Sunshine as Disinfectant: The Effect of State Freedom of Information Act Laws on Public Corruption

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Abstract

This paper investigates the effect of Freedom of Information Act (FOIA) laws on public corruption in the United States. Specifically, it assesses the impact of switching from a weak to a strong state-level FOIA law on corruption convictions of state and local government officials. The evidence suggests that strengthening FOIA laws has two offsetting effects: reducing corruption levels and increasing the probability that corrupt acts are detected. The conflation of these two effects led prior work to find little impact of FOIA on corruption. We find that corruption conviction rates approximately double after the switch, which suggests an increase in detection probabilities. However, corruption conviction rates decline from this new elevated level as the time since the switch from weak to strong FOIA increases. This decline is consistent with officials reducing the rate at which they commit corrupt acts by about forty percent. There is no concomitant change in the corruption convictions of federal officials in these same states.

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1 Introduction

Brett Blackledge, a reporter for *The Birmingham News*, won the 2007 Pulitzer Prize for Investigative Reporting for a series of articles that "[exposed] cronyism and corruption in the [Alabama's] two-year college system, resulting in the dismissal of the chancellor and other corrective action."[1] Central to his investigation was the collection of reams of financial records, contracts, and disclosure forms. Blackledge used this information to piece together a compelling story about state legislators and their associates receiving kickbacks and cushy jobs from various members of the school system administration. Many of the official records that he relied upon were uncovered in accordance with Alabama's public records law.

In another highly publicized case from 2007, reporters for the Detroit Free Press submitted a Freedom of Information Act (FOIA) request for documents dealing with a settlement with a police whistleblower. After much wrangling in court, the documents were eventually released. They revealed startling evidence of perjury and obstruction of justice by mayor Kwame Kilpatrick that eventually led to his resignation, prosecution, and conviction.[2]

These anecdotes, and many others like them, highlight the role that access to public documents can play in helping a free press check the abuse of power by public officials.[3] One of the most important changes in the relationship between public officials and the press in recent years has been the widespread adoption of FOIA laws at multiple levels of government. These laws provide clear guarantees regarding the rights of individuals and organizations to access information about government activities, and they make it easier for members of the press and members of the public at large to hold those in power accountable for their actions.

Most of the literature investigating governmental transparency and corruption has

[1] Pulitzer Citation and copies of Blackledge's prize-winning stories available at http://www.pulitzer.org/citation/2007,Investigative+Reporting.

[2] "Free Press Pushed for Freedom of Information," Detroit Free Press, September 5, 2008. http://www.freep.com/apps/pbcs.dll/article?AID=/20080905/NEWS01/809050340/1007/NEWS05

[3] In addition to the anecdotal evidence, there is a growing body of literature that addresses the role of the media in promoting government accountability. Some recent examples include Djankov et al. (2003), who find that state ownership of the media is associated with a number of undesirable characteristics (less press freedom, fewer political rights, inferior governance, underdeveloped capital markets, inferior health outcomes, etc.), Besley and Prat (2006), who develop a model that predicts that media capture by the government increases the likelihood that elected politicians engage in corruption and/or rent extraction and reduces the likelihood that bad politicians are identified and replaced, and Snyder and Strömberg (2010), who find that more active media coverage of U.S. House representatives leads to better informed voters, which increases monitoring, makes the representatives work harder, and results in better policies from the constituents' perspective.

lauded transparency (see, e.g., Klitgaard (1988), Rose-Ackerman (1999), Brunetti and Weder (2003), Peisakhin and Pinto (2010), Peisakhin (forthcoming)). Indeed, the literature suggests that gathering and analyzing information is one of the main weapons used to combat corruption. For example, Klitgaard (1988) discusses several information-gathering practices that are designed to thwart corruption, such as agents tasked with spot checking customs activities in Singapore, investigations of government officials for having "unexplained assets" in Hong Kong, and intelligence officers inspecting the lifestyles and bank accounts of officials in the Philippines. Such practices suggest that government officials recognize that information is a valuable resource in the fight against corruption.

Nonetheless, governmental transparency need not be beneficial. Bac (2001), for instance, contends that transparency can have a perverse effect on corruption. Specifically, he argues that while more transparency tends to decrease corruption, it may also provide better information to outsiders about whom to bribe. If the incentive to establish and exploit political connections for corrupt purposes is greater than the disincentive that results from the higher probability that corruption will be detected, then more transparency might actually increase corruption.

Prat (2005) also argues that complete transparency is not always desirable. He considers a principal-agent setting in which the principal can have two types of information about the agent: information about the consequences of the agent's action and information directly about the action itself. In his model, the former is always beneficial, while the latter can have detrimental effects. If the latter type of information is available, then the agent has an incentive to ignore useful private signals. This result may explain why most countries that adopt FOIA laws place restrictions on information disclosure during the pre-decision process, but make information freely available after decisions are implemented.

Although the weight of the empirical evidence clearly favors the view that increased transparency is beneficial, the evidence with respect to FOIA laws is rather limited. There have been a few recent studies of the impact of these laws on *perceptions* of corruption in cross-country settings. Islam (2006) constructs indices that measure (i) the frequency with which governments update publicly available economic data and (ii) the presence of FOIA laws and the length of time the laws have been in existence. She finds a negative correlation between these indices and her measures of perceived corruption. In contrast, Costa (2009) finds that the adoption of FOIA laws increases the perceived corruption level, particularly in the first five years after enactment of the legislation, perhaps because greater transparency increases citizens' awareness of government's actions. Escaleras et al. (2009) find no evidence of a significant relation between the existence of FOIA laws and perceived corruption levels for developed countries, but find a positive and significant correlation between FOIA laws and perceived corruption in developing countries. The authors attribute this latter finding to the fact that developing countries have relatively weak institutions that make FOIA laws less effective.

To our knowledge, our study is the first to examine the impact of state-level FOIA laws on the prevalence of public corruption among state and local government officials using U.S. data. We see three important advantages to undertaking such a study. First, parameter heterogeneity should be reduced given that the variation in the legal, social, cultural, and political institutions is much lower across states than across countries. Second, the data are objective. We can examine the number of state and local public officials actually convicted for corrupt acts rather than rely on the type of subjective survey-based data used in the cross-country studies. Hence, our results should provide an objective basis for assessing whether FOIA laws provide an effective tool for those who seek to expose and punish corruption. Finally, there is a set of identifiable public officials, federal employees, who should not be affected by state FOIA laws. This facilitates a straightforward falsification test.

We measure corruption using annual state-level data for 1986-2009 reported by TRACfed, which compiles information on corruption convictions from the Department of Justice Public Integrity Section. The TRACfed database lists criminal convictions in Federal District Courts of federal, state, and local public employees for official misconduct or misuse of office. We expect the number of corruption convictions of state and local officials, only, to respond to changes in state FOIA laws, and thus it is important to have separate measures of convictions at the state, local, and federal levels. TRACfed is the only database that reports the disaggregated convictions data.

Information on the provisions of state FOIA laws is obtained from the Open Government Guide. We construct measures of the strength of state FOIA laws by analyzing the open records statutes, case law, and Attorney General's opinions for each state. Our goal is to assess the effectiveness of these laws in promoting an open government and providing citizens with access to public records.

We expect states that create a presumption for disclosure, place limits on fees, and impose deadlines for responding to FOIA requests to have more open and transparent government. This should make it more difficult for corrupt public officials to escape public scrutiny. All states have some sort of law governing the public's access to records held by state and local officials, but the details of the statutory provisions of FOIA laws vary widely across states and over time. We classify states in two categories: those that provide strong access to public records (strong FOIA states) and those that provide weak access (weak FOIA states). Between 1986 and 2009, 12 states switched from weak to strong FOIA. When policy changes, there are substantial changes in corruption conviction rates for state and local public officials, but no obvious change in the conviction rates for federal officials. State FOIA laws affect either conviction or corruption rates of state and local officials.

Encouraged by this finding, we propose a simple reduced-form model to help disentangle changes in conviction rates from changes in corruption rates. The modeling exercise is important because a naïve analysis might simply attribute all changes in conviction rates to changes in the level of corruption, leading to the implausible conclusion that strengthening FOIA laws actually increases corruption. Under our model, strengthening FOIA laws has two effects: reducing corruption levels and increasing the probability that the corrupt acts are detected. By making plausible assumptions about the process by which corrupt acts are committed, uncovered and prosecuted, and otherwise exit the system (e.g., statutes of limitation, death of corrupt officials, etc.), we can partially separate the two effects.

Guided by our model, we investigate the impact of switching from weak to strong FOIA on corruption convictions of state and local officials. This specification controls for known determinants of corruption rates and includes a complete set of state and year dummy variables. Corruption conviction rates rise substantially after the switch, approximately doubling in most specifications, which suggests a significant increase in detection probabilities. However, corruption conviction rates decline by about forty percent from this new elevated level as the time since the switch from weak to strong FOIA increases, which suggests a substantial reduction in the underlying corruption level in response to strong FOIA enaction.

The remainder of the paper is organized as follows. In section 2 we develop a simple reduced-form model of policy, corruption and conviction. In section 3 we describe the data used in our analysis and our empirical strategy for identifying the impact of state-level FOIA laws on corruption. In section 4 we present the results of the empirical analysis and of several robustness exercises. In section 5 we interpret the results and offer a few concluding remarks.

2 Reduced-Form Model of Policy, Corruption and Conviction

We begin our analysis of the relation between state-level FOIA laws and corruption convictions by presenting a model that illustrates the nature of the empirical challenge. The model includes only the bare minimum features necessary to understand the corruption and conviction process and how FOIA laws might affect each. Thus we do not explicitly model public employees' corruption decisions. Instead, we develop a reduced-form specification that allows for the possibility that public employees alter their behavior in response to a change in FOIA policy but remains agnostic about the exact mechanism by which this response occurs.

2.1 The Model

Assume that in state s and year t under policy regime $j \in \{FOIA, NOFOIA\}$ there is a stock of corrupt acts that could potentially be prosecuted, $P_{s,t}$ (measured on a per-potential-offender basis). In a given policy regime, a fraction γ_j plus some random noise $\epsilon^C_{s,t,j}$ of these acts are successfully prosecuted and convicted, so total convictions (per-capita) is given by

$$C_{s,t,j} = \gamma_j P_{s,t} + \epsilon^C_{s,t,j}. \tag{1}$$

Finally, assume that in each period a fraction $(1 - \alpha)$ of the extant stock of corrupt acts degrade out of existence (maybe the criminal dies, or the crime passes the statute of limitations), but some additional corrupt acts are committed, which are made up of a policy-dependent constant $N_{s,j}$ plus noise $\epsilon^P_{s,t,j}$.

Under these assumptions, the stock transition is governed by the following equation

$$P_{s,t+1} = \alpha(P_{s,t} - C_{s,t,j}) + N_{s,j} + \epsilon^P_{s,t,j}. \tag{2}$$

If we replace appropriately to express everything in terms of observable convictions, we are left with

$$C_{s,t,j} = \alpha(1 - \gamma_j)C_{s,t-1,j} + \gamma_j N_{s,j} - \alpha\epsilon^C_{s,t-1,j} + \gamma\epsilon^P_{s,t,j} + \epsilon^C_{s,t,j}. \tag{3}$$

We are interested in estimating the average $N_{s,FOIA}/N_{s,NoFOIA}$, which measures the percent change in the arrival rate of new corrupt acts when a FOIA provision is adopted in the state, and $\gamma_{FOIA}/\gamma_{NoFOIA}$, which measures the percent change in the probability of getting caught when a strong FOIA law is enacted. These quantities cannot be directly observed, but with some further assumptions, we can identify them in our data.

We can express the steady-state rate of observed convictions in this model as

$$\overline{C}_{s,j} = \frac{\gamma_j N_{s,j}}{1 - \alpha(1 - \gamma_j)}. \tag{4}$$

This steady-state rate is easy to estimate for any given state and policy regime. The average level of convictions is a consistent estimator of the steady state. The relation between the steady-state rate of convictions and the rate of corrupt acts depends on the probability of conviction, γ_j, which will likely be affected by changing the policy regime.

2.2 Corruption versus Conviction

If we are willing to assume that the conviction rate changes quickly, while the corruption rate changes only with a lag, we can disentangle the conviction effect from the corruption effect. To see this, define three response periods: $r \in \{Pre, Short, Long\}$ and assume that $N_{s,Pre} = N_{s,Short} = N_{s,NoFOIA}$ and $N_{s,Long} = N_{s,FOIA}$, while $\gamma_{Pre} = \gamma_{NoFOIA}$ and $\gamma_{Short} = \gamma_{Long} = \gamma_{FOIA}$. With these assumptions, the following equalities hold in steady state:

$$\frac{N_{s,FOIA}}{N_{s,NoFOIA}} = \frac{\overline{C}_{s,Long}}{\overline{C}_{s,Short}} \tag{5}$$

$$\frac{\gamma_{FOIA}}{\gamma_{NoFOIA}} \approx \left[\frac{\gamma_{FOIA}}{\gamma_{NoFOIA}}\right]\left[\frac{1 - \alpha(1 - \gamma_{NoFOIA})}{1 - \alpha(1 - \gamma_{FOIA})}\right] = \frac{\overline{C}_{s,Short}}{\overline{C}_{s,Pre}} \tag{6}$$

We can identify the ratio of the parameters of interest by contrasting conviction rates in the three periods. Our estimate of the change in the probability of corrupt acts being convicted will be slightly biased downward, because a low conviction rate leads to a higher steady-state stock of corrupt acts, but this bias will be small for either small values of the conviction probability (γ) or small values of degradation probability (α). In practice, both probabilities are likely to be small. Accordingly, we expect the bias, which is a function of their product, to be *very* small.

To apply this model to data, we need suitable measures of corruption and FOIA strength. We also need to define a short-run and a long-run time horizon. Instead of defining them ex-ante, we will try to let the data inform our choices.

3 Data Description and Some Suggestive Patterns

3.1 The Data

Corruption Data

We obtain the corruption data from the TRACfed database maintained by the Transactional Records Access Clearinghouse (TRAC), a nonpartisan data gathering, research, and data distribution organization associated with Syracuse University.[4] This database lists criminal convictions in Federal District Courts of federal, state, and local public employees for official misconduct or misuse of office. These data are collected and reported annually by the Executive Office of U.S. Attorneys of the U.S. Justice Department. Each U.S. Attorney's office maintains detailed information on the workload of its employees and certifies the accuracy of the data each year. Our sample covers the years 1986 to 2009 for the 50 states. We report the summary statistics in Table 1. The statistics are reported for the full set of states and for the subset of states that switch from a weak to a strong FOIA law according to the definition developed below.

Corruption is measured by the number of state and local public officials convicted for corrupt acts per 10,000 full-time equivalent state and local government employees. These officials include governors, legislators, department or agency heads, court officials, law enforcement officials, mayors, city council members, city managers, and their staff. Corrupt acts include bribery of a witness, embezzlement or theft of government property, misuse of public funds, extortion, influencing or injuring an officer or a juror, and obstruction of criminal investigations.[5] Because we examine FOIA laws by state, it is important to have a breakdown of convictions by the level of government. State FOIA laws should not affect convictions of federal public officials, so we use the number of corruption convictions of federal employees for a falsification test.

If we consider only state and local convictions per government employee, then the most corrupt states for the years 1986-2009 are Montana, Mississippi, North Dakota, and New Jersey, and the least corrupt states are Iowa, Utah, Colorado, South Dakota,

[4]Appropriately enough for this paper, much of the TRACfed data results from vigorous use of federal FOIA law.

[5]These include convictions under the Hobbs Act for bribery that "obstructs, delays or affects interstate commerce "(the most common charge, about a quarter of all convictions), 18 U.S.C. 666 convictions for theft and bribery of officials in organizations receiving federal funds (second most common charge), as well as various convictions for mail-fraud and election-related offenses.

and New Hampshire. Our data on corruption convictions differs from the corruption convictions data provided by the Public Integrity section (PIN) of the Department of Justice. The PIN data, which the literature has used extensively to measure corruption in the U.S. (see, e.g., Glaeser and Saks (2006), Leeson and Sobel (2008), Cordis (2009)), do not differentiate between convictions of federal, state, and local employees. The correlation between the convictions (federal, state, local, and other) reported by the TRACfed and the convictions reported by the PIN is 0.75 for our sample period.

FOIA Laws Data

We obtain data on FOIA laws from the Open Government Guide, published by The Reporters Committee for Freedom of the Press, a comprehensive source of information about open government law and practice in each of the 50 states.[6] The guide, which is prepared by volunteer attorneys who are experts in open government laws in their respective states, contains information on state statutes, case law, and Attorneys General's opinions. The first edition of the guide was published in 1989.

Statutory provisions designed to provide citizens access to public records can be traced back to the early 1900s, and common law access provisions go back even further. Progress on guaranteeing access to information, however, was relatively limited until the 1970s. In the last 40 years, most states enacted new open records statutes, amended existing statutes, or substantially rewrote their statutes in an effort to strengthen the laws, often to clarify or broaden their scope in response to changing technology, judicial decisions or Attorneys General's opinions.

Arkansas, for example, enacted its FOIA law in 1967. Prior to this time the Arkansas code did little to provide for the inspection of public records. The FOIA law was passed as a result of a number of factors, including support from journalists, the results of a study by the Arkansas Legislative Council that looked at the laws of other states, and litigation by the state Republican Party that culminated in a state Supreme Court decision indicating a willingness on the part of the court to recognize an extensive right to access public records.[7] The law has been amended several times since its enactment. The amendments address judicial decisions or issues not anticipated by the law when it was initially passed. For instance, it was amended in 2001 to address access to records stored in electronic form.

Like Arkansas, Iowa also had few statutory provisions to guarantee access to infor-

[6]Available at http://www.rcfp.org/ogg/. Last accessed November 14, 2010.

[7]See *Republican Party of Arkansas v. State ex rel. Hall*, 240 Ark. 545, 400 S.W.2d 660, 1966.

mation prior to 1967. The first public records case considered by the Iowa Supreme Court, *Linder v. Eckard*, involved access to appraisal reports. The court ultimately held that appraisal reports were not public records. The unfavorable reaction to this decision from the public led the Iowa General Assembly to pass a bill to "protect the right of citizens to examine public records and make copies thereof" (chapter 68A of the Iowa Code). The law has been amended several times in the years since its passage.[8]

In Delaware, the General Assembly enacted a FOIA law in 1977 to "further the accountability of government to the citizens of this State." The law has been amended a number of times to address issues related to judicial decisions and to remedy other shortcomings. For example, it was amended in 1982 to delete a grants-in-aid exclusion, in 1985 to limit the grounds for conducting executive sessions and to improve the procedures for providing notice of these sessions, and in 1987 to permit courts to award attorneys' fees and costs to a successful plaintiff or defendant.

New Mexico, which has recognized a common law right of access to some public records since the 1920s, enacted its FOIA law in 1947. It has been amended several times. The most notable changes occurred in 1993, when the legislature added provisions that substantially strengthened the law. These provisions broadened the definition of public records, created a presumption that all records are public, and affirmed that public employees have a duty to provide access to public records. The 1993 amendments were largely the result of a campaign for greater access to public records by the New Mexico Foundation for Open Government.

As might be anticipated from these examples, there is substantial variation in statutory provisions across states, particularly with respect to the records that are subject to disclosure and the disclosure procedures. We analyze the open records statutes, case law, and Attorney General's opinions for each state to assess their effectiveness in promoting an open government and providing citizens with access to public records. Our analysis consists of a detailed examination of procedural requirements for obtaining public records, such as the presumption for disclosure and exemptions, fee provisions, agencies' response times to a request, administrative appeal provisions, and penalties imposed for violation of the statutes.

We determine each state's score with respect to freedom of information by giving one point for each of the following criteria: (1) a provision that creates a presumption

[8] For more details see "Iowa's Freedom Of Information Act; Everything You've Always Wanted To Know About Public Records But Were Afraid to Ask," *Iowa L. Rev.*, vol. 57, 1972.

in favor of disclosure and identifies specific records as exempt from public access; (2) the lack of a generic public-interest exemption provision; (3) a provision that limits the fees charged for processing FOIA requests; (4) a provision that prohibits charging a fee for the time required to collect records; (5) a provision for waiver of the cost of search for or duplication of public records if the agency determines that disclosure is in the interest of the public; (6) a provision for criminal penalties for an agency's noncompliance with its disclosure obligations; (7) a provision for civil penalties for an agency's noncompliance with its disclosure obligations; (8) a provision for the award of attorneys' fees and costs to a successful plaintiff in a public records case; (9) and a provision for administrative appeal of an agency's decision to deny a request for public records. In addition, we give one point for each of the following that is satisfied: time to respond to a request for access to public records is 30 days or less, time to respond is 15 days or less, and time to respond is 7 days or less. The total points for the states range from 1 to 11.[9] We then classify states into "strong FOIA" states (a score above 6) and "weak FOIA" states (a score between 0 and 6).[10] With this classification scheme, the number of states in each category is roughly equal. Many states transition from weak to strong FOIA laws during the sample period, and the transitions are only in one direction (none of the states switches to strong FOIA and back).

Consider, for example, the state of Pennsylvania. The state first enacted an open records act (known as the "Right to Know" Act) in 1957. The act was revised substantially in 2002, and then revised again in 2008. The 2002 version of the act provides that agencies may charge fees for access to public records (postage, duplication, etc.), but it places limits on these fees (actual mailing costs, duplication costs comparable to those that would be incurred for similar duplication services provided by local businesses, etc.). Agencies are prohibited from charging a fee for reviewing records to determine whether they are subject to access under the act, and an agency may waive the duplication fees if it considers that doing so is in the public interest. A willful violation of the act can result in civil penalties. The act does not provide explicitly

[9]We show how the FOIA score for each state evolves over time in Table A1 in the Appendix.

[10]The "strong" versus "weak" designation is somewhat arbitrary. However, our results are fairly robust to changes in the cutoff required to qualify as a "strong FOIA" state. Lowering the cutoff slightly has no significant effect on the magnitude of the estimated coefficients, but the estimates are less precise than with the original cutoff. Raising the cutoff slightly results in a number of states (WA, KY, NH, WV) that transition from weak FOIA to strong FOIA and back. With this pattern of transitions it is no longer possible to implement our timing strategy for separating the conviction and corruption effects.

for criminal liability. Denial of access to records is subject to administrative appeal, and attorney fees and costs may be awarded to a plaintiff who successfully challenges a denial. There is no specific exemption from disclosure because it is in the public interest. An agency has 10 days from the receipt of a written request to respond. In 2008, the act was revised to define public records more broadly, create a presumption in favor of disclosure, put the burden of showing that records are not public records on the agency holding them, reduce the time to respond to a request to five business days, and increase the civil penalties for noncompliance.

In light of these provisions, Pennsylvania is awarded one point for item (2) for the years 1986-2009, one point for items (3), (4), (5), (7), (8) and (9) for the years 2003-2009, two points for the time to respond to a request for the years 2003-2008, and three points for the time to respond to a request for the year 2009. One additional point is awarded for item (1) for the year 2009. Thus the total score for Pennsylvania is one for 1986-2002, nine for 2003-2008, and 11 for 2009. It is therefore classified as a "weak FOIA" state for the 1986-2002 period and as a "strong FOIA" state for the 2003-2009 period.[11]

By our metric, 12 states switched from weak to strong FOIA during our sample period: New Hampshire(1987), South Carolina(1988), Idaho(1991), Utah(1993), Washington(1993), West Virginia(1993), New Mexico(1994), Texas(1996), North Dakota(1998), Nebraska(2001), New Jersey(2002), and Pennsylvania(2003). Based on average scores, Connecticut, Indiana, Louisiana, Colorado, and Vermont are among the states with relatively stronger access laws, while South Dakota, Alabama, Arizona, Wyoming, and Nevada are among the states with relatively weaker access laws. Our measure of

[11]South Carolina provides an example of a state that switches categories following a less dramatic change in the FOIA law. The state first adopted a FOIA law in 1974. The law was revised in 1987 to allow governmental bodies to create their own exemptions from the open records requirements. The law does not contain a specific exemption from disclosure because it is in the public interest, nor does it contain a provision for administrative appeal from denial of access to public records. With respect to the fees charged for processing a request, an agency may collect fees for access to public records, but the fees should not exceed the actual cost of searching for and copying records. In addition, the law provides for a reduction in the cost of search for public records if the information benefits the general public. A willful violation of the law is a misdemeanor and subject to escalating fines and possible imprisonment for repeat offenses, and a plaintiff who successfully challenges an agency's denial to access can be awarded reasonable attorney fees and other costs of litigation. An agency has 15 days from the receipt of a written request to notify the requester of the agency's determination and the reasons for its position. If the agency fails to respond within this time frame, the request must be considered approved. In light of these provisions, South Carolina is awarded one point for item (1) for the years 1988-2009, one point for items (2), (5), (6) and (8), and two points for the time to respond to a request for all years in our sample. Thus it is classified as a "weak FOIA" state for the 1986-1987 period, with a score of six, and as a "strong FOIA" state for the 1988-2009 period, with a score of seven.

the strength of FOIA laws is positively correlated with measures that have appeared elsewhere. For example, several surveys conducted by the Better Government Association (BGA) and the Investigative Reporters and Editors, Inc. in 2002, and by the BGA and the National Freedom of Information Coalition in 2007, rank the U.S. states and the District of Columbia based on the strength of their FOIA laws. The correlation between our FOIA score variable and the scores provided by these surveys is 0.76 for 2002 and 0.73 for 2007. The Spearman rank correlation coefficient is 0.68 for 2002 and 0.64 for 2007.

Our analysis is based on the *de jure* provisions of the FOIA statutes (updated for case law and Attorneys General's opinions), including provisions for external enforcement mechanisms that could potentially work to keep reluctant officials in line. There can be substantial differences between the formal requirements of the law and the responsiveness of public officials in practice.[12] Nonetheless, stronger formal rules should be associated with better practical access to public records, especially in a country such as the U.S. that has a well-functioning legal system. In a 2002 survey of 191 investigative journalists across the U.S., the BGA found that the journalists ratings of their satisfaction with the FOIA laws in the state in which they practice were consistent with the BGA's ranking based on the formal provisions of the laws (Davis 2002).

3.2 Corruption and FOIA Enaction

Consistent with the weak and mixed international evidence, a casual investigation of the relation between state FOIA laws and public corruption does not reveal any strong patterns. This is illustrated in Figure 1, which plots the average FOIA score in the state over the 1986-2009 period versus the average rate of corruption convictions of state and local officials and federal officials, respectively. There is a weak negative correlation in the cross section, and it is actually slightly stronger for federal convictions than for state and local convictions.

There are two things to take away from this preliminary look at the data. First, documents subject to state FOIA laws are mainly those relating to the business of state and local officials. If strengthening state FOIA laws had any effect on corruption,

[12]See, for example, "N.C. open records requests can drag on," News & Observer, March 13, 2011, which discusses the failure of public officials to respond to requests for records in a timely manner. Available at http://www.newsobserver.com/2011/03/13/v-print/1049832/nc-open-records-requests-can-drag.html.

we should observe this effect mainly on these officials. Because state FOIA laws should not affect federal convictions, the causality for the correlation with federal convictions must flow from corruption to strong FOIA adoption or derive from some omitted factor that is correlated with both variables. Figure 1 provides some evidence, albeit weak, to suggest that states that are otherwise less corrupt are more likely to adopt stronger FOIA laws. Hence, we need to control for other factors that affect the underlying propensity for corruption when analyzing the impact of these laws.

Second, the lack of a clear pattern in the cross section for state and local convictions should not be surprising given the predictions of our reduced-form model. Suppose that strengthening FOIA laws both reduces corruption levels and increases the probability that corrupt acts are detected. The effects of these two changes on corruption conviction rates might largely offset one another in the long run. If this is the case, then we should be looking primarily for transitory changes in conviction rates around the time that FOIA laws are strengthened. It would be difficult to identify such changes using the average conviction rates plotted in Figure 1. However, if switching from a weak to a strong FOIA law produces a transitory increase in state and local conviction rates, this could explain why the negative correlation that we see for federal conviction rates is not apparent for state and local conviction rates.

To detect the transitory changes in conviction rates associated with strengthening FOIA laws, we align the data in event rather than calendar time. Figure 2 plots the conviction rates of state and local officials and federal officials, respectively, as a function of the number of years since strong FOIA was enacted. The diagram includes only the states that transitioned to a strong FOIA law during our sample period. The mix of states changes as they enter or leave our sample period, with each state appearing in exactly half the years. For example, South Carolina enacted a strong FOIA law in 1988, the third year of our sample. It is therefore included in the calculations from $Year = -2$ to $Year = 21$.

The two panels in Figure 2 suggest a change in state and local convictions around the time when stronger FOIA provisions were enacted, and whatever drives this change has no apparent effect on federal convictions. We would expect any effect of state FOIA provisions on federal officials to be very indirect. Some evidence of misdeeds might be apparent in documents subject to state FOIA laws, but compared to the state and local officials, this would be a relatively small risk. Given this difference, any large and distinct changes in the conviction rates of federal officials would be worrisome. It would imply that something else was changing alongside FOIA that affected corruption more generally. The contrast between the two graphs is certainly suggestive of a FOIA effect, but some care needs to be taken before we draw any solid conclusions about these differences.

First, as noted above, the mix of states changes as we move through the timeline of the two graphs. In particular, New Jersey drops out of the sample near the point where the state and local conviction rate falls, and Indiana joins the sample near the point where the conviction rate jumps upward. To prevent these mix effects from driving our results (as well as for general robustness considerations), all of our regression specifications include a complete set of state dummy variables. Second, there is a relatively consistent downward trend in both types of convictions. Since strong FOIA tends to persist once enacted (in fact, no states repeal strong FOIA in our sample), we control for the variation in corruption convictions over time, to avoid attributing this general decline to FOIA, by including a complete set of year dummies in all of our regression specifications.

Third, the literature has identified a number of underlying characteristics that are known to impact corruption, such as education and income. Since these factors vary within states over time, we need to be careful about their potential correlation with the enaction of strong FOIA. If, for example, states become more likely to strengthen their FOIA laws as their population becomes more educated, we might incorrectly attribute a change in corruption rates to a change in FOIA law, while in reality both are due to a change in education. To control for this possibility, our regression specifications include measures of education and income in each state in each year. Fourth, there will be some relation between law enforcement activity and the probability that corruption is detected and prosecuted. Accordingly, we control for variation in the effectiveness of law enforcement across states by including state judicial and legal expenditures per capita in the regressions. This variable measures the amount spent by each state for criminal and civil courts, appellate courts, prosecuting and district attorneys, legal departments, general counsels, and so on.

Fifth, to mitigate concerns about omitted variable bias, we also include several political variables in the regressions: a dummy for divided government that takes the value of one if a party other than the governor's party controls at least one chamber of the legislature, the number of years spent in unified (non-divided) government, and a dummy that takes the value of one for years in which unified control was lost, i.e., years in which the state switched from having a unified government to having a divided government, or from having a unified government under one political party to having a unified government under the other political party. These controls are especially salient, since evidence suggests that partisan bias can affect exactly these sorts of prosecutions (Gordon 2009).

Finally, and most importantly, we observe only corruption convictions, not the actual number of corrupt acts. If, as we would expect, the enaction of strong FOIA both decreases the number of corrupt acts committed and increases the probability that any given corrupt act is discovered and prosecuted, the overall effect on the number of convictions is theoretically ambiguous. Assuming that the behavioral response of corrupt agents is fixed in the short run, we can interpret the short-run effect of FOIA on corruption convictions as the pure conviction effect, and the long-run effect as the combination of the conviction effect and the response by agents. This response includes reducing the number of corrupt acts, but also includes avoidance behavior, as corrupt agents find alternative methods of avoiding detection through FOIA. Unfortunately, it is not possible to disentangle these two responses, even in the long run.

4 The Effects of FOIA

Encouraged by the movement in state and local conviction rates around the time of strong FOIA adoption, and reassured by the absence of a corresponding movement in federal conviction rates, we now turn to a more robust analysis of the effects of FOIA. The key is to deal appropriately with the confounders outlined above. Our first, and primary, analysis consists of fitting an OLS regression specification with state and year dummies. This specification allows us to adjust our estimates to account for any time-invariant differences across states and any state-invariant differences across time that would otherwise lead to biased estimates. We find a large positive impact of strong FOIA adoption on conviction rates and a significant decline in these conviction rates over time. These findings are qualitatively consistent with the predictions of our reduced-form model and suggest the importance of investigating the timing of the effects of FOIA.

In section 4.2, based on our reduced-form model, we use the timing of the FOIA effects to try to disentangle the effect of FOIA on corruption from the effect on conviction. Assuming that conviction rates adjust more quickly than corruption rates, we find that conviction rates increase sharply soon after the switch to strong FOIA, while corruption rates fall by about forty percent in the longer term. In section 4.3

we consider the issue of endogeneity. Finally, in section 4.4 we discuss a number of alternative specifications for estimating the conviction and corruption effects and how robust the estimates are to these alternatives.

4.1 OLS Regressions

Moving beyond the simple analysis of mean conviction rates presented above, our primary method for identifying the relationship between strong FOIA laws and corruption convictions is the OLS regression,

$$ConvicRate_{st} = \mathbf{y}_{st}'\beta + \mathbf{x}_{st}'\lambda + \delta_t + \gamma_s + \epsilon_{st}, \tag{7}$$

where *ConvicRate* measures the number of corruption convictions per 10,000 government employees, $\mathbf{y}$ is a vector of dummy variables that delineates time windows in the pre- and post-enactment periods for the strong FOIA laws, and $\mathbf{x}$ contains our controls: state income per capita, state-level educational attainment, state judicial and legal expenditures per capita, and the three political variables. The γ_s and δ_t denote coefficients for the state and year dummies. All of our regressions are weighted by the government employees in that state/year. This weighting allows us to interpret our estimates as the effect on the average state and local government employee. It also produces more efficient estimates if the error term is heteroscedastic with smaller states having higher variance. We consider two cases, one in which we contrast the pre- and post-enaction estimates of the expected conviction rates, and one in which we break the pre- and post-enaction timelines into 3-year windows and allow the estimates of expected conviction rates to differ by window.

In the 3-year-window specification, we exclude the window consisting of 2 to 4 years before strong FOIA enaction. This time interval serves as baseline for comparison. We also assume that the "enaction period" extends for three years. We do this for two reasons. First, there may be some pre-response before strong FOIA is officially enacted, if the enaction is foreseen, so the year immediately before enaction may not be "clean" of FOIA effects. One year should represent a reasonable time limit before which the enaction of FOIA would be unanticipated (or anticipated with enough uncertainty to ignore in practice). Second, implementation of a strong FOIA law is not instantaneous because the administration and courts must hash out exactly how the rules will be applied. Since this transition period may vary by state, we want to extend the enaction period to allow for all states to fully transition, grouping the potentially muddled years around enaction together. One year after the year of enaction should be a reasonable time frame to capture most of these transitions.

Table 2 presents the results of the regression analysis for four variants of the specification in (7). The first two columns contrast the conviction rates before and after the enaction of a strong FOIA law, for state and local officials and federal officials, respectively. For state and local officials, the conviction rates are significantly higher in years with a strong FOIA law.[13] The difference is about .069 convictions per 10,000 government employees per year, about half the mean level of convictions across all states. For federal officials, there is no significant difference in conviction rates between years with strong FOIA laws and the years without, and the point estimate is very small. These initial results bolster the earlier evidence from our analysis of mean conviction rates. Introducing state and year dummies and incorporating the controls does not alter the general nature of our findings.

The third and fourth columns of Table 2 illustrate how the conviction rates change over time. Column 3 presents the results for state and local officials. There is a reasonably consistent pattern in the years preceding the enaction of strong FOIA. The estimated coefficients for these three windows are statistically indistinguishable from the years just before enaction. In the enaction years, conviction rates jump by about .08 and continue to grow slightly, to about .10 in the 2 to 7 years after enaction. The mean is .12 convictions per 10,000 public employees, so this change is both statistically and economically significant. Beyond 7 years, the conviction rates fall back to a level indistinguishable from the baseline. Figure 3 illustrates the timing of these changes.

Column 4 presents the results for federal officials. There is no consistent pattern in conviction rates for these officials. The conviction rates are moderately higher in the enaction period, which could be indicative of reverse-causality, i.e., a rash of convictions spurs enactment of a strong FOIA law. But the estimate is not statistically significant (the t-statistic is about 1). There are more federal convictions 8 to 10 years before strong FOIA is enacted, and the estimate is significant at the 10 percent level. We have no reason to expect an elevated level of federal convictions for this period. With eight time windows, however, it would not be unusual to find one result that is significant at the 10-percent level by chance.

The results from the regressions that use the 3-year windows are suggestive of a

[13] Standard errors in Table 2 and all subsequent tables are clustered by state. Panel corrected standard errors are generally smaller than those reported in the tables.

spike in corruption convictions around the time of strong FOIA enaction followed by a decline in the conviction rates over subsequent years. With a total of nine separate windows, however, the coefficients are not estimated with sufficient precision to firmly conclude that this is the case. In the next subsection, we consider an alternative approach for defining the windows of interest that is motivated by our reduced-form model of corruption and conviction.

4.2 Separating Conviction from Corruption

To separate the effect of FOIA on conviction rates from the effect on corruption rates, we need to relate the results from the OLS regression analysis back to the structure of the model in section 2. To do so, we need to define the short run and the long run. We have no *a priori* basis on which to make this judgment, because the rate at which potentially corrupt officials alter their behavior is unknown. Fortunately, the estimates in Table 2 seem to fall nicely into three groups. This pattern suggests dividing the time around the enaction of strong FOIA into four distinct periods: a pre-period up to one year before strong FOIA is enacted; an enaction period including the year of enaction and the years before and after enaction; a short-run period from 2 to 7 years after strong FOIA was enacted; and a long-run period 8 or more years after strong FOIA was enacted.

Table 3 repeats the analysis from Table 2 using the dummies that correspond to these periods. Because the comparisons in Equations (5) and (6) include the conviction rate in the short run, this time window will form the base group for the analysis in this section. The first column of Table 3 identifies the differences between the corruption conviction rates in various time periods around FOIA adoption. To use the results of this specification to separate conviction from corruption, we need to construct an estimate of the baseline conviction rate. The short run is our baseline period, so we calculate our estimate of the baseline conviction rate by averaging conviction rates for the switcher states over the 2-to-7 year window following the switch to a strong FOIA law, weighting each observation by the number of government employees. This baseline rate is 0.199 convictions per 10,000 government employees.[14]

In the second column of Table 3, we present the results for a fixed-effects Neg-

[14]An alternative approach is to include all the years in the calculation, while adjusting the observations that fall outside the short-run period by applying the appropriate estimate from Table 3. This gives an essentially identical baseline rate of 0.197, so we decide to use the more transparent approach.

ative Binomial regression of the number of state and local convictions on the same covariates along with state and local government employment. Using a Negative Binomial specification avoids the complications of constructing a baseline conviction rate. Because the table reports the exponentiated coefficients (incident rate ratios), the values in the table can be used to directly compute the percentage change in the incidence rate of corruption convictions for a unit increase in the independent variable. In particular, if we subtract one from the reported coefficient for the pre-enaction and long-run windows and then multiply by one hundred in each case, we obtain the percentage change from the short-run baseline rate for these periods.

Under the assumptions of our reduced-form model, we can combine the results from Table 3 with the estimated baseline conviction rates (if necessary) to separately identify the effects of enacting a strong FOIA law on conviction rates and on corruption rates. This yields slightly different implications across the specifications. The enaction of a strong FOIA law leads to a substantial increase in the rate at which corrupt acts are convicted. Depending on the specification, the conviction rate increases by 40 to 100 percent. If taken at face value, this has important policy implications. States can substantially increase the probability that corrupt officials will be unmasked and prosecuted by enacting strong FOIA laws.

Of course we can expect those who engage in corrupt acts to alter their behavior in response to the increased risk of detection and prosecution. As they do, the conviction rates should decline from the elevated level that prevails in the short run. Assuming that all of the observed change in conviction rates is due to changes in the level of corrupt behavior, we see a drop in this behavior of 40 to 50 percent from the elevated short-run rates. If we take these results seriously, then our analysis suggests an elasticity of supply of corruption, with respect to the probability of apprehension, of about -1. In the long run, therefore, we should expect actual convictions to bear little relation to the probability of apprehension. Actual corruption, on the other hand, declines strongly.

We have emphasized the deterrent effect of the increased probability of detection leading to a long-run reduction in corruption rates. An alternative (but not mutually exclusive) reading of our results is that corrupt officials are learning to alter their behavior in order to avoid detection under FOIA.[15] Maybe they avoid written records

[15]A number of popular press articles report that public officials alter their behavior in order to avoid FOIA laws. See, for example, "Government Uses Commercial Email and Texting to Avoid FOIA Laws," Huffington Post, August 22, 2009, available at http://www.huffingtonpost.com/peter-scheer/government-uses-commercia_b_265809.html, or "FL Official: I Don't Email Because of Open

or destroy extant records. Such behavior would lead us to overstate the size of the deterrent effect, and without an independent measure of avoidance behavior we cannot disentangle these two. For most reasonable models of avoidance, however, corrupt officials would adjust along both dimensions. After all, avoidance must be costly, or they would be doing it already, and the additional costs of avoiding detection would make some otherwise attractive corrupt acts become unattractive. The degree to which we believe the estimates above overstate the deterrent effect of FOIA will depend on how costly we believe avoidance to be.

4.3 Endogeneity

We recognize that our analysis may raise questions about endogeneity, and we do worry that strong FOIA adoption could be spurred by either a rash of corruption convictions or by some omitted factor that is correlated with convictions. The standard approach to this problem is to instrument for FOIA status. Costa (2009) uses this method to good effect in a cross country setting by arguing that a country with neighbors who have a FOIA law is more likely to have a FOIA law itself. A similar approach is feasible in our setting for identifying the overall average consequence of the presence of a strong FOIA law on convictions.

The data show that a given state is more likely to have a strong FOIA law if its neighbors have such a law, and the likelihood increases with the fraction of neighbors with such a law.[16] Furthermore, the 2SLS estimate for the effect of strong FOIA on the conviction rates of state and local officials is nearly identical to that in column (1) of Table 2 (0.074 versus 0.069), but the 2SLS estimate is much less precise than the FE-OLS estimate, and it is not statistically different from zero.

We should emphasize, however, that this overall treatment effect is *not* the parameter that we are interested in estimating, because it conflates the conviction and corruption effects. Unfortunately, using a policy-diffusion instrument will not suffice for the purposes of disentangling the effects of FOIA over time. Put simply, the fraction of neighboring states that had strong FOIA 5-7 years ago versus 8-10 years ago does very little to explain whether a state adopted strong FOIA 5-7 years ago or 8-10

Records Laws," available at http://techpresident.com/short-post/fl-official-i-dont-email-because-open-records-laws, accessed June 13, 2011.

[16] A log-linear specification gives the strongest first-stage results, with an F-statistic of 4.64. In the first-stage regression for predicting strong FOIA, we include all the variables in our OLS regression, except the strong FOIA dummy, and also include the natural log of 1 plus the percent of the neighbors of state s in year t that have a strong FOIA law.

years ago, and that is the type of variation we need to obtain a clean causal estimate of the short-run versus long-run effects.

That said, the lack of bias in the overall effect leads us to be a little more optimistic that our OLS estimates of the time-varying effects are not seriously contaminated by endogeneity. If the OLS-derived estimates of the conviction and corruption effects are biased, they need to be biased in equal and opposite directions in order to lead to an overall effect that is unbiased. We cannot come up with a plausible story that would produce such an outcome. Nevertheless, we exclude the years that we think will be most heavily influenced by endogeneity from our calculations of the corruption and conviction effects, i.e., the years immediately surrounding the time of strong FOIA adoption. This is admittedly an imperfect solution to concerns about endogeneity, but we believe that it is the best available given the substantial empirical challenges of disentangling the corruption and conviction effects.

4.4 Robustness

The estimates presented in the preceding section are robust to a number of alternative modeling choices and analysis procedures. We discuss our robustness checks in broad terms below, and provide a set of detailed tables in the Appendix.

One could use a count model to improve efficiency provided that some restrictions are imposed on the distribution of the outcome variables. Table A2 presents the results obtained when we use a Negative Binomial model to replicate the analysis of Table 2.[17] The pattern of rise and fall in conviction rates in the two tables is quite similar, both for the state and local convictions and for the federal convictions. However, we do get a little more action out of the covariates with the Negative Binomial model. The estimates indicate that convictions are positively correlated with GDP/capita, educational attainment, and judicial & legal expenditures.

All of our baseline results are obtained using data for the full set of 50 states. Many of these states do not switch their FOIA status during the sample period. Using the non-switcher states should help estimate the effects of covariates, thereby reducing the size of the standard errors. The risk is that these non-switchers may be different in important ways from the switcher states, both in terms of corruption trends and

[17] We chose the Negative Binomial model over a Poisson model after plotting the corruption convictions data against a Poisson distribution with the same mean and a Negative Binomial distribution with the same mean and variance. Figure A1, which is included in the Appendix, shows that the Negative Binomial distribution is a much better fit to our data.

in the trends of the other covariates. If this is the case, then including them could actually make the estimates worse. Tables A3 and A4 replicate Table 2 and Table 3 while restricting the sample to those states that change their FOIA status. The results are consistent with those in the baseline regressions, with the conviction effect perhaps a bit smaller and the corruption effect perhaps a bit larger.

Although our baseline specifications try to control for differences among states and over time with state and year fixed effects, the resulting estimates are not robust to omitted variables that vary over time within a state. In an effort to address this issue, we introduce state-specific trends into the FE-OLS model. Table A5 presents the results for both the full set of states and for the switchers only. For the specification that uses all 50 states, the introduction of state-specific trends has little impact on the conviction effect, but it reduces the corruption effect significantly. But when we replicate this analysis looking only at the switcher states, both effects remain robust. Perhaps the non-switchers are similar to switchers in terms of overall trends, but differ in terms of deviation from trends. The limitations of the dataset prevent us from further teasing apart this difference.

Another possible concern is that results are driven by some shock to a single state. In Table A6, we calculate the conviction effect and corruption effect for both the FE-OLS and FE-Negative Binomial models, just as we did in Table 3, omitting one switcher state at a time from the dataset. Each row contains the two sets of estimates obtained with the indicated state omitted. The states are ordered in terms of the number of government employees. The removal of a single state never has much impact on the estimates, and the changes go in both directions. Thus the evidence suggests that our findings are indicative of a systematic FOIA effect rather than the idiosyncratic role of a single state in our analysis.

As a final robustness check, we explore whether larger changes in FOIA laws induce larger changes in convictions. To investigate this possibility, we estimate another set of regressions, similar to those in Tables 2 and 3, that include interaction variables that capture the magnitude of change in the FOIA score. In particular, we construct a variable that measures the change in the FOIA score at the time a state switches from weak to strong FOIA law, and interact this variable with the strong FOIA dummy in regressions (1) and (2), and with the pre-enaction, enaction, and long-run windows in regressions (3) and (4). The results are shown in Table A7. The estimated coefficient for the interaction term in column (1) is positive and statistically significant at the 5 percent level, suggesting that larger changes in the FOIA laws have a larger impact on corruption convictions.

Because the estimated coefficients in column (3) are difficult to interpret on their own, we use Figure A2 to illustrate the implied changes between the short-run and the pre-enaction and long-run periods. The two panels show the estimated expected change in the conviction rate moving from the short run to the pre-enaction period (A2a) and the long run (A2b), as a function of the size of the policy change when strong FOIA is enacted. The estimated expected change is negative regardless of the size of the policy change. Moreover, its magnitude seems to be increasing with the size of the policy change for the short run versus pre-enactment contrast, pointing to larger jumps in conviction rates following marked policy changes. These results are consistent with the baseline results in Table 3.

In contrast, the estimated expected change in the conviction rate for the short run versus long run seems to be shrinking with the size of the policy change. If the decline in convictions from the short run to the long run is a consequence of corrupt officials adjusting their behavior, then the results in Figure A2 suggest that either they adjust their behavior less in response to a marked policy change (a result we find implausible) or the manner in which they adjust their behavior is somehow different following such a change. Two possible explanations present themselves. First, it is possible that officials adjust quickly to large, more salient, changes in FOIA policy, so part of the adjustment actually occurs during the short-run period. Second, it is possible that the ability to circumvent disclosure is harder for large changes in FOIA policy, so the smaller decline in convictions occurs not because officials make smaller adjustments, but because the adjustments that they make are simply less effective in hiding corrupt behavior.

5 Interpretation and Conclusions

It is relatively well established that an effective and free press has an important role in keeping potentially corrupt officials in line. Several recent examples of the press successfully playing this watchdog role have featured investigative journalists employing some version of freedom of information laws. Despite the intuitive connection between open government laws and watchdog journalists, previous research has failed to find a very solid connection between such laws and corruption. In fact, some studies have even concluded, in contrast with the most straightforward economic theories of crime, that the introduction of the increased scrutiny of open government laws led to an *increased* rate of corruption.

In this paper, we argue that such findings are an artifact of confounding two effects of the policy change: an increase in the probability of conviction and a decrease in the probability of corruption. If we observe an increase in the amount of corruption detected after the adoption of open government laws, we should not be surprised. We would expect increased transparency to make it more likely that the corrupt acts committed in the past will come to light. This should not be interpreted as evidence that the underlying level of corruption has increased. Indeed, it is precisely the outcome that we would hope for given a policy objective of reducing corruption. If the probability of detection and conviction increases, then we should ultimately see a decline in the probability of corruption. Making some assumptions about the rate at which officials adjust their corrupt behavior, we can disentangle these two effects.

Using our model as a guide, we assess the impact of switching from a weak to a strong state-level FOIA law on corruption convictions for state and local government officials. State corruption conviction rates rise after the switch to strong FOIA, with no concomitant change in federal convictions. Under a variety of econometric specifications, the short-run effect is an approximate doubling in the probability that a corrupt act is detected and convicted. Corruption conviction rates decline from this new elevated level as the time since the switch from weak to strong FOIA increases. If the decline is solely due to officials adjusting their behavior, then it implies that they decrease the rate at which they commit corrupt acts by about 40 percent.

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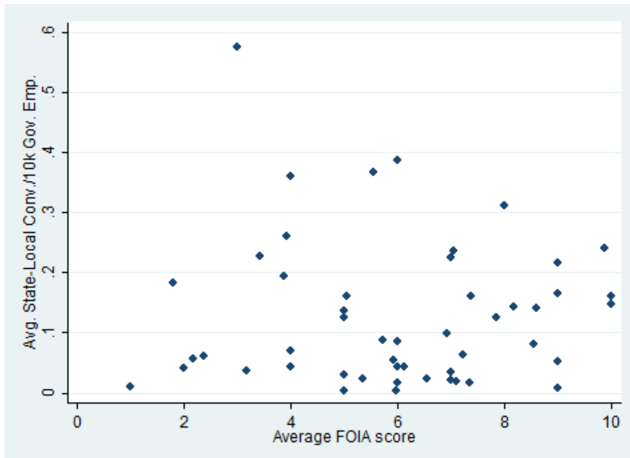
Figure 1: Average Convictions per 10,000 government employees and Average FOIA Score, 1986-2009

(a) State and Local



(b) Federal

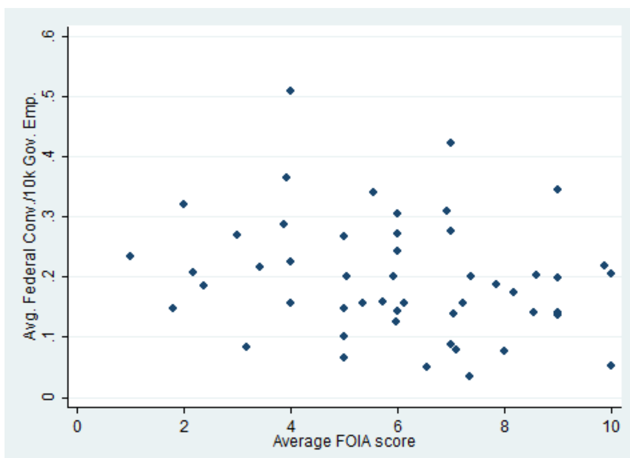
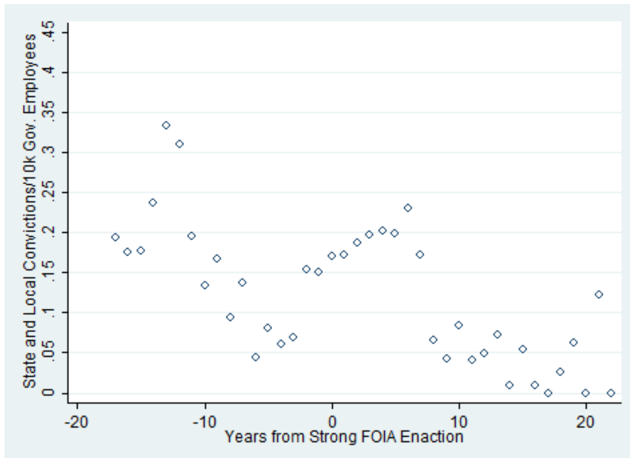


Figure 2: Convictions per 10,000 government employees for the states that switched to strong FOIA, before and after the switch

(a) State and Local



(b) Federal

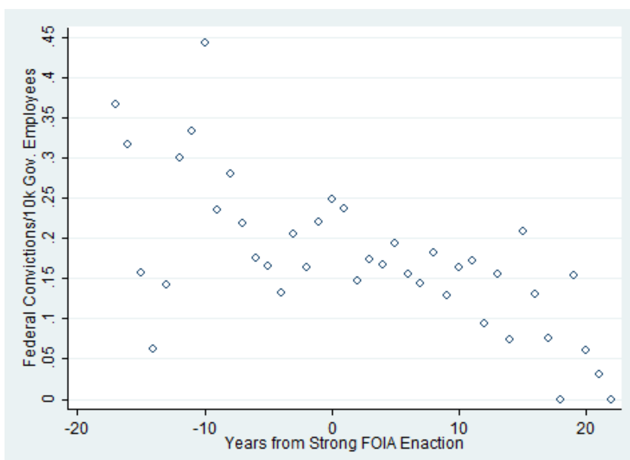


Figure 3: Estimated Difference in the Base Conviction Rate for State and Local Officials Before and After Strong FOIA Enaction

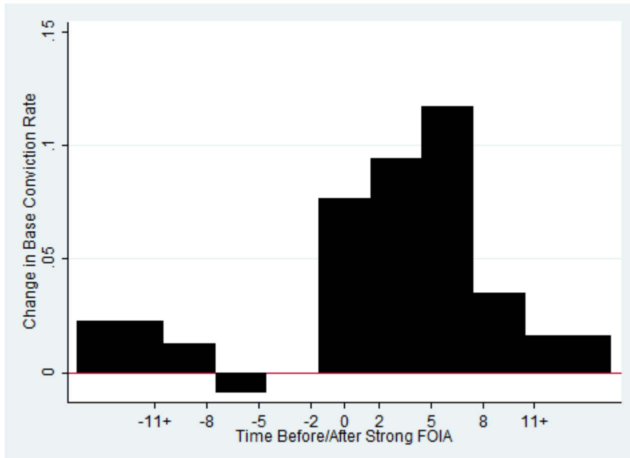


Table 1: Descriptive Statistics for FOIA Switchers and All States

Variable	Mean	Standard Deviation	Minimum	Maximum
Switchers Only		$N = 12$	$T = 24$	
State and Local Convictions	3.55	6.80	0.00	47.00
Federal Convictions	4.96	7.45	0.00	44.00
SL Conv. per 10k gov. emp.	0.12	0.27	0.00	2.91
Fed. Conv. per 10k gov. emp.	0.17	0.22	0.00	1.43
Pct. HS Grad.	82.52	6.42	61.62	92.50
GDP/cap (000s)	34.35	6.41	21.53	52.00
Population (M)	5.04	5.68	0.63	24.78
Strong FOIA Law	0.63	0.48	0.00	1.00
Judicial & Legal Exp./cap	41.97	25.36	8.84	132.57
Divided Government	0.47	0.50	0.00	1.00
Years Unified Gov.	3.45	5.02	0.00	25.00
Unified Gov. Ended	0.05	0.22	0.00	1.00
All States		$N = 50$	$T = 24$	
State and Local Convictions	3.50	5.77	0.00	47.00
Federal Convictions	6.16	10.36	0.00	83.00
SL Conv. per 10k gov. emp.	0.13	0.23	0.00	3.06
Fed. Conv. per 10k gov. emp.	0.20	0.24	0.00	2.84
Pct. HS Grad.	82.36	6.11	61.62	92.80
GDP/cap (000s)	36.40	7.73	21.20	72.36
Population (M)	5.46	5.99	0.45	36.96
Strong FOIA Law	0.49	0.50	0.00	1.00
Judicial & Legal Exp./cap	52.70	41.65	8.71	350.42
Divided Government	0.58	0.50	0.00	1.00
Years Unified Gov.	3.63	7.60	0.00	44.00
Unified Gov. Ended	0.06	0.23	0.00	1.00

Notes: Corruption convictions are from the TRACfed database (1986-2009). Strong FOIA is a dummy variable constructed from the Open Government Guide published by the Reporters Committee for Freedom of the Press (various years). Pct. HS Grad. is the share of the population aged 25 and up with a high school diploma or higher. GDP per capita data is from the Bureau of Economic Analysis. Demographic, public employment, and judicial & legal expenditures data are from the U.S. Census Bureau.

Table 2: FE-OLS of Conviction Rates (All States)

Dependent variable	sl/10k gov.	fed/10k gov.	sl/10k gov.	fed/10k gov.
	(1)	(2)	(3)	(4)
11+ Years Before			0.023	0.002
			(0.044)	(0.112)
8-10 Years Before			0.013	0.129*
			(0.024)	(0.069)
5-7 Years Before			−0.009	0.024
			(0.021)	(0.032)
Strong FOIA	0.069*	−0.013		
	(0.039)	(0.022)		
Enaction Period			0.076*	0.079
			(0.044)	(0.079)
2-4 Years After			0.094*	0.005
			(0.054)	(0.034)
5-7 Years After			0.117*	0.011
			(0.071)	(0.043)
8-10 Years After			0.035	0.024
			(0.044)	(0.038)
11+ Years After			0.016	−0.004
			(0.039)	(0.054)
Divided Gov.	0.007	0.026*	−0.000	0.025*
	(0.010)	(0.015)	(0.010)	(0.014)
Yrs. Unified Gov.	0.000	0.001	0.000	0.001
	(0.001)	(0.001)	(0.001)	(0.001)
Unified Gov. Ended	0.016	−0.001	0.015	−0.013
	(0.016)	(0.021)	(0.017)	(0.020)
GDP/Cap	−0.001	0.003	−0.001	0.003
	(0.004)	(0.006)	(0.003)	(0.006)
Pct. HS Grad	0.000	−0.006	0.000	−0.006
	(0.003)	(0.005)	(0.003)	(0.005)
Jud. & Legal Exp./Cap	0.000	−0.002***	0.000	−0.002***
	(0.000)	(0.001)	(0.000)	(0.001)

Notes: Regressions include all states (s=50, t=24). Corruption convictions are from the TRACfed database (1986-2009) and reported in convictions per 10,000 government employees. All regressions include state and year dummy variables. Standard errors, shown in parentheses, are clustered by state. *, **, and *** represent significance at the .10, .05, and .01 levels, respectively.

Table 3: Identifying Conviction versus Corruption using Short-Run and Long-Run Changes (All States)

Dependent variable	sl/10k gov.	sl
	Gov. Emp. Weighted	Neg. Bin.
	(1)	(2)
Pre-Enaction	-0.101^{**}	0.705^{**}
	(0.046)	(0.109)
During Enaction	-0.029	1.056
	(0.061)	(0.194)
Long Run	-0.080^{**}	0.529^{***}
	(0.036)	(0.095)
Divided Gov.	0.001	1.041
	(0.011)	(0.079)
Yrs. Unified Gov.	0.000	0.998
	(0.001)	(0.006)
Unified Gov. Ended	0.015	1.046
	(0.017)	(0.126)
GDP/Cap	-0.001	1.021^{*}
	(0.004)	(0.013)
Pct. HS Grad	-0.000	0.944^{***}
	(0.003)	(0.014)
Jud. & Legal Exp./Cap	0.000	0.993^{***}
	(0.000)	(0.002)
Gov. Employment (10k)		1.015^{***}
		(0.003)
$\bar{C}_{Short}$	0.199	
$\frac{\bar{C}_{Short}}{\bar{C}_{Pre}}$	2.03	1.41
$\frac{\bar{C}_{Long}}{\bar{C}_{Short}}$	0.60	0.53

Notes: Regressions include all states (s=50, t=24). Corruption convictions are from the TRACfed database (1986-2009). Specification (1) includes state and year dummy variables and is weighted by average state and local government employees. Specification (2) includes state and year fixed effects. Standard errors, shown in parentheses, are clustered by state. The coefficients in specification (2) are exponentiated, so they can be interpreted as marginal effects of a unit increase on the incidence rate of corruption convictions. *,**, and *** represent significance at the .10, .05, and .01 levels, respectively, where the null hypothesis in the negative-binomial specification (2) is that the exponentiated coefficient is equal to 1 (no difference among time-frames).

Table A1: Evolution of FOIA Score by State Over Time

St./Yr.	'86	'87	'88	'89	'90	'91	'92	'93	'94	'95	'96	'97	'98	'99	'00	'01	'02	'03	'04	'05	'06	'07	'08	'09
AL	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	1	1	1	1	1
AK	4	4	4	4	4	4	4	4	4	4	4	4	4	4	4	4	4	4	4	4	4	4	4	4
AZ	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2
AR	7	7	8	8	8	8	8	8	8	8	8	8	8	8	8	8	10	10	10	10	10	10	10	10
CA	6	6	6	6	6	6	6	6	6	6	6	6	6	6	6	6	6	6	6	6	6	6	6	6
CO	9	9	9	9	9	9	9	9	9	9	9	9	9	9	9	9	9	9	9	9	9	9	9	9
CT	10	10	10	10	10	10	10	10	10	10	10	10	10	10	10	10	10	10	10	10	10	10	10	10
DE	2	2	3	3	3	3	5	5	5	6	6	6	6	6	6	6	6	6	6	6	6	6	6	6
FL	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5
GA	7	7	7	7	8	8	8	8	8	8	8	8	8	8	8	8	8	8	8	8	8	8	8	8
HI	1	1	1	1	4	4	4	4	4	4	4	4	4	4	5	5	5	5	5	5	5	5	5	5
ID	1	1	1	1	1	8	8	8	8	8	8	8	8	8	8	8	8	8	8	8	8	8	8	8
IL	9	9	9	9	9	9	9	9	9	9	9	9	9	9	9	9	9	9	9	9	9	9	9	9
IN	10	10	10	10	10	10	10	10	10	10	10	10	10	10	10	10	10	10	10	10	10	10	10	10
IA	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5
KS	7	7	7	7	7	7	7	7	7	7	7	7	7	7	7	7	7	7	7	7	7	7	7	7
KY	8	8	8	8	8	8	8	8	8	7	7	7	7	7	7	7	7	7	7	7	7	7	7	7
LA	9	9	9	10	10	10	10	10	10	10	10	10	10	10	10	10	10	10	10	10	10	10	10	10
ME	7	7	7	7	7	7	7	7	7	7	7	7	7	7	7	7	7	7	7	8	8	8	8	8
MD	6	6	6	6	6	6	6	6	6	6	6	6	6	6	6	6	6	6	6	6	6	6	6	6
MA	7	7	7	7	7	7	7	7	7	7	7	7	7	7	7	7	7	7	7	7	7	7	7	7
MI	8	8	8	8	8	8	8	8	8	8	8	9	9	9	9	9	9	9	9	9	9	9	9	9
MN	4	4	4	4	4	4	4	4	4	4	4	4	4	4	4	4	4	4	4	4	4	4	4	4
MS	6	6	6	6	6	6	6	6	6	6	6	6	6	6	6	6	6	6	6	6	6	6	6	6
MO	7	7	8	8	8	8	8	8	8	8	8	8	8	8	8	8	8	8	9	9	9	9	9	9
MT	3	3	3	3	3	3	3	3	3	3	3	3	3	3	3	3	3	3	3	3	3	3	3	3
NE	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	8	8	8	8	8	8	8	8	8
NV	2	2	2	2	2	1	1	1	2	2	2	2	3	3	3	3	3	3	3	3	3	3	3	3
NH	4	8	8	8	8	8	8	8	8	8	8	8	7	7	7	7	7	7	7	7	7	7	7	7

Table A1: Evolution of FOIA Score by State Over Time (continued)

St./Yr.	'86	'87	'88	'89	'90	'91	'92	'93	'94	'95	'96	'97	'98	'99	'00	'01	'02	'03	'04	'05	'06	'07	'08	'09
NJ	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	8	8	8	8	8	8	8	8
NM	2	2	2	2	2	2	2	2	7	7	7	7	7	7	7	7	7	7	7	7	7	7	7	7
NY	9	9	9	9	9	9	9	9	9	9	9	9	9	9	9	9	9	9	9	9	9	9	9	9
NC	2	2	2	2	2	2	2	2	2	2	4	4	4	4	4	4	4	4	4	4	4	4	4	4
ND	3	3	3	4	4	4	4	4	5	5	5	5	7	7	7	7	7	7	7	7	7	7	7	7
OH	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5
OK	5	5	5	5	5	5	5	6	6	6	6	6	6	6	6	6	6	6	6	6	6	6	6	6
OR	6	6	6	6	6	6	6	6	6	6	6	6	6	6	6	6	6	6	6	6	6	6	6	6
PA	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	9	9	9	9	9	9	11
RI	8	8	8	8	8	8	8	8	8	8	8	8	8	8	8	8	8	8	8	8	8	8	8	8
SC	6	6	7	7	7	7	7	7	7	7	7	7	7	7	7	7	7	7	7	7	7	7	7	7
SD	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1
TN	3	3	3	4	4	4	4	4	4	4	4	4	4	4	4	4	4	4	4	4	4	4	4	4
TX	4	4	4	4	4	4	4	4	4	4	7	7	7	7	7	7	7	7	7	7	8	8	8	8
UT	1	1	1	1	1	1	1	8	8	8	8	8	8	8	8	8	8	8	8	8	8	8	8	8
VT	9	9	9	9	9	9	9	9	9	9	9	9	9	9	9	9	9	9	9	9	9	9	9	9
VA	7	7	7	7	7	7	7	7	7	7	7	7	7	7	7	7	7	7	7	7	7	7	7	7
WA	5	5	5	5	5	5	5	8	8	7	8	8	8	8	8	8	8	8	8	8	8	8	8	8
WV	6	6	6	6	6	6	6	7	7	7	7	7	7	7	7	7	8	8	8	8	8	8	8	8
WI	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5
WY	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	3	3	3	3

Notes: The FOIA score for each state is determined by giving one point for each of the following criteria: a provision that creates a presumption in favor of disclosure and identifies specific records that are exempt from public access; the lack of a generic public-interest exemption provision; a provision that limits fees charged for processing FOIA requests; a provision that prohibits charging a fee for the time required to collect records; a provision for waiver of the cost of search for or duplication of public records if it's determined that disclosure is in the public interest; a provision for criminal penalties for noncompliance with disclosure obligations; a provision for civil penalties for noncompliance with disclosure obligations; a provision for the award of attorneys' fees and costs to a successful plaintiff in a public records case; and a provision for administrative appeal of a decision to deny a request for public records. Finally, we give one point for each of the following that is satisfied: time to respond to a request for access to public records is 30 days or less, time to respond is 15 days or less, and time to respond is 7 days or less.

Table A2: Replication of Table 2, Fixed-Effects Negative Binomial Model

	sl	fed	sl	fed
	(1)	(2)	(3)	(4)
11+ Years Before			1.345	0.993
			(0.354)	(0.220)
8-10 Years Before			1.071	1.817***
			(0.297)	(0.337)
5-7 Years Before			0.762	1.170
			(0.214)	(0.219)
Strong FOIA	1.130	0.991		
	(0.137)	(0.095)		
Enaction Period			1.471*	1.336*
			(0.318)	(0.232)
2-4 Years After			1.379	0.979
			(0.299)	(0.184)
5-7 Years After			1.514**	1.089
			(0.316)	(0.194)
8-10 Years After			0.816	1.149
			(0.215)	(0.226)
11+ Years After			0.646*	0.965
			(0.165)	(0.180)
Gov. Employment (10k)	1.014***	1.014***	1.015***	1.014***
	(0.003)	(0.002)	(0.003)	(0.002)
Divided Gov.	1.104	1.061	1.025	1.066
	(0.084)	(0.066)	(0.078)	(0.067)
Yrs. Unified Gov.	0.999	1.004	0.999	1.004
	(0.006)	(0.005)	(0.006)	(0.005)
Unified Gov. Ended	1.053	1.019	1.066	0.967
	(0.127)	(0.103)	(0.129)	(0.098)
GDP/Cap	1.018	1.030***	1.022*	1.030***
	(0.013)	(0.011)	(0.013)	(0.010)
Pct. HS Grad	0.945***	0.986	0.944***	0.985
	(0.014)	(0.011)	(0.014)	(0.011)
Jud. & Legal Exp./Cap	0.995***	0.994***	0.994***	0.994***
	(0.002)	(0.001)	(0.002)	(0.001)

Notes: Regressions include all states (s=50, t=24). Corruption convictions are from the TRACfed database (1986-2009). All regressions include state and year fixed effects. The coefficients are exponentiated, so they can be interpreted as marginal effects of a unit increase on the incidence rate of corruption convictions. *,**, and *** represent significance at the .10, .05, and .01 levels, respectively, where the null hypothesis is that the exponentiated coefficient is equal to 1 (no difference among time-frames).

Table A3: FE-OLS of Conviction Rates (Switchers Only)

Dependent variable	sl/10k gov.	fed/10k gov.	sl/10k gov.	fed/10k gov.
	(1)	(2)	(3)	(4)
11+ Years Before			0.041	0.006
			(0.070)	(0.112)
8-10 Years Before			0.022	0.153***
			(0.060)	(0.047)
5-7 Years Before			0.002	0.060
			(0.034)	(0.037)
Strong FOIA	0.076*	0.007		
	(0.043)	(0.046)		
Enaction Period			0.049	0.085
			(0.035)	(0.079)
2-4 Years After			0.067*	−0.010
			(0.036)	(0.055)
5-7 Years After			0.075	−0.019
			(0.060)	(0.067)
8-10 Years After			−0.041	−0.016
			(0.042)	(0.076)
11+ Years After			−0.080	−0.030
			(0.055)	(0.094)
Divided Gov.	0.014	0.028	0.001	0.035
	(0.047)	(0.045)	(0.036)	(0.051)
Yrs. Unified Gov.	0.002	0.006	0.005	0.006
	(0.009)	(0.005)	(0.009)	(0.005)
Unified Gov. Ended	0.046*	0.064	0.060*	0.003
	(0.025)	(0.050)	(0.029)	(0.035)
GDP/Cap	0.018	0.009	0.016	0.006
	(0.016)	(0.010)	(0.015)	(0.008)
Pct. HS Grad	−0.006	−0.016**	−0.007	−0.021***
	(0.011)	(0.007)	(0.012)	(0.008)
Jud. & Legal Exp./Cap	0.005***	0.002	0.003***	0.002*
	(0.001)	(0.001)	(0.001)	(0.001)

Notes: Regressions include every state that enacted a strong FOIA law between 1986 and 2009 (s=12, t=24). Corruption convictions are from the TRACfed database (1986-2009) and reported in convictions per 10,000 government employees. All regressions include state and year dummy variables. Standard errors, shown in parentheses, are clustered by state. *, **, and *** represent significance at the .10, .05, and .01 levels, respectively.

Table A4: Identifying Conviction versus Corruption using Short-Run and Long-Run Changes (Switchers Only)

Dependent variable	sl/10k gov. empl. Gov. Emp. Weighted	sl Neg. Bin.
	(1)	(2)
Pre-Enaction	-0.097^{**}	0.753
	(0.045)	(0.234)
During Enaction	-0.038	0.876
	(0.060)	(0.183)
Long Run	-0.111^{***}	0.317^{***}
	(0.031)	(0.089)
Divided Gov.	-0.001	1.742^{**}
	(0.039)	(0.418)
Yrs. Unified Gov.	0.004	1.146^{***}
	(0.009)	(0.048)
Unified Gov. Ended	0.060^{*}	1.471^{*}
	(0.033)	(0.344)
GDP/Cap	0.016	1.056
	(0.016)	(0.044)
Pct. HS Grad	-0.008	0.901^{***}
	(0.011)	(0.033)
Jud. & Legal Exp./Cap	0.003^{***}	1.004
	(0.001)	(0.008)
Gov. Employment (10k)		1.036^{***}
		(0.010)
$\overline{C}_{Short}$	0.199	
$\frac{\overline{C}_{Short}}{\overline{C}_{Pre}}$	1.95	1.33
$\frac{\overline{C}_{Long}}{\overline{C}_{Short}}$	0.44	0.32

Notes: Regressions include every state that enacted a strong FOIA law between 1986 and 2009 (s=12, t=24). Corruption convictions are from the TRACfed database (1986-2009). Specification (1) includes state and year dummy variables and is weighted by average state and local government employees. Specification (2) includes state and year fixed effects. Standard errors, shown in parentheses, are clustered by state. The coefficients in specification (2) are exponentiated, so they can be interpreted as marginal effects of a unit increase on the incidence rate of corruption convictions. *,**, and *** represent significance at the .10, .05, and .01 levels, respectively, where the null hypothesis in the negative-binomial specification (2) is that the exponentiated coefficient is equal to 1 (no difference among time-frames).

Table A5: Robustness to State-Specific Trends, Full Sample and Switchers Only

	Full Sample	Switchers
	sl/10k gov	sl/10k gov
	(1)	(2)
Pre-Enaction	-0.107^{*}	-0.109^{**}
	(0.062)	(0.048)
During Enaction	-0.037	-0.042
	(0.059)	(0.066)
Long Run	-0.019	-0.081^{**}
	(0.035)	(0.034)
Divided Gov.	-0.001	0.021
	(0.013)	(0.047)
Yrs. Unified Gov.	-0.001	0.008
	(0.001)	(0.009)
Unified Gov. Ended	0.009	0.045
	(0.017)	(0.031)
GDP/Cap	-0.003	0.002
	(0.004)	(0.009)
Pct. HS Grad	0.000	-0.012
	(0.005)	(0.021)
Jud. & Legal Exp./Cap	-0.000	0.004
	(0.001)	(0.004)
$\bar{C}_{Short}$	0.199	0.199
$\frac{\bar{C}_{Short}}{\bar{C}_{Pre}}$	2.16	2.21
$\frac{\bar{C}_{Long}}{\bar{C}_{Short}}$	0.90	0.59

Notes: Regression (1) includes all states (s=50, t=24). Regression (2) includes every state that enacted a strong FOIA law between 1986 and 2009 (s=12, t=24). Corruption convictions are from the TRACfed database (1986-2009). All regressions include state and year fixed effects and state-specific trends. *,**, and *** represent significance at the .10, .05, and .01 levels, respectively, where the null hypothesis is a coefficient of zero.

Table A6: Robustness to Dropping Individual States

Dropped State	OLS $\frac{\overline{C}_{Short}}{\overline{C}_{Pre}}$	OLS $\frac{\overline{C}_{Long}}{\overline{C}_{Short}}$	Neg. Bin. $\frac{\overline{C}_{Short}}{\overline{C}_{Pre}}$	Neg. Bin. $\frac{\overline{C}_{Long}}{\overline{C}_{Short}}$
$-ND$	1.861	0.584	1.209	0.524
$-NH$	2.021	0.596	1.411	0.532
$-ID$	2.016	0.605	1.399	0.539
$-WV$	2.284	0.615	1.561	0.487
$-NE$	2.029	0.598	1.357	0.528
$-UT$	2.022	0.605	1.409	0.536
$-NM$	2.017	0.591	1.389	0.504
$-SC$	2.010	0.688	1.392	0.694
$-WA$	1.998	0.597	1.407	0.508
$-NJ$	1.857	0.619	1.492	0.544
$-PA$	2.151	0.528	1.653	0.498
$-TX$	1.987	0.633	1.465	0.516

Notes: Each row presents the estimated conviction and corruption effects computed in the same manner as for Table 3, but with coefficient estimates obtained using a dataset that excludes the indicated state. The estimates in the first two columns are for the FE-OLS model. Those in the last two columns are for the FE-Negative Binomial model. States are listed in order of increasing government employment.

Table A7: Including Interaction Variables to Capture the Magnitude of the Policy Change

Dependent variable	sl/10k gov.	fed/10k gov.	sl/10k gov.	fed/10k gov.
	(1)	(2)	(3)	(4)
Strong FOIA	−0.071	−0.106		
	(0.070)	(0.072)		
Strong FOIA x Policy Change	0.031**	0.020		
	(0.014)	(0.013)		
Pre-Enaction Period			−0.007	0.082
			(0.088)	(0.080)
Enaction Period			0.011	0.153
			(0.096)	(0.249)
Long Run			−0.092	−0.088*
			(0.058)	(0.050)
Pre-Enaction x Policy Change			−0.020	−0.013
			(0.018)	(0.016)
Enaction x Policy Change			−0.010	−0.019
			(0.028)	(0.049)
Long Run x Policy Change			0.008	0.031**
			(0.015)	(0.014)
Divided Gov.	0.004	0.024*	−0.000	0.023*
	(0.010)	(0.014)	(0.010)	(0.014)
Yrs. Unified Gov.	0.000	0.001	0.000	0.001
	(0.001)	(0.001)	(0.001)	(0.001)
Unified Gov. Ended	0.012	−0.004	0.015	−0.005
	(0.015)	(0.020)	(0.017)	(0.021)
GDP/Cap	−0.000	0.004	−0.000	0.004
	(0.003)	(0.006)	(0.004)	(0.006)
Pct. HS Grad	−0.001	−0.007	−0.001	−0.005
	(0.004)	(0.006)	(0.003)	(0.006)
Jud. & Legal Exp./Cap	0.000	−0.002***	0.000	−0.002***
	(0.000)	(0.001)	(0.000)	(0.001)

Notes: Regressions include all states (s=50, t=24). Corruption convictions are from the TRACfed database (1986-2009) and reported in convictions per 10,000 government employees. The policy change interaction variable measures the change in the FOIA score at the time a state switches from weak to strong FOIA law. All regressions include state and year dummy variables. Standard errors, shown in parentheses, are clustered by state. *, **, and *** represent significance at the .10, .05, and .01 levels, respectively.

Figure A1: Observed State and Local Corruption Convictions variable against a Poisson distribution with the same mean and a Negative Binomial distribution with the same mean and variance

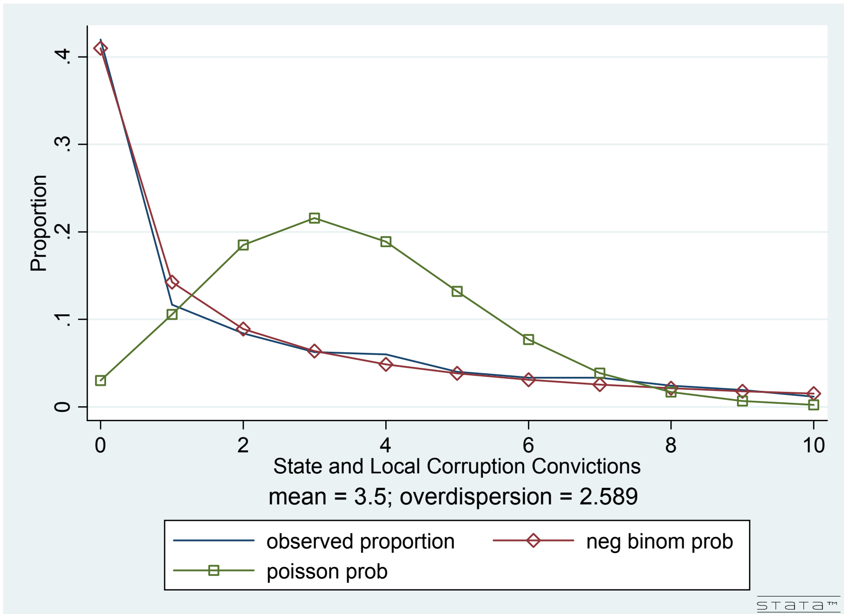
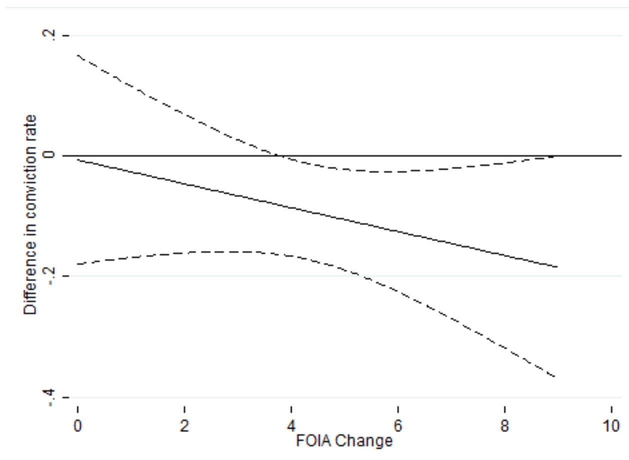


Figure A2: Pre-Enaction and Long-Run Effects as a Function of the Magnitude of the Policy Change

(a) Pre-Enaction



(b) Long run