Quality-Quantity Tradeoffs in Pricing Public Secondary Education[*]

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Abstract

Most governments provide some public education and must choose how to finance it from fiscal transfers and user fees. I study how a nationwide shift from partial user fees to pure fiscal transfers changed secondary school education outcomes in South Africa. Fee elimination increased enrollment in early grades of secondary school, leading to larger classes and more part-time and temporary teachers. Potentially due to this shift, fee elimination decreased both grade 12 enrollment and graduation exam passes. This demonstrates that demand-side subsidies without supply-side investments may have limited or even negative effects on education participation and attainment.

1 Introduction

Almost all governments around the world provide some form of public education and must choose how to finance it from fiscal transfers and user fees. Many countries have shifted this funding mix through time: fees, government loans, and tax incentives for tertiary education have changed in many developed and developing countries, while user fees for primary and secondary education have changed substantially in many developing countries. Different financing systems may lead to different levels of enrollment in public education, learning, and hence labor force composition. The optimal mix of fees and transfers is theoretically ambiguous. Fees may promote accountability of public education providers to users, and may screen out users with low returns to enrollment. However, fees may reduce enrollment for credit-constrained, impatient, or risk-averse students with high returns to enrollment. Fiscal transfers may have distortionary effects on consumption, investment, and labor supply through the tax system. Given this theoretical ambiguity, empirical evidence on the effects of different education finance systems is particularly important.

I study the effects of a nationwide shift in public education financing in South Africa. Primary and secondary public schools were historically funded by a mix of mandatory school fees and fiscal transfers.[1] Between 2007 and 2012, fees were eliminated in 75% of public primary and secondary schools. Fee elimination started in schools serving high-poverty neighborhoods and expanded to schools serving progressively less poor neighborhoods. I compare trends in enrollment and graduation exam performance across schools that eliminate fees at different points in time using both administrative and household survey data. I focus on secondary school enrollment and graduation, as primary school enrollment was close to universal before fee elimination.

Fee elimination immediately increased enrollment by 5-9% in the early grades of secondary school but reduced grade 12 enrollment and graduation exam passes by 4-5%. Over time, the effect of fee elimination of grades 10 and 11 steadily rose while the effects on grade 12 enrollment and exam passes remained negative. I explain these results with a simple dynamic framework, similar in spirit to Stange (2012). Agents make forward-looking enrollment decisions based on the cost of enrollment, probability of passing, and return to passing in the current grade, as well as the option value of enrollment in future grades. Fee elimination increases early grade enrollment in this

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[1]Fees for primary and secondary public schools exist in many developing countries, particularly in Africa. Primary and some secondary fees were eliminated or substantially reduced between the early 1990s and present time in at least Ethiopia, Ghana, Kenya, Lesotho, Malawi, Mozambique, Nigeria, South Africa, eSwatini, Tanzania, and Uganda.

framework by cutting the cost of enrolling in current and future grades. Fee elimination decreases grade 12 enrollment and exam passes because the dynamic cost reduction is smaller and school quality falls. Steadily rising grade 10 and 11 enrollment is driven by grade repetition as agents face low probabilities of passing those grades but potentially large returns to passing. Consistent with this framework, pre-elimination grade progession in treated schools is low and uncertain. Consistent with falling quality, fee elimination increased student:teacher ratios and reliance on part-time and temporary teachers. Evidence on other measures of school quality such as financial resources and peer group composition is inconclusive, partly due to data limitations.

These findings relate to three literatures. First, I contribute to research on education access in developing countries, which is a major area of policy interest. Both the Millenium and Sustainable Development Goals feature targets for school participation, though critics have argued these targets emphasize participation over learning (Pritchett, 2013). Policymakers have explored user fee reductions, conditional cash transfers, and school construction programs to increase education participation. A recent review, focusing on randomized evaluations, concludes that "reducing the out-of-pocket cost of education or instituting subsidies consistently increases school participation, often dramatically" (Kremer and Holla, 2009). Within this broader literature, my work is most closely related to studies of changes in public school user fees in developing countries.[2,3] This literature generally finds that primary enrollment is relatively sensitive to fee elimination.

I find modest effects of fee elimination on total secondary school enrollment, as negative effects in higher grades partly offset moderate positive effects in lower grades. This difference may reflect lower grade progression probabilities and higher psychic and labor market opportunity costs in secondary education. I find modest effects in a context with a high and convex earnings-education relationship (Lam, Ardington, Branson, Goostrey, and Leibbrandt, 2010). Intepreting this association naively, we might predict that eliminating fees would have large effects on enrollment and large labor market returns. My results show that very high Mincer 'returns' to grade *attainment*

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[2]Examples include Barrera-Osorio, Linden, and Urquiola (2007), Blimpo, Gajigo, and Pugatch (2019), Borkum (2012), Branson and Lam (2017), Brudevold-Newman (2016), Deininger (2003), Fafchamps and Minten (2007), and Lucas and Mbiti (2012). Borkum (2012) and Branson and Lam (2017) also study the South African fee elimination policy, using respectively administrative data on the beginning of the policy and household survey data on the later stages of the policy. Relative to these papers, I also study effects on high school graduation exam performance, use a wider geographic sample and longer time-series coverage, integrate additional administrative and household survey data sources, and propose an economic model of human capital investment to interpret the treatment effects.

[3]This also relates to work on pricing government health services, reviewed in Dupas (2014) and Dupas and Miguel (2017).

are consistent with low returns to *enrollment* for many prospective students and hence substantial non-enrollment even after large cost reductions. This contrasts with prior findings that labor market returns to education for students induced to enroll by cost reductions are not lower than Mincer returns (Card, 2001).[4] It is possible, though my data cannot definitely show this, that I find a different result because many prior studies use variation in individual-level cost constraints while I study a cost shift affecting only relatively poor, low-quality schools.

My findings add to a small literature studying the achievement and attainment effects of large-scale education expansions. Learning outcomes in many developing country school systems are very low, raising concerns about the academic and economic value of expanding access and the risk that crowding may worsen existing outcomes (Kremer, Brannen, and Glennerster, 2013; Pritchett, 2013). Prior work on fee elimination policies has found limited effects on performance on graduation examinations for primary school (Lucas and Mbiti, 2009; Valente, 2018) and secondary school (Brudevold-Newman, 2016). I find that fee elimination reduced graduation exam pass counts, consistent with dropout by students with positive counterfactual probabilities of passing. These results illustrate a potential cost of fee elimination policies, perhaps most relevant in settings where grade progression and graduation are uncertain.

This paper also relates to research on education-conditional cash transfers (Baird, McIntosh, and Ozler, 2011; Filmer and Schady, 2008; Fizbein and Schady, 2009; Schultz, 2004). Fee elimination is a price ceiling, potentially combined with supply-side subsidies, while cash transfers are demand-side subsidies. This means the effects of cash transfers and fee elimination may differ substantially, particularly if fee elimination induces quality shifts.[5] For example, when schools are allowed to charge user fees, demand-side subsidies may be partly appropriated by schools through higher fees (Turner, 2017). Like some studies of conditional cash transfers, I find that there are at least eleven inframarginal enrollers per marginal enroller, making this an expensive method to raise enrollment (Todd and Wolpin, 2006).

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[4]Card's review focused mainly on studies from developed countries. But Duflo (2001) and Duflo, Dupas, and Kremer (2017) find the same pattern in developing country settings. However, see Carneiro, Heckman, and Vytlacil (2011) and Heckman, Lochner, and Todd (2006) for an argument that comparing earnings across education levels instrumented by education cost shifters may be uninformative about economic returns to education.

[5]The same argument applies to subsidies for non-fee costs of education such as uniforms and transport. These do not affect schools' budgets but effectively provide a cash transfer to households conditional on children's enrollment in school. Most but not all studies of such subsidies find positive effects that may persist after the subsidies end (Evans, Kremer, and Ngatia, 2009; Hidalgo, Onofa, Oosterbeek, and Ponce, 2013) .

Second, my work relates to research on pricing and targeting public services. Most governments provide some public education and health services, which are not public goods and can be funded by either user fees or fiscal transfers. There are theoretical arguments for both funding mechanisms (Poterba, 1996). Some governments use differentiated fees based on users' socioeconomic status (e.g. means-tested financial aid for tertiary education). User-level price differentiation requires user-level data that may be costly to collect and verify and can impose high marginal tax rates that distort economic decisions (Brown, Ravallion, and Van de Walle, 2018) . I study a policy of school-level price differentiation rather than individual-level price differentiation or universal zero prices. I find this targeting was effective, in that fee elimination did not induce large student transfers from fee-charging to fee-eliminating schools.[6] However, this may not generalize to settings where there is less income segregation by neighborhood or a weaker correlation between school quality and prices.[7] This can be viewed as an example of pricing access to public services using self-targeting (Alatas, Banerjee, Hanna, Olken, Purnamasari, and Wai-Poi, 2016; Ravallion, 1991).

Third, I contribute to a growing literature analyzing education policy reforms at scale. South Africa eliminated fees in more than 20,000 schools serving 8.5 million students. This allows me to study mechanisms that may be absent or attenuated in smaller policy changes over shorter horizons (Muralidharan and Niehaus, 2017). I use a decade of data to identify dynamic enrollment effects and show that enrollment accumulates over time in later grades without translating into successful graduations. This complements studies showing that education policy effects may change over time in domains such as teacher contract structures (Bau and Das, 2017). Large-scale fee elimination may shift equilibrium sorting patterns between schools, particularly because South Africa allows partial school choice. However, I find at most small transfers between fee-eliminating and fee-charging schools in both administrative and survey data. This contrasts with prior work finding changes in income-based sorting after large-scale school funding reforms (Hsieh and Urquiola, 2006; Lucas and Mbiti, 2012).

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[6]South Africa allowed partial school choice during this period, so transfers were feasible without moving neighborhoods. Branson, Lam, and Zuze (2012) show that targeting was effective in another sense: students enrolled in the schools targeted for early fee elimination came from poorer households.

[7]There is a large difference in academic outcomes between schools in high and low-SES neighborhoods in South Africa and substantial income segregation by neighborhood. Students are allowed to attend schools outside their neighborhoods but this may impose large transport costs. In this setting, transfers from fee-charging to fee-eliminating schools are costly. However, I find no evidence of transfers even between early-eliminating schools in poor neighborhoods and late-eliminating schools in less poor neighborhoods.

I show that the teacher labor market may have mediated fee elimination effects by increasing student:teacher ratios and reliance on part-time and temporary teachers. This mechanism is less likely to operate in smaller policy changes that do not shift aggregate teacher demand. This echoes findings that scaling interventions may be constrained by teacher supply or union responses (Bold, Kimenyi, Mwabu, Ng'ang'a, and Sandefur, 2018; Jepsen and Rivkin, 2009). I also document some lags in fee elimination. This is consistent with challenges developing country governments face in implementing large-scale education reforms (Banerjee, Banerji, Berry, Duflo, Kannan, Mukherji, Shotland, and Walton, 2016). Finally, I study the universe of public schools, rather than a self- or government-selected pilot sample whose responses to fee elimination may not generalize even if elimination were randomly assigned (Allcott, 2015).

In Section 2 of the paper, I describe the South African education context. In Section 3, I describe the fee elimination policy and my research design. In Section 4, I report treatment effects of fee elimination: higher enrollment early in secondary school but lower grade 12 enrollment and graduation exam performance. In Section 5, I propose a dynamic economic framework that explains the treatment effects in terms of larger effective cost reductions for students in early grades and negative quality effects that reduce the benefit of enrollment in later grades. I confirm that fee elimination lowered some proxies for school quality. I also assess alternative explanations based on capacity constraints, ceiling effects, credit constraints, and behavioral factors. In Section 6, I show that there were at most small spillover effects of fee elimination on neighboring schools.

I report extensions and robustness checks in the appendices. In Appendix A, I describe the data sources and show that my results are robust to accounting for measurement error in the outcome and treatment data. In Appendix B, I convert effect sizes on enrollment into price elasticities, subject to some data-based caveats. In Appendices C and D, I show that my results are generally robust to different research designs and econometric strategies. In Appendix E, I replicate the main results of the paper using household survey data, subject to substantial data-based caveats.

2 Context

The South African school system consists of twelve grades. Most schools are either 'primary schools' offering grades 1-7 or 'secondary schools' offering grades 8-12. Students are expected to enter grade 1 in the calendar year that they turn 7 so a student completing one grade per

year would finish secondary school at age 18. Enrollment in formal education before grade 1 is low outside high-income households.[8] The only standardized assessments are secondary school graduation examinations at the end of grade 12. These are content-based exams in at least six subjects that build on the grade 10-12 curriculum and are set, graded, and moderated by the national education department. During the period I study South African public schools used a partial school choice system: students could enroll in any school but schools were allowed to prefer students living nearby for admission. Outside high-income households, 33% of students attended the closest school and 73% attended a school within 2km of the closest school (Branson, Lam, and Zuze, 2012). The private sector was historically small and expensive. The share of students enrolled in private schools rose from 2.8 to 3.2 percent between 2006 and 2012, but this measure misses some students in unregistered private schools (Department of Basic Education, 2006, 2012).

South Africa's public education system was racially segregated until the early 1990s, and per capita government expenditure on white schools was orders of magnitude larger than on black schools. Curricula at black schools were deliberately focused on non-academic subjects, reflecting the *apartheid* government's insistence on preparing black students for manual employment only. Few black students completed secondary schooling, pass rates on secondary school graduation examinations were low, and even fewer students took mathematics or physical science as secondary school subjects (Fedderke, Luiz, and de Kadt, 2000). State expenditure on black education rose substantially in the 1970s, 1980s, and 1990s and this was associated with rapidly rising enrollment rates (Seekings and Nattrass, 2005). However, the quality of education remained low in historically black schools. The education system was officially desegregated in the early 1990s but historically black schools still enrolled mostly black students in the period I study.

In the period 2003-2006, just before fee elimination, enrollment was fairly high but grade attainment was low. 92% of people aged 6-18 attended primary or secondary school (Figure 1A).[9] However, only 30 percent of people aged 18-24 in 2003-2006 had completed secondary school. I define the 17% of people aged 6-24 who are not attending school and have not graduated from secondary school as non-participants. This is the population whose education outcomes might be

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[8]Some schools offer a year of formal education before grade 1, called grade R. Data on grade R enrollment is limited (Van der Berg, Girdwood, Sheperd, Van Wyk, Kruger, Viljoen, Ezeobi, and Ntaka, 2013).

[9]All statistics are calculated from the annual, nationally representative General Household Survey for 2003-2006. Calculations use individual-level post-stratification weights provided by Statistics South Africa. All differences discussed in the text are statistically significant at the 1 percent level, using heteroskedasticity-robust standard errors.

Figure 1: Attendance and Attainment before Fee Elimination

Panel A: Attendance and Attainment by Age

Panel B: Attendance by Grade-Eligibility
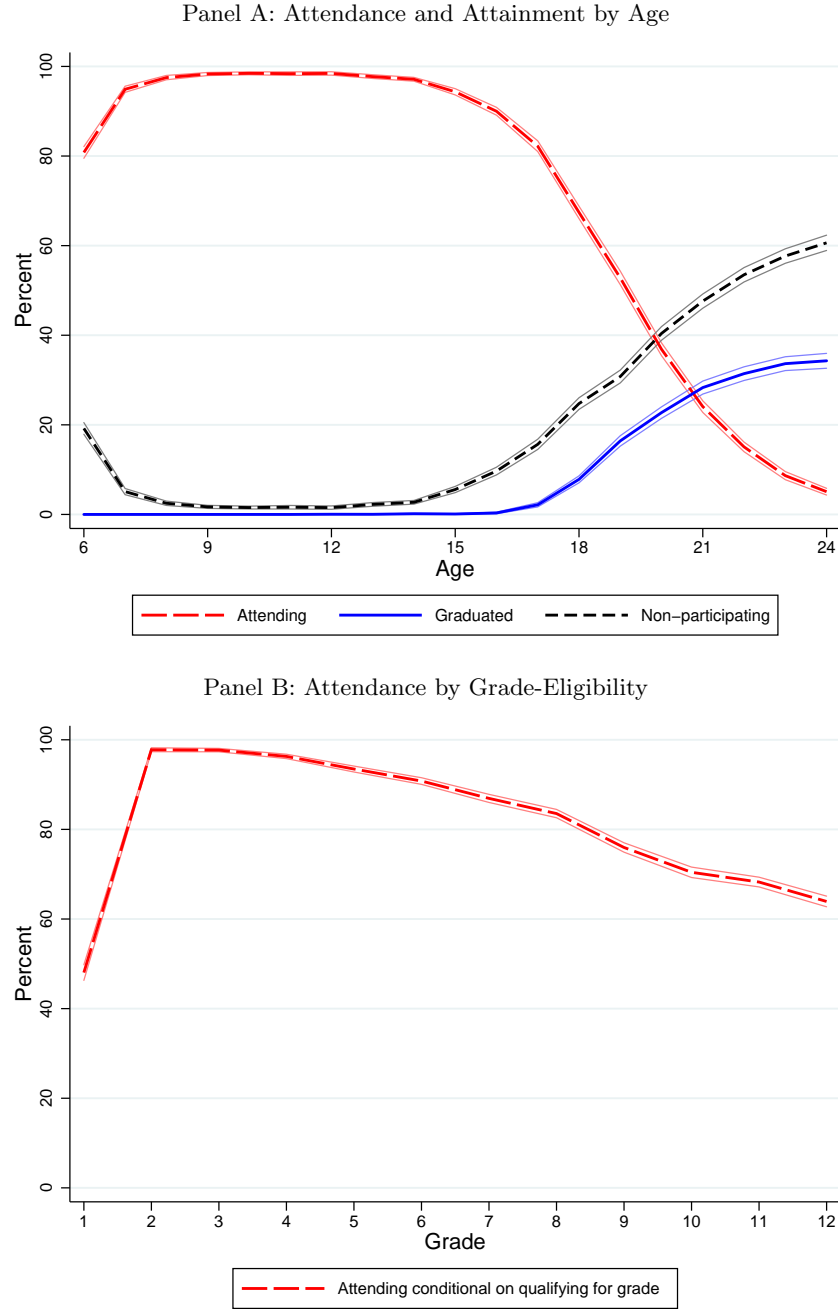
Notes: The first panel shows the shares of people who are currently attending primary/secondary education (red dashed line) and have completed secondary education (blue solid line). The remaining people (black dashed line) are still eligible to attend primary/secondary school but are not doing so. This 18% of the population provides an upper bound for the effect of fee elimination on school attendance and completion. The second panel shows the share of people eligible to attend each grade who are attending that grade. Conditional on eligibility, attendance in primary school (grades 1-7) is 90 percent and attendance in secondary school (grades 8-12) is 72 percent. 95% confidence intervals are based on heteroskedasticity-robust standard errors. All calculations use the General Household Surveys for 2003-2006, the four years preceding the fee elimination policy. Individuals are included in the sample if they are aged 6-24 and live in households with monthly total expenditure of less than USD520, approximately the 80th percentile of the expenditure distribution. All calculations use individual-level post-stratification weights constructed by Statistics South Africa, normalized to sum to one in each survey year.

affected by the fee elimination policy.[10] Non-participation was high at age 6 as some six year-olds were not yet eligible to enrollment, very low for ages 7-14, and then rose rapidly to 22% at age 18 and 43% at age 21. Non-participation at older ages was largely explained by drop-out during secondary school, with over 90% enrollment by grade-eligible students in primary school dropping to 64% by grade 12 (Figure 1B).

The combination of high enrollment and low attainment was driven partly by high grade repetition. Both grade repetition and dropout were rare in primary schools but rose monotonically by grade (Branson, Hofmeyr, and Lam, 2013). Anecdotally, schools barred progression mainly in grades 10 and 11 to stop academically marginal students progressing to grade 12 and failing graduation exams. The graduation exam results were high profile and schools worried that low pass rates led to criticism. Consistent with these anecdotes, the mean ratio of grade 12 passes to enrollment was 0.59 and of grade 12 passes to grade 10 enrollment two years before in the same school was 0.28. The low grade attainment rates and low pass rates are consistent with very poor performance by South African students on international literacy and numeracy assessments, even relative to poorer countries (Reddy, Visser, Winnar, Arends, Juan, Prinsloo, and Isdale, 2016; Van der Berg and Louw, 2007).

Educational attainment before and after fee elimination was highly valued in the labor market. The associations between years of completed education and both earnings and employment were both positive. The earnings-education relationship was convex: employed workers with grade 10 versus grade 9 earned 12% more, with differences of 16% for grade 11 versus grade 10 and 29% for grade 12 versus grade 11 (Lam, Ardington, Branson, Goostrey, and Leibbrandt, 2010). Completing post-secondary education was associated with even higher returns. These patterns were robust to conditioning on demographics and proxies for school quality and focusing only on younger cohorts likely to be educated closer to the fee elimination period (Branson, Ardington, Lam, and Leibbrandt, 2013; Branson and Leibbrandt, 2013). There are no systematic data on perceived rates of return to education. But an unpublished pilot study found that grade 11 and 12 students' perceived

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[10]People who start school at the target age and repeat no grades will graduate at age 18. However, national education policy at the time allowed students to enroll two years late and repeat grades up to four times, making 24 the nominal maximum age for secondary school enrollment (Burger, Van der Berg, and Von Fintel, 2012). Late school starts, grade repetition, and temporary drop-out followed by re-enrollment are common in South Africa, leading to a secondary school enrollment rate of 12% for people in their early 20s. The non-participation rate for ages 6-21 is still 12%.

Figure 2: Attendance, Attainment, and Expenditure by Household Expenditure before Fee Elimination

Panel A: Attendance and Attainment by Total Household Expenditure

Panel B: Share of Total Household Expenditure Spent on Education
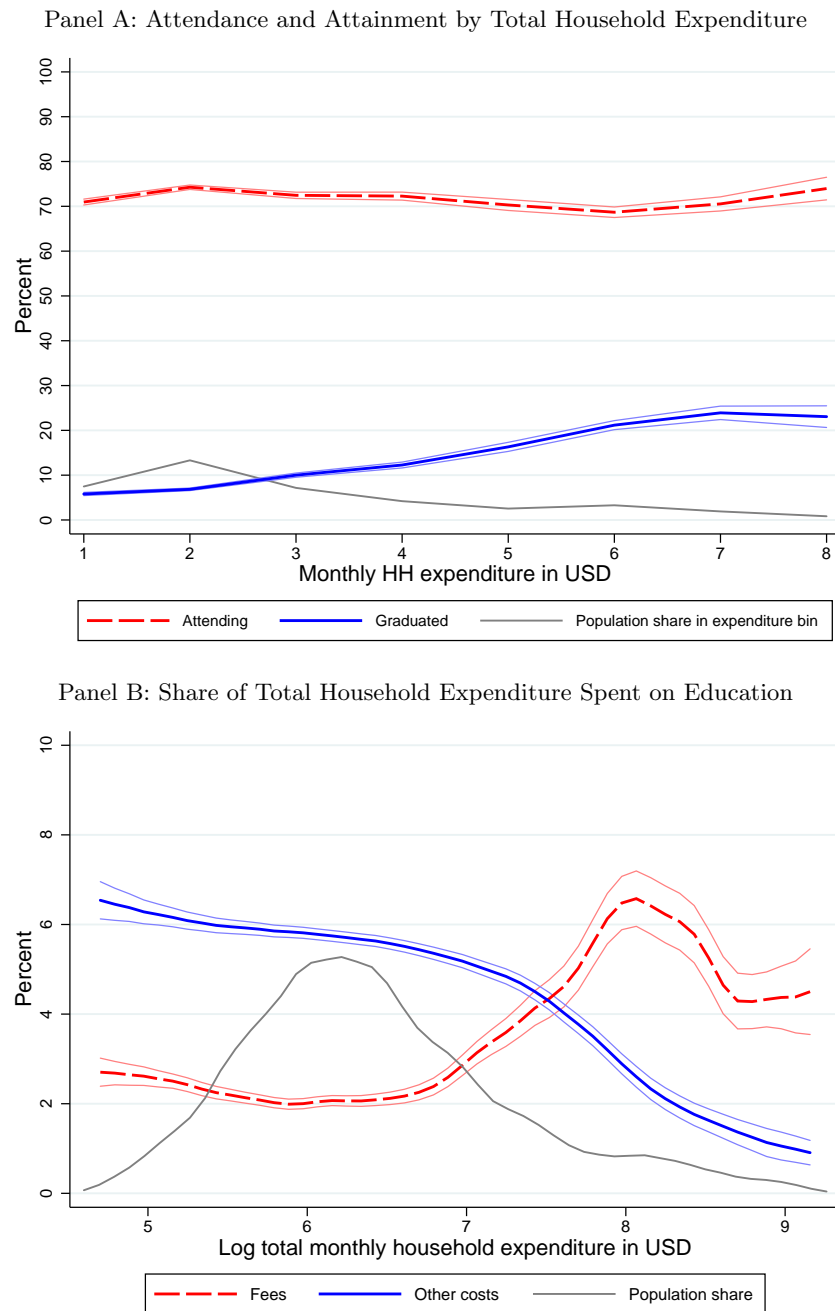
Notes: The first panel shows the share of people who are currently attending primary/secondary education (red dashed line) and have completed secondary education (blue solid line) by total monthly household expenditure. All calculations in the first panel use the General Household Surveys for 2003-2006, the four years preceding the fee elimination policy. Expenditure is only reported in bins, which are not adjusted for inflation, and income is not reported. All calculations use individual-level post-stratification weights constructed by Statistics South Africa, normalized to sum to one in each survey year. The second panel shows the share of total annual household expenditure spent on fees (red dashed line) and other education costs (blue solid line) in the 2005/6 Income and Expenditure Survey. All calculations exclude households with no members aged 6-24 who are eligible to enroll in primary or secondary school, exclude households with zero spending on any education category, and use post-stratification weights supplied by Statistics South Africa multiplied by the number of household members aged 6-24 who have not completed secondary school. These restrictions and weights are designed to approximate the school-attending population but will incorrectly include age-eligible non-attenders in households where another member is attending school. Expenditure converted from South African Rands to 2005 USD values using 2005 purchasing power parity-adjusted exchange rates from the World Development Indicators. 95% confidence intervals are based on heteroskedasticity-robust standard errors.

returns to education were similar to the estimates from the generalized Mincer models reported here (Branson and Hofmeyr, 2014). These patterns are consistent with young people attaching high values to grade attainment in secondary school but low probabilities of attaining conditional on enrolling.

Youth employment rates were very low, suggesting a low pecuniary opportunity cost of enrollment. In 2006, 15% of people aged 15-24 were employed, 15% were unemployed, and another 11% reported that they wanted to work but had given up searching (Statistics South Africa, 2006).

The population averages conceal important heterogeneity across race and total household expenditure (Figure 2A). I use expenditure as a proxy for socioeconomic status, as it is available in more datasets than income or wealth. Gender differences in primary and secondary education outcomes in this period were small. Attendance was high across most population groups but black and poor people had higher grade repetition, higher drop-out and hence lower attainment than white and less poor people. Secondary school graduation was 45 percentage points lower for black than white youths and 23 percentage points lower for people living in households with below-median total expenditure. Attendance varied less by race and expenditure: school attendance by 5-18 year olds was 1.7 percentage points lower for black than white youths and 2.5 percentage points lower for those living in households with below-median expenditure. Non-participation falls from 23 percent in the lowest expenditure bin to 3 percent in the highest bin. Non-participation is also 10 percent higher for black than white youths. The race and expenditure gradients are steeper for attainment than enrollment partly because grade repetition is higher in schools serving black students from poorer households. Grade repetition in these schools is also less strongly linked to independent measures of academic performance (Lam, Ardington, and Leibbrandt, 2010; Van der Berg and Shepherd, 2010).

The strong negative relationship between education participation and household expenditure suggests that tuition fees may have posed a barrier to secondary school attendance and graduation. All public schools in South Africa charged tuition fees to fund discretionary spending until 2007 (Pampallis, 2008). Provincial education departments paid for teacher salaries, infrastructure costs, and a per-student transfer for discretionary spending that was larger for schools in lower-SES neighborhoods. Fees and other education costs accounted for respectively 3 and 5.1% of total expenditure in households with members enrolled in school in 2005/6 (Figure 2B). In households

likely to be attending schools affected by the fee elimination policy, these shares are respectively 2.4 and 5.7%. This is large relative to education spending patterns of poor households reviewed by Banerjee and Duflo (2007) but probably underestimates the per-student cost of enrollment.[11] Youths in households that are eligible for means-tested cash transfers have higher school attendance rates, consistent with the existence of binding financing constraints (Eyal and Woolard, 2013). Fees increase by grade, so financing constraints may be more relevant in the later grades of secondary school (Table A.4 Panel A).

However, the negative relationship between education participation and household expenditure is also consistent with other explanations. Participation by people from low-SES households may be low because they face low probabilities of grade progression and/or low economic returns to grade attainment. If so, eliminating tuition fees will have a limited positive effect on attendance and graduation. Eliminating fees can even reduce attendance or graduation by reducing schools' financial resources or creating negative peer effects.

3 Fee Elimination Policy and Research Design

Fee elimination was announced in August 2006 and implemented from January 2007 onward, requiring selected schools to eliminate all mandatory fees (tuition, registration, etc.). The Department of Education committed to provide fee-eliminating schools with larger per-student transfers to offset the loss of fee revenue. Data appear on the relationship between pre-elimination fees and subsequent transfers are limited. I return briefly to this issue in Section 5.

Fee-eliminating schools were selected in a three-stage interaction between provincial and national governments, determined by the national Department of Education. First, provincial governments assigned each school in their province a "poverty score" based on characteristics of its neighborhood.[12] The scores ranked all schools within the province from least to most poor, with

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[11]These household-level expenditure shares on education probably underestimate the per-student cost of enrollment, due to data limitations. I would ideally observe education expenditure per household per member. However, I only observe household-level education expenditure, from the nationally representative Income Expenditure Survey from 2005/6. This survey measures household-level expenditure on tuition fees, other fees paid to the school (which are negligible), transport to school, school uniforms, and books. The survey does not measure attendance or enrollment. I approximate the population of enrolled youths by (1) restricting the sample to households that report positive expenditure on at least one education category and have at least one member aged 6-24 who has not graduated from secondary school and (2) multiplying the household-level post-stratification weights by the number of household members aged 6-24 who have not graduated from secondary school. This overweights households containing both age-eligible attenders and non-attenders relative to households containing only age-eligible attenders. As education spending on non-attenders is zero, this overweights households with low education spending.

[12]For the purposes of this policy, "neighborhoods" were defined as municipal electoral wards. These are not

ties permitted. Provinces were given neighborhood-level data on income, employment, education, health, and amenities from the 2001 census as a starting point for the assignment of poverty scores. Provinces could choose their own weighting of these five variables and make ad hoc adjustments to the scores to reflect within-neighborhood heterogeneity. They could not use any data collected directly from schools, such as administrative data on schools' physical facilities or student-teacher ratios. Wildeman (2008) anonymously interviewed provincial officials responsible for creating the poverty scores and found that most ad hoc adjustments were made for schools near neighborhood boundaries that served students from multiple neighborhoods. Wildeman's respondents reported no lobbying of provincial officials by schools to change their scores, although lobbying may have occurred in the third stage described below. The formulae used to determine the poverty scores varied by province and no province made its formula publicly available.

Second, the national government divided all schools in the country into five quintiles based on these poverty scores. Each quintile was intended to contain roughly 20% of the students in the country (based on 2006 enrollment data) and contain similarly poor schools in each province. In the relatively poor Eastern Cape province, 35% and 6% of all schools were assigned to the first and fifth quintiles respectively; in the relatively wealthy Western Cape province, 7% and 23% of all schools were assigned to the first and fifth quintiles respectively. The number of schools in each quintile was chosen after the poverty scores had already been assigned, so it was impossible for poverty scores to be manipulated precisely in the neighborhood of the thresholds between quintiles.

Third, provincial governments instructed specific schools to eliminate fees. The national government directed provinces to eliminate fees in quintile 1 and 2 schools in 2007 and quintile 3 schools in 2010. Provinces followed this guideline relatively closely, instructing 99% of quintile 1 and 2 schools to eliminate fees in 2007 and 90% of quintile 3 schools to eliminate fees in 2010. By 2012, provinces had instructed respectively 100, 100, 93 and 47% of quintile 1, 2, 3, and 4 schools to eliminate fees.

Figure 3 shows the rate of fee elimination using administrative data from schools (panel A) and nationally representative household survey data (panel B). Both data sources show that fee payment was nearly universal up to 2006 and that fee elimination occurred rapidly from 2007.[13]

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administrative unit but are the smallest geographic unit at which census data was available to provinces. Each ward contained on average two public secondary schools.

[13]Before, schools were required to waive fees for children from households with total income less than ten times

However, the the household survey data shows that implementation was smoother and slower than mandated by government. Implementation was slightly slower in grades 11 and 12 than grades 8-10. Throughout the paper I use the government's official classifications to define schools' treatment status and estimate treatment effects. In Appendix B I integrate the administrative and survey data to estimate a mean compliance rate of 83% and I rescale treatment effects to account for lagged implementation.

Given this policy structure, my research designs compares outcome trends across fee-eliminating and -charging schools. In the paper I focus on the double fixed effects model:

$$Y_{st} = \alpha_s + \beta_t + \mathbf{1}\left\{\text{Fees} = 0\right\}_{st} \cdot \delta + \epsilon_{st} \tag{1}$$

where Y_{st} denotes the outcome for school s in year t, α_s and β_t are respectively school and year fixed effects, and $\mathbf{1}\left\{\text{Fees} = 0\right\}_{it}$ is an indicator equal to one if and only if school s does not charge fees in year t. The school fixed effects account for time-invariant heterogeneity in Y across schools and the year fixed effects account for aggregate time-varing heterogeneity. If any school-specific time-varying heterogeneity is balanced across fee-charging and fee-eliminating schools, then δ is a weighted average of school-specific fee elimination effects on outcome Y for fee-eliminating schools.

Fee-charging and fee-eliminating schools are, by design, located in different communities. So the assumption of balanced time-varying heterogeneity is strong. I report robustness checks in Appendix C that recover treatment effects under weaker assumptions. I briefly outline these strategies here, with details in the appendix. First, I include school-specific linear trends in equation (1). These account for school-specific time-varying heterogeneity, provided it follows a linear structure. Second, I include linear trends in population and employment constructed from geography-specific census data and matched to schools using geocodes. These account for locality-specific time-varying heterogeneity via population and socioeconomic status, again provided it follows a linear structure.

Third, I include interactions between year fixed effects and time-invariant measures of location and historical resources in equation (1). Following Duflo (2001), these account for non-linear time-varying heterogeneity in schools' outcomes, provided it is fully explained by the three characteristics. Fourth, I instrument fee-charging status assigned by the provincial government with the elimination schedule announced by the national government. This accounts for deviations from the original

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the value of the fees. But 97% of students reported paying fees in 2006, showing that waivers were uncommon.

Figure 3: Fee Elimination Timeline

Panel A: Share of Students Enrolled in Schools Instructed to Eliminate Fees
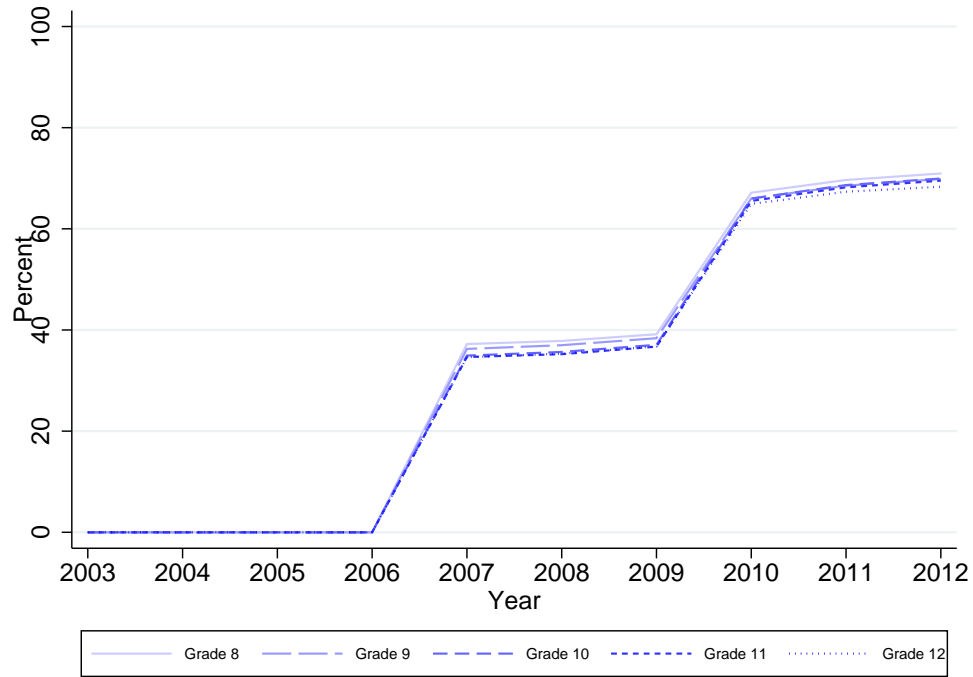
Panel C: Share of Enrolled Students Paying No Fees in Household Survey Data
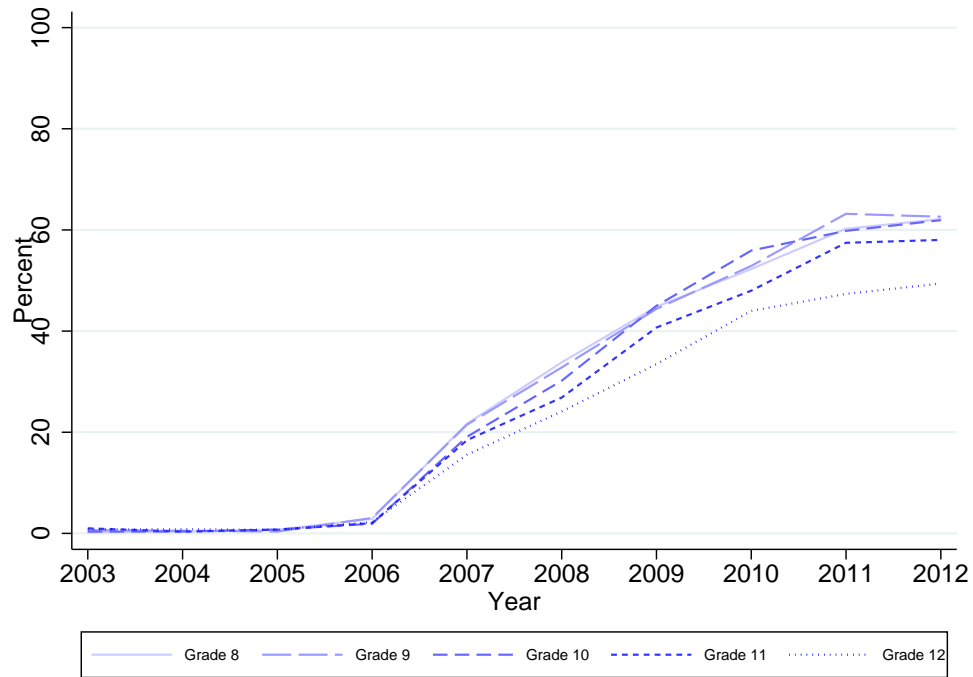
Notes: The first panel shows the enrollment-weighted share of public schools that were instructed to eliminate fees by year. The second panel shows the share of students aged 6-24 enrolled in primary or secondary school who report paying zero fees, using post-stratification-weighted student-level data from the General Household Survey.

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implementation schedule that are correlated with time-varying outcome heterogeneity. For example, schools might lobby to eliminate fees earlier than scheduled because they expect falling enrollment, leading to downward-biased treatment effect estimates. Instrumenting fee elimination with the original implementation schedule avoids this problem.

Fifth, I estimate equation (1) using log-transformed outcomes, as the assumption of balanced time-varying heterogeneity need not hold for all outcome scalings. I also explore several outcome scalings specifically designed to reduce the influence of outcome measurement error in Appendix A. Sixth, I use a regression discontinuity design to compare schools with poverty scores close to the thresholds determining the timing of fee elimination. This robustness check makes the weak assumption that time-varying heterogeneity is balanced near the thresholds. But this approach recovers a fee elimination effect of fee elimination only for schools near the thresholds and some features of the poverty score data complicate the discontinuity design, motivating my decision to use this only as a supplementary analysis.

4 Treatment Effects on Enrollment and Attainment

Fee elimination increases secondary school enrollment by 10.1 students per school or 3.2% of mean counterfactual enrollment (Table 1).[14] There are three ways to interpret the magnitude. First, pre-elimination enrollment in the relevant population was 72.1%, so this implies a 2.3 percentage point rise in enrollment.[15] This leaves 25.6% of the relevant population out of school, so the effect is small relative to the remaining non-enrollment rate. Second, 3.2 equals the 64th percentile of the distribution of annual school-specific changes in enrollment in fee-eliminating schools prior to elimination. This shows that fee elimination does not, on average, shift schools to a substantially different enrollment level. Third, this implies an enrollment elasticity with respect to pecuniary education costs of -0.114, rising to -0.137 once I account for policy non-compliance. See Appendix B for details and substantial caveats on estimating this elasticity. Carneiro, Das, and Reis (2016) collect price elasticities for the margin between fee-charging private and free public schools in devel-

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[14]I estimate counterfactual enrollment in school s in year t from equation (1) as $\hat{Y}_{st}^{CF} = \hat{\alpha}_s + \hat{\beta}_t$. I then define mean counterfactual enrollment as mean $\hat{Y}_{st}$ in fee-eliminating schools in the years after fee elimination.

[15]I use the General Household Survey data to calculate the enrollment rate for the relevant population, as this is not measured by school-level administrative data. I define the enrollment rate as the share of respondents aged 24 or younger who are grade-eligible to enroll in secondary school and do so. I exclude respondents from households above the 80th percentile of the expenditure distribution, to approximate the population likely to attend fee-eliminating schools. I use post-stratification weights and average over four years of pre-elimination data.

oping countries. These elasticities all exceed 0.2 in absolute value and we might expect enrollment to be more price sensitive at the margin between public and no school than between private and public school. All three interpretations suggest that either enrollment is relatively price insensitive or fee elimination caused enrollment-dampening changes in school quality.

Table 1: Treatment Effects on Enrollment

	(1)	(2)	(3)	(4)	(5)	(6)
	Total	Grade 8	Grade 9	Grade 10	Grade 11	Grade 12
Fee elimination	10.08***	6.26***	4.78***	6.74***	-1.25	-4.11***
	(1.83)	(0.65)	(0.65)	(1.01)	(0.86)	(0.69)
Effect as % of counterfactual mean	3.18	8.57	5.98	4.76	-1.01	-5.17
	0.58	0.89	0.82	0.72	0.69	0.87
Effect on enrollment rate	2.29	7.16	4.54	3.35	-0.69	-3.31
	0.42	0.74	0.62	0.50	0.47	0.56
Counterfactual mean outcome	318	73	80	142	124	79
# school-year obs	92756	84775	80874	53814	51470	50540
# schools	10317	9533	9042	5920	5650	5577
Adjusted R2	0.957	0.883	0.891	0.870	0.839	0.745

Coefficients are from regressing each outcome on an indicator for fee elimination, school fixed effects, and year fixed effects. Sample covers all public schools in 2003-2012 excluding those in high-income communities. Sample sizes exclude singleton units within fixed effect groups. Heteroskedasticity-robust standard errors are shown in parentheses, clustering by school. Counterfactual mean outcome = pre-elimination mean outcome in fee-eliminating schools adjusted for aggregate time changes.

The enrollment effect varies substantially across grades. I estimate equation (1) separately for each grade using grade-specific enrollment as an outcome, omitting schools that do not offer the relevant grade. Enrollment in grades 8, 9, and 10 rises by 4.8 - 6.7 students (4.8 - 8.6% of mean counterfactual enrollment). Enrollment in grade 11 does not change substantially while enrollment in grade 12 falls by 4.1 students (5.2%). Fee elimination thus raises the enrollment rate for grade 8 from 84 to 91%, for grade 9 from 76 to 81%, and for grade 10 from 70 to 74%, while lowering the enrollment rate for grade 12 from 64 to 61%. These results imply elasticities of up to -0.36, -0.24, and -0.18 in respectively grades 8, 9, and 10 once option value is taken into account (Appendix B).

The heterogeneity across grades may reflect persistent heterogeneity in grade-specific effects or transient length-of-exposure effects. The latter pattern occurs if fee elimination increases the size of each cohort starting secondary school but the larger cohorts are observed for more years in early grades. To separate these explanations, I estimate the treatment effects of fee elimination separately for each of the first six years after treatment. There are two clear patterns in Figure 4. First, fee elimination lowers grade 12 enrollment in all years with little evidence of a time trend.

Figure 4: Treatment Effects by Grade and Year Relative to Treatment
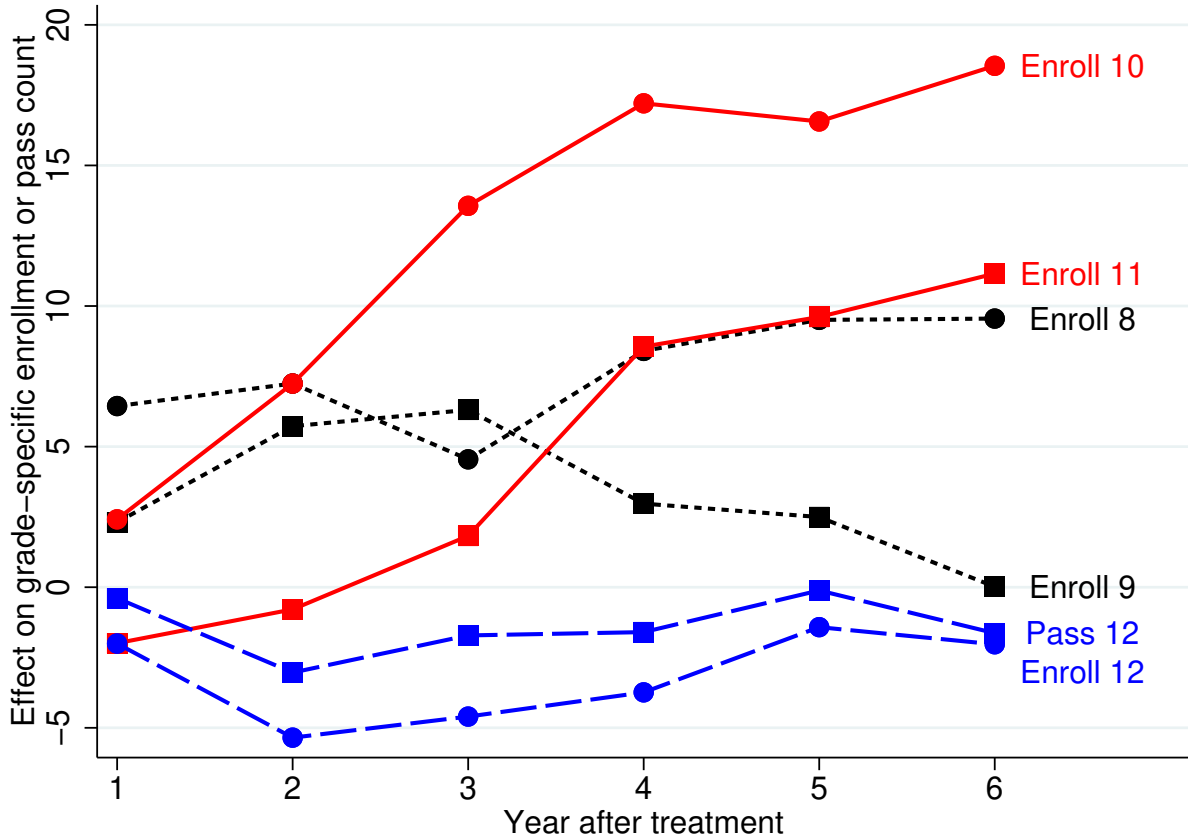
This figure shows point estimates from regressing enrollment in each grade and high school graduation exam pass counts on indicators for years 1, ..., 6 after fee elimination and school and year fixed effects. The second group of schools eliminating fees in 2010 are observed for only three years after treatment. So the estimates for years four, five, and six are identified only by schools eliminating fees in 2007. These should be compared to the estimates for years one, two and three with caution. Sample covers all public schools in 2003-2012 excluding those in high-income communities. Point estimates and standard errors are reported in Appendix Table A.10.

Second, the treatment effects on grade 10 and 11 enrollment rise steadily through time. Both patterns are consistent with low progression rates in higher grades. Fee elimination raises enrollment but students are unable to progress to grade 12 or pass graduation exams. Hence enrollment accumulates in grades 10 and 11. Grade 8 and 9 enrollment have suggestions of respectively upward and downward trends after treatment but these changes are smaller and not monotonic.

The fact that fee elimination measured in household survey data lagged the official elimination schedule (Figure 3B) slightly complicates interpretation of the time path of treatment effects. Treatment effects in the first two years after treatment will be attenuated due to slow compliance, particularly in higher grades. Slow compliance may contribute to the rise in treatment effects

Table 2: Treatment Effects on High School Graduation

	(1) Grade 12 enrollment	(2) # passes	(3) % pass	(4) % pass from grade 11	(5) % pass from grade 10
Fee elimination	-4.06***	-1.76***	-1.59***	-1.72***	-1.56***
	(0.70)	(0.44)	(0.35)	(0.27)	(0.25)
Effect as % of counterfactual mean	-5.11	-4.14			
	(0.88)	(1.03)			
Counterfactual mean outcome	79.4	42.5	56.3	35.4	30.2
# school-year obs	48351	48351	48351	47772	47390
# schools	5192	5192	5192	5190	5175
Adjusted R2	0.736	0.815	0.467	0.509	0.533

Coefficients are from regressing each outcome on an indicator for fee elimination, school fixed effects, and year fixed effects. Sample covers all public schools offering high school graduation exams in 2003-2012 excluding those in high-income communities and those that cannot be matched to enrollment records. Sample sizes exclude singleton units within fixed effect groups. Heteroskedasticity-robust standard errors are shown in parentheses, clustering by school. Counterfactual mean outcome = pre-elimination mean outcome in fee-eliminating schools adjusted for aggregate time changes.

from year one to two for grades 8-11 enrollment and fall for grade 12 enrollment. However, slow compliance cannot explain the persistently negative effects on grade 12 enrollment or steadily rising effects on grade 10 and 11 enrollment.

Fee elimination lowers both grade 12 enrollment and the pass count and rate on secondary school graduation exams (Table 2). I can match 96% of schools offering grade 12 to exam results (see Appendix A for details). In these schools, enrollment in grade 12 falls by 4.1 students and the pass count falls by 1.8 students, with no evidence of a trend in either effect (Figure 4). The pass rate in these schools falls from 56.3% to 54.7%. The ratios of the pass count in year y to grade 11 enrollment in year $y - 1$ and grade 10 enrollment in year $y - 2$ also fall, by respectively 1.7 and 1.6 percentage points. The counterfactual means for these ratios are substantially lower than the counterfactual mean pass rate, consistent with low progression rates from grades 10 and 11 to grade 12. The pass count and pass rate effects are driven by three mechanisms: effects on the grade 12 enrollment count, effects on the composition of enrolled students ('composition effects'), and effects on the academic performance of students whose enrollment decisions are unaffected by fee elimination ('learning effects'). I estimate bounds on these mechanisms in Section 5.3, which show that the learning effect is unlikely to be large.

In the next section I propose and evaluate a conceptual framework that explains the estimated treatment effects. In Appendices A and C I show that these results are robust to accounting for

potential measurement error in both enrollment and treatment assignments, time-varying hetero-geneity across schools that does differs between fee-charging and fee-eliminating schools, different scalings of the outcome variables, and different measures of performance on graduation exams.

5 Explaining the Enrollment and Attainment Effects

5.1 Theoretical Framework

In this section I propose a simple framework of enrollment and passing that can explain the esti-mated treatment effects. The framework only matches the observed treatment effects if fee elim-ination reduces school quality in some way, which I explore in Section 5.2. The framework omits potentially some important features, which I discuss in Section 5.5.

Assume that each agent i with g completed grades chooses whether to enroll in grade $g + 1 \in \{1, \ldots, 12\}$ in the current period. If she enrolls she pays a pecuniary cost $C_{i,g} > 0$ with certainty. If she enrolls she passes the grade with probability $P_{i,g}$. Passing has present value $V_{i,g+1}$, which includes the option value of enrollment in subsequent grades. Failing has present value $V_{i,g}$. I assume pass probabilities and values are increasing functions of school quality Q but I omit this from the notation for simplicity. I assume that all agents have discount factor β and that the value of failing is the value of the current state, delayed one period. This holds, for example, if the present value of the current state is the discounted flow of future earnings from all periods. Hence agent i's decision rule is

$$\varepsilon_{i,g} = \mathbf{1}\left\{P_{i,g} \cdot V_{i,g+1} + (1 - P_{i,g+1}) V_{i,g} - C_{i,g} \geqslant \frac{V_{i,g}}{\beta}\right\}. \tag{2}$$

Define $\Delta V_{i,g+1} \equiv V_{i,g+1} - V_{i,g}$ as the marginal return to attaining grade g and $U_{i,g+1} = P_{i,g} \cdot \Delta V_{i,g+1} - V_{i,g} \cdot (1 - \beta)/\beta$ as the expected marginal return net of opportunity cost, both in present discounted value terms. Using this definition the decision rule can be written as

$$\varepsilon_{i,g} = \mathbf{1}\left\{P_{i,g} \cdot \Delta V_{i,g+1} - V_{i,g} \cdot \left(\frac{1 - \beta}{\beta}\right) \geqslant C_{i,g}\right\} = \mathbf{1}\left\{U_{i,g+1} \geqslant C_{i,g}\right\} \tag{3}$$

Define the population share enrolling in grade g as $\varepsilon_g = F_{U|Z}(C_{i,g}|Z)$, where Z is a vector of school quality and all agent-level heterogeneity. We can interpret ε_g as the intersection of a demand curve for enrollment in grade g determined by $F_{U|Z}(\cdot|Z)$ and a cost curve $C_{i,g}$. Define $\pi_g \leqslant \varepsilon_g$ as the population share passing grade g. This framework predicts that both ε_g and π_g will be higher

when agents have high passing probabilities, high marginal returns to attaining grade g (including option values), low values of attaining grade $g - 1$, and low pecuniary costs. The relationship between both ε_g and π_g and school quality Q are theoretically ambiguous: quality raises passing probabilities and marginal returns to grade attainment but also raises the value of grade attainment and may raise pecuniary costs.

Define $\rho_g = \mathbb{E}\left[\Delta V_g\right] = \int (V_{i,g} - V_{i,g-1}) \, dF(Z)$ as the population average marginal return to attaining grade g. Define $\tilde{\rho}_g = \mathbb{E}\left[V_g | \text{attain } g\right] - \mathbb{E}\left[V_{g-1} | \text{attain } g\text{-}1\right]$ as the mean difference in realized value between agents who obtain grades g and $g - 1$. In this framework, $\rho_g - \tilde{\rho}_g < 0$. The gap will be larger when there is substantial heterogeneity in the marginal returns to grade attainment. The framework does not deliver a prediction about the relationship between $\rho_g - \tilde{\rho}_g$ and g without stronger assumptions.

This framework can help to interpret several features of the South African context, described in Section 2. Both ε_g and π_g are decreasing in g. Both C_g (averaged over enrolled agents) and $\tilde{\rho}_g$, proxied by earnings differences, are increasing in g. All of ε_g, π_g, and C_g are increasing in agents' socioeconomic status, which is correlated with the quality of schools they attend, at least based on proxies like class sizes and financial resources. Youth unemployment is very high, implying a low opportunity cost to enrollment, particularly in early grades. The framework suggests that enrollment is high in early grades because opportunity and pecuniary costs are low and passing is likely. The marginal value of early grade attainment may be low in the labor market (as $\rho_g < \tilde{\rho}_g$ and $\tilde{\rho}_g$ is low at early grades) but the option value may be high. Enrollment in higher grades falls because pecuniary and potentially opportunity costs rise and passing probabilities fall, even if marginal returns rise. The fall in passing probabilities is particularly relevant for agents enrolled in poorer schools, where pass rates are low and passing is weakly linked to independently measured student performance.

Fee elimination can enter this framework in two ways. Fee elimination reduces the pecuniary cost, which weakly increases ε_g. Define $\Delta \varepsilon_g(\Delta C) > 0$ as the change in enrollment in grade g induced by the cost reduction from fee elimination. If agents are forward-looking and believe fee elimination to be permanent, then fee elimination reduces the pecuniary cost of enrollment in all future grades. This raises the option value $V_{i,g+1}$ of enrollment in all grades below 12. This increases enrollment

in all grades and by more in early than late grades: $\Delta\varepsilon_{g-1}(\Delta C) > \Delta\varepsilon_g(\Delta C) > 0$, for all g.[16] The option value channel means that enrollment effects are higher in early grades even when, as in this setting, cost reductions are larger in later grades. $\Delta\varepsilon_{12}(\Delta C_{12})/\Delta C_{12}$ measures the local slope of the demand curve for enrollment in grade 12. For grades below 12, the denominator should be the discounted sum of cost reductions in the current and all future grades.

Fee elimination can also reduce school quality, which reduces $P_{i,g}$ and $V_{i,g+1}$ and hence reduces enrollment.[17] Fee elimination might reduce school quality through multiple mechanisms, which I discuss in Section 5.2. Define $\Delta\varepsilon_g(\Delta C, \Delta Q)$ as the change in enrollment in grade g induced by both the cost reduction and quality change from fee elimination. The simplest case occurs when fee elimination reduces costs in all grades and only reduces quality in grade 12. Then $\Delta\varepsilon_{12}(\Delta C, \Delta Q) < \Delta\varepsilon_{12}(\Delta C, 0)$ and the sign of $\Delta\varepsilon_{12}(\Delta C, \Delta Q)$ is ambiguous. In all earlier grades, the current-period cost of enrollment falls but the option value of enrollment in subsequent grades may fall – due to lower cost in each grade – or rise – due to lower quality in the terminal grade. Hence the sign of $\Delta\varepsilon_g(\Delta C, \Delta Q)$ is ambiguous for all g. If quality falls by an equal margin in all grades, then we still have $\Delta\varepsilon_g(\Delta C, \Delta Q) < \Delta\varepsilon_{g-1}(\Delta C, \Delta Q)$, for all g. In particular, enrollment effects can be positive in early grades and negative in later grades when all grades experience the same change in cost and quality.

The effect of fee elimination on the pass rate π_g depends on the enrollment rate ε_g and the distribution of $P_{i,g}$ for enrolled agents. If fee elimination only reduces costs, then π_g rises. The effect on the pass rate conditional on enrollment, π_g/ε_g, is ambiguous.[18] If fee elimination reduces costs and quality, the effect on π_g is ambiguous. Unlike enrollment, $\Delta\pi_g(\Delta C, \Delta Q)$ need not be monotonically decreasing by grade.

These predictions qualitatively match the treatment effects reported in Section 4. Enrollment in early grades rises substantially because the forward-looking cost effect dominates, while enrollment in grade 12 and initially grade 11 fall because the quality effect dominates. This heterogeneity

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[16]The latter ranking need not hold if pre-elimination enrollment in some grades is universal. But even post-elimination enrollment in this setting is below 100% in all grades.

[17]I assume the quality change has no effect on $V_{i,g}$ for agents who have already attained grade g. This is a minor restriction. It rules out psychic costs from observing quality reductions at one's alma mater and signalling models in which employers infer agent productivity from their school quality but not their cohort.

[18]This rate will fall if the marginal enrollers are negatively selected on latent passing probabilities. But negative selection is not guaranteed. For example, agents with high latent passing probabilities might also have high opportunity costs of enrollment.

across grades may occur because (i) quality falls in all grades and the effective cost reduction is larger in early grades or (ii) the quality drop affects $P_{i,g}$ and $V_{i,g+1}$ more in higher grades. The graduation exam pass count drops because some marginal grade 12 enrollers have positive latent passing probabilities (a composition effect) and/or quality lowers passing probabilities for inframarginal enrollers (a learning effect).

I do not structurally model the value functions $V_{i,g}$ or passing functions $P_{i,g}$. The framework can be interpreted as the reduced form of some explicit structural models. For example, $P_{i,g}$ may be derived from a learning model in which passing depends on agents' ability, which they learn over time by observing their passing/failing outcome in each grade. In this model, increasing early grade enrollment through fee elimination can generate additional welfare gains by subsidizing learning about graduation prospects. This approach may be particularly relevant at the beginning of secondary school when students are exposed to a potentially different learning environment to primary school. Similarly, the value functions $V_{i,g}$ may represent the present value of future earnings that agent i will earn after attaining grade g, as in Mincerian models. In such a model, the enrollment effects provide some information about the distribution of earnings returns to attainment. The substantial rate of non-enrollment in all grades after fee elimination is consistent with either low wage returns to attainment or low probabilities of passing for a substantial share of the treated population. However, we cannot infer the relative earnings returns to attainment for marginal and inframarginal enrollers without more data and/or structure on $P_{i,g}$. This constrains the scope to price the value of the fee elimination policy in terms of earnings effects.

5.2 Fee Elimination and School Quality

With a reduction in school quality, this framework qualitatively matches the enrollment and graduation effects. Fee elimination may change school quality in at least five ways. First, it may change class sizes due to changes in enrollment. Second, it may change peer group composition due to changes in enrollment and differences between marginal and inframarginal students. Third, it may change teacher behavior and composition due to changes in enrollment, class sizes, and class composition. Fourth, it may change non-personnel financial resources by removing fee revenue and potentially adding more transfers from state governments. Fifth, it may change parent-school relationships as parents are no longer directly paying the school.

I observe proxies for the first three quality dimensions. I estimate treatment effects of fee elimination on these measures and show that fee elimination changes class size and teacher measures. I observe respectively little and no data on the fourth and fifth quality dimensions. I draw on other research and contextual information to assess these dimensions. These measures of school quality are not perfect but neither value-added measures nor revealed preference measures such as school-linked home prices are available in South Africa.

Fee elimination lowers the number of teachers per student, particularly full-time and permanent teachers (Table 3).[19] The total number of teachers per student falls by 1.3%. This drop is larger for full-time teachers (6.1%) and permanent teachers (5.3%). These results suggest that fee-eliminating schools used more part-time and temporary teachers to accommodate rising grade 8-10 enrollment, but not enough to maintain their counterfactual teacher-student ratio. This could in turn raise grade 12 class sizes or change who teaches them, reducing the quality of grade 12 education and deterring enrollment. This mechanism requires that grade 12 enrollment falls in the same schools where grade 8-10 enrollment rises. I calculate the ratio of grade 12 to grade 8-10 enrollment in each school and find that fee elimination reduces this by 5% (Table 3 column 4). Using a similar exercise, fee elimination reduces the ratio of graduation exam passes to grade 8-10 enrollment by 7.5% (Table 3 column 5).

Changes in school-level teacher-student ratios will have different effects on learning experiences in schools with different grade structures. School grade structures differ in my sample: a few schools offer grades 1-12, many offer grades 8-12, and a few offer 1-9 or 10-12. The crowding hypothesis is most relevant in schools offering grades 8-12. In schools offering other grade combinations, this mechanism should be attenuated (grades 10-12, grades 1-12) or absent (grades 1-9). I therefore estimate fee elimination effects on teacher-student ratios separately for schools that do and not offer any primary grades and separately for schools that offer all or only some secondary grades. The number of full-time teachers per student drops by at least 2.5% in each subsample and the number of permanent teachers per student drops by at least 4.5% in each subsample. I conclude that the fall in teacher-student ratios is robust across school types, consistent with the crowding hypothesis.

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[19]I scale outcomes for this analysis as the number of teachers per 100 students. Results are substantively similar when using students per teacher but treatment effects are less precisely estimated due to outliers arising from small values in the denominator.

Table 3: Treatment Effects on Teacher-Student Ratios

	(1)	(2)	(3)	(4)	(5)
	Teachers per 100 students			Grade 12	Exam passes
	All	Permanent	Fulltime	/ Grade (8+9+10) enrollment	
Fee elimination	-0.049**	-0.218***	-0.203***	-0.013***	-0.010***
	(0.021)	(0.022)	(0.023)	(0.003)	(0.002)
Effect as % of counterfactual mean	-1.27	-6.14	-5.17	-4.85	-7.49
	(0.54)	(0.62)	(0.57)	(1.19)	(1.24)
Counterfactual mean outcome	3.81	3.56	3.92	0.27	0.14
# school-year obs	92756	92756	92756	50479	48780
# schools	10317	10317	10317	5573	5224
Adjusted R2	0.454	0.457	0.455	0.506	0.481

Coefficients are from regressing each outcome on an indicator for fee elimination, school fixed effects, and year fixed effects. Outcomes in columns 1-3 are scaled as number of teachers per 100 students. Sample covers all public schools in 2003-2012 excluding those in high-income communities. Sample sizes exclude singleton units within fixed effect groups. Heteroskedasticity-robust standard errors are shown in parentheses, clustering by school. Counterfactual mean outcome = pre-elimination mean outcome in fee-eliminating schools adjusted for aggregate time changes.

Fee elimination's effect on peer group composition is harder to quantify, due to data limitations. I observe two measures of peer group composition: the share of enrolled students who are orphans and the share of enrolled students whose families are eligible for means-tested welfare grants. Fee elimination increases these shares by respectively 0.06 and 0.96 percentage points (standard errors 0.10 and 0.53) from bases of respectively 6.0 and 20.5%. This is consistent with marginal enrollers having slightly lower socioeconomic status than inframarginal enrollers. However, these are small changes in weak proxies for peer group composition. Statistically, these are likely to be measured with error as principals have limited information about their students' orphanhood and social grant eligibility. Perhaps reflecting this uncertainty, there is substantial item non-response to these questions in principal surveys. Economically, prior research does not find a robust negative relationship between students' learning environment and their peers' socioeconomic status.[20] I conclude that the evidence on peer composition as a quality mechanism is inconclusive.

I can only partially measure how fee elimination changes financial resources, as I do not observe comprehensive data on fees and on non-personnel transfers from government. In one relatively

[20]Several studies find that adding students from low-income backgrounds to schools has little effect on the test scores of higher-income incumbents (Angrist and Lang, 2004; Imberman, Kugler, and Sacerdote, 2012; Muralidharan and Sundararaman, 2015; Rao, 2019). The only evidence for South Africa is from Garlick (2018), who finds no effect of dormitory peer group composition on high-achieving university students, who are disproportionately likely to come from high-income households. However, Bold, Kimenyi, Mwabu, and Sandefur (2014) and Lucas and Mbiti (2012) find that fee elimination in Kenyan public primary schools shifted high-income students into private schools. This suggests parents expected negative peer group composition effects even if their expectations were not correct with respect to test scores.

wealthy province, I observe that government budgeted USD27 million for additional transfers to secondary schools to offset lost fee revenue. This allowed USD77 for each secondary school student enrolled before elimination, compared to the pre-elimination fee mean of USD46 in that province. Hall and Giese (2008) survey schools in one relatively poor province for two years before and one year after elimination about fees and government transfers. They find that rising transfers exceeded lost fee revenue in 92% of schools. However, qualitative research in two relatively poor provinces finds that transfers were sometimes paid to schools late and required changes in financial procedures that schools struggled to implement (Setoaba, 2011; Thwala, 2010). These patterns echo experiences in other African countries reviewed by Mbiti (2016). Even if non-personnel resources do change, this will not necessarily shift enrollment or graduation counts. In the South African setting, Pellicer and Piraino (2019) show that quasi-experimental increases in non-personnel transfers to schools have little effect on secondary school enrollment or graduation counts.[21] Given these partial data and prior research, it seems unlikely that changes in non-personnel financial resources drove large quality changes.

I cannot measure how fee elimination changes parents' engagement with schools and scope to exercise local accountability. I do not observe comprehensive data on parents' engagement in either fee-charging or fee-eliminating schools. Pilot research by Cilliers, Garlick, Ozier, Taylor, and Zeitlin (2015) found parents at South African schools in poor neighborhoods were unlikely to criticize, complain to, or request action by teachers in parent-teacher meetings. Mbiti (2016) reviews several developing country studies showing mixed evidence on parents' ability to engage with schools in ways that change education outcomes. These findings from prior work do not provide strong evidence for or against local accountability as a quality mechanism.

This discussion shows that fee elimination increased crowding. This provides one mechanism for the hypothesized quality shift. This may have induced dropout by marginal students and/or hurt their learning outcomes, lowering both grade 12 enrollment and graduation exam pass counts. Evidence for quality changes through peer group composition, financial resources, and local accountability is less conclusive.

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[21]This relates to a broader international debate about the effect of financial resources on education outcomes. World Bank (2018) reviews a range of developing country studies and finds on average a very weak positive relationship. Jackson (2018) finds a stronger positive relationship for US-based studies with relatively credible claims for causal interpretation.

5.3 Composition and Learning Effects on Graduation Exam Passes

The pass count and pass rate effects in Section 4 are driven by three mechanisms: effects on the grade 12 enrollment count, on the composition of enrolled students ('composition effects'), and on the academic performance of students whose enrollment decisions are unaffected by fee elimination ('learning effects'). The change in enrollment count is identified; the second and third mechanisms are not identified without stronger assumptions. However, I can bound these effects under the monotonicity-style assumption that fee elimination changes grade 12 enrollment decisions in weakly the same direction for all agents. Then the change in the pass count in each school can be written as

$$\underbrace{N_I \cdot \int_{\mathcal{I}} P_{i12}(NF)dF(P(NF)) - N_I \cdot \int_{\mathcal{I}} P_{i12}(F)dF(P(F))}_{\equiv \text{ Learning effect}} \underbrace{-N_M \cdot \int_{\mathcal{M}} P_{i12}(F)dF(P(F))}_{\equiv \text{ Composition effect}}, \quad (4)$$

where $\mathcal{I}$ denotes the set of N_I inframarginal agents who enroll whether or not fees are charged, $\mathcal{M}$ denotes the set of N_M marginal agents who enroll only if fees are charged, and $P_{i12}(j) \sim F(P(j))$ denotes the pass probability for agent i in state $j \in \{F, NF\}$. Monotonicity allows me to consider only one group of marginal enrollers in the composition effect. The grade 12 enrollment analysis shows that the mean school contains 4.1 marginal enrollers and 75.3 inframarginal enrollers.

Under the monotonicity assumption, the composition effect is bounded between -4.1 and 0. The lower bound -4.1 holds when all marginal students would have passed if enrolled; the upper bound 0 holds when all marginal students would have failed if enrolled. The learning and composition effects must sum to -1.8, so the corresponding bounds on the learning effect are -1.8 and 2.3. At the lower bound, fee elimination shifted 1.8 of 75.3 inframarginal enrollers in the mean school from passing to failing, lowering the pass rate by 2.4 percentage points. At the upper bound, fee elimination shifted 2.3 of 75.3 inframarginal enrollers in the mean school from failing to passing, raising the pass rate by 3.1 percentage points. These bounds rule out large learning effects.

I can also evaluate the magnitude of the learning and composition effects by assuming only one effect exists. If there is no learning effect, then 1.8 of the 4.1 marginal enrollers would have passed if enrolled. This implies pass rates of 43.4% for marginal enrollers and 54.7% for inframarginal enrollers (standard errors 9.2 and 0.6 percentage points, p-value on difference = 0.221). This implies moderately positive selection into inframarginality on latent passing probabilities. If there

is no composition effect, then the untreated pass rate for both marginal and inframarginal students is 54.7% and 2.3 of 4.1 marginal enrollers would have passed. This implies that the learning effect shifted 0.5 of 75.3 inframarginal enrollers in the mean school from failing to passing, raising the pass rate by 0.6 percentage points (standard error 0.6).

I conclude that, under monotonicity, learning effects on inframarginal students' pass rates are not large and may be zero with moderate positive selection into inframarginal enrollment. The largest negative learning effect consistent with the data is 2.4 percentage points, occurring when marginal enrollers have very high untreated passing probabilities. In my framework, this could occur if latent passing probabilities are strongly positively correlated with opportunity costs of enrollment, or marginal students' latent passing probabilities or valuations of grade attainment are very sensitive to fee elimination-induced changes in quality. The opportunity cost explanation is inconsistent with prior work in South Africa showing that higher early grade test scores predict subsequent enrollment (Lam, Ardington, and Leibbrandt, 2010; Taylor, Van Der Berg, Reddy, and Janse van Rensburg, 2011). The sensitivity explanation does not appear to be testable in my data. Outside my framework, positive selection of marginal enrollers is possible if marginal enrollers transfer from fee-eliminating to fee-charging schools. I rule out large transfers in Section 6. I show in Appendix C that there may have been larger learning effects on a higher graduation exam performance threshold, subject to some data limitations.

5.4 Treatment Effect Heterogeneity Across Observed School Characteristics

The theoretical framework yields few testable implications about heterogeneity across observed school characteristics in either untreated outcomes or treatment effects. Most obviously, untreated enrollment and enrollment effects should be respectively decreasing and increasing in untreated fees, conditional on quality. But limited data on school-specific fees and weak proxies for quality make this prediction difficult to test across schools. The relationships between enrollment and graduation effects and pre-elimination quality proxies like graduation rates and teacher-student ratios are theoretically ambiguous. Formally, these relationships depend on the school-specific distributions of ability and the structure of the return, progression, and cost functions. In principle, I could estimate treatment effect heterogeneity and use this to infer features of these distributions and functions. However, the quality and cost proxies are not so strong that this is likely to be

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informative. Cost and quality are also likely to be strongly associated, so isolating heterogeneity on any one dimension is difficult.

Instead, I briefly report treatment effect heterogeneity over some time-invariant observed school characteristics and acknowledge multiple interpretations. For each characteristic, I estimate an extension of equation (1) that interacts fee elimination and the year fixed effects with the school characteristic. I convert continuous characteristics into indicators for above-median values. For time-varying characteristics, I calculate school-specific mean values for 2003-2006.

Treatment effects on all outcomes are slightly higher in schools with higher pre-elimination teacher-student ratios and graduation exam pass rates. To the extent that these are proxies for school quality, this shows higher enrollment effects in higher-quality schools. For early grade enrollment only, treatment effects are higher in schools in lower socioeconomic quintiles and rural schools. Interpreting this heterogeneity is difficult. Schools in lower SES quintiles are likely to have both lower cost and quality, while their untreated enrollment rates will be particularly low and hence least sensitive to ceiling effects. Rural schools are likely to be lower quality, their untreated enrollment rate is lower, and fees may be a smaller share of enrollment costs due to longer travel.

I also find no evidence of gender heterogeneity: both the pre-elimination education outcomes and the treatment effects are similar for women and men.

5.5 Alternative Explanations

In this section I discuss alternative explanations for the estimated treatment effects that are excluded from the framework in Section 5.1: capacity constraints, ceiling effects, credit constraints, and behavioral biases. I assume the effects are unbiased and focus on different economic explanations for them, with possible biases discussed in the appendices.

My framework does not model supply-side capacity contraints to enrollment. But schools may have fixed enrollment capacity and allocate capacity through endogenously chosen fees (only before the elimination policy) or by rationing spaces. In the presence of rationing, we cannot learn about the underlying enrollment decision process from treatment effects on school-by-grade enrollment counts. However, I argue that two patterns in the data are not consistent with time-invariant capacity constraints. First, the probability of observing identical school-by-grade enrollment values two years in a row ranges from 3 to 6% across grades (see Appendix A for details). Second, the

probability that maximum school-by-grade enrollment after fee elimination exceeds the maximum value before fee elimination ranges from 21 to 38% across grades. This probability is not higher in a matched control group of fee-charging schools. These patterns do not rule out capacity constraints that trend through time or bind in a small share of schools.

Ceiling effects on enrollment may also limit our scope to learn about the underlying enrollment decision process from treatment effects on enrollment. If enrollment is close to universal for a specific population, then fee elimination may have limited effects on enrollment even though individual enrollment decisions would be sensitive to increases in prices or larger shifts in quality. However, post-elimination enrollment in all secondary grades is not close to universal, ranging from 88.8% in grade 8 to 61.2% in grade 12.[22] It is possible that the demand curve for early secondary enrollment would be more elastic at lower initial enrollment. But the fee elimination policy does speak to an interior section of the (quality-adjusted) demand curve rather than hitting a ceiling.

My framework implicitly assumes that agents can borrow against future income to fund enrollment, so credit constraints do not bind. If credit constraints bind, then the estimated enrollment effects do not speak to the demand curve for enrollment in specific grades even absent quality effects. Both the treated and untreated enrollment rates will be weakly lower with credit constraints than without, as some agents with positive expected returns to enrollment cannot borrow to fund enrollment. But the treatment effect on enrollment in each grade may be higher or lower with credit constraints. This depends on the relative frequency of credit-constrained agents in two latent strata: (i) agents with positive expected returns to enrollment at the pre-elimination cost, and (ii) agents with positive expected returns to enrollment only at the post-elimination cost. These frequencies are not measureable without both more structure in the framework and household-level data on credit access. Few measures of credit access are available in this setting. Edmonds (2006) finds evidence that credit constraints bind on rural but not urban enrollment decisions in South Africa. I find slightly larger early-grade enrollment effects in rural areas. Edmonds's and my findings are consistent with credit constraints binding in stratum (i) in rural areas. But larger effects in rural areas may also reflect rural-urban differences in schools and/or labor markets.

My framework abstracts away from behavioral considerations. Risk aversion, higher discount rates, or present bias will all reduce the value of enrollment and reduce both treated and un-

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[22]See Appendix E for details on this calculation and some robustness checks.

treated enrollment (assuming uncertainty about passing outweighs across-grade heterogeneity in uncertainty about returns). But the relationship between these factors and the magnitude of fee elimination effects is theoretically ambiguous. Sunk cost bias might explain the drop in grade 12 enrollment without assuming a quality adjustment. If agents or their parents value enrollment more having paid for it, fee elimination may increase dropout in late secondary school. But if the magnitude of the sunk cost effect is increasing in the value of fees paid through time, then suk cost bias should reduce grade 12 enrollment steadily more in each year after fee elimination. This pattern is not visible in Figure 4. Direct evidence on sunk cost effects in education is also limited and mixed (Berry and Mukherjee, 2019; Hidalgo, Onofa, Oosterbeek, and Ponce, 2013; Ketel, Linde, Oosterbeek, and Van der Klaauw, 2016). I conclude that the quality explanation is more consistent with the time path of this drop and the co-occurrence of the drop with crowding than the arguably most prominent behavioral explanation.

6 Spillovers

In this section I show there are at most small spillover effects of fee elimination on neighboring schools. Theoretically, the effect of fee elimination on neighboring schools is ambiguous. Consider a generalization of the framework with two schools: school F charges fees while NF eliminates fees. Agents choose between enrollment in F, enrollment in NF, and non-enrollment. Agent i pays a cost $C_{i,g,s}$ for enrolling in school $s \in \{F, NF\}$. This agent-school-specific cost captures factors like the relative cost of transport to each school. Fee elimination reduces the cost of enrolling in NF. Holding quality at both schools constant, this will raise enrollment in NF, lower enrollment in F, and raise total enrollment. If fee elimination lowers quality at school NF through any mechanism discussed in Section 5.2, then enrollment in F may increase. It is possible for enrollment in F to change even when enrollment in NF does not change; this occurs if the net flow of agents between non-enrollment and enrollment in NF exactly equals the net flow of agents between enrollment in NF and enrollment in F. Graduation exam pass counts in F may also change via changes in grade 12 enrollment or ability composition in F.

Spillovers will be larger when many agents face a small difference between $C_{i,g,F}$ and $C_{i,g,NF}$. I use distance between schools as a proxy for this difference to construct a test for spillovers, comparing outcomes in fee-charging schools closer to and farther from fee-eliminating schools. I

estimate models of the form

$$Y_{gst} = \alpha_{gs} + \beta_{gt} + \text{Close}_{gst} \cdot \gamma + \text{Close}_{gst} \cdot \text{Post}_t \cdot \delta + \epsilon_{gst} \tag{5}$$

where gst indexes grade g in fee-charging school s in year t. I estimate these models using data from 2006, 2007, 2009, and 2010: one year before and one year after each of the major waves of fee elimination. Schools that still charge fees in 2010 appear in the sample in all four years. Schools that charge fees in 2007 but not 2010 appear in the sample only in 2006 and 2007. The school-by-grade fixed effect α_{gs} captures time-invariant school characteristics, including the proximity of neighbouring schools. The year fixed effect β_{gt} captures aggregate time changes. Close_{gst} captures proximity to fee-eliminating schools, using multiple measures of proximity defined below. For example, if school s offers grade g in years 2006 and 2007, then Close_{gst} might measure the number of nearby schools that offer grade g in years 2006 and 2007 and eliminate fees in 2007. Hence, γ captures the relationship between outcomes and local density of fee-eliminating schools. Post_t is an indicator equal to one in 2007 and 2010, so δ captures how the relationship between outcomes and local density of schools that eliminate fees changes when nearby schools actually eliminate fees. Close_{gst} is time-varying for two reasons. First, the set of nearby schools changes slightly through time as schools open, close, or change the grades they offer. Second, the set of nearby schools that eliminate fees is different in 2006/7 and 2009/10.

In the paper I define Close_{gst} as the number of schools within a 5 kilometer radius of school s in year t that offer grade g and eliminate fees in the relevant time period.[23] Figure 5 shows that roughly 50, 20, 10, and 10% of all school-grade observations have respectively 0, 1, 2, and 3-5 relevant neighbours within this radius. There are slightly more nearby fee-eliminating schools offering grades 8 and 9 than offering grades 10-12 and graduation exams.

I find that enrollment in fee-charging schools is largely unchanged when nearby schools eliminate fees (Table 4, Row 2). The largest effect is in grade 8: moving from the minimum exposure value to the 90th percentile (from 0 to 3 schools) lowers grade 8 enrollment by 1.7 students. This equals 1.2% of mean enrollment in fee-charging schools and only 28% of the direct effect of fee elimination on grade 8 enrollment. Point estimates for grades 9 and 10 are closer to zero and not statistically

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[23]5 kilometers is the median distance between a fee-charging school and the nearest fee-eliminating school offering the same grade. I winsorize the distribution of the number of schools at the top half percentile: 20. Winsorization slightly reduces the standard errors but does not change the point estimates.

Figure 5: Number of Fee-eliminating Schools Close to Each Fee-charging School
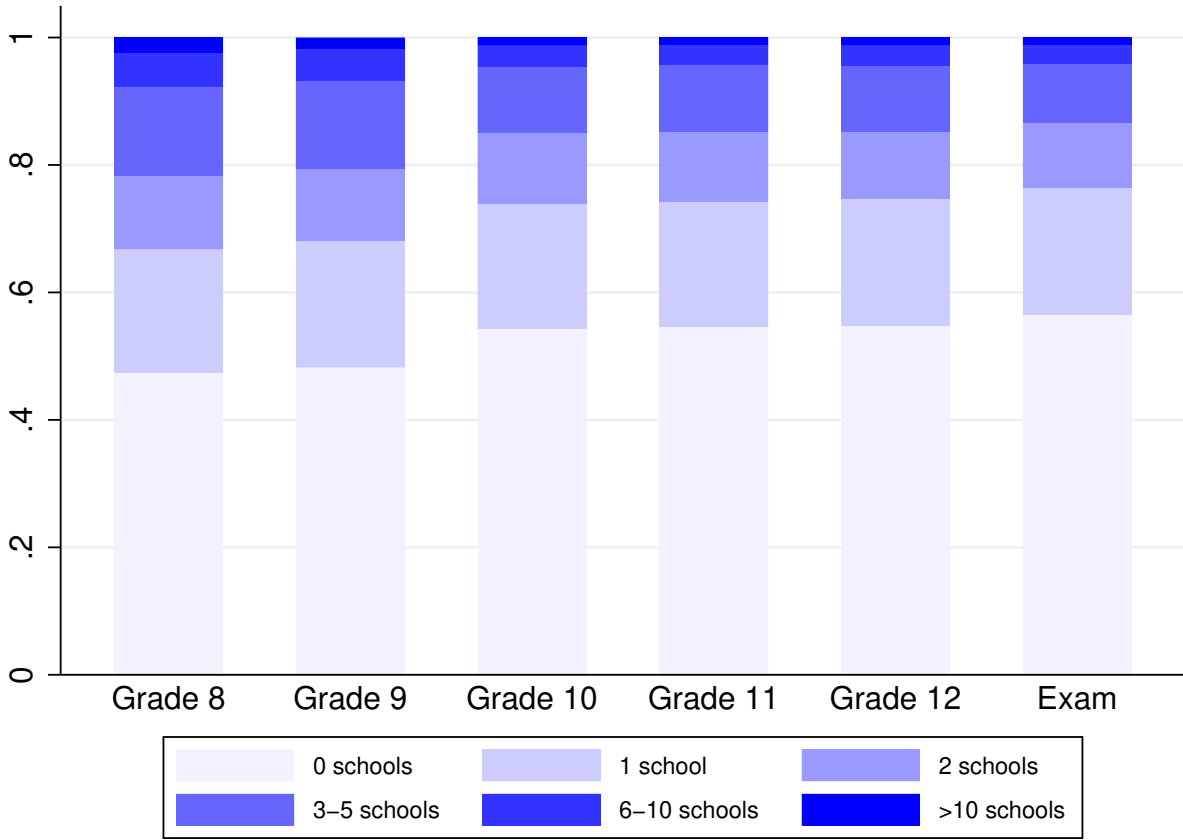
This figure shows the number of fee-eliminating schools close to each fee-charging school. The sample covers all fee-charging public schools in 2007 and all fee-charging public schools in 2010, excluding schools in high-income communities. Schools that charged fees in both years appear twice in the sample. For each school-grade-year unit in the sample, I calculate the number of schools (i) within a 5km radius that (ii) did not charge fees in the relevant year and (iii) offered the same grade in the relevant year. For the final bar, I calculate the number of schools that also offered high school graduation examinations.

significant at conventional levels. I conclude that transfers from fee-charging to fee-eliminating schools do not account for much of the positive direct effect of fee elimination on early-grade enrollment. Similarly, transfers from fee-eliminating to fee-charging schools do not account for the negative direct effect of fee elimination on grade 12 enrollment.[24] I also find that higher local density of fee-eliminating schools is associated with slightly lower enrollment in the early grades

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[24]To see this, note that the direct effect of fee elimination on grade 12 enrollment is -4.1 students with 95% confidence interval [-5.5,-2.8]. The spillover point estimate on grade 12 enrollment is negative, with a 95% confidence interval [-1.2,0.3]. Even at the upper bound of the two confidence intervals, transfers can fully explain the former effect only if each fee-eliminating school transfers 0.3 grade 12 students to each of 9 fee-charging neighbours. As the numbers of fee-charging and fee-eliminating schools are roughly equal, this is impossible. .

Table 4: Spillover Effects on Enrollment & Exam Passes

	(1)	(2)	(3)	(4)	(5)	(6)
	Grade 8	Grade 9	Grade 10	Grade 11	Grade 12	Exam passes
# schools within 5km	-1.420***	-1.697***	-1.571**	-0.615	0.304	-0.285
	(0.349)	(0.402)	(0.612)	(0.516)	(0.446)	(0.261)
. . . × post-elimination	-0.576**	-0.040	-0.128	-0.137	-0.450	-0.052
	(0.265)	(0.324)	(0.434)	(0.373)	(0.372)	(0.231)
Mean outcome	138.87	148.72	213.70	177.75	112.86	69.81
Mean regressor	1.8	1.7	1.4	1.3	1.3	1.2
# school-year obs	7242	6812	5593	5455	5352	5275
# schools	2838	2666	2135	2075	2039	2009
Adjusted R2	0.879	0.893	0.902	0.885	0.783	0.845

Coefficients are from regressing each outcome on the number of fee-eliminating schools within 5km, its interaction with an indicator for 2007 or 2010, school fixed effects, and year fixed effects. Sample covers all fee-charging public schools in 2006-2007 and 2009-10 excluding those in high-income communities. Sample sizes exclude singleton units within fixed effect groups. Heteroskedasticity-robust standard errors are shown in parentheses, clustering by school.

of secondary school (Table 4, Row 1). This relationship shows there is some scope for neighboring schools to cut into each others' enrollment. However, fee elimination does not substantially change this relationship.

This analysis has two potential limitations. First, it relies one specific definition of $Close_{gst}$. In Appendix D I show that the finding of minimal spillovers is robust to using several alternative definitions: a binary indicator for having any fee-eliminating school within 5 kilometers, the number of schools within smaller and larger radii, and inverse distance to the nearest fee-eliminating school. Second, this analysis only identifies net spillovers on fee-charging schools. It is possible that an equal number of agents transfer from fee-charging to fee-eliminating schools due to cost reductions and transfer from fee-eliminating to fee-charging schools due to quality shifts. The school-by-grade-by-year administrative data cannot separately identify the two different types of transfers. In Appendix E I show that there is no relationship in household survey data between proximity to fee-eliminating schools and distance traveled to schools. The only case consistent with the administrative and survey data results is if roughly equal numbers of students transfer in each direction without changing the average distance traveled. This is not impossible, but not likely.

7 Conclusion

I show that a nationwide program of tuition fee elimination had mixed effects on education outcomes in South Africa. Enrollment in early grades of secondary school rose by 5-9%. Enrollment in

grade 12 and graduation exam passes fell by 4-5%. I explain these results with a simple dynamic framework. Agents make forward-looking enrollment decisions based on the cost of enrollment, probability of passing, and return to passing in the current grade, as well as the option value of enrollment in future grades. Fee elimination increases early grade enrollment by cutting the cost of enrolling in current and future grades. Fee elimination decreases grade enrollment and exam passes because the dynamic cost reduction is smaller and school quality falls, lowering the probability of converting enrollment into attainment and/or lowering the return to attainment. Consistent with the quality reduction hypothesis, fee elimination increased student:teacher ratios and reliance on part-time and temporary teachers.

These findings are consistent with multiple different welfare interpretations. Earnings-education associations in this setting are convex and highest for grade 12 and post-secondary education. To the extent that these associations are informative about the economic returns to enrollment for marginal students, the policy decreased enrollment and attainment at the education levels most associated with earnings gains. However, in models of imperfect information, enrollment increases in early grades may have substantial ex ante welfare gains by allowing students to learn about their progression and graduation prospects. These imperfect information models seem relevant in this setting, given high rates of non-progression and weak associations between measured ability and progression.

Fee elimination was intended as a redistributive transfer, targeting schools in poor neighborhoods rather than means-testing individuals. Qualitatively, this targeting was effective: schools in poor neighborhoods disproportionately served students from poor households and fee elimination did not induce transfers into treated schools to take advantage of lower costs. School-level targeting using neighborhood-level census data was probably cheaper and less subject to manipulation than individual-level targeting. The absence of large transfers across schools suggests the school-level targeting did not distort school choice decisions, unlike individual-level targeting which risks distorting earnings decisions. However, this was not necessarily the most effective form of redistributive transfer. The transfers to schools that were meant to offset lost fee revenue were raised through a highly progressive tax system. But even after fee elimination, enrollment remained lowest in the poorest households, who thus did not benefit from the transfer. This is consistent with work showing that conditional cash transfers can exclude particularly vulnerable households for whom

enrollment is either not optimal or not feasible even transfers (Baird, McIntosh, and Ozler, 2011).

These findings sound a cautionary note about the scope to increase education attainment by lowering user costs. Policies such as user fee reductions, conditional cash transfers, and transport cost reductions are popular policy instruments in developing countries. At least twelve African countries have largely or entirely eliminated fees in the past two decades and cash transfer programs, often conditioned on education participation, cover at least 750 million people worldwide (Department for International Development, 2011). I show that fee elimination does increase enrollment in early grades of secondary school, consistent with previous research on primary school fees. But fee elimination actually lowers enrollment and attainment in later grades, where probabilities of passing are lower and may be further reduced by fee elimination. Even in the more price-sensitive early grades, there are at least eleven inframarginal enrollers for each marginal enroller, making fee elimination an expensive mechanism for promoting enrollment. Given the quality challenges in many developing country education systems, user fee cuts on their own may have limited returns.

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Appendices For Online Publication Only

A Data Sourcing, Matching, and Quality Checks

In this appendix I describe the main data sources used in the paper, how these are combined, and how I assess the sensitivity of my results to measurement error in both outcome and treatment data. In Appendix E I show that the main results of the paper can be replicated in two household survey datasets, though both datasets have important limitations.

A.1 Administrative Data Sources and Merging

Student and teacher data: I use administrative data on students and teachers collected by South Africa's Department of Basic Education. All public and private schools complete a survey during the first week of the academic year, called the SNAP surveys. Schools report enrollment by gender in each grade and the number of teachers in each cell of female/male × permanent/temporary × full-time/part-time. These reports are nominally checked by local education 'circuit managers' and 2% of schools are meant to have their survey responses audited each year. The SNAP survey data also report the province and socioeconomic quintile for each province. I obtain these data from public records at the DataFirst research center at the University of Cape Town. The socioeconomic quintiles do change through time but I use the 2006 quintile assignments for all analysis in the paper, as subsequent changes might reflect school lobbying in response to the fee elimination policy.

Schools are identified by a time-invariant 'EMIS number' (Education Management Information System). I use this identity number to merge the SNAP data with other data sources.

Exam data: I obtain data on the high school graduation examination directly from the Department of Basic Education (up to 2009) and from public records at DataFirst (2010 onward). These data are reported for exam centers, which are uniquely identified by exam numbers. Most exam centers have EMIS numbers but some have missing or outdated EMIS numbers. Where EMIS numbers are available, an RA uses these for an exact merge. Where EMIS numbers are not available, an RA implements a fuzzy merge using school/center name and province. An RA checks for false positives by calculating the Levenshtein distance between center names and school names, rescaling this by the mean length of the two strings, and manually inspecting all records with rescaled distance > 0.1. All these cases are correct matches with high Levenshtein distances

due to switched name order (e.g. 'senior secondary school' versus 'secondary senior school') or abbreviations (e.g. 'secondary school' versus 'sec school'). This algorithm matches 95.6% of schools offering grade 12 to exam centers. Not all schools offering grade 12 are exam centers, as some schools combine with neighbors to administer exams. So the unmatched 4.4% of schools are not necessarily failures to merge. I show in Section 4 that the treatment effects on grade 12 enrollment do not differ between the full sample and the matched sample.

Fee-charging status: Each provincial education department publishes an annual list of schools required to eliminate fees. This list is distributed to schools and to local governments 3-4 months before the academic year starts. Each list shows the school name and EMIS number. Many province-year lists also show schools' socioeconomic quintiles and five of nine provinces show the continuous poverty scores used to select fee-eliminating schools in at least one year. I obtain physical copies of these lists from government records and an RA digitizes the records and matches them using EMIS numbers. I match 51% of the schools in the enrollment dataset in 2007 to the fee elimination lists, rising to 75% in 2010. These match rates are consistent with the government's target rate of fee elimination through time. I assume any school not observed in the fee elimination lists is a fee-charging school.

Other school characteristics: The Department of Basic Education maintains and publishes an annual 'masterlist' of school characteristics. I merge masterlists onto the enrollment data to obtain each school's GIS code, historical racial classification, rural/urban status, and per-student transfer provided by the provincial government. I match 99.1% of schools in the enrollment data to masterlist data, though GIS codes are missing for 2.6& of schools in the masterlists.

Census data: I obtain census data on local population and adult employment rates from Statistics South Africa's public release data from the 2001 and 2011 censuses. These data are available for census 'subplaces,' which are designed to contain roughly 13,000 people on average. An RA matches schools to subplaces using school GIS codes and subplace shape files. This successfully matches 96% of schools in the enrollment data to census characteristics.

A.2 Assessing Sensitivity to Measurement Error in Administrative Data

Enrollment data: Enrollment data is reported by schools and may be reported with error. Measurement error will lead to biased treatment effect estimates only if it is correlated with fee elimi-

nation, conditional on the fixed effects. This correlation may arise for two reasons. First, schools may simply report identical data for each year to minimize the effort of completing forms. If fee elimination changes outcomes, this will lead to attenuated treatment effect estimates. Second, schools may inflate enrollment numbers to receive more funding from provincial governments. If per-student transfers are larger for fee-eliminating schools, this will lead to upward-biased treatment effect estimates. I call these two types of measurement error respectively 'excess persistence' and 'overstatement.' In this section I implement tests for each of these two types of measurement error. I also implement one general test and one general robustness check for measurement error in the enrollment data. I do not implement these tests for graduation exam data, which is constructed by provinces from student-level exam results and is not subject to misreporting by schools.

To test for excess persistence, I examine the intertemporal persistence of enrollment reports. The grade persistence probability, the probability that a school reports identical enrollment for a grade-gender cell for two consecutive years, ranges from 3.5 to 5.6% and is generally higher in earlier grades (Table A.1 column 1). For three consecutive years, this probability ranges from 0.2 to 0.5% (column 2). These results are not consistent with widespread failures to update enrollment measures. However, enrollment reports should be naturally somewhat persistent, even with perfect reporting. In particular, school-by-gender enrollment in grade g in year y should be similar to enrollment in grade $g-1$ in year $t-1$, with equality if there are no grade repetitions, drop-outs, or transfers. The cohort persistence probability, the probability that a school reports identical enrollment for these two grade-year-gender cells, ranges from 1.7 to 6.7% and is monotonically decreasing with grade (Table A.1 column 4). The cohort persistence probability is 0.5-1 percentage points higher than the grade persistence probability in grade 9, when repetition and drop-out are lowest. The cohort persistence probability falls in higher grades and is 2-2.3 percentage points lower than the grade persistence probability by grade 12, when repetition and drop-out are highest. These results show that schools are not systematically more likely to report identical enrollment in consecutive years, relative to a measure of the natural persistence of enrollment through time. These patterns are similar in fee-charging and fee-eliminating schools. This suggests that excess persistence is not attenuating the treatment effects of fee elimination.[25]

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[25] More complex forms of excess persistence are possible. Schools might count enrollment in grade 8 in each year and report this, but then use the grade 8 enrollment value in year t for grade 9 enrollment in year $t+1$, etc. Schools might also randomly add or subtract a few students to the previous year's enrollment count. The tests I describe above do not capture these forms of excess persistence but could be adapted to do so.

Table A.1: Persistence of Enrollment Reports Through Time

	(1)	(2)	(3)	(4)
	Probability of reporting identical enrollment for			
		same grade for		same cohort for
	2 years	3 years	all years	2 years
Grade 8 female	0.0558	0.0051	0.0027	-
Grade 8 male	0.0539	0.0049	0.0025	-
Grade 9 female	0.0556	0.0050	0.0012	0.0666
Grade 9 male	0.0558	0.0049	0.0020	0.0607
Grade 10 female	0.0349	0.0022	0.0018	0.0296
Grade 10 male	0.0349	0.0023	0.0010	0.0287
Grade 11 female	0.0353	0.0024	0.0003	0.0267
Grade 11 male	0.0367	0.0024	0.0003	0.0216
Grade 12 female	0.0361	0.0021	0.0005	0.0167
Grade 12 male	0.0416	0.0028	0.0012	0.0185

Columns 1-3 of the table show the probability that schools report identical enrollment for each grade-gender cell for two consecutive years (column 1), three consecutive years (column 2), or all years (column 3). Column 4 shows the probability that schools report identical enrollment for each cohort for two consecutive years, by gender (i.e. for grade g in year t and grade $g-1$ in year $t-1$). Sample covers all public schools in 2003-2012 offering the relevant grade for the relevant gender, excluding those in high-income communities.

It is difficult to test directly for overstatement. But I can document two patterns in the data that are not entirely consistent with overstatement. First, fee elimination does not increase primary school enrollment. The effect on enrollment is actually negative in five of seven primary school grades, though the effects are generally small and the confidence intervals include zero. It is possible that overstatement occurs in secondary but not primary schools, but the absence of overstatement in primary grades does make overstatement in secondary grades less likely. The enrollment decrease in grade 12 also provides some evidence against systematic overstatement. But this evidence is weaker than the primary school result. It may be easier for schools to overstate enrollment in early grades than in grade 12, when provinces can observe the number of students registering for graduation examinations.

Second, I show that treatment effects are not larger when the financial return to overstatement is higher. I observe the per-student transfer for fee-eliminating schools, but not for fee-charging schools. I estimate the primary treatment effect model, equation (1), including an interaction between the fee elimination indicator and an indicator for receiving above-median per-student transfers. The estimated interaction effect is positive but very small (0.51, with standard error 2.25). Including the interaction term does not substantively change the direct effect of fee elimination

(from 10.08 to 9.60, with standard error 2.22). This result provides some evidence against systematic overstatement. However, the result should be interpreted with caution. Per-student transfers are larger for schools in lower SES quintiles, so treatment effect heterogeneity by the value of transfers conflates heterogeneity in incentives for overstatement with heterogeneity in treatment effects by neighborhood characteristics.

I also implement a general test for measurement error in the enrollment data using Benford's Law. Benford's Law is a statistical pattern in many datasets describing the shares of first significant digits (FSDs): highest for 1 and decreasing monotonically to 9. Deviations from Benford's Law are used as a measure of data quality and test for data manipulation in both auditing, fraud, and survey research.[26] I compare the distribution of first and second significant digits in my data to the distribution predicted under Benford's Law, separately for fee-charging and fee-eliminating schools. I follow Cho and Gaines (2007) by estimating the Euclidean distance between the distributions for the first and second digits:

$$d_1 = \sqrt{\sum_{j=1}^{9}\left(e_{j1} - \log_{10}\left(1 + \frac{1}{j}\right)\right)^2}$$

$$d_2 = \sqrt{\sum_{j=1}^{9}\left(e_{j2} - \sum_{k=0}^{9}\log_{10}\left(1 + \frac{1}{10k+j}\right)\right)^2}$$

where e_{ji} is the observed share of observations with i^{th} significant digit j. I normalize both d_1 and d_2 by their maximum possible values so the statistics are bounded by [0,1]. I estimate both statistics for all ten measures reported by schools: enrollment counts by gender for each of grades 8 to 12.

I find no evidence that fee elimination decreases data quality, as measured by the Euclidean distances d_1 or d_2. Using a purely time-series comparison, distance d_1 is smaller for post-elimination years in fee-eliminating schools than pre-elimination years for six of the ten enrollment measures. Using a panel comparison, distance d_1 is smaller for school-year observations without fees than with fees for nine of the ten enrollment measures. For the second digit distribution, fee elimination reduces d_2 for nine of ten measures in the time series and seven of ten measures in the panel.

Using Benford's Law as a benchmark, fee elimination does not decrease data quality and might

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[26]See Judge and Schechter (2009) for a review of developing country survey datasets against this benchmark and Garlick, Orkin, and Quinn (2019), Mahadevan (2018) and Schündeln (2018) for other applications to administrative and survey data.

actually increase it. An increase is possible if fee elimination leads to more active enrollment monitoring by schools or more active monitoring and auditing by provinces, whose payments to schools are now more closely linked to enrollment. However, the differences are small enough – 0.004 for d_1 and 0.02 for d_2, averaging across the ten enrollment measures – relative to the range of the underlying measures that I interpret these results as most consistent with unchanged data quality.

Finally, I show that the main findings are robust to removing to within-school outliers in the enrollment data. Manually inspecting the enrollment data shows some outliers, where enrollment within a school jumps or falls substantially for a single year. These may reflect data entry errors, though the rate of data entry errors need not be correlated with treatment status. First, I trim all school-year observations where enrollment is less than 50% or more than 150% of the school-specific mean over all other years. This trims between 8 and 16% of the school-year observations, with the highest rate of trimming in grade 12. The treatment effects on trimmed enrollment are slightly attenuated but qualitatively similar to the treatment effects on untrimmed enrollment (Table A.2 Panel A). Second, I trim all school-year observations where enrollment is less than 50% or more than 150% of the school-specific mean over the previous and next year. This trims between 6 and 14% of the school-year observations, plus all observations in the first and last years of the sample, for whom the moving average is undefined. The treatment effects on trimmed enrollment very similar to the treatment effects on untrimmed enrollment (Table A.2 Panel B).

Treatment assignment: I also show that the main results are robust to accounting for two possible measurement errors in the fee elimination data. First, I do not observe the government-issued fee elimination lists in 2011 and 2012 for one province, the Eastern Cape. In the main paper I assign these schools their fee elimination status from the 2010 list. This risks misclassifying some fee-eliminating schools as fee-charging. As a robustness check, I assign these schools their fee elimination status from the next observed list in 2014. This reassignment changes the fee-charging status of 279 school-year units (0.3% of the sample). This reassignment slightly attenuates the grade 8-10 enrollment effects by 0.2 to 0.8 students (Table A.3 Panel A). Second, two provinces issued conflicting instructions about fee elimination in two years. The Gauteng and the Western Cape provinces announced in government budget speeches that fees would be eliminated in quintile 3 schools in 2008. The official fee elimination lists did not reflect this decision until 2010. In

Table A.2: Treatment Effects on Enrollment Trimming Outliers

	(1)	(2)	(3)	(4)	(5)
	Grade 8	Grade 9	Grade 10	Grade 11	Grade 12
Panel A: Trimming observations > 50% away from the school-specific mean					
Fee elimination	4.35***	2.65***	5.47***	-0.58	-2.46***
	(0.52)	(0.55)	(0.89)	(0.74)	(0.52)
Counterfactual mean outcome	77	81	143	123	76
# school-year obs	77344	74427	50328	47656	42679
# schools	9426	8959	5867	5607	5526
Adjusted R2	0.926	0.928	0.904	0.884	0.853
Panel B: Trimming observations > 50% away from the school-specific moving average					
Fee elimination	7.01***	4.66***	6.07***	-0.49	-3.03***
	(0.62)	(0.63)	(0.97)	(0.84)	(0.67)
Counterfactual mean outcome	73	78	141	123	78
# school-year obs	61719	59321	40434	38466	34636
# schools	8999	8625	5676	5446	5334
Adjusted R2	0.909	0.913	0.891	0.866	0.808

Coefficients are from regressing each outcome on an indicator for fee elimination, school fixed effects, and year fixed effects. Sample covers all public schools in 2003-2012 excluding those in high-income communities. Sample sizes exclude singleton units within fixed effect groups. Heteroskedasticity-robust standard errors are shown in parentheses, clustering by school. Counterfactual mean outcome = pre-elimination mean outcome in fee-eliminating schools adjusted for aggregate time changes.

the main paper I define treatment using the official fee elimination lists. This risks misclassifying some fee-eliminating schools as fee-charging. As a robustness check, I redefine all quintile 3 schools in these provinces as fee-eliminating in 2008 and 2009. This reassignment slightly attenuates the enrollment and exam pass count effects by 0.5 to 1.5 students students (Table A.3 Panel B). As neither adjustment changes the qualitative findings in the main paper, I conclude that the findings are robust to these two sources of possible measurement error in treatment assignments.

B   From Effect Sizes to Elasticities

In the main paper I focus on effect sizes on enrollment counts and enrollment rates. In this appendix I provide a rough conversion of effect sizes into elasticities. In the framework from Section 5.1, the quality-uncompensated price elasticity of demand for enrollment as $\frac{\Delta\varepsilon(\Delta C,\Delta Q(\Delta C))}{\Delta C}$. If fee elimination shifts education quality, this differs from the quality-compensated price elasticity and the latter parameter is not identified by in my research design. Without parametric assumptions, only the local price elasticity over the observed change in cost is identified. Define this local,

Table A.3: Treatment Effects on Enrollment Using Alternative Treatment Definitions

	(1)	(2)	(3)	(4)	(5)	(6)
	Grade 8	Grade 9	Grade 10	Grade 11	Grade 12	# passes
Panel A: Replacing missing treatment values for one province with next year's values						
Fee elimination	5.93***	4.59***	5.90***	-1.24	-4.07***	-1.85***
	(0.64)	(0.64)	(1.00)	(0.84)	(0.69)	(0.43)
Counterfactual mean outcome	73	80	142	124	79	43
# school-year obs	84775	80874	53814	51470	50540	48813
# schools	9533	9042	5920	5650	5577	5225
Adjusted R2	0.883	0.891	0.870	0.839	0.745	0.815
Panel B: Replacing official treatment values for two provinces with information from budget speeches						
Fee elimination	5.11***	3.25***	5.54***	0.31	-2.61***	-1.13***
	(0.66)	(0.66)	(1.01)	(0.84)	(0.69)	(0.44)
Counterfactual mean outcome	74	81	143	122	78	42
# school-year obs	84775	80874	53814	51470	50540	48813
# schools	9533	9042	5920	5650	5577	5225
Adjusted R2	0.883	0.890	0.870	0.839	0.744	0.815

Coefficients are from regressing each outcome on an indicator for fee elimination, school fixed effects, and year fixed effects. Sample covers all public schools in 2003-2012 excluding those in high-income communities. Sample sizes exclude singleton units within fixed effect groups. Heteroskedasticity-robust standard errors are shown in parentheses, clustering by school. Counterfactual mean outcome = pre-elimination mean outcome in fee-eliminating schools adjusted for aggregate time changes.

quality-adjusted elasticity for grade g as:

$$\epsilon_g^L = \mathbb{E}\left[\frac{\Delta \varepsilon_{ig}\left(\Delta C_{ig}, \Delta Q_g\left(\Delta C_{ig}\right)\right)}{\Delta C_{ig}}\right], \tag{6}$$

where the expectation is taken over the distribution of individual- and school-level heterogeneity in the population affected by fee elimination.

To estimate ϵ_g^L, I would ideally observe individual level data on enrollment, education spending, and treatment status through time in a single, linked dataset. However, I instead observe three separate dataset, each containing some of the data needed to estimate the elasticity. Given these data limitations, the elasticity estimates should be interpreted with caution. First, I observe school-by-grade administrative data on enrollment and treatment status through time, which I use in Section 4 to estimate the numerator in equation (6). Second, I observe household-level data on pre-elimination fee and non-fee education spending from an Income and Expenditure Survey (IES) but cannot link this to individuals within the household or to treatment status. Third, I observe individual-level data on fee spending in bins and from a General Household Survey (GHS) but do not observe non-fee education spending or treatment status.

I use the IES and GHS data to approximate the denominator in equation (6), separately by grade and pooling all secondary school grades. In both datasets, I exclude households in the top quintile of the expenditure distribution to approximate the treatment-relevant population. In the IES, I do not observe individual enrollment so I exclude all households with zero fee and non-fee expenditure to approximate the enrolled population. In the GHS, I observe only bin-coded fee expenditure, so I fit a lognormal distribution to the bin frequencies to estimate mean fees by grade. In both datasets, I use post-stratification weights for all calculations.

Using these datasets, I show that the pre-elimination mean fee paid by enrolled students was USD25, this was 2.4% of total household expenditure and 27.8% of total education expenditure (Table A.4, Panels A and B). Hence fee elimination decreased enrollment costs by roughly 28% and increased enrollment by 3.2% (Panel C). This implies an elasticity of -0.11 for secondary school enrollment (Panel D). As a benchmark, Carneiro, Das, and Reis (2016) collect price elasticities for the margin between fee-charging private and free public schools in developing countries and find that all exceed 0.2 in absolute value.

Comparing administrative and survey data on fee payment shows that fee elimination lagged the intended schedule (Figure 3). This will attenuate the estimated elasticity. To account for slow compliance, I calculate the share of enrolled students in the General Household Survey who report paying zero fees in each year from 2007 to 2012. I then calculate the number of students who 'should have' paid zero fees in each year from 2007 to 2012 if their schools eliminated fees on schedule, using mean 2003-2006 to approximate school-level enrollment. I divide the former number by the latter and average over the six-year period to obtain a compliance rate of 83%. The compliance-adjusted elasticity is -0.14.

I can adapt this approach to estimate grade-specific elasticities. Consider grade 8 as an illustration. The mean pre-elimination fee was USD12 and fee elimination increased enrollment by 8.6%. I do not observe a grade-specific breakdown of fee versus non-fee education spending, so I make the strong assumption that non-fee education spending is equal in each grade of secondary school. This implies that fee elimination decreases education costs in the current year by 17%. If agents are myopic and consider only the change in costs in the current year, the elasticity is -0.52. If agents are forward-looking, then the present discounted change in costs over five years of secondary school is much larger. With a 90% discount factor, fee elimination reduces the cost secondary

Table A.4: Elasticities Implied by Fee Elimination Effects

	(1)	(2)	(3)	(4)	(5)	(6)
	All	Grade 8	Grade 9	Grade 10	Grade 11	Grade 12
Panel A: Distribution of Pre-Elimination Fees						
Share paying $\leqslant$ USD29	0.509	0.716	0.548	0.486	0.416	0.358
Share paying USD 29 - 57	0.293	0.157	0.281	0.315	0.350	0.372
Share paying USD 57 - 286	0.149	0.082	0.128	0.148	0.189	0.205
Share paying > USD 286	0.050	0.045	0.043	0.052	0.045	0.065
Estimated mean fee	25	12	22	27	32	37
Present value of mean fees	.	101	99	85	65	37
Panel B: Pre-Elimination Fee & Other Education Spending						
Fee share of total ed spending	0.278	0.165	0.267	0.302	0.341	0.373
Panel C: Treatment Effects on Enrollment						
% treatment effect on enrollment	0.032	0.086	0.060	0.048	-0.010	-0.052
Panel D: Elasticities						
Elasticity, myopic	-0.114	-0.520	-0.224	-0.158	0.030	0.139
Elasticity, forward-looking	.	-0.319	-0.200	-0.150	0.030	0.147
Panel E: Compliance-Adjusted Elasticities						
Treatment compliance rate	0.832	0.885	0.837	0.850	0.818	0.761
Elasticity, myopic	-0.137	-0.588	-0.268	-0.186	0.036	0.182
Elasticity, forward-looking	.	-0.361	-0.239	-0.176	0.037	0.193

Panel A shows the distribution of fees paid by enrolled grade 8-12 students, based on binned fee data, and the mean estimated from a log-normal fit on the bin counts. The present value in the final row is calculated from a discount factor of 10% and assumes enrollment in all subsequent grades. All panel A calculations using post-stratification-weighted data from the General Household Surveys in 2003-2006, excluding students from households in the top expenditure quintile. Fees are converted from South African Rands to 2005 USD values using 2005 purchasing power parity-adjusted exchange rates from the World Development Indicators. Panel B shows the mean household-level ratio of fee to non-fee spending using post-stratification-weighted data from the Income and Expenditure Survey in 2005/2006, excluding households in the top expenditure quintile and households with zero spending in all education categories. The grade-specific ratios are not observed in the data and assume that non-fee spending is identical in each grade. Panel C shows the treatment effects of fee elimination on enrollment in % terms, taken from Table 1. Panel D shows elasticities, calculated as the % treatment effect on enrollment from panel C divided by the % change in education spending from panel B. The first row shows a myopic elasticity, considering only the change in education spending in the current year. The second row shows an elasticity considering the change in the present discounted value of education spending, with annual discount factor 0.9. Panel E shows elasticities taking into account lagged compliance with the fee elimination policy. The first row shows the ratio of enrolled students in the General Household Survey who report paying zero fees divided by the number of students who would have paid zero fees if fee were eliminated according to the government schedule, aggregated over years 2007-2012. The second row shows the compliance-adjusted myopic elasticity, equal to the previous myopic elasticity divided by the compliance rate. The third row shows the compliance-adjusted forward-looking elasticity, equal to the previous forward-looking elasticity divided by the compliance rate. Standard errors are omitted because the uncertainty in these estimates is driven mainly by data limitations rather than sampling variation.

school for a prospective grade 8 student by USD101, versus the current-year cost of USD16. The forward-looking elasticity is -0.32. Adjusting for compliance increases these elasticities to -0.59 and -0.36 respectively. The elasticities in grades 9 and 10 are smaller because fees are a smaller share of education spending and fee elimination affects fewer years of secondary education.

As a benchmark, I estimate the myopic price elasticities of enrollment from Mexico's Progresa conditional cash transfer program using results from Schultz (2004). The elasticities for the first, second, and third years of secondary school (comparable to grades 7-9 in South Africa) are respectively -0.25, -0.14, and -0.13.

The elasticities in grades 11 and 12 are positive, because fee elimination reduces enrollment. However, these are grades where the quality adjustment apparently dominates the cost adjustment, so the elasticities are unlikely to be informative about price sensitivity.

These calculations should be interpreted with a high degree of caution, for many reasons. I cannot match individual-level enrollment, spending, and treatment status. Hence I use a rough approximation to the denominator of ϵ_g^L. I estimate a ratio using separate estimators for the numerator and denominator. I observe fee and non-fee spending only for enrolled students, which may be a poor guide to the latent spending facing non-enrolled students. I assume non-fee spending is equal in all grades of secondary school because I do not observe the fee to non-fee ratio by grade. I do not observe a comprehensive measure of school quality that I could use to estimate quality-compensated price elasticities. Hence the elasticity estimates may be biased and the sign of the bias is unclear. Nonetheless, it seems plausible that the quality-uncompensated elasticity of enrollment in grade 8 is relatively high, shrinking rapidly in higher grades.

C   Robustness to Alternative Research Designs

In this appendix I show that that the enrollment, graduation, and teacher:student ratio results in Sections 4 and 5 are robust to alternative conditioning variables, research designs, and outcome measures. In the main paper I compare outcomes between fee-charging and fee-eliminating schools conditional on school and year fixed effects:

$$Y_{st} = \alpha_s + \beta_t + \mathbf{1}\left\{\text{Fees} = 0\right\}_{st} \cdot \delta + \epsilon_{st} \tag{7}$$

This design recovers the effect of fee elimination assuming that any time-varying heterogeneity (e.g. time trends or transient shocks) is balanced across fee-charging and fee-eliminating schools. This is a strong assumption, as fee-charging and fee-eliminating schools are, by design, located in different communities. I present results from seven alternative designs in this appendix.

Alternative panel models: First, I condition on school-specific linear trends, $t \cdot \gamma_s$, as controls in equation (7). This accounts for school-specific time-varying heterogeneity, provided the heterogeneity follows a linear structure. Treatment effects on grade 8 and 12 enrollment and exam passes are almost identical to those in the main paper (Table A.5, Panel A). Treatment effects on grade 9, 10, and 11 enrollment and the teacher:student ratio respectively slightly decrease, increase, decrease, and increase. But no sign changes and the confidence intervals always overlap with those in the main paper.

Second, I use census data from 2001 and 2011 to construct linear trends in local population and socioeconomic status. I observe census data and shape files for census 'subplaces,' which are designed to contain roughly 13,000 people on average. I match schools to subplaces using GIS codes and condition on linear trends, $\text{Population}_{st} \cdot \gamma_{pop} + \text{EmploymentRate}_{st} \cdot \gamma_{emp}$, in equation (7). This accounts for locality-specific time-varying heterogeneity via population and socioeconomic status, again provided it follows a linear structure. Treatment effects on grade 8-12 enrollment, exam passes and the teacher:student ratio are almost identical to those in the main paper (Table A.5, Panel B). This robustness check comes with two caveats. First, the subplace-level measures are approximations to the precise local conditions facing schools, both because subplaces are relatively large and because schools draw some students from outside their subplace. Second, I cannot match 4% of schools to subplaces and exclude them from this robustness check.

Third, I condition on three time-invariant school characteristics in equation (7) interacted with time, $\sum_{i=j}^{3} X_s \cdot t \cdot \gamma_j$. Following Duflo (2001), this allows time trends to vary across province, urban/rural location, and schools' historical racial classification. All three characteristics may influence the time paths of schools' outcomes: provinces are responsible for education budgets and policy implementation, both population growth and economic development differed in rural and urban parts of South Africa during this period, and historical racial classifications influence the accumulated stock of resources available to schools. These controls account for non-linear time-varying heterogeneity in schools' outcomes, provided it is fully explained by these three char-

Table A.5: Treatment Effects with Alternative Conditioning Variables, Outcome Scalings, and Sample Definitions

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Gr. 8	Gr. 9	Gr. 10	Gr. 11	Gr. 12	# passes	Teachers per
			enrollment				100 students
Panel A: Conditioning on School-Specific Linear Trends							
Fee elimination	6.20***	6.08***	2.62***	-4.18***	-3.59***	-1.86***	-0.08***
	(0.61)	(0.63)	(1.00)	(0.89)	(0.78)	(0.38)	(0.02)
# school-year obs	84775	80874	53814	51470	50540	48813	92756
# schools	9533	9042	5920	5650	5577	5225	10317
Adjusted R2	0.916	0.925	0.912	0.886	0.791	0.864	0.581
Panel B: Conditioning on Linear Trends Interacted with Local Population and Employment							
Fee elimination	6.31***	4.92***	6.56***	-0.88	-4.11***	-1.72***	-0.05**
	(0.67)	(0.67)	(1.05)	(0.88)	(0.72)	(0.45)	(0.02)
# school-year obs	81861	78024	51291	49105	48248	46582	89645
# schools	9187	8707	5623	5371	5308	4973	9945
Adjusted R2	0.883	0.892	0.871	0.840	0.745	0.816	0.427
Panel C: Conditioning on Province, Rural/Urban & Racial Classification Interacted with Year FEs							
Fee elimination	2.71***	2.66***	3.95***	-0.28	-2.56***	-1.10**	-0.07***
	(0.63)	(0.66)	(1.05)	(0.91)	(0.73)	(0.44)	(0.02)
# school-year obs	84775	80874	53814	51470	50540	48813	92756
# schools	9533	9042	5920	5650	5577	5225	10317
Adjusted R2	0.888	0.895	0.877	0.847	0.755	0.824	0.463
Panel D: Instrumenting Fee Elimination with Initial Schedule							
Fee elimination	8.97***	5.28***	11.00***	-0.62	-5.95***	-3.08***	-0.07***
	(0.85)	(0.92)	(1.48)	(1.24)	(0.97)	(0.62)	(0.03)
# school-year obs	84775	80874	53814	51470	50540	48813	92756
# schools	9533	9042	5920	5650	5577	5225	10317
Adjusted R2	0.002	0.002	0.001	-0.000	0.001	0.000	-0.000
Panel E: Log-Transforming Outcomes							
Fee elimination	0.0376***	0.0402***	0.0271***	0.0040	-0.0179**	-0.0646***	-0.0073**
	(0.0053)	(0.0050)	(0.0056)	(0.0058)	(0.0073)	(0.0082)	(0.0035)
# school-year obs	84772	80871	53811	51469	50539	48164	92151
# schools	9532	9042	5919	5650	5577	5192	10305
Adjusted R2	0.897	0.912	0.894	0.865	0.769	0.786	0.507
Panel F: Balanced Panel							
Fee elimination	6.80***	4.54***	6.86***	-1.17	-3.97***	-1.76***	-0.05**
	(0.70)	(0.71)	(1.08)	(0.91)	(0.72)	(0.46)	(0.02)
# school-year obs	70695	69177	47061	45395	44739	44411	77600
# schools	7146	7035	4780	4632	4592	4575	7760
Adjusted R2	0.886	0.891	0.870	0.840	0.739	0.819	0.425

Coefficients are from regressing each outcome on an indicator for fee elimination, school fixed effects, year fixed effects, and panel-specific conditioning variables. Sample covers all public schools in 2003-2012 excluding those in high-income communities, except in panel F where schools closed/missing in any year are units. Kleibergen-Paap instrument strength tests in panel D exceed 2000 in all columns. Sample sizes exclude singleton units within fixed effect groups. Heteroskedasticity-robust standard errors are shown in parentheses, clustering by school. Counterfactual mean outcome = pre-elimination mean outcome in fee-eliminating schools adjusted for aggregate time changes.

acteristics. Treatment effects on grade 8-12 enrollment and exam passes are all attenuated relative to those in the main paper (Table A.5, Panel C). But no sign changes and the confidence intervals mostly overlap with those in the main paper.

Fourth, I instrument fee-charging status assigned by the provincial government with the elimination schedule announced by the national government: quintile 1 and 2 schools in 2007 and quintile 3 schools in 2010. This accounts for deviations from the original implementation schedule that are correlated with time-varying outcome heterogeneity. For example, schools might lobby to eliminate fees because they expect falling enrollment. This would assign schools with systematically lower enrollment trends to eliminate fees. Using the original implementation schedule to instrument for fee elimination avoids this problem. Treatment effects on enrollment in most grades, exam passes, and the teacher:student ratio increase slightly in absolute value (Table A.5, Panel D). But no sign changes and the confidence intervals mostly overlap with those in the main paper. The instrument shifts the probability of fee elimination by 81.6 percentage points (standard error 0.3 percentage points) from a base of 4.8 percentage points, so weak instrument issues are not relevant.

Fifth, I estimate equation (7) replacing Y_{st} with $ln(Y_{st})$. The assumption of balanced time-varying heterogeneity may hold for some but not all outcome scalings. This robustness check assesses if the estimated treatment effects are sensitive to an alternative outcome scaling. The log transformation is particularly salient because most outcomes have positive/right skew. Treatment effects on all log outcomes have the same signs as treatment effects on the corresponding level outcomes (Table A.5, Panel E). The log estimates are smaller in absolute value than the level estimates expressed as percentages of counterfactual means for all outcomes except exam passes. But the confidence intervals in percentage points generally overlap.

Sixth, I estimate equation (7) on a balanced panel of schools observed in all years from 2003-2012. 23% of schools are missing at least one annual observation, due to school closings, school openings, or missing administrative data. Missingness is uncorrelated with fee elimination status, conditional on school fixed effects. The treatment effects from the balanced panel sample are almost identical to those from the full sample (Table A.5, Panel F).

These results show that the key treatment effects – higher enrollment in early grades, lower enrollment in grade 12, fewer exam passes, and lower teacher:student ratios – are all qualitatively robust across different conditioning variables, outcome scalings, and sample definitions. This pro-

vides reassurance that the findings are not driven by time-varying heterogeneity that is unbalanced across fee-eliminating and fee-charging schools. It is possible that there is some time-varying heterogeneity not covered by these checks. In particular, these checks do not account for nonlinear time trends or transient shocks that differ across fee-charging and fee-eliminating schools within the same province, urban/rural classification, and historical racial classification.

Regression discontinuity model: Seventh, I estimate treatment effects of fee elimination using a regression discontinuity. As discussed in Section 3, school-level fee elimination was determined partly by a continuous poverty score assigned to each school. Provincial governments assigned a poverty score to each school based partly on census data. National government then assigned schools to national quintiles based on these scores. Provincial governments were encouraged to eliminate fees in quintile 1 and 2 schools in 2007. This policy design means that schools just above and below the threshold poverty score between quintiles 2 and 3 should have discontinously higher and lower probabilities of treatment.

The discontinuity design recovers fee elimination effects without any of the restrictions on time-varying school-level heterogeneity assumed by the panel methods discussed above. However, this method has five limitations in this setting. First, treatment effects estimated by the regression discontinuity design are driven by schools near the threshold between quintiles 2 and 3. This provides little information on treatment effects in very poor schools. Second, I observe the poverty scores for only five of South Africa's nine provinces – Eastern Cape, Gauteng, KwaZulu-Natal, Northern Cape, and Western Cape. These five provinces are not representative of South Africa's population, with higher levels of urbanization, more diverse racial composition, and more high-income neighborhoods. Third, I do not observe the threshold values separating quintiles. I estimate province-specific threshold values following Card, Mas, and Rothstein (2008). Results reported below are robust to small changes in these estimated thresholds, but this does introduce potential error in the analysis. Fourth, different provinces appear to have used the poverty scores differently in assigning quintiles and fee elimination. The probability of fee elimination is increasing in the poverty score in all five provinces. But the province-specific discontinuities in the probability of fee elmination at the threshold have a wide range. Fifth, each province calculated poverty scores independently and they have completely different distributions. I recenter each province-specific distribution so the score at the estimated cutoffs equal zero and the score range equals one. This

Table A.6: Fee Elimination Effects at Poverty Score Cutoffs

	(1)	(2)	(3)	(4)	(5)
Estimator	Fixed effects		Discontinuity		
Panel A: Effects on Fee Elimination					
Treatment effect		0.547	0.526	0.588	0.539
		(0.038)	(0.043)	(0.032)	(0.036)
Effective sample size		1550	1235	2196	1720
Panel B: Effects on Enrollment					
Treatment effect	9.95	7.16	4.94	6.73	6.46
	(1.69)	(4.94)	(5.57)	(4.58)	(4.65)
Effective sample size	11642	1902	1327	2299	1967
Panel C: Effects on Exam Passes					
Treatment effect	-0.64	-0.97	-1.30	-0.80	-1.00
	(0.83)	(2.36)	(2.63)	(2.17)	(2.58)
Effective sample size	5208	1312	949	1663	1007
Running variable scale	-		Range = 1		Std dev. = 1
Bandwidth scale	-	1	0.75	1.25	1

Table reports estimated differences in outcomes for schools on either side of poverty score cutoffs in 2007. Top panel reports differences in probability of eliminating fees in 2007. Middle and bottom panels report differences in the changes from 2006 to 2007 in respectively total enrollment and total exam passes. Columns 2 - 4 report estimates with poverty scores standardized to have the same range in each province. Column 5 reports estimates with poverty scores standardized to have the same standard deviation in each province. Columns 2 and 5 use the default bandwidth estimates from Calonico, Cattaneo, and Titiunik (2014), imposing equal bandwidths on either side of the cutoff. Columns 3 and 4 use the bandwidth from column 2 multiplied by respectively 0.75 and 1.25. Effective sample sizes are those within the estimated bandwidth. As a benchmark, column 1 reports fixed effects estimates using only 2006 and 2007 data for the five provinces for which poverty scores are available. Standard errors in column 1 are heteroskedasticity-robust and clustered by school. Standard errors in columns 2-4 use the default heteroskedasticity-robust nearest neighbor estimator from Calonico, Cattaneo, and Titiunik (2014), as these estimates use only one observation per school.

yields more comparable scores but adds potential error relative to a setting where the scores are designed to be comparable. These factors together mean that the treatment effects use relatively little data and are imprecisely estimated and less robust than the estimates based on panel methods.

I show in Table A.6 that the regression discontinuity results are broadly consistent with the panel results. Schools just above the poverty score threshold are 55 percentage points more likely to eliminate fees in 2007 (top panel, column 2). Total enrollment in these schools increases from 2006 to 2007 by 7.2 more students (middle panel, column 2). Exam passes in these schools decrease by 1 student more (bottom panel, column 2). Using threshold crossing as an instrument for fee elimination increases these estimates to respectively 10.8 and -2.1 students. As a benchmark, I report treatment effects from estimating equation (7) using data from the same five provinces

for 2006 and 2007 (column 1). The panel and regression discontinuity estimates have the same signs and the former estimates fall within latter estimates' confidence intervals. But the regression discontinuity estimates' confidence intervals are roughly three times wider and include zero.

The regression discontinuity estimates' signs and magnitudes are robust to decreasing the bandwidth (column 3), increasing the bandwidth (column 4), and rescaling the province-specific distributions to have the same standard deviation rather than the same range (column 5). I do not report grade-specific enrollment effects, as these using even smaller samples and are less robust. Subject to this caveat, they replicate the general pattern that enrollment effects are positive in early grades and negative in grade 12.

These findings show that fee elimination has broadly similar effects across panel and regression discontinuity methods. This may show that panel methods' assumptions about time-varying heterogeneity are approximately correct in this setting. However, the panel and regression discontinuity methods recover different treatment effects under their assumptions. Panel methods recover a weighted average of school-specific fee elimination effects across all fee-eliminating schools. Regression discontinuity methods recover a weighted average of school-specific fee elimination effects across schools near the threshold. It is possible to propose economic models in which the estimates coincide because unmodeled time trends are higher in high-poverty neighborhoods while treatment effects are higher in low-poverty neighborhoods, or vice versa. However, the relative stability of the enrollment and graduation results across the main panel design and the seven alternative designs presented here seems reassuring.

Alternative measures of exam performance: In the main paper I report only treatment effects on the number and share of enrolled grade 12 students who pass the exam. This does not capture any effects of fee elimination on the distribution of student-level performance above or below the passing threshold. For 2003-2010 I also observe the number of students attaining a pass that qualifies them for admission to universities. Only 12% of grade 12 students in the schools I study attained this pass, compared to 56% for the basic pass. Treatment effects on both pass counts for 2003-2010 are similar: -2.4 (standard error 0.4) for the basic pass and -2.6 (standard error 0.2) for the higher pass. This shows that the negative effect of fee elimination on exam passes was not concentrated on a narrow portion of the latent achievement distribution. I can decompose the change in the high pass count into learning and composition effects, using the same approach as

in Section 5.3. Under monotonicity, the composition effect is bounded between -3.8 students (the effect on grade 12 enrollment for 2003-2010) and 0 students. This implies that the learning effect shifted between -2.5 and 1.6 students from missing to achieving high passes, equivalent to changing the pass rate by -3.3 to 1.6 percentage points from a 12% base. These are consistent with larger learning effects at the high pass than pass margin.

Correlated policy changes: There was one large education policy change that occurred in South Africa at roughly the same time as fee elimination. The high school graduation exam was restructured between 2007 and 2008 to add a seventh mandatory subject, change the set of subjects offered, and eliminate the option to take subjects at varying levels of difficulty. The mean exam pass count after the change in exam structure fell slightly from 49.6 to 47.8 students per school. This change occurred during the fee elimination period and may confound the effect of fee elimination on exam passes. The effect is confounded if the change in exam structure differentially affected exam pass counts in fee eliminating and fee charging schools. This hypothesis is not testable. However, I can test part of this hypothesis using the schools that had not yet eliminated fees in 2008. I divide schools into six groups: those that eliminate fees in 2007, 2008, 2009, 2010, and 2011, and 2012. The latter four groups had not eliminated fees when the exam structure changed in 2008. If the exam structure change differentially affected schools based on their fee elimination timing, then the changes in pass counts from 2007 to 2008 should differ across these four groups. I cannot reject joint equality of the mean changes across the four groups ($p = 0.246$). This shows that change in exam structure did not differentially affect exam pass counts across schools with different fee elimination dates, though it does not rule out all possible forms of confounding.

I am not aware of changes in other national policies at this time. National grade progression policies changed in 1998 and 2013 but were constant from 2003 to 2012. A national school meal program was rolled out between 2009 and 2011 but the incidence differed from the fee elimination policy and implementation was limited. Any changes in provincial policies that co-incided with fee elimination should be captured by the province-year fixed effects specification I report above.

D   Robustness to Alternative Proximity Definitions in Spillover Analysis

In this appendix I show that that the spillover results in Section 6 are robust to alternative measures of proximity to fee-eliminating schools. The spillover analysis is motivated by the prospect of

students transfering between schools. For any fee-charging school s, outcomes may depend on the enrollment cost and quality of all other schools that students might attend instead of s: $\mathcal{S}_s$. In particular, outcomes at s may depend on the fee-charging status of all schools in $\mathcal{S}_s$. I do not directly observe the set $\mathcal{S}_s$ and instead construct proxies based on proximity to s. In the paper I use the number of fee-eliminating schools within a 5 kilometer radius as a proxy for fee-charging status in $\mathcal{S}_s$. I find a small negative relationship for grade 8 enrollment and precisely estimated zero relationships for enrollment in other grades and exam passes. However, the estimated relationships may be biased due to measurement error in the proxy. Classical measurement is perhaps most likely, leading to attenuated estimates of spillover effect. But the measurement error may be correlated with enrollment if the relationship between my proxy and the latent measure of interest is systematically different across schools with larger versus smaller enrollment (e.g. in urban versus rural schools).

In this appendix I estimate the relationship between enrollment and exam passes and four alternative proxy measures:

- An indicator for having any fee-eliminating school within a 5 kilometer radius (mean = 0.49).

- The number of fee-eliminating schools within a 3 kilometer radius (mean = 0.6, interdecile range = [0,2]).

- The number of fee-eliminating schools within a 10 kilometer radius (mean = 4.7, interdecile range = [0,14]).

- The inverse distance in kilometers to the nearest fee-eliminating school (mean = 0.21, interdecile range = [0.05,0.44]).

I show in Table A.7 that there are few large spillover estimates for any measure and none are robust across measures. For most measures, the largest spillover estimate is for grade 8. But the largest effect of moving from the 10th to the 90th percentile of any proximity measure is to lower grade 8 enrollment by 3.5 students, equal to 2.6% of the mean (Panel D, Column 1). But this effect is imprecisely estimated and has a confidence interval including zero. Turning on the binary proximity measure increases grade 11 enrollment by 3.6 students (Panel A, Column 4) but no other measure produces a large or statistically significant spillover estimate for grade 11. No measure generates a large spillover effect for grade 9, 10 or 12 enrollment or for exam passes.

Table A.7: Spillover Effects on Enrollment and Exam Passes with Different Measures of Proximity to Fee-Eliminating Schools.

	(1) Grade 8	(2) Grade 9	(3) Grade 10 enrollment	(4) Grade 11	(5) Grade 12	(6) # passes
Panel A: Any School Within 5km						
Proximity	-0.532	-2.400	-3.363	-2.224	3.059	0.316
	(2.551)	(2.519)	(3.442)	(2.593)	(2.448)	(1.630)
. . . × post-elimination	-1.937	-0.075	1.720	3.627**	0.241	0.176
	(1.491)	(1.576)	(1.932)	(1.802)	(1.768)	(0.932)
Mean outcome	138.87	148.72	213.70	177.75	112.86	69.81
Mean regressor	0.6	0.5	0.5	0.5	0.5	0.5
# school-year obs	7242	6812	5593	5455	5352	5275
# schools	2838	2666	2135	2075	2039	2009
Adjusted R2	0.878	0.892	0.902	0.885	0.783	0.845
Panel B: # Schools Within 3km						
Proximity	-2.656***	-3.163***	-2.627***	-0.901	0.952	-0.431
	(0.638)	(0.712)	(0.916)	(0.957)	(0.734)	(0.477)
. . . × post-elimination	-1.499**	-0.024	-0.394	-0.503	-0.694	-0.443
	(0.641)	(0.753)	(0.733)	(0.728)	(0.648)	(0.405)
Mean outcome	138.87	148.72	213.70	177.75	112.86	69.81
Mean regressor	0.8	0.7	0.6	0.6	0.6	0.5
# school-year obs	7242	6812	5593	5455	5352	5275
# schools	2838	2666	2135	2075	2039	2009
Adjusted R2	0.880	0.893	0.902	0.885	0.783	0.845
Panel C: # Schools Within 10km						
Proximity	-0.508**	-1.013***	-1.360***	-0.443*	0.001	-0.089
	(0.238)	(0.240)	(0.347)	(0.245)	(0.238)	(0.149)
. . . × post-elimination	0.060	0.100	-0.118	-0.145	-0.135	0.053
	(0.117)	(0.127)	(0.215)	(0.182)	(0.166)	(0.101)
Mean outcome	138.87	148.72	213.70	177.75	112.86	69.81
Mean regressor	5.4	5.3	4.1	3.9	3.9	3.7
# school-year obs	7242	6812	5593	5455	5352	5275
# schools	2838	2666	2135	2075	2039	2009
Adjusted R2	0.878	0.893	0.903	0.885	0.783	0.845
Panel D: Inverse Kilometres to Nearest School						
Proximity	-18.027***	-20.835***	-22.608**	-1.450	9.368	-1.913
	(6.971)	(8.058)	(11.096)	(7.966)	(8.266)	(4.757)
. . . × post-elimination	-8.938	1.218	-0.357	-2.981	-0.199	1.459
	(5.661)	(5.707)	(6.699)	(6.549)	(6.158)	(3.029)
Mean outcome	138.87	148.72	213.70	177.75	112.86	69.81
Mean regressor	0.2	0.2	0.2	0.2	0.2	0.2
# school-year obs	7242	6812	5593	5455	5352	5275
# schools	2838	2666	2135	2075	2039	2009
Adjusted R2	0.878	0.893	0.902	0.885	0.783	0.845
Panel E: # Schools Within 5km, All Public Schools						
Proximity	-1.354***	-1.335***	-2.049***	-1.244***	-0.198	-0.732***
	(0.314)	(0.335)	(0.513)	(0.417)	(0.394)	(0.232)
. . . × post-elimination	-0.292	0.093	-0.415	-0.318	-0.520*	0.022
	(0.248)	(0.273)	(0.361)	(0.308)	(0.314)	(0.194)
Mean outcome	146.67	155.92	203.13	171.51	118.15	84.86
Mean regressor	1.7	1.6	1.2	1.2	1.2	1.1
# school-year obs	10433	9901	8553	8404	8014	8104
# schools	3682	3480	2909	2846	2738	2756
Adjusted R2	0.880	0.900	0.905	0.892	0.817	0.893

Coefficients are from regressing each outcome on the number of fee-eliminating schools within 5km, its interaction with an indicator for 2007 or 2010, school fixed effects, and year fixed effects. Sample covers all fee-charging public schools in 2006-2007 and 2009-10 excluding those in high-income communities. Sample sizes exclude singleton units within fixed effect groups. Heteroskedasticity-robust standard errors are shown in parentheses, clustering by school.

For all measures except binary proximity, higher local density of fee-eliminating schools is associated with lower enrollment in the early grades of secondary school (first row of each panel in Table A.7). This relationship shows there is some scope for neighboring schools to cut into each others' enrollment. However, fee elimination does not substantially change this relationship.

The preceding spillover analysis is restricted to fee-charging schools in my primary analysis sample. This excludes public schools in the highest SES quintile, which constitute roughly 30% of the fee-charging schools in the sample period. To test if these schools are affected by spillovers from fee-elimination, I replicate the spillover analysis in Section 6 with these schools included. Results are very similar with or without these schools included (Table A.7, Panel E). This reinforces the general evidence base against spillovers.

E   Supplementary Analysis Using Household Survey Data

In this appendix I show that the main results from administrative data analysis are also visible in two household surveys.

Student-level panel data: South Africa's National Income Dynamics Study (NIDS) is a nationally representative household panel survey conducted in every second year since 2008. This survey measures school enrollment and grade attainment for all household members and education spending and distance traveled to school for all enrolled household members. I can construct measures of grade progression and high school graduation from the enrollment and attainment data. I use household GIS codes from the restricted access data to calculate straight-line distance to all schools. I then generate a measure of 'exposure to fee-eliminating schools' in the 2008, 2010, and 2012 waves: the weighted average of fee elimination status for all schools within a 5km radius, with weights equal to the inverse straight-line distance to school.

I estimate models of the form

$$Y_{iht} = \text{Exposure}_{ht} \cdot \beta + \mathbf{X}_{iht} \cdot \Gamma + \epsilon_{iht} \tag{8}$$

where i, h, and t index respectively individuals, households, and years. $\mathbf{X}_{iht}$ is a vector of individual age, household income, highest educational attainment of an adult in the household, and a constant term. β is the change in outcome Y_{iht} associated with a change in the share of nearby fee-eliminating schools from 0 to 1.

Table A.8: Treatment Effects using Household Survey Data

	(1)	(2)	(3)	(4)
	Enrolled	Graduated/Progressed	Fees Paid	Distance to School
Exposure to fee-	0.069***	0.011	-193***	17.8
eliminating schools	(0.012)	(0.018)	(47)	(18.5)
Mean outcome	0.658	0.762	234	47.85
# respondents	12246	8375	7050	6828

Coefficients are from respondent-level regressions of each outcome on the fraction of schools within 5 kilometers of the respondents' home that have eliminated fees, weighted by the inverse distance to each school. Regressions condition on respondent age, total household income, and highest educational attainment by an adult in the household. Regressions use respondent-level post-stratification weights supplied by survey designer. Sample in column (1) covers all respondents aged 5-24 who were grade-eligible to enroll in secondary school in the current year. Sample in column (2) covers all respondents aged 5-24 who were enrolled in secondary school in the previous year. Sample in columns (3) and (4) covers all respondents aged 5-24 enrolled in secondary school in the current year with non-missing outcome values. Outcome in column (3) is measured in USD, converted from South African Rands using the 2010 purchasing power parity exchange rate. Outcome in column (4) is measured in kilometers. Heteroskedasticity-robust standard errors clustered by household in parentheses.

Table A.8 report estimates of β for different education outcomes and shows three patterns. First, exposure to fee-eliminating schools is associated with higher enrollment but not with higher grade progression or graduation (columns 1 and 2). These patterns are consistent with the enrollent and graduation effects in Section 4. The enrollment result is higher for students in the early grades of secondary school but the grade-specific estimates are generally not statistically significantly different to each other. The household survey data should not suffer from the misreporting concerns about the enrollment data. So the similarity of the results from administrative and survey data is reassuring. Second, exposure to fee eliminating schools is associated with a drop in fee payments, equal to 83% of the sample mean (column 3). This provides reassuring evidence that fees were indeed eliminated in schools assigned to eliminate them.

Third, exposure to fee elimination does not change the distance students travel to school (column 4). The large point estimate and standard error are driven by students who travel very long distances to boarding schools. Winsorizing these outliers or log-transforming the outcome yields smaller and more precisely estimated coefficients with confidence intervals that still cover zero. This is consistent with the lack of spillover effects in Section 6: if fee elimination led to transfers between schools, this could change the distance students traveled to school. All three patterns are robust to changing the radius to 3 or 10 kilometers, though the relationship between exposure and fees paid is attenuated with a 3km radius.[27]

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[27]The sample sizes differ for each outcome because progression/graduation, spending and distance are only observed

However, these results should be interpreted with caution, as the timing of the NIDS surveys is not ideal for this analysis. The panel starts in 2008, the second year of the fee elimination policy. By 2008, Exposure_{ht} is already 0.37, rising to 0.75 in 2012. I cannot condition on pre-elimination education measures or use a difference-in-differences analysis. This means that β is identified by both within-neighborhood changes schools' fee-charging status and across-neighborhood variation. The latter variation is likely to be correlated with unobserved neighborhood socioeconomic status, even after I condition on household SES. These omitted variables will bias estimates of β, most likely downward. Conditioning on individual or household fixed effects in equation (8) discards most of the variation in Exposure_{ht}, generating imprecise estimates of β. Given these caveats, I interpret the NIDS results as providing positive but weak support for the findings in the main paper.

Repeated student-level cross-sectional data: South Africa's General Household Survey (GHS) is an annual nationally representative cross-section of roughly 25,000. This survey measures grade attainment, enrollment, and fee expenditure (in bins) for all household members and total household expenditure (in bins). I use these data for several purposes. First, I estimate the age- and grade-specific enrollment rates and age-specific attainment rates reported in Section 2. Second, I estimate grade-by-year rates of fee elimination from the share of households paying zero fees for Section 3. Third, I estimate grade-specific baseline enrollment rates to convert effects on enrollment counts in Section 4 into effects on enrollment rates. The GHS does not contain geographic identifiers below the province level and does not match students to schools, so I cannot estimate households' exposure to the fee elimination level. Throughout the paper I focus on students in households in the bottom four quintiles of the total expenditure distribution, after applying post-stratification weights, to approximate the population likely to be exposed to fee elimination. This approximation is crude but will contain some information, given the geographic targeting of fee elimination and South Africa's income-segregated residential patterns.

In this appendix I show that the time trend in enrollment in the 'treated' population in the GHS roughly matches the enrollment effects estimated using administrative data. I estimate pre-

conditional on enrollment. There are also some missing values for spending and distance. Approximately 5% of respondents have no schools within 5 kilometers of their home. I set Exposure_{ht} to missing for these respondents and exclude them from the analysis. Extending the radius to 10 kilometers includes most of these respondents in the sample and yields similar results.

Table A.9: Comparing Pre- and Post-Elimination Enrollment Rates in Household Survey Data

| | (1) | (2) | (3) | (4) | (5) |
	Grade 8	Grade 9	Grade 10	Grade 11	Grade 12
Panel A: Time-Series Comparisons Using Household Survey Data					
Baseline enrollment rate	83.54	75.96	70.42	68.25	63.92
	(0.47)	(0.53)	(0.59)	(0.55)	(0.61)
Pctage point change in enrollment rate	5.24	5.39	5.93	1.36	-2.69
	(0.69)	(0.80)	(0.83)	(0.81)	(0.88)
% change in enrollment	6.27	7.09	8.42	2.00	-4.21
	(0.85)	(1.09)	(1.23)	(1.20)	(1.35)
Panel B: Panel Estimates Using Administrative Data					
% change in enrollment	8.57	5.98	4.76	-1.01	-5.17
	(0.89)	(0.82)	(0.72)	(0.69)	(0.87)

Panel A of the table shows the enrollment rate in each grade for the population aged 6-24 who have completed grade $g - 1$ and live in households outside the top income quintile. The first row shows the enrollment rate in 2003-2006, the four years immediately before the start of the fee elimination policy. The third row shows the change in the enrollment rate from 2003-2006 to 2010-2012, after more than 50% of schools had eliminated fees. The fifth row shows the change as a percentage of the baseline enrollment rate. All calculations in panel A use individual-level post-stratification weights constructed by Statistics South Africa, normalized to sum to one in each survey year, and show heteroskedasticity-robust standard errors in parentheses. Panel B of the table repeats the treatment effects on grade-specific enrollment in percentage terms from the panel analysis of administrative data in Table 1. Standard errors shown in parentheses in panel B are heteroskedasticity-robust and clustered by school.

treatment enrollment in grade g as the enrollment rate in 2003-2006 for the population aged 6-24 who have completed grade $g - 1$ and live in households outside the top income quintile. I estimate post-treatment enrollment using data from 2010-2012 with the same sample restrictions. Given some schools eliminate fees after 2010, this will slightly understate the full baseline-endline difference. In Table A.9 I report the baseline enrollment rates (rows 1-2), time change in enrollment rate (rows 3-4), and percentage change in enrollment rate (rows 5-6) all using GHS data. I also report the percentage change in enrollment from the panel model estimated on administrative data (rows 7-8), taken directly from Table 1.

Results are similar for the administrative panel analysis and household survey time-series analysis: substantial positive effects on enrollment in grades 8-10, a substantial negative effect on enrollment in grade 12, and a small and statistically insignificant effect on enrollment in grade 11. The time changes in household survey data are robust to restricting the age sample to 6-21, though the baseline enrollment rates rise slightly. The noisy definition of the treated sample in the household survey data means that this finding should be interpreted with caution. I interpret this finding as weak but positive support for findings in the main paper.

F Additional Results

This appendix displays additional results discussed briefly in the main text.

Table A.10: Treatment Effects on Enrollment by Grade and Year Relative to Treatment

	(1)	(2)	(3)	(4)	(5)	(6)
	Grade 8	Grade 9	Grade 10	Grade 11	Grade 12	# passes
year1	6.44***	2.31***	2.42***	-1.99**	-2.00***	-0.39
	(0.64)	(0.65)	(0.93)	(0.83)	(0.76)	(0.44)
year2	7.25***	5.73***	7.24***	-0.79	-5.35***	-3.03***
	(0.75)	(0.77)	(1.20)	(1.00)	(0.83)	(0.52)
year3	4.55***	6.31***	13.56***	1.82	-4.60***	-1.71***
	(0.85)	(0.84)	(1.42)	(1.15)	(0.90)	(0.58)
year4	8.40***	2.97***	17.21***	8.56***	-3.74***	-1.59**
	(1.11)	(1.13)	(1.81)	(1.50)	(1.16)	(0.76)
year5	9.51***	2.50*	16.56***	9.61***	-1.42	-0.11
	(1.24)	(1.30)	(2.15)	(1.77)	(1.29)	(0.89)
year6	9.56***	0.02	18.54***	11.15***	-2.03	-1.63
	(1.40)	(1.49)	(2.52)	(2.09)	(1.49)	(1.03)
p:effects in years 1-3 equal	0.000	0.000	0.000	0.000	0.000	0.000
p:effects in all years equal	0.000	0.000	0.000	0.000	0.000	0.000
Counterfactual mean outcome	72	81	138	119	79	42
# school-year obs	84775	80874	53814	51470	50540	48813
# schools	9533	9042	5920	5650	5577	5225
Adjusted R2	0.883	0.891	0.871	0.840	0.745	0.816

Coefficients are from regressing each outcome on indicators for 1, ... 6 years after treatment; school fixed effects; and year fixed effects. Sample covers all public schools in 2003-2012 excluding those in high-income communities. Sample sizes exclude singleton units within fixed effect groups. Heteroskedasticity-robust standard errors are shown in parentheses, clustering by school. Counterfactual mean outcome = pre-elimination mean outcome in fee-eliminating schools adjusted for aggregate time changes. The numbers of school-year units observed 1, 2, 3, 4, 5, and 6 years after treatment are respectively 8212, 7963, 7701, 5270, 5039, and 4614.