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ABSTRACT

The Politics of Public Service Reform*

This paper provides experimental evidence on the electoral effect of a large education reform in a developing democracy. The reform significantly improved school quality on many dimensions (Romero, Sandefur, & Sandholtz, 2020). In this paper, I show that the reform reduced the incumbent party's presidential vote share by 2.1 percentage points (5%). The reform also reduced teachers' job satisfaction, support for the incumbent government, and political engagement. Electoral effects were positively correlated with effects on teachers' political engagement; the reform lost most votes where it caused greatest political disengagement of teachers.

JEL Classification: I25, O10, P00, C93, D72, H41

Keywords: education, development, public service delivery, political economy, policy feedback, elections, randomized controlled trial, Liberia

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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). Providers are often deeply embedded not only in machine politics but in electoral infrastructure such as the staffing of polling booths (Larreguy, Montiel Olea, & Querubin, 2017; Neggers, 2018). The political role of civil servants therefore has important implications for 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 hesitate 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 in a politically selective way, making causal identification of their political effects difficult (Asher & Novosad, 2016; Burgess, Jedwab, Miguel, Morjaria, & Padró I Miquel, 2015; Brollo & Nannicini, 2012). 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 rare to be able 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. Rates of teacher absence surpass 50%, suggesting low accountability within the education system and an incentive for voters to enforce discipline through elections. 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 et al. (2020) show that the reform increased teacher-pupil ratios, student test scores, student attendance, teacher attendance, teacher time-on-task, student enrollment, student satisfaction, and parent satisfaction. However, the reform also increased teacher turnover, and led to the exclusion of some students where operators chose to enforce class size caps. More generally, the policy caused intense controversy both inside and outside Liberia: a UN Special Rapporteur denounced it and the largest teachers' union in Liberia staged a strike over it (United Nations, 2016; Front Page Africa, 2016a).

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 surveys conducted before and 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 5% fewer votes in polling booths near treatment schools compared to control schools (-2 percentage points from a base of 41 percent). Effects on turnout were also negative, but smaller and less significant, suggesting the electoral effect operated at least in part through persuasion. There was no effect

¹This measure comes from 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).

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 present results from various alternative empirical strategies, each yielding qualitatively similar results.

I also show that the reform eroded teachers’ job satisfaction and support for the ruling party. 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 12% (5 percentage points). 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). The policy reduced the rate of union membership in treated schools (likely via turnover), but perhaps surprisingly, union membership does not predict the political activity of individual teachers. Nor does a picture emerge of the union as an effective political force: in surveys, teachers were mostly unaware of the union’s stated opposition to the policy, few had their union representative’s contact information, and vast majorities said the union had little or no power. Teachers’ choices around political activity appear to be driven by their own preferences rather than obedience to union directives (similar to the findings of [Larreguy et al. \(2017\)](#)).

I show suggestive evidence that the mechanism driving the negative electoral effects was the political disengagement of teachers, not the opposition of ordinary voters. I test this by calculating local treatment effects on various outcomes — voting, learning, and teacher political engagement — in small groups of schools. I aim to uncover whether the treatment reduced incumbent vote share most in the places where it raised test scores least, or where it alienated teachers most. I then test for correlations between treatment effects on the different outcomes at the group level, removing small-sample bias using the deconvolution method outlined by [Walters \(2024\)](#). I find only a weak correlation between voting and learning treatment effects, while the bias-corrected correlation between treatment effects on incumbent vote share and teacher political engagement is 0.50. In other words, the reform lost most votes where it demobilized teachers most. If teacher effort at political persuasion had been the sole cause of the electoral effects, I calculate that a teacher vote multiplier of 12.4 would be sufficient to explain the reduction in vote share. This equates to each teacher persuading 1 out of every 273 voters near her school.

The correlation between voting effects and learning effects is also positive (though weaker), and house-

hold surveys confirm that the policy itself was broadly popular. A phone survey, conducted just before the election among adult household members of students from treatment and control schools, show that even in the control group, people had heard about the policy, supported it, and believed it worked — with each of these measures even higher in the treatment group. In other words, the policy increased support for itself (from a high base). Nevertheless, treated households were less likely to vote for the ruling party's presidential candidate. This is consistent with the treatment reducing support for the government through a channel besides people's view of the policy itself — perhaps teacher persuasion. Taken together, this evidence suggests that although the effect of teacher opposition was dominant overall, voters broadly appreciated the policy, lending some nuance to 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 B). 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.

Despite the negative electoral effects I measure, the new president's administration re-branded the PSL program and scaled it up. Philanthropic funding through the program became an important part of the education ministry's budget. This may be because the new administration was unaware of the policy's political costs, or calculated that such costs had already been paid by the previous government. It may also reflect that governments in aid-dependent democracies must balance accountability to voters and donors.

This 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 primarily qualitative (A. Banerjee et al., 2017). Existing literature documents teachers' political organization and capacity to shape policy, but does not directly demonstrate how these groups affect electoral outcomes (Bold et al., 2018; Larreguy et al., 2017; Ross Schneider, 2021). This paper examines effects on voting and service providers' political activity in the same context, providing strong suggestive evidence of the link between them and highlighting the importance of teacher buy-in for reforms to increase learning (Bold et al., 2017).

The paper also contributes to the broader literature on the political economy of public service provision

by identifying the electoral effects of a shock 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, 2024).² Cook, Kogan, Lavertu, and Peskowitz (2020) show that charter school openings affected school board election turnout in Ohio (USA), while Dias, Duque, Ferraz, and Sandholtz (2025); 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. This paper shows how a service quality shock itself affects voting.

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). I 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 in cost/benefit analyses (Hendren & Sprung-Keyser, 2020). This paper provides empirical evidence consistent with theoretical explanations for status-quo bias and pandering by politicians (Fernandez & Rodrik, 1991; Maskin & Tirole, 2004; Daley & Snowberg, 2011), 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 tests robustness; and Section 7 concludes.

2 Context

Liberia’s education system faced multiple challenges in 2016, including rampant teacher absenteeism, a net primary enrollment rate of 38%, and a youth literacy rate of 55% (Mulkeen, 2009; World Bank, 2014b,

²Related 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; Boudot-Reddy & Butler, 2024; 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.

2014a). President Ellen Johnson Sirleaf in 2013 declared that the educational system was “a mess” and a “national emergency” (Reuters, 2013; Sayon, 2013).

Partnership Schools for Liberia: In response, the Liberian Ministry of Education created the “Partnership Schools for Liberia” (PSL) program (MoE, 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 contributed USD\$50 per pupil per year — a doubling of baseline per-pupil expenditure. The operators had responsibility for the daily management of the schools, while the government could in theory hold them accountable for results. The heterogeneous group of operators included for-profit chains and non-profit NGOs, some local and some international (Edwards, 2017).

Access to the reform was randomized within a subset of 185 eligible public primary schools across 13 of Liberia’s 15 counties. These eligible schools were not representative — they had better facilities, internet access, and road access than the average school in the country. Treatment was randomized within strata consisting of pairs of schools matched on geography and baseline infrastructure. Romero et al. (2020) provide detail on the contours of the PSL reform, but highlights include: a) Operators were allowed to supplement the national curriculum with extra material and longer school days, but required to keep schools non-selective and fee-free; and b) Contract teachers were not allowed — only government-employed civil servants — but operators were allowed to request reassignment of underperforming teachers. Romero et al. (2020) report that the program increased test scores by 0.18σ (about 60% of a year’s worth of schooling).³ Teacher attendance rose by 50%, teacher time-on-task by 43%, and satisfaction of both students and parents by about 10%.⁴

PSL offers 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. 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 the evaluation of its effectiveness in meeting its own aims, a rarity for most implemented policy. Fourth, the policy was unusually salient for an education reform: it was implemented in

³This falls at the 70th percentile of effect sizes in education interventions in the developing world (Evans & Yuan, 2022).

⁴Romero 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.

an election year and garnered significant press attention.⁵ Finally, the involvement of the private sector in the provision of public services makes the electoral effects particularly interesting. To supporters, public-private partnerships (PPPs) offer the promise of improved service delivery where state capacity is weak, and various governments have now implemented PPPs in education ([Crawford & 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.

Politics in Liberia: Liberia is a young democracy; its first fully democratic national election was held in 2005. However, power is centralized and only federal officials are elected (local governments are presidential appointees). One democracy index ranked Liberia in 2017 just below Tanzania and Bangladesh, and just above Nepal and Kenya ([Economist Intelligence Unit, 2022](#)). 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. Presidents and representatives are elected for six-year terms, and senators for nine-year 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.

Liberian politics do not feature strong parties with consistent ideologies or entrenched policy aims; party platforms “tend not to differ to any great extent and actual divergences in policy are not prominent” ([Pailey & Harris, 2017](#)). Ethno-regional loyalties play a role, but are not as dominant as in some other African democracies. Party strongholds often shift from election to election, and in none of Liberia's counties did any party win 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, 2011, and 2023; and the Congress for Democratic Change (later the Coalition for Democratic Change) (CDC), whose presidential candidate made it to the runoff election in 2005, 2011, and 2023, 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. 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](#)).

The ruling Unity Party (UP)'s presidential candidate in the 2017 election was the “pseudo-incumbent”

⁵The RCT results — available as ([Romero, Sandefur, & Sandholtz, 2017](#)) — 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.

Joseph Boakai, vice president of the term-limited Sirleaf (Pailey & Harris, 2017). A dearth of opinion polling made it difficult to identify a front-runner prior to the election, but 19 of Liberia's 30 senators endorsed Boakai, including 13 from parties other than Boakai's (Front Page Africa, 2017). His principal rival was the world-famous former footballer George Weah.⁶ As a member of the CDC, Weah 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 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, which Weah won with 62% of the vote.

Beyond creating and implementing the PSL reform itself, the ruling Unity Party (UP) claimed credit for it in various ways during the campaign. 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 teacher union group reported 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." (COTAE, 2017)

The program provoked controversy and press coverage in Liberia and beyond. A United Nations special rapporteur on the right to education strongly condemned an early iteration of the proposed reform, and it attracted critical responses from some scholars of education (United Nations, 2016; Hook, 2017; Klees, 2018). In Liberia, local press reported on students who were forced out of their schools, and on others who were hungry at school after some PSL schools extended the school day past the lunch hour (Senah, 2016; Mukpo, 2017c, 2017b). Later, in 2018, at least one member of the House of Representatives went on record condemning one of the operators' operations in his constituency (Front Page Africa, 2018).

Teachers in Liberia have considerable political heft. In 2016 they made up about 1.5% of the labor force, with average monthly earnings of $3.1 \times$ GDP per capita (Evans, Yuan, & Filmer, 2022). The Ministry of Education is one of only a handful of ministries with a strong presence in all 15 counties in Liberia. Its employees 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

⁶Weah played for many years in top European leagues, and won the prestigious Ballon d'Or award in 1995, the first African player to do so.

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 elections and policy choices.⁷ Politicians court this political influence: during the 2017 campaign, 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 to derail education reforms in other countries (Finger, 2018; Ross Schneider, 2021; Bruns, Macdonald, & Schneider, 2019).

Liberia's largest teachers' union, the National Teachers Association of Liberia (NTAL), stridently opposed PSL. Its leader claimed that teachers had been fired for speaking out against the program, and urged Boakai to use his office to reinstate them (Mukpo, 2017a; Mulbah, 2017; Brooks, 2017). The union also led a strike calling for the abandonment of the policy, prompting a counter-strike by students in which the highway to the airport was blocked and public buildings were ransacked.⁸

The partnership schools program, and education more generally, were therefore unusually salient in the 2017 general election (IMANI, 2017). Although a minor opposition party took an explicit position against PSL, both of the main presidential candidates supported the policy (Daygbor, 2016; Nimely, 2016; Malkus, 2017).

Upon taking office, Weah's administration re-branded PSL as the Liberia Education Advancement Program (LEAP) and began scaling it up. The program transitioned from central management to a series of bilateral partnerships, with each operator responsible for providing their own funding (Republic of Liberia, Ministry of Education, 2019). Some providers dropped out, but those who remained began managing more public schools. By 2022, LEAP consisted of four school providers operating 487 schools across Liberia, with over 95,000 students – about 16% of total public enrollment in primary and pre-primary. The 5-year Education Sector Plan declared an aim to “actively promote an increase in the number of public-private partnership initiatives,” and projected USD 5.5 million per year in philanthropic LEAP funding — 10% of total donor financing (Republic of Liberia, Ministry of Education, 2022). The program

⁷Source: author's conversation with a former deputy minister of education.

⁸See NTAL (2017); Butty (2016); Ziamo (2016); Kwanue (2016b); Brooks (2016); Front Page Africa (2016b, 2016a); Kwanue (2016a)

may have helped to lose the election for Boakai, but Weah’s embrace of it underscores how governments in aid-dependent democracies must balance accountability to both voters and donors.

3 Data and Design

This paper’s identification strategy leverages the randomization of the PSL program and uses administrative voting data from the 2017 general election. The estimates of teachers’ political attitudes and behavior are drawn from a teacher survey collected in May/June 2017 (at 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 at the polling booth level.⁹ There were 2,080 polling booths in Liberia in the 2017 election. These polling booths represent highly geographically granular measures of voting outcomes: 637 votes were cast in the median booth in the first-round presidential election; 428 in the runoff. Electoral data at the polling 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 compliance: restricting the sample to polling booths near only one type of school limits the sample considerably, but including booths near both kinds of schools introduces contamination which may bias results toward zero. 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 but focuses on booths nearest to the schools; a wider radius includes more booths which may be more weakly treated.

In my main specification, I assign each polling booth the treatment status of its nearest experimental school, limiting attention to booths within a 10km radius.¹⁰ The 10km radius is wide enough to embrace

⁹In 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 “polling booth” in this paper according to more conventional usage.

¹⁰As 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.

at least one polling booth for all 185 schools and 92 school pairs in the RCT.¹¹ 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 6 tests robustness of the electoral results alternate specifications and other radii.

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 stratum p . Stratum fixed effects are represented by α_p . $Treatment_i$ is the treatment status of the booth's nearest RCT school. Controls at the level of the polling booth, included in some specifications, are represented by X_i : prior (2011) election outcomes for number of registered voters, total votes cast, turnout, and ruling party vote share for the three races (presidential first round, presidential runoff, legislative). Standard errors are clustered at the level of the polling booth's nearest RCT school.

To visualize the assignment of polling booths to their nearest school, it is helpful to tessellate the surface of Liberia into Voronoi cells (Aurenhammer, 1991). For each RCT school, a cell can be defined consisting of the set of points on the surface which are closer to that school than any other. Figure 1 shows this Voronoi tessellation of Liberia, overlaid with the location of RCT schools and polling booths. The color of each cell corresponds to its school — red for treatment, blue for control — and polling booths are sized in inverse proportion to their distance from the school in their cell.

¹¹Two matched pairs have only one polling booth within 10km; because I use stratum fixed effects in the main specification, this means the singleton booths get dropped from analysis, leaving an analysis sample of 1200 polling booths, and 90 of the 92 matched school pairs. While all 185 RCT schools are within 10km of at least one polling booth, 178 RCT schools are some polling booth's *closest* RCT school; that is, 7 RCT schools are less close to their nearest polling booth than another RCT school.

Figure 1: Polling booths and schools

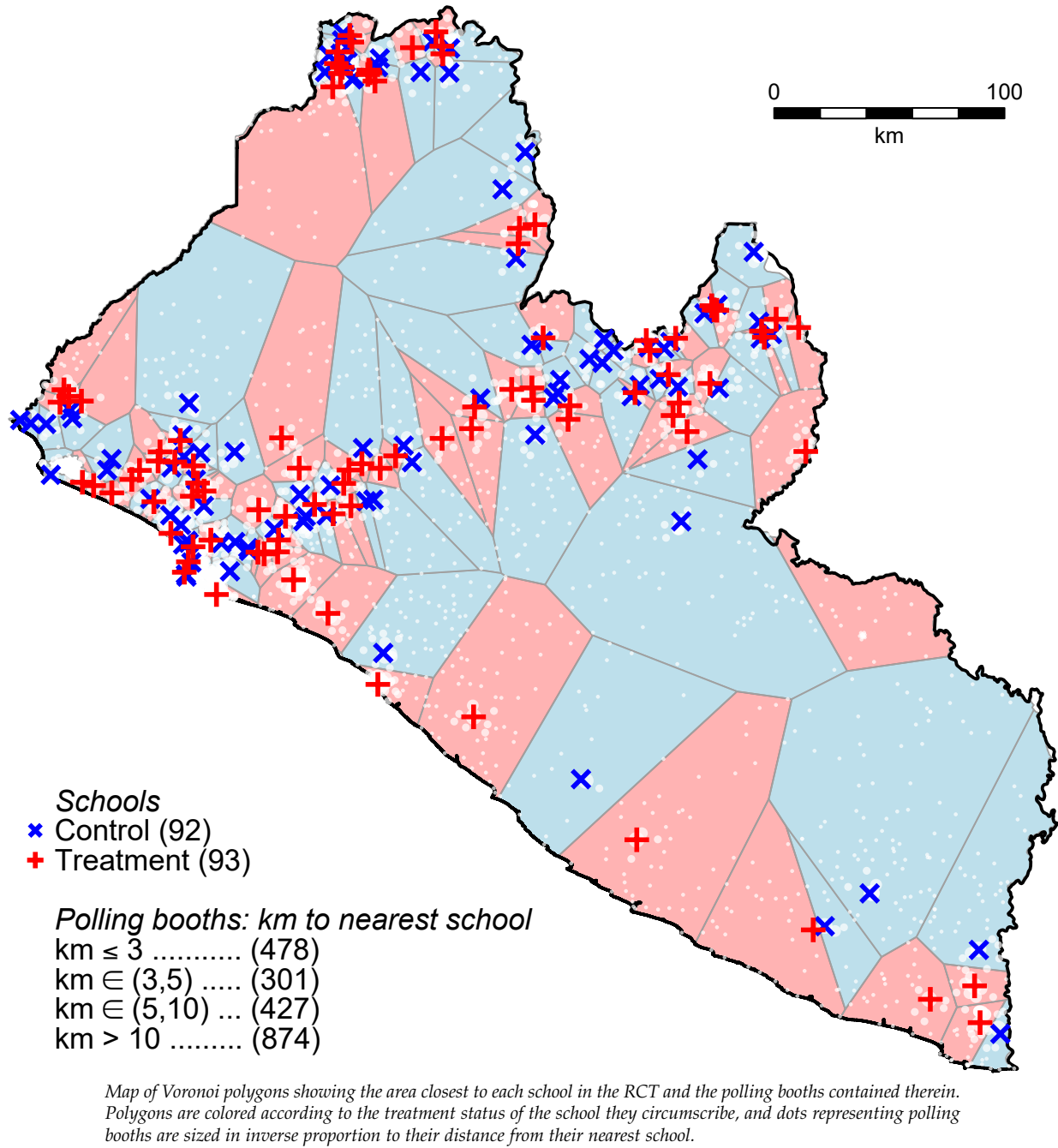


Table A1 and Table A.2 help confirm the exogeneity of this treatment assignment with additional balance checks employing Equation 1 to study the relationship between booths' treatment status and various dependent variables which should not vary systematically with treatment. Table A1 shows that

there are no significant differences in terms of school density — i.e., treated booths are not close to more RCT schools, nor to more public primary schools in general. Table A2 likewise shows essentially no significant differences in terms of 2011 electoral variables not included in the main tables, including registered voters, number of votes cast, and share of invalid votes from the presidential and legislative elections of 2011. (The exception is the share of invalid votes in the first round of the 2011 presidential election, which was 0.6 percentage points higher in booths near treated schools.)

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”).

3.4 Specification: teacher outcomes

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

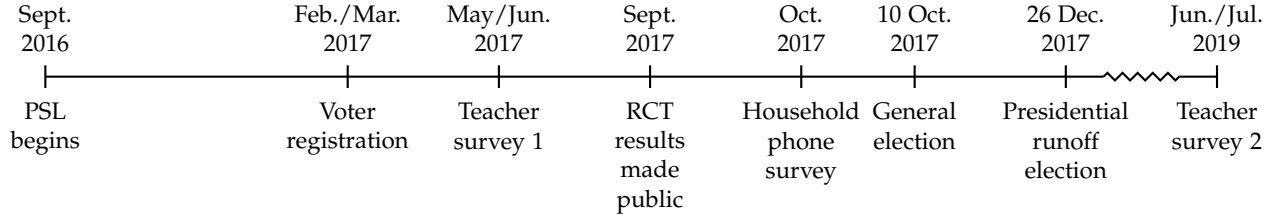
$$Y_{isp} = \alpha_p + \beta Treatment_s + \varepsilon_{isp} \quad (2)$$

Y_{isp} represents electoral outcomes for teacher i at school s in stratum p . α_p are stratum fixed effects. $Treatment_s$ is the school’s assigned treatment status. Standard errors are clustered at the level of the school. Because teachers were only surveyed after the reform’s implementation had begun, I am not able to check baseline balance for teacher outcomes.

3.5 Timeline

Figure 2 displays a timeline of relevant events.

Figure 2: Timeline



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 presidential vote share

Table 1 shows regression results on electoral outcomes using the specification in Equation 1. The main outcome of interest is the vote share for the ruling party candidate in the final round of the presidential election; other outcomes of interest are vote share for the ruling party candidate in the first round 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 in all races. Panel A checks balance, using the main specification on 2011 election outcomes (the last general election before the treatment). While most 2011 election variables are balanced, presidential ruling party vote share was somewhat lower in polling booths near treated schools, notably in the first round. I therefore show main results on the 2017 election with and without controlling for 2011 electoral outcomes (Panels C and B, respectively). In Panel C, the full battery of 2011 electoral outcome controls consists of ruling party vote share in the first and second rounds of the presidential election; ruling party vote share in the legislative race; turnout in each of those races; the total number of votes cast in each of those races; and the number of registered voters.

Table 1: Effect of school reform on electoral outcomes

	President: Runoff		President: First round		Legislative		
	Vote share:		Vote share:		Vote share:	Vote share:	
	Ruling party	Turnout	Ruling party	Turnout	Ruling party	Incumbent	Turnout
Panel A — Balance: 2011 Election Outcomes							
Treatment	-0.007* (0.004)	-0.012 (0.008)	-0.028*** (0.009)	-0.004 (0.003)	0.007 (0.011)	-0.010 (0.020)	-0.005 (0.003)
N	1200	1200	1200	1200	1200	1200	1200
Mean (control)	0.776	0.353	0.396	0.658	0.167	0.306	0.649
Panel B — Main Results: 2017 Election Outcomes							
Treatment	-0.032** (0.012)	-0.012 (0.009)	-0.023* (0.012)	-0.011* (0.006)	0.007 (0.011)	-0.002 (0.021)	-0.010 (0.006)
N	1200	1200	1200	1200	1200	1200	1200
Mean (control)	0.407	0.593	0.305	0.771	0.174	0.208	0.761
Panel C — Robustness: 2017 Election Outcomes With Controls							
Treatment	-0.021** (0.010)	-0.011 (0.007)	-0.016* (0.009)	-0.006 (0.005)	0.015 (0.011)	-0.006 (0.020)	-0.005 (0.005)
N	1200	1200	1200	1200	1200	1200	1200
Mean (control)	0.407	0.593	0.305	0.771	0.174	0.208	0.761
Controls	✓	✓	✓	✓	✓	✓	✓

Standard errors clustered by polling booth's nearest school. Regressions include polling booths from the 2017 election located within 10km of any school in the RCT; booth treatment assignment is that of nearest PSL school. Randomization strata fixed effects corresponding to the booth's nearest school are included. Missing outcome values have been replaced with zero, and indicator variables for whether values are missing have been included in all regressions. Controls included are the 2011 election values for the number of registered voters; total votes cast in each race; turnout in each race; and ruling party vote share in each race. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

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 2-3 percentage points, depending on whether controls are included.¹² Using the estimates from Panel C for calculations, this constituted a 5% reduction in the runoff (2.1 pp off a mean of 41%), and a 5% reduction in the first round (1.6 pp off a mean of 30.5%). (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, although point estimates are negative (and marginally significant in the first round when not including controls).¹³

¹²The correlation in this sample between ruling party presidential vote share in the first round and the runoff is 0.88.

¹³This finding stands in contrast to Cook et al. (2020), who find that the introduction of charter schools reduced voter turnout in

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 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.¹⁴

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.¹⁵ 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.¹⁶

4.2 The reform alienated teachers and disengaged them politically

Next 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 supported 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, only 14% had done so. This may lend weight to teachers' opinions in the

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.

¹⁴For 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).

¹⁵The 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.

¹⁶This 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.

community. 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. Among teachers in control schools, 40% 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, [Neggers \(2018\)](#) shows that the identity of polling booth staffers – a large fraction of whom are teachers – has effects on vote share large enough to swing elections. [Larreguy et al. \(2017\)](#) identify teachers in Mexico as important political brokers.

Ex ante, teachers in my data were much more likely than the general public to support the ruling party. Before the election, 71% of teachers in control schools said they planned to vote for the presidential candidate from the incumbent Unity Party.¹⁷ Contrast this with 36% of adults in the households of students in RCT schools in a survey around the same time, or the 29% Boakai received overall in first-round voting. Histograms of school-level averages of ruling party voting intentions, separately for households and teachers, are shown in Figure 3a. These histograms include data only from control schools.

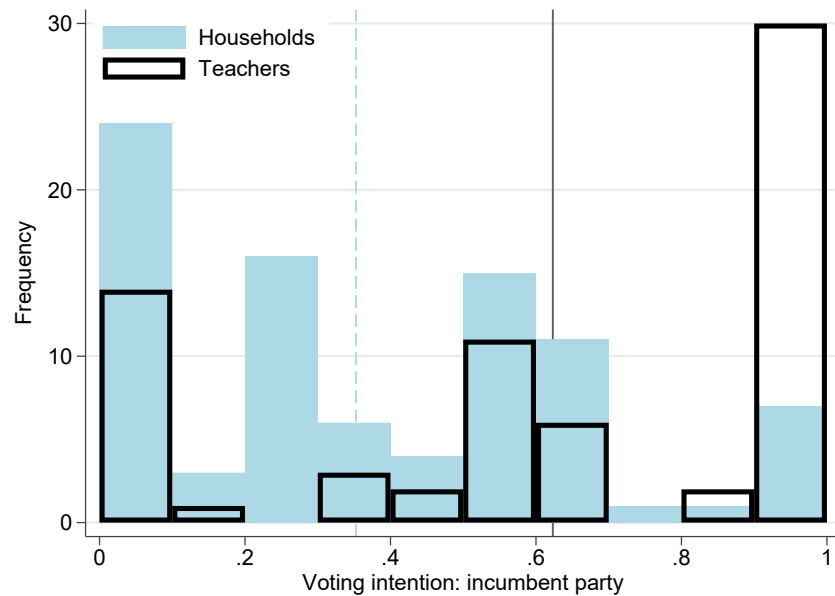
Another suggestive indicator of teachers' support for the ruling party – and possibly their powers of electoral persuasion – is that ruling party vote share is higher at polling booths located at schools than at other polling booths ($\beta = .043, p < .001$).¹⁸ A significant difference persists even when including electoral district fixed effects ($\beta = .013, p = .028$). There are many possible reasons why the subset of the electorate which votes in schools might tend to support the ruling party — perhaps the ruling party clusters its public good investments together, for example, which would confound the influence of schools with that of other government services. However, another important factor could be the influence of teachers who support the ruling party. Figure 3b plots the distribution of incumbent party vote share in the runoff election separately for polling booths which are located at a school vs. not.¹⁹

¹⁷This 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.

¹⁸A little over half of polling booths are located at a school, and about 54 RCT schools are polling booths. Treatment had no effect on whether a school was a polling booth. See Section A.6 for more details.

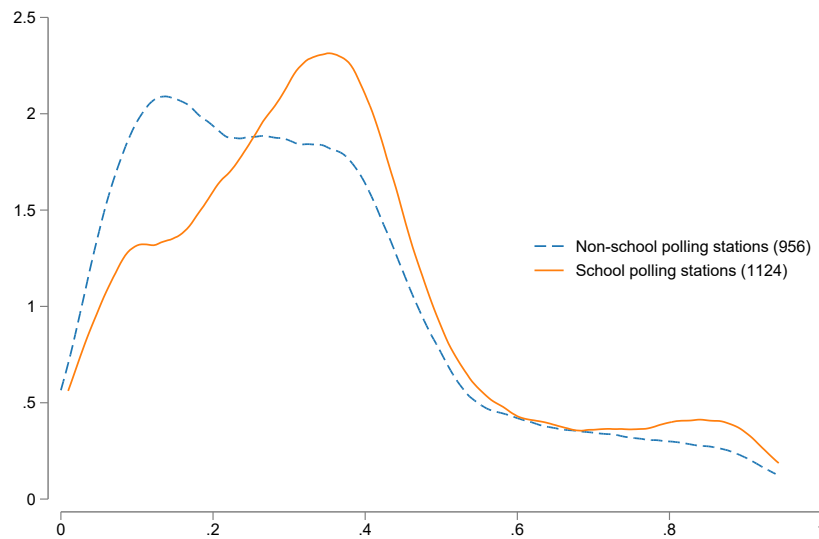
¹⁹School polling booths are identified by whether the word “school” appears in the polling booth’s name on official NEC records. This analysis considers polling booths located at any school, not just schools in the RCT.

Figure 3: Descriptive: Teachers support ruling party in status quo



(a) Histogram: support for ruling party, 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. Dashed blue line shows average for households; solid black line shows average for teachers.



(b) Density: Ruling party vote share in school vs. non-school polling booths

This figure depicts kernel density 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.

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. Panel A in Table 2 shows the effect of the reform on teachers' job satisfaction, hours, wage, union membership, opinion of public-private partnerships, and voting intentions for its presidential candidate.²⁰

Teachers in treated schools scored much lower on an index of job satisfaction.²¹ Table A8 shows that this was driven mainly by dissatisfaction over job location and working hours. They were also required to work significantly more hours per day (as measured by a survey question which asked 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 A9). Indeed, while Column 2 in Panel A of Table 2 shows that the hours per day worked by teachers increased by a bit over one, Table A9 show that teachers' time spent teaching per day on its own increased by nearly one hour per day (from a base of 3.5). More broadly, Table A9 makes clear that this large increase in time spent teaching explains more or less all of the positive effect on teachers' work hours. However, teachers' pay did not rise commensurately with their time spent working, and their effective hourly wage plummeted.

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 dramatically reduced the number of teachers in treated schools who identified as being members of the union. This could be due to a combination of teachers leaving the union as well as new hires being less likely to be union members in treated schools. The treatment also diminished the ruling party's support among teachers as measured by their stated voting intentions, which fell 4.8 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 A10.)

²⁰The examination of effects of the policy on teacher political attitudes was pre-specified in the pre-analysis plan: <https://www.socialscisearch.org/trials/1501>.

²¹Teachers 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.

4.2.3 The reform reduced teacher political engagement

In addition to souring teachers' stated support for the ruling party, the reform also 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). Panel B of Table 2 shows the effect of the policy on teachers' reported political activities.

Table 2: Effect of school reform on teachers

Panel A — Job Behavior and Attitudes						
	Job satisfaction	Hours per day	Effective hourly wage (USD)	PPPs are good	Union member	Vote for ruling party pres. cand.
Treatment	-0.172** (0.079)	1.110*** (0.338)	-1.305*** (0.326)	0.012 (0.025)	-0.112*** (0.030)	-0.048* (0.028)
N	764	764	764	764	764	764
Mean (control)	0.096	6.814	3.370	0.820	0.326	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. '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. * p<0.10, ** p<0.05, *** p<0.01						
Panel B — Political Engagement						
	Registration booths	Polling booths	Encourage participation	Campaign for a party or candidate	Involved in any	PCA index teacher involvement
Treatment	-0.035*** (0.012)	-0.054** (0.023)	-0.022 (0.020)	-0.044** (0.022)	-0.103*** (0.034)	-0.224*** (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 attitude measures from Figure 3 and Panel A of Table 2 suggest that this political action would likely have been largely 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 .²² This effect aggregates various channels. Teachers' dissatisfaction with the policy may have sapped enthusiasm for working to reelect the government responsible for it. The private operators of treated schools may have been less interested in allowing their teachers to spend time on electoral work, or interfered directly or indirectly with political parties' ability to monitor brokers, including teachers (Bowles, Larreguy, & Liu, 2020). Private operators also lengthened the school day and enforced stricter teacher attendance, producing a mechanical reduction in the time available for political engagement. Finally, staff turnover may have left treated schools with a less politically engaged teacher base. When interpreting all teacher results it is important to bear in mind that effects on both attitudes and behavior reflect some combination of changes in individual teachers as well as changes to the composition of teachers at the school (the lack of a true teacher baseline survey makes it difficult to disentangle these two channels). The clear upshot, however, is that the combined effect of these various channels made the teacher body at treated schools much less satisfied and much less politically active.

4.2.4 The role of unions

While the evidence appears clear that teachers play an important 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 2017, membership is far from universal: only 33% of surveyed teachers in control schools 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 union members scored no differently on an index of overall job satisfaction. Organization and communication appear to be 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

²²For 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.

union had, 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 its members and to other teachers. Only 6% of teachers reported that anyone 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 all 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, 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). That said, the reduction in ruling party support among teachers from Panel A of Table 2 was driven entirely by union members, perhaps reflecting their higher level of initial support (see Table A11).

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 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 A.7 for more discussion of how the reform affected union teachers.

5 Mechanisms

This section examines the relationship between the reform’s treatment effects on three outcomes of interest: voting, student test scores, and teacher political engagement. Whether the reform lost votes *because* or *in spite of* the way it affected students and teachers is a question of great policy interest. Under some theories of retrospective voting, electorates punish governments for poor performance; this could explain

the average negative electoral effect I find if voters either opposed the policy or felt it didn't measure up to expectations (Besley, 2006; Bursztyn, 2016). Other work emphasizes how powerful elites can manipulate voting through clientelism to undermine pro-poor policies, even where elections are nominally free and fair (Anderson, Francois, & Kotwal, 2015). Determining which of these potential channels is more likely to have driven the overall electoral results could inform efforts to design public service reforms in politically feasible ways.

Unfortunately, it is not empirically possible to identify whether treatment effects on one outcome are themselves the cause of treatment effects on another outcome (Angrist & Pischke, 2009). Instead, to illuminate potential links between effects on different outcomes, I measure treatment effects on each outcome within subdivisions of the sample, then examine patterns in the distribution of effects across these subdivisions. I focus here on examining whether electoral losses were concentrated in places where the reform a) was particularly ineffective at raising student test scores, and/or b) made teachers particularly aggrieved. These analyses can provide suggestive evidence on what may have driven the electoral effects and inform future research on the subject. I then present household survey results which help illuminate the potential mechanisms behind the average vote effects.

5.1 Comparing Conditional Average Treatment Effects

5.1.1 Comparing CATEs: Methodology

To study patterns in treatment effects on different outcomes, it is first necessary to define outcomes at a consistent unit of measurement: the school. I collapse each outcome measure to the level of the school; for electoral outcomes, I assign each polling station to its nearest RCT school (to include as much data as possible, I impose no distance limit for this exercise).

It is then necessary to subdivide the sample and measure treatment effects within these subdivisions. The original randomization was carried out within 92 strata consisting of two schools each. These pairs represent the greatest possible disaggregation of treatment-control differences. However, the relationships between the treatment effects of any two outcomes within one stratum are contaminated by the potential relationship between the *levels* of the same two outcomes: if levels of two variables are correlated, their within-stratum differences will be correlated mechanically.²³ To measure meaningful correlations between

²³It is possible as a descriptive exercise to compare within-stratum differences across different outcome variables, and the correlations between the levels of the variables of interest in this case are very small: the school-level correlation between levels of incumbent vote share and student test scores is -0.06 ($p = 0.44$), while the correlation between levels of incumbent vote share and teacher political engagement is -0.04 ($p = 0.59$). The correlation between stratum-level treatment-control differences in incumbent vote share and test scores is -0.02 ($p = 0.86$) while the correlation between pair-level treatment-control differences in incumbent vote

treatment effects on different outcome variables, it is therefore necessary to aggregate strata into fewer, but larger, groups. Larger groups imply reduced measurement error within each group, but also reduced power for making comparisons across groups, creating a bias-variance trade-off.²⁴

To compare treatment effects on different outcome variables, I compute conditional average treatment effects (CATEs) within 46 groups of schools created by collapsing multiple strata based on observed covariates. Each group consists of four schools (two strata). I define these groups by aggregating strata within operator, sorting on the baseline infrastructure index which was originally used to create the randomization strata — i.e., each group consists of schools managed by the same operator, with broadly similar baseline infrastructure. Group-level CATEs for each outcome are therefore less noisily measured than the raw within-stratum differences, while still leaving considerable variation across groups.

These group-level CATEs are still measured with noise, however, leading to bias in the measurement of the covariance of these CATEs. To debias this measure of covariance, I follow the procedure outlined in Walters (2024).²⁵ The first step of this procedure is to run a seemingly unrelated regression (SUR) including two outcome variables for each group:

$$Y_{ig}^k = \sum_g \alpha_g^k G_{ig} + \tau_g^k \cdot (Treatment_i \cdot G_{ig}) + \epsilon_{ig}^k, \quad k \in \{1, 2\}; g = 1, \dots, G \quad (3)$$

In Equation 3, Y_{ig}^k is a measure of outcome k in school i of group g ; $G_{ig} = 1$ if school i belongs to group g ; $Treatment_i = 1$ if school i is treated; and α_g^k represents group-level fixed effects for outcome k . In this application, $K = 2$ and $G = 46$. The τ_g^k s are the coefficients of interest: group- and outcome-specific treatment effects. The error term is ϵ_{ig}^k . The set of treatment effects on outcomes $k \in \{1, 2\}$ in group g can thus be represented as the 2×1 vector τ_g , with the SUR estimation yielding an estimate $\hat{\tau}_g$ of these measures as well as a $K \times K$ sampling variance matrix V_g for each group. The diagonal of matrix V_g contains squared standard errors s_{gk}^2 for each $\hat{\tau}_g^k$, and the off-diagonal elements of this matrix are sampling covariances between $\hat{\tau}_g^k$ and $\hat{\tau}_g^m$ for $m \neq k$.

Following the notation of Walters (2024), the distribution of these treatment effects can be expressed

share and teacher engagement is 0.27 ($p = 0.02$). These relationships are shown in Figure A3.

²⁴Another approach to bias correction, outlined by Lachowska, Mas, Saggio, and Woodbury (2023), uses a leave-out variance estimator — further underscoring the necessity of measuring treatment effects in groups which include more than one treatment unit and more than one control unit.

²⁵See in particular Equation 43 in the section on multivariate empirical Bayes.

as:

$$\hat{\tau}_g \mid \tau_g, V_g \sim \mathcal{N}(\tau_g, V_g), \quad (4)$$

$$\tau_g \mid V_g \sim \mathcal{N}(\mu_\tau, \Sigma_\tau), \quad (5)$$

where Equation 4 characterizes the distribution of sampled treatment effects vectors as a multivariate normal, and Equation 5 represents the distribution of true cross-outcome treatment effects. My main interest for the current exercise is in the $K \times K$ variance matrix Σ_τ , the off-diagonal terms of which describe how treatment effects on different outcomes are related. Deconvolution yields a bias-corrected estimate $\hat{\Sigma}_\tau$ of this matrix, removing excess variability in the $\hat{\tau}_g^k$ s due to both sampling error and noise from estimating covariances across outcomes:

$$\hat{\Sigma}_\tau = \frac{1}{G} \sum_g \left[(\hat{\tau}_g - \hat{\mu}_\tau) (\hat{\tau}_g - \hat{\mu}_\tau)' - V_g \right]. \quad (6)$$

The off-diagonal elements of $\hat{\Sigma}_\tau$ give the debiased covariance between different outcome measures.

I perform this SUR-based debiasing exercise twice: first with $k = 1$ corresponding to incumbent vote share and $k = 2$ corresponding to student test scores; then with $k = 1$ again measuring incumbent vote share while $k = 2$ measures teacher political engagement.²⁶ This allows me to characterize how group-level treatment effects on incumbent vote share covary individually with treatment effects on both student test scores and teacher political engagement.

5.1.2 Comparing CATEs: Results

Incumbent vote share and student test scores: Following the procedure outlined above, the bias-corrected covariance between group-level treatment effects on incumbent presidential vote share and student test scores is 0.002, corresponding to a bias-corrected correlation of 0.15.²⁷ In other words, even after accounting for the noise inherent in estimating treatment effects within relatively small samples, the correlation between group-level treatment effects on incumbent vote share and on student learning is positive but not particularly strong.

²⁶For student test scores I use the same standardized “Composite” measure of student test scores reported in Table 3 of [Romero et al. \(2020\)](#); for teacher political engagement I use the standardized index from Column 6 in Panel B of Table 2.

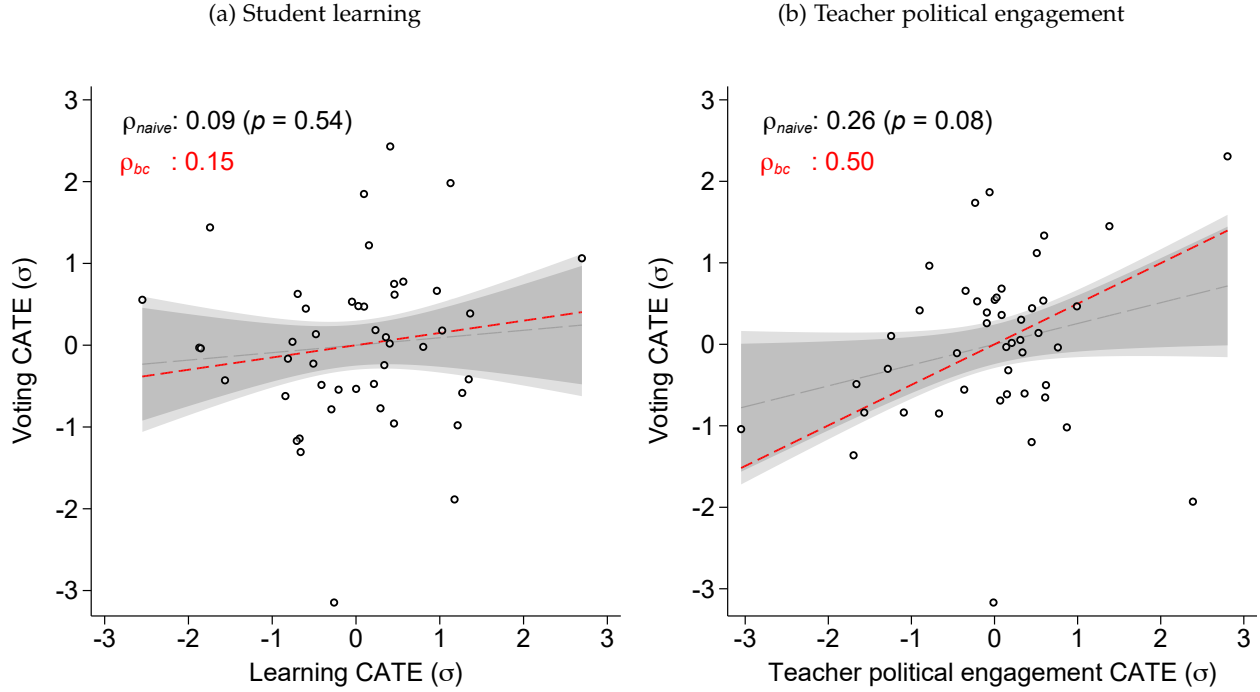
²⁷I convert these bias-corrected covariances into bias-corrected correlations according to the standard formula, $\rho_{bc} = \frac{\text{Cov}_{bc}(X,Y)}{\sqrt{\text{Var}_{bc}(X) \text{Var}_{bc}(Y)}}$.

Incumbent vote share and teacher political engagement: The bias-corrected covariance between group-level treatment effects on incumbent presidential vote share and teacher political engagement is 0.014. This corresponds to a bias-corrected correlation of 0.50 — much larger than the bias-corrected correlation with learning effects. 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.

To visualize these correlations, Figure 4 plots the estimated group-level CATEs (τ_g^k s). Both panels show voting CATEs on the Y-axis; Panel A shows learning CATEs on the X-axis while Panel B shows teacher political engagement CATEs on the X-axis. All τ_g^k s have been standardized for ease of exposition and so the slope of each scatter plot corresponds to the correlation. Each dot represents one of the 46 groups; their dispersion along both axes highlights the large amount of variation in measured group-level treatment effects. Each panel plots in gray the naïve (non-bias-corrected) linear fit and correlation, with confidence intervals at 90% and 95%. The red line in each panel represents the bias-corrected correlation.²⁸ Even before correcting for bias, the correlation in Panel B between incumbent vote share CATEs and teacher political engagement CATEs is positive and significant ($p < 0.1$); the bias correction makes the correlation even stronger. The correlation in Panel A between incumbent vote share CATEs and student learning CATEs, meanwhile, is not statistically significant.

²⁸The naïve covariance between treatment effects on incumbent vote share and student learning is 0.003, with a naïve correlation of 0.09; the naïve covariance between treatment effects on incumbent vote share and teacher political engagement is 0.019, with a naïve correlation of 0.26.

Figure 4: Group-level treatment effects in voting outcomes vs. student and teacher outcomes



Each point on each graph represents one group of four schools, consisting of two randomization strata grouped within operator by baseline infrastructure score: $N = 46$. The seven schools which are not any polling booth's nearest RCT school are excluded. Y-axis shows group-level CATEs in average vote share for the ruling party's presidential candidate. X-axis in panel (a) shows group-level CATEs in standardized student test scores. X-axis in panel (b) shows group-level CATEs in standardized index of teacher political engagement.

The small magnitude of the correlation between effects on voting and learning may suggest that voters in this context do not use elections to enforce accountability for this dimension of school quality (Besley, 2006). Under foundational theories of retrospective voting, elections allow voters to reward or punish government performance (Key, 1966; Fiorina, 1981; Ferejohn, 1986).²⁹ If voters were conditioning their choice partly on school quality, we might expect to see a strong positive correlation between effects on learning and on incumbent vote share. Conversely, if voters strongly opposed investments in improving schools, we might expect a negative correlation.³⁰ Alternatively, if voters simply opposed change due to status quo bias, this might also produce a negative correlation between effects on vote share and test scores, as larger changes to test scores imply larger disruptions to the status quo (Kahneman, Knetsch, &

²⁹On the other hand, this weak correlation could be consistent with sophisticated voters rightly concluding that local variation in policy effectiveness provides little information about the central government which was responsible for the reform (Ashworth & Bueno De Mesquita, 2014).

³⁰The literature includes germane precedent for negative electoral reactions to school investments: in Brazil, poor citizens opposed increased education spending, preferring instead that the government devote the same resources to direct cash transfers (Bursztyn, 2016).

Thaler, 1991; Samuelson & Zeckhauser, 1988). That the correlation between voting and learning effects is positive and small suggests that neither status quo bias nor the canonical formulation of retrospective voting are responsible for the overall observed negative effect on incumbent vote share.

This does not imply that effective reforms cannot deliver electoral benefits. For example, Biasi and Sandholtz (2024) analyze a school reform which did benefit the incumbent politically, though the context differs in many important respects from that of this paper. Perhaps one of the most important differences was that the school reform studied in Biasi and Sandholtz (2024) explicitly took aim at the political power of teacher unions.

The strong correlation between effects on voting and teacher political participation may suggest that teachers' electoral efforts (or lack thereof) played a role producing a negative overall treatment effect on incumbent vote share. Even before considering deliberate political efforts, the role of the mere identity of polling booth staffers in influencing voters has been highlighted by Neggers (2018); I show that the policy significantly reduced the fraction of teachers in treated schools who staffed polling booths. Other work has emphasized the important political effects of organized teacher political activity (Larreguy et al., 2017; Pierskalla & Sacks, 2019). I showed in Section 4 that in the absence of the reform, Liberian teachers tended to be much more supportive of the ruling party than the median Liberian voter. Diminishing this support and disengaging this otherwise supportive constituency may have been what turned the PSL reform into an electoral liability.

Figure A4 repeats this exercise for legislative races, comparing effects on vote share for the ruling party legislative candidate and vote share for the incumbent legislative candidate with effects on teacher political engagement. In both cases, the bias-corrected correlations are small (0.09 and -0.15, respectively). Table 1 found null effects on legislative election outcomes, and the results from this exercise are consistent with teachers' mobilization not affecting vote share for legislative candidates.

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

The results summarized in Figure 4 are consistent with teachers' political engagement playing an important role in voter persuasion. While the negative effect of the reform on incumbent vote share is too large to be entirely explained by teachers' own votes, their political activity may have had an "electoral multiplier" if they persuaded others. To compute a clear estimate of this multiplier, I focus on the subset of 285 polling booths which are within 10km of exactly one school in the sample (Section 6.3 shows that the electoral effects of the policy in this subsample have a similar magnitude and precision as those estimated

on the main analysis sample). Of the 52 schools near these polling booths, the average 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 8). 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.

Table 3 summarizes these calculations. If the treatment had caused each and every teacher to vote against the ruling party, this would explain a meaningful but small share — 8.1% — of the observed electoral treatment effect. The effect could be fully explained, however, by the treatment causing the average teacher to persuade 12.4 voters on net (potentially including herself, spouse, and family members) to vote against the ruling party. That amounts to each teacher persuading 0.4% of the 3385.2 total voters near the average school, or one out of every 273.

Table 3: Teacher vote multiplier necessary to explain full voting effect

Description	Value
Number of polling booths in subsample	285
Average number of teachers per school	13
Average number of booths per school	5.2
Average number of voters per booth	651
Treatment effect on pres. incumbent votes at the booth level	-31 votes
Total voters per school (booths per school \times voters per booth)	3,385.2
Treatment effect on pres. incumbent votes at the school level	-161.2 votes
Share of full voting effect explainable by teacher votes only	$\frac{13}{161.2} = 8.1\%$
Teacher multiplier necessary to explain full voting effect	12.4

Note: The table summarizes calculations based on the subset of 285 polling stations within 10km of exactly one school in the sample, and the 52 schools they are close to. The teacher multiplier reflects the average number of voters each teacher would need to persuade (including, potentially, herself) for teacher persuasion to fully explain the observed electoral treatment effect in this sample.

5.3 Household survey

Survey evidence corroborates the conclusion that the reform’s implementation reduced incumbent support, despite the reform itself being popular. A survey of adult members of the households of treatment and control school students was conducted by phone in October 2017, mere weeks before the election.³¹ 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

³¹The students (and hence the associated household members) comprising the analysis sample in Romero et al. (2020) were drawn from the previous year’s enrollment logs to avoid potential selection bias.

across households from treatment and control schools.³²

Table 4 shows the effects of treatment on these households' survey responses.

Table 4: Household survey outcomes (Oct 2017)

	Heard of PSL	Support PSL	PSL \Rightarrow Learning	PPPs are good	Satisfied w/ legislator	Express voting pref. (pres)	Vote ruling party (pres)
PSL	0.068* (0.036)	0.074** (0.037)	0.088** (0.037)	-0.015 (0.018)	0.106** (0.041)	0.031 (0.039)	-0.098* (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. This suggests that the average negative effects I find were not driven by opposition to the policy *per se*.³³

Another possibility is that experiencing privately-provided public goods is what changed voters' political attitudes and subsequent behavior. Davies (2023), studying a randomized school voucher reform in India, found that attending private schools strengthened households' market-oriented beliefs. In my context, the survey includes a measure of respondents' support for the general idea of public-private partnership, asking whether they agree with the statement “It is good for the government to work with private school companies to provide public education.” I find no treatment effect on this measure, from a very high baseline level: over 90% of both treatment and control households agreed.

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. As such, the data

³²The examination of effects of the policy on household political attitudes was pre-specified in the pre-analysis plan: <https://www.socialscisceregistry.org/trials/1501>.

³³Existing literature also documents that voters may oppose government spending on groups they don't belong to — whether defined by age, caste, or ethnicity (Poterba, 1997; Mukand & Rodrik, 2018; Posner, 2005). This channel is unlikely to explain the overall negative electoral effects in the case of PSL, given that a) the median voter in Liberia is under 30 years old, making them likely to favor educational investments; and b) the program was implemented nationwide, affecting students of nearly all the major ethnicities in Liberia.

suggest it is 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. The positive effect on satisfaction with Representatives is somewhat surprising given the null result on incumbent Representative vote share from Table 1. An alternate identification strategy focusing only on remote schools (where mapping of polling booths to schools is less complicated) does show evidence of positive treatment effects on incumbent Representative vote share, however (see Table 8). It could be the case that these positive results for the households of treated schools reflect the more local nature of the kind of retail politicking typically engaged in by local candidates, and which might be easier to pick up empirically in the sample of booths near remote schools. This may also reflect credit claiming behavior along the lines of [Cruz and Schneider \(2017\)](#) and [Guiteras and Mobarak \(2015\)](#). Indeed, households from treated schools were more likely to report legislative candidates offering to pay voters' school fees.

These household survey results support the idea that the reform reduced incumbent vote share in spite of ordinary voters' general support of 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 political disengagement of teachers.

6 Robustness

Estimating the effect of the school reform on electoral outcomes requires mapping the voting data from polling booths to the treatment assignment data defined at the school level. The main specification outlined in Section 3 does this by associating each polling booth with its nearest sample school, ignoring booths which are more than 10 km from any sample school. In this section I show that the main empirical

results are robust to other mapping choices. First, within the “nearest school” framework employed in Section 3, I vary the distance from sample schools within which booths are included and vary the inclusion of stratum fixed effects. I then present results from an alternate sample of relatively remote polling booths where exposure to treated or control sample schools yields an unambiguous treatment assignment. Across these methods, the results broadly align with those reported in Section 4.

6.1 Robustness checks: binary measure of treatment

The main specification in Equation 1 and the results in Table 1 define polling booths’ treatment status as a binary variable, using the treatment status of the booth’s nearest school, limiting attention to booths within 10 km of their nearest school, and including randomization stratum fixed effects based on the stratum of this nearest school. This subsection tests the robustness of those estimates to changing this radius as well as to dropping the randomization stratum fixed effects.

6.2 Alternate radii

The main results presented in Table 1 give polling booths the treatment status of their nearest RCT school, and consider only booths within 10 km of an RCT school. Table 5 shows results on the main outcome variable — ruling party vote share in the presidential runoff – varying the radius used to exclude polling booths from the sample. Column 1 considers only booths within 1 km of the nearest RCT school; the remaining columns do the same for distances of 2 km, 3 km, 5 km, and 10 km (where this last column corresponds to Column 1 of Panel B in Table 1). At each radius, point estimates are negative.

Table 5: Effect on ruling party presidential vote share, varying radius

	1km	2km	3km	5km	10km
Treatment	-0.058*** (0.019)	-0.040** (0.017)	-0.041*** (0.013)	-0.034*** (0.012)	-0.032** (0.012)
N	169	311	450	772	1200
Mean (control)	0.354	0.389	0.396	0.407	0.407

Outcome variable is ruling party vote share in the presidential runoff election. Standard errors clustered by polling booth’s nearest school. Regressions include polling booths from the 2017 election located within km of any school in the RCT; booth treatment assignment is that of nearest PSL school. Randomization strata fixed effects corresponding to the booth’s nearest school are included. Missing outcome values have been replaced with zero, and indicator variables for whether values are missing have been included in all regressions. ‘Mean (control)’ displays the mean of the dependent variable for polling booths whose nearest RCT school within the specified radius are control school. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 6 shows the results from the main specification in Equation 1, omitting stratum fixed effects. Although estimates are less precise than those in Table 1, they are not qualitatively different.

Table 6: Effect of school reform on electoral outcomes (without stratum fixed effects)

	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	-0.046* (0.026)	-0.024 (0.026)	-0.043 (0.027)	-0.007 (0.007)	-0.000 (0.019)	-0.003 (0.033)	-0.006 (0.007)
N	1202	1202	1202	1202	1202	1202	1202
Mean (control)	0.409	0.593	0.306	0.771	0.176	0.209	0.761

Standard errors clustered by polling booth's nearest school. Regressions include polling booths from the 2017 election located within 10km of any school in the RCT; booth treatment assignment is that of nearest PSL school. No randomization strata fixed effects are included. Missing outcome 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 7 shows results on presidential vote share in the runoff election without stratum fixed effects, varying the radius used to determine each polling booth's inclusion in the sample. Column 1 limits attention to polling booths within 1km of their nearest RCT school, while Column 5 uses the 10km radius from Table 6. Estimates are consistently negative, though statistical significance varies.

Table 7: Effect on ruling party presidential vote share, varying radius (without stratum fixed effects)

	1km	2km	3km	5km	10km
Treatment	-0.027 (0.041)	-0.059 (0.039)	-0.069** (0.034)	-0.050* (0.028)	-0.046* (0.026)
N	210	341	476	779	1202
Mean (control)	0.384	0.407	0.412	0.409	0.409

Outcome variable is ruling party vote share in the presidential runoff election. Standard errors clustered by polling booth's nearest school. Regressions include polling booths from the 2017 election located within km of any school in the RCT; booth treatment assignment is that of nearest PSL school. No randomization strata fixed effects are included. Missing outcome values have been replaced with zero, and indicator variables for whether values are missing have been included in all regressions. 'Mean (control)' displays the mean of the dependent variable for polling booths whose nearest RCT school within the specified radius are control school. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

6.3 Remote schools

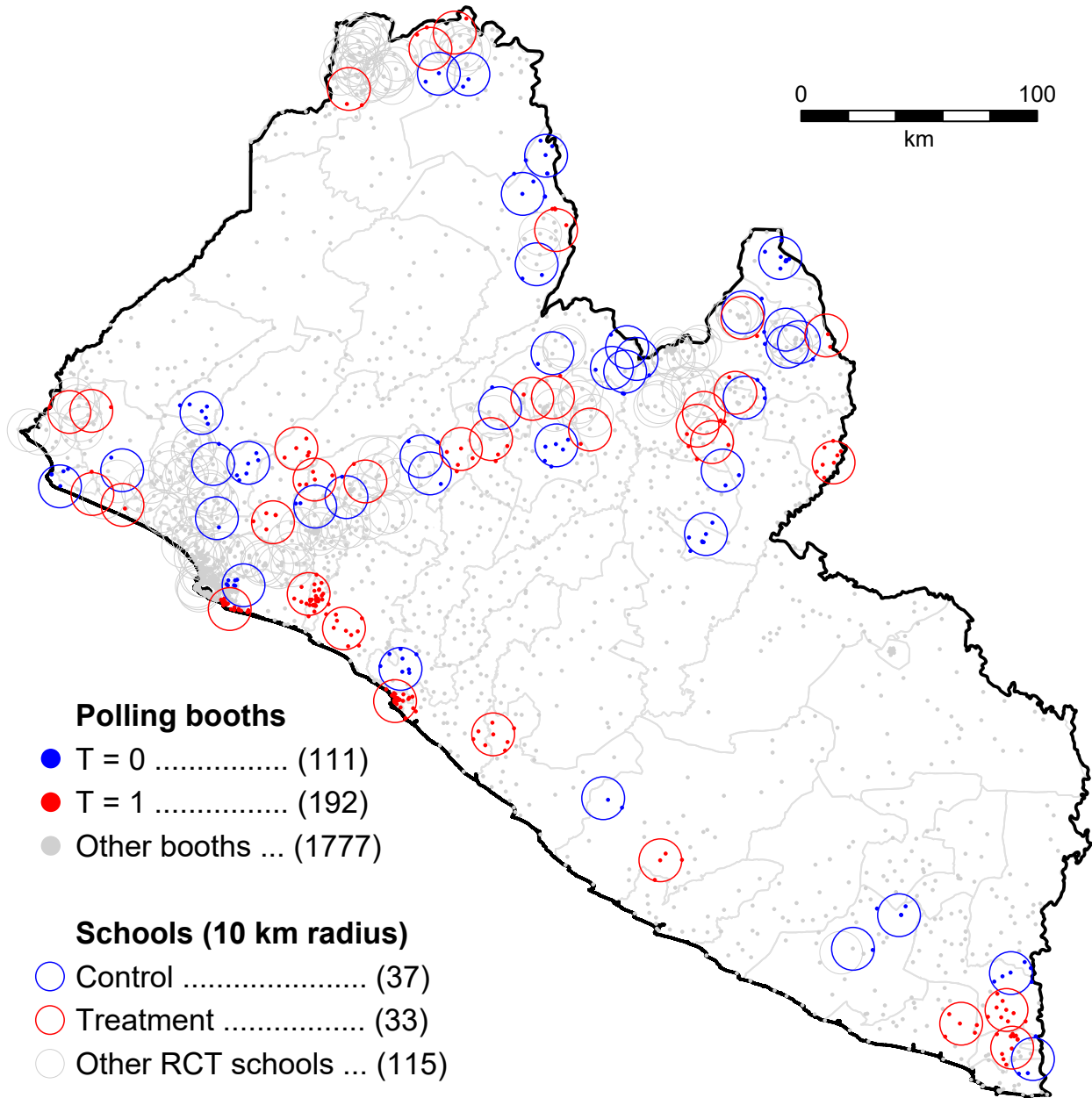
An alternative empirical approach is to limit attention to a subsample for which treatment definition is unambiguous: booths near remote schools. Intuitively, if a polling booth is near only one school in the

RCT, then there is no uncertainty over how to assign treatment to it, nor any expected bias in whether that school was randomly assigned to treatment or control. This allows the econometrician to abstract away from the problem of spillovers. This method makes concessions to external validity in the service of strengthening internal validity: it is certainly true that polling booths which are near exactly one RCT school are different from the average polling booth in Liberia. However, when assessed in conjunction with the other approaches presented in this paper, this analysis can offer a useful perspective.

Figure 5 illustrates this sample: blue and red dots depict the polling booths which lie within 10 km of exactly one control or treatment school respectively, with the circles representing the radii around the relevant schools. Gray dots represent booths which do not fit this criterion for treatment assignment; gray circles represent the schools which are no booth's only nearby school.

Table 8 shows results from estimating Equation 1 on a sample of polling booths which are located within 10 km of exactly one RCT school, assigning each booth that school's treatment status. Panel A checks balance on 2011 election outcomes; Panel B presents main results on 2017 election data; and Panel C shows results on 2017 election data controlling for 2011 election outcomes.

Figure 5: Map — Remote school sample



Map showing polling booths which lie within 10 km of exactly one RCT school, and 10-km radii around the 70 RCT schools close to these booths. Other schools and polling booths are colored gray. Internal boundaries demarcate the 73 electoral districts of the House of Representatives constituencies.

Table 8: Effect of school reform on electoral outcomes (remote school sample)

	President		President: First round		Legislative		
	Vote share:		Vote share:		Vote share:	Vote share:	
	Ruling party	Turnout	Ruling party	Turnout	Ruling party	Incumbent	Turnout
Panel A — Balance: 2011 Election Outcomes							
Treatment	0.019 (0.015)	0.011 (0.020)	0.008 (0.026)	0.012 (0.010)	0.004 (0.025)	0.040 (0.052)	0.013 (0.009)
N	285	285	285	285	285	285	285
Mean (control)	0.733	0.384	0.376	0.631	0.177	0.284	0.625
Panel B — Main Results: 2017 Election Outcomes							
Treatment	-0.049** (0.024)	0.048*** (0.015)	-0.041 (0.033)	0.021 (0.021)	-0.026 (0.027)	0.106** (0.050)	0.030* (0.016)
N	285	285	285	285	285	285	285
Mean (control)	0.402	0.515	0.310	0.769	0.149	0.157	0.755
Panel C — Robustness: 2017 Election Outcomes With Controls							
Treatment	-0.056*** (0.020)	0.041*** (0.015)	-0.042 (0.027)	0.021 (0.017)	-0.028 (0.028)	0.102** (0.047)	0.029* (0.015)
N	285	285	285	285	285	285	285
Mean (control)	0.402	0.515	0.310	0.769	0.149	0.157	0.755
Controls	✓	✓	✓	✓	✓	✓	✓

Standard errors clustered by polling booth's nearest school. Regressions include polling booths from the 2017 election located within 10km of exactly one school in the RCT; booth is given treatment assignment of that school. Missing outcome values have been replaced with zero, and indicator variables for whether values are missing have been included in all regressions. Controls included are the 2011 election values for the number of registered voters; total votes cast in each race; turnout in each race; and ruling party vote share in each race. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Results are largely consistent with those presented in Table 1. Outcomes from the 2011 election are well-balanced. Column 1 in Panel B shows that treatment reduced incumbent party presidential vote share in the runoff by 4.9 percentage points, with similarly large but noisier effects on first round voting. In this sample, treatment also appears to have increased turnout, both in the presidential runoff and the legislative election; legislative incumbents also appear to have benefited considerably, consistent with results from the household survey in 4.

Table 9 varies the radius drawn around each polling booth, with the sample defined in each case as polling booths for which exactly one RCT school falls within the radius. Point estimates are negative at most radii, and significant both at 1 km and 10 km.

Table 9: Effect on ruling party presidential vote share, varying radius (remote school sample)

	1km	2km	3km	5km	10km
Treatment	-0.060*** (0.021)	-0.010 (0.032)	-0.026 (0.023)	0.002 (0.020)	-0.049** (0.024)
N	145	172	215	324	285
Mean (control)	0.388	0.401	0.379	0.364	0.402

Standard errors clustered by nearest RCT school. Fixed effects included for the randomization stratum of 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. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

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; Ferreira & Sandholtz, 2024).³⁴ 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; Leff-Yaffe et al., 2023). But there is much less evidence on how voters – and public service providers – respond to interventions aimed at raising the quality of public services such as education which may be hard to observe (Harding & Stasavage, 2014; Biasi & Sandholtz, 2024; Armand, Mendonça, Sandholtz, & Vicente, 2025). 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

³⁴Governments have undemocratic reasons to expand educational access as well (Paglayan, 2020, 2022).

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).³⁵ Policymakers who seek to improve public service delivery often face the difficult task of shaking up entrenched systems full of committed supporters. They may lack credible evidence to predict how a given intervention is likely to work in their context (Pritchett & Sandefur, 2014). Meanwhile, they can be confident that any change will provoke opposition from those who benefit under the status quo (Fernandez & Rodrik, 1991). In these circumstances, clientelism or vote-buying may provide a less risky path to electoral victory than investing in public goods and services (Wantchekon, 2003; Cruz, Keefer, Labonne, & Trebbi, 2018).

The paper also shows, however, that voters pay attention to service quality. In Liberia – where democracy is young and literacy is around 50% – voters exhibit some sophistication in their attribution of credit and blame for an important and controversial school reform. Electoral rewards for the policy were positively (if weakly) correlated with its effectiveness: voters tended to reward the responsible politician where the policy worked well, and punish him where it worked poorly. But implementation varied widely from school to school. This suggests that reforms which are more effective at improving service quality will also be more politically feasible, *ceteris paribus* — though *ceteris paribus* may be quite a strong condition if angering providers is an unavoidable cost of improving quality. More credible and targeted policy evidence could reduce uncertainty, thereby lessening the cost for politicians to shift 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.³⁶ 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

³⁵Other 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).

³⁶Acemoglu (2010): “Political economy refers to the fact that the feasible set of interventions is often determined by political factors and that large counterfactuals will induce political responses from various actors and interest groups. . . . Although research in this area is expanding, given the importance of political economy for the problems of development, it remains surprising how few papers investigate key political economy channels using micro-data and careful empirical strategies.”

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 at scale without being politically implementable.

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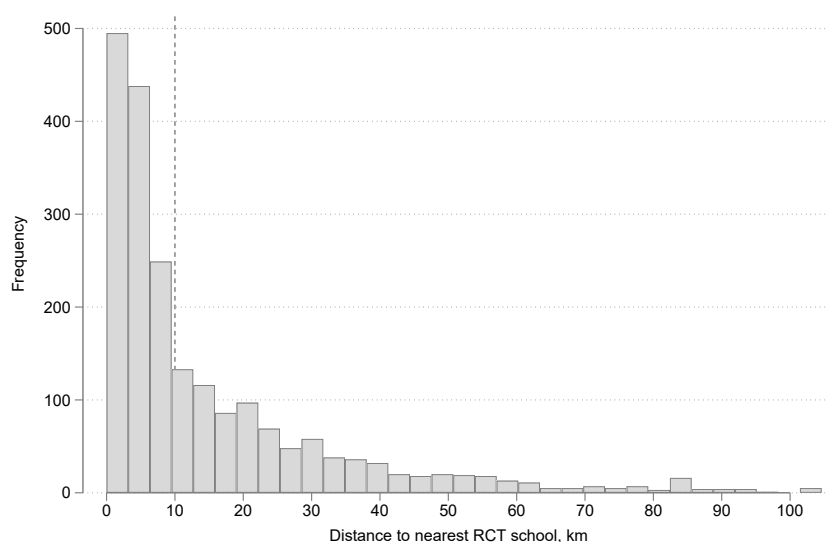
Appendix

A Additional tables and figures

A.1 Distance of polling booths to schools in the RCT

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.

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



Universe of 2080 polling booths from 2017 general election.

Considering instead the distance of RCT schools to polling booths, all 185 schools are within 10km of at least one polling booth; 98% are within 5km; 86% are within 3km; and 56% are within 1km.

A.2 Additional balance checks

Table A1 checks for balance on school density. The dependent variable in each column is the number of schools of a given type located within a 10-km radius of the polling booth. Polling booths whose nearest RCT school is treated exhibit no significant differences vs. control booths in terms of how many RCT

schools are nearby, how many public primary school in total are nearby, and how many public schools overall are nearby.

Table A1: Additional balance checks: School density

	RCT schools		Public primary schools		All public schools	
Treatment	-.503 (.356)	-.693 (.689)	-1.74 (1.36)	-2.93 (3.65)	-2.22 (1.56)	-3.71 (4.38)
N	1200	1202	1200	1202	1200	1202
Mean (ctrl)	4	4	19	19	21	21
Stratum FE	✓		✓		✓	

Standard errors clustered by polling booth's nearest school. Regressions include polling booths from the 2017 election located within 10 km of any school in the RCT; booth treatment assignment is that of nearest PSL school. Randomization strata fixed effects corresponding to the booth's nearest school omitted in odd columns and included in even columns. Missing outcome 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 A.2 provides additional balance checks, estimating Equation 1 on other 2011 election outcomes, both with and without including stratum fixed effects corresponding to the booth's nearest school: number of registered voters; number of votes cast in the presidential first and second round and the legislative race; and the share of invalid votes cast in each of the three races. Of the 14 tests here, only one shows a significant difference (the share of invalid votes in the first round of the 2011 presidential election was 0.6 percentage points higher in booths near treated schools).

Table A2: Additional balance checks: 2011 election outcomes

	Registered voters		Total votes pres. runoff		Total votes pres. 1st rd		Total votes leg.		Share invalid pres. runoff (0-100)		Share invalid pres. 1st rd (0-100)		Share invalid leg. (0-100)	
Treatment	142 (90)	47 (136)	30 (39)	-4.6 (48)	95 (65)	26 (105)	92 (64)	25 (103)	-.042 (.17)	-.045 (.32)	.6*** (.19)	1 (.69)	.012 (.21)	.29 (.4)
N	1200	1202	1200	1202	1200	1202	1200	1202	1200	1202	1200	1202	1200	1202
Mean (ctrl)	1038	1038	403	403	765	764	755	755	3.6	3.6	5.2	5.2	5.9	5.9
Stratum FE	✓		✓		✓		✓		✓		✓		✓	

Standard errors clustered by polling booth's nearest school. Regressions include polling booths from the 2017 election located within 10 km of any school in the RCT; booth treatment assignment is that of nearest PSL school. Randomization strata fixed effects corresponding to the booth's nearest school omitted in odd columns and included in even columns. Missing outcome values have been replaced with zero, and indicator variables for whether values are missing have been included in all regressions. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

A.3 Effects on invalid votes

Table A3: Effect of school reform on invalid votes

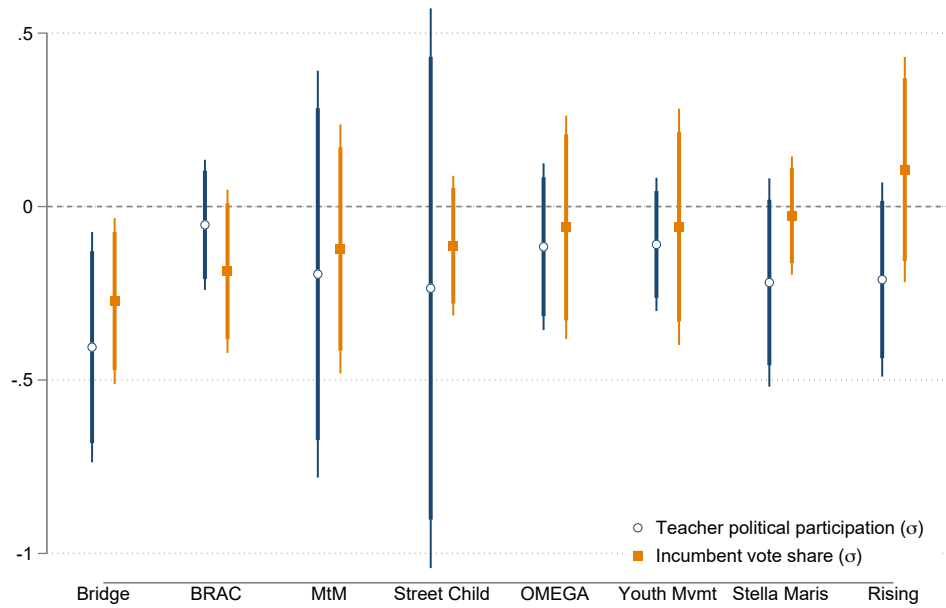
	President: Runoff	President: First round	Legislative
Treatment	0.002** (0.001)	0.001 (0.002)	0.001 (0.002)
N	1200	1200	1200
Mean (control)	0.022	0.050	0.049

Standard errors clustered by polling booth's nearest school. Regressions include polling booths from the 2017 election located within 10 km of any school in the RCT; booth treatment assignment is that of nearest PSL school. Randomization strata fixed effects corresponding to the booth's nearest school are included. Missing outcome values have been replaced with zero, and indicator variables for whether values are missing have been included in all regressions. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

A.4 Effects by operator

Figure A2 shows effects on incumbent vote share in the runoff election alongside effects on the index of teacher political engagement (both standardized), separately for each school operator. Although sample size issues make it difficult to distinguish estimates from one another statistically, the point estimates on both outcomes are largest — and only individually significant — for the largest and most controversial operator, Bridge.

Figure A2: Treatment effects by operator



Effects on vote share (using Equation 1 with no controls) and teacher political participation, separately by private school operator.

A.5 Additional effects on household survey responses

Table A.5 shows effects on other household survey responses. Column 1 records whether respondents agreed with the statement “Foreign NGOs have too much control over Liberia”; Column 2 records whether respondents agreed with the statement “Liberian tax money should pay for Partnership Schools”; Column 3 records whether respondents could accurately identify the name of their district’s legislator in the House of Representatives; Column 4 records whether the respondent reports having attended at least one campaign event for any candidate for Representative.

The treatment had no statistically significant effect on any of these measures, but the means are instructive. Nearly half of respondents agree there is too much influence of foreign NGOs in the country, and nearly four-fifths agree that Liberian tax money should pay for PSL. Voters appear politically involved and knowledgeable: 88% know their Representative’s name, and 35% have attended a campaign event.

Table A4: Additional effects on household survey responses

	Foreign NGOs have too much control	Liberian tax money should pay for PSL	Knows Rep's name	Has attended campaign event for any Rep.
PSL	-0.005 (0.044)	-0.008 (0.036)	-0.000 (0.022)	-0.060 (0.037)
N	489	489	489	489
Mean (control)	0.485	0.791	0.879	0.348

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

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

A.6 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 54 RCT schools are polling booths (as measured by comparing the names of schools in the RCT to names of polling stations).

A.6.1 Treatment did not make schools polling stations

Table A5 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 A5: Treatment did not make schools polling stations

	Name	Strict name	km 0.25	km 0.10	km 0.05
Treatment	-0.004 (0.062)	-0.032 (0.052)	-0.018 (0.064)	-0.025 (0.046)	-0.036 (0.036)
N	185	185	185	185	185
Mean (control)	0.261	0.185	0.272	0.120	0.087

School matched-pair fixed effects included. Outcome variable is whether the school is a polling booth (defined in columns 1-2 as whether the school shares a name and location with the polling booth, and in columns 3-5 as whether the distance from school to polling booth GPS coordinates is within the radius at the top of the column).

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

A.6.2 Differential electoral effects in polling booths which are RCT schools

Table A6 interacts treatment with a dummy for whether the polling station is one of the 54 polling stations (29 control, 25 treatment) which are located at the RCT school itself. Among this small number of polling stations located in experimental schools, the treatment effect on incumbent vote share in the presidential runoff election is positive. This could reflect voters' reaction to the salience of school improvements, as shown in the work of [Ajzenman and Durante \(2023\)](#).

Table A6: 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	-0.025** (0.010)	-0.011 (0.008)	-0.018** (0.009)	-0.006 (0.006)	0.014 (0.011)	-0.006 (0.020)	-0.005 (0.006)
Booth is RCT school	-0.015 (0.020)	0.009 (0.019)	0.000 (0.022)	0.006 (0.020)	-0.006 (0.030)	-0.044** (0.021)	0.006 (0.018)
Booth is Treatment × RCT school	0.077** (0.037)	0.017 (0.023)	0.044 (0.035)	-0.003 (0.024)	0.011 (0.044)	0.002 (0.036)	-0.007 (0.022)
N	1200	1200	1200	1200	1200	1200	1200
Mean (control)	0.407	0.593	0.305	0.771	0.174	0.208	0.761

Standard errors clustered by nearest RCT school; 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 the treatment status of its nearest RCT school. 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

A.6.3 Treatment did not affect teacher political activity differently at polling stations

Table A7 shows that treatment did not affect teachers' political behavior differentially at the 54 RCT schools which were polling stations.

Table A7: 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** (0.017)	-0.044 (0.031)	-0.052* (0.027)	-0.020 (0.032)	-0.094* (0.049)	-0.236** (0.109)
School is polling booth	-0.010 (0.024)	-0.072* (0.041)	-0.080* (0.044)	-0.021 (0.039)	-0.147** (0.063)	-0.260 (0.159)
Treatment × School is polling booth	-0.005 (0.036)	-0.027 (0.061)	0.130* (0.069)	-0.080 (0.064)	-0.009 (0.102)	0.099 (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

A.7 More results on teachers

A.7.1 Effect on components of job satisfaction

Table A8: Effect on components of job satisfaction

	Fraction 'very' or 'somewhat' satisfied with . . .							
	Salary	Job security	Job location	Support from parents/ cmtty	Support from govt	Support from principal	Working hours	Teaching inputs/ resources
Treatment	0.000 (0.038)	-0.049 (0.031)	-0.070** (0.030)	-0.024 (0.036)	0.042 (0.039)	-0.038* (0.022)	-0.219*** (0.030)	0.070* (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

A.7.2 Effects on teacher time use

Table A9 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 A9: 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*** (0.166)	0.047 (0.033)	0.073 (0.064)	-0.000 (0.055)	0.055 (0.085)	-0.086 (0.057)	1.139*** (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. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

A.7.3 Other effects on teachers

Table A10 shows other effects of the policy on teachers' (self-reported) experiences. Salaries went up (but not commensurately with hours worked, as shown in Table 2). 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 A10: Effect of reform on teachers' experience

	Monthly Salary (USD)	On govt payroll	Has other job	Express any vote preference
Treatment	11.355*** (3.316)	0.071** (0.035)	-0.089*** (0.029)	0.012 (0.031)
N	883	883	883	883
Mean (control)	93.692	0.531	0.241	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.
* p<0.10, ** p<0.05, *** p<0.01

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.³⁷ 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.³⁸ However, it appears to have carried a political cost.

A.7.4 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 A11 shows the effect of treatment, interacted with union status, on various aspects of teacher's jobs. These interactions should be thought of as exploratory, and interpreted with caution: as Table A10 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

³⁷It 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.

³⁸For 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).

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 A11 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 A11: Effect of reform on teacher attitudes, by union membership

	Gov payroll	Monthly Salary (USD)	Has other job	Expressed voting pref.	Vote for ruling party pres. cand.
Treatment	0.153*** (0.041)	23.621*** (3.656)	-0.118*** (0.042)	0.047 (0.042)	-0.009 (0.032)
Union member	0.416*** (0.059)	33.689*** (5.227)	-0.038 (0.058)	0.154** (0.064)	0.123** (0.048)
Treatment × Union member	-0.163** (0.075)	-26.802*** (6.784)	0.145** (0.073)	-0.106 (0.084)	-0.128** (0.062)
Treat + Treat × Union (SE)	-0.009 (0.063)	-3.181 (5.929)	0.028 (0.057)	-0.059 (0.076)	-0.137*** (0.055)
N	764	764	764	764	764
Mean (control)	0.539	91.298	0.236	0.633	0.633

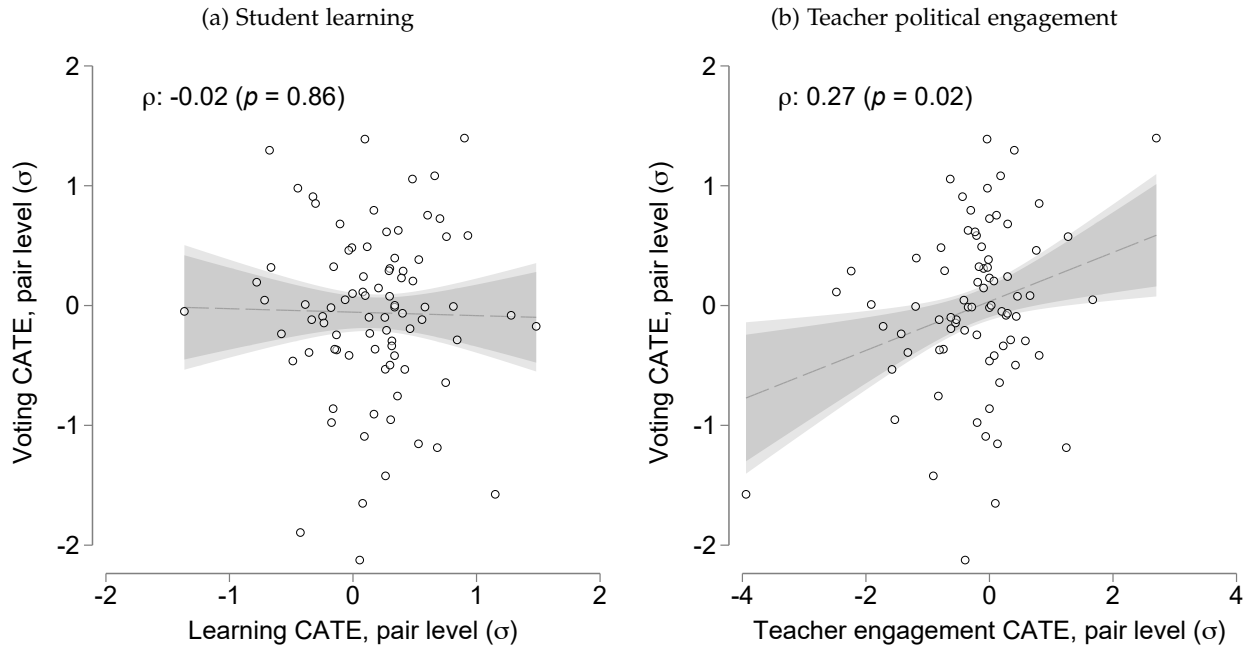
Standard errors clustered by school. School matched-pair fixed effects included. Outcomes from a May/June 2017 survey. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

A.8 More comparisons of treatment effects on different outcomes

A.8.1 School pair-level correlations in treatment effects on different outcomes

Figure A3 depicts simple treatment-control differences within randomization strata, each consisting of one treatment and one control school. The y-axis depicts treatment-control differences on ruling party vote share in the presidential runoff election. The x-axes show treatment-control differences on student test scores and teacher political engagement.

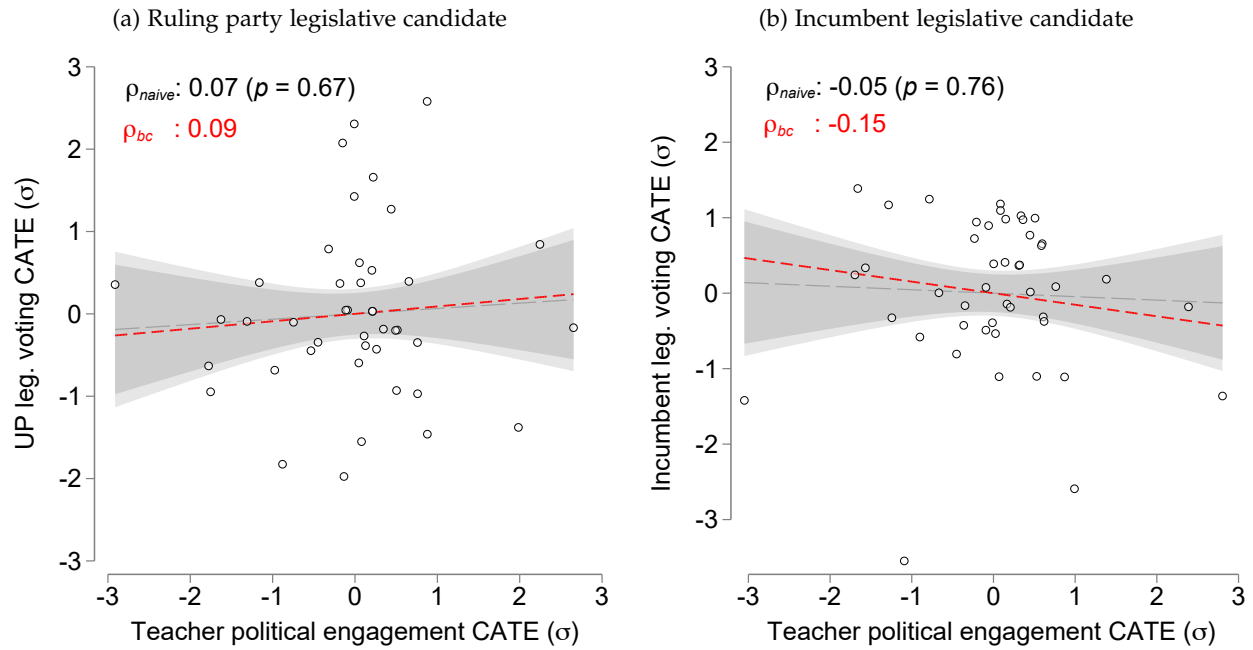
Figure A3: Pair-level treatment effects in voting outcomes vs. student and teacher outcomes



Each point on each graph represents one pair (randomization stratum) of schools: $N = 78$. The pairs containing the seven schools which are not any polling booth's nearest RCT school are excluded. Y-axis shows pair-level treatment-control differences in average standardized vote share for the ruling party's presidential candidate. X-axis in panel (a) shows pair-level treatment-control differences in standardized composite student test scores. X-axis in panel (b) shows pair-level treatment-control differences in standardized index of teacher political engagement.

A.8.2 Group-level correlations in treatment effects on legislative voting and teacher outcomes

Figure A4: Group-level treatment effects in legislative voting outcomes vs. teacher outcomes



Each point on each graph represents one group of four schools, consisting of two randomization strata grouped within operator by baseline infrastructure score: $N = 46$. The seven schools which are not any polling booth's nearest RCT school are excluded. Y-axis in panel (a) shows group-level CATEs in average vote share for the ruling party's legislative candidate. Y-axis in panel (b) shows group-level CATEs in average vote share for the incumbent legislative candidate (regardless of party). X-axis in both panels shows group-level CATEs in standardized index of teacher political engagement.

B Conceptual framework

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

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

B.1 Setup of the model

This framework builds on the model of Lizzeri and Persico (2004), which is built in turn on the model of redistributive politics of Lindbeck and Weibull (1987) and Dixit and Londregan (1996). Lizzeri and Persico (2004) consider the extension of the franchise, concluding that it induces parties to promise more public goods and fewer targetable transfers. In reality, public good provision remains low (and clientelism common) even in many parts of the world with universal suffrage. I modify Lizzeri and Persico's model in a way consistent with one explanation for this fact: politicians may employ civil servants both to provide public services *and* to engage in targetable transfers and other forms of direct persuasion. For simplicity, I abstract away from redistribution here, focusing only on public good and service provision. Monetary transfers likely play a large role in politicians' competition for votes in reality, not least through the channel of direct campaign vote-buying. But because I consider the case in which the money to increase

public good provision does not come from taxes but from outside donations, I hold direct monetary redistribution constant and focus instead on the human resource side of patronage politics. I also allow the electioneering efforts of bureaucrats to influence the voting decisions of voters.

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

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

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

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).

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 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

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

⁴¹I 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.

⁴²An 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.

⁴³In 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 .

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 :

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

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

$$S_R = n_0 \cdot F_0 \left[U(g(I_R, A_R)) - U(g(I_C, A_C)) + h(n_1, E) \right] \\ + n_1 \cdot F_1 \left[U(g(I_R, A_R) - f(A_R)) - U(g(I_C, A_C) - f(A_C)) \right]. \quad (7)$$

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

$$\max_A S_R$$

subject to

$$A + E = 1$$

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

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

B.3 Model predictions

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

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

B.4 Discussion of the model

The purpose of the model is to illuminate a politician's decision about A , that is, how much to direct civil servants to focus on public services rather than direct electioneering. The sign of $\frac{dS_R}{dA}$ is theoretically

ambiguous, and depends on the relative elasticities $\frac{dS_R}{dg} > 0$ and $\frac{dS_R}{dE} > 0$. Essentially, increasing A will only increase overall vote share if its positive effect on all voters through better public goods is bigger than its negative effect on bureaucrats through increased effort PLUS its negative effect on voters through reduced E . This offers one potential rationalization of the existing empirical literature's varied findings on the effects of public service provision on vote share.

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

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

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