Strategic Outrage:

The Politics of Presidential Scandal

by

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Dissertation submitted in partial fulfillment of the requirements for the degree of Doctor of Philosophy in the Department of Political Science in the Graduate School of Duke University
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Abstract

In this dissertation, I take a new approach to presidential scandal, which is frequently attributed to evidence of misbehavior. I argue instead that scandal is a socially constructed perception of misbehavior which opposition elites help create. I formalize this argument by developing a model of presidential scandal, which predicts that allegations of scandal by opposition legislators can influence the occurrence of scandal within some intermediate range of allegation scandalousness and credibility. I derive two comparative statics showing that the incidence of scandal should increase as the transaction costs of allegations decrease and as the critical mass of opposition legislators required to create a scandal decreases.

I then test the predictions of the model using monthly data from elite news reports for 1977–2006. I operationalized the critical mass comparative static using presidential approval among opposition party identifiers—a useful index of a polarized political climate. I find that the president is more vulnerable to the onset of scandal when his levels of opposition approval in the previous month were relatively low. Conversely, when the president is relatively popular with opposition identifiers (during “honeymoons,” foreign policy crises, and wars), scandals occur much less frequently. In addition, scandals appear to have become more common over time, which could be the result of increased party polarization. Finally, I show that the underlying hazard of scandal was greater for second-term presidents than for first-term presidents.
Clearly, however, scandals vary widely in their size and significance. As such, I also create a dependent variable measuring the total quarterly volume of presidential scandal coverage in the *Washington Post*, which should capture the aggregate severity of scandals in a given time period. I show that presidential approval among opposition identifiers in the previous month is negatively associated with this measure. By contrast, more scandal coverage is published during presidents’ second term in office and during election years.

Journalists and scholars frequently assert that divided government leads to a greater incidence of presidential scandal, but little systematic evidence exists to support these claims. An investigation reveals that divided government suffers from several important inferential problems, including a lack of comparable counterfactual data. After addressing these issues, I estimate treatment effects for divided government and opposition control of Congress on both high-profile investigations of the president and scandal coverage, but none reach conventional levels of statistical significance.

Next, I explore the factors predicting when individual members of Congress will make scandal allegations against the president and the executive branch. Specifically, I test hypotheses developed from my formal model on a new dataset of scandal allegations against the president in the Congressional Record between 1985 and 2006. Results from multilevel event count models indicate that scandal allegations decline as state- and district-level presidential vote increases among members of the opposition party in both the House and the Senate. Members of the Senate are also more likely to make allegations as they gain seniority within the chamber. Finally, members who are up for re-election in the Senate make fewer allegations than those who are not.

Finally, I analyze the allegation data as a series of social networks. I present a new approach to analyze clustering in these data, which helps us to charac-
terize patterns in allegations and member behavior. My analysis indicates that clustering among members—which suggests a convergence in scandal targets—is positively associated with increased scandal coverage at the Congress level. By contrast, I find that highly clustered allegations (i.e. those made by members who also made other allegations together) tend to receive less coverage than those that attract support from a broader coalition of members who would otherwise not be connected.
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Introduction

Bill Clinton and George W. Bush both took office under challenging circumstances. Due to the candidacy of H. Ross Perot, Clinton won the presidency despite winning only 43% of the popular vote in 1992. Bush also failed to win a majority of the popular vote (or even a plurality) and only won the presidency after the Supreme Court ended the Florida recount in 2000. In addition, both men suffered from scandals during their presidential campaigns that centered on their personal lives (specifically, their efforts to avoid the draft during Vietnam, Clinton’s history with women, and Bush’s history with alcohol). However, their experiences in office rapidly diverged, especially with respect to scandal. Within months Clinton was enmeshed in the first of a series of scandals that consumed much of his presidency. By contrast, Bush’s first two years were virtually scandal-free and he survived his time in office largely unscathed. Why?

It’s surprising how little we know about this question given the role that scandals have played in contemporary politics. Between 1969 and 2008, seven men served as president. Of those seven, one president resigned to avoid being removed from office (Richard Nixon), one was impeached but not convicted (Clinton), a third narrowly avoided impeachment (Ronald Reagan), and a fourth was described as a subject of a criminal investigation and accused of “misconduct” and “a cover-up” by an independent prosecutor (George H.W. Bush).\footnote{The statements above were made by Lawrence Walsh, the Iran-Contra prosecutor, after George H.W. Bush’s pardon of former Defense Secretary Casper Weinberger in December 1992 (Johnston}
controversy centered on a scandal. On a more prosaic level, scandals have become a central component of national politics in the post-Watergate era. The opposition party in Congress, outside groups, and media commentators now routinely direct scandal accusations against the president and the executive branch, some of which gain traction and spur new episodes of accusation and recrimination.

To date, the predominant explanations for presidential scandals tend to be journalistic in nature. Typically, scandals are “explained” using *post hoc* narratives that rely heavily on speculative psychological explanations of individual behavior. For instance, the Clinton scandals were frequently interpreted as the result of a psychodrama between Clinton, Independent Counsel Kenneth Starr, and Republicans in Congress.

While such explanations were once in vogue among political scientists (Barber 1977), scholars now tend to take a different approach, treating politicians as purposive actors. Nonetheless, the discipline has made little progress in understanding the process by which scandals are created, as two prominent scholars of the presidency have recently pointed out:

> Perhaps because the topic is so salacious, the politics of scandal has not received the degree of serious scholarly attention it probably deserves. But if scandal seeking and scandal mongering are normal political tactics, like raising money or constituency service, then political scientists need to learn their logic (Cameron 2002, 655).

[S]candal is a real commodity with a real market, and it should be studied accordingly. It may call for a new subdiscipline of political science, which some have entitled *scandology*...[S]trategic exposures of these corrupt acts must be dealt with on their own terms, from within the
context of political institutions, practices, and rules rather than from some personal perspective on the morality of the players (Lowi 2004, 71; italics in original).

One reason for the lack of progress is that researchers have too often relied on what Lowi calls “some personal perspective on the morality of the players” (2004, 71). In this dissertation, I pursue an alternative approach, treating scandal as the result of strategic behavior by political elites. My central claim is that presidential scandal is a socially constructed event. It represents a widely shared perception of normative violations that has been promoted by members of the opposition party in Congress. Their motivation is to damage the reputation of the president and accrue individual benefits for their role in the process. However, their success in doing so varies depending on the underlying political conditions—principally, the level of polarization in the political climate (not, as many claim, divided government). Similarly, despite sometimes seeming like crazed ideologues, members of Congress do respond to political incentives in making (or not making) scandal allegations against the president.

Previous political science research on scandal

Before we can develop this approach, however, it is first necessary to understand the state of political science knowledge about scandal. As I show below, several literatures in American politics have touched on scandal, but they have typically focused on its effects rather than its causes. As a result, scandal itself remains an elusive and poorly understood concept.
Presidential scandal

While a number of informal hypotheses about the causes of scandal have been proposed by political scientists and journalists, no one has systematically analyzed the conditions under which presidential scandals are more (or less) likely to occur. The closest analogue to this study is Cameron and Segal (2001), who create a Markov model of the Senate confirmation process for Supreme Court nominees with three states: searching for a scandal, investigating an alleged scandal, or terminating the process. Surprisingly, they conclude that the probability of scandal emergence does not vary by opposition control of the confirmation process. However, Cameron and Segal find that the opposition party will spend more time than the president’s party searching for scandals and more time investigating any scandals it does find. The other most closely related study is Mayhew’s research on high-profile Congressional investigations of executive misbehavior, which he found were no more frequent under divided government between 1946 and 1990 but may have been more frequent in the 1991–2002 period.²

Much greater effort has been devoted to studying scandal’s effects on public opinion toward the president and the executive branch. The most extensive literature concerns the macro-level effects of scandal on presidential approval and voting, which are typically found to be negative. For instance, Ostrom and Simon (1985) create a dataset of consequential presidential events, including scandals, that appear in prominent chronologies and were featured on the front page of the New York Times. This methodology yielded a list of six scandals that had a significant negative effect on approval during the 1953–1980 period.³ Similarly, Fackler

² Parker and Dull (N.d.), which builds on Mayhew’s work, find that the number of hearings and the number of pages of hearing records under divided government seems to have increased in the period since Watergate.

³ Newman (2002) extends the Ostrom and Simon events data and that of Ostrom and Smith (1992) through 2000 to again test its effects on presidential approval, but he lumps scandal into the
and Lin (1995) construct a macro measure of corruption coverage using the number of articles listed under the topic in the *Reader’s Guide of Periodical Literature* from 1890 to 1992. They find a structural break in the time series at approximately 1929. For the 1932–1988 period, they find that greater levels of corruption coverage are associated with lower incumbent party vote share in presidential elections.

At the individual level, results are less clear. Though Funk (1996) shows experimentally that the reported presence of scandal reduces evaluations of fictional candidates, a study of presidential campaign effects from 1952–1992, which coded 13 scandals from events compiled by Gallup or mentioned in campaign coverage and political biographies that were also covered in the *New York Times*, found that campaign scandals have little effect on the preferences of voters in trial heat polls (Shaw 1999). One explanation for this finding is that voters’ reactions are typically mediated by factors such as partisanship, personal values, and candidate preferences. For instance, Stoker (1993) finds that Democrats reacted very differently to the sexual scandal that engulfed Gary Hart during the 1988 presidential primaries depending on their prior views toward Hart. Those who previously supported Hart based on policy reacted defensively, while other Democrats generally turned against him.4

Finally, a handful of studies have examined whether scandal affects the fortunes of the president in Congress. Meinke and Anderson (2001) find that higher levels of scandal coverage in the *Washington Post* for three major scandals — Watergate, Iran-Contra, and Monica Lewinsky — were associated with reduced sup-

4 Woessner (2005) also shows that public reaction to alleged scandals is sensitive to elite framing. In one experiment, he finds that opposition cues were most important in determining reaction to a scandal involving a fictional governor. A second experiment revealed that manipulating the frame used to describe President Clinton’s transgressions had a small but significant effect on retrospective evaluations of his impeachment.
port by House members for legislation supported by the president. Two other studies have examined the presence of scandal during the Senate confirmation process for presidential appointees to the executive and judicial branches. Krutz, Fleisher and Bond (1998) find that allegations of wrongdoing, which are often considered scandals, are associated with significant reductions in presidential nominees’ chances of Senate confirmation. They also consider the timing of these allegations and find that most were made before Senate hearings, concluding that they were not simply post hoc rationalizations of a nominee’s defeat. Lastly, Cameron and Segal (2001) show that the presence of scandals significantly reduces opposition support for Supreme Court nominees, especially during periods of divided government.

**Congressional scandal**

The most extensive literature on scandal in political science studies allegations of impropriety and scandal against members of Congress. However, as in the case of presidential scandal, no clear understanding has emerged about when or why scandals are more likely to occur among legislators. When considering corruption charges involving candidates for the House of Representatives, Peters and Welch (1980) and Welch and Hibbing (1997) observe no clear time trend, partisan differences, or effect of length of incumbency, though more scandals involved incumbents than non-incumbents.

Instead, most research has focused on the effects of scandal on reelection races. Charges of corruption have been shown to substantially reduce incumbent vote share, push up retirement rates, and increase the likelihood of losing a reelection bid for members of both the House (Peters and Welch 1980; Abramowitz 1991; Welch and Hibbing 1997) and Senate (Abramowitz 1988). In addition, scandal-weakened incumbents are also more likely to face quality challengers who are
well-funded, as Roberds (2003) finds in examining Senate data. The Congressional scandal that has received the most scholarly attention is the epidemic of overdrafts from the House bank, which was widely publicized before the 1992 election. For instance, Jacobson and Dimock (1994) find that greater numbers of overdrafts were associated with substantially increased probabilities of retirement and electoral defeat. They also find that overdrafts increased the likelihood that an incumbent would face a primary challenger who had previously held elected office.

*State corruption*

Another literature considers state-level corruption as a dependent variable. Meier and Holbrook (1992) find that the number of corruption convictions of public officials per state increases with the size of government, gambling arrests, and the percent of the state population living in urban areas, while the percentage of college graduates and increased party competition are associated with fewer convictions. Hill (2003) finds that states with greater levels of party competition and voter participation have reduced levels of corruption in the Meier and Holbrook data. Finally, a study of corruption conviction data by Glaeser and Saks (2004) concludes that states with higher levels of income and education suffer from less corruption, while states with greater racial diversity have somewhat more corruption than others. They also find that higher levels of education and income in the distant past are associated with reduced corruption today.

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5 Beck and Jackman (1998) suggest that the Jacobson and Dimock results may be affected by the linearity assumption of their regression. Using a generalized additive model, they find that the negative effect of overdrafts was concentrated among those incumbents with the highest number of bad checks; the effect in most races was more modest than the original regression coefficient suggested. Alford et al. (1994) reach an identical conclusion.
Parliamentary coalition dissolution

Finally, the process by which events like scandals affect the stability of parliamentary coalitions is the subject of an extensive comparative politics literature. After finding that existing models did a poor job of explaining coalition dissolution, Browne, Frendreis and Gleiber (1986) proposed throwing out game theoretic and statistical models of cabinet tenure that use the initial characteristics of a coalition as predictive variables. Instead, they proposed modeling the rate at which “critical events” such as scandal lead to cabinet dissolution as a Poisson process, which set off a spirited debate (see Strøm et al. 1988). King et al. (1990) unified this literature, showing how event history statistics could incorporate a varying stochastic hazard rate while also estimating the effect of covariates on coalition survival. The literature then progressed rapidly over the following decade. (I take a similar approach in the empirical analysis in Chapter 2.)

An elite-driven approach to the study of scandal

Given the limited extent of formal and quantitative research on scandal, I turn my focus to the qualitative literature on the subject, which is defined by two principal approaches that I critique below. I then argue for a new approach that defines scandal as the result of elite competition under varying political conditions.

The “objectivists” and “constructivists”

The first school of thought about scandal, which Adut (2005) calls “objectivist,” defines scandal with reference to “the conditions and characteristics of significant (exceptionally costly or offensive) transgressions” (216–217). A number of writers define scandal in this manner. Lull and Hinerman, for instance, write that a “media scandal occurs when private acts that disgrace or offend the idealized,
dominant morality of a social community are made public and narrativized by the media” (1997, 3). Some define political scandal more specifically as violations of the norms governing elected officeholders—“activity that seeks to increase political power at the expense of process and procedure” as Markovits and Silverstein (1988, 6–7) put it.

However, the moral or procedural standards that objectivists would use to define scandal vary over time and are applied inconsistently—many potentially scandalous events do not become scandals, while a significant number of scandals are based on shaky evidence. By “[t]reating scandals as the epiphenomena of real transgressions,” Adut writes, objectivists “[ignore] that the latter need not be authenticated to occasion scandals (as evinced by the Whitewater affair), and that unpublished yet very well-known transgressions... often do not cause scandals at all” (2005, 216).

Unlike the objectivists, “constructivists” do not assume that the nature of transgressions determines the extent of a scandal. Instead, Adut notes, these writers focus “on the social reactions to and representations of transgressions” (216). In particular, some authors define scandal primarily in terms of the public’s response to disclosure of some transgression, taking a more agnostic perspective about whether scandals necessarily entail norm violations. For instance, Thomp-son writes that “‘[s]candal’ refers to actions or events involving certain kinds of transgressions which become known to others and are sufficiently serious to elicit a public response” (2000, 13).

The problem with this perspective is that the public response to scandal allegations is not spontaneous. Instead, the extent of the public reaction to an alleged scandal is shaped by the actions of opposition elites. This claim is supported by an experiment conducted by Woessner (2005), who presented subjects with a news article in which he manipulated the opposition party’s response to a fictional per-
sonal scandal involving the governor of Alaska. He found that cues from the opposition party were the most important predictor of subjects’ views of the governor.\textsuperscript{6}

\textit{A strategic constructivist approach}

My approach differs substantially from both the objectivist and the constructivist approaches (though it is closer in spirit to the latter). Unlike the objectivists, I interpret scandal as a socially constructed event that takes place when a public figure’s actions are \textit{widely interpreted} as contravening established moral, political, or procedural norms. And unlike the constructivists, I do not attribute scandal to the needs of society or its response to transgression. Instead, I highlight the role of the political climate in aiding or hindering the efforts of opposing political elites to foment scandal—an approach that could be described as \textit{strategic constructivist}.

Given the limitations of an evidentiary or normative definition of scandal, I instead define the term as a widely shared perception of normative violations by a public figure. From such a perspective, the key indicator of whether a scandal has occurred is the fact that elites recognize it as such at the time. This definition coincides with the approach to electoral mandates used by Peterson et al. (2003) and Grossback, Peterson and Stimson (2005, 2006), who set aside the unanswerable question of whether mandates are “real” and instead code contemporaneous news reports to see if a mandate was perceived to exist (2003, 411).

How are these perceptions of scandal created? In the case of presidential and executive branch scandal, I argue that the key players are opposition legislators in Congress, who try to create perceptions of scandal to damage the president’s reputation (often working in tandem with outside groups). As Ginsberg and Shefter

\textsuperscript{6} Similarly, Dewan and Myatt (2007b) propose a model in which policy activism by cabinet ministers in parliamentary systems increases their vulnerability to scandal allegations promoted by opposing interests.
(2002) put it, they are pursuing “politics by other means.” The principal reason to use this tactic is that citizens face steep information asymmetries with political leaders and are therefore liable to extrapolate from perceived misbehavior (Adut 2005, 221), which explains the findings described above (Ostrom and Simon 1985; Newman 2002) that scandal depresses presidential approval.\(^7\) In addition, publicity of scandal allegations has second-order effects on individuals and institutions associated with the offender. For instance, “[t]he publicity of Clinton’s adultery stained the Democrats, the Democratic Party, and the presidency” (Adut 2005, 220). This externality creates an additional incentive for the opposition to try to foment presidential scandal.\(^8\)

However, opposition legislators in Congress will only politicize possible scandals when it is in their interest to do so. This claim parallels the argument made by Lupia and Strøm (1995) about the effect of events such as scandals on the stability of governing coalitions in parliamentary democracies:

> [E]vents such as wars, scandals, and economic shocks are not inherently critical. Instead, events become critical through their effects on parliamentary bargaining. Thus what makes an event critical is the behavioral response it occasions among the bargaining parties. To put it bluntly, **potentially critical events are meaningful only if they affect politicians’ abilities to achieve their legislative and electoral goals** (651–652; emphasis mine).

In other words, not all potentially scandalous events become scandals. The fate

\(^7\) It is important to note that findings may vary depending on how scandal is coded—minor scandals may have little or no effect on presidents’ approval ratings. Nonetheless, presidents appear to take the threat of a damaging scandal very seriously.

\(^8\) These externalities create similar incentives for parties to promote scandal allegations against leaders in Congress and governors. By contrast, the incentive to manufacture scandals against individual members of Congress are more limited. As a result, when they are targeted by scandal allegations, it is typically law enforcement officials from their state or district who do so.
of such potential scandals depends on the strategic choices made by opposition legislators and the political context in which events occur. (These intuitions are made precise in the formal model presented in Chapter 1.)

Overview of the dissertation

The dissertation is structured into two sections. The first, which consists of Chapters 1–4, examines scandal at the macro level. The goal of this section is to understand the emergence and extent of presidential and executive branch scandals. The second, which consists of Chapters 5–6, focuses on individual members of Congress and the specific allegations of scandal that they make against the president. In this section, I seek to explain which individual members of Congress make allegations against the president and how the structure of the scandal allegation network relates to media coverage of those allegations.

Specifically, Chapter 1 develops the theoretical intuitions presented above into a formal model. The model, which is adapted from the macroeconomics literature on currency crises, captures several important aspects of the scandal generation process. First, it models the decisions of legislators rather than abstracting away from individual actors. Second, the model accounts for strategic complementarity among scandal allegations (i.e. legislators’ payoffs for allegations are increasing in the decisions of other legislators to make allegations), which can create difficulties for game theoretic models. Third, the model formalizes the interplay between the strategic choices of legislators, the characteristics of the alleged offense, and the underlying political environment. In equilibrium, I find that legislators cannot create scandals out of trivial offenses, but that attacks by opposition legislators can result in the occurrence of scandal for allegations in some intermediate range of seriousness and credibility. The rate at which scandals occur is found to be
influenced by both the transaction costs of allegations and the size of the critical mass of opposition legislators required to create the perception of scandal.

Chapter 2 then tests the predictions of the model from Chapter 1 on new quantitative data. I measure the dependent variable—the onset of scandal—as the first month that a controversy was described as a scandal in Washington Post news reports or as the first month in which a scandal was coded as occurring in expert-coded presidential events data based on New York Times news reports. I then operationalize the comparative statics derived from the formal model. First, I interpret the critical mass comparative static with respect to approval of the president by opposition party identifiers, which indexes the polarization of the political climate (in particular, it increases during presidential “honeymoons,” foreign policy crises, and popular wars). In a depolarized political climate (i.e., one with high opposition approval), the critical mass of opposition legislators required to create a scandal should be higher than in a more polarized climate. Second, I interpret the transactions costs allegation with respect to two variables—divided government, which should reduce transactions costs for opposition legislators, and party polarization (which I operationalize as a simple time trend given the correlation between polarization and time in this period). My statistical analysis, which employs a conditional gap time Cox proportional hazards model, shows that the likelihood of scandal onset increases as lagged opposition approval declines. The incidence of scandal has also apparently increased over time in tandem with polarization. However, the divided government prediction receives no empirical support.

Clearly, however, scandals vary widely in their size and significance. As such, Chapter 3 broadens the focus of my empirical analysis to consider the magnitude of scandal rather than just its onset. To do so, I create a new dependent variable at the quarterly level measuring the total volume of presidential scandal coverage.
in the *Washington Post*, which should capture the aggregate severity of scandals in any given period (thus accounting for variance in scandal size as well as possible confluences among multiple scandals). Using a Poisson autoregressive model, I show that lagged presidential approval among opposition identifiers is negatively associated with the magnitude of scandal coverage at the quarterly level. In addition, I find that more scandal coverage is published during presidents’ second term in office and during election years. By contrast, while the coefficient for divided government is in the expected direction (positive), it again fails to reach statistical significance.

In Chapter 4, I investigate the puzzling insignificance of divided government in the previous analyses, which contradicts the claims of numerous journalists and qualitative scholars regarding the causes of scandal. My findings are consistent with the most systematic analysis of high-profile investigations of the president (Mayhew 1993, 2005), which finds that such investigations are no more frequent under divided than unified government. However, I show that the literature on the effects of divided government has important methodological problems, especially model-based extrapolation due to a lack of counterfactual data. I use non-parametric matching to address these issues, which could explain the contradiction between theory and data, but still find no convincing evidence that divided government (or a unified opposition Congress) increases presidential investigations or scandals.

Next, I turn my focus to individual members of Congress in Chapter 5, which seeks to explain the process by which scandal allegations are generated. Using data coded from the Congressional Record, I estimate hierarchical event count models predicting the number of allegations made by Congress for opposition legislators. (These models are estimated separately to allow for chamber-level heterogeneity.) Given the comparative static predictions that members will respond
to transactions costs in making scandal allegations, I test whether the number of allegations made varies by district- and state-level electoral costs and seniority. As the president becomes more popular among the constituents in a member’s district or state, the transactions costs of scandal allegations should increase, thereby decreasing the number of allegations made. Similarly, the costs of allegations should increase for members of the Senate during the electoral cycle in which they are running for re-election. By contrast, increased levels of seniority should increase the stature of opposition legislators and thereby reduce the transactions costs of allegations. I find strong support for the first prediction—opposition House and Senate members make fewer allegations as district- or state-level presidential vote increases, as do senators who are up for re-election (though the effect is only marginally significant). However, the effects of seniority are only marginally significant in the Senate and fail to reach significance in the House.

Chapter 6 then seeks to gain insight into the linkages among legislators and the scandal allegations that they make. To do so, I construct Congress-level social networks from the allegation data in Chapter 5. After validating this approach, I present a new measure of clustering in this kind of data, which is called the bipartite redundancy coefficient. I argue that this measure has substantive importance in two respects. First, clustering by members around specific allegations indicates convergence on the most important or productive targets—a pattern that should be associated with greater news coverage of those allegations. Conversely, clustering of allegations around certain members indicates that a subset of legislators are issuing a stream of similar allegations. These allegations are likely to be perceived as less informative than those made by other legislators who do not engage in this behavior and thus should receive less scandal coverage. I find support for both predictions using the bipartite redundancy coefficient and data from the Washington Post.
Finally, the conclusion reviews my findings and discusses possible extensions of this research, including moving downward to the state level to study governors and extending the scope of my data over a longer sweep of American history using newly available historical newspaper archives now available online. In addition, I link my work in this dissertation with several other studies and suggest that a quantitative, scientifically neutral approach holds great promise for understanding other subjective political perceptions such as government performance failures. I close by reviewing the normative reasons why it is important to study scandal.
Modeling Scandal as a Strategic Process

To date, the literature has typically defined scandal with regard to either the severity of the alleged offense or the public response it elicits, but neither definition can adequately distinguish scandals from non-scandals. In addition, previous theories do not capture the strategic aspects of the process by which potential allegations become public controversies. First, political context (including how the public views the official in question) will influence the likelihood of any allegation being recognized as a scandal. Second, most alleged offenses only come to the public’s attention if they are politicized by opposing elites, who are sensitive to context in choosing when to make scandal allegations.

In the case of US presidential and executive branch scandal, I argue that the relevant elites are members of the opposition party in Congress, who are more likely to successfully promote allegations against the president when the political circumstances are favorable for scandalmongering. To analyze this process, I adapt a formal model of currency crises—a phenomenon that has similar dynamics to presidential scandal—and derive predictions from the model.
One potential problem is that opposition legislators’ decisions to promote scandal allegations can be seen as strategic complements (i.e. the expected payoff of an allegation increases as more legislators make that allegation). Such coordination games are often intractable, but I employ a type of model from macroeconomics known as a “global game” that makes it possible to derive a unique equilibrium and comparative statics. Using such a model, I show that when the “scandalousness” of the president’s actions is in some intermediate range, a sufficient number of allegations from opposition legislators can generate the critical mass necessary to give rise to a scandal.

What is a global game?

As Morris and Shin (2003) point out (see also 2001), players’ payoffs in many situations depend on both the state of the world and the actions of other players. To make models of these situations more tractable, analysts frequently impose the requirement that the state of the world and other players’ actions are common knowledge in equilibrium. In addition to being unrealistic, these assumptions frequently lead to multiple equilibria. Global games, which have not been used widely in political science, sidestep this difficulty via three plausible assumptions.¹ First, players are assumed to receive a noisy signal of the underlying state of the world rather than having perfect information. In addition, players do not know what signal other players received, but they do know the distribution of private signals. Finally, it is assumed that players place a uniform prior over other players’ actions, reflecting uncertainty over the signals the other players received. The combination of these assumptions is frequently sufficient to pin down unique equilibria.

¹ Dewan and Myatt (2007a) is apparently the only published article to date that has used a global games approach in its formal model.
For instance, Morris and Shin (1998), one of the most prominent global games, analyze currency speculators who attack a currency that is pegged to a fixed value, attempting to profit from an anticipated decline in value if it is allowed to trade freely. If an insufficient number attack the currency, the peg holds and the traders lose money. However, if enough traders attack, the government gives in, the value of the currency declines, and the traders profit. In this case, a perfect information model results in multiple equilibria—for some intermediate level of economic fundamentals, either no speculators attack because they believe no one else will attack or everyone attacks believing everyone else will attack. However, if speculators have some uncertainty about the fundamentals, a unique equilibrium can be derived.

Assumptions of the model

I reinterpret the traders in Morris and Shin’s model as opposition legislators who receive a noisy signal of the seriousness and credibility of a possible scandal. Each must decide whether to make an allegation in that period. Then, depending on the seriousness and credibility of the perceived offense, the political circumstances in which the game takes place, and the number of opposition legislators who make an allegation, a scandal results (or not).

Formally, assume a continuum of opposition legislators exists who are considering whether to make a scandal allegation against the president or the executive branch. We represent the president’s current political standing without a scandal as $s^*$ and the seriousness and credibility of the alleged offense as $\theta$, which is distributed uniformly on the unit interval ($\theta \sim U[0, 1]$) where 0 represents the most damaging offense possible. The opposition does not know $\theta$ but receives some noisy signal of its value. If the opposition successfully creates a scandal,
the president’s political standing is some function of the seriousness and credibility of the alleged offense \( f(\theta) \). We assume that a scandal will either make the president’s standing worse or have no effect \((f(\theta) \leq s^*)\) and that the president’s post-scandal standing increases as the seriousness and credibility of the alleged offense decreases \((f(\theta) \text{ is strictly increasing in } \theta)\).

The opposition legislator’s utility function has two components. First, define a reward function \( R(\theta) \) to a legislator who makes an allegation that becomes a scandal as \( R = s^* - f(\theta) \). Each legislator who successfully attacks receives a payoff that is increasing in the seriousness and credibility of the alleged offense (i.e. \( R \) is continuous and strictly decreasing in \( \theta \)). This payoff can be interpreted as reputational enhancement from the public for preventing wrongdoing (e.g. Senator Sam Ervin after Watergate) or from activists and fellow partisans for damaging the president (one example might be Rep. Christopher Cox, who ascended into the House GOP leadership after his aggressive denunciation of alleged Clinton scandals in 1993 and 1994).\(^2\) We normalize the payoff for legislators who do not attack to zero and assume a positive transaction cost \( t > 0 \) for those opposition members who allege a scandal. \( t \) can be interpreted as the opportunity cost of devoting one’s efforts to promoting a scandal (rather than, say, public policy or campaigning) or as the reputational cost of making a scandal allegation against the president.\(^3\) The payoff for legislators who attack is therefore \( R - t \) if they successfully promote a scandal allegation and \(-t\) if they fail.

Each opposition legislator receives an independent signal of the alleged of-

\(^2\) Note that the reward function above does not include any collective reputational benefits. All opposition party members share the benefits of a presidential scandal to their party brand, which is a public good for party members (Grynaviski 2002). Given that the probability mass of each legislator is zero in the model, the probability of any member being pivotal is zero and the collective benefits of scandal are thus irrelevant to their decisionmaking.

\(^3\) While views may differ on the magnitude of \( t \) in contemporary politics, it must only be strictly positive for the purposes of the model.
fense’s seriousness and credibility that is distributed uniformly within some range around the true value \( (x|\theta \overset{iid}{\sim} U[\theta - \epsilon, \theta + \epsilon]) \). This assumption can be interpreted as reflecting imprecision in the available evidence or bias in interpreting it. After receiving a private signal \( x \), each opposition legislator simultaneously decides whether to make an allegation or not.\(^4\) The likelihood of a scandal occurring is defined by some underlying continuous function \( g(\alpha, \theta) \) for which a scandal is more likely when the seriousness and credibility of the alleged offense is greater (i.e. \( g \) is decreasing in \( \theta \)) and when more legislators make an allegation (i.e. \( g \) is increasing in \( \alpha \)). Finally, we assume that a scandal results when this function exceeds some exogenous tipping point or threshold (i.e. a scandal results if \( g(\alpha, \theta) \geq m \) where \( m \) is given by nature).\(^5\)

We make two key assumptions about the relationship between the severity of the alleged offense and the existence of scandal. First, we assume that some offenses are so compelling that they will become scandals even if no one makes an allegation. (Define \( \bar{\theta} \) as the value of \( \theta \) that solves \( g(0, \theta) = m \). A scandal will always ensue if \( \theta < \bar{\theta} \) since a scandal takes place if \( g(\alpha, \theta) \geq m \) and \( g \) is decreasing in \( \theta \).) Conversely, we assume that some alleged offenses are so dubious or trivial that they are not worth the cost of making an allegation (\( \exists \bar{\theta} \) where \( \bar{\theta} > \bar{\theta} \) and \( \bar{\theta} < 1 \) for which \( R(\bar{\theta}) = t \)).\(^6\)

\(^4\) Of course, coordination between legislators in promoting scandal allegations is possible. However, modeling such coordination is difficult or impossible, particularly when it is unobserved. Here, we rely on the notion of the modern legislator as an individual entrepreneur responsible for his own electoral fate (Aldrich 1995).

\(^5\) One could think of the media as defining this threshold \( m \) or simply as reflecting it in news coverage.

\(^6\) Also, for technical convenience, we assume that these thresholds are not too close to the edges of the unit interval (\( \bar{\theta} > 2\epsilon \) and \( \bar{\theta} < 1 - 2\epsilon \)).
We can now construct a continuous function $a(\theta)$ that represents the critical mass of legislators who must make an allegation for some alleged offense to become a scandal. As assumed above, this function takes the value of 0 for the most damaging alleged offenses, which become scandals no matter what ($a(\theta) = 0 \forall \theta \leq \bar{\theta}$). Otherwise, $a(\theta)$ is the number of legislators who must attack to create a scandal for a given allegation (the value of $\alpha$ that solves $g(\alpha, \theta) = m$ for any value of $\theta$).\(^7\)

Substantively, the range of values of $\theta$ that are of greatest interest fall in the region $(\bar{\theta}, \bar{\theta})$. In these cases, scandals are possible but not assured—whether a scandal will ensue or not depends on whether the required critical mass of legislators makes an allegation. Figure 1.1 provides a graphical representation of this point by plotting an arbitrary representation of the critical mass function $a(\theta)$ over

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\(^7\) Note that we assume $a(\theta) < 1$ from the definition of $\bar{\theta}$.
the range of $\theta$. To restate the assumptions above, if $\theta \leq \bar{\theta}$, a scandal ensues no matter what. Thus, the required critical mass is 0. If $\theta \geq \bar{\theta}$, it will never be worthwhile for an opposition legislator to make an allegation. As a result, the critical mass required to create a scandal is undefined in that range. But if $\bar{\theta} \leq \theta \leq \bar{\theta}$, then the president is “ripe for attack” (Morris and Shin 1998, 590). In this intermediate range of allegation seriousness and credibility, politics matters. (As the figure makes clear, the required critical mass $a(\theta)$ is increasing in $\theta$ in this region.)

*Equilibrium comparative statics*

Using $a(\theta)$, we can define a reduced form game for the first stage and derive a unique equilibrium. Morris and Shin show that a unique solution exists such that opposition legislators make an allegation if and only if their private signal $x$ is less than a threshold level $x^*$ (i.e. $x < x^*$) and a scandal takes place if and only if the seriousness and credibility of the alleged offense is below a threshold level $\theta^*$ (i.e. $\theta \leq \theta^*$). This equilibrium is characterized in Appendix A. From it, we can derive two relevant comparative statics.

The first concerns the effect of a shift upward in the $a(\theta)$ function.

**Proposition 1.** As the critical mass of legislators $a$ required to create a scandal increases, the incidence of scandal decreases. (Proof: See Appendix A.)

This result, which is illustrated in Figure 1.2, makes a great deal of intuitive sense. In equilibrium, legislators will only attack when the expected benefits (the payoff for a successful allegation multiplied by the probability of success) are equal to or greater than the transaction cost of making an allegation. As $a$ increases for any given level of $\theta$ in the intermediate region where the president is ripe for attack, the number of legislators who must make an allegation for it to become a scandal increases. As such, the expected benefits of an allegation are
lower and fewer scandals will result.

The second comparative static considers the effect of a change in the transaction cost term $t$.

**Proposition 2.** As the transactions cost $t$ of scandal allegations increases, the incidence of scandal decreases. (Proof: See Appendix A.)

Again, the result is highly intuitive. When scandal allegations are more costly to make, legislators will make them less frequently. Conversely, when the cost of making allegations decreases, opposition legislators should be more willing to promote an alleged scandal.

Before we turn to empirics in the next chapter, it should be noted that neither comparative static is defined with respect to allegation seriousness and credibility $\theta$ or the number of allegations made against the president $\alpha$, which are intermediate parameters in the model. The comparative statics are instead defined with
respect to the underlying characteristics of the political environment—namely, the critical mass function $a(\theta)$ and transaction costs $t$.\(^8\)

Conclusion

In this chapter, I have presented a model that captures the interplay between the underlying political fundamentals and the actions of individual members of Congress in creating presidential scandal. In particular, the model helps explain the weak or nonexistent relationship between factual evidence of misbehavior and the occurrence of scandal, a stylized fact that often puzzles political commentators. I argue that there exists some middle range of “scandalousness” in which an alleged offense will only become a scandal if a sufficient number of opposition legislators promote the allegation. In this range, political factors may change either the critical mass required to create a scandal or the transaction costs of an allegation, both of which affect the incidence of scandal for reasons unrelated to factual evidence.

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\(^8\) To understand the motivation for this approach, consider the macroeconomics literature on currency crises that inspired this model. In that context, the primary goal is to understand the underlying factors that make currency crises more likely rather than focusing on intermediate factors such as government reserves or the size of the attack on the currency. This study takes a similar approach in an attempt to isolate the contextual factors that make presidents more vulnerable to scandal in general. Future work will examine the process by which individual allegations become scandals in greater detail.
When is the President Vulnerable to Scandal?

In this chapter, I test predictions from the model presented in Chapter 1 on two new measures of presidential scandal onset. To do so, I define the relevant set of independent variables, describe the construction of two dependent variables from *Washington Post* and *New York Times* data for 1977–2006, and introduce the statistical methods used to conduct my analysis. My principal finding is that the president becomes more vulnerable to the onset of scandal as approval from opposition party identifiers in the public declines. As expected, scandal onset is also found to increase over time in tandem with the trend toward greater party polarization. Surprisingly, however, the estimated effect for divided government is not significant.

Independent variables

Our first task is to operationalize the comparative statics from the theoretical model in the previous chapter.\(^1\) Proposition 1 states that the incidence of scan-

\(^1\) It is worth noting that I also follow the approach described in the previous chapter in treating allegations as intermediate parameters rather than independent predictors of scandal. They are
dal will decrease as the critical mass of legislators required to create a scandal increases. Conversely, as the critical threshold decreases, the incidence of scandal will decrease.

A natural interpretation of this finding is the prediction that presidents will become more vulnerable to scandal as public approval of their job performance declines (which might reduce the critical threshold to scandal onset). However, there is a well-known paradox in the contemporary era—Bill Clinton, who was famously impeached by Republicans in Congress for allegedly lying under oath about his affair with Monica Lewinsky despite having approval ratings above sixty percent. This action was the culmination of a years-long assault on Clinton by Congressional Republicans who did not abandon their pursuit of the president even as his approval ratings rose to high levels in the 1996–1998 period.

As a result of this disjunction, I instead interpret this prediction with respect to approval of the president by opposition party identifiers among the public, which should better capture the level of polarization of the political environment and therefore be a more accurate index of the critical mass of opposition allegations required to create scandal. I disaggregate approval in this way because independent approval is very highly correlated with overall approval and the president almost always has high approval from his own partisans in the contemporary period (Jacobson 2007). By contrast, there is wide variation in opposition approval, which is especially high during periods of reduced partisanship caused by so-called presidential honeymoons, foreign policy crises, and wars (Lebo and Cassino 2007), which often feature largely “one-sided” information flows (Zaller 1992). Under these circumstances, a relatively large critical mass of opposition leg-

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2 A possible exception occurs during 1979–1980, a period during which Jimmy Carter’s approval among Democrats intermittently dipped below 50%.

islators should be required to create a scandal, decreasing the expected incidence of scandal. Conversely, when opposition party approval is low and the public is sharply polarized along partisan lines, the critical mass required to create a scandal should be much lower, increasing the likelihood of a scandal occurring. This operationalization of the critical mass comparative static is qualitatively consistent with the experience of President Clinton, who started office with relatively low opposition approval (see Figure 2.2 below), and also helps to explain the silencing of Democratic scandal allegations that occurred during the Gulf War and the aftermath of the September 11, 2001 terrorist attacks, both of which pushed approval among Democratic party identifiers above eighty percent.

To measure this variable, I employ opposition party approval data from the Lebo and Cassino (2007) monthly dataset of partisan approval. The data are constructed as the monthly percentage of Republican and Democratic identifiers (but not leaners) who approve of the president in Gallup or CBS/New York Times data.\(^3\) To guard against endogeneity, this variable is lagged by one month.\(^4\)

Proposition 2 in Chapter 1 states that the incidence of scandal should decrease as the transaction costs of allegations increase, a prediction that has two straightforward interpretations in contemporary politics. First, it is plausible that opposition control of at least one house of Congress enables scandal-mongering by providing the opposition with subpoena power, thereby reducing the transaction costs of creating plausible allegations of scandal, and providing an institutional platform for opposition legislators to make scandal allegations against the president.\(^5\) This intuition is supported by several informal claims that the per-

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\(^4\) The results in this chapter also hold if this variable is lagged by two months.

\(^5\) While the opposition party’s willingness to pursue scandal-mongering instead of legislative
vasiveness of divided government has led to more scandal in the contemporary era (Valelly 1992; Ginsberg and Shefter 2002; Lowi 2004). It is also partially supported by empirical research described above (Mayhew 2005; Parker and Dull N.d.). Similarly, growing party polarization may have reduced the reputational cost associated with scandal allegations over time. Given the high levels of correlation between measures of Congressional polarization and time in the contemporary era, I use a simple time trend (the number of calendar years since 1976) to capture this potential change in the norms of political debate.

I also identify two other important control variables. First, scandals may be more likely to occur during election years, a period during which the two parties are often more aggressive in trading accusations (Williams 1998, 124). I operationalize this idea as a dummy variable that takes the value of 1 from January to November of election years (including midterms) and is 0 otherwise. Second, the now-defunct independent counsel statute may have increased the number of scandals by creating a mechanism for expansive investigations of the executive branch (Ginsberg and Shefter 2002). However, divided government and the existence of the independent counsel statute are closely confounded in the contemporary period (see Chapter 4). The effect of the independent counsel statute on scandal is difficult to disentangle from divided government in the present sample and it is therefore omitted from the model. (However, robustness tests described below consider the alternative strategy of controlling for a unified opposition Congress and the independent counsel statute, which are less closely confounded.)

6 It is of course possible that decreased polarization in the political environment could also increase the transaction costs of scandal allegations. Such a claim would be consistent with the prediction above that the expected incidence of scandal would decrease.

7 As noted below, the results in this chapter are robust to using a direct measure of polarization.
Dependent variables

As stated in the introduction, my primary definition of scandal is that a scandal exists when elites recognize it as such at the time. In particular, as I suggest in the exposition of the formal model in Chapter 1, we can use the coverage of the national media as a proxy for this perception. I operationalize this idea by constructing a new dependent variable in which a scandal is defined as occurring when the media explicitly describes a controversy as a “scandal” in its news reporting. Focusing on the word scandal should make my data more precise. First, it excludes controversies that might be described as scandals retrospectively but were not recognized as such at the time. It is precisely that contemporary understanding of the event that I hope to capture. Second, focusing on the word scandal is a well-defined coding technique that addresses possible problems with subjectivity in coding and makes the data easily replicable.

To measure this construct, I use news articles from the Washington Post, an elite news source that often sets the agenda for reporters around the country. The period covered is 1977–2006. Articles were coded according to the approach described above, which defines controversies concerning the president and executive branch as scandals if the word “scandal” was used as a description of the controversy in a Post reporter’s voice or the headline (further coding details are provided in Appendix B).

The resulting set of scandals corresponds closely with other accounts and popular perceptions. For instance, as Figure 2.1 illustrates, scandal references in the Post peak at the times we expect (i.e. Iran-Contra, Monica Lewinsky, etc.). In total,

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8 The electronic Post archives are available for 1977–present.
the Post referred to 61 controversies involving the president and executive branch as scandals in its news reporting between 1977 and 2006.

Some may object that a reference to a controversy as a scandal is not a meaningful event. However, reporters are highly systematic in their word choice, particularly for pejorative terms such as scandal. For instance, news outlets such as NBC News have devoted extensive effort to deciding whether the conflict in Iraq should be described as a civil war (Bender 2006). In this case, the data indicate that some controversies are almost immediately described as scandals when they emerge (such as the controversy over the treatment of prisoners at the Abu Ghraib prison in Iraq) while others are repeatedly described using other terms. For instance, the Post studiously avoided using the term “scandal” to describe the so-called Whitewater affair despite engaging in saturation coverage of the con-
troversy for almost a year. In almost two hundred news reports referring to the matter between October 31, 1993—the date of its first major post-inauguration report on the controversy (Schmidt 1993)—and the article published on June 8, 1994 that finally described it as a scandal (Woodward 1994), the Post referred variously to “the Whitewater affair” (e.g. Balz 1994), “the Whitewater matter” (e.g. Balz 1994), “the Whitewater controversy” (e.g. Devroy and Schneider 1994), “the Whitewater issue” (e.g. Isikoff and Devroy 1994), and “the Whitewater situation” (e.g. Isikoff 1994).

To ensure that my conclusions are not dependent on the details of this coding procedure or the idiosyncrasies of Washington Post reporting, I also assembled a second dataset of scandals based on expert-coded data from the New York Times for the same time period (1977–2006). The data for 1977–1988 comes from Ostrom and Simon (1985) and Ostrom and Smith (1992). As I briefly described in the introduction, they canvassed chronologies and then coded unexpected events that received front page coverage in the Times for one or more months. These data were extended for the period 1989–2000 by Newman (2002). I drew events from their data that could reasonably be described as scandals and then extended the list through the end of 2006 using presidential event data for 2001–2005 from Schier (2006) (which were themselves coded from almanacs) and events cited by reference almanacs for 2006. In my coding, I adhered to the Ostrom-Simon-Smith procedure of only including events that received front page coverage in the Times. (To ensure comparability with the other measure, I excluded events that were part of previously recognized scandals.) The combination of almanac inclusion and front page coverage in the Times creates a more stringent standard than the Post data, identifying a total of 26 scandals between 1977 and 2006.

9 The Times, like the Post, also helps to shape the agenda of national political coverage.
Statistical methods

Using the data described above, I constructed two monthly dependent variables for the 1977–2006 period. In a handful of cases, more than one new scandal emerges in the same month. Due to the difficulties posed by dynamics in event count data with a large number of zeroes (which make it impossible to apply the techniques described in Brandt et al. 2000 and Brandt and Williams 2001), I transform the dependent variable to a binary variable measuring whether one or more new scandals began in a given month. It can therefore be analyzed as a survival model in which the subjects (presidents) can potentially suffer from the event of interest (scandal onset) more than once during their lifetime (time in office).

In a repeated-events survival model such as this, it is necessary to account for potential dependence between events, which can lead to incorrect standard error estimates (Box-Steffensmeier and Zorn 2002). To address this concern, I estimate a conditional gap time Cox proportional hazards model in which the time at risk is defined as the time since the president’s last scandal rather than the time since the start of the president’s term (Prentice, Williams and Peterson 1981). In other words, the president is assumed to not be at risk for the $i$th scandal of his time in office until scandal $i-1$ has already occurred. The baseline hazard is defined with respect to this interevent period, which corresponds to the substantive intuition that the risk of scandal is a function of the number of months since the president’s last scandal rather than the number of months elapsed in the president’s term. As the literature recommends, I then use robust standard errors clustered by presi-

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10 January 1977, January 1981, January 1989, January 1993 and January 2001 are excluded from the data due to the transitions between presidents.

11 Each scandal can only be “new” (and therefore cause the dependent variable to take the value of 1) once.
dent to account for interdependence among events for each chief executive (Lin and Wei 1989).

Repeated events models must address two other potential threats to inference. The first is the possibility that the baseline hazard—the estimated risk over time when all covariates equal 0—varies by event. In other words, a president who falls victim to scandal may become more susceptible to another scandal. One solution is to stratify the baseline hazard of the Cox model by event number, which allows the risk over time of the 1st scandal to differ from the 2nd, etc. However, with only five presidents in the sample and a large number of strata (24 in the Washington Post data and 8 in the New York Times data), the model quickly becomes overparameterized. Instead, I build on historical research suggesting that presidents may be more vulnerable to scandals during their second term in office (Shogan 2006) and stratify the baseline hazard by presidential term. This allows the shape of the hazard to differ by term. (Alternatively, one could include a dummy variable for second term, but this is a more restrictive approach that only allows the baseline hazard to be shifted up or down by a fixed amount.)

A second possible issue is that some presidents may be more inherently vulnerable to scandal than others. One solution is to estimate a so-called “frailty” model (the equivalent of a random effects model for survival data). However, as Box-Steffensmeier and Zorn note, the arbitrary choice of parametric frailty distributions may dramatically influence one’s results (2002, 1072). In addition, the estimated frailties must be independent of covariates to avoid bias and inconsistency. These problems are compounded by the limitations of the current data, which only includes five presidents, making it difficult or impossible to estimate subject-specific frailties. As such, I again build on substantive knowledge and stratify the baseline hazard for Bill Clinton, who was widely considered to have been uniquely scandal-prone. Doing so allows Clinton’s baseline risk of scandal
over time to differ from that of Carter, Reagan, George H.W. Bush, and George W. Bush.\textsuperscript{12} (Coefficients are assumed to be equal across presidents.)

Results

This section presents the results of statistical models testing the theoretical propositions described above. As noted above, these propositions are defined with respect to the underlying characteristics of the political environment, not the intermediate variables of allegation seriousness and credibility or the number of allegations made against the president.\textsuperscript{13}

Before turning to statistical results, however, I present raw data illustrating the relationship between the incidence of presidential and executive branch scandal over time and its relationship to opposition party approval of the president. Figure 2.2 plots the incidence of scandal in the \textit{Washington Post} and \textit{New York Times} data against lagged opposition approval by month for 1977–2006. The data are suggestive of a strong relationship. In particular, scandal onset is rare when opposition approval is greater than 45\% and common when opposition approval is below 20\%.

The data also show that scandal incidence appears to have increased over time in tandem with growing party polarization in Congress. By contrast, the raw data (not plotted) are less clear for divided government. In the \textit{Post} data, scandals emerged in 24 of 120 months of unified government in the sample (20\%) and 36

\textsuperscript{12} Another alternative in non-survival data would be to include a fixed effect term for Clinton. However, such terms have been shown to lead to consistency problems in survival models and are thus rarely used (Box-Steffensmeier and Zorn 2002, 1072).

\textsuperscript{13} Even if we were to set aside the theoretical reasons to focus on the underlying characteristics of the political environment, these intermediate variables present serious empirical difficulties. It is difficult or impossible to code allegation seriousness and credibility in a non-subjective fashion. Similarly, cataloguing the number of allegations against the president would require canvassing a vast array of media sources, many of which censor the set of allegations that are presented (i.e. the most prominent members of Congress are more likely to be quoted, etc.).
of 233 months of divided government (15%)—the opposite pattern from what we expect. The *Times* data display the expected pattern but the difference is slight—scandals emerged in 7 of 120 months of unified government (6%) and 19 of 233 months of divided government (8%).

It is also worthwhile to test the necessity of stratifying the baseline hazard in the manner described above. First, I test whether the baseline hazard should be stratified for President Clinton using nonparametric Kaplan-Meier estimates of survival probabilities over time in Figure 2.3. The estimates from the *Washington Post* data in Figure 2.3(a) and from the *New York Times* data in Figure 2.3(b) indicate that President Clinton tended to fall victim to scandal more quickly than other presidents in the sample (as indicated by the lower survival estimates for Clinton over the range of the *Post* data and most of the range of the *Times* data).
Figure 2.3: Differences in scandal incidence by president

(a) Washington Post

(b) New York Times
Table 2.1: Cox proportional hazards model of scandal onset 1977–2006

<table>
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<tr>
<th></th>
<th>Wash. Post</th>
<th>N.Y. Times</th>
</tr>
</thead>
<tbody>
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<td>Opposition approval (lag)</td>
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<td>-0.020</td>
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<tr>
<td></td>
<td>(0.007)</td>
<td>(0.007)</td>
</tr>
<tr>
<td>Divided government</td>
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<td>0.374</td>
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<td></td>
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<tr>
<td>Year counter</td>
<td>0.024</td>
<td>0.018</td>
</tr>
<tr>
<td></td>
<td>(0.006)</td>
<td>(0.014)</td>
</tr>
</tbody>
</table>

Conditional gap time model with baseline hazard stratified by second term and Bill Clinton. Standard errors above are robust and clustered by president. Coefficients are not exponentiated.

Similarly, Kaplan-Meier estimates of survival by presidential term demonstrate the necessity of stratifying by term. Figure 2.4 demonstrates that second-term presidents appear to be more susceptible to scandal, though the results are less clear in the New York Times data in Figure 2.4(b). In particular, no second-term president goes more than fifteen months without a scandal in the Post data displayed in Figure 2.4(a), whereas first-term presidents have gone as long as thirty months without doing so.

I next estimate a conditional gap time Cox proportional hazards model of scandal onset for 1977–2006 with robust standard errors clustered by president and the baseline hazard stratified by President Clinton and presidential term. Results for both dependent variables are presented in Table 2.1. The model shows that the hazard of scandal decreases substantially as lagged opposition approval increases. The estimated effects are negative and statistically significant for both datasets. The hazard of scandal is also estimated to have increased over time, though the effect is only significant for the Post data. Finally, estimated coefficients are insignificant and vary in sign for both divided government and election

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14 Coefficients are not exponentiated to ease interpretation by readers who are not familiar with hazard models.
FIGURE 2.4: Differences in scandal incidence by term

(a) Washington Post

(b) New York Times
When estimating models such as these that may contain duration dependence, Carter and Signorino (2007) emphasize the importance of plotting and interpreting the estimated baseline hazards. These curves, which are presented in Figure 2.5, represent the estimated risk of scandal occurrence over time when all covariates equal 0. Figure 2.5(a) plots the baseline hazards for the *Post* data and Figure 2.5(b) plots the hazards for the *Times* data. The estimated baseline hazards suggest that the longest a president can expect to go without a scandal in the contemporary era is approximately 30 months. The hazards also indicate that Bill Clinton was more vulnerable to scandal than other first-term presidents in the sample. Carter, Reagan, George H.W. Bush and George W. Bush were much less likely to suffer from scandals during their first years in office than either Clinton or second-term presidents. Indeed, the shape of Clinton’s first-term hazard function resembles that of other second-term presidents in the sample (Reagan, Clinton, and Bush 43).

To make my results more interpretable, I estimate predicted effects in two realistic counterfactual scenarios. First, I consider the situation faced by President Clinton in June 1994, the first month in which the *Post* described the Whitewater controversy as a scandal. During the previous month, his approval ratings among Republicans had been just 26 percent. What would happen if some external event (for instance, a foreign policy crisis or domestic tragedy) had boosted his approval rating with GOP identifiers to 60 percent, the maximum value attained by a Democratic president in the 1977–2006 period? While it is not possible to

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15 Each baseline hazard curve is plotted over the observed range of months between scandals for that dataset. This range varies by category within each dataset due to the use of stratified hazard rates.

16 Specifically, Jimmy Carter’s approval rating among Republicans was 60% in March 1977 during his presidential honeymoon period. Clinton’s maximum approval from GOP identifiers was 45 percent in August 1998.
Figure 2.5: Estimated baseline hazards of scandal onset

(a) Washington Post

(b) New York Times
consider a counterfactual history of the Whitewater matter itself, we can simulate the predicted hazard of scandal going forward for Clinton for this alternative scenario. Figure 2.6 presents the results of this hypothetical approval boost holding other variables at their actual June 1994 values (unified government, an election year, year counter=18) and using the baseline hazard for Clinton in his first term. The estimated survival functions show substantial increases in the probability of Clinton avoiding a scandal over time for the Post and Times data. The simulations also reveal significant decreases in the predicted hazard rates of scandal for both datasets \( \frac{h(t|X_{\text{high}})}{h(t|X_{\text{low}})} = 0.61, SD = 0.15 \); Times: \( \frac{h(t|X_{\text{high}})}{h(t|X_{\text{low}})} = 0.52, SD = 0.13 \).

A second example concerns President George W. Bush. On September 30, 2003, the Justice Department announced that it would begin an investigation into the leak of CIA operative Valerie Plame’s name. President Bush’s approval rating among Democrats was 16 percent in September. Not surprisingly, the Post described the controversy as a scandal by October 5 (Kurtz 2003). I therefore consider a counterfactual scenario in which a new foreign policy crisis or terrorist attack sent Democratic approval of Bush back up to its post-9/11 peak of 80 percent during the month of October. Setting aside the specifics of the Plame case, what would the predicted effects of such an opposition approval boost be on the likelihood of scandal in that month? Figure 2.7 presents the results of simulations for both datasets holding other variables at their actual October 2003 values (divided government, not an election year, year counter=27) and using the baseline hazard for Bush in his first term. In both cases, the estimated survival functions suggest that the likelihood of a scandal would be substantially diminished in the approval boost scenario. Results also show statistically significant decreases in

\[ \text{All simulations were conducted in Zelig (Imai, King and Lau 2006).} \]
\[ \text{The range of the plots varies by the maximum number of months between scandals during Clinton’s first term for each dependent variable—seven months for the Post data and thirty months for the Times.} \]
Figure 2.6: Predicted effect of opposition approval surge in June 1994

(a) Washington Post

(b) New York Times
Figure 2.7: Predicted effect of opposition approval surge in October 2003

(a) Washington Post

(b) New York Times
hazard rates \( Post: \frac{h(t|X_{\text{high}})}{h(t|X_{\text{low}})} = .64, SD = .15; \) \( Times: \frac{h(t|X_{\text{high}})}{h(t|X_{\text{low}})} = .30, SD = .15 \).

These simulations are also consistent with qualitative historical evidence. For instance, President George H.W. Bush’s reported involvement in the so-called Iraqgate scandal, which concerned US government-backed loans to Iraq that were allegedly used to purchase weapons and munitions, became salient after Iraq’s invasion of Kuwait in August 1990 (Baker 1993). However, Bush’s approval ratings were extremely high with Democrats as well as Republicans and the controversy was not described by the \textit{Washington Post} as a scandal until March 17, 1992 (Lardner 1992), when his lagged approval with Democrats at the time was approximately 18 percent (the \textit{Times} data does not code it a scandal at all). Similarly, the potential force of President Bush’s ties to the energy trading firm Enron, which filed for bankruptcy in December 2001, were seemingly blunted by his stratospheric opposition approval ratings after the September 11, 2001 terrorist attacks. The \textit{Post} did not describe those ties as a scandal until December 11, 2003 (Lane 2003), when his lagged Democratic approval was 20 percent, and the \textit{Times} data do not code it as a scandal.

Finally, I summarize a series of tests demonstrating the robustness of these results. One possible concern is that the relative hazard of scandal onset may not be proportional over time and across different covariate values, which can lead to bias and incorrect standard errors (Box-Steffensmeier and Zorn 2001). However, I cannot reject the null hypothesis of proportional hazards for each coefficient in both models (Harrell 1986) or the null of global non-proportionality for either model (Therneau and Grambsch 1994). In addition, the opposition approval result holds under a variety of conditions (all results available upon request). For instance, results are consistent if we respecify the model as a discrete-time survival model estimated using logit with duration dependence varying by presidential
term and Clinton (Beck, Katz and Tucker 1998). The estimated model is also robust to changes in the set of control variables, including using a variable counting months until the election rather than an election year dummy, controlling for an opposition Congress and the presence of the independent counsel statute instead of divided government, or using a measure of party polarization in Congress estimated from Common Space ideal point estimates (Poole 1998) instead of a time trend. Finally, findings hold if we include a variable for the number of past scandals as Beck and Jackman (1998) suggest to allow for a possible monotonic increase in vulnerability with each new scandal. (The finding also holds if we include a squared term for past scandals as well.)

Conclusion

The results presented in this chapter show that presidents are more vulnerable to scandal allegations as the political environment becomes more polarized, which I operationalize using opposition presidential approval, and that the incidence of scandal seems to have increased in the contemporary era. These findings help illuminate a phenomenon that is omnipresent in contemporary American politics. In addition, my results suggest that scholars of American politics should consider disaggregating presidential approval by party. The wide divergence in approval between presidential party identifiers, independents, and opposition party identifiers during the contemporary era means that a general measure of approval may obscure meaningful variation at the subgroup level. In particular, my results suggest that the depolarized nature of approval during presidential honeymoons, foreign policy crises, and the early stages of wars may be best captured by approval among opposition identifiers.
The previous chapter considers the incidence of scandal, the subject of the model developed in Chapter 1. Both the model and the empirics consider scandal as a binary state variable. In other words, a scandal either exists or it does not. I believe this approach is generally consistent with the way scandal is experienced and understood in national politics. However, it does not allow for any distinctions between scandals in terms of magnitude or for flurries of scandals in a given time period.

In this chapter, I therefore consider the extent to which the factors considered in the previous chapter predict the volume of scandal coverage in the *Washington Post* (which I use as a proxy for scandal intensity) during the 1977–2006 period. To account for dynamics in the data (an event count time series), I employ the Poisson AR($p$) estimator proposed by Brandt and Williams (2001). Results indicate that presidents with lower levels of approval from opposition party identifiers and second-term presidents receive increased scandal coverage. In addition, presidents are found to receive more scandal coverage during mid-term and presiden-
tial election years. However, contrary to expectations, presidents are not found to receive more scandal coverage under divided government.

Data and statistical model

Following the approach developed in Chapter 2, we use news coverage data as a proxy for scandal—specifically, the *Washington Post* scandal coverage dataset for the period 1977–2006. However, the dependent variable in this chapter is the volume of scandal coverage in the *Post* during a given interval, which should capture the magnitude or intensity of scandal in that interval. Specifically, I construct a variable measuring the total number of *Post* stories that describe the controversy as a scandal and that were coded as focusing on the controversy in question.¹

As in Chapter 2, we have strong reason to suspect that there is sample autocorrelation (i.e. scandal coverage at time $t-1$ affects scandal coverage at time $t$). If so, we need to use an appropriate estimation technique. As Brandt et al. (2000) and Brandt and Williams (2001) show, failing to account for the effects of dynamics in event count time series data (or doing so incorrectly) can lead to biased coefficient estimates. It is therefore necessary to use a technique such as the Poisson exponentially weighted moving average model of Brandt et al. (2000) or the Poisson autoregressive model with order $p$ of Brandt and Williams (2001). However, it is difficult to estimate such models for series with low event counts and frequent zeroes (indeed, it is impossible with the monthly coverage data). As such, we aggregate the coverage data, which were organized at the monthly level in Chapter 2, to the quarterly level to reduce the number of low or zero values.² In addition,

¹ In other words, I exclude stories that contain brief references to past or current scandals but are primarily concerned with other topics.

² The first quarter of a new president’s term is attributed solely to that president even though they do not technically take office until January 20.
the PAR($p$) model cannot be estimated for a series that starts with a zero value.\(^3\) As such, the dependent variable was truncated to the period beginning with the first nonzero observation (Brandt and Sandler N.d.), which occurs in the third quarter of 1977. The data include each quarter thereafter through the end of 2006.

The resulting quarterly time series, which is presented in Figure 3.1, corresponds closely to intuitive perceptions of presidential and executive branch scandal magnitude in the post-Watergate era. (See Appendix C for a discussion of how this dependent variable relates to the scandal onset variable in Chapter 2.) In

\(^3\) See Brandt et al. 2000, 840, which describes this issue in the context of the closely related PEWMA model.
particular, we observe large spikes in scandal coverage in late 1986 and 1987 (Iran-Contra), George H.W. Bush’s term (Iran-Contra, the savings-and-loan scandal, and corruption at HUD), 1997–1999 (campaign fundraising and Monica Lewinsky), and 2004 (Abu Ghraib) as well as low levels of coverage before Iran-Contra and a virtual absence of scandal coverage from 2001–2003.

Before the data can be analyzed statistically, it is necessary to diagnose the series for autocorrelation and to use that diagnosis to select the appropriate statistical model. After adjusting for the characteristics of count data (Cameron and Trivedi 1998, 228), we plot the estimated autocorrelation function, which appears in Figure 3.2. We observe significant positive autocorrelation that declines rapidly.
toward zero as the number of lags increases—a pattern that suggests the use of the PAR($p$) estimator of Brandt and Williams (2001) rather than the PEWMA model of Brandt et al. (2000). The PAR($p$) model accounts for this pattern of autocorrelation as well as overdispersion in the dependent variable.\footnote{The PEWMA model is appropriate for positive autocorrelation that shows greater persistence over time. See Brandt and Williams 2001 for a discussion of how to determine which model is appropriate for a given event count time series.}

The change in the statistical model and the unit of observation from Chapter 2 necessitate some changes to the set of independent variables considered in the empirical analysis. Lagged opposition approval is entered into the model below as the mean approval level among opposition party identifiers during the previous quarter.\footnote{The model based on the following three assumptions. First, the dependent variable $y_t$ is assumed to be distributed Poisson with mean $m_t$:} The model also includes a second term dummy variable (rather than stratifying the hazard by term as in Chapter 2’s survival model), a divided government dummy variable (which is coded as 1 during the quarter in which Jim Jeffords left the Republican party), and an election year dummy variable (covering both midterm and presidential election years). In addition, unlike the survival

$$\text{Pr}(y_t|m_t) = \frac{m_t^{y_{t}} e^{-m_t}}{y_t!}$$

Second, the mean is assumed to follow a stationary AR($p$) process with autoregressive parameters $\rho_i, i = 1, ..., p$, which is parameterized as

$$m_t = \sum_{i=1}^{p} \rho_i y_{t-1} + (1 - \sum_{i=1}^{p} \rho_i) \exp(X_t \delta)$$

where $X$ is a matrix of exogenous covariates and $\delta$ is a vector of regression coefficients. Finally, we assume $\text{Pr}(m_{t-1}|Y_{t-1})$ is distributed Gamma($\sigma_{t-1} m_{t-1}, \sigma_{t-1}$) where $m_{t-1} = E[y_t|Y_{t-1}]$ and $\sigma_{t-1} = \text{Var}[y_t|Y_{t-1}]$. The resulting one-step-ahead predictive distribution $\text{Pr}(y_t|Y_{t-1})$ is negative binomial, which makes it possible to analyze overdispersed count data (i.e. cases in which the variance exceeds the mean, which violates the assumptions of the Poisson model). See Brandt and Williams (2001, 181–183) for further details.

\footnote{Because it is important to preserve the entire series (rather than dropping the first month or two of each presidency due to missing data), the first available value of opposition approval for new presidents is used as the lagged opposition approval term in their first quarter in office.}
model in Chapter 2, the PAR($p$) model treats the data as a single time series, so accounting for differences between presidents is essential. Rather than including a time trend, I included dummy variables for each president (excluding Carter), which should account for any differences in the mean number of scandals between presidents that are not accounted for by the covariates (including any trend toward successive presidents facing more scandals than their predecessors).

Results

The results presented below were estimated for the PAR($p$) model were estimated using Brandt’s PESTS software for R. Following Brandt and Williams (2001) and Brandt and Sandler (N.d.), I first fit the model with only an intercept term, vary the number of lags, and then choose the best model according to the Akaike Information Criterion (Akaike 1974). This procedure selects a model with three autoregressive lag terms (results available upon request). I then add the covariates described above and estimate a PAR(3) model, which yields the results presented in Table 3.1. These results confirm the previous finding that the dependent variable has substantial autocorrelation. The estimated autoregressive terms $\rho_1, \rho_2, \rho_3$ are each highly statistically significant and a Wald test overwhelmingly rejects the null hypothesis that they are jointly zero ($p < .01$). The signs of $\rho_1$ and $\rho_2$ are positive while $\rho_3$ is negative, suggesting that there is positive autocorrelation that declines rapidly toward zero (consistent with the autocorrelation plot above). Substantively, the implication is that the magnitude of scandal coverage in quarter $t$ is positively correlated with the extent of coverage in the ensuing quarter $t + 1$, $t + 2$, . . . but that the correlation declines rapidly toward zero over time.

We turn now to the covariates of interest. Several estimated relationships are
Table 3.1: PAR($p$) model of scandal coverage July 1977–December 2006

<table>
<thead>
<tr>
<th></th>
<th>Coefficient (SE)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Opposition approval (lag)</td>
<td>-0.012 (0.004)</td>
</tr>
<tr>
<td>Divided government</td>
<td>0.427 (0.280)</td>
</tr>
<tr>
<td>Election year</td>
<td>0.332 (0.119)</td>
</tr>
<tr>
<td>Second term</td>
<td>1.529 (0.180)</td>
</tr>
<tr>
<td>Ronald Reagan</td>
<td>-0.744 (0.509)</td>
</tr>
<tr>
<td>George H.W. Bush</td>
<td>2.032 (0.479)</td>
</tr>
<tr>
<td>Bill Clinton</td>
<td>0.411 (0.465)</td>
</tr>
<tr>
<td>George W. Bush</td>
<td>-0.109 (0.421)</td>
</tr>
<tr>
<td>$\rho_{t-1}$</td>
<td>0.418 (0.046)</td>
</tr>
<tr>
<td>$\rho_{t-2}$</td>
<td>0.102 (0.037)</td>
</tr>
<tr>
<td>$\rho_{t-3}$</td>
<td>-0.143 (0.025)</td>
</tr>
<tr>
<td>Constant</td>
<td>0.587 (0.402)</td>
</tr>
<tr>
<td>N</td>
<td>118</td>
</tr>
</tbody>
</table>

Consistent with those reported in Table 2.1. First, we find that lagged opposition approval is negatively associated with the magnitude of scandal coverage, which is consistent with the relationship to scandal onset reported in Chapter 2. In other words, as the president’s approval among opposition identifiers increases, the expected likelihood of scandal onset and magnitude of scandal coverage should decrease (the converse is also true). We also find that presidents in their second term face more scandal coverage (which echoes Chapter 2 as well). Finally, we
again find no significant association between divided government and scandal coverage (see Chapter 4 for more on this topic). However, we do find that the magnitude of scandal coverage is greater during election years—an association that was not found in the previous chapter’s analysis of scandal onset. In addition, while the previous chapter stratified the baseline hazard by Bill Clinton, this analysis finds (surprisingly) that the only statistically significant presidential effect is for George H.W. Bush, who is found to have substantially more news coverage than expected conditional on other covariates. This finding may be the result of the fact that he is the only vice presidential successor in the data and thus suffered from scandals that began under Ronald Reagan.\(^8\)

We next present estimated substantive effects for each of the statistically significant covariates. We start first with lagged approval of the president by opposition identifiers. To illustrate the effect of this variable, we present a scenario opposite to that described in Chapter 2. In that analysis, we considered the estimated effects of a hypothetical post-9/11 surge in opposition approval on the likelihood of a scandal in October 2003. Here we consider its opposite—a counterfactual scenario in which approval declines rather than increases. The scenario we consider is the second quarter of 1991, the immediate aftermath of the Gulf War in Iraq (which ended with a ceasefire on February 28). The war pushed up George H.W. Bush’s approval ratings, creating a rally-around-the-flag effect that protected him from economic discontent and reduced levels of scandal coverage dramatically. What would have happened if the Gulf War had not taken place?

To consider this scenario, Figure 3.3 presents a plot of the estimated number of stories in this counterfactual scenario as approval ranges from its observed value of 77.7% in the second quarter of 1991 (the high point of his presidency) to Bush’s

\(^8\) As noted in Appendix B, the data were coded in this manner to reflect the reality that Bush was held responsible for scandals dating to his time as vice president.
post-Gulf War low of 12.1% (from the fourth quarter of 1992). All other covariates were set to their observed values in that quarter. As approval moves from Bush’s peak to his low point, the mean predicted number of Post scandal stories for the second quarter of 1991 increases from 5.6 (95% CI: 3.9, 7.7) to 11.8 (95% CI: 8.5, 16.5), an increase of 119% (95% CI: 28%, 261%). The effect of the approval shock then continues to reverberate via the autoregressive parameters, boosting its estimated total long-term effect to 192% (95% CI: 45%, 425%).

Using the same approach, we can also compute the estimated immediate and long-term effects of the election year and second term variables using observed
Table 3.2: Effects of election year and term on scandal coverage (1991 Q1)

<table>
<thead>
<tr>
<th>Covariate</th>
<th>Immediate change (95% CI)</th>
<th>Long-term change (95% CI)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Election year</td>
<td>37% (11%, 69%)</td>
<td>61% (18%, 110%)</td>
</tr>
<tr>
<td>Second term</td>
<td>339% (194%, 531%)</td>
<td>545% (322%, 827%)</td>
</tr>
</tbody>
</table>

covariate values for the first quarter of 1991. These results are presented in Table 3.2 as percentage changes in the predicted number of scandal stories. In this scenario, the predicted effects of the election year variable are relatively modest—scandal coverage increases from 5.6 to 7.6 (95% CI: 5.3, 11), which leads to a 61% total increase in scandal coverage. By contrast, the estimated effects of the second term variable are massive. With observed covariate values for the second quarter of 1991, a shift in the second term variable from 0 to 1 is predicted to increase the number of scandal stories in the quarter from 5.6 all the way to 25 (95% CI: 13, 43). The total long-term increase in expected scandal coverage is a massive 545%! (95% CI: 322%, 827%)

Conclusion

Despite using a different dependent variable and statistical approach, the results in this chapter are largely consistent with those reported in Chapter 2. I find that the volume of total scandal coverage in the Post increases during election years, during presidents’ second terms in office, and when they have low approval among opposition party identifiers. Using a counterfactual scenario, I calculate substantive effects for these three variables. I conclude that the substantive effect of the second term variable is the greatest in magnitude but that lagged opposition approval also has a substantial impact.
The Democratic takeover of the House and Senate in the 2006 elections revived interest in questions about the importance of divided government. In particular, complaints soon began about the number of Congressional investigations of the Bush administration, such as this statement by President Bush on August 9, 2007 (Baker 2007):

I would hope Congress would become more prone to deliver pieces of legislation that matter as opposed to being the investigative body.
I mean, there have been over 600 different hearings, and yet they’re struggling with getting appropriations bills to my desk.

Interested parties were not the only ones expressing concern. The Los Angeles Times, for instance, warned Democrats in an April 2007 editorial that “too many inquisitions into minor affairs may prove to be counterproductive” (Los Angeles Times 2007).

These statements represent the latest episode in the longstanding debate about the merits of divided government—specifically, the hypothesis expressed by Valelly
(1992) and many others that divided government leads to an increase in scandal, diverts attention from legislation, and erodes public faith in the political system.

Divided government creates a climate of scandal-mongering, in which each branch of government expends political resources embarrassing the other (Watergate, Iran-contra, the S&L scandals, Iraqgate) rather than jointly tending to the national business. Over time, this discredits both parties, blurs responsibility, and generates still more voter contempt for government and politics generally (Valelly 1992).

These concerns drew particular attention after the polarizing investigations of Ronald Reagan and Bill Clinton during the 1980s and 1990s. More recent suggestions by political scientists that divided government has become a vehicle for scandal have come from Ginsberg and Shefter (2002) and Lowi (2004). Similarly, I argued in Chapter 2 that divided government should decrease the transactions costs of scandal allegations for opposition legislators and thereby increase the number of presidential scandals relative to periods of unified government (a prediction derived from the model in Chapter 1).

However, we have little empirical evidence to substantiate concerns that divided government leads to more presidential scandal. Mayhew (1993), the most detailed examination of Congressional investigations of the executive, finds that divided government does not increase the number of high-profile investigations for the 1946–1990 period—a surprising and counter-intuitive result. In the second edition of his book (2005), he finds an increase in investigations under divided government during the 1991–2002 period, but the overall picture remains unclear. Similarly, the results in Chapter 2 and 3 show no statistically significant relationship between divided government and either the onset of presidential scandal or the total volume of scandal news coverage.
What explains this puzzling contradiction between theory and data? Further investigation reveals several important methodological problems that plague research on divided government—a limited number of observations, time series estimation issues, a lack of comparable observations that can be used as counterfactuals, and the use of independent variables that induce post-treatment bias. This chapter reports the results of an effort to address these inferential issues and estimate a valid treatment effect. I find that it is possible to assemble a sample that is closely matched on relevant variables for both divided government and a unified opposition Congress, but the treatment effects again fail to reach conventional levels of statistical significance. As a result, we can draw no strong conclusions about the effect of opposition control of one or both chambers of Congress on either presidential investigations or scandal.

The state of the literature on divided government

During the 1990s, substantial effort was devoted to estimating the effect of divided government on the production of legislation, especially “important” laws (Mayhew 1993; Kelly 1993; Edwards, Barrett and Peake 1997; Coleman 1999; Howell et al. 2000). By contrast, there has been virtually no empirical research on the impact of divided government on scandal besides Mayhew’s *Divided We Govern*, which tests the “familiar claim” that “Congress acting as an investigative body will give more trouble to the executive branch when a president of the opposite party holds power . . . what causes the effect is a predicted difference between united and divided control” (2005, 3). To do so, he identifies all Congressional investigations of executive misbehavior that received substantial coverage in the *New York Times* (twenty or more front-page stories) during the 1946–1990 period.¹

¹ Based on the media attention devoted to these investigations, we can infer that contemporary observers viewed the administration actions in question as at least potentially scandalous. An ex-
As noted above, however, Mayhew finds no evidence that the number of high-profile investigations was greater under divided government. In the epilogue to the second edition of the book, he briefly notes additional evidence suggesting that the number of such investigations was greater under divided government in the 1991–2002 period, but does not estimate a model for the 1946–2002 as a whole.

Mayhew’s research on investigations of the president has received little scholarly attention compared with his study of the production of “important laws.” However, one study does build on his work. Noting that many investigations are excluded by Mayhew’s requirement of twenty or more days of front-page New York Times coverage, Parker and Dull (N.d.) gather more comprehensive data on hearings from the Congressional Information Service Index and find that divided government appears to increase the number of hearings and pages of hearing records investigating executive branch waste, fraud and abuse in the period since Watergate. While this finding is a useful complement to Mayhew’s work, it does not overturn his principal finding for our purposes—committee hearings can help to develop and publicize scandal allegations, but they are ultimately an input into the scandal generation process. By contrast, Mayhew’s reliance on the New York Times provides a measure of external significance more akin to the dependent variables used in Chapters 2 and 3.

One other unpublished finding on scandal and divided government is worth noting. Cameron and Segal (2001) reach the surprising conclusion that the probability of a scandal emerging during the Supreme Court nomination process does not vary by opposition control of the Senate. On the other hand, they show that the opposition party will, if it controls the Senate, search longer for a scandal and

\[2\]

More specifically, the coefficient for an interaction between divided government and the post-Watergate period is positive and significant in both models, but they do not compute the marginal effect of the variable directly (Brambor, Clark and Golder 2006).
investigate any scandals that it finds for longer than the president’s party. The finding is relevant but insufficient to answer the question at hand.

Methodological difficulties

Sorting out these conflicting results is the task at hand. However, closer inspection reveals that doing so will be more difficult than one might imagine. Divided government suffers from several important limitations as an explanatory variable. Two of these limitations are relatively well-known (small sample sizes and time series estimation issues), while the third (problems with a lack of comparable counterfactual data) has not previously been explored. I also note potential problems with controlling for variables that are affected by divided government, which can lead to biased coefficient estimates.

Sample size

The most obvious difficulty is that divided government is a Congress-level variable, which means that we have very few observations in the post-World War II era (Mayhew’s first edition had only 22 observations). As a result, analyses that depend on the asymptotic properties of frequentist statistical estimators may be flawed. In addition, testing hypotheses about temporal change—such as whether the effect of divided government has changed since, say, the GOP takeover in 1994—is often difficult or impossible.

Time series estimation

In addition, the time series nature of Congress-level variables can also threaten our ability to make valid inferences. In particular, autocorrelation violates the assumption that each observation is independent. Under this condition, the resulting standard errors will typically be too small, which can lead to spurious...
findings of statistical significance. More attention should be paid to these issues in the divided government literature, which has mostly treated the time series properties of its data as a nuisance. In particular, Mayhew (1993, 2005) does not discuss time series or autocorrelation issues in estimation.

Model-based extrapolation

A third potential problem is that estimating the effect of divided government may require substantial model-based extrapolation due to a lack of comparable counterfactual data (King and Zeng 2006, 2007). The problem, in short, is that statistical models will generate estimates for areas of the data that are sparsely populated or empty altogether. Without appropriate counterfactual observations, statistical models are forced to extrapolate beyond the boundaries of observed data.

Examples of this problem abound in empirical data. King and Zeng point out, for instance, the lack of an appropriate counterfactual with which to assess the potential for state failure if Canada became an autocracy (Esty et al. 1998). Due to the lack of an autocracy that resembles Canada in the observed data, any estimates of the estimated effect of democracy on Canada’s likelihood of state failure would be the result of model-based extrapolation.

The same problem is likely to apply to divided government. To estimate the treatment effect properly, we need observations under unified government that are comparable to episodes of divided government. But since the incidence of divided government tends to be correlated with other factors that may increase the likelihood of scandal (including party polarization, second-term presidents,

3 Notable exceptions include Brandt and Williams (2001), Brandt (2003), and Park (2006), who devote substantial attention to modeling dynamics in event count models featuring divided government or related variables as predictors. Edwards, Barrett and Peake (1997), Coleman (1999), Binder (1999), and Howell et al. (2000) also discuss methodological concerns related to the time series structure of the data, though their statistical remedies are less sophisticated.

4 This problem is exacerbated by the “curse of dimensionality” discussed in de Marchi (2005).
and the existence of the independent counsel statute), there are few comparable observations of unified government that we can use as counterfactuals.

Post-treatment bias

The last potential problem is post-treatment bias. As King and Zeng (2006) explain, conditioning on covariates that are partly consequences of the treatment of interest is likely to bias the estimate of the treatment effect. King (1991) points out, for instance, that a model measuring the effect of crude oil prices on public perceptions of an energy crisis would be misspecified if we controlled for TV coverage of oil price increases, which is partly a consequence of the change in prices.

Controlling for variables that may be partly the result of divided government is likely to cause similar problems. There are many examples in the literature on divided government. For instance, Mayhew’s important laws analysis (Mayhew 1993, 177) and others that follow his specification (Kelly 1993; Edwards, Barrett and Peake 1997; Coleman 1999) control for the average federal budget surplus/deficit for a given Congress as a measure of the financial constraints on new legislation. However, the fiscal balance of government may be partly a consequence of unified versus divided party control (Niskanen N.d.), particularly in the second year of the session. Including a covariate measuring the fiscal situation might then bias our estimate of the effect of divided government on, say, the creation of important laws.

Rosenbaum (1984) examines the bias that can result from controlling for such variables and shows that the inclusion of post-treatment controls is only justified under specific circumstances.
Data

Dependent variables

As mentioned above, Mayhew’s dependent variable (1993; 2005) counts Congressional committee investigations that generated at least twenty front-page New York Times stories reporting “a committee-based charge of misbehavior against the executive branch, or an executive response to such a charge” in a given Congress between 1946 and 2002 (I exclude 1946 to focus on complete sessions). This generates a list of 32 investigations which generated 1,682 front page stories. Though Mayhew compared the number of high-profile investigations under unified and divided government, I instead focus on the total number of stories generated by those investigations, a dependent variable that should better capture the intensity and magnitude of committee investigation coverage.

However, it is necessary to broaden the analysis further. While I want to generate a new estimate of the treatment effect of divided government based on Mayhew’s measure of investigation coverage (the total number of front-page New York Times stories among those investigations generating twenty or more front-page stories in a given Congress), investigations are ultimately an input to the scandal generation process. In this dissertation, my goal is to assess the output of that process. I therefore also consider the total number of months of front-page New York Times scandal coverage from the measure developed in Chapter 2 (Ostrom and Simon 1985; Ostrom and Smith 1992; Newman 2002). But while the measure in Chapter 2 took a value of 1 if one or more scandals were covered for the

---

6 Following Mayhew, I count separate House and Senate investigations as two. In addition, I remove from the data two investigations of previous administrations in which the sitting president was not directly involved—the 1953 investigation of alleged Soviet spy rings in Truman’s administration, which took place under Eisenhower, and the 1975–1976 investigations of CIA covert operations, which took place under Ford and focused on CIA activities before he assumed the vice presidency in 1973.

7 My substantive findings are identical using Mayhew’s original dependent variable.
first time on the front page of the *Times* in a given month and 0 otherwise, this measure sums the total number of scandal-months in the *Times* data by Congress (hopefully maximizing variance).  

Figure 4.1 illustrates the relationship between these variables by contrasting plots of the total number of articles per Congress in Mayhew’s data (Figure 4.1(a)) and the total number of scandal-months per Congress from the Ostrom-Smith-Newman data (Figure 4.1(b)). First, there were obvious peaks in committee investigation coverage during the 93rd Congress due to Watergate and the 106th Congress due to the campaign fundraising and Monica Lewinsky scandals. In addition, as Mayhew notes, there has seemingly been a general decline in the number of committee investigation stories over time (Mayhew 2005, 28). He attributes this to reporters’ increasing reliance on documents acquired through the Freedom of Information Act (1966) and official reports such as those from the Government Accountability Office (formerly the General Accounting Office). Mayhew also suggests that chief executives have become more skilled at fending off controversies by appointing official commissions.

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8 To ensure the robustness of my results to these coding decisions, I estimated Bayesian Poisson models for the number of investigations per Congress (Mayhew’s preferred measure) and the number of scandal onsets in the *Times* data (neither were overdispersed). The findings of those analyses were consistent with those reported below for the treatment effects of divided government and unified opposition Congresses (results available upon request).

9 However, the generally high correlation ($\rho = .63$) between Mayhew’s investigations measure and the Ostrom-Smith-Newman *New York Times* measure, which does not focus specifically on Congress or committees, suggests that the change over time may be more general. The decline may instead be a more general phenomenon, possibly related to the advent of the independent counsel or an increase in emphasis on “soft news” among newspapers. Another potential explanation is that presidents may have been more likely to be investigated by members of their own parties in the pre-Watergate era when the parties were internally divided among competing factions. As the parties became more internally homogenous, investigations have become an opposition-driven phenomenon. (I thank David Rohde for this suggestion, which is consistent with an argument made by Ginsberg and Shefter (2002, 23).)

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FIGURE 4.1: Investigation/presidential scandal coverage by Congress 1947–2006

(a) Mayhew measure

(b) Ostrom-Smith-Newman measure
Independent variables

Given the potential for post-treatment bias and the limited number of degrees of freedom, I must choose additional covariates carefully. Building on the theory and empirics presented in Chapters 2 and 3, I will focus on whether the president is in his second term\(^{10}\), party polarization in the previous Congress\(^{11}\), the presence of the independent counsel statute at the start of the session, and presidential approval among opposition party identifiers at the beginning of the session. All four should avoid post-treatment bias if we consider the treatment of divided government to be administered anew in each session of Congress.\(^ {12}\) However, the choice of opposition approval requires dropping the 81\(^{st}\) and 82\(^{nd}\) Congresses of 1949–1952 (a period for which the relevant data are not available). The final dataset therefore covers the period from the 83\(^{rd}\) to the 107\(^{th}\) Congress (1953–2002).

Results

To properly estimate the effect of divided government on presidential scandal, I first preprocess the data to avoid the analytical difficulties described above. I then estimate event count models that take into account the significant overdispersion present in both datasets. I find that the estimated treatment effects for both divided government and a unified opposition Congress fail to reach conven-

---

\(^{10}\) For the purposes of this chapter, I code 1965–1968 (the 89\(^{th}\) and 90\(^{th}\) Congresses) as Lyndon Johnson’s second term. Similarly, I code the 93\(^{rd}\) Congress of 1973–1974 as Nixon’s second term though he resigned in August 1974 and was succeeded by Gerald Ford.

\(^{11}\) As I noted in Chapter 2, party polarization is highly correlated with time in this period. Given that it makes little sense to match on calendar year, I instead employ a polarization measure that is calculated as the Euclidean distance in Common Space between the median member of each party in the first dimension (Poole 1998).

\(^{12}\) Specifically, the president has been elected (or re-elected) before the Congress begins; party polarization in the previous Congress cannot be affected by who controls Congress in the following session; the independent counsel statute is (or is not) in force at the beginning of the session; and opposition presidential approval at the beginning of the session cannot be affected by what takes place later in the session.
Preprocessing the data

First, we assess the time series properties of the data in order to determine whether it threatens our ability to make valid inferences. The autocorrelation function indicates there is little autocorrelation in the Mayhew data and we therefore conclude that modeling dynamics is not necessary. The ACF for the *Times* measure shows similar results.\(^{13}\)

Next, we consider the issue of model-based extrapolation. Descriptively, there are clear indications of significant imbalance between treated and control units in the data. For instance, the quantile-quantile plots of propensity scores (i.e. the predicted probability of divided government in a logistic regression of divided gov-

\(^{13}\) Autoregressive dynamics often differ depending on how the data is aggregated (Brandt and Sandler N.d.). In the case of scandal, the findings in this chapter differ from those presented in Chapters 2 and 3, where it was necessary to account for the time series properties of the data.
ernment on the other covariates) and the continuous covariates in Figure 4.2 show significant deviations from the 45° lines in each figure. The deviations from that line indicate that the empirical distributions of the probability of divided government differ between the treatment and control groups (ideally, the points would fall very close to the 45° line). Relative to unified government, the plots indicate that Congresses with divided government are also more polarized (Figure 4.2(b)) and feature lower levels of opposition approval (Figure 4.2(c)). Divided government is also more likely to occur during presidents’ second term in office (77.8% versus 62.5% for first-term presidents) and when the independent counsel statute is in effect (90% versus 53.3%). These findings suggest that model-based estimates of the effect of divided government may lack relevant counterfactuals (i.e. episodes of unified government with high polarization and low opposition approval that take place during a second term and/or a period with the independent counsel statute in place) and thus could require substantial extrapolation.

Using the R packages WhatIf (Stoll, King and Zeng 2006) and Matchit (Ho et al. 2004, 2007), we therefore preprocess the data to minimize model-based extrapolation. The literature discusses two procedures that can be used to identify and discard observations that require significant extrapolation. King and Zeng recommend using the convex hull (a geometric concept that measures proximity in multidimensional space\footnote{Specifically, the convex hull of a set of points is the smallest convex set that contains the points.}) to determine which units should be discarded (i.e. treated units outside the convex hull of control units and vice versa). A less aggressive approach is to discard treated observations whose predicted probability of receiving the treatment (the “propensity score”), which is typically estimated using logistic regression, is outside the observed range in the control group (and vice versa) (Rosenbaum and Rubin 1983).\footnote{King and Zeng (2006, 152) point out that this procedure depends on the unprovable assumption} After taking one of these two steps,
Ho et al. (2007) recommend using non-parametric matching techniques to select a subset of units that further maximizes covariate balance. In this way, one can minimize the extent of model-based extrapolation in our inferences.\footnote{It is important to be aware of the limitations of matching procedures. In particular, we must always be concerned that in balancing the sample on observables we are not selecting on un-observables. In addition, there are specific concerns related to matching in an extremely small sample. Matching works better in larger samples, which in general offer the possibility of more precise matches, as Rubin (1997) notes. A more complex question concerns the performance of matching techniques in small samples, which has been the subject of relatively limited research (Fr"olich 2004; Zhao 2004).}

Following the recommended approach, we first use the convex hull test to identify those treated observations for which there are no comparable counterfactuals. Unfortunately, it turns out that only two of the control units are inside the convex hull of the treated data (the 87\textsuperscript{th} Congress of 1961–1962 and the 103\textsuperscript{rd} Congress of 1993–1994). This finding suggests that our inferences about the effects of divided government may depend at least somewhat on model-based extrapolation. Following King and Zeng (2007, 225), I therefore shift from the convex hull to using propensity scores to minimize extrapolation and improve the robustness of the resulting inferences. Specifically, I drop those control observations whose propensity scores are outside the support of the treatment group (i.e. the range of predicted probabilities of receiving the treatment) as well as treated observations that are outside the support of the control group.

After doing so, I use 1:1 nearest neighbor matching without replacement to select control observations that are most comparable to my treated observations.\footnote{Ideally, we would use the genetic matching algorithm of Diamond and Sekhon (N.d.), which automates the difficult process of maximizing balance across a range of diagnostic measures (Sekhon N.d.). However, procedures such as Diamond and Sekhon’s that do not use 1:1 matching create observation-specific weights to maximize balance. These weights are not easily incorporated into the Bayesian negative binomial model described below, which is necessary to obtain correct estimates from such a small sample of overdispersed count data. Results from balance tests suggests that nearest neighbor matching achieved comparable levels of balance to the genetic matching algorithm.}
Results from this procedure indicate that it is not feasible to separately estimate the effect of the independent counsel law because it coincides so closely with divided government in the contemporary era.\textsuperscript{18} As a result, matching algorithms will invariably drop all Congresses in which the independent counsel law was in effect—we simply lack comparable observations from the contemporary period.

To circumvent this problem, I choose to estimate two treatment effects. First, I estimate a treatment effect for divided government for the matched data that excludes the independent counsel Congresses. Second, I estimate the treatment effect of a unified opposition Congress, which provides more comparable counterfactuals, particularly with regard to the independent counsel statute. In each case, I contrast the results of my parametric models with those using the original sample so that we can determine what effect (if any) the matching procedure has on our inferences.

The propensity score support restriction described above results in the exclusion of ten congresses with divided government. In other words, the predicted probability that these congresses would have divided government exceeded the observed range in the control group. The seven remaining treated units are matched with seven control units, leaving one unified Congress unmatched. Table 4.1 illustrates that the resulting improvement in balance between the treated and control groups is substantial. The difference of means between treated and control units is improved for each variable (see the sixth column for the percentage reduction in the mean difference), with substantial improvements on polarization and opposition approval and perfect balance on independent counsel (though this is achieved by dropping all Congresses that began with the independent counsel in

\textsuperscript{18} Specifically, the law was only in force at the beginning of a session in Congress once under unified government (the 96th Congress of 1979–1980); all other Congresses in which the law was in effect took place under divided government.
Table 4.1: Covariate balance for divided government

<table>
<thead>
<tr>
<th></th>
<th>Original</th>
<th>Matched</th>
<th>Improvement</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Treated</td>
<td>Control</td>
<td>Treated</td>
</tr>
<tr>
<td>Opposition approval</td>
<td>37.20</td>
<td>43.62</td>
<td>42.14</td>
</tr>
<tr>
<td></td>
<td>0.57</td>
<td>0.54</td>
<td>0.55</td>
</tr>
<tr>
<td>Polarization</td>
<td>0.41</td>
<td>0.25</td>
<td>0.43</td>
</tr>
<tr>
<td>Second term</td>
<td>0.53</td>
<td>0.12</td>
<td>0.00</td>
</tr>
<tr>
<td>Independent counsel</td>
<td>100%</td>
<td>100%</td>
<td>100%</td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td>Improvement</td>
</tr>
</tbody>
</table>

Table 4.2: Covariate balance for opposition Congress

<table>
<thead>
<tr>
<th></th>
<th>Original</th>
<th>Matched</th>
<th>Improvement</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Treated</td>
<td>Control</td>
<td>Treated</td>
</tr>
<tr>
<td>Opposition approval</td>
<td>39.19</td>
<td>39.33</td>
<td>42.71</td>
</tr>
<tr>
<td>Polarization</td>
<td>0.57</td>
<td>0.56</td>
<td>0.55</td>
</tr>
<tr>
<td>Second term</td>
<td>0.46</td>
<td>0.25</td>
<td>0.44</td>
</tr>
<tr>
<td>Independent counsel</td>
<td>0.46</td>
<td>0.33</td>
<td>0.44</td>
</tr>
</tbody>
</table>

force). In addition, results of \( t \)-tests (for the non-continuous variables) and univariate bootstrap Kolmogorov-Smirnov tests (for the continuous variables) show a significant improvement in overall balance across the covariates, squared covariates, and interactions.\(^{19}\)

When we match by opposition Congress, four treated units are dropped as outside common support. The remaining nine treated units are matched to nine control units, leaving three unmatched control units. Table 4.2 indicates that this matching procedure trades slight increases in mean imbalance on opposition approval and the second term variable for perfect balance on the independent counsel variable (again, see the sixth column for the change in the mean difference between samples). Again, results of \( t \)-tests (for non-continuous variables) and

\(^{19}\) Balance was assessed using the MatchBalance function in Sekhon’s Matching package (N.d.) using two-sample \( t \)-tests before matching and paired \( t \)-tests after matching for non-continuous variables.
univariate bootstrap Kolmogorov-Smirnov tests (for continuous variables) sug-
gest that overall balance for covariates, squared covariates, and interactions has
been substantially improved by the procedure.

Model estimation

Because the dependent variables are counts of news stories per Congress, we need
to use an appropriate estimator (King 1988). A standard approach for event count
data is the log-linear Poisson model. However, this model assumes that the mean
and variance of the dependent variable are equal, a condition that is frequently
violated for count data. When the variance exceeds the mean, this is known as
“overdispersion” and is typically the result of either unobserved heterogeneity or
“positive contagion” across units (i.e. higher values of Y in one period are associ-
ated with higher values in the next period) (King 1989). In this context, we would
expect overdispersion as a result of heterogeneity (all sessions of Congress are not
alike) since the autocorrelation function showed little evidence of positive con-
tagion. As a result, Poisson coefficient estimates will be inefficient and standard
errors will be biased.

Both the original and processed samples show significant overdispersion for
the investigation and scandal coverage dependent variables. I therefore estimate
a Bayesian negative binomial model with mean \( \mu \) and variance \( \mu + \alpha \mu^2 \) where
the dispersion parameter \( \alpha \) is estimated from the data (Cameron and Trivedi 1998,
62–63). This distribution can be constructed as a Poisson-gamma mixture if we as-
sume an unobserved observation-level heterogeneity term enters the mean mul-
tiplicatively\(^{20}\) and is distributed gamma with mean 1 (Cameron and Trivedi 1998,
100–103). I estimate the model using Markov Chain Monte Carlo techniques
(MCMC) in the Bayesian modeling program WinBUGS (Lunn et al. 2000). (The

\(^{20}\) That is, \( E[y_i|x_i, \nu_i] = \mu^*_i = \mu \nu_i \) with heterogeneity \( \nu_i = \exp(\epsilon_i). \)
Table 4.3: Effect of divided government on investigations and scandal 1953–2002

<table>
<thead>
<tr>
<th></th>
<th>Original sample</th>
<th>Matched sample</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Investigation</td>
<td>Scandal</td>
</tr>
<tr>
<td>Divided government</td>
<td>0.12</td>
<td>0.34</td>
</tr>
<tr>
<td></td>
<td>(-1.10, 1.53)</td>
<td>(-0.82, 1.80)</td>
</tr>
<tr>
<td>Opposition approval</td>
<td>0.05</td>
<td>0.13</td>
</tr>
<tr>
<td></td>
<td>(-0.77, 0.90)</td>
<td>(-0.50, 0.89)</td>
</tr>
<tr>
<td>Polarization</td>
<td>-0.43</td>
<td>0.44</td>
</tr>
<tr>
<td></td>
<td>(-1.47, 0.45)</td>
<td>(-0.40, 1.58)</td>
</tr>
<tr>
<td>Second term</td>
<td>0.85</td>
<td>1.55</td>
</tr>
<tr>
<td></td>
<td>(-0.36, 2.42)</td>
<td>(0.05, 2.97)</td>
</tr>
<tr>
<td>Independent counsel</td>
<td>0.06</td>
<td>0.18</td>
</tr>
<tr>
<td></td>
<td>(-1.54, 1.92)</td>
<td>(-1.11, 1.62)</td>
</tr>
<tr>
<td>Constant</td>
<td>3.38</td>
<td>0.19</td>
</tr>
<tr>
<td></td>
<td>(2.17, 4.64)</td>
<td>(-0.68, 1.28)</td>
</tr>
<tr>
<td>N</td>
<td>25</td>
<td>25</td>
</tr>
</tbody>
</table>

Bayesian negative binomial models

code used to estimate the models, which was adapted from Durham, Pardoe and Vega (2004), is provided in Appendix D.) All parameters were given relatively uninformative priors.\(^{21}\) Three independent Markov chains were run for 250,000 iterations after a 250,000 iteration burn-in period for each model and then thinned to facilitate analysis in R. Brooks-Gelman-Rubin diagnostics indicate that all Markov chains converged (Gelman and Rubin 1992; Brooks and Gelman 1998). For each coefficient below, I report the mean posterior draw \(\hat{\beta}\) and the 95% Bayesian credible interval.\(^{22}\)

Table 4.3 indicates that the effect of divided government does not reach conventional values of statistical significance for either dependent variable in either

\(^{21}\) Variance parameters were specified using uniform distributions on the standard deviation of the variance as recommended in Gelman (2006) and Gelman and Hill (2007). Continuous variables were standardized to facilitate convergence.

\(^{22}\) As a robustness check, I also estimated frequentist negative binomial models with bootstrapped standard errors. All findings reported in this chapter hold under this alternative procedure.
Table 4.4: Effect of opposition Congress on investigations and scandal 1953–2002

<table>
<thead>
<tr>
<th></th>
<th>Original sample</th>
<th>Matched sample</th>
<th></th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Investigation</td>
<td>Scandal</td>
<td>Investigation</td>
<td>Scandal</td>
</tr>
<tr>
<td>Opposition Congress</td>
<td>0.44 (-0.72, 2.06)</td>
<td>0.66 (-0.46, 2.09)</td>
<td>0.47 (-0.85, 2.14)</td>
<td>0.61 (-0.50, 2.02)</td>
</tr>
<tr>
<td>Opposition approval</td>
<td>0.12 (-0.64, 1.02)</td>
<td>0.14 (-0.48, 0.91)</td>
<td>-0.13 (-1.13, 0.79)</td>
<td>0.15 (-0.51, 0.93)</td>
</tr>
<tr>
<td>Polarization</td>
<td>-0.42 (-1.37, 0.40)</td>
<td>0.45 (-0.38, 1.59)</td>
<td>-0.44 (-1.68, 0.44)</td>
<td>1.49 (0.13, 2.89)</td>
</tr>
<tr>
<td>Second term</td>
<td>0.79 (-0.50, 2.41)</td>
<td>1.36 (-0.05, 2.82)</td>
<td>0.55 (-0.82, 2.40)</td>
<td>1.95 (0.27, 3.43)</td>
</tr>
<tr>
<td>Independent counsel</td>
<td>0.14 (-1.26, 1.87)</td>
<td>0.18 (-0.99, 1.54)</td>
<td>-0.41 (-2.51, 1.38)</td>
<td>-1.12 (-3.10, 0.49)</td>
</tr>
<tr>
<td>Constant</td>
<td>3.17 (2.15, 4.34)</td>
<td>0.12 (-0.72, 1.08)</td>
<td>3.67 (2.45, 5.05)</td>
<td>0.30 (-0.57, 1.36)</td>
</tr>
<tr>
<td>N</td>
<td>25</td>
<td>25</td>
<td>18</td>
<td>18</td>
</tr>
</tbody>
</table>

Bayesian negative binomial models

The estimated effect of divided government is centered near zero for Mayhew’s investigation data in both the original and matched sample—performing matching to reduce model-based extrapolation does not overturn his null finding. Similarly, while the estimated effect of divided government for the Ostrom-Smith-Newman scandal measure is somewhat larger, particularly for the matched sample, the 95% credible interval still overlaps zero for both samples. Because we are performing Bayesian inference, we can avoid the awkward contortions required to interpret \( p \)-values and instead directly calculate the probability that the effect of the divided government treatment on scandal is in the expected direction for the scandal dependent variable: \( p(\beta) > 0 = .69 \) for the original sample and \( p(\beta) > 0 = .73 \) for the matched sample. Neither approaches conventional levels of statistical significance.\(^{23}\)

\(^{23}\) Per Panagolpoulos and Green (2008), it might be useful to determine how strong one’s prior would have to be for the posterior distribution to reach statistical significance.
Table 4.4 presents similar results for the effect of the opposition Congress on investigation and scandal. As in the previous table, the effects are consistent with Mayhew’s finding. We observe that the opposition Congress treatment has an effect centered near zero for both investigation and scandal before and after the matching procedure. The probability that the treatment effect is positive never reaches conventional levels of statistical significant \( p(\beta) > 0 = .73 \) for the original and matched investigation data; \( p(\beta) > 0 = .84 \) for the original and matched scandal data). Again, despite including a larger number of Congresses that include those for which the independent counsel statute was in effect, the results do not support the hypothesis that opposition party control of one or both chambers of Congress increases investigations or scandal.\(^{24}\)

Conclusion

Surprisingly, little evidence exists to support the conventional wisdom that divided government increases the occurrence of Congressional investigations of the president or presidential scandal. Mayhew (1993, 2005) found that investigations of the president did not increase under divided government, but his research has important methodological limitations. This chapter attempts to remedy those problems and estimate more accurate treatment effects for divided government. I estimate treatment effects for divided government and opposition Congress on both presidential investigations and scandal, but none of them reach conventional levels of statistical significance. These findings should not be surprising given the extremely small sample sizes and the results reported in the previous two chapters. However, they run counter to the beliefs of a large number of experts on Congress and thus deserve further investigation.

\(^{24}\) Again, it would be interesting to determine how strong one’s prior would have to be for these results to reach conventional levels of statistical significance.
As partisanship and the frequency of divided government have increased in recent years, conflict between Congress and the president has increased dramatically. The legislative aspects of interbranch conflict have become an increasingly important part of the study of American politics. However, other aspects of Congress-presidency relations have received less attention. In particular, Congressional allegations of scandal against the president and the executive branch, which have become increasingly frequent in the post-Watergate period, are not well understood.

In this chapter, I develop three hypotheses about the factors that influence presidential scandal allegations by members of Congress and test them on allegations data from the Congressional Record between 1985 and 2006. I find that scandal allegations decline as state- and district-level presidential vote increases among members of the opposition party in both the House and the Senate. Members of the Senate are also more likely to make allegations as they gain seniority within the chamber, but members of the House are not. Finally, members who are
up for re-election in the Senate tend to make fewer allegations than those who are not.

Theory and hypotheses

Given the risks entailed in criticizing the nation’s top elected leader, why would individual members of Congress directly challenge the president by making an allegation of scandal? From an instrumental perspective, the most obvious reason to make allegations of presidential scandal is to damage the chief executive’s political power and reputation. But perhaps more importantly, publicizing an alleged scandal can tarnish the president’s party and its members, strengthening the standing of the opposition party (Adut 2005). These benefits are shared by all party members, creating a potential temptation to free ride. However, there may also be selective rewards for promoting allegations of scandal including media attention, improved standing with party activists and primary voters, and increased stature within the party caucus.

Of course, members of Congress are strategic actors who do not act in isolation. Their allegations are more likely to be effective if other members are making the same allegation. It is therefore necessary to model the process by which members of the opposition party promote scandal allegations against the president, which can be understood as an $n$-player coordination game with strategic complementarities. In Chapter 1, I model the process using an adaptation of Morris and Shin’s analysis of currency crises (1998), a “global game” that allows me to derive a unique equilibrium. From this equilibrium, I can prove the relevant comparative static:

**Proposition 3.** As the transactions cost of scandal allegations increases, the number of allegations (and scandals) will decrease. (See Appendix E for proof.)
To make the model tractable, the players’ costs and utility functions are symmetric. However, we can extend this intuition to propose two informal hypotheses concerning the influence of transaction costs on the incidence of scandal allegations among opposition legislators.¹

*Hypothesis 1:* As the electoral costs of making scandal allegations increase, opposition legislators will make fewer scandal allegations against the president.

*Hypothesis 2:* As opposition legislators increase in seniority within the chamber, they will make more scandal allegations against the president.

Both hypotheses follow straightforwardly from the logic of transaction costs. Given the stigma against scandalmongering, members of Congress are less likely to make a scandal allegation as doing so becomes more costly to their electoral prospects. These costs will vary by state or district depending on public opinion toward the president as well as the stage of the electoral cycle in the Senate. In addition, as members accumulate more seniority, their increased stature should give them more latitude to challenge the president, thereby reducing the transaction costs of scandal allegations.

Data and estimation strategy

Due to methodological and data collection challenges, political speech has typically been the domain of qualitative research. However, the availability of large amounts of digitized text has spurred a number of new studies that analyze texts

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¹ In this chapter, I also generalize from the formal model in analyzing the total number of scandal allegations made by members, not just those allegations that were made before a scandal was recognized as occurring. The intuitions derived from the model regarding transactions costs should extend straightforwardly.
ranging from floor speeches to party manifestos to blog posts (e.g. Laver and Garry 2000; Laver, Benoit and Garry 2003; Quinn et al. 2006; Thomas, Pang and Lee 2006; Fader et al. 2007; Hopkins and King 2007; Martin and Vanberg 2008). This study follows a similar approach, leveraging the electronic archives of the Congressional Record to quantify the number and type of presidential scandal allegations made by opposition legislators in Congress between 1985 and 2006.

**Dependent variable**

To reduce subjectivity in coding, I focus on statements in which members of Congress make direct allegations of “scandal” against the president or the executive branch, recording each instance in the Congressional Record for the House and Senate between 1985 and 2006. Unlike news reports, which are a heavily censored source of data, the Record is a fully detailed record of every statement made on the floor that can only be edited by the members themselves. I coded an allegation in each instance in which a member of Congress referred to actions taken by the administration or past actions by members of the administration as “a scandal,” “-gate,” or a violation of the law in their statement or in extensions of remarks entered into the record (see Appendix F for additional coding details). This relatively strict coding standard should increase the precision of the data, make it easier to replicate, and minimize subjectivity in coding.  

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2 The data were coded from the searchable archive in Lexis-Nexis Academic, which is not available electronically before 1985. It covers 2496 members who served one or more full terms during the 1985–2006 period. Members who served partial terms were omitted from the data for that Congress.

3 I excluded controversies concerning the actions of previous administrations except for the presidency of George H.W. Bush, who was held responsible for the actions taken by the Reagan administration when he was Vice President.

4 While it might be desirable in principle to expand the coding criteria to include critical statements that do not include a direct allegation of scandal, the ambiguities of political speech make such a task difficult. It is not clear how to define a looser standard that can be applied in a consistent and objective fashion.
The resulting data appear to capture the major scandals and scandal allegations of the last twenty years, including the Iran-Contra affair (111 allegations), the savings and loan scandal (267 allegations), Whitewater (60 allegations), Monica Lewinsky (57 allegations), and Abu Ghraib (120 allegations). Figure 5.1 displays the trajectory of opposition presidential scandal allegations over time in comparison to allegations from the president’s party for each chamber. While the series are correlated, the opposition is clearly more aggressive in attacking the president in both chambers—approximately 82% of the 1737 allegations coded were made by opposition party members. Given the likelihood that the predictors of scandal allegations differ by party, the analysis below focuses exclusively on the opposition.

Also, the number of allegations per year displays substantial variability that coincides with well-known periods in which scandal allegations were prominent in the press (Iran-Contra in 1987–1988, the savings and loan crisis in 1989–1990, Bill Clinton’s second term, and the aftermath of the invasion of Iraq). This pattern demonstrates the face validity of the data. In addition, it validates the analytical approach taken in the formal model, which suggests that opposition members are reacting to political circumstances and not just issuing a steady stream of allegations. Not only are the number of allegations high in years during which scandals are in the news, but it is also somewhat low in the “honeymoon” period for both Bill Clinton and George W. Bush. Allegations were at their lowest in 2001–2003, which includes the September 11, 2001 terrorist attacks, their aftermath, and the war in Iraq—a period in which it was costly to criticize the president due to his high approval ratings. Finally, the figure indicates that patterns of allegations over time are similar for the House and the Senate, though it appears that House Republicans were more aggressive than their Senate counterparts in making scandal allegations against President Clinton in the 1995–1998 period.
FIGURE 5.1: Scandal allegations by party and chamber 1985–2006

(a) House of Representatives

(b) Senate
Independent variables

Hypothesis 1 argues that the number of scandal allegations made by members of Congress will vary depending on the electoral costs of making them. The most straightforward operationalization of this concept is to use aggregate presidential vote totals by state and district as a predictor. However, the transaction costs of scandal allegations are unlikely to remain constant across the six-year electoral cycle of a member of the Senate since retrospective voters appear to have a relatively short memory (Bartels 2008). As such, I estimate models separately for the House and Senate. The Senate model includes a variable measuring whether a member of the Senate is going to be up for re-election in a given electoral cycle. I expect that members up for re-election will also make fewer scandal allegations due to increased electoral costs of scandal-mongering during this period. My second hypothesis is that opposition party members are more likely to make scandal allegations as their seniority within a chamber increases. I measure this using a simple count of the number of terms served in the chamber at the start of the Congress.\(^5\)

To account for other plausible institutional influences on scandal allegations, I add member-level controls for whether the senator is a member of the opposition leadership, which I define as being the first- or second-highest ranked member in each chamber.\(^6\) Members in either role may be expected to promote scandal allegations as part of their responsibilities.\(^7\)

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\(^5\) The variable is calculated from the data of Nelson (1994) and Stewart and Woon (2005). The value of the seniority variable is divided by 10 in the analysis below to increase the interpretability of the graphs presenting coefficient estimates.

\(^6\) For the House: Speaker of the House and majority leader for the majority party; minority leader and majority whip for the minority party. For the Senate: majority leader and majority whip for the majority party; minority leader and minority whip from the minority party. In cases in which a leadership change occurred during a Congress, the members who served in a position for the majority of the two-year period were coded as the relevant leaders.

\(^7\) I also considered a variable indicating which members are chair or ranking member of a Con-
Statistical model

Because the key explanatory variables of presidential vote and seniority vary at four- and two-year intervals, respectively, we define the unit of analysis as a member-Congress and construct the dependent variable as the total number of allegations made by a member in a given Congress. To analyze these data, I estimate a hierarchical generalized linear mixed model, which allows us to take heterogeneity among members and Congresses into account—an approach that has been increasingly recognized as a useful way to analyze data comprised of heterogeneous groups (Park, Gelman and Bafumi 2004; Bafumi et al. 2005; Beck and Katz 2007; Shor et al. 2007).

The GLMMs I estimate below allow the intercept term to vary by Congress. The size of the within-group variance of the intercept, which is estimated from the data, determines the degree to which the data are pooled in estimation. In this way, the random effects allow the model to account for Congress-level heterogeneity in the number of scandal allegations.\(^8\) Also, to account for unexplained heterogeneity among members as well as possible correlations between observations among members who serve in more than one Congress, I also include a non-nested random effect for each member.\(^9\) The Congress- and member-level random effects are non-nested to provide maximum flexibility in estimation.

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\(^8\) I also considered adding a random effect for presidential vote (i.e. a “random slope”) but there was not sufficient variance to estimate an effect.

\(^9\) This random effect should account for possible ideological differences among members if those differences affect the incidence of scandal allegations. It is not possible to control for ideology directly because member ideology (to the extent it can be measured) should be highly correlated with the partisanship of the district or state, which is reflected in the presidential vote. As a practical matter, it is also not clear how to measure opposition ideology in a comparable manner across parties.
Results

The dependent variable for this study—a count of the total number of scandal allegations made by an opposition legislator in a given Congress—would typically be modeled as a GLM using the Poisson distribution with the log link function. However, the allegation data shows evidence of substantial overdispersion in both chambers (House: $M = .35$, $SD = 1.73$; Senate: $M = .96$, $SD = 2.28$), which violates the Poisson assumption that the mean and variance of the dependent variable are equal and leads to underestimation of standard errors.\textsuperscript{10} While most opposition members do not make scandal allegations from the floor in a given Congress, a few do so repeatedly (the mean number of allegations is greater in the Senate but the pattern is similar in both chambers). For instance, 68\% of the observations in the Senate take a value of zero, but five senators made more than ten allegations in a given Congress: Robert Byrd in the 100\textsuperscript{th} (12), Harry Reid in the 109\textsuperscript{th} (13), James Inhofe in the 106\textsuperscript{th} (14), and Patrick Leahy in the 108\textsuperscript{th} (18) and 109\textsuperscript{th} (24). The distribution in the House is even more overdispersed—86\% of member-Congress observations in the House are zero, but ten members made more than ten allegations in a single Congress. The most prolific House members were Major Owens in the 101\textsuperscript{st} (20), Christopher Cox in the 104\textsuperscript{th} (26), Tom DeLay in the 105\textsuperscript{th} (27), Henry Gonzalez in the 102\textsuperscript{nd} (37), and Bob Dornan in the 104\textsuperscript{th} (38).

It is necessary to estimate an appropriate statistical model to account for this overdispersion. We are particularly interested in accounting for heterogeneity by member and Congress. I therefore estimate a version of the Bayesian negative binomial model introduced in Chapter 4 with Congress- and member-level random effects. The code used to estimate the models, which is adapted from Durham,\textsuperscript{10} As noted above, this variable includes repetitions of the same allegation within a given Congress in multiple floor statements.
Pardoe and Vega (2004), is provided in Appendix G. All parameters were given relatively uninformative priors.¹¹

Three independent Markov chains were run for 20,000 iterations after a 10,000 iteration burn-in period and then thinned to facilitate analysis using the R2WinBUGS package in R for the House and Senate. Brooks-Gelman-Rubin diagnostics indicate that all Markov chains converged for both models (Gelman and Rubin 1992; Brooks and Gelman 1998). For each model, I report the mean posterior draw \( \hat{\beta} \) and the 95% Bayesian credible interval, which are summarized in the text. Model results are presented graphically to facilitate interpretation with heavy lines representing the 50% CI and lighter lines representing the 95% CI (Gelman, Pasarica and Dodhia 2002).¹²

Figure 5.2 presents model results for the House of Representatives. Coefficient estimates for the fixed effects are presented in Figure 5.2(a) and estimated random effects by Congress are presented in Figure 5.2(b) (estimated member random effects are omitted for space but available upon request). Turning to hypothesis 1 first, we find strong support for the prediction that district-level presidential vote will be negatively related to presidential scandal allegations (\( \hat{\beta} = -4.91, 95\% \) CI: -6.35, -3.51). However, contrary to hypothesis 2, House seniority has no clear effect on scandal allegations (\( \hat{\beta} = 0.11, 95\% \) CI: -0.24, 0.53). Finally, opposition House leaders do not make significantly more scandal allegations than the average opposition member, though the estimated effect is nearly statistically significant at the 95% level (\( \hat{\beta} = 0.95, 95\% \) CI: -0.40, 2.45).

Examining the estimates of the Congress-level random effect (\( \hat{\sigma} = 1.48, 95\% \) CI: 1.10, 2.03).

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¹¹ To reduce autocorrelation, I omitted the constant term and estimated the constant as the mean of the Congress-specific random effects. Adjusted random effect terms were then calculated by subtracting the overall mean from each random effect term.

¹² All coefficient plots were constructed using the coefplot function in the arm package for R (Gelman et al. 2008). Tabular summaries of model results are available upon request.
Figure 5.2: Opposition scandal allegations in the House 1985–2006

(a) Coefficients

(b) Random effects

Coefficient estimates

Congress
we find that the fewest allegations were expected in the 107\textsuperscript{th} Congress, which includes the September 11, 2001 terrorist attacks (mean = -2.43, 95% CI: -3.46, -1.59). By contrast, the largest Congress random effect took place during the 101\textsuperscript{st} Congress (mean = 1.41, 95% CI: 1.08, 1.76), President George H.W. Bush’s first term, which included the savings and loan crisis and fallout from the Iran-Contra scandal. Estimated member random effects (\(\hat{\sigma} = 1.69, 95\%\ CI: 1.47, 1.91\)) ranged from -1.76 for William H. Gray III (95% CI: -4.39, 0.50), a member and eventual minority whip from an overwhelmingly Democratic district in Philadelphia who didn’t make a scandal allegation from the 99\textsuperscript{th} to the 102\textsuperscript{nd} Congress, to 5.51 for Bob Dornan (95% CI: 4.37, 6.89), a member from southern California who made 53 scandal allegations against Bill Clinton in the 103\textsuperscript{rd} and 104\textsuperscript{th} Congresses.

Figure 5.3 presents equivalent model results for the Senate. Fixed effects are again presented in Figure 5.3(a) and estimated random effects by Congress are presented in Figure 5.3(b). (Estimated random effects by member are again omitted for space but available upon request.) The Senate model strongly supports hypothesis 1. State-level presidential vote is again associated with fewer scandal allegations against the president (\(\hat{\beta} = -2.75, 95\%\ CI: -4.75, -1.30\)) as is the stage of the electoral cycle—senators who are up for re-election at the end of a given Congress are less likely to make scandal allegations than their counterparts, though the coefficient is only significant at the 90\% level (\(\hat{\beta} = -0.33, 95\%\ CI: -0.73, 0.01\)). Unlike the House, seniority is associated with more frequent scandal allegations at the 90\% level, a finding that supports hypothesis 2 (\(\hat{\beta} = 0.49, 95\%\ CI: -0.04, 1.03\)). Finally, unlike the House, opposition leaders in the Senate (majority or minority leader and majority or minority whip) make more allegations than other opposition party members (\(\hat{\beta} = 0.81, 95\%\ CI: 0.00, 1.64\)).

The Congress-level random effect for the Senate (\(\hat{\sigma} = 1.18, 95\%\ CI: 0.67, 2.03\)) again ranges from a minimum in the 107\textsuperscript{th} Congress (mean = -1.85, 95% CI: -2.78,
FIGURE 5.3: Opposition scandal allegations in the Senate 1985–2006

(a) Coefficients

(b) Random effects
to a maximum in the 101st Congress (mean = 1.48, 95% CI: 1.04, 1.94). The member-level random effect ($\hat{\sigma} = 1.14$, 95% CI: 0.85, 1.43) reaches its lowest and highest values with Democrats. The senator with the most negative random effect is Max Baucus of Montana (mean = -1.34, 95% CI: -3.07, 0.10), who made no allegations in the 99th–102nd Congress or the 107th–109th Congress despite his increasing seniority. The most positive random effect was for Vermont’s Patrick Leahy (mean = 2.24, 95% CI: 1.39, 3.18), who made 18 scandal allegations in the 99th–102nd Congresses and 43 in the 107th–109th Congresses.

To illustrate these results, I generate predicted values (including 95% credible intervals) for the effect of presidential vote and seniority on scandal allegations by simulating directly from the posterior distributions of the parameters in the model (Gelman and Hill 2007, 362–363). These simulations are conducted using representative covariate values to avoid extrapolation beyond the observed data (King and Zeng 2006, 2007).

Figure 5.4 presents smoothed predicted allegations for an opposition House member and an opposition senator in the 109th Congress as presidential vote increases from the 5th to 95th percentile value for that Congress (the member is assumed to have a random effect term of 0). Specifically, the predicted values for the House are calculated using the median seniority level of five terms for a non-leader as the presidential vote varies from .16 to .59—a range that parallels the difference in support for the president faced by Rep. Diane Watson (D-CA) and Rep. Dan Boren (D-OK) during that Congress. Similarly, predicted values for the Senate are calculated using the median seniority level of six terms for a non-leader who is not up for re-election as presidential vote varies from .39 to .63. This difference is equivalent to the gap between the state-level support for the president faced by Jim Jeffords (I-VT) and Patrick Leahy (D-VT) and that faced by Kent Conrad (D-ND) and Byron Dorgan (D-ND) during the 109th. While the expected
number of allegations is higher in the Senate, we observe that changes in presidential support correspond to significant changes in expected scandal allegations in both chambers. As presidential vote increases over the ranges described above, expected scandal allegations drop by half for Senate members (1.2 to .61) and decline all the way from 0.29 to zero for House members.

Figure 5.5 holds presidential vote constant at the 109th median values of .41 and .48 for the House and Senate, respectively, and presents predicted allegations (smoothed) as seniority varies from its 5th to 95th percentile values for that Congress (1–15 congresses for the House, 1–22 congresses for the Senate). As noted above, seniority is not found to have an effect in the House, but it dramatically increases the predicted number of allegations in the Senate from .66 for a newly elected member to 2.30 for a member serving in their 22nd Congress—
a range corresponding to the difference between Ken Salazar (D-CO) and Ted Kennedy (D-MA) in the 109th Congress.

As a robustness check, I re-estimate the models above as a constant dispersion frequentist negative binomial model in Stata 10 with member-level random effects\textsuperscript{13} and Congress-level fixed effects. The substantive findings are identical, though the coefficient for being up for re-election in the Senate just misses statistical significance ($p < .12$).

**Conclusion**

The results presented above show that scandal allegations among opposition party members decline significantly as voting support for the president increases in a

\textsuperscript{13} The variance is assumed to be $\mu + \alpha \mu$ and the inverse of the dispersion parameter $\alpha$ is assumed to be distributed Beta ($r, s$) (Stata Longitudinal/Panel-Data Reference Manual 2007, 383–390).
state or district. I also find that members of the Senate who are up for re-election make fewer allegations than those who are not and that seniority is positively related to scandal allegations in the Senate (but not the House). Finally, I show that opposition party leaders make more allegations than the party rank and file in both chambers. These findings indicate the need to understand the strategic nature of political scandal. Opposition party members who wish to damage the president do not simply issue allegations randomly. Instead, they react to political circumstances, making more allegations when the costs of doing so are lower. Scandal allegations are, indeed, politics by other means (Ginsberg and Shefter 2002).
The Allegation Network in Congress

One of the most famous political quotations of the 1990s is Hillary Clinton’s denunciation of “this vast right-wing conspiracy that has been conspiring against my husband” during an interview with Matt Lauer on NBC’s Today (Lauer 1998):

LAUER: [James Carville] has said that this is war between the President and Kenneth Starr. You have said, I understand, to some close friends, that this is the last great battle and that one side or the other is going down here.

CLINTON: Well, I don’t know if I’ve been that dramatic. That would sound like a good line from a movie. But I do believe that this is a battle. I mean, look at the very people who are involved in this. They have popped up in other settings. This is—the great story here for anybody willing to find it and write about it and explain it, is this vast right-wing conspiracy that has been conspiring against my husband.

While the phrase “conspiracy” is arguably overstated, it is a fact that a network of Clinton’s opponents actively worked to try to bring down his presidency by
developing and promoting scandal allegations against him (Conason and Lyons 2000). This network extended to members of Congress, as former Clinton aide George Stephanopoulos suggested during an interview with Paula Zahn on Fox News (2000), saying that “there were an awful lot of people [in Congress and elsewhere] out to uncover something on this president for an awful long time.”

How can we study the relationships among members of Congress who pursue scandal allegations? While many aspects of the scandal-generating process are unobservable (such as coordination among members), it is possible to use the tools of social network analysis to characterize the linkages between members and allegations, including the patterns of allegations directed against President Clinton. In this chapter, I use this approach to analyze the network of presidential scandal allegations for each Congress from 1985–2006. After constructing these networks and assessing their characteristics, I focus on patterns of clustering, which are a potentially important indicator of collaborative or imitative behavior among members. I find that Congresses in which there are higher levels of allegation clustering (i.e. those members who made allegations tended to make the same ones) tend to have greater levels of scandal coverage in the media. However, allegations made by highly clustered groups of members (i.e. those who jointly made several allegations) tend to receive less coverage than those which are made by broader coalitions of members.1 Finally, I show that the clustering we observe in the allegation networks exceeds that which we would expect in random networks with equivalent degree distributions and is comparable to other social networks that have been studied in the literature.

1 These definitions are made more precise below.
Analyzing scandal allegations as a social network

The study of social networks has exploded in recent years. While much of the network literature in sociology and political science focuses on the mass public, there is a rapidly expanding literature on networks among political elites, including Congressional committee networks (Porter et al. 2005, 2007), cosponsorship networks (Fowler 2006a,b; Zhang et al. 2008; Cho and Fowler N.d.; Epstein, Fowler and O’Halloran N.d.), caucus networks (Victor and Ringe N.d.), and campaign contribution networks (Nyhan and Tofias N.d.); Supreme Court citations (Fowler et al. 2007; Fowler and Jeon 2008); and the sharing of mailing lists among political organizations (Koger, Masket and Noel N.d.).

In this chapter, I build on those studies by analyzing the Congressional Record allegation data introduced in Chapter 5 as a bipartite social network. In such a network, there are two modes (i.e. types of nodes)—members of Congress and scandal allegations—and all links connect nodes from one mode to nodes from the other mode (i.e. members are connected to other members only through allegations).

In particular, the scandal allegation network is part of a class of two-mode networks known as affiliation networks. These are different from normal social networks because they have two modes but only one mode consists of human actors (Wasserman and Faust 1994, 30). Examples of affiliation networks include corporate boards of directors, club memberships, and the actor-movie network that gave rise to the game “Six Degrees of Kevin Bacon” as well as the Congressional, Supreme Court, and political mailing list data analyzed in the studies cited above. Studying such networks allows us to consider how people are connected through their affiliations (and, conversely, how the organizations that provide those affiliations are connected through affiliated people). In this case, the affiliation is less
direct than, say, working on the same movie or being a member of the same club. It is more akin to the ties among cosponsors of legislation.

The data considered in this chapter consist of bipartite allegation networks for each Congress in the 1985–2006 period. To provide intuition, Figure 6.1 presents an illustrative plot of the largest connected portion of the allegation-member network (i.e. the “main component”) for the 106th Congress of 1999–2000. In the figure, Republicans are represented by white circles, Democrats by gray circles, and scandal allegations by black squares; members are connected to those allegations that they made on the House or Senate floor; and line widths are proportional to
the number of times an allegation was repeated.

The plot helps us to identify the most important nodes among both members and allegations. First, we observe that the Lewinsky affair, the twin fundraising scandals of President Clinton’s 1996 re-election campaign (domestic and foreign campaign cash), and allegations of Chinese espionage at Los Alamos National Laboratory were the most frequently made scandal allegations during the 106th Congress. The figure also helps us to identify the key scandal players in that Congress. For instance, Senator James Inhofe (R-OK) was the only member to make all four primary allegations. Other key members include Rep. Tom Tancredo (R-CO), Rep. Jack Kingston (R-GA), Rep. J.D. Hayworth (R-AZ), Rep. Joel Hefley (R-CO), and Rep. Dan Burton (R-IN), all of whom made both the 1996 fundraising allegation and the Los Alamos allegation. Finally, we can characterize the partisan distribution of members making scandal allegations. As we would expect, Republicans dominate among those making scandal allegations; only a handful of Democrats make scandal allegations and they are scattered along the periphery of the network. The sole exception is Senator Joe Lieberman (D-CT), who was the only Democrat to make more than one of the primary scandal allegations. In fact, other than the aforementioned Republicans, Lieberman is the only member to describe the Lewinsky affair and the allegations of Chinese espionage at Los Alamos as scandals.²

For descriptive purposes, I also construct a single social network comprising all ten Congresses in the data. Figure 6.2 presents the main component of this network. As in the previous figure, Republicans are represented by white circles, Democrats by gray circles, scandal allegations by black squares, and line widths are proportional to allegation repetition. For visual clarity, labels are omitted for

² This disconnect between Lieberman’s behavior and his party membership can be seen as foreshadowing his subsequent primary defeat and decision to become an “Independent Democrat” in 2006.
all nodes except the fifteen allegations that were made most frequently. Though there are an infinite number of ways to display a given network, the plot is suggestive of a strong partisan divide in scandal allegations—the Clinton scandals are highly clustered on the right side of the plot, where more Republicans appear, while the Reagan-Bush 41-Bush 43 scandals are clustered on the left side of the plot along with a predominantly Democratic group of members. These findings reinforce the finding of the previous chapters that scandal allegations are strategic and driven by members of the opposition party.
Basic network characteristics

While plots of the data can be helpful, we wish to go beyond visual interpretation (which is often difficult for densely connected networks) and quantify the characteristics of the networks. One simple way to do this is to consider the density of the network. Because we are analyzing a valued network, the average density is the average number of allegations made by a member. Due to the large number of members who made no allegations, the average density of the two-mode scandal network ranges from a low of .003 for the 107th Congress to a high of .063 in the 101st Congress ($M=.020, SD=.018$).

Another way to do this is to calculate the degree distributions of the network among both members and allegations. This statistic measures the pattern of linkages among nodes in the network. In an undirected network such as this one, the degree of a node is simply the number of ties it has to other nodes. Thus, the degree of a member node reflects the number of allegations made by that member, while the degree of an allegation node is the number of times that allegation was repeated in the Congressional Record.

Figure 6.3 describes the degree distribution of the overall network, plotting the number of degrees by node on the $x$-axis and the density (i.e. the total proportion of nodes with that many links) on the $y$-axis. Due to the extremely skewed nature of the distributions, these data are presented as log-log plots for visual clarity. As such, the member degree distribution plots exclude those members who made no scandal allegations (log 0 is undefined). However, the densities plotted on the $y$-axis are calculated using all members.

We observe that the data display the characteristic degree distribution of social networks—link density is low but extremely unevenly distributed. As described in Chapter 5, most members made very few allegations (67% of those who
served during this period make no allegations and 92% of members made fewer than five). Similarly, 83% of allegations were made five times or fewer. However, among both members and allegations, a handful of nodes are very highly connected. The right tail of the member degree distribution consists of a tiny number of members who made a very large number of allegations (many of whom were discussed in the previous chapter): Rep. Major Owens (34), Senator Robert Byrd (37), Rep. Henry Gonzalez (46), Rep. Bob Dornan (55), and Senator Patrick Leahy (63). Similarly, a very small group of allegations were responsible for a large proportion of all allegations: Abu Ghraib (120 allegations), corruption in the Department of Housing and Urban Development (146), corruption in the Defense Department procurement process (150), the Iran-Contra affair (202), and the sav-
ings and loan crisis (267).³

Path lengths and node centrality

We are also interested in the relative positions of members and allegations within the scandal networks, particularly the extent to which they are (or are not) centrally located. The meaning of centrality (and the appropriate way to define it) is controversial and often ambiguous. In this case, our goal is to identify the most central nodes according to several well-known metrics as a check on the validity of the social network approach. We would expect that the most prominent scandalmongers and allegations will occupy central locations in the network. For instance, Rep. Bob Dornan should be a centrally located node in the allegation networks of the 103rd and 104th Congress. If this were not the case, it would suggest that the methodology is somehow flawed.

However, there is little consensus on how best to characterize centrality within a two-mode network (though many approaches have been proposed), so I follow standard practice in the literatures on networks ranging from scientific co-authoring (Newman 2001) to movie acting (Watts and Strogatz 1998) and transform the data into one-mode networks using a technique called “projection.”⁴ We can then analyze the resulting member network and allegation network matrices separately using standard analytic techniques for one-mode data.

Figure 6.4 provides illustrative plots of the resulting one-mode projection net-

³ This type of degree distribution is often described as behaving according to a power law (i.e. the density \( p(x) \propto x^{-\alpha} \) where \( \alpha \) is a scaling parameter). However, these characterizations are often made without sufficient statistical justification (Clauset, Shalizi and Newman N.d.). As such, I will not speculate about the statistical distribution of the observed data here.

⁴ The original data are a series of bipartite social networks with edges linking members to scandal allegations. For each Congress, I construct a two-mode sociomatrix \( B \) with \( i = 1, \ldots, m \) members and \( j = 1, \ldots, n \) allegations where \( b_{ij} \) represents the number of times member \( i \) made allegation \( j \). To convert these matrices to one-mode networks, we simply pre- or post-multiply the sociomatrix \( B \) by its transpose to create a one-mode network that directly links (for instance) members \( p \) and \( q \) who made the same allegation \( s \).
FIGURE 6.4: Projected one-mode scandal networks for the 106th Congress

(a) Member network

(b) Allegation network
works for both members (Figure 6.4(a)) and allegations (Figure 6.4(b)) in the 106th Congress. These plots, and the analyses presented below, removes isolates—those nodes that are not connected to other nodes (i.e. members who make no scandal allegations)—as well as nodes that are not part of the largest connected portion of the network. As before, line width is proportional to tie strength and Democrats appear as gray nodes, Republicans as white nodes, and scandal allegations as black squares. The plots reveal that the projected member-to-member scandal allegation network is densely connected (a fact that may be related to the projection process—see below) and suggest that Senator James Inhofe (R-OK) seems to be centrally positioned within the member network. Likewise, allegations of Chinese spying at Los Alamos and the 1996 fundraising and foreign campaign cash controversies appear to be highly central within the allegation network.

The visual impressions that we derive from network plots above can often be influenced by the particular algorithm used to lay out the networks. As such, we next use three classic measures of centrality to quantify the importance of nodes within the member and allegation networks. First, we use outdegree centrality, which simply measures the number of ties between a node and other nodes (Freeman 1979). Second, we use eigenvector centrality (Bonacich 1972a,b), which represents the weighted sum of the centrality of all nodes to which a given node is connected.

Finally, we use a measure of “closeness,” which we define as the inverse of the sum of the shortest path (known as the “geodesic distance”) between

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5 For members, tie strength is calculated as the sum of the pair’s minimum number of shared allegations. Thus, if member A made one allegations for scandal J and two for scandal K and member B made three allegations for scandal J and one for scandal K, the arc’s weighted value would be min(1,3)+min(2,1)=2. Tie strengths for the allegation network are similarly calculated as the sum of the pair’s minimum number of shared members in common.

6 If we define a $p \times p$ adjacency matrix $A$ for a network with $p$ nodes where $A_{ij} = 1$ if the nodes are connected directly and $A_{ij} = 0$ otherwise, the eigenvector centrality $e_i$ of unit $i$ can be defined as $\lambda e_i = \sum_j A_{ij} e_j$. ($\lambda$ is a constant that is needed to avoid a nonzero solution.) Equivalently, in matrix notation, $\lambda e = Ae$ where $e$ is the eigenvector of $A$ and $\lambda$ is its largest eigenvalue.
Table 6.1: The most central members in the Congressional scandal network

<table>
<thead>
<tr>
<th>Cong.</th>
<th>Degree</th>
<th>Eigenvector</th>
<th>Closeness</th>
</tr>
</thead>
<tbody>
<tr>
<td>99th</td>
<td>Byrd (D-WV)</td>
<td>Goldwater/Metzenbaum</td>
<td>Byrd (D-WV)</td>
</tr>
<tr>
<td>100th</td>
<td>Byrd (D-WV)</td>
<td>Byrd (D-WV)</td>
<td>Byrd (D-WV)</td>
</tr>
<tr>
<td>101st</td>
<td>Leahy (D-VT)</td>
<td>Conyers/Byrd/Leahy</td>
<td>Wirth (D-CO)</td>
</tr>
<tr>
<td>102nd</td>
<td>Kerry (D-MA)</td>
<td>Kerry/Ford</td>
<td>Gonzalez (D-TX)</td>
</tr>
<tr>
<td>103rd</td>
<td>Dornan (R-CA)</td>
<td>Dornan (R-CA)</td>
<td>Dornan (R-CA)</td>
</tr>
<tr>
<td>104th</td>
<td>Dornan (R-CA)</td>
<td>Cox/Dornan/Hefley</td>
<td>Dornan (R-CA)</td>
</tr>
<tr>
<td>105th</td>
<td>DeLay (R-TX)</td>
<td>DeLay (R-TX)</td>
<td>DeLay (R-TX)</td>
</tr>
<tr>
<td>106th</td>
<td>Inhofe (R-OK)</td>
<td>Inhofe (R-OK)</td>
<td>Inhofe (R-OK)</td>
</tr>
<tr>
<td>107th</td>
<td>Feingold/Schak./Filner</td>
<td>Feingold/Schak./Filner</td>
<td>Feingold/Schak./Filner</td>
</tr>
<tr>
<td>108th</td>
<td>Leahy (D-VT)</td>
<td>Leahy/Boxer/Laut./Inslee</td>
<td>Leahy (D-VT)</td>
</tr>
<tr>
<td>109th</td>
<td>Leahy (D-VT)</td>
<td>Reid (D-NV)</td>
<td>Leahy (D-VT)</td>
</tr>
</tbody>
</table>

a node and each other node in a network (Sabidussi 1966).\(^7\) Thus, the most central node is the one with the largest closeness value, which means they can reach all other nodes in the fewest total steps.

Table 6.1 reports the most central member for each Congress according to all three centrality measures. As expected, the measures show relatively high levels of concordance. In particular, they agree completely that Senator Robert Byrd (100\(^{th}\)), Rep. Bob Dornan (103\(^{rd}\)), Rep. Tom DeLay (105\(^{th}\)), and Senator James Inhofe (106\(^{th}\)) were the most central members of their respective networks. In general, we observe that both well-known partisans and party and committee leaders such as Byrd, Senator Patrick Leahy (D-VT), Rep. Tom DeLay, and Dornan make repeated appearances on the list, which suggests that the network approach is a valid way to study patterns of scandal allegations.

When we apply the same centrality measures to the one-mode allegation net-

---

\(^7\) If we let \(d(n_i, n_j)\) represent the shortest distance between nodes \(i\) and \(j\) in a network with \(p\) nodes, then this measure can be defined as \((\frac{1}{p} \sum_{j=1}^{p} d(n_i, n_j))^{-1}\). This measure is undefined when some nodes are unreachable. As such, the closeness measures reported below are calculated for the largest connected component for those networks that were not connected. The measures reported below treat each edge as equal in length rather than incorporating weights to the closeness calculation.
Table 6.2: The most central allegations in the Congressional scandal network

<table>
<thead>
<tr>
<th>Cong.</th>
<th>Degree</th>
<th>Eigenvector</th>
<th>Closeness</th>
</tr>
</thead>
<tbody>
<tr>
<td>99</td>
<td>DOD procurement</td>
<td>DOD procurement</td>
<td>DOD procurement</td>
</tr>
<tr>
<td>100</td>
<td>DOD procurement</td>
<td>DOD procurement</td>
<td>Iran-Contra affair</td>
</tr>
<tr>
<td>101</td>
<td>S&amp;L crisis</td>
<td>HUD corruption/S&amp;L crisis</td>
<td>S&amp;L crisis</td>
</tr>
<tr>
<td>102</td>
<td>S&amp;L crisis</td>
<td>S&amp;L crisis</td>
<td>S&amp;L crisis</td>
</tr>
<tr>
<td>103</td>
<td>Travel Office firings</td>
<td>Travel Office firings</td>
<td>Travel Office firings</td>
</tr>
<tr>
<td>104</td>
<td>Whitewater</td>
<td>Travel Office firings</td>
<td>Whitewater</td>
</tr>
<tr>
<td>105</td>
<td>Foreign $</td>
<td>Lewinskiy affair</td>
<td>Foreign $</td>
</tr>
<tr>
<td>107</td>
<td>DOD funds/cred. cards</td>
<td>N/A</td>
<td>N/A</td>
</tr>
<tr>
<td>108</td>
<td>Abu Ghraib</td>
<td>Abu Ghraib</td>
<td>Abu Ghraib</td>
</tr>
<tr>
<td>109</td>
<td>Abu Ghraib</td>
<td>Abu Ghraib/Abamoff-Safavian</td>
<td>Abu Ghraib</td>
</tr>
</tbody>
</table>

works, we obtain the results reported in Table 6.2. Again, levels of concordance among the measures are quite high. Interestingly, the most prominent and conflictual scandals of the contemporary era (Iran-Contra and the Lewinsky affair) make only infrequent appearances in the list of the most central allegations for their respective time periods. In both cases, scandals that were made by a broader group of members (DOD procurement and the fundraising scandals associated with the 1996 presidential election, respectively) were found to be more central according to a majority of the centrality measures considered.

It’s worth noting how the closeness measure used above is influenced by the uneven degree distributions discussed in the previous section. The pattern of highly uneven degree distributions has been shown in unipartite networks to reduce the average path length among nodes dramatically (Watts and Strogatz 1998). The reason is that any given node can reach any other node in the network in just a few steps via the highly connected nodes. For instance, almost all actors can be reached in six or fewer steps from Kevin Bacon even though he isn’t actually one of the most central nodes in the projected actors network (Watts 1999, 8

Due to the tiny number of scandal allegations in the 107th Congress, eigenvector and closeness centrality measures could not be computed.
145–147). In the projected member networks for the 103rd and 104th Congresses, we can observe a similar pattern. For instance, the prolific Rep. Bob Dornan is no more than two steps from any other non-isolate member, putting all those members to whom he is connected no more than three steps from any other non-isolate member. In fact, excluding the 107th Congress as an outlier, the average shortest path (which is known as the “characteristic path length”) between members in the projected networks ranges from 1.40 to 1.81 by Congress ($M=1.50$, $SD=0.18$). By contrast, an analysis of the main component of the projected actors network (which is of course much larger) found a characteristic path length of 3.65 (Watts 1999, 145). Similarly, the maximum path length between non-isolate members by Congress ranges from two to five steps, whereas the maximum path length to Kevin Bacon in the projected actors network is ten (Watts 2003, 94).

Clustering in bipartite network analysis

Beyond measuring centrality at the node level, we also wish to consider patterns of clustering among nodes, which suggest associative patterns in human networks that are not present in random networks. Substantively, clustering would indicate that if members of Congress A and B both made scandal allegation X, then member A is likely to make scandal allegation Y if member B did so. We could imagine this pattern occurring, for instance, in the 104th Congress when allied Republicans in Congress pursued multiple scandal allegations against Bill Clinton.

The simplest way to analyze these relationships would be to focus on the projected member and allegation networks. However, the process of projection exaggerates clustering and discards key information about the original bipartite network (see Appendix H for details). As such, it is necessary to analyze clustering in the bipartite member-allegation network rather than a projected unipartite
version of it. To do so, Latapy, Magnien and Vecchio (2008) introduce a bipartite “redundancy coefficient” analogous to the clustering coefficient used to study one-mode networks\(^9\). For a node \(v\) with degree two or greater and neighborhood \(N(v)\) (i.e. those nodes connected to \(v\)), the redundancy coefficient is defined as

\[
rc(v) = \frac{|\{\{u,w\} \subseteq N(v), \exists v \neq v', (v', u) \in E \text{ and } (v', w) \in E\}|}{(|N(v)|(|N(v)| - 1))/2}
\]

where \(E\) is the set of links in the network and \(|\mid\) captures the number of links in a given set.\(^{10}\) In words, \(rc(v)\) is the fraction of a node’s neighbors who would be connected in a projection if \(v\) did not exist. Equivalently, it represents the probability that two neighboring nodes to \(v\) have another neighbor in common.\(^{11}\)

In the case of scandal allegations in Congress, the redundancy coefficient allows us to answer two questions:

1. **Member redundancy**: What is the likelihood that any pair of allegations made by a given member were also jointly made by another member?

2. **Allegation redundancy**: What is the likelihood that any two members who made a given allegation also jointly made another allegation?

\(^9\) In unipartite networks, there are two primary definitions of the “clustering coefficient” (Latapy, Magnien and Vecchio 2008, 32). The most well-known is defined as

\[
cc^* = \frac{|E_{N(v)}|}{(|N(v)|(|N(v)| - 1))/2}
\]

for nodes of degree two or greater where \(E_{N(v)}\) is the total number of links between neighbors of node \(v\). This measure represents the average density of those nodes connected to \(v\). Typically, one calculates this measure for each node of degree two or greater and then calculates an average value for the network as a whole.

\(^{10}\) Note that this measure is defined for unweighted data. As such, the results that follow do not account for repetitions of allegations by members.

\(^{11}\) As Latapy, Magnien and Vecchio (2008, 43) point out, their definition is analogous to the generalization of the clustering coefficient to squares described by Lind, González and Herrmann (2005).
The names of these redundancy terms are somewhat confusing because of the dual nature of bipartite networks. Member redundancy actually concerns linkages among allegations. Conversely, allegation redundancy concerns linkages among members.

To make this definition more concrete, we demonstrate the calculation of the redundancy coefficient for the allegation directed at President Clinton’s 1996 fundraising practices for the 106th Congress. Figure 6.1 above depicts this allegation in the two-mode network. Figure 6.5 then compares the actual projected member network (Figure 6.5(a)) to the projected member network with the 1996 fundraising node removed (Figure 6.5(b)). By comparing the linkages among the 22 neighbors of the 1996 fundraising node between the two networks, we can calculate the redundancy of the 1996 fundraising node. In this case, there were 231 possible dyadic links among its neighbors in the original projected network. With the 1996 fundraising node removed, five of the neighbors are dropped from the projected network entirely as isolates, removing 95 possible linkages. In addition, Rep. Curt Weldon (R-PA) retains only two linkages and Rep. Steve Chabot (R-OH) retains seven, leaving a total of 115 links, which generates a redundancy coefficient of \( \frac{115}{231} = 0.498 \).

Following standard practice in the literature on clustering in unipartite networks, I average this measure by Congress (i.e. at the network level) in order to characterize aggregate clustering patterns.\(^{12}\) Figure 6.6 indicates that my data shows good variation in this measure by Congress for both member and allegation redundancy.

\(^{12}\) An alternative approach would focus on the unified allegation network presented in Figure 6.2 as a whole rather than considering allegations from each Congress as a separate network.
FIGURE 6.5: Illustrating redundancy in the 106th Congress member network

(a) Members (actual)

(b) Members, 1996 fundraising removed
Average redundancy by member

Consider first average redundancy by member, which is depicted in Figure 6.6(a). This measure represents the average fraction of neighboring allegations that would remain connected in a projection if a given member (who made two or more allegations) were removed. In our example of the 106th Congress, it would thus range from 0 for Rep. Bob Schaffer (R-CO), who is the only link between the four allegations that he made in the 106th, to 1 for members like Senator John McCain (R-AZ), who only made two popular allegations that would remain connected in a projection if he were removed from the network. The figure shows that average redundancy by member rises and then declines over the course of the Reagan-Bush years, peaking in the 101st Congress. It drops substantially in President Clinton’s first two years in office (the 103rd Congress) before rising over the course of his term. Subsequently, redundancy drops to zero in the 9/11 Congress of the 107th (Bush’s first two years) before bouncing back in the 108th and 109th. In general,
Table 6.3: Member redundancy and Congress-level scandal coverage

<p>| | |</p>
<table>
<thead>
<tr>
<th></th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td>Average member redundancy</td>
<td>1.26</td>
</tr>
<tr>
<td></td>
<td>(0.83, 1.70)</td>
</tr>
<tr>
<td>Constant</td>
<td>3.87</td>
</tr>
<tr>
<td></td>
<td>(3.56, 4.19)</td>
</tr>
<tr>
<td>N</td>
<td>11</td>
</tr>
</tbody>
</table>

redundancy by this measure is quite high, indicating that it is fairly rare for single members to link multiple allegations.

At the macro level, the average redundancy coefficient by member captures whether allegations are being made by multiple members, which is likely to occur when allegations are made numerous times. This convergence in targets is likely to be associated with more significant scandals, which we operationalize again as increased coverage of allegations as scandals by the *Washington Post*. Table 6.3 presents the results of a Bayesian negative binomial mode predicting total scandal coverage of the president within each Congress in the *Washington Post* using the average redundancy coefficient among members at the Congress level. The model is virtually identical in format and estimation to the one presented in Chapter 4. Despite the extremely small sample size (eleven Congresses), the average member redundancy coefficient is highly statistically significant. To make these results more tangible, Figure 6.7 plots predicted values from the model against the data.

13 As in Chapter 3, we use all *Post* stories that were coded as focusing on a given allegation and that explicitly describe it as a scandal.

14 In particular, three independent Markov chains were again run in WinBUGS for 250,000 iterations after a 250,000 iteration burn-in period for each model and then thinned to facilitate analysis in R. Brooks-Gelman-Rubin diagnostics indicate that all Markov chains converged (Gelman and Rubin 1992; Brooks and Gelman 1998). In order to facilitate convergence, the redundancy variable was standardized and the prior precision on the logalpha parameter was increased slightly (from 0.2 to 0.3). For each coefficient, I again report the mean posterior draw \( \hat{\beta} \) and the 95% Bayesian credible interval.
The plot is again presented on a log-log scale to increase visual clarity. The predicted values range from approximately two scandal stories when the average redundancy coefficient is zero (as in the 107th Congress) all the way up to approximately 167 scandal stories when the average redundancy coefficient is .94 (as in the 101st).

We next consider the converse measure, average redundancy by allegation, which is presented in Figure 6.6(b). This measure captures the tendency of members to make clustered patterns of allegations—specifically, the average fraction of pairs of neighbors of allegations (i.e. members) of degree two or greater who also both made one or more of the same allegations in the same Congress. In the

\[ y = \exp(X\beta) \]
example of the 106th Congress, it ranges from 0.05 for the Lewinsky affair, which links numerous members who would not be directly connected in a projection if that allegation were removed from the network, to 1 for three allegations. One example is the claim that President Clinton received illicit foreign campaign contributions, which could be removed from the network without severing any links between members in the projected network.

While the average value of this measure tends to be lower than redundancy by member (since there are fewer allegations than members in each Congress in the data), it captures the steep increase in clustered allegations expected in the early Clinton years, particularly the 104th Congress. The measure then declines somewhat in the 105th and 106th. Like the redundancy by member measure, it drops to 0 in the 107th before returning to normal levels in the 108th and 109th.

We can also use the redundancy coefficient to capture the breadth of the coalition making allegations against the president. In some cases, a handful of the most aggressive members of Congress issue streams of allegations. We would expect such allegations to receive less attention than those that attract support from a broader group of legislators. I therefore calculate the average redundancy coefficient by allegation, which indicates the extent to which an allegation is being made by a group of members who have also jointly made other allegations in a given Congress. I use this measure to predict the volume of scandal coverage a given allegation received in the Washington Post. (This measure is defined identically to the Congress-level measure above—I sum the number of stories focusing on an allegation that describe the controversy as a scandal.) Given the small sample size, I estimate another Bayesian negative binomial model in Table 6.4.

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16 The redundancy coefficient was averaged for those allegations that were made in more than one Congress.

17 I also estimated a frequentist zero-inflated negative binomial model due to the high proportion of zeroes in the data, but the model was not well-estimated (Stata’s algorithm failed to converge.
model finds that there is a strong negative association between allegation redundancy and scandal magnitude. In addition, despite the small sample size, the relationship is highly statistically significant.

To illustrate these results, Figure 6.8 again plots predicted values from the model on a log-log scale. The predicted number of scandal stories ranges from approximately 33 for a scandal with a redundancy coefficient of 0 to just .5 stories for an allegation with a redundancy coefficient of 1.\(^\text{18}\) Inspecting the data, we can see that the most well-known scandals (Monica Lewinsky, Iran-Contra, etc.) are clustered in the upper left since they attracted a broad coalition of members who would otherwise be unconnected. By contrast, scandals with a less diverse and bipartisan supporting coalition (e.g. the Bush administration’s interrogation techniques, the alleged transfer of sensitive technology to China by Loral, etc.) are clustered in the lower right part of the plot. From the levels of redundancy in this part of the plot, we can infer that these allegations were primarily made by members who had also made other allegations together. While it is not possible to draw a direct causal inference from these findings, they are at least suggestive that allegations with broader supporting coalitions are more likely to become scandals and to be covered as such by the press.

\(^\text{18}\) Again, the predicted values are calculated using estimated values from the Poisson component of the negative binomial model. It is also worth noting that the figure excludes the many allegations that never received scandal coverage in the *Post* since log 0 is undefined.)
The Congressional allegation network in comparative perspective

The previous section suggests that levels of redundancy are closely associated with levels of scandal news coverage. However, it is not clear whether these patterns are a significant indicator of non-random behavior. Since Watts and Strogatz (1998), scholars have sought to test whether observed levels of seemingly important network behaviors exceed benchmarks from randomly generated graphs. For instance, Latapy, Magnien and Vecchio (2008) show that observed levels of redundancy tended to differ substantially from equivalent random networks for four diverse bipartite network datasets.\textsuperscript{19}

I follow this approach in comparing my results against random network equivalents. Using the C++ program described in Giullaume and Latapy (2006), I generate random bipartite networks with identical degree distributions among members and allegations for each Congress and randomly link allegation and member nodes.\textsuperscript{20} I then compute average redundancy coefficients for each Congress, which allow me to see if observed levels of clustering are a trivial result of the degree distributions of the member and allegation data or if they reflect significant patterns of associative behavior. The observed levels of redundancy are plotted against their random equivalent in Figure 6.9, which again differentiates between redundancy by member (Figure 6.9(a)) and by allegation (Figure 6.9(b)). Relative to the dashed line, which represents equal levels of redundancy in the random and actual networks, we see that redundancy is almost always higher in the actual networks (the only exception is the redundancy by member in the 103\textsuperscript{rd} Congress).

This pattern differs significantly from what we would expect by chance and sug-

\textsuperscript{19} By contrast, they tested several other potential generalizations of the clustering coefficient to bipartite network data that did not exceed random benchmarks.

\textsuperscript{20} See Giullaume and Latapy (2006) for details on the algorithm. Their implementation sometimes generates duplicate links between two nodes, which I remove. They assert that these duplicates do not affect their results but it would be desirable to alter the software to correct this flaw.
gests that observed redundancy is not simply a consequence of the degree distributions of the member and allegation nodes. In particular, we observe significantly greater levels of clustering by allegation in the 103rd Congress and thereafter (excluding the anomalous 107th) relative to the random benchmark.

Another way to analyze these results is to compare them with other human social networks. Figure 6.10 plots mean redundancy by member and allegation (averaged across the ten Congresses) against redundancy in four other real-world bipartite networks analyzed by Latapy, Magnien and Vecchio (2008): a network of actors and movies constructed from the Internet Movie Database (IMDb), a scientific paper authoring network from arXiv.org, an occurrence network of words and sentences from the Bible, and a network of servers and files from a peer-to-peer network. The plots include both observed levels of redundancy for each network (the black circles) and the estimated level of random redundancy given the degree distributions of the networks (the open circles). In this context, we observe that redundancy by member of Congress is extremely high (greater than all other networks except the biblical occurrence data), while redundancy by allegation is comparable to scientific papers and substantially greater than that of movies or actors in the IMDb data. However, when compared to randomly expected levels of redundancy (either as a ratio or an absolute difference), the scandal allegation network data are actually less clustered than all the comparison networks.

21 Under the null hypothesis of no difference, the probability of observing higher clustering in the actual network is .5. To be conservative, we will count the two tied values in the 107th Congress as failures. Using the formula for binomial probabilities, we can still easily reject the null hypothesis for both redundancy by member ($p < .02$) and redundancy by allegation ($p < .01$).

22 It would be desirable to rewrite the random network generation program to allow for multiple repetitions in order to estimate confidence intervals for the average redundancy estimates. However, this is not currently practical due to the need to hand-code the randomly generated networks into the format needed to estimate the redundancy.

23 An alternative approach would compare the unified allegation network presented in Figure 6.2 with these other networks, which are much larger than the Congress-specific allegation networks.

24 This graph was constructed using R code from Kastellec and Leoni (2007).
networks except words and sentences in the Bible. In other words, the patterns of associative behavior beyond that which we would expect by chance are less pronounced than in many other network contexts.

**Conclusion**

This chapter demonstrates that scandal allegations in Congress can be usefully analyzed as a social network. Doing so reveals both substantive insights about the patterns of member allegations as well as interesting structural similarities to other social networks (including highly uneven degree distributions and short path lengths). My analysis focuses on the bipartite redundancy coefficient, the analogue to the clustering coefficient in unipartite networks, which helps us to examine patterns of clustering in scandal allegations. I find that average member redundancy at the Congress level is positively associated with higher levels of scandal coverage, suggesting that periods during which members have converged on shared targets generate the most scandal coverage. By contrast, at the allegation level, allegation redundancy is associated with less scandal coverage, suggesting that allegations that attract broader coalitions of members receive greater attention.
Figure 6.8: Allegation redundancy and scandal magnitude

Washington Post scandal coverage (log scale)

Average redundancy coefficient

Scandal allegations
Negative binomial fit

(Plot excludes allegations that were not covered in the Post)
FIGURE 6.9: Average redundancy in actual and random allegation networks

(a) By member

(b) By allegation

FIGURE 6.10: Scandal network redundancy in comparative perspective
Conclusion: The Study of Subjective Events

In this dissertation, I have taken a new approach to presidential scandal, which journalists usually attribute to evidence of misbehavior. I argue instead that scandal is a socially constructed perception of misbehavior which opposition elites help create. I formalized this argument by developing the first model of presidential scandal, which predicts that allegations of scandal by opposition legislators can influence the occurrence of scandal within some intermediate range of allegation scandalousness and credibility. I derived two comparative statics showing that the incidence of scandal should increase as the transaction costs of allegations decrease and as the critical mass of opposition legislators required to create a scandal decreases (and likewise for the converse).

In the following chapter, I tested the predictions of the model using data from elite news reports for 1977–2006. In particular, I operationalized the critical mass comparative static using presidential approval among opposition party identifiers—a useful index of a polarized political climate. I found that the president is more vulnerable to the onset of scandal when his levels of opposition approval are relatively low. Conversely, when the president is relatively popular with opposition identifiers (during “honeymoons,” foreign policy crises, and wars), scandals occur much less frequently. In addition, scandals appear to have become more common in the years since Jimmy Carter took office, which could be the result
of increased party polarization. Finally, I showed that the underlying hazard of scandal was greater for second-term presidents than for first-term presidents. (It was also greater for Bill Clinton than for other presidents during this period.)

Next, I shifted from considering the onset of scandal to its magnitude, which I operationalize using the volume of scandal coverage in the Washington Post. I found that scandal coverage increases as approval of the president among opposition party identifiers declines. I also showed that the volume of scandal coverage increases during presidents’ second terms in office and during election years. Despite using a very different statistical approach, these findings are largely consistent with those of the previous chapter.

Journalists and scholars frequently assert that divided government leads to a greater incidence of presidential scandal, but little systematic evidence exists to support these claims, which are not supported in the previous analyses. However, none of these studies uses an appropriate statistical approach for testing the influence of divided government on scandal. I argued that the literature on divided government suffers from several important inferential problems, including a lack of comparable counterfactual data and a resulting reliance on model-based extrapolation. After addressing these issues, I estimated treatment effects for divided government and opposition control of Congress on both high-profile investigations of the president and scandal coverage, but none of the estimates reaches conventional levels of statistical significance.

I subsequently explored the factors predicting when individual members of Congress will make scandal allegations against the president and the executive branch. Specifically, I tested hypotheses developed from my formal model of scandal on a new dataset of scandal allegations against the president in the Congressional Record between 1985 and 2006. Results from multilevel event count models indicated that scandal allegations decline as state- and district-level presi-
idential vote increases among members of the opposition party in both the House and the Senate. Members of the Senate are also more likely to make allegations as they gain seniority within the chamber. In addition, members who are up for re-election in the Senate make fewer allegations than those who are not.

Finally, I analyzed the allegation data as a social network for each Congress from 1985–2006. I presented a new approach to analyze clustering in these data, which helps us to characterize patterns in allegations and member behavior. My analysis indicates that clustering among members—which suggests a convergence in scandal targets—is positively associated with increased scandal coverage at the Congress level. By contrast, I found that highly clustered allegations (i.e. those made by members who also made other allegations together) tend to receive less coverage than those that attract support from a broader coalition of members who would otherwise not be connected.

Extensions in future research

My research on presidential scandal could be extended in several possible directions. One approach would be to disaggregate my dependent variables. First, it may be useful to consider whether the relationships I have analyzed vary by the content of the alleged scandal (sexual, financial, administrative, etc.) or its legal status (i.e. whether the target has been accused of illegal behavior). I have not done so in this dissertation because I have argued that scandals are socially constructed events. As such, within some intermediate range of “scandalousness,” political context can trump factual evidence of misbehavior. In addition, disaggregating my data reduces the variance that I can explain (scandal is a rare event). Still, this sort of disaggregation would at least be a worthwhile check on my empirical results. It would also be desirable to disaggregate the results in Chapter 6.
by chamber to test whether the Congressional social networks of scandal allegations differs substantially between the House and Senate.

A second extension would probe more deeply into the processes by which well-known scandals and allegations unfolded. For instance, how does the timing of allegations vary by party and ideology? Can allegations by members of the president’s party play a significant role in causing controversies to tip into scandal? A related set of questions could be explored using social network tools. One limitation of Chapter 6 is that the analyses are static within a given Congress, which limits our ability to assess the effects of network characteristics (a problem that plagues social network analysis). For instance, are patterns of clustering influencing scandal coverage, a reflection of latent scandal characteristics, or both? It would be desirable to group the allegation networks by party control of the presidency (i.e. 1985–1992, 1993–2000, 2001–2006) and examine the development of these networks over time using dynamic visualizations (Moody, McFarland and Bender-deMoll 2005). In addition, it may be possible to estimate the influence of redundancy patterns and other network relationships on linkages within the network (i.e. allegations) by estimating an exponential random graph model (Wang et al. 2009).  

A third way to extend the results in this dissertation would be to gather additional scandal coverage data from other media sources. Snyder and Puglisi (N.d.) find that newspapers that tend to endorse Democrats give more coverage to scandals involving Republican members of Congress, statewide elected officials, and prominent political figures and that the converse is true for Republican-leaning outlets.  

Unfortunately, however, these models are not dynamic. The best-known longitudinal statistical model for social networks is SIENA (Snijders et al. 2007), but it is not designed for bipartite networks.

It is worth noting that the scandals studied by Snyder and Puglisi (N.d.) include only one
age, which includes a variety of theoretical models (Mullainathan and Shleifer 2005; Gentzkow and Shapiro 2006; Baron 2006; Gasper 2007) and empirical studies (Kahn and Kenney 2002; Groseclose and Milyo 2005; Gentzkow, Glaeser and Goldin 2006; Gentzkow and Shapiro N.d.; Larcinese, Puglisi and Snyder N.d.; Peake 2007; Groeling 2008; Ho and Quinn N.d.). For present purposes, it is worth noting that these studies do not find a consistent pattern of slant by the Post.\footnote{Groseclose and Milyo (2005) use the think-tank citation patterns of the Post to peg its news coverage as somewhat liberal but both Gentzkow and Shapiro (N.d.) and Peake (2007) estimate that the newspaper’s coverage of the Bush administration was relatively neutral.}

I also hope to pursue two broader extensions of this research. First, I could test my model of scandal on state governors, who are frequently targeted by opposition scandal allegations in a similar manner to presidents. Moving from the federal to the state level would provide a much larger dataset, provide cross-sectional variation for each time point (something that is missing at the federal level), and offer a wider array of configurations of institutional control and media structure. Of course, working at the state level would also require some trade-offs. I could apply the coding methodology described in Chapter 2 to reports from the largest newspaper in the state or the one located in the state capitol, but data collection could be time-consuming and might be limited by the availability of electronic news archives for state-level media sources. A particular concern is that data might be missing non-randomly for some states, potentially biasing my results. One approach that might circumvent these problems would be to use Associated Press coverage of gubernatorial scandals, which should (at least in principle) cover all fifty states from a relatively unified editorial perspective.\footnote{Factiva includes a database of national Associated Press stories going back to 1985. The Associated Press State and Local Wire is only archived back to 1998 but includes much more coverage of state politics.}
Alternatively, I could use automated text-coding procedures (e.g. Gentzkow and Shapiro N.d. and Snyder and Puglisi N.d.) to try to reduce the demands of data collection.

A second larger-scale project could use new ProQuest Historical Newspapers electronic database to study the occurrence of presidential scandal and news media coverage of it over a much longer period of American history. Extending the period of analysis to earlier periods in American history will require some sacrifices in terms of the independent variables that are available. However, it would increase the power of my statistical models, extend the historical reach of my findings, and capture a number of features of interest. In particular, it would be possible to compare the Gilded Age era of high partisanship with contemporary politics, avoiding the correlation between polarization and time noted in the empirical analyses in Chapters 2 and 4. As Cameron (2002, 660) points out, the 1876–1896 period featured “highly polarized elites in Congress” and “is the only period in American history in which the probability of divided party government approximates that of today.” He notes that “[a] comprehensive and thoughtful study of presidential politics during the Gilded Age” including “the tactical use of scandals and impeachment threats. . . might well be extremely illuminating.”

The study of subjective political phenomena

In a more general sense, these results suggest formal and quantitative approaches can help illuminate debates over subjective concepts like scandal. We are unlikely to reach any sort of consensus on when or how to apply labels like scandal that have both descriptive and normative content. While debates over these issues should continue, scholars could make more progress by focusing instead on the conditions under which such labels are widely accepted as appropriate descrip-
tions of a given event.

For instance, as briefly noted in Chapter 2, there has been an extensive debate over whether electoral mandates exist or are even possible (in the sense of the electorate collectively providing a signal concerning their public policy preferences to elected officials). Peterson et al. (2003) take a different approach, defining a mandate as a collective interpretation of election results by nervous incumbents worried about re-election. When this interpretation takes hold (as in 1960, 1980, and 1994), members of Congress tend to deviate from their normal voting patterns in the direction of the mandate for several months, particularly those members whose winning margins decreased in the previous election. Eventually, however, the perception wears off and members return to their equilibrium voting patterns. While ultimately subjective in nature, the extent of this reaction is influenced by the underlying political context. Grossback, Peterson and Stimson (2005) find that the extent of these mandate reactions is driven by objective characteristics of the electoral results, including the president’s electoral margin and whether the party of the president changes. (See also Grossback, Peterson and Stimson 2006.)

Similarly, Shaw (1999, 415–416) finds that the occurrence of significant events during presidential campaigns is more likely when presidential approval polls are out of equilibrium with the forecasted outcome of the election. Specifically, when the margin between the leading candidates in pre-election polls deviates by more than five points from the expected margin, campaign events seem to have larger and more long-lasting effects, pushing the candidates toward the expected outcome in either a positive or negative direction. This finding is broadly consistent with the analysis in Chapter 2, which finds that the political environment can make presidents more or less likely to suffer from scandals. In both cases, the underlying political environment appears to affect the incidence of seemingly exogenous events. Another way of viewing this result is that collective interpretations
of events are being influenced by the underlying political conditions.

To be sure, not all literatures in American politics have failed to consider the creation of subjective perceptions from a scientifically neutral perspective. For example, contemporary research on agenda-setting studies the process by which political issues become widely viewed as important problems (e.g. Baumgartner and Jones 1993) while largely bracketing debate over whether those issues should be viewed as such. However, other politically consequential shared perceptions remain poorly understood. One example might be collective beliefs about performance failures by the federal government.29 Consider, for instance, the vastly greater public disapproval incurred by the Bush administration for failing to respond effectively to Hurricane Katrina in 2005 compared with its failure to capture Osama bin Laden near Tora Bora, Afghanistan in late 2001. While there are obvious differences between the two cases (foreign versus domestic policy, differing levels of media access to the events, etc.), the underlying political context seems to play an important role in the different ways the two events were understood.

Why scandal matters

Ultimately, the importance of studying scandal rests on its salience in contemporary American politics. As noted in the introduction, four of the last seven presidents were enmeshed in major scandals that either did or could have led to their removal or resignation from office. In a more prosaic sense, scandal politics have become a customary part of day-to-day life in Washington since Watergate. No president can hope to avoid dealing with scandals or scandal allegations, which can displace policy from public debate. Moreover, no member of the public can follow politics without frequently hearing about scandals and scandal allegations,

29 I am indebted to Jim Stimson for this suggestion as well.
which may increase cynicism toward government or reduce participation in politics. For these reasons, a greater understanding of scandal is a necessary component of understanding the modern presidency, its relationship with Congress, and the way that shared meanings are constructed in a democratic society.
Appendix A: Proofs for Chapter 1

Model equilibrium

Here I derive the equilibrium of the model as the noise term $\epsilon$ approaches 0 (following Heinemann 2000, who corrects an error in Morris and Shin 1998).

**Theorem 1.** As $\epsilon \rightarrow 0, \theta^* \rightarrow \theta_0 \in (\underline{\theta}, \bar{\theta})$ where $\theta_0$ solves $(1 - a(\theta_0))R(\theta_0) = t$.

**Proof.** Define the function $s(\theta)$ to represent the proportion of opposition legislators who receive signals less than $x^*$ and attack when the true state is $\theta^*$. In equilibrium, $s(\theta)$ must equal $a(\theta)$:

$$s(\theta^*) = \frac{x^* - \theta^* + \epsilon}{2\epsilon} = a(\theta^*)$$

We rewrite this as $\frac{\theta^* - x^* + \epsilon}{2\epsilon} = 1 - a(\theta)$.

Given that all players are employing a unique switching strategy in which they attack iff $x < x^*$ (Morris and Shin’s Theorem 1), we can characterize the indifference condition for an opposition legislator at $x^*$ as follows:

$$u(x^*) = \frac{1}{2\epsilon} \int_{x^* - \epsilon}^{\theta^*} R(\theta)d\theta - t = 0$$

Noting $R' < 0$, we define the following two inequalities:

$$\frac{1}{2\epsilon} \int_{x^* - \epsilon}^{\theta^*} R(\theta)d\theta = t > \frac{1}{2\epsilon} \int_{x^* - \epsilon}^{\theta^*} R(\theta^*)d\theta = (1 - a(\theta^*))R(\theta^*)$$
and

\[
\frac{1}{2\epsilon} \int_{x^* - \epsilon}^{\theta^*} R(\theta) d\theta = t < \frac{1}{2\epsilon} \int_{x^* - \epsilon}^{\theta^*} R(x^* - \epsilon) d\theta = (1 - a(\theta^*)) R(x^* - \epsilon)
\]

(1) indicates that \(x^*\) and \(\theta^*\) converge as \(\epsilon \to 0\). We previously specified that both \(a\) and \(R\) are continuous functions. (3) and (4) therefore imply the following:

\[
\lim_{\epsilon \to 0} (1 - a(\theta^* (\epsilon))) R(\theta^* (\epsilon)) = t
\]

Note that \(a\) is increasing in \(\theta\) \((a' > 0)\) and that \(R\) is decreasing in \(\theta\) \((R' < 0)\).

Given that \(a(\theta) = 0\) and \(R(\bar{\theta}) = t\), it follows that a unique solution \(\theta^*\) exists that solves (5) where \(\lim_{\epsilon \to 0} \theta^* (\epsilon) = \theta_0\) and \(\theta_0 \in (\theta, \bar{\theta})\).

**Proposition 1.** As the critical mass of legislators a required to create a scandal increases, the incidence of scandal decreases.

**Proof.** From Theorem 1 above, we know that as \(\epsilon \to 0, \theta^* \to \theta_0 \in (\theta, \bar{\theta})\) where \(\theta_0\) solves \((1 - a(\theta_0)) R(\theta_0) = t\). As \(a(\theta)\) shifts upward in \((\bar{\theta}, \bar{\theta})\), the term \((1 - a(\theta))\) must decrease for all \(\theta\). The reward \(R(\theta_0)\) must therefore be greater for the equality to hold. Since \(R'(\theta) < 0\) and \(\theta \sim U[0, 1]\), the critical threshold \(\theta^*\) shifts downward and a scandal is less likely to occur. The converse holds if \(a(\theta)\) shifts downward by an analogous argument.

**Proposition 2.** As the transactions cost \(t\) of scandal allegations increases, the incidence of scandal decreases.

**Proof.** Taking the derivative of \(\theta^*\) with respect to \(t\), we find that

\[
\frac{d\theta^*}{dt} = \frac{1}{(1 - a)R' - Ra'}
\]
We know that \((1 - a(\theta))R(\theta) = t\) in equilibrium, so we substitute in and find

\[
\frac{d\theta^*}{dt} = \frac{1}{t\frac{R'}{R} - Ra'}
\]

Since \(t > 0\), \(R' < 0\), \(R > 0\), and \(a' > 0\), the first term in the denominator must be negative and the second term must be positive, so \(\frac{d\theta^*}{dt}\) must be negative:

\[
\frac{d\theta^*}{dt} = \frac{1}{t\frac{R'}{R} - Ra'} < 0
\]
Appendix B: Washington Post coding procedure

• Using Lexis-Nexis Academic, I searched the Post for “[president name]” OR “president” OR “white house” in the fields for headline, lead paragraphs, or terms.

• I then narrowed the search to news articles that included the word “scandal” in the A section (previously called “First Section”). Featured news columnists who write in an opinionated voice (such as Mary McGrory or Dana Milbank) were excluded.

• If a specific controversy was referred to as a “scandal” in the reporter’s voice or the headline, the date and scandal were recorded.

• If more than one controversy was described as a scandal in the same article, then a separate entry was created for each scandal reference.

• References to “alleged” scandals were omitted, as were vague descriptions of people or organizations as “scandal-ridden” or “scandal-plagued.”

• Controversies about actions taken by the executive branch under previous administrations were excluded (except for those of President George H.W. Bush, who was held responsible for events that took place when he was Ronald Reagan’s Vice President).
• Scandals that did not directly involve the executive branch such as the collapse of Enron or bond trading scandals on Wall Street were also omitted.

• However, controversies over past actions taken by individuals in the executive branch such as Whitewater were included if those controversies took place while the administration held office.
Appendix C: Scandal onset and magnitude variables

One question that might be asked is how the scandal magnitude variable in this chapter relates to the scandal onset variable presented in Chapter 2. As expected, the variables are positively correlated ($r = .19, p < .05$). More importantly, as we would expect given the way the two variables were coded, the recognition of new scandals tends to precede greater volumes of scandal coverage. We use the cross-correlation function, which plots the correlation between scandal onsets at time $t$ and scandal coverage at time $t + k$ where $k$ is a lag term, to illustrate this point.

**Figure C1:** Cross-correlation of *Post* scandal onset and coverage 1977–2006
in Figure C1. We observe minimal correlation for negative lags, which indicates that there is no relationship between scandal coverage and subsequent scandal onsets. By contrast, we observe substantial positive correlations for positive lags, suggesting that scandal onsets increase the expected value of subsequent scandal coverage. (The contemporaneous correlation of .19 appears in the plot at lag 0.)

Figure C2 illustrates the relationship between the variables directly by adding points to Figure 3.1 (which plots scandal coverage over time) indicating when new scandals were recognized in the Post. Because the data have been aggregated to the quarterly level, the point sizes of the scandal onset variable are scaled to reflect the number of new scandals recognized in that quarter (the total ranges from 1 to 3). This plot and the preceding analysis help us to understand two stylized facts about scandals. One relevant claim is the idea that presidents are most

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1 As in Chapter 3, these correlations were computed after subtracting the sample mean from each variable.

2 The total number of scandal onsets in the graph differs slightly from the dependent variable in the survival analysis presented in Chapter 2. As I note there, the dependent variable took the value of 1 when one or more new scandals occurred in a given month and 0 otherwise—a change that was necessary to conduct a survival analysis.
damaged by waves of scandals that come in bunches. In this case, we observe
the largest clusters of scandal onsets at or near the leading edge of large spikes
in scandal coverage for both the Reagan-Bush peak (1986–1992) and the Clinton
peak (1997–1999), which corresponds to the cross-correlation finding above. By
contrast, during quieter periods, we rarely observe more than one scandal onset
in a quarter and total scandal coverage remains quiescent. Second, though to-
tal scandal coverage remained relatively low during Clinton’s first term by this
measure, he experienced ominous scandal onset clustering that was unlike that
experienced by any other president in the contemporary period.³

³ George H.W. Bush also had a cluster in his first quarter in office but his experience as a vice
presidential successor was unique.
Appendix D: WinBUGS code for divided government models

The code used in the analyses in this chapter, which is adapted from Durham, Par- 
doe and Vega (2004), is presented below. (The dependent and treatment variable 
were changed as appropriate.)

model {
  for (i in 1:N) {
    summh2[i] ~ dpois(mustar[i])
    mustar[i] <- r[i]*mu[i]
    r[i] ~ dgamma(alphastar, alphastar)

    log(mu[i]) <- b.constant + b.oppcong*oppcong[i] + b.lagoppapp*lagoppapp[i] + 
    b.secondterm*secondterm[i] + b.ic*ic[i] + b.lagcsdist1*lagcsdist1[i]
  }

  dev <- -2*sum(ll[])
  alphastar <- exp(logalpha)
  logalpha ~ dnorm(0, 0.2)

  b.oppcong ~ dnorm(0.0, tau.oppcong)
  tau.oppcong <- pow(sigma.oppcong,-2)
sigma.oppcong ~ dunif(0,3)

b.constant ~ dnorm(0.0, tau.constant)
tau.constant <- pow(sigma.constant,-2)
sigma.constant ~ dunif(0,3)

b.lagoppapp ~ dnorm(0.0, tau.lagoppapp)
tau.lagoppapp <- pow(sigma.lagoppapp,-2)
sigma.lagoppapp ~ dunif(0,3)

b.lagcsdist1 ~ dnorm(0.0, tau.lagcsdist1)
tau.lagcsdist1 <- pow(sigma.lagcsdist1,-2)
sigma.lagcsdist1 ~ dunif(0,3)

b.secondterm ~ dnorm(0.0, tau.secondterm)
tau.secondterm <- pow(sigma.secondterm,-2)
sigma.secondterm ~ dunif(0,3)

b.ic ~ dnorm(0.0, tau.ic)
tau.ic <- pow(sigma.ic,-2)
sigma.ic ~ dunif(0,3)
}

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Appendix E: Proof for Chapter 5

Proposition 3. As the transactions cost of scandal allegations increases, the number of allegations (and scandals) will decrease.

Proof. The equilibrium derived in Appendix A is the following (all terms are defined in Chapter 1):

Theorem 1. As $\epsilon \to 0, \theta^* \to \theta_0 \in (\underline{\theta}, \bar{\theta})$ where $\theta_0$ solves $(1 - a(\theta_0))R(\theta_0) = t$.

Taking the derivative of $\theta^*$ (the threshold of scandalousness above which a scandal occurs) with respect to $t$ (the transactions cost of making a scandal allegation), we find that

$$\frac{d\theta^*}{dt} = \frac{1}{(1 - a)R' - Ra'}$$

We know that $(1 - a(\theta))R(\theta) = t$ in equilibrium, so we substitute in and find

$$\frac{d\theta^*}{dt} = \frac{1}{tR' - Ra'} < 0$$

Given that $x^* \to \theta^*$ as $\epsilon \to 0$, $\frac{dx^*}{dt} < 0$ as $\epsilon \to 0$. This implies that the perceived scandalousness of the alleged misbehavior ($x^*$) must be greater for a member to make a scandal allegation as the transactions costs of allegations ($t$) increases. $\square$
Appendix F: Congressional Record coding procedure

- Allegations of scandal in articles or other documents entered into the record were excluded, as were statements by lawyers and House managers during the Senate impeachment trial of Bill Clinton.

- The rhetorical use of “scandal” to describe a disliked state of affairs was not included, nor were cases in which the administration was criticized for failing to prevent some external scandal.

- Scandals from Reagan’s first term were excluded because the Record is not available electronically before 1985.

- References to “alleged” or “potential” scandal were counted if they were not made in a blatantly pejorative context. However, the word “scandalous” was not, nor were general references to programs as “scandal-ridden,” “plagued by scandal,” etc.

- A member could be coded as making more than one allegation per day if the same speech included multiple different allegations. However, repeating the same allegation multiple times in one speech did not increase the number of allegations coded.
Appendix G: WinBUGS code for allegation models

Code for the Senate model, which is adapted from Durham, Pardoe and Vega (2004), is presented below. (Code for the House model is virtually identical.)

WinBUGS

model {
for (i in 1:num.obs) {
allegs[i] ~ dpois(mustar[i])
mustar[i] <- r[i]*mu[i]
r[i] ~ dgamma(alphastar, alphastar)

ll[i] <- loggam(allegs[i] + alphastar) +
alphastar*log(pstar[i]) + allegs[i]*log(1 - pstar[i]) -
logfact(allegs[i]) - loggam(alphastar)

pstar[i] <- alphastar/(alphastar+mu[i])

log(mu[i]) <- a[cong[i]] + b.presvote*presvote[i] +
b.leader*leader[i] + b.seniority*seniority[i] +
b.up*up[i] + c[idno[i]]

dev <- -2*sum(ll[
alphastar <- exp(logalpha)
logalpha ~ dnorm(0.0, 0.01)

for (j in 1:num.cong){
a[j] ~ dnorm(0.0, tau.cong)
a.adj[j] <- a[j] - mean.a
}

mean.a <- mean(a[])

tau.cong <- pow(sigma.cong,-2)
sigma.cong ~ dunif(0,10)

b.presvote ~ dnorm(0.0, tau.presvote)
tau.presvote <- pow(sigma.presvote,-2)
sigma.presvote ~ dunif(0,20)

b.up ~ dnorm(0.0, tau.up)
tau.up <- pow(sigma.up,-2)
sigma.up ~ dunif(0,20)

b.leader ~ dnorm(0.0, tau.leader)
tau.leader <- pow(sigma.leader,-2)
sigma.leader ~ dunif(0,20)
b.seniority ~ dnorm(0.0, tau.seniority)

tau.seniority <- pow(sigma.seniority, -2)

sigma.seniority ~ dunif(0, 20)

for (j in 1:num.id) {
    c[j] ~ dnorm(0.0, tau.id)
}

tau.id <- pow(sigma.id, -2)

sigma.id ~ dunif(0, 10)

}
Appendix H: The problems with projection

As Latapy, Magnien and Vecchio (2008) point out, the process of projecting a bipartite network to create a more tractable unipartite network has two key flaws. First, it may exaggerate clustering. The process of projection induces a clique\(^1\) among all members of one mode connected to a single node from the other mode. More formally, while we should not observe clustering\(^2\) in random networks, Newman, Strogatz and Watts (2001); Giullaume and Latapy (2004, 2006) find that significant levels of clustering can be expected in projections of random bipartite networks with equivalent degree distributions to observed networks. In other words, clustering in projections of bipartite networks (e.g. actors from the actors-movies network) that seems to represent associative behavior may simply be a consequence of the structure of the network. In addition, projection discards key information about the original bipartite network, failing to distinguish between highly connected nodes with many distinct linkages and sparsely connected nodes that are connected to a popular node from the other mode.

Figure H1 demonstrates these points, providing an example of how two different bipartite networks (the A/B matrices at the top) can yield the same projected

\(^1\) The term clique has a specific meaning in graph theory. Specifically, when a clique exists among a set of nodes \(A\), it means that a link exists between any two nodes in \(A\). For instance, all of the actors who appeared in a movie would be linked to each other in the projected actor-actor network.

\(^2\) See Chapter 6 for details on the measurement of clustering.
The bipartite networks at the top of the figure differ in the edges linking A1 and A4 to the B nodes, yet the projection fails to distinguish between A1 and A4—both are connected to all of the other A nodes. In addition, the projected one-mode network appears to be far more clustered than the original bipartite network.

In Figure H2 below, I illustrate how we can observe the problem by contrasting the plot of the two-mode member-allegation network for the 106th Congress of 1999–2000 (previously Figure 6.1) with the projected member-member network (previously Figure 6.4(a)). Compare, for instance, Representative David Wu (D-OR), who appears on the left edge of Figure H2(a), and Rep. John L. Mica (R-FL), who appears in the bottom left corner. Wu made a single allegation of scandal.

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3 It is worth noting that the information loss is permanent in the sense that the original bipartite networks cannot be directly recovered from the projection matrix.
FIGURE H2: 106\textsuperscript{th} Congress allegation network with and without projection

(a) Member-allegation network

(b) Projected member-member network
against President Clinton’s fundraising practices during the 1996 campaign. By contrast, Mica made five allegations, including three that were made by no other member of Congress during the 106th. Nonetheless, Wu and Mica, who are plotted near the bottom left of Figure H2(b), appear to be in structurally similar positions in the projected member network. The reason is that Wu was linked to a highly connected allegation node (Clinton’s 1996 fundraising practices). The projection induced a clique among all members who made that allegation, making Wu appear to be vastly more connected than he appeared in the original plot. Again, it is also apparent that the projected network is more dense and clustered than the original two-mode network.
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Biography

Brendan James Nyhan earned a Bachelor of Arts degree with High Honors in political science from Swarthmore College in 2000 and a M.A. in political science from Duke University in 2005. He will complete a Ph.D. in political science at Duke in 2009. He will then be a Robert Wood Johnson Scholar in Health Policy Research at the University of Michigan before joining the Department of Government at Dartmouth College as an assistant professor in fall 2011.

From 2001–2004, Ben Fritz, Bryan Keefer, and Brendan co-edited Spinsanity, a non-partisan watchdog of political spin that was syndicated in Salon in 2002 and the Philadelphia Inquirer in 2004. In addition, they wrote All the President’s Spin, a New York Times bestseller that was chosen as one of the ten best political books of 2004 by Amazon.com. Brendan’s political writing has been cited in the Washington Post, the New York Times, and many other publications, and he received an Award of Distinction from the Center on Media and Public Affairs in the 2003 Paul Mongerson Prize for Investigative Reporting on News Coverage competition.

From 2001–2003, Brendan managed new projects and then marketing and fundraising for Benetech, a Silicon Valley technology nonprofit. In 2000, he served as the Deputy Communications Director for the Bernstein for US Senate campaign in Nevada. Brendan was born October 17, 1978 in Mountain View, California.