

Natural Disasters, ‘Partisan Retrospection,’ and U.S. Presidential Elections

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Research investigating whether natural disasters help or hurt politicians’ electoral fortunes has produced conflicting results. Achen and Bartels (2002; 2016) argue that voters perform ‘blind retrospection’ by punishing elected officials indiscriminately in the wake of a natural disaster. Others contend that voters instead perform ‘attentive retrospection’ by incorporating elected officials’ subsequent relief efforts in their assessment. In this paper, we argue that an additional consideration affects voters’ response to natural disasters: the partisan affiliation of an elected official. We contend that whether voters reward or punish incumbents following a disaster is influenced by whether or not the official is a co-partisan. We look for evidence of ‘partisan retrospection’ by examining the effects of Hurricane Sandy on the 2012 presidential election, and find that voters in affected counties that were safely Democratic rewarded incumbent President Barack Obama, while those in affected counties that were safely Republican punished him. We then explore this hypothesis by investigating disasters and presidential elections between 1972 and 2004, using data collected by Gasper and Reeves (2011). We find that voters in disaster-affected counties that were safely in the incumbent party column rewarded candidates of that party, while voters in disaster-affected counties that were safely in the opposing party column punished incumbent party candidates.

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Introduction

In recent years, a number of studies have examined the effect of natural disasters on election outcomes. These studies are part of a broader research agenda on representative democracy, as they provide insight into the logic that underlies voters' reliance on retrospective voting – or their ability to evaluate the actions of elected officials that affect their wellbeing, and reward or punish said officials accordingly in subsequent elections (Key 1966; Fiorina 1981; Healy and Malhotra 2013). Since natural disasters are events that are outside of elected officials' control, voters should not hold them responsible for any resulting negative effects that they experience. If voters do in fact punish elected officials, however, it suggests that they rely on what Achen and Bartels (2016) have called “blind retrospection” – the act of indiscriminately lashing out at politicians in power for pain or discomfort that they experience.

There is no consensus in the literature as to how voters behave following natural disasters. Achen and Bartels (2002; 2016) and Heersink, Peterson, and Jenkins (2017) have identified cases in which voters did punish incumbent presidential candidates after natural disasters. Other scholars have found, however, that voters react as an “attentive electorate” by incorporating how elected officials respond in the wake of disasters. For example, Healy and Malhotra (2009) find no evidence that disaster damage influences presidential candidates' vote share, but do show a relationship between relief spending and support for the party of the incumbent president. Similarly, Healy and Malhotra (2010) and Gasper and Reeves (2011) find that while voters punish incumbent presidents for damage from severe weather, they also reward those presidents who provided aid by signing disaster declarations.

In this paper, we argue that an additional consideration might also affect voters' response to natural disasters: shared partisan affiliation with an elected official. An extensive literature has shown that voters use their own partisanship as a “perceptual screen” through which to interpret

political information (Campbell et al 1960; Bartels 2002; Evans and Andersen 2006; Gaines et al 2007; Jerit and Barabas 2012) and attribute credit and blame for political outcomes (Marsh and Tilly 2009; Tilley and Hobolt 2011; Bisgaard 2015). We test whether voters in areas affected by a natural disaster view events through a partisan lens, and punish or reward based on whether the incumbent president is of their political party. If voters do indeed rely on “partisan retrospection” – that is, if co-partisans reward and contra-partisans punish elected officials for the same extreme weather event – this introduces a new concern about their ability to accurately judge the performance of elected officials through retrospective voting.

We assess whether voters rely on partisan retrospection using two empirical tests, which embody two different approaches common in the study of natural disasters and elections. The first approach is based on showing the direct effect of natural disasters on elections (Achen and Bartels 2002; 2016). We do so by examining the effect of Hurricane Sandy on the 2012 presidential election. Sandy was a major natural disaster that occurred mere days before the election. As a result, the extent to which the federal government could provide real relief to those affected was limited. The second approach accounts for the actions of politicians by studying the impact of both disasters *and* government relief efforts (Healy and Malhotra 2009, 2010; Gasper and Reeves 2011). We do so by investigating cases of severe weather, subsequent relief operations, and their effects on presidential elections between 1972 and 2004, using data collected by Gasper and Reeves (2011).

In both tests, we find strong evidence that the effect of a disaster is conditional on shared partisan affiliation with the president. Voter responses to Hurricane Sandy diverged sharply across partisan lines: in affected counties in which Democratic incumbent Barack Obama enjoyed strong preexisting support, his vote share increased in Sandy’s wake. In affected

counties in which contra-partisans made up a majority of the electorate, Obama's vote share declined. In the broader test, encompassing the 1972-2004 period, we also find heterogeneous treatment effects of disaster damage, conditional on preexisting support for the incumbent party.

These results suggest that voters are not only blindly or attentively retrospective when judging the performance of elected officials following disasters. Rather, they also engage in "partisan retrospection," by using shared partisan ties to evaluate incumbents for outcomes beyond their control. These findings suggest that the democratic accountability mechanism provided by regular elections may be more problematic than previously thought. Voters who share the incumbent's party affiliation appear to incorporate natural disasters and the government's response, and reward politicians for them. Voters from the opposing party, however, respond to the same situations by punishing elected officials.

Natural Disasters, Elections, and 'Partisan Retrospection'

Studies of how natural disasters influence elections contribute to our understanding of retrospective voting. Voters' response to natural disasters is particularly important because disasters are exogenous events: occurrences over which elected officials have no direct influence.¹ For example, while presidents may have some ability to affect the country's economic performance or national security, they cannot prevent a natural disaster from occurring. Thus, if voters behave "blindly" and punish elected officials simply because they were exposed to an extreme weather event, they weaken the accountability mechanism of elections. However, if voters also incorporate the quality and quantity of politicians' post-disaster relief efforts into their voting behavior, they help preserve electoral accountability.

¹ It is debatable to what extent natural disasters are randomly distributed: certainly, some geographic areas are more likely than others to experience earthquakes, hurricanes, droughts, tornados, and other natural disasters. However, politicians do not have direct influence on the specific timing or location of the occurrence of a natural disaster.

To this point, studies of the effects of disasters and relief operations have come to contradictory conclusions. Achen and Bartels (2002, 2016) have established a foundation for blind retrospection, finding that citizens punished incumbent party candidates for (seemingly) random events like shark attacks off the coast of New Jersey in 1916 and droughts and floods across much of the 20th century. Heersink, Peterson, and Jenkins (2017) show that voters in the American South in the wake of the 1927 Mississippi Flood punished Republican presidential candidate Herbert Hoover – who had been personally responsible for administering the Coolidge administration’s post-flood relief operation – by more than 10 percentage points.²

In contrast, other studies find that voters are more sophisticated in their retrospection. That is, voters represent an “attentive electorate” that punishes *or* rewards elected officials based not only on the effects of a disaster but also subsequent relief operations. Healy and Malhotra (2009), for example, find a positive relationship between relief spending and voter support for an incumbent party in presidential elections. Similarly, Healy and Malhotra (2010) and Gasper and Reeves (2011) find that, while voters do punish incumbent presidents for severe weather damage, they also reward them for disaster declarations – and that the reward outweighs the negative effects of the disaster itself. Comparative studies have also found that voters incorporate relief operations in their vote choice, and that in some cases elected officials can actually *benefit* from natural disasters as long as they engage in such relief efforts (Bechtel and Hainmueller 2011; Cole, Healy, and Werker 2012; Gallego 2012).³

² Heersink, Peterson, and Jenkins (2017) rely on one measure in their statistical model to reflect both disaster and relief, because, in the case of the 1927 Mississippi Flood, disaster severity and aid distribution were highly correlated. As a result, the measure of disaster damage includes the effect of both the disaster and subsequent relief efforts.

³ Similarly, Eriksson (2016) argues that Swedish voters punished the incumbent party following Storm Gudrun in 2006, as a result of the government’s poor response in the wake of the disaster.

It is possible, however, that voters rely on an additional heuristic to interpret politicians' response to natural disasters: partisanship. An extensive literature has shown that party identification plays an important role in how citizens interpret (political) information, and determine their vote. On the most basic level, voters are generally consistent in supporting the candidates nominated by their own party (Campbell et al 1960). In addition, voters process information through a partisan lens, which shapes their interpretation of the state of the economy (Bartels 2002; Evans and Andersen 2006; Wlezien, Franklin, and Twigg 1997) and of war casualties (Gaines et al 2007), and affects their ability to remember factual information that is positive or negative about their preferred party (Jerit and Barabas 2012). Voters also appear to adjust how they attribute either credit or blame to elected officials based on their (lack of) shared partisanship (Rudolph 2003; Marsh and Tilly 2010; Tilley and Hobolt 2011; Bisgaard 2015).

While partisanship does not make voters entirely blind to reality – for example, experimental studies suggest that when the evidence is clear enough, partisan respondents are less inclined to reject it (Redlawsk et al 2010; Nyhan and Reifler 2017) – we argue that voters likely incorporate some level of partisanship in attributing credit or blame to elected officials following a natural disaster. Indeed, Malhotra and Kuo (2008), in a survey experiment conducted after Hurricane Katrina, show that respondents – at least to some extent – rely on partisan cues in attributing blame for the government's poor response to the disaster.⁴

If voters do indeed rely on “partisan retrospection” in the wake of natural disasters, this raises new concerns regarding their ability to accurately rely on retrospective voting to assess the performance of incumbent politicians. Certainly, voters can interpret elected officials' actions differently based on their own partisan worldview. For example, it is hardly surprising that

⁴ Note that Malhotra and Kuo (2008) rely on a national sample in this particular example, meaning that most if not all respondents were not directly affected by Hurricane Katrina or the botched relief efforts that followed it.

Democratic and Republican voters might have very different views of the Affordable Care Act (or “Obamacare”). As a result, in the 2012 election, they might have incorporated the Act differently in assessing Barack Obama’s first-term performance: Democrats may have rewarded him for fulfilling a campaign promise, while Republicans may have punished him for passing legislation they found particularly egregious. However, voters’ response to natural disasters – events outside of politicians’ control – should *not* be different. If voters are affected by the same natural disaster, partisanship should not play a role in their response to it.

We test whether voters rely on ‘partisan retrospection’ in response to a natural disaster in two ways. First, we assess a single important case: Hurricane Sandy and its effect on voting in the 2012 presidential election. Second, we conduct a more systematic analysis by examining all severe weather incidents and their effects on presidential elections between 1972 and 2004, using data from Gasper and Reeves (2011).

Hurricane Sandy and the 2012 Presidential Election

Hurricane Sandy – which hit the northeast United States in the fall of 2012 – provides an interesting test case for our partisan retrospection thesis, for three reasons. First, Sandy was a major natural disaster that caused considerable damage in Connecticut, Delaware, New York, Ohio, North Carolina, Maryland, Massachusetts, New Hampshire, New Jersey, Pennsylvania, Rhode Island, Virginia, and West Virginia. Sandy was the deadliest hurricane since Hurricane Agnes in 1972 (Diakakis et al 2015). At least 650,000 houses were damaged or destroyed, and 8.5 million customers lost power. Initial reports of the cost of these damages were close to \$50 billion, though subsequent assessments place it as high as \$68.3 billion (Blake et al 2013; NOAA National Centers for Environmental Information 2017). Second, Sandy hit between October 28 and November 2, days before the November 6 presidential election. Thus, voters in affected

counties were very recently ‘treated’ with this natural disaster, and it is likely to have been an especially salient topic as they went to the polls. Finally, Sandy directly affected the campaign: incumbent president and Democratic nominee Barack Obama oversaw the federal government’s response, while Republican presidential nominee Mitt Romney was forced to cancel his campaign appearances. Notably, the immediate assessment of Obama’s response to Sandy was positive, with even Governor Chris Christie (R-NJ) – a Romney surrogate in the 2012 campaign – publicly praising Obama as “wonderful,” “tremendous,” and “deserving great credit” for his administration’s response (Halperin and Heileman 2013, 455).

We study the impact of Hurricane Sandy on Barack Obama’s electoral performance in affected counties. In total, Sandy caused damage in 280 counties, in locations ranging from North Carolina to New Hampshire, including non-coastal states like West Virginia and Ohio (see Figure 1).⁵ The most severe destruction was concentrated in New Jersey, where early damage estimates were on the order of \$29.4 billion according to Governor Christie.

[Figure 1 about here]

Our outcome variable is President Barack Obama’s share of the two-party vote in 2012, measured at the county level. Our data on disaster damages are drawn from the Spatial Hazard Events and Losses Database (SHELDUS), which provides county-level estimates of crop and property damage for all affected counties. Following other authors in this literature (e.g., Gasper and Reeves 2011), we construct a measure of a county’s disaster damage that is the log of total damage per 10,000 residents. Restricting our attention to damage caused specifically by Sandy, the median *affected* county suffered \$12,745 in damage per 10,000 residents.

⁵ Note that, according to the SHELDUS data that we use, Vermont did not experience monetary damage from Hurricane Sandy. While it is possible Vermont was excluded yet still received damage from Hurricane Sandy, based on data provided by the Federal Emergency Management Agency (FEMA) it appears likely that such damage was either very low or nonexistent. Vermont did not receive a federal disaster declaration after Sandy – while both New York and New Hampshire did – and damage in counties bordering Vermont was relatively low.

Unlike the prevailing literature, our focus is on differential responses to disasters, conditional on pre-existing partisanship. That is, we are interested in the extent to which individuals interpret events through the perceptual screen of partisanship, such that (relatively speaking) Democrats would reward the incumbent Democratic president in the wake of a disaster and Republicans would punish him. To allow for heterogeneous treatment effects as a function of pre-existing partisanship, we interact our damage treatment variable with a measure of support for Obama in 2008, differentiating between safely-Democratic co-partisan counties (where Obama won more than 55 percent of the vote in 2008), swing counties (where Obama’s vote share fell between 45 and 55 percent in 2008), and safely-Republican contra-partisan counties (Obama won less than 45 percent of the vote in 2008).

We denote partisanship in county i with a set of dummy variables, γ^i , distinguishing co-partisan counties γ_{co}^i , swing counties γ_{sw}^i , and contra-partisan counties γ_{contra}^i . By interacting these three dummy variables for partisanship with the “Sandy Damage” treatment variable, we are able to estimate heterogeneous treatment effects of Sandy, depending on the pre-existing partisanship of the county. Where a non-interacted treatment variable averages treatment effects across all treated counties, our approach averages treatment effects within groups of counties – Democratic counties, swing counties, and Republican counties.

Our model takes the form

$$v_{2012}^i = B_0 + B_1\gamma_{sw}^i + B_2\gamma_{contra}^i + B_3(\gamma_{co}^i * T_i) + B_4(\gamma_{sw}^i * T_i) + B_5(\gamma_{contra}^i * T_i) + v_{2008}^i + \omega_i + \mu_i,$$

where v^i represents Barack Obama’s two-party vote share in county i and T_i is a treatment variable denoting the extent of damage caused by Sandy. As noted above, our outcome variable is Obama’s share of the two-party vote in 2012. The variables γ_{sw}^i and γ_{contra}^i – whose effects

are estimated as coefficients B_1 and B_2 – are included as constituent terms of the interaction variables, which are our focus. These variables partially control for preexisting support for Obama, since they are dummy variables that represent swing (45-55% Obama vote share in 2008) and contra-partisan (<45% Obama vote share in 2008), respectively. We control more precisely for preexisting Obama support by including Obama’s 2008 vote share, v_{2008}^i , as a regressor. Finally, we include a vector of additional control variables, ω_i , and a county-specific error term, μ_i .

Our substantive interest is in the coefficients B_3 , B_4 , and B_5 , attached to the three interaction terms of disaster damage and partisanship categories. These coefficients represent three distinct treatment effects – the impact of disaster damage in co-partisan (B_3), swing (B_4) and contra-partisan (B_5) counties. To test our theoretical expectations precisely, we focus on the difference between B_3 and B_5 . Our null and alternative hypotheses are:

$$H_0: B_3 - B_5 \leq 0$$

$$H_a: B_3 - B_5 > 0$$

This statement of the null hypothesis makes two aspects of our argument clear. First, we do not make explicit predictions regarding the direction or magnitude of either B_3 or B_5 in isolation. The former could, indeed, be negative, implying that co-partisans punished the incumbent in the wake of a disaster. Our expectation is merely that the difference between B_3 and B_5 will be positive. This implies that, in counties affected to the same extent by a natural disaster, co-partisan counties will be more generous than contra-partisan counties in terms of vote share for an incumbent candidate.

The results of our analysis of Hurricane Sandy are reported in Table 1. In column 1, we report the results of an aggregate model, without incorporating partisanship, that estimates the

average effect of Sandy damage on 2012 vote share, while controlling for Obama's vote share in 2008. In contrast to extant findings regarding blind retrospection, this initial model does not suggest that voters punished the incumbent, Barack Obama, in response to damage from Hurricane Sandy. As disaster damage increases, we estimate a small increase in Obama vote share, but this estimate is not statistically distinguishable from zero.

[Table 1 about here]

Column 2 provides the first direct test of our hypothesis regarding heterogeneous treatment effects, as we disaggregate the effect of Sandy damage by counties' preexisting levels of partisanship. The estimates for *Co-Partisan* \times *Damage* and *Swing* \times *Damage* are both positive and statistically significant at the 10 percent level. In contrast, the treatment effect in contra-partisan counties, represented by *Contra* \times *Damage*, is negative and statistically significant at the 5 percent level. These results are strikingly different from the aggregate estimates in column 1: while our aggregate (or naïve) treatment effect estimate suggested that Obama neither gained nor lost vote share as a result of Hurricane Sandy damage, our disaggregated results show that Sandy's effect varied sharply across different types of counties. In co-partisan and swing counties, voters reacted to disaster damage by rewarding President Obama; in contra-partisan counties, voters did the exact opposite.

Importantly, the comparisons that we report are *within* types of counties. That is, our estimate of Sandy's impact in Republican counties is a comparison between treated Republican counties and untreated Republican counties. Likewise, our estimate of Sandy's impact in Democratic counties is a comparison between treated Democratic counties and untreated Democratic counties. To test our hypothesis directly, we perform a one-sided Wald test, comparing the coefficients on *Co-Partisan* \times *Damage* (B_3) and *Contra* \times *Damage* (B_5).

Consistent with the null hypothesis described above, which is one-sided, we test whether B_3 is greater than B_5 . We report the p-value for this test at the bottom of Table 1 as H_0 P-Value. As shown, the difference in treatment effects between co- and contra-partisan counties is statistically significant at $p = .003$.⁶

To check the robustness of our results, we perform several additional tests of Hurricane Sandy's impact on vote share in 2012. In column 3 of Table 1, we incorporate additional county-specific, time-varying control variables that may explain Obama's electoral performance in 2012. Given the depth of the 2007-8 financial crisis and its spatial variation, local economic conditions are likely to shape vote choice. We control for this possibility by including measures of county-level economic change over the previous year, from 2011 to 2012: changes in mean per capita income, changes in the unemployment rate, and changes in mean home prices.⁷ In column 4 we broaden the set of control variables; we include a binary variable indicating whether counties received a disaster declaration in response to Sandy-related damage, and a measure of total disaster damage over the two years prior to the election.⁸ In both models our primary results remain unchanged: voters in co-partisan counties tended to reward Obama in the wake of Hurricane Sandy, while voters in contra-partisan counties punished him severely, with significant differences in treatment effects across the two types of counties.

⁶ Our expectations are slightly less clear regarding the difference between Republican (contra-partisan) counties and swing counties. In the models in Table 1, the difference between Republican and swing counties is also significant; given the similarity in coefficients between swing and Democratic counties, the difference in treatment effects between these two groups is not significant. These results are not included in the table for the sake of clarity.

⁷ Data on county-level home prices were obtained from Zillow, a real estate research firm. We used data from November 2011 and November 2012 to calculate the change in home prices. Home price estimates are either county-specific, where Zillow has such data, or are state-level estimates attributed to individual counties. Estimates of per capita income and unemployment rate were calculated from Internal Revenue Service data at the county level. Further details on variable construction are provided in the Supplemental Appendix.

⁸ Data on declarations in response to Hurricane Sandy were gathered directly from the FEMA website; data on all disaster damage at the county level over the prior two years was captured from the SHELDUS database.

To illustrate the differential effects of Hurricane Sandy by partisanship, Figure 2 plots the effect estimates and 95 percent confidence intervals for each interaction term from column 2 of Table 1. As Figure 2 shows, contra-partisan (Republican) counties reacted much differently from co-partisan (Democratic) or swing counties, appearing to punish Obama for the effects of the disaster. In the Supplemental Appendix, we show that our results are not driven by our modeling choices. For instance, we use different numbers of lagged previous elections to categorize counties as co-partisan, swing, and contra-partisan; we split counties into five – rather than three – partisanship categories; we incorporate a continuous measure of partisanship and interact it with Sandy-related damage; and we use a binary treatment variable for Sandy-affected counties. In each of these alternative specifications, our primary results remain unchanged: co-partisan counties affected by Sandy rewarded Obama, while contra-partisan counties punished him.

[Figure 2 about here]

Disasters and Presidential Elections, 1972-2004

Our analysis to this point takes advantage of a case with rich data on election outcomes and disaster damage. However, in any single case analysis – no matter how substantively interesting or important – voter reactions may have been idiosyncratic, limiting our ability to generalize. To address these concerns, we provide a more systematic test of our hypothesis by replicating and extending Gasper and Reeves’ (2011) study of attentive retrospection. Gasper and Reeves examine the impact of severe weather damage on incumbent party vote share in presidential elections between 1972 and 2004. Using this data, we estimate models that allow disaster damage to have heterogeneous effects on vote share, depending on the preexisting

partisanship of an affected county.⁹ Unlike our results regarding Hurricane Sandy, this analysis covers a time period that includes seven different presidents and a wide variety of natural disasters.

As in the Hurricane Sandy analysis, our hypothesis concerns heterogeneity in treatment effects. Our hypothesis does not necessarily imply that voters will reward or punish incumbents in the wake of a disaster in the aggregate. Rather, we contend that the extent to which voters will reward or punish an incumbent is conditional on whether the incumbent is a co-partisan. This implies that our interest is in the *difference* in treatment effects between co-partisan and contra-partisan counties.

To start, we replicate Gasper and Reeves' county-level model of presidential vote share. The outcome variable in their study, and our replication, is two-party vote share for the incumbent party in county i in time j . Our treatment variable is disaster damage. Similar to our case study of Hurricane Sandy – and like Gasper and Reeves' own operationalization – we define disaster damage as the logged dollar value of damages per 10,000 residents in the six months prior to an election.¹⁰ Following Gasper and Reeves, we include a county's median income and incumbent vote share in the two most recent presidential elections, and we also include county- and year-fixed effects.¹¹

Our primary interest is in the heterogeneous treatment effect of disaster damage. Compared to Gasper and Reeves, we are less interested in the impact of relief efforts provided by presidents. Gasper and Reeves incorporate the visible efforts of presidents to respond to disaster

⁹ The Gasper and Reeves dataset was obtained from the Harvard Dataverse. We thank the authors for making their replication data readily available online. See Gasper and Reeves (2011) for the relevant details.

¹⁰ The sole difference between this operationalization and that adopted in our analysis of Hurricane Sandy is that the former covers the six months prior to an election, while the latter included only damage specifically attributed to Hurricane Sandy.

¹¹ We utilized the Gasper and Reeves' data on median income and vote share in our primary replication models, though we note areas where we extend or diverge from their models below.

damage by including binary variables to indicate whether a president declared a federal disaster in a county in the previous six months, and whether the president turned down a governor's request for a disaster declaration in the same time period. To replicate Gasper and Reeves' full model, we also include these variables. However, we treat them largely as controls, and focus instead on the heterogeneous impact of disaster damage itself.

To fix ideas, the basic model we employ takes the form

$$v_{ij} = B_0 + B_1\gamma_{sw}^{ij} + B_2\gamma_{contra}^{ij} + B_3(\gamma_{co}^{ij} * T_{ij}) + B_4(\gamma_{sw}^{ij} * T_{ij}) + B_5(\gamma_{contra}^{ij} * T_{ij}) + \omega_{ij} + \theta_i + \delta_j + \mu_{ij},$$

where v_{ij} is vote share for the incumbent party in presidential elections in county i in year j .

Mimicking our analysis of Hurricane Sandy, coefficients B_3 , B_4 , and B_5 represent the treatment effect of disaster damage in co-partisan, swing, and contra-partisan counties, respectively. ω_i is a vector of control variables, including lagged values of incumbent vote share for the previous two presidential elections, disaster declarations, and presidential turndowns of declaration requests. θ_i is a vector of county fixed effects and δ_j is a vector of year fixed effects.

As in our analysis of Hurricane Sandy, our hypothesis concerns heterogeneity in the treatment effect of disaster damage. Our hypothesis is that co-partisan counties will reward an incumbent politician more – or punish him less – than contra-partisan counties. We are agnostic regarding the direction of the aggregate effect of disaster damage, and speculate that the overall effect could vary from disaster to disaster. Our interest is in the direction and size of the difference in treatment effects across types of counties. If B_3 represents the treatment effect of damage among co-partisan counties and B_5 represents the treatment effect of damage among contra-partisan counties, our null and alternative hypotheses are:

$$H_0: B_3 - B_5 \leq 0$$

$$H_d: B_3 - B_5 > 0$$

Again, our argument does not make predictions regarding the direction or magnitude of either B_3 or B_5 in isolation. Our theoretical expectation is that the difference between B_3 and B_5 will be positive.

The results of our replication efforts are shown in Table 2, columns 1 and 3, respectively. These models, and their results, mirror precisely those presented by Gasper and Reeves (2011, p. 352). Building on these models, we group counties into three levels of preexisting support for the incumbent party. This distinction between counties allows us to estimate the impact of disaster damages separately for groups that have different partisan affiliations with the incumbent president. Based on average vote share over the previous three presidential elections, we categorize counties as “co-partisan” with the incumbent (>55%), swing (45-55%) and “contra-partisan” with the incumbent (<45%). We interact these indicator variables with our treatment variable – disaster damage – to allow the impact of disaster damage to vary across levels of pre-existing pro-incumbent partisanship. To ensure that other specification choices do not affect our results, we leave all other aspects of Gasper and Reeves’ models intact, including county-fixed effects, year-fixed effects, and lagged values of incumbent vote share. We simply supplement the model with binary indicators of pre-treatment vote share and interaction terms that are necessary to estimate heterogeneous treatment effects. The interaction terms we use capture the impact of disaster damage among different types of counties.¹²

¹² In practice, we may be concerned about multicollinearity between the lagged values of the dependent variable included by Gasper and Reeves, and the binary indicators of pre-treatment vote share that we add to the model. This is a concern, since the lagged values used by Gasper and Reeves are the basis for our calculation of preexisting incumbent support. Importantly, this is not a criticism of Gasper and Reeves’ empirical strategy, but an admission that the empirical formulation is perhaps not as well-suited to the slightly different hypothesis that we wish to test. We report these models because they are the most faithful to Gasper and Reeves’ original approach, and they highlight the fact that our findings are not driven by our specification choices. In additional analyses, we estimate models that diverge more clearly from a pure replication, and which are arguably better suited to testing our specific

[Table 2 about here]

We extend the replication exercise in Table 2 to test the partisan retrospection hypothesis directly in columns 2 and 4. The results strongly support the partisan retrospection hypothesis. While Gasper and Reeves find small negative effects of disaster damage on vote share in the aggregate, we estimate sizable *increases* in vote share among the incumbent’s co-partisans, and substantively large *decreases* in vote share among the incumbent’s opponents.¹³ More importantly, the difference between effects – reported as H_0 P-Value at the bottom of Table 2 – is statistically significant at $p < 0.001$.¹⁴ The magnitude of treatment effect heterogeneity is illustrated starkly in the left panel of Figure 3, which plots treatment effect estimates and confidence intervals for each of the three groups in our main model (Table 2, column 4).

[Figure 3 about here]

Consider the difference in results between column 1 and column 2. In column 1, Gasper and Reeves estimate a negative overall effect of disaster damage on incumbent vote share, with a substantive effect size that could be considered moderate. In column 2, we divide that aggregate effect of disaster damage into three distinct treatment effects, one for each “type” of county in the data – strong co-partisans of the incumbent, strong contra-partisans of the incumbent, and counties with moderate levels of previous support for the incumbent party. Our estimates are not just different across these three groups, they are also substantively larger than those of Gasper and Reeves. A disaster in a pro-incumbent area can yield large gains in vote share for the

hypotheses. Our primary conclusions regarding partisan retrospection do not change. Results are available upon request.

¹³ Our analysis focuses on both “true incumbents” (i.e. George H.W. Bush in 1992 and Bill Clinton in 1996) and presidential candidates of the incumbent party (i.e. George H.W. Bush in 1988 and Al Gore in 2000). When restricting the sample to true incumbents only, we find that both co- and contra-partisan counties punished incumbents for disaster damage (see Table SA11 in the Supplemental Appendix). However, contra-partisan counties punished the incumbent to a far greater degree than co-partisan counties, and a Wald test comparing the two estimates continues to confirm our hypothesis.

¹⁴ This is the result of a one-sided Wald test of the equality of the two coefficients.

incumbent, while a disaster in an anti-incumbent area can generate sharp and overwhelming punishment by voters.¹⁵

The results presented thus far rely on at least two coding decisions that could be considered arbitrary. The most important is the decision of how to classify counties based on partisanship. We chose to use three broad categories, because grouping counties into just three categories – based on cutpoints at 45 and 55 percent of the two-party vote share – is a typical approach in the literature (Kriner and Reeves 2015; Lowande, Jenkins, and Clarke 2017). However, aggregation on this scale might miss nuances between co-partisan counties that are still relatively competitive, at 55 percent of the two-party vote share, and co-partisan counties that are incumbent strongholds (at, for instance, 75 percent vote share).

To ensure that our results are not driven by our chosen categorization scheme, we tried a number of alternative approaches. To investigate the possibility of more varied effects across levels of partisanship, we split counties into ten quantiles based on partisanship over the previous three elections. To illustrate, the first quantile captures the lowest level of prior support for an incumbent: all counties in this group averaged less than 36.6 percent support for the incumbent over the three previous elections.¹⁶ The tenth quantile captures the highest level of prior support for the incumbent, with counties in which the incumbent's party averaged at least 66.5 percent of the two-party vote share over the prior three elections. As in our main specifications in Table 2,

¹⁵ These results are broadly consistent with those of Heersink, Peterson, and Jenkins (2017), which show a large decrease in support for the 1928 Republican presidential candidate in counties affected by a catastrophic flood of the Mississippi River. While Heersink, Peterson, and Jenkins interpret this finding as support for blind retrospection, it is also consistent with partisan retrospection, since most of the affected counties were strongly opposed to the incumbent Republican Party. One advantage of a research design based on replicating Gasper and Reeves' findings is that it covers a much broader range of counties and allows for distinctions to be drawn between counties with high levels of co-partisan and contra-partisan affiliations, respectively, with the incumbent administration.

¹⁶ 36.6 percent of the two-party vote share, averaged over the prior three elections, is the cutoff for inclusion in this group. The actual average incumbent vote share among this group is 28.2 percent.

we interact these ten indicator variables with our treatment variable, monetary damage from disasters. Our interest is in treatment effect heterogeneity across quantiles.

The primary results of this approach are shown in the right panel of Figure 3. For ease of presentation, we provide the full regression results in the Supplemental Appendix. Figure 3 plots treatment effect estimates and 95 percent confidence intervals for each of the ten groups of counties. The top quantile (Q10) has the highest level of preexisting support for the incumbent's party, and the bottom quantile (Q1) has the opposite, i.e. is strongly contra-partisan. As expected, the differences across counties are dramatic, with co-partisan counties rewarding the incumbent generously and contra-partisan counties punishing him. Notably, the fact that treatment effects increase from negative to positive in a broadly monotonic manner as the level of co-partisanship increases provides additional confidence that these results are not contingent on our chosen binning strategy.

In addition to models with three or ten categories, we also employed five categories, with no substantive change in results. Beyond the categorization of counties, we also estimated models using different numbers of lags to sort counties into partisan categories, models without county fixed effects, and models after splitting our sample into cases with Republican and Democratic incumbents. Full results for all models are provided in the Supplemental Appendix. In the vast majority of the models, our primary results hold, with co-partisan counties reacting more generously to the incumbent than contra-partisan counties in the wake of a disaster.¹⁷

Beyond the formal hypothesis test whose results we reported in Table 2, our primary results are also substantively significant. In the average election year in our sample, nearly half

¹⁷ Most notably, our findings appear to be specific to Republican incumbents; when restricting the sample to Democratic incumbents, we no longer observe a difference in treatment effects between co- and contra-partisan counties. However, these results should be interpreted with caution, because our sample includes just three elections in which Democrats were the incumbent presidential party (1980, 1996 and 2000).

of all US counties experience some level of disaster damage in our data. Among these disaster-affected counties, the median level of damage suffered in the six months before an election is \$27,917 per 10,000 residents (equivalent to 10.237 in terms of our logged treatment variable). Taking the estimates from Table 2, column 4, this implies significant divergence in electoral effects across co-partisan and contra-partisan counties. In the median disaster-affected county that is co-partisan, our estimates suggest that the incumbent would gain 0.30 points of the two-party vote share, a statistically significant finding, but one that would not be meaningful in most elections. If the median disaster-affected county were contra-partisan, however, the incumbent would lose 1.03 points of the two-party vote share.

[Figure 4 about here]

The varying substantive effect of disaster damage on vote share is illustrated in Figure 4, which plots the estimated aggregate effect of different levels of disaster damage, depending on the co-partisanship of the affected counties. As Figure 4 shows, even at moderate levels of damage, the difference between co-partisan and contra-partisan counties are sufficiently stark that they produce substantively meaningful differences in treatment effects. Of course, these figures alone underestimate the true potential for disasters to reshape electoral outcomes, because they ignore the potential for a single disaster to affect many counties. Given the scale of damage caused by some natural disasters, these estimates suggest the possibility of large effects on electoral outcomes from a single widespread disaster, a finding that is consistent with the results presented in the Hurricane Sandy case study.

Conclusion

In this paper we estimate whether voters in areas affected by a natural disaster punish or reward incumbent presidents (or incumbent presidential parties) based on whether they share a

common party identification. To measure whether voters rely on ‘partisan retrospection,’ we conducted two separate analyses: first, we determined the effects of Hurricane Sandy on the 2012 presidential election, and second, we pursued a more systematic analysis by investigating cases of severe weather and their effects on presidential elections between 1972 and 2004, using data collected by Gasper and Reeves (2011).

Our results strongly suggest that voters do rely on partisanship as a ‘perceptual screen’ to interpret elected officials’ response to natural disasters. In the case of Hurricane Sandy – a major natural disaster that occurred days before the 2012 presidential election – the reactions of voters in counties that were affected by Sandy differed dramatically, depending on the preexisting partisanship of the county. In co-partisan (Democratic) and swing counties, voters rewarded incumbent President Barack Obama, while voters in contra-partisan (Republican) counties punished him. Consider the median Sandy-affected county, in terms of total disaster damage: if such a county were Democratic, our estimates suggest that Obama would have gained an additional 0.5 percentage points of the vote as a result of Hurricane Sandy. In contrast, if the county were Republican, Obama’s share of the vote would have fallen by 0.72 points instead.

Similarly, in our analysis of the Gasper and Reeves’ data on presidential disaster declarations in the wake of severe weather conditions, we find that in counties that were safely in the incumbent party’s column – co-partisan counties – candidates of that party were rewarded in the wake of a natural disaster. In swing counties, candidates of the incumbent party experienced no discernible impact on their electoral fortunes. In contrast, counties that were safely in the opposition party column punished incumbent party candidates severely.

These results are relevant both to the study of the effects of natural disasters on election outcomes and the broader question of whether voters can accurately rely on retrospective voting

in elections. Regarding the former, our findings indicate that while voters do appear to incorporate the combination of disaster and relief in their vote choices, and that on average they do reward ‘good behavior’ by elected officials, partisanship is also a crucial predictor in this regard. That is, we find strong evidence that voters reward or punish incumbent party candidates after a natural disaster based on the underlying partisanship in the county.

With respect to retrospective voting in general, our results suggest that voters do face considerable difficulty in accurately relying on their past experiences to determine their vote in the next election. Since voters’ partisan identification is at least partly a product of their own ideological beliefs, it is unsurprising that voters frequently respond differently to the same actions of an elected official. However, because elected officials cannot directly prevent natural disasters from occurring and, on average, affect Democratic and Republican voters similarly, partisanship should not be a significant predictor of how voters respond to them. Our conclusion that voters do rely on partisanship to assess elected officials in the wake of natural disasters thus raises new questions about their ability to incorporate past events in their future votes.

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Table 1: Impact of Hurricane Sandy on Obama Vote Share

	(1)	(2)	(3)	(4)
Damage	0.017 (0.02)			
Co-Partisan * Damage		0.060 ⁺ (0.03)	0.060 ⁺ (0.03)	0.053 (0.03)
Swing * Damage		0.065 ⁺ (0.03)	0.062 ⁺ (0.03)	0.057 ⁺ (0.03)
Contra * Damage		-0.068* (0.03)	-0.069* (0.03)	-0.076* (0.03)
Swing		-1.746* (0.21)	-1.751* (0.22)	-1.764* (0.22)
Contra		-1.472* (0.28)	-1.517* (0.29)	-1.519* (0.29)
Obama Vote Share, 2008	1.047* (0.00)	1.019* (0.01)	1.017* (0.01)	1.017* (0.01)
Income Change, 1 Year			-1.392* (0.52)	-1.324* (0.52)
Unemp. Change, 1 Year			0.000 (0.00)	0.000 (0.00)
Home Price Change, 2 Years			-0.335 (1.14)	-0.465 (1.14)
Disaster Damage, 2 Years				0.024* (0.01)
Declarations, 2 Years				0.059 (0.08)
Intercept	-5.021* (0.17)	-2.555* (0.50)	-2.381* (0.51)	-2.670* (0.52)
H₀ P-Value		.003*	.003*	.003*
N	3113	3113	3078	3078
r ²	0.960	0.961	0.960	0.960

Note: Dependent variable is Obama vote share in the 2012 presidential election, at the county level. Coefficient estimates are reported in the table, with standard errors in parentheses.

+ p < 0.10, * p < 0.05

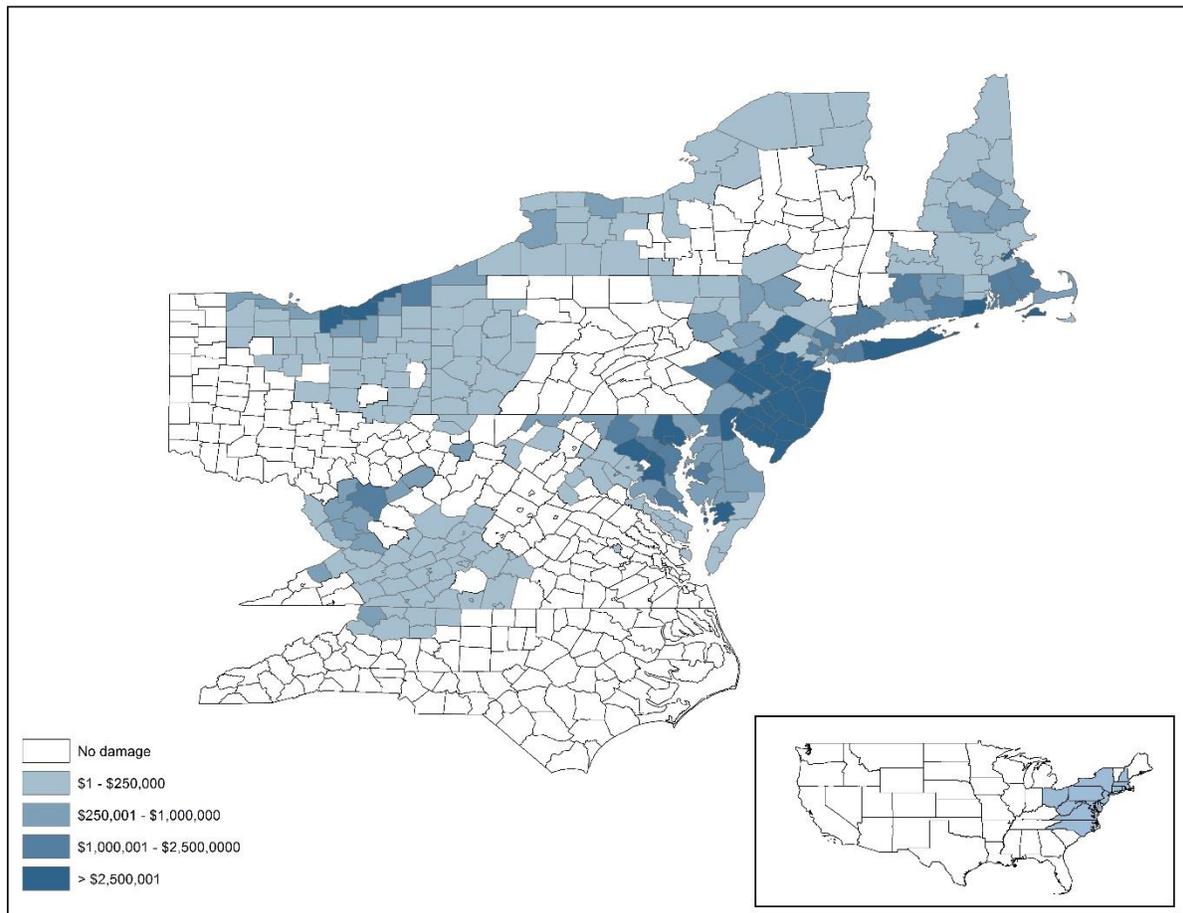
Table 2: Impact of Disaster Damage on Incumbent Vote Share, 1972-2004

	G&R Model 1	Ext. 1	G&R Model 3	Ext. 3
Disaster Damage	-0.017*		-0.025*	
	(0.01)		(0.01)	
Co-Partisan * Damage		0.037*		0.029*
		(0.01)		(0.01)
Swing * Damage		-0.013		-0.022
		(0.01)		(0.01)
Contra * Damage		-0.092*		-0.101*
		(0.01)		(0.01)
Incumbent Vote Share, L1	0.554*	0.546*	0.552*	0.545*
	(0.00)	(0.01)	(0.00)	(0.01)
Incumbent Vote Share, L2	0.342*	0.339*	0.344*	0.342*
	(0.00)	(0.00)	(0.00)	(0.00)
Median Income	0.015	0.015	0.020 ⁺	0.020 ⁺
	(0.01)	(0.01)	(0.01)	(0.01)
Swing		0.717*		0.754*
		(0.16)		(0.16)
Contra		0.227		0.295
		(0.21)		(0.21)
Declarations, 6 Months			0.518*	0.475*
			(0.12)	(0.12)
Turndowns, 6 Months			-0.940*	-0.952*
			(0.09)	(0.09)
Intercept	25.428*	25.897*	25.360*	25.714*
	(2.19)	(2.21)	(2.18)	(2.20)
H₀ P-Value		<.001*		<.001*
N	29746	29746	29746	29746
r ²	0.815	0.815	0.816	0.816

Note: Dependent variable is incumbent vote share in county i in time t . All models include county and year fixed effects. Coefficient estimates are reported in the table, with standard errors in parentheses; associated p-values are for two-sided tests. P-values associated with the Wald test of H_0 are one-sided.

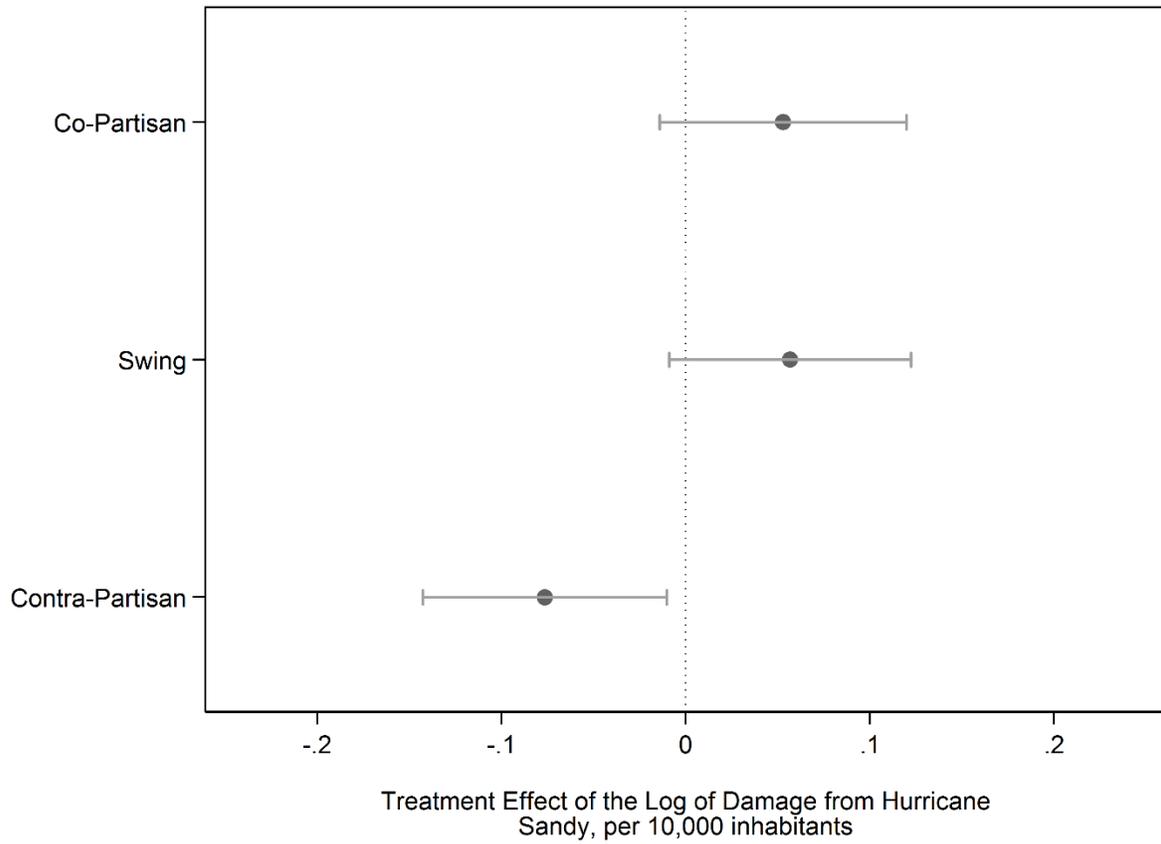
+ $p < 0.10$, * $p < 0.05$

Figure 1: Reported Property Damage Related to Hurricane Sandy



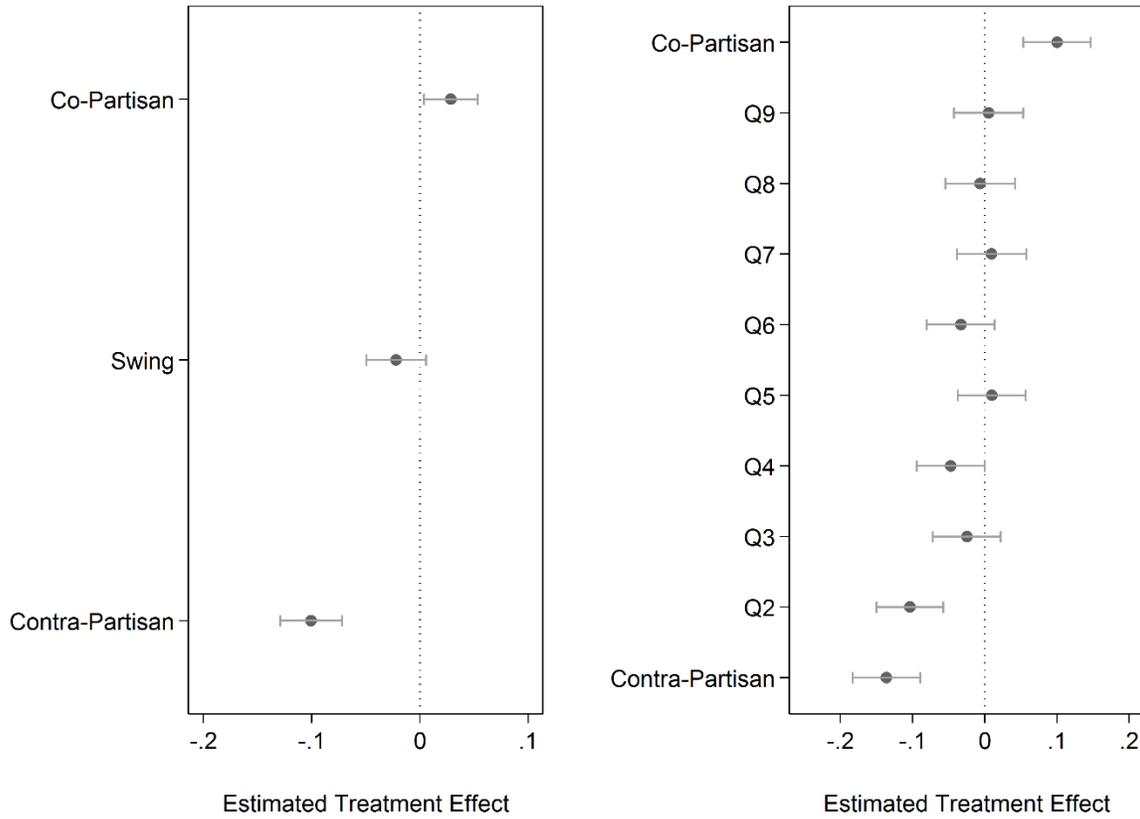
Source: Spatial Hazard Events and Losses Database (SHELDUS).

Figure 2: Treatment Effect of Disaster Damage from Hurricane Sandy, by Level of Preexisting Partisanship



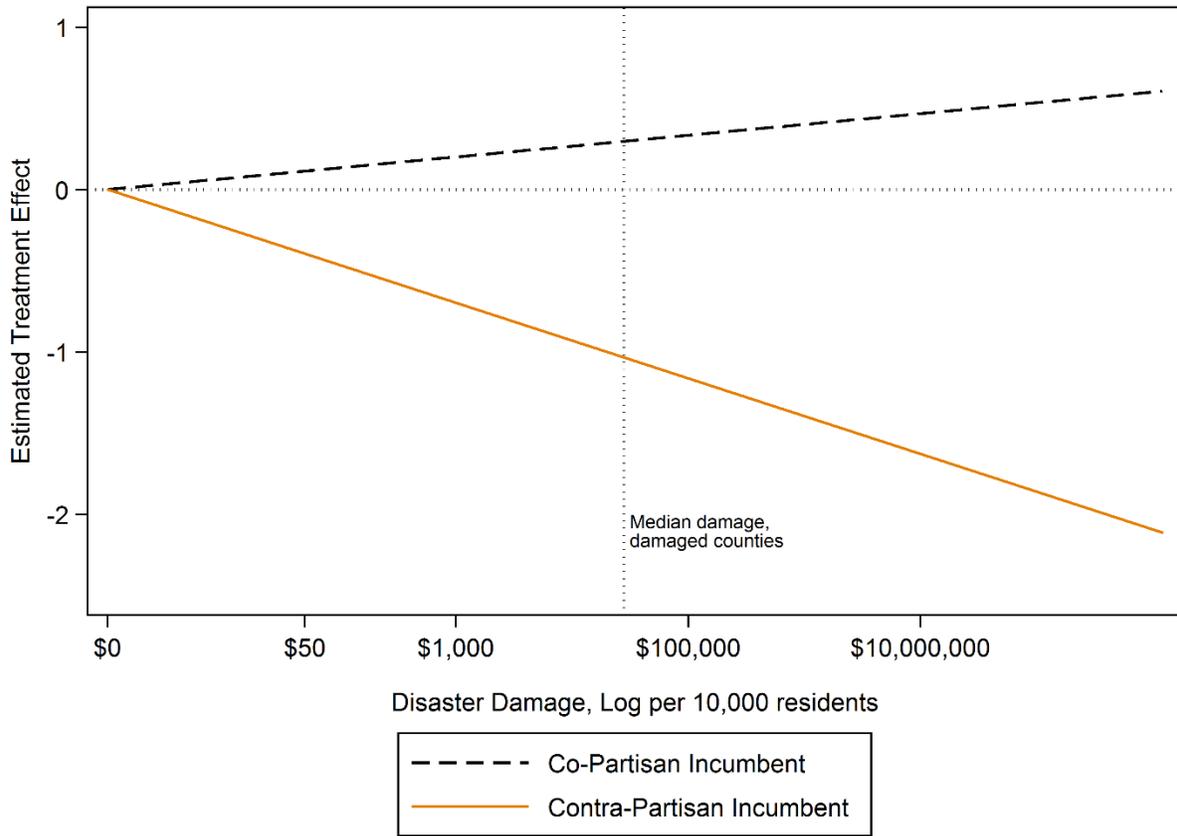
Note: Point estimates are presented, with 95 percent confidence intervals. Results are drawn from Table 1, Column 4.

Figure 3: The Treatment Effect of Disaster Damage (Logged, Per 10,000 Residents) on Incumbent Vote Share, by Preexisting Partisanship



Note: Point estimates, with 95 percent confidence intervals. Left Panel: Three partisanship categories – co-partisan (>55 percent vote share), swing (45-55 percent vote share), and contra-partisan (<45 percent vote share), defined over the previous three elections. Right Panel: Ten partisanship categories – contra-partisan to co-partisan – defined by deciles of incumbent vote share over the previous three elections.

Figure 4: The Substantive Effect of Varying Levels of Disaster Damage, by Co-Partisanship of Counties Affected



Note: The horizontal dotted line represents a null effect. The vertical dotted line represents the median of total damage, among county-years that experienced non-zero damage in the data. Effect estimates at varying levels of damage were calculated based on the results in Table 2, Column 4.