

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

Robin Harding University of Rochester
David Stasavage New York University

Does democracy affect the provision of basic services? We advance on existing empirical work on this subject 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 state capacity, in which it is 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. 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, precisely because executive actions on these issues are more difficult to monitor. We evaluate this claim by approaching it in three different ways, using cross-national as well as individual-level data, including an original data set on primary school fee abolitions. First we show that in Africa, democracies have higher rates of school attendance than nondemocracies. Moreover, evidence suggests that this is primarily due 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, in this case abolishing school fees, but not on outcomes over which politicians have much more indirect influence, such as local school quality.

There is a strong sense that governments subject to electoral competition are more likely to provide basic services to their citizens.¹ 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 affects service provision. There is by now an extensive empirical 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.² In looking for broad correlations, this literature generally sets aside a crucial issue—how are voters supposed to condition their support on a candidate's effort to improve basic service provision if this effort is not directly observable? Citizens may well know whether the school in their village lacks a roof, but they 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.

Presidential candidates in Africa often make promises with regard to education, health, or other services, but in an environment where there are problems of implementation, in the sense that presidential

¹All data and supporting materials necessary to reproduce the numerical results are available at <http://politics.as.nyu.edu/object/DavidStasavage>. An online appendix providing supplementary results and details on data coding can be found at <http://dx.doi.org/10.1017/S0022381613001254>.

²Contributions to this voluminous literature include Blaydes and Kayser (2011), Ross (2006), Kudamatsu (2012), Besley and Kudamatsu (2006), Baum and Lake (2003), Bueno de Mesquita et al. (2003), Stasavage (2005a), Brown and Hunter (2004), Adsera, Boix, and Payne (2003), and Min (2010). Lindert (2004) makes claims about democracy and basic service provision in a historical context, Sen (1999) has also long been associated with the idea that democratic governments might be more likely to fulfill certain needs of their population.

decisions may not be applied on the ground due to actions by subordinates, it is difficult for candidates and voters to form an implicit contract over what exactly constitutes a broken promise. We argue that under these conditions, and we will refer to these conditions as weak state capacity, a democratic transition is most likely to affect basic service provision in policy areas where voters can verify whether a promise has been kept. A candidate's promise to abolish user fees in health or education can be easily 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. Citizens may observe that the end outcome is a failure, but they may not know where to attribute blame, precisely because state capacity is low. As a result, we suggest that democracy may result in increased access to education, without having a similar effect on the provision of inputs. Our empirical results are consistent with theoretical work by Mani and Mukand (2007) and the literature on multitask principal-agent problems more generally that was initiated by Holmstrom and Milgrom (1991).

We follow a three-step empirical strategy to evaluate this argument. First we show that democracies in Africa have higher rates of primary school attendance than nondemocracies, and that this appears to be due to the proclivity of democracies to abolish user fees for primary schools. As a second step, we demonstrate the link between democracy and fee abolition by showing that African governments have been particularly likely to abolish primary school tuition fees in the immediate wake of competitive presidential elections. Finally, we use survey evidence from Kenya to provide support for the claim that voters and candidates can only form implicit contracts over verifiable policy promises. We show that only attributable policy outcomes influence citizens' voting intentions, while nonattributable outcomes have no such effect.

Considering our empirical results in greater detail, we first use data from the Demographic and Health Surveys (DHS) program to investigate whether children in African democracies are more likely to attend primary school and if so what the mechanism for democracy's influence appears to be. Individual-level data from 29 countries allow us to identify the effects of both democratic transitions and fee abolitions on the probability of an individual ever attending primary school. Doing so, we find that although democracy is positively related to school attendance, this effect is substantially attenuated by controlling for whether schooling is free. Moreover, a variable for free schooling is highly significant, with

its implied effect being to increase the probability of school attendance by more than 4 percentage points, even when controlling for democratic transitions. This evidence provides a strong indication that any effect of democracy on school-attendance may be attributable above all to the proclivity of democracies for abolishing school fees.

The DHS data provide a very effective way of investigating school attendance, but we lack similarly high-quality cross-country data on the provision of school inputs. As one feasible measure of an outcome that is affected by school inputs, we use cross-national annual data on numbers of teachers as reported in the African Development Indicators. Analysis of these data suggests that, contrary to what one might expect, democracies actually tend to have higher ratios of pupils to teachers than do nondemocracies. 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. These results support the principal claim of this paper, that in an environment of weak state capacity, democracy may prompt governments to increase education access, but not education inputs.

The second component of our empirical inquiry involves examining the conditions under which governments have abolished school fees. In the third section of this article, we present a new data set that records all recent episodes of primary school fee abolitions in African states. Using this dataset, we conduct an empirical analysis that provides strong evidence of a link between contested elections and fee abolition, 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 attributable policy changes, such as the abolition of school fees, will influence voting behavior, but outcomes for which responsibility is unclear (such as the existence of poor facilities) will be less highly correlated with voting intentions. Analysis of survey data from the Afrobarometer Series provides evidence that school inputs and school quality do not have a significant impact on electoral support, but it suggests that electoral support may be positively

affected by increased access resulting from the abolition of school fees.

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 often have difficulty knowing 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 candidates.

With the above said, one might conceive of the possibility that our empirical results might also be consistent with an alternative and more simple theoretical story. Perhaps voters anywhere just respond to policies, such as school fee abolition, that bring them an immediate financial gain, and our account emphasizing attributable and nonattributable outcomes is superfluous. However, it would seem that in the particular case of education and school fees, this argument is a nonstarter because if parents were concerned only about immediate benefits, then they wouldn't be sending their children to school in the first place.

Our findings in this article 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. Several contributions have used either current or historical evidence to suggest that such an effect does indeed exist.³ But there are also prominent examples of autocratic or oligarchic regimes that have pursued universal education policies.⁴ 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, democracy may indeed have an effect on basic education, but only on some policy dimensions and not on others.⁵ Second, our findings can apply more generally to other policies, such as basic health care, which involve 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. 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 empirical attempts to consider how, holding constant a low-information environment, candidates and elected officials will face incentives to prioritize actions where voters can directly attribute outcomes to executive actions.⁷

Why Would a Government Abolish School Fees?

What are the mechanisms by which democratic transitions may improve basic service provision? Our core claim is that in an environment where 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 policy promises where the link between executive effort and outcomes on the ground 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.⁸ However, in certain instances candidates have also begun to make more specific promises, to abolish primary school fees, to abolish fees for health clinics,

³See in particular Ansell (2010), Baum and Lake (2003), Brown and Hunter (2004), and Stasavage (2005a).

⁴Examples include the differential success of mass literacy programs in China and India (see Dreze and Loh 1995), the investment in mass education by an oligarchic regime in nineteenth-century Argentina (see Elis 2011), and the program of mass school construction undertaken by the Suharto regime in Indonesia (see Duflo 2001).

⁵Ansell (2010) examines how democratic politics influences different components of education policy.

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 (2010).

⁷Several theoretical articles have proposed this mechanism, including Mani and Mukand (2007), Ashworth (2005), and Bueno de Mesquita and Stephenson (2007).

⁸In 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).

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. These criticisms are certainly far from wholly unfounded because in many cases governments have lacked the ability to deliver quality services that have been promised for free. Setting aside the question of whether fee abolitions are actually welfare enhancing, a further 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 the executive.

As noted in the introduction, there is an extensive literature which suggests that basic service provision may be improved if voters have access to information from sources such as radio, newspapers, or public-information campaigns. Much less consideration has been given to whether 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 candidates will seek to make concrete promises but only on easily attributable policies where their efforts can actually be verified.

Since the seminal article 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.⁹ 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 multitask principal-agent problems 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 where the resulting outcome is not only visible but also easily attributable, in the sense that the executive’s own contribution to the outcome can be easily 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 problems of implementation make it more difficult to judge the extent to which outcomes on the ground result directly from an executive’s actions.¹⁰ There is, however, one difference between the environment we consider and that in multitask principal-agent models. While the multitask 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 (effort in using the education budget). Increased effort on one of these two dimensions does not necessarily imply diminished effort on the other. 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.

It is important to emphasize that while we have drawn out the implications of our theoretical argument for the African context, the broader claim about the effect of electoral competition on provision of attributable and nonattributable goods can certainly apply in other regions or countries. What we would suggest though is that the problem of weak state capacity in Africa, the difficulty for the center in implementing policy at the local level, makes the problem of nonattributable goods particularly prominent.

⁹Holmstrom and Milgrom’s (1991) 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, 1999b). For political economy applications, see in particular Mani and Mukand (2007), Ashworth (2005), and Bueno de Mesquita and Stephenson (2007).

¹⁰One major source of such implementation problems is corruption that results from channeling funds for basic services through local governments or agencies. See Reinikka and Svenson (2004) for a discussion of this problem in the context of Ugandan primary education.

As a final point, as will become apparent in the subsequent discussion, there is also one further difference between the empirical context we consider and that presented in a retrospective voting model of the sort analyzed by Mani and Mukand (2007) where politicians first take actions and then citizens decide whether to retain them. In the cases we consider in which school fees were abolished in proximity to a presidential election, this was actually a promise that was made during a campaign, most often by challengers, and it was not carried out until after the election took place. We can better understand this behavior by considering the theoretical work by Aragonés, Palfrey, and Postlewaite (2007) which suggests in a model of accountability, campaign promises, such as a promise to abolish school fees, create focal points that coordinate expectations of voters regarding whether a politician has performed adequately. Standard retrospective voting models of the sort analyzed by Mani and Mukand (2007) implicitly assume this problem away by considering the case of a representative voter.

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. Contrary to what one might expect, however, democracies do not tend to provide more teachers than nondemocracies. 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 nondemocracies 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, this correlation is no longer statistically significant. The broad message of these results may be that democracy prompts governments to increase access but not to increase inputs.

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.¹¹ 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. 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 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.¹²

The specification in Equation (1) controls for household-specific fixed effects μ_h . For the same

¹¹Kudamatsu (2012) and Franck and Rainer (2012) are the only previous political economy articles that we are aware of to adopt this strategy using pooled DHS data for a group of African states.

¹²The most recent year for which we have data on educational attainment varies by country as detailed in the appendix.

reasons as expressed in Kudamatsu (2012), we employ individual-level data in a specification including household fixed effects, as opposed to the alternative strategies of individual data with country fixed effects or aggregate (i.e. country-year) data with country fixed effects. Inclusion of household fixed effects implies that we are estimating the effect of democracy and fee abolition while considering only those households in which children were of school age both before and after a democratic transition, or before and after fee abolition. The result then is that we are more likely to be considering households with similar characteristics if it is the case that household characteristics vary systematically over time within a country.

While we conduct analysis using individual-level data, we need to adjust our standard errors to take account of the fact that our two key variables of interest, *multiparty* and *free*, will take common values for a cohort of individuals born in a given country in a given year. In other words, the democracy and fee abolition “treatments” occur at the cohort level and not the individual level. In order to take account of this, all standard errors for estimates of Equation (1) are clustered at the cohort level, where a cohort is defined as all individuals born in a given country in a given year.

Our two principal variables of interest in Equation (1) 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 0. 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.¹³ It should be noted that this is a minimalist definition of democracy that focuses strictly on procedure. It takes no account of whether the playing field for party competition is actually a level one. We adopt

this definition for two reasons. First, it has the great virtue of being an objective ex ante measure of formal institutions, as opposed to a subjective measure (such as Polity or Freedom House) that may be influenced by ex post outcomes. Second, even in those cases where the playing field is not level, and an incumbent thinks that they will win an election with high probability, there may still be an incentive to adopt policies such as free schooling to maximize vote share. The Ugandan presidential election of 1996 is arguably a good example of this phenomenon.¹⁴

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 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 1.¹⁵

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 all of 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 greater risk that we have incorrectly coded a government as abolishing fees when in practice the

¹³As 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.

¹⁴See Stasavage (2005a) for a study of this episode.

¹⁵The appendix provides a complete list of the documentation used to classify each abolition (or nonabolition).

TABLE 1 Primary School Fee Abolitions in Africa (1990–2007)

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	99	1
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	4
Kenya	2003	yes	yes	yes	62	31
Mozambique	2004	yes	yes	yes	64	32
Burundi	2005	yes	yes	yes	54	25
Ghana	2006	no				
Liberia	2006	yes	yes	yes	27	19
Benin	2006	yes	yes	yes	36	24
Congo (Brazza.)	2007	no				

Note: 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.

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 16 fee-abolition episodes that we have identified as occurring between 1990 and 2007, the context in which this occurred, and the date. In 11 of the cases fees were abolished in the immediate wake of an election. It is particularly interesting to note that among these 11 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 easily attributable policy changes early in their term of office in order to cement their reputation for the future.¹⁶

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 primarily due to the fact that democratic governments are more likely to

abolish school fees. We do not estimate specifications with an interaction term of *multiparty* \times *free* because in our sample fees have been abolished almost exclusively in countries where leaders are selected in multiparty elections.¹⁷

We also include three further controls denoted by the matrix 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. Dummy variables for birth year control for common time trends in school attendance while birth-order dummies control for the possibility that children higher in the birth-order may be less favored.

Table 2 reports the results of eight different specifications. 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 nondemocratic contexts, though the coefficient on *multiparty* is not quite significant

¹⁶This behavior would be consistent with the dynamic suggested by Ashworth (2005).

¹⁷The 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. (2000) data set. This is presumably because Burundi’s president was not yet directly elected by popular vote as is now the case.

TABLE 2 Estimates of Probability of Individual Having Any Schooling

	(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

Note: Linear probability model with standard errors clustered at the cohort level.

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 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. It should be acknowledged that the magnitude of the observed effect of fee abolition in the column (2) and column (3) estimates, which is of course an average across countries, is not terribly large in absolute terms. It is interesting to observe, however, that in the column (2) estimate the effect of fee abolition is nearly as large as the gender differential in enrollment, which is commonly referred to as a significant policy issue for African countries. Moreover, it should be remembered that any bias in our coding of fee abolitions would likely involve suggesting that fees had been abolished when they had in fact not. This would lead to a downward bias on the coefficient for the free schooling variable.

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.¹⁸ 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.

The specifications in columns (4) through (8) in Table 2 repeat specification (3) for separate quintiles of the wealth distribution. We do this for the following reason. We have no direct interest in developments at different levels of the wealth distribution, and our theoretical argument regarding problems of attribution is not specific to a certain type of household, either wealthy or poor. However, by looking at developments at different levels of the wealth distribution, we can get a better sense of whether it is actually school fee abolition in democracies that generated higher levels of school attendance. If it is school fee abolition that is doing the work, as opposed to some shock affecting all households, then we should not expect abolition to be correlated with increased attendance from households at the top of the wealth distribution, since such

¹⁸See Green, Ha, and Bullock (2010) on this question.

households would find it easier to pay for schooling, either public or private. Likewise, we should not expect abolition to result in increased attendance for those households at the very bottom of the wealth distribution. 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.

The DHS surveys include a variable for household wealth that is constructed through factor analysis of questions regarding a number of different household assets.¹⁹ 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 smaller than for households in the second quintile. This is a result that we would expect as stated above. 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.

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. To investigate the quality of instruction, one idea might be to investigate changes in standardized test scores in countries that abolished school fees and in those that didn't. Such scores are available for some of the countries in our sample for recent years, but certainly not for all. Even if such data were available for all of our sample countries, however, there would still be a further problem. When a government abolishes school fees, this may be associated with a change in the quality of instruction due to overcrowding, but it will also certainly result in a change in the social composition of student cohorts with entry of more poor children into the classroom. In short, if test scores decline after an abolition of fees, then we won't be able to know which of these effects has been present and to what degree. Another alternative for investigating the quality of instruction would be to use data on literacy rates, based on the expectation that if quality of instruction suffers drastically after a fee-abolition, then literacy rates in fee-abolition countries might be no higher, or potentially even lower, compared with countries in which fees had not been abolished. While a logical strategy, a significant problem is that most African countries that have abolished fees have done so only recently, and therefore the cohorts affected have not yet reached the age for which youth literacy statistics are reported (generally 15–24). This very significantly limits our possibility for effectively using either literacy rates collected by UNESCO or literacy data that are commonly part of DHS surveys.

In the absence of a better alternative for focusing directly on quality of instruction, one feasible direct measure of school inputs is the cross-national data on numbers of teachers that is reported in the African Development Indicators and which is originally collected by UNESCO. These data, which are 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.

¹⁹See Filmer and Pritchett (1999) for a discussion of this method for measuring household wealth.

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 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 0 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 eight 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 result from 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. This evidence is consistent with the idea that democracies have higher pupil/teacher ratios because they have a proclivity for abolishing school fees but without hiring enough extra teachers to keep pupil/teacher ratios stable.

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

TABLE 3 Estimates of Pupil/Teacher Ratios

	(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

Note: Ordinary Least Squares (OLS) estimates with standard errors clustered at the country level.

relative to the potential student population when compared with nondemocracies. This is true irrespective of whether we omit a variable for the fee regime, as in column (4), or whether we control for the fee regime, as in column (6). In column (5), we also see a substantively large negative correlation between the size of the school-age population relative to the number of teachers and the presence of school fees. However, this correlation is only at the borderline of statistical significance.

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 nondemocracies and no indication that countries abolishing fees make sufficient investments in inputs to keep pupil/teacher ratios stable.

Presidential Elections and Fee Abolition

In the previous section, we established that multi-party 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 0 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.²⁰

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

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 1 if a country has experienced a presidential election in the current or the previous year and 0 otherwise.²¹ 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 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

²⁰Following Beck, Katz, and Tucker (1998).

²¹This is a simple way of taking account of the fact that a fee abolition might occur swiftly after an election but not in the 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.

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 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, some specifications also control for country fixed effects (μ_i).²²

Table 4 presents 12 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 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 nonelectoral 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.²³ 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

²²There 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 nonlinear 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 (see Angrist 2001). 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).

²³This is based on the data set collected by Lindberg (2006) that he has subsequently updated.

TABLE 4 Estimates of Likelihood School Fees Will Be Abolished

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)	.072 (.030)	.043 (.020)	.052 (.020)	.076 (.032)	.050 (.021)	.075 (.023)	.126 (.042)	.083 (.026)	.080 (.026)	.130 (.044)	.089 (.028)
"Free and fair" election							yes	yes	yes	yes	yes	Yes
Election and winner share 80%							no	no	no	no	no	Yes
Year fixed effects	.051	.064	.050	.135	.149	.132	.045	.048	.039	.130	.136	.123
Country fixed effects							yes	yes	yes	yes	yes	Yes
R ²												
First-stage statistics												
F statistic on excluded instrument	619	577	619	619	577	619	243	80	217	210	67	191
Partial R ² excluded instrument							.574	.362	.505	.579	.366	.510
N=	619	577	619	619	577	619	600	558	600	600	558	600

Note: 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 two-stage least-squares estimates, the excluded instrument is "scheduled election."

particularly lopsided elections (those where the winner's share is greater than 80%) as being cases where the "election" variable is set equal to 0. This is based on the idea that a 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 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. The logic behind our instrumenting strategy is to use the legally mandated date for an election as an instrument for the date at which an election actually occurs.

For the estimates in columns (7) through (12), 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).²⁴ Since all of our

²⁴In 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 1 in 1992, 1997, 2002, and 2007. It takes a value of 0 in all other years.

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.²⁵ 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 reelection 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 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 overinterpret these results. Given the confidence intervals for both the Ordinary Least Squares (OLS) and two-stage least squares (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.

²⁵The nonpresidential 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.

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.

Survey Evidence on Voter Intentions in Kenya

So far we have argued that electoral competition has prompted African election candidates to promise policies where the outcomes can be directly attributed to their efforts, 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.²⁶ 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.²⁷ In what follows, we use 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).

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.²⁸ 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

²⁶See the interesting discussion in Oketch and Rolletson (2007) for background.

²⁷An excellent survey of developments in the Kenyan educational system is provided by Lucas and Mbiti (2010).

²⁸See the data appendix for the exact wording of this and all other Afrobarometer questions used in this section.

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 1 if the individual responded that schooling should be free and 0 otherwise. The pattern of responses to the free-schooling question varied between wealth quintiles in much the way we might expect given the estimation results reported above using the DHS survey data, and a full discussion of this can be found in the appendix.

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.

$$\Pr(Kibaki = 1)_{ij} = \Phi\left(\alpha + \beta_1 free_{ij} + \beta_2 schoolproblem_{ij} + \mu_j + \varepsilon_{ij}\right) \quad (4)$$

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 regressions. 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.

Table 5 reports results of seven different estimates of Equation (4) 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 5 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 all 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

TABLE 5 Probit Estimates of the Probability of Expressing Intention to Vote for President Kibaki

	(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

Note: All specifications include district-level fixed effects, and standard errors are clustered at the district level.

pursuing a policy designed to improve school inputs or quality.

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.

References

- Adsera, Alicia, Carles Boix, and Mark Payne. 2003. "Are You Being Served? Political Accountability and Quality of Government." *Journal of Law, Economics, and Organization* 19 (2): 445–90.
- Angrist, Joshua D. 2001. "Estimation of Limited Dependent Variable Models with Dummy Endogenous Regressors: Simple Strategies for Empirical Practice." *Journal of Business and Economic Statistics* 19 (1): 2–28.
- Ansell, Ben. 2010. *From the Ballot to the Blackboard: The Redistributive Political Economy of Education*. New York: Cambridge University Press.
- Aragones, Enriqueta, Thomas Palfrey, and Andrew Postlewaite. 2007. "Political Reputations and Campaign Promises" *Journal of the European Economic Association* 5 (4): 846–84.
- Ashworth, Scott. 2005. "Reputational Dynamics and Political Careers." *Journal of Law, Economics, and Organization*. 21 (2): 441–66.
- Baum, Matthew, and David Lake. 2003. "The Political Economy of Growth: Democracy and Human Capital." *American Journal of Political Science* 47 (2): 333–47.
- Beck, Nathaniel, Jonathan Katz, and Richard Tucker. 1998. "Taking Time Seriously: Time-Series Cross Section Analysis with a Binary Dependent Variable." *American Journal of Political Science* 42 (4): 1260–88.
- Beck, Thorsten, George Clarke, Alberto Groff, Philip Keefer, and Patrick Walsh. 2001. "New Tools in Comparative Political Economy: The Database of Political Institutions." *World Bank Economic Review* 15 (1): 165–76.
- Bentaouett, Raja. 2006. "School Fees." Background paper prepared for the 2006 Education for All Global Monitoring Report.

- Besley, Timothy, and Robin Burgess. 2002. "The Political Economy of Government Responsiveness: Theory and Evidence from India" *Quarterly Journal of Economics* 117 (4): 1415–51.
- Besley, Timothy, and Masayuki Kudamatsu. 2006. "Health and Democracy." Working paper, London School of Economics.
- Birdsall, Nancy, and Francois Orivel. 1996. "Demand for Primary Schooling in Rural Mali: Should User Fees be Increased?" *Education Economics* 4 (3): 279–96.
- Blaydes, Lisa, and Mark Kayser. 2011. "Counting Calories: Democracy and Distribution in the Developing World." *International Studies Quarterly* 55 (4): 887–908.
- Bleck, Jaimie, and Nicolas van de Walle. 2010. "Parties and Issues in Francophone West Africa: Towards and Theory of Non-Mobilization," Working Paper, Cornell University.
- Bratton, Michael. 2007. "Are You Being Served? Popular Satisfaction with Health and Education Services in Africa." Afrobarometer Working Paper No. 65.
- Bratton, Michael, and Nicolas van de Walle. 2007. *Democratic Experiments in Africa: Regime Transitions in Comparative Perspective*. Cambridge: Cambridge University Press.
- Brown, David S., and Wendy Hunter. 2004. "Democracy and Human Capital Formation: Education Spending in Latin America." *Comparative Political Studies* 37 (7): 842–64.
- Bueno de Mesquita, Ethan, and Matthew Stephenson. 2007. "Regulatory Quality Under Imperfect Oversight." *American Political Science Review* 101 (3): 605–20.
- Bueno de Mesquita, Bruce, Alastair Smith, Randolph Siverson, and James Morrow. 2003. *The Logic of Political Survival*. Cambridge, MA: MIT Press.
- Dewatripont, Mathias, Ian Jewitt, and Jean Tirole. 1999a. "The Economics of Career Concerns: Part II. An Application to Missions and Accountability in Government Agencies." *Review of Economic Studies* 66 (1): 183–98.
- Dewatripont, Mathias, Ian Jewitt, and Jean Tirole. 1999b. "The Economics of Career Concerns: Part I. Comparing Information Structures." *Review of Economic Studies* 66 (1): 183–98.
- Dreze, Jean and Jackie Loh. 1995. "Literacy in India and China." *Economic and Political Weekly* 30 (45): 2868–2878.
- Duflo, Esther. 2001. "Schooling and Labor Market Consequences of School Construction in Indonesia: Evidence from an Unusual Policy Experiment." *American Economic Review* 91 (4): 795–813.
- Elis, Roy. 2011. "Redistribution Under Oligarchy: Trade, Regional Inequality, and the Origins of Public Schooling in Argentina, 1862–1912." Stanford University. Mimeo.
- Filmer, Deon, and Lant Pritchett. 1999. "The Effect of Household Wealth on Educational Attainment: Evidence from 35 Countries" *Population and Development Review* 25 (1): 85–120.
- Franck, R., and I. Rainer. 2012. "Does the Leader's Ethnicity Matter? Ethnic Favoritism, Education and Health in Sub-Saharan Africa." *American Political Science Review* 106 (2): 294–325.
- Gottlieb, Jessica. 2010. "Is Democracy Working? Determinants of Local Government Performance (Failure) in Mali." Stanford University. Unpublished manuscript.
- Green, Donald, Shang Ha, and John Bullock. 2010. "Enough Already about Black Box Experiments: Studying Mediation Is More Difficult than Most Scholars Suppose." *ANNALS of the American Academy of Political and Social Science* 628 (1): 200–208.
- Harding, Robin. 2010. "Urban-Rural Differences in Support for Incumbents Across Africa." Afrobarometer Working Paper No. 120.
- Holmstrom, Bengt, and Paul Milgrom. 1991. "Multi-Task Principal Agent Analyses: Incentive Contracts, Asset Ownership, and Job Design." *Journal of Law, Economics, and Organization* 7: 24–52.
- Horowitz, Jeremy. 2009. "Ethnic Groups and Campaign Targeting in Kenya's 2007 Elections." Dartmouth College. Unpublished manuscript.
- Keefer, Philip, and Stuti Khemani. 2003. "Democracy, Public Expenditures, and the Poor." *World Bank Research Observer* 20 (1): 1–27.
- Keefer, Philip, and Stuti Khemani. 2012. "Do Informed Citizens Receive more... or Pay more. The Impact of Radio on the Government Distribution of Public Health Benefits." World Bank Policy Research Working Paper No. 5952.
- Kudamatsu, Masayuki. 2012. "Has Democratization Reduced Infant Mortality in Sub-Saharan Africa? Evidence from Micro Data." *Journal of the European Economic Association* 10 (4): 1294–1317.
- Lindberg, Staffan, 2006. *Democracy and Elections in Africa*. Baltimore: Johns Hopkins University Press.
- Lindert, Peter (2004). *Growing Public: Social Spending and Economic Growth Since the Eighteenth Century*. New York: Cambridge University Press.
- Lucas, Adrienne, and Isaac Mbiti. 2009. "Access, Sorting and Achievement: The Short Run Effects of Free Primary Education in Kenya." Working paper, Wellesley College and Southern Methodist University.
- Mani, Anandi, and Sharun Mukand. 2007. "Democracy, Visibility and Public Good Provision." *Journal of Development Economics* 83 (3): 506–29.
- Min, Brian. 2008. "Democracy and Light: Global Evidence on Public Goods Provision from Satellite." University of Michigan. Mimeo.
- Oketch, Moses, and Caine Rolletson. 2007. "Policies on Free Primary and Secondary Education in East Africa: Retrospect and Prospect." *Review of Research in Education* 31 (1): 131–58.
- Reinikka, Ritva, and Jakob Svensson. 2004. "Local Capture: Evidence from a Central Government Transfer Program in Uganda." *Quarterly Journal of Economics* 119 (2): 679–705.
- Resnick, Danielle. 2010. "Populist Strategies in African Democracies" UNU-WIDER Working Paper No. 2010/114.
- Ross, Michael. 2006. "Is Democracy Good for the Poor?" *American Journal of Political Science* 50 (4): 860–74.
- Sen, Amartya. 1999. *Development as Freedom*. Oxford: Oxford University Press.
- Stasavage, David. 2005a. "Democracy and Education Spending in Africa." *American Journal of Political Science* 49 (2): 343–58.
- Stasavage, David 2005b. "The Role of Democracy in Uganda's Move to Universal Primary Education." *Journal of Modern African Studies* 43 (1): 53–73.
- Stromberg, David. 2004. "Radio's Impact on Public Spending." *Quarterly Journal of Economics* 119 (1): 1415–51.
- Tomasevski, Katarina. 2006. "The State of the Right to Education Worldwide." *Free or Fee 2006 Global Report*. Copenhagen, Denmark.

Robin Harding, Assistant Professor, Department of Political Science, University of Rochester, NY 14627.

David Stasavage, Professor, Department of Politics, New York University, NY 10012.