

Do voters reward service delivery? Experimental evidence from Liberia

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(Job Market Paper)

December 10, 2019[†]

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Abstract

Do voters provide incentives for politicians to focus on public service delivery rather than patronage? This paper examines the electoral effects of a randomized Liberian school reform, previously shown to have caused increased teacher attendance and student test scores. In the subsequent election, this seemingly successful policy caused a 3pp (10%) *reduction* in average vote share for the responsible party's presidential candidate. This may reflect disruptions to the patronage network: treated teachers were less likely to support the incumbent party, staff polling booths, or campaign for candidates. However, heterogeneity in the policy's effectiveness reveals that voters did perceive and reward school quality improvements. In places where the treatment caused large improvements (reductions) in test scores, it also caused large electoral gains (losses). Electoral effects appear only for presidential, not legislative, candidates – suggesting that sophisticated voters attributed credit or blame at the correct level of government. Survey experiments among both candidates and voters confirm the picture of an electorate well informed about the policy's effectiveness and provenance. This paper highlights the political risks of moving from an electoral strategy based on patronage to one based on public service delivery – but it also suggests that increased policy effectiveness has the potential to counteract the opposition of entrenched interests.

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[†]E-mail: wasandholtz@ucsd.edu. I am particularly grateful to Mauricio Romero and Justin Sandefur. I also thank current and former Ministry of Education officials and staff, especially George K. Werner, Gbovadeh Gbilia, Binta Massaquoi, Nisha Makan, and Kammi Sheeler. Thanks for outstanding data collection to Alpha Simpson and his team at Q&A; Kou Gbaintor-Johnson and her team at Center for Action Research and Training; and Arja Dayal, Dackermue Dolo, and their team at Innovations for Poverty Action. Excellent research assistance was provided by Avi Ahuja, Dev Patel, Benjamin Tan, Chen Wei, Tobias Stodieck, Sophie Zhang, Sofia Godoy, and Hanxiao Dai. For helpful conversations, I thank Eli Berman, Prashant Bharadwaj, Jeremy Bowles, Daniel Butler, Michael Callen, Cesi Cruz, Brian Dillon, Mitch Downey, Rebecca Fraenkel, Sam Krumholz, Horacio Larreguy, Remy Levin, Craig McIntosh, Karthik Muralidharan, Simeon Nichter, Paul Niehaus, Agustina Paglayan, Mahvish Shaukat, and many seminar participants. A randomized controlled trial registry and pre-analysis plan are available at: <https://www.socialscienceregistry.org/trials/2506>. IRB approval was received from UCSD (protocols # 171543 and #171544) and the University of Liberia. The evaluation was supported by Center for Global Development and UCSD. I acknowledge financial support from the Institute for Humane Studies. The views expressed here are mine. All errors are my own.

1 Introduction

In democracies, public officials may seek votes through patronage or public service delivery. Political agency models typically operate on the assumption that while patronage can prevent improvements to the quality of service delivery, voters would reward such improvements if given the chance. Testing this assumption is vital for understanding democratic accountability. But it is a difficult assumption to test: elections are blunt instruments of preference expression, exogenous shocks to public services are rare, and even measuring impacts on service delivery quality is hard.¹

There are a number of ways voter accountability for service delivery may be diluted or crowded out by the patronage norms common to many developing democracies (Vicente & Wantchekon, 2009; Cruz, Keefer, Labonne, & Trebbi, 2018). Relatively low levels of education may imply a less-well informed electorate more susceptible to capture by special interests (Grossman & Helpman, 1996). Policymakers themselves may not know which policies improve service quality (Hjort, Moreira, Rao, & Santini, 2019). And even seemingly effective policies can be prevented from scale-up by civil servants in patronage jobs (Banerjee, Duflo, & Glennerster, 2008; Bold, Kimenyi, Mwabu, Ng, & Sandefur, 2018). Understanding the extent to which voters provide direct electoral incentives for public service delivery is necessary for characterizing the set of politically feasible policies.

This paper leverages a unique experimental setting to test how voters react to changes in public service quality, overcoming many of the obstacles that have hampered previous work. In 2016, the government of Liberia implemented a randomized school reform which improved school quality on average. Under the Partnership Schools for Liberia (PSL) program, the government outsourced the management of 93 public primary schools to one of several private school operators in a public private partnership. Crucially, the policy was evaluated via randomized control trial (RCT): treated schools were chosen randomly from within pairs of eligible schools matched

¹Cash transfers have been shown to be politically effective, but they represent a fundamentally different form of government spending than service provision. Also, a small but insightful quasi-experimental literature documents electoral effects of public goods, but these largely focus on visible public spending such as construction.

on pre-program characteristics.² This randomization, unusual for a government policy, gives the researcher a great degree of confidence in the policy's effect on the quality of public services: the program improved average student test scores by .18 standard deviations, and teacher attendance by 50%, after one school year (Romero, Sandefur, & Sandholtz, 2019). The randomization also provides rare exogeneity in *which areas* received these improvements to service quality, allowing a comparison of voters' reactions near treatment versus control schools. Attribution of credit was relatively straightforward: education policy is set the executive branch, and Liberia has no elected local politicians. The program's funding came from external donors and was earmarked specifically for the program; this allows a direct test of the electoral effect of this policy, unconfounded by voters' preferences over any possible counterfactual use of the funds. Finally, the policy was implemented in an election year and garnered significant press attention, making it an unusually salient education reform – the evaluation's results were announced in a press conference in September 2017, one month before the October general election. PSL therefore provides a rare exogenous shock to school quality, and furthermore, the unusual matched-pair randomization provides a valid counterfactual all along the distribution of shocks to school quality.

This paper shows that the policy caused a 3-percentage-point (10%) *reduction* in vote share for the responsible party's presidential candidate. This may have been partly due to popular anger over the actions of the highest-profile private school operator, which dismissed students from schools with large class sizes and secured the dismissal of a large fraction of its existing teachers. These moves made headlines and provoked heated opposition; teacher unions inside and outside the country came out strongly against the program. Teachers are often key political brokers (Pierskalla & Sacks, 2019; Larreguy, Montiel Olea, & Querubin, 2017), and this paper shows that the program appears to have disrupted traditional forms of patronage-based electioneering. It caused a reduction in teachers' support for the incumbent government – especially among unionized teachers. It also caused a decline in participation in political activities: staffing of polling stations and campaigning for candidates both went down by nearly a third among teachers in

²The schools in the study were not a representative sample of all Liberia's schools. But they did constitute 3.4% of public primary schools, and 8.6% of public primary and early-childhood education students in the country. The sample included schools from 13 of the 15 counties in Liberia.

treated schools.

However, the negative average effect does not imply that voters are insensible to the quality of service delivery. Heterogeneity in the policy's effectiveness reveals that voters were attuned to school quality, and rewarded or punished the responsible party in proportion. The matched-pair randomization design of the program means that the local average treatment effect measured for each individual pair of schools is unbiased. While comparing these LATEs to each other is not experimental, it is illustrative. The pair-level treatment effect on incumbent party vote share follows the gradient of the pair-level treatment effect on test scores. In other words, the treatment increased or decreased the incumbent party's vote share in proportion to how much it increased or decreased test scores (see Figure 5). Where test scores improved more than about 0.5σ (around the 80th percentile in the school distribution), the policy caused significant gains for the incumbent party's candidate. Where test scores worsened by more than about 0.3σ (around the 20th percentile), it caused significant losses. This suggests that voters were able to perceive changes to school quality and reward or punish them. It is notable that these electoral effects, both average and heterogeneous, appear only at the presidential level, suggesting voters attribute credit or blame to the appropriate political actor.

Survey experiments among both candidates and voters confirm the picture of a well-informed electorate. Two information experiments, conceived and implemented prior to the election, were designed to test whether information frictions inhibit accountability for public services. The first randomly provided evidence on the program's effectiveness and popularity to a pool of over 600 legislative candidates. The second randomly provided candidates' policy positions to the households of children from PSL study schools. However, baseline knowledge among both groups was so high as to leave little room for improvement, and the extra information does not seem to have shifted priors.

This paper highlights the political risks entailed in moving from an electoral strategy based on patronage to one based on public service delivery. Overall, a policy which improved test scores had a negative level effect on the electoral fortunes of the responsible candidate. The data suggests a loss of patronage support as a likely explanation – teachers became less likely

to engage in electioneering. However, the paper also suggests that increased policy effectiveness has the potential to counteract the opposition of entrenched interests. In places where the policy worked well, it created positive electoral effects which completely counteracted the negative level effect. Insofar as continued research can reduce uncertainty on how to improve public services, it may empower politicians who seek to bypass patronage-driven interests and appeal directly to voters who value service quality.

This paper contributes to the literature on electoral rewards for public services by focusing on the intensive margin of service quality. Handing out money, whether clientelistically or programmatically, is a proven vote-winner (Wantchekon, 2003; Vicente & Wantchekon, 2009; Manacorda, Miguel, & Vigorito, 2011; De La O, 2013; Golden & Min, 2013). This direct redistribution is distinct from investment in public goods and services, about which creative studies have also been written (Harding, 2015; Litschig & Morrison, 2013; Marx, 2018; Samuels, 2002; Zimmermann, 2018). But this literature mostly focuses on visible inputs such as roads and school-building.³ The current paper is unusual in being able to measure voters' reactions not to school inputs, but school quality. The most closely related paper is Dias and Ferraz (2019), which examines electoral returns to information about test scores in Brazil.

Another strand of literature this study adds to is that on the political economy of adoption and scale-up of reforms. From the theoretical literature, Majumdar and Mukand (2004) posit that policy experimentation sends a *negative* signal about politician ability to voters, which could lead to risk aversion in policy provision. This is consonant with the empirical findings of the current paper. A related paper is Bursztyn (2016), which shows that not all voters prefer public service spending to direct redistribution.

Finally, this paper contributes to the literature on voter information, by showing that even poorly-educated voters inform themselves about electorally consequential policies in sophisticated ways. A certain tension exists in the current literature between studies like Ferraz and Finan (2008), which shows that voters respond to information dispersed through known sources, and (Dunning et al., 2019), which finds no effect of information through new sources created by

³Closely related is the credit-claiming literature demonstrating that opportunistic politicians can sometimes receive unearned benefits from public good provision (Cruz & Schneider, 2017; Guiteras & Mobarak, 2015).

researchers. The present paper reconciles these findings somewhat by showing how difficult it is for a researcher to shock voters' priors about policies they already care enough about to change their votes over.⁴

The rest of this article is structured as follows: Section 2 provides context about Liberia and the policy; Section 3 outlines the empirical strategy; Section 4 presents the main results; Section 5 describes the information experiments; and Section 6 concludes.

2 Context

Education often ranks among governments' central public service responsibilities. 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."

2.1 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

⁴However, this paper contrasts somewhat with studies which have found plenty of room to improve politicians' informedness about their constituents' preferences (Butler & Nickerson, 2011; Liaqat et al., 2018; Hjort et al., 2019).

to one of eight private school operators in a public-private partnership. 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.

2.2 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. (2019) showed that the program increased test scores by around 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 independent evaluation team’s report of these results was released in a press conference about one month before nationwide elections for the presidency and the House of Representatives, which happened in October 2017.

A central finding of the evaluation was the wide heterogeneity in treatment effects on school quality. Because randomization was carried out within pairs of schools matched on pre-treatment characteristics, it is possible to recover an unbiased (if noisy) effect of treatment on test scores for each school pair. Those treatment effects are plotted in Figure 1, demonstrating the degree of

variation in treatment effects experienced at the local level:

[Figure 1 about here.]

2.3 The political context

The program 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 was 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 was 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 gave the policy a strong role in their campaign.

A few factors made it difficult to predict *ex ante* how voters would react to this school policy. On the one hand, the program brought more resources into communities and improved average test scores, which parents might have 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 outsourcing a fundamental responsibility

to private entities with limited accountability, many foreign and some for-profit.

3 Design

3.1 Timeline

This paper's main results leverage the randomization of the PSL program and use administrative voting data from the October 2017 general election as outcomes. The paper also presents the effects of the PSL program on households' political attitudes, using data collected at the end of the first school year in which the policy was in place: May/June 2017. Finally, it presents results from two information experiments carried out in the weeks leading up to the election. Figure 2 diagrams which data collection efforts happened at what time, and how they informed each other.

[Figure 2 about here.]

3.2 Data

There were 2,080 polling booths in Liberia in the 2017 election, with 637 votes cast in the median booth. Electoral data at the voting booth level, as well as booth GPS coordinates, were obtained from the National Elections Commission (NEC) of Liberia (<http://www.necliberia.org>).

Studying the effect of PSL on voting outcomes necessitates deciding how to map the unit of treatment assignment (schools) onto the unit of outcome measurement (polling booths). One possible scheme would assign all booths the treatment status of their nearest school. However, this raises the risk of contamination: consider a booth which is infinitesimally closer to a control school than a treatment school. The treatment school may be expected to exert at least as much influence on voters' choices as the control school, yet it would be classified as control.

Instead, this paper considers a certain radius around each booth, defining the booth's "fraction treated" as the number of treated schools divided by the number of total RCT schools within the

radius.⁵ This in turn leaves the question of how wide to draw the radius. Under the assumption that the strength of a school’s impact on voters’ choices decreases as some function of distance from the school, this implies a trade-off between the sample size and effect size; booths beyond the radius will be discarded.

The main specifications in this paper use a radius of 10km, for three reasons. 1) This radius is wide enough to embrace at least one polling booth for all 185 schools in the RCT (though in some cases only one). 2) A clear majority (58%) of booths lie within 10km of an RCT school, and the density of polling places drops off precipitously after this threshold (see Figure 3). 3) 97 % of students in the RCT live within 10km of their school. Figure shows a map of Liberia depicting the 185 schools from the RCT and the 1200 booths within 10km of at least one of them.

[Figure 3 about here.]

[Figure 4 about here.]

Appendix B will provide a more detailed discussion of the tradeoffs involved and a comparison of findings under other potential treatment assignment schemes, which are qualitatively similar to the main specification.

3.3 Empirical Specification

The main results in this paper come from the following specification:

$$Y_{isp} = \alpha_p + \beta \text{FracTreated}_i + \gamma X_i \varepsilon_{isp} \tag{1}$$

Y_{isp} represents electoral outcomes for polling booth i near school s in pair p . α_p are matched-pair fixed effects (stratification dummies based on the pair p corresponding to the booth’s nearest school s). FracTreated_i is defined as the number of treated schools with 10km of booth i over the total number of RCT schools within the same radius. X_i are booth-level controls, consisting of the

⁵Following Romero et al. (2019), this paper considers the original ITT treatment assignment of the schools, despite the fact that there was some noncompliance in which schools actually came under private administration.

election outcome at that booth from the previous election in 2011. Standard errors are clustered at the level of the electoral district ($J = 63$).

3.4 Balance

Table 1 checks that this specification provides a set of polling places which are balanced on election outcomes from 2011 (the last nationwide election before the treatment), regressing treatment status on 2011 election measures as “outcomes.” While not rising to the level of statistical significance at even the 10% level here, the point estimate on the difference in ruling party presidential vote share is somewhat large, and in some alternate specifications this difference shows up as significant. Therefore, all specifications include controls for 2011 election outcomes.

[Table 1 about here.]

Appendix B provides a more detailed discussion of the tradeoffs involved and a comparison of findings under other potential treatment assignment schemes, which are qualitatively similar to the main specification.

4 Results

4.1 Main electoral results

The paper’s main outcome of interest is how the school reform affected electoral outcomes in the October 2017 general election. Table 2 displays them.

[Table 2 about here.]

The school policy reduced average vote share for the presidential candidate from the incumbent Unity Party, in both the first round and the runoff election held a month later. However, it appears to have had no effect on the electoral outcomes for federal legislative candidates, the only other politicians on the ballot. This is consistent with a picture of an electorate well enough

informed to know that the policy was the product of the executive branch, and the legislative branch's ability to affect it was minimal.

4.2 Heterogeneity in election outcomes by policy impact

Because randomization happened within matched pairs, each pair can be considered a mini-experiment, and local *learning* treatment effects can be defined at the level of the pair as the difference between average test scores at the treatment and control school. The following table interacts the treatment variable with a dummy for whether the polling booth's nearest school is part of a treatment pair in which the treatment effect on test scores was above the median. That is, the coefficient on this interaction term represents the additive effect on electoral outcomes of treatment in places where test scores improved a lot. Table 3 tests whether treatment assignment had a differential effect on electoral outcomes in pairs of different local learning treatment effects.

[Table 3 about here.]

The treatment's effect on electoral outcomes was a function of how well the program worked at boosting school quality. Negative electoral effects were driven by places where the program caused small increases, or reductions, in test scores. Where the program caused big test score increases, treatment more than counteracted the negative average effect.

Again, the policy only affected voters' choices in the presidential race. Legislative candidates had no influence on the policy, but they might have been expected to attempt to claim credit in their districts – especially incumbents (Cruz & Schneider, 2017; Guiteras & Mobarak, 2015). Instead, voters' choices in the legislative race were largely unaffected.

Figure 5 depicts a similar analysis non-parametrically, plotting mean electoral treatment effects (and 95% bootstrapped confidence intervals) along the distribution of local learning treatment effects.

[Figure 5 about here.]

4.3 Effects on household attitudes

A policy as high-profile as PSL offers a lot of information about a government, even to voters who are not directly affected because their kids attend the pilot schools. In order to shed light on whether electoral results are driven by those directly affected, this section presents the effects of the policy on household members of students from the treatment and control school. These attitudes were measured in May/June 2017, about five months before the election. (The RCT measures of the program's popularity which were presented to candidates as part of the candidate information experiment in Section 5 came from this survey.)

[Table 4 about here.]

Table 4 shows that treated households became more satisfied with their children's education,⁶ more impressed with the government's performance on schools, and more likely to say schools were their top priority for government spending. Although the PSL treatment consisted in outsourcing school management to private school operators, nearly all households in both treated and control schools still saw their children's schools as government schools. Treated households were more likely to feel sanguine about Liberia's future. However, they were no more likely to be satisfied with the performance of the president (whose administration created the policy), or the legislator representing them in Congress. Finally, they were no more likely to report planning to vote for the ruling Unity Party (UP) which had created the policy (although very few people were willing to divulge voting intentions before the election).

The fact that households directly affected by the policy were no more likely to report differential satisfaction with the current administration, or intentions to vote for its continuation, suggests that the aggregate electoral results were not driven by the households of students in the schools themselves.

⁶This particular result from this was previous reported in (Romero et al., 2019).

4.4 Effects on teachers' political participation

In many parts of the world, public sector teaching jobs function as patronage, with the expectation that those in them will help turn out people to vote for the politicians who provided the job. (Larreguy et al., 2017; Pierskalla & Sacks, 2019) If PSL succeeded in professionalizing the teaching force, it might have caused teachers to engage less in political activities. On the other hand, given reports in the press that the national teachers' union opposed PSL, it might have been the case that the program galvanized opposition and caused more teachers to organize and participate in the political process. Indeed, Romero et al. (2019) reported that teachers in treated schools were significantly more likely to be dismissed.

A May/June 2017 teacher survey, a few months before the election, asked teachers about some of their political attitudes and voting intentions. The effects of treatment on these attitudes is summarized in Table 5.

[Table 5 about here.]

During the election season, not all teachers were willing to share political opinions, but this does not appear to have differed by school treatment status (column 1). These smaller sample sizes make estimates on teacher attitudes a bit imprecise, but treatment appears to have reduced both satisfaction with the incumbent government and intentions to vote for its party in the next election (columns 2 and 4). Columns 3 and 5 interact treatment with a dummy for the teacher's union membership. Treatment made unionized teachers *much* less likely to express satisfaction with the incumbent government or to want to vote for its candidate.

on the union teachers who (endogenously) remained in treated schools appears to have caused a dip in support for the incumbent president's government

A follow-up survey of teachers at PSL treatment and control schools in June-July 2019 asked teachers about their political activities during the 2017 election, including whether they had staffed registration booths and/or polling stations, encouraged participation in general, and campaigned for a particular party or candidate. Table 6 shows the impact of the policy on teachers' reported political activities.

[Table 6 about here.]

In the absence of treatment, sizable minorities of teachers were politically involved – 40% reported participating in at least one of the political activities asked about. However, treatment appears to have disrupted traditional patronage activities: teachers were much less likely to report being politically involved in the election.

5 Information experiments

The administrative voting results provide evidence that education policy was an important election issue for many voters. But accountability also requires that politicians have enough awareness of voters’ preferences to respond to them. Politicians may not know how much voters care about service delivery, or they may not know which policies are likely to work to improve service quality. Politicians may not know how much voters care about service delivery, or they may not know how to hold the bureaucracy accountable to actually improve service quality. The next two experiments were designed to test whether politicians were constrained by a lack of information about either policy effectiveness or voters’ preferences – and whether voters were constrained by a lack of information on politicians’ policy positions.⁷

5.1 Candidate experiment

The candidate information experiment was conducted through a phone survey in which survey enumerators attempted to call all 992 candidates running for seats in the House of Representatives.⁸ The sample consists of the 681 candidates reached (69%). These candidates received fewer votes on average than non-participating candidates, but they were not uniformly inconsequential: the sample includes 112 “veteran” candidates who ran for Congress in the previous election

⁷The experiments described here were pre-registered along with pre-analysis plans at <https://www.socialsciregistry.org/trials/1501> (policy impact on political attitudes) and <https://www.socialsciregistry.org/trials/2506> (information experiments for candidates and households).

⁸A randomized controlled trial registry entry and the pre-analysis plan for both the candidate and household experiments are available at: <https://www.socialsciregistry.org/trials/2506>.

of 2011; 25 of the 73 incumbent House incumbents (of whom 22 were standing for reelection); and 32 of the 73 eventual winners. 13% of the sample ended up as the winner or the runner-up in their district. Figure 6 plots the density of vote shares for candidates who did and did not participate in the survey.

[Figure 6 about here.]

The treatment consisted of random provision of the RCT evidence of the program’s treatment effects on a) learning outcomes and/or b) the the program’s popularity among affected households.⁹ Conditional on being reached by phone a candidate was randomized into one of four treatment arms:

1. “Control:” basic description of the school policy, and one sentence about what supporters and opponents of the policy said about it;
2. “Impact information:” control language plus a brief summary of the findings of the independent evaluation, including positive effects on test scores and teacher attendance, as well as student and teacher dismissals;
3. “Popularity information:” control language plus a brief summary of effects on political attitudes (those seen in this paper in Table 4);
4. “Both:” control condition, impact information, and popularity information.

The exact text of these information interventions is in Appendix A.

5.1.1 Candidate balance and summary statistics

The balance check on candidates’ characteristics and pre-treatment survey responses is in Table 7. For simplicity of comparison, it pools all information treatments into a single “any information” treatment.

⁹Learning outcome RCT evidence took the form of a very concise synthesis of the main results from Romero et al. (2019). Popularity RCT evidence took the form of the results on household attitudes presented in Section ??, Table 4 in this paper.

[Table 7 about here.]

It is rare to survey such a large body of politicians; even the descriptive statistics are illuminating, and consistent with politicians who are reasonably well-informed about their constituents. 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), perhaps evincing ideological flexibility. Although over 80% of candidates themselves agreed that the government should work with private providers of education, and that the PSL program had increased learning, only 57% of candidates said they thought voters supported the program on average – qualitatively consistent with the divergent electoral outcomes measured from the administrative voting data. Candidates estimates' of their voters' support of the program correlated with the average treatment effect of the program within their constituency. 72% said their voters (correctly) credited the executive branch with responsibility for the program (compared to only 12% who thought their voters credited the legislative branch with the program).

The main outcome of interest was the candidate's position on the PSL policy. In order to elicit "public" policy positions that went beyond cheap talk, survey enumerators offered to communicate the candidate's PSL policy position of choice to voters as part of a sensitization campaign (see next section). Candidates were asked 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;
- The PSL program should be immediately discontinued;
- No position.

52% of candidates interviewed said the program should be expanded; 30% said it should be tested further first; and 7% said it should be discontinued. The other 11% gave no position.

5.1.2 Candidate results

Table 8 shows the results of the experiment; however, at least as important as the treatment estimates are the summary means at the bottom of the table. An overwhelming majority of candidates already believed the policy was successful at improving test scores, and a large majority also thought their constituents supported the policy (which, while contradicted by the eventual election outcome, was consistent with the household survey data provided in the information experiment). Hardly any candidate opposed the idea that governments should work with private companies to provide education. The independent evaluation of the PSL program had become public a few weeks earlier, and was reported in local press; candidates may have read about the results already.

[Table 8 about here.]

Given that candidates' priors were reasonably in tune with the information provided, it is perhaps unsurprising that the policy information failed to shift them much; neither the learning impact information nor the popularity information had a significant effect on the average candidates' answers to key survey questions. The first column shows the main outcome – whether candidates would go on record as supporting the policy. Candidates who received information about the school policy were, on average, no more likely to tell voters they supported expanding it. Nor did the information change their opinions on the (non-public) survey outcomes in the other columns: whether the program should be funded with taxpayer money, whether the government should work with the private sector in education, or whether foreign NGOs have too much control in Liberia. Perhaps most strikingly, neither the impact information nor the popularity information had any effect on candidates' likelihood of thinking that voters support the program. (If anything, popularity information seems to have decreased candidates' belief that the program boosted test scores.)¹⁰

¹⁰This nonresult stands somewhat in contrast to Hjort et al. (2019)'s recent experimental results showing that Brazilian mayors demand rigorous policy evidence. However, in the Brazilian context, the mayors who were the experimental subjects of the study do in fact have direct jurisdiction and autonomy over the policies about which evidence was given. The route to impact on education policy for federal legislators in Liberia is more roundabout.

5.2 Household experiment

A separate information experiment sought to measure whether households' approval ratings and voting intentions responded to information about candidates' positions on the PSL program. Conducted in October 2017, this survey re-contacted by phone a subset of the households who had been previously interviewed (in person) as part of the 1-year evaluation of the school policy (those interviewed in Table 4). 489 households participated, of the 833 for whom at least one unique phone number was available (59%). While this subset of households likely differs from the May 2017 sample (households with phones are likely to be wealthier), Table 9 shows within-sample balance in terms of the randomization into the information treatment.

All participants in this household follow-up survey – information treatment and control – received a brief summary of the PSL program's impacts on test scores. Treatment consisted in informing the household about presidential and legislative *candidates'* PSL policy positions. Legislative candidates' positions came from the candidate survey; only the positions of candidates in the household's legislative district were provided. The median (interquartile range) household in the information experiment received information about the positions of 4 (2,6) legislative candidates, representing 30 (24,46)% of the legislative candidates on the ballot in their district. Presidential candidates' positions came from a presidential debate held a few weeks before the election, which included a question about PSL. Only three candidates participated, none of whom would go on to make the runoff election. One (Cummings) had a broadly supportive position about PSL; the other two (Cooper and Jones) were more skeptical.¹¹ The text of the treatment conditions can be found in Appendix A.

5.2.1 Household balance and summary statistics

Table 9 shows balance of the October 2017 household sample on pre-information-treatment characteristics.

¹¹This 26 September debate seems to have been the only presidential debate in which moderators asked about the PSL program. The three mentioned candidates were the only ones who attended the debate; as mentioned, none of them were front-runners. The eventual vote shares received by Cummings, Jones, and Cooper in the general election were 7.2%, 0.8%, and 0.7% respectively.

[Table 9 about here.]

The assignment of the Candidate Information treatment is mostly balanced in terms of the original PSL treatment. The one measurable difference between the Candidate Information treatment and the control group is that households randomly assigned to receive candidate information also happened to be in districts for which policy position information was available for slightly fewer legislative candidates (35% vs 32% of the total candidates on the ballot).

As with the candidate experiment, summary statistics here are highly informative. Households in the 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. Nearly everyone (correctly) does *not* credit the legislature with creating the policy. 44% gave an accurate answer about who was responsible for it (either the executive branch, private school companies, or foreign NGOs). Most of the rest said "don't know." 15% said they had heard at least one candidate mention PSL.

5.2.2 Household results

Table 10 presents the effects of information about candidates' positions on approval ratings and voting intentions for presidential candidates.

[Table 10 about here.]

These results show that the information had little impact on voters' views regarding these presidential candidates. Columns 1 and 2 correspond to Alexander Cummings, whose debate statement about PSL was broadly favorable. Columns 3 and 4 correspond to MacDella Cooper, whose statement on PSL was skeptical. Columns 5 and 6 correspond to Mills Jones, whose position was stridently opposed to PSL. Candidate information reduced approval rates for can-

didates who opposed PSL in the presidential debate, and the effect sizes are sizable compared to the mean approval. This might be considered a manipulation check, as this is a context where experimenter demand effects are operative. Passing this manipulation check, then, shows that households did indeed listen to the survey and take it seriously. That gives the non-effect of information on voting intentions more weight. Voters appear not to have been swayed in their voting intentions by the information provided.¹²

Table 11 shows the effects of information on candidates' policy positions on electoral outcomes for *legislative* candidates.

[Table 11 about here.]

Candidate information has no discernible effect on voting patterns for representatives. Households receiving the information were no more likely to plan to vote for representatives who supported (or opposed) the expansion of the policy, nor for candidates about whom they had received no information. They were no more likely to vote for representatives from the ruling party or for incumbents. They were also no more or less likely to express a voting preference at all.

While somewhat imprecise, these null results are consistent with the administrative voting data results in Section 4. Those results showed that the PSL program affected voters' choices for presidential candidates, but not for legislative candidates. This household survey shows that households *knew* legislators were not responsible for the program. They may have seen information on legislative candidates' PSL policy positions as immaterial. The large electoral results from before also imply that people were willing to invest enough research to form their own opinions without the help of information provided by researchers. This highlights a paradox of voter information interventions: people already have incentives to learn about what they want to know, so if they don't know it already it's because they don't care about it. This makes it difficult to shock their information set.

¹²To be fair, baseline voting intentions were already very low for these candidates, leaving little room for downward movement anyway. Sample nonresponse to these questions also likely attenuates any potential result here; only about 2/3 of respondents chose to express a voting preference for either presidential or legislative candidates.

6 Conclusion

Understanding whether and how voters create electoral incentives for politicians to improve public services is a fundamental question of political economy. Its urgency may be even greater in Liberia, a post-conflict country and one of the poorest countries in the world, where democracy is young and literacy is around 50%. This paper shows that Liberian voters are sophisticated consumers of public services; both politicians and voters are well-informed about the effects of a controversial school reform. Electoral rewards for the policy were contingent on its effectiveness: voters rewarded the responsible politician where the policy worked well, and punished him where it worked poorly. Overall, however, this worked out to a negative average electoral effect for the responsible politician, likely at least in part due to the opposition of organized groups who perceived the reform as a threat to the status quo.

This highlights the risks of policy experimentation (Majumdar & Mukand, 2004). Policy-makers who seek to improve public service delivery often face the unenviable task of shaking up entrenched systems with motivated supporters. They often lack credible evidence on how a given intervention is likely to work in their context (Pritchett & Sandefur, 2014). Meanwhile, they can be confident that any change will provoke opposition from those who benefit under the status quo (Acemoglu, 2010). In these circumstances, clientelism or vote-buying may provide a less risky path to electoral victory (Wantchekon, 2003; Cruz et al., 2018). It is just possible, however, that better research can reduce policymakers' uncertainty on forecasts of policy effectiveness and thereby improve the odds that voter gratitude for a given policy's improved services outweighs opposition from those who stand to lose from change. Further research, of course, is necessary to help answer the question of whether further research can ease the transition from patronage- to public service-based politics.

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Figures

Figure 1: Heterogeneity in treatment effect on test scores

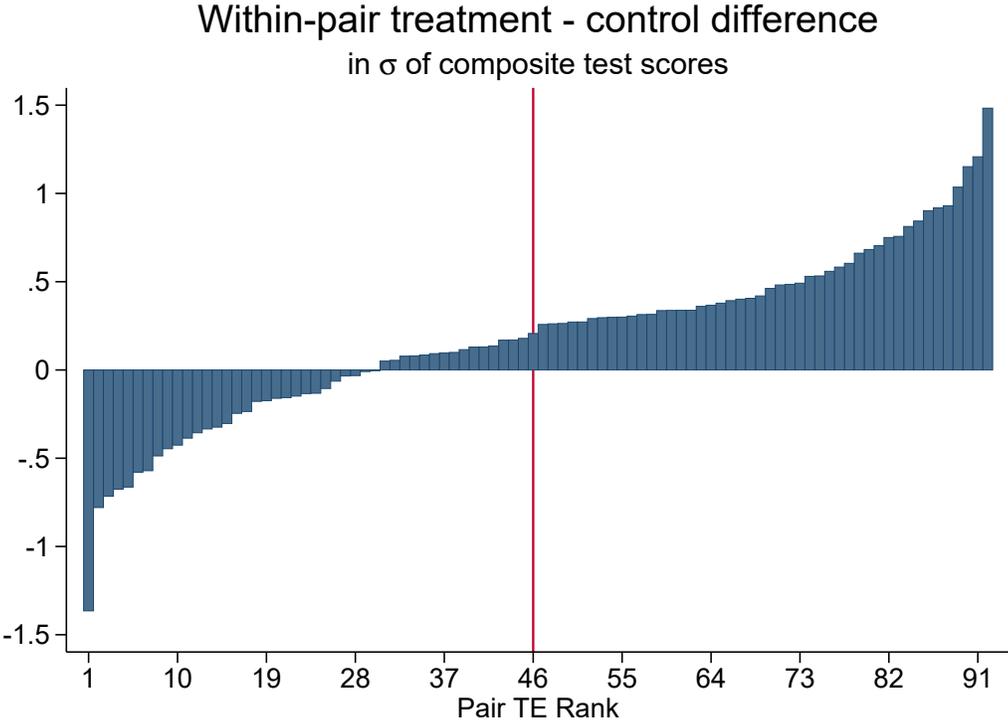
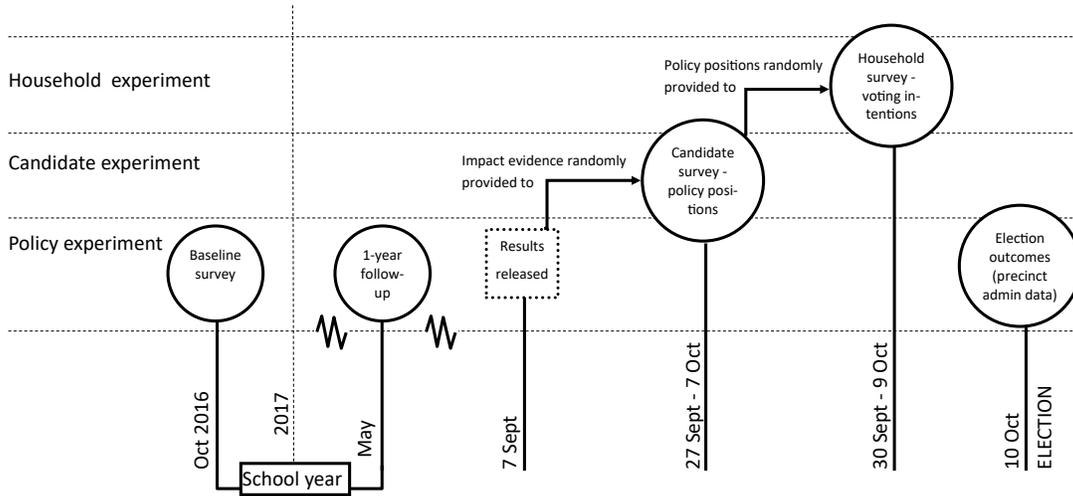
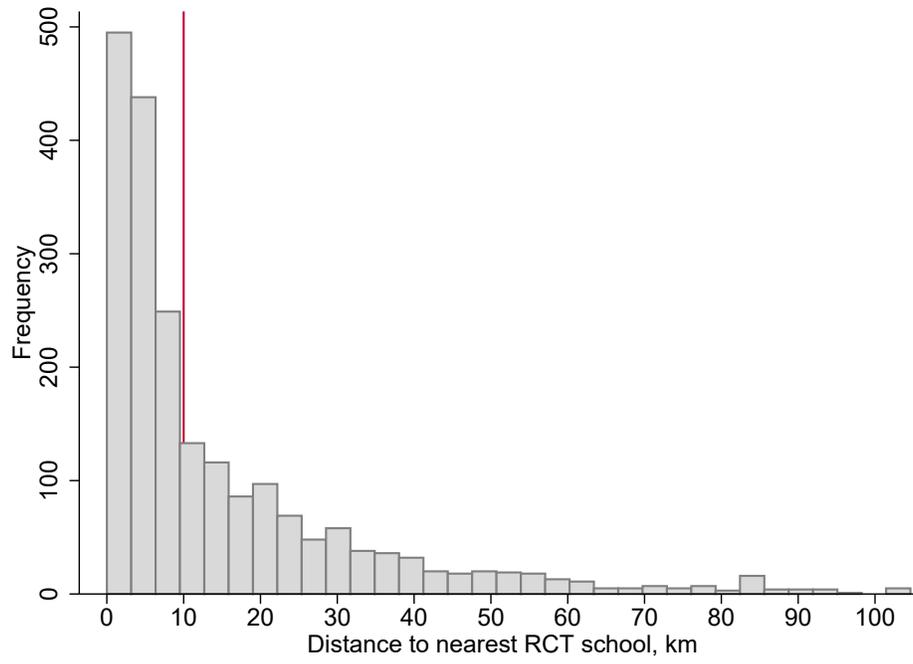


Figure 2: Timeline



Timeline not to scale. Circles indicate data collection efforts, and the height of the circle indicates the relevant experiment.

Figure 3: Histogram of polling booths by distance to nearest RCT school



Universe of 2080 polling booths from 2017 general election.

Figure 4: Map of treatment and control schools, and polling booths within 10km

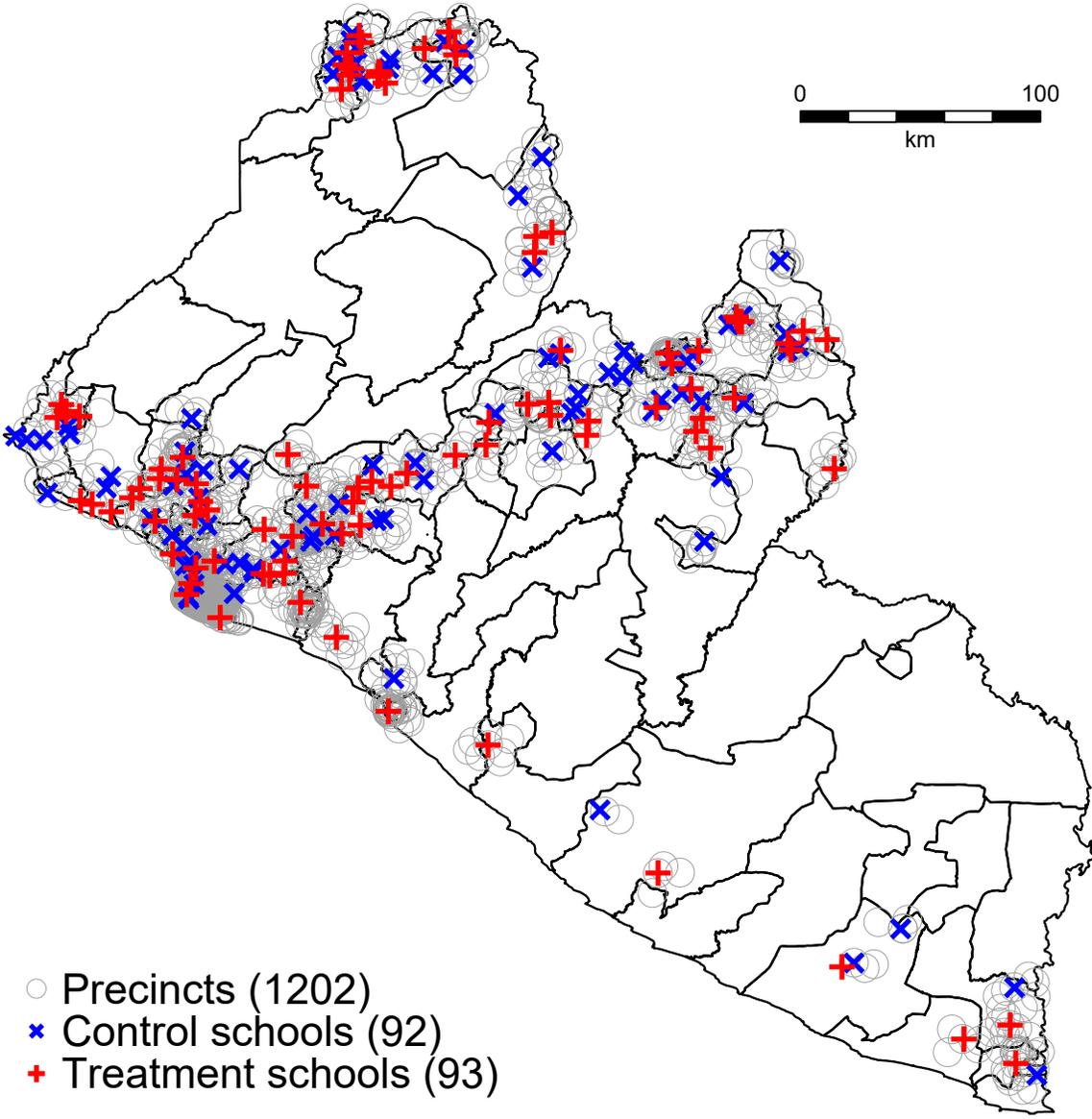
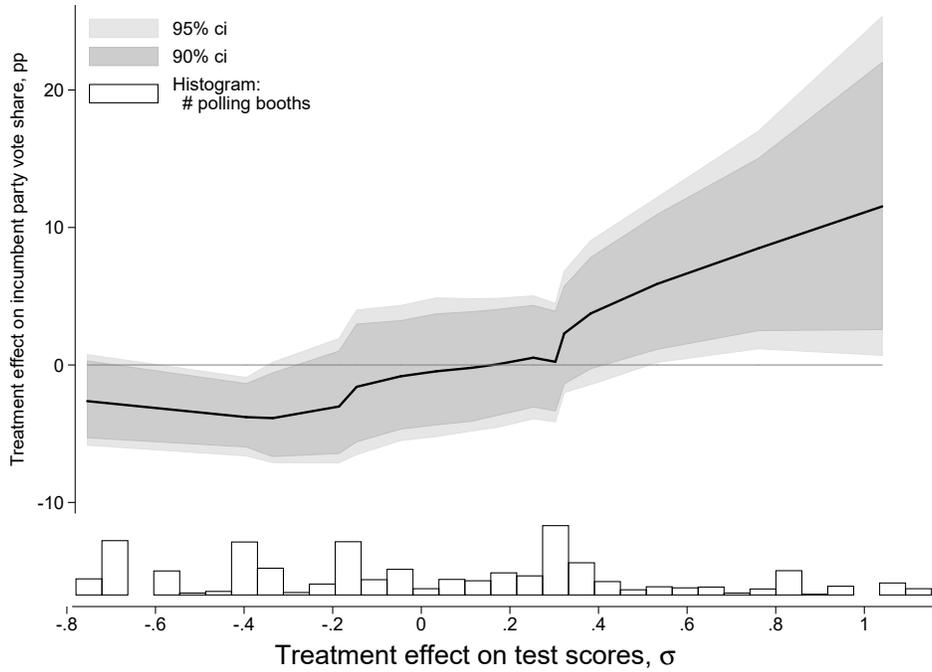
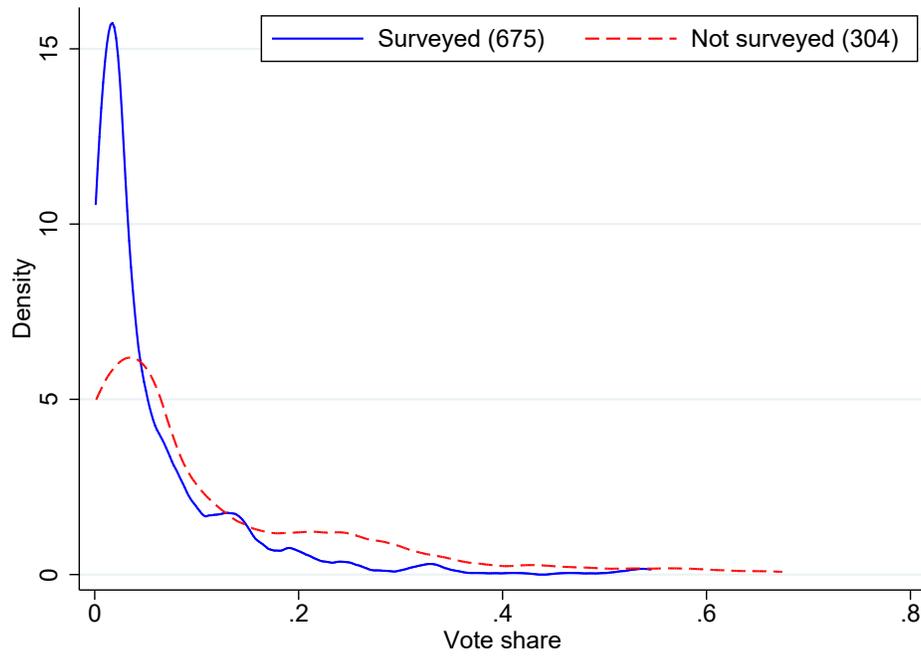


Figure 5: Effect of PSL on responsible party's presidential vote share, by local learning treatment effect



This figure plots the lowess-smoothed coefficients of the fraction of schools treated on UP presidential candidate's vote share (controlling for its 2011 pre-treatment value) in bins corresponding to 20iles of matched-pair-specific treatment effects on test scores. Constructing the bootstrapped confidence intervals consisted in calculating the same estimates from 1000 resamples of the original data, keeping the 2.5th, 5th, 95th, and 97.5th percentile of the distribution of the estimates from this resampling procedure. The histogram shows data winsorized at 1%/99%.

Figure 6: Distribution of vote shares for surveyed and unsurveyed candidates



Note: Figure excludes about 20 surveyed candidates from two districts for which election results were still pending as of the time of writing

Tables

Table 1: Balance: pre-treatment outcomes (2011 election)

	Registered voters / pop.	Votes cast / pop.	Turnout (rep.)	Share invalid (rep.)	Ruling party (pres. 1st rd.)	Ruling party (pres. runoff)	Ruling party (rep.)	Share for 2017 incumbent
Fraction RCT schools treated	0.010 (0.011)	0.006 (0.008)	-0.008 (0.008)	-0.004 (0.005)	-0.023 (0.017)	-0.005 (0.010)	0.017 (0.028)	-0.019 (0.051)
N	1200	1200	1200	1200	1200	1200	1200	1200
Mean (control)	0.124	0.091	0.724	0.073	0.429	0.842	0.179	0.337

Standard errors clustered by electoral district. School matched-pair fixed effects included for booth's nearest school. Regressions include precincts from the 2011 election located within 10km of any RCT school, with treatment of the precinct defined as fraction of RCT schools assigned to the PSL treatment. 134 precincts which are within 10km of a RCT school were newly created between 2011 and 2017 and hence have missing values for 2011 election variables. Missing values have been replaced with zero, and indicator variables for whether values are missing have been included in all regressions.

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

Table 2: Average school policy effects on vote share

	Ruling party: president (1st round)		Ruling party: president (runoff)		Ruling party: legislative		Incumbent: legislative	
Fraction RCT schools treated	-0.032** (0.015)	-0.029** (0.012)	-0.033** (0.016)	-0.028* (0.015)	0.003 (0.021)	0.008 (0.020)	0.021 (0.033)	0.015 (0.032)
N	1200	1200	1200	1200	1200	1200	1200	1200
Mean (control)	0.293	0.293	0.382	0.382	0.128	0.128	0.201	0.201
Controls		✓		✓		✓		✓

Standard errors clustered by electoral district. Matched-pair fixed effects corresponding to precinct's nearest school included. Regressions include precincts from the 2017 election located within 10km of any RCT school, with treatment of the precinct defined as fraction of RCT schools assigned to the PSL treatment. Missing values have been replaced with zero, and indicator variables for whether values are missing have been included in all regressions. Controls: number of registered voters in 2011; total votes cast in the first-round presidential election in 2011; 2011 UP presidential 1st-round vote share.

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

Table 3: Effects on 2017 vote share, interacted with learning treatment effect

	Ruling party: president (1st round)		Ruling party: president (runoff)		Ruling party: legislative		Incumbent: legislative	
Fraction RCT schools treated	-0.056*** (0.018)	-0.047*** (0.014)	-0.059*** (0.017)	-0.048*** (0.016)	-0.015 (0.022)	-0.008 (0.021)	0.048 (0.036)	0.045 (0.035)
Fraction RCT schools treated \times TE > p50	0.077*** (0.021)	0.060*** (0.020)	0.086*** (0.025)	0.068** (0.028)	0.060 (0.052)	0.053 (0.050)	-0.089 (0.073)	-0.098 (0.073)
N	1200	1200	1200	1200	1200	1200	1200	1200
Mean (control)	0.293	0.293	0.382	0.382	0.128	0.128	0.201	0.201
Controls		✓		✓		✓		✓

Standard errors clustered by electoral district. Nearest school matched-pair fixed effects included. Regressions include precincts from the 2017 election located within 10km of any RCT school, with treatment of the precinct defined as fraction of RCT schools assigned to the PSL treatment. Missing values have been replaced with zero, and indicator variables for whether values are missing have been included in all regressions. Controls: polling-place level electoral outcomes from previous general election in 2011 (number of registered voters; total votes cast; presidential vote share for ruling Unity Party candidate).

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

Table 4: Effect of PSL on household and teacher survey outcomes (May 2017)

	Treatment	Control	Difference	Difference (F.E.)
Household midline survey (N = 1271)				
Considers child's school a gov school	0.917 (0.276)	0.937 (0.244)	-0.020 (0.020)	-0.011 (0.015)
Satisfied w/ child's edu	0.743 (0.437)	0.689 (0.463)	0.054* (0.028)	0.069*** (0.020)
Gov performance on edu is good	0.566 (0.496)	0.549 (0.498)	0.017 (0.032)	0.034* (0.020)
Schools top priority for gov spending	0.811 (0.392)	0.739 (0.440)	0.072*** (0.026)	0.068*** (0.020)
Liberia is moving forward	0.577 (0.494)	0.507 (0.500)	0.070** (0.032)	0.071*** (0.019)
Satisfied with president	0.651 (0.477)	0.632 (0.483)	0.019 (0.032)	0.020 (0.023)
Satisfied with legislator	0.545 (0.498)	0.535 (0.499)	0.009 (0.036)	0.004 (0.024)
Plans to vote for UP	0.178 (0.383)	0.198 (0.399)	-0.020 (0.028)	-0.021 (0.017)

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 5: Effect of PSL on teachers' political participation

	Willing to state voting intention	Satisfied w/ incumbent pres.		Intends to vote for ruling party pres. candidate	
Treatment	0.022 (0.035)	-0.043 (0.031)	0.018 (0.039)	-0.077* (0.044)	-0.007 (0.053)
Union member			0.188*** (0.056)		0.156** (0.071)
Treatment × Union member			-0.208*** (0.080)		-0.250*** (0.095)
N	948	782	782	473	473
Mean (control)	0.524	0.771	0.771	0.642	0.642

Standard errors clustered by school. School matched-pair fixed effects included. Outcomes from a May/June 2017 survey. Columns 3 and 5 interact treatment with an (endogenous) dummy for union membership.

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

Table 6: Effect of PSL on teachers' political participation

	Support ruling party	Registration booths	Polling booths	Encourage participation	Campaign for a party or candidate	Involved in any
Treatment	-0.067 (0.043)	-0.035*** (0.012)	-0.054** (0.023)	-0.022 (0.020)	-0.044** (0.022)	-0.103*** (0.034)
N	385	847	847	847	847	847
Mean (control)	0.732	0.059	0.174	0.149	0.152	0.396

Standard errors clustered by school. School matched-pair fixed effects included. Outcome in column 1 is whether the teacher said in a May/June 2017 survey that they intended to vote for presidential candidate from the outgoing administration in the upcoming Oct 2017 presidential election; sample limited to those willing to state a voting intention for any candidate. Other outcomes come from a June/July 2019 follow-up survey asking teachers to recall their political activities from the election.

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

Table 7: Balance – Candidate experiment

Variable	Control	Any info	Difference
Incumbent	0.024 (0.154)	0.041 (0.198)	0.017 (0.017)
Eventual winner or runner-up	0.128 (0.335)	0.094 (0.292)	-0.034 (0.027)
UP (incumbent pres. party)	0.036 (0.187)	0.050 (0.219)	0.014 (0.019)
CDC (main opposition)	0.072 (0.260)	0.060 (0.238)	-0.012 (0.022)
Number of attempts necessary to interview	2.554 (1.949)	2.417 (1.813)	-0.137 (0.165)
Has own children in primary	0.601 (0.491)	0.596 (0.491)	-0.005 (0.044)
Candidate has Univ. degree	0.717 (0.452)	0.726 (0.446)	0.009 (0.040)
It's good for gov't to work w/ private companies to provide edu.	0.931 (0.254)	0.905 (0.293)	-0.026 (0.026)
Heard of PSL	0.596 (0.492)	0.642 (0.480)	0.046 (0.043)
Heard of any PSL operator	0.892 (0.312)	0.882 (0.323)	-0.010 (0.029)
'Strongly' or 'Somewhat' approve of teachers' union	0.981 (0.136)	0.978 (0.146)	-0.003 (0.013)
Believes voters hold exec. branch responsible for education	0.842 (0.365)	0.858 (0.350)	0.015 (0.032)
Believes voters hold exec. branch responsible for PSL	0.722 (0.450)	0.721 (0.449)	-0.001 (0.044)
Believes more than 'a few' voters have heard of PSL	0.193 (0.396)	0.175 (0.380)	-0.018 (0.034)
Observations	166	515	681

Candidates were randomized to receive information about PSL's popularity, its effectiveness, or both. For simplicity, this table compares the group who received no information with the pooled group of those who received any information, but comparisons among all interactions are available upon request.

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

Table 8: Policy information's effect on average candidate survey outcomes

	Expand PSL	Fund PSL w/ taxes	Gov should work w/ pvt. edu.	Too much foreign control	PSL \Rightarrow \uparrow learning	Voters support PSL
Popularity info	0.075 (0.056)	0.008 (0.052)	0.013 (0.035)	-0.038 (0.055)	-0.079** (0.035)	-0.077 (0.054)
Impact info	0.050 (0.055)	0.034 (0.050)	0.013 (0.034)	-0.048 (0.053)	-0.012 (0.029)	0.003 (0.052)
Popularity info \times Impact info	-0.063 (0.077)	-0.014 (0.071)	-0.014 (0.047)	0.065 (0.075)	0.079* (0.046)	0.086 (0.074)
N	681	681	681	681	681	681
DV Mean	0.476	0.650	0.883	0.578	0.921	0.614
Controls	N	N	N	N	N	N
District FE	N	N	N	N	N	N

Robust standard errors in parentheses. 'Expand PSL' means the candidate asked us to tell their voters they support expanding the school policy. Other columns are candidate survey outcomes.

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

Table 9: Balance – Household experiment (N = 494)

Variable	Control	Candidate Info	Difference	N
In PSL treatment group	0.543 (0.499)	0.514 (0.501)	-0.028 (0.045)	494
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
Correctly identifies responsible party for PSL	0.441 (0.498)	0.417 (0.494)	-0.024 (0.045)	494
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
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
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
Any candidate has talked about PSL	0.146 (0.354)	0.160 (0.367)	0.014 (0.034)	444
Fraction of district's candidates who provided info to candidate survey	0.354 (0.165)	0.317 (0.148)	-0.037** (0.015)	467

Notes

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

Table 10: Candidate information's effect on households' approval and voting intentions of presidential candidates

	Pro-PSL candidate (Cummings)		Anti-PSL candidate 1 (Cooper)		Anti-PSL candidate 2 (Jones)	
	Approve	Vote	Approve	Vote	Approve	Vote
Candidate info	0.056 (0.037)	0.030 (0.027)	-0.056* (0.029)	0.004 (0.004)	-0.108*** (0.034)	-0.011 (0.013)
N	494	494	494	494	494	494
DV Mean	0.309	0.147	0.167	0.000	0.276	0.043
District FE	N	N	N	N	N	N

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. Treatment consists of informing households of candidates' positions regarding PSL.

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

Table 11: 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 Any Rep.
Candidate info	0.001 (0.024)	0.041 (0.033)	0.026 (0.023)	0.003 (0.026)	-0.053 (0.045)
N	494	494	494	494	494
DV Mean	0.138	0.677	0.147	0.273	0.632
Controls	N	N	N	N	N
FE	N	N	N	N	N

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.

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

Appendix

A Information experiments - detail

A.1 Text of candidate information treatments

CONTROL CONDITION:

In the Partnership Schools program, 93 government primary schools became Partnership Schools, managed by one of eight private and NGO school providers. [Sentence describing which providers operated in the candidate's county] 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 from foreign donors: 50 US per student. Supporters of Partnership Schools believe that private management can bring innovation and improvement to Liberia's schools. Opponents of Partnership Schools believe that the resources would be better spent within the public system, without private contractors.

IMPACT INFORMATION CONDITION: [Control condition language, plus:]

The Ministry of Education commissioned an independent scientific evaluation of Partnership Schools using state-of-the-art methodology. The study was carried out by academics at institutions based in the United States: The Center for Global Development and the University of California.

The evaluation showed how the outcomes for students and teachers were different in Partnership Schools. The children in Partnership Schools learned 60% more math and English than children in the traditional public schools. That means that children in a Partnership School learned more in 6 months than children in a traditional public school learned in a whole school year. The evaluation also found that teachers in Partnership Schools were twice as likely to attend school.¹³ The evaluation also identified some problems: in some schools run by Bridge

¹³This was an inadvertent error. Teachers were in fact 50% more likely to attend, not twice as likely.

International Academies, some students were kicked out and had to transfer to different schools, and over half of the teachers were removed. The program is also expensive: the partnership schools cost at least twice as much to run as government schools, and in some cases much more.

POPULARITY INFORMATION CONDITION: [Control condition language, plus:]

The Ministry of Education commissioned an independent scientific evaluation of Partnership Schools using state-of-the-art methodology. The study was carried out by academics at institutions based in the United States: The Center for Global Development and the University of California.

The researchers interviewed voters whose children went to Partnership Schools and traditional government schools, as well as teachers in these schools.

They found that voters whose children went to Partnership Schools were: 10% MORE satisfied with their children's education, 7% MORE likely to say the government's performance on education was good, 11% MORE likely to say education is their top priority for government, and 14% MORE likely to say Liberia is moving forward .

Teachers in Partnership Schools were: 21% LESS likely to be satisfied with the teachers' union (NTAL), and 7% MORE likely to say Liberia is moving forward.

The fourth condition contained the control language, the impact information, and the popularity information.

A.2 Text of household information treatment

The control condition consisted of 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 candidate's 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 who had participated in the candidate survey, who had asked survey enumerators 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]

B Robustness: alternate specifications

Coming soon.