Information as an Incentive: Experimental Evidence from Delhi

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Abstract

Do anticipated performance disclosures influence politician behavior and, in turn, party and voter behavior? Two years prior to Delhi's 2012 municipal elections, a random sample of councillors were informed that a leading daily newspaper will report on their performance just before elections and to enhance credibility a sub-sample also received midterm report cards. To check whether improved information directly influences councillor performance, we implemented a cross-cutting experiment wherein treated councillors received 'for your eyes only' audit reports on the condition of toilets and garbage dumps in slums present in their wards. A final source of variation arises from the unanticipated expansion of gender quotas four months before the elections – the fraction of wards reserved for women was raised from 33% to 50%. We have four findings. First, in high slum density wards disclosures caused councillors to move spending closer to slum-dweller preferences. Second, the public nature of disclosures matter: the 'for your eyes only' audit report card intervention led to a limited and, arguably, perverse effect wherein the incidence of closed toilets rose in treatment wards. Third, newspaper disclosures influenced party ticket allocation. Specifically, treated incumbents who undertook more pro-poor spending but were unable to recontest from own ward due to gender quotas were more likely to receive a party ticket for a different ward. Fourth, councillors who were subject to anticipated performance disclosures benefited electorally in the 2012 elections.

1 Introduction

Can local democratic governance structures empower the urban poor? While only 20% of the world's poor live in urban areas, the poor are urbanizing faster than the population as a whole (Ravallion et al. 2007). Arguably an important contributor to the lower than average pace of urban poverty reduction is the quality of living conditions for the urban poor: 46% of urban population in non-high income countries live in slum settlements characterized by poor infrastructure which constitute significant health hazards (The World Bank 2014).

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Urban elected local bodies play an important role in affecting living standards for slum-dwellers: Given weak property rights, the main access of slum-dwellers to state amenities is often political. In Delhi, the setting of our study, surveyed slum-dwellers report that the primary official they approach for service-delivery related issues is an elected politician. Yet, despite often forming an electoral majority in a local jurisdiction, the quality of local services remains low and slum dwellers report limited awareness of the services they are eligible for.

In this paper we focus on Delhi – the world's second largest city – and examine whether enabling a higher-information political environment creates incentive effects among politicians. The theoretical motivation for our study goes back to models of political agency (Barro 1973; Ferejohn 1986; Persson et al. 2000; Besley and Coate 1997). Providing additional information in a pure agency model with no selection improves voter ability to target punishments and creates an *incentive* effect – by reducing the risk of punishing good performers, it increases the incentives for improved politician performance.

Our study covers the lowest level of elected government in Delhi – the Municipal Corporation. The Corporation is divided into 272 single-member wards, each of which elects a councilor every five years. A councilor receives an annual discretionary development fund to spend on local development, generally on the improvement of local infrastructure. Committees on which these councilors sit manage the distribution of publicly provided private goods and public goods across Delhi. Councilors are expected to be responsive to demands of their own constituents (such as for improved sanitation in their wards), and thus routinely receive citizens in their local offices.

We report on a multi-year, and multi-pronged, field experiment with elected councillors in Delhi. In the primary intervention, a randomly selected set of councillors were sent letters in May 2010 informing them that their performance will be reported on in a leading newspaper in March 2012 – the month prior to the election. A subset of the treated councillors also received mid-term reportcards during May and June 2010. In a second, cross-cutting, experiment we examined whether private information disclosures to councillors impacts their behavior. We provided a random sample of councillors audit report cards on the quality of toilets and garbage removal facilities in three slums in their ward during 2011. A final source of variation that we exploit is an unanticipated expansion in gender quotas across wards – the number of wards reserved for women was increased from 33% to 50% in January 2012. The choice of wards that were so reserved in the 2012 election was randomized and 80 of the 240 councillors were rendered ineligible for election in their current ward.

Prior to the intervention being launched in 2010, we also undertook a detailed survey of slum dwellers in high-slum density wards in our sample. This provided us with information on slum-dweller policy preferences.

We use this setting to examine how do information disclosures and quotas shape the political fortunes of urban politicians. We have four sets of findings.

First, slum dweller policy preferences shape incentive effects of anticipated public information

disclosures. Councillors in high-slum wards moved their spending decisions to be 0.65 standard deviations more pro-poor. We also observe a 0.37 standard deviation increase in committee attendance in this group. ,Second, incumbents react differentially to media disclosures v/s information only available to politicians – only media disclosures cause politicians to be responsive to citizen preference. Third, media disclosures influence party ticket allocation – treated high quality incumbents who are ineligible to contest re-election from their own ward due to gender quotas are 15 percentage points more likely to be allocated a party ticket for a different ward. And, fourth, improved party ticket allocation in treatment wards translates into electoral gains.

Our paper contributes to a large and growing literature on how political outcomes are influenced by information disclosures. Several papers that exploit random variation in information disclosures across the voter population demonstrate selection effects, such that corrupt and worse-performing incumbents are likely to fare worse in subsequent elections (Ferraz and Finan 2008; Banerjee et al. 2011; Avis et al. 2017). Voters also respond to information inferred from campaign quality (Bidwell et al. 2016; Wantchekon 2003). An important caveat is turnout as an intermediating variable. The positive link between improved voter information and better candidate selection maybe muted or absent if turnout declines in response to improved information (Chong et al. 2014).

How do performance disclosure on the eve of elections influence politician behavior? The evidence on short-run effects is mixed: there is some evidence from Philippines of more vote buying in areas where beliefs most adversely affected by information campaign (Cruz et al. 2016). There is, however, also evidence of more politician attention in areas that had debates (Bidwell et al. 2016). Finally, comparing clientelistic to non-clientelistic town hall meetings (Fujiwara and Wantchekon 2013) find that non-clientelistic meetings reduce vote share only in 'dominant' villages .

The evidence on the longer term impacts of information is more mixed and points to different channels that vary in their importance across different institutional settings. Bobonis et al. (2012) use information on Puerto Rico audits which occur according to a fixed schedule. They find that anticipated information disclosures are most effective just before election suggesting that, in this context, the longer term impact of information continues to largely reflect selection effects. In contrast, Avis et al. (2017) find that in Brazil the longer term positive impacts of malfeasance disclosures reflects importance of legal processes against those found corrupt. Finally, Bidwell et al. (2016) find evidence that areas that saw debates see greater accountability of politicians. This could reflect either greater citizen activism or improved incentive effects. Finally, a recent field experiment in Uganda with a NGO finds that providing politicians performance score cards during the election cycle improves their performance (Grossman and Michelitch 2018).

Another set of papers that examine longer term impacts of information are media studies. These papers find that areas with more media presence are characterized by better policies (Besley and Burgess 2002; Strömberg 2004; Snyder Jr and Strömberg 2010). However, this literature is less able to disentangle incentive and selection effects.

An important contribution of our paper is to initiate analysis of the role of information in affecting party behavior. In the case of decentralization, Riker (1964) argued that the success of decentralization depends on the quality of party competition. Enikolopov and Zhuravskaya (2007) provide supportive cross-country evidence. Turning to within-country evidence on party behavior, our work is related to Besley et al. (2017) who found that gender quotas increased intra-party male competition in Sweden.[1] We are able to make progress on identification by exploiting two sources of exogenous variation: the expansion of gender quotas across Delhi wards (which were randomly implemented) and random variation in performance information on incumbents available to parties and citizens.

To summarize, our paper contributes to the existing literatures on the role of information in enhancing electoral accountability by reporting on a large scale media experiment that quantifies the incentive impacts previously identified by quasi-experimental media studies. We demonstrate a discernible link between voter preferences, information disclosures and policy outcomes. We also show that anticipated disclosures via newspapers can suffice to create this link in a setting with lower literacy rates. Finally, using a second experiment we provide evidence that these effects likely reflect the public nature of disclosures. Finally, using variation in councillor ability to recontest from own ward (created by gender quotas) we show that the link between increased intra-party competition and candidate quality is mediated by availability of information.

2 Setting, Data and Summary Statistics

2.1 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 to half of Delhi's population lives in slums (Delhi Human Development Report 2006).[2]

Delhi has a two-tier elected governance structure – a state legislature and a municipal corporation – each of which controls a different set of public services. This paper focuses on Delhi's elected city government, or municipal corporation (MCD).[3] This political body was constituted as part of India's local body decentralization (under the 74th constitutional amendment), with the first set of elections

[1] More broadly, on gender quotas, Beaman et al. (2012) found that they increased political entry by women in the longer run.

[2] The difference in estimates largely reflects differences in which settlements are included as slums – squatter settlements, illegal sub-divisions and/or unauthorized colonies. A majority of slums are built on public land – owned by municipal bodies, railways, state government, or other public entities. Slums in Delhi are also typically surrounded by residential areas. In terms of legal status, slums can be categorized as notified or non-notified. Notified slums were legally notified or declared as slum areas under the Slum Areas (Improvement and Clearance) Act of 1956. Non-notified slums such as JJ clusters are considered to be an illegal encroachment on land (DUSIB 2010)

[3] Delhi also enjoys the status of a state, and separate state-level elections are used to choose state legislators.

held in 1997. MCD Elections occur at the ward-level via plurality rule every five years, and our interventions occurred with incumbents elected in 2007 at which point the number of wards was set to 272.

India's decentralization act also introduced political reservation; in Delhi, reservation occur for two groups – scheduled castes (SC) and women. In 2007, 46 wards were reserved for SC candidates. 16 of the SC-reserved and 76 of the other wards were reserved for female candidates. The rule for identifying SC seats was based on the twin criteria of ranking wards in decreasing proportion of SC population and limiting them to not more than two per state legislature constituency. For women, the criterion of reservation was of allotting every third seat from the list of wards arranged as per the serial numbers of the wards.

Our interventions spanned the two years in the run-up to the April 2012 election. In December 2011, the Delhi government announced that gender quotas would be increased to 50%. In the two categories of SC reserved wards and other wards, half the wards would be reserved for women. In each category wards would be listed by serial number and every odd seat reserved for a woman.

MCD elections tend to be party-based. In 2007, India's two main national parties BJP and INC had the largest share of councillors at 65% and 25% respectively. Turning to the role of parties, India's two main national parties – BJP and INC – were also the most important parties in Delhi elections. The party candidate choice for each ward is directly determined by party leaders and there are no primaries.

B. Councillor policy powers

Broadly, councillors enjoy four policy levers. First, legislative activism during municipal council assembly. Between 2007 and 2012, the legislature met, on average, 24 times per session. The average councillor attended 19 times per session. Second, legislators belong to standing committees which are intended to be responsive to citizen complaints.

Third, councillors receive an annual discretionary fund for development works in their ward. The annual amount varies and averaged 700,000 USD per year during 2007-2102 election cycle.[4] 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 measured in the household survey of slum-dwellers and by neighborhood associations (these surveys are described in the data section. Overall, we see very little alignment between councilor spending choices and citizen preferences. Councillors spend a majority of their funds (57%) on roads. In contrast, slum-dwellers and leaders of neighborhood associations report the most problematic issues in their areas to be water, sewage and garbage. Water in Delhi is privately provided; however, the provision of drains and toilets constitutes a far lower proportion of councillor funds (relative to roads) at 17%. The

[4]The amounts were Rs. 71 lakhs in 2007–08, Rs. 2 crore in 2008–09, Rs. 50 lakhs in 2009–10 and Rs. 50 lakhs in 2010–11.

next two expense categories for councillors do not obviously meet slum-dwellers' interests either: provision and repair of lights (8%) and the improvement of parks and provision of gates (7%).

Fourth, the provision and maintenance of local public goods including public toilets, garbage removal and cleaning of drains. In the case of Delhi, much of this is via management or concession contracts with private or non-government organizations.[5] In addition to directly spending resources in this sector, councillors play a role in choosing the contractor, and can play an important role in lobbying/chiding contractors.[6]

2.2 Data

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

A. Citizen preferences

We are interested in councillor engagement with slum-dwellers and poor citizens, especially in wards where these groups form an electoral majority. Our NGO partner's direct citizen activism work (which was corroborated by our previous work with the NGO reported in Banerjee et al 2011) suggested that councillors' faced strong incentives to be responsive to constituents in densely populated low income neighborhoods where the infrastructure needs much more improvement. In these neighborhoods, a rupee spent went very far in making potential voters happy.

We therefore focused our household survey on low-income neighborhoods. Alongside, we fielded a survey with office-holders of Resident Welfare Associations (RWAs). These are neighborhood associations that are registered with Delhi government and typically include neighborhoods, rich and poor, that have full legal status. We describe the two surveys in turn.

A.1 Household survey data

The household survey was conducted in May 2010 and covered a sample of 5481 households across Delhi slums in 107 wards. The Appendix describes our ward selection and also how we established a sample frame of urban slums.

[5] The typical public toilet contract sets the maximum user price, states which facilities should be available, and requires regular cleaning of toilets. Contracts are awarded separately for each toilet facility for a period of 20–30 years 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 two bins, one for non-biodegradable and one for recyclable/biodegradable waste, segregate the waste, and collect it daily (IL & FS Ecosmart Limited 2007). The typical garbage contract is awarded at the zonal level for a period of nine years and includes a performance evaluation mechanism.

[6] Delhi Human Development Report (2006) summarizes councillors' ability to affect the well-being of Delhi's citizens as *"The role of councilors in policy-making is minimal and entails 'getting things done' through their interface with citizens on the one hand, and the executive wing of the* MCD*, on the other. The councilors enjoy a greater status, as they control the constituency funds and this enables them to decide which works will be undertaken and where. The councilors also exercise some power over officials: directing them, causing transfers to be effected, and reporting accounts of corrupt practices or of insensitivity towards citizen demands."* Also see Singh (2010)

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, usage and difficulties with respect to social services (such as health facilities, sanitation, schools, water, electricity and law and order) and transfers (such as subsidized food rations and pensions) as well as their knowledge of the local government system, interactions with public officials and politicians, and political preferences and participation.

A.2 RWA survey data

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 2011).

Our RWA sample was drawn from the Delhi government list, which we matched to wards based on the RWA's stated address. Overall, we surveyed 250 heads or members of RWAs across 94 of our 107 baseline wards. This survey gives us a measure of the areas of public good provision that the RWA leaders consider as the most problematic.

Figure 1 provides ward-level descriptive statistics of councillor spending and constituent preferences. We measure the latter using data from the survey of slum dwellers and the RWA survey. For both groups, sewage and drainage and garbage collection are the biggest problem areas.

B. Audit data on local public goods

In each ward our surveyors audited, an average, three low-income slums at three points in time: between April–June 2011, November–January 2011/2012, and April–June 2012.

All audits covered toilets and garbage points (dhalaos) and the second and third audit also covered drains. For each facility audited, 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 a confirmation was received from at least three residents.

During a facility audit the surveyors observed and noted the quality of the public amenities and interviewed two respondents per 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.

C. Councilor performance data

Our data on official councilor performance – which both featured in the report cards and forms an important outcome measure for us – was obtained by SNS under the Right to Information Act. These data were obtained for all wards and similarly collated for treatment and control wards; only treatment ward data was subsequently released.

The report card included data on the total discretionary funds spent by the councillor and category-wise spending. The categories were: (i) Roads and Lanes (included construction/repair of roads and lanes); (ii) Sewage and drainage (includes drains and toilets); (iii) Streetlights (includes providing, repairing street lights and high mast lights); (iv) Parks and greenery(Improvement of parks, providing designer gates); (v)Education and schools (Improvement of MCD schools); (vi) Garbage removal (Supply of material, engaging truck in the ward; Construction of garbage dumps; Removal of *malba* (rubble)); (vii) Others.[7].The average pattern of spending is reported in Figure 1 in the paper. Roads get more than 50% of the spending.

The report card also reported councillor attendance in the legislature and also on committees. After the standing committee and zonal committees, some of the largest committees by membership included those on health, the environment, slums, remunerations, malaria, law, Hindi, gardens, national festivals, and appointments. The average assembly attendance rate was 81%, and the average overall committee attendance rate was 66%.

D. Electoral results 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 whether the ward was reserved for women or Scheduled Castes. The reservation status of wards was announced in January 2012. For each incumbent councillor we coded a *Ineligible* dummy which equalled one if the councillor cannot recontest from own ward due to the quota.

3 Experimental Design

3.1 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, set up 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 closely 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 information about the functioning of the government. The presumption is that such requests will typically be granted unless there is a special reason not to.

[7]This includedConstruction of MCD offices; Improvements to staff quarters; Improvements to *katras* (high-slum-index tenements); Improvements to community centers, health centers; Construction of boundary walls; Providing grills on *chabuttras* (monuments); Providing street name boards; and Other

SNS has a long history of creating and disseminating report cards on the performance of incumbents and record of leading candidates for state and national legislatures. They typically partner with media outlets for this. Banerjee et al (2011) evaluated one such intervention by SNS during the 2010 Delhi state elections and as already mentioned, found that the intervention increased the vote share of the better performing candidates.

Our second partner was a leading Hindi daily newspaper *Dainik Hindustan*, which is second in terms of market share in Delhi. The newspaper published report cards on incumbents (prepared by SNS) in 2010 and 2012. Figure 2 is an example of one daily report: it includes report cards on four politicians, as was typical. It includes each politician's photo, patterns of spending from his discretionary funds, which committees he served on, and his attendance at those committees.

Our third partner was the in-house JPAL South Asia team that conducted public service (toilet, garbage, and drain) audits and then disseminated audit report cards to politicians.

3.2 Field Experiment: Design

We conducted two separate interventions between 2010 and 2012, and we describe them in turn.

3.2.1 Newspaper report card intervention

The newspaper intervention was designed to assess whether informing politicians that their performance is being reported to voters has incentive effects on politician behavior.

Sample Our intervention sample consists of 240 wards, and our assignment procedure stratified on incumbent party and zone.[8] We randomly assigned 72 to be control wards, 58 to only receive pre-election report-cards (T1) and 110 to receive mid-term and pre-election report cards (T2).

Informing politicians about report cards Councillors 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 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

[8] Overall Delhi has 272 wards and we excluded 32 wards from our intervention sample: 5 because our partner NGOs were already doing extensive work in the communities; 10 because their Councillors were elected in by-elections less than two years ago, and zones 9 and 10, with 17 wards, were dropped because they contained rural areas or had very few to no slums. There are twelve geographically contiguous zones in Delhi, each comprising an average of 15 wards.

elections, we 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 has now prepared mid-term report cards of Councillors of the MCD to inform people of the development work being done by Councillors for the welfare of the residents of their wards. Due to limited resources, we have not been able to prepare report cards for all 272 councillors 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 *Unfortunately, your ward is not in the list of 120 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 letter for T2 councillors 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 Councillors for the welfare and betterment of their wards.*

The councillors 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).

Both T1 and T2 received a reminder letter in February 2011.

Report-card format The information featured in report cards came from our NGO partner Satark Nagrik Sangathan's use of the RTI Act to obtain data from the MCD on Councillor spending, meeting attendance, and committee membership.

The midterm report cards were published over (roughly) a month starting May 27th 2010. Every day one half-page of the newspaper featured 3 report-cards.Each report-card included the councillor's photo, patterns of spending from their discretionary funds, which committees they were on, and committee attendance. The data reported on covered the period April 2007-March 2009.

The 2012 report cards had the same format as those issued in 2010. They covered the time-period April 2007-March 2011. As data is released by fiscal year we could not include spending in the last year of the councillor's term. The report-cards were published in March 2012 – the month prior to the election and on a given day the newspaper featured four ward councillors.

The format of data provided in the report-card was held constant in 2010 and 2012. The report-cards were attributed (in the newspaper) as having been collated by SNS using official data obtained under the Right to Information Act.

An important reason for multiple treatment arms - T1 and T2 - was to ensure credibility of our main pre-election treatment in 2012. The two treatment arms do not, by themselves, allow us to

disentangle information and incentive effects. To provide some evidence on the relative importance of these two potential mechanisms we implemented a second field experiment over the same time-period.

3.3 For Your Eyes Only: Audit report card intervention

We designed this intervention to examine the impact of directly providing councillors audit reports on the state of toilets and garbage dumps in low-income neighbourhoods in their wards.

Sample Our audits were conducted in the 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 Councillor receive a ward report card and, out of the 55 ACs, 27 were randomly assigned to receive a AC reportcards.[9]

Report-cards The report cards were based on audits of public toilets and garbage dumps (dhalaos[10]) conducted in 317 low-income neighbourhoods, predominantly slum areas, located in these 100 wards. In total, three rounds of audits were conducted, with report cards based on the first two mailed to councillors (and legislators) in treatment groups.

The first round of report cards was distributed in August 2011, about eight months before the elections for the ward councillors, and the second round was sent in February 2012, only a couple months prior to the elections. The first summarized the baseline audits (Round 1), conducted between April and June 2011, and the second compiled Audits (Round 2) conducted between November 2011 and January 2012. The final set of audits was conducted immediately after the MCD elections, from April through June 2012.

Information on the state of drains was also collected, but not shared with politicians, during the second and third rounds of audits to check if there were any spillover effects on this service (these could have been positive, or negative if there was a diversion of effort away from drain provision).

[9] 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 Councillor received a report card, and 26 were both the MLA and MCD Councillor received report cards.

[10] The Master Plan for Delhi defines a dhalao as "a premise used for collection of garbage for its onward transportation to sanitary landfills" (Chintan Environment Research and Action Group 2004). The City Development Plan (2007) defines dhalaos as "large masonry dustbins."

4 What to expect

Political agency models consider elections as contracts between citizens (as the principals) and politicians (as agents) who are responsible for making policy decisions. It is reasonable to anticipate both adverse-selection and moral-hazard problems (Coate and Morris). Provided politicians value holding office in the future, elections can generate incentives for politicians to appear honest and able, and can deter politicians from choosing corrupt policies. We are interested in examining whether this *incentive* effect is strengthened when politicians learn that voters will gain information on their performance prior to the election.

Information can incentivize politician behavior through multiple mechanisms in political agency models. Providing additional information to citizens in a pure agency model with no selection improves voter ability to target punishments and creates an *incentive* effect – by reducing the risk of punishing good performers, it increases the incentives for performance.The incentive effect may be magnified by a potential *activism* effect – where informed voters demand better performance from politicians. A separate channel is the *selection* effect which comes from the ability of voters and/or parties to get rid of poor performing candidates (under the assumption that performance, in part, is a personal trait). A third channel is a direct information effect if report-cards provide politicians information they didn't have, or prime them about it.

4.1 Impact on councillor behavior

The *incentive effect* captures the idea that anticipated disclosure of performance would cause the councillor to adjust performance so as to make his/her report card look more attractive to the voters. Our working hypothesis was that councillors' would target these changes to better match public good preferences of low income voters. Councillor discretionary funds are often the primary source of infrastructure spending in slums – deploying discretionary funds towards slum-dwellers can benefit a large number of people. Therefore, in addition to examining effects on total spending, committee attendance etc, we will be interested in the type of spending. In particular, we ask whether the spending becomes more pro-poor, and are these effects concentrated in wards with relatively high slum populations. Arguably, in low slum wards we may even see effects in the opposite direction.

The *information effect* is largely associated with T2: the group of councillors who received report-cards in 2010 would have improved information of their own performance.

Thus, comparing across T1 and T2 provides some, but limited, ability to disentangle incentive and information mechanisms. Stronger effects for T2 relative to T1 could reflect the additional direct impact of improved information for T2 councillors. Specifically, since total spending in any single category reflects spending on many separate projects in different locations and years, councillors may not always see the shape of their own spending and know how that differs from other councillors. However, T1 councillors could arguably infer much of this information with small effort: they know

own performance and can now see their colleagues report cards.

That said, the incentive effect for T2 may be made stronger by activism by citizens informed by the midterm reportcards on councillor performance. Another reason would be that report card credibility was higher among those who got twice and this strengthened the incentive effect for this group.

Our main specification for the newspaper reportcard analysis focuses on examining the pooled effect of the two treatments on politician and party behavior (prior to elections). We always use an intention-to-treat (ITT) framework.

To provide evidence on the independent effect of improved information, we turn to the FYEO experiment. Here, the idea is to examine whether any of the outcomes reported in the first two report cards on the number of open toilets, their quality, price and usage improve by the time we collect data for the final round. Specifically, by providing the councillors with specific actionable information at two points in time (where one was close to elections), we would test the hypothesis that councillor ability to undertake actions favored by citizens is constrained by information. The nature of public goods being audited lent itself to simple things councillors could do to make things better: spend discretionary funds to fix the toilets, put pressure on the contractors who run the toilets and on the municipal employees who are meant to pick up the garbage. We also directly examine the impact on spending.

Finally, councillors recognized that the reservation status of their ward could change in 2012. Whether this enhances or dampens the incentive effect at work is ambiguous. Those who perceive a higher probability of being term-limited (men) or facing stronger electoral competition (women who's wards switch from being reserved to unreserved) could either decide to 'give' up or they may be incentivized to work harder in order to get a party ticket in a different or same jurisdiction. As we discuss below, we exploit the additional quota surprise due to the unexpected expansion of reservation policy announced in December 2011.

4.2 Impact on electoral outcomes

Councillor's electoral outcomes could be affected through multiple channels.

First, it could affect the likelihood that the councillor gets a party ticket and is able to rerun for election in 2012. This effect could be present for all incumbents, but would likely be stronger for incumbents' who are term-limited by the reservation of their ward for a demographic category that excludes the incumbent. For high-performance incumbents, this would (a) show the party that he/she can get things done (b) impress potential voters who do not know him/her with her performance. For low performance incumbents, the incumbent may want the report card suppressed, but the party could still condition on a published report card. Arguably, parties could access such data independently so the additional impact of newspaper reporting was greater salience and/or lower costs of data collation.

Second, conditional on receiving a party ticket, treated councillors may see their vote share be affected. The information provided in a report card could directly influence voters' choice. The direction of effect depends on voters' priors and how they process information.If the report card incentivized better performance by incumbents' then this could directly improve his/her electoral prospects. However, if voters' know that politicians' anticipated the information disclosure then they may choose to discount what they consider to be 'signalling' behavior. This, of course, assumes that voters care about politician's innate preferences *not* just their performance.Arguably, that is unlikely. More likely is the case that voters' take into account the fact that politicians' who are more responsive are more competent and/or more willing to focus on low income voters.This effect would especially dominate if voters don't know that politicians were forewarned (and we have little reason to believe voters had such information).

Another channel through which information processing could work is yardsticking:the newspaper printed four report cards every day, so voters can compare their councillor to others in the city. Banerjee et al. (2011) reports evidence of voters using other incumbents as yardsticks. The sign of the average effect of yard-sticking is ambiguous and depends on how the average politician performed relative to the voters' priors. For example, if voters start from the assumption that most politicians only spend on roads, the report card may make them realize that, on average, this is incorrect making them more inclined to vote for incumbents in general. Of course if voters are less sophisticated and take information at face value, they will also weight the relative aspect of the information and favor voting mfor better performers.

5 Implementation and Balance Checks

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

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 reportcards were published. Within our sample of ITT councillors, we randomized the order in which we sent reportcards for publication to the newspaper.

Throughout we estimate ITT effects. Appendix Table **??** and **??** provide balance checks. Panel A and B of Appendix Table **??** considers pre-intervention councillor 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 **??** we consider 2007 electoral outcomes as the dependent variable. Here, we examine both the direct

effect of report cards and the effect of remaining eligible (in that incumbent is not deemed ineligible to run from own ward due to reservation).

6 Does information influence councillor behavior?

First, we are interested in the incentive effect on councillors knowing that voters will be informed about their performance prior to the election.

6.1 Anticipated disclosures: Newspaper reportcards

Our base specification for estimating treatment effects in ward w is an ANCOVA specification which can account for the possibility that changes in outcomes may be smaller in wards where baseline investments in that outcome were already high.

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

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

T_w is the treatment status of the ward – our main specification combines T1 and T2. In Table I we show the specification where we pool T1 and T2. In Table II we report the specification where we separate T1 and T2. Throughout we report ITT estimates.

In terms of outcome variable y_{w1}, we consider three outcomes: total spending, pro-poor spending and councillor attendance. All three are measured for the fiscal years 2010-2012. The lagged dependent variable, y_{w0} represents the average for the years 2007-2009 of the same variables.

To the extent that rich and poor households differ in their spending preferences we anticipate that our treatment will have a positive outcome in high slum density wards.

We use data on slum incidence to define a a dummy for high-slum wards: H_w. A ward is 'high slum' if the fraction of slums in that ward are above the median value computed across all wards (the median value is 45%). Of the 94 wards where we sampled RWAs, 57 were high slum and of the 106 wards where we surveyed slum-dweller preferences 86 are high slum. In Appendix Table A.V we report specifications where we interact treatment with a dummy for whether the ward was a surveyed ward - given that we sampled more slummy wards, we see similar results across the specifications.

We are interested in examining whether the anticipation of being reported on just prior to the elections caused treated councillors to move spending in the direction favored by voters. As discussed above, we have information on preferences of low-income voters via our slumdweller and RWA baseline surveys. We use these data to construct a pro-poor spending index as follows: we consider log (spending amount) in five categories of spending: Roads and lanes, Sewage and drainage, Parks and greenery, Education and Schools and Garbage removal. We separately use

data from the slum dweller and RWA surveys and weight spending by citizen preference weights. In the case of slumdweller survey, we (separately) use three different weighting criteria. First, the fraction of slum households in the city reporting that (1) the issue is the most problematic in the area, (2) it is a problem for them, (3) it is a problem for the community. In Table I we consider the mean z-score of these three (log-weighted) spending amount measures. In Appendix Table A.III we consider the three weighted log spending amounts as separate outcomes and show that the results are consistent across all three measures. Similarly, for RWA preferences and weight log spending in each category by the fraction of RWAs that report it as one of the three top problem issues.

For attendance, our measure is meeting attendance in 2010–12 by the ward's councilor. We construct the mean z-score across two measures: (1) annual attendance in legislature and, (2) annual overall attendance across all of the councilor's committee meetings.

Table I reports the results where we pool treatment effects. Columns (1) and (2) consider log total spending. Ward councillors typically spent their entire budget. Reflecting this, anticipation of report cards has no discernible effect, either on average or in high slum wards. Next, we turn to the preference weighted spending measure where we use slum-dweller preferences (column 3). The impacts differ across wards: When we examine across high and low slums we see that the effect of high slum interacted with treatment is always positive and significant (column 4). The effect of un-interacted treatment, on the other hand, is noisily estimated but negative. This picks up the effect in the low slum wards, and is consistent with the fact that the preferences in the low slum wards are likely to be very different.[11] In columns (5) and (6) we consider spending weighted by RWA preferences and observe very similar patterns.

Finally, in columns (7) and (8) we consider our attendance index. The impacts of report cards is again concentrated in high slums, which potentially capture the fact that several of the main committees – such as the slum development and ration committee – are particularly relevant to low income households. Table II consider the impacts separated by T1 and T2. In general, we cannot reject identical effects across the two treatment arms, even though the effects of T2 are typically more precisely estimated.

6.2 Private Disclosures: FYEO audit reportcards

The newspaper report card results show a significant politician response to reportcards. While we observe similar reactions across T1 and T2 groups, there remains an open question of intermediating mechanisms: Did information received in 2010 directly produced the observed effect by causing politicians in both treatment groups to become better informed about spending patterns? To provide some evidence on the direct effect of informing politicians on policy choices, we turn to the *For your eyes only* audit report card experiment.

[11] In Appendix Table X we report similar findings when we use a dummy for being a survey ward instead of being a high slum ward and find very similar results

We consider two sets of outcomes: first, for comparability we consider spending on different categories where our primary interest is spending on garbage/malba and drains (we show all spending criteria for comparability). Here, we estimate regressions of the same form as in equation (2) with the difference that the treatment category is whether the councillor received audit report cards. The results are in Table III. We see no significant impact of the treatment on the spending categories relevant for audit report cards: drains and sewage (column 2) and garbage (column 3). If anything, we see some rise in road spending which is not a category of spending prioritized by slum-dwellers.

However, one may be concerned that the spending categories are too broad. Further, councillors may influence toilet and garbage outcomes by direct pressure on those responsible for provision rather than spending. We, therefore, consider a second set of regressions where the outcomes come from the midline and endline audits. Here the unit of observation is at the slum s level within a ward w

$$Service_{sw} = \beta_1(Treat_{sw} \times Post_t) + \beta_3(Post_t) + X_w + \epsilon_{sw}, \tag{3}$$

$Service_{sw}$ is one of multiple measures of toilet and garbage provision and utilization measured in the midline and endline surveys. $Post_t$ is an indicator for being in themidline or endline survey. The regressions include ward fixed effects.

Table IV considers toilet related outcomes. We observe a significant increase in the incidence of closed toilets in treatment wards which is accompanied by a significant decline in female users. The decline in male use is also large but noisily estimated. One possible explanation is that the councillor asked contractors to improve the worst toilets and they chose to simply close them. A less favorable view is that the councillor felt that closing dirty toilets would improve appearance of toilet quality and he/she was less concerned about actual usage.

Table V considers garbage related outcomes and we observe no impact of receiving audit report-cards on garbage outcomes.

Taken together these results support the view that the reports card provide relatively strong incentives for politicians, but simply improving information on policy issues for politicians *per se* seems to not to matter. This conclusion is consistent with T1 and T2 having similar impacts and also the fact that audit reports had no positive effects.

7 Does information influence party and voter behavior?

Next, we turn to the political implications of the newspaper treatment. Were councillors right in responding to the electoral incentives provided by the report cards?

We first examine party decision on whether to field the incumbent. Here, we also account for whether the councillor is still able to contest the election from his/her ward. We create a dummy 'ineligible' which equals one if the councillor is ineligible to stand for re-election from own ward. This could occur, for instance, if a male councillor's ward is reserved for women. Our main specifications

reports pooled estimates across the two treatment arms. We first examine the average effect of reportcards and then whether these impacts differ by councillor eligibility. Finally, we provide a more descriptive examination of whether councillor performance on spending and attendance influences party behavior.

Does their performance make a difference? Column 1 of Table VI shows that on average incumbents in treatment wards were 12 percentage points more likely to contest re-election. The average for control wards shows that 48% of incumbents reran for election. In column (2) the point estimates on re-contesting elections, conditional on being in the treatment group, are quite similar across eligible and ineligible councillors. However, column (3) suggests heterogeneity across eligible and ineligible councillors when it comes to party responsiveness to their performance – the only significant effect shows that the ineligible incumbents who perform well in terms of pro-poor spending and have a report card have a significantly increased chance of rerunning. The corresponding effect on attendance is also positive but insignificant. A likely reason is this asymmetry is that the presumption is that an eligible incumbent will retain his/her seat and that the party will pay the higher cost of allocating a different ward to an ineligible incumben only if he/she is very good.

To explore this further we turn to the outcome of whether the councillor runs from a different ward, and we see that the treatment-induced increase in contesting elections is coming from treatment councillors who contest election from other wards (column 4). Column (5) shows that the effects are concentrated among councillors who engaged in more pro-poor spending in high-slum wards. Columns (7) and (8) show that these effects are concentrated in high poverty wards. Finally, in columns (9) and (10) we see that these ineligible councillors who run from a different ward tend to run from a ward that had an incumbent from the same party.

Table VII considers electoral outcomes. In column (1) we observe close to a 5 percentage point rise in the voteshare of ward councillors' who received a report card. In column (2) we see that this impact seems evenly split across eligible and ineligible councillors. Column (3) suggests that only the gains for ineligible councillors are concentrated among those who engaged inmore pro-poor spending. Eligible councillors who engage in greater pro-poor spending dont appear to benefit electorally. In columns (4)-(6) we see that the effect of spending more on eligible candidates voteshare is actually negative, though insignificant. The probability of winning, reported has a very similar pattern. Appendix Table A.VI shows that the treatment didn't affect voter registration or turnout.

8 Conclusion

TBC

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9 Data Appendix

9.1 Slum Identification

We identify slums following a methodology based on the UN-HABITAT and Indian census definition of slums.[12] 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,

[12]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.

uncovered and unimproved drains, low coverage of private toilet facilities, high incidence of trash piles and frequent cohabitation with animals.[13]

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 high slums and others as low slums.

Between 9 and 126 households were surveyed in each ward[14], 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 neighbourhoods (fewer than five slum characteristics).[15]

[13]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.

[14]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.

[15] 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.

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

	Log total spending (2010–12)		Mean z-score of three log preference-weighted spending amounts (2010–12)		Log of spending on issues, each weighted by fraction of RWAs in city reporting that it is one of the top three issues (2010–12)		Mean z-score of two attendance measures (2010–12)	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Report card × High slum		–0.049		0.617**		1.215**		0.372*
		(0.074)		(0.288)		(0.483)		(0.196)
Report card	0.005	0.050	–0.010	–0.309	–0.018	–0.606*	0.030	–0.227
	(0.040)	(0.057)	(0.133)	(0.201)	(0.224)	(0.337)	(0.100)	(0.138)
High slum		–0.002		–0.230		–0.518		–0.238
		(0.065)		(0.250)		(0.420)		(0.166)
Pre-treat outcome control	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Strata (zone–party) FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Control mean	4.552	4.555	–1.854	–1.879	1.209	1.165	–0.394	–0.373
Control s.d.	0.243	0.248	1.053	1.059	1.807	1.815	1.162	1.171
F-test of effect in high-slum		0.000		0.127		0.061		0.565
p-value of of F-test		0.994		0.722		0.805		0.453
Observations	240	227	240	227	240	227	240	227

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

Ward-level OLS regression. Slum resident preference measures are the city-level mean of hh survey responses. "Report card" 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 any newspaper report card treatment on MCD councilor spending according to slum preferences

	Log total spending (2010–12)		Mean z-score of three log preference-weighted spending amounts (2010–12)		Log of spending on issues, each weighted by fraction of RWAs in city reporting that it is one of the top three issues (2010–12)		Mean z-score of two attendance measures (2010–12)	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
T1: 2012 report (ITT) × High slum		–0.085		0.493		1.037*		0.091
		(0.102)		(0.369)		(0.623)		(0.234)
T2: 2010/12 reports (ITT) × High slum		–0.023		0.684**		1.318***		0.521**
		(0.082)		(0.300)		(0.507)		(0.221)
T1: 2012 report (ITT)	0.072	0.131*	0.018	–0.227	0.058	–0.450	0.037	–0.094
	(0.054)	(0.077)	(0.162)	(0.246)	(0.277)	(0.418)	(0.115)	(0.148)
T2: 2010/12 reports (ITT)	–0.031	0.004	–0.024	–0.356*	–0.058	–0.696**	0.026	–0.304*
	(0.042)	(0.062)	(0.143)	(0.208)	(0.243)	(0.352)	(0.112)	(0.168)
High slum		–0.004		–0.234		–0.525		–0.246
		(0.066)		(0.251)		(0.422)		(0.168)
Pre-treat outcome control	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Strata (zone–party) FE	No	No	Yes	Yes	Yes	Yes	Yes	Yes
Control mean	4.552	4.555	–1.854	–1.879	1.209	1.165	–0.394	–0.373
Control s.d.	0.243	0.248	1.053	1.059	1.807	1.815	1.162	1.171
Observations	240	227	240	227	240	227	240	227

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

Ward-level OLS regression. Slum resident preference measures are the city-level mean of hh survey responses. Spending is categorized by lexical heuristic.

Table III: Effect of MCD audit 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
MCD Audit × High slum	0.240	–0.660	0.061	–0.395	1.058**	–2.472
	(0.269)	(1.068)	(0.052)	(0.381)	(0.509)	(2.002)
MCD Audit	–0.029	0.728	–0.015	0.409	–0.605	2.506
	(0.177)	(0.987)	(0.031)	(0.339)	(0.413)	(1.897)
High slum	–0.199	0.498	–0.010	0.542	–0.701*	1.332
	(0.177)	(0.810)	(0.014)	(0.329)	(0.408)	(1.674)
Pre-treat (2007–11) spending control	Yes	Yes	Yes	Yes	Yes	Yes
Newspaper report card control	Yes	Yes	Yes	Yes	Yes	Yes
Strata (zone–party) FE	Yes	Yes	Yes	Yes	Yes	Yes
Control mean	3.764	1.318	–0.373	–0.395	2.909	–1.390
Control s.d.	0.744	1.571	0.000	0.698	1.247	2.793
F-test of effect in high-slum	0.011	0.511	1.748	3.125	0.476	0.693
p-value of of F-test	0.917	0.477	0.190	0.081	0.492	0.408
Observations	102	102	102	102	102	102

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

Ward-level OLS regression. "MCD Audit" indicates observations in a ward of which the MCD councilor received the results of an audit (ITT). Amount of spending is in lakh rupees. Spending is categorized by lexical heuristic.

Table IV: Effect of audit report cards on public toilet provision

	(1) Total toilets	(2) Open toilets	(3) Closed toilets	(4) Toilet price	(5) Adult users (#)	(6) Facilities
Panel A: Male toilets						
Mid/Endline × Any audit	0.083	–0.032	0.114**	–0.066	–1.122	–0.099
	(0.082)	(0.068)	(0.057)	(0.170)	(0.892)	(0.236)
Ward FE	Yes	Yes	Yes	Yes	Yes	Yes
Baseline control mean	1.378	0.902	0.476	1.031	5.280	2.395
Observations	932	932	932	422	932	424
Panel B: Female toilets						
Mid/Endline × Any audit	0.037	–0.077	0.114*	–0.089	–1.093*	–0.089
	(0.092)	(0.082)	(0.058)	(0.122)	(0.657)	(0.144)
Ward FE	Yes	Yes	Yes	Yes	Yes	Yes
Baseline control mean	1.244	0.768	0.476	0.703	3.183	2.447
Observations	932	932	932	419	932	421

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

Slum-level OLS regression. Main effects and constant are omitted from DID model. "Any audit" indicates observations in a slum of which the MLA or MCD councilor received the results of an audit (ITT). "Mid/Endline" indicates observations that took place in the second or third round of audits.

Table V: Effect of audit report cards on garbage facilities

	(1) Total informal piles	(2) Informal piles not recently collected (%)	(3) Informal piles overflowing (%)	(4) Total dhalaos	(5) Dhalaos with proper structure (%)	(6) Dhalaos overflowing (%)	(7) Dhalaos regularly collected (%)
Mid/Endline × Any audit	2.802	–0.004	–0.046	–0.041	–0.158	0.001	0.048
	(2.723)	(0.048)	(0.042)	(0.074)	(0.152)	(0.125)	(0.130)
Ward FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Baseline control mean	22.355	0.742	0.273	0.390	0.338	0.750	0.375
Observations	867	867	867	932	328	328	328

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

Slum-level OLS regression. Main effects and constant are omitted from DID model. "Any audit" indicates observations in a slum of which the MLA or MCD councilor received the results of an audit (ITT). "Mid/Endline" 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. "Informal piles overflowing" is the fraction of informal garbage piles in the slum with a garbage spread greater than the footprint of a minibus. "Total dhalaos" is the number of dhalaos (formal garbage collection points) in the slum. "Dhalaos with proper structure" is the fraction of dhalaos in the slum with walls and a roof. "Dhalaos overflowing" is the fraction of dhalaos in the slum with a garbage spread greater than the footprint of a minibus, relative to the number of dhalaos at the baseline. "Dhalaos regularly collected" is the fraction of dhalaos in the slum regularly collected, relative to the number of dhalaos at the baseline.

Table VI: Effect of newspaper report card publication and attendance 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) ...from high-slum ward	(8) ...from low-slum ward	(9)	(10)	(11)
Treatment											
Report card	0.120*	0.071	0.206	0.039*	–0.012	0.037	0.345	–0.045	0.044**	0.007	0.020
	(0.067)	(0.086)	(0.302)	(0.022)	(0.019)	(0.080)	(0.216)	(0.122)	(0.018)	(0.014)	(0.060)
Report card × Ineligible		0.091	–0.424		0.156***	0.172	–0.415	0.261		0.116**	0.112
		(0.114)	(0.420)		(0.056)	(0.245)	(0.303)	(0.394)		(0.050)	(0.218)
Report card × Attendance			–0.198			–0.078	–0.424	0.051			–0.026
			(0.433)			(0.117)	(0.283)	(0.200)			(0.080)
Report card × Spending			–0.067			–0.048	–0.149	–0.012			–0.005
			(0.096)			(0.034)	(0.095)	(0.031)			(0.014)
Report card × Ineligible × Attendance			0.792			–0.017	0.771	–0.225			0.012
			(0.603)			(0.355)	(0.467)	(0.554)			(0.322)
Report card × Ineligible × Spending			0.229*			0.125**	0.291**	0.084			0.074
			(0.118)			(0.058)	(0.136)	(0.068)			(0.051)
Controls											
Ineligible		–0.473***	–0.512*		–0.028	–0.061	0.221	–0.190		–0.009	–0.064
		(0.079)	(0.290)		(0.022)	(0.100)	(0.176)	(0.181)		(0.016)	(0.083)
Attendance			–0.269			0.050	0.302	–0.140			0.028
			(0.370)			(0.117)	(0.259)	(0.231)			(0.072)
Spending			0.077			0.032	0.136	0.002			–0.008
			(0.089)			(0.034)	(0.090)	(0.032)			(0.015)
Ineligible × Attendance			0.037			0.056	–0.267	0.292			0.087
			(0.429)			(0.148)	(0.240)	(0.277)			(0.117)
Ineligible × Spending			–0.131			–0.046	–0.174*	–0.012			–0.007
			(0.100)			(0.037)	(0.094)	(0.026)			(0.024)
Strata (zone–party) FE	Yes	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	0.000
Eligible control mean	0.478	0.478	0.478	0.022	0.022	0.022	0.038	0.000	0.000	0.000	0.000
Observations	240	240	240	240	240	240	116	112	240	240	240

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

Councilor-level cross section estimated with OLS. "Attendance" is overall councilor attendance at MCD committees of which they are a member, 2007–10. "Spending" is the mean z-score of three log preference-weighted spending amounts (2007–11), analogous to the dependent variable in Table A.V, column 2, for the pre-publication period. Command: areg councilor_cannibal i.councilor_anytreat##ib1.councilor_eligible##(c.coun_overall_committee_att0 c.coun_lamt_cmean_z0) if 1 == 1 & !councilor_slum_prop_high & !mi(councilor_slum_prop_high), absorb(overall_strat) vce(r)

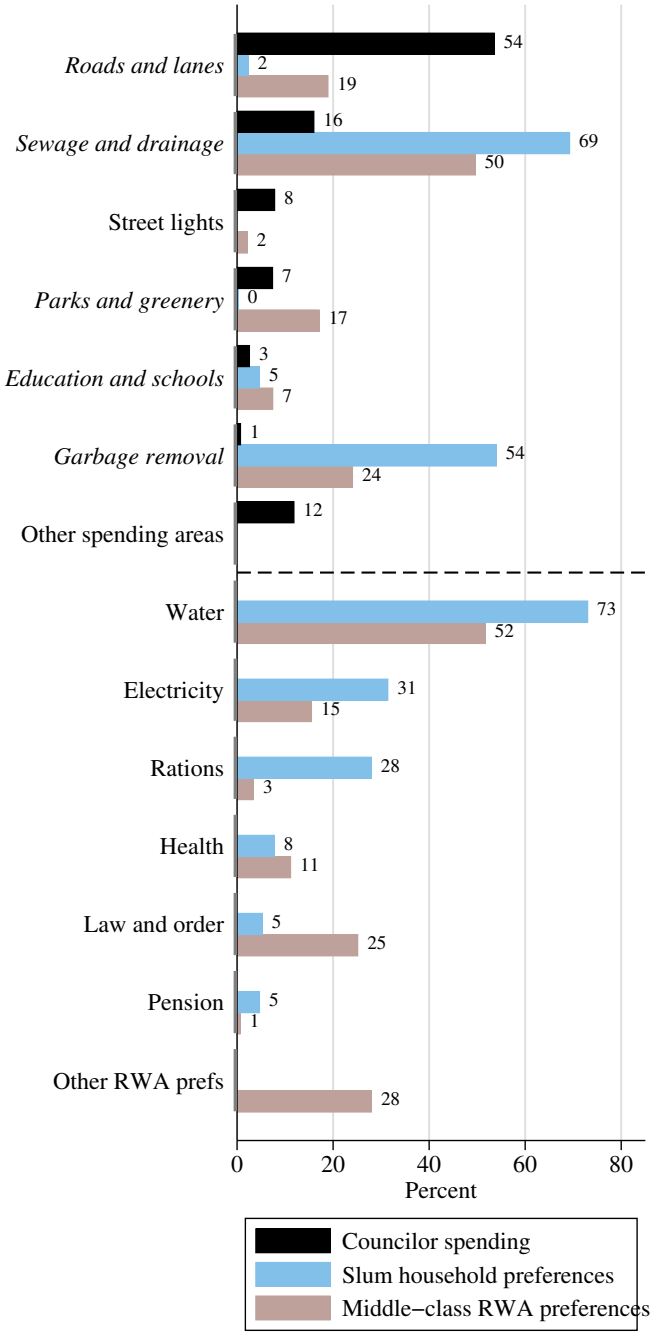
Table VII: Effect of newspaper report card publication and attendance on councilor-level electoral outcomes

	Councilor's vote share (0 if didn't run)			Councilor wins in any ward		
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment						
Report card	0.047*	0.032	0.048	0.029	–0.014	–0.033
	(0.027)	(0.035)	(0.121)	(0.057)	(0.080)	(0.336)
Report card × Ineligible		0.024	–0.176		0.093	–0.266
		(0.047)	(0.170)		(0.098)	(0.414)
Report card × Attendance			–0.022			0.021
			(0.165)			(0.479)
Report card × Spending			–0.046			–0.131
			(0.038)			(0.104)
Report card × Ineligible × Attendance			0.314			0.583
			(0.233)			(0.595)
Report card × Ineligible × Spending			0.096**			0.230**
			(0.047)			(0.117)
Controls						
Ineligible		–0.175***	–0.205*		–0.308***	–0.208
		(0.033)	(0.104)		(0.076)	(0.312)
Attendance			–0.227*			–0.188
			(0.131)			(0.429)
Spending			0.040			0.103
			(0.035)			(0.098)
Ineligible × Attendance			0.026			–0.182
			(0.152)			(0.457)
Ineligible × Spending			–0.063			–0.150
			(0.040)			(0.104)
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	0.171	0.171	0.171	0.304	0.304	0.304
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. "Attendance" is overall councilor attendance at MCD committees of which they are a member, 2007–10. "Spending" is the mean z-score of three log preference-weighted spending amounts (2007–11), analogous to the dependent variable in Table A.V, column 2, for the pre-publication period. Command: areg councilor_win i.councilor_anytreat##ib1.councilor_eligible##(c.coun_overall_comm_att0 c.coun_lamt_cmean_z0) if 1 == 1, absorb(overall_strat) vce(r)

Figure 1: Ward-level descriptive statistics of councilor spending and constituent preferences



"Councilor spending" is the ward-mean of MCD councilor spending (mean annual fraction 2007–2012). (The total is 1 by construction.) "Slum household 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. "Middle-class RWA preference" is the ward-mean of residential welfare associations who specify each area in response to the question, "Which areas are problematic for this community? (Mark the three most important areas.)" (The total is less than 3 because not all RWAs named three areas.) RWA responses are weighted within wards by number of families the RWA represents. *Emphasis* indicates areas that are included in preference-weighted spending measures. Areas below the dotted line are not areas of councilor spending.

Figure 2: Example MCD councillor report cards in the Hindustan in 2012

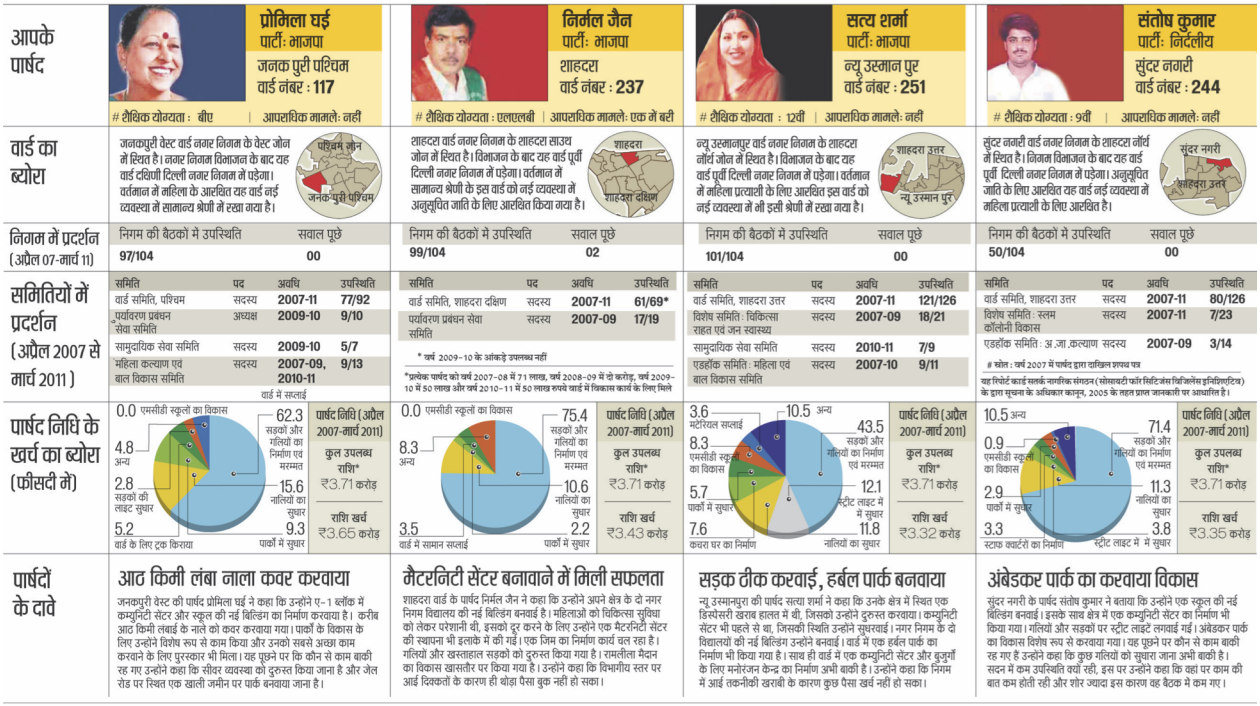


Figure 3: Project Timeline Events

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	
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

10 Appendix: Tables and Figures

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

	(1) Roads and lanes	(2) Sewage and drainage	(3) Parks and greenery	(4) Education and schools	(5) Garbage removal	(6) Other areas	(7) Total
Panel: Spending (all wards)							
Report card	–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)							
Report card	–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 electoral outcomes

	2007 Election					2012 Election
	(1) Log registered voters	(2) Log turnout	(3) Seat reserved for minority	(4) Number of candidates	(5) Winner's vote share	(6) Eligible for reelection
Report card	–0.055	–0.031	0.011	–0.101	0.025	0.040
	(0.034)	(0.042)	(0.096)	(0.873)	(0.023)	(0.067)
Eligible (2012)	–0.002	0.017	0.438***	–0.405	–0.013	
	(0.038)	(0.044)	(0.106)	(0.948)	(0.022)	
Report card × Eligible (2012)	0.051	0.009	–0.072	0.214	0.032	
	(0.047)	(0.057)	(0.128)	(1.162)	(0.041)	
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. * $p < .10$, ** $p < .05$, *** $p < .01$.

Command: regress frac_winnervotes2007 i.t1_or_t2_intend##i.eligible, vce(ro)

Table A.III: Effect of any newspaper report card treatment on MCD councilor spending according to slum preferences: z-scores decomposed

	Mean z-score of three log preference-weighted spending amounts (2010–12)	Log of spending on issues, each weighted by fraction of slum HHs in city reporting that...			Mean z-score of two attendance measures (2010–12)	Assembly attendance (2010–2012)	Committee attendance (2010–2012)
	(1)	(2) ...it is the most problematic in the area	(3) ...it is a problem for them	(4) ...it is a problem for the community	(5)	(6)	(7)
Report card × 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)
Report card	–0.309	–0.686	–0.650	–0.657	–0.227	–0.045**	–0.019
	(0.201)	(0.438)	(0.426)	(0.433)	(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
Strata (zone–party) FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Control mean	–1.879	–0.183	2.076	1.997	–0.373	0.776	0.574
Control s.d.	1.059	2.323	2.231	2.268	1.171	0.165	0.225
F-test of effect in high-slum	0.127	0.193	0.092	0.106	0.565	0.877	0.010
p-value of of F-test	0.722	0.661	0.762	0.745	0.453	0.350	0.919
Observations	227	227	227	227	227	224	227

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

Ward-level OLS regression. Slum resident preference measures are the city-level mean of hh survey responses. "Report card" 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.IV: 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
Report card × 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)
Report card	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
F-test of effect in high-slum	0.000	0.543	0.394	0.036	0.483	2.084
p-value of of F-test	0.994	0.462	0.531	0.850	0.488	0.150
Observations	227	227	227	227	227	227

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

Ward-level OLS regression. "Report card" 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. Example command: reghdfe lamt_parks1 i.t1_or_t2_intend##i.hh_sample_w lamt_total0 , a(overall_strat_numeric) vce(ro)

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

	Log total spending (2010–12)		Mean z-score of three log preference-weighted spending amounts (2010–12)		Log of spending on issues, each weighted by fraction of RWAs in city reporting that it is one of the top three issues (2010–12)		Mean z-score of two attendance measures (2010–12)	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Report card × Survey ward		–0.006		0.529*		1.045**		0.025
		(0.080)		(0.281)		(0.477)		(0.211)
Report card	0.005	0.006	–0.010	–0.234	–0.018	–0.475	0.030	0.025
	(0.040)	(0.063)	(0.133)	(0.194)	(0.224)	(0.318)	(0.100)	(0.144)
Survey ward		–0.008		–0.084		–0.261		0.039
		(0.068)		(0.240)		(0.401)		(0.170)
Pre-treat outcome control	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Strata (zone–party) FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Control mean	4.552	4.552	–1.854	–1.854	1.209	1.209	–0.394	–0.394
Control s.d.	0.243	0.243	1.053	1.053	1.807	1.807	1.162	1.162
F-test of effect in high-slum		0.014		1.033		0.789		0.345
p-value of of F-test		0.904		0.311		0.375		0.557
Observations	240	240	240	240	240	240	240	240

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

Ward-level OLS regression. Slum resident preference measures are the city-level mean of hh survey responses. "Report card" 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.VI: Effect of newspaper report card publication and attendance on ward-level electoral outcomes

	Voter turnout			Voter registration		
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment						
Report card	–0.008	–0.008	–0.074**	–744.579	–702.669	3343.741
	(0.007)	(0.009)	(0.035)	(1420.318)	(1941.576)	(11956.598)
Report card × Ineligible		–0.001	0.089		–460.830	–4065.476
		(0.015)	(0.063)		(2519.533)	(14371.841)
Report card × Attendance			0.097*			–5912.027
			(0.054)			(16480.880)
Report card × Spending			–0.009			1765.208
			(0.013)			(2774.616)
Report card × Ineligible × Attendance			–0.135			5555.580
			(0.093)			(20582.760)
Report card × Ineligible × Spending			–0.003			–1464.691
			(0.017)			(3481.315)
Controls						
Ineligible		–0.013	–0.057		–1878.878	–2771.498
		(0.012)	(0.050)		(2024.447)	(11242.534)
Attendance			–0.063			–4158.561
			(0.044)			(14841.126)
Spending			0.007			–2104.358
			(0.013)			(2618.485)
Ineligible × Attendance			0.066			805.661
			(0.075)			(15998.239)
Ineligible × Spending			–0.004			1812.847
			(0.015)			(2748.826)
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" is overall councilor attendance at MCD committees of which they are a member, 2007–10. "Spending" is the mean z-score of three log preference-weighted spending amounts (2007–11), analogous to the dependent variable in Table A.V, column 2, for the pre-publication period. Command: areg registeredvoters i.anytreat##ib1.eligible##(c.overall_committee_att0 c.lamt_cmean_z0) if 1 == 1, absorb(overall_strat) vce(r)