Public Information is an Incentive for Politicians: Experimental Evidence from Delhi Elections

Abhijit Banerjee, Nils Enevoldsen, Rohini Pande and Michael Walton[*]

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Abstract

In 2010, two years prior to Delhi's municipal elections, we informed a random set of municipal councilors that a newspaper would report on their performance just before the next election. To evaluate this intervention we collected data on the infrastructure spending preferences of slum dwellers and created an index of pro-poor spending for each councilor. In wards dense with slums, the anticipation of future disclosures caused councilors to increase their pro-poor spending by 0.6 standard deviations over the next two years. A cross-cutting intervention that privately provided councilors with information about the state of infrastructure in the slums had no effect, suggesting that only public disclosures incentivize councilors. Party and voter responses support this interpretation: treated councilors were 12 percentage points more likely to receive a party ticket for re-election. The effect was concentrated among councilors who undertook more pro-poor spending in high-slum wards, and translates into a substantially higher vote share.

1   Introduction

Why are policy choices in lower-income democracies often unresponsive to the development preferences of their electorates? Is it because politicians expect voters to remain ignorant of their performance record? Or is it because the votes of poor citizens, even when they are informed and constitute a majority, are largely determined by ethnic loyalties and vote-buying ploys?

Answers to these questions are critical for understanding which institutional investments are most effective in making democracies work for their citizens. In this paper, we use a set of field experiments, conducted at scale in one of the world's most populous cities, to demonstrate that a

credible promise of revealing performance information to voters makes politicians more responsive to voter preferences.It also makes voters' electoral choices more responsive to politician performance. Finally, it alters the pool of politicians: parties start weeding out those with the weakest records.

Our field experiments occurred in the context of Delhi's Municipal Corporation, an elected local body. A councilor enjoys multiple policy levers. She receives an annual discretionary development fund to spend on local development, typically improvement of local infrastructure. Committees on which she sits manage the distribution of publicly provided private goods and public goods. More broadly, she is expected to be responsive to demands of her own constituents, and constituents often visit her offices to demand better services and other help.

Our primary intervention was conducted at scale, covering 240 of Delhi's 272 wards. In each ward, citizens elect a single member for a five year term. In 2010, two years prior to Delhi's municipal elections, a random sample of incumbent councilors were informed that a leading Hindi newspaper would report on their performance a month before the next election. For a randomly chosen subset of treated councilors the newspaper also published midterm report cards, identical in structure to the final report cards, in May–June 2010. This potentially enhanced the credibility of our primary intervention. It also allows us to examine performance impacts of early information due, for instance, to informing citizens about what to ask for or by making performance indicators salient to the councilors who were reported on.

Policy preferences, especially over local infrastructure spending, will vary by income and other neighborhood characteristics. Prior to the 2010 information campaign, we surveyed slum-dwellers in over 100 high-slum density wards on their spending preferences in order to obtain preference weights for different items in the infrastructure budget. Our focus on slum-dwellers had multiple motivations. First, slum infrastructure investments – for instance, building and repairing drains – are relatively inexpensive high-return investments for councilors. Second, unlike other administrative funds that can only be used in legal settlements, councilor discretionary funds can be invested in slums. Third, given our own budget constraints, collecting preference data from low-income communities was easier and more cost-effective. Finally, our sampling choice reflected our interest in examining whether anticipated performance disclosures can make democratic governments more accountable to relatively poor citizens.

We document a large divergence between slum dwellers' infrastructure priorities and councilors' spending choices. Nearly 70% of our respondents describe sanitation (sewage and drainage) as a concern, but only 16% of councilor funds are spent on it, against 54% on roads, despite the fact that only 2% of our respondents complain about roads. Given this disparity, we hypothesize that at least for high-slum wards, defined as wards where the fraction of slum area is above the median value computed across all wards (45%), anticipated public disclosures of spending patterns should incentivize councilors to move spending in a more pro-poor direction.

By checking for similarity of effects across councillors who also got the midterm report cards and

those who only received the letter informing them of the future reportcard, we gain some information on whether an incentive effect is at work. Of course, one could argue that those who did not get the midterm report card might have created their own report cards and been surprised by them. Any movement in the pro-poor direction could then just be the effect of better information about the mismatch between the ward's needs and the councilor's spending.

To directly evaluate the effect of providing the councilors information about the needs of their constituents, we implemented a second cross-cutting experiment within the set of high-slum wards. Motivated by the central importance of sanitation problems in the slums,[1] we provided a random sample of councilors with State of Sanitation Information (SSI) for their wards. This was based on our own audits, and included geo-located information on quality of public toilets, sewers, and garbage removal in three slums. The information was collected and disseminated twice – eight months and two months prior to the election.[2] The information was never made public, and allows us to ask whether informing councilors about sanitation issues – something the report cards might have done as well – influences their performance.

We have three sets of findings.

First, anticipated public performance disclosures led councilors in high-slum wards to make infrastructure investment decisions that were 0.62 standard deviations more pro-poor. We also observe a 0.37 standard deviation increase in assembly and committee attendance in this group.

The estimated treatment effects are similar across councilors who only anticipated disclosure in 2012 and those who additionally received a midterm report card in 2010. This argues against any additional value from early information associated with the 2010 report card.

Second, consistent with the absence of a salience or information effect from the midterm report cards, but even more striking, actionable information provided via the private SSI intervention did not improve sanitation in slums. Why would councillors react to the newspaper report cards in order to please the voters, but not to the audits?

One possibility is that the set of voters who directly benefit from the sanitation improvements (the slum residents) is too small to be politically relevant – perhaps because there are no credible mechanisms by which this performance information would reach a broader voting public. The performance report cards, in contrast, have an amplified effect due to publication in a newspaper.

The alternative possibility is that voters put little weight on policies and, instead, focus on candidate's ethnic identity or money provided by parties on the election eve. In this case, newpaper report cards matter because it provides party selection committees with performance information which they use to allocate party tickets.

To evaluate these explanations, we turn in our third set of findings on how the the treatment impacts party ticket allocation and, subsequently, voter behavior. In the 2012 elections, ticket

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[1]Other than water, which is outside the jurisdiction of the councilors.

[2]The earlier round was intended to give the councilor time to act, the latter to see if the urgency created by the imminent election prompts action.

allocation gained greater salience due to an unanticipated expansion in gender quotas for councilor seats. In January 2012, the government announced that the number of wards reserved for women in the April 2012 elections would increase from 33% to 50%, with the choice of reserved wards being randomized. This created a randomly chosen set of 80 incumbents (from our sample of 240 incumbents) who were ineligible for election in their current ward (now on ineligible councillors). Their parties had to decide whether to allocate them party tickets to other (unreserved) wards.

Media disclosures influenced party ticket allocation. Treated incumbents were 12 percentage points more likely to run for re-election. The treatment effect captured the ticket allocation to ineligible councilors with a more pro-poor spending record. Specifically, ineligible treated incumbents with a pro-poor spending a standard deviation above their counterparts were 13 percentage points more likely to get a party ticket for a different ward. Further, this effect was concentrated among ineligible incumbents in high-slum wards (which is where slum-spending is the most valued) who are moved to other high-slum wards. These ticket allocation effects translate into a higher vote share. (Naturally one has to run to gain votes, but running does not guarantee winning). Treated ineligible incumbents with a pro-poor spending record that is one standard deviation above their counterparts are 23 percentage points more likely to win in the next election.

Newspaper report cards may have independent *advertisement* effects over and above any information effect. For one, publication in a newspaper makes the information more credible.Even the voters who probably did not have the full information may be subject to an advertizement effect – they may react to the fact, for example, that the information about the candidates (that they perhaps already knew) is now common knowledge. A pure advertising interpretation is also consistent with the lack of effect of private disclosures – the parties and politicians have the relevant information, they only care about convincing the voters. It would also explain why this effect is more important for ineligible candidates – they are less known in their new wards.

Our intervention, however, does not let us evaluate the relative importance of a pure information versus an advertizement effect. What we can say is that pro-poor spending was popular when it was credibly documented and therefore the impact of the intervention goes beyond just the reassignment of party tickets.

2   Contribution to the Literature

Observational studies of the impact of media, from rich and poor countries, typically report a strong positive link between media presence and quality of policies but have limited ability to disentangle selection and incentive effects (Besley and Burgess 2002; Strömberg 2004; Snyder Jr and Strömberg 2010). We contribute to this literature by providing the first at-scale experimental study, conducted in conjunction with a large daily newspaper, that isolates how credible media can engender electoral incentive effects for politicians.

More recently, there has been an expansion of experimental studies on information disclosures.

A first strand of this literature reports on selection by voters in response to pre-election information disclosures. These disclosures, especially when publicized by the media, are associated with corrupt incumbents faring worse in subsequent elections (Ferraz and Finan 2008), and likewise for low-performing incumbents (Banerjee et al. 2011; Kendall et al. 2015). However as pointed out by Chong et al. (2014), this selection effect may be counteracted by a negative turnout effect. The impacts also appear to be muted or absent when information is delivered through non-media sources such as pamphlets delivered by an NGO (Dunning et al. 2019). Finally, when the information provided is soft – for instance, via candidate debates – then the impacts are sensitive to what the voters infer from it (Bidwell et al. 2018; Wantchekon 2003; Fujiwara and Wantchekon 2013).

A second strand of this literature examines short-run impacts of information disclosure on politician behavior: In Philippines, areas where beliefs were most adversely affected by information campaign saw more vote-buying (Cruz et al. 2016). Relatedly, candidates in Sierra Leone increased campaign expenditures in areas where candidate debates were screened (Bidwell et al. 2018). These areas also saw greater accountability of politicians in the next election cycle.

The final strand of this literature, and the one that is closest to our paper, evaluates longer-term effects of anticipated disclosures on incumbent policy choices. Bobonis et al. (2016) takes advantage of the fixed schedule of municipality audits in Puerto Rico. Mayors expecting pre-election information disclosures are honest for the period before the election, but their corruption catches up after the election. In contrast, Avis et al. (2018) find evidence of more durable positive impacts of expected audits in Brazil on corruption of mayors.

These results demonstrate that corruption by politicians is sensitive to the electoral incentives they face, at least in the short run. However, corruption and other governance failures differ in important ways. First, while accusations and counter-accusations of corruption are common in electoral politics, the key is to be credible, and to be credible the auditing authority must be independent and professional and the evidence must be solid. For this reason such exposures are typically rare, unless a lot of resources are put behind the anti-corruption authority.[3] Put differently, credible corruption accusations are expensive to bring about and big news when they happen.

Second, corruption is illegal, and behind every investigation there is the lurking possibility of a prosecution. Avis et al. (2018) argue that the fear of prosecution can explain the entire effect on corruption that they find. While Bobonis et al. (2016) does not disentangle the judicial and electoral incentives, if pre-election report cards attract more attention than those published after, then at least part of the impact could from the fear of the extra attention leading to a prosecution. Moreover, voters may worry about their mayor ending up in court, paralyzing their performance during their coming term.

By focusing on councilor performance, captured by objective information about spending of discretionary constituency development funds, we consider a substantively different, and increasingly

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[3]Avis et al. (2018) emphasize the prestige and credibility of a Brazilian auditor's job.

important, aspect of performance. While India and Kenya are among the most well-known examples of countries that provide elected officials with discretionary constituency development funds, the use of such funding is on the rise in the developing world (International Budget Partnership 2010). Alongside, the rise of freedom of information acts in low and lower middle-income countries makes such performance disclosures increasingly feasible (Mendel 2014). The information is routinely collected by the government and is not controversial, and therefore does not require a highly credible authority to release it. On the other hand, it is possible that voters already have this type of performance information from their everyday experience and/or don't particularly take it as seriously as the corruption information.[4]

In summary, we are looking at the impact of information that is cheap and easy to obtain, whereas the corruption audit studies focus on much costlier and more sensitive information. The fact that one works tells us relatively little about whether the other will work.

The paper that comes closest to ours is Grossman and Michelitch (2018), a field experiment in Uganda where a cso produced annual performance score cards for politicians. Similar to our study, they found that public disclosure to citizens was critical (though they only observe performance improvements in electorally competitive districts). However, different fro our study they rely on intermediation by a cso rather than pure disclosure in the media.[5]

Our paper traces out the full causal chain from the announcement of future report cards, through politician behavior, all the way to the impact on the electoral fortunes of targeted politicians and their parties. In doing so, we study an important but understudied question: how do information disclosures influence the competition for party nomination?[6] Related to our work is Besley et al. (2017) who found that gender quotas increased intra-party competition among Swedish men and thereby leadership quality.[7] In both cases, binding gender quotas engender greater selection among men, our experimental design allows us to demonstrate how information mediates the nature of selection and, therefore, provides causal evidence in favor of the idea at the heart of the model in Besley et al. (2017) that the pressure for better selection comes from a desire to win elections.

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[4]Certainly the impact of corruption information on electoral outcomes as reported in Ferraz and Finan (2008) are substantially larger than the impact of performance information, as reported in Banerjee et al. (2011).

[5]Specifically, the cso created an aggregate assessment of each politician, which involves making judgments about how to weigh different attributes, but makes it easier for voters to process the multiple dimensions of performance. In our case, the ngo preferred to report the raw data and let the voters decide how to use it, in part because, as we well see, low and high income voters have different preferences. In addition, the cso in Uganda disseminated report cards through about 350 public meetings, which were preceded by community mobilization. The intervention we study, by contrast, simply published the report cards in a local language newspaper.

[6]Riker (1964) argued that the success of decentralization depends on the quality of party competition. Enikolopov and Zhuravskaya (2007) provide supportive cross-country evidence.

[7]This work is also related to papers on the impact of gender quotas in Indian politics – for instance, Beaman et al. (2012) find that quotas increased political entry by women.

3 Setting and Experimental Design

3.1 Electoral and administrative structure of Delhi

A. Elections

With a population of over 18 million, Delhi is regularly ranked among the world's largest cities. Estimates suggest that between a quarter and a half of Delhi's population lives in slums (Delhi Human Development Report 2006).[8]

Delhi has a two-tier elected governance structure – a state legislature and a city government – each of which controls a different set of public services. We focus on the city government, which is called the municipal corporation (MCD). Party-based MCD elections occur every five years in each of 272 wards, each of which elects a councilor, based on plurality rule. The party/coalition with the majority of councilors controls the corporation.

Our interventions spanned the two years in the run-up to the April 2012 election. Of the 272 incumbents elected in the previous election in 2007, India's two main national parties, BJP and INC, had the largest share of councilors at 65% and 25% respectively. Party leaders nominate candidates for each ward; there are no primaries.

These elections are also subject to political affirmative action via reserved wards where only members of the identified group can stand for election. Wards are reserved for two groups: scheduled castes (SC) and women. Ward reservation for SC candidates is based on SC ward population, with priority given to wards with higher SC population. Four months prior to 2012 elections, the Delhi government announced that gender quotas would be increased to 50%. Gender reservation was to be stratified by SC reservation status; within the two categories of SC reserved wards and other wards, half the wards were reserved for women by listing wards in each category by (ascending) serial number and reserving every odd seat for a woman. Therefore the wards reserved for women were chosen effectively at random.[9]

B. Councilor policy powers

Very broadly, councilors have access to four policy levers. First, via legislative activism in the municipal corporation general assembly. Between 2007 and 2012, the corporation met, on average, 24 times per session. The average councilor attended 19 times per session.

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[8]The difference in estimates reflects differences in the definition of a slum: for instance, whether they only include notified slums (which were legally notified or declared as slum areas under the Slum Areas (Improvement and Clearance) Act of 1956) or also include non-notified slums (typically illegal encroachment on land) (DUSIB 2010).

[9]In fact what happened was slightly more complicated. In 2011 Delhi was split into 3 separate municipal corporations, with all existing wards being assigned to one of the three. The SC reservation was implemented within each municipal corporation and then the reservation for women was implemented separately within each set of SC reserved and unreserved wards, as described.

Second, councilors are assigned to standing committees which are intended to meet regularly and be responsive to citizen complaints about various government services.

Third, councilors receive an annual discretionary fund for development works in their ward. The annual amount varies and averaged 700,000 usd per year during 2007–2012 election cycle.[10] In Figure 1 we show the fraction spending to different categories for our pre-intervention years of 2007/08 and 2008/09. Alongside, we summarize areas of perceived problems, as reported in a household survey of slum-dwellers and by neighborhood associations (these surveys are described in the data section). *Prima facie* there seems to be some evidence of a mismatch between reported needs and spending – we discuss this further in Section 3.3.

Fourth, the provision and maintenance of local public goods including public toilets, garbage removal, and drain cleaning. Toilets and garbage removal is managed on a contractual basis by private or non-government organizations.[11] Councilors play a role in choosing the contractor, and can pressure or sanction them for non-delivery, in addition to directly spending their discretionary funds, fixing up toilets or improving the garbage collection process.

3.2   Experimental Design

A. Field Experiment Partners

Our two-year experiment on information disclosure was made possible by three partners. First, Satark Nagrik Sangathan (sns) or Society for Citizens Vigilance Initiatives. Founded in 2003, sns is a citizens' group with a mandate to promote transparency and accountability in government functioning and to ensure active participation of citizens in governance. It was involved with India's Right to Information movement, which eventually led to the 2005 Right to Information law (rti). This law allows citizens to file petitions requesting the release of available information about the functioning of the government. sns has a long history of creating and then, in partnership with media outlets, disseminating report cards on incumbent performance and qualifications of leading candidates for state and national legislatures. Banerjee et al. (2011) evaluated one such intervention during the 2010 Delhi state elections and, as already mentioned, found that it increased the vote share of better performing candidates.

Our second partner was a leading Hindi daily newspaper, *Dainik Hindustan*, which is the second largest newspaper in terms of market share in Delhi. The newspaper published report cards on incumbents (prepared by sns) in 2010 and 2012. Figure 3 is an example of one daily report in 2012: it includes report cards on four politicians, as was typical. It includes each politician's

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[10]The amounts were Rs. 7.1M in 2007–08, Rs. 20M in 2008–09, Rs. 5.0M in 2009–10 and Rs. 5.0M in 2010–11.

[11]The typical public toilet contract sets maximum user price, states which facilities should be available, and requires regular cleaning of toilets. 20–30 year contracts are awarded separately for each toilet facility, with a clause that should "unsatisfactory" conditions fail to be improved within 15 days after notice is given, the contract may be rescinded. Garbage contracts stipulate that operators provide bins for non-biodegradable and recyclable/biodegradable waste, segregate the waste, and collect it daily (IL & FS Ecosmart Limited 2006). The typical garbage contract is nine years, awarded at the zonal level, and includes a performance evaluation mechanism.

photo, patterns of spending from his discretionary funds, which committees he served on, and his attendance at those committees. There is however no overall assessment or grade; the reader is invited to make his or her own mind up based on these relatively objective numbers.

Our third partner was the in-house JPAL South Asia team that conducted public service (toilet, garbage, and drain) audits and disseminated the state of sanitation information privately to incumbent politicians.

B. Newspaper report card intervention

Sample Our intervention sample consists of 240 wards, and our treatment assignment was stratified on incumbent party and zone.[12] We randomly assigned 72 to be control wards, 58 to only receive pre-election report-cards (T1) and 110 to receive both midterm and pre-election report cards (T2).

Informing politicians about report cards Councilors assigned to T1 and T2 received a letter in June 2010 from our partner organization SNS. Appended to the letter was a copy of the first set of report cards published by the newspaper on May 27, 2010. Common text in the two letters was:

> SNS uses the RTI Act to obtain objective information on the functioning of elected representatives and disseminates this information in the form of Report Cards. We collaborate with the media to disseminate these Report Cards.

> Prior to the Delhi Assembly elections in 2008, SNS developed Report Cards to disseminate information about the functioning of all 70 MLAs in Delhi. Similarly, before to the 2009 Lok Sabha elections, they developed Report Cards on the performance of about 250 prominent Members of Parliament of the country. SNS ran joint campaigns with Hindustan, the Times of India, Outlook magazine etc to disseminate these report cards.

> SNS prepared midterm report cards of MCD councilors to inform people of the development work being done by councilors for the welfare of ward residents. Due to limited resources, we have not been able to prepare report cards for all 272 councilors in Delhi. We have randomly selected 110 wards for which report cards are being prepared. The party-wise break up of the sample is the same as in the MCD. As you might be aware, these report cards are being published by the Hindi newspaper Hindustan (please find attached the report cards that appeared on May 27, 2010 in Hindustan)

The T1 letter then went on to state

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[12]Overall Delhi has 272 wards. We excluded 32 wards from our intervention sample: 5 because our partner NGOs were doing extensive work in the communities; 10 because their councilors were elected in by-elections less than two years ago, and zones 9 and 10 (Delhi is divided into 12 geographically contiguous zones), with 17 wards, because they contained rural areas or had few or zero slums.

Unfortunately, your ward is not in the list of 110 wards for which report cards are being prepared this time. However, in 2012, we will again be preparing report cards for these and more wards in Delhi. In 2012, we intend to include your ward.

The T2 letter instead stated

Your ward is one of the 110 wards for which the report card has been prepared. In 2012, we will again be preparing report cards for these 110 wards and for more wards in Delhi. We hope that dissemination of these report cards based on objective information will help people understand the development efforts being made by councilors for the welfare and betterment of their wards.

The councilors in control wards received a letter that they were not selected for report cards and would not be reported on until at least 2014 (two years after the election). All treatment councilors received a reminder letter in February 2011.

Report card format Our NGO partner SNS used the RTI Act to obtain data from the MCD on councilor spending, meeting attendance, and committee membership.

For the midterm report cards, over (roughly) a month starting May 27th 2010 the newspaper featured three report cards daily. Each report card included the councilor's photo, spending patterns for discretionary funds, and committee attendance. The data reported on covered the period April 2007 to March 2009.

The 2012 report cards had the same format as those issued in 2010, but were published in groups of four. They covered the time period April 2007 to March 2011. As data is released by fiscal year, we lacked spending data for last year of the councilor's term. The report cards were published in March 2012, the month prior to the election. On a given day the newspaper featured four ward councilors. Because of publication constraints, we were unable to publish all 2012 report cards before the 2012 election. We prioritized the publication of report cards for councilors eligible for reelection. Of 168 report cards for ITT councilors, 124 were published, comprising all report cards for eligibles and half of those for ineligibles.[13] Throughout, we report ITT estimates.

The publication of four report cards on a single newspaper page also allows us to examine the possibility of yardstick competition, i.e. whether parties (and voters) valued the relative performance of a councilor. The process for choosing which four councilors were placed together on a newspaper page occurred in two stages. First, report cards were categorized into comparable groups (by zone, eligibility, and "slumminess") and within each group the wards were grouped into pairs. Two pairs were then randomly chosen to feature on the same page.

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[13]See Appendix 8.2 for details.

C. Private State of Sanitation Information (SSI) intervention

Can greater awareness of public good provision problems in the ward, without public disclosure, suffice to influence councillor behavior? To evalauet this possibility, the SSI intervention provided councilors private information on the state of toilets and garbage dumps in slums in their wards.

Sample   Our audits covered the sample of 108 high-slum wards where we also undertook baseline surveys. We undertook a two-stage randomization. First, we randomized state election jurisdictions into treatment and control and then randomized wards within treatment jurisdictions into treatment and control. In each ward we audited, on average, three slums giving us a sample of 310 slums across 108 wards. See Appendix 8.3 for details.

Information reporting   The reports were based on audits of public toilets and garbage dumps (dhalaos[14]) in each slum. Three rounds of SSI audits were conducted, with reports based on the first two mailed to councilors (and legislators) in treatment groups. The first round of SSI was distributed in August 2011, about eight months before the elections for the ward councilors, and the second round was sent in February 2012, only a couple of months prior to the elections. The first summarized the baseline SSI audits conducted between April and June 2011 (Round 1), and the second compiled SSI audits conducted between November 2011 and January 2012 (Round 2). The final round of SSI audits was conducted immediately after the MCD elections, from April through June 2012 (Round 3). Cover letters sent to the legislators along with the audit result, explained the context behind the SSI audits. A cover letter for the second round SSI audit read in part:

> As part of a study on urban poor in Delhi, we conducted a survey in 2010, which was done with [number of] families of your ward [ward identifier] in slum settlements and in low-income areas. Of these families, [percentage] reported that public toilets and sanitation are a major problem for their community, and [percentage] reported that garbage was a major problem for their community.

> You may recall, in September 2011, we sent you a report card in which we highlighted the condition of the garbage collection points and public toilet facilities in your ward's slum settlements and in low-income areas. We have recently completed the second round of audits of garbage and toilet facilities in these areas, and we are again sharing this information with you. We hope this can be useful in your ward-related work. Our auditors have tried to cover all garbage points and public toilets in the slum settlements and in low-income areas that were audited.

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[14]The Master Plan for Delhi defines a dhalao as "a premise used for collection of garbage for its onward transportation to sanitary landfills" (Delhi Development Authority 1990). IL & FS Ecosmart Limited (2006) defines dhalaos as "large masonry dustbins."

Information gathered by our auditors in your ward is enclosed with this letter. In the first part of the report card there is a summary of the status of audited garbage and toilet facilities, and in the latter part, their details. We are also enclosing a map of all these facilities where your attention is needed. Each facility has been assigned a code which is placed on its location on the map, so that you can easily identify it.

Information on the state of drains was also collected, but not shared with politicians, during the second and third rounds of SSI audits, to check if there were any spillover effects on this service. These could have been positive, if, for example, the audits made them more aware of the problems in those slums, or negative if there was a diversion of effort away from drains.

3.3 Data and Descriptive Statistics

Our interventions, and subsequent analysis, draw upon multiple sources of data.

A. Citizen preferences and slum incidence

Our NGO partner's citizen activism work (corroborated by our previous work with the NGO reported in Banerjee et al. (2011)) suggested that electoral incentives prompt councilors' to be responsive to slum populations with significant infrastructure needs. We use aerial data on slum incidence to define $High_w$, a dummy for high-slum wards that takes a value of one if the fraction of slum area in that ward exceeds the median value computed across all wards (about 1%). High-slum wards have a mean of 7% slum area and 13% slum population. The Appendix describes our ward selection, and how we established a sample frame of urban slums.

Before launching our intervention, we conducted (in April-May 2010) a household survey covering a sample of 5,481 slum households in 107 wards.[15] The slum-dwellers we surveyed were typically long-term migrants to Delhi: our average respondent had lived in Delhi for seventeen years. The survey collected extensive data on slum-dwellers' access to, usage of, and difficulties with social services and private transfers, knowledge of the local government system, interactions with public officials and politicians, and political preferences and participation.

Figure 1 shows very little alignment between (category-wise) councilor spending and problem areas identified by slum dwellers. Councilors spend a majority of their funds (57%) on roads, an area where significant corruption in contracting has been documented (Lehne et al. 2018). In contrast, slum-dwellers report clean water, sewage management, and garbage collection as the most problematic issues. Water in Delhi is privately provided and is beyond the remit of the councilors; however drains and toilets are not. We see that despite this, spending on drains and toilets at 17% constitutes a far lower proportion of councilor funds (relative to roads). The next two highest

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[15]This was intended to be identical to the audit sample. Due to slum mis-identification and other surveying errors, the survey sample does not include four audit wards, but includes three other wards.

expense categories for councilors also do not meet slum-dwellers' interests: provision and repair of lights (8%) and the improvement of parks and provision of gates (7%). In particular, slum dwellers express no interest in spending on parks and greenery, for the unsurprising reason that these are almost always in non-slum neighborhoods

Figure 2 reports on slum dweller beliefs and preferences over councilor spending decisions. Over a quarter of slum dwellers believe that councilors spend the most on roads and lanes and the least on sewage and drains. Roughly a third of them would like the councilor to spend more on sewage and drains.

In 94 baseline survey wards we also surveyed 250 office-holders of Resident Welfare Associations (RWAs) which represent households with full legal status in the ward (and are, on average, richer than slum dwellers).[16] This survey identifies areas of public good provision that the RWA leaders consider as the biggest problems. Slum dwellers and RWA residents have different preferences: RWA residents are ten times more likely to name roads as an important problem, put less (but still substantial) emphasis on sewage, drainage, and garbage disposal and are very keen on parks and street lights, two items that the slum dwellers never name.[17]

B. Councilor performance data

SNS obtained official councilor performance data for all wards and collated them for both treatment and control wards; only treatment ward data was subsequently released as newspaper report cards. The report card included category-wise spending of discretionary funds by the councilor.[18] Figure 1 reports the average spending breakdown.

We combine these data with survey data on slum-dwellers good preferences to construct a *pro-poor spending* index as follows. We use a five category classification of councilor spending: Roads and lanes, Sewage and drainage, Parks and greenery, Education and schools, and Garbage removal. We then weight each spending category by slum-dweller preferences. We use three different measures of slum-dweller preference, creating three separate indices: the fraction of slum households in the city reporting (1) that the issue is the biggest problem in their area, (2) that it is a problem for them, or (3) that it is a problem for the community. For each weighting criteria, we calculate the sum of the logs of each weighted spending amount (wherever the value was zero it was bottom-coded

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[16]RWAs were introduced by the Delhi state government as a formal mechanism for neighborhood associations to be formed and to interact with state agencies (Government of National Capital Territory of Delhi 2014). Our RWA sample was drawn from the Delhi government list, which we matched to wards based on the RWA's stated address. Not all survey wards had registered RWAs.

[17]Detailed results are available from authors on request.

[18]The categories were: (i) Roads and lanes (construction and repair); (ii) Sewage and drainage (including toilets); (iii) Street lights (provision and repair of street lights and high mast lights); (iv) Parks and greenery (improvement of parks and providing designer gates); (v) Education and schools (improvement of MCD schools); (vi) Garbage removal (supply of material, trucking; construction of garbage dumps; removal of *malba* (rubble)); and (vii) Others (this included: Construction of MCD offices; Improvements to staff quarters; Improvements to *katras* (high-slum-index tenements); Improvements to community centers and health centers; Construction of boundary walls; Providing grills on *chabuttras* (monuments); Providing street name boards; and Miscellaneous.)

to the first percentile of the spending distribution, prior to taking logs). Finally we calculate the mean z-score of these three log-weighted spending measures, and the result serves as our pro-poor spending index. We also report the three log-weighted spending measures as separate outcomes.

The report card also reported councilors' assembly and committee attendance - on average, assembly and committee attendance rates were 81% and 66%, respectively. Our second measure of councilor quality is an attendance index which is the mean z-score of attendance in: (1) MCD's general assembly and (2) MCD committees of which the councilor is a member. We consider absolute committee attendance, without regard to the number of meetings of each committee. The pro-poor spending index and the attendance index are positively correlated with $\rho = 0.10$.

C. Audit data from SSI

All audits covered toilets and garbage points (dhalaos) and the second and third audit also covered drains. For each facility, the auditor was required to survey the entire slum and identify all facilities. To ensure audits were complete, auditors asked slum-dwellers where they disposed of their trash and which public toilet they used. The garbage disposal point or public toilet was audited when at least three residents reported using it.

During a facility audit the surveyors observed and noted the quality of the public amenities and interviewed two respondents for each garbage/toilet/drainage point to obtain information on frequency of cleaning and prices. Finally, to obtain data on usage, the surveyor counted the number of people who used the toilet in a randomly chosen observation time of 15 minutes between 3–5 PM.

D. Electoral data

For the April 2012 election we obtained ward-level data on electoral outcomes. This provided information on turnout, candidate list (with party) and candidate-wise vote-share.

We also collected data on the ward reservation status, which was announced in January 2012. For each incumbent councilor we coded an *Ineligible* dummy which equalled one if the quota rendered the councilor ineligible for re-contest from own ward. Exactly one third (80 of 240) of our incumbent councilors were so ineligible, though some fraction of those, get reassigned elsewhere.

However, the fraction of incumbents who do not re-run anywhere exceeds the fraction who are affected by quotas. Only two fifths (95 of 240) of incumbent councilors contested an election in any ward, including just over half (87 of 160) of those who were eligible to rerun in the same ward.

3.4 Implementation and Balance Checks

Our experiment was implemented in multiple stages. In late May and June 2010, report cards on T2 councilors were published. Actual treatment was close to ITT: 109 out of the 110 councilors were reported on, excluding one ward in which the councilor had died. In June 2010 letters were sent out to all councilors.

In March 2012, the second round of report cards was published. Here, actual treatment was significantly lower than intended treatment. This mainly reflected space constraints in the newspaper. Out of the 58 wards assigned to T1 45 were treated and of the 110 T2 wards 79 were treated. A total of 124 report cards were published. Within our sample of ITT councilors, we randomized the order in which we sent report cards for publication to the newspaper, but we prioritized publishing report cards of councilors eligible for reelection. The appendix provides details.

Throughout we estimate ITT effects. Appendix Table A.I and A.II provide balance checks. Panel A and B of Appendix Table A.I considers pre-intervention councilor spending outcomes (for 2007–09) for all wards and the wards where we did the slum survey, respectively. Panel C considers slum households self-reports on which areas they faced problems in over the last year (using the spending categories). We observe no differences between treatment and control wards. In Appendix Table A.II we consider 2007 electoral outcomes as the dependent variable. We observe no differences between treatment and control wards for number of registered voters in 2007, turnout, seat reservation, or number of candidates, but 5.6% percentage points higher vote share among treated councilors.

4   What to expect

Political agency models assume that citizens infer a politician's underlying type (say, how much he cares about the voter's welfare) from imperfectly observed politician actions. This inferred type partly determines whether his party will select him to run again and whether the voters subsequently vote for him. Overall, such retrospective electoral accountability generates incentives for politicians to appear to do their job well. We now discuss how, in such a setting, changing available information on politician performance alters politician, party, and voter behavior.

4.1   Impact on councilor behavior

A desire to be re-elected should cause councilors to choose policies that result in a report card that appeals to party leaders and/or voters, assuming that the councilors believe that both groups react to the report cards in making their choices. Our measures, most clear cut measure is total spending of discretionary funds, which presumably all voters want to be maximized. Attendance at the general assembly and at committee meetings should also be an unambiguous good, unless citizens favor some councilor actions unobserved to us and there is a time budget constraint.

The effect on spending composition is less clear; slum residents and wealthier residents have different preferences, and most incumbents' spending decisions appear to favor wealthier residents. Given this, we evaluate a narrower hypothesis: in slum-dense wards (which is where we conducted the baseline survey), treated councilors should alter their spending decisions to better match public good preferences of low-income voters. This should be especially true because it is easier to make a difference in these neighborhoods given that there is very little other investment; councilor

discretionary funds are often the primary source of infrastructure spending in slums. By contrast, building a new park, something that RWA owners want, is expensive in land-scarce Delhi.

Both report card treatments should create incentive effects, though the additional credibility of T2 (midterm *and* pre-election report card) should plausibly increase its impact.[19]

In addition to this *incentive effect*, the T2 treatment may have an *information effect* if midterm report cards raise the salience of certain issues or provide new information to candidates. Arguably, since total spending in any single category reflects spending on multiple projects in different locations and years, councilors may not recognize the composition of their own spending and/or benchmark it correctly against spending by other councilors. On the other hand, the difference in effects between T2 and T1 councilors is unlikely to be large; after all, T1 councilors could have collated the data for their wards to construct own report cards.

Thus, a comparison of T1 and T2 may have limited power to disentangle the incentive and information effects, especially since the incentive effect of T2 may also be strengthened by activism by citizens informed by the midterm report cards about councilor performance.

Therefore, we turn to the private SSI experiment where we provided councilors with actionable information at two points in time (one of which was very close to elections) and test the hypothesis that giving the information *just to the councilor* improves service delivery, suggesting that councilor's ability to deliver what citizens prefer is constrained by the lack of information (or lack of salience) about the state of public goods. Our audit focused on public goods that lent themselves to simple improvements that councilors could in principle affect: she could spend her discretionary funds to fix the toilets, improve monitoring of contractors who run the toilets, and put pressure on the municipal employees in charge of garbage collection.

4.2   Impact on party and voter behavior when quotas exist

For voters, the publication of report cards may provide new information about their councilor or make the information more salient, and that may change their voting preferences. Better performers should benefit more, because voters will interpret good performance as indicative of a type that will also do more in the future. The one complication is if voters knew their councilor's treatment status. If they knew he was anticipating a report card, and therefore trying to make himself look better, they may want to discount the signalling aspect of performance.[20] In this case, a treated councilor with the same performance as a councilor in a control ward may attract less support. However, we have little reason to believe voters knew that politicians had been forewarned.

Second, by providing party leaders with new information about councilor performance, report cards could affect the likelihood that a councilor receives a party ticket for the upcoming election.

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[19] Avis et al. (2018) highlights the credibility effect by comparing municipalities that differ in whether, in addition to a pre-election corruption report, the municipality also received a corruption report at start of the term.

[20] Unless they value the ability or willingness to respond to incentives.

Of course the party may have been aware of the intervention and the incentives it created, and therefore partly discounted the observed performance.

Third, even if party leaders don't receive new information on a candidate, they may recognize that voters prefer high performers and therefore value those high performers more. This effect may be most important in situations where the voters do not know the candidate (e.g. because the candidate needs to change wards). A positive report card with its authenticity established by a credible newspaper is a good way to present a new candidate. We call this effect on the party selection process that happens even when the party learns nothing new, an *advertizement effect*.

The expansion of gender quotas strengthened the party selection effect for incumbents' who were rendered ineligible to run in their ward. The party has to decide whether to give these councilors a ticket from another ward; information that an ineligible councilor is a high-performer may sway the party decision.

Finally, an additional source of information arose from the way the information was presented. In 2012, the newspaper printed four report cards daily, allowing the voters to benchmark their councilor performance. Our baseline survey shows that, in general, constituents were pessimistic about their councilors' spending. Of the 81% of slum households who answered the question on their councilor's relative performance, 71% thought their councilor spent less money than councilors in other wards. (9% thought more, and 20% thought about the same.) For Delhi state elections, Banerjee et al. (2011) report evidence showing that voters use other incumbents' performance to benchmark their elected representative's performance. This has two implications. First, the act of comparing should improve voters' view of their own councilors, by correcting their bias. This should favor the treated councilors. Second, to the extent that the voters learn more about what is possible for a councilor , this should encourage benchmarking the performance of their own councilor by that of the others on the same page. In general, we would predict that being a better performer among the four featured councilors, *controlling for actual performance*, should attract more votes. That said, there is a potential countervailing effect: seeing how poorly the entire group performs may discourage voters. They might, for example, vote for someone outside the mainstream parties or decide not to turn out, which would depress the votes of the treated relative to those in control.

4.3 Summary of likely treatment impacts

To the extent that *councilors* believe that report cards provide voters with information or makes known information salient, being treated should incentivize them to perform. In high-slum wards, this should imply increased pro-poor spending.

If *voters* receive new information or the salience of known information increases, voters should switch their votes towards better performers. Moreover they should favor the better performers among those published on the same page of the newspaper. However the main effect of treatment is ambiguous; it depends on the voters' priors and how they get updated based on the new information.

Finally *parties* should favor the good performers in treatment wards when allocating party tickets, because of both the information effect and the advertizement effect. Moreover the advertizement effect is likely to be most important for the selection of which councilors to move to new ward where the voters do not know the candidate.

5   Does information influence councilor behavior?

We start with examining evidence on an incentive effect for councilors who know that voters will be informed about their performance prior to the election.

5.1   Anticipated public disclosures: Newspaper report cards

A. Specification

Our base specifications for estimating treatment effects in ward w are ANCOVA specifications which allows for smaller changes in wards where baseline investments in that outcome were already high:

$$y_{w1} = \alpha y_{w0} + \beta Treat_w + X_w + \epsilon_w, \tag{1}$$

$$y_{w1} = \alpha y_{w0} + \beta Treat_w + \gamma High_w + \lambda Treat_w \times High_w + X_w + \epsilon_w, \tag{2}$$

$$y_{w1} = \alpha y_{w0} + \beta Treat_w + \gamma High_w + \delta Inelig_w + \lambda Treat_w \times High_w + \mu Treat_w \times Inelig_w$$
$$+ \nu High_w \times Inelig_w + \xi Treat_w \times High_w \times Inelig_w + X_w + \epsilon_w, \tag{3}$$

$Treat_w$ is the ward treatment status, $High_w$ is an indicator for high-slum ward, $Inelig_w$ is whether the councilor is ineligible for reelection in that ward, X_w is a fixed effect for randomization stratum (the zone–party combination), and other variables are as described below. Throughout we report ITT estimates. The main tables pool the $T1$ and $T2$ treatment arms and Appendix Tables report impacts disaggregated by treatment status.

Our councillor performance measures y_{w1} include: log total spending, a pro-poor spending index, a councilor attendance index, and components of both indices. All outcomes are measured for post-treatment fiscal years 2010–2012. The lagged dependent variable, y_{w0} represents the average for pre-treatment fiscal years 2007–2009 of the same variables.

The equation 1 specification considers average treatment effects for the full sample. To allow for preference heterogeneity by residential status, equation 2 examines heterogeneous treatment effects across high- and low-slum-density wards. A final source of random variation comes from quota-induced councilor ineligibility. Roughly half of eligible councilors re-run, so we might expect bigger incentive effects for ineligible councilors who lose incumbency benefits. Equation 3 examines what happens when we also control for eventual ineligibility.

5.1.1 Results

Table I presents our main specifications. Ward councilors typically spent their entire budget. Reflecting this, columns (1)–(3) show that anticipation of report cards has no effect on log total spending, estimated precisely, either on average or in high-slum wards. Councilor ineligibility for future elections also doesn't affect overall spending.

Columns (4)–(6) consider our primary incentive outcomes: the pro-poor spending index. Consistent with our discussion in Section 4, the treatment impact on councilor spending depends on the type of ward they are in: we do not detect any overall treatment impact on spending (column 4). However, there is a large *negative* (but noisy) impact on pro-poor spending in low-slum wards, and relative to these treatment wards, a large positive impact on high-slum treatment wards (higher by 62% of a standard deviation; see column 5).Again, councilor ineligibility has no impact on nature of spending, in high- or low-slum wards (column 6). As a robustness check, Appendix Table A.VIII presents specifications using an alternative definition for being high-slum: specifically, whether the ward was one in which we did a household survey. Recall that a survey selection criterion was (verified) slum density. Unsurprisingly, we see similar results to the main specification.

Appendix Table A.VI presents specifications where spending outcomes are individual spending categories; Figure 1 summarizes spending totals on these categories. While the impacts are noisily estimated, we find suggestive evidence that treated councilors in high-slum wards spent significantly more on drainage and sewage (Column (2)) and less on schools and parks (columns 4 and 6).

Columns (7)–(9) of Table I report on the attendance index. Interestingly, the pattern of impacts of report cards is similar to that on the spending index–the treatment effect on attendance in high-slum wards is significantly higher than that in low-slum wards.[21] Ineligibility doesn't influence councilor behavior in high-slum wards. We find weak evidence that attendance of councilors belonging to low-slum wards is negatively impacted by treatment. However, this the only outcome where councilor ineligibility matters – given that, we read the evidence from Table I as being consistent with the idea that future ineligibility largely had no impact on councilor behavior prior to elections, possibly because the whole eligibility process was shrouded in uncertainty.

Appendix Table A.IV presents specifications in which we separate T1 and T2 treatment arms – in general, we cannot reject identical impacts on the spending and attendance indices across the two arms, but the T2 effects are more precisely estimated due to a larger sample.

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[21]Appendix Table A.V considers index components. Columns (3), (4), and (5) consider components of the pro-poor spending index, and show that findings are robust to the constituent preference weighting chosen. Columns (8) and (9) consider the components of the attendance index. Changes to the index are driven by general assembly attendance rather than committee attendance, although patterns are similar between them. This is despite that several of the most-frequently meeting committees – such as the slum development and ration committees – are relevant to low-income households. The fact that the attendance effects are different between low- and high-slum neighborhoods is somewhat surprising–we would have expected that everyone would want their legislators to be more involved. It might reflect that the middle classes are more cynical about what politicians do.

5.2 Private disclosures: State of Sanitation Information

Councilors' policy response to anticipated newspaper disclosures is consistent with an *incentive* effect. The fact that T1 and T2 yield similar effects suggests that midterm report cards did not have any additional information or priming effect. To further explore the relevance of the information channel we consider the private ssi experiment.

5.2.1 Specification

First, for comparison with Table I, we consider impact of the ssi intervention on pro-poor spending. We estimate regressions of the same form as equation 2 where the treatment indicator is whether the councilor received an ssi report. The sample is restricted to the 103 wards that are in the survey and audit sample.

Next, other than spending, councilors may improve public toilets and garbage piles via direct pressure on contractors responsible for their provision. We, therefore, use the three rounds of audits to estimate slum-level difference-in-differences regressions:

$$Service_{sw} = \beta Post_t \times Treat_{sw} + \gamma Post_t + X_w + \epsilon_{sw}, \tag{4}$$

where $Treat_{sw}$ is the treatment status of the slum's ward, $Post_t$ is an indicator for the midline or endline audit, X_w is a ward fixed effect, and $Service_{sw}$ public services outcome variables are as described below. Throughout we report itt estimates.

5.2.2 Results

Table II presents our private disclosure results. First, we consider our (ward-level) pro-poor spending and observe no significant impact of the audits (Column 1) In contrast, columns (3) and (4) of Appendix Table A.VIII show that, consistent with our results in Table 1, the newspaper report cards differentially raised pro-poor spending in treated wards for this sub-sample of wards.

Columns (2)–(9) consider impacts on audited outcomes (that we reported on). Relative to control wards, slums in treatment wards show an insignificant decline in total toilets but a significant decline in the number of open usable toilets (columns 2 and 3). While toilet prices are unaffected, the number of adult users declines in treatment wards (columns 4 and 5). One possible explanation is that the councilor asked contractors to improve the worst toilets and they chose to close and/or remove them. A less favorable view is that the councilor favored closing and/or removing dirty toilets in order to improve the appearance of toilet quality and was less concerned with usage. We observe no impact of receiving the ssi treatment on garbage outcomes, either informal piles or formal garbage collection points (dhalaos) (columns 6 through 9). Appendix Table A.IX shows that the intervention also had no impact on total number of drains or their quality (as measured by fraction of drains with disposal facilities and fraction of drains that are clogged).

Taken together, the private SSI having no beneficial impacts supports the view that the impact of the anticipated public performance disclosures on councilor behavior that we find is likely to reflect an incentive effect and is not a result of councilors being better informed. This conclusion is also consistent with T1 and T2 having similar impacts.

6   Does information influence party and voter behavior?

Next, we evaluate the electoral implications of the newspaper report card treatment. This is interesting by itself, and also helps us understand whether councilors' response to the information disclosures could be justified by subsequent voter and party reactions.

6.1   Specification

We first examine party decision to field the incumbent councilor for re-election. We continue to report pooled estimates across the two treatment arms. For each electoral outcome, we consider three specifications with increasing saturation. First, we examine the average effect of report cards. Next, we ask whether these impacts differ by councilor eligibility to rerun from own ward in 2012 elections. Finally, we consider a more descriptive specification to see whether councilor performance with respect to pro-poor spending and attendance influences party behavior.

$$Outcome_{w,c} = \beta Treat_{w,c} + X_w + \epsilon_{w,c}, \tag{5}$$

$$Outcome_{w,c} = \beta_1 Treat_{w,c} \times Inelig_w + \beta_2 Treat_{w,c} + \gamma Inelig_w + X_w + \epsilon_{w,c}, \tag{6}$$

$$Outcome_{w,c} = \beta_i Treat_{w,c} \times Inelig_w \times Y_{w,c} + \beta_j Treat_{w,c} \times Y_{w,c} + \gamma_i Inelig_w \times Y_{w,c} + \lambda_i Y_{w,c} + X_w + \epsilon_{w,c}, \tag{7}$$

where $Treat_{w,c}$ is the ward/councilor treatment status, $Inelig_w$ is whether the ward's councilor is ineligible for reelection in that ward, $Y_{w,c}$ is a vector of councilor's pro-poor spending index, attendance index, and the identity, X_w is a fixed effect for our randomization stratum (the zone–party combination), and other variables are as described below. Throughout we report ITT estimates.

We start with specifications where we consider the incumbent councilor as the relevant unit. Our councilor specifications interpret treatment, ineligibility and performance outcomes as councilor-level characteristics that "follow" the councilor. As councilor-level outcome variables $Outcome_c$, we consider (1) whether the incumbent runs for re-election in *any* ward, (2) whether s/he runs in a *different* ward, (3) whether s/he runs in a different ward where the [retiring or ineligible] incumbent also belongs to their party, (4) his/her vote share (set to zero if the councilor does not run), and (5) whether s/he wins an election in *any* ward.

We also report a set of specifications where we consider electoral outcomes for the incumbent party as the ward-level outcome variables. This is qualitatively different in spirit than the previous

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regressions. The treatment impact on the incumbent can come entirely from a party selection effect: if the party uses the report cards to select the incumbents who get to re-run, say because of the advertizement effect, we will see an effect on treated incumbents. In other words, even if the support among the voters for every single incumbent is unaffected by treatment, the fact that the party uses the report card to choose who will re-run is enough to generate a positive treatment effect at the *incumbent* level. It will not, however, generate a treatment effect at the *ward* level. Since someone from the party would always run in each ward, a ward-level treatment effect is only possible if the report card alters preferences of voters targeted by the policy. This is therefore the only way we have to tell whether the report cards affected voter preferences, and, if so, how.

Finally we report specifications that consider councilor performance relative to those councilors who were also featured in the same issue of the newspaper, controlling for their actual pro-poor spending. In other words, do the other councilor's report card serve as a yardstick? This comparison is only defined for the 124 councilors whose report cards were actually published in newspapers (as opposed to ITT), and the sample is restricted to these treatment wards. We estimate

$$Outcome_c = \beta_1 Rank_c \times Inelig_c + \beta_2 Rank_c + \gamma Inelig_c + Y_c + X_c + \epsilon_c, \tag{8}$$

where $Rank_c$ is the rank of the councilor's pro-poor spending index among the four councilors whose report cards appeared in the same newspaper, $Inelig_c$ is whether the councilor is ineligible for reelection in their current ward, Y_c is a vector of the councilor's pro-poor spending index and the spending index interacted with ineligibility, and X_c is a fixed effect for our randomization stratum (the zone–party combination). We report TOT estimates.

6.2 Results

48% of eligible incumbents in control wards ran for re-election. Column (1) of Table III shows that treated incumbents were 12 percentage points more likely to receive a party nomination to contest re-election. In fact, no ineligible councilor from a control ward ran for reelection. In contrast, column (2) shows that the magnitude of the treatment effect for the ineligible, while more than twice as large as that for the eligible, is not statistically different from that for the eligible. Even the effect on the eligible is a non-trivial 7 percentage points, though not statistically different from zero. However, when we consider whether the councilor ran in a different ward, we observe a treatment effect only among ineligible councilors: column (4) shows that the treatment-induced increase in contesting elections from a different ward is coming entirely from ineligible councilors who contest elections in new wards. In other words, eligible incumbent councilors either re-run in their home ward or are dropped from the party list. Finally, columns (5) and (6) show that councilors who run in a new ward tend to displace incumbents from the same party, and this group consists of ineligible councilors.

Just eight councilors who became ineligible received a party nomination to contest an election in another ward. Since this effect comes from a small number of wards, we also report the results of a robustness check. Appendix Table A.X has corresponding results estimated with randomization inference rather than OLS. This method of non-parametric inference is appropriate to address concerns about the sample size. The randomization inference p-value of treated councilors receiving a party nomination rises to 10%. The treatment effect on ineligible councilors contesting different wards remains significant at the 5% level, and this effect is still driven by parties reassigning tickets for wards they already control. These effects are somewhat less precise than those estimated with OLS, but display a similar pattern.

All of this is suggestive of a party selection process where the party drops its weaker incumbents. Such selection would predict that a party which can re-run an incumbent in their home ward should do so, because s/he is known to be popular (since they won the last time). However those who cannot make it even in their home ward are unlikely to be competitive after being moved to another one, especially since they are competing against the entire pool of ineligibles. The latter group are randomly selected (by the assignment of the quotas) and, therefore, contain some of the best performing incumbents.

We next turn to the direct evidence of party selection based on performance on the report cards. Table IV presents results on this issue. Column (1) shows a heterogeneous impact of spending performance on receiving a ticket across eligible and ineligible councilors; Specifically, only ineligible treated incumbents with a good pro-poor spending record are more likely to be given a ticket to rerun. Ineligible treated incumbents are 23 percentage points more likely to be given a ticket for each standard deviation increase in their pro-poor spending record. We interpret the absence of a performance effect among eligible incumbents as reflecting a strong incumbency effect wherein the party will pay the cost of displacing an eligible incumbent to field an ineligible councilor only when the latter has an extremely good performance record.

Consistent with the idea that parties want their legislators to be responsive to the needs of their constituents, columns (2) and (3) show that parties favor giving tickets to ineligible councilors in high-slum wards with high pro-poor spending, but not to those in low-slum wards with high pro-poor spending. Columns (5) and (6) ask which councilors get to move to other wards, and finds that only ineligible councilors with high pro-poor spending from high-slum wards get to move to a different ward. On the margin, ineligible treated incumbents from high-slum wards are 29 percentage points more likely to be given a ticket to run elsewhere, for each standard deviation increase in their pro-poor spending record. Those with a similarly good spending record from low-slum wards do not get the same advantage, though there too few such councilors to be able to say anything definitive about this comparison.

Columns (8) and (9) show that that parties favor moving ineligible councilors with high pro-poor spending into new party-controlled wards that are high-slum wards, which makes sense since

the evidence that they have to advertise relates to performing well in high-slum wards. On the margin, ineligible treated incumbents are 21 percentage points more likely to be given a ticket to run in another high-slum ward controlled by their party for each standard deviation increase in their pro-poor spending record.

To summarize, parties retain ineligible councilors who had a good performance record with respect to serving their own *low-income* constituency and field them in other slum-dense wards. It is also interesting that these benefits do not accrue to untreated councilors, even if they have high pro-poor spending, suggesting either that parties only have access to the spending information through the reports in media, or that the parties put a lot of weight on the advertisement effect.

Our results show that councilors were correct to respond to the electoral incentives provided by report cards since having a good report card helped them get a ticket to run. Those who get to run should also get more votes and should be more likely to win. Table V confirms this, and shows that the advantage that high pro-poor spenders in treated high-slum wards get is sizeable. Column (1) shows a 5 percentage point rise in the vote share of ward councilors who received a report card. Column (2) shows that this impact might be slightly larger for ineligible councilors, but the difference is nowhere near being significant. Column (3) suggests that treated ineligible councilors (who necessarily run elsewhere if they run at all) gain more in vote share if their pro-poor spending is higher, but this is not true of treated eligible councilors. On the margin, ineligible treated incumbents earn 10 percentage points higher vote share for each standard deviation increase in their pro-poor spending record. This suggests that a positive report card is beneficial for councilors to earn votes from less familiar voters. Columns (4)–(5) show that the effect is concentrated among councilors who came from high-slum wards, so that their pro-poor spending would have been aligned with the preferences of their former constituents. On the margin, ineligible treated incumbents from high-slum wards earn 21 percentage points higher vote share for each standard deviation increase in their pro-poor spending record. Columns (6)–(10) show a similar pattern for winning elections, but the binary outcome results in noisier estimates. In particular, on the margin, ineligible treated incumbents from high-slum wards are 49 percentage points more likely to win an election for each standard deviation increase in their pro-poor spending record. Appendix Table A.XIII shows that the treatment didn't affect voter registration or turnout.

Table VI presents ward-level results for electoral outcomes – that is, electoral rewards that might accrue to a party's benefit even if their incumbent councilor departs. As we mentioned earlier, this is direct evidence on whether the treatment altered voter preferences. Column (1) and (2) show that there is no residual vote share benefit for the party for simply having had a report card published, but column (3) shows that in wards where the incumbent can't rerun, there is a benefit for having had a report card published showing high pro-poor spending, and columns (4)–(5) confirm that this effect is driven by high-slum wards. As in Table V, columns (6)–(10) show a similar pattern for winning election, but the binary outcome results in noisier estimates. High levels of pro-poor

spending in high-slum wards, when reported in the newspaper, increases support among voters for the incumbent's party in wards where the incumbent can't rerun.

Table VII presents councilor-level results for the "yardstick" results – whether it helps councilors to have performed well relative to those councilors who were featured in the same issue of the newspaper. Columns (1), (2), and (4) show a yardstick effect on the party decision to run an ineligible councilor in a different ward. The effect is driven by report cards for ineligible candidates. For each rank (out of four) that an ineligible councilor drops with respect to pro-poor spending, the party is 20 percentage points less likely to give that councilor a ticket to run. Column (4) shows that for each drop in rank, the vote share of an ineligible councilor from a high-slum ward goes down by 4 percentage points. Column (5) shows that this effect is absent in low-slum wards. The results with respect to winning, reported in columns (6)–(10), follow a similar pattern, and while the effects are potentially large, none of them are significant.

7   Conclusion

This paper provides evidence on the impact of public and private information disclosures on choices made by incumbent politicians, political parties, and voters.

The promise of public performance disclosures motivated politicians to change spending priorities to align better with their constituents: politicians representing poorer areas changed their spending to reflect the policy priorities of their poorer constituents.

Politicians were right to change their priorities in this way. Political parties are more likely to give tickets to treated politicians from low-income neighborhoods, who, according to their report cards, were high-performing in the sense of having served low-income voters well. Those high-performing incumbents from low-income wards who were no longer eligible to run from their home constituency because of reservations benefited the most from this, and got to run and win from other constituencies. These effects are consistently very large; ineligible treated incumbents from high-slum wards are 29 percentage points more likely to be given a ticket to run elsewhere, for each standard deviation increase in their pro-poor spending record.

Finally, voters in low-income wards also reward the party associated with high-performing incumbents who got a report card, so the parties were probably right in rewarding those incumbents.

On the other hand, disclosing information privately to politicians has no discernible positive effects on their spending priorities or on the actual provision of public infrastructure in slums.

That publication in the media plays such a critical role in decisions at all levels is important at a time when media freedom is threatened. It is also worrying, since it suggests that politicians and even parties may not care about performance unless it gets highlighted in the media. Since space in the credible print media is limited, this may limit the scope for providing effective incentives to politicians. This suggests an important direction for future research: is there some way to provide

the public with credible performance information on a routine basis, and get them to pay attention to it, without actually printing report cards in the newspaper before the election?

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8 Data Appendix

8.1 Slum Identification

We identify slums following a methodology based on the UN-HABITAT and Indian census definition of slums.[22] A list of nine common criteria closely correlated to the census definition of slums was drawn up and included high density of housing, poor quality housing structure and material, lack of internal household infrastructure, poor road infrastructure, access to water and water infrastructure, uncovered and unimproved drains, low coverage of private toilet facilities, high incidence of trash piles and frequent cohabitation with animals.[23]

We used a two-stage process: first, we compiled a list of potential areas from inspection of the visual appearance from aerial photographs of Delhi using satellite imagery, based on housing density and appearance, complemented by Delhi government listings. This was then verified by field visits; locations that prominently featured at least five of these nine characteristics were marked as more slummy and others as less slummy.

Between 9 and 126 households were surveyed in each ward[24], with the exact number in a slum dependent on the number of potential slums identified by satellite image in each ward and the physical size of the slum. To the extent that population density is similar across different slums, this approximates a Probability Proportional to Size (PPS) sampling procedure. To select households within slums we also used a spatial method: an overall map of each slum was created, and then surveyors were stationed at randomly selected points within the slums. Surveyors then followed the "right hand rule," where each surveyor moves from their start point along the right hand side of the wall, interviewing every X households (where X is determined by the population of the slum).

Overall, we had just over 3,400 households in high-slum areas and 2,000 households in 8 low-slum

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[22]The 2011 Indian census defines a slum as a "compact housing cluster or settlement of at least 20 households with a collection of poorly built tenements which are, mostly temporary in nature with inadequate sanitary, drinking water facilities and unhygienic conditions will be termed as slums."; UN-HABITAT defines a slum household as "a group of individuals living under the same roof that lacks any one of meet the following conditions: insecure residential status, inadequate access to safe water, inadequate access to sanitation and other infrastructure, poor structural quality of housing and overcrowding." The main difference between the two is UN-HABITAT's inclusion of insecure residential status; this is an issue that will be explored within the survey work, but since this is the case to some degree in most Delhi slums, we safely omit it.

[23]Housing: Whether the space separating households was sufficiently wide for vehicles larger than motorcycles; housing materials: Whether the majority of houses are made of unimproved brick or lower quality material, including metal and plastic sheeting; internal household infrastructure: Whether household chores (e.g. washing, cooking) were frequently done outside of the house as a proxy for the quality of households' internal infrastructure, since households who conduct these activities outside tend to lack household water supply/drainage or ventilation for cooking smoke; road infrastructure: Whether the majority of roads in the area were unpaved, badly maintained, and of poor quality; water: Whether households receive water from hand pumps, tanker trucks, or lower-grade options; animal cohabitation: Whether non-domestic animals (buffalo, goats, pigs, donkeys) resided in the same tenements as people.

[24]In ten wards, it was found that surveys had been conducted in the wrong areas. In these cases, surveyors were sent back out, and the surveys were redone in the proper areas. In some cases, the incorrect surveys were still conducted in slum areas, so have been included in the data; thus ten wards have sixty or more surveys. In other cases, the wrongly done surveys were dropped.

neighborhoods (fewer than five slum characteristics).[25]

8.2   2012 Newspaper Report Card Publication Sequence Randomization

All 2010 report cards were published as intended, but subject to space constraints in each newspaper and time constraints, we were unable to publish all 2012 report cards before the 2012 election. We did not publish any report cards after election day. Of 168 report cards for ITT councilors, 124 were published. Six wards were dropped because the councilor was suspended for corruption or died; seven were dropped because they were never sampled (in two of these cases another ward was sampled instead); one was dropped because there were no slums in the ward; and the last thirty were dropped because they could not be published by election day. Out of the 58 T1 wards, 45 were treated, and of the 110 T2 wards, 79 were treated.

First, report cards were categorized according to zone (of which there were ten), to whether the councilor was eligible for reelection, and to whether the ward had above- or below-median slum fraction by area. Then, within these forty categories, each of which had about four report cards, we randomly assigned report cards to pairs. We then assigned a stratum to each pair according to treatment status (T1/T1, T1/T2, or T2/T2) and political party affiliation (no BJP councilor, or at least one BJP councilor). We randomly assigned publication sequence to each pair of report cards for eligible councilors, distributing the above six strata evenly across the publication sequence. Then we repeated the process for half (i.e. as many as possible) of the report cards for ineligible councilors, and these were placed after the eligible councilors in the publication sequence. Report cards for ineligible councilors were published after the deadline for parties to assign candidate tickets. Two pairs of report cards were published in each daily issue of the newspaper.

8.3   State of Sanitation Information Randomization

Our audits covered the sample of 108 high-slum wards that entered our baseline survey. These wards, in turn, were situated in 55 state assembly constituencies (ACs). All ACs were randomized into treatment and control, followed by a balanced randomization of the wards within an AC. In the event that a ward was split across two ACs, it was put in the AC with an unbalanced number of wards. We then separately randomized report card distribution across the two levels of government: 51 wards were randomly assigned to have the MCD councilor receive a ward report card and, out of the 55 ACs, 27 were randomly assigned to receive a AC report cards.[26] In each ward we audited, on average, three slums giving us a sample of 310 slums across 108 wards.

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[25]The survey was typically carried out with the household head (in 51% of the cases) or, in the case where the household head was unavailable or away on two consecutive visits made to the household, with his or her spouse (49% of the cases) or other household member. If a household proved unwilling or unavailable after multiple visits, another was selected using the same method.

[26] Because Wards and ACs are not perfectly aligned, this made for a total of 118 Ward–AC combinations: 30 control, 30 where only the MLA received a report card, 32 where only the MCD councilor received a report card, and 26 were both the MLA and MCD councilor received report cards.

Table I: Effect of any newspaper report card treatment on MCD councilor spending according to slum preferences and attendance

	Log total spending (2010–12)			Pro-poor spending index (2010–2012)			Attendance index (2010–12)		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Treatment × High slum		−0.049	−0.097		0.617**	0.752**		0.372*	0.128
		(0.074)	(0.100)		(0.288)	(0.373)		(0.196)	(0.225)
Treatment	0.005	0.050	0.051	−0.010	−0.309	−0.384	0.030	−0.227	0.006
	(0.040)	(0.057)	(0.081)	(0.133)	(0.201)	(0.278)	(0.100)	(0.138)	(0.147)
Ineligible × Treatment × High slum			0.115			−0.479			0.694
			(0.160)			(0.618)			(0.497)
Ineligible × Treatment			0.035			0.255			−0.668*
			(0.119)			(0.425)			(0.368)
Remaining interactions	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Pre-treat outcome control	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Strata (zone–party) FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Pre-treat control mean	5.709	5.721	5.721	−0.000	0.019	0.019	0.000	−0.004	−0.004
Pre-treat control s.d.	0.157	0.109	0.109	1.000	0.923	0.923	0.893	0.912	0.912
p-value: $T \times High + T + High = 0$		0.994			0.722			0.453	
p-value: $T \times High + T + High = T$		0.254			0.012			0.248	
p-value: $T \times High + T + High = High$		0.977			0.123			0.301	
Observations	240	227	227	240	227	227	240	227	227

Heteroskedasticity-robust standard errors in parentheses. * $p < .10$, ** $p < .05$, *** $p < .01$.

Ward-level OLS regression. Pro-poor spending index components are each log spending on issues, with each issue weighted by the fraction of slum households in the city reporting that (1) it is the most problematic in the area, (2) it is a problem for them, and (3) it is a problem for the community. Attendance index components are councilor attendance at (1) the general assembly and (2) councilor committee meetings. "Treatment" indicates observations of a ward in which a report card on the performance of the MCD councilor was published in a newspaper during the 2012 pre-election period (T1 or T2, ITT). Spending is categorized by lexical heuristic.

Table II: Effect of State of Sanitation Information on public services

	(1) Pro-poor spending index	(2) Total toilets	(3) Open toilets	(4) Toilet price	(5) Adult toilet users (#)	(6) Total dhalaos	(7) Dhalaos regularly collected (%)	(8) Total informal piles	(9) Informal piles recently collected (%)
SSI treatment	−0.049								
	(0.190)								
Post × SSI treatment		−0.125	−0.204**	0.065	−1.931*	−0.087	−0.004	1.310	−0.038
		(0.133)	(0.090)	(0.108)	(0.996)	(0.058)	(0.089)	(2.209)	(0.044)
Post		0.280**	0.206**	0.047	1.389	0.196***	−0.054	−3.236**	0.416***
		(0.112)	(0.081)	(0.061)	(0.847)	(0.049)	(0.065)	(1.365)	(0.026)
Ward FE	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Strata (zone–party) FE	Yes	No	No	No	No	No	No	No	No
Pre-treat outcome control	Yes	No	No	No	No	No	No	No	No
Baseline control mean	0.102	2.718	1.859	0.973	9.994	0.436	0.304	19.207	0.253
Observations	103	932	932	430	932	932	328	867	867

Heteroskedasticity-robust standard errors in parentheses. * $p < .10$, ** $p < .05$, *** $p < .01$.

Slum-level OLS regression. "SSI treatment" indicates observations in a slum of which the MCD councilor received State of Sanitation Information (ITT). "Post" indicates observations that took place in the second or third round of audits. "Total informal piles" is the number of informal garbage piles in the slum. "Informal piles not recently collected" is the fraction of informal garbage piles in the slum not collected in the past week. "Total dhalaos" is the number of dhalaos (formal garbage collection points) in the slum. "Dhalaos regularly collected" is the fraction of dhalaos in the slum regularly collected, relative to the number of dhalaos at the baseline. 529 of 932 slums have a public toilet. "Closed toilets" is the number of closed toilets in the slum. "Toilet price" is the average price of toilets in the slum. "Adult users (#)" is the number of adult toilet users in the slum.

Table III: Effect of newspaper report card publication on councilor-level electoral outcomes

	Councilor runs in any ward		Councilor runs in other ward		Councilor runs in other ward controlled by party	
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment × Ineligible		0.091		0.156***		0.116**
		(0.114)		(0.056)		(0.050)
Treatment	0.120*	0.071	0.039*	−0.012	0.044**	0.007
	(0.067)	(0.086)	(0.022)	(0.019)	(0.018)	(0.014)
Ineligible		−0.473***		−0.028		−0.009
		(0.079)		(0.022)		(0.016)
Strata (zone–party) FE	Yes	Yes	Yes	Yes	Yes	Yes
Ineligible control mean	0.000	0.000	0.000	0.000	0.000	0.000
Eligible control mean	0.478	0.478	0.022	0.022	0.000	0.000
Observations	240	240	240	240	240	240

Heteroskedasticity-robust standard errors in parentheses. * $p < .10$, ** $p < .05$, *** $p < .01$.

Councilor-level cross section estimated with OLS.

Table IV: Effect of newspaper report card publication, attendance, and spending on councilor-level electoral outcomes, by ward slumminess

	Councilor runs in any ward			Councilor runs in other ward			Councilor runs in other ward controlled by party		
	(1)	(2) ...*from* high-slum ward	(3) ...*from* low-slum ward	(4)	(5) ...*from* high-slum ward	(6) ...*from* low-slum ward	(7)	(8) ...*to* high-slum ward	(9) ...*to* low-slum ward
Treatment × Ineligible × ...	0.229*	0.414*	0.136	0.125**	0.291**	0.084	0.074	0.207*	−0.002
...Pro-poor Spending Index	(0.118)	(0.213)	(0.155)	(0.058)	(0.136)	(0.068)	(0.051)	(0.108)	(0.086)
Treatment × Ineligible × ...	0.792	0.670	2.037**	−0.017	0.771	−0.225	0.012	0.614	−0.668
...Attendance Index	(0.603)	(0.757)	(1.015)	(0.355)	(0.467)	(0.554)	(0.322)	(0.415)	(0.605)
Treatment × ...	−0.067	−0.216	0.009	−0.048	−0.149	−0.012	−0.005	−0.037	0.030
...Pro-poor Spending Index	(0.096)	(0.173)	(0.131)	(0.034)	(0.095)	(0.031)	(0.014)	(0.040)	(0.042)
Treatment × ...	−0.198	0.014	−0.704	−0.078	−0.424	0.051	−0.026	−0.173	−0.098
...Attendance Index	(0.433)	(0.566)	(0.626)	(0.117)	(0.283)	(0.200)	(0.080)	(0.148)	(0.173)
Treatment × Ineligible	−0.424	−0.322	−1.627**	0.172	−0.415	0.261	0.112	−0.324	0.620
	(0.420)	(0.483)	(0.695)	(0.245)	(0.303)	(0.394)	(0.218)	(0.246)	(0.448)
Treatment	0.206	0.092	0.712*	0.037	0.345	−0.045	0.020	0.159	0.049
	(0.302)	(0.375)	(0.425)	(0.080)	(0.216)	(0.122)	(0.060)	(0.124)	(0.126)
Nontreatment Interactions	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Strata (zone–party) FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Ineligible control mean	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
Eligible control mean	0.478	0.577	0.294	0.022	0.038	0.000	0.000	0.000	0.000
Observations	240	116	112	240	116	112	240	115	114

Heteroskedasticity-robust standard errors in parentheses. * $p < .10$, ** $p < .05$, *** $p < .01$.

Councilor-level cross section estimated with OLS. "Attendance Index" is overall councilor attendance at MCD committees of which they are a member, 2007–10. "Pro-poor Spending Index" is the mean z-score of three log preference-weighted spending amounts (2007–11), analogous to the dependent variable in Table I, column 3, for the pre-publication period.

Table V: Effect of newspaper report card publication, attendance, and spending on councilor-level electoral outcomes

	Councilor's vote share (0 if didn't run)					Councilor wins in any ward				
	(1)	(2)	(3)	(4) ...*from* high-slum ward	(5) ...*from* low-slum ward	(6)	(7)	(8)	(9) ...*from* high-slum ward	(10) ...*from* low-slum ward
Treatment × Ineligible × ... ...Pro-poor Spending Index			0.096** (0.047)	0.213** (0.085)	0.032 (0.065)			0.230** (0.117)	0.494*** (0.181)	0.046 (0.131)
Treatment × Ineligible × ... ...Attendance Index			0.314 (0.233)	0.223 (0.305)	1.096*** (0.306)			0.583 (0.595)	0.340 (0.877)	1.620** (0.706)
Treatment × ... ...Pro-poor Spending Index			−0.046 (0.038)	−0.115* (0.069)	0.013 (0.058)			−0.131 (0.104)	−0.296* (0.160)	−0.027 (0.115)
Treatment × ... ...Attendance Index			−0.022 (0.165)	0.160 (0.255)	−0.402* (0.203)			0.021 (0.479)	0.290 (0.777)	−0.384 (0.505)
Treatment × Ineligible		0.024 (0.047)	−0.176 (0.170)	−0.098 (0.209)	−0.875*** (0.206)		0.093 (0.098)	−0.266 (0.414)	−0.074 (0.609)	−1.226** (0.483)
Treatment	0.047* (0.027)	0.032 (0.035)	0.048 (0.121)	−0.066 (0.180)	0.364** (0.138)	0.029 (0.057)	−0.014 (0.080)	−0.033 (0.336)	−0.195 (0.557)	0.347 (0.340)
Nontreatment Interactions	No	Yes	Yes	Yes	Yes	No	Yes	Yes	Yes	Yes
Strata (zone–party) FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Ineligible control mean	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
Eligible control mean	0.171	0.171	0.171	0.204	0.114	0.304	0.304	0.304	0.346	0.176
Observations	240	240	240	116	112	240	240	240	116	112

Heteroskedasticity-robust standard errors in parentheses. * $p < .10$, ** $p < .05$, *** $p < .01$.

Councilor-level cross section estimated with OLS. "Attendance Index" is overall councilor attendance at MCD committees of which they are a member, 2007–10. "Pro-poor Spending Index" is the mean z-score of three log preference-weighted spending amounts (2007–11), analogous to the dependent variable in Table I, column 3, for the pre-publication period.

Table VI: Effect of newspaper report card publication, attendance, and spending on ward-level party electoral outcomes

	Incumbent's party's vote share in same ward					Incumbent's party wins in same ward				
	(1)	(2)	(3)	(4) ...in high-slum ward	(5) ...in low-slum ward	(6)	(7)	(8)	(9) ...in high-slum ward	(10) ...in low-slum ward
Treatment × Ineligible × ... ...Pro-poor Spending Index			0.095*** (0.035)	0.096** (0.045)	0.087 (0.070)			0.165 (0.163)	0.294 (0.237)	0.025 (0.212)
Treatment × Ineligible × ... ...Attendance Index			0.303 (0.221)	0.242 (0.211)	0.519 (0.479)			0.305 (0.833)	−0.401 (1.239)	2.229 (1.344)
Treatment × ... ...Pro-poor Spending Index			−0.017 (0.024)	−0.070* (0.036)	0.038 (0.052)			−0.144 (0.096)	−0.435*** (0.158)	0.102 (0.151)
Treatment × ... ...Attendance Index			0.063 (0.112)	0.134 (0.163)	−0.106 (0.218)			−0.293 (0.510)	0.010 (0.834)	−0.988 (0.804)
Treatment × Ineligible		−0.000 (0.041)	−0.181 (0.152)	−0.141 (0.147)	−0.415 (0.336)		−0.033 (0.149)	−0.248 (0.562)	0.261 (0.833)	−1.914** (0.939)
Treatment	−0.002 (0.018)	−0.001 (0.022)	−0.047 (0.080)	−0.096 (0.120)	0.085 (0.153)	−0.026 (0.069)	−0.012 (0.088)	0.178 (0.344)	−0.007 (0.594)	0.739 (0.528)
Nontreatment Interactions	No	Yes	Yes	Yes	Yes	No	Yes	Yes	Yes	Yes
Strata (zone–party) FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Ineligible control mean	0.408	0.408	0.408	0.381	0.449	0.615	0.615	0.615	0.500	0.769
Eligible control mean	0.347	0.347	0.347	0.371	0.331	0.500	0.500	0.500	0.577	0.353
Observations	240	240	240	116	112	240	240	240	116	112

Heteroskedasticity-robust standard errors in parentheses. * $p < .10$, ** $p < .05$, *** $p < .01$.

Ward-level cross section estimated with OLS. "Attendance Index" is overall attendance at MCD committees of which they are a member, 2007–10. "Pro-poor Spending Index" is the mean z-score of three log preference-weighted spending amounts (2007–11), analogous to the dependent variable in Table I, column 3, for the pre-publication period.

Table VII: Effect of newspaper report card spending yardstick on councilor-level electoral outcomes among treated councilors

	Councilor runs in any ward	Councilor runs in other ward	Councilor runs in other ward controlled by party	Councilor's vote share (0 if didn't run)	Councilor wins in any ward
	(1)	(2)	(3)	(4)	(5)
Ineligible × Rank	−0.256**	−0.128*	−0.050	−0.072*	−0.044
	(0.121)	(0.076)	(0.046)	(0.042)	(0.087)
Rank	0.052	0.012	0.017	0.032	0.006
	(0.066)	(0.027)	(0.027)	(0.028)	(0.058)
Pro-Poor Spending controls	Yes	Yes	Yes	Yes	Yes
Ineligible FE	Yes	Yes	Yes	Yes	Yes
Strata (zone–party) FE	Yes	Yes	Yes	Yes	Yes
Ineligible treated mean	0.115	0.115	0.077	0.022	0.038
Eligible treated mean	0.592	0.010	0.010	0.222	0.337
Observations	124	124	124	124	124

Heteroskedasticity-robust standard errors in parentheses. * $p < .10$, ** $p < .05$, *** $p < .01$.

Councilor-level cross section estimated with OLS. "Rank" is the rank of the councilor's mean z-score of three log preference-weighted spending amounts among the four councilors whose report cards appeared in the same newspaper. (Highest = 0; lowest = 3.) 'Pro-Poor Spending controls' are 'Pro-Poor Spending Index' and 'Ineligible × Pro-Poor Spending Index'.

Figure 1: Ward-level descriptive statistics of councilor spending and constituent preferences
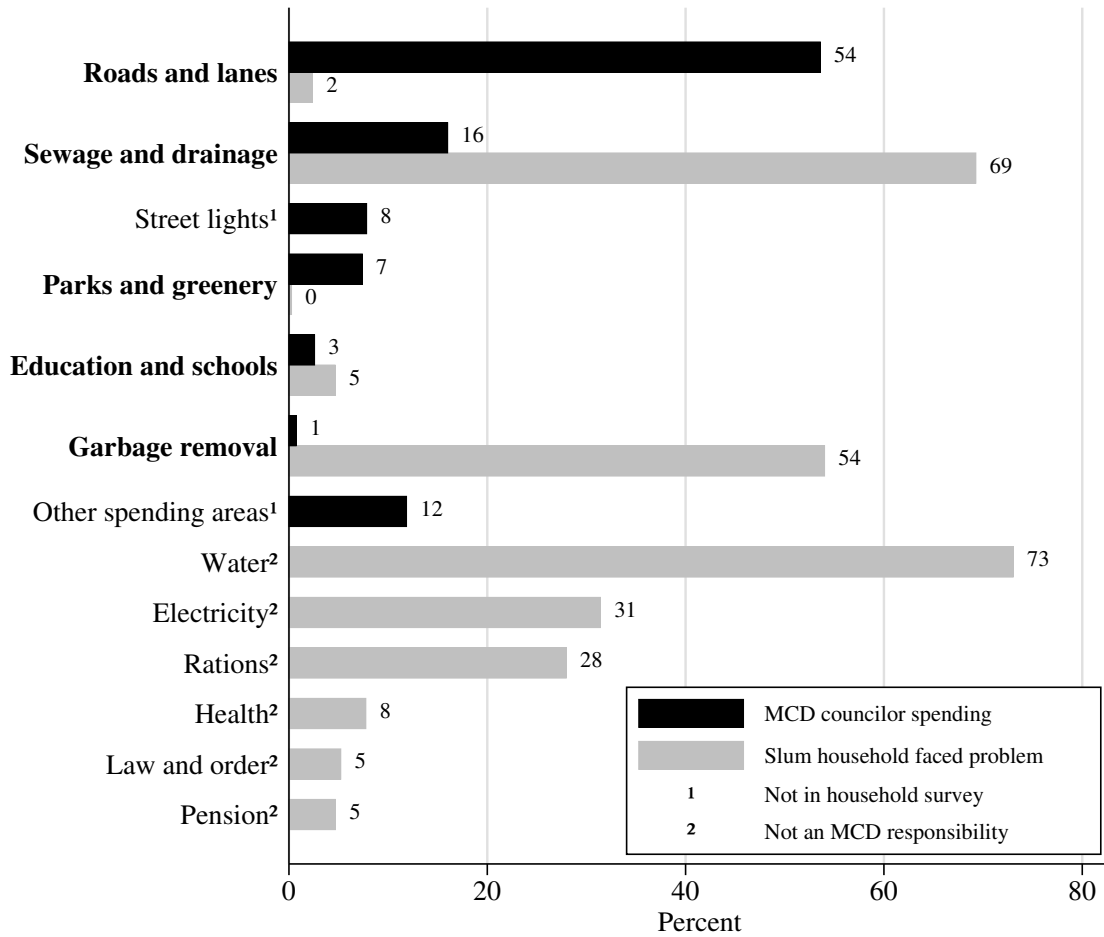
"MCD councilor spending" is the ward-mean of MCD councilor spending (mean annual fraction 2007–2012). (The total is 1 by construction.) "Slum household faced problem" is the ward-mean of households in slum areas who specify each area in response to the question, "In which of the following areas have you personally faced problems in the last year?" (The total is the mean number of areas named by households.) Household responses are weighted within wards to correct for differential coverage of surveys between slums. Bold face indicates areas that are included in preference-weighted spending measures.

Figure 2: Constituent beliefs and preferences about councilor spending
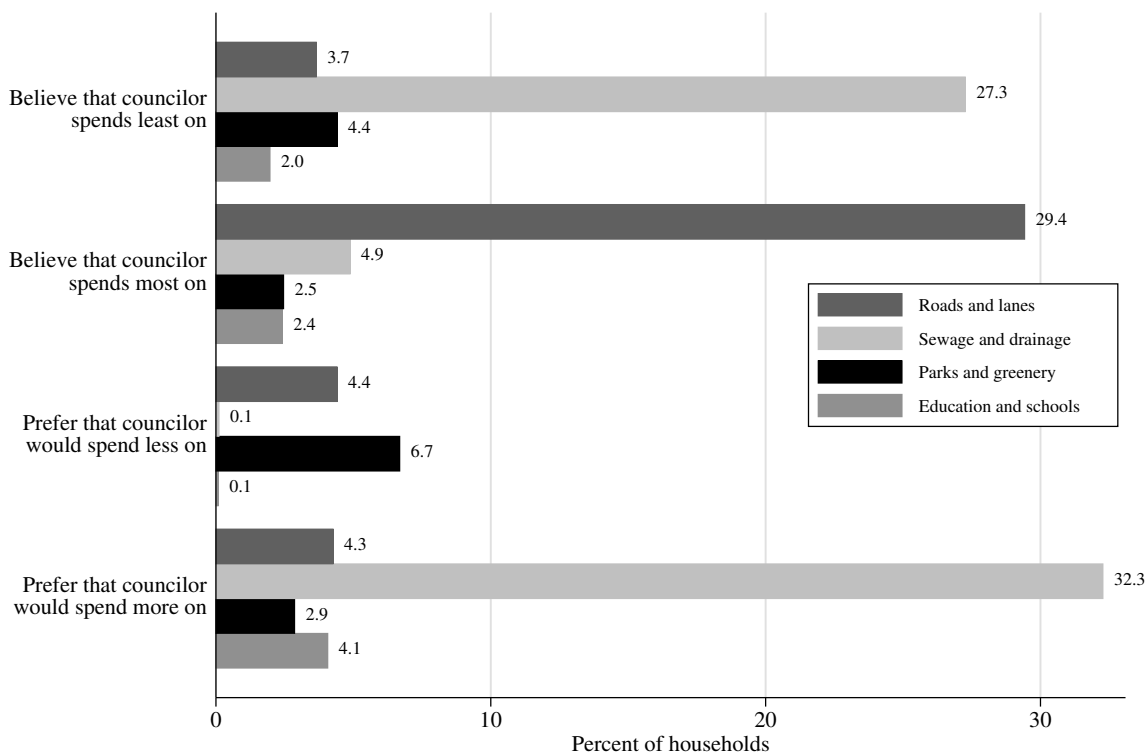
Each bar represents the ward-mean of households in slum areas who specify each area in response to the questions, "What type of project do you think your councilor spends most (least) on?" and, "What type of project do you think you councilor spends too much (little) money on?" Respondents were prompted with the project types in the figure, as well as with "Water", "Health", and "Community centres". Household responses are weighted within wards to correct for differential coverage of surveys between slums. The figure only presents areas that are included in preference-weighted spending measures. Beliefs and preferences about spending on garbage collection were not directly elicited.

Figure 3: Example MCD councilor report cards in the Hindustan in 2012

आपके पार्षद

	प्रोमिला घई	निर्मल जैन	सत्य शर्मा	संतोष कुमार
पार्टी	भाजपा	भाजपा	भाजपा	निर्दलीय
	जनक पुरी पश्चिम	शाहदरा	न्यु उस्मान पुर	सुंदर नगरी
वार्ड नंबर	117	237	251	244
शैक्षिक योग्यता	बीए	एलएलबी	12वीं	9वीं
आपराधिक मामले	नहीं	एक में बरी	नहीं	नहीं

वार्ड का ब्योरा

निगम में प्रदर्शन (अप्रैल 07-मार्च 11)

	निगम की बैठकों में उपस्थिति	सवाल पूछे
प्रोमिला घई	97/104	00
निर्मल जैन	99/104	02
सत्य शर्मा	101/104	00
संतोष कुमार	50/104	00

समितियों में प्रदर्शन (अप्रैल 2007 से मार्च 2011)

समिति	पद	अवधि	उपस्थिति
वार्ड समिति, पश्चिम	सदस्य	2007-11	77/92
पर्यावरण प्रबंधन सेवा समिति	अध्यक्ष	2009-10	9/10
सामुदायिक सेवा समिति	सदस्य	2009-10	5/7
महिला कल्याण एवं बाल विकास समिति	सदस्य	2007-09, 2010-11	9/13

समिति	पद	अवधि	उपस्थिति
वार्ड समिति, शाहदरा दक्षिण	सदस्य	2007-11	61/69*
पर्यावरण प्रबंधन सेवा समिति	सदस्य	2007-09	17/19

समिति	पद	अवधि	उपस्थिति
वार्ड समिति, शाहदरा उत्तर	सदस्य	2007-11	121/126
विशेष समिति : चिकित्सा राहत एवं जन स्वास्थ्य	सदस्य	2007-09	18/21
सामुदायिक सेवा समिति	सदस्य	2010-11	7/9
एडहॉक समिति: महिला एवं बाल विकास समिति	सदस्य	2007-10	9/11

समिति	पद	अवधि	उपस्थिति
वार्ड समिति, शाहदरा उत्तर	सदस्य	2007-11	80/126
विशेष समिति: स्लम कॉलोनी विकास	सदस्य	2007-11	7/23
एडहॉक समिति : अ.जा. कल्याण	सदस्य	2007-09	3/14

पार्षद निधि के खर्च का ब्योरा (फीसदी में)

प्रोमिला घई — कुल उपलब्ध राशि* ₹3.71 करोड़, राशि खर्च ₹3.65 करोड़

निर्मल जैन — कुल उपलब्ध राशि* ₹3.71 करोड़, राशि खर्च ₹3.43 करोड़

सत्य शर्मा — कुल उपलब्ध राशि* ₹3.71 करोड़, राशि खर्च ₹3.32 करोड़

संतोष कुमार — कुल उपलब्ध राशि* ₹3.71 करोड़, राशि खर्च ₹3.35 करोड़

पार्षदों के दावे

आठ किमी लंबा नाला कवर करवाया

मैटरनिटी सेंटर बनवाने में मिली सफलता

सड़क ठीक करवाई, हर्बल पार्क बनवाया

अंबेडकर पार्क का करवाया विकास

9 Appendix: Tables and Figures

Table A.I: Baseline check of newspaper report card treatment with councilor spending and constituent preferences

	(1)	(2) Sewage and drainage	(3)	(4) Education and schools	(5)	(6)	(7)
	Roads and lanes	Sewage and drainage	Parks and greenery	Education and schools	Garbage removal	Other areas	Total
Panel: Spending (all wards)							
Treatment	−0.004	−0.005	0.007	0.004	0.001	−0.003	0.000
	(0.022)	(0.016)	(0.008)	(0.007)	(0.002)	(0.013)	–
Strata (zone–party) FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Control mean	0.552	0.178	0.060	0.024	0.008	0.178	1.000
Control s.d.	0.183	0.130	0.067	0.034	0.013	0.114	0.000
Observations	240	240	240	240	240	240	240
Panel: Spending (slum survey wards)							
Treatment	−0.017	0.018	−0.003	0.001	−0.001	0.002	0.000
	(0.037)	(0.028)	(0.015)	(0.005)	(0.003)	(0.019)	–
Strata (zone–party) FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Control mean	0.542	0.187	0.073	0.019	0.008	0.171	1.000
Control s.d.	0.194	0.142	0.070	0.023	0.012	0.094	0.000
Observations	106	106	106	106	106	106	106
Panel: Slum HH preference							
Report card	0.005	−0.014	0.001	−0.006	0.026	−0.079	−0.067
	(0.011)	(0.040)	(0.003)	(0.016)	(0.045)	(0.073)	(0.099)
Strata (zone–party) FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Control mean	0.020	0.688	0.002	0.046	0.513	1.581	2.851
Control s.d.	0.043	0.180	0.007	0.063	0.195	0.389	0.411
Observations	106	106	106	106	106	106	106

Heteroskedasticity-robust standard errors in parentheses. * $p < .10$, ** $p < .05$, *** $p < .01$.

Ward-level OLS regression. "Spending (all wards)" is the fraction of total MCD councilor spending (calculated over pre-treatment period) booked for each area. "Spending (slum survey wards)" is an equivalent measure restricted to wards in which we surveyed slum households. Spending is categorized by lexical heuristic. "Slum HH preference" is the ward-mean of households in slum areas who specify each area in response to the question, "In which of the following areas have you personally faced problems in the last year?" (The total is the mean number of areas named by households.) Household responses are weighted within wards to correct for differential coverage of surveys between slums.

Table A.II: Baseline check of newspaper report card treatment with councilor electoral outcomes

	2007 Election					2012 Election
	(1) Log regis-tered voters	(2) Log turnout	(3) Seat reserved for minority	(4) Number of candidates	(5) Winner's vote share	(6) Eligible for reelection
Treatment × Ineligible (2012)	−0.051	−0.009	0.072	−0.214	−0.032	
	(0.047)	(0.057)	(0.128)	(1.162)	(0.041)	
Treatment	−0.004	−0.022	−0.060	0.113	0.056*	0.040
	(0.032)	(0.038)	(0.086)	(0.767)	(0.034)	(0.067)
Ineligible (2012)	0.002	−0.017	−0.438***	0.405	0.013	
	(0.038)	(0.044)	(0.106)	(0.948)	(0.022)	
Control mean	10.500	9.643	0.472	9.472	0.395	0.639
Control s.d.	0.168	0.198	0.503	4.121	0.097	0.484
Observations	240	240	240	240	240	240

Heteroskedasticity-robust standard errors in parentheses. * $p < .10$, ** $p < .05$, *** $p < .01$.

Table A.III: Characteristics of wards in 2012 MCD elections by slum fraction

Variable	(1) Low slum Mean/SE	(2) High slum Mean/SE	t-test p-value (1)-(2)
Incumbent eligible	.65 (.045)	.68 (.043)	.64
Incumbent's pro-poor spending index	−.21 (.12)	−.016 (.089)	.20
Incumbent's attendance index	.65 (.017)	.68 (.017)	.24
Incumbent runs in any ward	.37 (.046)	.42 (.046)	.39
Incumbent runs in other ward	.027 (.015)	.052 (.021)	.33
Incumbent wins in same ward if eligible	.21 (.038)	.19 (.037)	.77
Incumbent's vote share in same ward	.13 (.019)	.13 (.018)	.88
Incumbent's party wins in same ward	.50 (.047)	.54 (.046)	.52
Incumbent's party's vote share in same ward	.37 (.016)	.37 (.012)	.86
Voter turnout	.54 (.0062)	.54 (.0049)	.59
Voter registration	41806 (1058)	41115 (830)	.61
N	112	116	

Heteroskedasticity-robust standard errors in parentheses. * $p < .10$, ** $p < .05$, *** $p < .01$.

Table A.IV: Effect of any newspaper report card treatment on MCD councilor spending according to slum preferences

	Log total spending (2010–12)		Pro-poor spending index (2010–2012)		Attendance index (2010–12)	
	(1)	(2)	(3)	(4)	(5)	(6)
T1: 2012 report (ITT) × High slum		−0.085		0.493		0.091
		(0.102)		(0.369)		(0.234)
T2: 2010/12 reports (ITT) × High slum		−0.023		0.684**		0.521**
		(0.082)		(0.300)		(0.221)
T1: 2012 report (ITT)	0.072	0.131*	0.018	−0.227	0.037	−0.094
	(0.054)	(0.077)	(0.162)	(0.246)	(0.115)	(0.148)
T2: 2010/12 reports (ITT)	−0.031	0.004	−0.024	−0.356*	0.026	−0.304*
	(0.042)	(0.062)	(0.143)	(0.208)	(0.112)	(0.168)
High slum		−0.004		−0.234		−0.246
		(0.066)		(0.251)		(0.168)
Pre-treat outcome control	Yes	Yes	Yes	Yes	Yes	Yes
Strata (zone–party) FE	Yes	Yes	Yes	Yes	Yes	Yes
p-value: $T1 \times High + T1 = T2 \times High + T2$		0.330		0.794		0.141
p-value: $T1 = T2$	0.042	0.097	0.772	0.510	0.923	0.226
Observations	240	227	240	227	240	227

Heteroskedasticity-robust standard errors in parentheses. * $p < .10$, ** $p < .05$, *** $p < .01$.

Ward-level OLS regression. Pro-poor spending index components are each log spending on issues, with each issue weighted by the fraction of slum households in the city reporting that (1) it is the most problematic in the area, (2) it is a problem for them, and (3) it is a problem for the community. Attendance index components are councilor attendance at (1) the general assembly and (2) councilor committee meetings. Spending is categorized by lexical heuristic.

Table A.V: Effect of any newspaper report card treatment on spending and attendance index components

	Pro-poor spending index (2010–2012)		Spending index components			Attendance index (2010–12)		Attendance index components	
	(1)	(2)	(3) Biggest problem	(4) Problem for individual	(5) Problem for community	(6)	(7)	(8) Assembly	(9) Committees
Treatment × High slum		0.617**	1.425**	1.263**	1.287**		0.372*	0.069**	0.044
		(0.288)	(0.631)	(0.607)	(0.617)		(0.196)	(0.033)	(0.040)
Treatment	−0.010	−0.309	−0.686	−0.650	−0.657	0.030	−0.227	−0.045**	−0.019
	(0.133)	(0.201)	(0.438)	(0.426)	(0.433)	(0.100)	(0.138)	(0.023)	(0.030)
High slum		−0.230	−0.531	−0.474	−0.478		−0.238	−0.043	−0.023
		(0.250)	(0.548)	(0.528)	(0.536)		(0.166)	(0.028)	(0.033)
Pre-treat outcome control	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Strata (zone–party) FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Pre-treat control mean	−0.000	0.019	3.196	6.480	6.403	0.000	−0.004	0.816	0.651
Pre-treat control s.d.	1.000	0.923	2.034	1.941	1.973	0.893	0.912	0.127	0.190
p-value: $T \times High + T + High = 0$		0.722	0.661	0.762	0.745		0.453	0.350	0.919
p-value: $T \times High + T + High = T$		0.012	0.009	0.015	0.015		0.248	0.183	0.395
p-value: $T \times High + T + High = High$		0.123	0.091	0.144	0.140		0.301	0.324	0.357
Observations	240	227	227	227	227	240	227	224	227

Heteroskedasticity-robust standard errors in parentheses. * $p < .10$, ** $p < .05$, *** $p < .01$.

Ward-level OLS regression. Pro-poor spending index components are each log spending on issues, with each issue weighted by the fraction of slum households in the city reporting that (1) it is the most problematic in the area, (2) it is a problem for them, and (3) it is a problem for the community. Attendance index components are councilor attendance at (1) the general assembly and (2) councilor committee meetings. "Treatment" indicates observations of a ward in which a report card on the performance of the MCD councilor was published in a newspaper during the 2012 pre-election period (T1 or T2, ITT).

Table A.VI: Effect of any newspaper report card treatment on MCD councilor spending on selected spending categories

	Log spending...					
	(1) ...total	(2) ...on drains	(3) ...on garbage/malba	(4) ...on schools	(5) ...on roads	(6) ...on parks
Treatment × High slum	−0.049	0.683	0.129	−0.556	−0.016	−0.131
	(0.074)	(0.439)	(0.151)	(0.404)	(0.146)	(0.460)
Treatment	0.050	−0.325	−0.158	0.115	−0.015	0.516
	(0.057)	(0.311)	(0.105)	(0.291)	(0.106)	(0.348)
High slum	−0.002	−0.117	−0.034	0.386	−0.045	0.123
	(0.065)	(0.380)	(0.129)	(0.343)	(0.126)	(0.398)
Pre-treat spending control	Yes	Yes	Yes	Yes	Yes	Yes
Strata (zone–party) FE	Yes	Yes	Yes	Yes	Yes	Yes
Control mean	4.555	1.847	−0.179	−0.418	3.865	0.710
Control s.d.	0.248	1.539	0.567	1.410	0.500	1.655
p-value: $T \times High + T + High = 0$	0.994	0.462	0.531	0.850	0.488	0.150
p-value: $T \times High + T + High = T$	0.254	0.018	0.216	0.468	0.488	0.975
p-value: $T \times High + T + High = High$	0.977	0.235	0.774	0.108	0.755	0.207
Observations	227	227	227	227	227	227

Heteroskedasticity-robust standard errors in parentheses. * $p < .10$, ** $p < .05$, *** $p < .01$.

Ward-level OLS regression. "Treatment" indicates observations of a ward in which a report card on the performance of the MCD councilor was published in a newspaper during the 2012 pre-election period (T1 or T2, ITT). Amount of spending is in lakh rupees. Spending is categorized by lexical heuristic.

Table A.VII: Effect of any newspaper report card treatment and ward's slum area fraction on MCD councilor spending according to rwa preferences

| | Log of spending on issues, each weighted by fraction of RWAs in city reporting that… | | | |
| | (1) | (2) | (3) | (4) |
	…it is the most problematic	…it is one of the two most problematic	…it is one of the three most problematic	…residents have approached them for it
Treatment × High slum	1.215**	0.691**	0.663**	0.244
	(0.483)	(0.302)	(0.289)	(0.230)
Treatment	−0.606*	−0.392*	−0.385*	−0.166
	(0.337)	(0.217)	(0.208)	(0.174)
High slum	−0.518	−0.389	−0.383	−0.203
	(0.420)	(0.270)	(0.256)	(0.197)
Pre-treat spending control	Yes	Yes	Yes	Yes
Strata (zone–party) FE	Yes	Yes	Yes	Yes
Control mean	1.165	3.185	3.625	5.565
Control s.d.	1.815	1.090	1.037	0.729
p-value: $T \times High + T + High = 0$	0.805	0.708	0.649	0.510
p-value: $T \times High + T + High = T$	0.009	0.056	0.068	0.755
p-value: $T \times High + T + High = High$	0.069	0.149	0.161	0.613
Observations	227	227	227	227

Heteroskedasticity-robust standard errors in parentheses. * $p < .10$, ** $p < .05$, *** $p < .01$.

Ward-level OLS regression. "Treatment" indicates observations of a ward in which a report card on the performance of the MCD councilor was published in a newspaper during the 2012 pre-election period (T1 or T2, ITT). Amount of spending is in lakh rupees. Spending is categorized by lexical heuristic.

Table A.VIII: Effect of any newspaper report card treatment on MCD councilor spending according to slum preferences

	Log total spending (2010–12)		Pro-poor spending index (2010–2012)		Attendance index (2010–12)	
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment × Survey ward		−0.006		0.529*		0.025
		(0.080)		(0.281)		(0.211)
Treatment	0.005	0.006	−0.010	−0.234	0.030	0.025
	(0.040)	(0.063)	(0.133)	(0.194)	(0.100)	(0.144)
Survey ward		−0.008		−0.084		0.039
		(0.068)		(0.240)		(0.170)
Pre-treat outcome control	Yes	Yes	Yes	Yes	Yes	Yes
Strata (zone–party) FE	Yes	Yes	Yes	Yes	Yes	Yes
Post-treat control mean	4.552	4.552	−1.854	−1.854	−0.394	−0.394
Post-treat control s.d.	0.243	0.243	1.053	1.053	1.162	1.162
p-value: $T \times Svy + T + Svy = 0$		0.904		0.311		0.557
p-value: $T \times Svy + T + Svy = T$		0.742		0.003		0.587
p-value: $T \times Svy + T + Svy = Svy$		0.999		0.130		0.734
Observations	240	240	240	240	240	240

Heteroskedasticity-robust standard errors in parentheses. * $p < .10$, ** $p < .05$, *** $p < .01$.

Ward-level OLS regression. Pro-poor spending index components are each log spending on issues, with each issue weighted by the fraction of slum households in the city reporting that (1) it is the most problematic in the area, (2) it is a problem for them, and (3) it is a problem for the community. Attendance index components are councilor attendance at (1) the general assembly and (2) councilor committee meetings. "Treatment" indicates observations of a ward in which a report card on the performance of the MCD councilor was published in a newspaper during the 2012 pre-election period (T1 or T2, ITT). Spending is categorized by lexical heuristic.

Table A.IX: Effect of State of Sanitation Information on drainage management

	(1) Total drains	(2) Drains with proper disposing (%)	(3) Drains clogged (%)
SSI treatment	−0.006	−0.087	−0.048
	(0.089)	(0.052)	(0.133)
Control mean	1.200	0.136	0.500
Observations	132	132	132

Standard errors clustered by ward in parentheses. * $p < .10$, ** $p < .05$, *** $p < .01$.

Slum-level OLS regression. "SSI treatment" indicates observations in a slum of which the MCD councilor received State of Sanitation Information (ITT). "Total drains" is the number of drains in the slum. "Drains with proper disposing (%)" is the fraction of drains from which extracted garbage was taken to a dhalao or a landfill, rather than left by the drain or burned. "Drains clogged (%)" is the fraction of drains which are so clogged with trash at any point that the water is not visible.

Table A.X: Effect of newspaper report card publication on councilor-level electoral outcomes, estimated with randomization inference

	Councilor runs in any ward		Councilor runs in other ward		Councilor runs in other ward controlled by party	
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment × Ineligible		0.091		0.156**		0.116*
		[0.428]		[0.043]		[0.088]
Treatment	0.120	0.071	0.039	−0.012	0.044	0.007
	[0.103]	[0.405]	[0.209]	[0.594]	[0.105]	[0.899]
Strata (zone–party) FE	Yes	Yes	Yes	Yes	Yes	Yes
Ineligible control mean	0.000	0.000	0.000	0.000	0.000	0.000
Eligible control mean	0.478	0.478	0.022	0.022	0.000	0.000
Observations	240	240	240	240	240	240

Randomization inference p-values in brackets. * $p < .10$, ** $p < .05$, *** $p < .01$.

Councilor-level cross section estimated with RI, 1000 repetitions. Randomization strata were fixed. This is the RI version of Table III.

Table A.XI: Effect of newspaper report card publication by treatment arm on councilor-level electoral outcomes

	Councilor runs in any ward	Councilor runs in other ward	Councilor runs in other ward controlled by party	Councilor's vote share (0 if didn't run)	Councilor wins in any ward
	(1)	(2)	(3)	(4)	(5)
T1: 2012 report (ITT) × Ineligible	0.186	0.144*	0.097	0.008	0.030
	(0.151)	(0.086)	(0.073)	(0.059)	(0.128)
T2: 2010/12 reports (ITT) × Ineligible	0.043	0.166**	0.129**	0.032	0.121
	(0.128)	(0.070)	(0.064)	(0.055)	(0.111)
T1: 2012 report (ITT)	0.013	−0.017	0.003	0.042	0.046
	(0.112)	(0.019)	(0.013)	(0.048)	(0.104)
T2: 2010/12 reports (ITT)	0.096	−0.009	0.009	0.028	−0.040
	(0.091)	(0.021)	(0.016)	(0.037)	(0.085)
Ineligible	−0.473***	−0.027	−0.009	−0.175***	−0.308***
	(0.080)	(0.022)	(0.016)	(0.033)	(0.076)
Strata (zone–party) FE	Yes	Yes	Yes	Yes	Yes
Ineligible control mean	0.000	0.000	0.000	0.000	0.000
Eligible control mean	0.478	0.022	0.000	0.171	0.304
Observations	240	240	240	240	240

Heteroskedasticity-robust standard errors in parentheses. * $p < .10$, ** $p < .05$, *** $p < .01$.

Councilor-level cross section estimated with OLS.

Table A.XII: Effect of newspaper report card publication, attendance, and spending by treatment arm on councilor-level electoral outcomes

	Councilor runs in any ward			Councilor runs in other ward			Councilor runs in other ward controlled by party		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
T1: 2012 report (ITT) × Ineligible × … …Pro-poor Spending Index			0.325** (0.127)			0.159** (0.065)			0.111* (0.059)
T1: 2012 report (ITT) × Ineligible × … …Attendance Index			1.615** (0.807)			1.106 (0.679)			1.087* (0.629)
T2: 2010/12 reports (ITT) × Ineligible × … …Pro-poor Spending Index			0.172 (0.138)			0.108 (0.073)			0.049 (0.066)
T2: 2010/12 reports (ITT) × Ineligible × … …Attendance Index			0.458 (0.659)			−0.406 (0.332)			−0.336 (0.292)
T1: 2012 report (ITT) × … …Pro-poor Spending Index			−0.104 (0.106)			−0.040 (0.033)			0.002 (0.017)
T1: 2012 report (ITT) × … …Attendance Index			−0.315 (0.560)			−0.124 (0.117)			−0.059 (0.087)
T2: 2010/12 reports (ITT) × … …Pro-poor Spending Index			−0.059 (0.101)			−0.050 (0.033)			−0.007 (0.017)
T2: 2010/12 reports (ITT) × … …Attendance Index			−0.097 (0.481)			−0.052 (0.133)			−0.004 (0.100)
T1: 2012 report (ITT) × Ineligible		0.186 (0.151)	−0.876 (0.564)		0.144* (0.086)	−0.617 (0.447)		0.097 (0.073)	−0.657* (0.392)
T2: 2010/12 reports (ITT) × Ineligible		0.043 (0.128)	−0.255 (0.476)		0.166** (0.070)	0.423 (0.258)		0.129** (0.064)	0.345 (0.229)
T1: 2012 report (ITT)	0.076 (0.083)	0.013 (0.112)	0.217 (0.395)	0.035 (0.032)	−0.017 (0.019)	0.062 (0.079)	0.039 (0.027)	0.003 (0.013)	0.039 (0.063)
T2: 2010/12 reports (ITT)	0.143* (0.074)	0.096 (0.091)	0.162 (0.335)	0.041 (0.025)	−0.009 (0.021)	0.023 (0.093)	0.047** (0.021)	0.009 (0.016)	0.009 (0.075)
Nontreatment Interactions	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Strata (zone–party) FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Ineligible control mean	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
Eligible control mean	0.478	0.478	0.478	0.022	0.022	0.022	0.000	0.000	0.000
Observations	240	240	240	240	240	240	240	240	240

Heteroskedasticity-robust standard errors in parentheses. * $p < .10$, ** $p < .05$, *** $p < .01$.

Councilor-level cross section estimated with OLS. "Attendance Index" is overall councilor attendance at MCD committees of which they are a member, 2007–10. "Pro-poor Spending Index" is the mean z-score of three log preference-weighted spending amounts (2007–11), analogous to the dependent variable in Table I, column 3, for the pre-publication period.

Table A.XIII: Effect of newspaper report card publication, attendance, and spending on ward-level electoral outcomes

	Voter turnout			Voter registration		
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment × Ineligible × … …Pro-poor Spending Index			−0.003 (0.017)			−1464.691 (3481.315)
Treatment × Ineligible × … …Attendance Index			−0.135 (0.093)			5555.580 (20582.760)
Treatment × … …Pro-poor Spending Index			−0.009 (0.013)			1765.208 (2774.616)
Treatment × … …Attendance Index			0.097* (0.054)			−5912.027 (16480.880)
Treatment × Ineligible		−0.001 (0.015)	0.089 (0.063)		−460.830 (2519.533)	−4065.476 (14371.841)
Treatment	−0.008 (0.007)	−0.008 (0.009)	−0.074** (0.035)	−744.579 (1420.318)	−702.669 (1941.576)	3343.741 (11956.598)
Nontreatment Interactions	No	Yes	Yes	No	Yes	Yes
Strata (zone–party) FE	Yes	Yes	Yes	Yes	Yes	Yes
Ineligible control mean	0.534	0.534	0.534	40226.269	40226.269	40226.269
Eligible control mean	0.554	0.554	0.554	43269.239	43269.239	43269.239
Observations	240	240	240	240	240	240

Heteroskedasticity-robust standard errors in parentheses. * $p < .10$, ** $p < .05$, *** $p < .01$.

Ward-level cross section estimated with OLS. "Attendance Index" is overall councilor attendance at MCD committees of which they are a member, 2007–10. "Pro-poor Spending Index" is the mean z-score of three log preference-weighted spending amounts (2007–11), analogous to the dependent variable in Table I, column 3, for the pre-publication period.

Figure 4: Project Timeline Events

Delhi Voter Project Timeline

Event	Start	End	Category	Description
MLA elections	2003-12-01	2003-12-01	Elections	
MCD spending data (Including 2012, not reported)	2007-04-01	2012-04-01	Newspaper intervention	
MCD spending data (2012 newspaper report card)	2007-04-01	2011-04-01	Newspaper intervention	
MCD spending data (2010 newspaper report card)	2007-04-01	2009-04-01	Newspaper intervention	
MCD elections	2007-04-17	2007-04-17	Elections	BJP victory
MLA elections	2008-10-29	2008-10-29	Elections	INC victory
HH Baseline #1	2010-02-01	2010-08-01	HH baseline	
Treatment councilors informed of 2012 report cards	2010-05-01	2010-06-01	Newspaper intervention	control councilors informed no report cards until 2014 at earliest
RWA Baseline	2010-05-10	2010-06-15	RWA intervention	
Newspapers published #1	2010-06-15	2010-07-21	Newspaper intervention	
HH Baseline #2	2010-10-02	2011-07-07	HH baseline	
Audit Baseline	2011-03-03	2011-08-06	Audit intervention	Garbage, Toilet Observation, Toilets
RWA phone interviews	2011-07-21	2011-08-31	RWA intervention	
Audit Mailing #1	2011-08-01	2011-09-01	Audit intervention	From audit #1
RWA mobilization	2011-08-11	2011-08-11	RWA intervention	
Slum characteristics survey	2011-11-17	2012-01-09		
Audit Midline	2011-11-17	2012-02-03	Audit intervention	Drains, Garbage, Toilet Observation, Toilets
Counselors learned eligibility	2012-01-27	2012-01-27	Elections	
Audit Mailing #2	2012-02-01	2012-03-01	Audit intervention	From audit #2
Newspapers published #2	2012-03-01	2012-04-01	Newspaper intervention	
Candidate rolls finalized	2012-03-20	2012-03-25	Elections	
MCD elections	2012-04-15	2012-04-15	Elections	BJP victory
Audit Endline	2012-05-01	2012-07-01	Audit intervention	Drains, Garbage, Toilet Observation, Toilets
RWA Endline	2013-04-03	2013-06-11	RWA intervention	
MLA elections	2013-12-04	2013-12-04	Elections	Hung assembly

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