Does Information Disclosure Reduce Corruption?
Evidence from Field-Experiments in India

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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 (Rose-Ackerman 2004, Svensson 2005, Shleifer and Vishny 1993). When referring to corruption in this paper I mean specifically administrative corruption, i.e. abuse of public office for private gain by government officials, and use the standard definition of corruption as "behavior which deviates from the formal duties of a public role because of private regarding (personal, close family, private clique), pecuniary or status gains" (Nye 2001[1967]: 284). While there is agreement in the literature that corruption is bad for growth, there is little consensus on how to most effectively reduce graft. Suggestions for anti-corruption strategies range from increasing state capacity to raising government officials' salaries to strengthening civil society. The reason there is no obvious agreement on the relative effectiveness of anti-corruption initiatives is because it is very difficult to directly measure their impact, as self-reporting of corrupt practices is notoriously unreliable. In this paper I explore the unexpected anti-corruption potential of information disclosure laws and measure the effectiveness of this anti-corruption tool against the baseline of corrupt behavior. Specifically, I present data from two field experiments that I conducted in India to demonstrate that citizens who use freedom of information laws alongside a standard application for a government service receive almost the same quality of service as those who bribe. This surprising finding suggests that information disclosure laws can be effective anti-corruption tools, as they present citizens with a free and legal alternative to bribery.

Scholars and policy makers have had a substantial interest in the anti-corruption potential of information disclosure (Sen 1984, Stiglitz 2002, World Bank 2004). Freedom of Information Acts (FOIAs) have become a staple of good governance initiatives; fifty-six of the sixty-six nations that had a FOIA in place by 2006 had adopted disclosure of information laws in the last twenty years (Ackerman and Sandoval-Ballesteros 2006: 85). Greater availability of information on government activities allows the public to more effectively exercise oversight over the policy making and administrative process.

Politicians and civil servants are less likely to engage in corruption when their decisions are open to public scrutiny. FOIAs are just one facet of the drive for greater information disclosure to make governments more responsible to the citizenry. A growing literature stresses the importance of the print media in making governments less corrupt and more responsive to public needs (Besley and Burgess 2001, Reinikka and Svensson 2005, Gentzkow et al. 2006). The problem with newspapers spreading information about government initiatives is that in many developing countries large segments of the population are illiterate. In India, for instance, only sixty-five percent of the population is literate according to the 2001 Census; it is the most vulnerable members of society and therefore most dependent on the state for survival who are the least literate (Bardhan and Mookherjee 2000: 138).

This paper is part of a small but growing empirical literature on corruption. It is notoriously difficult to collect reliable data on corruption given the government officials' reluctance to discuss this practice and frequent reticence among the public to disclose their behavior. Yet, empirical data on corruption is essential if we are to find effective remedies to this problem and go beyond a theoretical debate. Among notable recent empirical work on corruption is Wade's study on graft in the irrigation system in the Indian state of Kerala (1982, 1985), Davis' research on corruption in the water and sanitation sectors in India (2004), Reinikka and Svensson's study of graft in Uganda's schooling system (2004), work by Meagher, Upadhyaya, and Wilkinson on the effect of local empowerment programs on corruption levels in small public works projects in Nepal (2000), Di Tella and Schargrodsky's study of the impact of audits on corruption in the public hospitals of Buenos Aires (2003), Bertrand et al.'s field-experiment on graft in the issuance of driving licenses in New Delhi, and a series of well-executed studies on corruption in public goods' provision in Indonesia by Olken (2005, 2007). These authors describe the pervasiveness of corruption in developing countries and suggest policy remedies that include empowerment of individuals and the strengthening of civil society (Meagher et al. 2000, Olken 2005), strengthening of state capacity (Di Tella and Schargrodsky 2003), and provision of better quality information about government activities (Reinikka and Svensson 2004).

This paper engages directly with the themes raised in existing literature and seeks to explore whether empowerment of the citizenry via freedom of information laws provides an alternative to corruption. When a free and legal alternative to bribing is available members of the public will prefer to avail themselves of it instead of paying a bribe, therefore existence of functional FOI laws must necessarily bring about a lower willingness to pay bribes. I conceptualize FOI laws as an auditing mechanism that can be triggered by members of the public. This paper picks up where the current literature left off by assessing whether targeted empowerment of individuals via a FOI law is effective at securing a government service that is directed at individuals (and therefore cannot be secured via concerted grass-roots action).

I consider the most basic public good assigned to citizens—the right to vote. Democratic countries cannot deny voting rights to eligible citizens, and the right to vote comes with certain status-related privileges (proof of citizenship, age, etc.), which can be useful in securing employment and social benefits. In India, where the field experiments described in this paper were executed, placement on the electoral roll is not always automatic. In fact, in certain states those legally eligible and wanting to be added to the electoral roll resort to bribery in order to secure their right to vote out of exasperation, as they are either omitted in the infrequent electoral censuses or their applications to be added to the electoral roll remain unprocessed for years. Some states are better than others at granting citizens the right to vote, and, for instance, in Maharashtra and Karnataka, bribery to be placed on the electoral roll is rare to non-existent. These field-experiments were carried out in New Delhi, India's national capital, where bribing to secure the right to vote is commonplace.

The field experiments that I describe were designed to test the effectiveness of India's FOI law, the Right to Information Act (2005), in helping citizens secure a place on the electoral roll. I carried out two identical experiments among the urban poor and middle class individuals residing in New Delhi. Individuals participating in this study were randomly assigned to three conditions: the Right to Information Act (RTI) treatment, the corruption treatment (bribe), and the control treatment. Those randomized

into the RTI treatment were subject to the information intervention. These individuals were informed about the RTI as a free and legal alternative to bribing and were asked to submit an RTI request alongside their electoral roll application asking government officials about the status of their application and about the average processing time for applications of this type. To establish the relative effectiveness of the RTI application I compare the results of this intervention to processing times for applications filed by confederates who bribed and those who applied in a standard fashion.

The same experiment was carried out in a representative slum in New Delhi and also in a middle class area. As a result, in addition to assessing the effectiveness of recourse to information to help bypass corruption I set out to achieve two further aims: (i) to provide a description of how corruption operates (building on Bertrand et al.'s work on bribery to obtain driving licenses in New Delhi (2006)), and (ii) to establish in a non-experimental way whether the urban poor and the middle classes are subject to differential treatment by public officials. There is general consensus in the literature on developing countries that ethnicity, and in India's case also caste affiliation, matter a great deal when legislators come to assign public goods to specific communities. Legislators frequently favor their own ethnic or social group when allocating public spending even if they promised otherwise during the election campaign (Pande 1999, Chandra 2004, Rudolph & Rudolph 1987, Banerjee and Somanathan 2007, Besley et al. 2004). In this paper I set out to test whether civil servants, mostly upper caste Brahmins, also discriminate against the underprivileged, who are either lower caste Hindus or Muslims. To the best of the author's knowledge the question of discriminatory behavior by government officials remains largely unexplored in literature on field experiments (with a notable exception of Fried et al. 2008). The study described in this paper is a complement to a field experiment on corruption in the provision of ration cards in New Delhi (Peisakhin and Pinto 2008).

My primary finding is that recourse to the freedom of information law is an effective free and legal substitute to bribery. Those randomized into the RTI intervention were added to the electoral roll on average only twenty-five days after the applicants who

bribed. The average processing time for those in the RTI treatment was 164 days among the urban poor and 150 days among the middle class applicants, as compared to the baseline processing time for those who bribed of 140 days for the urban poor and 123 days for the middle classes. The difference between the "bribe" and "RTI" treatments is negligible if we discount the time it takes for RTI applications to be processed. When data collection was discontinued for logistical reasons (approximately 11 months after the first applications were filed) the majority of those in the "control" condition (i.e. those who followed the standard application procedure) still had not been added to the electoral roll. This means that those who apply to be registered to vote without payment of bribe or recourse to the RTI are either not granted the right to vote at all or at the very least have to wait minimally twice as long to be added to the electoral roll.

Non-experimental evidence suggests that there are systematic differences in the way government officials deal with the middle classes and the urban poor: middle class applicants receive preferential treatment in control and bribe conditions. Curiously, middle class and urban poor applicants subject to the RTI intervention receive comparable treatment. This suggests that empowerment of the underprivileged by means of information provision can have the effect of breaking down status barriers between the wealthy and the poor when it comes to public service provision. Of course, this finding is tentative as it is not clear that it would hold once the poor start using the RTI Act in greater numbers. With regards to the process of bribery, I find that public officials do not collect bribes directly, instead relying on touts as mediators. Qualitative evidence indicates that there is a well-established corruption market operating in every major Indian city with prices for various services fixed across the city. This is consistent with other work on corruption and the role of middlemen (Peisakhin and Pinto 2008, Oldenburg 1987, Bertrand et al. 2006).

This paper is structured as follows: Section I outlines the context behind this study, describes India's freedom of information law, and lays out the randomization procedure. In Section II I describe the experimental design and then touch on the ethical

concerns that this study raises in Section III. I present results of the field experiments in Section IV and discuss them in Section V.

Section I: The Context

a. India's Freedom of Information Law

The Right to Information Act is India's freedom of information law. The RTI Act had a painful birth and is India's second attempt at setting up a functional national FOI statute. In 1975 the Indian Supreme Court urged the government to disclose more information to the public, arguing that India's citizens cannot fulfill their constitutional right to freedom of speech if they are not fully informed about public policy (*Raj Narain v Indira Gandhi)*. In a landmark ruling in 1982 (*S.P. Gupta v Union of India*) 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". The country's civil society activists answered the Supreme Court's rallying cry, and in the early 1990s began to agitate for FOI laws at state level. Tamil Nadu was the first state to adopt a FOI law in 1997, and under pressure from groups like the MKSS and Parivartan landmark FOI legislation was put in place in Rajasthan in 2000 and in New Delhi in 2001. The first version of a national FOI statute was adopted in 2002 as the Freedom of Information Act. 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 adopted left it largely up to the civil service whether to furnish information to the public and was stripped of all punitive provisions. The 2002 FOI Act never came into force. Another attempt at a national FOI provision followed in 2005—the Right to Information Act was passed by parliament largely in the form drafted by civil society activists following Sonia Gandhi's personal intervention. The RTI Act came into effect on 12 October 2005.

The RTI Act's stated function is to ensure that the citizenry is fully informed about the government's activities—the Act even contains a provision that instructs government departments to proactively disclose information through publications and via

the internet. Yet in the run up to the Act's adoption it became apparent that this statute was going to be used by the public to ensure better provision of public services; it is telling that the father of the original draft of the Freedom of Information Act 2002, Hari Dev Shouri, was India's first consumer rights activist. In short, the RTI Act provides an opportunity for members of the public to directly audit the activities of legislators and civil servants—it thus restores the link between the people, the ultimate principal in democracies, and the public servants. Written requests for information (although in the state of Bihar they can also be phoned in) are lodged with a public information officer (PIO) of the government department most immediately relevant upon payment of a minimal fee (Rs. 10=$0.20). The PIO then has up to thirty days to respond to the information request.

The Act provides for complaint and appeal procedures. If the PIO's answer is not forthcoming within thirty days the applicant may file a complaint with the state information commission. If the applicant is not satisfied with the information furnished by the PIO, she may appeal for a fuller disclosure to the first appellate authority (usually a departmental secretary or deputy secretary), and if still dissatisfied then 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 state information commissioners. India's Chief Information Commissioner holds the rank of a national cabinet minister and commands great administrative clout. As I will demonstrate later in the paper, penalties are very rarely imposed. Nevertheless, in India's hyper-competitive civil service failure to disclose information under the RTI Act can hamper one's professional development in an informal way. According to senior civil servants we interviewed all administrative mishaps are noted in the civil servants' personal files, and even the smallest incident can be cause enough for an official not to be promoted. The fear of an administrative reprimand that might carry hefty consequences is particularly prevalent in numerous states where retired heads of the provincial civil service have been appointed information commissioners.

b. Selection of Experimental Sites and the Randomization Procedure

In this paper I discuss data from two separate but identical randomized field-experiments. Background attributes of the participants and the location where the experiment was carried out are the only two things that are different between these two experiments. In this section I describe the two pools of subjects and demonstrate that the treatment groups within each pool are well balanced.

The urban poor participants in this experiment come from northeastern Delhi, an area with a high density of shantytowns, and all reside in the same local slum. Due to budgetary constraints I was not able to randomly select a slum for this field experiment. In many slums local community groups have become sufficiently commercialized to expect large amounts of money from outsiders operating in the area on humanitarian and research projects. The fact that the experimental slum was not selected randomly inevitably negatively affects my ability to make extensive generalizeable claims from these data. However, the slum that was selected for the experiment is representative of New Delhi slums in all important respects other than religion. Like 91% of the city's slums it is non-notified (located on public land). Sixty percent of slum residents there have access to tap water and electricity, as compared to 71% of Delhi's slum dwellers who have access to tap water, and 61% who have electricity. Like 76% of Delhi's slums it has open sewers, and it gets waterlogged during heavy rain just like 72% of all the city's slums. This slum is located within close proximity to a primary school (like 68% of New Delhi's slums). The slum's tenements constructed of pucca and semi-pucca materials are typical of a non-notified slum in any large Indian city. The only thing that conceivably distinguishes it from the majority of Delhi's slums is that there is no hospital located nearby; 61% of the city's slums are situated within walking distance from a hospital.[1]

A pool of 61 slum residents eligible to vote but not yet registered in New Delhi was drawn up with the assistance of a local community worker. Those on the list were

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[1] Statistical description of New Delhi's slums is drawn from the 2002 National Sample Survey (NSS) on conditions in urban slums (NSSO 2003). The NSS is a government program administered by the Ministry of Statistics.

then randomly assigned to the three experimental conditions: information treatment (RTI), bribe treatment, and control. The bribe treatment was capped at 17 subjects during randomization due to budget limitations. The subject pool is described in Table 1. A representative confederate is a Muslim semi-literate male in his mid to late twenties who has lived in Delhi for most of his life, is currently employed in a semi-skilled job (daily laborer, security guard, etc.), and gets by on a little over $1.50 a day. There are no statistically significant differences between the treatment groups and the control. This demonstrates that the treatment conditions are well-balanced on the subjects' background characteristics.

[TABLE 1 GOES HERE]

The urban poor participating in this experiment are all Muslims, and in this important respect are different from an average slum dweller in Delhi, who is a Hindu. Muslims make up approximately 20% of slum dwellers of New Delhi. In the slum where the experiment took place 60% of the population is Muslim. I ended up with an exclusively Muslim sample because the community worker who assisted me in the field is himself a Muslim. Muslims are considered to be the most disadvantaged group in India's multi-cultural society after the scheduled tribes (cf. Banerjee and Somanathan 2007). At the same time, all slum-dwellers are from highly disadvantaged groups, be they members of the scheduled castes, scheduled tribes or India's sizeable Muslim population. It bears stressing that although the experimental subjects are all Muslims, they are representative of an average New Delhi slum dweller by other socioeconomic measures. Seventy percent of the experimental participants are literate, as compared to an overall literacy rate of 67% among Delhi's slum dwellers (both figures include those who cannot write) (Government of India 2001). On average they have 4.7 years of schooling; this is similar to the average 4.5 years of education among Delhi's urban poor.[2]

I organized the second field experiment in a middle class area of the city. In this instance I was again uanble to randomly select the experimental site, as I needed an area

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[2] Data on education levels among New Delhi's slum residents is drawn from Jha et al. 2005 (27). In their study World Bank researchers sampled 802 households in 20 randomly selected slums in New Delhi.

with an abundance of middle class individuals not already registered to vote. A city district close to a major university and known for its quality housing and shopping areas was deemed suitable. I assumed that it would be easier to find unregistered voters near to a major university because younger people would tend to live in such an area. The subject pool is described in Table 2. A representative subject is a 23 year-old Muslim male with a bachelor's degree who has lived in Delhi for at least eight years. The average income is deceptively low in each of the treatment conditions. Only 25 of the 62 experimental participants are employed—the remainder are either students or homemakers. In fact, the average salary among those who are employed is Rs. 148,320 (approximately $3,700); this is seven times higher than the amount earned annually by an average slum dweller. Mean occupation is also somewhat of a misleading category. All the confederates are highly skilled with 14 of the 60 holding postgraduate qualifications, but homemakers, who I coded as "unskilled labor" are pulling down the average. There are no statistically significant differences in background characteristics between the various treatments and the control; this proves that the treatment conditions are well balanced.

[TABLE 2 GOES HERE]

Muslims and men are overrepresented in my sample. Muslims make up only 13% of the Indian population (Government of India 2001), whereas 83% of subjects in this experiment are Muslim. This misbalance is due to the fact that local university specializes in educating Muslim Indians. I do not consider this misbalance to be problematic, as the urban poor subjects are also predominantly Muslim. It is regrettable that few women took part in the study. In fact, researchers commonly encounter problems recruiting middle class Indian women to participate in experiments (for instance, in Bertrand et al.'s pilot all the women participants dropped out (2006: 7)); therefore, in this respect my study is no exception. Evidence from interviews suggests that middle class women are less likely to participate in research studies than men because in traditional families men are expected to take care of any official or semi-official matters. This is very different from the dynamic in slums, where women are expected to make a living and deal with government officials alongside their male relatives.

b. The Application Process

The right to vote is constitutionally guaranteed in India. The functioning of India's electoral system is regulated by the Representation of the People Act 1950 (RoP Act), and the Registration of Electors Rules 1960. Specifically, section 23 of the RoP Act establishes the office of the Election Registration Officer (ERO) at the level of municipal and rural state assembly electoral districts. The ERO and his staff are responsible for updating electoral rolls. Detailed instructions concerning maintenance of electoral rolls are issued by national and state-level Chief Electoral Officers.

The electoral roll is updated in three different ways: via (i) intensive revision, (ii) summary revision, and (iii) continuous revision. Intensive revision of the rolls is supposed to occur every five years: during the period of intensive revision government officials are expected to visit every household in the electoral district to record any changes to the current register of electors. Evidence from qualitative interviews indicates that in some districts intensive revision happens as infrequently as every seven years; even when it is done in a timely fashion, many households (particularly in slums) are omitted. Summary revision happens more frequently, ordinarily every three years immediately preceding a major election. During this period the EROs post the latest version of the electoral roll in public locations, and invite district residents to report any changes. Anecdotal evidence suggests that summary revision is quite effective, as the EROs are pressured by political parties in the run up to elections to ensure that the rolls are correct. Finally, administrative rules also provide for continuous revision of the electoral rolls, when those wishing to register to vote or to alter their registration may file the necessary paperwork at the ERO's office at any time other than during 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 during elections (summer 2007).[3] Conversations

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[3] In fact, because data collection took such an unexpectedly long time (upwards of 11 months), the summary revision process in the run up to a state assembly election scheduled for the autumn of 2008 had

with government officials and with civil society activists indicate that a substantial proportion of India's poor make use of the continuous revision of the rolls when registering to vote. Among middle classes mostly only mobile and relatively young people take recourse to the continuous revision process. The National Election Commission instructs the EROs to register electors within 10 days during the period immediately preceding an election or otherwise within the maximum of 60 days in between elections (Election Commission of India 2006: Item 78). Thus, at the outset I expected that the subjects' names would appear on draft electoral rolls within *10-60 days* of the application date. This expectation was somewhat tempered by the admission of senior election officials that continuous revision is often flawed, as the EROs are known to refuse registration requests outright or create administrative hurdles for those applying outside of the summary revision period (Election Commission of India 2006).[4]

To register to vote the applicant is required to furnish proof of Indian citizenship (usually a birth certificate, a ration card, a tax ID, or a driving license), proof of age (usually a birth certificate, a school leaving certificate, or a driving license), and proof of residence in a specific electoral district (ordinarily a utilities bill, a ration card, or a house rent receipt). Individuals whose names are listed on the electoral roll are entitled to obtain the Elector Photographic Identity Card (EPIC), known commonly as the voter ID card. Voter ID is obtained via a relatively straightforward procedure once the voter's name is entered on the electoral roll. This document is the most common form of identity proof in India, as driving licenses and passports are rare. Voter ID is commonly required when applying for a job, opening a bank account, and obtaining government benefits. In short, being registered to vote brings with it financial and professional benefits. Opportunities that come with voter registration are particularly alluring for the urban poor.

Voter registration applications are accepted only at district Election Registration Offices covering the aspiring voter's residential area. The middle class and urban poor

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commenced in the slum by the time the experiment was being wound down in late June 2008; data collection ceased on 30 June.

[4] According to interview subjects, the summary and intensive revision processes can also be highly inefficient—many legally eligible individuals are either never registered to vote or have to wait for several years before their name appears on the roll.

participants in the field experiments lodged their applications at two different Election Registration Offices. These offices are representative of a standard Election Registration Office in New Delhi. Overall, there are seventy Election Registration Offices in the city, corresponding to the number of Delhi assembly constituencies. These offices are designed to be identical. Because each office has a wide coverage area, all Election Registration Offices serve urban poor and middle class city residents. Five officials and two assistants are ordinarily employed at each office: the ERO, two upper division clerks, three lower division clerks, and two peons. The ERO and the clerks rotate between various tasks like maintenance of the electoral roll, residential verifications, and filing of reports—the exact assignment of responsibilities rests in the hands of the Sub District Magistrate/Deputy Commissioner, who is also the immediate superior of the ERO. The EROs and the clerks are rotated between district offices, and often also between government departments, every three to five years. The ERO and the clerks are 35-45 year old males holding bachelor degrees and are upper caste Hindus. In the spring of 2008 the EROs were paid approximately Rs. 108,000/annum, upper division clerks Rs. 54,000, and lower division clerks Rs. 42,000. In addition to their fixed salaries these civil servants also receive free benefits from the government, and bonuses for additional work. On average, seventy to eighty applicants are seen by ERO officials daily; these individuals come to the Election Registration Office to apply to be added to the electoral roll or to obtain EPIC cards. Applicants with new queries are received in the morning, whereas documentation and existing queries are processed in the afternoon.

Section II: The Field Experiment

a. The Information Treatment

In this section I describe the experimental interventions. The information treatment is the most important of these. Under this treatment the subjects were informed about the existence of the Right to Information Act, and were asked to file RTI requests shortly after submitting paperwork to be added to the electoral roll. The RTI request was addressed to the relevant public information officer and it contained two questions: (i) what is the status of this individual's application, and (ii) how long does one have to wait on average to be added to the electoral roll in this district? It bears noting that the PIO is

the district Deputy Commissioner, i.e. the immediate superior of the civil servant who accepted the subjects' electoral roll applications. Thus, when the RTI request is received it is passed down from a superior to an immediate subordinate; this likely strengthens the audit effect of the information intervention. To ensure consistency of treatment my research associates completed and mailed all the RTI requests along with the requisite administrative fee of Rs. 10. Half of the RTI requests were written in English, and the other half in Hindi—these two languages are used interchangeably in India. In addition, the filing of electoral roll applications, and the mailing of RTI requests were staggered. This was done so that the PIO would not receive a large volume of identical requests on the same day.

Twenty days lapsed between when the slum residents applied to be registered to vote and the mailing of the RTI requests. I decided to wait for this long to give election commission officials time to start processing the electoral roll applications—this way they could not have ignored incoming RTI requests on the grounds that they had not seen the requesters' names. The waiting period between the date when the application was submitted and when the RTI request was mailed was thirty-seven days in the middle class experiment. There, the original RTI requests were returned one week after they were mailed with a written request that the payment of Rs. 10 be made out to the same PIO under a different designation (PAO instead of PIO). Upon reflection, the waiting period was a conservative measure and the RTI requests could have been filed on the same day as the electoral roll applications. The issue of the waiting period will come up once again when we consider the effectiveness of the information intervention vis-à-vis other treatments. Finally, it bears noting that a written request from a slum resident is a rarity from the perspective of government officials, and signals that an individual or an organization of higher social status is involved. In this sense, for the urban poor the RTI intervention is a composite treatment—it is a signal that the officials' work is being audited and is also indicative of the fact that the applicant is being supported by a middle class organization or individual.

b. The Comparison Baseline

In order to assess the effectiveness of the RTI intervention it is necessary to compare processing times under RTI to the length of time it takes to be placed on the electoral roll under the bribe treatment and in untreated control. The bribe treatment takes the form of a bribe payment alongside an individual's electoral roll application. The bribe amount is Rs.1,000 (approximately $25). Evidence from qualitative interviews with middlemen indicates that this "tariff" is fixed across Delhi. Rs. 1,000 is a substantial amount, particularly for the urban poor, for whom it represents half their average monthly wage. Notably, the "tariff" for registering to vote is higher than the price charged for obtaining a ration card (Rs.800--Peisakhin and Pinto 2008) or a driving license (about Rs.650—Bertrand et al. 2006), even though the application process is identical for the ration card and placement on the electoral roll. Participants in the field experiments were compensated for bribe payments out of the research budget.

Notably, government officials do not accept bribes directly, likely out of fear of being caught red-handed in journalistic sting operations that have become common in recent years. Bribe payment is collected by middlemen/touts who shuttle between applicants and government officials. Middlemen operate in most government offices in Mumbai, Delhi, Kolkata and Bangalore where I collected qualitative data on corruption (on the role of middlemen see Oldenburg 1987, Bertrand et al. 2006, and Peisakhin and Pinto 2008). They are commonly individuals with substantial informal influence in the locality, often shopkeepers and 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. These networks set standard "tariffs" and agree codes of practice. Some touts said that they pay a set amount to government officials at their office on a monthly basis without regard to how much work they are able to secure—this first closely with Wade's account of the highly structured operation of India's corruption market (1985,1982). In short, a sophisticated market in corrupt services has evolved in India's major cities at least in relation to provision of most common government services (right to vote, ration card, driving license, etc.). Prices

appear to be indifferent to social status, as "tariffs" are the same for middle class and urban poor applicants.

Applications randomly assigned to the bribe treatment were submitted to middlemen in bulk to ensure proper implementation of the bribe treatment, and to prevent experimental subjects from directly offering money to touts. Middlemen informed us that government officials have no knowledge of which applications come in bulk, as these are mixed in with all the other applications that middlemen receive. In addition, bulk submission of applications is common; this is something that business owners do when they need to rapidly obtain paperwork for their employees. Applications randomized into the bribe treatment were delivered to middlemen in four small installments (of 3-5 applications each) on different days. Applications were randomly assigned to these four installments.

Applicants assigned to the bribe treatment saved a substantial amount of time by comparison to other participants in this experiment. They did not need to come to the office in person or to spend time procuring additional documents beyond the basic minimum of paperwork proving citizenship, age, and residential status. By a conservative estimate this represented a saving of approximately 10 hours for an average urban poor applicant and of about 4 hours for a middle class individual. District-level election officials did everything in their power to indirectly encourage applicants to turn to middlemen for assistance. Subjects in both field experiments were asked to provide additional documents that are not legally required. Almost everybody was asked to present a certificate from their previous place of residence confirming that they were not registered to vote there. The urban poor presented notarized statements to the effect that they were not registered to vote elsewhere to bypass this requirement. Middle class applicants had to pay between Rs.25-100 ($0.50-$2.50) directly to government officials to have them reconsider this requirement. All the urban poor applicants had to return to the Election Registration Office two or three times before their applications were finally accepted. Whereas middle class applicants received relatively courteous treatment from government officials, slum residents were frequently insulted. One clerk called the

applicants "flies and mosquitoes", told them that their applications would be discarded, and wondered aloud why they wanted to register to vote.

The second comparison condition is an untreated control. Those who were randomized into this group submitted their applications following the standard procedure. Control applications were also staggered so that government officials would not suspect that an academic study was underway. Overall, it took us a little over a month to complete the application process for 61 urban poor applicants and two weeks to see the 60 middle class applicants through. The non-experimental time differential between the two groups is suggestive of the difficulties that the urban poor face in accessing government services when they do not seek recourse to bribery.

Section III: Ethics

This field experiment touches on important ethical issues. Bribery of government officials was an important component of the research strategy. According to Indian law bribery is illegal; officials who accept bribes, and citizens who offer them are legally liable. Ethical issues were foremost in my mind when designing this experiment, and I went ahead with it for a number of reasons. First, it is impossible to do a policy audit experiment in the absence of a meaningful baseline. Quality data on bribery are hard to come by, because most people are reticent to provide the necessary details. Also, there is a substantial risk that those individuals who bribe and are willing to talk about their experiences in detail are somehow different from the population at large. General attitude towards graft are easier to collect among the middle classes for whom bribery is part of the daily routine, however slum dwellers prefer not to discuss corrupt practices. Social networks in slums are much tighter, and the culture of shame is pervasive. In short, an assessment of the effectiveness of free and legal alternatives to graft would have been impossible without reliable data on the practice of corruption.

At the outset I considered potential security risks to the applicants and the research team. I did not find any stories in India's major newspapers about applicants for basic public services being charged or arrested for offering bribes; arrests of corrupt public officials are common of course. In addition, it is common knowledge that bribery

is institutionalized in India, and often is the *only* way to obtain a public service. In short, on the basis of historical evidence I assessed security risks to the applicants to be low. In addition, this project received approval from my institution's Human Subjects Committee.

The crucial factor in deciding to go ahead with the project was qualitative evidence furnished by civil society activists and city residents that few individuals who follow standard procedure during continuous revision of the rolls are ever registered to vote. I was confident that at the very least the experiment would have helped some slum dwellers, who could not otherwise afford to pay the bribe, to register to vote. I made an effort to ensure that everybody who participated in the experiment was eventually added to the roll. To this end, when data collection was discontinued in June 2008 we immediately filed RTI requests on behalf of all those who had not yet been registered to vote.

Section IV: Results

I now present the experimental results. Findings from the urban poor and middle class areas are shown side by side for convenience. The dependent variable is measured in two stages. The ultimate variable of interest is the length of time before an applicant is added to the draft district electoral roll. Draft electoral rolls are registers of local residents who have been recently registered to vote; they are compiled every two weeks by the ERO. This variable is calculated by subtracting the subject's application date from the date on which her name appears on the roll. In addition, I also report data on time to residence verification. Residence verification is a mandatory component in voter registration both among the urban poor and middle class applicants and takes the form of government officials visiting the applicants' homes shortly after applications are submitted. We had applicants self-report the date when the government inspectors visited their home.

a. Time to Residence Verification

Time to residence verification is a useful intermediary measure of the dependent variable because it demonstrates how the interventions fared against one another in the early stages of application processing. In addition, these data serve as a useful check confirming that all the applications were properly submitted and began to be processed. Data on time to residence verification are presented in Table 3 and Figures 1 and 2. I focus on median values as I am working with sufficiently small numbers within each treatment to render the means highly sensitive to outliers. Government officials visited all the subjects' homes. This means that all the applications were submitted successfully. The way the data are clustered within the two treatments and the control demonstrates that the variable treatment effects were at work early on in the processing of the applications. Those in the bribe treatment receive residence verification first (median of 15 days for middle classes vs. 23 days for the urban poor), and are shortly followed by the RTI (24 days vs. 38 days) and the control groups (54 days vs. 74 days). At this stage middle class applicants receive higher overall quality of public service—all the middle class groups fare better than equivalent groups among the urban poor. Importantly, those assigned to the information intervention did better than subjects in the untreated control but worse than those who bribed.

[TABLE 3 GOES HERE]

[FIGURES 1 AND 2 GO HERE]

Next I present results from a non-parametric difference of means test to demonstrate that within each experimental group the two treatments are statistically different from one another and from the control. I use the Mann-Whitney-Wilcoxon nonparametric ranking test as I cannot assume normality because the groups are so small. Mann-Whitney-Wilcoxon (WMW) ranks test is particularly useful here because it makes no distributional assumptions and is robust to smaller-N sample sizes. The null hypothesis assumes that the probability distributions of the two treatment group samples are the same; that is, H_0: *f(x) = g(x)*. Test results are reported in Table 4. These results confirm that differences between the treatments, and the treatments and the control are

statistically significant. They demonstrate that bribing results in the shortest wait for residence verification, and that those who resort to the RTI are better off than subjects in the untreated control.

[TABLE 4 GOES HERE]

b. Application Processing Time

I now present data on application processing times, which is the dependent variable of ultimate interest. Not all the subjects were placed on the electoral roll when data collection was discontinued eleven months after the first applications were filed. Specifically, among the urban poor 2 out of 22 subjects in the RTI treatment, and 17 out of 24 in the control were not yet registered to vote; the corresponding figures among middle class applicants are 1 out of 21 in the RTI group, and 9 out of 21 in the control. Non-availability of data for these subjects complicates results presentation—all we know about these 19 slum residents and 10 middle class individuals is that they were either placed on the roll some time in the future or were never registered to vote.

To get around this problem, the data are presented in two stages. First, I make the generous assumption that all those who had not yet been registered to vote on the last day when data was collected would have been placed on the roll the next day. Data presented in table 5 and figures 3 and 4 are subject to this assumption that truncates the real median processing times for RTI and control groups. The only treatment group with a 100% success rate within the timeframe of the experiment is the bribe group. Subjects randomized into the bribe intervention are all registered to vote in the shortest period (123 days for the middle class, and 140 for the urban poor). Those in the RTI group do much better than applicants in the untreated control—almost everybody subject to the information intervention is placed on the roll, unlike the majority of subjects in the control pool. Subjects in the RTI treatment are also registered to vote much faster than those in control (164 vs. 331 days among slum residents, and 150 vs. 319 days among middle class applicants). Results from the WMW difference-of-means test (Table 6) demonstrate that differences between the treatments, and the treatments and the control

are all statistically significant. I also ran an OLS regression of treatments and demographic covariates on application processing time to determine whether any of the covariates have an independent effect on the duration of application processing. These results are reported in the appendix. Among the urban poor none of the demographic covariates are statistically significant—there, treatment assignment fully accounts for variance in application processing times. However, in the middle class pool every additional year lived in Delhi decreases waiting time by 2 days, and there is weak evidence that women receive faster service than men (the waiting period for 60 middle class women is 19 days shorter).

[TABLE 5 GOES HERE]
[FIGURES 3 AND 4 GO HERE]
[TABLE 6 GOES HERE]

Duration modeling is a more precise statistical method for assessing differences between treatments in a dataset where some of the observations are censored (on duration analysis in the social sciences see Box-Steffensmeier and Jones 2004). In this instance, censoring occurred because data collection stopped before some subjects were registered to vote. I use the Weibull parametric duration model in this paper because it demands less statistical power than the more flexible nonparametric Cox model (I am operating with a small number of subjects), and it is more flexible than other parametric duration models.[5] The hazard, i.e. the probability of failure, in the Weibull model is nonconstant but monotonic. In other words, the probability of failure decreases or increases steadily over time. The Weibull hazard is expressed as $h(t) = \lambda p(\lambda t)^{p-1}$, where p is a shape parameter that determines whether the hazard increases, decreases, or remains constant overtime. Results from duration analysis presented in Table 7 confirm that subjects randomized into the bribe and RTI interventions are registered to vote much sooner than those assigned to control. Among the urban poor, those who bribed are 124 times (exp(4.822)) more likely to be registered to vote in the next moment in time by

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[5] I ran the middle class data using the Cox nonparametric model and the resultant coefficients are not substantially different from the Weibull ones that I report here. There are too few uncensored observations in the urban poor control for the Cox model to converge.

comparison to subjects in the untreated control conditional on not having already been registered to vote; the corresponding coefficient for middle class applicants is 104. By comparison, slum residents who sought recourse to the RTI are 24 times more likely, and middle class RTI applicants are 14 times more likely, to be placed on the electoral roll in the next moment in time than subjects in control conditional on not having already been placed on the electoral roll. The bribe intervention is 4.4 times more effective than the RTI treatment among the urban poor, and 5.8 times more effective in the middle class area. As expected, the shape parameter coefficient indicates that the hazard rate rises overtime, i.e. the probability of voter registration increases. An interesting nonexperimental conclusion from this data is that the information intervention is more effective among the urban poor (both in relation to control and bribing) than among middle class applicants.

[TABLE 7 GOES HERE]

Section V: Discussion

(a) Principal Findings

Experimental evidence demonstrates that recourse to the RTI substantially speeds up the voter registration process by comparison to standard application procedure. The urban poor randomly assigned to the RTI treatment are twenty-four times more likely to be placed on the roll than those in the untreated control, and the RTI intervention is fourteen times more effective than control among middle class applicants. In short, the threat of information disclosure forces public officials to operate more efficiently. Interestingly, the RTI is more effective among the urban poor than among middle class applicants.

Recourse to the RTI provides an effective free and legal alternative to bribery. At first sight those who bribe attain faster processing times than subjects assigned to the information intervention: bribery is 4.4 more effective than RTI among the urban poor, and 5.8 times more effective among middle class applicants. However, let us recall that

slum residents mailed their RTI requests 20 days after filing voter registration applications, and the time gap between application date and the mailing of requests was 37 days among middle class subjects. If RTI requests had been filed on the same day as the electoral roll applications then the median slum resident should have been registered to vote within 144 days (compared to 140 days for those bribing). The median middle class applicant in the RTI treatment should have been placed on the electoral roll within 113 days, as compared to 123 days for those bribing. In short, the information intervention is likely *just as effective* as bribing.

One of my secondary aims in this paper was to describe corrupt transactions as I encountered them in New Delhi. I observed a sophisticated and highly rigid corruption market, where public officials and middlemen collude on bribe pricing. Bribe prices are fixed and insensitive to variance in social status. These findings on corruption in provision of public services are consistent and complementary to other studies on everyday graft in India (Peisakhin and Pinto 2008, Bertrand et al. 2006).

Finally, we consider the nonexperimental differences between the experiences of the urban poor and middle class subjects. It is apparent that there is systematic discrimination against the urban poor at every stage of the application process. Government officials are rude and dismissive towards slum residents, who have to spend more than twice as much time as middle class applicants at the ERO's office. The urban poor also have to wait longer for residence verification. Differences in time to residence verification are statistically significant for all treatments across the two experiments. Middle class individuals who bribe obtain their cards statistically faster than the urban poor randomized into the bribe intervention. This finding is surprising, as I expected bribe payment to extinguish any differences between middle classes and slum residents. Similarly, middle class subjects in the untreated control do statistically better than slum dwellers in the control condition. Against this backdrop, it is therefore particularly surprising that there should be no statistical difference for processing times under the RTI across the two experiments (t=0.872). *In short, whereas a display of wealth cannot erase the gap between the urban poor and more affluent members of society, access to*

information can. This tentative finding holds much promise for anti-discriminatory policies and deserves further attention.

(b) Robustness and Generalizability

As any small-N study this field experiment faces a robustness challenge. Although randomization was successful, and the results are extremely clear by social science standards some might argue that these are accidental. I contend that the findings are unlikely to be anomalous because treatment effects are so substantial. In addition, these findings, including the effectiveness of the RTI intervention, are supported by other work on corruption in the provision of public services in Delhi (Peisakhin and Pinto 2008, Bertrand et al. 2008).

The generalizability challenge is much more difficult to overcome. Ultimately, additional empirical work is required to prove that a credible threat of information disclosure results in better quality of public service provision in settings other than New Delhi and India. It bears noting that evidence from qualitative interviews in Mumbai, Bangalore and Kolkata demonstrates that the RTI Act is effective in helping members of the public obtain higher quality of public services in places other than the capital. Based on what I know about governance structures and state-citizen relations in other developing countries my hunch is that recourse to FOI laws ought to have similar positive side effects there too.

(c) Recourse to a FOIA as an effective way to bypass corruption

This paper demonstrated that a FOI law can be used effectively to eliminate the need to pay bribes. Yet, for a FOI statute to have this effect in practice it needs to be widely used. While the number of complaints under the RTI Act has been increasing steadily since the Act's adoption in 2005, today primary users of the Act are middle classes and government officials (for details on RTI Act usage see Peisakhin and Pinto 2008). Public servants ordinarily use the Act to obtain information that assists them in professional advancement. Middle classes frequently use India's FOI statute to force the government to repair roads in affluent residential districts, and increasingly to obtain

sensitive commercial data. The poorest and neediest members of society are almost entirely left out of this picture largely because of high illiteracy levels among this segment of the population. The poor are unlikely to use the Act *en masse* until the law is revised to permit oral filing of RTI requests. It is worth noting that under pressure from Parivartan, the country's most effective civil society advocate for RTI usage among the poor, the state of Bihar recently allowed its residents to phone in RTI requests. In addition, the level of awareness about the Act's existence is pitifully low among the poor, and there is an enormous opportunity here for non-governmental organizations to raise awareness.

There are also concerns about the Act's effectiveness overtime. Will the RTI Act lose its sting as its novelty wears off? Qualitative evidence suggests that public officials are currently frightened of the Act, not least because they know so little about it. Will this fear dissipate overtime, particularly as the Act's penalty provision is rarely invoked? For instance, in the state of Karnataka, where the State Information Commission has been imposing the heaviest penalties, only 19 penalties were levied between November 2005 and June 2007 with the average penalty only at $150 (4,000 complaints and 300 appeals were filed over this period). Furthermore, as more urban poor start using the Act will public officials gradually stop paying attention to RTI requests? The RTI Act's long-term effectiveness is a topic for another research project. Evidence from state-level FOI acts, like the 2000 Rajasthan and the 2001 Delhi statutes, suggests that FOI acts do not become less effective over time if their legal design is not tampered with.

Conclusion

Results from two randomized field experiments presented in this paper demonstrate that citizens assigned to the RTI intervention do considerably better than those in the untreated control when applying for a basic public service. Free and legal recourse to the RTI Act resulted in application processing times that are comparable to the ones attained through bribery. In addition, I have shown that government officials consistently discriminate against slum residents by comparison to middle class applicants. The only exception to this rule are the urban poor who seek recourse to the

RTI—application processing times for this group were statistically indistinguishable from those among middle class subjects. The role of information in alleviating inequalities due to social, cultural, and economic differences is something that deserves to be explored further. Long-term effectiveness of the RTI Act, and the operation of FOI laws in other contexts, are also promising avenues for future research.

Appendix I

Appendix Table 1: Treatments and demographic covariates regressed on application processing time

	Urban Poor	*Middle Class*
RTI	-141.030***	-136.26***
Bribe	-182.738***	-180.631***
Gender (% Male)	6.250	18.768*
Age	0.204	0.013
Education	0.006	-0.532
Occupation[++]	-2.63	-1.380
Income	0.000	0.000
Years Lived in City	-0.234	-2.057***
Literacy Level[+]	-0.145	
Islam		-6.535
Constant	328.097***	320.681***
Number of Observations	*61*	*60*
R-squared	*0.841*	*0.883*

[+] Literacy: 0=none; 1=read only; 2=full
[++] Occupation: 1=unskilled; 2=semi-skilled; 3=skilled

Statistical significance: * $p<0.1$ ** $p<0.05$ ***$p<0.01$

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Table 1: The Urban Poor Subject Pool

	RTI	Bribe	Control		Difference of means test (t-value reported)	
				RTI vs. Bribe	RTI vs. Control	Bribe vs. Control
Number of Confederates	22	16	23			
% Male	0.82 (0.39)	0.56 (0.51)	0.61 (0.50)	1.782	1.566	-0.305
Age	24 (7.63)	27 (11.42)	29 (14.09)	-0.972	-1.471	-0.470
Literacy level[+]	0.95 (0.79)	1.25 (0.86)	0.91 (0.85)	-1.114	0.163	0.115
Years of Schooling	4.00 (3.69)	6.06 (4.06)	4.52 (4.77)	-1.630	-0.408	1.052
Occupation[++]	1.91 (0.92)	1.81 (1.17)	2.04 (0.98)	0.295	-0.458	-0.666
Annual Income, Rs.	26,636 (8,910)	22,625 (12,909)	20,913 (12,402)	1.135	1.771	0.417
Years Lived in City	14.41 (4.92)	15.56 (4.19)	16.04 (8.12)	-0.756	-0.810	-0.217
% Muslim	1	1	1	0.000	0.000	0.000

Standard deviations are reported in brackets next to the coefficients
[+] Literacy: 0=none; 1=read only; 2=full
[++] Occupation: 1=unskilled; 2=semi-skilled; 3=skilled

Table 2: The Middle Class Subject Pool

	RTI	Bribe	Control	*Difference of means test (t-value reported)*		
				RTI vs. Bribe	RTI vs. Control	Bribe vs. Control
Number of Confederates	21	18	21			
% Male	0.90 (0.30)	0.89 (0.32)	0.81 (0.40)	0.101	0.825	0.682
Age	24 (5.81)	24 (9.13)	25 (7.55)	0.000	-0.481	-0.375
Literacy level[+]	2	2	2	0.000	0.000	0.000
Years of Schooling	15.29 (1.87)	14.61 (2.50)	15.48 (1.89)	0.970	-0.328	-1.236
Occupation[++]	2.86 (0.48)	2.78 (0.55)	2.62 (0.80)	0.485	1.179	0.715
Annual Income, Rs.	71,333 (89,921)	56,222 (76,238)	57,048 (77,472)	0.561	0.552	-0.033
Years Lived in City	10.19 (4.70)	8.78 (5.09)	10.10 (5.08)	0.899	0.060	-0.808
% Muslim	0.76 (0.44)	0.72 (0.46)	0.90 (0.30)	0.277	-1.205	-1.467

Standard deviations are reported in brackets under the coefficients
[+] Literacy: 0=none; 1=read only; 2=full
[++] Occupation: 1=unskilled; 2=semi-skilled; 3=skilled

Table 3: Time To Residence Verification

	Urban Poor:				Middle Class:		
	Bribe	RTI	Control		Bribe	RTI	Control
Median Time to Residence Verification (days)	23	38	74		15	24	54
Mean Time To Residence Verification (days)	22	43	76		15	24	56
Standard deviation (days)	6	13	12		3	3	9
25th Percentile (days)	18	35	68		13	23	47
75th Percentile (days)	25	51	87		17	26	61
Number of Subjects	16	22	23		18	21	21

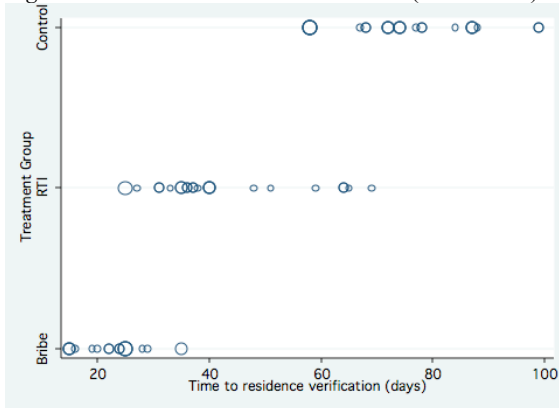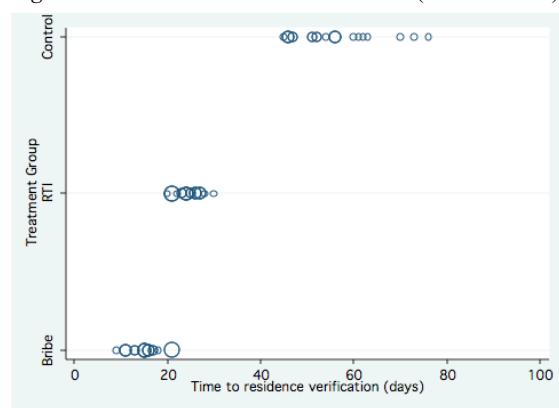*Figure 1:* Time to Residence Verification (Urban Poor) *Figure 2:* Time to Residence Verification (Middle Class)

Size of the marker represents the number of observations at any given data point

Table 4: Wilcoxon-Mann-Whitney ranks test for group differences in time to residence verification

	Urban Poor:			Middle Class:	
	Bribe	RTI		Bribe	RTI
Control as the comparison baseline	-5.263***	-5.228***		-5.333***	-5.555***
RTI as the comparison baseline	-4.871***			-5.199***	

*Z-score reported; z of 2.33 or greater statistically significant at the p = 0.01 level (denoted by ***)*

Table 5: Application Processing Times

	Urban Poor:			Middle Class:		
	Bribe	RTI	Control	Bribe	RTI	Control
Number of Subjects	16	22	23	18	21	21
Number of Subjects Registered to Vote when Data Collection Discontinued	16	20	6	18	20	12
Median Application Processing Time (days)	140	164	331	123	150	319
Mean Application Processing Time (days)	146	189	330	125	168	309
Standard deviation (days)	23	55	10	15	50	22
25th Percentile (days)	129	153	326	119	127	296
75th Percentile (days)	158	220	339	133	196	325

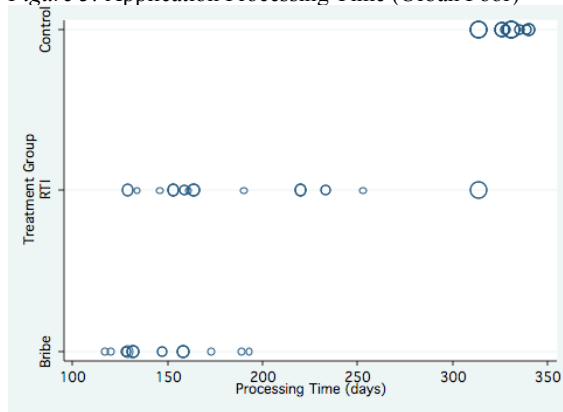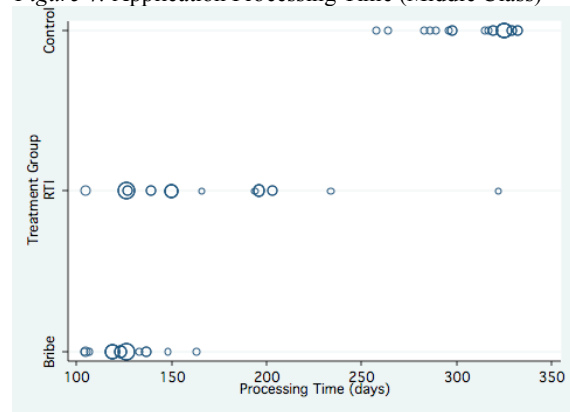*Figure 3:* Application Processing Time (Urban Poor)   *Figure 4:* Application Processing Time (Middle Class)

Size of the marker represents the number of observations at any given data point

Table 6: Wilcoxon-Mann-Whitney ranks test for group differences in application processing time

	Urban Poor:			Middle Class:	
	Bribe	RTI		Bribe	RTI
Control as the comparison baseline	-5.272***	-5.672***		-5.336***	-5.255***
RTI as the comparison baseline	-2.961***			-3.816***	

*Z-score reported; z of 2.33 or greater statistically significant at the p = 0.01 level (denoted by ***)*

Table 7: Weibull duration rate analysis for differences between treatments, and between treatments and control

	Urban Poor			Middle Class	
	Control as the comparison baseline	RTI as the Comparison Baseline		Control as the comparison baseline	RTI as the comparison Baseline
Number of observations	61	38		60	39
Number of censored observations	19	2		10	1
Bribe	4.822*** (0.610)	1.483*** (0.404)		4.693*** (0.557)	1.763*** (0.427)
RTI	3.159*** (0.486)			2.654*** (0.385)	
p/shape parameter	4.496	4.093		4.666	4.200

Standard errors are reported in brackets under the coefficients.
Statistical significance: *** p<0.01