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DO PARENTS KNOW BEST? THE SHORT AND LONG-RUN EFFECTS OF ATTENDING THE SCHOOLS THAT PARENTS PREFER

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ABSTRACT

Recent studies document that, in many cases, sought after schools do not improve student test scores. Three explanations are that (i) existing studies identify local average treatment effects that do not generalize to the average student, (ii) parents cannot discern schools' causal impacts, and (iii) parents value schools that improve outcomes not well measured by test scores. To shed light on this, we employ administrative and survey data from Barbados. Using discrete choice models, we document that most parents have strong preferences for the same schools. Using a regression-discontinuity design, we estimate the causal impact of attending a preferred school on a broad array of outcomes. As found in other settings, preferred schools have better peers, but do not improve short-run test scores. We implement a new statistical test and find that this null effect is not due to school impacts being different for marginal students than for the average student. Looking at longer-run outcomes, for girls, preferred schools reduce teen motherhood, increase educational attainment, increase earnings, and improve health. In contrast, for boys, the results are mixed. The pattern for girls is consistent with parents valuing school impacts on outcomes not well measured by test scores, while the pattern for boys is consistent with parents being unable to identify schools' causal impacts. Our results indicate that impacts on test scores may be an incomplete measure of school quality.

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I Introduction

There is a growing literature documenting that parental preferences for schools are often unrelated to schools' casual impacts on test scores. For example, while a handful of studies find that attending sought-after selective secondary schools improves students' academic achievement (e.g. Jackson 2010; Pop-Eleches and Urquiola 2013) several studies document that the most sought-after elite schools confer modest or no test score improvement (Clark 2010; Abdulkadiroğlu et al. 2014; Bui et al. 2014; Lucas and Mbiti 2014; Dobbie and Fryer 2014; Ajayi 2015). Relatedly, many studies find mixed or no impacts of winning a public school choice lottery on test scores (Cullen et al. 2006; Hastings et al. 2009; Deming et al. 2014) and despite weakly positive evidence on the impacts of private school vouchers (see Rouse and Barrow 2009), Abdulkadiroğlu et al. (2018) find that students who used vouchers to attend private schools had *worse* test scores.[1]

The lack of robust achievement effects of attending schools that parents prefer is something of a puzzle. One possible explanation is statistical. Most studies in this literature rely on regression-discontinuity designs that compare the outcomes of applicants to preferred schools who just made or just missed some admissions test score cutoff (e.g. Jackson 2010; Clark 2010; Pop-Eleches and Urquiola 2013; Abdulkadiroğlu et al. 2014; Bui et al. 2014; Lucas and Mbiti 2014; Dobbie and Fryer 2014; Ajayi 2015). These studies identify local average treatment effects (Imbens and Angrist, 1994) for the marginal applicant. If the marginal applicant benefits less from preferred schools than the average applicant, it could explain why parents, *on average*, may have strong preferences for schools with small impacts on the marginal applicant.

Another potential explanation is that parents have flawed information so that the schools that they perceive as being best are not.[2] Alternatively, preferred schools may improve students' longer-run outcomes in ways not well-measured by test scores.[3] Schools that do not improve test scores may confer important long-run benefits for several reasons: (i) preferred schools may improve non-cognitive and social skills that are unrelated to test scores but rewarded in the labor market (Heckman et al. 2006; Glaeser et al. 2007; Jackson 2018); (ii) preferred schools may promote productive behaviors such as reduced criminality and teen childbearing even if they do not improve test scores (Milligan et al. 2004; Deming 2011; Beuermann et al. 2018); (iii) employers may use

[1] Note that Deming et al. (2014) finds that winning a lottery increases college going for girls, but not for boys.

[2] One plausible scenario is that parents base their decisions on observed ex-post outcomes that may reflect student selection rather than schools' ability to improve examination performance. Also parents may overestimate the direct benefit of exposure to higher achieving peers. Finally, admission to a preferred school might trigger behavioral responses (e.g. reduced parental effort) that undo the potential academic benefits (Pop-Eleches and Urquiola 2013). However, none of these explanations would lead to improved longer-run outcomes as we find in this paper.

[3] In an important contribution, Clark and Del Bono (2016) examine the longer-run effect of attending one of three elite schools in Aberdeen Scotland during the 1960s. However, they do not examine whether the long-run impacts relate to school impacts on test scores, they do not examine how these impacts relate to parental preferences, and they are unable to determine whether their impacts for the marginal admit are similar to the impacts for the average student.

preferred schools as a signal of ability (Spence 1973; MacLeod and Urquiola 2015); and (iv) preferred schools might provide access to better-connected social networks that can be leveraged to improve employment and social opportunities (Ioannides and Loury 2004; Schmutte 2015).

We seek to shed light on these issues using data from Barbados. We examine whether there is broad agreement among parents regarding which schools are preferred over others, and also discuss the characteristics of preferred schools. We then exploit quasi-random variation to estimate the causal effect of attending a preferred school on examination performance. Next, we implement a new statistical test to examine whether the causal impacts for the marginal student differ from those of the average student – this is a useful methodological contribution because this new test can be implemented in other settings. Finally, we leverage survey data to estimate the causal effect of attending a preferred school on a broad set of social and economic outcomes measured in adulthood.

A few key features of the Barbados data and context are well-suited for this study. At the end of primary school, students take the Barbados Secondary School Entrance Examination (BSSEE). At BSSEE registration, students submit a ranked list of preferred secondary schools to the Ministry of Education, Science, Technology and Innovation (METI), and the METI use a deferred acceptance algorithm (Gale and Shapley 1962; Abdulkadiroğlu et al. 2005) to assign students to schools based on their choices and their test scores. Dubins and Freedman (1981) and Roth (1982) show that, among the set of schools listed, truthfully ranking schools is a weakly dominant strategy. As such, using the ordered list of choices, we can reasonably infer preferences for schools. Being able to measure parental preferences is important because a disconnect between parental preferences for schools and schools' causal impacts would have much broader implications if *all* parents share a common view of which schools are better than others. Unlike studies that implicitly assume that most parents prefer the same set of elite schools, we can examine this empirically.

The second key feature is that the assignment rule used by the METI creates a test score cutoff for each school above which student applicants are admitted and below which they are not. This feature allows us to employ a regression discontinuity design to identify the causal effect of attending a preferred school. A third key feature is that, while many school systems use test score cutoffs to assign students to the most elite schools (e.g. in Boston, Chicago, New York, the United Kingdom, and others), test score cutoffs are used for *all* schools in Barbados. This allows us to identify effects of attending a preferred school across the entire distribution of school desirability. This distinguishes our paper from existing work and allows us to examine any differences between the impacts of the most elite schools and other preferred schools.

We exploit administrative data on the BSSEE (the secondary school admissions test), and all secondary school applications and assignments for twenty five years (1987 through 2011). To track educational outcomes, we merge these student-level BSSEE data to administrative school exam records taken at the end of secondary and post-secondary studies between 1993 and 2016. To track long-run outcomes, we link the administrative BSSEE records to the 2016 Barbados Survey of Living Conditions and focus on cohorts aged 25 or older at the time of the survey. This survey is an official parish level representative two-percent survey of the population that was executed by the Barbados Statistical Service. It contains data on demographics, education, health, fertility, migration, consumption, employment, and income.

Because we are interested in whether parents tend to prefer schools that improve child outcomes, our main treatment is attending a more "preferred" school. To better understand the "preferred" school treatment, we first examine parental preferences. We find that preferences for secondary schools are nearly universally shared and stable over time. Specifically, we estimate a rank-ordered logit model and follow Avery et al. (2013) to construct a revealed-preference ranking of secondary schools. The correlation between school rankings in 1987 and 2011 is 0.96. To gain a sense for how broadly held these rankings are, we examine how *individual* rankings in 2011 deviate from the aggregate 1987 rankings. Over 95 percent of individuals in 2011 listed a top choice school with a higher 1987 ranking than their bottom choice school. In sum, there is broad consensus regarding what schools are most desirable, and this was largely unchanged over time. Because there is broad agreement about which schools are preferred, and students are assigned to schools based on test scores, we show that "preferred" schools are almost always more selective schools.

Using a regression discontinuity design, we show that attending a preferred school is associated with higher-achieving peers, more academically homogeneous peers, and smaller cohorts. However, we find no improvement on secondary-school exam performance for girls and potentially *worse* performance for boys. This lack of improved test scores echoes findings from Cullen et al. (2006) in Chicago, Deming et al. (2014) in North Carolina, Clark (2010) in the United Kingdom, Abdulkadiroğlu et al. (2014) in Boston, Dobbie and Fryer (2014) in New York, Lucas and Mbiti (2014) in Kenya, and Ajayi (2015) in Ghana.

To assess the claim that the reason for the null effect (in this study and others) is that marginal applicants are less responsive to school quality differences than the average applicant, we implement a new empirical test. For all the applicants to each school, we estimate the impact of scoring above the cutoff for that school. This uncovers the impact of being admitted to that school relative to the next preferred schools for the *marginal admits* (who scored just above the cutoff). We then use fixed effects models to estimate the value-added of each school for the *average* admit. We predict what the cutoff effect would be if school impacts for the *marginal* admit were equal to the *average* value-added. The cutoff effects for the marginal admits are almost identical to the predicted impacts – making it highly unlikely that a difference in responsiveness between the average and the marginal admits can explain the null impact of attending preferred schools.[4]

[4]Note that this test differs from that presented in Angrist and Rokkanen (2015) who make some reasonable but strong assumptions to extrapolate treatment impacts away from the cutoff. Our approach relies on much weaker

Looking at medium- and longer-run outcomes tells a different story from the short-run test score impacts. In administrative data, students at preferred schools are more likely to earn a post-secondary credential. In survey data, attending a preferred school is associated with more years of completed formal education. Consistent with the educational attainment effects of older individuals in our analytical sample, persons who attended a preferred school were less likely to be in the labor force between the ages of 17 and 24, but more likely to be in school during those ages. However, these longer-run educational benefits are largely driven by women. Consistent with these education patterns, attending a preferred school has no effect on earnings among males, but does increase female earnings. Clark and Del Bono (2016) hypothesize that male earnings may be unaffected because preferred schools may lead to more formal schooling at the expense of (possibly higher-return) informal technical/vocational programs. We test this directly, and find no evidence of this in our data. An exploration into mechanisms for women reveals that the earnings increase for women is likely mediated by them being employed in higher status occupations (as opposed to being more productive at the same job), and we provide evidence that the improved social networks at preferred schools may facilitate securing these higher status jobs.

To further help explain the gender differences, we examine fertility. Attending a preferred school is associated with reduced teen motherhood but no change in total fertility.[5] This teen motherhood effect may explain why women at preferred schools are more likely to be in school between the ages of 17 and 24, attain more years of education by age 25, and have higher earnings while there is no such effect for men. It can also explain the sizable long-run benefits for women despite no test score effects, and offers a *potential* explanation for the finding that women may benefit more from attending better high schools than men (Jackson 2010, Deming et al. 2014). To our knowledge, this is the first evidence of a *causal* link between school quality and teen motherhood.

While economists have long proposed a causal link between educational attainment and health (Cutler and Lleras-Muney 2006; Clark and Royer 2013; Buckles et al. 2016; Malamud et al. 2018), *to our knowledge*, we are the first to use quasi-random variation to isolate the causal impact of individual schools on health.[6] Both women and men who attended preferred schools are more likely to be of normal weight, and less likely to be overweight or obese in adulthood. We also find positive effects on lifestyle behaviors (such as exercising regularly and having regular dental checkups). The overall health benefits are similar for both men and women, even though preferred schools increase the likelihood of having medical health insurance only for women. This suggests

assumptions and compares average impacts to marginal impacts.

[5]This pattern of delayed childbearing is similar to the finding of reduced female fertility found for attending elite UK schools in Clark and Del Bono (2016). However, delayed childbearing and reduced fertility are distinct phenomena.

[6]Jones et al. (2011) use matching methods and find that students who attend better schools have better health. In related work, Aaronson et al. (2017) and Frisvold and Golberstein (2011) find that African American that had access to better schools in the early to mid 1900s had improved health outcomes.

that preferred schools may promote productive habits and attitudes that are not measured by test scores. This may represent a significant, previously undocumented return to school quality.

Many related studies examine the causal impact of attending one or a few elite schools within a local area (e.g. Clark 2010; Abdulkadiroğlu et al. 2014; Dobbie and Fryer 2014; Lucas and Mbiti 2014; Ajayi 2015; Clark and Del Bono 2016). To better relate our analysis of all preferred schools to that of the elite school literature, we explore whether the patterns observed are due to elite schools or a more general pattern across all schools. By and large, our results suggest that the marginal impacts are similar for elite preferred schools and non-elite preferred schools such that the disconnect between parental preferences for schools and schools' causal impacts (especially for boys), exists for all schools and not just the most elite.

Our results suggest that there is considerable agreement among parents regarding which schools are "good" and which are not. The patterns for girls suggest that school impacts on test scores may understate their impacts on adult well-being such that parents *may be* rational in their desires to send their children to schools that have no effect on test scores. However, the strong parental preferences for schools that have relatively small short- or long-run impacts for boys suggest that parents may be uninformed about the impacts of schools on boys. As such, there may be benefits to the provision of information to parents on the causal impacts of schools for different kinds of children. To our knowledge, this is the first paper to simultaneously examine parental preferences for schools, and also estimate the effect of attending a preferred school on test scores, educational attainment, health, and labor market outcomes. Accordingly, we build on, and bring together, the literature on the long-run effect of schools, the literature on the effect of elite schools on test scores, the literature on the effect of education on health, and the literature on parental preferences for schools (e.g. Black 1999; Hastings et al. 2006; Burgess et al. 2015).

The remainder of this paper is as follows: Section II presents background information on the Barbados education system and on the allocation mechanism used to assign students to secondary schools. Section III presents the discrete choice analysis of parental preferences for schools. Section IV describes the data used to estimate school impacts. Section V outlines the identification strategy, Section VI present the results, and Section VII concludes.

II The Barbados Education System

The Barbados education system evolved from the English education system. At the end of primary school (after grade 6), students register to take the BSSEE and provide a list of ranked secondary school choices to the METI. Between 1987 and 1995, students could rank all schools, while after 1996 students could list up to nine school choices. The BSSEE is comprised of three subjects that all students take: mathematics, English language, and an essay. The total BSSEE score is the sum of the scores on the individual sections and ranges from 0 to 200. Using a computerized system, the METI ranks students by their BSSEE score and gender. No other criteria are used (e.g., sibling preferences or geographic proximity) for the ranking. Individual school capacity by gender is pre-determined. The algorithm assigns the highest ranked student to her first choice. It then moves on to the second and treats her similarly. At some point, the procedure will reach a student whose first choice is full. At that point, it tries to assign the student to her second choice. If full, to the third choice and so on. Only once this student has been assigned to a school does the algorithm move onto the next person. Under this allocation mechanism, within the set of schools listed, truthfully revealing the rankings is a dominant strategy (Haeringer and Klijn 2009; Pathak and Sönmez 2013). This feature will allow us to infer parental preferences from the choice lists. As pointed out in Abdulkadiroglu et al. (2017), when the number of choices is constrained (as in many settings), the algorithm is not strategy proof. We show that the inferred school preferences are very similar during the 1987-1995 period (when there were unlimited choices) and thereafter (when there were nine choices), so that this is not a cause for concern in our setting.

Secondary school begins in first form (the equivalent of 7th grade) and ends at fifth form (the equivalent of 11th grade) when students take the Caribbean Secondary Education Certification (CSEC) examinations. These are the Caribbean equivalent of the British Ordinary levels (O-levels) examinations and are externally graded by the Caribbean Examinations Council (CXC). The CSEC examinations are given in 33 subjects. Passing five subjects (including the two core subjects of English language and mathematics) is a sufficient entry requirement for less prestigious tertiary institutions such as community colleges, technical schools, or training schools. It can also be used for entry at some colleges in the United States. Students who complete these requirements continue their studies at a tertiary institution (if accepted) or pursue the Caribbean Advanced Proficiency Examination (CAPE), also externally graded by CXC.

The CAPE is a tertiary-level program. Students seeking to eventually attend University (as opposed to a community college) will take the CAPE. The CAPE is the equivalent of the British Advanced levels (A-levels) examinations and was launched in 2005. The CAPE is a two-year program and includes three two-unit subjects (each unit taken in different academic years) and two core subjects (Caribbean Studies and Communication Studies). While requirements vary across programs, passing at least two CAPE units is typically required for entry to the University of the West Indies. Passing six CAPE units is accepted as a general admission requirement to British higher education institutions. The post-secondary qualification of a CAPE Associate's Degree is awarded for passing seven CAPE units, including Caribbean Studies and Communication Studies.

III The Revealed Preference Ranking of Schools

One key contribution of this work is that we shed light on what drives parental preferences by linking preferences for schools directly to schools' causal impacts. To better understand parental preferences for schools, we follow Avery et al. (2013) and exploit the choice data to construct a revealed-preference ranking of secondary schools in Barbados. Intuitively, because each student lists a set of schools they wish to attend, in order of desirability, and the allocation algorithm is truth revealing, one can determine which are more preferred schools by seeing which individual schools tend to be systematically higher in individuals' choices. The ranking approach is similar to that used for ranking players in tournaments where players are observed in several head-to-head match-ups. Schools that tend to be preferred in many head-to-head comparisons (i.e. ranked above other schools on the list) are more highly ranked, and schools that are preferred over more highly ranked schools are themselves more highly ranked. Because each list of X ranked schools includes $\sum_{n=1}^{X-1}(X-n)$ such head-to-head comparisons and thousands of students submit such lists each year, constructing such rankings from the choice data is feasible. We expand on the model below.

Each student i, has a utility value, U_{ij}, for each secondary school j, given by (1) below.

$$U_{ij} = \theta_j + \varepsilon_{ij} \tag{1}$$

The parameter θ_j is an index of the overall desirability of school j, and the random error term is ε_{ij}. The parameter θ_j does not vary at the student level and therefore represents a school's average desirability. Let $\theta_j^{r_{is}}$ be the desirably of the school j that individual i ranked s ($r_i = s$) in their list of options R_i. Let $U_{ij}^{r_{is}}$ be the utility individual i gets from school j that she ranked in position s, so that $U_{ij}^{r_{i1}}$ is her utility from the school she ranked first, $U_{ij}^{r_{i2}}$ her utility for the school ranked second, and so on. Because the assignment mechanism is truthfully revealing (Haeringer and Klijn 2009; Pathak and Sönmez 2013), we make the behavioral assumption that higher ranked schools are preferred to lower ranked schools. It therefore follows that the probability that an individual i submits a particular ranking over the set of listed schools is

$$Pr\left[(U_{ij}^{r_{i1}} > U_{ik}^{r_{im}}, 1 < m, \forall m \in \{2,\ldots,R_i\}) \cap \ldots \cap (U_{ij}^{r_{iR_i-1}} > U_{ik}^{r_{iR_i}})\right] \tag{2}$$

As is common practice in the discrete-choice literature, we assume that ε_{ij} follows an extreme value distribution so that the probability that an individual i submits a particular ranking over all ranked schools is a product of standard logit formulas. The likelihood (or probability) that individual i chooses ranking $\{r_{i1}, r_{i2}, \ldots, R_i\}$ is now:

$$l_i(\theta) = Prob[r_{i1}, r_{i2}, \ldots, R_i] = \frac{\exp(\theta_j^{r_{i1}})}{\sum_{k=1}^{R_i} \exp(\theta_{ik}^{r_{ik}})} \cdot \frac{\exp(\theta_j^{r_{i2}})}{\sum_{k=2}^{R_i} \exp(\theta_{ik}^{r_{ik}})} \cdots \frac{\exp(\theta_j^{r_{iR-1}})}{\exp(\theta_j^{r_{iR-1}}) + \exp(\theta_k^{r_{iR_i}})} \tag{3}$$

The full log likelihood of observing all the choices is simply the sum of the log of the individual likelihoods across all individuals.

$$\log L(\theta) = \sum_{i=1}^{N} \log l_i(\theta) \tag{4}$$

One can obtain estimated preferences for each school $\hat{\theta}_j$ by finding the θ_js that maximize the full log likelihood. This is achieved by estimating a rank-ordered logit model with a full set of indicator variables for each school in Barbados. Because proximity is a strong predictor of parents' choices, we obtained rankings based on models that both include and exclude proximity to each school choice as a covariate. Reassuringly, the rankings are identical across both models. Schools with larger $\hat{\theta}_j$s are those that tend to be listed higher up in individuals' ordered lists. The school with the highest $\hat{\theta}_j$ will be the school that is most likely to be preferred (on average) in head-to-head comparisons with other schools. After running this model, we rank schools by their estimated desirability to obtain a revealed-preference ranking over all schools. If students who list both schools A and B tend to list school A above school B, and students who list both schools B and C tend to list school B above school C, our approach will rank school A above B and B above C.

III.1 The Estimated School Rankings

To determine whether the preference rankings are meaningful, we first establish that they are stable over time. The top five schools in 1987 remain the top five schools in 2011 with the only difference being that the top two schools swapped places.[7] While there is some movement among the lower-ranked schools, the rankings are quite stable across this 25 year period. Overall, the correlation between the revealed preference rank in 1987 and the revealed preference rank in 2011 is 0.96. The similarity in rankings when parents can rank all schools (and therefore truthful revelation is a dominant strategy) and when they can rank up to nine schools, indicates that one can reliably infer parental preferences from choice data when parents can only rank nine choices. A scatter-plot of the rankings across these two years is presented in the left panel of Figure 1. The regression predicting the rank in 2011 based on the rank in 1987 has a slope of 0.97 and an R-squared of 0.91. The *p*-value test that the slope is equal to 1 is 0.7. This suggests that the average view regarding what schools are most desirable has been very stable over time.

Having established that aggregate school rankings are stable over time, we now explore how much the *average* view is shared among *individuals*. To do this, we rank schools in each year, and then estimate the likelihood of a given school being listed as a preferred school in a given year as a function of its aggregate ranking in that year.[8] If there is widespread agreement among parents

[7]Using the reveled preference rankings, the five top ranked schools in 1987 were (1) Harrison College (HC), (2) Queens College (QC), (3) Combermere School (CS), (4) St. Michaels School (SM), (5) Christ Church Foundation (CF). A quarter century later in 2011, the top ranked schools were (1) QC, (2) HC, (3) CS, (4) SM, (5) CF.

[8]We estimate a rank ordered logit model in which the aggregate ranking enters the model as the sole predictor

about what the most desirable schools are, aggregate rankings would predict being ranked more highly by parents, and rank reversals (i.e. putting a lower-ranked school higher in one's choice list) would be very uncommon. Conversely, if there is considerable heterogeneity in parents' views regarding which schools are more desirable, aggregate rankings may predict being ranked more highly by parents *on average*, but rank reversals would be common. The average ranking in a given year is a very strong predictor of individual choices in that year. A school is 44 percent more likely to be more highly ranked by an individual if it is one rank higher in the aggregate, 3 times as likely to be more highly ranked if it is three ranks higher in the aggregate, and 38 times as likely to be more highly ranked if it is 10 ranks higher in the aggregate.

To assuage concerns that the analysis above uses an in-sample prediction (for which there may be some mechanical correlation), we also we rank schools based on the choice lists in 1987, and then estimate the likelihood of a given school being listed as a preferred school in 2011 as a function of its ranking in 1987. We estimate this using a rank ordered logit model on the 2011 choices in which the 1987 ranking enters the model as the sole predictor. Because we use the rankings from a different year, this model will understate the extent to which the individual choices are similar to the average view. However, the patterns are very similar. The 1987 ranking is an extremely powerful predictor of rankings in subsequent years. A school is 33 percent more likely to be more highly ranked in 2011 if it is one rank higher in 1987, 2.4 times as likely to be more highly ranked in 2011 if it is three ranks higher in 1987, and more than 19 times as likely to be more highly ranked in 2011 if it is 10 ranks higher in 1987.[9] These patterns suggests that while parents may disagree regarding which schools are most desirable among very similarly ranked schools, there is considerable agreement regarding which group of schools are most desirable. To allow for the possibility that boys and girls may have different preferences for schools, we examined differences by student gender and the results are virtually identical.[10]

Because the highest-achieving students are admitted to their top choices first, if most students rank schools similarly, then the more preferred schools will also be more selective than the less preferred schools. To show that this is borne out in the data, the right panel of Figure 1 presents the cumulative distribution of the mean peer incoming BSSEE scores of students' school choices. The distribution of mean BSSEE scores of first-choice schools is to the right of the second-choice schools, which is to the right of the third-choice schools, and so on. That is, parents and students tend to place schools with higher-achieving peers higher up on their preference ranking. This is

[9]Put differently, a rank reversal would occur only about 42 percent of the time for schools that were one rank apart in 1987, under 30 percent of the time for schools that were four ranks apart in 1987, and less than six percent of the time for schools that were ten ranks apart. Appendix Figure A1 shows the estimated likelihood that a parent would rank a school above another school in 2011 as a function of the difference in the school rankings in 1987.

[10]We calculated revealed preference rankings pooling all BSSEE cohorts separately by gender. The correlation between girls' rankings and boys' rankings is 0.996.

further evidence that most parents agree on which schools are most desirable. As above, we allow for the possibility that boys and girls may have different preferences for schools, we examined differences by student gender, and the results are virtually identical. Given that the impact of schools may differ by student gender, this is an important finding.

Our analysis reveals that preferences have been stable over time, and that there was considerable agreement regarding which schools were the most desirable. If parents were rational and well-informed, preferred schools should have improved child outcomes (in either the short or the long run, or both). The extent to which these clear preferences for "preferred" schools reflect schools' actual impacts on students is the empirical question we tackle in the remainder of this paper.

IV Data

Our analytic sample is the full population of students who applied to a public secondary school in Barbados between 1987 and 2011.[11] We obtained the official administrative BSSEE data for each of these years. These data include each student's name, date of birth, gender, primary school attended, parish of residence, total score on the BSSEE exam, the ranked list of secondary schools the student wished to attend, and the administrative assignment by the Ministry of Education.[12]

Administrative Examination Data: To track student performance in secondary school we collected data on the CSEC examinations (taken five years after secondary school entry, typically at age 16). The CSEC data are available for all years between 1993 and 2016. These data include the students name, date of birth, gender, scores for each subject examination taken, and the secondary school attended. The CSEC data were linked to the 1987 through 2011 BSSEE cohorts by full name (first, middle, and last), gender, and date of birth.[13] To track post-secondary outcomes for the full population, we also collected data from the CAPE examinations (completed after two years of post-secondary school studies, typically at age 18). The CAPE data are available for years 2005 through 2016, and are linked to the 1998 through 2009 BSSEE cohorts by name, gender, and date of birth.[14] As with the CSEC, these data contain scores for each subject examination taken.

Survey Data: Our longer-run outcomes come from survey data. Our survey data are from the 2016 Barbados Survey of Living Conditions. The reference BSSEE cohorts (1987-2011) were between 17 and 40 years old when surveyed in 2016. This survey is a large parish level representative two-percent survey of the population. It included 2,508 households and 7,098 individuals. The survey data was collected over a 12-month period (12 randomly distributed sub-samples from

[11] Around 91 percent of secondary students in Barbados are enrolled in the public education system.

[12] Appendix Figure A2 shows the raw broadsheets with the student names redacted. These broadsheets were scanned and digitized for merging with other datasets.

[13] We matched 90 percent of individuals observed in the CSEC administrative records to the BSSEE records. The 10 percent rate of unmatched individuals closely mimics the 9 percent enrollment rate in private secondary schools who would not have taken the BSSEE.

[14] We matched 96 percent of individuals observed in the CAPE administrative records to the BSSEE records.

February 2016 until January 2017). The survey collected data on demographics, education, health, fertility, migration, consumption, employment, and income. The national survey was purposely designed to be matched with the BSSEE administrative data at the individual level and asked for full names at age 10 (to account for any name changes), date of birth, and gender. For individuals who would have been in the 1987 through 2011 BSSEE cohorts (primarily born between years 1976 and 1999) we match roughly 90 percent of respondents to the administrative data. Appendix Figure A3 displays the geographical distribution of the matched survey observations. As we show in Section VI.1, admission to a preferred school is unrelated to being matched with the survey. Also, in Appendix Table 1 we show that the test score effects measured at the end of secondary school (CSEC) are similar in both the full administrative dataset and among those who are linked to the surveys. Accordingly, we are reasonably confident that our estimated long-run impacts on the survey sample generalize to the full population.

IV.1 Summary Statistics

Table 1 presents summary statistics for the full administrative data. The population is roughly half female and the average admitted cohort size across all schools is about 157 students. Overall about 68 percent of students took at least one CSEC subject.[15] The average student passed about two CSEC subjects and 26.8 percent passed five subjects including English language and mathematics (i.e. qualified for tertiary education). We also break up the sample by the rank of the student's assigned school (based on the revealed preference rankings from Section III). Among those assigned to the top ranked schools, incoming BSSEE scores are roughly one standard deviation higher than the average of the population (column 2). As one might expect given the large differences in incoming scores, students at these schools have much better outcomes than average. About 60 percent of students at the most preferred schools qualify for a tertiary education (column 2), while only 4.1 percent of students at the least selective schools do (column 4). In terms of post-secondary education, among students at the most preferred schools, 36.8 percent took at least one CAPE unit and 21 percent earned an Associate's degree. Remarkably almost no students in the bottom third ranked schools continue to take the post-secondary exam.

The top panel of Table 2 presents summary statistics for individuals who are matched to the survey data (1,545 observations) by age. For individuals between the ages of 17 and 24 (column 2), roughly 56 percent were in the labor market and 31.4 percent were in school. However, consistent with most individuals completing their schooling by age 25, among those between the ages of 25 and 40 (column 3), roughly 86 percent were in the labor market and only 1 percent was in school. The lower panel of Table 2 shows that, among those who would have completed schooling (ages 25

[15] Because CSEC taking is not mandatory, this measure is not equivalent to secondary school completion. Students receive a School Leaving Certificate regardless of CSEC taking after completing five years of secondary school. About 87.5 percent of surveyed individuals in our reference BSSEE cohorts report having completed secondary school.

to 40), the average years of completed education is 11.33. About 19.6 percent of individuals have a university degree and 18.2 percent have a technical or vocational degree (column 1).

The lower panel of Table 2 also breaks up the sample of individuals 25-40 years of age when surveyed by the rank of the assigned school. Students at the most preferred schools average 15.4 years of completed education, and 52.3 percent of them hold a university degree (column 2), while students at the least preferred schools average 8.5 years of education and only 2.6 percent hold a university degree (column 4). Employment quality is noticeably better for individuals assigned to the most selective schools. Among them, 33 percent have a managerial or professional position and average monthly earnings are about US$1,936. This contrasts with individuals of the middle and bottom third of schools, among whom only 10.2 and 2.8 percent, respectively, hold a managerial or professional position and for whom average salaries are roughly US$1,230 and US$950, respectively. One interesting pattern is that individuals at the most selective schools rely more on their school network when seeking employment. Indeed, 1.1 percent of individuals at the most selective schools engaged in networking with fellow students while almost nobody within the middle and bottom third of schools reported doing so. This provides the first suggestive evidence that access to better job-referral networks may be one benefit of attending a more selective school.

Table 2 also reports the incidence of teen motherhood. There are large differences in the rate of teen motherhood across schools. Only 3.6 percent of women assigned to the most selective schools had a live birth before age 18, while 9.8 and 17 percent of women in the middle and bottom third of schools did. Preventive health behaviors are also more prevalent among individuals assigned to more selective schools. Indeed, the likelihoods of having medical insurance, attending yearly dental checkups, and attending a gym at least once a week are higher among individuals at the most selective schools (column 2). These likelihoods monotonically decrease for the middle and bottom third of schools (columns 3 and 4, respectively).

The descriptive statistics reveal better outcomes for individuals assigned to more preferred (or selective) schools in a wide range of domains. Next, we describe the identification strategy used to determine whether these observed relationships are causal.

V Empirical Strategy

As described in Section II, the assignment mechanism creates a test score cutoff above which student applicants to that school are admitted and below which they are not. This setup lends itself to a regression discontinuity design. Because of the assignment mechanism, the likelihood of attending a preferred school increases in a discontinuous manner as a student's BSSEE score goes from below to above the cutoff score for a preferred school. If nothing else differs for those with test scores just above and below the cutoff, any discontinuous change in outcomes as a student's BSSEE score goes from below to above the cutoff score for a preferred school can be attributed to attending a preferred school (Hahn et al. 2001). We exploit the discontinuity in the admission probability through the cutoff by estimating the following two-stage least-squares (2SLS) regression:

$$Attend_{ijt} = \pi \cdot Above_{ijt} + f_1(BSEE_{it}) + X_{ijt}\gamma_1 + C_{1,jt} + P_{1,ijt} + \varepsilon_{1,ijt} \tag{5}$$

$$Y_{ijt} = \beta \cdot Attend_{ijt} + f_2(BSEE_{it}) + X_{ijt}\gamma_2 + C_{2,jt} + P_{2,ijt} + \varepsilon_{2,ijt} \tag{6}$$

In the first stage equation (5) we predict whether an individual i attends school j at time t, $Attend_{ijt}$, as a function of scoring above the cutoff for preferred school j at time t, ($Above_{ijt}$), and controls. To account for latent outcomes that vary smoothly through the cutoffs, we control for a cubic in BSSEE and a cubic of BSSEE interacted with the $Above_{ijt}$ indicator ($f_1(BSEE_{it})$). We also include parish of residency fixed effects and gender (included in X_{ijt}).[16] Following Jackson (2010) and Pop-Eleches and Urquiola (2013), we stack the data across all application pools for each year to each school (that is, we stack data for all the cutoffs into a single cutoff), and include cutoff fixed effects ($C_{1,jt}$). The inclusion of cutoff fixed effects ensures that all comparisons are among students who applied to the same school in the same year. In the second stage (equation 6), we regress the outcomes of interest (Y_{ijt}) on preferred school attendance (estimated in the first stage) and the same set of controls as in equation (5). The second stage excluded instrument is $Above_{ijt}$. Because individuals can enter the data for multiple cutoffs, the estimated standard errors are adjusted for two-way clustering at the student and BSSEE relative score levels.

One important feature of our data is that we observe the ranked school choices of every student. These choices reflect student preferences and are much stronger predictors of student outcomes than variables typically observed in most datasets. We exploit these data by adding choice group fixed effects ($P_{1,jt}$) as additional controls to increase precision. These choice group fixed effects define the unique set of schools in a student's list along with the unique ranking of those schools. As such, students in the same choice group list the same set of schools in the exact same order. Importantly, we show that all of our results are robust to excluding these powerful controls.

The key identifying assumption behind this RD-based model is that nothing other than the change in likelihood of attending a preferred school changes in a discontinuous manner through the cutoff. We test this assumption in several ways. First, following McCrary (2008), we test for a discontinuity in density through the cutoff and find no economically or statistically significant change in density either in the full population or in the matched survey sample (see Table 3, panel A). Second, as an additional test for smoothness through the cutoff, we estimate reduced form models on each of our predetermined covariates. These include indicators for month of birth, the average BSSEE standardized score of the primary school attended, the average BSSEE standardized score of the incoming class corresponding to each secondary school choice, and indicators for

[16] Note that the BSSEE scores are included as relative scores (i.e., net of the cutoff score for a preferred school)

the parish of the primary school (Appendix Table 2). None of the 33 coefficients in either the full population or the survey sample is statistically significant at the 5 percent level. Third, to summarize impacts on all these covariates into a more efficient test, we create the predicted number of CSEC subjects passed, predicted number of CAPE units passed, predicted years of education, and predicted wages for each student (based on all these covariates). We report the estimated coefficients on the $Above_{ijt}$ indicator on the predicted outcomes in Table 3, panel B.[17] Reassuringly, in all subsamples, predicted outcomes vary smoothly through the cutoffs. Having determined that our estimation strategy is likely valid, we now present our regression-discontinuity estimates.

VI Results

VI.1 The First Stage and Survey Representativeness

Table 3, panel C presents the first stage estimates on the $Above_{ijt}$ indicator from equation (5). In the full sample (columns 1 and 2), scoring above a cutoff increases the likelihood of attending a preferred school by 81 percentage points. Among observations within 0.75 standard deviations from the cutoff, this falls to 74 percentage points. The left panel of Figure 2 illustrates the first stage, showing the discontinuous jump in the likelihood of attending a preferred school through the cut-off. To ensure that the survey is representative of the population (as it was designed to be), we estimate the first stage on the survey sample (columns 5 and 6). The first stage estimate is almost identical in the population and the survey sample.[18] We also test whether the matching rate with our survey data varies through the cutoffs (Table 3, panel D). Scoring above the cutoff for a preferred school is unrelated to being observed in the survey – lending credibility to the survey results.

To describe the "preferred school" treatment, Table 3, panel E reports 2SLS estimates of attending a preferred school on various school characteristics. Attending a preferred school increases peer quality (average BSSEE scores) by 0.25 standard deviations (right panel of Figure 2). This effect is consistent across all samples and specifications. Also, attending a preferred school reduces the school-level coefficient of variation of incoming BSSEE scores. This implies that attending a preferred school not only increases peer quality but also provides an environment with more homogeneous students in terms of incoming academic achievement. While the peer effects literature is mixed (Sacerdote, 2014), one might expect that higher-achieving and more homogeneous peers would lead to improved outcomes. We now examine empirically the extent to which this is true. [19]

[17] We also report reduced form impacts on predicted CSEC and CAPE outcomes, and predicted years of education and wages across different samples by gender in Appendix Table 3. All estimates are indistinguishable from zero.

[18] The first stage estimates are also equivalent between women and men.

[19] Attending a preferred school also reduces cohort size by about 12 students. We also calculated the Herfindahl-Hirschman Index (HHI) in terms of Parish of residence for each incoming class. This measure summarizes how geographically diverse classes are. We find no robust effect on geographic diversity.

VI.2 Effects on Secondary School Academic Achievement

Table 4, panel A presents estimated impacts on several CSEC outcomes measured at the end of secondary school. To retain consistency with the cohorts for which CAPE data are also available, we examine outcomes for the BSSEE cohorts from 1998 to 2009.[20] We present estimated preferred school impacts in models that do not include the choice group effects (odd numbered columns) and those that do (even numbered columns). Given the similarity of the results, in the interest of brevity, we focus our discussion on the full model with all controls.

Students who attend preferred schools do not perform better on the secondary school leaving exams, and may in fact do slightly worse. Overall (column 2), we find no effect on taking the CSEC exams, no effect on the number of exams passed, and a small negative effect on the likelihood of qualifying for tertiary education (passing five subjects including English language and mathematics). This negative impact on passing the secondary school exam is marginally statistically significant at the 10 percent level. Looking at boys and girls separately, there is a negative impact on the number of subjects passed for boys (marginally statistically significant) and a small positive (not statistically significant) impact on that for girls. Looking at the overall effect on taking the CSEC, the effect is similar across both groups and cannot be distinguished from zero. The reduced form impacts of scoring above the cutoff on qualifying for tertiary education are presented visually in the top panel of Figure 3. The visual evidence is consistent with the regression results. The top panel of Appendix Figure A4 shows that the results are similar for any choice of bandwidth.

The results suggest no impact of attending a preferred school on secondary school test scores and possible deleterious effects for boys. These findings echo studies documenting zero effects on test scores from attending more selective schools (Clark 2010; Abdulkadiroğlu et al. 2014; Bui et al. 2014; Lucas and Mbiti 2014; Ajayi 2015). The possible ill effects for boys are consistent with evidence documenting negative effects of elite school attendance on school completion (Dustan et al., 2017).[21] These estimated secondary school test score impacts, coupled with the strong documented preferences these schools, beg the question of whether marginal admits are less responsive to preferred school attendance than the average student. We now tackle this question.

VI.3 Do the Null Effects Generalize to the Average Student?

Because our estimated preferred school effects are based on applicants who score just above or just below the cutoff for a preferred school, this local treatment effect may not reflect the experiences of the average student at a preferred school. This limitation applies to all similar studies that rely on test score cutoffs to identify school impacts (e.g. Jackson 2010; Clark 2010; Pop-Eleches

[20] Appendix Table 1, panel A shows estimated CSEC effects using the BSSEE cohorts 1987-2002 (25-40 years old when surveyed) in whom we focus to estimate longer-term effects later. Results are similar.

[21] Appendix Table 1, panel B shows estimated effects on the same outcomes but restricting the sample to individuals that were matched with the survey data. Results are similar suggesting null effects.

and Urquiola 2013; Abdulkadiroğlu et al. 2014; Bui et al. 2014; Lucas and Mbiti 2014; Dobbie and Fryer 2014; Ajayi 2015). In these studies (as here) the estimated treatment effect is *the impact of being the lowest scoring student at a preferred school relative to being a more typical student at a less preferred school* which may be different from *the average effect of attending a preferred school relative to a less preferred school.* If so, the small benefits to attending a preferred school for the marginal admit could be reconciled with strong parental preferences for such schools if the average impacts were more positive than those for the marginal student who scores just above the cutoff. Contributing to this literature methodologically, we implement a test for whether the estimated school impacts for the marginal students are similar to those for the average student. This test will help potentially explain the null impacts we find in Barbados and possibly other settings.

VI.3.1 The Empirical Test

We now introduce some notation. The impact of attending school *j* for the average student is μ_{j1} while that for the marginal student is μ_{j2}. The outcome for marginal student *i* at school *j* is

$$Y_{ijt} = \mu_{j2} + f(BSEE_{it}) + X_{ijt}\gamma + C_{jt} + P_{ijt} + \varepsilon_{ijt} \tag{7}$$

The estimated effect on outcome Y_{ijt} of scoring above the admissions cutoff for any school *j* is $\Gamma_{j,actual} = E(Y_{ijt}|Above = 1) - E(Y_{ijt}|Above = 0)$. Substituting (7) into this expression and taking expectations, in the neighborhood of the cutoff yields

$$E[\Gamma_{j,actual}] = E(\mu_{j2}|Above = 1) - E(\mu_{j2}|Above = 0) \tag{8}$$

In expectation, the RD estimate of scoring above the cutoff for school *j* simply reflects the difference through that cutoff in the attended school impacts *for the marginal students*. This is intuitive; scoring above the cutoff for school *j* increases the likelihood of attending school *j* and reduces the likelihood of attending the next preferred schools. If school *j* is no more effective for the marginal admit (on average) than the next preferred schools, then the cutoff effect for school *j* will be zero. Conversely, the cutoff for school *j* will only have a positive impact if school *j* is more effective at improving outcomes for the marginal admit (on average) than the next preferred schools.

Now consider impacts for the average admit, μ_{j1}. One can estimate the impact of school *j* for the *average* student, μ_{j1}, in a value-added framework. Where $I_{J=j}$ is an indicator variable equal to 1 if student *i* attends school *j*, the outcome for average student *i* at school *j* is

$$Y_{ijt} = I_{i,J=j}\mu_{j1} + f(BSSEE_{it}) + X_{ijt}\gamma + C_{jt} + P_{ijt} + \varepsilon_{ijt} \tag{9}$$

One can obtain an estimate of the value-added of school j for the average attendee by estimating equation (9) by OLS. The resulting estimate $\hat{\mu}_{j1}$ is simply a school fixed effect that reflects the school-level average outcomes after accounting for observable student characteristics such as incoming test scores, choices, and demographics.[22]

As discussed above, the RD estimate of scoring above the cutoff for school j on outcomes reflects the difference through that cutoff in the attended school impacts (i.e. $\delta(\mu_{j2})/\delta(\text{Above})$) for the marginal admits. We define $\Gamma_{j,predicted}$ as the difference through that cutoff in the estimated value-added of the attended school (i.e. $\delta(\hat{\mu}_{j1})/\delta(\text{Above})$) *among those same marginal admits*. If (i) the value-added estimate is unbiased such that $E[\hat{\mu}_{j1}] = \mu_{j1}$, and (ii) the effect of school j for the marginal admit is the same as the average admit such that $\mu_{j1} = \mu_{j2}$, then (iii) in expectation, the change in the average estimated value-added of the school attended through the cutoff for school j should be equal to the actual change in outcomes through that cutoff.[23] We test this empirically by estimating $\Gamma_{j,actual}$ and $\Gamma_{j,predicted}$ for each school j across all the CSEC outcomes, and then we regress one on the other. **To avoid endogeneity, we use out-of-sample (or leave-year-out) estimates of school value-added.** If our school value-added estimates are biased, then the slope of this regression will differ from 1. In addition, if the school impacts are different for the marginal student from those for the average student, then this slope will also differ from 1. However, if (a) our school value-added estimates are unbiased, and (b) the school impacts are the same for the marginal student as for the average student, then the slope *will* be equal to 1.

Pooling the estimated impacts for each cutoff (preferred school) across all CSEC outcomes, we plot the estimated impacts against the difference in school value-added in Figure 4. The estimated slope is 0.97, revealing that on average the predicted impacts are very similar to the actual impacts. The p-value associated with the null hypothesis that the slope is zero has a p-value of less than 0.001, and the p-value associated with the null hypothesis that the slope is 1 has a p-value of 0.836. This is compelling evidence that the null impacts on short-run test scores are not because the impact for the marginal student is more negative than that for the average student.[24]

[22]Under the assumption that $E[\varepsilon_{ijt}|I_{i,J=j}, BSSEE_{it}, X_{ijt}, C_{jt}, P_{ijt}] = 0$, this will be an unbiased estimate.

[23]One can estimate the impact of scoring above the cutoff for school j on the average value-added of the schools students attend, $\hat{\mu}_{j1}$, by replacing the actual outcomes with the estimated value-added of the attended school and estimating the model below.

$$\hat{\mu}_{j1} = Above_{ijt}\zeta_j + f(BSEE_{it}) + X_{ijt}\gamma + C_{jt} + P_{ijt} + \varepsilon_{ijt} \tag{10}$$

The parameter ζ_j is the difference in school value-added between those who score just above the cutoff for school j and those who score just below. *In the neighborhood of the cutoff*, this is

$$E[\zeta_j|X_i, BSEE_i] = E(\mu_{j1}|Above = 1) - E(\mu_{j1}|Above = 0) \tag{11}$$

[24]This test also serves as a validation of the school fixed effects (i.e. value-added estimates).

VI.4 Effects on Post-Secondary Certification and Educational Attainment

Having established that the lack of a positive preferred school effect on secondary school outcomes was not driven by the marginal students, but was common to all students, we now examine the possibility than preferred schools impact longer-run outcomes. Looking beyond secondary school outcomes, we examine CAPE taking (a measure of continued education beyond secondary school) and earning a CAPE Associate's degree in the middle panel of Table 4. Overall, columns 1 and 2 show that attending a preferred school increases the likelihood of taking the CAPE by about 2 percentage points and increases the likelihood of earning an Associate's degree by 2.1 percentage points (significant at the 1 percent level). That is, despite a slight reduction in the likelihood of passing the secondary school exam, students at preferred schools are more likely to enter and complete the CAPE post-secondary education. In the results that look at men and women separately, there is no evidence of any differential effect by gender. The reduced form impacts of scoring above the cutoff on earning an Associate's degree are presented visually in the lower panel of Figure 3. As before, the visual evidence is consistent with the regression results. Similarly, the lower panel of Appendix Figure A4 shows that results remain positive and significant for any choice of bandwidth.

Preferred schools improve outcomes for those on the margin of pursuing post-secondary education and hurt outcomes for those on the margin of not passing the secondary school exam. Such patterns are consistent with schools focusing their efforts on the high-achieving students to the detriment of their lower-achieving classmates. If parents were aware of such dynamics, parents of low-achieving children should prefer less selective schools, but the choice data do not show this to be the case. While these positive effects may seem small in absolute magnitudes, they are substantial relative to the population mean. The estimated positive effect on CAPE taking of 2 percentage points is equivalent to 13.16 percent of the sample average, and almost 100 percent of the average at the least preferred (selective) schools.[25] The remaining outcomes we examine come from survey data. As such, to assuage any lingering concerns regarding the representativeness of our survey or biases in the survey sample, we show that our academic achievement results are similar when we restrict the sample to those linked to the surveys (see Appendix Table 1).

Our measure of completed educational attainment comes from survey data and is measured for respondents who were between 25 and 40 years old at the time of the survey (this is the age range for which we find that individuals have completed their education in Table 2). Attending a more selective school increases the likelihood of earning a CAPE post-secondary credential. Given that many individuals pursue university studies after the CAPE, one might expect to see increases in overall years of educational attainment. Overall, Table 4, panel C shows that attending a preferred

[25] Given that for the longer-term outcomes we focus on BSSEE cohorts 1987-2002, Appendix Table 1, panel C shows estimated CAPE effects for the cohorts that have CAPE data availability and overlap with this longer-term sample (i.e. BSSEE cohorts 1998-2002). While less precise, the results are similar.

school increases years of educational attainment by 0.677 years (p-value<0.1). However, this effect is entirely driven by women. Women who attend a preferred school have 1.644 more years of education (column 4), while there is no average effect for men (columns 5 and 6). This increased years of education for women is reflected in a 17.4 percentage point higher likelihood of having a university degree (column 4). There is no effect on the likelihood that men complete university. The reduced form visual evidence is presented in Figure 5. While the figures are noisy in the survey data (owing to a smaller sample size), one can see a clear discontinuity in years of education for women that is not present for men. Consistently, Appendix Figure A5 shows that estimated effects for women, while noisier for narrower bandwidths, remain positive and stable for any choice of bandwidth. Given that both male and female students who attend preferred schools experienced increased CAPE completion, the lack of an overall educational effect for men is surprising. We examine possible mechanisms behind this asymmetric result in Section VI.7.

Given the increased years of education for women (but not men), a standard human capital model (e.g. Becker 1975) would predict improved labor market outcomes for women (and perhaps none for men). However, if attending a preferred school grants both men and women access to better job referral networks (e.g. Ioannides and Loury 2004; Schmutte 2015) or serves as a signal of ability in the labor market (e.g. Spence 1973; MacLeod and Urquiola 2015), there could be large gains to attending a preferred school for both sexes. We examine this possibility next.

VI.5 Effects on Labor Market Outcomes

The first labor market outcome we examine is the likelihood of being employed. We find positive effects of attending a preferred school on adult employment. Panel A of Table 5 shows that attending a more selective school increases the likelihood of being employed by 10.1 percentage points (column 2). This is a 14.2 percent increase relative to the sample average. While the positive effect on employment is larger for women, we can't reject the null of equality of effects between women and men. However, the source of these positive effects differs between women and men.

For women, the increase in employment is almost entirely explained through an equivalent reduction in unemployment. That is, preferred school attendance does not affect the likelihood of participating in the labor market, but it shifted women from unemployment to employment. In contrast, the increased male employment does not correspond to a decrease in unemployment but rather a decrease in being out of the labor force. Attending a preferred school reduces the likelihood that a man is out of the labor market by 11.4 percentage points (p-value<0.05). While the estimated effects on unemployment and employment are not significant for men, it appears that men who attended a preferred school were more likely to be in the labor market, and as a result, more likely to be employed and more likely to be searching for a job (unemployed). In sum, the effects of attending a preferred school are unambiguously positive for women, and mixed or neutral for men. This mirrors the educational attainment results, suggesting that increased human capital is the operative mechanism for women. We also examine effects on occupational prestige by classifying reported occupations into those that are managerial or professional versus technical or clerical. Table 5 panel B shows strong positive effects for women. For women, preferred school attendance increased the likelihood of being employed as a manager or professional (as opposed to a technical or clerical role) by 24.6 percentage points (a doubling of the sample average) (column 4). For men, the estimated effect is negative and marginally significant; selective school attendance *decreased* the likelihood of being employed as a manager or professional by about 9 percentage points. The reduced form Regression Discontinuity plot is presented in the top panel of Figure 6. The sensitivity of these effects to bandwidth choices is presented in the top panel of Appendix Figure A6. Results for women are positive, stable and significant for any choice of bandwidth.

As one might expect given the pattern of results, women who attend a preferred school have higher earnings while men do not. This can be seen visually in the lower panel of Figure 6. Attending a preferred school increases women's monthly wages by about 42 percent (p-value$<$0.01). This is a large estimated effect, but the 95 percent confidence interval lies between 10 and 71 percent. For men, the estimated effect is small and not statistically distinguishable from zero. The lower panel of Appendix Figure A6 shows consistent positive effects for women across all choices of bandwidth, while effects for men are close to zero and insignificant. These results are consistent with the increased educational attainment associated with attending a preferred school (among women) being rewarded in the labor market. The fact that we observe no wage increase for men (for whom there was no increase in overall educational attainment) suggests that the benefits for women are not driven by more elite schools signaling ability.

VI.6 Effects on Adult Health

In addition to educational attainment and labor market performance, health status is another key component of human capital. Preferred school attendance could have potentially affected long-term health through increasing healthy behaviors such as getting regular physical exercise. Panel A of Table 6 shows the estimated preferred school effects on adult preventive health behaviors. Attending a preferred school increases the likelihood of attending a gym at least once per week by 12.5 percentage points on average (column 2). The effects are similar for both women and men. Relative to the sample average, this represents a sizable 98.4 percent increase. With respect to having medical insurance, there is no effect overall. However, there is suggestive evidence that women who attend a preferred school are more likely to have health insurance. Consistent with the notion that attending a preferred school may lead to better practices, attending preferred school increases the likelihood of having an annual dental checkup by 11 percentage points (column 2). We compute a summary index of preventive health behavior by averaging the incidence of gym attendance, medical insurance, and yearly dental checkup. Attending a more selective school increases overall preventive health behavior by 8.6 percentage points (column 2). This effect is similar for both women and men and can be seen visually in the top panel of Figure 7.[26] The fact that we observe improved behaviors despite no appreciable increase in health insurance, suggests that these improvements are due to improved behavioral norms at more preferred schools. Importantly, even though there were only educational and employment benefits for women, long-term preventive health behaviors improved for both women and men because of selective school attendance.

Since health behaviors improved, one might expect objective health outcomes to have improved also. Table 6, panel B shows that selective school attendance increased the likelihood of being within a normal BMI range by 16.7 percentage points (column 2).[27] This effect is similar for both women and men. This increased likelihood is largely driven by a decreased incidence of being overweight or obese. The visual evidence presented in the lower panel of Figure 7 illustrates this.[28] The patterns suggest that the improved health practices caused by attending a preferred school may have translated into improved objective health outcomes for both sexes.

VI.7 Mechanisms

Here we explore some potential mechanisms operating behind the observed effects. Because we do not have independent variation in all the potential causal pathways, the patterns presented in this section are suggestive. One possible benefit to attending a preferred school (that would not be related to test score impacts) is that individuals who attend more selective schools might gain access to better-connected social networks thus facilitating higher quality social capital that can be leveraged to improve employment opportunities (Schmutte, 2015). To examine this possibility, we test whether preferred school attendance influences the likelihood of being referred for one's current job during adulthood by somebody in one's secondary school network. Table 7, panel A shows that the likelihood of having been referred to one's current job by somebody in one's secondary school network increases by 3.7 percentage points due to attending a preferred school. This effect is large given that only 0.6 percent of individuals in the population benefit from such a referral. Columns 4 and 6 reveal that the referral effect is similar for women and men.[29] The fact that both men and women experience similar increases in the likelihood of employment suggests that improved referral networks may be a mechanism. It would also explain why men who attend preferred

[26] The top panel of Appendix Figure A7 shows that estimates remain stable for any choice of bandwidth.

[27] Using the objective anthropometric measures captured in the survey, we calculated Body Mass Indexes (BMI) and classified persons by whether they are within normal weight, underweight, or overweight or obese. Following international standards, we classified persons as underweight if they have a BMI below 18.5. Persons are classified within normal weight if they have a BMI below 25 but on or above of 18.5. Persons with a BMI of 25 or above are classified as overweight or obese.

[28] Appendix Figure A7 shows that estimates are similar and significant for any choice of bandwidth.

[29] The estimated effect can be seen visually in the top left panel of Appendix Figure A8.

schools experience higher employment even though they do not have more years of education. In contrast, men (who have no increase in education) have no wage increases while women do, suggesting that referrals do not explain the higher wages for women. Taken together, the pattern of results is consistent with the referral network effect leading to increased employment for both men and women, and the increased years of education (for women) increasing wages (for women) conditional on being employed. We examine the education mechanism further below.

We found that attending a preferred school increased CAPE taking and obtaining an Associate's degree for both women and men, but we only observed increased years of education and college-going for women. One plausible explanation for these findings is that the gains in educational attainment for men who pass the CAPE were offset by some losses among those who do not pass the secondary school exam in the aggregate. Indeed, the reduction in qualifying for tertiary education (based on CSEC performance) and earning a CAPE Associate's degree are opposite in sign and almost identical in magnitude. However, the *differences* in overall educational attainment between men and women would imply that women who complete the CAPE were more likely than men to continue their studies and pursue a university degree. To test for such differential studying behaviors by sex, we rely on younger cohorts. Among these younger cohorts aged between 17 and 24 when surveyed (BSSEE cohorts 2003 – 2011), we examine whether being a full-time student is listed as the main occupation. We refer to this as "studying" for short. Panel B of Table 7 shows that attending a more preferred school increases the likelihood that a person between the ages of 17 and 24 is a full-time student by about 11.6 percentage points. These effects, while imprecisely estimated by gender, are larger for women than for men.[30]

One possible explanation for the lack of increased male earnings at preferred schools is that preferred schools led males to pursue more academic oriented programs (as evidenced by increase CAPE passing) at the expense of pursuing technical and vocational training. If such technical/vocational training has a higher rate of return than academic programs *for these marginal males*, the labor market impacts could be negative.[31] To assess this, we examine the impact of attending a preferred school on having a technical/vocation credential (see Table 4). The point estimates indicate that attending a preferred school has no impact on having a technical/vocation credential –inconsistent with preferred schools reducing technical vocational training.

The results above suggest that girls who attend preferred schools may be more likely than boys to continue their studies between the ages of 17 and 24. More generally, while school quality has been often found to improve outcomes for girls more than for boys (e.g. Jackson 2010; Deming et al. 2014; Clark 2010) the reasons for the gender differences are not well understood. One po-

[30]The estimated effect for women can be seen visually in the top right panel of Appendix Figure A8.

[31]This explanation is proposed in Clark and Del Bono (2016) to explain why males may not have higher earnings from attending elite schools in Scotland during the 1960s. They are unable to test this hypothesis using their data.

tential explanation is that attending a better school reduces the likelihood of teen pregnancy (which disproportionately impacts girls). Given that teen motherhood has been shown, by some, to adversely impact educational attainment and earnings (e.g. Fletcher and Wolfe 2009), this could explain the pattern of results. We examine this using questions about the dates of birth of one's children in the sample of women aged 25 to 40. Panel C of Table 7 shows that the likelihood of giving birth by age 18 is reduced by 6.2 percentage points (p-value$<$0.05). Relative to the average in the population, this represents a considerable 59 percent decrease.[32] To examine whether our estimated effects reflect decreased fertility or delayed fertility, we also examine overall fertility. We find no impact on the likelihood of having a baby by the age of 25, having at least one baby ever, or on the number of children (Appendix Table 4). This suggests that preferred schools lead to delayed child bearing rather than reduced fertility. This result, in conjunction with the improved health behaviors, suggests that preferred schools may lead to greater patience and possibly reduced risk-taking. Given the strong documented associations between teen motherhood and educational attainment, this is plausible evidence that decreased teen motherhood may have played a key role in explaining the long-term improvements in educational attainment and employment for women.

VI.8 Elite Schools or Preferred Schools?

Much of the literature on the effects of attending a selective school has focused on the most elite schools. For example, Clark and Del Bono (2016) focus on the effect of three selective schools in Aberdeen, Scotland; while Abdulkadiroğlu et al. (2014) examine three elite schools in Boston and three elite schools in New York City. To relate our results to existing work, we examine the extent to which the patterns we uncover are common to all preferred schools, or if the most elite schools are different. We classify elite schools as the most preferred seven schools based on the revealed preference ranking across all years. This cut corresponds to those schools where the average incoming BSSEE score of applicants is above the population mean. We estimate the main results separately for cutoffs to elite and non-elite schools and report them in Table 8. We focus our discussion on the educational and labor market outcomes.

Looking first at girls, (columns 2 and 5), we see that there is no appreciable impact on short run examination performance in either the elite or non-elite schools. However, the improved tertiary outcomes appear to be driven almost entirely by the most elite schools. Attending an elite preferred school increases the likelihood of women earning a CAPE associates degree by 4.4 percentage points (p-value$<$0.05), while the effect for a non-elite preferred school is small and not significantly different from zero.[33] Consistent with this, elite school attendance increases years of education by 1.84 years (p-value$<$0.1) and increases the likelihood of having a university degree

[32] Teen motherhood estimated effects can be seen visually in the lower panel of Appendix Figure A8.

[33] This is visually presented in the top panel of Appendix Figure A9.

by 31 percentage points (p-value$<$0.05). The impacts of attending a non-elite preferred school are smaller than for elite schools, not statistically significantly different from zero, but are positive. The results indicate that attending a preferred school increases girls' educational attainment, but that the increases are larger and more robust for the most elite schools. This conclusion is supported by the pattern among the younger cohorts (panel D); attending a preferred elite school increases the likelihood of women being in school between the ages of 17 and 24. The non-elite schools have no appreciable effect on the likelihood of a woman being a student between the ages of 17 and 24.

For all of the labor market outcomes, the estimated impacts for elite and non-elite schools are similar, and for none of the labor market outcomes can one reject that the impacts are the same in the two sets of schools. By and large, irrespective of the elite status of the school, for women, attending a preferred school increases employment by between 7 and 16 percentage points, reduces unemployment by between 15 and 21 percentage points, increases the likelihood of having a managerial of professional occupation by between 25 to 33 percentage points, and increases the wage by between 30 and 65 percent. In both elite and non-elite schools one is more likely to have been referred for their current job by a high school friend, but the point estimate is larger for elite schools. Looking at teen motherhood, however, there are some differences. Almost all of the reduced teen motherhood is from attending a less elite preferred school.[34] This result is not surprising given that teen motherhood rates are very low among women at the most elite schools. However, it may suggest that the improved labor market outcomes for women at the most elite schools are not driven by teen motherhood alone, but by relatively larger gains in educational attainment and somewhat higher reliance in their high school social networks.[35] Taken together, the results for women suggest that preferred schools improve longer-run outcomes despite having no impact on short run test scores. However, elite schools have more robust positive impacts on educational attainment, while non-elite schools have more robust impacts on teen motherhood. Overall, both groups of preferred schools are associated with similar labor market gains for women.

For men, attending a more preferred school has a negative impact on secondary school performance at both elite and non-elite schools (columns 3 and 6). The most elite schools appear to reduce the likelihood of CSEC taking by about 4.3 percentage points, while the impact of non-elite schools is smaller and not statistically significant. However, looking at the likelihood of qualifying for tertiary education, both elite and non-elite preferred schools reduce qualifying for tertiary education by about 2 percentage points. Despite the negative impact on secondary outcomes, attending an elite preferred school has positive impacts on male post-secondary outcomes. For men, attending the most elite schools increases the likelihood of earning a CAPE Associate's degree by

[34]The visual evidence presented in the lower panel of Appendix Figure A9 illustrates this.

[35]Having said this, it is worth noting that there could be sizable impacts on teen *pregnancy* in both settings, but that it only reduced teen *motherhood* at the non-elite schools.

3.1 percentage points. As with women, the positive impact on the CAPE are driven entirely by the elite schools. Despite the positive impact on the CAPE, the point estimates for attending an elite school are negative for years of education and having a university degree. This may reflect that positive impacts on educational attainment for some men, are offset by some ill effects on educational attainment for others (recall the lower likelihood of CSEC taking).[36]

We find no robust impacts on labor market outcomes for males, irrespective of the elite status of the secondary school. This may reflect offsetting positive impacts (increases in CAPE passing and better social connections) and negative impacts (decreased CSEC taking). The one exception is that attending a preferred non-elite school does appear to reduce the likelihood that men will be out of the labor market (with no statistically significant impact on employment or wages). Overall, attending a preferred school (irrespective of the elite status of the school) may have some deleterious impacts on male outcomes in the short-run, but little impact on longer-run outcomes.

VII Discussion and Conclusions

Using administrative education data from Barbados, we document that there is considerable agreement regarding which schools are preferred, and that these preferences are held by parents of both girls and boys. We also document that a preferred school and a more selective school are almost synonymous. Given these patterns, one would expect that attending a more preferred school would confer sizable benefits to the students who attend them. However, using examination performance in secondary school, we find little evidence that this is the case. Attending a more selective school has no appreciable effect on girls' outcomes and may *decrease* high school examination performance among boys. We implement a new empirical test to examine the extent to which these impacts for the marginal admit differ from those of the average student. This test reveals that the average impacts of schools (measured using value-added) are statically indistinguishable from that of the marginal admit (using the RD variation through the cutoff). These findings can be explained by (a) parents being uninformed about which schools improve child outcomes, or (b) parents valuing school impacts on a broader set of outcomes than those measured by achievement tests, or both.

Our results support both explanations. Specifically, for girls, despite the lack of test score impacts in the short run, we find considerable longer-run benefits to attending a preferred (or more selective) school. Girls who attend a more preferred school attain more years of education, are more likely to have a university degree, are more likely to be employed, have higher status jobs, and have higher labor market earnings. They are also less likely to have a teen birth (but not less overall fertility) and enjoy better health. In sum, for girls, school impacts on secondary school

[36]We find no significant impact of attending a non-elite school on the overall educational attainment of men. However, among the younger cohorts (panel D), there is some suggestive indication that attending a preferred non-elite school may increase the likelihood of being studying between the ages of 17 and 24.

examinations are a poor measure of impact on girls' longer-run outcomes. Given the broad array of improved outcomes we document, it would be reasonable for parents to prefer more selective schools despite the lack of any secondary school test score gains. The pattern of results for girls born in Barbados in the 1970s and 1980s are remarkably similar to those of girls born in the 1950s in England (Clark and Del Bono, 2016). They are also in line with a pattern of girls benefiting more from better schools than boys (e.g. Jackson 2010, Deming et al. 2014).

In contrast to the patterns for girls, the pattern of results and school preferences for boys are more difficult to rationalize. For boys, attending a preferred school may actually decrease high school examination performance with no impact on qualifying for tertiary education. Looking at longer-run outcomes, boys who attend more preferred schools do have improved post-secondary outcomes (as measured by the CAPE), but do not have any more years of educational attainment and may be less likely to attend university. Consistent with this, we find little evidence that attending a preferred school improves the labor market outcomes for men. We *do* find that attending a preferred school decreases the likelihood of being out of the labor market and increases the chances of being referred for one's current job by a high-school colleague. It is possible that men who would have not worked leverage connections from preferred schools to find employment. However, we also find an increase in men searching for employment, suggesting that men who otherwise would not have been seeking employment (or working) are more likely to seek employment and not find work. While this is highly speculative, such patterns could reflect the fact that social norms among individuals at more selective schools are more work and employment oriented. One area for which we do find positive effects is on health as preferred school attendance increased the practice of preventive health behaviors and reduced the incidence of being overweight or obese. However, overall, evidence of long-run benefits for men is mixed. The fact that parental preferences for selective schools are similar for parents of boys and girls, indicates that parents of boys may be relatively uninformed of the causal impacts of schools on their sons.

It is important to note that, on average, more preferred schools do confer important long-run benefits in terms of educational attainment and labor market outcomes. As such, parental preferences *in the aggregate* are reasonable. The disconnect between parental preferences and causal impacts (for boys) can be explained by parents not differentiating between effects for boys and effects for girls. From a policy perspective our results suggest that school impacts on test scores may not be the best measure of a school's impacts on longer-run outcomes. Accordingly, policymakers should be cautious (and thoughtful) regarding using test score impacts in accountability systems and incentive pay schemes. The findings also suggest that parents are relatively well informed about schools that improve outcomes on average, but that many parents (especially those of boys) could benefit from better information about the heterogeneous causal impacts of particular schools for various outcomes.

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Figure 1. School Choices

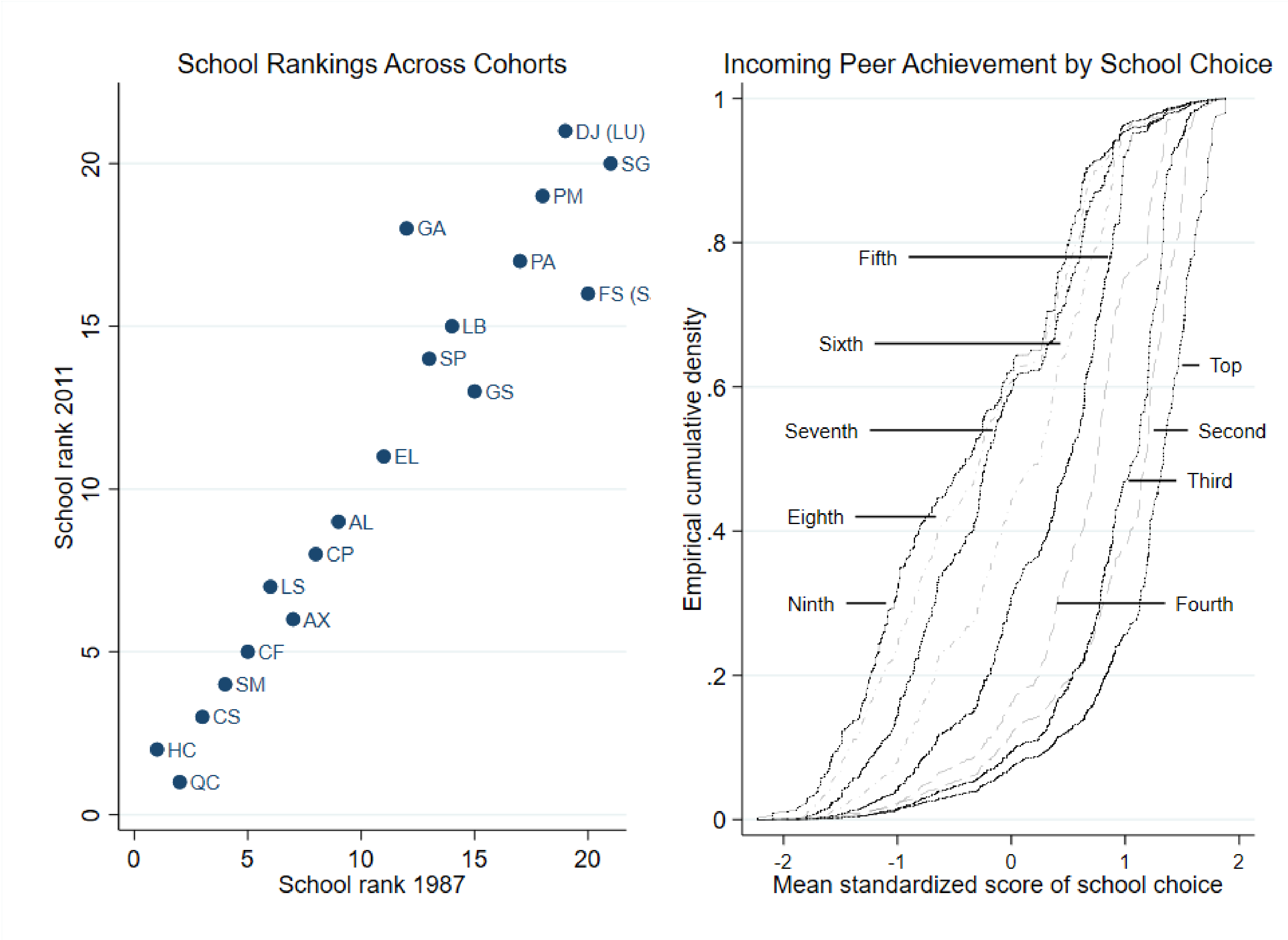
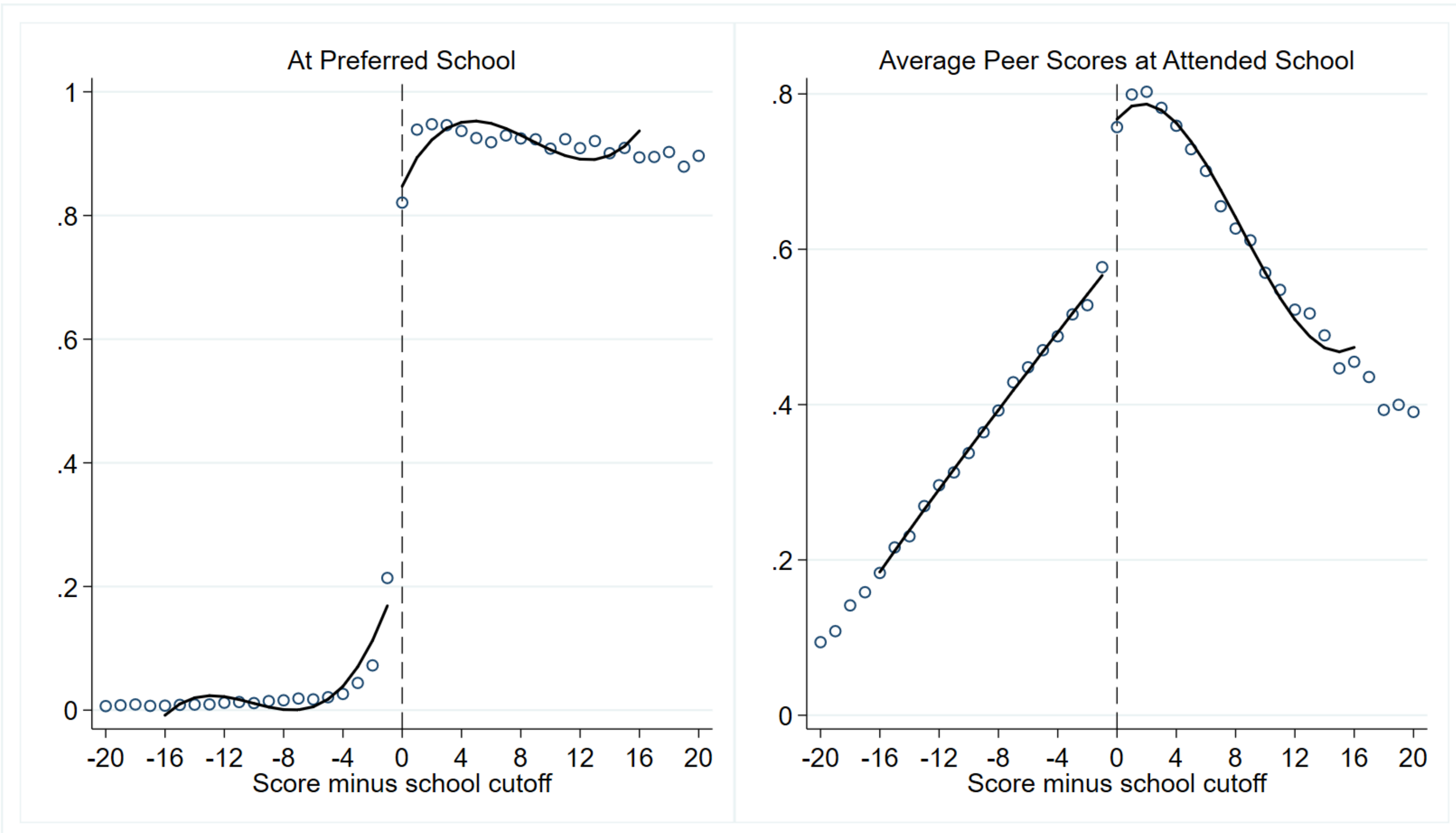
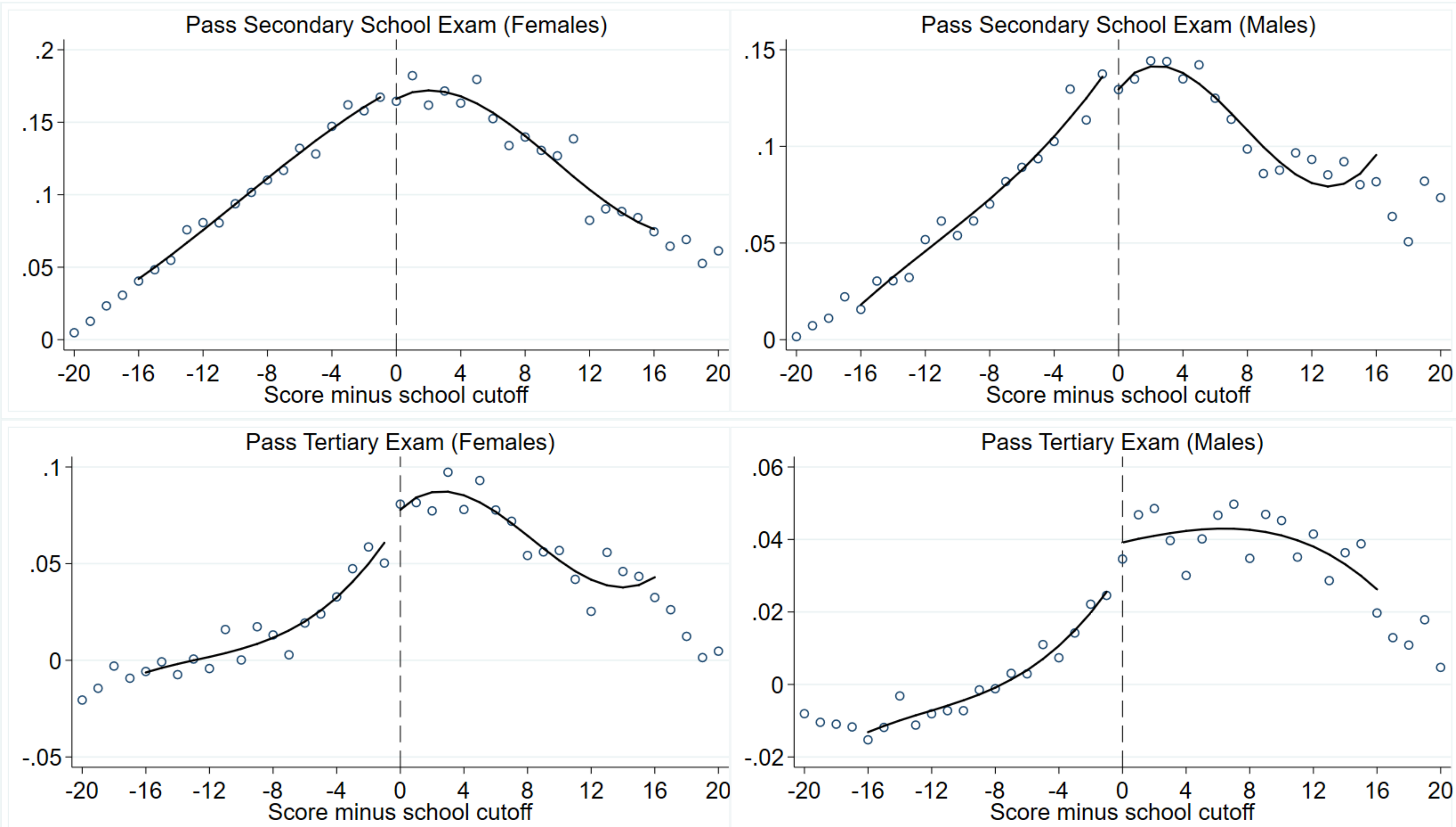


Figure 2. RD Effect on Attending a Preferred School and Peer Incoming Scores



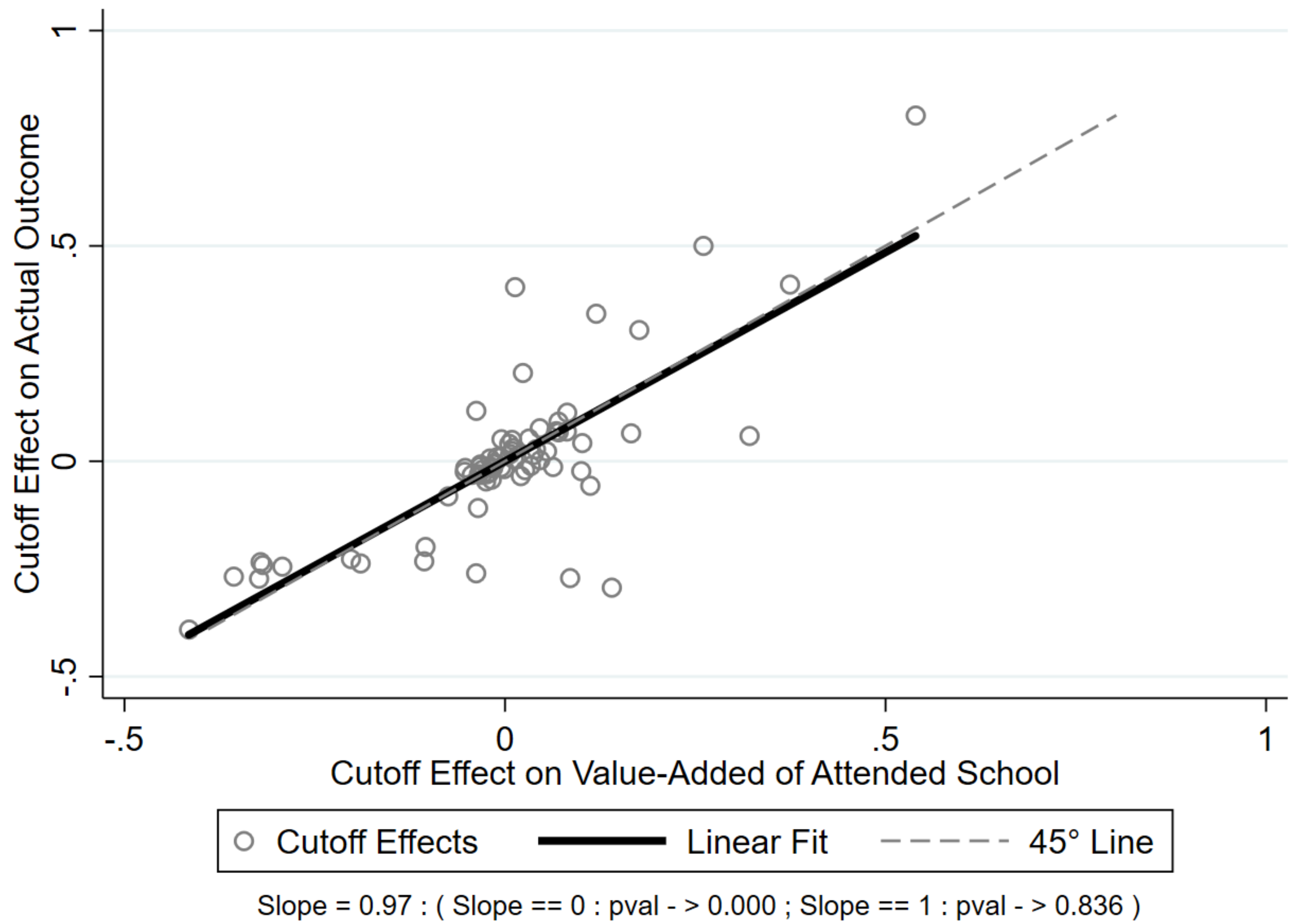
Notes: The X-axis is the BSSEE score relative to the cutoff. The circles are outcome means corresponding to 1-point bins of the relative score. The solid lines are the fitted outcomes generated by fitting a third degree polynomial of the relative score fully interacted with the 'Above' indicator.

Figure 3. RD Effect on Secondary and Tertiary Education for Females and Males



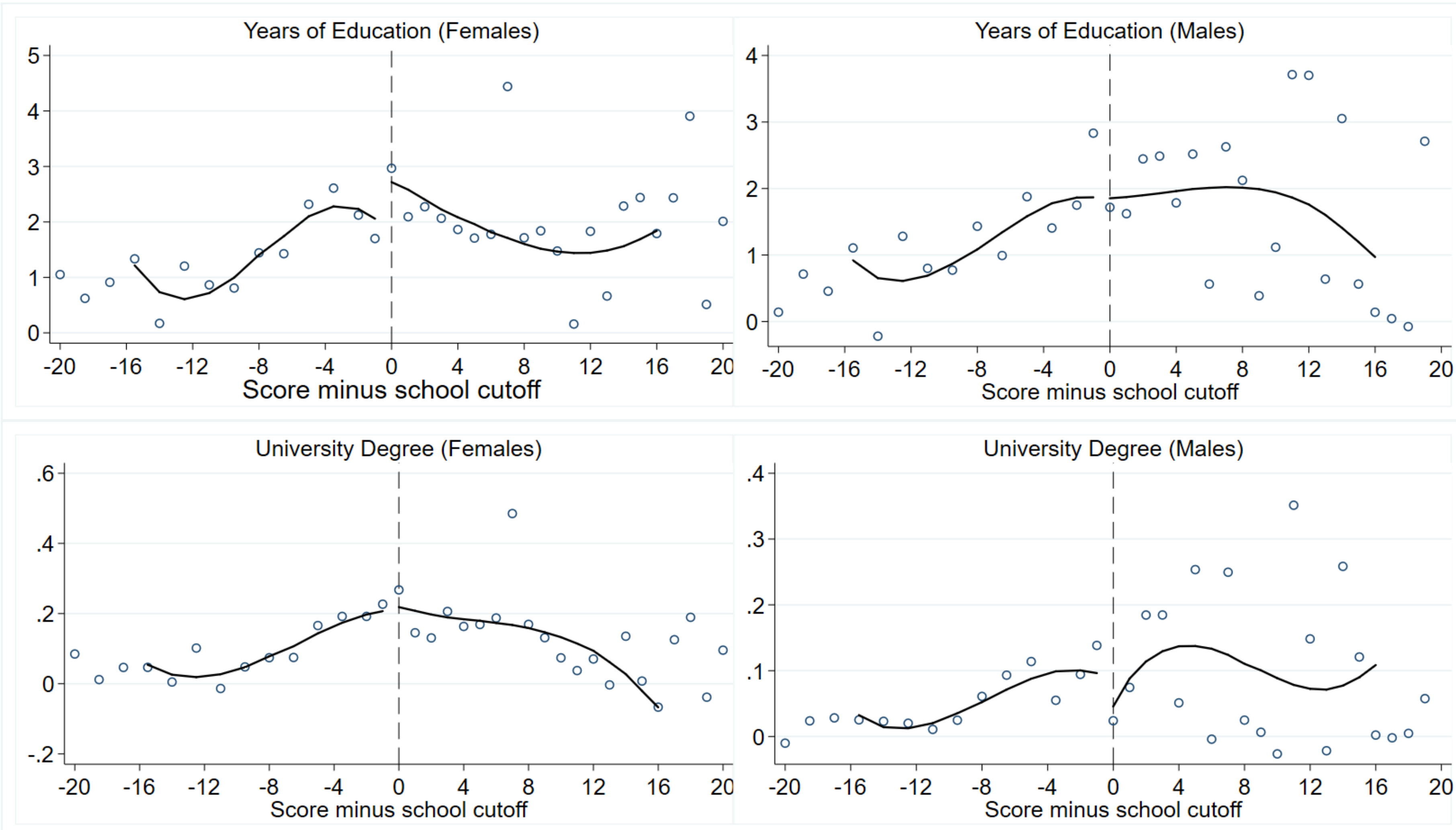
Notes: The X-axis is the BSSEE score relative to the cutoff. The Y-axis is the mean outcome for each relative score (net of the mean for the cutoff). The circles represent 1-point bins of the relative score. The solid lines are the fitted outcomes generated by fitting a third degree polynomial of the relative score fully interacted with the 'Above' indicator.

Figure 4. Predicted Cutoff Effects vs Actual Cutoff Effects - CSEC Outcomes



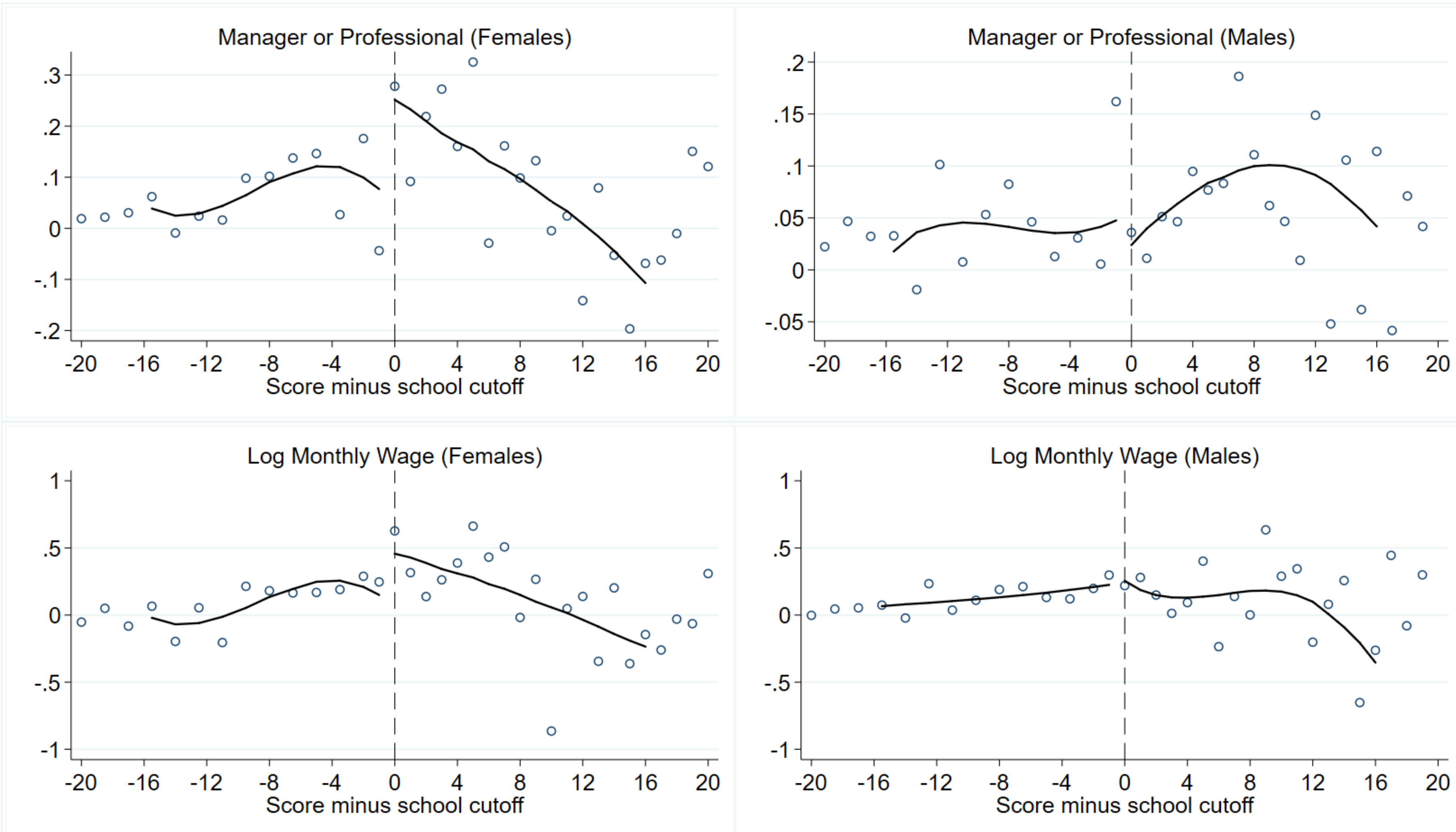
Notes: The X-axis represents the estimated coefficients on the 'Above' indicator resulting from equation (10); estimated for each school j and for each CSEC outcome (school value-added measures enter as dependent variables). The Y-axis represents the estimated coefficients on the 'Above' indicator resulting from reduced form models as in equation (5); estimated for each school j and for each CSEC outcome (individual level outcomes enter as dependent variables).

Figure 5. RD Effect on Educational Attainment for Females and Males



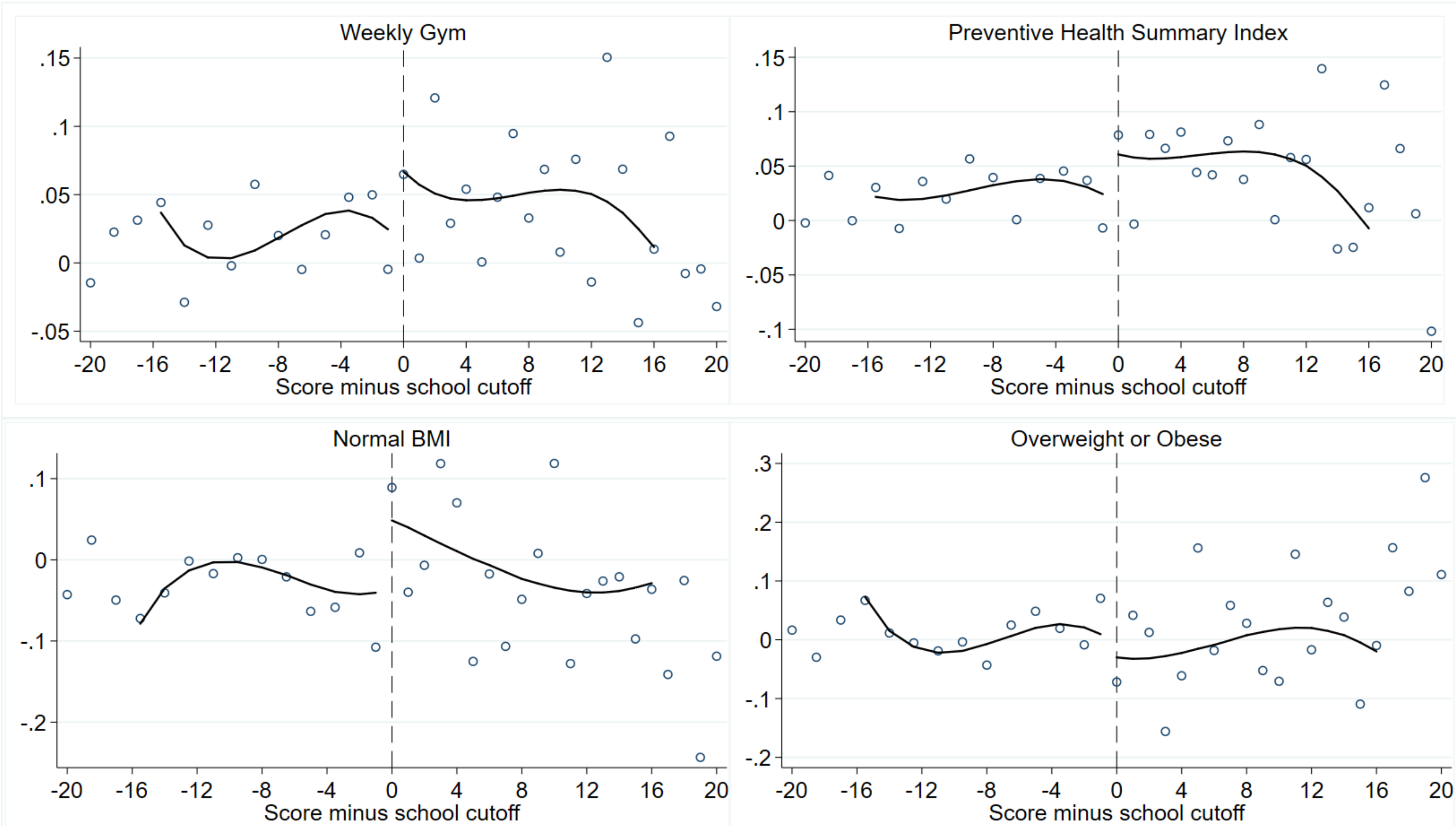
Notes: The X-axis is the BSSEE score relative to the cutoff. The Y-axis is the mean outcome for each relative score (net of the mean for the cutoff). The circles represent 1-point bins of the relative score. The solid lines are the fitted outcomes generated by fitting a third degree polynomial of the relative score fully interacted with the 'Above' indicator.

Figure 6. RD Effect on Employment Quality and Wages for Females and Males



Notes: The X-axis is the BSSEE score relative to the cutoff. The Y-axis is the mean outcome for each relative score (net of the mean for the cutoff). The circles represent 1-point bins of the relative score. The solid lines are the fitted outcomes generated by fitting a third degree polynomial of the relative score fully interacted with the 'Above' indicator.

Figure 7. RD Effect on Health Outcomes (Males and Females Combined)



Notes: The X-axis is the BSSEE score relative to the cutoff. The Y-axis is the mean outcome for each relative score (net of the mean for the cutoff). The circles represent 1-point bins of the relative score. The solid lines are the fitted outcomes generated by fitting a third degree polynomial of the relative score fully interacted with the 'Above' indicator.

Table 1. Summary Statistics

School Rank Range (by revealed preferences):	All Schools (1)	1 - 8 (2)	9 - 16 (3)	17 - 24 (4)
Panel A: Baseline Characteristics (prior to secondary school enrollment)				
Standardized BSSEE score	0.000	1.049	0.052	-0.983
	(1.000)	(0.451)	(0.475)	(0.621)
Female	0.497	0.507	0.625	0.384
	(0.500)	(0.500)	(0.484)	(0.486)
Admitted cohort size	156.88	157.84	161.66	151.74
	(47.70)	(21.34)	(54.67)	(58.54)
Panel B: CSEC Performance (after 5 years of secondary school)				
Took at least 1 subject	0.677	0.883	0.776	0.411
	(0.468)	(0.321)	(0.417)	(0.492)
Number of subjects passed	2.188	4.550	1.868	0.331
	(2.910)	(3.252)	(2.176)	(1.037)
Qualified for tertiary *	0.268	0.597	0.184	0.041
	(0.443)	(0.490)	(0.387)	(0.198)
Individuals	95,391	31,567	28,613	35,211
Panel C: CAPE Performance (after 2 years of post-secondary studies)				
Took at least 1 unit	0.152	0.368	0.060	0.010
	(0.359)	(0.482)	(0.237)	(0.102)
Number of units passed	0.865	2.210	0.230	0.039
	(2.338)	(3.346)	(1.180)	(0.506)
Earned Associate Degree	0.080	0.210	0.017	0.003
	(0.272)	(0.407)	(0.128)	(0.056)
Individuals	43,984	15,542	13,477	14,965

Notes: All individuals who took the BSSEE between 1987 and 2011 are included in panels A and B. However, as CAPE examinations started in 2005, panel C only includes individuals who took the BSSEE between 1998 and 2009. Standard deviations are reported in parentheses below the means. * Qualification for tertiary education requires passing five CSEC examinations including English and Math.

Table 2. Summary Statistics cont'd

BSSEE Cohorts (Age at Survey):	1987-2011 (17-40)	2003-2011 (17-24)	1987-2002 (25-40)	
	(1)	(2)	(3)	
Main Occupation				
In labor force	0.739	0.558	0.860	
	(0.438)	(0.499)	(0.335)	
Studying	0.131	0.314	0.010	
	(0.336)	(0.466)	(0.092)	
Individuals	1,545	605	940	
School Rank Range (by revealed preferences) - BSSEE cohorts 1987-2002:	All Schools	1 - 8	9 - 16	17 - 24
	(1)	(2)	(3)	(4)
Educational Attainment				
Years of education	11.331	15.359	11.377	8.498
	(4.413)	(3.873)	(3.268)	(3.434)
University degree	0.196	0.523	0.111	0.026
	(0.384)	(0.500)	(0.308)	(0.176)
Technical/Vocational degree	0.182	0.094	0.247	0.201
	(0.385)	(0.316)	(0.429)	(0.388)
Individuals	888	258	256	374
Employment Quality and Social Netwoks (only employed persons)				
Manager or professional	0.136	0.330	0.102	0.028
	(0.330)	(0.448)	(0.311)	(0.178)
Monthly gross wage (2016 US$)	1358.9	1936.4	1230.9	948.8
	(973.8)	(1192.4)	(843.3)	(628.9)
Referred to current job by school network	0.006	0.011	0.009	0.000
	(0.084)	(0.115)	(0.098)	---
Individuals	697	225	209	263
Teen Motherhood (only females)				
Baby by age 18	0.105	0.036	0.098	0.170
	(0.307)	(0.183)	(0.309)	(0.372)
Individuals	401	116	151	134
Preventive Health Behaviors				
Have medical insurance	0.301	0.496	0.256	0.193
	(0.451)	(0.500)	(0.434)	(0.380)
Attends yearly dental checkup	0.441	0.571	0.479	0.326
	(0.496)	(0.498)	(0.501)	(0.469)
Attends gym at least once per week	0.127	0.199	0.147	0.065
	(0.336)	(0.413)	(0.345)	(0.247)
Individuals	940	272	269	399

Notes: Standard deviations are reported in parentheses below the means. Statistics are weighted by the inverse of sampling probability to reflect survey design.

Table 3. First Stage

Estimation Sample:	Full Population: all observations		Full Population: +/- 0.75 SD from cutoff		Face to Face Survey		(4)=(6)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: Cutoff manipulation test							
Differential density	-0.7302		-0.8439		-0.9015		
[p-value]	[0.4652]		[0.3987]		[0.3673]		
Panel B: Predicted Outcomes - Reduced Form							
Predicted: CSEC subjects passed	0.005		0.001		0.002		0.976
	(0.010)		(0.029)		(0.047)		
Predicted: CAPE units passed	-0.009		<0.001		<0.001		0.812
	(0.009)		(0.002)		(0.003)		
Predicted: Years of education					0.117		
					(0.140)		
Predicted: Log monthly wage					0.021		
					(0.049)		
Panel C: First Stage							
Attended preferred school	0.811***	0.811***	0.745***	0.744***	0.745***	0.744***	0.975
	(0.024)	(0.024)	(0.016)	(0.016)	(0.033)	(0.033)	
Panel D: Survey Match Rate - 2SLS							
Matched with survey	<0.001	<0.001	-0.003	-0.004			
	(0.001)	(0.001)	(0.003)	(0.003)			
Panel E: School Environments Effects - 2SLS							
Peers BSSEE score	0.241***	0.240***	0.250***	0.254***	0.256***	0.247***	0.750
	(0.008)	(0.008)	(0.015)	(0.013)	(0.023)	(0.021)	
BSSEE coef. of variation	-0.009***	-0.010***	-0.010***	-0.010***	-0.012***	-0.009***	0.187
	(0.001)	(0.001)	(0.002)	(0.001)	(0.002)	(0.001)	
Cohort size	-11.356***	-11.379***	-12.906***	-13.237***	-13.344***	-10.799***	0.481
	(0.982)	(0.994)	(3.095)	(2.506)	(4.399)	(3.971)	
Parish HHI	-0.006***	-0.006***	-0.002	-0.001	-0.002	-0.005	0.392
	(0.001)	(0.001)	(0.002)	(0.002)	(0.005)	(0.005)	
Observations	375,131	375,131	184,595	184,595	5,610	5,610	
Sociodemographics	Yes	Yes	Yes	Yes	Yes	Yes	
BSSEE cubic spline	Yes	Yes	Yes	Yes	Yes	Yes	
Cutoff fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	
Preferences fixed effects	No	Yes	No	Yes	No	Yes	

Notes: Estimated standard errors in parenthesis two-way clustered at the individual and BSSEE score levels. Sociodemographic controls include student gender and parish fixed-effects. Panel A reports the results of the McCrary (2008) cutoff manipulation test. Panels B and C report estimated coefficients on the 'Above' indicator resulting from reduced form models as in equation (5) of the text. Panels D and E report estimated 2SLS coefficients on 'Attend' a preferred school using 'Above' as the excluded instrument (resulting from equation system (5) - (6) in the text). Models with predicted outcomes as left-hand side variables (Panel B) do not control for preferences as the selectivity of preferences were used when predicting the outcomes. Regressions in columns (5) and (6) are weighted by the inverse of sampling probability to reflect survey design. Column (7) reports the p-value of a test for the equality of estimates reported in columns (4) and (6). Sample corresponds to BSSEE cohorts 1987 - 2002 (25 - 40 years old when surveyed). + Significant at 15%; * Significant at 10%; ** significant at 5%; *** significant at 1%

Table 4. 2SLS Effects on Educational Outcomes

	All		Women		Men		(4)=(6)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: CSEC Performance. Sample: BSSEE cohorts 1998 - 2009 (Full Population +/- 0.75 SD from cutoff)							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Took at least 1 subject	0.002	0.000	0.006	0.005	-0.003	-0.005	0.611
	(0.010)	(0.010)	(0.013)	(0.012)	(0.015)	(0.015)	
Number of subjects passed	-0.028	-0.026	0.110	0.106	-0.152*	-0.145*	0.030
	(0.061)	(0.061)	(0.091)	(0.091)	(0.078)	(0.077)	
Qualified for tertiary	-0.017*	-0.017*	-0.019	-0.020	-0.016	-0.014	0.784
	(0.010)	(0.010)	(0.016)	(0.016)	(0.013)	(0.013)	
Observations	106,670	106,670	54,631	54,631	52,039	52,039	
Panel B: CAPE Performance. Sample: BSSEE cohorts 1998 - 2009 (Full Population +/- 0.75 SD from cutoff)							
Took at least 1 unit	0.020**	0.019*	0.019	0.018	0.020*	0.019*	0.977
	(0.010)	(0.010)	(0.015)	(0.016)	(0.012)	(0.012)	
Number of units passed	0.197***	0.193***	0.175*	0.171*	0.218***	0.213***	0.735
	(0.061)	(0.061)	(0.103)	(0.103)	(0.068)	(0.068)	
Earned Associate Degree	0.021***	0.021***	0.027**	0.026**	0.017**	0.016**	0.491
	(0.007)	(0.007)	(0.012)	(0.012)	(0.008)	(0.008)	
Observations	106,670	106,670	54,631	54,631	52,039	52,039	
Panel C: Educational Attainment. Sample: BSSEE cohorts 1987 - 2002 (25 - 40 Years old at Survey)							
Years of education	0.615*	0.677*	1.409**	1.644***	-0.189	-0.238	0.022
	(0.365)	(0.369)	(0.568)	(0.580)	(0.535)	(0.527)	
University degree	0.043	0.045	0.168**	0.174***	-0.063	-0.066	0.007
	(0.043)	(0.042)	(0.067)	(0.066)	(0.056)	(0.056)	
Technical/Vocational degree	0.017	0.025	-0.037	-0.008	0.053	0.049	0.512
	(0.051)	(0.046)	(0.050)	(0.048)	(0.082)	(0.072)	
Observations	5,277	5,277	2,510	2,510	2,767	2,767	
Sociodemographics	Yes	Yes	Yes	Yes	Yes	Yes	
BSSEE cubic spline	Yes	Yes	Yes	Yes	Yes	Yes	
Cutoff fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	
Preferences fixed effects	No	Yes	No	Yes	No	Yes	

Notes: This table reports estimated 2SLS coefficients on 'Attend' a preferred school using 'Above' as the excluded instrument (resulting from equation system (5) - (6) in the text). Estimated standard errors in parenthesis two-way clustered at the individual and BSSEE score levels. Sociodemographic controls include student gender and parish fixed-effects. Samples in Panels A and B correspond to BSSEE cohorts that have both CSEC and CAPE data available (BSSEE cohorts 1998 - 2009). This because the earliest CAPE outcome data corresponds to year 2005 which is associated with the 1998 BSSEE cohort; while the latest CAPE data corresponds to year 2016 which is associated with the 2009 BSSEE cohort. Panel C uses the matched survey data covering BSSEE cohorts 1987-2002 (25 - 40 years old when surveyed) and these regressions are weighted by the inverse of sampling probability to reflect survey design. Column (7) reports the p-value of a test for the equality of estimates reported in columns (4) and (6). + Significant at 15%; * Significant at 10%; ** significant at 5%; *** significant at 1%

Table 5. 2SLS Effects on Labor Market Outcomes

BSSEE Cohorts:	1987 - 2002: 25 - 40 Years old at Survey						
	All		Women		Men		(4)=(6)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: Main Occupation							
Employed	0.097*	0.101**	0.140*	0.133*	0.065	0.076	0.566
	(0.053)	(0.050)	(0.076)	(0.070)	(0.074)	(0.071)	
Unemployed	-0.022	-0.039	-0.110*	-0.126**	0.052	0.037	0.032
	(0.044)	(0.039)	(0.064)	(0.054)	(0.059)	(0.055)	
Out of labor force	-0.075**	-0.062*	-0.030	-0.007	-0.117**	-0.114**	0.179
	(0.036)	(0.037)	(0.051)	(0.054)	(0.056)	(0.054)	
Observations	5,610	5,610	2,616	2,616	2,994	2,994	
Panel B: Employment Quality (only employed persons)							
Manager or professional	0.057	0.056	0.248***	0.246***	-0.094*	-0.088+	0.001
	(0.048)	(0.049)	(0.078)	(0.078)	(0.056)	(0.057)	
Log monthly wage	0.176*	0.158+	0.420***	0.413***	-0.015	-0.043	0.014
	(0.097)	(0.099)	(0.158)	(0.159)	(0.113)	(0.117)	
Observations	4,003	4,003	1,772	1,772	2,231	2,231	
Sociodemographics	Yes	Yes	Yes	Yes	Yes	Yes	
BSSEE cubic spline	Yes	Yes	Yes	Yes	Yes	Yes	
Cutoff fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	
Preferences fixed effects	No	Yes	No	Yes	No	Yes	

Notes: This table reports estimated 2SLS coefficients on 'Attend' a preferred school using 'Above' as the excluded instrument (resulting from equation system (5) - (6) in the text). Estimated standard errors in parenthesis two-way clustered at the individual and BSSEE score levels. Sociodemographic controls include student gender and parish fixed-effects. Regressions are weighted by the inverse of sampling probability to reflect survey design. Column (7) reports the p-value of a test for the equality of estimates reported in columns (4) and (6). + Significant at 15%; * Significant at 10%; ** significant at 5%; *** significant at 1%

Table 6. 2SLS Effects on Health Outcomes

BSSEE Cohorts:	1987 - 2002: 25 - 40 Years old at Survey						
	All		Women		Men		(4)=(6)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: Preventive Health Behaviors							
Attends gym at least once per week	0.117***	0.125***	0.140***	0.136***	0.092+	0.107*	0.683
	(0.039)	(0.039)	(0.051)	(0.050)	(0.056)	(0.056)	
Have medical insurance	0.027	0.028	0.136*	0.102	-0.065	-0.035	0.196
	(0.055)	(0.052)	(0.073)	(0.072)	(0.080)	(0.076)	
Attends yearly dental checkup	0.107*	0.111**	0.006	0.043	0.200**	0.176**	0.225
	(0.055)	(0.054)	(0.080)	(0.076)	(0.079)	(0.077)	
Summary index	0.083***	0.086***	0.089**	0.090**	0.078*	0.083*	0.902
	(0.030)	(0.029)	(0.040)	(0.039)	(0.044)	(0.043)	
Observations	5,166	5,166	2,444	2,444	2,722	2,722	
Panel B: Objective Health Outcomes (based on BMI)							
Normal weight	0.183***	0.167***	0.186**	0.200**	0.190**	0.129+	0.586
	(0.065)	(0.064)	(0.094)	(0.095)	(0.090)	(0.085)	
Overweight or Obese	-0.145**	-0.141**	-0.171*	-0.191**	-0.126+	-0.082	0.383
	(0.064)	(0.063)	(0.093)	(0.092)	(0.087)	(0.082)	
Underweight	-0.038+	-0.026	-0.015	-0.009	-0.064*	-0.047+	0.433
	(0.026)	(0.024)	(0.038)	(0.038)	(0.036)	(0.031)	
Observations	4,361	4,361	2,146	2,146	2,215	2,215	
Sociodemographics	Yes	Yes	Yes	Yes	Yes	Yes	
BSSEE cubic spline	Yes	Yes	Yes	Yes	Yes	Yes	
Cutoff fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	
Preferences fixed effects	No	Yes	No	Yes	No	Yes	

Notes: This table reports estimated 2SLS coefficients on 'Attend' a preferred school using 'Above' as the excluded instrument (resulting from equation system (5) - (6) in the text). Estimated standard errors in parenthesis two-way clustered at the individual and BSSEE score levels. Sociodemographic controls include student gender and parish fixed-effects. Regressions are weighted by the inverse of sampling probability to reflect survey design. Column (7) reports the p-value of a test for the equality of estimates reported in columns (4) and (6). + Significant at 15%; * Significant at 10%; ** significant at 5%; *** significant at 1%

Table 7. 2SLS Effects on Mechanisms

	All		Women		Men		(4)=(6)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: Social Networks. Sample: BSSEE cohorts 1987 - 2002 (25 - 40 years old when surveyed)							
Referred to current job by	0.038**	0.037**	0.042*	0.043*	0.034+	0.033+	0.772
school network	(0.017)	(0.017)	(0.025)	(0.026)	(0.021)	(0.023)	
Observations	4,003	4,003	1,772	1,772	2,231	2,231	
Panel B: Youth Main Occupation. Sample: BSSEE cohorts 2003 - 2011 (17 - 24 years old when surveyed)							
Studying	0.133*	0.116*	0.173*	0.134	0.096	0.109	0.860
	(0.068)	(0.070)	(0.099)	(0.095)	(0.104)	(0.109)	
Observations	2,618	2,618	1,443	1,443	1,175	1,175	
Panel C: Teen Motherhood. Sample: BSSEE cohorts 1987 - 2002 (25 - 40 years old when surveyed)							
Baby by age 17			-0.067*	-0.057**			
			(0.040)	(0.023)			
Baby by age 18			-0.055	-0.062**			
			(0.053)	(0.030)			
Observations			2,271	2,271			
Sociodemographics	Yes	Yes	Yes	Yes	Yes	Yes	
BSSEE cubic spline	Yes	Yes	Yes	Yes	Yes	Yes	
Cutoff fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	
Preferences fixed effects	No	Yes	No	Yes	No	Yes	

Notes: This table reports estimated 2SLS coefficients on 'Attend' a preferred school using 'Above' as the excluded instrument (resulting from equation system (5) - (6) in the text). Estimated standard errors in parenthesis two-way clustered at the individual and BSSEE score levels. Sociodemographic controls include student gender and parish fixed-effects. Regressions are weighted by the inverse of sampling probability to reflect survey design. Panel B shows estimated effects on main occupations restricted to interviewed individuals who were part of BSSEE cohorts 2003 - 2011 (17 - 24 years old when surveyed). All other panels include interviewed individuals who were part of BSSEE cohorts 1987 - 2002 (25 - 40 years old when surveyed). Column (7) reports the p-value of a test for the equality of estimates reported in columns (4) and (6). + Significant at 15%; * Significant at 10%; ** significant at 5%; *** significant at 1%

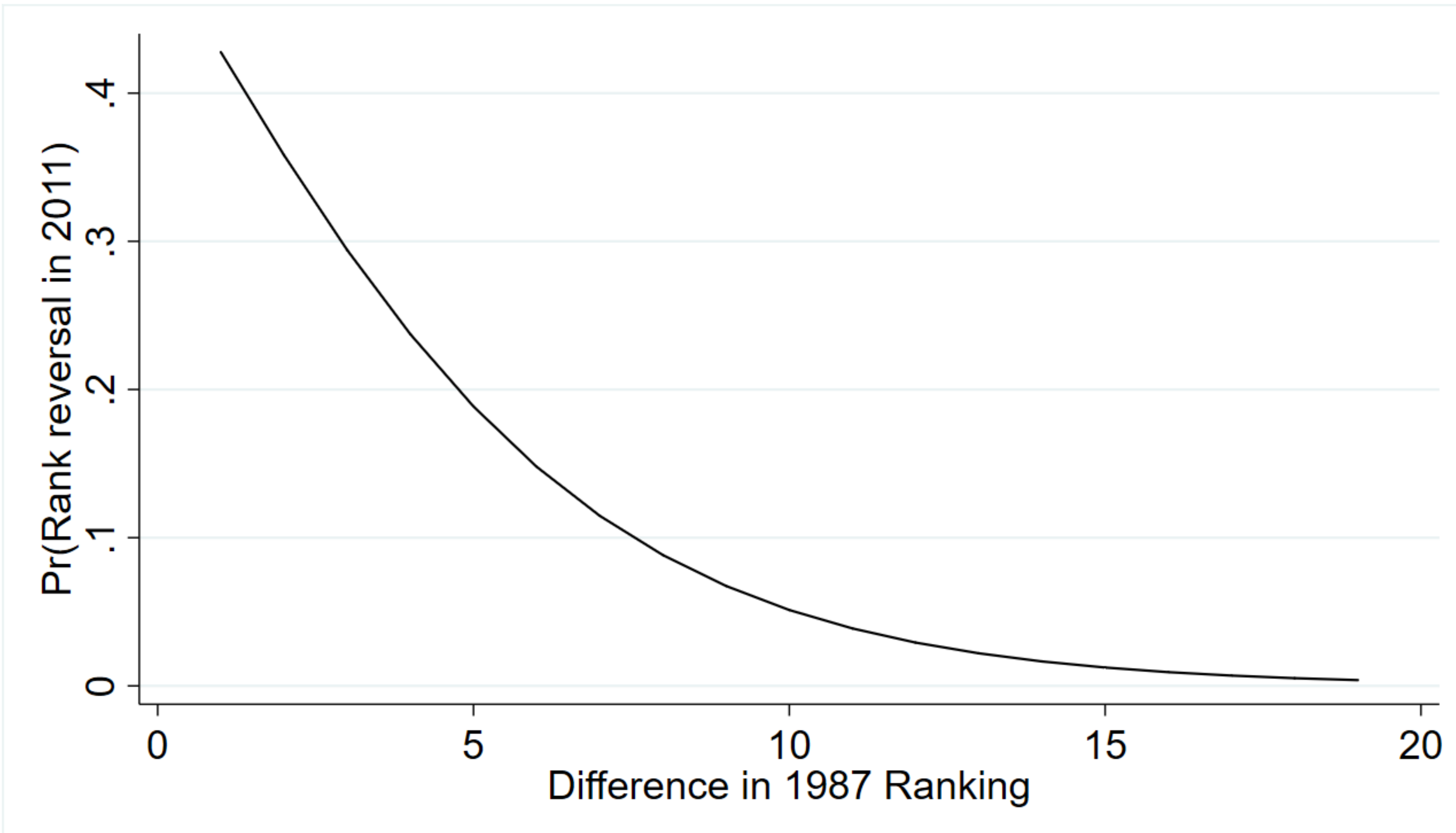
Table 8. 2SLS Effects by Elite School Status

	Elite Cutoff			Non-Elite Cutoff		
	All	Women	Men	All	Women	Men
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: CSEC Performance. Sample: BSSEE cohorts 1987 - 2002 (Full Population +/- 0.75 SD from cutoff)						
Took at least 1 subject	-0.021*	-0.003	-0.043**	-0.019	-0.022	-0.017
	(0.012)	(0.017)	(0.018)	(0.016)	(0.026)	(0.023)
Number of subjects passed	-0.174*	-0.009	-0.380***	-0.004	-0.005	-0.025
	(0.099)	(0.136)	(0.140)	(0.056)	(0.103)	(0.059)
Qualified for tertiary	-0.002	0.013	-0.020	-0.017*	-0.019	-0.019*
	(0.015)	(0.021)	(0.022)	(0.011)	(0.020)	(0.011)
Panel B: CAPE Performance. Sample: BSSEE cohorts 1998 - 2009 (Full Population +/- 0.75 SD from cutoff)						
Took at least 1 unit	0.036**	0.030	0.039+	-0.008	-0.010	-0.006
	(0.018)	(0.025)	(0.024)	(0.008)	(0.015)	(0.006)
Number of units passed	0.333***	0.262+	0.401***	-0.021	-0.020	-0.020
	(0.116)	(0.180)	(0.146)	(0.033)	(0.070)	(0.026)
Earned Associate Degree	0.038***	0.044**	0.031*	-0.002	-0.002	-0.001
	(0.014)	(0.022)	(0.016)	(0.004)	(0.008)	(0.003)
Panel C: Survey Sample BSSEE Cohorts 1987 - 2002 (25 - 40 years old when surveyed)						
Years of education	0.681	1.839*	-0.699	0.209	1.456	-0.623
	(0.820)	(1.103)	(1.170)	(0.562)	(1.057)	(0.673)
University degree	0.010	0.314**	-0.263*	0.011	0.091	-0.030
	(0.083)	(0.127)	(0.141)	(0.053)	(0.112)	(0.037)
Employed	0.030	0.068	-0.006	0.151+	0.160	0.134
	(0.066)	(0.119)	(0.122)	(0.097)	(0.136)	(0.135)
Unemployed	-0.057	-0.151**	0.006	-0.055	-0.210+	0.077
	(0.058)	(0.061)	(0.093)	(0.077)	(0.135)	(0.090)
Out of labor force	0.027	0.083	0.000	-0.096	0.050	-0.211**
	(0.045)	(0.099)	(0.095)	(0.073)	(0.116)	(0.103)
Manager or professional	0.060	0.254**	-0.189	0.098+	0.334**	0.005
	(0.100)	(0.121)	(0.142)	(0.062)	(0.159)	(0.050)
Log monthly wage	0.284	0.273	0.204	0.223	0.627	0.014
	(0.203)	(0.192)	(0.345)	(0.228)	(0.465)	(0.226)
Referred to current job by	0.056*	0.074	0.035+	0.031+	0.042	0.023
school network	(0.033)	(0.061)	(0.023)	(0.021)	(0.038)	(0.026)
Baby by age 18		-0.028			-0.176**	
		(0.076)			(0.080)	
Panel D: Survey Sample BSSEE cohorts 2003 - 2011 (17 - 24 years old when surveyed)						
Studying	0.246**	0.510***	0.008	0.080	-0.090	0.250*
	(0.119)	(0.176)	(0.154)	(0.097)	(0.126)	(0.145)

Notes: This table reports estimated 2SLS coefficients on 'Attend' a preferred school using 'Above' as the excluded instrument (resulting from equation system (5) - (6) in the text). Estimated standard errors in parenthesis two-way clustered at the individual and BSSEE score levels. Sociodemographic controls include student gender and parish fixed-effects. All regressions include a cubic spline in the BSSEE relative score interacted with the 'Above' indicator, cutoff fixed effects and preference fixed effects. Regressions are weighted by the inverse of sampling probability to reflect survey design. + Significant at 15%; * Significant at 10%; ** significant at 5%; *** significant at 1%

VIII Appendix

Appendix Figure A1. Probability of Rank Reversal



Appendix Figure A2. Raw Data Broadsheet

M I N I S T R Y O F E D U C A T I O N

DATE: 87/06/23 BARBADOS SECONDARY SCHOOLS ENTRANCE EXAMINATION - 1987

LISTING OF STUDENT MARKS BY PRIMARY SCHOOL PAGE 1

PRIMARY SCHOOL 014 : BOSCOBEL BOYS CENTRE C03

SEQ NO.	NAME OF STUDENT	SEX	AGE	S C H O O L C H O I C E S	ENG	MATHS	ESSAY	AVERAGE
001	[redacted]	M	11.10	CS SM AX CP AL EL SJ LB LU	036	028.75	C	032.375
002	[redacted]	M	11.00	CS SM CP AX AL EL SJ LU BB	037	030.00	D	033.500
003	[redacted]	M	11.00	CS SM CP AX EL SJ LU LB RS	022	015.00	D	018.500
004	[redacted]	M	11.07	SJ LB LU EL GS PA PM RS CP	026	006.25	D	016.125
005	[redacted]	M	11.08	HC CS AX CP SM EL LU SJ RS	070	055.00	C	062.500
006	[redacted]	M	11.06	HC QC CS SM CP AX EL RS SJ	039	021.25	D	030.125
007	[redacted]	M	11.09	CS CP AX AL EL SJ LB LU SM	017	042.50	E	029.750
008	[redacted]	M	11.02	HC QC CS AX CP AL EL SJ LU	053	027.50	C	040.250
009	[redacted]	M	11.07	LU EL SJ LB CP PM GA LS GS	017	001.25	E	009.125
010	[redacted]	M	11.11	HC QC SM CS AX CP AL EL SJ	044	013.75	E	028.875
011	[redacted]	M	11.03	CS AX CP AL EL SJ LB LU RS	019	010.00	E	014.500
012	[redacted]	M	11.06	CS SM AX CP AL EL SJ LB LU	063	031.25	D	047.125
013	[redacted]	M	11.00	CS AX CP AL EL SJ LB LU RS	033	023.75	D	028.375
014	[redacted]	M	11.09	LU LB SJ SG RS EL AX AL CP	018	003.75	E	010.875
015	[redacted]	M	11.01	AL CP AX LU SM LB SJ	038	020.00	E	029.000
016	[redacted]	M	11.02	CS SM CP AX AL EL SJ LU LB	056	035.00	C	045.500

M I N I S T R Y O F E D U C A T I O N

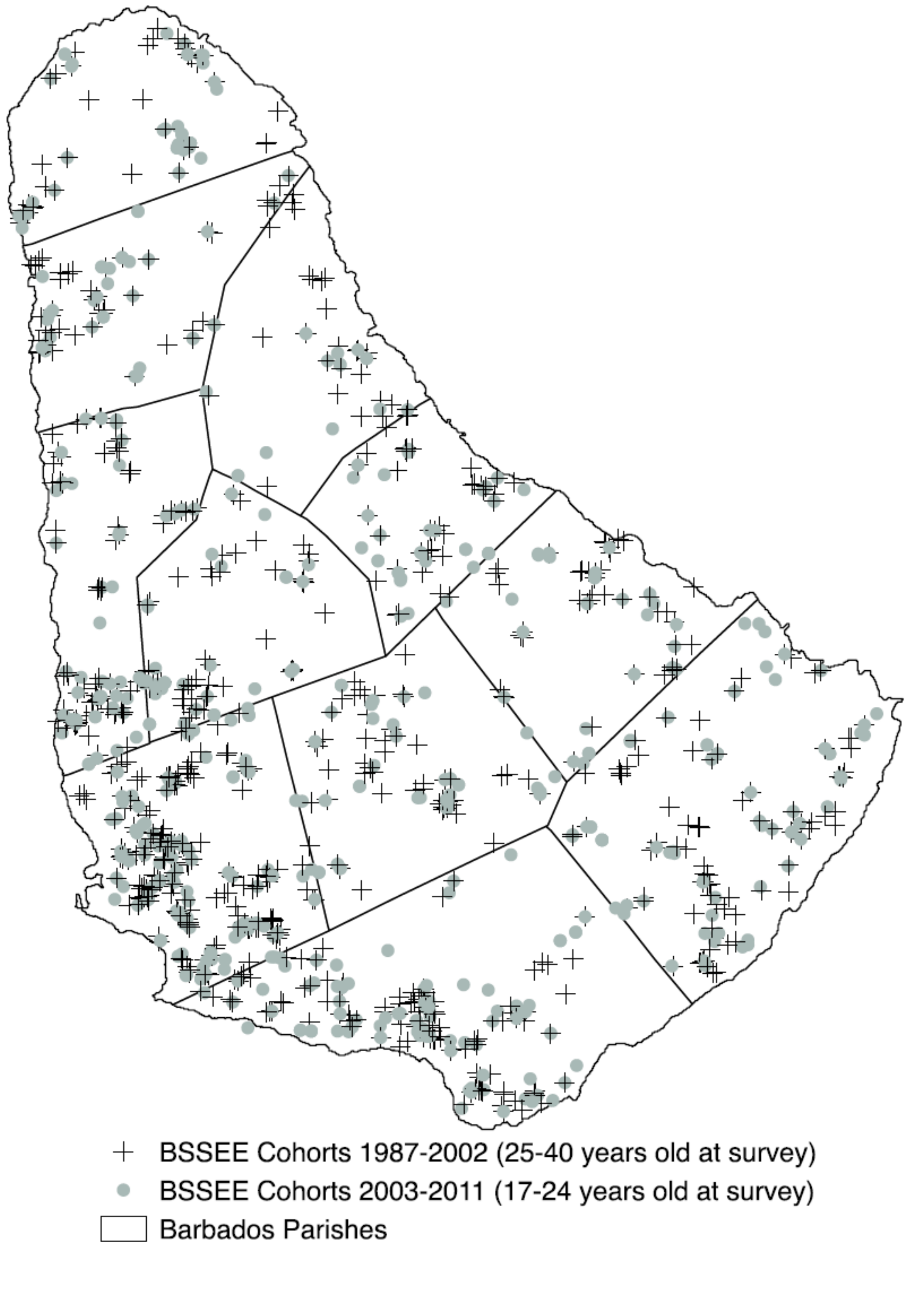
87 06 16 BARBADOS SECONDARY SCHOOLS ENTRANCE EXAMINATION ALLOCATION LISTING - 1987 PAGE 17

BOSCOBEL BOYS

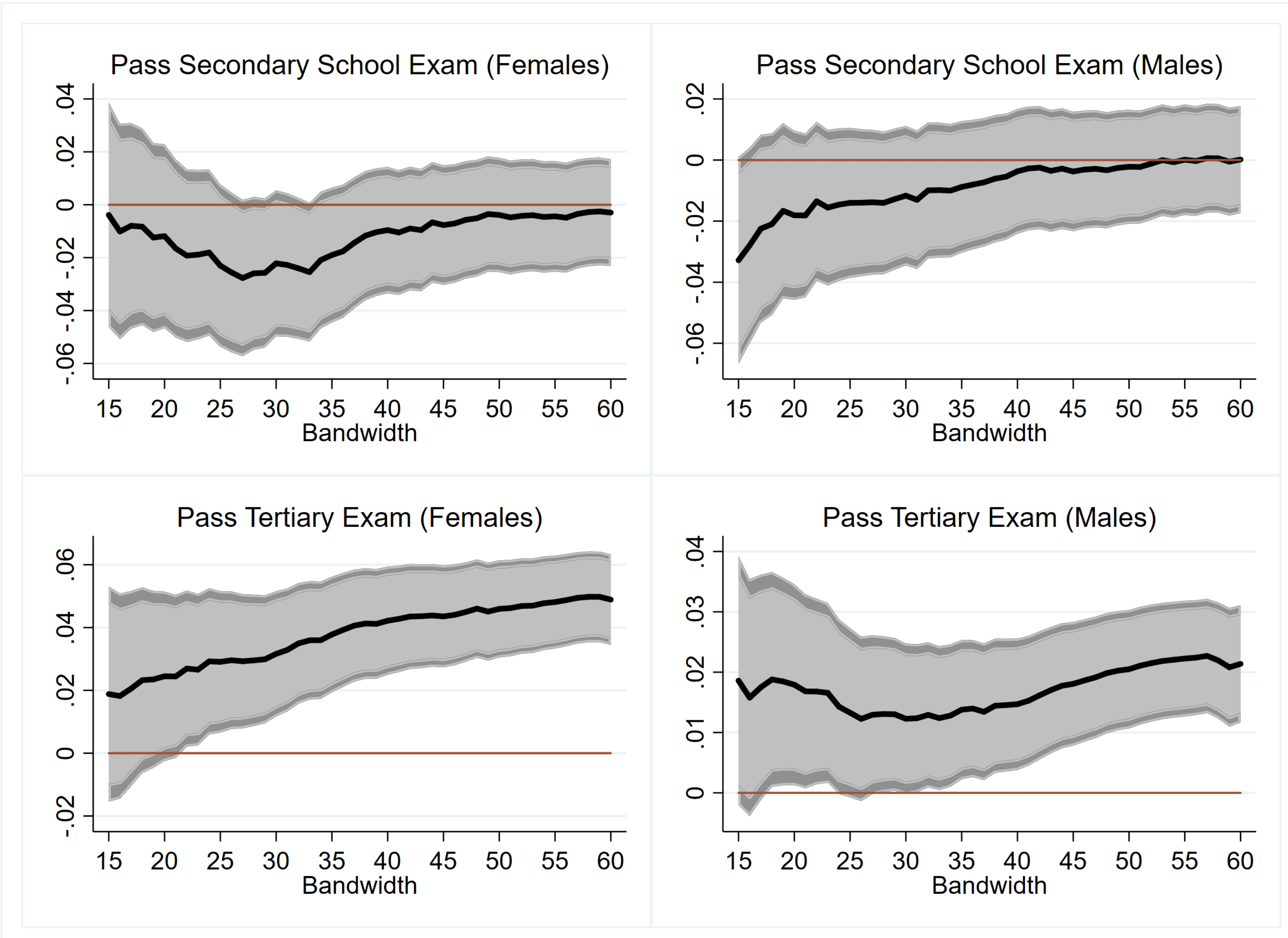
STUDENT NUMBER	N A M E O F S T U D E N T	A D D R E S S		ALLOCATION
0308	[redacted]	GAYS	E	ST LEONARD'S BOYS' SECONDARY
0309	[redacted]	GAYS	E	ST JAMES SECONDARY
0310	[redacted]	FRENCH VILLAGE	E	ST LUCY SECONDARY
0311	[redacted]	BOSCOBEL	A	BURSARY
0312	[redacted]	BOSCOBEL	A	ELLERSLIE SECONDARY
0313	[redacted]	BOSCOBEL	E	ST LEONARD'S BOYS' SECONDARY
0314	[redacted]	BALTIC	E	ST LEONARD'S BOYS' SECONDARY
0315	[redacted]	DATE TREE HILL	E	ST JAMES SECONDARY
0316	[redacted]	BOSCOBELLE	E	
0317	[redacted]	BOSCOBEL	E	ST LUCY SECONDARY
0318	[redacted]	GAYS	E	BURSARY
0319	[redacted]	MOORE HILL	E	ST JAMES SECONDARY
0320	[redacted]	GAYS	E	ST LEONARD'S BOYS' SECONDARY
0321	[redacted]	CASTLE	E	BURSARY
0322	[redacted]	ISOLATION RD BELLEPLAINE	A	ST LUCY SECONDARY
0323	[redacted]	BOSCOBEL	E	ST JAMES SECONDARY

Notes: Example of raw administrative BSSEE records for 1987. Similar records covering the 1987-1995 BSSEE cohorts were scanned and digitalized as only hard copies were available.

Appendix Figure A3. Matched Survey Geographical Distribution

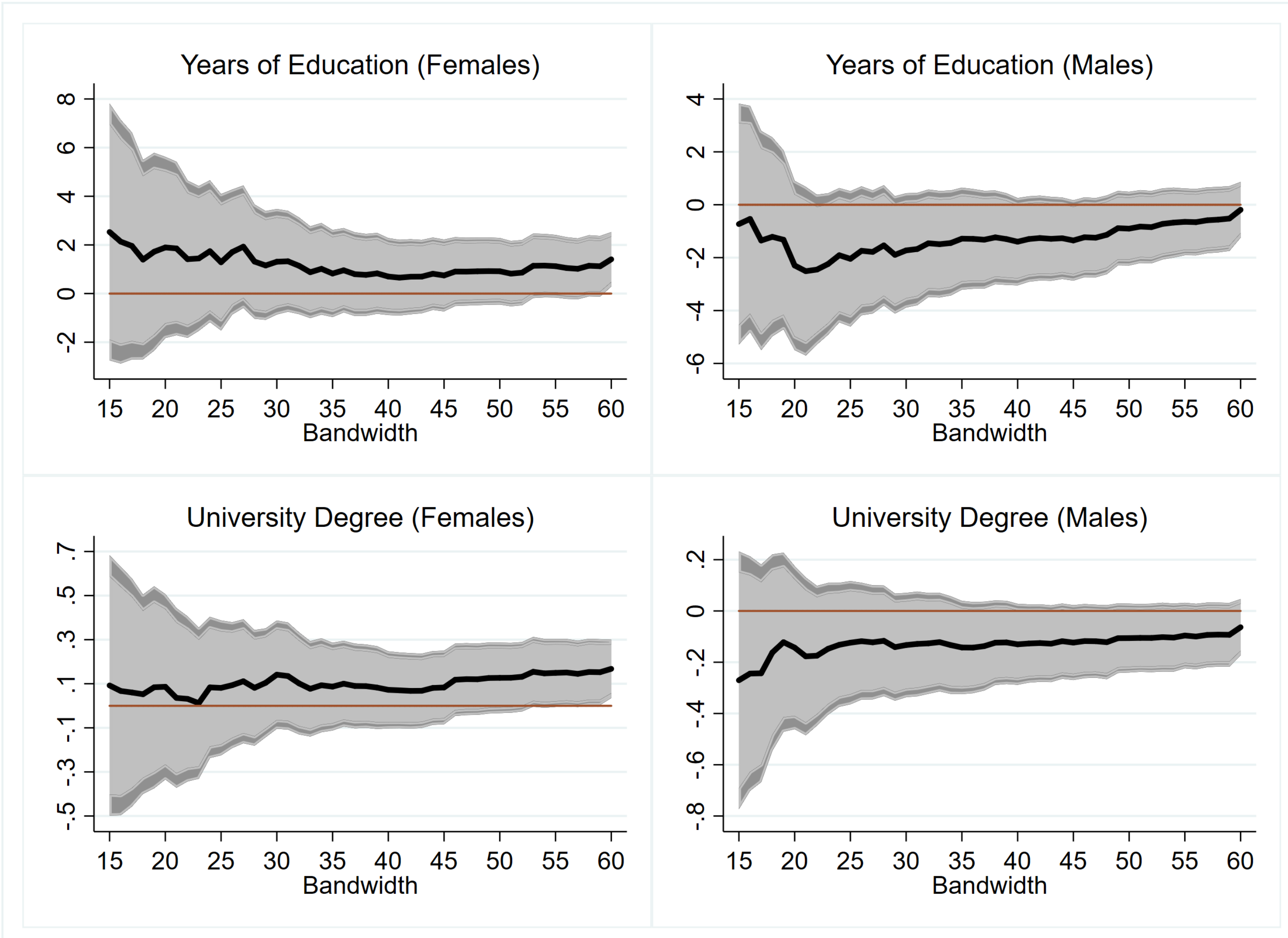


Appendix Figure A4. 2SLS Effects on Secondary and Tertiary Education by Bandwidth



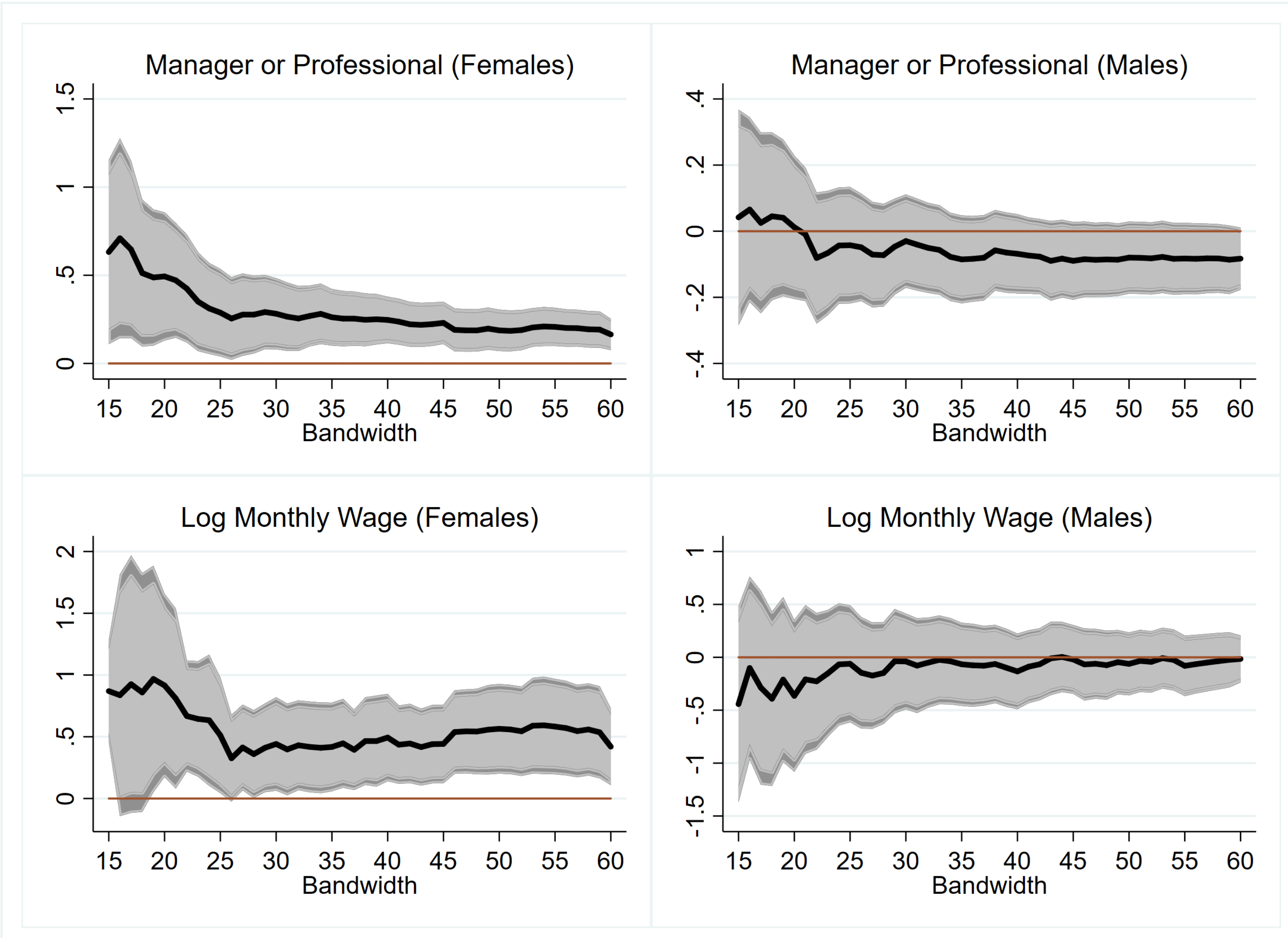
Notes: This figure depicts estimated 2SLS coefficients on 'Attend' a preferred school using 'Above' as the excluded instrument (resulting from equation system (5) - (6) in the text). The estimated 2SLS effects are reported for each bandwidth between +/-15 (+/-0.6sd) and +/-60 (+/-2.5sd). The 90 percent confidence interval for the estimate is presented in light gray, and the 95 percent confidence interval is in dark gray.

Appendix Figure A5. 2SLS Effects on Educational Attainment by Bandwidth



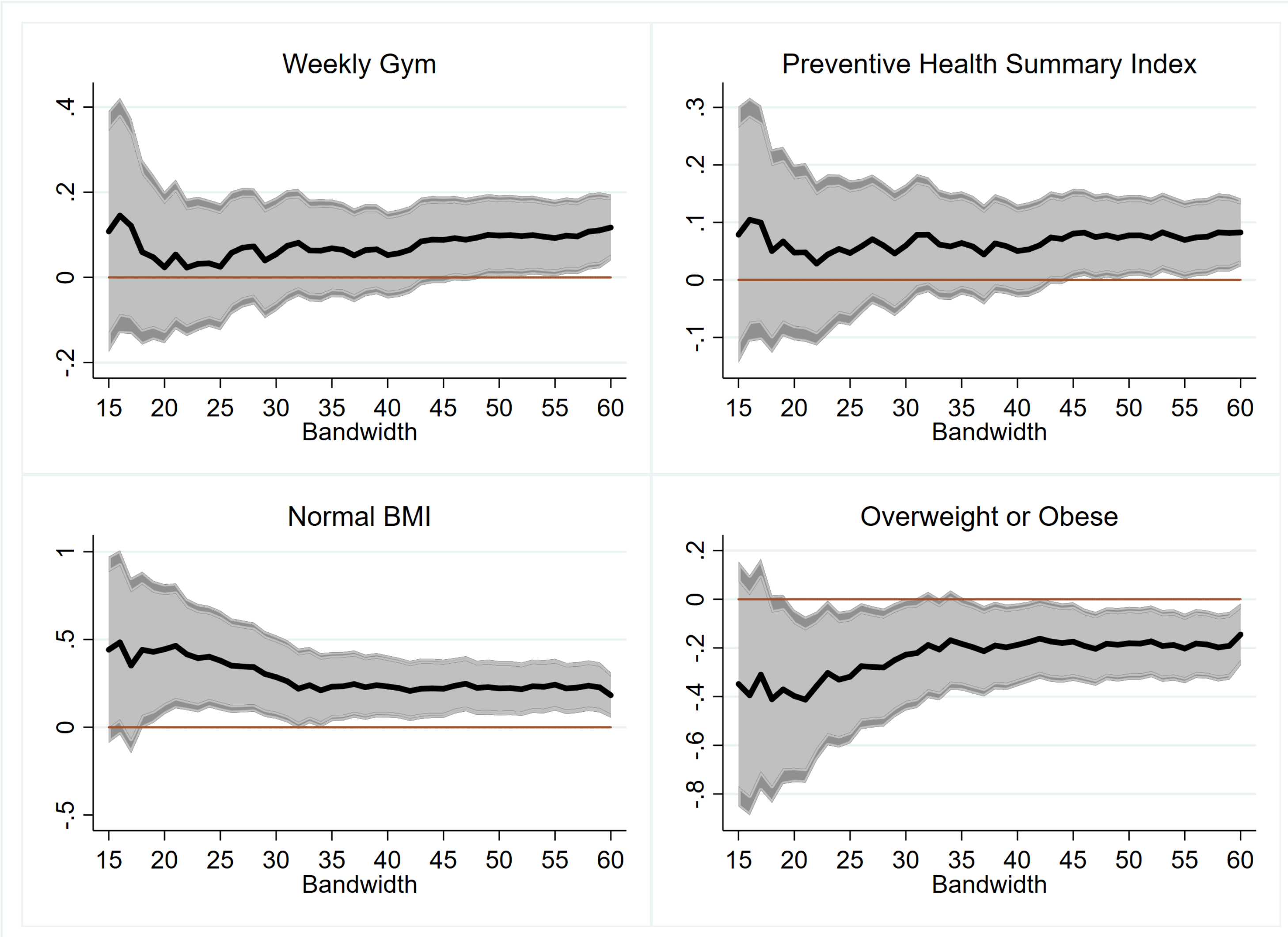
Notes: This figure depicts estimated 2SLS coefficients on 'Attend' a preferred school using 'Above' as the excluded instrument (resulting from equation system (5) - (6) in the text). The estimated 2SLS effects are reported for each bandwidth between +/-15 (+/-0.6sd) and +/-60 (+/-2.5sd). The 90 percent confidence interval for the estimate is presented in light gray, and the 95 percent confidence interval is in dark gray.

Appendix Figure A6. 2SLS Effects on Employment Quality and Wages by Bandwidth



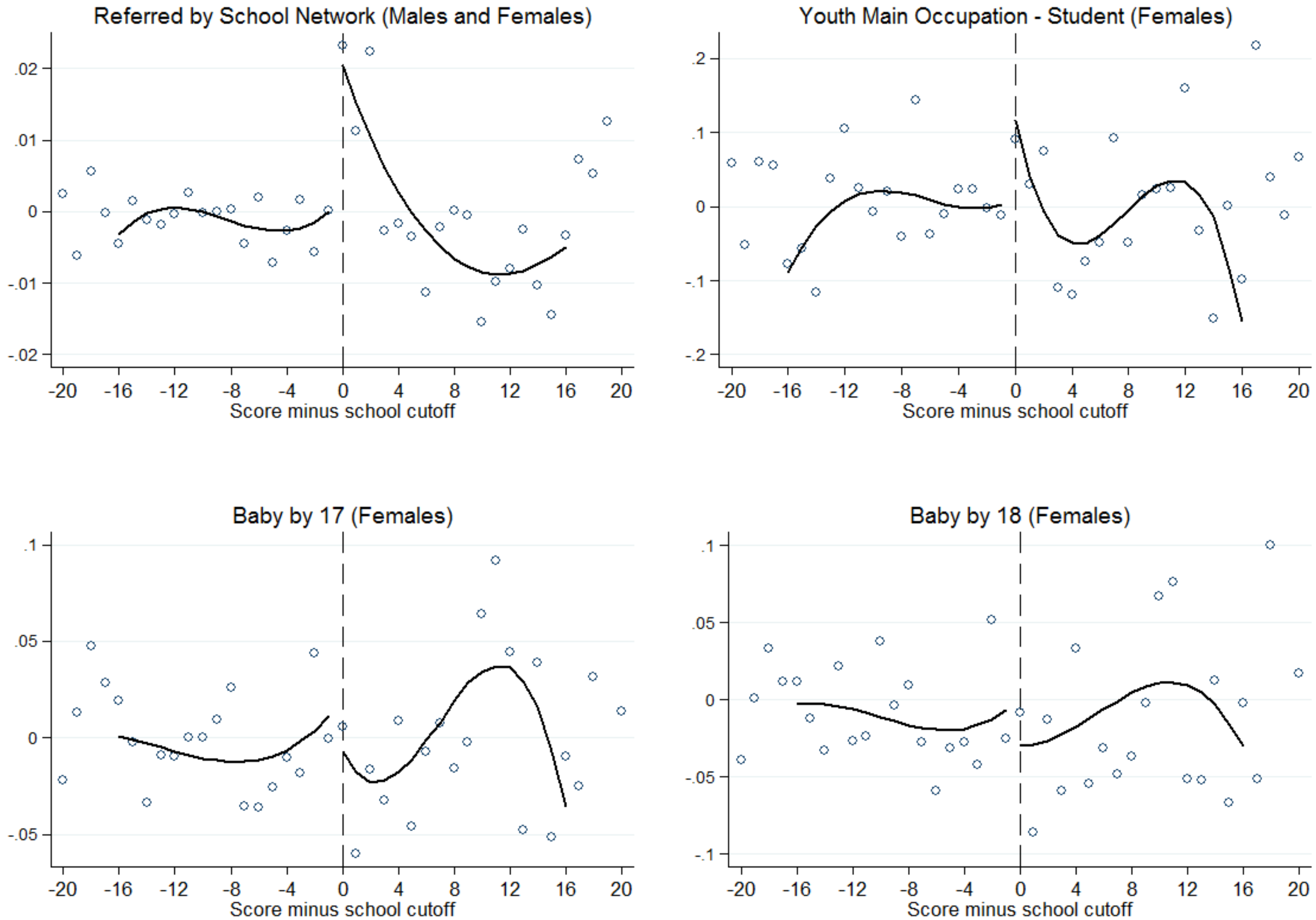
Notes: This figure depicts estimated 2SLS coefficients on 'Attend' a preferred school using 'Above' as the excluded instrument (resulting from equation system (5) - (6) in the text). The estimated 2SLS effects are reported for each bandwidth between +/-15 (+/-0.6sd) and +/-60 (+/-2.5sd). The 90 percent confidence interval for the estimate is presented in light gray, and the 95 percent confidence interval is in dark gray.

Appendix Figure A7. 2SLS Effects on Health Outcomes by Bandwidth (Males and Females Combined)



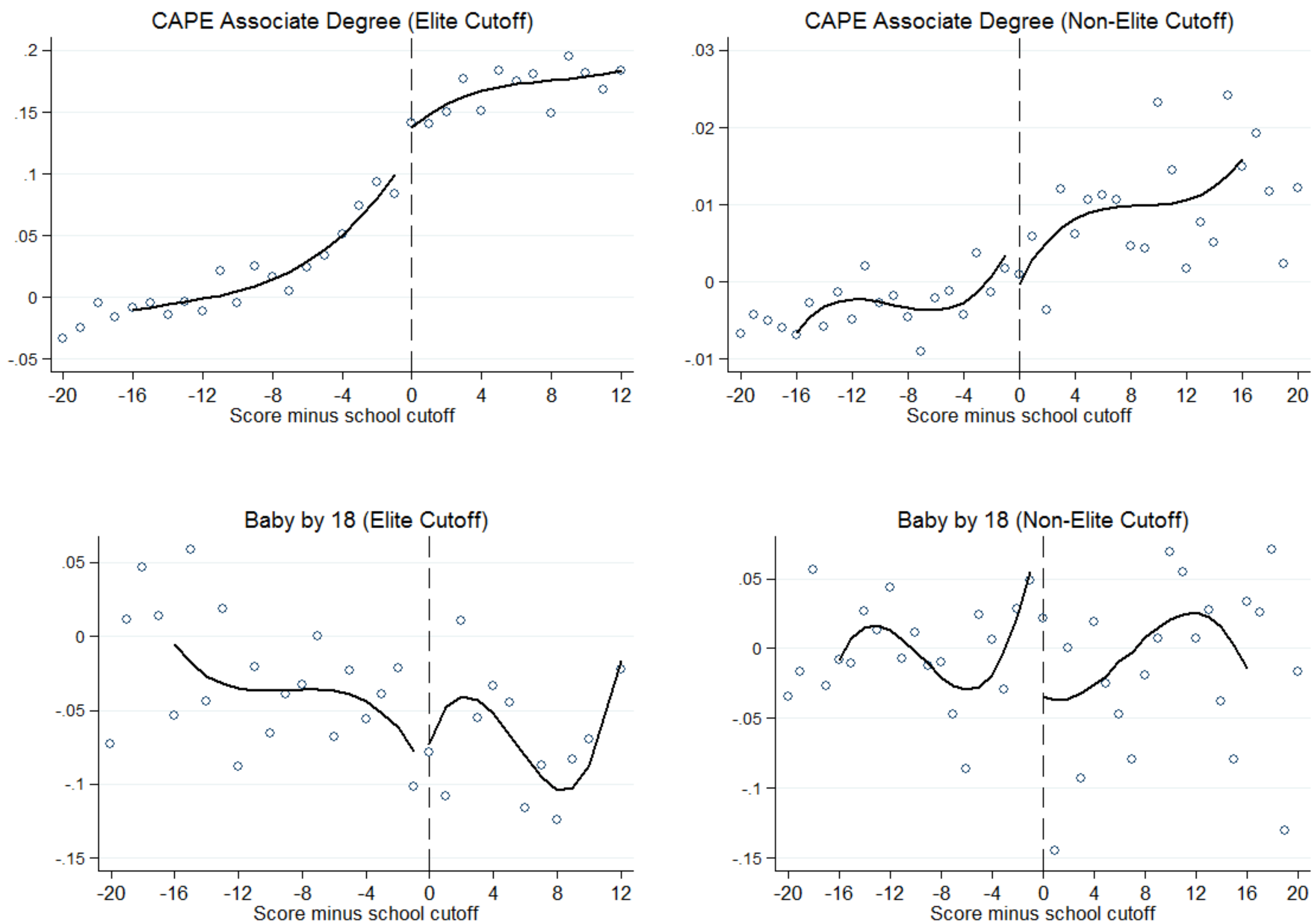
Notes: This figure depicts estimated 2SLS coefficients on 'Attend' a preferred school using 'Above' as the excluded instrument (resulting from equation system (5) - (6) in the text). The estimated 2SLS effects are reported for each bandwidth between +/-15 (+/-0.6sd) and +/-60 (+/-2.5sd). The 90 percent confidence interval for the estimate is presented in light gray, and the 95 percent confidence interval is in dark gray.

Appendix Figure A8. RD Effect on Mechanisms



Notes: The X-axis is the BSSEE score relative to the cutoff. The Y-axis is the mean outcome for each relative score (net of the mean for the cutoff). The circles represent 1-point bins of the relative score. The solid lines are the fitted outcomes generated by fitting a third degree polynomial of the relative score fully interacted with the 'Above' indicator.

Appendix Figure A9. RD Effect by Elite vs Non-Elite Cutoffs - Females



Notes: The X-axis is the BSSEE score relative to the cutoff. The Y-axis is the mean outcome for each relative score (net of the mean for the cutoff). The circles represent 1-point bins of the relative score. The solid lines are the fitted outcomes generated by fitting a third degree polynomial of the relative score fully interacted with the 'Above' indicator.

Appendix Table 1. 2SLS Effects on Educational Outcomes - Alternative Samples

	All		Women		Men		(4)=(6)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: CSEC Performance. Sample: BSSEE cohorts 1987 - 2002 (Full Population +/- 0.75 SD from cutoff)							
Took at least 1 subject	-0.020**	-0.019**	-0.010	-0.009	-0.030**	-0.029**	0.322
	(0.010)	(0.009)	(0.014)	(0.014)	(0.014)	(0.014)	
Number of subjects passed	-0.072	-0.069	0.003	0.004	-0.161**	-0.159**	0.109
	(0.054)	(0.054)	(0.078)	(0.078)	(0.070)	(0.070)	
Qualified for tertiary	-0.004	-0.004	0.003	0.002	-0.012	-0.011	0.454
	(0.009)	(0.009)	(0.014)	(0.014)	(0.012)	(0.012)	
Observations	184,596	184,596	94,353	94,353	90,243	90,243	
Panel B: CSEC Performance. Sample: BSSEE cohorts 1987 - 2002 (matched survey observations)							
Took at least 1 subject	0.039	0.041	0.124*	0.072	-0.048	0.010	0.351
	(0.047)	(0.041)	(0.066)	(0.054)	(0.063)	(0.050)	
Number of subjects passed	-0.049	-0.022	0.463	0.299	-0.481	-0.286	0.141
	(0.240)	(0.198)	(0.321)	(0.281)	(0.329)	(0.270)	
Qualified for tertiary	0.021	0.028	0.066	0.020	-0.020	0.036	0.842
	(0.044)	(0.034)	(0.065)	(0.053)	(0.062)	(0.050)	
Observations	5,610	5,610	2,616	2,616	2,994	2,994	
Panel C: CAPE Performance. Sample: BSSEE cohorts 1998 - 2002 (Full Population +/- 0.75 SD from cutoff)							
Took at least 1 unit	0.015	0.015	0.007	0.008	0.021	0.022	0.602
	(0.013)	(0.013)	(0.021)	(0.021)	(0.016)	(0.016)	
Number of units passed	0.130*	0.135*	0.072	0.067	0.178**	0.190**	0.416
	(0.076)	(0.077)	(0.126)	(0.127)	(0.088)	(0.088)	
Earned Associate Degree	0.014+	0.014+	0.017	0.016	0.010	0.011	0.743
	(0.009)	(0.009)	(0.015)	(0.015)	(0.010)	(0.010)	
Observations	45,874	45,874	23,299	23,299	22,575	22,575	
Sociodemographics	Yes	Yes	Yes	Yes	Yes	Yes	
BSSEE cubic spline	Yes	Yes	Yes	Yes	Yes	Yes	
Cutoff fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	
Preferences fixed effects	No	Yes	No	Yes	No	Yes	

Notes: This table reports estimated 2SLS coefficients on 'Attend' a preferred school using 'Above' as the excluded instrument (resulting from equation system (5) - (6) in the text). Estimated standard errors in parenthesis two-way clustered at the individual and BSSEE score levels. Sociodemographic controls include student gender and parish fixed-effects. Panel B shows estimated effects using only observations that belong to the matched survey data and regressions are weighted by the inverse of sampling probability to reflect survey design. Column (7) reports the p-value of a test for the equality of estimates reported in columns (4) and (6). + Significant at 15%; * Significant at 10%; ** significant at 5%; *** significant at 1%

Appendix Table 2. Reduced Form Estimates on Baseline Characteristics

Estimation Sample:	Full Population: +/- 0.75 SD from cutoff			Face to Face Survey		
	All	Women	Men	All	Women	Men
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Month of Birth						
January	0.008*	-0.004	0.022***	-0.016	-0.038*	-0.008
	(0.005)	(0.007)	(0.007)	(0.020)	(0.023)	(0.024)
February	-0.007	0.003	-0.016**	-0.032	-0.012	-0.027
	(0.004)	(0.006)	(0.006)	(0.026)	(0.022)	(0.024)
March	-0.001	0.003	-0.006	0.032	-0.014	0.043
	(0.004)	(0.006)	(0.006)	(0.021)	(0.021)	(0.029)
April	<0.001	0.005	-0.005	0.013	-0.019	0.037*
	(0.004)	(0.006)	(0.006)	(0.023)	(0.033)	(0.021)
May	-0.001	-0.003	0.001	0.010	0.031	-0.028
	(0.004)	(0.006)	(0.007)	(0.025)	(0.026)	(0.025)
June	0.004	<0.001	0.009	0.007	0.007	-0.020
	(0.004)	(0.006)	(0.006)	(0.022)	(0.026)	(0.022)
July	-0.003	-0.001	-0.005	0.011	0.002	0.020
	(0.004)	(0.006)	(0.006)	(0.022)	(0.024)	(0.020)
August	0.002	0.005	-0.002	-0.007	0.014	-0.007
	(0.005)	(0.006)	(0.007)	(0.017)	(0.021)	(0.023)
September	0.001	-0.003	0.006	0.014	-0.014	0.026
	(0.005)	(0.007)	(0.007)	(0.020)	(0.022)	(0.022)
October	-0.001	0.006	-0.009	-0.025	-0.004	-0.048**
	(0.005)	(0.007)	(0.007)	(0.021)	(0.025)	(0.024)
November	0.001	-0.009	0.011	-0.016	0.024	-0.007
	(0.005)	(0.007)	(0.007)	(0.020)	(0.023)	(0.025)
December	-0.003	-0.001	-0.005	0.008	0.023	0.019
	(0.005)	(0.007)	(0.007)	(0.021)	(0.022)	(0.030)
Panel B: Selectivity of Primary School and Secondary School Choices (BSSEE score of incoming class)						
Primary school	0.004	0.011	-0.006	0.007	0.005	0.015
	(0.006)	(0.008)	(0.009)	(0.022)	(0.029)	(0.028)
Choice 1	0.001	0.001	0.001	-0.002	0.001	0.003
	(0.001)	(0.001)	(0.001)	(0.004)	(0.004)	(0.005)
Choice 2	<0.001	0.001	-0.001	0.001	<0.001	-0.013**
	(0.001)	(0.001)	(0.001)	(0.004)	(0.004)	(0.005)
Choice 3	<0.001	-0.001	<0.001	0.007*	0.006*	0.004
	(0.001)	(0.001)	(0.002)	(0.004)	(0.004)	(0.005)
Choice 4	-0.002	-0.001	-0.002	0.002	0.003	-0.000
	(0.001)	(0.002)	(0.002)	(0.005)	(0.005)	(0.006)

Appendix Table 2 cont'd. Reduced Form Estimates on Baseline Characteristics

Choice 5	<0.001	0.001	-0.001	-0.008	-0.012*	-0.010*
	(0.001)	(0.002)	(0.002)	(0.006)	(0.006)	(0.005)
Choice 6	-0.002	-0.002	-0.001	0.001	0.002	-0.005
	(0.001)	(0.002)	(0.002)	(0.006)	(0.005)	(0.008)
Choice 7	-0.001	<0.001	-0.001	0.011*	0.017***	-0.002
	(0.002)	(0.002)	(0.002)	(0.006)	(0.006)	(0.006)
Choice 8	0.001	<0.001	0.002	<0.001	-0.002	0.004
	(0.002)	(0.002)	(0.002)	(0.006)	(0.007)	(0.006)
Choice 9	-0.002	-0.004	-0.001	0.012*	0.019**	0.011
	(0.002)	(0.003)	(0.003)	(0.007)	(0.007)	(0.008)
Panel C: Parish of Residency (before admission to secondary school)						
Parish 1	<0.001	<0.001	<0.001	-0.004	<0.001	-0.008
	(0.001)	(0.001)	(0.002)	(0.003)	(<0.001)	(0.006)
Parish 2	<0.001	<0.001	<0.001	-0.002	<0.001	-0.007
	(0.001)	(0.001)	(0.002)	(0.003)	(<0.001)	(0.005)
Parish 3	<0.001	<0.001	0.001	-0.002	<0.001	-0.008
	(0.001)	(0.001)	(0.002)	(0.003)	(<0.001)	(0.005)
Parish 4	<0.001	<0.001	-0.001	-0.003	<0.001	-0.003
	(0.001)	(0.001)	(0.002)	(0.003)	(<0.001)	(0.004)
Parish 5	<0.001	<0.001	0.001	-0.003	<0.001	-0.008
	(0.001)	(0.001)	(0.002)	(0.003)	(<0.001)	(0.005)
Parish 6	<0.001	<0.001	<0.001	-0.003	<0.001	-0.007
	(0.001)	(0.001)	(0.002)	(0.003)	(<0.001)	(0.005)
Parish 7	-0.001	<0.001	-0.001	-0.003	<0.001	-0.008
	(0.001)	(0.001)	(0.002)	(0.003)	(<0.001)	(0.005)
Parish 8	<0.001	<0.001	<0.001	-0.004	<0.001	-0.010
	(0.001)	(0.001)	(0.002)	(0.004)	(<0.001)	(0.007)
Parish 9	<0.001	<0.001	0.001	-0.003	<0.001	-0.009
	(0.001)	(0.001)	(0.002)	(0.003)	(<0.001)	(0.006)
Parish 10	-0.001	<0.001	-0.001	-0.002	<0.001	-0.006
	(0.001)	(0.001)	(0.002)	(0.002)	(<0.001)	(0.005)
Parish 11	<0.001	<0.001	<0.001	-0.003	<0.001	-0.009
	(0.001)	(0.001)	(0.002)	(0.003)	(<0.001)	(0.006)
BSSEE cubic spline	Yes	Yes	Yes	Yes	Yes	Yes
Cutoff fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	184,595	94,352	90,243	5,551	2,575	2,972

Notes: This table reports estimated coefficients on the 'Above' indicator resulting from reduced form models as in equation (5) of the text. Estimated standard errors in parenthesis two-way clustered at the individual and BSSEE score levels. Sample corresponds to BSSEE cohorts 1987 - 2002 (25 - 40 years old when surveyed). Regressions in columns 4-6 are weighted by the inverse of sampling probability to reflect survey design. + Significant at 15%; * Significant at 10%; ** significant at 5%; *** significant at 1%

Appendix Table 3. Reduced Form Estimates on Predicted Outcomes

	All	Women	Men	(2)=(3)
	(1)	(2)	(3)	(4)
Panel A: Predicted CSEC Performance. Sample: BSSEE cohorts 1998 - 2009 (Full Population +/- 0.75 SD from cutoff)				
	(1)	(2)	(3)	(4)
Predicted: Took at least 1 subject	<0.001	-0.001	-0.001	0.982
	(0.002)	(0.003)	(0.004)	
Predicted: Number of subjects passed	-0.013	-0.009	-0.010	0.963
	(0.013)	(0.020)	(0.016)	
Predicted: Qualified for tertiary	-0.001	-0.001	-0.001	0.849
	(0.002)	(0.003)	(0.002)	
Observations	106,669	54,631	52,039	
Panel B: Predicted CAPE Performance. Sample: BSSEE cohorts 1998 - 2009 (Full Population +/- 0.75 SD from cutoff)				
Predicted: Took at least 1 unit	-0.001	<0.001	-0.001	0.847
	(0.001)	(0.002)	(0.001)	
Predicted: Number of units passed	-0.005	-0.004	-0.004	0.981
	(0.006)	(0.009)	(0.008)	
Predicted: Earned Associate Degree	<0.001	<0.001	<0.001	0.943
	(0.001)	(0.001)	(0.001)	
Observations	106,669	54,631	52,039	
Panel C: Predicted Education and Earnings. Sample: BSSEE cohorts 1987 - 2002 (25 - 40 Years old at Survey)				
Predicted: Years of education	0.117	-0.050	0.223	0.313
	(0.140)	(0.189)	(0.192)	
Predicted: Log monthly wage	0.021	0.034	0.008	0.792
	(0.049)	(0.087)	(0.049)	
Observations	5,277	2,510	2,767	
BSSEE cubic spline	Yes	Yes	Yes	
Cutoff fixed effects	Yes	Yes	Yes	

Notes: This table reports estimated coefficients on the 'Above' indicator resulting from reduced form models as in equation (5) of the text. Estimated standard errors in parenthesis two-way clustered at the individual and BSSEE score levels. Samples in Panels A and B correspond to BSSEE cohorts that have both CSEC and CAPE data available (BSSEE cohorts 1998 - 2009). This because the earliest CAPE outcome data corresponds to year 2005 which is associated with the 1998 BSSEE cohort; while the latest CAPE data corresponds to year 2016 which is associated with the 2009 BSSEE cohort. Panel C shows estimated effects on predicted educational attainment and wages obtained from the matched survey data covering BSSEE cohorts 1987-2002 (25-40 years old when surveyed). Column (4) reports the p-value of a test for the equality of estimates reported in columns (2) and (3). Regressions do not control for preferences as the selectivity of preferences were used when predicting the outcomes. Regressions are weighted by the inverse of sampling probability to reflect survey design. + Significant at 15%; * Significant at 10%; ** significant at 5%; *** significant at 1%

Appendix Table 4. 2SLS Effects on Fertility

Sample:	Women	
	(1)	(2)
Panel A: Sample: BSSEE cohorts 1987 - 2002 (25 - 40 years old when surveyed)		
Baby by 25	0.023	0.018
	(0.077)	(0.056)
At least 1 baby ever	0.049	-0.001
	(0.068)	(0.051)
Total fertility	0.032	-0.114
	(0.174)	(0.166)
Observations	2,271	2,271
Panel B: Sample: BSSEE cohorts 1987 - 1992 (35 - 40 years old when surveyed)		
At least 1 baby ever	0.133	0.084
	(0.108)	(0.096)
Total fertility	0.504	0.328
	(0.353)	(0.237)
Observations	900	900
Panel C: Sample: BSSEE cohorts 1987 - 1991 (36 - 40 years old when surveyed)		
At least 1 baby ever	0.108	0.005
	(0.112)	(0.091)
Total fertility	0.455	0.393
	(0.369)	(0.278)
Observations	724	724
Sociodemographics	Yes	Yes
BSSEE cubic spline	Yes	Yes
Cutoff fixed effects	Yes	Yes
Preferences fixed effects	No	Yes

Notes: This table reports estimated 2SLS coefficients on 'Attend' a preferred school using 'Above' as the excluded instrument (resulting from equation system (5) - (6) in the text). Estimated standard errors in parenthesis two-way clustered at the individual and BSSEE score levels. Sociodemographic controls include student gender and parish fixed-effects. Regressions are weighted by the inverse of sampling probability to reflect survey design. + Significant at 15%; * Significant at 10%; ** significant at 5%; *** significant at 1%