The Politics of Public Service Reform

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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 reform reduced the incumbent party's presidential vote share by 3 percentage points (9%). This does not imply that ordinary voters opposed the policy's aims: electoral effects were positively correlated with school quality effects, and household surveys showed strong support. However, the reform also reduced teachers' job satisfaction, support for the incumbent government, and political activity. The reform lost the most votes where it caused the greatest political disengagement of teachers.

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1 Introduction

The political power of public service providers is widely seen as an obstacle to quality-improving reforms (World Bank, 2004; Hoxby, 1996). Raising quality often requires not just higher spending, but institutional changes to the terms of providers' employment (Mbiti et al., 2019). When providers oppose these measures, their coordinated action can neutralize the effectiveness of implemented reforms (Bold, Kimenyi, Mwabu, Ng, & Sandefur, 2018). In theory, the threat of providers' political opposition can discourage policymakers from embarking on certain reforms in the first place (Olson, 1965). A large fraction of polling station workers in many countries are teachers and other frontline civil servants, making them electorally important (Neggers, 2018). The political role of civil servants is therefore an important constraint in the design of implementable and effective public service reforms.

However, there is limited empirical evidence on how service providers affect the political feasibility of reform. This is partly because relatively few disruptive reforms are attempted: governments tend to be hesitant to alienate providers, especially where public sector jobs are dispensed as political patronage (Akhtari, Moreira, & Trucco, 2022; Callen, Gulzar, Hasanain, Khan, & Rezaee, 2023). Where reforms are adopted, they are often allocated selectively, making causal identification of political effects difficult (Burgess, Jedwab, Miguel, Morjaria, & Padró I Miquel, 2015). Geographically granular electoral data are often unavailable, and few existing surveys collect microdata on the political activities of public service providers. As a result, it is difficult to credibly identify the average political effects of public service reforms or the mechanisms driving them (Acemoglu, 2010).

This paper measures the effect of a large randomized education reform in Liberia on the political activity of voters and teachers. Liberia offers an unusually apt setting in which to study how reform affects the politics of both users and providers of public services, for at least three reasons. First, many of the systemic problems that afflict public education systems worldwide are particularly stark in Liberia. Across the developing world, public school teacher absenteeism has been measured at 20% or higher (Chaudhury, Hammer, Kremer, Muralidharan, & Rogers, 2006). In Liberia, rates of teacher absence surpass 50%, suggesting that accountability is low. Second, Liberia's education reform was both large and randomized. Most policy reforms are implemented without variation that allows evaluation; those which are randomized are often too small-scale to permit meaningful inference about political effects. Liberia's reform was randomized at the school level, and affected 9% of the country's public primary school students in 13 of the country's 15 counties. Third, Liberia is a lively (if flawed) democracy where political

constraints matter. Since 2006, Liberia has been an electoral democracy; at the time of the reform, this described a plurality of the world's population (38%).[1] It has free and fair multiparty elections, and losers leave office. As in most low- and middle-income countries, voters in Liberia express strong preferences for improved public goods and services (Grossman & Slough, 2022). The median Liberian voter is highly likely to be affected by education policy: children aged 0-14 made up 42% of the total population in 2016, and 69% of households have at least one child under 18 (World Bank, 2015; Liberia Institute of Statistics & Geo-Information Services (LISGIS), 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 political power of teachers (whose union vocally opposed the policy), the scheme also prohibited operators from employing non-government teachers. Romero, Sandefur, and Sandholtz (2020) show that the reform reduced pupil-teacher ratios and increased teacher attendance, teacher time-on-task, student enrollment, student attendance, and student test scores. The reform also increased teacher turnover. Overall, however, the program increased the satisfaction of students and parents alike.

I exploit the random assignment of the policy to measure its causal effect on electoral outcomes and on teachers' political mobilization. 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 compare the political attitudes and activities of teachers in treatment and control schools using a survey conducted after the election.

My main finding is that the reform significantly reduced the ruling party's vote share. Its presidential candidate – a member of the incumbent presidential administration which created and implemented the policy – received 7% fewer votes in polling booths near treatment schools compared to control schools (3 percentage points from a base of 38 percentage points). There was no effect on turnout, suggesting the electoral effect operated through persuasion. There was also no effect on vote share for legislative candidates — who had no responsibility for the policy — consistent with a correct attribution on the part of the voters.

I also show that the reform eroded teachers' job satisfaction and support for the ruling party. This is

[1] According to the V-Dem (Varieties of Democracy) Institute's "Regimes of the World (RoW)" classification, which includes 4 categories: closed autocracy, electoral autocracy, electoral democracy, liberal democracy (Coppedge et al., 2024).

important because under the status quo ante, teachers were among the government's strongest supporters. However, the treatment reduced their job satisfaction by 0.18σ, driven primarily by frustrations with working hours and job location. In the absence of the policy, teachers were significantly more likely than the general public to support the incumbent government: in control schools, 63% of teachers planned to vote for the ruling party. The reform lowered this fraction by 8 percentage points (12%). It also decreased teachers' participation in election work. In control schools, 40% of teachers worked in electoral activities such as operating polling booths and registration booths, or campaigning for a party or candidate. The treatment reduced these mobilization efforts by 25% (10 percentage points).

I show suggestive evidence that the mechanism driving the negative electoral effects was the opposition of teachers, not ordinary voters. I test this using variation in treatment-control differences across the *ex-ante* matched school pairs used as randomization strata, measuring how pair-level electoral treatment effects correlate with treatment effects on 1) teacher political mobilization and 2) school quality.[2] Teacher disgruntlement appears to have affected other voters' behavior: the reform lost the most votes where it demobilized teachers most. The vote multiplier necessary to explain all electoral effects through teacher persuasion is 12.4 persuaded voters per teacher, or 1 out of every 273 voters near their school.

On the other hand, effects on vote share are increasing in effects on school quality, whether measured by student test scores, capital investments, or one controlling for the other. Household survey data also shows that the policy increased support for itself (from a high base). This suggests that although the effect of teacher opposition was dominant overall, voters broadly appreciated the policy and rewarded the government where it worked sufficiently well, consistent with the predictions of retrospective voting theory (Ferejohn, 1986; Key, 1966).

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 C). Intuitively, higher-quality services raise voters' support for the incumbent government, but disaffected 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.

Could an alternative version of this policy have won votes on net? Alienating teachers does not seem to have been a necessary condition for improving school quality: the correlation between pair-level

[2]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. 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.

treatment effects on student learning and teacher mobilization is near zero. 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 pair-level treatment effects on learning and on teacher political activity had been at what was in fact the 25th percentile, the net effect on incumbent vote share would have been positive (see Section 6).

The paper's primary contribution is to link empirically the causal electoral effects of a public service reform to its effect on the political mobilization of service providers. Researchers studying the scale-up of effective policies in education have long acknowledged the importance of political economy constraints, but evidence has been largely qualitative (A. Banerjee et al., 2017). Existing literature documents teacher' political organization and capacity to shape policy, but does not directly demonstrate how these groups affect electoral outcomes (Bold et al., 2018; Larreguy, Montiel Olea, & Querubin, 2017; Ross Schneider, 2021).

The paper also contributes to the broader literature on the political economy of public service provision by identifying the electoral effects of a change to public service quality. Some existing work examines the electoral effects of increased spending on public services (Bursztyn, 2016; Litschig & Morrison, 2013; Golden & Min, 2013). But the correlation between spending and service quality is weak (World Bank, 2004). The evidence on electoral effects of broad-based growth-promoting public goods and services *per se* – especially education – is much more sparse, despite abundant empirical evidence of externalities which warrant government intervention (Bau, 2022; Andrabi, Bau, Das, Karachiwalla, & Khwaja, in press).[3] Cook, Kogan, Lavertu, and Peskowitz (2020) show that charter school openings affected school board election turnout in Ohio (USA), while Dias and Ferraz (2019); Firpo, Pieri, and Souza (2017), and Cox, Eyzaguirre, Gallego, and García (2023) show that information about school performance influenced electoral outcomes in Brazil and Chile, respectively.

Finally, the paper underscores political feasibility as a policy design constraint worthy of researchers' attention. It is well-known that political economy constraints frequently hinder the adoption of "first-best" trade and environmental policies (Autor, Dorn, Hanson, & Majlesi, 2020; Che, Lu, Pierce, Schott, & Tao, 2016; Ostry, Furceri, & Ganslmeier, 2021). My results show that they are also a first-order concern for public service reform. Policy alterations which are politically necessary for scale-up should be included

[3]Related work measures the electoral effects of cash transfers (Vicente & Wantchekon, 2009; Manacorda, Miguel, & Vigorito, 2011) or large visible infrastructure projects (Voigtlaender & Voth, 2019; Goyal, 2024; Butler & Boudot Reddy, 2023; Harding & Stasavage, 2014; Leff-Yaffe, Nakab, & Sandholtz, 2023). The link between public services and other indices of civic participation has also received some attention: tax collection can increase political participation (Weigel, 2020) and public services can raise tax compliance (Krause, 2020; Sandholtz & Vicente, 2024). See Hartmann and Sandholtz (2023) for a review of the nascent empirical literature on the electoral effects of public good and service provision.

in cost/benefit analyses, where they might affect both the numerator and the denominator (Hendren & Sprung-Keyser, 2020). My paper provides empirical evidence consistent with theoretical explanations for status-quo bias and pandering by politicians (Fernandez & Rodrik, 1991; Maskin & Tirole, 2004), and challenges the notion that electoral pressure can improve public service delivery through the "long route of political accountability" (World Bank, 2004).

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 examines mechanisms; Section 6 discusses policy counterfactuals; and Section 7 concludes.

2 Context

Liberia offers a natural context in which to study the politics of education reform. Its public school system faces many of the same challenges that are common across the developing world, including teacher absenteeism (Chaudhury et al., 2006). 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, including in education. The Ministry of Education's efforts to remove ghost teachers from the payroll were short-lived, while teacher absenteeism was endemic (Rosenberg, 2016; 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 reform

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.[4] The operators were given responsibility for (though not ownership of) the resources the government normally uses to provide education – schools, classrooms, materials, and teachers – as well as for the daily management of the schools, with the understanding that the government could hold them accountable for results. The operators were very heterogeneous: some were for-profit chains backed by high-profile Western investors, while others were non-profit NGOs; some were based in Liberia and some were based in other countries (Edwards, 2017).

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 geography and administrative data on school resources; 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. Operators were required to deliver the Liberian national curriculum, but allowed to supplement it with remedial programs, longer school days, and non-academic activities. 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 55-65 students depending on the operator), unlike public schools. PSL teachers were also required to be government-employed civil servants, unlike in PPP schemes in other countries which sometimes allow operators to hire contract teachers. This placed some limitations on operators' ability to hold teachers accountable. 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. However,

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

no formal mechanisms to hold operators accountable were created in the policy's first year of operation.

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 by the executive branch, and Liberia has no elected local politicians, so the incumbent president's party could clearly and credibly claim credit.[5] 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 randomization of the policy permits researchers to measure its overall effectiveness, a rarity for most implemented policy. In addition, the matched-pair randomization allows the measurement of variation in policy effectiveness, as each treatment school has a valid counterfactual school with which to compare. 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.[6] 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). Detractors argue that PPPs represent an abdication of one of the state's core responsibilities (United Nations, 2016). This reform offers a chance to test whether governments can win votes through radical attempts to improve public services, even via unconventional means such as outsourcing service delivery to the private sector.

2.2 The reform's effects on learning

Romero et al. (2020) provide a comprehensive picture of the program's effect on education outcomes after one year; 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. This is a large effect: within the context of the literature on education interventions in the developing world, this falls at the 70th percentile (Evans & Yuan, 2022). Other dimensions of school quality also went up: teacher attendance by 50%, teacher time-on-task by 43%, and satisfaction of both students and parents by about

[5] 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.

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

10%.[7]

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, and some practices which would be unsustainable in a scaled-up version of the program were permitted. For example, one operator chose to enforce the class size caps permitted by the contract. This forced hundreds of students to find a new school. The same operator successfully requested that the Ministry reassign 74% of its teachers, creating large teacher turnover and likely imposing negative externalities for the broader system.

The matched-pair randomization permits an unbiased estimate of treatment effects for each school 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 an 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 B4, demonstrating the degree of variation in treatment effects experienced school-by-school.

2.3 Politics in Liberia

Liberia is a young democracy. After decades of civil war, 2005 saw the country hold a fully democratic national election for the first time in its history.[8] The 2005 election, as well as subsequent general elections in 2011, 2017, and 2023, were deemed broadly free and fair by international observers. Both the 2017 and 2023 elections featured peaceful transitions of presidential power from one party to another. However, electoral politics are limited to the federal level and power is centralized: local government heads are appointed by the president. As a result, Liberia is categorized by the V-Dem (Varieties of Democracy) Institute's "Regimes of the World (RoW)" measure as an "electoral democracy."[9] Another democracy

[7] 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.

[8] For 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).

[9] This classification denotes "de-facto free and fair multiparty elections and a minimum level of Dahl's institutional prerequisites for polyarchy as measured by V- Dem's Electoral Democracy Index (v2x_polyarchy), but either access to justice, or transparent law enforcement, or liberal principles of respect for personal liberties, rule of law, and judicial as well as legislative constraints on the

index classified Liberia in 2017 as a "hybrid regime" – just below Tanzania and Bangladesh, and just above Nepal and Kenya (Economist Intelligence Unit, 2022).

The constitutional political structure is modeled on the United States' system, with some important differences. Voters elect a president – who is head of 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. Counties are further divided into 73 congressional constituencies, each of which is represented by one member of the House of Representatives. 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.

One reason that policy might be expected to have electoral consequences is that Liberian politics do not feature strong parties with consistent ideologies or entrenched policy aims. This could in theory make voters more persuadable by policy changes. According to close observers of the political system, 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 general 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).

Another reason education policy could be politically salient is that school expenses are a large focus of patronage spending. Patronage is important at all levels of Liberian politics: the presidency of Ellen Johnson Sirleaf (2005-2017) has been described as "carefully based upon Liberian patronage networks

executive not satisfied as measured by V-Dem's Liberal Component Index (v2x_liberal)" (Coppedge et al., 2024).

. . . in an intricate and omnipotent network of big men and followers" (Bøås & Utas, 2014). At the legislative level, the three-time House election winner Zoe Pennue of Grand Gedeh County 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 2017 election 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.[10]

2.3.1 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). Sirleaf chose to respect the constitutional limits preventing her from seeking a third six-year term. 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 he won the congressional endorsement race: 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 not assigned to a particular polling station based on where they live; instead they 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.

[10]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.

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 creation, 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 and press coverage 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). At least one member of the House of Representatives went on record condemning Bridge's operations in his constituency in 2018 (Front Page Africa, 2018)

2.5 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 are among the only members of the community who have an education and a wage-earning job. This confers on teachers leadership and economic influence, with people looking to them 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.[11] Comparative data from across sub-Saharan Africa support the notion that teaching is a lucrative and respected job in Liberia: average monthly earnings are 3.1× GDP

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

per capita (compared with 3.2× GDP per capita in sub-Saharan Africa overall). Teachers in 2016 made up about 1.5% of the labor force, similar to in peer countries, and teacher-pupil ratios were about 4%, slightly higher than in peer countries. 85% of teachers are male (vs 61% across sub-Saharan Africa) and their average age is 45 (vs. 38 across sub-Saharan Africa) (Evans, Yuan, & Filmer, 2022).

Politicians appear cognizant of the value of teacher support. During the 2017 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).

Evidence from around the world shows that teachers' political clout is important both for elections and for policy. In many places, 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 et al., 2017; Pierskalla & Sacks, 2019). This empowerment has allowed teacher unions, and their links to political machines, to succeed in derailing education reforms in other countries (Finger, 2018; Ross Schneider, 2021; Bruns, Macdonald, & Schneider, 2019).

2.5.1 The response of teacher unions to the reform

Liberia's largest 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 highly visible 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 Education 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 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 estimates of the reform's electoral effect leverage the randomization of the PSL program and use administrative voting data from the October 2017 general election (and December 2017 runoff election) as outcomes. The estimates of teachers' political attitudes and behavior are 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.[12] There were 2,080 polling booths in Liberia in the 2017 election. 637 votes were cast in the median booth in the first-round presidential election; 428 in the runoff. Electoral data at the voting booth level, as well as booth GPS coordinates, were obtained from the National Elections Commission (NEC) of Liberia.

Because the main outcome (voting data) and the treatment (the PSL school reform) are administered at different geographical levels, outcome data must be assigned a treatment status through some mapping method. Fundamentally, any method represents a tradeoff between sample size and possible contamination: restricting the sample to polling booths near only one type of school limits the sample considerably, but including booths near both kinds of schools may introduce contamination which biases results downward. A similar tradeoff is involved in deciding how wide of a radius to use to define which booths are "near" a school. A small radius limits the sample size; a wider radius may include booths which are more weakly treated.

In my main specification, I define each polling booth's treatment value as the number of treated schools with 10km of the booth, divided by the total number of treatment and control schools within the same radius.[13] This permits the inclusion of over half of the polling booths in the country, while also incorporating the intuition that the intensity with which voters experience the treatment is a function of the share of nearby RCT schools which were randomized into treatment. The 10km radius is wide enough to embrace at least one polling booth for all 185 schools and 92 school pairs in the RCT.[14] A majority of booths (58%) lie within 10km of an RCT school, and 97% of students in the RCT live within 10km of their school (Figure A1 shows a histogram of all polling booths by their distance to the nearest RCT school). Section A.1 shows that results are robust to other radii and treatment definitions.

Figure 1 maps all 2,080 polling booths in Liberia alongside the 185 schools from the RCT. Blue X's indicate control schools; red crosses indicate treatment schools. The 1,202 polling booths within 10km of a treatment or control school are displayed as 10km-wide circles, colored according to treatment status:

[12] In the jargon of the Liberian National Election Commission, a voting site is called a "precinct." Precincts in this context are not defined as geographically-bounded polygons (as in some other countries), 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 across polling places within precincts. A precinct is what I refer to as a "voting booth" or a "polling booth" in this paper according to more conventional usage.

[13] 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.

[14] 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.

the 144 unambiguous control booths are blue, the 255 unambiguous treatment booths are red, and the rest are varying shades of purple according to the relative density of treatment vs. control school within 10km of them. 55% of these 1,202 polling booths have a treatment status of 0, 1, or 0.5 (see Figure A2 for a histogram). The 878 polling booths which are more than 10km away from the nearest treatment or control school are displayed as small gray dots.

3.2 Specification: electoral outcomes

I estimate the treatment effect of the reform using the following specification:

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

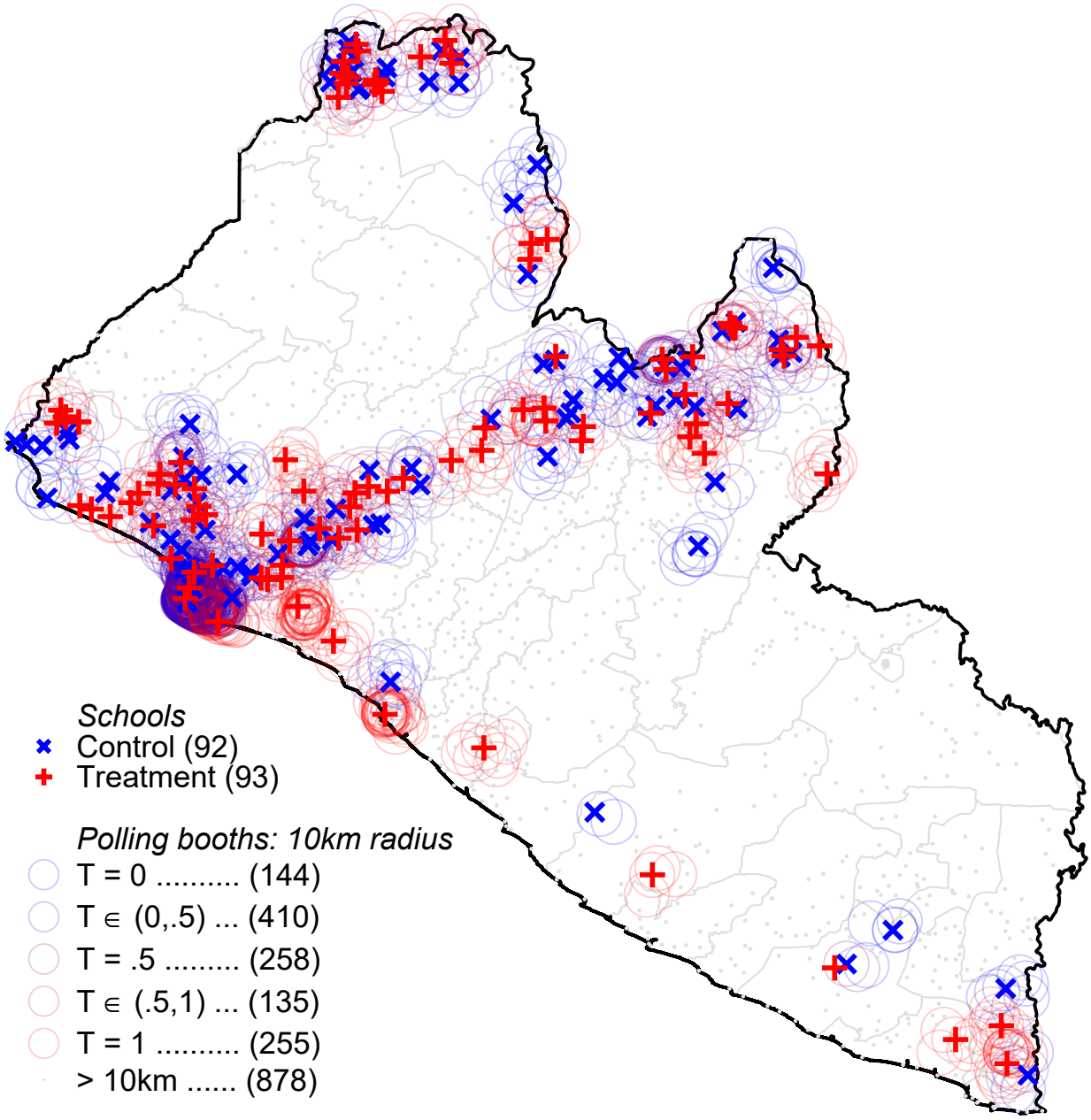
Y_{isp} represents electoral outcomes for polling booth i whose nearest treatment or control school is school s in pair p. α_p are matched-pair fixed effects (stratification dummies). $Treatment_i$ is defined as above. X_i are booth-level controls included in some specifications, which consist of 2011 election outcomes: registered voters, votes cast, and 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]

3.3 Teacher survey data

All teachers of Math and English in treatment and control schools were surveyed in May/June 2017; a follow-up survey was conducted with all teachers at these schools in June/July 2019. In addition to their teaching practices and job satisfaction, 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").

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

Figure 1: Map of treatment and control schools and polling booths



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

3.4 Teacher specification

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 straightforward:

$$Y_{isp} = \alpha_p + \beta Treatment_s + \varepsilon_{isp} \quad (2)$$

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.

3.5 Balance

Table 1 checks balance, using the main specification on 2011 election outcomes (the last general election before the treatment). The outcomes of interest are vote share for the ruling party candidate in both rounds of the presidential election; vote share for the ruling party candidate and for the local incumbent party's candidate in the legislative election (which was held concurrently with the first round of the presidential election); and turnout. The coefficient on the treatment variable is not statistically significant for any of these variables. Section A shows that the main results are robust to including controls for 2011 election outcomes.

Because teachers were only surveyed after the reform's implementation had begun, I am not able to check baseline balance for teacher outcomes.

Table 1: Balance: Prior election outcomes (2011)

	President: runoff		President: First round		Legislative		
	Vote share: Ruling party	Turnout	Vote share: Ruling party	Turnout	Vote share: Ruling party	Vote share: Incumbent	Turnout
Treatment	-0.005	-0.001	-0.023	-0.008	0.017	-0.019	-0.008
	(0.010)	(0.016)	(0.017)	(0.008)	(0.028)	(0.051)	(0.008)
N	1200	1200	1200	1200	1200	1200	1200
Mean (control)	0.842	0.421	0.429	0.731	0.179	0.337	0.724

Standard errors clustered by electoral district. Regressions include polling booths located within 10km of any school in the RCT, with treatment defined as fraction of these schools assigned to the PSL treatment. School matched-pair fixed effects included for the booth's nearest 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 'Mean (control)' displays the mean of the dependent variable for polling booths with Treatment = 0. * p<0.10, ** p<0.05, *** p<0.01

4 Results

In this section, I begin by showing that the reform considerably reduced average vote share for the incumbent party's presidential candidate. I then demonstrate that the policy reduced teachers' job satisfaction, support for the incumbent government, and engagement in election work.

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 2017 general election.

Table 2: Average school policy effects on vote share

	President: runoff		President: First round		Legislative		
	Vote share: Ruling party	Turnout	Vote share: Ruling party	Turnout	Vote share: Ruling party	Vote share: Incumbent	Turnout
Treatment	-0.033**	0.010	-0.032**	-0.000	0.003	-0.011	0.002
	(0.016)	(0.015)	(0.015)	(0.009)	(0.021)	(0.041)	(0.008)
N	1200	1200	1200	1200	1200	1200	1200
Mean (control)	0.382	0.519	0.293	0.775	0.128	0.166	0.762

Standard errors clustered by electoral district. 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 matched-pair fixed effects included for the booth's nearest 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 'Mean (control)' displays the mean of the dependent variable for polling booths with Treatment = 0. * p<0.10, ** p<0.05, *** p<0.01

The school reform reduced average vote share for the presidential candidate from the incumbent Unity Party (UP), in both the first round and in the runoff election, by about 3 percentage points.[16] This constituted a 9% reduction in the runoff (off a mean of 38.2%), and an 11% reduction in vote share in the first round (off a mean of 29.3%). (The vote share for the UP's candidate Joseph Boakai in polling booths which are not part of the analysis sample – i.e., further than 10km from the nearest RCT school – was 20.4% in the first round and 26.5% in the runoff – significantly lower than in booths near either treatment or control schools in the analysis sample.) The policy had a reasonably precisely estimated zero effect on turnout.[17] While it is technically possible that the policy changed the composition of who chose to turn out in a way that did not affect the overall number of voters, this serves as highly suggestive evidence that the vote

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

[17] This finding stands in contrast to Cook et al. (2020), who find that that the introduction of charter schools reduced voter turnout in school board elections in Ohio, USA. That study's context is quite different; in Liberia, school policy is almost completely centralized in the national executive branch, while in the US most powers over school policy are devolved to the local level.

share effects came through the margin of persuading voters to vote for a different candidate, rather than the margin of affecting the turnout of decided supporters of a particular candidate or candidates.[18]

The policy had no statistically significant effect on vote share or turnout in legislative races, either for ruling party legislative candidates or for incumbent legislators in general.[19] 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 electorate who is aware of the executive branch's responsibility for the policy and of the legislative branch's minimal involvement. [20]

Table A4 shows that including 2011 electoral variables as controls does not meaningfully change the results.

4.2 The reform alienated teachers and disengaged them politically

In this section I show that the policy antagonized teachers by reducing their job satisfaction, and that it decreased their political support for the ruling party and disengaged them from political activity. Teachers can exercise political influence through their own votes or by persuading others; I present evidence that the policy reduced their support for the government on both margins.

4.2.1 Under the status quo, teachers support the ruling party

To contextualize the policy's decimation of teacher support for the government, it is important to understand the strength of their connection under the status quo. In this section I characterize teachers' relationship to the incumbent government in the absence of the policy, using descriptive data.

Teacher survey data confirms that teachers are well-placed to be effective vectors of persuasion in Liberia. They are significantly more educated than the average member of their communities. Virtually all teachers surveyed had at least completed senior secondary school; for adult household members of students in the experiment, that figure is 14%. This may lend weight to teachers' opinions in the community.

[18] For another example of a randomized policy's non-effect on turnout being interpreted as consistent with voter persuasion, see López-Moctezuma, Wantchekon, Rubenson, Fujiwara, and Pe Lero (2022).

[19] 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.

[20] This didn't stop legislative candidates from attempting to benefit electorally from the policy, however. Sandholtz (2023a) shows that randomly providing information about the policy's effectiveness to legislative candidates changed their campaign behavior and electoral outcomes, and that voters' support was responsive to randomly provided information about legislative candidates' positions regarding the policy.

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. The vast majority of teachers in the experiment live near their school, giving them a visible and tangible presence in the community. 80% report walking to work, and most have a commute time of 20 minutes or less.

Teachers are also deeply involved in the execution of elections. 40% of those in control schools participated in the 2017 election via staffing registration or polling booths, campaigning, or generally encouraging other to participate. This kind of low-level face-to-face interaction with voters has the potential to be electorally consequential. In India, the identity of polling booth staffers – a large fraction of whom are teachers – has effects on vote share large enough to swing elections (Neggers, 2018).

Ex ante, teachers are much more likely than the general public to support the ruling party. In May/June of 2017, 71% of teachers in control schools said they planned to vote for the presidential candidate from the incumbent Unity Party in the upcoming presidential election.[21] Contrast this with the 51% of adults in the households of students in the RCT schools (from an October phone survey just before the election), or the 29% Boakai got in the first-round election. Histograms of school-level averages of ruling party voting intentions, separately for households and teachers, are shown in Figure 2a.

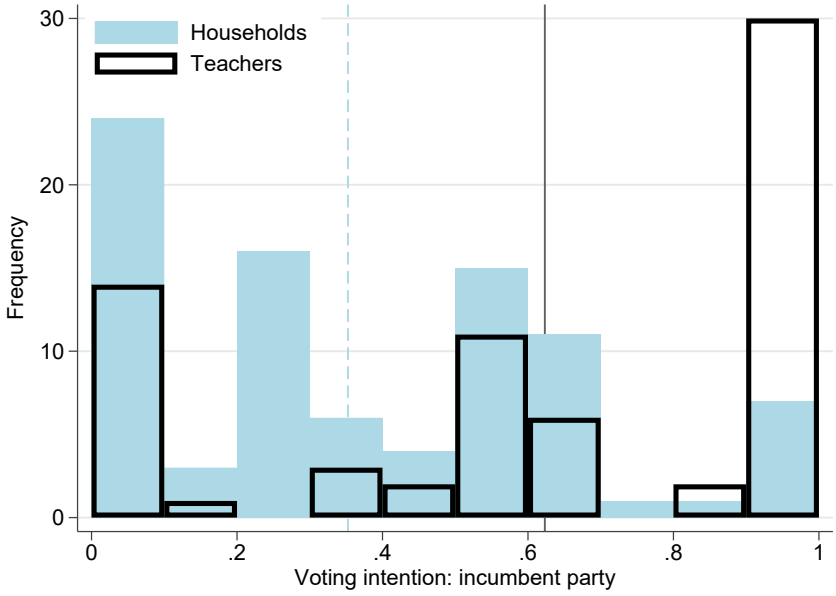
Another suggestive indicator of teachers' support for the ruling party – and possibly their powers of electoral persuasion – is that vote share for the ruling party is higher at polling booths located at schools than at other polling booths ($\beta = .043, p < .001$).[22] A significant difference persists even when including electoral district fixed effects ($\beta = .013, p = .028$). Figure 2b plots the distribution of incumbent party vote share in the runoff election separately for polling booths which are located at a school vs. not.[23]

[21] This figure includes only those who expressed a voting preference (not "Don't know"). Even if the "don't know"s are all coded as not supporting the UP, 63% of teachers said they planned to vote for the UP.

[22] A little over half of polling booths are located at a school, and about 50 RCT schools are polling booths. Treatment had no effect on whether a school was a polling booth, and the effects of treatment on electoral outcomes were not different at schools which were polling booths. It may be the case that some of the treatment effects on teachers' political behavior are driven by schools which are polling stations, but the effects are noisy. See Section B.1 for more details.

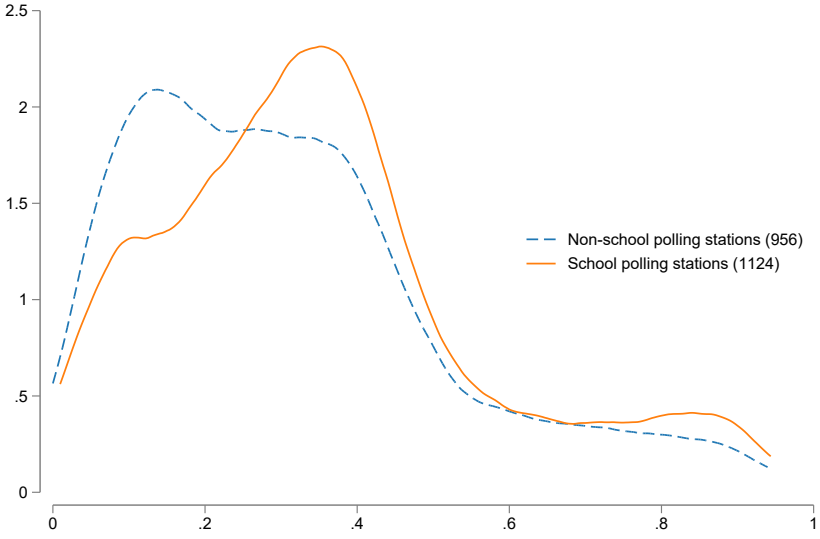
[23] School polling booths identified by whether the word "school" appears in the polling booth's name on official NEC records.

Figure 2: Descriptive data: high teacher support for ruling party in the status quo



(a) Histogram: voting intentions for ruling party candidate, teacher and household survey

Histogram showing unweighted school-level averages of indicator variable for whether teachers and household members of students plan to vote for Unity Party presidential candidate (excludes don't know / refuse). From a May/June 2017 survey. Includes control schools only.



(b) Distribution of ruling party vote share in school vs. non-school polling stations

This figure depicts kdensity plots of raw data of incumbent party vote share in the runoff election at the level of the polling station, separately by whether polling booths are located at schools. Includes all 2,080 polling stations.

This pattern is similar to that which Larreguy et al. (2017) identify in Mexico as consistent with the role of teachers as political brokers.

4.2.2 The reform reduced teacher satisfaction and support for the ruling party

To test how the policy affected teachers' attitudes and 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 3 shows the effect of the reform on teachers' job satisfaction, hours, wage, on-time salary receipt, opinion of public-private partnerships, and voting intentions for its presidential candidate.

Table 3: Teachers' job behavior and attitudes

	Job satisfaction	Hours per day	Effective hourly wage (USD)	Salary rec'd on time (# months)	PPPs are good	Vote for ruling party pres. cand.
Treatment	-0.172**	1.110***	-1.526***	-0.645**	0.012	-0.077*
	(0.079)	(0.338)	(0.450)	(0.313)	(0.025)	(0.045)
N	764	764	612	618	764	455
Mean (control)	0.096	6.814	3.289	3.983	0.820	0.633

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' or 'somewhat 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. 'Hours per day' is the sum of hours teachers reported spending on the most recent school day on teaching regular and extra classes, correcting homework and tests, preparing, and spending other time with students. 'Salary rec'd' is the teachers' report of how many months of the school year they received their salary on time. 'Effective hourly wage' imputed from multiplying daily time spent by 20 (work days per month), then dividing by self-reported monthly salary. '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.' Missings replaced with zeros, and dummies for missing included, in all regressions except for 'Vote for ruling party pres. cand.' * $p<0.10$, ** $p<0.05$, *** $p<0.01$

Teachers in treated schools scored much lower on an index of job satisfaction.[24] Table B10 shows that this was driven mainly by dissatisfaction over job security, job location, and working hours. They were also required to work significantly more hours per day (as measured by a survey question which asked

[24]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.

teachers to report how they had spent their time on the previous day in which they were at schools). This was almost entirely driven by an increase in reported time spent teaching (see Table B11). Teachers' pay did not rise commensurately with their time spent working, and their effective hourly wage plummeted. Teachers also became less likely to report receiving their salary on time.

In contrast to the rhetoric of the leaders of the teachers' union, teachers' dissatisfaction does not seem to have been driven by opposition to the policy's broad purpose. 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" ("PPPs are good" in the table).

The treatment also diminished the ruling party's support among teachers as measured by their stated voting intentions, which fell 7.7 percentage points from a base of 63.3%. (Because voting intentions are somewhat sensitive, not everyone chose to answer this question, but response rates were not different by treatment status – see Table B12.)

4.2.3 The reform reduced teacher political engagement

In addition to souring teachers' stated support for the ruling party, the reform aslo 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 4 shows the average effect of the policy on teachers' reported political activities.

Table 4: Teachers' political engagement

	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 3 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.[25] 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 this was much less political activity by teachers at treated schools.

4.2.4 The role of unions

While it is undeniable that teachers play a crucial role in Liberian politics, this role does not appear to be strongly mediated through the organization of the teachers union – at least not along the political dimensions I am able to measure. Looking at the survey from 2019, membership is far from universal: only 27% of surveyed teachers reported being union members. Union membership is associated with some apparent benefits: members tend to be older and to have higher salaries than non-union teachers. 64% were "very" or "somewhat" satisfied with the union, though they scored no differently on an index of overall job satisfaction. Communication is imperfect: 69% reported knowing their union representative's name, but only 15% had their union rep's phone number. When asked to rate (on a four-point scale) how much political power the union had, fully 91% of teachers said "little power" or "no power" (the other choices were "some power" and "a lot of power"). Strikingly, this fraction was almost identical for teachers in and out of the union.

The teacher survey also gives little evidence that the union is very successful in coordinating the political actions of its members, or even of communicating its positions to members. Only 6% of teachers reported that anyone at all from the school had asked them to support a candidate or party in the 2017 election; in only two cases did a request come from a union official. When asked about the union's level of support for the PSL policy, only 3% of teachers correctly answered that the union had opposed the policy ("strongly" or "somewhat"). Union members were a bit more likely than other teachers to say, correctly,

[25] 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.

that the union had opposed the policy. But they were also much more likely to assert, incorrectly, that the union had supported the policy.

Nor does there appear to be a strong correlation between union membership and political activity. In the pre-election survey, union members were not differentially supportive of the incumbent party. In the post-election survey, union members were not differentially likely to report participating in any one of the four electoral activities measures (operating registration booths, operating polling booths, encouraging general participation, campaigning) or the aggregated index.

While teacher unions undoubtedly play an important role in Liberian civil society, most teacher political behavior in the 2017 elections seems to have been more or less independent of the union itself. This is in fact consistent with how teacher political power has been found to operate even in some of the places most associated with teacher union politicking. Larreguy et al. (2017) show that the largest teacher union in Latin America, Mexico's SNTE, draws its effectiveness not from a highly organized system of monitoring and incentives, but from the partisan attachment of teachers themselves. See Section B.2 for more discussion of the reform affected union teachers.

5 Mechanisms

In this section I study whether the reform's average electoral effects were likely to have been driven primarily by the persuasive efforts of teachers, or by ordinary voters' direct electoral response to the policy (consistent with retrospective voting theory).

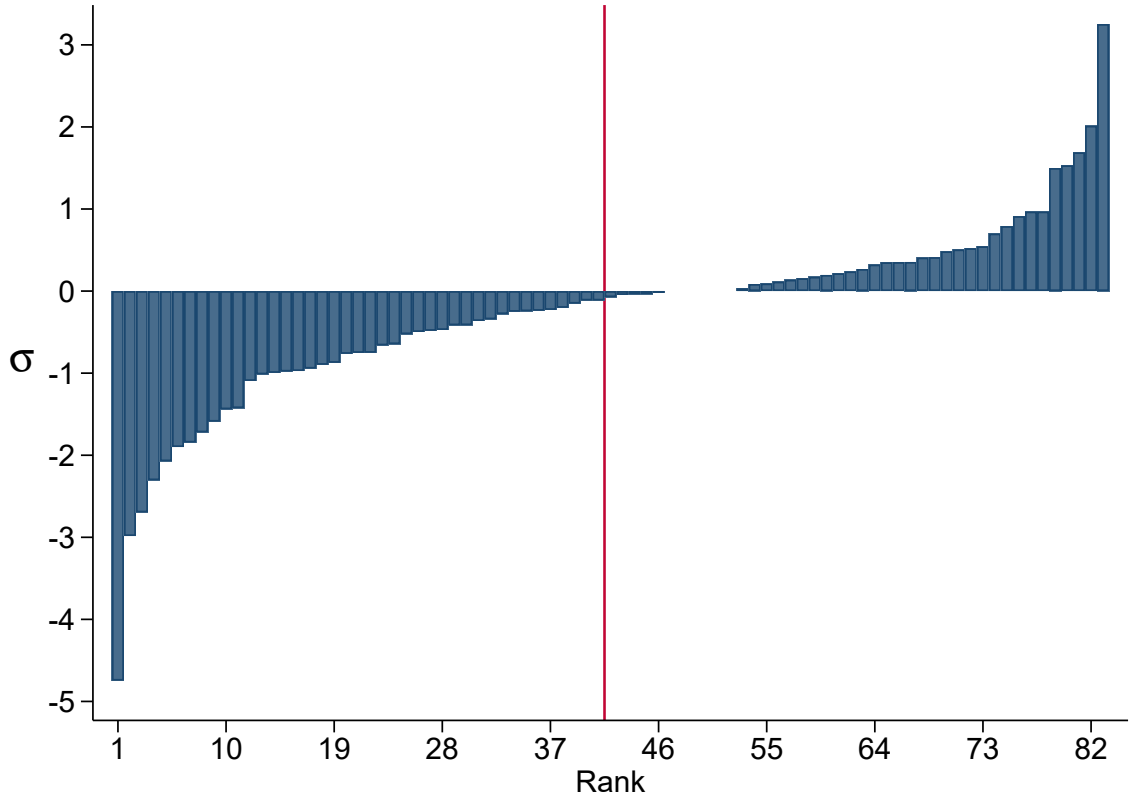
5.1 Mechanism: persuasive teachers

Did teacher disenchantment and disengagement cause the reduced incumbent vote share effects? Answering this question authoritatively would require an independent source of random variation in teacher attitudes and behaviors. Such variation is not available. What was randomly assigned was the school policy, and effects on both votes and teachers were downstream of that. However, the structure of the randomization protocol permits an analysis which provides useful suggestive evidence.

I compare how the school-pair-level treatment effect on voting varied with the school-pair-level treatment effect (TE) on teacher outcomes. The policy consisted fundamentally of private management of public schools, but implementation varied dramatically across the eight different private operators which implemented the policy. Because treatment was randomly assigned within pairs of ex-ante similar schools,

the treatment-control difference in each pair is an unbiased (albeit noisy) estimate of the local treatment effect in that pair of schools. These pair-level differences vary considerably: see Figure 3

Figure 3: Heterogeneity in treatment effect on teacher political engagement



Each bar shows the treatment-control difference in an index of teachers' political involvement, measured in standard deviations, of one of the 83 school pairs in the experiment at the end of the policy's first school year for which it was possible to survey teachers. The median pair's difference was -0.07σ. 37% of pairs had a positive difference.

In order to test whether the treatment had different electoral effects in places where it affected teachers differently, I interact the treatment dummy with a dummy for whether the nearby schools exhibited low vs. high teacher treatment effects at the pair level. These pair-level treatment effects are not themselves randomly assigned across school pairs, and so cannot be interpreted causally.[26] However, they do not correlate strongly with other pre-treatment variables. A lasso analysis sought to predict teacher political behavior effects using measures of baseline school inputs, baseline electoral outcomes, and indicators for school operator and county.[27] Of all these variables, the only one selected by the lasso was the within-pair

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

[27]Specifically, the lasso considered the sum, mean, and absolute value difference between the schools in the pair, for the following

difference in pumped water at the school. The results I present here are robust to the inclusion of this variable as a control.

5.1.1 Specification: mechanisms

Equation 3 displays the estimation specification I use to leverage variation across school pairs in the local treatment effect of the program on teachers' political behavior. This specification resembles that of the main analysis, with the addition of an interaction of the treatment variable with an indicator for whether the local treatment effect in the pair corresponding to the polling booth's nearest school was above the median:

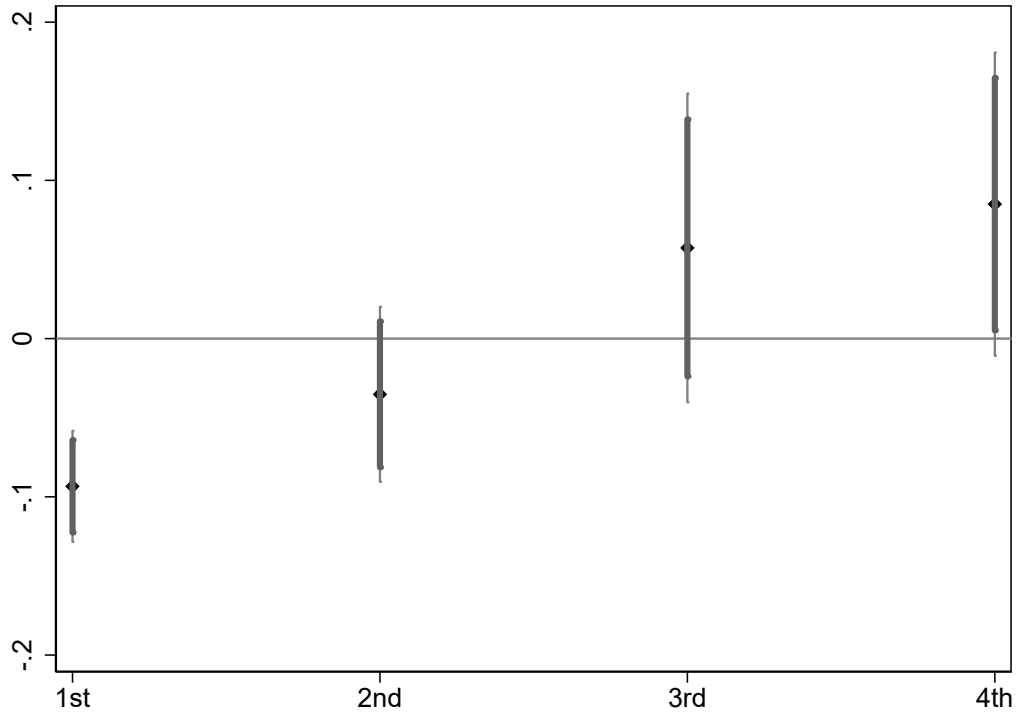
$$\begin{aligned} Y_{isp} = \alpha_p + \beta_1 Treatment_i + \beta_2 \mathbb{1}(TE_p > p50) \\ + \beta_3 Treatment_i \times \mathbb{1}(TE_p > p50) + \gamma X_i + \varepsilon_{isp} \end{aligned} \tag{3}$$

5.1.2 The reform lost most votes where it disengaged teachers most

The pattern of results is consistent with teachers' political disengagement hampering voter support for the incumbent: where teachers disengaged most, the ruling party lost the most votes. Table B15 shows for both measures of presidential incumbent party vote share a positive coefficient on the interaction of treatment with the dummy for high treatment effects on the teacher engagement index. (Table B16 performs a similar analysis by each dimension of teacher political activity separately.) This means that the reform caused a reduction in vote share in proportion to how much of a reduction in teachers' political engagement it caused. In Figure 4, I plot the differential effect of treatment on incumbent vote share by how much the treatment affected teacher political engagement, dividing school pairs into quartiles of pair-level teacher engagement treatment effects.

measures: teachers, classrooms, chairs, benches, chalkboards, and books per student; whether the school had a solid building, a generator, a toilet, a staff room, a generator, and water from a pipe, pump, or well; the number of students; the amount of money received from the ministry; the number of qualified teachers; and the sum of registered voters, total votes, and ruling party votes in the first and second round presidential elections of 2011, at all polling stations which were nearer to that RCT school than any other.

Figure 4: Effect on ruling party vote share, by quartile of treatment effect on teacher political engagement



This figure plots the coefficients from the regression of the ruling party's presidential candidate's vote share on treatment (with 90% and 95% confidence intervals), by quartile of matched-pair-level treatment effects on an index of teachers' political activity.

Negative treatment effects on incumbent party vote share are concentrated in the places where the treatment caused the biggest political disengagement of teachers. There is a positive relationship between treatment effects on teacher political involvement and incumbent vote share. Where the reform reduced teachers' political involvement, it also tended to reduce incumbent vote share. This is consistent with teachers' political engagement playing an important role in voter persuasion, and in the reform's overall electoral effect — though as with Table 5, 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 on the interaction specification in Equation 3, using outcomes from the 2011 election prior to the reform. It shows that pre-treatment electoral variables were not significantly different near treated schools from pairs with high teacher engagement treatment effects.

5.1.3 What size of teacher vote multiplier would explain the electoral effects?

While the electoral effects I measure are too big to be entirely explained by teachers' own votes, they could plausibly be due to teachers' persuasive efforts even if their "electoral multiplier" is of a reasonable magnitude. To compute a clear estimate of this multiplier, I focus on the subset of polling stations which are within 10km of exactly one school in the sample: 285 polling stations near 52 schools, with an average of 5.2 nearby polling stations per school (Section A.1 shows that the electoral effects of the policy in this subsample have a similar magnitude and precision as those estimated on the full analysis sample).[28] The average school in this subsample had 13 teachers in 2017. The average booth in this subsample had 651 voters in the presidential election, and the booth-level average treatment effect on the number of votes for the ruling party's candidate was -31 votes (-4.9% $\times$ 651 voters – see Table A2). Multiplying these booth-level figures by 5.2 booths per school yields 3385.2 total voters per school and a treatment effect of -161.2 votes at the school level.

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. The effect could be fully explained, however, by the treatment causing the average teacher to persuade 12.4 voters on net (potentially including herself) to vote against the ruling party. That amounts to persuading 0.4% of the 3385.2 total voters near the average school, or one out of every 273.

5.2 Mechanism: retrospective voting

Another possible mechanism for the negative electoral effects of the policy – potentially coexisiting with the mechanism of teacher persuasion – is that voters simply did not agree with the policy's aims and punished the government for it. 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). It is similarly well-documented that voters may oppose government spending on groups they don't belong to, whether defined by age, caste, or ethnicity; perhaps one of these dimensions of identity predisposed the median Liberian voter to oppose the implementation of the school reform (Poterba, 1997; Mukand & Rodrik, 2018; Posner, 2005). Alternatively, it is possible that voters instinctively opposed any change to the status quo.

[28] Considering the 285 schools which formed part of a matched pair.

5.2.1 Variation in policy effectiveness

In any of the above cases, we would expect the reform's 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 school quality treatment effects (TE) is once again 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. Figure B4 plots the distribution of pair-level treatment effects on student learning.

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 3) 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. As before, since local learning treatment effects are not assigned randomly, caution is in order when interpreting these results. But Table A5 performs a balance test on 2011 data, showing at least that the pair-level learning treatment effects are not correlated with pre-existing patterns in electoral variables. Table A5 performs a balance test on 2011 data. As expected, it shows that 2011 election variables are not significantly different by treatment status or according to the pattern of high learning treatment effects.

5.2.2 The policy won more votes where it was more effective

Table 5 shows the estimates.

Table 5: Effects on 2017 vote share, interacted with learning treatment effect

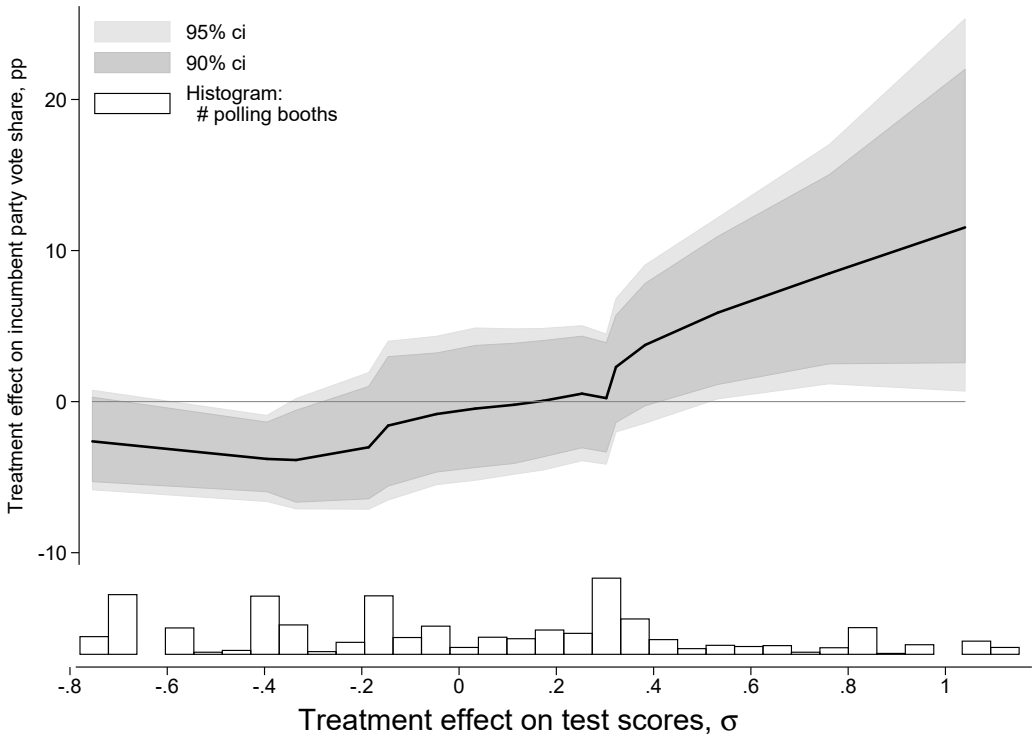
	President: runoff		President: First round		Legislative		
	Vote share: Ruling party	Turnout	Vote share: Ruling party	Turnout	Vote share: Ruling party	Vote share: Incumbent	Turnout
Treatment	-0.059***	0.011	-0.056***	0.008	-0.015	0.010	0.013
	(0.017)	(0.020)	(0.018)	(0.012)	(0.022)	(0.051)	(0.011)
TE>p50: Treatment × Learning	0.086***	-0.004	0.077***	-0.028	0.060	-0.069	-0.037**
	(0.025)	(0.025)	(0.021)	(0.017)	(0.052)	(0.084)	(0.016)
N	1200	1200	1200	1200	1200	1200	1200
Mean (control)	0.382	0.519	0.293	0.775	0.128	0.166	0.762

Standard errors clustered by electoral district. Nearest school matched-pair fixed effects included. Regressions include polling booths 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. * 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.

Figure 5 depicts a similar analysis non-parametrically, plotting mean electoral treatment effects (on first-round presidential vote share) on the y-axis against the distribution of local learning treatment effects on the x-axis.

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



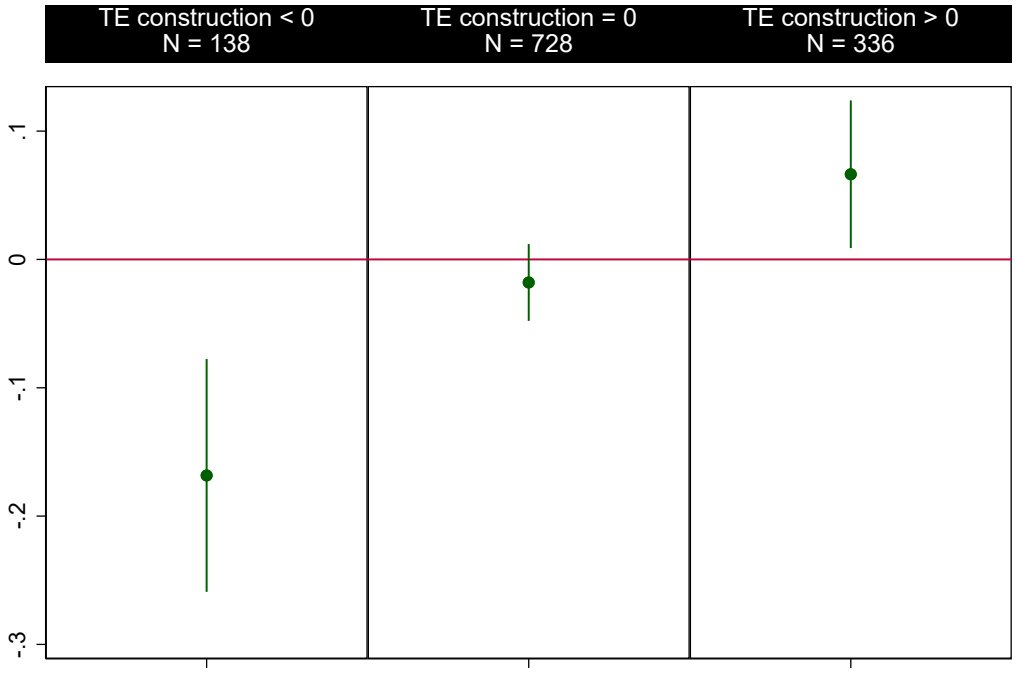
This figure plots the lowess-smoothed coefficients of the fraction of schools treated on ruling party presidential candidate's first-round vote share (controlling for its 2011 pre-treatment value) in bins corresponding to 20iles of matched-pair-specific treatment effects on test scores. Constructing 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 et al., 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.[29] 62% of treated schools, and 48% of control schools, had undergone any construction or repairs by this measure – the average effect of the reform on capital investments was positive and significant.

[29]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.

Figure 6 shows the effect of the reform on first-round 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 6: 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 first-round 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 B14 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.[30] 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.

5.2.3 Household survey: the reform was popular, but it decreased support for ruling party

Survey evidence corroborates the conclusion that the policy was popular among those who experienced it, and that it 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 6 shows the effects of treatment on these households' survey responses.

Table 6: 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

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 still in the treatment group. Over 90% of both treatment and control

[30] 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.

households agreed that "It is good for the government to work with private school companies to provide public education." This all suggests the program on its own terms was popular, even among those who did not receive it, but especially among those who did. It is true that this sample of households is selected in the sense that they all have a child in primary school and may be particularly supportive of education spending. But recall that Liberia is one of the world's youngest countries, and 69% of households have a child under 18 at home: the median voter in Liberia is likely a parent of primary school aged children. This all makes it unlikely that the reform reduced incumbent party vote share because voters disagreed with its premise, or even because they thought it didn't work.

And yet, despite expressing greater support for the policy, treated households were *less* likely to say they planned 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). This reduced support is not indicative of some wider anti-government sentiment evoked by the reform: treated households were significantly more likely to express satisfaction with their Representative's performance over the last 12 months.

These household survey results shore up the idea that the reform reduced incumbent vote share in spite of ordinary voters' general support of it and its policy aims, not because they opposed it. This implies that the reform's electoral effects came through a channel separate from direct policy feedback from voters, and is consistent with the driving mechanism being the persuasive efforts of teachers (or lack thereof). Table B19 regresses electoral impacts on interacted dummies indicating above-median pair-level treatment effects for 9 different outcomes relating to teachers, schools, and students. The results indicate that the most predictive effect of the policy for electoral outcomes was its effect on teacher political activity.

6 Discussion

This section builds on the empirical findings about where the policy won and lost votes to examine what might have made the reform both quality-improving and politically feasible.

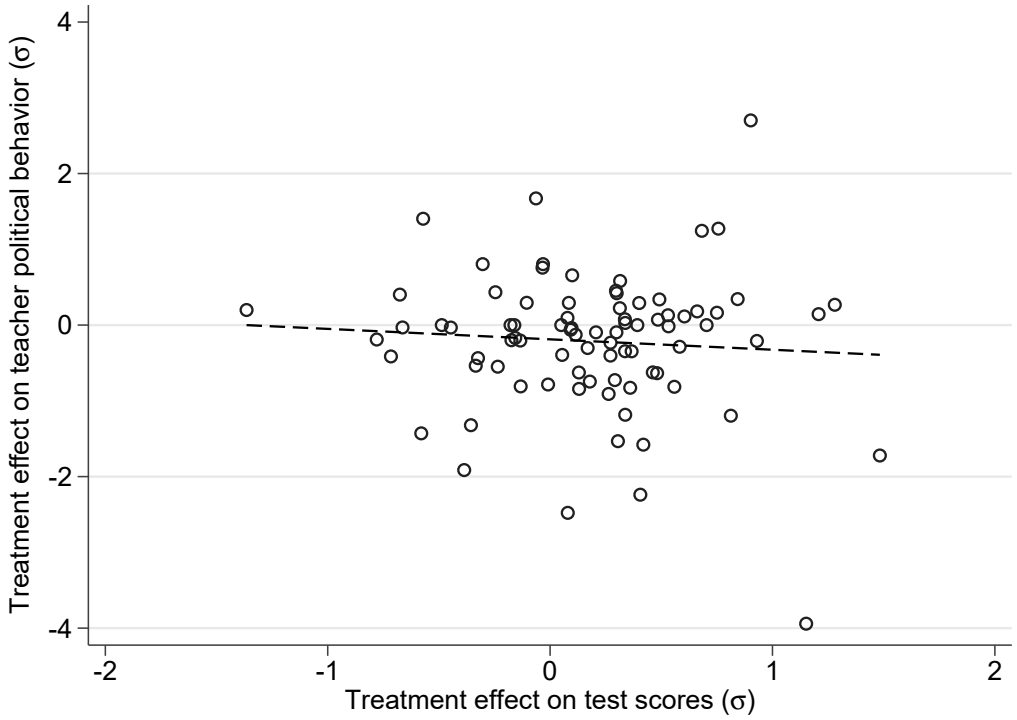
6.1 No direct tradeoff between student learning and teacher political activity

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 just -0.07.[31] Figure 7 is a scatter plot of the pair-level treatment effects on student learning and an index of teacher political activity.

[31] 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 7: 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 B17 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 idea that the electoral effect which operates through improved school quality is independent of electoral effect which operates through alienated teachers.

Table B18 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 outcome variables – union membership, government payroll status, support for the incumbent government, and involvement in political activities – treatment effects of the policy on these outcomes were not a function of the policy's effectiveness at raising learning

outcomes.

6.2 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. As it turned out, in the case of Liberia's reform, the positive effect of school quality on incumbent electoral outcomes was not sufficient to overcome the negative effect of disgruntled teachers.

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

I use estimated coefficients like those from the heterogeneous effects models in Equation 3 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 3, 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 a parsimonious set of controls $\mathbf{X}_i$: 2011 measures of registered voters, total votes cast, and first-round ruling party vote share. 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.

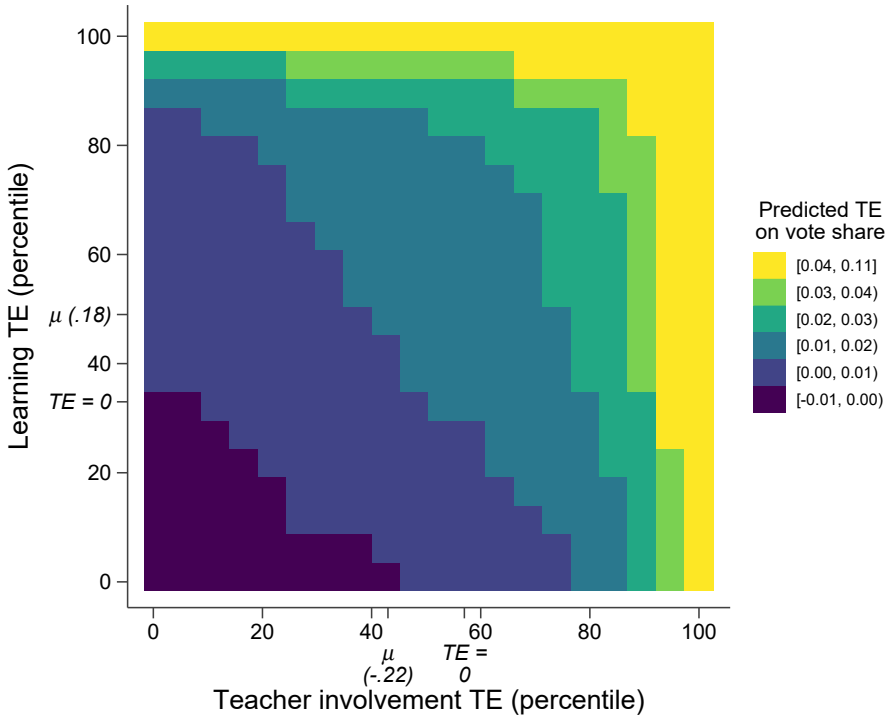
I estimate Equation 4 on the true data to recover estimated coefficients. Then I change the values of

$TE_Learning_p$ 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 8 summarizes predicted vote share for counterfactual distributions of treatment effects on student learning (y-axis) and teacher political involvement (x-axis). The baseline model prediction – using the true distributions of treatment effects – is in the bottom left corner. Each (x, y) coordinate represents the predicted vote share from a counterfactual 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 8: Predicted counterfactual treatment effect on vote share, raising the floor on learning and teacher treatment effects



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

7 Conclusion

Understanding how to make effective public service reforms politically feasible and scalable is a fundamental question in development. 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).[32] And a growing body of work studies the electoral incentives for visible public goods such as transportation infrastructure (Goyal, 2024; Garfias, Lopez-Videla, & Sandholtz, 2021). But there is much less evidence on how voters – and public services providers – respond to interventions aimed at raising the quality of public services such as education (Harding & Stasavage, 2014; Biasi & Sandholtz, 2024). Public services providers constitute a large group of relatively well-educated and motivated people, whose significant organizational power can grind service delivery to a halt, often with devastating consequences (A. V. Banerjee, Duflo, & Glennerster, 2008; Gruber & Kleiner, 2012). The question of how to design reforms that are incentive-compatible for providers is therefore urgent, perhaps particularly so 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. Can improving public service delivery be incentive compatible for politicians?

This paper shows that improving education quality alone is not sufficient to win votes. Overall, Liberia's public-private school partnership 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 the handful of places where was most successful at improving school quality. But the reform antagonized a crucial constituency – teachers – who withdrew their support for the incumbent government and disengaged from electoral work. This seems to have been consequential: where the policy reduced teachers' political engagement most, it also lost the most votes.

This evidence lends empirical credence to theorized explanations for policymakers' hesitancy to experiment with policy reforms (Majumdar & Mukand, 2004).[33] 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

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

[33] 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).

(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 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 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, *ceteris paribus*, also be more politically feasible. More credible and targeted policy evidence could reduce policymakers' uncertainty, thereby lessening the cost of switching from an electoral strategy based on the patronage of interest groups to one based on broad voter demand for service delivery.

The paper highlights the vital role of political incentive compatibility in the design of policies to improve public services.[34] 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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Appendix

A Robustness

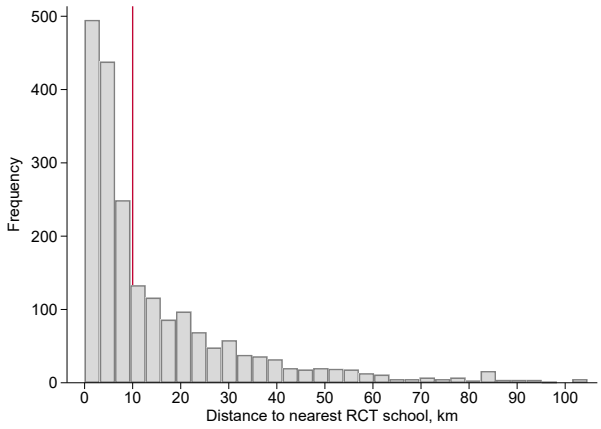
A.1 Alternate treatment definitions

As mentioned in Section 3, estimating the effect of the school reform on electoral outcomes entails mapping the electoral outcome data to the treatment assignment of "nearby" schools. In this section I describe three different ways to do this, varying for each method the radius considered "nearby."[35] Across all methods and most radii, the main results are in line with those reported in Section 4.

A.1.1 Fraction treated

The main specifications employed in the paper for estimating the effect of the reform on electoral outcomes define treatment as the fraction of RCT schools within a 10km radius which are treated. 97% of students in the RCT live within 10km of their school. Figure A1 shows a histogram of all polling booths in the country, by distance to the nearest RCT school, with a red vertical line indicating the 10km mark. A majority of polling booths (58%) fall within a 10km radius of an RCT school, with a steep decline in the number of polling booths as distance increases beyond that point. All schools are within 10km of at least one polling booth.

Figure A1: Histogram of polling booths by distance to nearest treatment or control school

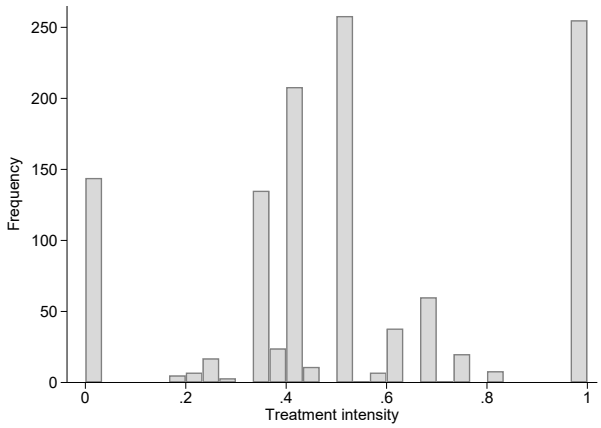


Universe of 2080 polling booths from 2017 general election.

[35] A possible alternative scheme which I do not consider might define treatment as the number of treated schools divided by the total number of schools (RCT or not) within the radius. I avoid this scheme as it would confound treatment status with overall school density.

Under this definition, a majority of polling booths have a treatment status of 0, 1, or 0.5. This is clear in Figure A2, which shows a histogram of this definition of the treatment variable for the 1202 polling booths which are within 10km of at least one school in the RCT.

Figure A2: Histogram: treatment intensity



Histogram: "Fraction Treated" or "treatment intensity" definition 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.

However, this "Fraction Treated" construction of the treatment variable could also be constructed for other radii. Table A1 estimates the results on the main outcome (presidential vote share in the runoff election) using this variable definition under various radii.

Table A1: Treatment definition: fraction treated

	President: runoff			
FractionTreated	-0.031	-0.033**	-0.029*	-0.039
	(0.025)	(0.016)	(0.017)	(0.030)
N	1396	1200	772	311
Mean (control)	0.382	0.382	0.377	0.386
Spec	Fraction	Fraction	Fraction	Fraction
Radius (km)	15	10	5	2

Standard errors clustered by electoral district. School matched-pair fixed effects included for the booth's nearest 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 'Mean (control)' displays the mean of the dependent variable for polling booths with Treatment = 0. * p<0.10, ** p<0.05, *** p<0.01

The estimates lose significance at smaller radii, perhaps because smaller radii imply smaller samples.

The increased noisiness could also indicate the presence of spillovers if voters at control booths are *de facto* treated by treatment schools which are beyond the 5- or 2-km radius.

A.1.2 Remote schools

One way to abstract away from the problem of spillovers is to consider only remote schools. Intuitively, if a polling booth is near only one school in the RCT, then treatment assignment is straightforward. As before, however, the definition of "near" could imply various different radii. Table A2 limits attention to polling booths which are within a certain radius of only one school in the RCT, and assigns each polling booth that school's treatment status.

Table A2: Treatment definition: remote schools

	President: runoff			
Treatment	-0.066**	-0.049**	0.002	-0.010
	(0.029)	(0.023)	(0.021)	(0.035)
N	251	285	324	172
Mean (control)	0.388	0.402	0.364	0.401
Spec	Remote	Remote	Remote	Remote
Radius (km)	15	10	5	2

Standard errors clustered by electoral district. School matched-pair fixed effects included for the booth's nearest 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 'Mean (control)' displays the mean of the dependent variable for polling booths with Treatment = 0. * p<0.10, ** p<0.05, *** p<0.01

In this case, a large radius is what guarantees the "remoteness" of the school and hence the clarity of the treatment assignment; drawing a smaller radius not only reduces the sample size but may introduce contamination.

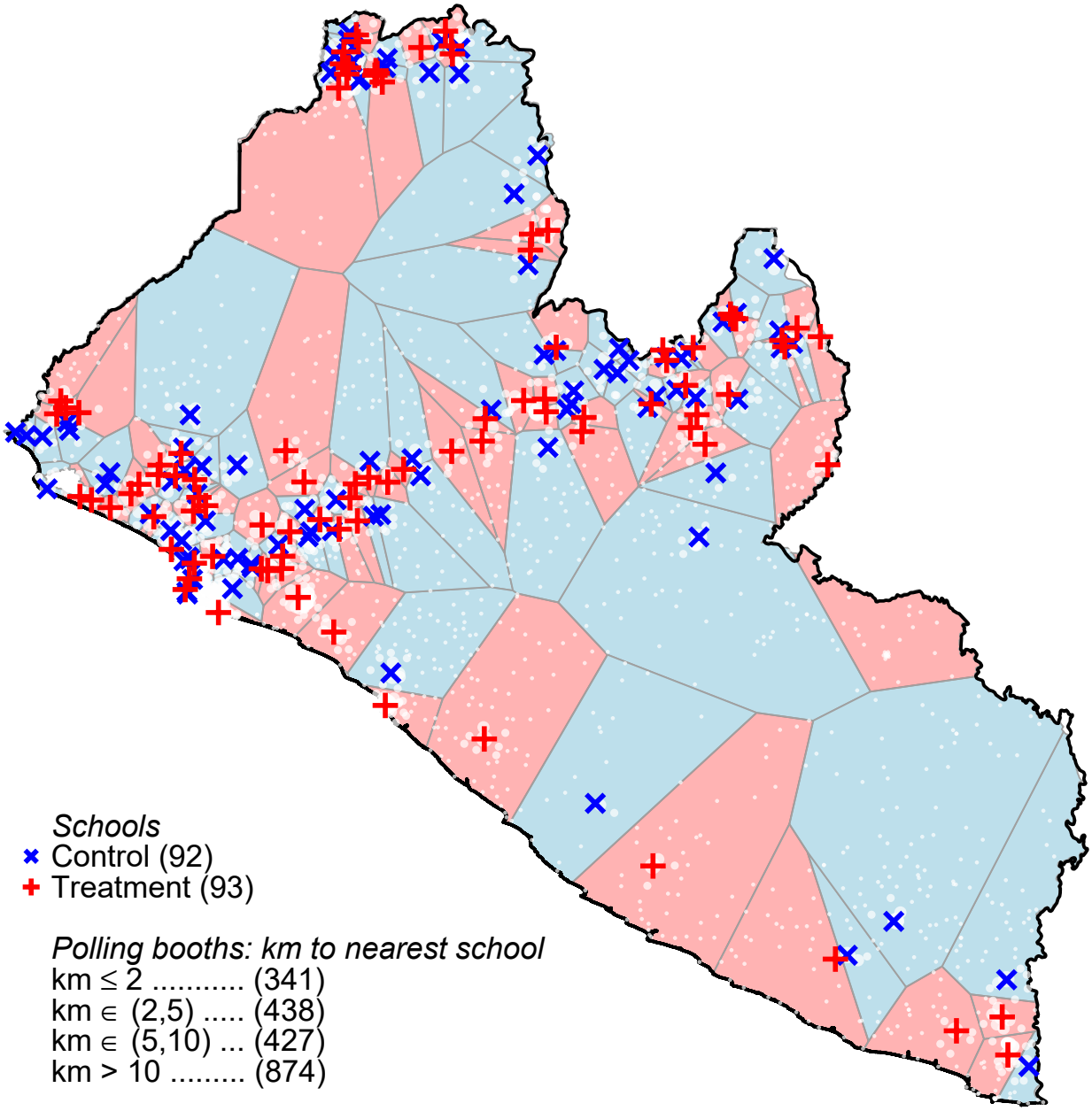
A.1.3 Voronoi tessellation

Another way to define treatment is to assign each polling booth the treatment status of its nearest RCT school. This method has the benefit of being easy to define and also permits the inclusion of all polling booths in the territory of Liberia (though in practice it may make sense to still limit attention to booths within a certain radius of a school).

To visualize the assignment of polling booths to their nearest school, it is helpful to tessellate the surface of Liberia into Voronoi cells. A cell can be defined for each RCT school which consists of the set

of points on the surface which are closer to that school than any other. Figure A3 shows this Voronoi tessellation of Liberia, overlaid with the location of treatment schools and polling booths. Each Voronoi cell takes on the color of its school, and polling booths are sized in inverse proportion to their distance from the school in their cell.

Figure A3: Voronoi tessellation



Map showing Voronoi cells drawn around each school in the RCT. This method breaks up Liberia's territory into polygons containing all points which are closer to a given RCT school than any other. Polygons are colored according to the treatment status of the school they circumscribe, and polling booths within each polygon are sized in inverse proportion to their distance from the school within that cell.

Table A3: Treatment definition: Voronoi tessellation

	President: runoff				
Treatment status of nearest school	-0.026**	-0.029**	-0.030**	-0.034***	-0.040**
	(0.012)	(0.012)	(0.012)	(0.012)	(0.019)
N	2078	1396	1200	772	311
Mean (control)	0.382	0.382	0.382	0.377	0.386
Spec	Voronoi	Voronoi	Voronoi	Voronoi	Voronoi
Radius (km)	All	15	10	5	2

Standard errors clustered by electoral district. School matched-pair fixed effects included for the booth's nearest 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 'Mean (control)' displays the mean of the dependent variable for polling booths with Treatment = 0. * p<0.10, ** p<0.05, *** p<0.01

A.2 Controlling for 2011 election outcomes

Table A4 uses the same specification as that in Section 4 to estimate the average electoral effect of the school reform on electoral outcomes in the October 2017 general election, controlling for 2011 electoral outcomes. Each regression includes a control for the outcome variable from 2011, as well as the number of registered voters and the total votes cast in that election. Missing values for all these variables are replaced with zeros, and indicators for which observations have missing data for each variable are included.

Table A4: Including controls for 2011 election outcomes

	President: runoff		President: First round		Legislative		
	Vote share: Ruling party	Turnout	Vote share: Ruling party	Turnout	Vote share: Ruling party	Vote share: Incumbent	Turnout
Treatment	-0.032**	0.008	-0.029**	0.004	-0.003	-0.012	0.006
	(0.015)	(0.015)	(0.012)	(0.008)	(0.019)	(0.041)	(0.008)
N	1200	1200	1200	1200	1200	1200	1200
Mean (control)	0.382	0.519	0.293	0.775	0.128	0.166	0.762
Controls	✓	✓	✓	✓	✓	✓	✓

Standard errors clustered by electoral district. 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 matched-pair fixed effects included for the booth's nearest 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 'Mean (control)' displays the mean of the dependent variable for polling booths with Treatment = 0. Each regression includes a control for the 2011 value of the outcome variable, as well as the number of registered voters and total votes cast in that election in 2011. * p<0.10, ** p<0.05, *** p<0.01

The results are robust to the inclusion of controls from the 2011 election.

A.3 Supplemental balance tables

A.3.1 Balance on 2011 electoral outcomes, interacting treatment with pair-level treatment effects

Section 5 interacted the treatment variable with indicators for whether the pair-level treatment effects (on teacher mobilization or student learning) were above the median. Pair-level treatment effects are not themselves randomly distributed, however. If some other important variation which affects electoral outcomes happens to be correlated with pair-level treatment effects, we might expect it to show up in a balance test which interacts treatment with these pair-level treatment effect dummies looking at 2011 election outcomes.

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

Table A6: Balance, interacting treatment with teacher engagement 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

Tables A5 and A6 show that the interaction between treatment and the pair-level treatment effect dummies are not significant for 2011 election outcomes.

B Additional tables and figures

B.1 Polling stations at schools

Because many polling station are themselves schools, the reaction of voters and teachers to a school reform may be different where these two roles overlap. A little over half of all polling stations are located at a school – as measured by matching the regex string "school" in the polling station name in the NEC data. About 50 RCT schools are polling booths (this is measured with error by hand, by comparing the names of schools in the RCT to names of polling stations).

B.1.1 Treatment did not make schools polling stations

Table B7 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 B7: 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

B.1.2 Treatment reduced turnout for some offices in places where RCT schools were polling stations

Table B8 interacts treatment with a dummy for whether the polling station is one of the 54 RCT schools which are polling stations.

Table B8: Differential effects of treatment in polling stations: electoral outcomes

	President: runoff		President: First round		Legislative		
	Vote share: Ruling party	Turnout	Vote share: Ruling party	Turnout	Vote share: Ruling party	Vote share: Incumbent	Turnout
Treatment	-0.034**	0.011	-0.032**	0.002	0.005	-0.012	0.004
	(0.016)	(0.015)	(0.015)	(0.009)	(0.021)	(0.042)	(0.009)
Booth is Treatment × RCT school	0.007	-0.037	-0.017	-0.059**	-0.045	0.036	-0.071**
	(0.051)	(0.037)	(0.044)	(0.027)	(0.055)	(0.062)	(0.027)
N	1200	1200	1200	1200	1200	1200	1200
Mean (control)	0.382	0.519	0.293	0.775	0.128	0.166	0.762

Standard errors clustered by electoral district. Nearest school matched-pair fixed effects included. Regressions include polling booths 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.

B.1.3 Treatment did not affect teacher political activity differently at polling stations

Table B9 shows that treatment did not affect teachers political behavior differentially at the 54 RCT schools which were polling stations.

Table B9: Effects on teacher political behavior at polling station schools

	Registration booths	Polling booths	Encourage participation	Campaign for a party or candidate	Involved in any	PCA index teacher involvement
Treatment	-0.035**	-0.044	-0.052*	-0.020	-0.094*	-0.236**
	(0.017)	(0.031)	(0.027)	(0.032)	(0.049)	(0.109)
School is polling booth	-0.010	-0.072*	-0.080*	-0.021	-0.147**	-0.260
	(0.024)	(0.041)	(0.044)	(0.039)	(0.063)	(0.159)
School is Treatment × polling booth	-0.005	-0.027	0.130*	-0.080	-0.009	0.099
	(0.036)	(0.061)	(0.069)	(0.064)	(0.102)	(0.273)
N	836	836	836	836	836	836
Mean (control)						

School matched-pair fixed effects included. * p<0.10, ** p<0.05, *** p<0.01

B.2 More results on teachers

B.2.1 Effect on components of job satisfaction

Table B10: 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	0.000	-0.049	-0.070**	-0.024	0.042	-0.038*	-0.219***	0.070*
	(0.038)	(0.031)	(0.030)	(0.036)	(0.039)	(0.022)	(0.030)	(0.037)
N	764	764	764	764	764	764	764	764
Mean (control)	0.457	0.768	0.783	0.607	0.517	0.906	0.921	0.742

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' or 'somewhat satisfied' to each of these questions. * p<0.10, ** p<0.05, *** p<0.01

B.2.2 Effects on teacher time use

Table B11 shows that the treatment increased the average amount of time teachers reported spending on school-related activities in a day, driven primarily by time spent teaching.

Table B11: Effect of reform on teacher time use

	Teaching	Teaching extra classes	Class prep	Correcting homework	Correcting tests	Other time with students	Total
Treatment	0.972***	0.047	0.073	-0.000	0.055	-0.086	1.139***
	(0.166)	(0.033)	(0.064)	(0.055)	(0.085)	(0.057)	(0.326)
N	883	883	883	883	883	883	883
Mean (control)	3.482	0.237	1.114	0.775	0.727	0.793	5.687

Standard errors clustered by school. School matched-pair fixed effects included. Outcomes from a May/June 2017 survey asking teachers how many hours they spent on each of these activities on the most recent school day. 'Teaching extra classes' does not include private tutoring. 'Other time with students' includes activities such as counseling and talking with parents. * p<0.10, ** p<0.05, *** p<0.01

B.2.3 Other effects on teachers

Table B12 shows other effects of the policy on teachers' (self-reported) experiences. Salaries went up (but not commensurately with hours worked, as shown in Table 3). Teachers in treated schools were more

likely to be on the official government payroll. Treatment made teachers less likely to report having another job, without reducing the pay they received conditional on having a job (though estimates are noisy). Treatment had no impact on the likelihood teachers were willing to state their presidential voting preference.

Table B12: Effect of reform on teachers' experience

	Union member	Monthly Salary (USD)	On govt payroll	Has other job	Weekly pay from other job	Express any vote preference
Treatment	-0.102***	11.355***	0.071*	-0.089**	19.414	0.012
	(0.026)	(3.316)	(0.035)	(0.029)	(18.577)	(0.031)
N	883	883	883	883	94	883
Mean (control)	0.317	93.692	0.531	0.241	18.957	0.630

Standard errors clustered by school. School matched-pair fixed effects included. Outcomes from a May/June 2017 survey. 'Monthly Salary' refers to total net compensation per month (including housing allowance etc). 'Has other job' is a self-reported indicator for whether the teacher works a second job when not teaching. 'Weekly pay from other job' is self-reported income from a second job, for those who report having one. 'Express any' is a dummy for whether the teacher was willing to state which presidential candidate they plan to vote for. Missings replaced with zeros, and dummies for missing included, in all regressions except for 'Weekly pay from other job', where conditionality matters.

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.[36] Dismissal of teachers was a salient issue for union leadership. The reinstatement of dismissed 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.[37] However, it carried a political cost.

B.2.4 Policy's benefits to teachers bypassed those in the union

The policy made some aspects of teachers' jobs better, but few of these benefits seem to have accrued to union teachers. Table B13 shows the effect of treatment, interacted with union status, on various aspects

[36] 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.

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

of teacher's jobs. These interactions should be thought of as exploratory, and interpreted with caution: as Table B12 shows, union membership itself is an outcome affected by treatment.

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 B13 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. Reductions in voting intentions for the ruling party were concentrated among union teachers.

Table B13: Effect of reform on teacher attitudes

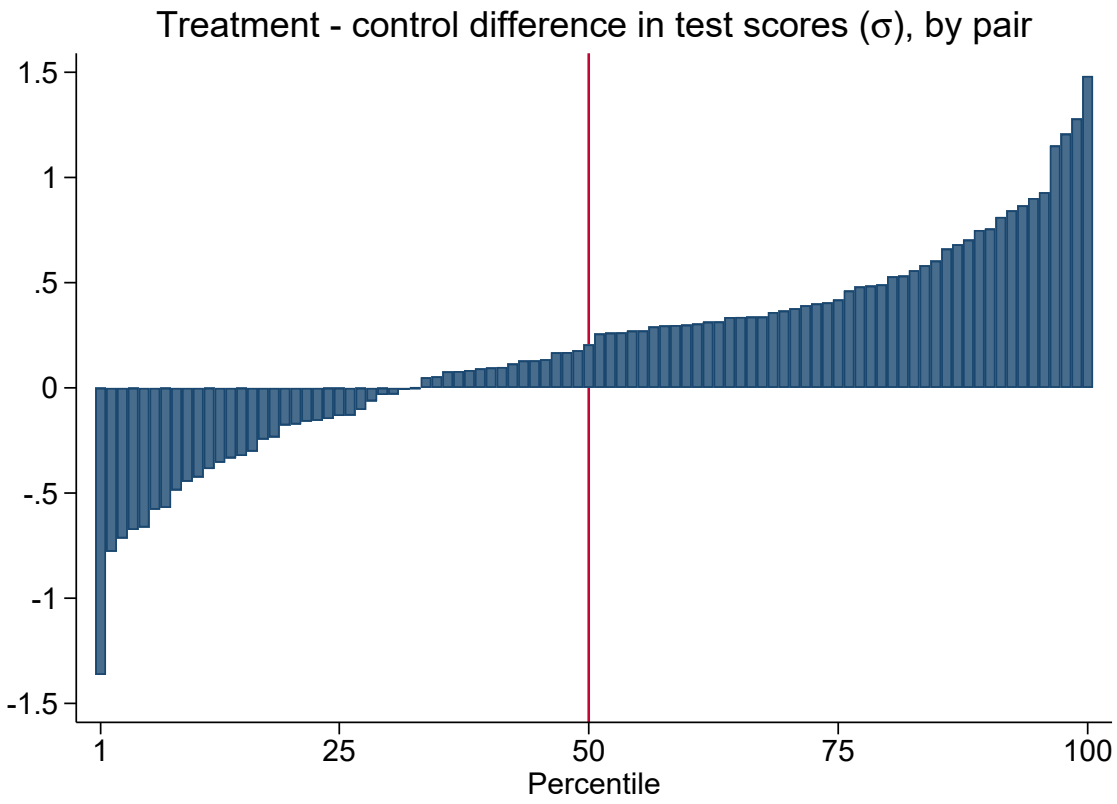
	Gov payroll	Salary (USD)	Other job	Expressed voting pref.	Vote for ruling party pres. cand.
Treatment	0.153***	24.369***	-0.118***	0.047	-0.014
	(0.041)	(4.739)	(0.042)	(0.042)	(0.054)
Union member	0.416***	37.120***	-0.038	0.154**	0.157**
	(0.059)	(5.972)	(0.058)	(0.064)	(0.072)
Treatment × Union member	-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

B.3 Measures of heterogeneity

B.3.1 Variation in pair-level treatment effects on student learning

Figure B4: 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.

B.4 Various measures of school quality

Table B14 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.[38] 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.

Table B14: 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

[38] 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.

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.

B.4.1 Effect of the reform on incumbent vote share, by effect on teacher engagement

Table B15: Effect of PSL on incumbent vote share by size of treatment effect on teacher engagement (index)

	President: runoff		President: First round		Legislative		
	Vote share: Ruling party	Turnout	Vote share: Ruling party	Turnout	Vote share: Ruling party	Vote share: Incumbent	Turnout
Treatment	-0.063***	0.014	-0.050**	0.013	-0.002	-0.035	0.012
	(0.017)	(0.021)	(0.020)	(0.012)	(0.024)	(0.056)	(0.012)
Treatment × TE>p50: Teacher mobilization	0.132***	-0.014	0.073***	-0.013	0.004	-0.028	-0.013
	(0.034)	(0.028)	(0.027)	(0.023)	(0.041)	(0.078)	(0.023)
N	1084	1084	1084	1084	1084	1084	1084
Mean (control)	0.381	0.508	0.291	0.772	0.132	0.178	0.763

Standard errors clustered by electoral district. Nearest school matched-pair fixed effects included. Regressions include polling booths 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. * p<0.10, ** p<0.05, *** p<0.01

Table B16: Effect of PSL on incumbent vote share by size of treatment effect on teacher engagement (individual activities)

	Ruling party: president (runoff)				
	(1)	(2)	(3)	(4)	(5)
Treatment	-0.005	-0.003	-0.033	-0.016	-0.035
	(0.033)	(0.035)	(0.035)	(0.033)	(0.033)
Treatment × TE>p50: Registration booths	0.078				
	(0.080)				
Treatment × TE>p50: Polling booths		0.028			
		(0.055)			
Treatment × TE>p50: Encourage Participation			0.134***		
			(0.047)		
Treatment × TE>p50: Campaign				0.106*	
				(0.059)	
Treatment × TE>p50: Teacher political activity					0.110***
					(0.036)
N	962	962	962	962	962
Mean (control)	0.382	0.382	0.382	0.382	0.382
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. * p<0.10, ** p<0.05, *** p<0.01

B.4.2 Little to no correlation between effects on student learning and teacher political activity

Table B17 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 B17: 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 B18 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 B18: 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.4.3 Controlling for many potential mechanisms

Table B19 interacts treatment with many different indicators of high pair-level treatment effects on a given potential mediator of electoral treatment effects. The best predictors of electoral treatment effects tend to be effects on teachers' well-being.

Table B19: Electoral effect of PSL, interacted with treatment effects on many potential mediators

	Ruling party: president (runoff)
Treatment	-0.066
	(0.078)
Treatment × TE>p50: Teacher political activity	0.171***
	(0.042)
Treatment × TE>p50: Teacher job satisfaction	0.118**
	(0.054)
Treatment × TE>p50: Teacher attendance	0.124*
	(0.068)
Treatment × TE>p50: Teacher daily hours	0.042
	(0.034)
Treatment × TE>p50: Teacher hourly wage	-0.087**
	(0.039)
Treatment × TE>p50: Num. teachers fired	-0.125***
	(0.034)
Treatment × TE>p50: new construction or repairs	-0.061
	(0.054)
Treatment × TE>p50: Student attendance	-0.005
	(0.049)
Treatment × TE>p50: Student learning	0.046
	(0.032)
N	953
Mean (control)	0.382
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

C 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.

C.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.[39] For the purposes of this analysis, I consider the case in which I is held constant.[40] I focus instead on changes to the non-monetary inputs to the public good function captured in A. I assume that $\frac{\delta g(I,A)}{\delta A} > 0$.

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

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

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

Bureaucrats, by contrast, are characterized by working in the civil service that provides public goods and services. The government can direct civil servants to perform two types of work: A, attendance at their public-service-providing job; and E, electioneering. E could encompass legitimate and legal behavior such as encouraging registration, campaigning, organizing, donating, soliciting donations, and getting out the vote. It could also include things like vote-buying and intimidation efforts. Bureaucrats have measure 1 of work hours, and I include the strong assumption that bureaucrats obey the government's directives, so $A \in [0, 1]$ and $E = 1 - A$ mechanically.[41] 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

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

[40] 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).

[41] 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.

that bureaucrats do not experience disutility from E, because their incentives are aligned with the ruling party which is the source of their job.[42] 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, religious, philosophical, or ethnic affinities). F_i is the c.d.f. of X_i, with f_i the density (which I assume to be differentiable). Parties know the distribution F_i of the voters, but not the realizations of x.

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.[43] 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:

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

[42] 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.

[43] 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.

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

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

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

$$\max_A S_R$$

subject to

$$A + E = 1$$

C.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.

C.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.

C.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.