

How Voters Punish and Donors Protect Legislators Embroiled in Scandal*

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Abstract

Previous studies have largely overlooked three key components of a scandal that could determine how it shapes election outcomes: the extent to which it is covered in the media, the potential that donors respond differently than voters, and the likelihood that the impact of scandals have changed over time. Examining U.S. House scandals between 1980 and 2010, we find that while scandal-tainted politicians receive fewer votes and are less likely to win than otherwise-similar legislators not embroiled in scandal, donors actually contribute more money to their campaigns after the scandal's revelation. Both of these effects, however, are limited to financial and sex scandals that garnered national media attention. Moreover, both voters and donors are less punitive in the post-1994 period of nationalized electoral politics.

Scandal is an omnipresent force in American politics. Indeed, at least one member of the U.S. House has been embroiled in a scandal during each Congress since 1991, resulting in dozens of indictments, resignations, and failed reelection bids over the past 25 years (Basinger 2013). Given their status as such an enduring feature of American politics, academics and media alike have devoted a great deal of attention to understanding the effects of political scandals. The general expectation, given the litany of defeats and high-profile resignations, is that scandals should be detrimental to a political career. Yet despite their apparent challenges, many scandal-tainted politicians ultimately win reelection (e.g., Basinger 2013; Jacobson and Dimock 1994; Peters and Welch 1980; Rottinghaus 2014; Welch and Hibbing 1997).¹ While this puzzle has been oft-identified in the extant literature, previous research has yielded only limited insight into why some scandalous politicians manage to overcome their circumstances while others fail.

One important finding in existing work is that not all scandals are equal in terms of their damaging potential. Previous research has therefore sought to explain variation in the electoral fortunes of scandal-tainted legislators by developing a typology of scandals based on the particulars of the alleged conduct. For instance, prior work has considered whether scandals involving financial impropriety are more damaging than those featuring a “moral” transgression—with mixed results (Basinger 2013; Carlson and Hyde 2000; Doherty, Dowling, and Miller 2011, 2014; Welch and Hibbing 1997). Other work has categorized scandals by recency, evaluating whether the immediate effects of scandals are worse than those that occurred in the past (Doherty, Dowling, and Miller 2014; Praino, Stockemer, and Moscardelli 2013). Though findings in previous work do not always agree, they typically underscore not only that the details of a scandal do matter, but also that individuals are reasonably sophisticated in how they evaluate scandal-tainted politicians (e.g., Doherty, Dowling, and Miller 2011).

These findings mark an important contribution to our understanding of how scandals impact political careers. However, we believe that important questions remain largely unexplored. Using data on scandals in the U.S. House between 1980 and 2010, we compare outcomes for scandal-tainted Members of Congress (MCs) to those for similar non-scandal tainted MCs with a *selection on observables* identification strategy. We do so in an effort to engage three im-

portant aspects of scandals, all of which are likely to be important determinants of a scandal's power.

First, the categorization of scandals typically employed in previous research will sometimes fail to adequately reflect the circumstances under which “bad behavior” becomes “scandal,” because the *visibility* of such behavior (i.e., the extent to which it receives major media coverage) is likely to be a more important determinant of its political significance than its “type.” Previous observational work has rightly seen “scandal” as a fairly subjective term, and has therefore taken great pains to implement clear identification and/or classification strategies (e.g., Basinger et al. 2014). These typologies are quite useful for engaging questions such as whether “financial” scandals are more damaging than “moral” scandals on average, but they are less useful for explaining why one sex scandal is more damaging than another sex scandal. The inherent problem is that an identification scheme that employs some rule to define the existence of a scandal (i.e., a public allegation or an Ethics Committee investigation) and then classifies scandals by content will group all such scandals together.

Yet, a dismissed charge of financial malfeasance should be less damaging than one that results in a criminal conviction, largely because (we believe) the media will recognize the distinction and report accordingly. Previous work has examined the conditions under which legislative and executive scandals are likely to garner more media attention (Galvis, Snyder, and Song 2016; Nyhan 2015, 2017; Puglisi and Snyder 2011). However, existing work has not fully considered how a scandal's media visibility affects its potential to damage a political career. We therefore begin with the supposition that media attention is an important—and largely unexamined—factor in how congressional scandals are likely to affect subsequent politics. Simply, scandals that are discussed in national media should be more significant political events, with more consequential effects on political behavior.

Second, donors' responses to even the most visible scandals may attenuate the overall damage to a politician's political future. Previous research has generally focused on the behavior of voters, usually under the assumption that public reaction to a scandal will be negative. We do not challenge this expectation, but we do posit that campaign contributors should react differently to scandal than voters do, since donors are more ideologically motivated than voters,

on average (Barber 2016*b*; Bonica 2014). Previous work in campaign finance (e.g., Francia et al. 2003) has also suggested that large donors often possess an investor mentality, supporting candidates who will deliver on favored policy goals. Indeed, donors with such a mentality target candidates with a high degree of sophistication (Barber, Canes-Wrone, and Thrower 2017), which implies that they are willing to take a long view of a politician's career. Thus, rather than seeking means to punish a scandalous politician, donors might instead take action to contribute *more* money in the wake of a scandal in order to protect their investment.

Third, the manner in which voters and donors respond to scandal today may differ from the way that people reacted to scandals in earlier time periods. We therefore consider how the incentives for voters and donors to punish/protect scandal-tainted politicians may have changed during the thirty-year period that our data encompass. This question is particularly salient given recent work arguing that American electoral politics have become increasingly nationalized of late (e.g., Bonica and Cox n.d.; Hopkins 2018; Lee 2016). One narrative underpinning this trend is that since 1994, it is more possible than before for an election cycle to determine subsequent control of Congress. This has implications for how strategic-minded partisans might view a MC's bad behavior, as punishing scandal-tainted politicians may seem easier when doing so does not risk the policy gains that come from holding the House majority. We therefore might expect voters to be less punitive and donors to be even more supportive in the wake of scandals that occurred during this period.

To test these expectations, we examine how the revelation of a scandal affects the subsequent behavior of both the mass voting public and campaign contributors. We distinguish between scandals that garnered coverage in a national newspaper (*The New York Times*) and those that received no such national media attention. We report four key findings. First, we demonstrate that MCs involved in a scandal receive fewer votes and are less likely to win re-election than MCs not embroiled in a scandal. Second, we show that MCs involved in a scandal raise more money in the immediate aftermath of a scandal than do similar non-scandal tainted legislators. Third, while these first two findings point to differing motivations of voters and donors, they are constrained to scandals that received national media attention, with particularly strong effects in nationally visible financial and sex scandals (as opposed to "political" or

“other” types of scandals). Fourth, consistent with recent theories of electoral competition and the nationalization of politics, we find that all of these effects are significantly different in the post-1994 period than they were before; voters appear to be less willing to punish politicians during this period, while donors offer more support to scandal-tainted members of Congress.

In short, our findings suggest that voters do not punish, and donors do not make effort to rescue, scandalous MCs unless the scandal is serious enough to garner media attention in the national press. Moreover, this is a fairly new development that has coincided with the rise of national political media and the 24-hour news cycle. These results therefore suggest the need to augment a typology of scandal with additional considerations, such as the extent to which it was covered by the media, as well as the need to consider the motivations and behavior of the many political actors who respond to scandal in the current media environment.

Scandal, Voters, and Donors

All else equal, scandals should be expected to negatively impact politicians’ subsequent electoral performance. Indeed, a robust literature demonstrates that accusations of scandalous behavior contribute to diminished evaluations of politicians (e.g., Carlson and Hyde 2000; Doherty, Dowling, and Miller 2011, 2014; Funk 1996; Jacobson and Dimock 1994; Peters and Welch 1980; Welch and Hibbing 1997). In many cases, the effect is sizable. Peters and Welch (1980), for example, show an average vote share decrease of six to eleven percent. The resultant perception of vulnerability appears to have substantial electoral ramifications; politicians involved in scandals fare worse in the election after the scandal breaks (Basinger 2013), in part because they are more likely to face a strong challenge in subsequent primary elections, general elections, or both (Basinger 2013; Lazarus 2008; Hirano and Snyder 2012).

Yet despite these difficulties, most legislators implicated in scandal ultimately retain their seats (Basinger 2013; Peters and Welch 1980; Welch and Hibbing 1997). For example, Basinger (2013) shows that 73% of scandal-tainted incumbents survived the subsequent primary, and of those, 81% won the next general election. Likewise, Peters and Welch (1980) find that 75% of corrupt members who reach the general election win reelection. These figures might seem

high, particularly given the importance of trust (e.g. Fenno 1978, 56) and perceived ethics in the dyadic legislator-constituent relationship (Butler and Powell 2014). This begs a question: If scandal is so detrimental to voters' immediate judgment of politicians, why are so many scandalous politicians re-elected?

One explanation for the seemingly high rate of scandal survival is that the oft-noted incumbency advantage (e.g., Gelman and King 1990) helps MCs to blunt their effects. However, the story is almost certainly more complex. Previous work has recognized that the capacity of a scandal to harm a politician's subsequent electoral prospects likely depends in large part upon the scandal itself. Much extant research has therefore employed some categorization of scandal in an effort to determine which ones are especially damaging. For instance, a growing literature has recognized a difference between "moral" offenses—those involving sexual or some other kind of social impropriety—and "financial" transgressions featuring misbehavior relating to a MC's taxes, spending, or campaign finance accounts. When it comes to the question of which scandal type is worse, the results have not always agreed. Experimental work has typically found that voters judge financial scandals more harshly, while observational studies tend to reach the opposite conclusion (Basinger 2013; Carlson and Hyde 2000; Doherty, Dowling, and Miller 2011, 2014; Welch and Hibbing 1997).

That said, there is an important undercurrent in existing work that has compared different kinds of scandals: voters approach their judgment of a scandal-tainted politician with a fairly high degree of sophistication. Doherty, Dowling, and Miller (2011), for instance, find that "financial" scandals result in voters downgrading assessments of politicians' personal *and* professional characteristics, while "moral" scandals damage only the former; an abuse of official power during the scandal amplifies these effects. Another line of research has examined the timing of a scandal relative to the election, and has found that older scandals are less damaging than those occurring closer to Election Day (Doherty, Dowling, and Miller 2014; Praino, Stockemer, and Moscardelli 2013). In short, there is growing evidence that when it comes to predicting which scandals will harm a political career, the details matter.

Yet, scandal typologies in previous work may leave something to be desired in terms of explaining how damaging a scandal is likely to be—or even whether it is considered a scandal

at all. This claim stems from the fact that observational scandal research effectively presents a two-stage problem: first, identifying the presence of a scandal, and second, determining the criteria that are likely to make it a serious threat to a political career. Basinger et al. (2014) make an important contribution in identifying and categorizing congressional scandals as one of four types (for a fuller description, see below). In doing so, Basinger et al. (2014) recognize the measurement difficulties associated with scandal, in which secret criminal, unethical or immoral behavior must be separated from less nefarious (even if damaging) acts such as gaffes before being defined as “scandalous.” The implication is that when it comes to examining the effects of scandals on the electoral fortunes of MCs, identifying and “typing” a scandal may not fully capture its potential to damage an MC’s political future.

Presumably, a scandal can only harm a politician if news of it reaches voters. The nature of the alleged conduct certainly contributes to media coverage; however, a “political” scandal that amounts to nothing more than an Ethics Committee inquiry is likely to garner substantially less attention than one that results in a criminal conviction. Indeed, in the former case, the typical voter may never be aware that a “scandal” occurred, even if the MCs’ conduct meets some established definition. We therefore believe that in terms of understanding the likelihood that a given scandal will impair an MC’s reelection prospects, a characterization of a scandal as belonging to a certain type should be augmented with a simple consideration: How visible is the scandal to voters?

Previous work has shown that news coverage can shape how the public views politicians (Miller and Krosnick 2000). Moreover, high-information voters are more likely to vote against corrupt MCs (Klašnja 2017), which suggests that exposure to information about a scandal is an important determinant of its effect. We therefore expect that the politics of a given scandal will be significantly shaped by whether the media pay attention to it—a factor that may not necessarily depend wholly on the details of the scandal (e.g., Fogarty 2013). Contextual factors such as media congestion and/or approval of the opposition party are important determinants of the extent to which media covers presidential scandals (Nyhan 2015), and either low approval ratings or few competing news stories can increase media attention to scandals affecting U.S. governors (Nyhan 2017). Other political factors, such as the partisan leanings of a newspaper,

and even non-political factors like the degree of competition among newspapers in a given media market, can determine whether particular scandals are reported on or ignored by the media (Galvis, Snyder, and Song 2016; Puglisi and Snyder 2011). These findings underscore the fact that media coverage may be partially unrelated to the particulars of any one scandal. Indeed, if the political inclinations of a given media outlet drive scandal coverage, then its decision of whether to cover a scandal may not depend even on the severity of the allegations. Thus, rather than focusing on media coverage as a dependent variable—that is, considering which scandals are likely to attract media attention—we posit that media attention is an important *predictor* of subsequent political actions.

Voters tend to separate their assessments of their own representative from that of Congress as a whole (Hibbing and Theiss-Morse 1995), a fact that MCs can exploit to bolster their own standing even if Congress is unpopular (Fenno 1978). That said, voters have high expectations for politicians (e.g., McAllister 2000), and trust is a crucial component of how voters judge individual MCs (Parker and Davidson 1979). Exposure to information about scandals is likely to diminish voters' assessments of their personal characteristics, job performance, or both (e.g., Doherty, Dowling, and Miller 2011), which should in turn reduce a scandal-tainted MC's margin in subsequent elections (e.g., Welch and Hibbing 1997). Accordingly, we expect scandals that have drawn the attention of a major media outlet to be more damaging in terms of lost votes than those escaping substantial media scrutiny.

That said, donors may respond to a highly visible scandal differently than voters do. Ideological congruence is an important factor in determining whether individuals contribute to a politician (Barber 2016a; Bonica 2014). However, while pure ideologues may contribute to candidates regardless of their chances of winning, for many donors (especially those affiliated with groups) there is also a forward-looking, “investor” mentality as donors support candidates who will advance the contributors' preferred policies (Francia et al. 2003). To this end, donors can display fairly sophisticated behavior. Gimpel, Lee, and Pearson-Merkowitz (2008) argue that out-of-district individual donors—who on average make up more than two-thirds of a MCs donor pool—are also strategic and motivated by partisan power; such donors tend to funnel money into the most competitive districts in hope of influencing the outcome (also

see Hill and Huber 2017 for a similar argument about the importance of partisan and electoral competition). Moreover, Barber, Canes-Wrone, and Thrower (2017) suggest that individual contributors are quite sophisticated with respect to the politicians to whom they contribute, taking both policy agreement and committee assignments into account. And, there is evidence that donors' efforts to secure representation pays off; legislators are more responsive to donors' requests for access (Kalla and Broockman 2014), and there is more ideological congruence between legislators and donors than between legislators and voters (Barber 2016*b*).

If donors are able to extract such gains, it stands to reason that they will work to protect their investment if the continued success of a favored MC is threatened, since a strategic investor might reason that the risk of a less optimal replacement supersedes any negative sentiment about a scandal-tainted MC's conduct. There is reason to believe that donors do respond to exogenous threats. For instance, some previous work on the relationship between campaign funding and electoral outcomes has found that incumbents' spending displays a null (or even slightly negative) relationship with their vote share on average, which suggests that incumbents are more likely to raise and spend more money when they believe themselves to be in danger of losing (e.g., Jacobson 1978, 1980, 1990; Ansolabehere and Gerber 1994). Indeed, congressional incumbents have demonstrated a capacity to raise funds as needed to combat threats to their re-election—such as a strong challenge (Krasno, Green, and Cowden 1994).

A scandal is an obvious threat to a political career. As such, donors should be more likely than voters to see a scandal as a challenge to overcome in order to protect their own interests, rather than an offense that warrants punishment. Rather than providing a reason for a donor to withdraw support, the scandal revelation may actually send a signal that help is needed. Donors may therefore wish to contribute *more* to a scandal-tainted MC out of the desire to protect a known quantity. Ultimately, then, one reason for politicians' ability to exceed expectations in the wake of scandals is that donors step in to aid them in their darkest hour. Below, we therefore engage the question of whether donors attempt to bolster the finances of an accused candidate after a scandal breaks, presumably in an effort to aid a politician who will advance donors' policy goals.

Finally, we believe that the political environment could further shape how voters and

donors react to a scandal's revelation. Punishing a scandal-tainted MC is a straightforward proposition when the political stakes are low. If for instance a voter believes that there is little chance that an election will lead to a change in the party controlling the House majority, then the costs of voting out a co-partisan scandalous member are less acute. While this was a safe proposition with respect to the U.S. House throughout most of the 1980s, the competitive environment changed substantially in the early 1990s as it became clear that the long-held Democratic House majority was not secure, and/or any Republican gains might be short-lived. (Bonica and Cox n.d.; Hopkins 2018; Lee 2016). Bonica and Cox (n.d.) and Lee (2016), in particular, focus on the changing nature of competition for party control of the House during this period. Both argue that following 1994, majority status was always within reach of the minority, which has created a nationalized political climate since every seat could conceivably matter.

We might therefore expect a broader-thinking public since the mid-1990s, with donors, activists, and voters from both parties more apt to focus on winning the majority, rather than electing particular individuals to office. This has clear implications for the public's reaction to scandal. Specifically, voters should be more forgiving of bad behavior that they might punish in a lower-stakes situation. Similarly, donors should be even more responsive to threats in this period, in an effort to protect not just a single seat, but a legislative majority. We might therefore expect voters to punish scandalous MCs *less* post-1994, and donors to contribute even *more* to scandalous MCs after 1994. We test these expectations in the following sections.

Data

To examine the impact of scandals on voters and donors, we use data on U.S. House scandals between 1980 and 2010 from Basinger et al. (2014). We retain Basinger et al. (2014)'s classification scheme that places each scandal in one of four categories: financial, sex, political, and other. In this classification, financial scandals involve allegations of bribery, tax evasion, corruption, or some other usage of public funds for personal gain. Political scandals typically include allegations of professional misconduct, such as misuse of campaign funds or House

resources. Sex scandals include behaviors such as an “extramarital affair, sexual harassment, solicitation, or sodomy” (Basinger et al. 2014). Finally, scandals in the “other” category are not readily classified, but typically include criminal infractions that do not involve financial graft. Most of these scandals occurred after either allegations of drug use or an arrest for driving under the influence.

Next, we coded the “break date” of each scandal. The break date is the earliest instance of any media report describing the scandal that we could find in the *Lexis Nexis* database.² Our measure allows us to capture the time at which the scandal became publicly known, relative to the subsequent election within a given Congress.³ This is an important dimension to our research design, which examines fundraising totals of paired candidates before and after the scandal broke (see below).

We also attempted to capture the national visibility of the scandal, due to our expectation that scandals that are subjected to more intense media scrutiny could shape subsequent behavior differently than less-visible scandals. There are a number of ways in which news coverage could be used to determine the significance of a scandal. For instance, a researcher might attempt to count the number of stories about the scandal in the largest newspaper (or all newspapers) in a congressional district. However, in light of our research questions, we believe this approach is problematic for three reasons. First, considering that the time frame of our data extend to the early 1980s, it can be difficult to ensure that reliable measures of local news coverage are obtained. Second, the media environment can differ considerably between congressional districts. Even the smallest ethics violation might receive coverage in a rural district where there is less competition for local news, while the same scandal might escape close scrutiny in a large, urban district. Third, out-of-district donors could reside thousands of miles from the local newspaper office. Such donors are likely to learn of far-flung scandals via the national media, rather than from perusing far-flung local media outlets. As such, media coverage of a scandal among outlets in the MC’s district is likely not an internally valid measure of its impact on a national donor base.

With that in mind, we coded a scandal as “nationally visible” if at least one story about it appeared in *The New York Times* in the 30 days on or after the break date of the scandal.⁴

We believe that this dichotomous measure of whether a scandal was covered at all provides a clean capture of the theoretically relevant concept. As noted above, national coverage is the most likely means by which out-of-district donors would learn about a scandal, and if it is reported in a national outlet, the scandal is almost certain to be also widely covered in local media. Furthermore, a dichotomous variable indicating *some* coverage mitigates some methodological problems that might result from coding the *number* of such stories, such as the distinct likelihood of diminishing impact after a certain threshold of news coverage. Thus, we believe that the measure we employ is a sensible metric for the visibility of the scandal in the immediate aftermath of its breaking, and should provide us a sense for just how damaging we should expect the scandal to be.

We limit the scope of our analysis to legislators who stood for reelection (in the general election) during the scandal cycle. That is, among the scandal-tainted legislators, we exclude those members who resigned from office following the scandal break, as well as those—both scandalous and non-scandalous—who were defeated by a primary challenger. We do so for a simple reason: these members' general election vote share is unknown, and they clearly would have received fewer campaign contributions following the scandal simply by virtue of either a shortened campaign timeline or representing a pointless investment opportunity for donors.⁵ We also exclude legislators who were serving in their first term in office during the Congress of the scandal.⁶ Finally, in cases where a member was involved in more than one scandal in a cycle (e.g., Rep. David Scott in 2008), we use the first break date and ignore the second. Following this pre-processing, we are left with a dataset of 103 unique scandal-tainted members.

Table 1 shows the distribution of those scandals by type and coverage in *The New York Times*. As is seen, the majority of scandals involve allegations of financial wrongdoing; sixty-five of 103 unique scandals (63%) fall under the financial classification.⁷ The least common scandals are political scandals. In terms of national media coverage, in all categories except political, scandals are more likely to be covered in *The New York Times* than not. While the analyses that follow will emphasize the differential effects of scandal across combinations of type and salience, we recognize the limited number of cases in all categories except financial.

Table 1: Distribution of Scandal by Visibility and Type

	Covered	Non-Covered	<i>Total</i>
Financial	35	30	<i>65</i>
Sex	11	6	<i>17</i>
Political	3	5	<i>8</i>
Other	9	5	<i>14</i>
<i>Total</i>	<i>58</i>	<i>46</i>	<i>104</i>

Members of Congress serve as the unit of analysis in our study. We gathered data on each MC's vote share and campaign contributions. Election data were provided by Gary Jacobson; the outcome of interest is coded as the incumbent candidate's share of the two-party vote in the election immediately following the break date of the scandal. We collected contribution data from Adam Bonica's *Database on Ideology, Money in Politics, and Elections* (DIME; Bonica 2013, 2014), which includes all contributions to candidates for federal office from 1979 to 2012. Importantly, the DIME data include the date of each contribution made to a MC; these data therefore allow us to calculate the sum of campaign contributions before and after news of a given scandal broke for each MC running in that election cycle. This is straightforward for scandal-tainted members. However, because we ultimately pair a non-scandalous MC to each scandal case as a matched comparison (see below), we also calculate the sum of contributions (pre- and post-break) for each unique break date in that particular election cycle for every non-scandal tainted MC running in the same election.

To be clear, in cycles with more than one break date (because of more than one member experiencing a scandal), we calculate the sum of contributions for each break date/non-scandal tainted MC combination, such that the number of times each non-scandalous MC appears in our pre-matched data equals the number of break dates during that particular electoral cycle. Doing so is important for our research design (see below), as it allows for each non-scandal tainted MC in a given cycle to potentially match to a scandal-tainted legislator. The resulting pre-matched dataset includes 26,890 observations. Note, though, this is a pre-analysis step that provides the universe of *possible* combinations for our matching exercise. Below we describe the motivation behind the matching process and the matching process itself, which parses this dataset to one that balances covariates between the group of MCs that experienced a scandal and those that did not.

Research Design: Selection on Observables

Our goals are to examine whether MCs embroiled in scandal fare better or worse electorally than non-scandal tainted members, and also whether they raise more or less money once the

scandal is revealed. One approach to this end would be to compare outcomes for scandalous members to those from the full set of reelection-seeking, non-scandal tainted members. But this approach would be hindered by a substantial challenge: If these two sets of legislators differ in ways that influence the likelihood of scandal, the level of electoral success, or contribution patterns, our estimates might be biased. For example, we might expect legislators embroiled in a scandal to be less experienced than non-scandal tainted legislators. We might also expect non-scandalous MCs to fare better at the polls and to raise more money than scandal-tainted legislators. If both of these are true, then seniority would be predictive of both being involved in a scandal (our treatment) and our two outcomes of interest. In such a case, even if we detected an effect of scandal on campaign contributions we could not be sure that these effects are not instead driven by differential levels of seniority between scandal-tainted and non-scandal tainted legislators.

To avoid this problem, we opt for a selection-on-observables strategy that will first balance covariates between the “treatment” group of scandal-tainted members and the “control” group in which no scandal occurred, and then examine differences between them on the outcomes of interest. In short, our strategy is to pair each scandal-tainted MC to a set of members who were not embroiled in a scandal, but who are otherwise similar on observable covariates, while parsing non-comparable MCs from the data. Our hope is that doing so results in treatment and control groups similar with respect to the distribution of observables, so that any difference in the outcomes between the two groups can be attributed to the presence of a scandal. We implement this strategy with a genetic matching algorithm (e.g., Diamond and Sekhon 2013) that identifies control units similar to the treated units on a number of covariates observed prior to the break date. Genetic matching searches over the data for the set of matches that minimize the difference in the distribution of potential confounders in the treatment and control groups, thereby maximizing the balance between the two groups. Importantly, all matched pairs are made within the two-year election cycle in which a given scandal occurred.

We match on nine pre-treatment covariates. *Vote percentage* is the MC’s share of the two-party vote received in the previous cycle. *DW-NOMINATE* is the MC’s ideological ideal point, where higher values indicate conservatism. *Seniority* is the number of terms served up to and

including the previous Congress. *Legislative effectiveness* is a score indicating the ability of a legislator to advance items through the legislative process and into law in the previous Congress (Volden and Wiseman 2014). *District presidential vote* is the district-level Democratic share of the two-party presidential vote share in the previous presidential election. *Pre-scandal funds raised* is the sum of funds raised during the scandal cycle prior to the “break date,” converted to 2010 dollars and logged (we are able to do this because as noted above, we calculate this total for every control MC-scandal combination). We also exact match on *Democrat*—an indicator variable equal to 1 if the legislator is a Democrat and 0 otherwise—and *female*, an indicator variable equal to 1 if the legislator is female and 0 otherwise.

The final matching variable is the scandal *break date*. We exact-match on break date by assigning each unique scandal break date an ID number reflecting the date during the cycle on which the scandal broke.⁸ For example, the first break date in our dataset was given the ID 1, the second 2, and so on. Because we calculate both the pre-treatment covariate and the outcome variable by member for each break date, we are therefore able to hold constant the timing (within the cycle) of the scandal breaking. This is particularly important for our fundraising outcome, because MCs may raise money continuously throughout an election cycle. Matching on the break date ensures that the pre-post comparison we make between a scandal-tainted legislator and a non-scandal tainted legislator is made at the same point in time, and is confounded by neither the year in which the scandal took place (e.g., 1992 vs. 2008), nor by the timing of the scandal within a particular year (e.g., October in the year prior to the election vs. October in the year of the election). Plainly, exact matching on the break date ensures that we do not compare the amount of money raised by a scandalous politician in the month prior to the election to the amount of money raised by a non-scandalous MC in the six months prior to the election. In total then, we believe that this set of covariates adequately taps into the set of factors that may influence scandal, vote share, and contribution patterns.⁹

We conducted a one-to-three match, where each scandal-tainted member is matched to three non-scandal tainted “control” legislators.¹⁰ We match with a population size of 12,500 (i.e., the number of cases used to solve the optimization problem, which notably is unrelated to the size of the dataset) and with replacement in order to reduce conditional bias in the estimator

(e.g., Abadie and Imbens 2006), breaking ties randomly. Additionally, because our analyses emphasize the importance of media visibility and the interaction between media coverage and scandal type, we ran 17 separate genetic matches—one for all scandals, one for all scandals that received coverage in *The New York Times*, one for all scandals that received no such coverage, one for each combination between scandal type and all scandals, covered scandals, and non-covered scandals, and finally, one match for all scandals pre-1994 and one for all scandals post-1994. In each match, we retained MCs in the treatment condition and their paired controls, and parsed non-matched cases from the data. Each time, our algorithm therefore searched the original dataset (described above) of 26,890 observations, and paired three controls each to the 103 scandal-tainted MCs, leaving 412 cases for analysis.

As noted, the aim of matching is to balance the groups so that they are similar with respect to their observed covariates. We assess balance with p-values from both t-tests and Kolmogorov-Smirnov tests (with 5,000 bootstraps). Tables A1-A15 provide the balance tables for each separate match. These tables show whether the matching process increased the comparability between our scandalous MCs and the non-scandalous MCs. For example, in the match of all scandals, Table A1 suggests that there were statistically significant differences between scandalous and non-scandalous legislators with respect to seniority, legislative effectiveness, and district presidential vote prior to matching: Scandal-tainted MCs were more senior, more effective, and represented more Democratic districts. Following matching, these differences are no longer statistically distinguishable. Across all matches, we were able to either improve balance or maintain existing balance along every single covariate. None of our matches returned covariates that still show statistically distinguishable differences between scandalous MCs and non-scandalous MCs.

Ultimately, what this means is that the analyses that follow are effectively controlling for the relevant factors that we posit should influence the propensity for scandal, vote shares, and contribution patterns. Because there exist no observable differences between scandal-tainted MCs and non-scandal tainted MCs in our matched dataset, it is much less likely that any observed effect of scandal can be attributed to seniority, or legislative effectiveness, for example. In short, we are confident that comparing scandal-tainted MCs to the smaller set

of matched, non-scandal tainted legislators is much less likely to produce biased estimates. Instead, our estimates should more closely approximate *only* the effect of a MC moving from the non-scandal tainted to scandal-tainted condition.

Results: Effect of Scandal by National Media Visibility

In the analyses that follow, we report the average treatment effect on the treated (ATT). In essence, the ATT represents a linear regression (with weights supplied by the genetic matching exercise) employing each matching covariate listed above.¹¹ Table 2 presents the results for each of our outcomes of interest—win/loss, vote share, and post-scandal sum of funds raised (converted to 2010 dollars and logged)—among all scandals, nationally visible scandals, and scandals that escape national media attention. Our results suggest that overall, scandal-tainted legislators fare just over four percentage points worse at the ballot box than do similar non-scandal tainted legislators, and are 11 points less likely to win. Still, as anticipated, these effects are particularly acute during scandals covered in the national media, where legislators can expect an almost eight percentage point drop in vote share. Indeed, while the effect of a non-covered scandal on both the likelihood of victory and vote share is negatively signed, neither coefficient achieves statistical significance. Overall then, our findings that voters punish scandalous politicians findings are consistent with previous studies in direction, magnitude, and substance, but differ in that our results more specifically capture how contextual factors, such as national media visibility, mediate the relationship between scandal and voting behavior: Highly covered scandals depress vote shares, but non-covered scandals do not.

Table 2 also suggests that donors respond much differently to a scandal than voters. Specifically, our results demonstrate that campaign contributors supply members involved in scandal with *more* money than they might have otherwise. Overall, MCs embroiled in a scandal raise about 35% more following the scandal than do members who are not involved in a scandal. Once again though, we find that the results for donors—like voters—are driven by responses to nationally visible scandals. Scandalous MCs embroiled in nationally visible scandals raise almost 60% more than do their colleagues.¹² The effects on fundraising of scandals that es-

cape national media visibility, while positive, do not achieve statistical significance. These results highlight a fundamental tension in how political actors evaluate scandal-tainted politicians: voters lose trust and punish these MCs in the general election, while donors seek to defend their own. And, indeed, the most dramatic effects are found among the most visible scandals.¹³

Table 2: Effect of Scandal on Vote Share and Money by National Media Visibility

	(1) Win			(2) Vote Share			(3) Money		
	All	Covered	Non-Covered	All	Covered	Non-Covered	All	Covered	Non-Covered
Scandal	-0.11** (.04)	-0.16** (.05)	-0.07 (.05)	-4.33** (1.63)	-7.77*** (2.12)	-4.85 (2.88)	.30*** (.10)	.47** (.14)	.27 (.14)
Observations _{TR}	103	58	45	103	58	45	103	58	45
Observations _{CO}	309	174	135	309	174	135	309	174	135

Standard errors in parentheses

*** p<.001; ** p<.01

Results: Effect of Scandal by National Media Visibility and Type

Previous work has found the effects of scandal to be context-dependent; voters tend to evaluate financial scandals (e.g., tax evasion) more negatively than moral scandals (Carlson and Hyde 2000; Doherty, Dowling, and Miller 2011; Funk 1996; but see Brown 2006; Peters and Welch 1980; Welch and Hibbing 1997 for observational studies showing reverse or even null differences). These findings lead us to expect that the propensity for voters/donors to punish/rescue members embroiled in scandal might vary by context, too. Below, we therefore evaluate the relationship between scandal type and both vote share and contributions, while examining how these relationships differ depending on whether the scandal was covered in *The New York Times*.

Tables 3, 4, and 5 present the results for win/loss, vote share, and fundraising, respectively, by scandal type.¹⁴ In short, the pattern for sex and financial scandals is consistent with that of scandals overall: we find evidence that voters punish those involved in *covered* financial scandals and *covered* sex scandals, and that donors contribute more in the wake of such scandals. Notably, we find particularly large effects among sex scandals, a finding that runs in contrast to some existing work (Carlson and Hyde 2000; Doherty, Dowling, and Miller 2011; Funk 1996). Our results suggest that legislators embroiled in a sex scandal covered in *The New York Times* can expect to fare almost eleven percentage points worse than a similar non-scandal tainted MC, and are 25 points more likely to lose reelection. Legislators involved in a covered financial scandal receive about six percentage points less in vote share than do similar non-scandalous MCs. Consistent with our expectations, we detect no effect of scandal on votes among non-covered scandals.

Campaign contributors are also particularly responsive to highly visible sex scandals. Indeed, the same MCs who fare almost eleven percentage points worse at the ballot box raise a staggering 159% more than a similar non-scandal tainted legislator in the aftermath of sex scandals warranting *Times* coverage. MCs embroiled in covered financial scandals also draw the support of donors, to the tune of a 59% increase. We also find suggestive evidence that

donors rescue MCs involved in nationally visible political scandals. We hesitate to draw firm conclusions, however, about political and other scandals, given the small sample size and wide confidence intervals associated with these estimates. Overall though, our results are quite consistent and indicate the importance of national media visibility in mediating the relationship between both scandal and votes and scandal and money.¹⁵

Table 3: Effect of Scandal on Winning by National Media Visibility and Type

	(1) Financial			(2) Sex			(3) Political			(4) Other		
	All	Covered	Non-Covered	All	Covered	Non-Covered	All	Covered	Non-Covered	All	Covered	Non-Covered
Scandal	-.10*	-.15*	-.00	-.25*	-.24	-.33	-	-	-	-.17	-.11	-.33
	(.05)	(.07)	(.07)	(.11)	(.13)	(.19)	(-)	(-)	(-)	(.09)	(.10)	(.21)
Observations _{TR}	65	35	30	17	11	6	-	-	-	14	9	5
Observations _{CO}	195	105	90	51	33	18	-	-	-	42	27	15

Standard errors in parentheses
 *** p<.001; ** p<.01; * p<.05

Table 4: Effect of Scandal on Vote Share by National Media Visibility and Type

	(1) Financial			(2) Sex			(3) Political			(4) Other		
	All	Covered	Non-Covered	All	Covered	Non-Covered	All	Covered	Non-Covered	All	Covered	Non-Covered
Scandal	-3.65 (2.33)	-5.54* (2.60)	-2.43 (3.76)	-10.24*** (2.76)	-10.68*** (3.06)	-6.86 (6.92)	-7.73 (4.46)	-12.68 (6.74)	-6.78 (6.07)	-5.28 (3.15)	-4.73 (5.19)	-10.65* (5.16)
Observations _{TR}	65	35	30	17	11	6	8	3	5	14	9	5
Observations _{CO}	195	105	90	51	33	18	24	9	15	42	27	15

Standard errors in parentheses
 *** p<.001; ** p<.01; * p<.05

Table 5: Effect of Scandal on Money by National Media Visibility and Type

	(1) Financial			(2) Sex			(3) Political			(4) Other		
	All	Covered	Non-Covered	All	Covered	Non-Covered	All	Covered	Non-Covered	All	Covered	Non-Covered
Scandal	.39** (.15)	.47** (.18)	.09 (.19)	.89*** (.22)	.95*** (.24)	.51 (.27)	.27 (.39)	.58* (.24)	-.06 (.36)	.21 (.22)	.01 (.27)	.88* (.34)
Observations _{TR}	65	35	30	17	11	6	8	3	5	14	9	5
Observations _{CO}	195	105	90	51	33	18	24	9	15	42	27	15

Standard errors in parentheses
 *** p<.001; ** p<.01; * p<.05

Results: Nationalization of Electoral Politics

To test the possibility that scandals affected voters and donors differently prior to the present, nationalized period of congressional elections (Bonica and Cox n.d.; Hopkins 2018; Lee 2016), we divided our sample of scandals into two categories: pre-1994 and post-1994. We run two new genetic matches, using the same criteria and covariates identified above. Our results are contained in Table 6. We first consider the effect of scandal on the likelihood of an MC's victory pre-1994 (and including) vs. post-1994. Before 1994, scandal-tainted legislators were 24 points less likely to win re-election, relative to similar non-scandal tainted MCs. Post-1994, however, the effect disappears completely. Likewise, the negative effect of scandal on vote share is larger (in absolute terms) pre-1994 than post-1994. Both of these findings are consistent with expectations.

Donors behave as anticipated, too, and in dramatic fashion. We find that all of the positive effect of scandal on fundraising found above is driven by the post-1994 electoral landscape. Before 1994, donors actually gave *less* to politicians embroiled in scandal—although this effect is notably not statistically distinguishable from zero. In sum, the consequences of scandal for votes and money depends on whether the scandal occurred before or after the rise of competition for majority control that previous work has identified as part of a new, nationally oriented politics (Bonica and Cox n.d.; Lee 2016). Post-1994, political actors of all kinds became increasingly focused on backing their party at all costs—even when a particular politician is embroiled in scandal.

Table 6: Effect of Scandal on Vote Share and Money by Pre- vs. Post-1994

	(1) Win		(2) Vote Share		(3) Money	
	Pre-1994	Post-1994	Pre-1994	Post-1994	Pre-1994	Post-1994
Scandal	-.24*** (.07)	-.02 (.04)	-6.55** (2.46)	-4.32* (2.17)	-.05 (.14)	.28* (.13)
Observations _{TR}	45	58	45	58	45	58
Observations _{CO}	135	174	135	174	135	174

Standard errors in parentheses
 *** p<.001; ** p<.01; * p<.05

Conclusion

Previous work on scandal has focused on the character of the alleged offense, and has evaluated whether certain “kinds” of scandal (e.g., sex and financial) are more consequential than others. We contribute a new facet to scandal classification, including a measure of whether a scandal received coverage in the national press. Our goal was not to explain why certain scandals receive national news coverage, but rather, to advance our understanding of why some scandals are more damaging than others. National coverage is an important element of a scandal’s power; a scandal that receives substantial attention beyond the boundaries of a congressional district is, we believe, qualitatively different than one that national political media deem unworthy of coverage. Simply, national media coverage can serve as a proxy for the damaging potential of a scandal, regardless of its “type.” Thus, such media coverage should be a determinant of subsequent reactions from both voters and donors.

Though we believe the inclusion of media coverage to be an important aspect in our design, ours is certainly not the first analysis to consider how voters react to political scandals. However, we forge new ground in differentiating between the behavior of the voting public and donors. Previous work yields substantial evidence that voters punish scandalous politicians, but has also found that the majority of scandal-tainted politicians who run for re-election are able to sustain their political careers. We posit that one explanation for the durability of scandal-plagued politicians is that donors’ incentives diverge from punitive-minded voters. Donors, many of whom treat their contribution as an investment in a policy and partisan outcome, might see a scandal as an exogenous threat to an ally; as such, rather than see donors abandon a scandal-tainted politician, we might actually expect a spike in contributions following a scandal revelation.

We found strong evidence that donors’ reaction to scandal differs considerably from that of voters. On the whole, politicians embroiled in scandal garner fewer votes than their non-scandalous colleagues, but they also raise *more* money. We find however that the apparent effects of scandals are conditional on how visible they are. Specifically, the effects we observe on donors and voters alike were present only in scandals that drew attention from *The New*

York Times. Overall, this pattern holds for the two most common scandal types that have been identified in previous research: financial scandals and sex scandals. Notably however, further investigation revealed that both voters *and* donors became more protective of scandal-tainted politicians after 1994, which has been identified in recent research as an important moment where national concerns (i.e., winning a majority) supplanted more localized ones (winning a single seat) for many voters.

Our analysis therefore offers three possibilities for why some scandalous politicians are able to win reelection while others do not. First, our findings underscore the limitations of using scandal typologies alone to predict the impact of any given offense committed by a member of Congress. We find that the damaging character of a scandal is heavily dependent on the extent to which it is discussed in the national media. These results suggest that defining a “scandal” by some pre-defined metric (such as an Ethics Committee investigation) might be an overly broad approach if the goal is to define behaviors likely to be damaging, because it cannot answer an important measurement question: How serious is the scandal? Including serious scandals with less-serious ones, including behavior that many people may not find “scandalous” at all, should reduce the apparent effect of scandal overall, which could explain some of the results in existing work. We take a different approach, allowing national media coverage to serve as a proxy for a scandal’s likely significance, and find considerable variation in behaviors following highly visible scandals than those that largely escape national media scrutiny.

These findings suggest a need for more precise measurement in research on scandal, which we believe is a topic ripe for further exploration. We show media coverage to be an important—but understudied—aspect of a scandal. However, our design captures only a binary measure of whether a scandal was covered in a single newspaper (*The New York Times*). As we describe above, we believe that our measure of media visibility is sensible, but future work could develop broader measures of scandal coverage across multiple national outlets. Alternatively, coverage in local media might be considered. Or, perhaps both of these could be included in a single analysis. The majority of scandals in the database compiled by Basinger et al. (2014) were covered at least once in local media outlets. But, scandal researchers might more fully

consider how the *intensity* of coverage (i.e., the number of stories), the *type* of coverage (i.e., the character of content therein), or some other aspect of media coverage that impacts the effect of scandal. Future research should also examine what particular attributes of either a scandal or the broader political environment contribute to its (local or national) coverage. Doing so could advance our understanding of what politician traits, scandal context, or political conditions are likely to result in a damaging congressional scandal. So, more work is clearly warranted on the questions of both what makes a damaging scandal and the media's role in shaping political behavior.

Second, consistent with previous work on contributor motivations, our findings suggest that donors work to mitigate a scandal's damage. Faced with an external threat to their investment, we find evidence that donors come to the aid of a scandal-tainted MC. The money that they contribute in the days and weeks following a scandal revelation can be used to stem the negative tide, which may explain in part why we see so many scandal-tainted incumbents damaged—but not defeated. These results underscore the importance of further exploration not only of donors' role in helping politicians to overcome scandal, but also that of other political elites, such as fellow legislators and party leaders. Future work should extend the analysis of scandal (and similar exogenous threats to individual MCs) to these areas.

Third, our comparison of pre-1994 scandals to those occurring since suggests a need to more fully consider how the nationalization of the political environment since the mid-1990s has affected the dynamics of congressional elections. Our results imply that today's scandals may not be quite so damaging as they were in the past. We believe this is because voters are more likely to see themselves as members of a political "team" that is responsible for protecting its majority—even when doing so requires supporting scandal-tainted politicians. Our findings might therefore reflect a broader strategy among the party faithful to defend their own, without risking the possibility of damaging the party valence brand (and with it, the potential for legislative majorities) in the process. Future research should explore this possibility not only as it relates to scandal, but also to other aspects of political campaigns. Finally, particularly in the more recent period, our results shed light on the potentially deleterious effect of political contributors on election outcomes: donors allow politicians with allegations of misbehavior off

the hook by helping them maintain relative electoral security. More broadly then, our findings suggest the need for continued research that explores the effects of these contributions, if any, on election outcomes.

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Notes

¹This phenomenon is not unique to politicians in the United States. Corrupt politicians in Italy, India, and Japan also win reelection at surprisingly high rates (Fisman and Golden 2017).

²Where no mention of the scandal could be found in *Lexis Nexis*, we broadened the search via Google and/or local media outlets.

³In some cases, the first mention of the scandal came after the general election. We drop these cases from the analyses that follow.

⁴One additional concern might be that politicians from New York might receive coverage even if their circumstances of their scandal would not traditionally warrant such coverage. We replicated our main finding—that scandal reduces vote shares and the probability of victory, and increases funds raised—excluding all legislators from New York, and the results do no change.

⁵Since most members who retired likely judged their scandals as quite damaging, we believe their exclusion means that if anything, our analysis results in a conservative estimate of scandals' impact.

⁶We do so primarily because of missing data. For these members, legislative effectiveness scores are unavailable. Additionally, their previous vote percentage reflects the dynamics surrounding the candidacy of a challenger, not an incumbent. This is a necessary consequence of the matching plan we describe below.

⁷Note: one scandal—Rep. Don Sherwood (R-PA) in 2006—was classified both as a sex and “other” scandal, and thus makes the total number of scandals 104. There are, however, 103 unique scandal-tainted legislators.

⁸Because we assign the break date ID on the basis of the date, and not the member, politicians involved in “group scandals”—such as the House banking scandal—are treated with the same ID.

⁹Still, we recognize that some post-treatment variables—such as challenger quality—are important factors in driving vote share and fundraising. While challenger quality is often determined post-treatment (i.e., a quality challenger emerges only after an incumbent is embroiled in scandal), we are still able to assess balance on this dimension, even if it cannot be included as a covariate in the match itself.

¹⁰This approach, as opposed to a one-to-one match, allows us to increase our sample size while nevertheless striving to maintain strong balance on all dimensions.

¹¹We also replicated each of the below analyses using linear regression, using the complete dataset of scandal-tainted and non-scandal tainted legislators and including each matching covariate as a control variable. Our results are substantively similar. Incumbent politicians experience a greater decrease in their vote share, and raise more money, when the scandal is nationally visible, relative to when the scandal is not visible. We note again, however, that given significant pre-treatment differences between scandal-tainted and non-scandal tainted legislators, our ATT estimates are likely more precise and closer to casually-identified.

¹²These (and all) figures on percentage change in fundraising are calculated by subtracting one from the exponentiated coefficient.

¹³We also broke apart our sample of scandals into groups: scandal-tainted legislators who shared a break date with another scandal-tainted legislator, vs. those who did not. We re-ran the matches as done in the paper (doing so across visibility), and found no statistically significant findings regarding winning, vote shares, and money among group scandals. But we found effects consistent with our main results among solo scandals.

¹⁴Note that for political scandals we were unable to generate a set of matches that gives variation on the win/loss variable.

¹⁵We also created a “media intensity” variable, which is the number of articles in *The New York Times* about the scandal in the 30 days following the break date. This was coded at the politician-level, but for conceptual and analytical reasons, politicians embroiled in a group scandal were later assigned the same number of hits: the largest number within the group. We then added 1 for each politician and logged the variable. We ran an OLS regression on the full dataset, including each matching covariate as a control. The key coefficient is an interaction between scandal and the media intensity variable, which tells us whether the effect of scandal on votes and money depends on the extent of national media visibility.

Consistent with expectations, the interaction term gives a negative effect on votes and a positive effect on money. Neither coefficient, however, is statistically distinguishable from zero.

Supplementary Appendix

Table A1: Balance Statistics—All Scandals

	(1) Before Matching				(2) After Matching			
	Mean _{TR}	Mean _{CO}	p-value	KS p-value	Mean _{TR}	Mean _{CO}	p-value	KS p-value
Vote percentage	72.61	70.53	.14	.09	72.61	71.62	.17	.18
DW-NOMINATE	-.06	-.01	.29	.08	-.06	-.07	.34	.71
Seniority	6.69	5.02	.00	.00	6.69	6.46	.08	.98
Legislative effectiveness	1.77	.96	.01	.01	1.77	1.43	.08	.71
District presidential vote	53.99	50.16	.02	.09	53.99	52.71	.18	.44
Democrat (exact)	.60	.55	.32	NA	.60	.60	1	NA
Female (exact)	.09	.11	.44	NA	.09	.09	1	NA
Pre-scandal funds raised	11.29	11.14	.68	.62	11.29	11.24	.49	.15
Break date (exact)	41.89	43.17	.61	.76	41.89	41.89	1	1
<i>Challenger quality</i>	.14	.14	.86	NA	.14	.12	.77	NA

Table A2: Balance Statistics—Covered Scandals

	(1) Before Matching				(2) After Matching			
	Mean _{TR}	Mean _{CO}	p-value	KS p-value	Mean _{TR}	Mean _{CO}	p-value	KS p-value
Vote percentage	70.81	70.71	.96	.53	70.81	71.09	.83	.29
DW-NOMINATE	-.14	-.01	.04	.00	-.14	-.13	.42	.19
Seniority	6.57	5.08	.01	.00	6.57	6.42	.22	.99
Legislative effectiveness	2.42	.98	.01	.02	2.42	1.87	.19	.67
District presidential vote	57.11	49.87	.00	.05	57.11	55.69	.17	.29
Democrat (exact)	.66	.56	.14	NA	.66	.66	1	NA
Female (exact)	.10	.11	.95	NA	.10	.10	1	NA
Pre-scandal funds raised	11.71	11.44	.52	.93	11.71	11.69	.90	.77
Break date (exact)	39.69	43.09	.35	.31	39.69	39.69	1	1
<i>Challenger quality</i>	.16	.15	.85	NA	.16	.13	.68	NA

Table A3: Balance Statistics—Non-Covered Scandals

	(1) Before Matching				(2) After Matching			
	Mean _{TR}	Mean _{CO}	p-value	KS p-value	Mean _{TR}	Mean _{CO}	p-value	KS p-value
Vote percentage	74.93	70.35	.04	.11	74.93	73.56	.18	.75
DW-NOMINATE	.05	-.01	.39	.71	.05	-.00	.15	.52
Seniority	6.84	4.96	.01	.02	6.84	6.52	.13	.91
Legislative effectiveness	.93	.95	.89	.45	.93	.89	.58	.41
District presidential vote	49.99	50.45	.82	.69	49.99	50.88	.65	.15
Democrat (exact)	.53	.55	.86	NA	.53	.53	1	NA
Female (exact)	.07	.11	.24	NA	.07	.07	1	NA
Pre-scandal funds raised	10.76	10.83	.91	.24	10.76	10.55	.16	.13
Break date (exact)	44.73	43.25	.67	1	44.73	44.73	1	1
<i>Challenger quality</i>	.11	.14	.56	NA	.11	.09	.72	NA

Table A4: Balance Statistics—All Financial Scandals

	(1) Before Matching				(2) After Matching			
	Mean _{TR}	Mean _{CO}	p-value	KS p-value	Mean _{TR}	Mean _{CO}	p-value	KS p-value
Vote percentage	75.57	70.43	.01	.00	75.57	74.65	.31	.18
DW-NOMINATE	-.09	-.00	.08	.06	-.09	-.12	.36	.75
Seniority	7.51	5.18	.00	.00	7.51	7.32	.27	.93
Legislative effectiveness	2.07	.96	.02	.00	2.07	1.65	.23	.74
District presidential vote	56.77	50.19	.00	.04	56.77	55.29	.23	.74
Democrat (exact)	.66	.56	.08	NA	.66	.66	1	NA
Female (exact)	.12	.12	.89	NA	.12	.12	1	NA
Pre-scandal funds raised	11.19	11.22	.96	.97	11.19	11.09	.34	.19
Break date (exact)	45.8	48.51	.42	.39	45.8	45.8	1	1
<i>Challenger quality</i>	.12	.14	.64	NA	.12	.11	.85	NA

Table A5: Balance Statistics—Covered Financial Scandals

	(1) Before Matching				(2) After Matching			
	Mean _{TR}	Mean _{CO}	p-value	KS p-value	Mean _{TR}	Mean _{CO}	p-value	KS p-value
Vote percentage	75	70.71	.09	.04	75	73.99	.42	.13
DW-NOMINATE	-.21	.01	.00	.01	-.21	-.19	.31	.21
Seniority	7.66	5.39	.01	.00	7.66	7.44	.30	.99
Legislative effectiveness	2.97	.97	.02	.01	2.97	1.99	.13	.56
District presidential vote	61.82	51.35	.00	.02	61.82	59.92	.15	.29
Democrat (exact)	.71	.56	.06	NA	.71	.71	1	NA
Female (exact)	.14	.13	.87	NA	.14	.14	1	NA
Pre-scandal funds raised	12.06	12.26	.67	.32	12.06	12.12	.76	.68
Break date (exact)	46.57	57.08	.04	.01	46.57	46.57	1	1
<i>Challenger quality</i>	.14	.15	.87	NA	.14	.13	.90	NA

Table A6: Balance Statistics—Non-Covered Financial Scandals

	(1) Before Matching				(2) After Matching			
	Mean _{TR}	Mean _{CO}	p-value	KS p-value	Mean _{TR}	Mean _{CO}	p-value	KS p-value
Vote percentage	76.23	70.23	.05	.10	76.23	74.71	.22	.40
DW-NOMINATE	.03	-.01	.67	.87	.03	-.03	.17	.19
Seniority	7.33	5.03	.01	.06	7.33	6.94	.19	.96
Legislative effectiveness	1.02	.95	.68	.10	1.02	.97	.49	.61
District presidential vote	50.87	49.36	.53	.35	50.87	51.77	.60	.46
Democrat (exact)	.6	.55	.61	NA	.6	.6	1	NA
Female (exact)	.1	.11	.89	NA	.1	.1	1	NA
Pre-scandal funds raised	10.19	10.47	.75	.31	10.19	10.37	.71	.20
Break date (exact)	44.9	42.38	.58	.99	44.9	44.9	1	1
<i>Challenger quality</i>	.1	.13	.54	NA	.1	.08	.77	NA

Table A7: Balance Statistics—All Sex Scandals

	(1) Before Matching				(2) After Matching			
	Mean _{TR}	Mean _{CO}	p-value	KS p-value	Mean _{TR}	Mean _{CO}	p-value	KS p-value
Vote percentage	67.47	70.81	.25	.55	67.47	66.98	.68	.58
DW-NOMINATE	.25	-.02	.03	.01	.25	.25	.96	.51
Seniority	4.65	4.74	.89	.69	4.65	4.51	.61	.64
Legislative effectiveness	1.55	.98	.29	.70	1.55	1.46	.54	.83
District presidential vote	45.08	50.73	.08	.01	45.08	45.57	.69	.49
Democrat (exact)	.24	.55	.01	NA	.24	.24	1	NA
Female (exact)	.06	.09	.63	NA	.06	.06	1	NA
Pre-scandal funds raised	11.06	10.67	.65	.69	11.06	11.17	.77	.49
Break date (exact)	30.82	32.37	.74	.99	30.82	30.82	1	1
<i>Challenger quality</i>	.24	.14	.39	NA	.24	.16	.42	NA

Table A8: Balance Statistics—Covered Sex Scandals

	(1) Before Matching				(2) After Matching			
	Mean _{TR}	Mean _{CO}	p-value	KS p-value	Mean _{TR}	Mean _{CO}	p-value	KS p-value
Vote percentage	64.36	70.56	.06	.29	64.36	65.21	.41	.89
DW-NOMINATE	.16	-.03	.29	.09	.16	.13	.69	.39
Seniority	3.91	4.65	.42	.80	3.91	4.61	.43	.61
Legislative effectiveness	1.81	.99	.26	.19	1.81	1.60	.43	.79
District presidential vote	48.14	49.88	.69	.29	48.14	48.39	.82	.77
Democrat (exact)	.36	.56	.23	NA	.36	.36	1	NA
Female (exact)	.09	.07	.82	NA	.09	.09	1	NA
Pre-scandal funds raised	10.05	9.89	.90	.86	10.05	10.07	.98	.78
Break date (exact)	24.64	26.24	.78	.99	24.64	24.64	1	1
<i>Challenger quality</i>	.27	.14	.37	NA	.27	.21	.64	NA

Table A9: Balance Statistics—Non-Covered Sex Scandals

	(1) Before Matching				(2) After Matching			
	Mean _{TR}	Mean _{CO}	p-value	KS p-value	Mean _{TR}	Mean _{CO}	p-value	KS p-value
Vote percentage	73.17	71.23	.73	.56	73.17	70	.69	.38
DW-NOMINATE	.41	-.00	.00	.01	.41	.40	.85	.66
Seniority	6	4.89	.34	.32	6	5.83	.74	.98
Legislative effectiveness	1.07	.95	.89	.37	1.07	1.06	.93	.39
District presidential vote	39.48	52.12	.01	.03	39.48	42.65	.45	.41
Democrat (exact)	0	.53	.00	NA	0	0	1	NA
Female (exact)	0	.12	.00	NA	0	0	1	NA
Pre-scandal funds raised	12.91	11.94	.06	.18	12.91	12.74	.44	.4
Break date (exact)	42.17	42.52	.95	.99	42.17	42.17	1	1
<i>Challenger quality</i>	.17	.14	.88	NA	.17	.22	.84	NA

Table A10: Balance Statistics—All Political Scandals

	(1) Before Matching				(2) After Matching			
	Mean _{TR}	Mean _{CO}	p-value	KS p-value	Mean _{TR}	Mean _{CO}	p-value	KS p-value
Vote percentage	70.63	69.85	.80	.29	70.63	70.88	.93	.34
DW-NOMINATE	.03	.00	.92	.20	.03	.06	.38	.21
Seniority	7.38	4.90	.23	.07	7.38	6.88	.41	.92
Legislative effectiveness	.43	.93	.00	.39	.43	.47	.79	.36
District presidential vote	53.01	52.56	.96	.37	53.01	54.06	.64	.59
Democrat (exact)	.38	.53	.44	NA	.38	.38	1	NA
Female (exact)	0	.13	.00	NA	0	0	1	NA
Pre-scandal funds raised	13.28	13.03	.47	.59	13.28	13.19	.72	.39
Break date (exact)	47.5	48.53	.82	.99	47.5	47.5	1	1
<i>Challenger quality</i>	.13	.14	.90	NA	.13	.04	.57	NA

Table A11: Balance Statistics—Covered Political Scandals

	(1) Before Matching				(2) After Matching			
	Mean _{TR}	Mean _{CO}	p-value	KS p-value	Mean _{TR}	Mean _{CO}	p-value	KS p-value
Vote percentage	64.67	69.33	.40	.77	64.67	69.33	.42	.24
DW-NOMINATE	.06	.00	.91	.69	.06	.13	.38	.57
Seniority	7.33	5.07	.12	.05	7.33	7.33	1	.46
Legislative effectiveness	.32	.97	.05	.59	.32	1.33	.38	.55
District presidential vote	48.00	52.05	.81	.18	48.00	48.24	.95	.25
Democrat (exact)	.33	.53	.61	NA	.33	.33	1	NA
Female (exact)	0	.13	.00	NA	0	0	1	NA
Pre-scandal funds raised	13.72	13.10	.38	.18	13.72	13.75	.91	.57
Break date (exact)	45.67	46.79	.89	.89	45.67	45.67	1	1
<i>Challenger quality</i>	.33	.13	.61	NA	.33	.11	.45	NA

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Table A12: Balance Statistics—Non-Covered Political Scandals

	(1) Before Matching				(2) After Matching			
	Mean _{TR}	Mean _{CO}	p-value	KS p-value	Mean _{TR}	Mean _{CO}	p-value	KS p-value
Vote percentage	74.2	70.18	.29	.14	74.2	72.93	.59	.79
DW-NOMINATE	.01	.00	.98	.36	.01	.02	.92	.56
Seniority	7.4	4.79	.45	.74	7.4	6.87	.57	.79
Legislative effectiveness	.49	.91	.02	.58	.49	.43	.75	.29
District presidential vote	56.01	52.89	.78	.53	56.01	57.18	.74	.86
Democrat (exact)	.4	.52	.65	NA	.4	.4	1	NA
Female (exact)	0	.13	.00	NA	0	0	1	NA
Pre-scandal funds raised	13.02	12.99	.94	.87	13.02	13.12	.63	.55
Break date (exact)	48.6	49.61	.88	.98	48.6	48.6	1	1
<i>Challenger quality</i>	0	.15	.00	NA	0	.07	.58	NA

Table A13: Balance Statistics—All Other Scandals

	(1) Before Matching				(2) After Matching			
	Mean _{TR}	Mean _{CO}	p-value	KS p-value	Mean _{TR}	Mean _{CO}	p-value	KS p-value
Vote percentage	67.71	70.89	.41	.29	67.71	67.55	.90	.66
DW-NOMINATE	-.25	-.02	.03	.05	-.25	-.25	.85	.72
Seniority	4.71	4.87	.85	.73	4.71	4.71	1	.71
Legislative effectiveness	1.31	.97	.66	.73	1.31	1.10	.61	.72
District presidential vote	51.52	48.15	.28	.12	51.52	51.49	.98	.89
Democrat (exact)	.86	.56	.01	NA	.86	.86	1	NA
Female (exact)	0	.09	.00	NA	0	0	1	NA
Pre-scandal funds raised	10.93	10.39	.59	.60	10.93	10.86	.62	.88
Break date (exact)	35.21	35.57	.96	1	35.21	35.21	1	1
<i>Challenger quality</i>	.07	.14	.33	NA	.07	.14	.52	NA

Table A14: Balance Statistics—Covered “Other” Scandals

	(1) Before Matching				(2) After Matching			
	Mean _{TR}	Mean _{CO}	p-value	KS p-value	Mean _{TR}	Mean _{CO}	p-value	KS p-value
Vote percentage	64.44	71.29	.08	.22	64.44	65.11	.73	.61
DW-NOMINATE	-.33	-.03	.04	.09	-.33	-.33	.76	.66
Seniority	5.33	4.85	.69	.88	5.33	5.26	.93	.54
Legislative effectiveness	1.76	.97	.52	.36	1.76	1.62	.84	.66
District presidential vote	52.75	45.92	.16	.04	52.75	52.40	.77	.66
Democrat (exact)	.89	.57	.02	NA	.89	.89	1	NA
Female (exact)	0	.08	.00	NA	0	0	1	NA
Pre-scandal funds raised	11.71	10.82	.31	.86	11.71	11.64	.73	.67
Break date (exact)	29.33	29.78	.96	.99	29.33	29.33	1	1
<i>Challenger quality</i>	0	.14	.00	NA	0	.22	.15	NA

Table A15: Balance Statistics—Non-Covered “Other” Scandals

	(1) Before Matching				(2) After Matching			
	Mean _{TR}	Mean _{CO}	p-value	KS p-value	Mean _{TR}	Mean _{CO}	p-value	KS p-value
Vote percentage	73.6	70.16	.70	.61	73.6	73.2	.85	.83
DW-NOMINATE	-.11	.01	.42	.59	-.11	-.11	.81	.85
Seniority	3.6	4.91	.16	.29	3.6	3.53	.94	.40
Legislative effectiveness	.49	.97	.09	.50	.49	.53	.73	.56
District presidential vote	49.29	52.30	.38	.60	49.29	49.52	.89	.56
Democrat (exact)	.8	.54	.27	NA	.8	.8	1	NA
Female (exact)	0	.12	.00	NA	0	0	1	NA
Pre-scandal funds raised	9.54	9.61	.98	.72	9.54	9.44	.74	.84
Break date (exact)	45.8	46.34	.97	.98	45.8	45.8	1	1
<i>Challenger quality</i>	.2	.15	.83	NA	.2	.4	.46	NA

Table A16: Balance Statistics—Pre-1994 Scandals

	(1) Before Matching				(2) After Matching			
	Mean _{TR}	Mean _{CO}	p-value	KS p-value	Mean _{TR}	Mean _{CO}	p-value	KS p-value
Vote percentage	73.73	71.75	.41	.11	73.73	73.26	.66	.66
DW-NOMINATE	-.17	-.07	.13	.01	-.17	-.18	.79	.53
Seniority	6	4.82	.03	.01	6	5.76	.25	.52
Legislative effectiveness	1.89	.99	.06	.07	1.89	1.49	.22	.62
District presidential vote	53.87	45.91	.00	.00	53.87	52.51	.22	.22
Democrat (exact)	.73	.61	.08	NA	.73	.73	1	NA
Female (exact)	.04	.05	.86	NA	.04	.04	1	NA
Pre-scandal funds raised	9.73	9.32	.56	.50	9.73	9.78	.71	.88
Break date (exact)	17.4	16.41	.51	.09	17.4	17.4	1	1
<i>Challenger quality</i>	.18	.14	.47	NA	.18	.14	.63	NA

Table A17: Balance Statistics—Post-1994 Scandals

	(1) Before Matching				(2) After Matching			
	Mean _{TR}	Mean _{CO}	p-value	KS p-value	Mean _{TR}	Mean _{CO}	p-value	KS p-value
Vote percentage	71.74	69.78	.25	.65	71.74	70.98	.39	.49
DW-NOMINATE	.03	.03	.96	.74	.03	.03	.90	.59
Seniority	7.22	5.14	.00	.00	7.22	6.93	.16	.77
Legislative effectiveness	1.68	.95	.09	.08	1.68	1.43	.19	.91
District presidential vote	54.1	52.76	.57	.27	54.1	53.69	.78	.23
Democrat (exact)	.5	.52	.79	NA	.5	.5	1	NA
Female (exact)	.12	.14	.58	NA	.12	.12	1	NA
Pre-scandal funds raised	12.51	12.25	.40	.33	12.51	12.34	.33	.19
Break date (exact)	60.89	59.49	.51	.78	60.89	59.49	1	1
<i>Challenger quality</i>	.10	.15	.30	NA	.10	.08	.65	NA