Media Scandals Are Political Events:
How contextual factors affect public controversies over alleged misconduct by U.S. governors

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Abstract

When political scandals erupt in the press, we usually blame misconduct by public officials, but these episodes are political events whose occurrence and severity also depends in part on the political and media context. Using data on U.S. governors, I show that several key factors affect the likelihood and intensity that alleged misconduct will be politicized by the opposition and publicized by the press. First, lower approval ratings, which decrease the cost of politicizing and publicizing an allegation, are generally associated with more frequent and intense media scandals. By contrast, competing news events can crowd potential scandals off the news agenda. However, no evidence is found that opposition control of state political institutions leads to more media scandal. These results suggest that the occurrence of media scandal depends more on circumstance than we typically assume.

Keywords: scandal, misconduct, corruption, governor, media, press

Coverage of alleged scandals often dominates political news in the United States, potentially increasing public cynicism about government and displacing policy issues from public debate. While the most familiar examples are presidential scandals like Watergate and the Lewinsky affair, we see a similar pattern at the state level, where scandals have dominated the headlines in coverage of governors like Rod Blagojevich and Chris Christie. Despite the significance of these episodes, social scientists know very little about when or why scandals occur. A greater understanding of this topic can thus help us better understand contemporary American politics.

One reason little is known about scandal is that our understanding of the term is too simplistic. Scandal is often conceptualized as the public exposure of corrupt, illegal, or unethical behavior by public officials and largely (implicitly) treated as an exogenous event. It is true, of course, that many scandals are the result of the disclosure of unambiguous misconduct or criminal acts. However, the conventional definition fails to explain the patterns we observe in the timing of these revelations; why some allegations of misbehavior turn into media scandals when the evidence is ambiguous or contested and others do not; or why media scandals are more prevalent in some contexts than others.

Because underlying patterns of executive misconduct are largely unobservable, I instead study the occurrence of media scandal as *a political event*, focusing on the public recognition and subsequent coverage of alleged misconduct by a public official as a scandal in the mainstream press. Using this measurement strategy, I show that the process that generates media scandals is systematically influenced by the political context and news environment. When conditions are favorable for the opposition party to politicize a potential allegation and for the press to publicize it, chief executives are more likely to fall victim to media scandals. By contrast, even well-supported allegations may be suppressed or ignored under less favorable conditions.

Based on this theory, which has previously been tested only at the presidential level (Nyhan 2015), I identify three political and contextual factors that are likely to influence the vulnerability of chief executives to media scandal: the chief executive's approval rating, which should affect the costs of pursuing potential scandals for the opposition and the

press; media congestion, which should increase the opportunity costs of covering possible scandals and thus make it less likely that they receive press coverage; and opposition party control of state political institutions, which should enable investigations that reduce the cost of identifying potential scandals. Each factor is likely to affect the incentives for the opposition party to *politicize* a potential scandal and/or for the press to *publicize* it and thereby change the expected likelihood and intensity of media scandal.

I test these hypotheses using a comprehensive new dataset on U.S. governors for the 1977–2010 period and a series of novel research designs that allow me to conduct new and more rigorous tests of the effects of political and news context on the incidence of media scandal. First, I show that lower approval ratings are associated with more frequent and intense media scandals in the future. In addition, I show that news congestion reduces the probability of media scandal. However, opposition control of the legislature or attorney general's office has no effect on media scandal, contradicting many prior claims. As with presidents, the occurrence of gubernatorial media scandal seems to depend in part on circumstance and context, not just objective evidence of misconduct.

Why we should study media scandal as a political event

Until recently, quantitative political science has neglected the study of scandal (Cameron 2002, 655). With the exception of Nyhan (2015), previous studies that have been conducted largely focus on the effects of scandal or ethics charges on presidents, members of Congress, or other politicians rather than their causes (see, e.g. Welch and Hibbing 1997; Meinke and Anderson 2001; Basinger 2013; Rottinghaus 2014a,b, 2015).

A key obstacle to research in this area is the difficulty of defining and measuring scandal. The most common approach is to try to study the incidence of official misconduct, which is often equated with scandal. Unfortunately, we not only lack objective definitions of misconduct (which are often seen to extend beyond violations of the law to various perceived improprieties), but we cannot observe the existence or absence of misconduct — the private behavior of public officials is largely unobservable. Others prefer instead to

equate scandal with the *disclosure* of a moral or ethical transgression by a public figure (e.g., Markovits and Silverstein 1988). However, this definition is also inadequate. First, disclosures of genuine transgressions do not always generate scandals (in part because the standards used to judge them are applied inconsistently), while in other cases, scandals occur despite little evidence of an actual transgression (Adut 2005; Entman 2012).

We should instead analyze media scandal as a political *event*, which requires measuring when scandal was generally *perceived* to exist within the political system. This approach is similar to how scholars study the legal system. Though we by definition lack data on unobserved violations of the law, careful research designs allow scholars to estimate how political and institutional factors influence arrests and prosecutions (e.g., Gordon 2010).

More generally, research suggests political factors — not just evidence and facts — may influence the realization of many important types of events like scandal. For instance, Reeves (2011) considers how political incentives can even influence disaster declarations. Similarly, Shaw (1999, 414–416) finds that significant campaign events are more likely when presidential candidates deviate from expected levels of public support. In other words, the likelihood that an "event" will occur and perceived as meaningful is shaped by the underlying political context, not just the facts of what took place — the same pattern that I describe below for scandal.

This article focuses specifically on the event I call "media scandal," which I use to refer to the identification of a controversy involving a chief executive as a scandal in mainstream press coverage. This measurement approach reflects the centrality of news coverage to the existence of scandal in contemporary politics (Waisbord 2004, 1079). Most importantly, defining the outcome in this way allows me to distinguish politically important cases of alleged misconduct from fringe allegations while remaining agnostic about the existence of misconduct in any particular case. The result is not only improved theoretical clarity and measurement precision but new insight into how the prevalence and severity of media scandals are influenced by political and other contextual factors.

In this study, I focus specifically on media scandal among U.S. governors, an important topic that has been largely neglected. Previous studies have been largely qualitative and

tend to rely on *post hoc* judgments about the existence of scandal from chronologies and other historical sources. By contrast, I draw on contemporaneous news coverage to construct novel longitudinal measures of the actual incidence of gubernatorial media scandal and to estimate the effects of political and other contextual factors on its likelihood and intensity. This study represents the first systematic quantitative analysis of the onset and magnitude of gubernatorial media scandal, which has previously only been studied among presidents (Nyhan 2015).[1]

The need for this research is great. As the chief executives of their states, governors frequently suffer from media scandals, but we know even less than for presidents about why these events occur. In this article, I provide a theoretical argument for why the same political and media forces that affect scandal at the presidential level should also affect governors, which is not necessarily clear given the differences in institutional powers, news environment, etc. that we observe between the two offices. This sort of extension is increasingly common as American politics scholars explore the extent to which theories of federal executive and legislative politics apply at the state level (e.g., Shor and McCarty 2011; Kousser and Phillips 2012). The cumulation of scientific knowledge in American politics is only possible by linking levels of analysis in this way.

The primary contribution of the article, however, is empirical. Studying governors allows us to test whether the findings in Nyhan (2015) extend to the state level while addressing threats to inference that cannot easily be addressed in studies of presidential scandal. State data provides variation in key explanatory factors that is either unavailable (e.g., the partisanship of the attorney general) or insufficient (e.g., divided government, which only varies at the Congress level) at the federal level. In addition, studying governors makes it possible to analyze a much larger dataset of chief executives, better account for national-level trends that may affect the likelihood of media scandal over time, and estimate the effects of a wider array of institutional and political configurations than is possible in a study of the presidency. Finally, the literature on executive politics often struggles to make convincing causal claims due to a lack of available data and small samples of presidents, but my analyses below uses new state-level data and research design approaches to provide

more credible estimates of the causal effects of interest — the effects of contextual factors on the incidence and intensity of executive media scandal.

A theory of executive media scandal

The occurrence of media scandal among chief executives — the leading political figures in their state (governor) or the country (president) — can be interpreted as a "co-production" of the opposition party and the press (Nyhan 2015), the two institutions that jointly take part in a "negotiation of newsworthiness" over allegations that could in principle give rise to a public controversy over alleged misconduct (Cook 2005). Both the media and the opposition have incentives to promote potential scandals in this way under some circumstances — either to acquire journalistic prestige and try to appeal to audience interest in the case of the press (e.g., Woodward and Bernstein 1974) or to damage the political standing of the chief executive for the opposition (e.g., Kriner and Schickler 2014). Without the participation of both groups, a media scandal is unlikely to occur.

Though contextual factors are of course likely to affect the aggressiveness of the media and opposition in pursuing potential scandals through investigatory means,[2] they play their most important role in how they treat potential scandals that have been revealed. The opposition party is necessary for the *politicization* of a potential scandal and the media are necessary for giving it *publicity* — the two critical steps in the co-production of media scandal. In particular, the interaction of the opposition party and the press help to create the positive feedback dynamics that characterize scandals.

Without the participation of both groups, potential media scandals are unlikely to materialize. First, opposition party participation is typically necessary for a media scandal to reach critical mass. News coverage tends to track elite debate (Bennett 1990) and intra-party scandal allegations are relatively rare in contemporary politics, particularly early in the scandal generation process. In the absence of opposition allegations, reporters lack material for coverage and risk appearing biased within the norms of objectivity that prevail in the mainstream U.S. media (Tuchman 1972). As a result, scandals typically fail to materi-

alize when opposition elites do not politicize them (for examples, see, e.g., Bennett 2004, 292–293 on Bush administration ties to Enron and Fogarty 2013 on the Abramoff scandal). Likewise, the opposition party depends on reporters to publicize scandal allegations. Without significant coverage, a media scandal by definition cannot occur.

By contrast, the chief executive and her administration play a largely passive role in this process. We should expect them to be defensive and resistant to scandal allegations regardless of the circumstances or the severity of the underlying misconduct (if any) given the political damage that even innuendos can inflict. It is possible, of course, that the governor's administration could seek to anticipate and forestall scandal as the risk increases. However, it is important to be clear that such a response would work *against* the hypotheses described below. If governors could fully adjust their behavior to offset the increased risk resulting from the time-varying political and media factors I analyze below (approval ratings, media congestion, and opposition control of state political institutions), we should observe no change in the likelihood of media scandals. The problem governors face, however, is that preventing or suppressing possible scandals through contemporaneous behavior is often difficult or impossible. Many media scandals concern prior events that took place when scandal risk was lower or even before the governor took office. In addition, efforts to prevent or deny scandals frequently lack credibility and can even be counterproductive, raising suspicions and drawing unwanted attention to potential controversies.

How contextual factors affect media scandal vulnerability

Next, I identify the political or contextual factors that are most likely to affect the vulnerability of chief executives to media scandal under the theory presented above. Each factor that I identify — gubernatorial approval ratings, media congestion, and opposition control of state institutions — affects the incentives for the opposition party to *politicize* a potential scandal and/or for the press to *publicize* it. As such, they should change the expected likelihood and intensity of media scandal.

Approval ratings

Public support for the chief executive is one of the most important contextual factors affecting the incentives for the press and opposition party to take part in the co-production of media scandal. First, despite norms against commercialism in journalism, media content tends to respond to audience demand (e.g., Gentzkow and Shapiro 2010; Puglisi and Snyder 2011). As chief executives become unpopular, public demand for negative news should increase. Clayman et al. (2007), for instance, finds that journalists become more adversarial at presidential press conferences as unemployment and interest rates increase. Conversely, when chief executives are popular, the media may be reluctant to challenge them or to publish negative information, potentially offsetting reporters' commercial and professional incentives to pursue potential scandals. Similarly, the reputational risks of politicizing a potential scandal to the opposition should increase with the public standing of the chief executive. Such an effect should in turn diminish the likelihood of media scandals still further because of reporters' reliance on opposition sources for critical stories, which allows them to maintain a reputation for objectivity and avoid bias accusations (e.g., Bennett 1990). Without opposition cooperation, potential scandals are unlikely to gain traction in the press (e.g., Entman 2004).

In this study, I analyze the effect of governors' prior approval ratings on gubernatorial media scandal, including events involving the state executive branch (for which governors are held politically responsible). Specifically, I test whether governors with higher approval ratings are less vulnerable to media scandal due to weaker opposition incentives to pursue scandal allegations and reduced audience demand for scandal reporting.

Media congestion

Media scandals may also be less likely when competing stories are drawing attention from the press. Coverage of executive scandal requires the commitment of finite media resources to gather information about a story and to feature it in print, online, or broadcast formats. Such coverage necessarily comes at the expense of other potential stories given scarce

resources and media attention. The opportunity costs of scandal coverage are therefore an important factor in determining whether a potential scandal becomes real. When the news agenda is congested due to a focus on one or more major stories, other topics may be displaced and receive little attention (e.g., Eisensee and Strömberg 2007). As a result, allegations of misconduct may be neglected or ignored by the press or foregone by an opposition party that anticipates the difficulty of attracting attention to them, reducing the likelihood and intensity of media scandal (Nyhan 2015).[3]

Estimating the effect of news congestion on media scandals is challenging given that political leaders can make news for strategic reasons, including focusing attention away from actual or potential scandals. In addition, the presence of competing stories could be confounded with the factors that predict media scandals (e.g., an economic downturn could reduce approval ratings for elected officials and also attract significant coverage). I therefore estimate the effect of exogenous competing events on gubernatorial media scandals by measuring the effects of exogenous natural disasters in a neighboring state whose news coverage spills over into the target state via a shared media market — an approach that builds on prior research examining spillovers between states within media markets (e.g., Ansolabehere, Snowberg, and Snyder 2006) and cross-border news spillovers (e.g., Butler and De La O 2010). Such disasters should attract extensive coverage from news outlets in the target state and thereby reduce the share of the news agenda available to cover potential scandals while not affecting the political standing of the governor directly via two mechanisms. First, coverage of these disasters will be received by consumers in the shared media market within the target state, distracting them from other news and displacing coverage of in-state political events, including potential scandals. For instance, Idaho residents likely received substantial coverage of Washington's Mount Saint Helens eruption in 1980 since more than one-fifth of the population lived in the Spokane media market at the time. Second, the presence of cross-border media markets reflect interstate ties that are likely to result in substantial exogenous coverage of disasters among outlets within the target state, crowding out potential scandal coverage.[4] The devastating effects of Hurricane Katrina on Louisiana in 2005, for example, prompting an urgent disaster response from Texas, where

many displaced residents subsequently moved. As a result, even though only a few Texas residents get news from Louisiana (those in the Shreveport media market), Katrina was an important story for outlets in Texas more generally.

Opposition control of political institutions

A third contextual factor that could affect the prevalence of executive media scandal is party control of other political institutions, which may reduce the costs of identifying possible allegations of misconduct or increase the likelihood of successfully politicizing them. Either mechanism would make media scandals more likely and presumably more severe. Building on prior research into the relationship between divided government and investigations of the president (e.g., Mayhew 2005; Kriner and Schwartz 2008; Parker and Dull 2009) and the effects of political incentives on corruption enforcement (e.g., Gordon 2010; **?**), I investigate whether the likelihood of gubernatorial media scandals increases when the opposition controls key state institutions that can be used to promote allegations of misconduct or conduct investigations.

Specifically, I consider party control of the state legislature, which provides a platform for opposition parties to promote and investigate potential scandals, and the office of the state attorney general, who is the chief law enforcement official and separately elected from the governor in many states.[5] Both offices may facilitate the production of media scandals by using institutional routines to produce news that keeps potential scandals in the press (e.g., Entman 2012, 25–27). Moreover, partisan incentives are likely to affect the behavior of actors in both institutions. For instance, many attorneys general go on to seek higher office, which requires strong support among elite co-partisans. AGs may therefore be more likely to pursue a high-profile case against a governor from the other party or to decline to pursue one against a governor from their own party. Likewise, members of the state legislature have incentives to target opposition governors — and withhold criticisms of co-partisans — to maintain support within the party, which could be a critical factor in being selected for the party leadership or nominations to higher office.

Hypotheses

Based on the theories above, I specifically test the following hypotheses about how the political context or news environment may affect media scandal:

- H1: As governors' approval ratings increase, the likelihood and intensity of media scandal will decrease;
- H2: Media congestion will reduce the likelihood and intensity of media scandal;
- H3: Opposition control of state political institutions will increase the likelihood and intensity of media scandal;

Data

Dependent variable: Media scandal

As described above, I define media scandal not as the exposure of misconduct but as a *widespread perception* of misconduct by a political figure that is recognized in the press — the most important factor in determining when alleged misconduct is politically significant (Waisbord 2004). To measure the occurrence of this event, I rely on contemporaneous news reports, which tend to track the views of elites (e.g., Bennett 1990) and are less vulnerable to bias in retrospective judgments than the chronologies or historical scholarship on which previous studies of scandal typically rely. However, collecting comparable data on gubernatorial media scandal across the fifty states is a difficult challenge. One approach would be to analyze coverage from different newspapers, but data would be missing non-collegely for some state-years, potentially biasing my results. Outlets also differ in their use of the term "scandal," partisan/ideological slant, etc. in ways that are likely to be correlated with audience preferences (e.g., Gentzkow and Shapiro 2010; Puglisi and Snyder 2011), creating endogeneity in any analysis that considers public opinion (as I do).

To circumvent these problems, I instead rely on Associated Press (AP) news coverage of governors, which includes all fifty states, for the 1977–2010 period (the earliest allowed

by the LexisNexis Academic database). Though coverage frequency will of course vary by state (which I account for in my statistical analyses[6]), the AP provides a unified editorial perspective and coverage style and thereby minimizes concerns about substantial differences in media slant or language use across states — indeed, it was founded to provide neutral coverage that could be published in a variety of newspapers (e.g., Sparrow 1999, 122). At the same time, AP coverage can serve as a useful proxy for state political coverage more generally due to the pervasive practice of intermedia cue-taking, which tends to produce relatively homogenous patterns of coverage across outlets (e.g., Sparrow 2006).

In measuring the occurrence of media scandal, it is important to avoid the definitional problems inherent in previous studies, which frequently conflate normative judgments about the extent of the alleged misconduct with empirical assessments of how that misconduct was treated by political elites and the media. I instead focus on the *perceived* occurrence of gubernatorial scandal, which I measure using journalistic word choice in AP news reports — specifically, wire service reporters' use of the term "scandal" in their own voice during media coverage. Since media reports tend to index elite conflict (e.g., Bennett 1990), particularly on sensitive or controversial issues, the use of this term is a meaningful indicator of widespread elite belief that a governor or her administration engaged in misconduct. Moreover, reporters tend to be careful about using potentially controversial language, reducing idiosyncratic variation (see, e.g., Bender 2006). The AP in particular is known for being extremely careful about word choice, including using a detailed official stylebook (2014).

Two measures of media scandal are considered in the analyses below.[7] The first outcome measure I examine is whether a media scandal occurred under this definition. A total of 45 unique gubernatorial scandals in 23 states were identified in AP coverage using this approach from 1977 to 2010. The list of scandals identified, which is provided in the Online Appendix, has very high face validity, suggesting that AP reporters did not ignore scandals that originated in other news outlets in the state (as a wire service, they seek to be comprehensive in their coverage and to summarize events of interest) and also that AP reporters do use the term "scandal" in a haphazard or sensationalistic manner. If

anything, reporters are quite conservative in their use of the term — usage of the term is relatively rare (2.5% of all governor-year cases in my data). The second outcome is the severity or intensity of the media scandal, which is measured using the volume of scandal coverage. This measure varied widely; in all, the controversies I identified were described as "scandals" in 435 articles.[8]

Key independent variables

The set of all governors, their party affiliations, and the dates they served in office were collected from the National Governors Association website for each year of the 1977–2010 period. A total of 294 governors from the major parties were identified who served for at least one month in the data during this period: 162 Democrats, 129 Republicans, two governors who changed their party affiliation while in office, and one who was elected separately as a nominee of both major parties.[9]

Approval data needed to test H1 were obtained from the U.S. Officials' Job Approval Ratings project (Beyle, Niemi, and Sigelman 2010); Survey USA's archive of 50 state polls for 2005–2006; and the Cooperative Congressional Election Study. To facilitate interpretation, approval is measured as a proportion of state residents who approve of the governor on a 0–1 scale. To test H2, a measure was constructed for the occurrence of a natural disaster in a neighboring state that has a media market which spills over into the state in question using the Spatial Hazards Events and Losses Database for the United States and data on Nielsen media markets. For H3, data were collected on attorney general partisanship and vote shares from the *Book of the States* and other sources; Carl Klarner generously provided data on party seat totals in state legislatures.[10]

Results

It is not possible to adequately test all of my hypotheses about the effects of the political context and news environment on media scandal within a single research design. The key independent variables in this study also vary over different time scales (i.e., by year,

legislative term, and cross-sectionally). I therefore introduce the most appropriate design for testing each hypothesis below and present the results separately for H1–H3. Together, these analyses allow us to assess the weight of the evidence that several key political and contextual factors significantly influence media scandal.[11]

H1: The effect of approval on media scandal

I first estimate the effect of political context by testing whether less popular governors are vulnerable to more frequent and intense media scandals.

Research design

My test of H1 uses data on each state-year or partial state-year served by U.S. governors from 1977–2010. The unit of analysis in these data is thus the governor-year. I examine the relationship between gubernatorial approval and two dependent variables: a binary measure of whether a new controversy involving a sitting governor or her administration was first identified as a "scandal" in that year (scandal onset) and the total number of articles referring to scandals in the current governor's administration in that year (scandal intensity).

The governor's approval rating is the key independent variable of interest for testing this hypothesis. Specifically, lagged approval in year $t-1$[12] is used to predict media scandal onset and coverage intensity in year t to guard against endogeneity in the following model:

$$Y_{it} = \beta_0 + \beta_1 A_{it-1} + \mathbf{X}_{it}\beta^{\sim} + \varepsilon_t.$$

In these models, Y_{it} is the dependent variable of interest for governor i and year t (either a binary measure of media scandal onset or the total number of scandal articles in that year), A_{it-1} represents the lagged value of approval for that governor, and $\mathbf{X}_{it}$ is a vector of governor or state control variables from year t discussed further below.

To account for time-invariant differences between states (such as institutions or patterns of corruption), I estimate OLS models with fixed effects for states for both dependent vari-

ables as well as conditional logit and negative binomial models grouped by state to account for the binary onset measure and the count measure of scandal coverage, respectively. I also include year fixed effects to account for common time shocks and second-degree polynomials for time in office to account for duration dependence (Carter and Signorino 2010). Finally, the standard errors are clustered by state or governor to account for any remaining non-independence (e.g., autocorrelation). These models also include a series of key control variables that could otherwise confound the effect estimate: opposition party control of one or more chambers of the state legislature or state attorney general's office, the presence of a state ethics commission, whether a gubernatorial election was held during that year; total AP coverage of the state's governor in the prior year; and an indicator for governors who resigned or died before completing a full year in office. (The construction and coding of these variables and summary statistics are provided in the Online Appendix.)

Findings

The results of these models, which appear in Table 1, demonstrate that governors with higher approval ratings in the prior year are generally less likely to suffer from media scandals and to receive less scandal coverage than those who previously had lower approval. As noted above, these models include state fixed effects that account for time-invariant differences between states such as differences in historical patterns of corruption and year fixed effects to account for any common shocks to the likelihood of media scandal like major national news events that crowd out other stories.

The media scandal onset measure is binary. As such, I analyze it using both a conditional logit model with standard errors clustered by state (model 1)[13] and linear probability models estimated using OLS with standard errors clustered by state (model 2) and governor (model 3).[14] The estimated relationship between lagged approval and media scandal onset in these models is statistically significant in two of the three cases (models 1 and 3), providing some support for H1. However, the relationship between media scandal intensity and lagged approval is negative and more precisely estimated. We can reject the null hypothesis of no relationship across all three models estimated (OLS models clustered by state or

governor or a negative binomial model with robust standard errors clustered by state).[15]

[Table 1 about here.]

The substantive effect of gubernatorial approval is illustrated in Figure 1, which plots the predicted likelihood of media scandal as approval decreases. The predicted probabilities are calculated using estimates from model 1 with the state fixed effect set to 0 and other variables set to means (continuous) or medians (binary variables and measures of time).[16]

[Figure 1 about here.]

If lagged approval declined from its 90th percentile value of 69% to its 10th percentile value of 40%, the predicted probability of media scandal would more than double, increasing from 15% to 32%.

The results also indicate that the likelihood of media scandal rises and then falls over a governor's term conditional on state, year, and the covariates in the model (predicted effects are available upon request). By contrast, however, most of the control variables do not have a measurable relationship with media scandal onset or coverage across any of the specifications in Table 1. In particular, opposition party control of one or more chambers of the state legislature or the attorney general's office has no measurable effects on media scandal. (I examine the effects of these variables further in my tests of H3 below.) In model 2, for instance, 95% confidence intervals exclude an increase of 1.5 percentage points in the probability of scandal as a result of a change from unified to divided government (95% CI: -.041, .013) and a 2.5 percentage point increase in the probability of scandal as a result of going from a co-partisan to an opposition party attorney general (95% CI: -.024, .023).[17] The estimated effect of an ethics commission on media scandal onset and coverage is similarly null.

H2: The effect of media congestion on media scandal

Next, I test the contextual effect of news congestion by examining whether it can crowd out gubernatorial media scandal using data from states that are subject to exogenous news

spillovers from their neighbors (specifically, those in which a portion of residents live in media markets dominated by a neighboring state). Specifically, the incidence of natural disasters should be exogenous to the news agenda in the neighboring state as well as the political standing of its governor or other factors that might affect the likelihood of media scandal. As such, we can interpret the effect of disasters in neighboring states on media scandal as causal in the models below. If we additionally assume that the effect of disasters in neighboring states on media scandal is mediated by their effect on news congestion (the most plausible mechanism), these results provide a novel test of H2.[18]

Research design

To test H2, I estimate a separate set of models that are nearly identical to those above:

$$Y_{it} = \beta_0 + \beta_1 A_{it-1} + \beta_2 D_{it} + \beta_3 I_{it} + \mathbf{X}_{it}\beta^{\sim} + \varepsilon_t.$$

As noted above, these models are estimated separately from those used to test H1 due to the exclusion of seven states from the data for which news spillovers cannot occur under my definition. The model specification and variables used differ from those used to test H1 in three other respects. First, I include D_{it}, an indicator for a disaster in a neighboring state with a shared media market, to test for news spillover effects on the likelihood and intensity of media scandal. Second, I include I_{it}, an indicator for a disaster within the state, to account for correlated weather shocks.[19] Third, I omit a logit model of scandal onset due to separation (no scandals in years with neighboring-state disasters).

Findings

The results in Table 2 support the news congestion hypothesis for media scandal onset.

[Table 2 about here.]

First, the incidence of disaster is not correlated with other covariates conditional on state and year fixed effects, which is consistent with the assumption that disasters are exogenously assigned (details available upon request). Moreover, new scandals are significantly

less likely when a disaster occurs in a neighboring state whose media market spills over into the target state regardless of the error structure (clustering by governor or state). The point estimate corresponds to a two percentage point decrease in the likelihood of scandal (-0.020, 95% CI: -0.003, -0.037), which is a substantial effect in relative terms given the low base rate of scandal in the governor-years in the sample (2.5%). The relationship to media scandal intensity is somewhat less consistent and precisely estimated, though still suggestive of a negative relationship. The coefficient for a disaster in a neighboring state with a media market spillover is marginally significant in model 3 and highly significant in model 5 but fails to reach significance in model 4. These results are robust to excluding neighboring state disasters for which a disaster also occurred in the target state or including the excluded states and coding the neighboring state disaster measure as 0 (results available upon request). By contrast, the indicator for within-state disasters has no significant effect, though expectations for this variable are ambiguous.[20]

H3: The effect of opposition institutional control on media scandal

Third, I examine whether opposition party control of one or more chambers of the state legislature or the attorney general's office contributes to gubernatorial media scandal per H3. While Table 1 found no correlation between opposition control of either institution and scandal, any such observational finding is vulnerable to confounding. The analyses that follow provide more rigorous tests of the divided government hypothesis using a regression discontinuity approach.

Research design

I estimate the effect of an opposition party attorney general or opposition control of one or both chambers of the state legislature on media scandal. As noted above, these factors are not randomly assigned — the partisanship of the attorney general or legislature may be affected by the partisanship of the governor, for instance. I therefore employ a regression discontinuity (RD) approach. If the assumptions of the RD models are satisfied (most

notably, that candidates or parties cannot sort perfectly around the 50% threshold), then the as-if random variation in opposition control of these institutions can be used to estimate a local average treatment effect at the discontinuity in party control that can be interpreted in causal terms. In both cases, I examine how many unique media scandals began (onset) as well as the total volume of scandal coverage (intensity) in the period until the next legislative or executive (governor or attorney general) election. Due to the relatively small samples of statewide elections, I estimate local linear regression models with margins of 10% and 20% (vote share for governor or attorney general) or 20% and 30% (seat share in the closest state legislative chamber) around the discontinuities at 50%. (See the Online Appendix for more details on this approach, including model specifications, balance tests, and graphical summaries of the results.)

Findings

Table 3 presents results from local linear regressions estimating the effect of opposition party attorney general control on gubernatorial media scandal in relatively closely contested elections. To maximize robustness, I estimate two sets of RD models using local linear regression in which the so-called "running" or "forcing" variable is the two-party vote share of the candidate in question — either the gubernatorial candidate or the attorney general candidate — holding the partisanship of the other official fixed at its observed value after the election. Table 3(a) presents estimates from gubernatorial elections (conditional on attorney general partisanship) while Table 3(b) presents estimates from attorney general elections (conditional on gubernatorial partisanship).

[Table 3 about here.]

Despite estimating somewhat different local treatment effects (see Online Appendix), none of the results are consistently significant for either outcome variable. These results are also null when states are divided based on the prosecutorial powers of the attorney general (available upon request).

An alternative possibility is that opposition control of one or more chambers of the

state legislature could increase media scandal. I therefore estimate a series of regression discontinuity models using the seat share margin between divided and unified government as the running variable. As Table 4 indicates, however, the results are again null.

[Table 4 about here.]

Though the estimates are not precise, the effect of divided government in the legislature on media scandal is not significant regardless of seat share margin or outcome variable (scandal likelihood or intensity). There is no clear evidence of a discontinuity in media scandal by opposition control of one or more chambers of the legislature. (These results do not vary by the professionalism or subpoena powers of the state legislature; details and results available upon request.)

Conclusion

To understand the causes of the scandals that frequently engulf governors and other chief executives, it is necessary to analyze the process as a political event. In this article, I focus on the conditions under which alleged misconduct by a governor or her administration is publicly recognized as a scandal in the mainstream press, which I call a "media scandal." Results indicate that the political context and news environment have a powerful effect on the vulnerability of chief executives to media scandal. Like presidents (Nyhan 2015), governors appear to suffer from more frequent and especially more intense controversies about alleged misconduct when their prior approval ratings are lower. Likewise, media scandals are less likely when news shocks such as disasters in neighboring states crowd out potential scandals that might otherwise have received significant coverage.

However, the conventional wisdom about the importance of divided government for opposition scandal-mongering is not supported. As with presidents (Nyhan 2015), no consistent evidence is found that opposition control of key state institutions increases the incidence of gubernatorial media scandal. Chief executives might adjust their behavior to offset the increased risk of scandal under these circumstances (e.g., Kriner and Schickler

2014, 5). But if governors could calibrate their behavior in this way, we might expect them to also adjust for the potential effects of low approval ratings, which they apparently fail to do. Alternatively, the relationship between divided government and scandal may be confounded by other factors (e.g., tenure in office) or vary based on contingent opposition choices to politicize potential scandals (Fogarty 2013); more research is needed.

Of course, these analyses have limitations that should be noted. First, as in the study of any topic of this kind (e.g., crime), I by definition cannot observe or analyze underlying patterns of unobserved misconduct. As noted above, data and research design limitations also prevent me from considering several concepts of interest and make it inadvisable to disaggregate the outcome measures by the type of alleged misconduct. I also cannot account for unobserved governor-level characteristics that might confound the relationship between approval ratings and scandal. In addition, I use state fixed effects rather than examining the many time-invariant factors that may be associated with corruption or media scandals but cannot be adequately disentangled in panel data. Finally, concerns about post-treatment bias preclude accounting for substantively important phenomena that may be consequences of allegations of misconduct such as damage control efforts and primary challengers.

Ultimately, however, these results demonstrate that the media scandals that so often dominate the headlines are not exogenous but instead the result of a fundamentally political process. We cannot understand when and why chief executives suffer from scandals without considering the role of strategic behavior and the context in which events take place. Allegations of misconduct that are widely publicized and inspire significant outrage in one context might never be made or would be rejected or ignored in another. In this sense, the occurrence and interpretation of political events may be even more subjective and context-sensitive than scholars have previously recognized.

Notes

[1]Rottinghaus (2014b) examines which factors are correlated with negative scandal outcomes among the set of observed gubernatorial scandals. This design conditions on a factor (the existence of a scandal) that is likely affected by the same causes as the dependent variable (see also Basinger and Rottinghaus 2012a,b), which can produce biased effect estimates (see, e.g., Elwert and Winship 2014). I therefore do not condition on scandal in any of my analyses below.

[2]It is important to note that the information or allegations that help give rise to scandals are not always exposed by the media or opposition party (though many are). They can also originate from prosecutors, ethics commissions, interest groups, dissidents within an administration, etc.

[3]The crowd-out effect from news congestion that I test below is the opposite of the claim that slow news periods may be especially prone to media scandals due to the low opportunity costs of scandal coverage and corresponding opposition party opportunism (e.g., Kurtz 1991).

[4]Absent highly unusual events, the size of the news agenda is relatively fixed. Coverage of disasters will therefore reduce the space for potential scandal coverage. In rare cases, the worst disasters can increase the total volume of news coverage by generating special local news coverage (i.e., live TV), but in these circumstances I would expect that both the relative share and absolute size of the available news agenda would still be diminished.

[5]There are other possible sources of investigations into potential scandal such as state ethics commissions (which I control for below) or federal prosecutors but the legislature and attorney general are especially important because they can come under the control of the opposition party.

[6]I account below for time-invariant differences in AP resources and coverage intensity between states using state fixed effects. My models also include the log of lagged gubernatorial coverage, which should account for any state-specific trends not captured by state and year fixed effects.

[7]Public and journalistic reactions may of course vary depending on the type of scandal allegation (financial, sexual, etc.). However, I do not disaggregate the outcome measures by scandal type due to the relatively small number of events, a lack of strong theory about how or why causal effects would vary by type, and concerns about misspecification (e.g., scandals of type A might affect the likelihood of scandals of type B). Controlling for or conditioning on the type or characteristics of scandal (e.g., Basinger and Rottinghaus 2012a,b) would also likely cause post-treatment bias.

[8]The Online Appendix provides more detail on the coding and validity of these measures. Most notably, I show that all of the scandals identified as such by the AP were also identified as scandals

in the AP State and Local Wire and in prominent newspapers from five randomly selected states during the periods for which coverage data was available from both sources.

[9]I treat party switchers as new governors after switching and exclude five independents.

[10]The Online Appendix provides further details on the construction and coding of each of these variables (lagged approval, neighboring state disasters, and attorney general partisanship).

[11]It is by definition not possible to measure unobserved misconduct, but if the assumptions of these designs are satisfied, my models will estimate the effect of contextual factors on the incidence of media scandal as a political event.

[12]Mean values were used when more than one poll was conducted in the previous year. If no polls were conducted that year, the most recently available prior poll or vote share measure was used as a proxy for approval. See the Online Appendix for further details on the construction of this measure.

[13]The conditional logit models are grouped by state, which is the equivalent of fixed effects. As a result, all states with no media scandals are dropped. Conditional logit also requires that the groups (states) be nested within clusters, which precludes the estimation of a model in which the standard errors are clustered by governor.

[14]I estimate linear probability models as well as conditional logit because OLS makes fewer distributional and functional form assumptions and is more robust to violations of those assumptions than GLM models (Angrist and Pischke 2009, 197–198).

[15]Again, I estimate models 4 and 5 using OLS because it makes weaker assumptions and is more robust to violations of those assumptions than GLM models (Angrist and Pischke 2009, 197–198). Model 6 is instead estimated as a fixed effects negative binomial model with robust standard errors clustered by state, which accounts for overdispersion and time-invariant differences between states.

[16]The standard errors for a conditional logit model are not available in closed form (Cameron and Trivedi 2010, 627–630). Confidence intervals are therefore omitted from this figure.

[17]The base rate of scandal onset in the data is 2.5% per year.

[18]This design (the equivalent to the reduced form of an IV model) assumes that the effect of disaster in a neighboring state with a shared media market mirrors the effects of normal variation in news congestion, which is necessary in any study of natural experiments (Dunning 2007).

[19]Expectations for the in-state disaster variable are ambiguous — it could crowd out scandal coverage in the state (Nyhan 2015) and thereby reduce the chance of scandal, but it could also either *decrease* gubernatorial approval (a different mechanism) or instead *increase* gubernatorial

approval if the state receives a federal disaster declaration (Gasper and Reeves 2011).

[20]Again, the indicator for within-state disasters is included as a control variable to account for potentially correlated weather shocks, not to test the news congestion hypothesis. Disasters could reduce media scandals via increased news congestion or higher approval due to a disaster declaration, but they could also increase media scandals if the governor is blamed for the damage or an inadequate response and loses public support (see, e.g., Gasper and Reeves 2011).

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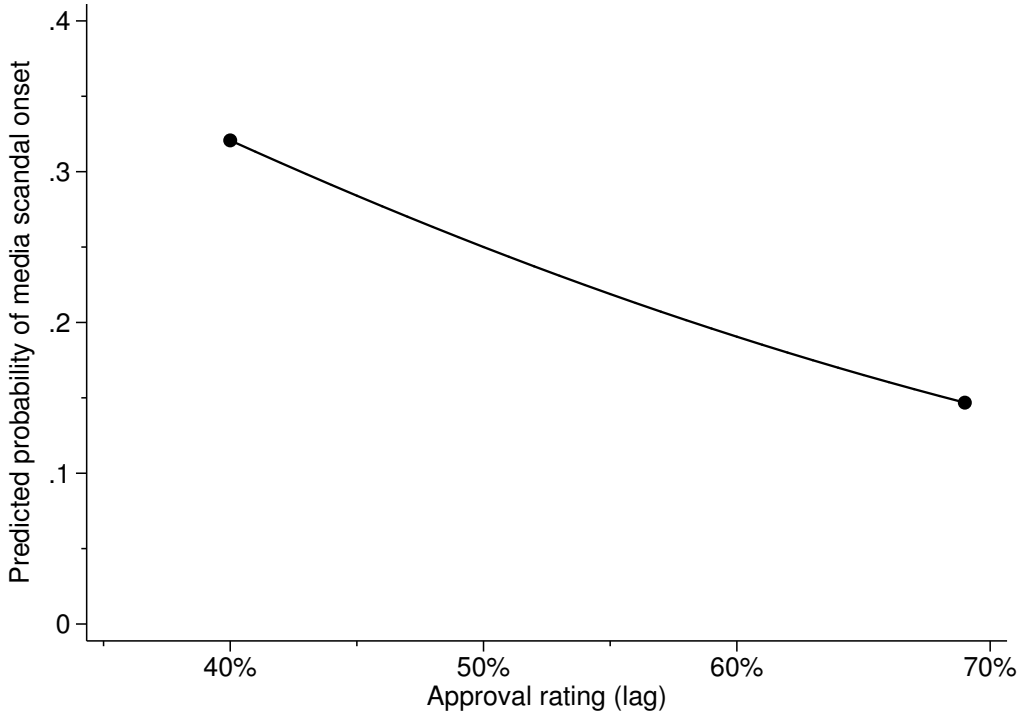
Biographical paragraph

Brendan Nyhan is a professor in the Department of Government at Dartmouth College whose research focuses on misperceptions about politics and health care. Before coming to Dartmouth, he was a Robert Wood Johnson Scholar in Health Policy Research at the University of Michigan. Nyhan received his Ph.D. at Duke University.

Figure 1: Predicted effects: Gubernatorial approval (lag)



Predicted probabilities from the conditional logit model in Table 1 with the state fixed effect set to zero. Continuous variables were set to their means; binary indicators and measures of time were set to their medians.

Table 1: Models of gubernatorial media scandal

	Scandal onset			Scandal coverage		
	(1)	(2)	(3)	(4)	(5)	(6)
Estimator	Logit	OLS	OLS	OLS	OLS	Neg. bin.
Approval (lag)	-3.481*	-0.075	-0.075*	-2.597*	-2.597**	-4.577*
	(2.030)	(0.049)	(0.044)	(1.299)	(1.101)	(2.762)
Divided government	-0.572	-0.016	-0.016	-0.021	-0.021	-0.121
	(0.564)	(0.012)	(0.012)	(0.140)	(0.152)	(0.611)
Opposition A.G.	0.142	-0.001	-0.001	-0.130	-0.130	-0.136
	(0.503)	(0.011)	(0.010)	(0.189)	(0.159)	(0.685)
Ethics commission	1.029	0.033	0.033	0.349	0.349	0.057
	(0.720)	(0.027)	(0.022)	(0.536)	(0.435)	(0.816)
Election year	-1.194**	-0.025**	-0.025*	-0.068	-0.068	-0.614
	(0.524)	(0.012)	(0.013)	(0.093)	(0.096)	(2.712)
Years in office	0.483*	0.008*	0.008*	0.111	0.111*	0.198
	(0.257)	(0.004)	(0.004)	(0.069)	(0.060)	(0.258)
Years in office^2	-0.038	-0.001*	-0.001**	-0.007	-0.007	-0.010
	(0.024)	(0.0004)	(0.0003)	(0.006)	(0.005)	(0.256)
Coverage intensity (lag)	-0.317	-0.006	-0.006	0.117	0.117	-0.003
	(0.505)	(0.010)	(0.009)	(0.098)	(0.105)	(0.374)
Full year served	1.132	0.019	0.019	-0.079	-0.079	-0.650
	(1.259)	(0.018)	(0.017)	(0.166)	(0.172)	(0.551)
Constant		0.051	0.051	1.098	1.098	-0.627
		(0.039)	(0.041)	(0.771)	(0.722)	(8.232)
State & year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Clustering	State	State	Gov.	State	Gov.	State
N	802	1684	1684	1684	1684	802

* $p < .10$; ** $p < .05$; *** $p < .01$; cluster-robust standard errors in parentheses (negative binomial: 1000 replications in a panel bootstrap).

Table 2: The effects of disaster news spillovers on gubernatorial media scandal

	Scandal onset		Scandal coverage		
	(1)	(2)	(3)	(4)	(5)
Estimator	OLS	OLS	OLS	OLS	Neg. bin.
Disaster (adjacent state)	-0.020**	-0.020**	-0.118*	-0.118	-15.634***
	(0.008)	(0.009)	(0.069)	(0.086)	(1.998)
Disaster (within state)	0.003	0.003	0.069	0.069	-0.245
	(0.015)	(0.016)	(0.118)	(0.111)	(7.459)
Approval (lag)	-0.114**	-0.114**	-3.260**	-3.260**	-8.674***
	(0.055)	(0.049)	(1.528)	(1.316)	(3.347)
Divided government	-0.014	-0.014	0.002	0.002	0.108
	(0.013)	(0.013)	(0.163)	(0.175)	(0.694)
Opposition A.G.	-0.000	-0.000	-0.158	-0.158	-0.278
	(0.012)	(0.011)	(0.213)	(0.181)	(0.739)
Ethics commission	0.043	0.043*	0.428	0.428	0.189
	(0.030)	(0.025)	(0.617)	(0.493)	(1.444)
Election year	-0.025	-0.025	-0.120	-0.120	-0.851
	(0.015)	(0.016)	(0.112)	(0.117)	(2.730)
Years in office	0.008*	0.008*	0.118	0.118	0.170
	(0.005)	(0.005)	(0.081)	(0.072)	(0.290)
Years in office^2	-0.001*	-0.001*	-0.008	-0.008	-0.011
	(0.000)	(0.000)	(0.007)	(0.006)	(0.028)
Coverage intensity (lag)	-0.006	-0.006	0.085	0.085	-0.082
	(0.010)	(0.010)	(0.123)	(0.127)	(0.592)
Full year served	0.012	0.012	-0.187	-0.187	-0.884
	(0.023)	(0.024)	(0.242)	(0.266)	(0.772)
Constant	0.080*	0.080*	1.651	1.651*	2.266
	(0.045)	(0.046)	(0.995)	(0.933)	(8.977)
State & year fixed effects	Yes	Yes	Yes	Yes	Yes
Clustering	State	Gov.	State	Gov.	State
N	1442	1442	1442	1442	693

* $p < .10$; ** $p < .05$; *** $p < .01$; cluster-robust standard errors in parentheses (negative binomial: 1000 replications in a panel bootstrap).

Table 3: Opposition attorneys general: Effects on gubernatorial media scandal

(a) *Gubernatorial elections*

	Scandal onset		Scandal coverage	
	(1)	(2)	(3)	(4)
Governor in party opposing A.G.	-0.03	-0.03	-1.66	-1.50
	(0.07)	(0.06)	(1.44)	(1.47)
Vote share margin	10%	20%	10%	20%
N	260	349	260	349

(b) *Attorney general elections*

	Scandal onset		Scandal coverage	
	(1)	(2)	(3)	(4)
Opposition attorney general	-0.05	-0.03	-0.27	-0.02
	(0.16)	(0.14)	(0.78)	(0.61)
Vote share margin	10%	20%	10%	20%
N	171	283	171	283

* $p < .10$; ** $p < .05$; *** $p < .01$; standard errors in parentheses. Models estimated are local linear regressions corresponding to the specified vote share range (e.g., $\pm 10\%$) with optimal bandwidths calculated following Imbens and Kalyanaraman (2012).

Table 4: State divided government: Effects on gubernatorial media scandal

	Scandal onset		Scandal coverage	
	(1)	(2)	(3)	(4)
Divided government	0.01	0.01	-0.68	1.46
	(0.08)	(0.08)	(0.91)	(1.20)
Seat share margin	20%	30%	20%	30%
N	314	499	314	499

* $p < .10$; ** $p < .05$; *** $p < .01$; standard errors in parentheses. Models estimated are local linear regressions corresponding to the specified seat share range (e.g., $\pm 20\%$) with optimal bandwidths calculated following Imbens and Kalyanaraman (2012).

Online Appendix

Media scandal coverage coding procedure

- Using the LexisNexis news database, Associated Press articles from the January 1, 1977–December 31, 2010 period were collected that included "governor," "Gov.," or "gubernatorial" in the headline or lead paragraph or "governor" in the terms and the word "scandal" anywhere in the text.

- To reduce false positives in the search results, all articles originating in the following sections were excluded: international news, business news, Washington dateline, sports news, entertainment news, lifestyle, commentary, and feature material.

- News articles were recorded that described a specific controversy involving the sitting governor or her state's executive branch (including their past actions before taking office) as a "scandal" in the reporter's own voice or in the headline or a photo caption. This definition excludes uses of the term in quotes and transcripts or those that are explicitly attributed to others by the reporter, descriptions of "alleged" or "potential" scandals or other qualified references to the term, or general references to scandal (e.g., "scandal-plagued") that do not identify specific controversies.

- Scandals that concern independently-elected officials in the state executive branch or specific elementary or secondary schools are excluded, as were news columns/analysis, book reviews, and opinion articles such as commentaries, op-eds, and editorials.

- Controversies about actions taken by the state executive branch under previous administrations are excluded unless the governor was previously lieutenant governor and was held politically responsible for the scandals of the previous administration. Scandals can only be new once and thus cannot be counted as beginning under both the administration of a governor and a successor.

- Scandals involving executive officials appointed under a previous governor were attributed to the serving governor if those officials continued to serve under her (including lieutenant governors who succeed the previous governor).

- Scandals involving state government contractors were excluded if there were no allegations of misconduct on the part of the sitting governor's administration.

- Applying the rules above, the scandal onset measure takes the value of 1 for each state-year in which at least one article with a qualifying reference to "scandal" devoted at least one of the first three paragraphs to the controversy. (The initial paragraphs of a news article describe the focus of a story or summarize its most important elements in the inverted pyramid structure typically used in American print journalism.)

- In two cases, more than one scandal began in a year (Ohio in 2005 and New York in 2010). In both cases, I treat the dependent variable as binary because the scandals were closely linked.

- In years in which one governor stepped down and was replaced, I count the number of media scandals and scandal articles for both governors and control for the number of months that each served in office during that year.

- The scandal coverage measure was calculated as the total number of articles with qualifying references to "scandal" each state-year. (Controversies that were never referred to as a "scandal" in the first three paragraphs of a story were excluded from the coverage data.)

The results from this procedure were highly correlated with the more detailed state-level coverage for a shorter time period (1999–2010) from the AP State and Local Wire, suggesting they are measuring the same underlying phenomenon. The correlation in scandal articles per year by state during the overlapping period is 0.83. Moreover, all 28 controversies first described as scandals in the AP's reporting during this period were also described as such

in AP State and Local Wire coverage (61% were described as scandals in both the AP and AP State and Local Wire in the same month, 85% within two months of each other, and 100% within six months).

In addition, a research assistant who was blind to the manuscript or its findings replicated its findings for high-profile media outlets in five randomly selected states. He was instructed to randomly select a state, identify the largest newspaper in the state by circulation, and determine if full-text archives for that newspaper exist in LexisNexis Academic database for at least fifteen years during the 1977–2010 period. If not, he was instructed to identify another newspaper that is among the top 100 by circulation in the U.S. from that state or is based in the state capital city and meets the standard of fifteen or more years of full-text archives in LexisNexis Academic. He then replicated the search procedure used in the article for the media outlets identified by this procedure (the *New York Times*, *Austin American-Statesman*, *Daily Oklahoman*, *Atlanta Journal-Constitution*, and *Bismarck Tribune*). His findings indicate that each of the scandals identified by the Associated Press during the period for which electronic archives are available from the identified outlets was also identified as such in the outlets themselves.

Finally, an additional validation check verified that the articles identified using this procedure were almost always written by AP reporters who focused on covering the state in question. In a random sample of identified articles, a search of other articles published by the same reporter within two weeks of the article in question found that 83% (43 of 52) of reporters had written exclusively about the same city or state in which the scandal occurred and 94% (49 of 52) had written more than 70% of their articles about that city or state.

Media scandals identified in AP coverage (1977–2010)

State	Scandal (charges/allegations)	Date first identified
WY	A.G. Frank Mendicino: Blocking embezzlement probe	10/31/1977
TN	Gov. Ray Blanton: Parole-selling	12/8/1978
NJ	Casino Control Commission: Abscam probe	2/6/1980
PA	Revenue Sec. Howard Cohen: Lottery scandal	12/2/1980
CA	Gov. Edmund Brown: Misuse of state-funded computers	7/13/1981
MA	Transportation Sec. Barry M. Locke: Bribery/conspiracy	3/19/1982
CA	Corrections Supt. Bertram S. Griggs: Laxity and special privileges	4/28/1982
CT	Transportation Comm. Arthur B. Powers: Corruption	5/5/1983
AK	Gov. Bill Sheffield: North Slope audit	5/27/1985
AK	Gov. Bill Sheffield: Steering lease to friend	2/23/1986
LA	Gov. Edwin Edwards: Pardon bribery	10/24/1986
TX	Gov. Bill Clements: SMU football recruiting	3/7/1987
AZ	Gov. Evan Mecham: Obstruction of justice/misuse of state funds	4/3/1988
FL	Dept. of Health and Rehabilitative Services: Overpayments	3/29/1993
FL	Gov. Lawton Chiles: "Phonegate" scandal	12/4/1995
AZ	Gov. Fife Symington III: Fraud, extortion, and perjury	6/15/1996
IL	Gov. Jim Edgar: Bribery charges	7/28/1997
UT	Gov. Mike Leavitt: Connections to Olympic bribery scandal	2/13/1999
IL	Gov. George Ryan: Selling trucking licenses	2/26/1999
MA	Massport Director Peter Blute: "Booze cruise"	8/24/1999
AL	State Troopers: Fixing traffic tickets	10/2/1999
MD	Juvenile Justice: Abuse by guards	12/26/1999
MA	Gov. Jane Swift: "Chopper Mom" scandal	1/13/2000
SC	Corrections Director Doug Catoe: Prison sex abuse	1/11/2001
AL	Department of Youth Services: State girls prison sex abuse	6/17/2001
KY	Gov. Paul Patton: Sexual harassment lawsuit	9/27/2002
CT	Gov. John G. Rowland: Summer home scandal	12/12/2003
NJ	Gov. James E. McGreevey: Sex scandal	8/19/2004
NY	Thruway Authority: Sweetheart building deal	12/30/2004
OH	Bureau of Workers Compensation: Rare coin investments	6/1/2005
OH	Gov. Bob Taft: Failure to report outings paid for by others	8/17/2005
KY	Gov. Ernie Fletcher: Hiring practices scandal	9/14/2005
IL	Teachers' Retirement System: Corruption	9/16/2005
IL	Gov. Rod Blagojevich: Hiring practices scandal	7/2/2006
TX	Youth Commission: Juvenile inmate sex abuse	3/1/2007
NY	Gov. Eliot Spitzer: Aides' plot to discredit rival	7/23/2007
NY	Gov. Eliot Spitzer: Prostitution scandal	3/10/2008
WV	W.V. University: MBA improperly awarded to governor's daughter	5/6/2008
AK	Sarah Palin: "Troopergate" scandal	9/20/2008
IL	Gov. Rod Blagojevich: Senate seat scandal	12/10/2008
NM	Gov. Bill Richardson: Corruption scandal	1/6/2009
SC	Gov. Mark Sanford: Infidelity	6/26/2009
NY	Gov. David Paterson: Aide domestic violence allegations	2/26/2010
NY	Gov. David Patterson: World Series ticket scandal	3/3/2010
IA	Film Office: Mismanagement/improper tax credits	10/21/2010

Coding of key independent variables

Gubernatorial approval

The percentage of respondents who approved of the governor was collected from the U.S. Officials' Job Approval Ratings project, which aggregates public polls from numerous sources (Beyle, Niemi, and Sigelman 2010); Survey USA's online archive of 50 state polls for the 2005–2006 period (`http://www.surveyusa.com/50StateTracking.html`); and the 2006–2009 common content of the Cooperative Congressional Election Study (`http://projects.iq.harvard.edu/cces/data`).[1] If more than one poll was conducted in a given year, the mean value was taken.

Due to the inconsistent availability of state-level polls, particularly in the early years of the sample, it was necessary to account for missing data. Approval ratings were available for 866 governor-years — approximately 50% of the 1729 observations in the data — and at least one approval poll is available for 78% of the governors in the data (231 of 296). The following procedures were used:

- When data were missing at the beginning of a governor's term, her initial vote share among the two major party candidates was recorded as her approval rating until the first poll was taken.

- The governor's overall vote share was used for independents or when the governor faced an independent in a runoff (Mills E. Godwin, Jr. of Virginia in 1977). The

[1] For the Cooperative Congressional Election Study data (CCES), only data from states with 500 or more respondents were used due to concerns about precision in small samples. The CCES studies use a five-point scale for approval. Respondents who strongly or somewhat approved of the governor were coded as approving; those who were unsure, neither approve nor disapprove, or somewhat or strongly disapprove were coded as not approving.

governor's primary vote share was used when the governor was unopposed in a runoff election (Edwin Edwards of Louisiana in 1975).

- Among unelected governors, the first available poll was used as their approval rating from the month they assumed office.
- Each poll was taken to be the governor's approval rating until the next available poll in the data.
- If polls were missing at the end of a governor's time in office, the last available poll was extended forward.

After these procedures, only 11 observations lack approval data (all are replacement governors for whom no election data or polls are available).[2]

Natural disasters in neighboring states with media market spillovers

To test H2, data were collected on whether a natural disaster took place in a neighboring state that has a media market which spills over into the state in question. Annual weather damage was calculated from the Spatial Hazards Events and Losses Database for the United States (also used in Healy and Malhotra 2009 and Gasper and Reeves 2011). These data provide a precise measure of weather damage costs (normalized as a percentage of state GDP) that can be compared between events and avoids confounding with political influences on disaster declarations and assistance (e.g., Reeves 2011; Husted and Nickerson 2014). We then identified those states with counties that are part of a media market that is dominated

[2]The presence of missing data at the start and/or end of some governors' terms preclude the use of moving averages, interpolation, or other related approaches for estimating approval between available public polls.

by another state.[3] Finally, we identified the most significant damage among neighboring states whose media markets spill over into the state in question. Given the non-linearity of damage from the most severe storms and the disproportionate attention they receive, I define a neighboring state disaster as a case in which the total damage from weather events exceeds the 95th percentile value in the worst-hit neighboring state whose media market spills over into the target state (1.91% of state GDP).[4] Finally, because these disasters could also affect the state in question, I construct an equivalent target-state disaster measure and control for it (damage exceeding 0.86% of GDP).[5]

Attorney general and state legislative data

Testing H3 requires data on attorney general partisanship and vote shares and legislative seats held by the major parties. To identify the effect of having an opposition party attorney

[3]I map counties to Nielsen media markets (DMAs) using data provided by Gentzkow and Shapiro (2008a; see 2008b). The available data uses 2002/2003 DMA definitions, but the counties and their mappings to DMAs rarely change (Gentzkow and Shapiro 2008b, 287). Within each market, I define the dominant state as the one with the largest share of within-DMA population. (Ansolabehere, Snowberg, and Snyder 2006 use a different definition, but their approach requires dropping units that do not qualify, which creates potential selection bias concerns.)

[4]There are 73 disasters in the data. The full list is provided in the Online Appendix below along with the most significant weather events during the state-years in question. It is important to note that this disaster measure is missing for states that are not part of media markets dominated by other states (Alaska, Hawaii, Louisiana, Maine, North Dakota, Rhode Island, and Utah). However, my results are robust to treating these as 0s (available upon request).

[5]Only six of the 73 cases with neighboring state disasters also suffered from an in-state disaster.

general on media scandal, it was necessary to create a dataset of all attorneys general, their party affiliation, and their method of selection for the 1977–2010 period using historical editions of the *Book of the States*. Among these, we identified all attorneys general from the major parties who were independently elected in contested races to serve alongside major party governors. We also collected the vote shares they received in their campaigns. The resulting dataset consists of 185 attorneys general in 314 elections from 1977–2009. These attorneys general served alongside 193 governors in 43 states, only 57% of whom were members of their party. The corresponding analysis of the effect of opposition control of one or more chambers of the state legislature uses legislative seats data that was generously provided by Carl Klarner.[6]

[6]Nebraska is excluded from my analyses due to its unicameral non-partisan legislature.

Weather events in neighboring states

Year	State(s)	Neighbor	Losses (GDP)	Most significant weather event
1978	OK	AR	-2.2%	Severe flooding
1978	IL, KY, MI, OH	IN	-3.1%	Blizzard of 1978 followed by flood
1978	IA, KS	NE	-2.2%	Storms, ice jams, melting, and flooding
1978	MO	NE	-2.2%	Severe flooding
1978	MA	RI	-2.1%	Snow and ice
1979	FL, GA, MS, TN	AL	-8.1%	Hurricane Frederic
1979	AL, AR	MS	-14.4%	Hurricane Frederic
1979	MN, MT, SD	ND	-3.8%	Red River flooding
1980	OK	AR	-6.5%	Tornadoes
1980	ID, MT, OR	WA	-20.0%	Eruption of Mount St. Helens
1983	AL, AR	MS	-8.2%	Flooding of Mississippi River
1983	CO, ID, NV, WY	UT	-5.5%	Flooding and mudslides
1984	NH, NY	VT	-3.0%	Flash flooding
1985	AL, AR	MS	-4.6%	Hurricane Elena
1985	KY, OH, VA	WV	-4.4%	Flooding
1988	MO, NE, SD	IA	-10.1%	The Tornado of 1988
1989	GA, NC	SC	-12.8%	Hurricane Hugo
1992	GA	FL	-9.6%	Hurricane Andrew
1992	OR, WY	ID	-2.9%	Tornadoes
1992	NH	ME	-3.1%	Flooding
1993	MO, NE, SD	IA	-12.6%	The Great Flood of 1993
1994	AZ	CA	-3.1%	Northridge Earthquake
1995	AR, MS, OK, TX	LA	-2.9%	New Orleans flood
1996	SC, VA	NC	-2.7%	Hurricane Fran
1997	MN, MT, SD	ND	-27.7%	Red River Flood of 1997
1999	SC, VA	NC	-2.1%	Hurricane Floyd
2000	AZ, CO	NM	-3.4%	Cerro Grande fire
2001	MT, SD	ND	-2.1%	Flooding
2004	GA	FL	-3.4%	Hurricane Ivan
2005	MS, OK, TX	LA	-26.8%	Hurricane Katrina
2005	AL, AR	MS	-32.4%	Hurricane Katrina
2008	MO, NE, ND	IA	-5.6%	Iowa flood of 2008

Further details on control variables in Table 1

First, I account for the potential institutional role of the opposition party in fomenting media scandals by including indicator variables for opposition party control of one or more chambers of the state legislature and whether an opposition party member served as state attorney general for one or more months in that year. As noted in the main text, opposition parties with majority control of one or more chambers of the state legislature or the state attorney general's office could investigate or publicize potential scandals in the governor's office or administration more effectively. Conversely, co-partisan control of the state legislature or the attorney general's office might help to prevent media scandals or reduce their severity. (I examine the effects of these variables more systematically in the tests of H3 in the main text.) I also account for possible effects of a state ethics commission[7] and for any contextual effects of a gubernatorial election year, which occurs on the federal election cycle in every state except Kentucky, Louisiana, Mississippi, New Jersey, and Virginia, which hold state elections in odd-numbered years. Another concern is differences in gubernatorial coverage levels. While state fixed effects will account for any time-invariant differences in state political coverage, I control for the total number of AP stories concerning the state's governor during the previous year to account for any differential trends in coverage of state politics.[8] Finally, I include an indicator for whether the governor was *potentially* at risk of scandal for a full year, which accounts for the small number of observations (97 out of 1753)

[7]State ethics commissions data were obtained from the National Commission on State Legislatures (`http://www.ncsl.org/research/ethics/state-ethics-commissions.aspx`).

[8]Specifically, coverage frequencies are measured using the Nexis search described in the Online Appendix for the prior calendar year $t-1$ excluding the "scandal" keyword. (The lag is necessary to avoid endogeneity — coverage in year t could increase as a result of additional coverage of a media scandal.)

in which a governor resigned or died before the year was complete for non-scandal-related reasons and was therefore at risk of scandal for a shorter period of time. The nine governors who left office mid-year amidst a scandal or ethics-related controversy were coded as being potentially at risk of serving for the full year to avoid post-treatment bias (King and Zeng 2006).[9]

[9]Unfortunately, it is not possible to account directly for state legislative polarization in these models. The best measures only go back to the 1990s (Shor and McCarty 2011; Bonica 2014). In addition, I do *not* control in this or other research designs for governors' use of damage control tactics (Basinger and Rottinghaus 2012a,b) or other outcomes or behaviors that could result from scandal or factors that precipitate it (e.g., primary challengers). Including such measures would create post-treatment bias (Rosenbaum 1984; King and Zeng 2006) if they are the result of the factors I identify as potential causes of media scandals in my hypotheses. For this reason, I also do *not* control for potentially endogenous measures of state-level corruption, which could bias my treatment effect estimate for approval ratings.

Summary statistics

	Mean	S.D.	n
Variables in Tables 1 and 2			
Scandal onset	0.02	0.16	1729
Scandal coverage	0.25	2.24	1729
Approval (lag)	0.55	0.11	1718
Divided government	0.54	0.50	1695
Opposition A.G.	0.39	0.49	1729
Ethics commission	0.60	0.49	1729
Election year	0.50	0.50	1729
Years in office	4.18	2.7	1729
Years in office^2	24.9	32.2	1729
Coverage intensity (lag)	3.94	0.95	1729
Full year served	0.95	0.23	1729
Disaster (adjacent state)	0.05	0.22	1488
Disaster (within state)	0.05	0.22	1720

Regression discontinuity model specification details

In the models presented in Table 3(a) in the main text, the running variable is the two-party vote share of the gubernatorial candidate who is not from the party of the attorney general or winning attorney general candidate. This design follows Leigh (2008), Folke and Snyder (2012), and Erikson, Snyder, and Folke (N.d.) in using an RD approach to obtain exogenous variation in partisan control of the governor's office.

The models presented in Table 3(b) in the main text instead use the two-party vote share of the opposition party's attorney general candidate (i.e., the one not affiliated with the governor or the winning gubernatorial candidate). These models again estimate the opposition party attorney general effect using data on the frequency and intensity of gubernatorial scandals in states that had an attorney general candidate from the party opposing the governor either narrowly win or lose election (either simultaneously or during their term).[10]

Both designs exploit plausibly exogenous variation in partisan divergence between the attorney general and governor, though they estimate different local average treatment effects on overlapping but distinct samples.[11] In either case, an observation consists of the relevant

[10]By estimating the model both ways, I account for possible restrictions on the range of the estimated local treatment effect. The models in which attorney general vote share is the running variable exclude attorneys general who were elected in lopsided elections or unopposed, but these officials — who might the most willing or able to challenge a sitting governor — can be included in the sample in which gubernatorial vote is the running variable if that vote share is in the specified range.

[11]Of the 427 governor/attorney general dyads in the two datasets for which the opposition party candidate's vote share was between 30% and 70% of the two-party vote, 242 appear in both datasets (57%). An additional 146 appear only in the governor vote data, while 39 only appear in the attorney general vote data. (The differences are attributable to factors such as successions, the timing of elections for the different offices, and the incidence of lopsided or unopposed elections.)

two-party vote share total as well as measures of the prevalence of gubernatorial scandal onset and intensity in the post-election period, which consists of the time until a resignation, death, or party switch by either the governor or the attorney general or the next election.[12]

By contrast, the running variable in the RD models of opposition party control of one or more chambers of the state legislature presented in Table 4 in the main text is the margin of seats in the relevant chamber that would be required to switch the legislature from unified control by the governor's party to divided government (opposition control of one or more chambers) or vice versa. I then estimate the difference in the likelihood and intensity of scandal after elections where the governor's party narrowly retained control of the state legislature and those where it narrowly lost control of one or more chambers.[13] The validity of this design requires the assumption that variation in seat shares around the discontinuity in party control at 50% is as-if random, which I evaluate using balance tests in the main text.[14]

Finally, it is important to be clear that the RD models reported in the main text estimate the difference in expected scandal frequency and intensity at the discontinuity in question

[12]Conditioning on the outcome of such an election could induce post-treatment bias (King and Zeng 2006) so I instead truncate the sample at the next relevant election (either governor or attorney general). The lengths of these post-election periods vary, but in practice, most cases consist of two- or four-year periods between elections depending on term lengths in a state and whether attorneys general and governors are elected simultaneously or on a staggered schedule. I provide balance tests below to evaluate the distribution of this and several other relevant covariates around the 50% vote share discontinuity.

[13]I exclude cases in which a chamber is divided evenly due to the ambiguity of how power will be distributed between the parties in this scenario.

[14]See also Feigenbaum, Fouirnaies, and Hall (N.d.), who use a more complex multidimensional RD design that seeks to identify exogenous variation in majority status in state legislatures using district-level vote shares.

(see Imbens and Lemieux 2008 or Lee and Lemieux 2010); they do not simply compare the difference in outcomes on either side of the discontinuity within the window that the model considers.

Balance tests

It is important to assess the extent to which the data satisfy the assumptions of a regression discontinuity model. The balance tests reported below indicate that cases in which the opposition attorney general candidate was narrowly elected were not significantly different from those in which the opposition candidate narrowly lost on several relevant covariates that could affect the likelihood of media scandal — the length of a governor's previous tenure, gubernatorial vote share, and state presidential vote share for the governor's party (10% vote share margin). I do observe that governor/attorney general dyads are observed for somewhat longer when they are from opposing parties (four months on average). This difference should *increase* the length of time in which the governor is vulnerable to media scandal from an opposition. However, the results below are again null even though this difference should bias them in *favor* of finding an effect. In addition, no significant differences were observed between cases in which gubernatorial candidates from the party opposing the attorney general were narrowly elected or defeated (10% vote share margin).

Similarly, balance tests for divided control of the state legislature find no significant difference in prior months in office, total months in session, governor vote share, or the share of the state presidential vote received by the governor's party in the most recent presidential election (20% seat share margin). Cases of divided government in these data did have higher levels of legislative professionalism than those with unified government (Squire 2007), but this difference should again bias the test in *favor* of finding an effect if opposition parties in more professionalized legislatures are more likely to have the resources and expertise to use control of a chamber to investigate the governor and foment scandal.

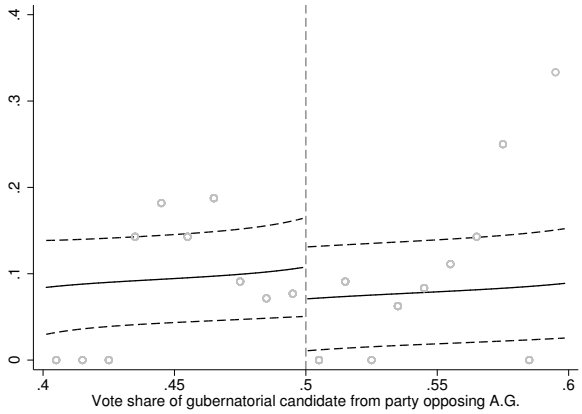
IV: Attorney general vote	Opposition A.G.	Co-partisan A.G.	p-value
Prior months in office (gov.)	25.9	23.4	0.58
Total months observed (gov.)	42.9	38.9	0.03
Governor vote share	56.7	57.5	0.38
Presidential vote (gov. party)	53.0	54.0	0.38
IV: Governor vote	Opposition A.G.	Co-partisan A.G.	p-value
Prior months in office (gov.)	17.8	15.5	0.50
Total months observed (gov.)	41.1	40.4	0.61
Attorney General vote share	80.2	84.	0.42
Presidential vote (gov. party)	53.0	54.6	0.11
IV: Legislative seat share	Divided government	Unified government	p-value
Prior months in office (gov.)	37.4	31.6	0.13
Total months in session	22.4	22.3	0.84
Governor vote share	58.3	57.4	0.25
Presidential vote (gov. party)	53.4	54.0	0.44
Legislative professionalism	0.25	0.19	0.0001

Attorney general and governor data represent major-party officials elected with 60% of the two-party vote or less (n = 171 for attorneys general and n = 260 for governors). Legislature data represents chambers under major-party governors with a margin of 20% of seats or fewer dividing unified from divided control. Cases in which one chamber was evenly divided were excluded (n = 314). Legislative professionalism data were compiled from Squire (2007) by Lindquist (2007); values are carried forward during intervals in which new ratings are not available. Governor and attorney general vote share are calculated as a percentage of the two-party vote. p-values are from two-sample t-tests with unequal variances.

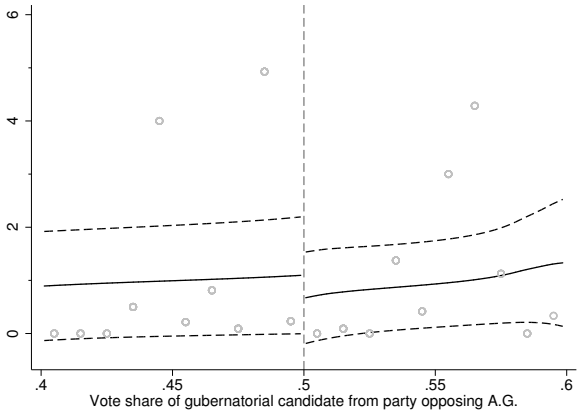
Additional figures

Figure A1: Opposition party control of the attorney general's office

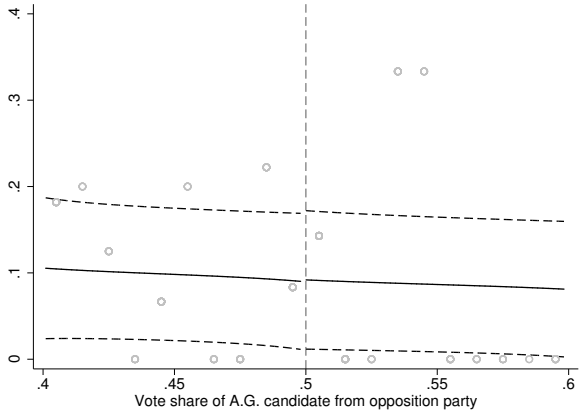
(a) *Governor vote: Scandal onset*



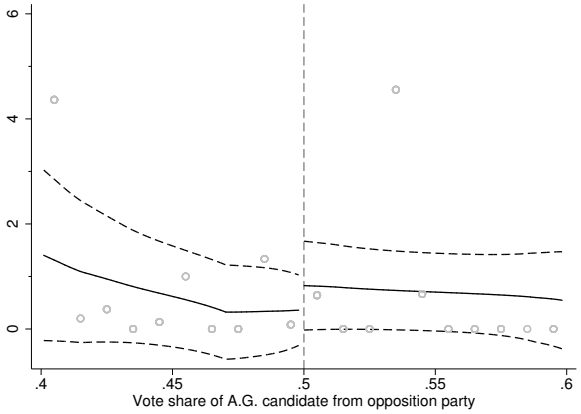
(b) *Governor vote: Scandal coverage*



(c) *Attorney general vote: Scandal onset*



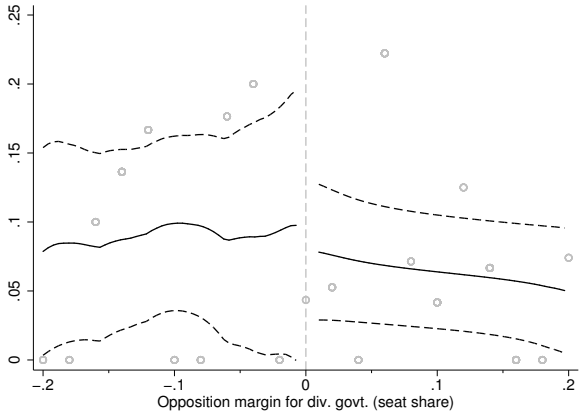
(d) *Attorney general vote: Scandal coverage*



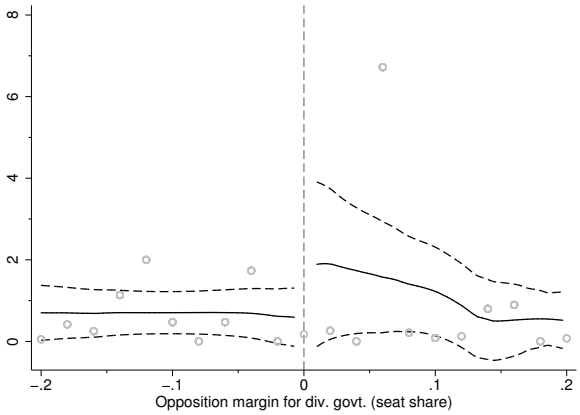
Local polynomial smoothing calculated using `lpoly` in Stata 13 (Epanechnikov kernel; rule-of-thumb bandwidth estimator). Points in the scatterplot represent binned average outcomes. Sample consists of contested gubernatorial and attorney general elections with a two-party vote share margin of less than 20% between major-party candidates in U.S. states from 1976–2009; media scandal data from Associated Press stories for the 1977–2010 period. Attorney general sample restricted to states with independently elected attorneys general. See text and Online Appendix for further details.

Figure A2: Opposition party control of the state legislature

(a) *Scandal onset*



(b) *Scandal coverage*



Local polynomial smoothing calculated using `lpoly` in Stata 13 (Epanechnikov kernel; rule-of-thumb bandwidth estimator). Points in the scatterplot represent binned average outcomes. Sample consists of contested gubernatorial and attorney general elections with a two-party vote share margin of less than 20% between major-party candidates in U.S. states from 1976–2009; media scandal data from Associated Press stories for the 1977–2010 period. Attorney general sample restricted to states with independently elected attorneys general. See text and Online Appendix for further details.

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