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# The politics of policy reform: Experimental evidence from Liberia

Wayne Aaron Sandholtz \*

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## Abstract

Public service reform often entails broad benefits for society and concentrated costs for interest groups. Do the electoral benefits outweigh the costs for politicians who implement reform? This paper examines the electoral effects of a randomized Liberian school reform which increased student learning but antagonized teachers. The policy reduced ruling party vote share by 3 percentage points (10%). It also reduced teachers' job satisfaction by  $0.18\sigma$  and political involvement by  $0.22\sigma$ . I use the evaluation's pairwise randomization to show that the effect on vote share was positively correlated with student learning, and negatively correlated with teacher political disengagement.

**Keywords:** Electoral returns; Policy feedback; Public service delivery; Policy experimentation; Education; Political economy; Elections; Randomized controlled trial; Liberia; Information

**JEL Codes:** O10, C93, D72, P16, H41, I25

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# 1 Introduction

*. . . There is nothing more difficult and dangerous, or more doubtful of success, than an attempt to introduce a new order of things in any state. For the innovator has for enemies all those who derived advantages from the old order of things, whilst those who expect to be benefited by the new institutions will be but lukewarm defenders.*

— Niccolò Machiavelli, *The Prince*

The persistence of poor public service quality in developing democracies presents a puzzle. Public services such as education benefit society broadly by creating positive externalities and growth (Romer, 1986; Hanushek & Woessmann, 2008; Martinez-Bravo, 2017). In theory, citizens can use elections to hold governments accountable for socially beneficial policies (Besley, 2006). But in many low- and middle-income democracies, public service provision remains poor, both in the judgment of citizens and in comparison to developed countries (World Bank, 2004, 2018).

One possible explanation is that electoral majorities do not prioritize service reform. For example, while voters in Africa consistently rank service provision as a high priority, they have many other priorities as well (Afrobarometer, 2018). Groups favoring any given reform may not constitute a majority; in the case of primary education, the direct beneficiaries are too young to vote (Poterba, 1997; Boas, Hidalgo, & Toral, 2021). Voters may prefer more direct redistribution (Bursztyn, 2016; Weitz-Shapiro, 2012). Or they may be more swayed by appeals to identity (S. Mukand & Rodrik, 2018; Posner, 2005).

Another possible explanation is that interest groups create a status-quo bias in policy-making (Fernandez & Rodrik, 1991; S. W. Mukand & Rodrik, 2005). Reform often delivers diffuse benefits with concentrated costs (Olson, 1965). In the case of public services, these costs tend to fall on frontline bureaucrats, who may organize to oppose reform (Finan, Olken, & Pande, 2017; Flavin & Hartney, 2015; Bruns, Macdonald, & Schneider, 2019). Education accounts for a sixth of government spending in developing countries, and the vast majority of this funding goes on salaries for teachers (World Bank, 2015). Their opposition can be politically costly, especially if they are part of a patronage machine (Chaudhury, Hammer, Kremer, Muralidharan, & Rogers, 2006; Robinson & Verdier, 2013). Bureaucrat opposition has derailed many seemingly effective reforms (Banerjee, Duflo, & Glennerster, 2008; Bold, Kimenyi, Mwabu, Ng, & Sandefur, 2018; Dhaliwal & Hanna, 2017).

Identifying the binding constraint can inform policy experimentation and create an appetite for policy evidence (Majumdar & Mukand, 2004; Hjort, Moreira, Rao, & Santini, 2021). Politicians have little incentive to improve a service voters don't prioritize. Entrenched bureaucrat resistance takes other reforms off the table. But in the space of policy changes to which voters and bureaucrats are responsive, better

implementation could turn a vote-losing policy into a winner.

Empirical evidence on this question is hindered by various challenges. Exogenous shocks to public services are rare, making it difficult to identify even their causal effects on electoral outcomes. A growing quasi-experimental literature finds that the reduced-form electoral effects of public goods vary significantly in magnitude and sign.<sup>1</sup> This may be partly due to at least two sources of variance. First, different voters experience widely diverging implementations of a policy, but most studies can estimate only average treatment effects. Second, few studies are equipped to examine voters' reactions alongside other important political actors such as interest groups.

This paper aims to disentangle voters' and bureaucrats' electoral responses to a public service reform. It examines the electoral effects of a Liberian public-private school partnership, which improved student learning but provoked fierce opposition from teacher unions (Romero, Sandefur, and Sandholtz (2020) – see Section 2 for details). Although the reform carried the government's imprimatur, it was implemented by eight different subcontractors across 93 public primary schools. This created wide variation in both the policy's effect on learning and its effects on teachers. Treated schools were selected randomly from within pairs (or strata) of eligible schools matched on pre-reform characteristics (Bruhn & McKenzie, 2009). This means each treated school had a valid counterfactual school. To measure the policy's effect on electoral outcomes, I link polling booths in the administrative data to nearby treatment and control schools. To measure the policy's effect on teachers' political activity, I surveyed teachers at treatment and control schools.

On average, the reform hurt the ruling party at the polls and among teachers. It caused a 3-percentage-point (10%) reduction in vote share for the ruling party's presidential candidate. This effect appears only at the presidential level, suggesting voters attribute credit or blame to the appropriate level of political responsibility. It also lowered teachers' job satisfaction by  $0.18\sigma$ , and reduced their voting intentions for the ruling party by 8percentage points (12%). It reduced by  $0.22\sigma$  an index of their involvement in political behavior; teacher staffing of polling stations and campaigning for candidates both fell by nearly a third at treated schools.

The pairwise randomization permits the comparison of voting treatment effects with learning and teacher treatment effects. The difference in outcomes between treatment and control schools in each pair gives an unbiased estimate of the treatment effect, and these locally-measured treatment effects vary across the 92 school pairs in the experiment. By interacting the treatment variable with the size of the treatment

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<sup>1</sup>See e.g. Assunção and Estevan (2019); de Kadt and Lieberman (2017); Dias and Ferraz (2019); Goyal (2019); Habyarimana, Opalo, and Schipper (2021); Harding (2015); Litschig and Morrison (2013); Marx (2018); Samuels (2002); Zimmermann (2020).

effect on learning (or teachers' political activity), I can study how the policy affected vote share differently in places where it caused big vs. small learning gains (or much vs. little teacher alienation). This pair-level variation in local treatment effects is not itself randomly assigned, so the interactions should not be interpreted causally. But this heterogeneity is vital for illuminating mechanisms (see e.g. Balboni, Bandiera, Burgess, Ghatak, and Heil (2022)).

The reform increased the incumbent party's vote share in proportion to how much it increased learning (see Figure 5). Where test scores improved more than about  $0.5\sigma$  (around the 80th percentile in the pair-level distribution of learning treatment effects), the policy caused significant gains for the incumbent party's candidate. Where test scores worsened by more than about  $0.3\sigma$  (around the 20th percentile), it caused significant losses. This suggests that voters were attuned to changes in school quality, and rewarded or punished the responsible party commensurately.

The reform decreased the incumbent party's vote share in proportion to how much it disengaged teachers politically. Negative effects on vote share were concentrated in places where the reform also reduced teachers' participation in staffing polling or registration booths and campaigning. This suggests that teachers play an important role in mobilizing voters, and that alienating them has electoral costs.

What might a counterfactual, vote-winning version of the reform have looked like? Alienating teachers was neither necessary nor sufficient for improving schools: the pair-level correlation between treatment effects on student learning and teacher political involvement was  $-0.07$ . Section 5 uses the estimated linear coefficients to predict what the policy's average electoral effect would have been under counterfactual scenarios raising the floor of treatment effects on learning and teacher involvement. These suggest that the policy could have won votes on net if the floor on learning effects had been the 27th percentile, and the floor on teacher political involvement effects had been the 30th percentile. This suggests that better evidence on how to make reforms effective and palatable might expand the set of electorally feasible public service improvements.

Two information experiments involving candidates and voters confirm the picture of a well-informed electorate. In order to test whether information frictions inhibit accountability for public services, I conducted two linked information experiments with legislative candidates and voters. I varied the provision of policy evidence to candidates, then varied the provision of these candidates' policy positions to voters. Both groups had high baseline knowledge of the policy, leaving little room for shifting priors. See Section 6 for details.

This paper's contribution is fourfold. First, I provide rare experimental evidence on the electoral effects

of a reform to public service delivery. While cash transfers have been shown to be politically effective, evidence on electoral returns to broad-based growth-promoting public goods and services is more sparse (see [Golden and Min \(2013\)](#) for a review). Second, I contrast the electoral benefit of helping voters with the cost of antagonizing frontline bureaucrats. The ideas that voters reward good services ([Key, 1966](#); [Ferejohn, 1986](#)) and that interest groups oppose reform ([Olson, 1965](#)) are not new, but this study is the first to decompose these two effects empirically in the same context. Third, I offer empirical evidence on the theoretical tendency toward status quo bias ([Fernandez & Rodrik, 1991](#)). Service delivery reforms that alienate public sector unions often cause an immediate loss of union backing, and may only generate support from voters after the effects of the reform have a chance to be seen ([Chaudhury et al., 2006](#)). Finally, the paper contributes to the literature on voter information, by showing that even poorly-educated voters inform themselves about electorally consequential policies, making it hard to shock voters' priors. This helps reconcile the tension in the literature between the large effects of naturally-occurring voter information (e.g. [Ferraz and Finan \(2008\)](#)) and the non-effects of researcher-provided information (e.g. [Dunning et al. \(2019\)](#)).<sup>2</sup>

The rest of this article is structured as follows: Section 2 provides context about Liberia and the policy; Section B outlines a conceptual framework; Section 3 outlines the empirical strategy; Section 4 presents the main results; Section 5 discusses policy counterfactuals; Section 6 describes the information experiments; and Section 7 concludes.

## 2 Context

Liberia's public school system has struggled to provide a level of education deemed basic by international institutions – or its own citizens. The civil wars of 1999-2003 and the Ebola epidemic of 2014 diminished the capacity of the Ministry of Education to manage a national public school system. An effort to clean thousands of ghost teachers from Ministry payrolls was cut short ([Rosenberg, 2016](#)), and while systematic data is scarce, teacher absenteeism appears common ([Mulkeen, 2009](#)). In 2014, net enrollment of primary students was among the world's lowest, at 38% ([World Bank, 2014b](#)). The literacy rate for youth (age 15-24) was 55% in 2015 ([World Bank, 2014a](#)). In 2013, not a single one of the 25,000 high school graduates sitting the state university entrance exam received a passing score. This prompted a withering indictment from President Ellen Johnson Sirleaf: "The students' failure did not come from the university, but rather from

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<sup>2</sup>Some examples exist of experimenter-provided information affecting electoral outcomes ([De Figueiredo, Hidalgo, & Kasahara, 2011](#); [Cox, Eyzaguirre, Gallego, & García, 2020](#)).

the schools that prepared them,” she declared in a statement, adding, “It tells me that the educational system is a mess” (Reuters, 2013). In the following months, Sirleaf reaffirmed that education was “priority number one” for the government and the government’s Information Minister called the education crisis a “national emergency” (Dawn, 2013; Sayon, 2013).

## 2.1 The policy

In response to these challenges, the Liberian Ministry of Education created the “Partnership Schools for Liberia” (PSL) program in 2016 (Ministry of Education, Republic of Liberia, 2016). The program contracted out the management of 93 government primary schools to one of eight private school operators in a public-private partnership. External donors, in partnership with the government, provided these operators with funding at the level of USD\$50 per-pupil. This extra grant represented a doubling of the baseline level of per-pupil expenditure.<sup>3</sup> The operators were given responsibility for (though not ownership of) the resources the government normally uses to provide education – schools, classrooms, materials, and teachers – as well as for the daily management of the schools, with the understanding that the government could hold them accountable for results. The operators were very heterogeneous: some were for-profit chains backed by high-profile Western investors (Edwards, 2017). Other operators were non-profit NGOs, some based in Liberia and some based elsewhere.

The government commissioned an independent randomized controlled trial evaluation of the policy. The 185 public primary schools which were declared eligible for the program were not a representative sample of public schools in the country – they had better facilities, internet access, and road access than the average school in the country. But they constituted a sizable subset of the school system: 3.4% of the country’s public primary schools, and 8.6% of public primary and early-childhood education students, across 13 of Liberia’s 15 counties. These eligible schools were split into pairs matched on administrative data, and treatment was assigned randomly within matched pairs.

Treated schools had much in common with regular public schools but differed in important ways. In the PSL scheme, treated schools were required to be non-selective – i.e., operators were not allowed to choose which students to enroll, and were told to enroll students on a first-come first-serve basis. However, PSL operators were permitted to limit class sizes (at 65 students per classroom), unlike public schools. PSL school buildings remained under the ownership of the government. PSL teachers, unlike those in PPP

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<sup>3</sup>In the first year of the program, the extra funds came from outside philanthropic donors, but the Ministry’s stated goal was that the government eventually cover these costs and scale up insights from the PSL program, raising spending in all Liberian public schools to USD \$100 (Werner, 2017a, 2017b)

schemes in many other countries, were required to be civil servants on the government payroll, limiting operators' ability to hold teachers accountable for learning outcomes. Still, operators were allowed to test teachers themselves and request that the Ministry reassign underperformers elsewhere - a prerogative which at least one operator exercised significantly. Public primary schools, while ostensibly free, generally charged ancillary "PTA" and other fees; early childhood education (ECE) in public schools at the time carried an official cost of about \$40 USD per year. PSL schools were forbidden from charging any fees whatsoever, including for ECE. PSL's private providers were required to agree to school inspections and to provide the necessary data to evaluate performance – ostensibly for accountability purposes – although no formal mechanisms to hold operators accountable were created in the policy's first year of operation. Operators were required to deliver the Liberian national curriculum, but allowed to supplement it with remedial programs, longer school days, and non-academic activities.

This reform provides an attractive context for measuring electoral responses to public service provision for a number of reasons. First, attribution was relatively direct: education policy is set centrally by the executive branch, and Liberia has no elected local politicians, so the incumbent president's party could clearly and credibly claim credit.<sup>4</sup> Second, the program's funding came from external donors and was earmarked specifically for the program; so any measurement of the electoral effect of this policy is unconfounded by voters' preferences over possible counterfactual use of funds. Third, the matched-pair randomization permits the measurement of variation in policy effectiveness. Fourth, the policy was unusually salient for an education reform: it was implemented in an election year and garnered significant press attention. The RCT results were first publicly reported in a press conference about one month before the October 2017 nationwide elections for the presidency and the House of Representatives.<sup>5</sup> Finally, the involvement of the private sector in the provision of public services adds another layer of interest to the electoral effects of this policy. To supporters, public-private partnerships (PPPs) offer the promise of improved service delivery where state capacity is weak, and various governments have now implemented PPPs in education (Crawford & Hares, 2021).

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<sup>4</sup>See e.g. Cruz and Schneider (2017); Guiteras and Mobarak (2015) for examples of local politicians successfully claiming credit for policies they didn't create.

<sup>5</sup>The one-year midline report made public in September 2017 was Romero, Sandefur, and Sandholtz (2017); later published as Romero et al. (2020).

## 2.2 The policy's effects on learning

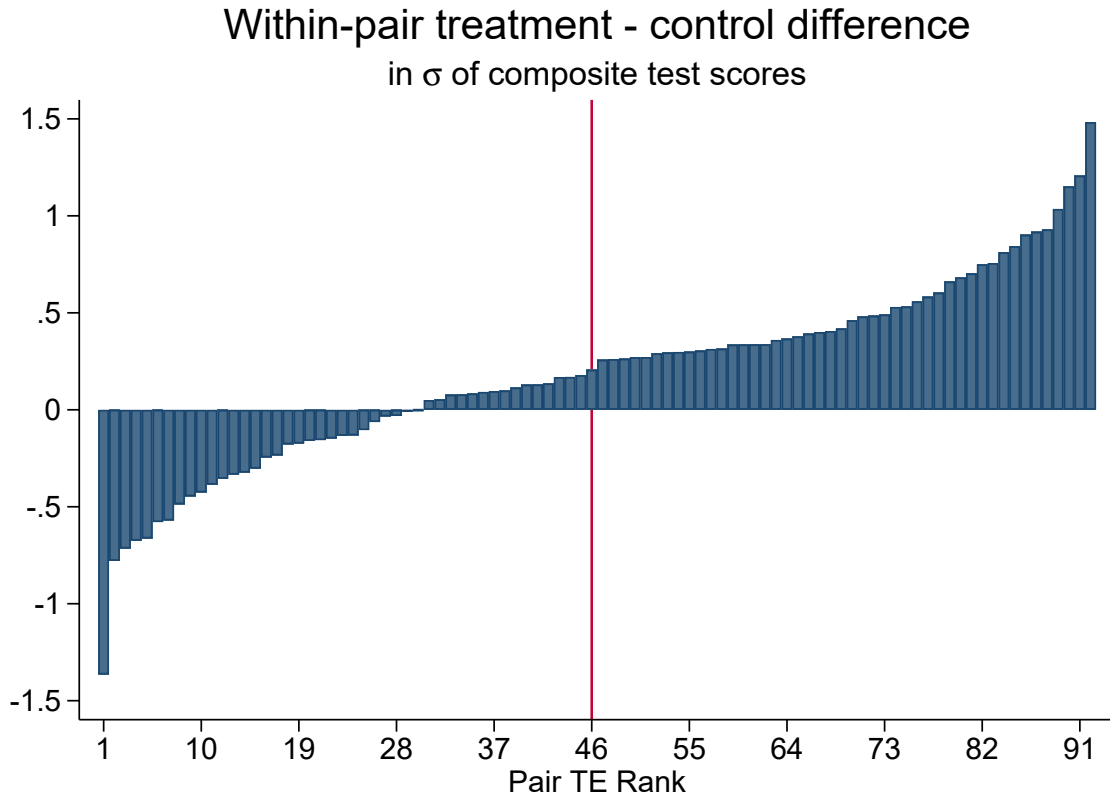
Romero et al. (2020) provides a comprehensive picture of the program's effect on education outcomes after one year (roughly the time of the 2017 general election); here I present some important highlights. The program increased test scores by  $0.18\sigma$ , corresponding to around a 60% increase over what students in control schools learned in a year. Teacher attendance increased by 50%, teacher time-on-task increased by 43%, and satisfaction of both students and parents increased by about 10%.<sup>6</sup>

The matched-pair randomization permits an unbiased estimate of treatment effects for each school pair. While the average effect on learning was positive, not all schools experienced learning gains. The operators' management practices varied a great deal. For example, although the contract governing the policy permitted operators to enforce limits on class sizes, in practice only one operator chose to do so. As a result of this enforcement, hundreds of students from the biggest classes were forced to find a new school. The same operator successfully requested reassignment from the Ministry of 74% of its schools' teachers, creating large teacher turnover and negative externalities for the broader system. The pair-level treatment effects on test scores are plotted in Figure 1, demonstrating the degree of variation in treatment effects experienced school-by-school:

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<sup>6</sup>Romero and Sandefur (2021) show the longer-term effect of the program on educational outcomes after three years. I focus mostly on the one-year outcomes here, as these were the outcomes that had been realized by the time of the 2017 general election.

Figure 1: Heterogeneity in treatment effect on test scores



### 2.3 The political context

Liberia is a young democracy. After decades of civil war, the country held broadly free and fair general elections in 2005, 2011, and 2017. These were arguably the first fully democratic elections in the country's history.<sup>7</sup> For presidential races, a majority is required to win; in the case that no candidate receives an outright majority in the first round, the top two vote-getters advance to a runoff. Senate and House elections are first-past-the-post and require only a plurality to win.

Liberian politics do not feature strong parties with consistent policy aims. Party platforms "tend not to differ to any great extent and actual divergences in policy are not prominent" (Pailey & Harris, 2017). Ethno-regional loyalties play a role, but their role is not as dominant as in some other African democracies. Party strongholds often shift from election to election. In all three elections from 2005-2017, there was no

<sup>7</sup>From most of the period from independence to 1980, Liberia was a one-party state ruled by the True Whig Party, and only elites were permitted to vote (Pailey & Harris, 2020).

“clean sweep” of presidential and legislative races for any party in any of Liberia’s 15 counties. The two largest parties are the Unity Party (UP), the ruling party after 2005 and 2011 elections; and the Congress for Democratic Change (later the Coalition for Democratic Change) (CDC), whose presidential candidate made it to the runoff election in 2005 and 2011, and won in 2017. Between the two of them, these two “main” parties in 2005 held only 23 of the 73 seats in the House of Representatives. This number rose to 35 in 2011 and 41 in 2017, but neither party is close to a majority. Nor is party loyalty particularly strong among politicians. In 2017, 31 incumbent representatives ran for different parties than those they had represented in 2011. The number of seats held by independents rose from 9 in 2011 to 13 in 2017 (Pailey & Harris, 2020).

Patronage is important at all levels of Liberian politics. The presidency of Ellen Johnson Sirleaf has been described as “carefully based upon Liberian patronage networks . . . in an intricate an omnipotent network of big men and followers” (Bøås & Utas, 2014). At the legislative level, the three-time House election winner Zoe Pennue of Grand Gedeh County has found success through personal patronage, “from paying school fees and hospital bills to donating cars” (Pailey & Harris, 2020).

### 2.3.1 Teachers and politics in Liberia

Teachers in Liberia have considerable political heft. The Ministry of Education is one of only a handful of ministries with a strong presence in all 15 counties in Liberia. Its employees – including teachers – constitute 40% of the country’s entire civil service, making them “the largest special interest group,” according to a former deputy minister of education. In many rural parts of the country, teachers might be among the only members of the community who have an education and a wage-earning job. This gives them leadership and economic influence. People look to teachers for advice. In urban areas, they can “take to the streets and disturb the city” by going on strike. In these and other ways, teachers can influence election and policy choices.<sup>8</sup> Politicians are cognizant of the value of teacher union support: during the campaign, the UP’s presidential candidate Joseph Boakai donated 200 bags of cement to the country’s largest teacher union (NTAL) for the construction of its new headquarters (Brooks, 2017).

Teachers’ political clout is important both for elections and for policy adoption. 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, Montiel Olea, & Querubin, 2017; Pierskalla & Sacks, 2019). Teacher unions, and their links to political machines, have successfully

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<sup>8</sup>Source: author’s conversation with a former deputy minister of education

derailed education reform in other contexts ([Finger, 2018](#); [Ross Schneider, 2021](#); [Bruns et al., 2019](#)).

### **2.3.2 The 2017 election**

The 2017 election decided the successor to president Ellen Johnson Sirleaf, who had won the previous elections in 2005 and 2011 on the ticket of the Unity Party (UP). Constitutional limits prevented Sirleaf from seeking a third six-year term, and in 2017 the UP's presidential candidate was Joseph Boakai, Sirleaf's vice president, making him a "pseudo-incumbent" ([Pailey & Harris, 2017](#)). A dearth of opinion polling made it difficult to identify a front-runner prior to the election, but 19 of Liberia's 30 senators endorsed Boakai, including 13 from parties other than Boakai's ([Front Page Africa, 2017](#)). His principal rival was former footballer George Weah of the CDC, who had also run unsuccessfully as either a presidential or vice-presidential candidate in the two prior elections. All 73 seats of the House of Representatives were also up for election (also to six-year terms), and the Unity Party held a plurality (24) of seats prior to the election.

Voter registration took place between 1 February and 14 March 2017. Citizens are free to decide where to register to vote, but are only allowed to vote at the polling station where they registered. The election took place 10 October 2017, with 75.2% of registered voters casting a ballot. The presidential runoff was held on 26 December 2017, after some losing candidates' allegations of irregularities in the first round were adjudicated ([Pailey & Harris, 2020](#))

## **2.4 The school reform in the 2017 election**

The ruling Unity Party (UP) took various actions to claim credit for the partnership school program. Most importantly, of course, was the incumbent UP administration's creation of the policy. Reforming the education sector was a priority for the administration, and both the president and the vice president took action to associate themselves with the program. Sirleaf met with potential funders in New York City to promote the program alongside Education Minister George Werner ([Executive Mansion, 2017](#)). Boakai spoke at the graduation ceremony of a government teacher training institute which trained many of the partnership schools' teachers ([Front Page Africa, 2016c](#)). A report by a teacher union umbrella group that opposed the policy claimed that the Ministry of Education carried out "numerous public relations activities (press releases, radio talk shows, jingles and etc.) and engagement with potential donors [endeavoring] to mobilize moral and financial support for the program." ([Coalition for Transparency and Accountability in Education, 2017](#))

The program provoked controversy in Liberia and beyond. A United Nations special Rapporteur on the right to education condemned an early iteration of the proposed policy: “Public schools and their teachers, and the very concept of education as a public good, are under attack,” he said (United Nations, 2016). The policy also attracted critical responses from some scholars of education (Hook, 2017; Klees, 2018). In Liberia, local press reported on students who were forced out of their schools (Senah, 2016; Mukpo, 2017c). It was also reported that because the school day had been lengthened in some operators’ schools, children who used to go home at lunchtime were now going hungry (Mukpo, 2017b).

#### **2.4.1 The response of the teachers’ union**

The country’s primary teachers’ union, the National Teachers Association of Liberia (NTAL), stridently and vocally opposed the policy. The Ministry of Education, perhaps anticipating opposition from teacher unions, had designed the policy with civil servant teachers in mind. Private operators were not allowed to hire non-government contract teachers (as is sometimes permitted in similar education public-private partnerships around the world). In principle, according to an op-ed by the Minister of Education, PSL teachers were allowed to be members of teacher unions (Werner, 2017a). But teachers in at least one of the schools under private management said the operator threatened to fire them for speaking with union officials, and the NTAL’s president claimed that the government fired senior leaders of the teachers’ union for speaking out against the program (Mukpo, 2017a; Mulbah, 2017).

The NTAL mobilized significant political action in protest. It called for the abandonment of the policy, and spearheaded a strike calling for the resignation of the Minister of Education (NTAL, 2017; Butty, 2016). Adherence to the strike was wide but not universal, and the Minister resisted the calls to resign (Ziamo, 2016; Kwanue, 2016b). But the strike escalated as students protested by blocking the main highway to the country’s international airport, demanding that the government and the teachers’ union send the teachers back to class (Brooks, 2016a; Front Page Africa, 2016b, 2016a). In at least one city, the protesters turned violent and ransacked public buildings (Kwanue, 2016a). President Sirleaf condemned the protest, supported the Minister, and ordered the dismissal of teachers linked to the protest; the strike ended after a few days (Brooks, 2016b). A few months before the election, on the occasion of Boakai’s donation of cement to the NTAL, its executives reiterated their rejection of the PSL program and urged him to “use his office to reinstate teachers that were dismissed because of their opposition” to PSL (Brooks, 2017).

The partnership schools program, and education more generally, were therefore unusually salient in the 2017 general election. A pre-election report from the Ghanaian think tank IMANI stated: “The 2017

Liberian election has education at the apex of issues with numerous promises or proposals from political parties on addressing the access to quality education” (IMANI, 2017). At least one opposition party (though not the main one) took an explicit stance against the policy in the run-up to the election (Daygbor, 2016; Nimely, 2016). Both of the main candidates, Boakai of the UP and the challenger George Weah of the Coalition for Democratic Change (CDC), professed support for the policy (Malkus, 2017). But Weah’s campaign built on frustration with the technocratic ethos embodied by the Western-educated Sirleaf. An informal campaign slogan in Liberian English – “Da book we’ll eat?” – connoted the perception that the government’s technical competence and coziness with Western donors hadn’t translated into gains for ordinary Liberians (Posthumus, 2017). Weah went on to defeat Boakai in the runoff.

### 3 Design

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. I also present results drawn from a teacher survey collected in May/June 2017 – the end of the policy’s first year of implementation but prior to the October 2017 election – and from a follow-up teacher survey carried out in June/July 2019. Section 6 presents the design and results of a series of information experiments carried out with candidates and households in the run-up to the 2017 election.

#### 3.1 Administrative electoral data

The main outcomes in this paper use administrative election data from the voting booth level.<sup>9</sup> There were 2,080 polling booths in Liberia in the 2017 election. 637 votes were 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.

The policy treatment was assigned at the school level, so I define polling booths’ treatment status according to the treatment assignment of the school(s) within a certain radius around each booth.<sup>10</sup> Some booths are close to treatment and control schools; I define each booth’s “treatment intensity” continuously

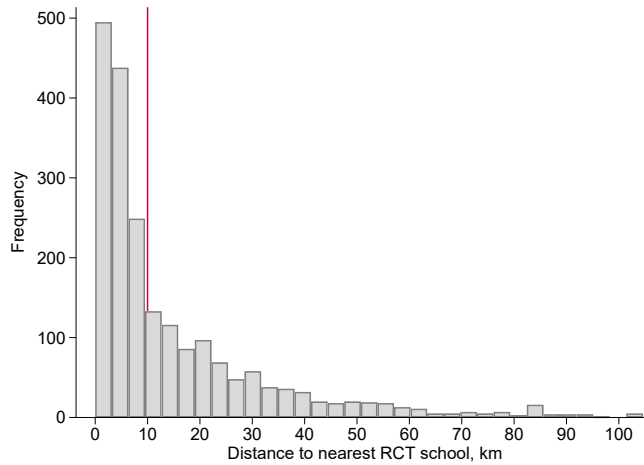
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<sup>9</sup>In the jargon of the Liberian National Election Commission, a voting site is called a “precinct.” Precincts are not defined as geographically-bounded polygons, but rather as locations where voters can register and vote – a school, for example. Each precinct consists of one or more “polling places,” with more polling places added within a precinct to accommodate the number of voters registered there. I aggregate all “polling places” up to the precinct level as there is no geographic variation within precincts. A precinct is what I call a “voting booth” or a “polling booth” in this paper.

<sup>10</sup>As in Romero et al. (2020), this paper considers the original ITT treatment assignment of the schools; a few schools assigned to treatment never actually came under private administration.

as the number of treated schools divided by the number of total treatment and control schools within the radius.<sup>11</sup> 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: a smaller radius leaves a smaller number of booths, while a larger radius includes booths which may be more weakly treated. Figure 2 displays a histogram of all 2080 polling booths in the 2017 election by their distance to the nearest treatment or control school.

Figure 2: Histogram of polling booths by distance to nearest treatment or control school



Universe of 2080 polling booths from 2017 general election.

The main specifications in this paper define treatment using a radius of 10km, for three reasons. 1) This radius is wide enough to embrace at least one polling booth for all 185 schools and 92 school pairs in the RCT.<sup>12</sup> 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 2). 3) 97% of students in the RCT live within 10km of their school. Figure 3 shows a map of Liberia depicting the 185 schools from the RCT and the 1202 booths within 10km of at least one of them.

Figure 4 is a histogram displaying the number of booths which take the different values of treatment intensity (between 0 and 1). 255 booths have a treatment intensity of 1 (they are within 10km of at least

<sup>11</sup>A possible alternative scheme might 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 the booth would be classified as control. Another potential scheme might define treatment as the number of treated schools divided by the total number of schools within the radius. But this would confound treatment status with overall school density.

<sup>12</sup>Two matched pairs have only one polling booth within 10km; because I use matched pair fixed effects in all specifications, this means the singleton booths get dropped from analysis, leaving 1200 polling booths, and 90 of the 92 matched school pairs, in the analysis sample. While all 185 RCT schools are within 10km of at least one polling booth, 178 RCT schools are some polling booth’s closest RCT school; that is, 7 RCT schools are less close to their nearest polling booth than another RCT school.

Figure 3: Map of treatment and control schools, and polling booths within 10km of them

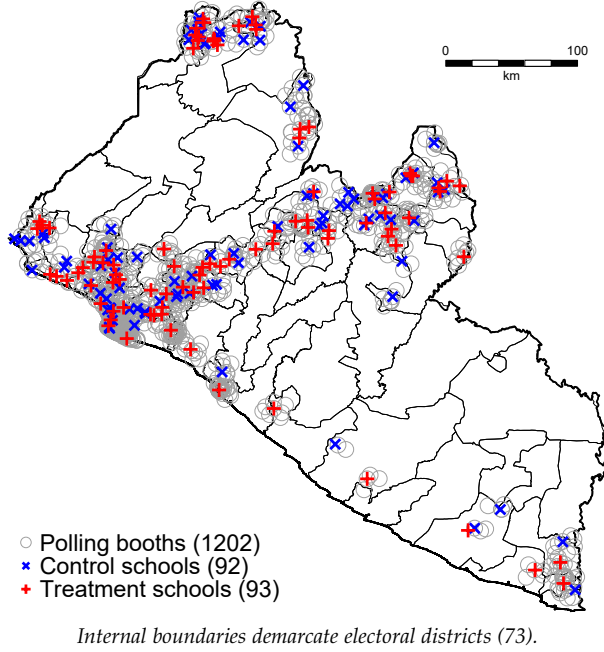
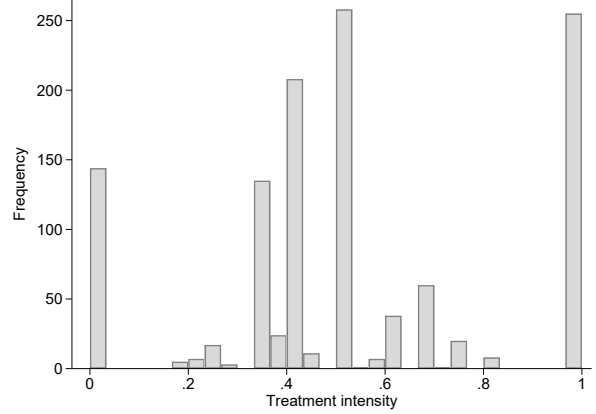


Figure 4: Histogram: treatment intensity



one treatment school and zero control schools); 144 booths have a treatment intensity of 0 (they are within 10km of at least one control school and zero treatment schools). 258 booths are within 10km of an equal number of treatment and control schools, giving them a treatment intensity of 0.5. The remaining 545 booths lie somewhere in between.

### 3.2 Empirical Specifications

The average treatment effect of the reform is estimated using the following specification:

$$Y_{isp} = \alpha_p + \beta \text{TreatIntensity}_i + \gamma X_i + \varepsilon_{isp} \quad (1)$$

$Y_{isp}$  represents electoral outcomes for polling booth  $i$  whose nearest treatment or control school is school  $s$  in pair  $p$ .  $\alpha_p$  are matched-pair fixed effects (stratification dummies).  $\text{TreatIntensity}_i$  is defined as the number of treated schools with 10km of booth  $i$  divided by the total number of treatment and control schools within the same radius.  $X_i$  are booth-level controls consisting of 2011 election outcomes: registered voters, votes cast, and first-round presidential ruling party vote share. In all specifications with electoral outcomes, standard errors are clustered at the level of the electoral district, i.e. the House of

Representatives constituency (J = 63).<sup>13</sup>

The “extensive margin” effect of the program can be measured by limiting attention to the 285 polling booths which are within 10km of exactly one treatment or control school. Limiting the sample this way does not diminish internal validity, but bear in mind that this subsample is necessarily more rural and isolated. Table A1 shows the main electoral results using this subsample. While the reduced sample size affects statistical power, the results are qualitatively similar to those of the main specification.

### 3.2.1 Heterogeneous treatment effects

To test how the effect of the policy on electoral outcomes covaries with its effect on student learning, I interact the treatment variable with an indicator for whether the treatment effect in the pair corresponding to the polling booth’s nearest school was above the median:

$$Y_{isp} = \alpha_p + \beta_1 TreatIntensity_i + \beta_2 1(TE_p > p50) + \beta_3 TreatIntensity_i \times 1(TE_p > p50) + \gamma X_i + \varepsilon_{isp} \quad (2)$$

### 3.3 Teacher survey data

All teachers in treatment and control schools were surveyed in May/June 2017; a follow-up survey was conducted with teachers at these schools in June/July 2019. As well as asking teachers about their teaching behavior, the 2017 survey asked them about their opinions of the PSL policy, their views of the government, and their voting intentions in the upcoming elections. The 2019 survey asked teachers about what political behaviors they had been involved in during the 2017 election – staffing polling booths, staffing registration booths, encouraging others to vote in general, and encouraging others to vote for a particular party or candidate (“campaigning”).<sup>14</sup>

Because teachers correspond to a given school and treatment is defined at the level of the school, the empirical specification for teacher survey outcomes is more straightforward:

$$Y_{isp} = \alpha_p + \beta Treatment_s + \varepsilon_{isp} \quad (3)$$

$Y_{isp}$  represents electoral outcomes for teacher  $i$  at school  $s$  in pair  $p$ .  $\alpha_p$  are matched-pair fixed effects (stratification dummies).  $Treatment_s$  is the school’s assigned treatment status. Standard errors are clustered

<sup>13</sup>There are 73 electoral districts in Liberia but only 63 with polling booths near treatment or control schools.

<sup>14</sup>See Romero et al. (2020) and Romero and Sandefur (2021) for more details on these teacher surveys.

at the level of the school. Because I have no teacher surveys from prior to the treatment, I cannot check balance on teacher survey data.

### 3.4 Balance

Table 1 checks balance on 2011 election outcomes (the last nationwide election before the treatment). The coefficient on treatment is not statistically significant for any of these outcomes. However, the point estimate on the difference in ruling party presidential first-round vote share is non-negligible. Therefore, subsequent tables show specifications with and without including controls for 2011 election outcomes.

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 |
|---------------------|--------------------------|-------------------|-------------------|----------------------|------------------------------|-----------------------------|---------------------|--------------------------|
| Treatment intensity | 0.010<br>(0.011)         | 0.006<br>(0.008)  | -0.008<br>(0.008) | -0.004<br>(0.005)    | -0.023<br>(0.017)            | -0.005<br>(0.010)           | 0.017<br>(0.028)    | -0.019<br>(0.051)        |
| N                   | 1200                     | 1200              | 1200              | 1200                 | 1200                         | 1200                        | 1200                | 1200                     |
| Mean                | 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. 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

## 4 Results

### 4.1 Average electoral treatment effects

Table 2 shows the average electoral effect of the school reform on electoral outcomes in the October 2017 general election. All specifications include stratification dummies; odd columns include no controls, while even columns include controls for 2011 registration, votes cast, and ruling party first-round presidential vote share.

Table 2: Average school policy effects on vote share

|                     | Ruling party:<br>president<br>(1st round) |                     | Ruling party:<br>president<br>(runoff) |                    | Ruling party:<br>legislative |                  | Incumbent:<br>legislative |                  |
|---------------------|---|---------------------|--|--------------------|------------------------------|------------------|---------------------------|------------------|
| Treatment intensity | -0.032**<br>(0.015)                       | -0.029**<br>(0.012) | -0.033**<br>(0.016)                    | -0.027*<br>(0.015) | 0.003<br>(0.021)             | 0.008<br>(0.020) | 0.021<br>(0.033)          | 0.015<br>(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. School matched-pair fixed effects included. Regressions include polling booths from the 2017 election located within 10km of any school in the RCT, with treatment defined as fraction of these 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. The row labeled displays the mean of the dependent variable for polling booths with Treatment = 0. 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

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, by about 3 percentage points.<sup>15</sup> This constituted a 10.9% reduction in vote share in the first round (off a mean of 29.3%), and an 8.6% reduction in the runoff (off a mean of 38.2%). (The vote share for the UP’s candidate Joseph Boakai in the country as a whole was 28.8% in the first round and 38.5% in the runoff.)

The policy had no statistically significant effect on vote share in legislative races, either for ruling party legislative candidates or for incumbent legislators in general.<sup>16</sup> In one sense, this is to be expected: the policy was an initiative of the executive branch, and legislators had no formal role in its design or execution. But politicians have been known to successfully claim undeserved credit elsewhere (Cruz & Schneider, 2017; Guiteras & Mobarak, 2015). This non-result is consistent with a well-informed electorate, aware of the executive branch’s responsibility for the policy and the legislative branch’s minimal involvement.

Survey evidence from households of students in treatment and control schools corroborates these findings. In two different survey waves, conducted five months and one month prior to the election, treatment households were 2.1 percentage points and 5.1 percentage points less likely to report intending to vote for the ruling party’s presidential candidate, respectively – though neither effect is statistically significant. However, the treatment did significantly increase parents’ support of the PSL program, and satisfaction with their child’s education, the government’s performance on education, their own legislator’s perfor-

<sup>15</sup>The correlation in this sample between ruling party presidential vote share in the first round and the runoff is 0.88.

<sup>16</sup>The correlation in this sample between ruling party first-round presidential vote share and ruling party legislative vote share is 0.45.

mance, and their overall view of the country’s forward trajectory. This underlines the nuance of the political response to the policy: the policy made its beneficiaries more satisfied, but that did not translate straightforwardly into electoral credit for the party in power. See Section C.4 for more detail on the policy’s effect on household political attitudes.

## 4.2 Average teacher treatment effects

The reform affected teachers as well as voters, and may have affected voters indirectly through teachers. This may explain why the policy’s average effect on incumbent vote share was negative, despite the evidence that voters value and reward improvements in school quality. If PSL professionalized the teaching force at treated schools, 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. \(2020\)](#) reported that teachers in treated schools were significantly more likely to be dismissed.

A survey conducted a few months before the election (May-June 2017) asked teachers about their political attitudes and voting intentions. In this survey, 99% of teachers reported having registered to vote, and 97% reported planning to vote in the election. The effects of treatment on these attitudes is summarized in Table 3.

Table 3: Effect of reform on teacher attitudes

|                | Union member         | Job satisfaction (Std. PCA Index) | School PPP is good | Willing to state voting intention |
|----------------|----------------------|-----------------------------------|--------------------|-----------------------------------|
| Treatment      | -0.100***<br>(0.027) | -0.178*<br>(0.095)                | 0.012<br>(0.025)   | 0.009<br>(0.038)                  |
| N              | 910                  | 680                               | 764                | 764                               |
| Mean (control) | 0.322                | 0.091                             | 0.820              | 0.633                             |

Standard errors clustered by school. School matched-pair fixed effects included. Outcomes from a May/June 2017 survey.

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

Table 3 shows that the policy had large effects on teacher attitudes and behavior. Teachers in treated schools were much less likely to report being a member of a teachers’ union. This effect is the sum of at least two possible mechanisms: dismissal of unionized teachers, and teachers’ own disassociation from

the union.<sup>17</sup> They also reported lower job satisfaction (as measured on an index aggregating teachers' satisfaction with various elements of their jobs). Because the NTAL teachers' union leadership opposed the public-private school partnership, it is natural to wonder whether treatment affected teachers' ideological beliefs about public-private partnerships in education. It did not: teachers in treated and control schools were equally likely (82%) to agree that "it is good for the government to work with private school companies to provide education." The treatment also had no effect on the likelihood that teachers were willing to talk about their voting intentions in the survey.

Table 4: Effect of reform on teacher political attitudes, by union membership

|                          | On govt payroll |          | Satisfied w/ incumbent pres. |          | Intends to vote for ruling party pres. candidate |          |
|--------------------------|-----------------|----------|------------------------------|----------|--|----------|
| Treatment                | 0.062*          | 0.136*** | -0.034                       | 0.026    | -0.077*  | -0.014   |
|                          | (0.035)         | (0.038)  | (0.032)                      | (0.040)  | (0.045)  | (0.054)  |
| Union member             |                 | 0.428*** |                              | 0.194*** |  | 0.157**  |
|                          |                 | (0.053)  |                              | (0.058)  |  | (0.072)  |
| Treatment × Union member |                 | -0.152** |                              | -0.190** |  | -0.214** |
|                          |                 | (0.068)  |                              | (0.081)  |  | (0.095)  |
| N                        | 910             | 910      | 748                          | 748      | 455  | 455      |
| Mean (control)           | 0.539           | 0.539    | 0.768                        | 0.768    | 0.633  | 0.633    |

Standard errors clustered by school. School matched-pair fixed effects included. Outcomes from a May/June 2017 survey. Even columns interact treatment with an (endogenous) dummy for union membership at the time of the survey in May/June 2017.

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

Table 4 shows that treatment reduced teachers' support for the incumbent government, especially among the unionized teachers who remained. The odd columns in this table present the average treatment effect on various outcomes, while the even columns interact treatment with a dummy for whether the teacher was a member of the union. Union membership is not randomly assigned – and Table 3 showed that treatment has an independent effect on union membership – so these interactions should be interpreted with caution. But they offer an illuminating glimpse into one possible channel through which the treatment might have provoked teacher opposition. Overall, teachers in treated schools were more likely to be on the official government payroll. However, column 2 shows that this increase was driven entirely by non-unionized teachers. Overall, teachers in treated schools were also less likely to express satisfaction with, or an intention to vote for, the incumbent party (columns 3 and 5), though these effects

<sup>17</sup>NTAL representatives claimed that at least one of the school operators threatened unionized teachers with dismissal (Mukpo, 2017a).

are imprecisely measured. Unionized teachers in control schools tended to be much more supportive of the incumbent government than non-unionized teachers. But among unionized teachers who remained in treated schools, this extra support evaporated. Again, this could indicate that treatment turned union supporters of the government into opponents, or it could indicate that the union members who most strongly supported the government were those most likely to be dismissed. Either way, the the corps of unionized teachers supporting the government fell precipitously at treated schools.

These negative effects extended to political activities as well. A follow-up survey in June-July 2019 asked teachers at treatment and control schools about their political activities during the 2017 election, including whether they had staffed registration booths and/or polling stations, encouraged participation in general, or encouraged others to support a particular party or candidate (“campaigning”). Table 5 shows the average effect of the policy on teachers’ reported political activities.

Table 5: Effect of PSL on teachers’ political participation

|                | Registration booths  | Polling booths      | Encourage participation | Campaign for a party or candidate | Involved in any      | PCA index teacher involvement |
|----------------|----------------------|---------------------|-------------------------|-----------------------------------|----------------------|-------------------------------|
| Treatment      | -0.035***<br>(0.012) | -0.054**<br>(0.023) | -0.022<br>(0.020)       | -0.044**<br>(0.022)               | -0.103***<br>(0.034) | -0.224***<br>(0.077)          |
| N              | 847                  | 847                 | 847                     | 847                               | 847                  | 847                           |
| Mean (control) | 0.059                | 0.174               | 0.149                   | 0.152                             | 0.396                | 0.085                         |

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

In the absence of treatment, sizable minorities of teachers were politically involved – 40% reported participating in at least one of these political activities. However, treatment significantly reduced the likelihood that teachers reported being politically involved on most of these dimensions. This effect aggregates both the disengagement of incumbent teachers from the political process – including from activities traditionally associated with patronage machines such as getting out the vote – and the effect of school operators hiring less politically active teachers.<sup>18</sup>

<sup>18</sup>Because of turnover in the 18 months from the end of the 2017 election to the 2019 survey, not all teachers surveyed were stationed at the same schools as they were during the election, highlighting the importance of the channel of the professionalization of the teacher force, as opposed to simply changes in the actions of a static pool of incumbent teachers.

### 4.3 Heterogeneity in election outcomes by policy effectiveness

What explains the negative electoral treatment effect shown in Table 2? Do voters reflexively oppose any change to the status quo, or is voter opposition driven by teacher alienation? Looking at heterogeneity in the policy's effectiveness can help answer these questions. If voters are indifferent to school improvements (or oppose them outright), then their electoral responses should not be positively correlated with the policy's effectiveness at boosting school quality. The basic theory of retrospective voting posits that voters reward incumbents who provide good services. If the policy creates more electoral support in places where it more effectively improves school quality, that constitutes *prima facie* evidence consistent with that theory.

It is normally difficult to measure variation in treatment effects, but the design of this experiment makes it possible. Because randomization happened within matched pairs, each pair can be considered an internally valid, if noisy, mini-experiment. For each school pair, I define a local learning treatment effect as the difference between average student test scores at the treatment and control school. Table 6 tests whether treatment had a differential effect on electoral outcomes in school pairs with different local learning treatment effects. This specification (Equation 2) interacts the treatment variable with a dummy for whether the polling booth's nearest school is part of a treatment pair whose local learning treatment effect 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 6: Effects on 2017 vote share, interacted with learning treatment effect

|   | Ruling party:<br>president<br>(1st round) |                      | Ruling party:<br>president<br>(runoff) |                      | Ruling party:<br>legislative |                   | Incumbent:<br>legislative |                   |
|---|---|----------------------|--|----------------------|------------------------------|-------------------|---------------------------|-------------------|
| Fraction RCT schools treated            | -0.056***<br>(0.018)                      | -0.047***<br>(0.014) | -0.059***<br>(0.017)                   | -0.048***<br>(0.016) | -0.015<br>(0.022)            | -0.008<br>(0.021) | 0.048<br>(0.036)          | 0.045<br>(0.035)  |
| Fraction RCT schools treated × TE > p50 | 0.077***<br>(0.021)                       | 0.060***<br>(0.020)  | 0.086***<br>(0.025)                    | 0.068**<br>(0.028)   | 0.060<br>(0.052)             | 0.053<br>(0.050)  | -0.089<br>(0.073)         | -0.098<br>(0.073) |
| Main + interaction                      | 0.021*<br>(0.012)                         | 0.013<br>(0.014)     | 0.026<br>(0.018)                       | 0.021<br>(0.023)     | 0.045<br>(0.047)             | 0.045<br>(0.045)  | -0.041<br>(0.063)         | -0.053<br>(0.063) |
| 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                                |   | ✓                    |  | ✓                    |                              | ✓                 |                           | ✓                 |

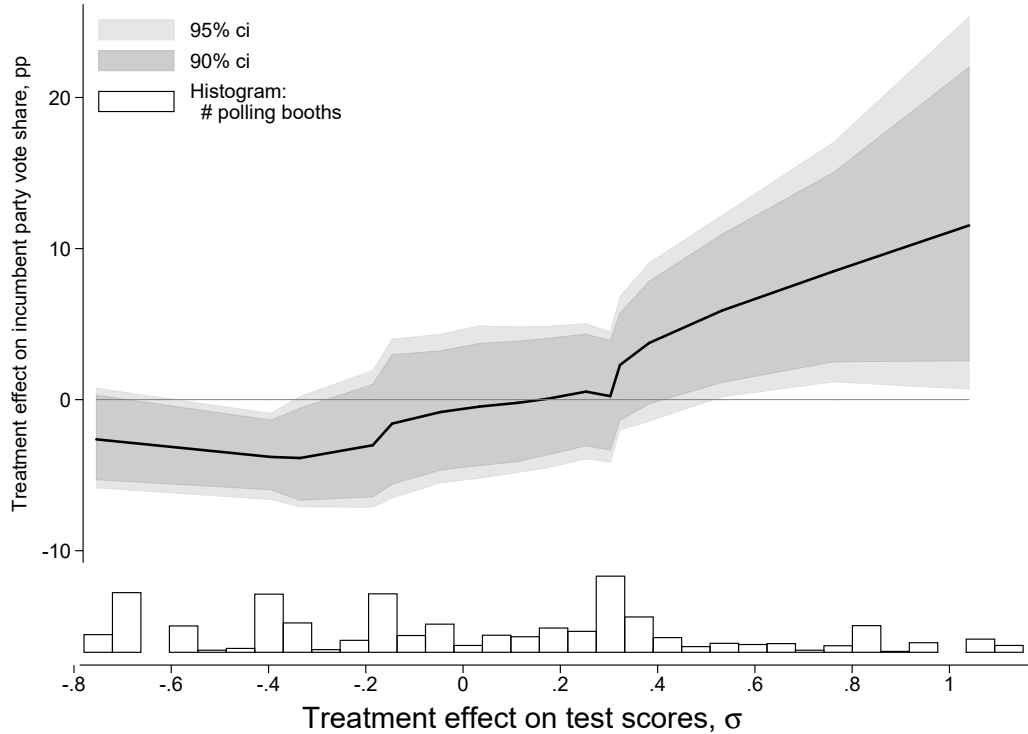
Standard errors clustered by electoral district. Nearest school matched-pair fixed effects included. Regressions include polling booths from the 2017 election located within 10km of any RCT school, with treatment of the polling booth defined as fraction of RCT schools assigned to the PSL treatment. Missing 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

The treatment decreased incumbent vote share where it improved student learning least. Negative electoral effects were driven by places where the program caused the smallest increases (or largest reductions) in test scores. In schools where the program caused big test score increases, the electoral effect was positive. As with the average effect, the policy only affected voters' choices in the presidential race.

Figure 5 depicts a similar analysis non-parametrically, plotting mean electoral treatment effects (with 90% and 95% bootstrapped confidence intervals) on the y-axis, against the distribution of local learning treatment effects on the x-axis.

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



*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%.*

It may be surprising that voters are able to perceive and reward something like learning gains, which are difficult even for PhD researchers to measure. Conversely, people living near these schools may observe important school quality variables which are invisible to researchers. In any case, some caution is in order in interpreting these results – learning treatment effects are not assigned randomly.<sup>19</sup> It’s possible that learning treatment effects correlate with other, more easily observable dimensions of heterogeneity which are what voters really care about. Especially when polling places are often schools, the salience of visible school improvements can be electorally meaningful (Ajzenman & Durante, 2020).

To check this, Table 7 reports results from similar analyses to Table 6, but with other observable dimensions of school quality. All columns in this table look at the same outcome variable: first-round vote share for the ruling party’s presidential candidate. All include controls for polling-place-level electoral outcomes from the previous presidential election in 2011. Each column looks separately at a dimension of

<sup>19</sup>For another recent example of useful non-random variation combined with an experiment, see Balboni et al. (2022).

school quality which voters might plausibly observe – and which was improved by the reform.<sup>20</sup> Column 1 interacts treatment with a dummy for whether the voting booth’s nearest school is part of a pair exhibiting above-median treatment effects on teacher attendance. Column 2’s interaction is with treatment effects on student attendance rates. Column 3 interacts treatment with a dummy for whether the treatment school underwent any construction or major repairs when the control school did not.<sup>21</sup> Finally, Column 4 includes both the construction interaction and the learning interaction from Table 6.

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<sup>20</sup>Romero et al. (2020) found that the reform significantly improved both student and teacher attendance, and furthermore, that teacher attendance was one of the best predictors of learning gains. The reform also caused an improvement in construction, though this was not reported in Romero et al. (2020).

<sup>21</sup>This variable took a value of 1 if the principal reported *either* new construction *or* major repairs in the foregoing year to any of the following: classrooms, office/staff rooms, store rooms, toilets/latrines, staff housing, library, playground, water source. 62% of treated schools, and 48% of control schools, underwent any construction or repairs by this measure. “Treatment effect” at the pair level here simply means the difference in this indicator between the treatment and control schools in a pair. The distribution of pair-level treatment effects is 28% positive, 60% zero, and 12% negative. So while I have expressed the dummy here as “above the median treatment effect,” this ends up being simply a dummy for a positive treatment effect. NB also that this variable is self-reported by principals. Alternatively, using an enumerator’s measure of observed classroom quality in this analysis yields a similar and significant result.

Table 7: Effects on 2017 vote share, interacted with treatment effect on various dimensions of school quality

|   | Ruling party: president (1st round) |         |           |           |
|---|-------------------------------------|---------|-----------|-----------|
|   | (1)                                 | (2)     | (3)       | (4)       |
| Fraction RCT schools treated  | -0.029*                             | -0.029  | -0.041*** | -0.059*** |
|   | (0.014)                             | (0.021) | (0.013)   | (0.014)   |
| Fraction RCT schools treated × TE>p50: teacher attendance                             | -0.001                              |         |           |           |
|   | (0.028)                             |         |           |           |
| Fraction RCT schools treated × TE>p50: student attendance                             |                                     | -0.001  |           |           |
|   |                                     | (0.031) |           |           |
| Fraction RCT schools treated × TE>p50: new construction or repairs                    |                                     |         | 0.093**   | 0.138**   |
|   |                                     |         | (0.036)   | (0.055)   |
| Fraction RCT schools treated × TE>p50: learning                                       |                                     |         |           | 0.067***  |
|   |                                     |         |           | (0.020)   |
| Fraction RCT schools treated × TE>p50: learning × TE>p50: new construction or repairs |                                     |         |           | -0.116    |
|   |                                     |         |           | (0.075)   |
| N   | 1200                                | 1200    | 1200      | 1200      |
| Mean (control)  | 0.293                               | 0.293   | 0.293     | 0.293     |
| Controls  | ✓                                   | ✓       | ✓         | ✓         |

Standard errors clustered by electoral district. Nearest school matched-pair fixed effects included. Regressions include polling booths from the 2017 election located within 10km of any RCT school, with treatment of the polling booth 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).

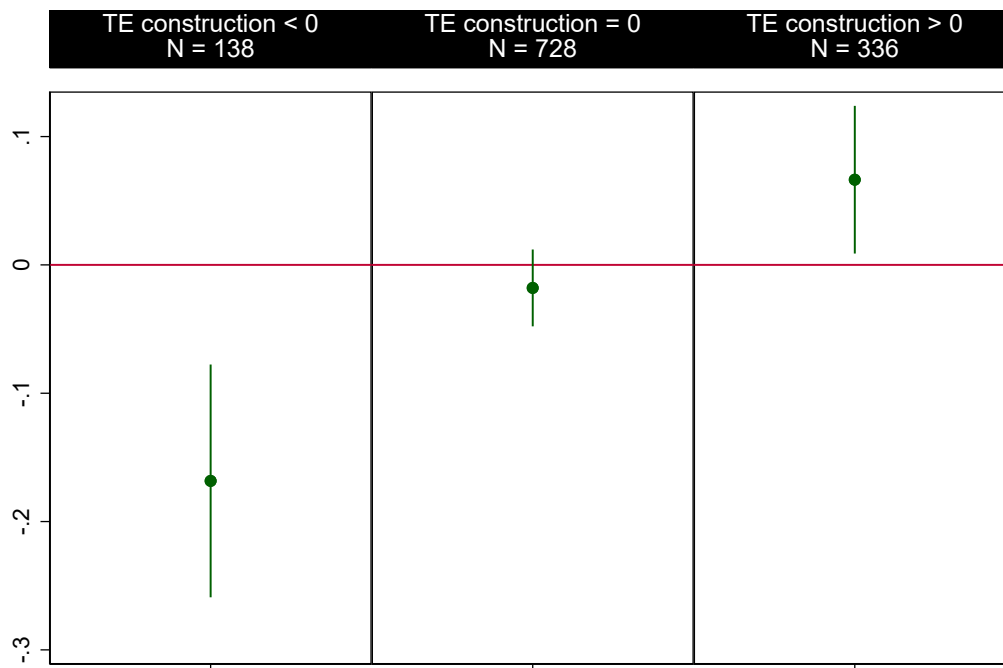
\* p<0.10, \*\* p<0.05, \*\*\* p<0.01

Although the reform has previously been shown to have caused gains in both teacher and student attendance overall, big treatment effects in these variables did not predict treatment effects in relevant electoral outcomes. Electoral gains did appear wherever school construction happened in the pair's treatment school but not in its control school. Figure 6 shows the symmetry of this effect: treatment caused significant electoral gains in places where treatment schools had construction and control schools didn't; it caused very large electoral losses where control schools had construction and treatment schools didn't; and it had (a reasonably precise) zero electoral effect where there was no difference in construction be-

tween treatment and control schools.

Does this mean voters are simply observing treatment effects on construction, which happen to correlate with treatment effects on learning? No. At the pair level (N=92), the correlation between learning TE and and construction TE is only 0.16. Column 4 of Table 7 interacts treatment with dummies for high treatment effects in both learning and construction, showing that both interactions have predictive power. This suggests that voters observe and reward learning gains independently of their perceptions and rewards of construction gains.

Figure 6: Effect of PSL on responsible party’s presidential vote share, by treatment effect on construction and repairs



*This figure plots the coefficient and 95% confidence intervals of the treatment effect of PSL on voting-booth-level vote share for the incumbent Unity Party’s presidential candidate in the 2017 election, for three different groups of polling booths. The leftmost column shows the coefficient for the booths whose nearest school is part of a matched pair where the control school underwent any new construction or major repairs and the treatment school did not. The middle column shows the coefficient for booths whose nearest school was part of a pair where both treatment and control schools had the same construction and repair status. The rightmost column shows the coefficient for booths whose nearest school was part of a pair where the treatment school experienced new construction or repairs while the control school did not.*

Overall, this evidence strongly suggests that voters reward improvements in school quality as measured along multiple dimensions.

#### 4.4 Heterogeneity in election outcomes by treatment effect on teacher political involvement

The model in Section B posits that teacher disengagement will reduce vote share for the ruling party; Table 8 tests this hypothesis. Its structure mirrors that of Table 6, showing electoral outcomes at the polling booth level, but interacting the treatment variable with a continuous measure of the school-pair-level treatment effect on various teacher outcomes from the booth's nearest school. This is a way of measuring whether the reform's effect on electoral outcomes was different in places where it affected teachers differently. The setup of this table mirrors that of Tables 6 and 7, and the same caveats apply: treatment effects are not randomly assigned across school pairs. While this table shows us how the causal effect of treatment differed across the dimensions of the interaction effects, it does not imply that the interacted variable *per se* was the cause of those differences. But it may suggest so.

There is a positive relationship between treatment effects on teacher political involvement and incumbent vote share. The places where treatment reduced teacher political involvement tended to be the same places where treatment reduced incumbent vote share. This is consistent with the mechanism of teacher alienation contributing to the reform's overall negative electoral effect for the incumbent party.

Table 8: Effect of PSL on incumbent vote share by size of treatment effect on teacher outcomes

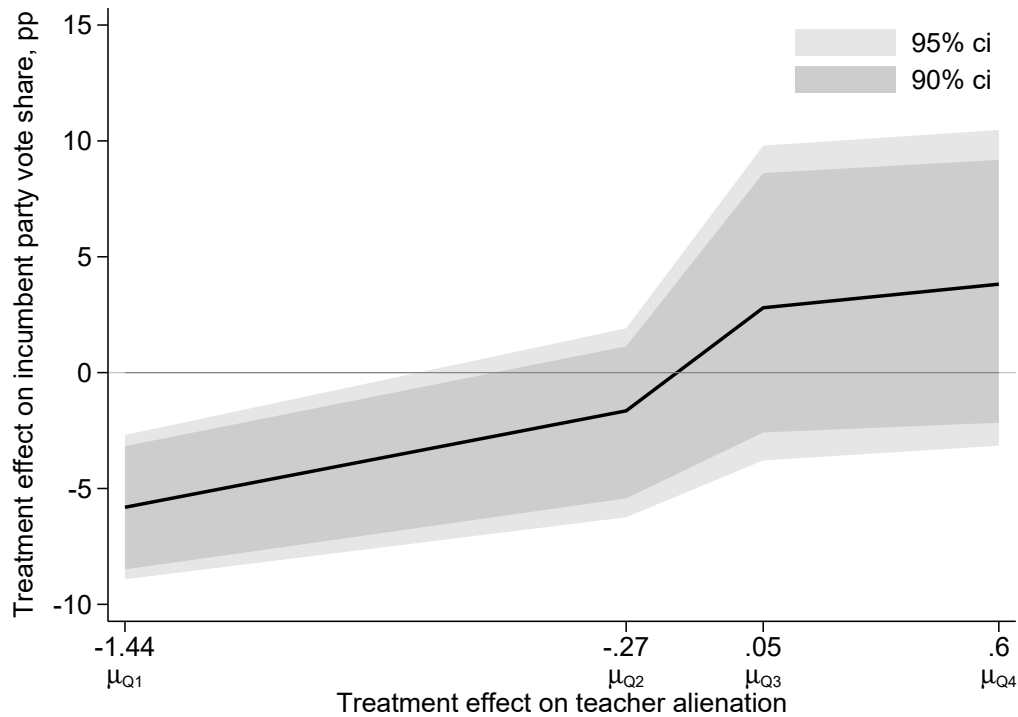
|   | Ruling party: president (1st round) |                   |                    |                   |                     |
|---|-------------------------------------|-------------------|--------------------|-------------------|---------------------|
|   | (1)                                 | (2)               | (3)                | (4)               | (5)                 |
| Fraction RCT schools treated                              | -0.005<br>(0.010)                   | -0.014<br>(0.014) | -0.011<br>(0.013)  | -0.021<br>(0.013) | -0.003<br>(0.012)   |
| Fraction RCT schools treated × TE Registration booths     | 0.312***<br>(0.084)                 |                   |                    |                   |                     |
| Fraction RCT schools treated × TE Polling booths          |                                     | 0.074<br>(0.067)  |                    |                   |                     |
| Fraction RCT schools treated × TE Encourage Participation |                                     |                   | 0.095**<br>(0.038) |                   |                     |
| Fraction RCT schools treated × TE Campaign                |                                     |                   |                    | 0.012<br>(0.042)  |                     |
| Fraction RCT schools treated × TE Involvement Index       |                                     |                   |                    |                   | 0.030***<br>(0.010) |
| N   | 1084                                | 1084              | 1084               | 1084              | 1084                |
| Mean (control)  | 0.293                               | 0.293             | 0.293              | 0.293             | 0.293               |
| Controls  | ✓                                   | ✓                 | ✓                  | ✓                 | ✓                   |

Standard errors clustered by electoral district. Nearest school matched-pair fixed effects included. Regressions include polling booths from the 2017 election located within 10km of any RCT school, with treatment of the polling booth 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

Figure 7 shows the same phenomenon graphically. Negative treatment effects on incumbent party vote share are concentrated in the places where the treatment caused the biggest political disengagement of teachers.

Figure 7: Effect of PSL on responsible party’s presidential vote share, by treatment effect on construction and repairs



*This figure plots the coefficient and 95% confidence intervals of the treatment effect of PSL on voting-booth-level vote share for the incumbent Unity Party’s presidential candidate in the 2017 election, for three different groups of polling booths. The leftmost column shows the coefficient for the booths whose nearest school is part of a matched pair where the control school underwent any new construction or major repairs and the treatment school did not. The middle column shows the coefficient for booths whose nearest school was part of a pair where both treatment and control schools had the same construction and repair status. The rightmost column shows the coefficient for booths whose nearest school was part of a pair where the treatment school experienced new construction or repairs while the control school did not.*

## 5 Discussion

Given that the policy won more votes in places where it increased learning more *and* in places where it caused more teacher political involvement, the policy implications depend on the relationship between these two outcomes. If the things which are necessary to increase learning are also the things which antagonize teachers, then policymakers face an inevitable direct tradeoff between improving learning and keeping the politically important teacher unions happy. However, if teacher disengagement is unrelated to the reform’s effectiveness at increasing learning, there may exist ways to craft a reform whose benefits outweigh the political costs.

In fact, the pair-level correlation between treatment effects on student learning and teacher political

activity was -0.07. To test this relationship another way, Table 9 examines the effect of the school reform on incumbent vote share, interacting the treatment variable with a dummy for whether the school pair had above-median treatment effects in learning *and* another dummy for whether the school pair had above-median treatment effects on teacher political participation. Both interaction terms in the regression are positive and statistically significant. The triple interaction between treatment and the two above-median treatment effect dummies is not statistically significant. This is consistent with improved school quality and alienated teachers having independent effects on electoral outcomes.

Table 9: Effect of PSL on incumbent vote share by size of treatment effect on teacher outcomes

|  | Ruling party: president (1st round) |                      |
|--|-------------------------------------|----------------------|
|  | (1)                                 | (2)                  |
| Fraction RCT schools treated   | -0.068***<br>(0.023)                | -0.056***<br>(0.017) |
| Fraction RCT schools treated $\times$ TE>p50: learning   | 0.088**<br>(0.036)                  | 0.072**<br>(0.032)   |
| Fraction RCT schools treated $\times$ TE>p50: Teacher political activity                           | 0.092**<br>(0.042)                  | 0.091***<br>(0.033)  |
| Fraction RCT schools treated $\times$ TE>p50: learning $\times$ TE>p50: Teacher political activity | -0.089<br>(0.060)                   | -0.088<br>(0.057)    |
| N  | 1084                                | 1084                 |
| Mean (control)   | 0.293                               | 0.293                |
| Controls   |                                     | ✓                    |

Standard errors clustered by electoral district. Nearest school matched-pair fixed effects included. Regressions include polling booths from the 2017 election located within 10km of any RCT school, with treatment of the polling booth 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 A2 provides another test of whether policymakers face a necessary tradeoff between broad voter appreciation and concentrated public servant ire. It tests the effect of the treatment on various measures of teacher political involvement, and interacts treatment with a pair-level indicator for whether the policy increased learning by more than the median amount in that pair. This is analogous to setup of Table 6, but with teacher behavior outcomes rather than electoral outcomes.

Table A2 shows that the policy's effect on teacher opposition is not a function of its effectiveness at

raising learning outcomes. In other words, the places where the reform caused the biggest learning gains were not the same places where it caused the biggest disruptions to teachers' political involvement. Across a range of relevant teacher outcomes – union membership, government payroll status, stated support for the incumbent government, and involvement in political activities – treatment effects did not correlate with treatment effects on learning. This suggests that teachers' political reactions to the policy were not conditioned on things that correlated with its effectiveness at increasing school quality.

## 5.1 Policy counterfactuals

How different would the policy have to be to win votes on net?

I use the estimated coefficients from the heterogeneous effects model to predict counterfactual policy scenarios that would have resulted in net vote gains for the incumbent party. I want to model the counterfactual impact of incremental changes, so I interact treatment with continuous measures of heterogeneity in learning and teacher alienation (unlike in Equation 2, which interacted treatment with a dummy for high treatment effects). For computational simplicity, I create a simplified treatment variable  $TreatSimple_i$ , by rounding the continuous treatment variable  $TreatIntensity_i$  to the nearest .5.  $TreatSimple_i$  is defined at the level of the polling booth  $i$  and has three categories: 0, 0.5, and 1, allowing it to capture some variation in the intensity of a polling booth's treatment. This entails the strong assumption of linear heterogeneous treatment effects. The outcome variable I focus on is vote share for the incumbent party in the first round of the presidential election. I include the same parsimonious set of controls  $\mathbf{X}_i$  for 2011 election outcomes that I used in Table 2 and Table 6. Equation 4 lays out the specification used for this exercise:

$$\begin{aligned}
 Y_{isp} = & \alpha_p + \beta_1 TreatSimple_i + \beta_2 TE\_Learning_p + \beta_3 TE\_TeacherInvolvement_p \\
 & + TreatSimple_i \times TE\_Learning_p + TreatSimple_i \times TE\_TeacherInvolvement_p \\
 & + TE\_Learning_p \times TE\_TeacherInvolvement_p \\
 & + TreatSimple_i \times TE\_Learning_p \times TE\_TeacherInvolvement_p + \mathbf{X}_i + \varepsilon_{isp}
 \end{aligned} \tag{4}$$

Here,  $TE\_Learning_p$  is the school-pair-level treatment effect on learning, and  $TE\_TeacherInvolvement_p$  is defined as the school-pair-level treatment effect on a standardized PCA index of teachers' political activities.

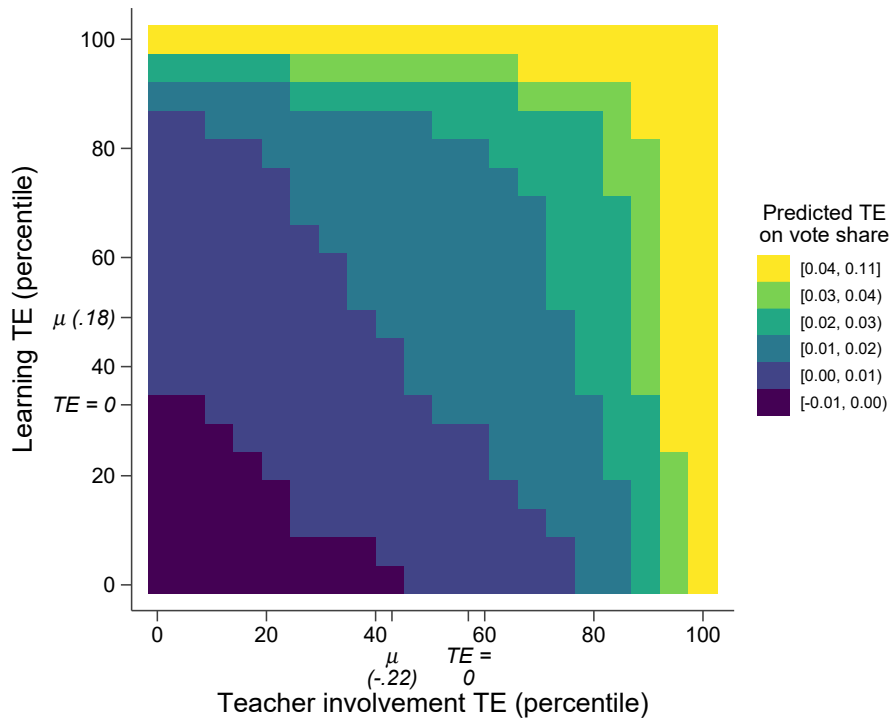
I estimate Equation 4 on the true data to recover estimated coefficients. Then I change the values of  $TE\_Learning_p$  and/or  $TE\_TeacherInvolvement_p$  iteratively, plugging in each instantiation of these counter-

factual values to the equation with the true estimated coefficients to predict incumbent vote share. In each counterfactual scenario, I test whether the average incumbent vote share in treated and partially treated polling booths exceeds that of the average control polling booth.

This model predicts that a counterfactual policy would have won votes on net if it raised the lowest treatment effects on learning to the 27th percentile, and raised the lowest treatment effects on teacher involvement to the 30th percentile. This would correspond to raising the floor on pair-level treatment effects to  $-0.105\sigma$  and  $-0.436\sigma$ , respectively. The policy could also become a net vote winner by raising the lowest learning treatment effects to the 52th percentile while holding teacher alienation constant, or by increasing the lowest teacher involvement treatment effects to the 47th percentile while holding learning constant.

Figure 8 summarizes predicted vote share for counterfactual distributions of treatment effects on student learning (y-axis) and teacher political involvement (x-axis). The baseline model prediction – using the true distributions of treatment effects – is in the bottom left corner. Each  $(x, y)$  coordinate represents the predicted vote share from a counterfactual winsorized pair of distributions, in which the floor on teacher involvement treatment effects is raised to the  $x$ th percentile and the floor on student learning effects is raised to the  $y$ th percentile.

Figure 8: Predicted counterfactual treatment effect on vote share, raising the floor on learning and teacher treatment effects



Each  $(x, y)$  on the graph represents predicted average treatment effect on ruling party vote share for a counterfactual policy in which the distribution of treatment effects on learning ( $y$ ) and teacher political involvement ( $x$ ) were left-tail-winsorized to  $(x, y)$ . The true average electoral treatment effect is the bottom left corner, with no winsorization of the distribution of treatment effects on learning or teacher involvement. On each axis, the true average treatment effect on learning or teacher involvement is indicated by  $\mu$ , and the zero treatment effect point is indicated by “TE = 0.”

Alternatively, it is possible to hold constant the shape of the distribution of treatment effects and predict counterfactual electoral effects in scenarios which shift the distributions of learning and teacher treatment effects to the right. The policy would be a net vote winner if it raised average treatment effects on learning and teacher involvement by at least  $.19\sigma$  and  $.19\sigma$  respectively. Focusing only on learning, the policy could have been a net vote winner if it increased average learning treatment effects alone by  $.29\sigma$  more than it actually did. Focusing only on teachers, the policy could have been a net vote winner if it increased average teacher involvement effects alone by  $0.59\sigma$  more than it actually did.

## 6 Information experiments

Another potential avenue for making public service provision politically incentive-compatible is increased voter information. A large body of literature posits that voter information aids electoral accountability.<sup>22</sup> In the context of this study, Section 4 established that the policy increased incumbent vote share most in the places where it increased learning the most. This suggests that voters reward better services. But 4 also established that the policy reduced teachers' political involvement, and that the largest reductions in teacher involvement coincided with the largest reductions in incumbent vote share, suggesting the importance of teachers as political actors. Might better-informed voters have rewarded the government for the policy, independently of teachers' political activity?

I designed two linked information experiments to test whether information constrains political accountability in this context. Accountability may break down through a lack of information on the part of either voters or politicians. Politicians can't respond to voters' preferences for services if they don't know which services voters care about, or which policies actually work to improve service quality. Similarly, voters need to know politicians' policies in order to elect those who champion the policies they favor.

The first experiment tests how politicians react to evidence on the policy's effectiveness and popularity. I surveyed 681 candidates for the House of Representatives on their beliefs about the Partnership Schools for Liberia program, varying whether I provided them with evidence about the policy's effects on learning and on voters' attitudes. To raise the stakes and avoid experimenter demand effects, survey enumerators told each candidate that they would soon be conducting a voter sensitization campaign, and offered to communicate the candidate's position on the policy to voters. The candidates' position provided in response to this offer constitutes my primary outcome.

The second experiment tests how voters react to the provision of candidates' positions. I surveyed 489 households of students from treatment and control schools, varying whether I shared the policy positions provided by the legislative candidates running in their district. I also shared positions about the policy expressed by presidential candidates in a public debate. I measured whether this affected households' voting intentions and attitudes.

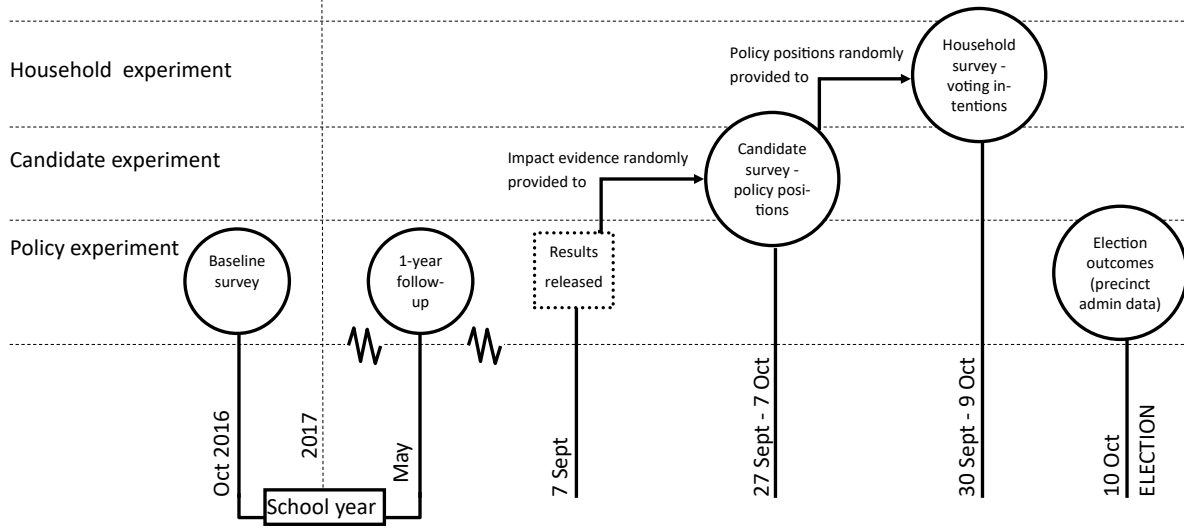
Figure 9 diagrams which data collection efforts happened at what time, and how they informed each other.<sup>23</sup>

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<sup>22</sup>Ferraz and Finan (2008) and Dunning et al. (2019) are two examples of empirical tests of this hypothesis.

<sup>23</sup>The experiments described here were pre-registered along with pre-analysis plans at <https://www.socialscienceregistry.org/trials/1501> (policy impact on political attitudes) and <https://www.socialscienceregistry.org/trials/2506> (information experiments for candidates and households).

Figure 9: Timeline



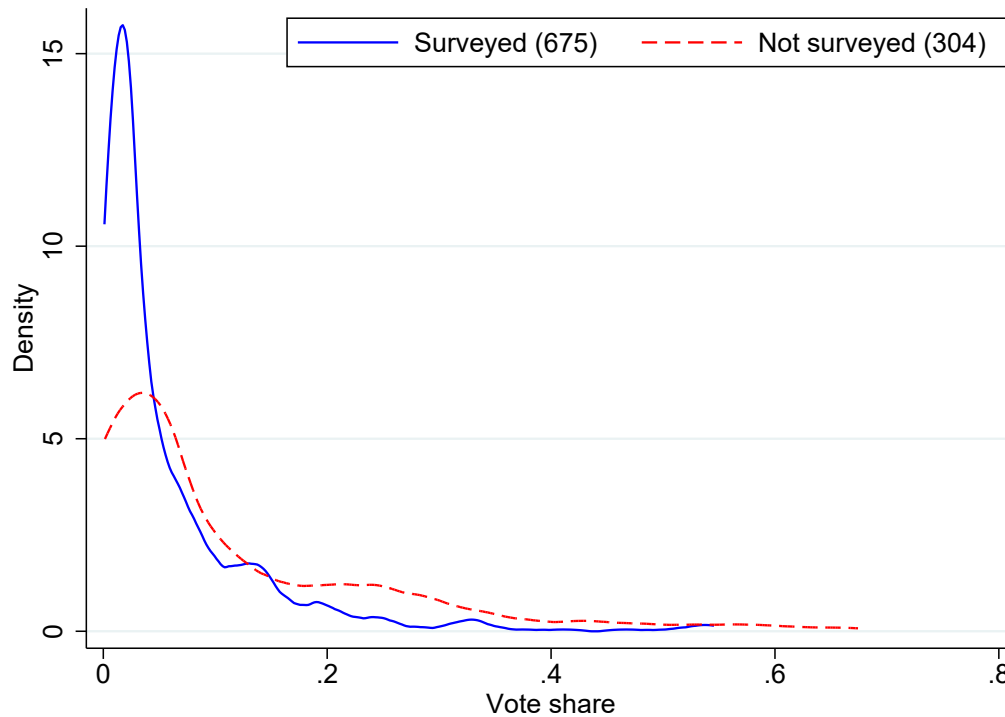
Timeline not to scale. Circles indicate data collection efforts, and the height of the circle indicates the relevant experiment.

## 6.1 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.<sup>24</sup> 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 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 10 plots the density of vote shares for candidates who did and did not participate in the survey.

<sup>24</sup>A randomized controlled trial registry entry and the pre-analysis plan for both the candidate and household experiments are available at: <https://www.socialscisceregistry.org/trials/2506>.

Figure 10: Distribution of vote shares for surveyed and unsurveyed candidates



*Note: Figure excludes 6 surveyed candidates in 6 different districts who dropped out of the race after being surveyed.*

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.<sup>25</sup> 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 voters’ attitudes (those seen in this paper in Table A4);
4. “Both:” control condition, impact information, and popularity information.

<sup>25</sup>Learning outcome RCT evidence took the form of a very concise synthesis of the main results from [Romero et al. \(2020\)](#). Popularity RCT evidence took the form of the results on household attitudes presented in Subsection A.1, Table A4 in this paper.

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

Table C.1 in the appendix shows balance on candidates' characteristics and pre-treatment survey responses.

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.

### 6.1.1 Candidate results

Table 10 shows that the information seems to have had little effect on candidates' stated support for, or opinions about, the policy. Column 1 is the primary outcome of interest: an indicator for whether the

candidate asked us to tell their voters that they favored the expansion of the PSL program. Columns 2-6 are indicator variables for whether the candidate agreed with the following statements: The government should fund PSL through tax revenues; the government should work with private education providers; there is too much foreign control of education in Liberia; PSL increases students' learning; and voters support the PSL program. Neither the popularity info, the effectiveness info, nor their combination seems to have affected any of these measures of candidates' opinions.

However, the summary means of the survey outcomes are illuminating. 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 10: 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<br>(0.056)  | 0.008<br>(0.052)  | 0.013<br>(0.035)             | -0.038<br>(0.055)        | -0.079**<br>(0.035)                   | -0.077<br>(0.054)  |
| Impact info                          | 0.050<br>(0.055)  | 0.034<br>(0.050)  | 0.013<br>(0.034)             | -0.048<br>(0.053)        | -0.012<br>(0.029)                     | 0.003<br>(0.052)   |
| Popularity info $\times$ Impact info | -0.063<br>(0.077) | -0.014<br>(0.071) | -0.014<br>(0.047)            | 0.065<br>(0.075)         | 0.079*<br>(0.046)                     | 0.086<br>(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.

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

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.)<sup>26</sup>

## 6.2 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 A4). 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 C.2 in the appendix 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.<sup>27</sup> The text of the

<sup>26</sup>This nonresult stands somewhat in contrast to Hjort et al. (2021)'s 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.

<sup>27</sup>This 26 September debate seems to have been the only presidential debate in which moderators asked about the PSL program.

treatment conditions can be found in Appendix C.

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.

### 6.2.1 Household results

Table 11 presents the effects of information about candidates' positions on approval ratings and voting intentions for presidential candidates.

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

|                | Pro-PSL<br>candidate<br>(Cummings) |                  | Anti-PSL<br>candidate 1<br>(Cooper) |                  | Anti-PSL<br>candidate 2<br>(Jones) |                   |
|----------------|------------------------------------|------------------|-------------------------------------|------------------|------------------------------------|-------------------|
|                | Approve                            | Vote             | Approve                             | Vote             | Approve                            | Vote              |
| Candidate info | 0.056<br>(0.037)                   | 0.030<br>(0.027) | -0.056*<br>(0.029)                  | 0.004<br>(0.004) | -0.108***<br>(0.034)               | -0.011<br>(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.

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

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 candidates 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.<sup>28</sup>

Table 12 shows the effects of information on candidates' policy positions on electoral outcomes for *legislative* candidates.

<sup>28</sup>To 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.

Table 12: Candidate information’s effect on household voting intentions: Representative

|                | Vote Rep.<br>PSL Yes | Vote Rep.<br>PSL No Info | Vote Rep.<br>UP  | Vote Rep.<br>Incumbent | Vote Any<br>Rep.  |
|----------------|----------------------|--------------------------|------------------|------------------------|-------------------|
| Candidate info | 0.001<br>(0.024)     | 0.041<br>(0.033)         | 0.026<br>(0.023) | 0.003<br>(0.026)       | -0.053<br>(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

Candidate information has no discernible effect on voting patterns for representatives. Column 1 is the primary outcome of interest: did the respondent intend to vote for a candidate who told survey enumerators they support the expansion of PSL? Households receiving the information were no more likely to plan to vote for representatives who supported (or opposed) the expansion of the policy. The mean at the bottom of Column 2 shows that the majority of respondents planned to vote for a candidate for whom I had no stated policy position (an artefact of the fact that more popular candidates were less likely to participate in the experiment). They were no more likely to vote for representatives from the ruling party (Column 3) nor for incumbents (Column 4). They were also no more or less likely to express a voting preference at all (Column 5).

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 suggests that households *knew* legislators were not responsible for the program. They may have seen information on legislative candidates’ PSL policy positions as immaterial. The effects of the policy on voting outcomes (as measured by both administrative data and survey outcomes) also imply that people were willing to invest enough research to form their own opinions without needing researcher-provided information. This highlights a difficulty of voter information intervention studies: people already have strong incentives to learn about issues they care about, so researchers can effectively only shock priors on issues that don’t matter much.

## 7 Conclusion

Economists are keen on identifying policy interventions in the effective set; we would do well to devote more energy to identifying the politically feasible set.<sup>29</sup>

Understanding whether and how elections create incentives to improve public services is a fundamental question of political economy, and one which needs more empirical evidence. The question is particularly urgent in a place like Liberia, a post-conflict country and one of the poorest countries in the world. Good public services, like education, can play an important role in creating the conditions necessary for economic growth. Voters consistently say in surveys that they prize good public services. Do they say so at the ballot box?

This paper shows that in Liberia – where democracy is young and literacy is around 50% – voters are sophisticated in their attribution of credit and blame for an important and controversial school reform. Both politicians and voters were reasonably well-informed about the policy’s effects. Electoral rewards for the policy were commensurate with its effectiveness: voters rewarded the responsible politician where the policy worked well (as measured by test scores and school infrastructure), and punished him where it worked poorly.

The paper also shows that policy reformers ignore interest groups at their peril. The school reform in question alienated teachers, as measured by their attitudes and their participation in political behavior. This seems to have had electoral consequences: the school reform caused greater electoral losses in the places where it alienated teachers most.

Overall, the policy caused a significant reduction in vote share for the candidate of the party that crafted it. But its implementation varied widely from school to school. Back-of-the-envelope counterfactual calculations suggest that the policy could well have been a net vote winner by modestly curbing its worst failures.

This paper highlights the risks and rewards of policy experimentation (Majumdar & Mukand, 2004). Policymakers who seek to improve public service delivery often face the unenviable task of shaking up entrenched systems full of committed supporters. They often lack credible evidence to predict 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 (Fernandez

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<sup>29</sup> Acemoglu (2010): “Political economy refers to the fact that the feasible set of interventions is often determined by political factors and that large counterfactuals will induce political responses from various actors and interest groups. . . . Although research in this area is expanding, given the importance of political economy for the problems of development, it remains surprising how few papers investigate key political economy channels using micro-data and careful empirical strategies.”

& Rodrik, 1991). In these circumstances, clientelism or vote-buying may provide a less risky path to electoral victory than investing in public goods and services (Wantchekon, 2003; Cruz, Keefer, Labonne, & Trebbi, 2018). It is possible, however, that more credible and targeted policy evidence could reduce policymakers' uncertainty on forecasts of policy effectiveness. If so, this could improve the odds that a reform's rewards from voters outweigh the opposition from interest groups. Can further research ease the transition from a politics of patronage to one based on public service delivery? Further research is needed.

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## Appendix

### A Additional tables

Table A1 shows the average electoral effect of the school reform on electoral outcomes in the October 2017 general election, limiting the sample to polling booths within 10km of exactly one treatment or control school.

Table A1: Average school policy effects on vote share

|                     | Ruling party:<br>president<br>(1st round) |                    | Ruling party:<br>president<br>(runoff) |                      | Ruling party:<br>legislative |                   | Incumbent:<br>legislative |                     |
|---------------------|---|--------------------|--|----------------------|------------------------------|-------------------|---------------------------|---------------------|
| Treatment intensity | -0.041<br>(0.028)                         | -0.045*<br>(0.024) | -0.049**<br>(0.023)                    | -0.055***<br>(0.018) | -0.026<br>(0.035)            | -0.027<br>(0.035) | 0.125***<br>(0.046)       | 0.125***<br>(0.046) |
| N                   | 285                                       | 285                | 285                                    | 285                  | 285                          | 285               | 285                       | 285                 |
| Mean (control)      | 0.310                                     | 0.310              | 0.402                                  | 0.402                | 0.149                        | 0.149             | 0.199                     | 0.199               |
| Controls            |   | ✓                  |  | ✓                    |                              | ✓                 |                           | ✓                   |

Standard errors clustered by electoral district. School matched-pair fixed effects included. Regressions include polling booths from the 2017 election located within 10km of any school in the RCT, with treatment defined as fraction of these 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. The row labeled displays the mean of the dependent variable for polling booths with Treatment = 0. 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 A2: Effect of PSL on teachers' political participation by learning treatment effects

|                              | Union member      | On govt payroll  | Intends to vote for ruling party pres. candidate | PCA index teacher involvement |
|------------------------------|-------------------|------------------|--|-------------------------------|
| Treatment                    | -0.063<br>(0.060) | 0.044<br>(0.076) | -0.117<br>(0.116)                                | -0.219*<br>(0.112)            |
| Treatment × TE>p50: learning | -0.057<br>(0.078) | 0.078<br>(0.108) | 0.004<br>(0.163)                                 | -0.012<br>(0.153)             |
| N                            | 421               | 421              | 183  | 847                           |
| Mean (control)               | 0.337             | 0.572            | 0.681  | 0.085                         |

Standard errors clustered by school. School matched-pair fixed effects included. Outcomes for columns 1-3 come from a May-June 2017 survey, and column 4's outcome is from a June/July 2019 follow-up survey. TE > p50 is an indicator for whether the teacher is at a school from a pair in which the pair-level treatment effect on learning is above the median.

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

Table A3: Treatment did not make schools polling stations

|                 | km 0.25           | km 0.10           | km 0.05           |
|-----------------|-------------------|-------------------|-------------------|
| (max) treatment | -0.018<br>(0.064) | -0.014<br>(0.045) | -0.025<br>(0.035) |
| N               | 185               | 185               | 185               |
| Mean (control)  | 0.272             | 0.109             | 0.076             |

School matched-pair fixed effects included. Outcome variable is whether the school is a polling booth (defined as whether the distance from school to polling booth GPS coordinates is within the radius at the top of the column).

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

### A.1 Effect of the policy on household survey responses

I supplement the foregoing results on administrative voting outcomes with survey data from the household members of students in treatment and control schools. Table A4 presents the effects of the policy on these household members' attitudes, as surveyed in May/June 2017, about five months before the election.<sup>30</sup>

<sup>30</sup>The RCT measures of the program's popularity which were presented to candidates as part of the candidate information experiment in Section 6 came from this survey.

Table A4: Effect of PSL on household attitudes (May 2017)

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

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

Table A4 shows that household members of students in treatment schools were broadly more satisfied with the government. First, although the PSL treatment consisted in outsourcing school management to private school operators, this does not seem to have affected attribution: over 90% of parents accurately perceived that the schools remained ultimately under government control and ownership. Households of students from treated schools became more satisfied with their children's education, as previously reported in [Romero et al. \(2020\)](#). Here I also displays new results on household attitudes: treatment caused them to be more impressed with the government's performance on schools, and more likely to say schools were their top priority for government spending. Treated households were more likely to agree with the statement that Liberia is "moving forward." However, there was no measurable effect on their satisfaction with the performance of the president (whose administration created the policy), or the legislator representing them in Congress. Effects on stated voting intentions for the ruling Unity Party (UP) which created the policy were negative but imprecise, due in part to the small number of respondents willing to divulge voting intentions.

Table A5: Effects of treatment on household attitudes by treatment effectiveness (Oct 2017)

|   | Heard of<br>PSL |         | Support<br>PSL |          | PSL $\Rightarrow$<br>Learning |         | Satisfied w/<br>legislator |         |
|---|-----------------|---------|----------------|----------|-------------------------------|---------|----------------------------|---------|
| In PSL treatment group                      | 0.068*          | 0.122** | 0.068*         | 0.115*** | 0.053**                       | 0.015   | 0.097**                    | 0.036   |
|   | (0.036)         | (0.052) | (0.035)        | (0.040)  | (0.027)                       | (0.039) | (0.041)                    | (0.057) |
| In PSL treatment group $\times$<br>TE > p50 |                 | -0.102  |                | -0.090   |                               | 0.073   |                            | 0.115   |
|   |                 | (0.071) |                | (0.070)  |                               | (0.052) |                            | (0.081) |
| N   | 489             | 489     | 489            | 489      | 489                           | 489     | 489                        | 489     |
| Mean (control)                              | 0.604           | 0.604   | 0.769          | 0.769    | 0.801                         | 0.801   | 0.264                      | 0.264   |
| FE?   | Pair            | Pair    | Pair           | Pair     | Pair                          | Pair    | Pair                       | Pair    |

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

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

Table A6: Effects of treatment on household voting intentions by treatment effectiveness (Oct 2017)

|   | Vote Pres.<br>UP |         | Vote Rep.<br>UP |         | Vote Rep.<br>Incumbent |         | Vote Rep. who<br>supports PSL |         |
|---|------------------|---------|-----------------|---------|------------------------|---------|-------------------------------|---------|
| In PSL treatment group                      | -0.051           | -0.019  | -0.002          | -0.008  | -0.002                 | -0.014  | 0.025                         | -0.034  |
|   | (0.033)          | (0.047) | (0.026)         | (0.041) | (0.030)                | (0.045) | (0.030)                       | (0.035) |
| In PSL treatment group $\times$<br>TE > p50 |                  | -0.060  |                 | 0.012   |                        | 0.022   |                               | 0.113*  |
|   |                  | (0.066) |                 | (0.052) |                        | (0.061) |                               | (0.059) |
| N   | 489              | 489     | 489             | 489     | 489                    | 489     | 489                           | 489     |
| Mean (control)                              | 0.510            | 0.510   | 0.156           | 0.156   | 0.238                  | 0.238   | 0.148                         | 0.148   |
| FE?   | Pair             | Pair    | Pair            | Pair    | Pair                   | Pair    | Pair                          | Pair    |

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

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

## B Conceptual framework

In this section I develop a conceptual framework for thinking about the countervailing electoral effects of voter rewards for, and teacher opposition to, public service reform.<sup>31</sup> It is inspired by this observation: although many canonical models present public good provision through the lens of redistribution, spending more money on public services often fails to move important dimensions of service quality in the empirical literature.<sup>32</sup> Improving public services may therefore sometimes depend at least as much on

<sup>31</sup>This paper refers to public services and public goods in the broad sense of positive externalities which will be undersupplied by the market relative to the social optimum, not in the narrow sense of goods which are non-excludable and non-rival.

<sup>32</sup>e.g. de Ree, Muralidharan, Pradhan, and Rogers (2018); Mbiti et al. (2019)

the political dynamics undergirding civil servant performance as on reallocating resources.

This framework builds on the model of [Lizzeri and Persico \(2004\)](#), which is built in turn on the model of redistributive politics of [Lindbeck and Weibull \(1987\)](#) and [Dixit and Londregan \(1996\)](#). [Lizzeri and Persico \(2004\)](#) consider the extension of the franchise, concluding that it induces parties to promise more public goods and fewer targetable transfers. However, in many parts of the world with universal suffrage, public good provision remains low and clientelism remains common. I modify [Lizzeri and Persico's](#) model in various ways to try to match this stylized fact. First, I abstract away from redistribution, focusing only on public good provision. Targetable transfers obviously affect utility in reality, and likely play a large role in politicians' competition for votes, not least through the channel of direct campaign vote-buying. But because I consider the case in which the money to increase public good provision does not come from taxes but from outside donations, I hold direct monetary redistribution constant and focus instead on the human resource side of patronage politics. Second, where [Lizzeri and Persico](#) permit an arbitrary number of voter groups, I consider the case of just two groups, which I call bureaucrats and voters. Third, I allow the different groups to have different utility functions – specifically, bureaucrats experience disutility from the work of providing public goods, as will be shown. Fourth, I allow the electioneering efforts of bureaucrats to influence the voting decisions of voters

## B.1 Model setup

Imagine there are two parties,  $R$  and  $C$  (ruling and challenging), which offer policy promises in order to maximize their vote share. In this context, a limited pilot program (like the Liberian intervention I study) can be considered a policy promise to scale up the policy.

A continuum of citizens of measure 1 is divided into two groups: voters and bureaucrats. Let  $i \in \{0, 1\}$ , where  $i = 0$  designates the voter group and  $i = 1$  designates the bureaucrat group. Mass  $n_1$  of the citizens are in the bureaucrat group and the remainder are voters. “Bureaucrats” here denotes people who work for the government in civil service jobs; my context specifically considers teachers. I assume for simplicity that citizens do not switch groups.

A public good can be produced using the technology  $g(I, A)$ , where  $I$  is the amount of money invested in the public good and  $A$  is a measure of civil servant effort (e.g. attendance). I follow the [Lizzeri and Persico \(2004\)](#) assumptions that  $g$  is strictly increasing, strictly concave, and twice differentiable, with  $g'(0, 0) = \infty$ . This includes, for tractability, the assumption that  $\frac{\delta g(I, A)}{\delta I} > 0$ . However, recent empirical evidence suggests that in many cases, increasing funding on its own has no impact on public service

quality.<sup>33</sup> For the purposes of this analysis, I consider the case in which  $I$  is held constant.<sup>34</sup> I focus instead on changes to the non-monetary inputs to the public good function captured in  $A$ . I assume that  $\frac{\delta g(I, A)}{\delta A} > 0$ .

The public good  $g(I, A)$  affects citizens' utility, but in different ways for different groups. Consider the utility function of voters:

$$U_0(g(I, A)).$$

Voters, in this simplified model, receive utility only from public goods and services, which are a function of  $I$  and  $A$ .

Bureaucrats, by contrast, are characterized by working in the civil service that provides public goods and services. The government can direct civil servants to perform two types of work:  $A$ , attendance at their public-service-providing job; and  $E$ , electioneering.  $E$  could encompass legitimate and legal behavior such as encouraging registration, campaigning, organizing, donating, soliciting donations, and getting out the vote. It could also include things like vote-buying and intimidation efforts. Bureaucrats have measure 1 of work hours, and I include the strong assumption that bureaucrats obey the government's directives, so  $A \in [0, 1]$  and  $E = 1 - A$  mechanically.<sup>35</sup> Bureaucrats' utility function is:

$$U_1(g(I, A) - f(A)).$$

$f(A)$  is a function characterizing the disutility of work, and it takes as its input the amount of civil service work assigned.  $A$  decreases bureaucrats' utility: I assume that  $\frac{\delta f(A)}{\delta A} > 0$ . For simplicity, I assume that bureaucrats do not experience disutility from  $E$ , because their incentives are aligned with the ruling party which is the source of their job.<sup>36</sup> Therefore, reforming public service provision by increasing  $A$  increases the utility of both bureaucrats and voters through the channel of better public goods – i.e.  $\forall i$ ,  $\frac{\delta g(I, A)}{\delta A} > 0$ . But it additionally and separately decreases bureaucrats' utility by encroaching on their leisure.

Parties simultaneously choose platforms by choosing a value of  $A$ , with  $A + E = 1$ .

Voters also have ideological party preferences. Each voter has an individual parameter  $x$ , which de-

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<sup>33</sup>e.g. de Ree et al. (2018); Mbiti et al. (2019)

<sup>34</sup>I also follow the Lizzeri and Persico (2004) assumption that the function  $g$  is strictly concave, so another way to think about my setting is that I consider the domain of a graph of  $g$  in which returns to  $I$  are exponentially diminishing (nearly flat).

<sup>35</sup>I assume here that bureaucrats carry out the tasks they're assigned, but it is also possible to imagine a model in which the actual realizations of  $A$  and/or  $E$  are determined partly by bureaucrats themselves and are endogenous to bureaucrats' utility calculation.

<sup>36</sup>An interesting extension of this model might include in this utility function a further element  $h(I)$  which denotes the utility bureaucrats receive from investments in public goods (presumably through higher wages or better conditions). I ignore this possibility in the present case, as the reform in question did not explicitly include increased funding for teachers.

notes the additional utility they realize if party C is elected. This  $x$  is drawn from a random variable distribution  $X_i$  specific to their group, and can be positive or negative. It captures preferences over any part of the party's platform which is unrelated to the provision of public goods (e.g. geographic, religious, philosophical, or ethnic affinities).  $F_i$  is the c.d.f. of  $X_i$ , with  $f_i$  the density (which I assume to be differentiable). Parties know the distribution  $F_i$  of the voters, but not the realizations of  $x$ .

## B.2 Bureaucrats' vote choice

Bureaucrats prefer that the ruling party  $R$  is elected if and only if

$$U(g(I_R, A_R) - f(A_R)) - U(g(I_C, A_C) - f(A_C)) > x_1.$$

Bureaucrats behave as if they are pivotal; if they prefer a party, they vote for it. The probability a bureaucrat votes for party  $R$  is therefore the same as the share of bureaucrats who vote for party  $R$ , denoted  $S_{R1}$ :

$$S_{R1} = F_1 \left[ U(g(I_R, A_R) - f(A_R)) - U(g(I_C, A_C) - f(A_C)) \right].$$

## B.3 Voters' vote choice

Voters' voting decision looks similar to that of bureaucrats, except that they are also swayed by the electioneering efforts  $E$  of bureaucrats on behalf of the government.<sup>37</sup> Voters also have some ideological preference for the challenging party,  $x_0$ , which is drawn from a distribution and can be positive or negative. But their vote choice is also influenced by the persuasion of bureaucrats,  $h()$ , which takes as arguments  $n_1$  the share of the electorate which is bureaucrats, and  $E$  the effort exerted by the bureaucrats.  $h()$  is assumed to be increasing in both  $n_1$  and  $E$ . Voters prefer that party  $R$  is elected if and only if

$$U(g(I_R, A_R)) - U(g(I_C, A_C)) + h(n_1, E) > x_0.$$

Just like bureaucrats, voters behave as if they are pivotal; if they prefer a party, they vote for it. The

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<sup>37</sup>In reality, the influence of civil servants comes not just from their attachment to the state but also from their organizational capacity, something that could conceivably be mobilized in favor of the ruling party or the challenging party, but for simplicity here I assume that  $E$  only nudges voters toward party  $R$ .

vote share for party  $R$  among voters is therefore equal to the probability a voter votes for party  $R$ :

$$S_{R0} = F_0 \left[ U(g(I_R, A_R)) - U(g(I_C, A_C)) + h(n_1, E) \right].$$

Party  $R$ 's total vote share is then the weighted sum of its vote share among bureaucrats and voters:

$$S_R = n_0 \cdot F_0 \left[ U(g(I_R, A_R)) - U(g(I_C, A_C)) + h(n_1, E) \right] \\ + n_1 \cdot F_1 \left[ U(g(I_R, A_R) - f(A_R)) - U(g(I_C, A_C) - f(A_C)) \right]. \quad (5)$$

Given party  $C$ 's platform, party  $R$  chooses a platform that solves the following maximization problem:

$$\max_A S_R$$

subject to

$$A + E = 1$$

#### B.4 Model predictions

**Proposition 1:**  $\frac{dS_R}{dg} > 0$ . The direct effect of increased public good provision is greater vote share for the ruling party.

**Proposition 2:**  $\frac{dS_R}{dE} > 0$ . The direct effect of increased electioneering is greater vote share for the ruling party.

#### B.5 Discussion of the model

The purpose of the model is to illuminate a politician's decision about  $A$ , that is, how much to direct civil servants to focus on public services rather than direct electioneering. The sign of  $\frac{dS_R}{dA}$  is theoretically ambiguous, and depends on the relative elasticities  $\frac{dS_R}{dg} > 0$  and  $\frac{dS_R}{dE} > 0$ . Essentially, increasing  $A$  will only increase overall vote share if its positive effect on all voters through better public goods is bigger than its negative effect on bureaucrats through increased effort PLUS its negative effect on voters through reduced  $E$ . This offers one potential rationalization of the existing empirical literature's varied findings on the effects of public service provision on vote share.

The model also illuminates comparative statics for  $n_1$ , the share of the electorate which are bureaucrats. Crucially, the sign of  $\frac{dS_R}{dn_1}$  depends on bureaucrats' vote choice. Increased  $n_1$  always increases voters' support for the ruling party through  $h(n_1, E)$ . But this effect could potentially be outweighed by the direct negative effect of bureaucrats' vote choice if they oppose the ruling party – especially if that opposition is due to increased attendance  $A$ , which implies decreased electioneering  $E$ .

Another relevant lever potentially available to policymakers is the functional form of the public good technology. While not explicitly modeled in this framework, in reality governments sometimes experiment and research in order to learn other public service production functions; such was the partial rationale for PSL. A function  $g(\cdot)$  which turns  $A$  into public goods more efficiently can theoretically lead politicians to a higher or lower allocation of  $A$ , depending again on the relative elasticities  $\frac{dS_R}{dg} > 0$  and  $\frac{dS_R}{dE} > 0$ .

The model's central insight is this: when the front-line workers responsible for providing public goods also play a role in campaigning and electioneering, any change in public good provision becomes a gamble that the electoral benefits will outweigh the costs.

## C Information experiments - detail

### C.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.<sup>38</sup> The evaluation also identified some problems: in some schools run by Bridge 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.

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<sup>38</sup>This was an inadvertent error. Teachers were in fact 50% more likely to attend, not twice as likely.

## C.2 Candidate balance and summary statistics

The balance check on candidates' characteristics and pre-treatment survey responses is in Table C.1. For simplicity of comparison, it pools all information treatments into a single "any information" treatment.

Table C.1: Balance – Candidate experiment

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

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

### C.3 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]*

#### **C.4 Household balance and summary statistics**

Table C.2 shows balance of the October 2017 household sample on pre-information-treatment characteristics.

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

| Variable   | Candidate        |                  | Difference          | N   |
|--|------------------|------------------|---------------------|-----|
|  | Control          | Info             |                     |     |
| In PSL treatment group   | 0.543<br>(0.499) | 0.514<br>(0.501) | -0.028<br>(0.045)   | 494 |
| Heard of PSL   | 0.644<br>(0.480) | 0.615<br>(0.487) | -0.028<br>(0.044)   | 494 |
| Heard of any operator  | 0.862<br>(0.345) | 0.842<br>(0.365) | -0.020<br>(0.032)   | 494 |
| Legislature created PSL  | 0.008<br>(0.090) | 0.008<br>(0.090) | 0.000<br>(0.008)    | 494 |
| Correctly identifies responsible party for PSL                             | 0.441<br>(0.498) | 0.417<br>(0.494) | -0.024<br>(0.045)   | 494 |
| It's good for gov to work w/<br>private companies to provide sch           | 0.962<br>(0.190) | 0.942<br>(0.235) | -0.021<br>(0.020)   | 480 |
| PSL should be expanded and funded<br>through the national budget.          | 0.803<br>(0.398) | 0.782<br>(0.413) | -0.021<br>(0.037)   | 478 |
| Children learn more in PSL schools   | 0.827<br>(0.379) | 0.850<br>(0.358) | 0.022<br>(0.035)    | 446 |
| Knows current Representative's name  | 0.822<br>(0.383) | 0.866<br>(0.341) | 0.045<br>(0.033)    | 494 |
| Satisfied with Representative  | 0.342<br>(0.475) | 0.308<br>(0.463) | -0.034<br>(0.043)   | 477 |
| Related to a rep. candidate  | 0.170<br>(0.376) | 0.184<br>(0.388) | 0.014<br>(0.034)    | 492 |
| Related to member of teachers' union                                       | 0.333<br>(0.472) | 0.277<br>(0.448) | -0.056<br>(0.042)   | 485 |
| Attended any campaign event  | 0.358<br>(0.480) | 0.329<br>(0.471) | -0.029<br>(0.043)   | 489 |
| Any candidate has talked about PSL   | 0.146<br>(0.354) | 0.160<br>(0.367) | 0.014<br>(0.034)    | 444 |
| Fraction of district's candidates who<br>provided info to candidate survey | 0.354<br>(0.165) | 0.317<br>(0.148) | -0.037**<br>(0.015) | 467 |

Notes

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