

Access, Sorting, and Achievement: The Short-Run Effects of Free Primary Education in Kenya[†]

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We examine the impact of the Kenyan Free Primary Education program on student participation, sorting, and achievement on the primary school exit examination. Exploiting variation in pre-program dropout rates between districts, we find that the program increased the number of students who completed primary school, spurred private school entry, and increased access for students from disadvantaged backgrounds. We argue that the program was welfare enhancing as it promoted educational access without substantially reducing the test scores of students who would have been in school in the absence of the program. (JEL H52, I21, I28, O15)

Education is often viewed as a key driver of economic development. In conjunction with donors and non-governmental organizations, developing countries have invested heavily in efforts aimed at achieving the Millennium Development Goal of universal primary education by 2015. As school fees have been found to be a major deterrent to educational access in a variety of settings (Holla and Kremer 2008), governments in these countries have instituted policies that reduce or eliminate user fees in order to boost school enrollments. In particular, since 1994, 17 sub-Saharan African countries have implemented free primary education programs. Despite their prevalence, very little research has assessed the impacts of such programs on enrollment, educational attainment, and student achievement in Africa. Further, the extent to which these programs can generate general equilibrium effects, such as sorting, social stratification, and a private school supply

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response has yet to be quantified. This paper uses the Free Primary Education (FPE) program in Kenya to examine the impact of the nationwide elimination of public school fees on access and achievement outcomes of students in eighth grade (the final grade of primary school) and on system-wide outcomes, such as sorting and private school entry.

In January 2003, the Kenyan government abolished all fees in public primary schools. While the government was lauded internationally for eliminating fees, reports of a strained public school system struggling to absorb new students without additional teachers or classrooms began to surface in the popular press shortly after the start of the program (e.g., Murphy 2003). These concerns were amplified by additional reports of overcrowding at previously prestigious public primary schools. For instance, Sanders (2007) described the impact of FPE on Olympic Primary School in Nairobi. Prior to the FPE program, Olympic Primary was consistently among the top-performing public schools in the country. In 2002, total school enrollment was about 1,700 students. In the first year of FPE, 3,000 new students sought enrollment. As a result of its mandate as a public school to accept as many students as possible, the average class size rose to about 84 students in 2007, almost double the pre-program level, and its academic performance declined.

While FPE programs could significantly increase the number of economically disadvantaged students who enroll and complete primary school, the above reports highlight some of the adverse consequences such programs can have on the quality of the public school system, such as increasing the pupil to teacher ratio and generally reducing the resources available per child. Additionally, FPE could spur the movement of richer or higher ability peers into private schools (Hsieh and Urquiola 2006). This, in turn, could lower peer quality and, subsequently, achievement in the presence of positive peer effects. Within-district heterogeneity could augment the desire by some groups to sort into private schools (e.g., Fairlie and Resch 2002; Betts and Fairlie 2003; Hsieh and Urquiola 2006; McEwan, Vegas, and Urquiola 2008). Finally, such a large policy change could induce a private school supply response, further promoting sorting and stratification, while ameliorating school supply constraints. Overall, the combination of these effects would be reflected in the number and composition of students in public and private schools and in their scores on the primary school exit exam.

We identify the impact of FPE on student and system-wide outcomes by exploiting its effective differential impact across Kenyan districts, where the magnitude of the impact in each district is proportional to the dropout rate prior to the policy change.¹ Intuitively, the policy should have little to no effect in a region where there was 100 percent retention rate (or a 0 percent dropout rate) prior to the policy, and considerably greater impact in regions with the lowest retention rates. Under this assumption, we identify the effect of the program using a difference-in-differences strategy, comparing the outcomes before and after the policy

¹ Because of the timing of the program (2003) and our focus on students who were eighth graders in 2000–2007, we use the dropout rates instead of other measures of the total number of students out of school. This is discussed further in Section IV.

change across regions differentially affected by the program. We further employ the same identification strategy in a changes-in-changes framework (Athey and Imbens 2006) to estimate the effect of FPE on achievement across the distribution of test scores. Based on this analysis, we find that the program successfully increased student access, particularly for poorer students, and led to only small score declines for the students who would have graduated without FPE, even though public school capacity did not increase. Consistent with the popular press, we find the largest increases in demand for (pre-FPE) high-quality schools, such as Olympic Primary. We also find some suggestive evidence of sorting by socioeconomic status as the demand for private schooling was higher in districts with greater inequality. Additionally, we find that FPE led to an increase in the supply of private schools, which perhaps alleviated some of the pressure on the public school system. Overall, our findings suggest that, in contrast to the well publicized reports, the FPE program was welfare enhancing as it provided primary school access for a significant number of children without substantially compromising the quality of the education system in the short run.

Our findings are consistent with prior studies that found that a variety of programs such as school construction or conditional cash transfers were effective at boosting enrollments, especially for poor children or children in underserved regions (e.g., Duflo 2001 in Indonesia; Schultz 2004 in Mexico; and Barrera-Osorio, Linden, and Urquiola 2007 in Colombia). The existing research on free primary education programs in sub-Saharan Africa has mostly relied on cross-cohort differences in exposure, potentially attributing unrelated changes over time to the free education program. In general, these papers found that FPE programs increased enrollment, especially among poorer students, and reduced the incidence of delayed primary school entry (Deininger 2003, Grogan 2009; and Nishimura, Yamano, and Sasaoka 2008 in Uganda; and Al-Samarrai and Zaman 2007 in Malawi). A recent paper on the impact of Kenya's FPE program by Bold et al. (2010) also examined the extent to which the program reduced the perceived public school quality and led to the movement of more affluent students from public schools to private schools. However, their analysis relied on a number of strong identifying assumptions. Overall, these papers alluded to the potential trade-off that many developing countries face between increasing educational access and increasing educational quality. However, they did not address the impact of these programs on the joint outcomes of access, sorting across public and private schools, private school entry, and student achievement.

Our identification strategy, exploiting the differential intensity of the nationwide program across districts in Kenya, enables us to observe general equilibrium effects that typically do not occur under local, randomized experiments. Our methods capture the total short-run effects of the program and can provide a more complete assessment of the total costs and benefits of similar programs under consideration or implemented elsewhere. As other African countries move toward national fee reduction and elimination plans, an accurate estimation of the effects of a similar program provides crucial information that can guide future policy decisions. Furthermore, the general equilibrium effects induced by the program have important implications for educational policy throughout the world.

I. Primary Education in Kenya

Kenyan education follows an 8-4-4 system with eight years of primary education, four years of secondary education, and four years of university education. Primary school in Kenya consists of grades (standards) 1–8. In order to earn a primary school diploma, students must take the national primary school exit exam, the Kenya Certificate of Primary Education (KCPE), after the completion of grade 8. Almost all students who complete grade 8 take the KCPE.² Students are eligible to start grade 1 if they are at least 6 years of age at the start of the school year in January. Even though almost all children attend at least some primary school, delayed entry, grade repetition, and dropping out are common. Analysis of the 1999 Kenyan Census shows that 90 percent of young adults aged 18–20 attended at least some primary school, but only 48 percent completed grade 8. Relative to other countries in sub-Saharan Africa in 2002, Kenya had above average youth literacy (80 percent versus 72 percent regionally) and net enrollment in primary education (67 percent versus 64 percent) (UNESCO 2005).

Prior to 2003, both private and public schools charged fees.³ For public schools the school management committee at each school determined the fee level. Average public school fees were approximately US\$16 per year in 1997 (World Bank 2004); however, some public schools charged up to US\$350 per year per child (Wax 2003). These fees paid for all tuition, textbooks, supplies, and construction and maintenance of physical facilities. The government allocated and paid for teachers through the Teacher Service Commission. Ninety-nine percent of public spending at the primary level was devoted to teacher salaries (World Bank 2004).

While most primary schools in Kenya are public, the number of private schools has recently increased. In 2007, 11 percent of primary schools in Kenya that administered the KCPE were private, up from 5 percent in 2000. Based on calculations from the 1997 Welfare Monitoring Survey, students in private schools were from wealthier households, and had better educated parents than students in public schools. The quality and cost of private schools vary. Low-cost private schools cater to the poor and are often found in urban slums. These schools charge approximately KSh1,200 (approximately US\$16) per year, which put the cost on par with the pre-FPE program cost of public schools (Tooley 2005). On the other end of the spectrum, some private schools that catered to richer households cost \$4,000 per year (Bauer, Brust, and Hubbert 2002). With the exception of those in low-cost private schools, teachers in private schools earn higher salaries than their public school counterparts. Public school teachers earn approximately \$2,000 per year, whereas teachers in private schools could earn three times as much (World Bank 2004; and Bauer, Brust, and Hubbert 2002).

One of the first priorities of the Kenyan government elected on December 27, 2002 was the elimination of school fees for all public primary schools. The FPE

² The KCPE exam is the placement exam for secondary school and is thus essential for students who are considering schooling beyond grade 8. For other students, the examination acts as proof of primary school completion and some jobs require a minimum KCPE score of applicants.

³ Prior attempts at free primary education in 1974 and 1978 were not comprehensive. Any fees reduced under these programs were increased as a part of the structural adjustment programs in the late 1980s.

program eliminated all such fees as of the start of the school year in January 2003. As a part of the fee elimination scheme, schools were granted KSh1,020 (approximately US\$14) per pupil to cover the formerly collected school fees.⁴ As with the formerly collected fees, this funding supported physical facilities and textbooks.

II. Empirical Strategy

FPE was implemented simultaneously across the entire country in January 2003. Simply comparing outcomes before and after 2003 would not identify the impact of FPE, as it would attribute all nationwide changes in primary school outcomes to the program. To overcome this concern, this paper exploits the pre-program differences in the grade-specific dropout rates between districts to examine the educational outcomes of eighth grade pupils. Even though the program was nationwide, the effective intensity of the program for a district in a given year depends on the number of students who could have been induced by the FPE to complete primary school. Thus, the effective program intensity varies both by district and by year, based on the grade-specific dropout rates in different districts. Due to data limitations, our analysis is focused on students in eighth grade, the final year of primary school. Since all the students in our data started primary school at least three years prior to FPE, the program will mainly affect their primary school completion by preventing attrition.⁵ Therefore, we are only able to examine the short-run effects of the program on completion, sorting, and achievement of students who had already completed some primary school pre-FPE. Hence, our estimates do not provide the total impact of the program, which would include the impact of new students entering primary school in addition to the effect on retention that we are able to examine.

To estimate the effect of FPE, we rely on a difference-in-differences specification:

$$(1) \quad y_{sjt} = \beta_0 + \beta_1(intensity_{jt} \times public_s) + \beta_2(intensity_{jt} \times private_s) \\ + \delta_j + \delta_j \times public_s + \delta_j \times trend_t + \delta_t + \varepsilon_{sjt},$$

where y_{sjt} is the outcome (e.g., number of graduates) for school type s (either public or private) in district j in year t ; $intensity_{jt}$ is the effective intensity of the program; $public_s$ is an dummy variable equal to 1 for public schools; $private_s$ is a dummy equal to 1 for private schools; δ_j and δ_t are district and time fixed effects; $\delta_j \times public_s$ are a set of school type-district fixed effects; and $\delta_j \times trend_t$ are district-specific linear trends for all but one district.⁶ Following Bertrand, Duflo, and Mullainathan

⁴ This public funding was reduced to KSh845 in 2004 and increased back to KSh1,020 for 2005 (Alubisia 2004, and Ministry of Education, Science, and Technology 2005).

⁵ New entrants to primary school start at first grade regardless of their age. A well-publicized example of this policy is the 84-year-old who enrolled in first grade in 2004. The full impact of the program on new enrollees will not be observed in the exit examination data until 2010, when the first cohort fully exposed to FPE completes primary school.

⁶ A concern with any difference-in-differences identification strategy is that we could be calling pre-existing trends between districts a program effect (e.g., districts that had high primary school dropout rates prior to the program are trending differently than other districts). To control for these potentially confounding trends, we include district-specific time trends in all specifications to ensure that we are identifying a program effect separate from incidental district trends. We control for any time-invariant differences between districts with district fixed effects.

(2004), standard errors are clustered at the district level. The year in which a student takes the KCPE exam and the district in which he takes it determine $intensity_{jt}$, or exposure to the program. Prior to the introduction of FPE, $intensity_{jt}$ is zero for all students. However, starting with 2003, we compute $intensity_{jt}$ for each district-year from district-level completion rates prior to the program.⁷ β_1 and β_2 measure the effect of FPE on private and public schools. We estimate variations of equation (1) at the district-school type-year and school-year level to examine the impact of the program on the number of students who take the KCPE (our measure of student enrollment and primary school completion), the parental characteristics of enrollees, new school construction, and teacher employment.

FPE could alter the distribution of KCPE scores because of changes in the composition of students who took the exam and in the quality of educational inputs. We examine the impact of the FPE program on the entire distribution of test scores using the changes-in-changes (CIC) model of Athey and Imbens (2006). The CIC is a generalization of the difference-in-differences estimator that allows us to compute the entire counterfactual distribution of the program under a set of plausible assumptions. Intuitively, instead of comparing means as in a difference-in-differences specification, this method first nonparametrically estimates the change in the entire test score distribution of the control group pre- and post-intervention. The counterfactual for the treatment group is then identified under the assumption that the treatment group's test score distribution would experience a similar change in the absence of treatment.

Following the standard potential outcome framework, we denote $SCORE_i^N$ and $SCORE_i^I$ as the potential test score for an untreated (N) individual i and a treated (I) individual i , respectively. We employ the standard 2×2 CIC estimator, with two time periods ($T_i \in \{0, 1\}$, i.e., pre-FPE and post-FPE) and two groups ($G_i \in \{0, 1\}$, i.e., control and treatment). Since our intensity measure is continuous, we create a binary treatment indicator by defining all districts with an intensity above the average as "treated" and those with an intensity at or below the average as "untreated."

The following assumptions are necessary to identify the effect of FPE using the CIC method. First, we assume that the potential test score for an untreated individual ($SCORE_i^N$) is a function of ability and the time period in which the test was taken i.e.,

$$(2) \quad SCORE_i^N = h(U_i, T_i),$$

where U_i is the unobserved (test-taking) ability of a student who took the KCPE, and T_i is the time period. As $h(\cdot)$ does not vary by group, all differences in test scores are due to differences in the realizations of U_i (denoted as u). Second, the CIC framework requires that the distribution of (test-taking) ability (U_i) be time invariant within each group:

$$(3) \quad U_i \perp T_i | G_i.$$

⁷ The details of the computation of $intensity_{jt}$ are provided in Section IIIA.

This assumption imposes the restriction that the composition of the group (i.e., the distribution of ability within a group) does not change over time. Third, the CIC imposes a strict monotonicity assumption, where the production function $h(U_i, T_i)$ is strictly increasing in u , which allows inference concerning the change in the production function over time. Finally, the CIC framework requires the following support condition for the distributions of U in the post-period, $U_{i,1}$, and in the pre-period, $U_{i,0}$: $U_{i,1} \subseteq U_{i,0}$.

Under these assumptions Athey and Imbens (2006) show that the unobserved counterfactual CDF of the treatment group in the post-treatment period, $F_{SCORE^N,11}$, can be obtained by the following transformation, which can be estimated using empirical distributions:

$$(4) \quad F_{SCORE^N,11} = F_{SCORE,10} \left(F_{SCORE,00}^{-1} \left(F_{SCORE,01}(\text{score}) \right) \right),$$

where $F_{SCORE,10}$ is the CDF of the test scores of the treatment group pre-FPE; $F_{SCORE,01}$ is the CDF of the control group post-FPE; and $F_{SCORE,00}^{-1}$ is the inverse CDF of the control group pre-FPE. The treatment effect, τ_q^{CIC} , at any quantile q can then be computed as

$$(5) \quad \tau_q^{CIC} = F_{SCORE,11}^{-1}(q) - F_{SCORE^N,11}^{-1}(q),$$

i.e., the difference between the inverse CDF of the treated group post-FPE ($F_{SCORE,11}^{-1}$) and the estimated counterfactual inverse CDF ($F_{SCORE^N,11}^{-1}$), both evaluated at the same quantile (q).

We control for covariates following the parametric approach suggested by Athey and Imbens (2006). We estimate

$$(6) \quad \text{score}_i = \mathbf{D}_i' \delta + \mathbf{X}_i' \beta + \varepsilon_i,$$

where score_i is an individual's KCPE score; $\mathbf{D}_i = ((1 - T_i)(1 - G_i), T_i(1 - G_i), (1 - T_i)G_i, T_i G_i)'$ is the vector of group and time effects; and \mathbf{X}_i is a vector that contains district dummy variables and district specific linear trends. We then subtract the estimated effects of the \mathbf{X}_i vector from each score_i to construct residuals that still contain the group-time effects (\mathbf{D}_i):

$$\widehat{\text{score}}_i = \text{score}_i - \mathbf{X}_i' \hat{\beta} = \mathbf{D}_i' \hat{\delta} + \hat{\varepsilon}_i.$$

We apply the CIC estimator in equation (5) to the empirical distribution of these "adjusted" residuals.

In order to apply the CIC to our setting, we need to make a few adjustments to the data to ensure that none of the CIC assumptions are violated. The time invariance assumption stipulates that unobserved (test-taking) ability is fixed within each group over time. However, since FPE changes the composition of students who take the KCPE, this assumption would be violated. Since we cannot identify the students who progressed to eighth grade as a result of the FPE program, we employ a trimming procedure similar to Lee (2009). We assume that the students who take the

KCPE because of FPE are the worst performing public school students and exclude them from the analysis. The number excluded is based on our estimates of the predicted number of FPE induced students from equation (1). As long as the “new students” scored anywhere below quantile q^* , then we can use the CIC to examine the impact of the program on the distribution of test scores at or above quantile q^* .⁸ As FPE can also lead to students sorting into different types of schools, we follow an approach analogous to Hsieh and Urquiola (2006), where we examine the impact of the program on the overall test score distribution rather than examine the public and private school test score distributions separately. This circumvents the empirical complications that arise from student sorting between different school types, albeit in an imperfect fashion. By truncating the distribution as described above, we effectively control for the compositional changes in the student population induced by FPE. Therefore, the effect of FPE on test scores that we estimate would be net of compositional changes and only capture changes in achievement related to changes in schooling inputs (e.g., crowding or teacher or peer quality). Moreover, we can interpret these estimates as the effect of FPE on the individuals who would have taken the KCPE test regardless of the FPE program.

III. Data

We define the market for primary schooling as a district. Ninety percent of students attend school within their district of permanent residence (RePEAT 2007). Furthermore, the district is the finest level of disaggregation that is used for administrative and planning purposes by the Ministry of Education. According to the 1999 census, the 69 districts in Kenya had an average population of 407,980, an average population of individuals aged 6–18 of 144,260, and an average size of 8,416 km².

We use several sources of data to evaluate the short-run effect of FPE in Kenya. We combine the administrative records of the Kenya National Examination Council, data from the Education Management Information System (EMIS) of the Ministry of Education, and 4 data sources collected by the Kenyan National Bureau of Statistics (the 5 percent IPUMS samples from the 1989 and 1999 Kenyan Censuses, the 1997 Welfare Monitoring Survey (WMS), and the 2005/2006 Kenya Integrated Household Budget Survey (KIHBS)).

The Kenyan National Examination Council records contain student level KCPE scores for all test takers in Kenya from 2000 to 2007 (approximately 5 million students). Information on individual and school attributes is limited to the test taker’s gender and school type and category (private or public, boarding or day, single-sex or coeducational, urban or rural). Using the examination data, we compute the number of test takers for each school and generate the empirical CDFs for the CIC

⁸ Because we are examining the impact of the program at various quantiles above q^* , we only need to assume those taking the test because of FPE score below quantile q^* , as removing the worst-performing students below quantile q^* is equivalent to removing the same number of students from anywhere below quantile q^* . Due to the number of students that we remove and their likely place in the score distribution, we focus on the distribution of test scores at and above the median.

estimator.⁹ Since school rankings based on test scores are published in the newspaper for each district, we test for differential responses to FPE by pre-FPE school quality (i.e., the 2002 school average KCPE score).

Over this period, the number of schools administering the KCPE increased from 15,177 to 19,765, resulting in a sample of over 139,000 observations at the school by year level. Only schools that are registered testing centers appear in our data. In order to become a registered testing center a school must register at least 15 students for the KCPE in its first year as a testing center. Once a school is registered, the number of students taking the exam in subsequent years can fall below 15. Students who attend schools that are not registered testing centers take the KCPE in a nearby school that is a registered testing center. As a result our data capture all the graduates and test scores, but potentially understate the total number of schools. Since the registration regulations have been in effect for all years of our data, the potential undercounting of schools should be consistent over time and unrelated to the FPE program. For simplicity, we refer to “schools” instead of “testing centers” throughout the paper. Table 1 shows selected summary statistics.

We link the test score information to the EMIS database, the WMS, the KIBHS, and the censuses at the district level. The EMIS data contain school-level information on student enrollments and teachers in public schools. Unfortunately, the system is not fully updated, and we were only able to obtain this information for 2001, 2003, and 2004. The 1997 WMS and the 2005 KIHBS, two nationally representative household surveys, contain basic demographic information, parental characteristics, and the type of school the individual is currently attending. Unfortunately, we cannot link these surveys to the test score data at the individual or school level. Instead, we use these repeated cross sections to examine the changes in the household characteristics of students in private and public schools due to FPE. Additionally, we use the WMS to determine the district level variation in household expenditures and poverty rates that we use in some specifications.

Our final sources of data are the 5 percent IPUMS samples of the 1989 and 1999 Kenyan Censuses. From these data, we compute our measure of intensity (discussed below). We also use the 1999 census to measure district-level unemployment, population growth, and the number of individuals who did not complete grade 1, three measures that we use to check the robustness of our main results.

A. Computation and Measurement of Intensity

We use the 5 percent IPUMS sample of the 1999 census to calculate a separate effective FPE intensity, $intensity_{jt}$, for each district-year from district-level completion rates for each grade prior to the program.¹⁰ Prior to the introduction of FPE in

⁹ The number of test takers is an approximation for the number of students enrolled in grade 8. Due to data limitations we are only able to compare the ratio of test takers to the number of students enrolled in eighth grade at district level for students in public schools. This ratio is close to one for all district-years, suggesting that almost all eighth graders take the KCPE test. Additionally, regression estimates show that the percentage of test takers is not a function of our program intensity measure, mitigating concerns about the program changing the nature of selection into the test. Results available upon request.

¹⁰ Because of the timing of our test score data (2000–2007) and the program start date (2003), our measure of intensity is designed to capture changes in transition rates between grades, not entry of students who had not

TABLE 1—SUMMARY STATISTICS

Variable	Pre-FPE (2000–2002)	Post-FPE (2003–2007)	Increase
Number of schools	15,711	18,477	2,766
Public schools	14,775	16,696	1,921
Private schools	936	1,781	845
Number of test takers per year	498,359	651,784	153,425
Public schools	467,910	597,637	129,727
Private schools	30,449	54,147	23,698
Number of test takers per school	31.72	35.28	3.56
Public schools	31.67	35.80	4.13
Private schools	32.53	30.40	−2.13
Standardized KCPE score	0.0007	0.0011	0.0004
Public schools	−0.041	−0.067	−0.026
Private schools	0.646	0.757	0.111

Note: Counts calculated as annual averages over relevant period.

Source: Author calculations from Kenya National Examination Council data

2003, for every 100 students who started primary school, only 53 completed primary school, with attrition occurring between each grade (calculation from 1999 census). Panel A of Figure 1 demonstrates this pattern for a hypothetical cohort. Intuitively, the effective intensity of the program is increasing with the number of students that could be induced by the program to complete primary school relative to the number who were completing it already.

Before FPE, the intensity in all districts was 0 as no student who took the exam in those years attended primary school as a result of the FPE program (i.e., $intensity_{jt} = 0$ for $t < 2003$). The students who took the exam in 2003 were subject to FPE starting in January 2003, during the transition between grades 7 and 8. Students who would have dropped out between grades 7 and 8 could be induced to stay in school by the fee elimination. Within a district, we specify the effective intensity of FPE as the potential proportional increase in the number of students in grade 8 if the students who completed grade 7, but dropped out between grades 7 and 8 pre-FPE, continued to grade 8 (i.e., the number of dropouts between grades 7 and 8 divided by the current eighth grade cohort). This is equivalent to calculating the proportional increase in the test-taking cohort if attrition (or dropout) rates for the cohort were set to zero starting in 2003.¹¹ Panel B of Figure 1 demonstrates this for the 2003 test-taking cohort. For test takers in 2004, we imposed an attrition rate of zero for the transition from the end of grade 6 (December 2002) all the way to the KCPE (grade 8 in 2004). Thus for this cohort, the intensity in 2004 is the number of individuals who completed grade 6, but did not complete grade 8, divided by the number who were in grade 8. Panel C of Figure 1 provides a visual depiction.

Ideally we would use data from 2002 to construct our intensity measure. As a close approximation, we use the 1999 census. We start with the number of individuals

previously attended primary school. FPE also changed school entry decisions. Since all students, regardless of age, start primary school with grade 1, students who entered school in 2003 would not be taking the KCPE until 2010 at the earliest.

¹¹ We do not know the exact number of students who dropped out as a result of fees prior to the program. Our measure provides an estimate of the maximum impact that the program could have.

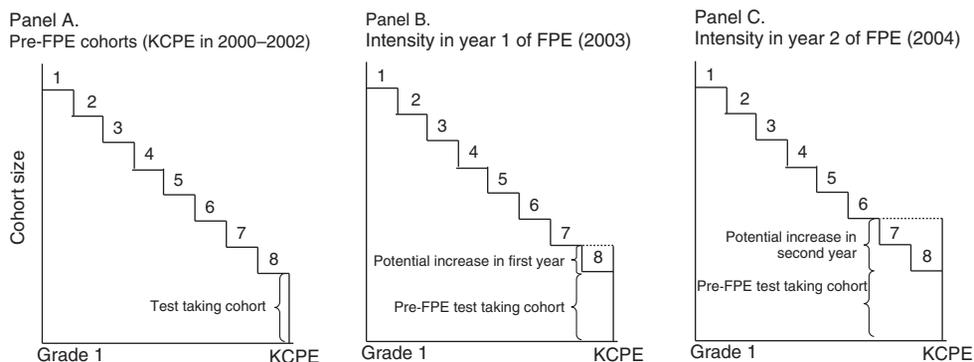


FIGURE 1. CALCULATION OF PROGRAM INTENSITY

Note: By district and year, intensity = (potential increase)/(pre-FPE test taking cohort).

who are younger than 18 and who completed each grade in a given district.¹² We then compute the number of individuals that dropped out after completing a given grade by subtracting the number of individuals who completed primary school or are still in primary school. The intensity in a given district is the number of students who would have dropped out divided by the size of the grade 8 cohort. Table A1 displays the detailed sample calculation for Homa Bay, a district of average intensity. Based on our calculations, there was a high degree of variation between districts in the effective intensity of FPE. Pre-FPE in the relatively wealthy farming district of Muranga, the vast majority of students who completed grade 7 continued on to grade 8. In the first year of FPE, the effective intensity of FPE for Muranga is 0.099, indicating that if all students who completed grade 7 sat for the KCPE, the number of test takers would increase by 9.9 percent. In contrast, there was a high degree of attrition pre-FPE between grade 7 and grade 8 in Malindi, a coastal district with a large tourist industry, with a potential increase in the number of test takers of 70.7 percent if all students who completed grade 7 pre-program went on to sit for the KCPE. Therefore, even though the program was nationwide, FPE had the potential of a larger impact in Malindi, where there was a higher degree of attrition prior to the program. Table A2 contains the calculated intensity measures for each district-year.

While we use a continuous intensity measure for much of our analysis, the CIC methodology we employ to analyze the FPE induced changes in the distribution of test scores requires us to use a dichotomous intensity measure. We define two groups for our CIC analysis: a “treatment group” (or a high program intensity group) that are districts where the intensity is above average, and a “control group” (or a low program intensity group) that is defined conversely.

Because we are using dropout rates from 1999, an additional assumption is that any changes in attrition patterns that occurred between 1999 and 2002 are uniform across the country or uncorrelated with prior attrition. Country-wide changes in attrition patterns will be absorbed by year fixed effects. To test for differential changes in

¹² We use age 18 as the cutoff, since almost everyone who completes primary education does so prior to age 18. We include younger cohorts as their educational experience occurred closer in time to FPE.

attrition patterns, we compare the intensity ratios calculated from the 1999 census to ones calculated from the 5 percent IPUMS sample of the 1989 census. We find that the correlation between these measures is 0.80, suggesting that the ratios had been relatively stable for the previous 10 years. Therefore, with no compelling reason to think that these ratios changed in the three years after 1999, we feel that this is a valid assumption.

This year- and district-specific intensity measure is well suited for our analysis since retention is the main channel through which FPE would affect grade 8 completion prior to 2010. Other possible measures, such as the pre-program, nonenrollment rate and pre-program school fees, capture the total enrollment effects of FPE (new entrants plus retained students) and would not accurately capture the potential impact of the program on students who took the KCPE from 2003 to 2007. While we argue that our measure is better suited for our analysis, there are some potentially important concerns that arise. First, this measure could be driven by initial pre-program school quality, where districts with poor quality schools have higher dropout rates. Additionally, there may be other contemporaneous programs that were instituted in a manner that is correlated with our intensity measure. Another potential concern is that this intensity measure reflects changes in population growth over time. In order to mitigate these concerns, we perform a number of robustness checks to ensure that our results are not spurious. Overall, we find that our baseline results are robust to all of these checks and discuss these results in more detail in Section V.

IV. Results

A. *Sorting across Schools*

Table 1 provides suggestive evidence of the impact of FPE on student access. Overall, the total number of schools and test takers increased after FPE was introduced in 2003. Even with school construction, the increase in the number of test takers per school shows the potential for the deterioration of public school quality. Over this period the average KCPE score increased slightly even though the average score of students who took the test in public schools decreased.

Figure 2 plots the total number of test takers in the country. The dashed line is the linear trend based on the 2000–2002 data. The number of students post-FPE exceeds the number predicted by the pre-FPE trend. Figure 3 shows that this increase in total students had a differential effect on the per school cohort size for public and private schools. Prior to 2003, the average number of test takers per school was similar for public and private schools. However, after the introduction of FPE, the number of test takers per public school increased, and the size of private school cohorts decreased, such that by 2007 the average test taking cohort in a public school was 17 percent larger than in a private school.

The estimates of equation (1) are shown in column 1 of Table 2. As expected, the number of test takers increased more in districts that were more intensely treated by the program. The average value of our intensity measure post-FPE is approximately one, indicating a 100 percent potential increase in the number of test takers, or a doubling in the number of test takers. At this intensity, the number of test takers

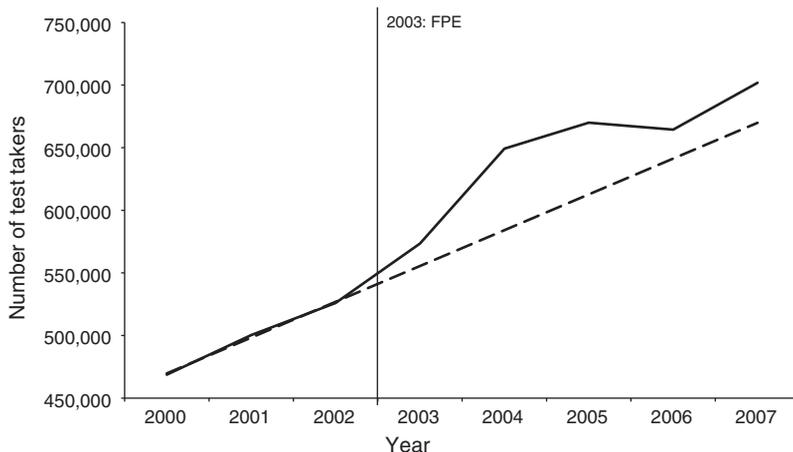


FIGURE 2. TOTAL NUMBER OF TEST TAKERS

Notes: Calculated from Kenya National Examination Council data. Solid line: calculated aggregates. Dashed line: linear trend based on 2000–2002 data.

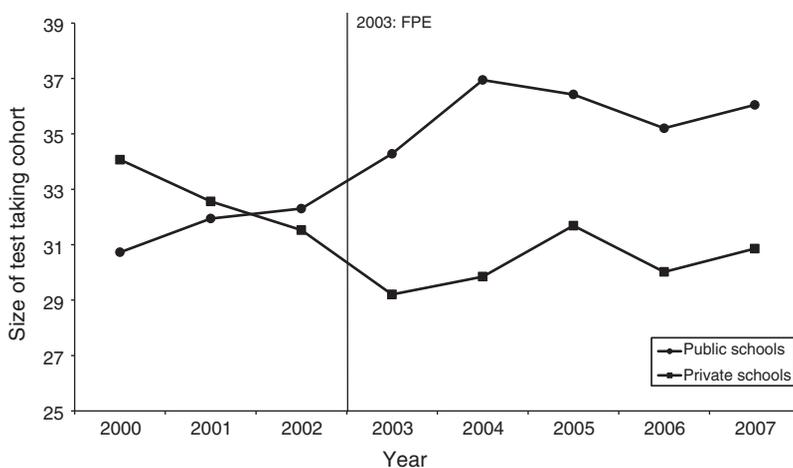


FIGURE 3. NUMBER OF TEST TAKERS PER SCHOOL

Note: Calculated from Kenya National Examination Council data.

in the average district is expected to increase by 690 test students, with 776 more students in public schools and 86 fewer students in private schools. The change in the number of public school test takers is statistically different from zero, but the predicted private school change is not. These estimates imply that FPE increased the number of test takers by 3 percent in 2003 and 18 percent in 2007, all relative to 2002. We estimate equation (1) at the school-year level in column 2. These results show that the average number of test takers per public school increased by almost 3 students in the average district, an increase of 10 percent over pre-FPE levels. The predicted change in the average number of test takers per private school is positive but not statistically significant.

TABLE 2—SORTING ACROSS SCHOOLS, CHANGES IN COHORT SIZES

	Dependent variable: Cohort size		
	District-school type level	School level	
	(1)	(2)	(3)
Intensity \times public school	776.2*** (153.2)	2.895*** (1.034)	2.644** (1.027)
Intensity \times private school	-85.96 (118.0)	1.983 (1.382)	2.085 (1.377)
Public school	7,229*** (38.26)	14.14*** (0.569)	14.27*** (0.866)
Public schools in top 10 percent in 2002			2.342** (0.946)
Intensity \times public schools in top 10 percent in 2002			4.805*** (0.661)
Public schools in bottom 10 percent in 2002			-3.947*** (0.906)
Intensity \times public schools in bottom 10 percent in 2002			-1.106* (0.613)
Private schools in top 10 percent in 2002			2.684 (2.301)
Intensity \times private schools in top 10 percent in 2002			4.868*** (1.505)
Private schools in bottom 10 percent in 2002			1.348 (2.570)
Intensity \times private schools in bottom 10 percent in 2002			-3.183** (1.223)
Observations	1,104	139,519	139,519
R^2	0.989	0.26	0.27

Notes: Standard errors clustered at the district level appear in parentheses. All regressions include the district linear trends and district times school type and year dummy variables. Column 1: the unit of observation is a district-school type-year. Columns 2 and 3 include urban and boarding school dummy variables. The unit of observation is a school year.

***Significant at the 1 percent level.

**Significant at the 5 percent level.

*Significant at the 10 percent level.

The program could differentially affect schools of varying (pre-program) quality. The case of Olympic Primary School in the introduction highlighted the potential for FPE to generate excess demand for (pre-FPE) high-quality public schools. In contrast, the impact of FPE on high-quality private schools would depend on their ability to practice selective admissions (or “cream-skimming”) and on the trade-off between limiting access and generating profits. The results in column 3 suggest that parents overwhelmingly tried to enroll their children in high-performing public schools.¹³ Within a district, highly-ranked public schools saw the largest increases in

¹³ To examine any differential effect by school quality, we classify schools based on the average school score in 2002. We create three different quality designations at the district level: top 10 percent of schools, middle 80 percent of schools, and bottom 10 percent of schools. Our classification of schools into the three tiers is rather arbitrary. In results not shown, we consider two additional specifications. First, in a more flexible specification, we interact the pre-FPE average KCPE score directly as the triple difference specification. Second, we categorize schools as top

TABLE 3—CHANGES IN STUDENT CHARACTERISTICS

Dependent variable:	All primary grades		Grade 7 and 8 only	
	Paternal literacy (1)	Maternal literacy (2)	Paternal literacy (3)	Maternal literacy (4)
Intensity \times public school	-0.0590** (0.0277)	-0.0555* (0.0313)	-0.0885* (0.0472)	-0.0948* (0.0488)
Intensity \times private school	-0.0105 (0.0304)	0.0209 (0.0485)	-0.0386 (0.0577)	-0.0168 (0.0699)
Public school	-0.0666*** (0.0172)	-0.0832*** (0.0260)	-0.0752* (0.0397)	-0.109** (0.0494)
Observations	20,252	25,888	3,471	4,593
R^2	0.17	0.17	0.15	0.16

Notes: Sample consists of primary school students surveyed as a part of the 1997 WMS or 2005 KIHBS. Average percent literate in 1997: 77 percent of fathers and 63 percent of mothers. Additional controls: current grade, district, and year fixed effects. Standard errors clustered at the district level appear in parentheses.

***Significant at the 1 percent level.

**Significant at the 5 percent level.

*Significant at the 10 percent level.

the number of the test takers. In a district with average intensity, top-ranked public schools would receive 7.4 additional test takers (a 21 percent increase relative to 2002), middle-ranked public schools would receive 2.6 additional test takers (an 8 percent increase), and low-ranked public schools would only gain an additional 1.5 test takers (a 5 percent increase). Top-ranked private schools would gain about 7 additional test-takers (a 21 percent increase), while the number of test takers in low-ranked private schools would decrease by 1.1 students (a 3 percent decrease). This decline could indicate movement of these students to free public schools, or a move to better-ranked private schools.

We use data from the 1997 WMS and the 2005/2006 KIHBS to examine changes in the observable characteristics of students in public and private schools. Because the surveys do not contain comparable income measures, we focus on the parental education of primary school children as a measure of student SES. Specifically, we estimate equation (1) as a linear probability model with a binary measure of parental literacy as the dependent variable and a primary school student as the unit of observation. The estimates of β_1 and β_2 capture the FPE-induced changes in the socioeconomic status of students in each school type. The results in Table 3 show that the parental literacy of public school students declined in higher intensity districts as FPE allowed students with less-educated parents to take the KCPE. Based on this measure of SES, we do not find any significant change in the composition of students in private schools.¹⁴ The net effect is a widening of the gap in SES between public and private schools.¹⁵

25 percent, middle 50 percent, and bottom 25 percent in the triple difference specification. In all cases, we obtain substantively similar results.

¹⁴ Parental literacy may fail to capture the level of parental education at which students sort between public and private schools.

¹⁵ We reject the hypothesis that the estimates across school types are equal within columns 1 and 2, however, we fail to reject this hypothesis within columns 3 and 4.

TABLE 4—STUDENT SORTING

	Dependent variable: Cohort size			
	Variation in household expenditures		Urban versus rural	
	District-school type level (1)	School level (2)	District-school type level (3)	School level (4)
Intensity × public school	852.5*** (174.2)	3.149*** (1.056)	733.2*** (174.2)	2.810*** (1.043)
Intensity × private school	−280.4 (179.9)	1.626 (1.092)	−88.290 (133.2)	1.636 (1.143)
Intensity × public school × high expenditure variation	−103.4 (214.6)	−0.263 (1.015)		
Intensity × private school × high expenditure variation	311.0* (157.3)	0.768 (1.357)		
Intensity × public school × urban			151.3 (295.2)	1.056 (1.598)
Intensity × private school × urban			11.54 (151.5)	0.95 (1.889)
Public school	4,261*** (53.8)	4.525*** (1.384)	7,240*** (42.9)	13.94*** (0.443)
Public school × high expenditure variation	−2,513*** (379.5)	12.09*** (2.021)		
Public school × urban			−1,282*** (116.1)	21.41*** (7.41)
Observations	1,024	137,447	1,104	139,519
R ²	0.99	0.27	0.99	0.27

Notes: Standard errors clustered at the district level appear in parentheses. All columns include year and district times school type dummy variables and district linear trends. Columns 1 and 2: districts with high variation in expenditures are those with higher than median standard deviations in total household expenditures calculated from the 1997 WMS. Columns 2 and 4: include urban and boarding dummy variables. Column 3: the 18 urban districts are those with any schools labeled “urban” by the National Examination Council. Column 4: urban designation at the school level from the National Examination Council.

***Significant at the 1 percent level.

**Significant at the 5 percent level.

*Significant at the 10 percent level.

We explore the potential for “rich flight” from public schools to private schools as a result of the FPE program. Assuming a parental concern for peer groups, parents would have greater incentives to leave the public school system in more heterogeneous districts. We use the variance within a district in per capita annual expenditures calculated from the 1997 WMS to examine this possibility in columns 1 and 2 of Table 4 using a triple difference specification. Consistent with the “rich flight” hypothesis, we find that the FPE program led to increased demand for private schooling in districts with a higher expenditure variation.

The potential for student sorting across schools could also be greater where there is a greater density of schools. Our only indication of population or school density is the designation of a school as “urban” in the test score data. We use this “urban” school indicator in a triple difference specification shown in columns 3 and 4 of Table 4. In column 3, the 18 districts with at least one urban school are classified as “urban.” In column 4, we use the school-specific urban indicator. While the point

TABLE 5—SCHOOL OPENINGS

	Dependent variable: Newly opened schools					
	All schools		Private schools		Public schools	
	(1)	(2)	(3)	(4)	(5)	(6)
Intensity	-2.768 (3.753)		3.000** (1.216)		-5.768 (3.674)	
Lagged intensity		-0.7532 (4.265)		3.145** (1.333)		-3.899 (4.226)
Observations	483	483	483	483	483	483
R ²	0.40	0.40	0.77	0.77	0.34	0.34

Notes: Standard errors clustered at the district level appear in parentheses. All regressions include district dummy variables, district linear trends, and year dummy variables. Newly opened schools are schools that administered the KCPE for the first time. Years 2001–2007 only.

***Significant at the 1 percent level.

**Significant at the 5 percent level.

*Significant at the 10 percent level.

estimates indicate that urban schools had larger increases in students relative to rural areas, these differences are not statistically different from zero.

B. School Market Response

One potential market response to the increase in the number of students due to FPE would have been the construction of new schools. As shown in Table 1, the number of schools that administered the KCPE was higher after FPE. To test whether these schools entered the market in the districts with the greatest FPE intensity, we estimate equation (1) using data aggregated to the district level with the number of newly opened schools as the dependent variable.¹⁶ Therefore, the coefficient of interest is on $intensity_{jt}$ instead of the coefficient on the interaction of $intensity_{jt}$ times $public_{sj}$. As with our baseline specifications, we include district dummy variables, district linear trends, and year dummy variables. The results in columns 1 and 2 of Table 5 show that total school construction (both private and public) was not responsive to contemporaneous or lagged FPE intensity.¹⁷ However, when we focus only on newly opened private schools in columns 3 and 4, we see that private schools did respond to FPE by opening an average of three additional schools each year in a district of average intensity. In contrast, columns 5 and 6 show that public schools construction was not related to FPE intensity. This suggests that non-FPE related factors, such as the level of community organization (Kremer 2006) or other political considerations, were more important in determining the location of the 1,374 public schools opened from 2003 to 2007.

School construction is not the only potential market response. Existing schools could expand through the addition of teachers. Using EMIS data collected by the

¹⁶ We approximate the opening date of a school as the first year in which a school administered the KCPE. Without data from prior years, we must omit data from 2000 as we cannot separate newly opened schools from continuing schools.

¹⁷ The results are similarly insignificant for specifications up to four lags (i.e., school openings in 2007 responding to FPE intensity in 2003). Results not shown.

TABLE 6—SCHOOL CHARACTERISTICS

Dependent variable:	Number of teachers (1)	Students per teacher (2)
Intensity	268.2* (136.8)	7.635*** (2.182)
Year = 2003	-77.27 (60.85)	3.740*** (0.751)
Year = 2004	-276.2** (121.6)	3.099** (1.464)
Observations	207	207
R^2	0.995	0.928

Notes: Standard errors clustered at the district level appear in parentheses. Data for public schools and years 2001, 2003, and 2004 only. All regressions include district dummy variables.

***Significant at the 1 percent level.

**Significant at the 5 percent level.

*Significant at the 10 percent level.

Ministry of Education, we estimate the equivalent of equation (1) with the total number of primary school teachers as the dependent variable. Due to data limitations, we only have data for public schools and for the years 2001, 2003, and 2004.¹⁸ Thus, the coefficient on $intensity_{jt}$ is the coefficient of interest in these regressions. These results show that teachers were allocated differentially based on the FPE intensity (Table 6, column 1). However, between 2001 and 2004, the total number of public school teachers in Kenya fell by 3 percent. While districts with higher intensities were less affected by the nationwide decline in teachers, the only districts with an expected increase in the number of teachers are the 6 districts in 2004 with an intensity greater than 1.03 (i.e., the ratio of the coefficient on the 2004 year dummy variable and the coefficient on intensity). Some of the overall decline was due to resignations, retirements, and transfers to private schools by some of the more highly qualified teachers in response to deteriorating working conditions after FPE (Sanders 2007). There is also some evidence that more qualified teachers were moved to secondary schools, where the shortage of qualified teachers was even more acute (Mills 2009). Combined with the increases in student enrollments in public schools, these teacher movements resulted in an increase in the student to teacher ratio by over 7 students (a 26 percent increase relative to the average in 2001) in a district of average intensity (column 2).

C. Achievement

Figure 4 shows the average standardized KCPE score by school type from 2000 to 2007. Average KCPE scores in public schools were declining prior to FPE and continued this downward trend following the introduction of the program. Average scores in private schools reveal a striking increase that coincided with the introduction of FPE. Because private schools were a relatively small part of the market, based on a

¹⁸ Because 2001 is the only pre-FPE year in these data, we do not include district linear trends.

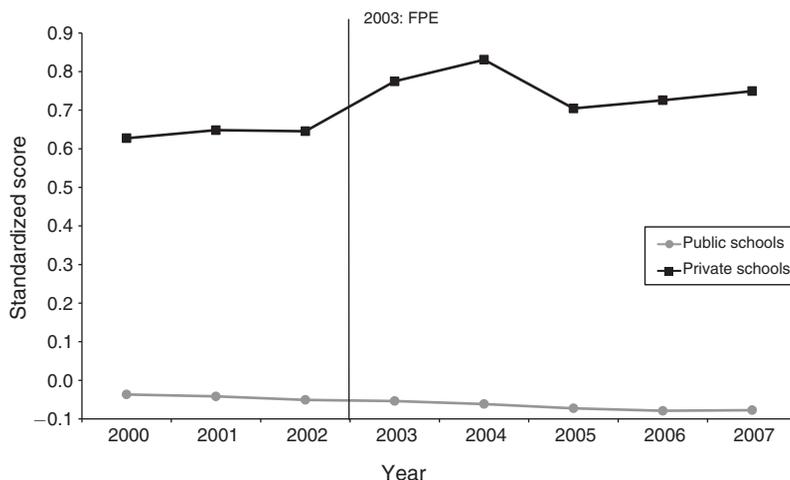


FIGURE 4. AVERAGE KCPE SCORES

Note: Calculated from Kenya National Examination Council data.

simple difference in the pre- and post-FPE academic performance, we find that average scores increased by 0.0004 standard deviations (Table 1). However, as previously discussed these simple comparisons do not differentiate districts based on the effective FPE intensity. Even performing an analysis using equation (1) with test scores as the dependent variable would not be sufficient to understand whether test scores changed because of a deterioration in inputs (e.g., peer effects, crowding, or teacher quality) or an altered composition of students (i.e., lower achieving students taking the KCPE).

We estimate the impacts of the FPE program on various quantiles of the overall district-level KCPE score distribution that includes both private and public school students. We examine the impact on quantiles at and above the median, approximating the score changes for students who would have taken the exam regardless of the program. As explained in Section III, since we removed close to 260,000 students (roughly 16 percent) from the post-FPE group in order to compare fixed ability distributions and implement the CIC methodology, examining these higher quantiles will yield the most reliable estimates. Table 7 displays the CIC estimates on the quantiles at and above the median. The results show that the treatment effect (on the treated) at the median is small, positive, and not statistically distinguishable from zero. However, at higher quantiles, we find that point estimates on the impact of the program are consistently negative but never more than 0.051 of a standard deviation in absolute magnitude. The estimates show that the program reduced test scores of the students who scored in the seventy-fifth and eightieth percentiles by about 3 percent of a standard deviation. Overall, these point estimates imply that students who would have taken the exam in the absence of the FPE in an average intensity district would score between 0 and 5 percent of a standard deviation lower as a result of the program. Therefore, our results suggest that FPE increased student access but only lead to minor decreases in student achievement for the students who would have completed primary school without the program.

TABLE 7—EFFECT OF FPE BY PERCENTILE

Percentile	Effect of FPE on Standardized KCPE Score
50	0.007 (0.010)
60	-0.006 (0.010)
70	-0.022 (0.013)
75	-0.028* (0.016)
80	-0.036* (0.019)
90	-0.044 (0.028)
95	-0.051 (0.033)

Notes: Standard errors clustered at the district level appear in parentheses. Estimates are from a changes-in-changes model. See text for additional details.

***Significant at the 1 percent level.

**Significant at the 5 percent level.

*Significant at the 10 percent level.

V. Robustness

A potential concern with our results is that they may be driven by other programs that are correlated with our measure of FPE intensity. In 2003, the government created the Constituency Development Fund (CDF) that distributed funds based on the incidence of poverty within a district to projects aimed at alleviating poverty. If districts with a high effective FPE intensity also received larger CDF funds, then our estimates may have overestimated the attendance response and underestimated the adverse achievement effect of FPE. To examine this, we include an interaction between the proportion of households that are under the poverty line from the 1997 WMS, the same survey data that determined the CDF allocation, with year dummies as additional regressors. Columns 3 and 4 of Table 8 and column 2 of Table 9 show that our results are robust to the inclusion of the poverty-time interactions, suggesting that our results are not driven by other contemporaneous anti-poverty programs.

Other government programs could have targeted districts with the highest unemployment rates. In columns 5 and 6 of Table 8 and column 3 of Table 9, we interact the district-level unemployment rate as calculated from the 1999 census, with year dummy variables as additional controls. Our results are robust to these inclusions.

Differential population growth could bias our results. In columns 7 and 8 of Table 8 and column 4 of Table 9, we include the size of the 14-year-old cohort based on the size of the relevant age group in the 1999 census, and find estimates similar to the baseline.

Since our intensity measure is derived from district-level dropout rates, a possible concern is that districts with lower-quality schools had higher dropout rates

TABLE 8—ROBUSTNESS—ADDITIONAL CONTROLS

	Dependent variable: Cohort size			
	Baseline specification		Control for constituency development funds	
	District-school type (1)	School (2)	District-school type (3)	School (4)
Intensity × public school	776.2*** (153.2)	2.895*** (1.034)	785.3*** (158.9)	2.724*** (1.011)
Intensity × private school	-85.96 (118.0)	1.983 (1.382)	-117.3 (114.2)	1.88 (1.376)
Observations	1,104	139,519	1,024	137,447
R ²	0.99	0.26	0.99	0.27

	Dependent variable: Cohort size			
	Control for pre-program district unemployment × year		Control for changes in population size	
	District-school type (5)	School (6)	District-school type (7)	School (8)
Intensity × public school	692.4*** (173.4)	2.961*** (1.085)	775.0*** (152.5)	2.903*** (1.032)
Intensity × private school	-169.7 (133.8)	2.052 (1.410)	-87.15 (117.2)	1.99 (1.381)
Observations	1,104	139,519	1,104	139,519
R ²	0.99	0.26	0.99	0.26

Notes: Standard errors clustered at the district level appear in parentheses. All regressions include district times school type, public school, and year dummy variables and district linear trends. Even columns include urban and boarding school dummy variables. Column 1: Table 2, column 1. Column 2: Table 2, column 2. Columns 3 and 4: percent of households in a district below the poverty line in 1997 (calculated from the 1997 WMS) times year as an additional control. Columns 5 and 6: district unemployment level (calculated from the 1999 Census) times year dummy variables as additional controls. Columns 7 and 8: number of 14-year-olds in a district (calculated from the 1999 census) as an additional control.

***Significant at the 1 percent level.

**Significant at the 5 percent level.

*Significant at the 10 percent level.

TABLE 9—ROBUSTNESS—KCPE SCORE

Percentile	Dependent variable: Standardized KCPE score			
	Baseline specification (1)	Control for constituency development funds (2)	Control for pre-program district unemployment × year (3)	Control for changes in population size (4)
50	0.007 (0.010)	0.006 (0.009)	0.002 (0.011)	0.008 (0.010)
60	-0.006 (0.010)	-0.007 (0.009)	-0.012 (0.010)	-0.007 (0.010)
70	-0.022 (0.013)	-0.019 (0.013)	-0.026 (0.013)	-0.020 (0.013)

(Continued)

TABLE 9—ROBUSTNESS—KCPE SCORE (*Continued*)

Dependent variable: Standardized KCPE score				
Percentile	Baseline specification (1)	Control for constituency development funds (2)	Control for pre-program district unemployment \times year (3)	Control for changes in population size (4)
75	-0.028* (0.016)	-0.025 (0.016)	-0.034* (0.016)	-0.029* (0.016)
80	-0.036* (0.019)	-0.031* (0.019)	-0.042* (0.020)	-0.036* (0.020)
90	-0.044 (0.028)	-0.040 (0.027)	-0.051 (0.028)	-0.046 (0.028)
95	-0.051 (0.033)	-0.044 (0.031)	-0.057 (0.033)	-0.051 (0.034)

Dependent variable: Standardized KCPE score				
Percentile	Control for pre-program school quality			
	Average district score 2000–2002 \times year (5)	Percentage with less than grade 1 \times year (6)	Control for province \times year (7)	Fake FPE program in 2002 (2000–2002 data only) (8)
50	0.010 (0.011)	0.001 (0.010)	0.008 (0.008)	0.095*** (0.009)
60	-0.002 (0.0120)	-0.013 (0.010)	-0.006 (0.009)	0.098*** (0.010)
70	-0.020 (0.016)	-0.029** (0.014)	-0.020 (0.014)	0.098*** (0.011)
75	-0.026 (0.018)	-0.033** (0.016)	-0.026 (0.017)	0.097*** (0.011)
80	-0.036* (0.020)	-0.041** (0.020)	-0.036* (0.020)	0.089*** (0.012)
90	-0.046* (0.027)	-0.050* (0.029)	-0.044 (0.028)	0.083*** (0.014)
95	-0.041 (0.034)	-0.058* (0.034)	-0.051 (0.033)	0.072*** (0.02)

Notes: Standard errors clustered at the district level appear in parentheses. Estimates are from a changes-in-changes model. See text for additional details. Additional controls as in Tables 8, 10, and 11.

***Significant at the 1 percent level.

**Significant at the 5 percent level.

*Significant at the 10 percent level.

and therefore would have higher $intensity_{jt}$. As a result, we could overestimate the negative impact of the FPE on achievement. To address this concern, we use the average district-level pre-FPE test score as a measure of initial district-level school quality. We interact this quality measure with year dummies and include them as controls in our regressions. Assuming that the pre-FPE test score is a good measure of quality, this interaction allows districts of different initial school quality to have different trajectories. We find that our results are robust to the inclusion of

TABLE 10—ROBUSTNESS—ADDITIONAL CONTROLS

	Dependent variable: Cohort size						
	Baseline specification		Control for pre-program school quality				Balanced school panel (7)
			Average district score 2000–2002 × year		Percentage with less than grade 1 × year		
	District- school type (1)	School (2)	District- school type (3)	School (4)	District- school type (5)	School (6)	
Intensity × public school	776.2*** (153.2)	2.895*** (1.034)	776.3*** (154.0)	2.873*** (1.026)	679.5*** (137.0)	2.547** (1.030)	
Intensity × private school	–85.96 (118.0)	1.983 (1.382)	–85.88 (119.1)	1.945 (1.377)	–182.7* (108.3)	1.636 (1.392)	0.091 (1.575)
Observations	1,104	139,519	1,104	139,519	1,104	139,519	116,256
R ²	0.99	0.26	0.99	0.26	0.99	0.26	0.30

Notes: Standard errors clustered at the district level appear in parentheses. All regressions include district times school type, public school, and year dummy variables and district linear trends. Even columns include urban and boarding school dummy variables. Column 1: Table 2, column 1. Column 2: Table 2, column 2. Columns 3 and 4: average district KCPE score (from 2000–2002) times year dummy variables as additional controls. Columns 5 and 6: percentage of 16–18-year-olds in a district who did not complete grade 1 (calculated from 1999 census) times year dummy variables as additional controls. Column 7: sample limited to those schools that have test takers for all years 2000–2007.

***Significant at the 1 percent level.

**Significant at the 5 percent level.

*Significant at the 10 percent level.

these controls, suggesting that differences in initial school quality do not drive our results (columns 3 and 4 of Table 10 and column 5 of Table 9). In a district with low quality schools, KCPE scores might not reflect the quality of instruction because students might not start school. As an alternative measure of quality, from the 1999 census, we calculate the percentage of 16–18-year-olds in a district that did not complete grade 1 and interact this with year dummy variables. Columns 5 and 6 of Table 10 and column 6 of Table 9 show that our results are robust to the inclusion of these controls.

The entry of new schools over this period could affect the school-level results. As a robustness check we re-estimate the coefficients using a balanced panel of schools. We find that the results in column 7 of Table 10 are not significantly different from our baseline.

As additional checks in columns 3 and 4 of Table 11, we include the interaction of district dummy variables with an indicator for “post-FPE” ($t \geq 2003$) to control for endogeneity in the change in the initial level associated with our effective intensity.¹⁹ Thus, we are identifying the effect of FPE off of the value of $intensity_{jt}$ from 2003 to 2007, controlling for the level change from 2002 to 2003. In columns 5 and 6 of Table 11 and column 7 of Table 9, we include interactions between province dummy variables and year dummy variables to control for any differences between provinces from one year to the next. In these specifications, we are identifying the FPE effect from differences in the dropout pattern between districts within

¹⁹ We are not able to perform this check on our KCPE score results because our measure of intensity does not vary by time in the changes-in-changes framework.

TABLE 11—ROBUSTNESS—ADDITIONAL CONTROLS

	Dependent variable: Cohort size			
	Baseline specification		Control for district \times post	
	District-school type (1)	School (2)	District-school type (3)	School (4)
Intensity \times public school	776.2*** (153.2)	2.895*** (1.034)	728.5*** (164.3)	1.948* (1.087)
Intensity \times private school	-85.96 (118.0)	1.983 (1.382)	-133.7 (113.0)	1.059 (1.325)
Observations	1,104	139,519	1,104	139,519
R^2	0.989	0.262	0.99	0.26
	Dependent variable: Cohort size			
	Control for province \times year		Fake FPE Program in 2002 (2000–2002 data only)	
	District-school type (5)	School (6)	District-school type (7)	School (8)
Intensity \times public school	672.2*** (176.3)	1.720* (0.951)	219.9 (543.4)	-2.759 (4.101)
Intensity \times private school	-190.0 (122.5)	0.804 (1.080)	-672.4 (536.0)	-5.6950 (4.126)
Observations	1,104	139,519	414	47,133
R^2	0.99	0.26	0.997	0.29

Notes: Standard errors clustered at the district level appear in parentheses. All regressions include the district times school type, year, and public school dummy variables and district linear trends. Column 1: Table 2, column 1. Column 2: Table 2, column 2. Columns 3 and 4: district dummy variables time post as additional controls. Columns 5 and 6: province dummy variables times year dummy variables as additional controls. Columns 7 and 8: fake program start date of January 2002 when no program took place.

***Significant at the 1 percent level.

**Significant at the 5 percent level.

*Significant at the 10 percent level.

the same province. Further, these variables help control for any differences that may be driven by the change in the flow of resources to different regions as a result of the change in political regimes in 2003. Overall, we find that the results are robust to inclusion of all these controls. Finally, in columns 7 and 8 of Table 11 and column 8 of Table 9, we examine the impact of a fake intervention in 2002, when no actual intervention took place, and replicate our analysis using data from 2000 to 2002 only. We find no impact of the fake program on the number of students, and our CIC estimates find positive impact of the program on test scores, suggesting that our results are not spurious.

VI. Conclusions

At the start of the 2003 school year, Kenya's Free Primary Education (FPE) program eliminated school fees in all public primary schools. We find that this program increased the number of students who took the primary school exit exam (the KCPE) in public schools, reflecting an increase in educational access. The

program especially increased primary school access for poorer students, and increased the demand for private schools in districts with greater inequality. The public school capacity, as measured by the number of teachers or school construction, did not increase, leading to the deterioration of the student to teacher ratio in public schools. We do find that the supply of private schools increased in response to the program.

In contrast to the media reports that raised concerns about FPE overwhelming the public school system, we find, based on our changes-in-changes estimates, that the program had only a small negative impact on the achievement of students who would have completed primary school even in the absence of FPE. We hypothesize that the adverse effects of FPE were mitigated by the substantial private school supply response. Overall our findings suggest that in the short run, the FPE program improved welfare, as it led to significantly increased primary school completion with only a modest reduction in test scores among those who would have completed primary school in the absence of the program. However, the long-run (or full) impact of the program will not be known until we are able to observe students whose entire primary experience occurred during the FPE regime. The estimated effects presented in this paper are relevant for students who entered primary school in or before 2000, three years prior to FPE. Additionally, as the effects of declining school input quality and the equilibrium level of sorting may not be observed for some time, FPE could have a very different impact in the long run. Therefore, appropriate policies that encourage continued quality and leverage the private school market should be implemented to ensure that educational quality is not compromised in the long run.

APPENDIX

TABLE A1—CALCULATION OF INTENSITY FOR HOMA BAY

Grade in 1999, proxy for year prior to FPE (1)	Number of students who completed grade (2)	Currently in grade (3)	Of students who completed grade, those who eventually dropped out (4)	Intensity (5)	Relevant KCPE year (6)
8	367	261			
7	707	368	79	0.30	2003
6	1,203	389	207	0.79	2004
5	1,700	405	315	1.21	2005
4	2,185	469	395	1.51	2006
3	2,721	580	462	1.77	2007

Notes: Columns 2 and 3: From the 1999 census. Column 4: Number of students who completed the grade minus those still in primary school and those who completed primary school. Column 5: Column 4 divided by the number of students in grade 8 in 1999.

TABLE A2—INTENSITY BY YEAR AND DISTRICT

District	Intensity							
	2000	2001	2002	2003	2004	2005	2006	2007
Baringo	0.00	0.00	0.00	0.13	0.31	0.43	0.60	0.78
Bomet	0.00	0.00	0.00	0.26	0.77	1.20	1.63	2.01
Bondo	0.00	0.00	0.00	0.38	0.91	1.37	1.79	2.25
Bugoma	0.00	0.00	0.00	0.27	0.67	0.99	1.31	1.62
Buret	0.00	0.00	0.00	0.34	0.78	1.11	1.37	1.58
Busia	0.00	0.00	0.00	0.32	0.88	1.41	1.90	2.51
Butere/Mumias	0.00	0.00	0.00	0.29	0.90	1.52	2.09	2.68
Central Kissi	0.00	0.00	0.00	0.15	0.40	0.62	0.83	1.13
Embu	0.00	0.00	0.00	0.31	0.68	1.08	1.40	1.71
Garissa	0.00	0.00	0.00	0.31	0.55	0.80	1.14	1.52
Homa Bay	0.00	0.00	0.00	0.30	0.79	1.21	1.51	1.77
Isiolo	0.00	0.00	0.00	0.23	0.79	1.28	1.77	2.36
Kajiado	0.00	0.00	0.00	0.37	0.86	1.32	1.82	2.25
Kakamega	0.00	0.00	0.00	0.25	0.68	1.10	1.45	1.91
Keiyo	0.00	0.00	0.00	0.16	0.34	0.46	0.61	0.76
Kericho	0.00	0.00	0.00	0.35	0.93	1.39	1.89	2.39
Kiambu	0.00	0.00	0.00	0.23	0.45	0.64	0.82	1.00
Kilifi	0.00	0.00	0.00	0.29	0.75	1.21	1.69	2.38
Kirinyaga	0.00	0.00	0.00	0.21	0.61	0.95	1.28	1.57
Kisumu	0.00	0.00	0.00	0.48	1.02	1.48	1.82	2.10
Kitui	0.00	0.00	0.00	0.12	0.37	0.67	0.96	1.27
Koibatek	0.00	0.00	0.00	0.14	0.38	0.64	0.81	0.99
Kuria	0.00	0.00	0.00	0.29	0.82	1.20	1.60	2.13
Kwale	0.00	0.00	0.00	0.26	0.77	1.37	2.12	3.03
Laikipia	0.00	0.00	0.00	0.13	0.36	0.58	0.81	1.03
Lamu	0.00	0.00	0.00	0.40	1.52	2.80	4.04	5.00
Lugari	0.00	0.00	0.00	0.29	0.76	1.11	1.57	2.03
Machakos	0.00	0.00	0.00	0.16	0.41	0.72	1.00	1.26
Makueni	0.00	0.00	0.00	0.13	0.39	0.69	0.95	1.26
Malindi	0.00	0.00	0.00	0.71	1.76	2.97	4.38	5.84
Mandera	0.00	0.00	0.00	0.29	0.57	0.89	1.16	1.63
Maragua	0.00	0.00	0.00	0.24	0.58	0.91	1.24	1.59
Marakwet	0.00	0.00	0.00	0.14	0.35	0.57	0.81	0.99
Marsabit	0.00	0.00	0.00	0.18	0.68	1.07	1.64	2.11
Mbeere	0.00	0.00	0.00	0.15	0.43	0.75	0.98	1.24
Meru Central	0.00	0.00	0.00	0.15	0.37	0.57	0.85	1.06
Meru North	0.00	0.00	0.00	0.27	0.87	1.55	2.28	3.28
Meru South	0.00	0.00	0.00	0.17	0.39	0.60	0.85	1.15
Migori	0.00	0.00	0.00	0.53	1.32	1.94	2.38	2.77
Mombasa	0.00	0.00	0.00	0.50	1.05	1.55	1.95	2.39
Moyale	0.00	0.00	0.00	0.29	0.88	1.76	2.29	3.12
Mt Elgon	0.00	0.00	0.00	0.38	0.86	1.38	1.82	2.15
Muranga	0.00	0.00	0.00	0.10	0.28	0.46	0.64	0.78
Mwingi	0.00	0.00	0.00	0.12	0.38	0.77	1.19	1.66
Nairobi	0.00	0.00	0.00	0.51	1.01	1.43	1.81	2.18
Nakuru	0.00	0.00	0.00	0.30	0.64	0.97	1.22	1.50
Nandi	0.00	0.00	0.00	0.17	0.49	0.84	1.18	1.50
Narok	0.00	0.00	0.00	0.42	1.14	1.70	2.27	2.93
Nyamira	0.00	0.00	0.00	0.20	0.52	0.79	1.09	1.39
Nyandaura	0.00	0.00	0.00	0.27	0.57	0.85	1.15	1.42
Nyando	0.00	0.00	0.00	0.29	0.67	0.98	1.23	1.47
Nyeri	0.00	0.00	0.00	0.16	0.37	0.54	0.68	0.83
Rachuonyo	0.00	0.00	0.00	0.29	0.65	0.94	1.25	1.48
Samburu	0.00	0.00	0.00	0.52	0.91	1.48	2.09	3.17
Siaya	0.00	0.00	0.00	0.33	0.87	1.35	1.81	2.27
South Kissi	0.00	0.00	0.00	0.21	0.48	0.75	0.98	1.22
Suba	0.00	0.00	0.00	0.49	1.18	1.76	2.11	2.57

(Continued)

TABLE A2—INTENSITY BY YEAR AND DISTRICT (Continued)

District	Intensity							
	2000	2001	2002	2003	2004	2005	2006	2007
Taita Taveta	0.00	0.00	0.00	0.22	0.55	0.73	1.01	1.18
Tana River	0.00	0.00	0.00	0.18	0.50	1.16	2.16	2.89
Teso	0.00	0.00	0.00	0.15	0.59	1.01	1.43	1.90
Tharaka	0.00	0.00	0.00	0.20	0.48	1.02	1.68	2.59
Thika	0.00	0.00	0.00	0.22	0.50	0.73	0.95	1.18
Trans Mara	0.00	0.00	0.00	0.36	0.88	1.53	2.28	3.24
Trans-Nzoia	0.00	0.00	0.00	0.29	0.71	1.10	1.51	1.95
Turkana	0.00	0.00	0.00	0.46	0.90	1.18	1.56	2.41
Uasin-Gishu	0.00	0.00	0.00	0.24	0.57	0.94	1.26	1.58
Vihiga	0.00	0.00	0.00	0.23	0.63	0.99	1.31	1.65
Wajir	0.00	0.00	0.00	0.30	0.61	0.87	1.26	1.64
West-Pokot	0.00	0.00	0.00	0.27	0.84	1.55	2.24	3.33
Minimum	0.00	0.00	0.00	0.10	0.28	0.43	0.60	0.76
Maximum	0.00	0.00	0.00	0.71	1.76	2.97	4.38	5.84
Average	0.00	0.00	0.00	0.28	0.69	1.11	1.52	1.96
Median	0.00	0.00	0.00	0.27	0.67	1.02	1.40	1.71

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