

Information, accountability, and public goods: Evidence from three linked field experiments in Liberia

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Abstract

Democracy, in theory, allows voters to hold politicians accountable for providing growth-inducing public goods which are undersupplied by the market, such as education. But there is very little empirical evidence on whether and when there are direct electoral rewards for public good provision – and the persistence of poor-quality public goods in many democracies implies the existence of nontrivial obstacles to this kind of accountability. We provide the first experimental measure of the impact of improved schools on electoral outcomes, leveraging the geographically randomized pilot of a public-private school partnership in Liberia. The policy caused an increase in voter registration and vote share for incumbent representatives, especially those from the ruling party. Electoral gains were concentrated in places where test score gains were largest, suggesting that voters perceive and reward school quality. We then examine informational and institutional obstacles to accountability, using two additional experiments that randomly vary information to politicians and households, respectively. Politicians who were informed about the school policy’s impact were twice as likely to support it in public debates broadcast by radio in their district. Households who were informed of candidates’ positions on the policy were more likely to support candidates who favored the policy, especially in districts where elections were competitive. Taken together, our results show that even in a consolidating democracy, politician and voter behavior is consistent with a model of electoral accountability for public goods, and that competitive elections and richer information have the potential to further align politicians with voters’ preferences.

Keywords: Evidence-based policy; Public-private partnership; Public goods; Public service delivery; Political economy; Elections; Randomized controlled trial; Liberia; Information; Competition

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1 Introduction

Venerable models of democratic accountability assume that voters reward politicians for providing public goods (Key et al., 1966; Ferejohn, 1986).¹ But these theories underlying much of modern political economy have surprisingly patchy empirical support, and they coexist uneasily with the observation that poor-quality public goods persist even in many places with broadly free and fair elections. Some democracies provide poor public goods in spite of voters' preferences (A. V. Banerjee, Green, McManus, & Pande, 2014), while elsewhere poor public goods seem in fact to reflect voters' preferences (Bursztyn, 2016).² Even where voters do prioritize public goods, institutional and informational deficiencies can prevent elections from providing strong accountability. Policymakers may not be aware of voters' preferences (Butler & Nickerson, 2011), or may not face pressure to respond to them (Besley & Burgess, 2002). Or they may be unsure how a given policy will affect public good quality, and fear that voters will interpret policy experimentation as a negative signal (Majumdar & Mukand, 2004).

The question of whether and where political returns to public good provision exist is qualitatively distinct from (though related to) the extant literature on clientelism. Cash transfers (targeted either clientelistically or programmatically) can buy votes; this fact is amply documented (Wantchekon, 2003; Manacorda, Miguel, & Vigorito, 2011). But public goods are a different class of political activity, constituting one of Smith (1776)'s central duties of governments:

The third and last duty of the sovereign or commonwealth is that of erecting or maintaining those public institutions and those public works, which, although they may be in the highest degree advantageous to a great society, are, however, of such a nature, that the profit could not repay the expense to any individual or small number of individuals, and which it therefore cannot be expected that any individual or small number of individuals should erect or maintain.

Government provision of public goods does not merely reallocate resources within society, but can create the conditions necessary for broader economic growth. It is therefore crucial to understand how and whether agents within the government have incentives to provide these kinds of goods. We focus on the public good of education, which creates positive spillovers on economic growth (Moretti, 2004) and

¹Here we use the term "public good" in the Smith (1776) sense of goods which provide positive externalities and therefore will be supplied by the market at a less-than-socially-optimal level, rather than the Samuelson (1954) sense of goods which are non-excludable and nonrival. Our focus is education, which is a public good in the first sense but not the second.

²Bursztyn (2016) quotes Cristovam Buarque – a Brazilian senator, former minister of Education, and presidential candidate – saying "Let's be frank: we do not give importance to education because the voters do not give it either. Nobody wins an election talking about education: I, incidentally, am an example."

political participation (Larreguy & Marshall, 2016). To our knowledge, ours is the first paper to measure political rewards for education provision.

Democratic accountability for public goods can be framed as an “O-ring” style production function: the process can fail if any one of a number of conditions fails (Kremer, 1993). For example, it must at least be the case that 1) public good provision matters in voters’ decision functions; 2) politicians are aware of voters’ preferences; 3) politicians know which policies will improve public goods; 4) voters know politicians’ policy platforms; 5) the available candidates offer sufficient variation in policy proposals. There are empirical studies in the literature which examine most of these conditions individually.

In this paper, we examine all of these conditions in the same context, employing three different experiments to follow the impact of an exogenous shock to public good provision in Liberia through various stages in the accountability apparatus. We document the reduced-form effect of a policy that improved public good provision on electoral outcomes; the effect of information about the policy’s effectiveness on politicians’ campaign behavior; and the effect of information about politicians’ policy statements on households’ electoral choices.

Like many nascent democracies, Liberia’s politics are widely thought to be characterized by cults of personality and clientelism rather than consistent policy platforms.³ Whether Liberian politics is likely to exhibit anything approximating accountability for public service provision is far from obvious ex ante.

We focus on a school policy experiment, carried out and evaluated via a randomized control trial in the year preceding Liberia’s 2017 general election. The Partnership Schools for Liberia (PSL) program, implemented by president Ellen Johnson Sirleaf’s Ministry of Education, transferred management of 93 randomly selected public primary schools (and provided additional resources) to private school organizations, starting in the 2016-2017 school year. The program was controversial, prompting frequent media coverage in the national and international press, leading to unusually high salience for a school policy. The policy was sharply opposed by Liberia’s politically powerful teachers’ unions. Romero, Sandefur, and Sandholtz (2017) show that the program increased test scores by 60% ($.19\sigma$), teacher attendance by 50%, and satisfaction of both students and parents by about 10%. These results were first reported publicly in September 2017, about one month before nationwide elections for the presidency and the House of Representatives.

The random assignment of the program permits us to measure its impact on political outcomes, including registration, turnout, and vote shares in the election at polling station level. We also gathered

³For example, Maffioli (2017) found suggestive evidence that the government manipulated its response to the Ebola crisis for electoral gain.

household survey responses five months before the election. This allows us to directly measure changes to voters' attitudes toward government in response to the school policy change. We then conducted an information experiment among candidates for Liberia's House of Representatives, varying the provision of policy evidence ahead of a series of debates to be broadcast over the radio in candidates' districts. This allows us to measure whether information about policy impact is a binding constraint on candidates' attempts to appeal to voters, and whether their behavior is consistent with the belief that voters will hold them accountable for their position on the school reform. Finally, we conducted a separate information experiment among a subset of households, varying the provision of information about candidates' positions on the school policy, and recording their voting intentions. This allows us to measure whether these voters' electoral choices are consistent with a desire to support politicians who support the school policy.

Our results, using administrative and survey data, provide evidence that voters reward improved public goods and politicians maneuver to capture these rewards. The number of people who registered to vote increased by 24% in polling stations near treated schools relative to polling stations near control schools, suggesting that improved public services lead to increased engagement in the democratic process. Polling stations near treated schools saw a 12-percentage-point increase in vote share for incumbent representatives, from a base of 21% – even though these politicians had no responsibility for the program's design or implementation.

Consistent with these electoral gains, we show that the school policy also broadly improved attitudes toward the government, although not by large amounts: treated households were 3 percentage points more likely to approve of the government's performance on education relative to the control group and 7 percentage points more likely to say education is their top priority for government spending. The school policy also gave both households and teachers a more hopeful view of the country's future, and decreased the satisfaction of the schools' teachers with the teacher union by 9 percentage points.

While these impacts on political outcomes are consistent with the idea of political accountability, many conceivable channels exist. Heterogeneity analyses helps us to illuminate which mechanisms are likely to be at play. We show that the ruling party (which implemented the policy at the national level) saw increased vote share at the presidential level only in districts where the incumbent representative was from the ruling party, supporting the idea that entrepreneurial representative politicians claimed credit for the policy. The ruling party's gains were also higher in places where the policy's effect on test scores was higher, suggesting that voters were not merely rewarding the presence of a new program, but that they can perceive and reward school quality on dimensions researchers can measure.

Next we measure politicians' behavioral responses to information about the policy. A series of debates were held among House candidates in most of the legislative districts in the country. These debates were held in front of a live audience of constituents, and were later recorded and rebroadcast in the district via radio. We focus on 8 of these districts in Bomi, Grand Cape Mount, and Montserrado counties where debate moderators included a question about the PSL program. Within each of these districts, we selected a random group of candidates to treat with information about the policy. A day or two before each debate, we sent each selected candidate an email containing an Innovations for Poverty Action-branded four-page policy brief, as well as a text message summarizing the findings of Romero et al. (2017).⁴ We then had research assistants (who were unaware of the treatment assignment) code the transcripts of the debates. Informed candidates were 10 percentage points more likely to favor the program in the debate, in the intent-to-treat (ITT) estimate. A separate survey showed that informing candidates of the policy's popularity had no impact on their support for the policy, suggesting that candidates were already aware of voters' preferences on this issue. This shows that politicians' campaign efforts included attempts to appeal to voters through support for the policy, and also suggests that better policy information can strengthen accountability.

Finally, we examine the effect of information about candidates on voters' choices. Voters can only reward politicians for championing their favored policies if they know which politicians support the policies. We collected information on politicians' positions regarding the policy. For presidential candidates, we recorded the positions the candidates articulated in a debate conducted a few weeks before the election (though not all candidates participated). For representative candidates, we conducted a survey of 681 candidates by phone, telling them we planned to sensitize voters in their district about the policy and asking them what message they would like us to convey to voters on their behalf.⁵ Thus, for each district, we obtained a (non-comprehensive) list of candidates' positions on the policy, at both the presidential and the representative level. We then conducted a phone survey of households, randomly varying the provision of the list of candidates' positions, and asking about voting intentions and approval. Informing households of presidential candidates' positions reduced approval rates of candidates who opposed the expansion of the policy. Informing households of representative candidates' positions shifted households' stated voting intentions toward politicians who supported the policy, but only in competitive districts.⁶

These results may usefully inform attempts to understand the political economy of public good provi-

⁴See the appendix for both of these communications.

⁵Logistical constraints prevented us from gathering candidates' stated positions from the policy debates in time to provide them to voters before the election.

⁶Defined as districts with a margin of victory in the previous election (2011) of less than 5 percentage points.

sion in other contexts, but it is worth highlighting some important features of the context we study. This policy was funded by outside donors, so implementing it did not involve the same fiscal tradeoffs governments face when funding programs using domestic revenue. However, the phenomenon of donor-funded public goods is pervasive across much of developing world – especially in post-conflict states like Liberia.⁷ Nor was the policy costless for the government. It consumed the Ministry of Education’s physical, human, and political capital (the powerful teacher union strongly opposed the policy).⁸ In a similar vein, we note that the politicians we study are not directly responsible for the provision of the service in question, and their behavior can be characterized as credit-claiming (Cruz & Schneider, 2016).⁹ However, we hesitate to ascribe the effects we observe to voter naiveté. Most households in our survey correctly stated that the legislature was not responsible for the policy, and it is not necessarily irrational for voters to glean useful information from the way politicians react to events beyond their control (Ashworth, Bueno de Mesquita, & Friedenber, 2018).

We show that public good provision, and information about public good provision, can be electorally consequential in a young democracy. Our paper is the first to provide experimental evidence on electoral rewards for improved schools. It is also the first to examine electoral incentives for public goods through the lens of administrative vote tallies, politician responses, and voter attitudes in the same place at the same time. In this context, information about public goods seems to have the capacity to improve an extant electoral accountability mechanism: voters are happier and more engaged in places where the school policy took effect; candidates appeal to voters on the basis of the policy when they know it is effective; voters support the candidates who support the policy they like. We stress that these are just three steps in the process of democratic accountability, perhaps necessary but probably not sufficient to ensure the improvement of public good provision.

In the next section, we review the literature our study speaks to. Then we describe the school policy in question and detail our experimental methodology. Section 4 presents the results of the three experiments. Section 5 concludes.

⁷Liberia is one of the most aid-dependent countries in the world. In recent years, development aid has been roughly the same size as gross national income.

⁸The timing of this controversial program – implemented by a lame-duck president in her last year in office – is consistent with the prediction of the policy experimentation theoretical literature.

⁹Legislators could conceivably enshrine some version of the program into law, but for the most part education policy is centrally dictated by the executive branch.

2 Literature

Theory holds that political competition leads to growth by allowing voters to hold governments accountable for providing public goods (Stigler, 1972; Besley, Persson, & Sturm, 2010; Acemoglu, Naidu, Restrepo, & Robinson, 2014), and empirical work documents an association between political competition and government responsiveness to public preferences (Besley & Burgess, 2002). The underlying assumption is that voters reward politicians at the ballot box for championing their preferred policies. This assumption, while consistent with the observational data, has largely been taken as an article of faith in the literature until very recently.

One reason for this dearth of evidence is that exogenous policy shocks delivered at an electorally meaningful geographic level, and at an electorally salient time, are rare. Public good provision is often targeted to maximize parties' perceived incumbency advantage, e.g. by redistributing based on factors such as co-ethnicity or party affinity (Kramon, Posner, et al., 2016). A large body of work establishes that voters reward politicians for direct redistribution (Manacorda et al., 2011; Pop-Eleches & Pop-Eleches, n.d.) and clientelistic transfers (Wantchekon, 2003). There are comparatively few studies on the electoral returns to programmatic policy thought to bring broad-based growth – the few notable exceptions bear mentioning. Larreguy, Marshall, and Trucco (2015) find that Mexican voters reward federal politicians for land titling reforms, and Harding (2015) finds that incumbents in Ghana benefit electorally where roads improve. However, the picture admits significant nuance. de Kadt and Lieberman (2017) find that improvements in service provision *decrease* support for local incumbents in South Africa. Bursztyn (2016) finds that in poor Brazilian municipalities, increased public education spending is associated with worse reelection outcomes for incumbents.

If the literature provides little conclusive direct evidence on electoral returns to public good provision, another way to measure political accountability would be to test whether politicians behave as if voters hold them responsible for implementing good policies – i.e., do they react to information about their constituents' opinions? However, working with elected officials as experimental subjects is difficult. In the US, Butler and Nickerson (2011) find that New Mexican state legislators who receive information about their constituents' opinions are much more likely to vote accordingly. But it's not clear lawmakers in younger democracies would react similarly, and we know of no studies in the developing world which have measured this.

Finally, it should similarly be possible to test whether voters behave as if they can hold politicians

accountable by seeing how information on public good provision affects their political actions. Many studies provide information to voters, but nearly all focus on exposing corruption (A. V. Banerjee et al., 2014; De Figueiredo, Hidalgo, & Kasahara, 2011; Ferraz & Finan, 2008) or measuring performance (A. Banerjee, Kumar, Pande, & Su, 2011; Grossman & Michelitch, 2016). These are important avenues for accountability, but we know of little research studying the more direct channel of providing information related to the public goods politicians are supposed to produce. (Bursztyn (2016) is a notable exception here too, showing somewhat surprisingly that poor Brazilian respondents rate their local government more negatively upon learning that it has reallocated spending from cash transfers to education.)

Not all information is relevant to political action: for example, Lieberman, Posner, and Tsai (2014) find zero effect of providing children's' school information on parents' political action in Kenya. They give some examples of when information might reasonably be expected to drive political action: " ... for information to generate citizen action it must be understood; it must cause people to update their prior beliefs in some manner; and it must speak to an issue that people prioritize and also believe is their responsibility to address. In addition, the people at whom the information is directed must know what actions to take and possess the skills for taking these actions; they must believe that authorities will respond to their actions; and, to the extent that the outcome in question requires collective action, they must believe that others in the community will act as well."

3 Experimental design

3.1 Background

Education is often seen as one of the main public goods which it is a government's role to provide. However, in many developing countries, the public school system fails to provide the level of education deemed basic by international institutions. Liberia's moribund public education system exemplifies this failure. The civil wars of 1999-2003 and the Ebola epidemic of 2014 left the Ministry of Education with little capacity to run a national school system. An effort to clean thousands of ghost teachers from Ministry payrolls was cut short (New York Times, 2016), and while systematic data is scarce, teacher absenteeism appears common (Mulkeen, 2009). Nearly two-thirds of primary aged children are not in school, including over 80% of children in the poorest quintile, placing Liberia in the lowest percentile of net enrollment rates in the world, and at the 7th percentile in youth (15-24) literacy (EPDC, 2014). Demographic and Health Surveys show that among adult women who did not go to secondary school, only six percent can read

a complete sentence. In 2013, after all 25,000 high school graduates who sat the University of Liberia's college entrance exam failed, President Ellen Johnson Sirleaf said the education system was "a mess."

3.1.1 The policy

In response, the Liberian Ministry of Education announced a pilot program – "Partnership Schools for Liberia" (PSL) – which contracts the management of 93 government primary schools to one of eight private school operators in a Public-Private Partnership (PPP). The government (and donors) provide these operators with funding on a per-pupil level. The operators were given responsibility for (though not ownership of) the resources the government normally uses to provide education – schools, classrooms, materials, and teachers, and a grant equal to USD\$50 per pupil on top of that. In exchange, operators are responsible for the daily management of the schools, and can be held accountable for results. The operators include high profile, for-profit chains with investors like Bill Gates, Mark Zuckerberg, the World Bank, and DFID. Other operators are non-profit NGOs based in Liberia and abroad, and one operator is a respected Liberian religious institution.

3.1.2 The evaluation

Based on criteria established by the evaluation team, the Ministry of Education, and operators, 185 PSL-eligible schools across 12 of Liberia's 15 counties were identified. These schools are not a representative sample of public schools in the country – they have better facilities, internet access, and road access than the average school in the country. The eligible schools were split into pairs matched on administrative data, and treatment was assigned randomly within matched pairs.

Romero et al. (2017) showed that the program increased test scores by 60%, teacher attendance by 50%, and satisfaction of both students and parents by about 10%. The evaluation also showed that at least some of the critiques of program detractors were well-founded: one operator chose to enforce class size limits, forcing hundreds of students to leave their regular school (though nearly all enrolled elsewhere). The same operator also requested reassignment of 74% of the teachers in the schools it operated. The official report of these results became public about one month before nationwide elections for the presidency and the House of Representatives.

3.1.3 The political context

The program has enjoyed a relatively high profile, garnering attention from local and international news outlets, and receiving condemnation from a UN Special Rapporteur, who wrote that Liberia was abrogating its responsibilities under international law. The National Teacher Association of Liberia staged a strike, calling for the resignation of the Minister of Education. In response, students blocked the main highway to the country's international airport, demanding that the government and the teachers' union send the teachers back to class.

The policy has been championed by the incumbent president, Ellen Johnson Sirleaf of the Unity Party (UP). At the time of the policy's implementation, Sirleaf was nearing the end of her second (and constitutionally-mandated final) term as president. Her vice president, Joseph N. Boakai, was the UP's presidential candidate in the 2017 election. Reforming the education sector has been a priority for the administration. Both the president and the vice president championed PSL and went out of their way to associate themselves with the program during its first year. (President Sirleaf attended a Flag Day celebration at one of the private operator's schools, and Vice President Boakai spoke at the graduation ceremony for the same operator's teacher training course.) However, as the election neared, the politically powerful National Teachers Association of Liberia (NTAL), which opposes the program, became more vocal in their opposition. Neither Boakai nor any of the other presidential candidates have given this policy a strong role in their campaign.

A few factors make it difficult to predict how voters will react to this school policy. On the one hand, the program brought more resources into communities and improved test scores, something which many parents likely perceived. On the other hand, it could alienate voters who see the privatization of education provision as a dereliction of the government's duty. Voters may be inclined to reward politicians for acknowledging the inadequacy of public provision and offering other options. Or they may punish politicians for abdicating their fundamental responsibility to provide education to private entities with limited accountability, many foreign and some for-profit.

3.2 Experimental design: Overview and timeline

In this paper, we present results from three different experiments. We devote one subsection below to the design of each experiment, including sampling, balance, and summary statistics. The first experiment uses the random assignment of schools to the policy in order to measure the policy's impact on political

attitudes and outcomes. We collected this data at the end of the first school year in which the policy was in place: May/June 2017. The second experiment varies the provision of RCT evidence on the policy to political candidates and measures how it affects their support for the policy. It was carried out on 14 September, 2017. The third experiment varies the provision of information on politicians' support of the policy to households and measures how it affects their voting intentions. It was carried out between 2 October and 9 October, 2017. The election was held on 10 October, 2017. The household experiment described here was pre-registered along with pre-analysis plans at <https://www.socialscisceregistry.org/trials/1501> (policy impact on political attitudes) and <https://www.socialscisceregistry.org/trials/2506> (information experiments for candidates and households).

3.3 Policy experiment: Design and Sampling

We examine outcomes in two sets of data using the random variation from the policy evaluation itself: household survey data and official electoral results. The household data were collected from a survey of the households of a random subset of students in the evaluation of the policy.¹⁰

Electoral data at the voting precinct level were collected from the website of the National Elections Commission (NEC) of Liberia (<http://www.necliberia.org>). Precinct coordinates were obtained from NEC. In all our analyses involving the policy's impact on election results, there are various ways to link voting precincts to treatment and control schools. While the randomized evaluation included only 3% of the country's public primary schools, 61% of the 2,080 polling places for the 2017 election were within 10km of one of these RCT schools. We consider a 10km radius as the catchment area for a school, and limit our attention to schools within 10km of *exactly* one school in the RCT in order to be able to assign each polling place cleanly to either treatment or control.^{11 12} This choice of sample has implications for external validity – the schools we examine here are, mechanically, more remote than the average school in the RCT – but maintains internal validity.

3.3.1 Policy experiment: Balance and summary statistics

The PSL policy was randomly assigned within pairs of schools that had been matched on geography, resources, and other variables. We check that our treatment definition provides a set of polling places

¹⁰More details on this sampling process can be found in Romero et al. (2017).

¹¹The appendix will eventually include analyses employing alternative samples and specifications.

¹²Due to poor data and logistical constraints, the list of schools which ended up being operated privately differed slightly from the list of schools originally assigned to treatment – Romero et al. (2017) gives a more complete discussion of the treatment assignment process, and uses the originally-assigned intent-to-treat schools in its analyses. We likewise assign treatment according to ITT.

which are balanced on 2011 election outcomes. Columns 1 and 2 show the means and standard deviations for the treatment and control group respectively. Column 3 shows the raw difference between the two, and Column 4 shows the difference when controlling for matched-pair fixed effects.

Table 1: Balance – Policy Experiment (2011 Election Results)

	Treatment	Control	Difference	Difference (F.E.)
Precinct-level data (N = 271)				
Distance to nearest RCT school	5.34 (2.73)	6.07 (2.95)	-0.73 (0.45)	-0.47 (0.42)
Num. public primary schools within 10 km	9.20 (4.04)	7.84 (4.08)	1.37 (1.14)	1.26 (1.09)
Voters registered 2011	1,119.39 (759.11)	861.66 (581.48)	257.73 (166.93)	127.15 (81.84)
Invalid pres. vote share 2011	0.08 (0.05)	0.07 (0.04)	0.00 (0.01)	-0.00 (0.01)
Voter turnout (pres.) 2011	0.71 (0.07)	0.73 (0.08)	-0.02 (0.01)	0.01 (0.01)
UP pres. vote share 2011	0.38 (0.17)	0.43 (0.21)	-0.06 (0.05)	0.00 (0.03)
CDC pres. vote share 2011	0.28 (0.17)	0.20 (0.15)	0.08* (0.05)	-0.02 (0.03)
LP pres. vote share 2011	0.11 (0.15)	0.08 (0.17)	0.03 (0.06)	0.02 (0.04)
INVALID	0.07 (0.04)	0.07 (0.04)	0.00 (0.01)	-0.01 (0.01)
Voter turnout (rep.) 2011	0.70 (0.06)	0.72 (0.08)	-0.02 (0.01)	0.01 (0.01)
UP rep. vote share 2011	0.20 (0.20)	0.19 (0.21)	0.01 (0.07)	0.00 (0.03)
CDC rep. vote share 2011	0.15 (0.15)	0.11 (0.16)	0.04 (0.04)	-0.00 (0.03)
Invalid rep. vote share 2011	0.11 (0.14)	0.10 (0.16)	0.01 (0.02)	-0.03 (0.02)
No UP rep. candidate 2011	0.16 (0.36)	0.13 (0.33)	0.03 (0.08)	-0.08** (0.04)
No CDC rep. candidate 2011	0.19 (0.40)	0.32 (0.47)	-0.12 (0.08)	-0.13** (0.06)
No LP rep. candidate 2011	0.10 (0.31)	0.14 (0.35)	-0.04 (0.04)	-0.11** (0.04)
Rep. margin < 5%	0.18 (0.39)	0.33 (0.47)	-0.15 (0.11)	-0.19 (0.15)

Sample consists of precincts within 10 km of a school in the Partnership Schools RCT evaluation. Precincts are classified as treated if they are within 10 km of a school operated as a partnership school and control if not.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

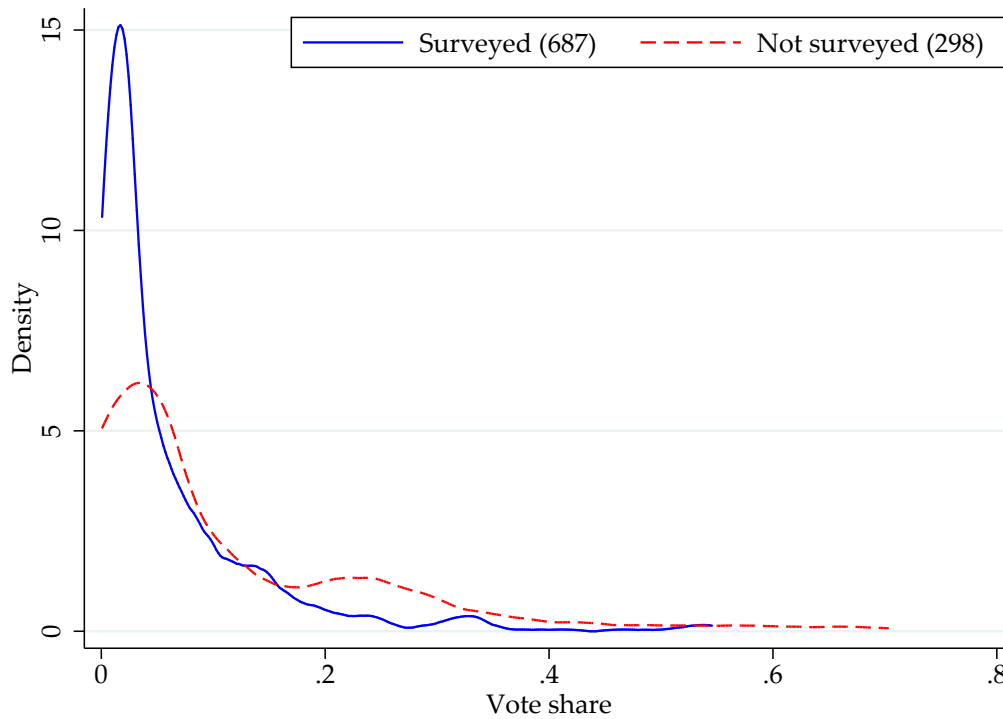
3.4 Candidate experiment: Design and Sampling

Next we examine the idea that information frictions may play an important role in campaign choices for politicians. A series of policy debates were held among House candidates in most of the legislative districts in the country. These debates were held in front of a live audience of constituents, and were later recorded and rebroadcast in the district. We focus on 8 of these districts in Bomi, Grand Cape Mount, and Montserrado counties where debate moderators were willing to add a question to the debate about the PSL program. Within each of these districts, we selected a random group of candidates to send information to. A day or two before each debate, we sent each selected candidate an email and a text message summarizing the findings of (Romero et al., 2017). The Appendix contains the full text of the messages we sent to treated candidates. Candidates in each of these districts then took part in these debates, and the audio transcripts were transcribed by the Liberian Observer newspaper. We then had research assistants (who were not aware of the treatment assignment) code the transcripts of the debates.

3.5 Household experiment: Design and sampling

As preparation for our household survey, we conducted a phone survey of as many House of Representatives candidates as we could find – 681 (of 992 total). The pattern of participation suggests that important candidates were disproportionately unlikely to be represented in our sample – perhaps due to a high opportunity cost of time or a greater risk aversion. However, we did not exclusively reach inconsequential candidates: of the 66 House incumbents standing for reelection our sample includes 22, as well as 3 of the 7 retiring candidates. We also reached 32 of the 73 candidates who eventually won, and 13% of our sample ended up as the winner or the runner-up in their district. Figure 1 plots the density of vote shares for candidates we did and did not survey.

Figure 1: Distribution of vote shares for surveyed and unsurveyed candidates



Note: Figure excludes about 15 surveyed candidates from two districts for which election results were still pending as of the time of writing

A few summary statistics are worth mentioning here. Most candidates had heard of the school policy's name ("PSL"), and nearly all had heard of at least one of the operators associated with it. Nearly all supported the idea of public-private partnerships in education, but nearly all also approved of the teachers' union (which officially opposes the PSL program). Most candidates think that their voters (correctly) attribute responsibility for the school policy to the executive branch, but only about 19% think that more than 'a few' of their voters have heard of the policy.

We told candidates we would be sensitizing voters in their districts, and offered to convey the candidate's position on the program to the voters. We asked candidates to select the statement that most closely aligned with their view: the PSL program should be expanded and paid for with tax revenues; the PSL program should be tested before any significant expansion; or the PSL program should be immediately discontinued. 52% of candidates said the program should be expanded; 30% thought it should be tested further first; and 7% said it should be discontinued.

Our household information experiment was also conducted as a phone survey. We called households who had been previously interviewed (in person) as part of the 1-year evaluation of the school policy. We reached 494 of the 833 households for whom we had at least one unique phone number (59%).

It is important to note that this sample is not nationally representative, in that it consists only of households with children in (a non-representative set of) public primary schools. Our results should not be interpreted as informative about the average Liberian voter, but rather a subpopulation for whom a school policy may be expected to be particularly salient.

The control condition consisted in this brief mention of the three presidential candidates who took part in a debate:

Thank you. We are near the end of the survey. Now I just want to give you some information about the candidates.

Liberia's last presidential debate was on September 26th. The three candidates who attended the debate were: MacDella Cooper from LRP, Alexander Cummings from ANC, and Mills Jones from MOVEE.

The treatment condition included that prelude as well as the candidates words regarding the school policy from that debate:

In that debate, each candidate made a statement about Partnership Schools or PSL. I'm going to read you a part of each candidate's statement. Please listen:

MacDella Cooper said: "It's a test project. Maybe at the end of the test, we'll see . . . Putting the Liberian public school in the hands of a private organization, I don't see the benefit yet."

Alexander Cummings said "We should also be open to different solutions. And we can't be fixated on only one traditional way of doing things. We got to be creative. We got to be bold."

Mills Jones said: "We are not going to do it. It suggests to me that we have given up on our own capacity to solve our problems and so we must look outside for help. We're not going to do that."

The treatment condition also included a list of the representative candidates we had spoken to in the candidate survey, who had asked us to let their voters know their position on the school policy:

Some of the candidates for Representative in YOUR district also have made statements about PSL, which they wanted us to share with you. Please listen carefully:

*These candidates say PSL should be taken into more schools, and supported by the national budget:
[names]*

These candidates say PSL needs to be tested more before making a decision: [names]

*These candidates say PSL should be stopped immediately, and normal government schools should get
that support: [names]*

The median (interquartile range) household in our sample received information about the positions of 4 (2,6) candidates, representing 30 (24,46)% of the candidates on the ballot in their district.

3.5.1 Balance and summary statistics: Households

We check balance of our household subsample on pre-information-treatment characteristics.

Table 2: Balance – Household experiment (N = 494)

Variable	Candidate			N
	Control	Info	Difference	
School policy treatment	0.543 (0.499)	0.514 (0.501)	-0.028 (0.045)	494
Fraction of candidates about whom we give information	0.354 (0.165)	0.317 (0.148)	-0.037 (0.015)**	467
Knows current Representative's name	0.822 (0.383)	0.866 (0.341)	0.045 (0.033)	494
Satisfied with Representative	0.342 (0.475)	0.308 (0.463)	-0.034 (0.043)	477
Related to a rep. candidate	0.170 (0.376)	0.184 (0.388)	0.014 (0.034)	492
Related to member of teachers' union	0.333 (0.472)	0.277 (0.448)	-0.056 (0.042)	485
Attended any campaign event	0.358 (0.480)	0.329 (0.471)	-0.029 (0.043)	489
It's good for gov to work w/ private companies to provide sch	0.962 (0.190)	0.942 (0.235)	-0.021 (0.020)	480
Any candidate has talked about PSL	0.146 (0.354)	0.160 (0.367)	0.014 (0.034)	444
Heard of PSL	0.644 (0.480)	0.615 (0.487)	-0.028 (0.044)	494
Heard of any operator	0.862 (0.345)	0.842 (0.365)	-0.020 (0.032)	494
Legislature created PSL	0.008 (0.090)	0.008 (0.090)	0.000 (0.008)	494
Executive branch created PSL	0.259 (0.439)	0.271 (0.446)	0.012 (0.040)	494
Reps. have authority to change PSL	0.294 (0.457)	0.263 (0.441)	-0.032 (0.047)	359
PSL should be expanded and funded through the national budget.	0.803 (0.398)	0.782 (0.413)	-0.021 (0.037)	478
Children learn more in PSL schools	0.827 (0.379)	0.850 (0.358)	0.022 (0.035)	446

Notes

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

The assignment of the Candidate Information treatment is balanced in terms of the original PSL treatment. Households randomly assigned to receive candidate information also happened to be in districts for which we had information for slightly fewer candidates.

Households in our sample are reasonably well-informed. Almost all respondents to the household survey could correctly name their current Representative, but only 34% are satisfied with him or her. 36%

have attended at least one campaign event for a Representative candidate, and 17% are related to someone running for office. A third are related to a member of the teachers' union, but almost everyone supports the idea of public-private partnerships in education, and 4 in 5 think children learn more in PSL schools and that the policy should be expanded. But it's not clear to which political actors voters attribute the policy, if any. Nearly everyone (correctly) does *not* credit the legislature with creating the policy; 29% believe representatives have the authority to change it, which could be true in a broad sense. Only a quarter believed (correctly) that the executive branch had created it. 15% said they had heard at least one candidate mention PSL, a group we will come back to later to help define outcomes.

4 Experimental results

In this section we present three sets of results.

1. The impact of the randomized school reform itself on political outcomes
2. The effect of policy evidence on candidates' positions
3. The effect of information about candidates' positions on households' stated voting intentions

4.1 Policy experiment results

In this section we present two subsets of results which exploit the random variation of the school policy itself: survey outcomes on political attitudes, measured in May/June 2017; and vote tallies from the October 2017 election at the precinct level.

4.1.1 Survey outcomes

Table 3: Selected household and teacher survey outcomes (policy midline)

	Treatment	Control	Difference	Difference (F.E.)
Household midline survey (N = 1207)				
Satisfied w/ child's edu (HH)	0.743 (0.437)	0.689 (0.463)	0.054* (0.028)	0.069*** (0.020)
Gov performance on edu is good (HH)	0.566 (0.496)	0.549 (0.498)	0.017 (0.032)	0.034* (0.020)
Schools top priority for gov spending (HH)	0.811 (0.392)	0.739 (0.440)	0.072*** (0.026)	0.068*** (0.020)
Liberia is moving forward (HH)	0.577 (0.494)	0.507 (0.500)	0.070** (0.032)	0.071*** (0.019)
Teacher midline survey (N = 752)				
Satisfied w/ teacher union (teachers)	0.394 (0.489)	0.434 (0.497)	-0.040 (0.046)	-0.089** (0.040)
Liberia is moving forward (teachers)	0.745 (0.436)	0.690 (0.464)	0.056 (0.038)	0.053* (0.030)

This table presents the mean and standard error of the mean (in parentheses) for the control (Column 1) and treatment (Column 2) groups, as well as the difference between treatment and control (Column 3), and the difference taking into account the randomization design (i.e., including "pair" fixed effects) in Column 4. Standard errors are clustered at the school level. The sample is the original (intent-to-treat) treatment and control allocation.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 3 shows how the school policy itself affected some political attitudes of the households affected by it. Households became more impressed with the government's performance on schools and more likely to say schools were their top priority for government spending. Teachers likewise became more hopeful about the country's direction in general, and became less satisfied with the teachers' union. It should be noted, however, that many other political outcomes were tested; most saw no effect (see Appendix ??).

4.1.2 Precinct-level voting outcomes

Using precinct-level election returns, we show how the school policy affected voting patterns. While the randomized evaluation included only 3% of the country's public primary schools, 61% of the 2,080 polling places for the 2017 election were within 10km of one of these RCT schools. We consider a 10km radius as the catchment area for a school, and exclude all polling centers more than 10km from any RCT school. We also limit our sample to those polling places within 10km of *exactly* one RCT school. We then define treated polling places as those within 10km of exactly one treated school (and no treated schools), and

control polling places as those within 10km of exactly one control school (and no treated schools).¹³

Table 4: School policy effects on 2017 vote share

	Ruling party (pres. 1st rd.)	Ruling party (pres. runoff)	Ruling party (rep.)	Incumbent (rep.)	Share invalid (rep.)	Turnout (rep.)	Voters registered
Treated school (ITT) within 10 km	-0.041 (0.034)	-0.049* (0.025)	-0.026 (0.028)	0.125*** (0.044)	0.003 (0.003)	0.030* (0.016)	285.548*** (92.103)
N	303	303	303	303	303	303	303
DV Mean	0.239	0.344	0.106	0.208	0.054	0.756	1176.703
Controls	N	N	N	N	N	N	N
Pair FE	Y	Y	Y	Y	Y	Y	Y

Standard errors clustered by school. School matched-pair fixed effects included. Sample consists of precincts within a 10-km radius of exactly one school in the Partnership Schools RCT evaluation. Precincts are classified as treated if this school is assigned to be a partnership school and control if not.

* p<0.10, ** p<0.05, *** p<0.01

The school policy increased the number of registered voters in nearby polling stations, by about 25%. Despite this increase in the denominator, it also seems to have increased turnout, by about 3 percentage points. The program also increased the vote share of incumbent representatives, even though they had no hand in designing or implementing the program. This could suggest that incumbents successfully claimed credit for the program.

4.1.3 Heterogeneity in election outcomes by party incumbency

We test whether the electoral gains were concentrated by party incumbency by interacting the treatment variable with a (non-randomly-assigned) dummy for whether the Unity Party, the party of the incumbent president, was also the party of the incumbent representative in the district.

¹³Due to poor data and logistical constraints, the list of schools which ended up being operated privately differed slightly from the list of schools originally assigned to treatment— see Romero et al. (2017) for a more complete discussion of the treatment assignment process. That paper uses the originally-assigned intent-to-treat schools in its analyses. In this paper’s analyses, we defined “treated” schools as schools which actually came under private operation. Using the intent-to-treat assignment does not substantively change the results, which are available upon request.

Table 5: School policy effects on 2017 election, interacted with dummy for incumbent UP

	Ruling party (pres. 1st rd.)	Ruling party (pres. runoff)	Ruling party (rep.)	Incumbent (rep.)	Share invalid (rep.)	Turnout (rep.)	Voters registered
Treated school (ITT) within 10 km	-0.051 (0.031)	-0.057* (0.031)	-0.071** (0.032)	0.074 (0.047)	0.006 (0.006)	0.020 (0.019)	288.962* (147.292)
Incumbent UP	0.020 (0.027)	0.016 (0.030)	-0.026 (0.052)	-0.078 (0.054)	0.003 (0.008)	-0.025 (0.023)	1.990 (138.998)
Treated school (ITT) within 10 km × Incumbent UP	0.088*** (0.030)	0.069 (0.045)	0.160*** (0.053)	0.130* (0.066)	-0.006 (0.013)	0.008 (0.022)	-13.820 (163.748)
N	303	303	303	303	303	303	303
DV Mean	0.239	0.344	0.106	0.208	0.054	0.756	1176.703
Controls	N	N	N	N	N	N	N
Pair FE	Y	Y	Y	Y	Y	Y	Y

Standard errors clustered by school. School matched-pair fixed effects included.

* p<0.10, ** p<0.05, *** p<0.01

This seems to suggest that the lion’s share of electoral gains went to incumbent representatives from the ruling party. In places where the ruling party was in charge, the school program also increased vote share for the ruling party presidential candidate. In other places, it seems to have increased the vote share of other parties’ candidates for both president and representative, consistent with a story in which representatives from all parties sought somewhat successfully to claim credit, creating an incidental benefit for the presidential candidate at the top of the ticket.

4.1.4 Heterogeneity in election outcomes by policy impact

Did these electoral effects happen *because* of the school improvements, or were they driven by something else – for example, more money being spent in the village? We can’t directly answer this question, but we can see whether the effect was largest in places where the effects on test scores were biggest. Because the evaluation was designed using matched pairs, we can see whether treatment mattered more in places where the policy had a big effect. Table 6 shows the effect of interacting treatment assignment with the size of the local treatment effect – defined as the difference between a matched pair’s treatment and control schools’ average test scores.

Table 6: School policy effects on 2017 vote share, interacted with treatment effect

	Ruling party (pres. 1st rd.)	Ruling party (pres. runoff)	Ruling party (rep.)	Incumbent (rep.)	Share invalid (rep.)	Turnout (rep.)	Voters registered
Treated school (ITT) within 10 km	-0.063 (0.040)	-0.076*** (0.026)	-0.076*** (0.017)	0.114** (0.054)	0.002 (0.004)	0.045** (0.017)	303.085*** (113.340)
Treated school (ITT) within 10 km × TE above median	0.097 (0.062)	0.117** (0.053)	0.220*** (0.076)	0.049 (0.083)	0.004 (0.007)	-0.069** (0.034)	-77.561 (154.635)
N	303	303	303	303	303	303	303
DV Mean	0.239	0.344	0.106	0.208	0.054	0.756	1176.703
Controls	N	N	N	N	N	N	N
Pair FE	Y	Y	Y	Y	Y	Y	Y

Standard errors clustered by school. School matched-pair fixed effects included.

* p<0.10, ** p<0.05, *** p<0.01

Places where the policy had a bigger impact saw increased vote shares for representatives and presidential candidates from the presidential-incumbent Unity Party, consistent with voters rewarding the responsible politicians.

Our results from information experiments with candidates and households help illuminate the mechanisms behind these findings.

4.2 Candidate experiment results

4.3 Debate information results

Table 7: Policy information on candidates' debate positions

	Participated	Support PSL (ITT)	Support PSL (TOT)	Have knowledge of program (TOT)
debateinfo	0.017 (0.082)	0.097* (0.054)	0.216** (0.097)	0.013 (0.097)
N	120	120	65	65
DV Mean	0.542	0.117	0.215	0.317
Controls	N	N	N	N
District FE	Y	Y	Y	Y

Robust standard errors in parentheses.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

The information we sent (which can be seen in the appendix) had no impact on candidates' likelihood to show up to debates. Only about half of the candidates showed up to the debate. In an intent-to-treat framework that treats debate non-participation as a zero on the outcome measure, information increased candidates' likelihood of saying in the debate that they support the PSL program, by about 10 percentage points. Using a treatment-on-the-treated (TOT) framework that restricts attention to the candidates who in fact showed up, the effect of information was 22 percentage points – on a base of 22 percentage points. The information *doubled* candidates' likelihood of saying they support the policy.

We also coded candidates' statements for whether they demonstrated any actual understanding of how the program works. The information we sent had no impact on whether candidates seemed to know what the program does in their debate statements.

4.4 Household experiment results

Here we describe the results of providing households with information about candidates' positions.

First we present results for presidential voting intentions and approval.

Table 8: Candidate information's effect on household voting intentions: Presidential candidates

	Vote any pres. candidate	Vote pro-PSL pres. candidate	Vote anti-PSL pres. candidate 1	Vote anti-PSL pres. candidate 2
Candidate info	-0.017 (0.043)	0.053 (0.042)	0.006 (0.007)	-0.017 (0.024)
N	485	305	305	305
DV Mean	0.625	0.164	0.003	0.036
Controls	N	N	N	N
District FE	Y	Y	Y	Y

Robust standard errors in parentheses clustered at the school. Sample consists of a subset of households originally contacted as part of PSL midline evaluation, reached by phone for this follow-up survey about one week before the election on 10 October 2017.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 9: Candidate information's effect on households' approval: Presidential candidates

	Approval of pro-PSL candidate	Approval of anti-PSL candidate 1	Approval of anti-PSL candidate 2
Candidate info	0.052 (0.054)	-0.090** (0.040)	-0.142*** (0.047)
N	358	355	359
DV Mean	0.332	0.124	0.206
Controls	N	N	N
District FE	Y	Y	Y

Robust standard errors in parentheses clustered at the school. Sample consists of a subset of households originally contacted as part of PSL midline evaluation, reached by phone for this follow-up survey about one week before the election on 10 October 2017.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Information about candidates' positions did not have a statistically significant effect on stated voting intentions or presidential candidates, although the signs of the effects do go in the direction we expect. We also asked households to rate their approval of six of the frontrunner candidates. Candidate information reduced approval rates for candidates who opposed PSL in the presidential debate, and the effect sizes are sizeable compared to the mean approval.

We next turn our attention to information about *representative* candidates positions on the PSL policy.

Table 10: Candidate information's effect on household voting intentions: Representative

	Vote Rep. PSL Yes	Vote Rep. PSL No Info	Vote Rep. UP	Vote Rep. Incumbent	Vote Rep. We gave info	Vote Any Rep.
Candidate info	0.012 (0.040)	0.058 (0.052)	0.062 (0.051)	0.016 (0.049)	-0.040 (0.074)	-0.054 (0.045)
N	317	317	236	272	163	494
DV Mean	0.139	0.707	0.195	0.279	0.730	0.605
Controls	N	N	N	N	N	N
District FE	Y	Y	Y	Y	Y	Y

Robust standard errors in parentheses clustered at the school. Sample consists of a subset of households originally contacted as part of PSL midline evaluation, reached by phone for this follow-up survey about one week before the election on 10 October 2017.

* p<0.10, ** p<0.05, *** p<0.01

Candidate information has no discernible effect on voting patterns for representatives, on average. Households receiving the information were no more likely to vote for representatives who we had told them supported (or opposed) the expansion of the policy, or for candidates for whom we gave no information.

4.5 Heterogeneity in household outcomes

Table 11: Voting for representatives by competitiveness of elections

	Vote Rep. PSL Yes	Vote Rep. PSL No Info	Vote Rep. UP	Vote Rep. Incumbent	Vote Rep. We gave info	Vote Any Rep.
Candidate info	-0.061 (0.046)	0.153** (0.060)	0.031 (0.066)	0.022 (0.065)	-0.014 (0.076)	-0.061 (0.052)
Candidate info × 2011 District won by < 5 pp	0.242*** (0.080)	-0.349*** (0.106)	0.097 (0.105)	-0.082 (0.122)	-0.024 (0.162)	-0.057 (0.098)
N	317	317	236	272	163	467
DV Mean	0.139	0.707	0.195	0.279	0.730	0.640
Controls	N	N	N	N	N	N

Robust standard errors in parentheses. Sample consists of a subset of households originally contacted as part of PSL midline evaluation, reached by phone for this follow-up survey about one week before the election on 10 October 2017.

* p<0.10, ** p<0.05, *** p<0.01

In competitive districts (as measured by whether the election in 2011 was decided by less than 5 percentage points), information about which candidates support the policy sharply increased voting intentions for one of the candidates who supports the policy. This suggests that information may only be useful if people

feel elections are competitive.

5 Conclusion

Accountability for public good provision is assumed in many models of political economy, but the empirical evidence for these mechanisms is slim. Observing the low quality of many democratic countries' public goods compounds the puzzle. Democratic accountability for public service provision has a lot of moving parts. It seems to require that voters pay attention to public goods and to what politicians say and do about them, and it seems to require that politicians believe voters notice these things and act on them. This is probably just the tip of the iceberg of what democratic accountability requires, but at least as regards this particular school policy in 2017 (especially in competitive districts), meeting those requirements can be facilitated by the wider availability of more accurate information about policies and politicians.

We show that the policy in question led to increased satisfaction with the government's performance on education, increased voter registration, and greater vote share for incumbent representatives. Even though these representatives had little to no hand in creating the policy, it is not necessarily irrational for voters to reward them, since viewing the politicians' reaction to the policy could give voters new information about the candidates (Ashworth et al., 2018). Information about the policy's effect made candidates more likely to voice support for the policy in public debates. Information about which candidates support the policy, in turn, led households to disapprove of presidential candidates who disfavor the policy, and – in competitive districts – to vote more frequently for the representative candidates who support the policy.

Some important caveats apply: our study does not involve a representative sample of polling stations, politicians, or households. The polling stations we studied are those near relatively remote schools. The politicians we studied are those who selected themselves into the debate. The voters are the parents of children in a selected set of government primary schools. But in each case, the experimental results are internally valid.

It should also be noted that how the effects of more information play out in equilibrium is far from certain. Policy evidence might be met with policy counterevidence, and it remains unclear how all the relevant actors would judge the authority of competing claims. Elements of society who lose out with the strengthening of inclusive economic institutions have incentives to throw every wrench in the gears of democratic accountability – in the words of Acemoglu (2010), “large-scale shocks and policy interventions will create political economy responses from those who see their economic or political rents threatened or

from those that see new options to increase these rents.”

Still, exogenous changes to public good provision are rare. The Partnership Schools for Liberia program, coupled with its timing just before the October 2017 Liberian presidential election, provides a valuable opportunity to document that the electoral behavior of both voters and politicians is responsive to improved public good provision in a young democracy.

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Appendix

A Information provided to candidates

A.1 Email

Esteemed Candidate:

As an independent research team, we have recently conducted a rigorous study of the Ministry of Education's Partnership Schools for Liberia program, in conjunction with the Center for Global Development in Washington DC, and Innovations for Poverty Action in Liberia.

Because education is such an important issue for so many Liberians, we want to make sure that in this election season, candidates know what we learned about this policy.

Partnership schools are fee-free public schools managed by private operators or NGOs

93 public primary schools became Partnership Schools, managed by one of eight private and NGO school providers. In Grand Cape Mount County, these providers were Bridge and Street Child. Teachers in Partnership Schools remain on government payroll, and buildings remain the property of the government and free to students. These schools also received extra resources. Partnership Schools must teach the Liberian national curriculum, but are free to innovate in how they conduct their schools.

Our study is independent and scientific

We conducted a rigorous study using state-of-the-art methodology: the randomized control trial (RCT). This allows us to identify, with a high degree of statistical confidence, how the outcomes for students and teachers differed between Partnership Schools and the traditional government schools we compared them with.

Partnership Schools increased learning

While our analysis shows that the program was not flawless, the children in Partnership Schools learned a lot more than children in the traditional public schools we compared them with. One year in a Partnership Schools was equivalent to roughly 1.6 years of schooling in a traditional public school. We saw these test gains in both math and English. We also learned that teachers in Partnership Schools were twice as likely to attend school, and were much more likely to spend their time actually teaching in school. Parents and students were more satisfied in Partnership Schools. We also identified some problems: in some especially big Partnership Schools, students had to transfer to different schools. And one private operator reassigned over half of its teachers. We've made the Ministry aware of these problems, and they

have committed to fix them.

We've attached to this email the policy brief that describes these results in more detail. It, and the full report, can be found online at <https://www.cgdev.org/blog/can-outsourcing-improve-liberias-schools-preliminary-rct-results>

We hope this information helps you decide which policies to support this election season.

Sincerely,

Dr Justin Sandefur, PhD

Center for Global Development

Mauricio Romero

University of California, San Diego

Wayne Sandholtz

University of California, San Diego

Figure A.1: Policy brief



After one year, public schools managed by private operators raised student learning by 60 percent compared to standard public schools. But costs were high, performance varied across operators, and contracts authorized the largest operator to push excess pupils and under-performing teachers into other government schools.

“Partnership Schools” are free, public schools managed by private operators

- **Liberia's education system lags behind most of the world in both access and quality.** Net primary enrollment was only 38 percent in 2015, and in 2013, among adult women who finished elementary school, only 25 percent could read a complete sentence.¹
- **Under the new Partnership Schools for Liberia (PSL) program, the Government of Liberia delegated management of 93 public schools to eight operators:** BRAC, Bridge International Academies, Youth Movement for Collective Action², More than Me, Omega Schools, Rising Academies, Stella Maris, and Street Child.
- **PSL schools remained public schools:** teachers in PSL schools remained on government payroll; schools remained property of the government and free to students; and contractors were prohibited from screening students based on ability or other characteristics.
- **In addition to new management, the PSL program also brought extra resources.** The government runs ordinary public schools on a budget of approximately \$50 (USD) per pupil, ranking 145th among 161 countries for which recent World Bank data is available. PSL operators received an additional \$50 per pupil from a pool of philanthropic funds managed by Ark, an education charity; the total of \$100 was deemed a realistic medium-term goal for public expenditure on primary education nationwide (and would represent a jump to 134th place). While teachers are in short supply in Liberia's public schools, the Ministry of Education made special staffing arrangements for PSL, providing more than two additional teachers per school.³
- **The evaluation randomly assigned existing government schools to become PSL schools.** Liberia's Ministry of Education commissioned a rigorous, independent evaluation of PSL's effectiveness. Because assignment of schools to the PSL or comparison groups was random, differences between the two groups can be attributed to the program. Schools were randomized *after* operators agreed on a school list, and students in the sample were selected from the enrollment logs of the school year *before* operators arrived. Therefore the results are not biased by operators selecting schools or rejecting students.

¹ The World Bank. “School enrollment, primary (% net).” World Development Indicators, accessed August 28, <http://data.worldbank.org/indicator/SE.PRM.NENR?locations=LR>.

Liberia Institute of Statistics and Geo-Information Services. *Liberia demographic and health survey 2013*. Liberia Institute of Statistics and Geo-Information Services.

² Formerly “The Liberian Youth Network.”

³ Because they were not subject to the same contracts, neither Bridge International Academies nor Stella Maris received the extra \$50 per pupil.

Researchers: Mauricio Romero, Justin Sandefur, Wayne Aaron Sandholtz

Country: Liberia

Sample: 3,499 students in 185 schools

The results presented here are an early analysis of the PSL program's first-year data and represent our best effort given the limited timeframe in which policy decisions are being made. An academic advisory committee of experienced researchers has provided feedback, but these results are preliminary and have not yet been extensively peer-reviewed.

A.2 SMS

Esteemed Candidate: My name is Wayne Sandholtz and I am part of the independent research team which recently conducted a rigorous study of the Ministry of Education's Partnership Schools for Liberia program. Partnership Schools are fee-free public schools managed by private operators or NGOs. Our independent and scientific study shows that Partnership Schools increased learning by 60%. To read more, feel free to contact me at 0778228464 or wasandholtz@ucsd.edu, or visit <https://www.cgdev.org/blog/can-outsourcing-improve-liberias-schools-preliminary-rct-results>