DISCUSSION PAPER SERIES

DP12504

FIRMS AND COLLECTIVE REPUTATION: A STUDY OF THE VOLKSWAGEN EMISSIONS SCANDAL

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INDUSTRIAL ORGANIZATION

ISSN 0265-8003

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Discussion Paper DP12504
Published 13 December 2017
Submitted 12 December 2017

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JEL Classification: D12, D22, D90, L14, L15, L62

Keywords: automobiles, collective reputation, Demand estimation, difference-indifferences, Google trends, reputation externalities, Twitter sentiment, Volkswagen emissions scandal

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*We would like to thank Ward's Automotive and Networked Insights for providing data, as well as seminar and conference participants for helpful discussions. All errors are our own. E-mail contact: rbachman@nd.edu, gehrlich@umich.edu, yingfan@umich.edu, or dimitrije.ruzic@insead.edu. This version of the paper supersedes its first version titled "Firms and Collective Reputation: the Volkswagen Emissions Scandal as a Case Study" and published as CEPR-DP 12504 and CESifo-WP 6805 in December 2017.

1 Introduction

Do firms have economically important collective reputations? If so, collective reputations could influence outcomes for a group's members and, as a result, one firm's actions may have spillovers both on the reputations and on the outcomes of other firms in the group. In a seminal paper, Tirole (1996) develops a theoretical framework for modeling collective reputation showing that an original sin by elder group members can have long-lasting effects on a group. However, there is limited empirical evidence that group reputation and reputational externalities exist and that they matter economically, especially for firms. We fill this gap by identifying and quantifying group reputation externalities in an important setting.

We use the 2015 Volkswagen (VW) emissions scandal as a natural experiment and argue that it provides an ideal setting to study reputation externalities. On September 18, 2015, the U.S. Environmental Protection Agency (EPA) served a Notice of Violation to the VW Group alleging that approximately 500,000 VW and Audi diesel-engine vehicles sold between 2009 and 2015 in the United States contained a defeat device that allowed these vehicles to appear to comply with emissions regulations in the test box, while having higher on-road emissions.[1] This date marks the public eruption of one of the major industrial scandals in recent history, with a prolonged legal fallout in the United States, leading to approximately $15 billion in fines and other costs for VW (see Zycher (2017)).

Several features of the scandal make it an appealing natural experiment to study reputation spillovers: (1) For the general public, the scandal was a clear surprise in September 2015, and it immediately generated extensive media coverage. (2) The German auto manufacturers featured the notion of "German engineering" prominently in their U.S. advertising, creating a natural reputational group. (3) Individual automotive makes are salient to consumers, enabling us to use novel company-specific data on U.S. social-media sentiment and internet searches to directly establish the existence of reputational externalities.

Adding to its appeal as a natural experiment, the scandal occurred within an important setting: (4) The auto manufacturing industry is large and important in Germany. In 2014, the year prior to the scandal, vehicles amounted to 18 percent of Germany's total exports according to the German Federal Statistical Office (Destatis (2015)), and were thus Germany's largest export category. Also as of 2014, Germany captured by far the largest share of world vehicle exports in UN trade statistics (United Nations (2017)), with 22.7 percent in dollar and 18.5 percent in unit terms, followed by Japan with, respectively, 12.5 percent and 10.7 percent. (5) German vehicles are a large share of the U.S. market: in 2014 German auto manufacturers accounted for 8.1 percent of all U.S. light vehicle sales,

[1] The Volkswagen Group consists of Volkswagen proper plus Audi and Porsche.

making Germany the second-largest source for foreign-branded vehicles. (6) The scandal had serious public health consequences. Oldenkamp et al. (2016) estimate that the excess emissions caused by VW diesel vehicles cost 45,000 disability-adjusted life years, with a value of life lost of approximately $39 billion.[2] We add to this a calculation of the economic damage for the other German auto manufacturers. Finally, (7) the scandal also sparked a public discussion regarding the mechanism at the center of our paper: country-related reputational spillovers (see, for example, Bruckner (2015), Chambers (2015), Nienaber (2015), Werz (2016), and Remsky (2017)). Our paper provides numbers to this debate.

We conduct our study in three steps. In the first step, we use a difference-in-differences approach to provide evidence that there was a country-specific spillover from the VW scandal to the other German auto manufacturers. In the second step, we estimate a demand model to confirm and quantify this spillover effect. In the third step, we provide evidence that this spillover effect is best interpreted as a reputational spillover effect.

As the first step, we show that the VW scandal reduced the vehicle sales of the non-VW German auto manufacturers—BMW, Mercedes-Benz, and Smart—relative to their non-German counterparts.[3] We document this pattern by comparing how vehicle sales changed over time for non-VW German auto manufacturers relative to the non-German auto manufacturers. The scandal's differential impact on the non-VW German automakers is a robust feature of the data: it holds individually for each non-VW German auto manufacturer, and it exists for diesel and non-diesel sales alike.

This estimated differential impact of the VW scandal on the vehicle sales of the other German auto manufacturers is a result of three forces. First, the VW scandal led some consumers to substitute away from VW, increasing the vehicle sales of all other auto manufacturers; the extent of that substitution may have differed for German versus non-German auto manufacturers. Second, because the VW scandal centered on diesel vehicles, changes in consumers' preference for diesel may have differentially affected auto manufacturers based on their varying levels of exposure to the diesel market. Third, the scandal could have led to a substitution away from German auto manufacturers in the form of a systematic, country-specific spillover.

While the difference-in-differences approach does not quantify one channel separately from the others, our results suggest that the spillover effect is a key outcome of the scandal. For one, if other German makes are closer substitutes for VW than are non-German makes, substitution away from VW should have increased demand more for the other German makes than for the non-Germans. This substitution pattern would bias against our finding

[2] Barrett et al. (2015) estimate 59 premature deaths and a social cost of $450 million; Holland et al. (2016) estimate similar numbers.

[3] Opel, a German auto manufacturer and formerly a subsidiary of General Motors, does not sell in the United States.

of a negative difference-in-differences coefficient.[4] Moreover, our difference-in-differences results show a decline in the sales of the non-VW German auto manufacturers even when we exclude all diesel sales from the analysis, suggesting that the diesel channel alone cannot explain our finding. Taken together, these two arguments imply the existence of the third channel, that is, the spillover channel.

In the second step, we estimate a model of vehicle demand and use it to decompose the scandal's overall effect into its three constituent forces. The model allows consumers to value certain vehicle characteristics, such as a diesel engine or a German origin, differently before and after the VW scandal. In counterfactual simulations, we calculate vehicle sales for counterfactual worlds in which consumers did not change their valuations of some of these vehicle characteristics after the VW scandal. These counterfactual simulations allow us to quantify the scandal's overall effect and decompose it into the three forces.

We find that the VW scandal reduced the sales of the other German auto manufacturers and that this decline was principally driven by a country-specific spillover. Specifically, the overall effect on those manufacturers amounted to a decline in sales of 104,661 vehicles valued at $5.2 billion in 2016, based on the list prices in the data. Behind this overall effect was a larger country-specific spillover effect, which decreased the non-VW German auto manufacturers' 2016 sales by 472,084 units worth $26.5 billion. This decline from the spillover effect was partially offset by an increase in sales as consumers substituted away from VW, and it was reinforced by substitution away from diesel vehicles.

The finding that the spillover effect coexists with a countervailing substitution effect away from VW—and that, therefore, the spillover effect is larger in absolute value than the scandal's combined effects—is unlikely to be a coincidence. Firms that are associated closely enough to have a collective reputation (so that the spillover effect exists) are also likely to produce closer substitutes (which determines the substitution effect). Therefore, despite our focus on a specific scandal in a specific industry, the pattern we uncover is likely to hold more broadly.

In the third step of our study, we present several pieces of evidence to support our argument that the German-specific spillover is best interpreted as arising from a collective reputation. As background, we show that German automakers leveraged a notion of "German engineering" in their marketing. Moreover, media mentions of the phrase "German engineering" spiked after the scandal. We also use social media data from Twitter to document changes in sentiment toward the non-VW German automakers that are indicative of harm to their collective reputation. In addition, to rule out an alternative explanation that works through information instead of reputation, we use internet search data to show

[4]Subsequent demand estimates show that vehicle sales data indeed reflect this pattern of substitution.

that consumers did not exhibit any heightened interest toward the non-VW German auto manufacturers. By contrast, consumer search interest in Volkswagen displayed a large spike following the scandal. We provide further independent evidence that a U.S. customer would have had no technical or economic reason to believe that the other German auto manufacturers were implicated in the VW emissions scandal.

Our results thus substantiate the opening claim in Tirole (1996) that: "Collective reputations play an important role in economics and the social sciences. Countries [...] are known to be hard-working, honest, corrupt, hospitable or belligerent." We show that the actions of one member of a group of firms can materially damage the group's reputation, producing reputational externalities from the standpoint of individual firms. We thereby provide evidence for reputational spillover effects of major corporate scandals and their economic consequences.

Our results also relate to the theoretical work of Bordalo et al. (2013) on the importance of salient product features in consumer choice. The discovery of VW's malfeasance precipitated a major industrial scandal in which a German origin was a salient product attribute. The scandal arguably heightened the salience of a German origin in the market for light vehicles at the same time that it led consumers to reassess their valuations of this attribute, generating the reputational externalities we find.

We contribute to the literature by studying group reputation and reputation spillovers on the economic outcomes of firms. The focus on group reputation distinguishes our paper from an existing literature on individual reputation, including Cabral and Hortaçsu (2010), Li (2010), Mayzlin et al. (2014), Fan et al. (2016), Luca (2016), Luca and Zervas (2016), and Li et al. (2018).[5] In addition to this literature on individual reputation, Nosko and Tadelis (2015) study how the quality of a transaction between a seller and a buyer on eBay affects the buyer's probability to use the eBay platform again. Our paper also relates to other work studying the reputational effects of industrial scandals, such as Jonsson et al. (2009) for the Swedish finance industry, Freedman et al. (2012) for the U.S. toy industry, and Bai et al. (2018) for the Chinese dairy industry.[6]

Additionally, there is a small but growing literature that studies the economic conse-

[5]There is also a finance literature that studies how a variety of corporate events adversely affect firm enterprise values and interprets such effects as reputational losses; see, for example, Fiordelisi et al. (2014) for a summary of this literature. We supplement our main analyses, which document spillovers to economic activity in terms of vehicle sales and direct measures of consumer sentiment, with a similar event study using stock prices in appendix D.

[6]The aforementioned event study literature using financial data has also recently studied spillovers from corporate events; see, for example, Gleason et al. (2008) and Kang (2008). There is also an agricultural economics literature studying the group reputation of regional appellations such as Bordeaux Wines: Castriota and Delmastra (2014) on the determinants of collective reputation, and Landon and Smith (1998) on the correlation of prices and collective reputation. Finally, Yu et al. (2002) and Yu and Lester (2008) study reputation spillover theoretically from a management perspective.

quences of the Volkswagen emissions scandal. Strittmatter and Lechner (2017), Ater and Yoseph (2018), and Che et al. (2018) focus on the scandal's effect on Volkswagen vehicles as opposed to spillovers to other auto manufacturers, and they examine the used vehicle market rather than the new vehicle market. Griffin and Lont (2018) and Barth et al. (2019) study the scandal's effects on equity, bond, and credit default swap markets for VW and other large automakers. Finally, Alexander and Schwandt (2019) use the geographic distribution of diesel vehicles involved in the emissions scandal as exogenous variation for local pollution to study the health effects of vehicle exhaust.

Our study also speaks to three additional strands of literature: First, a recent literature in international macroeconomics, for instance di Giovanni and Levchenko (2012), di Giovanni et al. (2014) and di Giovanni et al. (2018)), emphasizes the importance of large international firms for aggregate fluctuations and international comovement; our results suggest that misbehavior at such firms can damage the collective reputation of particular national powerhouse industries and thus may contribute to these fluctuations. Second, the international economics literature has examined the extent to which taste shocks for domestic versus foreign goods can explain the comovement of international business cycles (Stockman and Tesar (1995)). Our results suggest that the misbehavior of large multinational firms might generate such taste shocks through reputational spillovers. Third, our results provide a case study for the recent macroeconomic literature on customer capital; our evidence shows how customer capital can decline through reputational spillovers and quantifies the economic consequences of such a loss (Drozd and Nosal (2012) and Gourio and Rudanko (2014)).

The remainder of the paper is organized as follows: section 2 provides a more detailed explanation and timeline of the VW emissions scandal and describes the scandal's effect on VW. Section 3 provides difference-in-differences estimates that suggest the existence of a German-specific spillover effect. Section 4 presents a model of vehicle demand and quantifies the spillover effect. Section 5 provides support for our interpretation of the spillover effect as a reputational spillover, and discusses alternative interpretations. A final section 6 concludes.

2 The VW Emissions Scandal as a Natural Experiment

In this section, we describe the timeline of the VW emissions scandal in more detail and argue that it provides a good setting to study the spillovers arising from collective reputation. Using data from print publications, the stock market, and social media, we show that the scandal was largely unanticipated. We then provide evidence substantiating the claim that German auto manufacturers share a group identity.

2.1 Timeline of the Scandal

In May 2014, West Virginia University's Center for Alternative Fuels Engines and Emissions found discrepancies between high on-road emissions by VW diesel vehicles and earlier test results. The EPA and the California Air Resources Board (CARB) permitted a voluntary recall of VW diesel vehicles in December 2014. In May 2015, CARB conducted new tests, and again the on-road emissions failed to match the test-box results for VW diesel vehicles. In July 2015, the agencies informed VW about these tests and threatened not to certify the 2016 diesel vehicles. On September 3, 2015, VW admitted to the EPA and CARB that it had used a defeat device in its software, which regulated emissions and produced fake test results in the test box (see Breitinger (2018) for a more complete timeline). The scandal entered its public phase on September 18, 2015, when the EPA served a Notice of Violation to the Volkswagen Group.

Volkswagen's culpability quickly became a matter of public knowledge: on September 20, two days after the start of the scandal, Volkswagen admitted publicly to the deception and issued an apology. VW Chief Executive Officer Martin Winterkorn resigned three days later, on September 23.[7] On September 28, German authorities opened a fraud investigation of the former CEO, and in October they authorized a police raid on the VW headquarters. The U.S. Congress called the VW U.S. CEO Michael Horn to testify on October 8, 2015, and he formally resigned his post in early March 2016. In anticipation of the fines and settlements associated with the scandal, VW set aside more than $18 billion in fiscal year 2015. The scandal's legal resolution in the United States began in April 2016. On July 26, 2016, VW and a U.S. court agreed on a civil settlement totaling $15 billion.

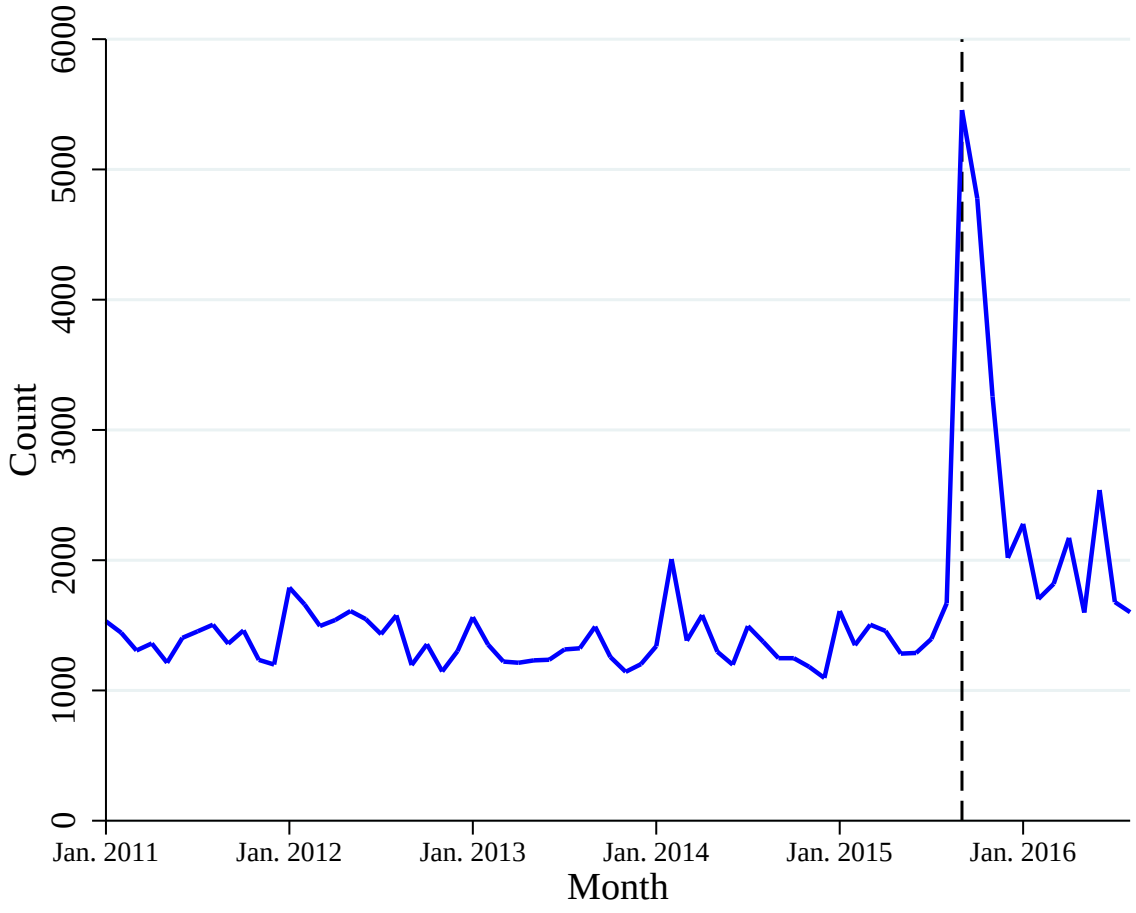
Major news outlets across many countries covered the scandal and its aftermath. On September 19, the morning after the scandal, the front page of the New York Times read: "U.S. Orders Major VW Recall Over Emissions Test Trickery." The Wall Street Journal used a more accusatory tone: "Volkswagen Faked EPA Exhaust Test, U.S. Alleges." Spiegel Online and Zeit Online, the online platforms of two major German newspapers, frequently reported about the scandal, which also quickly spilled over into popular culture. For example, on October 13, 2015, Paramount Pictures and Leonardo DiCaprio's production company announced that they had secured the rights to shoot a film about the scandal, and on September 22, 2016, VW was awarded the satirical Ig Noble Prize in chemistry (Improbable Research (2016)).

[7] He was charged with fraud in the United States in May 2018.

2.2 The Scandal Surprised the General Public

Monthly print media mentions of "Volkswagen" more than tripled in September 2015, suggesting that the scandal came as a complete surprise to the general public. We quantify the media prominence of the scandal using data from the Newsbank news aggregator on print media mentions of "Volkswagen" in the United States. The database covers roughly 5,000 U.S. newspapers, newswires, journals, and magazines. Figure 1 shows that mentions of "Volkswagen" spiked from a pre-scandal monthly average of 1,500 to 5,500 in September 2015. This sudden increase suggests that the scandal caught the media and public by surprise.

Figure 1: Monthly Print Media Mentions of "Volkswagen" in the United States



Note: Dashed line shows the month of the Volkswagen emissions scandal, September 2015. Data come from the Newsbank news aggregator, which covers roughly 5,000 U.S. newspapers, newswires, journals, and magazines. Time period covered is January 2011 to August 2016.

Along with the adverse attention in the media, VW's stock price declined precipitously following the EPA's announcement; the visually evident discontinuity on September 18 in

figure 2 suggests that the scandal came as a surprise to market participants. Volkswagen's end-of-day stock price fell by 33 percent in the two trading days following the scandal.[8] The stock price subsequently recovered some of its losses over the rest of the year, but at the end of August 2016 it remained 24 percent lower than its pre-scandal closing price.

Figure 2: End-of-Day Stock Price for Volkswagen Group

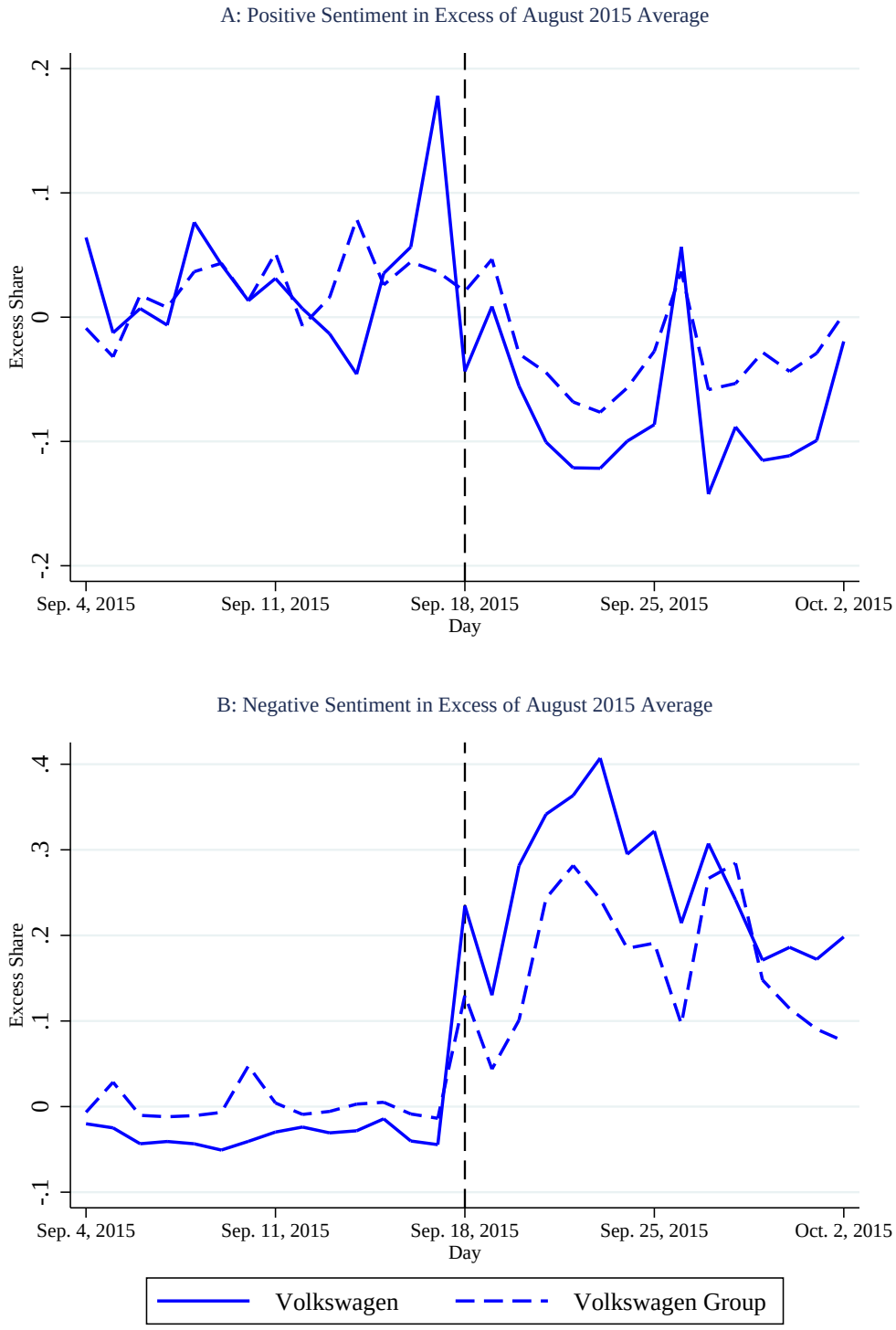
Note: Dashed line shows the date of the Volkswagen emissions scandal, dated September 18, 2015. End-of-day price shown for Volkswagen ADR listed on U.S. stock exchanges. Data come from the Bloomberg database. Time period covered is January 2011 to August 2016.

Furthermore, the tone of social media discussion regarding Volkswagen suddenly shifted, with positive sentiment declining and negative sentiment spiking in the aftermath of the scandal. We document this point with novel sentiment measures from Networked Insights.[9] We focus on sentiment data from Twitter, an online social media networking service where some 300 million active monthly users share short messages. The sentiment

[8]To focus on the effects within the United States and to avoid currency effects from the euro-based VW listing on the Frankfurt Stock Exchange, we use the price of the VW American Depository Receipt (ADR) traded on U.S. markets. ADRs are issued by a U.S. depository bank and entitle the owner to shares in an international security; they are priced and pay dividends in U.S. dollars, and are traded through broker-dealers.

[9]Networked Insights is a data analytics company, founded in 2006, that provides a platform for real-time semantic analyses of social media posts; its primary clients are consumer-facing companies that use the platform to manage their brands.

Figure 3: Daily Twitter Sentiment Towards Volkswagen



Note: Dashed vertical lines show the date of the Volkswagen emissions scandal, dated September 18, 2015. The figure shows the normalized shares of tweets expressing positive/negative Twitter sentiment towards a particular make. The denominator of these shares includes positive, negative and neutral sentiments. Sentiment shares are normalized by subtracting the average sentiment share from August 10 to August 31, 2015. We show a window of ± 14 days around September 18, 2015. Volkswagen Group is defined as Volkswagen, Audi, and Porsche. Data come from Networked Insights.

measures in our data set are calculated from a 10 percent random sample from Twitter. Networked Insights categorizes tweets as displaying positive, neutral, or negative sentiment toward the mentioned company. Posts are excluded from the analysis if they are not written in English or if the user accounts are associated with locations outside the United States. Networked Insights also constructs brand identifiers. An identifier for Volkswagen, for instance, is meant to collate mentions of "Volkswagen," "VW," "#Volkswagen," and the like. Given the size of the underlying data set, Networked Insights only retains the past 13 months of data. We requested the data in September 2016, so our time series begins on August 10, 2015, a little over a month before the scandal became public. We first create average daily sentiment shares (positive/negative/neutral) for August 2015 for each vehicle make in our data to serve as a pre-scandal baseline. We then construct sentiment shares in excess of this August baseline for each day.

Figure 3 displays these sentiment metrics for VW and the VW Group two weeks before and after the scandal. Panel A shows a decrease in positive sentiment toward VW, from an average of 3 percentage points higher than its August baseline in the two weeks prior to the scandal to an average of 8 percentage points below in the two weeks following the scandal. Panel B displays a sharp increase in negative sentiment toward VW: from an average of 3 percentage points below to an average of 26 percentage points above.[10] The results for the entire Volkswagen group (which includes Audi and Porsche) are similar. Together, these two panels suggest that Volkswagen's reputation suffered in the aftermath of the September 18 EPA announcement.

2.3 "German Engineering" as a Group Identity

Having established that the VW scandal was a shock and that it affected VW's reputation, we now provide evidence that there is a collective German reputation through which the scandal may have had a spillover effect on the other German automakers.

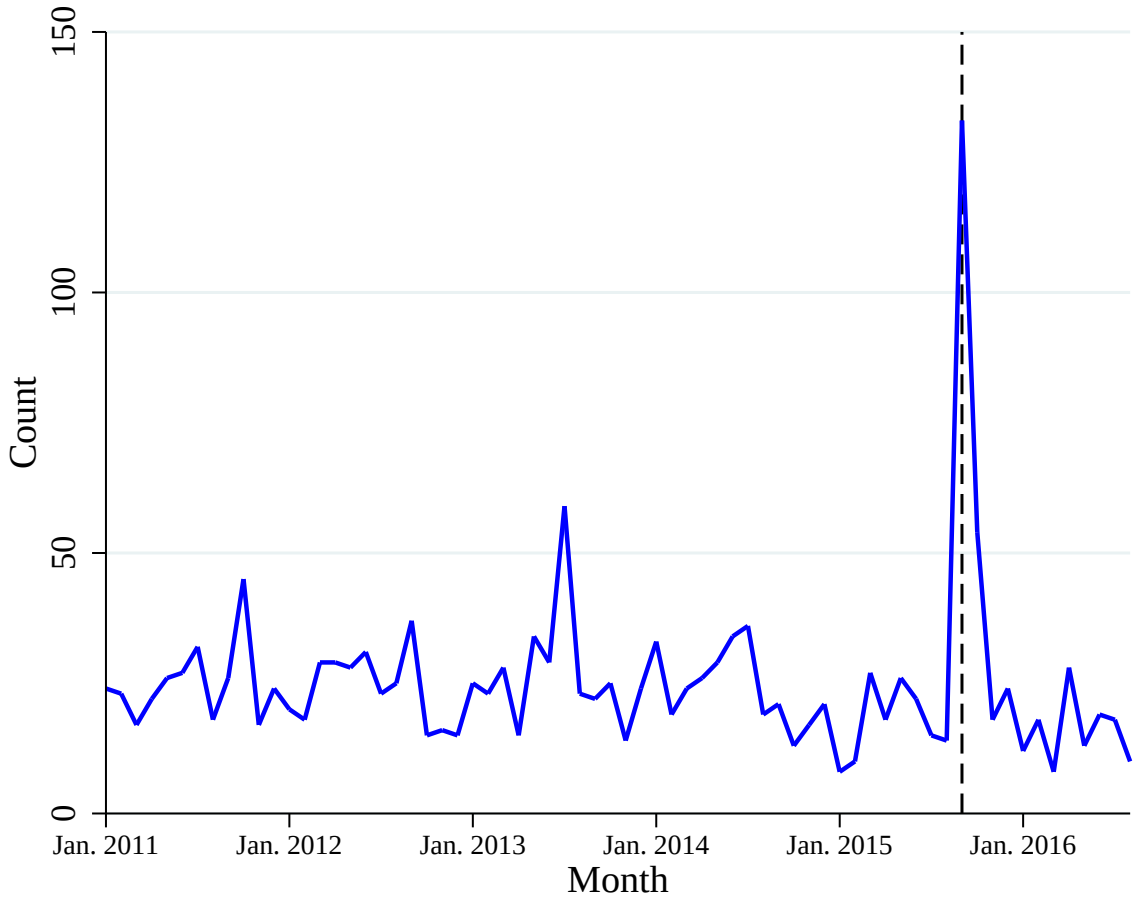
We first note that German auto manufacturing companies have historically leveraged the broader reputation of "German engineering" in their marketing. For instance, a VW commercial from 2014 states, "... Everyone knows that the best cars in the world come from Germany." The ad fades out to the question: "Isn't it time for German engineering?", and then pivots to the German phrase "Das Auto" ("The Car"), presumably in order to associate VW and "German engineering" with the idea of the archetypical car.

[10]The pre-scandal and post-scandal means are statistically different at the 1 percent significance level for both positive and negative sentiment.

It is, therefore, not surprising that following the scandal, media attention to "German engineering" spiked, with 130 print articles mentioning the term in September 2015, a five-fold increase over the preceding months. We illustrate this increase in figure 4 using data from the Newsbank aggregator. A recurring theme in this news coverage was the notion that the scandal might tarnish the broader reputation of German manufacturing firms. As part of this coverage of the scandal, Reuters published an article on September 22, 2015, titled "VW scandal threatens 'Made in Germany' image" (Chambers (2015)). A day later, Reuters doubled down with an article titled "Volkswagen could pose bigger threat to German economy than Greek crisis" (Nienaber (2015)), which included the claim: "The broader concern for the German government is that other car makers such as Mercedes-Benz and BMW could suffer fallout from the Volkswagen disaster."[11]

[11] See also Bruckner (2015), Werz (2016), and Remsky (2017).

Figure 4: Monthly Print Media Mentions of "German Engineering" in the United States



Note: Dashed line shows the month of the Volkswagen emissions scandal, September 2015. Data come from the Newsbank news aggregator, which covers roughly 5,000 U.S. newspapers, newswires, journals, and magazines. Time period covered is January 2011 to August 2016.

3 Difference-in-Differences Evidence on the Spillover Effect

In this section, we present difference-in-differences evidence that the VW emissions scandal had a spillover effect on the other German auto manufacturers (BMW, Mercedes-Benz, and Smart). We show that the scandal substantially reduced the U.S. sales growth of the other German automakers relative to their non-German counterparts. Note that because VW vehicles and other vehicles are potential substitutes, there may not be a cleanly untreated control group for a difference-in-differences analysis. Therefore, we interpret these results as uncovering the relative effects of the scandal for the non-VW German and non-German automakers. Nonetheless, we argue that the difference-in-differences results

do provide evidence for the existence of a German-specific spillover, despite the presence of two other mechanisms. In the ensuing section 4, we estimate a model of vehicle demand to quantify the sales decline of the non-VW German automakers caused by the scandal and to isolate the spillover effect.

3.1 Regression Results

To study the effects of the VW emissions scandal, we obtain data on light vehicle sales from WardsAuto, one of the premier automotive industry publications. WardsAuto receives sales data from all auto manufacturers in the United States. It is thus in principle a complete count of light vehicle sales in the United States.[12] An individual observation in the data contains identifiers for the vehicle make (e.g., Honda or Volkswagen), the vehicle model (e.g., Civic or Jetta), and the vehicle powertype (e.g., gas or diesel). The data set contains 37 makes, listed in appendix table A.1, and 357 distinct models. The sample period is January 2010 to August 2016. We identify six makes as of German origin: Audi, BMW, Mercedes-Benz, Porsche, Smart, and Volkswagen.[13]

We use a difference-in-differences regression specification to show how the scandal affected German auto manufacturers relative to the non-German auto manufacturers. We begin the analysis by constructing a total sales measure for each make, so that an observation is a make-month (e.g., Honda in January 2016). We focus on 12-month log sales growth, which parsimoniously accounts for make-specific trends and make-month-specific seasonality in the level of vehicle sales; they start in January 2011. Following a standard difference-in-differences regression specification (e.g., Angrist and Krueger (1999)), we estimate the following regression:

$$y_{it} = \beta_i + \gamma_t + \delta T_{it} + \varepsilon_{it}, \tag{1}$$

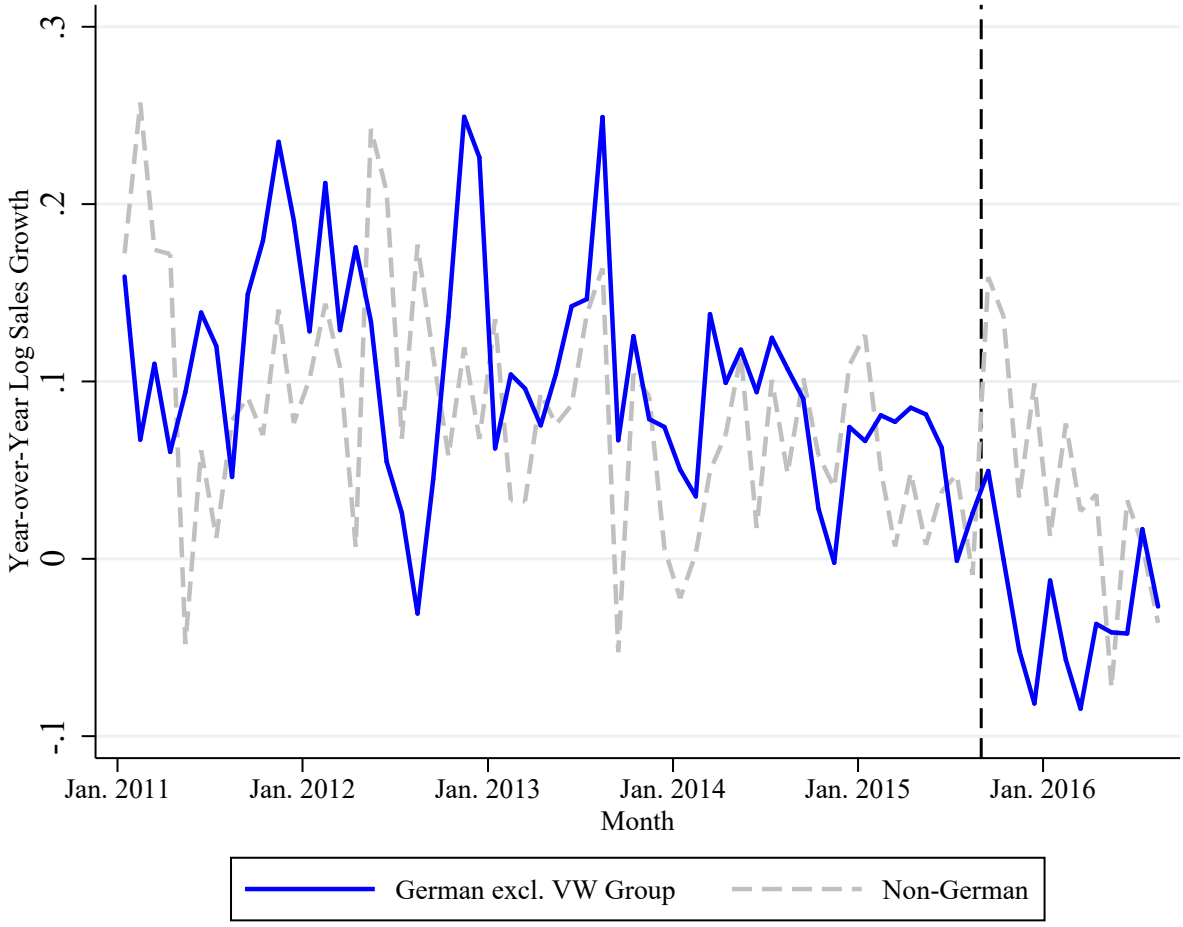
where $y_{it} = \ln \mathit{Sales}_{it} - \ln \mathit{Sales}_{it-12}$ is the 12-month log sales growth rate of vehicle make i at time t. β_i is a make-specific fixed effect, capturing make-level heterogeneity in growth rates. γ_t is a fixed effect for each month in the sample, capturing seasonality in vehicle sales and the potential impacts of time-varying fuel prices. T_{it} is an indicator taking value one for the German auto manufacturers on and after the scandal date, and zero otherwise.

[12] The official U.S. vehicle sales statistics in the national accounting data are based on the same data we use.

[13] Mini, the present-day incarnation of a line manufactured by the British Motor Corporation and its successors between 1959 and 2000, is currently owned by BMW. Given its historical association with Britain, we classify Mini as not of German origin. We consider alternative classifications in appendix B.1 and show that the results are not sensitive to this choice.

We exclude the Volkswagen Group from the sample to focus the analysis on the economic consequences of reputation for German automakers not directly implicated by the scandal. We weight this regression by the square root of sales volumes to dampen the impact of highly volatile sales growth rates of small sales levels. Figure 5 shows that the pre-scandal trends in sales growth for the non-VW German and non-German auto manufacturers are comparable, suggesting the non-Germans as a reasonable comparison group for studying the scandal's effects.[14] The coefficient of interest, δ, captures the scandal's differential impact on non-VW German auto manufacturers relative to non-German auto manufacturers.

Figure 5: Differences in U.S. Light Vehicle Sales Growth



Note: Dashed vertical line shows the month of the Volkswagen emissions scandal, September 2015. Data come from Ward's Automotive. Volkswagen Group is defined as Volkswagen, Audi, and Porsche.

[14] Appendix B.2 adds some additional evidence for this assessment.

The estimation results in table 1 show that the scandal reduced the sales growth rates of the non-VW German automakers by 9.2 percentage points relative to their non-German counterparts (column (1)). Furthermore, columns (2)–(4) document that the scandal reduced sales growth for each of the three non-VW German auto manufacturers individually: BMW's sales growth rate by 13.1 percentage points, Mercedes-Benz's by 5.5 percentage points, and Smart's by 30.3 percentage points. Appendix B.3 shows that our finding of a relative decline in the non-VW German automakers' sales is robust to estimating an unweighted regression, to using a different growth rate measure, and to estimating an alternative regression in which we use log sales levels as the dependent variable and include make-specific time trends and seasonal patterns.

Table 1: Difference-in-Differences Estimates—U.S. Light Vehicle Sales Growth German vs. Non-German Auto Manufacturers, Excl. VW Group

Dependent Variable	12-month Log Sales Growth			
	Baseline	BMW	Mercedes-Benz	Smart
	(1)	(2)	(3)	(4)
German × Post-Scandal	-0.092	-0.131	-0.055	-0.303
	(0.030)	(0.011)	(0.011)	(0.012)
Time Fixed Effects	Yes	Yes	Yes	Yes
Make Fixed Effects	Yes	Yes	Yes	Yes
R^2	0.303	0.307	0.306	0.307
N	2150	2014	2014	2014

Note: Unit of observation is a make-month (e.g., the log growth of all BMW sales from January 2014 to January 2015). Time period covered is January 2011 to August 2016. Standard errors clustered at the make level in parentheses. VW Group (VW, Audi, and Porsche) excluded from all regressions. The comparison group in all four columns is the non-German automakers. VW emissions scandal dated September 18, 2015. Sales are measured in units sold. All regressions are weighted by the square root of sales volumes. Data come from Ward's Automotive.

3.2 Discussion of the Spillover Effect

In this section, we review three forces through which the VW emissions scandal could have driven the relative decline in vehicle sales documented in the previous section, and we provide suggestive evidence that a country-specific spillover exists even when taking into account the two other forces.

The first force is substitution away from VW: the scandal led consumers to substitute away from VW to other makes. This substitution effect should have *increased* sales for both

the other German and the non-German auto manufacturers. However, figure 5 shows that sales for the non-VW German auto manufacturers *declined* following the scandal. Moreover, the strength of this effect may have differed for the two groups. If VW vehicles are more closely substituted by German rather than non-German vehicles—as our estimates in section 4 will show—then the substitution away from VW should have increased German sales by more than non-German sales. Therefore, if this substitution effect had been the only operative force, the difference-in-differences coefficient estimate in the previous section should have been positive. Our estimated coefficient of –9.2 percent thus suggests that substitution away from VW alone cannot explain the effect of the scandal documented in the previous section.

The second force is substitution away from diesel vehicles arising from the scandal's origins in the diesel market. The strength of this effect may have differed across automakers based on their different exposures to that market. To assess whether the non-VW German automakers' relative decline in sales was fully driven by this force, we repeat the difference-in-differences regression for diesel sales and non-diesel sales separately. The estimation results in table 2 show that the scandal reduced German automakers' sales growth of non-diesel vehicles by 8.4 percentage points and that of the diesel vehicles by a larger—albeit less precisely estimated—21.5 percentage points relative to that of non-German auto manufacturers. While the latter result on diesel sales confirms that this

Table 2: Difference-in-Differences Estimates Separating Diesel and non-Diesel German vs. Non-German Auto Manufacturers, Excl. VW Group

Dependent Variable	12-month Log Sales Growth		
Power Type	Baseline	non-Diesel	Diesel
	(1)	(2)	(3)
German × Post-Scandal	-0.092	-0.084	-0.215
	(0.030)	(0.033)	(0.126)
Time Fixed Effects	Yes	Yes	Yes
Make Fixed Effects	Yes	Yes	Yes
R^2	0.303	0.300	0.295
N	2150	2150	428

Note: Unit of observation is a make-month (e.g., the log growth of all BMW sales from January 2014 to January 2015). Time period covered is January 2011 to August 2016. Standard errors clustered at the make level in parentheses. VW Group (VW, Audi, and Porsche) excluded from all regressions. VW emissions scandal dated September 18, 2015. Sales are measured in units sold. All regressions are weighted by the square root of sales volumes. Data come from Ward's Automotive.

force may contribute to our estimate, the negative and statistically significant decline in non-diesel sales growth indicates that substitution away from diesel cannot, on its own, explain the scandal's relative effects on the other German automakers; a third channel must have been at play.[15]

The third force could be a reputational spillover, consistent with the aforementioned notion of "German engineering" and the surge in the media mentions of "German engineering" following the scandal. In sum, even though the substitution forces away from diesel and away from VW to non-VW German vehicles relative to non-German vehicles were likely at play, they cannot on their own explain the difference-in-differences results. We consider this evidence as strongly suggestive of the existence of a German-specific reputational spillover from the scandal.

Nonetheless, the analyses in this section do not allow us to quantify the spillover independently of the two substitution forces nor to interpret it unambiguously as arising from collective reputation. We therefore estimate a model of vehicle demand in the next section and conduct counterfactual simulations to quantify separately the sales loss to the non-VW German auto manufacturers from each of the three forces. We finalize our argument in the subsequent section 5 by providing evidence suggesting that the spillover effect indeed arose from collective reputation.

4 Demand Estimation and Decomposition: Quantifying the Spillover Effect

In this section, we estimate a model of vehicle demand and use it to decompose the scandal's impact on the sales of non-VW German automakers into three forces: substitution away from VW , substitution away from diesel, and the spillover effect.

4.1 Demand Model

Demand for vehicles is captured by a nested-Logit model where the indirect utility u_{ijt} that consumer i derives from purchasing a vehicle j in year t is given by:

$$u_{ijt} = \alpha p_{jt} + \boldsymbol{x}_{jt}\boldsymbol{\beta} + \xi_{jt} + \varepsilon_{ijt}, \tag{2}$$

where p_{jt} is the price of vehicle j in year t, and the vector $\boldsymbol{x}_{jt}$ captures key observable attributes of the vehicle, for example, miles per gallon and vehicle weight. $\boldsymbol{x}_{jt}$ also in-

[15]Also note that Smart never sold diesel vehicles in the United States, yet, as table 1 shows, was still impacted by the scandal.

cludes fixed effects at the make, power type, and year levels, as well as make-specific time trends. Finally, $\boldsymbol{x}_{jt}$ contains interaction terms such as *VW*$_j \times$ *Post-Scandal*$_t$: the interaction of *VW*$_j$, which takes the value 1 if the make of vehicle j is Volkswagen and 0 otherwise, and *Post-Scandal*$_t$, which takes the value 1 if year t is after the scandal and 0 before the scandal. Other such interaction terms are *Other German*$_j \times$ *Post-Scandal*$_t$ and *Diesel*$_j \times$ *Post-Scandal*$_t$. The model thus allows the scandal to affect vehicle demand through these interactions with an indicator for the post-scandal period. The term ξ_{jt}, captures vehicle characteristics that are not observable to the econometrician but are known to consumers and auto manufacturers. Finally, ε_{ijt} is an idiosyncratic taste shock. Consumers have an outside option of not buying a vehicle, the utility of which we normalize to $u_{i0t} = \varepsilon_{i0t}$.

Given our focus on collective reputation at the country level, we view a nested-Logit model with country-level nests as a parsimonious way to allow vehicles with the same country origin and those with different country origins to have different degrees of substitutability. Specifically, we assume that idiosyncratic tastes ε_{ijt} follow a generalized extreme value distribution that reflects this different substitutability. That is, ε_{ijt} and $\varepsilon_{ij't}$ are allowed to be correlated if and only if the makes of j and j' have the same country of origin. Following Berry (1994), the nested-Logit model gives us the following estimation equation:

$$\ln s_{jt} - \ln s_{0t} = \alpha p_{jt} + \boldsymbol{x}_{jt}\boldsymbol{\beta} + \sigma \ln s_{j|gt} + \xi_{jt}, \tag{3}$$

where $(s_{jt}, s_{0t}, s_{j|gt}, p_{jt}, \boldsymbol{x}_{jt})$ are data, $(\alpha, \boldsymbol{\beta}, \sigma)$ are parameters to be estimated, and ξ_{jt} is the error term. We use s_{jt} to denote the market share of vehicle j in year t, s_{0t} to denote the share for the outside option of not purchasing a vehicle, and $s_{j|gt}$ to denote the market share of vehicle j within the total sales of all vehicles from country g.[16] The nesting parameter $\sigma \in [0, 1]$ captures the correlation in preferences for products of the same country of origin. Large values of σ indicate stronger correlation and more substitution within the same country of origin; as σ approaches zero, the model becomes a standard Logit model.

4.2 Data and Estimation

To estimate our model of vehicle demand, we supplement the WardsAuto sales volume data described in section 3.1 with another dataset from WardsAuto that contains information on vehicle characteristics and list prices (i.e., manufacturers' suggested retail prices) for all vehicles between 2010 and 2016. Unlike the sales volume data, which is at the

[16] Specifically, in our estimation, the countries of origin are Germany, Japan, Korea, the United States, and all others. We combine Italy, UK and Sweden into an "others" group because each of them provides only a few vehicles to the U.S. market. When we unpack this group, we obtain similar results but some estimates are less precise.

monthly frequency, the vehicle characteristics and price data is at the annual frequency. Consequently, much like the broader literature on estimating vehicle demand (for example, Berry et al. (1995), and Berry et al. (1999)), we estimate the model at an annual level. Since the scandal became public in September 2015, we define all years prior to 2015 as pre-scandal, 2016 as post-scandal, and exclude 2015 from our estimation sample.

To simplify notation, we designate a vehicle—indexed by j in section 4.1—as a make-model-power type (e.g., a Honda Civic with a gasoline engine). The annual vehicle characteristics and price data are reported at the vehicle-trim level, as opposed to the monthly sales volume data, which are reported at the vehicle level. We thus aggregate the price and characteristics data for each vehicle, as a baseline, through the median across trim options.[17] We also investigate and report the robustness of our results with respect to two different aggregation methods by using the mean or the minimum across trims.

Following the aforementioned literature, we define market size by the number of households in the United States. We use the all-items Consumer Price Index for all urban consumers published by the Bureau of Labor Statistics to express prices in constant 2015 dollars. Appendix C.1 provides summary statistics of prices and characteristics.

Because the price p_{jt} and the within-group market share $s_{j|gt}$ of a vehicle might be correlated with the unobservable demand shock ξ_{jt}, ordinary least squares estimation of equation (3) could lead to biased estimates of the model coefficients. Consequently, we use an instrumental-variables approach. Specifically, we construct instrumental variables based on the characteristics of competing products, as in the work of Berry et al. (1995). In the oligopolistic market for automobiles, the price and market share of vehicle j depend not only on the vehicle's own characteristics, but also on the characteristics of all the other vehicles available in the market. This correlation between the characteristics of other vehicles and the potentially endogenous variables (p_{jt}, $s_{j|gt}$) makes these characteristics relevant instruments. Under the timing assumption that automakers decide the characteristics of their vehicles before the realization of the demand shocks, the characteristics of other vehicles are uncorrelated with vehicle j's demand shock, and hence they are also exogenous instruments. Appendix C.2 presents the first-stage regressions for prices and within-group market shares, and details the instrumental variables we use.

Table 3 reports our estimation results for the nested-Logit model in equation (3). In the baseline specification in column (1), we include separate interactions of the *Post-Scandal*

[17]For instance, Hyundai supplied three trims of the Genesis four-door sedan in 2016: the Genesis 3.8, the Genesis 3.8 AWD (all-wheel drive), and the Genesis 5.0 Ultimate. Their highway miles per gallon were 18, 16, and 15, respectively, so in our baseline specification we would assign a value of 16 miles per gallon to the 2016 Hyundai Genesis. Our dataset does not break out model sales by trim type, so we cannot calculate sales-weighted average characteristics for each model.

indicator with indicators for VW, Audi, Porsche, and with a single indicator for the group of non-VW German auto manufacturers to capture how the scandal affected the consumer valuations of these makes. Subsequently, in column (2), we unpack the set of non-VW German auto manufacturers and include separate interactions for BMW, Mercedes-Benz, and Smart. In columns (3) and (4), we re-examine the baseline specification with, respectively, mean and minimum aggregation of characteristics across trims. All specifications include make fixed effects,[18] power-type fixed effects, and year fixed effects to capture heterogeneity at these levels that may not be captured by other covariates in the specification. All specifications also include a make-specific time trend.

The estimated coefficients in table 3 show that consumer utility decreases with a vehicle's price and increases with a vehicle's size, fuel efficiency, and horsepower/weight. Comparing results across the four columns, the coefficient estimates are stable. Therefore, in what follows, we take column (1) as our baseline.

The estimated parameters show that the scandal reduced consumer valuations for makes in the VW Group, with the strongest decline for Volkswagen proper. Specifically, the estimated coefficient on $VW_j \times \textit{Post-Scandal}_t$ is negative (–0.275) and statistically significant, implying a decrease in consumer valuation of VW after the scandal. To interpret the magnitude of this coefficient, we compute the marginal effect of the $VW_j \times \textit{Post-Scandal}_t$ term as follows: we set this term to be zero while holding all other variables and coefficients constant, and recompute VW sales in 2016, the post-scandal year. The simulated VW sales are 989,446, while the sales in the data are 322,902. The marginal effect is therefore a 67% decline $\big($i.e., (989,446-322,902)/989,446$\big)$ in VW sales.

Reinforcing the evidence from section 3, the estimates from the vehicle-demand model show that the scandal generated negative spillovers to the other German auto manufacturers. The estimated coefficient for $\textit{Other German}_j \times \textit{Post-Scandal}_t$ in column (1) is negative and statistically significant, with a marginal treatment effect corresponding to a 41% decline in sales for the non-VW German auto manufacturers. In column (2) we look at the changes in valuation for these German auto manufacturers—BMW, Mercedes-Benz, and Smart—separately. We again find a negative effect of the VW scandal on the consumer valuation of each of these three automakers.

We also find a decline in consumers' tastes for diesel vehicles following the VW scandal. The marginal effect of $\textit{Diesel}_j \times \textit{Post-Scandal}_t$ was a 61% sales decline for diesel vehicles. Given the scandal's origin in the diesel segment, this decline is expected.

The estimated nest parameter σ is 0.85, indicating that consumers viewed vehicles within the same country grouping as closer substitutes than vehicles across country group-

[18] We group makes with fewer than ten observations in our sample into an "other make" indicator.

Table 3: Demand Estimation Results—U.S. Light Vehicle Sales

Trim Aggregation	Median	Median	Mean	Min
	(1)	(2)	(3)	(4)
Price ($1000)	-0.008	-0.008	-0.010	-0.006
	(0.002)	(0.002)	(0.003)	(0.002)
Horsepower/Weight	2.636	2.688	4.839	0.698
	(1.284)	(1.304)	(1.738)	(0.993)
Weight (1000 lb)	0.049	0.049	0.078	0.007
	(0.025)	(0.025)	(0.032)	(0.020)
Length	0.004	0.005	0.004	0.005
	(0.001)	(0.001)	(0.001)	(0.001)
Miles Per Gallon	0.009	0.009	0.010	0.008
	(0.002)	(0.002)	(0.003)	(0.002)
VW × Post-Scandal	-0.275	-0.276	-0.274	-0.252
	(0.123)	(0.124)	(0.129)	(0.105)
Audi × Post-Scandal	-0.084	-0.083	-0.094	-0.091
	(0.088)	(0.090)	(0.094)	(0.075)
Porsche × Post-Scandal	-0.111	-0.110	-0.107	-0.125
	(0.107)	(0.109)	(0.141)	(0.089)
Other German × Post-Scandal	-0.185		-0.194	-0.172
	(0.079)		(0.091)	(0.066)
BMW × Post-Scandal		-0.111		
		(0.107)		
Mercedes-Benz × Post-Scandal		-0.239		
		(0.112)		
Smart × Post-Scandal		-0.259		
		(0.174)		
Diesel × Post-Scandal	-0.151	-0.153	-0.158	-0.150
	(0.082)	(0.083)	(0.086)	(0.079)
Nest Parameter (σ)	0.851	0.849	0.843	0.864
	(0.040)	(0.041)	(0.042)	(0.039)
Make Fixed Effects	yes	yes	yes	yes
Power-Type Fixed Effects	yes	yes	yes	yes
Year Fixed Effects	yes	yes	yes	yes
Make-Specific Time Trends	yes	yes	yes	yes

Note: Unit of observation is vehicle-year. A vehicle is defined as a make-model-power type (e.g., a Honda Civic with a gasoline engine). Time period covered is 2010 to 2016, with 2015 omitted as the scandal year. Data come from Ward's Automotive. The number of observations in all four specifications is 2,090.

ings. In other words, a consumer switching away from Volkswagen was much more likely to prefer another German vehicle to a non-German one, *ceteris paribus*.

Overall, the estimation results suggest three channels through which the VW scandal affected the sales of the non-VW German auto manufacturers. First, we find a significant decline in the consumer valuation of VW. In a market where differentiated vehicles are (imperfect) substitutes, this decline in consumer valuation of VW drove sales away from VW toward vehicles from other makes. Further, this force drove sales disproportionately toward other German auto manufacturers, because their vehicles are closer substitutes to VW than are non-German vehicles. We refer to this increase in sales as the "substitution away from Volkswagen Group" effect. Second, we find a decrease in consumers' taste for diesel vehicles. Since BMW and Mercedes-Benz continued to sell diesel vehicles after the scandal, their sales declined due to this substitution away from diesel.[19] Third, according to our estimates, consumer valuations of the other German auto manufacturers declined above and beyond the reduced valuation for diesel vehicles, leading to a further decline in their sales. In what follows, we quantify the overall effect of the scandal as well as the importance of each of these three channels.

4.3 Quantifying Each Effect

To quantify the effect of the scandal and decompose the overall effect into the substitution, diesel, and country-specific spillover channels, we conduct three counterfactual simulations for the year 2016. Table 4 summarizes the design of these three simulations. In the first simulation (CF1), we turn off all three channels and simulate what would have happened had the scandal never transpired. To do so, we set the post-scandal interaction dummies to zero and recompute vehicle sales. Therefore, the comparison of the simulated sales and revenues of CF1 and those in the data gives us the overall effect of the scandal. In the second simulation (CF2), we turn on the channel of substitution away from the VW Group. Specifically, we keep the estimated post-scandal change in the consumer valuations of the three VW Group automakers (Volkswagen, Audi, and Porsche) by setting their post-scandal interaction dummies to one, while leaving the other interaction dummies at zero. The difference between CF2 and CF1, therefore, quantifies the effect of substitution away from the VW Group. The third simulation (CF3) extends the second by also allowing for the estimated change in consumers' tastes for diesel vehicles. As a result, the difference between CF3 and CF2 isolates the diesel effect. Finally, because CF3 includes two of the three channels but not the country-specific spillover channel, the difference between the (post-scandal) data and CF3 quantifies the spillover effect.

[19] As has been mentioned, Smart has never sold diesel vehicles in the United States.

Table 4: Counterfactual Designs

	CF1	CF2	CF3	Data
Substitution Away from VW Group	no	yes	yes	yes
Substitution Away from Diesel	no	no	yes	yes
Spillover to Other German Automakers	no	no	no	yes

Table 5: VW Scandal's Impact on Other German Auto Manufacturers

	Comparison	Vehicle Sales	Revenue ($ billion)
Overall Scandal	Data – CF1	–104,661	–$5.2
Substitution Away from VW Group	CF2 – CF1	+398,032	+$22.0
Substitution Away from Diesel	CF3 – CF2	–30,609	–$0.7
Spillover to Other German Automakers	Data – CF3	–472,084	–$26.5

Note: Counterfactual designs are defined in table 4. The simulated effects are for 2016, the first calendar year after the scandal. Revenue effects are expressed in 2015 dollars.

Table 5 reports the overall effects of the scandal on non-VW German auto manufacturers and a decomposition of the overall effects into the three channels. Together, the scandal's three channels reduced the sales of non-VW German auto manufacturers by 104,661 units worth $5.2 billion.[20] That decline amounts to 13 percent of their counterfactual sales in the absence of the scandal.[21] From table 5, we can also see that this decline was mainly driven by the spillover effect, which lowered sales for the non-VW German automakers by 472,084 vehicles and reduced their revenues by $26.5 billion. This effect was partially offset by the increase in sales for the non-VW German auto manufacturers, as consumers switched away from Volkswagen (by 398,032 units worth $22 billion), and reinforced by a decline in sales as tastes for diesel fell (by 30,609 units worth $0.7 billion). As a result, the overall effect was substantially smaller than the spillover effect.

[20]The change in revenue is computed based on the list prices in the data.

[21]According to the simulation results from CF1, their sales would have been 798,580 units had the VW scandal not taken place.

The coexistence of a group reputation spillover and a substitution effect, which partially offset each other, is unlikely to be a coincidence: automakers that are associated closely enough to have a collective reputation are also likely to produce closer substitutes. Indeed, our estimated nest parameter $\sigma = 0.85$ suggests that vehicles with the same country of origin are closer substitutes. We believe that the pattern that the overall effect was smaller than the spillover effect is likely to generalize to other settings in which collective reputation plays an important role. Because a shock to a group member that generates a decline in collective reputation is also likely to produce large within-group substitution, the combined effect of such a shock that would be measured by difference-in-differences methods even with an untreated control group is likely to understate the spillover effect.

The three counterfactuals we discuss in this section are not the only way to decompose the overall effect. For instance, in table 5, we quantify substitution away from the VW Group by comparing two counterfactuals, CF1 and CF2, only one of which allows for this channel to be in effect. We could alternatively quantify this channel by comparing the data, in which all channels are active, to a counterfactual which turns off only the substitution away from VW Group channel. In principle, the non-linearity of the demand model means that the order in which each channel is turned on or off could affect the decomposition results. For ease of exposition, we detail all possible such counterfactuals and comparisons in appendix C.3. In table 6, we show the minimum and maximum effects of each channel over all possible comparisons. The ranges for the decomposition are rather narrow quantitatively, demonstrating that our results are robust to different decompositions.

Table 6: Bounding VW Scandal's Impact on non-VW German Auto Manufacturers

	Vehicle Sales		Revenue ($ billion)	
	Min	Max	Min	Max
Substitution Away from VW Group	+378,045	+405,450	+$21.2	+$22.7
Substitution Away from Diesel	–38,137	–22,628	–$1.5	–$0.7
Spillover to Other German Automakers	–472,084	–444,569	–$26.5	–$24.9

Note: The simulated effects are for 2016, the first calendar year after the scandal. Revenue effects are expressed in 2015 dollars. Appendix C.3 details all possible counterfactual designs and results that give rise to the bounds in the table.

5 Interpretation of the Spillover Effect

In this section, we argue that the most plausible interpretation of the spillover effect documented and quantified in the previous two sections is that it arises from a collective reputation shared by all German automakers selling vehicles in the United States. As mentioned in section 2, German auto manufacturers use the notion of "German engineering" in their advertising, providing *prima facie* evidence that they share a collective reputation. Here, we begin by presenting more direct evidence that the VW emissions scandal led to a deterioration in public perceptions of the other German automakers sharing a collective reputation. We then discuss an alternative information-based interpretation and argue that it is less persuasive. Finally, we describe subsequent scandals involving the German automakers, clarify that they are only tangentially related to the original VW emissions scandal, and thus conclude that these subsequent scandals are unlikely to explain our documented spillover effect.

We use the Twitter sentiment data described in section 2.2 to show that perceptions of the non-VW German automakers suffered in the aftermath of the VW emissions scandal. Specifically, we estimate the difference-in-differences specification in equation (1) with Twitter sentiment data for the non-VW German automakers. The unit of observation is a make-day.[22] Column (1) of table 7 shows a statistically significant decline of 3.5 percentage points in positive sentiment toward non-VW German auto manufacturers as a result of the scandal (we discuss the results for negative Twitter sentiment in column (2) later in this section). To put this number in perspective, the share of tweets expressing positive sentiment toward those companies averaged 12.3 percent in August 2015. We consider this deterioration in positive social-media sentiment towards the other German automakers as suggesting that the sales spillover effect we document arose from a country-specific collective reputation.

An alternative interpretation of the spillover effect to the non-VW German auto manufacturers is that, following the scandal, consumers suspected these other German auto manufacturers of having engaged in cheating behavior similar to VW's. For example, consumers might have suspected that the German automakers cheated by implicitly colluding through a common labor market for research and development personnel or through shared upstream manufacturers in the supply chain. Such a suspicion could have arisen

[22] Of the 37 auto makes with light vehicle sales in the Ward's U.S. data (our data source for vehicle sales), Alfa Romeo, Saab, Suzuki and Tesla did not have identifiers in the Networked Insights database; see table A.2 in appendix A. The estimation sample is a window of ± 14 days around the scandal eruption date of September 18, 2015, and the outcome variable is the share of tweets expressing positive/negative Twitter sentiment towards a particular make. The denominator of these shares includes positive, negative and neutral sentiments.

Table 7: Difference-in-Differences Estimates—Twitter Sentiment
German vs. Non-German Auto Manufacturers, Excl. VW Group

Dependent Variable	Positive Sentiment	Negative Sentiment
	(1)	(2)
German × Post-Scandal	-0.035	0.002
	(0.006)	(0.006)
Time Fixed Effects	Yes	Yes
Make Fixed Effects	Yes	Yes
R^2	0.348	0.268
N	840	840

Note: Unit of observation is a make-day. Sentiment shares are normalized by subtracting the average sentiment share from August 10 to August 31, 2015. The denominator of these shares includes positive, negative and neutral sentiments. The estimation period comprises 14 days before and after scandal date of September 18, 2015. Volkswagen Group is defined as Volkswagen, Audi, and Porsche. All regressions include make and time fixed effects, and are weighted by tweet volume. Data come from Networked Insights.

fundamentally from a shared German identity, in which case this alternative channel is ultimately connected to group reputation. If, however, such a suspicion arose from independent information available to consumers around the time of the scandal, this alternative interpretation would indeed be distinct from our notion of collective reputation.

We now present four arguments against this alternative interpretation. Our first two arguments provide evidence suggesting that any such "shared suspicion" was not substantiated by information available to consumers around the time of the VW scandal. Our third argument is that patterns of social media sentiment are indeed inconsistent with consumers possessing information that implicated the other German auto manufacturers in malfeasance similar to VW's. Our fourth argument goes further and presents online search patterns suggesting that consumers did not possess such suspicions in the first place.

First, there was no concurrent notion of malfeasance by the non-VW German automakers. The West Virginia University study that ultimately led to the discovery of the VW scandal focused on three diesel vehicles: a VW Passat, a VW Jetta, and a BMW X5. The VW vehicles failed the test, whereas the BMW vehicle passed. No Mercedes-Benz vehicles were tested (see Thompson et al. (2014)). In addition, Mercedes-Benz and BMW had little technical reason to resort to cheating devices, as their diesel vehicles tended to be larger than VW's. As a result, they used exclusively the Selective Catalytic Reduction (SCR) system as their NO_x-control system, which adds urea and water to the exhaust flows. This

system is more effective and more reliable in reducing NO_x emissions but requires more space for the urea and water containers. VW, with on average smaller vehicles in its fleet, developed a different system, a nitrogen oxide trap, which the VW engineers could never get to operate with the same efficiency as SCR systems.[23] This difficulty was what led to VW's deception in the first place: VW would not have been able to comply with U.S. regulations without cheating (Zycher (2017)). Finally, media at the time wrote that non-VW German automakers were not implicated in the scandal. On September 22, 2015, for instance, CNN wrote (Petroff (2015)): "But before you start worrying about the complete collapse of the German auto industry, it's worth repeating that—at least for now—the scandal is limited to Volkswagen. Other German automakers such as Daimler, which owns Mercedes-Benz, and BMW have said they're not affected." Even in the fall of 2017, Zycher wrote (Zycher (2017)): "Note that VW is the only manufacturer accused of explicitly installing such systems to defeat NO_x emissions control systems. [...] In short, it is perverse to fail to distinguish between the behavior of VW and that of the rest of the industry."

Second, an expert on the German automotive industry we interviewed dismissed the idea of implicit collusion through the labor market: research and development personnel are too valuable to lose to a competitor in the hope of potential collusion gains. Consistent with that notion, non-compete clauses for crucial engineers are widespread in the German auto industry. This expert also rejected the possibility of implicit collusion through parts manufacturers: key account managers at upstream part firms are strictly separated by downstream auto manufacturers and are secluded from any development projects conducted with competitor companies.

Third, the reaction of Twitter sentiment toward the other German automakers displayed a very different pattern after the scandal than did the reaction of Twitter sentiment toward VW. As mentioned in section 2.2, negative sentiment toward VW increased sharply following the scandal, while positive sentiment declined moderately. This evidence suggests that suspicion of malfeasance manifests mainly as an increase in negative sentiment towards the wrongdoer. Therefore, if consumers indeed had information that other German automakers were cheating too, we should expect a similar pattern for their social-media sentiment. However, as shown in column (2) of table 7, we find no meaningful change in negative Twitter sentiment following the scandal for the other German automakers. By contrast, positive sentiment toward them declined, as shown in column (1). This decline in positive sentiment without a corresponding increase in negative sentiment suggests that perceptions of non-VW German automakers suffered after the scandal for reasons other than consumers possessing information about malfeasance.

[23] Only the tested VW Passat had a SCR system installed, but, according to Zycher (2017), the system was operating with reduced efficiency to not inconvenience the driver with too frequent refills of the urea.

Fourth, although online searches for Volkswagen spiked following the scandal, consumers showed no heightened inquisitiveness toward the other German auto manufacturers. Figure 6 contains four panels, each of which plots a time series of a single Google search term ("Volkswagen", "VW", "BMW", and "Mercedes").[24] Searches for "Volkswagen" and "VW" in panels A and B increased dramatically in the aftermath of the scandal; the week of September 18, the date of the EPA announcement, coincides with z-scores of 22 and 15. By contrast, searches for the two main non-VW German makes, "BMW" in panel C and "Mercedes" in panel D, are indistinguishable from their regular fluctuations. The Google search data implies that consumers did not become more inquisitive about the other German auto manufacturers following the scandal. If consumers suspected those automakers of cheating similar to VW's, they did not display it in a similar accompanying search for information.

Taken together, these four arguments suggest that the scandal's effects on non-VW German automakers were unlikely to be driven by information. Rather, the scandal must have tarnished the reputations of the other German auto manufacturers through their association with Volkswagen, consistent with the notion of a collective reputation.

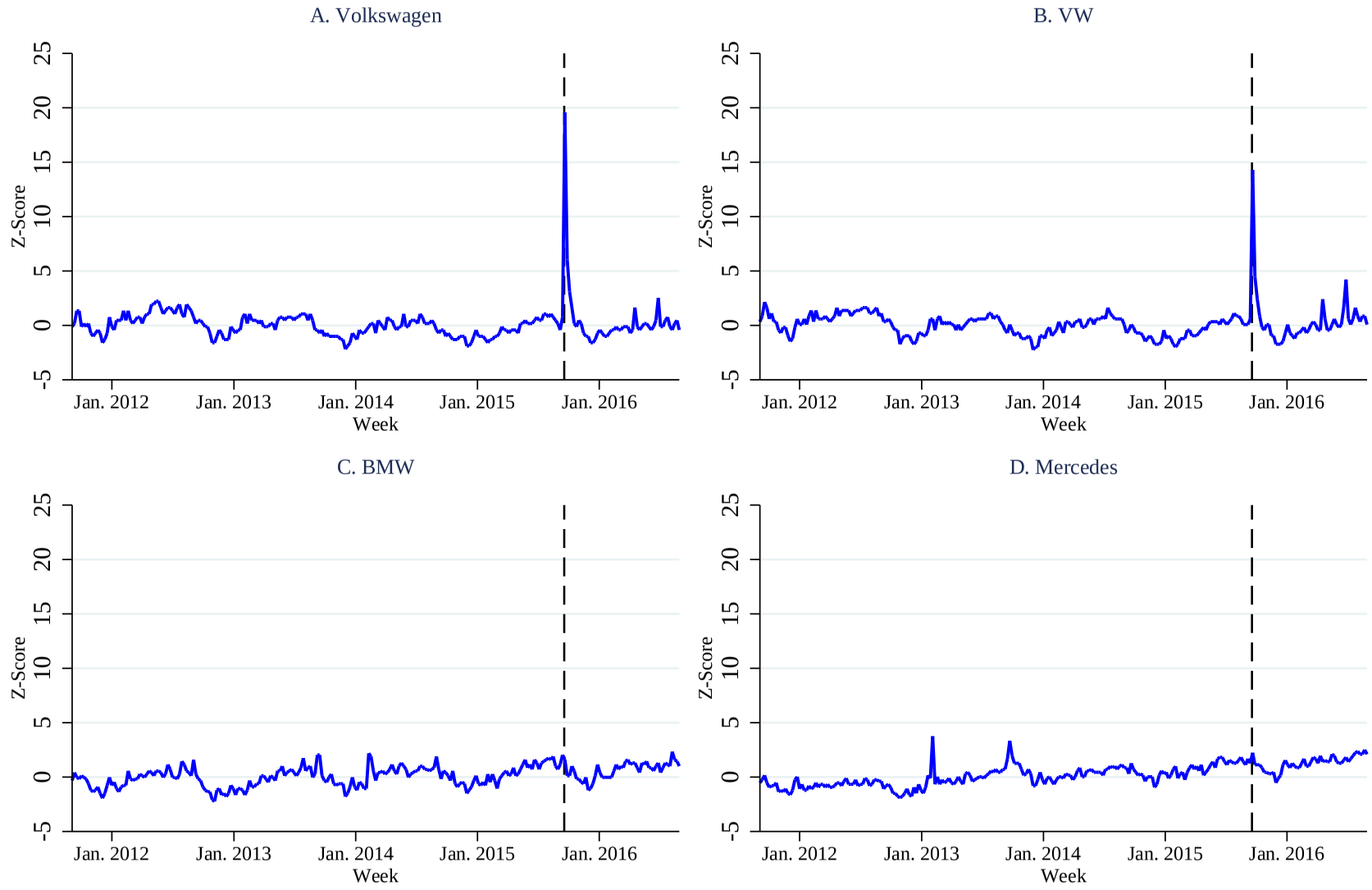
We conclude this section with a brief discussion of some scandals involving the German auto manufacturers, clarifying both that they are unrelated to the 2015 VW emissions scandal and that they erupted well after the end of our study period. In the summer of 2017, it was suggested that Mercedes-Benz had also manipulated emissions (Zeit Online (2017)), although Mercedes-Benz never admitted wrongdoing in the United States. Note that these accusations arose almost two years after the VW scandal broke and one year after the end of our study period. Later, in the spring of 2018, BMW had its own cheating device scandal, which led to raids of its corporate offices in Germany (Ewing (2018)). However, as Ewing (2018) also reports, this affair was much smaller (11 thousand vehicles affected versus 11 million in the VW scandal). More importantly, it had no U.S. impact as none of the affected vehicles were in the United States.[25]

VW, BMW, and Mercedes-Benz are currently in unrelated legal trouble with the European competition authority (BBC (2018)). The case centers on collusion to standardize parts in two areas: particle filters for gas engine vehicles and urea containers in SCR sys-

[24]The underlying data on Google trends is weekly, and it is scaled by Google so that 100 corresponds to the largest number of searches per week in the search period. For weekly data, Google trends only allows users to download a few pre-defined search periods. We chose a five-year window from August 2011 to August 2016. We normalize the series and express weekly values as z-scores, deviations from the mean that are scaled by the standard deviation. A z-score of 1 indicates a 1-standard-deviation increase over the mean. Both the means and the standard deviations are constructed using the period prior to September 2015.

[25]Like Mercedes-Benz, BMW never admitted to any wrongdoing. BMW executives blamed a simple mistake that led them to install the wrong software for the implicated vehicles. The subsequent fines imposed by German authorities were $11.6 million, orders of magnitude smaller than the combined $25 billion of fines for VW in the United States and Germany (see Dobush (2018)).

Figure 6: Google Trends of Searches for German Auto Manufacturing Firms



Note: Dashed lines show the week of the Volkswagen emissions scandal, dated September 18, 2015. The underlying data on Google trends is weekly and it is scaled by Google so that 100 corresponds to the largest number of searches per week in the sample period (August 2011 to August 2016). We construct z-scores as the deviation from the mean of the series, normalized by its standard deviation. The z-scores on the vertical axis are constructed using the mean and standard deviation for each search term over the period prior to September 2015.

tems for diesel engine vehicles (which VW had only used in a few of its larger vehicles). Notably, "The Commission said it had no indications that the automakers coordinated with each other on the use of illegal emissions-cheating 'defeat devices'." (BBC (2018)). Collusion for the purposes of standardization of parts is very different from collusion on defeat devices. Moreover, such collusion is actually legal under European competition law for efficiency purposes, as long as the colluding parties seek permission from the competition authorities (which they appear not to have done in this case).

To sum up: at the time that the VW scandal broke, U.S. consumers had neither a technical nor an economic reason to believe that BMW and Mercedes-Benz were culpable of malfeasance similar to VW. The documented Google search behavior and Twitter sentiment expression indeed do not appear to be consistent with the notions that BMW and Mercedes-Benz were implicated in the same way as VW, nor that U.S. consumers believed in collective malfeasance. By contrast, the evidence is consistent with our interpretation of the spillover effect as arising from collective reputation.

6 Conclusion

This paper documents that firms have economically important collective reputations in the context of a key industry featuring national powerhouse companies. Using the 2015 VW emissions scandal as a natural experiment, we show that these collective reputations can give rise to reputation spillovers with large economic consequences; in particular, a scandal implicating one group member can have adverse impacts on the other group members not implicated in the wrongdoing. We begin by using a difference-in-differences approach to provide evidence that there is a country-specific spillover from the VW emissions scandal to the other German auto manufacturers. The impact of the VW emissions scandal on the other German automakers is a combination of three forces: substitution away from VW, substitution away from diesel, and the reputational spillover. We estimate a model of vehicle demand to decompose the scandal's effect into these three constituent forces. Our results suggest that the scandal had an economically important country-specific reputational spillover. We thus provide empirical support for the existence both of collective reputation for firms and of group reputation externalities.

Our results also contextualize the economic harm of one of the largest industrial and public health scandals in recent U.S. history. As a complement to the literature on the scandal's health costs for the general public, we document its economic costs to companies not directly tainted by the scandal but sharing a collective reputation with the wrongdoer. Our findings suggest the need to study whether there are policy steps that could correct the potentially adverse externalities arising from the collective reputations of firms.

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A List of Automotive Makes

Table A.1: Makes in WardsAuto Data

Acura	Honda	Nissan
Alfa Romeo	Hyundai	Porsche
Audi	Infiniti	Ram
BMW	Jaguar	Saab
Buick	Jeep	Scion
Cadillac	Kia	Smart
Chevrolet	Land Rover	Subaru
Chrysler	Lexus	Suzuki
Dodge	Lincoln	Tesla
Fiat	Mazda	Toyota
Ford	Mercedes-Benz	Volkswagen
GMC	Mini	Volvo
	Mitsubishi	

Note: We exclude Mercury from the dataset because it was discontinued in January 2011, the first month of our analysis.

Table A.2: Makes in Twitter Data

Acura	Honda	Mini
Audi	Hyundai	Mitsubishi
BMW	Infiniti	Nissan
Buick	Jaguar	Porsche
Cadillac	Jeep	Ram
Chevrolet	Kia	Scion
Chrysler	Land Rover	Smart
Dodge	Lexus	Subaru
Fiat	Lincoln	Toyota
Ford	Mazda	Volkswagen
GMC	Mercedes-Benz	Volvo

Note: Data come from Networked Insights.

B Robustness: Difference-in-Differences Estimation

In this appendix, we first show that our treatment of Mini as a non-German make in the baseline specification does not drive our results. We then provide additional visual evidence to support the notion of parallel trends in sales growth rates between the non-VW German and non-German auto manufacturers prior to the scandal. Finally, we explore a number of alternative econometric specifications.

B.1 Classification of Mini

Table B.1: Difference-in-Differences Estimates—Robustness to Classification of Mini

Dependent Variable	12-month Log Sales Growth		
Treatment of Mini	Baseline (considered non-German)	Excluded from the Sample	Considered German
	(1)	(2)	(3)
German × Post-Scandal	-0.092	-0.093	-0.099
	(0.030)	(0.030)	(0.029)
Time Fixed Effects	Yes	Yes	Yes
Make Fixed Effects	Yes	Yes	Yes
R^2	0.303	0.305	0.304
N	2150	2082	2150

Note: Unit of observation is a make-month (e.g., the log growth of all BMW sales from January 2014 to January 2015). Time period covered is January 2011 to August 2016. Standard errors clustered at the make level in parentheses. Volkswagen Group (Volkswagen, Audi, and Porsche) excluded from all regressions. Volkswagen emissions scandal dated September 18, 2015. Sales are measured in units sold. All regressions are weighted by the square root of sales volumes. Data come from Ward's Automotive.

In our baseline country classification of auto makes, we include Mini—a company with historical roots in Britain that is now owned by BMW—as a non-German make. Our classification is supported by BMW board member Peter Schwarzenbauer, who told Reuters in a 2017 interview that the "brand being perceived as British, that's important... Most people don't know where the cars are produced" (Pitas (2017)). This focus on the country of brand association rather than the country of production or ownership drives our baseline classification choice. Nonetheless, we show here that this choice does not impact our results. Column (2) of table B.1 excludes Mini from the analysis altogether; the resulting estimate of a 9.3 percentage point decline in non-VW German vehicle sales growth hardly differs from the baseline estimate of 9.2 percentage points. Classifying Mini as Ger-

man through its ownership by BMW in column (3) leads to an estimated decline of 9.9 percentage points, which is also similar to the baseline result.

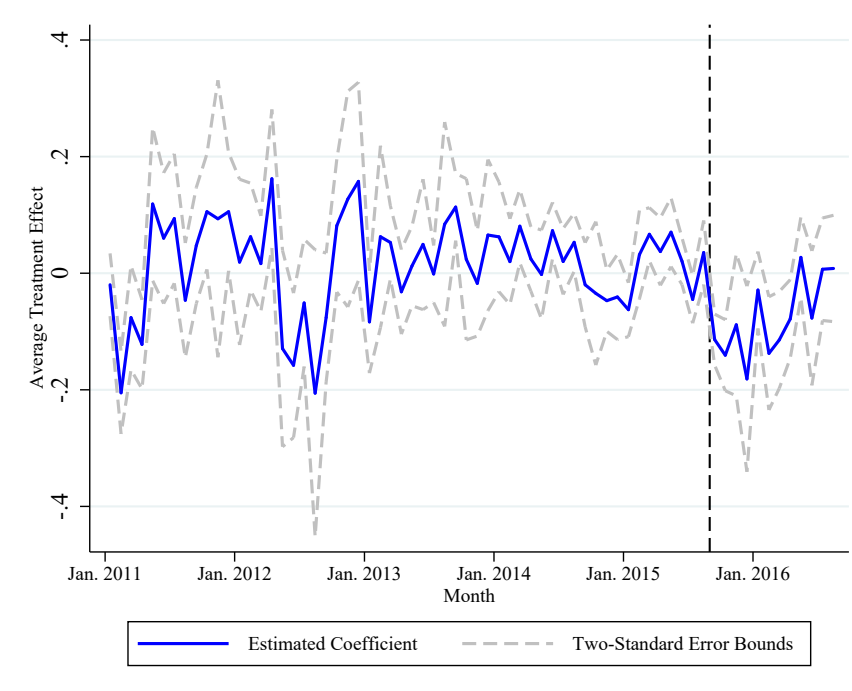
B.2 Assessing Parallel Pre-Trends

To examine whether the trends in sales growth displayed in Figure 5 are parallel prior to the scandal, we estimate month-by-month differences in the sales growth rates of German and non-German auto manufacturers. Specifically, we estimate a regression of the form:

$$\ln \textit{Sales}_{it} - \ln \textit{Sales}_{it-12} = \gamma_t + \beta_t \textit{non-VW German}_i + \varepsilon_{it}, \tag{B.1}$$

where $\gamma_t, t = 1, ..., T$ is a set of month dummies for January 2011 to August 2016; *non-VW German*$_i$ is a dummy variable that equals one if the make i is BMW, Mercedes-Benz, or Smart; and β_t is the difference in sales growth between German and non-German auto manufacturers, which we allow to vary over time. Figure B.1 displays the estimated coefficients β_t and the corresponding two-standard-error confidence bands based on standard errors clustered at the make level. These estimates are centered at zero prior to the scandal, suggesting that the sales growth of the non-German auto manufacturers constitutes a reasonable comparison for the sales growth of the non-VW German auto manufacturers.

Figure B.1: Differences in Sales Growth for non-VW German vs. non-German Automakers



Note: Dashed vertical line shows the month of the VW emissions scandal, September 2015. The solid line displays the estimated coefficients β_t from equation (B.1). Confidence bands are calculated from standard errors clustered at the make level. Regression excludes the VW Group. Data come from Ward's Automotive.

B.3 Alternative Econometric Specifications

We show here that our difference-in-differences estimates are not sensitive to several alternative econometric specifications. Throughout the paper, we have weighted observations by the square root of the make's monthly sales volume. Column (2) of table B.2 shows that our choice to weight the observations is conservative: the unweighted estimate of the difference-in-differences coefficient is 14 percentage points. Moreover, instead of natural log-differences, in column (3) we consider mid-point growth rates, where the change in sales volume between period t and period $t-12$ is divided by the average level of sales in the two periods. The estimated difference-in-differences coefficient under this alternative measure is 9.4 percentage points, which is similar to the baseline result in column (1). Finally, column (4) shows that using log sales levels as the dependent variable and adding make-specific linear time trends and make-specific months dummies (in addition to make and time fixed effects) again yields a similar result.

Table B.2: Difference-in-Differences Estimates—Alternative Specifications

Dependent Variable	12-month Log Sales Growth			Log Sales Level
Specification	Baseline	Unweighted	Mid-Point	
	(1)	(2)	(3)	(4)
German × Post-Scandal	-0.092	-0.140	-0.094	-0.096
	(0.030)	(0.058)	(0.030)	(0.028)
Time Fixed Effects	Yes	Yes	Yes	Yes
Make Fixed Effects	Yes	Yes	Yes	Yes
Make-Specific Linear Time Trends	No	No	No	Yes
Make-Specific 12 Month Dummies	No	No	No	Yes
R^2	0.303	0.131	0.322	0.994
N	2150	2150	2150	2556

Note: Unit of observation is a make-month. Time period covered in columns (1) through (3) is January 2011 to August 2016 and in column (4) is January 2010 to August 2016. Standard errors clustered at the make level in parentheses. Volkswagen Group (Volkswagen, Audi, and Porsche) excluded from all regressions. Volkswagen emissions scandal dated September 18, 2015. Sales are measured in units sold. All regressions are weighted by the square root of sales volumes. Regressions in columns (1) through (3) include make and time fixed effects. The regression in column (4) uses log sales levels as the dependent variable and includes make-specific linear time trends and make-specific dummies for the twelve calendar months (in addition to make and time fixed effects). Data come from Ward's Automotive.

C Additional Details on Demand Estimation & Counterfactuals

In this appendix, we present additional details on the estimation of the model of vehicle demand from section 4. First, we provide summary statistics for the list prices and vehicle characteristics used in the estimation. Second, we present the first-stage regression results, in which we regress the endogenous variables (vehicle prices and within-group market shares) on the instrumental variables. Third, we present alternative decompositions of the three channels by which the VW emissions scandal affected the vehicle sales of the other German auto manufacturers.

C.1 Summary Statistics

Our estimation sample consists of 2,090 vehicle-year combinations, where a vehicle is defined as a make-model-power type. Table C.1 reports the summary statistics of list prices and vehicle characteristics.

Table C.1: Summary Statistics

	Mean	Std. Dev.	Min	Max
Prices (2015-$1000)	41.40	24.18	12.41	228.04
Horsepower/Weight	0.06	0.02	0.02	0.19
Weight (1000 lb)	4.01	0.94	1.81	7.15
Length (inches)	188.67	19.50	106.10	274.20
Miles per Gallon	29.14	12.76	10.00	119.00
Diesel	0.07	0.26	0	1
Gas	0.78	0.41	0	1
Other Power Type	0.15	0.36	0	1
Germany	0.23	0.42	0	1
Japan	0.33	0.47	0	1
Korea	0.06	0.24	0	1
United States	0.31	0.46	0	1
Other Country	0.07	0.25	0	1
N		2,090		

Note: Unit of observation is a vehicle-year. A vehicle is defined as a make-model-power type (e.g., a Honda Civic with a gasoline engine). Time period covered is 2010 to 2016, with 2015 omitted as the scandal year, just as in table 3, which reports our demand estimates. Data come from Ward's Automotive.

C.2 First-Stage Regressions

Table C.2: First-Stage Regressions for Demand Estimation

Dependent Variable	Price	Within-Group Market Share
	p_{jt}	$s_{j\|gt}$
IV1	-0.261	0.008
	(0.055)	(0.002)
IV2	0.012	0.001
	(0.014)	(0.001)
IV3	-0.020	0.004
	(0.017)	(0.001)
IV4	1.576	-0.053
	(5.343)	(0.228)
IV5	-0.067	0.130
	(2.004)	(0.085)
IV6	-0.198	-0.006
	(0.439)	(0.019)
IV7	-0.027	-0.012
	(0.107)	(0.005)
Horsepower/Weight	1070.145	-33.626
	(97.529)	(4.156)
Weight (1000 lb)	10.805	-0.095
	(2.495)	(0.106)
Length	-0.168	0.021
	(0.132)	(0.006)
Miles per Gallon	0.150	0.019
	(0.127)	(0.005)
VW $\times$ Post-Scandal	-0.873	-1.826
	(20.293)	(0.865)
Audi $\times$ Post-Scandal	-6.606	-0.903
	(20.913)	(0.891)
Porsche $\times$ Post-Scandal	-7.755	-0.004
	(24.958)	(1.064)
Other German $\times$ Post-Scandal	-3.828	0.708
	(12.366)	(0.527)
Diesel $\times$ Post-Scandal	1.997	-1.318
	(10.062)	(0.429)
Time Fixed Effects	Yes	Yes
Make Fixed Effects	Yes	Yes
Power-Type Fixed Effects	Yes	Yes
Make-Specific Time Trends	Yes	Yes

Note: Excluded instrumental variables: IVs 1–3 are the sum of vehicle characteristics over closely competing vehicles. Let $x_{jt}^{(k)}$ denote the value of characteristic k for vehicle j at time t, then $IV_k = \sum_{j' \neq j} 1\left(\left|x_{j't}^{(k)} - x_{jt}^{(k)}\right| < d_k\right) x_{j't}^{(k)}$, where for $k = 1, 2, 3$ represents the horsepower/weight, length, mpg of vehicle j, and d_k is the standard deviation of attribute k in the data. IVs 4–6 are the sum of characteristics over competitors' vehicles. IV 7 is the number of vehicles of the same country origin and power type as vehicle j in year t.

C.3 Additional Counterfactuals

In section 4.3, we conduct three counterfactual simulations in which we turn on the three channels sequentially in order to quantify the effect of each channel. There, the sequence of turning on the channels is "Substitution Away from VW Group," followed by "Substitution Away from Diesel," and finally "Spillover to Other German Automakers." That sequence is, however, not unique. Given the nonlinearity of the demand model, there are four unique quantifications of each channel's effect. This is because we can quantify the effect of each channel by comparing two scenarios: one in which this channel is turned, and one in which it is turned off. For each comparison, the other two channels are either both in play, they are both turned off, or one of them is turned off. We summarize the counterfactual designs used in these four quantifications in table C.3, and we report the results of the four quantifications in table C.4 (bold table entries correspond to the baseline decomposition in table 5 in the main text). The range of the three channels' effects reported in table 6 is based on comparing the results across the four columns in table C.4. As noted, the results are stable across these different quantifications.

Table C.3: Counterfactual Designs

	CF1	CF2	CF3	Data	CF4	CF5	CF6	CF7
# of Active Channels	0	1	2	3	1	1	2	2
Substitution Away from VW Group	no	yes	yes	yes	no	no	no	yes
Substitution Away from Diesel	no	no	yes	yes	yes	no	yes	no
Spillover to Other German Automakers	no	no	no	yes	no	yes	yes	yes

Table C.4: Quantifications of Each Channel's Effect on the non-VW German Automakers

	Other Two Channels			
	Off/Off	On/Off		On/On
Substitution Away from VW Group	**CF2 – CF1**	CF3 – CF4	CF7 – CF5	Data – CF6
Vehicle Sales	**+398,032**	+405,560	+387,292	+378,045
Revenue ($ billion)	**+$22.0**	+$22.7	+$21.4	+$21.2
Substitution Away from Diesel	CF4 – CF1	**CF3 – CF2**	CF6 – CF5	Data – CF7
Vehicle Sales	–38,137	**–30,609**	–22,628	–31,874
Revenue ($ billion)	–$1.5	**–$0.7**	–$1.0	–$1.2
Spillover to Other German Automakers	CF5 – CF1	CF7 – CF2	CF6 – CF4	**Data – CF3**
Vehicle Sales	–460,078	–470,818	–444,569	**–472,084**
Revenue ($ billion)	–$25.4	–$26.0	–$24.9	**–$26.5**

Note: Bold table entries correspond to the baseline decomposition in table 5. Positive numbers indicate that the channel increased vehicle sales or revenue for the non-VW German automakers; negative numbers mean the opposite. The entries in the column titled "Off/Off" report the channel's effect in a counterfactual without the other two channels; e.g., the first row of that column reports the effect of the substitution away from VW channel if the scandal had not also led to substitution away from diesel and a spillover to the other German automakers. The entries in the columns titled "On/Off" report the channel's effect in a counterfactual with one of the other two channels, as indicated by the counterfactual definitions in table C.3. Finally, the entries in the column titled "On/On" report the channel's effect in a counterfactual with both of the other two channels. The simulated effects are for 2016, the first calendar year after the scandal. Revenue effects are expressed in 2015 dollars.

D The Scandal's Impact on non-VW German Stock Returns

In this appendix, we show that the VW emissions scandal's effect on vehicle sales was mirrored in stock returns. To this end, we combine two complementary data sources. We construct daily U.S. stock returns from the Center for Research in Securities Prices (CRSP) database, which covers primary listings on NYSE, NYSE MKT, NASDAQ, and NYSE Arca.[26] We supplement this data with American Depository Receipts (ADRs) for publicly-listed automotive firms from other countries.[27] ADRs allow us to calculate the daily returns for foreign automotive firms even on days when the underlying stocks were not traded in their home markets. For instance, the Tokyo Stock Exchange was closed for holidays on September 21–23, 2015; U.S. exchanges were open on those days. If we constructed daily returns of Japanese securities, e.g., Mazda or Nissan, from the Japanese exchange, we would have no observations on those U.S. trading days.

Table D.1: Automakers in Stock Price Data

Automaker (Holding Company)	Ticker
BMW	BAMXY:US
Daimler (Mercedes-Benz, Smart)	DDAIY:US
Ford	F:US
Fiat-Chrysler	FCAU:US
Fuji-Subaru	FUJHY:US
General Motors	GM:US
Honda	HMC:US
Mazda	MZDAY:US
Nissan	NSANY:US
Toyota	TM:US
Tesla	TSLA:US
Tata Motors (Jaguar and Land Rover)	TTM:US
Volkswagen Group	VLKAY:US

Note: Stock prices for automakers come from Bloomberg. Ford, General Motors, and Tesla are listed on U.S. stock exchanges; all remaining stock prices come from American Depository Receipts. The sample is restricted to companies that sell light vehicles in the United States.

[26] NYSE is the New York Stock exchange and the premier market place. NYSE MKT is the marketplace within the NYSE for small market capitalization companies. NASDAQ is the second largest marketplace for stocks in the world after NYSE, with a certain specialization in high-tech companies. Finally, NYSE Arca is another specialized electronic-trading marketplace for U.S. stocks.

[27] Appendix table D.1 lists the holding companies of auto makes used in our analysis (ADRs for the holding companies of some auto makes in the Ward's sales data were insufficiently frequently traded to be used). Ford, General Motors, and Tesla are listed on U.S. stock exchanges; all remaining prices come from ADRs.

To compare expected and realized stock returns, we follow the market model and assume that an individual stock's expected return depends only on the stock's covariance with the market return, its so-called beta. The difference between the expected return on stock i at time t and its actual return is referred to as the "abnormal return," AR_{it}. We measure each stock's abnormal return by estimating the following regression:

$$R_{it} = \alpha_i + \beta_i R_{market,t} + \varepsilon_{it} \tag{D.1}$$

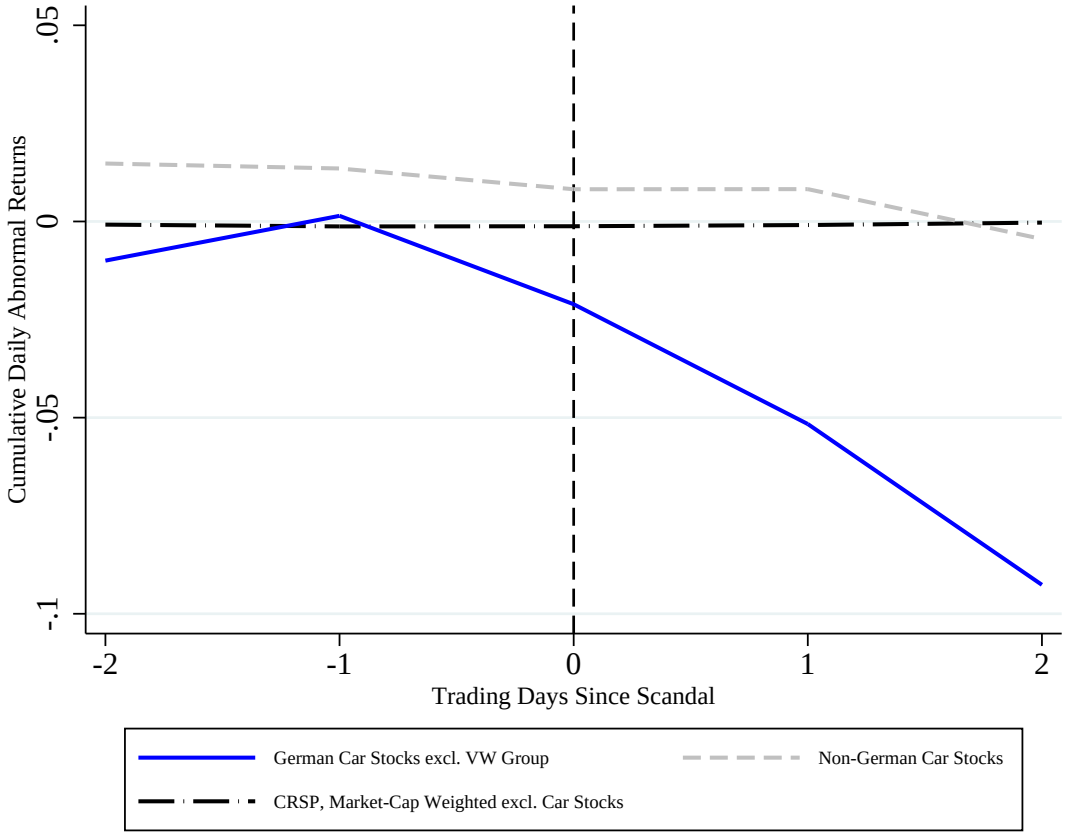
where i indexes individual stocks and t represents market trading days. The regression sample covers the trading year, approximately 250 trading days, ending thirty days before the scandal. In equation (D.1), R_{it} is the daily stock return for stock i from day $t-1$ to day t, and $R_{market,t}$ is the return on the CRSP value-weighted market portfolio (which does not include the ADRs). The abnormal return is the difference between the stock's actual return and its return predicted from the regression: $AR_{it} = R_{it} - \hat{R}_{it}$. The cumulative abnormal return, CAR_{it}, is then defined as $\sum_{s=0}^{t} AR_{is}$. The starting point in our definition of cumulative abnormal returns, $s = 0$, is September 16, 2015, two days before the scandal.

Figure D.1 shows that the cumulative abnormal return averaged over the German automotive firms excluding Volkswagen was negative 10 percent within two trading days of the scandal. That decline contrasts sharply with abnormal returns for the non-automotive stocks in the CRSP database, which remained near zero after the scandal. Similarly, the non-German auto stocks exhibited only slight abnormal return movements on and around the scandal date.

We complement the visual evidence in figure D.1 using two quantitative approaches. First, we estimate two sets of difference-in-differences regressions, where the outcome variable is either AR_{it} or CAR_{it}. Owing to the high-frequency nature of the data, we use data for September 16 and 17, 2015, as the pre-scandal period, and data for September 18, 21, and 22, 2015, as the post-scandal period. In one set of regressions, we find that relative to non-German auto stocks, the non-VW German automakers experienced roughly 2 percent lower daily abnormal returns (see columns (1) and (2) of table D.2). In the other set of regressions, the outcome variable is cumulative abnormal returns at the end of the pre- and post- scandal periods for each stock. We find that relative to non-German auto stocks, the non-VW German auto stocks experienced roughly 6 percent lower cumulative abnormal returns. Both results are statistically significant.

Second, we conduct an event study, a methodology commonly used in the finance literature (e.g., MacKinlay (1997)). The market model assumes that the return distributions are the same during the estimation period prior to the scandal and during the event win-

Figure D.1: Cumulative Abnormal Stock Returns



Note: Dashed line shows the date of the VW emissions scandal, September 18, 2015. Automotive stock data come from the Bloomberg database; CRSP index comes from the Center for Research in Security Prices. Estimates based on the market model defined by equation (D.1); cumulative abnormal returns averaged across individual stocks in each group.

dow surrounding it. Under that assumption, the abnormal return obtained by estimating equation (D.1) should equal zero in expectation for any individual stock if the scandal had no effect on stock returns. Consequently, the cumulative abnormal returns for stocks on the three days of September 18, 21, and 22, 2015 should also have been approximately zero. We can test this hypothesis by computing the following test statistic for each stock i:

$$\frac{CAR_{iT}}{\Big(T * Var[AR_i]\Big)^{\frac{1}{2}}}, \tag{D.2}$$

where CAR_{iT} is the cumulative abnormal return of stock i between September 16 and September 22, 2015; $T = 5$ (trading) days in the event window; and $Var[AR_i]$ is an estimate of the variance of the abnormal return of stock i. Following MacKinlay (1997), we

Table D.2: Difference-in-Differences Estimates—Abnormal Stock Returns

Dependent Variable	Abnormal Returns		Cumulative Abnormal Returns	
	(1)	(2)	(3)	(4)
German × Post-Scandal	-0.019	-0.019	-0.064	-0.061
	(0.004)	(0.005)	(0.013)	(0.015)
Weighting	None	Sales Volume	None	Sales Volume
Time Fixed Effects	Yes	Yes	Yes	Yes
Company Fixed Effects	Yes	Yes	Yes	Yes
R^2	0.687	0.600	0.882	0.799
N	60	60	24	24

Note: Unit of observation in columns (1) and (2) is the daily abnormal stock return for the periods before and after the event date. In columns (3) and (4) it is the cumulative abnormal returns at the end of the pre- and post-scandal periods. Abnormal returns are calculated using a market model (equation (D.1)). Automotive stock prices come from Bloomberg and include U.S.-listed stocks (Ford, General Motors, and Tesla) and ADRs (all other auto make holding companies). The weighted regressions are sales-weighted using the Ward's Automotive sales data. Robust standard errors in parentheses. The pre-scandal period comprises September 16 and 17, 2015, and the post-scandal period comprises September 18, 21, and 22, 2015 (September 18 was a Friday in 2015).

use the abnormal return variance over the estimation window for equation (D.1). Assuming that stock returns are normally distributed, this statistic is distributed approximately standard normal. BMW's cumulative abnormal return of negative 7.2 percent and Daimler's negative 11.4 percent have respective test statistics of 2.42 and 4.17.[28] These test statistics lead us to reject the null hypothesis that the VW scandal had no effect on the companies' stock returns.

The behavior of automotive companies' stock returns after the VW emissions scandal thus reinforces the findings from our analysis of vehicle sales that the scandal materially harmed the other German automakers.

[28]Note that both Mercedes-Benz and Smart are subsidiaries of Daimler.