School fees and student enrollment in post-*apartheid* South Africa*

Robert Garlick†

August 17, 2011

VERY PRELIMINARY AND INCOMPLETE. PLEASE DO NOT CITE OR CIRCULATE WITHOUT THE AUTHOR'S PERMISSION.

Abstract

I study a unique school fee reform in South Africa, in which tuition fees were abolished in a subset of public schools between 2006 and 2007. The fact that schools were designated for "no fee" status based in part on independently measured historical characteristics of the electoral wards in which they were located limits the scope for manipulation of assignment. I employ a difference-in-differences strategy that contrasts changes in enrollment at no fee schools and schools that continue to charge fees. Intention-to-treat estimates suggest that the policy increased enrollment by approximately 0.7%, with most of the change occurring in high schools. These effects are large relative to historical variation in South African enrollment. I argue that this constitutes some of the cleanest causal evidence on the effect of tuition fees on enrollment in the development literature. This design improves on prior studies that typically rely on nationwide policy changes and so cannnot isolate the effect of the policy change from secular trends in enrollment.

*I thank John Bound, Brian Jacob, David Lam, Jeffrey Smith and seminar participants at the University of Michigan for helpful comments. I thank Christo Lombaard, Hersheela Narsee, and Siza Shongwe from the Department of Basic Education for invaluable assistance in obtaining the data used in this study. All errors remain my own.

†PhD candidate in economics and public policy, University of Michigan; rgarlick@umich.edu

1 Introduction

Increasing school enrollment is an important goal for policy makers in many developing and developed countries. The second millenium development goal, adopted as part of the United Nations Millenium Declaration in 2000, commits countries to ensuring that all children complete a full course of primary education. Many national development plans and goals list increased access to education as an important target and a variety of interventions have been proposed to achieve this target.

Given this active interest by policy-makers and the general public, economists have invested considerable effort in evaluating interventions that aim to increase enrollment: The nature of these interventions has varied substantially across different contexts. In Latin America, where students typically pay low or zero school fees, many interventions have taken the form of conditional cash transfers paid to households whose age-eligible children are enrolled. These transfers are designed to offset the lost contributions to home production and labor market earnings from enrolled children (Schultz, 2004; Todd and Wolpin, 2006). In contexts where the capacity of the public schooling system is strained, interventions have created new private schools (Alderman, Kim, and Orazem, 1999) or reduced the cost of attending private schools (Angrist, Bettinger, Bloom, King, and Kremer, 2002). Other programs have used performance-based scholarships both to increase enrollment and provide incentives for increased student effort (Filmer and Schady, 2008; Kremer, Miguel, and Thornton, 2009).

In countries where tuition fees were historically common in public schools, a number of interventions have reduced or eliminated these fees. Primary school fees have recently been abolished in Madagascar (Fafchamps and Minten, 2007), Malawi (Al-Samarrai and Zaman, 2000), Kenya (Lucas and Mbiti, 2009), and Uganda (Deininger, 2003) and in each instance these abolitions have been accompanied by moderate to large increases in enrollment. However, rigorous evaluations of these policies have been hampered by the design of the interventions. In most instances the policies were implemented simultaneously across the entire country, making it impossible to separate the effect of the fee abolitions from secular trends in enrollment and the effect of other policy changes occurring at the same time. This raises doubts about whether the observed changes in enrollment are causally related to the abolition of school fees.

A small number of papers have employed research designs that at least partially address this concern. Fafchamps and Minten (2007) note that the abolition of fees in Madagascar was implemented at different times in different districts and that the variation in timing was driven by exposure to civil war, rather than differences in educational infrastructure or norms across districts. They suggest that this provides exogeneous variation in the timing of fee abolitions that allows late-abolition districts to be used as a control group for early-abolition districts. Deininger (2003) argues that Ugandan districts with different baseline characteristics were likely to be affected in different ways by fee abolitions and that this allows him to isolate their causal effect on enrollment. However, both of these studies rely on strong and untestable assumptions to interpret changes in enrollment as caused by fee abolitions. Perhaps the most convincing causal evidence comes from Barrera-Osorio, Linden, and Urquiola (2007), who study an interesting natural experiment in Bogota, Colombia. The city government reduced school fees for households whose socio-economic status fell below a threshold level, allowing the researchers to use a regression discontinuity design to compare enrollment in households on either side of the eligibility threshold for reduced fees. Under the relatively weak assumptions discussed by Hahn, Todd, and van der Klaauw (2001) and Lee and Lemieux (2010), this estimates the causal effect of school fee reductions on households with poverty scores near the eligibility threshold.

Despite these contributions, the stock of well-identified evidence on the causal effect of school fees on enrollment in developing countries is small. I contribute to this literature by studying a unique school fee reform in South Africa. All electoral wards in the country were ranked by a multi-dimensional index of social and economic deprivation, based on data from the 2001 Census. This allowed the education department to rank schools based on the electoral ward in which they were located. Schools were then divided into five groups, each of which contained approximately one fifth of the population of enrolled students at the time of the classification. Schools in the bottom two quintiles were then designated as "no fee" schools and from 2007 onward these schools were not permitted to levy compulsory school fees. This did not reduce the cost of enrollment to zero, as many households still paid for uniforms, stationary and transport. However, it substantially changed the cost of education for students enrolled in no fee schools. I use a difference-in-differences design that compares the change in enrollment between 2006 and 2007 for no fee schools to the change for schools that continued to charge fees. Under the assumption that the changes in enrollment in the absence of the no fee intervention would have been the same on average, this difference is the causal effect of school fee abolition of enrollment. In contrast, research designs that compare enrollment before and after nationwide school fee abolitions must assume that there would be *no change* in enrollment in the absence of the intervention. I argue that the research design I employ thus generates some of the most convincing evidence to date on the causal effect of education costs on enrollment.

In addition to the small literature from the developing world, there are several papers that study the enrollment effects of changes in the cost of education in the developed world. See Dynarski (2003), Dynarski, Gruber, and Li (2009), Kane (1994), and Seftor and Turner (2002) for examples and Neal (2002) for a discussion of the challenges faced by this research agenda. However, these research designs typically focus on margins different to those in the development literature: the choice between private and (free) public primary or secondary education and the choice between different types of postsecondary institutions with different costs. These are very different to the choice between fee-charging public and no primary or secondary education, which is perhaps the more relevant margin in much of the developing world. While these studies provide useful insights into the nature of education investment decisions, they do not reduce the need for better evidence on the effect of school fees on enrollment.

I explain the background of the South African policy experiment in more detail in section 2. Section 3 presents my research design, outlining the linear and nonlinear difference-in-differences estimation strategies I use and presenting the conditions under which they yield estimates of the effect of the no fee policy on enrollment. I then discuss the results in section 4, showing that there is strong evidence that the introduction of the no fee policy was associated with a clear rise in enrollment in affected schools. This association is robust to a variety of estimation strategies and measures of enrollment and is large relative to historical changes in enrollment.

Borkum (2011) also studies the South African school fee reform, using a similar research design. He finds that enrollment in new no fee schools rises between 2006 and 2007 by 2 percentage points more than enrollment in schools that still charge fees and interprets this as an effect of the new policy. This paper will, once complete, extend on Borkum's contribution by using a more comprehensive dataset, analyzing the heterogeneity of the treatment effect and developing a formal theoretical framework that uses the treatment effect to identify households' willingness to pay for education. In the current draft, however, my additional contribution is marginal relative to Borkum's existing work.

2 Background

Enrollment in South African schools rose rapidly through the latter part of the 20^{th} century, driven in large part by improved provision of education in black communities. Table 1 shows that by enrollment rates had stabilized by 2006, with approximately than 9 in 10 primary school age children and 2 in 3 secondary school age youths enrolled. However, some households reported that they faced challenges affording tuition fees and this was offered in some circles as an explanation for the fact that enrollment remained considerably below the 100% Pampallis (2008). This concern created political pressure for the creation of policies to reduce financial barriers to enrollment, including fee exemptions for learners from very low income households. This intervention was, however, considered to be insufficient and legislation passed in 2006 called for the national education department to bar selected schools from charging tuition fees.

The implementation of the no fee school policy was closely linked to an earlier policy to classify schools by the poverty rate of the surrounding community in order to facilitate progressive targeting of expenditure.[1] The methodology used to assign schools to socio-economic quintiles varied across provinces but was meant to be based on the socio-economic characteristics of the electoral ward (as established by the 2001 census) in which the school was located. I have not yet been able to establish the criteria employed by provinces in the early years of the system. From 2005 onward, the provincial education departments were supplied with an "index of multiple deprivation" (Noble, Babita, Barnes, Dibben, Magasela, Noble, Ntshongwana, Phillips, Rama, Roberts, Wright, and Zungu, 2006) that ranked all electoral wards by province on a weighted average of education, employment, health, income, and living environment. Provinces were encouraged to use this index to rank schools, though anonymous interviews by Wildeman (2008) with provincial education department officials suggest that provinces sometimes departed from the index where they felt it was based on outdated information or that there was substantial socio-economic variation within electoral wards.

After receiving school rankings from the provincs, the national education department, in consultation with the national treasury, assigned each school in the country to a quintile. The assignments were intended to ensure that the wards with comparable levels of poverty across provinces were assigned to the same national quintiles Wildeman (2008), though I have not yet been able to establish the exact formulae used in this process. I am still in the process of creating a database of each electoral ward's poverty ranking and so I am not yet able to determine how closely provinces used this information to determine quintile assignments. However, I do have data on quintile assignments in each year from 2005 onward and so can provide some discussion on the nature of these assignments. Table 2 shows the proportion of schools in each province assigned to each quintile, weighted by enrollment in 2005. Poorer provinces such as the Eastern Cape and KwaZulu-Natal have slightly more than one fifth of their learners in the bottom quintiles and less than one fifth in the top quintiles, while the pattern is reversed for wealthier provinces such as the Western Cape.

Table 3 shows that the quintile assignments in most provinces are highly but not perfectly persistent between 2005 and 2010. Very few schools change quintile assignments from 2005 to 2006, apart from what appears to be an almost complete reshuffle in the Eastern Cape that may be an error in the Department of Basic Education's database. There are substantial changes from 2006 to 2008 in the Free State, Limpopo, Mpumalanga, and North West, though the Eastern Cape largely returns to its 2005 assignments.[2] There are almost no further changes between 2008 and 2010. An obvious concern with the extensive changes between 2006 and 2008 is that schools may have lobbied provincial education departments to reassign

[1]This section draws heavily on Borkum (2011) and Wildeman (2008).
[2]The 2007 quintile assignments are unfortunately not available in the data I have collected to date but I expect to correct this omission in the future.

them into different quintiles and hence change their status from no fee to fee charging or *vice versa*. I believe that it is most likely that this lobbying would have occured after the initial 2005 assignments, rather than before. The possibility of implementing no fee schools was first raised in 2003 but the specific policy details were first laid out in 2005, so schools would have needed to be extremely forward-looking to begin lobbying to change their quintile assignment before 2005 (Department of Education, 2003, 2006). I discuss in section 3 how I adapt my empirical strategy to address the possibility of quintile manipulation after 2005.

Table 4 shows that despite the substantial changes in schools' quintile assignments, the 2005 quintile assignments still strongly predict no fee status in 2007 and in subsequent years. Between 70 and 80% of schools (weighted by 2005 enrollment) were assigned in 2007 to the no fee status predicted by their 2005 quintile (no fee if in quintile 1 or 2, fee charging if in quintile 3, 4 or 5). Figure 1 presents the same information graphically, showing that the probability of being a no fee school is monotonically declining in the quintile assignment in each year for the country as a whole. Figures 2, 3, and 4 show that this relationship holds across almost all provinces in 2007, 2008, and 2009 respectively. There is a slight rise in the proportion of no fee schools between 2007 and 2008 but table 5 shows that there is a very high correlation between no fee assignments through time, with substantial changes occurring only in the North West province. All of this suggests that despite the possibility of lobbying by schools to change their quintile assignments, initial assignments strongly predict whether they are ultimately designated as no fee schools.

3 Empirical strategy

I begin the analysis with a standard difference-in-differences design that compares the difference in enrollment between 2006 and 2007 for no fee schools to the difference for fee charging schools. This strategy identifies the mean effect of school fee abolitions under the relatively weak restriction on the data generating process that the time trend in enrollment for the two groups of schools would have been identical in the absence of the intervention.

More formally, this design identifies the the average treatment effect on the treated, defined as

$$\Delta^{ATT} = \mathbb{E}\left[\tilde{Y}_i | D_i = 1, T_i = 1\right] - \mathbb{E}\left[Y_i | D_i = 1, T_i = 1\right] \tag{1}$$

The values of D_i and T_i indicate the treatment to which a school is actually exposed: $D_i = 1$ and $D_i = 0$ denote no fee and fee charging schools respectively, while $T_i = 1$ and $T_i = 0$ denote 2006 (prior to the no fee policy) and 2007 (the first year of the no fee policy). $\tilde{Y}_i$ denotes enrollment at school i if it is exposed to the treatment (i.e. is a no fee school in 2007) and Y_i denotes enrollment at school i if it is not exposed to the treatment (i.e. is a no fee school in 2006 or a fee charging school in either year). The first term in equation (1) is directly observed, while the second is identified only under suitable restrictions on the data generating process. Note that identification here requires restrictions only on the data generating process *when fees are charged* and allows us to remain agnostic as to the process generating enrollment when fees are abolished.[3] However, the average treatment effect on the treated (ATT) cannot be used to predict directly what would happen to enrollment in quintile 3, 4, or 5 schools if they were to charge fees.

The standard difference-in-differences strategy imposes the identifying assumptions that if the no fee policy were not introduced:

(A1) $Y_i^{DT} = h(D_i, T_i, \epsilon_i) = \alpha + \beta D_i + \gamma T_i + \epsilon_i$

(A2) $\mathbb{E}[\epsilon_i|D = 1, T = 1] - \mathbb{E}[\epsilon_i|D = 1, T = 0] - \mathbb{E}[\epsilon_i|D = 0, T = 1] - \mathbb{E}[\epsilon_i|D = 0, T = 0]$

where ϵ_i^{DT} is a scalar capturing the unobserved characteristics of school i. The first assumption requires that the outcomes be generated by a single index function that is additively separable in D, T, and ϵ. The second assumption requires that the change in the mean of the unobserved characteristics from 2006 to 2007 be identical for no fee and fee charging schools if no there had been no policy change. Note that these assumptions will not jointly hold for multiple monotonic but non-affine rescalings of the outcomes. In particular, the assumptions cannot be true for an outcome measured both in levels and logs Meyer (1995). Also note that these assumptions require that the structure of the data-generating process be identical for all schools charging fees: fee charging schools in 2006 and 2007 and no fee schools in 2006. This is a substantive restriction but is unavoidable in all difference-in-differences analyses, which are based on assuming some common structure for the outcomes in the non-intervention group (fee charging schools) and in the intervention group in the absence of the intervention (no fee schools in 2006).

Under assumptions (A1) and (A2),

$$\begin{aligned}
&\mathbb{E}\left[Y_i|D_i = 1, T_i = 0\right] + \mathbb{E}\left[Y_i|D_i = 0, T_i = 1\right] - \mathbb{E}\left[Y_i|D_i = 0, T_i = 0\right] \\
&\qquad = \alpha + \beta + \mathbb{E}\left[\epsilon_i|D_i = 1, T_i = 0\right] + \alpha + \gamma + \mathbb{E}\left[\epsilon_i|D_i = 0, T_i = 1\right] - \alpha - \mathbb{E}\left[\epsilon_i|D_i = 0, T_i = 0\right] \\
&\qquad = \alpha + \beta + \gamma + \mathbb{E}\left[\epsilon_i|D_i = 1, T_i = 0\right] + \mathbb{E}\left[\epsilon_i|D_i = 0, T_i = 1\right] - \mathbb{E}\left[\epsilon_i|D_i = 0, T_i = 0\right] \\
&\qquad = \mathbb{E}\left[Y_i|D_i = 1, T_i = 1\right].
\end{aligned}$$

[3]This is an advantage of restricting attention to the average treatment effect on the treated. Identifying the average treatment effect requires knowledge of two counterfactuals: enrollment that would have occurred with fees being charged and enrollment that would have occurred with fees not being charged. Hence, a model of enrollment in the absence of fees is required to identify the average treatment effect. See Heckman and Robb (1985) for a more general discussion of the difference between these two parameters and the nature of the restrictions required to identify them.

This yields the familiar difference-in-differences result

$$\Delta^{ATT} = \mathbb{E}\left[\tilde{Y}_i | D_i = 1, T_i = 1\right] - \mathbb{E}\left[Y_i | D_i = 1, T_i = 0\right]$$
$$-\mathbb{E}\left[Y_i | D_i = 0, T_i = 1\right] + \mathbb{E}\left[Y_i | D_i = 0, T_i = 0\right] \quad (2)$$

which can be consistently estimated by sample analogues

$$\hat{\Delta}^{ATT} = \frac{1}{N_1}\sum_i D_i T_i Y_i - \frac{1}{N_1}\sum_i D_i(1-T_i)Y_i$$
$$-\frac{1}{N_0}\sum_i (1-D_i)T_i Y_i + \frac{1}{N_0}\sum_i (1-D_i)(1-T_i)Y_i \quad (3)$$

or equivalently by

$$\hat{\Delta}^{ATT} = \frac{1}{N_1}\sum_i D_i \Delta Y_i - \frac{1}{N_0}\sum_i (1-D_i)\Delta Y_i \quad (4)$$

where N_d is the number of observations in the group $D = d$ and ΔY_i is the change in enrollment in school i between 2006 and 2007.

Alternatively, assumptions (A1) and (A2) can be relaxed in favor of the conditional assumptions:

(A3) $Y_i^{DT} = h(D_i, T_i, X_i, \epsilon_i) = \alpha + \beta D_i + \gamma T_i + X_i'\delta + \epsilon_i$

(A4) $\mathbb{E}[\epsilon_i | X_i, D_i = 1, T_i = 1] - \mathbb{E}[\epsilon_i | X_i, D_i = 1, T_i = 0] - \mathbb{E}[\epsilon_i | X_i, D_i = 0, T_i = 1] - \mathbb{E}[\epsilon_i | X_i, D_i = 0, T_i = 0]$

where X is a vector of province fixed effects. Note that these conditions require merely common trends in the unobserved characteristics within each province, not averaged across all provinces. A model may be conditionally identified but not unconditionally identified. Under these assumptions, estimating the regression model

$$Y_i = \mu_{11} D_i T_i + \mu_{10} D_i(1-T_i) + \mu_{01}(1-D_i)T_i + \mu_{00}(1-D_i)(1-T_i) + X_i'\delta + \epsilon_i.$$

generates a consistent estimator of the conditional average treatment effect on the treated:

$$\hat{\Delta}^{ATT} = \hat{\mu}_{11} - \hat{\mu}_{10} - \hat{\mu}_{01} + \hat{\mu}_{00}. \quad (5)$$

An equivalent strategy estimates

$$\Delta Y_i = \mu_1 D_i + \mu_0(1-D_i) + X_i'\delta + \epsilon_i,$$

yielding

$$\hat{\Delta}^{ATT} = \hat{\mu}_1 - \hat{\mu}_0 \tag{6}$$

as the average treatment effect on the treated.

This linear difference-in-differences strategy does not impose any assumptions about the distribution of treatment effects and so allows any form of heterogeneity. However, the average treatment effect on the treated that it estimates averages across this heterogeneity rather than exploring how treatment effects vary across schools. Estimators (4) and (6) can be adapted to explore heterogeneity with respect to observed school characteristics. In particular, I estimate (6) separately for schools serving different phases: primary, intermediate, and secondary schools. This estimates the average treatment on the treated for schools in each phase and permits a test of whether these effects differ across phase.

Estimating average effects for selected subgroups of students provides some information about the heterogeneity of the effects of fee abolitions. However, it cannot speak to full distribution of outcomes if fees were still charged. In this section I therefore turn to the nonlinear difference-in-differences strategy proposed by Athey and Imbens (2006) that identifies the full counterfactual distribution of outcomes if no change had taken place in school fees. This stronger result comes at the cost of a stronger assumption: that there would have been no change in the distribution of unobserved characteristics for either no fee or fee charging schools if no policy change had taken place.

More formally, the Athey-Imbens strategy recovers the full set of quantile treatment effects on the treated (QTT), defined by

$$\tau^{QTT}(q) = F^{-1}_{\tilde{Y}^{11}}(q) - F^{CF-1}_{Y^{11}}(q) \tag{7}$$

where $F_{\tilde{Y}^{11}}$ is the observed distribution of enrollment under the no fee treatment and $F^{CF}_{Y^{11}}$ is the counterfactual distribution of enrollment in the same schools that would have occurred if fees were still charged. (Recall that I define $\tilde{Y}_i$ as the enrollment for a no fee school in 2007 and Y_i as the enrollment for a school that charges fees. The inverse distribution functions are defined as $F^{-1}_Y(q) = \inf \{y \in \mathbb{Y} : F_Y(y) \geq q\}$. This strategy assumes that in the absence of the treatment:

(B1) $Y_i^{DT} = h(T_i, \epsilon_i^D)$ with $h()$ strictly increasing in the scalar unobserved characteristic ϵ_i for $T \in \{0, 1\}$

(B2) $\epsilon_i \perp T_i | D_i$

(B3) $\varepsilon^1 \subseteq \varepsilon^0$, where ε^D is the support of ϵ_i^D for $D_i \in \{0, 1\}$.

To build intuition for their strategy, consider two schools from 2006 with the same enrollment Y_i, one from the no fee group and one from the fee charging group. The assumption that $h()$ is invertible and common to the two groups means that they must have the same value of the scalar unobserved characteristic $\epsilon_i = h^{-1}(Y_i; T_i = 0)$. The assumption that $h()$ is common to fee charging schools in 2006 and 2007 means that the fee charging school would have the enrollment $h\left(h^{-1}\left(Y_i; T_i = 0\right), T_i = 1\right)$ in 2007. Comparing this outcome to Y_i reveals the counterfactual outcome that the no fee school would have experienced in 2007 if it still charged fees. Replicating this analysis for all levels of enrollment in no fee schools in 2006 generates the full counterfactual distribution of outcomes, as assumption (B2) rules out changes in the distribution of the unobserved scalar through time. Finally, assumption (B3) requires that for every no fee school, there exists some fee charging school with equal 2006 enrollment. If this final assumption is violated, then the model is identified only for those values of ϵ_i^1 that are contained in ε_i^0. Under these identifying assumptions, the counterfactual distribution is given by

$$F_{Y^{11}}^{CF}(y) = F_{Y^{10}}\left(F_{Y^{00}}^{-1}\left(F_{Y^{01}}\left(y\right)\right)\right) \tag{8}$$

so the treatment effect can be consistently estimated by sample analogues:

$$\begin{aligned} \hat{\tau}^{QTT}(q) &= \hat{F}_{\tilde{Y}^{11}}^{-1}(q) - \hat{F}_{Y^{11}}^{CF-1}(q) \\ &= \min\left\{y : \hat{F}_{\tilde{Y}^{11}} \geq q\right\} - \min\left\{y : \hat{F}_{Y^{10}}\left(\hat{F}_{Y^{00}}^{-1}\left(\hat{F}_{Y^{01}}\left(y\right)\right)\right) \geq q\right\} \end{aligned} \tag{9}$$

where the cumulative distribution functions are estimated by the empirical distribution, so $\hat{F}_{Y^{DT}}(y) = N_{DT}^{-1}\sum_i D_i T_i \mathbf{1}\{Y_i \leq y\}$.

There are two salient distinctions between this model and the standard difference-in-differences strategy. First, the Athey-Imbens model relaxes assumptions (A1) and (A3) that outcomes are a linear function of group membership, time, and unobserved characteristics, all of whose effects are additively separable. Relaxing this restriction allows for a richer class of data generating processes and in particular removes the sensitivity to monotonic but non-affine transformations of the outcomes. Second, the model now requires that there be no changes in the distribution of the unobserved characteristic through time, which is strictly stronger than the assumption of parallel mean time trends in the standard model. Note that both the standard and Athey-Imbens models impose the restriction that the structure of the data generating process is the same for non-dormitory students and dormitory students in the mixing period. This is again restrictive but unavoidable.

The key advantage of this second strategy is that it generates the full counterfactual distribution of enrollment that would have prevailed in the absence of the no fee intervention, or equivalently, the full set of quantile treatment effects on the treated. Note that these should be interpreted as treatment effects for each quantile of the enrollment distribution, rather than for any particular school. To identify treatment effects on specific schools, we require the stronger assumption that all no fee schools would have the same rank in the distribution of enrollment in 2006 and 2007. Heckman, Smith, and Clements (1997) provide a more detailed discussion of this point.

In section 2 I noted the risk that some schools may have successfully lobbied for their quintile assignment to change between 2005 and 2007 in order to change their no fee status. Consider, in particular, the possibility that schools that expected to experience unusually large increases in enrollment lobbied to avoid being designated for no fee status, as they were worried about their facilities being strained by more learners. This would biased the treatment effects toward zero, as only those schools for whom the fee abolition would have had little effect would actually have been exposed to treatment. However, if schools that expected to experience unusually large increases in enrollment lobbied to be designated no fee status in order to gain additional government money for their larger number of learners, then the treatment effect would be biased away from zero.

To reduce the potential for these problems to bias my estimates of the treatment effect, I use an *intention to treat* analysis, in which I analyze the relationship between intended treatment status (being in quintile 1 or 2 in 2005) and enrollment, rather between actual treatment status (being designated as a no fee school). The main results reported in the next section are intention to treat estimates but I also present one table of results using the instrumental variables difference-in-differences estimand

$$\Delta^{ATT-IV} = \frac{\frac{1}{N_1}\sum_i D_i \Delta Y_i - \frac{1}{N_0}\sum_i (1-D_i)\Delta Y_i}{\frac{1}{N_1}\sum_i D_i NF_i - \frac{1}{N_0}\sum_i (1-D_i)(1-NF_i)} \tag{10}$$

where D_i is an indicator equal to one for schools in quintile 1 or 2 in 2005 and NF_i is an indicator equal to one for schools designated for no fee status in 2007. The estimand can be consistently estimated by sample analogues or implemented in a standard instrumental variables regression. This estimator rescales the linear difference-in-differences estimator by a measure of how strongly intended treatment status predicts actual treatment status: the proportion of all schools whose actual treatment status equals their intended treatment status. This rescaling "corrects" the magnitude of the difference-in-differences estimate for the fact that not all schools that are intended to be treated are treated and hence they do not experience the treatment. Provided schools only lobby in one direction, this recovers a consistent estimate of the average treatment effect on the treated for those schools that do comply their intended treatment status.

Finally, all results reported in the next section use school-specific enrollment in 2005 for sample weights and use an asymptotic covariance matrix that allows for any correlation structure in unobserved school characteristics within each electoral ward. The results are relatively robust to omitting the weights or weighting schools by 2004 or 2006.

4 Results

Before implementing the formal difference-in-difference estimators, I explore level and change in enrollment between 2006 and 2007 in tables 6 and 7. The first and fifth columns of table 6 show that there the enrollment, measured in levels or logs, is highly persistent between 2006 and 2007: regressing 2007 enrollment on 2006 enrollment yields a point estimate very close to one with very high explanatory power. The point estimates on the indicator for schools in quintiles 1 and 2 (i.e. the indicator for schools intended to receive treatment) are positive but very small and insignificant. Adding province fixed effects in columns two and six has little effect on the regressions. The third and seventh columns regress the change in enrollment and change in log enrollment respectively on an intention to treatment indicator.[4] The point estimates suggest that schools in these quintiles increased enrollment by a precisely estimated seven students or 0.8% more than schools in the higher quintiles. These estimates are robust to adding fixed effects in columns four and eight, suggesting that there may have been an effect of the no fee schools changes in enrollment.

Table 7 repeats this exercise but uses distinct indicators for each quintile (with quintile 3 as the omitted category) to explore which schools are driving the pattern. All specifications suggest that the intention to treat estimates are driven by quintile 1 schools, who see their enrollment rise by between 7 and 15 students between 2006 and 2007 or 1.5 and 2%. In contrast, the point estimates for quintile 2 are negative and insignificant across all specifications. Interesting, enrollment in quintile 5 schools also rises significantly from 2006 to 2007, relative to quintile 3 schools. This might be driven in part by students transfering from quintile 3 or 4 schools into quintile 5 schools that they expect to provide better education, though I cannot directly test this hypothesis. I several specifications I cannot reject equality of the (log) enrollment changes in quintiles 1 and 5.

These results provide *prima facie* evidence that the changes in enrollment between 2006 and 2007 differ across no fee and fee charging schools. In table 8 I report estimates of the linear difference-in-differences estimators (4) and (6). They suggest that no fee schools may have experienced a small fall in enrollment between 2006 and 2007 but that fee charging schools experienced a considerably larger fall. Under the assumption that the time changes would have been identical in the absence of treatment, the abolition of school fees raised enrollment by a precisely estimated 8 students. Including province fixed effects reduces this estimate to 7 students. Using the change in log enrollment as an outcome yields similar results, with the no fee policy raising enrollment by between .7 and .8%. These are relatively small effects but are non-trivial given the high baseline levels of enrollment.

[4] I use both enrollment and log enrollment as outcome variables as robustness checks. As discussed in section 3, the linear difference-in-differences estimates may be sensitive to such rescalings of the outcomes. Furthermore, both measured are skewed, with skewness coefficients of approximately 1 (enrollment) and -1 (log enrollment), so the neither measure is clearly prefered from variance stabilization perspective.

Table 9 implements the conditional linear difference-in-differences estimator (6) separately for primary, intermediate, secondary and combined schools. This provides some evidence of the potential heterogeneity of effects. The results in the previous table appear to be driven largely by secondary schools, in which the no fee policy increases enrollment by 16 students or 1.3%. One of the point estimates for intermediate schools is negative but this should be interpreted with caution given the small number of schools in this category and the sensitivity of the estimate to whether the outcome is measured in level or log changes. Taken together, tables 8 and 9 suggest that the no fee policy came at a time of decreasing enrollment in South African schools but that the abolition of fees stabilized enrollment.

Figure 7 shows the full set of quantile treatment effects on the treated with enrollment measured in levels and logs (panels B and D respectively). Both panels suggest that the treatment effect is positive for schools with low to medium enrollment (i.e. for all quantiles up to the 60^{th} percentile). Although the effects are also positive from the 75^{th} and higher percentiles, these are very imprecisely estimated with wide enough confidence intervals that reliable inference is infeasible.[5] On balance, the nonlinear difference-in-differences estimator suggests that there is limited heterogeneity in the treatment effects and that the average treatment effect on the treated provides a reasonable measure of the treatment effect throughout the distribution.

As discussed in section 3, the linear difference-in-differences estimand equals the average treatment effect on the treated only if the change in enrollment between 2006 and 2007 would have been the same in no fee and fee charging schools if the no fee policy were not introduced. This assumption is not directly testable but I can assess its plausibility by examining a longer time series of enrollment changes. Table 10 reports the linear difference-in-differences estimates for each year from 2004 to 2009, in levels and logs, with and without province fixed effects. The time series suggests that the enrollment changes in the two groups were not identical even before the policy was introduced but that the increase in enrollment in no fee schools from 2006 to 2007 was unusually large. This provides some reassurance that the differential baseline trends between the two groups of schools does not explain the estimated treatment effect. However, it also suggests that there is considerable year-to-year variation in enrollment numbers between schools and that the difference-in-difference effect may capture some of this variation, contaminating estimation of the treatment effect.

Finally, table 11 reports the estimates from implementing the linear instrumental variables difference-in-differences estimator (10). The first two columns report the first stage estimates, with and without province fixed effects. This shows the quintile assignments in 2005 strongly predict whether schools are

[5]Note that these confidence intervals should be expected to have the correct coverage only pointwise, or for significance tests on individual quantiles. They are not uniform and hence should be used with caution to perform inference on the curve as whole.

assigned no fee status: a school in quintile 1 or 2 is approximately 50 percentage points more likely to be a no fee school than a school in quintile 3, 4, or 5. Column 5 show that on average, schools in quintiles 1 and 2 that go on to be declared no fee schools increase their enrollment from 2006 to 2007 by 15 more students than schools in the other quintiles. Column 6 shows that this result is robust to including fixed effects and columns 9 and 10 demonstrate that this effect can alternatively be measured as a 1.5% increase in enrollment. These treatment effects are approximately twice as large as those reported in table 8, reflecting the fact that quintile assignments do no perfectly predict no fee status. As a reference point, columns 3, 4, 7 and 8 show that naively implementing the second stage regression without instrumenting for no fee status yields point estimates that are qualitatively similar to those from the instrumental variables regression but smaller in magnitude (although the differences in magnitude are not statistically significant).

Taken as a whole, these results imply that assignment to quintile 1 or 2 in 2005 led schools to increase their enrollment between 2006 and 2007 by approximately 7 students or .8% more than schools in other quintiles. This was driven by large increases of 15 students or 1.5% in schools from these quintiles that went on to abolish fees. Given the relatively high levels of baseline enrollment in South Africa, these effects are not trivially small. The disproportionate rise in enrollment occurred primarily in secondary schools, reflecting in part the far lower enrollment rates amongst teenagers than children.

References

Al-Samarrai, S., and H. Zaman (2000): "Abolishing school fees in Malawi: The impact on education access and equity," *Education Economics*, 15(3), 359–375.

Alderman, H., J. Kim, and P. Orazem (1999): "Design, evaluation, and sustainability of private schools for the poor: the Pakistan urban and rural fellowship school experiments," *Economics of Education Review*, 22(3), 265–274.

Angrist, J., E. Bettinger, E. Bloom, E. King, and M. Kremer (2002): "Vouchers for private schooling in Colombia: Evidence from a randomized natural experiment," *American Economic Review*, 92(5), 1535–1558.

Athey, S., and G. Imbens (2006): "Identification and inference in nonlinear difference-in-differences models," 74(2), 431–497.

Barrera-Osorio, F., L. Linden, and M. Urquiola (2007): "The effects of user fee reductions on enrollment: Evidence from a quasi-experiment," Mimeo, Columbia University.

Borkum, E. (2011): "Can eliminating school fees in poor districts boost enrollment? Evidence from South Africa," *Economic Development and Cultural Change*, Forthcoming.

Deininger, K. (2003): "Does cost of schooling affect enrollment by the poor? Universal primary education in Uganda," *Economics of Education Review*, 22, 291–305.

Department of Education (2003): *Report to the Minister: A review of the financing, resourcing and costs of education in public schools*. Government Printers, Pretoria, ZA.

——— (2006): *Education Laws Amendment Act (No. 24 of 2005)*. Government Printers, Pretoria, ZA.

——— (2009): *Trends in Education Macro Indicators Report*. Government Printers, Pretoria, ZA.

Dynarski, S. (2003): "Does aid matter? Measuring the effect of student aid on college attendance and completion," *American Economic Review*, 93(1), 279–288.

Dynarski, S., J. Gruber, and D. Li (2009): "Cheaper by the dozen: Using sibling discounts at Catholic schools to estimate the price elasticity of private school attendance," Discussion Paper 15461, National Bureau of Economic Research.

Fafchamps, M., and B. Minten (2007): "Public service provision, user fees and political turmoil," *Journal of African Economies*, 16(3), 485–518.

Filmer, D., and N. Schady (2008): "Getting girls into school: Evidence from a scholarship program in Cambodia," *Economic Development and Cultural Change*, 56(3), 581–617.

Hahn, J., P. Todd, and W. van der Klaauw (2001): "Identification and estimation of treatment effects with a regression-discontinuity design," *Econometrica*, 69(1), 201–209.

Heckman, J., and R. Robb (1985): "Alternative Methods for Estimating the Impact of Interventions," in *Longitudinal Analysis of Labor Market Data*, ed. by J. Heckman, and B. Singer. Cambridge University Press.

Heckman, J., J. Smith, and N. Clements (1997): "Making the most out of programme evaluations and social experiments: Accounting for heterogeneity in programme impacts," *Review of Economic Studies*, 64(4), 487–535.

Kane, T. (1994): "College entry by blacks since 1970: The role of college costs, family background, and the returns to education," *Journal of Political Economy*, 102(5), 878–911.

Kremer, M., E. Miguel, and R. Thornton (2009): "Incentives to learn," *Review of Economics and Statistics*, 91(3), 437–456.

Lee, D., and T. Lemieux (2010): "Regression discontinuity designs in economics," *Journal of Economic Literature*, 48(2), 281–355.

Lucas, A., and I. Mbiti (2009): "The effect of free primary education on student participation, stratification and achievement: Evidence from Kenya," Mimeo, Wellesley University.

Meyer, B. (1995): "Natural and quasi-natural experiments in economics," *Journal of Economic and Business Statistics*, 13, 151–162.

Neal, D. (2002): "How vouchers could change the market for education," *Journal of Economic Perspectives*, 16(4), 25–44.

Noble, M., M. Babita, H. Barnes, C. Dibben, W. Magasela, S. Noble, P. Ntshongwana, H. Phillips, S. Rama, B. Roberts, G. Wright, and S. Zungu (2006): "The provincial indices of multiple deprivation for South Africa 2001," Published by the Centre for the Analysis of South African Social Policy, Oxford University, UK.

Pampallis, J. (2008): *School Fees*. Centre for Education Policy Development, Johannesburg, ZA.

SCHULTZ, P. (2004): "School subsidies for the poor: Evaluating the Mexican PROGRESA poverty program," *Journal of Development Economics*, 74(1), 199–250.

SEFTOR, N., AND S. TURNER (2002): "Back to school: Federal student aid policy and adult college enrollment," *Journal of Human Resources*, 37(2), 336–352.

TODD, P., AND K. WOLPIN (2006): "Assessing the impact of a school subsidy program in Mexico: Using a social experiment to validate a dynamic behavioral model of child schooling and fertility," *American Economic Review*, 96(5), 1384–1417.

WHITE, H. (1984): *Asymptotic Theory for Econometricians*. Academic Press, San Diego, CA.

WILDEMAN, A. (2008): "Reviewing eight years of the implementation of school funding norms," Idasa Economic Government Programme Research Paper.

Table 1: Net enrollment rates through time

	Primary education		Secondary education
	Dept of Ed.	Surveys	Dept of Ed.
1997	.92		.56
1998	.93		.59
1999	.93	.94	.60
2000	.90		.60
2001	.89	.95	.67
2002	.89	.95	.64
2003	.89	.96	.66
2004	.87	.97	.67
2005	.85	.96	.67
2006		.97	

Notes: Adapted from Department of Education (2009).
Population totals are mid-year imputations from census data.
Survey data is from the 1999 October Household Survey, the 2001-2002 Labour Force Surveys, and the 2003-2006 General Household Surveys
Net enrollment rate is defined as the number of enrolled age-eligible individuals divided by the number of age-eligible individuals.

Table 2: Quintile assignments in 2005

	(1)	(2)	(3)	(4)	(5)
	Quintile 1	Quintile 2	Quintile 3	Quintile 4	Quintile 5
South Africa	22	19	22	19	18
Eastern Cape	29	20	19	15	16
Free State	18	20	21	21	21
Gauteng	20	20	21	20	20
KwaZulu-Natal	24	19	26	17	14
Limpopo	22	22	22	21	14
Mpumalanga	19	22	22	20	17
Northern Cape	20	20	21	19	21
North West	20	20	20	20	21
Western Cape	11	11	25	24	29

Notes: Sample includes all open public schools from 2005 onward.
Proportions are weighted by school enrollment in 2005.

Table 3: Correlation of quintile assignments through time

	(1)	(2)	(3)	(4)
	2006	2008	2009	2010
South Africa	.746	.656	.66	.66
Eastern Cape	-.29	.828	.828	.828
Free State	1	.518	.527	.527
Gauteng	1	.713	.714	.714
KwaZulu-Natal	1	.764	.776	.776
Limpopo	1	.453	.448	.448
Mpumalanga	.965	.411	.418	.418
Northern Cape	1	.641	.642	.642
North West	.954	.351	.364	.364
Western Cape	.88	.913	.913	.913

Notes: Sample is all open public schools from 2005 onward.
Pearson correlation coefficients between the original 2005 quintile allocations and those in each subsequent year
Correlations are weighted by school enrollment in 2005.
Quintile allocations for 2007 are not available

Table 4: No fee status by quintile assignment through time

Panel A: 2007

	No fee	Fee charging
Quintile 1/2	.7	.3
Quintile 3/4/5	.2	.8

Panel B: 2008

	No fee	Fee charging
Quintile 1/2	.73	.27
Quintile 3/4/5	.23	.77

Panel C: 2009

	No fee	Fee charging
Quintile 1/2	.72	.28
Quintile 3/4/5	.22	.78

Notes: Sample is all open public schools from 2005 onward.
Probabilities are weighted by school enrollment in 2005.

Table 5: Correlation of no fee status through time

	(1)	(2)	(3)
	2008	2009	2010
South Africa	.86	.858	.858
Eastern Cape	.979	.978	.978
Free State	.754	.717	.717
Gauteng	.989	.986	.986
KwaZulu-Natal	.756	.744	.744
Limpopo	.867	.866	.866
Mpumalanga	.956	.958	.958
Northern Cape	.724	.712	.712
North West	.451	.503	.503
Western Cape	.996	.996	.996

Notes: Sample is all open public schools from 2005 onward.
Pearson correlation coefficients between the original 2007 no fee assignments and those in each subsequent year
Correlations are weighted by school enrollment in 2005.

Table 6: Enrollment levels and changes in 2007 by intended treatment status

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dependent variable	Enrollment		Δ enrollment		Log enrollment		Δ log enrollment	
Quintile 1/2	.1	-.9	7.7***	6.8***	.002	.001	.008***	.008***
	(2.5)	(2.4)	(2.1)	(2.1)	(.003)	(.003)	(.002)	(.002)
Lagged enrollment	.958***	.949***						
	(.006)	(.006)						
Lagged log enrollment					.99***	.984***		
					(.003)	(.003)		
Province fixed effects		×		×		×		×
Sample size	22841	22841	22841	22841	22841	22841	22841	22841
Number of clusters	3587	3587	3587	3587	3587	3587	3587	3587
R^2	.933	.933	.001	.007	.93	.931	0	.009

Notes: Sample is all open public schools between 2005 and 2008.
Regressions are weighted by school enrollment in 2005.
Standard errors are clustered by electoral ward, following White (1984).
***, ** and * denote significance at the 1, 5 and 10% levels respectively.

Table 7: Enrollment levels and changes in 2007 by quintile

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dependent variable	Enrollment		Δ in enrollment		Log enrollment		Δ log enrollment	
Quintile 1	8**	7.3**	14.8***	14.2***	.017***	.016***	.019***	.019***
	(3.5)	(3.3)	(3.3)	(3.2)	(.005)	(.005)	(.004)	(.003)
Quintile 2	-4.2	-4.8	0	-.1	-.002	-.002	-.003	-.002
	(3.4)	(3.3)	(3.5)	(3.4)	(.005)	(.005)	(.004)	(.004)
Quintile 4	-1.2	-.5	-4.4	-3.9	.002	.003	-.005	-.003
	(3.8)	(3.8)	(3.8)	(3.8)	(.006)	(.006)	(.004)	(.004)
Quintile 5	8**	8.4**	5.3	6.2*	.017***	.019***	.007*	.009**
	(3.5)	(3.5)	(3.5)	(3.6)	(.005)	(.006)	(.004)	(.004)
Lagged enrollment	.958***	.949***						
	(.003)	(.003)						
Lagged log enrollment					.99***	.984***		
					(.003)	(.003)		
P-value for test of Q1=Q2	0	0	0	0	0	0	0	0
P-value for test of Q1=Q4	.021	.048	0	0	.001	.003	0	0
P-value for test of Q1=Q5	.997	.766	.002	.014	.877	.461	.001	.003
P-value for test of Q2=Q4	.406	.23	.204	.291	.359	.209	.695	.724
P-value for test of Q2=Q5	0	0	.097	.057	0	0	.003	.003
P-value for test of Q4=Q5	.008	.009	.005	.004	0	0	.002	.002
Province fixed effects		×		×		×		×
Sample size	22841	22841	22841	22841	22841	22841	22841	22841
Number of clusters	3587	3587	3587	3587	3587	3587	3587	3587
R^2	.933	.934	.004	.01	.93	.931	.003	.011

Notes: Sample is all open public schools between 2005 and 2008.
Regressions are weighted by school enrollment in 2005.
Standard errors are clustered by electoral ward, following White (1984).
***, ** and * denote significance at the 1, 5 and 10% levels respectively.

Table 8: Linear difference-in-differences results in 2007

	(1)	(2)	(3)	(4)
Dependent variable	Change in enrollment		Log change in enrollment	
Quintile 1/2	-6.8***	-1.8	-.002	-.012***
	(1.6)	(2.7)	(.002)	(.003)
Quintile 3/4/5	-14.5***	-8.7***	-.01***	-.019***
	(1.5)	(1.2)	(.002)	(.002)
Difference	7.7***	6.9***	.008***	.007***
	(2.1)	(2.2)	(.002)	(.002)
Province fixed effects		×		×
Sample size	22841	22841	22841	22841
Number of clusters	3587	3587	3587	3587
R^2	.001	.007	0	.009

Notes: Sample is all open public schools between 2005 and 2008.
Regressions are weighted by school enrollment in 2005.
Standard errors are clustered by electoral ward, following White (1984).
***, ** and * denote significance at the 1, 5 and 10% levels respectively.

Table 9: Linear difference-in-differences results in 2007 by phase

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dependent variable	Change in enrollment				Log change in enrollment			
Quintile 1/2	.8	-52.5***	6.1	-.7	-.022***	-.14	-.01**	-.009
	(2)	(14.5)	(4.6)	(3.8)	(.003)	(.036)	(.005)	(.007)
Quintile 3/4/5	-.1	-19.4***	-10***	-1.8	-.02***	-.095***	-.022***	-.012***
	(.8)	(6.5)	(1.8)	(1.7)	(.001)	(.017)	(.002)	(.003)
Difference	.9	-33.1**	16.1***	1	-.002	.044	.013***	.003
	(1.6)	(12.9)	(3.9)	(3.5)	(.003)	(.034)	(.005)	(.006)
Primary schools	×				×			
Intermediate schools		×				×		
Secondary schools			×				×	
Combined schools				×				×
Province fixed effects	×	×	×	×	×	×	×	×
Sample size	13845	387	5236	3372	13845	387	5236	3372
Number of clusters	3391	297	2674	1141	3391	297	2674	1141
R^2	.022	.076	.018	.037	.014	.059	.01	.017

Notes: Sample is all open public schools between 2005 and 2008.
Regressions are weighted by school enrollment in 2005.
Standard errors are clustered by electoral ward, following White (1984).
***, ** and * denote significance at the 1, 5 and 10% levels respectively.

Table 10: Linear difference-in-differences results for each year

	(1)	(2)	(3)	(4)
Dependent variable	Change in enrollment		Log change in enrollment	
2004	3.4*	4.4**	.005**	.003
	(1.8)	(1.9)	(.003)	(.003)
2005	-5.6*	-4.7*	.009	.01
	(2.7)	(2.5)	(.008)	(.008)
2006	-.8	-3.3	-.006***	-.008***
	(2.8)	(2.5)	(.002)	(.002)
2007	7.7***	6.8***	.008***	.006**
	(2.1)	(2.2)	(.002)	(.002)
2008	-3.8**	-3.9**	.007**	.006**
	(1.7)	(1.8)	(.002)	(.003)
2009	3.8*	3.6	-.005	.004
	(2.3)	(2.4)	(.003)	(.003)
Province fixed effects		×		×

Notes: Sample is all open public schools between 2005 and 2008.
Regressions are weighted by school enrollment in 2005.
Standard errors are clustered by electoral ward, following White (1984).
***, ** and * denote significance at the 1, 5 and 10% levels respectively.

Table 11: Linear instrumental variables difference-in-differences results in 2007

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Dependent variable	No fee school		Change in enrollment				Log change in enrollment			
Quintile 1/2	.496***	.478***								
	(.011)	(.011)								
No fee school			10.3***	11.7***			.01***	.014***		
			(2.1)	(2.4)			(.002)	(.003)		
Predicted no fee school					15.5***	14.4***			.016***	.014***
					(4.3)	(4.6)			(.005)	(.005)
Province fixed effects		×		×		×		×		×
Instrumental variables					×	×			×	×
First stage F-statistic	2156	1874								
Sample size	22911	22911	22911	22911	22911	22911	22911	22911	22911	22911
Number of clusters	3599	3599	3599	3599	3599	3599	3599	3599	3599	3599
R^2	.25	.25	.002	.003	.002	.003	.001	.002	.001	.002

Notes: Sample is all open public schools between 2005 and 2008.
Regressions are weighted by school enrollment in 2005.
Standard errors are clustered by electoral ward, following White (1984).
***, ** and * denote significance at the 1, 5 and 10% levels respectively.

Figure 1: Proportion of no fee schools by quintile in each year

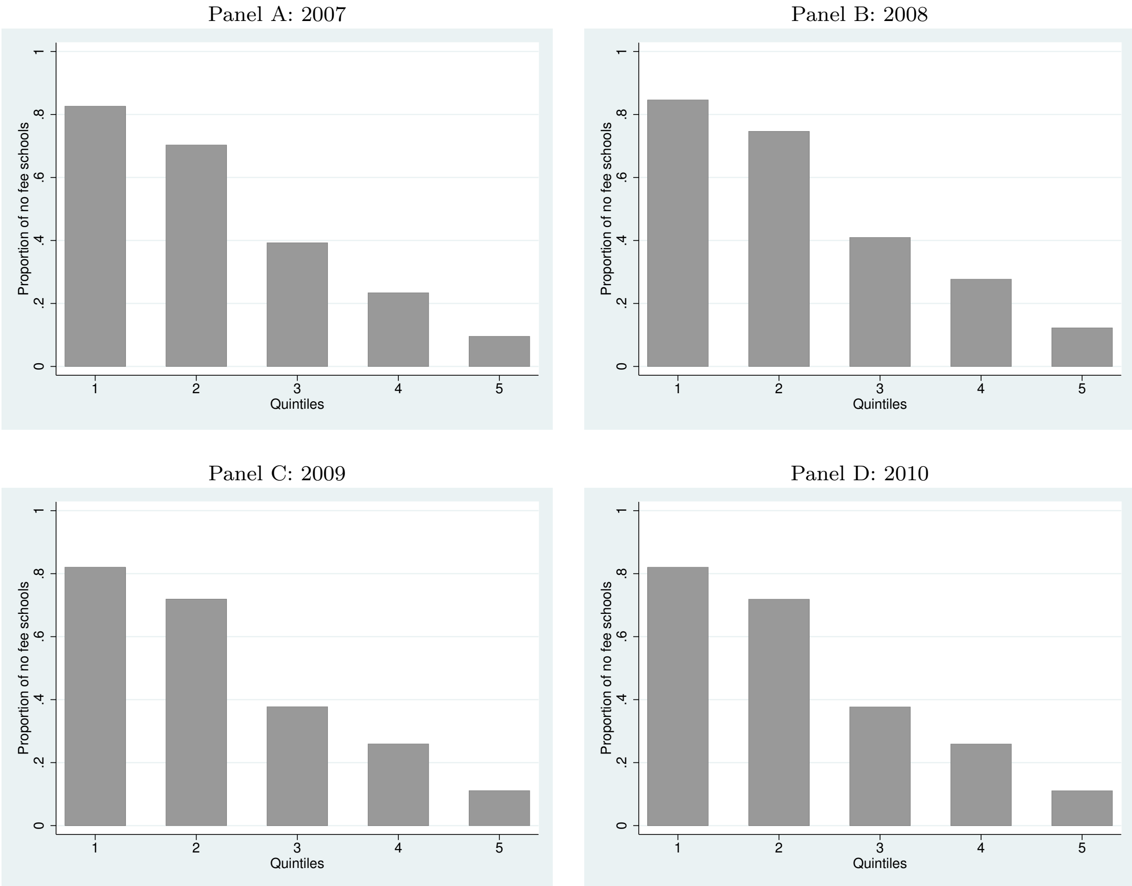


Figure 2: Proportion of no fee schools by quintile for each province in 2007

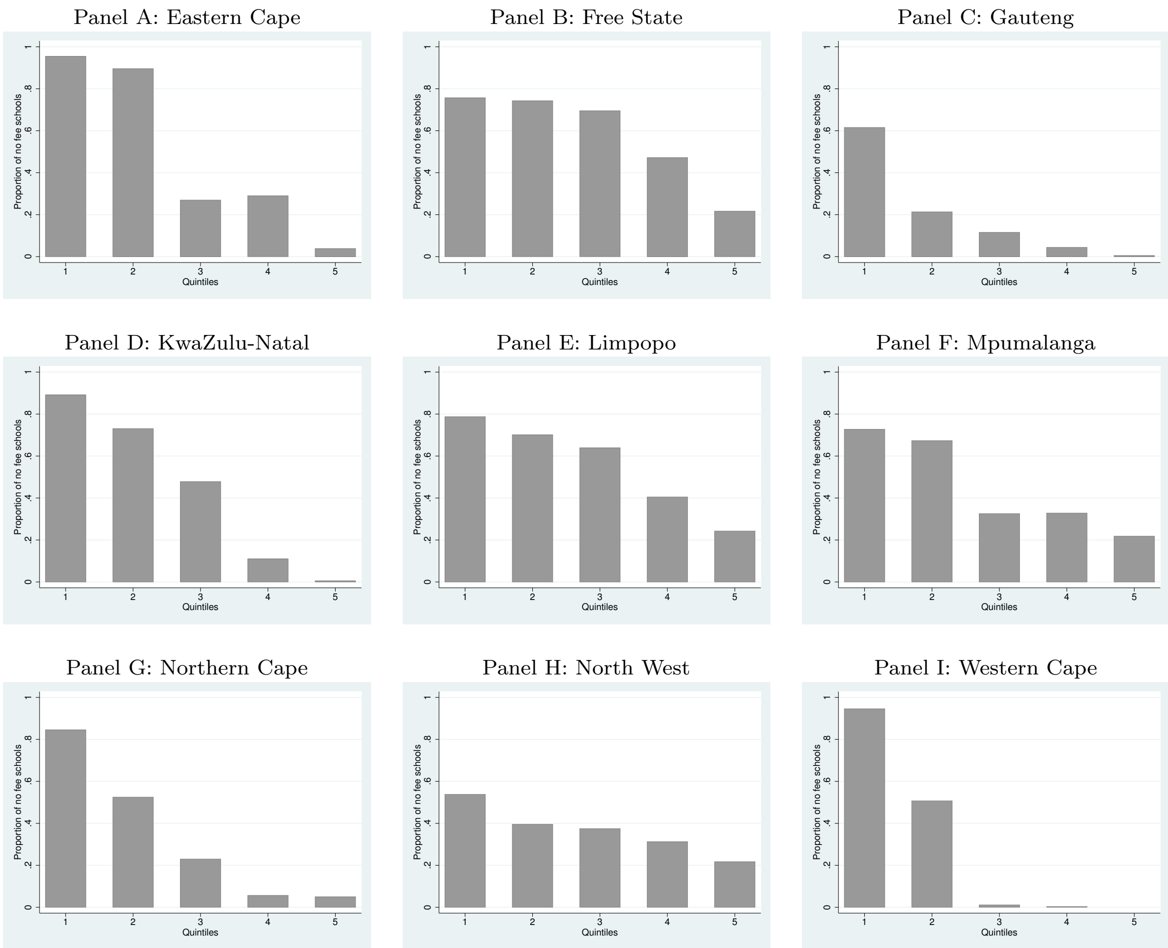


Figure 3: Proportion of no fee schools by quintile for each province in 2008

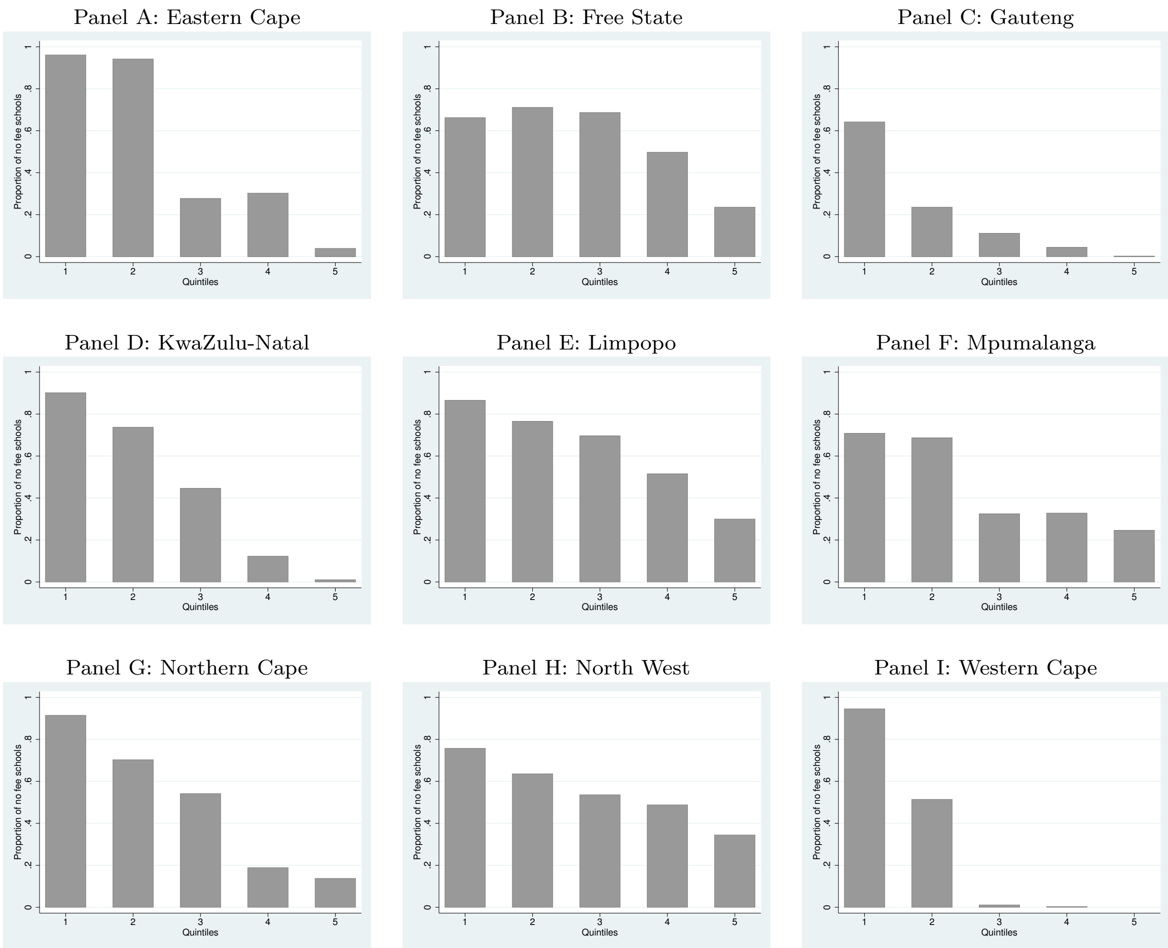


Figure 4: Proportion of no fee schools by quintile for each province in 2009

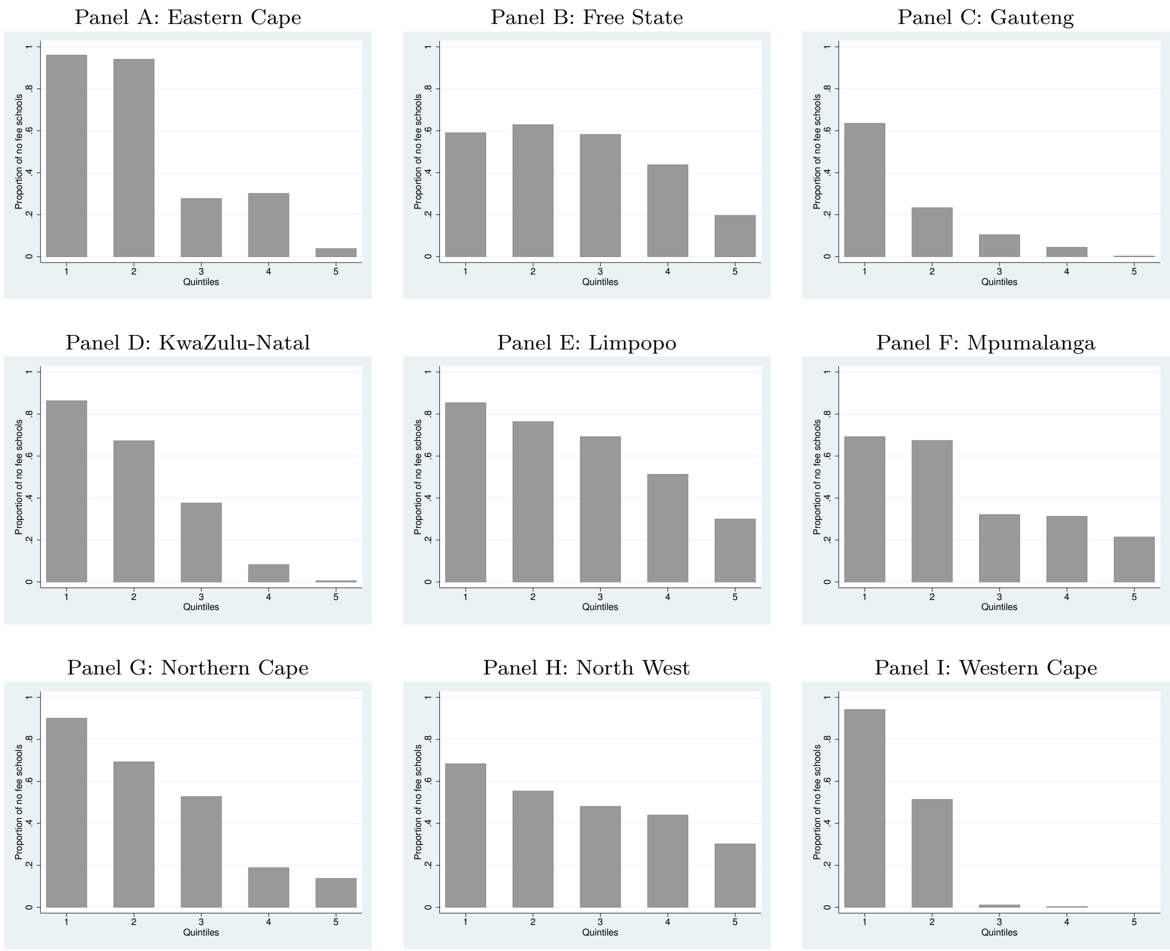


Figure 5: Total enrollment by quintile through time

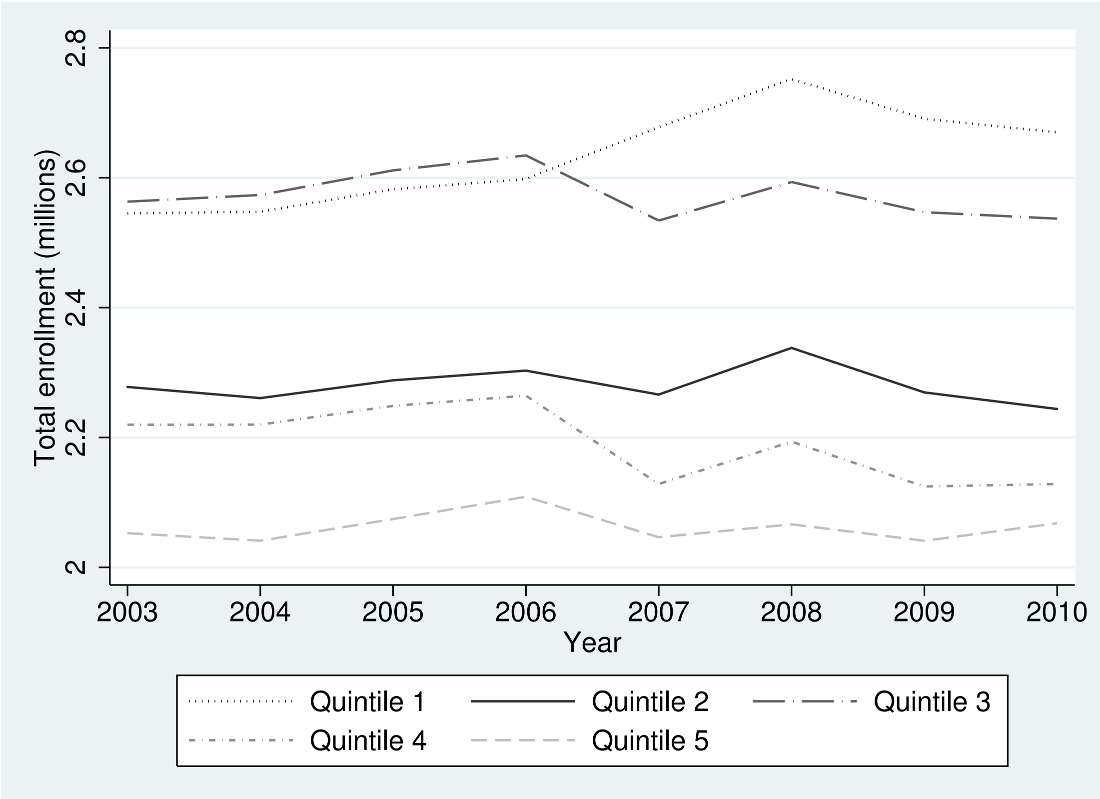


Figure 6: Total enrollment by quintile through time for each province

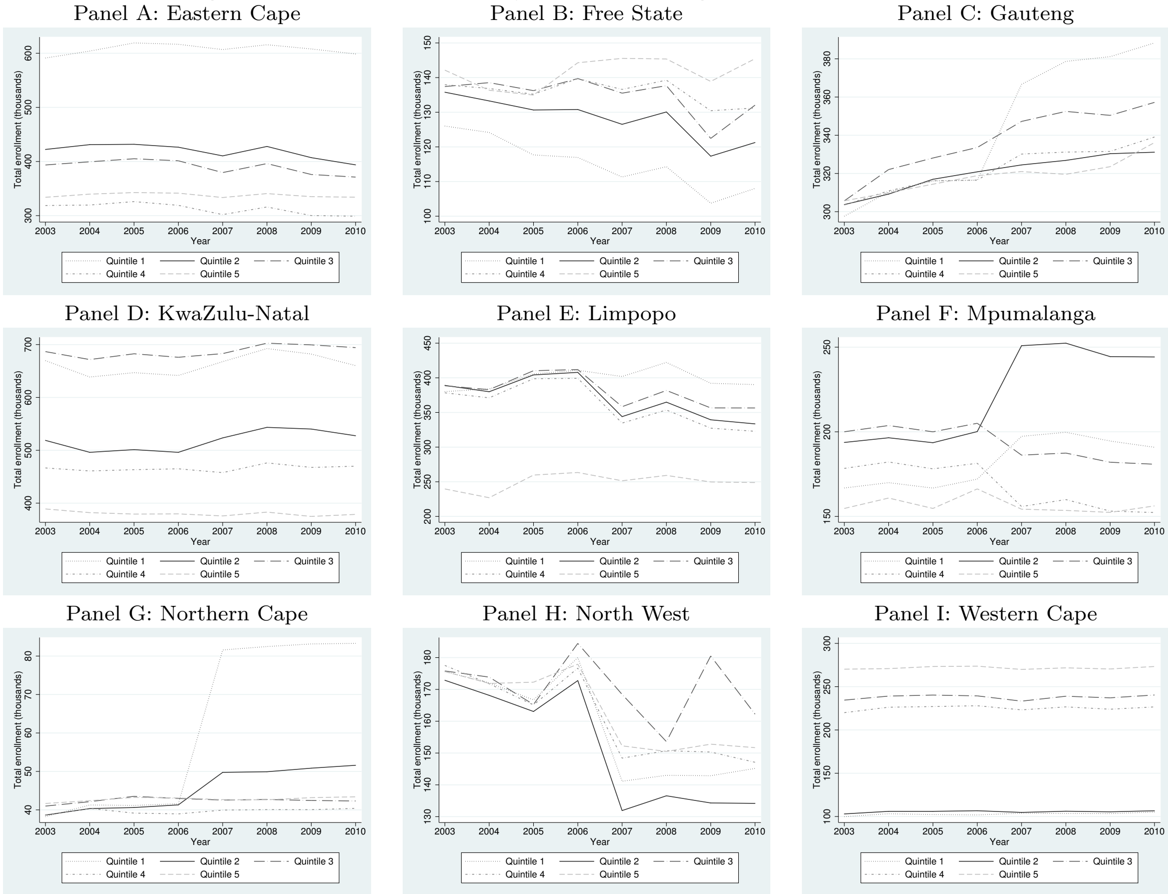
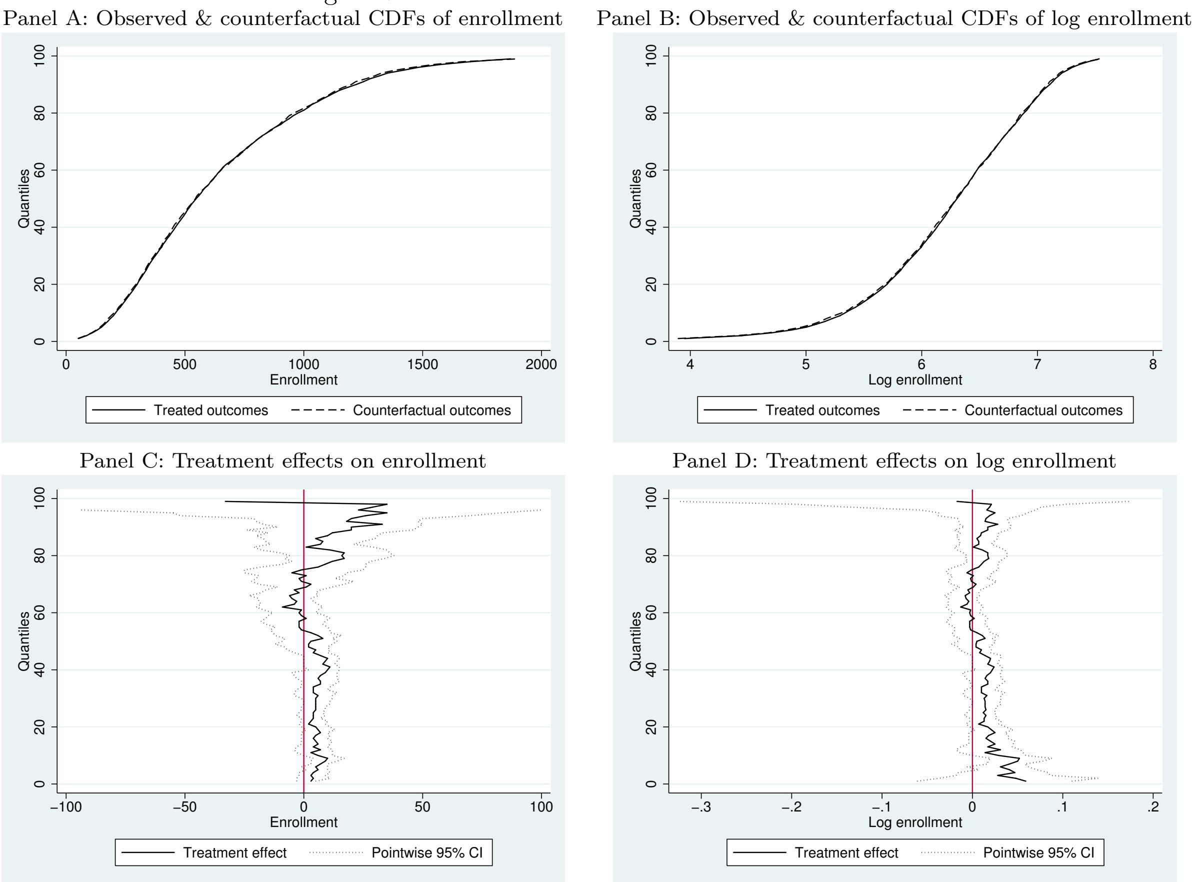


Figure 7: Treatment effects of increased enrollment



Notes: The pointwise confidence intervals are the difference between percentiles 2.5 and 97.5 of the distribution of treatment effects from 500 iterations of a cluster bootstrap.