PREFERRING A POUND OF CURE TO AN OUNCE OF PREVENTION:

RETROSPECTIVE VOTING AND FAILURES IN ELECTORAL ACCOUNTABILITY

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Abstract

Do voters effectively hold elected officials accountable for policy decisions? What are the consequences of failures in electoral accountability? Using a novel dataset on natural disasters, government spending, and election returns, we show that voters reward incumbents for delivering disaster relief spending but not for investing in disaster prevention spending. This failure of electoral accountability distorts the incentives of government actors, leading the government to underinvest in disaster prevention. This underinvestment causes substantial public welfare losses; we estimate that a dollar spent on prevention is over ten times more valuable than a dollar spent on relief in terms of net present value. Although our results suggest that the absence of a political imperative to provide prevention causes significant losses, we also explore a potential means of improving electoral accountability. We evaluate a federally-sponsored pilot program and find that grass-roots prevention efforts that emphasize communication with local leaders may encourage voters to reward government action that increases public welfare.

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Do voters effectively hold elected officials accountable for policy decisions? What are the consequences of failures in electoral accountability? How can citizens avoid making such failures? These questions are central to our understanding of politics since the ability of voters to hold elected officials accountable for their actions is a fundamental aspect of democratic government. If voters do not reward and punish politicians for their actions, then elected officials do not have incentives to be competent, responsive, and honest. Indeed, accountability is the central characteristic that distinguishes democracies from autocracies.

This study explores political accountability via an examination of natural disasters — stochastic, exogenous events that offer a unique opportunity to assess the reciprocal relationship between government actions and voter responses. As discussed below, we analyze how voters react to government efforts to *prevent* and *respond* to disasters, and assess the consequences of these behaviors on the parts of politicians and the public. In contrast to economic conditions, the area on which most previous research on retrospective voting has focused (e.g. Kramer, 1971; Fiorina, 1981; Lewis-Beck 1988),[1] disaster prevention is a domain in which we can actually estimate the effectiveness of specific, identifiable policy decisions. Consequently, we can determine how voters respond to those policies and test how effectively voters hold their elected officials accountable for their decisions. In addition, examining disaster relief and prevention makes it possible to address a principal limitation of previous work that has focused on economic voting. With economic policy, it is not possible to definitively establish the long-term costs associated with policies that produce short-term gains. If voters reward short-term economic growth, they may not be effectively holding government accountable if the long-term costs dwarf the immediate gains. For disasters, on the other hand, by comparing the way in which voters respond to prevention spending versus relief

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[1] Since the state of the national economy is the product of myriad factors, it is generally unclear how to interpret the empirical results on economic voting in terms of accountability (Alesina, Londregan, and Rosenthal, 1993). For example, Alesina, Londregan, and Rosenthal (1993, 26) argue that "the analysis of growth gives no evidence that voters *should* use information about aggregate growth to learn about competence."

spending, we can evaluate whether voters provide officeholders with the incentives to pursue the public welfare, or if voters instead only reward highly visible actions with immediate benefits.

Through this approach, this paper makes three novel contributions to the study of retrospective voting. First, by examining the electoral consequences of specific government actions in response to stochastic events, we are able to disentangle voters' responses to random events outside of incumbents' control from their responses to incumbents' efforts to prevent or ameliorate the damages produced by those events. Accordingly, we go beyond recent studies that have also examined voter responses to non-economic conditions (e.g., Achen and Bartels 2004a), but have only explored the direct effect of the events themselves. By directly analyzing policy choices that are tied to outcomes, we also build on previous work that has focused solely on the outcomes (e.g. Berry and Howell, 2007). Our results indicate that voters do a poor job of holding government accountable and providing public officials with the incentives to maximize public welfare. Voters significantly reward disaster relief spending. At the same time, voters show no response at all, on average, to prevention spending, even though investing in prevention is found to significantly reduce the extent of future damage.

Second, in addition to documenting the presence of suboptimal retrospection (which previous research has done as well), we are the first to estimate the loss of public welfare stemming from failures in accountability. We find that prevention spending significantly reduces future damages caused by natural disasters, whereas relief spending has no such impact. Our estimates suggest that the average dollar spent on disaster prevention reduces future disaster damage by more than five dollars in a single election cycle, and that the net present value of a dollar of prevention spending is about eleven dollars. Despite the large implied gains from increasing spending on prevention, disaster prevention for hurricanes, earthquakes, and floods fell during the sample period from 1985 to 2004, at the same time that disaster relief spending increased dramatically. This paper

shows that the incentives created by American voter behavior may help explain the fact that public policy has favored relief over prevention, providing a partial explanation for the government's failure to take additional *ex-ante* measures to mitigate damages from events such as Hurricane Katrina and the I-35 bridge collapse in Minnesota.

Third, we propose a potential means of improving citizens' abilities to retrospectively evaluate government performance. A significant part of voters' inabilities to reward prevention efforts may lie with politicians and the media, who often fail to publicize prevention efforts, making it difficult for voters facing information and attention constraints to reward public officials for their actions. To examine this issue, we consider a specific prevention program, Project Impact, undertaken late in the Clinton administration, which emphasized community involvement and communication with local leaders. We conclude by showing that voters rewarded the incumbent administration in response to Project Impact. The results suggest that grass-roots prevention efforts may provide a way for politicians to pursue actions that simultaneously win votes and maximize public welfare.

This paper is organized as follows. In the first section, we present some background and an overview of the theoretical issues underlying our analyses. The following section describes the data and discusses our empirical approach. The final two sections present the results and their implications for the study of democratic accountability.

Background and Conceptual Framework

In developing the theoretical bases for our hypotheses, we draw upon the extant literature on two sets of relationships: (1) the effect of conditions and events on elections and voting behavior, and (2) the influence of voters' attitudes and behavior on policymaking. We extend these literatures

by empirically examining each direction of the relationship between voting behavior and policy decisions, whereas the existing literature has generally focused on one or the other.

As mentioned above, the bulk of the literature on retrospective voting and electoral accountability has explored how voters hold governments accountable for economic conditions (e.g. Kramer, 1971; Kinder and Kiewiet, 1979; Fiorina, 1981; Hibbing and Alford, 1981; Lewis-Beck, 1988; Stein, 1990; Rudolph, 2003a, 2003b; Gomez and Wilson, 2006). Scores of studies have found evidence that economic conditions affect voting behavior, both in the United States and internationally, using both individual-level and aggregate data. Voters tend to reelect incumbents who have been stewards of healthy economies, and vote them out during times of economic hardship.

Recognizing the limitations of analyzing macroeconomic conditions, some recent empirical research has explored retrospective voting in non-economic domains. Achen and Bartels (2004a) found that voters punish incumbents in response to droughts, floods, and other natural disasters. Grose and Oppenheimer (2007) and Karol and Miguel (2007) observed that military deaths harm incumbents' reelection prospects. Berry and Howell (2007) found that citizens vote against incumbent school board members in response to falling test scores, but only when the media make education issues salient in voters' minds. As mentioned above, none of this existing literature has directly tied voting behavior to government actions related to these events, nor has it determined the welfare costs of lapses in accountability.

Another body of research has argued that government policies are strongly driven by politicians' desires for reelection. Even if politicians have higher-minded goals of pursuing sound and beneficial public policies, reelection is the proximate objective and elections serve as a mechanism to select for "single-minded seekers of reelection" (Mayhew, 1974). Previous research has found evidence for this proposition even in seemingly non-ideological domains in which

politicians are not pandering to narrow constituencies, such as disaster relief and prevention. For example, Cohen and Werker (2008) describe how factors ranging from a government's preferences to the localized nature of most natural disasters can lead to underinvestment in disaster prevention. Garrett and Sobel (2003) find that nearly half of all Federal Emergency Management Administration (FEMA) disaster relief payments were motivated by political considerations as opposed to need. Besley and Burgess (2002) show that politicians respond more aggressively to crises when there is a greater media presence, arguing that an active media is important for creating electoral accountability. Even the decision to make a presidential disaster declaration, a prerequisite for the availability of federal aid in some cases, appears to be politically motivated. Presidential disaster declarations go up markedly in election years (Sobel and Leeson, 2008). Finally, Chen (2008) finds evidence suggesting that FEMA may have strategically delivered grants to maximize vote share and buy turnout for the incumbent administration after the Florida hurricane season in 2004. Taken together, the extant literature underscores the importance of political implications in determining how governments respond to disasters.[2]

More generally, the literature shows that politicians respond to electoral incentives in all policy areas. Some of the best examples of this line of research have leveraged gubernatorial term limits in the United States to identify the relationship between incentives and policy. For example, List and Sturm (2006) use gubernatorial term limits to show that electoral incentives affect policy even on matters of secondary interest to voters where it is often assumed that special interests have a greater impact than do voters. Using a similar empirical strategy, Besley and Case (1995) argue that governors who are not term-limited, and thus have electoral incentives, respond more vigorously to disasters.

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[2] See Cole, Healy, and Werker (2008) for evidence that voters are more sensitive to government responses to more severe disasters.

In this study, we tie these two literatures together via an empirical analysis of government responses to natural disasters and voter responses to government action. Similar to how voters reward politicians for positive economic conditions such as economic growth and low unemployment, we find that incumbents can gain votes by providing disaster relief spending. At the same time, voters are not responsive to prevention spending, which leads to public welfare losses since investment in prevention significantly reduces future damage. In making policy, reelection-minded politicians will react to this incentive structure by focusing more on relief than on prevention compared to a welfare-maximizing outcome.

Why do citizens behave in this fashion? Based on existing research, we hypothesize that voters may treat relief and prevention efforts in different ways because prevention spending is difficult to observe and the benefits are not realized immediately whereas the benefits of relief spending, which often come in the form of direct payments, are highly salient and immediate. Relief spending is thus similar to traditional forms of particularistic distributive spending, which has been found to enhance politicians' electoral fortunes (Levitt and Snyder, 1997). In general, citizens have been found to be disinterested in politics, and to possess limited information about the specifics of public policies (e.g. Delli Carpini and Keeter, 1996; Kuklinski et al., 2000). Hence, seeking out information about prevention programs and their expected benefits may be too cognitively demanding for most citizens. Alternatively, responding positively to direct financial payments after a traumatic disaster takes place is a much simpler heuristic. Moreover, the news media often publicize relief efforts after newsworthy disasters take place, but often do not discuss more pedestrian prevention efforts. Since the news media have been found to have the power to set the agenda on the issues which citizens use to evaluate politicians (e.g., Iyengar and Kinder 1987; Miller and Krosnick 2000), citizens may be primed to consider relief spending when evaluating government performance. Berry and Howell (2007) also suggest this mechanism. Voters only

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reward/punish incumbents in local school board races for higher/lower test scores when the media make such information salient in voters' minds.

Empirical Strategy and Data

Data

In this paper, we analyze data on voting behavior, disaster damage, and federal government spending for the 3,141 counties or county-equivalent units in the United States.[3] All regressions in this paper are run at the county level, and include both county and year fixed effects. We describe each of the data sources in detail in the data appendix, and we briefly introduce those sources here.

For the voting data, we focus on presidential election results, since the executive branch has substantial control over providing relief and prevention spending via FEMA and other agencies. To cover the same time frame as is covered by the spending data, we use the 1988 through 2004 election results to construct the dependent variables in the voting regressions. The 1984 election results are also included in the analysis, since some of the regressions include the incumbent party's vote share in the previous election as a control. The election results from 1992 to 2004 come from Congressional Quarterly's Voting and Elections collection. The 1984 and 1988 results are from the Inter-university Consortium for Political and Social Research (ICPSR).

The spending data come from the Census Bureau's Consolidated Federal Funds Report (CFFR). The CFFR describes all federal expenditures in "the following categories: grants, salaries and wages, procurement contracts, direct payments for individuals, other direct payments, direct

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[3] The Census Bureau describes there being 3,141 counties and county-equivalent units in the United States (see http://www.census.gov/Press-Release/www/releases/archives/facts_for_features_special_editions/000795.html), although other authors have cited the number as 3,155 (Ansolabehere, Gerber, and Snyder, 2002). Parishes in Louisiana and boroughs in Alaska are equivalent to counties in the other 48 states. In addition to counties, there are some county-equivalent units that consist of census areas in Alaska, along with independent cities that have no county affiliation and are primarily in Virginia.

loans, guaranteed or insured loans, and insurance."[4] Each spending item includes the county in which the expenditure occurs and a five-digit code to identify the purpose of the spending. The first two digits of the spending code identify the agency (e.g. FEMA) doing the spending. The entire five-digit code describes the program under which the allocation was made. The Catalog of Federal Domestic Assistance (CFDA) contains names for each of the spending codes and more detailed descriptions of the spending programs in most cases. The catalog descriptions make it possible to identify both disaster relief spending and disaster prevention spending.

In Table 1, we describe all of the programs that we have classified as relief and prevention spending for which there are observations in the data. For some spending codes that appear to be disaster relief or prevention, no actual expenditures actually occurred, and so we do not list those expenditure categories. For each category in which expenditures occurred, we include the program name and, if it is available, a brief description of the program that is excerpted from the full description available from the CFDA. As Table 1 shows, many of these expenditures were made by FEMA, but there are also expenditures from the Department of Commerce (DOC), Health and Human Services (HHS), and six different expenditure categories under the Department of Agriculture (USDA).

The first part of Table 1 refers to relief spending items. Almost all of the disaster relief spending items consist of direct payments made to affected individuals.[5] Altogether, 99% of relief spending consisted of direct payments. All of the relief spending has in common its *ex-post* nature. Each category refers to spending that deals with the consequences of a disaster. Some, but not all, of the expenditure categories require a presidential disaster declaration. The second part of Table 1 describes the government's *ex-ante* expenditures to either prevent disasters or mitigate their

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[4] The Census Bureau description of the data can be accessed at: http://www.census.gov/govs/www/cffr.html.
[5] The Department of Agriculture and Small Business Administration administer loan programs in response to disasters that we do not include in the spending measures. We include only grants to individuals and local governments to construct our measure of disaster relief.

impacts. These prevention spending categories include items to increase flood protection, training of firefighters, and contingency planning for earthquakes and hurricanes. All of the spending data is at the county level. In the analysis, we focus on the aggregate relief and prevention spending that the federal government allocates in a given unit of time.

Following the attacks of September 11th, there was a dramatic increase in prevention spending devoted to firefighting. Although the vast majority of the firefighting grants were administered by the Department of Homeland Security's Office for State and Local Government Coordination and Preparedness, which is responsible for coordinating preparation for terrorism, many of the purchases made under the program could also be useful in dealing with natural disasters. Since it is unclear whether these expenditures should be included in prevention spending for natural disasters, we conducted our analyses both including and excluding the firefighting expenditures. We present summary statistics of data that both include and exclude spending on firefighting, but exclude the firefighting expenditures when estimating the regression models. The parameter estimates are very similar to those obtained by including firefighting spending, both in terms of statistical significance and substantive meaning.

To supplement the spending data, we use the comprehensive data on natural disasters contained in the Spatial Hazard Events and Losses Database for the United States (SHELDUS), collected by the Hazards and Research Vulnerability Institute at the University of South Carolina. The database contains estimates of the amount of damage to property and crops caused by natural disasters. Among the events included in the data are severe thunderstorms, earthquakes, hurricanes, tornadoes, and floods. All events that caused damages of at least $50,000 are included in the database.

We combine the voting, spending, and disaster damage data in each county for which it is possible to do so.[6] For some disasters, the data do not break damage down by county. For example, if a disaster affected ten counties and caused a total of $1,000,000 in damage and two fatalities, the data for some disasters will simply express damage as $100,000 and fatalities as 0.2 for each of the ten counties. Since damage is apportioned evenly for the ten counties, this has the effect of making per-capita damage appear larger for small counties, since they are allocated the same amount of damage as larger counties, with the damage being spread out over fewer people. To deal with this issue, we have taken the total damage associated with each individual disaster and apportioned it according to the population in each affected county. Continuing with the above example, if a county has 1% of the combined population in the ten affected counties, we would assign 1% of the damage (here $10,000) to that county. Our results remain essentially the same whether we use this measure of damage or the measure obtained by apportioning damage equally to all affected counties.

Empirical Strategy

To estimate preferences in a county for the incumbent party at time *t*, we use the percentage of the presidential vote for the incumbent party in the county, *c*, which we denote by *IncumVote$_{ct}$*. Alternatively, one could consider only votes for the two major parties (as in, for example, Markus, 1988). Results are nearly identical using the incumbent party's share of the two-party vote, which is to be expected since we include year effects that will account for the overall strength of third-party opposition in a given year.

To measure disaster relief spending, we use the total relief spending in a state in the three years preceding an election. Total relief spending is the sum of all spending in the expenditure categories listed under relief spending in Table 1. To deal with the heavy skew in the spending

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[6] Because Alaska does not report election results at the county level, it is excluded from the analysis.

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measures, we follow other authors (e.g., Ansolabehere, Gerber, and Snyder, 2002) and convert the spending measures into logarithms. The spending measure that we use is the logarithm of per-capita spending plus one. The one is added so that, when we take the logarithm, the relief spending measure is mapped back to zero for the case of zero spending. In equation form, where *RelSpend* is relief spending in inflation-adjusted January 2008 dollars and *Population* is the county's population, we use the following measure of relief spending:

$$Relief = \ln\left(\frac{RelSpend}{Population} + 1\right) \tag{1}$$

We operationalize prevention spending and disaster damage similarly. For each variable, we examine the amounts in the three years preceding the election. We consider spending and damage three years before the election because we are interested in estimating how effective prevention spending is in preventing future damage, and we want to consider prevention spending that takes place beyond just election years. To make those regressions comparable to the ones we use to estimate voting behavior, we consider spending and damage in the three years preceding an election. However, since evidence suggests that it is election-year economic conditions that influence voters' decisions (Fair, 1978; Achen and Bartels, 2004b), it is reasonable to expect that spending and disaster damage in the year before the election might particularly affect voting behavior. Due to this concern, we also considered specifications that included only spending and damage in the year preceding the election. Those results (not reported) are similar, with the estimated effects of relief spending on voting slightly larger when looking only at spending in the year immediately preceding the election, as previous research would suggest.

To model the voting decision, we start by extending the baseline model that many previous researchers (e.g. Markus, 1988, 1992; Partin, 1995; Nadeau and Lewis-Beck, 2001; Achen and Bartels, 2004b) have used to estimate voting behavior as a function of economic conditions,

allowing for a voter's decision to depend specifically on the actions that the government takes for disaster relief and prevention. We aggregate the individual voting decision to the county level for estimation purposes. The main voting results are obtained by estimating the following equation at the county level for the five presidential elections from 1988 through 2004:

$$
\begin{aligned}
IncumVote_{ct} = \alpha + \gamma_c + \eta_t + \lambda CountyConditions_{ct} + \beta_1 Damage_{ct} \\
\beta_2 Relief_{ct} + \beta_3 Prevention_{ct} + \beta_4 IncumVote_{c(t-1)} + u_{ct}
\end{aligned}
\tag{2}
$$

In the equation, relief, damage, and prevention are each their respective amounts in the three years preceding the election. To account for time-invariant county characteristics, we include county fixed effects, γ_c, in each regression. To control for national-level conditions in a given year, such as the identity of the incumbent party and the national growth rate in personal income, we include time fixed effects, η_t. The inclusion of the county and year fixed effects ensures that our coefficients of interest are identified by variation in spending at the county level in a given election cycle.

In our basic specifications, we include the fixed effects and the damage and spending variables. We then expand the regression to include the incumbent party's previous vote share in the county and *CountyConditions*, which are county-level variables that can change over time. These conditions include items such as county-level changes in economic conditions since the previous election and a county's racial composition. Following the extant literature, we also consider the interaction of racial composition with the identity of the incumbent party. The data on county demographic characteristics come from the Census Bureau's Historical Population Estimates series. The data on county-level income come from the Census Bureau's Small Area Income and Poverty Estimates.

<center>**Results**</center>

Summary Statistics

Natural disasters of some type affect most of the country, and the amount of damage they cause is substantial. For 95% of the county-election observations in the data, there is at least one natural event causing damage of $50,000 or more during the three years before an election. In some locations and at certain times, disaster damage is small and no federal relief spending occurs. Federal relief spending is positive for 45% of the observations in the data set. Prevention spending is positive for 35% of the observations in the data set. From 1985 to 2004, the average annual damage caused to property and crops averaged $16.5 billion per year (in January 2008 prices). Over the same set of years, the government has spent an average of $3.05 billion on disaster relief and $195 million on disaster prevention, annually.

Table 2 shows the time trends for disaster relief and prevention spending since 1988. As shown in the table, the disparity between the amount that the government spends on disaster relief and the amount that it spends on disaster prevention has grown considerably. Per-capita relief spending was $5.00 in the three years preceding the 1988 election. By the 2004 election cycle, relief spending had increased to $68.18 per person. If we consider the amount spent on relief by the federal government per dollar of disaster damage, this ratio increased more than five times from .111 for the three years preceding the 1988 election to .586 for 2004.[7] In other words, in the years preceding the 2004 election the federal government spent 59 cents in relief for every dollar in disaster damage, while it only spent 11 cents in the years before the 1988 election.

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[7] Disaster damage varied considerably over the five election cycles considered in this paper, with the 1996 election cycle witnessing considerably more damage than the other four. Total damage in the three years preceding the 1988 election was $10.8 billion, with damage rising to $28.9 billion for the 1992 election cycle. The damage amounts for the three years preceding the 1996, 2000, and 2004 elections were $61.8 billion, $46.7 billion, and $32.4 billion, respectively.

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As described earlier, prevention spending follows a very different path over time. Prevention spending on hurricanes, floods, and earthquakes (excluding firefighting expenses) fell during the sample period. Per-capita prevention spending was $1.41 in the years preceding the 1988 election, somewhat lower during the following two election cycles, and it then increased to $1.59 for 2000 before falling to $0.53 for 2004. A shift in priorities towards responding to terrorism appears to explain the fall in natural disaster prevention spending during the Bush Administration's first term. If we include fire prevention spending in overall prevention spending, most of which was related to terrorism for the 2004 election cycle, we find that per-capita prevention spending increased to $3.55 during Bush's first term.[8] Even this increase is still much smaller than the rise in relief spending over the same time period. After spending $0.28 in prevention for every dollar spent on relief during the 1988 election cycle, the federal government spent only $0.05 on prevention for every dollar spent on relief in the 2004 election cycle. If firefighting spending is excluded from prevention, spending fell precipitously in the 2004 election cycle to only $0.01 on prevention for every dollar spent on relief.

The Value of Prevention

Irrespective of how prevention is measured, the summary statistics in Table 2 show a large increase in relief expenditures relative to prevention expenditures. Did this shift in priorities come with efficiency losses? Our experiences with Hurricane Katrina and other disasters certainly suggest an underinvestment in disaster prevention in earlier years. Here, we attempt to quantify the extent of the underinvestment by estimating the effectiveness of previous government prevention efforts.

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[8] Almost all of the firefighting spending in the 2004 election cycle, and 84% of total prevention spending, was administered by the Office for State and Local Government Coordination and Preparedness, an arm of DHS responsible for dealing with terrorism.

To estimate the effectiveness of government prevention spending, we regress disaster damage in a county on prevention spending during the previous two election cycles, as well as county and year fixed effects. We also consider a specification that includes relief spending and damage during the previous two election cycles as additional independent variables. The inclusion of relief spending in the regression makes it possible to test the hypothesis that relief may itself affect future damage. The presence of lagged damage accounts for mean reversion in damage.

As the results in Table 3 show, prevention spending has led to significant reductions in disaster damage. The first column reports the results obtained by regressing disaster damage on prevention spending in the previous two election cycles, as well as county and year fixed effects. The coefficient estimate of -.152 indicates that a 1% increase in prevention spending during the previous two election cycles led to a .152% fall in damage during the current cycle. This estimate falls somewhat, but is still significant, when we include relief spending and damage as control variables. These results in the second column also show that relief spending has no significant effect on future damage and that, not surprisingly, there is significant mean reversion in disaster damage, as indicated by the negative coefficient for lagged damage. The coefficient for prevention is -.104, suggesting that a 1% increase in prevention spending during the previous two election cycles led to a .104% fall in damage during the current cycle.

Using this more conservative estimate of the impact of prevention spending on future damage, we can estimate the value of a dollar of prevention spending. The coefficient estimate of -.104 suggests that a $1 increase in prevention spending resulted in an approximately $7.30 decrease in disaster damage, and this estimate captures only the reduction in damage that occurs during a single election cycle.[9] We can use this figure to estimate the net present value of a dollar of

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[9] Total damage in the 1996, 2000, and 2004 election cycles averaged $47.0 billion per cycle. We estimate the average annual damage prevented by a 1% increase in prevention spending during the previous two election cycles to be .01*.104*$47.0 billion = $48.9 million. Total non-firefighting prevention spending over the previous two election

prevention spending in terms of reduced future damage from natural disasters. Assuming a 4% annual interest rate and a 6% depreciation rate for prevention investments,[10] we estimate that the net present value (NPV) of $1 of disaster prevention to be about $15.[11]

In columns (3) and (4), we consider the effectiveness of prevention projects that were started and completed within a single presidential term. We break down prevention in this way because in later tables we will estimate the effect that prevention has on voting behavior. Since some prevention projects may start in one presidential term and end in another, we want to separately consider prevention efforts that are confined to a single presidential term. We refer to this sort of spending as *flow prevention*. Approximately 45% of total prevention spending falls into the flow category. The results in columns (3) and (4) show that this sort of prevention spending is about equally successful in preventing future damage as overall prevention spending.

While our estimates strongly suggest that prevention spending reduces a substantial amount of future damage, it is important to note that the estimated effect represents the *average* effect of prevention spending on disaster damage. If the government pursues only the most urgent prevention projects, the *marginal* effect associated with the government pursuing the best project that it currently ignores will be somewhat less than the average effect. In addition, our estimate of the effectiveness of prevention has a relatively wide confidence interval. Nevertheless, the results clearly demonstrate that prevention spending significantly reduces future losses. Along with the anecdotal evidence on our recent failure to adequately prepare for disasters, our findings strongly suggest that there are large efficiency losses associated with underinvestment in disaster prevention.

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cycles averaged $670 million, so that we estimate a 1% increase in prevention to be .01*$670 million= $6.7 million. Combining these results, the estimated effect of a dollar of prevention spending is $\left(\dfrac{\$48.9 \text{ million}}{\$6.7 \text{ million}} \right) = \$7.30$.

[10] Nadiri and Prucha (1996) estimate 5.9% to be the annual depreciation rate of physical capital for US manufacturing.

[11] A 4% annual interest rate translates to 17% over a four year period. A 6% annual capital depreciation rate translates to 26% over a four-year period. The NPV of $1 of prevention is then $\dfrac{1}{(1.17)(1.26)-1}(\$7.30) = \dfrac{1}{0.48}(\$7.30) = \$15.21$.

The Determinants of Disaster Spending

Before describing the effects that relief and prevention spending have on voting behavior, we examine the determinants of spending. To do this, we regress our relief and prevention spending measures on disaster damage, the incumbent party's previous vote share in the county, as well as county and year fixed effects. We include the incumbent party's previous vote share to account for the fact that more government spending on disasters is directed to areas that give more electoral support to the incumbent party (Chen, 2008).

The results in Table 4 show that government relief expenditures are significantly affected by disaster damage, as one would expect. When disaster damage increases by 1%, the regression results suggest that relief expenditures increase by .072%. There appears to be a significant political component to disaster relief spending, too. The results in Table 4 show that counties that more strongly supported the incumbent party in the previous election received more relief spending than did other counties, an effect that would not occur if relief spending only reflected response to disasters. The effect of previous vote share on spending is substantial and highly significant ($p <$ .001). The coefficient of .018 indicates that a one percentage point increase in the incumbent's previous vote share in the county leads to an average increase of 1.8% in the amount of relief spending that a county receives. The results show that incumbents are either more prone to respond to disasters that occur in supportive counties or that they dress up other transfers in the guise of disaster relief and then direct those payments to supportive areas.[12]

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[12] Chen (2008) found that directing resources to supportive areas was a vote-maximizing strategy for the Bush administration to follow with regards to 2004 hurricane relief in Florida because voters were more sensitive to spending in Republican areas than in Democratic ones.

In contrast to relief spending, the second part of Table 4 indicates that prevention spending does not significantly respond to disaster damage.[13] Still, the coefficient is positive, suggesting that prevention spending may go up somewhat after a disaster. The coefficient is about one-sixth the size of the effect that damage has on relief spending. The result in column (2) of Table 4 shows that counties that supported the incumbent party are not significantly more likely to receive disaster prevention spending, and the point estimate is close to zero. The fact that the effect of political considerations on spending is much smaller for prevention spending than for relief spending follows logically from the results that follow, which demonstrate that voters do not value prevention spending.

Voter Responses to Relief and Prevention Spending

Figures 1 and 2 display graphically the results of nonparametric regressions of the incumbent party's vote share on disaster relief and prevention spending. We use the nonparametric regressions (created with Stata's *lpoly* command) to illustrate the general shape of the relationship between incumbent party vote share and spending. The figures suggest a correlation between relief spending and support for the incumbent administration, as well as the absence of such a relationship between prevention spending and incumbent support. Figure 1 shows two curves, one for the relationship between the incumbent party's vote share and relief spending, and the other for the relationship between the incumbent party's vote share and prevention spending. The curve for relief spending suggests a potentially strong relationship between voting behavior and spending; the curve starts at around 45% for zero spending and increases almost monotonically until peaking at around 70% for the 99th percentile of spending, which was around $1700 per person for the three years

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[13] The insignificant coefficient for damage occurs even though, in the early years of the data, it was official government policy, under the Hazard Mitigation Grant Program, for some disaster prevention spending to be dedicated to areas that had experienced recent damage. This policy was formally changed in 1995 (Wachtendorf, et al., 2002, 11).

preceding an election. On the other hand, the curve for prevention spending is nearly flat, suggesting the absence of a relationship between voting decisions and prevention spending.

The strong relationship between voting decisions and relief spending seen in Figure 1 is suggestive, but the politicized nature of relief spending seen in Table 4 underscores the idea that much of the observed correlation is not causal. In other words, the curve in Figure 1 reflects to some extent the fact that areas that are already predisposed to vote for the incumbent party receive higher amounts of relief. The curve thus picks up some of this effect of prior voting behavior on current voting behavior, in addition to any effect of spending on voting behavior.

To better isolate the effect of spending on voter decisions, we also run nonparametric regressions of the *change* in the incumbent party's county-level vote share from the previous election to the current one on the levels of relief and prevention spending in the county. Figure 2 shows the results of the regressions. Even controlling for the vote share that the incumbent party received in the previous election, the figure shows a remarkably strong relationship between voter behavior and relief spending. On average, the incumbent party loses about 5 percentage points in counties where relief spending is near zero, but it gains about 2 percentage points in counties where relief spending is very high. The relationship between the change in the incumbent party vote share and prevention spending is, in contrast, nearly flat, averaging around a drop of around 4% for all levels of prevention spending. This drop of 4% reflects the regression to the mean in the incumbent party vote share. Since it won the previous election, the incumbent party by definition did well in the previous election, and it generally does less well in the current election.

The figures importantly suggest a relationship, and potentially a strong one, between voter decision-making and relief spending, as well as the absence of such a relationship between voter decisions and prevention spending. Table 5 presents the results of regressions that subject this apparent relationship to a variety of controls. The first column reports the results obtained by

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including county and year fixed effects in a regression of the incumbent party vote share on disaster damage, relief spending, prevention spending, and the incumbent party's previous vote share in the county. The second column expands the regression to include lagged prevention spending as well as an interaction between prevention spending and disaster damage. Since prevention spending has been shown to reduce damage, including the interaction between lagged prevention and damage allows us to account for the fact that damage is endogenous to previous prevention spending. In the third and fourth columns, we consider analogous regressions that use the flow measure, spending on projects completed within a single presidential term, as the prevention variable.

As in the figures, the coefficients indicate that relief spending has a significant effect on voter decisions and that prevention spending has no significant effect. In the first two columns, an increase in relief spending predicts a significant increase in the incumbent party's vote share ($p = .004$ in column (1), $p < .001$ in column (2)), while prevention spending has no significant effect and the point estimate is close to zero. When we consider the flow measure of prevention, the coefficient for relief remains highly significant and prevention continues to have no significant effect. Comparing the coefficients associated with the spending variables via an F-test, the null hypothesis that relief and prevention spending have the same effect on voting can be rejected ($p < .001$ in columns (2) and (4)) in all specifications.

Damage enters insignificantly in all specifications, but its negative sign suggests that disaster damage, in general, may have some negative effect on the incumbent party, consistent with Achen and Bartels (2004a). In the second and fourth columns, the interaction between previous prevention and damage enters the regression with approximately zero coefficients, suggesting that voters do not treat damage averted due to earlier prevention any differently than they treat damage in general.

In column (4), the size of the coefficient for relief spending of .415 indicates that, on average, a 1% increase in relief spending – an increase of about .01 in the log of relief spending – increases the incumbent party's vote share by about .0042 percentage points. The *t*-statistic for this effect of 3.40 indicates that the relationship between relief spending and voting decisions is strongly significant. The estimated coefficient confirms the visual evidence in Figure 2, indicating that large increases in relief spending are predicted to lead to substantial vote gains for the incumbent party. If the incumbent party increases relief expenditures in a county from $1 per person (the 66th percentile for spending in the data) to $10 per person (the 93rd percentile in the data), the regression results estimate that the incumbent party will gain about 0.71 percentage points in the county.[14] Another way to think about this estimated effect is in terms of the estimated amount of disaster spending required to buy an additional vote. The estimated coefficient suggests that approximately an additional $29,000 in relief spending will buy one additional vote.[15] This effect is of a roughly similar and slightly largeer magnitude to the average amount that Chen (2008) found that it cost to buy a vote with relief for Florida hurricanes in 2004.

Do Voters Reward Relief Spending More When a Disaster Actually Happens?

In Table 6, we expand the specification to include a variety of controls for time-varying county characteristics. We include controls for the share of nonwhite voters in the county, an interaction between the identity of the incumbent party and the share of nonwhite voters in the county, as well as the growth in median household income in the county during the preceding

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[14] This estimate comes from multiplying the coefficient of 0.415 for the relief spending measure in Table 6 by the change in the relief spending measure (ln (10+1) – ln (1+1)).

[15] This calculation can be done as follows. A 1% increase in relief spending over an election cycle is approximately $122 million (.01*$3.05 billion/year*4 years). Given that approximately 100 million people voted, on average, in the five election years in the data, a 1% increase in relief spending is estimated to gain about .000042*(100 million votes) = 4,200 votes for the incumbent party. Combining these numbers gives an estimate of $\frac{\$122 \text{ million}}{4,600 \text{ votes}} = \$28,900 / \text{vote}$.

election cycle.[16] This set of controls is similar to the one Markus (1988) used to examine individual voting decisions. Like Markus, we define the incumbent party variable to be +1 if there is a Democratic incumbent and -1 if there is a Republican incumbent.[17]

The results obtained by including these additional controls are shown in columns (1) and (2) of Table 6. In column (2), we also control for interaction terms that consider the possibility that voters respond differently to spending that is actually addressing disaster damage and other spending that is not in response to a disaster. Specifically, we interact a dummy variable for zero disaster damage in the county with the relief and prevention spending measures. If voters are seeking competent politicians, we would expect them to particularly reward relief spending that represents a response to disaster damage. On the other hand, it may be the case that voters appreciate receiving payments from the government, and reward the governments that provide those payments irrespective of the merits of those expenditures.[18] In column (3), we include terms for prevention and relief in neighboring counties. Specifically we include the amount of (log) per-capita prevention and relief in the five nearest counties according to the county centroids. We consider these specifications to account for the fact that prevention in neighboring counties may come with benefits – for example a dam built in a county that is upriver – and we want to account for the fact that voters may respond to those prevention efforts. In columns (4)-(6), we consider analogous specifications where we replace total prevention with the flow measure.

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[16] We observe county-level demographic characteristics for the following years: 1986, 1992, and 1998. We use 1986 values to proxy for the 1988 values, 1992 values to proxy for the 1992 and 1996 election cycles, and 1998 values to proxy for the 2000 and 2004 election cycles. The county-level income data are not available for 1988, 1992, and 1996, and so we use the 1989, 1993, and 1997 amounts as proxies. Since county-level income data are not available for each year, we use income growth since the previous election in the regressions.

[17] If the regression also includes a control for the total per-capita federal expenditure in the county, the coefficients on the relief and prevention variables remain almost the same.

[18] Homeland security funding provides one recent example of unnecessary government spending used to either curry electoral support or to reward supportive areas. From 2003 to 2004, Alaska received $92 per citizen, while California received $22 and Texas and Florida only $21 per person. This discrepancy allowed small communities relatively unthreatened by terrorism to purchase substantial amounts of equipment. For instance, the Northwest Arctic Borough, which has only 7,300 people, purchased $233,000 worth of gear such as headlamps, radio equipment, and night vision goggles (Murphy, 2004).

Throughout all the specifications in Table 6, relief continues to have a significant effect on voting, while prevention has no significant effect. As shown in column (1), the interaction of the percentage nonwhite variable with the identity of the incumbent party enters significantly and with the expected sign. The coefficient in column (1) of .274 indicates that if the nonwhite share of a county increases by 1 percentage point, then a Democratic incumbent will gain .274 percentage points in that county relative to a Republican incumbent. County-level per-capita income growth also enters significantly and with the expected sign. The interaction between the nonwhite share and the incumbent party dummy enters insignificantly since the previous vote share soaks up most of the variation in the incumbent party's current vote share.[19] Relief spending and growth, however, continue to be significant predictors of current vote share.

In columns (2) and (5), the interaction terms between spending and damage enter insignificantly. At the same time, the negative sign on the interaction between relief spending and disaster damage in columns (2) and (5) is what we would expect; the sign suggests that voters place less weight on relief spending that has nothing to do with disaster-caused damage. The coefficient is about one-half as large as the coefficient for the linear term in disaster damage. While we cannot reject the hypothesis that voters treat spending that has nothing to do with a disaster in the same way that they treat disaster-related spending, the coefficients suggest that voters may respond to disaster-related relief about twice as strongly as they respond to relief that has nothing to do with a disaster.

Finally, the results in columns (3) and (6) indicate that voters do not significantly respond to relief or prevention spending in neighboring counties. Voters do not reward neither prevention in their own county nor prevention in nearby counties.

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[19] If previous vote share is excluded from the regression, the interaction between the nonwhite share and the Democratic incumbent dummy is highly significant with the expected sign.

The Cost of Imperfect Retrospection

Consistent across all the results is the finding that voters reward relief spending, but not prevention spending. What is the cost associated with this behavior? Here, we consider a back-of-the-envelope calculation that gives some idea of the loss in public welfare. Using the results in Table 3, we previously estimated that a $1 increase in prevention spending is associated with a reduction of about $15 in future damage in net present value terms. We can combine this estimate with the results from the voting regressions to obtain an estimate of the welfare loss resulting from voter behavior.

If politicians attempt to win just one out of every ten thousand votes, 0.01% of the vote, with politically targeted relief, our estimates in Table 5 suggest that this would involve an average expenditure of $62 million during our sample period.[20] Suppose that $1 of relief offsets $1 of disaster damage. Given this assumption, a dollar of prevention spending is worth about 14 dollars more than a dollar of relief in terms of social welfare. If politicians devoted this $62 million to prevention spending instead of to relief, our estimates suggest that voters would realize a welfare gain of about $880 million. Even a modest political incentive for prevention spending – enough to cause politicians to shift just the amount of relief that wins 0.01% of the vote from relief to prevention – could thus cause a substantial increase in social welfare.

Confirming Causality: Matching

The inclusion of county and year fixed effects and the incumbent party's previous vote share should ensure that our estimates for relief and prevention are unbiased. To confirm that our

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[20] To increase vote share by 0.01 percentage points, the regression estimate of .458 from Table 4 suggests that the log of per-capita relief spending must go up by 0.022, since $\frac{0.01}{0.458} = 0.022$. An increase of 0.022 in log per-capita relief corresponds to an increase in per-capita relief from about $10.15 per year (the mean level in our data) to $10.38 per year. This increase of $0.23 per capita corresponds to an increase of about $62 million, given average population of 268 million during the sample period.

estimates accurately reflect the effect that spending has on voting, we also consider estimates obtained by matching on the other covariates that are included in the regression. While most matching procedures consider bivariate treatment variables, our continuous treatment variables require another method. We employ the procedure developed in Imai and Van Dyk (2004) to conduct causal inference through matching on continuous treatment variables.

The method is relatively straightforward. The idea behind this procedure is to compare spending between counties that have similar levels of predicted spending according to damage and other variables. In step one, we regress spending on the other covariates in Table 6 along with population. We cannot include county dummies in the procedure, so we consider the deviation of each variable from its mean over time, except for the election dummies and the incumbent party's previous vote share in the county. The predicted values from this regression are used to break the data into five classes according to predicted spending. In step two, we separately regress the incumbent party's vote share in the county on all the covariates for each of the five classes. We take the weighted mean of the coefficients obtained to estimate the average treatment effect associated with spending. To get standard errors, we conduct 1000 bootstrap simulations that account for clustering at the state*year level.

The results are reported in Table 7. The matching procedure gives an estimate for relief spending of .316 with a standard error of .092. This estimate is a little larger than the estimate of .274 seen in column (1) of Table 6. For prevention spending, the matching procedure gives an estimate of -.065 with a standard error of .090. This estimate is slightly closer to zero than the estimate of -.107 seen in column (1) of Table 6. The matching results thus confirm the earlier regression results that relief spending has a significant effect on voter decisions, while prevention spending has no effect.

Creating Electoral Incentives for Disaster Prevention: The Case of Project Impact

Here, we attempt to shed some light on the reason why voters usually fail to reward valuable prevention spending. To do this, we examine Project Impact (originally called the Disaster Resistant Communities Initiative), a disaster prevention initiative that FEMA announced in October 1997 and began to implement in 1998. Under Project Impact, FEMA selected a group of seven areas from across the country as pilot cases to receive $1,000,000 in funds to pursue disaster prevention efforts. In late 1997, FEMA selected an additional fifty communities to participate in the first wave of Project Impact. These communities received fewer funds and less attention from the national organization than did the pilot communities (Wachtendorf and Tierney, 2001). Still, significant efforts were undertaken and appropriations averaged about $400,000 per community per year at the peak of the program.[21] A complete list of the Project Impact communities can be found in Table 8.

The idea behind the program was to bring "businesses, citizens, and communities" together in the cause of disaster prevention and mitigation.[22] At its height, the federal government allocated only about $20 million to the project (Holdeman, 2005), but the design of the program makes it possible to show how a certain kind of disaster prevention initiative can reach voters. A variety of prevention and mitigation efforts were pursued under Project Impact, ranging from educational efforts at the community level to funding hurricane straps for a community's disaster shelter, with each project having in common its focus on cooperation between individuals, businesses, and government. A simple way to describe Project Impact is as a grass-roots approach to disaster mitigation, involving the community in the prevention process.

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[21] Even the small amounts allocated under Project Impact appear to have had an effect. For example, the efforts undertaken in King County, Washington, were credited with helping to limit damage from the Nisqually earthquake on February 28, 2001. Ironically, it was announced that Project Impact would be discontinued on that same day (Holdeman, 2005).
[22] As FEMA Director James Lee Witt (1998) described it, there were "three tenets" behind Project Impact: 1) Mitigation is a local issue. It is best addressed through a local partnership involving government, business and private citizens. 2) Private sector participation is essential. Disasters threaten the economic and commercial growth of our cities, towns, villages and counties. Without the participation of the private sector, comprehensive solutions will not be developed. 3) Mitigation is a long-term effort that requires long-term investment. Disaster losses will not be eliminated overnight.

To isolate the effect that Project Impact had on voting behavior, it would be optimal if it was randomly assigned to certain areas via a lottery. Although Project Impact was not explicitly assigned in this way, it turns out that the program communities show little evidence of selection on observable variables. In Panel A of Table 9, we run a linear regression where the dependent variable is a dummy for whether a community participated in Project Impact and the dependent variables are state fixed effects and a variety of controls that we employed in previous regressions. Results are essentially identical if a logistic regression is used instead. None of these variables significantly predicts whether a community was chosen for Project Impact (either individually or jointly), and all coefficients are close to zero. For example, counties that more strongly supported Bill Clinton in 1996 were not more likely to be chosen for Project Impact, meaning that the government did not strategically reward supportive areas.

To estimate the effect that Project Impact had on voters, we regress the change in the incumbent Democratic Party's vote share in the county from 1996 to 2000 on a dummy for whether a county participated in Project Impact. We consider specifications that include state fixed effects and specifications that include other controls. We report these results in Panel B of Table 9. The result in column (1) of Table 9 suggests that Project Impact counties gave approximately 1% more of their votes to the incumbent party than did other counties. This estimate increases to 1.18% in column (2), where we introduce damage, demographics, and economic growth as control variables. In columns (3) and (4), we consider analogous regressions that also include state dummies. Including the state dummies reduces the estimated effect of Project Impact to 0.77% in the specification that includes all of the controls. Across all specifications, Project Impact is estimated to have significantly increased the Democrats' vote share from 1996 to 2000.

The case of Project Impact illustrates that prevention spending, even when not massive in size, can provide electoral benefits for the government that undertakes it. The fact that Project

Impact connected with voters suggests that voters are capable of valuing actions with future benefits if prevention efforts are made salient. A government interested in both maximizing public welfare and in winning reelection should thus be able to further its pursuit of both those objectives by engaging in effectively-communicated prevention spending that encourages public participation.

Discussion

While voters appear to have responded significantly to a prevention program that emphasized communication and community involvement, that behavior stands in stark contrast to the ambivalence they display for prevention spending in general. A government responding to these incentives will shift resources toward relief and away from prevention, when deciding between the two. The failure of voters to effectively hold government accountable appears to contribute to significant inefficiencies in government spending, since the results show that prevention spending substantially reduces future disaster damage. Hence, this lapse in democratic accountability reduces public welfare by discouraging reelection-minded politicians from investing in protection while encouraging them to send in the cavalry after damage has been done and lives have been lost.

Our results apply more broadly to any area where prevention might matter. If electoral incentives encourage poor policy decisions with respect to natural disaster prevention, a similar explanation likely helps to cause policy failures with respect to preventative medicine or preventative action with respect to climate change. To the extent that voters also do not reward prevention in these areas, we can expect politicians to devote an inefficiently low amount of attention and resources to these sorts of efforts.

Our findings advance the study of retrospective voting and political behavior in several ways. First, by studying temporally isolated and random shocks such as natural disasters, we have attempted to better understand the dynamics of accountability, a task that is more difficult when

studying the economy. Although there appears to be a strong link between some government actions and voter reward/punishment, this relationship actually creates perverse incentives for elected officials instead of producing welfare-maximizing outcomes. Second, we go beyond previous research that has also found voters to be limited in their abilities to hold accountable their elected officials and demonstrate the apparent costs of such behavior. Whereas previous studies have focused on voter responses to conditions and outcomes in isolation, we simultaneously estimate the determinants of voter decisions, government policy decisions, and the outcomes associated with those policies. This empirical strategy enables us to draw inferences about how politicians' actions affect voting behavior, and vice versa. By estimating the determinants of voter behavior, the determinants of relief and prevention spending, and also the effectiveness of government prevention, we obtain results that strongly suggest that failures to adequately prevent damage from disasters like Hurricane Katrina likely stem from the absence of a political imperative to do better. Finally, we present a potential mechanism for improving voters' abilities to hold government accountable and to incentivize it to maximize public welfare. If prevention efforts are made visible and salient, citizens may be able to reward politicians more appropriately.

There are several possibilities for future research to build on the results presented here. Most obviously, scholars can explore other domains in which retrospective voting behavior leads to tradeoffs between reelection prospects and sound public policy. Future work could apply our empirical strategy of simultaneously examining voting decisions, government policy, and associated outcomes to issues such as education or health care. Additionally, future research could further explore the mechanisms behind our findings. For instance, does education or information enhance accountability? Does increased salience of prevention information lead voters to reward politicians? Our results regarding Project Impact suggest the answer to each of these questions may be yes. Such questions may be further explored using surveys as well as laboratory and field experiments to

complement the evidence presented here from a naturalistic context. If we acknowledge that there are important limitations to electoral accountability, this line of research will assist us in understanding what can be done to help voters do better.

Appendix 1: Data Sources

Elections Data: For 1984 and 1988, the county-level voting data come from *General Election Data for the United States, 1950-1990*, a database held at the Inter-university Consortium for Political and Social Research (ICPSR). For 1992-2004, the voting data are from Congressional Quarterly's Voting and Elections Collection.

Natural Disaster Data: The data on natural disaster events and damages for 1984-2004 come from the Spatial Hazard Events and Losses Database for the United States (SHELDUS), an online database hosted by the University of South Carolina. The data can be accessed at: http://www.sheldus.org.

Government Spending Data: The government spending data come from the *Consolidated Federal Funds Report*. For 1993-2004, the data come from the U.S. Census Bureau's online database: http://www.census.gov/govs/www/cffr.html. For 1985-1992, the data are held at the ICPSR. The spending categories are identified using the five-digit code names provided by the Catalog of Federal Domestic Assistance. These data can be accessed through the historical index link at: http://www.cfda.gov. The data on per-capita federal government spending come from *Historical, Demographic, Economic, and Social Data: The United States, 1790-2000*, a database held at the ICPSR.

County Income Data: The county-level income data come from the Small Area Income and Poverty Estimates constructed by the Census Bureau, primarily from the Current Population Survey. The data can be accessed at: http://www.census.gov/hhes/www/saipe/index.html.

County Demographic Data: The data on the demographic composition of counties come from the Census Bureau's Historical Population Estimates series. The data can be accessed at: http://www.census.gov/popest/archives/.

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Relief spending

Catalog of Federal Domestic Assistance code	Name	Description
10.054 (USDA)	Emergency Conservation Program	"...to rehabilitate farmlands damaged by wind erosion, floods, hurricanes, or other natural disasters..."
10.073 (USDA)	Crop Disaster Program	"...to provide disaster assistance to producers who suffered crop losses in the 2000 crop year because of adverse weather conditions..."
10.077 (USDA)	Livestock Compensation Program	"...immediate assistance to livestock producers in counties that have received primary disaster designation due to drought…"
10.082 (USDA)	Tree Assistance Program	"...assistance to tree, bush and vine owners who have trees, bushes or vines lost by a natural disaster…"
10.444 (USDA)	Direct Housing: Natural Disaster Loans and Grants	"To assist qualified recipients to meet emergency assistance needs resulting from natural disaster…"
10.445 (USDA)	Direct Housing: Natural Disaster	"To assist qualified lower income rural families to meet emergency assistance needs resulting from natural disaster to buy, build, rehabilitate, or improve dwellings in rural areas."
11.477 (DOC)	Fisheries Disaster Relief	"To deal with commercial fishery failures due to fishery resource disasters… . Disaster causes may be (a) natural; (b) man-made…"
83.516 (FEMA)	Disaster Assistance: General heading, split into more specific categories in 1997	
83.542 (FEMA)	Fire Suppression Assistance	"To provide grants to States, Indian tribal governments and local governments… for the mitigation, management, and control, of any fire…"
83.543 (FEMA)	Individual and Family Grants	
83.544 (FEMA)	Public Assistance Grants	"To assist State and local governments in recovering from the devastating effects of disasters by providing assistance for debris removal…"

Catalog of Federal Domestic Assistance code	Name	Description
93.003 (HHS)	Public Health and Social Services Emergency Fund	"To provide supplemental funding for public health and social service emergencies."

Prevention spending

Catalog of Federal Domestic Assistance code	Name	Description
10.904 (USDA)	Watershed Protection and Flood Prevention	"To provide technical and financial assistance in carrying out works of improvement to protect, develop, and utilize the land and water resources in watersheds."
12.101 (DOD)	Beach Erosion Control Projects	"To control beach and shore erosion…"
12.106 (DOD)	Flood Control Projects	"To reduce flood damages…" (Army Corps of Engineers)
15.031 (DOI)	Indian Community Fire Protection	"To provide funds to perform fire protection services for Indian Tribal Governments…"
16.006 (DOJ)	Municipal Fire and Emergency Services Domestic Preparedness Equipment Support Program	
16.559 (DOJ)	Local Firefighting and Emergency Services Training	
83.009, 83.405 (FEMA), 97.018 (DHS)	National Fire Academy Training Assistance	"To provide travel stipends to students attending [National Fire] Academy courses."
83.203, 83.505, 83.520, 83.521, 83.535 (FEMA)	Emergency Management Performance Grants	"To assist the development, maintenance, and improvement of State and local emergency management capabilities…"
83.411 (FEMA), 97.019 (DHS)	National Fire Academy Educational Program	"To increase the professional level of the fire service and others responsible for fire prevention and control."
83.506 (FEMA)	Earthquake and Hurricane Loss Study and Contingency Planning Grants	
83.519 (FEMA)	Hazard Mitigation Assistance	
83.536 (FEMA), 97.029 (DHS)	Flood Mitigation Assistance	"To assist States and communities in implementing measures to reduce or eliminate the long-term risk of flood damage to buildings…"

83.548 (FEMA), 97.039 (DHS)	Hazard Mitigation Grant	"To provide States and local governments financial assistance to implement measures that will permanently reduce or eliminate future damages and losses from natural hazards…"
83.553 (FEMA), 97.043 (DHS)	State Fire Training Systems Grants	"To provide financial assistance to State Fire Training Systems…"
83.554 (FEMA), 97.044 (DHS)	Assistance to Firefighters Grant	"To provide financial assistance directly to fire departments and nonaffiliated EMS organizations to enhance their capabilities with respect to fire and fire-related hazards"
97.045 (DHS)	Cooperating Technical Partners	"To increase local involvement in, and ownership of, the development and maintenance of flood hazard maps…"

Notes: (1) A full description of most spending items can be found at the following internet address:

http://12.46.245.173/pls/portal30/CATALOG.PROGRAM_TEXT_RPT.SHOW?p_arg_names=prog_nbr&p_arg_values=,

where the equal sign in the above address is followed with the code of interest (e.g., 97.045).
(2) The agency codes are: USDA: United States Department of Agriculture, DOC: Department of Commerce,
FEMA: Federal Emergency Management Administration, HHS: Health and Human Services,
DHS: Department of Homeland Security, DOI: Department of the Interior, DOJ: Department of Justice

Table 2: Damage and Spending Over Time

	Election cycle				
	1988	1992	1996	2000	2004
Relief spending					
Per-capita relief spending	$5.00	$7.20	$37.42	$34.50	$68.18
Per-capita damage	$45.24	$113.54	$230.98	$166.82	$116.30
Relief spending / Disaster damage	0.111	0.063	0.162	0.207	0.586
Prevention spending (Excluding firefighting)					
Per-capita prevention spending	$1.41	$1.00	$1.11	$1.59	$0.53
Non-firefighting prevention / relief spending	0.281	0.138	0.030	0.046	0.008
Prevention spending (Including firefighting)					
Per-capita prevention spending	$1.41	$1.02	$1.22	$1.75	$3.55
Total prevention spending / relief spending	0.281	0.142	0.033	0.051	0.052

Note: Election cycle refers to the three fiscal years preceding an election. For example, the measures for the 1988 election cycle include all spending from October 1, 1985-September 30, 1988.

Table 3: The Value of Prevention Spending

Dependent variable: Disaster damage measure

	All prevention		Flow prevention	
	(1)	(2)	(3)	(4)
Prevention measure for previous	-.152**	-.104**	-.157**	-.107**
two election cycles	(.041)	(.039)	(.046)	(.044)
Relief measure for previous		-.031		-.034
two election cycles		(.036)		(.036)
Damage measure for previous		-.46**		-.46**
two election cycles		(.025)		(.025)
R^2	.503	.555	.503	.555
Number of observations	9232	9232	9232	9232

Notes: (a) Standard errors are corrected for clustering at the county level.

(b) Each regression includes county fixed effects and year fixed effects.

(c) Flow

(d) * $p < .05$, ** $p < .01$ (two-tailed)

Table 4: The Determinants of Disaster Spending

A. Dependent variable: Relief spending measure

	(1)	(2)
Disaster damage measure	.072**	.073**
	(.019)	(.018)
Incumbent party's previous		.018**
vote share in the county		(.004)
R^2	.704	.715
Number of observations	15458	15453

B. Dependent variable: Prevention spending measure

	(1)	(2)
Disaster damage measure	.012	.012
	(.007)	(.007)
Incumbent party's previous		.001
vote share in the county		(.001)
R^2	.485	.485
Number of observations	15458	15453

Notes: (a) Standard errors are corrected for clustering at the state*year level.

(b) All the regressions include county fixed effects.

Table 5: Voter Responses to Relief and Prevention Spending

Dependent variable: Presidential vote share for the incumbent party in the county

	All prevention		Flow prevention	
	(1)	(2)	(3)	(4)
Disaster damage measure	-.042	-.059	-.045	-.058
	(.076)	(.073)	(.076)	(.069)
Relief spending measure	.304**	.415**	.297**	.409**
	(.105)	(.122)	(.105)	(.123)
Prevention spending measure	-.098	-.121	.027	-.006
	(.083)	(.097)	(.039)	(.047)
Lagged prevention spending		-.068		-.045
		(.134)		(.052)
Lagged prevention spending* Disaster damage measure		.012		.001
		(.029)		(.009)
Incumbent party's previous vote share in the county	.935**	.940**	.935**	.940**
	(.013)	(.014)	(.013)	(.014)
R^2	.926	.933	.926	.933
Number of observations	15448	12339	15448	12339

Notes: (a) Standard errors are corrected for clustering at the state*year level.

(b) All the regressions include county and year fixed effects.

Dependent variable: Presidential vote share for the incumbent party in the county

	All prevention			Flow prevention		
	(1)	(2)	(3)	(4)	(5)	(6)
Disaster damage measure	-.028	-.046	-.030	-.032	-.052	-.037
	(.071)	(.076)	(.075)	(.071)	(.076)	(.072)
Relief spending measure	.274*	.296**	.270**	.265*	.288*	.263**
	(.111)	(.113)	(.082)	(.112)	(.113)	(.083)
Prevention spending measure	-.107	-.107	-.071	.028	.031	.024
	(.081)	(.08)	(.067)	(.039)	(.04)	(.032)
Incumbent party's previous	.930**	.930**	.932**	.929**	.929**	.931**
vote share in the county	(.015)	(.015)	(.016)	(.015)	(.015)	(.016)
Disaster damage = 0 *		-.159			-.155	
Relief spending measure		(.12)			(.121)	
Disaster damage = 0 *		.042			-.079	
Prevention spending measure		(.245)			(.139)	
Relief spending in five			.005			-.012
neighboring counties			(.083)			(.084)
Prevention spending in five			-.167			.029
neighboring counties			(.099)			(.045)
Percent non-white	.045	.047	.039	.047	.048	.040
	(.084)	(.084)	(.083)	(.084)	(.085)	(.083)
Percent non-white*	.004	.004	.002	.004	.004	.003
Identity of incumbent party	(.012)	(.012)	(.012)	(.012)	(.012)	(.012)
Growth in personal income	.061**	.061**	.061**	.061**	.061**	.061**
during the election cycle	(.013)	(.013)	(.013)	(.013)	(.013)	(.013)
R^2	.929	.929	.930	.929	.929	.930
Number of observations	14975	14975	14744	14975	14975	14744

Notes: (a) Standard errors are corrected for clustering at the state*year level.

(b) The regression in column (3) includes a dummy for disaster damage being zero.

(c) All the regressions include county and year fixed effects.

(d) * $p < .05$, ** $p < .01$ (two-tailed)

Table 7: Matching Estimators

	Relief spending	Prevention spending
Effect of spending on incumbent party vote share	.316**	-.065
	(.092)	(.090)

Notes: (a) Bootstrapped standard errors, based on 1000 replications, are in parentheses. The bootstrap accounts for clustering at the state*year level.

(b) * $p<.05$, **$p<.01$ (two-tailed)

Table 8: Project Impact Communities

State	Locality or Localities Affected	County or Counties Affected	State	Locality or Localities Affected	County or Counties Affected
Alabama	Baldwin County	Baldwin	Nebraska	City of Beatrice	Gage
Alaska	Municipality of Anchorage	Anchorage Borough	Nevada	City of Sparks	Washoe
Arizona	City of Tempe	Maricopa	New Hampshire	Town of Peterborough	Hillsborough
Arkansas	Clay County	Clay	New Jersey	City of Trenton	Mercer
California	Oakland and Berkeley*	Alameda	New Mexico	City of Hobbs	Lea
California	Santa Barbara County	Santa Barbara	New York	Village of Freeport; City of Rye	Westchester
Colorado	City of Ft. Collins	Larimer	North Carolina	Tucker and Randolph Counties*	Tucker and Randolph
Connecticut	Town of Westport	Fairfield	North Carolina	Mecklenburg County	Mecklenburg
Delaware	City of Lewes	Sussex	North Dakota	City of Fargo	Cass
District of Columbia	City of Washington		Ohio	Licking County	Licking
Florida	Deerfield Beach and Broward County*	Broward	Oklahoma	City of Tulsa	Tulsa
Georgia	Camden, Glynn, and McIntosh Counties	Camden, Glynn, and McIntosh	Oregon	Benton and Tillamook Counties	Benton and Tillamook
Idaho	City of Boise	Ada	Pennsylvania	Lycoming County	Lycoming
Illinois	City of Carbondale	Jackson	Puerto Rico	City of Culebra	
Indiana	Vanderburgh County	Vanderburgh	Rhode Island	City of Warwick	Kent
Iowa	City of Denison	Crawford	South Carolina	City of Florence	Florence
Kansas	Riley County	Riley	South Dakota	City of Aberdeen	Brown
Kentucky	Jefferson County	Jefferson	Tennessee	Lincoln County	Lincoln
Louisiana	City of Baton Rouge	East Baton Rouge Parish	Texas	Harris County	Harris
Maine	City of Saco	York	Utah	City of Centerville	Davis
Maryland	Allegany County*	Allegany	Vermont	Lamoille County	Lamoille
Massachusetts	Town of Marshfield	Plymouth	Virginia	Roanoke County	Roanoke
Michigan	City of Midland	Midland	Washington	Seattle and King County*	King
Minnesota	Steele County	Steele	Washington	Pierce and King Counties	Pierce and King
Mississippi	Pascagoula and Jackson County*	Jackson	West Virginia	Tucker and Randolph Counties*	Tucker and Randolph
Missouri	City of Cape Girardeau	Cape Girardeau	Wisconsin	City of Wauwatosa	Milwaukee
Montana	Lincoln County	Lincoln	Wyoming	Fremont County	Fremont

Source: Witt (1998) and Federal Emergency Management Administration (1998)

* denotes a community that was one of the seven pilot communities.

Table 9: Voters' Responses to Project Impact Prevention Spending

A. Dependent variable: Dummy for whether the county was
a Project Impact participant

	(1)	(2)	(3)	(4)
Democrats' 1996 vote share in the county	.00033	.00035	.00019	.00018
	(.00035)	(.00034)	(.00041)	(.00042)
Disaster damage measure in 2000		.00162	.00011	.00013
		(.00222)	(.00198)	(.00198)
Percent non-white			.00024	.00024
			(.00031)	(.00031)
Growth in personal income				.00045
during the election cycle				(.00073)
R^2	.057	.057	.062	.062
Number of observations	3108	3108	2956	2956

B. Dependent variable: Change in the county's vote share
for the Democrats from 1996 to 2000

	(1)	(2)	(3)	(4)
Project Impact dummy	.991**	1.184**	.587*	.772**
	(.369)	(.319)	(.244)	(.194)
Disaster damage measure in 2000		-.08		-.092
		(.167)		(.047)
Percent non-white		.037*		.067**
		(.018)		(.009)
Growth in personal income		-.022		.01
during the election cycle		(.042)		(.016)
State dummies?	N	N	Y	Y
R^2	.003	.027	.52	.557
Number of observations	3108	2956	3108	2956

Notes: (a) Standard errors are corrected for clustering at the state level.

(b) Each regression in Panel A includes state fixed effects.
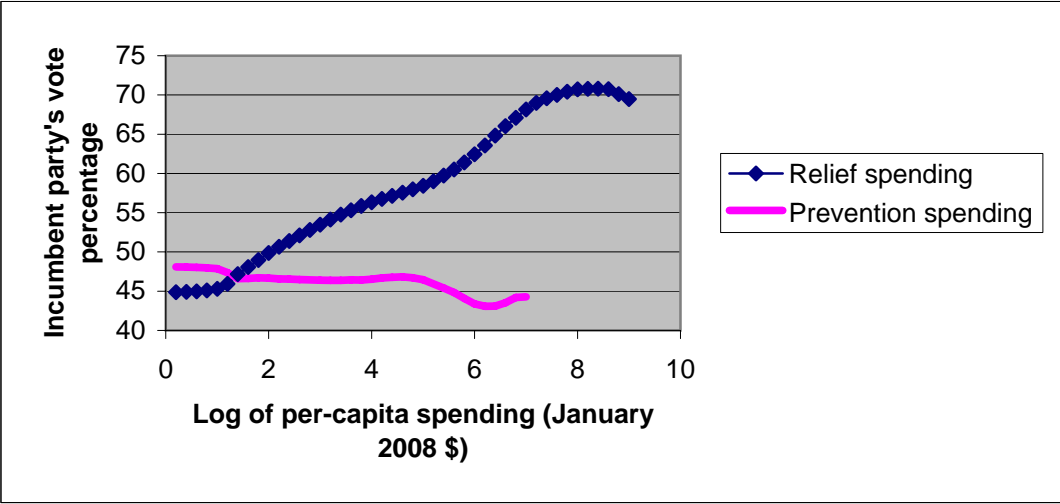
(c) * $p < .05$, ** $p < .01$ (two-tailed)
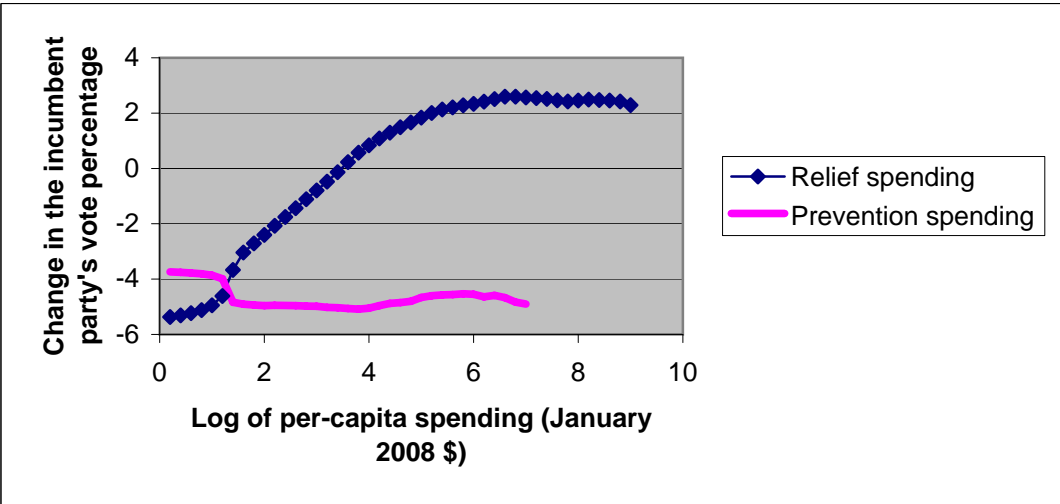
Figure 1: Incumbent party vote share and spending

Figure 2: Change in incumbent vote share and spending