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Egregiousness and Boycott Intensity

Evidence from the BP Deepwater Horizon *Oil Spill*

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RESOURCES FOR THE FUTURE

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Abstract

Consumer boycotts are triggered by egregious events, but the literature has not distinguished the level of egregiousness from consumers' preferences or disutility associated with a given level of egregiousness, nor has the literature studied how these two components of egregiousness affect boycott intensity. We provide a model of market-level boycotts that distinguishes the two egregiousness components. Consistent with the predictions of our model, the BP *Deepwater Horizon* oil spill triggered a market-level boycott effect only after its egregiousness exceeded a threshold level and boycott intensity then increased with its level of egregiousness, approximated by the officially reported daily amount of oil leaked into the ocean, and with consumers' disutility from egregiousness, approximated by an area's environmentalism and its proximity to the Gulf of Mexico. We also integrate media coverage within our models and find that the weekly intensity of media coverage affects boycott intensity.

Key Words: egregiousness, boycott, BP oil spill, environmentalism

JEL Classification Numbers: M3, Q50

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1. Introduction

Consumer boycotts are typically defined as public protests to stop the triggering events (e.g., Friedman 1985; Sen et al. 2001; John and Klein 2003). A large number of corporations have been subject to consumer boycotts (e.g., John and Klein 2003; Chavis and Leslie 2009), and a large proportion of consumers in various countries have reported participating in boycotts. For example, according to the World Values Survey, 16% of the survey subjects in the United States in the 1982 wave indicated participation in a boycott, and 26% of the survey subjects in the 2000 wave reported participation.[1] Furthermore, in 2005, a Global Market Insite poll found that 36% of 15,500 consumers polled in 17 countries boycotted at least one brand.[2]

Boycotts are triggered by egregious events. Intuitively, an egregious event triggers a boycott effect only if its level of egregiousness is above a threshold level, and boycott intensity increases with the level of egregiousness. However, the existing literature on consumer boycotts has largely ignored the fundamental role of the level of egregiousness in explaining the intensity of boycotts. John and Klein's (2003) formal mathematical model of boycott participation is an important exception. Their model predicts that boycott intensity increases with the "perceived egregiousness" of the offending event. Their model, however, does not distinguish the level of egregiousness from consumers' preference or disutility associated with a given level of egregiousness. As such, their term "perceived egregiousness" conflates preference (i.e.,

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[1] The survey was conducted by the European Values Study Group and World Values Survey Association. Data used to compute the statistics were downloaded from the website http://www.worldvaluessurvey.org.

[2] http://www.holmesreport.com/opinion-info/4471/More-Than-a-Third-of-Worlds-Consumers-Boycott-Brands.aspx (accessed September 12, 2013).

perception) with egregiousness level. Following John and Klein (2003), the literature that examines what motivates consumers to boycott has also used the term perceived egregiousness and has found, through survey methods, that consumers' perceived egregiousness of an offending event affects their likelihood of boycotting (e.g., Klein et al. 2002 and 2004; Hoffmann and Müller 2009; Balabanis 2013). In this paper, we offer a model of boycotts that distinguishes egregiousness level from consumer preference, and we empirically study how these two egregiousness components separately affect boycott intensity.

All consumers presumably agree that some negative events (e.g., the BP oil spill) are more egregious than others (e.g., a gasoline spill incident at a local retail station). That is, the egregiousness of negative events, like the quality of goods, can be vertically differentiated. In our model of consumer boycotts, all consumers would agree on the ranking of the egregiousness level, but their preference associated with a given level of egregiousness is heterogeneous. This separation of egregiousness level from consumer preferences is similar to the textbook economic model of vertical differentiation in quality, where consumers agree on the ranking of quality but differ in their valuations of a given level of quality. Following John and Klein (2003), we assume that a consumer boycotts if and only if her benefit from boycotting exceeds her cost of doing so. Our model predicts that an egregious event triggers a market-level boycott effect only if its egregiousness is above a threshold level, boycott intensity increases with egregiousness level, and boycott intensity is stronger in markets with greater negative consumer valuations of the offending event.

The BP oil spill was a single event, but the officially reported level of its egregiousness (i.e., the volume of oil leaking) varied over time. Suppose some consumers, on a day when the oil was still leaking, asked this question: What is the egregiousness of the oil spill? To these consumers, the egregiousness of the oil spill could be approximated by the official flow rate of the oil spill (i.e., the daily volume of oil leaking into the ocean), which increased significantly over the duration of the spill. Presumably, all consumers would agree that the oil spill became more egregious when the reported official flow rate increased, for example, from 1,000 barrels per day to 5,000 barrels per day. Therefore, the BP oil spill can be treated as a natural experiment in which we observe a series of events that differ in egregiousness but are otherwise identical. After the oil spill was stopped, its egregiousness level no longer changes over time—the fact that the BP oil spill is an environmental disaster does not change over time, but consumers' disutility from the oil spill becomes smaller over time. We would not be able to make this distinction without separating egregiousness level from consumer preference.

Disutility from the oil spill at a given level of egregiousness may differ among individuals. We use variations in market characteristics as proxies of variations in preference in regard to the egregiousness of the oil spill. The market characteristics that we consider are an area's geographic proximity to the Gulf of Mexico and an area's environmental orientation or preference, which is measured by donations to Green Party political committees from individuals living in that area. We expect the oil spill, at any given level of egregiousness, to have caused greater disutility in markets closer to the Gulf Coast region and in markets with stronger environmental preferences.

We measure an area's boycott intensity by decreases in BP brand's market share because the oil spill did not cause supply disruptions to BP refineries or BP-branded retail stations. We have access to a weekly panel data set of BP gasoline brand's volume share and price differentials in 257 large counties in the United States before, during, and after the oil spill.

We also study the impact of media coverage on boycott intensity, because media coverage may have affected consumers' awareness of the spill and because some consumers may have inferred the egregiousness level of the oil spill from the intensity of media coverage. We measure the amount of the media's coverage of the oil spill by the number of oil spill–related news articles and transcripts across the United States during and after the oil spill was capped.

We find strong evidence that drivers boycotted BP gasoline during the spill, but the boycott effect disappeared quickly after the spill was stopped, even though the BP oil spill was "by far the world's largest accidental release of oil into marine waters" (Robertson and Krauss 2010) and "the worst environmental disaster America has ever faced" (White House 2010). More important, we find evidence of a boycott effect only after the official flow rate was above a threshold level, and we find strong evidence that boycott intensity then increased as the official flow rate increased. We also find strong evidence that boycott intensity was stronger in the Gulf Coast region and in areas with stronger environmental preferences. We find that the weekly number of spill-related news items directly affected boycott intensity, though it had minimal impact on the positive relationship between boycott intensity and the level of egregiousness (i.e., the flow rate).

1.1. Relationship with Existing Literature

Our findings have important implications for two separate strands of boycott literature. The first is the growing literature that frequently uses survey methods to study why individuals may or may not participate in a particular boycott. Many of these studies do not consider the egregiousness of the triggering event (e.g., Kozinets and Handelman 1998; Sen et al. 2001;

Braunsberger and Buckler 2011), whereas another group of studies find that "perceived egregiousness" is one of the important motivating factors. Because each of the latter group of studies essentially focuses on a single event with a fixed level of egregiousness, this literature overlooks the fundamental role of varying egregiousness levels in explaining boycott participation. Variations in "perceived egregiousness" across survey subjects in this literature essentially reflect consumers' heterogeneous perceptions of (or disutility from) a fixed level of egregiousness.

The second strand is the literature that studies the impact of boycotts on firms' stock price (e.g., Pruitt and Friedman 1986; Pruitt et al. 1988; Koku et al. 1997) and the effectiveness of boycotts in influencing firms' behavior (e.g., Friedman 1985, 1991; Garrett 1987; King 2008). This literature does not measure or control for the egregiousness of the boycott-triggering events, but our findings imply that it is necessary to do so when studying the causal impact of boycotts on the offending firms. A more egregious event leads to a stronger boycott, which in turn is more likely to have a greater impact on the offending firm. Hence, whether a sample of boycotts has a significant impact on the offending firms depends critically on the egregiousness level of the boycott-triggering events. This helps explain why the findings of this literature are mixed and why, in the words of Baron (2001, 14), "the anecdotal and empirical evidence on the effect of boycotts on firms is sketchy and inconclusive."

Only a small number of papers have studied whether an egregious event has a boycott effect at the market level (e.g., Ashenfelter et al. 2007; Chavis and Leslie 2009; Hong et al. 2011). These papers each study a single event with a fixed level of egregiousness.[3] Because of data constraints, these studies generally use the intensity of media coverage to identify ex ante the time period during which a boycott may have taken place. In this paper, we are able to identify ex post the timing (e.g., the start, the peak, and the end of the oil spill) of the boycott because of the large number of cross-sectional units in our data set.

Our finding that the boycott effect disappeared quickly once the oil spill stopped suggests that firm-specific boycotts, unlike some societal boycotts (e.g., some Chinese consumers' boycott of Japanese goods), do not last long, thus supporting Ettenson et al.'s (2006, 6)

[3] In their study of the boycott of French wine in the United States because of the French opposition to the Iraq War in early 2003, Chavis and Leslie (2009) examine whether political preferences affect boycott intensity. Their proxies for political preferences are cities (e.g., Boston versus Houston), which is similar in spirit to our proxies for horizontal egregiousness. Their data cover only four cities, and they find that political preferences did not affect boycott participation.

conclusion that “even in high-profile, company-specific boycotts, drops in sales—although sometimes substantial—are typically short-lived.”

Examining the BP boycott also helps us better understand consumers’ specific purposes for boycotting at the market level. Most boycotts are thought to have the instrumental purpose of stopping the egregious events or causing firms to change their behavior.[4] Some studies find that the instrumental purpose is the main motivation (e.g., Klein et al. 2002); others find that both instrumental and noninstrumental purposes matter (e.g., Kozinets and Handelman 1998; Braunsberger and Buckler 2011). We argue that the BP boycott lacked a clear instrumental purpose, as it focused on expressing anger and punishing BP rather than encouraging BP to stop the spill, as BP was already under extensive regulatory pressure to do this. Thus our finding of a boycott effect is strong evidence of a noninstrumental boycotting purpose.

Our paper is also related to two strands of literature that deal with consumers’ private purchase behavior. A large body of literature studies whether and how individual consumers, in the absence of a public protest, engage in private boycotting (i.e., respond privately to firms’ behavior that can be perceived as offending). For example, Anderson and Simester (2008, 2010) used field experiments, implemented through catalog publishing companies, to study how individual consumers respond to firms’ pricing strategies that can be perceived as unfair. Their method of field experiments is not practical for studying boycotts that are public protests against the offending firms. We use the BP oil spill as a natural experiment to study market-level responses to an egregious event.

Studies in the environmental economics literature have examined how consumers’ environmental preferences affect their consumption patterns. This literature finds that areas with stronger environmental preference buy more “green” products and less “brown” products (e.g., Kahn 2007; Kahn and Vaughn 2009). These findings support the view that markets with stronger environmental preferences would react more negatively to the BP oil spill. Our paper extends this literature by studying the role of environmental preferences in environmentally motivated boycotts. This is important, as many boycotts are triggered by environmental concerns (King 2008).

[4] There are other examples of boycotts that do not appear to have a clear instrumental purpose. For example, Smith and Li (2010) find that Chinese consumers continue to boycott Japanese goods because of atrocities during World War II.

2. Theoretical Considerations

2.1. Model and Predictions

In this section, we describe our mathematical model of consumer boycotts. The purpose of our model is to formalize the distinction between egregiousness level and preference and to generate predictions on how these affect market-level boycott intensity. Our model is based on the decisions of individual consumers within markets but makes predictions on how this influences the offending firm, which we assess as overall changes in market share. We built our model with the BP oil spill in mind. Since we argue in Section 2.2 that the BP oil spill boycott lacked a clear instrumental purpose (i.e. it is largely noninstrumental), our model does not consider the instrumental motive consumers may have for boycotting. That is, our model does not consider the free rider problem that arises when consumers participate in a boycott for the instrumental purpose of stopping the egregious event. John and Klein (2003) do not consider the free rider problem either when they model noninstrumental boycotts.

Consider an egregious event that lasts for T periods. Assume that the egregiousness of this event at time t is e_t. Assume for the moment that the offending firm's N customers in a given market are all aware of the egregious event. If $e_{t+1} > e_t$, then all customers agree that the event becomes more egregious from time t to time $t + 1$. The egregious event causes the firm's customers to derive disutilities; they may feel guilty (Smith 1990) or frustrated (Friedman 1991). When the event's egregiousness level is e_t, it causes a customer to have a disutility of $v \cdot e_t$, where v is a parameter that measures the consumer's disutility from the event. These consumers' preference over egregiousness may differ among individuals, and this is represented by the assumption that v is uniformly distributed between two positive values, $\underline{v}$ and $\bar{v}$, where $0 < \underline{v} < \bar{v}$. A consumer's benefit from boycotting the offending firm is the avoidance of her disutility from the egregious event.

These model specifications mirror those of the textbook model of vertical product differentiation in quality, in which e_t would represent product quality and v would represent consumers' preference over quality (Tirole 1988). John and Klein's (2003) model does not distinguish consumers' preference over egregiousness (i.e., the variable v in our model) from the level of egregiousness (i.e., the variable e_t in our model). These two concepts are represented by a single variable in their model, and thus their model fails to assess them separately.

A consumer's cost of boycotting is the utility loss she experiences when she changes her consumption bundle from one with the boycotted brand to one without. For simplicity, assume all consumers have the same cost of boycotting, which is $c > 0$. Then a consumer would boycott at time $t \leq T$ if and only if

$$v \cdot e_t \geq c. \qquad (1)$$

That is, a consumer boycotts if and only if her preference parameter v is at least c/e_t. Therefore, the number of consumers who boycott the offending firm at time $t \leq T$ is

$$N\,(\bar{v} - c/e_t)/(\bar{v} - \underline{v}). \qquad (2)$$

In the context of the BP oil spill, we use changes in BP's volume share to assess boycott intensity (i.e., declines in volume share represent increases in boycott intensity). BP's volume share should vary closely with the proportion or number of consumers who participated in the boycott. It is also reasonable to assume that the cost of boycotting BP is small and constant, because gasoline is a relatively homogeneous product with alternative-branded gasoline available at many stations. Therefore, our model predicts the following testable hypothesis:

H1: *Boycott intensity increases with the egregiousness level of the oil spill.*

For the number of boycott participants to be positive, it must be the case that $\bar{v} - c/e_t \geq 0$. However, if the value of e_t is small, it is possible that the above inequality does not hold. In other words, for a positive number of consumers to boycott, the egregiousness of the offending event must be at least $c/\bar{v}$. For us to detect a boycott effect at the market level in the context of the BP oil spill, the number of boycott participants may need to be larger than a minimum positive number. That is, $\bar{v} - c/e_t$ may need to be greater than a positive value instead of zero. We hypothesize the following:

H2: *The BP oil spill triggers a boycott effect at the market level if and only if the egregiousness of the oil spill is above some threshold level.*

The BP oil spill was and is still an environmental disaster, and this fact does not change over time. That is, after the oil spill was stopped, its egregiousness level did not decrease over time. However, it is reasonable to imagine that consumers' disutility from an egregious event like the BP oil spill becomes smaller after the event is stopped. For example, anger fades over time (Chavis and Leslie 2009). If we do not separate egregiousness level from preference, we would not be able to make the distinction that after the oil spill was stopped, consumers' disutility from the oil spill, but not the egregiousness level of the oil spill, decreases over time.

In our model, the idea that consumers' disutility from the oil spill decreases over time can be represented by multiplying the preference parameter by a discount term. A consumer's disutility from egregiousness, for example, may take the form of $\delta^t \cdot v$, where $0 < \delta < 1$. Assume that the egregiousness level of the offending event, after it was stopped, remains at the level it is at time T, e_T. Then a consumer would boycott at time $t > T$ if and only if $\delta^t \cdot v \cdot e_T \geq c$. Notice the difference between this inequality and equation (1). The number of boycott participants at time $t > T$ is then $N[\bar{v} - c/(\delta^t e_T)]/(\bar{v} - \underline{v})$. This term becomes smaller as t becomes larger. In the context of the BP oil spill, we thus have the following hypothesis:

H3: *Boycott intensity exhibits a downward trend after the spill was stopped.*

We now introduce news media into the model, as news media may affect boycott intensity by providing information to consumers so that more consumers may be aware of the egregious event (Denis-Remis et al. 2013). This informational role of the media can be easily represented in the model by assuming that the number of consumers who are aware of the egregious event, N_t, is a function of the cumulative amount of media coverage, $m = m_1 + m_2 + \cdots + m_t$, where m_t is the current amount of media coverage. Assume the function $N_t(m)$ is increasing in m. By replacing N with $N_t(m)$ in equation (2), we have the following hypothesis:

H4(a): *Boycott intensity varies with the cumulative amount of media coverage.*

The news media may affect boycott intensity through a second mechanism. Our measure of the egregiousness of the BP oil spill is the official daily flow rate, but some consumers may be unaware of the flow rate and may infer the egregiousness level of the oil spill from the intensity of the media coverage (m_t) at a given point in time. The intensity of media coverage may also capture non-flow negative information such as reports of the economic or environmental harms caused by the oil spill. If this conjecture is true, then the following hypothesis needs to be assessed:

H4(b): *Boycott intensity varies with the current amount of media coverage.*

We have so far implicitly assumed that all consumers are located in a single market and we have not considered possible variations across markets. However, consumers in some markets (e.g., those consumers who are directly harmed by the offending event) may have stronger negative preference over egregiousness than consumers in other markets.[5] To capture this idea, we introduce a second market into the model. This second market is identical to the first market except that consumers in this market have stronger negative preference over egregiousness. What drives the variations in market-level boycott intensity across markets, as measured by declines in volume share, is the proportion of customers who boycott in the markets, not the absolute number of customers. Therefore, we can normalize the number of customers in both markets to be a unit mass so that we can focus on comparing the proportion of boycotting consumers between the two markets. Assume that the preference parameter of the unit mass of consumers in market 2 is uniformly distributed between $\underline{v}+a$ and $\bar{v}+a$, where $a>0$. Then the proportion of boycott participants in market 2 is $(a+\bar{v}-c/e_t)/(\bar{v}-\underline{v})$, which is greater than the proportion of consumers who boycott in market 1, $(\bar{v}-c/e_t)/(\bar{v}-\underline{v})$, at a given level of egregiousness level.

In this paper, we consider two proxies for variations in consumer preference over egregiousness. Our first proxy is whether a market in our sample is in the Gulf Coast region. Reasonably, consumers in the Gulf Coast region may consider the oil spill as more egregious than consumers in other areas, because the economy and environment in the Gulf Coast region were most directly harmed by the spill (Aldy 2011; Barbier 2011; Lee and Blanchard 2012; National Commission 2011). Thus our model predicts the following:

> H5(a): *At a given level of egregiousness of the oil spill, boycott intensity in the Gulf Coast region is stronger than that in other areas*.

Our second proxy is an area's environmental preference. The environmental economics literature has shown that environmentally oriented individuals are more likely to consume "green" products and voluntarily restrain their consumption of "brown" products. For example, in their study of electric power consumption in Michigan, Kotchen and Moore (2008) find evidence of environmentalists voluntarily restraining their consumption of electricity. Kahn (2007) and Kahn and Vaughn (2009) present evidence that consumers in areas with stronger environmental preference use more green products (e.g., hybrid vehicles) and fewer brown products (e.g., Hummers). Based on this literature, we expect areas with stronger environmental

[5] For example, consumers in a city where a firm is closing may respond more negatively, because job losses directly influence their community, than consumers in another market where the firm is not a major employer, who may view the issues less negatively (Klein et al. 2004).

preferences to have stronger negative preference for BP over the oil spill and thus have higher levels of boycotting BP. Therefore, we hypothesize the following:

> H5(b): *At a given level of egregiousness of the oil spill, boycott intensity is stronger in areas with stronger environmental preference.*

We do not attempt to explicitly model how firms respond to boycotts, because competition in retail gasoline markets can be characterized by standard economic models of price competition with differentiated products (e.g., Hastings 2004). In such a model,[6] a boycott can be modeled as a negative demand shock to the offending firm. In equilibrium, the negative demand shock causes the offending firm to cut its price. Because of strategic complementarity, rival firms would cut their prices as well, but the size of their price cuts would be smaller than that of the offending firm.[7] As a result of the boycott, the offending firm's price, relative to those of its rival firms, would become smaller. The decrease in the offending firm's relative price is larger if the demand shock is larger. Therefore, if the oil spill causes consumers to boycott BP branded gasoline and if the boycott effect is stronger in those markets where consumers have stronger negative preference over the spill, then we have the following hypotheses:

> H6(a): *BP stations' relative retail price becomes smaller in response to the oil spill;*
>
> H6(b): *The decrease in BP stations' relative price is larger in the Gulf Coast region than in other areas; and*
>
> H6(c): *The decrease in BP stations' relative price is larger in areas with stronger environmental preference than in other areas.*

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[6] Consider a simple model in which two firms, 1 and 2, compete over the price of a differentiated product. A firm's demand is decreasing in its own price and increasing in its competitor's price. Suppose firm 1 is the offending firm. One then can add an exogenous negative shock to firm 1's demand function to represent the boycott effect. In this setting, one can easily show that (1) firm 1's equilibrium response is to cut its price, (2) firm 2's response is to cut its price as well (because of strategic complementarity), and (3) firm 2's price cut is smaller than that of firm 1.

[7] The prediction that rival firms cut their prices results from the assumption that a boycott has no direct impact on their demand (it has a direct negative impact only on the offending firm's demand). If one assumes that a boycott has a direct and positive impact on rival firms' demand as well, then rival firms may not cut their prices in response to the boycott. In that case, rival firms face the pressure of cutting prices because of strategic complementarity, but they also have the incentive to increase prices because of the positive demand shock. The sign of the rival firms' price changes depends on the parameters of the model. However, the prediction that the offending firm's price, relative to those of its rivals, becomes smaller continues to hold for reasonable parameters.

It is worth noting that on June 29, 2010, BP announced that it would offer price rebates to distributors of BP gasoline for some periods (Weber 2010). The price rebate was 2 cents per gallon in the Gulf Coast region and 1 cent per gallon in the rest of the country. Price cuts, if passed on to consumers, are expected to attenuate the possible decreases in BP's market share due to boycotting behavior. However, there is no suggestion that the price rebates were larger in areas with stronger environmental preference.

2.2. Specific Purposes of Boycotting BP Gasoline

In this section, we argue that the BP boycott lacked a clear instrumental purpose. Of course, some individuals may have boycotted BP for some vague instrumental purposes, such as signaling to BP and other firms that they should not commit socially irresponsible acts in the future.

In the public calls for boycotting BP-branded gasoline, there was never any discussion that boycotting the firm would help stop the spill or speed up BP's activities. *Public Citizen* was the main organization that urged consumers to boycott BP-brand gasoline. Yet in its May 14, 2010, press release, *Public Citizen* called for boycotting BP gasoline and emphasized consumer anger and punishment, not concrete goals: "BP must pay. A consumer boycott will deliver that message and give people a way to act on their anger. We are urging people to send a clear message to BP that its shoddy oversight of this project and its history of environmental and worker safety violations is unforgivable" (Public Citizen 2010). A "Boycott BP" Facebook page attracted more than 800,000 "likes" at one point during the spill. Yet the creator of this website never mentioned specific goals for boycotting BP gasoline.

In fact, BP already had strong incentives to stop the spill because of the tremendous pressure from the US federal government and because of the enormous legal liabilities and potential criminal investigations. For example, an article on the *Knowledge@Wharton* website in June 2010 reported the views of several academic scholars on the BP boycott and suggested that a BP boycott was "likely to be only a nuisance when compared to the outsized legal liability the company is facing from the Gulf spill." In addition, Wharton management professor Lawrence G. Hrebiniak indicated that "boycotts tend to be more successful when there is a clear connection between the act of boycotting and some desired outcome. 'It's not clear that if we boycott [BP] it solves the leak in the Gulf.' After all, . . . BP is clearly aiming massive resources at stopping the spill, and a boycott doesn't make success in that arena any more likely." Further, even the president of *Public Citizen* agreed that "BP's liability from the spill may be far greater than the impact of a boycott" (Knowledge@Wharton 2010).

There were also many calls for not boycotting BP gasoline, and the arguments centered on whether boycotting BP would be an effective way to punish BP or to cleanse one's conscience and avoid guilty feelings—not on whether boycotting could help stop the oil spill. For example, a May 31, 2010, *Los Angeles Times* editorial presumed that people would boycott BP to express their anger or punish BP, arguing, "It's not surprising that people want to take action to express their despair … [but there] are far better and more effective outlets for public outrage." The editorial gave two reasons why BP was the wrong target: First, if people boycotted BP gasoline, where would they fill up? Boycotting one oil company was problematic as "in the oil business, nobody has clean hands." Second, "BP stations are independently owned, so a boycott hurts individual retailers more than London-based BP" (Los Angeles Times 2010).

3. Data

Our data set covers the official daily flow rate, the weekly amount of news coverage of the oil spill, BP-branded gasoline's volume share and price differentials in 257 US counties, and these counties' level of environmentalism and geographic proximity to the Gulf. Table 1 presents the main variables used in the regressions and their summary statistics.

Table 1. Summary Statistics of the Main Variables

Variable	Unit	Obs.	Mean	Std. dev.	Min	Max
Official daily flow rate, $t = 1, \dots, 13$	1,000 barrels/day	13	24.868	20.265	1.571	47.500
Cumulative amount of oil spilled up to week $t = 1, \dots, 13$	1,000 barrels	13	1,699	1,708	11.00	4,322
Weekly # of spill news items, $t = 1, \dots, 44$	1,000	44	0.425	0.463	0.039	1.972
Cumulative # of news items up to week t	1,000	44	13.637	5.537	39	18.745
BP's volume share in a county						
before spill	%	257	16.089	8.673	1.975	53.792
during and post-spill	%	257	15.453	8.245	2.292	48.647
BP's price differential with competitors						
before spill	cents/gallon	257	1.409	1.504	-3.458	8.680
during and post-spill	cents/gallon	257	1.381	1.718	-2.641	13.674
Variability of BP's pre-spill volume share	%	257	1.109	0.632	0.261	5.699
Whether a county is in the Gulf Coast area		257	0.136	0.344	0.000	1.000
County's distance from the Gulf	1,000 miles	257	0.535	0.305	0.000	1.185
% of individuals who contributed to Green Party committees from 2003 to 2010	%	257	0.000	0.001	0.000	0.006
Per capita contributions to Green Party committees from 2003 to 2010	$	257	0.002	0.005	0.000	0.057
Green index		257	0.000	1.876	-0.777	19.555

Note: Green index is the sum of the standardized values (the z-scores) of the two measures of environmentalism.

3.1. Flow Rates and Media Coverage of the Oil Spill

Our measures of the egregiousness of the BP oil spill are based on the official daily flow rates, which are summarized in a report by Staff of the National Commission on the BP Deepwater Horizon Oil Spill and Offshore Drilling (2011). The Deepwater Horizon oil rig exploded on April 20, 2010, but the accident was not reported by major newspapers until Thursday, April 22 (National Commission 2011). Because our gasoline data are of weekly frequency and a week is defined as the seven days from Thursday through Wednesday, we treat the seven days from April 15 through April 21 as the week prior to the accident. The reported official daily flow rate between April 20 and April 27 was 1,000 barrels per day. It was increased to 5,000 barrels on April 28 and to 12,000–19,000 barrels on May 27. It was further increased to 25,000–30,000 barrels on June 10 and to 35,000–60,000 barrels on June 15. The oil leak stopped on July 15, when the well was capped, though considerable uncertainties remained as to whether capping the well would force oil to leak through the rock until August 4, when "static kill" was declared a success.[8] The US federal government declared on September 19, 2010, that the BP well was "effectively dead" after a relief well was completed and the BP well was permanently sealed.

Figure 1. The Official Flow Rate and Media Coverage of the BP Oil Spill

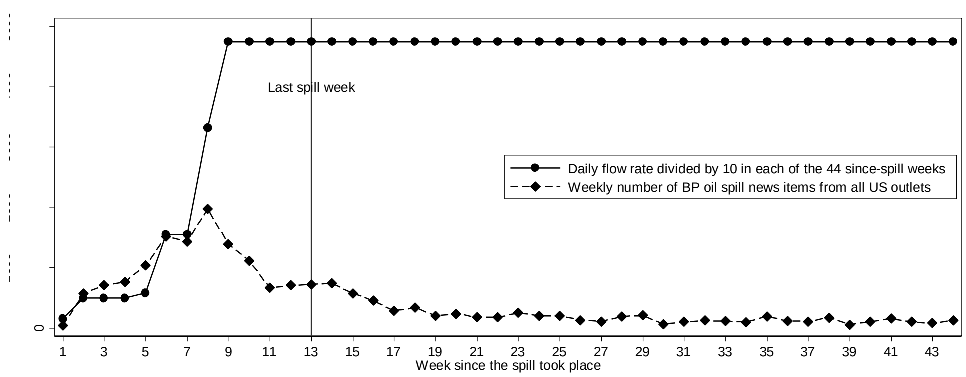


Figure 1 shows the daily official flow rate (in thousands of barrels) and the number of US media news items on the BP oil spill in each of the 44 weeks since the spill started. We call these 44 weeks the "since-spill" weeks. Because the oil leak was stopped during the 13th week, we refer to the first 13 since-spill weeks as the "during-spill" weeks and the remaining 31 since-spill

[8] Static kill is the procedure of pumping heavy drilling mud into the well to push oil and gas back into the reservoir.

weeks after the leak was stopped as the "post-spill" weeks. When two different flow rates were reported during one of the 13 during-spill weeks, we use the weighted average of the two daily flow rates, where the weights are the number of days during the week that correspond to a particular flow rate. If the flow rate for a particular day is a range (e.g., 12,000–19,000 barrels), we use the average (e.g., 15,500 barrels). The official flow rate reported for the 31 post-spill weeks in Figure 1 (i.e., weeks 13-44) is set at the official flow rate on week 13, the last during-spill week. The actual amount of oil leaking into the ocean during the post-spill weeks, by definition, was zero, but the egregiousness of the oil spill during the spill weeks presumably remained at the level it was on the date it was stopped, even though consumers' disutility from egregiousness may decline over time.

In the introduction, we mentioned that for consumers who, during one of the spill weeks, were uncertain about the egregiousness level of the oil spill, a reasonable estimate is the official flow rate in that week. The rationale for this supposition is that the damage of the oil spill can be approximated by the official daily flow rate multiplied by the duration of the oil spill days (i.e., the total number of days between the first and last days of the spill). If consumers' estimate of the total number of spill days,[9] during any of the spill weeks, is roughly a constant, then variations in the official daily flow rate are a reasonable measure of the variations in the egregiousness level of the oil spill. Consumers' estimate of the number of spill days is a constant if they have rational expectations—if they are correct in their estimate of the day on which the oil spill would be stopped.

One may also argue that what matters to consumers' boycott decisions during any spill week is the egregiousness level of the oil spill up to that week. If this is the case, then the cumulative amount of oil that had leaked into the ocean up to that week may be a better measure of egregiousness than the official daily flow rate. Our regressions in Table 2 consider both cumulative flow and daily flow, allowing the regressions to determine which measure is a better predictor of boycott intensity.

Figure 1 also shows the weekly media coverage for each of the 44 since-spill weeks (13 during-spill weeks and 31 post-spill weeks). Our measure of news coverage of the BP oil spill is based on the number of news articles and media transcripts whose first paragraph contains the

[9] The total number of spill days is the sum of days during which oil had been leaking and the additional number of days during which it would continue to leak into the ocean. Consumers, during any of the spill weeks, needed only to estimate the number of additional days that oil would continue to leak into the ocean.

key words “BP” and “oil spill.” We searched all articles and transcripts of news reports of the oil spill in Access World News, a news database that covers the full-text content of 2,473 newspapers, 214 newswires, 243 TV channels, 299 online-only sources, and a number of other news sources in the United States. We restricted our search to the first paragraph of an article in an attempt to eliminate those articles that mention the key words in passing but whose main content is not about the oil spill. The weekly number of BP oil spill news items increased from 39 in the first week of the spill to 566 in the second week, reaching 1,972 in the eighth week, and then started to decrease. The weekly number of oil spill news items roughly tracked the official flow rate in the first seven weeks but started to diverge from week eight on; although the flow rate increased from week eight to nine and remained constant until the 13th week, when the spill was capped, the number of news items started to decrease in week eight.

The number of consumers who were aware of the oil spill in a week should be correlated with the cumulative number of news articles and transcripts up to that week, not just the number of news and transcripts in that week only, suggesting that we should use the cumulative number of news items and transcripts when testing hypothesis 4(a).

Hypothesis 4(b) is based on the idea that some consumers may have inferred the egregiousness of the oil spill from the weekly intensity of the media’s coverage of the oil spill. We now elaborate this idea using the flow rate and media data. For example, from the second to the fifth since-spill week, the official flow rate remained essentially constant at 5,000 barrels/day, but the weekly number of news items increased substantially, from 566 to 1,041. It is reasonable to imagine that some consumers may have inferred from the increased intensity of media coverage that the egregiousness of the oil spill had increased from the second week to the fifth week even though the official flow rate did not change during this period. If this conjecture is valid, the weekly number of news items may capture some of the variations in the egregiousness level of the oil spill that are not captured by the official flow rate.

Variations in the cumulative (or weekly) number of news items are reasonable indicators of the variations in the cumulative amount (or weekly intensity) of the media’s coverage of the oil spill over time. We do not attempt to measure the variations in the intensity of the media’s coverage of the oil spill across local markets because consumers obtain their news from multiple sources, including the Internet and cable or broadcast TV news programs that are not location-specific.[10]

Because of the large number of oil spill news items each week, we cannot identify the effect of a single piece of news. For example, the BP CEO's comment on May 30, "I'd like to get my life back," was a public relations disaster that may have affected some consumers' boycott decisions. However, we cannot identify the effect of this event because our gasoline data are of weekly frequency and because several other major spill-related events took place during the same week: On May 27, President Obama announced a six-month moratorium on new deepwater oil drilling; on May 29, BP declared that its first attempt to kill the well—the "top kill"—was a failure; and on the same day, BP and the US federal government announced that BP would install a device—the "top hat"—to channel the oil to surface ships.

3.2. Gasoline Data

We are able to use reductions in BP's volume share as a measure of boycott intensity because the oil spill did not cause supply disruptions to BP refineries or BP-branded retail stations. In fact, gasoline sold at BP-branded stations may not come from BP refineries, crude oil processed at BP refineries may not come from BP-drilled oil wells, and gasoline sold at non–BP-branded stations sometimes comes from refineries or wholesalers that BP owns (see Lieber 2010).

Our retail gasoline data are provided by Oil Price Information Service (OPIS), which collects gasoline price and quantity data from the transaction records of the Wright Express fleet cards. These data are used by OPIS's clients, including gasoline retailers, to monitor retail gasoline markets. OPIS gasoline data have also been used in many academic studies (e.g., Taylor et al. 2010; Busse et al. 2013). Wright Express states that upward of 4 million drivers from more than 280,000 businesses (small businesses, large corporations, and government agencies) use the fleet cards, which are accepted at more than 90 percent of the retail fueling stations in the United States. Users of Wright Express cards may buy gasoline from any participating stations.

We observe, by county, two main weekly frequency market outcome variables—BP brand's volume share and its price differential with competing brands—for 112 weeks, from

[10] The weekly number of BP oil spill news items from outlets in the five Gulf States is much smaller than the weekly number of news items from all US outlets and is highly correlated with the weekly number of spill news items from all US outlets (the correlation coefficient is 0.97).

January 2009 through February 2011. Our sample covers 68 weeks prior to the accident and 44 weeks afterward. This is why Figure 1 covers 44 since-spill weeks. Price differential is defined as the average price of BP-branded gasoline in a county minus the average price of all competing brands in the same county.

Our analysis focuses on large counties, defined as those with at least 50 retail stations and at least 5 BP-branded stations. The data for large counties are more reliable because they tend to have more Wright Express users. Our sample covers 257 such counties in 27 states and the District of Columbia. The 27 states are all located to the east of the Rocky Mountains.[11] BP-brand gasoline is sold in three of the five states bordering the Gulf of Mexico (Alabama, Florida, and Mississippi), and 44 sample counties are located in these three states.

Figure 2(a). BP's Weekly Volume Share

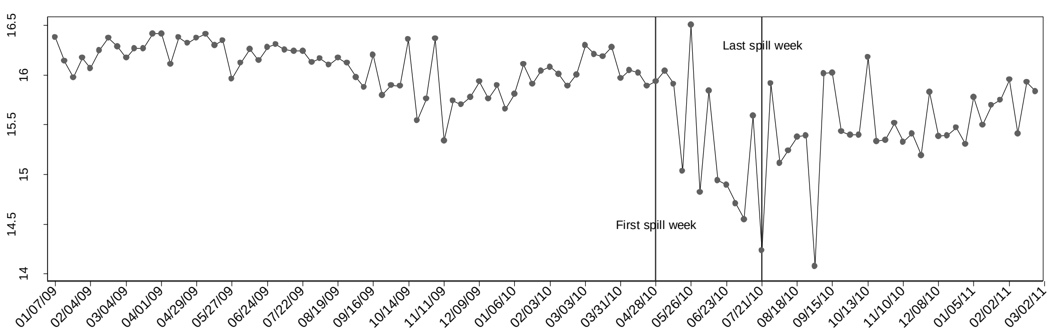
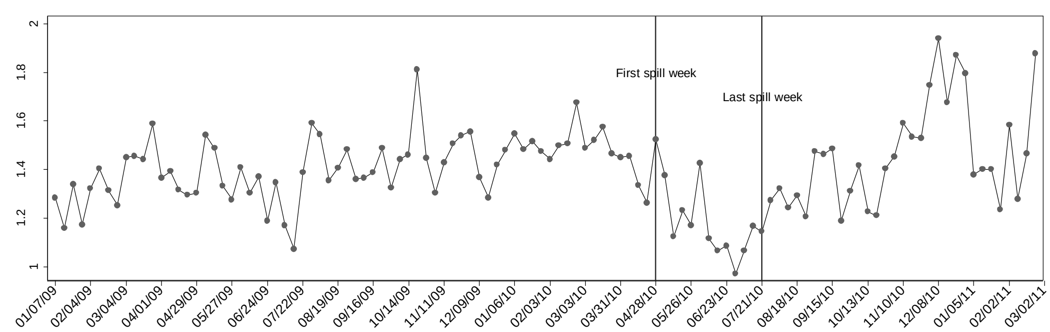


Figure 2(b). Weekly Price Differential between BP and Competing Brands



11 The 27 states are Alabama, Connecticut, Delaware, Florida, Georgia, Indiana, Illinois, Iowa, Kansas, Kentucky, Maryland, Michigan, Minnesota, Missouri, Mississippi, North Carolina, Nebraska, New Jersey, New York, Ohio, Pennsylvania, South Carolina, South Dakota, Tennessee, Virginia, Wisconsin, and West Virginia.

Figures 2(a) and 2(b) present BP's weekly volume share and price differential with competing brands averaged over all sample counties during the entire sample period. The patterns shown in these two figures are simple averages without any controls. Figure 2(a) suggests that BP's volume share did not decrease in the first three spill weeks but then became quite volatile and considerably smaller than the pre-spill level for a number of weeks. The strong volatility in BP's weekly volume share since the spill took place may be due to the fact that consumers were being constantly influenced by new information, some of which was positive and some negative. Shortly after the spill was stopped, volume shares became less volatile and approached pre-spill levels. Figure 2(b) suggests that BP retail stations cut prices in early spill weeks. The upward trend in BP's price differential starting in late October 2010 may be related to the expiration of the price rebates that BP offered to gasoline distributors.

We also assess the effect of variability in BP's volume share over the 68 weeks prior to the oil spill on boycott intensity. If BP's pre-spill volume share, after controlling for the effects of outlet share and price differential, was more volatile in a county, it is reasonable to expect the boycott intensity in that county to have been stronger, for several possible reasons. First, a more volatile volume share may indicate that drivers in that area are less loyal (Dekimpe et al. 1997) to BP-branded gasoline. Second, a more volatile volume share may simply indicate that it is easier for drivers in that area to switch from BP to other brands, possibly because BP stations are located closer to competing stations. Third, a more volatile volume share may also result from more price-sensitive consumers.

We measure the variability of BP's pre-spill volume share in a county by the standard deviation of the residuals from regressing BP's volume share in the county on a constant, BP's share of retail outlets in that county, and BP's price differential with competing brands in that county.

3.3. Geographic Proximity to the Gulf

To test hypothesis 5(a), we need to evaluate a sample county's geographic proximity to the Gulf, by measuring the county's distance from the Gulf by the shortest Euclidean distance between the centroid of the county and the Gulf. We follow the *Encyclopædia Britannica* by defining the Gulf Coast as the land areas within 100 miles of the Gulf. By this definition, a total of 35 counties are in the Gulf Coast area, with 33 counties in the three states bordering the Gulf and 2 counties in Georgia.

3.4. Environmentalism

Our measures of a county's environmentalism are based on individuals' donations to Green Party political committees. Our first measure is the average per capita contribution to Green Party political committees from 2003 through 2010,[12] and our second measure is the percentage of individuals in a county who contributed at least $200 to any Green Party political committee. The population for each county comes from the 2010 census, and the campaign contribution data come from the Federal Election Commission (FEC), which records individual contributions of at least $200. The FEC data contain the zip code of the area where an individual lives, allowing us to use ArcGIS to identify the individual's county of residence. The correlation coefficient between these two measures of environmentalism is 0.76.

These two measures are similar in spirit to those used by Kahn (2007) and Kahn and Vaughn (2009), who measured environmentalism of California neighborhoods by the percentage of individuals who are registered members of the Green Party. Unfortunately, we were unable to obtain Green Party registration data for the 257 counties in our sample and thus relied on donations to green parties.

The percentage of individuals who made contributions to Green Party political committees is quite small, averaging 0.0003 per country, and the per capita contribution in the average county is $0.002. There were no individuals in 169 (65.8%) of the 257 counties who contributed at least $200 to any Green Party political committee between 2003 and 2010. Kahn and Vaughn (2009) note that membership of Green Parties, even in California, was very small, at 0.009 percent. We also create and assess a composite measure which is the sum of the standardized values (the z-scores) of our two measures of environmentalism.

4. Econometric Models and Results

We organize our empirical analyses into four sections. In Section 4.1, we study the effects of the official flow rate and the media's coverage of the oil spill (H1, H2, and H4(a) and (b)). In Section 4.2, we estimate boycott intensity of weekly frequency (H3). In Section 4.3, we study the effects of proximity to the Gulf and of environmentalism (H5(a) and (b)). And in

[12] Barrage et al. (2014) use an index to measure an area's environmental preference. Their index is the sum of the standardized values of four variables, one of which is the average per capita contribution to Green Party political committees. They did not consider average per capita contribution as an independent measure.

Section 4.4, we study changes in BP's price differential with its competitors (H6(a), (b), and (c)) and the effect of relative price changes on volume share.

4.1. Effects of Flow Rate and Media Coverage

We use the following baseline model to study the impact of the oil spill on BP's volume share:

$$BP_{it} = c + flow_t + CumulativeNews_t + \alpha_i + T_{it} + T'_t + M_t + \varepsilon_{it}. \qquad (3)$$

The dependent variable BP_{it} is a measure of BP's market outcome in county i in week t; $flow_t$ is the official daily flow rate in week t divided by 1,000 (so that the unit is 1,000 barrels per day); $CumulativeNews_t$ is the cumulative number of oil spill–related news items up to week t divided by 1,000; α_i is a county fixed effect; T_{it} is a county-specific time trend; T'_t is the overall time trend; $M_t$is a month dummy;[13] and ε_{it} is the error term. The reported standard errors are those suggested by Davidson and MacKinnon (1993), which are robust to heteroskedasticity and serial correlations. We will add and change variables later.

When estimating any specification with the term $flow_t$, we exclude the 31 post-spill weeks because, as discussed in the theory section, after the oil spill was stopped, the egregiousness level of the oil spill no longer changes but the relationship between egregiousness level and boycott intensity changes over time because consumers' disutility from the same level of egregiousness is likely to decay over time.

Equation (3), where the dependent variable is BP's weekly volume share, can be used to test hypothesis 1, that the marginal impact of egregiousness level on boycott intensity is positive, and hypothesis 4(a), that boycott intensity varies with the cumulative amount of media coverage. Model 1 in Table 2 reports the estimated results. The estimated coefficient for the flow rate variable is –0.024 and is highly statistically significant. This result indicates that BP's market share decreases by 0.024 percentage points if the daily flow rate increases by 1,000 barrels, supporting hypothesis 1. However, the estimated coefficient for the cumulative amount of news in Model 1 is not statistically significant, which does not support hypothesis 4(a).

[13] If the seven days of a week fall in two different months, we treat the week as a part of the month in which most days of the week fall. Only 7 out of the 112 weeks in our sample are affected by this rule.

In Model 2, we add into equation (3) an additional measure of egregiousness: the cumulative amount of oil that had leaked into the ocean up to week t, which is the official daily flow rate for week t times the number of days from April 22 to week t (in thousands of barrels). The coefficient for the cumulative flow variable is not statistically significant and thus has no effect on overall boycotting behavior. The coefficient for the daily flow rate variable remains negative and statistically significant and the coefficient for the cumulative news variable remains insignificant. These results suggest that the daily flow rate is a better measure of egregiousness than the cumulative amount of oil spills. For this reason, we will use the flow rate variable as the egregiousness measure in the regressions to follow.

In Model 3, we add into equation (3) a new variable, $WeeklyNews_t$, which is the number of oil spill–related news items in week t (divided by 1,000). This allows us to test hypothesis 4(b). The coefficient for the flow rate variable remains significant and similar to those in Models 1 and 2. The coefficient for the weekly number of news items variable is negative and statistically significant, and cumulative news is still insignificant. Our interpretation of this result is that the weekly number of oil spill news items variable captures some of the variations in the egregiousness level of the oil spill that are not captured by the official flow rate variable.

In Models 4–7 of Table 2, we conduct a series of robustness checks of Model 3. In Model 4, we estimate the Model 3 equation without the time trends or the month dummy. In Model 5, we estimate the equation with the time trends but without the month dummy. In Model 6, we estimate the equation with the month dummy but without the time trends. In Model 7, we estimate the equation without the time trends or the month dummy but with a first-order autoregressive (AR(1)) error term to see if serial correlations alter our findings. The estimates for these four models indicate a consistent pattern. The flow rate variable always has a negative and statistically significant coefficient, supporting H1. The cumulative number of news items variable never has a statistically negative coefficient, thus rejecting H4(a). The weekly number of news items variable, in most specifications, has a negative and statistically significant coefficient, thus supporting H4(b). Because of this result, we will use only the weekly number of news items in other regressions.

It is unclear why boycott intensity does not vary with the cumulative amount of media coverage. It is possible that almost all consumers became aware of the oil spill fairly quickly, perhaps within the first three weeks. If this is the case, variations in the cumulative amount of media coverage after the first three weeks no longer reflect variations in the number of consumers who are aware of the oil spill. Since our results in Section 4.2 indicate that market-level boycott effect does not exist until the fourth week, it is not surprising that boycott intensity is not associated with the cumulative amount of media coverage.

In Model 8 of Table 2, we use a threshold model to test hypothesis 2, that the spill generates a boycott effect only after its level of egregiousness is above a threshold level. The threshold model is the following:

$$BP_{it} = c + I_1[flow_t \leq 1000] * flow_t + (1 - I_1[flow_t \leq 1000]) * flow_t + WeeklyNews_t + \alpha_i + T_{it} + T_t' + M_t + \varepsilon_{it} \quad (4)$$

The indicator variable $I_1[flow_t \leq 1000]$ in equation (4) equals 1 when the official flow rate is 1,000 barrels per day. We chose 1,000 barrels per day as the cutoff point because the official flow rate jumped from 1,000 barrels per day in the first week to 5,000 barrels per day in the second week and remained essentially constant for the next four weeks. If we were to choose 5,000 as the cutoff point, we would have to assume there is no boycott effect in the first five spill weeks, which, according to both Figure 2(a) and results in Section 4.2, is untrue. We do not expect the flow rate in week 1 to affect BP's market share.

The estimates for equation (4), as reported in Model 8, support hypothesis 2: the flow rate in the first week did not have an effect on BP's market share, and the estimated coefficient for the flow rate since the second week is statistically significant and similar to estimates from previous models.[14]

[14] We also explored the idea of a threshold effect for the weekly number of news items. We replaced the variable $WeeklyNews_t$ in equation (4) with two variables: one variable that equals $WeeklyNews_t$ when $t \leq 3$ and 0 otherwise and another variable that equals $WeeklyNews_t$ when $t \geq 4$ and 0 otherwise. That is, we chose the third week as the cutoff week. We find that the weekly number of news items in the first three weeks does not have a boycott effect and the weekly number of news items from the fourth week on has a boycott effect. The results would be very similar if we had chosen the first or second week as the cutoff week.

Table 2. The Effects of Flow Rate and Media Coverage Intensity on Boycott Intensity

Variables	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6	Model 7	Model 8
Flow rate	-0.024***	-0.028***	-0.027***	-0.027***	-0.029***	-0.026***	-0.056***	
	(0.006)	(0.007)	(0.006)	(0.008)	(0.006)	(0.008)	(0.005)	
Cumulative amount of oil spilled up to week *t*		0.000						
		(0.000)						
Cumulative amount of news items up to week *t*	-0.005	-0.010	0.019	0.013	0.031	0.005	0.119***	
	(0.024)	(0.025)	(0.026)	(0.036)	(0.026)	(0.037)	(0.023)	
Weekly number of news items			-0.125**	-0.176***	-0.067	-0.252***	-0.206***	-0.119**
			(0.055)	(0.060)	(0.044)	(0.071)	(0.062)	(0.051)
Flow rate in the first week								-0.087
								(0.062)
Flow rates after the first week								-0.023***
								(0.002)
Weekly number of news items in the first three weeks								
Weekly number of news items from the fourth week on								
Time trend	-0.037***	-0.037***	-0.037***		-0.037***			-0.036***
	(0.006)	(0.006)	(0.006)		(0.006)			(0.006)
Constant	22.004***	22.000***	21.993***	20.603***	22.109***	20.536***	16.079***	21.988***
	(0.226)	(0.226)	(0.226)	(0.156)	(0.236)	(0.152)	(0.010)	(0.226)
County fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
County-specific time trend	Yes	Yes	Yes	No	Yes	No	No	Yes
Month fixed effects	Yes	Yes	Yes	No	No	Yes	No	Yes
Observations	20,817	20,817	20,817	20,817	20,817	20,817	20,560	20,817
R-squared	0.975	0.975	0.975	0.964	0.975	0.964		0.975

Notes: Dependent variable is BP's weekly volume share. Standard errors are in parentheses. Model 7 is estimated by the STATA command "xtregar" with the fixed-effects option, which produces an R-square (not reported here) that does not take into account the portion of variations explained by the county fixed effects. The other seven models are estimated by ordinary least squares (OLS), and in these cases, Davidson and MacKinnon (1993) robust standard errors are in parentheses. *** p<0.01, ** p<0.05, * p<0.

4.2. Weekly Boycott Effects

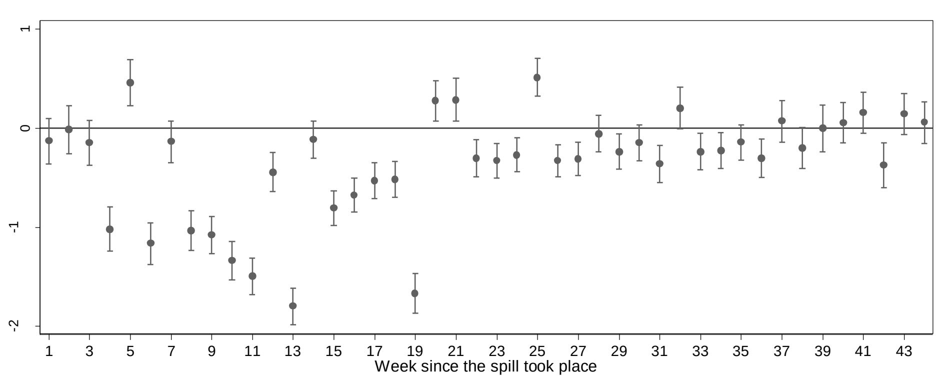
To test hypothesis 3, that boycott intensity exhibits a downward trend after the oil spill was stopped, we estimate the following model:

$$BP_{it} = c + w_1 + \ldots + w_{44} + WeeklyNews_t + \alpha_i + T_{it} + T_t' + M_t + \varepsilon_{it}. \qquad (5)$$

In equation (5), w_j is a dummy variable that equals 1 for the j^{th} since-spill week and 0 otherwise. Recall that there are 44 since-spill weeks. All the other variables in equation (5) are as previously defined. By using weekly dummies instead of the flow rate, we obtain estimates of weekly boycott intensity (i.e., the weekly change in BP's market share from its average volume share prior to the spill). This approach allows us to identify the start, the peak, and the end of the boycott and the trend in boycott intensity. As such, this model allows us not only to test hypothesis 3, but also to examine hypotheses 1 and 2 from a different perspective.

Figure 3 presents the estimated coefficients for the 44 weekly dummies and the associated 95% confidence intervals. Model 1 in the Appendix presents the full regression results of equation (5). The estimated coefficients (in absolute value) during the post-spill weeks exhibit a downward trend, supporting hypothesis 3. The boycott effect stayed at more than 0.5 percentage points consecutively from week 15 through week 19, the first few post-spill weeks, and became much smaller afterward. Toward the end of our sample period, most estimated coefficients are no longer statistically significant, suggesting that the boycott effect had become very weak or nonexistent by the end of our sample period.

Figure 3. Mean and 95 Percent Confidence Interval of the Weekly Boycott Effects



The results also suggest that BP's volume share did not decrease in the first three weeks of the spill. This further supports H2 that there is a minimum threshold required for a boycott to occur. The estimated boycott effect is the largest in the 13th week (July 15–21, 2010), which is consistent with the fact that the egregiousness level of the oil leak (i.e., the daily flow rate) reached its peak in this week. The absolute level of BP's volume share in week 13 when the well was capped was 1.8 percentage points smaller than the average pre-spill level. Because BP's average volume share was 16.11 percent before the spill, BP's volume share in the 13th week declined by about 11.11 percent (=1.8 percent /16.11 percent). The estimated coefficients (in absolute value) on and before week 13 exhibit an upward trend, indicating an intensification of boycotting behavior; the correlation coefficient between the estimated weekly boycott intensity (in absolute value) and the daily flow rate during the 13 spill weeks is 0.71, with a p-value of 0.007. This upward trend in boycott intensity is further evidence in support of hypothesis 1 that as spill levels increase, boycotting behavior increases.

The coefficients for weeks 5, 20, 21, and 25 are positive and statistically significant. A plausible explanation for this unexpected result is that it was caused by the strong volatility in BP's volume share starting from the fourth spill week.

4.3. Effects of Proximity to the Gulf and of Environmentalism

To test hypotheses 5(a) and 5(b) (that boycott intensity is stronger in the Gulf Coast region and in areas with stronger environmental preference at a given level of egregiousness), we estimate the following model:

$$BP_{it} = c + flow_t + flow_t * CountyCharacter_i + flow_t * VarPrespill_i + WeeklyNews_t + \alpha_i + T_{it} + T'_t + M_t + \varepsilon_{it}, \quad (6)$$

where $CountyCharacter_i$ indicates area $i's$ proximity to the Gulf and/or its environmental preference, and $VarPrespill_i$ indicates the variability in BP's pre-spill volume share. This model allows us to compare the boycott intensity in different areas at a given level of egregiousness of the oil spill.

The variability in BP's pre-spill market share is integrated into all of the models in Table 3. The estimated coefficients for pre-spill variability are negative and statistically significant in all models, thus indicating that boycott intensity is larger in counties where BP's pre-spill volume shares are more volatile. This result is consistent with the idea that consumers in counties with more volatile pre-spill volume shares are less loyal to BP-branded gasoline, but it is also consistent with the other explanations discussed in Section 2.

Table 3. Effects of Proximity to the Gulf and of Environmentalism during the Oil Spill

Explanatory variables	Model 1	Model 2	Model 3	Model 4	Model 5
Flow rate	-0.012***	-0.014***	-0.013***	-0.014***	-0.014***
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
Flow rate * Gulf Coast	-0.012***	-0.010***			-0.011***
	(0.003)	(0.003)			(0.003)
Flow rate * distance is between 0.1 and 0.3 thousand miles from the Gulf		-0.001			-0.002
		(0.003)			(0.003)
Flow rate * distance is between 0.3 and 0.74 thousand miles from the Gulf		0.005**			0.005**
		(0.002)			(0.002)
Flow rate * per capita donations to Green Party committees			-0.586***		
			(0.148)		
Flow rate * % of individuals who gave to Green Party committees				-3.933***	
				(1.519)	
Flow rate * green index					-0.002***
					(0.000)
Flow rate * variability of BP's pre-spill volume share	-0.008***	-0.008***	-0.008***	-0.008***	-0.009***
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
Weekly # of BP oil spill news items	-0.108**	-0.108**	-0.108**	-0.108**	-0.108**
	(0.050)	(0.050)	(0.050)	(0.050)	(0.050)
Time trend	-0.034***	-0.034***	-0.037***	-0.037***	-0.035***
	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)
Constant	21.933***	21.933***	22.011***	22.012***	21.938***
	(0.230)	(0.230)	(0.225)	(0.225)	(0.230)
Observations	20,817	20,817	20,817	20,817	20,817
R-squared	0.975	0.975	0.975	0.975	0.975

Notes: The dependent variable is BP's weekly volume share. All regressions include county fixed effects and county-specific time trends. Gulf Coast refers to counties that are within 100 miles of the Gulf of Mexico. Davidson and MacKinnon (1993) robust standard errors are in parentheses. Green index is the sum of the standardized values of our two primary measures of a county's environmentalism: per capita donations to Green Party committees and percentage of individuals who donated to Green Party committees. *** p<0.01, ** p<0.05, * p<0.1.

In Model 1 of Table 3, the county characteristics variable is whether a sampled county is in the Gulf Coast region. As expected, the flow rate variable is negative and highly statistically significant, and the interaction term is also negative and highly statistically significant, supporting hypothesis 5(a), that boycott intensity, at a given egregiousness level of the oil spill, is stronger in the Gulf Coast counties (i.e., those within 100 miles of the Gulf) than in other areas.

In Model 2 of Table 3, we study the boycott intensity in the non–Gulf Coast counties. The findings from this model suggest that boycott intensity does not necessarily decrease linearly with an area's distance from the Gulf. In this model, we divide the non-Gulf areas into three regions: region 2, which is between 100 and 300 miles away from the Gulf; region 3, which is between 300 and 740 miles away; and region 4, which is at least 740 miles away. We chose 300 miles as the cutoff for region 2 because it is about the maximum distance by which a sampled county in the three sampled Gulf states is away from the Gulf. We chose 740 miles as the cutoff point for region 3 because this makes the number of sampled counties in regions 3 and 4 equal to each other. Region 4 is left out of Model 2 of Table 3, as the comparison group. The interaction term between the flow rate variable and region 2 is not statistically significant, implying that the boycott intensity in region 2, at a given egregiousness level of the spill, is the same as the boycott intensity in region 4. The interaction term between the flow rate variable and region 3 is positive and statistically significant, indicating that the boycott intensity in region 3, at a given egregiousness level of the spill, is weaker than the boycott intensity in region 4. Thus, boycotting intensity does not diminish linearly as counties are further away from the Gulf.

In Models 3 and 4 in Table 3, we test hypothesis 5(b), that boycott intensity is stronger in counties with stronger environmental preferences at a given egregiousness level of the spill. The interaction term between the flow rate and the per capita donations to Green Party committees has a negative and highly statistically significant coefficient in Model 3, as does the interaction term between the flow rate and the percentage of individuals who made donations to Green Party committees in Model 4. These results support hypothesis 5(b).

In Model 5 of Table 3, we consider five interaction terms together: the three interaction terms between the flow rate and the three region dummies, the interaction term between the flow rate and a composite environmentalism variable, and the interaction term between the flow rate and the variability in BP’s pre-spill volume share variable. In this model, the composite measure of environmentalism (i.e., green index) is the sum of the standardized values (the *z*-scores) of our two measures of environmentalism. The results remain similar as before.

Equation (6) allows us to compare boycott intensity in different areas at a given egregiousness level of the oil spill. However, it has the disadvantage of restricting our estimation to the 13 spill weeks.

A second approach to test hypotheses 5(a) and 5(b) is the following equation, which does not use the flow rate variable:

$$BP_{it} = c + dur + dur * CountyCharacter_i + post + post * CountyCharacter_i + dur * VarPrespill_i + post * VarPrespill_i + WeeklyNews_t + \alpha_i + T_{it} + T'_t + M_t + \varepsilon_{it}, \quad (7)$$

where dur and $post$ are two time dummies that indicate, respectively, the 13 during-spill weeks and the 31 post-spill weeks. The other variables in equation (7) are as previously defined. This approach allows us to compare boycott intensity in different areas in the 13 during-spill weeks and in the 31 post-spill weeks, but it implicitly assumes that boycott intensity in any area takes on a single value during the 13 spill weeks and takes another value during the 31 post-spill weeks.

The models in Table 4 estimate equation (7). Each model in Table 4 shares the same county characteristics variables with the model of the same number in Table 3. The interaction effects in Table 4 during the spill weeks are generally consistent with those in Table 3. During the spill weeks, boycott intensity is stronger in the Gulf Coast counties (i.e., supporting hypothesis 5(a)), in counties where our two measures of environmentalism are larger (i.e., supporting hypothesis 5(b)), and in areas where BP's pre-spill volume shares were more volatile; boycott intensity is weakly stronger in region 2 (between 100 and 300 miles from the Gulf) than in region 4 (at least 740 miles away from the Gulf), which is the only result that differs from those in Table 3; boycott intensity is weakly weaker in region 3 (between 300 and 740 miles from the Gulf) than in region 4.

The results in Table 4 indicate that during the post-spill weeks, boycott intensity is no longer stronger in the Gulf Coast counties, but boycott intensity is still stronger in counties where environmentalism is higher. The results in Table 4 also indicate that boycott intensity across all areas during the 31 post-spill weeks is much weaker than that during the 13 spill weeks, which is consistent with the results in Section 4.2.

In Section 4.2, we estimated weekly boycott effects (equation (5)) for all sampled areas. Here we estimate the weekly boycott effects for the Gulf Coast region only. The estimates are reported in Model 2 of the Appendix. The results indicate that the Gulf Coast region did not exhibit a boycott effect until the fourth week either. Most of the estimated boycott effects for the Gulf Coast region, up to week 23, are larger than those for all counties, which is consistent with expectations. However, after week 23, we essentially no longer observe boycott effects in the Gulf Coast region, but we continue to observe boycott effects if the sample includes all counties. Why is it that the boycott effect disappeared more quickly in the Gulf Coast region? A possible explanation is that BP's $20 billion trust fund started to accept claims on August 23, the 18th since-spill week. By week 24, a significant number of consumers in the Gulf Coast region may have received benefits from the BP trust fund.

Table 4. Effects of Proximity to the Gulf and of Environmentalism on Boycott Intensity during and after the Oil Spill

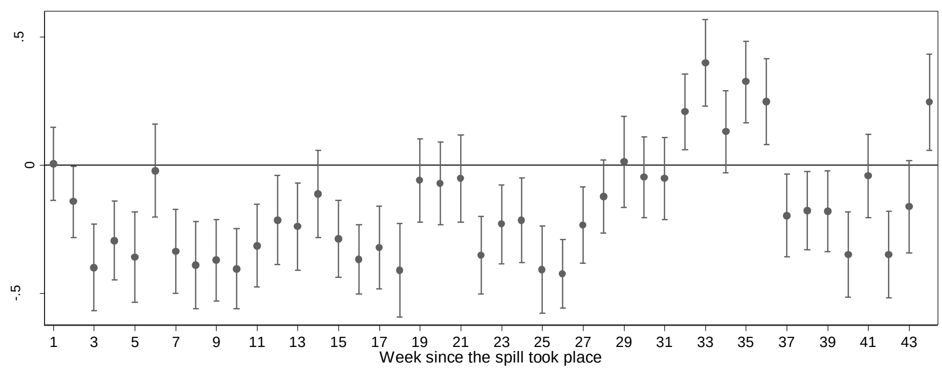
Explanatory variables	Model 1	Model 2	Model 3	Model 4	Model 5
During-spill time dummy	-0.099	-0.147	-0.153	-0.163	-0.119
	(0.122)	(0.133)	(0.122)	(0.121)	(0.133)
Post-spill time dummy	-0.252**	-0.388***	-0.254**	-0.240*	-0.367***
	(0.128)	(0.141)	(0.128)	(0.125)	(0.141)
During-spill * Gulf Coast	-0.533***	-0.501***			-0.555***
	(0.097)	(0.107)			(0.108)
Post-spill * Gulf Coast	-0.182*	-0.060			-0.102
	(0.104)	(0.114)			(0.116)
During-spill * distance is 0.1 to 0.3		-0.238			-0.293*
thousand miles from the Gulf		(0.149)			(0.150)
Post-spill * distance is 0.1 to 0.3		-0.174			-0.217
thousand miles from the Gulf		(0.153)			(0.154)
During-spill * distance is 0.3 to 0.74		0.162**			0.152*
thousand miles from the Gulf		(0.081)			(0.081)
Post-spill * distance is 0.3 to 0.74		0.356***			0.348***
thousand miles from the Gulf		(0.088)			(0.089)
During-spill * per capita donations			-23.961***		
to Green Party committees			(6.215)		
Post-spill * per capita donations to			-15.119**		
Green Party committees			(7.200)		
During-spill * % of individuals who				-157.066**	
gave to Green Party committees				(62.505)	
Post-spill * % of individuals who				-165.954**	
gave to Green Party committees				(69.381)	
During-spill * green index					-0.082***
					(0.020)
Post-spill * green index					-0.063***
					(0.023)
During-spill * variability of BP's	-0.369***	-0.351***	-0.348***	-0.334***	-0.359***
pre-spill volume share	(0.121)	(0.117)	(0.121)	(0.121)	(0.117)
Post-spill * variability of BP's	0.088	0.104	0.091	0.100	0.098
pre-spill volume share	(0.133)	(0.131)	(0.133)	(0.133)	(0.130)
Weekly # of BP oil spill news items	-0.229***	-0.229***	-0.229***	-0.229***	-0.229***
	(0.057)	(0.057)	(0.057)	(0.057)	(0.057)
Constant	21.875***	21.875***	21.935***	21.942***	21.883***
	(0.207)	(0.207)	(0.205)	(0.205)	(0.207)
Observations	28,784	28,784	28,784	28,784	28,784
R-squared	0.973	0.973	0.973	0.973	0.973

Notes: The dependent variable is BP's weekly volume share. All regressions include county fixed effects and county-specific time trends. Gulf Coast refers to counties that are within 100 miles of the Gulf of Mexico. Davidson and MacKinnon (1993) robust standard errors are in parentheses. Green index is the sum of the standardized values of our two primary measures of a county's environmentalism: per capita donations to Green Party committees and percentage of individuals who donated to Green Party committees. *** p<0.01, ** p<0.05, * p<0.1.

4.4. Price Differential

We use equation (5), where the dependent variable is BP stations' price differential with their competitors, to test hypothesis 6(a), that BP stations cut retail prices as a response to the oil spill. Figure 4 presents the estimated coefficients for the 44 weekly dummies and the associated 95 percent confidence interval. The full set of results is reported in Model 3 of the Appendix. The estimated coefficients from week 2 through week 28 are all negative, and the overwhelming majority of these coefficients are statistically significant at the 1 percent level or higher. The largest coefficient, in absolute value, is 0.42 cents per gallon in week 26. The estimated coefficients from week 29 on are no longer uniformly negative. These results indicate that BP stations reacted to the oil spill rather quickly, even though the magnitude of price cuts was not very large (i.e., hypotheses 6(a) is supported). Recall that BP announced at the end of week 10 that it would offer BP distributors a price support. The price support announcement does not appear to have led BP retail stations to cut their prices further.

Figure 4. Mean and 95-Percent Confidence Interval of Changes in BP's Price Differentials from Its Pre-Spill Level in Each of the 44 Since-Spill Weeks



We use equation (7), where the dependent variable is BP's price differential, to test hypothesis 6(b), that BP stations' price decreases were larger in the Gulf Coast region, and hypothesis 6(c), that price decreases were larger in areas with stronger environmental preference.

The estimates, reported in Model 1 of Table 5, indicate again that BP stations cut their retail prices during both the spill weeks and the post-spill weeks, and that the price cuts during the spill weeks were larger in the Gulf Coast counties or in counties between 100 and 300 miles away from the Gulf. These results support hypothesis 6(b) and provide additional support for hypothesis 6(a). During the post-spill weeks, however, price cuts were not necessarily larger in counties that are closer to the Gulf. The results for Model 1 of Table 5 also suggest that BP stations' price cuts were not larger in areas with stronger environmental preferences, which is inconsistent with hypothesis 6(c). We suspect the reason for this result is that BP retail stations did not take environmental preference into account when responding to the oil spill, which is more difficult to assess than geographic proximity. As we noted in Section 2, it was widely reported that BP's price rebates to its distributors were larger in the Gulf Coast region, but we are not aware of reports suggesting that BP's price rebates were larger in areas with stronger environmental preference.

Because BP stations cut their retail prices, the boycott effects estimated from equations (3) through (7) in the previous tables are expected to be smaller than the actual boycott effect if BP prices were held constant. Without the price cuts, the decreases in BP stations' share would be expected to have fallen more; BP stations' price cuts therefore may have prevented some consumers from switching to other brands and may even have attracted some consumers to the BP brand. Because BP stations' price cuts during the spill weeks were larger in the counties that are closer to the Gulf, the estimated difference in the boycott effect among counties is attenuated as well. Even though it is an endogenous variable, we add price differential as an explanatory variable into equations (6) and (7). The results are reported in Models 2 and 3 in Table 5, respectively.[15] As expected, the coefficients for the price differential variables are negative and statistically significant, suggesting price cuts reduce boycotting behavior. However, the estimated coefficients for other variables did not differ significantly from those in models not including price differentials.

[15] We do not estimate a system of price and quantity equations here because the estimates from such a system are exactly the same as the ones from estimating the equations separately; the explanatory variables in both equations are the same.

Table 5. Regressions with Price Differentials

	Model 1	Model 2	Model 3
Explanatory variables	Price differential	Volume share	Volume share
Flow rate		-0.014***	
		(0.003)	
Flow rate * Gulf Coast		-0.011***	
		(0.003)	
Flow rate * distance is between 0.1 and 0.3 thousand miles from the Gulf		-0.003	
		(0.003)	
Flow rate * distance is between 0.3 and 0.74 thousand miles from the Gulf		0.005**	
		(0.002)	
Flow rate * green index		-0.002***	
		(0.000)	
Flow rate * variability of BP's pre-spill volume share		-0.009***	
		(0.003)	
During-spill time dummy	-0.154*		-0.126
	(0.085)		(0.133)
Post-spill time dummy	-0.211***		-0.376***
	(0.077)		(0.141)
During-spill * Gulf Coast	-0.387***		-0.571***
	(0.096)		(0.109)
Post-spill * Gulf Coast	-0.002		-0.102
	(0.110)		(0.116)
During-spill * distance is between 0.1 and 0.3 thousand miles from the Gulf	-0.307***		-0.306**
	(0.089)		(0.150)
Post-spill * distance is between 0.1 and 0.3 thousand miles from the Gulf	-0.186**		-0.224
	(0.095)		(0.154)
During-spill * distance is between 0.3 and 0.74 thousand miles from the Gulf	0.012		0.153*
	(0.063)		(0.081)
Post-spill * distance is between 0.3 and 0.74 thousand miles from the Gulf	0.175**		0.355***
	(0.070)		(0.089)
During-spill * green index	0.040**		-0.080***
	(0.016)		(0.020)
Post-spill * green index	0.034**		-0.062***
	(0.017)		(0.023)
During-spill * variability of BP's pre-spill volume share	0.132***		-0.353***
	(0.049)		(0.117)
Post-spill * variability of BP's pre-spill volume share	0.085*		0.102
	(0.050)		(0.131)
Weekly # of BP oil spill news items	-0.165***	-0.118**	-0.236***
	(0.044)	(0.051)	(0.058)
Price differential		-0.054***	-0.042***
		(0.011)	(0.009)
Time trend	-0.001	-0.035***	-0.034***
	(0.002)	(0.006)	(0.004)
Constant	1.336***	22.029***	21.939***
	(0.138)	(0.231)	(0.208)
Observations	28,784	20,817	28,784
R-squared	0.671	0.975	0.973

Notes: All regressions include county fixed effects, county-specific time trends, and month fixed effects. Gulf Coast refers to counties that are within 100 miles of the Gulf of Mexico. Davidson and MacKinnon (1993) robust standard errors are in parentheses. Green index is the sum of the standardized values of our two primary measures of a county's environmentalism: per capita donations to Green Party committees and percentage of individuals who donated to Green Party committees. *** p<0.01, ** p<0.05, * p<0.10.

5. Conclusion

In this paper, we have highlighted how the level of egregiousness and consumers' preference associated with a given level of egregiousness affects market-level boycott intensity. Our conceptual contribution is to recognize that the egregiousness of negative events is analogous to the quality of goods and to distinguish egregiousness level from preference. This seemingly obvious yet novel insight allows us to model the concept that consumers may all agree on the ranking of egregiousness level but may differ in their disutility associated with a given level of egregiousness. Our empirical contribution is to treat the BP oil spill as a natural experiment in which we observe a series of events that differ in egregiousness level but are otherwise identical. Consistent with the predictions of our model, the BP oil spill triggered a market-level boycott effect only after its egregiousness exceeded a minimum level; boycott intensity then increased with its level of egregiousness; and variations in consumers' preference over egregiousness, approximated by a market's environmentalism and proximity to the Gulf, translated into market-level variations in boycott intensity.

By formally separating the level of egregiousness from consumers' preference over egregiousness, we highlight a new direction for boycott research that is concerned with what motivates consumers to boycott: separating "perceived egregiousness" into perception (i.e., preference) and egregiousness level and using survey methods to identify their separate effects on consumers' willingness to boycott. Such a study would need to ask consumers to assess a fair number of negative events with various degrees of egregiousness.

Our idea that the egregiousness of negative events is analogous to the quality of products also naturally invites questions that we have not attempted to study in this paper. To what extent are boycott-triggering events horizontally differentiated? What are the implications of horizontal differentiation in egregiousness on boycott intensity? Horizontal differentiation in egregiousness, which is analogous to horizontal differentiation in products, refers to the idea that consumers may not agree with each other on which negative event is more egregious.

By presenting evidence that boycott intensity varies with egregiousness level, we also highlight the need for researchers to control the egregiousness of the triggering events when studying the effect of boycotts on firms' stock price or behavior. Otherwise, such studies may suffer from omitted variable biases. For example, the BP oil spill caused a huge decrease in BP's market capitalization, from about $190 billion on April 20, 2010, to about $95 billion by mid-June 2010 (Aldy 2011). However, it would be wrong to attribute the stock price decrease to the BP gasoline boycott; rather, it was largely caused by the enormous legal liabilities generated by the oil spill. It may be difficult to measure the egregiousness of a sample of boycott-triggering events, but our results suggest that researchers must at least acknowledge the need to control the egregiousness of the triggering events when attempting to study the effect of boycotts on firms.

Our paper also has studied, both theoretically and empirically, the impact of media coverage on boycott intensity. We find evidence that the weekly intensity of media coverage has a positive effect on boycott intensity, and we interpret this finding to reflect that some consumers may infer the egregiousness level of the BP oil spill from the current intensity of media coverage. However, we did not find evidence that the cumulative amount of media coverage affects boycott intensity.

Our results—that market-level boycott effect did not arise until the egregiousness level of the oil spill was above some level, and that boycott intensity increases with the egregiousness level—suggest that firms' best defense against large-scale consumer boycotts is to adopt measures to avoid the occurrence of highly egregious events in the first place. When a negative event has already occurred and triggered a boycott, our finding that the BP boycott died out quickly after the spill was stopped suggests that the firm-specific boycott would stop quickly after the target firm addresses the underlying issue.

Our finding that variations in consumer/market characteristics can magnify into variations in boycott intensity at the market level suggests that firms might be able to adopt market-specific strategies to mitigate the impact of a boycott. BP-branded retail stations in the Gulf Coast region cut prices more, and BP offered those stations larger price supports, which are market-specific mitigating measures. However, BP retail stations in areas with stronger environmental preference failed to cut prices more.

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