The politics of public service reform: Experimental evidence from Liberia

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Abstract

This paper provides experimental evidence on the electoral effect of a large education reform in a developing democracy. Despite significantly improving school quality, the policy reduced the incumbent party's presidential vote share by 3 percentage points (10%). This does not imply that voters fundamentally oppose service improvements: household surveys showed strong support for the policy, and variation in school-pair-level treatment effects shows that the more the policy raised test scores, the more it increased incumbent vote share. Instead, the negative average electoral effect was driven by opposition from teachers. The policy reduced teachers' job satisfaction, their support for the incumbent government, and their political engagement. The more the policy reduced teacher political engagement, the more it reduced incumbent vote share. Counterfactual simulations suggest that relatively small improvements in effectiveness and/or teacher engagement could have made the policy a net vote winner. This paper empirically demonstrates the importance of political feasibility in the design of public service reforms.

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.*
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> — Niccolò Machiavelli, *The Prince*

Improving public services at scale requires political support (A. Banerjee et al., 2017). In democracies, this generally means that even effective policies won't be implemented in equilibrium if they don't win votes. But the electoral returns to public service quality are theoretically ambiguous. In theory, public service users reward improved services by supporting incumbents at the ballot box (Persson & Tabellini, 2000). But better services tend to imply unwanted change to the status quo for the providers of those services, who are often organized and politically important (Chaudhury, Hammer, Kremer, Muralidharan, & Rogers, 2006; Bold, Kimenyi, Mwabu, Ng, & Sandefur, 2018). Improving services can only be politically incentive-compatible when the electoral rewards from service users outweigh the cost of antagonizing service providers (Finan, Olken, & Pande, 2017; Olson, 1965).

Empirical evidence on this topic is hampered by the difficulty of measuring the electoral benefits and costs of public service provision (Acemoglu, 2010). While a growing literature documents the positive electoral returns to government spending, the effects of service quality itself are harder to measure, and the correlation between spending and service quality is weak (Golden & Min, 2013; World Bank, 2004). Exogenous shocks to service quality are rare; politicians often target investments to maximize perceived political benefit (Burgess, Jedwab, Miguel, Morjaria, & Padró I Miquel, 2015). Even where sufficiently granular and geo-located electoral data are available, few surveys measure the political activities of public service providers, making it hard to get a complete picture of the mechanisms behind any potential electoral effects.

This paper provides experimental evidence on the electoral effect of a large education reform in a developing democracy. The context of the study is Liberia, which is paradigmatic of the circumstances in which political feasibility is a binding constraint on public service quality. Elections have been held since 2005, and are imperfect but consequential; losers leave office. In 2017 Liberia's democracy was classified as a "hybrid regime," along with 18% of the world's population – just below Tanzania and Bangladesh, and just above Nepal and Kenya (Economist Intelligence Unit, 2022). As in most low- and middle-income countries, voters in Liberia express strong preferences for improved public goods and services (Grossman & Slough, 2022). Also as in most low- and middle-income countries, Liberia's public school teachers have

steady job security, a high wage premium, and a strong political voice (Glewwe & Muralidharan, 2016).

The reform in question was a public-private partnership in education which improved school quality along several dimensions. The Partnership Schools for Liberia (PSL) program designated 93 schools, randomly selected from a list of eligible government primary schools, to be managed by private school operators, and to receive more teachers and funding. The private operators were prohibited from charging fees or screening out students, and the buildings remained government property. In a nod to the power of the teacher's union (which vocally resisted the policy), the scheme also prohibited operators from employing non-government teachers. Romero, Sandefur, and Sandholtz (2020) evaluate the program's educational outcomes. The reform reduced pupil-teacher ratios and increased teacher attendance, teacher time-on-task, student enrollment, student attendance, and student test scores. There was also fierce opposition to the reform, fueled in part by high-profile controversies such as a private school operator's decision to cap class sizes and reassign teachers. Despite these problems, the program increased the overall satisfaction of students and parents alike.

I exploit the random assignment of the policy to measure its causal effect on average electoral outcomes. Treatment was assigned randomly within pairs of ex-ante similar schools. Using geo-located polling booth-level administrative data from the 2017 general election, one year after the policy began, I compare vote tallies at booths near treatment schools to those near control schools. I also measure the reform's effect on the political attitudes and activities of teachers, using a teacher survey conducted in treatment and control schools after the election.

My main finding is that the reform significantly reduced the ruling party's vote share. Its presidential candidate, who had been a part of the administration which created and implemented the policy, received 10% fewer votes in polling booths near treatment schools compared to control schools (3 percentage points from a base of 29 percentage points). There was no significant effect on vote share for incumbent or ruling-party legislative candidates — who had no responsibility for the policy — consistent with a correct attribution on the part of the voters.

The reform also antagonized teachers. It lowered teachers' job satisfaction on average by 0.18σ, and reduced their voting intentions for the ruling party by 8 percentage points (12%). It also diminished teachers' political engagement. In Liberia, teachers play a significant electoral role: in control schools, 40% of them report participating in some kind of political activity, such as staffing polling stations or campaigning for parties or candidates. In treated schools, this engagement was reduced by 25% (10 percentage points).

The interpretation of the average electoral effect, and its policy implications, depend upon the mechanisms behind it. Is the disaffection of teachers driving the voting outcomes? Or do voters simply oppose increased investments in public services, favoring other priorities? I test these mechanisms using variation in treatment-control differences within the *ex-ante* similar school pairs used as randomization strata. I test whether electoral treatment effects at the school pair level correlate with treatment effects on 1) school quality and 2) teacher political activity. While the pair-level variation in local treatment effects is not randomly assigned, and should not be interpreted causally, this exercise sheds light on the drivers of the experimentally-estimated average treatment effects.[1]

Treatment effects on incumbent vote share are increasing in treatment effects on school quality, suggesting that the negative average electoral effect is not driven by opposition or apathy from service users. Where the reform improved schools most, it also increased incumbent vote share. This holds whether school quality is measured using student test scores, capital investment such as classroom construction and repairs, or one controlling for the other. This result is consistent with the predictions of retrospective voting theory (Ferejohn, 1986; Key, 1966).

Instead, I find evidence that the negative effect on vote share is driven by the political disengagement of dissatisfied teachers. Teacher disaffection appears to have affected other voters' behavior: there is a negative gradient between treatment effects on teacher political disengagement and treatment effects on incumbent vote share. Where the policy disengaged teachers the most, it also reduced incumbent vote share the most. I illustrate these opposing mechanisms in a conceptual framework which models vote choice as a function of public service quality and the persuasive efforts of providers, who receive an efficiency wage for electioneering in addition to providing public services (see section B). Intuitively, higher-quality services raise voters' support for the incumbent government, but disgruntled teachers reduce it. The sign of the average net electoral effect is a function of the effectiveness of the policy, the strength of providers' opposition, and the persuasiveness of providers.

Is there an alternate version of this policy that would have won votes on net? Alienating teachers does not seem to have been a necessary condition for improving school quality: the correlation between treatment effects on learning and treatment effects on teachers' political activity is very weak. In a set of policy counterfactual simulations, I show that the program could have been a vote winner with relatively small improvements. If the minimum treatment effect on learning and on teacher political activity had been what was in fact the 25th percentile, the net effect on incumbent vote share would have been positive

[1] See Balboni, Bandiera, Burgess, Ghatak, and Heil (2022) for another context where non-random variation provides a vital test of mechanisms in a randomized experiment.

(see Section 5).

The paper makes several contributions to the literature on public services and political economy. First, it provides experimental evidence on the electoral consequences of a reform to improve public service delivery. Previous studies show that politicians can win votes with distributive transfers, whether clientelistic or programmatic (Vicente & Wantchekon, 2009; Manacorda, Miguel, & Vigorito, 2011). But the evidence on electoral returns to broad-based growth-promoting public goods and services – especially education – is much more sparse.[2] This paper advances the literature by measuring the electoral impacts of a rigorously-evaluated education policy using administrative voting data, illustrating that policy effectiveness is not sufficient for political feasibility. The study's second contribution is to decompose the electoral effect of a policy reform into rewards from service users and punishments from providers. Papers such as Litschig and Morrison (2013) and Bursztyn (2016) have examined service users' political responses to education spending, while Ross Schneider (2021) considers how teachers' political organization affects the shape of reforms. This paper measures both channels in the same context, highlighting the difficulty of interpreting studies which report average electoral effects alone. Previous work has suggested that voters can't exercise accountability for intangibles such as school quality (Harding & Stasavage, 2014). I show that voters are in fact attuned to quality, though other factors can obscure this.

Finally, this paper underscores the importance of considering political incentive compatibility when designing policies intended to improve public services. Researchers studying the scale-up of effective policies have long acknowledged the importance of political economy constraints, but systematic empirical evidence on these constraints (and how to alleviate them) has lagged behind (A. Banerjee et al., 2017; Acemoglu, 2010). This paper shows that a credible threat of electoral challenges belongs on the list of ways service providers can scuttle proposed reforms (Callen, Gulzar, Hasanain, Khan, & Rezaee, 2023; Bold et al., 2018; A. V. Banerjee, Duflo, & Glennerster, 2008). It also illustrates a powerful driving force behind the status quo bias in service delivery reforms, furnishing empirical evidence to support the theoretical prediction that politicians rationally "pander" to voters rather than solve tough social problems (Fernandez & Rodrik, 1991; Maskin & Tirole, 2004). Political economy constraints frequently hinder the adoption of policies considered "first-best" by economists in domains such as trade and environmental regulation (Autor, Dorn, Hanson, & Majlesi, 2020; Che, Lu, Pierce, Schott, & Tao, 2016; Ostry, Furceri, & Ganslmeier, 2021). They are also a first-order concern for the design of public service policies. The notion that public services improve through electoral pressure has been called the "long route of political accountability"

[2]See Hartmann and Sandholtz (2023) for a review of the nascent empirical literature on the electoral effects of public good and service provision.

(World Bank, 2004). This paper shows that without careful design, the long route of accountability can run backwards.

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

2 Context

In 2016, Liberia's Ministry of Education was looking for inspiration. After the country was ravaged by civil war and the Ebola epidemic, the capacity of the government to deliver meaningful results for its citizens was inhibited on almost every front. The Ministry of Education was no exception. It had been struggling to provide a level of education deemed basic by international institutions – or by its own citizens. Efforts to remove ghost teachers from the payroll were short-lived (Rosenberg, 2016), while teacher absenteeism was endemic (Mulkeen, 2009). In 2014-15, net enrollment of primary students was among the world's lowest, at 38%, and the youth literacy rate (age 15-24) was 55% (World Bank, 2014b, 2014a). University prospects were little better: 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 the schools that prepared them . . . 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, with the government's Information Minister calling the education crisis a "national emergency" (The New 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 per year. This extra grant represented a doubling of the baseline level of per-pupil expenditure.[3] The operators were given responsibility for (though not ownership of)

[3]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

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), while others were non-profit NGOs, some based in Liberia and some based further afield.

The government commissioned an independent randomized controlled trial to evaluate 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. PSL school buildings remained under the ownership of the government. Like regular public schools, PSL schools were required to be non-selective – i.e., operators were not allowed to choose which students to enroll, and were required 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 teachers were also required to be civil servants on the government payroll, unlike in PPP schemes in other countries, limiting the operators' ability to hold teachers accountable for learning outcomes. However, operators were allowed to test teachers themselves and request that the Ministry reassign underperformers elsewhere - a prerogative which one operator exercised significantly (Romero et al., 2020). 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. Fees were forbidden at the PSL schools, including for ECE, and the private operators were required to agree to school inspections and data sharing for the evaluation of their performance. (Despite this element, no formal mechanisms to hold operators accountable were created in the policy's first year of operation.) Finally, 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 in Liberia is set centrally

schools to USD $100 (Werner, 2017a, 2017b)

by the executive branch, and Liberia has no elected local politicians, so the incumbent president's party could clearly and credibly claim credit.[4] 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.[5] Finally, the involvement of the private sector in the provision of public services makes the electoral effects particularly interesting. 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 (Crawfurd & Hares, 2021). This reform offers a chance to test whether governments can win votes by improving public services, even by outsourcing service delivery to the private sector.

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 0.18σ, 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%.[6]

By design, the policy featured a wide variation in teaching practices across the different private school operators. Some emphasized longer school days, others teacher training, others technology. The premise of the program was that successful practices could be identified and shared across the system. The reality was somewhat different. For example, although the contract governing the policy permitted operators to enforce limits on class sizes, only one operator chose to do so, forcing hundreds of students to find a new school. The same operator requested that the Ministry reassign 74% of its teachers, creating large teacher turnover and negative externalities for the broader system.

The matched-pair randomization permits an unbiased estimate of treatment effects for each school

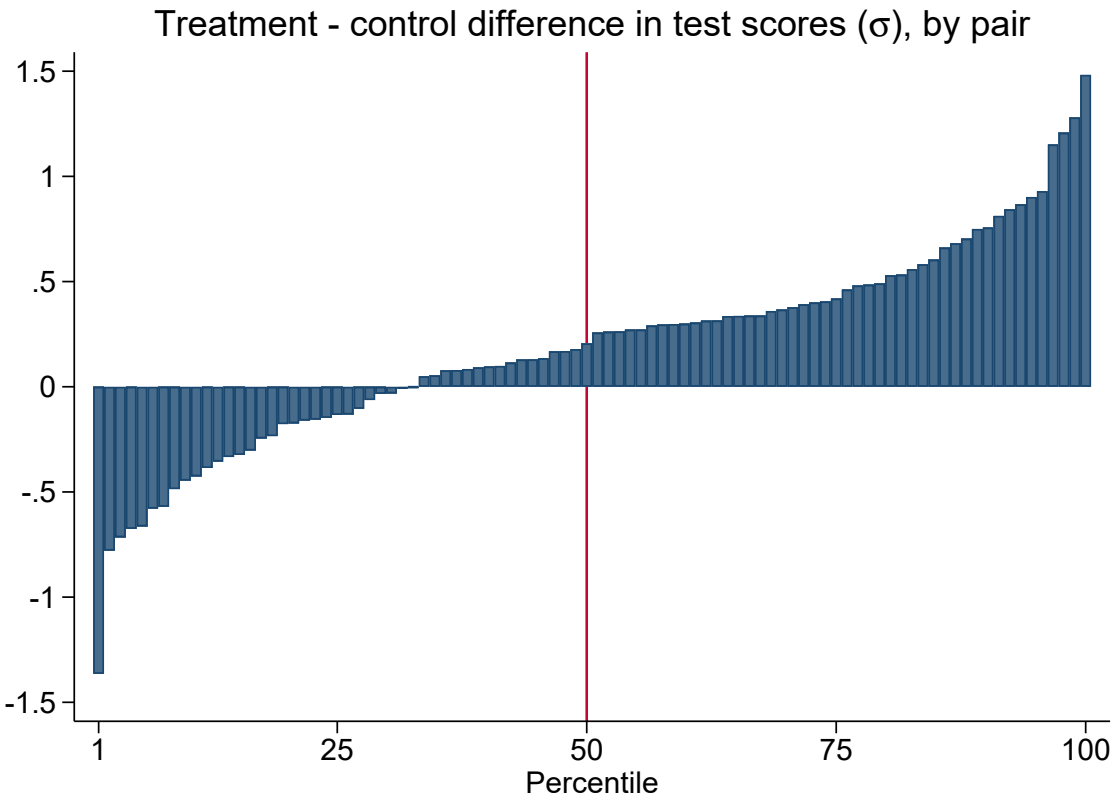
[4] See e.g. Cruz and Schneider (2017); Guiteras and Mobarak (2015) for examples of local politicians successfully claiming credit for policies they didn't create.

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

[6] 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.

pair. Prior to randomization, schools were matched into pairs by their level of enrollment and resources. This pairwise matching has benefits for statistical power (Bruhn & McKenzie, 2009). It also enables local measurement of treatment effects, as every individual program school has a counterfactual. Differences in learning outcomes between the treatment and control schools within each pair provide a unbiased (if noisy) measure of pair-level treatment effects. While the overall average effect on learning was positive, a significant minority of school pairs featured higher test scores in the control school. 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:

Figure 1: Heterogeneity in treatment effect on test scores



Each bar shows the treatment-control difference in composite test scores, measured in standard deviations, of one of the 92 school pairs in the experiment at the end of the policy's first school year. The median pair's difference was 0.23σ. *67% of pairs had a positive difference. The overall average treatment effect was* .18σ.

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.[7]

The constitutional political structure is modeled on the United States' system. Voters elect a president, who is head state and the head of the executive branch, and a bicameral legislature consisting of the Senate and the House of Representatives. Each of Liberia's 15 counties is represented by two senators, which each member of the House of Representatives represents one of Liberia's 73 constituencies (as of 2017). Presidents and representatives are elected for six-year terms, and senators for nine-year terms. There are no term limits in either legislative chamber, but presidents are limited to two terms. For presidential races, an outright majority is required; in the case that no candidate receives over 50% of votes in the first round, the top two vote-winners 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, and in none of Liberia's counties has any party won a "clean sweep" of presidential and legislative races in any of the three elections from 2005-2017. The two largest parties are the Unity Party (UP), which won the presidency in 2005 and 2011; 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. But 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 on the ticket of a different party than 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 and omnipotent network of big men and followers" (Bøås & Utas, 2014). For an example at the legislative level, the three-

[7] 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).

time House election winner Zoe Pennue of Grand Gedeh County was reported to have found success through personal patronage, "from paying school fees and hospital bills to donating cars" (Pailey & Harris, 2020). A survey conducted among households of the students in treatment and control schools in the weeks leading up to the election (as part of a companion paper) sought to assess the prevalence of patronage-like campaign tactics through candidates paying for voters' education expenses (Sandholtz, 2023a). 26% reported that at least one candidate had paid school fees in their district that year. 38% reported candidates paying for education expenses more broadly.[8]

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, with people looking to teachers for advice. In urban areas, going on strike gives them the power to unsettle the city's daily life. In these and other ways, teachers can influence election and policy choices.[9] 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. These jobs come with the expectation that those given them will help turn out people to vote for the politicians who gave them their role (Fergusson, Harker, Molina, & Yamín, 2023; Larreguy, Montiel Olea, & Querubin, 2017; Pierskalla & Sacks, 2019). Teacher unions, and their links to political machines, have successfully derailed education reform in other contexts (Finger, 2018; Ross Schneider, 2021; Bruns, Macdonald, & Schneider, 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

[8]This broader category included school fees, uniforms, school construction, school maintenance, teacher salaries, or other supplies like textbooks or copybooks. By far the most common form of education-related patronage was school fees.

[9]Source: author's conversation with a former deputy minister of education

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 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, 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 first round of the election took place 10 October 2017, with 75.2% of registered voters casting a ballot. George Weah and Joseph Boakai took the top two spots, with 38% and 29%, respectively. 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). Weah won the runoff, with 62% of the vote.

2.4 The school reform in the 2017 election

The ruling Unity Party (UP) claimed credit for the partnership school program in various ways during the 2017 election. It was the administration's cration, and they had made reform of the education system a clear priority. 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 Nation's 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. Perhaps anticipating this opposition, the Ministry of Education had designed the policy with civil servant teachers in mind, prohibiting private operators from hiring 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). However, 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 response to the policy. 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 significant but not universal, and the Minister resisted the calls to resign (Ziamo, 2016; Kwanue, 2016b). But the strike escalated as students responded to their teachers' strike by blocking the main highway to the country's international airport, demanding that the government and the teachers' union order the teachers back to class (Brooks, 2016a; Front Page Africa, 2016b, 2016a). In at least one city, the protests turned violent, and public buildings were ransacked (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), supported the policy (Malkus, 2017). However, 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 reliance on technical competence and its coziness with Western donors (elements characteristic of the PSL program) hadn't translated into gains for ordinary Liberians (Posthumus, 2017).

3 Data and 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.

3.1 Administrative electoral data

The main outcomes in this paper use administrative election data from the polling booth level.[10] 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.[11] Some booths are near both treatment and control schools; I define each booth's "treatment intensity" continuously as the number of treated schools divided by the number of total treatment and control schools within the radius.[12] 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 8 displays a histogram of all 2080 polling booths in the 2017 election by their distance to the nearest treatment or control school.

The main specifications in this paper define treatment using a radius of 10km. This radius is wide

[10] 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.

[11] 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.

[12] 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 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.

enough to embrace at least one polling booth for all 185 schools and 92 school pairs in the RCT.[13] 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 8). 97% of students in the RCT live within 10km of their school. Figure 2 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 9 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 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.[14]

3.2 Empirical Specifications

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

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

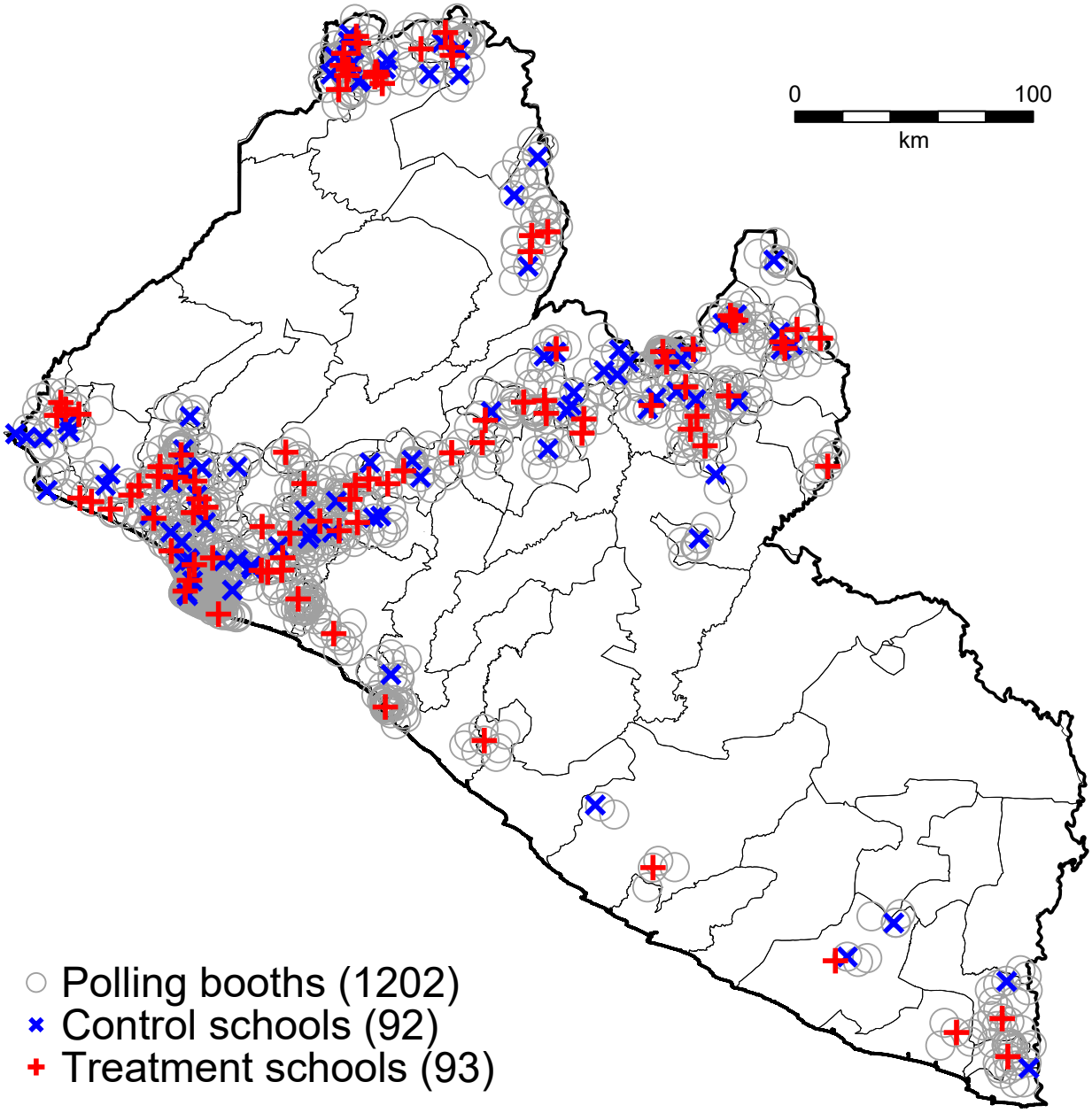
Y_{isp} represents electoral outcomes for polling booth i whose nearest treatment or control school is school s in pair p. α_p are matched-pair fixed effects (stratification dummies). $TreatIntensity_i$ is defined as the number of treated schools with 10km of booth i divided by the total number of treatment 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).[15]

[13] 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.

[14] As a robustness check, Section A.2.1 conducts the main analyses on the subset of 285 polling booths which are within 10km of exactly one RCT school. Defining the treatment variable on this sample is simpler, but this sample is also necessarily more rural and isolated, affecting external validity. This can be thought of as the extensive margin of the program. 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.

[15] There are 73 electoral districts in Liberia but only 63 with polling booths near treatment or control schools.

Figure 2: Map of treatment and control schools, and polling booths within 10km of them



Internal boundaries demarcate electoral districts corresponding to House of Representatives constituencies (73).

3.2.1 Heterogeneous treatment effects

In order to study mechanisms, I leverage variation across school pairs in the local treatment effect of the program on student learning. These specifications resemble those from the main analysis, interacting 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:

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

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 follow-up survey asked teachers about what political behaviors they had been involved in during the 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").[16]

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:

$$Y_{isp} = \alpha_p + \beta Treatment_s + \varepsilon_{isp} \tag{3}$$

Y_{isp} represents electoral outcomes for teacher i at school s in pair p. α_p are matched-pair fixed effects (stratification dummies). $Treatment_s$ is the school's assigned treatment status. Standard errors are clustered 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, using the main specification but on 2011 election outcomes (the last nationwide election before the treatment). The coefficient on treatment is not statistically significant for any of these

[16] See Romero et al. (2020) and Romero and Sandefur (2021) for more details on these teacher surveys.

outcomes. However, the point estimate on the difference in ruling party presidential first-round vote share is non-negligible. 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 (pres. 1rd)	Share invalid (pres. 1rd)	Ruling party (pres. 1st rd.)	Ruling party (pres. runoff)	Ruling party (rep.)	Share for 2017 incumbent (rep.)
Treatment intensity	0.010	0.006	-0.008	0.001	-0.023	-0.005	0.017	-0.019
	(0.011)	(0.008)	(0.008)	(0.005)	(0.017)	(0.010)	(0.028)	(0.051)
N	1200	1200	1200	1200	1200	1200	1200	1200
Mean	0.124	0.091	0.731	0.075	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 varables for whether values are missing have been included in all regressions. * $p<0.10$, ** $p<0.05$, *** $p<0.01$

4 Results

This paper's primary result is that the reform considerably reduced average vote share for the incumbent party's presidential candidate. This is an important finding, as it highlights that improvements to public services do not presumptively enhance a government's electoral prospects. But this does not mean that effective reforms cannot delivery electoral benefits. It is important to understand the drivers of any negative effect so as to illuminate routes to potential politically feasible reforms. Here, I show that voters did not oppose the policy's aims: it won more votes where it was more effective. Household survey responses corroborate this: the policy increased support for the policy itself, but decreased support for party that implemented it. The negative electoral effect was instead driven by the opposition it generated among teachers. In the absence of the treatment, 40% of teachers were politically engaged – through campaigning, staffing polling booths, etc. – and most supported the ruling party, especially those who were unionized. The reform reversed this. It wiped out union teachers' disproportionate support of the ruling party, and dramatically reduced their political engagement. This political disengagement appears to have had electoral consequences: where the policy caused most teacher disengagement, it also reduced incumbent vote share most.

4.1 The reform reduced incumbent vote share on average

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: president (1st round)		Ruling party: president (runoff)		Ruling party: legislative		Incumbent: legislative	
Treatment intensity	-0.032**	-0.029**	-0.033**	-0.027*	0.003	0.008	-0.011	-0.015
	(0.015)	(0.012)	(0.016)	(0.015)	(0.021)	(0.020)	(0.041)	(0.040)
N	1200	1200	1200	1200	1200	1200	1200	1200
Mean (control)	0.293	0.293	0.382	0.382	0.128	0.128	0.166	0.166
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 'Mean (control)' 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; 1st round 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 (UP), in both the first round and in the runoff election held a month later, by about 3 percentage points.[17] This constituted a 10% reduction in vote share in the first round (off a mean of 29.3%), and a 7% 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 – i.e., including polling booths far from schools in the RCT – 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.[18] 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). The null result here is consistent with a well-informed

[17] The correlation in this sample between ruling party presidential vote share in the first round and the runoff is 0.88.

[18] The correlation in this sample between ruling party first-round presidential vote share and ruling party legislative vote share is 0.45. This highlights how partisan attachment is not particularly strong in this context.

electorate who is aware of the executive branch's responsibility for the policy and of the legislative branch's minimal involvement. Sandholtz (2023a) shows that voters were relatively well-informed about politics in general and about attribution of the policy in particular.

The interpretation and policy implications of this negative average effect depend on what drives it. I next turn to illuminating two important potential mechanisms: voter opposition to the policy, and teacher opposition to the policy.

4.1.1 The policy won more votes where it was more effective

The simplest explanation for the negative electoral effect would be that voters simply did not agree with the policy's aims. There is precedent: in Brazil, poor citizens opposed increased education spending, preferring instead that the government devote the same resources to direct cash transfers (Bursztyn, 2016). Alternatively, perhaps voters instinctively opposed any change to the status quo. In either case, we would expect the electoral effect to be inversely related to the degree to which the policy accomplished its aims.

I test whether the negative electoral effect was driven by voters' opposition to the policy by exploiting variation in its effectiveness. The policy was designed and presented as a way to improve school quality: voters' reactions to different degrees of quality improvements can illuminate the extent to which they supported that goal.

Measuring variation in treatment effects (TE) is made possible by the matched-pair randomization design. Each pair of *ex-ante*-similar schools 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. I then test whether the treatment effect on electoral outcomes was a function of the pair-level effect on quality (proxied by test scores). 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 (see Figure 1). Because local learning treatment effects are not assigned randomly, caution is in order in interpreting these results.[19] Table 3 shows the results.

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

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

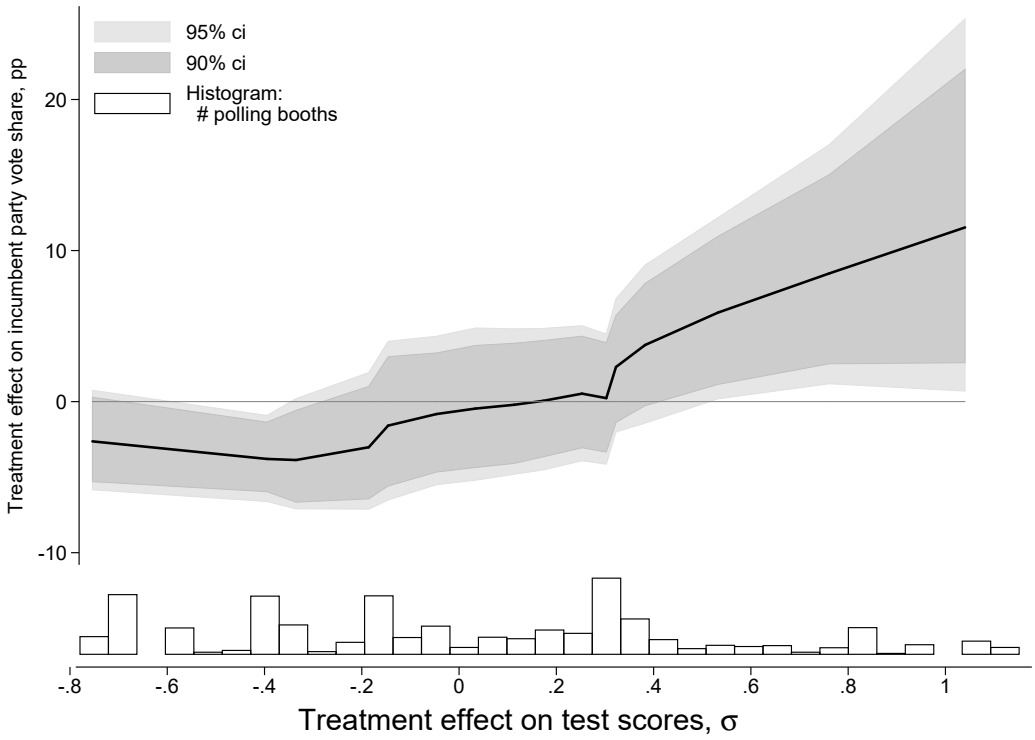
	Ruling party: president (1st round)		Ruling party: president (runoff)		Ruling party: legislative		Incumbent: legislative	
Fraction RCT schools treated	-0.056***	-0.047***	-0.059***	-0.048***	-0.015	-0.008	0.048	0.045
	(0.018)	(0.014)	(0.017)	(0.016)	(0.022)	(0.021)	(0.036)	(0.035)
Fraction RCT schools treated × TE > p50	0.077***	0.060***	0.086***	0.068**	0.060	0.053	-0.089	-0.098
	(0.021)	(0.020)	(0.025)	(0.028)	(0.052)	(0.050)	(0.073)	(0.073)
Main + interaction	0.021*	0.013	0.026	0.021	0.045	0.045	-0.041	-0.053
	(0.012)	(0.014)	(0.018)	(0.023)	(0.047)	(0.045)	(0.063)	(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 policy decreased incumbent vote share most 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 large test score increases, the electoral effect was positive. As with the average effect, the policy only affected voters' choices in the presidential race. (Table A5 performs a balance test on 2011 data, showing that pre-treatment electoral outcomes were not significantly different near treated schools from pair with high learning treatment effects.)

Figure 3 depicts a similar analysis non-parametrically, plotting mean electoral treatment effects on the y-axis against the distribution of local learning treatment effects on the x-axis.

Figure 3: Effect of PSL on responsible party's presidential vote share, by treatment effect on learning



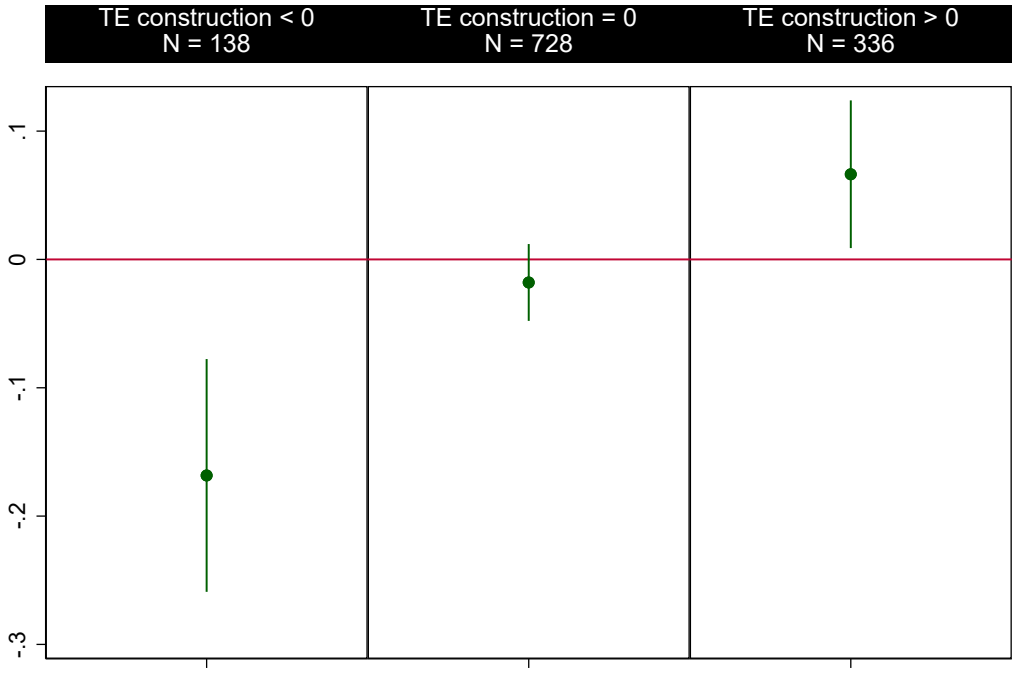
This figure plots the lowess-smoothed coefficients of the fraction of schools treated on UP presidential candidate's vote share (controlling for its 2011 pre-treatment value) in bins corresponding to 20iles of matched-pair-specific treatment effects on test scores. Constructing the bootstrapped confidence 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 the number of school pairs with each treatment effect (winsorized at 1%/99%.)

Student learning is an important dimension of school quality, but not the only one. Previous research raises the possibility that voters are more attuned to visible markers of quality such as capital investments (Harding & Stasavage, 2014; Leff-Yaffe, Nakab, & Sandholtz, 2023). Ajzenman and Durante (2023) have shown that visible school improvements can be especially salient at election time, especially when polling places are often schools. I test whether electoral effects vary by the policy's effect on capital investments, here measured with an indicator variable for whether the school experienced any construction or repairs in the last year.[20] 62% of treated schools, and 48% of control schools, had undergone any construction or repairs by this measure – the average effect of treatment on capital investments was positive and

[20]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. "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 for this variable, "above the median treatment effect" is a dummy for a positive treatment effect. NB: this variable is self-reported by principals. An alternate measure using observations of classroom quality in this analysis yields a similar and significant result.

significant. Figure 4 shows the effect of the reform on vote share for the incumbent party's presidential candidate, broken out by whether the within-pair treatment effect for the booth's nearest school was positive, negative, or zero.

Figure 4: Effect of PSL on incumbent party presidential vote share, by treatment effect on construction



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.

The policy caused electoral gains in the pairs where it increased capital investment, and electoral losses where it reduced capital investment. (It had a reasonably precise zero electoral effect where there was no difference in construction between treatment and control schools.)

Treatment raised incumbent vote share where test scores go up *and* where construction increases – and it is not the case that one is simply a proxy for the other. At the pair level (N=92), the correlation between treatment effects on learning and construction is only 0.16. Table A4 highlights this point, interacting treatment with the policy's effect on various measures of school quality: student learning, capital invest-

ments, teacher attendance, and student attendance.[21] The coefficient on the interaction between treatment and learning treatment effects is positive and significant even when including interactions with treatment effects for each of these other dimensions of school quality, as well as when including all of them in the same "kitchen sink" regression. The coefficient on the interaction term between treatment and treatment effect on capital investments is also still positive and significant in the kitchen sink regression. This suggests that the positive electoral effect of student learning operated independently of the positive electoral effect of capital investments.

4.1.2 Household survey: increased support for policy, decreased support for ruling party

Survey evidence corroborates that the policy was popular among those who experienced it, and that the policy reduced support for the incumbent government. A survey of adult members of households of treatment and control school students was conducted by phone in October 2017, mere weeks before the election. Of the 833 households for which a phone number was available, 489 (59%) consented to and completed the interview, representing 159 of the 185 schools in the RCT. Response rate was statistically identical across households from treatment and control schools. Table 4 shows the effects of treatment on these households' survey responses.

The PSL program was well-known and widely supported, especially among households from treated schools. A large majority of *control* group households had heard of the "Partnership Schools for Liberia" program, supported expanding the program, and believed children learned more in PSL schools. These majorities were around 10% larger in the treatment group. Over 90% of both treatment and control households agreed that "It is good for the government to work with private school companies to provide public education." This all suggests it was unlikely that the program reduced incumbent vote share because people thought it didn't work, or disagreed with the premise of the public-private partnership.

Despite expressing greater support for the policy, treated households were *less* likely to vote for the incumbent government's presidential candidate. Only about 60% of respondents were willing to give an answer about who they planned to vote for in the upcoming election (this fraction was not different across treatment and control). A bare majority of control respondents favored the incumbent party's candidate, but in the treatment group this fraction was 10 percentage points lower (though a bit imprecisely estimated). Nor did treatment evoke an anti-government sentiment across the board; in fact, treated households were significantly more likely to express satisfaction with their Representative's performance

[21] 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.

over the last 12 months. This reinforces the notion that treatment reduced support for the politicians responsible for the policy – and not for incumbent politicians in general – due to factors other than the policy *per se*.

Table 4: Household survey outcomes (Oct 2017)

	Heard of PSL	Support PSL	PSL ⇒ Learning	PPPs are good	Satisfied w/ legislator	Express voting pref. (pres)	Vote ruling party (pres)
PSL	0.068*	0.074**	0.088**	-0.015	0.106**	0.031	-0.098*
	(0.036)	(0.037)	(0.037)	(0.018)	(0.041)	(0.039)	(0.055)
N	489	489	489	489	489	489	304
Mean (control)	0.604	0.739	0.700	0.935	0.252	0.609	0.514

Standard errors clustered by school. School matched-pair fixed effects included.
* p<0.10, ** p<0.05, *** p<0.01

4.2 The reform antagonized teachers and disengaged them politically

In this section I present evidence that the policy antagonized teachers and disengaged them from political activity – and that this disengagement cost the government votes. Teachers can exercise political influence through their own votes or by persuading others; I show that the policy reduced their support for the government on both margins.

Teachers are well-placed to be effective electoral campaigners in Liberia. They are well-connected and integrated in the communities they serve: 30% of student households in the experiment report being related to a member of the teachers' organization or union. Further, the vast majority of teachers in the experiment live near their school. 80% report walking to work, and most have a commute time of 20 minutes or less, giving them a visible and tangible presence in the community. Teachers are also much more educated than most of their neighbors, which may lend weight to their political opinions. Virtually all teachers surveyed had at least completed senior secondary school. For adults in the households survey, that figure is 14%.

Electoral effects of the size I measure could plausibly be driven in large part by changes in teachers' political beliefs and actions. The ratio of teachers to other voters near a school tends to be quite high. To compute a conservative estimate of this ratio, I focus on the subset of polling stations which are within 10km of exactly one school in the sample, and the corresponding subset of schools. The average school in this sample had 13 teachers in 2017. The average polling station in this sample had 896 voters in the first

round of the presidential election. The average treatment effect on ruling party presidential vote share was 4.5 percentage points, which corresponds to 40 votes (see Table A1). If the treatment had caused each and every teacher to vote against the ruling party, this would make a meaningful dent in the observed electoral treatment effect, but not be sufficient to account for it completely. But the effect could be explained by treatment causing the average teacher to persuade 3.1 voters on net (potentially including herself) to vote against the ruling party. While I cannot measure how many voters were persuaded by teachers, the effect sizes I measure make it a plausible mechanism.

4.2.1 Reduced teacher satisfaction and support for government

To test how the policy affected teachers' political support, I use a survey conducted among all teachers of Math or English at treatment and control schools a few months before the election (May-June 2017), which asked teachers about their political attitudes and voting intentions. 99% of these teachers had registered to vote, and 97% planned to vote in the election. Table 5 shows the effect of the reform on teachers' job satisfaction, opinion of public-private partnerships, union membership, satisfaction with the incumbent government, and voting intentions for its presidential candidate.

Table 5: Effect of reform on teacher attitudes

				Interact w/ union	
	Job satisfaction	PPPs are good	Union member	Satisfied w/ incumbent pres.	Vote for ruling party pres. cand.
Treatment	-0.275***	0.012	-0.112***	0.020	0.016
	(0.080)	(0.025)	(0.030)	(0.039)	(0.038)
Union				0.192***	0.206***
				(0.060)	(0.063)
Treatment × Union				-0.179**	-0.185**
				(0.083)	(0.078)
Treat + Treat × Union				-0.16***	-0.17***
(SE)				(0.07)	(0.07)
N	764	764	764	764	764
Mean (control)	0.157	0.820	0.326	0.757	0.401

Standard errors clustered by school. School matched-pair fixed effects included. Outcomes from a May/June 2017 survey of teachers of Math and English at treatment and control schools. Teachers were asked to rate on a five-point scale, from 'Very satisfied' to 'Very dissatisfied,' how satisfied they were with 8 aspects of their job: current salary, job security, current posting/location, support from parents & community, support from government, working hours, and teaching input/resources. Dummies were created for whether respondents answered 'Very satisfied' to each of these questions, and those eight dummies were aggregated into a PCA index, which was then standardized to create the measure of job satisfaction in this table. 'PPPs are good' means the teacher agreed with the statement 'It is good for the government to work with private school companies to provide education.' * p<0.10, ** p<0.05, *** p<0.01

Teachers in treated schools scored much lower on an index of job satisfaction.[22] Table A7 shows that this was driven mainly by dissatisfaction over job security, job location, and working hours.

Despite the rhetoric of the leaders of the teachers' union, this dissatisfaction was not driven by opposition to the policy's broad purpose. As in the household survey results, teachers in treated and control schools were highly and equally likely (82%) to agree that "it is good for the government to work with private school companies to provide education."

However, the policy triggered a dramatic disruption of union membership at treated schools. In control schools, 33% of teachers reported belonging to the union; in treatment schools the fraction was 11 percentage points lower. This aggregates the effect of dismissing unionized teachers and hiring new non-union teachers.[23] Dismissal of teachers was a salient issue for union leadership. The reinstatement of dismissed

[22] Teachers were asked to rate on a five-point scale, from 'Very satisfied' to 'Very dissatisfied,' how satisfied they were with 8 aspects of their job: current salary, job security, current posting/location, support from parents & community, support from government, working hours, and teaching input/resources. Dummies were created for whether respondents answered 'Very satisfied' to each of these questions, and those eight dummies were aggregated into a PCA index, which was then standardized to create the measure of job satisfaction in this table.

[23] It is also possible for teachers to remain employed by the school but to leave the union – 24% of those interviewed in both 2017 and 2019 reported a different union membership status from one wave to the next. But this behavior was no different in treatment and control schools.

teachers was a focus of NTAL complaints, and at least one of the school operators was reported to have threatened unionized teachers with dismissal (Mukpo, 2017a; Brooks, 2017). Teacher turnover was indeed much higher in treated schools (Romero et al., 2020). This may indicate an effort by school operators to professionalize the teaching staff and to turn away from the patronage model of civil servant hiring common in many parts of the world.[24] However, it carried a political cost.

Among union teachers who remained in treatment schools, support for the government collapsed. Columns 4 and 5 of Table 5 show the effect of treatment on teachers' satisfaction with the incumbent president, and their intention to vote for the ruling party's presidential candidate — interacted with union membership.[25] The table shows that unionized teachers, in the absence of the policy, tended to be much more supportive of the incumbent government than non-unionized teachers. In control schools, 76% of teachers were satisfied with the incumbent president's performance, and 40% planned to vote for the ruling party's presidential candidate.[26] It is worth noting that this represents an overwhelming plurality: the next-highest category is "Don't know" at 25%, followed by "Refused" at 12% and "No party" at 6%. Among those who planned to vote for any party, 71% were for the ruling party. This highlights the vast advantage in teacher support enjoyed by the ruling party under the status quo.

This vast advantage for the ruling party under the status quo was almost completely negated by the treatment. Among union teachers, treatment reduced satisfaction and voting intentions by 16 and 17 percentage points, respectively. It had no effect on either measure among non-union teachers. Bearing in mind the negative effect on union membership, this reduction in incumbent support among union teachers could indicate that treatment antagonized union teachers, or that the strongest union supporters of the incumbent were the most likely to be dismissed and replaced, or both. In any case, the corps of union teachers supporting the government fell precipitously at treated schools.[27] This reduced support manifested itself not only in survey responses and voting intentions, but also in political action and campaigning.

[24] For studies examining the deleterious effect of patronage hiring on student learning, see Akhtari, Moreira, and Trucco (2022); Fagernäs and Pelkonen (2020); Estrada (2019); Kingdon and Muzammil (2013); Kingdon et al. (2014).

[25] Effects on job satisfaction and opinions on PPPs were not significantly different among union teachers.

[26] This measure of voting intentions is an indicator variable that takes a value of 1 if the respondent stated they planned to vote for the Unity Party's presidential candidate Joseph Boakai, and a 0 otherwise, including if they responded with "don't know" or refused. Table A8 shows that the treatment had no effect on the expression of a voting intention, and that the results look the same when limiting attention to respondents who expressed a voting intention.

[27] One potential driver of this effect is that unionized teachers were less likely to share in the benefits of the policy. Although it reduced average job satisfaction, the policy did succeed in bringing teachers onto the official government payroll. It also raised salaries significantly, leading to fewer teachers holding a second job. However, Table A8 shows that these benefits of the policy went entirely to non-union teachers. This may help explain unionized teachers' opposition to the incumbent party in treatment schools.

4.2.2 Reduced teacher political engagement

The reform sharply reduced teachers' political activity. To measure political activity, I use a follow-up survey of teachers at treatment and control schools from June-July 2019, which asked teachers about their political engagement during the 2017 election. The surveyed activities included the staffing of registration booths and/or polling stations, the encouragement of others to participate in the elections in general, and the encouragement of others to support a particular party or candidate (i.e. campaigning). Table 6 shows the average effect of the policy on teachers' reported political activities.

Table 6: 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***	-0.054**	-0.022	-0.044**	-0.103***	-0.224***
	(0.012)	(0.023)	(0.020)	(0.022)	(0.034)	(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 engaged – 40% reported participating in at least one of these political activities. The teacher attitudes from Table 5 suggest that this political action would have been mostly on behalf of the ruling party.

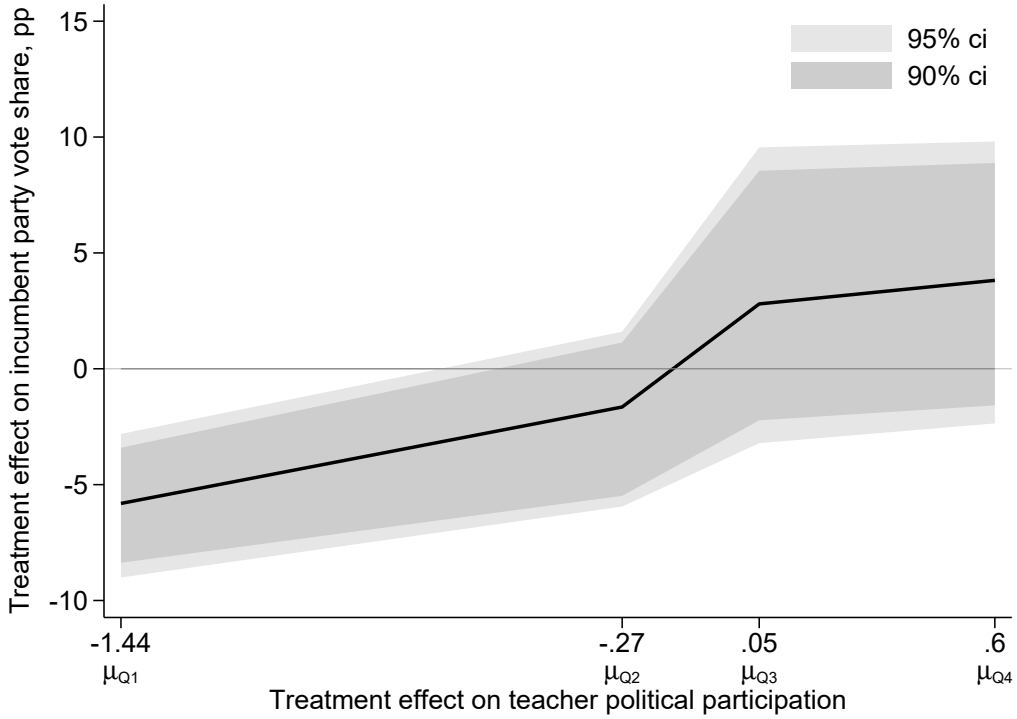
The reform significantly reduced teachers' political activity on most of these dimensions, as well as on an index aggregating them. Staffing of registration and polling booths fell by half and by a third, respectively. Campaigning fell by a quarter, as did the likelihood of performing at least one political activity on the list. An index aggregating these measures of political involvement fell by 0.22σ.[28] This effect aggregates various channels. Dissatisfaction with the policy may have sapped enthusiasm for working to reelect the government responsible for it. Treated schools also lengthened the school day and enforced stricter teacher attendance, leaving less time for political engagement. Finally, staff turnover may have left treated schools with a less politically engaged teacher base. The result of all of this was much less political activity by teachers at treated schools.

[28]For this index, the dummies for participation in each of the four political activities under consideration were aggregated into a PCA index, which was then standardized.

4.2.3 The reform lost most votes where it disengaged teachers most

The loss of teachers' political engagement appears to have significantly hampered voter mobilization: where teachers disengaged most, the ruling party lost the most votes. In Figure 5, I estimate non-parametrically the differential effect of treatment on incumbent vote share by how much the treatment affected teacher political engagement at the school pair level. I divide up school pairs into four bins based on the pair-level treatment-control difference in the aggregate index of teacher political involvement, constructing bootstrapped standard errors to show confidence intervals.

Figure 5: Effect of PSL on responsible party's presidential vote share, by treatment effect on teacher political participation



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 quartiles of matched-pair-specific treatment effects on an index of teachers' political activity. 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.

Negative treatment effects on incumbent party vote share are concentrated in the places where the treatment caused the biggest political disengagement of teachers. (Table A9 performs a similar analysis by each dimension of teacher political activity separately.) There is a positive relationship between treatment

effects on teacher political involvement and incumbent vote share. Where treatment reduced teacher political involvement, also tended to reduce incumbent vote share. This is consistent with teachers' political engagement playing an important role in voter mobilization, and in the reform's overall electoral effect — though as with Table 3, treatment effects are not randomly assigned across school pairs, so the results here should be interpreted with caution. However, Table A6 performs a balance test using outcomes from the 2011 election prior to the reform, showing that pre-treatment electoral outcomes were not significantly different near treated schools from pairs with high teacher engagement treatment effects.

5 Discussion

In this section I build on the empirical findings on where the policy won and lost votes in order to examine what could have made the reform both quality-improving and politically feasible.

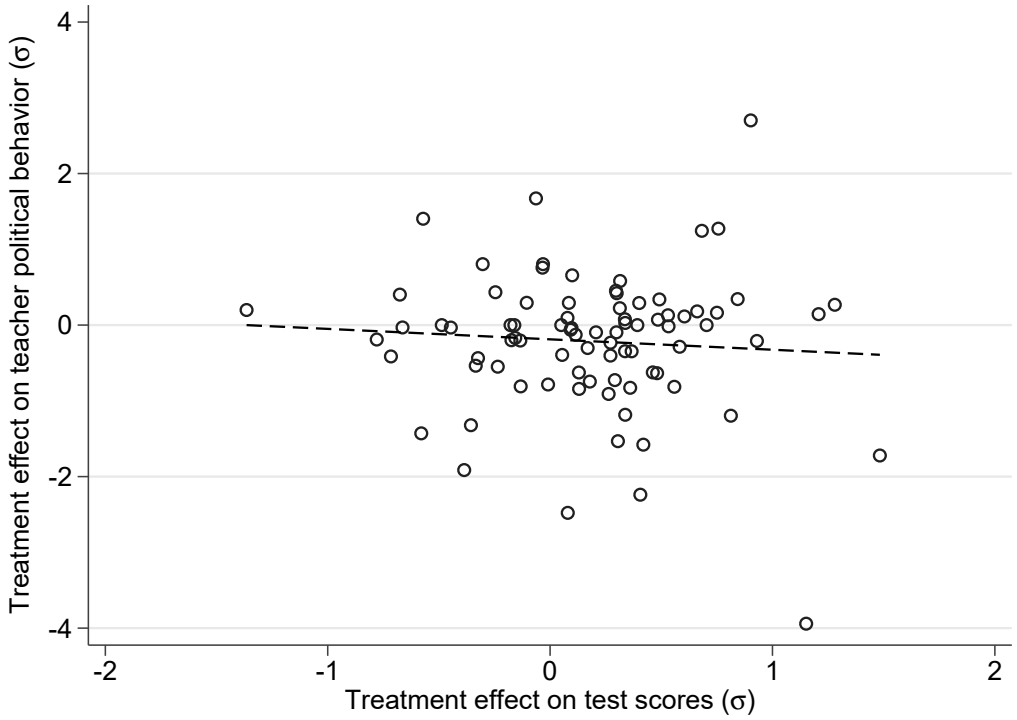
5.1 No direct tradeoff between student learning and teacher political activity

Given that the policy won more votes in places where it increased learning, but lost votes where it alienated teachers, the policy implications depend on the relationship between these two outcomes. If the elements of the policy which are necessary to increase learning are the same ones which antagonize teachers, then policymakers face an inevitable (and unenviable) tradeoff between improving student learning and mollifying politically important teachers. Such a tradeoff is suggested by descriptive work showing that the students of unionized teachers learn less, and that unions tend to oppose some classes of reform (Kingdon & Muzammil, 2013; Hoxby, 1996; Ross Schneider, 2021). However, insofar as teacher political activity is unrelated to the reform's effectiveness at increasing learning, it may be possible to improve service quality such that the political benefits outweigh the costs.

In fact, the pair-level correlation between treatment effects on student learning and teacher political activity was -0.07.[29] Figure 6 is a scatter plot of the pair-level treatment effects on student learning and an index of teacher political activity.

[29]To examine another dimension of school quality, the pair-level correlation between treatment effects on school construction/repairs and teacher political activity was positive, though also rather small, at 0.18.

Figure 6: Pair-level correlation between treatment effects on student learning and teacher political activity is small and insignificant



Scatter plot of treatment effects on student test scores and an index of teacher political behavior, measured at the level of school pairs. N = 83 pairs for which treatment effects for both variables are measurable. Correlation = -0.074

To test this relationship another way, Table A10 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. This is consistent with the electoral effect which operates through improved school quality being independent of that which operates through alienated teachers.

Table A11 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 attitudes and activity, interacting treatment with a pair-level indicator for high treatment effects on learning. Across a range of relevant teacher outcomes – union membership, government payroll status, support for the incumbent government, and involvement in political activities – treatment effects were not a function of the policy's effectiveness at raising learning outcomes.

5.2 Policy counterfactuals

The previous subsection shows that meaningful reform to improve learning is politically delicate, but not politically doomed. Voters reward better quality, and it is possible to improve quality without antagonizing influential public servants.

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

I use the estimated coefficients from the heterogeneous effects models to predict the electoral outcomes that would have resulted from counterfactual policy scenarios. I want to model the counterfactual impact of incremental changes, so I interact treatment with continuous measures of treatment effects on learning and teacher engagement (unlike in Equation 2, which interacted treatment with a dummy for high treatment effects). This entails the strong assumption of linear heterogeneous 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. 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 3. 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.

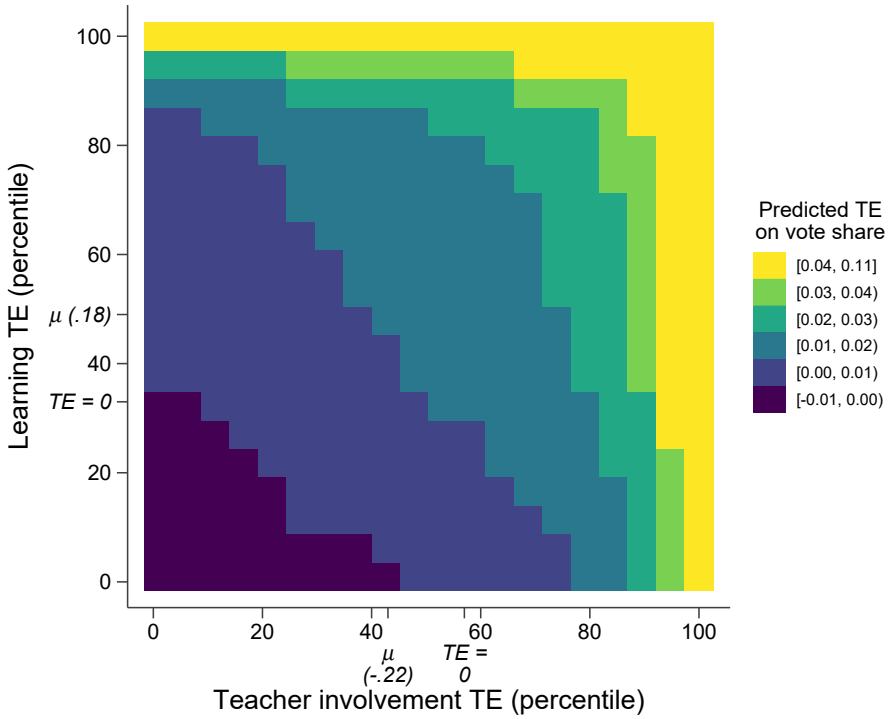
I estimate Equation 4 on the true data to recover estimated coefficients. Then I change the values of $TE_Learning_p$ and/or $TE_TeacherInvolvement_p$ iteratively, plugging in each instantiation of these counterfactual values to the equation with the true estimated coefficients to predict incumbent vote 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 floor on

learning treatment effects to the 27th percentile, and raised the floor on teacher engagement treatment effectsto the 30th percentile. This would correspond to raising the floor on pair-level treatment effects to -0.105σ and -0.436σ, respectively. The policy could also become a net vote winner by raising the lowest learning treatment effects to the 52nd percentile while holding teacher engagement constant, or by increasing the lowest teacher involvement treatment effects to the 47th percentile while holding learning constant.

Figure 7 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 pair of left-winsorized distributions, in which the floor on teacher involvement treatment effects is raised to the xth percentile and the floor on student learning effects is raised to the yth percentile.

Figure 7: 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 μ, 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 both learning and teacher involvement at least $.19\sigma$ above where they actually were. This would correspond more or less to doubling the average treatment effect on learning, and raising the average effect on teacher engagement to approximately zero. 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 0.59σ more than it actually did. These would be large changes to the average, highlighting the potential benefit to minimizing detrimental effects at the tails.

6 Conclusion

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. Many African governments in recent decades have expanded access to public services – notably education – under political pressure, or in conjunction with campaign promises (Stasavage, 2005; Habyarimana, Opalo, & Schipper, 2021; Sandholtz, 2023b).[30] And a growing body of work studies the electoral incentives for public goods such as transportation infrastructure (Goyal, 2019; Garfias, Lopez-Videla, & Sandholtz, 2021). But there is much less evidence on the electoral returns to the quality of public services (Harding & Stasavage, 2014). The question is particularly urgent in the developing world. Good public services like education play an important role in creating the conditions necessary for economic growth (Hanushek & Woessmann, 2008). Voters consistently say in surveys that they value good public services. Do they say so at the ballot box?

This paper shows that improving education quality alone is not sufficient to win votes. Overall, the policy caused a significant reduction in vote share for the candidate of the party that crafted it. But this was not due to voters' opposition to the policy *per se*. Household surveys revealed wide support for the policy, and indeed the policy won votes in places where it accomplished its aim of improving learning most successfully. Instead, the reform antagonized teachers, reducing their support for the incumbent government and leading them to exert less effort in political activity on average. This seems to have been consequential: where the policy reduced teachers political engagement most, it also lost the most votes.

This lends empirical credence to theorized explanations for policymakers' hesitancy to experiment with policy reforms (Majumdar & Mukand, 2004).[31] Policymakers who seek to improve public service delivery often face the difficult task of shaking up entrenched systems full of committed supporters. They may 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 & 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).

The paper also shows, however, that voters pay attention to service quality. In Liberia – where democracy is young and literacy is around 50% – voters exhibit some sophistication in their attribution of credit

[30] Though governments may have undemocratic reasons to expand educational access as well (Paglayan, 2020, 2022).

[31] Other work has also identified an electoral penalty for "successful" policies outside of education: improved services may at times reduce vote share by ratcheting expectations, or by freeing citizens from clientelistic relationships that require political support in exchange for services (de Kadt & Lieberman, 2017; Blattman, Emeriau, & Fiala, 2018).

and blame for an important and controversial school reform. 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. 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 suggests that reforms which are more effective at improving service quality will also be more politically feasible. More credible and targeted policy evidence could reduce policymakers' uncertainty and reduce the cost of an electoral strategy that banks on voter rewards from service delivery rather than the patronage support of interest groups.

The paper highlights the vital role of political incentive compatibility in the design of policies to improve public services.[32] The democratic tension between maintaining support by through competent governance versus distributing spoils has been shown to matter for public sector hiring (Brierley, 2021; Callen et al., 2023). The economics literature on education reform tends to highlight the apparent tradeoff between increasing learning and appeasing teacher unions (Kingdon & Muzammil, 2013; Hoxby, 1996; Ross Schneider, 2021). But reformers may do well to learn from cases in which policies have been crafted carefully to improve learning without antagonizing these important political actors (Muralidharan & Sundararaman, 2011; Leaver, Ozier, Serneels, & Zeitlin, 2021). In the end, no policy can be effective without being implementable.

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[32] 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."

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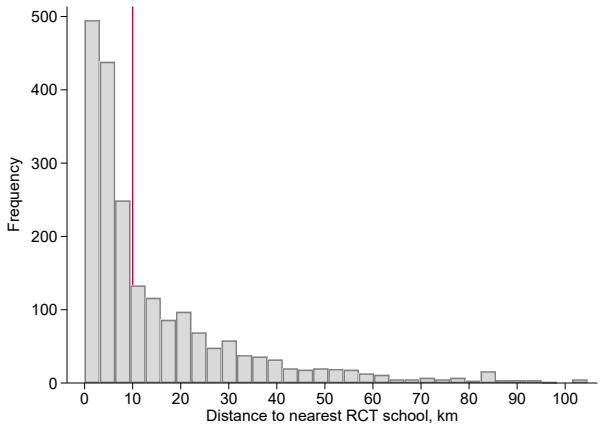
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Appendix

A Additional tables and figures

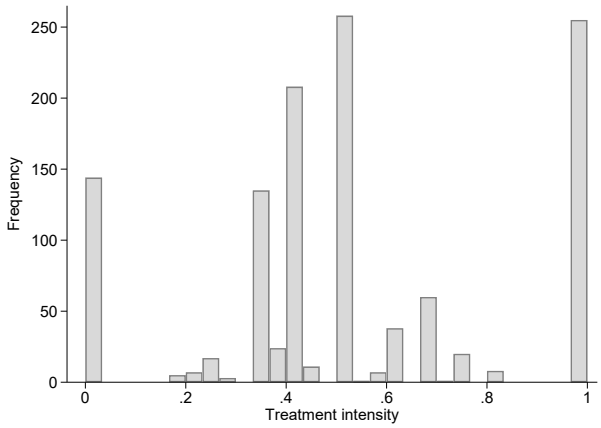
A.1 Details on definition of the treatment variable

Figure 8: Histogram of polling booths by distance to nearest treatment or control school



Universe of 2080 polling booths from 2017 general election.

Figure 9: Histogram: treatment intensity



Histogram: treatment intensity at polling booth level, for 1202 polling booths within 10km of at least one school in the RCT. Treatment intensity defined as number of treatment schools within the radius over the number of total schools in the RCT within the radius.

A.2 Robustness checks

A.2.1 Alternate treatment definition

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: president (1st round)		Ruling party: president (runoff)		Ruling party: legislative		Incumbent: legislative	
Treatment intensity	-0.041	-0.045*	-0.049**	-0.055***	-0.026	-0.027	0.106**	0.107**
	(0.028)	(0.024)	(0.023)	(0.018)	(0.035)	(0.035)	(0.050)	(0.050)
N	285	285	285	285	285	285	285	285
Mean (control)	0.310	0.310	0.402	0.402	0.149	0.149	0.157	0.157
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

A.2.2 Effect does not differ by school density or school distance

Table A2 interacts the effect of treatment with measures of school density (for all polling booths near any RCT school) and distance to the school (for polling booths near exactly one RCT school).

Table A2: Effect on 1st-round ruling party presidential vote share, by school density and distance

	Interact w/ school density		Interact w/ distance to school	
Treatment intensity	-0.080**	-0.071**	-0.066	-0.064
	(0.036)	(0.028)	(0.053)	(0.043)
School density	-0.001	-0.002		
	(0.001)	(0.001)		
Treatment intensity × School density	0.005	0.004		
	(0.003)	(0.003)		
Distance to school			-0.004	-0.002
			(0.004)	(0.004)
Treatment intensity × Distance to school			0.004	0.003
			(0.006)	(0.005)
N	1200	1200	285	285
Mean (control)	0.293	0.293	0.310	0.310
Controls		✓		✓

Standard errors clustered by electoral district. School matched-pair fixed effects included. For interaction with school density, 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. School density measured by total number of public primary schools within 10km of the polling booth. For interaction with distance to school, regressions include polling booths from 2017 election located within 10km of exactly one school in the RCT, with treatment taking the treatment status of this school. 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). * $p<0.10$, ** $p<0.05$, *** $p<0.01$

A.2.3 No effect on whether schools are polling places

Table A3 shows that treatment did not make schools more likely to be polling stations, for various measures of whether a school is a polling station. Columns 1 and 2 code a school as a polling station if the school's name shows up in the list of polling station names obtained from the National Elections Commission; Column 2 considers only exact name matches, while Column 1 allows more lenient matching. Columns 3-5 consider a school to be a polling station if its GPS coordinates are within a certain radius of the coordinates of a polling station on NEC's list.

Table A3: Treatment did not make schools polling stations

	Name	Strict name	km 0.25	km 0.10	km 0.05
(max) treatment	-0.004	-0.032	-0.018	-0.004	-0.014
	(0.062)	(0.052)	(0.064)	(0.046)	(0.036)
N	185	185	185	185	185
Mean (control)	0.261	0.185	0.272	0.098	0.065

School matched-pair fixed effects included. Outcome variable is whether the school is a polling booth (defined in columns 1-2 as whether the school shares a name and location with the polling booth, and in columns 3-5 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.2.4 Various measures of school quality

Table A4 reports results from regressing incumbent vote share on treatment, interacted with pair-level treatment effects on various measures of school quality. All columns in this table use first-round vote share for the ruling party's presidential candidate as the outcome variable. All include controls for polling-place-level electoral outcomes from the previous presidential election in 2011.

Each column reports results from a separate regression of incumbent vote share on treatment, interacting treatment with a dummy for high pair-level treatment effects on learning *and* a different dimension of school quality which voters might plausibly observe – and which was improved by the reform.[33] Column 1 interacts treatment with a dummy for whether the treatment school underwent any construction or major repairs when the control school did not. Column 2's interaction is 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 3 interacts treatment with a dummy for high treatment effects on student attendance rates. Column 4 includes all these interactions.

[33] 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.

Table A4: 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		-0.054***	-0.048***	-0.048**	-0.056**
		(0.014)	(0.016)	(0.023)	(0.028)
Fraction RCT schools treated ×	TE>p50: learning	0.049**	0.060***	0.060***	0.049**
		(0.020)	(0.019)	(0.020)	(0.020)
Fraction RCT schools treated ×	TE>p50: new construction or repairs	0.080**			0.083**
		(0.038)			(0.039)
Fraction RCT schools treated ×	TE>p50: teacher attendance		0.002		0.008
			(0.028)		(0.028)
Fraction RCT schools treated ×	TE>p50: student attendance			0.002	-0.005
				(0.030)	(0.030)
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

Interactions of treatment with pair-level treatment effects on both learning and capital investments have predictive power. This suggests that voters observe and reward learning gains independently of their perceptions and rewards of construction gains. 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. Overall, this evidence strongly suggests that voters reward improvements in school quality as measured along multiple dimensions.

A.2.5 Balance on 2011 electoral outcomes, interacting treatment with effect on student learning

Table A5: Balance, interacting treatment with learning treatment effect

	Registered voters / pop.	Votes cast / pop.	Turnout (pres. 1rd)	Share invalid (pres. 1rd)	Ruling party (pres. 1st rd.)	Ruling party (pres. runoff)	Ruling party (rep.)	Share for 2017 incumbent (rep.)
Treatment intensity	0.007	0.004	-0.001	0.003	-0.036*	-0.005	0.014	-0.002
	(0.013)	(0.009)	(0.011)	(0.005)	(0.018)	(0.011)	(0.026)	(0.069)
Treatment intensity × TE>p50: Learning	0.011	0.005	-0.022	-0.005	0.042	-0.000	0.011	-0.058
	(0.033)	(0.023)	(0.017)	(0.010)	(0.038)	(0.019)	(0.072)	(0.091)
N	1200	1200	1200	1200	1200	1200	1200	1200
Mean	0.124	0.091	0.731	0.075	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. Treatment interacted with a dummy for whether the pair-level treatment effect on student test scores was above the median. 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 varables for whether values are missing have been included in all regressions. * p<0.10, ** p<0.05, *** p<0.01

A.2.6 Balance on 2011 electoral outcomes, interacting treatment with effect on teacher engagement

Table A6: Balance, interacting treatment with learning treatment effect

		Registered voters / pop.	Votes cast / pop.	Turnout (pres. 1rd)	Share invalid (pres. 1rd)	Ruling party (pres. 1st rd.)	Ruling party (pres. runoff)	Ruling party (rep.)	Share for 2017 incumbent (rep.)
Treatment intensity		0.006	0.001	-0.015	0.006	-0.034*	-0.018	0.006	-0.024
		(0.016)	(0.011)	(0.011)	(0.005)	(0.018)	(0.011)	(0.033)	(0.076)
Treatment intensity	TE>p50: Teacher political × engagement	0.026	0.020	0.016	-0.005	0.018	0.015	-0.016	-0.003
		(0.036)	(0.024)	(0.016)	(0.012)	(0.041)	(0.017)	(0.071)	(0.109)
N		1084	1084	1084	1084	1084	1084	1084	1084
Mean		0.124	0.092	0.737	0.076	0.441	0.854	0.188	0.334

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. Treatment interacted with a dummy for whether the pair-level treatment effect on a PCA index of teacher political engagement was above the median. 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 varables for whether values are missing have been included in all regressions. * p<0.10, ** p<0.05, *** p<0.01

A.2.7 Effect on components of job satisfaction

Table A7: Effect on components of job satisfaction

	Fraction 'very' satisfied with . . .							
	Salary	Job security	Job location	Support from parents/ cmty	Support from govt	Support from principal	Working hours	Teaching inputs/ resources
Treatment	-0.017	-0.120***	-0.126***	-0.027	-0.022	-0.042	-0.356***	0.102**
	(0.024)	(0.035)	(0.034)	(0.034)	(0.031)	(0.035)	(0.034)	(0.040)
N	764	764	764	764	764	764	764	764
Mean (control)	0.131	0.513	0.607	0.315	0.195	0.693	0.809	0.431

Standard errors clustered by school. School matched-pair fixed effects included. Outcomes from a May/June 2017 survey. Teachers were asked to rate on a five-point scale, from 'Very satisfied' to 'Very dissatisfied,' how satisfied they were with 8 aspects of their job: current salary, job security, current posting/location, support from parents & community, support from government, working hours, and teaching input/resources. Dummies were created for whether respondents answered 'Very satisfied' to each of these questions. * p<0.10, ** p<0.05, *** p<0.01

A.2.8 Policy's benefits to teachers bypassed those in the union

The policy made some aspects of teachers' jobs better, but few of these benefits accrued to union teachers. Table A8 shows the effect of treatment, interacted with union status, on various aspects of teacher's jobs. Treatment raised the likelihood that teachers would be added to the official government payroll, and it raised salaries significantly. It also reduced the likelihood that teachers held a second job, an indicator that more teachers could afford to focus on their primary job. However, all of these effects were entirely concentrated among *non-union* teachers. The effect of treatment on unionized teachers, for each of these outcomes, was statistically indistinguishable from zero. This could be part of what drove union teachers' dissatisfaction with the incumbent government.

Table A8 also shows that treatment made teachers no more likely to be willing to state a voting preference, among union or non-union teachers — though union teachers were much more willing to talk about their voting preferences in general. The final column shows the policy's effect on teachers' intention to vote for the ruling party's presidential candidate, excluding those who said "don't know" or refused to answer.

Table A8: Effect of reform on teacher attitudes

	Gov payroll	Salary (USD)	Other job	Expressed voting pref.	Vote for ruling party pres. cand.
Treatment	0.153***	24.369***	-0.118***	0.047	-0.014
	(0.041)	(4.739)	(0.042)	(0.042)	(0.054)
Union	0.416***	37.120***	-0.038	0.154**	0.157**
	(0.059)	(5.972)	(0.058)	(0.064)	(0.072)
Treatment × Union	-0.163**	-27.784***	0.145**	-0.106	-0.214**
	(0.075)	(7.547)	(0.073)	(0.084)	(0.095)
Treat + Treat × Union	-0.009	-3.415	0.028	-0.059	-0.23***
(SE)	(0.06)	(6.27)	(0.06)	(0.08)	(0.08)
N	764	620	764	764	455
Mean (control)	0.539	91.669	0.236	0.633	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

A.2.9 Effect of the reform on incumbent vote share, by effect on teacher engagement

Table A9: Effect of PSL on incumbent vote share by size of treatment effect on teacher engagement

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

A.2.10 Little to no correlation between effects on student learning and teacher political activity

Table A10 shows that the positive coefficient on the interaction between treatment and learning treatment effect size remains positive and significant even when controlling for the interaction between treatment and teacher political activity treatment effect size, and vice versa.

Table A10: Electoral effect of PSL, interacted with treatment effects on learning and on teacher political engagement

	Ruling party: president (1st round)	
	(1)	(2)
Fraction RCT schools treated	-0.068***	-0.056***
	(0.023)	(0.017)
Fraction RCT schools treated × TE>p50: learning	0.088**	0.072**
	(0.036)	(0.032)
Fraction RCT schools treated × TE>p50: Teacher political activity	0.092**	0.091***
	(0.042)	(0.033)
Fraction RCT schools treated × TE>p50: learning × TE>p50: Teacher political activity	-0.089	-0.088
	(0.060)	(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 A11 shows that the treatment's effect on teacher political participation did not vary by whether the treatment caused large learning gains. In other words, there is no evidence of a direct trade-off between increasing student learning and affecting teachers' political behavior.

Table A11: 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	-0.096**	0.062	-0.023	-0.181*
	(0.043)	(0.057)	(0.050)	(0.093)
Treatment × TE>p50: learning	-0.039	0.030	-0.050	-0.010
	(0.060)	(0.075)	(0.069)	(0.127)
N	764	764	764	847
Mean (control)	0.326	0.539	0.401	0.070

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

B Conceptual framework

This section presents a conceptual framework of public service reform which accounts for the interacting incentives of voters, politicians, and bureaucrats. While many canonical models present public good provision through the lens of redistribution, the empirical literature has found that spending more money on public services often fails to move important dimensions of service quality (de Ree, Muralidharan, Pradhan, & Rogers, 2018; Mbiti et al., 2019). Changing the quality of public services may therefore depend crucially on affecting the behavior of the civil service bureaucrats responsible for providing them, through channels other than salary. In many contexts, these bureaucrats have significant political clout (Chaudhury et al., 2006). A model that takes into account politicians' dependence on the political support of bureaucrats is needed.

The framework below models the vote choices of ordinary voters and of frontline bureaucrats such as teachers. While not modeled explicitly here, these bureaucrats may be thought of as receiving efficiency wages (Shapiro & Stiglitz, 1984). Wages are higher (and/or required effort lower) than in the private sector, and the price of these rents is political work at election time. In the absence of wage increases, increasing the bureaucrats' effort on one dimension (school attendance) entails a reduction of effort in the other dimension (campaigning). To increase effort on both dimensions, wages must be raised sufficiently to compensate the lost utility from increased effort. See Baland and Robinson (2008) for an example of political efficiency wages that is similar in spirit.

B.1 Setup of the model

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. In reality, public good provision remains low (and clientelism common) even in many parts of the world with universal suffrage. I modify Lizzeri and Persico's model in a way consistent with one explanation for this fact: politicians may employ civil servants both to provide public services *and* to engage in targetable transfers and other forms of direct persuasion. For simplicity, I abstract away from redistribution here, focusing only on public good and service provision. Monetary transfers likely play a large role in politicians' competition for votes in reality, 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. I also allow the electioneering efforts of bureaucrats to influence the voting decisions of voters.

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.[34] For the purposes of this analysis, I consider the case in which I is held constant.[35] I focus instead on changes to the non-monetary inputs to the public good function captured in A. I assume that

[34] e.g. de Ree et al. (2018); Mbiti et al. (2019)

[35] 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).

$\frac{\delta g(I,A)}{\delta A} > 0$.

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.[36] 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.[37] 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 denotes 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, reli-

[36] 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.

[37] 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 require increased funding for teachers.

gious, 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.

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.[38] 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

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

Just like bureaucrats, voters behave as if they are pivotal; if they prefer a party, they vote for it. The vote share for party R among voters is therefore equal to the probability a voter votes for party R:

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

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

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

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

$$\max_A S_R$$

subject to

$$A + E = 1$$

[38] 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.

B.2 Politician's choice: what work to assign to the bureaucrat

The incumbent politician chooses how much to ask bureaucrats to work in order to maximize her likelihood of reelection.

B.3 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.4 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 $g()$ 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.