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# BLIND RETROSPECTION ELECTORAL RESPONSES TO DROUGHT, FLU, AND SHARK ATTACKS

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**Abstract**

Students of democratic politics have long believed that voters punish incumbents for hard times. Governments bear the responsibility for the economy in the modern era, so that replacing incompetent managers with capable alternatives appears to be a well-informed, rational act. However, this vision of a sophisticated retrospective electorate does not bear close examination. We find that voters regularly punish governments for acts of God, including droughts, floods, and shark attacks. As long as responsibility for the event itself (or more commonly, for its amelioration) can somehow be attributed to the government in a story persuasive within the folk culture, the electorate will take out its frustrations on the incumbents and vote for out-parties. Thus, voters in pain are not necessarily irrational, but they are ignorant about both science and politics, and that makes them gullible when ambitious demagogues seek to profit from their misery. Neither conventional understandings of democratic responsiveness nor rational choice interpretations of retrospective voting survive under this interpretation of voting behavior.

**Blind Retrospection**
**Electoral Responses to Drought, Flu, and Shark Attacks**[1]

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, somebody has to take the blame. For ancient Israel, disasters were God's punishment for sin—perhaps the ruler's sin, perhaps Israel'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. When it failed to do so, as happened repeatedly in the famines and political disorder of the First Intermediate Period (ca. 2200 BCE), some scholars believe that the pharaoh's reign was shortened, and perhaps his life as well (Bell 1971; Hassan 1994).

Through the centuries, competitors of the ruler have been well aware that disaster presents them with an opportunity. When disasters take on catastrophic dimensions, not just the ruler but the entire regime can come under suspicion. Writing of the Black Death in the 14th century, which may have killed a third or more of the European population, Herlihy (1997, 64) remarks:

> 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.

During the plague years, spontaneous religious and political movements arose to threaten church and government. A few of these bands agitated against the groaning inequities of medieval society. Most were less attractive. Some, fired by conspiracy theories, targeted disliked minorities such as beggars and Jews. Across Europe, thousands of Jews were murdered before the traditional spiritual and secular authorities managed to halt the fury of the mobs (Herlihy 1997, 66).

Contemporary democratic rulers have little aura of divinity about them, nor have they faced Biblical famines or medieval plagues. Nonetheless, the citizenry continues to hold them responsible for routine hardships and misfortune when election time comes. In this paper we examine electoral responses to natural disasters that are clearly beyond the control of incumbent politicians. We find that voters regularly punish incumbent governments for such events, as long as they can find some psychologically appealing connection—whether plausible or not—between the disaster and the government.

**Holding Incumbents Responsible: Theory and Evidence**

The theory of retrospective voting provides the most compelling account we have of the relationship between leaders and citizens in democratic political systems. The theory is attractive, in significant part, because it seems to rescue voters from the charge that they are

too uninformed or too disengaged to play a meaningful role in the democratic process. For example, the influential works of Key (1966), Kramer (1971), and Fiorina (1981) all portrayed retrospective voting as a rational alternative to the rather bleak portrait of habitual, socially determined political behavior provided by Berelson, Lazarsfeld, and McPhee (1954), Campbell et al. (1960), and other early analysts of electoral choice.

Key famously put the "perverse and unorthodox argument" of his book in a nutshell: that "voters are not fools" (1966, 7).[2] Kramer described his study as "an attempt to put a modified form of the rationality hypothesis to a different and in some respects more direct test than is readily possible with survey data" (1971, 131), and characterized his results as demonstrating "that election outcomes are in substantial part responsive to objective changes occurring under the incumbent party; they are not 'irrational,' or random, or solely the product of past loyalties and habits, or of campaign rhetoric and merchandising" (1971, 140). And Fiorina (1981, 3-4) devoted the opening pages of his book to criticizing "behavioral political scientists" of the 1950s and 1960s who "made sport of civics-book accounts of American politics" without "formulating alternatives" capable of accounting for the sort of "rough justice" evident in the election outcomes of 1932, 1952, 1958, 1968, and 1974.

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[2] Key's (1966, 75-76) discussion of this point seems worth quoting at length: "The election of 1952 may . . . be more than plausibly characterized as a verdict of dissatisfaction with the past performance of the Democratic Administration. It may, of course, be said that a vote against Mr. Stevenson because one disapproved of Mr. Truman's performance represented an irrational act. Mr. Truman, after all, was not the candidate. Moreover, Mr. Stevenson went to some pains to attempt to disassociate himself from the Truman Administration. He maintained a separate campaign headquarters in Springfield and in other ways sought to establish himself in the public mind as independent of the Truman Administration. Yet voters—or a significant number of them—voted against Mr. Stevenson because they disapproved of Mr. Truman, despite Mr. Stevenson's most adroit efforts in evasion and avoidance. It is well that a political party cannot avoid accountability for its past performance. The only really effective weapon of popular control in a democratic regime is the capacity of the electorate to throw a party from power. Not only was Mr. Stevenson rejected; other Democrats also fell by the wayside and the party leadership collectively lost, at least for a time, the fruits of power. In effect, many Democrats felt the effects of public dissatisfaction with their party's performance. Had Mr. Stevenson not been saddled with responsibility for past Democratic performance—or were other like-situated candidates not usually so saddled—the electorate would be deprived of its most effective instrument for control of governments."

At the same time, the theory of retrospective voting has the considerable virtue of requiring less of citizens than alternative conceptions of democracy do. In particular, it dispenses with the unrealistic notion that ordinary citizens vote on the basis of detailed preferences regarding every issue that might conceivably come before their future leaders. As Fiorina (1981, 5) put it, "They need *not* know the precise economic or foreign policies of the incumbent administration in order to see or feel the *results* of those policies. . . . In order to ascertain whether the incumbents have performed poorly or well, citizens need only calculate the changes in their own welfare."

While we attach great importance to these virtues, we believe that the theory as it stands fails to do justice to the very considerable logical and informational difficulties faced by retrospective voters in the process of translating evaluations of "changes in their own welfare" into evaluations of incumbent political officials. Fiorina's sketch of how this might be done provides a useful starting point for our analysis. In the passage just cited (1981, 5), he notes that voters

> typically have one comparatively hard bit of data: they know what life has been like during the incumbent's administration. They need *not* know the precise economic or foreign policies of the incumbent administration in order to see or feel the *results* of those policies. And is it not reasonable to base voting decisions on results as well as on intentions? In order to ascertain whether the incumbents have performed poorly or well, citizens need only calculate the changes in their own welfare. If jobs have been lost in a recession, something is wrong. If sons have died in foreign rice paddies, something is wrong. If thugs make neighborhoods unsafe, something is wrong. If polluters foul food, water, or air, something is wrong.

But is it true that voters can "ascertain whether the incumbents have performed poorly or well" simply by calculating "changes in their own welfare"? If jobs have been lost in a recession, something is wrong, but is that the president's fault? If it is not, then voting on the basis of economic results is no more rational than killing the pharaoh or beating up the Jews.[3]

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[3] Fiorina (1981, 202) noted in passing that the normative appeal of the theory of retrospective voting hinges in significant part on the assumption "that the *electorate does a passable job of attributing responsibility to government decision makers*." However, his brief discussion of the problems raised by that assumption focused almost entirely on the issues of divided government and responsible parties, rather than on the more basic questions considered here.

As it happens, a large fraction of the empirical scholarly literature on retrospective voting focuses specifically on economic voting. Politicians and journalists have long believed that weak economies lead to lost elections for incumbents. In the past few decades, Kramer (1971), Markus (1988), Lewis-Beck (1988), and many others have provided an impressive body of systematic evidence that voters reward incumbent politicians for good economic times and punish them for bad times. While details of the causal path are still disputed, a virtual consensus has emerged that the effect of "pocketbook voting" is real and substantial.

One strand of the voluminous literature on economic voting focuses on the possibility that myopic voters can be fooled by irresponsible policies that stimulate the economy around election time, leaving the costs to be borne after the votes are counted (Tufte 1978). But for the most part, the strong tendency of voters to reward incumbents for good economic times and punish them for bad times is viewed as a mark of the rationality of democratic electorates. Like ancient peoples backed by their cherished worldviews, contemporary analysts and observers think of governmental responsibility for the economy as part of the natural order of things. Economists tell us that competent governments can reduce the likelihood of recession and ameliorate it when it happens, and most other elites take their pronouncements seriously. Though the recommended rituals to prevent governments from going astray seem to change every few decades, we think of those mutations as scientific progress. Thus voters troop off to the polls to replace incumbents in hard times, engaging in what seems undeniably a rational act, not at all like kicking the dog in frustration.

In truth, of course, the rational basis for pocketbook voting is murky at best. Whether the economy flourishes or falters, the president may or may not be responsible. Moreover, whatever he did right or wrong may or may not be relevant in the circumstances, possibly quite different, that prevail after the election. If pocketbook voting worked as advertised, voters would get more of the same if they reelect the incumbent, a fresh draw if they do not. But no one has ever demonstrated that the temporal continuity of economic conditions is

significantly affected by election outcomes, as it would have to be for retrospective economic voting to be prospectively rational.[4]

Another difficulty with contemporary self-understanding is that electorates seem to have punished their leaders for economic hardship well before Keynes taught governments how to control (more or less) a modern industrial economy. Here is 19th century British Cabinet member and former prime minister Benjamin Disraeli, writing to Lord Lytton on August 14, 1879 (Monypenny and Buckle 1929, 1347):

> Although we have entered 'the sixth year of our reign,' our parliamentary majority, instead of diminishing, has increased. . . . The only danger and difficulty which the present Ministry has to encounter are natural. . . . After four bad harvests in this country, we are apparently about to meet a fifth dearth.

Indeed, much to the surprise of contemporary observers, the following March brought a disastrous loss of more than 100 Tory seats and the defeat of the government (Monypenny and Buckle 1929, 1394).[5]

The same forces have had larger consequences elsewhere. Poor weather and bad harvests have been given substantial credit for the rise of the Populists in Nebraska (Barnhart 1925) and in Kansas (Miller 1925) during the last decade of the 19th century. The same argument has been made for Socialist success in Saskatchewan, Canada, during the Great Depression (Lipset 1959) and for the astonishing takeover of the Canadian province of Alberta by the funny-money Social Credit Party after a Thirties plague of drought and grasshoppers (Irving 1959). Most dramatically, virtually all Weimar historians regard Hitler's

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[4] Of course, voters who find themselves in the midst of an economic depression during the campaign can forecast with some confidence that it will persist beyond Election Day; but that is true regardless of how they vote. Our point is that the state of the economy in the months before the election—the usual presumed basis of pocketbook voting—has never been shown to provide a reliable indication of whether replacing the incumbent government would be likely to make things better or worse.

[5] We are indebted to W. Phillips Shively for this reference.

electoral triumphs in rural Germany, so essential to his takeover, as stemming in large part from the grinding economic distress of the farmers (for example, Kershaw 1983, ch. 1).

In none of these cases did the voters imagine that governments controlled the harvests, but that made no difference. As Disraeli put it in discussing his party's 1880 loss at the polls (Monypenny and Buckle 1929, 1395),

> Never was so great a discomfiture with a cause so inadequate. I think, as far as I can collect, 'hard times' was the cry against us. The suffering want a change—no matter what, they are sick of waiting.

The simple argument that hard times threaten governments is not new. There are records of Egyptian court officials wishing their pharaoh a good Nile flood. The historical evidence for angry retrospective citizens runs across many countries and many centuries, and the recent literature on pocketbook voting merely extends it to the current era.

We wish to go beyond that simple correlation, however, to argue a deeper point. Our assertion is that retrospection is blind. When the voters are in pain they kick the government, justifying themselves with whatever plausible cultural constructions are made available to them. Only if no such constructions are available, or if no ambitious contender for the throne can think of 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, even in situations where objective observers can find little rational basis to suppose that those incumbents have had any part in producing the voters' pain.

This argument rests on a base of evidence that is historically quite broad but uncomfortably thin. The supporting claims are built at best on rough correlations across years or regions. Only for recent decades, and only for the case of pocketbook voting, have scholars produced the kind of evidence that quiets skeptics. Unfortunately, most pocketbook voting in the post-Keynesian world is arguably perfectly sensible, and thus irrelevant to our argument.

A more detailed recent analysis by Wolfers (2002) is more directly relevant to our argument. He focuses on distinguishing electoral responses to economic conditions plausibly attributable to incumbent politicians and those clearly outside of any incumbent's control. Using data from U.S. gubernatorial elections, he shows that both categories of economic conditions have significant electoral effects. Voters evaluating their own governor "appear to perform simple rule-of-thumb adjustments" that take into account (presumably exogenous) national economic fluctuations, but they fail to adjust appropriately for other sources of good or bad luck (Wolfers 2002, 14). For example, "voters in oil-producing states tend to re-elect incumbent governors during oil price rises, and vote them out of office when the oil price drops," notwithstanding the fact that governors can have little or no effect on oil price changes. Wolfers concludes that "voters make systematic attribution errors and are best characterized as quasi-rational" (2002, abstract).

Fortunately (for analytical purposes, that is), the welfare of voters is sometimes drastically affected by factors less ambiguous than economic policy—factors whose causes are clearly beyond the control of incumbent politicians. In particular, natural disasters provide a fertile field in which to examine the reactions of voters to significant changes in their welfare that are not plausibly attributable to government officials. To the extent that voters engage in sophisticated attributions of responsibility they should be entirely unresponsive to natural disasters, at least on average; to the extent that they engage in blind retrospection, they should exhibit "systematic attribution errors" of much the sort discovered by Wolfers in his analysis of economic voting.

Unfortunately, only a handful of studies have addressed the effect of natural disasters on electoral outcomes (Abney and Hill 1966; Arceneaux and Stein n.d.), and only local elections have been studied. In the remainder of this paper we provide a somewhat broader analysis of natural disasters and their electoral consequences. We focus on American electoral politics in the 20th century, examining a variety of cases in which voters may have been tempted to punish incumbent politicians for natural disasters. The next section takes up the first of these, the unjustly neglected topic of how shark attacks influence the presidential vote.

**Shark Attacks in New Jersey, 1916: 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.[6] 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. 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, also ventured out past the lifelines at Spring Lake beach, some forty five 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.

Nearly all of the diminished numbers of Jersey Shore swimmers stuck close to shore in the days after the two deaths. However, no one worried about boys swimming in a creek on July 12 in the town of Matawan, about two miles from open water. One was attacked and killed by a shark, as was 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* had a front-page headline on July 14: 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.

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[6] Unless otherwise noted, the historical rendition follows Fernicola (2001), the most complete account. See also Capuzzo (2001).

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.[7] (Fernicola 2001, 22). The general climate of skepticism led the *New York Times* to place its article about the first attack only 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.[8]

In the aftermath of the attacks, governments, particularly the federal government, were called on for help. The resorts were losing money rapidly, with a quarter million dollars in reservations cancelled within a week. Some resorts had 75 percent vacancy rates in the midst of their high season (Capuzzo 2001, 274). Losses may have amounted to perhaps as much as $1 million for the season altogether, a sizable sum in 1918 (Fernicola 2001, 174). Letters poured into Congressional offices from the affected counties, demanding federal action, though there was little any government agency could do. Fernicola (2001, 70) describes 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 reelection in November, using the shark fin as the symbol for his potential loss. 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."

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,

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[7] 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.

[8] Parallels to the film "Jaws" and its sequels are no accident. Peter Benchley, the author of the book on which the film was based, is a New Jersey resident, and the film version, though set on Long Island, New York, includes a reference to the 1916 New Jersey attacks.

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 problem disappeared 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 re-election 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 were a natural disaster causing several deaths plus considerable emotional and financial distress to entire communities. Second, the government was thought to be responsible for dealing with the crisis, and high federal officials were present at the scene from the beginning. Third, the election followed the crisis quickly enough that the summer's events would have been fresh in the minds of the voters. The fact that no government has any influence over sharks should have been irrelevant.

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 at the New Jersey county level. The Wilson vote in 1916 is the variable to be explained. Our key independent variable is "beach county,"

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 also include two control variables 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.[9] For in the East, the Roosevelt vote from 1912 returned almost entirely to Charles Evans Hughes, the Republican candidate, in 1916. 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. Across New Jersey counties, Wilson's 1912 vote is an excellent predictor of his 1916 vote. As the rest of this section will show, the same was true even at the township level. 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 linear regression fit when 1912 was statistically controlled.[10]

One other control variable is needed due to the very heavy immigration into New Jersey throughout the late 19th century, and especially during the early 20th century. (The state's official Census population grew by 35 percent between 1900 and 1910.) New York City was the port of entry for nearly all these immigrants, and many of them and their children ended up in those New Jersey counties near the city. Urban bosses, particularly though not exclusively Democratic Party officials, arose to tend to these new voters and to

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[9] Socialist and other minor candidates, include Prohibition advocates, were also running both years, but of course only Roosevelt was a serious third-party contender for the presidency.

[10] Adding the Roosevelt proportion of the vote from 1912 generated a small positive, statistically insignificant coefficient. Wilson in 1916 undoubtedly picked up some small fraction of Roosevelt's 1912 New Jersey adherents, but visual inspection of the data indicates that the effect is too small and too erratic to detect reliably with county-level data. Keeping the Roosevelt variable made no difference in subsequent analyses, and so it was dropped, but not forgotten—see note 15.

mobilize them at election time. The notorious Frank Hague was getting his start in Jersey City at this time, for example (McKean 1940, ch. 3).

Shortly after Wilson's election as governor, the New Jersey bosses turned against him, and they opposed his nomination for president in 1912 (Link 1947, chs. 8, 9, and pp. 427-28).[11] Between 1912 and 1916, Wilson's control of the New Jersey Democratic party, once nearly complete, slipped away (Blum 1951, 39, 76; Link 1947, 288), making the machine counties less enthusiastic about working for him in 1916. For example, Hague took over in Jersey City from the progressive Wilson ally, H. Otto Wittpenn, during this period (McKean 1940, ch. 3; Connors 1971, ch. 3).

Thus, we include a control variable for "machine counties," defined as those counties with at least 30,000 voters in 1916 and 60 percent or more "foreign" citizens in the Census of 1910. "Foreign" here means that the citizen was foreign-born or had at least one parent foreign-born (the so-called "hyphens" in the vernacular of the time). The counties so defined turn out to be Bergen, Hudson, Essex, and Union counties, adjacent to each other and just across the state line from New York City.

Hudson (Jersey City) and Essex (Newark) were particularly well known for boss control. In fact, alone among New Jersey's counties, Wilson never did got 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). 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. Simply put, Essex County in this electoral period does not act like the rest of New Jersey at the polls and does not belong in the same regression. We therefore excluded it from the analysis. The other 20 counties make up our sample.

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[11] 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.

The results of regressing Wilson's 1916 presidential vote in each county (except Essex) on a variable for beach county, a variable for machine county, and Wilson's 1912 vote are presented in Table 1. All of the variables are substantively significant and sensibly sized, and each of them is statistically significant beyond the .01 level. The overall multiple correlation R = .973, and the regression has a prediction error of just 1.7 percentage points.[12]

The estimated negative effect on Wilson's vote in the beach counties is a little more than 3 percentage points, with a 95% confidence interval confined between 1.2 and 5.2. The shark attacks indeed seem to have had an impact—about one-fourth the effect that the Great Depression had on Herbert Hoover's vote in New Jersey 16 years later.[13]

**Table 1. The Effect of Shark Attacks on the 1916 Presidential Vote**

Parameter estimates from ordinary least squares regression analyses of 1916 Woodrow Wilson vote fraction (two-party %) by New Jersey county

| | Coefficient | Standard Error | *t*-ratio |
|---|---|---|---|
| **Beach County** | −3.23 | 1.0 | 3.27 |
| **Machine County** | −5.66 | 1.1 | 5.19 |
| **Wilson 1912 Vote (3-way fraction)** | .947 | .061 | 15.4 |
| **Intercept** | 4.54 | 2.8 | 1.63 |
| std err of reg | 1.70 | | |
| adjusted $R^2$ | .94 | | |
| N | 20 | | |

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[12] None of the residuals falls more than two standard deviations from zero, and only one of them (Salem) is near it, 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 regression, amply confirming its exclusion from the sample.

[13] Hoover's vote share in New Jersey fell from 59.8% in 1928 to 47.6% in 1932.

The efficiency of the estimate is due to the very consistent effect across the beach counties, as may be seen from Figure 1. This figure shows the partial regression plot of Wilson's 1916 vote on his 1912 vote with the machine-county variable controlled. The ordinary least-squares fitted lines are estimated separately for beach and non-beach counties, with Essex excluded. As the graph shows, the beach counties are each depressed nearly the same amount from their expected 1916 vote, and the consistency of the effect both confirms the specification visually and tightens the standard errors statistically.[14]



Figure 1: Wilson in New Jersey Counties, 1916 vs. 1912

(machine county dummy controlled)

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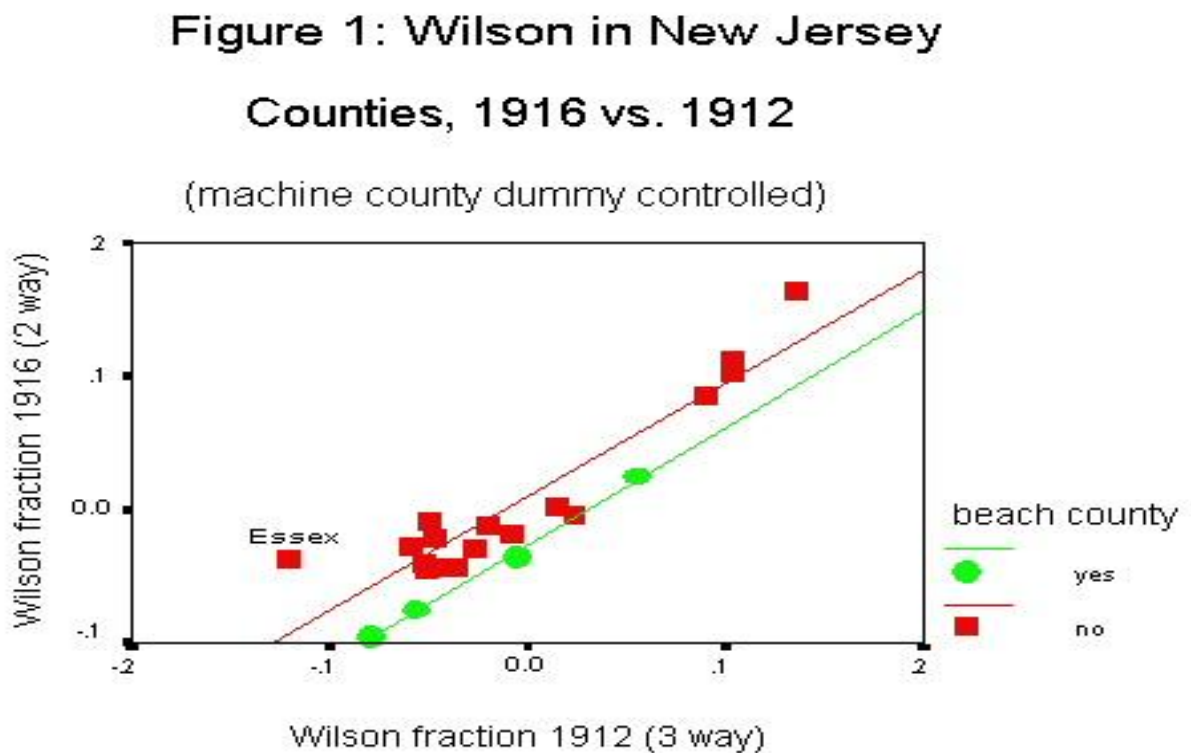
[14] The non-beach county squarely on the beach regression line is Middlesex, the shoreline county just north of Monmouth though not normally considered "Jersey Shore." The machine counties included in the regression would not fit badly on the beach-county regression line either. They are themselves on the water or adjacent to the Hudson River. Capuzzo (2001, 270-273) notes 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 or most of the negative "machine county" effect is due to the sharks.

We ran many other regressions with alternate versions of the partisanship controls. As mentioned earlier, none fit as well as the 1912 vote, and the estimated effect of the shark attacks remained at 2 to 4 percentage points so long as the 1912 vote was entered as a control. We also tried controls for the proportion Irish, the proportion German, 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 the 1916 Easter Rising in Dublin (which the British suppressed) or with his potential entry into World War I on the British side. We found no effect, which is consistent with the conclusions of analysts of the national vote at the time: 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 Leary 1967 and Cuddy 1969 on the Irish, and Link 1954, 232-251 on the Germans.) In short, the shark effect stands up well under alternate specifications.[15]
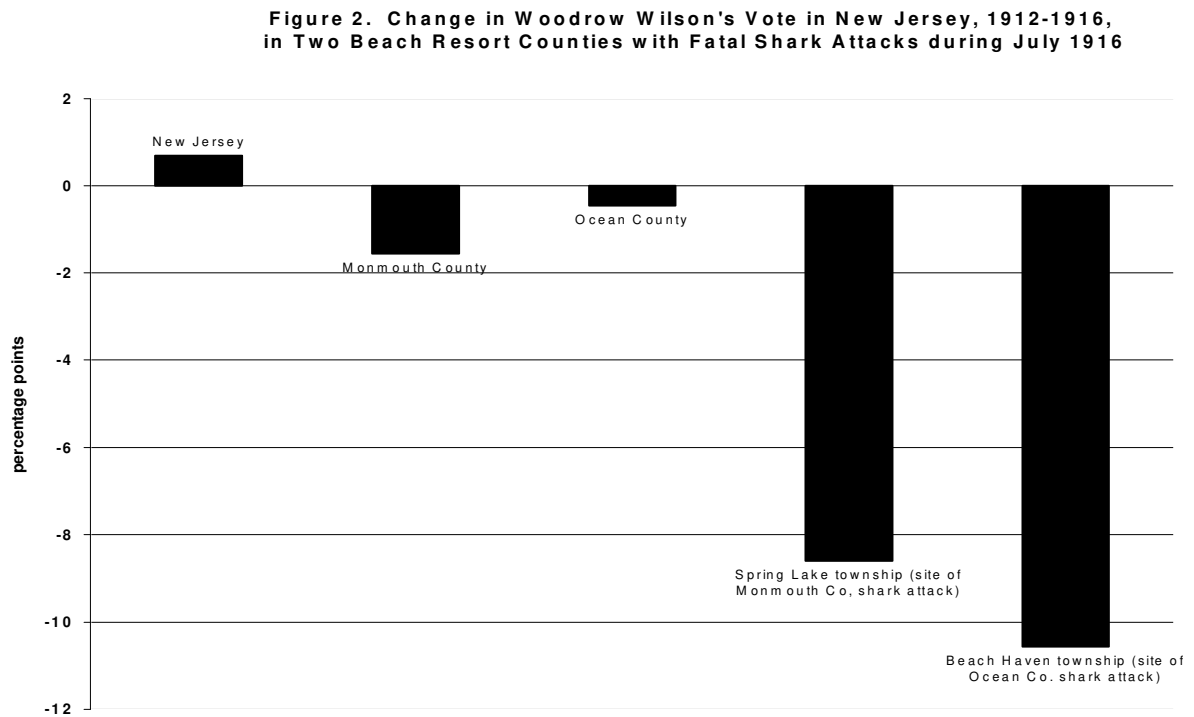
We undertook two additional investigations with different samples. First, we examined the vote in the first two shore townships where the attacks took place.[16] Both Beach Haven and Spring Lake were small, stable communities, making comparison sensible.[17] Figure 2 shows the vote change for Wilson 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 negligible changes in the Wilson vote in their counties and in the state. These are vote losses equal to those Herbert Hoover suffered statewide in New

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[15] 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 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 1.

[16] Matawan Township and Matawan Borough, where the final two shark deaths occurred, 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 case, the rapid growth in the number of voters in both places between 1912 and 1916 makes comparison impossible; more than a quarter of the 1916 voters in Matawan township had not been there in 1912.

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

Jersey in 1932 at the height of the Great Depression. It is apparent that something drastically reduced enthusiasm for Woodrow Wilson in these two townships.

**Figure 2. Change in Woodrow Wilson's Vote in New Jersey, 1912-1916, in Two Beach Resort Counties with Fatal Shark Attacks during July 1916**



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 communities, nearly all on a bank of land clearly separated from the mainland. Thus there is no difficulty in separating those seven communities right on the beach from the twelve near the beach but not on it.[18] The western border of the near-beach area was set to the current New Jersey turnpike, which runs within a few miles of the shore in Ocean County. These two areas had nearly identical Democratic percentages for Wilson in 1912 (36.3% at the beach and 34.1% in the near-beach), and thus are comparable.

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[18] 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.

In each area, we compared Wilson's vote percentages in 1912 and 1916. If the argument of this paper is correct, the beach voters should show the largest drop in support for Wilson, while the near-beach citizenry should be largely unaffected. The actual vote change turns out to be a drop of 8.2 percentage points in the beach area, compared to a tiny 0.2 percentage point gain in the near beach, an easily statistically significant difference.[19] Again, we find that disaffection for Wilson was widespread in the beach areas whose livelihood was most directly affected by the shark attacks, far different from the otherwise comparable areas next door, where Wilson's vote was nearly constant.[20]

In summary, then, every indication in the New Jersey vote returns for 1912 and 1916 is that the horrifying shark attacks during the summer of 1916 reduced Wilson's vote in the fall. Retrospection here was surely blind. If bathers insist on swimming in the ocean, governments then and now can do nothing about shark attacks, as the subsequent attacks in New Jersey in 1960 and the regular encounters in Florida, California, South Africa, and Australia demonstrate (Fernicola 2001, ch. 5). Shark attacks are natural disasters in the purest sense of the term, and they have no governmental solution. Yet the voters punished anyway.[21]

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[19] There were 311 beach and 2645 non-beach voters in 1912, and 349 beach and 2859 non-beach voters in 1916. This comparison includes Point Pleasant Beach Borough as part of the near-beach. In spite of its name, the overwhelming bulk of its population lived in Point Pleasant, which is not on the beach. However, this borough's 30 percent increase in the vote from 1912 to 1916 is the largest of any beach or near-beach community, making its two presidential years less comparable and suggesting that it should be excluded from the analysis. If Point Pleasant Beach Borough is excluded, the near-beach vote change alters from +0.2 percentage points to −0.6, still very different from the −8.2 effect at the beach. Similarly, if all beach and non-beach communities with more than 20% increase in the vote from 1912 to 1916 are excluded, the non-beach vote change becomes −0.5, while the beach change is −11.8. In short, these alternate versions of the sample lead to precisely the same substantive conclusion.

[20] The same finding from the Ocean County township sample holds when medians are used in place of means, and when (weighted or unweighted) regressions are run with the townships as units of observation.

[21] On 17 December 1967 Australian prime minister Harold Holt disappeared while swimming in shark-infested waters at Cheviot Beach near Portsea, Victoria. His body was never found. Being devotees of democracy, however, we disapprove of this apparent attempt by the sharks to cut out the middleman.

Of course, it is possible that the voters did not blame the government for the attacks themselves, but did blame it for not helping them with their economic distress. In that case, retrospection might not be blind. No doubt voters told themselves something like that at the time. Yet in the case of the sharks, it is not clear what the government could have done to help the local economy. 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. Of course, from the perspective of a century later, it is obvious that extending 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. In sum, for the case of the New Jersey shark attacks, "failed disaster assistance" seems a weak hypothesis driven by insufficient historical perspective.

**Droughts and Floods, 1896-2000**

Having examined one especially colorful instance of blind retrospection, we turn in this section to a broader analysis covering the entire continental United States and the entire 20th century. That analysis focuses on electoral responses to climatic conditions—in particular, to droughts and floods—in presidential elections. Using data on drought and flood conditions in 48 states throughout the century, we show that voters do indeed punish the incumbent party at the polls for presiding over bad weather.

The comprehensiveness of the drought and flood data with respect to time and space plays a crucial role in our argument, because it effectively rules out two significant objections to our demonstration of blind retrospection in the case of the New Jersey shark attacks—or, for that matter, in *any* specific instance of natural disaster. On one hand, as Barnhart (1925, 529) wrote in his pioneering study of drought and the rise of the Populist Party, in the absence of "detailed analysis . . . the relation between lack of rainfall and voting can be dismissed by the dubious as a coincidence." With more than 5000 state-level observations covering more than a century, we have unusual leverage to conduct the "detailed analysis"

necessary to eliminate coincidence as a plausible alternative hypothesis. On the other hand, even a genuine negative electoral response to any particular drought, flood, or other natural disaster may be interpreted as (perhaps quite rational) punishment for *exacerbating* or failing to *ameliorate* the disaster rather than as (manifestly irrational) punishment for the disaster itself. To quote again from Barnhart (1925, 540):

> 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 drouth 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.

We acknowledge that any particular drought, flood, or other natural disaster may be an occasion for rational punishment of incumbent politicians, either because those politicians are responsible for some of the conditions exacerbated by the disaster or because they fail to take adequate steps to ameliorate its effects. However, it is manifestly irrational for voters to punish incumbent politicians for droughts, floods, or other natural disasters *in general*. A rational 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. Thus, we should expect to observe rational electorates *rewarding* incumbents for better-than-average responses to natural disasters as often as we observe them *punishing* incumbents for worse-than-average responses.

In contrast, our account of blind retrospection suggests that whenever voters are in pain they will punish the incumbent government, as long as some more or less plausible cultural understanding connects the pain with the government. That connection may involve a good deal of ideological substance and suggest important policy implications, as in the case of Barnhart's Nebraska farmers pondering the railroad rates. It may be heightened by the salience of governmental activities intended to prepare for and respond to disasters (Arceneaux and Stein n.d., 7-8). On the other hand, it may involve a much more amorphous sense that "something is wrong" (Fiorina 1981, 5). Sometimes particularly skillful

incumbents, or incumbents with inept competitors, may escape blame. But most of the time they will fail.

The data on droughts and floods employed in our analysis 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.[22] The available data consist of monthly readings from 1895 to the present of temperature, precipitation, and hydrological conditions in each of 344 climate divisions covering the contiguous 48 states. These data represent summaries of 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.[23] A PHDI reading of zero represents a balance between moisture supply and demand, while negative values represent droughts and positive values represent wet spells.[24] 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.[25] 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.[26]

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[22] 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.

[23] 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.

[24] 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 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.

[25] This calculation assumes that equally severe droughts and wet spells are equally painful to voters. We investigated that assumption by repeating our regression analyses using separate measures of droughts and wet spells. The estimated effects were generally similar. For example, in the simplest regression model presented in Table 2 the estimated average effect of the absolute PHDI value is

The result of these calculations is an index of climatic pain running from .04 to 49.08, with a mean value of 11.01 and a standard deviation of 6.28. Low values of the index are good and high values are bad for voters and thus, according to our account, for incumbent presidents. Our 5088 observations (for each of 48 states in each of 106 years) include 657 (12.9 percent) with absolute PHDI values in excess of 18, the equivalent of a full year of "severe" drought or wetness; 207 observations (4.1 percent) 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 regression analyses of state-level voting data in 27 presidential elections (from 1896 through 2000). Our analyses employ eight different versions of our drought index.[27] In each case, the dependent variable is the incumbent party's vote percentage; we expect the effect of drought on the incumbent party vote to be negative. Table 2 presents the key results.

As a rough control for other factors affecting the incumbent party's fortunes in each state in each election, we include the incumbent party's vote in the same state in the previous two presidential elections, percent rural, and a dummy variable for southern states. The effects of all of these factors are allowed to vary from one election to the next, so there are 135 coefficients in each regression model in addition to those reported in Table 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.

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−.061 (with a standard error of .023); the corresponding analysis with separate coefficients produces estimates of −.059 (.031) for droughts and −.063 (.035) for wet spells.

[26] 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.

[27] All our regression 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. Standard errors are computed appropriately using the weighted regression routine in STATA.

**Table 2. Droughts, Floods, and Presidential Elections, 1896-2000**

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

| | Unweighted Drought Index | | | | Rural Drought Index | | | |
|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| **Election Year Drought Index** | −.0607 (.0228) | −.0531 (.0237) | --- | --- | −.1733 (.0595) | −.1437 (.0630) | --- | --- |
| **(Election−1) Drought Index** | --- | −.0407 (.0230) | --- | --- | --- | −.1087 (.0623) | --- | --- |
| **(Election−2) Drought Index** | --- | .0182 (.0242) | --- | --- | --- | .0293 (.0652) | --- | --- |
| **(Election−3) Drought Index** | --- | −.0397 (.0244) | --- | --- | --- | −.0177 (.0628) | --- | --- |
| **Cumulative Drought Index** | --- | --- | −.0256 (.0091) | --- | --- | --- | −.0590 (.0228) | --- |
| **Time-Weighted Drought Index** | --- | --- | --- | −.1041 (.0326) | --- | --- | --- | −.2518 (.0771) |
| std err of reg | 3.65 | 3.64 | 3.64 | 3.64 | 3.64 | 3.64 | 3.65 | 3.64 |
| adjusted $R^2$ | .88 | .88 | .88 | .88 | .88 | .88 | .88 | .88 |
| N | 1271 | 1271 | 1271 | 1271 | 1271 | 1271 | 1271 | 1271 |

The simplest version of our analysis, reported in Column 1 of Table 2, employs the absolute PHDI value for each state in each election year. The negative parameter estimate indicates that, on average, voters punished incumbent parties for droughts and wet spells; the *t*-statistic for this parameter estimate is −2.7, so the effect of wet and dry spells on election outcomes cannot easily "be dismissed by the dubious as a coincidence" (Barnhart 1925, 529). 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 seven-tenths of a percentage point, while "extreme" droughts or wet spells (absolute PHDI values of 24 or more) cost incumbents about 1.5 percentage points.

Column 2 of Table 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 variables.[28] Here, the estimated effect of election year drought is quite similar to the estimated effect in Column 1, and drought values in two of the three preceding years appear to have additional (albeit slightly smaller) negative effects. In Column 3 our drought variable is the sum of absolute PHDI values in all four years of each president's term, and it too has a significant negative effect. In Column 4 we employ a time-weighted cumulative drought index created by assuming that the separate parameter estimates reported in Column 2 approximate a series of geometrically declining weights attached to previous droughts and wet spells.[29] 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 −3.2) and of considerable magnitude (costing the incumbent party about 1.1 percentage points in a typical state and year).

The remaining four columns of Table 2 repeat the analyses reported in the first four columns, but with each drought variable multiplied by the proportion of the population living

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[28] Our drought data do not extend back to 1893 and 1894, but we include the 1896 election in this and subsequent analyses by simply assuming that the 1893 and 1894 drought values were the same as in 1895. The 1895 data are not unusual, and none of our results are significantly affected by excluding the 1896 election from our analyses.

[29] The weights attached to drought index values in the four years leading up to each election in this calculation are .079, .147, .271, and .503.

in rural areas in each state and year. The resulting rural drought indices allow for the possibility that wet and dry spells may be consequential only—or primarily—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 is quite similar. For example, the estimated effect of election year rural drought in Column 5 has a $t$-statistic of −2.9 (as compared with −2.7 in Column 1) and implies that the incumbent party lost .6 percent of the vote in a typical state and year (as compared with .7 percent in Column 1). The estimated effect of time-weighted cumulative rural drought in Column 8 has a $t$-statistic of −3.3 (as compared with −3.2 in Column 4) and implies that the incumbent party lost .8 percent of the vote in a typical state and year (as compared with 1.1 percent in Column 4).[30]

The strength and consistency of these results across a variety of regression analyses employing different versions of our drought index should leave little doubt that droughts and floods *in general* have a negative effect on electoral support for the president's party.[31] That negative effect is not coincidental; nor is it simply a matter of voters rationally punishing particular presidents for failing to prepare adequately for or respond adequately to particular disasters. It is a pervasive risk to the reelection chances of every incumbent party, and no more controllable than the rain.

The great advantage of the summary results presented in Table 2 is that they effectively average over the entire distribution of good and bad performances by incumbent governments in responding to the droughts and floods of the 20th century. The corresponding disadvantage

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[30] The weights attached to rural drought index values in the four years leading up to each election in the creation of the cumulative time-weighted rural drought variable are .040, .100, .247, and .613.

[31] In addition to the variety of regression analyses reported in Table 2 we have examined models with separate effects for droughts and wet spells, models with non-linear variants of our drought indices, models allowing for secular trends in 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 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. That variation is conveyed by Figure 3, which presents separate estimated effects of election year drought and cumulative time-weighted drought on the incumbent party's vote share in each election.

Figure 3: Election-Specific Estimates of Drought and Flood Effects, 1900-2000
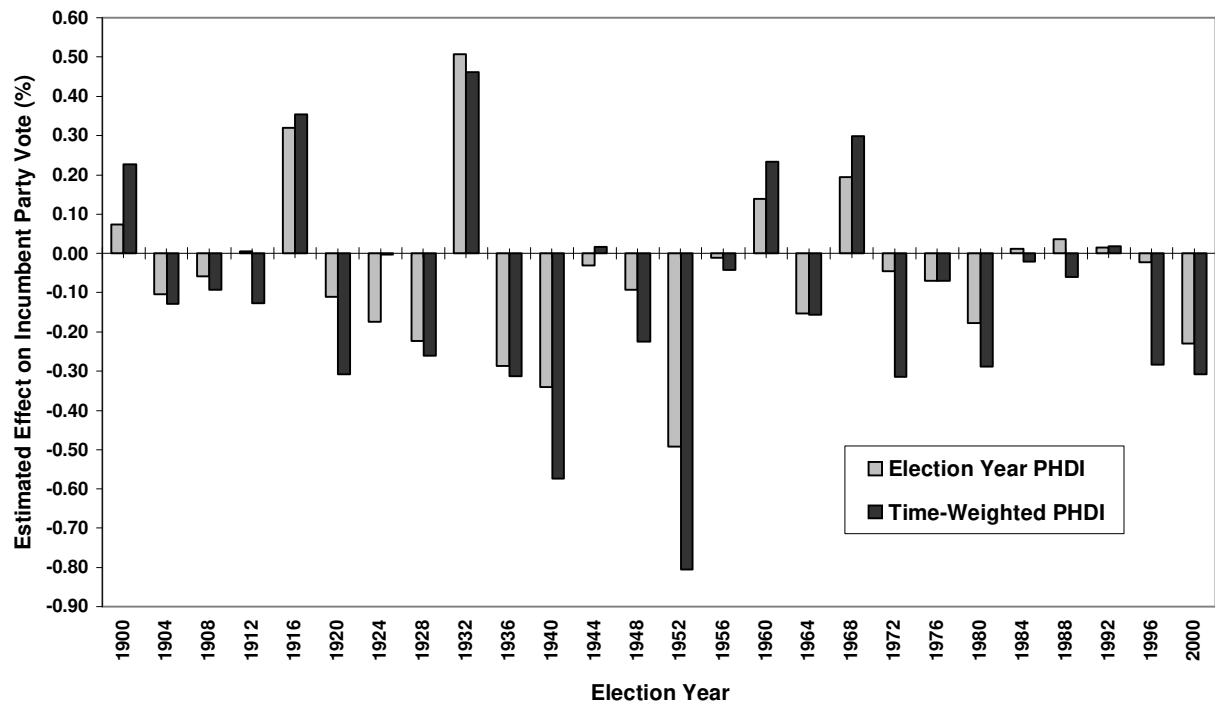


The estimated effects of droughts and floods are clearly quite variable, with almost half of the election-specific estimates more than twice as large—and a few as much as five times as large—as the corresponding overall estimates in Table 2. Of course, much of this apparent variability is simply due to sampling error; with 52 distinct parameter estimates, we should expect a few to be "significantly" positive and a few to be "significantly" negative purely by chance. However, 5 of the positive estimates and 11 of the negative estimates have *t*-statistics large enough to be "significant" at the .05 level, whereas only 2.6 of each could be expected

by chance. Thus, there seems to be at least some real information about the varying responses of electorates to climatic troubles in the election-specific estimates in Figure 3.[32]

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 a comprehensive 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 antiquarianism. We do so by providing a thoroughly modern examination of 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 ten percent higher than the historical average, with severe drought in parts of the South and West and excessive wetness in the Dakotas, New York and Vermont.[33] Table 3 presents our analysis of the impact of

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[32] The largest and most interesting anomaly in Figure 3 is the striking (and fairly precise) *positive* estimate of the effect of the drought index on President Hoover's vote share in 1932. Phillips (1969, 370) suggests that "By 1932, the Wheat Belt had become a Drought Belt. Roosevelt . . . scored his biggest gain over Smith in parched Kansas, which had overwhelmingly rejected Smith *and* LaFollette." However, that account is anachronistic: Kansas, Nebraska, and the Dakotas, all of which saw declines of 25 percentage points or more in Hoover's vote share in 1932—and all of which would indeed be "parched" in the next few years—all had *below* average drought scores in 1932, and only one instance of "severe" drought (in South Dakota in 1931) in the entire course of the Hoover administration. Three of the four states with the highest absolute PHDI values in 1932—Arizona, Utah, and Texas—were too *wet*, not too *dry*; in any case, Hoover's 1932 vote share in those states was only 9.2 percentage points below Coolidge's share in 1924 (a better comparison than 1928, given the electoral perturbations produced by Al Smith's candidacy in 1928), whereas the corresponding loss for the country as a whole was 14.3 percentage points. Nor does the apparent positive effect of drought in 1932 seem to be attributable to any coincidental correlation with the more general (and probably more salient) economic distress brought about by the Great Depression: adding measures of state income change from 1931 to 1932 or from 1929 to 1932 leaves the estimated drought effects strongly positive. We have no explanation to offer for this anomalous result.

[33] 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 20.3 to 15.8.

these climatic conditions on the 2000 presidential vote. We consider three different versions of our drought index, with previous Democratic votes, percent rural, and a dummy variable for southern states as control variables. All three versions of the regression model 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 suggest a great deal of continuity with previous partisan voting patterns, but with some significant defections from the Democratic ticket in rural states.

**Table 3. The Effect of Drought on the 2000 Presidential Vote**

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

|  | (1) | (2) | (3) |
|---|---|---|---|
| **Election Year Drought Index** | −.231 (.073) | --- | --- |
| **Rural Drought Index** | --- | −.546 (.259) | --- |
| **Time-Weighted Drought Index** | --- | --- | −.308 (.104) |
| **1996 Clinton Vote (%)** | .915 (.113) | .896 (.119) | .792 (.116) |
| **1992 Clinton Vote (%)** | .206 (.121) | .237 (.127) | .300 (.121) |
| **Rural (%)** | −.098 (.026) | −.032 (.040) | −.117 (.027) |
| **South** | −.60 (.76) | −.96 (.79) | −1.66 (.68) |
| **Intercept** | −.14 (3.33) | −3.14 (3.40) | 3.56 (3.91) |
| std err of reg | 1.94 | 2.06 | 1.97 |
| adjusted $R^2$ | .92 | .91 | .92 |
| N | 48 | 48 | 48 |

The three versions of our drought index all have strong negative effects on the Democratic vote in the regression analyses, with *t*-statistics ranging from −2.1 to −3.2. The magnitudes of these 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." Voters responded to climatic distress in 2000—as they have repeatedly throughout the past century—by punishing the incumbent government at the polls.

**A Negative Case: The Influenza Pandemic of 1918**

The Spanish Influenza pandemic of 1918 was one of the most important events of the 20th century. Most estimates of the worldwide death toll range from 20 million to 40 million, with some as high as 100 million. The flu killed approximately half a million people in the U.S. alone—about one-half of one percent of the nation's population, 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). The peak of the pandemic occurred in the last four months of 1918. During that period, more than 300,000 deaths were recorded in the 24 "registration states" for which detailed data are available—about 11 times the usual number of deaths from influenza and pneumonia. An additional 120,000 deaths were recorded in the same states in the first six months of 1919 (Crosby 1989).

If voters simply punished the incumbent government whenever they felt significant pain, the millions of people who lost friends or family members to influenza would have

produced the greatest anti-incumbent landslide in American electoral history. But our claim is that the connection between pain and electoral punishment 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.

The voters might have chosen quite reasonably to punish the government. Troop trains and camps were crowded because of the war; only well after the epidemic began was unnecessary crowding eased. Large war-bond rallies were held during this period with government encouragement; the close proximity of thousands of citizens undoubtedly helped the flu spread. And the disorganized and underfunded condition of the United States Public Health Service and its state-level counterparts was quickly revealed once the epidemic began (Crosby 1989, chs. 1, 2, esp. pp. 49-51.)

Curiously, given the vast magnitude of the disaster, the pandemic received remarkably little public attention. As one historian put it, "When you talk to people who lived through it, they think it was just their block or just their neighborhood" (Crosby, quoted by Kolata 1999, 8). Another wrote that the flu's "enormous, almost unthinkable impact somehow had escaped attention" (Kolata 1999, 8). Most importantly, no thread of elite rhetoric or popular discourse seems to have suggested any attribution of responsibility to President Wilson or other public officials.

As a result, as best we can tell, the electorate utterly failed to respond to the influenza pandemic. True, the Republican opposition narrowly regained control of the Senate in 1918 and was swept back into the White House in 1920. However, both of those events can reasonably be interpreted as a restoration of the electoral alignment prevailing before the spectacular fracturing of the long-standing Republican majority in 1912, rather than as punishment of the party that had presided (at the national level) over the greatest public health catastrophe in U.S. history.

Crosby (1989, 174) noted that the midterm election of 1918 was "the only truly momentous American election ever to take place in the midst of a major pandemic, and it is tempting to suggest that the pandemic must have played a role in deciding such a close election." However, he adduced remarkably little evidence of electoral impact, aside from canceled meetings, parades, and speaking tours and, perhaps, a general decline in turnout. The one important race that seems to have been directly affected by the pandemic was the Senate contest in New Mexico, in which Republican Albert B. Fall prevailed by fewer than 2,000 votes. Fall had been "singled out" for "personal attack" by President Wilson at the same time he was grieving over the deaths from influenza of two of his children. Crosby (1989, 175) concluded that, "To the extent that sympathy for the bereaved Fall caused Wilson's attack to backfire (and it most certainly did), Spanish influenza was responsible for his election and helped win the Senate for the Republicans."

Our own more systematic analysis focuses on whether the states and cities that were hardest hit by the influenza pandemic responded distinctively at the polls. 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. We examine the voting patterns in midterm gubernatorial elections, looking for evidence of electoral retribution aimed either at Democratic gubernatorial candidates or at incumbent governors regardless of party.[34] While in principle voters could have blamed either the national incumbent party or local incumbents for their distress, our claim is that in fact they blamed no one, instead viewing the pandemic as simply an act of God.

Our analyses of the 1918 midterm vote are presented in Table 4. In the top panel of the table we examine voting patterns in 16 "registration states" with gubernatorial elections in 1918.[35] In the bottom panel of the table we examine voting patterns in 29 major cities within

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[34] As it happens, 13 of the 16 states included in our analysis of gubernatorial election results 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 president.

[35] "Registration states" were those for which the federal government gathered detailed data on the spread of the pandemic, including mortality rates. The registration states with gubernatorial

**Table 4 . The Effect of Influenza on the 1918 Gubernatorial Vote**

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

| | Incumbent Vote | Democratic Vote |
|---|---|---|
| **Registration States with Gubernatorial Elections in 1918** | | |
| **Excess Flu Deaths (per 1000)** | 1.40 (1.34) | −1.29 (.92) |
| **Previous Vote (%)** | .687 (.509) | 1.217 (.242) |
| **Democratic Incumbent** | −.93 (5.00) | −.86 (3.66) |
| **Intercept** | 10.63 (29.92) | −4.08 (9.22) |
| std err of reg | 6.94 | 4.66 |
| adjusted $R^2$ | .02 | .70 |
| N | 16 | 16 |
| **Major Cities in Registration States with Gubernatorial Elections** | | |
| **Excess Flu Deaths (per 1000)** | 2.26 (1.05) | 1.09 (1.12) |
| **Previous Vote (%)** | .927 (.180) | .892 (.147) |
| **Democratic Incumbent** | 9.74 (3.79) | −.33 (3.95) |
| **Intercept** | −11.41 (9.68) | 5.19 (9.71) |
| std err of reg | 8.00 | 7.47 |
| adjusted $R^2$ | .60 | .66 |
| N | 29 | 29 |

elections in 1918 are California, Colorado, Connecticut, Kansas, Kentucky, Maine, Maryland, Massachusetts, Michigan, Minnesota, New Hampshire, New York, Ohio, Pennsylvania, Rhode Island, Vermont, and Wisconsin. Statewide election results are from Rusk (2001). We omit California from the analysis in Table 4 because the incumbent governor was a Progressive, no Democratic candidate appeared on the ballot in 1918, and the Republican party nearly doubled its vote (from 29 percent in 1914 to 56 percent in 1918). Including California would have only a modest effect on the results of our analysis, reducing the apparent positive impact of flu deaths on incumbent votes by about 6 percent and increasing the apparent negative impact of flu deaths on Democratic votes by about 16 percent.

those states.[36] For these analyses we count excess deaths from influenza or pneumonia (per 1000 population) in the last four months of 1918 only.[37] In each case, we include the incumbent or Democratic vote in the preceding gubernatorial election plus a dummy variable for Democratic incumbents as control variables.

Not surprisingly, given the limitations of the available data, all of the results presented in Table 4 are fairly imprecise and, in some cases, quite sensitive to changes in the sample or variable definitions. On the whole, however, these results support our assertion that the flu pandemic had little or no political effect. Only the estimate for Democratic votes at the state level has the "correct" (negative) sign, and even that estimate is of very modest magnitude, suggesting that the worst-hit state (Pennsylvania, with 6.7 excess deaths per 1000) would be about 5 percentage points less Democratic than the state with the lowest death rate (Michigan, with 2.6 excess deaths per 1000).[38] The largest and most "significant" estimate in

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[36] The cities for which we have data on flu deaths from Crosby (1989) are (CA) Los Angeles, Oakland, San Francisco, (CO) Denver, (CT) Bridgeport, New Haven, (MA) Boston, Cambridge/Lowell, Fall River, Worcester, (MI) Detroit, Grand Rapids, (MN) Minneapolis, St. Paul, (NE) Omaha, (NY) Albany, Buffalo, New York, Rochester, Syracuse, (OH) Cincinnati, Cleveland, Columbus, Dayton, Toledo, (OR) Portland, (PA) Philadelphia, Pittsburgh, Scranton, (RI) Providence, (TN) Memphis, and Nashville. Election results are from the counties containing those cities (from *County Level Voting Data: United States Historical Election Returns*, available from the Inter-University Consortium for Political and Social Research). We exclude the California data from our analysis for the reasons indicated in the preceding note; including them would increase the apparent positive impact of flu deaths on incumbent votes by about 28 percent and would more than double the apparent positive impact of flu deaths on Democratic votes.

[37] Pyle (1986, 46-47) reported more detailed data for a subset of the major cities included in our analysis in the bottom panel of Table 4. Thus, we were able to repeat our analysis using deaths through 2 November 1918 (that is, before Election Day) for 24 of the 29 counties analyzed in Table 4. The estimated effects of excess flu deaths through Election Day were 1.33 (with a standard error of 1.82) for incumbent votes and 3.70 (1.33) for Democratic votes. (The corresponding estimates using the less detailed flu data employed in Table 4 for the same 24 counties are 3.04 and 2.83, respectively.) All of these effects are positive, suggesting that gubernatorial incumbents and Democrats both did *better* in areas with higher death rates. In no case is there any evidence in the county-level data that the electorate punished anyone for the flu pandemic.

[38] The Democratic vote share in Pennsylvania fell from 41 percent in 1914 to 34 percent in 1918—a few points further than suggested by our regression model. But in Philadelphia, which suffered more than 8500 flu deaths in the four weeks preceding the election, the Democratic vote share *doubled* from 13 percent in 1914 to 26 percent in 1918.

the table suggests that incumbent governors actually *gained* votes in the major cities with the highest death rates.[39]

In our view, the striking absence of an electoral response to the flu pandemic dramatically illustrates the importance of voters' cultural understandings of causation and responsibility. In the language of Stone's (1989) typology of causal frameworks, voters thought of the pandemic as part of the natural world rather than as part of the social world: "The natural world is the realm of fate and accident . . .. The social world is the realm of control and intent" (Stone 1989, 283). 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 force in the political process. And, as far as we can tell, no one *did* supply such an argument. 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.

Was it rational or irrational for voters to treat the flu pandemic as a natural disaster rather than a social problem? It is no more possible to answer that question than it is to specify confidently when and how modern presidents influence the course of the economy. Attributions of political responsibility are inevitably, in part, a matter of guesswork and social construction. Subsequent governments certainly believed that the political cost of a flu epidemic might be considerable, as witnessed by the Ford administration's aggressive—in retrospect, overly aggressive—response to the Swine Flu scare in the 1970s (Neustadt and Fineberg 1983).

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[39] We also examined the impact of the flu pandemic on the 1920 presidential vote using a state-level analysis similar in form to the analyses of drought effects presented in Tables 3 and 4. Perhaps not surprisingly, given that the election occurred nearly two years after the height of the pandemic, variation in the severity of the flu from state to state appears to have had little or no effect on the 1920 election outcome. The parameter estimate for flu deaths is quite small, and suggests that, if anything, Democratic nominee James Cox probably did slightly *better* in states that had higher death rates, other things being equal. Clearly, there is no evidence here, either, that voters held President Wilson's party responsible for the human devastation wrought by the pandemic.

Even with due allowance for the less-developed public health technology of 1918, it seems likely that tens of thousands of flu victims could have been saved by more effective government action, perhaps most obviously by prohibiting public gatherings in areas where the pandemic was raging. Some efforts along those lines were made, but they seem to have been surprisingly half-hearted. Some public health departments issued gauze masks to stem the contagion; in Philadelphia, where thousands were dying, "health officials began a public campaign against coughing, spitting, and sneezing"; even government efforts to track the spread of the epidemic seem to have been slow and disorganized (Kolata 1999, 22-23, 19, 10).

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. Somehow, that never happened. Hundreds of thousands died, but no one thought to blame the pharaoh.

**Conclusion**

Our account of democratic politics strikes directly at key assumptions in two different contemporary schools of thought. Perhaps most obviously, it questions the ability of ordinary citizens to assess their public life critically, listen to the proposals for change coming from contenders for public office, and then choose between the candidates in accordance with their own values. Like most survey researchers who have talked extensively to real voters, we believe that few such citizens exist. The present paper is one more item of evidence. The central fact about democracies is that the voters understand little beyond their own and their community's pain and pleasure, and they think about causes and effects as the popular culture advises them to think. The romantic vision of thoughtful democratic participation in the

common life is largely mythical. Democracy must be defended some other way, if it is to be defended at all.

Our work also strikes a blow at the customary fallback position for contemporary defenders of democracy, namely the view that the voters may know very little, but they can recognize good and bad government performances when they see them. Hence they can choose retrospectively in a defensible way. In most recent scholarly accounts, retrospection is a natural and rational feature of democratic politics. In our view it is natural but not so obviously rational. 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, more or less primitive understanding of the connections (if any) between incumbent politicians' actions and their own pain or pleasure. As a result, rational retrospective voting is harder than it seems, and blind retrospection sometimes produces consistently misguided patterns of electoral rewards and punishments.

What we have *not* offered here is any systematic account of the circumstances under which citizens will find—and accept—a cultural understanding that holds public officials responsible for changes in the public's welfare. We know that the framing of news by the mass media may increase or decrease the likelihood that citizens will attribute responsibility for social problems to the government (Iyengar 1991). We know that politicians themselves may be more or less successful in "managing blame," exploiting competing explanations to exonerate themselves (McGraw 1991).[40] These alternate explanations are always present: some medieval towns blamed the plague on 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

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[40] Ideological commitments may play a significant independent role in elite constructions of explanations for natural or social disasters, as with the Federalists' and Republicans' competing explanations of the Yellow Fever epidemic of 1793 (Pernick 1972). 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 (Pernick 1972, 562-563). In fact, Pernick notes, "both sides were right."

Spanish Influenza pandemic two years later feared that "plague germs were inserted into aspirin made by the German drug company Bayer" (Kolata 1999, 3).

When is one explanation accepted rather than another? Much seems to depend on plausibility within the folk culture. Unfortunately, a general theory of political accountability explaining when and why specific attributions or evasions of responsibility actually work is nowhere in sight. The development of such a theory strikes us as a very high priority for students of democratic politics.

We end, then, on a discouraging note. For those who take the evidence about voter capacities seriously, neither Rousseau nor Downs will save us. Democracies take their electoral direction from human beings with fewer capacities for self-government than either writer imagined. Under sufficient pressure, those voters may lash out blindly. Such events are not bizarre historical footnotes rendered irrelevant by modern education and hygiene. They are inevitable consequences of human cognitive limitations—limitations which democratic government has not altered. Thus, as Sophocles taught and as the destruction of the Weimar Republic reminds us, when the inevitable hard times appear, tragedy may ensue.

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