Beaches or Boarding?: Shark Attacks, Tourism, and the 1916 Presidential Election*

Abstract

Many recent studies have offered contrasting conclusions as to whether voters punish elected officials in response to "irrelevant events." One prominent case considers whether shark attacks in New Jersey in 1916, which negatively impacted the tourism industry in the state, prompted voters to punish Woodrow Wilson in his reelection bid. Achen and Bartels (2004, 2016) provide evidence of an electoral penalty, while Fowler and Hall (2018) argue there is no consistent evidence of this penalty – findings Achen and Bartels rebut (2018). This short article revisits this debate by introducing new data on communities' susceptibility to the negative economic consequences alleged to have prompted electoral harm for Wilson: the size of the local hotel industry. Incorporating information about New Jersey hotels obtained from a contemporaneous travel guide, we find no evidence communities proximate to beaches with large hotel industries exacted stronger penalties against Wilson relative to beach communities without a large concentration of hotels or to non-beach communities. Our findings highlight the importance of incorporating into studies of the electoral consequences of irrelevant events historical context and tests of the mechanisms posited to link events to electoral outcomes.

*The quantitative analysis presented in this manuscript is pre-registered through the Open Science Framework (https://osf.io/ydwex/?view_only=5d8e093c97c2449490b675f6ee8e8ab7).

In recent years, political scientists have explored the effects of "irrelevant events" on public opinion and elections. While many studies suggest voters hold incumbents responsible for outcomes over which they wield no power, such as floods, droughts, and sporting events (e.g., Busby et al. 2017; Graham et al. 2023a; Healy et al. 2010), subsequent studies, many of which replicate and extend prior findings, have inspired lively debates about whether voters rely on "blind retrospection," are attentive to the context in which events like natural disasters occur, and use government responses to learn about incumbent quality (e.g., Ashworth et al. 2018; Fowler and Montanges 2015, 2023; Freeder and O'Brian 2022; Gasper and Reeves 2011; Graham et al. 2023b; Muller and Kneafsey 2023; Rapeli and Soderlund 2022; Uttermark et al. 2024).

One of the most prominent irrelevant events cases concerns a spate of four shark attacks in New Jersey in the summer of 1916 that may have hampered President Woodrow Wilson's electoral performance there. In their seminal work on blind retrospection, Achen and Bartels (2004, 2016) show these attacks prompted a decline in beach tourism on the Jersey Shore which, in turn, reduced electoral support for Wilson. However, Fowler and Hall (2018) criticize some of Achen and Bartels' measurement and modelling choices and present analyses that do not provide consistent evidence for a relationship between shark attacks and the 1916 election results - challenges which Achen and Bartels rebut (2018). Fundamental to Achen and Bartels' rebuttal is their argument that Fowler and Hall's analyses do not reflect the "unusual features" of the 1916 case – particularly that the attacks "caused very substantial local business losses, with shore resorts suffering 75% vacancy rates in the midst of their high season" (Achen and Bartels, 2018, 1439).[1] As a result, Achen and Bartels present Fowler and Hall's findings as criticism "grounded in generic methodological skepticism rather than careful engagement with the relevant theory and evidence" (Achen and Bartels, 2018, 1450).[2] Achen and Bartels stress they welcome challenges that incorporate new historical data or evidence relevant to the specific historical context of the 1916 shark attacks and their (potential) electoral consequences.

In this paper, we aim to do so by revisiting this case with new data that better captures the mechanism posited to link shark attacks to electoral outcomes: the scale of local beach tourism (Achen and Bartels 2016, 118-121; 2018, 1438-1440). Specifically, we incorporate information about hotel industry size, which accounts for the *scale* of each community's tourism industry. By extending prior work to include hotel industry size, we better reflect the conceptual link between shark attacks and electoral outcomes and allow for a more granular analysis of Achen and Bartels' expectations. Our analyses do not provide evidence that shark attacks hampered Wilson's electoral performance. These findings should encourage future work on irrelevant events to utilize measures that directly reflect the theoretical mechanisms expected to link events to political outcomes while remaining sensitive to the historical context of those cases.

Measuring Susceptibility to Shark Attack Effects through Proximity to Beaches

[1] For a detailed discussion of Achen and Bartels' argument for why shark attacks would have produced electoral punishment in the 1916 presidential election, Fowler and Hall's criticisms, and Achen and Bartels' rejoinder, see Supplemental Information Section A.

[2] In an online response, Fowler and Hall take issue with Achen and Bartels' claim that "our enthusiasm for rigorous quantitative evidence has somehow led us to neglect history or context" (Fowler and Hall n.d.).

In arguing shark attacks depressed support for Wilson, Achen and Bartels assert the "substantial economic losses" they caused in tourism-dependent communities, coupled with fear and contemporary blame directed at Wilson and the federal government, led voters to punish Wilson (2018, 1438). To model this, the prior studies operationalize affected communities as those proximate to beaches, as "they are the places in which the shark attacks would have had the most pronounced economic effects" (Achen and Bartels 2016, 121). In doing so, they presume all communities proximate to beaches have comparable tourism industries such that shark attacks would manifest homogeneous negative economic, and subsequently electoral, effects.

However, beach-related tourism activity was not uniformly distributed among New Jersey communities in 1916. While some locales, like Atlantic City, served as major tourist destinations, other cities and towns proximate to beaches had smaller or nonexistent tourism industries at that time (Mazzagetti 2018, 72). For instance, while Bay Head is coded by both Achen and Bartels and Fowler and Hall as a beach town, it had few hotels in the early 20th century and even today "remains a quiet upper-income community for homeowners and renters… [with] few hotels and restaurants and little or no nightlife" (2018, 161). Further, while Brigantine City was a resort in the late 19th century and is coded as such, most of its commercial efforts had failed by the 1910s, leaving travelers with few establishments to visit (2018, 199).[3]

Variation in the extent to which beach communities in 1916 New Jersey relied on tourism, and thus were susceptible to the economic and electoral effects of the consequences of the shark attacks, suggests relying on binary indicators for whether communities are proximate to

[3] See Supplemental Information Section A for more information about variation in the presence of tourism activity among communities proximate to beaches in 1916 New Jersey.

the beach may introduce measurement error. Suppose there is a true effect of shark attacks on support for Wilson in beach communities with large tourism industries, but *not* in beach communities with little tourist activity. If both types of communities are coded as "beach," the effect of shark attacks in tourism-reliant beach communities will be diluted by the absence of an effect in beach communities less dependent on tourism. Since such measurement error makes estimates less precise, even slight changes to model specification, such as those Fowler and Hall (2018) implement, could prompt substantial shifts in parameter estimates and test statistics.

In overlooking heterogeneity in communities' tourism industry size, Achen and Bartels and Fowler and Hall also forego opportunities to probe "theoretically relevant heterogeneous effects" that facilitate a more thorough test of their expectations (Graham et al. 2023a). Specifically, given Achen and Bartels' expectation that economic harm caused by depressed tourist activity prompts electoral harm for Wilson, we should expect that as economic harm increases, the electoral penalty increases as well. However, as the studies measure susceptibility to adverse effects through a binary indicator, they cannot probe this more granular expectation.

Measuring Susceptibility to Shark Attack Effects through Hotel Industry Size

To improve the measurement approach described above, we utilize information about the size of the hotel industry in New Jersey communities in 1916. While hotels do not account for all tourism-related economic activity, they provide an important signal of the relevance of tourism to a locale, especially in the 1910s when transportation was sparse and time-consuming such that tourists traveling even short distances to New Jersey's beaches would stay in hotels. Thus, the size of a community's hotel industry facilitates a more fine-grained measure of its reliance on tourism and, thus, susceptibility to the negative effects of shark attacks.

We obtain information on hotels in New Jersey communities from the 1916 edition of the *American Hotel Directory Travelers' Blue Book of All America* (*AHD*), which lists the name and number of rooms for each hotel in each community.[4] According to *AHD*, New Jersey contained 1,180 hotels with 55,718 rooms in 1916.[5] Figure 1 displays the relative density of hotels (right pane) and rooms (left pane) across New Jersey's 21 counties. While the majority of hotels and rooms are in the four Jersey Shore counties Achen and Bartels and Fowler and Hall code as "beach counties" (Atlantic, Cape May, Monmouth, and Ocean), there is considerable variation in hotel industry size among them—for instance, Monmouth contains more than twice as many hotels (393) as Atlantic (179) and Cape May (152) and nearly four times as many as Ocean (99). Additionally, some "non-beach" counties have sizable hotel industries, such as Morris, whose 71 hotels approach the number of those in Ocean. Substantial variation also exists among communities in the Jersey Shore counties used in Achen and Bartels and Fowler and Hall's town-level analyses. For instance, the "beach" communities of Atlantic City and Asbury Park have high numbers of hotels (158 and 122, respectively), but 8 of the 33 "beach" communities have 0 hotels. Further, while communities that have no beaches or include substantial non-beach land area tend to have fewer hotels, the number of hotels in some of these communities, such as Lakewood and Neptune, are above the 90th percentile among Jersey Shore communities (77 and

[4] See Supplemental Information Section B for more information about *AHD*.

[5] While our counts of hotels and hotel rooms are highly correlated (r>0.90 at the county- and town-level), they represent different concepts. To assess the robustness of our results across concepts, we utilize both measures.

37, respectively).[6] This variation *among* and *between* "beach" and "non-beach" communities enables us to better measure communities' susceptibility to economic harm caused by shark attacks and probe how the scale of their tourism industries condition the effect of those attacks.

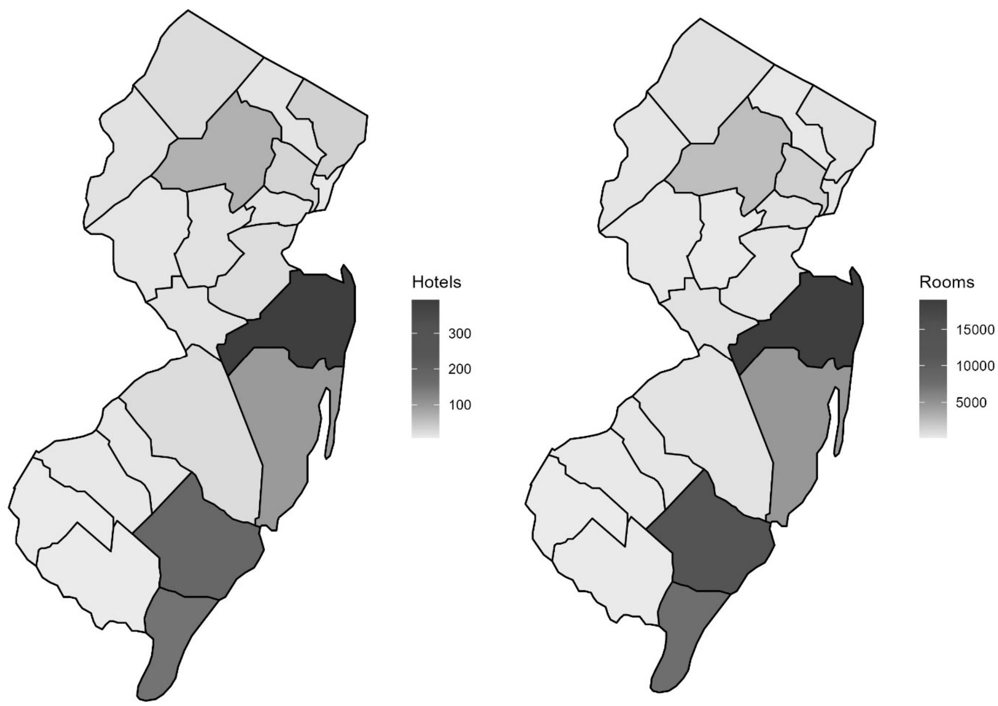

Figure 1: Distribution of Hotels and Hotel Rooms in New Jersey Counties, 1916

Results

In Tables 1 and 2, we extend the county- and town-level analyses, respectively in Tables 3 and 4 of Fowler and Hall (2018), which substantively replicate Achen and Bartels (2004, 2016), by adding interactions between their binary indicators for whether a county or town was a

[6] See Supplemental Information Section B.2 for visual representations of the distributions of hotels and rooms at the town-level.

beach community and our measures of hotel industry size.[7] In the regression analyses, we measure hotel industry size on a per capita basis (hotels or hotel rooms per 1,000 voters or 100 voters at the county- and town-levels, respectively) and rescale them to range from 0 to 1.[8] In doing so, the coefficients on hotel industry size correspond to the effect of moving from a locality with minimal hotel industry presence to one with maximal hotel industry presence relative to the size of the locality. The key coefficient of interest is the interaction term, which the theory of blind retrospection expects to be negative and statistically distinguishable.[9]

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[7] Following Graham et al. (2023a), we collected original data for variables used in prior analyses where feasible (see Supplemental Information Section C).

[8] In our pre-registration, we planned to use the counts of hotels and hotel rooms to measure hotel industry size. However, to ease interpretability and account for the magnitude of a county or town's hotel industry relative to its size, we deviate from our pre-registration by using per capita measures rescaled to range from 0 to 1 (see Supplemental Information Section D for further discussion). Following Achen and Bartels and Fowler and Hall, we measure county and town population as the total number of voters in the 1916 election. We present the corresponding analyses that use hotel and hotel room counts in Tables SI.1 and SI.2; the results from these analyses are substantively similar to those presented in Tables 1 and 2.

[9] In Supplemental Information Section D, we implement best practices for interactions by examining marginal effects (Brambor et al. 2006) and probing the plausibility of the linear interaction effect and common support assumptions for each model (Hainmueller et al. 2019). Most of the models exhibit violations for at least one of these assumptions, in which case we utilize alternative methods to account for those violations such as a semi-parametric kernel

Table 1: Interactive Effect of Hotel Industry Size and Community Type (County-Level)

	Model 1		Model 2		Model 3		Model 4		Model 5		Model 6	
	(a)	**(b)**	**(a)**	**(b)**	**(a)**	**(b)**	**(a)**	**(b)**	**(a)**	**(b)**	**(a)**	**(b)**
Beach County	-0.02	-0.02	-0.03	-0.02					-0.01	-0.01	0.00	-0.00
	(0.03)	(0.04)	(0.04)	(0.05)					(0.04)	(0.06)	(0.05)	(0.06)
Attack County					-0.30	-0.28						
					(0.87)	(0.98)						
Coastal County							-0.01	-0.01				
							(0.01)	(0.01)				
Hotels Per Capita (Rescaled)	-0.06		-0.03		-0.04 *		-0.07		0.15		0.23	
	(0.12)		(0.16)		(0.02)		(0.14)		(0.18)		(0.21)	
Beach: Hotels Per Capita (Rescaled)	0.04		0.02						-0.16		-0.24	
	(0.13)		(0.17)						(0.18)		(0.21)	
Attack: Hotels Per Capita (Rescaled)					0.50							
					(1.49)							
Coastal: Hotels Per Capita (Rescaled)							0.03					
							(0.14)					
Hotel Rooms Per Capita (Rescaled)		0.01		0.07		-0.04 *		-0.04		0.25		0.33
		(0.18)		(0.23)		(0.02)		(0.19)		(0.26)		(0.28)
Beach: Hotel Rooms Per Capita (Rescaled)		-0.03		-0.08						-0.27		-0.35
		(0.18)		(0.24)						(0.27)		(0.29)
Attack: Hotel Rooms Per Capita (Rescaled)						0.48						
						(1.72)						
Coastal: Hotel Rooms Per Capita (Rescaled)								0.00				
								(0.19)				
Machine (Achen and Bartels)	-0.06 *	-0.06 *	-0.04 *	-0.04 *	-0.06 *	-0.06 *	-0.05 *	-0.05 *				
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)				
Machine (Mayhew)											0.01	0.01
											(0.02)	(0.02)
Wilson 1912 Vote Share	0.96 *	0.95 *	0.87 *	0.86 *	0.98 *	0.96 *	0.95 *	0.94 *	0.90 *	0.90 *	0.90 *	0.90 *
	(0.07)	(0.07)	(0.09)	(0.08)	(0.07)	(0.07)	(0.07)	(0.07)	(0.11)	(0.10)	(0.11)	(0.10)
Intercept	0.04	0.05	0.08 *	0.08 *	0.03	0.04	0.05	0.06	0.05	0.05	0.04	0.04
	(0.03)	(0.03)	(0.04)	(0.04)	(0.03)	(0.03)	(0.03)	(0.03)	(0.05)	(0.05)	(0.05)	(0.05)
Include Essex County			Y	Y								
R^2	0.95	0.95	0.90	0.90	0.95	0.95	0.95	0.95	0.86	0.86	0.87	0.87
Num. obs.	20	20	21	21	20	20	20	20	20	20	20	20

***** denotes *p*<0.05. The outcome in each model is Wilson's two-party vote share in 1916. (a) and (b) models denote usage of number of hotels or number of rooms per capita (rescaled to range from 0 to 1) to measure hotel industry size, respectively. Models 1-6 correspond with those in Table 3 of Fowler and Hall (2018).

Looking first at the county-level analyses in Table 1, we find no evidence that the electoral penalty for Wilson increases in "beach" counties relative to "non-beach" counties as the size of the county's hotel industry grows. Across models, the interaction terms are inconsistently signed

estimator. In implementing these alternative methods, none of the models provide conclusive evidence of an interactive effect consistent with the blind retrospection expectation.

and never distinguishable from zero, and corresponding marginal effects indicate no range of the support of hotels or rooms for which the expected negative relationship manifests.

Table 2: Interactive Effect of Hotel Industry Size and Community Type (Town-Level)

	Model 1		Model 2		Model 3		Model 4		Model 5		Model 6	
	(a)	(b)	(a)	(b)	(a)	(b)	(a)	(b)	(a)	(b)	(a)	(b)
Beach Town	-0.29 *	-0.35 *	-0.04	-0.15	-0.14	-0.34	-0.06	-0.16	0.06	0.04	0.03 *	0.03 *
	(0.10)	(0.11)	(0.14)	(0.16)	(0.19)	(0.18)	(0.15)	(0.16)	(0.08)	(0.07)	(0.02)	(0.02)
Hotels Per Capita (Rescaled)	0.23 *		0.23		0.23		0.23		0.11		-0.03	
	(0.08)		(0.13)		(0.15)		(0.15)		(0.11)		(0.04)	
Beach Town:	-0.01		-0.24		-0.04		-0.20		-0.20		-0.08	
Hotels Per Capita (Rescaled)	(0.14)		(0.22)		(0.32)		(0.23)		(0.18)		(0.06)	
Hotel Rooms Per Capita		0.27		0.27		0.25		0.34		0.13		-0.06
(Rescaled)		(0.12)		(0.20)		(0.16)		(0.24)		(0.20)		(0.09)
Beach Town:		0.02		-0.13		0.12		-0.18		-0.22		-0.09
Hotel Rooms Per Capita (Rescaled)		(0.18)		(0.28)		(0.26)		(0.30)		(0.27)		(0.11)
Intercept	-0.02	-0.02	-0.02	-0.02	-0.02	-0.02	-0.02	-0.02	-0.02	-0.01	-0.02	-0.01
	(0.01)	(0.01)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.01)	(0.01)
Include Sea Side Park			Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Drop cases of boundary changes					Y	Y	Y	Y	Y	Y	Y	Y
Merge Long Beach and Beach Haven							Y	Y	Y	Y	Y	Y
Include towns with total vote change >25%									Y	Y	Y	Y
Include all beach counties											Y	Y
County fixed effects											Y	Y
R^2	0.71	0.69	0.28	0.24	0.26	0.40	0.26	0.27	0.08	0.05	0.17	0.16
Num. obs.	15	15	16	16	13	13	14	14	19	19	71	71

***** denotes $p<0.05$. The outcome in each model is the change in Wilson's vote share between 1916 and 1912. (a) and (b) models denote usage of number of hotels or number of rooms per capita (rescaled to range from 0 to 1) to measure hotel industry size, respectively. Models 1-6 correspond with those in Table 4 of Fowler and Hall (2018).

Turning to the town-level analysis in Table 2, we again find no consistent evidence for blind retrospection. While the interaction terms are negative in 10 of the 12 specifications, none attain statistical distinguishability. Additionally, while most of the models we, Achen and Bartels, and Fowler and Hall estimate utilize a small number of observations (21 or fewer), which may prompt concerns about precision, even Models 6a and 6b, which follow Fowler and Hall's expansion to include all Jersey Shore counties besides Ocean, utilize substantially larger sample sizes (71), do not yield evidence of a distinguishable effect consistent with blind retrospection.[10] Further, when examining the models' marginal effects, electoral support for Wilson often *increases* as the size of the hotel industry grows among beach communities, albeit at slower rates than among non-beach communities, though this differential trend is never distinguishable from zero (see Supplemental Information Section D). Taken together, the results presented in Tables 1 and 2 provide scant evidence the 1916 shark attacks prompted penalties for Wilson in locales susceptible to the tourism-related economic harm they are thought to have caused. [11]

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[10] Another benefit of Models 6a and 6b in Table 2 is that they also exhibit the widest range of common support for the hotel industry size measures across beach and non-beach communities (encompassing roughly 70% of observations). Thus, among all specifications extended from Fowler and Hall (2018), these models offer the best opportunity to obtain precise estimates from observations that demonstrate substantial variation in hotel industry size. While the standard errors are fairly large relative to the coefficients, further inspection of the marginal effects from these and alternative model specifications cast doubt on the existence of a hotel industry size-conditional effect of shark attacks on change in vote share in excess of a negative 0.05 decrease (see Supplemental Information Section D.6 for more details).

[11] While our measures of hotel industry size account for all hotels and hotel rooms in each community, they do not account for tourism activity that may occur in neighboring locales that contribute to the community's economy (e.g., a person who lives in a given community but is employed by a hotel in an adjacent community). Additionally, workers may have similarly lived (and voted) in neighboring areas without hotels. To the extent that such spillover was present in 1916 New Jersey, our measurement strategy may underestimate communities' reliance on

Conclusion

In this note, we revisit one of the most prominent cases in the irrelevant events literature—did shark attacks in New Jersey preceding the 1916 election sway public opinion on President Woodrow Wilson?—to introduce new historical data that helps better reflect the mechanism underlying the blind retrospection expectations initially posed by Achen and Bartels (2004, 2016). Our analyses, which incorporate the size of communities' hotel industries to account for their susceptibility to the tourism-related economic harm thought to prompt electoral punishment for Wilson, yield scant evidence to support the original expectations.

These results could lead to at least two plausible implications. First, despite Achen and Bartels' rebuttal, our results might bolster the conclusions of Fowler and Hall (2018, n.d.) that the initial findings of Achen and Bartels (2004, 2016) were false positives and that the shark attacks did not affect support for Wilson in 1916 New Jersey. Such a conclusion may cast doubt on the notion of blind retrospection and the lack of confidence in voters' competence to award credit and blame in elections. Second, our null findings may suggest that if blind retrospection manifested in this case, it did so through a mechanism other than the tourism-related economic harm Achen and Bartels emphasized. For instance, perhaps the local media's coverage of the shark attacks that placed blame on Wilson and the federal government, rather than depressed tourist activity, harmed Wilson at the polls (Achen and Bartels 2016, 118-121). Alternatively, the shark attacks may have affected voter behavior by activating anxiety, which subsequently reduced support for Wilson

tourism. To account for potential spillover, we repeat our analyses in Table SI.3 at the town-level by including in our hotel industry size measures the hotels and hotel rooms in each town and all neighboring towns; these analyses yield results substantively similar to those in Table 2.

(Lehrer et al. 2024). We encourage future scholars studying irrelevant events to consider and thoroughly probe the theoretical mechanisms and related measures they expect to link the events themselves to the political outcomes of interest given the historical context of the events they explore, as doing so will cultivate a more complete and rigorous understanding of how irrelevant events manifest political implications (Achen and Bartels 2018; Fowler and Hall n.d.).

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Supplemental Information for "Beaches or Boarding?: Shark Attacks, Tourism, and the 1916 Presidential Election"

Section A: Achen and Bartels (2004, 2016, 2018) versus Fowler and Hall (2018) and Measuring Susceptibility to Economic Harm

This paper contributes to the debate between Achen and Bartels and Fowler and Hall concerning whether shark attacks in New Jersey in 1916 resulted in voters in affected areas punished incumbent president Woodrow Wilson in the presidential election that year. In this section, we highlight the specific arguments and tests each set of authors presented in their respective publications, where the authors continue to disagree at the end of their exchange, and how our paper contributes to the ongoing debate.

Christopher Achen and Larry Bartels first introduced the 1916 New Jersey shark attacks case in a conference paper in the early 2000s. While their use of this example of 'blind retrospection' was already cited extensively as a working paper, it was first published in 2016 as part of a chapter in their book *Democracy for Realists: Why Elections Do Not Produce Responsive Government* (Achen and Bartels, 2016). In this book, Achen and Bartels introduce the case as evidence for the broader consideration "that natural disasters threaten rulers and regimes" and that, in the modern democratic context, "when election time comes, the electorate continues to hold rulers responsible for calamities and disasters that are clearly beyond their control" (Achen and Bartels, 2016, 117).

In explaining *why* the 1916 shark attacks in New Jersey could have caused electoral punishment for the incumbent president, Woodrow Wilson, that year, Achen and Bartels provide considerable historical context. The shark attacks took place between July 1 and 12, 1916 off the coast at Beach Haven and Spring Lake beach and, inland, by Matawan. While the attacks ended after the killing of a great white shark near Matawan Creek, by July 14, the attacks received media coverage across the United States – in part because shark attacks against humans were novel at the time. Notably, Wilson (as well as Secretary of the Treasury William McAdoo and Wilson's aide Joseph Tumulty) were all vacationing in New Jersey at or around the time the attacks occurred. While the administration received requests for support from locals, and Wilson held a cabinet meeting to discuss the attacks, no major efforts were undertaken by the federal government to provide assistance (Achen and Bartels, 119-120). In identifying their logic for why the shark attacks could cause electoral punishment against incumbent president Woodrow Wilson, Achen and Bartels argue there are three reasons for why the attacks may have prompted New Jersey voters punish Wilson:

> "First, the attacks caused several deaths plus considerable emotional and financial distress to shore communities. Second, the election occurred just a few months after the summer's events, increasing the likelihood that they would be fresh in the minds of the voters as they went to the polls. Third, high federal officials were present at the scene from the beginning, reinforcing the notion that the federal government should have done *something* to deal with the crisis. The fact that no government has any influence over sharks would, from our perspective, have been irrelevant to the voters" (Achen and Bartels, 2016, 120).

Notably, these three arguments do not explicitly mention tourism – the core focus of our paper. However, both before and after this section of the chapter, it is clear that the "financial distress" Achen and Bartels reference concerns a decline in tourism in response to media coverage of the attacks. For instance, in their description of the shark attacks and their consequences on the preceding, Achen and Bartels describe how

> "[in] the aftermath of the attacks, the federal government was called on for help. The resorts were losing money rapidly, with $250,000 in reservations cancelled within a week. Some resorts had 75% vacancy rates in the midst of their high season. […] Losses may have amounted to as much as $1 million for the season altogether […]. Letters poured into congressional offices from the affected communities demanding federal action, though there was little any government agency could do." (Achen and Bartels, 2016, 119)

More importantly, in defining which New Jersey counties they believe should be considered 'treated' by the shark attacks, Achen and Bartels define their as "beach counties" as "Monmouth, Ocean, Atlantic, and Cape May" because they were

> "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" (Achen and Bartels, 2016, 121)

Crucially, the counties Achen and Bartels consider treated include two counties in which shark attacks took place (Monmouth and Ocean) and two (Atlantic and Cape May) where no attacks occurred. Thus, in their view, the 'treatment' of the shark attacks was *not* where the attacks occurred, but where the economic (and, presumably also, emotional) effects of the attacks should have occurred: the areas of the state with beach tourism. Similarly, in their discussion of town-level data Achen and Bartels note that they exclude two of the locations where the actual attacks occurred because "Matawan Township and Matawan Borough […] are not beach resort communities and thus suffered no widespread economic loss from their shark attacks or anyone else's" (Achen and Bartels, 2016, 124, fn 13).

Achen and Bartels test their theory of blind retrospection in the wake of the shark attacks through a statistical model in which the dependent variable is Wilson's vote share in 1916, and the independent variable is whether a county in New Jersey was one of the four beach counties.[1] They find a negative and statistically significant effect for beach counties on Wilson's 1916 vote share, suggesting that voters in those four counties were less likely to support Wilson than were voters in other New Jersey counties. In addition to their main statistical analysis, Achen and Bartels also present evidence showing the decline in Wilson's vote share in the beach townships that saw some of the attacks (Spring Lake and Beach Haven) and beach townships in Ocean County relative to the rest of the state and to towns that were near but not on the beach (Achen and Bartels, 2016, 125 and 127). In doing so they again stress the importance of the economic impact of the attacks, arguing that comparing beach and near-beach townships is a helpful test of their theory because the townships "right on the beach" had "economies [that] were damaged by the shark attacks" while the other townships were "near the beach but not on it" and that their

[1] Their model includes two control variables: Wilson's 1912 vote share (in the form of a three-way fraction) and a dummy variable indicating whether a county should be considered a 'machine count' or not.

"economies were less susceptible to harm" (Achen and Bartels, 2016, 126) because of this distinction.

In their 2018 *Journal of Politics* article, Fowler and Hall test Achen and Bartels' shark attack theory in multiple ways. Among these are a new statistical analysis testing whether deadly shark attacks caused electoral punishment in presidential elections over the period 1872-2012, alternative model specifications for the 1916 county-level analysis Achen and Bartels relied on,[2] models replicating and adjusting the statistical analysis assessing the effect of the 1916 attacks at the town level, and several placebo tests (Fowler and Hall, 2018a). Collectively, these analyses fail to show a consistent negative and statistically significant relationship between shark attacks and presidential voting, either across time and elections or in the 1916 case specifically.

However, Fowler and Hall's interpretation of the mechanism that could connect shark attacks to potential electoral punishment is relatively direct. While they acknowledge that Achen and Bartels "offer a number of reasons for focusing on this single case, including that there was an 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" (Fowler and Hall, 2018a, 1425), they do not directly incorporate the presence of the specific economic consequences Achen and Bartels note as part of their measures.[3] For example, their test of the effect of deadly shark attacks across 1872-2012 on presidential elections focuses on the relationship between attacks and support for the incumbent party or president, regardless of whether the attacks had economic consequences. Similarly, Fowler and Hall show that if the definition of a New Jersey county being treated by the shark attacks is adjusted to either only those counties where the attacks took place *or* all coastal counties (that is, not just the "beach counties" but any New Jersey county on the coast), the effects are not statistically significant.

In their response to Fowler and Hall, Achen and Bartels (2018) – also in the *Journal of Politics* – dismiss many of their alternative models and tests as irrelevant to the 1916 case in large part because Fowler and Hall, they argue, do not incorporate the specific reasons for why voters would have been susceptible to blind retrospection in that context. For example, Achen and Bartels reject the argument that the data showing no consistent impact of deadly shark attacks on presidential elections between 1872-2012 is relevant to their case because they claim that the 1916 example of shark attacks is unique due to a variety of "unusual features" related to the attacks – first among which being that "the attacks caused very substantial local business losses, with Shore resorts suffering 75% vacancy rates in the midst of their high season" (Achen and Bartels, 2018, 1439). Indeed, throughout their response, Achen and Bartels again stress the importance of the economic downturn related to beach tourism in defining what areas of New Jersey were treated by the shark attacks in 1916. For example, Achen and Bartels reject Fowler and Hall's alternative county-level models that either limit treatment to those counties in which the attacks took place or expand it to all coastal counties because they "fly in the face of both common sense and standard accounts of the distinctive dependence on tourism of exactly the four Jersey Shore counties we identified" (Achen and Bartels, 2018, 1442). Achen and Bartels also stress the importance of tourism in their defense of comparing beach and near-beach townships in Ocean County since "apart from the beach communities having an Atlantic

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[2] This includes including Essex County in the data for one model, defining counties as treated if they were the location of actual shark attacks *or* on the coast independent of their beach tourism, and relying on a different measure of machine politics.

[3] Indeed, the terms "tourism," "tourist," "resort," or "hotel" do not appear in Fowler and Hall's article.

shoreline and summer visitors, the communities on each side are similar" (Achen and Bartels, 2018, 1446).

On this basis, Achen and Bartels reject Fowler and Hall's article and categorize it as a type of criticism that is "grounded in generic methodological skepticism rather than careful engagement with the relevant theory and evidence" (Achen and Bartels, 2018, 1450). Achen and Bartels conclude their rebuttal with the assessment that their finding that the 1916 shark attacks negatively affected Wilson's vote share remains true for "the economically affected areas that fall" (Achen and Bartels, 2018, 1450) despite Fowler and Hall's alternative models and test consistently showing null findings. While Achen and Bartels are not convinced by Fowler and Hall's critique of their findings, they do stress that they welcome "serious future scholarship" that incorporates "additional evidence and insight" (Achen and Bartels, 2018, 1451) relevant to the specific historical context in which the 1916 shark attacks happened and may have affected voting behavior.

In a final response to Achen and Bartels published online, Fowler and Hall (n.d.) reject the claim that they failed to engage with history appropriately – noting that this argument is "especially unpersuasive since it was our historical research that uncovered consequential errors in their analyses" (Fowler and Hall, n.d., 1). In their rebuttal to the rebuttal, Fowler and Hall (among others) argue that, with regards to the 1916 case, major concerns with Achen and Bartels' approach and findings remain – including their reliance on 1912 election results as a control variable and their presentation of standard errors. Fowler and Hall also criticize Achen and Bartels' (new) arguments for excluding the town Seaside Park from their analyses and answer multiple questions raised regarding their town-level data. Notably, however, Fowler and Hall do not engage with Achen and Bartels' argument that the 1916 shark attack effect required the economic downturn and that – for geographic areas in New Jersey to be considered treated, they would need to both be coastal areas *and* have had considerable tourism – leaving at least one element of Achen and Bartels' response unanswered.

This paper aims to take up this remaining Achen and Bartels argument and their challenge to find "additional evidence and insight": if the decline in tourism income is one of the crucial element of the mechanism that connects the shark attacks in New Jersey in 1916 to where voters in the state would have punished Woodrow Wilson – which is what Achen and Bartels are arguing in both their book and their 2018 article – then identifying a better measure of what geographical areas of the state saw more beach tourism in 1916 should produce a more accurate test of the shark attacks theory. While a dichotomous coding of geographic areas having or not having beach tourism is an understandable basic approach to measuring which areas would have been affected, a continuous measure of the size of the hotel industry in counties and towns in New Jersey in 1916 provides a more detailed measure of exactly where the biggest economic losses – and, thus, electoral punishment – was and should have been.

Section A.1: Limitations of Proximity to Beaches as a Measure of Susceptibility to Economic Harm

We reassess the "shark attacks" debate by proposing a new measure of which areas of New Jersey should be expected to have been affected by the causal theory Achen and Bartels propose in their seminal work on this topic. While they disagree on many measurement decisions, both Achen and Bartels – in their original study – and Fowler and Hall – in their replication and extensions of this study – rely on a binary measure of which areas in New Jersey were affected by the shark attacks and their subsequent consequences on tourism. Relying on binary indicators may help distinguish the communities most susceptible to economic harm following the attacks from those unlikely to experience negative consequences. However, this approach assumes that the magnitude of the harm is homogenous across beach communities. This is certainly not true across all coastal areas in New Jersey. For example, as part of their rebuttal of Fowler and Hall, Achen and Bartels note that certain ocean connected parts outside of the "Jersey shore" are unlikely to have faced economic consequences since they consisted of "gritty beachside docks and warehouses" (2018a, 1442) where no form of tourism occurred. Because of this, Achen and Bartels reject Fowler and Hall's reliance on an "all coast" measure of affected counties as they argue such a measure does not reflect the causal mechanism Achen and Bartels identified.

But even within the Jersey shore part of New Jersey there was differentiation in the size of the tourism industry. In some beach areas no major tourism industry developed: for example, Bay Head is coded as a beach town in the Fowler and Hall dataset, but it had only a small number of hotels in the early 20th century (Mazzagetti, 2018, 161). Other areas did see major tourist-centered economic developments at some point either before or after but not *in* 1916. For example, Brigantine City was a resort community in the late 19th century but by the 1910s few if any of such establishments were left (Mazzagetti, 2018, 199). And, even among coastal towns and cities that did attract beach tourism in 1916, there was major variation in scope: since the early 20th century, Atlantic City has been a major beach tourist destination with a boardwalk "dominated by grand hotels, each more spectacular and architecturally alluring than the last," but many other communities that attracted beach travelers had a much smaller tourist industry (Mazzagetti, 2018, 72). Combined, there is thus reason to be concerned that a binary measure of towns or even counties being 'on' the Jersey Shore could introduce substantial measurement error – conflating areas that had little to no tourism (and, thus, should not be affected by a decline in visitors after the shark attacks) with those that did.

To assuage these concerns, we propose an alternative measure of which areas were more likely to be affected by the shark attacks and the economic downturn they caused: coastal areas with higher levels of tourism industry as measured by the number of hotels (and hotel rooms) in each area. Crucially, we argue this measure better reflects the causal mechanism proposed by Achen and Bartels (and the evidence they presented of economic downturn).

Section B: Hotel Industry Size as a Measure of Susceptibility to Economic Harm

Section B.1: Coding of Hotel Industry Data

To measure the size of the hotel industry across New Jersey in 1916, we rely on the 1916 edition of the *American Hotel Directory Travelers' Blue Book of All America* (henceforth *AHD).* By its own account, the *AHD* covers "practically every hotel on the North and South American continents and islands adjacent" (*American Hotel Directory*, 1916, i).[1] First published in 1915, the *AHD*'s second edition a year later was much expanded and relied on a more comprehensive approach to gathering hotel data from across the United States. Specifically, the guide's publisher contacted "the mayor, leading merchant and postmaster of every small town or village," as well as the editors of newspapers based in those towns, to get a list of local hotels (*American Hotel*

[1] Or at least those "fit for a white man to stop at" (*American Hotel Directory*, 1916, iv).

Directory, 1916, v). In cities with fewer than 50,000 inhabitants, the guide also contacted the local Chamber of Commerce or Board of Trade, while for larger cities a representative of the guide visited the area. Combined, the guide's publisher employed "[e]ighteen stenographers, translators, and clerks" who "spent ten months in securing and compiling" the hotel data (*American Hotel Directory*, 1916, v).

For each municipality in each US state, *AHD* lists the names of each hotel it identified operating there, and for each hotel provides information about its number of rooms and the price per room per night. For some hotels, *AHD* also indicates whether it is a summer or winter resort and a subjective evaluation of the quality or character of the hotel. In a small number of cases (28) the guide lists a name but no additional information except "Data suspended." The guide does not provide additional explanation on how to interpret that, however, by cross checking these hotels with competing hotel guides and other historical sources we have found no evidence to suggest these hotels were no longer in business in 1916. Thus, we include these hotels in our hotel-count data. Since the listings do not include the number of hotel rooms, these entries are excluded from the room-count measure.

In transcribing the *AHD* listings for each New Jersey municipality such that they would be compatible with the administrative units used in the New Jersey legislature's official manual to report election results (Fitzgerald 1913, 1917), we noticed some cases in which the towns listed by *AHD* did not match with any towns listed in the manual. When we encountered these cases, we utilized historical resources such as Snyder (1969), which provides a detailed discussion of the development of New Jersey civil boundaries between 1606-1968. We describe these discrepancies and how we addressed them below:

- The *AHD* has listings for Elwood and Mays Landing in Atlantic County. Elwood was part of Mullica township and Mays Landing was part of Hamilton township. We code any hotels listed as such.
- The *AHD* lists hotels in Anglesea, Cape May Court House, Holly Beach, Tuckahoe, and Peermont in Cape May County. Anglesea was part of North Wildwood township, Holly Beach was part of Wildwood township, Tuckahoe was part of Upper township, and Peermont was part of Avalon township. We code all hotels accordingly.
- The *AHD* lists hotels in Branchport, Brielle, Elberon, Keansburg, Little Silver, Locust Point, Matawan, Ocean Grove, Oceanic, Sea Girt, and West End in Monmouth County. Most of these listings are relatively straightforward to adjust: Branchport, Elberon, and West End were part of Long Branch township in 1916. Brielle became its own township in 1919 but was part of Wall township before. Similarly, Sea Girt became its own township in 1917 but in 1916 was still part of Wall as well. Little Silver was part of Shrewsbury prior to 1923. Ocean Grove was part of Neptune township in 1916. Oceanic was part of Rumson township. We code all hotels accordingly. The data from Monmouth does include two more complicated issues:
    - The *AHD* has listings of significant number of hotels in Keansburg, which does not become its own township until 1917. Prior to that, the geographic areas that would become this township were located in two different townships: Raritan and Middletown. We are unable to determine whether the different hotels were in Raritan or in Middletown. As a result, we merge the two towns into one entry: Middletown-Raritan.

    - The *AHD* has listings for hotels in Matawan but this could refer to one of two different townships: Matawan township and Matawan borough. For each hotel listed we were able to determine which town the listing should be coded for:
        - A post on the Matawan Historical Society's website mentions that Matawan House was situated "near the junction of Main Street and Maiden Lane."[2] An intersection of two streets by these names occurs only in Matawan Borough. Consequently, we match this hotel listing with Matawan Borough.
        - A post on a local history blog references the Aberdeen Inn as having been "opposite Mattawan railroad station."[3] The station was on Main Street in Mattawan borough. We match this hotel with Matawan Borough.
- The *AHD* lists hotels in Barnegat, Forked River, Lakehurst, New Egypt, Pine Beach, Tom's River, and Waretown. Forked River was part of Lacey township. Lakehurst became its own township in 1921 but in 1916 was part of Manchester. New Egypt was part of Plumsted township. Pine Beach was part of Berkeley township. Tom's River's official name is Dover. Waretown was part of Ocean township.
    - The *AHD* lists hotels for Barnegat and Barnegat City which cover two distinct areas: Barnegat (which currently is known as Barnegat Light) and Union (which today is known as Barnegat township). The AHD lists two hotels (Almont Inn and Clarence Hotel) as having been in Barnegat. The Almont Inn is located in what is today Barnegat township and in 1916 was Union.[4] The Clarence Hotel was on the corner of West Bay and Railroad Avenue which is also in modern-day Barnegat township and thus Union in 1916.[5] The other *AHD* listings were for Barnegat City which are therefore matched to Barnegat.

Section B.2: Potential Limitations of Hotel Industry Measure and Remedial Approaches

To be sure, relying on counts of hotels and hotel rooms at the county- and town-levels with the goal of representing the extent to which a decline in tourism affected New Jersey voters is not perfect. First, hotels do not account for the full amount of tourism-related economic activity a community may enjoy: after all, visitors of the Jersey Shore are likely to have also spent money in other types of establishments, such as restaurants and stores. However, the size of the hotel industry in coastal areas represents the relevance of tourism to the community: since transportation was sparse and time consuming in the 1910s, tourists traveling even minimal distances to the Jersey Shore typically stayed in hotels. Thus, information about the size of the hotel industry in New Jersey counties and towns would facilitate a more fine-grained measure of

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[2] "Bell, George Washington (1814-1897)." *Matawan Historical Society*. https://matawanhistoricalsociety.org/bell-george-washington-1814-1897/.
[3] "History: Aberdeen Inn, Matawan NJ – Part 2," *Aberdeen NJ Life*, https://aberdeennjlife.blogspot.com/2011/06/history-aberdeen-inn-matawan-nj-part-2.html.
[4] "Take a Walking Tour of Historic Barnegat," *Asbury Park Press*, May 29, 2024, https://www.app.com/story/life/2014/05/29/barnegat-walking-tour/9719717/.
[5] See: https://thehistorygirlnj.tumblr.com/post/167719036274/mansardmonday-the-clarence-hotel-in-barnegat.

the extent to which specific geographic areas relied on tourism and were more likely affected by the shark attacks and subsequent downturn in tourism in the summer of 1916.

Second, while we argue the hotel industry is a good measure of the size of the local tourist industry, the economic effects associated with those hotels are not necessarily confined to the localities in which they reside *or* equally distributed across its populations. Regarding the latter, the number of hotels and hotel rooms should reflect the size of the tourist industry but not necessarily how much it affected voters – for example, areas with different numbers of hotels may also have had different population sizes and, thus, different per capita effects of economic downturns related to hotel cancellations. Additionally, the negative economic effects could have spilled over to neighboring localities. From the perspective of tourists, while the convenience and efficiency of travel was more constrained in 1916 as it is in modern-day and required tourists to board at establishments close to their destinations, in some areas it was feasible for tourists to stay at a hotel in one locality but – at least theoretically – spend their leisure time and money in a neighboring locality. Similarly, from the perspective of residents, in some areas it was possible for people to live and vote in one locality but work at a hotel or another tourism-related business in a nearby locality.

For instance, the Jersey Shore in Monmouth County was composed of numerous small towns that would have been easy for tourists to travel between within the course of a single day (e.g., Bradley Beach, Neptune, and Neptune City were tightly clustered on and near the shore and each had many hotels). To the extent that tourists spread their activities out beyond the boundaries of the locality in which they boarded and residents worked in different localities than they lived, our analysis may fail to account for spillover effects—that the effect of shark attacks on tourism-related economic conditions within a given locality includes not only the decrease in the number of tourists boarding or residents working within the locality, but also the decrease in the number of tourists who may board in a neighboring locality and spend some of their time and money in the locality of interest or residents who may work in a tourism-related job in a neighboring locality but live and vote in the locality of interest.

To alleviate both concerns we present alternative specifications of our main analyses in Supplemental Information Section D: one in which we evaluate the effect of per capita hotels and hotel rooms and another in which we account for the numbers of hotels and rooms in each locality and the localities bordering it. Both models produce similar results to the findings presented in the main paper.

Section B.3: Descriptive Statistics of Hotel Industry Data

After transcribing the *AHD* listings for each New Jersey municipality, we constructed two different measures of hotel industry size for each New Jersey county and for each of the towns in the four Jersey Shore counties used in Achen and Bartels and Fowler and Hall's town-level analysis (Atlantic, Cape May, Monmouth, Ocean). First, we tabulated the number of hotels listed in each geographic unit, which represents the scope of independent lodging businesses based in each unit. The 1916 *AHD* lists 1,180 hotels in New Jersey, 823 of which are in the four Jersey Shore counties. Second, we calculated the number of rooms listed for the hotels listed in each geographic unit, which represents the volume of tourism activity the hotel industry in that unit could accommodate. We utilize this second measure because hotels can vary widely in scale; while the largest hotel in New Jersey at the time, the Traymore Hotel in Atlantic City, had 700 rooms, 54 hotels listed in

AHD contained as few as 10 rooms. The 1916 *AHD* lists a total of 55,718 rooms across the state of New Jersey, with 45,308 of those rooms located in the four Jersey Shore counties.

Figure SI.1, which is the same as Figure 1, displays the relative density of hotels (right pane) and hotel rooms (left pane) across the 21 New Jersey counties in 1916. For both measures, the vast majority of the hotel industry's footprint is in the four Jersey Shore counties. While this concentration is consistent with Achen and Bartels and Fowler and Hall's characterizations of areas proximate to the ocean as most susceptible to the negative effects of shark attacks, this figure communicates two important pieces of information about the limitations of their binary coding scheme. First, there is considerable variation with respect to hotel industry size among the Jersey Shore counties; for instance, Monmouth contains more than two times as many hotels (393) as Atlantic (179) and Cape May (152), and nearly four times as many as Ocean (99). If the means by which shark attacks diminish political support for Wilson is the negative consequences they have for tourism, this variation in hotel industry size implies that the magnitude of the electoral penalty for Wilson should be larger in Monmouth than in the other counties. Second, outside the Jersey Shore counties, we still observe variation in the size of the hotel industry, with Morris County containing nearly as many hotels as Ocean (71) but Cumberland and Salem Counties containing very few hotels (8 each). This variation allows us to gain insight on whether units with larger hotel industries exhibited uniformly lower support for Wilson or if those political consequences manifested only in units whose tourism industries depended on proximity to the ocean.

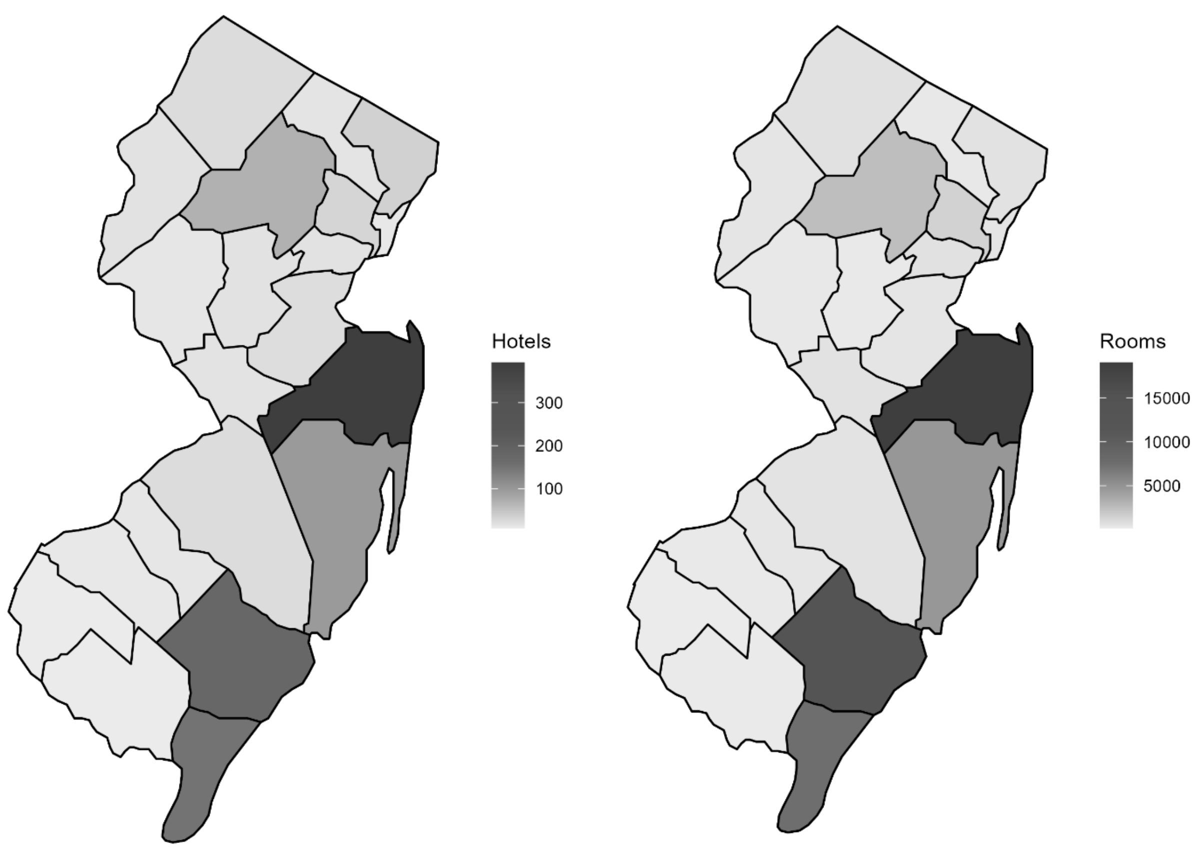

Figure SI.1: Distribution of Hotels and Hotel Rooms in New Jersey Counties, 1916

At the town level, variation across towns proximate to and distant from the beach is—unsurprisingly—even wider.[6] Figure 2 presents histograms for the number of hotels (right pane) and hotel rooms (left pane) in the four Jersey Shore counties. The shading in these histograms reflects our independent application of the coding scheme used by Achen and Bartels and Fowler and Hall to categorize towns as beach towns ("beach"), proximate to the beach but contains little or no coastline ("near beach"), or containing elements of both preceding town types ("both"). First, we see substantial variation in the size of the hotel industry across beach communities. For instance, while some beach communities such as Atlantic City and Asbury Park have high numbers of hotels (158 and 122, respectively) and hotel rooms (13,512 and 7,413, respectively) 8 of the 33 beach communities have zero hotels. Thus, whereas the town-level analyses in Achen and Bartels and Fowler and Hall assume that beach communities should express uniformly stronger negative effects on support for Woodrow Wilson relative to near beach and both communities, our measures of hotel industry size demonstrate that some beach communities were more vulnerable to the consequences of shark attacks for tourism and therefore should express effects of larger magnitudes than other beach communities.

Second, while the towns with the largest hotel industry presence are Beach communities, nearly half of the near beach or both communities have at least one hotel (18 of 38) and several have substantial numbers of hotels and hotel rooms, such as Lakewood (37 and 1,762) and Neptune (77 and 3,005). This variation across community type allows us to assess whether hotel industry concentration moderated the effect of shark attacks on support for Wilson more strongly in the communities posited to experience the largest drops in tourism activity—beach communities. Taken together, the hotel industry data not only better capture the theoretical mechanism linking shark attacks to reduced electoral support for the incumbent president, but also provide richer variation in measuring the exposure of communities to those posited negative effects that facilitates more granular analyses.

[6] The histograms in Figure 2 include the 71 towns in Atlantic, Cape May, Monmouth, and Ocean Counties that we will utilize in our replication of Model 6 in Table 4 from Fowler and Hall (2018), which is the model that utilizes the most unique towns. See the Appendix for further information on which towns we include, either as standalone units or as part of combined units.

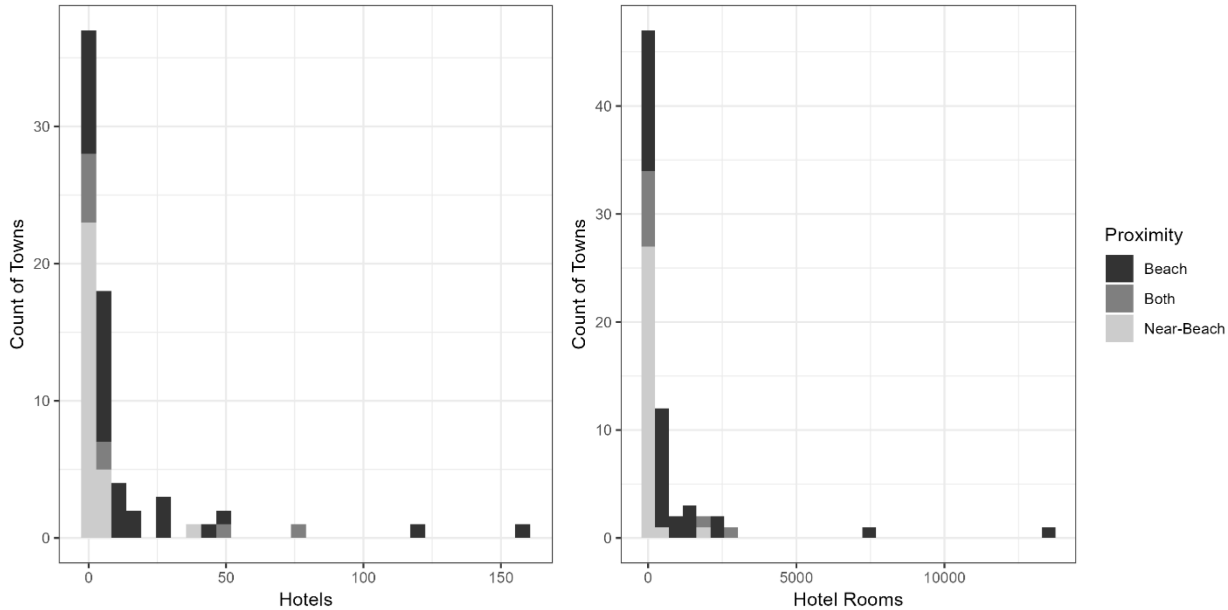

Figure SI.2: Distribution of Hotels and Hotel Rooms in Jersey Shore Communities, 1916

Section B.4: Comparison of AHD Information to Alternative Travel Guides

The *AHD* was not the only hotel travel guide published regularly in this era, but in comparison to its main competitors—such as *The Official Hotel Red Book and Directory* (henceforth "*Red Book*") and *The John Willy Hotel Directory* (henceforth "*Willy's*")—it provides a considerably more detailed representation of the size of the hotel industry in different locations. Specifically, we chose to use *AHD* as the basis of our analyses because of two advantages it offers over its competitors. First, *AHD* covers a much broader range of hotels: while *AHD* aimed to collect practically *all* hotels, the other guides were more selective. For example, by its own description the *Red Book* covered "the Best Hotels in the United States and Canada, including Summer and Winter Resorts" (*The Official Hotel Red Book and Directory*, 1917, p. 5). As a result, the *AHD* presents a much more comprehensive list of New Jersey hotels from across the entire state than the other guides: to illustrate, the *Willy's* published in 1916 has 353 entries for the state of New Jersey, the 1916 *Red Book* has 478, but the 1916 *AHD* lists 1,180 hotels. The more extensive measure of hotels is beneficial precisely because it allows for a test of the core theory – namely that a decline in tourism after the shark attacks would affect hotels on the beach but not those in other parts of the state. Second, the *AHD* is the only hotel guide that lists the number of hotel rooms in each establishment. As a result, the *AHD* listings allow us to measure the number of hotel rooms by county and town.

While we do not use them in our analysis directly, the hotel listings in *Red Book* and *Willy's* provide us with an opportunity to assess the reliability of the listings provided by *AHD*. To do so, we transcribed the hotel entries for each municipality in New Jersey from the 1916 editions of the *Red Book* and *Willy's* directories and, as we did for *AHD*, standardized municipality names (and combined municipal units where necessary) to match with those used in the 1913 and 1917 editions of the New Jersey legislature's official manual (Fitzgerald 1913, 1917). When hotels listed in the same county had similar names but were listed as in different towns across guides, we drew on historical resources to determine whether the listings referred to the same hotel (and, if so, in which town it was truly located) in order to limit duplicates.[7]

Across the three guides, we identified 1,328 unique hotels. Comparing the coverage of hotels in *AHD* to those in the alternative guides across the full state of New Jersey, we find that *AHD* includes 388 of the 478 (81%) listed in *Red Book* and 271 of the 353 (77%) listed in *Willy's*. To ensure that our measures of hotel industry presence across our geographic units of interest—counties and towns—are substantively similar across guides, we also assessed the county-level and, among the four counties included in the town-level analysis (Atlantic, Cape May, Monmouth, and Ocean), town-level correlations of the number of hotels listed in each geographic unit. At both levels, the *AHD* count of hotels is highly correlated with those in the alternative guides; at the county-level the correlation with *Red Book* is 0.94 and with *Willy's* is 0.71, and at the town-level the correlation with *Red Book* is 0.90 and with *Willy's* is 0.80. Taken together, the substantial overlap across *AHD* and *Red Book* and *Willy's*, as well as the strong correlations in hotel counts from the alternative guides across geographic units, demonstrate substantial reliability of *AHD's* hotel listings.

Section C: Original Collection of Existing Data

Following Graham et al. (2023), we collect all data other than that on hotels and machine counties independently rather than relying on the publicly available replication data from Fowler and Hall (2018a).[8] We detail how we collected each variable below:

- *Election returns*: We transcribe 1912 and 1916 presidential election returns at the county and town level from the 1913 and 1917 editions of the New Jersey legislature's official manuals, respectively (Fitzgerald 1913, 1917). This election data is used in the county-level analyses to measure the outcome—Wilson's share of the two-party vote in 1916—as well as one of the control variables—Wilson's share of the three-party vote in 1912. For the town-level analyses, this election data is used to measure the outcome variable—the change in Wilson's vote share between the 1912 and 1916 elections—and weight our observations by the total number of votes cast in each town in 1916.

  One important complication in constructing units of observation measured at two points in time for the town-level analyses is that the boundaries of several of the towns in the

[7] When we were able to conclusively determine that listings across guides referred to the same hotel, we retained only the entry corresponding with the hotel's true location. If we were not able to conclusively make this determination, we retained both entries.

[8] To account for areas where party bosses turned against Woodrow Wilson between the 1912 and 1916 elections, Achen and Bartels (2016) initially coded Bergen, Hudson, Essex, and Union Counties as "machine" counties, as they had at least 30,000 votes in 1916 and 60% or more foreign citizens in the 1910 Census. In addition to adopting this coding procedure used by Achen and Bartels (2016), Fowler and Hall (2018) also utilize a measure of "machine" counties from Mayhew (1986), which instead identifies Camden, Essex, Hudson, Mercer, Middlesex, and Passaic Counties as "machine" counties. We are unaware of any other plausible coding schemes for "machine" counties and therefore adopt those of Achen and Bartels (2016) and Fowler and Hall (2018) as described.

four New Jersey counties included—Atlantic, Cape May, Monmouth, and Ocean—changed boundaries between 1912 and 1916. Shifting boundaries were a central point of debate between Achen and Bartels (2016) and a and Hall (2018)—particularly the towns of Sea Side Park and Seaside Heights in Ocean County. In independently collecting town-level data, we identified cases of shifting boundaries using John F. Snyder's 1969 book, *The Story of New Jersey's Civil Boundaries 1606-1968*, which the New Jersey Geographic Information Network cites as a reference for generating its own maps. We did not identify any cases of shifting boundaries that were not identified by either Achen and Bartels (2016) or Fowler and Hall (2018), and use this space to discuss our own disposition of these cases:

- Sea Side Park and Seaside Heights (Ocean County)—In their original analysis, Achen and Bartels exclude Sea Side Park because it "apparently split into two between 1912 and 1916 and jointly nearly doubled in size," rendering it an inconsistent unit of analysis (2016, 126). Fowler and Hall (2018a) argue that Snyder's 1969 book demonstrates that Seaside Heights instead drew territory from Berkley and Dover, not Sea Side Park, and subsequently include Sea Side Park in most of their analyses. Achen and Bartels (2018) counter that the historical record suggests that Seaside Heights' election returns before its incorporation in 1913 were included in those of Sea Side Park, such that its creation substantially changed the composition of the electorate reported as Sea Side Park and the town should be excluded from the analysis. In replicating Table 4 of Fowler and Hall (2018a), we will naturally assess whether the inclusion or exclusion of Sea Side Park substantively affects our results; therefore, we do not establish a firm coding rule of our own but instead incorporate those from both sets of authorship teams.
- Beach Haven and Long Beach (Ocean County)—In 1913, Beach Haven absorbed some territory from Long Beach. Both towns are in Ocean County on the Jersey Shore and did not exchange territory with any other municipalities between 1912 and 1916. Consequently, Fowler and Hall (2018a) merge these towns in Models 4-6 of Table 4 to construct a consolidated unit that maintains its boundaries across the two periods. Given that in replicating Table 4 of Fowler and Hall (2018) we will naturally compare models that do not and do count these towns as a consolidated unit (Models 3 and 4, respectively), we again do not need to establish our own coding rule but instead will treat Beach Haven and Long Beach as did both sets of authorship teams.
- Atlantic Highlands, Highlands, Middleton, and Raritan (Monmouth County)—Between 1912 and 1916, pieces of Middletown were reallocated to Atlantic Highlands and Highlands, but the boundaries of these three towns were otherwise stable. Because combining the three municipalities would create a consolidated unit that is stable across the two periods, Fowler and Hall (2018a) create a single observation that utilizes as its election returns variables the sums of each of the three towns. After reviewing Snyder's accounts of boundary changes, we agree with this assessment. Further, because the *AHD* hotel listings for Keansburg required us to merge Middletown with Raritan, we combine all four towns into a single observation.

- Middle and Stone Harbor (Cape May County)—In 1914, Stone Harbor was created using part of the territory previously included in Middle. Fowler and Hall (2018) drop Middle from their analyses because of this change. However, the creation of Stone Harbor from part of Middle was the *only* change to Middle's boundaries between 1912 and 1916, such that creating a consolidated unit that included both Middle and Stone Harbor would create an observation whose boundaries remain consistent across the two periods. Accordingly, we depart from Fowler and Hall (2018a) by merging Middle and Stone Harbor to retain them as a single observation.

- *Beach/Attack/Coastal*: In their county-level analysis, Achen and Bartels (2016) code the four counties that contained the Jersey Shore—Atlantic, Cape May, Monmouth, and Ocean—as "beach" counties. Fowler and Hall (2018) carry forward this coding, and also estimate alternative specifications that instead identify counties as either experiencing a shark attack or being on the coast. The four deadly shark attacks occurred in Monmouth and Ocean Counties, and we follow Fowler and Hall (2018) in coding these two counties as "attack" counties. Fowler and Hall (2018a) subsequently code counties as "coastal" if the National Oceanographic and Atmospheric Administration (NOAA) classifies them as "Coastal Shoreline Counties" that "have a coastline bordering the open ocean or the Great Lakes, or contain coastal high hazard areas (V-zones)."[9] We utilized the current NOAA list of Coastal Shoreline Counties to code counties as "coastal," and our codings match those of Fowler and Hall (2018a).

  In their town-level analysis, Achen and Bartels focus on towns in Ocean County and subset to those that are either "beach" towns or "near beach" towns, the latter category being those towns that are west of the current Garden State Parkway but do not themselves contain beachfront territory (2016: 129). Fowler and Hall (2018a) extend this analysis to include the other three "beach" counties—Atlantic, Cape May, and Monmouth—and apply the same coding rules to towns in those counties. While our codings generally match those of Achen and Bartels (2016) and Fowler and Hall (2018a), we note several points of departure below based on our own assessments of the contemporary town boundaries according to Snyder's 1969 book and ancillary historical records.
  - Atlantic Highlands, Highlands, Middleton, and Raritan (Monmouth County)—As we depart from Fowler and Hall (2018) by consolidating Atlantic Highlands, Highlands, Middletown, and Raritan into a single observation, we also need to reconcile the codings of coastal proximity for the individual municipal units. Fowler and Hall (2018a) coded each unit as follows: Atlantics Highlands, near beach; Highlands, near beach; Middletown, both; Raritan, non-beach. We agree with the original codings of Atlantic Highlands and Highlands, but arrive at different codings for Middletown and Raritan as elucidated below:

[9] "Economics: National Ocean Watch (ENOW), Counties List." National Oceanographic and Atmospheric Administration. November 2017. https://coast.noaa.gov/data/digitalcoast/pdf/enow-counties-list.pdf.

    - Middletown—Snyder indicates that in the 1910s Middletown did not contain any parcels that were on the Jersey Shore proper (1969: 182). Consequently, we consider Middletown "near beach."
    - Raritan—Similar to Matawan Township, Fowler and Hall (2018a) code Raritan as "non-beach" even though it is on the "ocean" side of the Garden State Parkway. To determine how Raritan, which in 1916 did have a stretch of coastline, should be classified, we again explored whether any beaches existing in this municipality or any adjacent municipalities in 1916. We identified at least one major beach-based tourist attraction that existed in Raritan in 1916. In 1904, William Gehlhaus broke ground on the Keansburg Amusement Park "in hopes of creating a resort area" and built "a boardwalk and vacation bungalows" in addition to traditional amusements. By 1909, Gehlhaus also created a steamboat company to ferry tourists from New York City. While the land including the amusement park and surrounding amenities split from Raritan in 1917, forming Keansburg (Snyder 1969: 184), it was within the boundaries of Raritan for the 1916 shark attacks and presidential election. Given the presence of the Keansburg Amusement Park and surrounding attractions in 1916 as well as the extensive coastline of Raritan at the time and its proximity to the Keyport Yacht Club (see above discussion of Matawan Township), we code Raritan as a "beach" community.

  Consequently, three of the units in this consolidated observation we code as "near beach," and one we code as "beach." We reconcile this by coding the entire unit as "both," as it contains both beach and near beach communities.
- Lacey (Ocean County)—Achen and Bartels (2016) and Fowler and Hall (2018a) express disagreements over the coding of Lacey. Fowler and Hall report that they learned through private correspondence with Achen and Bartels that they initially coded Lacey as neither "beach" nor "near beach" as the majority of its land area (but not its population) is west of the Garden State Parkway (2018: 1431). Fowler and Hall retain this coding and note that how Lacey is coded does not affect the substantive results they present. However, Snyder indicates that until 1933, Lacey included a parcel of land directly on the Jersey Shore that would later become Island Beach (1969: 203). Consequently, we coded Lacey as "both."
- Matawan Township (Monmouth County)—Following the coding rule established by Achen and Bartels (2016) that towns west of the Garden State Parkway are to be excluded from the analysis, Fowler and Hall (2018a) code Matawan Township (now Aberdeen) as "non-beach." However, Matawan Township is on the "ocean" side of the Garden State Parkway (though better defined as the "northern" side of the Garden State Parkway, as the throughway changes directions once it approaches the northern end of the Jersey Shore); given that the Achen and Bartels (2016) Garden State Parkway boundary was initially established for only Ocean County, where the throughway ran strictly north-south, the location of Matawan Township relative to the ocean and the Garden State Parkway makes

this case ambiguous. We sought to resolve this ambiguity by investigating whether any beaches existed in that township or adjacent townships in 1916, as beaches in or proximate to the township would suggest the town should be classified as a beach or near beach town, respectively. Because the main beach in Matawan Township, Cliffwood Beach, was not developed until the 1920s, we determined that it should not be classified as a beach town.[10] However, Matawan Township was adjacent to Keyport, which contained notable ocean-related attractions including an entertainment venue built on the water named Pavilion Beach and the Keyport Yacht Club, both of which were constructed in the first decade of the 20th century (Jeandron 2003: 136-137). Consequently, we code Matawan Township as a "near beach" community, as it was adjacent to a town that was especially susceptible to economic harm due to the shark attacks but did not itself have significant exposure to ocean-related tourism.

- Middle/Stone Harbor (Cape May County)—As we depart from Fowler and Hall (2018a) by including a consolidated observation consisting of Middle and Stone Harbor rather than only including Middle, we need to reconcile the codings of coastal proximity. As Middle is a "near beach" community while Stone Harbor is a "beach" community, we code the unit as "both."
- Ocean (Ocean County)—Fowler and Hall (2018a) code Ocean as "near beach." However, Snyder indicates that until 1933, Ocean included a parcel of land directly on the Jersey Shore that would later become Island Beach (1969: 204). Consequently, we coded Ocean as "both."
- Upper (Cape May County)—Fowler and Hall (2018a) code Upper as "near beach." This coding aligns well with the town's modern-day boundaries, but in 1916 Upper contained much of modern-day Ocean City, which is part of the Jersey Shore and contains beachfront territory. Subsequently, we code Upper as "both."
- West Cape May (Cape May County)—Fowler and Hall (2018a) code West Cape May as "non-beach." While this municipality does not have any coastline of its own, it is separated from the ocean by only small slivers of Cape May and Cape May Point. As the Garden State Parkway terminates just north of West Cape May, it is not clearly "east" or "west" of the parkway, but it is on the "ocean" side of the parkway. Consequently, we code West Cape May as "near beach."

Section D: Empirical Analyses

[10] "Summers Past at Cliffwood Beach." *Matawan-Aberdeen Hometown Shopper*. Summer 1994, https://mapl.org/resources/archive/Town%20shopper%20local%20history/1994%20Summer%20-%20Cliffwood%20Beach.pdf

Our empirical analyses were pre-registered through the Open Science Framework.[11] Our pre-registered empirical analyses extend the analyses presented in Tables 3 and 4 of Fowler and Hall (2018a), which also substantively replicate Achen and Bartels (2004, 2016).[12] Specifically, we utilize the same model specifications in those analyses while adding an interaction between the binary indicator for whether a community is a "beach" community and a measure of the size of its hotel industry (as well as the constituent term for the hotel industry size measure). In our pre-registration plan, we stated that our measure of hotel industry size would be the raw counts of the number of hotels and hotel rooms in each county and town; however, after completing our analysis and circulating our manuscript for feedback, we realized that the resulting small coefficient estimates were difficult to display and interpret and that this measurement approach did not account for the size of the hotel industry relative to the size of the county or town itself (i.e., a similarly sized hotel industry may be more important to a small town than a large city).[13] Consequently, in a deviation from our pre-registration plan, we present in the main text analyses that measure the size of the hotel industry on a per capita basis (per 1,000 voters for counties, per 100 people for voters) that is rescaled to range from 0 to 1.[14,15] This measure then represents the size of the hotel industry relative to the size of the community, where 0 indicates a community with no reliance on tourism and 1 indicates the community with the highest reliance on tourism on a per capita basis in our dataset.[16]

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[11] Our pre-analysis plan is available here: https://osf.io/ydwex/?view_only=5d8e093c97c2449490b675f6ee8e8ab7. We kept the data files containing information on county- and town-level hotel locations and on the other variables used in the original analyses of Achen and Bartels (2016) and Fowler and Hall (2018) stored separately until our pre-analysis plan was submitted to OSF's registries repository. This data management protocol of collecting but not merging data for observational studies until after the pre-registration process has been completed has been used by several recently published studies (e.g., Graham et al. 2023a; Fraga and Miller 2022).

[12] Fowler and Hall (2018) note that their analyses which mirror those in Achen and Bartels (2016) are near-replications and utilize the same modeling strategies; thus, replicating Tables 3 and 4 in Fowler and Hall (2018) substantively replicates Achen and Bartels (2016).

[13] For example, while Avalon, Red Bank, and Wall each have 7 hotels, our analysis does not consider whether those 7 hotels might be more substantial contributors to the economies relative to the other two towns. As these towns vary substantially in terms of population (measured by the number of votes cast in the 1916 election), with Avalon holding only 76 persons while Red Bank and Wall holding 1460 and 759, respectively, we may have reason to believe that the 7 hotels in Avalon are more significant contributors to the town's economy than the same number of hotels are to Red Bank and Wall.

[14] While the number of voters in a locality is not equivalent to the number of persons living there, both Achen and Bartels (2004, 2016) and Fowler and Hall (2018) use turnout to approximate population in their analyses, and we follow their analyses by doing so in our own.

[15] Because many specifications utilize slightly different subsets of counties/towns, we rescale the per capita measures of hotels and hotel rooms for each governmental unit for each unique subset such that the measure takes on values from 0 to 1 within each model.

[16] To maintain fidelity to our pre-registration plan, we also present our originally planned analyses using the raw counts of hotels and hotel rooms in this Supplemental Materials

While our hotel industry size measures for each county and town are highly correlated (*r*>0.90 at the county- and town-level), they represent different concepts; while the relative concentration of hotels communicates information about the number of independent lodging businesses operating within a given county or town, the relative concentration of hotel rooms provides insight on the volume of tourist activity those hotels and the larger community typically service. Thus, to assess the robustness of our results across concepts, we pre-registered that we would present results using both measures of community exposure to the hotel industry in our main analysis.

The generic formula for our linear regression models at the county level is as follows:

$$\mathrm{1916vote_i} = \alpha + \beta_1\mathrm{beach_i} + \beta_2\mathrm{hotel\_industry_i} + \beta_3\mathrm{beach\_hotel\_industry_i} + \beta_4\mathrm{1912vote_i} + \beta_5\mathrm{machine_i} + \epsilon_\mathrm{i}$$

$\mathrm{hotel\_industry_i}$ represents our measure of community exposure to the hotel industry as the rescaled number of hotels or the number of hotel rooms per capita across models. The key coefficient of interest is β_3, which represents the multiplicative effect of the size of the hotel industry in beach counties. The theory of blind retrospection anticipates this coefficient to be negative and statistically significant, such that the electoral penalty of shark attacks among beach counties is stronger for those counties with larger hotel industries.

The generic formula for our linear regression models at the town level is as follows:[17]

$$\mathrm{vot}echange_i = \alpha + \beta_1\mathrm{beach}_i + \beta_2\mathrm{hotel\_industry_i} + \beta_3\mathrm{beach\_hotel\_industry_i} + \epsilon_i$$

The key coefficient of interest is again is β_3, which represents the multiplicative effect of the size of the hotel industry in beach towns. The theory of blind retrospection again anticipates this coefficient to be negative and statistically significant, such that the electoral penalty of shark attacks among beach towns is stronger for those towns with larger hotel industries.

Section D.1: Visual Representations of Marginal Effects

When utilizing interactions in a regression model, best practices dictate that researchers move beyond presenting only model coefficients and also provide visual representations of the marginal effects for the variable(s) of interest, as those effects rely on both the constituent term *and* the interaction term in the model and therefore cannot be discerned directly from the coefficients (Brambor et al. 2006). As our main analyses include 24 different models, each of which manifests substantively similar results, we provide here marginal effects plots for a subset of these models rather than plots for *each* of those 24 models.

Figure SI.3 presents the marginal effects plots for Model 1a and Model 1b in Tables 1 and 2, which are the model specifications analogous to those originally used by Achen and Bartels (2004, 2016). For the country-level models, the slopes of the lines are close to flat and the 95% confidence intervals include zero across the full support of the hotel industry size measures, suggesting there is no evidence that Wilson experienced an electoral penalty in beach counties that

document. These results, presented in Tables SI.1 and SI.2, are substantively similar to those in the main paper.

[17] Following Fowler and Hall (2018), Model 6 in Table 4 also includes county fixed effects.

grew larger as the size of the hotel industry in those communities grew larger. For the town-level models, the intercept for beach towns is distinguishably lower than for non-beach towns, echoing Achen and Bartels' original finding that beach towns penalized Wilson at the ballot box, but the slopes for beach and non-beach towns are not only remarkably similar, but are both also positive, indicating that beach and non-beach towns with more hotels were more supportive of Wilson; these results again provide no evidence for the expected heterogeneous effect whereby beach communities expressed lower levels of support for Wilson as their hotel industries grew in size.

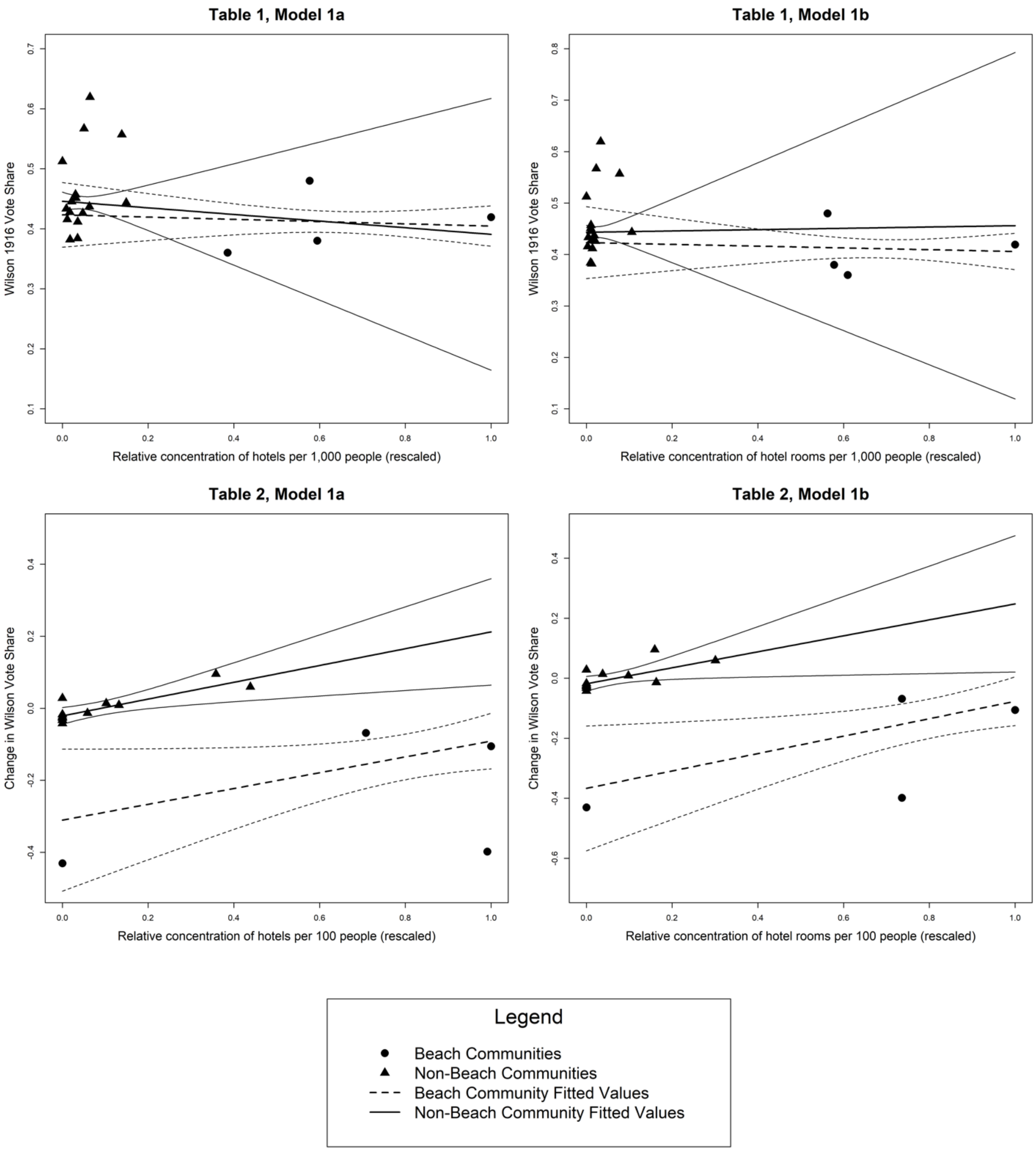

Figure SI.3: Marginal Effect of Hotel Industry Size on Electoral Support for Wilson. The four panels of this figure present the fitted values for communities designated as "beach" communities (solid line) or communities not designated as such (dotted line) using the regression models indicated in the title of each panel. Thinner lines represent 95% confidence intervals around these fitted values. Circles and triangles indicate the observed values of hotel industry size and electoral support for Wilson among beach communities and communities not designated as such, respectively.

Section D.2: Alternative Operationalization with Raw Counts of Hotels and Hotel Rooms

As we state at the beginning of Section D, our pre-registration stated that our main analyses would use as measures of hotel industry size the counts of hotels and hotel rooms in each governmental unit, but that after we conducted our analyses (which we could not do before filing the pre-registration) and circulating the manuscript for feedback, we realized that the small coefficient estimates were difficult to interpret and that the specifications did not account for the size of the hotel industry relative to the size of the governmental unit itself. Consequently, we deviate from this plan in this manuscript by measuring hotel industry size on a per capita basis (number of hotels and hotel rooms per 1,000 and 100 people, respectively) and rescaling this per capita measure to range between 0 and 1.

To maintain fidelity to our pre-registration, we also present here our analyses that use the original counts of hotels and hotel rooms. As with our results presented in Tables 1 and 2 of the main paper, the corresponding results in Tables SI.1 and SI.2 below also yield null results for the expectation that beach counties and towns that were more dependent on tourism punished Wilson electorally.

Table SI.1: Interactive Effect of Hotel Industry Size and Community Type (County-Level, Counts of Hotels and Hotel Rooms)

	Model 1		**Model 2**		**Model 3**		**Model 4**		**Model 5**		**Model 6**	
	(a)	**(b)**	**(a)**	**(b)**	**(a)**	**(b)**	**(a)**	**(b)**	**(a)**	**(b)**	**(a)**	**(b)**
Beach County	-0.03	-0.03	-0.04	-0.03					-0.03	-0.02	-0.02	-0.02
	(0.02)	(0.02)	(0.03)	(0.03)					(0.03)	(0.04)	(0.04)	(0.04)
Attack County					-0.03	-0.03						
					(0.03)	(0.03)						
Coastal County							-0.01	-0.01				
							(0.01)	(0.01)				
Num. Hotels	-0.00		0.00		-0.00		-0.00		-0.00		-0.00	
	(0.00)		(0.00)		(0.00)		(0.00)		(0.00)		(0.00)	
Beach:Num. Hotels	0.00		-0.00						0.00		0.00	
	(0.00)		(0.00)						(0.00)		(0.00)	
Attack:Num. Hotels					0.00							
					(0.00)							
Coastal:Num. Hotels							0.00					
							(0.00)					
Num. Hotel Rooms		0.00		0.00		-0.00		-0.00		0.00		0.00
		(0.00)		(0.00)		(0.00)		(0.00)		(0.00)		(0.00)
Beach:Num. Hotel Rooms		-0.00		-0.00						-0.00		-0.00
		(0.00)		(0.00)						(0.00)		(0.00)
Attack:Num. Hotel Rooms						0.00						
						(0.00)						
Coastal:Num. Hotel Rooms								0.00				
								(0.00)				

	Model 1		**Model 2**		**Model 3**		**Model 4**		**Model 5**		**Model 6**	
	(a)	**(b)**	**(a)**	**(b)**	**(a)**	**(b)**	**(a)**	**(b)**	**(a)**	**(b)**	**(a)**	**(b)**
Machine (Achen and Bartels)	-0.06 *	-0.06 *	-0.04 *	-0.04 *	-0.06 *	-0.06 *	-0.05 *	-0.05 *				
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)				
Machine (Mayhew)											0.00	0.00
											(0.02)	(0.02)
Wilson 1912 Vote Share	0.95 *	0.95 *	0.86 *	0.88 *	0.95 *	0.95 *	0.95 *	0.95 *	0.92 *	0.92 *	0.92 *	0.92 *
	(0.07)	(0.07)	(0.09)	(0.08)	(0.07)	(0.07)	(0.07)	(0.07)	(0.11)	(0.11)	(0.11)	(0.11)
Intercept	0.05	0.04	0.08	0.07	0.05	0.05	0.05	0.05	0.05	0.05	0.05	0.04
	(0.03)	(0.03)	(0.04)	(0.04)	(0.03)	(0.03)	(0.04)	(0.04)	(0.05)	(0.05)	(0.06)	(0.05)
Include Essex County			Y	Y								
R^2	0.95	0.95	0.90	0.91	0.94	0.94	0.94	0.94	0.86	0.86	0.86	0.86
Num. obs.	20	20	21	21	20	20	20	20	20	20	20	20

* denotes p<0.05. The outcome in each model is Wilson's two-party vote share in 1916. (a) and (b) models denote usage of number of hotels or number of rooms to measure hotel industry size, respectively. Models 1-6 correspond with those in Table 3 of Fowler and Hall (2018).

Table SI.2: Interactive Effect of Hotel Industry Size and Community Type (Town-Level, Counts of Hotels and Hotel Rooms)

	Model 1		Model 2		Model 3		Model 4		Model 5		Model 6	
	(a)	(b)	(a)	(b)	(a)	(b)	(a)	(b)	(a)	(b)	(a)	(b)
Beach Town	-0.28 *	-0.29 *	-0.08	-0.12	-0.33 *	-0.38 *	-0.09	-0.12	-0.07	-0.07	0.02	0.01
	(0.09)	(0.09)	(0.11)	(0.12)	(0.13)	(0.12)	(0.11)	(0.12)	(0.06)	(0.06)	(0.01)	(0.01)
Num. Hotels	0.01	0.00	0.01	0.00	0.02	0.00	0.02	0.00	0.00	0.00	0.00	0.00
	(0.00)	(0.00)	(0.01)	(0.00)	(0.01)	(0.00)	(0.01)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Beach Town:Num. Hotels	0.02		-0.00		0.06		-0.01		0.01		-0.00	
	(0.01)		(0.02)		(0.03)		(0.02)		(0.01)		(0.00)	
Beach Town:Num. Hotel Rooms		0.00		0.00		0.00		-0.00		0.00		-0.00
		(0.00)		(0.00)		(0.00)		(0.00)		(0.00)		(0.00)
Intercept	-0.02	-0.01	-0.02	-0.01	-0.02	-0.02	-0.02	-0.02	-0.01	-0.00	0.01	0.00
	(0.01)	(0.01)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.01)	(0.02)
Include Sea Side Park			Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Drop cases of boundary changes					Y	Y	Y	Y	Y	Y	Y	Y
Merge Long Beach and Beach Haven							Y	Y	Y	Y	Y	Y
Include towns with total vote change >25%									Y	Y	Y	Y
Include all beach counties											Y	Y
County fixed effects											Y	Y
R^2	0.64	0.62	0.18	0.17	0.54	0.61	0.26	0.26	0.12	0.13	0.09	0.08
Num. obs.	15	15	16	16	13	13	14	14	19	19	71	71

* denotes p<0.05. The outcome in each model is the change in Wilson's vote share between 1916 and 1912. (a) and (b) models denote usage of number of hotels or number of rooms to measure hotel industry size, respectively. Models 1-6 correspond with those in Table 4 of Fowler and Hall (2018).

Section D.3: Potential Spillover Effects

In our main analyses, we measure the size of the hotel industry in each locality using the rescaled count of hotels and hotel rooms per capita within that locality. However, as discussed in Section B.2 of the Supplemental Information, the economic effects associated with those hotels are not necessarily confined to the localities in which they reside—put differently, those economic effects could have spilled over to neighboring localities. To assess the potential for spillover effects, we repeat our town-level analyses by adding to each town's counts of hotels and hotel rooms the numbers of hotels and hotel rooms in bordering towns, then rescaling those counts on a per capita basis (per 100 people) for the residents of the town of interest.[18]

The results from this analysis are presented in Table SI.3. With the exception of Models 5(a) and 5(b), the results are substantively similar to those in Table 2 in the main paper However, for Models 5(a) and 5(b), the interaction terms are negatively signed and statistically distinguishable as expected. To probe the robustness of these results, we used the `interflex` package in `R` to assess the plausibility of the linear interaction effect assumption (Hainmueller et al. 2019; see Section D.5 for more details). In both cases, the underlying data was not consistent with the linear interaction effect assumption. We subsequently reassessed the interaction effect using a more flexible kernel estimator, which accounts for nonlinear effects, and the interaction effects from the kernel estimator were no longer statistically distinguishable.[19] Given that only two of the twelve models yielded statistically distinguishable interaction terms and that the interaction effects for those two models were not robust to further scrutiny, accounting for potential spillover effects does not yield substantively different, robust results than those obtained by accounting for only the hotels and hotel rooms within a given town.

Table SI.3: Interactive Effect of Hotel Industry Size and Community Type Accounting for Potential Spillover (Town-Level)

	Model 1		Model 2		Model 3		Model 4		Model 5		Model 6	
	(a)	(b)	(a)	(b)	(a)	(b)	(a)	(b)	(a)	(b)	(a)	(b)
Beach Town	-0.12	-0.20	0.01	0.07	0.08	0.08	0.06	0.04	0.09	0.08	0.01	0.01
	(0.07)	(0.13)	(0.08)	(0.10)	(0.10)	(0.12)	(0.10)	(0.10)	(0.05)	(0.05)	(0.01)	(0.01)
Hotels Per Capita (Rescaled)	0.11		0.11		0.11		0.11		0.10		-0.07	
	(0.07)		(0.09)		(0.10)		(0.09)		(0.08)		(0.10)	
Beach Town:Hotels Per Capita (Rescaled)	-0.09		-0.31		-0.39		-0.28		-0.33 *		-0.09	
	(0.18)		(0.22)		(0.25)		(0.18)		(0.13)		(0.16)	
Hotel Rooms Per Capita (Rescaled)		0.12		0.12		0.12		0.12		0.11		-0.09
		(0.08)		(0.11)		(0.12)		(0.11)		(0.10)		(0.12)
Beach Town:Hotel Rooms Per Capita (Rescaled)		0.01		-0.39		-0.39		-0.26		-0.31 *		-0.07
		(0.25)		(0.26)		(0.29)		(0.19)		(0.14)		(0.19)

[18] We do not perform a similar spillover analysis at the county-level because New Jersey's counties were sprawling in size such that tourists would be unlikely to have routinely traveled between counties with any regularity in the course of a single vacation.

[19] Our exploration of the linear interaction effect assumption and alternative estimation approach are not presented here but are available in our replication code.

	Model 1		Model 2		Model 3		Model 4		Model 5		Model 6	
	(a)	(b)	(a)	(b)	(a)	(b)	(a)	(b)	(a)	(b)	(a)	(b)
Intercept	-0.02	-0.02	-0.02	-0.02	-0.02	-0.02	-0.02	-0.02	-0.01	-0.01	-0.01	-0.01
	(0.02)	(0.02)	(0.02)	(0.02)	(0.03)	(0.03)	(0.02)	(0.02)	(0.02)	(0.02)	(0.01)	(0.01)
Include Sea Side Park			Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Drop cases of boundary changes					Y	Y	Y	Y	Y	Y	Y	Y
Merge Long Beach and Beach Haven							Y	Y	Y	Y	Y	Y
Include towns with total vote change >25%									Y	Y	Y	Y
Include all beach counties											Y	Y
County fixed effects											Y	Y
R^2	0.51	0.47	0.25	0.21	0.25	0.21	0.25	0.21	0.29	0.27	0.09	0.09
Num. obs.	15	14	16	15	13	13	14	14	19	19	71	71

* denotes $p<0.05$. The outcome in each model is the change in Wilson's vote share between 1916 and 1912. (a) and (b) models denote usage of the rescaled number of hotels or number of rooms per capita to measure hotel industry size, respectively. Measures of hotels and hotel rooms per capita for each observation include both the numbers of hotels and hotel rooms within each town and the numbers of hotels and hotel rooms in all bordering towns. Models 1-6 correspond with those in Table 4 of Fowler and Hall (2018).

Section D.4: Influential Observations & Alternative Estimation Methods

As we noted in our pre-registration document, given the skewed distributions of our hotel industry measures (see Figures SI.1 and SI.2), we anticipated that one or more of our data points may exhibit high leverage. While storing our hotel industry measures separately from the other data necessary to conduct our analyses prevented us from knowing ex ante if any data points with high leverage would also exhibit an outlier value for their outcome values, we noted the possibility that our models may contain influential observations and established protocols for addressing such observations.

First, we committed to assess whether any of our observations are associated with abnormally large Cook's distance values and report the summary statistics of those values. Table SI.4 presents the number of observations in the corresponding model in the main paper with Cook's distance values that exceed the cutoff of n-k-1 (Chatterjee and Hadi 1988). Most models include at least one observation that exceeds its respective threshold.

Table SI.4 Cook's Distance Values for Main Analyses

Model	Cook's Distance Cutoff (4/(n-k-1)	Number of Influential Observations	Largest Cook's Distance Value	Smallest Cook's Distance Value Above Cutoff
Table 3, Model 1a	0.29	0	0.23	-

Table 3, Model 1b	0.29	1	3.30	3.30
Table 3, Model 2a	0.27	1	0.63	0.63
Table 3, Model 2b	0.27	1	0.61	0.61
Table 3, Model 3a	0.29	1	0.39	0.39
Table 3, Model 3b	0.29	0	0.24	-
Table 3, Model 4a	0.29	0	0.22	-
Table 3, Model 4b	0.29	0	0.24	-
Table 3, Model 5a	0.27	0	0.24	-
Table 3, Model 5b	0.27	1	0.28	0.28
Table 3, Model 6a	0.29	1	0.42	0.42
Table 3, Model 6b	0.29	2	0.39	0.39
Table 4, Model 1a	0.36	2	3.94	0.92
Table 4, Model 1b	0.36	3	1.35	0.71
Table 4, Model 2a	0.33	3	2.54	0.63
Table 4, Model 2b	0.33	3	2.63	0.95
Table 4, Model 3a	0.44	2	7.70	1.09
Table 4, Model 3b	0.44	4	5.05	0.50
Table 4, Model 4a	0.40	3	1.74	0.46
Table 4, Model 4b	0.40	4	1.79	0.81
Table 4, Model 5a	0.27	3	1.99	0.0.27
Table 4, Model 5b	0.27	2	4.16	0.40
Table 4, Model 6a	0.06	7	1.48	0.13
Table 4, Model 6b	0.06	6	0.97	0.09

Second, when influential observations were detected, we committed to repeat our analyses with robust regression (Huber estimator), which down-weights the contribution of influential observations to the model estimates.[20] In all but two cases, repeating our analyses with robust regression does not yield model parameters that support conclusions substantively different from those presented in Tables 1 and 2 (not presented here but available in our replication code); we discuss these two exceptions and why they do not alter our overall conclusions below:

- Town-level (Table 4) Model 2(a)—When using robust regression to account for influential observations, the coefficient on the interaction term is negative and statistically distinguishable from zero, and the predicted values from this model indicate an effect consistent with Achen and Bartels' blind retrospection expectation; moving from 0 to 1 on the rescaled measure of hotels per capita is associated with a statistically distinguishable decrease in the predicted change in vote share for Wilson from approximately -0.01 to -0.10 among beach towns, while the same movement in the hotel industry measure among non-beach towns is associated with a distinguishable shift from -0.02 to 0.21. However, when using the `interflex` package in `R` to generate diagnostic plots, we detected both nonlinearity in the interaction effect and lack of common support beyond the midpoint of the hotel industry size measure. When we reassess this interactive effect using a kernel estimator to account for the nonlinearity, we obtain null results. Further, the corresponding analysis that uses hotel rooms rather than hotels as the basis of the hotel industry size measure yields null results, casting further doubt on this finding.
- Town-level (Table 4) Model 4(a)—When using robust regression to account for influential observations, the coefficient on the interaction term is negative and statistically distinguishable from zero at the $p<0.10$ level. However, the predicted values from this model indicate that the interactive effect exists for non-beach, rather than beach, towns; while moving from 0 to 1 on the rescaled measure of hotels per capita is associated with a small, statistically indistinguishable *increase* in predicted change in vote share for Wilson from -0.08 to -0.04 among beach towns, it is associated with a statistically distinguishable increase in predicted change in vote share among non-beach towns from-0.02 to 0.21. Further, when using the `interflex` package in `R` to generate diagnostic plots, we detected both nonlinearity in the interaction effect and lack of common support beyond the midpoint of the hotel industry size measure. When we reassess this interactive effect using a kernel estimator to account for the nonlinearity, we obtain null results. Additionally, the corresponding analysis that uses hotel rooms rather than hotels as the basis of the hotel industry size measure yields null results. Together, this evidence does not lend support for the blind retrospection expectation.

Section D.5: Interaction Effect Assumptions & Alternative Estimation Methods

[20] In our pre-registration, we committed to repeat our analyses when influential observations were present with both robust regression and logging our measures of hotel industry size. However, because the specifications we ultimately utilize in the final version of the manuscript use a rescaled measure of hotel industry size that ranges between 0 and 1, the distributions of these measures do not exhibit substantial skew and we forego logging these measures.

As in the case of influential observations, storing different parts of our data separately until after the pre-registration process was completed prevented us from assessing the plausibility ex ante of the linear interaction effect and common support assumptions (Hainmueller et al. 2019). However, we recognized that violations of these assumptions could manifest and committed to both assessing the plausibility of the assumptions for each model and, when we detected violations, employing alternative methods that accommodate effect heterogeneity.

For each model, we used the `interflex` package in `R` to assess the plausibility of the linear interaction effect and common support assumptions and, if we deemed the assumptions implausible, assessed the interaction effect using a kernel estimator, which accommodates violations of both assumptions. With the exceptions of Models 6a and 6b from Table 4, our investigation of these assumptions and alternative modeling approaches are not presented here but are available in our replication code.

Most models in Tables 1 and 2 of the main paper violate at least one of these assumptions, with violations of the common support assumption being the more consistent violation. For most sets of observations included in these models, counties or towns coded as "beach" had much larger values of hotel industry size than did "non-beach" communities, such that the largest value among non-beach communities was often smaller than the smallest value among beach communities. For instance, in the top panes of Figure SI.3, which present the marginal effects for Models 1a and 1b in Table 1, we lack support for the hotel industry size measure among non-beach counties for values of more than 0.2 for our hotel industry size measure and among beach counties for values of less than 0.4. Consequently, the marginal effects for these two groups calculated beyond those regions are extrapolated. Again, while models 1a-5b in Table 2 at the town-level have some degree of overlap because a small number of beach towns have no hotels, all beach towns with a hotel industry of non-zero size have larger hotel industries than all non-beach towns, meaning that we lack common support across beach and non-beach towns above a hotel industry size of zero (see the bottom panes of Figure SI.3 for examples).

Concerns about common support are minimized for Models 6a and 6b in Table 2, as there is common support for the hotel industry size measure across beach and near-beach towns for substantial ranges; for the hotel-based industry size measure, there is common support without excessive sparsity between data points from 0 to approximately 0.30 and for the hotel room-based industry measure there is common support from 0 to approximately 0.14—both of which include roughly 70% of all observations. Taken together with their relatively larger sample size, these two models offer the best opportunity to obtain precise estimates from observations that demonstrate substantial variation in hotel industry size, and we therefore present our investigation of how well they satisfy modeling assumptions and results obtained through alternative estimation approaches here.

Figure SI.4 presents the observations in Model 6a (left pane) and 6b (right pane) according to whether they are beach ("Treatment = 1") or non-beach ("Treatment = 0") towns and the values they take on for hotel industry size (x-axis) and change in vote share for Wilson (y-axis). The blue line represents the linear fit, and the red line represents a LOESS fit. The LOESS fits exhibit mild to moderate violations of the linear interaction effect assumption, and we note lack of common support without excessive data sparsity beyond 0.30 for the hotel-based measure and 0.14 for the hotel room-based measure.

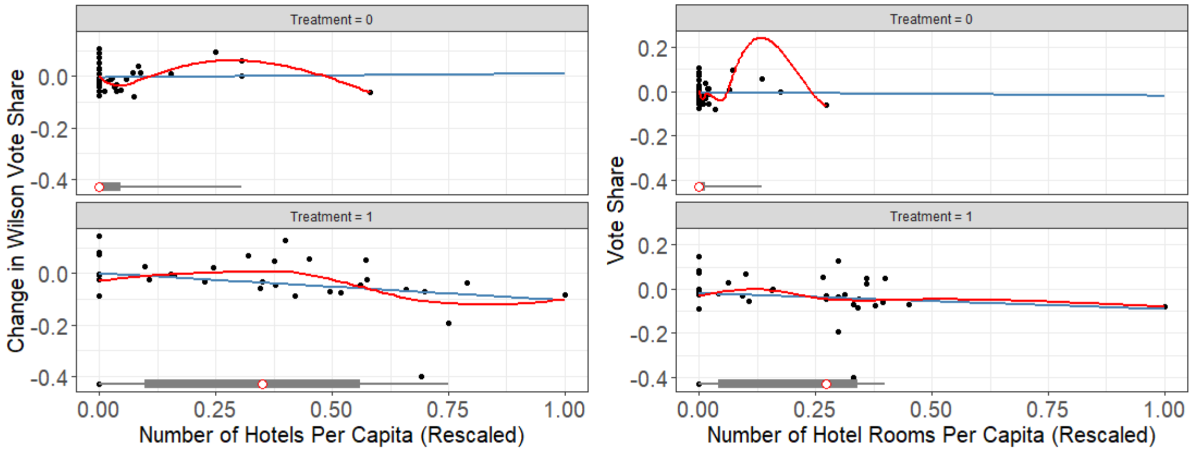

Figure SI.4: Diagnostic Interaction Plots for Models 6a and 6b from Table 2 Diagnostic plots generated using `interflex`.

When violations of the linear interaction effect and common support assumptions manifest, Hainmueller et al. (2019) suggest evaluating interaction effects using a semiparametric kernel estimator, which allows for flexibility in the effects across the range of the moderator and accounts for sparsity of observations in different regions of the moderator by expanding the confidence intervals at relevant points. In Figure SI.5, we present the graphical representations of these analyses for Models 6a (left pane) and 6b (right pane). These results do not provide evidence of a statistically distinguishable interaction effect, even for the range for which we have common support across beach and non-beach towns; rather the marginal effect of hotel industry size is consistently close to zero across the range of the moderator.

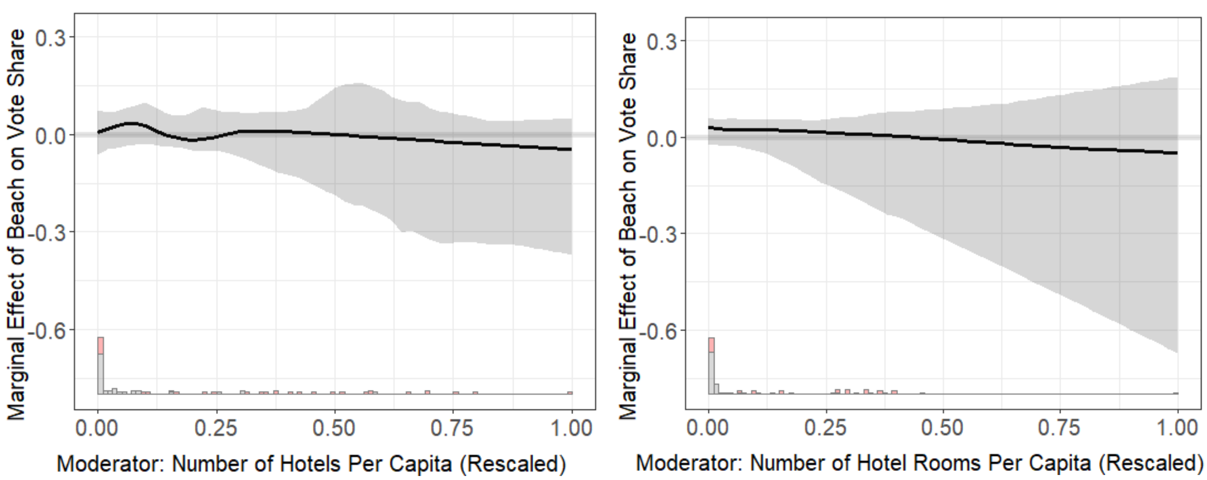

Figure SI.5: Kernel Estimation for Models 6a and 6b from Table 2 Kernel estimation conducted and plots generated using `interflex`.

In situations where researchers lack common support across the range of the moderator across groups when estimating an interaction effect, Liu et al. (n.d.) alternatively suggest trimming the data. Because the data underlying Models 6a and 6b include ranges of the hotel industry size measure with substantial common support, we also utilized the kernel estimator with only observations within the range of common support (i.e., values of hotel industry size that are less than the 70th percentile value). The results using the trimmed data, presented in Figure SI.6, again yield no evidence of a statistically distinguishable interaction effect; again, the marginal effect of hotel industry size is consistently close to zero across the range of the moderator.

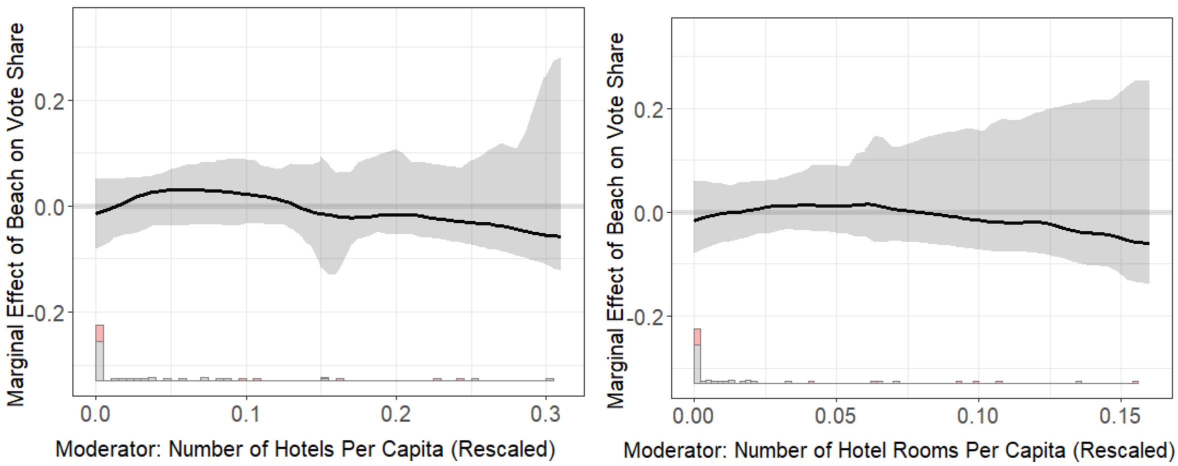

Figure SI.6: Kernel Estimation for Models 6a and 6b from Table 2 (Trimmed Data) Kernel estimation conducted and plots generated using `interflex`. Analyses include all observations with values for hotel industry size that are less than the 70th percentile value

Section D.6: Estimate Precision & Alternative Specifications

One challenge inherent in the analyses of the 1916 presidential election in New Jersey in Achen and Bartels (2016) and Fowler and Hall (2018), as well as those presented here, is the small sample size; in the county-level analyses, researchers are naturally constrained by the number of New Jersey counties (21), and in the town-level analyses researchers are constrained by the number of towns in Ocean County or across the four Jersey Shore counties (at most 19 and 71 with complete data, respectively). Consequently, precise estimates are difficult to obtain; as Fowler and Hall (n.d.) note, "[w]ith a small number of observations and with correlated shocks between beach and non-beach areas, the effective standard errors associated with these kinds of regressions are huge" as a matter of course, such that the most reasonable path to substantiating claims of an electoral penalty of shark attacks in the 1916 election is "if multiple independent sources of evidence all point in the same direction."

Our analyses in Tables 1 and 2 of our main paper do little to provide evidence in support of the original claims in Achen and Bartels (2016). However, it is possible that our inability to detect a heterogeneous effect of hotel industry size across beach and non-beach communities is a consequence of imprecise estimates rather than a true lack of an effect. While we cannot definitively eliminate this concern, we discuss here some inherent limitations of our analyses in Tables 1 and 2 that may inflate our standard errors and findings from supplemental analyses that address some of those limitations to reduce uncertainty in our estimates. In doing so, we focus our discussion primarily on the final specifications in Table 2 (Models 6a and 6b), which examine all towns in the four Jersey Shore counties; we do so because these specifications utilize the largest sample size across specifications, thus offering the most precise estimates.

That these supplemental analyses cast further doubt on the existence of a true effect in excess of a decrease in vote share for Wilson from 1912 to 1916 of 0.05, taken together with the findings of Fowler and Hall (2018) which already call into question the original findings, we argue that our analyses meaningfully contribute to extant investigations of the effect of shark attacks on the 1916 presidential election in New Jersey by adding further evidence in favor of a null effect.

Precision of Analyses in the Main Paper

The standard errors for our estimates in Tables 1 and 2 of the main paper are fairly large relative to the magnitude of the coefficients; for instance, the interaction term in Model 6b of Table 2 has a coefficient of -0.09 and standard error of 0.11, which is indicative of a 95% confidence interval of (-0.31, 0.13). That this confidence interval contains not only all plausible effect sizes, but also many implausible effect sizes, may call into question our ability to detect a true heterogeneous effect. However, as we discuss earlier in Supplemental Information Section D, we know from post-estimation diagnostics that the models in Tables 1 and 2 have undesirable properties that can inflate the standard errors. First, as we discuss in Supplemental Information Section D.4, most models have at least one influential observation, which can distort coefficient estimates and standard errors. Second, as we discuss in Supplemental Information Section D.5, we lack common support for most of the range of our hotel industry size measures, which induces extrapolation and model fragility. While our preregistered approach to this study precluded us from identifying these problems ex ante (as we could not merge the data sets until the preregistration was complete), we were cognizant that they could be potential problems and identified ways to address them (e.g., for interaction effects, utilizing alternative methods recommended by Hainmueller et al. 2019). Given these known issues, the standard errors presented in Tables 1 and 2 likely overstate the degree of uncertainty the underlying data facilitates.

Alternative Specifications

To try to address these underlying issues with the specifications in the main paper, we present here results from two different types of alternative specifications: those from the kernel estimations we present in Figures SI.5 and SI.6, and a separate set of results from simplified specifications that binarize the presence of the hotel industry in beach and non-beach communities.

1. First, as we explain in Supplemental Information Section D.5, our implementation of a semiparametric kernel estimator from Hainmueller et al. (2019) allows for flexibility in the effects across the range of the moderator and accounts for sparsity of observations in the different regions of the moderator by expanding the confidence intervals at relevant points. Importantly, the flexibility of the confidence intervals provides us with a sense of the precision of our estimate of the marginal effect of being a beach community across values of hotel industry size that account for the lack of common support. In Table SI.5, we present in tabular format marginal effects obtained from these implementations that we present visually in Figure SI.5 for the full data, and in Table SI.6 we provide a similar presentation of the marginal effects obtained when using the trimmed data that we present visually in Figure SI.6.

While the 95% confidence intervals at high values of hotel industry size are very large when utilizing the full data (e.g., for the left panel of Figure SI.5, the 95% confidence interval for the marginal effect when the rescaled number of hotels per capita is at its maximum value of 1 is (-0.37, 0.05)), they are more modestly sized at lower levels of hotel industry size; for instance, at a rescaled value of hotels per capita of 0.20 (approximately 2 hotels per capita), the estimated marginal effect of being a beach community is -0.02 with a 95% confidence interval of (-0.05, 0.07). While this confidence interval includes many of the estimated effect sizes of being a beach community in Achen and Bartels (2016) and Fowler and Hall (2018), it does not contain other plausible negative effect sizes that could manifest given the theory of blind retrospection (e.g., comparing beach and non-beach communities with approximately 2 hotels, a vote share penalty of -0.06 in the wake of shark attacks would not be unreasonable were it to manifest). Additionally, the 95% confidence intervals from the analyses using the trimmed data (Figure SI.6 and Table SI.6) also exhibit more modestly-sized confidence intervals for the marginal effect of being a beach community; for instance focusing on a rescaled value of hotels per capita of 0.20, the confidence interval for the marginal effect is -0.02 with a 95% confidence interval of (-0.05, 0.11), which again does not contain some plausible negative effect sizes.

Table SI.5: Point Estimates and 95% Confidence Intervals from Kernel Estimation of Specifications Used for Models 6a and 6b from Table 2 (Full Data)

Moderator (Hotel Industry Measure)	Moderator Value (Hotel Industry Measure Value)	Point Estimate	95% Confidence Interval
Number of Hotels Per Capita	0.00	0.01	(-0.06, 0.07)
Number of Hotels Per Capita	0.10	0.03	(-0.03, 0.10)
Number of Hotels Per Capita	0.20	-0.02	(-0.05, 0.07)
Number of Hotels Per Capita	0.30	0.01	(-0.07, 0.07)
Number of Hotels Per Capita	0.40	0.01	(-0.12, 0.07)
Number of Hotels Per Capita	0.50	0.00	(-0.19, 0.14)
Number of Hotels Per Capita	0.60	-0.01	(-0.25, 0.14)
Number of Hotels Per Capita	0.70	-0.02	(-0.32, 0.08)
Number of Hotels Per Capita	0.80	-0.03	(-0.33, 0.05)
Number of Hotels Per Capita	0.90	-0.04	(-0.34, 0.04)
Number of Hotels Per Capita	1.00	-0.05	(-0.37, 0.05)

Number of Hotel Rooms Per Capita	0.00	0.03	(-0.03, 0.06)
Number of Hotel Rooms Per Capita	0.10	0.02	(-0.04, 0.06)
Number of Hotel Rooms Per Capita	0.20	0.02	(-0.11, 0.05)
Number of Hotel Rooms Per Capita	0.30	0.01	(-0.18, 0.06)
Number of Hotel Rooms Per Capita	0.40	0.00	(-0.25, 0.08)
Number of Hotel Rooms Per Capita	0.50	-0.01	(-0.32, 0.09)
Number of Hotel Rooms Per Capita	0.60	-0.02	(-0.39, 0.10)
Number of Hotel Rooms Per Capita	0.70	-0.03	(-0.45, 0.12)
Number of Hotel Rooms Per Capita	0.80	-0.03	(-0.53, 0.14)
Number of Hotel Rooms Per Capita	0.90	-0.04	(-0.60, 0.17)
Number of Hotel Rooms Per Capita	1.00	-0.05	(-0.67, 0.19)

This table reports the point estimates and 95% confidence intervals for the kernel estimation analyses presented visually in Figure SI.5 for the specified values of the specified moderators.

Table SI.6: Point Estimates and 95% Confidence Intervals from Kernel Estimation of Specifications Used for Models 6a and 6b from Table 2 (Trimmed Data)

Moderator (Hotel Industry Measure)	Moderator Value (Hotel Industry Measure Value)	Point Estimate	95% Confidence Interval
Number of Hotels Per Capita	0.00	-0.01	(-0.08, 0.05)
Number of Hotels Per Capita	0.05	0.03	(-0.04, 0.07)
Number of Hotels Per Capita	0.10	0.02	(-0.03, 0.09)
Number of Hotels Per Capita	0.15	-0.01	(-0.12, 0.09)
Number of Hotels Per Capita	0.20	-0.02	(-0.05, 0.11)
Number of Hotels Per Capita	0.25	-0.03	(-0.07, 0.09)
Number of Hotels Per Capita	0.30	-0.05	(-0.11, 0.25)
Number of Hotel Rooms Per Capita	0.00	-0.02	(-0.08, 0.06)

Number of Hotel Rooms Per Capita	0.03	0.01	(-0.04, 0.07)
Number of Hotel Rooms Per Capita	0.06	0.02	(-0.05, 0.12)
Number of Hotel Rooms Per Capita	0.09	-0.01	(-0.06, 0.15)
Number of Hotel Rooms Per Capita	0.12	-0.02	(-0.08, 0.19)
Number of Hotel Rooms Per Capita	0.15	-0.05	(-0.12, 0.23)

This table reports the point estimates and 95% confidence intervals for the kernel estimation analyses presented visually in Figure SI.6 for the specified values of the specified moderators.

2. Second, we also repeat our analyses in Table 2 of the main paper by replacing our continuous measures of hotel industry size with a binary indicator of hotel industry presence (i.e., whether a town had at least one hotel). In doing so, we place less strenuous demands on the relatively small sample size and absolve the issue of lack of common support, as these models only compare the presence or absence of hotels in beach and non-beach communities rather than trying to estimate the effect of hotel industry size across the full range of the moderator. While this approach eliminates the granularity provided by the continuous measure of hotel industry size, the simplified measurement facilitates more precise estimates that can provide at least a coarse sense of whether communities with hotel industry presence are differentially affected by shark attacks given their beach or non-beach status.

   This alternative approach is presented in Supplemental Information Table SI.7. While the point estimates for Models 1-4 are suspiciously large, we caution readers against placing weight on them because of the composition of observations in the models given beach/non-beach status and presence/no presence of the hotel industry. Specifically, only one beach community, Surf City, lacks any hotels, and this community has the fifth-smallest population among all communities in our Jersey Shore towns analyses (19) and the largest change in vote for Wilson between the 1912 and 1916 elections (-0.43). Thus, our Beach Town term essentially functions as a fixed effect for Surf City—a small community that exhibited an abnormally large vote swing—and our interaction term is forced to counterbalance that fixed effect.

   Once we incorporate additional communities in our analyses in Models 5 and 6 such that Surf City is no longer the only beach community with no hotel presence, our estimates shrink considerably and approach the size of the effects obtained by Achen and Bartels (2016) and Fowler and Hall (2018). In Model 6, which utilizes the largest sample size available (71 communities), the coefficient estimate and standard error for the interaction term is 0.04 and 0.04, respectively, which is associated with a 95% confidence interval of (-0.04, 0.12)—an interval which does not include all plausible negative effect sizes (e.g., given the theory of blind retrospection, a negative interactive effect of a 0.05 decrease in vote share for Wilson would not be unreasonable to expect). Further, this and all other interaction term coefficients in Table SI.7 are positive, suggesting vote share for Wilson is higher in beach communities with hotel industry presence relative to other communities, which is counter to the theoretical expectations of blind retrospection.

In addition to the model estimates provided in Table SI.7, we also provide in Table SI.8 the estimates and 95% confidence intervals for the generalized linear hypothesis tests based on the coefficient estimates from Model 6 comparing all possible combinations of beach/non-beach status and presence/no presence of the hotel industry. The estimates from these generalized linear hypotheses are generally small, and the corresponding 95% confidence intervals typically do not include all plausible effects.

Table SI.7: Interactive Effect of Hotel Industry Presence and Community Type (Town-Level)

	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6
Beach Town	-0.41 *	-0.41 *	-0.41 *	-0.41 *	-0.05	-0.03
	(0.12)	(0.17)	(0.17)	(0.17)	(0.09)	(0.04)
Any Hotels	0.04	0.04	0.07	0.07	0.04	-0.02
	(0.02)	(0.03)	(0.04)	(0.04)	(0.03)	(0.02)
Beach Town:Any Hotels	0.28 *	0.35	0.36	0.33	0.04	0.04
	(0.12)	(0.17)	(0.18)	(0.17)	(0.10)	(0.04)
Intercept	-0.02	-0.02	-0.02	-0.02	-0.02	0.01
	(0.01)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
Include Sea Side Park		Y	Y	Y	Y	Y
Drop cases of boundary changes			Y	Y	Y	Y
Merge Long Beach and Beach Haven				Y	Y	Y
Include towns with total vote change >25%					Y	Y
Include all beach counties						Y
County fixed effects						Y
R^2	0.71	0.44	0.51	0.50	0.12	0.09
Num. obs.	15	16	13	14	19	71

***** denotes $p<0.05$. The outcome in each model is the change in Wilson's vote share between 1916 and 1912. Models 1-6 correspond with those in Table 4 of Fowler and Hall (2018).

Table SI.8: Generalized Linear Hypothesis Tests for All Combinations of Beach/Non-Beach Communities and Presence/No Presence of Hotel Industry from Table SI.6

Comparison Groups	Difference in Estimates	95% Confidence Interval for Difference
Non-beach, no hotels vs. non-beach, hotels	-0.02	(-0.05, 0.01)
Non-beach, no hotels vs. beach, no hotels	-0.03	(-0.11, 0.05)
Non-beach, no hotels vs. beach, hotels	-0.01	(-0.04, 0.02)
Non-beach, hotels, vs. beach, no hotels	0.01	(-0.07, 0.08)
Non-beach, hotels vs. beach, hotels	0.01	(-0.01, 0.03)

Beach, no hotels vs. beach, hotels	0.02	(-0.06, 0.09)

All generalized linear hypothesis tests conducted using `glht` in the `multcomp` `R` package and drawing on parameter estimates from Model 6 of Table SI.7.

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