

CHAPTER FIVE

Blind Retrospection: Electoral Responses to Droughts, Floods, and Shark Attacks

And Moses stretched forth his rod over the land of Egypt, and the Lord brought an east wind upon the land all that day, and all that night; and when it was morning, the east wind brought the locusts. And the locusts went up over all the land of Egypt, and rested in all the coasts of Egypt: very grievous were they; before them there were no such locusts as they, neither after them shall be such. For they covered the face of the whole earth, so that the land was darkened; and they did eat every herb of the land, and all the fruit of the trees which the hail had left: and there remained not any green thing in the trees, or in the herbs of the field, through all the land of Egypt.

Then Pharaoh called for Moses and Aaron in haste; and he said, I have sinned against the Lord your God, and against you.

—Exodus 10:13–16 (King James Version)

When collective misfortune strikes a society, someone must be blamed. For ancient Israel, disasters were God's punishment for sin—perhaps the ruler's sin, perhaps the people's. Theology did not single out the guilty party, but it structured the search and set limits on what counted as a credible explanation.

In the theology of classical Egypt, pharaohs were divine beings responsible for making the Nile flood annually. Some scholars believe that when it failed to do so, as happened repeatedly in the First Intermediate Period (ca. 2200 BCE), the resulting famines and political disorder shortened the pharaoh's

reign, and perhaps his life as well (Bell 1971; Hassan 1994). Not surprisingly, there are records of Egyptian court officials wishing their pharaoh a good Nile flood.

Through the centuries, rulers and their potential challengers have been well aware of the potential political significance of natural disasters. Poor weather and bad harvests have been given substantial credit for the rise of the Populists in Kansas (Miller 1925) and Nebraska (Dixon 1898, 637; Barnhart 1925) in the 1890s. More recently, one Republican presidential campaign official said in 1992 that "it wouldn't be so bad" if Hurricane Andrew left Florida and instead "blew on up to Kentucky and the rust-belt states" where incumbent George H. W. Bush had less chance to win electoral votes (Schneider 1995, 96).

When disasters take on truly catastrophic dimensions, not just the ruler but the entire regime may face a crisis of legitimacy. Islamic missionaries in Java and Sumatra successfully blamed Dutch rule for the 1883 volcanic explosion on Krakatoa (Winchester 2003, 317–338). An earlier catastrophic eruption in 11th-century Arizona apparently triggered social upheavals among the people living nearby; their Hopi descendants still preserve folk memories of the event, which they interpret as retribution for their ancestors' "morally imbalanced and corrupt" culture (Gidwitz 2004, 52). Similarly, the European famine of the early 14th century led to outbreaks of heresy and heretic burning in Silesia (Jordan 1996). Later in the 14th century the Black Death, which may have killed one-third or more of the population of Europe, generated numerous spontaneous religious and political movements to threaten church and government (Herlihy 1997, 64): "The plague also discredited the leaders of society, its governors, priests, and intellectuals, and the laws and theories supported by them. These elites were obviously failing in their prime social function, the defense of the common welfare, in the name of which they enjoyed their privileges."

The argument that natural disasters threaten rulers and regimes is not new. However, the base of evidence on which it rests, while impressively broad historically, is also uncomfortably thin. In this chapter we take up the challenge of providing more precise and comprehensive evidence about the political impact of natural disasters. We focus on modern history and electoral politics, where the data are sufficiently detailed and reliable to trace the political consequences of hard times.

Contemporary democratic rulers have little aura of divinity about them, nor have they faced epic famines or medieval plagues. Nonetheless, we find

that when election time comes, the electorate continues to hold rulers responsible for calamities and disasters that are clearly beyond their control. We go beyond that simple correlation, however, to argue a deeper point. While voters' reactions to natural disasters are of importance in their own right, our interest here is in what they can tell us about democratic accountability more broadly. From that perspective, electoral responses to natural disasters are just particularly illuminating instances of the broader phenomenon of retrospective voting.

Our assertion is that voters' retrospections are blind, not just in natural disasters but in hardships of all kinds. When they are in pain they are likely to kick the government, so long as they can justify doing so with whatever plausible cultural constructions are available to them. Only if no such constructions are available, or if no ambitious challengers emerge to articulate them, will people take out their frustrations on other scapegoats, or just suffer. In most cases, incumbents will pay at the polls for bad times, whether or not objective observers can find a rational basis for blame.

Our analysis begins with the unjustly neglected electoral impact of shark attacks.

SHARK ATTACKS IN NEW JERSEY: THE VOTERS BITE BACK

On the four-day Fourth of July weekend in 1916, the beaches of New Jersey were packed with crowds happy to escape the summer heat of nearby cities.¹ On Saturday, July 1, a young Ivy League graduate from Philadelphia, Charles Vansant, was swimming just beyond the breakers in four feet of water at Beach Haven when he was attacked by a shark. Skillful lifeguards managed to get him to shore, but he died soon after from blood loss. Five days later, a young Swiss bellhop named Charles Bruder, a strong swimmer like Vansant, ventured out past the lifelines at Spring Lake beach, some 45 miles north of Beach Haven. He, too, was attacked by a shark. Though rescued by lifeguards in a small boat, he died of his wounds before reaching shore.

In the days after the two deaths, nearly all of the diminished numbers of Jersey Shore swimmers stuck close to shore. However, no one worried about boys swimming in a creek on July 12 in the town of Matawan, about two miles from open water. Yet one was attacked and killed by a shark, as was

¹ Unless otherwise noted, our historical account follows that of Farnicola (2001); we also draw upon Capuzzo (2001).

a young man from the town who dove in to recover the boy's body. Downstream, another group of boys were swimming at the same time in ignorance of the attacks. Within half an hour, one of them had his leg mauled by a passing shark. However, he was quickly pulled from the water, reached the local hospital, and survived.

By this time, the mounting panic reached a crescendo. Even the distant *San Francisco Chronicle* trumpeted the shark attacks in a July 14 front-page headline: "EAST COAST BEGINS WAR ON RAVENOUS MAN-EATERS" (Fernicola 2001, 87). Steel mesh was being installed at beaches. Bounties were offered, and sharks were killed in sizable numbers along the shore. Finally, one great white shark was hauled in near Matawan Creek with what appeared to be human bones in its stomach. Perhaps for that reason, the attacks stopped, ending the most serious string of shark-related fatalities in American history.

Before the attacks, no arm of government had patrolled for sharks or set up barriers against them in New Jersey, since there had never been a recorded shark attack in the history of the state. Indeed, prominent American scientists doubted that unprovoked shark attacks on human beings ever occurred, certainly not as far north as New Jersey (Fernicola 2001, 22).² The general climate of skepticism led the *New York Times* to bury its article about the first attack on page 18, headlined "Dies after Attack by Fish"—no doubt a consolation to the New Jersey resort owners, who were anxious to avoid publicity.³

In the aftermath of the attacks, the federal government was called on for help. The resorts were losing money rapidly, with \$250,000 in reservations cancelled within a week. Some resorts had 75% vacancy rates in the midst of their high season (Capuzzo 2001, 274). Losses may have amounted to as much as \$1 million for the season altogether, a sizable sum in 1916 (Fernicola 2001, 174). Letters poured into congressional offices from the affected communities demanding federal action, though there was little any government agency could do. Fernicola (2001, 70) described the atmosphere, as the shark attacks entered popular imagery and became a metaphor for other political crises as well: "Newspaper cartoons now portrayed Wilson's chances for re-election in November, using the shark fin as the symbol for his potential loss.

² Indeed, two scientists who were later called in to investigate the attacks, Dr. John T. Nicols, an ichthyologist and director of the Fishes Wing of the American Museum of Natural History, and Dr. Frederick Lucas, director of the museum, had recently coauthored with a third scientist an article arguing that unprovoked sharks never attack human beings.

³ Parallels to the film *Jaws* and its sequels are no accident. Peter Benchley, the author of the novel on which the film was based, was a New Jersey resident, and the film version, though set on Long Island, New York, included a reference to the 1916 New Jersey attacks.

The black fin labeled ‘defeat’ was shown slicing through shark-infested northeast regions. Other political cartoons of the day showed lawyers, represented by sharks heading toward a beleaguered sailboat, embossed with ‘Union Bank.’ At the stern of the bank boat, a chewed and legless victim dangled over the gunnel depicting ‘deposits.’⁴

As it happened, the Secretary of the Treasury, William McAdoo, had a summer home in Spring Lake and was in residence at the time of the second attack. Joseph Tumulty, Wilson’s powerful aide for political affairs, had a summer home in Asbury Park, about five miles north of Spring Lake. President Wilson himself, a former president of Princeton University and former governor of New Jersey, had been looking for a summer White House in New Jersey as well, and chose a hotel in Asbury Park, moving there shortly after the attacks ended. Thus the attacks received immediate federal attention. Wilson held a cabinet meeting to discuss the attacks (Fernicola 2001, 70), but the Bureau of Fisheries could suggest nothing beyond killing sharks at random and warning bathers. “No certainly effective preventive measure could be recommended,” they said (Capuzzo 2001, 277). The president could only direct the Coast Guard to inspect the beaches and patrol the water. However, the shark attacks ended and autumn arrived before much could be done.

By election time in November, Wilson was back at his Asbury Park headquarters, but other election issues, notably potential U.S. entry into World War I, took over the headlines (Link 1954, 247–251). In the end, Wilson lost nearly all the northeastern and Great Lakes states, including New Jersey, but managed to squeak out his reelection by adding most of the Great Plains, Mountain States, and West to the Democrats’ customary Solid South.

Did the shark attacks influence the presidential election in the affected areas of New Jersey? Hitherto, sharks have not been suspects in any electoral analysis. Nonetheless, if our argument is correct, they should have reduced Wilson’s vote. First, the attacks caused several deaths plus considerable emotional and financial distress to shore communities. Second, the election occurred just a few months after the summer’s events, increasing the likelihood that they would be fresh in the minds of the voters as they went to the polls. Third, high federal officials were present at the scene from the beginning, reinforcing the notion that the federal government should have done *something* to deal with the crisis. The fact that no government has any influence over sharks would, from our perspective, have been irrelevant to the voters.

The evidence for a shark effect turns out to be rather strong. We now turn to the first piece of that evidence, using election returns from New Jersey

counties.⁴ The Wilson vote in 1916 is the outcome to be explained. Our key explanatory factor is an indicator for “beach counties,” defined as Monmouth, Ocean, Atlantic, and Cape May counties. These were, and are, the classic “Jersey Shore” counties listed in the guidebooks, whose beach areas are heavily dependent upon summer tourism. They are the places in which the shark attacks would have had the most pronounced economic effects. The attacks themselves took place in Monmouth (three deaths) and Ocean (one).

We include two additional factors in our county-level analysis. The first is the Wilson vote in 1912, a measure of both partisanship and candidate appeal, including favorite son effects. Wilson’s 1912 vote predicts his 1916 showing well, despite the fact that 1912 was a three-way race with former president Teddy Roosevelt running as a Progressive.⁵ By contrast, the four presidential elections prior to 1912 (and their mean) were less correlated with the 1916 vote, and they added nothing to the accuracy of the statistical analysis once 1912 was included.⁶

One other control variable is needed to capture an important change in New Jersey politics between 1912 and 1916. Having supported Wilson for governor in 1910, the New Jersey bosses turned against him shortly after his election.⁷ They initially opposed his nomination for president in 1912, but fell in line once it became inevitable (Link 1947, chaps. 8–9 and 427–428). After he became president, however, Wilson’s control of the New Jersey Democratic Party, once nearly complete, slipped away (Blum 1951, 39, 76; Link 1947, 288). For example, the infamous Jersey City political boss Frank Hague supplanted a progressive Wilson ally during this period (McKean 1940, chap. 3; Connors 1971, chap. 3). To take account of Wilson’s reduced power over the

⁴ New Jersey electoral data are from the official reports published in the *Legislative Manual of the State of New Jersey*, various years.

⁵ Throughout the Northeast, the Roosevelt vote from 1912 returned almost entirely to Charles Evans Hughes, the Republican candidate, in 1916. (Socialist and other minor candidates, including Prohibition advocates, were also running in both years, but of course only Roosevelt was a serious third-party contender for the presidency.) Wilson gained less than a percentage point statewide in New Jersey in 1916 from his 1912 totals, and similar results held in other northeastern states. Wilson’s 1912 vote is an excellent predictor of his 1916 vote across New Jersey counties, and even at the township level.

⁶ Adding the Roosevelt proportion of the vote from 1912 generated a small positive, statistically insignificant coefficient. Keeping the Roosevelt variable made no difference in subsequent analyses, and so it was dropped.

⁷ For this reason, Wilson’s vote for governor in 1910 is poorly correlated with his showing in both presidential elections and was not used as a statistical control in our analysis of his 1916 vote.

bosses in 1916, we include a control variable for “machine counties,” defined as those counties with at least 30,000 voters in 1916 and 60% or more “foreign” citizens in the census of 1910.⁸ The counties so defined are Bergen, Hudson, Essex, and Union, adjacent to each other and just across the state line from New York City.

Two of these machine counties, Hudson (Jersey City) and Essex (Newark), were particularly well known for boss control. In fact, alone among New Jersey’s counties, Wilson never did get so much as partial control of the Essex Democratic machine, which was under the thumb of James Smith, Wilson’s bitter political enemy, throughout this period (Blum 1951, 39–40; Link 1947, 288, 424). For that reason, Wilson’s 1912 vote in Essex was so low relative to its electoral history that the county becomes a substantial outlier in predicting the 1916 vote, even beyond its status as a machine county. Simply put, Essex County in this electoral period does not act like the rest of New Jersey at the polls; we therefore excluded it from our analysis. The other 20 New Jersey counties make up our sample.

Table 5.1 presents the results of a statistical analysis estimating the difference in Wilson’s 1916 presidential vote share between beach counties and non-beach counties, controlling for machine counties and for Wilson’s 1912 vote share. All of the parameter estimates are substantively significant and sensibly sized, and each of them is statistically significant beyond the .01 level. The analysis accounts for Wilson’s 1916 vote share with an average error of just 1.7 percentage points, and the correlation between actual and predicted 1916 vote shares is .97.⁹

The estimated negative effect on Wilson’s vote in the beach counties is a little more than three percentage points, with a 95% confidence interval confined between 1.3 and 5.2. The shark attacks indeed seem to have had an impact. The statistical significance of the estimate is due to the very consistent effect across the beach counties, as may be seen from figure 5.1. This figure shows the statistical relationship between Wilson’s 1916 vote share and his 1912 vote share with the machine county variable controlled. The linear relationships are estimated separately for beach and non-beach counties, with Essex excluded. As the graph shows, the beach counties are each depressed

⁸ “Foreign” here means that the citizen was foreign-born or had at least one foreign-born parent (the so-called hyphens in the vernacular of the time).

⁹ None of the residuals from this regression analysis falls more than two standard deviations from zero, and only one of them (Salem’s) is near that level, about what would be expected by chance. By contrast, the excluded Essex County observation has a residual 4.6 standard deviations from zero in this analysis, amply justifying its exclusion from the sample.

Table 5.1. The Effect of Shark Attacks on the 1916 New Jersey Presidential Vote

Beach county	-3.2 (1.0)
Machine county	-5.7 (1.1)
Wilson 1912 vote (three-way fraction)	0.95 (0.06)
Intercept	4.5 (2.8)
Standard error of regression	1.7
Adjusted R^2	.94
N	20

Parameter estimates from ordinary least squares regression analysis (with standard errors in parentheses) of Woodrow Wilson’s vote share (two-party %) in New Jersey counties, 1916.

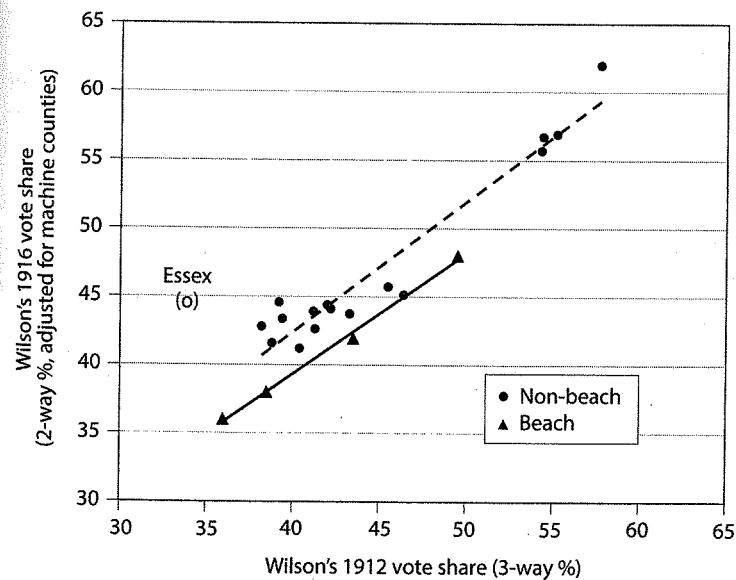


Figure 5.1. Support for Woodrow Wilson in New Jersey Counties, 1916 versus 1912

nearly the same amount from their expected 1916 vote, and the consistency of the effect bolsters the plausibility of the specification visually while tightening the standard errors statistically.¹⁰

We explored a variety of other statistical specifications using different measures of partisanship. None worked as well as Wilson's 1912 vote share, and the estimated effect of the shark attacks remained fairly constant—two to four percentage points—so long as the 1912 vote share was included. We also tried including measures of the proportion German, the proportion Irish, and the total proportion “foreign,” since speculation was rampant at the time of the 1916 election that these communities would be unhappy with Wilson over his potential entry into World War I on the British side or over the British suppression of the 1916 Easter Rising in Dublin. We found no effects of this sort, which is consistent with the conclusions of analysts of the national vote at the time.¹¹ Similarly, weighting the counties by their total 1916 turnout, or using the change in Wilson's vote share from 1912 to 1916 as the dependent variable, or both, never reduced the estimated impact of the sharks by more than a tenth of a percentage point. In fact, all the turnout-weighted estimates were larger by a few tenths of a percentage point. Thus the shark effect stands up well under a variety of alternative statistical specifications.¹²

We also undertook two additional investigations with different samples. First, we examined the vote in the first two shore townships where the attacks took place.¹³ Both Beach Haven and Spring Lake were small, stable commu-

10 If the machine counties were counted as beach counties, they would fit nicely on the regression line. They are themselves on the water or adjacent to the Hudson River. Capuzzo (2001, 270–273) noted that fear extended well beyond the Jersey Shore counties, up through the machine counties and onto New York State beaches, where the economy was also harmed. One shark was killed with a revolver near a yacht club in machine-controlled Hudson County (Fernicola 2001, 27). Thus it is possible that some of the negative “machine county” effect is, in fact, due to the sharks.

11 Two days after the election (November 9), the *New York Times* headlined “Both Candidates Got Hyphen Vote.” For subsequent treatments reaching the same conclusion, see Link (1954, 232–251) on the Germans and Leary (1967) and Cuddy (1969) on the Irish.

12 Another possibility we considered was that Roosevelt might have run worse in the beach counties than in the rest of the state, leaving Wilson fewer voters there to pick up from Progressive Republicans in 1916. This would have created an artificial drop in Wilson's 1916 vote in the beach counties. To the contrary, however, Roosevelt ran *better* along the shore than in the rest of the state, so that the shark attack effect is, if anything, slightly underestimated in table 5.1.

13 Matawan Township and Matawan Borough, where the final two shark deaths occurred in a river, were excluded from this analysis since they are not beach resort communities and thus suffered no widespread economic loss from their shark attacks or anyone else's. In any

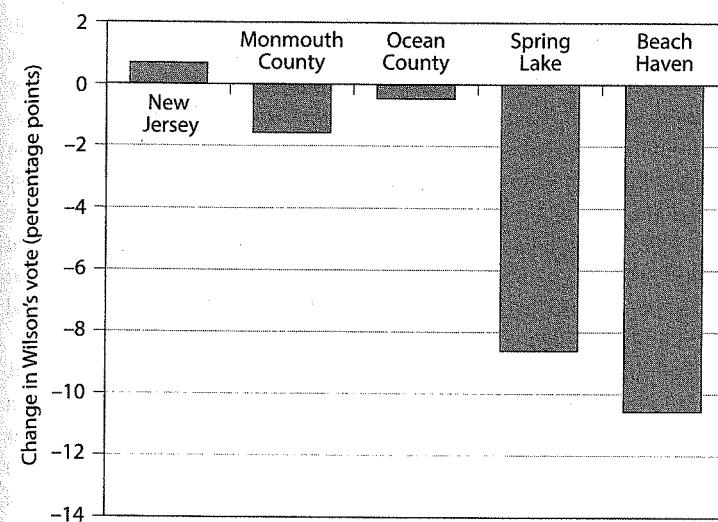


Figure 5.2. Change in Woodrow Wilson's Vote Share (1912–1916) in Counties and Townships with Shark Attacks

nities, making comparison sensible.¹⁴ Figure 5.2 shows the vote change for Wilson between 1912 and 1916 in these two communities, and compares it with the change in their respective counties and in New Jersey as a whole. Both townships show remarkable drops in Wilson's support, 11 points in Beach Haven and 9 in Spring Lake—far more than the modest decline in the Wilson vote in their counties. It is apparent that something drastically reduced enthusiasm for Woodrow Wilson in these two townships.

We also investigated whether Beach Haven and Spring Lake were typical of beach areas. To answer this question, we examined the townships in Ocean County near the water. Ocean was chosen because it has many beach

case, the rapid growth in the number of voters in both places between 1912 and 1916 makes reliable comparison impossible; more than a quarter of the 1916 voters in Matawan Township had not been there in 1912.

14 Beach Haven cast 112 votes for president in 1912 and 119 in 1916. The corresponding numbers for Spring Lake are 271 and 265.

communities, nearly all on a bank of land (Long Beach Island) clearly separated from the mainland. Thus there is no difficulty in separating those eight communities right on the beach, whose economies were damaged by the shark attacks, from the eleven towns near the beach but not on it, whose economies were less susceptible to harm.¹⁵ New Jersey was growing rapidly in this era; to ensure that vote shares in 1912 and 1916 would be meaningfully comparable, we dropped townships whose vote totals grew or shrank by more than 25% in this four-year period.¹⁶ This left us with 14 towns, 4 of them on the beach and 10 nearby. These two sets of communities had very similar overall Democratic percentages for Wilson in 1912 (37.1% at the beach and 33.5% in the near-beach), making them comparable.

In each area, we compared Wilson's vote percentages in 1912 and 1916. If our argument is correct, the beach voters should show the largest drop in support for Wilson, while the near-beach voters should have been largely unaffected. As shown in figure 5.3, the actual vote change turns out to be a drop of 13.3 percentage points in the beach area, compared to a tiny loss of half a percentage point in the near-beach area, an easily statistically significant difference.¹⁷ Again, we find that disaffection for Wilson was widespread in the beach areas where livelihoods were most directly affected by the shark attacks, far different from the otherwise comparable areas next door, where Wilson's vote was nearly constant.

In summary, then, every indication in the New Jersey election returns is that the horrifying shark attacks during the summer of 1916 reduced

¹⁵ The western border of the near-beach area was set to the current Garden State Parkway, which runs within a few miles of the shore in Ocean County.

¹⁶ One beach township, Sea Side Park, apparently split into two between 1912 and 1916 and jointly nearly doubled in size; we dropped it from the analysis.

¹⁷ The simplest approach is a differences-in-differences regression model weighted by the 1916 total vote (to take account of the wide range of electorate sizes in these boroughs). Thus, with the change in the Democratic vote percentage from 1912 to 1916 as the dependent variable and beach township as the explanatory variable, we obtain a coefficient of -12.8 percentage points, with a standard error of 4.4 (and a *t*-statistic of 2.9). Alternately, a weighted regression with the Democratic vote in 1916 as the dependent variable, and with beach township and the 1912 Democratic vote as explanatory variables, yields a beach effect of -11.1 percentage points with a standard error of 3.2 (and a *t*-statistic of 3.5). Unweighted regressions, though arguably substantively inappropriate, yield even larger beach coefficients. Finally, if we eliminate two townships with fewer than 50 voters in 1916, the differenced regression produces a beach coefficient of -8.4 percentage points, while the second regression version yields -8.8, both with *t*-statistics exceeding 2.5. In short, alternate versions of the beach versus non-beach comparison lead to precisely the same substantive conclusion, which we summarize as a loss of about 10 percentage points in the areas most directly affected by the shark attacks.

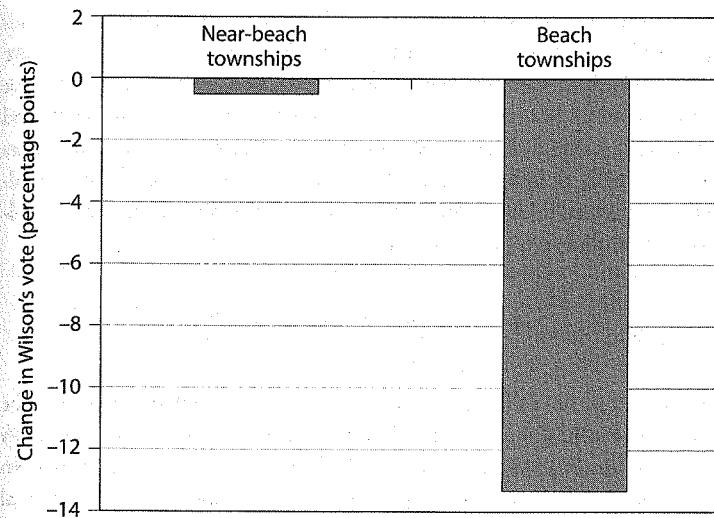


Figure 5.3. Change in Woodrow Wilson's Vote Share (1912–1916) in Ocean County Townships

Wilson's vote in the beach communities by about ten percentage points. An effect of that size may sound modest to those unfamiliar with American electoral experience, but by those standards it is a near-earthquake. (A full earthquake, the Great Depression, reduced Herbert Hoover's vote in New Jersey by 12 percentage points, from 59.8% in 1928 to 47.6% in 1932.)

In the case of the shark attacks, retrospection was surely blind. Shark attacks are random events in the purest sense of the term, and they have no governmental solution. If bathers insist on swimming in the ocean, governments then and now cannot save them, as subsequent attacks in New Jersey in 1960 and regular encounters in Florida, California, South Africa, and Australia demonstrate (Fernicola 2001, chap. 5). Nor could the aftermath of the 1916 attacks be repaired by governmental action. The truth could not be covered up. The vacationers could not be compelled to come to the beach, nor could the sharks be forced to stay away. The government was helpless. Yet the voters punished anyway.¹⁸

¹⁸ On one occasion, sharks apparently had a more direct and unfortunate impact on an incumbent political leader. On December 17, 1967, Australian Prime Minister Harold Holt

From the perspective of a century later, of course, it is obvious that extending federal welfare benefits and unemployment compensation would have helped. But these social programs did not exist at the time, they could not have been put in place quickly, and no one would expect them to be enacted in response to a single local disaster in any case. Thus the idea that the voters blamed Wilson for failing to extend federal disaster assistance, as some readers have suggested to us, is a form of historical presentism—a serious misreading of circumstances at the time.

A CENTURY OF DROUGHTS AND FLOODS

Any single instance of blind retrospection, no matter how dramatic, may be dismissed as coincidence or anomaly. Our argument is that voters *consistently* and *systematically* punish incumbents for conditions beyond their control. To assess that broader argument we turn to a comprehensive analysis of electoral responses to droughts and floods by voters in presidential elections throughout the 20th century. We show that voters do indeed punish the incumbent party at the polls for presiding over dry or wet spells.

The data on droughts and floods employed in our analysis consist of monthly readings from 1897 to 2000 of hydrological conditions in each of 344 climate divisions covering the contiguous 48 U.S. states.¹⁹ Each observation summarizes daily data from several weather stations within each climate division. We measure wet and dry spells using the Palmer Hydrological Drought Index (PHDI), an index of long-term moisture supply.²⁰ A PHDI reading of zero represents an ideal moisture level; negative values represent droughts and positive values represent wet spells.²¹

disappeared while swimming in shark-infested waters at Cheviot Beach near Portsea, Victoria. His body was never found.

19 The data were generated by the U.S. government and are publicly available from the National Climatic Data Center (NCDC), a unit of the National Oceanic and Atmospheric Administration in the U.S. Department of Commerce. See <http://lwf.ncdc.noaa.gov/oa/climate/onlineprod/drought/readme.html>; also http://ingrid.ldeo.columbia.edu/SOURCES/.NOAA/.NCDC/.CIRS/.ClimateDivision/.dataset_documentation.html.

20 We believe that the Palmer Hydrological Drought Index provides a better measure of the damage associated with droughts and floods than the Palmer Drought Severity Index, which measures the severity of dry or wet spells of weather rather than long-term moisture supplies.

21 PHDI values between 2 and 3 represent “moderate” droughts, values between 3 and 4 represent “severe” droughts, and values less than 4 represent “extreme” droughts, and similarly

We aggregate the monthly data for each climate division by computing the absolute value of the sum of monthly PHDI readings from May through October of each calendar year. For simplicity we refer to the result of this calculation as a “drought index,” but it is important to bear in mind that the absolute values reflect both wet and dry spells.²² We further aggregate the data to the level of states by computing a simple average of the annual absolute PHDI values for the climate divisions in each state.²³

The result of these calculations is an index of climatic pain running from 0.04 to 49.08, with a mean value of 11.03 and a standard deviation of 6.29. Low values of the index are good and high values are bad for voters and thus, according to our account, for incumbent presidents. Our 4,992 observations (for each of 48 states in each of 104 years) include 649 (13%) with absolute PHDI values in excess of 18, the equivalent of a full year of “severe” drought or wetness; 203 observations (4%) have absolute PHDI values in excess of 24, the equivalent of a full year of “extreme” drought or wetness.

We investigate electoral responses to droughts and floods by conducting statistical analyses of popular support for incumbent party candidates in 26 presidential elections (from 1900 through 2000). Thus our analysis is not based on a single, possibly idiosyncratic drought or flood. Rather, we examine an entire century of wet and dry spells, relying upon the random occurrence of numerous droughts and floods to distinguish their common effects from potentially confounding specific circumstances. Our analyses employ state-level voting data and six different versions of our drought index.²⁴ In each

for positive values indicating wet spells. The distribution of PHDI values is approximately normal, with no asymmetry apparent between the severity of wet spells and dry spells.

22 This calculation assumes that equally severe droughts and wet spells are equally painful to voters. We investigated that assumption by repeating our statistical analyses using separate measures of droughts and wet spells. The estimated effects were generally similar. For example, in the simplest regression model presented in the first column of table 5.2, distinguishing between droughts and wet spells produced estimated effects of .067 (with a standard error of .047) and .066 (with a standard error of .050), respectively.

23 Most states are composed of between seven and nine climate divisions; eight states have one, two, or three divisions. The climate division boundaries sometimes reflect geographical features such as coastal areas or mountain ranges, but more often follow county lines.

24 All our statistical analyses weight each state in each year by the number of votes cast in the presidential election; thus, populous states and those with heavy turnout get more weight than those with fewer voters, and more recent elections get more weight than those earlier in the century covered by our analysis.

Table 5.2. Droughts, Floods, and Presidential Elections, 1900–2000

	Drought index			Rural drought index		
	(1)	(2)	(3)	(4)	(5)	(6)
Election-year drought index	−0.060 (0.031)	−0.052 (0.034)	—	−0.176 (0.083)	−0.140 (0.082)	—
(Election−1) drought index	—	−0.043 (0.029)	—	—	−0.116 (0.088)	—
(Election−2) drought index	—	0.016 (0.036)	—	—	0.023 (0.102)	—
(Election−3) drought index	—	−0.043 (0.040)	—	—	−0.024 (0.102)	—
Time-weighted drought index	—	—	0.104 (0.045)	—	—	−0.273 (0.122)
Standard error of regression	3.61	3.60	3.60	3.61	3.61	3.60
Adjusted R^2	.88	.88	.88	.88	.88	.88
N	1,233	1,233	1,233	1,233	1,233	1,233

Parameter estimates (with standard errors in parentheses) from ordinary least squares regression analyses of incumbent vote (%) by state; states weighted by turnout; observations clustered by election year. Election-specific intercepts and election-specific effects of lagged incumbent vote, twice-lagged incumbent vote, % rural and South not shown.

case, we expect droughts and floods to depress the incumbent party's popular vote share. Table 5.2 presents the key results.

In order to allow for other factors affecting the incumbent party's fortunes in each state in each election, we take account of the incumbent party's vote share in the same state in each of the previous two presidential elections, the percentage of the population living in rural areas, and an indicator variable for southern states. The effects of all of these factors are allowed to vary from one election to the next, so that there are 130 coefficients in each regression model in addition to those reported in table 5.2—an intercept for each

election, a coefficient for the lagged incumbent vote in each election, and so on. Only the effects of drought are assumed to be constant across elections.

The simplest version of our analysis, reported in the first column of table 5.2, employs the absolute PHDI value for each state in each election year as the primary explanatory factor. The negative estimated effect indicates that, on average, voters punished incumbent parties for droughts and wet spells; the t -statistic for this parameter estimate is −1.9, so the effect of wet and dry spells on election outcomes cannot easily "be dismissed by the dubious as a coincidence" (Barnhart 1925, 529).²⁵ Nor is the estimated effect trivial in magnitude. It implies that wet or dry conditions in a typical state and year (an average absolute PHDI value of 11) cost the incumbent party 0.7 percentage points, while "extreme" droughts or wet spells (absolute PHDI values of 24 or more) cost incumbents about 1.5 percentage points.

The second column of table 5.2 reports the results of a slightly more complicated analysis in which the drought index values for all four years of each president's term appear as separate explanatory factors. Here, the estimated effect of election year drought is quite similar to the estimated effect in the first column, and drought values in two of the three preceding years appear to have additional (albeit slightly smaller) negative effects. In the third column we employ a time-weighted cumulative drought index in which drought conditions in each year of a president's term get twice as much weight as those in the preceding year.²⁶ Once again, the effect of droughts and wet spells on the incumbent party's vote share is clearly negative (in this case with a t -statistic of −2.3) and of considerable magnitude (costing the incumbent party about 1.1 percentage points in a typical state and year).

The remaining three columns of table 5.2 repeat the analyses reported in the first three columns, but with each drought variable multiplied by the proportion of the population living in rural areas in each state and year. The resulting rural drought indices allow for the possibility that wet and dry spells

25 The observations in the regression analyses reported in table 5.2 are clustered by election year, which allows the unmeasured factors affecting incumbent party support in each year to be correlated across states. The result of clustering is to increase the estimated standard errors (and reduce the associated t -statistics) by about 35%.

26 The resulting weights attached to drought index values in the four years leading up to each election are .06667, .13333, .26667, and .53333. Deriving geometrically declining weights from the separate estimates reported in the second (or fifth) column of table 5.2 would produce roughly similar weights.

may be particularly consequential in rural areas where farming, ranching, and forestry are major economic activities. However, allowing for the difference in scales between the original and rural drought indices, the pattern of estimated effects turns out to be quite similar. For example, the estimated effect of election year rural drought in the fourth column implies that the incumbent party lost 0.6% of the vote in a typical state and year (as compared with 0.7% in the first column).

The strength and consistency of these results across a variety of analyses employing different versions of our drought index should leave little doubt that droughts and wet spells *in general* had a negative effect on electoral support for the president's party.²⁷ Climatic distress is a pervasive risk to the re-election chances of every incumbent, and no more controllable than the rain.

An important disadvantage of the summary results presented in table 5.2 is that they conceal a great deal of potentially interesting variation in effects across election years, some of which may be attributable to more or less effective governmental responses and some of which may reflect other factors. For example, as noted by Barnhart (1925, 536–539), insufficient rainfall has less impact on livestock ranchers than on farmers. Thus we expect that some droughts will have substantial economic and political impacts and others less so, depending on where they occurred. That variation is conveyed by figure 5.4, which presents separate estimated effects of election-year drought on the incumbent party's vote share in each election.

The estimated effects of droughts and wet spells are clearly quite variable, with almost half of the election-specific estimates more than twice as large—and a few as much as five times as large—as the corresponding overall estimates in table 5.2. A detailed examination of those varying responses might shed very useful light on the psychology and sociology of voters' attributions of responsibility for natural disasters. However, that sort of detailed examination is beyond the scope of the present study.

Rather than attempting to provide a detailed analysis of climatic retrospection in each election, we propose here merely to emphasize that our analysis of droughts and floods cannot be dismissed as a bit of Dust Bowl

27 In addition to the variety of regression analyses reported in table 5.2 we examined models with separate effects for droughts and wet spells, models with nonlinear variants of our drought indices, models allowing for secular trends in the magnitude of drought effects, models allowing drought effects to vary with prior partisanship, and models employing interactions between local climatic conditions and national climatic conditions. All of these models produced clear evidence of drought effects, but none added significantly—in terms of statistical fit or substantive insight—to the simpler analyses reported in table 5.2.

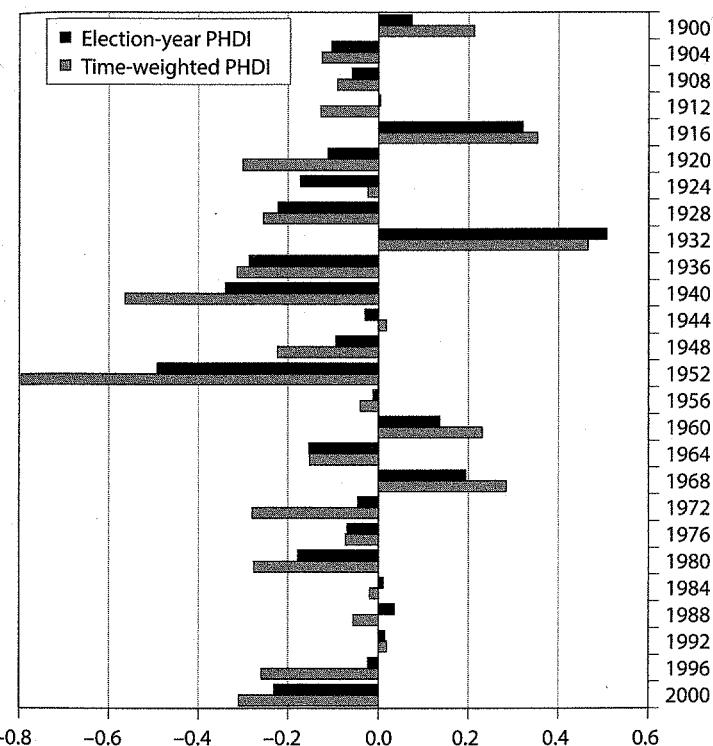


Figure 5.4. Election-Specific Estimates of Drought and Flood Effects, 1900–2000

antiquarianism. We do so by examining the electorate's response to droughts and floods in the 2000 presidential election. The 2000 election occurred under relatively unfavorable climatic conditions for the incumbent party. The average absolute PHDI value was about 10% higher than the historical average, with severe drought in parts of the South and West and excessive wetness in the Dakotas and New York and Vermont.²⁸

28 Drought conditions were most severe in Arizona and Alabama, which had drought index values in excess of 20; Louisiana, Montana, Georgia, Mississippi, Texas, Utah, Wyoming, New Mexico, and Nevada also had drought values in excess of 15. At the opposite extreme, the Dakotas, Vermont, and New York had absolute PHDI values ranging from 15.8 to 20.3.

Table 5.3. The Effect of Drought on the 2000 Presidential Vote

	(1)	(2)	(3)
Election-year drought index	-0.231 (0.073)	—	—
Rural drought index	—	-0.546 (0.259)	—
Time-weighted drought index	—	—	-0.310 (0.103)
1996 Clinton vote (%)	0.915 (0.113)	0.896 (0.119)	0.802 (0.115)
1992 Clinton vote (%)	0.206 (0.121)	0.237 (0.127)	0.291 (0.120)
Rural (%)	-0.098 (0.026)	-0.032 (0.040)	-0.116 (0.027)
South	-0.60 (0.76)	-0.96 (0.79)	-1.57 (0.68)
Intercept	-0.14 (3.33)	-3.14 (3.40)	3.40 (3.86)
Standard error of regression	1.94	2.06	1.96
Adjusted R^2	.92	.91	.92
N	48	48	48

Parameter estimates (with standard errors in parentheses) from ordinary least squares regression analyses of Democratic vote (%) by state; states weighted by turnout.

Table 5.3 presents our analysis of the impact of climatic conditions on the 2000 presidential vote. We consider three different versions of our drought index, with statistical controls for previous Democratic votes, percentage rural, and an indicator variable for southern states. All three versions of the statistical analysis fit the data very well, with average errors of less than two percentage points in the predicted Democratic vote share and adjusted R^2 values in excess of .90.

The results of these analyses suggest a great deal of continuity with previous partisan voting patterns, albeit with some significant defections from the Democratic ticket in rural states. The three versions of our drought index all have strong negative estimated effects on the Democratic vote, with t -statistics ranging from -2.1 to -3.2. The magnitudes of these estimated effects suggest that the Democrats' national vote share was from 1.6 to 3.6 percentage points lower than it would have been had voters not been inclined to make the incumbent party feel their climatic pain. The aggregate effect implied by the best-fitting model, employing the simple election year drought index, falls in the middle of that range at 2.7 percentage points.

This estimate implies that 2.8 million people voted against Al Gore in 2000 because their states were too dry or too wet. As it turned out, Gore could have used those votes. Attributing them to the various states in proportion to their drought scores suggests that climatic retribution cost Gore seven states—Arizona, Louisiana, Nevada, Florida, New Hampshire, Tennessee, and Missouri—and almost three times as many electoral votes as Florida's infamous “butterfly ballot” (Wand et al. 2001). Voters responded to climatic distress in 2000, as they have repeatedly throughout the past century, by punishing the incumbent government at the polls.

HOLDING INCUMBENTS RESPONSIBLE: WHY SO MUCH PUNISHMENT?

When voters endure natural disasters they generally vote against the party in power, even if the government could not possibly have prevented the problem. In our experience, this simple fact induces in many readers a powerful urge to defend the sophistication and rationality of the electorate. Here we take up their arguments.

First, it is possible that voters did not blame the government for the disasters themselves, but did blame it for *exacerbating* or failing to *ameliorate* the damage. In that case, retrospection might not be blind. The point is not a new one. As Barnhart (1925, 540) wrote of 19th-century Nebraska,

To suggest that the farmer held the politician responsible for the shortage of rainfall would be an unwarranted exaggeration of the thoughtlessness of the voters. But it is quite another matter to suggest that the drought in Nebraska made a bad set of agricultural conditions worse and that the politicians were held responsible for some of the conditions. Perhaps some held them responsible for most of them. The situation of

many farmers forced them to think about the things that had brought about that situation.... They could not make it rain, but they thought they could lower railroad rates.

The difficulty with this argument is its strong policy component. If the voters learn in disasters what they had hitherto not suspected—namely that stronger government intervention in the economy is needed—then droughts ought to push electorates to the ideological left. What they actually do, however, is reduce support for incumbents regardless of their ideological commitments. “Throwing the rascals out” after droughts did lead to left-wing gains in Nebraska in the 1890s; but where left-wing governments have been in power the lack of rain has created surges of right-wing voting, as in the American Dust Bowl in the mid-1930s. Similarly in the case of the sharks, if New Jersey voters in 1916 wanted a better government social safety net enacted, then their swing from Wilson to his Republican opponent was politically quite illogical.

Moreover, whatever the voters learn in natural disasters has a very short half-life. As many scholars have noted, Populism declined rapidly as economic conditions improved in the farming states. Confirming that historical pattern, table 5.2 suggests that droughts in the first half of a president’s term have no consistent influence on the voters. Similarly, by 1920 the Jersey Shore was back to its customary partisanship among New Jersey counties, and the beach areas of Ocean County that had suffered most from the shark attacks were 8 percentage points more favorable to the party of Wilson than the near-beach, just the reverse of their views at the time of the attacks four years earlier. In sum, the voters quickly forgot their grievances. Short-term anti-incumbent voting without substantial policy content is the only common pattern in the electorate’s response to natural disasters.

An alternative defense of voter rationality is that the electorate punishes incumbents not for the *occurrence* of natural disasters, which are clearly beyond their control, but for insufficient *responses* to those disasters. In this view, voters monitor incumbents’ performances in the aftermath of disasters in much the same way, and for much the same reasons, that they monitor other aspects of incumbent performance—imperfectly, based on observed results, in order to *select* competent leaders and provide *incentives* for future performance. This sort of punishment seems wholly consistent with the logic of retrospection we set out in chapter 4.

The problem with this interpretation is that it is manifestly irrational for well-informed voters to punish incumbent politicians for droughts, floods,

or other natural disasters *in general*. Any particular drought, flood, or other natural disaster may be an occasion for rational punishment of incumbent politicians who fail to take adequate steps to ameliorate its effects. However, a competent electorate must recognize that incumbents’ preparations for, and reactions to, the substantial physical and social dislocations resulting from major disasters are, by definition, better than average about half the time. And while it may be sensible for voters to punish incumbents who perform less well than average in the wake of a disaster, punishing those who perform *better* than average is counterproductive both from the standpoint of selecting competent leaders and from the standpoint of providing proper incentives for future incumbents to do their best under difficult circumstances.

Thus, we should expect to find rational electorates *rewarding* incumbents for better-than-average responses to natural disasters as often as they *punish* incumbents for worse-than-average responses. The evidence strongly contradicts this expectation. Energetic politicians can sometimes successfully claim credit for federal disaster assistance or shift the blame to other levels of government (Abney and Hill 1966; Arceneaux and Stein 2006; Healy and Malhotra 2010; Gasper and Reeves 2011; Brader n.d.). However, the one-sided tendency for voters to punish rather than reward their leaders in times of distress is evident in our statistical results. The estimated drought effects in table 5.2 represent average effects on incumbent vote shares of all the droughts and floods of the 20th century, including effective responses as well as ineffective ones. If reward and punishment were distributed impartially, the *average* effects of droughts and floods would be zero. Instead, they are preponderantly negative. Voters are much more apt to punish their leaders than to reward them.

In human terms, the voters’ behavior is understandable. They see friends and neighbors pitching in to help immediately after the disaster. They do not understand why the government cannot do the same. To citizens, government bureaucracies with their rules to prevent fraud and their accounting regulations may dispense disaster assistance with a heartless attitude and a lethargic spirit. Or, if the money is dispersed more freely, citizens may complain about waste and abuse (Schneider 1995, 70–71). Either way, in the wake of a disaster the government will look inept or uncaring to a devastated and emotionally shaken electorate. Hence, the voters will punish most of the time. But doing so can neither relieve their distress nor produce more competent or caring political leaders.

One final version of the principal-agent argument is more consistent with our evidence. On this view, voters simply punish incumbent leaders any time

their own well-being falls below “normal” levels, regardless of whether the incumbents have performed well or badly. Disasters are very likely to cause hardship even when incumbents’ responses are vigorous and competent, so this sort of decision rule leads to frequent punishing after disasters, thus matching the evidence. And it is “rational” in the technical sense set forth in chapter 4—if voters are incapable of distinguishing relevant from irrelevant sources of subjective well-being, the best they can do may be to respond identically to both. In that case, voters will respond in much the same way to shark attacks and droughts as they do to poor tax policies and disastrous foreign wars. Incapable of assessing causation, they will mechanically translate pain into electoral punishment. But that is just what we mean by blind retrospection.

THE SOCIAL CONSTRUCTION OF BLAME

Thus far we have written of retrospective voting as if hardship itself created electoral backlash. Like many other scholars, we have deemphasized the voters’ interpretation of their plight, as if it had little causal importance. In fact, however, we believe that voters’ attributions of blame are often crucial in their decisions to punish incumbents. Positive or negative events that voters themselves would recognize as politically irrelevant, such as the outcomes of local college football games, may also influence voting behavior through subconscious effects on voters’ moods (Healy, Malhotra, and Mo 2010). But substantial punishment at the polls is likely to be grounded in a belief, however farfetched, that the government is somehow responsible for the voters’ pain.

It is easy to overlook the need for social interpretation of hard times, since suggestions about their meaning are typically mass-produced. Political and ideological entrepreneurs have an incentive to construct explanations and solutions, often self-serving, for people’s hardships. Amplified by the mass media, these ideas may increase or decrease the likelihood that citizens will attribute responsibility for social problems to the government (Iyengar 1991). In garden-variety economic recessions, the accepted stories about blame are familiar, and the process of generating common understandings occurs so smoothly and easily that its importance may go unnoticed. Natural disasters, by contrast, create deeper and unfamiliar hardships, which lead to uncertainty and even fear. The old complacent assumptions are shown to be mistaken, and a search begins for new explanations that will avoid a repetition of the disaster (Cantril 1958, chap. 1; Birkland 1997). People are ready to listen.

After disasters, the more popular attributions of blame and proposals for reform often come from widely trusted sources and appeal in a clear, simple way to broadly shared values, though not necessarily those that intellectual elites rely on for their political judgments. As Hadley Cantril (1941, 67) put it, “There are short-cut rationalizations which fire the imagination and spread because they somehow express the dissatisfactions from which people have been suffering and at the same time imply a new direction and purpose.” If a single nutty or dangerous vision comes to be sufficiently widely shared, demagogues may be able to ride it to power.

Elite culture is usually (though not always) less susceptible to nutty or dangerous visions. It may even play some role in discouraging the most ignorant or vicious attributions of blame. But popular culture is never entirely under the control of the respectable. A variety of unconventional interpretations and nostrums may be available, and under the right circumstances deviant doctrines may attract considerable popular acceptance. Some medieval towns blamed the plague on Jews, prostitutes, beggars, or foreign agents (Herlihy 1997, 65–67). Some New Jersey residents in 1916 thought that German U-boats might have induced the sharks to attack (Fernicola 2001, 166–170). Some Americans in the grip of the Spanish Influenza pandemic two years later feared that “plague germs were inserted into aspirin made by the German drug company Bayer” (Kolata 1999, 3).

Different sectors of the population, immersed in distinct subcultures, may find different explanations appealing. Ideological commitments may color the plausibility of alternative explanations, as with the Federalists’ and Republicans’ competing accounts of the yellow fever epidemic of 1793. Physicians “divided bitterly over the cause of the epidemic,” with Republicans generally attributing it to poor sanitation, climatic conditions, and the unhealthy location of Philadelphia, while Federalists blamed disembarking refugees from Haiti; in fact, “both sides were right” (Pernick 1972, 562–563). If available interpretations are sufficiently contested, and if incumbents can exploit competing explanations to exonerate themselves and blame others, they may sometimes escape blame altogether (McGraw 1991; Arceneaux and Stein 2006).

In other cases, blame may fail not because there are too many available interpretations of disaster but because there are too few. In 1874, for example, locust swarms devastated large swaths of western Nebraska and adjacent states. By fall, many farmers literally faced starvation. The Army had clothing and food supplies stored in the area, but refused to distribute the clothes

until several weeks after the fall election, and did not give out food until the following February (Lockwood 2004, 80–84). Nonetheless, the incumbent Republicans sailed to victory in Nebraska in 1874, and repeated plagues of locusts throughout the mid-1870s did not notably dent their popularity in either gubernatorial or presidential elections (Nebraska Legislative Reference Bureau 1918, 436–506). The voters did not punish. The simplest explanation is probably that in the thinly populated farming areas of Nebraska at that time, communication was poor and no shared interpretation of the disaster emerged. A strong ethic of self-reliance also militated against expecting assistance from the government (Lockwood 2004, 38–39). And, perhaps most important, the Populists were not well organized until a decade later and did not mount a serious campaign for governor until 1890. At that point, farmers suddenly had a credible explanation for their troubles and a target for their frustrations—and punishment began.

If our interpretation of the cultural element in natural disasters is correct, then it should be possible to point to a major disaster for which a government was plausibly responsible, yet for which it escaped electoral blame because the case for responsibility was never constructed by political opponents. The Spanish Influenza pandemic of 1918 represents a remarkable case of just that sort. The magnitude of the disaster was epic; most estimates of the worldwide death toll range from 20 million to 40 million, with some as high as 100 million. In the United States alone, the flu killed approximately half a million people—more than the total number of American battle deaths in World War I, World War II, Korea, and Vietnam combined (Crosby 1989; Kolata 1999, 285, ix–x). This was no mere blown college football game. If voters punished the incumbent government whenever they felt significant unhappiness, the millions of people who lost friends or family members to influenza in 1918 would have produced the greatest anti-incumbent landslide in American electoral history. But electoral retribution requires voters to imagine, however plausibly or implausibly, that incumbent leaders could have prevented or ameliorated their pain. In the case of the flu pandemic, that crucial attribution of political responsibility was lacking. As a result, as best we can tell, the electorate utterly failed to respond to the greatest public health catastrophe in U.S. history.

The 1918 midterm election occurred just as the pandemic was at its peak in many parts of the country, with flu deaths numbering more than one thousand per week in some major cities. Using detailed data on influenza mortality rates from 16 states and 29 major cities (Crosby 1989; Pyle 1986, 46–47), we examined voting patterns in the 1918 midterm election, looking for evidence

of electoral retribution aimed either at Democratic gubernatorial candidates or at incumbent governors regardless of party.²⁹ We also examined the 1920 presidential vote. In no case did we find reliable statistical evidence that voters in the worst-hit states and cities punished anyone at the polls.³⁰

One important race was almost certainly affected by the pandemic—the Senate contest in New Mexico, in which President Wilson clumsily attacked Republican Albert B. Fall at the same time Fall was grieving over the deaths from influenza of two of his children. Fall was elected by fewer than 2,000 votes, and Alfred Crosby (1989, 175) quite plausibly argued that “sympathy for the bereaved Fall caused Wilson’s attack to backfire.” In this isolated case, the horrific effect of the pandemic became a potent political issue; but in the country as a whole, remarkably, it did not.

It is impossible to know, even in retrospect, how much could have been done to minimize the loss of life in what was, after all, a vast and virtually unprecedented tidal wave of human misery. Nevertheless, it seems clear that a rational electorate could reasonably have held its leaders accountable, in part, for the devastating consequences of this natural disaster. Even with due allowance for the less developed public health technology of 1918, there is little reason to doubt that tens of thousands of flu victims could have been saved by more effective government action. Efforts to stem the contagion, or even to track its spread, were slow and disorganized (Crosby 1989, chaps. 1, 2, esp. 49–51; Kolata 1999, 10, 19, 22–23).

So why no electoral retribution? For one thing, blaming the government was not easy: the country was at war, making criticism seem unpatriotic. Both the government and the press downplayed flu risks (Barry 2004, chap. 29). Indeed, the pandemic seems to have received remarkably little national attention. As one historian put it, “When you talk to people who lived through it,

²⁹ As it happens, 13 of the 16 “registration states” for which detailed mortality data are available had Republican governors in 1918. Thus, voting patterns would look very different depending on whether voters chose to punish incumbent governors or the party of the Democratic president.

³⁰ In most cases, we examined the impact of influenza mortality rates in the final four months of 1918; for some cities we also had more detailed data that allowed us to examine the impact of flu deaths in the weeks immediately preceding the election. Not surprisingly, given the limitations of the available data, all of our statistical results were fairly imprecise and, in some cases, quite sensitive to changes in the sample or variable definitions. On the whole, however, it seems clear that the flu pandemic had little or no political effect. Some of the estimates suggest, quite implausibly, that incumbent governors actually gained votes in the major cities with the highest death rates. Only the estimate for Democratic votes at the state level had the “correct” (negative) sign, and even that estimate was of very modest magnitude.

they think it was just their block or just their neighborhood" (Crosby, quoted by Kolata 1999, 8). Victims were widely scattered around the country; and since people died of influenza every year, no one could be certain that their own spouse or parent or child was one of the "excess deaths" from the epidemic, much less a death that the government might have prevented.

Most important, no thread of elite rhetoric or popular discourse seems to have suggested any attribution of responsibility to President Wilson or other public officials. As long as no one supplied a convincing argument that the government did control or should have controlled the spread of the pandemic or its horrific consequences, the pain of millions failed to have any electoral impact. President Wilson was berated for the insufficiency of his efforts to stem the tide of shark attacks in New Jersey in 1916 and taunted with editorial cartoons featuring shark fins; but there is no evidence of a comparable outcry over his handling of the flu pandemic, except in an isolated instance in which he insensitively attacked a political opponent whose children had been among the victims.

The striking absence of a broad-based electoral response to the flu pandemic dramatically illustrates the importance of voters' cultural understandings of causation and responsibility. In the language of Deborah Stone's (1989, 283) typology of causal frameworks, voters thought of the pandemic as part of the natural world ("the realm of fate and accident") rather than as part of the social world ("the realm of control and intent"). Obviously, such cultural understandings are subject to change.³¹ But at the time, while hundreds of thousands of people died, no one thought to blame the pharaoh.

CONCLUSION

In most recent scholarly accounts, retrospective voting is a natural and rational feature of democratic politics. In our view it is natural, but not so obviously rational. Indeed, blind retrospection of the sort we have documented in this chapter seems to us to provide a significant challenge to the conventional understanding of political accountability in modern democracies.

We have shown that voters sometimes punish incumbent political leaders for misfortunes that are clearly beyond the leaders' control. Moreover, we have

³¹ Modern governments have certainly believed that the political cost of a major flu epidemic might be considerable, as witnessed by the Ford administration's aggressive—as it turned out, overly aggressive—response to the swine flu scare in the 1970s (Neustadt and Fineberg 1983).

shown that they do so with considerable regularity. The fact that American voters throughout the 20th century punished incumbent presidents at the polls for droughts and floods seems to us to rule out the possibility that they were reacting to subpar *handling* of misfortunes rather than to the misfortunes themselves. After all, it is hard to see how incumbent presidents' handling of droughts and floods could have been substantially worse than average over the course of an entire century.

Of course, voters may themselves contribute to poor disaster preparedness by insisting on low taxes and less intrusive government. In that case, government performance in response to disasters will nearly always seem poor in some absolute sense, and incumbents will be punished accordingly. But this sort of retrospective punishment is self-defeating in exactly the way we have suggested, since the randomness of the punishment from the standpoint of incumbents makes it pointless (in an electoral sense) for any incumbent to invest *ex ante* in adequate preparations for disasters (Healy and Malhotra 2009).

What, if anything, is wrong with blind retrospection? In a world of great uncertainty and costly attentiveness, perhaps this is exactly what voters should do to hold their leaders accountable—"only calculate the changes in their own welfare," as Fiorina (1981, 5) put it, and vote accordingly. Maybe there was something Woodrow Wilson could have done for the Jersey Shore, even if no informed person at the time could think of what that might be. And if a few pharaohs perish needlessly as a result of fanciful causal chains in the voters' minds, is that such a high price to pay for a system in which every incumbent has a strong incentive to do whatever she can to maximize her citizens' welfare? In short, aren't voters behaving rationally when they reward or punish incumbents for good or bad times?

In one sense, this view of retrospective voting is quite right. When voters are utterly ignorant about whether and how their leaders' actions affect their own welfare, blind retrospection may be "rational" in a narrow, technical sense. However, that does not imply that it will be sensible or prudent. Crazy beliefs can make crazy behavior "rational." But as the models presented in chapter 4 demonstrate, ignorance about reality can be quite costly in the realm of democratic politics, just as it is in other aspects of life.

Our account strikes directly at the heart of the common normative justification for the retrospective theory of political accountability. In that view, while voters may know very little, they can at least recognize good or bad government performance when they see it. Thus, they can retrospectively reward or punish leaders in a sensible way. We agree that voters operating on the basis of a valid, detailed understanding of cause and effect in the realm of public

policy could reward good performance while ridding themselves of leaders who are malevolent or incompetent. But real voters often have only a vague understanding of the connections (if any) between incumbent politicians' actions and their own well-being. Even professional observers of politics often struggle to understand the consequences of government policies. Politics and policy are complex. As a result, retrospective voting is likely to produce consistently misguided patterns of electoral reward and punishment.

To sensibly translate an assessment of economic or social *conditions* into an assessment of political *performance*, citizens must find—and accept—a valid cultural understanding of the causal relationships linking the actions of public officials to changes in the public's welfare. When is one such understanding accepted rather than another? A healthy democratic culture among political elites can, no doubt, help significantly to constrain the scapegoating impulses of democratic electorates. Yet just as much or more seems to depend on the political folk culture among ordinary citizens, or on different folk subcultures for different groups.

Tracing how a specific political attribution of blame attains plausibility among inattentive citizens suddenly in want of an explanation for their troubles is a daunting task. The young and old, the rich and poor, the educated and uneducated are all swept along by the ideas popular in their groups, and sometimes all are swept along together. Certain looks, certain sounds, and certain arguments meet widespread needs in a particular culture at a particular time, nearly always for complex reasons unforeseeable in advance. The only certainty is that there is nothing very rational about the process.

Our analysis suggests that “blind” retrospection on the basis of overall well-being, with no consideration of the impact of government policies on that well-being, is very unlikely to provide much in the way of effective accountability, notwithstanding the fact that it may be “rational” in a narrow sense. Voters ignorant about evidence and causation, but supplied with a tale of incumbent responsibility, will punish incumbents whenever their subjective well-being falls below some fixed standard, regardless of whether or not their pain is in fact traceable to the incumbents’ policies.

The “rough justice” (Fiorina 1981, 4) embodied in the electoral verdicts rendered by such voters is likely to be very rough indeed. And the rougher it is, the less incentive reelection-minded incumbents will have to exert themselves on the voters’ behalf. As a result, voters who cannot distinguish the effects of shark attacks and droughts from the effects of tax policies and foreign wars are likely to experience more than their share of misguided tax policies and disastrous foreign wars. This sort of voting is hard to square with rosy in-

terpretations of retrospective accountability, and even harder to square with the folk theory of democracy, in which ordinary citizens assess their public life critically, weigh the qualifications of competing candidates for public office, and then choose between the candidates in accordance with their own values.

Democracies take their electoral direction from human beings with limited capacities for self-government. Human passions remain powerful, and human understanding remains weak. Under sufficient pressure, voters sometimes lash out blindly. Such events are not quaint historical footnotes rendered irrelevant by modern education and hygiene. Indeed, in just the past century many citizens—and many prominent intellectuals—have been enthusiastic supporters of Nazis, Bolsheviks, Mao’s Communist guerillas, and a host of other brutal demagogues whose policies seemed to offer attractive solutions to fundamental social problems that the previous incumbent rulers had failed to master.

Blind retrospection afflicts us all. It is the inevitable consequence of bewildering social complexity and human cognitive limitations—limitations that the rise of democratic government has not altered. The conventional account of retrospective voting, minimalist as it is, fundamentally underestimates the limitations of democratic citizens and, as a result, the limitations of democratic accountability.