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Author(s): Leonid Peisakhin
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Transparency and Corruption: Evidence from India

Leonid Peisakhin *Juan March Institute*

Abstract

Theories of corruption suggest that higher levels of transparency are necessarily associated with lower levels of corruption. Yet in highly hierarchical societies in which the gulf between government officials and the most underprivileged members of society is very wide, this relationship may not hold. In this paper, I test the link between transparency and corruption by means of a field experiment. I ask how effective recourse to a freedom-of-information law is in comparison to bribery for both slum dwellers and middle-class individuals in India as they apply for basic public services. I demonstrate that applicants who make use of the freedom-of-information law attain almost the same rate of success as those who bribe. Recourse to a freedom-of-information law comes close to erasing class differences; that is, it results in comparable processing times for slum dwellers and middle-class individuals.

1. Introduction

Corruption is widely perceived as one of the most serious impediments to development—it retards growth, creates a system of perverse incentives for government officials and the public, and distorts the effects of redistribution programs (Shleifer and Vishny 1993; Rose-Ackerman 2004). In this paper, I specifically discuss administrative corruption, that is, abuse of public office for private gain by government officials. While there is agreement in the literature that corruption is bad for growth, there is little consensus on how to combat it

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effectively. Among the many proposals for graft reduction are initiatives that increase state capacity, raise the salaries of government officials, and impose harsher penalties on those who accept bribes. A foundational theoretical claim is that greater availability of information concerning the nature and incidence of corrupt transactions acts as a deterrent against bribe-seeking behavior because it increases the likelihood that corrupt officials are exposed and punished (Becker 1968, pp. 177–78). Further, Becker and Stigler (1974, p. 14) suggest that the most enforced crimes are the ones for which potential victims are themselves the enforcers, as long as they possess sufficient information to press charges.

In this paper, I test the theoretical claim that greater transparency in the provision of government services results in both improved quality of service to customers who do not resort to bribery and a reduction in the incidence of graft overall. It might first appear that the empirical connection between higher levels of transparency and lower incidence of corruption is so obvious that it does not need to be demonstrated. However, in highly stratified and corrupt societies, the least well-off, that is, those who are most in need of government assistance, are often completely powerless against government officials. It seems reasonable to assume that in such societies, only the middle class benefits from increased transparency, while the poor must continue to pay bribes to obtain basic services from the state because they are not a credible threat to corrupt civil servants. By way of exploring the notion of differential effects of legislation that increases transparency, I test the effect of the introduction of a well-drafted freedom-of-information act (FOIA) on the quality of service and incidence of graft in the process of voter registration among the middle class and urban poor of New Delhi.

I present data from two field experiments, one involving New Delhi's middle-class residents and the other involving the city's slum dwellers, in which confederates were randomized into three experimental intervention groups: an FOIA group, a bribe group, and an untreated control group. I find that recourse to an FOIA—in this instance, India's Right to Information Act of 2005 (RTIA)—is an effective free and legal substitute for bribery for both demographics. Middle-class applicants in the FOIA group were registered to vote within 150 days (in comparison to 123 days for those who bribed), and processing times for Delhi's urban poor were 164 days in the FOIA group and 140 days in the bribe group. If we discount the time that it takes for RTIA applications to be processed, the difference between recourse to FOIA and bribe interventions is negligible. It is important to note that processing times for slum dwellers and middle-class applicants are nearly equal only in the FOIA group. This finding suggests that empowerment of the underprivileged by means of information provision can break down status barriers between the wealthy and the poor when it comes to provision of public services. Most applicants in the control group in both experiments were not registered to vote within the experimental window of 11 months.

This study is part of a growing empirical literature on the relationship between

transparency and corruption (for an overview, see Rose-Ackerman 2004, pp. 316–22). Much of this literature is coalescing around the consensus that "voice related variables have a larger effect on corruption and the quality of services than institutions" (Kaufmann, Mehrez, and Gurgur 2002, p. 3). What sets this study apart from most other work on corruption is that it is based on experimental evidence and is therefore not subject to the biases inherent in survey findings on perceptions of graft (for a review of field experiments on corruption, see Peisakhin 2011). In this paper, I build on other experimental work on the relationship between transparency and corruption, notably Reinikka and Svensson's (2005) study demonstrating that graft among Uganda's school administrators decreased by 60 percent when the government started publishing data on education spending in local newspapers.

This paper is an extension of Peisakhin and Pinto (2010). In that study, we followed 86 slum dwellers as they applied to obtain ration cards from one of New Delhi's Food and Civil Supplies offices; our confederates were randomly assigned to an FOIA, bribe, nongovernmental organization (NGO) support, or comparison group. We found that almost all the applicants who sought recourse through an FOIA were able to obtain ration cards within a median time of 120 days (compared with 82 days for those who submitted a bribe with their application), whereas only eight of 39 confederates in the comparison and NGO support groups were issued ration cards within the experimental window of 11 months. In this paper, I replicate the ration card experiment in the context of voter registration and extend it via a nonexperimental comparison of the experiences of slum dwellers and middle-class residents as the two groups apply for the same basic public service. To the best of my knowledge, the experiment that I present here is the only study to compare the effectiveness of the same anticorruption measure for individuals from middle-class and underprivileged backgrounds.

I proceed by outlining the context of this study and describing the genesis of India's freedom-of-information law in Section 2. I also lay out the randomization procedure and provide a description of the experimental sites. In Section 3, I describe the experimental design. The experimental results are presented in Section 4 and discussed in Section 5. The paper concludes with a call for more empirical work on the anticorruption potential of policy measures that aim to increase transparency.

2. Historical and Political Context

2.1. India's Freedom-of-Information Law

The RTIA is India's freedom-of-information law. In 1975, India's Supreme Court urged the government to disclose more information, arguing that India's citizens could not fulfill their constitutional right to freedom of speech if they were not fully informed about public policy (*Uttar Pradesh v. Raj Narain,* A.I.R.

1975 S.C. 865). In a landmark ruling in 1982 (*Gupta v. Union of India,* A.I.R. 1982 S.C. 149), the Supreme Court maintained that "no democratic government can survive without accountability, and the basic postulate of accountability is that people should have information about the functioning of the government" (para. 63). Civil society activists were inspired by these rulings, and in the early 1990s they began to agitate for freedom-of-information laws at the state level. Mazdoor Kisan Shakti Sangathan (MKSS), operating among villagers in Rajasthan, was the first NGO to pressure government officials to disclose more information about administrative expenditures at the local level (Jenkins and Goetz 1999).[1] Tamil Nadu was the first state to adopt a freedom of information law in 1997, and under pressure from groups like the MKSS and Parivartan, landmark freedom-of-information legislation was put in place in Rajasthan in 2000 and in New Delhi in 2001. The first version of a national statute was adopted in 2002 as the FOIA. Legislators from all political parties and senior civil servants opposed introduction of this law and succeeded in hollowing it out through numerous revisions; the document that was drafted left it largely up to the civil service to furnish information to the public and did not contain any punitive provisions. In the end, the 2002 FOIA was never enacted. A more successful attempt at creating a national freedom-of-information provision came in 2005. Following personal intervention by Sonia Gandhi, the leader of the Congress Party, the RTIA was passed by Parliament largely in the original form drafted by civil society activists. The RTIA went into effect on October 12, 2005.

The RTIA's explicit function is to ensure that the citizenry is fully informed about the government's activities. Yet in the run-up to the act's adoption, it became apparent that this statute would be used by the public to ensure better provision of government services by creating an institutional mechanism whereby government officials are required to respond to public complaints. Written requests for information (and, more recently, phoned or even online requests) are lodged with a public information officer (PIO) of the relevant department upon payment of a minimal fee (Rs. 10, about $.25). The PIO then has up to 30 days to respond to the information request. The act also provides for complaint and appeal procedures. If the PIO does not respond within 30 days, the applicant may file a complaint with the state information commission. Further, if the applicant is not satisfied with the information furnished by the PIO, he or she may appeal for fuller disclosure to the first appellate authority (usually a senior government official in the same department as the PIO) and from there to the state information commission. State information commissions are legally empowered to fine PIOs and first appellate authorities for failure to fulfill their duties; the magnitude of the fine is at the discretion of information commissioners, who are generally retired senior civil servants. Although penalties are

[1] From interviews with Arvind Kejriwal, president of the nongovernmental organization (NGO) Parivartan and one of India's leading Right to Information Act (RTIA) activists (June 27, 2007), and with Aloke Tikku, RTIA reporter for the *Hindustan Times* (July 2, 2007).

very rarely imposed by information commissions, failure to disclose information under the RTIA can hamper a civil servant's professional development. According to senior officials I interviewed, all administrative mishaps are noted in civil servants' personal files, and even the smallest incident can be cause for an official not to be promoted.[2]

2.2. Experimental Sites and Confederates

The urban poor confederates came from northeastern New Delhi, an area with a high density of shantytowns, and all resided in the same slum. I was not able to select a slum randomly because I was concerned about interference from local politicians and NGO workers. Therefore, I sought to locate a slum with no single dominant community network, and the research team recruited confederates from the first slum that we found in which community workers and party representatives did not show an immediate interest in our work. The slum that was selected is representative of other New Delhi slums in all respects other than the fact that it is predominantly Muslim. Like 91 percent of the city's slums, it is located on public land. Sixty percent of the slum's residents have access to tap water and electricity, compared with 71 percent of all of New Delhi's slum dwellers who have access to tap water and 61 percent who have access to electricity. Like 76 percent of New Delhi's slums, it has open sewers, and it gets waterlogged during heavy rain, as do 72 percent of the city's slums. There is a primary school nearby, and the tenements, constructed of a mixture of brick, mud, and wood, are standard for this type of locality.[3] Seventy percent of the confederates were literate, compared with an overall literacy rate of 67 percent among New Delhi's slum dwellers (Government of India 2007).[4] On average, they had 4.7 years of schooling; this is similar to the average of 4.5 years of education among New Delhi's urban poor (Jha, Rao, and Woolcock 2005, p. 27).

We sought the help of a local community worker to assist us with the enlistment of 61 confederates. Participation was open to all slum residents who were legally eligible to vote but who had not yet been registered on voter lists. The bribe group was capped at 16 confederates for budgetary reasons, whereas the RTIA and control groups had 22 and 23 confederates, respectively, for a total of 61 participants.

Confederates for the middle-class experiment were drawn from a city district close to a major university and known for its quality housing. Here too I was not able to randomly select a middle-class area because I needed to tap into a community of young people, who are the most likely to want to register to vote.

[2] From interviews with Wajahat Habibullah, India's chief information commissioner (June 28, 2007), and with two information commissioners in the state of Kerala (June 23, 2007).

[3] The statistical description of New Delhi's slums is from a report on conditions in urban slums (National Sample Survey Organisation 2003). The National Sample Survey Organisation is a government program administered by the Ministry of Statistics and Programme Implementation.

[4] Both figures include those who cannot write.

Table 1
Urban Poor Experiment

	Confederate Group			Test of Covariate Balance		
	RTIA	Bribe	Control	RTIA versus Bribe	RTIA versus Control	Bribe versus Control
Confederates	22	16	23	38	45	39
% Male	.82	.56	.61	−1.69^{+}	1.53^{+}	−.29
	(.39)	(.51)	(.50)			
Age	24	27	29	.82	−1.28	−.37
	(7.63)	(11.42)	(14.09)			
Literacy level	.95	1.25	.91	1.13	.19	1.22
	(.79)	(.86)	(.85)			
Years of schooling	4.00	6.06	4.52	1.63^{+}	−.18	1.05
	(3.69)	(4.06)	(4.77)			
Occupation	1.91	1.81	2.04	−.26	−.53	−.60
	(.92)	(1.17)	(.98)			
Annual income (Rs.)	26,636	22,625	20,913	−.66	1.61^{+}	.63
	(8,910)	(12,909)	(12,402)			
Years lived in city	14.41	15.56	16.04	.66	−.36	.36
	(4.92)	(4.19)	(8.12)			
% Muslim	1	1	1	N.A.	N.A.	N.A.

Note. Means are reported for the confederate group, and *z*-scores from Mann-Whitney nonparametric difference-of-means tests are reported for covariate balance. Standard deviations are in parentheses. No *z*-scores are reported for the percentage of Muslim confederates because there is no variation across the experimental groups. For literacy levels, 0 = none, 1 = read only, and 2 = full. For occupation, 1 = unskilled, 2 = semiskilled, and 3 = skilled. RTIA = Right to Information Act; N.A. = not applicable.
$^{+}$ $p < .10$.

As such, I deemed a university neighborhood to be a suitable place for a recruitment drive. In this experiment, the bribe group was capped at 18 confederates for financial reasons, and there were 21 people each in the RTIA and control groups, for a total of 60 participants.

Table 1 contains a basic description of confederates in the urban poor experiment, and Table 2 provides descriptive statistics for participants in the middle-class experiment. A representative urban poor confederate is a Muslim semiliterate male in his mid- to late 20s who has lived in New Delhi for most of his life, is currently employed in a semiskilled job (daily laborer, security guard, and the like), and gets by on a little over $1.50 a day. By contrast, the average salary among the middle-class confederates who were employed (in fact, only 25 of the 60 people in the middle-class experiment worked; the rest were students or homemakers) is seven times that of a slum dweller ($3,700 per year). A representative middle-class confederate is a 24-year-old Muslim man with a bachelor's degree who has lived in New Delhi for at least 8 years. All of the middle-class confederates were well educated—14 of the 60 had postgraduate qualifications—and those who worked were employed in high-skilled jobs (information technology, accounting, and the like).[5] In Tables 1 and 2, I also report

[5] The mean occupation scored just below a skilled job (which is scored 3) because homemakers, who are coded as unskilled labor (a score of 1), pull down the average. Semiskilled labor is scored as 2.

Table 2
Middle-Class Experiment

	Confederate Group			Test of Covariate Balance		
	RTIA	Bribe	Control	RTIA versus Bribe	RTIA versus Control	Bribe versus Control
Confederates	21	18	21	39	42	39
% Male	.90	.89	.81	−.16	.87	.68
	(.30)	(.32)	(.40)			
Age	24	24	25	−.50	−.04	−.55
	(5.81)	(9.13)	(7.55)			
Literacy level	2	2	2	N.A.	N.A.	N.A.
Years of schooling	15.29	14.61	15.48	−.68	−.02	−.81
	(1.87)	(2.50)	(1.89)			
Occupation	2.86	2.78	2.62	−.63	.95	.36
	(.48)	(.55)	(.80)			
Annual income (Rs.)	71,333	56,222	57,048	−.42	.48	.05
	(89,921)	(76,238)	(77,472)			
Years lived in city	10.19	8.78	10.10	−1.26	−.13	−1.13
	(4.70)	(5.09)	(5.08)			
% Muslim	.76	.72	.90	−.28	−1.23	−1.46^{+}
	(.44)	(.46)	(.30)			

Note. Means are reported for the confederate group, and *z*-scores from Mann-Whitney nonparametric difference-of-means tests are reported for covariate balance. Standard deviations are in parentheses. No *z*-scores are reported for literacy level because there is no variation across the experimental groups. For literacy levels, 0 = none, 1 = read only, and 2 = full. For occupation, 1 = unskilled, 2 = semiskilled, and 3 = skilled. RTIA = Right to Information Act; N.A. = not applicable.
$^{+}$ $p < .10$.

the results of a covariate balance test for each of the experimental populations. While there are some weakly statistically significant differences in demographic characteristics, especially in the experiment involving the urban poor, these differences are likely due to the small number of confederates in each of the experimental groups, and balance tests suggest that the randomization procedure was successful overall. When presenting the findings, I include demographic controls in one of the analyses to demonstrate that minor differences on some of the covariates are not driving the experimental findings.

Although neither the slum nor the middle-class area was selected randomly, I do not think that this limitation challenges the validity of the findings. As I have shown, the slum that I picked is very similar to most other slums in New Delhi on all basic characteristics, and the confederates are comparable to a representative slum dweller in their socioeconomic characteristics. As for the middle-class area, it seems reasonable to assume that students at a major university would be representative of the middle class overall.

Where the experimental populations are distinct is in religious affiliation: all the urban poor were Muslim (compared with 20 percent of Muslims among New Delhi's slum residents), and 80 percent of the middle-class participants were Muslim too.[6] Middle-class confederates were predominantly Muslim be-

[6] Muslims make up 13 percent of India's population (Government of India 2006–7).

cause the local university has an almost exclusively Muslim student body. On the one hand, the prevalence of Muslims in both experiments is a positive factor—it allows for greater confidence in the validity of the nonexperimental comparison between the two studies. On the other hand, it limits my ability to project the findings of this experiment onto the population at large. In general, it would seem reasonable to expect the intervention effects to be even more pronounced among non-Muslims, as Muslims are often subject to discrimination at the hands of mostly Brahmin civil servants. I should also highlight that there are very few female participants in the middle-class study. This is of course regrettable, and unfortunately researchers commonly encounter problems recruiting middle-class Indian women to participate in experiments (for instance, all female participants dropped out of the pilot experiment implemented by Bertrand and her colleagues [2006, p. 7]). Evidence from interviews suggests that middle-class women are less likely than men to participate in research studies because in traditional families men are expected to take care of any official or semiofficial business.

2.3. The Right to Vote, Electoral Rolls, and the Application Procedure

This experimental study is designed around a member of the public applying to register to vote. The right to vote is guaranteed by articles 325 and 326 of India's constitution. India's electoral system is regulated by the Representation of the People Act of 1950 and the Registration of Electors Rules of 1960. To be specific, section 23 of the Representation of the People Act establishes the office of the election registration officer (ERO) at the level of the municipal and rural state assembly electoral districts. Election registration officers and their staff are responsible for updating electoral rolls, which are registers of eligible voters. Electoral rolls are updated in three different ways: intensive revision, summary revision, and continuous revision. Intensive revision is supposed to take place every 5 years. During the intensive revision process, government officials must visit every household in their electoral districts to record changes to the current register. Evidence from interviews with civil society leaders and slum residents indicates that intensive revision occurs as infrequently as every 7 years in many districts; even when it is done in a timely fashion, many households are omitted.[7] Summary revision takes place more frequently, ordinarily every 3 years, immediately preceding a major election. During summary revision, the EROs post the latest version of the electoral roll in public places and invite district residents to report recent changes to this list. Finally, administrative rules also provide for continuous revision of electoral rolls, when those wishing to register to vote or to alter the details of their registration may file the necessary paperwork at the

[7] From interviews with Leena Joshi, leader of a community development organization active in Mumbai's slums (May 30, 2007), and slum residents and community organizers in New Delhi (early August 2007).

ERO's office. Such applications may be filed at any time other than the election period proper (Election Commission of India 2006).

All participants in both field experiments applied to register to vote under the continuous-revision process in a lull between elections in the summer of 2007.[8] Conversations with government officials and civil society activists indicate that a substantial proportion of India's poor make use of continuous revision of the rolls when registering to vote. Among the middle classes, mostly only the mobile and the relatively young seek recourse through the continuous-revision process. The National Election Commission instructs the EROs to register voters within 10 days in the immediate run-up to elections or within a maximum of 60 days between elections (Election Commission of India 2006, item 78). To register to vote, an applicant is required to furnish proof of Indian citizenship, proof of age, and proof of residence in a specific electoral district. Once the applicant's name is entered on the rolls, he or she is entitled to an electoral photographic identity card, known colloquially as the "voter ID." The voter ID card is the most common form of identity proof in India (driver's licenses and passports are relatively rare), and this document is commonly required for job applications, receipt of government benefits, and numerous other administrative procedures. In short, voter registration carries important financial and socio-economic benefits. These benefits are of particular value to poorer Indians and to those just starting their careers.

Voter registration applications can be filed only at district election registration offices covering the applicant's residential area. The middle-class and urban poor participants applied at different election registration offices, as the slum and the middle-class area are located on opposite sides of the city. However, all of New Delhi's election registration offices are identical by design.[9] Overall, there are 70 election registration offices in the city, corresponding to the number of constituencies of the New Delhi legislative assembly. Because each office has a wide area of coverage, every election registration office serves both urban poor and middle-class residents. Five officials and two assistants are employed at each office: the ERO, two upper-division clerks, three lower-division clerks, and two assistants, or peons. The ERO and the clerks rotate between various administrative tasks, such as maintaining the electoral roll, verifying voters' residential status, and filing reports. They move between district offices, and often across government departments, every 3–5 years.

Typically, the ERO and the clerks are 35- to 45-year-old university-educated upper-caste Hindu men. In the spring of 2008, EROs were paid approximately Rs. 108,000 per year ($2,800), upper-division clerks earned Rs. 54,000 ($1,400), and lower-division clerks were paid Rs. 42,000 ($1,100). In addition to their

[8] In fact, because data collection took such an unexpectedly long time (a little longer than 11 months), the summary revision process in the run-up to a state assembly election scheduled for fall 2008 had commenced in the slum by the time the experiment was winding down in late June 2008.

[9] Interviews conducted by Aftab Alam and Aftab Alam with middlemen at six randomly selected election registration offices confirm this (early August 2007).

fixed salaries, these civil servants also received free benefits from the government and bonuses for overtime work. On average, officials at an election registration office see 70–80 applicants daily; new applications are received in the morning, and documentation and ongoing cases are processed in the afternoon.

3. The Field Experiment

3.1. The Information Intervention

The information intervention is the most important of the three experimental interventions. Confederates randomized into the information group were informed about the existence of the RTIA and were asked to file RTIA requests shortly after submitting paperwork to register to vote. Each RTIA request was addressed to the local PIO and contained two questions: what is the status of this individual's application, and how long is the average wait to be added to the electoral roll in this district? The intent behind the RTIA request was to spur local officials into action, given that the maximum legally mandated processing time for such applications is 60 days. In this instance, the PIOs were deputy district commissioners and therefore the immediate superiors of the civil servants who were processing our confederates' applications. To ensure consistency of implementation, my research assistants filled out and mailed all of the RTIA requests along with the requisite administrative fee of Rs. 10 ($.25). Half of the RTIA requests were written in English and the other half in Hindi, and the exact wording of the requests varied somewhat from letter to letter in order to avoid arousing suspicion among officials. All government offices in New Delhi are bilingual, and it is commonplace to see paperwork in both English and Hindi. The filing of electoral roll applications and the mailing of RTIA requests were staggered over a period of about 4 weeks so that the PIO would not receive a large volume of identical requests on the same day.

In the slum area, we mailed the RTIA requests on average 20 days after confederates applied to have their names added to the electoral roll. I decided to wait this long in order to give election commission officials time to start processing the electoral roll applications—this way they could not ignore incoming RTIA requests on the grounds that they had not seen the requesters' names. However, there was a glitch with the RTIA requests in the middle-class experiment. There, all the RTIA requests that we sent out after a 20-day waiting period returned with a request to readdress the RTIA fees (enclosed as post-office drafts) to a different official. As a result, 37 days elapsed before the RTIA requests completed by middle-class confederates reached government officials. Upon reflection, I determined that this self-imposed waiting period was an unnecessary measure; so many voter registration requests are received daily that our confederates' requests represented just a small fraction of the total. I return to the issue of the waiting period when I consider the effectiveness of the information intervention vis-à-vis that of other experimental groups in Section 5.

3.2. The Comparison Baselines

Of course, findings on the effectiveness of the RTIA intervention are meaningless in the absence of a relevant comparison. In these experiments, there are two comparison baselines: the waiting time for the applicants who followed the standard application procedure and the waiting time for those who bribed officials to grease the wheels of the administrative machinery. The bribe intervention took the form of an illicit payment submitted with an individual's electoral roll application. The bribe amount was Rs. 1,000 ($25). Interviews with middlemen indicate that this "tariff" is fixed across New Delhi, and applicants have to pay the same amount irrespective of their socioeconomic status.[10] This is a substantial amount of money, as it is equivalent to half of a slum dweller's monthly wage.[11] Government officials do not accept bribes directly, likely out of fear of being caught in the journalistic sting operations that are now common. Instead, bribes are collected by middlemen/touts who shuttle between applicants and government officials. Touts are individuals with substantial informal influence in the locality, often shopkeepers or notaries public. Three to four middlemen operate at every district election commission office in New Delhi. In informal conversations with my research associates, middlemen divulged that they operate in citywide networks that connect touts to government officials. Some touts said that they pay a monthly set amount to government officials at their offices without regard to how much work they are able to secure.

Applications from confederates randomly assigned to the bribe intervention were submitted to middlemen in bulk to ensure proper implementation of the experimental script and to prevent confederates from being directly implicated in offering bribes. Thus, applications were delivered to middlemen in four installments of three to five applications per transaction. It is in fact standard practice for small and medium-sized business owners to submit applications in bulk to middlemen, as this is the fastest way to obtain the necessary paperwork for new employees. The two middlemen with whom we dealt (one in each electoral district where the experiments took place) informed us that government officials have no knowledge of which applications come in bulk, as all applications that middlemen receive are mixed together. As an aside, it bears noting that officials at election registration offices did almost everything in their power to indirectly encourage applicants in the RTIA and control groups to turn to middlemen for assistance. For instance, those confederates were asked to provide additional documents that are not legally required.[12] As a result, those random-

[10] Interviews conducted by Aftab Alam and Aftab Alam with middlemen at six randomly selected election registration offices confirm this (early August 2007).

[11] The "tariff" for registering to vote is higher than the illicit fee charged for obtaining a ration card (Rs. 800; Peisakhin and Pinto 2010) or a driver's license (Rs. 650; Bertrand et al. 2006).

[12] The finding that government officials go out of their way to force applicants to pay bribes for basic public services is consistent with evidence provided by Bertrand and colleagues (2006) in their quasi-experimental study on the issuance of driver's licenses in New Delhi.

ized into the bribe group were spared several return trips to the election commission offices.

The second comparison condition is an untreated control. Those who were randomized into this group submitted their applications in accordance with the standard procedure. Submission of applications in the control group was also staggered so that government officials would not suspect that a research study was underway. In this group, it took us a little over a month to complete the application process for 61 urban poor applicants and 2 weeks to see the 60 middle-class applicants through. This time difference is indicative of the difficulties that the urban poor face in dealing with public officials.[13]

4. Results

Results for the urban poor and middle-class experiments are reported in Tables 3, 4, and 5. Differences between experimental groups are so large that I present some basic summary statistics that give a general sense of the effectiveness of the RTIA in Tables 3 and 4. Table 5 presents results from an ordinary least squares (OLS) regression in which the application-processing time is the dependent variable and dummies for each of the experimental interventions are the primary independent variables of interest; the constant is the mean processing time for the control group. I also include all the demographic controls in this regression. Presentation of results is somewhat complicated by the fact that many confederates were still not registered to vote by the time we stopped collecting data 11 months into the experiment. This delay was especially common in the control groups, in which 74 percent of the slum dwellers and 43 percent of the middle-class applicants were still waiting to have their names added to the electoral rolls. To circumvent this problem, I present the results in two different ways. In Tables 3–5, data are presented as if everyone who was still waiting to be added to the register on the last day of data collection was suddenly added to the rolls that day. Differences between experimental groups are so large that this assumption does little to diminish the substantive effects of the interventions.[14]

[13] Some readers might have ethical qualms about the nature of this study given that it involves bribery of public officials. This is an issue that I thought about at length before implementing the experiment. This study conforms with the requirements of the Common Rule (Federal Policy for the Protection of Human Subjects, 45 C.F.R. 46 [2009]), regulations governing experimental work involving human participants that stress respect for persons, beneficence, and justice. All confederates were informed about the nature of potential risks, and no confederate chose to leave the study. It is important to note that petty corruption is so entrenched in India that government officials do not regard offers of illicit payments as anything other than standard practice. This study was approved by Yale University's Institutional Review Board. For a discussion on the ethics of field experimentation, see Peisakhin (2011).

[14] It is standard practice when reporting findings from field experiments to separately report intent-to-treat and treatment-on-treated effects because at times not all the confederates or subjects comply with the experimental intervention (Angrist et al. 1996). In this instance, I do not report treatment-on-treated effects because no confederates dropped out of these experiments, and therefore the experimental scripts were properly implemented.

Table 3
Differences in Registration and Verification Times by Intervention

	Urban Poor Experiment			Middle-Class Experiment		
	Bribe	RTIA	Control	Bribe	RTIA	Control
Confederates	16	22	23	18	21	21
Voter registration:						
Confederates registered after 11 months	16	20	6	18	20	12
Median processing time (days)	140	164	331	123	150	319
Mean processing time (days)	146	189	330	125	168	309
	(23)	(55)	(10)	(15)	(50)	(22)
Residence verification:						
Confederates visited	16	22	23	18	21	21
Median time to verification (days)	23	38	74	15	24	54
Mean time to verification (days)	22	43	76	15	24	56
	(6)	(13)	(12)	(3)	(3)	(9)

Note. Standard deviations are in parentheses. RTIA = Right to Information Act.

Table 4
Difference of Means between Experimental Groups

	Urban Poor Experiment			Middle-Class Experiment		
	RTIA versus Bribe	RTIA versus Control	Bribe versus Control	RTIA versus Bribe	RTIA versus Control	Bribe versus Control
Voter registration	−2.96**	−5.67**	−5.27**	−3.82**	−5.26**	−5.34**
Residence verification	−4.87**	−5.23**	−5.26**	−5.20**	−5.56**	−5.33**

Note. The *z*-scores are from Mann-Whitney nonparametric difference-of-means tests. RTIA = Right to Information Act.
** $p < .01$.

The results are very clear, and Figures 1 and 2 are helpful for visualizing them.[15] Applicants in the RTIA and bribe groups do remarkably well in comparison to the control group in both experiments; however, the only group with a 100 percent success rate within the time frame of the experiment is the bribe group. While bribing is the most effective experimental intervention, recourse to the RTIA nevertheless halves standard processing times. Middle-class applicants in the RTIA group are placed on the rolls within the median of 150 days, compared with 319 days for those in the control group; the corresponding times for slum residents are 164 days for the RTIA intervention group and 331 days for the control group.[16] Differences between the interventions within each ex-

[15] In the figures, the dashed line in each box indicates the median value, the lower and upper bounds of each box are the 25th and 75th percentiles, and box whiskers represent the 5th and 95th percentiles. In Figure 2, outliers are shown as individual dots.

[16] I report medians rather than means because of the relatively small number of observations in each experimental group and because individuals who are not added to the register within 11 months push the averages upward. Means are reported in Table 3 and are the basis for the ordinary least squares regression analysis in Table 4 and are not substantially different from the medians.

Table 5
Intervention Effects with Covariates

	Urban Poor Experiment	Middle-Class Experiment
Bribe	−183.80**	−189.69**
	(12.69)	(11.02)
RTIA	−142.07**	−144.86**
	(12.03)	(10.32)
Gender	7.93	18.81
	(13.15)	(17.60)
Age	.22	−.09
	(.49)	(.97)
Literacy level	−1.71	
	(19.53)	
Years of schooling	.39	−.85
	(3.85)	(2.23)
Occupation	−2.05	−.29
	(5.87)	(10.22)
Annual income	−.00	.00
	(.00)	(.00)
Years lived in city	−.38	−2.06*
	(.87)	(.94)
% Muslim		−6.69
		(11.09)
Constant (control)	330.45	332.70
N	61	60
R^2	.84	.88

Note. Coefficients and standard errors (in parentheses) from ordinary least squares regressions are reported. RTIA = Right to Information Act.
* $p < .05$.
** $p < .01$.

periment are statistically significant, as is demonstrated by the difference-of-means test results shown in Table 4.

In Table 5, I present the results of an OLS regression that includes all the demographic covariates. As expected, differences between experimental groups are due exclusively to the nature of the intervention, as only one of the demographic covariates (in the middle-class experiment) is statistically significant: the length of time that the applicant has lived in New Delhi. Every additional year lived in New Delhi decreases processing time on average by 2 days. This effect is likely due to the fact that public officials run fewer checks on middle-class applicants who have lived in New Delhi for a long time. While substantively interesting, this covariate does not detract substantially from the magnitude of the intervention effects.

In Table 3, I also report the average amount of time that elapses before residence verification takes place. Residence verification is the first step in processing an application, when government inspectors visit the applicant at home to confirm that the application is not fraudulent. Data on residence verifications are a useful reality check—the fact that everybody in both experiments had an inspector visit their homes confirms that officials at election registration offices

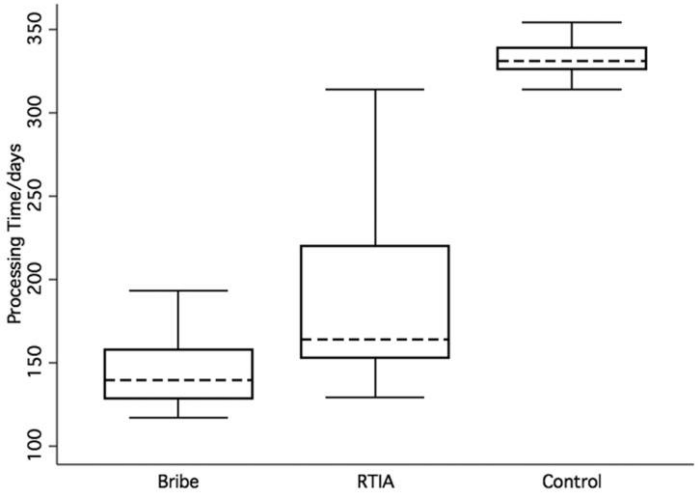

Figure 1. Application-processing times for the urban poor

received all the applications and had started processing them. Already at this early stage there are differences between experimental groups, and all of these differences are statistically significant in both experiments, as is reported in Table 4. As with application-processing times, confederates assigned to the bribe group have the shortest times, and those in the RTIA intervention receive residence verification in half the time it takes the individuals in control groups to have an inspector visit their homes.

I can now dispense with the generous assumption underlying the presentation of findings in Tables 3–5 and consider the true magnitude of the differences between experimental groups by turning to duration modeling. Duration modeling is a more precise statistical method for assessing the magnitude of differences between populations in data sets in which some observations are censored, which is to say that the event of interest (in this instance, an individual being registered to vote) has not yet occurred for a segment of the population under study. To assess the true magnitude of the differences between the various experimental groups, I use the Weibull parametric duration model. The Weibull model is designed to deal with monotonic survival rates and is suitable in this instance because the probability of being added to the roll conditional on not having been registered in the previous period is increasing over time.[17]

[17] The Cox nonparametric duration model makes fewer assumptions about the nature of data distribution than the Weibull model does, but it also requires greater power because it calculates a separate hazard rate for each of the experimental groups. The Cox model does not converge in the urban poor experiment because the number of individuals placed on the electoral roll is too low in the control group. I ran the Cox model for the middle-class experiment, and the resultant coefficients

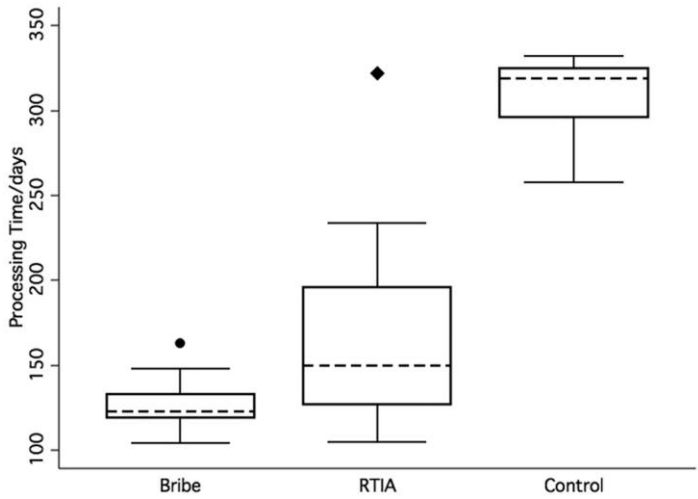

Figure 2. Application-processing times for the middle class

Duration analysis results are presented in Table 6. Among the urban poor, those who bribed are 124 times (exp(4.82)) more likely than applicants in the control group to be registered to vote at time t given that they had not yet been placed on the roll at time t_{-1}. Confederates in the RTIA group are 24 times more likely than those in the control group to be added to the register, and applicants who bribed are 4.4 times more likely to be registered to vote than those who used the RTIA. In the middle-class experiment, bribery is 104 times more likely to secure an applicant a place on the electoral roll at time t in comparison to the standard application procedure, given that he or she had not been registered to vote at time t_{-1}. Those in the RTIA group are 14 times more likely than applicants in the control group to be added to the register, and confederates in the bribe group are 5.8 times more likely to be registered to vote than those subject to the RTIA intervention. All differences between experimental groups are statistically significant.

The reason that the bribe intervention is dramatically more effective than the standard application procedure is because the probability of being registered to vote is so extremely low for confederates in the control group. For instance, in the urban poor experiment, the survival rate (that is, the probability of not yet having been registered to vote) on day 193, the day that the last confederate in the bribe group was placed on the electoral register, is .00 for confederates in the bribe group, .36 for those in the RTIA group, and 1.00 for applicants ran-

are not substantially different from those reported here from the Weibull model. In addition, I also ran a log-logistic model for both experiments, and the magnitude of the effects is similar to that in the Weibull model.

Table 6
Duration Rate Analysis

	Urban Poor Experiment		Middle-Class Experiment	
	Control	RTIA	Control	RTIA
Bribe	4.822**	1.483**	4.693**	1.763**
	(.610)	(.404)	(.557)	(.427)
RTIA	3.159**		2.654**	
	(.486)		(.385)	
k (shape parameter)	4.496	4.093	4.666	4.200

Note. Coefficients from a Weibull duration model are reported. Standard errors are in parentheses. RTIA = Right to Information Act.
** $p < .01$.

domly assigned to the control group. An additional 121 days elapse before anyone in the control group is registered to vote. Similarly, in the middle-class experiment, on day 163, the survival rates are .00 for the bribe group, .43 for the RTIA group, and 1.00 for the control group, and 95 additional days pass before the first confederates in the control treatment are registered to vote. In short, recourse to the RTIA and bribing exponentially increase one's chances of being registered to vote.

5. Discussion

5.1. Principal Findings

I have demonstrated that India's RTIA provides a free and effective alternative to bribery. In a more abstract sense, the experimental results show that higher levels of transparency are indeed associated with greater government efficiency and lower incidence of bribery (assuming that as people learn about the effectiveness of the RTIA, their willingness to offer bribes decreases), even in societies where there is a substantial power differential between government officials and the least privileged members of the public. The duration analysis shows that slum residents randomized into the RTIA group are considerably more likely to be registered to vote than their peers who file a standard application. The RTIA is also very effective for middle-class applicants.

It seems initially that bribery results in the shortest processing times: slum residents assigned to the bribe intervention are placed on the voter register 4.4 times faster than those in the RTIA intervention, and among middle-class applicants, bribery is 5.8 times more effective than the RTIA. Yet recall that slum residents had a gap of 20 days and middle-class confederates had a gap of 37 days between filing voter registration applications and mailing their RTIA requests. Had RTIA requests been filed on the same day as electoral roll applications, the median slum resident should have been registered to vote within 144 days (compared with 140 days for those bribing). The median middle-class

Table 7
Nonexperimental Comparison of Means between the Urban Poor and Middle Class Experiments

	z-Score	*N*
Bribe	2.80**	34
RTIA	1.73+	43
Control	3.71**	44

Note. The *z*-scores are from Mann-Whitney nonparametric difference-of-means tests. RTIA = Right to Information Act.
+ $p < .10$.
** $p < .01$.

applicant in the RTIA group would have been placed on the electoral roll within 113 days, compared with 123 days for those bribing. These findings provide some weak evidence for the proposition that the information intervention could be as effective as bribing. This was not an expected result and is yet another testament to the effectiveness of greater transparency.

A comparison of the two experiments suggests that middle-class applicants are added to the electoral register faster than their less privileged counterparts. The urban poor have to spend more time at government offices filing their paperwork, and even their bribes do not erase the class difference. Results of a nonexperimental nonparametric difference-of-means test between the same experimental groups across the two studies are presented in Table 7. These differences are only suggestive, since they are nonexperimental and because it might genuinely take government inspectors longer to locate slum dwellers as part of the residence verification process, as addresses can be difficult to locate in slums in which tenements are not constructed in a regular pattern. In this context, it is particularly surprising that there should be only a weakly statistically significant difference between processing times for the RTIA groups across the two experiments ($z = 1.73$, $p = .08$). Access to information appears to empower the poor to the point of receiving almost the same treatment as middle-class individuals at the hands of civil servants. This leveling of treatment is something that payment of a bribe cannot do. This tentative finding suggests that, going forward, policy makers might want to use regulations that promote greater transparency to combat discrimination against the poorest members of society.

5.2. Limitations

This study is not without its limitations. One of its biggest weaknesses is that it cannot shed much light on the specific mechanism behind the RTIA's effectiveness. From the perspective of the experimental design, I have to treat the civil service as a black box. However, interviews with senior civil servants suggest that the act is effective because government officials fear that failure to disclose information or to otherwise handle requests from the public in an efficient

manner will negatively affect their chances of promotion.[18] Other work on corruption in India (notably Wade 1985) shows that anything that effectively interferes with government officials' chances of promotion is taken very seriously by public servants. In addition, some civil servants might also be concerned about the act's penalty provisions, although it is common knowledge that these are rarely applied.

On the methodological front, these field experiments face challenges to robustness and generalizability. To take the issue of robustness first, the small number of confederates in each of the experimental groups might seem to be an area of some concern. However, the intervention effects are so substantial that my findings concerning the relative effectiveness of the RTIA are unlikely to be entirely spurious. More important, the effects that I report here are consistent with those in other studies of bribery in New Delhi (compare Bertrand et al. 2006) and have been confirmed in a different setting in New Delhi (Peisakhin and Pinto 2010) and in rural India (Development Alternatives 2007). The generalizability challenge is common to all experimental work, and it would certainly be justifiable to wonder whether the effects that I demonstrate apply beyond India's capital and carry over to other developing countries. The only way to address this in a satisfactory fashion is to provide new empirical evidence from other contexts. It bears noting, however, that existing studies on voice and the quality of public service provision are generally consonant with the view that higher levels of government transparency are associated with improved outcomes for the poor and the underprivileged (compare Jenkins and Goetz 1999, Kaufmann, Mehrez, and Gurgur 2002, and, with caveats, Olken 2006).

5.3. Future of the Right to Information Act

It may be that the picture I paint here of an effective FOIA is just a snapshot in time that might no longer depict reality 10 or even 5 years down the road. More important, for the RTIA to be a force for good, ordinary citizens must know about its existence. We know that 7 years into the act's existence, awareness about it is far from universal: a government-commissioned report estimates that 33 percent of India's urban population knows about the RTIA, and this number is as low as 15 percent among India's least privileged groups, such as members of tribes and scheduled castes (PricewaterhouseCoopers 2009). However, the act's usage has been rising exponentially, and the number of applications filed at all federal government offices increased from 24,436 in 2005–6 to 263,261 in 2007–8 (Peisakhin and Pinto 2010, p. 277). High illiteracy rates among India's poor hinder active use of the act by the country's most underprivileged citizens. For the poor to use the RTIA, there must be an option to file complaints by

[18] Interviews with Wajahat Habibullah, India's chief information commissioner (June 28, 2007), two state information commissioners from Kerala (June 23, 2007), a senior retired Indian Administrative Service official in Bangalore (June 24, 2007), and the secretary of the State Information Commission of West Bengal (June 11, 2007).

telephone. Commendably, such a practice is already in place in several states, mostly notably in Bihar, and India's NGO community is working hard to push more state governments to allow members of the public to phone in their complaints.

The question still remains whether the act's effectiveness will persist once its novelty wears off and RTIA requests start to eat into civil servants' illicit revenue stream. There is certainly good cause for hope 7 years into the act's existence. Anecdotal accounts in the press indicate that the act continues to be very effective even for the poorest members of rural Indian communities (see Polgreen 2010). What keeps the RTIA alive is not so much its penalty clause as the fact that civil society is maintaining a constant pressure on the government to ensure the act's continuing effectiveness.

6. Conclusion

In this paper, I have demonstrated the validity of the theoretical insight that higher levels of transparency result in greater efficiency of public service provision and lower levels of corruption even in societies where there are vast inequalities between government officials and the least privileged members of society. Leveraging data from two field experiments in which middle-class residents and slum dwellers of New Delhi applied to register to vote, I have shown that recourse to India's FOIA law, the RTIA, results in dramatically faster processing times than for the standard application procedure. Furthermore, recourse to the RTIA is almost as effective as bribery. Even more surprising, while I have confirmed that middle-class applicants generally receive a higher quality of service, application-processing times for middle-class confederates and the urban poor who file RTIA requests are close to indistinguishable. This suggests that recourse to information has the power to erase social, cultural, and economic differences. This insight deserves further exploration in other settings.

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