

What Democracy Does (and Doesn't do) for Basic Services:
School Fees, School Inputs, and African Elections

Robin Harding	David Stasavage
New York University	New York University
robin.harding@nyu.edu	david.stasavage@nyu.edu

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Abstract

We advance on existing work by exploring the potential mechanisms through which a democratic transition may prompt a government to alter provision of basic services to its citizens. In an environment of weak and uncertain state capacity, which makes it particularly difficult for voters to attribute outcomes to executive actions, we suggest that electoral competition is most likely to lead to changes in policies where executive action is verifiable so that candidates and voters can form an implicit contract. Considering the context of African primary education as an example, we suggest that electoral competition will therefore give governments an incentive to abolish school fees, but it will have less effect on the provision of school inputs and on school quality, precisely because executive actions on these issues are more difficult to monitor. To support our claim we use both cross-national and individual level data, including an original data set on primary school fee abolitions. We show that in Africa, democracies have higher rates of school attendance than non-democracies. Moreover, this appears to be due above all to the fact that democracies are more likely to abolish school fees, not to the fact that they provide more inputs. We then estimate the likelihood that a government will abolish school fees subsequent to an election, taking account of endogeneity concerns involving election timing. Finally, we use survey data from Kenya to provide evidence suggesting that citizens condition their voting intentions on an outcome that a politician can control directly, such as abolishing school fees, but not on outcomes over which politicians have much more indirect influence, such as local school quality.

1 Introduction

There is a strong sense that governments subject to electoral competition may be more likely to provide basic services to their citizens. Yet there is little agreement on whether this is the case in practice and even less consensus on the precise mechanisms through which electoral competition leads to changes in service provision. There is by now an extensive empirical (and mostly cross-national) literature on this topic that tends to use broad measures of democracy and then examines whether these measures are correlated with outcomes assumed to be influenced by government policy (such as infant mortality) or activity measures (such as levels of education spending) that are assumed to have an effect on outcomes.¹ In looking for broad correlations this literature generally sets aside a crucial issue - how are voters supposed to condition their support for a candidate on effort in the area of basic service provision if this effort is not directly observable? You may well know whether the school in your village lacks a roof, but you may not know whether this is attributable to insufficient allocation of funds at the central level, or to some implementation failure at the local level. Candidates in election campaigns may make promises with regard to education, health, or other services, but in an environment where there are also problems of implementation (i.e. weak capacity), we will suggest that it is difficult for candidates and voters to form an implicit contract over what exactly constitutes a broken campaign promise. We argue that under these conditions, a democratic transition will have an effect on basic service provision to the extent that voters can verify whether a promise has been kept. A candidate's promise to abolish user fees in health or education can be verified *ex post*. A promise to exert more effort to hire teachers, to construct schools, or to improve education quality may be much more difficult to verify. A voter may observe that the end outcome is a failure but without knowing where to attribute

¹Some of the contributions to this voluminous literature include Blaydes and Kayser (2011), Ross (2006), Kudamatsu (2010), Besley and Kudamatsu (2006), Baum and Lake (2003), Bueno de Mesquita, Smith, Siver-son, and Morrow (2003), Stasavage (2005), Brown and Hunter (2004), Adsera, Boix, and Payne (2003), and Min (2010). For an interesting investigation of these issues at the local level in Africa see Gottlieb (2010). Lindert (2004) makes claims about democracy and basic service provision in a historical context. Amartya Sen (1999) has also long been associated with the idea that democratic governments might be more likely to fulfill certain needs of their population. A prediction that democratically elected governments in poor countries will be more likely to provide basic services would also seem to fit with the broad views of democracy and redistribution presented by either Acemoglu and Robinson (2006) or by Boix (2003).

blame. Therefore democracy may tend to result in reforms that increase access without having a similar effect on provision of inputs or quality.

Our findings in this paper have implications for several distinct literatures. First, and most directly, we provide new conclusions relevant to an existing debate about whether democratization leads to increased expansion of basic education to citizens. Several contributions have used either current or historical evidence to suggest that such an effect does indeed exist.² But there are also prominent examples of autocratic or oligarchic regimes that have pursued universal education policies, and in some cases more active policies than comparable democracies.³ Our conclusions suggest that in countries with weak state capacity, by which we mean that executive actions may often fail to translate into implemented outcomes, then democracy may indeed have an effect on basic education, but only on some policy dimensions and not on others.⁴

Second, our findings can apply more generally to other policies, such as basic health care, where policy involves both actions that can be directly implemented by an executive, such as the level of fees, as well as those areas, such as the supply of medicines, where executives can take actions but outcomes on the ground then depend on actions taken by subordinates. There are a number of political economy papers on the link between democracy and health outcomes, but they have not considered this differential mechanism.⁵

Third, our findings also have direct implications for more general debates about the role of information in facilitating electoral accountability. There are a number of contributions that investigate what happens to the relationship between voters and elected officials when voters suddenly acquire more information.⁶ But there have been far fewer attempts to consider how,

²See in particular Ansell (2010), Baum and Lake (2003), Brown and Hunter (2004), and Stasavage (2005).

³One of the best examples of this is the differential success of mass literacy programs in China and India over the past half century, as reviewed by Dreze and Loh (1995). Recently, Elis (2011) has established how an oligarchic regime in nineteenth century Argentina invested in mass education. Finally, the case of mass school construction in Indonesia under the Suharto regime, the effects of which were analyzed by Duflo (2001), provides another example.

⁴To date, the work by Ansell (2010) is the most detailed political economy study that examines how democratic politics influences different components of education policy.

⁵See in particular Kudamatsu (2010), Besley and Kudamatsu (2006), Ross (2006), and Blaydes and Kayser (2011).

⁶See in particular Besley and Burgess (2003), Stromberg (2004), Gottlieb (2010), Keefer and Khemani (2003), and Reinikka and Svensson (2004). See also the somewhat contrary evidence in Keefer and Khemani

holding constant a low information environment, election candidates and elected officials will face incentives to prioritize actions where voters can directly attribute outcomes to executive actions. There have been several theoretical papers that have proposed this mechanism, but no broad based empirical investigation of the possibility.⁷

Finally, our paper also serves as a useful complement to normative economic debates about the desirability of user fees for basic services in developing countries. While there is by now a very large economic literature on the desirability of removing user fees for health, education, and other services in developing countries, there have been few if any positive analyses that investigate when and why governments actually take this step in practice.⁸ We argue that if changing intellectual sentiment may have influenced the current move by many African countries to abolish fees for health or education, the nature of electoral competition in an environment of weak state capacity has also played a very important role.

We follow a three step empirical strategy to evaluate our core argument. We first show that within the countries in our sample, democracies have higher rates of school attendance, but this appears to be above all because democratic governments have focused on increasing access by abolishing fees, not because they have increased provision of resources, such as by hiring more teachers than non-democracies. As a second step, we demonstrate the link between democracy and fee abolition by showing that African governments have been particularly likely to implement the observable action of fee abolition in the immediate wake of competitive presidential elections. Finally, using survey evidence from Kenya we provide further support for our core argument by examining how highly observable and less observable education policy choices influence individual voting intentions.

Considering our empirical results in greater detail, we first use individual level data on school attendance in 29 countries, drawn from the Demographic and Health Surveys (DHS) program, to investigate whether children in African democracies are more likely to attend

(2010).

⁷These theoretical contributions, which we will consider below, include Mani and Mukand (2007), Ashworth (2005), and Bueno de Mesquita and Stephenson (2007).

⁸For surveys of recent (generally experimental) findings regarding user fees see Holla and Kremer (2009) and Duflo (2010).

primary school, and if so what the mechanism for democracy's influence appears to be. Our school attendance data is in the form of a cross section within which we have information on educational attainment for different age cohorts. Our estimation procedure is to regress a dummy variable measuring whether an individual attended primary school on a second variable that records whether an individual was of normal school age when their country was "democratic". By "democratic" we mean that a country had a chief executive elected in multiparty electoral competition. We also regress the school attendance indicator on a third variable recording whether schooling was free during the period when an individual was of school age. This indicator is constructed using original data that we have collected, with full details reported in the appendix to this paper. In our specifications we control for several types of omitted or unobserved factors that might otherwise lead to a spurious conclusion that either democracy or fee abolition has a causal effect on school attendance.

Our results show that when the fee abolition variable is omitted, the democracy variable has a coefficient that is close to statistical significance ($p=0.058$), and the implied effect of democracy is to increase the probability of school attendance by 4 percentage points. When we replace the democracy variable with the fee abolition variable, the latter is highly significant, and its implied effect on school attendance is larger (5 percentage points). Finally, when we include both the democracy and fee abolition variables simultaneously, the coefficient on the fee abolition variable is only slightly smaller whereas there is not a statistically significant coefficient on the democracy variable, and its magnitude is substantially attenuated. This evidence is certainly far from definitive, but it provides one indication that any effect of democracy on school attendance may be attributable above all to the proclivity of democracies for abolishing school fees.

If the data from the Demographic and Health Surveys provide us with an effective way of investigating school attendance over time, we lack similar high quality cross-country data sources involving provision of inputs or school quality. Observers of recent fee abolitions have suggested that while abolishing fees has undoubtedly improved access, this has often been accompanied by a significant deterioration in the quality of schooling, with one potential

reason being a dramatic increase in class sizes.⁹ As a preliminary means of considering this issue, we investigate the evolution of pupil teacher ratios across African countries. Our data suggest that, contrary to what one might expect, democracies actually tend to have higher ratios of pupils to teachers than do non-democracies. In addition, governments that abolish school fees have higher pupil-teacher ratios than do those in which fees are still applied, implying that fee abolitions are not accompanied by teacher hiring and school construction sufficient to keep class sizes stable. We also perform an analysis based on the number of potential pupils (the total school age population) relative to the number of teachers. The idea here is that this allows us to separate out the effect of teacher numbers and control for the fact that fee abolition will result in increased enrollments. Using this measure we observe some indication that governments that abolish fees actually employ fewer teachers relative to the size of their school age populations than do governments that charge for schooling. Moreover, there is no indication that democracies employ more teachers than non-democracies. Overall, our results regarding teacher numbers support the principal claim of this paper. In an environment of weak state capacity, democracy may prompt governments to increase access while having a more muted effect on the provision of inputs.

As referred to above, the second component of our empirical inquiry involves examining the conditions under which governments have abolished school fees. In Section 3 of this paper we present a new data set that records all recent episodes of primary school fee abolitions in African states, and we conduct an empirical analysis that points to a causal link between contested elections and fee abolition. At first glance we might think that external forces have been the primary force leading to the abolition of fees. Intellectual opinion among economists has swung against fees as increasing empirical evidence has accumulated demonstrating that even very low fee levels can have dramatic effects on service take-up by the poorest members of society.¹⁰ But the statistical evidence that we present in Section 3 suggests that changes

⁹Teacher absenteeism is also a very substantial problem. See Duflo (2010) as well as Banerjee and Duflo (2011) on the broad context in which fee abolition increases access without necessarily ensuring that new school entrants learn. See Habyarimana (2007) for a study of teacher absenteeism with striking evidence. Deininger (2003) considers these issues in light of Uganda's 1997 abolition of school fees. Bleck and Guindo (2010) consider the Malian context.

¹⁰For a recent comprehensive review on the issue see Holla and Kremer (2009). For an earlier argument

within African societies themselves have actually played as large, or perhaps an even larger role than external forces in prompting governments to make schooling free. It is hard to ignore evidence for an association between fee abolitions and the electoral calendar. A common pattern within numerous states has been for an election candidate to promise to abolish primary school fees if elected and then to implement the promise soon after his or her election. In Section 3 we present the first systematic evidence for this electoral link, and we provide evidence that the relationship may indeed be causal. This claim is based upon results of an instrumental variables estimation in which we instrument for election timing (which may be endogenous) using the original officially scheduled date for an election.

The third step in our empirical inquiry involves the use of survey evidence on education policy and voting intentions in Kenya. These data allow us to more directly examine a core assumption of our argument - that observable policy changes, such as the abolition of school fees, will influence voting behavior, but that outcomes for which responsibility is unclear (such as the existence of poor facilities) will be less highly correlated with voting intentions. Primary education has been a very salient political issue in Kenya following the Kibaki government's 2002 decision to abolish school fees, a decision taken to fulfill a recent election pledge. A survey from the third round of the Afrobarometer program (conducted in 2005) asked Kenyan interviewees several questions that are very relevant for tracing out the effect of education policy and outcomes on voting intentions. A first question asks respondents whether primary school should be free even if this implies a lower quality of education (due to lower levels of funding), or whether it was preferable to have fees in order to maintain quality. The Afrobarometer survey also contained a series of questions regarding school quality issues, asking whether respondents had directly experienced problems such as overcrowding, insufficient instructional materials, or classrooms in a poor state of repair. Finally, the Afrobarometer survey asked whether respondents would vote for President Kibaki if an election was held that day.

Normally, with survey responses of the sort described above we would want to be ex-

about the potential welfare benefits of user fees see Birsdall and Orivel (1996).

tremely cautious about concluding, for example, that a correlation between responses on the policy questions and support for President Kibaki imply that individuals supported Kibaki *because of* these policies. It might instead be the case that individuals with a prior preference for Kibaki would respond that they support his policies. The implication is that if we regressed an indicator variable for Kibaki support on a variable measuring support for free schooling and we obtained a positive coefficient, a result that we do indeed find, we would expect there to be an upward bias on this regression coefficient. We should logically expect the same bias for school quality responses; individuals with a prior preference for President Kibaki might be more likely to respond that their child's school was in good condition. Given this expectation, it is particularly interesting that when voting intentions for President Kibaki are regressed on any of the three school quality questions, there is not a statistically significant coefficient. Given the likely direction of any bias, we can take this as fairly strong evidence that school inputs and school quality do not have a significant impact on support for President Kibaki, in the way that, as the evidence suggests, increased access might.

Taken together, our three sets of empirical evidence suggest a way forward for researching electoral politics and basic service provision. In a context that applies in many developing countries, where voters observe outcomes on the ground but find it particularly difficult to know when to attribute them to executive actions, policy-oriented electoral competition may still exist. Under these conditions, competition is likely to hinge on the type of policies where it is possible for voters to assign credit or blame, forming the basis for a potential implicit contract between voters and election candidates.¹¹

2 Why Would a Government Abolish School Fees?

We suggested in the introduction that many scholars have debated the broad question of whether democratic transitions improve basic service provision, but that less progress has been made in understanding the mechanisms through which democracy might have this ef-

¹¹This is a prediction that has been made previously by Keefer and Khemani (2003).

fect. Our core claim is that in an environment in which decisions taken centrally may not lead to implementation on the ground, it is hard for citizens to verify whether or not candidates' campaign promises are actually kept. Therefore, in order to facilitate the formation of implicit contracts with voters, candidates will tend to focus on those policy promises that can actually be verified. As a consequence, if basic service provision is poor under autocracy then a democratic transition will primarily lead to policy changes on dimensions where outcomes can be clearly traced back to executive actions. In African countries in recent years candidates for presidential elections have often made extravagant promises regarding health, education, and development, promises that citizens no doubt heavily discount.¹² However, in certain cases candidates have also begun to make more specific promises, to abolish primary school fees, to abolish fees for health clinics, or to offer certain specific services, such as free maternal care. Local critics often suggest that such actions are "populist" or "demagogic" because governments lack the ability to effectively deliver services in the absence of fees. Setting aside the question of whether fee abolitions are actually welfare enhancing, an alternative interpretation is to see these actions as the natural development of policy-oriented campaigning in an environment in which voters face difficulties in tracing specific outcomes back to actions taken by a chief executive.

As we suggested in the introduction, there is by now a very extensive literature which suggests that there is better provision of basic services in environments in which voters have access to information from sources such as radio, newspapers, or public information campaigns. There has been much less effort to consider how politicians will respond to an environment of low information by altering certain types of policies but not others. In an environment of poor information combined with low state capacity, candidates in elections still need to construct electoral majorities in some manner. One option under these conditions is to construct support via patronage networks involving transfers of particularistic benefits. A less frequently considered possibility is that election candidates will seek to make concrete

¹²In Gabon's 2009 presidential election, for example, the opposition candidate Paul Mba Abessole campaigned with the somewhat ambitious slogan, "Free education, free medical care, employment and housing for all." (Xinhuanet.com, 29th August 2009).

promises, but only on very visible policies where their actions can actually be verified. Since the seminal paper by Holmstrom and Milgrom (1991), it has been observed that in principal-agent relationships in which agents pursue multiple tasks, and where effort with regard to some tasks is more easily observable, then agents will face incentives to bias effort towards those dimensions where their own efforts are most directly observable. There have been a small number of applications of this idea in a political economy context.¹³ Among these applications, the most directly relevant to our study is the contribution by Mani and Mukand (2007) who extend a standard retrospective voting model to a context in which a government is charged with producing two public goods with outcomes for one of the two goods being subject to more noise (where effort is therefore less “visible”). They predict that greater democratization (modeled as the likelihood that elections are held) prompts an elected official to widen the gap in resource allocation between the good in which effort is observable and the good in which effort is not easily observable.

We can apply the insights from the literature on multi-task principal-agent problems directly to the case of African primary education. For a candidate in an African presidential election, fulfilling a promise to abolish school fees constitutes an action that is not only very visible, but which is also easily attributable, in the sense that the executive’s own contribution can be established. In strong contrast, while a promise to hire more teachers or build more classrooms may also result in visible outcomes, in an environment of low state capacity these outcomes are less attributable, because it is much more difficult to judge the extent to which outcomes on the ground result from an executive’s direct actions. There is, however, one difference between the environment we consider and that in multi-task principal-agent models. While the multi-task models assume that an agent has a budget or time constraint and must then divide this between two tasks, the scenario we have in mind involves a single observable dimension of action (the choice of fee level) and a second unobservable dimension

¹³Holmstrom and Milgrom’s model is one in which the principal establishes an explicit contract for an agent charged with multiple tasks. Their insight was then extended to a context of implicit contracting (such as that found in a retrospective voting model) by Dewatripont, Jewitt, and Tirole (1999a, b). For political economy applications see in particular Mani and Mukand (2007), Ashworth (2005) and Bueno de Mesquita and Stephenson (2007).

(effort in using the education budget). Increased effort on one of these two dimensions does not necessarily imply diminished effort on the other dimension. We therefore make no claim that opting for free education prompts an official to make less effort with regard to provision of inputs. We simply suggest that democratization may influence behavior to a greater extent with regard to access rather than with regard to inputs or quality.

3 Correlates of Education Outcomes

We begin our empirical analysis by examining the correlates of education outcomes across African countries, focusing on school attendance and pupil-teacher ratios. The former provides a measure of access to education, or more explicitly of service take-up given conditions set by the government. The latter provide an indicator of the extent to which a government supplies a crucial education input. For each indicator we investigate first whether countries with chief executives elected in multiparty electoral competition tend to have systematically different education outcomes when compared with states in which executives are either unelected or are elected in single party contests. The results suggest that democracies have higher attendance rates, although by only a relatively small amount when compared with the non-democratic group. Contrary to what one might expect, however, democracies do not tend to provide more teachers than non-democracies. The next step in our analysis is to examine the potential mechanism through which democracy may be influencing education outcomes. Using information from an original dataset examining school fee abolitions in African states, we introduce a variable for the fee regime into our regressions. For school attendance we observe that countries in which primary school fees have been abolished have notably higher rates of attendance, and in addition once we control for the fee regime there is no longer a statistically significant difference between democracies and non-democracies in terms of attendance rates. In other words, democracy seems to matter because democratic governments are more likely to abolish school fees. For pupil-teacher ratios we observe a similar pattern. Democracies tend to have higher pupil-teacher ratios, but once we control for the fee structure of education (free or fee) this correlation is no longer statistically sig-

nificant, and in addition we do observe a very significant effect whereby countries with free primary education have higher pupil-teacher ratios. The broad message of these results may be that democracy prompts governments to increase access but not to increase inputs.

3.1 School Attendance

To measure access to education we have used information from the Demographic and Health Surveys (DHS) in 29 African countries to construct indicators of school attendance for members of different age cohorts. We restrict our attention to individuals born since 1980. Within each household surveyed the DHS records the level of educational attainment for each member. Since household members vary in age, we are then able to use this information to examine the correlates of educational attainment for different age cohorts.¹⁴ If multiparty electoral competition has a significant effect on school attendance, then we should observe that cohorts of individuals who reached the official entry age for primary school subsequent to a democratic transition are more likely to have attended primary school. The same test can be performed for school fee abolition.

$$S_{ihc} = \alpha + \beta_1 \text{Multiparty}_{ihc} + \beta_2 \text{Free}_{ihc} + \gamma \mathbf{X}_{ihc} + \mu_h + \varepsilon_{ihc} \quad (1)$$

We estimate equation (1) with a linear probability model, a method that facilitates inclusion of household fixed effects, clustering of standard errors by country cohort, and interpretation of marginal effects.¹⁵ For our dependent variable in these estimates we focus on a dummy indicator S that records whether individual i , in household h in country c , has attended primary school even if they did not complete primary school. The choice to

¹⁴Kudamatsu (2010) and Franck and Rainer (2009) are the only previous political economy papers that we are aware of to adopt this strategy using pooled DHS data for a group of African states.

¹⁵When estimating equation (1) without household fixed effects using alternatively a linear probability model and a logit model we obtained almost identical results across these two models, both with regard to the level of statistical significance for the coefficient on our free education variable, as well as with regard to its marginal effect. This provides some confidence that estimation of a linear probability model with household fixed effects will not produce a seriously misleading estimate. Use of an unconditional logit model with dummies incorporated for fixed effects is not practical for equation (1) because of the very large number of households. The final alternative of using a conditional logit model makes it difficult to obtain meaningful marginal effects from the estimates.

focus on school attendance, as opposed to school completion, is dictated by the fact that in a number of our sample countries fees have been abolished quite recently, and so the first cohort that has reached the official entry age subsequent to fees being abolished has not yet reached the standard age for leaving primary school.¹⁶ The specification in equation (1) controls for household specific fixed effects μ_h (and therefore also fixed effects at the country level). We also include three further controls denoted by the matrix \mathbf{X}_{ihc} . This includes a set of dummy variables for birth order, a set of dummy variables for birth year, and finally a dummy variable for females.

Our two principal variables of interest are those indicating whether an individual reached school age subsequent to either multiparty competition or free education being established. The variable *multiparty* takes a value of 1 for all cases in which an individual reached the official age of school entry subsequent to a country's becoming democratic. If an individual reached the normal school leaving age prior to a country becoming democratic, then *multiparty* takes a value of zero. Finally, in cases where an individual reached the normal age of school entry before a country was democratic, but the country subsequently became democratic before they reached the normal school leaving age, then *multiparty* takes a value between 0 and 1 that is equal to the proportion of their normal school age spent under democracy.¹⁷

The variable *free* is coded in an analogous manner to the *multiparty* variable. It takes a value of 1 for all individuals for whom fees were abolished prior to their reaching the normal age of school entry. For individuals for whom fees were in place until their normal age of school leaving, then *free* takes a value of 0. Finally, in cases where an individual reached the normal age of school entry before a country abolished fees, but the country abolished fees

¹⁶The most recent year for which we have data on educational attainment varies by country as detailed in the appendix.

¹⁷As mentioned above, "democratic" here refers to a country in which a chief executive is elected in multiparty competition. Data are from the Database of Political Institutions (Beck et al 2001), and are described in more detail in the Data Appendix. While there are numerous measures of "democracy" available, this is the most appropriate for the task at hand, since our theoretical argument focuses on the role of electoral competition. Repeating the analysis using data from the Polity IV and Freedom House databases (also described in the Data Appendix) provides further support for our primary conclusion, that school attendance is not increased by democracy per se, but by the propensity for democracies to abolish school fees

Country	Year	Following Election?	New leader elected?	Free and fair?	Victor %	Second %
Malawi	1994	yes	yes	yes	47	34
Ethiopia	1994	no				
Uganda	1997	yes	no	yes	74	24
Lesotho	1999	yes	yes	yes	98.8	1.3
Cameroon	2000	no				
Sierra Leone	2001	no				
Tanzania	2001	yes	no	yes	75	17
Zambia	2002	yes	yes	no	28	27
Rwanda	2003	yes	no	no	95.0	3.6
Kenya	2003	yes	yes	yes	62	31
Mozambique	2004	yes	yes	yes	63.7	31.7
Burundi	2005	yes	yes	yes	54.2	25.4
Ghana	2006	no				
Liberia	2006	yes	yes	yes	27.2	19.0
Benin	2006	yes	yes	yes	35.6	24.2
Congo (Brazza.)	2007	no				

Table 1: **Primary School Fee Abolitions in Africa (1990-2007)**. See text and appendix for full description of the data and sources. Following Election is coded "yes" if an election occurred in the same year or the year preceding a fee abolition. Free and fair is coded yes if an election was judged by international observers to have been free and fair as coded by Lindberg (2006). Victor % and Second % show the proportion of votes garnered by the winner and runner-up, respectively.

before they reached the normal school leaving age, then *free* takes a value between 0 and 1 that is equal to the proportion of their normal school age in which schooling was free. We have used a wide variety of sources to construct a new data set that records each instance in which an African government since 1990 has abolished primary school fees. All episodes of fee abolition are listed in Table 1.¹⁸

Before proceeding further, we should emphasize that pupils in African primary schools are subject to a range of potential fees, and our dataset certainly does not fully capture this variation. Common fees include official tuition fees sanctioned by a government, unofficial fees levied by associations (often referred to as PTA fees), fees for uniforms, and fees for sitting exams. When African governments have abolished fees in recent years this has most commonly applied to official tuition fees, where such fees exist, while also often including provisions regarding association fees levied by schools. We have classified a government as having abolished fees if there is clear evidence that a government has introduced and implemented a law or ministerial decree abolishing tuition or PTA fees. Given the nature of the information we have available, there is probably little risk that we have ignored significant fee abolitions. There is a somewhat greater risk that we have incorrectly coded a government as abolishing fees when in practice the move had only a minimal effect on the cost of primary education. This should bias our school attendance estimates against finding an effect of fee abolition.

Table 1 provides a list of the sixteen fee abolition episodes that we have identified as occurring between 1990 and 2007, the context in which this occurred, and the date. In eleven of the cases fees were abolished in the immediate wake of an election. It is particularly interesting to note that among these eleven cases, eight occurred when a new leader was elected. The fact that it was principally new leaders who took this step suggests two reasons why fee abolitions happened immediately after, rather than immediately prior to, elections. First, they weren't in office prior to the election. Second, if they do not have a well formed reputation upon election, then they might face a particularly strong incentive to engage in

¹⁸The appendix provides a complete list of the documentation used to classify each abolition (or non-abolition).

highly visible policy changes early in their term of office in order to cement their reputation for the future.¹⁹

We will estimate specifications in which we include *multiparty* and *free* jointly, as well as specifications where we include them separately. The objective will be to assess whether democracy may have an effect on school attendance rates, and whether the effect of democracy may be attributable above all to the fact that democratic governments are more likely to abolish school fees. We do not estimate specifications with an interaction term *multipartyXfree* because in our sample fees have been abolished almost exclusively in countries where leaders are selected in multiparty elections.²⁰

Table 2 reports the results of eight different specifications. In each of these specifications standard errors are clustered to allow for arbitrary correlation within “country cohorts”, with a country cohort defined as all individuals born in a given country in a given year. This is important since our multiparty and free schooling variables do not vary within country cohorts. In the first specification we see that, on average, children of school age in democracies are more likely to attend at least some primary school than are children in non-democratic contexts, though the coefficient on multiparty is not quite significant at conventional levels ($p=.058$). The implied “effect” of democracy here is a 4 percentage point increase in the probability of having at least some primary schooling. The second column reports results of an estimate in which we substitute the “free schooling” variable for the multiparty variable. We observe a positive and statistically significant β_2 coefficient and the implied effect of fee abolition is relatively large. Abolishing school fees at the outset of a child’s normal school years is estimated to increase the probability that they have at least some schooling by 5.5 percentage points. In the third column we consider the full specification in which both the *multiparty* and *free* variables are included. In this specification the coefficient on the

¹⁹This behavior would be consistent with the dynamic suggested by Ashworth (2005).

²⁰The sole exceptions are Burundi and Ethiopia. Ethiopia abolished school fees in 1994, the same year in which it had an election to a constituent assembly. However, it did not have a multiparty legislative election until 1995. In the case of Burundi, the government abolished fees following a multiparty election in 2005, but Burundi is not actually coded as having an executive elected in multiparty elections in the Beck et al data set. This is presumably because Burundi’s president was not yet directly elected by popular vote as is now the case.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	All individuals			Estimates by Wealth Quintile				
				1st	2nd	3rd	4th	5th
Multiparty Democracy	.040 (.021)		.029 (.022)	.010 (.027)	-.006 (.028)	.001 (.026)	-.019 (.027)	-.023 (.016)
Free schooling		.055 (.021)	.042 (.021)	.023 (.027)	.056 (.028)	.049 (.027)	.057 (.026)	.008 (.017)
Female	-.060 (.004)	-.060 (.004)	-.060 (.004)	-.065 (.006)	-.064 (.005)	-.060 (.004)	-.055 (.004)	-.052 (.004)
Household fixed effects	yes	yes	yes	yes	yes	yes	yes	yes
Birth year dummies	yes	yes	yes	yes	yes	yes	yes	yes
Birth order dummies	yes	yes	yes	yes	yes	yes	yes	yes
R ² (within)	0.07	0.07	0.07	0.10	0.10	0.09	0.08	0.04
N=	522,914	522,914	522,914	90,331	87,094	92,713	96,018	110,063

Table 2: **Estimates of Probability of Individual Having Any Schooling.** Linear probability model with standard errors clustered at the country-cohort level.

multiparty competition variable is no longer close to being statistically significant, and it is substantially smaller than in column (1). In strong contrast, the implied effect of a shift to free primary education is only slightly smaller than in the column (1) estimates, and it remains statistically significant.

The evidence from the specification in column (3) is consistent with our interpretation that African democracies tend to have higher rates of school attendance primarily because democracies are more likely to abolish school fees. However, we should quickly acknowledge that the presence of unobserved factors in the data might be leading us to an erroneous conclusion on this question.²¹ One possibility is that when democracies abolish school fees they also take other policy steps, such as building more schools and hiring more teachers, and these factors, which are unobserved in our specification, might also influence attendance. A second potential concern is that governments abolish fees when there is a change in perceived economic returns to schooling, and changes in perceived economic returns to schooling have a direct effect on decisions by families whether to send their children to school.

The specifications in columns (4) through (8) in Table 2 repeat specification (3) for separate quintiles of the wealth distribution. The DHS surveys include a variable for household wealth that is constructed through factor analysis of questions regarding a number of different household assets.²² The index is then divided into quintiles. It should be emphasized that the wealth quintile measure we use here will be a noisy indicator of true household wealth because while the wealth quintile is constructed by country, our estimates are pooled across countries. This will introduce a further degree of noise to the extent that, for example, a household in the second quintile in the Central African Republic will be significantly poorer than a household in the second quintile in a richer country like Senegal. With these caveats in mind, the results of the specifications in columns (4) through (8) suggest that fee abolition primarily benefits poorer families. There is no effect of fee abolition for the richest quintile (column (8)), and then a larger effect of abolition on attendance for the middle quintiles. The exception concerns the bottom quintile for which the effect of fee abolition is estimated to be

²¹See Green, Ha, and Bullock (2010) on this question.

²²See Filmer and Pritchett (1999) for a discussion of this method for measuring household wealth.

smaller than for households in the second quintile. This is a result that we would expect; even after fees are abolished, households still face significant direct costs of schooling, such as those for uniforms, in addition to facing opportunity costs of foregone economic activities by their children. It is logical that for a range of the poorest families, abolition of school fees will therefore not prompt them to send their children to school.²³ Finally, it should also be noted that when compared with the estimates that pool together households from all wealth quintiles, in the separate estimates by wealth quintile there is even less evidence for an effect of democracy independent of fee abolition

What do the estimation results from Table 2 suggest? They provide an indication that there is an effect of democracy on African primary education, and furthermore this effect may be due above all to the fact that democratically elected African governments are more likely to abolish fees. We should emphasize that this evidence is certainly only preliminary. Though we have adopted a robust estimation strategy, there remains the possibility that unobserved and omitted factors, such as a shift in the demand for schooling, might simultaneously prompt governments to abolish school fees and to expand education. One way to tentatively explore this possibility is to add country specific linear time trends to the specification in equation (1). When doing so we continued to observe very similar results regarding democracy and fee abolition. Even so, it is certainly possible that some unobserved factors, such as sudden increases in returns to schooling, may be correlated with either fee abolitions or democratization and may not be adequately controlled for even with the inclusion of country specific time trends. Finally, as we suggested above it may be the case that the positive coefficient on the school fees variable is picking up the effect of other reforms that governments launch simultaneously with fee abolition, such as new investments in teachers and schools. Given available data, this issue is difficult to deal with in a cross-country comparative context. However, in the next sub-section we will examine evidence on numbers of teachers and pupil-teacher ratios as one such possibility. At least as far as

²³Another prominent issue is whether abolishing school fees leads to a reduction in gender differentials in school attendance rates, a result that Deininger (2003) found for Uganda. Using our pooled DHS data we do not find evidence of differential effects by sex.

teacher numbers are concerned, the evidence in the next section will show that governments which abolish fees do not hire more teachers when compared with governments that maintain fees.

3.2 Numbers of Teachers

While the DHS surveys provide us with high quality data on school attendance that can be used in a comparative setting, we lack a similar source when it comes to school inputs or quality of instruction. This in itself is not surprising because school quality is inherently difficult to measure, and school inputs are costly to catalogue.²⁴ As one feasible measure that is affected by school inputs, we will focus on the cross-national data on numbers of teachers that is reported in the African Development Indicators and which is originally collected by UNESCO. This data, which is based on self-reported questionnaires compiled by national governments, may certainly contain a substantial degree of error and/or bias. Our sample for this analysis includes 38 countries over the period between 1990 and 2007, but this is a highly unbalanced panel due to the large number of missing observations.

We will use two separate dependent variables in our analysis here. The first is the ratio of pupils in school to the number of teachers - *pupils/teachers*. The second dependent variable is the number of children under age 15 in a country (our best proxy for the school age population) divided by the number of teachers - *potential pupils/teachers*. The first variable is a measure of input provision conditional on actual demand for education. The second variable is a measure of input provision relative to potential total demand.

We will examine whether African democracies have systematically different teacher numbers (with y representing one of our two dependent variables) once we control for country fixed effects and common time effects. We will also examine if the presence or absence of

²⁴Some data do exist on expenditures on primary and total education, but a simple number suggesting that a government budgeted a certain percentage of GDP for primary education may provide only a very weak indication of implemented outcomes on the ground. Moreover, any campaign promises made by a government are likely to focus on actual inputs, such as hiring teachers and building schools, as opposed to focusing on a spending target.

primary school fees is associated with different teacher numbers.

$$y_{it} = \alpha + \beta_1 \text{multiparty}_{it} + \beta_2 \text{fees}_{it} + \mu_i + \eta_t + \varepsilon_{it} \quad (2)$$

In equation (2) above the dependent variable is either *pupils/teachers* or *potential pupils/teachers*. Within our sample, the mean value for the former is 47 and the standard deviation is 12. The sample mean for the latter is 166 and the standard deviation is 90. We regress our two dependent variables on: (1) A dummy variable that takes a value of 1 if a chief executive is elected in multiparty competition and zero otherwise, (2) a dummy variable for whether primary school fees are in place at the beginning of the year, (3) a set of country fixed effects (μ_i), and (4) a set of year dummies (η_t). Standard errors are clustered by country.

Three specifications are reported in Table 3 that use the actual number of pupils divided by the number of teachers as the dependent variable. We see in the first specification that democracies actually tend to have higher ratios of enrolled pupils to numbers of teachers when compared with non-democracies. In the second specification we observe that countries with primary school fees in place are estimated to have roughly 8 fewer students per classroom than are countries in which fees have been abolished. This is a large effect, representing two thirds of a standard deviation. This fits with what has been observed in a number of countries in which school fee abolitions have resulted in significant increases in enrollments, and even after a number of years (taking account of the fact that new teachers cannot be recruited instantaneously) pupil-teacher ratios have remained above pre-reform levels. However, we cannot, of course, tell from this estimate to what extent higher pupil-teacher ratios in countries that have abolished fees are attributable to increased demand for education versus changes in the supply of teachers. Now consider the third column in Table 3, in which the multiparty and fees variables are included simultaneously. The coefficient on the fees variable remains virtually unchanged from the specification in column 2. In contrast, the coefficient on the multiparty variable is now substantially smaller than in the column 1 estimation, and it is no longer statistically significant.

	(1)	(2)	(3)	(4)	(5)	(6)
	Enrolled Pupils			Potential Pupils		
Multiparty Democracy	4.40 (2.16)		3.14 (1.96)	0.80 (14.4)		-1.28 (14.5)
School Fees in place		-8.14 (2.52)	-7.85 (2.70)		-24.7 (12.4)	-22.7 (12.3)
Year fixed effects	yes	yes	yes	yes	yes	yes
Country fixed effects	yes	yes	yes	yes	yes	yes
R ² (within)	.086	.167	.182	.382	.432	.413
N=	341	364	330	329	351	318

Table 3: **Estimates of Pupil-Teacher Ratios.** OLS estimates with standard errors clustered at the country level.

The specifications in columns (4) through (6) in Table 3 use the potential number of pupils divided by the number of teachers as a dependent variable. Using the potential number of pupils provides us with one way of focusing on education inputs independent of take-up of educational services. In these specifications we see essentially no evidence that democracies provide different numbers of teachers relative to the potential student population when compared with non-democracies.

How should we interpret the above results? First of all, any interpretation we attempt should be made with caution. With this said, it is striking how little evidence we see in Table 3 to support the idea that democracies might increase education provision by employing more teachers or that governments abolishing school fees will make accompanying efforts to hire substantially more teachers. Teacher numbers have been increasing across the board within the set of African countries in our sample, a fact controlled for by the year fixed effects in these regressions. But once we control for this fact, there is no indication that democracies appear to be any different from non-democracies and no indication that countries abolishing fees make sufficient investments in inputs to keep pupil-teacher ratios stable.

4 Presidential Elections and Fee Abolition

In the previous section we established that multiparty electoral competition is associated with higher rates of school attendance (potentially reflecting greater access) as well as with higher pupil-teacher ratios (potentially suggesting that inputs have not increased in step). However, once we control for whether school fees are present, democracy is no longer significantly correlated with these two education outcomes. This suggests that if electoral competition has recently made a difference for primary education in African countries, it is above all through democracy's effect in prompting governments to abolish school fees. In this section we continue the inquiry by examining the conditions under which governments have abolished fees. We find that governments have been particularly likely to abolish fees in the immediate wake of presidential elections. This supports our interpretation that a promise to abolish school fees is a declaration that can be made in a campaign and which can subsequently be

subject to verification, even in an environment of weak state capacity. In our analysis we pay particular attention to the possibility that any observed correlation between elections and fee abolition might be endogenous - elections might only take place in “good times” where it is easier to deliver on a promise to abolish fees.

Our primary purpose in this section is to consider the conditions under which African governments have switched from allowing primary schools to levy fees to a no fees regime. Since over the last two decades this has been very much a one-way movement, with no governments that abolished fees officially reinstating them, it makes sense to conduct a survival type of analysis in which we examine how long a government “survives” with fees before abolishing them. To do this in a simple but sound way we estimate the following equation in which a dummy variable *abolition* takes a value of zero for all years in which a country has primary school fees, a value of 1 in the year in which fees are abolished, and then the country is dropped from the sample for all subsequent years. This ensures that we are estimating the probability of a fee abolition.²⁵

$$\Pr(\text{abolition})_{it} = F(\alpha + \beta_1 \text{election}_{it} + \mu_i + \eta_t + \varepsilon_{it}) \quad (3)$$

In equation (3) the probability of fee abolition in country *i* in year *t* is estimated for a sample of 39 African countries between 1990 and 2007. We include a dummy variable, *election*, that takes a value of one if a country has experienced a presidential election in the current or the previous year and zero otherwise.²⁶ If we wanted to test the proposition that candidates are most likely to promise to abolish fees if an election is expected to be particularly competitive, then we might want to use a measure that also incorporates further information about the electoral environment. However, in doing so we might then introduce a greater possibility of endogeneity bias in our regressions. Factors causing an election to be competitive might also have a direct effect on the feasibility of abolishing fees. As a

²⁵ following Beck, Katz, and Tucker (1998)

²⁶ This is a simple way of taking account of the fact that a fee abolition might occur swiftly after an election but not in same calendar year. This was the case, for example, with Kenya in 2003. It is worth noting that in practice, all of the electorally connected fee abolitions in our sample have occurred subsequent to elections rather than prior to them.

consequence, for our core estimates we will stick with a more minimalist measure of whether an election occurred irrespective of the electoral environment, though we will consider several alternative measures below. Even with our minimalist measure, we might still be concerned that there is a risk of bias in our estimates. Whether an election takes place at all depends on certain conditions, such as a modicum of political stability, and the decision by an incumbent regime to actually face the electorate. It may be that when these conditions are favorable, conditions are also more favorable for abolishing school fees, perhaps because a country’s public finances are relatively sound. To deal with this possibility, we will also present instrumental variables estimates in which we instrument for whether an election occurs using a variable based on the officially scheduled election date, as determined at the previous election. In addition to the *election* variable, we also include a set of year fixed effects (η_t), to take account of the possibility that as time has elapsed since 1990, systemic features, which could include pressure from donors or changing ideas about optimal policy, have made it increasingly likely that any country would abolish primary school fees. Finally, in some specifications we also control for country fixed effects (μ_i).

There is some debate whether the most desirable way to estimate an equation with an endogenous dummy variable (as is the case with the election variable in equation (3)) is with a non-linear model (such as probit and ivprobit) that is constrained to produce estimated probabilities between 0 and 1, or, alternatively, whether a linear probability model (estimated via either OLS or 2SLS) is preferable because it is not dependent on as restrictive a set of assumptions.²⁷ In practice we obtained quite similar results using both approaches. As a result, in this section we will report the linear probability model estimates, which are more straightforward to interpret. In the “further results” section of the appendix we then report probit and instrumental variables probit estimates of equation (3).

Table 4 presents twelve different estimates of the probability of fee abolition. In the first column the election variable is coded so that it takes a value of 1 if there has been an election in the previous or current year, irrespective of whether the election was judged to be free and

²⁷See Angrist (2001).

fair or whether the election outcome was particularly lopsided. Here we observe that the coefficient on the election variable is statistically significant. The implied effect of having an election is also relatively large. In a non-electoral year we would expect a country to have a 1.3% chance of shifting to a no fees regime. In an electoral year we would expect this probability to rise to 5.8%. For the estimate presented in column (2) we alter the definition of the “election” variable, coding as 0 all cases in which there was an election but it was not judged by international observers to be free and fair.²⁸ When we do this we observe that the coefficient on the election variable remains statistically significant. The implied magnitude of the effect is now also substantially larger. For the estimate in column (3) we reclassify particularly lopsided elections (those where the winner’s share is greater than 80%) as being cases where the “election” variable is set equal to zero. This is based on the idea that a very lopsided election outcome may indicate that the election outcome was preordained and therefore there was less need to form any sort of a contract with voters over abolishing school fees. In column (3) the coefficient on the election variable remains statistically significant and of similar magnitude to that in the column (1) estimate. The three specifications in columns (4), (5), and (6) repeat the initial specifications while including a full set of country fixed effects. These will control for any constant and observed differences between countries, such as those due to colonial heritage, geographic location, or the fixed length of electoral terms, in addition to any constant and unobserved sources of heterogeneity. As can be seen, the results when including country fixed effects are very similar to those reported in the first three columns.

The estimates in the first six columns of Table 4 control for a number of different possible time specific or country specific factors that might produce a spurious correlation between presidential elections and the abolition of school fees. Nonetheless, it is still entirely possible that these results are influenced by a form of endogeneity bias in which some time variant and country specific factor simultaneously prompts rulers to hold elections and to abolish school fees. It might, for example, be the case that when economic growth is robust and/or public

²⁸This is based on the data set collected by Staffan Lindberg (2006) that he has subsequently updated.

Presidential election variable	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	Linear Probability Model						Instrumental Variables (2SLS)					
Election (any)	.045 (.019)			.052 (.020)			.075 (.023)			.080 (.026)		
“Free and fair” election		.072 (.030)			.076 (.032)			.126 (.042)			.130 (.044)	
Election & winner share < 80%			.043 (.020)			.050 (.021)			.083 (.026)			.089 (.028)
Year fixed effects	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Country fixed effects	no	no	no	yes	yes	yes	no	no	no	yes	yes	yes
R ²	.051	.064	.050	.135	.149	.132	.045	.048	.039	.130	.136	.123
First stage statistics												
F statistic on excluded instrument							243	80	217	210	67	191
Partial R ² excluded instrument							.574	.362	.505	.579	.366	.510
N=	619	577	619	619	577		600	558	600	600	558	600

Table 4: **Estimates of Likelihood School Fees Will be Abolished.** Dependent variable takes a value of 0 for all years with fees, 1 in year of abolition, and the country is dropped from data set for subsequent years. Standard errors are clustered at the country level. In 2SLS estimates the excluded instrument is "scheduled election".

finances are sound, rulers will be more willing to face the electorate and simultaneously more able to eliminate school fees. To deal with this possibility, in columns (7) through (12) of Table 4 we present a set of instrumental variables estimates. We construct an instrumental variable, called “scheduled election”, that is defined in the following manner. The “scheduled election” variable takes a value of 1 for years in which an election would normally be scheduled, counting forward from a country’s previous presidential election (of any sort).²⁹ Since all of our sample countries have had executive elections of one sort or another, this is an instrumental variable that can be calculated for each country in the sample. Since the vast majority of African countries have presidential systems with fixed terms, in practice the “scheduled election” variable is a very strong predictor of the “election” variable.³⁰ We also need to have confidence that the exclusion restriction for this instrumental variable is satisfied. The exclusion restriction would be violated if when choosing an election date, a leader decided to hold an election in year $t + 1$ as opposed to year t because they were taking into account the difference between anticipated future conditions in year $t + n$ as opposed to year $t + n + 1$ (where n is the constitutional electoral term). More concretely, and at the risk of repetition, in a country with a five year presidential term it would have to be the case that a president would delay an election scheduled for 2002 for one year, not only because of his assessment that the likelihood of re-election would be greater in 2003, but also because of an assessment that the likelihood of victory would be higher for a subsequent election held in 2008, as opposed to 2007. This seems implausible.

In columns (7) through (12) of Table 4 we present the instrumental variables estimates. In each case with these estimates the coefficient on the election variable is statistically significant, and it is now also larger in magnitude than in the previous estimates contained in columns (1) through (6). The larger magnitudes for the coefficient on the election variable might be explained by the fact that when rulers alter the timing of elections away from a

²⁹In cases where a country held a presidential election but then failed to hold one at the scheduled date, the instrument is constructed by continuing to count forward. So, for example, Angola had its last presidential election in 1992, and the official presidential term is five years. For Angola "scheduled election" takes a value of one in 1992, 1997, 2002, and 2007. It takes a value of zero in all other years.

³⁰The non-presidential regimes in our data set are Lesotho, Ethiopia, and Burundi (though Burundi later shifted to presidential elections). For these countries we focus on legislative elections.

previous schedule, then they are less likely to offer policy changes such as the elimination of school fees. With this said, we should certainly not over interpret these results. Given the confidence intervals for both the OLS and 2SLS estimates and our relatively small sample size, the difference in coefficients may simply be attributable to sampling variance. We can also see from the first stage statistics at the bottom of Table 4 that our 2SLS estimates definitely do not suffer from a weak instruments problem.

In addition to the instrumental variables estimates, we also considered a number of additional (unreported) specifications in order to establish whether the correlation between presidential elections and school fee abolitions may be driven by an omitted variable. We first investigated whether the fact that governments abolish school fees following elections simply reflects the fact that they are likely to abolish any type of policy immediately after they have been given an electoral mandate. As one way of considering this, we investigated whether governments were more likely to alter trade policy (in this case measured by the average simple tariff) following elections. There was no indication that this was the case.

Second, we also considered the possibility that a country's status with the donor community influences the decision to abolish school fees. In practice, countries receiving higher levels of total overseas development assistance were slightly more likely to abolish school fees. However, the most observable pattern in the data was, not surprisingly, that governments that abolished fees tended subsequently to receive more aid after this policy move. This raises the possibility of the following endogeneity problem - governments might be in good standing with donors if they were democracies, and therefore they might be more likely to abolish fees because they would anticipate substantial aid flows following this decision. In this case the effect of electoral competition on fee abolition would follow an indirect path, rather than the direct path that we have suggested. However, if electoral competition had only this indirect effect, then we would not expect democracies to be particularly likely to abolish fees in the immediate wake of elections as opposed to during any other year. Since we do find this, we retain confidence in our hypothesized mechanism.

Overall, the estimates in Table 4 provide a strong indication that elections and primary

school fee abolitions have been strongly correlated and that there is very likely a causal relationship between the two variables.

5 Survey Evidence on Voter Intentions in Kenya

So far we have argued that electoral competition has prompted African election candidates to promise actions upon which they can visibly deliver, such as abolishing school fees, but it has not allowed candidates and voters to enter into implicit contracts over education inputs or school quality. As evidence to support this claim, we have shown that African democracies tend to have higher rates of enrollment, combined with higher pupil-teacher ratios, and both of these effects appear to be driven above all by the abolition of school fees. We have then provided estimates to show that having an election significantly increases the likelihood that an African government will abolish fees. What we have not provided is more direct evidence to show that voting behavior might actually be influenced by education outcomes and education policy. To do this we now make use of a survey of individuals in Kenya that was implemented after the Kenyan government's abolition of primary school fees, a decision made in December of 2002. While promises to make primary education free had been made at various times in Kenya since independence, this was the first instance of a concrete promise made in the immediate run up to a multiparty election.³¹ It was a promise that the victorious opposition candidate, Mwai Kibaki, soon acted upon with the announcement that as of 1 January 2003, primary schools in Kenya would no longer be allowed to levy fees. Subsequent to this decision, there was a large enrollment response. This was combined with the observation that class sizes in many schools grew very considerably, and complaints began to emerge that without sufficient increases in education inputs (i.e. teachers, classrooms, materials), education quality would suffer.³²

In what follows we first consider whether survey evidence shows that individuals from poorer households are more likely to suggest that they favor a free primary education policy

³¹See the interesting discussion in Oketch and Rolletson (2007) for background.

³²An excellent survey of overall developments in the Kenyan educational system is provided by Lucas and Mbiti (2010).

even if this involves compromising quality. Such a finding would provide further support for the core argument of this paper. We then use the survey evidence to examine whether expressed intention to vote for President Kibaki is correlated with a policy outcome where his actions are observable (school fee abolition) as well as with outcomes where his actions were not observable (provision of inputs).

5.1 Preferences for Free Schooling

Two years after the abolition of school fees, round 3 of the Afrobarometer survey was conducted in Kenya, with respondents asked whether they would vote for President Kibaki if an election were held that day. In addition, interviewees were asked whether they preferred to have primary education be free even if this meant lower quality of education, or whether they instead preferred that fees be charged so as to maintain standards.³³ In the original Afrobarometer data individuals were given the option of agreeing strongly with this proposition, agreeing, disagreeing, disagreeing strongly, or responding that they “did not know”. In practice very few individuals responded that they “did not know”, and in the regressions reported in this section we lose little estimation precision by excluding the “don’t knows” and then dichotomizing responses between individuals who either agreed (strongly or not) or disagreed (strongly or not) with the proposition. In the remaining analysis in this section we will therefore refer to a dichotomous variable “free” which takes a value of one if the individual responded that schooling should be free and zero otherwise.

The pattern of responses to the free schooling question varied between wealth quintiles in exactly the way we might expect given the estimation results reported above using the DHS survey data. This provides further support for the main argument of this paper as it pertains to distributional politics. To demonstrate this we used responses to six Afrobarometer questions involving household assets to construct a wealth index in the same way that the DHS wealth index is constructed.³⁴ We then divided this index into quintiles and investigated

³³See the data appendix for the exact wording of this and all other Afrobarometer questions used in this section.

³⁴The Afrobarometer survey includes questions on only a small number of goods, but these still may be useful for constructing a wealth index. The six goods included are whether the household has a book, a radio,

how responses to the fees question varied by wealth quintile using the following specification.

$$\Pr(\textit{free} = 1)_{ij} = \Phi(\alpha + \beta\mathbf{W} + \mu_j + \varepsilon_{ij}) \quad (4)$$

In equation (4) the probability of preferring free education is estimated as a function of a matrix of dummy variables for each wealth quintile \mathbf{W} , in addition to a set of district fixed effects (μ_j). The estimated likelihood of saying that education should be free was highest for the lowest quintile and decreased with each successive quintile. The differences were quite dramatic, with 60% of individuals in the bottom quintile saying that education should be free, even if this compromises quality, whereas only 38% of individuals in the top quintile responded in the same manner.³⁵ There continued to be a statistically significant difference in responses to the fees question between wealth quintiles even when we also introduced a set of fixed effects for self-identified ethnic affiliation. This is significant because of the fact that wealth levels certainly do vary in Kenya between ethnic groups.³⁶

In sum, the Afrobarometer evidence provides a strong indication that African respondents are sensitive to the distributional effects of free education policies and that there is a trade-off involved. This is a particularly interesting result given the paucity of recent academic discussion of distributional politics involving different income or wealth groups in Africa, as opposed to distributional considerations between ethnic groups.³⁷

a television, a bicycle, a motorcycle, and a car. Using factor analysis we constructed an index based on the first factor from responses to these six questions.

³⁵Based on a probit estimation with standard errors clustered at the district level, the predicted probabilities derived from the probit estimation are as follows. An individual from a household in the 1st (bottom) quintile is estimated to have a 60% probability (standard error .027) of preferring free schooling. An individual in the 2nd quintile would have a 53% probability (standard error .026). An individual in the 3rd quintile would have a 53% probability (standard error .026). An individual in the 4th quintile would have a 45% probability (standard error .037). An individual in the 5th quintile would have a 38% probability (standard error .035).

³⁶We were also able to repeat the above exercise for a pooled sample including responses from all countries in round three of the Afrobarometer survey. We found a similar relationship; individuals at successively higher quintiles of the wealth distribution were less likely to respond that they favored free education.

³⁷Though see the interesting work by Resnick (2010) in this regard.

5.2 Voting Intentions

In addition to asking respondents whether schooling should be free, the Kenya Afrobarometer survey also included several specific questions about experience with local schools. We can use this information to try to establish whether responses on the free schooling and school conditions questions are correlated with the expressed intent to vote for the incumbent, President Kibaki. The Afrobarometer survey asked whether individuals had experienced problems with schools involving overcrowding, lack of materials, or poor facilities. We might expect that individuals experiencing any of these three problems might be less likely to express willingness to vote for President Kibaki to the extent that these problems reflected insufficient efforts at the central level to steer funds toward schools. To consider this question we use the following model to estimate voting intentions for President Kibaki as a function of the different survey responses.

$$\Pr(\text{kibaki} = 1)_{ij} = \Phi(\alpha + \beta_1 \text{free}_{ij} + \beta_2 \text{school problem}_{ij} + \mu_j + \varepsilon_{ij}) \quad (5)$$

In the above equation, an indicator variable for expressed intention of individual i in district j to vote for Kibaki if an election were held that day is regressed (using a probit model) on two survey response variables in addition to a set of district fixed effects that control for unobserved factors that might influence Kibaki support. The dichotomous variable “free” involving preference for free schooling remains defined as above. The variable “school problem” takes a value of 0 if the individual has never experienced the problem in question, 1 if the individual has experienced the problem “once or twice”, 2 if the problem has been experienced “a few times”, and 3 if the problem has been experienced “often”. Respondents were also given the option of saying that they had no experience with schools (presumably because they did not have school age children), and we excluded these individuals from the sample, hence the smaller sample size for some regression. Three separate school problems are considered, each of which reflects a lack of inputs: (1) overcrowded classrooms (2) poor conditions of facilities, and (3) problems with textbooks and supplies.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Respondents prefer free schools	.276 (.112)				.303 (.149)	.300 (.154)	.316 (.152)
Experienced overcrowded classrooms		-.065 (.070)			-.073 (.069)		
Experienced poor classroom facilities			-.089 (.082)			-.075 (.083)	
Experienced problems with textbooks				-.073 (.057)			-.078 (.063)
N=	936	554	549	559	524	520	529

Table 5: **Probit Estimates of the Probability of Expressing Intention to Vote for President Kibaki.** All specifications include district level fixed effects, and standard errors are clustered at the district level.

Table 5 reports results of seven different estimates of equation (5) with standard errors clustered at the district level. In the first column we see a positive and statistically significant correlation in which individuals preferring free primary schooling are more likely to express the intention of voting for President Kibaki. The implied effect of free schooling here is also large, an individual believing that schooling should be free is estimated to have a 57% probability of voting for Kibaki whereas an individual believing otherwise is estimated to have a 48% probability of voting for Kibaki. We must remember, of course, that there is a severe risk of bias in these estimates, and they should not be given a causal interpretation. Individuals with a prior preference for President Kibaki might be likely to take his actions on free schooling as a cue that determines their response to the free schooling survey question.

Columns 2, 3, and 4 in Table 6 consider the correlation between Kibaki voting intentions and reports of school quality problems. Interestingly, there is no evidence whatsoever that individuals who report having experienced problems with overcrowding, lack of supplies, or poor facilities are less likely to say that they would vote for President Kibaki. This is particularly noteworthy, because we would normally expect there to be a bias on the β_2 coefficient for the reason described in the previous paragraph. In columns 5 through 7 we estimate the full specification including the response to the fees question together with responses on the different school quality questions. Here we continue to observe that individuals preferring free schooling also express the intention to vote for President Kibaki, but experiencing school problems has no apparent correlation with Kibaki voting intentions. We also estimated an (unreported) specification in which we used factor analysis to produce a single variable drawing information from three school conditions questions and then entered this into the regression. We continued to observe no statistically significant relationship between reported experience with school conditions and Kibaki voting intentions, and the coefficient on free schooling remained significant.

The major implication of the estimates in Table 6 is that if individuals do not blame a chief executive for problems experienced by their individual schools, there may not be very large electoral rewards for pursuing a policy designed to improve school inputs or quality.

6 Conclusion

There is not yet a consensus on whether the institution of electoral democracy has a positive effect on the provision of basic services in poor countries. We know even less about the specific ways in which such an effect might operate. Starting from the principle that poor countries, such as those in Africa, tend to have limited and uncertain state capacity, we suggested that this makes it more difficult to attribute policy outcomes to executive actions. In this environment abolishing primary school fees provides an important example of an attributable policy that victorious election candidates can actually implement directly in a short amount of time and where voters can easily judge whether a promise is kept. An alternative promise, such as to exert effort to build more schools or to hire more teachers, cannot be as easily verified. So, when it comes to provision of these inputs, we should expect the arrival of electoral democracy to have a more muted effect on outcomes. We have provided three types of evidence to support this argument. First, using data from the Demographic and Health Surveys we have suggested that if African democracies tend to have a higher percentage of children that attend primary school, this is due primarily to the fact that democratically elected governments are more likely to abolish primary school fees. Second, using the first cross-country evidence on school fee abolitions, we have shown that this phenomenon appears to be electorally determined. Finally, we have used survey evidence on voter intentions in Kenya to provide micro evidence in support of our argument. Kenyan citizens who express support for free primary education tend to support the incumbent President who adopted this policy. Kenyan citizens who lodge complaints about the quality of their schools are no less likely to support the incumbent President. Taken together, this evidence offers support for our assertion that electoral democracy can increase the provision of basic services, in areas of policy where that provision can be directly attributed to executive actions.

A Further Results

A.1 Probit Estimates of Probability of Fee Abolition

The following table reports results of probit and ivprobit estimates of equation (3) as an alternative to the linear probability model reported in Table 4.³⁸ As in our linear probability model estimates, the coefficient on the election variable is statistically significant in all specifications, and we again observe that the magnitude of the implied effects is slightly larger in the instrumental variables estimates. Based on the column (2) specification, in a non-election year a country would be estimated to have a 1.2% chance of shifting to a no fees regime. In an electoral year this probability would rise to 5.4%. This result is nearly identical to that obtained using a linear probability model. Based on the column (4) estimate, in any given non-electoral year, a country would have a probability of close to zero of shifting to free primary education. In contrast, in an election year a country would have a roughly ten percent probability of doing so.

Presidential election variable	(1)	(2)	(3)	(4)	(5)	(6)
	Probit			IV Probit		
Election (any)	.690 (.250)			1.25 (0.26)		
Election judged free and fair		.872 (.266)			1.84 (0.40)	
Election where winner share < 80%			.649 (.239)			1.35 (0.28)
N=	619	577	619	600	558	600

Table 6: **Estimates of Likelihood School Fees Will be Abolished.** Probit Model where dependent variable takes a value of 0 for all years with fees, 1 in year of abolition, and country dropped from data set for subsequent years. Standard errors clustered to allow for arbitrary within-country correlation. All specifications include a cubic function of time to control for unobserved time effects. These estimates do not include country fixed effects.

B Data Appendix

This appendix describes all procedures for coding and analyzing data that have not previously been presented in the main text. We discuss the 29 DHS surveys that were used for the Section 2 estimates, the Afrobarometer survey data, and we devote the most extensive attention to describing the coding of our school fees variable.

B.1 Demographic and Health Surveys Data

The estimates in Section 2 use data from DHS surveys in 29 African countries. In each case we used the most current survey available at the time of writing. The DHS surveys used

³⁸We also make one alteration to the specification, substituting a cubic function of time for the set of year dummies. This follows the recommendations of Carter and Signorino (2010).

for the analysis were: Benin (2006), Burkina Faso (2003), Cameroon (2004), Central African Republic (1994), Chad (2004), Republic of the Congo (2005), Cote d'Ivoire (2005), Democratic Republic of the Congo (2007), Ethiopia (2005), Gabon (2000), Ghana (2003), Guinea (2005), Kenya (2003), Lesotho (2004), Liberia (2007), Malawi (2004), Mali (2006), Mozambique (2003), Namibia (2006), Niger (2006), Nigeria (2003), Rwanda (2005), South Africa (1998), Swaziland (2006), Tanzania (2007), Togo (1998), Uganda (2006), Zambia (2007), Zimbabwe (2005). The DHS uses a standard Household Schedule survey instrument across countries (available at <http://www.measuredhs.com/aboutsurveys/dhs/questionnaires.cfm>), which makes it straightforward to merge the datasets from different surveys using a country-specific identification code.

B.2 Democracy and Elections Data

The dates of presidential elections were taken from <http://africanelections.tripod.com>. We code a country as "democratic" for any year in which the chief executive was in place as a result of multiparty electoral competition in which more than one party won votes. This data comes from the World Bank's Database of Political Institutions constructed by Thorsten Beck, Philip E. Keefer and George R. Clarke (<http://go.worldbank.org/2EAGGLRZ40>). We use the database's Executive Index of Electoral Competitiveness, which codes the elected status of the executive for each country-year on a 7-point scale: 1 - No Executive; 2 - Unelected Executive; 3 - Elected, 1 candidate; 4 - 1 party, multiple candidates; 5 - multiple parties are legal but only one party won votes; 6 - multiple parties DID win votes but the largest party received more than 75% of the votes; 7 - largest party got less than 75%. We take a country to be democratic for any year in which it is coded 6 or 7 on this scale.

For robustness we repeated the analysis using data from the Polity IV and Freedom House databases. For the Polity IV data we code a country as "democratic" for any year in which the Combined Polity Score is greater than zero. This data comes from the Polity IV Project: Political Regime Characteristics and Transitions, 1800-2010 constructed by Ted Robert Gurr, Keith Jagers and Monty G. Marshall (<http://www.systemicpeace.org/polity/polity4.htm>). For the Freedom House Data we code a country as "democratic" for any year in which the country was classified as "Free" or "Partly Free". This data comes from the Democracy Time-series Data Release 3.0 constructed by Pippa Norris (<http://www.hks.harvard.edu/fs/pnorris/Data/Data.htm>), and is available through to 2007.

B.3 Afrobarometer Data

The estimates in Section 5 used Kenyan data from Round 3 of the Afrobarometer surveys. We report here the exact wording for the key questions that we used in the analysis.

1. Question 10: "Which of the following statements is closest to your view? Choose Statement A or Statement B. A: It is better to have free schooling for our children, even if the quality of education is low. B: It is better to raise educational standards, even if we have to pay school fees." Possible responses: Agree Very Strongly with A, Agree with A, Agree with B, Agree Very Strongly with B, Agree with Neither, Don't Know.

2. Question 73B: “Have you encountered any of these problems with your local public school during the past 12 months? Lack of textbooks or other supplies” Possible Responses: Never, Once or Twice, A Few times, Often, No experience with public schools in the past twelve months.
3. Question 73E: “Have you encountered any of these problems with your local public school during the past 12 months? Overcrowded classrooms” Possible Responses: Never, Once or Twice, A Few times, Often, No experience with public schools in the past twelve months.
4. Question 73F: “Have you encountered any of these problems with your local public school during the past 12 months? Poor Conditions of Facilities” Possible Responses: Never, Once or Twice, A Few times, Often, No experience with public schools in the past twelve months.

The same Afrobarometer survey also asked several questions regarding experience with “poor teaching” and “absent teachers”. As these would be less likely to be affected by presidential policy we did not include them in the regressions.

Using the Afrobarometer data we constructed a wealth index by conducting factor analysis of responses to Questions Q93A, Q93B, Q93C, Q93D, Q93E, and Q93F using maximum likelihood. We then used the first factor as an index of wealth, and divided individuals into wealth quintiles. To the greatest extent possible using Afrobarometer data, this replicates the method used to produce the DHS wealth indicator.

B.4 Coding of School Fee Abolitions

We used a variety of types of evidence to code for 39 African countries covering the period between 1990 and 2007 whether a government maintained a policy of allowing fees to be levied for primary education, or whether a government acted to abolish fees. As discussed in the main text of this paper, African primary school students can encounter a range of different costs that could be called fees. These include official tuition fees, fees levied by community associations (often referred to as PTA fees), exam fees, and fees for uniforms. Moreover, actual practice may vary from region to region within individual countries. There is only a very limited number of existing studies that attempt to document the extent and type of fees prevailing for a group of African countries for a set point in time. The study by Raja Bentaouet Kattan (2006) is the most detailed example of which we are aware. No existing study attempts to track the evolution of fees for a set of countries over time, though the report by Katarina Tomasevski (2006) does provide a wealth of information on a country by country basis, that often refers to policy changes. Given these constraints, we have sought to use these two sources supplemented with a number of additional sources for individual countries in order to produce a relatively crude binary indicator that records whether a government has announced an abolition of tuition and/or PTA fees and then taken legal or ministerial action to actually implement this policy. Here we list the countries for which we have constructed our fees indicator and note cases where we have used information sources other than Kattan (2006) and Tomasevski (2006)

Angola - Tomasevski (2006 p.18) suggests that primary school fees were abolished in 1977, but then commitment to free compulsory education disappeared during the 1990s. We

found no indication from any source that Angola has subsequently abolished school fees.

Benin - Benin abolished school fees with the 2006/7 school year. We confirmed this with (1) RESEN report 2008 - *Le système éducatif Béninois: Analyse sectorielle pour une politique éducative plus équilibrée et plus efficace*. (2) *Le Matinal*, 6 Mai 2006, “Yayi Boni confirme la gratuité de l’école à la base”.

Botswana - This is the one country that actually abolished fees prior to 1990, and so it is not included in our estimation. See the article by Dov Chernichovsky. 1985. “Socioeconomic and Demographic Aspects of School Enrollment and Attendance in Rural Botswana”, *Economic Development and Cultural Change* 33:319-332.

Burkina Faso - Burkina Faso has passed a law abolishing school fees, but as of 2008 this was only implemented in 45 of the country’s 300 departments. In addition, as shown in the study by Héma (2008) the wording of the Burkinabé statute quite explicitly introduces the possibility that official tuition fees be replaced by association fees. For this reason Héma suggests that free education was therefore “proclaimed and immediately invalidated.” For this reason we have coded Burkina Faso as not having abolished school fees. Issouf Héma (2008) “L’offre d’éducation primaire dans la commune de Réo (province du Sanguié, Burkina Faso)” Étude Recit n.25, Labo Citoyennetés.

Burundi - The government of Burundi abolished school fees in 2005. See “Burundi: diagnostic et perspectives pour une nouvelle politique burundaise dans le contexte de l’éducation gratuite pour tous”, June 2006.

Cameroon - Cameroon abolished fees effective at the beginning of the 2000/2001 school year. See “Rapport d’Etat du Système Educatif National Camerounais: Eléments de diagnostic pour la politique éducative dans le contexte de l’EPT et du DSRP”

Central African Republic - There is no indication that the Central African Republic has abolished school fees. See RESEN 2008 - “Le système éducatif Centrafricain: Contraintes et marges de manoeuvre pour la reconstruction du système éducatif dans la perspective de la réduction de la pauvreté.”

Chad - The government of Chad has not abolished school fees. The following study refers quite explicitly to the fact that school fees continue to exist. “RESEN 2007 Le système éducatif Tchadien: Eléments de diagnostic pour une politique éducative nouvelle et une meilleure efficacité de la dépense publique.”

Republic of Congo - One report, in addition to press sources, suggest that the Congolese government abolished school fees in 2007. See RESEN Congo 2010 “Le système éducatif congolais: Diagnostic pour une revitalisation dans un contexte macroéconomique plus favorable.”

Côte d’Ivoire - Identifying the exact status of school fees for Côte d’Ivoire was particularly difficult, something that is no doubt related to the country’s recent history of political instability. Drawing on a number of different sources, we were only able to establish that the Ivoirian government at one point made the first year of instruction free. The most detailed study available was that produced by Eric Lanoue (2003) “Background paper prepared for the Education for All Global Monitoring Report 2003/4: Gender and Education for All: the Leap to Equality.”

Democratic Republic of Congo - There is no indication that the Congolese government has moved to abolish school fees.

Equatorial Guinea - Evidence on Equatorial Guinea was particularly limited, but we

found no indication that its government had ever abolished school fees, and a US Department of State report dating from 2003 refers explicitly to the existence of school fees. See “Equatorial Guinea: Country Reports on Human Rights Practices: Bureau of Democracy, Human Rights, and Labor.”

Eritrea - No indication that school fees have been abolished and Bentaouet Kattan (2006) documents that they were in place.

Ethiopia - Fees abolished in 1994. Bentaouet Kattan (2006) confirms no tuition fees, though other fees are levied.

Gambia - There is no indication that the Gambian government has abolished school fees. A report by the Gambian government available at statehouse.gm/kids/girlchild/index.html refers to the fact that direct costs of schooling are still an obstacle to girls becoming educated.

Ghana - Ghana proceeded in steps towards fee abolition, with final abolition of fees in 2005/6. This is confirmed in Bentaouet Kattan (2006).

Guinea - According to Bentaouet Kattan (2006) Guinea has no official tuition fees, but there are PTA fees and community contributions. The following report also confirms that fees were in place “Le Système Educatif Guinéen; Diagnostic et Perspectives pour la Politique Educative dans le Contexte de Contrainte Macro-économiques Fortes et de Réduction de la Pauvreté”, 2005.

Guinea-Bissau - No indication that fees have been abolished and Bentaouet Kattan (2006) documents existence of PTA/community contributions.

Kenya - The Kenyan case is well known. During the 2002 presidential election campaign the challenger, Mwai Kibaki, promised to abolish primary school fees if elected. This policy was enacted in January 2003.

Lesotho - Both Tomasevski (2006) and Bentaouet Kattan (2006) concur that school fees were abolished in 2000 (with the announcement being made in 1999). For a third concurring opinion see Roger Avenstrup (2004) “Kenya, Lesotho, Malawi and Uganda: Universal Primary Education and Poverty Reduction.”

Liberia - A promise to abolish school fees was part of President Johnson-Sirleaf’s election campaign in 2005. The School Fee Abolition Initiative report 2006/7 indicates that this action has been implemented. Bentaouet Kattan (2006) reports numerous fees existing, but this would pre-date the election.

Madagascar - Bentaouet Kattan (2006) reports that tuition fees were abolished in 2003. However, local press articles from 2008 and 2009 refer clearly to fees existing and even being increased. For an example see “Enseignement: Les frais de scolarité en hausse” July 3, 2008. Given this information, we have coded Madagascar as not having abolished school fees.

Malawi - Abolished school fees in 1994. For context see Samer Al-Samarrai and Hassan Zaman. 2007. “Abolishing School Fees in Malawi: The Impact on Education Access and Equity” *Education Economics*, 15:359-375.

Mali - Both Tomasevski (2006) and Bentaouet Kattan (2006) refer to fees being collected unofficially. There is no evidence of an abolition event, and the following study provides further support for coding Mali as not having abolished fees. “L’éducation au Mali: Diagnostic pour le renouvellement de la politique éducative en vue d’atteindre les objectifs du millénaire”, 2007.

Mauritania - Bentaouet Kattan (2006) refers to the existence of fees. No indication from any other source that fees have been abolished.

Mozambique - Abolition of school fees was an election pledge in 2003 implemented in 2004. For details see *Abolishing School Fees in Africa: Lessons from Ethiopia, Ghana, Kenya, Malawi, and Mozambique*, World Bank (2009).

Namibia - Both Tomasevski (2006) and Bentaouet Kattan (2006) refer to fees being paid unofficially. A local press article from 2008 reports a parliamentary discussion clearly indicating that significant PTA fees are collected. Based on this, we have coded Namibia as not having abolished school fees. See “School Fees Bone of Contention”, *The Namibian*, July 9, 2008.

Niger - No indication that the government of Niger has abolished fees.

Nigeria - No indication that the government of Nigeria has abolished fees.

Rwanda - Education reform abolished primary school fees in 2003. See “Rwanda’s Reforms Boost Progress in School Enrolment”, United Nations Development Programme (2010).

Senegal - No indication that the Senegalese government has abolished school fees.

Sierra Leone - Free Primary Education Act introduced in 2001. However, Bentaouet Kattan (2006) refers to the existence of PTA fees. This is therefore an uncertain case. We have coded Sierra Leone as having abolished school fees in 2001. Note: this coding choice actually reduces the observed correlation between school fee abolitions and elections.

Swaziland - Clear from both Tomasevski (2006) and Bentaouet Kattan (2006) that fees are present.

Tanzania - A well known case in which primary school fees were abolished in 2001. Well documented in numerous reports.

Togo - Neither Tomasevski (2006) nor Bentaouet Kattan (2006) are helpful for coding Togo. However, press reports indicate that the Togolese government abolished school fees effective October 2008. See “La gratuité de l’école de base décrétée au Togo” Lomé (PANA).

Uganda - A well known case in which tuition fees were eliminated in 1997. See Deininger (2003) and Stasavage (2005b) for context.

Zambia - Bentaouet Kattan (2006) refers to school fees as having been abolished in 2002.

Zimbabwe - Fees were abolished after Zimbabwe’s independence but then reinstated during the 1990s. See Tomasevski (2006) and Bentaouet Kattan (2006).

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