

The Impact of Secondary Schooling in Kenya

A Regression Discontinuity Analysis

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Abstract

This paper estimates the impacts of secondary school on human capital, occupational choice, and fertility for young adults in Kenya. The probability of admission to government secondary school rises sharply at a score close to the national mean on a standardized 8th grade examination, permitting the estimation of causal effects of schooling in a regression discontinuity framework. The analysis combines administrative test score data with a recent survey of young adults

to estimate these impacts. The results show that secondary schooling increases human capital, as measured by performance on cognitive tests included in the survey. For men, there is a drop in the probability of low-skill self-employment, as well as suggestive evidence of a rise in the probability of formal employment. The opportunity to attend secondary school also reduces teen pregnancy among women.

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The Impact of Secondary Schooling in Kenya: A Regression Discontinuity Analysis*

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1 Introduction

The expansion of schooling in Sub-Saharan Africa over the last 50 years has made basic education more accessible to many of the world's poorest: between 1970 and 2005, average schooling attained by young Africans rose from 2.6 years to 6.1 years, and continues to grow.¹ Increases in educational participation and attainment have coincided with rising literacy and formal sector employment across the continent, though the direction of causality is not clear. Wage returns to education have been shown in other developing country contexts (Duflo 2001), but similar patterns have not been demonstrated as convincingly in Africa. One complicating factor is that rates of employment are quite low. In 2008, for example, only 38 percent of Kenyan men were employed by someone outside their family.² In this context, the effects of education on human capital accumulation, occupational choice, and fertility decisions may in fact be more socially relevant measures of the returns to schooling than wage effects.

Most empirical studies find little evidence that African schools have positive effects on outcomes. Two recent papers find no positive academic effects at all: Lucas and Mbiti (2014) show that admission to higher quality secondary schools in Kenya neither raises the probability of completing secondary school, nor increases 12th grade test scores; de Hoop (2010) estimates that admission to a higher quality secondary school in Malawi increases the probability of remaining enrolled in an assigned school, but has no effect on test scores. These studies, however, only measure the change in academic per-

¹Source: Barro and Lee (2010), tabulation based on 33 countries in sub-Saharan Africa.

²Source: DHS (2009). This includes all age groups and covers both rural areas and urban centers.

formance brought about by increases in secondary school quality. One might reasonably expect the effect of attending *any* secondary school to differ from the effect of increased school quality. The rise in primary school completion associated with the achievement of the Millennium Development Goals means that a large cohort is about to reach the age of secondary schooling, which until now has been rationed in much of Sub-Saharan Africa. Despite the policy urgency of this issue, few studies to date have identified the effect of relaxing this constraint: the impact of secondary schooling on the marginal student in low-income country contexts.³ A rare exception, Filmer and Schady (2014), found that scholarship-driven attendance of lower secondary school in Cambodia had no effect on either test scores or fertility choices.

In this paper, I use a regression discontinuity approach to estimate the impacts of secondary schooling in Kenya. The discontinuity I use is based on a standardized 8th grade test, the Kenya Certificate of Primary Education (KCPE). Probability of admission to government secondary school rises sharply at a cutoff score close to the national mean on the examination. I collect an administrative KCPE dataset, and combine it with a recent, detailed survey of young adults in Kenya that includes educational attainment, along with a number of other outcomes. With these two datasets, I use a technique from time series econometrics to identify the structural breaks in patterns of secondary school completion, thereby locating the test score cutoffs in Kenya’s secondary school admission policy. I am able to confirm that the

³Lucas and Mbiti (2012) find that the increased school participation in Kenya brought about by the abolition of *primary* school fees actually reduces average test scores, with composition effects explaining less than half the decline. This, however, is a very different population from those who are on the margin of attending secondary school.

KCPE score popularly perceived to constitute “passing” the examination is empirically the most important for boys, while a slightly lower cutoff is more relevant for girls. This is consistent with a recent survey of local secondary school administrators, who report lower admissions criteria for girls.

At the admissions cutoff, I find a 15 percent jump in the probability of completing high school. This is a large effect compared to many commonly used instruments for education. I perform relevant specification tests, and find that this effect is significant and stable across a range of specifications, bandwidths, controls, and sample restrictions.

Students on either side of the admissions cutoff are very similar demographically, and in a neighborhood of the test score cutoff, admission to secondary school is “as good as randomized” (Lee 2008). This allows me to treat the rise in schooling at the admissions cutoff as a source of exogenous variation for estimating the impact of secondary school. I find that completing secondary school has a substantial impact on human capital accumulation, as measured by performance on vocabulary and reasoning tests in adulthood. I estimate a performance improvement of 0.6 standard deviations attributable to the completion of secondary school. This is the first paper to show such positive effects of secondary schooling in Africa.

For labor market outcomes, I consider rates of employment and low-skill self-employment. I find clear causal effects: for men in their mid-twenties, completing secondary school decreases the probability of low-skill self-employment by roughly 50 percent. There is also suggestive evidence of a 30 percentage point increase in the probability of formal employment, though this is not significant in all specifications. It is important to note that

most self-employment in this context is not innovative entrepreneurship. Instead, it is what Lewis (1954) refers to as “casual labour” or “petty trade;” it is the transition away from this sector that marks economic development (Lewis 1954, p.189).

I also find that secondary schooling causes a sharp drop in the probability of teen pregnancy. Studies of the correlation between education and fertility have emphasized on a number of possible ramifications: human capital accumulation in the next generation, rates of population growth, and household bargaining, for example (Strauss and Thomas 1995). I establish a strong causal effect of secondary schooling on early fertility, opening an avenue for further study as this population grows older. While my estimates of the effect are relatively large, this sort of reduction is in accord with the findings of Ferré (2009) and Duflo, Dupas, and Kremer (forthcoming) in Kenya, as well as Baird, Chirwa, McIntosh, and Özler (2010) in Malawi. This contrasts with the recent work of McCrary and Royer (2011), who find that increases in educational attainment in the US induced by age-at-school-entry rules have no such impact; their instrument acts through a different channel on a different subpopulation, however, partially explaining the difference in findings.

Thus, I show large effects of secondary schooling on a number of important outcomes. A feature of this work, as compared to other recent studies on secondary schooling in Africa, is that I estimate impacts on the marginal student who attends secondary school. As a result, these estimates are directly interpretable as consequences of potential policy changes that would make secondary school rationing less restrictive. The magnitude of these effects in a population of this age suggests that permanent differences may be

revealed as this cohort grows older, opening a clear avenue for further study.

An additional contribution of this paper is to show whether cross-sectional analysis, controlling for available covariates, delivers estimates comparable to the causal effects estimated in the regression discontinuity design. Following the general approach of Altonji, Elder, and Taber (2005), I specifically explore the stability of cross-sectional results in the framework suggested by Oster (2015), with implications for other settings in which quasi-experimental variation may not be available.

The remainder of the paper is organized as follows: Section 2 provides a description of relevant facets of the Kenyan educational system, and the data I use for estimation.⁴ Section 3 explains the estimation strategies employed for different types of analysis. Section 4 presents the detailed specification checks I carry out and the results of my analysis, Section 5 discusses the robustness of cross-sectional analysis of the same questions, and Section 6 concludes.

2 Context and Data

Since 1985, the Kenyan education system has included eight years of primary schooling and four of secondary (Eshiwani 1990, Ferré 2009). At the end of primary school, students take a national leaving examination, the KCPE. A score of 50% or higher—currently 250 points out of 500—is considered to be a passing grade. This examination is the chief determinant of admission to secondary schools (Glewwe, Kremer, and Moulin 2009).

⁴The data appendix provides additional details on the assembly of these datasets.

Those who are not admitted to any government school may choose to re-take the examination the following year, or may consider schooling in Uganda, vocational education, or private schools with different standards. Though an official letter of admission to a government secondary school is rare below this cutoff, it is still not guaranteed for those above it because the number of candidates passing the KCPE may exceed the number of spaces available in public schools (Aduda 2008, Akolo 2008). Among those who are admitted to secondary school, however, many are still unable to afford tuition and assorted fees: while primary school has been inexpensive for many years, and was made nominally “free” in 2003, even the lowest-tier district secondary schools cost hundreds of dollars per year during the period observed in this study.⁵

2.1 Data: KLPS2 surveys

This admission rule suggests a fuzzy regression discontinuity design for estimating the impacts of secondary schooling. The primary dataset used in this study is the Kenyan Life Panel Survey (KLPS), an ongoing survey of respondents originally from Funyula and Budalangi Divisions of Busia District, Kenya (Baird, Hamory, and Miguel 2008). The respondents were sampled from the population attending grades 2 through 7 at rural primary schools in 1998. The first round of surveying (KLPS1) was carried out from 2003 to 2005, while the second (KLPS2) ran from 2007 to 2009, both times tracking respondents across provincial and even national boundaries. Because the

⁵Policy changes after 2008 made low-tier secondary schools considerably less expensive in Kenya.

outcomes of interest occur only for adult respondents, I mainly use the more recent round of survey data (KLPS2), treating it as cross-sectional data for 5,084 individuals.

The KLPS2 survey is comprehensive, including questions on education, employment, and fertility, as well as cognitive tests. The education section includes yearly school participation, from which secondary school completion, grade repetition, and other measures can be constructed; it also includes self-reported KCPE scores for students who complete primary school. The cognitive tests administered as part of the survey assess English vocabulary and non-verbal reasoning; the labor market section includes employment and self-employment history, including the dates and sectors of employment, as well as wages.⁶

In order to use a regression discontinuity design, I restrict analysis to respondents reporting a KCPE score in the survey, which reduces sample size from $N=5,084$ to $N=3,305$, including only pupils who complete primary school and take the KCPE. Table 1 shows summary statistics for the restricted KLPS2 sample. The 3,305 respondents reporting test scores have higher educational attainment, more educated parents, and lower teen pregnancy rates than the full sample; this is to be expected, since these are the respondents who did not drop out during primary school.

⁶Non-verbal reasoning is measured using Raven's Matrices, one of the more reliable measures of general intelligence (Cattell 1971); the vocabulary instrument is based on the Mill Hill test, originally designed by J. C. Raven to complement the Matrices.

2.2 Data: Test scores

While most KLPS variables are quite stable over survey rounds, self-reported KCPE scores are not. Grade in school in 1999, for example, has a correlation of 0.95 between responses given in KLPS1 and four years later in KLPS2, while self-reported test score has a correlation closer to 0.7. The noise in test scores could pose several problems, since I use KCPE score as the regression discontinuity running variable.⁷ Noise in the form of classical measurement error for only a random subset of the data would simply reduce the power of the regression discontinuity design. Classical measurement error in all of the data could eliminate the discontinuity entirely.⁸ On the other hand, non-classical error could invalidate the regression discontinuity design, if either mis-reporting or test repetition were driven by unobservables correlated with outcomes.⁹ A histogram of the self-reported scores, in the upper left panel of Figure 1, shows that the distribution of scores shows signs of non-classical error, in the form of manipulation of the reported scores around the “passing” point; a test for density smoothness proposed by McCrary (2008), shown in the lower left panel, rejects at this point.

This feature of the distribution could arise simply from repeated test-taking: if many of those who fail the test try again until they pass, the distribution of most recent test scores could include more mass just to the right of the cutoff than to the left.¹⁰ To see whether this phenomenon is solely responsible for the shape of the distribution, I consider a slice of the

⁷Some authors, such as Imbens and Lemieux (2008), refer to this as a “forcing variable.”

⁸More detailed discussion of these points is provided in Appendix A.2.

⁹See Martorell (2004) for discussion of multiple potential effects of test repetition.

¹⁰See Appendix A.2 for a concrete example.

data, available in KLPS1, in which respondents provided every test score for as many times as they had taken the KCPE. Even if the most recent test score is endogenous with respect to the respondent’s type and the location of the cutoff, the first test score should not be. Appendix Figure A2 shows that although the problem is less severe in this restricted sample, even these first scores do not have a smooth density at the discontinuity. However, administrative data on scores in the region display no such irregularity at the cutoff score, so I conclude that the self-reports are, in many cases, incorrect, and administrative data must be matched to the KLPS2 dataset in order to use a regression discontinuity design.¹¹

To complement the KLPS data, I gathered an auxiliary dataset of 17,384 official KCPE scores from District Education Offices and, when the district-level offices did not have the records, directly from primary schools. The official records I was able to collect in 2009 and 2010 include roughly 88 percent of the KLPS schools during the years of interest in this study.¹² Based on name, year, and school, I am able to cross-check KCPE scores for roughly 77 percent of the KLPS respondents who report taking the KCPE. While many self-reported scores are in accordance with the official records, there is substantial misreporting.¹³

Using the 88 percent coverage of the administrative data I could collect, a

¹¹I show the distribution of regional 2008 test scores in Appendix A.3.

¹²Every KLPS source school with missing records was visited at least once by me or another member of the data collection team; recent re-districting and political upheaval in Kenya, combined with local problems with record storage over the last 12 years, prevented the collection of the last 12 percent.

¹³I discuss the matching process and characterize misreporting in Appendix section A.1.5.

matching algorithm¹⁴ is used to identify corresponding administrative records for more than 2,500 of the 3,305 respondents reporting a score. For 2,273 respondents, I find exactly one test score; for 263 more, I find two scores in different (typically consecutive) years. Using the KLPS2 survey to determine whether matched scores are first or second attempts, I am able to clearly identify 2,167 first test scores.¹⁵ Their distribution is plotted in the upper right panel of Figure 1 and is tested for a density break in the lower right panel. I find no evidence of manipulation of administratively reported first test scores.

2.3 Gender-specific discontinuities

The KCPE cutoff for secondary school admission is well-known in Kenya; national media recently reported that “Out of the over 695,000 candidates who sat the KCPE examination, 350,000 candidates attained over 250 marks, making them eligible to join secondary school.”¹⁶ However, a survey of secondary schools in the area suggests that, though 250 is the modal 2009 cutoff score reported by school administrators, many competitive schools use higher cutoffs.¹⁷ Further, many schools report different cutoffs for boys and girls: seven out of eighteen reporting cutoff scores for girls report a value below 250. As such, 250 may not be the cutoff where the largest fraction of girls are

¹⁴The procedure is described in Appendix A.1.5.

¹⁵For some cases where I observe only one test score, it either appears to be a second score, or it is unclear whether it is a first or second score. I exclude these when using only first test score.

¹⁶Excerpted from Akolo (2008).

¹⁷Edward Miguel and Matthew Jukes, unpublished data (2009).

exogenously induced to attend secondary schools.¹⁸ To address these, I apply a technique from the structural break literature, following Card, Mas, and Rothstein (2008): I first restrict attention to a window of scores between 150 and 350 points on the KCPE exam; I then regress the outcome (completing secondary school) on indicators for hypothetical discontinuities from 200 to 300 points and a piecewise linear control for KCPE score, one potential discontinuity at a time, separately for men and women. For each sex, I consider the discontinuity whose regression produces the highest value of R^2 to be the “true” cutoff. Results are shown in Figure 2. For men, the R^2 -maximizing cutoff is 251 points rather than 250 (a close second place). For women, the best cutoff in this sense is 234 points. Considering these to be the “true” discontinuities, I use these values for the cutoff, c , in the specification checks for the first stage and in the estimation that follows.¹⁹

¹⁸Because the recent survey only included 2009 cutoffs, I re-visited secondary schools to find out their history of admissions rules, but current school administrations were not able to provide records of admissions rules covering the period of study in this paper.

¹⁹Several features of this process are worth noting. Prior to Card, Mas, and Rothstein (2008), this technique was also used in the context of schooling by both Kane (2003) and Chay, McEwan, and Urquiola (2005). Estimation of the location of the discontinuity, in the presence of a discontinuity, is super-consistent (Hansen 2000), and the error is not asymptotically normally distributed; this is also evident in Monte Carlo simulations using a data generating process designed to mimic the one I estimate here. Sampling error in the location of the discontinuity can be ignored in estimation of the magnitude of the discontinuity, so standard errors in subsequent estimation need not be adjusted (Card, Mas, and Rothstein 2008). I use the same data for estimating the location of the discontinuity as for estimating the impact on outcomes; Card, Mas, and Rothstein (2008) have a much larger sample, and are able to use half the data to locate the discontinuity, and the other half to estimate the rest of their model. Since my use of the data could create an endogeneity concern, I carry out robustness checks (selected checks shown in the Appendix) with the highest discontinuity for women below 250 reported by any surveyed secondary school in the region—240 rather than 234—and the *ex ante* cutoff of 250 rather than 251 for men. I obtain similar empirical results, though the first stage loses power substantially for women.

3 Empirical Strategy

Consider an equation characterizing the causal relationship between whether an individual completes secondary school, Sec_i , and outcome Y_i :

$$Y_i = \pi_0 + \pi_1 Sec_i + \pi_2 KCPE_i + \pi_3 X_i + \varepsilon_i \quad (1)$$

Equation 1 controls for academic ability, proxied by KCPE score, $KCPE_i$; other observable individual characteristics, X_i ; and both a constant term π_0 and idiosyncratic error ε_i . Direct application of OLS to equation 1 may lead to biased estimates of π_1 for the usual reasons: measurement error in educational attainment could bias coefficients downwards, while any positive correlation between ε_i and Sec_i , perhaps due to unobserved ability, could bias estimates upwards (Griliches 1977, Card 2001).

Instead, I use a regression discontinuity approach to identify the effect of secondary school on outcomes. As described in Section 2, Kenyan students who take the primary school leaving examination (KCPE) face an admission rule: below a cutoff score, c_i , it is more difficult to gain admission to secondary school. The identifying assumptions in my analysis are that all other outcome-determining characteristics except for the probability of secondary school attendance vary smoothly near the cutoff, and that outcomes change at the cutoff only because of the induced change in schooling. Because the probability of attendance does not jump from zero to one, this is a “fuzzy” regression discontinuity (Imbens and Lemieux 2008), so the causal effect of

secondary school on outcomes is:

$$\tau_{FRD} = \frac{\lim_{k \downarrow c_i} E[Y|KCPE = k] - \lim_{k \uparrow c_i} E[Y|KCPE = k]}{\lim_{k \downarrow c_i} E[Sec|KCPE = k] - \lim_{k \uparrow c_i} E[Sec|KCPE = k]} \quad (2)$$

As long as the order of polynomial in the running variable and the data window are the same for the first and second stage outcomes, estimation of τ_{FRD} in equation 2 is equivalent to an instrumental variables approach, where the first and second stages are:

$$Sec_i = \alpha_0 + \alpha_1 Above_i + \alpha_2 K_i + \alpha_3 K_i \cdot Above_i + \alpha_4 X_i + \zeta_i \quad (2a)$$

$$Y_i = \beta_0 + \beta_{FRD} \widehat{Sec}_i + \beta_2 K_i + \beta_3 K_i \cdot Above_i + \beta_4 X_i + \xi_i \quad (2b)$$

In equations 2a and 2b, I use normalized KCPE scores, $K_i = KCPE_i - c_i$, shifted so that the discontinuity occurs at $K_i = 0$; the variable $Above_i$ is equal to 1 if $K_i \geq 0$, and 0 otherwise; the parameter of interest is β_{FRD} ; I allow the relationship between Y_i and K_i to have different slopes on either side of the discontinuity. This is an estimation based on *compliers*, the population who would not complete secondary school if they had scored below the cutoff, but who would if they score above it. The estimated effect is a local average treatment effect at the point in the test score distribution where the cutoff falls. By definition, it is the policy-relevant cutoff for a policy change that would consider moving the cutoff slightly and changing the number of available slots in secondary schools. In this case, however, the cutoff also falls very near the median (and mean) of the test score distribution, which suggests that the effects I measure are relevant for the median Kenyan KCPE-taker,

rather than for outliers in the education or skill distribution.²⁰

3.1 Other estimation approaches

In the case of binary outcome variables, such as whether a respondent is pregnant by age 18, a nonlinear instrumental variables approach may be appropriate. In particular, I consider the IV probit, with the same first stage given in equation 2a, but with second stage:

$$\Pr [Preg18_i = 1] = \Phi \left(\gamma_0 + \gamma_{FRD} \widehat{Sec}_i + \gamma_2 K_i + \gamma_3 K_i \cdot Above_i + \gamma_4 X_i \right) \quad (3)$$

The IV probit estimation procedure is only correctly specified when the first stage residuals are asymptotically normally distributed, and when the first stage is linear.²¹ An alternative, when the first stage outcome is binary, is the bivariate probit (Maddala 1983):²²

$$Sec_i = \mathbb{1} (\delta_0 + \delta_1 Above_i + \delta_2 K_i + \delta_3 K_i \cdot Above_i + \delta_4 X_i + \tau_i > 0) \quad (4)$$

$$Y_i = \mathbb{1} (\phi_0 + \phi_1 Sec_i + \phi_2 K_i + \phi_3 K_i \cdot Above_i + \phi_4 X_i + \omega_i > 0) \quad (5)$$

This approach uses Sec_i rather than $\widehat{Sec}_i$ in the second stage, because it explicitly models endogeneity through the correlation, ρ , between τ_i and ω_i .

²⁰By contrast, many US studies relying on date-of-birth identification strategies are focused on relatively low-achieving students; studies such as the work of Saavedra (2008) in Colombia estimate the returns only to the highest-quality universities. Neither class of coefficient is necessarily relevant for the bulk of the population.

²¹A binary endogenous regressor would typically not yield asymptotically normal residuals.

²²Maddala (1983) presents the model on pp. 122-3; Greene (2007) discusses the model further on pp.823-6; Wooldridge (2002) also discusses it on p.478.

I follow Greene (2007) and others in imposing a bivariate normal distribution on the error terms. Though in practice, IV probit and bivariate probit yield marginal effects estimates that are often quite similar to those given by 2SLS, they have the advantage that, when correctly specified, they can provide greater statistical power when the probability of an outcome variable is very close to either zero or one.²³ The cost of this power is additional distributional assumptions, however, so I present results from each of these estimation techniques, when appropriate.

4 Results

4.1 Specification: bandwidth and polynomial order

For the first stage, I consider a window of data symmetric about the discontinuity, and regress completion of secondary school on an indicator for scoring above the discontinuity and piecewise linear controls in test score. I plot the resulting estimates of the discontinuity magnitude in the left panel of Figure 3, as a function of the width of the data window; here, I scale down scores by a factor of 100 so that coefficient estimates in subsequent tables are read more easily. The discontinuity estimate fluctuates slightly, but remains significant and of similar magnitude no matter which bandwidth I use.²⁴ At each bandwidth, I carry out a specification test in which in addition to the discontinuity dummy and the piecewise linear controls, I include indicators

²³This can be shown in Monte Carlo simulations, for example.

²⁴Here I use the term *bandwidth* in the sense of Imbens and Lemieux (2008), Lee and Lemieux (2010), and others in the regression discontinuity literature to mean the window of data used for estimation; this is not a non-parametric regression; I do not weight data differently according to distance from the discontinuity.

for narrow-width bins of KCPE scores: 251-260, 261-270, et cetera.²⁵ I test these indicators for joint significance; if they are significant, I consider the piecewise linear first stage to be mis-specified. This test rejects for widths of 90 points and higher on either side of the discontinuity. The same is true when I include a piecewise quadratic control in test score. Thus, for the rest of this paper, I use a bandwidth of 80 points on either side of the discontinuity.²⁶ Finally, I use Akaike’s information criterion to confirm that the first-order polynomial control is sufficient: piecewise linear (as opposed to constant, quadratic, cubic, or quartic) is the “best” specification according to AIC for both the 80-point bandwidth and nearly all other bandwidths under consideration. I use the same bandwidth and order of polynomial (linear) in both the first and second stage estimation, so that I can simply use 2SLS both for estimation and standard errors.²⁷

I carry out validity tests of the smoothness assumption using observables, four of which are depicted graphically in Figure 4. Gender, age, and mother’s and father’s education vary smoothly at the boundary, with differences that are neither large enough to be important nor statistically significant. This contrasts with Urquiola and Verhoogen (2009), who show that schools’ responses to a class-size policy discontinuity in Chile can invalidate a regression discontinuity research design. While they find large and significant differences in parents’ education levels at the discontinuity (as well as sharp changes in

²⁵For this test, I follow Lee and Lemieux (2010) and Lee and McCrary (2009). The results are similar when I vary bin width, for example using a width selected by a leave-one-out cross-validation procedure.

²⁶Alternatively, I can use the procedure suggested by Imbens and Kalyanaraman (2012); this yields similar “optimal” bandwidths for most outcomes, though smaller bandwidths for a few. Results are largely unchanged.

²⁷ See, in particular, Lee and Lemieux (2010) Section 4.3.3.

the class size histogram near cutoffs), I find no such patterns here.²⁸

4.2 First stage: Discontinuity

The first stage discontinuity is shown in the upper-left pane of Figure 5, and in a regression framework in Table 2.²⁹ In Table 2, the discontinuity is estimated first with genders pooled (columns 1-3), then separately among men (columns 4-6) and women (columns 7-9). I show the results with and without a piecewise quadratic control and controls for other covariates: age, gender, parents' education levels, and cohort dummies. I cannot reject that the discontinuities for men and women are of the same magnitude, though the smaller point estimate for women is consistent with the lower overall level of secondary schooling for women in this setting. My preferred specifications are given in columns (2), (5), and (8), in which the discontinuity is measured as a 16-percentage-point change in the probability of completing secondary school for men; a 13 percent change for women, and a 15 percent change when

²⁸See Section A.1.5 and the right panels of Figure 1. In particular, while I cannot rule out all types of cheating on the KCPE, as in the Texas testing context investigated by Martorell (2004), none of the known mechanisms for cheating on the exam would permit endogenous sorting around the discontinuity.

²⁹In this case, because the data window constrains predictions to within the unit interval, a logit or probit specification yields marginal effects that are almost identical in magnitude and significance to the discontinuity estimated here in a linear probability model.

pooled.^{30 31} That controls do not substantially change the point estimate is unsurprising, given that they do not change significantly at the discontinuity.

When the estimation is carried out separately by gender, the discontinuity is significant for both men and women, but the F-statistic is now below the rule of thumb for weak instruments for the subsample of women (Stock and Yogo 2002)—though I cannot reject the equality of the discontinuities for men and women. However, because the model is just-identified, the weak-instruments bias towards OLS is not present (Angrist and Pischke 2009), though tests may not be correctly sized.

In the right panel of Figure 3, I show the estimated difference between the cumulative distribution functions for education of the populations on either side of the discontinuity. For each point in the right panel, I estimate a separate regression of the probability that respondents attain more than x years of education on a piecewise linear control and an indicator for the discontinuity; the plot shows the coefficients and confidence intervals on the discontinuity for each of these outcomes. The KCPE discontinuity

³⁰Decomposition as suggested by Gelbach (forthcoming) shows that the change in coefficient magnitude from column (7) to column (9) is mostly due to the inclusion of the covariate controls; the slightly larger standard error is brought about because of the inclusion of the piecewise quadratic in the running variable. A separate issue is that small fraction of the sample is still in school; this fraction varies slightly at the discontinuity, and as such, the completion of secondary schooling may be viewed as a censored outcome in the first stage, which could be the source of some bias. In practice, restricting the sample to respondents who are surveyed at least five years after they take the KCPE does not substantially alter the results.

³¹Also note that while I find a larger discontinuity for men than women, Uwaifo Oyelere (2010) found that variation in free primary education in Nigeria predicts years of education equally well for men and women. This could be because free primary school induces additional schooling at too young an age for womens' early marriage and fertility decisions to be relevant, and would have been especially true in the period when Nigeria's primary education system was first coming into existence, included in Uwaifo Oyelere's (2010) analysis.

as an instrument clearly predicts secondary schooling, and moreover, secondary school completion. The estimates, however, drop to insignificance when estimating the probability of attaining more than 12 years of schooling: the KCPE score that induces a marginal student to attend and complete secondary school does not induce the student to attend college.

4.3 Estimation of outcomes

4.3.1 Human capital

I begin with analysis of the impact of schooling on human capital. The KLPS2 survey includes a commonly used test of cognitive ability—a subset of Raven’s Progressive Matrices—and an English-language vocabulary test based on the Mill Hill synonyms test. Adaptations of both measures have been used internationally for several decades, and each captures different aspects of intelligence.³² I standardize both outcomes so that they are measured in terms of standard deviations in the KLPS2 population, and show both OLS and 2SLS results for a combined Z-score³³ and separately by test in Panel A of Table 3: completing secondary school improves performance on these tests by 0.6 standard deviations, with very similar estimates given

³²Though standardized to have mean zero and standard deviation one in the population, in Table 1 these two cognitive measures have positive mean and standard deviations slightly less than one, because these summary statistics are only shown for the sample with a restricted range of first KCPE scores. The “Matrices” are often considered to measure something akin to “fluid” intelligence, while the vocabulary test measures something more related to what specialists in the field call “crystallized” intelligence (Cattell 1971). The relationship of the two measures appears similar here to in other settings: in these data, as elsewhere (Raven 1989), their correlation is near 0.5.

³³The combined Z-score is equivalent to the “mean effect” of Kling, Liebman, and Katz (2007) when no data are unevenly missing and the estimation procedure is the same for both.

by 2SLS and (potentially biased) OLS.³⁴ This estimate is robust to the inclusion of controls (column 4), and when decomposed, is driven by the larger and more precisely estimated effect in vocabulary. The reduced form effect, roughly 0.1 standard deviations at the discontinuity, is shown in the upper right panel of Figure 5. To the extent that subsequent outcomes depend on a mixture of human capital and signaling, this is evidence that secondary schooling in Kenya does not play a purely signaling role: students measurably gain skills from schooling.³⁵

These results contrast with the recent work of Filmer and Schady (2014) in Cambodia, who show that increased secondary schooling has no impact on subsequent test scores, as well as the work of Lucas and Mbiti (2014), who show that increased quality of secondary schooling in Kenya (at higher discontinuities in KCPE score) has no impact on subsequent academic outcomes. This appears to be true even when the marginal student admitted into the school is not the worst student in the higher-quality school. A clue to reconciling Lucas and Mbiti’s findings with mine may lie in the recent work of Urquiola and Pop-Eleches (2010). Using a similar multiple-discontinuity design to estimate the returns to secondary school quality in Romania, they find

³⁴Note that this is an ideal OLS specification: it includes the KCPE score as a control, and restricts the sample substantially; more discussion of OLS and 2SLS agreement is provided in Section 5.

³⁵A pessimistic interpretation might hypothesize that the longer respondents have been out of school, the worse they perform on tests; since secondary schooling delays exit from school, the apparent positive effect is simply a delayed deterioration of human capital. The data do not support such an interpretation: the longer respondents have been out of school (and thus the older they are), the better they do on the tests administered during KLPS2; the coefficient is too small (around 0.02 standard deviations per additional year out of school) to explain an effect more than an order of magnitude larger; and the effect remains significant and of the same magnitude in both OLS and 2SLS after controlling for duration out of school.

very modest positive effects, around .04 standard deviations on an academic test. These effects are simply too small to be detectable in the Lucas and Mbiti (2014) study, and when compared with the results I show in Table 3, it is clear that attending *any* secondary school could simply have a much larger effect than increasing the quality of the secondary school. de Hoop (2010) also finds no positive effects of secondary school quality on a standardized test outcome in Malawi, but this is in keeping with the aforementioned studies. On the other hand, the Lucas and Mbiti (2012) finding that increased primary schooling actually reduced average performance on the KCPE exam is driven by the setting: the universal primary education policy they study couples an increase in years of schooling with increased enrollment. While test scores might have risen for some students who received more schooling, Lucas and Mbiti (2012) note that the class size and compositional changes overwhelm any positive effect on test scores. Reconciling the results here with those of Filmer and Schady is more difficult; similarly to their setting, additional completed grades are associated with between 0.2 and 0.3 standard deviations on the tests in the KLPS instrument. The LATE that Filmer and Schady estimate in Cambodia may be for lower-ability students, less able to benefit from additional schooling, but I cannot rule out that any other difference between the Cambodian and Kenyan contexts is responsible for the difference in findings.

4.3.2 Self-employment and employment

Next, I examine the impact of education on labor market outcomes. Because many of the younger respondents are still in school, and because men are

typically primary earners in Kenya, I consider only the oldest two cohorts of men for this analysis, so that the incapacitation effect of continued schooling does not dominate the patterns of interest.³⁶ According to 2008 Demographic and Health Survey (DHS) data, young men in Kenya without secondary school have a higher employment rate at age 20 than do men who complete secondary school, since the latter group has had less time to look for jobs. At roughly age 25 (the mean age of the older two male KLPS2 cohorts), DHS data show roughly equal employment rates in these two groups; as they grow older still, the better educated are more likely to be employed. I confirm exactly this pattern in KLPS2, shown in Panel A of Table 4, columns 1 and 2. OLS shows a fairly precise zero effect of secondary schooling on employment at this age. However, the regression discontinuity approach gives very different results: the coefficient on schooling is positive and significant depending on controls, shown in IV probit and bivariate probit specifications in columns 3-6. While 2SLS is positively signed, it is insignificant; this is in part because 2SLS is less efficient than estimation via IV probit and bivariate probit when the true model is nonlinear and the mean of the response variable is close to zero or one, as in this case.³⁷ Depending on the specification, I find a rise in employment of between 24 and 43 percent in response to secondary schooling.

Besides being employed by someone outside their family, many respon-

³⁶As shown in Panel C of Table 1, only 13 percent of the men in the oldest two cohorts are still in school, as compared to 44 percent in the younger four cohorts. Human capital effects of secondary school remain broadly similar when limiting the sample to respondents who were in standards 6 and 7 in 1998, though standard errors widen (predictably) with the lower sample size; results shown in Panel B of Table 3.

³⁷As a diagnostic, predicted values from 2SLS clearly lie outside the unit interval.

dents are self-employed. Of these, 88 percent have no employees: common self-employment occupations in KLPS2 include fishing, hawking assorted wares, and working as a “boda-boda” bicycle taxi driver. On the other hand, among the employed respondents, the degree of skill varies among unskilled (loader of goods onto vehicles), semi-skilled (factory worker, carpenter, mechanic), and high-skill professional occupations (electronics repair, teachers, and other government and NGO employees).

As in other labor market studies of relatively young men (Griliches 1977, Zimmerman 1992), I use sector of employment rather than wage to estimate the impact of secondary schooling. Clear patterns emerge when I measure the effect of education on (implicitly low-skill) self-employment, shown as a reduced form graph in the lower left panel of Figure 5, and presented in the second row of Table 4. While secondary education and self-employment are negatively associated in the cross-section (columns 1 and 2), the causal impact of secondary schooling on low-skill self-employment is much larger; marginal effects from IV probit and bivariate probit estimation are in broad agreement with the 2SLS coefficients: a 40-50 percent lower probability of being self-employed among those who go to secondary school because they pass the KCPE cutoff.

4.3.3 Fertility

While labor market outcomes are of interest for the men in this sample, fertility and health outcomes are of more importance for the women: women are less than half as likely to be employed as men in each of the six KLPS2

cohorts.³⁸

In a reduced form graph, shown in the lower right panel of Figure 5, and in Panel B of Table 4, I look at the probability of pregnancy by age 18 among female KLPS2 respondents. The association between secondary schooling and decreased early fertility is strong: in the last two columns, OLS shows a roughly twelve percentage point drop in teen pregnancy among secondary school finishers. While these are only cross-sectional associations, their sign agrees with associations seen in Colombia; Taiwan, China; and the United States; summarized by Schultz (1988). Two-stage least squares predicts outside the unit interval, since again, this is a low-probability outcome, so I use IV probit and bivariate probit estimation in the first four columns and find a near elimination of teen pregnancy among compliers at the discontinuity, robust to the inclusion of the usual controls.

This finding contrasts with the work of McCrary and Royer (2011), who find no conclusive effect of education on timing of womens' first births. As McCrary and Royer (2011) point out, however, their study is based on a manipulation of the age at school entry rather than the age at school exit, as is the case here. In effect, when a girl starts school one year earlier than her counterparts because her birthday falls before a cutoff date, she has one more year of education by the time she considers dropping out of school at a particular age, perhaps in relation to the legal minimum. Their date-of-birth instrument thus predicts educational attainment among those who, for the most part, do not go on to tertiary schooling and in fact stop schooling al-

³⁸At the discontinuity, men appear slightly less likely to be married by survey time, and women appear slightly more likely to be married, but neither effect is significant. Conditional on marriage, spouse education rises slightly at the discontinuity (as one might expect), but this effect is also statistically insignificant (results not shown).

most as soon as possible. However, if pregnancy in the McCrary and Royer (2011) population is timed in relation to age rather than schooling, such variation in educational attainment would have no effect. In my case, however, young teens are given or denied the opportunity to continue schooling (thus varying age at exit) at the KCPE discontinuity. The KCPE discontinuity only has an effect on those who choose to continue beyond primary education (delaying school exit), and who must be considering tradeoffs between continuing their education and raising a family. These may be higher ability students, relative to the Kenyan distribution, than are the McCrary and Royer (2011) respondents in relation to the US distribution. Thus, while they find essentially no impact of education on early fertility using variation in age at school entry, it may still be sensible that in contrast to their work, I find large effects. Filmer and Schady find no effect of additional schooling in Cambodia on pregnancy rates, but the non-effect in their setting may be due to the fact that the median respondent in their survey was still only 14 years old.

Other studies in Sub-Saharan Africa have found similar (though smaller) effects of schooling on teen pregnancy in relation to those I show here. Ferré (2009) finds that a policy shift reclassifying 8th grade from secondary to primary school increased the fraction of students reaching 8th grade, thereby reducing teen pregnancy by 10 percentage points in Kenya in the 1980s. Duflo, Dupas, and Kremer (forthcoming) observe a 1.5 percentage point reduction in teen childbearing in Kenya in response to a school uniform distribution program that helped girls stay in school; and Baird, Chirwa, McIntosh, and Özler (2010) find that a conditional cash transfer to bring dropouts back into

school reduces teen pregnancies by 5 percentage points in Malawi.

Since many of the secondary schools are single-sex, one interpretation could be that teens in secondary school simply see members of the opposite sex less frequently than they otherwise would, so lower rates of pregnancy follow. This interpretation is not supported by the data, though: when I categorize secondary schools as single-sex or mixed, I see no significant difference in the pregnancy decline across the two types of schools.³⁹

In Kenya, dropping out of school is more common among girls than boys, and is most pronounced once girls enter their teens (Kremer, Miguel, and Thornton 2009). This is closely linked to pregnancy: girls in the Kenyan schools are “required to discontinue their studies for at least a year”⁴⁰ if they become pregnant. Schooling and childbearing in Kenya are in practice nearly mutually exclusive, as is true in many other contexts (Field and Ambrus 2008). Though I am aware of no *rule* prohibiting teen mothers from returning to school—though rules of that sort exist in other Sub-Saharan countries (Ferré 2009)—teen mothers still face stigmatization in Kenyan primary and secondary schools (Omondi 2008), so even after giving birth, they are unlikely to continue their schooling. The practical mutual exclusivity of pregnancy and schooling means that high-ability girls at the discontinuity face a trade-off between attending secondary school and starting a family immediately; this policy may also differ from the policy environment in the US.

³⁹In the cross section, the reductions in teen pregnancy associated with going to the two types of schools are also similar and statistically indistinguishable: 9 percentage points for girls at mixed schools, and 10 percentage points for those who attend all-girls’ schools.

⁴⁰Excerpted from Ferré (2009), p. 5.

4.4 Interpretation of the discontinuity

Though the probability of secondary schooling changes sharply at that point, covariates do not. If the probability of non-government secondary schooling changed at the discontinuity, however, it could be interpreted differently. For example, in order to attend secondary school without attaining the cut-off score, students may choose to enroll in secondary school in Uganda, rather than Kenya. Less than five percent of the sampled respondents attend secondary school in Uganda, however, and at the discontinuity, there appears to be no jump in the probability of attending secondary school in Uganda.⁴¹

The discontinuity may also be interpreted as an increase in years of schooling rather than an increase in the probability of secondary school completion. This version of the first stage is shown in Appendix Table A2. This first stage is evident in all the same specifications as before, and the coefficient magnitudes are roughly four times larger, since the indicator for completing secondary school represented four years of schooling. Appendix Tables A3, A4, and A5 show the results under this first stage, and for the most part, the coefficients are simply four times smaller. This interpretation is misleading, however: while compliers at the discontinuity do gain approximately 0.16 standard deviations on the cognitive tests for each additional year of schooling (Appendix Table A3, columns 3 and 4), this is true because nearly all the compliers at the discontinuity gain exactly 4 years of schooling (right panel of Figure 3), and thus just above 0.6 standard deviations on the tests (Table 3, columns 3 and 4). The relevant policy experiment is not to extend

⁴¹The lack of a jump at the discontinuity is robust to the controls used throughout this paper; the point estimate is usually positive and between 0.005 and 0.013, but statistically indistinguishable from zero; results not shown.

secondary school by an additional year, but to change the cutoff so that a larger fraction of the population attends—and completes—secondary school. Nevertheless, results are largely robust to the alternative specification.

5 Cross-sectional analysis with key covariates

Though the present analysis turns on a clear source of quasi-experimental variation, not all contexts offer such opportunities. Without such variation, any analysis—of the impacts of schooling, in this case—is concerned with whether omitted variables may complicate the measurement or interpretation of empirical patterns.

The present study creates at least two opportunities for assessing the level of omitted variable or selection bias in a cross-sectional analysis. The first approach is to simply compare the RD estimates to OLS estimates. If they are in agreement, OLS may not be a bad approach in this setting. While the cross-sectional approach estimates the average treatment effect (ATE), and the regression discontinuity estimate instead estimates the local average treatment effect (LATE) only at the discontinuity, this comparison may still be informative.

The second approach is to measure whether the OLS estimates vary with the inclusion of controls. Following Altonji, Elder, and Taber (2005), the movement of point estimates with the inclusion of controls speaks to selection on observables; if one assumes an econometric similarity between selection on observables and selection on unobservables, the stability of estimates under the inclusion of controls speaks to its stability more generally. In this frame-

work, Oster (2015) has pointed out that the movements in R-squared are as important as movements in point estimates; intuitively, if the observables explain very little variation, they may simply be the wrong variables for the question at hand, and may not tell us much about the variation explained by the unobservables. I follow Oster (2015) and Gonzalez and Miguel (2014) in assessing the stability of OLS estimates.

In Tables 5, 6, 7, and 8, I show how the cross-sectional (OLS) coefficient changes over two sample restrictions, and the inclusion of KCPE score as a control. The first sample restriction is a restriction to KCPE test-takers; the second is the 80-point bandwidth of test scores near the cutoff.

The estimated relationship between secondary schooling and human capital (combined vocabulary and Raven’s Matrices score), shown in the first column of Table 5, is twice as large as the regression discontinuity estimate (and is statistically distinguishable from it). Restricting the sample in the second and third columns of the table brings the coefficient much closer to the regression discontinuity point estimate, and including KCPE score as a control reduces it even further (though none of columns 2, 3, and 4 is statistically distinguishable from the estimate in column 5). This pattern is consistent with OLS being biased by unobserved ability in column 3 as compared to column 4. A regularity across these specifications is that secondary schooling always appears to have an effect on subsequent test scores in this context.

Including a measure of earlier ability (KCPE) as a control increases the R^2 appreciably; another way of saying this is that the KCPE control itself has a T-statistic of 12.8 in this regression: KCPE score before secondary school-

ing is strongly predictive of the survey-based test score years later. Using the criterion suggested by Oster (2015), I test whether additional unobservables could drive the true cross-sectional effect to zero, and find that under the assumptions she suggests, unobservables would not change the finding that secondary schooling in Kenya increases subsequent human capital (test scores).⁴² Thus, having a pre-treatment measure of academic capabilities could be sufficient for (reasonably) unbiased analysis of impacts on learning outcomes.

When I turn to employment outcomes in Tables 6 and 7, the patterns are quite different. For both outcomes, the range of values provided by the cross-sectional point estimates does not include the point estimate from the regression discontinuity; however, for both outcomes, given the wide confidence interval in the regression discontinuity design, it is not statistically distinguishable from the OLS values.

In the case of employment, the cross-sectional relationship is always statistically indistinguishable from zero, in the case of self-employment, is always significantly negative. These regressions never have very high R^2 values, however, and the inclusion of the KCPE control does not change this by very much. As such, the bounding approach of Oster (2015) is very sensitive to reasonable choices of the maximum R^2 that inclusion of unobservables could yield. For both outcomes, the bounding approach cannot reject null effects or effects of the opposite sign with $R_{max} = 0.1$. However, with Oster’s suggested

⁴²I use Oster’s `psacalc` program in Stata, first with the R^2 scale-up factor of $\Pi = 1.3$, then with $R_{max} = 0.5$, following the suggestions in Oster (2015) in light of the discussion in Gonzalez and Miguel (2014); the Vocabulary + Raven’s outcome in the KLPS survey is constructed from relatively few items (compared to an hour-long test like the PIAT to which Oster refers).

approach (II, the ratio of maximum R-squared to the current R-squared, of 1.3), the self-employment pattern appears robust in the cross-section ($\delta > 1$). It is worth noting that an alternative control, respondent age, is much more predictive of employment outcomes than KCPE score is, but this control does not improve R^2 for self-employment at all. In sum, this does not lead to any instructive conclusions for non-experimental estimates of schooling impacts on the labor market in this setting; OLS and 2SLS are not in close accord, and R-squared is low, so the potential role of omitted variables still looms large.

For pregnancy by age 18, the pattern is striking, even if KCPE score is as unpredictable as for the employment outcomes. In every cross-sectional regression, secondary schooling and pregnancy by age 18 are almost exactly mutually exclusive. The low R^2 could lead to insignificance in the bounding approach, but the KCPE control actually makes the pattern stronger (as can be seen by comparing the first coefficient in column 3 to that in column 4), so the bounded β from Oster’s approach is even more negative than the original cross-sectional estimates.

6 Conclusion

Secondary schooling in Kenya has large effects on human capital, reducing low-skill self-employment and weakly increasing formal employment for older cohorts of young men by the time of the survey. Teen pregnancy is dramatically reduced by secondary schooling.

The discontinuity occurs at a highly policy-relevant position, near the

mean score on the national primary school leaving examination: perhaps as externally valid as a single “fuzzy” discontinuity could be. An expansion of secondary schooling that preserved the quality of secondary schools but reduced the minimum required score would be likely to bring about the effects I estimate on roughly 15 percent of the population near the discontinuity: the compliers. As governments (including Kenya’s) consider the expansion of secondary schooling against other policy options, this study should provide a useful guidepost for understanding the consequences of such an expansion, as long as the expansion does not substantially alter the characteristics of the schools.

The difference between the unambiguously positive human capital findings in this paper and the less cheery conclusions from other studies of education in Kenya suggest that increased school enrollment in Sub-Saharan Africa will have varying consequences, depending on how it is undertaken. The findings in this paper, and in other experimental and quasi-experimental papers, are contingent on the nature of the exogenous variation: the secondary school admission instrument I use, at the KCPE discontinuity, induces both a rise in secondary school completion, and a resulting delay in pregnancy among female compliers; a date-of-birth instrument in the US that also induces additional secondary education has no such effect, however, both because of the timing of the education effects and because of the underlying skills and preferences of compliers with the different instruments.

OLS and 2SLS do not always produce similarly signed effects in this analysis: cross-sectional analysis does not reveal the impact of secondary schooling on employment on this age, but in a causal framework, the pattern

emerges. In the cross-section, controlling for ability is clearly important for outcomes that are closely linked to ability, such as the human capital measure in this study. Ability, traditionally linked to academic tests, may not be nearly as useful a control for other outcomes, at least for the young Kenyan adults in this study.

This study also highlights a possible avenue for researchers interested in the consequences of education throughout sub-Saharan Africa: many countries have examinations much like the KCPE, with analogous cutoff rules for secondary school admission. An important caveat is that while some questions in survey data show a very high degree of reliability, this cannot be said for KCPE scores. Combining administrative test data with a rich follow-up survey overcomes this obstacle, and may yield novel findings establishing causal links between education, fertility, and labor markets throughout the developing world.

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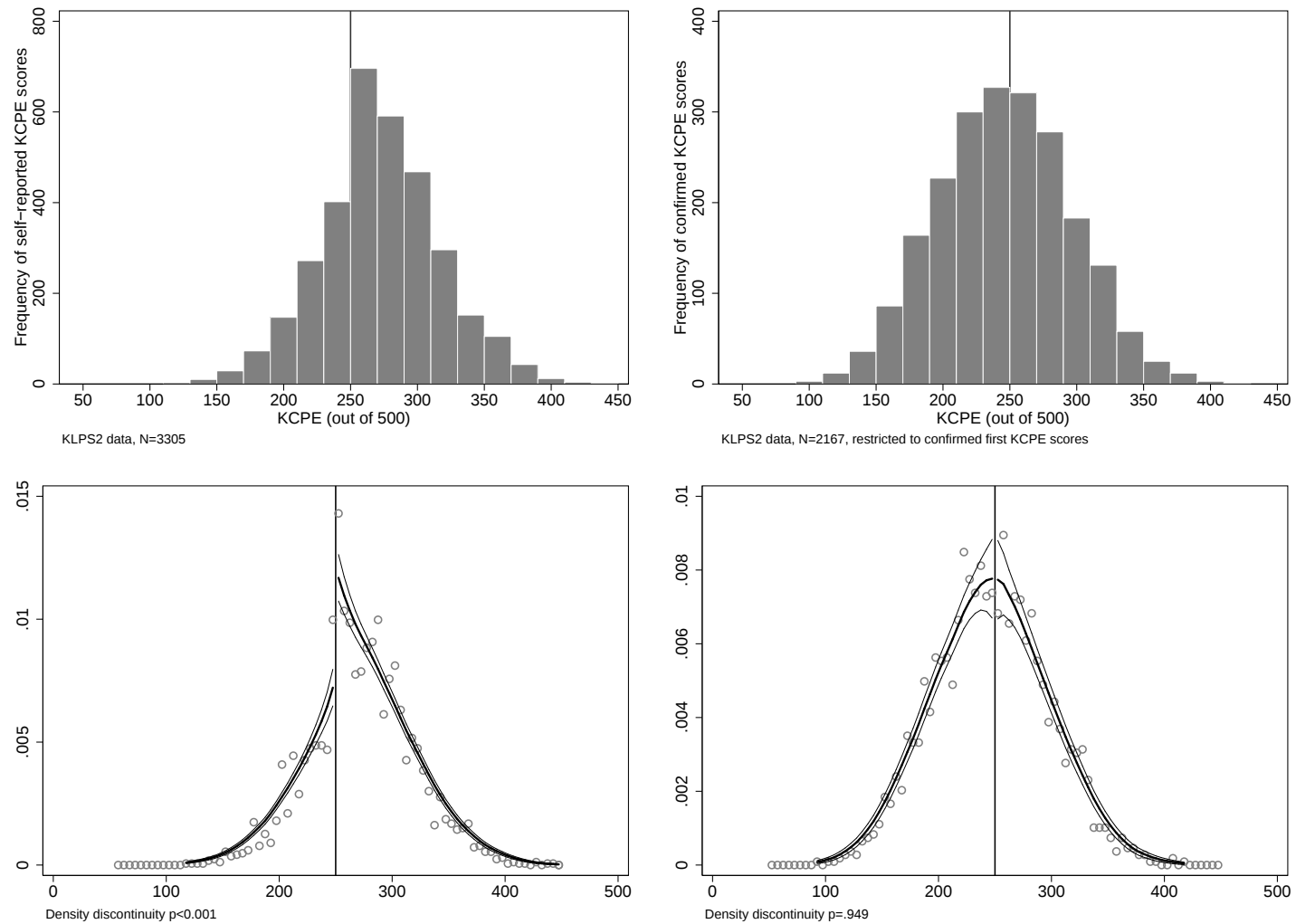
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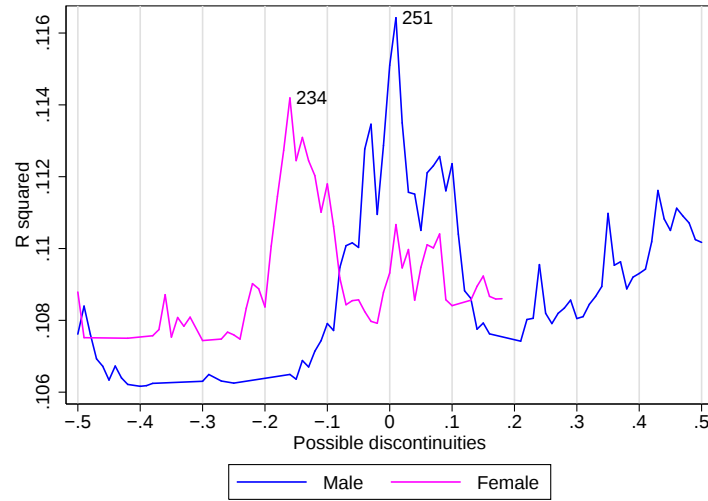
Figure 1: Self-reported and confirmed KCPE scores with density tests.



Left panels: self-reported scores; right panels: confirmed official scores.

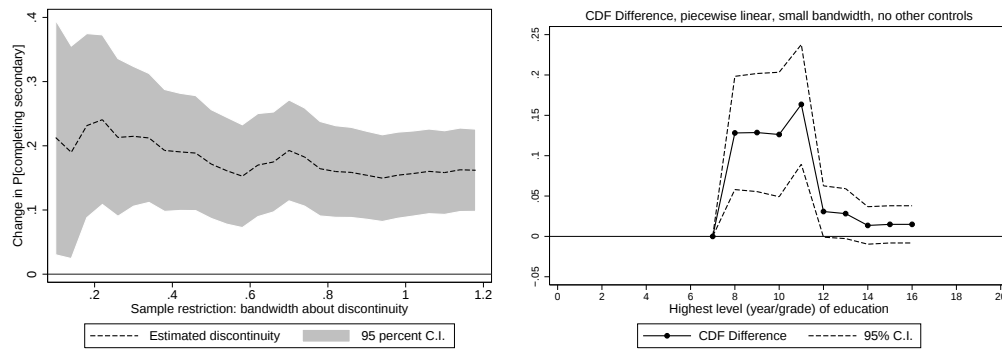
Note: KCPE scores prior to 2001 are converted to the current 500-point scale; density graphs generated by the McCrary (2008) Stata program.

Figure 2: Structural break search



Estimation based on method used in Card, Mas, and Rothstein (2008).

Figure 3: Discontinuity: a function of bandwidth, and CDF difference



Left panel provides estimates and confidence intervals based on piecewise linear specification; right panel shows the difference in cumulative distribution functions for years of education at the discontinuity.

Figure 4: RD Validity: local quadratic regressions of covariates on KCPE scores.

42

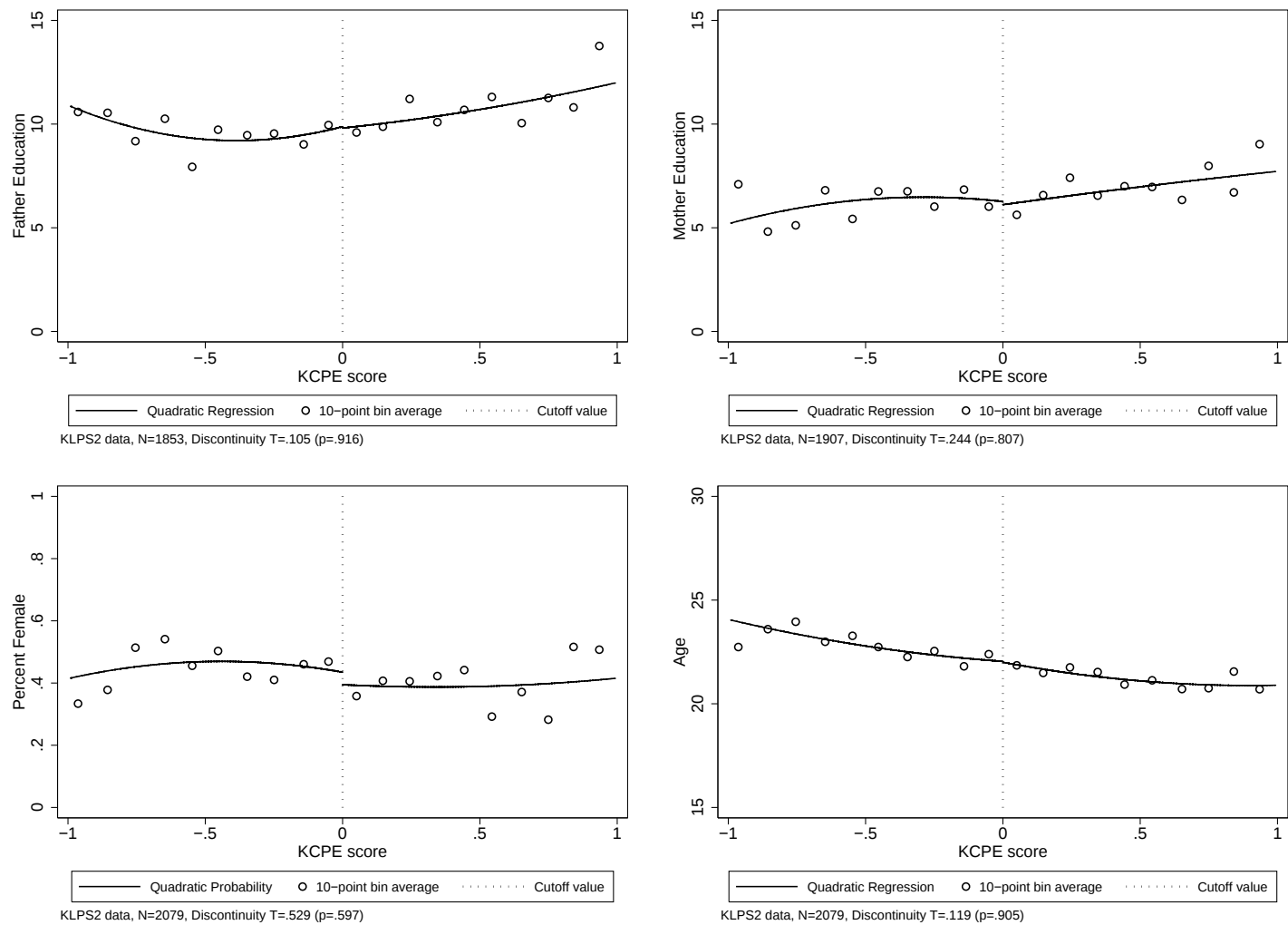


Figure 5: First stage and reduced forms: cognitive performance; self-employment among older men; pregnancy by 18 among women.

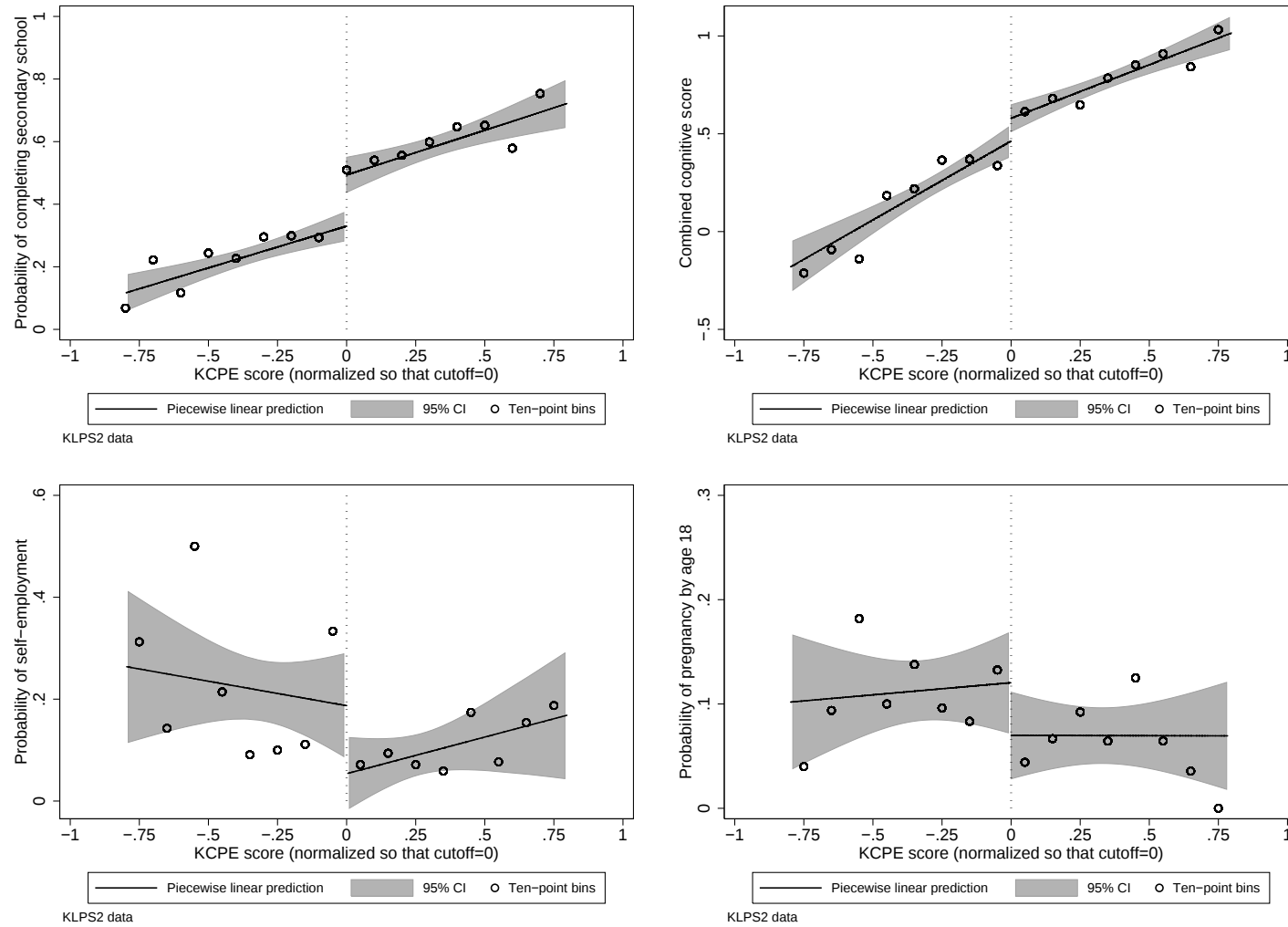


Table 1: KLPS2 Summary Statistics

CHARACTERISTIC	MEAN	STANDARD DEV.	N
<i>Panel A: Respondent characteristics among those with KCPE scores</i>			
Age	22.05	(2.57)	3305
Female	0.45	(0.50)	3305
Father's level of education	10.06	(4.99)	2953
Mother's level of education	6.61	(4.18)	3049
<i>Panel B: Educational characteristics among those with KCPE scores</i>			
Self-reported KCPE Score (out of 500)	254.49	(52.23)	3305
Years of Education	10.14	(2.09)	3305
Still attending school	0.30	(0.46)	3305
Any secondary schooling	0.62	(0.49)	3305
Complete (4y) secondary schooling	0.37	(0.48)	3305
Post-secondary schooling	0.04	(0.18)	3305
<i>Panel C: Outcome variables in subsamples used for estimation</i>			
Vocabulary test (standardized)	0.55	(0.69)	1923
Raven's matrices (standardized)	0.35	(0.91)	1904
Standardized vocabulary + Raven's	0.51	(0.76)	1904
Attending school male	0.33	(0.47)	1058
Attending school male, oldest cohorts	0.13	(0.34)	375
Employed male	0.21	(0.41)	1058
Employed male, oldest cohorts	0.34	(0.47)	375
Self-employed male	0.10	(0.30)	1058
Self-employed male, oldest cohorts	0.16	(0.37)	375
Pregnant by 18 female, at least 18 y.o.	0.09	(0.29)	853

Note that this is a subsample of the KLPS2 data; by conditioning on the presence of a KCPE score, I eliminate all respondents who left school before completing 8th grade (N=3,305 rather than 5,084). Also note that the average grade in 1998 is between 4 and 5 because the KLPS sample has essentially equal numbers of pupils drawn from each grade from 2 through 7. Apart from survey non-response, the sample is reduced due to restrictions for variables with descriptions including “| female,” and “| male,” and other conditions. The variable “Still attending school” is measured in 2007, 2008, or 2009, depending on when the survey took place; as one would expect, it declines with age and grade cohorts; likewise, employment rates trend in the opposite direction. KCPE scores prior to 2001 have been converted to the current 500-point scale.

Table 2: Discontinuity (First Stage) Estimation.

OUTCOME: COMPLETING SECONDARY SCHOOL									
SAMPLE RESTRICTION:	POOLED			MALE			FEMALE		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
KCPE $\geq$ cutoff	0.16*** (0.04)	0.15*** (0.03)	0.17*** (0.05)	0.17*** (0.05)	0.16*** (0.05)	0.21*** (0.06)	0.16*** (0.06)	0.13** (0.05)	0.12* (0.07)
KCPE centered at cutoff	0.27*** (0.06)	0.27*** (0.05)	0.07 (0.18)	0.3*** (0.09)	0.28*** (0.09)	0.07 (0.31)	0.24*** (0.08)	0.25*** (0.08)	0.06 (0.26)
(KCPE $\geq$ cutoff) $\times$ KCPE	0.02 (0.09)	0.006 (0.08)	0.2 (0.3)	-0.02 (0.11)	-0.01 (0.11)	-0.03 (0.41)	-0.006 (0.14)	0.05 (0.13)	0.5 (0.48)
Constant	0.33*** (0.02)	0.44*** (0.14)	0.41*** (0.14)	0.39*** (0.04)	0.41** (0.17)	0.37** (0.18)	0.27*** (0.04)	0.34* (0.19)	0.32 (0.19)
Piecewise Quadratic	No	No	Yes	No	No	Yes	No	No	Yes
Controls	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Discontinuity F-stat	19.46	21.55	14.87	11.13	12.42	10.94	7.50	5.81	2.71
Observations	1943	1943	1943	1064	1064	1064	879	879	879
R^2	0.14	0.23	0.23	0.14	0.24	0.24	0.12	0.2	0.2

NOTES FOR ALL REGRESSION TABLES: Standard errors, clustered at the KCPE-score level, are in parentheses. * denotes significance at the 10% level, ** at the 5% level, and *** at the 1% level. Coefficients on KCPE score and interactions with it have been scaled up by a factor of 100. KCPE score has been re-centered at the discontinuity (251 for men; 234 for women), so that the coefficient on the discontinuity may be interpreted directly. KCPE scores prior to 2001 have been converted to the current 500-point scale. Controls, when indicated, include age, parents' education levels (and an indicator for survey nonresponse), and indicators for all but one of the six KLPS cohorts.

Table 3: Human capital: all cohorts

Outcome:	Mean effect:					
	Vocabulary and Raven's Matrices				Vocabulary	Matrices
	(1)	(2)	(3)	(4)	(5)	(6)
	OLS	OLS	2SLS	2SLS	2SLS	2SLS
<i>Panel A: Full sample</i>						
Completing Std 12	0.612*** (0.032)	0.584*** (0.033)	0.67** (0.282)	0.596** (0.3)	0.645** (0.275)	0.399 (0.432)
KCPE centered at cutoff	0.663*** (0.085)	0.607*** (0.086)	0.637*** (0.168)	0.602*** (0.17)	0.607*** (0.16)	0.448* (0.232)
(KCPE $\geq$ cutoff) $\times$ KCPE	-0.311** (0.127)	-0.302** (0.124)	-0.311** (0.127)	-0.302** (0.123)	-0.468*** (0.112)	-0.061 (0.175)
Female	-0.19*** (0.029)	-0.222*** (0.03)	-0.183*** (0.042)	-0.22*** (0.051)	-0.136*** (0.047)	-0.25*** (0.073)
Constant	0.361*** (0.031)	1.055*** (0.204)	0.334** (0.14)	1.048*** (0.274)	1.580*** (0.219)	0.273 (0.389)
Controls	No	Yes	No	Yes	Yes	Yes
Discontinuity F-stat	.	.	20.496	23.070	23.070	23.070
Observations	1923	1923	1923	1923	1923	1923
R^2	0.331	0.345	0.33	0.345	0.404	0.153
<i>Panel B: Sample restricted to oldest two cohorts</i>						
Completing Std 12	0.689*** (0.049)	0.648*** (0.05)	0.685* (0.385)	0.62 (0.429)	0.958** (0.379)	0.129 (0.569)
Controls	No	Yes	No	Yes	Yes	Yes
Discontinuity F-stat	.	.	10.783	9.041	9.041	9.041
Observations	693	693	693	693	693	693
R^2	0.42	0.428	0.42	0.428	0.452	0.184

(See NOTES FOR ALL REGRESSION TABLES below Table 2.) In Panel B, though only the coefficient on secondary schooling is shown, the specifications are the same as in Panel A, except that the sample is restricted to the oldest two cohorts.

Table 4: Employment outcomes for men (oldest two cohorts) and fertility outcomes for women

Outcome	Estimation							
	(1) OLS	(2) OLS	(3) IVP	(4) IVP	(5) BVP	(6) BVP	(7) 2SLS	(8) 2SLS
<i>Panel A: Employment outcomes, men</i>								
P[Formally employed]	-0.036 (0.055)	0.036 (0.058)	0.263 (0.253)	0.427** (0.216)	0.240 (0.192)	0.359** (0.171)	0.291 (0.352)	0.549 (0.486)
P[Self-employed]	-0.104*** (0.040)	-0.12** (0.049)	-0.459*** (0.092)	-0.516*** (0.103)	-0.464*** (0.147)	-0.347** (0.136)	-0.502* (0.273)	-0.601* (0.359)
Controls	No	Yes	No	Yes	No	Yes	No	Yes
Discontinuity F-statistic	.	.	9.031	5.986	9.031	5.986	9.031	5.986
Observations	378	378	378	378	378	378	378	378
<i>Panel B: Fertility, women</i>								
P[Pregnant by 18]	-0.119*** (0.020)	-0.138*** (0.022)	-0.454 (0.300)	-0.583*** (0.191)	-0.199** (0.086)	-0.184 (0.123)	-0.333 (0.238)	-0.389 (0.286)
Controls	No	Yes	No	Yes	No	Yes	No	Yes
Discontinuity F-statistic	.	.	6.993	5.589	6.993	5.589	6.993	5.589
Observations	853	853	853	853	853	853	853	853

(See NOTES FOR ALL REGRESSION TABLES below Table 2.) Only the coefficient on completed secondary schooling is shown; each coefficient comes from a separate regression. Abbreviations: BVP and IVP denote bivariate probit and IV probit, respectively; marginal effects are shown for both. Standard errors for bivariate probit estimates are obtained via bootstrapping with 1,000 draws.

Table 5: Controls and bias: human capital, all cohorts

	Outcome: combined vocab/Raven's score				
	(1)	(2)	(3)	(4)	(5)
	OLS	OLS	OLS	OLS	2SLS
Completing Std 12	1.226*** (0.022)	0.821*** (0.033)	0.754*** (0.034)	0.612*** (0.032)	0.67** (0.282)
Constant	-0.196*** (0.02)	0.213*** (0.03)	0.26*** (0.03)	0.309*** (0.026)	0.334** (0.14)
Restrict to KCPE takers	No	Yes	Yes	Yes	Yes
Restrict KCPE bandwidth	No	No	Yes	Yes	Yes
Control for KCPE	No	No	No	Yes	Yes
Observations	4885	2149	1923	1923	1923
R^2	0.326	0.292	0.271	0.329	0.33

(See NOTES FOR ALL REGRESSION TABLES below Table 2.) In Tables 5 through 8, Column (1) uses only robust standard errors, however, because KCPE data are not included in the first specification. All columns in this table include a control for respondent gender.

Table 6: Controls and bias: employment for older two cohorts of men

	Outcome: formally employed					
	(1)	(2)	(3)	(4)	(5)	(6)
	OLS	OLS	OLS	OLS	OLS	2SLS
Completing Std 12	-0.014 (0.036)	-0.017 (0.047)	-0.039 (0.051)	-0.035 (0.055)	0.015 (0.058)	0.291 (0.352)
Constant	0.327*** (0.026)	0.344*** (0.033)	0.362*** (0.036)	0.36*** (0.038)	-0.857*** (0.316)	0.223 (0.197)
Restrict to KCPE takers	No	Yes	Yes	Yes	Yes	Yes
Restrict KCPE bandwidth	No	No	Yes	Yes	Yes	Yes
Control for KCPE	No	No	No	Yes	Yes	Yes
Control for age	No	No	No	No	Yes	No
Observations	664	429	378	378	378	378
R^2	0.0002	0.0003	0.002	0.002	0.036	.

See Notes for Table 5.

Table 7: Controls and bias: self-employment, older two cohorts of men

	Outcome: self-employed					
	(1)	(2)	(3)	(4)	(5)	(6)
	OLS	OLS	OLS	OLS	OLS	2SLS
Completing Std 12	-0.145*** (0.026)	-0.125*** (0.037)	-0.127*** (0.04)	-0.105*** (0.04)	-0.101** (0.044)	-0.502* (0.273)
Constant	0.246*** (0.02)	0.225*** (0.03)	0.23*** (0.033)	0.218*** (0.032)	0.111 (0.276)	0.403** (0.17)
Restrict to KCPE takers	No	Yes	Yes	Yes	Yes	Yes
Restrict KCPE bandwidth	No	No	Yes	Yes	Yes	Yes
Control for KCPE	No	No	No	Yes	Yes	Yes
Control for age	No	No	No	No	Yes	No
Observations	802	429	378	378	378	378
R^2	0.034	0.029	0.03	0.034	0.034	.

See Notes for Table 5.

Table 8: Controls and bias: fertility outcome, women

	Outcome: pregnant by age 18				
	(1) OLS	(2) OLS	(3) OLS	(4) OLS	(5) 2SLS
Completing Std 12	-0.278*** (0.012)	-0.118*** (0.016)	-0.117*** (0.017)	-0.119*** (0.02)	-0.333 (0.238)
Constant	0.29*** (0.011)	0.133*** (0.014)	0.134*** (0.015)	0.135*** (0.016)	0.214** (0.089)
Restrict to KCPE takers	No	Yes	Yes	Yes	Yes
Restrict KCPE bandwidth	No	No	Yes	Yes	Yes
Control for KCPE	No	No	No	Yes	Yes
Observations	2350	937	853	853	853
R^2	0.07	0.04	0.037	0.037	.

See Notes for Table 5.

A Online Appendix - not for print publication

A.1 Data

A.1.1 KCPE logistics

When students take the exam,⁴³ they indicate a list of secondary schools they would prefer to attend, including one or two from each of three tiers of secondary schools: national, provincial, and district.⁴⁴ National schools are the most competitive and are considered to be of the highest quality; district schools, the least so.

Initial admission cutoffs are determined centrally by the Ministry of Education. The cutoffs may differ for boys and girls, and vary according to characteristics of each secondary school.⁴⁵ After the cutoffs are decided and test results are available, heads of secondary schools meet in groups to determine matches between schools and students, and make admission offers. Following this initial selection round, a variety of additional selection mechanisms are employed: students may accept offers; students may contact other schools which they would prefer to attend, to see whether the admissions committee is willing to accept them;⁴⁶ and school leaders may meet again to carry out a second round of selection if an insufficient number of students accept offers in the first round. Nevertheless, the odds of secondary school admission jump up sharply at a KCPE score of 250, and continue to rise thereafter, as does the quality of the school to which a student is admitted.

A.1.2 KCPE scoring

From 1985 to 2000, the KCPE covered seven subjects, each scored on a 100-point scale: English, Swahili, math, science, geography, arts, and business;

⁴³The Kenyan academic year coincides with the calendar year; the KCPE takes place in early November, and results are announced in the last few days of December.

⁴⁴This mirrors the “rule-of-thumb” tiered system of secondary school choice that Ghana adopted in 2008, for example (Ajayi 2010).

⁴⁵Some school cutoffs are below 250, while others are above. The cutoff governing the largest fraction of schools in the region, however, is 250.

⁴⁶Often the school-imposed KCPE cutoff is higher for these cases, but exceptions are made at the discretion of the admissions committee for especially meritorious or needy students.

from 2001 onward, the last two of these—arts and business—were removed (Orlale 2000, Kremer, Miguel, and Thornton 2009). As a consequence, the KLPS data include observations in which the maximum score is 700, and observations where the maximum is 500. Throughout this paper, I normalize all scores to the 500-point scale.

Those who are not admitted to any government school have several options if they wish to continue their education: they may repeat eighth grade and re-take the KCPE; they may still have access to private secondary schools and vocational schools; or they may travel to Uganda to enroll in school there.

A.1.3 Re-taking the KCPE

One clear pattern both from the survey data and the administrative records is that students sometimes re-take the test. In the 2003-2005 round of surveying (KLPS1), the questionnaire asked not only for respondents' KCPE score, but also how many times they had taken the KCPE. Of KCPE-takers in the older cohorts (who had reached eighth grade before being interviewed in KLPS2), approximately 87 percent said they took it exactly once, 13 percent said that they had taken the exam twice, and around one tenth of one percent said they took it three times. The reason such a small fraction re-take such an important examination, according to my interviews with both teachers and pupils, is that it is costly: they have to repeat eighth grade in order to do it. The survey data are in agreement: more than 98 percent of respondents who report re-taking the KCPE also report repeating standard 8; conversely, of those who take the KCPE only once, comparatively few respondents (less than 3 percent) repeat standard 8 for any reason. While a pupil's decision to re-take the test is conditioned on the the pupil's unobserved ability as well as the relationship of her first score to the discontinuity—thereby skewing the second score distribution and any conclusions drawn from it, as in the case studied by Martorell (2004)—a pupil's first test score should not show any sign of manipulation around the discontinuity.

A.1.4 KLPS data

All 73 schools are rural, and together represent 80% of the schools in those two administrative Divisions. From this population of roughly 22,000 students, a representative 7,530 pupils were randomly sampled for two follow-ups. The survey acts both as a follow-up to the Primary School Deworming

Project (PSDP) (Miguel and Kremer 2004), and as a longitudinal study representative of an entire region.

Of 7,530 sampled pupils, 5,084 were surveyed during KLPS2. Though this is only 67.5 percent of the sample, some of the original sample has been confirmed deceased, and because some pupils were easier to locate than others after ten years, each survey wave was carried out in two phases: regular and intensive. Only a random sample of respondents who were not found during the regular phase were sought during the intensive phase: 63 percent of respondents were located in the regular phase, and of the remaining 37 percent, more than half of an intensive sample was located, bringing the effective tracking rate of KLPS2 to above 80 percent.⁴⁷

A.1.5 Matching and correcting KCPE scores

If pupils taking KCPE could somehow manipulate their test score to place themselves just above the secondary school cutoff, it would invalidate the research design. Administrative data for a recent year across the entire province, however, depicted in Figure A1, does not show this characteristic. Examination papers from any particular school are graded by separate teachers for each subject, and are never graded by teachers from the school where the papers originated, so precise manipulation around the discontinuity would not be straightforward in any event. To resolve the discrepancy between the distributions of self-reported and administratively reported KCPE scores, I gathered an auxiliary dataset of 17,384 official KCPE scores from the Government of Kenya, via district education offices and school visits. These official data do not include all schools in all years for several reasons: recent political upheavals made some records inaccessible, re-districting changed which offices were responsible for maintaining the records in question; and record-keeping over the past eleven years at local primary schools has occasionally been frustrated by natural disasters. Nevertheless, the data I gathered include roughly 88 percent of the KLPS schools⁴⁸ during the years

⁴⁷Discussion of tracking logistics, intensive sampling, and sample attrition in the earlier 2003-2005 round of this survey, KLPS1, may be found in Baird, Hamory, and Miguel (2008).

⁴⁸In the process of visiting many of the schools myself in order to collect this data, in addition to all the schools in the original Primary School Deworming Project study, I was also able to visit a number of schools in neighboring districts where some KLPS respondents had transferred by the time of their KCPE examinations. These records are included in the 17,384 total.

of interest in this study. I match the KCPE records to the KLPS surveys by pupil name,⁴⁹ and by the year(s) and school(s) in which the pupil took the KCPE. After condensing spelling variations of the same name, I am able to match KCPE records to KLPS2 surveys whenever there is no better match in the year and school in which the respondent took KCPE—and at least two names agree across the two datasets.

Comparing the administrative test scores with the survey data, I am able to ask what predicts misreporting. A graph of misreporting as a function of initial test score is shown in Appendix Figure A3: the lower the true score, the higher the chances of misreporting. In Appendix Table A7, I consider other predictors in a linear regression framework. Respondents with low ability as measured by cognitive tests at survey time, those who round their test scores to a multiple of five, and those who took the test further in the past are all more likely to misreport the score. None of these, however, is a very reliable predictor: they do not yield large differences in the probability of misreporting. The only sharp predictor is whether the respondent took the KCPE before the KLPS1 survey was administered, and reports the same score in KLPS1 and KLPS2; in that case, there is an 86 percent chance that the respondent is reporting the truth, though this is without conditioning on any other predictors. Nearly all of these cases, of course, are respondents who scored above 250 on the test. I am able to include these data in robustness checks that expand the sample slightly, from 2,167 to 2,236 first test scores, and my results do not change appreciably.

Based on confirmed first test scores, I am also able to chart the probability of re-taking the test, shown in Appendix Figure A4. As expected, the probability of re-taking the test is highest for respondents whose first score is below 250 points. The average test score improvement from the first attempt to the second is 54 points, just above one standard deviation on the test.

⁴⁹Names in Kenya are not as fixed as in the United States: they may be spelled differently even within the same document (“Winnstone” for “Winston,” for example); order of names is also typically not fixed (so that “Juma Winston” is likely to be the same person as “Winston Juma”); and the subset of names reported (“Juma Winston Wandera”) varies from record to record. I should also note that the distribution of names is skewed more towards the most common names in western Kenya than in it is in the United States. “Smith” was the most common surname in the 1990 US census, with just over one percent of the population; no other surname exceeded one percent. In this region, there are five names that occur with frequencies above three percent each. Despite this concentration, unique identification of pupils is made feasible by the typically small exam cohorts from each school.

A.2 Measurement error and re-taking

Following the notation and discussion of Hahn, Todd, and Van der Klaauw (2001), consider outcome y_i , and a binary indicator for secondary schooling, x_i . Let $y_i = \alpha_i + x_i \cdot \beta$. Label KCPE score (centered at the admission cutoff) z_i , so that $\Pr[x_i = 1|z_i = z]$ is discontinuous at $z = 0$. Define:

$$\begin{aligned} x^+ &= \lim_{z \rightarrow 0^+} E[x_i|z_i = z] \\ x^- &= \lim_{z \rightarrow 0^-} E[x_i|z_i = z] \\ y^+ &= \lim_{z \rightarrow 0^+} E[y_i|z_i = z] \\ y^- &= \lim_{z \rightarrow 0^-} E[y_i|z_i = z] \end{aligned}$$

Assuming that these limits exist, and that $E[\alpha_i|z_i = z]$ is continuous in z at 0:

$$\beta = \frac{y^+ - y^-}{x^+ - x^-}$$

This is true no matter what the correlation between α_i and x_i is, as long as the continuity assumption holds. If $E[x_i|z_i = z] = \Pr[x_i = 1|z_i = z]$ has a discontinuity of $x^+ - x^- = \phi$ at $z = 0$, then $y^+ - y^- = \phi \cdot \beta$, and the result follows; this is the Hahn, Todd, and Van der Klaauw (2001) argument for identification in the regression discontinuity design.

A.2.1 Continuous classical measurement error in running variable

Let $\tilde{z}_i = z_i + \eta_i$, where η_i is a continuous random variable independent of x_i , z_i , and α_i with probability density function $f_\eta(\cdot)$. Suppose we observe y_i , x_i , and $\tilde{z}_i$, but not z_i . Define:

$$\begin{aligned} \tilde{x}^+ &= \lim_{z \rightarrow 0^+} E[x_i|\tilde{z}_i = z] \\ \tilde{x}^- &= \lim_{z \rightarrow 0^-} E[x_i|\tilde{z}_i = z] \end{aligned}$$

By iterated expectations:

$$E[x_i|\tilde{z}_i = z] = \int_{-\infty}^{\infty} E[x_i|z_i = t]f_\eta(z - t)dt$$

If $f_\eta(\cdot)$ is differentiable everywhere, application of Leibniz' rule to the expression for $E[x_i|\tilde{z}_i = z]$ above shows that $E[x_i|\tilde{z}_i = z]$ is differentiable everywhere, even if $E[x_i|z_i = z] = \Pr[x_i = 1|z_i = z]$ is not. Thus, in this setting, $\tilde{x}^+ - \tilde{x}^- = 0$: there is no discontinuity when the running variable is measured with this type of error.

A.2.2 Alternative forms of measurement error

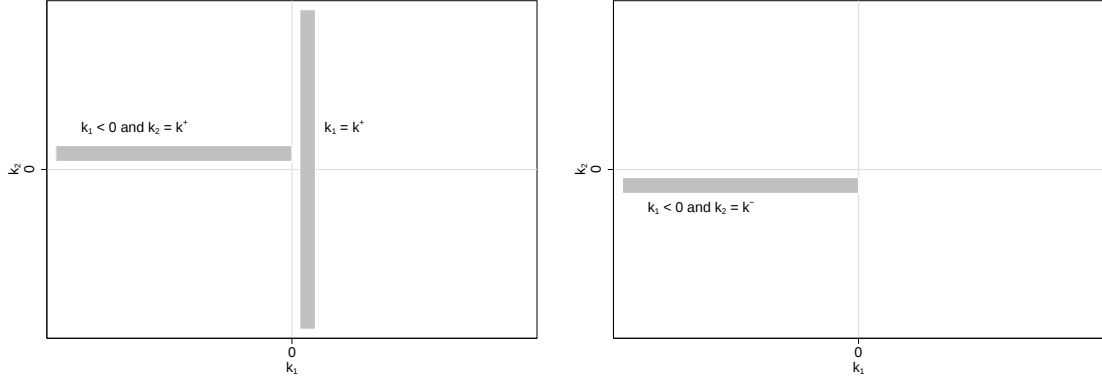
Suppose that instead of $\tilde{z}_i = z_i + \eta_i$, we observe $\tilde{z}'_i = z_i + \eta_i \cdot \zeta_i$, where ζ_i is binary. Let $\Pr(\zeta_i = 0) = p$, with ζ_i independent of η_i , x_i , z_i , and α_i . Using analogously defined limit expressions, $\tilde{x}'^+ - \tilde{x}'^- = p \cdot \phi$, and $\tilde{y}'^+ - \tilde{y}'^- = p \cdot \phi \cdot \beta$, so the regression discontinuity can still be used to consistently estimate β , though the discontinuity is made smaller. In neither of these cases ($\tilde{z}_i$ or $\tilde{z}'_i$), however, should the density of the observed running variable be discontinuous at $z = 0$ if the underlying density of z_i is smooth at $z = 0$.

A.2.3 Re-taking

In the presence of test re-taking, a regression discontinuity estimate might or might not yield the desired local average treatment effect, as discussed by Martorell (2004). However, it could easily yield an “artificial” discontinuity in the *density* of reported test scores, as follows: For this discussion, let k_1 and k_2 be the first and second test scores a student receives on the KCPE. Let $k_1, k_2 \sim iid \mathcal{N}(0, \sigma^2)$ with mean zero (at the cutoff). The student only learns the second test score if he does not pass the first time. The student might then report only the most recent score; the first score if he passes the first time, the second if he takes the test a second time.

$$k_{recent} = k_1 \cdot \mathbb{1}[k_1 \geq 0] + k_2 \cdot \mathbb{1}[k_1 < 0]$$

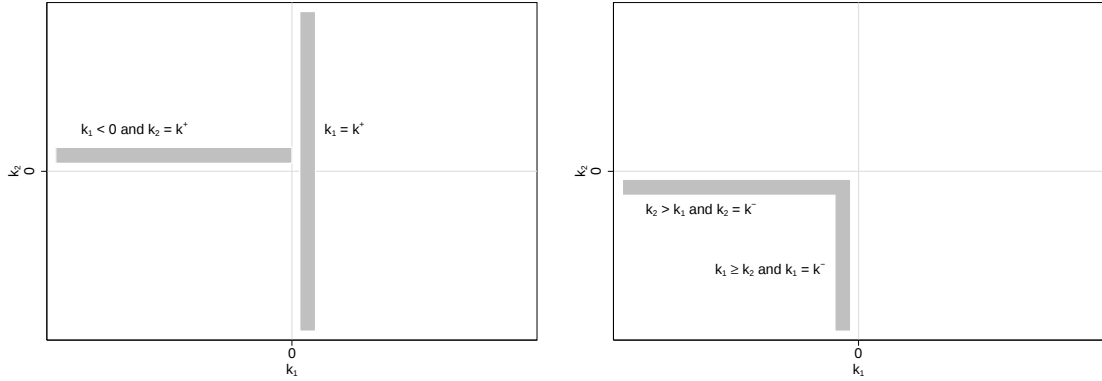
Though the distributions of k_1 , k_2 , and even $\max(k_1, k_2)$ are smooth, the density of k_{recent} is discontinuous at the cutoff. This is because for $k^+ \geq 0$, of k_{recent} can take the value k^+ either when $k_1 < 0$ and $k_2 = k^+$, or when $k_1 = k^+$. For $k^- < 0$, only the former condition applies. Graphically:



Alternatively, the student might follow the same re-taking rule, then report the best score:

$$k_{best} = k_1 \cdot \mathbb{1}[k_1 \geq 0] + \max(k_1, k_2) \cdot \mathbb{1}[k_1 < 0]$$

Again, the density of k_{best} is discontinuous at the cutoff, because for $k^+ \geq 0$, of k_{best} can still take the value k^+ either when $k_1 < 0$ and $k_2 = k^+$, or when $k_1 = k^+$. But now, for $k^- < 0$, a modification of the former condition applies: $k_{best} = k^-$ either when $k_2 = k^-$ and $k_2 > k_1$, or when $k_1 = k^-$ and $k_1 > k_2$. Graphically:



In either case, the density of scores just to the left of the cutoff will be lower than to the right, and the McCrary (2008) test should reject smoothness at the cutoff point.

A.3 Additional figures and tables

Figure A1: True administrative distribution from 2008

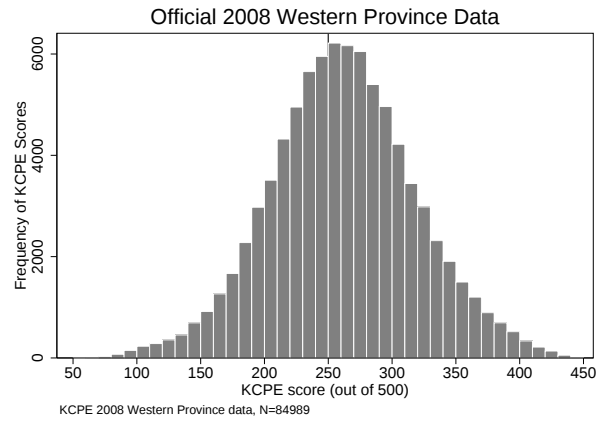
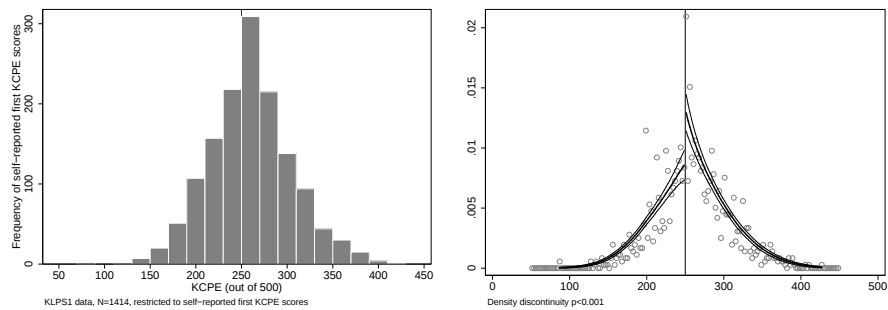
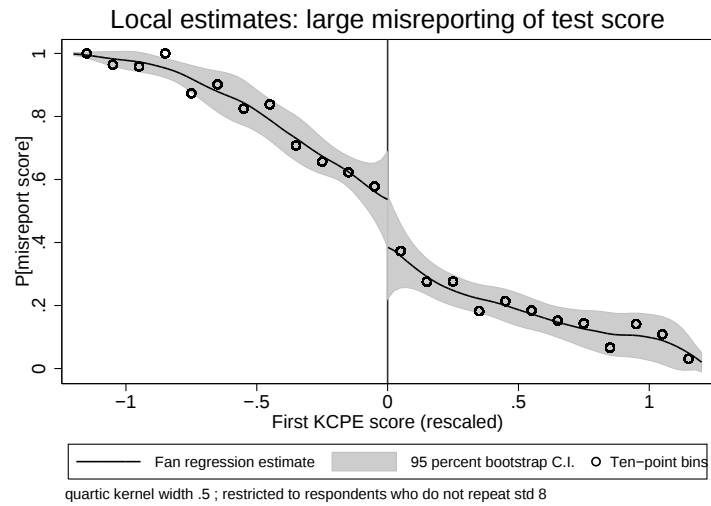


Figure A2: Self-reported KLPS1 first KCPE scores with smoothness test



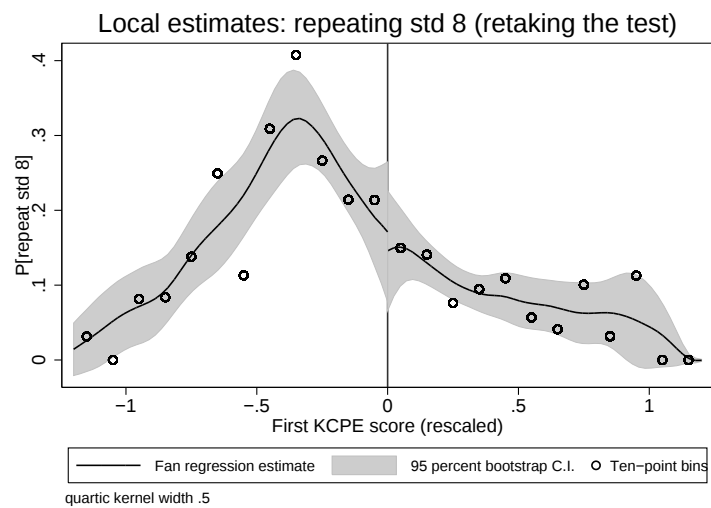
Generated using the Stata program developed by McCrary (2008).

Figure A3: Misreporting test score, as a function of true test score: local linear estimates



Graph generated using the algorithm proposed by Fan (1992).

Figure A4: Retaking the test: local linear estimates



Graph generated using the algorithm proposed by Fan (1992).

Table A1: Cross-section relationship between cognitive performance and wage

OUTCOME: LOG(WAGE), CONDITIONAL ON OBSERVATION			
	(1)	(2)	(3)
Standardized cognitive measure	0.256*** (0.035)	0.199*** (0.039)	0.291*** (0.084)
Constant	7.874*** (0.035)	7.940*** (0.04)	8.052*** (0.082)
Observations	772	592	208
R^2	0.066	0.042	0.055

In column 1, wages are regressed on the standardized measure of cognitive ability (standardized sum of vocabulary and Raven's Matrices Z-scores). In column 2, the sample is restricted to men; in column 3, it is restricted to men in the oldest two cohorts (Standards 6 and 7 in 1998). Wages are reported in Kenyan Shillings per month.

Table A2: Robustness: alternative discontinuity (first stage) estimation.

OUTCOME: HIGHEST GRADE LEVEL OF EDUCATIONAL ATTAINMENT									
SAMPLE RESTRICTION:									
	POOLED			MALE			FEMALE		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
KCPE $\geq$ cutoff	0.65*** (0.16)	0.57*** (0.14)	0.83*** (0.22)	0.68*** (0.21)	0.6*** (0.19)	0.84*** (0.29)	0.64*** (0.24)	0.56** (0.23)	0.82** (0.34)
KCPE centered at cutoff	1.80*** (0.25)	1.59*** (0.24)	0.92 (0.78)	1.76*** (0.33)	1.53*** (0.33)	1.32 (1.14)	1.86*** (0.37)	1.63*** (0.37)	0.4 (1.22)
(KCPE $\geq$ cutoff) $\times$ KCPE	-0.52 (0.41)	-0.56 (0.37)	-1.39 (1.38)	-0.34 (0.53)	-0.32 (0.49)	-1.86 (1.85)	-1.10** (0.56)	-0.91 (0.57)	-0.76 (2.06)
Constant	10.06*** (0.1)	13.04*** (0.59)	12.90*** (0.59)	10.24*** (0.12)	12.70*** (0.74)	12.64*** (0.76)	9.88*** (0.16)	12.76*** (0.87)	12.55*** (0.85)
Piecewise Quadratic	No	No	Yes	No	No	Yes	No	No	Yes
Controls	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Discontinuity F-stat	16.63	15.73	13.98	10.22	9.88	8.46	7.25	5.68	5.73
Observations	1943	1943	1943	1064	1064	1064	879	879	879
R^2	0.18	0.28	0.28	0.19	0.29	0.29	0.16	0.25	0.25

(See NOTES FOR ALL REGRESSION TABLES below Table 2.)

Table A3: Robustness: human capital, all cohorts

Outcome:	Mean effect:				Vocabulary	Matrices
	Vocabulary and Raven's Matrices					
	(1) OLS	(2) OLS	(3) 2SLS	(4) 2SLS		
Educational attainment	0.162*** (0.009)	0.152*** (0.009)	0.167** (0.07)	0.153** (0.077)	0.166** (0.067)	0.103 (0.112)
KCPE centered at cutoff	0.53*** (0.086)	0.521*** (0.086)	0.517** (0.203)	0.519*** (0.2)	0.518*** (0.184)	0.392 (0.284)
(KCPE≥cutoff)×KCPE	-0.21* (0.125)	-0.21* (0.123)	-0.207 (0.138)	-0.21 (0.136)	-0.368*** (0.127)	0.0008 (0.193)
Female	-0.181*** (0.029)	-0.203*** (0.03)	-0.178*** (0.044)	-0.202*** (0.059)	-0.117** (0.051)	-0.238*** (0.086)
Constant	-1.070*** (0.096)	-0.666*** (0.233)	-1.125 (0.744)	-0.676 (1.058)	-0.286 (0.9)	-0.882 (1.560)
Controls	No	No	Yes	Yes	Yes	Yes
Discontinuity F-stat	.	.	17.290	16.965	16.965	16.965
Observations	1923	1923	1923	1923	1923	1923
R ²	0.352	0.358	0.352	0.358	0.443	0.15

(See NOTES FOR ALL REGRESSION TABLES below Table 2.) Note that this differs from Table 3 in that the first stage uses highest level of educational attainment rather than an indicator for completing secondary school.

Table A4: Robustness: employment outcomes, older two cohorts of men

Outcome	Estimation					
	(1) OLS	(2) OLS	(3) IVP	(4) IVP	(5) 2SLS	(6) 2SLS
P[Formally employed]	-0.022** (0.01)	-0.009 (0.011)	0.151 (0.159)	0.087* (0.046)	0.062 (0.078)	0.118 (0.113)
P[Self-employed]	-0.019*** (0.006)	-0.023*** (0.008)	-0.382*** (0.075)	-0.12*** (0.026)	-0.106* (0.059)	-0.129 (0.08)
Controls	No	Yes	No	Yes	No	Yes
Discontinuity F-stat	.	.	8.118	5.346	8.118	5.346
Observations	378	378	378	378	378	378

(See Notes for Table 4.) Note that this differs from Panel A of Table 4 in that the first stage uses highest level of educational attainment rather than an indicator for completing secondary school; the bivariate probit is not appropriate for a continuous first stage and is omitted.

Table A5: Robustness: fertility outcome (women)

Outcome	Estimation					
	(1) OLS	(2) OLS	(3) IVP	(4) IVP	(5) 2SLS	(6) 2SLS
P[Pregnant by 18]	-0.038*** (0.006)	-0.045*** (0.007)	-0.544*** (0.197)	-0.134*** (0.048)	-0.079 (0.055)	-0.09 (0.064)
Controls	No	Yes	No	Yes	No	Yes
Discontinuity F-stat	.	.	6.993	5.624	6.993	5.624
Observations	853	853	853	853	853	853

(See Notes for Table 4.) Note that this differs from Panel B of Table 4 in that the first stage uses highest level of educational attainment rather than an indicator for completing secondary school; the bivariate probit is not appropriate for a continuous first stage and is omitted.

Table A6: Robustness: alternative discontinuity location.

OUTCOME: HIGHEST GRADE LEVEL OF EDUCATIONAL ATTAINMENT									
SAMPLE RESTRICTION:	POOLED			MALE			FEMALE		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
KCPE $\geq$ cutoff	0.14*** (0.04)	0.14*** (0.04)	0.15*** (0.05)	0.14*** (0.05)	0.15*** (0.05)	0.17*** (0.07)	0.13** (0.06)	0.13** (0.05)	0.14* (0.08)
KCPE centered at cutoff	0.3*** (0.07)	0.28*** (0.06)	0.26 (0.23)	0.32*** (0.09)	0.28*** (0.09)	0.11 (0.32)	0.28*** (0.09)	0.27*** (0.09)	0.4 (0.34)
(KCPE $\geq$ cutoff) $\times$ KCPE	0.02 (0.1)	0.004 (0.09)	-0.05 (0.33)	-0.006 (0.12)	-0.001 (0.11)	0.11 (0.43)	0.009 (0.15)	0.03 (0.13)	-0.29 (0.51)
Constant	0.35*** (0.03)	0.46*** (0.13)	0.46*** (0.14)	0.4*** (0.04)	0.43** (0.17)	0.41** (0.18)	0.31*** (0.04)	0.38** (0.19)	0.4** (0.2)
Piecewise Quadratic	No	No	Yes	No	No	Yes	No	No	Yes
Controls	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Discontinuity F-stat	10.66	14.49	7.96	7.16	9.99	6.81	4.94	5.81	3.21
Observations	1935	1935	1935	1059	1059	1059	876	876	876
R^2	0.14	0.23	0.23	0.14	0.24	0.24	0.12	0.21	0.21

(See NOTES FOR ALL REGRESSION TABLES below Table 2.) Note that this table differs from Table 2 in that the discontinuity is located at KCPE=250 for men and KCPE=240 for women, rather than the locations detected automatically

Table A7: Predictors of (large) misreporting of KCPE score.

	(1)	(2)	(3)	(4)
KCPE: self-reported	-0.18*** (0.03)	-0.01 (0.03)	-0.01 (0.03)	0.06* (0.03)
Reporting a multiple of 5	0.16*** (0.02)	0.13*** (0.02)	0.13*** (0.02)	0.09*** (0.03)
Reporting exactly 250	0.12* (0.07)	0.08 (0.07)	0.08 (0.07)	0.02 (0.08)
Grade in 1998	0.03*** (0.007)	0.04*** (0.007)	0.04*** (0.007)	0.02** (0.01)
Raven cognitive test	.	-0.02*** (0.005)	-0.02*** (0.005)	-0.01** (0.006)
KLPS1 and KLPS2 scores agree	.	.	.	-0.6*** (0.03)
Female	.	.	0.008 (0.02)	0.03 (0.03)
Constant	0.3*** (0.04)	0.81*** (0.05)	0.76*** (0.06)	0.85*** (0.08)
Control for mother education	No	No	Yes	No
Observations	1906	1888	1888	935
R^2	0.08	0.15	0.15	0.44

Here, “large” misreports are survey responses which differ from the true test score by more than 5 points.