooled Gender					
High Intensity*FSE period	-0.014	0.005	0.002	0.03	0.052
	(0.025)	(0.024)	(0.027)	(0.05)	(0.048)
Observations	11684	11684	11684	11684	11684
R^2	0.09	0.108	0.107	0.11	0.112
B. Female Only					
High Intensity*FSE period	-0.026	-0.005	-0.015	0.042	0.085
	(0.029)	(0.025)	(0.03)	(0.072)	(0.065)
Observations	8246	8246	8246	8246	8246
R^2	0.091	0.106	0.105	0.109	0.112
C. Male Only					
High Intensity*FSE period	0.002	0.029	0.04	-4.09e-06	-0.011
	(0.037)	(0.033)	(0.039)	(0.081)	(0.09)
Observations	3438	3438	3438	3438	3438
R^2	0.106	0.145	0.145	0.151	0.162
Control variables:					
Constituency development funds * birth year		✓			✓
2009 unemployment rate * birth year			✓		✓
County linear trend				✓	✓

Note: Reported coefficients are the estimated interaction coefficient between a dummy variable for high treatment intensity counties and a post period dummy equal to one for all individuals born in 1991 or later. Sample restricted to individuals who have completed at least primary school and excludes the partially impacted cohorts born in 1991 and 1992. All regressions include birth year, county, and ethnicity/religion fixed effects. Standard errors are clustered at the county level. Regressions are weighted using DHS survey weights. Transition rate defined as the percentage of primary school graduates who attend secondary school. Initial transition rate defined as the average transition rate in each county for students born in either 1989 or 1990. FSE period defined as birth cohorts after and including 1991. *** indicates significance at the 99 percent level; ** indicates significance at the 95 percent level; and * indicates significance at the 90 percent level.

Table 2.A.5: OLS estimates: fertility behaviors

	Mean dep. var		Est. treatment effect	
	Pooled	Female	Pooled	Female
	(1)	**(2)**	**(3)**	**(4)**
First intercourse before age:				
16	0.226	0.186	-0.033***	-0.037***
			(0.002)	(0.002)
17	0.341	0.302	-0.045***	-0.055***
			(0.003)	(0.003)
18	0.460	0.425	-0.053***	-0.067***
			(0.003)	(0.003)
19	0.604	0.573	-0.052***	-0.066***
			(0.002)	(0.003)
20	0.700	0.678	-0.047***	-0.060***
			(0.002)	(0.003)
First marriage before age:				
16	0.046	0.063	-0.013***	-0.017***
			(0.001)	(0.002)
17	0.080	0.109	-0.023***	-0.031***
			(0.001)	(0.002)
18	0.130	0.176	-0.034***	-0.046***
			(0.002)	(0.002)
19	0.197	0.262	-0.047***	-0.062***
			(0.002)	(0.002)
20	0.281	0.364	-0.055***	-0.071***
			(0.002)	(0.002)
First birth before age:				
16		0.052		-0.011***
			(0.001)	
17		0.099		-0.022***
			(0.002)	
18		0.175		-0.043***
			(0.002)	
19		0.273		-0.061***
			(0.002)	
20		0.384		-0.077***
			(0.003)	

Note: Dependent variable is equal to one if the event (intercourse/marriage/birth) happened before the individual turned age X. Reported values are the estimated coefficients on years of education. Standard errors clustered at the county level are reported in parenthesis. Sample restricted to individuals who have completed at least primary school. All regressions include birth year, county, and ethnicity/religion fixed effects. Regressions are weighted using DHS survey weights. *** indicates significance at the 99 percent level; ** indicates significance at the 95 percent level; and * indicates significance at the 90 percent level.

Table 2.A.6: Instrumental variables estimates: fertility behaviors

	Mean dep. var		Est. treatment effect	
	Pooled	Female	Pooled	Female
	(1)	**(2)**	**(3)**	**(4)**
First intercourse before age:				
16	0.226	0.186	-0.020	-0.046*
			(0.016)	(0.024)
17	0.341	0.302	-0.055**	-0.095***
			(0.024)	(0.033)
18	0.460	0.425	-0.071**	-0.098***
			(0.034)	(0.035)
19	0.604	0.573	-0.157***	-0.181***
			(0.049)	(0.052)
20	0.700	0.678	-0.161***	-0.205***
			(0.055)	(0.068)
First marriage before age:				
16	0.046	0.063	-0.024*	-0.038**
			(0.013)	(0.018)
17	0.080	0.109	-0.050***	-0.076***
			(0.014)	(0.019)
18	0.130	0.176	-0.067***	-0.096***
			(0.018)	(0.024)
19	0.197	0.262	-0.090***	-0.109***
			(0.028)	(0.029)
20	0.281	0.364	-0.133***	-0.157***
			(0.033)	(0.044)
First birth before age:				
16		0.052		-0.023
				(0.014)
17		0.099		-0.035*
				(0.019)
18		0.175		-0.034
				(0.026)
19		0.273		-0.096***
				(0.037)
20		0.384		-0.149***
				(0.053)

Note: Dependent variable is equal to one if the event (intercourse/marriage/birth) happened before the individual turned age X. Reported values are the estimated coefficients on years of education where years of education is instrumented with cohort * county level exposure. The F-statistics for the pooled sample are 10.46, 10.46, 10.46, 12.43, and 14.38 for age 16, 17, 18, 19, and 20, respectively. The first birth F-statistics are 18.08, 18.08, 18.08, 22.76, and 13.34. Standard errors clustered at the county level are reported in parenthesis. Sample restricted to individuals who have completed at least primary school. All regressions include birth year, county, and ethnicity/religion fixed effects. Regressions are weighted using DHS survey weights. *** indicates significance at the 99 percent level; ** indicates significance at the 95 percent level; and * indicates significance at the 90 percent level.

Table 2.A.7: Instrumental variables estimates: contraceptive use

	Uses any contraceptive (1)	**Uses modern method (2)**	**Uses condoms (3)**	**Can get condoms (4)**
Years of education	0.017	-0.005	0.013	0.035
	(0.048)	(0.034)	(0.025)	(0.033)
Constant	0.358	0.473	-0.062	0.181
	(0.438)	(0.312)	(0.239)	(0.347)
Observations	8298	8298	8298	3868
First stage F-stat:	15.365	15.365	15.365	7.122

Note: Years of education instrumented with cohort * county level exposure. Standard errors clustered at the county level are reported in parenthesis. Sample restricted to individuals who have completed at least primary school and have had intercourse. All regressions include birth year, county, and ethnicity/religion fixed effects. Regressions are weighted using DHS survey weights. *** indicates significance at the 99 percent level; ** indicates significance at the 95 percent level; and * indicates significance at the 90 percent level.

Table 2.A.8: Instrumental variables estimates: desired fertility

	Pooled (1)	**Female (2)**
Years of education	-0.148	-0.056
	(0.117)	(0.108)
Constant	4.307***	4.079***
	(1.072)	(1.088)
Observations	8465	4502
First stage F-stat:	9.145	8.655

Note: Dependent variable is the respondent's ideal number of children. Years of education instrumented with cohort * county level exposure. Standard errors clustered at the county level are reported in parenthesis. Sample restricted to individuals who have completed at least primary school. All regressions include birth year, county, and ethnicity/religion fixed effects. Standard errors are clustered at the county level. Regressions are weighted using DHS survey weights. *** indicates significance at the 99 percent level; ** indicates significance at the 95 percent level; and * indicates significance at the 90 percent level.

Table 2.A.9: Instrumental variables estimates: sector of work

	Skilled Work (1)	**Unskilled Work (2)**	**Agricultural Work (3)**	**No Work (4)**
Panel 1. Age 18 and over				
Years of education	0.069***	-0.06	-0.18***	0.171**
	(0.022)	(0.064)	(0.039)	(0.079)
Observations	4525	4525	4525	4525
First stage F-stat:	22.909	22.909	22.909	22.909
Panel 2. Age 19 and over				
Years of education	0.074***	-0.047	-0.169***	0.142**
	(0.023)	(0.059)	(0.037)	(0.07)
Observations	4295	4295	4295	4295
First stage F-stat:	24.347	24.347	24.347	24.347
Panel 3. Age 20 and over				
Years of education	0.082***	-0.037	-0.137***	0.092
	(0.025)	(0.057)	(0.033)	(0.067)
Observations	3935	3935	3935	3935
First stage F-stat:	16.226	16.226	16.226	16.226

Note: Dependent variable is a binary variable equal to one if respondent works in sector X. Years of education instrumented with cohort * county level exposure. Unskilled labor aggregates household/domestic work, service jobs, and unskilled manual labor. Skilled labor aggregates professional/technical/managerial/clerical and skilled manual labor. Standard errors clustered at the county level are reported in parenthesis. Sample restricted to women who have completed at least primary school. All regressions include birth year, county, and ethnicity/religion fixed effects. Regressions are weighted using DHS survey weights. *** indicates significance at the 99 percent level; ** indicates significance at the 95 percent level; and * indicates significance at the 90 percent level.

Table 2.A.10: School openings (by type)

	(1)	(2)	(3)	(4)
(1-transition rate)*FSE period	0.136	0.13	0.176**	0.171**
	(0.099)	(0.105)	(0.081)	(0.083)
(1-transition rate)*FSE period*Public	.	.	.	.
(1-transition rate)*FSE period*Private	.	.	.	.
Observations	423	423	423	423
R^2	0.968	0.97	0.975	0.976
Control variables:				
Constituency development funds * birth year		✓		✓
2009 unemployment rate * birth year		✓		✓

Note: Regressions are run at the county-year level. Dependent variable is the change in number of schools from 2006 levels. Standard errors clustered at the county level are reported in parenthesis. All columns include year and county fixed effects as well as county linear trends. *** indicates significance at the 99 percent level; ** indicates significance at the 95 percent level; and * indicates significance at the 90 percent level.

Table 2.A.11: Class size changes expansion

	(1)	(2)	(3)	(4)
(1-transition rate)*FSE period	4.279	4.991	.	.
	(3.695)	(3.901)		
(1-transition rate)*FSE period*Public	.	.	0.418	0.334
			(4.235)	(3.934)
(1-transition rate)*FSE period*Private	.	.	10.616	14.892*
			(7.314)	(8.009)
Observations	52797	52797	52797	52797
R^2	0.283	0.283	0.301	0.301
Control variables:				
Constituency development funds * birth year		✓		✓
2009 unemployment rate * birth year		✓		✓
County linear trend		✓		✓

Note: Dependent variable is number of students in school cohort and the regressions are run at the school-year level. Standard errors clustered at the county level are reported in parenthesis. All columns include year and county fixed effects. *** indicates significance at the 99 percent level; ** indicates significance at the 95 percent level; and * indicates significance at the 90 percent level.

2.B Robustness samples

2.B.1 Drop Nairobi/Mombasa

The Kenya DHS does not include information on where individuals received their schooling or their county of birth. As my identification exploits geographic variation in exposure to FSE, internal migration poses a potential threat. In this section, I repeat the main educational attainment analysis excluding Kenya's two largest cities and main migration destinations: Nairobi and Mombasa. Figure 2.B.1 depicts a similar shape to the interactions coefficient figure suggesting that the intensity measure is unrelated educational gains prior to the program and correlated with gains following the program. Similarly, table 2.B.1 presents the difference-in-differences estimates illustrating similar impacts on education as the full sample.

Figure 2.B.1: No cities

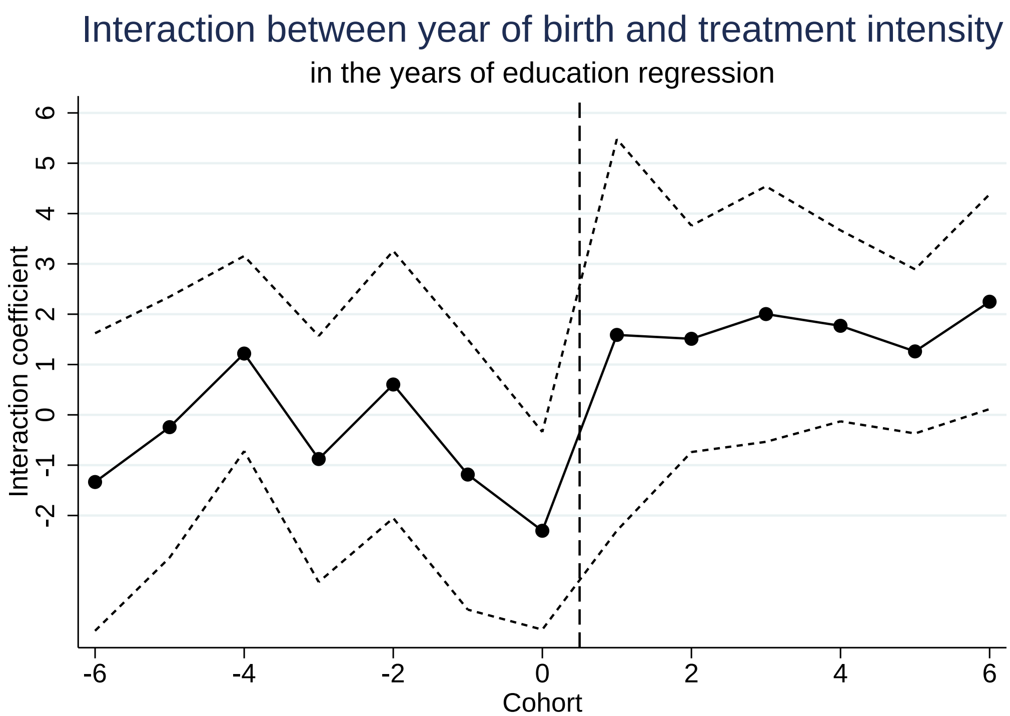


Table 2.B.1: Difference-in-differences estimates: education - no cities

	(1)	**(2)**	**(3)**	**(4)**	**(5)**
Panel 1: years of schooling					
(1-transition rate)*FSE period	2.086***	2.064***	2.024***	2.760***	2.560**
	(0.438)	(0.442)	(0.45)	(1.039)	(1.028)
Observations	12485	12485	12485	12485	12485
R^2	0.092	0.094	0.093	0.098	0.102
Panel 2: completed secondary school					
(1-transition rate)*FSE period	0.153	0.15	0.151	0.188	0.163
	(0.109)	(0.106)	(0.112)	(0.252)	(0.226)
Observations	12485	12485	12485	12485	12485
R^2	0.102	0.104	0.104	0.106	0.109
Control variables:					
Constituency development funds * birth year		✓			✓
2009 unemployment rate * birth year			✓		✓
County specific linear trends				✓	✓

Note: All regressions include birth year, county, and ethnicity/religion fixed effects. Standard errors are clustered at the county level. Regressions are weighted using DHS survey weights. Transition rate defined as the percentage of primary school graduates who attend secondary school. Initial transition rate defined as the average transition rate in each county for students born in either 1989 or 1990. FSE period defined as birth cohorts after and including 1991.

2.B.2 Drop smallest population counties

As described in Section 2.5.1, I define my primary to secondary transition rates using individuals in the 1989 and 1990 cohorts. In this section, I repeat the analysis excluding the smallest population counties for whom the transition rate is calculated based on a small number of observations and thus may be particularly susceptible to measurement error: Garissa, Mandera, Marsabit, Samburu, Turkana, and Wajir. Figure 2.B.2 depicts a similar shape to the interactions coefficient figure suggesting that the intensity measure is unrelated educational gains prior to the program and correlated with gains following the program. Similarly, table 2.B.2 presents the difference-in-differences estimates illustrating similar impacts on education as the full sample

Figure 2.B.2: No small counties

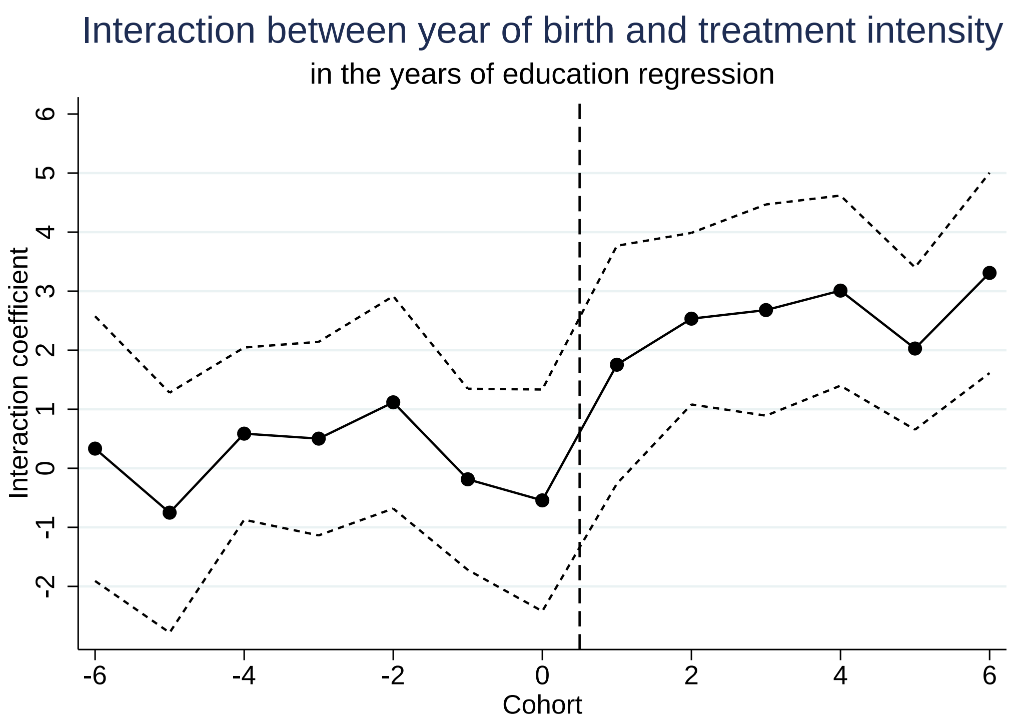


Table 2.B.2: Difference-in-differences estimates: education - no small counties

	(1)	**(2)**	**(3)**	**(4)**	**(5)**
Panel 1: years of schooling					
(1-transition rate)*FSE period	2.252***	2.255***	1.970***	2.029***	2.176***
	(0.316)	(0.318)	(0.369)	(0.731)	(0.688)
Observations	12970	12970	12970	12970	12970
R^2	0.099	0.101	0.1	0.104	0.106
Panel 2: completed secondary school					
(1-transition rate)*FSE period	0.124*	0.143**	0.092	0.157	0.182
	(0.073)	(0.068)	(0.094)	(0.139)	(0.13)
Observations	12970	12970	12970	12970	12970
R^2	0.104	0.105	0.104	0.107	0.109
Control variables:					
Constituency development funds * birth year		✓			✓
2009 unemployment rate * birth year			✓		✓
County specific linear trends				✓	✓

Note: All regressions include birth year, county, and ethnicity/religion fixed effects. Standard errors are clustered at the county level. Regressions are weighted using DHS survey weights. Transition rate defined as the percentage of primary school graduates who attend secondary school. Initial transition rate defined as the average transition rate in each county for students born in either 1989 or 1990. FSE period defined as birth cohorts after and including 1991.

2.B.3 Unrestricted DHS sample (1983-1996)

The main analysis restricts attention to a sample of primary school completers for whom the program could change their decision to attend secondary school. This section relaxes that focus and examines the impacts in the full DHS sample born between 1983 and 1996.

Figure 2.B.3: Full DHS sample

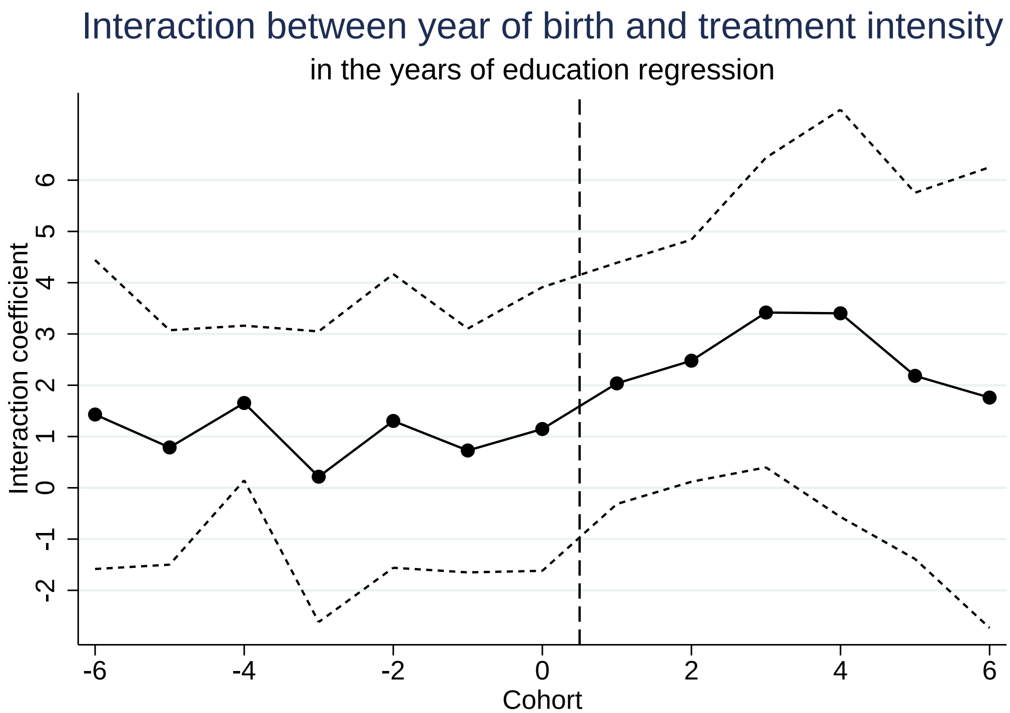


Table 2.B.3: Difference-in-differences estimates: secondary education

	(1)	(2)	(3)	(4)	(5)
Panel 1: years of education					
A. Pooled Gender					
(1-transition rate)*FSE period	1.750**	1.727**	2.328***	1.426	2.073**
	(0.79)	(0.693)	(0.692)	(0.87)	(0.886)
Observations	20458	20458	20458	20458	20458
R^2	0.291	0.292	0.293	0.298	0.299
Panel 2: completed secondary school					
A. Pooled Gender					
(1-transition rate)*FSE period	0.081	0.092*	0.099*	0.066	0.129
	(0.051)	(0.05)	(0.057)	(0.114)	(0.112)
Observations	20458	20458	20458	20458	20458
R^2	0.149	0.15	0.149	0.151	0.152
Control variables:					
Constituency development funds * birth year		✓			✓
2009 unemployment rate * birth year			✓		✓
County linear trend				✓	✓

Note: All regressions include birth year, county, and ethnicity/religion fixed effects. Standard errors are clustered at the county level. Regressions are weighted using DHS survey weights. Transition rate defined as the percentage of primary school graduates who attend secondary school. Initial transition rate defined as the average transition rate in each county for students born in either 1989 or 1990. FSE period defined as birth cohorts after and including 1991. *** indicates significance at the 99 percent level; ** indicates significance at the 95 percent level; and * indicates significance at the 90 percent level.

2.B.4 Alternative treatment definition

The main student achievement results above assumes that individuals in cohorts prior to 2011 are not impacted by FSE. While students in the pre-2011 cohorts were not induced by the program to attend secondary school, there may be resource dilution effects where the quality of the schooling provided to these students is lower due to larger cohorts in the grades below. In this section, I rerun the analysis with an alternative definition of treatment that accounts for the potential impacts of these larger cohorts on the achievement of the pre-FSE cohorts.

2.B.4.1 Defining treatment

Consider a student in the cohort that completed secondary school in 2008. This student completed the first three years of secondary school before FSE was announced and was only impacted by FSE by having a larger cohort in form 1 while this student was in form 4. Therefore, for three years, the student was not impacted and in the final year, the student was impacted only by having a larger cohort in one of the younger grades. Therefore, of the 16 cohorts that were in school while this student attended secondary school, only one was admitted under FSE. Similarly, for a student who completed secondary school in 2009, three of the 16 cohorts were FSE cohorts. With this in mind, I define an alternative intensity measure for county j and cohort k, $\hat{I}_j k$ as the county intensity measure, $I_j k$, multiplied by the fraction of the overlapping cohorts that were admitted under the FSE regime. Compared to the original treatment intensity multiplier which switches from 0 to 1 in 2011, this alternative measure ramps up as shown in figure 2.B.4.

This alternative approach is not valid for the analysis of education attainment but may be more representative of the impact of FSE on educational achievement. With that in mind, I present the results analogous to regression 2.18 and table 2.12.

Figure 2.B.4: Treatment intensity multiplier

	Intensity multiplier	Alternative intensity multiplier
2006	0.000	0.000
2007	0.000	0.000
2008	0.000	0.063
2009	0.000	0.188
2010	0.000	0.375
2011	1.000	0.625
2012	1.000	0.813
2013	1.000	0.938
2014	1.000	1.000
2015	1.000	1.000

Note: Alternative intensity multiplier calculated as the fraction of cohorts that were admitted under FSE during the four years leading up to the KCSE examination. Using the 2010 cohort as an example, when they entered secondary school in 2007, none of the cohorts were FSE cohorts. In form 2, one of the four cohorts were FSE cohorts. In form 3, two of the four cohorts were FSE cohorts. In form 4, three of the four cohorts were FSE cohorts. This generates an average FSE cohort fraction of (0+1+2+3)/16 = 6/16.

Table 2.B.4: Student achievement: alternative treatment intensity

	(1)	(2)	(3)	(4)
A. Full sample				
Alternative treatment intensity	-0.023	0.251	.	.
	(0.068)	(0.175)		
Alternative treatment intensity*Female	.	.	-0.059	0.141
			(0.069)	(0.188)
Alternative treatment intensity*Male	.	.	0.024	0.372**
			(0.076)	(0.184)
Observations	3321504	3321504	3321504	3321504
R^2	0.039	0.221	0.049	0.238
B. High performers				
Alternative treatment intensity	0.151	0.419	.	.
	(0.259)	(0.334)		
Alternative treatment intensity*Female	.	.	0.188	-0.211
			(0.287)	(0.378)
Alternative treatment intensity*Male	.	.	.	0.751**
				(0.328)
Observations	269436	269436	269436	269436
R^2	0.357	0.409	0.361	0.418
Control variables:				
Constituency development funds * birth year		✓		✓
2009 unemployment rate * birth year		✓		✓

Note: Dependent variable is standardized KCSE score. Standard errors clustered at the county level are reported in parenthesis. All columns include county fixed effects and county linear trends while Panel B also includes year fixed effects. Columns 2 and 4 also include dummies for public schools, single gender schools, and district level schools. Controls are interacted with gender for columns 3 and 4. *** indicates significance at the 99 percent level; ** indicates significance at the 95 percent level; and * indicates significance at the 90 percent level.

2.C Simulation

2.C.1 Simulation adding lower quality students

This section details a simulation designed to measure the impact of a hypothetical policy that only adds lower quality students. In the pre-FSE period, I keep all students and their grades. For the post-FSE period, I first keep the highest performing students in each county where the number of students kept is equal to the 2010 county cohort size. This yields a sample of individuals, in each county and for each year, that is equal in size to the 2010 cohort. I then add any students observed in the exam but not included in this sample to the sample with an assigned score of 0. For all post-FSE individuals I then randomly draw a value from a uniform [0,1] distribution which is added to their score. I then rescale the post-FSE grades to match the empirical pre-FSE distribution. As desired, this process yields a sample where any additional students added after the introduction of FSE are assumed to be lower performing than the existing student body: the high performing students are of the same size and distribution across counties as the last pre-FSE cohort and all new students are assigned random grades and across counties in proportion to actual student body growth. I bootstrap this process 1,000 times.

Table 2.C.1: Simulated impact on student achievement (under no credit constraints)

	(1)	**(2)**
(1-transition rate)*FSE period	-0.303***	-0.335***
	(0.001)	(0.001)
Observations	3326790	3073281
R^2	0.019	0.213
Control variables:		
Constituency development funds * birth year		✓
2009 unemployment rate * birth year		✓
County linear trend		✓

Note: Dependent variable is adjusted standardized KCSE score. Scores in post-FSE period simulated assuming all additional students in a county beyond 2010 county registration are the lowest performing students in the county. Scores were randomly generated for these students and then normalized to match the 2010 score distribution. All columns include county fixed effects. Estimates obtained from bootstrapped simulation. R^2 from single run. *** indicates significance at the 99 percent level; ** indicates significance at the 95 percent level; and * indicates significance at the 90 percent level.

Chapter 3: Can Government School Upgrades Up Grades? Evidence from Kenyan Secondary Schools

3.1 Introduction

Over the past 20 years, countries throughout sub-Saharan Africa have dramatically increased education access. Between 1999 and 2012, the region's net primary enrollment rate increased from 59% to 79% (UNESCO, 2015).[1] Student learning, however, remains low as students from developing countries consistently and substantially underperform relative to their counterparts in developed countries (Pritchett, 2013). Given the demonstrated relationship between human capital development and growth, and the recognition that human capital is better captured by cognitive skills than schooling attainment, low quality schooling may lower economic growth (Hanushek and Wößmann, 2007). This may be particularly true at the post-primary level as secondary education has been shown to decrease the probability of self-employment and increase the probability of skilled work (Ozier, Forthcoming; Brudevold-Newman, 2016) and labor flows from low-productivity sectors to high-productivity sectors have been shown to be a key driver of development (McMillan

[1] UNESCO Institute for Statistics estimates that the region's net secondary enrollment rate increased from 20% to 33% over the same period.

and Rodrik, 2011; McMillan, Rodrik, and Verduzco-Gallo, 2014).

This paper evaluates the impact of a government funded school promotion program for high-performing public secondary schools. The promotion from a mid-level tier to the top-level tier afforded the selected schools flexibility to charge higher fees, granted higher priority in teacher assignment, and came with a USD300,000 grant to improve school facilities. There are three main contributions of this paper. First, it estimates the impact on academic outcomes of a government implemented program specifically targeting school quality. While the number of low-cost private schools is growing, public schools remain the dominant source of education for many, suggesting that any meaningful improvements in school quality will likely depend on government programs improving quality at public institutions. Second, I provide estimates of the impact of a particularly large block grant issued directly to schools, and use detailed descriptions of how the schools spent their additional funding to identify correlates of increased academic performance. Finally, I measure the impact of the program on the composition of the student body, illustrating how the implementation of the national central student assignment mechanism likely caused a decrease in the quality of students attending the upgraded schools following the program.

My identification of causal impacts exploits the school eligibility criteria used in the implementation of the program. I use a difference-in-differences approach to compare the outcomes of students at schools selected for the program against students at schools that met the upgrade program eligibility criteria but were not selected for the program. This methodology relies on two main assumptions. First, the selection of the upgraded schools from the pool of eligible schools needs to be attributable to fixed characteristics, so that any selection bias is absorbed by school fixed effects. I demonstrate that the highest scoring eligible schools were often upgraded suggesting that pre-program average examination results were the primary driver of school selection. The second assumption is common trends across the treated and comparison schools. I use pre-treatment data to demonstrate that, despite a difference in performance levels, the trends in test performance of students at upgraded schools tracked closely with the performance of students at the comparison schools.

As the program raised the prestige of upgraded institutions, one threat to identification is the possibility that students responded to the program by differentially seeking enrollment at treated schools, changing their composition relative to comparison schools. With this in mind, I use cohorts of students who enrolled in the sample schools before the program was announced; thus, the sample students all enrolled in medium-tier schools, some of which were subsequently upgraded to national-tier schools. I also demonstrate that within these cohorts, upgraded schools did not experience differential cohort growth.

My difference-in-differences estimates suggest heterogeneous program impact: while the program had no measurable impact for girls, the program improved overall examination scores for boys by 0.12 standard deviations with larger gains estimated for English and Swahili scores. The improved scores for boys appear to be driven by shifting up the lower tail of the test score distribution. I also demonstrate that among the upgraded schools, improvements in test scores are correlated with having spent program funds on basic furniture for students such as desks and tables.

This paper contributes to several literatures. First, it contributes to the wide variety of interventions attempting to improve school quality that have been implemented and evaluated, and which have led to an extensive set of systematic reviews (Glewwe, Hanushek, Humpage, and Ravina, 2011; Kremer, Brannen, and Glennerster, 2013; Murnane and Ganimian, 2014; McEwan, 2014). These reviews consistently recommend three classes of interventions: programs that tailor teaching to student skills (such as streaming or certain ICT interventions), repeated teacher training, and improving teacher accountability (Evans and Popova, 2015). Two of the reviews also suggest that interventions which change the students' daily learning experiences may also be particularly effective (Glewwe, Hanushek, Humpage, and Ravina, 2011; Murnane and Ganimian, 2014). The reviews are consistent in finding no positive impact of increased monetary resources on academic achievement.

Despite discouraging results from the existing literature, the fact that it was high-performing secondary schools that were impacted by the upgrade program, combined with the research examining elite-oriented curricula may help account for the positive impacts found here.[2] The majority of the existing evidence on the efficacy of additional funding on student outcomes stems from studies conducted at the primary school level. The lack of demonstrated impact at this level may be attributable to other binding constraints such as a lack of accountability that

[2]While tracking and repeated teacher training have been shown to improve academic outcomes there is no evidence to suggest either was implemented as a result of the program. Also, while larger systems changes such as local contract teachers or enhanced parental involvement in parent-teacher associations have been successful, these policies are unlikely to be affected by the program given the national nature of the schools.

are not eased by additional resources; secondary schools may face a different set of constraints and may be able to benefit from additional funding. Earlier research has highlighted that the Kenyan curriculum is set at a level appropriate for students in elite schools and may be inappropriate for the majority of students attending schools frequently burdened by barriers such as high teacher absence (Glewwe, Kremer, and Moulin, 2009; Kremer, 2003). This elite-focused system may explain the lack of demonstrated impact among earlier interventions if they targeted lower-performing schools and were not designed to overcome this curriculum-based barrier to learning. On the other hand, high-performing schools with high quality students, as is the case in this program, may be better equipped to benefit from additional resources.

My results also complement earlier research examining the marginal impact of elite-tier secondary schools on student outcomes. Using a regression discontinuity approach, Lucas and Mbiti (2014) find no evidence of value-added from the Kenyan national-tier schools suggesting that observed differences in outcomes across school quality tiers are the result of student selection rather than differential learning. The authors also reject heterogeneous treatment effects for students of different quality within the top tier schools which consists of students admitted to county schools. As they focus on students just above and below the national school cutoffs, their sample likely aligns with the upper end of my sample. In line with their results, I find no evidence of an impact on the upper end of the sample student distribution for either boys or girls. My finding that the upgrade program shifted the lower end of the distribution to the right for students at the upgraded boys' schools suggests that lower ability students may benefit from the additional resources afforded to the higher tier schools.

Finally, my results also contribute to the literature focusing on school choice and student preferences in centralized allocation systems. Recent empirical work has exploited individual-level preferences to demonstrate the potential for the mechanisms to stratify students by socioeconomic status in both the U.S. and Ghana (Hastings and Weinstein, 2008; Ajayi, 2013), measure efficiency gains associated with eliciting more preferences (Ajayi and Sidibe, 2015), explore gender differences in submitted preferences (Ajayi and Buessing, 2015), and examine patterns in preference submission errors (Lucas and Mbiti, 2012b). I highlight that a decrease in admitted student quality is likely attributable to the specific structure of the preference submission mechanism.

The remainder of the paper is structured as follows: Section 3.2 provides a background of Kenya's education system and the school upgrade program, Section 3.3 describes the data, Section 3.4 describes my difference-in-differences and changes-in-changes identification strategy, Section 3.5 presents the impacts of the upgrade program on student achievement, Section 3.6 examines the impacts of the program on the composition of the student body, and I conclude in Section 3.7.

3.2 Kenya's Education System and the School Upgrade Program

Kenya's education system consists of 8 years of primary school, 4 years of secondary school, and 4 years of university. Standardized tests are administered at the conclusion of both primary school and secondary school: the Kenya Certificate of Primary

Education (KCPE) is used to determine admission into secondary school while the Kenya Certificate of Secondary Education (KCSE) determines admission and funding for higher education and is also used as a credential on the labor market. The exams are conducted by a national testing organization - the Kenya National Examinations Council - and are centrally developed and graded. The public secondary education system is tiered with schools categorized as either national, county, or district schools.[3] Admission to public secondary schools is obtained through a central mechanism that allocates students based on KCPE scores and student submitted preferences over schools. Students submit ranked lists over schools in each of the three public school tiers, submitting four national school choices, three county school choices, and a district school choice. The student preferences are submitted at the time of registration for the KCPE examination, approximately 9 months before the exam. Students are assigned via a student-proposing deferred acceptance algorithm similar to that of Gale and Shapley (1962). In general, this mechanism assigns the top performers from each county to schools in the national tier, high performers to schools in the county tier, and the remaining students to either the district or are left unassigned due to capacity constraints.[4] Because students can only list four national schools, they will be assigned to either a county or district school if all of the national schools they listed are full, even if there is capacity remaining at a

[3]Of the 8,228 secondary schools that administered the secondary school completion examination in 2014, 94 were national schools, 1,222 were county schools, 5,444 were district schools, while 1,468 were private schools. All national schools are single gender while 75% of county schools and 10% of district schools are single gender.

[4]Of the 2014 secondary school graduates, only 11% were assigned to either national or county tier secondary schools when they joined secondary school in 2010. Ozier (Forthcoming) details how the KCPE score is used to determine eligibility for secondary school admission.

national school that they did not list and which they may prefer over their assigned school.

In addition to receiving preferential assignment of students, national schools also have better educated staff with more experience and more extensive facilities, such as computer labs and classroom space (Lucas and Mbiti, 2014).[5] The numerous advantages afforded to the national schools bear out in their performance on the KCSE: the average grade of a student in a national school in 2010 was 67 (B+/B) while that for county and district schools was 39 (C/C-) and 28.95 (D+) respectively.[6] This paper evaluates a government program to upgrade selected schools from county-level schools to national-level schools.

Between 2011 and 2014, the Kenyan government upgraded 76 county schools to national schools with the explicit goal of ensuring that each county had two national schools: one for boys and one for girls. The upgrade eligibility criteria were established by the Ministry of Education and based on school KCSE performance over the prior 5 years, existing physical infrastructure, geographic equity, and community support (Kenya National Assembly Official Records, 2011).[7] To meet the performance criteria, each school had to have a mean KCSE grade of C+ or higher

[5] In contrast to Kenyan primary schools, overall attrition at the secondary school level is quite low with secondary school survival rates of around 92% (Ministry of Education, 2008b). Similarly, repetition rates at the secondary school level are low: KCSE registration data show that almost 80% of students proceed through secondary school in 4 years. Both repetition and dropout are likely to be lower for students attending the national and county schools where students are generally the children of the elite or middle class and where credit constraints or the price of schooling are less likely to influence schooling decisions.

[6] A full description of the KCSE examination is provided in Section 3.3.

[7] Although not explicitly listed by the Ministry of Education as an eligibility requirement, all upgraded schools were public. Ministry of Education officials confirmed that two schools were selected for the upgrade program but declined: Kapropita Girls in Baringo county and Chebisass Boys in Uasin Gishu county. These schools are excluded from the analysis.

over the 2006-2010 period. For counties that did not have any schools that met the grade eligibility criteria, lower KCSE thresholds of C and C- were used for boys' and girls' schools, respectively. The infrastructure criteria required that each school be single gender and have existing boarding facilities. The geographic criteria were inherent in the program's design; two eligible schools were upgraded in each county, one girls' school and one boys' school.

Each selected school was allocated KSh25 million (USD300,000) for improvements. While the Ministry of Education was explicit that the funding had to be spent on school infrastructure, the specific purchases were left to the schools with ministry audits confirming the expenditures. The first group of 30 upgraded schools was announced in 2011 and began admitting students as a national school in 2012. The second group of 30 upgraded schools was announced in 2012 and began admitting students as a national school in 2013. The last group of 16 upgraded schools was announced in 2013 and began admitting students as a national school in 2014.

3.3 Data

This paper makes use of two administrative datasets: the first comprises the KCSE examination results of all students who took the exam between 2006 and 2014 with the exception of the 2012 cohort while the second contains KCPE scores, submitted secondary school preferences, and assigned secondary schools for students assigned to either national or county schools between 2010 and 2014.[8]

[8]The test data are a combination of publicly available data from 2006-2008 together with data scraped from the national examination council's website for 2009-2011 and 2013-2014. The national examination council web site did not have the 2012 KCSE results publicly available.

3.1 KCSE and Secondary School Data

The KCSE consists of a minimum of 7 exams across four subject categories: three compulsory subjects (English, Kiswahili, and math), 2 science subjects, 1 humanities subject, and 1 practical subject.[9] Each subject is graded on a 12(A)-1(E) scale with a maximum total score of 84 points. Each student is assigned an aggregate grade between A and E based on their composite score.[10] Detailed subject grades are available from 2009 to 2014, while only the overall letter grades are available prior to 2009. The data prior to 2009 is used to identify schools that met the upgrade program eligibility criteria while the primary analysis uses the detailed results available from 2009 to 2014. Summary statistics for all students for each exam between 2009 and 2014 are presented in Table 3.1. Column 2 details the average grades of students from the national schools in 2009 and illustrates the stronger performance of the national school students who average between a 10 (B+) and 8 (B-) for each subject while the overall average is generally between a 6 (C) and 3 (D).[11]

[9]Science options include biology, chemistry, and physics. Humanities options are history/government and geography. Practical subjects include Christian religious education, Islamic religious education, Hindu religious education, home science, art and design, agriculture, woodwork, metalwork, building construction, power mechanics, electricity, drawing and design, aviation technology, computer studies, French, German, Arabic, Kenyan sign language, music, and business studies.

[10]Overall KCSE grades are assigned as follows: a score between 84 and 81 is an A, 80 to 74 is an A-, 73 to 67 is a B+, 66 to 60 is a B, 59 to 53 is a B-, 52 to 46 is a C+, 45 to 39 is a C, 38 to 32 is a C-, 31 to 25 is a D+, 24 to 18 is a D, 17 to 12 is a D- and below 12 is an E.

[11]The differences between the summary statistics of the national schools and all schools also highlights the difficulty inherent in measuring education intervention effect sizes in standard deviations. The standard deviation of overall test scores for students in national schools is about 25% less than the overall test taking population. In an intervention designed to improve high-performing schools, it is not clear which of the standard deviations is a more relevant benchmark. As such, I report raw grade impacts and reference the standard deviation of the overall student body in 2009.

Each student record within the dataset is identified by a 9-digit student number that is unique within each year. The first six digits of the student number indicate the school at which the test was administered while the last three denote the student within the school. Each upgraded school received a new school code at the time it was promoted. The new national school codes are mapped back to the county school codes using the school name and county. Additional data on school characteristics come from the Ministry of Education's Kenyan Schools Mapping Project conducted in 2007. I supplement my analysis with information from the Ministry of Education detailing how each school reported spending their upgrade grant. While the dataset includes spending type, it does not include the amount spent on each category. Figure 3.1 illustrates the spending categories selected by upgraded schools.

3.2 KCPE and School Preference Data

The KCPE consists of five subject tests - English, Swahili, math, science, and social studies/religious education - each of which is graded out of 100 points. An overall KCPE test score is assigned as the sum of the five subject grades and is out of 500 points. This paper makes use of an administrative dataset of individual-level KCPE examination results between 2010 and 2014. I combine the examination results together with individual-level data on the submitted preferences over secondary schools and their assigned school. The preference and assignment data were available only for students assigned through the central mechanism and covers approximately the top 20% of the student body in each year.

3.4 Identification Strategy

3.1 Main specification

I identify the effect of school upgrading on student achievement by comparing the KCSE results of students who were admitted to county schools that were then upgraded ("upgraded schools") to students admitted to other county schools that met the government's eligibility criteria but which were not selected to be upgraded ("eligible schools"). For this difference-in-differences approach, the primary regression is of the form:

$$y_{ijt} = \beta_0 + \beta_1 T + \beta_2 X_{jt} + \lambda_t + \gamma_j + \varepsilon_{ijt} \tag{3.1}$$

where y_{ijt} is the KCSE score of student i in school j in year t. The school upgrade program is represented by T, which is an indicator variable equal to one for upgraded schools once they have been upgraded. I include annual fixed effects, λ_t, to account for any differences in test difficulty and a vector of school characteristics that change over time, X_{jt}, which will include the number of students registered for the exam at school j in year t. Had the upgrade program been randomly assigned to eligible schools, a regression of y_{ijt} on T for all eligible schools would consistently estimate the impact of the upgrade program. I also include school fixed effects, γ_j, to capture school specific characteristics and ensure that any school specific attributes that led to upgrading are not relegated to an unobservable correlated with treatment.

As the upgrade program was implemented with the goal of introducing two national schools in each county, one for boys and one for girls, each upgraded school represents a county-gender pair. While some counties had a number of schools that met the eligibility criteria, other counties had only a single school that met the criteria so that there are no natural comparison schools.[12] In cases where only one school was eligible and upgraded, the school is excluded from the analysis. The eligibility criteria identify 104 eligible, but not upgraded, schools that pair with 49 of the 76 upgraded schools.[13]

Table 3.2 presents summary statistics for the upgraded and eligible schools. The upgraded schools are slightly higher performing, closer to cities and main roads, and also have more teachers and acreage although the differences are insignificant for all variables except acreage. The comparison of upgraded and eligible schools across genders are similar although upgraded schools are closer to cities and roads for the boys' schools and further for the girls' schools. Figure 3.2 maps the sample schools which are mainly concentrated in the former Central, Eastern, Rift Valley, and Western provinces of Kenya.

To avoid student selection issues, I focus only on students attending and admitted to the sample schools prior to their upgrade to national status. The analysis makes use of the fact that the students at the upgraded schools, like the students at the comparison schools, were originally admitted to middle-tier schools. The

[12]In the regressions, eligible schools are weighted based on the number of eligible schools in the county so results are not biased by specific counties with a large number of eligible schools. Appendix Table 3.A.3 presents an alternative analysis that includes only one comparison school per county. The comparison school is chosen as the school with the closest mean KCSE score over the prior five years. There are no substantive changes in the results.

[13]In line with the implementation of the program, I consider public status as an additional eligibility criteria of the program. The sample approximately splits by gender with female schools comprising 27 of the 49 upgraded schools and 53 of the 103 comparison schools. Appendix Table 3.A.4 presents an alternative analysis that includes only counties where both the boys' and girls' schools have eligible comparison schools.

students at the upgraded schools differ in that they subsequently received one or more years of education at a national-tier institution. As the first set of students admitted to the newly upgraded schools enrolled in 2011, they took the KCSE in 2015; my data includes KCSE results through 2014 which should mitigate the possibility that individuals selected into treated schools as the sample students were all initially admitted to schools of the same tier quality. Further, I test for differential cohort size growth at treatment schools. The main identifying assumption of this difference-in-differences fixed effects approach is that the two groups of schools (upgraded and eligible) follow common trends prior to the intervention.

Figure 3.3a uses the 2006-2011 data on aggregate KCSE score to evaluate the comparability of the upgraded and upgrade-eligible schools prior to the implementation of the program. The trend lines of the upgraded and eligible schools suggest that it is unlikely that the program was randomly assigned among all eligible schools as the upgraded schools outperformed other eligible schools not selected. However, once we account for the differences in levels, the trends of the two groups follow very closely supporting the inclusion of fixed effects in the above regression. Figures 3.3b and 3.3c present equivalent figures for the split sample of boys' schools and girls' schools. Evident in Figures 2 and 3 - and Figure 1 to a lesser extent - are small deviations from common trends in 2007 and 2008.[14] Given these deviations and the greater detail test score results, I restrict attention in the analysis to the post 2008 period where the figures suggest that the common trends hold.

[14]These small deviations could be the result of the Kenyan election and subsequent post-election violence in late 2007 into 2008.

I examine the validity of the common trends assumption more formally by looking at whether the grades of students in upgraded schools changed differentially between 2006 and 2010 relative to the grades of students in the eligible schools.[15] This is equivalent to running a regression of the form:

$$y_{ijt} = \delta_0 + \delta_1 \tau + \delta_2 T * \tau + \gamma_j + \epsilon_{ijt} \tag{3.2}$$

where y_{ijt} are individual test scores of student i at school j in time t, τ is a time trend, T is a dummy variable equal to one for the eventually upgraded schools, and γ_j are school fixed effects.

Another threat to identification in the above model could be that students respond to the new national schools by transferring to the upgraded schools which would result in potential composition effects. I can test for differential cohort growth across the new national schools by running regressions of the form:

$$n_{jt} = \alpha_0 + \alpha_1 T + \lambda_t + \gamma_j + \varepsilon_{ijt} \tag{3.3}$$

where n_{jt} is the KCSE cohort size of school j in time t. α_1 captures the impact that the national school upgrade has on the number of students taking the KCSE. A significant coefficient for α_1 could indicate that students are transferring to the school or that there is a compositional effect whereby the school is registering a

[15] As subject specific data are available only from 2009 onwards, I examine the common trends between 2009 and 2010. I restrict attention prior to 2011 to avoid any contamination from schools that may have received some benefit from the upgrade program between when it was announced in 2011 and when it admitted students as a national school in 2012.

greater or fewer number of students to take the KCSE exam in the year. Results from this regression are presented in table 3.4. Upgraded schools did not see a significant change in the number of students registering for the exam following their promotion to the national tier.

Another possible identification strategy would exploit the phased-in nature of the upgrade program and compare the outcomes of schools that were upgraded early to those that were phased in later. Without the 2012 cohort, this amounts to comparing the outcomes of students at schools upgraded in 2012/2013 to those upgraded in 2014. Figures 3.4a and 3.4b again use the 2006-2011 data on aggregate KCSE score to evaluate the comparability of the schools phased in first to those upgraded later. The very different trends in test scores suggest that the two groups are not comparable and that this alternative identification strategy is not valid.

I examine the impact on the composition of the incoming student body using regressions of the form represented by equation 3.1. In this analysis, I use the KCPE scores of incoming students to test whether the composition of the incoming cohorts are different than those entering before the upgrade program.

3.2 Changes-in-changes

The upgrade program could also alter the distribution of KCSE results if the benefits of the program accrued to students at a certain point in the test score distribution. I employ the changes-in-changes (CiC) model of Athey and Imbens (2006) to examine the impact on the entire distribution of test scores. The CiC model is a generalization of the difference-in-differences estimator that estimates the entire counterfactual distribution of a treated group which is identified under the assumption that the changes in the distribution of the treated and comparison groups would, absent treatment, be the same. The standard estimator considers the impact of a binary treatment across two time periods. I consider the pre- and post-upgrade periods and compare the upgraded schools to the eligible but not upgraded schools. The treatment effect at quantile q is calculated as:

$$\tau_q^{CiC} = F^{-1}_{Y^1,11}(q) - F^{-1}_{Y^N,11}(q) = F^{-1}_{Y^1,11}(q) - F^{-1}_{Y,01}\left(F_{Y,00}\left(F^{-1}_{Y,10}(q)\right)\right) \tag{3.4}$$

where $F_{Y^1,gt}$ is the cumulative distribution function of group g in time t. The CiC model imposes three main assumptions.[16] First, the potential test scores of untreated individuals $\left(KCSE_i{}^N\right)$ should satisfy:

$$KCSE_i{}^N = h(U_i, T_i) \tag{3.5}$$

where U_i is an underlying unobserved ability and T_i is the time period in which the test was taken. Second, CiC imposes a strict monotonicity framework that the test score production function $h(U_i, T_i)$ be strictly increasing in u. Third, the underlying ability distribution within a group can not vary over time:

$$U_i \perp T_i | G_i \tag{3.6}$$

[16] These are laid out in Athey and Imbens (2006) Assumption 3.1-3.3. An additional common support assumption (Athey and Imbens (2006) assumption 3.4) requires that outcomes of the treated group in any period be a subset of the untreated outcomes.

I consider students who were all admitted to the schools prior to the upgrade program, when the upgraded schools were all known as high performing county schools. As such, it seems likely that the students are of consistently high ability.[17] I control for the school cohort size following the parametric approach suggested by Athey and Imbens (2006) and which is both employed and described in a similar context by Lucas and Mbiti (2012a).

3.5 Student Achievement Results

Table 3.5 presents the difference-in-differences estimates represented by equation 3.1 for the core KCSE subjects, where each coefficient represents the impact of attending an upgraded school. Column 1 shows that there is a positive but insignificant effect of upgrading across all schools. Overall, the program is estimated to have marginally significantly increased only English scores. Columns 2 and 3 split the sample to examine the impact of the upgrade program separately for the sample of boys' schools and girls' schools. Column 2 shows that the program is estimated to have significantly increased examination scores at upgraded boys' schools by 0.35 points (0.16 standard deviations) where a one point increase represents a one letter point increase (e.g C to C+) in each subject.[18] Conversely, column 3 shows

[17] Lucas and Mbiti (2012a) employ the CiC framework to examine the impact of free primary education in Kenya. To satisfy the requirement that the underlying ability distribution within a group not vary over time, they restrict their focus to the top half of the distribution where free primary education was less likely to have impacted their schooling decisions but would have still impacted their schooling inputs. The current context avoids the composition changes by focusing on students already enrolled in the sample schools prior to the announcement of the upgrade program.

[18] The impact reported in standard deviations is relative to the overall examination standard deviation. The sample standard deviation is much smaller so that the 0.36 point increase represents a 0.40 standard deviation increase.

that the program is estimated to have had a negative but insignificant effect on the academic achievement of students at upgraded girls' schools. The second row examines the impact on the percentage of students who qualify for preferential university admission and funding, which requires scoring above a threshold score.[19] Male students at upgraded schools were 8% more likely to qualify for the preferential admission and funding. Across the two columns, the coefficients show that the overall significant coefficient for English is entirely driven by the large (0.16 standard deviations) and highly significant impact on boys English scores as the coefficient for girls is negative and insignificant. The program is also estimated to have had a positive and significant impact on boys Swahili scores of 0.29 standard deviations and a weakly negative impact on girls Swahili scores. Treatment appears to have had no impact on any test scores for students at the upgraded girls' schools.[20]

Table 3.6 presents the changes-in-changes estimates across the score distributions for the boys' schools and girls' schools.[21] For the boys' schools, the estimated impact is significant across the lower end of the distribution, a trend also evident in the Swahili and math results. Importantly, these gains did not appear to come at the expense of students at the upper end of the distribution where there is no evidence of negative impacts. The results also indicate that the upgrade program improved English scores across the whole distribution, including at the upper end.

[19] The threshold score for males was 63 in 2009-2011 and 60 in 2013-2014. The requisite score for females was 2 points lower (61 and 58).

[20] Appendix Table 3.A.1 shows that the difference between the estimated coefficients for boys and girls in pooled gender regressions is at least weakly significant across all subjects.

[21] Appendix Table 3.A.5 similarly explores whether the upgrade program had heterogeneous effects at different achievement levels within the schools by showing school-level regressions examining the impact of the upgrade program on the 25th, 50th, 75th percentile scores.

In contrast, the upgrade program is not estimated to impacted test scores for students attending the upgraded girls' schools at any point in the distribution and on any of the exams.

Table 3.7 examines whether the upgrade program impacted the standard deviation of the scores of the treatment schools. As suggested by Table 3.6 where the gains are larger for the lower end of the distribution, the estimated impact of treatment on the standard deviation is negative. This is true for the overall score, Swahili score, and math score for all schools, as well as the Swahili and overall KCSE scores for the boys' schools.

Taken together, Tables 3.6 and 3.7 suggest an upward shift and compression of the test score distribution for the test scores of boys' schools and no change for the distribution of test scores of girls' schools. While the upward shift of the boys' scores are observed in the overall KCSE scores, the compression of the test score distribution arises from greater gains for lower performing students relative to higher performing students and is confirmed by smaller school test score standard deviations. This suggests that the upgrade program conveyed the greatest benefits to students at the lower end of the test score distribution.

Table 3.8 examines the impact of receiving a larger relative grant by exploiting the fact that upgraded schools were of different sizes so that the grants, which were of constant dollar amount, were of different value in terms of dollars per student. I split the sample in half based on their 2011 cohort size to create two different dummy variables. The first dummy variable equals one in post-upgrade periods for the smaller upgraded schools (more dollars per student) and the second dummy variable equals one in post-upgrade periods for the larger schools (fewer dollars per student).[22] Using 2011 cohort numbers, the high dollars per student category received about 40% more per student than the low dollar per student category. In spite of the greater relative funds, I am unable to reject equal impact on overall test scores.[23]

One mechanism that could lead the upgraded schools to improve grades without improving human capital would be to encourage students to take easier elective subjects. Table 3.1 shows that students taking government/history score between 0.5-1 point higher than students taking geography, a subject that meets the same curriculum requirement. Similarly, of the sciences, both biology and physics consistently award higher grades than chemistry. Table 3.9 presents regression results looking at whether the upgrade program changed the subjects students choose. The table shows coefficients from a series of linear probability model regressions for each of the optional subjects from the science and humanities categories.[24] The table suggests that there was shifting of students towards geography and away from government/history which would be expected to lower overall performance. The coefficients in the last line of the table indicate an insignificant increase at both the boys and girls schools in the number exams taken by each student.

Table 3.10 examines the correlates of improved test performance by running regressions with the estimated impact on school test scores as the dependent variable

[22]As the upgraded girls' schools are larger on average, the sample is split by gender before splitting again by school size so that the indicator variables are balanced by gender.

[23]Appendix Table 3.A.2 presents the results separately for the core subjects with similar findings.

[24]English, Swahili, and math are compulsory for all students and so are not affected by the upgrade program.

and binary variables for grant spending as the independent variables. Unfortunately, the small sample provides little insight into positive spending: overall only spending on student furniture which included student desks, tables, and beds, is associated with improved outcomes. This finding is consistent with earlier literature reviews which found that interventions that provide additional resources which change the daily learning environment have the most impact.

3.6 Student Composition and Preference Changes

As described in Section 3.2, students are centrally allocated to secondary school based on their primary school completion examination score and listed preferences. In this section, I use incoming student preference and assignment data to demonstrate that the composition of the new national-tier schools' student body changed following the introduction of the national school designation.

Figure 3.5 shows the proportion of the total national-tier preference slots assigned to each national-school type from 2010 to 2014.[25] As expected, in 2010 and 2011 almost all of the students list the original national schools in their four national school slots.[26] The 2012 introduction of 30 new national school options appears to have been known and have opened up desirable schools as slightly less than half of the preferences submitted in 2012 were for the new schools. This contrasts with the 46 schools upgraded in 2013 and 2014 which account for less than 20% of the listed

[25] Recall that while the number of national schools increased between 2010 and 2014, students remained constrained to providing a ranked list of only four national schools.

[26] Lucas and Mbiti (2012b) detail the causes and consequences of errors made in the listing of secondary school preferences. These errors could account for the listing of non-original national schools.

preferences in 2014.

The limited interest in some of the new national schools together with the higher number of national schools relative to national school preference slots suggests an ambiguous impact on incoming student quality. If a district is allocated a slot in a national school, the allocation mechanism will run through all students in that district in descending score order until it finds a student who listed that school among his/her preferences. With an additional 76 schools to list and the number of slots held constant at four, the probability that a lower performing student is the first to list a certain national school increases, which could serve to decrease the quality of students entering the new national schools.

Table 3.11 presents the results of regressions represented by equation 3.1 where the dependent variable is the primary school completion examination (KCPE) scores of incoming students. Overall, the quality of the admitted students in the upgraded schools decreased following their promotion to national school status. This result could be attributable to the larger number of national schools and the fact that the allocation mechanism needed to go further down the student list before finding a student from each district who listed each national school as one of the four national school preferences.[27] This decrease in incoming student quality suggests that a simple before-and-after analysis of student performance at upgraded schools that made use of students admitted after the upgrade program was implemented

[27]In 2016, the National Examinations Council and the Ministry of Education changed the preference submission structure. Instead of listing any of the national schools in each of the national school slots, students were required to select schools only from a list for each of the slots. By placing the traditional national schools in a single category, students were unable to consistently list the same schools making it less likely that low-ability students would be the first to list a certain national school.

would be inappropriate. Also, if peer effects across grades are particularly strong, it is possible that the relatively lower quality of incoming students in the girls' schools could bias downwards the impact of the upgrade program on students already enrolled.

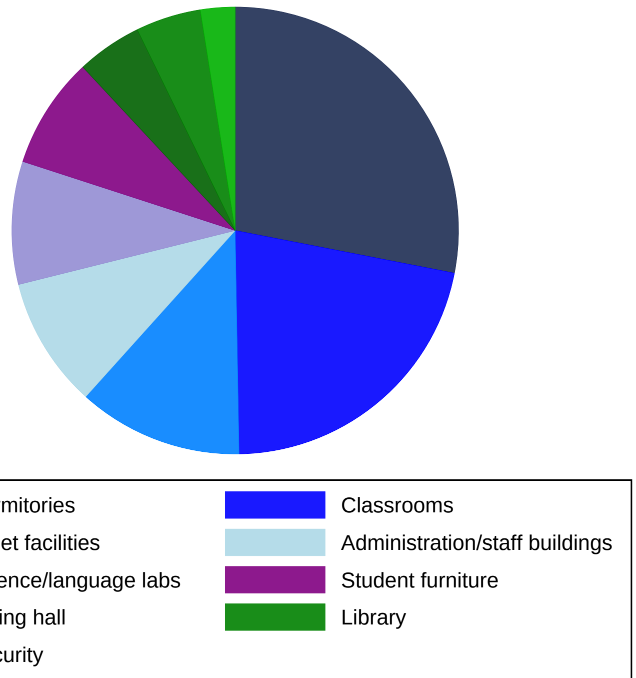
3.7 Conclusion

Between 2011 and 2013, the Kenyan government upgraded selected high-performing secondary schools. I identify the impact of the program by comparing students already in the selected schools to students in other schools that were eligible but not selected for the program. The program had a heterogeneous impact on academic achievement of students at the impacted schools. The program improved examination scores for students at upgraded boys' schools but had no impact on upgraded girls' schools. Boys' mean overall grades at upgraded schools increased by 0.16 standard deviations. Boys' English and Swahili examination scores increased by 0.16 and 0.23 standard deviations respectively. In addition to the increase in examination scores, the program led to a rightward shift and compression of the test score distribution resulting from benefits that accrued to lower-performing students at the upgraded schools. While the overall test score gains are small relative to the resources allocated to the program, it does represent a government implemented school improvement program that successfully increased academic outcomes.

There are a number of possible explanations for the fact that the program improved achievement at boys' schools but not girls' schools. First, average performance was higher at the upgraded boys' schools; it is possible that the school inputs affected by the program are not the binding constraint facing the slightly lower achievement girls' schools.

In addition to demonstrating impact on student achievement, I also demonstrate that the program decreased the composition of students admitted to the upgraded schools. I attribute this counterintuitive result to the structure of the central student assignment mechanism and detail why common preference for the original national schools could lead to a decrease in the average ability of incoming students. A new policy, attempting to address low-ability students being admitted to national-tier schools, was introduced in 2016 and changed the way students are required to list their preferences. The ad-hoc nature of these policies provides interesting experiments that could, in future research, provide insights into the underlying preferences of students over schools. The new policy in particular imposes restrictions on the listing of schools and presents interesting opportunities to examine the welfare changes.

Figure 3.1: Reported Grant Spending Categories



Note: Schools could spend on multiple categories. 125 categories reported across 49 upgraded schools. School grant spending was audited.

Figure 3.2: Sample Schools

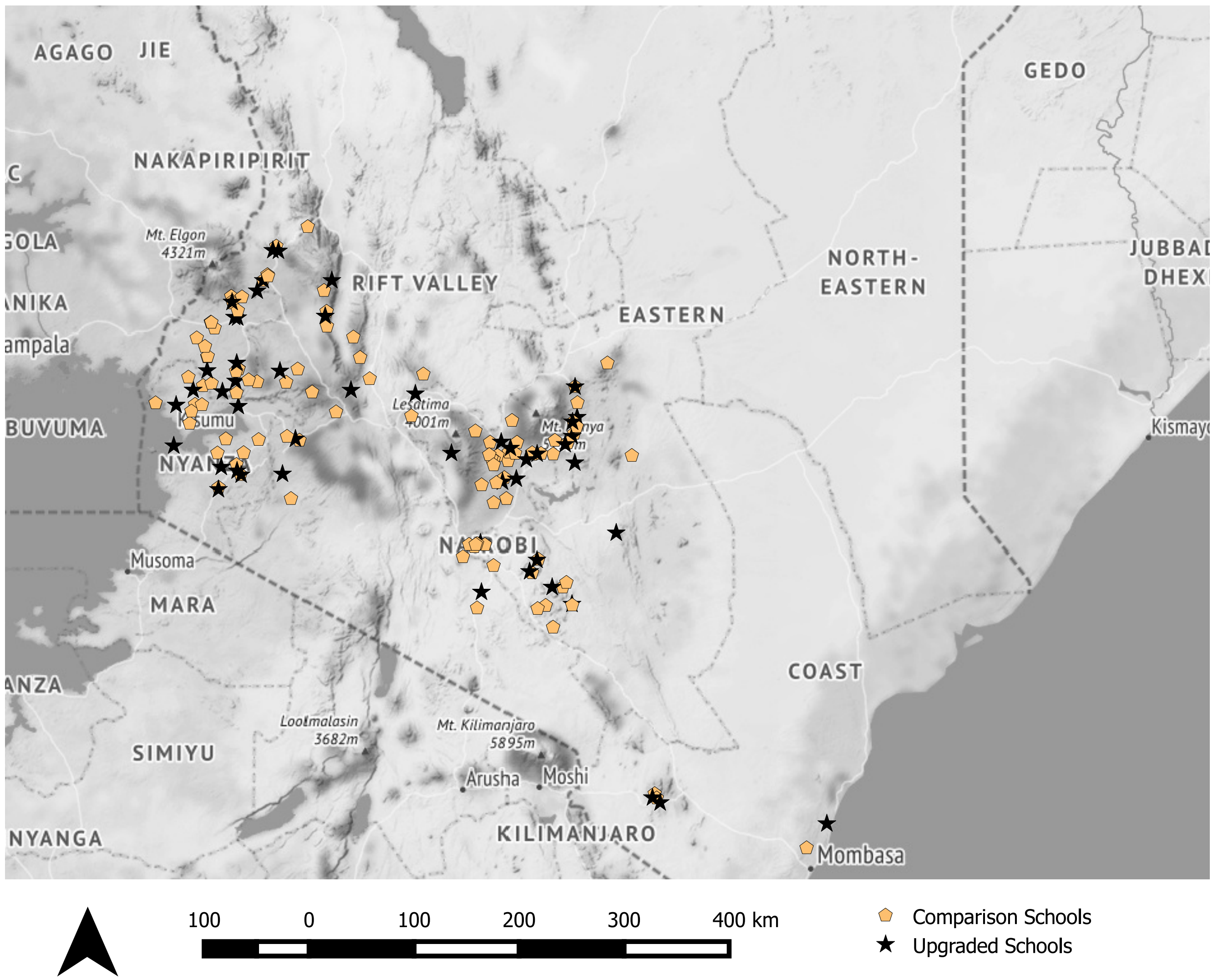


Figure 3.3: Mean KCSE grades of Upgraded and Eligible Schools (2006-2011)

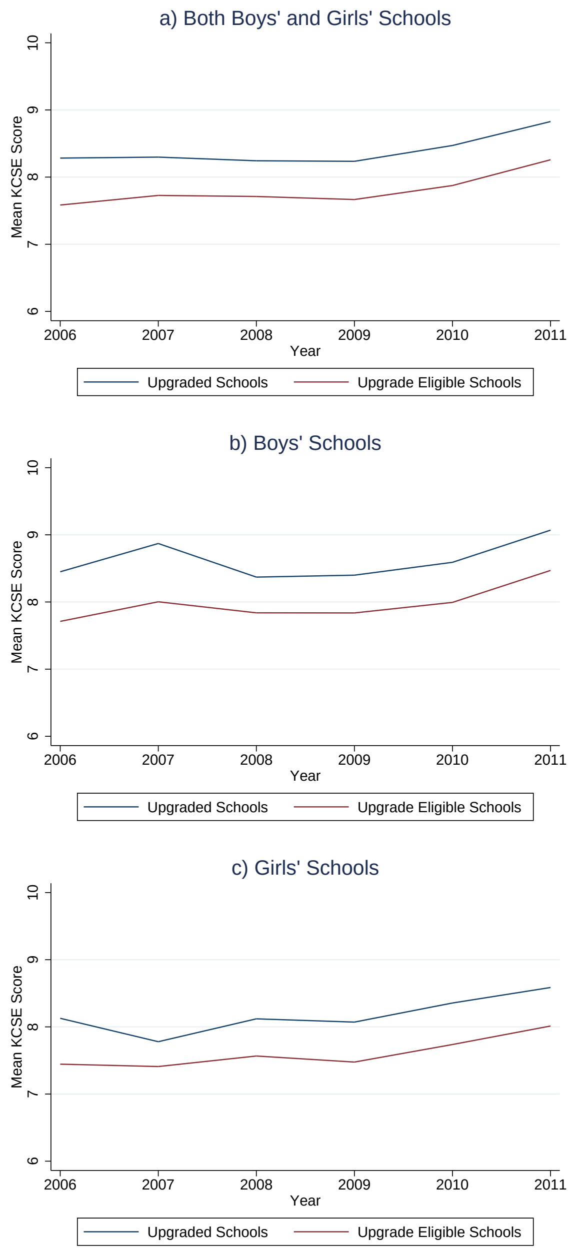


Figure 3.4: Mean KCSE grades of Phase 1/2 Schools and Phase 3 Schools (2006-2011)

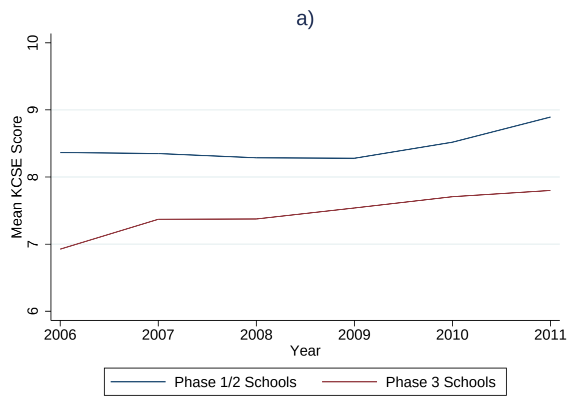


Figure 3.5: Percent of Student Preferences for Original National Schools and Upgraded National Schools

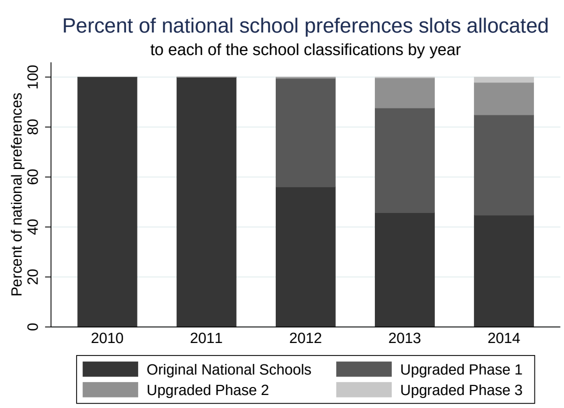


Table 3.1: KCSE Summary Statistics

	2009		2010	2011	2013	2014
	All	Nat	All	All	All	All
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
Students	337387	15472	357447	413510	449214	486430
Overall Score	4.94	9.60	5.15	5.25	5.12	5.38
	(2.32)	(1.74)	(2.42)	(2.47)	(2.49)	(2.47)
Core Subjects						
English	5.36	9.94	5.52	5.38	4.90	5.59
	(2.48)	(1.35)	(2.55)	(2.51)	(2.43)	(2.42)
Swahili	5.05	9.42	5.43	5.72	5.40	5.65
	(2.43)	(1.88)	(2.50)	(2.61)	(2.70)	(2.65)
Math	3.40	9.01	3.54	3.67	3.92	3.64
	(2.92)	(2.99)	(3.05)	(3.22)	(3.38)	(3.19)
Humanities						
Gov't/History	5.93	10.15	6.05	5.79	5.87	6.15
	(2.60)	(1.69)	(2.77)	(2.66)	(2.70)	(2.69)
Geography	4.98	9.24	4.91	5.51	5.56	5.53
	(2.62)	(2.08)	(2.59)	(2.85)	(2.87)	(2.93)
Sciences						
Biology	4.70	9.70	4.86	5.09	4.93	4.99
	(2.68)	(1.99)	(2.80)	(2.79)	(2.84)	(2.87)
Physics	4.82	9.04	5.19	5.33	5.51	5.40
	(2.89)	(2.65)	(3.04)	(3.12)	(3.16)	(3.10)
Chemistry	3.68	8.39	3.97	3.91	3.96	4.41
	(2.31)	(2.76)	(2.60)	(2.71)	(2.70)	(2.73)

Note: Standard deviation reported in parenthesis. Overall score is the mean score across the three core subjects, 1 subject from the humanities, 2 subjects from the sciences, as well as one additional practical subject from the list presented in footnote 8.

Table 3.2: School Summary Statistics

	Sample Schools			Upgraded Schools			Eligible Schools		
	All	Boys'	Girls'	All	Boys'	Girls'	All	Boys'	Girls'
Schools	153	75	78	49	22	27	104	53	51
Overall Score	8.11	8.28	7.95	8.46	8.76	8.21	7.95	8.08	7.81
	(0.90)	(0.80)	(0.96)	(0.95)	(0.88)	(0.96)	(0.83)	(0.69)	(0.95)
Cohort Size (2010)	198.05	204.60	191.74	222.08	238.50	208.70	186.72	190.53	182.76
	(69.72)	(68.21)	(71.01)	(68.90)	(82.54)	(53.36)	(67.51)	(56.41)	(77.76)
TSC Teachers	26.07	26.43	25.73	27.94	28.50	27.48	25.19	25.57	24.80
	(13.00)	(14.83)	(11.05)	(14.72)	(18.20)	(11.49)	(12.08)	(13.29)	(10.81)
Total Teaching Staff	30.49	31.75	29.28	32.61	34.41	31.15	29.49	30.64	28.29
	(13.67)	(15.65)	(11.43)	(15.31)	(18.36)	(12.46)	(12.79)	(14.42)	(10.84)
Distance to City	17.46	18.86	16.12	15.22	11.78	18.01	18.52	21.80	15.11
	(12.66)	(13.55)	(11.68)	(12.02)	(11.62)	(11.81)	(12.88)	(13.29)	(11.61)
Distance to Road	9.76	11.17	8.41	8.05	9.34	6.99	10.57	11.93	9.16
	(10.99)	(12.62)	(9.04)	(11.49)	(14.96)	(7.77)	(10.71)	(11.58)	(9.64)
Religious Sponsor	0.63	0.60	0.65	0.61	0.50	0.70	0.63	0.64	0.63
	(0.49)	(0.49)	(0.48)	(0.49)	(0.51)	(0.47)	(0.48)	(0.48)	(0.49)
Government Sponsor	0.35	0.37	0.33	0.39	0.50	0.30	0.34	0.32	0.35
	(0.48)	(0.49)	(0.47)	(0.49)	(0.51)	(0.47)	(0.47)	(0.47)	(0.48)
Acreage	29.30	36.56	22.24	37.88	52.77	25.85	25.39	30.14	20.36
	(32.16)	(41.67)	(16.17)	(46.71)	(65.05)	(17.12)	(21.85)	(25.73)	(15.50)

Note: Standard deviation reported in parenthesis. Sample schools include all upgraded and eligible schools. Upgraded schools were selected for upgrade to the national tier in either 2011, 2012, or 2013. Eligible schools met the upgrade criteria but were not selected to be upgraded. Overall score reflects the school mean overall KCSE performance. TSC teachers refers to the number of government certified teachers.

Table 3.3: Common Trends Regressions

	Full Sample	**Boys Sample**	**Girls Sample**
A. Common trends from 2006-2010			
N	115651	59594	56057
Overall Score	-0.015	-0.048	0.011
	(0.034)	(0.053)	(0.043)
B. Common trends from 2009-2010			
N	50656	26215	24441
Overall Score	-0.015	-0.042	0.042
	(0.076)	(0.096)	(0.110)
English Score	-0.196*	-0.087	-0.255*
	(0.103)	(0.126)	(0.151)
Swahili Score	0.074	0.082	0.115
	(0.120)	(0.181)	(0.154)
Math Score	-0.150	-0.361*	0.090
	(0.140)	(0.207)	(0.170)

Notes: Each coefficient in the table is the result from a separate regression and is the coefficient on an interaction between a time trend and a binary treatment variable. Exam scores are normalized by year. Subject specific scores are not available prior to 2009. *** indicates significance at the 99 percent level; ** indicates significance at the 95 percent level; and * indicates significance at the 90 percent level.

Table 3.4: Impact of Treatment on School Cohort Size

	All Schools (1)	**Boys' Schools (2)**	**Girls' Schools (3)**
Upgraded Schools	5.720	6.646	5.048
	(9.072)	(13.156)	(12.505)
Constant	265.684***	265.342***	174.157***
	(3.395)	(5.888)	(4.671)
Observations	763	375	388
R^2	0.834	0.849	0.814

Note: Dependent variable is school cohort size and the regression is run at the school-year level. Five school-year pairs are excluded because the exam results were nullified. All regressions include year and school fixed effects. Standard errors are clustered at the school level. Upgraded School is a binary variable equal to one once the school has received its national school designation. *** indicates significance at the 99 percent level; ** indicates significance at the 95 percent level; and * indicates significance at the 90 percent level.

Table 3.5: Estimated Treatment Coefficients

	Full **(1)**	**Boys** **(2)**	**Girls** **(3)**
N	134966	69398	65568
Examination Outcomes			
Overall Score	0.055	0.359**	-0.208
	(0.122)	(0.174)	(0.160)
Higher Ed. Funding Cutoff	0.016	0.085**	-0.044
	(0.029)	(0.041)	(0.038)
Core Subject Scores			
English Score	0.192*	0.397**	-0.021
	(0.099)	(0.155)	(0.116)
Swahili Score	0.067	0.560**	-0.368*
	(0.153)	(0.212)	(0.194)
Math Score	-0.016	0.314	-0.284
	(0.157)	(0.212)	(0.202)

Note: Each coefficient in the table is a result from a separate regression. All regressions include school and year fixed effects. Standard errors are clustered at the school level. Treatment is a binary variable equal to one once the school received its national school designation. *** indicates significance at the 99 percent level; ** indicates significance at the 95 percent level; and * indicates significance at the 90 percent level.

Table 3.6: Upgrade Treatment Effect by Percentile

	Boys				Girls			
Percentile	**Overall (1)**	**English (2)**	**Swahili (3)**	**Math (4)**	**Overall (5)**	**English (6)**	**Swahili (7)**	**Math (8)**
0.10	0.371	0.677***	0.780**	0.651**	-0.064	-0.013	-0.369	-0.169
	0.240	0.209	0.332	0.296	0.187	0.223	0.280	0.233
0.20	0.539**	0.442**	0.775**	0.599*	-0.050	-0.072	-0.208	-0.093
	0.268	0.186	0.303	0.329	0.182	0.140	0.304	0.281
0.25	0.417*	0.564**	0.861***	0.841***	-0.041	-0.130	-0.283	-0.205
	0.243	0.220	0.291	0.262	0.188	0.215	0.230	0.231
0.30	0.474**	0.221	0.865***	0.273	-0.069	-0.125	-0.228	-0.368
	0.237	0.263	0.330	0.326	0.256	0.150	0.239	0.335
0.40	0.481**	0.340*	0.647**	0.447	-0.050	-0.032	-0.424	-0.334
	0.230	0.205	0.276	0.304	0.170	0.171	0.260	0.351
0.50	0.347	0.488**	0.549*	0.411	-0.088	-0.049	-0.252	-0.334
	0.217	0.207	0.285	0.299	0.196	0.102	0.202	0.332
0.60	0.333	0.211	0.526*	0.390*	-0.285	0.000	-0.103	-0.245
	0.220	0.203	0.306	0.221	0.237	0.164	0.235	0.289
0.70	0.202	0.177	0.347	0.070	-0.049	-0.141	-0.087	-0.237
	0.189	0.161	0.270	0.127	0.156	0.177	0.192	0.284
0.75	0.254	0.363*	0.227	0.042	0.000	-0.050	-0.146	-0.284
	0.196	0.190	0.221	0.127	0.166	0.107	0.193	0.256
0.80	0.210	0.516**	0.154	0.081	-0.017	0.043	-0.023	-0.225
	0.221	0.218	0.257	0.230	0.197	0.094	0.171	0.231
0.90	0.070	0.263	0.045	0.223	0.041	0.019	-0.201	-0.085
	0.149	0.173	0.208	0.195	0.101	0.152	0.152	0.156
0.95	0.112	0.558**	0.000	0.000	-0.017	0.075	-0.217	-0.346
	0.221	0.231	0.152	0.173	0.181	0.111	0.153	0.291

Note: Standard errors clustered at the school level. Estimates are from a changes-in-changes model. *** indicates significance at the 99 percent level; ** indicates significance at the 95 percent level; and * indicates significance at the 90 percent level.

Table 3.7: Estimated Impact on Standard Deviation

	Overall (1)	**Male** (2)	**Female** (3)
N	755	358	397
Examination Outcomes			
Overall Score	-0.082*	-0.123*	-0.042
	(0.042)	(0.067)	(0.043)
Core Subjects			
English Score	-0.029	-0.079	0.013
	(0.033)	(0.048)	(0.044)
Swahili Score	-0.128**	-0.221**	-0.050
	(0.054)	(0.088)	(0.062)
Math Score	-0.105*	-0.110	-0.092
	(0.062)	(0.097)	(0.072)

Note: Dependent variable is the standard deviation of scores in each school in each year. The regression is run at the school-year level where five school-year pairs are excluded because the exam results were nullified. Each coefficient in the table is a result from a separate regression. All regressions include school and year fixed effects. Standard errors are clustered at the school level. Treatment is a binary variable equal to one once the school received its national school designation. *** indicates significance at the 99 percent level; ** indicates significance at the 95 percent level; and * indicates significance at the 90 percent level.

Table 3.8: Estimated Treatment Coefficients by Relative Grant Size

	Overall	**Male**	**Female**
High dollar per student	0.024	0.352	-0.284
	(0.163)	(0.261)	(0.187)
Low dollar per student	0.077	0.364*	-0.152
	(0.153)	(0.208)	(0.22)
Constant	9.674***	9.891***	7.868***
	(0.314)	(0.259)	(0.256)
Observations	134966	69398	65568
R^2	0.28	0.257	0.286
F-test: high=low (p-value)	0.791	0.968	0.621

Note: All regressions include cohort size as an additional independent variable as well as year and school fixed effects. Standard errors are clustered at the school level. High (low) dollar per student is a binary variable equal to one for schools with a student body less (more) than the median student body once the school received its national school designation. The sample includes the set of students at schools that were upgraded as well as students at schools that were eligible but not upgraded. *** indicates significance at the 99 percent level; ** indicates significance at the 95 percent level; and * indicates significance at the 90 percent level.

Table 3.9: Impact of Treatment on Subject Selection

	Overall (1)	**Male** (2)	**Female** (3)
A: Linear Probability Model			
Gov't/History Score	-0.020	-0.070*	0.023
	(0.024)	(0.037)	(0.032)
Geography Score	0.044*	0.047	0.040
	(0.024)	(0.043)	(0.025)
Biology	0.013	0.016	0.010*
	(0.009)	(0.018)	(0.006)
Physics	0.009	0.015	0.006
	(0.022)	(0.037)	(0.026)
Chemistry	0.002	0.003	0.000
	(0.002)	(0.005)	(0.001)
B: OLS			
Number of Subjects	0.061**	0.072	0.048
	(0.028)	(0.047)	(0.033)

Note: All regressions include year and school fixed effects as well as cohort size. Standard errors are clustered at the school level. Treatment is a binary variable equal to one once the school received its national school designation. *** indicates significance at the 99 percent level; ** indicates significance at the 95 percent level; and * indicates significance at the 90 percent level.

Table 3.10: Grant Spending Correlates of Treatment Effects

	Overall (1)	**Male (2)**	**Female (3)**
Dormitories	0.166	0.476	0.111
	(0.22)	(0.342)	(0.307)
Classrooms	-0.387*	-0.404	-0.318
	(0.226)	(0.78)	(0.296)
Science/language labs	0.436	0.484	0.432
	(0.281)	(0.683)	(0.361)
Library	0.241	-0.05	0.191
	(0.235)	(0.688)	(0.303)
Toilet facilities	0.345	0.216	0.603**
	(0.278)	(0.845)	(0.282)
Administration/staff buildings	-0.221	0.568	-0.596
	(0.244)	(0.482)	(0.426)
Dining hall	0.175	0.979	0.211
	(0.269)	(0.734)	(0.408)
Security	0.351	1.041	-0.126
	(0.402)	(0.668)	(0.314)
Student furniture	0.707**	0.768	0.929*
	(0.289)	(0.527)	(0.553)
Constant	-0.237	-0.524*	-0.327
	(0.219)	(0.302)	(0.309)
Observations	49	22	27
R^2	0.342	0.411	0.459

Note: All spending is categorized and represented by a dummy variable equal to one if the school spent funds on the category. *** indicates significance at the 99 percent level; ** indicates significance at the 95 percent level; and * indicates significance at the 90 percent level.

Table 3.11: Impact of Treatment on Test Scores of Incoming Students

	Incoming Student KCPE Scores		
	Overall	**Male**	**Female**
	(1)	**(2)**	**(3)**
Upgraded Schools	-9.107***	-7.696***	-10.226***
	(1.787)	(2.637)	(2.431)
Constant	353.244***	356.067***	350.614***
	(0.883)	(1.248)	(1.242)
Observations	140585	72680	67905
R^2	0.279	0.29	0.262

Note: All regressions include year and school fixed effects. Standard errors are clustered at the school level. Upgraded School is a binary variable equal to one once the school has received its national school designation. *** indicates significance at the 99 percent level; ** indicates significance at the 95 percent level; and * indicates significance at the 90 percent level.

Appendix

3.A Additional Tables and Figures

Table 3.A.1: Pooled Regressions

	Mean KCSE Grade (1)	**English (2)**	**Swahili (3)**	**Math (4)**
Upgraded Male Schools	0.35**	0.421***	0.559***	0.302
	(0.17)	(0.158)	(0.209)	(0.209)
Upgraded Female Schools	-0.211	-0.013	-0.369*	-0.288
	(0.162)	(0.116)	(0.194)	(0.205)
Constant	8.251***	9.265***	8.363***	7.105***
	(0.303)	(0.159)	(0.317)	(0.549)
Observations	134966	134866	134898	134966
R^2	0.282	0.306	0.271	0.201
p-value (male = female):	0.017	0.028	0.001	0.042

Note: All regressions include year and school fixed effects. Standard errors are clustered at the school level. Upgraded Male School and Upgraded Female School are binary variables equal to one once the school has received its national school designation.

Table 3.A.2: Estimated Treatment Coefficients by Relative Grant Size

	English			**Swahili**			**Math**		
	Overall	Male	Female	Overall	Male	Female	Overall	Male	Female
High dollar per student	0.191	0.458*	-0.013	-0.079	0.371	-0.47**	-0.1	0.34	-0.515**
	(0.144)	(0.24)	(0.147)	(0.202)	(0.317)	(0.236)	(0.193)	(0.276)	(0.261)
Low dollar per student	0.193*	0.354**	-0.027	0.17	0.695***	-0.294	0.043	0.296	-0.115
	(0.117)	(0.167)	(0.157)	(0.188)	(0.249)	(0.261)	(0.195)	(0.275)	(0.253)
Constant	10.126***	9.602***	9.351***	9.569***	9.652***	7.919***	9.178***	9.658***	6.052***
	(0.168)	(0.283)	(0.093)	(0.354)	(0.316)	(0.314)	(0.626)	(0.321)	(0.506)
Observations	134866	69350	65516	134898	69361	65537	134966	69397	65569
R^2	0.304	0.293	0.314	0.267	0.274	0.268	0.2	0.146	0.219
F-test: high=low (p-value)	0.989	0.692	0.946	0.310	0.389	0.588	0.539	0.903	0.226

Note: All regressions include cohort size as an additional independent variable as well as year and school fixed effects. Standard errors are clustered at the school level. High (low) dollar per student is a binary variable equal to one for schools with a student body less (more) than the median student body once the school received its national school designation. The sample includes the set of students at schools that were upgraded as well as students at schools that were eligible but not upgraded. *** indicates significance at the 99 percent level; ** indicates significance at the 95 percent level; and * indicates significance at the 90 percent level.

Table 3.A.3: Estimated Treatment Coefficients - Closest School

	Full (1)	**Boys** (2)	**Girls** (3)
N	87687	41657	46030
Overall Score	0.012	0.439**	-0.311*
	(0.140)	(0.217)	(0.168)
Higher Ed. Funding Cutoff	0.003	0.098*	-0.069*
	(0.033)	(0.052)	(0.040)
Core Subjects			
English Score	0.162	0.368*	-0.034
	(0.113)	(0.198)	(0.124)
Swahili Score	0.055	0.774***	-0.482**
	(0.178)	(0.246)	(0.200)
Math Score	-0.056	0.389	-0.341
	(0.185)	(0.266)	(0.226)

Note: Each coefficient in the table is a result from a separate regression. All regressions include school and year fixed effects. Standard errors are clustered at the school level. Treatment is a binary variable equal to one once the school received its national school designation.

Table 3.A.4: Complete Counties Estimated Treatment Coefficients

	Full (1)	**Boys** (2)	**Girls** (3)
N	105021	58651	46370
Overall Score	0.105	0.303	-0.087
	(0.142)	(0.191)	(0.206)
Higher Ed. Funding Cutoff	0.029	0.080*	-0.023
	(0.033)	(0.046)	(0.047)
Core Subjects			
English Score	0.177	0.392**	-0.030
	(0.121)	(0.174)	(0.156)
Swahili Score	0.071	0.532**	-0.374
	(0.171)	(0.216)	(0.238)
Math Score	0.011	0.137	-0.056
	(0.164)	(0.222)	(0.224)

Note: Each coefficient in the table is a result from a separate regression. All regressions include school and year fixed effects. Standard errors are clustered at the school level. Treatment is a binary variable equal to one once the school received its national school designation.

Table 3.A.5: Estimated Treatment Coefficients: School Level

	A: Overall			**B: Male**			**C: Female**		
	25th	**50th**	**75th**	**25th**	**50th**	**75th**	**25th**	**50th**	**75th**
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**	**(8)**	**(9)**
Overall Score	0.183	0.054	-0.015	0.421	0.242	0.336*	-0.020	-0.106	-0.298
	(0.170)	(0.150)	(0.135)	(0.253)	(0.213)	(0.191)	(0.224)	(0.210)	(0.179)
Core Subjects									
English Score	0.170	0.126	0.106	0.382	0.403**	0.244	-0.008	-0.108	-0.008
	(0.148)	(0.130)	(0.112)	(0.238)	(0.197)	(0.177)	(0.187)	(0.160)	(0.143)
Swahili Score	0.207	0.008	-0.070	0.739**	0.521**	0.330	-0.218	-0.397	-0.386*
	(0.207)	(0.188)	(0.157)	(0.330)	(0.258)	(0.208)	(0.239)	(0.253)	(0.215)
Math Score	0.256	-0.036	-0.182	0.558*	0.268	0.035	0.000	-0.283	-0.341
	(0.221)	(0.246)	(0.226)	(0.315)	(0.336)	(0.300)	(0.312)	(0.347)	(0.307)

Note: Each coefficient in the table is a result from a separate regression. All regressions include school and year fixed effects. Standard errors are clustered at the school level. Treatment is a binary variable equal to one once the school received its national school designation.

Chapter 4: A Firm of One's Own: Experimental Evidence on Credit Constraints and Occupational Choice

Joint with Maddalena Honorati, Pamela Jakiela, and Owen Ozier

4.1 Introduction

Youth underemployment is a major challenge facing developing nations, particularly in Africa Filmer and Fox (2014). Young people are more likely to be unemployed than older adults (Kluve *et al.* 2016). In low-income countries, unemployment figures also typically underestimate the proportion of youths who cannot find productive jobs Fares, Montenegro, and Orazem (2006). After leaving school, it often takes young adults in low-income countries several years to find gainful employment or launch a viable household enterprise; during that transition from school to the labor market, many youth are forced to rely on family members for support between stints of work in irregular, informal positions World Bank (2006). Demographics make the problem of youth underemployment particularly acute in Sub-Saharan Africa, where more than half the population is under 25. Filmer and Fox (2014) estimate that, over the next ten years, only a quarter of the African youth entering the labor market will be able to find paid employment.

Since formal sector jobs are scarce in low-income settings, many policymakers have advocated entrepreneurship promotion programs intended to help unemployed youth generate an income through self-employment United Nations Development Programme (2013); Franz (2014). The simplest entrepreneurship promotion programs are credit market interventions such as loans or one-off grants of money or physical capital. Economic theory suggests that such interventions can help potential entrepreneurs who have limited opportunities to save or borrow to start or expand profitable businesses, and one recent study suggests that cash grants can help unemployed youth launch businesses and increase their incomes Blattman, Fiala, and Martinez (2014). However, a growing body of evidence on the the returns to capital among entrepreneurs suggests that credit constraints may not be the main obstacle limiting the growth of female-owned microenterprises: evaluations to date have found that, in most cases, cash grants to female entrepreneurs do not lead to sustained increases in business profits or income (De Mel, McKenzie, and Woodruff 2008, De Mel, McKenzie, and Woodruff 2009, Fafchamps, McKenzie, Quinn, and Woodruff 2011, Fiala 2014, Karlan, Knight, and Udry 2015, Blattman *et al.* 2016).[1] Taken together, these results suggest that many women who operate small businesses are "subsistence entrepreneurs" Schoar (2010) who lack either the ability or the inclination to expand their enterprises; if this is true, access to capital (alone) is unlikely to have major impacts.

[1]Recent evaluations also suggest that microfinance loans, the canonical credit market intervention intended to help subsistence entrepreneurs, do not lead to significant increases in income or, in most cases, microenterprise profits (Angelucci, Karlan, and Zinman 2015, Attanasio *et al.* 2015, Augsburg, De Haas, Harmgart, and Meghir 2015, Banerjee, Duflo, Glennerster, and Kinnan 2015, Crépon, Devoto, Duflo, and Parient 2015, Tarozzi, Desai, and Johnson 2015).

In fact, though capital drop interventions are becoming increasingly common, many youth entrepreneurship programs offer more than just capital, for example, start-up capital together with skills training or ongoing business mentoring (Kluve *et al.* 2016). The theory of change underlying such multifaceted approaches is that young entrepreneurs face many different obstacles and constraints that need to be addressed simultaneously in order to launch a successful microenterprise. For example, they may lack the vocational skills needed to attract customers in competitive markets, they may not have access to the start-up capital needed to launch a business, and they may not know how to manage an enterprise successfully after it is launched. Several recent studies suggest that multifaceted programs that combine vocational education and start-up capital with life skills training may improve the income prospects of young women, in particular (cf. Adoho *et al.* 2014, Bandiera *et al.* 2014).[2]

We evaluate one such multifaceted entrepreneurship intervention: a "microfranchising" program that offered young women in some of Nairobi's poorest neighborhoods a combination of vocational and life skills training together with start-up capital and ongoing business mentoring. Like many entrepreneurship programs, the microfranchising model is premised on the idea that many youth do not have the skills and experience necessary to be competitive in the labor market, and also lack the financial and human capital needed to start a successful enterprise (for example,

[2]There is also evidence that multifaceted programs which combine skills training and asset transfers can improve the income-generating capacity of vulnerable adults (not just youth and not just women). Banerjee *et al.* (2015) demonstrate that one such multipronged approach, the ultrapoor Graduation Program implemented by the NGO BRAC, led to large increases in income, food security, and rates of savings. A recent meta-analysis also highlights the relative effectiveness of multifaceted entrepreneurship promotion programs Cho and Honorati (2014).

the ability to conduct market research and develop a business plan). The franchise treatment that we study attempts to overcome these barriers by providing motivated young women with an established business model and the specific capital and supply chain linkages needed to operate the business. The franchise treatment was designed and implemented by the International Rescue Committee (the IRC) in cooperation with local community-based organizations.[3]

We estimate the impacts of this franchise treatment on applicants via a randomized trial. We not only measure the program's impacts in relation to a control group, but also compare those impacts to the effects of a simpler cash grant intervention that relaxed the credit constraint without providing any additional training or support. We interpret our findings through the lens of a simple model of investment decisions when individuals differ in terms of their labor productivity. High productivity types who have limited opportunities to save or borrow may be unable to launch profitable businesses because they cannot accumulate the required capital. In such cases, credit market imperfections may create a poverty trap, and one-off transfers of money or capital, such as those in our study, can lead to permanent increases in income. One of the key insights from the model is that credit constraints are only an obstacle to productive entrepreneurship for a subset of individual types; less productive types are unable to sustain a business in any steady state. Nonetheless, savings constraints can also affect the investment decisions and occupational choices of lower productivity types who receive one-off infusions of funding or cap-

[3]See International Rescue Committee (2016b) for an overview of the IRC's economic development programs.

ital; though these individuals cannot sustain businesses, they may invest in capital and launch unproductive firms because enterprise capital is a technology for saving, albeit at a negative interest rate. Thus, short-term impacts of one-off transfers on entrepreneurship should not be taken as evidence that a program relieved a credit constraint or addressed a poverty trap; the critical issue is whether impacts on income persist over the longer-term.

We find that both the franchise treatment and the grant treatment led to substantial increases in income in the year after the interventions. Point estimates suggest impacts that are both economically and statistically significant: the franchise treatment increased weekly income by 30 percent, up 1.6 US dollars from a mean of 5.5 dollars in the control group (p-value 0.035); the grant treatment increased weekly income by 3.2 dollars (p-value 0.008) or 56 percent. As expected, these impacts appear to be driven by a shift from paid work to self-employment; women assigned to either the franchise or the grant treatment are approximately 10 percentage points more likely to be self-employed than those in the control group. Women assigned to the grant treatment also increased their labor supply (hours worked) substantially.

Though both interventions increased income in the relatively short-run, data from endline surveys conducted between 14 and 22 months after treatment indicate that the observed impacts on income disappeared in the second year after the program(s). At endline, women assigned to either the franchise treatment or the grant treatment are more likely to be self-employed than women in the control group, but the treatments are not associated with increases in income or labor supply.

In addition, we find no impacts of treatment on food security, expenditures, living conditions, or empowerment at endline. Seen through the lens of our model, these findings are consistent with the existence of savings constraints; large impacts on income and occupational choice that disappear relatively quickly make sense if enterprise capital is one of the few viable savings technologies available to young women in a poor urban area. However, our findings do not suggest that credit constraints had been preventing productive entrepreneurs from launching profitable, sustainable businesses.

This paper makes several contributions. First, we measure the impact of an active labor market program on young women in an urban area in a developing country. Here, we contribute to an active literature on active labor market programs and youth unemployment.[4] Our work is most closely related to Bandiera *et al.* (2014) and Adoho *et al.* (2014), who also evaluate multifaceted labor market interventions for young women in Sub-Saharan Africa.

We compare the impacts of a multifaceted entrepreneurship promotion intervention to those of a one-off cash grant; this provides a natural cost-effectiveness benchmark without any of the contextual caveats that would accompany a more traditional cost-benefit analysis. Though evaluations of cash grants are becoming more common Haushofer and Shapiro (2016)cf., the use of cash as a benchmark within program evaluation is still relatively uncommon. Our results, like those of Karlan, Knight, and Udry (2015), suggest that unrestricted cash grant treatments can provide an extremely useful alternative to the traditional control group (that

[4]See Kluve *et al.* (2016) for a recent survey.

receives no treatment).[5]

We measure both interventions' impacts over time, expanding our understanding of the dynamics of the estimated impacts. In addition, we present a model, building on previous work (cf. Fafchamps *et al.* 2011, Blattman, Fiala, and Martinez 2014, Blattman *et al.* 2016), that yields a straightforward interpretation of the estimated program impacts in relation to credit and savings constraints. Our model suggests that the patterns of impacts that we observe are more likely to be explained by savings constraints than by credit-constraint-based poverty traps. This conclusion resonates with other recent evidence that the poor, particularly poor women, have a very limited menu of savings technologies Dupas and Robinson (2013a,b).

Finally, we capitalize on the program evaluation setting to test whether participants hold accurate beliefs about program impacts; in so doing, we provide a framework for comparing methods of belief elicitation. Our work builds directly on the contributions of Smith, Whalley, and Wilcox (2011) and Smith, Whalley, and Wilcox (2012). Like McKenzie (2016a), we find the program participants do a poor job of estimating their own counterfactual (probabilistic) outcomes. However, we extend the existing set of best practices by demonstrating that participants are quite good at estimating average treatment impacts on the population once behavioral biases are taken into account.

The remainder of this paper is organized as follows. Section 4.2 outlines our theoretical model. Section 4.3 describes our research design and the specific fran-

[5]Supporting this argument, Özler (2016) has also remarked that "the interesting comparison is not against 'no support' ... it's against cost-equivalent alternative efforts."

chise and grant treatments that we evaluate. Section 4.4 presents our main results. Section 4.5 characterizes participants' beliefs about the impacts of the program. Section 4.6 concludes.

4.2 Conceptual Framework

To understand the impacts of capital infusions and other credit market interventions, we require a framework for interpreting individual responses to these interventions. We propose a simple model of labor supply decisions in the presence of credit market imperfections, when individuals may face credit constraints and may also be unable to save. We show that high productivity individuals who are are unable to save or borrow may find themselves in a poverty trap in which they never launch a business, even though their enterprises would be profitable once launched. In this constrained environment, a large capital transfer enables these individuals to start lasting businesses. In contrast, low productivity individuals are unable to sustain an enterprise in any steady state; because these individuals cannot sustain a profitable enterprise, the fact that they are not accessing loans does not indicate a market failure. However, in a savings-constrained environment, low productivity types may open businesses after receiving a large capital transfer, using enterprise capital as a savings vehicle when other savings technologies are unavailable. These businesses are temporary (because low productivity individuals cannot sustain businesses in the steady state), and are eventually closed after the initial capital investment depreciates.

We begin by considering a simple model in which production in each period depends on labor and capital. Labor is allocated between two activities: own-enterprise production, characterized by production function $f^e(K, L^e)$, and wage labor, characterized by production function $f^w(L^w)$. Individuals allocate their labor across sectors subject to the constraint: $L^e + L^w \leq 1$. Importantly, we follow other recent work Blattman, Fiala, and Martinez (2014)cf. in assuming that own-enterprise production requires a capital investment that exceeds some minimum scale; thus, potential entrepreneurs who are credit-constrained and unable to save cannot launch arbitrarily small businesses that could then grow over time. This minimum scale requirement creates the potential for a poverty trap. Both production functions are characterized by diminishing returns with respect to individual inputs; we assume that the enterprise production function, $f^e(K, L^e)$, is homogeneous of degree one above the minimum scale.

We make the following specific assumptions about the own-enterprise production function, $f^e(K, L^e)$:

$$f^e(K, L^e) \equiv 0 \ \ \forall K \leq K_{min} \ \ (\textit{minimum scale}) \tag{A1}$$

$$\frac{\delta^2}{\delta K^2} f^e(K, L^e) < 0 < \frac{\delta}{\delta K} f^e(K, L^e) \ \ \forall K \geq K_{min} \ \ (\textit{diminishing returns}) \tag{A2}$$

$$\frac{\delta^2}{\delta L^2} f^e(K, L^e) < 0 < \frac{\delta}{\delta L} f^e(K, L^e) \ \ \forall K \geq K_{min} \ \ (\textit{diminishing returns}) \tag{A3}$$

$$\frac{\delta^2}{\delta L \delta K} f^e(K, L^e) > 0 \ \ \forall K \geq K_{min} \ \ (\textit{inputs are complements}) \tag{A4}$$

$$\lim_{L \to 0} \frac{\delta}{\delta L} f^e(K, L^e) = +\infty \ \ \forall K \geq K_{min} \ \ (\textit{Inada}) \tag{A5}$$

$$\lim_{K \to K_{min}} \frac{\delta}{\delta K} f^e(K, L^e) = +\infty \qquad (\textit{Inada}) \tag{A6}$$

$$\lim_{K \to +\infty} \frac{\delta}{\delta K} f^e(K, L^e) = 0 \qquad (\textit{Inada}) \tag{A7}$$

With respect to the wage labor production function, $f^w(L^w)$, we assume that standard Inada conditions hold.[6] In other words, we assume

$$f^w(0) = 0 \tag{A8}$$

$$\frac{\delta}{\delta L} f^w(L^w) > 0 \tag{A9}$$

$$\frac{\delta^2}{\delta L^2} f^w(L^w) < 0 \tag{A10}$$

$$\lim_{L \to 0} \frac{\delta}{\delta L} f^w(L^w) = +\infty \tag{A11}$$

In each period t, the agent has capital K_t and one unit of labor to divide between activities such that $L^e + L^w \leq 1$. The agent produces using whatever allocation of labor she chooses, yielding $\mathbb{F}(K_t, L^w) = f^w(L^w) + f^e(K_t, 1 - L^w)$. The

[6] In the Online Appendix, we show that the same argument can be extended for a constant wage rate.

maximum level of production in a given period results from the optimal allocation of labor between the two possible sectors:

$$\mathbb{F}^*(K_t) = \max_{0 \leq L^w \leq 1} \mathbb{F}(K_t, L^w) \tag{1}$$

Proposition 1 characterizes the properties of $\mathbb{F}^*(K_t)$. Because of the minimum level of capital required to produce output in the own-enterprise sector, the function $\mathbb{F}^*(K_t)$ has a characteristic shape, which is shown in Figure 4.1. The characteristic shape of $\mathbb{F}^*(K_t)$ drives the predictions of our model.

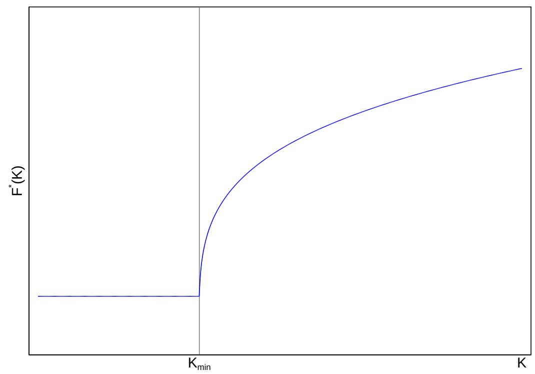
Proposition 1. $\mathbb{F}^*(K_t)$, *the total production function conditional on the optimal allocation of labor across the wage labor and own enterprise sectors, has the following properties:*

1. *For all* $K_t \leq K_{min}$, $\mathbb{F}^*(K_t) = f^w(1)$*; hence, the first and second derivatives of* $\mathbb{F}^*(K_t)$ *are equal to 0 for all* $K_t \leq K_{min}$.
2. *For all* $K_t > K_{min}$, $\mathbb{F}^*(K_t)$ *has a positive first derivative.*
3. *For all* $K_t > K_{min}$, $\mathbb{F}^*(K_t)$ *has a negative second derivative.*

Proof: see Online Appendix.

Intuitively, $\mathbb{F}^*(K_t)$ is flat for $K_t \leq K_{min}$. Levels of capital below the minimum level required to operate a business, K_{min}, do not contribute to total output and simply depreciate; hence, for individuals who have access to a range of savings technologies, there is no reason to invest $K < K_{min}$ in the own-enterprise sector. At levels of capital exceeding K_{min}, $\mathbb{F}^*(K_t)$ inherits the properties of the production function in the own enterprise sector; it is always optimal to allocate one's capital and some of one's labor to the own enterprise sector and operate a business at some scale because the marginal product of capital approaches infinity as $K_t \rightarrow K_{min}^+$.

Figure 4.1: Shape of the Production Function, $\mathbb{F}^*(K_t)$



After production, the previous period's capital depreciates, so that it becomes $K_t(1-\delta)$. The agent also chooses a level of consumption, c_t, in period t. Capital in the next period is thus given by:

$$K_{t+1} = \mathbb{F}^*(K_t) - c_t + K_t(1-\delta) \tag{2}$$

A steady state is characterized by a level of capital, K_{ss}, and a level of consumption, c_{ss}, that satisfy the following condition:

$$K_{ss} = \mathbb{F}^*(K_{ss}) - c_{ss} + K_{ss}(1-\delta) \tag{3}$$

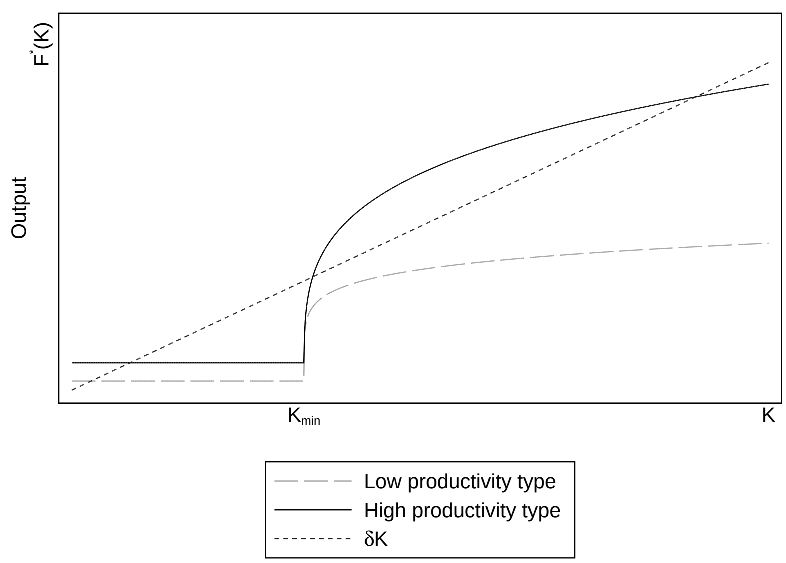
Rearranging, and because consumption cannot be negative, this becomes:

$$c_{ss} = \mathbb{F}^*(K_{ss}) - \delta K_{ss} \geq 0 \tag{4}$$

For any individual, the steady state level of capital cannot exceed the highest value of K_t such that $\mathbb{F}^*(K_t) = \delta K_t$.

Because δK_t is a ray from the origin, it may cross the production function, $\mathbb{F}^*(K_t)$, at most three times: it may cross the flat region of $\mathbb{F}^*(K_t)$ (where $0 < K_t < K_{min}$) at most once, and it may cross $\mathbb{F}^*(K_t)$ in the curved region (where $K_t \geq K_{min}$) at most twice. Examples of production functions (and their intersections with δK_t) are shown in Figure 4.2.

Figure 4.2: Examples of Production Functions



Individuals differ in terms of their productivity, which is characterized by the shape of the production function $\mathbb{F}_i^*(K_t)$. We define high productivity individuals as those that can sustain a self-employment activity in any steady state.

Definition 1. *Individual i is a **high productivity type** if she is able to sustain a business in any steady state, i.e. if there exists K_t such that $\mathbb{F}_i^*(K_t) > \delta K_t$ and $K_t > K_{min}$. A **latent entrepreneur** is a high productivity type with at least one steady state that satisfies the condition $\mathbb{F}_i^*(K_{ss}) > f^w(1)$.*

Being a high productivity type is a necessary condition for successful entrepreneurship: individuals who are not high productivity types are unable to sustain an enterprise in any steady state.[7] If high productivity individuals are sufficiently patient and they are able to save at a sufficiently non-negative interest rate, then those who prefer operating their own businesses to working (exclusively) in the wage sector will do so — they will save up the funds needed to make the initial profitable capital investment of $K_{ss} > K_{min}$ and launch their own businesses. Alternatively, high productivity types who face sufficiently low borrowing costs can borrow the funds needed to launch their businesses. However, when opportunities for saving and borrowing are limited, high productivity types who wish to launch their own enterprises may not be able to do so — creating a poverty trap.

Savings constraints also shape individual responses to cash grant interventions. When individuals are able to save, investing a transfer in enterprise capital (or in any other illiquid asset) is only attractive if the return on the investment exceeds the return on saving. However, when saving is impossible, investing in business capital

[7]Whether a high productivity type prefers entrepreneurship to wage labor will depend on their preferences. For many preferences specifications, opening a business is attractive when $f^w(1) \leq max_{K_{ss}}\mathbb{F}_i^*(K_{ss})$. However, the predictions of the model do not depend on specific assumptions about the utility function.

and launching a small-scale enterprise may be one of the only ways to smooth positive income shocks across periods. We assume that capital stock is carried forward (minus depreciation) as long as an individual allocates at least $\epsilon > 0$ units of labor to the own-enterprise sector; we allow ϵ to be arbitrarily small.

The first key prediction of the model is that a one-off transfer to a latent entrepreneur can lead to a permanent increase in income. Individuals who have access to a zero-interest savings technology will invest enough in their businesses to transition to their preferred steady-state level of capital. In this case, income will immediately rise from $f^w(1)$ to $\mathbb{F}_i^*(K_{ss})$, and will remain there indefinitely. Consumption may also be directly impacted if individuals save and consume transferred funds without investing them in microenterprises (though these direct impacts on consumption should not be associated with changes in occupational choice).

When latent entrepreneurs are unable to save, they will invest any transfers received in their businesses.[8] If the amount of the transfer exceeds the lowest possible steady state capital stock, income rises from $f^w(1)$ to $\mathbb{F}_i^*(K_{transfer})$ and then settles toward the individual's optimal steady state value of $\mathbb{F}_i^*(K_{ss}) > f^w(1)$ over time. Thus, the short-term impacts of capital infusions on income may be larger than the long-term impacts, but the long-term impacts on income are positive.

In contrast, for lower productivity individuals — those for whom δK_t only crosses $\mathbb{F}_i^*(K_t)$ once, in the flat region where $K_t < K_{min}$ — a capital transfer does not have permanent impacts. These individuals cannot operate their own enterprises

[8]Transfer recipients may choose to consume of the transferred funds upon receipt; this does not impact the predictions of our model. $K_{transfer}$ should then be interpreted as the amount that is not immediately consumed.

in a steady state. Even when they are able to save at a non-negative interest rate, saving money to invest in the own-enterprise sector is not an attractive proposition. Even when they are able to borrow at low interest rates, borrowing the funds to launch a business is unattractive (if one is required to eventually repay the loan).

However, when individuals who cannot sustain a profitable enterprise receive a large transfer, they may choose to invest the money in a business if they are savings constrained. Intuitively, enterprise capital is a means of saving at a negative interest rate of $\frac{\mathbb{F}_i^*(K_t)}{K_t} - \delta$. For large infusions of capital, launching a business, consuming the business income, and allowing the business to shrink over time as the capital depreciates will sometimes be preferable to immediately consuming all of the capital received. Operating that business, even if depreciation exceeds production, is still better than letting the capital depreciate without production. Thus, savings-constrained individuals who are not productive enough to sustain enterprises may operate temporary businesses if given a cash infusion. The key distinction between latent entrepreneurs and lower productivity types is that one-off infusions of capital can permanently increase the incomes of latent entrepreneurs, while such infusions of capital have impacts on lower productivity types that disappear over time.

4.3 Research Design and Procedures

We conducted a randomized evaluation of two labor market interventions targeted to young women aged 18 to 19 in three of Nairobi's poorest neighborhoods, Baba

Dogo, Dandora, and Lunga Lunga.[9] Applicants to the program were stratified by neighborhood and application date and then randomly assigned to one of three treatment arms: a franchise treatment, a cash grant treatment, and a control group. This design allows us to estimate the impact of the franchise and grant treatments on those invited to the program, and to compare the impacts of the cash grant treatment — which relaxes the credit constraint but provides no other training or support — to a multifaceted program designed to address many of the obstacles to youth entrepreneurship simultaneously.

4.1 Two Labor Market Interventions

4.1.1 The Franchise Treatment

Credit constraints may prevent potential entrepreneurs from launching profitable businesses. However, credit constraints may not be the only obstacle to entrepreneurial success; potential entrepreneurs — particularly young people — may also lack the market intelligence and business training needed to launch a successful enterprise Berge, Bjorvatn, and Tungodden (2014). We evaluate a multifaceted "microfranchising" program that provided eligible applicants with an established business model and the specific training, capital, and business linkages (for example, with wholesale suppliers) needed to make the business operational. Microfranchisees supply

[9] Applications were solicited from women between the ages of 16 and 19; in practice, relatively few of the applicants (only 14.6 percent) were below 18 years of age when they applied. Only those women who had attained the age of legal majority were eligible to receive cash grants, so our analysis focuses on those who were in the two oldest age cohorts (randomization to treatment was stratified by age). The cash grant treatment was not announced in advance; women applied for a business training program and were then randomized into one of the three treatment arms.

their labor, and are free to expand their microenterprises as they see fit. Thus, a microfranchise has features in common with both a formal sector job and self-employment: while microfranchisees do not need to devise business models, they work with very little managerial supervision and considerable latitude for creativity — managing their own time and entrepreneurial effort. Thus, microfranchising strikes a middle ground between entrepreneurship and wage employment.

We evaluate a microfranchising intervention geared toward young women in Nairobi's poorest neighborhoods. The program helped young women launch branded franchise businesses, either salons or mobile food carts. The intervention combined a number of distinct elements: business skills training, franchise-specific vocational training, start-up capital (in the form of the specific physical capital required to start the franchise), and ongoing business mentoring. Several of the intervention's components are common to many entrepreneurship promotion and job skills programs; what distinguishes microfranchise programs from other interventions is the focus on a small number of specific franchise business models that are tailored to the skills and constraints of program participants (i.e. poor young women in urban Nairobi) and to local market conditions. In this case, the implementing organization (the IRC) partnered with two Kenyan businesses looking to expand their presence in slum neighborhoods — a maker of hair extensions and a poultry producer known for its fast food restaurants. The franchise partners are both relatively well-known firms (within Kenya), and their reputations added value to the franchise package that program participants received.

The first component of the franchise program was a two-week training course.

In addition to a standard curriculum of business and life skills training topics, the training included modules about the two specific franchise business models. At the end of the course, participants indicated their ranking of the two franchise partners and were then matched with one of them (almost always their first choice).

After the business skills course, program participants received training from the franchise business partner with whom they had been matched. Women assigned to the salon franchise received six weeks of classroom training and then completed a two-week internship with a local salon. At the end of the internship, participants organized themselves into small groups and received their business start-up kits (which included branded aprons, a hair washing sink, a hair dryer, and a variety of hair cutting and styling products).

For women assigned to the food cart franchise, the franchise-specific training was a one-day session where franchisees were introduced to the brand, available products, and appropriate preparation methods. Following the franchise training, program participants organized themselves into small groups and received business start-up kits that included a mobile cart, an apron or t-shirt displaying the company logo, and an initial stock of smoked chicken sausages.

Each franchise business launched through the program was assigned a mentor who visited the business every few weeks. Mentors helped the young women in the program get their businesses off the ground — for example, by coordinating additional training with the franchise partners, helping the businesses set up bank accounts, or assisting with financial management and record keeping.

4.1.2 The Grant Treatment

Applicants assigned to the cash grant treatment were offered an unrestricted transfer of 20,000 Kenyan shillings (or 239 US dollars at the prevailing exchange rate of 83.8 shillings to the dollar).[10] Individuals assigned to the grant arm were contacted by phone and invited to meet privately with a member of the disbursement team to discuss the grant. During the meeting, individuals were told that there were no restrictions on how the grant could be used and that the grant did not need to be paid back. Disbursements to the grant recipients were timed to coincide with the launch of the microfranchise businesses.

4.2 Data Collection

Our analysis draws on three main sources of data. First, we administered a brief baseline survey to all eligible applicants prior to randomization. We also conducted a midline survey 7 to 10 months after the end of the intervention.[11] The midline surveys were conducted via phone. The midline included detailed questions about income-generating activities, but did not ask about a broader range of outcomes (this was not feasible in a short phone survey). We conducted a more comprehensive endline survey 14–22 months after the end of the intervention.

[10] Though the US dollar value of the shilling has since declined, the exchange range was fairly constant during the grant disbursement period (from November 1, 2013 to January 13, 2014). The value of the grant was selected to make it roughly comparable to the value of the microfranchising package of training and capital; the 20,000 shilling amount is also identical to the grant size in another study of cash grants for Kenyan youth Hicks, Kremer, Mbiti, and Miguel (2016).

[11] We also conducted an extremely brief phone survey 2 to 5 months after the intervention, but we did not ask about income-generating activities at that time. The goal of that survey was to collect better contact information than had been gathered at baseline.

Attrition rates are extremely low in both the midline and the endline surveys: we successfully surveyed 94.0 percent of the baseline sample at midline and 92.5 percent of the baseline sample at endline. Regressions testing for differential attrition across treatment arms are reported in the Online Appendix. Attrition is not associated with either treatment.

4.3 Sample Characteristics

Table 4.1 describes the baseline characteristics of the young women in our sample. As expected, there is little variation in age: 94.6 percent of the young women in the sample were 18, 19, or 20 years of age at baseline. 11.6 percent of women in our sample did not have a living parent at the time of the baseline survey. 16.5 percent were married or cohabitating, and 40.9 percent had given birth. The median number of years of schooling in the sample is 10; 92.4 percent of baseline respondents finished primary school, while only 41.1 percent finished secondary school.[12] 34.5 percent had done some form of vocational training prior to the program.

Only 14.6 percent of the sample was engaged in an income-generating activity (IGA) at the time of the baseline survey, but 54.6 percent had been involved in an IGA at some point in the past. 23.2 percent had been self-employed at some point in the past. The young women in the sample spent a considerable amount of time doing unpaid work at home: the median number of hours of unpaid housework (in

[12]The average level of education among women aged 18-20 in Nairobi is 10.6 years; 28 percent are currently married or living with a partner, and 26 percent have had a child (Kenya DHS 2014). Thus, relative to the general population of comparably-age women in Nairobi, our sample is slightly less educated, less likely to be married or cohabitating, and more likely to have had a child. These differences likely reflect the program's focus on Nairobi's poorest neighborhoods.

the week prior to the baseline) was 21. Only 8.8 percent of women in the sample had a bank account at baseline, and only a third had any savings in money or jewelry. Among those with savings, the median amount of savings was (equivalent to) 8.91 US dollars.

Balance checks (i.e. tests of the hypothesis that observable characteristics are balanced across treatments) are reported in the Online Appendix. Observable characteristics were relatively balanced prior to the program. Out of 75 hypothesis tests, we find 3 differences across treatments that are significant at greater than 95 percent statistical confidence.[13]

4.4 Compliance with Treatment

As is typical in training programs (McKenzie and Woodruff, 2014), not all the women assigned to the program participated in it, and not all those who started the business training completed the program. Table 4.2 reports the proportion of women in the treatment and control groups who completed each stage of the program.[14] 61 percent of those assigned to the franchise treatment attended the initial two-week business training course at least once; 39 percent of those assigned to the franchise treatment completed the franchise-specific business training and launched a microfranchise. Though these modest take-up rates are not out of line with those

[13] Women assigned to the control group come from slightly larger households, and are somewhat more likely to have given birth prior to the program. Women assigned to the cash grant treatment had, on average, about half a year less schooling than those assigned to the franchise treatment and the control group. Controls for those variables that are not balanced across treatments are included in our main specifications (though results are nearly identical when controls are omitted).

[14] The table is based on administrative data from the implementing NGO and the franchise partners, though self-reports line up with administrative records.

observed in comparable training programs McKenzie and Woodruff (2014), they have important ramifications for the interpretation of intent-to-treat estimates of program impacts (a point we return to below). Unsurprisingly, the take-up rate is extremely high in the cash grant treatment: 95 percent of those assigned to the grant treatment accepted and received the grant. We also find very little evidence of imperfect compliance with the evaluation design on the part of the implementing organization: no women assigned to the control group attended the business training, and only 1 percent were involved in starting a microfranchise.

4.4 Analysis

Our theoretical model predicts that infusions of funding will increase self-employment and income over the relatively short-term if individuals are unable to save through channels other than enterprise capital. For relatively unproductive individuals, these increases in income are temporary; they disappear as capital depreciates. Thus, impacts on entrepreneurship and income over the short-term do not indicate that capital infusions relieved a credit constraint or helped potential entrepreneurs to escape a poverty trap. In the presence of savings constraints, the key distinction between latent entrepreneurs and less productive individuals is that latent entrepreneurs can transform one-off infusions of capital into permanent increases in income. A comparison of shorter-term versus longer-term impacts indicates whether capital transfers are likely to have alleviated a poverty trap.

The cash grant intervention is exactly the type of unrestricted financial transfer described by our model. If the cash grant impacts occupational choice and income in the relatively short-term, analysis of longer-term impacts allows us to assess the extent to which the capital infusion relieved a poverty trap. Of course, if low productivity individuals are not savings constrained, there is little reason for them to knowingly launch an unproductive enterprise. In that case, an infusion of capital could increase consumption, savings, or assets (though possibly only over the relatively short-term), but would not impact occupational choice.

We model the impact of an infusion of capital, but our analysis compares two distinct interventions. An important question is whether an equivalently-valued intervention that offers enterprise capital in a more restricted form (including some in the form of human capital) has comparable impacts. Women assigned to the franchise treatment who did not wish to start a business and were not savings-constrained had the option of selling the physical capital that they received through the program, though we would expect the market value of, for example, a mobile food cart to be well below the cost of providing the entire microfranchise package of training and mentoring plus capital. Thus, if low productivity individuals who are not savings constrained participated in the program, we would not expect them to launch businesses, and the impacts on (e.g.) consumption might be relatively small. Alternatively, if credit and savings constraints are the main obstacles to successful entrepreneurship (and business training and mentoring add little value), we might expect the impacts of the franchise treatment to be smaller than the impacts of the grant treatment (because much of the program spending paid for training that, by assumption, would not be the relevant barrier to entrepreneurship for these individuals). On the other hand, the training and mentoring provided through the franchise program might impact participants' productivity, increasing the fraction of high productivity types. If this were the case, we would expect the impacts of the franchise treatment to be more persistent than those of the grant treatment — though they might initially be smaller in magnitude, depending on the initial mix of types in the population and the value of the capital transferred to franchise program participants.

We test these predictions using data from two rounds of surveys: midline surveys that were conducted between 7 and 10 months after the interventions and endline surveys that were conducted 14 to 22 months after the interventions. Both the midline and endline surveys contain detailed data on involvement in income-generating activities. The endline survey also includes a range of measures of consumption, expenditure, and well-being — which might be impacted by treatment if participants saved or consumed the value of the capital they received without launching a small business.

4.1 Estimation Strategy

In our main analysis, we report intent-to-treat (ITT) estimates of the impacts of the franchise treatment and the cash grant treatment on women assigned to each treatment group. Treatment assignment was random within strata, so the impacts of the interventions on any outcome Y_i can be estimated via the OLS regression specification:

$$Y_i = \alpha + \beta \cdot Franchise_i + \gamma \cdot Grant_i + \delta_{stratum} + \phi_{enumerator} + \zeta_{month} + \eta \cdot X_i + \varepsilon_i \quad (5)$$

where $Franchise_i$ and $Grant_i$ are indicators for, respectively, random assignment to the franchise treatment or the grant treatment, $\delta_{stratum}$ is a randomization stratum fixed effect, $\phi_{enumerator}$ is a survey enumerator fixed effect, ζ_{month} is a fixed effect for the month the survey was administered, X_i is a vector of individual controls, and ε_i is a conditionally-mean-zero error term.[15,16]

We also report treatment-on-the-treated (TOT) estimates that instrument for take-up (specifically, indicators for starting the business training portion of the franchise program and receiving the cash grant). Since take-up is almost universal among those assigned to the grant treatment, ITT and TOT estimates are nearly identical. However, the TOT estimates give us a better sense of how the franchise program impacted those who chose to participate (subject, of course, to additional assumptions).

[15] In our main specifications, we include controls for baseline household size, education level, and indicators for having given birth, having received any vocational training, or having any paid work experience prior to the baseline survey. Results are similar in magnitude and significance when these controls are omitted.

[16] We do not correct for the false discovery rate in our analysis of medium-term labor market outcomes: we consider a relatively small set of outcomes (because the midline survey did not collect data on a broader range of outcomes), none of which can be treated as statistically independent. As will become apparent in the subsequent discussion, most of these outcomes are impacted by the treatments over the medium-term; so the overall pattern of findings is unlikely to be explained by multiple testing. In our analysis of longer-term impacts, we look at a broad range of outcomes; as almost none are impacted by either treatment, there is little need to correct for the false discovery rate. However, we implement the multiple test correction procedure proposed by Benjamini and Hochberg (1995), following the procedures suggested by Anderson (2008). Results are discussed below.

4.2 Labor Market Outcomes 7–10 Months after Treatment

We summarize the (relatively) short-term impacts of the franchise and grant interventions on labor market outcomes in Table 4.3. Both the franchise treatment and the grant treatment had a positive and significant effect on the likelihood of self-employment, though they did not increase the likelihood of involvement in any income-generating activity. Women assigned to both treatments used the capital that they received to launch businesses. Point estimates suggest an extremely large effect: 24.5 percent of women assigned to the control group were self-employed at midline; the franchise and grant treatments both increased the likelihood of self-employment by approximately 10 percentage points. Coefficient estimates suggest that both interventions also reduced the likelihood of paid work for others, though the coefficients are not statistically significant at conventional levels.[17] As expected, the franchise treatment increased the likelihood of operating a microfranchise, while the grant treatment did not (Table 4.3, Panel B).

Though the grant and franchise treatments had similar impacts on the likelihood of self-employment and paid work, they had distinctly different impacts on labor supply (as shown in Table 4.3, Panel C). The grant treatment had a large positive impact on hours worked (over the week prior to the survey). The coefficient estimate indicates that women assigned to the grant treatment worked 6.8 more hours (p-value 0.019), which represents a 38 percent increase in hours worked. In

[17] The coefficient estimate on the franchise treatment suggests a marginally significant impact on the likelihood of paid work (p-value 0.061). The coefficient on the grant treatment is not even marginally significant (p-value 0.116).

contrast, the franchise treatment did not have a significant impact on the total number of hours worked (p-value 0.607), and we can reject the hypothesis that the two treatments had comparable impacts on hours worked (p-value 0.046). As expected, both treatments increased self-employment hours substantially; these increases are partially offset by modest (and insignificant) declines in the number of hours of paid work for others. The increases in self-employment hours are both large in magnitude and statistically significant. Assignment to the franchise treatment is associated with 4.1 additional self-employment hours per week (p-value 0.002), which represents an 87 percent increase in self-employment hours. Assignment to the grant treatment is associated with 7.6 additional hours of work in self-employment per week (p-value < 0.001), or a 162 percent increase in self-employment hours. Thus, both treatments are associated with substantial increases in both the likelihood of self-employment and the number of hours devoted to entrepreneurial activities.

Panel D of Table 4.3 summarizes the impacts of the treatment on income (excluding transfers). Neither treatment impacts the overall likelihood of reporting an income, but both the franchise treatment and the grant treatment had positive and significant impacts on income. The franchise treatment increased weekly income by 1.6 dollars (p-value 0.035); this represents about a 30 percent increase over the mean income in the control group of 5.5 dollars per week. The grant treatment increased income by 3.2 dollars a week (p-value 0.008), or 56 percent relative to the control group mean. Though the coefficient on the grant treatment is larger in magnitude than the coefficient on the franchise treatment, we cannot reject the hypothesis that the two treatments had statistically indistinguishable impacts on income (p-value 0.208). Results are similar if we focus on log transformations of income. As expected, the impacts on income are driven by extremely large (and statistically significant) increases in self-employment income that are not offset by any statistically significant changes in income from paid work. Thus, our results provide clear evidence that both the franchise treatment and the grant treatment encouraged young women to become self-employed; this shift into self employment was associated with large increases in income over the year after the interventions.

In the Online Appendix, we report instrumental variables estimates of the impact of the franchise and grant treatments on compliers (i.e. treatment-on-the-treated estimates). As expected, ITT and TOT estimates are nearly identical for the grant treatment, since 95 percent of those assigned to treatment received the grant. We can never reject the hypothesis that the TOT impacts of the franchise and grant treatments are identical. Thus, the evidence does not support the hypothesis that the franchise treatment had larger impacts on compliers than the grant treatment. The one important difference between our ITT and our TOT results is that we can no longer reject the hypothesis that the two treatments had different impacts on hours worked (p-value 0.140), though the point estimate suggests a much larger TOT effect for the grant treatment (7.1 additional hours versus 1.9 additional hours). Both the ITT and TOT effects of the treatments on income and occupational choice are statistically indistinguishable.

4.3 Labor Market Outcomes 14–22 Months after Treatment

In Table 4.4, we examine labor market outcomes 14 to 22 months after treatment. Looking across the range of outcomes related to occupational choice (Panels A and B), hours worked (Panel C), and income (Panel D), a clear pattern emerges: the impacts on hours and income that we observed at midline disappeared completely by the time of the endline survey. Looking at income, we see that neither treatment is associated with a significant increase in income at endline, and the point estimates for both treatments are negative. Moreover, the lack of significance is not simply the result of noise. The 95 percent confidence interval for the impact of grant treatment is $[-2.4, 2.3]$; this range does not include the point estimate (of 3.153) for the impact of the grant treatment after 7 to 10 months.[18] There is also no evidence that either treatment had a significant impact on hours worked (in the last week) 14 to 22 months after treatment. The coefficients on both the franchise treatment and the grant treatment are small and not statistically significant. Moreover, once again we find that the point estimate for the impact of the grant treatment on hours worked at midline is outside the 95 percent confidence interval for the impact at endline: the 95 percent confidence interval for the impact of the grant treatment on hours worked at endline is $[-3.7, 6.1]$; the point estimate for the impact on hours worked at midline was 6.8.[19]

[18] Similarly, the point estimate for the impact of the franchise treatment at midline, 1.6, is near the extreme end of the 95 percent confidence interval for the impact of the franchise treatment on incomes at endline. The 95 percent confidence interval is is $[-2.2, 1.7]$.

[19] The franchise treatment did not have a significant impact on hours worked at midline — so, of course, we cannot reject the hypothesis that the non-effects at midline and endline are identical.

Looking across the range of labor market outcomes, the clear pattern that emerges is that, by the time of the endline survey, impacts on hours and income had disappeared; however, impacts on occupational choice persisted. Both the franchise and the grant treatments increased the likelihood of self-employment at endline. The franchise treatment caused an 11.8 percentage point increase in the likelihood of self-employment (p-value 0.001) while the grant treatment led to a 12.9 percentage point increase in the likelihood of self-employment (p-value 0.003). Both effects are large in magnitude relative to the rate of self-employment in the comparison group, which is 24.3 percent. Both the franchise treatment and the grant treatment are also associated with large increases in self-employment hours and, to some extent, increases in income from self-employment (we observe significant impacts on log self-employment income, but not on the level of self-employment income).

Thus, the overall picture at endline is that the impacts of both the franchise treatment and the grant treatment are confined to the domain of occupational choice. Both treatments shift young women into self-employment, but have no overall impact on income or labor supply. One somewhat anomalous finding is that assignment to the franchise treatment is associated with a significant increase in the likelihood of reporting any income-generating activity. Though the increase is relatively large in magnitude (the coefficient estimate suggests a 7.6 percentage point increase in the likelihood of involvement in any IGA), it is difficult to interpret since the franchise treatment does not lead to increases in the total number of hours worked or the likelihood of reporting any income over the seven days prior to the survey.

In the Online Appendix, we show that the franchise treatment increased the likelihood of working in the salon or beauty sector at endline; otherwise, neither the franchise treatment nor the grant treatment had a significant impact on occupational sector at endline.[20] We also find no evidence of impacts on labor market churning: women assigned to treatment are not more likely to have either started or closed a business between midline and endline, nor are they more likely to have left a job or started a new job.

4.4 Impacts of Treatment on Firm Structure

In Table 4.5, we examine the impacts of the two labor market interventions on the characteristics of microenterprises. As always, we estimate Equation 5 in the full sample of women who completed the endline survey, but we also report the results of analogous specifications in a restricted sample of self-employed women. These latter specifications help to test the hypothesis that the interventions led to the creation of enterprises that differed in structure from those started by women in the control group.[21] As one would expect, we see that the franchise treatment increased the likelihood that a woman operates an enterprise that is directly linked to vocational training that she has received.[22,23] The grant treatment leads to significant increases

[20]The impact of the franchise treatment on the probability of working in the salon or beauty sector is robust to the multiple hypothesis testing procedure proposed by Benjamini and Hochberg (1995) (corrected q-value 0.010).

[21]In other words, the restricted sample helps us to distinguish between impacts that occur because the interventions increased the likelihood of self-employment, but without changing the character of self-employment, and impacts that are not the direct result of the overall increase in the self-employment rate among women assigned to treatment.

[22]This variable is equal to one if a woman who has received salon skills training operates a salon or beauty business, if a woman who has received tailoring training works as a self-employed tailor, or if a woman who has received culinary training operates a prepared food business.

[23]In the Online Appendix, we show that the franchise and grant treatments had significant impacts on the industrial sector in which women worked (in either self-employment or paid work for others) at midline, but that these effects had largely disappeared by the time of the endline

in the amount invested to start a business and the likelihood that a business was started with NGO funding; moreover, the businesses launched by women assigned to the grant treatment are significantly larger (in terms of the amount invested in them when they were launched) than the business operated by women assigned to either the control group or the franchise treatment. More interestingly, businesses launched by women in the grant treatment are also significantly more likely to employ others. The point estimate suggests that women assigned to the grant treatment are 5.8 percentage points more likely to run a business that employs anyone than women assigned to the control group (p-value 0.007), while businesses operated by women assigned to the grant treatment are 13.3 percentage points more likely to have employees than businesses operated by women in the control group (p-value 0.029). Thus, though the treatment effects on participant incomes disappear in the second year after treatment, positive spillovers on employees may persist.

4.5 Impacts on Other Outcomes

Though the impacts of the labor market interventions we evaluate dissipated over time, an important question is whether the treatments might have had longer-term impacts on other outcomes. As discussed above, women who are not savings constrained and are not productive entrepreneurs might save the funds that they received through the cash grant intervention; thus, the grants might increase con-

survey. At midline, both treatments were associated with a decrease in the likelihood of doing janitorial or trash collection work and an increase in the likelihood of working in the retail sector. The franchise treatment was also associated with an increase in the probability of working in the salon sector, while the grant treatment was associated with a decline in the probability of working in the salon sector. Only the impact of the franchise treatment on the likelihood of work in the salon sector persisted at endline.

sumption or expenditure without impacting income (except at the moment that the grant is disbursed) or occupational status. Alternatively, women might use grant money or resulting temporary increases in income to purchase durable assets that would improve their living conditions or quality of life over the relatively long-term. A third possibility is that the experience of receiving training and/or launching a business impacted self-confidence or empowerment. In any of these cases, we might expect the labor market interventions to have persistent impacts on overall welfare, even if labor market impacts are temporary.

In the Online Appendix, we estimate the impacts of the franchise and grant treatments on a range of outcomes: household assets, food security, expenditures, living arrangements and conditions, savings, time use, self-esteem, and empowerment. We find almost no evidence that the treatments had long-run impacts on any of these outcomes.[24] There is no evidence that the treatments improved women's living conditions or food security or increased their expenditures, nor is their any evidence of improvements in self-esteem or empowerment.[25] Thus, the evidence does not provide any meaningful support for the hypothesis that the interventions had temporary impacts on income but impacted overall welfare in a more permanent manner.

[24]In the Online Appendix, we report the estimated impacts of the franchise and grant treatments on 81 different outcomes. The estimated impacts of the franchise treatment on the likelihood of working in the salon sector or having done any vocational training are significant at the 99 percent level after implementing the multiple hypothesis testing correction proposed by Benjamini and Hochberg (1995). Those assigned to the grant treatment are also more likely to have paid school fees for someone else's child in the year after receiving the grant (Benjamini-Hochberg q-value 0.01). No other outcomes are significantly related to either treatment with adjusted q-values below 0.05.

[25]We use a range of measures including the Rosenberg self-esteem, the Ladder of Life, and Grit scales, plus the entire range of empowerment measures used by Bandiera *et al.* (2014) and Adoho *et al.* (2014).

4.6 Comparing Implementation Costs

The two treatment arms of our study allow for natural cost comparisons, complementing our overall estimates of each program's impacts. Costs in the cash grant arm are relatively straightforward. The cash grant itself was worth 239 US dollars. Because compliance was slightly below 100 percent, the average disbursement per respondent in the cash grant arm was 228 dollars. Besides simply transferring the money, administrative tasks supporting this arm included having field team members meet participants twice (once to explain the no-strings-attached grant, once for the actual transfer); confirming, via fingerprint reader, that the individuals our team met with were indeed the intended recipients; and data, accounting, and other indirect costs. These administrative tasks cost a total of roughly 82 dollars per intended recipient. Thus, the total cost of the cash grant arm, per intended recipient, was roughly 310 dollars.

Costs in the microfranchising intervention are more complicated. We begin with all costs that the IRC incurred implementing the program over three fiscal years. This study evaluates only the final calendar year of the program, but other participants were involved in the prior calendar year, and setup costs were required beforehand to make the program possible. Once we arrive at a total cost figure (the numerator), we divide by the total number of participants across all program years (the denominator). We face a number of decisions in both arriving at a total cost figure and in arriving at the number of participants, so we report upper and lower bounds on our cost estimates.[26]

One of the smallest cost items in the IRC budget is international staff support costs. We exclude this for simplicity. A larger cost is internationally hired staff in Kenya, including portions of the country director's time. Our upper bound includes these costs; our lower bound excludes them on the basis that they are needed most intensely for the startup phase of a project. The rest of the costs (national staff time, business support, trainings, office expenses, etc.) are concentrated in the two fiscal years in which the program trained most participants, but there are some costs from the first fiscal year in which the program began and in which the first participants started training. Our upper bound includes these costs; our lower bound includes only half of the first fiscal year's costs, on the basis that continued program operation or operation at larger scale would involve lower startup costs. The upper bound figure for the total cost of the program is roughly 763,000 dollars; the lower bound is 637,000 dollars. Either way, half of the costs come from providing trainings, including the (substantial) costs of providing refreshments for hundreds of participants each day.

These total cost estimates translate into a cost of between 616 dollars and 809 dollars per participant in the microfranchising arm.[27] However, this figure is the

[26] In order to determine cost per activity, each project expense was allocated, completely or partially, to either entrepreneurship activities, cash dispersements, or other non-treatment activities, and summed to determine total cost per activity. Total values were then divided by number of clients served to get an average cost per client. See International Rescue Committee (2016a) for a detailed discussion of the costing methodology.

[27] The number of participants in the microfranchising program was carefully recorded by the local partner organizations that helped run the training sessions. Over the duration of the program, there were 898 participants in these sessions: 297 in the first program year, and 601 in the second. Women launching businesses were encouraged to involve others in their enterprises, but in the first year, records only indicated 45 additional participants of this type. This leads to the lower bound figure of $898 + 45 = 943$ participants. We were unable to obtain detailed records of any others

cost associated with the treatment on the treated — not the cost for the intention to treat. This distinction matters because while 95 percent of those assigned to the grant treatment received a grant, only 61 percent of those assigned to the microfranchising treatment actually started the training. The intervention costs per individual *assigned* to the relevant treatment are thus roughly 286 dollars for the grant arm, and between 376 dollars and 494 dollars for the microfranchising arm.

The point estimates in Tables 4.3 and 4.4 for impacts of the cash grant are generally larger than (though not statistically distinguishable from) the point estimates for the microfranchising intervention; this suggests that they are comparable in effectiveness, though the point estimates suggest that the cash grant is slightly more effective. The somewhat higher costs of the microfranchising treatment do not substantially change this picture, though they tilt it further in favor of the cash grant: point estimates for the cash grant suggest it is more cost-effective than microfranchising across a range of outcomes and follow-up durations. The difference is statistically significant at the 10 percent level for 7–10 month effects on income, but otherwise is generally not statistically significant.

A full cost-benefit analysis involves measuring the extent of the benefits that accrued to participants over time. We only measure the benefits at two points in time: 7–10 months after treatment, and 14–22 months after treatment. The effects we find are statistically significant at the first of these follow-ups, but not at the second. We arrive at a lower bound on the benefits by multiplying the shorter-term

involved in new enterprises in the second year, but we can extrapolate that it is proportional to the number of participants, so roughly twice the number in the second year as in the first. This leads us to an upper bound estimate of $898 + 45 + 91 = 1034$ participants overall.

impacts on income by the period between the start of the program and the survey, assuming that the impacts disappeared immediately after the 7–10 month follow-up; this is, in essence, the area of a rectangle 7–10 months wide and as tall as the impact estimate. A reasonable upper bound extends these impacts (the width of the rectangle) until just before the 14–22 month follow-up.[28] Using these approaches, and the coefficients on income in Table 4.3, the microfranchising intervention had total income benefits of between 60 dollars and 116 dollars; the cash grant had total income benefits of between 128 dollars and 247 dollars.

Neither intervention shows signs of the benefits exceeding the costs. However, the amount of the grant (239 dollars) falls between the upper and lower bounds of the estimated impacts on income over the year after the intervention. This suggests that grant recipients do a relatively efficient job of smoothing their income by investing grants in enterprise capital. If such one-off grants could be distributed with minimal overhead costs (as in larger programs like GiveDirectly), or the distributional benefits of making transfers to vulnerable populations justified a modest level of transaction costs, cash transfers could be socially desirable. The franchise treatment that we study achieves lower (temporary) income gains at higher cost; it is therefore reasonable to conclude that cash grants are a more efficient approach to achieving the same level of redistribution.

[28] A nearly-equivalent approach to the upper bound calculation assumes a downward ramp shape: large impacts at first, tapering linearly to zero at the 14–22 month follow-up, and with a height that is only measured at the 7–10 month follow-up. The area of the resulting triangle is just slightly larger than that of the upper bound rectangle, since the follow-up when the "height" is measured is just under halfway along the "base" of the triangle. This approach generates similar estimates of the total program impacts on income.

4.5 Participant Evaluations

Given the tremendous lengths one must go to in order to produce credible estimates of a program's impacts, an important question is whether participants themselves understand the effects of the programs in which they participate. It is not uncommon for labor market programs to survey participants *ex post*; however, Smith, Whalley, and Wilcox (2012) find that such *ex post* assessments of a program's impact are not highly correlated with objective measures of program effects. Understanding participants' beliefs about program impacts is important for two reasons. Most obviously, if — through their participation — participants obtain reasonable estimates of program impacts, this information may be a feasible, low-cost alternative to formal impact evaluation. On the other hand, if program participants do not understand a program's impacts, even after they have participated in the program, it is hard to imagine that they are making optimal decisions about whether or not to participate.

4.1 Empirical Approach and Practical Considerations

As Smith, Whalley, and Wilcox (2012) point out, one reason participant evaluations of programs may differ from rigorous estimates of program impacts is that participant evaluation questions are often quite open-ended. For example, participants in the National Job Training Partnership Act program were asked "Do you think that the training or other assistance that you got from the program helped you get a job or perform better on the job?" (Smith, Whalley, and Wilcox, 2011,

p. 9). This question is obviously problematic because it is not at all clear whether better on-the-job performance should be linked to any measurable outcome (e.g. income); moreover, the link between the fraction of participants who believe that the program had a positive impact and the estimated treatment effect of the program is unclear, making it difficult to test whether participants' subjective evaluations are accurate. Smith, Whalley, and Wilcox (2012) suggest replacing such subjective evaluation questions with alternatives that (i) clearly specify the outcomes and time periods of interest, (ii) ask for continuous (as opposed to binary) responses that can be directly compared to ITT estimates, and (iii) make the counterfactual nature of the question transparent.

We follow the recommendations of Smith, Whalley, and Wilcox (2012) and ask participants in the franchise and grant treatments to estimate the counterfactual probabilities of self-employment and paid work for a reference group of women similar to themselves. Specifically, we ask women in each of the two treatment arms the question: "I would like you to imagine 100 women from [your neighborhood] who applied to the [name of treatment arm] program but who were not admitted into it. In other words, please think about 100 women similar to yourself who were not selected to the [name of treatment arm] program. Out of 100 women, how many do you think are currently running or operating their own business?" We also ask an analogous question about involvement in paid work for others. Smith, Whalley, and Wilcox (2012) suggest using this question to construct a perceived counterfactual, which can then be compared with the average outcome in the treatment group. We take a different approach, asking each participant to estimate how many of 100 women similar to themselves who "applied for and were admitted into" the program were (at the time of the survey) operating their own business (and, in a subsequent question, we ask how many were doing paid work for others). We calculate each participant's belief about the treatment effect of the program (on, for example, self-employment) by taking the difference between the perceived frequency of self-employment among women invited to participate in the program and the perceived frequency of self-employment among similar women who were not invited to participate.

We also test a second method proposed by Smith, Whalley, and Wilcox (2012): asking participants about the probability that they would be self-employed (or doing paid work for others) in the absence of the program. These individual-level beliefs about one's own counterfactual can then be combined with data on actual outcomes to construct estimates of perceived treatment effects. However, as Smith, Whalley, and Wilcox (2012) emphasize, there are several drawbacks to this approach. First, program participants may find it inherently difficult to imagine what their lives would have been like in the absence of the program. For example, psychological studies of "hindsight bias" suggest that people have a difficult time remembering the beliefs they held in the past and tend to assume that realized outcomes were always foreseeable Fischhoff (1975); Madarász (2012). In our context, we might expect that those who have received vocational training and gained self-employment experience might have a difficult time remembering that they had not always known how to operate a business; thus, hindsight bias might inflate participants' estimates of their own counterfactual, particularly among successful microentrepreneurs. Estimates of one's own counterfactual may also be biased by the tendency to attribute one's own success to individual agency as opposed to external factors Miller and Ross (1975). This would lead those who have benefited from business or vocational training to overstate the likelihood that they would have started a successful business in the absence of the program.

In the context of our evaluation, a third problem with questions designed to elicit beliefs about one's own counterfactual probability of self-employment (or paid work) is that they are unlikely to work well when respondents have low levels of numeracy. Though almost 92 percent of the women in our sample completed primary school, a relatively large number are not familiar with the concept of percentages. Roughly one in four cannot (correctly) answer the question: "If there is a 75 percent chance of rain and a 25 percent chance of sun, which type of weather is more likely?" While it is possible to elicit probabilistic expectations from subjects with no prior knowledge of probability, it is costly and time-consuming to do so. Instead, we asked every subject categorical questions about their counterfactual probabilities of self-employment and paid work, and collected more specific data on counterfactual probabilities from those who successfully answered the screening question described above.[29]

[29]We worded the categorical question to make responses directly comparable to probability estimates. Respondents chose one of the following options: (1) *In the absence of the program, I would definitely be self-employed*, (2) *In the absence of the program, I would probably be self-employed but it is not certain*, (3) *In the absence of the program, the chances of me being self-employed or not self-employed are equal*, (4) *In the absence of the program, I would probably not be self-employed but it is not certain*, or (5) *In the absence of the program, I would definitely not be self-employed*.

4.2 Framework for Interpreting Empirics

To facilitate comparisons between different approaches to belief elicitation, we introduce a simple conceptual framework that formalizes the measurement issues highlighted above. First, consider an outcome, y, and a program whose causal effect on that outcome is to increase its expected value by $\beta > 0$. Let γ denote the expected value of y in the absence of the program: $E[y_j|T_j = 0] = \gamma$.

We wish to know whether program participants hold accurate beliefs about β. Let

$$\tilde{\beta}_i = \tilde{\beta} + \phi_i \tag{6}$$

denote participant i's belief about the impact of the program, and let

$$\tilde{E}[y_j|T_j = 0] = \tilde{\gamma} + \nu_i \tag{7}$$

be participant i's belief about the expected value of the outcome of interest for an untreated individual j who is outwardly similar to her. $\tilde{\beta}$ is the average belief about the impact of the program, and $\tilde{\gamma}$ is the average belief about the outcome of interest in the eligible population in the absence of the program. ϕ_i is the idiosyncratic component of beliefs about the impact of the program; without loss of generality, we assume that the distribution of ϕ_i is mean zero, and we let σ_ϕ denote its variance. ν_i can be decomposed into a mean-zero error term and a term which reflects the perceived difference between the population average of y and one's own counterfactual:

$$\nu_i = \tilde{\alpha}_i \cdot \mathbb{1}(j = i) + \epsilon_i. \tag{8}$$

As discussed above, asking participants about their own counterfactuals may be problematic (for example, because of hindsight bias), and the population mean of these $\tilde{\alpha}_i$ values, $\tilde{\alpha} = E[\tilde{\alpha}_i]$ may not be equal to 0.[30] Combining and generalizing these expressions, respondents report:

$$\tilde{E}[y_j|T_j] = \tilde{\beta} \cdot T_j + \tilde{\gamma} + \tilde{\alpha}_i \cdot \mathbb{1}(j = i) + \phi_i \cdot T_j + \epsilon_i \tag{9}$$

Specifically, when asked to report the rate of self-employment among 100 potential program participants who were not invited to participate in the program, a respondent in our study reports:

$$\tilde{E}[y_j|T_j = 0] = \tilde{\gamma} + \epsilon_i. \tag{10}$$

When asked to report the rate of self-employment among 100 potential program participants who were invited to participate in the program, she reports:

$$\tilde{E}[y_j|T_j = 1] = \tilde{\beta} + \tilde{\gamma} + \phi_i + \epsilon_i. \tag{11}$$

Finally, when asked to report her own counterfactual probability of self-employment,

[30] This may be thought of as a "Lake Wobegon" effect.

a participant reports:

$$\tilde{E}[y_i|T_i = 0] = \tilde{\gamma} + \tilde{\alpha}_i + \epsilon_i. \tag{12}$$

The framework presented above helps to clarify the distinctions between the different approaches to estimating participant beliefs. First, consider an estimate of participant beliefs constructed by taking the average belief about one's own counterfactual (in our context, the counterfactual probability of self-employment) and subtracting this from the observed outcome in the treatment group. The expected value of this estimator is:

$$\begin{aligned} E[y_j|T_j = 1] - E[\tilde{E}[y_i|T_i = 0]] &= \beta + \gamma - (\tilde{\gamma} + \tilde{\alpha} + E[\epsilon_i]) \\ &= \beta + (\gamma - \tilde{\gamma}) - \tilde{\alpha} \end{aligned} \tag{13}$$

since $E[\epsilon_i] = 0$. Thus, this estimator will be biased if participants hold inaccurate beliefs about the counterfactual probability of self-employment, and it will be biased when psychological factors such as hindsight bias lead participants to overstate their own counterfactual probability of self-employment. The second estimator proposed by Smith, Whalley, and Wilcox (2012) is constructed by subtracting the mean rate of self-employment in a reference group of untreated women from the observed rate of self-employment in the treatment group. The expected value of this estimator is given by:

$$\begin{aligned} E[y_j|T_j = 1] - E[\tilde{E}[y_j|T_j = 0]] &= \beta + \gamma - (\tilde{\gamma} + E[\epsilon_i]) \\ &= \beta + (\gamma - \tilde{\gamma}) \end{aligned} \tag{14}$$

This estimator overcomes the behavioral issues inherent in estimating one's own counterfactual. However, when estimates of participant beliefs constructed in this manner diverge from actual program impacts, it is impossible to determine whether participants hold inaccurate beliefs about the impact of the program or inaccurate beliefs about the counterfactual.

The outcomes of interest in impact evaluations are often difficult to measure, and considerable effort goes into the design and pre-testing of questionnaires. Nonetheless, there is no guarantee that outcome measures derived from survey questions (for example, about labor market participation) and participant responses to belief-elicitation questions will line up, particularly in low-income settings where formal, full-time employment is relatively uncommon (and there is continuous variation in the number of hours worked, and labor supply varies substantially from week to week).[31] Impact evaluation questions designed to measure beliefs about the counterfactual may reveal systematic deviations between participants' beliefs about outcome levels and actual outcome levels; however, such measurement error is only problematic if it cannot be separated from the quantity of interest. To address this issue, we propose an estimate of participant beliefs that is calculated by taking the difference between beliefs about the mean outcome of interest in a reference

[31] Smith, Whalley, and Wilcox (2012) are aware of this issue and recommend asking extremely specific questions: for example, what fraction of participants meet a well-specified criterion for employment — for example, working more than 35 hours per week — which can then be used to construct the empirical estimate of the programs impact. However, such precisely worded questions are not always feasible. In our context, we worried that any question of the form "Out of 100 women, how many spend at least X hours operating their own business?" would be substantially more difficult to answer than a less specific question because few people work full-time and there is no obvious break in the distribution of hours worked at any point.

population of treatment versus control individuals:

$$
\begin{aligned}
E[\tilde{E}[y_j|T_j = 0]] - E[\tilde{E}[y_j|T_j = 0]] \\
&= \tilde{\beta} + \tilde{\gamma} + E[\phi_i] + E[\epsilon_i] - (\tilde{\gamma} + E[\epsilon_i]) \\
&= \tilde{\beta}
\end{aligned}
\tag{15}
$$

Such an estimator allows for a direct test of the hypothesis that participants hold accurate beliefs about program impacts; moreover, collection of the relevant data necessarily also allows researchers to assess the related issue of whether participants can estimate the counterfactual — allowing for a comparison of the different approaches of belief estimation.

4.3 Results

Our results, which are summarized in Figure 4.1, suggest that participants hold remarkably accurate beliefs about program impacts. The figure compares ITT estimates of program impacts to estimates of participant beliefs about program impacts calculated by taking the difference in reference group probabilities for the treatment and control groups.[32] For example, the ITT estimates suggest that the franchise treatment increased the likelihood of self-employment by 11.9 percentage points; those assigned to the program believe that it increased the likelihood of self-employment by 12.3 percentage points. Similarly, those assigned to the cash grant

[32]In other words, beliefs were estimated by asking women assigned to each treatment group to estimate reference group probabilities (frequencies) for both the treatment and comparison groups. Women assigned to the control group were not asked to estimate a reference group probability for those assigned to the treatment groups since they were not familiar with the details of each treatment.

treatment believe that it increased the likelihood of self-employment by 10.6 percentage points; the ITT estimates suggest a 12.9 percentage point increase. Those assigned to the franchise treatment also have remarkably accurate beliefs about the program's impact on the likelihood of paid employment. Those assigned to the cash grant treatment have less accurate beliefs about the program's impact on paid employment, though they are appropriately signed and well within the confidence interval of the estimated treatment effect. Thus, our results suggest that participants' do a reasonably good job of estimating the impact of programs that they have participated in. For the outcome most directly impacted by the treatments (self-employment), participants do a remarkably good job of estimating the program's impacts.

Figure 4.2 compares beliefs about the probability of self-employment and paid work to levels observed in the treatment and control groups, and compares beliefs about one's own counterfactual to beliefs about a reference population of untreated women. Several patterns are apparent. First, women in the franchise treatment group underestimate the probability of paid work in both the treatment and the control group. Consequently, an estimate of the impact of the franchise program on the probability of paid work that compared counterfactual beliefs to observed levels in the treatment group would perform very poorly. Women in both the franchise and grant treatments hold more accurate beliefs about the level of self-employment (in both the treatment and control groups); however, women in both treatment arms seem to overestimate the frequency of self-employment and underestimate the frequency of paid work in both the treatment and the control groups. Thus, differences between observed outcome levels and participant beliefs appear to be systematic, suggesting that it will typically be better to estimate program beliefs by comparing beliefs about the control group to beliefs about the treatment group (rather than the observed outcome levels in the treatment group).

The figure also demonstrates that concerns that estimates of one's own counterfactual might be biased appear well-founded: the average of own counterfactual estimates is consistently higher than the estimated outcome for a reference population of untreated women. This pattern is particularly pronounced for the franchise treatment, most dramatically when participants are asked to report their own counterfactual probability of self-employment. Though participants hold accurate beliefs about the level of self-employment in both the treatment and control groups, own counterfactual estimates are so inflated that they suggest a negative impact of the program on self-employment. Thus, our evidence clearly supports the view that own counterfactual estimates are of little use in estimating treatment effects. This finding is consistent with recent work by McKenzie (2016a); he finds that program participants (business owners) do a very poor job of estimating the counterfactual. Our results support his conclusion, but suggest that an alternative approach to eliciting participants' beliefs performs substantially better.

4.6 Conclusion

We report the results of an impact evaluation comparing two labor market interventions that were offered to young, unemployed women in some of Nairobi's poorest neighborhoods. The multifaceted franchise program we evaluate provided participants with business and life skills training, vocational training, business-specific capital and supply chain linkages, and ongoing mentoring. This program was meant to simultaneously address both credit constraints and other obstacles to youth entrepreneurship. The cash grant program was a simple intervention that provided participants with an unrestricted grant of 20,000 Kenyan shillings (equivalent to 239 US dollars in 2013). Both treatments were randomly assigned (offered) to eligible applicants to the franchise program; our randomized design allows us to compare the two programs, and to compare both programs to a control group.

We find that both programs increased the likelihood of self-employment among eligible participants. In addition, both the franchise treatment and the grant treatment had large and statistically significant impacts on income in the year after the program. However, the impacts on income did not persist. By the second year after treatment, women assigned to both the franchise and grant treatments looked similar to the control group in terms of income, labor supply, food security, expenditures, living conditions, and empowerment.

Seen through the lens of a simple theoretical model, our findings suggest that individuals in our sample are savings-constrained; they launch unsustainable businesses to stretch out the capital infusions provided by the interventions. Our findings suggest that the training component of the franchise intervention did not increase individual productivity sufficiently to create enduring, profitable entrepreneurship. Our findings are also not consistent with the existence of a credit-constraint-based poverty trap. Of course, our results should not be taken as evidence that credit constraints *never* generate poverty traps. Recent studies by Blattman, Fiala, and Martinez (2014) and Blattman *et al.* (2016) suggest that credit constraints may well be preventing latent entrepreneurs from launching successful businesses in recently conflict-affected regions of northern Uganda. However, our findings resonate with a number of recent studies of cash grants and other credit market interventions. Studies of the return to capital among microenterprises operated by women in developing countries have consistently failed to find positive impacts on business profits, though cash grants do help *men* expand their businesses in some contexts (cf. De Mel, McKenzie, and Woodruff 2008, De Mel, McKenzie, and Woodruff 2009, Fafchamps, McKenzie, Quinn, and Woodruff 2011, Fiala 2014, Karlan, Knight, and Udry 2015). Recent randomized evaluations of microfinance also suggest that access to credit has, at best, a limited impact on enterprise profits (cf. Angelucci, Karlan, and Zinman 2015, Attanasio *et al.* 2015, Augsburg, De Haas, Harmgart, and Meghir 2015, Banerjee, Duflo, Glennerster, and Kinnan 2015, Crépon, Devoto, Duflo, and Parienté 2015, Tarozzi, Desai, and Johnson 2015). Our findings also coincide with the estimated (short-term) impact of the cash grant program offered by the NGO GiveDirectly: Haushofer and Shapiro (2016) find that grants led to increased revenues from farm and non-farm enterprises, but not increased profits (see Haushofer and Shapiro 2016, Online Appendix Table 77). Taken together, these studies suggest that credit constraints are not the main obstacle preventing the poor — particularly poor women — from launching and expanding profitable, sustainable businesses.

Yet, even when they don't lead to permanent increases in income, cash grants may have important impacts. Haushofer and Shapiro (2016) find that cash transfers improved psychological wellbeing. Our results show that grants lead to economically large and statistically significant impacts on income for almost a year after treatment; it is reasonable to conclude that these increases in income were also associated with improved wellbeing within that time frame. Moreover, as in other studies of cash transfers, we see no sign of excessive spending on temptation goods Evans and Popova (2016). Also as in other studies of cash transfers, we see that if anything, cash grants temporarily induced an increase in labor force participation, with no evidence of a decrease in either the short or long term Banerjee, Hanna, Kreindler, and Olken (2015). Thus, our results are consistent with the view that one-off cash transfers are a simple, direct way of improving the wellbeing of the poor and vulnerable. Because grants were used to launch small-scale businesses, impacts persisted for some time, though they were not permanent.

Point estimates suggest that the cash grant was more cost effective than the franchise treatment. Other populations or subgroups could, of course, experience different benefits. Within our sample, the impacts of the franchise treatment were probably greatest among the 39 percent who actually launched businesses, relative to the 22 percent who only did some of the training but never launched businesses or the remainder of those assigned to the franchise treatment, who chose not to participate in the program. Better targeting could potentially improve impacts.[33] However, our protocol did include a reasonably high degree of screening based on non-monetary

[33]Several recent studies find positive impacts of cash grants on potential entrepreneurs who were required to submit detailed business plans Blattman, Fiala, and Martinez (2014); McKenzie (2016b)cf.. However, the interventions we study were intended to assist poor young women with very limited work experience, many of whom might not have been able to produce detailed business plans prior to the program.

effort costs Dupas, Hoffmann, Kremer, and Zwane (2016): everyone in our sample first filled out an application form and then visited the implementing organization's office to complete a baseline survey. Moreover, a lengthier application process would also come with its own implementation costs. Thus, given the observed pattern of impacts, the cash grant intervention appears both simpler and more cost-effective.

Our results emphasize the importance of examining relatively long-run outcomes and collecting multiple rounds of post-treatment data whenever possible. We show that while participants in our study may face credit constraints, these constraints are not acting as a poverty trap; savings constraints provide a better explanation for the patterns of outcomes that we observe. Though transforming unemployed young women into profitable entrepreneurs is a laudable policy goal, our results suggest that it may be difficult to achieve in urban contexts, where markets are active and potentially quite competitive. However, one-off cash transfers can work as a relatively cost-effective means of income support for vulnerable young women; helping these vulnerable individuals may be a sufficient policy goal in and of itself.

Table 4.1: Sample Characteristics at Baseline

	Obs.	Mean	S.D.	Median	Min.	Max.
Panel A. Demographics and Household Composition						
Age	905	18.780	0.787	19	17	20
At least one parent alive	903	0.884	0.321	1	0	1
Household size	905	4.882	2.168	5	1	13
Married or cohabitating	905	0.165	0.371	0	0	1
Has given birth	905	0.409	0.492	0	0	1
Panel B. Educational Background						
Father's education, if known	554	9.773	2.990	11	0	16
Mother's education, if known	714	9.036	2.868	8	0	16
Years of education	905	9.894	2.055	10	0	12
Any vocational training	905	0.345	0.476	0	0	1
Panel C. Involvement in Income-Generating Activities						
Any (paid) work experience	905	0.546	0.498	1	0	1
Engaged in any income-generating activities	905	0.146	0.353	0	0	1
Any self-employment activity	905	0.052	0.232	0	0	2
Any paid work for someone else	905	0.099	0.303	0	0	2
Hours of housework in last week	884	26.072	15.295	21	4	84
Panel D. Assets, Saving, and Living Conditions						
Food insecurity index	904	0.259	0.175	0.250	0	0.929
Has a personal bank account	901	0.088	0.283	0	0	1
Has any savings (including jewelry)	904	0.330	0.470	0	0	1
Value of savings (in USD)	905	4.938	14.774	0	0	104.886
Value of savings, if any (in USD)	248	18.022	23.709	8.911	0.593	104.886
Owns a personal mobile phone	905	0.734	0.442	1	0	1
Household has electricty	905	0.750	0.433	1	0	1
Household has piped water	905	0.490	0.500	0	0	1
Household owns a television	905	0.568	0.496	1	0	1
Household owns a radio	905	0.685	0.465	1	0	1
Household asset index	905	-0.000	1.000	-0.080	-1.670	3.933

The food insecurity access scale is an adaptation of the measure proposed by the Food and Nutrition Technical Assistance (FANTA) Project; the measure used at baseline is based on 7 questions, and is rescaled to range from 0 (no food insecurity) to 1 (the maximum level of food insecurity). Savings balances are first deflated using CPI data from the Kenya National Bureau of Statistics to reflect prevailing prices in July 2013, when the first baseline surveys were conducted; balances are then converted to US dollars using the average exchange rate from July 2013 (84.04 Kenyan shillings to the dollar). The top 1 percent of values of the VALUE OF SAVINGS variable are trimmed. The household asset index is calculated by taking the first principal component of the indicators for whether a respondent's household or dwelling has power, piped water, a radio, a television, a gas or electric stove, a refrigerator, a motorcycle, a bicycle, a DVD player, and a computer; the first principal component is then normalized to be mean-zero and have a standard deviation of one.

Table 4.2: Compliance with Treatment

	Control (1)	**Franchise Treatment (2)**	**Grant Treatment (3)**
Completed baseline survey	1.00	1.00	1.00
Attended business training	0.00	0.61	0.01
Helped to start a microfranchise	0.01	0.39	0.01
Received a cash grant	0.00	0.00	0.95
Observations	363	360	182

Compliance rates for the franchise treatment are calculated using administrative records (attendance sign-in sheets) from the implementing organization and its local partners. Compliance rates for the cash grant treatment are calculated from the disbursement records of the research organization. Estimates of compliance based on self-reports of program participation (recorded during the first Midline Survey) yield nearly identical compliance rates.

Table 4.3: Intent to Treat Estimates: Labor Market Outcomes after 7–10 Months

	Obs. (1)	Control Mean (2)	Treatment Effects: Franchise Treatment (3)	Treatment Effects: Grant Treatment (4)	p-value: F = G (5)
Panel A. Involvement in Income-Generating Activities (Previous Month)					
Engaged in any income-generating activities	851	0.586	0.019 (0.038)	0.024 (0.046)	0.918
Any self-employment activity	851	0.245	0.098*** (0.035)	0.101** (0.043)	0.940
Paid work for someone else	851	0.382	-0.069* (0.037)	-0.070 (0.045)	0.973
Panel B. Likelihood of Operating a Microfranchise (Previous Month)					
Operates a microfranchise	851	0.000	0.085*** (0.015)	-0.001 (0.004)	0.000
Operates a salon microfranchise	851	0.000	0.050*** (0.012)	-0.003 (0.003)	0.000
Operates a food cart microfranchise	851	0.000	0.036*** (0.010)	0.001 (0.003)	0.001
Panel C. Labor Supply (Previous 7 Days)					
Hours worked in last week	851	17.945	1.097 (2.131)	6.831** (2.903)	0.046
Self-employment hours	851	4.723	4.127*** (1.353)	7.634*** (2.012)	0.104
Hours of paid work for someone else	851	13.017	-2.880 (1.787)	-0.871 (2.342)	0.365
Panel D. Income Excluding Transfers (Previous 7 Days)					
Reports any labor income	851	0.466	0.056 (0.038)	0.060 (0.047)	0.939
Income excluding transfers (in USD)	851	5.476	1.637** (0.775)	3.153*** (1.179)	0.208
Log income (in USD)	851	-1.436	0.508** (0.253)	0.560* (0.317)	0.870
Self-employment income (in USD)	851	2.617	1.305** (0.615)	2.306** (1.001)	0.314
Log of self-employment income (in USD)	851	-3.158	0.633*** (0.215)	0.705** (0.277)	0.802
Income from paid work for someone else (in USD)	851	2.901	0.092 (0.480)	0.489 (0.650)	0.557
Log of income from paid work (in USD)	851	-2.595	-0.087 (0.222)	-0.063 (0.273)	0.931

Robust standard errors in parentheses. *, **, and *** indicate significance at the 90, 95, and 99 percent confidence levels, respectively. OLS regressions reported. All specifications include controls for baseline household size, education level, and indicators for having given birth, having received any vocational training, or having any paid work experience prior to the baseline survey, in addition to survey enumerator and survey month fixed effects. Incomes are deflated to July 2013 levels using CPI data from the Kenya National Bureau of Statistics, then converted to US dollars using the average exchange rate from July 2013 (84.04 Kenyan shillings to the dollar). The top 1 percent of values of all hours and income variables are trimmed.

Table 4.4: Intent to Treat Estimates: Labor Market Outcomes after 14–22 Months

			Treatment Effects		
	Obs.	Control Mean	Franchise Treatment	Grant Treatment	p-value: F = G
	(1)	(2)	(3)	(4)	(5)
Panel A. Involvement in Income-Generating Activities (Previous Month)					
Engaged in any income-generating activities	837	0.657	0.076**	0.057	0.655
			(0.035)	(0.043)	
Any self-employment activity	837	0.243	0.118***	0.129***	0.798
			(0.035)	(0.043)	
Works for someone else	837	0.497	-0.040	-0.063	0.635
			(0.040)	(0.048)	
Panel B. Likelihood of Operating a Microfranchise					
Operates a microfranchise	837	0.000	0.038***	-0.002	0.001
			(0.011)	(0.003)	
Operates a salon microfranchise	837	0.000	0.028***	-0.002	0.003
			(0.009)	(0.003)	
Operates a food cart microfranchise	837	0.000	0.009*	-0.000	0.087
			(0.005)	(0.002)	
Panel C. Labor Supply (Previous 7 Days)					
Hours worked in last week	837	19.130	1.490	1.223	0.919
			(2.103)	(2.520)	
Self-employment hours	837	3.509	3.094***	4.406***	0.427
			(1.141)	(1.441)	
Hours of paid work for someone else	837	15.559	-1.758	-3.180	0.538
			(1.961)	(2.267)	
Hours of unpaid work in the last week	837	23.364	-0.952	-0.995	0.975
			(1.278)	(1.459)	
Panel D. Income Excluding Transfers (Previous 7 Days)					
Reports any labor income	837	0.556	0.036	0.062	0.584
			(0.039)	(0.047)	
Income excluding transfers (in USD)	837	9.106	-0.239	-0.038	0.858
			(1.013)	(1.198)	
Log income (in USD)	837	-0.655	0.252	0.435	0.577
			(0.270)	(0.326)	
Income from self-employment (in USD)	837	2.849	1.022	1.373	0.679
			(0.715)	(0.863)	
Log of income from self-employment (in USD)	837	-3.276	0.575***	0.988***	0.184
			(0.221)	(0.292)	
Income from paid work for someone else (in USD)	837	6.060	-1.107	-0.958	0.862
			(0.765)	(0.883)	
Log of income from paid work (in USD)	837	-1.331	-0.304	-0.514	0.552
			(0.302)	(0.351)	

Robust standard errors in parentheses. *, **, and *** indicate significance at the 90, 95, and 99 percent confidence levels, respectively. OLS regressions reported. All specifications include controls for baseline household size, education level, and indicators for having given birth, having received any vocational training, or having any paid work experience prior to the baseline survey, in addition to survey enumerator and survey month fixed effects. Incomes are deflated to July 2013 levels using CPI data from the Kenya National Bureau of Statistics, then converted to US dollars using the average exchange rate from July 2013 (84.04 Kenyan shillings to the dollar). The top 1 percent of values of all hours and income variables are trimmed.

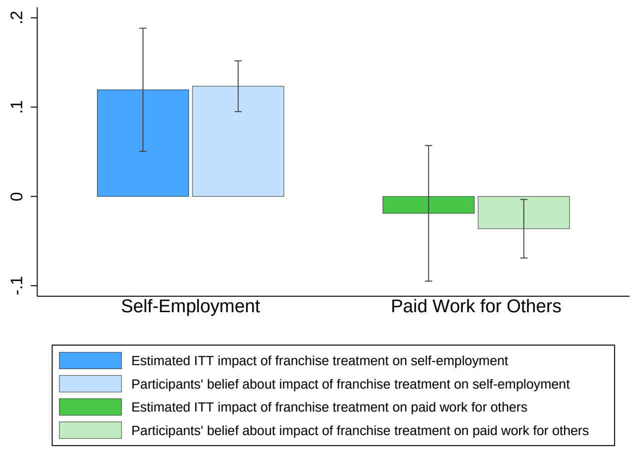
Table 4.5: Firm Structure and Business Practices after 14–22 Months

	Not Conditional on Self-Employment				Conditional on Self-Employment			
		Treatment Effects				"Treatment" Effects		
	Control Mean (1)	Franchise Treatment (2)	Grant Treatment (3)	p-value: F = G (4)	Control Mean (5)	Franchise Treatment (6)	Grant Treatment (7)	p-value: F = G (8)
Received IGA-relevant business or skills training	0.062	0.162***	0.063*	0.009	0.256	0.337***	0.103	0.014
		(0.028)	(0.032)			(0.072)	(0.097)	
Amount invested to start business (in USD)	5.877	1.650	13.273***	0.003	24.223	-10.296	20.977*	0.001
		(2.323)	(3.842)			(7.911)	(10.913)	
Used bank or MFI loan to start business	0.000	0.000	0.000	.	0.000	0.000	0.000	.
		(.)	(.)			(.)	(.)	
Used funding from NGO to start business	0.000	0.013*	0.070***	0.008	0.000	0.041	0.189***	0.006
		(0.007)	(0.020)			(0.025)	(0.053)	
Only used own savings to start business	0.083	0.023	-0.003	0.349	0.341	-0.106	-0.173**	0.377
		(0.023)	(0.027)			(0.074)	(0.082)	
Is co-owner of a business	0.038	0.021	0.045*	0.362	0.159	-0.011	0.055	0.303
		(0.017)	(0.023)			(0.059)	(0.064)	
Employs others	0.015	0.030**	0.058***	0.222	0.061	0.042	0.133**	0.133
		(0.014)	(0.022)			(0.052)	(0.061)	
Keeps IGA accounts separate from personal funds	0.101	0.107***	0.090**	0.654	0.415	0.116	0.044	0.378
		(0.028)	(0.035)			(0.083)	(0.093)	
Works in a concrete building	0.346	-0.043	-0.042	0.997	0.427	-0.166*	-0.142	0.823
		(0.043)	(0.051)			(0.100)	(0.117)	

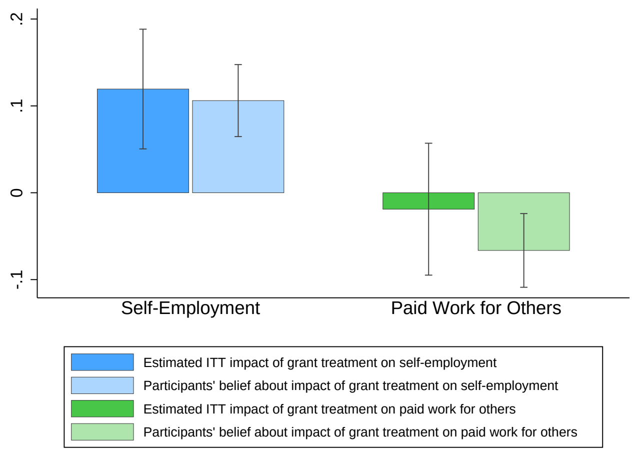
Robust standard errors in parentheses. *, **, and *** indicate significance at the 90, 95, and 99 percent confidence levels, respectively. OLS regressions reported. All specifications include controls for baseline household size, education level, and indicators for having given birth, having received any vocational training, or having any paid work experience prior to the baseline survey, in addition to survey enumerator and survey month fixed effects. Money amounts are deflated to July 2013 levels using CPI data from the Kenya National Bureau of Statistics, then converted to US dollars using the average exchange rate from July 2013 (84.04 Kenyan shillings to the dollar). The top 1 percent of values of all hours and income variables are trimmed.

Figure 4.1: Participants' Beliefs about Impacts of Treatments

Panel A: Beliefs about Impact of Franchise Treatment



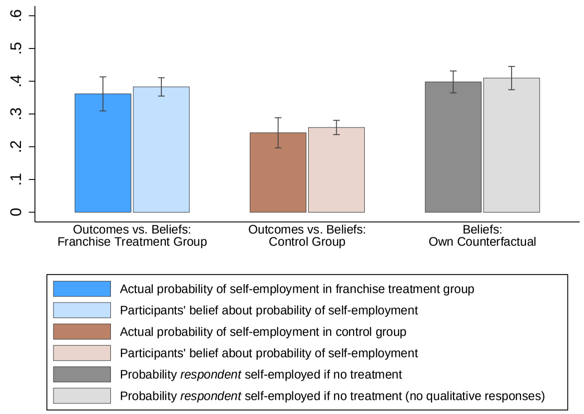
Panel B: Beliefs about Impact of Grant Treatment



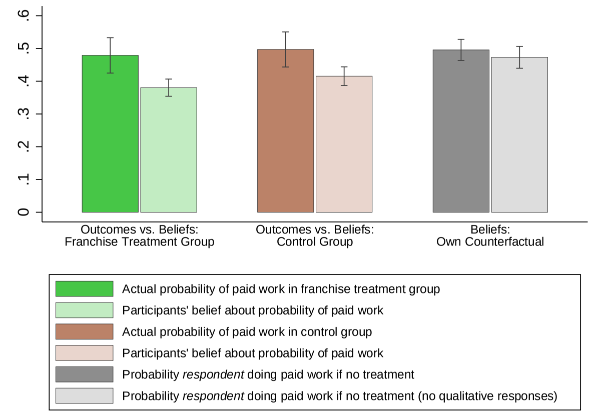
ITT estimates of treatment are estimated via OLS, controlling for stratum fixed effects (we omit other controls included in our main specifications to make ITT estimates as comparable to self-reported beliefs as possible, though these controls have minimal impacts on estimated coefficients). Beliefs are estimated using estimates of the frequency of outcomes in a reference class of young women similar to oneself. For example, the estimate of the impact of the franchise treatment on the probability of self-employment is constructed using average responses to two questions: (1) "I would like you to imagine 100 women from [your neighborhood] who applied to the [name of treatment arm] program and were admitted into it, just as you were. In other words, please think about 100 women similar to yourself. Out of 100 women, how many do you think are currently running or operating their own business?" and (2) "Now I would like you to imagine 100 women from [your neighborhood] who applied to the [name of treatment arm] program and but who were not admitted into it. In other words, please think about 100 women similar to yourself who were not selected to the [name of treatment arm] program. Out of 100 women, how many do you think are currently running or operating their own business?" The difference in responses to these two questions (divided by 100) is the individual-level estimate of the average treatment effect of the program on self-employment.

Figure 4.2: Participants' Beliefs about Impacts of Treatments

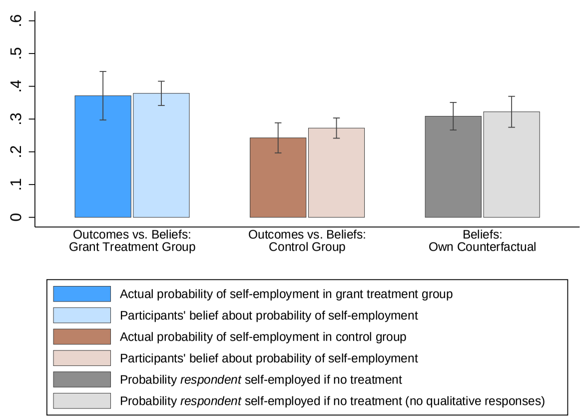
Panel A: Franchise Treatment Group:
Beliefs about Self-Employment



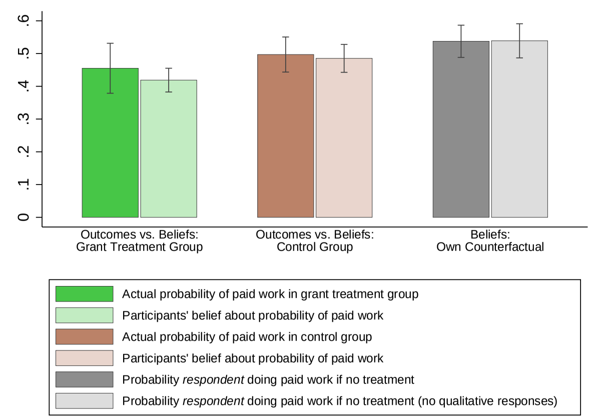
Panel B: Franchise Treatment Group:
Beliefs about Paid Work for Others



Panel C: Grant Treatment Group:
Beliefs about Self-Employment



Panel D: Grant Treatment Group:
Beliefs about Paid Work for Others



The figure compares observed levels of self-employment and paid work in the treatment groups and the control group to beliefs about levels held by women assigned to the franchise and grant treatment arms. See Figure 4.1 for a description of the belief elicitation questions. The probability that a respondent would be doing paid work or in self-employment in the absence of treatment is the average response to a question about the counterfactual likelihood of involvement in the labor market.

Appendix

4.A Proof and extension of Proposition 1

4.1 Proof of Part (1)

To show Part (1) of Proposition 1, that for all $K \leq K_{min}$, $\mathbb{F}^*(K_t) = f^w(1)$, it is straightforward to proceed by contradiction. If any labor is allocated to the enterprise sector, the resulting production in the enterprise sector is zero, following Assumption A1. Thus, for any choice of the quantity of wage labor, L^w, the total output, $\mathbb{F}(K_t, L^w)$, is equal to $f^w(L^w)$. Because this is an increasing function of L^w by Assumption A9, $\mathbb{F}(K_t, L^w) < F(K_t, 1)\ \forall L^w < 1$. Therefore, for all $K \leq K_{min}$, L^w cannot be less than 1. Thus, for all $K \leq K_{min}$, no labor is allocated to the enterprise sector, all labor is allocated to the wage sector, and $\mathbb{F}^*(K_t) = f^w(1)$. In other words, $\mathbb{F}^*(K_t)$ is flat for $K \leq K_{min}$. $\square$

4.2 Proof of Part (2)

To show Part (2) of Proposition 1, that for $K_t \geq K_{min}$, the function $\mathbb{F}^*(K_t)$ has a positive first derivative, we reason as follows. Consider $K_t \geq K_{min}$, and $K_t' \geq K_t$. Recall that $\mathbb{F}^*(K_t)$ maximizes, over L^w, the value of $\mathbb{F}(K_t, L^w) = f^w(L^w) + f^e(K_t, 1 - L^w)$. Because $K_t \geq K_{min}$, and $K_t' \geq K_t$, we apply Assumption A2 ($f_k^e > 0$) implies that $f^e(K_t', 1 - L^w) > f^e(K_t, 1 - L^w)$. Thus, $\mathbb{F}(K_t', L^w) > \mathbb{F}(K_t, L^w)$. Because $\mathbb{F}^*(K_t)$ maximizes, over L^w, the value of $\mathbb{F}(K_t, L^w)$, it must be the case that $\mathbb{F}^*(K_t)$ is weakly greater than $\mathbb{F}(K_t', L^w)$ (which is achieved without adjusting the allocation of labor between activities). Thus, $\mathbb{F}(K_t', L^w) > \mathbb{F}(K_t, L^w)$, so $\mathbb{F}^*(K_t)$ has a positive first derivative.[34] $\square$

4.3 Proof of Part (3)

To show Part (3) of Proposition 1, that the function, $\mathbb{F}^*(K_t)$ has a negative second derivative for $K_t \geq K_{min}$, it is useful to provide first a lemma, then a diagram.

Lemma 1. *The derivative of $\mathbb{F}^*(K_t)$ is equal to the partial derivative of $f^e(K_t, L^e)$ with respect to capital at the optimum value of L^e.*

[34] Because of Assumption A5, the optimal allocation of labor across sectors is an interior solution in L^e for all $K_t > K_{min}$. This remains true for arbitrarily large K_t because of Assumption A11. Intuitively, the interior nature of the solution follows immediately from the fact that the marginal product of labor approaches infinity as labor goes to zero in either sector. If the marginal product of labor in the wage labor sector were constant, the optimal allocation of labor across sectors could involve no wage labor at some values of $K_t > K_{min}$.

Proof. Though this can be shown as a direct applicaton of the envelope theorem, it can also be argued succinctly as follows:

$$\mathbb{F}^*(K_t) = f^w(L^w) + f^e(K_t, L^e)$$
$$\frac{d}{dK_t}\mathbb{F}^*(K_t) = \frac{df^w}{dL^w}\frac{dL^w}{dK_t} + \frac{\delta f^e}{\delta K_t} + \frac{\delta f^e}{\delta L^e}\frac{dL^e}{dK_t}$$

But because $\frac{df^w}{dL^w} = \frac{df^e}{dL^e}$ (marginal products are equated) at the optimum, and because $\frac{dL^w}{dK_t} = -\frac{dL^e}{dK_t}$ at the constraint (since $L^e + L^w = 1$, so any movement in one is accompanied by an opposite movement in the other), this becomes:

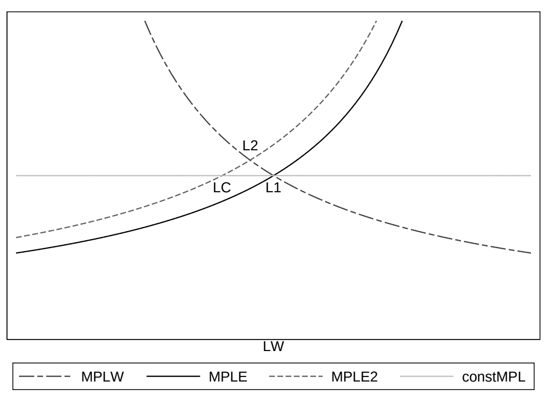
$$\frac{d}{dK_t}\mathbb{F}^*(K_t) = -\frac{df^e}{dL^e}\frac{dL^e}{dK_t} + \frac{\delta f^e}{\delta K_t} + \frac{\delta f^e}{\delta L^e}\frac{dL^e}{dK_t}$$

The first and last terms cancel, proving that:

$$\frac{d}{dK_t}\mathbb{F}^*(K_t) = \frac{\delta f^e}{\delta K_t}$$

□

Next, we provide a diagram for reference. The graph below shows (on the vertical axis) the marginal products of labor in the two sectors, as labor (L^W, on the horizontal axis) shifts between them. At the left side of the diagram, $L^W = 0$; at the right, $L^W = 1$.



The two curves showing the marginal product of labor in each sector, MPL^w and MPL^e, are given their shapes by Assumptions A3 and A10. Where the marginal products are equated, the curves cross at the optimal allocation of labor, $L1$.

With reference to this diagram, Part (3) of Proposition 1 concerns changes in K. If K increases, the change in the allocation of labor depends on the shape of f^w. By Assumption A4, that inputs are complements, an increase in K increases the entire MPL^e curve so that it becomes MPL^e_2, shown in dashes. As a benchmark, we now consider if, instead of a diminishing returns f^w, we instead had a constant returns $f^w = bL$ for some constant b. The graph shows how it too could intersect MPL^e at the point $L1$. If the wage sector were constant returns, then when the MPL^e curve shifts to become MPL^e_2, the new optimal allocation would be at LC, and the optimal marginal product of labor would remain unchanged. But with the actual f^w, the new optimum is at $L2$, which is a smaller shift in labor allocation: it must be the case that $LC < L2 < L1$. But because

f^e is homogeneous of degree one above the minimum scale, we know that the marginal products of capital and labor in the enterprise sector are functions only of the capital-labor ratio. Thus, if the marginal product of labor is higher at the new equilibrium, it is because labor did not adjust enough to preserve the capital-labor ratio. The new marginal product of capital in the enterprise sector is thus lower at $L2$ than at $L1$: the value of $\frac{\delta f^e}{\delta K_t}$ is declining in K_t at the optimum. Application of Lemma 1 now implies that $\frac{d}{dK_t}\mathbb{F}^*(K_t)$ is also declining in K_t, so $\mathbb{F}^*(K_t)$ has a negative second derivative. □

4.4 Extension of Proposition 1: constant wage

If, instead of Assumption A10 (that $\frac{\delta^2}{\delta L^2} f^w(L^w) < 0$), the wage labor sector is characterized by a constant wage $\frac{\delta^2}{\delta L^2} f^w(L^w) = 0$, then Part 3 of Proposition 1 changes to: "For all $K_t > K_{min}$, $\mathbb{F}^*(K_t)$ has a weakly negative second derivative. Specifically, there exists $\bar{K}$ such that for all K_t above K_{min} but below $\bar{K}$, $\mathbb{F}^*(K_t)$ has a second derivative equal to zero; and for all K_t above $\bar{k}$, $\mathbb{F}^*(K_t)$ has a negative second derivative."

The proof is straightforward. By Assumtion A5, above K_{min}, the marginal product of labor in the enterprise sector crosses the fixed wage in the enterprise sector at some point ($L1$ in the diagram above) where a nonzero fraction of labor is allocate to the enterprise sector ($L^W > 0$). Any increase in capital shifts the MPL^e curve upward, moving the optimum allocation to LC in the diagram above. Because the marginal product of labor did not change, and because f^e is homogeneous of degree one above the minimum scale, the capital-labor ratio did not change. Thus, as long as the MPL^e curve intersects the fixed wage line, for every increase in capital, there is an exactly proportionate shift in labor from the wage to the enterprise sector. Because f^e is homogeneous of degree one, this produces a proportionate shift in output. The change in output at the optimum is thus linear in capital, as long as the MPL^e curve intersects the fixed wage line. At some level of capital, $\bar{K}$, the MPL^e curve rises entirely above the fixed wage line in the diagram above. After this point, the optimum allocation of labor is a corner solution setting $L^W = 0$. At this point, the shape of $\mathbb{F}^*(K_t)$ is necessarily the shape of f^e, which by diminishing returns (Assumption A2) means it has a negative second derivative. This slight variation on the characteristic shape of $\mathbb{F}^*(K_t)$ yields the same possible numbers of crossings as before, so the definitions of latent entrepreneurial types that are used in the paper still hold. □

4.B Additional Tables and Figures

Table 4.B.1: Baseline Covariates, by Treatment Status

	Control	Franchise Treatment	Grant Treatment	Differences		
	(1)	(2)	(3)	**F – C**	**G – C**	**G – F**
Panel A. Demographics and Household Composition						
Age	18.758	18.803	18.780	0.044	0.023	−0.021
	[0.802]	[0.748]	[0.832]	(0.055)	(0.069)	(0.068)
At least one parent alive	0.890	0.878	0.884	−0.013	−0.005	0.007
	[0.314]	[0.328]	[0.321]	(0.024)	(0.029)	(0.029)
Household size	5.127	4.700	4.753	−0.421***	−0.375*	0.047
	[2.258]	[1.986]	[2.291]	(0.154)	(0.203)	(0.197)
Married or cohabitating	0.149	0.189	0.148	0.039	0.000	−0.039
	[0.356]	[0.392]	[0.356]	(0.027)	(0.031)	(0.032)
Has given birth	0.364	0.439	0.440	0.076**	0.077*	0.002
	[0.482]	[0.497]	[0.498]	(0.036)	(0.044)	(0.044)
Panel B. Educational Background						
Father's education, if known	9.596	9.761	10.142	0.158	0.519	0.361
	[3.245]	[2.820]	[2.767]	(0.290)	(0.341)	(0.321)
Mother's education, if known	8.955	9.137	9.007	0.162	0.047	−0.115
	[2.949]	[2.798]	[2.847]	(0.239)	(0.285)	(0.285)
Years of education	10.033	9.914	9.577	−0.122	−0.459**	−0.337
	[1.998]	[2.015]	[2.213]	(0.147)	(0.191)	(0.191)
Any vocational training	0.369	0.319	0.346	−0.050	−0.023	0.027
	[0.483]	[0.467]	[0.477]	(0.035)	(0.042)	(0.042)
Panel C. Involvement in Income-Generating Activities (IGAs)						
Any (paid) work experience	0.537	0.544	0.566	0.007	0.029	0.022
	[0.499]	[0.499]	[0.497]	(0.037)	(0.045)	(0.045)
Engaged in any IGAs	0.124	0.167	0.148	0.042	0.024	−0.018
	[0.330]	[0.373]	[0.356]	(0.026)	(0.031)	(0.033)
Any self-employment activity	0.041	0.064	0.049	0.022	0.008	−0.014
	[0.213]	[0.256]	[0.217]	(0.018)	(0.020)	(0.021)
Any paid work for someone else	0.085	0.111	0.104	0.025	0.019	−0.007
	[0.280]	[0.315]	[0.324]	(0.022)	(0.028)	(0.029)
Hours of housework in last week	25.881	26.192	26.215	0.305	0.329	0.025
	[15.716]	[15.545]	[13.961]	(1.184)	(1.337)	(1.330)
Panel D. Assets, Savings, and Living Conditions						
Food insecurity index	0.257	0.265	0.254	0.009	−0.003	−0.012
	[0.185]	[0.173]	[0.162]	(0.013)	(0.015)	(0.015)
Has a personal bank account	0.088	0.092	0.078	0.003	−0.010	−0.013
	[0.284]	[0.289]	[0.269]	(0.021)	(0.025)	(0.025)
Has any savings	0.339	0.336	0.298	−0.002	−0.039	−0.037
	[0.474]	[0.473]	[0.459]	(0.035)	(0.042)	(0.042)
Value of savings (in USD)	4.688	5.225	4.872	0.543	0.153	−0.390
	[14.250]	[15.334]	[14.744]	(1.103)	(1.321)	(1.358)
Value of savings, if any (in USD)	17.364	17.913	19.706	0.395	2.875	2.481
	[23.138]	[24.129]	[24.400]	(3.571)	(4.227)	(4.336)
Owns a personal mobile phone	0.741	0.731	0.725	−0.013	−0.015	−0.002
	[0.439]	[0.444]	[0.448]	(0.031)	(0.038)	(0.038)
Household has electricty	0.749	0.758	0.736	0.009	−0.013	−0.021
	[0.434]	[0.429]	[0.442]	(0.032)	(0.040)	(0.039)
Household has piped water	0.490	0.494	0.478	0.003	−0.013	−0.017
	[0.501]	[0.501]	[0.501]	(0.033)	(0.041)	(0.041)
Household owns a television	0.567	0.575	0.555	0.007	−0.014	−0.021

Continued on next page

Table 4.B.1 – *Continued from previous page*

	Control	Franchise Treatment	Grant Treatment	Differences		
	(1)	(2)	(3)	**F – C**	**G – C**	**G – F**
	[0.496]	[0.495]	[0.498]	(0.036)	(0.044)	(0.045)
Household owns a radio	0.716	0.664	0.665	−0.053	−0.054	−0.001
	[0.451]	[0.473]	[0.473]	(0.034)	(0.042)	(0.043)
Household asset index	-0.003	0.013	-0.020	0.012	−0.021	−0.034
	[0.971]	[1.024]	[1.013]	(0.073)	(0.089)	(0.091)
Observations	363	360	182			

Standard deviation in brackets, robust standard errors in parentheses. Columns 4 through 6 report differences in means across treatments, with significance levels estimated controlling for strata fixed effects (as in our main specifications). *, **, and *** indicate significance at the 90, 95, and 99 percent confidence levels, respectively. Asset PCA is the first principal component of the set of indicators for: whether the household has electricity, whether the household has piped water, and whether anyone in the household owns or has a television, a refrigerator, a stove, a computer, a DVD player, a motorcycle, a bicycle, or a lamp.

Table 4.B.2: Attrition from the Sample

	OLS	**OLS**	**OLS**
	(1)	(2)	(3)
Franchise treatment	0.009	0.017	-0.379
	(0.02)	(0.028)	(0.73)
Grant treatment	0.014	0.018	-0.662
	(0.024)	(0.034)	(0.857)
Age	.	0.019	0.003
		(0.016)	(0.026)
At least one parent alive	.	0.057	0.076
		(0.046)	(0.086)
Household size	.	-0.01	-0.004
		(0.006)	(0.01)
Married or cohabitating	.	-0.019	-0.004
		(0.043)	(0.07)
Has given birth	.	-0.003	0.023
		(0.033)	(0.058)
Father's education, if known	.	-0.009*	-0.0007
		(0.005)	(0.008)
Mother's education, if known	.	0.011**	0.008
		(0.005)	(0.009)
Years of education	.	-0.003	-0.005
		(0.008)	(0.014)
Any vocational training	.	0.033	0.04
		(0.028)	(0.043)
Any (paid) work experience	.	-0.015	0.008
		(0.028)	(0.044)
Engaged in any IGAs	.	-0.065	0.026
		(0.042)	(0.073)
Hours of housework in last week	.	0.00007	0.00003
		(0.0008)	(0.001)
Food insecurity index	.	-0.017	-0.142
		(0.081)	(0.125)
Has a personal bank account	.	0.01	0.032
		(0.046)	(0.074)
Has any savings	.	0.023	0.044
		(0.028)	(0.045)
Household asset index	.	-0.013	-0.029
		(0.015)	(0.024)
Age x franchise treatment	.	.	0.027

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Table 4.B.2 – *Continued from previous page*

	OLS (1)	**OLS** (2)	**OLS** (3)
			(0.039)
At least one parent alive x franchise treatment	.	.	0.003
			(0.113)
Household size x franchise treatment	.	.	-0.0006
			(0.015)
Married or cohabitating x franchise treatment	.	.	-0.037
			(0.098)
Has given birth x franchise treatment	.	.	-0.041
			(0.079)
Father's education, if known x franchise treatment	.	.	-0.024**
			(0.011)
Mother's education, if known x franchise treatment	.	.	0.002
			(0.013)
Years of education x franchise treatment	.	.	0.011
			(0.019)
Any vocational training x franchise treatment	.	.	0.034
			(0.064)
Any (paid) work experience x franchise treatment	.	.	-0.052
			(0.064)
Engaged in any IGAs x franchise treatment	.	.	-0.129
			(0.099)
Hours of housework in last week x franchise treatment	.	.	-0.001
			(0.002)
Food insecurity index x franchise treatment	.	.	0.381**
			(0.186)
Has a personal bank account x franchise treatment	.	.	0.007
			(0.104)
Has any savings x franchise treatment	.	.	-0.056
			(0.063)
Household asset index x franchise treatment	.	.	0.044
			(0.035)
Age x grant treatment	.	.	0.046
			(0.044)
At least one parent alive x grant treatment	.	.	-0.058
			(0.128)
Household size x grant treatment	.	.	-0.025
			(0.017)
Married or cohabitating x grant treatment	.	.	0.038
			(0.122)
Has given birth x grant treatment	.	.	-0.032
			(0.093)
Father's education, if known x grant treatment	.	.	0.012
			(0.015)
Mother's education, if known x grant treatment	.	.	-0.008
			(0.016)
Years of education x grant treatment	.	.	-0.009
			(0.022)
Any vocational training x grant treatment	.	.	-0.086
			(0.079)
Any (paid) work experience x grant treatment	.	.	0.008
			(0.077)
Engaged in any IGAs x grant treatment	.	.	-0.119
			(0.124)
Hours of housework in last week x grant treatment	.	.	0.003
			(0.002)
Food insecurity index x grant treatment	.	.	-0.0003
			(0.231)
Has a personal bank account x grant treatment	.	.	-0.113
			(0.134)
Has any savings x grant treatment	.	.	0.011
			(0.078)
Household asset index x grant treatment	.	.	0.032
			(0.041)
Constant	0.069***	-0.271	-0.05

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Table 4.B.2 – *Continued from previous page*

	OLS	OLS	OLS
	(1)	(2)	(3)
	(0.014)	(0.314)	(0.489)
Observations	905	499	499
R^2	0.0004	0.036	0.097
F-Test: observables (p-value)		0.371	0.959
F-Test: treatment-observables interactions (p-value)			0.545

Robust standard errors in parentheses. *, **, and *** indicate significance at the 90, 95, and 99 percent confidence levels, respectively. OLS regressions reported. The dependent variable in all specifications is an indicator for attrition from the sample (between baseline and endline). The last two rows of the table report p-values from associated F-tests of whether the observable characteristics and observable characteristics interacted with treatment are jointly significant in the attrition regressions in columns 2 and 3.

Table 4.B.5: Intent to Treat Estimates: Occupational Sector and Other Outcomes after 14–22 Months

			Treatment Effects		
	Obs.	**Control Mean**	**Franchise Treatment**	**Grant Treatment**	**p-value: F = G**
	(1)	**(2)**	**(3)**	**(4)**	**(5)**
Panel A. Occupational Sectors					
Domestic services	837	0.198	-0.021	0.014	0.374
			(0.031)	(0.039)	
Salon and beauty	837	0.166	0.114***	-0.002	0.002
			(0.031)	(0.035)	
Retail and hawking	837	0.121	0.009	0.048	0.274
			(0.026)	(0.034)	
Food service and catering	837	0.056	0.022	-0.013	0.116
			(0.020)	(0.021)	
Sells prepared food or cooked snacks	837	0.053	0.013	-0.000	0.560
			(0.019)	(0.022)	
White collar or professional	837	0.047	-0.022	-0.005	0.272
			(0.014)	(0.019)	
Janitorial work and trash collection	837	0.041	-0.008	-0.003	0.741
			(0.015)	(0.018)	
Sells uncooked fruits and vegetables	837	0.027	0.013	0.016	0.894
			(0.014)	(0.018)	
Works in light industry (factory work)	837	0.024	0.002	0.010	0.690
			(0.013)	(0.017)	
Wholesale and distribution	837	0.024	-0.003	-0.005	0.853
			(0.011)	(0.014)	
Tailoring, sewing, and arts and crafts	837	0.021	0.001	0.026	0.177
			(0.011)	(0.018)	
Entertainment or professional sport	837	0.012	-0.003	-0.013*	0.120
			(0.009)	(0.007)	
Construction, security, and manual labor	837	0.009	-0.003	0.001	0.548
			(0.005)	(0.007)	
Farming or agricultural labor	837	0.000	0.012*	0.018*	0.624
			(0.006)	(0.010)	
Sex worker	837	0.000	0.002	-0.000	0.329
			(0.002)	(0.001)	
Panel B. Labor Market Churning					
Closed a business between midline and endline	812	0.183	0.010	0.024	0.715

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Table 4.B.5 – *Continued from previous page*

	Obs. (1)	Control Mean (2)	Treatment Effects: Franchise Treatment (3)	Treatment Effects: Grant Treatment (4)	p-value: F = G (5)
			(0.032)	(0.040)	
Left a paid job between midline and endline	812	0.241	-0.051	-0.056	0.894
			(0.033)	(0.040)	
Started a business between midline and endline	812	0.177	0.039	0.047	0.850
			(0.031)	(0.037)	
Started a new paid job between midline and endline	812	0.387	-0.055	-0.058	0.941
			(0.039)	(0.047)	
Panel C. Occupational Sector					
Years of education	837	10.198	-0.032	-0.083	0.605
			(0.092)	(0.092)	
Curently enrolled in school	837	0.101	-0.014	-0.016	0.934
			(0.022)	(0.026)	
Has done any vocational training	837	0.568	0.292***	0.035	0.000
			(0.033)	(0.045)	
Has done business skills training	837	0.098	0.149***	0.001	0.000
			(0.028)	(0.029)	
Business skills score (scaled 0 to 5)	837	1.036	0.129	-0.103	0.037
			(0.095)	(0.109)	
Has done salon skills training	837	0.213	0.289***	0.003	0.000
			(0.034)	(0.039)	
Salon skills score (scaled 0 to 9)	837	4.580	0.136	-0.485***	0.000
			(0.128)	(0.159)	
Has done tailoring training	837	0.062	0.003	0.018	0.564
			(0.019)	(0.026)	
Tailoring skills score (scaled 0 to 8)	837	1.325	-0.021	0.035	0.610
			(0.092)	(0.112)	
Has done computer training	837	0.237	-0.069**	0.003	0.032
			(0.027)	(0.034)	
Seconds required to complete typing test	835	100.935	5.298	13.055**	0.145
			(4.385)	(5.285)	
Panel D. Household Composition and Living Arrangements					
Household size	837	4.716	-0.082	0.133	0.289
			(0.169)	(0.205)	
Married or cohabitating	837	0.269	0.012	-0.040	0.208
			(0.034)	(0.041)	
Had an additional child (after program)	837	0.145	0.061**	0.045	0.663
			(0.030)	(0.037)	
Lives with own child	837	0.453	0.071**	0.091**	0.624
			(0.032)	(0.040)	
Lives in Nairobi	837	0.891	-0.023	-0.049	0.416
			(0.024)	(0.031)	
Panel E. Household Assets and Living Conditions					
Household has electricity	837	0.849	-0.023	-0.041	0.645
			(0.028)	(0.037)	
Household has piped water	837	0.470	0.039	0.068	0.488
			(0.035)	(0.043)	
Household has flush toilet	837	0.388	0.056	0.076*	0.618
			(0.034)	(0.041)	
Household owns a TV	837	0.598	-0.042	-0.020	0.631
			(0.039)	(0.046)	
Household owns computer	837	0.080	-0.051***	-0.040**	0.486
			(0.017)	(0.019)	
Owns a personal mobile phone	837	0.891	-0.011	-0.062*	0.134
			(0.025)	(0.034)	

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Table 4.B.5 – *Continued from previous page*

	Obs. (1)	Control Mean (2)	Treatment Effects: Franchise Treatment (3)	Treatment Effects: Grant Treatment (4)	p-value: F = G (5)
Owns a personal SIM card	837	0.950	-0.011 (0.019)	-0.010 (0.023)	0.984
Panel F. Consumption, Expenditures, and Savings					
Food Insecurity Access Scale (out of 27)	837	9.571	-0.224 (0.512)	0.903 (0.621)	0.072
Women's Dietary Diversity Score (scaled 0 to 9)	805	4.745	0.156 (0.122)	0.049 (0.145)	0.484
Expenditures on self and children (in USD)	837	7.837	0.050 (0.735)	-0.528 (0.845)	0.477
Log of expenditures on self and children (in USD)	837	1.266	0.172 (0.114)	0.045 (0.143)	0.365
Spent money on tea, soda, or sweets in past week	837	0.663	0.020 (0.036)	0.017 (0.043)	0.956
Spent money on alcohol in past week	837	0.059	-0.036** (0.016)	-0.025 (0.019)	0.514
Transfers (in USD)	837	2.725	-0.006 (0.367)	0.286 (0.470)	0.524
Log of transfers (in USD)	837	-0.859	0.216 (0.200)	0.168 (0.251)	0.849
Savings (in USD)	837	49.211	-9.496 (7.967)	2.118 (9.783)	0.199
Change in savings relative to last year	837	-0.139	-0.169** (0.072)	-0.156* (0.084)	0.875
Paid school fees for self or own child in 2014	837	0.107	-0.003 (0.024)	0.019 (0.030)	0.469
Paid school fees for someone else's child in 2014	837	0.071	0.009 (0.021)	0.124*** (0.034)	0.001
Panel G. Time Use on Week Day Prior to Survey					
Hours of income-generating activities	837	2.746	0.283 (0.356)	0.498 (0.420)	0.623
Self-employment hours	837	0.364	0.287* (0.163)	0.431** (0.201)	0.534
Hours of paid work for others	837	2.382	-0.004 (0.341)	0.067 (0.398)	0.862
Hours of unpaid household work	837	5.544	0.142 (0.258)	-0.055 (0.299)	0.512
Hours of unpaid work in a business	837	0.393	-0.085 (0.121)	0.023 (0.141)	0.421
Hours of job search	837	0.086	-0.007 (0.055)	0.026 (0.074)	0.670
Hours commuting or in transit	837	0.166	-0.062 (0.050)	0.265* (0.155)	0.029
Hours of leisure	837	10.260	-0.431 (0.276)	-0.722** (0.314)	0.362
Hours of education or training	837	0.595	0.071 (0.167)	-0.037 (0.181)	0.571
Hours of religious observance, visiting the sick	837	0.154	0.089 (0.096)	0.022 (0.092)	0.481
Panel H. Indices Capturing Empowerment, Self-Esteem, etc.					
Rosenberg self-esteem scale (0 to 30)	837	19.130	0.363 (0.310)	-0.348 (0.370)	0.056
Ladder of Life wellbeing scale (scaled from 0 to 10)	837	6.491	0.139 (0.110)	-0.000 (0.126)	0.280
Grit (scaled from 1 to 5)	837	2.006	-0.004	0.006	0.737

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Table 4.B.5 – *Continued from previous page*

	Obs. (1)	Control Mean (2)	Treatment Effects: Franchise Treatment (3)	Treatment Effects: Grant Treatment (4)	p-value: F = G (5)
			(0.022)	(0.029)	
Panel I. Empowerment Measures Used in Bandiera et al (2015)					
Gender Empowerment Index (scaled 0 to 100)	837	48.352	-0.740	2.171	0.171
			(1.749)	(2.096)	
Business Confidence Index (scaled 0 to 100)	837	71.915	0.589	-2.267*	0.015
			(0.942)	(1.175)	
Suitable age for a woman to marry	837	24.828	-0.357*	-0.205	0.530
			(0.207)	(0.230)	
Suitable age for a man to marry	837	28.281	-0.328	0.085	0.182
			(0.263)	(0.281)	
Desired age of marriage for daughter	788	26.101	-0.226	-0.300	0.759
			(0.206)	(0.238)	
Desired age of marriage for son	813	28.856	-0.283	0.073	0.209
			(0.242)	(0.280)	
Suitable age for a woman to have a child	837	24.891	-0.294	-0.161	0.672
			(0.251)	(0.291)	
Number of children desired	837	2.757	0.039	0.057	0.824
			(0.069)	(0.084)	
Number of boys desired	835	1.494	-0.061	0.044	0.086
			(0.054)	(0.065)	
Desired proportion of male children	835	0.537	-0.030**	0.021	0.003
			(0.014)	(0.018)	
Panel J. Empowerment Measures Used in Adoho et al (2014)					
Self Confidence Index (scaled from 1 to 6)	837	4.257	0.066	-0.094	0.133
			(0.087)	(0.106)	
Respondent has her own money	837	0.805	0.081***	0.066*	0.660
			(0.028)	(0.036)	
Controls money she earns from IGAs	644	0.956	0.012	0.013	0.963
			(0.017)	(0.022)	
Needs permission to spend earnings	837	0.050	0.009	0.006	0.912
			(0.018)	(0.022)	

Robust standard errors in parentheses. *, **, and *** indicate significance at the 90, 95, and 99 percent confidence levels, respectively. OLS regressions reported. All specifications include controls for baseline household size, education level, and indicators for having given birth, having received any vocational training, or having any paid work experience prior to the baseline survey, in addition to survey enumerator and survey month fixed effects. Money amounts are deflated to July 2013 levels using CPI data from the Kenya National Bureau of Statistics, then converted to US dollars using the average exchange rate from July 2013 (84.04 Kenyan shillings to the dollar). The top 1 percent of values of all hours and income variables are trimmed. The estimated impacts of the franchise treatment on the likelihood of working in the salon sector (see Panel A) or having done any vocational training (see Panel C) are significant at the 99 percent level after implementing the multiple hypothesis testing correction proposed by Benjamini and Hochberg (1995). Those assigned to the grant treatment are also more likely to have paid school fees for someone else's child in the year after receiving the grant (Benjamini-Hochberg q-value 0.01; see Panel F). No other outcomes are significantly related to either treatment with adjusted q-values below 0.05.

Table 4.B.3: Treatment on the Treated: Labor Market Outcomes after 7–10 Months

	Obs. (1)	Control Mean (2)	TOT Estimates: Started Franchise Program (3)	TOT Estimates: Received Grant (4)	p-value: F = G (5)
Panel A. Involvement in Income-Generating Activities (Previous Month)					
Engaged in any income-generating activities	851	0.586	0.032	0.025	0.908
			(0.061)	(0.047)	
Any self-employment activity	851	0.245	0.163***	0.105**	0.307
			(0.055)	(0.044)	
Paid work for someone else	851	0.382	-0.114*	-0.073	0.473
			(0.059)	(0.046)	
Panel B. Labor Supply (Previous 7 Days)					
Hours worked in last week	851	17.945	1.853	7.105**	0.140
			(3.442)	(2.953)	
Self-employment hours	851	4.723	6.856***	7.938***	0.680
			(2.180)	(2.047)	
Hours of paid work for someone else	851	13.017	-4.756	-0.903	0.167
			(2.893)	(2.383)	
Panel C. Income Excluding Transfers (Previous 7 Days)					
Reports any labor income	851	0.466	0.093	0.062	0.607
			(0.061)	(0.048)	
Income excluding transfers (in USD)	851	5.476	2.720**	3.278***	0.699
			(1.250)	(1.198)	
Log income (in USD)	851	-1.436	0.842**	0.582*	0.525
			(0.407)	(0.323)	
Self-employment income (in USD)	851	2.617	2.167**	2.397**	0.843
			(0.988)	(1.018)	
Log of self-employment income (in USD)	851	-3.158	1.049***	0.733***	0.382
			(0.345)	(0.282)	
Income from paid work for someone else (in USD)	851	2.901	0.154	0.508	0.675
			(0.776)	(0.661)	
Log of income from paid work (in USD)	851	-2.595	-0.143	-0.066	0.825
			(0.359)	(0.278)	
Panel D. First-Stage F-Statistics on Excluded Instruments					
Franchise treatment			277.723		
Grant treatment			2440.035		

Robust standard errors in parentheses. *, **, and *** indicate significance at the 90, 95, and 99 percent confidence levels, respectively. OLS regressions reported. All specifications include controls for baseline household size, education level, and indicators for having given birth, having received any vocational training, or having any paid work experience prior to the baseline survey, in addition to survey enumerator and survey month fixed effects. Money amounts are deflated to July 2013 levels using CPI data from the Kenya National Bureau of Statistics, then converted to US dollars using the average exchange rate from July 2013 (84.04 Kenyan shillings to the dollar). The top 1 percent of values of all hours and income variables are trimmed.

Table 4.B.4: Intent to Treat Estimates: Occupational Sectors after 7–10 Months

			Treatment Effects		
	Obs.	Control Mean	Franchise Treatment	Grant Treatment	p-value: F = G
	(1)	(2)	(3)	(4)	(5)
Panel A. Occupational Sectors					
Domestic services	851	0.143	-0.045*	-0.049	0.904
			(0.025)	(0.030)	
Salon and beauty	851	0.146	0.088***	-0.073**	0.000
			(0.029)	(0.030)	
Retail and hawking	851	0.090	0.043*	0.112***	0.055
			(0.024)	(0.035)	
Food service and catering	851	0.026	-0.007	0.006	0.434
			(0.013)	(0.016)	
Sells prepared food or cooked snacks	851	0.061	0.037*	0.034	0.916
			(0.020)	(0.025)	
White collar or professional	851	0.020	0.002	0.016	0.373
			(0.011)	(0.016)	
Janitorial work and trash collection	851	0.026	-0.020*	-0.028***	0.196
			(0.011)	(0.009)	
Sells uncooked fruits and vegetables	851	0.038	-0.021	0.031	0.010
			(0.014)	(0.022)	
Works in light industry (factory work)	851	0.035	-0.009	0.005	0.409
			(0.014)	(0.019)	
Wholesale and distribution	851	0.006	0.018*	0.010	0.450
			(0.009)	(0.009)	
Tailoring, sewing, and arts and crafts	851	0.015	-0.000	-0.005	0.667
			(0.010)	(0.011)	
Entertainment or professional sport	851	0.017	-0.015**	-0.009	0.353
			(0.007)	(0.010)	
Construction, security, and manual labor	851	0.009	-0.005	0.004	0.246
			(0.006)	(0.007)	
Farming or agricultural labor	851	0.015	-0.013**	-0.004	0.257
			(0.006)	(0.011)	
Sex worker	851	0.000	0.002	-0.000	0.325
			(0.002)	(0.001)	

Robust standard errors in parentheses. *, **, and *** indicate significance at the 90, 95, and 99 percent confidence levels, respectively. OLS regressions reported. All specifications include controls for baseline household size, education level, and indicators for having given birth, having received any vocational training, or having any paid work experience prior to the baseline survey, in addition to survey enumerator and survey month fixed effects..

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