

# DO POLITICAL PROTESTS MATTER? EVIDENCE FROM THE TEA PARTY MOVEMENT\*

ANDREAS MADESTAM  
DANIEL SHOAG  
STAN VEUGER  
DAVID YANAGIZAWA-DROTT

Can protests cause political change, or are they merely symptoms of underlying shifts in policy preferences? We address this question by studying the Tea Party movement in the United States, which rose to prominence through coordinated rallies across the country on Tax Day, April 15, 2009. We exploit variation in rainfall on the day of these rallies as an exogenous source of variation in attendance. We show that good weather at this initial, coordinating event had significant consequences for the subsequent local strength of the movement, increased public support for Tea Party positions, and led to more Republican votes in the 2010 midterm elections. Policy making was also affected, as incumbents responded to large protests in their district by voting more conservatively in Congress. Our estimates suggest significant multiplier effects: an additional protester increased the number of Republican votes by a factor well above 1. Together our results show that protests can build political movements that ultimately affect policy making and that they do so by influencing political views rather than solely through the revelation of existing political preferences. *JEL* Code: D72.

## I. INTRODUCTION

How does political change come about? Although freedom of speech and assembly are central pillars of democracy, recognized as intrinsically valuable, it is unclear how effective the exercise of these freedoms is in bringing about change. Though there are numerous historical episodes in which political change has been

\*We are grateful to Edward Glaeser, Andrew Hall, Lawrence Katz, Nathan Nunn, Rohini Pande, Jeremy Stein, Justin Wolfers, and three anonymous referees, as well as seminar participants at the AEA/ASSA Annual Meeting, the American Enterprise Institute, Americans for Tax Reform, BI Norwegian Business School, Bocconi University, the Brookings Institution, the Federal Reserve Board of Governors, the Graduate Institute Geneva, the Harvard Kennedy School, the Harvard Departments of Economics and Government, the Heritage Foundation, MIT, and the Stockholm Institute of Transition Economics for valuable comments. Emma Bennett, Annalise Blum, and Itai Nixon provided excellent research assistance. We also thank Devin Burghart for providing us with Tea Party membership and rally attendance data.

© The Author(s) 2013. Published by Oxford University Press, on behalf of President and Fellows of Harvard College. All rights reserved. For Permissions, please email: journals.permissions@oup.com

*The Quarterly Journal of Economics* (2013), 1633–1685. doi:10.1093/qje/qjt021.  
Advance Access publication on August 11, 2013.

associated with political demonstrations, such as the French Revolution, the civil rights movement, and the recent Arab Spring, it is unclear to what extent these protests *caused* change. Protests are likely to occur alongside other changes in society, and it is difficult to disentangle whether they cause political change or simply reflect unobservable changes in preferences and beliefs. Empirical evidence of the causal effects of protests is scarce. In fact, to our knowledge, there is almost no empirical work quantifying the causal effects of protests on subsequent political outcomes. It is an open question as to what extent political protests can cause political change, and this article sheds light on these issues.

Specifically, we investigate the impact of the Tea Party movement protests in the United States on policy making and citizen political behavior. The Tea Party movement is a conservative-libertarian political movement in the United States that has organized protests and supported candidates for elected office since 2009. This setting is a well-suited testing ground for hypotheses regarding the effectiveness of political protests and one of the few such settings for which extensive data are available. The movement propagates an agenda that is systematically to the right of the status quo, which makes the measurement of policy changes in the direction desired by the movement straightforward. In addition, the largest protests in the early stage of the movement were the nationwide 2009 Tax Day Rallies. Because this date was preset, it allows us to test whether the size of local protests on Tax Day affected subsequent local political outcomes.

The main empirical challenge in estimating the effect of protests is that unobservable political preferences are likely to determine both the number of protesters and policy outcomes. A naive regression of policy on protest size is therefore unlikely to reflect a causal effect. We address this problem by exploiting variation in rainfall during the day of the protest. The idea is simple: people are more prone to participate in protests if it does not rain. Conditional on the likelihood of rain, rainfall is a random event, arguably uncorrelated with other factors that affect political outcomes. Under the assumption that absence of rainfall affects policy and voting behavior only through the number of protesters, this allows us to estimate the effect of protest size using an instrumental variables approach. Even when relaxing this assumption, our estimates demonstrate the overall importance of these initial events to the movement's success.

We use data from a large number of sources to measure the influence of the Tax Day protests on the Tea Party. The importance of the initial protests to local movement strength is evident in outcomes as diverse as participation in Tea Party online social networks, political action committee contributions, the number of protesters at subsequent protests, and survey measures of local political beliefs. We show that these political protests and the movements they build affect policy making and voting behavior. Incumbent representatives vote more conservatively following large protests in their district, and a rain-free rally in a district increases the likelihood that a Democratic incumbent retires. Larger protests increase turnout in the 2010 elections, primarily favoring Republican candidates. In particular, our baseline estimate shows that a 0.1 percentage point increase in the share of the population protesting corresponds to a 1.9 percentage point increase in the share of Republican votes. The Tea Party protests thus seem to have caused a shift to the right in terms of policy making, both directly and through the selection of politicians in elections.

In addition to providing exogenous variation in rally outcomes, variation in rainfall can be used to assess the statistical significance of these results. We compare the effect of rainfall on the true date of the rally to the distribution of placebo estimates of rainfall on other days. We find that none of the placebo dates in 1980–2008 produce a cumulative effect as large as the effect estimated for Tax Day 2009. This finding, when combined with numerous additional robustness checks, demonstrates the reliability and significance of the results.

Our results relate to the large body of empirical and theoretical work that has attempted to explain which factors drive political participation. Most empirical work on why people vote has identified simple correlations between political activism and citizen characteristics (see, e.g., Blaise 2000 for a review). Papers that inform us about the the determinants of protest participation include Cicchetti et al. (1971), Finkel and Opp (1991), and Finkel and Muller (1998), but there is little research on the causal effect of political rallies. An exception is Collins and Margo's (2004, 2007) work on the effects of the riots following the assassination of Martin Luther King Jr. on income, labor, and housing market outcomes for African Americans. Similar in spirit to this article, they exploit rain during the month of April 1968 as an instrument for riot severity. Madestam and Yanagizawa-Drott's (2011) use of

daily rainfall to generate variation in outdoor participation on July 4 to study the effect of celebrating Independence Day is another example of such an approach.

Theoretical work has generally suggested that a sense of civic duty or consumption value drives political involvement (Downs 1957; Riker and Ordeshook 1968; Coate and Conlin 2004; Feddersen and Sandroni 2006). Political theorists rationalizing why people protest offer explanations based on the importance of peer pressure within smaller political groups (McCarthy and Zald 1977; Uhlaner 1989; Oberschall 1994), people's (unrealistic) perception that they can be politically influential (Opp 1989), and bandwagon effects (Kuran 1989). However, these results leave unanswered the question of why protests would matter as instruments for political change.

One attempt to answer this question focuses on social dynamics within groups and networks of citizens, and their influence on individuals' desire to attain certain political goals (Zuckerman 2005). Another influential strand of papers, written by Lohmann (1993, 1994a, 1994b), emphasizes the role of information.<sup>1</sup> Lohmann (1993, 1994a) models the role of visible political activism in revealing private information to the public at large and to policy makers, and in signaling the costs and benefits of participating (1994b). We provide evidence suggesting that this mechanism is unlikely to fully explain our results. First, it is unclear why weather-driven variation in protest size should provide a signal about underlying beliefs or preferences, if weather on the protest day is orthogonal to beliefs and preferences. Second, even if policy responds to protest size because it provides information about beliefs or preferences, differences across districts with and without rainfall on the protest day should decrease as additional information arrives. We find no evidence of the effects on incumbent behavior decreasing over time. Our results are therefore difficult to reconcile with Lohmann's framework.

Instead, because the effects are very much local, they suggest that personal interaction within small groups of citizens serves as a crucial channel for the transmission of new political views and that leads to increases in political activism, in line with Zuckerman's (2005) "social logic of politics" and the shaping of a

1. See also Bueno de Mesquita (2010) for an information model in which a revolutionary vanguard engages in public violence to mobilize protesters.

new social context that motivates citizens to “call folk, hustle, [and] outwork [their] foe” (Texans for John Cornyn 2008). In our discussion we argue that Lohmann’s information-driven model of the effectiveness of political activism cannot fully explain our results and that social networks, mobilization, and/or habit formation are key missing elements that must be incorporated into a full model of political protests. This argument is broadly consistent with the qualitative evidence presented by Skocpol and Williamson (2011). In their study of the Tea Party movement, based on interviews with activists and an analysis of their (online) activity, they emphasize the role rallies played in shaping the movement: “From interviews and tracking local Tea Parties in public sources, we have learned that these groups were often launched by sets of organizers who did not know one another personally before they met in rallies or other protest settings” (Skocpol and Williamson 2011: 93). These local groups then helped sustain the momentum of the movement through regular meetings and grassroots organizing, often but not always facilitated by individual members’ previous experience in other mediating institutions (Skocpol and Williamson 2011: 37–44), which could be seen as analogous to the value of preexisting institutions to the civil rights movement in its heyday (McAdam 1985). We argue that through this mechanism, and not solely through the revelation of privately held, preexisting policy views, initial rally turnout affected political and policy outcomes for the rest of the election cycle. Personal interaction is, after all, a highly effective campaign instrument (Green and Gerber 2008).

The remainder of the article is structured as follows. In Section II we provide background information on the Tea Party movement. Section III contains a discussion of the data we use. In Section IV we present the estimation framework and in Section V our empirical results. Section VI assesses the robustness of the analysis. In Section VII we discuss and interpret our findings before we conclude.

## II. THE TEA PARTY MOVEMENT

### *II.A. Tea Party Goals and Organization*

The 1773 Boston Tea Party has been a potent symbol for American antitax activists over the past few decades, and its

iconic value has regularly been exploited for protests and fundraisers (e.g., Holmes 1991; Levenson 2007). More recently, starting in early 2009, a broader political movement has coalesced under the Tea Party banner (McGrath 2010). The movement's supporters have come together in a loose coalition of national umbrella organizations that vary in their degree of centralization and ideological focus. Though the movement is unified by opposition to the Democrat-dominated federal government and mostly supports Republican candidates for office, it is not explicitly partisan. That said, there is broad consensus that the emanations of their endorsements and exaltations constitute a penumbra desire to shift policy "to the right," in an across-the-board conservative direction (see, e.g., Skocpol and Williamson 2011).

### *II.B. Tea Party Activism*

The outbreak of the 2008 financial crisis triggered a substantial policy response from both the outgoing Bush administration and the incoming Obama administration. The Emergency Economic Stabilization Act of 2008 and the American Recovery and Reinvestment Act of 2009, in particular, involved extensive public resources in an effort to stabilize the U.S. economy, but also mobilized resistance on the political right. When CNBC Business News editor Rick Santelli delivered a televised and rapidly popularized attack on the U.S. government's plans to refinance mortgages and called for a "Chicago Tea Party," he inspired several relatively small local protests in February 2009. As a broader protest movement started to take shape in the form of online and real-life "Tea Party" groups, plans for larger coordinated protests culminated in the first large national showing of activism on April 15, 2009 (Tax Day), when the groups held a large number of rallies across the United States.

There were approximately 440,000–810,000 individuals protesting nationwide on Tax Day 2009 (for data sources, see Section III). Figure I shows that more than 500 rallies took place. These rallies can be seen as the true starting point of the national Tea Party movement, as evidenced, for example, by the fact that it was the moment when most of the Tea Party activists interviewed in Skocpol and Williamson (2011) "got involved for the first time." Data from Google Insights over the period 2007–11 on the intensity of web searches of the term "Tea Party" support this as well. These data are presented in Figure II, and show that such web

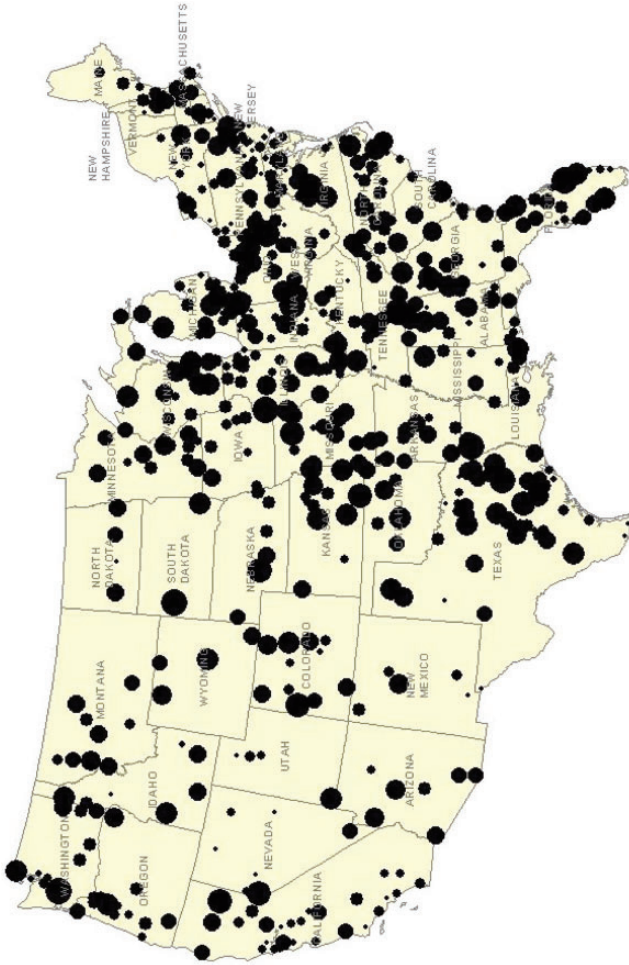


FIGURE I

Tea Party Rallies on April 15, 2009.

Total number of counties with rallies is 542, with a mean attendance of 815 and a standard deviation of 1,506. The size of the circles reflect the share of the population turning out to protest.



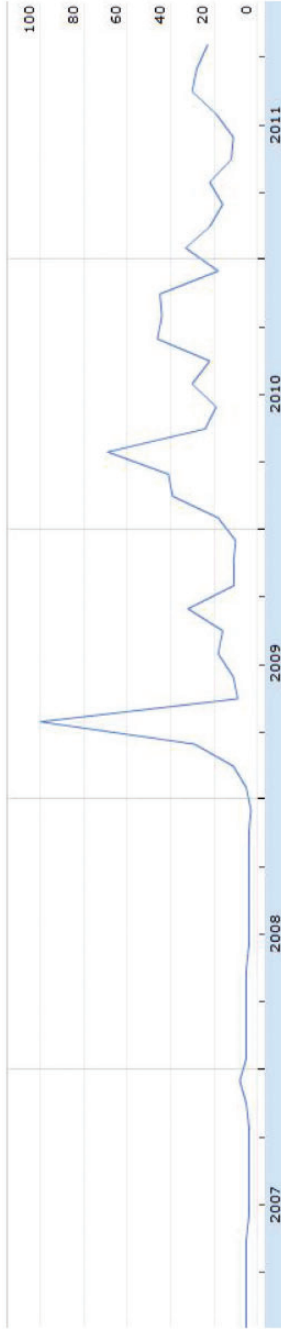


FIGURE II  
Web Search Level of Interest

This graph shows the evolution of the number of Google web searches for “Tea Party,” excluding “Boston Tea Party” searches, normalized by total web search volume and indexed to a peak search level of 100.



searches became much more common around the time of the rallies in April 2009. In this article, we study the effect of these rallies on subsequent movement strength, political beliefs in the population, and political outcomes, both in elections and in the legislature.

After the initial protests, much of the organizational effort of the different Tea Party groups shifted focus away from public protests to fund-raising and the construction of a more localized social-movement infrastructure (Skocpol and Williamson 2011), and to direct engagement with the institutionalized political process. In the remainder of our article, we study the significance of the 2009 Tax Day rallies to the effectiveness of these efforts in the year and a half that followed, up to the 2010 midterm elections. On the organizational side, we analyze the recruitment of volunteers by local Tea Party groups in different locales, turnout at 2010 Tax Day rallies, the fund-raising prowess of Tea Party Express' Political Action Committee, Our Party Deserves Better PAC, and changes in local political preferences. On the more directly policy-focused side, we assess the consequences of the 2009 rallies for local politicians' decisions to retire, election results, and the votes cast by incumbent members of the House of Representatives.

### III. DATA AND SUMMARY STATISTICS

To construct our dataset we extract information from a number of sources in order to collect data on rainfall, Tax Day rally attendance, Tea Party activism, media coverage, political beliefs, voting outcomes, and policymaking. The following subsections present these sources and how they are matched. Details on the data and how the variables are constructed can be found in the Data Appendix in the Online Appendix (see the supplementary material online).

#### *III.A. Rainfall Data*

Information on precipitation comes from the National Oceanic and Atmospheric Administration and contains data from approximately 12,000 weather stations over the period 1980–2010. We construct our rainfall measure by aggregating the weather station data to the appropriate geographic level (county or congressional district) and then extract the mean daily rainfall (in inches). In our baseline measure, days with

rainfall below 0.10 inch count as nonrainy; higher precipitation levels are defined as rainy. Based on historical weather observations for the period 1980–2008, we also create a measure of the probability that a county or congressional district experiences at least 0.10 inch of rain on a given day in April (see Section IV.A.).

### *III.B. Rally Attendance, Movement, and PAC Contributions Data*

We collect attendance estimates for the Tax Day rallies held on April 15, 2009 from three different sources: Tea Party self-reports (SurgeUSA.org 2009), the *New York Times* (Silver 2009), and the Institute for Research and Education on Human Rights (IREHR 2010), a think tank in Kansas City. Figure I depicts a map of the 542 rallies in our data set. In the analysis, turnout is aggregated by county. Because the sources sometimes differ in the number of attendees reported, we use the mean across all three as well as the maximum. Although the mean is a reasonable approximation if the measurement error is classical, our estimates could be biased if discrepancies in the reports lead to nonclassical measurement error.<sup>2</sup> If less-attended rallies occur in counties with small populations, and these events are more likely to be neglected, the measurement error would be correlated with population size. Our data show that the likelihood that one source fails to report a rally in a county when the other two do report declines in the county population. In addition, the within-county cross-source variance in turnout is decreasing in population size (see Online Appendix Figure A.1 for these discrepancies in reported rally size). To alleviate the concern of systematic misreporting, we present estimates using both mean and maximum attendance across the three sources. We also report population-weighted per capita estimates to account for any population-driven variance. Overall, approximately 440,000–810,000 individuals protested nationwide on Tax Day 2009.

To measure local Tea Party activism, we use data from IREHR on the number of social network profiles posted on the websites of the five main Tea Party nonprofit organizations and from Federal Election Commission campaign finance reports on

2. A simple OLS regression of election outcomes on rally size results in attenuation bias if there is classical measurement error. In Table A.17, we show that the OLS estimates are indeed smaller in magnitude than the instrumental variable estimates.

donations to Tea Party Express. Our data set includes the total number of profiles of the following factions: Tea Party Patriots, Americans for Prosperity, and FreedomWorks, as well as two smaller organizations, 1776 Tea Party and ResistNet. These groups maintain their own social networking sites, with minimal privacy protections, allowing the IREHR to collect data on a daily basis since 2010. The “members” included are typically the leadership of local chapters. Though Tea Party affiliation is largely unofficial, these online profiles, much like donations to Tea Party Express, serve as reasonable proxies for the number of activists involved in local Tea Party organizing. The total number of profiles posted on these sites nationwide was approximately 150,000 in 2010. In addition to the membership measures we also gauge local Tea Party activism by including attendance data for 2010 Tax Day rallies from EconomyPolitics (2010).

Information on financial contributions in 2009 and 2010 to Our Country Deserves Better PAC, the fund-raising wing of the Tea Party Express, was obtained from the Federal Election Commission campaign finance reports. We aggregate individual donation information to the county level. The advantage of using this particular PAC is that it has no ties to a particular officeholder or region, and federal campaign finance legislation limits individual contributions to \$5,000 per annum. It therefore provides a reasonable measure of grassroots support of the national Tea Party movement.

### *III.C. Media Coverage*

To measure local media coverage of the protests, we use news articles from the NewsLibrary database matched to Audit Bureau of Circulations county-level circulation data. Newslibrary.com archives over 4,000 titles, but not those of large national newspapers such as the *Wall Street Journal* or the *New York Times*. We collect information on all articles from newspapers with circulation over 15,000 containing the phrase “Tea Party” from January 1, 2009, through June 20, 2010, and merge these data to geographic regions using the county-level circulation information, ending up with 255 publications across 46 states. To decide whether the area covered by a newspaper was rainy or not on a given day, we use the circulation-weighted amount of precipitation. Over the full time period these publications contain some 40,000 articles including the term “Tea Party.”

### *III.D. Political Beliefs*

To study whether the rallies affected public support for the Tea Party movement as well as political beliefs more broadly, we use the Evaluations of Government and Society Study from the American National Election Studies (ANES). Interviews were conducted in October 2010, weeks before the midterm election, and included a set of questions about the Tea Party and political opinions associated with the movement's leaders. The data also contain sociodemographic variables, voting behavior in the 2008 election, and each respondent's reported likelihood of voting in the 2010 midterm election. We have information for a total of 42 states at the congressional-district level that we match with rainfall, census, and survey data.<sup>3</sup>

### *III.E. Voting, Policy Making, and Demographic Data*

Our political outcomes include election results in the 2010 midterm elections for the House of Representatives, the decisions of incumbent congressmen to retire prior to the 2010 midterms, and congressional voting behavior. To control for past electoral outcomes we use county and congressional-district level data for the 2006 and 2008 House of Representatives elections and the 2008 presidential election. The election data come from David Leip's Atlas of U.S. Presidential Elections. Information on incumbent congressmen's decisions whether to seek reelection was obtained from Wikipedia, whereas the ideological bent of congressional voting records is measured using yearly roll-call ratings from the American Conservative Union. Finally, sociodemographic county and district-level data (income, population, race, immigrants, and unemployment) come from the 2000 and 2010 Census and the 2009 American Community Survey.

Table I presents summary statistics for our county-level pre-rally variables.<sup>4</sup> It shows that the counties that were plagued by rain are fairly similar in terms of past voting behavior, past donations to Our Country Deserves Better PAC, population, racial composition of the population, and unemployment compared to the rainless counties. The district-level analogue,

3. The ANES lack county identifiers, barring an analysis at the county level.

4. There are 2,758 counties in our sample. We lose some counties because of a combination of lack of data on precipitation, demographics, and election outcomes.

TABLE I  
SUMMARY STATISTICS BY COUNTY AND RAINFALL

	(1)	(2)	(3)
	Rain	No Rain	Difference
<i>Weather April 15, 2009</i>			
Precipitation (hundredths of inches)	0.386 (0.021)	0.008 (0.002)	0.379*** (0.021)
Probability of rain	0.248 (0.015)	0.199 (0.014)	0.049*** (0.018)
<i>Election 2008</i>			
Republican House vote (% of votes)	50.368 (2.993)	51.829 (2.916)	-1.461 (3.965)
Republican House votes (% of population)	21.996 (1.479)	22.406 (1.144)	-0.410 (1.704)
Votes for Obama (% of votes)	42.766 (1.460)	40.848 (1.684)	1.917 (1.996)
Democratic House votes (% of population)	20.189 (1.218)	19.713 (1.613)	0.477 (1.889)
Total House votes (% of population)	42.970 (1.135)	43.180 (1.282)	-0.210 (1.508)
<i>Election 2006</i>			
Republican House vote (% of votes)	51.953 (2.207)	51.697 (2.047)	0.255 (2.916)
Republican House votes (% of population)	16.226 (1.237)	16.146 (0.823)	0.081 (1.302)
Democratic House votes (% of population)	13.716 (0.716)	14.778 (1.271)	-1.062 (1.343)
Total votes (% of population)	30.519 (1.461)	31.595 (1.643)	-1.075 (1.865)
<i>Tea Party movement</i>			
Tea Party Express donations pre-Tax Day 2009 ('000)	0.026 (0.011)	0.018 (0.006)	0.007 (0.012)
<i>Demographic controls 2009</i>			
Median household income	43,477 (1,648)	42,544 (811)	933.064 (1,686)
Unemployment Rate (%)	9.819 (0.512)	8.820 (0.467)	1.000* (0.571)
Population	114,816 (21,885)	94,164 (17,646)	19,652 (26,501)
Rural population (%)	57.061 (3.325)	60.286 (1.797)	-3.225 (3.571)
White population (%)	85.750 (2.789)	87.347 (1.867)	-1.597 (2.927)
African American population (%)	10.699 (2.713)	8.105 (1.904)	2.594 (2.828)
Immigrant population (%)	3.899 (0.709)	4.367 (0.698)	-0.468 (0.933)
Hispanic population (%)	4.873 (0.898)	9.495 (2.873)	-4.623 (2.937)
Number of observations	588	2,170	

*Notes.* The unit of analysis is a county. It is defined as rainy if there was significant rain in the county (at least 0.1 inch) on the rally day (April 15, 2009). The variables and the data sources are described in Section III, Section IV.A, and the Online Appendix. Robust standard errors in parentheses, clustered at the state level. Column (3) reports \*\*\* 1%, \*\* 5%, \* 10% significance.

Online Appendix Table A.1, displays a similar picture.<sup>5</sup> Tables A.3a, A.3b, and A.3c contain descriptive statistics at the county, district, and individual respondent level for the precipitation measures and our outcome variables.

#### IV. EMPIRICAL FRAMEWORK

The main challenge in measuring the effectiveness of political protests is that unobserved political beliefs or a culture of activism are likely to be correlated with both the number of protesters and other political behavior, such as voting.

How, then, do we assess the effect of larger rally attendance? We investigate the Tea Party rallies held on April 15, 2009, but to estimate their effects we cannot simply assume that the variation in turnout is orthogonal to future developments in the same local area. Instead, we rely on an approach that exploits the fact, established shortly, that people are less likely to attend a rally if it rains. This allows us to estimate the causal impact of variation in rally attendance if we are willing to assume that rainfall on the rally day only affects the outcomes of interest through rally attendance. This exclusion restriction seems plausible, though a valid concern is that bad weather may also make a rally less pleasant for actual attendees, energizing attendees and the consequent movement less. We would then be measuring the effect of a combination of rally size and rally effect per attendee as determined by the likelihood of new social ties forming, among other things. A similar concern is that weather directly, rather than through the number of attendees, affects the likelihood that mass media cover the protests. If there is such a direct effect and media coverage of political protests affects voting behavior and policy making, the exclusion restriction would again be violated. With these potential caveats in mind, we nevertheless believe it is useful to scale the effects of weather by rally attendance to get a quantitative measure of the importance of the Tea Party protests. We also present reduced-form effects of protest day rainfall for all outcomes, where the exclusion restriction is not a necessary identifying assumption for our interpretations. These

5. Online Appendix Table A.2 reports the county and district voting outcomes in levels. Again, there is no significant difference across rainy and nonrainy counties or districts.

results demonstrate the overall importance of the demonstrations for future political outcomes.

#### IV.A. Specifications and Hypotheses

To estimate the effect of the protests, we first investigate whether rainfall decreases attendance by regressing the number of protesters in a county on a dummy variable that indicates whether there was significant rain in the county. We use the dummy specification primarily for ease of interpretation. In Section VI we show that the results are robust to the way in which rainfall is measured. Specifically, we estimate the following equation:

$$(1) \quad \text{Protesters}_c = \text{Rainy Rally}_c \theta' + \text{Probability of Rain}_c \delta' + \mu_r + x_c \gamma' + \varepsilon_c,$$

where *Protesters* is a measure of rally attendance in county  $c$ , *Rainy Rally* is a dummy equal to 1 if there was more than 0.1 inch of rain in the county on the day of the rally (April 15, 2009), and *Probability of Rain* is a set of dummies controlling for the likelihood of rain on the day of the protest. Furthermore,  $\mu$  captures four U.S. Census region fixed effects, and  $x$  is a vector of predetermined county covariates. To exploit weather variation across counties with similar baseline likelihoods of rainfall on the protest day, we control for the rain probability flexibly. Specifically, we include dummy variables corresponding to the deciles in the historical rain probability distribution. To derive this distribution, we take the fraction of historical days that were rainy as defined by the 0.1 inch threshold.<sup>6</sup> Because rainfall is likely to decrease attendance at the rallies, we expect  $\theta < 0$ .

Our baseline specification includes a set of predetermined county controls. This inclusion is not necessary for our identification strategy if rainfall is uncorrelated with other determinants of political outcomes, but will have the benefit of reducing

6. As rain across adjacent days tends to be positively correlated, we restrict our data to April 1, 7, 15, 21, and 30 to ensure that we use independent and identically distributed draws. In the end, this procedure yields 140 past realizations for estimating the rainfall probability. The results are insensitive to dropping the rainfall dummies completely and to the set of historical days we use. For example, the findings are robust to employing only April 15, or all days in April.



residual variation and improving the precision of our estimates.<sup>7</sup> The standard set of covariates includes flexible controls for population size (decile dummies) and other demographic controls: log of population density, log of median income, unemployment rate, increase in unemployment between 2005 and 2009, share of whites, share of African Americans, share of Hispanics (the omitted category consists of other races and ethnicities), and share of immigrants (in 2000). We also include election covariates: county vote share for Barack Obama in the 2008 presidential election and outcomes from the two preceding U.S. House of Representatives elections (Republican Party vote share, number of votes for the Republican Party in total or per capita, number of votes for the Democratic Party in total or per capita, and turnout in total or per capita). Since the true functional form relating rainfall to attendance and later political outcomes is unknown, we present regressions with outcomes both in per capita and levels (the level results are relegated to the Online Appendix), where the per capita regressions are population-weighted.<sup>8</sup> For regressions in per capita terms (levels), we include election controls per capita (levels) to match the outcome variables. The Online Appendix contains a detailed description of how the variables are defined and constructed.

7. In Section VI we investigate the sensitivity of the estimates to the set of control variables and fixed effects. In general, the results are qualitatively and quantitatively similar.

8. Dickens (1990) demonstrates that population weighting in geographically grouped data is only desirable when group sizes are small enough that the variance of the average of individuals is larger than the variance of the group component. For county-level observations, group sizes are frequently quite small. More than 10% of the counties in our data have fewer than 5,000 residents, meaning that the individual error component in per capita election and (especially) Tea Party movement outcomes is large. Although population-weighting can induce heteroskedasticity, a test recommended by Dickens to gauge this effect (i.e., regressing the squared residuals from the weighted least squares attendance regression on population size) returns a small and statistically insignificant result. Additionally, measurement error is likely to be more problematic in small counties. For example, the gap between the largest and smallest rally attendance estimate in per capita terms declines with population size, and that relationship is statistically significant (see Online Appendix Figure A.1). Population weighting, or, alternatively, a minimum population filter, is thus the correct approach and improves the precision of the per capita specifications. Nevertheless, to demonstrate the robustness of our results, we include unweighted specifications for key outcomes in Online Appendix Table A.18. These tests also find that rain on April 15, 2009, negatively affected attendance and Tea Party outcomes.

We cluster standard errors at the state level in all regressions. Clustering at the state level allows for arbitrary within-state correlation and assumes that there is no cross-state correlation. If there is substantial spatial correlation, this assumption may be too strong. To alleviate such concerns, Figure III depicts the residual variation in rainfall that identifies the model. As can be seen in the figure, there is variation within the various regions of the country. In Section VI we show that inference is also robust to alternative methods.

In the second stage of our estimation, we examine whether the protests affected the strength of the Tea Party movement and voting behavior, by using rainfall as a proxy for protest effectiveness:

$$(2) \quad y_c = \text{Rainy Rally}_c \kappa' + \text{Probability of Rain}_c \delta' + \mu_r + x_c \gamma' + \varepsilon_c,$$

where  $y$  is a variety of postrally outcomes.

A limitation of these estimates is that it may be hard to interpret the size of  $\kappa$ , the coefficient on our rainfall dummy. We therefore also produce estimates that provide a per attendee scaling of the weather effect on later outcomes:

$$(3) \quad y_c = \text{Protesters}_c \lambda' + \text{Probability of Rain}_c \delta' + \mu_r + x_c \gamma' + \varepsilon_c,$$

where  $y$  again represents a variety of postrally outcomes. We estimate this equation using a two-stage least squares (2SLS) approach, with equation (1) being the first-stage regression. If rainfall affects outcomes only through the size of the rally, we can give a strict causal interpretation to  $\lambda$ , which would be a consistent instrumental variable estimator of the causal effect of an additional protester on outcomes. If protests have heterogeneous effects on outcomes, we will identify the local average treatment effect (LATE) on counties where attendance is sensitive to whether it rains. For example, if attendance by relative moderates is lower under worse weather conditions and has a larger spillover effect on other individuals, the LATE is larger than the average effect.

As described in the data section, we use mean rally attendance from three reporting sources for our baseline estimates. To address the possibility of nonclassical measurement error, we also show estimates in the Online Appendix using the maximum reported attendance across the three sources. With this framework in mind, our main hypothesis is that the protests

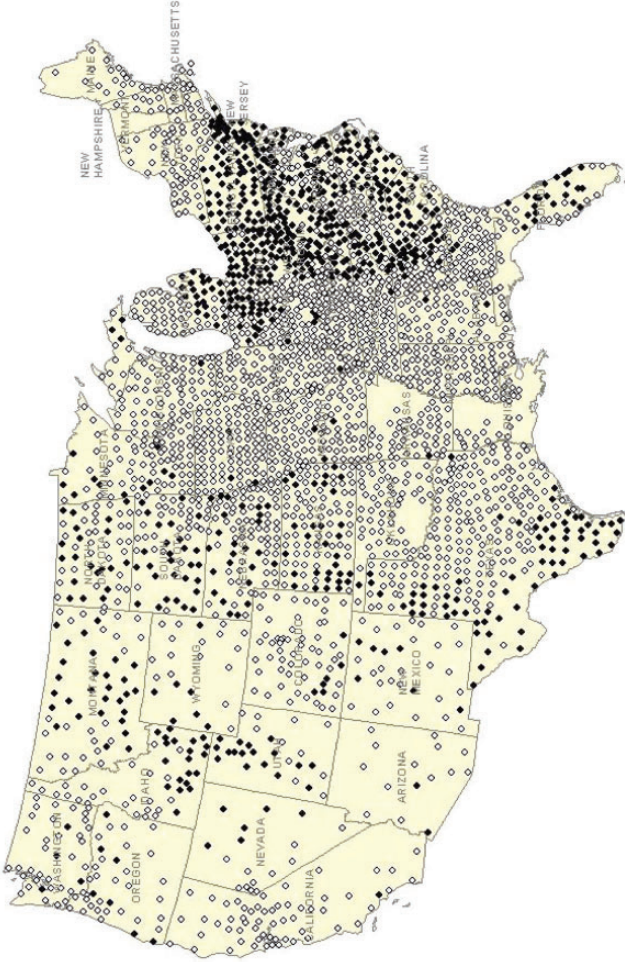


FIGURE III

Rainfall Residuals on April 15, 2009

The map shows the geographic distribution of the rain residuals in equation 1. Black dots indicate positive rain shocks, and white dots indicate negative shocks.

strengthened the consequent Tea Party movement and had a positive effect on votes for the Republican Party,  $\lambda > 0$ .

A natural channel through which rallies may have long-run effects is through increased local media coverage. Media coverage of a political movement can serve as a device for spreading information about a movement's policy agenda, which in turn may energize and grow the movement, or persuade voters and policy makers. To test this mechanism, we estimate the effects of weather on the protest day on local newspapers' coverage of the Tea Party movement. We run cross-sectional regressions week by week at the paper level, where the dependent variable is a count of the number of articles containing the phrase "Tea Party" and the independent variable is the measure of rain on Tax Day 2009 described in Section III. By estimating week-by-week effects using an equation analogous to equation (2), we can test whether rainfall affects media coverage immediately after the rallies, as well as whether there is an effect on later events that were important to the Tea Party movement.<sup>9</sup>

To assess whether the protests increase support for the movement and its political views, we use ANES survey data on political beliefs. District identifiers for survey participants are matched with district rainfall, which enables us to estimate a specification that is essentially identical to equation (2), using respondent outcomes, covariates, district rain probability and population size decile dummies, log population density, and region fixed effects:

$$(4) \quad y_i = \text{Rainy Rally}_i \beta' + \text{Probability of Rain}_i \delta' + x_i \gamma' + x_d \theta' + \varepsilon_i.$$

The covariates on sociodemographics are age, education, race, income, unemployment status, rural, and foreign-born status. Predetermined election covariates, from 2008, are dummies indicating whether the respondent voted for the Republican Party in the election for the House of Representatives.<sup>10</sup>

Finally, political protests may also affect policy. To investigate whether the Tea Party protests were successful in getting their policies implemented, we estimate the reduced-form

9. Because we do not have election and demographic data at the newspaper level, the media regressions are estimated without these covariates. Since the unit of observation is not a county, we also do not scale the effects by rally size.

10. Because we do not have rally attendance numbers at the district level, we cannot scale the weather effect by attendees.

relationship between rain during the protests and later policy-making outcomes in congressional districts. These outcome variables are the American Conservative Union's (ACU) assessment of congressmen's voting behavior, and a dummy variable indicating whether the incumbent congressman decided to retire prior to the 2010 midterm election. The ACU scores measure the percentage of scored votes that accord with the ACU position and are scaled from 0 (most liberal) to 100 (most conservative). As the ACU scores are available for every year, we estimate separate cross-sectional regressions for 2009 and 2010 along the lines of equation (2):

$$(5) \quad y_d = \text{Rainy Rally}_d \beta' + \text{Probability of Rain}_d \delta' + \mu_r + x_d \gamma' + \varepsilon_d,$$

where we include the same set of demographic and election controls employed in the county-level regressions. In addition, we also account for the past two years of ACU scores (decile dummies) and the identity of the victorious party in the last two elections (dummy variables identifying whether the past two elections were won by the Democrats, Republicans, or a combination of the two).<sup>11</sup> We use equation (5) to test the hypothesis that the Tea Party protests affected policy making in a conservative direction through either incumbents' voting behavior or by differentially affecting Republican and Democratic incumbents' likelihood of retirement. Our hypothesized underlying mechanism for this effect is that a lack of rain increases protest attendance (equation (1)), which strengthens the Tea Party movement and shifts the electorate toward more conservative policies (equations (2–4)), which policy makers ultimately respond to for reelection purposes (equation (5),  $\beta < 0$ ).

#### IV.B. Exogeneity Check

A key identifying assumption is that rainfall in equation (1) is uncorrelated with other determinants of political outcomes. As shown in Table I and Appendix Tables A.1 and A.2, rainy and nonrainy counties and districts are quite similar on average. To more carefully address potential concerns regarding our

11. As the ACU score distribution is bimodal with Democrats and Republicans clustered around different means, a change from one party to the other can have a large effect, making our estimates imprecise. We increase precision by including controls for such shifts in the past to capture prior differences across districts that account for the switching.

identifying assumption in equations (1)–(5), we present exogeneity checks at both the county and the district level. Table II shows estimates produced by regressing prerally values of outcome variables on a dummy variable representing whether it rained on Tax Day 2009. The dependent variables used include the results of the 2008 House and presidential elections. The regressors are identical to those in equation (1), with the exception that the 2008 political covariates constitute the outcome variables and 2006 election controls are included to account for previous political trends. Table II shows that the rainfall dummy in our specification is not significantly correlated with any of the prerally political outcomes. (Online Appendix Table A.4 reports the results for voting outcomes in levels and the prerally donations to Our Country Deserves Better PAC.)

Online Appendix Table A.5 presents district-level estimates for the regression equivalent of equation (5) for the ACU's roll-call scores in years before the rally, where covariates analogous to those in equation (5) are included for roll-call scores and election outcomes in preceding years. The rain dummies do not contribute significantly to explaining the variation in roll-call scores in any of these cases. Together, Table II and Online Appendix Tables A.4 and A.5 lend credibility to our identification strategy.

## V. RESULTS

### V.A. *The Effect of Rainfall on Rally Attendance*

Table III presents estimations of equation (1). It shows that rainfall decreases attendance at the Tea Party Tax Day rallies. Columns (1)–(4) estimate the effects in per capita, where the dependent variable is scaled by the percent of the population attending. The estimate in column (1) uses the mean attendance across reporting sources and implies that rainfall decreases the share of the county population protesting by 0.082 percentage points ( $t = 3.98$ ). Given a (population-weighted) sample mean of 0.16% of the county population attending, rainfall decreases rally size by approximately 51%. To address the possibility of measurement error and under-reporting (see Section III.B), column (2) estimates the effect using the highest reported attendance across the three sources. The estimate indicates that bad weather decreased attendance by 0.17 percentage points ( $t = 3.69$ ), or, analogously, a 58% reduction in rally size. Column (3) shows a



TABLE II  
EXOGENEITY CHECK AT THE COUNTY LEVEL

Dependent variable	Republican votes, U.S. House, 2008		Democratic votes, U.S. House, 2008		Turnout, U.S. House, 2008		Obama vote share, 2008			
	(1) Votes, % of population	(2) 0.46 (0.52)	(3) Votes, % of votes	(4) 0.99 (1.32)	(5) Votes, % of population	(6) 0.23 (0.62)	(7) Votes, % of population	(8) 0.47 (0.62)	(9) 1.25 (1.39)	(10) 0.58 (1.00)
Rainy protest	0.53 (0.50)	0.46 (0.52)	0.61 (1.25)	0.99 (1.32)	0.66 (0.63)	0.23 (0.62)	0.93 (0.63)	0.47 (0.62)	1.25 (1.39)	0.58 (1.00)
Observations	2,758	2,758	2,758	2,758	2,758	2,758	2,758	2,758	2,758	2,758
R-squared	0.77	0.79	0.74	0.76	0.76	0.79	0.73	0.78	0.66	0.81
Election Controls	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Demographic Controls	N	Y	N	Y	N	Y	N	Y	N	Y
Dep. Var. mean	17.56	17.56	43.69	43.69	21.37	21.37	40.05	40.05	52.27	52.27

Notes. The unit of analysis is a county. *Rainy protest* is based on the precipitation amount in the county on the rally day (April 15, 2009). The dummy variable is equal to 1 if there was significant rain in the county (at least 0.1 inch) and 0 otherwise. All regressions include flexible controls for the probability of rain, population, and region fixed effects. The 2006 election controls account for the outcomes of the U.S. House of Representatives elections in 2006 and include the Republican Party vote share, the number of votes for the Republican Party per capita, the number of votes for the Democratic Party per capita, and turnout per capita. The demographic controls include log of population density, log of median income, the unemployment rate, the change in unemployment between 2005 and 2009, the share of white population, the share of African American population, the share of Hispanic population, the share of immigrant population, and the share of the population that is rural. More information on the variables, the data sources, and our specification are described in Section III, Section IV.A, and the Online Appendix. Robust standard errors in parentheses, clustered at the state level. \*\*\* 1%, \*\* 5%, \* 10% significance.



TABLE III  
THE EFFECT OF RAIN ON THE NUMBER OF TEA PARTY PROTESTERS IN 2009

Dependent variable	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Rainy protest	-0.082 <sup>***</sup> (0.021)	-0.170 <sup>***</sup> (0.046)	-0.128 <sup>***</sup> (0.036)	-0.108 <sup>***</sup> (0.034)	-0.096 <sup>***</sup> (0.023)	-0.190 <sup>***</sup> (0.051)	-0.165 <sup>***</sup> (0.055)	-0.228 <sup>***</sup> (0.096)	-0.473 <sup>***</sup> (0.211)
Observations	2,758	2,758	2,758	542	2,758	2,758	2,758	542	478
R-squared	0.16	0.14	0.15	0.22	0.41	0.41	0.41	0.40	0.43
Protesters variable	Mean	Max	Mean	Mean	Mean	Max	Mean	Mean	Mean
Rain variable	Dummy	Dummy	Continuous	Dummy	Dummy	Dummy	Continuous	Dummy	Dummy
Sample counties	All	All	All	Protesters > 0	All	All	All	Protesters > 0	Protesters > 0
Election controls	Y	Y	Y	Y	Y	Y	Y	Y	Y
Demographic controls	Y	Y	Y	Y	Y	Y	Y	Y	Y
Dep. var. mean	0.161	0.295	0.161	0.240	0.160	0.293	0.160	0.815	6.598

Notes. The unit of analysis is a county. *Rainy protest* is based on the precipitation amount in the county on the rally day (April 15, 2009). The dummy variable is equal to 1 if there was significant rain in the county (at least 0.1 inch) and 0 otherwise. The continuous variable in columns (3) and (7) is the precipitation amount in inches. All regressions include flexible controls for the probability of rain, population, and region fixed effects. The election controls account for the outcomes of the U.S. House of Representatives elections in 2006, 2008, and 2010. The per capita regressions we include the Republican Party vote share, the number of votes for the Republican Party per capita, the number of votes for the Democratic Party per capita, and turnout per capita. The level regressions include the Republican Party vote share, the total number of votes for the Republican Party, the total number of votes for the Democratic Party, and total turnout. Column (9) takes the natural logarithm of the election controls. The demographic controls include log of population density, log of median income, the unemployment rate, the change in unemployment between 2005 and 2009, the share of white population, the share of African American population, the share of Hispanic population, the share of immigrant population, and the share of the population that is rural. More information on the variables, the data sources, and our specification are described in Section III, Section IV.A, and the Online Appendix. *Mean* denotes the average turnout across the three sources of attendance data. *Max* is the highest reported turnout in any given location. Robust standard errors in parentheses, clustered at the state level. \*\*\* 1%, \*\* 5%, \* 10% significance.

significant relationship when we instead use the precipitation amount (hundreds of inches), and column (4) shows that rainfall decreases attendance when the equation is estimated on the sample of 542 counties for which there was a reported rally.<sup>12</sup> Columns (5)–(8) present results for analogous specifications where the dependent variable is measured in thousands of protesters and show a similar pattern of highly significant coefficients. The estimates in column (5) imply that rainfall decreases attendance by 96 protesters ( $t = 4.25$ ) on average, and the upper bound estimate using the highest reported number of attendees in column (6) implies a deterrent effect of bad weather of 190 fewer protesters ( $t = 3.71$ ).<sup>13</sup> Finally, when the dependent variable is scaled in logs, we find that rainfall decreases rally size by approximately 50% (0.473 log points, column (9)), which is consistent with the previous results.<sup>14</sup> In the remainder of the article, we focus on per capita and mean specifications (the equivalent level and maximum results can be found in the Online Appendix).

### V.B. *Movement Outcomes*

One of the primary mechanisms through which protests are thought to influence policy is by strengthening associated political movements. Historically, it has been difficult to obtain data on this type of activity, but the Tea Party's online-era birth allows us to measure local activism along some dimensions. For example, though Tea Party affiliation is largely informal, the number of social network profiles posted on the websites of the five main Tea Party factions is a good proxy for the number of

12. Conditioning the sample will introduce a bias if rainfall decreases the likelihood that there is a rally, so this estimate should be interpreted with caution. In fact, precipitation is negatively correlated with the likelihood of having a reported rally (results not shown for brevity).

13. In Online Appendix Table A15, we investigate whether the “complier” counties, that is, counties where the protest size responds to rainfall, are counties of a particular political leaning. We do so by dividing counties into categories reflecting whether they are Republican leaning, Democratic leaning, or swing counties in the 2010 midterm election, based on the predicted Republican vote share from previous elections and sociodemographics. We show that weather has an effect on protest size in all three types of counties.

14. Because the natural logarithm is undefined at zero, this regression is estimated on the sample of reported rallies. Also, to be consistent across specifications, the election covariates are measured in logs, which reduces the sample further by excluding observations where there were zero votes for a party in the preceding elections (i.e., where races were uncontested).

activists involved in local Tea Party organizing. As discussed in the data section, the IREHR has provided us with geocoded tallies as of July 1, 2010. In addition, we use information on numbers of donors to Tea Party Express. The first two columns of Table IV show that lack of rain during the 2009 Tax Day rallies causes more local organizers; column (1) implies that nonrainy counties have a 0.0077 percentage point higher share of the population engaged as local Tea Party organizers, on average, compared to rainy counties (significant at the 5% level), or approximately an increase of 13% from a (weighted) sample mean of 0.058. When making the additional assumption that the effects are driven purely by larger rally attendance, the estimates of equation (3) imply that a 1 percentage point increase in the number of protesters causes a 0.093 percentage point increase in the share of the local population joining the Tea Party movement as organizers (column (2)).<sup>15</sup> Although the absolute magnitudes are modest, relative to overall activity measured by these social networking sites, the effect of Tax Day rain is substantial. The overall effect of Tax Day rain on participation may be considerably larger if unmeasured involvement is similarly affected.

We provide further evidence that protests can have a persistent effect on future activism by estimating whether they increase Tax Day rally participation the following year (April 15, 2010). Columns (3) and (4) of Table IV show that a lack of rain during the 2009 rallies indeed leads to higher attendance during the 2010 rallies; a rain-free rally in 2009 causes a 0.065 percentage point higher share of the population to show up (significant at the 5% level). This is a nontrivial effect, since the average share of the population attending in 2010 is 0.070, which implies that lack of rain in 2009 approximately doubles the size of the protests locally the following year.<sup>16</sup> Scaled by attendees, the estimate in column (4) indicates that a 1 percentage point increase in the number of protesters in 2009 causes a 0.79 percentage point increase in the share of the county population protesting in 2010. Thus, Table IV

15. In Online Appendix Table A6 we show that the level outcomes and the specifications using the maximum number of attendees yield similar results.

16. This effect is quite large, but partly comes from the fact that attendance in 2010 was lower across the board than in 2009. One explanation for this decline in rally attendance is provided by Skocpol and Williamson (2011: 85): "Following the big DC rally in September 2009, more of the same seemed 'anticlimactic,'" explains Lynchburg Tea Partier John Patterson.

TABLE IV  
THE EFFECT OF TEA PARTY PROTESTS ON LOCAL TEA PARTY ACTIVITY AND PAC CONTRIBUTIONS

Dependent variable	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Tea Party organizers, 2010	Second-stage 2SLS estimates	Tea Party protesters, 2010	Second-stage 2SLS estimates	2009	2010	PAC contributions	2009-10
			Persons, % of population				PAC contributions, \$ per capita	
Rainy protest	-0.0077** (0.0030)		-0.065** (0.027)		-0.00032** (0.00013)	-0.0011*** (0.0004)	-0.0014*** (0.0005)	
% of pop. protesting scaling		0.0931** (0.0382)		0.794*** (0.277)				1.700** (0.698)
Observations	2,758	2,758	2,758	2,758	2,758	2,758	2,758	2,758
R-squared	0.04	—	0.05	—	0.16	0.20	0.23	—
Protesters variable	—	Mean	—	Mean	—	—	—	Mean
Election controls	Y	Y	Y	Y	Y	Y	Y	Y
Demographic controls	Y	Y	Y	Y	Y	Y	Y	Y
Dep. var. mean	0.058	0.058	0.070	0.070	0.002	0.008	0.010	0.010

Notes. The unit of analysis is a county. *Rainy protest* is based on the precipitation amount in the county on the rally day (April 15, 2009). The dummy variable is equal to 1 if there was significant rain in the county (at least 0.1 inch) and 0 otherwise. We instrument for the number of protesters using *Rainy protest* and 2SLS to derive the scaling estimates in columns (2), (4), and (8). % of pop. protesting scaling is based on the percent of people attending on the rally day relative to the county population. All regressions include flexible controls for the probability of rain, population, and region fixed effects. The election controls account for the outcomes of the U.S. House of Representatives elections in 2008 and include the Republican Party vote share, the number of votes for the Republican Party per capita, the number of votes for the Democratic Party per capita, and turnout per capita. The demographic controls include log of population density, log of median income, the unemployment rate, the change in unemployment between 2005 and 2009, the share of white population, the share of African American population, the share of Hispanic population, and the share of immigrant population, and the share of the population that is rural. More information on the variables, the data sources, and our specification are described in Section III, Section IV A, and the Online Appendix. *Mean* denotes the average turnout across the three sources of attendance data. Robust standard errors in parentheses, clustered at the state level. \*\* 5%, \* 10% significance.

lends credence to the idea that protests can facilitate the building of a movement, and that protests beget protests.

### *V.C. Monetary Contributions*

The strength of a political movement can partly be assessed by the willingness of its supporters to contribute in monetary terms. Table IV, columns (5)–(8), presents the effect of rain on the day of the rally on contributions to Our Country Deserves Better PAC. The reduced-form regressions in columns (5) and (6) demonstrate that lack of rain on the date of the rally significantly reduces contributions from individuals residing in the county. The estimate on the rain dummy in column (5) is for 2009 postrally contributions and implies that good weather caused approximately a \$0.00032 increase (significant at the 5% level). Column (6) provides further evidence that the protests had a persistent and strong effect on support for the movement, as rain-free rallies lead to a \$0.0011 per capita increase in 2010 contributions.<sup>17</sup> Given the sample means, this corresponds to a 16% increase in 2009 contributions and a 14% increase in 2010. Column (7) shows the effect on the sum across the two years. The scaled result in the final column indicates that a 1 percentage point increase in the population protesting leads to a \$1.7 increase in per capita contributions. This increase in monetary contributions may seem small in absolute terms, but the data we use are for only one specific PAC. If contributions to other affiliated groups are similarly affected, the total monetary effect could be substantial. Together with the effects presented in columns (1) through (4), our results show that political rallies can trigger both growth of and support for a movement as individuals volunteer as organizers and contribute monetarily, and that such effects can last for extended periods of time.

### *V.D. Media Coverage*

An additional mechanism through which protests can create support for a movement and further its policy agenda is media coverage. That is, if mass media report on the protests and the policies promoted by the protesters, the movement may itself be energized or get the attention of the general population and, ultimately, policy makers, who are known to over-respond to news

17. The 2010 data contain contributions up to the November elections.

coverage (Eisenberg and Strömberg 2007). Figure IV plots the time series of the estimated coefficient and confidence intervals. As expected, rain on April 15, 2009, has no significant effect on the level of media coverage prior to the Tax Day rallies, which are marked by the vertical bar. On Tax Day itself, a rainy rally leads to a statistically significant decrease of one article per week and newspaper or about 20–25% of the mean level of coverage. We interpret this effect as media finding it worthwhile to cover and report on a protest if it is sufficiently large, rather than small and insignificant (or, of course, nonexistent). The remainder of the figure tracks the effect of rain on April 15, 2009, on coverage in subsequent weeks. For most of the sample, the measured effect is slightly negative (though close to 0) and statistically insignificant. This coefficient becomes significant for only four events. Interestingly, all four statistically significant dates correspond to important events for the Tea Party movement. A drop in coverage of a size similar to the 2009 Tax Day drop occurs on Tax Day 2010, when attendance, as we saw in Section V.B, was driven down by rainfall on Tax Day 2009. This is consistent with the idea that media report on political activism once it is sufficiently substantial in magnitude. Smaller but still statistically significant differentials were also found around July 4, when there were many local events (Freedomworks 2009), and around the 2009 off-cycle elections. This means that even though there is no clear constant increase in media coverage of the movement in rain-free areas, we cannot exclude that some of the effects we find are reinforced by spikes in media coverage around key protest dates.

### V.E. *Political Beliefs*

As mentioned in Section II, Tea Party protesters commonly display discontent with the state of affairs in the country, and the movement, broadly speaking, promotes a conservative-libertarian political agenda. Table V presents survey evidence from about two weeks before Election Day 2010, showing that the protests increase popular support for the movement, and that the local population in areas with large rallies adopts political opinions typically expressed by the protesters and the Tea Party's leaders.<sup>18</sup>

18. All regressions include demographic controls. The results are robust to the exclusion of the controls, with similar point estimates, and significance at least at the same levels (results not shown for brevity). Because attendance data is not available at the district level, we do not scale these estimates.

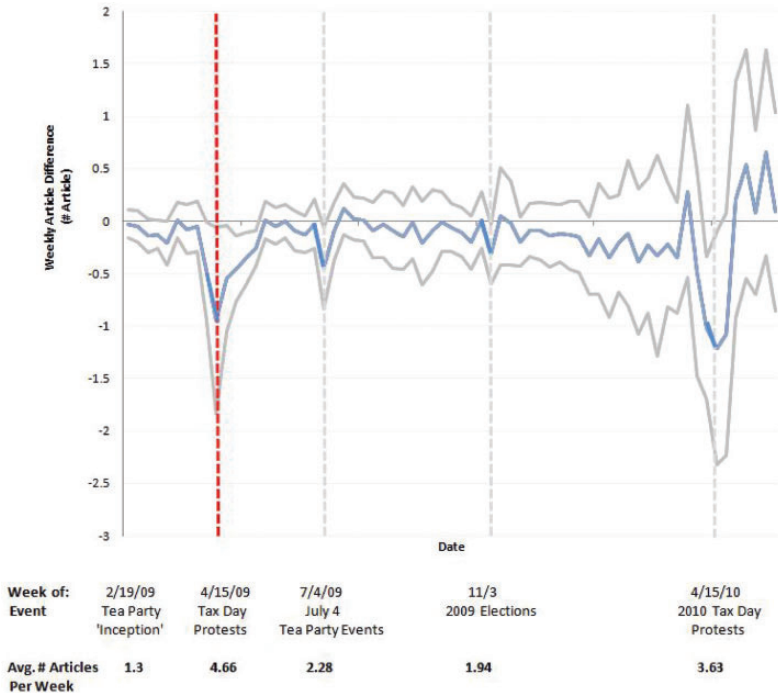


FIGURE IV

Newspaper Coverage Differential by Rain on April 15, 2009.

This graph shows the evolution of local media coverage of the Tea Party, as a function of rainfall on the day of the Tea Party rallies (April 15, 2009). The dark-shaded line represents the point estimates, with light-shaded lines corresponding to the 95 percent confidence intervals.

Respondents in nonrainy districts are approximately 6 percentage points more likely to express strong support for the Tea Party movement (column (1)). From a sample mean of 12.0%, this corresponds to approximately a 45% increase in the number of Tea Party supporters a year and a half later. Good weather also produces more favorable views towards former Alaska Governor Sarah Palin (column (2)), who was one of the movement's most outspoken leaders in 2010.<sup>19</sup> According to the estimates, the discontent expressed by the protesters spill over to the local

19. For example, she participated as a speaker in the rallies organized throughout the country as a part of the Tea Party Express Bus Tours of 2009 and 2010. She was, of course, also the Republican vice presidential candidate in 2008.



TABLE V  
POLITICAL BELIEFS, ANES SURVEY 2010

Dependent variable	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Strongly supports the Tea Party movement, dummy	Favorable view on Sarah Palin, dummy	Feels outraged about the way things are going in country, dummy	Opposes raising taxes on income >\$250K, dummy	Believes Americans today have less freedom compared to 2008, dummy	Unfavorable feelings toward President Obama, dummy	Average belief effect	Reported likelihood of voting in the 2010 midterm election
Rainy protest	-0.057** (0.025)	-0.057** (0.026)	-0.046** (0.021)	-0.058* (0.030)	-0.065** (0.026)	-0.046* (0.024)	-0.13*** (0.037)	-0.067*** (0.024)
Observations	1,146	1,140	1,142	1,140	1,138	1,145	—	1,092
R-squared	0.172	0.300	0.101	0.226	0.120	0.292	—	0.303
Election controls	Y	Y	Y	Y	Y	Y	Y	Y
Demographic controls	Y	Y	Y	Y	Y	Y	Y	Y
Dep. var. mean	0.120	0.311	0.174	0.228	0.438	0.245	—	0.701

Notes. The unit of analysis is a survey respondent, from the 2010 ANES survey data. The survey took place in October 2010. *Rainy protest* is based on the precipitation amount in the district on the rally day (April 15, 2009). The dummy variable is equal to 1 if there was significant rain in the district (at least 0.1 inch) and 0 otherwise. All regressions include flexible controls for the probability of rain, population, and region fixed effects. The election controls account for whether the respondent voted for the Republicans in the U.S. House of Representatives elections in 2008. The demographic controls include age, education, race (white, African American, Hispanic), household income, unemployment status (currently working), living in a rural area, and foreign born. More information on the variables, the data sources, and our specification are described in Section III, Section IV.A, and the Online Appendix. Robust standard errors in parentheses, clustered at the state level. \*\*\* 1%, \*\* 5%, \* 10% significance.

population, as 26% more individuals in nonrainy districts report feelings of outrage about the way things are going in the country (column (3) shows a 4.6 percentage point increase). There is also evidence that the protesters' and movement's small-government, largely libertarian, and anti-Obama views spill over to the population: respondents are 5.8 percentage points more likely to oppose raising taxes on incomes above \$250,000 (column (4)), 6.5 percentage points more likely to believe that Americans have less freedom compared to 2008 (column (5)), and 4.6 percentage points more likely to have unfavorable feelings toward President Obama (column (6)). Following Kling et al. (2004) and Kling, Liebman, and Katz (2007), we also derive the average effect across all outcomes (see Section VI for an extensive discussion of how we construct the average-effect measure for our study). Column (7) presents the average belief effect of rally rainfall on the family of political beliefs and shows that rain reduces preferences in line with the Tea Party's agenda by 0.13 standard deviation on average (significant at the 1% level). Finally, column (8) provides evidence suggesting that the protests (and consequent growth of the Tea Party movement) not only move beliefs in a conservative direction but also facilitate voter mobilization. When asked to assess the likelihood that the respondent will vote in the midterm elections, individuals living in nonrainy districts report a 6.7 percentage point higher likelihood of turning out.

#### *V.F. Election Outcomes*

Did these individuals actually turn out to vote in the 2010 elections to the House of Representatives? Table VI provides evidence that rallies lead to more votes for Republicans and a larger Republican vote share. Column (1) presents the reduced-form effect, which shows that counties that lack rain during the protests see a 1.04 percentage points larger share of the population voting for the Republican Party on average. Given a mean of 14.97, this yields an overall effect of 7%. Scaling the effect by the number of protesters, column (2) suggests that a 1 percentage point increase in the share of the population protesting increases the share of the population voting for the Republican Party by 12 percentage points (significant at the 1% level). Columns (3)–(4) show that there is little evidence that the protests affect votes for the Democratic Party, which suggests that the Tea Party protests, together with the consequent increase in media coverage

TABLE VI  
THE EFFECT OF TEA PARTY PROTESTS ON VOTING BEHAVIOR, 2010 U.S. HOUSE

Dependent variable	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Republican Party votes		Democratic Party votes		Republican Vote share		Votes, % of district votes
	Second-stage 2SLS estimates		Second-stage 2SLS estimates		Second-stage 2SLS estimates		
	Votes, % of county population		Votes, % of county population		Votes, % of county votes		
Rainy protest	-1.04*** (0.30)		-0.14 (0.35)		-1.55** (0.69)		-1.92*** (0.68)
% of pop. protesting scaling		12.59*** (4.21)		1.73 (4.14)		18.81** (7.85)	
Observations	2,758	2,758	2,758	2,758	2,758	2,758	435
R-squared	0.88	—	0.87	—	0.89	—	0.91
Protesters variable	—	Mean	—	Mean	—	Mean	—
Election controls	Y	Y	Y	Y	Y	Y	Y
Demographic controls	Y	Y	Y	Y	Y	Y	Y
Dep. var. mean	14.97	14.97	12.76	12.76	52.47	52.47	50.86

Notes. The unit of analysis is a county except for column (7), where we analyze the congressional district. *Rainy protest* is based on the precipitation amount in the county (or district) on the rally day (April 15, 2009). The dummy variable is equal to 1 if there was significant rain in the county (or district) (at least 0.1 inch) and 0 otherwise. We instrument for the number of protesters using *Rainy protest* and 2SLS to derive the scaling estimates in columns (2), (4), and (6). % of pop. protesting scaling is based on the percent of people attending on the rally day relative to the county population. All regressions include flexible controls for the probability of rain, population, and region fixed effects. The election controls account for the outcomes of the U.S. House of Representatives elections in 2008 and include the Republican Party vote share, the number of votes for the Republican Party per capita, the number of votes for the Democratic Party per capita, and turnout per capita. In addition, the congressional-district specification in column (7) also includes prior roll call controls to account flexibly for the past two years of ACU scores and election controls of the identity of the victorious party of the past two elections. The demographic controls include log of population density, log of median income, the unemployment rate, the change in unemployment between 2005 and 2009, the share of white population, the share of African American population, the share of Hispanic population, the share of immigrant population, and the share of the population that is rural. More information on the variables, the data sources, and our specification are described in Section III, Section IV.A, and the Online Appendix. *Mean* denotes the average turnout across the three sources of attendance data. Robust standard errors in parentheses, clustered at the state level. \*\*\* 1%, \*\* 5%, \* 10% significance.

and the strength of the movement, raise turnout in favor of the Republican Party. Since the (population-adjusted) marginal protester brings an additional 12 votes to the Republican camp, these estimates provide additional evidence indicating that political protests have large spillover effects on nonprotesters. The 95% confidence interval rejects the null hypothesis that the coefficient is equal to 1, that is, that there are no spillover effects. We provide a discussion as to how such spillover effects may arise.

This number of additional Republican votes generated may seem large at first glance, but it is important to realize that extra protesters lead to larger membership, higher contributions, and more conservative beliefs locally, thereby creating momentum reminiscent of the momentum caused by the early voters in Knight and Schiff (2010), who find that early voters in Democratic primaries have “up to 20 times the influence of late voters in the selection of candidates.”

Columns (5) and (6) of Table VI estimate the electoral advantage for the Republican Party, where the outcome variable is Republican vote share. The effects are nontrivial, implying that lack of rain increases the Republican vote share by 1.55 percentage points (significant at the 5% level), and that a 0.1 percentage point increase in the share of the population protesting leads to a 1.8 percentage point increase in the Republican vote share.<sup>20</sup> Column (7) shows the implications at the congressional-district level: good rally weather raises the Republican vote share by almost 2%. In other words, our results show that the Tea Party

20. In Online Appendix Table A16, we compare the scaling estimates to the ordinary least squares (OLS) estimates. In general, the OLS estimates are smaller in magnitude. This may be due to several reasons. First, as described in the data section and depicted in Online Appendix Figure A1, there is nontrivial measurement error in the reported rally size. This can lead to attenuation bias that the scaling estimates are not subject to. Also, as the measurement error is likely to be nonclassical and driven by under-reporting for small counties, population weighting partly addresses this. Unweighted regressions result in even larger two-stage least squares estimates, shown in Online Appendix Table A17, since more weight is put on smaller counties with under-reported rally size. Second, under the exclusion restriction and heterogeneous effects, the scaling estimate captures the local average treatment effect. This may be larger than the average effect of protest size. One reason for this may be that weather affects attendance in counties with many potential swing voters (see Online Appendix Table A15). Finally, as mentioned in Section IV, rainfall may affect voting behavior not only through protest size but also by directly increasing media coverage of the protests, which in turn has an effect on voting behavior.

protests were highly effective in getting out the vote and bringing electoral success to Republicans in the 2010 House of Representatives election.

Assessing the nationwide effect of the Tea Party protests based on our estimates is a difficult task. First, there could be spillover effects that we are not able to capture within our cross-sectional framework. Also, the scaled estimates using rainfall as an instrument will capture the LATE, which may differ from the average effect of protesters, or the effect under identical weather conditions. Finally, if the functional form is misspecified or the exclusion restriction of equation (2) is violated, the estimates would misrepresent the true average causal effect. With these caveats in mind, we do a simple back-of-the-envelope calculation assessing the nationwide effect of the Tea Party protests, using our estimates of equation (2). According to our data across all 542 reported rallies, there were an approximate 440,000–810,000 individuals protesting nationwide on Tax Day 2009. Using our per protester estimate, the protests mobilize an estimated 25,000–46,000 additional local Tea Party organizers, 170,000–310,000 additional protesters during the 2010 Tax Day rallies, and an increase in donations to the Tea Party Express's PAC of \$840,000–\$1,540,000. Furthermore, the protests have an estimated nationwide effect on the 2010 midterm election corresponding to 3.2–5.8 million additional votes for the Republican Party in the 2010 House elections.<sup>21</sup> Our results thus provide support for the commonly held notion that the Tea Party Movement played an important role in the Republican Party's landslide win in the 2010 House elections. Our results also indicate that the initial nationwide Tea Party protests on Tax Day in 2009 were key in building the Tea Party movement and driving the conservative shift in the electorate.

### *V.G. Policy Outcomes*

Ultimately people care about political rallies and movements because they have the potential to change policy. One channel is

21. The calculations are based on multiplying the total number of protesters with the per protester scaled estimates. They are taken from columns (2), (4), and (8) of Table IV, and column (2) of Table VI, respectively. These are, of course, point estimates, and as such one should take into account that there is uncertainty about the true value of the coefficient. For a highly conservative measure, one can take the lower bound of the 95% confidence interval, in which case the estimated nationwide effects are much smaller.

through elections. Another is through policy making by incumbents. Though the Tea Party umbrella encompasses many policy positions, in practice the vast majority of these positions are to the right of the median voter. We therefore test whether exogenous changes in the size of Tea Party rallies across districts affected the voting record of representatives as evaluated by a group with similar political preferences, the ACU. Each year the ACU assigns each congressperson a score based on his or her votes on a select number of House bills. This score, which ranges from 0 to 100, measures the extent to which the votes accord with the preferences of the ACU. In Table VII, we explore the effect of protest attendance on this measure of voting behavior.

Columns (1) through (4) indicate that rain on the date of the rally has significant effects on voting records in 2009 and 2010, in spite of the fact that representatives from rainy and nonrainy rally districts had similar voting records through 2008. The estimates indicate that scores in districts with smaller rallies due to rain were less conservative by 1.9–2.8 ACU points in 2009 (significant at the 5% and 1% levels, respectively), when the sample mean equaled 41.<sup>22</sup> For comparison, the difference between the average Democrat and the average Republican is about 85, whereas the standard deviation within the Republican caucus is about 12.5. The effect in 2010 is estimated at 3.2–4.3 points (significant at the 1% and 5% levels), with slightly lower point estimates when taking the difference between 2010 and 2008 scores. As the ACU score is based on 24 roll-call votes on which the ACU has an explicit position, with one vote for the conservative position giving a score of 100/24, the effect of nonrainy rallies corresponds to approximately one additional conservative vote in 2010. The fact that the estimates in 2010 are slightly larger suggests, much like the election results discussed before, that the policy effect of the initial rallies does not fade over time. It is also important to note that these changes are driven by incumbent House members. These results therefore demonstrate that the politicians in office respond to rallies and the perceived beliefs

22. In column (1) we estimate the effect on the full sample of all congressmen. However, since a substantial fraction of representatives did not vote on all the bills scored by the ACU, we provide estimates on the sample of representatives that actually voted on all 25 scored bills in 2009 (24 in 2010). The sample size is therefore smaller.

TABLE VII  
POLICY MAKING EFFECT

Dependent variable	2009		2010		Δ Score 2010 – 2008		(7) Republicans	(8) Democrats
	(1) Full	(2) All votes	(3) Full	(4) All votes	(5) Full	(6) All votes		
Rainy protest	-1.922** (0.937)	-2.827*** (1.021)	-4.296*** (1.258)	-3.181** (1.411)	-3.371** (1.310)	-2.405 (1.849)	0.049 (0.064)	-0.094*** (0.034)
Observations	435	327	435	279	435	191	179	256
R-squared	0.979	0.982	0.961	0.973	0.804	0.894	0.242	0.235
Election controls	Y	Y	Y	Y	Y	Y	Y	Y
Demographic controls	Y	Y	Y	Y	Y	Y	Y	Y
Dep. var. mean	41.14	41.44	41.45	39.17	-0.164	-0.157	0.0447	0.0469

Notes. The unit of analysis is a congressional district. *Rainy protest* is based on the precipitation amount in the district on the rally day (April 15, 2009). The dummy variable is equal to 1 if there was significant rain in the district (at least 0.1 inch) and 0 otherwise. *Full* denotes using the full sample of all congressmen. *All votes* includes only the congressmen that voted on all scored votes. All regressions include flexible controls for the probability of rain, population, and region fixed effects. The prior roll call controls account flexibly for the past two years of ACU scores. The election controls account for outcomes in the last two elections to the U.S. House of Representatives and include the identity of the victorious party, the Republican Party vote share, the total number of votes for the Republican Party, the total number of votes for the Democratic Party, and total turnout. The demographic controls include log of population density, log of median income, the unemployment rate, the change in unemployment between 2005 and 2009, the share of white population, the share of African American population, the share of Hispanic population, the share of immigrant population, and the share of the population that is rural. More information on the variables, the data sources, and our specification are described in Section III, Section IV.A, and the Online Appendix. Robust standard errors in parentheses, clustered at the state level. \*\*\* 1%, \*\* 5%, \* 10% significance.



of their constituents. Of course, not every change in voting behavior has direct legislative effects, as many pieces of legislation would have passed regardless. Still, the significant effect of Tax Day rain suggests that these results may indicate substantive shifts in voting records rather than just symbolic changes. Also, columns (7) and (8) show that there is an additional selection effect through the reelection behavior of incumbents: no rain on the day of the rally is estimated to make it about 9.4 percentage points likelier for Democrat incumbents to retire, whereas there is no such effect for Republican incumbents. This suggests that the Tea Party protests were effective in shifting the electorate toward more conservative policies (as shown in Tables VI and VII), forcing some Democratic incumbents into retirement.

To summarize, we find that the weather-driven exogenous variation in rally attendance on Tax Day 2009 affects the eventual effect of these rallies. Where it did not rain, the number of local Tea Party activists was larger than where it did. Grassroots organizing increased, as did contributions to associated PACs and attendance at subsequent rallies. The population at large adopted the conservative-libertarian views of the protesters, and voters mobilized. This then led to more conservative voting both in the 2010 midterm elections and in the U.S. House of Representatives and encouraged Democrat incumbents to retire.

## VI. ROBUSTNESS

To assess the sensitivity of the results to our baseline econometric specifications we perform a set of robustness tests. The outcomes of these tests are presented in a series of tables in the Online Appendix.

### VI.A. *Specification*

First, in Online Appendix Tables A.9a and A.9b, we show that the main results are robust to how the covariates are specified. Specifically, we run regressions excluding the demographic covariates (Panel A) or the region fixed effects (Panel B), as well as a specification with flexible covariates (Panel C).<sup>23</sup> The

23. In the flexible controls specification, we include nine dummies for each variable, where each dummy corresponds to a decile in that variable's distribution (one decile is the omitted category).

estimated coefficients in these regressions are of the same sign, similar in magnitude, and significant at the 5% level for all outcomes. It should be noted that the point estimates are generally less precisely estimated when covariates are excluded (e.g., see Table VI, columns (5) and (7), for comparisons with our baseline specification). When covariates are excluded for the Republican vote share outcome, the point estimate is significant at the 10% level or insignificant at the county level, whereas it remains significant at the 5% level at the congressional-district level (Panel A and B, columns (13) and (14)). The district-specific results in Online Appendix Table A.9b are quite similar to the main results in terms of significance and magnitude for ACU scores, incumbents' decision to retire, as well as average political belief effect. Online Appendix Table A.10 presents reduced-form estimates for the main outcomes using the 9 census division fixed effects. Column (2) shows that the coefficients are more noisily estimated, which is unsurprising given there is less variation in rainfall. Columns (3) and (4) account for the fact that some divisions only had a few counties and districts with rainfall by restricting the sample to divisions with meaningful within-division variation in rainy protests. The estimates are robust to these sample permutations, though the significance level occasionally drops below conventional cutoffs for future protests and PAC contributions. In sum, our findings are quite insensitive to the set of covariates or regional controls included in our baseline specification.

Second, we present results using different measures of rain on the day of the protest in Online Appendix Tables A.11a and A.11b. In particular, we use a higher precipitation threshold for the rainfall dummy (Panel A), the natural logarithm of the precipitation amount (Panel B), or a rainfall dummy that uses all weather stations within 10 miles of the county centroid as opposed to the stations within a county's borders (Panel C). In Panel A, the coefficients are estimated using a precipitation threshold for significant rain defined at 0.35 inch, instead of the baseline specification of 0.1 inch. Essentially all the coefficients in both the county- and district-level regressions are equal to or larger than those flowing from the baseline specification, and significant at the 5% or 10% level.<sup>24</sup> The estimates in Panel B show

24. The share of counties with a rainfall dummy equal to 1 decreases from 0.2 to 0.08 when we use the higher precipitation threshold. Because there is less variation in the rainfall variable, the standard errors become larger.

that the results are qualitatively similar when using a continuous measure of rainfall. Finally, restricting rainfall to a circle with a 10-mile radius around the county or district centroid (Panel C) changes little for our county results, whereas some of our findings on the district level are more sensitive to this rainfall definition.<sup>25</sup> For example, the 2009 ACU score and the average effect on political beliefs are no longer significant and smaller in magnitude. Taken together, however, the tests indicate that the results are reasonably robust to the construction of the rainfall variable on the day of the protest.

Third, in Online Appendix Table A.12 we estimate the coefficients using a nonparametric estimation method by checking the sensitivity of our results using the nearest-neighbor matching estimator (Abadie et al. 2004). Each county with rainfall is matched to the four nonrainy counties with the closest values of the controls using a procedure that is bias-corrected and includes robust standard errors. The match is based on the discrete distribution of the controls employed in our baseline specification (identical to Online Appendix Table A.9a, Panel C). The estimates are significant at the 5% level and similar or larger in magnitude compared to the ordinary least squares (OLS) estimates.

Fourth, in Online Appendix Tables A.13 and A.14 we restrict the sample in two ways. Online Appendix Table A.13 shows the results when we exclude counties with a population size below 10,000 or above 1,000,000 people. Our findings hold up well in this setting, suggesting that outliers in terms of population size are unlikely to drive the results. Online Appendix Table A.14 limits the data to those counties where at least one of the three sources reports that a rally was held on Tax Day in 2009. Restricting the sample in this way will lead to unbiased estimates under the assumption that rainfall does not affect the likelihood of holding a rally or being of significant enough size for the rally to be reported. This assumption may not be realistic, however, as behavioral mechanisms or preferences that drive lower attendance on the intensive margin are likely to affect the extensive margin as well.<sup>26</sup> Nevertheless, the results are qualitatively similar when the sample is conditioned in this way.

25. Because there are counties without rainfall stations within 10 miles of the county radius, the sample size is smaller.

26. In fact, depending on the specification, we can reject the null hypothesis that rainfall does not affect the likelihood of having a reported rally.

### VI.B. Inference

The baseline specification clusters the standard errors at the state level, which requires an assumption of zero cross-state spatial correlation. Because this assumption may be rather strong, we conduct three robustness tests to assess the sensitivity of the results to spatial correlation.

First, we calculate standard errors that account for spatial dependence parametrically, following the procedure developed by Conley (1999). This procedure allows for spatial dependence in each spatial dimension (latitude and longitude) that declines in distance between units (county or district centroids, in our case) and equals 0 beyond a maximum distance. Since this maximum distance is unknown, we provide standard errors with four different maximum distances: 5, 10, 15, and 20 degrees, respectively.<sup>27</sup> Online Appendix Table A.15 presents the results together with the state-clustered standard errors for the main outcomes.<sup>28</sup> In general, the results are still significant when using these alternative ways of calculating the standard errors, and the magnitude of the spatial standard errors is broadly comparable to that of the state-clustered ones.

Second, to assess whether our effects are driven by an influential county or area, we run regressions where we drop each state. Online Appendix Figure A2 plots the distribution of coefficients, and shows that the results are not driven by a particular state.<sup>29</sup>

Third, and perhaps most important, we conduct a series of placebo tests using rainfall on other historical dates in April. These placebos are drawn from the same spatially correlated distribution as rainfall on April 15, 2009. If rainfall on the protest day has a causal effect, the actual estimate of rainfall ought to be an outlier in the distribution of placebo coefficients. To implement

27. A degree is approximately 68.3 miles (110 km), depending on where on Earth it is measured. Five degrees is about the shortest east–west distance of Utah’s state boundaries.

28. The procedure developed by Conley does not allow for population-weighted regressions. However, if the unweighted standard errors here are comparable in magnitude to the unweighted state-clustered standard errors, there is no obvious reason one would suspect population-weighted standard errors not to be comparable.

29. Region-by-region and state-by-state Fama-MacBeth regressions show similar results.

this procedure, we rerun each regression (for the main outcomes) replacing the rainfall dummy on the protest day (April 15, 2009) with the rainfall dummy from a historical day in April between 1980 and 2008. Because there are days when there are no (or very few) counties in the entire country that experienced significant rain, we run the placebo regression only on the dates where at least 10% of the counties experienced significant rain.<sup>30</sup> There are 627 placebo dates in the sample at the county level. Figure V presents the cumulative distributions of placebo coefficients for the main outcomes, together with the actual estimate from the 2009 protest day (the black line). (The remaining placebo graphs can be found in Online Appendix Figure A3.) It also reports the fraction of placebo estimates that are larger in magnitude than the actual estimate (in absolute terms, or in terms of a larger negative value). It shows that the actual estimate is indeed an outlier in the distribution of placebo dates in essentially all regressions. For example, only 0.4% of the placebo estimates of the effect of rainfall on Tea Party protesters in 2009 are more negative than the actual estimate, and 1.0% of the estimates are larger in absolute magnitude. For local Tea Party organizers, 1.5% of the estimates are more negative, and 4.8% are larger in absolute magnitude. Similarly, only 3.7% of the placebo estimates of the effect of rainfall on Republican votes are more negative than the actual estimate, and 9.1% of the estimates are larger in absolute magnitude. These tests strengthen the claim that the rainfall truly caused a stronger Tea Party movement, as well as more conservative policy making and voting behavior in the general population.

Finally, our findings do not rest on any individual result alone but on the fact that so many different measures of Tea Party strength and effect are affected in the hypothesized direction. To evaluate the likelihood of finding so many consistent results, we follow the method used in Kling et al. (2004), Kling, Liebman, and Katz (2007), and Clingingsmith, Khwaja, and

30. By requiring that there is at least some nontrivial amount of variation in rainfall across counties, we help avoid that each placebo estimate is driven by a few outlier counties. As the mean share of counties with significant rainfall across dates in the placebo sample is 0.20, and the actual share of counties with significant rainfall on the April 15, 2009, protest day is 0.22, the actual rainfall realization can be viewed as a typical draw from the placebo date distribution (standard deviation is 0.11).

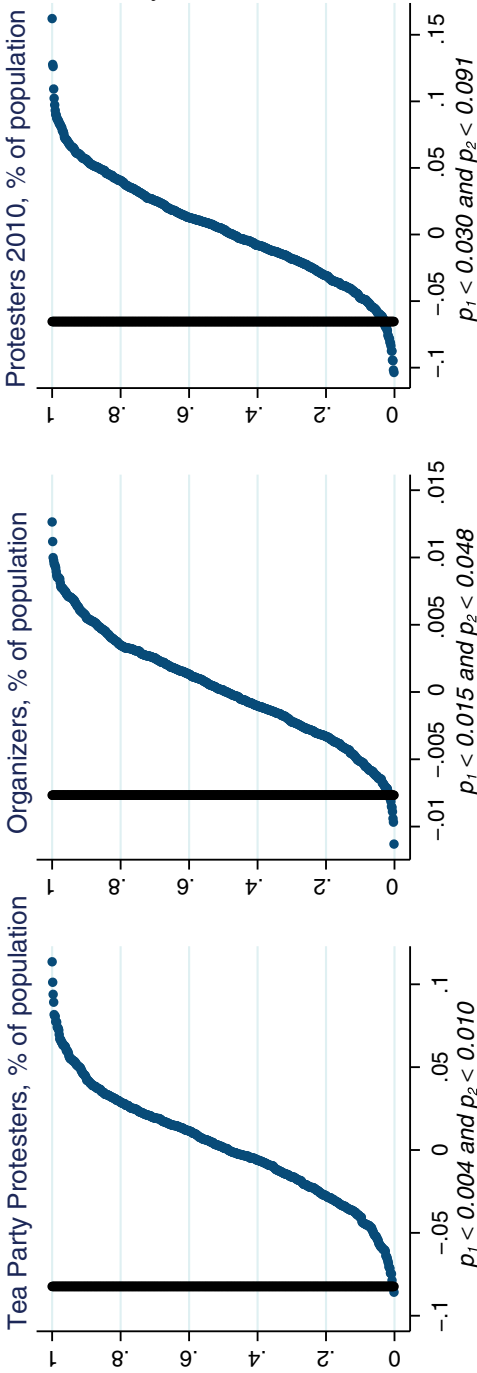


FIGURE V

Effect of Rainfall

The graphs show the main effect of rainfall on the day of the Tea Party rallies (April 15, 2009), compared to the cumulative distribution of estimates for rainfall from the placebo dates for a selected set of our outcomes. The Online Appendix contains the graphs for the remaining results. The placebo dates consist of each day in April between 1980 and 2008 where at least ten percent of the counties experience rain. The black line indicates the estimated coefficient on the day of the rallies. Under each graph two summary statistics are presented, where  $p_1$  is the fraction of placebo estimates with more negative values compared to the estimate on the day of the rally, and  $p_2$  is the fraction with larger absolute values.

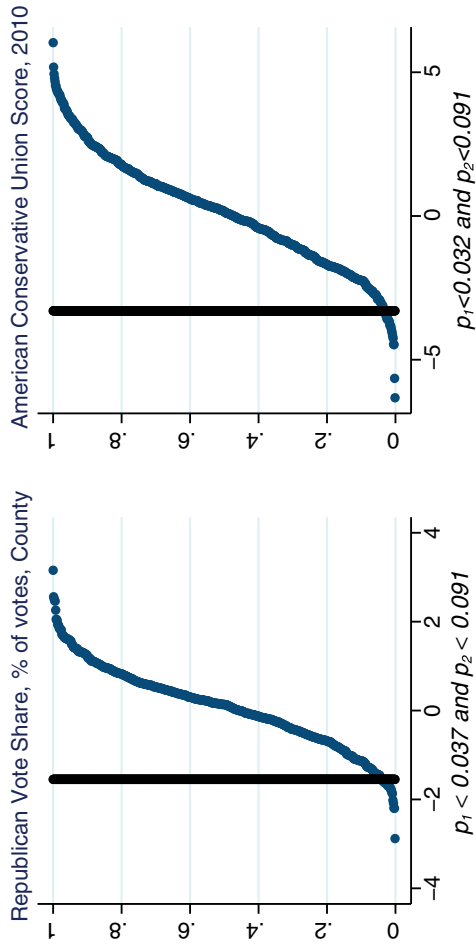


FIGURE V  
Continued



Kremer (2009) and construct an average-effect measure. Specifically, we define the average-effect size for  $K$  outcomes as:

$$(6) \quad \tau = \frac{1}{K} \sum_{k=1}^K \frac{\pi_k}{\sigma_k},$$

where  $\pi_k$  is equal to the effect of rainfall on outcome  $k$ , and  $\sigma_k$  is the standard deviation of outcome  $k$  in the comparison nonrain-fall group. We construct this measure using all outcome variables (contributions, percentage of votes, vote share, political beliefs, organizers, ACU scores, and subsequent rally attendance in both level and per capita terms, where relevant) for the weather of every day in April from 1980 to 2008.

In Figure VI we plot the distribution of these placebo average effects against the average effect measured for the day of the rally, April 15, 2009. We present placebos for all the main outcomes as well as the average standardized effect across all outcomes together (bottom right). The standardized effect across all outcomes shows that the true average effect has a larger negative value than any given placebo draw, with only 2.9% of the placebos being larger in absolute magnitude. This is further evidence that our results are statistically meaningful even when drawn from a distribution with the same spatial correlation patterns.

To summarize, our robustness tests indicate that it is highly unlikely that the results were driven by random weather patterns that did not have a true causal effect on the Tea Party protests, the movement, and consequent local political dynamics.

## VII. DISCUSSION AND CONCLUSION

This article provides novel evidence on the effects of political protests on policy making and elections. The existing political-economy framework that analyzes how protest size affects voting behavior and policy was first developed by Lohmann (1993, 1994a), as discussed in Section I. We assess whether this framework can sufficiently explain our main results. In Lohmann's framework, protests affect policy through a Bayesian learning process. We present a simplified version of the model here. Specifically, when the distribution of policy preferences in society is unobservable when protesting is costly, the number of protesters expressing their beliefs in favor of a policy

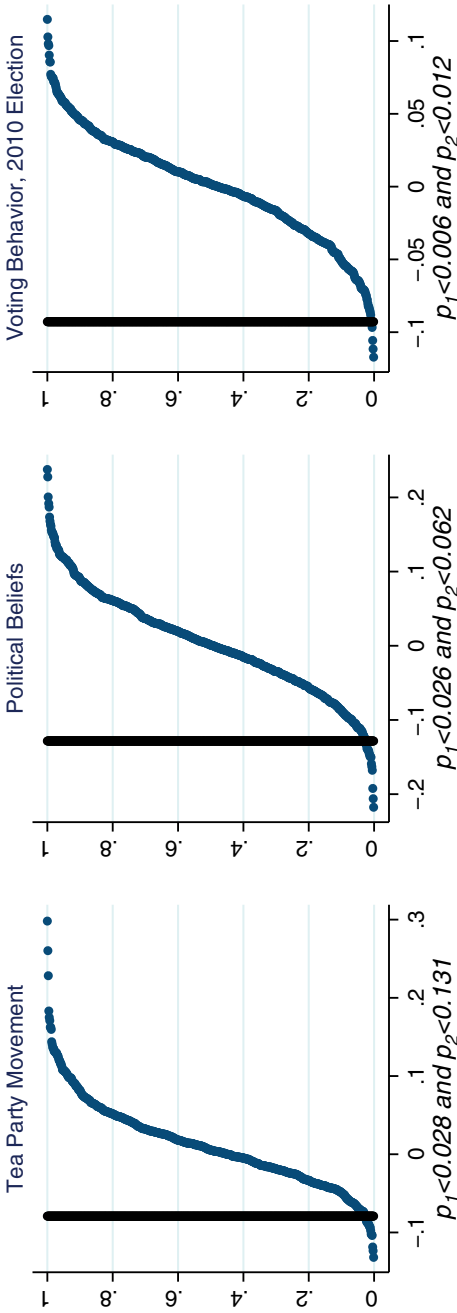


FIGURE VI

## Average Standardized Effect of Rainfall

The graphs show the average standardized effect of rainfall on the day of the Tea Party rallies (April 15, 2009), compared to the cumulative distribution of estimates from the placebo dates. The placebo dates consist of each day in April between 1980 and 2008 where at least 10% of the counties experience rain. The black line indicates the estimated coefficient on the day of the rallies. The outcomes are: Tea Party movement organizers 2010, protesters 2010, and PAC contributions, both the level and per capita specifications of Table IV); political beliefs (same as columns (1)–(6), of Table V); voting behavior in the 2010 election (Republican Party votes and vote share, same as columns (1), (2), and (7) of Table VI); and policy-making (ACU Score 2009, ACU Score 2010, and Retirement of Democrats, same as columns (1)–(4) and (8) of Table VII). The bottom-right graph is the average standardized effect across all those outcomes. Under each graph two summary statistics are presented, where  $p_1$  is the fraction of placebo estimates with more negative values compared to the estimate on the day of the rally, and  $p_2$  is the fraction with larger absolute values.

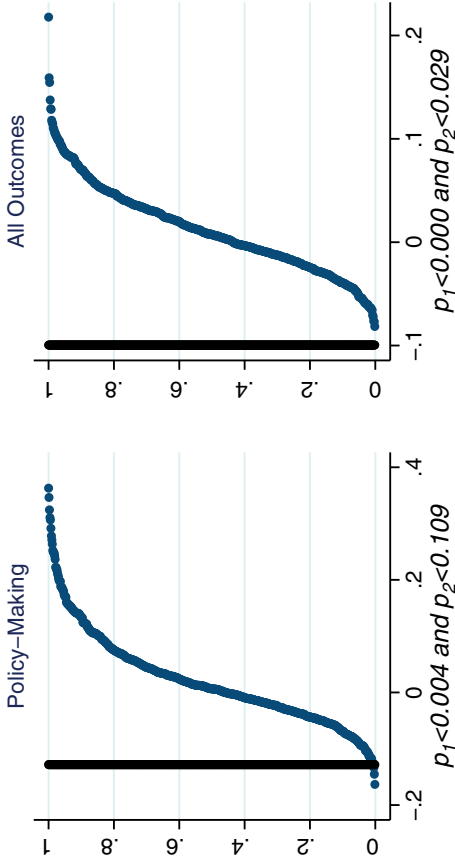


FIGURE VI  
Continued

change is a sufficient statistic describing the distribution of beliefs. When they observe a surprisingly large number of protesters, policy makers update their beliefs about preferences and the policy they choose to set.<sup>31</sup>

### VII.A. A Simple Information Revelation Model

Suppose that there is a continuum of voters in a congressional district, where the population measure is normalized to 1. Let  $g_{c,t}$  be the policy position set by the incumbent in district  $c$  at time  $t$ . We can think of  $g_{c,t}$  as corresponding to the left–right political spectrum on the real line, where a higher  $g_{c,t}$  corresponds to more conservative roll-call voting. Each voter  $i$  has single-peaked preferences in  $g$  and therefore a strictly preferred (bliss) policy. The distribution of voters' preferred policy in a district is  $g_{i,c} f(\bar{g}_c, \sigma)$ , where  $f$  is the normal probability density function. Because the distribution is symmetric,  $\bar{g}_c$  is also the preferred policy of the median voter. There is uncertainty about the median voter so that  $g_c = \bar{g}_c + e_c$ , where  $e_c$  is drawn from a normal distribution with mean 0 and standard deviation  $\sigma_e$  and only  $\bar{g}_c$  is observable.

Incumbents set policy to maximize the likelihood of getting reelected. Suppose that it is always optimal for the incumbent to set policy  $g_{c,t}$  equal to the median voter's preferred policy. Since the distribution of voters' preferences is not directly observable, the incumbent in district  $c$  will set policy at time  $t$  based on his expectation of the median voter:

$$(7) \quad g_{c,t} = E_t[g_c | I_{c,t}].$$

Initially, the policy is  $g_{c,0}$ . Suppose that at time  $t = 1$ , before policy is set, voters can protest for a more conservative policy  $g_p$ , where  $g_p > g_{c,0}$ . A leader coordinates the protests and exogenously sets the protesters' policy  $g_p$ . Only voters with sufficiently conservative preferences will prefer this proposed policy. Protesting is associated with some cost,  $q_c$ , because it is unpleasant to stand outdoors in bad weather or because there is an opportunity cost. We focus on how weather affects cost. Protesting in the rain is unpleasant, so the cost of protesting is higher on a rainy day,  $q_r$ , than on a sunny day,  $q_s$ , making  $q_r > q_s$ . For simplicity's sake, we

31. We assume heterogeneous preferences among voters. Lohmann (1994a) uses heterogeneous beliefs with common preferences. For our purposes, the distinction is not important.

assume that the cost is homogeneous among voters in a given district and that the weather is observable to voters and policy makers alike.

We assume that people protest sincerely, because they like to express their political preferences, and that the payoff from protesting,  $h(g_i, c)$ , is strictly increasing in the benefit of the proposed policy,  $h' > 0$ .<sup>32</sup> There is, therefore, a cutoff value above which voters will protest and below which they will not:

$$(8) \quad h(g_i, c) > q_c.$$

It follows that the number of protesters in a district,  $p_c = \text{Prob}(h(g_i, c) > q_c)$ , depends on the weather,  $p_c(q_c)$ . As in Lohmann,  $p_c$  is a sufficient statistic for identifying the median voter. Incumbents will then, in periods  $t > 0$ , update their beliefs and set policy conditional on the number of protesters in  $t = 1$ .

Now suppose there are  $N$  of these congressional districts. Define  $\beta_t$  as the mean difference between policy set in rainy and sunny districts. This difference will reflect the difference between incumbents' expectations of the median voter's bliss policy in the two types of districts:

$$(9) \quad \beta_t = E[g_{c,t}(\text{rain}) - g_{c,t}(\text{sun})] = E[g_c|\text{rain}] - E[g_c|\text{sun}].$$

Our question is what this framework predicts for the reduced-form effect of weather on policy,  $\beta_t$ . If weather and  $p_c$  are both perfectly observable to policy makers, it is obvious that policy should not differ across districts ( $\beta_t = 0$ ). Policy makers will simply adjust the number of protesters for the weather effect. This simple case suggests that information revelation *with no changes in political preferences among voters* is unlikely to drive our results.<sup>33</sup> Suppose, instead, that the quality of information through which protest size reflects underlying preferences depends on the weather. Weather could then affect incumbents' beliefs about voter preferences. A straightforward example is a situation in which policy makers get their information from newspapers, and newspapers only view large protests as newsworthy. To formalize this, suppose that incumbents only observe  $p_c$  when

32. Even in a more sophisticated game with strategic protesting and collective action problems, such as in Lohmann (1994a), only those with sufficiently conservative preferences protest.

33. This statement is, of course, also directly supported by the survey evidence showing that political beliefs shifted in the conservative direction.

it is sunny. This implies that in sunny districts the median voter is revealed at  $t = 1$ , whereas in rainy districts uncertainty persists past  $t = 1$ . The key implication is that in any time period  $t > 0$ , as long as additional information about voters' preferences continues to arrive, the absolute difference in policy between the two types of districts should decrease.<sup>34</sup>

We thus claim the following: if weather on the protest day has no effect on preferences and only affects policy through learning, then any initial learning effect should decrease over time as additional information makes its way to the rainy districts:

$$(10) \quad |\beta_t| > |\beta_{t+1}|.$$

We do not, however, find evidence that the effects decrease over time. The results in Table VII show that the effects in 2010 are, if anything, larger than the effects in 2009. It is thus unlikely that protest size only affects policy making through the learning mechanism proposed by the standard framework. Instead, this suggests that preferences in the voting population actually shifted, so that the median voter position became more conservative in sunny districts as compared to rainy districts.<sup>35</sup> The next section highlights some alternative mechanisms that would be consistent with such a shift.

### VII.B. *Alternative Mechanisms*

If learning does not fully explain our results, a natural question is what does explain them. One strand of literature that would be consistent with political beliefs actually shifting is the social interactions literature (e.g., Glaeser, Sacerdote, and Scheinkman 1996, 2003; Topa 2001; Calvó-Armengol and Jackson 2004). Protesters may be affected by interactions with other protesters at the Tea Party rally, and nonprotesters may be affected by interactions with protesters. For example, moderate independents could become persuaded by the Tea Party policy agenda at the protests. Convinced conservatives may feel energized when many people show, even if only because of nice

34. Note that within a given district the difference in supported policy between two candidates, in this simple model, will not increase, because both will adjust their position in the same direction.

35. Note that when turnout is less than full, the median voter can shift to the right because of increased turnout among more conservative citizens.

weather, and become more passionate proselytizers, as seems to be the case for many of the local Tea Party activists portrayed by Skocpol and Williamson (2011). This may be highly effective, as interactions in person are the most effective campaign instruments available, at least when it comes to raising voter turnout (Green and Gerber 2008). Furthermore, if political beliefs spread in social networks, protesters may persuade nonprotesters. This would explain why a shift occurred in the voting population toward the conservative position, and why that shift went beyond those voters initially involved in the Tax Day rallies.<sup>36</sup>

Another potential mechanism is that protests build a stronger political organization with the resources to support candidates in elections. The lobbying literature predicts that if a group of voters in society is politically organized, policy is more likely to be set according to this group's policy preferences (Baron 1994; Grossman and Helpman 1996; Persson and Tabellini 2000). The crucial mechanism here is that candidates interested in maximizing the probability of winning an election will find it optimal to cater to the organized group, because otherwise the group will provide support to other candidates. This mechanism goes a long way in explaining our findings regarding incumbent behavior.

Finally, the estimated persistence in political activism is consistent with habit formation models (Murphy and Shleifer 2004; Mullainathan and Washington 2009; Gerber, Huber, and Washington 2010). According to this literature, the act of protesting itself makes people more committed to the proposed policy agenda, and political attitudes shift as a result of having protested. This would explain why we see that attendance at future protests increases when many people protested initially. This would not, however, explain why we estimate increases in number of Republican votes that are larger than the total number of protesters.

Combinations of all three of these alternative mechanisms could be relevant, of course. Since the data do not allow us to fully separate between these potential alternative mechanisms, it would be helpful if further research pinpointed the precise

36. This argument parallels the findings of Banerjee et al. (2012). Studying the diffusion of a microfinance intervention, they show that one third of the impact detected on the diffusion of the program comes from people who themselves were not active in taking up the program.



mechanisms through which protests affect voting behavior and policy making and under which conditions.

### *VII.C. Conclusion*

We show that larger political protests can both strengthen the movement they support and help advance the political and policy agenda of the movement. We find that the 2009 Tax Day Tea Party protests increased turnout in favor of the Republican Party in the subsequent congressional elections, and increased the likelihood that incumbent Democratic representatives decided to retire. Incumbent policy making was also affected, as representatives responded to large protests in their district by voting more conservatively in Congress. In addition, we provide evidence that these effects were driven by a persistent increase in the movement's strength. Protests led to more grassroots organizing, to larger subsequent protests and monetary contributions, and to stronger conservative beliefs, as documented qualitatively by Skocpol and Williamson (2011). Finally, the estimates imply significant spillover effects: a 0.1 percentage point increase in the share of the population protesting corresponds to 1.9 percentage point increase in the Republican vote share. Our results suggest that political activism does not derive its usefulness solely from the provision of information or its consumption value, but that the interactions produced at rallies and protests can affect citizens' social contexts in ways such that a movement for political change persists autonomously. This confirms the importance of social dynamics in networks of citizens for the realization of political change and seems relevant not only in the context of representative democracies but also at the onset of revolutionary movements.

STOCKHOLM UNIVERSITY  
HARVARD KENNEDY SCHOOL  
AMERICAN ENTERPRISE INSTITUTE  
HARVARD KENNEDY SCHOOL

### SUPPLEMENTARY MATERIAL

An Online Appendix for this article can be found at QJE online ([qje.oxfordjournals.org](http://qje.oxfordjournals.org)).

## REFERENCES

- Abadie, Alberto, Guido W. Imbens, David Drukker, and Jane L. Herr, "Implementing Matching Estimators for Average Treatment Effects in Stata," *STATA Journal*, 4 (2004), 290–311.
- Banerjee, Abhijit, Arun G. Chandrasekhar, Esther Duflo, and Matthew O. Jackson, "The Diffusion of Microfinance," NBER Working Paper 17743, 2012.
- Baron, David P., "Electoral Competition with Informed and Uninformed Voters," *American Political Science Review*, 88 (1994), 33–47.
- Blais, André, *To Vote or Not to Vote? The Merits and Limits of Rational Choice Theory* (Pittsburgh: Pittsburgh University Press, 2000).
- Bueno De Mesquita, Ethan, "Regime Change and Revolutionary Entrepreneurs," *American Political Science Review*, 104 (2010), 446–466.
- Calvó-Armengol, Antoni, and Matthew O. Jackson, "The Effects of Social Networks on Employment and Inequality," *American Economic Review*, 94 (2004), 426–454.
- Cicchetti, Charles J., A. Myrick Freeman III, Robert H. Haveman, and Jack L. Knetsch, "On the Economics of Mass Demonstrations: A Case Study of the November 1969 March on Washington," *American Economic Review*, 61 (1971), 719–724.
- Clingingsmith, David, Asim Ijaz Khwaja, and Michael Kremer, "Estimating the Impact of the Hajj: Religion and Tolerance in Islam's Global Gathering," *Quarterly Journal of Economics*, 124 (2009), 1133–1170.
- Coate, Stephen, and Michael Conlin, "A Group Rule-Utilitarian Approach to Voter Turnout: Theory and Evidence," *American Economic Review*, 94 (2004), 1476–1504.
- Collins, William J., and Robert A. Margo, "The Labor Market Effects of the 1960s Riots," in *Brookings-Wharton Papers on Urban Affairs 2004*, William G. Gale, and Janet Rothenberg Pack, eds. (Washington, DC: Brookings Institution, 2004).
- , "The Economic Aftermath of the 1960s Riots in American Cities: Evidence from Property Values," *Journal of Economic History*, 67 (2007), 849–883.
- Conley, Timothy G., "GMM Estimation with Cross Sectional Dependence," *Journal of Econometrics*, 92 (1999), 1–45.
- Dickens, William T., "Error Components in Grouped Data: Is it Ever Worth Weighting?," *Review of Economics and Statistics*, 72 (1990), 328–333.
- Downs, Anthony, *An Economic Theory of Democracy* (New York: Harper and Row, 1957).
- EconomyPolitics, "How Many People Have Attended the Tea Parties? (Update 6)," April 17, 2010; available at <http://www.economy-politics.com>.
- Eisenberg, Thomas, and David Strömberg, "News Droughts, News Floods, and U.S. Disaster Relief," *Quarterly Journal of Economics*, 122 (2007), 693–728.
- Feddersen, Timothy, and Alvaro Sandroni, "A Theory of Participation in Elections," *American Economic Review*, 96 (2006), 1271–1282.
- Finkel, Steven E., and Karl-Dieter Opp, "Party Identification and Participation in Collective Political Action," *Journal of Politics*, 53 (1991), 339–371.
- Finkel, Steven E., and Edward N. Muller, "Rational Choice and the Dynamics of Collective Political Action: Evaluating Alternative Models with Panel Data," *American Political Science Review*, 92 (1998), 37–50.
- Freedomworks.org, "What's Next?," Google Map, 2009, available at <http://tinyurl.com/cghcfm>.
- Gerber, Alan S., Gregory A. Huber, and Ebonya Washington, "Party Affiliation, Partisanship, and Political Beliefs: A Field Experiment," *American Political Science Review*, 104 (2010), 720–744.
- Glaeser, Edward L., Bruce Sacerdote, and José A. Scheinkman, "Crime and Social Interactions," *Quarterly Journal of Economics*, 111 (1996), 507–548.
- , "The Social Multiplier," *Journal of the European Economic Association*, 1 (2003), 345–353.
- Green, Donald P., and Alan S. Gerber, *Get Out the Vote: How to Increase Voter Turnout*, 2nd ed. (Washington, DC: Brookings Institution Press, 2008).
- Grossman, Gene M., and Elhanan Helpman, "Electoral Competition and Special Interest Politics," *Review of Economic Studies*, 63 (1996), 265–286.

- Institute for Research and Education on Human Rights, *Tea Party Nationalism*, Special Report, Fall 2010.
- Holmes, Michael, "Republicans Urge Tea for Texas/Legislators Seek Cuts before Taxes," *Houston Chronicle*, December 7, 1991.
- Kling, Jeffrey R., Jeffrey B. Liebman, and Lawrence F. Katz, "Experimental Analysis of Neighborhood Effects," *Econometrica*, 75 (2007), 83–119.
- Kling, Jeffrey R., Jeffrey B. Liebman, Lawrence F. Katz, and Lisa Sanbonmatsu, "Moving to Opportunity and Tranquility: Neighborhood Effects on Adult Economic Self-Sufficiency and Health from a Randomized Housing Voucher Experiment," Mimeo, NBER, October 2004.
- Knight, Brian, and Nathan Schiff, "Momentum and Social Learning in Presidential Primaries," *Journal of Political Economy*, 118 (2010), 1110–1150.
- Kuran, Timur, "Sparks and Prairie Fires: A Theory of Unanticipated Political Revolution," *Public Choice*, 61 (1989), (1989), 41–74.
- Levenson, Michael, "Ron Paul Raises Millions in Today's Boston Tea Party Event," *Boston Globe*, December 16, 2007.
- Lohmann, Susanne, "A Signaling Model of Informative and Manipulative Political Action," *American Political Science Review*, 87 (1993), 319–333.
- , "Information Aggregation through Costly Political Action," *American Economic Review*, 84 (1994a), 518–530.
- , "The Dynamics of Informational Cascades: The Monday Demonstrations in Leipzig, East Germany, 1989–91," *World Politics*, 47 (1994b), 42–101.
- Madestam, Andreas, and David Yanagizawa-Drott, "Shaping the Nation: The Effect of Fourth of July on Political Preferences and Behavior in the United States," Mimeo, Stockholm University and Harvard Kennedy School, 2011.
- McAdam, Doug, *Political Process and the Development of Black Insurgency, 1930–1970* (Chicago: University of Chicago Press, 1985).
- McCarthy, John D., and Mayer Zald, "Resource Mobilization and Social Movements: A Partial Theory," *American Journal of Sociology*, 82 (1977), 1212–1241.
- McGrath, Ben, "The Movement: The Rise of Tea Party Activism," *New Yorker*, February 1, 2010.
- Mullainathan, Sendhil, and Ebonya Washington, "Sticking with Your Vote: Cognitive Dissonance and Political Attitudes," *American Economic Journal: Applied Economics*, 1 (2009), 86–111.
- Murphy, Kevin M., and Andrei Shleifer, "Persuasion in Politics," *American Economic Review*, 94 (2004), 435–439.
- Oberschall, Anthony R., "Rational Choice in Collective Protests," *Rationality and Society*, 6 (1994), 79–100.
- Opp, Karl-Dieter, *The Rationality of Political Protest: A Comparative Analysis of Rational Choice Theory* (Boulder, CO: Westview, 1989).
- Persson, Torsten, and Guido Tabellini, *Political Economics: Explaining Economic Policy* (Cambridge, MA: MIT Press, 2000).
- Riker, William H., and Peter C. Ordeshook, "A Theory of the Calculus of Voting," *American Political Science Review*, 62 (1968), 25–42.
- Skocpol, Theda, and Vanessa Williamson, *The Tea Party and the Remaking of Republican Conservatism* (New York: Oxford University Press, 2011).
- Silver, Nate, "How Many People Actually Attended Tea Parties?," Huffington Post, April 16, 2009; available at [http://www.huffingtonpost.com/2009/04/15/nate-silver-how-many-peop\\_n\\_187544.html](http://www.huffingtonpost.com/2009/04/15/nate-silver-how-many-peop_n_187544.html).
- SurgeUSA.org, "Nationwide Estimates of Tax Day Tea Party Protest Crowds," SurgeUSA.org, May 23, 2009.
- Texans for John Cornyn, "Big John," Texas Republican Party Convention video, 2008.
- Topa, Giorgio, "Social Interactions, Local Spillovers and Unemployment," *Review of Economic Studies*, 68 (2001), 261–295.
- Uhlener, Carole J., "Rational Turnout: The Neglected Role of Groups," *American Journal of Political Science*, 33 (1989), 390–422.
- Zuckerman, Alan S., ed., *The Social Logic of Politics: Personal Networks as Contexts for Political Behavior* (Philadelphia: Temple University Press, 2005).

This page intentionally left blank