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Transparency by Conformity: A Field Experiment Evaluating Openness in Local Governments

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Abstract: *Sunshine laws establishing government transparency are ubiquitous in the United States; however, the intended degree of openness is often unclear or unrealized. Although researchers have identified characteristics of government organizations or officials that affect the fulfillment of public records requests, they have not considered the influence that government organizations have on one another. This picture of independently acting organizations does not accord with the literature on diffusion in public policy and administration. This article presents a field experiment testing whether a county government's fulfillment of a public records request is influenced by the knowledge that its peers have already complied. The authors propose that knowledge of peer compliance should induce competitive pressures to comply and resolve legal ambiguity in favor of compliance. Findings indicate peer conformity affects both in the time to initial response and in the rate of complete request fulfillment.*

Practitioner Points

- Transparency advocates should publicize instances of public records releases in order to induce pressure on government organizations to conform.
- When requesting records from multiple organizations, requesters should use a sequential process in which new requests indicate prior instances of fulfillment.
- When requesting records from multiple organizations, requesters should experiment with their choice of words in order to identify the most effective wording.

Government organizations in the United States often fail to fulfill public records requests to the extent required by local, state, and federal laws (Geraghty and Velez 2011). When deciding whether to fulfill a request, government officials exercise considerable discretion and must handle multiple sources of ambiguity. For example, in addition to applying confidentiality and security exemptions, they must assess the cost of fulfillment and evaluate the potential legal and political risks associated with denial (Kimball 2003). These challenges cause organizations' request fulfillment rates to vary considerably.

Researchers have only recently begun to study the factors that affect organizations' responses to public records requests. For example, Wood and Lewis (2015) found that agencies with higher levels of responsiveness to elected officials were less responsive to Freedom of Information Act (FOIA) requests from the public; Cuillier and Davis (2011) found that friendly records requests were more effective than threatening ones; and Relyea (2009) surveyed amendments made to the FOIA to improve its efficient operation.

Although researchers have identified characteristics of government organizations or officials that affect the fulfillment of records requests, they have not examined the influence that government organizations have on one another. This picture of independently acting organizations does not accord with the literature on public policy diffusion, which shows that governments follow the behavior of their peers (e.g., Berry and Berry 1990; Boehmke 2009; Desmarais, Harden, and Boehmke 2015; Shipan and Volden 2006; Tolbert, Mossberger, and McNeal 2008; Walker 1969). Government organizations often have incentives to look to one another's decisions when evaluating the legality of and costs or benefits associated with request fulfillment. Furthermore, implementations of transparency laws are closely tied to innovations in e-government (Bertot, Jaeger, and Grimes 2010). Therefore,

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recent literature that identifies patterns of geographic diffusion in e-government innovations (e.g., Anderson, Lewis, and Dedehayir 2015; Jun and Weare 2011; Lee, Chang, and Berry 2011) should lead researchers to test for diffusion in government transparency.

In this article, we present a randomized field experiment in which we requested internal e-mail archives from 100 county governments in North Carolina. We selected e-mail archives for two reasons. First, e-mail is the standard communication medium for most government organizations. Second, e-mail is part of the public record in many jurisdictions. We deployed a sequential request process to test whether the county governments' request fulfillment behavior was influenced by the knowledge that their peers had already fulfilled the same request. We expected that the governments would view other county governments that are subject to the same set of state-level sunshine laws as a peer reference group—that is, a set of peers against which they would compare and adjust their attitudes and behaviors (Collins 1996; Festinger 1954; Suls and Wheeler 2000). Ultimately, we found that the governments did conform to their peers' request fulfillment behavior.

Our experiment allowed us to perform design-based causal inference while retaining the external validity commonly associated with observational studies. As Bozeman and Scott state, field experiments are attractive in research on public policy and administration because they "place the … researcher in a setting that the consumer of the research—the policy actor or public manager—understands and trusts implicitly" (1992, 294). Randomized field experiments have seen widespread use by researchers in public policy and administration (e.g., Bellé 2015; Grohs, Adam, and Knill 2015; Hock, Anderson, and Potoski 2013; Jakobsen 2012; Nielsen and Baekgaard 2013; Wood and Lewis 2015). However, our experiment is the first to focus on peer effects in public administration.

Beyond its contribution to the literature on government transparency, our experiment is relevant to researchers in several other areas. We contribute to policy diffusion research by providing experimental evidence of the importance of geographic proximity in moderating peer effects. We also advance policy network research (e.g., Dowding 1995; Howlett 2002; Klijn and Koppenjan 2000) by showing how peer network effects can facilitate greater transparency. Finally, we contribute to the e-government literature (e.g., Heeks and Bailur 2007; Kim and Lee 2012; Moon 2002) by demonstrating the feasibility of accessing large corpora of internal government e-mail for the purpose of scholarly research.

Reactive Government Transparency

Government transparency promotes accountability and provides citizens with information about government activities. Although transparency is a foundation of democracy, it is notoriously difficult to define. The term was first used in its modern-day sense by the philosopher Jeremy Bentham, and researchers have continued to debate its definition ever since (Hood 2006). While transparency can be simply defined as the unrestricted flow of information within a polity, some researchers have characterized it as a multidimensional concept (Hollyer, Rosendorff, and Vreeland 2014; Michener and Bersch 2013).

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One important dimension of transparency distinguishes between proactive and reactive transparency. Proactive transparency covers information that is voluntarily made public, absent individual requests for that information; reactive transparency covers information that is made available to individual members of the public in response to specific requests (Darbishire 2010). For example, placing information on government websites—the scenario studied by La Porte, Demchak, and de Jong (2002); Pérez, Bolívar, and Hernández (2008); Wong and Welch (2004)—is a form of proactive transparency, while the scenarios studied by Cuillier and Davis (2011) and Wood and Lewis (2015) are forms of reactive transparency. In this article, we focus on reactive transparency. Unlike the proactive transparency process, the reactive transparency process is not completely internal to government organizations. As a result, it is more amenable to experimental research. Reactive transparency is also a more appropriate context for studying governments' responsiveness to the public.

Peer Conformity in Reactive Transparency

Peer conformity—the tendency of individuals and organizations to conform to the behaviors of their peers—is central to multiple research areas. For example, Shipan and Volden (2012) summarize decades of public policy research demonstrating that public policy innovations diffuse from one government to another, while Cialdini and Goldstein (2004) review developments in social psychology relating to conformity and social influence. Despite their disconnected intellectual histories, both of these areas, along with management and organization science (e.g., Albert, Ashforth, and Dutton 2000; Hatch and Schultz 2002), draw on the idea of a peer reference group—that is, a set of peers against which individuals and organizations compare and adjust their attitudes and behaviors.

Researchers have used peer reference groups to identify significant peer conformity effects in diverse contexts. For example, Shang and Croson (2009) found that potential donors made greater contributions to a public radio station's fund-raising campaign when they were informed of other donors' contributions; Goldstein, Cialdini, and Griskevicius (2008) found that hotel guests were more likely to participate in a towel reuse program when they were prompted by signs encouraging them to join their fellow guests; and Nolan et al. (2008) found that people were more likely to conserve electricity at home when they were informed of a trend toward conservation in their neighborhood.

Although these experiments did not involve government employees in the workplace, their findings resonate with the cue-taking, cooperative, and competitive behaviors that characterize policy decisions in state and local government (Baybeck, Berry, and Siegel 2011; Miller and Richard 2010). Furthermore, in the context of reactive transparency, governments—especially those that are subject to the same set of sunshine laws—often look to one another's decisions to assess legal precedence, as well as to evaluate the potential costs and benefits (e.g., Pasquier and Villeneuve 2007; Wang and Van Wart

2007) associated with request fulfillment. We therefore formulated the following hypothesis: *A local government is more likely to fulfill a public records request when it is aware that its peer governments have already fulfilled the same request.* In other words, we expected to find peer conformity effects in reactive transparency at the local government level.

Experimental Design

To test our hypothesis, we designed a randomized field experiment. We focused on county governments within a single state—North Carolina—to ensure that our requestees were subject to the same sunshine laws. We decided to request internal e-mail archives because e-mail is routinely used by most government organizations. These choices meant that we could issue the same request, without modification, to multiple comparable governments.

North Carolina is notable in its transparency regarding official e-mail communications: the North Carolina General Statutes state that (1) all e-mails made or received in the transaction of government business are considered public records, and (2) the cost of redacting information covered by a confidentiality exemption must not be passed along to the requester.[1] Researchers have used this openness to study the content and network structure of local government e-mails (Krafft et al. 2012).

We randomly divided the counties in North Carolina into two samples: a pilot sample consisting of 40 counties and an experimental sample consisting of 60 counties. We further divided the latter sample into a treatment group and a control group, each consisting of 30 randomly selected counties. We depict these samples in figure 1. We used the pilot sample to obtain a set of compliant governments, which we then used to create the treatment.

The reader may be concerned that we placed too great a burden on county governments simply for the purpose of observing their responses. However, our experiment also implemented the data collection portion of a project studying local government e-mail communication, funded by the National Science Foundation.[2] The data have been and will continue to be used for scholarly research (e.g., Denny et al. 2015). More importantly, we did not push counties to fulfill our requests if they expressed resistance for any reason. We also made every effort to accommodate counties' concerns and to minimize the amount of work involved in fulfilling our requests.

Pilot Sample

In September 2013, we issued public records requests to the 40 North Carolina counties in our pilot sample. North Carolina's county governments function as semiautonomous organizations. Each county has one or more county managers, who act as executives, and several department managers, who oversee the activities of the various county departments (e.g., social services). For each county, we requested all nonprivate e-mails, spanning a randomly selected three-month time frame, archived from the inboxes and outboxes of the county managers and the department managers. Our request explained that we were researchers and that the request was part of a study covering all North Carolina counties. We examined each county's website to identify the person most likely to be able to address our request and then e-mailed that person. For most counties, this person was the county manager, but some larger counties (e.g., Charlotte, the county seat of Mecklenberg) had officials who were explicitly responsible for handling records requests.

Our decision to request three months of e-mail reflected two objectives: (1) we wanted to ensure that our requests were not trivially easy to fulfill, which might result in all counties complying, and (2) we wanted to obtain a corpus of e-mail from each compliant county that was large enough for comprehensive analyses using the method of Krafft et al. (2012).

We waited 40 days before tallying the counties' various responses. We initiated no follow-up communication with county officials during this time frame. Forty days was a logical juncture at which to tally the responses; none of the counties responded after 40 days without additional prompting. In total, 13 of the 40 pilot counties did not reply to our request at all, 19 counties replied but did not fulfill our request, and 8 counties fulfilled it in full. The relatively low number of compliant counties is likely driven by two factors: (1) we requested a relatively large amount of e-mail from each county, and (2) we did not push counties to fulfill our requests if they resisted at all.

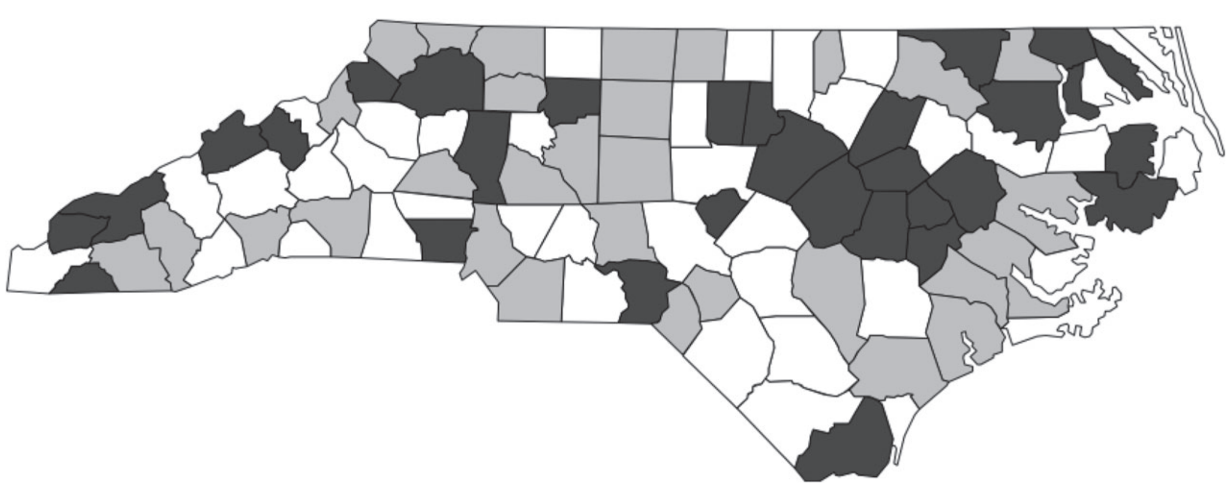

Figure 1 Pilot Sample (White), Treatment Group (Dark Gray), and Control Group (Light Gray)

Experimental Sample

We followed roughly the same procedure for the counties in the experimental sample; however, we waited 60 days before initiating any follow-up communication with county officials, and we included the following passage in our requests to the treatment counties:

> For your reference, we would like to let you know that we have issued this request to other county governments in North Carolina. A number of them have already fulfilled our request, including Polk County, McDowell County, Columbus County, Person County, Lincoln County, Alexander County, Dare County, and Transylvania County.

We listed all eight of the compliant counties from the pilot sample in order to maximize the salience of each treatment county's peers. Although we could be certain that each treatment county received the passage, we had no way of knowing whether the officials responsible for making fulfillment decisions actually read it. This scenario is analogous to nonadherence in clinical medical trials (Farmer 1999). Analyzing experimental outcomes in the presence of incomplete adherence may result in an underestimate of the treatment effects. Therefore, our results constitute lower bounds on peer conformity effects.

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Results

Several researchers have studied how quickly government organizations respond to public records requests (e.g., Grunewald 1998; Hazell and Worthy 2010; Ratish 2007; Relyea 2009; Sinrod 1994; Wood and Lewis 2015). Delays can create barriers to using the requested information to make time-sensitive decisions. Therefore, when analyzing the outcome of our experiment, we focused on two aspects of the experimental counties' fulfillment behavior: the number of days that elapsed before they provided an initial response to our request and, if they responded, whether they ultimately fulfilled our request. We censored our measurement of each county's time to initial response at 60 days to ensure that any follow-up communication with officials would not influence our results. Forty-four of the 60 counties provided us with an initial response within 60 days. Twenty of these were from the treatment group, while 24 were from the control group.

We considered two operationalizations of a county's peer reference group. First, we defined a county's peers to be all other counties in North Carolina—that is, every county is a member of every other county's reference group. We refer to this operationalization as *All.* Because the counties all comply with the same state-level sunshine laws, the behavior of one county should be informative to the others, at least from a legal perspective.

For our second operationalization, we drew on the policy diffusion literature and defined a county's peer reference group to be its bordering counties and the counties that border those counties (again, restricted to counties within North Carolina). We refer to this operationalization as *Neighbors.* When developing and implementing policies, government organizations tend to follow the behavior of their neighbors (Baybeck, Berry, and Siegel 2011; Berry and Berry 1990; Berry and Baybeck 2005; Boehmke 2005; Pacheco 2012; Walker 1969). This geographic diffusion can stem from either competition or social learning (Mooney 2001). Governments often compete with their neighbors in order to win zero-sum games involving natural resources, residents, skilled workers, the location and expansion of businesses, and tax bases. Counties do not wish to lose these games by appearing to be less transparent or responsive than their neighbors. Governments exhibit social learning by looking to similar governments to emulate successful solutions to problems. Because nearby counties are often similar to one another in terms of their political, economic, and demographic characteristics, counties may emulate their neighbors' request-fulfillment decisions. Furthermore, in North Carolina, counties coordinate strategies and activities through regional councils (McKinney and Huskins 2012). Therefore, the counties within a single region have additional incentives to follow one another's behavior.[3]

Table 1 contains the mean number of days to initial response and the rate of request fulfillment for the experimental counties with one or more compliant pilot counties in their peer reference groups under both operationalizations (*All* and *Neighbors*). To calculate the mean number of days to initial response, we considered all eligible counties—60 under the *All* operationalization (30 from the treatment group and 30 from the control group) and 36 under the *Neighbors* operationalization (13 from the treatment group and 23 from the control group). We used a value of 60 days for the counties that did not respond within that time frame. To calculate the rate of request fulfillment, we considered only those counties that provided us with an initial response within 60 days. Under the *All* operationalization, this was 44 counties (20 from the treatment group and 24 from the control group); under the *Neighbors* operationalization, this was 26 counties (9 from the treatment group and 17 from the control group). On average, the counties in the treatment group took fewer days to respond than the counties in the control group. They also fulfilled our request at a higher rate.

When working with small samples, randomization will not necessarily ensure balanced treatment and control groups (Bruhn and McKenzie 2009). Therefore, we performed two sets of regressions to adjust for any systematic differences. For the regressions in the first set, the dependent variable was the time to initial response; for the regressions in the second set, it was an indicator of request fulfillment. We performed three regressions within each set. For the first two, the data points were the

Table 1 Mean Number of Days to Initial Response and Rate of Request Fulfillment

	Mean Number of Days to Initial Response	
	All	*Neighbors*
Treatment	40.900 (from 30 counties)	40.692 (from 13 counties)
Control	43.800 (from 30 counties)	48.609 (from 23 counties)
	Rate of Request Fulfillment	
	All	*Neighbors*
Treatment	0.350 (from 20 counties)	0.222 (from 9 counties)
Control	0.208 (from 24 counties)	0.176 (from 17 counties)

experimental counties with one or more compliant pilot counties in their peer reference groups. In one of these regressions, we used the *All* operationalization; in the other, we used the *Neighbors* operationalization. For the third regression in each set, the data points were the experimental counties that we did not use as data points in the *Neighbors* regression—that is, those counties with no compliant pilot counties in their peer reference groups under the *Neighbors* operationalization.

In all six regressions, we used four control variables, along with a binary variable indicating whether each county was in the treatment or control group. First, we controlled for work demands on county officials by including the population of each county. We also controlled for government capacity by including the number of officials working for each county. We included the interaction between these variables because we expected that a larger population would result in a longer response time but that this effect would be dampened by a larger number of employees. To control for overall transparency level, we included a transparency score (A, B, C, or D) for each county. We obtained these scores from the John Locke Foundation[4] and converted them to integers as follows: A = 4, B = 3, C = 2, and D = 1. The Foundation issues scores to counties based on the online availability of a standard list of important documents, such as annual financial reports, audits, and employee salaries. We expected that higher transparency scores would correlate with shorter response times and higher rates of fulfillment. Finally, we included the region of each county within North Carolina to control for any unmeasured variables that vary systematically by region, such as policies developed by the regional councils. There were 17 regions in the *All* regressions, 15 in the *Neighbors* regressions, and 9 in the *Non-Neighbors* regressions. We expressed these regions using indicator variables.

Table 2 contains the mean values for the first three of these control variables. Counties with one or more compliant pilot counties in their peer reference groups had higher populations and more employees under the *Neighbors* operationalization than under the *All* operationalization. We used Student's *t*-tests to compare the values of the control variables for the treatment and control counties under both operationalizations (Sekhon 2011). These tests indicated strong balance: all two-tailed *p*-values were greater than 0.75.

For the time-to-initial-response regressions, we used a Weibull accelerated failure survival model (Box-Steffensmeier and Zorn 2001). We found that our results were robust to both log-normal and log-logistic specifications. Using a survival model enabled us to account for the counties that did not respond to us within 60 days. For each of these counties, we knew only that their time to initial response was at least 60 days. For the rate-of-request-fulfillment regressions—which included only those counties that provided us with an initial response—we used ordinary least squares to handle the relatively small number of data points. We found that our results were robust to both logit and probit specifications. We chose these particular models to make interpreting the effects of the treatment easier. For the time-to-initial-response model, each coefficient represents the change in the natural log of the expected number of days to initial response given a one-unit increase in the value of the corresponding variable. Similarly, for the rate-of-request-fulfillment model, each coefficient represents the change in the expected rate of request fulfillment given a one-unit increase in the value of the corresponding variable.

Table 2 Mean Values for Three Control Variables

	Population	
	All	*Neighbors*
Treatment	108,726	150,656
Control	114,390	126,358
	Number of Employees	
	All	*Neighbors*
Treatment	690	875
Control	729	801
	Transparency Score	
	All	*Neighbors*
Treatment	2.033	2.130
Control	2.033	2.231

We used permutation testing (Ludbrook and Dudley 1998) to calculate the *p*-value for the treatment effect from each regression. This is a particularly appropriate method for analyzing the outcome of a randomized experiment (Still and White 1981). Sinclair, McConnell, and Green (2012); Fowler (2013); King, Pan, and Roberts (2013); and Hill and Jones (2014) have published recent studies that used permutation testing; the first of these—an experimental study of peer influence—also used regression adjustments.

Specifically, we did not rely on parametric assumptions to establish the null distribution of the treatment effect—that is, the distribution of the coefficient for the treatment variable under the assumption of no relationship between the treatment and the outcome. Instead, we created a simulated distribution. We randomly divided the counties in the experimental sample into treatment and control groups 1,000 times, while maintaining their experimental outcomes. We then re-ran the regression for each division. The resulting coefficients for the treatment variable constitute a simulated distribution of the treatment effect under the null hypothesis. For each time-to-initial-response regression, we calculated a left-tailed *p*-value by finding the proportion of these coefficients that were less than the actual coefficient; for each rate-of-request-fulfillment regression, we

Table 3 Regression Coefficients and *p*-values (Left-Tailed) for the Three Time-to-Initial-Response Regressions

	Time to Initial Response		
	All	*Neighbors*	*Non-Neighbors*
Intercept	5.484*	8.407*	4.646*
p-value	0.000	0.000	0.000
Population	–8.144*	–8.178*	–7.914*
p-value	0.000	0.000	0.000
Number of Employees	2.952	–2.739	0.036
p-value	0.309	0.204	0.988
Number of Employees × Population	4.676*	4.827*	7.100*
p-value	0.011	0.000	0.000
Transparency Score	0.492*	0.519*	0.273
p-value	0.078	0.001	0.490
Treatment	–0.565	–0.675*	0.254
p-value	0.312	0.068	0.427
Permutation *p*-value	0.271	0.094	0.635
Total Number of Counties	60	36	24

*Statistical significance at the 0.1 level

Table 4 Regression Coefficients and *p*-values (Right-Tailed) for the Three Rate-of-Request-Fulfillment Regressions

	Rate of Request Fulfillment		
	All	*Neighbors*	*Non-Neighbors*
Intercept	0.912*	0.923	2.271
p-value	0.051	0.112	0.387
Population	−0.035	0.015	2.451
p-value	0.871	0.947	0.512
Number of Employees	0.131	0.385	−1.8607
p-value	0.796	0.575	0.700
Number of Employees × Population	−0.012	−0.037	−0.930
p-value	0.744	0.457	0.724
Transparency Score	−0.053	−0.071	−0.748
p-value	0.731	0.706	0.455
Treatment	0.283*	0.579*	0.338
p-value	0.225	0.150	0.677
Permutation *p*-value	0.084	0.078	0.334
Total Number of Counties	44	26	18

*Statistical significance at the 0.1 level

calculated a right-tailed *p*-value by finding the proportion of these coefficients that were greater than the actual coefficient.

Tables 3 and 4 contain the coefficients and *p*-values for all six regressions. These results support our hypothesis that a county government is more likely to fulfill a public records request when it is aware that its peers have already fulfilled the same request. The treatment effects are strongest under the *Neighbors* operationalization: for the time-to-initial-response regression, we found that the treatment effect was statistically significant according to parametric and permutation-based *p*-values; for the rate-of-request-fulfillment regression, we found that the treatment effect was statistically significant according to just the permutation-based *p*-value. In contrast, we found statistically significant negative treatment effects for the *Non-Neighbors* regressions. This finding suggests that geographic proximity plays an important role in defining a county's peer reference group.

Our findings involving control variables are more ambiguous. We found that the transparency score variable correlated positively with time to initial response. Although this finding is counterintuitive, we think it indicates that counties with higher levels of proactive transparency had greater capacities to critically evaluate our request and to delay or even deny its fulfillment. None of the control variables had a statistically significant relationship with rate of request fulfillment. We did, however, find a statistically significant positive interaction effect for population and number of employees. This was not the relationship we expected; however, we hypothesize that it may be attributable to the amount of e-mail that we requested from counties with large populations and large numbers of employees.

We can interpret the treatment effects in both the time-to-initial-response regressions and the rate-of-request-fulfillment regressions. For the former, the coefficient for the treatment variable represents the change in the natural log of the expected response time given a move from the control group to the treatment group, holding everything else (i.e., population, number of employees, transparency score, region) constant. Under the *Neighbors* operationalization, where this coefficient is −0.675, the change in the expected response time is therefore $\exp(-0.675) * 100 = 50.92$ percent. In other words, the expected response time for a county in the treatment group is almost 51 percent of the expected response time for a county in the control group with the same population, number of employees, and transparency score and within the same region. This effect is substantial and explains why we were able to identify it using only 36 counties. For the rate-of-request-fulfillment regressions, the coefficient for the treatment variable represents the change in the expected rate of request fulfillment given a move from the control group to the treatment group, holding everything else constant. Under the *Neighbors* operationalization, this coefficient is 0.579. Therefore, the expected rate of request fulfillment for a county in the treatment group is approximately 58 percentage points higher than that of a county in the control group. We note that these treatment effects are peer network effects: one or more counties had to fulfill our request in order for there to be a peer conformity effect on the others.

To explore the implications of our findings, we conducted a simulation to see whether we could achieve a greater level of transparency within a fixed time frame by inducing peer conformity effects. We used our time-to-initial-response model under the *Neighbors* operationalization to simulate the process of issuing requests to all 100 counties over 6-, 9-, and 12-month time frames. For each time frame, we compared a staggered request procedure, designed to induce peer conformity effects, to a baseline of issuing requests on the first day and giving the counties the full time frame to respond.

We experimented with request phases of various lengths for the staggered request procedure. For the 6-month time frame, we experimented with 12 different phase lengths, from 15 days to 180 days, in increments of 15 days; for the 9-month time frame, we experimented with 13 lengths, from 20 days to 260 days, in increments of 20 days; and for the 12-month time frame, we experimented with 14 lengths, from 25 days to 350 days, in increments of 25 days.

Other than the lengths of the request phases, the procedure was identical for all three time frames. For each length l, we iterated over the 100 counties and, for each county, simulated the day on which to issue our request by drawing a day uniformly at random, with replacement, from $\{1, 2, \ldots, l\}$. Because we sampled with replacement, multiple counties could receive our request on the same day. We then iterated over the counties again and, for each county, simulated its time to initial response and the day on which it fulfilled our request. We did this by first determining whether the county should be in the treatment group or the control group. If the any of the county's peers (under the *Neighbors* operationalization) had already complied, then we assigned it to the treatment group. We simulated its response time using the coefficients from our time-to-initial-response model and its simulated value for the treatment variable. Finally, we simulated the day on which the county fulfilled our request by adding its response time to its request day. Before moving on to the next phase length, we repeated the procedure 1,000 times to obtain 1,000 simulated request days, response times, and fulfillment days for each county.

For the baseline, we followed roughly the same steps; however, we eliminated the request phase and issued the requests on the first day

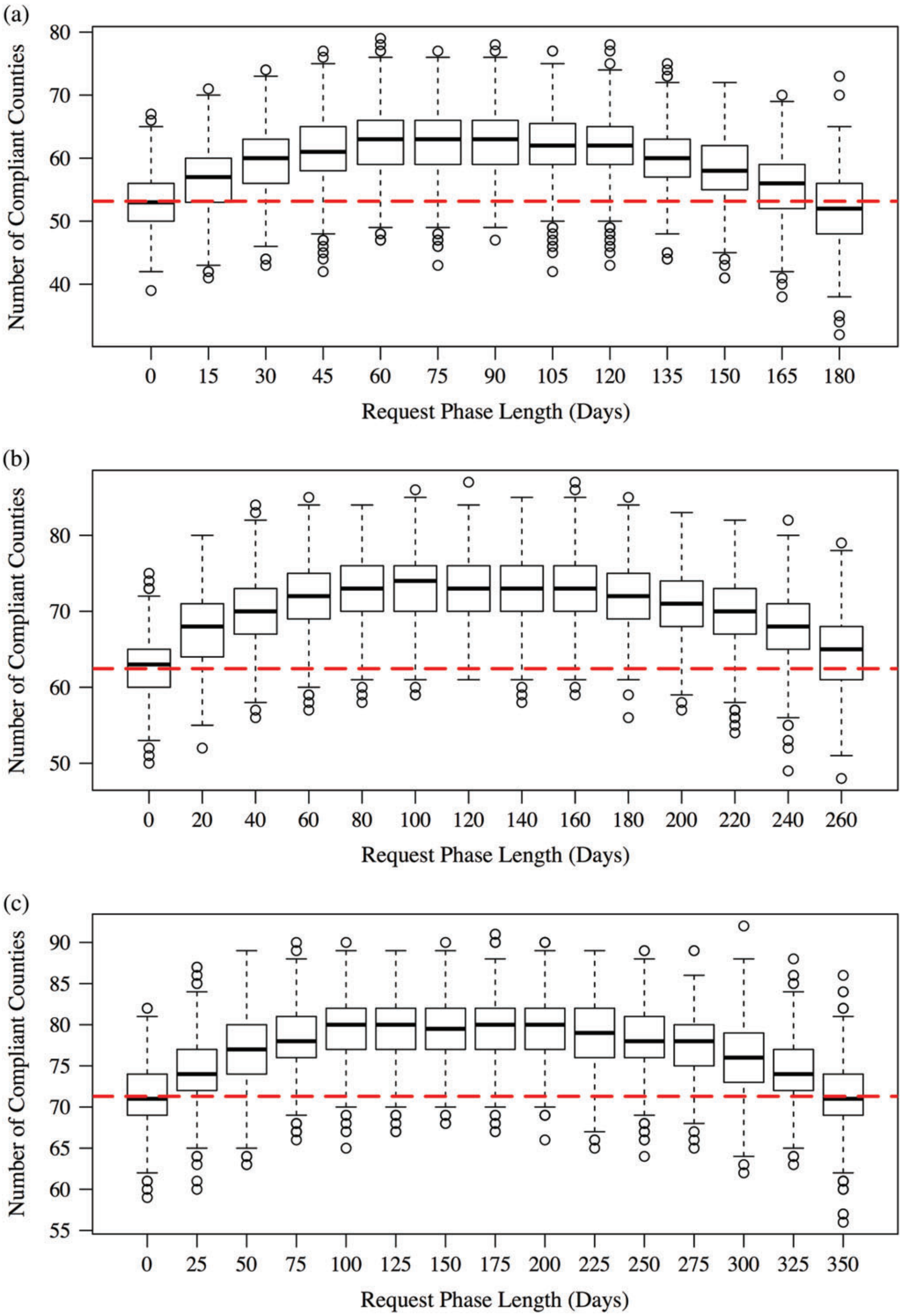

Figure 2 Simulation Results

of the time frame. Therefore, we assigned each county to the control group when simulating its time to initial response.

Figure 2 depicts the numbers of counties that fulfilled our request by the end of each time frame for both the baseline (indicated on each plot by a request phase of length zero) and the staggered request procedure. For all three time frames, the peer conformity effects induced by the staggered request procedure ultimately led to higher rates of request fulfillment. These increases were slightly more pronounced for the 9- and 12-month time frames. We also note that for each of the three time frames, the request phase lengths that resulted in the greatest impacts were around half of the entire time frame.

Discussion

Our findings advance research on government transparency and public policy in multiple ways. First, our experiment is the first to focus on peer conformity effects in reactive government transparency. Second, we contribute to the literature on peer effects in public policy. For decades, researchers have found empirical patterns of policy diffusion that suggest that governments influence and are influenced by their neighbors. However, none of these studies was conducted within an experimental framework. In contrast, our experiment allowed us to perform design-based causal inference. Therefore, we were able to provide causal evidence of peer conformity effects in public policy administration.

Our findings offer implications for practitioners. As illustrated by our simulation, when requesting records from multiple organizations, requesters should consider publicizing instances of request fulfillment. If a requester has a relatively long time frame within which to obtain their desired information, a staggered request procedure will likely result in higher rates of request fulfillment. Our findings also offer implications for researchers. First, government transparency researchers should consider how networks of officials, responsible for managing public records, could facilitate the diffusion of information disclosure practices. Second, our experimental design is highly portable and could be used to understand peer conformity effects in other groups of comparable governments. Third, although not the main focus of our experiment, we found that general properties of county governments may not correlate strongly with their responses to a single request, and that the literature contains very little research on the variables that should or should not correlate with governments' responsiveness to records requests. This latter finding highlights an important direction for government transparency researchers to explore.

Looking beyond the immediate implications of our findings for both practice and research, our experiment contributes to a growing set of examples that demonstrate how a network-theoretic approach to understanding governance and policy can lead to fruitful insights (Lecy, Mergel, and Schmitz 2014). As O'Toole notes in his call for additional research on networks in public administration, "we need to know much more about the ways in which networks and networking behavior can shape performance and affect the most salient values in our governance systems" (2015, 368). Our findings illustrate that randomized field experiments offer a powerful framework through which we can learn about an important type of network dynamic—peer effects—in public administration.

Conclusion

Government transparency holds the potential to provide unprecedented information on the deliberation, decision making, and behaviors of public officials. The payoff to encouraging openness, therefore, is extremely high. However, when deciding whether to fulfill a public records request, officials often face considerable ambiguity. Sunshine laws cover even preliminary, private, and informal information, and the implications of inadvertently disclosing something that should have been kept secret are potentially very serious.

In this article, we hypothesized that a county government is more likely to fulfill a public records request when it is aware that its peer governments have already fulfilled the same request. We designed a randomized field experiment to test this hypothesis, and found that providing counties with information about their peers' fulfillment behavior decreased the average time to initial response and increased the rate of request fulfillment.

Because we used a randomized field experiment to identify these peer conformity effects, we are confident that our results reflect the causal effects of notifying government officials of other governments' fulfillment of a public records request. However, identifying a causal effect and understanding the underlying causal mechanism are two different tasks. The effects that we identified are consistent with (1) officials making a conscious assessment that their peers have seen the value in fulfilling the request or (2) a subconscious, psychological framing effect induced by seeing that their peers have already complied. Future research could replicate our study while varying the information provided to officials. This experiment would test whether specific fulfillment information is instrumental in officials' decisions, rather than there being a simple framing effect.

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Notes

1. The North Carolina General Statutes are available at http://www.ncga.state.nc.us/gascripts/Statutes/StatutesTOC.pl.
2. National Science Foundation award IIS-1320219, "Organizational Responsiveness to Open Outside Input: A Modeling Approach based on Statistical Text and Network Analysis."
3. The official North Carolina regional breakdown is available at http://www.ncregions.org/regional-map/.
4. The John Locke Foundation Scores are available at http://www.nctransparency.com/.

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