Do Shark Attacks Influence Presidential Elections?
Reassessing a Prominent Finding on Voter Competence

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Abstract

We reassess Achen and Bartels' (2002, 2016) prominent claim that shark attacks influence presidential elections. First, we assemble data on every fatal shark attack in U.S. history and county-level returns from every presidential election between 1872 and 2012, and we find no systematic evidence that shark attacks affect elections. Second, we show that Achen and Bartels' county-level finding for New Jersey in 1916 becomes substantively smaller and statistically weaker under alternative specifications. Third, we find that their town-level finding in Ocean County significantly shrinks when we correct errors and does not hold for the other beach counties. Lastly, implementing placebo tests in state-elections where there were no shark attacks, we demonstrate that Achen and Bartels' result was likely to arise even if shark attacks do not influence elections. Overall, there is little compelling evidence that shark attacks influence presidential elections, and any such effect—if one exists—is substantively negligible.

Achen and Bartels (2002, 2016) report that shark attacks in New Jersey affected voter behavior in the 1916 U.S. presidential election. This finding has been cited over 400 times in a range of academic fields including political science, psychology, economics, public policy, climate science, law, and philosophy, and this analysis is a centerpiece of Achen and Bartels' recently published and widely anticipated book, *Democracy for Realists: Why Elections Do Not Produce Responsive Government.* The claim has also entered the popular media; for example, in 2012, Reuters ran an article that mused that "Obama can take heart that Florida beachgoers haven't suffered from a spate of shark attacks this year, which could have hurt his prospects there" (Sullivan 2012). And this finding has spawned an entire literature of studies testing for the electoral effects of arguably irrelevant events (see Healy and Malhotra 2013 for a review).

For the past half century, scholars of elections and political behavior have sought to assess the competence, knowledge, and rationality of the typical American voter. On one hand, voters appear politically ignorant in surveys (e.g., Campbell et al. 1960), but on the other hand, studies suggest that, in the aggregate, voters respond reasonably to government performance (Kramer 1971; Lenz 2012), candidate ideology (Hall 2015; Hirano et al. 2014), and candidate quality (Alt, Bueno de Mesquita, and Rose 2011; Fowler 2016; Hirano and Snyder 2009). Of course, when voters respond to performance, they might do so in myopic or irrational ways (Bartels 2008; Huber, Hill, and Lenz 2012). When they respond to ideology, they might do so in a tribal way (Green, Palmquist, and Schickler 2002). And when they respond to quality, they might focus on superficial dimensions of quality (Todorov et al. 2005; but see also Atkinson, Enos, and Hill 2009). Clearly, whether or how voters are competent, and what this means for democratic accountability, are far from settled matters. Some of the most influential studies in this literature are those showing that election results appear to be influenced by politically irrelevant factors. For example, Healy, Malhotra, and Mo (2010) report that college football games influence elections, although Fowler and Montagnes (2015)

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show that this result is most likely a false positive. Achen and Bartels' finding on shark attacks lies at the forefront of this lively debate.

The impact of the shark-attack finding likely stems from what it could mean for democracy. Achen and Bartels argue that their finding shows that voters are irrational, or at least imprudent, and that our conventional view of democratic accountability is severely undermined. They write, "[B]lind 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" (2016, p. 142). Echoing this sentiment, Somin (2013) cites the shark-attack finding as evidence of public ignorance and uses it to argue that government should be decentralized and limited. On the other side of this debate, some scholars point out that, although Wilson was not responsible for the shark attacks, rational voters might nonetheless have punished him because natural disasters reveal information about politicians' competence in preparing for and responding to unexpected events (Ashworth, Bueno de Mesquita, and Friedenberg 2015). Furthermore, rationality and knowledge are not necessarily good for voter welfare (Ashworth and Bueno de Mesquita 2014), so even if we accept that the shark-attack result reflects irrationality, what this means for democracy is normatively ambiguous.

Regardless of one's views on what the shark-attack result means for democracy, this debate is irrelevant if the empirical claim is invalid. Therefore, in this paper, we reassess the evidence that shark attacks influence presidential elections. There is virtually no compelling evidence that shark attacks influence elections, and any such effect—if it exists—must be substantively tiny. Whether or not voters are informed or competent in general, the evidence that shark attacks affect voter decisions is not nearly strong enough to generate dismay over the democratic process.

Our investigation proceeds in four parts. First, we assemble data on every recorded fatal shark attack in U.S. history along with county-level returns from every presidential election between

1872 and 2012 to provide the most comprehensive possible tests of the effects of shark attacks on presidential elections. The resulting estimates are substantively small and statistically null, suggesting that any potential effect of shark attacks on presidential elections is likely modest. Second, we assess the robustness of Achen and Bartels' county-level result from New Jersey. The original analysis makes several important specification choices, and we demonstrate that these choices are consequential for the substantive size and statistical strength of the estimated effect. Second, we show that Achen and Bartels' town-level result from Ocean County attenuates markedly once issues related to town boundaries are addressed, and does not hold for the other beach counties of New Jersey. Lastly, consistent with the simulations of Bertrand, Duflo, and Mullainathan (2004), we demonstrate that the standard errors resulting from Achen and Bartels' empirical strategy considerably overstate the level of statistical confidence in the estimated effect. Across nearly 600 placebo tests in state-elections where there were no shark attacks, we obtain a point estimate as large as the New Jersey estimate in 32 percent of the cases, and we obtain a statistically significant result in 27 percent of the cases. Because coastal counties are subject to different idiosyncratic political shocks than inland counties, Achen and Bartels' result could easily arise even if shark attacks have no electoral effect. In short, there is little compelling evidence that shark attacks influence presidential elections generally or that they did so in New Jersey in 1916.

Data

In our subsequent analyses, we utilize data on presidential election results, fatal shark attacks, and official government classifications of coastal and non-coastal counties. County-level presidential election returns come from Clubb, Flanigan, and Zingale (1986) for 1872-1972, and from uselectionatlas.org for 1976-2012. Town-level presidential election results in New Jersey in 1912 and 1916 come from the Manuals of the State Legislature of New Jersey (Fitzgerald 1913; 1917).

Information about every fatal shark attack in U.S. history comes from The Shark Research Institute, a multi-disciplinary nonprofit scientific research organization that, to our knowledge, maintains the most comprehensive database available of shark attacks.[2] Finally, official government classifications of coastal counties come from the National Oceanic and Atmospheric Administration (NOAA).[3] Coastal counties are defined by NOAA to be any county having a coastline bordering the open oceans or Great Lakes, or any county containing coastal high hazard areas. Because of our focus on shark attacks, we remove from NOAA's list any county that does not border the open ocean, which means that we do not classify counties on the Great Lakes or that contain coastal high hazard areas but are not shore-adjacent as coastal counties.

Fatal Shark Attacks and Presidential Elections, 1872-2012

Achen and Bartels' famous study focuses on the case of shark attacks in New Jersey in 1916. The authors offer a number of reasons for focusing on this single case, including: that there was unusual cluster of four attacks, which made them more salient; that the attacks occurred after the President had vacationed in the area the previous summer; and that the attacks affected the local economy. But if salient yet irrelevant events generally influence presidential elections as Achen and Bartels hypothesize, there is little reason to focus on this particular case. Either the New Jersey finding, if valid, exposes a systemic flaw in voters' reasoning—in which case we should find similar behavior in other instances—or it is a one-off case relying on an incredibly unusual constellation of circumstances, and therefore has little to say about democratic accountability and voter competence in general. Therefore, we begin our analysis by attempting to conduct the most comprehensive possible test of the conjecture that shark attacks influence presidential elections. We utilize data on

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[2] See sharkattackfile.net and sharks.org for more information.
[3] https://coast.noaa.gov/digitalcoast/training/enow-counties-list.html

every presidential election from 1872-2012 and every fatal shark attack in the U.S. during this period—97 in total.

Specifically, we implement differences-in-differences regressions of the following form:

(1) $Democratic\ Vote\ Share_{ist} = a*Attack_{ist} + \beta*Attack_{ist}*IncParty_t + \gamma_i + \delta_{st} + \varepsilon_{ist}.$

Democratic Vote Share$_{ist}$ is the two-party vote share of the Democratic candidate in county *i*, in state *s*, and in election *t*; γ_i represents county fixed effects; and δ_{st} represents state-election fixed effects. When county boundaries change, we create a new county identifier. We also create a new county identifier every 5 presidential election cycles to allow for changes in the normal partisanship of a county over time, so our county fixed effects are actually county-period fixed effects, where 5 presidential election cycles are nested into each 20 year period. *Attack*$_{ist}$ is an indicator for a fatal shark attack in county *i* in state *s* and in the four-year election cycle leading up to presidential election *t*. *IncParty*$_t$ takes a value of 1 if a Democratic president held office during the four-year period leading up to election *t*, and it takes a value of −1 if a Republican president held office. This variable is interacted with *Attack*$_{ist}$, but it cannot be included on its own because its main effect is subsumed by the state-year fixed effects. We exclude from our analysis states that never had a fatal shark attack because they contribute no useful information for testing our hypothesis.

The coefficient α tells us whether shark attacks systematically benefit Democratic vs. Republican presidential candidates. Since we have no compelling theoretical reason to expect a systematic partisan effect, this coefficient is of little interest. However, the interactive coefficient β represents the effect of shark attacks on the incumbent party's vote share; this is our primary quantity of interest.[4] Column 1 of Table 1 shows the results of this regression. The interactive

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[4] We could have alternatively coded the dependent variable as the incumbent party's vote share, and this would have allowed us to avoid using interactive terms. However, our specification provides more statistical power because the county fixed effects control for the normal partisan voting behavior of each county. We have little reason to suspect that some counties are more pro-

Table 1. Effect of Shark Fatalities in the County of the Attack, 1872-2012

	DV = Two-party Democratic Vote Share				
	(1)	(2)	(3)	(4)	(5)
Attack	.009				
	(.008)				
Attack*IncParty	**−.005**				
	(.007)				
Attacks (count)		.007			
		(.006)			
Attacks (count)*IncParty		**−.006**			
		(.005)			
Election-Year Attack			−.004		
			(.007)		
Election-Year Attack*IncParty			**.002**		
			(.007)		
Attack*Reelection				.015	
				(.010)	
Attack*Reelection*IncParty				**.001**	
				(.010)	
Election-Year Attack*Reelection					.004
					(.008)
Election-Year Attack*Reelection*IncParty					**.009**
					(.008)
State-Year Fixed Effects	X	X	X	X	X
County-Period Fixed Effects	X	X	X	X	X
Observations	39,449	39,449	39,449	39,449	39,449
R-squared	.922	.922	.922	.922	.922

*County-clustered standard errors in parentheses; ** p<.01, * p<.05.*

coefficient of −.005 suggests that a fatal shark attack in a county hurts the incumbent party's vote share in the next presidential election by 0.5 percentage points, and this estimate is not statistically distinguishable from zero. As a benchmark, Achen and Bartels report that shark attacks decreased Wilson's vote share in affected counties by more than 3 percentage points, so this estimate is less than one-sixth the substantive size of their estimate.

We might think that the number of fatal shark attacks in each county-election matters. What if voters especially punish the incumbent party when there are multiple attacks? Of course, fatal

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incumbent than others, but there are significant differences in partisanship across counties, and this specification allows us to account for that.

shark attacks are rare, but in our data, there are 7 cases of 2 fatal attacks within the same county and election cycle and 2 cases of 3 attacks. In column 2, we replace the indicator for an attack with a count variable indicating the number of fatal attacks, and the resulting estimate is not notably different from that in Column 1 where we simply use a binary indicator.

The regressions in Columns 1 and 2 consider all attacks that occurred at any point throughout the presidential election cycle. If voters are myopic and overweigh recent events (Bartels 2008), we might hypothesize that shark attacks should only matter if they occur in the election year. To test this hypothesis in Column 3, we implement the same specification from Equation 1, replacing the indicator for any attack with an indicator for an election-year attack. Here, we estimate that an election-year attack actually *helps* the incumbent party by a substantively tiny 0.2 percentage points, and again, this estimate is not statistically distinguishable from zero.

We might also hypothesize that shark attacks only matter when an incumbent seeks reelection. Perhaps voters penalize the incumbent president, but when he retires, they don't carry over the blame to the new candidate from the same party. Therefore, in Column 4, we replace the attack variable with an indicator for an attack in an election cycle where the president is seeking reelection. With this specification, we again obtain an essentially null result. The coefficient of .001 is in the opposite direction hypothesized by Achen and Bartels and statistically insignificant.

Combining the logic from Columns 3 and 4, we might hypothesize that shark attacks only matter when they occur in an election year *and* when the incumbent seeks reelection. Therefore, in Column 5, we utilize an indicator for an attack in an election year when an incumbent seeks reelection. In this specification, the estimated coefficient is .009, again opposite the hypothesized direction but still statistically insignificant.

The analyses in Table 1 presume that the effects of shark attacks should be concentrated in the county of the attack. However, one might also hypothesize that the effects of shark attacks

Table 2. Effect of Shark Fatalities in All Coastal Counties within the State, 1872-2012

	DV = Two-party Democratic Vote Share				
	(1)	(2)	(3)	(4)	(5)
Attack	.002				
	(.005)				
Attack*IncParty	**−.007***				
	(.003)				
Attacks (count)		.003			
		(.002)			
Attacks (count)*IncParty		**−.002**			
		(.001)			
Election-Year Attack			.011		
			(.007)		
Election-Year Attack*IncParty			**−.009**		
			(.006)		
Attack*Reelection				.004	
				(.005)	
Attack*Reelection*IncParty				**−.004**	
				(.003)	
Election-Year Attack*Reelection					.016
					(.009)
Election-Year Attack*Reelection*IncParty					**.003**
					(.012)
State-Year Fixed Effects	X	X	X	X	X
County-Period Fixed Effects	X	X	X	X	X
Observations	39,449	39,449	39,449	39,449	39,449
R-squared	.922	.922	.922	.922	.922

*Standard errors, clustered by state-coastal regions, in parentheses; ** p<.01, * p<.05.*

spread to all coastal counties in the state, perhaps because shark attacks have the greatest psychological and economic effects for people who live near the water. Therefore, in Table 2, we closely replicate all of the analyses from Table 1, but we code all coastal counties in the state as affected if there is a fatal shark attack anywhere in the state. For instance, in Column 1 of Table 2, the attack variable takes a value of 1 if there was an attack in the state *and* the county is coastal, and it takes a value of 0 if a county is non-coastal or if there was no attack in the state. In Column 2, the attack variable is a count of the number of fatal attacks in the state for coastal counties and zero otherwise, and so on.

Across the ten regressions in Tables 1 and 2, six of the point estimates of interest are negative, four are positive, and none have an absolute magnitude greater than one percentage point. Only one of the ten estimates is statistically significant, and one in twenty is expected to be significant under the null, so this could simply be the result of chance.[5] In every case, we can statistically reject any purported effect of shark attacks below −2.2 percentage points, and in some cases, we can statistically reject effects beyond −1 percentage point. In summary, we find little support for the hypothesis that shark attacks decrease support for incumbent presidents or their parties, and any such effect, if it exists, appears to be substantively small.

Reassessing the County-Level Result from New Jersey in 1916

The preceding analyses suggest that shark attacks do not affect presidential elections in general. But perhaps there was something special about New Jersey in 1916, the case investigated by Achen and Bartels. Perhaps we'd expect particularly large effects in this case because the attacks occurred in an election year when an incumbent president sought reelection. However, there have been 11 such cases, and the regressions in Column 5 of Tables 1 and 2 show that there is no systematic evidence of an electoral effect even in these cases. We now investigate the New Jersey case more closely in order to reconcile the large results reported by Achen and Bartels with our null results above.

In the summer of 1916, there were four fatal shark attacks in or off the coast of Monmouth and Ocean counties, New Jersey. In November of that year, Woodrow Wilson sought reelection as president. Achen and Bartels report that these shark attacks caused the incumbent president to lose votes in the most affected counties. The study's primary evidence comes from a county-level

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[5] If these tests were statistically independent and if there were no electoral effects of shark attacks, we would expect at least one statistically significant estimate 40 percent of the time $(1 - .95^{10} \approx .401)$.

regression, which we replicate in Column 1 of Table 3. Specifically, Achen and Bartels regress Wilson's 1916 two-party vote share in each county on an indicator for "beach county," controls for Wilson's 1912 three-way vote share,[6] and an indicator for "machine county." The beach counties are Atlantic, Cape May, Monmouth, and Ocean—those that Achen and Bartels argue should have been affected by the attacks. The machine counties are Bergen, Essex, Hudson, and Union—those that Achen and Bartels argue were different enough from the rest of the state that they deserve their own control variable. Finally, Achen and Bartels exclude Essex County from the regression entirely. Using our electoral data, we replicate Achen and Bartels' result almost exactly. They estimate that Wilson lost 3.2 percentage points of vote share in the beach counties as a result of the shark attacks, and this estimate is statistically significant ($p < .01$).

This specification involves several choices. Even if each choice is defensible, we might worry about how the final estimate depends on the full set of choices—or, on what Gelman and Loken (2013) call the "garden of forking paths." Because flexibility in data analysis can lead to false-positive results (Simmons, Nelson, and Simonsohn 2011), we explore how Achen and Bartels' results would have differed under other defensible specification choices. First, Achen and Bartels exclude Essex County from their regression, writing "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." How they decided that Essex was an anomaly while the other 20 counties were worth inclusion is unclear. Therefore, in Column 2 of Table 3, we replicate the original specification while also including Essex. The point estimate doesn't change much, although the estimated standard error and corresponding p-value become larger.

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[6] In 1912, there were three major candidates. Theodore Roosevelt ran under the Progressive Party and finished in second place over Taft, the Republican incumbent. Because of this unusual circumstance, Wilson's three-way vote share from 1912 is a better predictor of his two-party vote share in 1916 than his two-party vote share in 1912. Nonetheless, we might worry that these unusual circumstances make 1916 a difficult election in which to study the effects of shark attacks.

Table 3. Robustness of New Jersey County Result across Specifications

	DV = Wilson's two-party vote, 1916					
	(1)	(2)	(3)	(4)	(5)	(6)
Beach County	−.032**	−.034*			−.023	−.021
	(.010)	(.013)			(.015)	(.017)
Attack County			−.028			
			(.015)			
Coastal County				−.021		
				(.010)		
Machine County (A&B)	−.056**	−.038**	−.053**	−.039*		
	(.011)	(.013)	(.013)	(.013)		
Machine County (Mayhew)						.004
						(.015)
Wilson three-way vote, 1912	.945**	.866**	.980**	.933**	.919**	.923**
	(.062)	(.076)	(.071)	(.073)	(.097)	(.101)
Constant	.046	.081*	.026	.055	.047	.044
	(.028)	(.034)	(.032)	(.034)	(.044)	(.047)
Include Essex County		X				
Observations	20	21	20	20	20	20
R-squared	.946	.904	.926	.929	.856	.857

*Uncorrected standard errors in parentheses; ** p<.01, * p<.05*

Next, Achen and Bartels hypothesize that the effects of the shark attack should have appeared only in Atlantic, Cape May, Monmouth, and Ocean counties. This choice is defensible because these four counties contain the beach communities of the Jersey Shore. But another principled approach would be to focus on the two counties—Monmouth and Ocean—that actually experienced attacks. Therefore, in Column 3 of Table 3, we replicate the regression in Column 1 but replace the *beach county* variable with *attack county*, an indicator for the two counties directly affected. When we make this simple change, the estimated effect of shark attacks shrinks to −2.8 percentage points and is no longer statistically significant.

If we think the effects of shark attacks spread to other coastal areas of New Jersey, perhaps because shark attacks have the greatest psychological and economic effects in places where people live near the water, another reasonable classification of affected counties would include *all* coastal counties in New Jersey. In Column 4, we replicate the analyses from the first two columns but using

a *coastal county* indicator for all counties on the coastline. Again, the estimated effect of shark attacks in this specification is substantively smaller than in the original specification, −2.1 percentage points, and not statistically significant.

Another choice by Achen and Bartels is their decision to control for machine counties. They define machine counties as those "with at least 30,000 voters in 1916 and 60 percent or more 'foreign' citizens in the Census of 1910" (2016, p. 122). Even if we agree that populous counties with large immigrant populations were different than the rest of the state and may have differentially shifted their support for Wilson between 1912 and 1916, we might wonder how they settled on these exact numbers and the precise set of four counties included in this category and we should know if these choices are consequential for their result. In Column 5 of Table 3, we replicate the original regression but excluding the control for machine counties. Again, the point estimate shrinks—to −2.3 percentage points—and is no longer statistically significant.

If we think accounting for the effects of political machines in this regression is important, perhaps because Democratic machines did not support Wilson in 1916 as much as they did in 1912, Bergen, Essex, Hudson, and Union are not necessarily the most obvious counties to categorize as having strong political machines. In a systematic review of partisan political machines across the U.S., Mayhew (1986, pp. 48-49) identifies Camden, Essex, Hudson, Mercer, Middlesex, and Passaic counties as those with strong Democratic Party machines. To be clear, Mayhew's codings reflect New Jersey politics in the 1960s, not the 1910s. However, the county machines he identifies are likely to have been long-lived, including those in key New Jersey cities, namely, Newark (Essex County), Jersey City (Hudson County), Camden (Camden County), and Trenton (Mercer County). Therefore, in Column 6 of Table 3, we replicate Achen and Bartels' specification but using Mayhew's coding of Democratic machine counties instead. Again, the point estimate is substantively smaller—−2.1 percentage points—and statistically insignificant.

Figure 1. County-Level Estimates across All Plausible Specifications
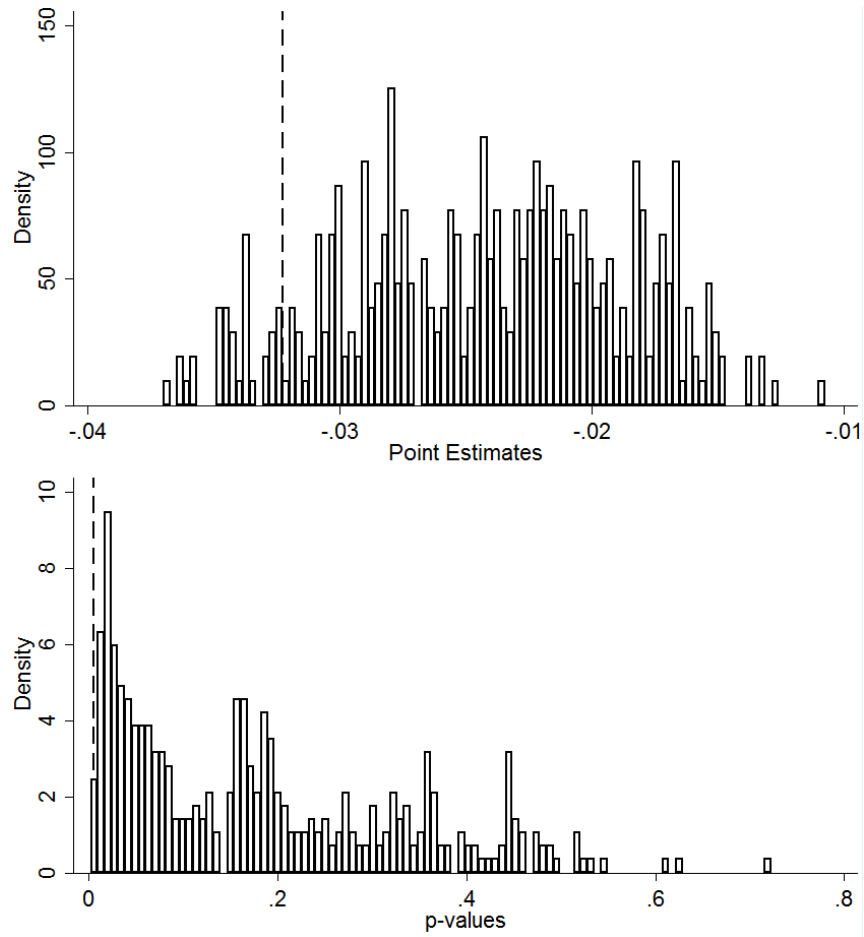
The figure shows the point estimates and p-values arising from 396 different, defensible, town-level specifications. The results from Achen and Bartels' specification are denoted by dashed lines. See the text for details.

To show that the specifications in Table 3 were not cherry picked, Figure 1 shows the estimates resulting from all possible specifications where a researcher has access to the degrees of freedom utilized by Achen and Bartels. In each specification, the researcher must decide whether to classify attack, beach, or coastal counties as affected. Instead of controlling for the lagged value of the dependent variable, the researcher could implement a first-differences design—a strategy that Achen and Bartels utilize for their town-level analysis. The researcher can choose not to control for machine counties or to utilize either Mayhew's or Achen and Bartels' codings. And lastly, the researcher can choose to drop any single county or leave them all in the regression. In total, this

leads to 396 possible specifications that could be chosen. Figure 1 shows the point estimates and p-values arising from those 396 regressions, with Achen and Bartels' reported estimate and p-value denoted by dashed lines. In 361 cases (91%), the point estimate is greater than that in Achen and Bartels' specification. In 293 cases (74%), the estimate is not statistically significant at the conventional 5% threshold. And in 393 cases (99%), the p-value is greater than that in Achen and Bartels' specification.

The results in Table 3 and Figure 1 suggest that the evidence for the electoral impact of shark attacks in the 1916 presidential election in New Jersey is probably weaker than one would infer by looking at the single county-level regression result presented by Achen and Bartels. Their preferred specification involves several important choices. Although the reasons for the specific choices may each have been reasonable, the fact that almost all are necessary to maintain the result suggests the finding is not robust.

Figure 2 illustrates this county-level data visually. In the left panel, we plot each county's 1916 vote share across its 1912 vote share. The beach counties coded by Achen and Bartels as affected by shark attacks are denoted by red squares. If shark attacks depress vote share, we would expect these points to be notably below other counties with comparable vote shares in 1912, but as we see in the graph, several other counties actually experienced bigger vote decreases than the beach counties. The circles around these points show that they are among the counties coded by Achen and Bartels as machine counties. The right panel shows what happens when we adjust for the machine county indicator. Specifically, we regress 1916 and 1912 vote shares on an indicator for machine county, respectively, and plot the residuals. Now, we can see the beach counties shift down relative to the other counties, illustrating why adjusting for machine counties is important for the original finding. Both plots also illustrate that the other coastal counties, denoted by blue triangles,

Figure 2. County-level Vote before and after the 1916 Shark Attacks
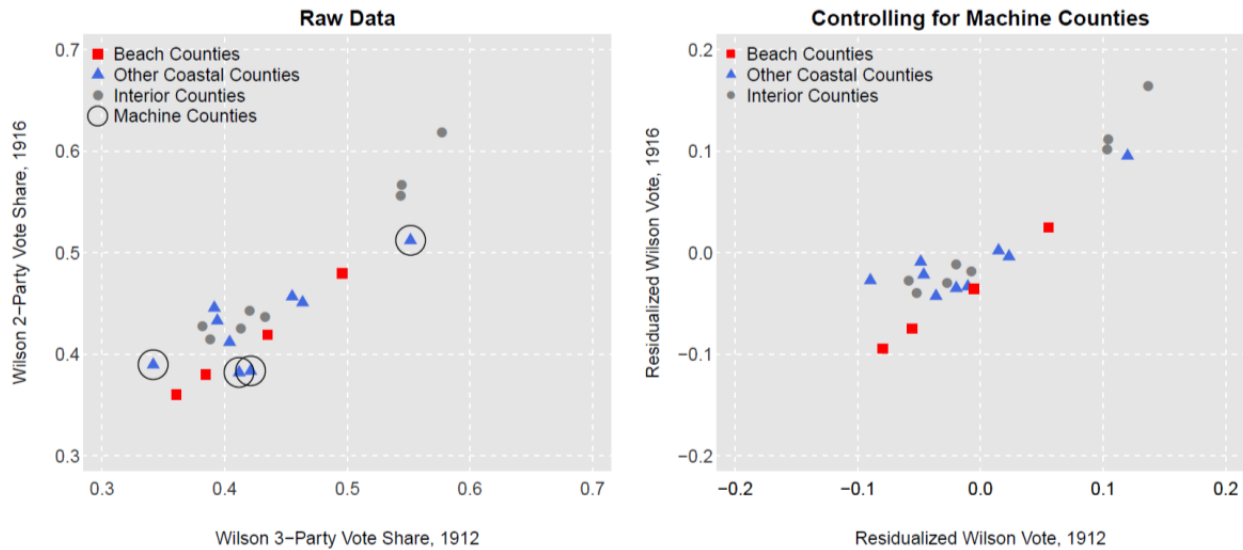
The plots show Wilson's vote share in 1912 (horizontal axis) and 1916 (vertical axis) across the counties of New Jersey. The left panel presents the raw data. The right panel shows residuals after regressing vote share on a machine county indicator.

tend to exhibit positive swings toward Wilson in 1916, explaining why the estimate weakens markedly when all coastal counties are coded as affected.

Reassessing the Town-Level Result from Ocean County in 1916

Achen and Bartels also conduct secondary analyses at the town level. First, they show that Wilson lost vote share in the towns of Spring Lake and Beach Haven, two of the four towns that experienced fatal attacks (2016, p. 125). These were small towns with less than 300 voters in each election, so seemingly large swings in vote share from one election to another could occur even in the absence of shark attacks. Also, we have discovered that Beach Haven's town boundary changed in 1913 (see Snyder 1969), so some of the change in vote share could be attributable to the compositional change in the town. Furthermore, while Achen and Bartels do not report it, Wilson actually *gained* vote share in Matawan Borough and Matawan Township, the two inland municipalities that also experienced fatal shark attacks (two of the four fatal attacks occurred in a

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river). The authors do provide an explanation for excluding them from this particular analysis (2016; pp. 124-125), speculating that because these two townships were not beach communities, the attacks should not have affected them. They also note that the number of voters increased considerably in both townships between 1912 and 1916.

To investigate whether the changes in Spring Lake and Beach Haven are anomalous, Achen and Bartels conduct a town-level analysis in Ocean County, comparing changes in Wilson's vote share for "beach" and "near beach" townships. They do not fully explain how they classify these towns, writing "there is no difficulty in separating those eight communities right on the beach" (2016, p. 126). By our count, nine municipalities in Ocean County are unambiguously beach communities—Barnegat (now Barnegat Light), Bay Head, Beach Haven, Harvey Cedars, Lavallette, Long Beach, Mantoloking, Sea Side Park (now Seaside Park), and Surf City—lying entirely on Long Island Beach or the Barnegat Peninsula. Five additional municipalities—Berkeley, Brick, Dover (now Toms River), Little Egg Harbor, and Point Pleasant Beach—are ambiguous, with portions in beach communities and portions on the mainland. Near-beach towns are defined as non-beach towns east of the current Garden State Parkway,[7] and Achen and Bartels classify all ambiguous towns as "near beach." Achen and Bartels further remove towns where the number of total votes cast changed by more than 25 percent between 1912 and 1916, and they remove Sea Side Park, writing that it "apparently split into two between 1912 and 1916" (2016, p. 126). Following their coding rules, we are left with 4 beach and 10 near-beach towns. In Column 1 of Table 4, we

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[7] Achen and Bartels write that "The western border of the near-beach area was set to the current Garden State Parkway" (2016, p. 126, fn. 15). Through private correspondence, we learned that Achen and Bartels included all towns with any territory to the east of the current Garden State Parkway with the exception of Lacey, which has most of its territory (but not its population) to the west of the Garden State Parkway. There was no clear rule for inclusion, but in any case, the inclusion or exclusion of Lacey holds little consequence for the subsequent results, and we utilize their exact coding rules in Table 4. When we later extend this analysis for other counties, in order to have a principled coding rule, we include all towns with any territory to the east of the Garden State Parkway.

Table 4. Replicating, Correcting, and Extending the Town-Level Analysis in Ocean County

	DV = Wilson's Vote Share Change (1916 – 1912)					
	(1)	(2)	(3)	(4)	(5)	(6)
Beach Town	−.128*	−.056	−.029	−.042	−.042	.004
	(.044)	(.050)	(.069)	(.052)	(.041)	(.010)
Constant	−.005	−.005	−.009	−.009	.001	
	(.013)	(.017)	(.023)	(.022)	(.015)	
Weight by Total Votes 1916	X	X	X	X	X	X
Include Sea Side Park		X	X	X	X	X
Drop Cases of Boundary Changes			X	X	X	X
Merge Long Beach and Beach Haven				X	X	X
Include Towns w/ Total Vote Change > 25%					X	X
Include All Beach Counties						X
County Fixed Effects						X
Observations	14	15	12	13	18	67
R-squared	.412	.088	.017	.056	.063	.079

*Uncorrected standard errors in parentheses; ** p<.01, * p<.05*

replicate Achen and Bartels' differences-in-differences analysis, regressing the change in Wilson's vote share on an indicator for beach town, including only beach and near-beach towns in Ocean County in the analysis, and weighting each town according to the total number of votes cast in 1916. The estimate of −12.8 percentage points is substantively large and statistically significant (p < .05).

In Column 2 of Table 4 we correct an apparent error made by Achen and Bartels. Sea Side Park did not, in fact, split into two between 1912 and 1916. This mistake would have been easy to make because a new town next to Sea Side Park, called Seaside Heights, was incorporated in 1913 (Snyder 1969). Around the same time, Sea Side Park changed its name to Seaside Park, so one might infer that Sea Side Park split into Seaside Park and Seaside Heights. However, we examined all changes to civil boundaries during this era, made available by Snyder (1969), and we learned that Seaside Heights drew territory from Berkeley and Dover; the boundaries of Sea Side Park did not change between 1912 and 1916.[8] Therefore, excluding Sea Side Park from this analysis is

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[8] We have also confirmed that Sea Side Park did not split via email correspondence with a local historian of the area.

unwarranted. When we include it in the regression, the estimated effect shrinks from −12.8 to −5.6 percentage points and is no longer statistically significant.

As mentioned above, although the boundaries of Sea Side Park did not change, the boundaries of several other towns in Ocean County *did* change between 1912 and 1916. Specifically, Berkeley and Dover gave up territory for the creation of Seaside Heights, and Beach Haven absorbed part of Long Beach. Therefore, in Column 3, we re-run the regression from Column 2 after dropping the towns that lost or gained territory (the sample size only drops by 3 because Long Beach was already excluded because its total votes decreased by more than 25 percent). After removing these cases, the point estimate shrinks further to −2.9 percentage points and is still statistically insignificant.

Because Beach Haven absorbed part of Long Beach, changes in their vote shares might be attributable to this territorial change rather than changes in the actual voting behavior of residents. However, both towns are beach communities, and they didn't exchange territory with any other towns, so if we treat the two towns as a single beach community, we can regain some statistical power by including this additional observation in the regression. We employ this strategy in Column 4 of Table 4, but it has little impact our inferences. The estimate of −4.2 percentage points remains statistically insignificant.

Now that we have accounted for all the territorial changes in Ocean County during this period, one might argue that Achen and Bartels' decision to exclude towns where the total number of votes changed by more than 25 percent is no longer necessary. Therefore, in Column 5 of Table 3, we repeat the procedure from Column 4 but include all beach and near-beach towns, regardless of changes in the total number of votes. This provides some additional statistical power, but the impact is minimal because most of the towns with large swings are small, and we weight by the total number of votes in the town. Accordingly, the resulting estimate is largely unchanged. Overall, there

is little evidence that the shark attacks hurt Wilson in the beach towns of Ocean County. We do obtain negative point estimates—between 2.9 and 4.2 percentage points for specifications that account for boundary changes—but these estimates are imprecise and not statistically distinguishable from zero.

Achen and Bartels conducted this analysis only for Ocean County, but if their previous claims are correct, we should also expect to see this pattern in the other three beach counties—Atlantic, Cape May, and Monmouth. Therefore, in Column 6 of Table 4, we replicate the analysis from Column 5 but pool data from all four beach counties. Consistent with our analyses from Ocean County, we classify every town in each of the four counties as a beach community, a near-beach town, or neither if they are entirely west of the current Garden State Parkway. We classify all ambiguous towns as near-beach, although the subsequent results are unchanged if we drop them or classify them as beach. We drop Middle Township in Cape May County because it lost territory to a newly incorporated town, Stone Harbor, in 1914, and we combine Atlantic Highlands, Highlands, and Middletown in Monmouth County into a single hybrid town, because they are all near-beach towns and they exchanged territory in 1914 and 1915. This leaves 34 beach and 33 near-beach towns (counting the two hybrid towns as single observations). In Column 6, we also include county fixed effects, allowing for the possibility that the counties followed different trends between 1912 and 1916, although this decision has no impact on the results. When we pool all four beach counties in this analysis, we gain significant precision. The estimate of 0.4 percentage points is substantively tiny, statistically insignificant, and opposite of the hypothesized sign. Looking at all four beach counties, we find that beach towns did not differentially decrease their support for Wilson after the shark attacks.

To be thorough and transparent, we again consider all possible town-level specifications using the previously discussed degrees of freedom. We allow the proverbial researcher to pick any

Figure 3. Town-Level Estimates across All Plausible Specifications
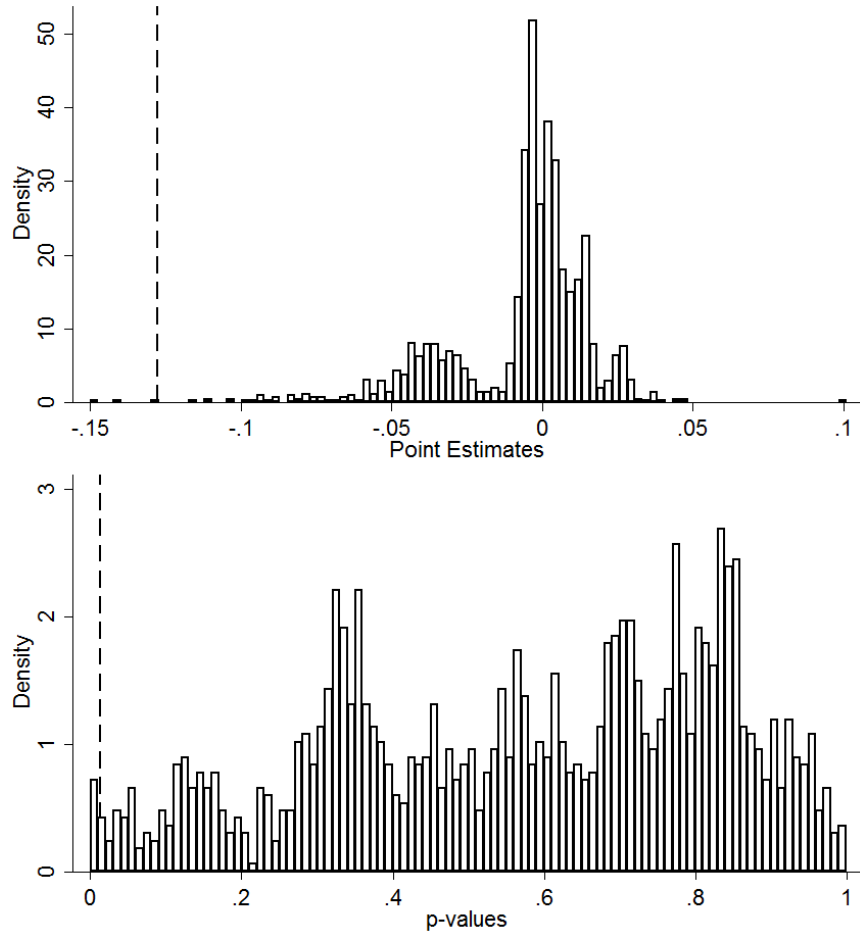
The figure shows the point estimates and p-values arising from 5,800 different, defensible, town-level specifications. The results from Achen and Bartels' specification are denoted by dashed lines. See the text for details.

single beach county to analyze or to pool all four, drop any single town from the analysis or keep them all, merge towns that exchanged territory or not, drop or keep instances of boundary changes, drop or keep cases where the population changed by more than 25 percent, and either implement a first-differences design or control for the lagged dependent variable. These choices create the opportunity to run 1,678 different regressions, and Figure 3 shows the point estimates and p-values that arise from each of these specifications, with the results of Achen and Bartels' specification denoted by dashed lines. In total, only 23 of those specifications (1%) produce negative, statistically significant estimates. 1,675 (99.8%) of the point estimates and 1,663 (99.1%) of the p-values are

Figure 4. Town-level Vote before and after the 1916 Shark Attacks
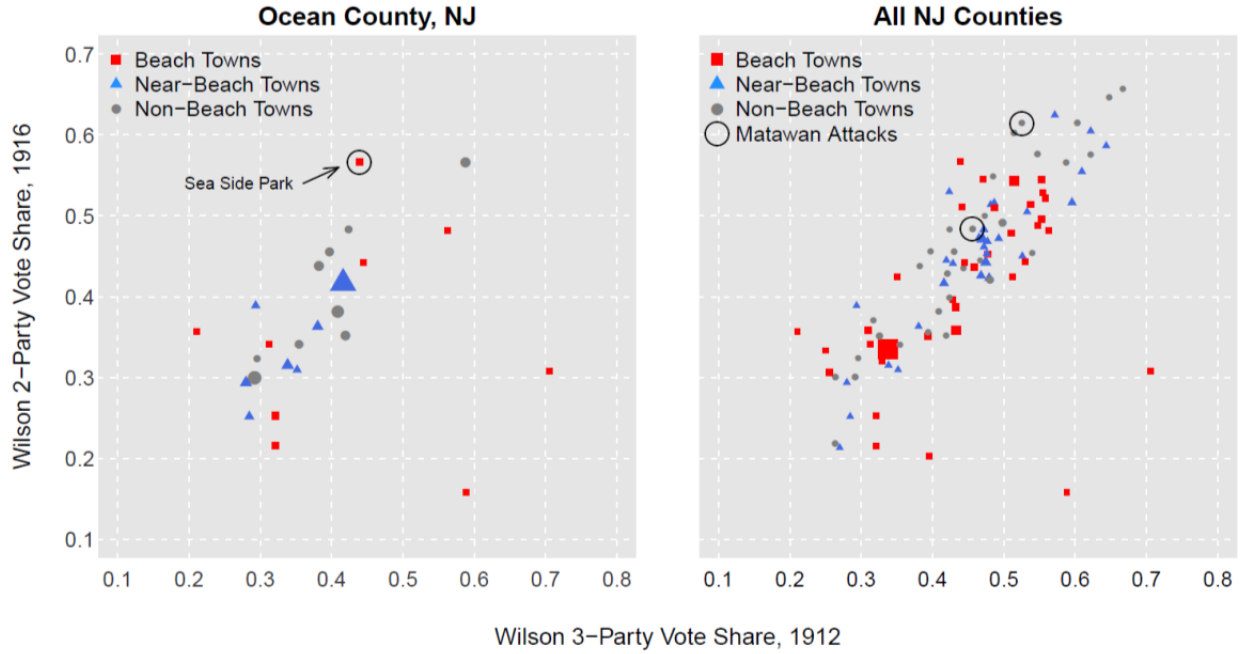
The plots show Wilson's vote share in 1912 (horizontal axis) and 1916 (vertical axis) across the towns of Ocean County (left panel) and all beach counties (right panel). The size of each point corresponds to the total votes cast in 1916.

greater than those reported by Achen and Bartels. In other words, the town-level estimate reported by Achen and Bartels is an extreme outlier that only arises when a very particular set of specification choices is made. Overall, we find little compelling evidence that shark attacks affected presidential voting in 1916 in New Jersey.

Figure 4 illustrates the town-level data visually. The left panel plots the 1916 and 1912 vote shares for the towns of Ocean County. The size of each point corresponds to the number of votes cast in 1916. We see that Achen and Bartels' result is primarily driven by two small towns that swung away from Wilson in 1916, although the inclusion of Seaside Park (circled), which was erroneously omitted by Achen and Bartels, notably dampens the differences between beach and near-beach towns. The right panel includes the towns from all four beach counties, not just Ocean County, and we see that those two outlying towns are particularly anomalous. There are no clear differences

between beach towns and near-beach towns; the two clouds of points (denoted by red squares and blue triangles, respectively) lie essentially on top of each other. Furthermore, the two inland towns that experienced fatal attacks—Matawan Township and Matawan Borough (circled)—increased support for Wilson between 1912 and 1916. The visual evidence confirms the findings from Table 4 that there is no evidence that the shark attacks hurt Wilson in New Jersey's beach towns.

Standard Errors, Placebo Tests, and Coastal Shocks

A separate concern with the original Achen and Bartels result has to do with the standard errors. The p-value for the coefficient of interest in their county-level regression is less than .01, implying that if shark attacks do not influence presidential elections, there is less than a 1 percent chance that we would have observed a result as large as theirs. However, there are good reasons to be skeptical of their standard error estimates and the corresponding claims of statistical significance. First, there are only 20 counties in the analysis, and standard error estimates—even if they are consistent—can be significantly biased for such small sample sizes. More importantly, this regression treats all counties as independent observations, but the beach and non-beach counties could be subject to different idiosyncratic shocks, making differences likely even in the absence of shark attacks.

Achen and Bartels' empirical strategy is close in spirit to a differences-in-differences design. Because they control for Wilson's vote share in 1912, they effectively compare the change in Wilson's vote share between 1912 and 1916 for beach counties and non-beach counties. In an influential critique of these kinds of designs, Bertrand, Duflo, and Mullainathan (2004) point out that the reported standard errors are often untrustworthy. Specifically, correlated errors may lead comparison units, under the null, to follow different trends more often than conventional standard errors would suggest, and depending on the circumstances, there may be no easy solution to this

problem. In this particular context, we might worry that the beach counties in New Jersey are subject to correlated political shocks not experienced by the rest of the state, or vice versa. Therefore, even if Achen and Bartels' empirical strategy is unbiased in expectation, it might produce statistically significant results more often than it should under the null.

To explore the extent of this problem, we assemble county-level returns from every presidential election between 1872 and 2012. Our general strategy is to reproduce the regression of Achen and Bartels in states and election years *when there were no shark attacks* to see how often we obtain results as large as theirs simply by chance. Of course, we cannot exactly replicate Achen and Bartels' strategy in every state-election. We do not know which counties they would have classified as beach counties, which counties would have been classified as machine counties, and which counties would necessitate being dropped. Nonetheless, we can closely mirror their approach by comparing coastal and non-coastal counties.

We focus on the 20 states that have both coastal and non-coastal counties; otherwise, we would be unable to make such a comparison. To ensure a clean placebo design, we drop every state-election where there was a fatal shark attack in the current or previous presidential election cycle. This leaves 593 state-elections for which we can conduct a placebo test. For each state-election, we regress the two-party Democratic vote share in each county on an indicator for coastal counties and a control for the two-party Democratic vote share in the previous election. Figure 5 presents histograms of the point estimates (top panel) and p-values (bottom panel) resulting from these 593 placebo tests.

In 187 (32%) of our placebo tests, the absolute value of the point estimate exceeds .032, that of Achen and Bartels' preferred specification in New Jersey in 1916, suggesting that point estimates as large as this occur frequently even when there are no shark attacks to exhibit electoral effects. In 161 (27%) of our placebo tests the estimated p-value is less than .05—a conventional bar for

Figure 5. Placebo Tests Show the New Jersey Result is Not Unusual under the Null
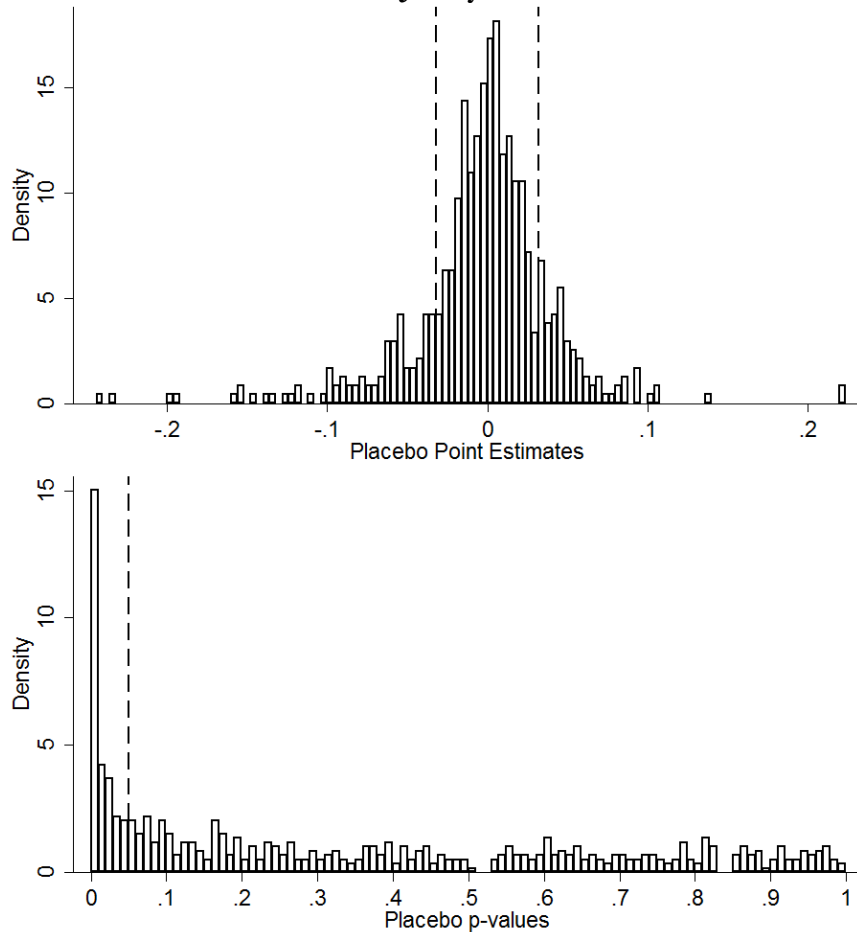
The figure presents the results of 593 placebo tests using every presidential election from 1876 to 2012 and every state with both coastal and non-coastal counties. State-elections with actual shark attacks in the current or previous election cycle are excluded. The top panel shows a histogram of point estimates. 32 percent of the point estimates have an absolute value greater than .032 (dashed lines), the estimated effect from Achen and Bartels. The bottom panel shows a histogram of p-values. If the standard errors were trustworthy, the distribution of p-values should be uniform, but we see that they are highly skewed, with 27 percent of the p-values below .05 (dashed line).

statistical significance, suggesting that the estimated standard errors from the New Jersey estimation strategy are not trustworthy. Under the null hypothesis, which is guaranteed in our placebo tests since we're excluding state-elections with shark attacks, the distribution of p-values should be uniform. However, the bottom panel of Figure 5 shows a highly skewed distribution, with 15 percent of cases below .01—as opposed to the 1 percent figure that we would expect if the standard errors are valid. Low p-values arise much more often than they should, suggesting that Achen and Bartels' standard errors dramatically overstate the precision and strength of the New Jersey result.

The results of our placebo tests in Figure 5 suggest that Achen and Bartels' result for New Jersey in 1916 was somewhat likely to arise even if shark attacks have no effect on presidential elections. The standard errors are biased downward because nearby coastal counties are subject to correlated political shocks. While Achen and Bartels report a p-value of about .01, a more appropriate estimate of their p-value is about .32, the frequency with which we would expect to see an estimate as large as theirs if shark attacks have no effect. Furthermore, although we do not have the data necessary to conduct comparable placebo tests for their town-level analysis, the same general concerns apply there. Beach towns are likely subject to correlated shocks, leading the standard errors associated with their town-level estimates to be misleading as well.

Discussion and Conclusion

In summary, we conclude that the there is little evidence supporting the influential claim that shark attacks affect elections. At best, the evidence is inconclusive, and any such effect, if genuine, is substantively small. Our analyses suggest that recent concerns about voter competence, at least as they relate to voters' abilities in ignoring irrelevant events, are overblown. The evidence on shark attacks and presidential voting is not strong enough to warrant dismay toward voters or the democratic process.

Beyond the topics of elections and voter behavior, this study adds to the current debates on replication, transparency, and reporting bias. Although Achen and Bartels followed the standard practices in their field, they appear to have significantly overstated the strength of evidence in favor of their hypothesis. To see how this problem can arise, imagine what would have happened had Achen and Bartels found no evidence in favor of their hypothesis. Most likely, they would not have written a paper confirming that shark attacks do not influence elections, and that finding would not have been the centerpiece of a book about how elections *do* produce responsive governments. When

a result would only have been reported if the results had come out in a particular way, we should naturally be more skeptical and demand further evidence. We have no way of knowing how many similar tests came up null for the research community before this newsworthy result arose.

To their credit, Achen and Bartels do report that some of their hypothesis tests produced null results. Specifically, they report that they tested for the electoral effects of drought, locust plagues, and Spanish Influenza, and they reportedly identified electoral effects for drought but not locusts or Spanish flu. They criticize voters for responding to sharks and drought, and surprisingly, they also express dismay toward voters for *not responding* to locusts and Spanish flu, writing "it seems clear that a rational electorate could reasonably have held its leaders accountable" (2016, p. 141). Why rational voters *should not* have responded to shark attacks or droughts but *should* have responded to locust plagues and Spanish Influenza is never clarified, and this interpretational flexibility should be troubling for Achen and Bartels' conclusions. An empirical test cannot be compelling if *any* result can be interpreted as supporting the same conclusion.

Our analyses also raise concerns about tests of purportedly irrational behavior more generally. Though published studies have presented a variety of irrelevant factors that purportedly influence behavior, we have no way of knowing how many other irrelevant factors were examined and found to have no effect. Furthermore, when a researcher has decided to test for a purportedly irrational behavior, all bets are off regarding the nature of that effect. For example, should the effects of shark attacks appear only in beach communities and not in the near-beach communities? Should they appear in all beach communities in the state or only in those that experienced the attacks? Theory provides no guidance, so any conjecture and corresponding specification might be just as defensible as the next. The sheer number of hypotheses and specifications that could be presented as consistent with irrational behavior should make readers worried about the validity of the few statistically significant effects reported. In the case of irrational voters responding to

irrelevant events, our results in this paper, in conjunction with those of Fowler and Montagnes (2015), suggest that the consensus in the academic literature may have been skewed by a few influential false-positive results.

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