Journal of Public Economics 115 (2014) 18–36

Contents lists available at ScienceDirect

Journal of Public Economics

journal homepage: www.elsevier.com/locate/jpube

Sunshine as disinfectant: The effect of state Freedom of Information Act laws on public corruption☆

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ARTICLE INFO

Article history:
Received 22 June 2012
Received in revised form 20 March 2014
Accepted 28 March 2014
Available online 18 April 2014

JEL classification:
D73
D78
H11
K0

Keywords:
FOIA
Sunshine
Corruption
Open government

ABSTRACT

We assess the effect of Freedom of Information Act (FOIA) laws on public corruption in the United States. Specifically, we investigate 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 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 conviction rates approximately double after the switch, which suggests an increase in detection probabilities. However, 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 20%. These changes are more pronounced in states with more intense media coverage, for those that had more substantial changes in their FOIA laws, for FOIA laws which include strong liabilities for officials who contravene them, for local officials, and for more serious crimes. Conviction rates of federal officials, who are not subject to the policy, show no concomitant change.

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 corruption in Alabama's 2-year college system.[1] He collected reams of financial records, contracts, and disclosure forms that revealed a compelling story of 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 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

☆ We thank Tom Chang, Tal Gross, Brian Knight, Jeanne Lafortune, Tom Mroz, Charles Thomas, and two anonymous referees for their helpful comments. Earlier versions of the paper were presented at the MIT Development Lunch, the Association of Private Enterprise Education Conference, the Global Conference for Transparency Research, and the Public Choice Society meetings. This research was begun while Warren was studying under a National Science Foundation Graduate Research Fellowship.

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[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.

http://dx.doi.org/10.1016/j.jpubeco.2014.03.010
0047-2727/

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 lauded transparency (see, e.g., Klitgaard (1988), Rose-Ackerman (1999), Brunetti and Weder (2003), Peisakhin and Pinto (2010), Peisakhin (2012)). 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 may not always be beneficial. Bac (2001) for instance, contends that transparency can have a perverse effect on corruption. Specifically, he argues that transparency may 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: information about the consequences of the agent's action and information directly about the action itself. The former is always beneficial, while the latter can have detrimental effects, because 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 favors the view that increased transparency is beneficial, the evidence with respect to FOIA laws is 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 (2013) finds that the adoption of FOIA laws increases the perceived corruption level, particularly in the first 5 years after enactment. Escaleras et al. (2010) 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 objective prevalence of public corruption among state and local government officials. 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. Finally, there is a set of identifiable public officials — federal employees — who should not be affected by state FOIA laws. This feature facilitates a straightforward placebo test.

We measure corruption by using annual state-level data for 1986–2009 reported by the Transactional Records Access Clearinghouse, which compiles information on corruption convictions from the Executive Office of U.S. Attorneys. The database maintained by this organization 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, but not federal officials, 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. This is the only database that reports the disaggregated conviction 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 record 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, impose deadlines for responding to FOIA requests, and punish officials who fail to properly respond to information requests to have more open and transparent governments. This openness should make it more difficult for corrupt public officials to escape public scrutiny.

All states have some sort of law that governs 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. Our analysis reveals that 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. Thus state FOIA laws affect either conviction or corruption rates of state and local officials.

Encouraged by this finding, we propose a reduced-form model to help disentangle changes in conviction rates from changes in corruption rates. This exercise is important because a naïve analysis might simply attribute all changes in observed conviction rates to changes in the level of corruption, possibly leading to the implausible conclusion that strengthening FOIA laws actually increases corruption. The model predicts that strengthening FOIA laws has two effects: reducing corruption rates 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), we can partially separate the two effects.

Using an approach motivated by the model, we investigate the impact of switching from weak to strong FOIA on corruption convictions of state and local officials. Our specifications control for known determinants of corruption rates, include a complete set of state and year dummy variables and state-specific trends, and employ a set of propensity-score-matched control states. We find throughout that corruption conviction rates rise substantially after strong FOIA adoption, approximately doubling in most specifications, which suggests a significant increase in detection probabilities. However, corruption conviction rates decline by about 20% from this new elevated level as the time since the adoption of strong FOIA increases, which suggests a substantial reduction in the underlying corruption level in response to strong FOIA enaction. There is no concomitant change in the corruption convictions of federal officials in these same states.

To provide additional insights on the effects of FOIA, we decompose our measure of the strength of state FOIA laws into four components: liability, time, money, and discretion. The liability component measures civil and criminal penalties for violating FOIA provisions, the time component measures the limitations on the time allowed to respond to FOIA requests, the money component measures the allowable fees for requests, and the discretion component measures the strength of limitations on the discretion of officials in providing requested information. Examining each of the components individually suggests that liability is the most important dimension of FOIA. In particular, the pattern of estimated coefficients for our specifications suggests that the impact of strong FOIA enaction on the corruption rate of state and local officials is largely confined to the subset of states that put FOIA responders at a real risk of loss for ignoring requests.

We also investigate whether the magnitude of the policy change that causes a state to cross the strong FOIA threshold plays a role in our findings. Some states switched from weak to strong FOIA by making relatively minor legislative changes, while others enacted much more dramatic changes. Our analysis suggests that the observed changes in conviction rates are primarily driven by those states with large changes to their FOIA policy. We view this finding as qualitatively consistent with the predictions of our reduced-form model. Because the states with large policy changes had very low FOIA scores prior to the enaction of a strong FOIA law, our model suggests that these states probably had a larger stock of corrupt acts (on a per government employee basis) than states much closer to the strong FOIA threshold. We would therefore expect the heightened scrutiny that follows enaction of a strong FOIA law to generate a more dramatic rise in conviction rates for these states than for the other states.

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, in Section 5 we investigate heterogeneous effects of FOIA, and in Section 6 we present several robustness tests. In Section 7 we interpret the results and offer a few concluding remarks.

2. Reduced-form model of policy, corruption and conviction

We begin our analysis 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. We do not model public employees' corruption decisions. Instead, we allow for the possibility that public employees alter their behavior in response to a change in FOIA but remain agnostic about the mechanism of this response.

2.1. The model

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

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

In each period a fraction $(1 - \alpha)$ of the 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 $\epsilon^P_{s,t,j}$.

The stock transition is governed by the equation

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

In terms of convictions, this relation becomes

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

We refer to the average value of $N_{s,FOIA}/N_{s,NoFOIA}$ as the "corruption effect" because it measures the average percentage change in the arrival rate of new corrupt acts when strong FOIA laws are enacted. Similarly, we refer to the average value of $\gamma_{FOIA}/\gamma_{NoFOIA}$ as the "conviction effect" because it measures the average percentage change in the probability of conviction when strong FOIA laws are enacted. These quantities cannot be directly observed, but we can bound them in our data.

2.2. Corruption versus conviction

The policy-specific steady-state rate of observed corruption convictions under the model is

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

The average level of convictions in state s and regime j is a consistent estimator of $\overline{C}_{s,j}$, so with a long enough time series we could use the difference in average conviction rates for the years before and after a strong FOIA law is enacted to estimate the overall FOIA effect. But doing so would provide few insights regarding the corruption and conviction effects, since both γ and N influence the steady state.

The key to separating the corruption effect from the conviction effect is to recognize that changes in the probability of conviction affect observed convictions quickly, while changes in corruption behavior affect observed convictions more slowly. This difference occurs for two reasons. First, a change in the probability of conviction affects both the new corrupt acts and the stock of corrupt acts established under the old policy regime. That stock adjusts toward the new steady state, but only slowly. Second, potentially corrupt officials may not immediately alter their behavior in response to changes in policy, or they may do so only incompletely. Just as we expect firms to react more completely to a price change in the long run than they do in the short run, so might we expect relatively small changes in corruption behavior in the short run because potentially corrupt officials are uncertain about the efficacy of the law, or because it takes time to unwind their corrupt practices (see Sah (1991) for a micro-founded formalization of this idea).

To formalize this intuition, define three response periods: $r \in \{Pre, Short, Long\}$ for pre-FOIA, short-run post-FOIA, and long-run post-FOIA. Define these period such that $N_{s,Pre} = N_{s,Short} = N_{s,NoFOIA}$ and $N_{s,Long} = N_{s,FOIA}$, while $\gamma_{Pre} = \gamma_{NoFOIA}$ and $\gamma_{Short} = \gamma_{Long} = \gamma_{FOIA}$. If the system persisted in each response period for an extended time, then using the average conviction rate in period r as an estimate of $\overline{C}_{s,r}$ would be a feasible strategy. But this approach is problematic for the short-run period, which is transient by definition. Even the long-run steady state may be difficult to reliably estimate, because states that enact FOIA will both spend some time in the short-run regime and also require some time to fully transition to the long-run steady state. For late adopters of FOIA, there may not be enough time for a full transition before our sample ends.

In light of these complications, we explore the system's behavior at the transition to and from the short run in greater detail. Let $t = \underline{t}$ represent the first year and $t = \overline{t}$ the last year of the short-run period. Iterating on Eq. (3), noting that $E\left[C_{s,\underline{t}-1}\right] = \overline{C}_{s,Pre}$, and assuming that $\gamma_{NoFOIA} \le \gamma_{FOIA}$, we obtain

$$\frac{\gamma_{FOIA}}{\gamma_{NoFOIA}} = \frac{E\left[C_{\underline{t},s}\right]}{\overline{C}_{s,Pre}} \ge \frac{E\left[C_{\underline{t}+1,s}\right]}{\overline{C}_{s,Pre}} \ge \cdots \ge \frac{E\left[C_{\overline{t},s}\right]}{\overline{C}_{s,Pre}} \ge \frac{\overline{C}_{s,Short}}{\overline{C}_{s,Pre}}, \tag{5}$$

where the inequalities are strict if $\gamma_{NoFOIA} < \gamma_{FOIA}$. Thus the ratio of the short-run conviction rate to the pre-enaction conviction rate for any given year in the short-run period provides an estimate of the conviction effect, that estimate is biased downward for years beyond $\underline{t}$, and the magnitude of the bias is larger for short-run years farther from the enaction date.

Similarly, we can show that

$$\frac{E\left[C_{s,\bar{t}+1}\right]}{\overline{C}_{s,Short}} \geq \cdots \geq \frac{E\left[C_{s,T-1}\right]}{\overline{C}_{s,Short}} \geq \frac{E\left[C_{s,T}\right]}{\overline{C}_{s,Short}} \geq \frac{N_{s,FOIA}}{N_{s,NoFOIA}} \tag{6}$$

$$= \frac{\overline{C}_{s,Long}}{\overline{C}_{s,Short}} \geq \frac{\overline{C}_{s,Long}}{E\left[C_{\bar{t},s}\right]} \geq \frac{\overline{C}_{s,Long}}{E\left[C_{\bar{t}-1,s}\right]} \geq \cdots \geq \frac{\overline{C}_{s,Long}}{E\left[C_{\underline{t},s}\right]},$$

so the ratios of the long-run conviction rates to the short-run conviction rates provide estimates of the corruption effect. These estimates are biased for observations far from the steady state, but the biases in the numerator and denominator offset if we are using observations far from the long-run steady state for the numerator and far from the short-run steady state for the denominator. Although the net direction of the bias is theoretically ambiguous, we show in a numerical analysis in Table 1 that for reasonable parameter choices, we obtain upwardly-biased estimates of the corruption effect, i.e., observed conviction rates move less than underlying corruption rates.

Several econometric difficulties arise in moving from this theory to the actual estimation of the effects. First, $\bar{t}$ is unknown, and may even vary among states, depending on how quickly potentially corrupt officials adjust their behavior. Second, $\underline{t}$ is imperfectly observed, because the date at which states fully de facto implement FOIA may be different from the date at which the law de jure goes into effect, and there will be uncertain delays between the enaction date and the first set of convictions arising from the change. Finally, even if the exact cutoffs were known, we still face a tradeoff between bias and variance when deciding on the number of short-run years to include in the estimation of each effect. If there were infinitely many states, and we observed exactly when the conviction probabilities changed, then we could simply use the contrast between the first short-run year and the pre-enaction steady state to construct an unbiased estimate of the conviction effect, and then contrast the last year of the short-run period and the long-run steady state to construct the least-biased estimate of the corruption effect.

In fact, only 12 states switched their FOIA status during our sample period. We may therefore wish to construct lower variance but more biased estimates by including additional years from the short-run period when constructing the estimates.[4] The interaction between the timing difficulties and the bias/variance tradeoff is also an important consideration. The least-biased years are those we are least certain actually fall within the short-run period. Because we are left with very weak guidance overall on which observations to give the greatest weight, we eschew weighting and/or adjusting the estimates altogether. Instead, we simply use the entire short-run period to construct the estimates, and keep in mind that the direction of the biases makes the estimates conservative.

To further illustrate these points, Table 1 presents the expected values of the proposed estimators for various combinations of the underlying parameters and short-run estimation windows.[5] The conviction rate (γ) changes at $t = 0$, but the date at which the corruption rate (N) changes varies from $t = -1$ (where the potentially corrupt officials change their behavior in anticipation of the policy change) to $t = 10$ (where they respond very slowly). We also consider two persistence rates (α) for the stock of corrupt acts, and two short-run estimation windows: one in which we mis-specify the short-run by starting the window too soon ($t = -1$ to $t = 5$) and one in which we mis-specify the short-run by starting the window too late ($t = 2$ to $t = 7$).

The top half of the table is for a parameter configuration that implies a true conviction effect of 1.4 and a true corruption effect of 0.60. If the persistence rate is 0.9, and we use a short-run window of $t = 2$ to $t = 7$, our estimate of the conviction effect varies from 1.09 (if officials preemptively act at $t = -1$) to 1.38 (if officials are extremely slow in reacting, waiting until $t = 10$). Similarly, our estimate of the corruption effect varies from 0.77 (for moderate reaction speed) to 0.85 (at the extremes of reaction). The bottom half of the table presents the same calculations for an economy where FOIA has smaller effects. The results are similar. In all cases, the estimates of both the conviction and corruption effects are consistently biased toward 1, i.e., toward finding no result. If we had sufficient data that were well suited to a structural time-series approach, then we could perhaps get better estimates. But given how lumpy the data actually are, we believe that our "lower bound" estimates of the corruption and conviction effects are the best available.[6] Fig. 1 plots the path of expected convictions for the four different reaction dates.

Table 1
Numerical analysis of bias for proposed estimators of FOIA effects.

Estimated effect	Short-run window	α	Date of corruption adjustment			
			$t = -1$	$t = 0$	$t = 5$	$t = 10$
Model 1	$\mathbf{N_{NoFOIA}}=.1\ \mathbf{N_{FOIA}} = 0.06\ \gamma_{\mathbf{NoFOIA}} = 0.01\ \gamma_{\mathbf{FOIA}} = 0.014$ Conviction effect = 1.4 and corruption effect = 0.60					
Conviction	t = 2 to t = 7	0.9	1.09	1.13	1.33	1.38
Corruption	t = 2 to t = 7	0.9	0.84	0.83	0.77	0.85
Conviction	t = 2 to t = 7	0.7	0.9	0.93	1.25	1.39
Corruption	t = 2 to t = 7	0.7	0.93	0.91	0.69	0.75
Conviction	t = −1 to t = 5	0.9	1.15	1.19	1.33	1.33
Corruption	t = −1 to t = 5	0.9	0.86	0.85	0.87	1.00
Conviction	t = −1 to t = 5	0.7	0.97	1.04	1.33	1.33
Corruption	t = −1 to t = 5	0.7	0.88	0.82	0.74	0.97
Model 2	$\mathbf{N_{NoFOIA}}=.1\ \mathbf{N_{FOIA}} = 0.08\ \gamma_{\mathbf{NoFOIA}} = 0.01\ \gamma_{\mathbf{FOIA}} = 0.012$ Conviction effect = 1.2 and corruption effect = 0.80					
Conviction	t = 2 to t = 7	0.9	1.07	1.08	1.17	1.19
Corruption	t = 2 to t = 7	0.9	0.93	0.92	0.89	0.93
Conviction	t = 2 to t = 7	0.7	0.98	0.99	1.14	1.20
Corruption	t = 2 to t = 7	0.7	0.97	0.96	0.86	0.87
Conviction	t = −1 to t = 5	0.9	1.09	1.11	1.16	1.16
Corruption	t = −1 to t = 5	0.9	0.94	0.93	0.94	1.01
Conviction	t = −1 to t = 5	0.7	1.01	1.04	1.16	1.16
Corruption	t = −1 to t = 5	0.7	0.96	0.93	0.88	0.99

Notes: The table presents the expected values of the proposed estimators for various combinations of the underlying parameters and short-run estimation windows.

3. Data description and some suggestive patterns

3.1. The data

3.1.1. 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.[7] The database lists criminal convictions in Federal District Courts of federal, state, and local public employees for official misconduct or misuse of office.[8] These data are collected and reported annually by the Executive Office of U.S. Attorneys (EOUSA) of the U.S. Department of Justice (DOJ). 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 summary statistics in Table 2 for the full set of

[4] Theoretically, one could adjust for the relative biases building the estimate, but the degree of bias is complex and depends on a number of unknown parameters. Formally, $\frac{E\left[C_{\underline{t}+j+1,s}\right]}{E\left[C_{\underline{t}+j,s}\right]} = \frac{1-\alpha+\alpha\gamma_{NF}+\alpha(\gamma_F-\gamma_{NF})\alpha^j(1-\gamma_F)^j}{1-\alpha+\alpha\gamma_{NF}+\alpha(\gamma_F-\gamma_{NF})\alpha^{j-1}(1-\gamma_F)^{j-1}} < 1.$

[5] These calculations are easily performed in any spreadsheet program, given the model structure outlined above. Detailed calculations are available from the authors.

[6] For example, using time-series methods to directly fit Eq. (3) may seem like a way to construct better estimates. But the inherent non-normality of the dependent variable and the large number of zeroes in the data make such estimates extremely unstable.

[7] We obtained the data under license from TRACfed (http://tracfed.syr.edu/).

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

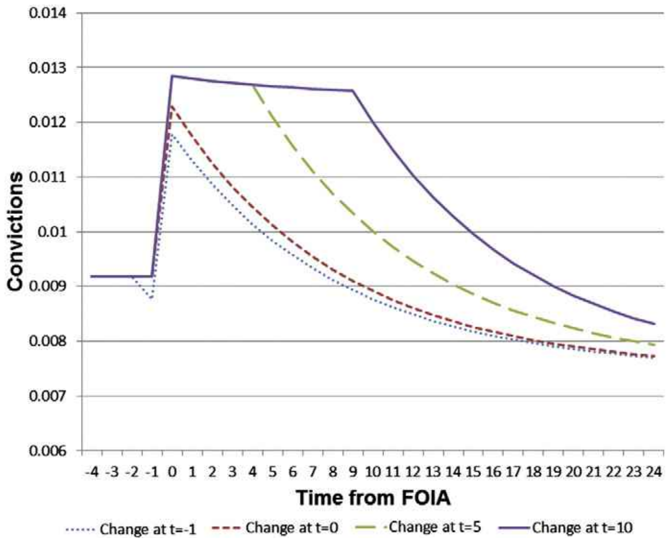

Fig. 1. Time path of convictions as a function of time since strong FOIA enaction, for four different lags at which potentially corrupt agents adjust their behavior.

Table 2
Descriptive statistics for FOIA switchers and all states.

Variable	Mean	Std. dev.	Min	Median	Max
Switchers only		$N = 12$	$T = 24$		
State and local convic.	2.99	5.17	0	0	29
Fed. convic.	4.90	7.03	0	2	37
SL convic./10 k gov. emp.	0.10	0.28	0	0	2.91
Fed. convic./10 k gov. emp.	0.18	0.21	0	0.11	1.04
Inc. Cap.	34.1	5.50	23.2	32.9	48.9
Pct. HS grad.	83.3	5.48	68.5	84.8	92.1
Jud. and legal exp cap.	42.2	23.2	12.5	39.9	100.4
Daily papers	27.4	27.2	6	18	93
Daily paid circ.	925.9	969.5	161	412	3181
TV stations	44.3	40.4	10	33.5	174
News/talk radio	16.2	12.4	1	13	52
Divided gov	0.48	0.50	0	0	1
Years in power	3.15	4.57	0	1	19
Lost power	0.076	0.27	0	0	14
Dem. Senate	0.42	0.47	0	0	1
Dem. House	0.40	0.47	0	0	1
Dem. Govern.	0.40	0.49	0	0	1
All states		$N = 50$	$T = 24$		
State and local convic.	3.38	5.69	0	1	45
Fed. convic.	6.33	11.0	0	3	83
SL convic/10 k gov. emp.	0.12	0.23	0	0.043	2.91
Fed. convic/10 k gov. emp.	0.19	0.23	0	0.13	2.84
Inc. cap.	35.8	6.56	23.2	34.9	62.6
Pct. HS grad.	83.2	4.88	68.5	84	92.8
Jud. and legal exp cap.	52.6	40.5	10.1	43.6	350.4
Daily papers	30.1	23.7	2	23	108
Daily paid circ.	1124.6	1347.7	86	673.2	6985
TV stations	45.5	29.7	5	44	174
News/talk radio	19.5	13.9	1	15	63
Divided gov	0.59	0.49	0	1	1
Years in power	3.82	8.45	0	0	44
Lost power	0.073	0.26	0	0	1
Dem. Senate	0.53	0.49	0	1	1
Dem. House	0.57	0.48	0	1	1
Dem. Govern.	0.45	0.49	0	0	1

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). Income per capita data are from the Bureau of Economic Analysis. Pct. HS Grad. is the share of the population aged 25 and up with a high school diploma or higher. Public employment and judicial & legal expenditures data are from the U.S. Census Bureau. Daily newspapers and paid circulation data are from the Statistical Abstract of the United States. TV and news/talk radio stations data are from the Broadcasting Yearbook.

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.[9] Because we examine FOIA laws by state, it is important to have a breakdown of convictions by level of government. State FOIA laws should not affect convictions of federal officials, so we use the number of corruption convictions of federal employees for a placebo test.

We believe that corruption conviction data from TRACfed is superior to that provided by the Public Integrity Section (PIN) of the DOJ. Although the PIN data have been used extensively in prior research (see, e.g., Glaeser and Saks (2006), Leeson and Sobel (2008), Cordis (2009)), they do not differentiate between convictions of federal, state, and local employees. This is a problem for our analysis. Data quality is also an issue. A recent study by Cordis and Milyo (2014) raises a number of concerns about the reliability of the PIN data of.[10] Because the TRACfed data are provided through a subscription service, there is an incentive for the provider to establish and maintain a reputation for delivering a high-quality product.[11]

Some corruption cases are prosecuted in state rather than federal court, and hence the resulting convictions do not show up in either the TRACfed database or in the PIN convictions series. However, recently developed evidence from Cordis and Milyo (2014) suggests that only a small percentage of corruption cases fall into this category. The authors conduct a detailed comparison of the PIN and TRACfed data. This includes conducting a search of media reports for any mention of state and local prosecutions of public corruption. Cross checking the results of this search against the prosecutions listed in the TRACfed database suggests that over 95% of corruption cases that involve public officials over the 1986 to 2010 period are brought in federal court.[12]

3.1.2. FOIA laws data

Data on state FOIA laws are obtained 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.[13] The guide, which is prepared

[9] These include convictions under the Hobbs Act (18 U.S.C. 1951) for bribery that "obstructs, delays or affects interstate commerce" (the most common charge, about a quarter of all convictions), convictions for theft and bribery in programs receiving federal funds (18 U.S.C. 666) (second most common charge), as well as various convictions for fraud, conspiracy, false statements, bribery, conflict of interests, and so on.

[10] For example, aggregating the convictions listed by judicial district in Table 2 of the annual PIN report to Congress produces figures that are strikingly different than the aggregate number of convictions listed in Table 3 of the same report for many of the years prior to 1994. In addition, the aggregate PIN convictions series contain many more convictions than are present in the aggregate yearly numbers obtained from the statistical report of the EOUSA. After conducting an extensive investigation that includes checking news reports to find unreported convictions of public officials, Cordis and Milyo (2014) uncover no evidence to suggest that corruption convictions are missing from the EOUSA data (or from the TRACfed data, which are in close agreement with the EOUSA data). These findings raise questions as to whether the PIN conviction series is contaminated by convictions that are unrelated to public corruption.

[11] See Long et al. (2004) for a description of strategies used by TRAC employees to ensure data quality.

[12] Augmenting the TRACfed convictions data with the data on convictions of state and local officials in state courts collected from media reports by Cordis and Milyo (2014) has a negligible effect on our results.

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

by volunteer attorneys who are experts in open government laws in their respective states, contains information on state statutes, case law, and Attorney 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 record statutes, amended existing statutes, or 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 Attorney 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.[14] 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, the state of Iowa also had few statutory provisions to guarantee access to information 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.[15]

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 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.[16] On the basis of the scores, we divide the states into "strong FOIA" states (a score above 6) and "weak FOIA" states (a score between 0 and 6).[17] 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 charged by local business that provide duplication services, 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 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 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

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

[15] 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.

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

[17] 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.

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.[18]

By our metric, 12 states switched from weak to strong FOIA during our sample period: New Hampshire in 1987, South Carolina in 1988, Idaho in 1991, Utah in 1993, Washington in 1993, West Virginia in 1993, New Mexico in 1994, Texas in 1996, North Dakota in 1998, Nebraska in 2001, New Jersey in 2002, and Pennsylvania in 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 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 Attorney 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.[19] Nonetheless, stronger formal rules should be associated with better practical access to public records, especially in a country such as the United States that has a well-functioning legal system. In a 2002 survey of 191 investigative journalists across the United States, 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 Fig. 2, 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.

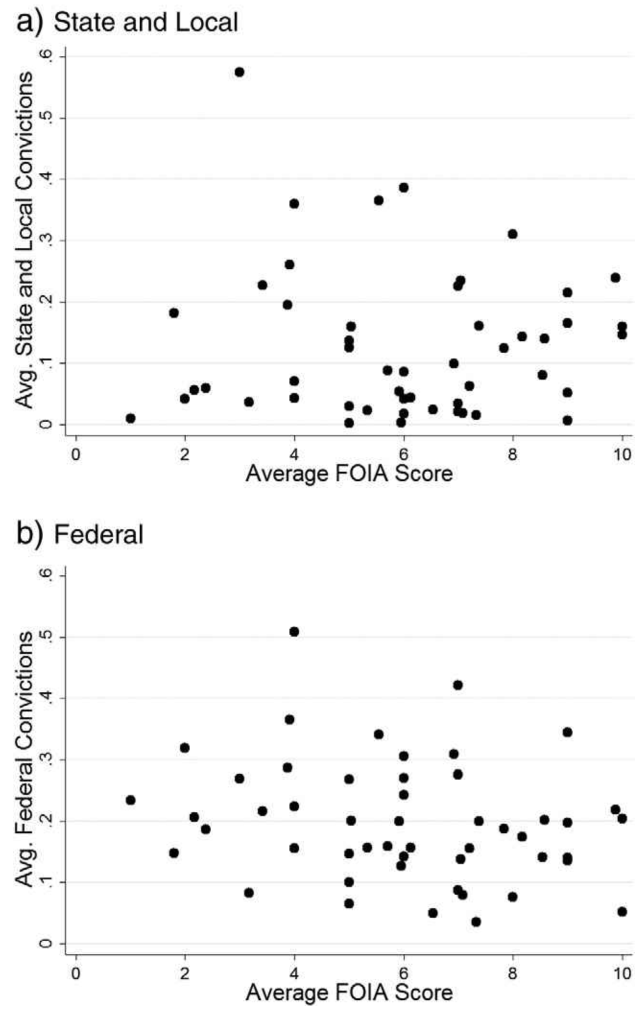

Fig. 2. Average convictions per 10,000 government employees and average FOIA score, 1986–2009.

There are two things to take away from this preliminary look at the data. First, the 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, 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. Fig. 2 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 Fig. 2. 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.

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[18] 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.

[19] 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.

To detect the transitory changes in conviction rates associated with strengthening FOIA laws, we align the data in event rather than calendar time. Fig. 3 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 Fig. 3 suggest a change in state and local convictions around the time stronger FOIA provisions were enacted, and whatever drives this change has no apparent effect on federal convictions. As noted earlier, 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. Thus any large and distinct changes in the conviction rates of federal officials would be worrisome, because this would imply that something else was changing alongside FOIA that affected corruption more generally.

The contrast between the two graphs in Fig. 3 is certainly suggestive of a FOIA effect, but some care needs to be taken before we draw any firm conclusions about these differences. It is important to remember that 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.

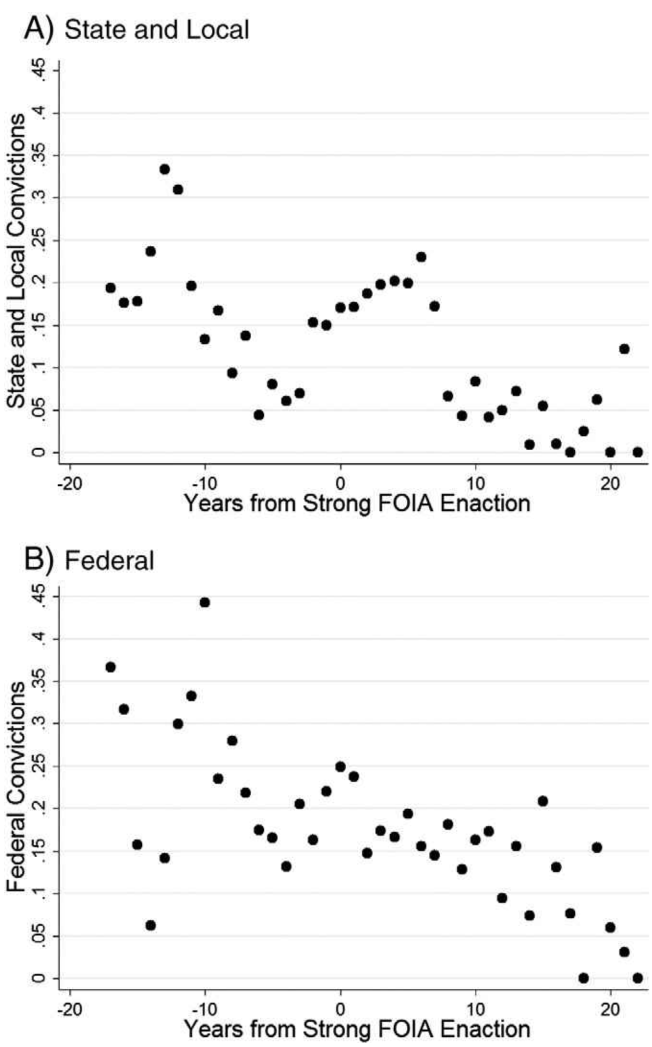

Fig. 3. Convictions per 10,000 government employees for the states that switched to strong FOIA, before and after the switch.

3.3. Endogeneity issues

If the decision to enact a strong FOIA law is related to corruption, then we face additional econometric challenges. Fig. 3 suggests that this may be the case. The number of corruption convictions for state and local officials appears to increase in the years that immediately precede enaction of strong FOIA laws, and there is some indication of a similar increase for federal officials. These patterns suggest that the enactment of strong FOIA laws might be spurred by either a rash of corruption convictions or by some omitted factor that is correlated with convictions. The standard approach to this endogeneity problem would be to instrument for FOIA status. However, no credible instruments are readily available.[20]

Because we lack credible instruments, we use a matching strategy as our primary means of addressing endogeneity. The basic idea of matching estimators is to estimate the average effect of an intervention by comparing outcomes for the "treated" group to outcomes for an "untreated" group that is selected based on its similarity to the treated group on a set of observed characteristics. The simplest form of matching pairs each member of the treated group with a single member of the untreated group. We use a more sophisticated approach that employs propensity-score matching. Specifically, we match each state that enacts a strong FOIA law with the subset of eligible control states that are most similar in terms of the predicted probability of enacting strong FOIA. Similar matching estimators have been used in a variety of settings. Recent examples include Bergemann et al. (2009) Gobillon et al. (2012) and Millard-Ball (2012).

We also employ placebo tests to provide insights on whether endogeneity is likely to be an important contributing factor to our results. The most straightforward placebo is based on the convictions of federal officials in states that enact strong FOIA laws. The federal officials are similar to state and local officials in a number of important respects, but they are not subject to state FOIA laws. If the enactment of strong FOIA laws is spurred by a surge of corruption convictions, then we might find some evidence of an elevated level of federal convictions in the first few years following enactment. However, we should not see the post-enactment patterns in the corruption and conviction effects that are predicted by our reduced-form model.

Finally, we further guard against the potential impact of endogeneity by separately estimating the corruption rate for the years that we think will be most heavily influenced by endogeneity, and excluding that "enaction period" estimate from our estimation of the corruption and conviction effects. These are the years immediately surrounding the date that the strong FOIA law is enacted. The primary concern is reverse causality, where a rash of high-profile corruption cases could have led to the adoption of strong FOIA. In fact, the convictions under such circumstances might not take place until after the year of adoption. It can take over a year for cases to progress from initial reports in the media to

[20] Costa (2013) constructs an instrument in the cross-country setting by showing that a country with neighbors who have a FOIA law is more likely to have a FOIA law itself. We investigated a similar approach for estimating the overall average effect of enacting a strong FOIA law. 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. A log-linear specification of the instrument $((log(1 + \#\,neighborswithFOIA/\#\,neighbors)))$ gives the strongest first-stage results, with a clustered F-statistic on the excluded instrument of about 4 in the unweighted model and 7 in the employment-weighted model. The 2SLS estimate for the effect of strong FOIA on the conviction rates of state and local officials is similar to that in column (3) of Table 3 (0.088 versus 0.066), but it is much less precise than the FE-OLS estimate, and hence not statistically significant. Nonetheless, 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.

conviction of the perpetrators. Cordis and Milyo (2013) report that the average time from filing to conviction in corruption cases is 296 days for the years 1986 to 2011, and media disclosure for the sort of cases we have in mind could even predate the filing of charges. Thus we also include the year after adoption in the enaction period.[21]

Although none of our measures provides a perfect solution to concerns about endogeneity, we believe the combination of measures is effective in addressing the substantial empirical challenges of disentangling the corruption and conviction effects.

4. The effects of FOIA

4.1. Empirical strategy

Moving beyond the simple analysis of mean conviction rates presented above, our primary method for identifying the relation between strong FOIA laws and corruption convictions is by fitting variants of the regression specification

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

where *ConvicRate* measures the number of corruption convictions per 10,000 government employees, **y** is a vector of dummy variables that delineates time windows in the pre- and post-enactment periods for the strong FOIA laws, and **x** contains our controls: state income per capita, state-level educational attainment measured by the share of population aged 25 and up with a high school diploma or higher, state judicial and legal expenditures per capita, four measures of media penetration (log count of TV stations, log count of News/talk radio stations, log count of daily newspapers, and log daily newspaper paid circulation), and six political variables (a dummy for divided government, the number of years of unified government, a dummy for a year in which unified government ended, and dummies for Democratic party control of each chamber of the legislature and of the governorship).[22] The γ_s and δ_t denote coefficients for the state and year dummies. Our preferred approach weights all of the regressions by state/year government employment, since weighting allows us to interpret our estimates as the effect on the average state and local government employee. Moreover, it delivers more efficient estimates if the error term is heteroscedastic with smaller states having higher variance. We also show the unweighted results for the main tables in which we calculate the effects of interest.

In the basic fixed-effects regressions, states that never enact strong FOIA laws serve as implicit controls for those that do. Since many of the non-adopters may be quite different from the adopting states along important dimensions, we also perform propensity-score-matched difference-in-difference (DID) regressions in order to use only the best of the non-adopters as controls. The set of 12 states that enact strong FOIA law form the "treated" group. For each treated state, we identify the four "non-treated" states that are most similar in terms of their propensity to enact a strong FOIA law in the year in which the treated state enacted its strong FOIA law. This is accomplished by using the Millard-Ball (2012) approach to propensity-score matching in a panel.

We start by fitting the Probit regression

$$StrongFOIA_{st} = \alpha + \beta year_{st} + \delta nfoia_{st} + \mathbf{x}'_{st}\lambda + \epsilon_{st}, \qquad (8)$$

where the sample is restricted to states that did not have a strong FOIA law in year *t-1*. The controls are the same as in the fixed-effects regression, but we replace the state and year fixed-effects with a linear time trend and include the fraction of neighbors who have already enacted a strong FOIA law as an additional regressor. The "control group" for each treated state is comprised of the four non-treated states whose predicted probability of strong FOIA enactment most closely matches that of the treated state for the year that it enacted strong FOIA.[23] The states in the control group are assigned a pseudo-enaction year equal to the treated state's enaction year. We then estimate a variant of Eq. (7) in which each treated state receives a weight of 1, each control state receives a weight of 0.25 for each time it appears in one of the control groups, and the pre- and post-enactment time windows are interacted with a dummy for treatment status.[24]

4.2. Descriptive results

Table 3 presents the basic results. It shows the coefficient estimates and standard errors for the model in Eq. (7) using two different time-window specifications. The first simply contrasts the pre- and post-enaction estimates of the expected conviction rates. The second breaks the pre- and post-enaction timelines into 3-year windows and allows 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 3 years. We do so for two reasons. First, there may be some 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. 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 and FOIA requesters must learn to use the system. Finally, once a corrupt act is discovered, it will take some time for the wheels of justice to turn to bring about a conviction. We know from Cordis and Milyo (2013), for example, that from 1986 to 2011, the average time from filing to conviction is 296 days, and the filing itself must be preceded by time-consuming investigation by media, police, and prosecutors. Because 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.

Column 3 contrasts the conviction rates before and after the enaction of a strong FOIA law for state and local officials. Column 4 repeats that analysis for federal officials. For state and local officials, the conviction rates are significantly higher in the years following strong FOIA enaction.[25] The difference is about .07 convictions per 10,000 government employees per year, which is about half the mean level of convictions across all states. For federal officials, there is no significant difference in conviction rates between the years before and after enaction, and the point estimate is very small. In other words, we find no evidence of a placebo effect.

Columns 1 and 2 illustrate how the conviction rates change over time. For state and local officials (column 1), there is a reasonably consistent pattern in the years preceding the enaction of strong FOIA. The

[21] If we include the first year after FOIA enaction in the short-run period or the first year before adoption in the pre period, the general pattern of the estimated coefficients is similar to that outlined below, and the differences between the two set of coefficients are not statistically significant. We also obtain a similar pattern if we extend the "enaction period" for an additional year on each side.

[22] Three of our control variables, TV stations, News/talk radio stations, and the share of population aged 25 and up with a high school diploma or higher, were not available every year, so we interpolate them. This is accomplished by fitting OLS regressions with a state dummy, time, time squared, time cubed, and a full set of interaction terms as the explanatory variables.

[23] We also tried using three controls or five controls. The results are similar.

[24] The control states obtained by propensity-score matching, with the number of occurrences in parentheses, are: Alabama (1), Arizona (4), California (1), Florida (5), Iowa (1), Minnesota (2), Mississippi (4), Montana (2), Nevada (1), North Carolina (3), Ohio (4), Oklahoma (6), Oregon (1), South Dakota (1), Tennessee (8), Wisconsin (1), and Wyoming (3).

[25] Standard errors in Table 3 and all subsequent tables are clustered by state. Panel corrected standard errors are generally smaller than those reported in the tables.

Table 3
FE-OLS regressions.

Dependent variable	sl/10 k gov.	fed/10 k gov.	sl/10 k gov.	fed/10 k gov.
	(1)	(2)	(3)	(4)
Strong FOIA			0.066*	−0.026
			(0.036)	(0.026)
11+ years before	0.030	0.011		
	(0.039)	(0.093)		
8–10 years before	0.022	0.144*		
	(0.029)	(0.081)		
5–7 years before	−0.001	0.037		
	(0.021)	(0.036)		
Enaction period	0.075	0.073		
	(0.045)	(0.081)		
2–4 years after	0.090*	−0.008		
	(0.052)	(0.038)		
5–7 years after	0.116*	0.003		
	(0.067)	(0.050)		
8–10 years after	0.046	0.025		
	(0.045)	(0.042)		
11+ years after	0.025	−0.009		
	(0.042)	(0.059)		
Divided gov.	0.003	0.024	0.009	0.025
	(0.010)	(0.016)	(0.011)	(0.015)
Years unified gov.	0.000	0.001	0.000	0.000
	(0.001)	(0.001)	(0.001)	(0.001)
Unified gov. ended	0.013	−0.013	0.013	−0.001
	(0.017)	(0.019)	(0.016)	(0.020)
GDP/cap	−0.000	0.003	−0.000	0.003
	(0.003)	(0.005)	(0.004)	(0.005)
Pct. HS grad.	−0.000	−0.005	−0.001	−0.005
	(0.003)	(0.005)	(0.003)	(0.005)
Judicial and legal exp./cap	0.000	−0.002***	0.000	−0.002***
	(0.000)	(0.001)	(0.000)	(0.001)
Dem. Senate	−0.003	−0.023	0.000	−0.017
	(0.019)	(0.018)	(0.019)	(0.020)
Dem. House	0.013	0.028	0.018	0.026
	(0.019)	(0.019)	(0.018)	(0.019)
Dem. Governor	0.014	0.030**	0.015	0.031**
	(0.011)	(0.014)	(0.011)	(0.014)
Log(daily papers)	−0.017	−0.187	−0.009	−0.166
	(0.090)	(0.122)	(0.088)	(0.129)
Log(daily paid circulation)	−0.097	−0.051	−0.096	−0.034
	(0.104)	(0.109)	(0.099)	(0.112)
Log(TV stations)	0.016	−0.015	0.008	−0.021
	(0.013)	(0.014)	(0.015)	(0.017)
Log(news/talk radio)	−0.019	0.020	−0.032	0.016
	(0.036)	(0.047)	(0.037)	(0.044)
R^2	0.40	0.31	0.39	0.30
N	1200	1200	1200	1200

Notes: The dependent variables are corruption convictions of state and local (sl) and federal (fed) officials per 10,000 government employees, respectively. All regressions include the full set of controls, state and year dummy variables, and are weighted by average state and local government employees. Standard errors, shown in parentheses, are clustered by state. *, **, and *** represent significance at the .10, .05, and .01 levels, respectively.

estimated coefficients for these three windows are statistically indistinguishable from one another and from zero. Thus there are no statistically significant deviations from the baseline. In the enaction period, conviction rates jump by about .08 and continue to grow slightly to about .12 in the 2 to 7 years after enaction. This change is both statistically and economically significant. Beyond 7 years, the conviction rates fall back to a level that is statistically indistinguishable from the baseline.

For federal officials (column 2), there is no consistent pattern in conviction rates in the years following strong FOIA enaction. The conviction rates are higher in the enaction period, which could be indicative of reverse-causality, i.e., a rash of convictions spurring enactment. But the estimate is not statistically significant (the t-statistic is about 1). The only evidence of deviation from the baseline is for 8 to 10 years before strong FOIA is enacted. The estimate for this window is positive and significant at the 10% 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% level by chance.

Table 4 presents the parallel analysis for the propensity-score-matched DID regressions. Column 3 contrasts the conviction rates before and after the enaction of a strong FOIA law for state and local officials and shows the estimated treatment effect. Column 4 repeats that analysis for federal officials. For state-and-local officials, the estimated treatment effect is positive and statistically significant at the 10% level. Thus the evidence indicates that conviction rates for state and local officials are higher in states with strong FOIA laws than in the matched control-group states. In contrast, the estimated treatment effect for federal officials is statistically indistinguishable from zero.

Columns 1 and 2 illustrate how the estimated treatment effects change over time. The general patterns are similar to those in Table 3. There is no evidence of an effect in any of the pre-enaction years. However, the estimated treatment effect is positive and statistically significant for the enaction, 2–4 years after, and 5–7 years after windows. There is no evidence of a concomitant movement in federal convictions,

Table 4
Propensity-score-matched DID regressions.

Dependent variable	sl/10 k gov.	fed/10 k gov.	sl/10 k gov.	fed/10 k gov.
	(1)	(2)	(3)	(4)
Strong FOIA			0.011	0.007
			(0.011)	(0.021)
Treat × Strong FOIA			0.056*	−0.049
			(0.031)	(0.038)
Treat × 11+ years before	0.048	0.070		
	(0.048)	(0.084)		
Treat × 8–10 years before	0.043	0.216**		
	(0.034)	(0.081)		
Treat × 5–7 years before	0.021	0.031		
	(0.031)	(0.050)		
Treat × Enaction period	0.088**	0.035		
	(0.041)	(0.086)		
Treat × 2–4 years after	0.081*	−0.051		
	(0.047)	(0.056)		
Treat × 5–7 years after	0.115*	−0.013		
	(0.056)	(0.059)		
Treat × 8–10 years after	0.041	0.011		
	(0.048)	(0.058)		
Treat × 11+ years after	0.025	−0.027		
	(0.044)	(0.072)		
2–4 years after	−0.001	0.011		
	(0.012)	(0.034)		
5–7 years after	−0.013	−0.025		
	(0.014)	(0.025)		
8–10 years after	−0.011	−0.015		
	(0.017)	(0.033)		
11+ years after	−0.042***	−0.024		
	(0.011)	(0.029)		
Enaction period	−0.021**	0.014		
	(0.009)	(0.020)		
5–7 years before	−0.019	0.019		
	(0.017)	(0.029)		
8–10 years before	−0.021	−0.061		
	(0.019)	(0.049)		
11+ years before	−0.030	−0.045		
	(0.018)	(0.037)		
R^2	0.47	0.29	0.45	0.26
Sample size	$N^{treated} = 12, T = 24$			

Notes: Regressions include treated states and four matched control states for each treated state (each control receives a weight of 0.25). The dependent variables are corruption convictions of state and local (sl) and federal (fed) officials per 10,000 government employees, respectively. All regressions include state and year dummy variables, the full set of controls, and are weighted by average state and local government employees. Standard errors, shown in parentheses, are clustered by state. *, **, and *** represent significance at the .10, .05, and .01 levels, respectively.

but the odd spike in federal convictions 8–10 years before FOIA enaction is still evident. We also see small, but statistically significant, declines in state-and-local conviction rates for the control group both during the enaction period and 11 + years after enaction, relative to the 2–4 years before enaction. This could indicate that the controls identified by propensity matching have somewhat elevated conviction levels in the years shortly preceding enaction.

Fig. 4 illustrates the timing of the estimated changes in conviction rates based on the most flexible feasible specification, which includes a dummy for each of the pre- and post-enaction years, in addition to all the normal controls. We take the third year prior to enaction as our baseline, i.e., we omit the corresponding dummy and assign this year a value of zero. We choose this year because it is the middle of the omitted categories in Tables 3 and 4. The dotted line tracks the estimated coefficients on the appropriate dummies in the baseline weighted fixed-effects model, while the solid line tracks the estimated treatment effects from the propensity score matched model.

We again see evidence of a rise in conviction rates for state and local officials in the years after FOIA enaction and then an eventual decline. In fact, the estimates look remarkably similar to the basic sample means from Fig. 2. There is also evidence of a general downward trend in convictions, especially in the unmatched estimates. This suggests that the inclusion of state-specific time trends will be important for identifying the true effects of FOIA. The plot for federal officials shows little indication of post-enaction changes in conviction rates. In both graphs, we include a histogram that shows the number of treated states that are used to estimate each of the coefficients (on the right-hand axis). Of course, the coefficients for the years right around enaction are based on the greatest number of states, while those for years far from enaction are based on relatively few states. Thus the estimates for years far from enaction are likely to be less precise.

Taken together, the results in this section point to an increase in corruption convictions, relative to trend, around the time of strong FOIA enaction and shortly thereafter, followed by a decline in the conviction

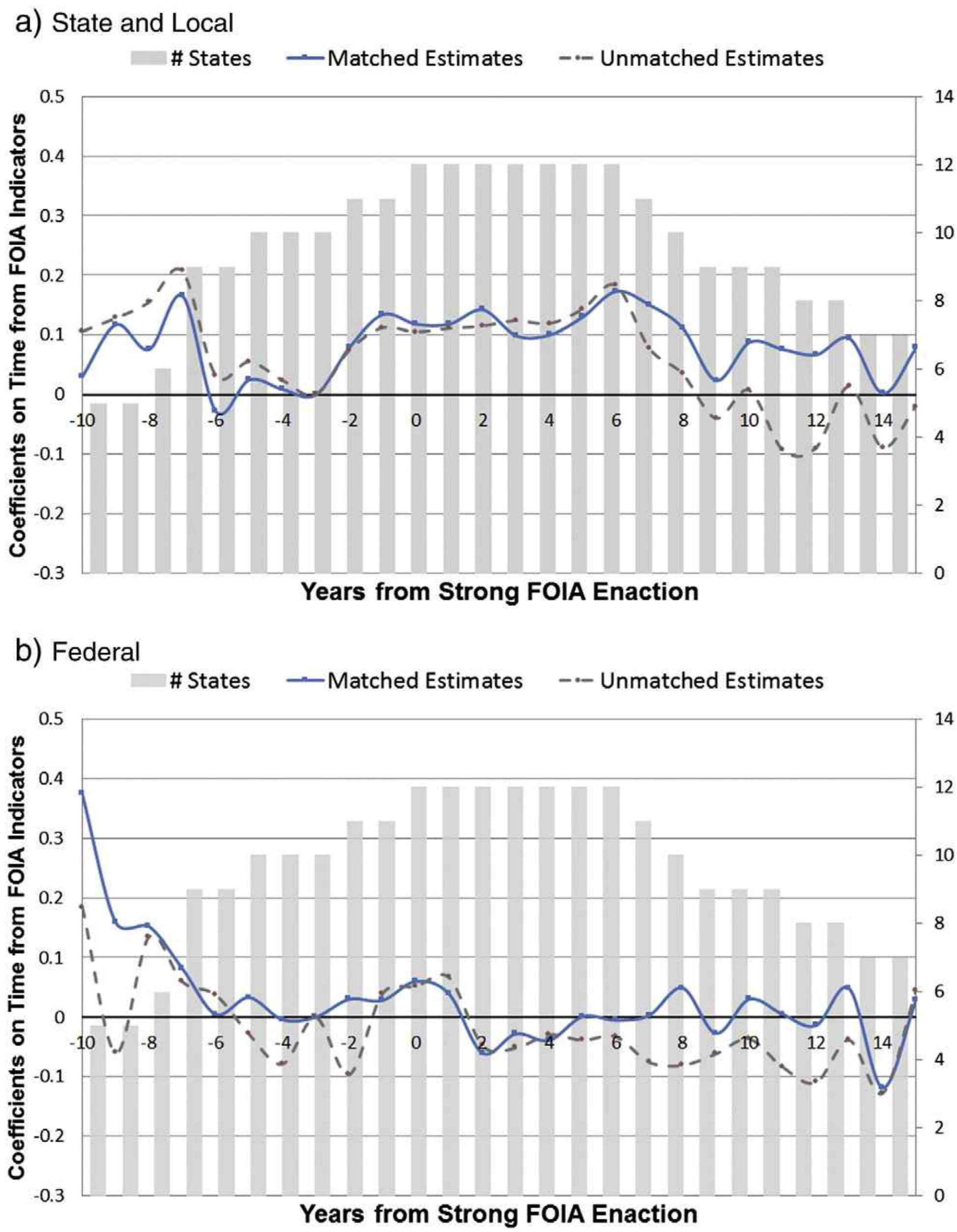

Fig. 4. Years from FOIA enaction and SL convictions/10 k gov. employees.

Table 5
Separating conviction from corruption effects using short- and long-run changes.

Dependent variable	sl/10 k gov.	fed/10 k gov.	sl/10 k gov.	fed/10 k gov.	sl/10 k gov.	fed/10 k gov.	sl/10 k gov.	fed/10 k gov.
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treat × enaction					0.037	0.040	0.077	0.026
					(0.046)	(0.079)	(0.049)	(0.078)
Treat × short run					0.084*	−0.047	0.083**	−0.012
					(0.043)	(0.089)	(0.040)	(0.071)
Treat × long run					0.047	−0.050	0.050	0.069
					(0.059)	(0.108)	(0.051)	(0.073)
Enaction period	0.021	0.074	0.076	0.067	−0.009	0.020	−0.006	0.018
	(0.043)	(0.076)	(0.064)	(0.081)	(0.011)	(0.026)	(0.010)	(0.014)
Short run	0.101**	−0.039	0.121*	0.015	0.011	−0.025	0.018**	−0.002
	(0.042)	(0.074)	(0.060)	(0.071)	(0.014)	(0.023)	(0.007)	(0.019)
Long run	0.060	−0.052	0.109	0.074	−0.023	−0.024	−0.005	−0.014
	(0.057)	(0.095)	(0.082)	(0.092)	(0.017)	(0.024)	(0.009)	(0.020)
Emp. weight	No	No	Yes	Yes	No	No	Yes	Yes
Matched	No	No	No	No	Yes	Yes	Yes	Yes
State-trends	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.43	0.29	0.48	0.37	0.45	0.28	0.53	0.34
N	1200	1200	1200	1200	$N^{treated}, T = 24$			
$\overline{C}_{Pre}$	0.083	0.171	0.092	0.176	0.083	0.171	0.092	0.176
$\frac{\overline{C}_{Short}}{\overline{C}_{Pre}}$	2.21	0.77	2.32	1.09	2.01	0.73	1.90	0.93
$\frac{\overline{C}_{Long}}{\overline{C}_{Short}}$	0.78	0.90	0.94	1.31	0.78	0.98	0.81	1.49

Notes: The dependent variables are corruption convictions of state and local (sl) and federal (fed) officials per 10,000 government employees, respectively. All regressions include the full set of controls, state and year dummy variables, and state-specific trends. Standard errors, shown in parentheses, are clustered by state. *, **, and *** represent significance at the .10, .05, and .01 levels, respectively.

rates over subsequent years. This is broadly consistent with the predictions of our reduced-form model of corruption and conviction.

4.3. 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 regression analysis back to the structure of the model in Section 2. This requires that we define what constitutes 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, but the estimates in Table 3 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 5 repeats the analysis of Tables 3 and 4 using windows that correspond to these periods. The regressions include state and year fixed-effects, the same covariates as in the previous tables, and state-specific time trends. We present both unmatched (col. 1–4) and propensity-score-matched DID (col. 5–8) regressions, and both unweighted (col. 1,2,5, and 6) and weighted (col. 3,4,7, and 8) regressions. The results of the unmatched regressions are broadly consistent with those in Table 3. The estimated coefficients for the short run are positive and statistically significant for state and local officials, but statistically indistinguishable from zero for federal officials. Similarly, the results of the matched regressions are broadly consistent with those in Table 4. The treatment effect is statistically significant in the short run for state and local officials, but not for federal officials. In short, there is nothing surprising in light of our prior findings.

To use the regression results to separate conviction from corruption, we need to construct an estimate of the baseline conviction rate. We use the average conviction rate in the adopting states for the period 2-to-7 years prior to the enactment of strong FOIA, weighting each observation by the number of government employees if the regression estimates are so weighted. The results are shown in the row labeled $\overline{C}_{Pre}$. The baseline rate is about 0.09 state and local convictions and 0.17 federal convictions per 10,000 government employees.[26] We combine these baseline conviction rates with the short-run and long-run coefficient estimates to calculate estimates of expected conviction rates in the long run and short run.

Under the assumptions of our reduced-form model, we can use these estimated expected conviction rates to bound the conviction and corruption effects. The estimated bounds are presented in the bottom two rows of the table. The evidence indicates that enaction of a strong FOIA law leads to a substantial increase in the rate at which corrupt acts of state and local officials are convicted. Depending on the specification, the estimated increase in the conviction rate is between 90 and 130%. Given that our numerical model suggests that these are actually lower bounds for the true conviction effect, the key policy implication of our analysis is clear: 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. If we assume that all of the observed change in conviction rates is due to changes in the level of corrupt behavior, then we see an estimated drop in this behavior of 6 to 22% from the elevated short-run rates. Using these figures, the analysis suggests an elasticity of supply of corruption with respect to the probability of apprehension of between −0.05 and −0.24. In the long run, therefore, we expect corruption convictions to increase when a state enacts a strong FOIA law, even though the actual underlying corruption is expected to decline.

An alternative explanation for the drop in corruption rates is that corrupt officials eventually learn to reduce their chances of being detected under FOIA via avoidance techniques.[27] Maybe they avoid

[26] Using the whole pre-enaction period to estimate $\overline{C}_{Pre}$ and using only the 2–4 years prior to enaction gives similar results.

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

Table 6
Corruption convictions and FOIA dimensions.

Dependent variable	sl/10 k gov.	sl/10 k gov.	sl/10 k gov.	sl/10 k gov.	sl/10 k gov.
	(1)	(2)	(3)	(4)	(5)
Liability	0.079**				
	(0.038)				
Money	0.021				
	(0.019)				
Time	0.026				
	(0.050)				
Discretion	0.003				
	(0.031)				
Enaction × liability		0.076***			
		(0.032)			
Short run × liability		0.187***			
		(0.055)			
Long run × liability		0.159***			
		(0.057)			
Enaction × money			0.167*		
			(0.096)		
Short run × money			0.049		
			(0.048)		
Long run × money			0.028		
			(0.037)		
Enaction × time				−0.112	
				(0.080)	
Short run × time				−0.016	
				(0.120)	
Long run × time				0.057	
				(0.149)	
Enaction × discretion					0.015
					(0.098)
Short run × discretion					0.026
					(0.043)
Long run × discretion					−0.000
					(0.053)
Enaction period		0.029	0.052	0.101	0.064
		(0.059)	(0.046)	(0.072)	(0.106)
Short run		−0.043	0.112*	0.125	0.099**
		(0.064)	(0.059)	(0.093)	(0.050)
Long run		−0.033	0.096	0.095	0.110
		(0.076)	(0.072)	(0.118)	(0.080)
State-trends	No	Yes	Yes	Yes	Yes
R^2	0.40	0.48	0.48	0.48	0.48
N	1200	1200	1200	1200	1200

Notes: The dependent variables are corruption convictions of state and local (sl) officials per 10,000 government employees. All regressions include the full set of controls, state and year dummy variables, and are weighted by average state and local government employees. Standard errors, shown in parentheses, are clustered by state. *, **, and *** represent significance at the .10, .05, and .01 levels, respectively.

creating written records or destroy extant records. This would lead us to overstate the size of the deterrent effect. Although we cannot assess the extent to which this is an issue without an independent measure of avoidance behavior, most reasonable models of avoidance predict that 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.

5. Heterogenous FOIA effects

5.1. Differences in FOIA statutes adopted

To provide additional insights on the effects of FOIA, we extend our analysis in two ways. First, we decompose our measure of the strength of state FOIA laws into four components — liability, time, money, and discretion — and investigate which of the components have the greatest impact on conviction rates for state and local officials. The liability component measures the levels of civil and criminal penalties to which those who violate the FOIA provisions are subject, the time component measures the limitations on the time allowed to respond to FOIA requests, the money component measures the allowable fees, and the discretion component measures the strength of limitations on the discretion of officials in providing requested information.[28]

Table 6 presents the results of the regression analysis. All of the regressions include the full set of controls and are weighted by average state and local government employees. The first column reports results for specifications that do not condition on switching from weak overall FOIA to strong overall FOIA. In other words, we simply regress the conviction rate on the component dummy, so the analysis is not confined to the switcher states. The estimates for these unconditional specifications clearly suggest that liability is the most important dimension of FOIA. The estimated coefficient on the liability dummy is positive and statistically significant. None of the other provisions produce statistically significant results.

Columns 2 through 5 report the results for specifications that condition on switching from weak overall FOIA to strong overall FOIA. In each

[28] Formally, *liability* = 1 if the law provides for at least two elements of {civil penalties, criminal penalties, award of attorney fees}, *time* = 1 if the law requires response in 7 days or less, *money* = 1 if the law provides for at least two elements of {fee exemptions for public interest, a prohibition of charging for record-gathering time, an overall limit on fees}, and *discretion* = 1 if the law provides for at least two elements of {a presumption for disclosure, an administrative appeal procedure, no generic public interest exemption from disclosure}.

Table 7
Corruption convictions and the magnitude of FOIA policy change.

Dependent variable	sl/10 k gov.	fed/10 k gov.	sl/10 k gov.	fed/10 k gov.	sl/10 k gov.	fed/10 k gov.	sl/10 k gov.	fed/10 k gov.
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treat × strong FOIA							0.104***	−0.035
							(0.027)	(0.034)
Treat × strong FOIA × small							−0.092*	−0.027
							(0.054)	(0.042)
Strong FOIA					0.133***	0.001	0.014	0.012
					(0.028)	(0.025)	(0.012)	(0.021)
Strong FOIA × small change					−0.123**	−0.050	−0.003	−0.008
					(0.047)	(0.048)	(0.004)	(0.006)
Treat × short run			0.167***	−0.069				
			(0.057)	(0.062)				
Treat × long run			0.164*	−0.077				
			(0.085)	(0.114)				
Treat × short Run × small			−0.151*	0.101				
			(0.079)	(0.127)				
Treat × long run × small			−0.193	0.215				
Short run	0.235***	−0.026	0.007	0.010				
	(0.057)	(0.098)	(0.009)	(0.017)				
Long run	0.274***	−0.020	−0.004	−0.016				
	(0.089)	(0.167)	(0.009)	(0.019)				
Short run × small change	−0.225***	0.079	0.014	−0.019				
	(0.072)	(0.148)	(0.010)	(0.013)				
Long run × small change	−0.310***	0.153	−0.005	0.000				
	(0.106)	(0.206)	(0.004)	(0.005)				
Emp. weight	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Matched	No	No	Yes	Yes	No	No	Yes	Yes
State-trends	Yes	Yes	Yes	Yes	No	No	No	No
R^2	0.48	0.37	0.53	0.34	0.40	0.30	0.46	0.26
N	1200	1200	$N^{treated} = 12, T = 24$		1200	1200	$N^{treated} = 12, T = 24$	

Notes: The dependent variables are corruption convictions of state and local (sl) and federal (fed) officials per 10,000 government employees, respectively. Small Change is a dummy indicating a less than 4 points change in the FOIA score at the time a state switches from weak to strong FOIA law. All regressions include the full set of controls and 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.

case, we include dummies for the enaction, short-, and long-run periods, and interact each of these dummies with the dummy for the dimension of FOIA under consideration. With the exception of the presence of the interaction terms, the specifications are identical to the one described in column 1 of Table 5. If the estimated coefficient for one or more of the interaction terms is statistically significant, then we conclude that switcher states that are strong on the indicated FOIA dimension have a different conviction rate during the associated period than otherwise strong FOIA states without strength in that particular dimension.

Once again the results point to liability as the most important dimension of FOIA. The estimated coefficients on both the short- and long-run interaction dummies are positive and statistically significant, while the estimated coefficients for the non-interacted short- and long-run dummies are statistically insignificant. This pattern suggests that the impact of strong FOIA enaction on the corruption rate of state and local officials in switcher states is largely confined to the subset of states that put FOIA responders at a real risk of loss for ignoring requests. We also find some mild evidence that the money dimension of FOIA matters for the switcher states, although only the enaction period is statistically significant. Neither of the other two provisions seem particularly important.

We also investigate whether the magnitude of the policy change that causes a state to cross the strong FOIA threshold plays a role in our findings. Some states, such as South Carolina, moved from a law that scored just below our strong FOIA cutoff to one that scored just above it, while other states, such as Pennsylvania, had much more dramatic changes. By splitting the sample of 12 switcher states in half, we can obtain evidence on whether the changes in conviction rates for states whose FOIA score increased by less than four points (small changers) are less marked than for states with more dramatic increases in transparency.

Table 7 presents the results of this analysis for both the FE-OLS and propensity-score matched specifications. The evidence suggests that the observed changes in conviction rates are primarily driven by those states with large changes to their FOIA policy. There seems to be no statistically significant movement in conviction rates for the other switcher states in any of the periods.[29] We view this finding as qualitatively consistent with the predictions of our reduced-form model. Because the states with large policy changes had very low FOIA scores prior to the change (scores of 1, 1, 1, 2, 2, and 4), our model suggests that these states probably had a larger stock of corrupt acts (on a per government employee basis) than the small-changer states. We would therefore expect the heightened scrutiny that follows enaction of a strong FOIA law to generate a more dramatic rise in conviction rates for these states than for the small-changer states.

5.2. Crimes and perpetrators affected

In addition to exploring differences in FOIA laws along the liability, time, money, and discretion dimensions, we can also dig more carefully into the effects of the laws along other dimensions. We focus on identifying the type of corrupt acts and the categories of potentially corrupt officials that seem to be most affected by the enaction of strong FOIA laws.

First, we identify the most frequent offenses in the convictions data, sort them into “high-penalty” and “low-penalty” categories using the severity of the penalties prescribed by law, and investigate how each category responds to strong FOIA enactment. The frequency analysis is based on the lead charge specified by prosecutors. The four most common lead charges for state and local officials are Hobbs Act (18 USC 1952), Theft and Bribery in Programs Receiving Federal Funds (18 USC 666), Frauds and Swindles (18 USC 1341), and Conspiracy to Commit Offense or to Defraud the U.S. (18 USC 371). Offenses under Hobbs Act and Frauds and Swindles are classified as high-penalty crimes because

[29] We omit the enaction-period dummies from the table for readability in the presence of triple-interactions. Neither the direct nor interacted enaction-period estimates are significantly related to convictions.

they are punishable with fines and/or imprisonment of up to 20 years and fines and/or imprisonment for up to 30 years, respectively. Offenses under Theft and Bribery in Programs Receiving Federal Funds and Conspiracy to Commit Offense or to Defraud the U.S. are classified as low-penalty crimes because they are punishable with fines and/or imprisonment of up to 10 years and fines and/or imprisonment of up to 5 years, respectively.

Table 8 presents the results of this analysis. We include the full set of controls in all of the regressions and weight by average state and local government employees. Columns 1 and 2 report the estimated coefficients for the unmatched sample. For the high-penalty offenses, the estimated coefficients grow with the time since enaction, but only the long-run coefficient is statistically different from zero. The regression for the low-penalty offenses does not produce any statistically significant results, although the pattern is consistent with the predictions of our model. Columns 3 and 4 report the estimated coefficients for the matched sample. For the high-penalty offenses, the estimated treatment effect is positive and statistically significant for all three periods. As in the unmatched sample, the regression for the low-penalty offenses does not produce any statistically significant results, but the pattern matches our predictions.

One potential concern with the convictions measure is that prosecutors have limited resources to pursue corruption cases. Consequently, they may be more likely to file cases against high-profile officials because these cases generate publicity that is helpful to their careers, which could lead to low-level corruption being ignored. Unfortunately, it is difficult to investigate this hypothesis because the TRACfed data do not contain the job title or position of the officials convicted for corruption. A distinction that might prove useful, however, is whether the official is employed by the state or local government. In general, we would expect state officials to have a higher profile than local officials. Although this is not wholly satisfactory, it is the best we can do using the available data.

Table 9 fits our basic specifications for state officials and local officials, separately. In fact, the specifications for state officials produce little evidence that enacting strong FOIA has an impact on corruption convictions. The estimated effect for the long-run period is statistically significant using our preferred specification, but it is negative rather than positive. This finding offers some reassurance that prosecutors are not just targeting high-profile state officials. However, it raises the question of why the results for state and local officials are so different. A closer look at the data reveals that there is simply a paucity of convictions for state officials. This suggests that tests using only convictions of state officials may suffer from a sample-size problem. The fact that local officials make up the majority of convictions of state and local officials is not that surprising if both categories of officials engage in corruption at similar rates. Typically, the number of local officials in a state is much larger than the number of state officials.

Another possibility is that corrupt state officials are simply better at avoiding detection than corrupt local officials. Perhaps they are more attuned to the potential for exposure that arises from the strong FOIA law, or they engage in more types of avoidance behavior. Alternatively, there may be some sense in which the average corrupt state official is more sophisticated than the average corrupt local official, and therefore engages in the type of corrupt acts that are more easily hidden. Of course the limitations of the data make it impossible to tests such hypotheses.

5.3. Strong media and FOIA efficacy

In our discussion of the reasons why FOIA legislation might be important, we emphasize the role of a watchdog media in exposing corruption. Hence an obvious question is whether the data provide empirical support for a media effect. To address this question, we construct an indicator variable that is designed to identify the subset of switcher states that have the most intense media coverage. We construct this

Table 8
Corruption convictions by lead charge.

Dependent variable	High penalty per 10 k gov.	Low penalty per 10 k gov.	High penalty per 10 k gov.	Low penalty per 10 k gov.	High penalty per 10 k gov.	Low penalty per 10 k gov.	High penalty per 10 k gov.	Low penalty per 10 k gov.
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treat × strong FOIA							0.015	0.032
							(0.011)	(0.022)
Strong FOIA					0.013	0.038*	0.005	0.010**
					(0.020)	(0.022)	(0.008)	(0.004)
Treat × enaction			0.032*	0.025				
			(0.016)	(0.027)				
Treat × short run			0.066***	0.040				
			(0.022)	(0.026)				
Treat × long run			0.073***	0.015				
			(0.023)	(0.041)				
Enaction period	0.017	0.031	−0.006	0.003				
	(0.017)	(0.036)	(0.005)	(0.004)				
Short run	0.059	0.053	0.005	0.007**				
	(0.037)	(0.034)	(0.005)	(0.003)				
Long run	0.088**	0.035	−0.000	0.004				
	(0.042)	(0.050)	(0.004)	(0.004)				
Emp. weight	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Matched	No	No	Yes	Yes	No	No	Yes	Yes
State-trends	Yes	Yes	Yes	Yes	No	No	No	No
R^2	0.37	0.34	0.47	0.39	0.31	0.26	0.42	0.32
N	1200	1200	$N^{treated} = 12, T = 24$		1200	1200	$N^{treated} = 12, T = 24$	
$\overline{C}_{Pre}$	0.036	0.021	0.036	0.021				
$\frac{\overline{C}_{Short}}{\overline{C}_{Pre}}$	2.64	3.52	2.83	2.90				
$\frac{\overline{C}_{Long}}{\overline{C}_{Short}}$	1.31	0.76	1.07	0.59				

Notes: The dependent variables consist of the number of state and local officials convicted for corruption under the most common lead charges in the dataset per 10,000 government employees. “High Penalty” includes the Hobbs Act (18 USC 1951) and Frauds and Swindles (18 USC 1341), punishable with fines and/or imprisonment for up to 20 years (30 years for violations related to major disasters or emergencies under 18 USC 1341). “Low Penalty” includes Theft or Bribery in Programs Receiving Federal Funds (18 USC 666) and Conspiracy to Commit Offense or to Defraud US (18 USC 371), punishable with fines and imprisonment for up to 10 years and fines and imprisonment for up to 5 years, respectively. All regressions include the full set of controls and 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 9
Corruption convictions, separating state vs. local officials.

Dependent variable	State conv./ 10 k gov.	Local conv./ 10 k gov.	State conv./ 10 k gov.	Local conv./ 10 k gov.	State conv./ 10 k gov.	Local conv./ 10 k gov.	State conv./ 10 k gov.	Local conv./ 10 k gov.
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treat × enaction					−0.008	0.045	−0.010	0.086*
					(0.022)	(0.057)	(0.017)	(0.046)
Treat × short run					−0.031	0.114***	−0.030	0.113***
					(0.029)	(0.035)	(0.020)	(0.033)
Treat × long run					−0.045	0.092	−0.04*	0.095**
					(0.035)	(0.060)	(0.022)	(0.043)
Enaction period	−0.007	0.028	0.003	0.073	−0.001	−0.008	−0.003	−0.004
	(0.026)	(0.053)	(0.014)	(0.057)	(0.004)	(0.011)	(0.004)	(0:010)
Short run	−0.013	0.115***	0.006	0.115**	0.012**	−0.001	0.011***	0.007
	(0.030)	(0.036)	(0.022)	(0.049)	(0.004)	(0.012)	(0.004)	(0.006)
Long run	−0.031	0.090	0.002	0.107	−0.000	−0.023	−0.002	−0.002
	(0.035)	(0.062)	(0.029)	(0.066)	(0.004)	(0.017)	(0.004)	(0.009)
Emp. weight	No	No	Yes	Yes	No	No	Yes	Yes
Matched	No	No	No	No	Yes	Yes	Yes	Yes
State-trends	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.25	0.41	0.26	0.47	0.26	0.45	0.26	0.52
N	1200	1200	1200	1200	$N^{treated} = 12, T = 24$			
$\overline{C}_{Pre}$	0.03	0.053	0.024	0.068	0.03	0.053	0.024	0.068
$\frac{\overline{C}_{Short}}{\overline{C}_{Pre}}$	0.57	3.17	1.25	2.69	−0.03	3.15	−0.25	2.66
$\frac{\overline{C}_{Long}}{\overline{C}_{Short}}$	−0.06	0.85	0.87	0.96	15.00	0.87	3.50	0.90

Notes: The dependent variables are corruption convictions of state officials and local officials per 10,000 government employees, respectively. All regressions include the full set of controls and 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.

variable using the distributions of the average number of daily newspapers and average daily newspaper circulation in the switcher states.[30] Specifically, the indicator is set equal to 1 if a state is in the upper 50% of *both* distributions and 0 otherwise. We assess the impact of media intensity on corruption convictions by interacting the indicator with the post-enactment time windows in our regression specifications. Table 10 reports the results.

The non-matched specifications (Columns 1 & 2) produce the only statistically significant results. The estimated coefficients on the interactions between the media-intensity dummy and the short- and long-run windows are positive and statistically significant at 10%, and the magnitude of the estimates suggests that the effects of FOIA are amplified by media intensity. Although the estimated coefficients on these interactions are also positive for the matched regressions (Columns 3 & 4), they do not rise to the level of statistical significance. Thus there is weak evidence that points to the presence of a media-intensity effect, but we do not estimate the effect with enough precision to give strongly significant results. To some extent, this probably reflects the inherent limitations of trying to identify it using a very small subset of states (fewer than half of the 12 states that switch from weak to strong FOIA status during our sample period).

The lack of a good measure of the intensity of media coverage is another potential reason for the relatively weak evidence of a media effect. In general, we would expect vigorous investigative reporting to be the hallmark of an effective watchdog media. We use the average number of daily newspapers and average daily newspaper circulation as proxies for this characteristic, but this approach probably achieves fairly limited success. Unfortunately, it is very difficult to construct good measures of the intensity of media coverage using publicly available data.

Overall our more in-depth analysis of the effects of FOIA points to variation in corruption conviction rates across states that arises from heterogeneity in both FOIA laws and media markets, and to differential effects of FOIA laws across types of corruption-related crimes and categories of public officials. The effects of strong FOIA enaction are more pronounced for states that enact larger policy changes, include provisions that make respondents liable for failing to properly disclose, and have stronger print media. In addition, they seem to be larger for more serious crimes, and to affect the behavior of local officials more so than state officials. Although these extensions to our baseline analysis produce a smaller contrast between the short-run and long-run convictions rates, this may just be an indication that the long run is particularly difficult to estimate when we split the relatively small treated sample along additional dimensions.

6. Robustness

We perform a variety of robustness checks to instill greater confidence that our results are not driven by data deficiencies, modeling assumptions, or endogeneity issues. In the interests of space, we keep our discussion of our methodology and findings relatively brief, and provide a set of detailed tables in the Online Appendix.

6.1. Placebo tests

The placebo analysis using federal officials has been discussed already. It is worthwhile, however, to consider the implications of the analysis in more detail. By fitting the regressions to conviction data for federal officials, we obtain evidence on the response of individuals who are similar to state and local officials in a number of important respects, but are not subject to state FOIA laws. The results are consistent across all regressions: conviction rates in the pre-enaction period are statistically indistinguishable from those in the enaction and post-enaction periods. Moreover, if we use the regression results for federal officials to estimate the conviction and corruption effects, there is no discernible pattern across regressions. Sometimes the estimated conviction effect is positive (greater than 1) and sometimes it is negative, and when it is positive, its magnitude is much smaller than that for state and local officials. Similar variation is observed for the estimated corruption effect.

These findings are hard to reconcile with scenarios in which something besides the enaction of strong FOIA laws is driving our results. Suppose the pattern of corruption and conviction effects for the state and local officials is due to law enforcement devoting additional resources to rooting out corruption. In this case we should see a similar pattern for federal officials. Suppose the pattern for state and local

[30] The newspaper data are obtained from the Statistical Abstract of the United States, available at www.census.gov.

Table 10
Corruption convictions and the media.

Dependent variable	sl/10 k gov.	fed/10 k gov.	sl/10 k gov.	fed/10 k gov.	sl/10 k gov.	fed/10 k gov.	sl/10 k gov.	fed/10 k gov.
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treat × strong FOIA							0.010	−0.073
							(0.084)	(0.082)
Treat × strong FOIA × media							0.064	0.034
							(0.086)	(0.078)
Strong FOIA					0.007	−0.060	0.012	0.008
					(0.086)	(0.081)	(0.010)	(0.021)
Strong FOIA × big media					0.074	0.043	−0.005	−0.005
					(0.089)	(0.081)	(0.007)	(0.007)
Treat × short run			0.007	−0.078				
			(0.047)	(0.091)				
Treat × long run			−0.045	−0.025				
			(0.054)	(0.094)				
Treat × short run × media			0.110	0.106				
			(0.065)	(0.087)				
Treat × long run × media			0.147	0.168				
			(0.097)	(0.134)				
Short run	−0.005	−0.047	0.016*	−0.005				
	(0.050)	(0.084)	(0.009)	(0.017)				
Long run	−0.065	0.008	−0.003	−0.016				
	(0.061)	(0.068)	(0.009)	(0.022)				
Short run × big media	0.171*	0.086	0.003	−0.009				
	(0.091)	(0.123)	(0.020)	(0.023)				
Long run × big media	0.247*	0.100	−0.006	−0.009				
	(0.132)	(0.161)	(0.007)	(0.013)				
Emp. weight	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Matched	No	No	Yes	Yes	No	No	Yes	Yes
State-trends	Yes	Yes	Yes	Yes	No	No	No	No
R^2	0.48	0.37	0.53	0.35	0.40	0.30	0.45	0.26
N	1200	1200	$N^{treated} = 12, T = 24$		1200	1200	$N^{treated} = 12, T = 24$	

Notes: The dependent variables are corruption convictions of state and local (sl) and federal (fed) officials per 10,000 government employees, respectively. Big Media is a dummy indicating states in the upper 50% of both distributions of the average number of daily newspapers and of the average daily newspaper circulation in the switcher states. All regressions include the full set of controls and 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.

convictions reflects a reallocation of law enforcement resources to target state and local corruption. In this case we should see convictions of federal officials moving in the opposite direction from convictions of state and local officials, because both types of corruption are prosecuted (and to a large extent pursued) by the same justice officials. Similarly, if the pattern for state and local officials is due to changes in demand for corruption that are correlated with the enaction of strong FOIA laws, then we should see a shift in the pattern for federal officials, with the direction of the shift depending on whether a corrupt federal official is a substitute or a complement for a corrupt state and local official. The lack of any relation between the enaction of strong FOIA laws and the conviction rate for federal officials makes each of these alternatives implausible.

Table 11 presents the results of another type of placebo analysis: a timing placebo. We construct the table by assuming (counterfactually) that each state enacted its strong FOIA law 5 years after the actual enaction date.[31] The idea is to see whether the pattern of estimated corruption and conviction effects under the counterfactual is similar to that obtained for the actual enaction date. In fact, they look very different. None of the regressions produce statistically-significant treatment effects for state and local officials (or federal officials). In addition, the estimated corruption and conviction effects are usually quite small and frequently go the "wrong" way, with estimated conviction rates actually falling in the short run and then rising from the short-run level over time.

The basic message that we take away from the placebo tests is that they produce no evidence of the pattern in conviction rates predicted by our reduced-form model. This reinforces our belief that the regression evidence is reliable. We see the pattern of conviction rates that our model predicts, at the time that it predicts, for the type of officials that it predicts. The predicted pattern is not evident at other times or for other types of officials.

6.2. Other robustness tests

Some might be concerned that the conviction data are too narrow a measure of official corruption because the standards of proof required to achieve a conviction are quite high. As an alternative, we can use prosecutorial filings for corruption cases. This measure, which is readily available from the TRACfed database, is broader than convictions because it also includes cases that either result in an acquittal or are dismissed.

If we use filings rather than convictions to fit the models described in Table 5, the results do not alter our general conclusions regarding the FOIA effect. For our preferred specification, which includes state-specific trends and uses both propensity-score matching and government employees weights, the strong FOIA treatment effect for state and local officials is significant across all treatment periods. The estimated coefficient on the control short-run dummy is also significant, which provides an indication that factors unrelated to FOIA contribute to the rise in corruption convictions for this period. However, the magnitude of the estimated coefficient suggests that this effect is relatively small.

[31] Under the usual approach to a timing placebo, we would assume that each state enacted its strong FOIA law *before* the actual enaction date. We depart from this approach because many states enacted their strong FOIA laws in the late 1980s and early 1990s. Consequently, they would drop out of the analysis (become strong-FOIA states for the entire sample under the placebo) if we assumed that enaction occurred 5 years before actual enaction. This would not leave enough data to reliably estimate the corruption and conviction effects. We face a similar problem for assumed enaction dates more than 5 years after actual enaction. The assumed enaction date is not critical for our purposes because the analysis focuses on the *pattern* of corruption and conviction effects. In other words, we use the placebo to assess whether the observed pattern for state and local officials is likely to have occurred due to factors unrelated to FOIA.

Table 11
Separating conviction from corruption effects using short- and long-run changes (placebo-5 year).

Dependent variable	sl/10 k gov.	fed/10 k gov.	sl/10 k gov.	fed/10 k gov.	sl/10 k gov.	fed/10 k gov.	sl/10 k gov.	fed/10 k gov.
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treat × enaction					0.116	−0.041	0.010	−0.028
					(0.114)	(0.050)	(0.065)	(0.047)
Treat × short run					−0.008	−0.115	−0.068	−0.011
					(0.072)	(0.077)	(0.060)	(0.056)
Treat × long run					0.015	−0.106	−0.072	0.025
					(0.123)	(0.104)	(0.078)	(0.085)
During enaction	0.157	−0.045	0.060	−0.007	0.054**	−0.013	0.036***	−0.005
	(0.129)	(0.045)	(0.074)	(0.051)	(0.020)	(0.026)	(0.010)	(0.019)
Short run	0.054	−0.061	0.010	0.024	0.033*	0.036*	0.021*	0.004
	(0.067)	(0.075)	(0.064)	(0.065)	(0.016)	(0.021)	(0.012)	(0.017)
Long run	0.092	−0.079	0.041	0.083	0.036*	0.006	0.018	−0.000
	(0.108)	(0.100)	(0.085)	(0.092)	(0.019)	(0.023)	(0.016)	(0.014)
Emp. weight	No	No	Yes	Yes	No	No	Yes	Yes
Matched	No	No	No	No	Yes	Yes	Yes	Yes
State-trends	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.43	0.29	0.48	0.37	0.48	0.25	0.52	0.30
N	1200	1200	1200	1200	$N^{treated} = 12, T = 22$			
$\overline{C}_{Pre}$	0.125	0.198	0.172	0.198	0.125	0.198	0.172	0.198
$\frac{\overline{C}_{Short}}{\overline{C}_{Pre}}$	1.43	0.77	1.06	1.12	0.94	0.42	0.60	0.94
$\frac{\overline{C}_{Long}}{\overline{C}_{Short}}$	1.21	0.90	1.17	1.27	1.20	1.11	0.96	1.19

Notes: Placebo treatment where pseudo-policy change occurs 5 years after actual change. Regressions 1–4 include all states (s = 50, t = 24), while regression 5–8 include only changers and a set of 4 control states for each changer. The dependent variables are corruption convictions of state and local (sl) and federal (fed) officials per 10,000 government employees, respectively. All regressions include the full set of controls and 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.

There is no evidence of a statistically-significant treatment effect for federal officials.

Because our analysis is conducted using counts of convictions, we could potentially obtain more efficient estimates by employing count-data methods. To see if this might alter our findings, we replicate the analysis of Table 3 using a Negative Binomial model.[32] Table A8 in the Online Appendix presents the results. The Negative-Binomial estimates suggest that employing count-data methods would do little to change our results. As in Table 3, the estimates indicate that mean conviction rates for state and local officials jump in the 2 to 7 years after enaction, and the jump is both statistically and economically significant. Beyond 7 years, the conviction rates fall back to a level that is indistinguishable from the baseline.

Because only 12 states switched from weak- to strong-FOIA status, a single state could potentially play a large role in determining the estimated FOIA effects. Table A9 in the Online Appendix addresses this concern by illustrating the impact of excluding individual states on the estimated corruption and conviction effects. We construct the estimates in the same way as those reported in Table 5. Each row of the table reports the estimated effects with the indicated state omitted for the unweighted and government-employee-weighted regressions using the full sample (Columns 1 through 4) and the matched sample (Columns 5 through 8). The states are ordered from lowest to highest number of government employees. Although the estimated corruption and conviction effects vary from one row to the next, the differences relative to our baseline estimates are not very large, and they go in both directions. Thus the evidence suggests that our findings are indicative of a systematic FOIA effect, not an idiosyncratic phenomenon that is confined to a single state.

Some studies that use country-level data, such as Graeff and Mehlkop (2003) and Goel and Nelson (2005), find a link between the degree of economic freedom in a country and its perceived corruption level. Because the degree of government regulation varies substantially across states, we perform a final robustness check by fitting regressions in which we control for differences in economic freedom using the state-level index developed by the Fraser Institute.[33] The coefficient estimates suggest that the relation between economic freedom and corruption is negative, as expected, but the relation is not statistically significant. Moreover, including the economic freedom index as a control does not affect our general conclusions regarding the statistical significance of the estimated FOIA effects.

7. Interpretation and conclusions

Previous research suggests that open government laws do little to combat official corruption. In fact, some studies have even concluded, in contrast with the most straightforward economic theories of crime, that open government laws lead to an *increased* rate of corruption. 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. It should not be surprising to find that public officials are convicted at a higher rate after the adoption of open government laws, because increased transparency makes it more likely that the corrupt acts committed in the past will come to light. This is precisely the outcome 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.

We find clear support for this line of reasoning. Using a simple reduced-form model as a guide, we investigate the impact of switching from a weak to a strong state-level FOIA law on corruption convictions for state and local government officials. These 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

[32] We chose the Negative Binomial model over a Poisson model after plotting the corruption conviction 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 Online Appendix, shows that the Negative Binomial distribution is a much better fit to our data.

[33] Available at http://www.freetheworld.com/efna2010.html.

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 between 10 and 20%. We believe that these are conservative estimates because we use estimators that are biased toward finding no effect.

The evidence also points to variation in corruption conviction rates across states that arises from heterogeneity in both FOIA laws and media markets, and to differential effects of FOIA laws across types of corruption-related crimes and categories of public officials. The effects of strong FOIA enaction are more pronounced for states that enact larger policy changes, include provisions that make respondents liable for failing to properly disclose, and have stronger print media. In addition, they seem to be larger for more serious crimes, and to affect the behavior of local officials more so than state officials. Although these extensions to our baseline analysis produce a smaller contrast between the short-run and long-run convictions rates, this may just be an indication that the long-run is particularly difficult to estimate when we split the relatively small treated sample along additional dimensions.

From a policy standpoint, our findings strongly favor the ongoing trend toward increased transparency in government. At a minimum they suggest that the enaction of strong FOIA laws leads to a notable increase in the likelihood that corrupt public officials are detected, prosecuted and convicted. This alone is a sufficient reason to promote increased access to information. More broadly, however, they suggest that there is a meaningful reduction in the prevalence of the most serious forms of public corruption following the enactment of strong FOIA laws. This finding is consistent with the predictions of economic theories of crime, and should be a key consideration in policy discussions on the benefits of open government.

Appendix A. Supplementary data

Supplementary data to this article can be found online at http://dx.doi.org/10.1016/j.jpubeco.2014.03.010.

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