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Itai Ater and Nir Shlomo Yoseph

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Centre for Economic Policy Research
33 Great Sutton Street, London EC1V 0DX, UK
Tel: +44 (0)20 7183 8801
www.cepr.org

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## Abstract

This study explores how exposure of fraud committed by a durable-goods manufacturer affects secondary markets for that manufacturer's products. Specifically, we examine the effect of Volkswagen's 2015 emissions scandal ("Dieselgate") on the used car market in Israel. Using a difference-in–differences research design and administrative and proprietary data, we find that, after Dieselgate, the number of transactions involving VW-mishandled cars fell by 18%, and the resale price of these cars fell by 6%. The drop in the number of transactions was concentrated among private sellers. We discuss alternative explanations and suggest that reputational concerns and adverse selection following Dieselgate could explain our findings.



JEL Classification: D12, D80, L14, L62

Keywords: secondary markets, Durable goods, Vehicles, reputation, fraud, Product recall



Itai Ater - itai.ater@gmail.com
*Tel Aviv University and CEPR*

Nir Shlomo Yoseph - nir972@gmail.com
*Tel Aviv University*

# The Impact of Environmental Fraud on the Used Car Market: Evidence from Dieselgate*

Itai Ater† and Nir S. Yoseph†

November 13, 2020

**Abstract**

This study explores the effects of Volkswagen's 2015 emissions scandal ("Dieselgate") on the used car market in Israel. Using a difference-in–differences research design and administrative and proprietary data, we find that after Dieselgate the number of transactions involving VW-manipulated cars fell by 18%, and the resale price of these cars fell by 6%. The drop in the number of transactions was concentrated among private sellers. We discuss alternative explanations and suggest that lower willingness-to-pay and adverse selection following Dieselgate could explain our findings.

JEL: L14, L62, Q51

Keywords: secondary markets, vehicles, reputation, environmental fraud, product recall

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†Authors' Affiliation: Coller School of Management (Tel Aviv University), Tel-Aviv, Israel.

*email: ater@post.tau.ac.il*

*email: niryoseph@mail.tau.ac.il*

# I Introduction

Global climate concerns have led policymakers to adopt regulations that encourage car manufacturers to design and sell environmentally friendly cars. Car manufacturers can respond to these regulations in a variety of ways: adopting greener production technologies; changing the mix of cars that they sell; or trying to bypass these regulations, perhaps through manipulation or fraud. The path a manufacturer chooses will depend on, among other things, how markets respond when gaming or fraud related to environmental regulations is exposed. As global climate concerns continue to grow, understanding the incentives for a car manufacturer to comply with environmental regulations and the impact of such decisions will become increasingly relevant and important.

In this paper, we explore this question by examining the effects of Dieselgate, one of the major industrial scandals in recent history on the used car market. Dieselgate erupted in September 2015 after Volkswagen ("VW") admitted that its diesel vehicles had been modified to deceive emissions tests. The vehicles involved in the scam used software that dramatically reduced vehicle emissions only when air pollution tests were being conducted. Wordlwide, VW sold 11 million cars that deceitfully appeared to meet strict environmental standards. We consider Dieselgate as a natural experiment in which the scandal harmed VW's reputation. We empirically examine how the price and quantity of VW used cars changed after Dieselgate, and propose mechanisms that may have driven our findings.

The impact of Dieselgate and the associated negative information on transaction volume in the secondary market depends on the way that buyers and sellers respond to the scandal. On one hand, it seems likely that buyers' willingness-to-pay for VW used cars would fall, pointing towards fewer transactions at lower prices after the scandal. The adverse effect on the number of transactions might be larger if the scandal also exacerbates informational asymmetries between buyers and sellers. That would be the case if VW car owners, based on their first-hand experience are unswayed by the negative information and prefer not to sell their cars at a reduced price, while at the same time potential buyers, concerned that the cars on the market are lemons, choose not to buy (Akerlof [1970]). On the other hand, the volume of transactions might increase if after the scandal many owners seek to sell their cars. This might happen if owners are particularly sensitive to the new negative information about their cars.[1] If they are more likely to sell their cars after Dieselgate, then the equilibrium effect on the volume of transactions, which takes into account the impact of Dieselgate on both demand and supply, is ambiguous.

[1]This assumption is often referred to as the efficient sorting mechanism and is commonly used in theoretical and empirical papers that study secondary markets (e.g., Hendel and Lizzeri [1999], Peterson and Schneider [2014]). In these papers, owners of used cars are more sensitive to the quality of their car compared to potential buyers of used cars. As the quality of the used car depreciates, the owner sells it in the used-car market and then upgrade to a new car.

Our setting is the Israeli used car market and our analysis considers the effects of Dieselgate on the transaction volume and the resale price of VW cars, focusing on VW cars that had their emission systems manipulated. We use administrative data on all vehicle transactions that took place in Israel during the two-year period surrounding Dieselgate, coupled with data from Yad2, Israel's largest online classifieds portal. To identify the effects of Dieselgate, we use a difference-in-differences research design that compares differences before and after Dieselgate, across VW-manipulated cars (the treatment group) and all other diesel vehicles manufactured by non-VW brands, which we define as non-VW cars (the control group). Figure 1 provides preliminary evidence regarding the effects of Dieselgate on transaction volume for the control and treatment groups. The figure shows that after September 2015, the number of transactions involving manipulated vehicles dropped substantially compared with the vehicles in the control group. Our regression analysis, which controls for potential confounding variables, shows that after the scandal erupted, the number of transactions involving VW-manipulated vehicles decreased by approximately 18%. We also examine how equilibrium prices changed after Dieselgate. Figure 2 shows the listing prices for the control and treatment groups, before and after Dieselgate. After September 2015, the prices of manipulated vehicles dropped substantially compared with the vehicles in the control group. Our regression analysis confirms this pattern, and finds that the resale value of manipulated vehicles fell by nearly 6%.

<Place Figure 1 about here>

<Place Figure 2 about here>

The drop in the volume of transactions and in prices is driven to some extent by lower willingness-to-pay for VW cars after Dieselgate. In the next step of the analysis, we examine whether the drop in transactions might be explained by forces other than lower willingness-to-pay. To do so, we distinguish between private and non-private sellers (e.g., companies or leasing firms) and examine how the volume of cars sold by each type of seller changed after Dieselgate. This distinction is potentially useful because if the primary driver for the drop in transactions is a change in willingness-to-pay then private and non-private sellers would experience a roughly equivalent drop in the volume of transactions. If, however, the drop in volume is concentrated predominantly among private or among non-private sellers, then we might suspect that other forces, which are related to the behavior of sellers, are also important in explaining our findings.

The results from our regression analysis show that after Dieselgate the number of transactions by private sellers decreased by 31% compared to the number of transactions by non-private sellers. The number of transactions by non-private sellers hardly changed after Dieselgate. Thus, changes in the behavior of sellers are likely also driving our findings. We suggest that these changes could be related to increased adverse selection in the used-car market, and hence might explain why the drop in volume is observed primarily among private sellers. Specifically, following the reputational damage that VW's brand suffered, potential buyers of VW used cars were concerned about the quality of these cars. In return, current owners chose to continue using their cars rather than selling them at a lower price. In contrast, non-private sellers could better handle informational frictions (e.g., offer warranties to potential buyers) and therefore we do not find a significant drop in the volume of transactions by non-private sellers. Hence, a higher level of adverse selection in the used car market could manifest itself through fewer transactions involving private owners of VW used cars, and these cars are sold at lower prices. Note however that we only observe equilibrium outcomes and therefore we cannot rule out that some private sellers decided to sell their cars due to the scandal.

A potential concern with our findings is that the demand for cars in the control group was also affected by the scandal, as these cars could be purchased as substitutes for the cars in the treatment group. Thus, our analysis might capture only the relative effect of Dieselgate and our coefficients might be potentially biased upward. To alleviate this concern, in section IV(v) we conduct several analyses. In one of these analyses, we rerun the main empirical analysis using alternative derivations of the treatment and control groups. The construction of these new groups is meant to decrease the substitution between vehicles that belong to the control group and vehicles that belong to the treatment group. For instance, we include only passenger cars in the treatment group, which are the most common cars involved in the scandal, and in the control group we include only non-passenger cars, which are the most common cars within the control group (non-VW diesel cars). If the concern is valid then the coefficients we obtain in this analysis should be smaller. However, we find that the coefficients in all the specifications we conduct are not statistically different from the base specification, suggesting that substitution between the groups is unlikely to completely explain our findings.

Our paper is related to three streams of literature. First, our paper complements studies that examine gaming and fraud by firms. Several theoretical and empirical papers examine, though typically not in the context of regulations, how gaming and fraud by firms impact market outcomes (e.g., Rhodes and Wilson [2018], Karpoff and Lott Jr [1993], Karpoff, Lott and Wehrly [2005]). In two recent papers, Reynaert and Sallee [forthcoming] and Reynaert [forthcoming]) find that car manufacturers use technology adoption and gaming of emission tests to comply with the EU-wide emission standards. Few studies specifically investigate the implications of Dieselgate. Notably, Bachmann et al. [2019] find that sales in the primary market of non-VW German car manufacturers (BMW, Mercedes-Benz and Smart) significantly dropped after Dieselgate. They also quantify the substitution patterns that drive this effect and provide evidence that collective reputation externalities matter for firms. Strittmatter and Lechner [2020], Che, Katayama and Lee [2020] examine the effect of Dieselgate on the used-car market but only observe list prices.

Second, our paper is related to the literature on trade in durable-goods markets. At least since Akerlof [1970], economists have been studying the efficiency of secondary markets and the role of frictions in these markets. The theoretical discussion in this literature points out that adverse selection (Akerlof [1970]) and sorting (e.g., Swan [1970], Swan [1971] and Waldman [1996]) are important mechanisms in durable good markets. Hendel and Lizzeri [1999] is the first theoretical paper that combines both adverse selection and sorting in one framework. Our analysis lends support to Hendel and Lizzeri [1999], showing that the volume and the price of less reliable brands fell more than the respective change in the price and the volume of used cars manufactured by more reliable brands. The used car market, which is probably the largest secondary market,[2] was the focus of most of this work. Studies that used data from used-car markets have shown that secondary markets allow traded goods to be allocated to those who value them most Gavazza, Lizzeri and Roketskiy [2014] and affect car manufacturers' profits (Chen, Esteban and Shum [2013]).

Finally, our paper is related to the product recall literature. Product recalls are prevalent in many industries, including the toy, food, drug and auto industries. Early studies investigated how safety recalls in the auto industry affect stock markets and sales in primary markets (e.g., Jarrell and Peltzman [1985], Marcus, Swidler and Zivney [1987], and Barber and Darrough [1996]). Recent studies also explored the effects of recalls in the toy and food industries (Freedman, Kearney and Lederman [2012] and Ferrer and Perrone [2018], respectively). Two studies explored the effects of recalls on prices of used cars. Hartman [1987] used aggregated data on resale prices and found that the resale value of recalled products diminishes with the severity of the safety event that triggered the recall. Hammond [2013] studied the consumer response to Toyota's unintended acceleration recalls and did not find a significant effect on the resale price of the recalled vehicles. Our paper adds to this literature by exploring how a recall triggered by a fraudulent violation of an environmental regulation affects both the volume of transactions and the resale value in the secondary market. Our capacity to examine how different types of market participants respond to the scandal enables us to better understand the mechanisms through which recalls might affect market outcomes.

[2] Based on Edmunds's used vehicle report, over 10 million cars were traded in the U.S. during the 2nd quarter of 2019. See https://static.ed.edmunds-media.com/unversioned/img/industry-center/insights/used-market-reports/q2-2019-used-car-report.pdf

# II Dieselgate

## II(i) Dieselgate throughout the world

Dieselgate, one of the largest environmental frauds of all time, began on September 18, 2015, after the U.S. Environmental Protection Agency revealed that VW diesel vehicles had been modified to deceive emissions tests. The installed software detected when the car was undergoing official emissions testing, and it turned on full emissions controls only during the test. During normal operation, the vehicles emitted nitrogen oxides (NOx) at levels of up to 40 times the standard.[3] On September 22, 2015, VW confirmed the EPA's allegations, admitting that the illegal software was installed in 11 million vehicles worldwide and that it had made a provision of €6.5 billion to cover the expected fines and recall costs. This provision increased to €16.2 billion by the end of 2015 and to €29 billion by the end of 2018. VW's deception was not aimed just at passing the emissions tests. During those years VW enjoyed substantial tax credits and subsidies for selling "clean" diesel vehicles. For example, in the U.S. buyers of a 2010 2.0L VW Jetta received roughly a $1,300 tax credit, and in Israel the average tax credit was more than $1,500 per car.

Soon after the fraud was exposed, VW's stock price plunged nearly 35%, and the company's CEO resigned. By October 2, shareholders' losses had reached nearly $45 billion. Dieselgate also resulted in the largest recall in the history of VW—a recall that was notable in that, unlike other large recall events, it did not involve safety issues but rather concerned a violation of an environmental regulation.[4] The recall included most of the diesel passenger cars VW produced between 2009 and 2015, and amounts to approximately 18% of VW's total new car transactions between 2009 and 2015. By November 2015, VW announced that it had found a technical solution for most of the recalled vehicles. This solution was useful in Europe and in other markets that used the European emission standards, including Israel. Due to stricter NOx limits in the U.S. and Canada, a solution for vehicles sold in those markets was only offered in 2017.

[3] Valentine. J.P, 2015, "EPA, California Notify Volkswagen of Clean Air Act Violations / Carmaker allegedly used software that circumvents emissions testing for certain air pollutants," United States Environmental Protection Agency, September 18. Available at: https://19january2017snapshot.epa.gov/newsreleases/epa-california-notify-volkswagen-clean-air-act-violations-carmaker-allegedly-used_.html.

[4] Clane, J., 2015, "Volkswagen Emissions Scandal: What 'Dieselgate' Means for VW Owners", INDEPENDENT, October 1. Available at: www.independent.co.uk/life-style/motoring/motoring-news/volkswagen-emissions-scandal-what-diesel-gate-means-for-vw-owners-a6675376.html.

## II(ii) Dieselgate in Israel

As of 2015, 3.7% of the passenger cars in Israel ran on diesel.[5] The share of diesel passenger cars in Israel is much lower than the share of diesel vehicles in Europe (approximately 41.2%), but it is somewhat higher than the corresponding figures in the U.S. (3%) and in Japan (1.4%).[6] At the time of Dieselgate, the market share of VW (including all its brands) in the diesel segment in Israel was slightly greater than 10%, and the number of recalled vehicles was approximately 11,000. As in other countries, these vehicles were recalled to be fixed—free of charge—such that they would be in compliance with emissions standards.

<Place Figure 3 about here>

The Israeli media covered the scandal and its implications extensively. To provide a sense of the public attention paid to the Dieselgate scandal in Israel, Figure 3 compares the Google Trends index for the search terms "Volkswagen" and "Mazda", for searches conducted in Israel from October 2014 to September 2016. The index values represent the search interest relative to the highest point of a given search, which is defined by the keyword of the search, the region of the search and its time frame. The Mazda search term is a useful comparison group because at the end of October 2015, Mazda initiated in Israel a recall of more than 70,000 gasoline-powered vehicles manufactured between 1998 and 2005.[7] Unlike Dieselgate, the Mazda recall was triggered by a minor safety-related issue and did not involve fraudulent behavior. As Figure 3 shows, in the period before Dieselgate, the average Google Trends index of the term "Volkswagen" was 29, and the average Google Trends index of the term "Mazda" was 69. The difference in the search intensity for the two terms is probably driven by the fact that Mazda's market share in Israel is 2.3 times larger than Volkswagen's. More importantly, following the scandal, the popularity of the search term "Volkswagen" in Israel was nearly 180% higher than usual, whereas the popularity of the search term "Mazda" was only 5.7% higher. In fact, during the scandal, the popularity of the search term "Volkswagen" was higher than the popularity of the search term "Mazda", despite the considerable difference between the two manufacturers' market shares in Israel.

[5] CBS (Israel), 2016, "3.09 Million Motor Vehicles in Israel in 2015," Central Bureau of Statistics (Israel), March 30. Available at: http://www.cbs.gov.il/www/hodaot2016n/27_16_085b.pdf.

[6] Source: ACEA (European Automobile Manufacturers' Association) statistics. Available at: http://www.acea.be/statistics/tag/category/passenger-car-fleet-by-fuel-type.

Chambers M. & Schmitt R., 2015, "Diesel-powered Passenger Cars and Light Trucks," United States Department of Transportation, Bureau of Transportation Statistics, October 2015, Available at: https://www.rita.dot.gov/bts/sites/rita.dot.gov.bts/files/DieselFactSheet.pdf.

Japan Automobile Manufacturers Association. "Motor Vehicle Statistics of Japan, 2016," Available at: http://www.jama.org/wp-content/uploads/2016/11/mvs2016.pdf.

[7] Posek, H., 2015, "Huge recall for Mazda in Israel - 71 thousands vehicles", Ynet, Available at: https://www.ynet.co.il/articles/0,7340,L-4720387,00.html.

# III Data and descriptive statistics

## III(i) Data

We use data from Israel's Ministry of Transport and Road Safety ("MOT") and from Yad2, Israel's largest online classifieds portal. The MOT administrative dataset includes rich information about the universe of transactions in both the primary and the secondary markets of vehicles in Israel. In particular, these data include information about the transaction date, number of previous owners, make, mileage, vehicle ID, vehicle brand and model, and buyer and seller type. The seller type takes one of the following values: "Private", "Company", "Lease", "Dealer" and "Rent". Because of the low number of monthly transactions for some of these sub-categories, we group all non-private types into one category. We restrict attention to diesel cars that were manufactured in the same years as VW-manipulated cars (between 2009 and 2015), and focus on 60,900 transactions for these vehicles in the secondary market between October 2014 and September 2016. In the main analysis, we aggregate these data to the brand-month level,[8] and in further analysis aggregate the data to the seller type-brand-month level.

Since the MOT data do not contain information on resale price, we use data from Yad2 to investigate Dieselgate's effect on the prices of used VW-manipulated vehicles. Yad2 receives more than 500,000 daily visits (more than 16 million per month, almost double of Israel's total population) and 10,000 new listings per day. Yad2 leads all the important classified categories (real estate, vehicles and used items) and is ranked fourth among all local Israeli websites.[9] Each of Yad2's vehicle listings contains data about the listing date (date of creation), listing price (the final posted price of the listing), and information describing the vehicle offered for sale, such as the brand, model, gear type, year of manufacture, engine displacement (liters), distance traveled, number of previous owners, and the category of each vehicle (subcompact, compact, mid-size, full-size, sport, commercial vehicle, SUV, minivan and van). All listings on Yad2 are for private sellers. Unfortunately, we cannot match the data from Yad2 with the MOT data and therefore do not know which listings resulted in a sale. In the analysis, we consider Yad2 listings of diesel vehicles posted between September 2014 and August 2016, for which the listing price and the information on car characteristics were available. To be consistent with the MOT data, we consider only listings of vehicles produced by manufacturers with more than 240 transactions during the two-year period. The Yad2 sample contains 11,648 observations that meet these criteria after the elimination of

[8] We drop 847 transactions of vehicles manufactured by car manufacturers that had fewer than 240 transactions during the two year period. The excluded manufacturers include Audi and Seat, two makes that belong to the VW Corporation. The results are robust to the inclusion of these brands.

[9] Gillmore, 2015, "Israel's Leader in Classifieds", Axel Springer. Available at: http://www.axelspringer.de/dl/21029312/11_Gillmore_Yad2.pdf.

outliers.[10]

## III(ii) Descriptive statistics

Table I presents descriptive statistics for the main variables in the MOT and Yad2 samples. For example, the listing price ranges from $1,026 to $98,718,[11] the distance traveled ranges from 5,200 kilometers to 495,000 kilometers, and the engine displacement ranges from 1.0L to 7.2L. To assess the representativeness of the Yad2 data, we computed the Pearson's correlation between the MOT and Yad2 data for the market shares of each manufacturer and car category (0.8 and 0.9, respectively). As shown in panel c of Table I, Renault and Citroen have the largest market shares in both data sources, each with over 10%. Likewise, Opel, Rover, Chrysler and Volvo have the smallest market shares in both data sources.

Our research design considers VW-manipulated vehicles as the treatment group ("Dieselgate Volkswagens"),[12] and non-VW diesel vehicles manufactured between 2009 and 2015 as the control group ("other brands"). As previously mentioned, Figure 1 suggests that after September 2015, the number of transactions involving Dieselgate Volkswagens declined relative to the control group. Specifically, in the 12 months before the scandal (the "baseline" period), the average number of monthly transactions involving Dieselgate Volkswagen vehicles was 230, and the average number of monthly transactions involving other brands was 1,931. In the 12 months following the scandal (the "follow-up" period), the average number of monthly transactions involving Dieselgate Volkswagens vehicles was 243, whereas the average number of monthly transactions involving other brands was 2,587. Although the average number of monthly transactions increased in both groups, the relative increase in the treatment group was much smaller: 5.7% vs. 33.9% in the control group.

An alternative means of observing the change in volume of transactions following Dieselgate is to focus on "share-of-stock", i.e., the number of vehicles sold during a given period out of the stock of vehicles of the same brand that were in operation during that period. Figure 4 presents this information at the monthly level, for the control and treatment groups. Specifically, for a given month, we compute a vehicle's "stock" in that month as the number of those vehicles sold in the

[10] We exclude 599 observations with extreme values in terms of list price (less than $1,000 or more than $100,000) and distance traveled (less than 5,000 KM and more than 500,000 KM). The fact that users manually fill out Yad2 listings is the main cause of outliers.

[11] We convert the original list price (in shekels) to USD using an exchange rate of 3.9 NIS per USD, which is roughly the average exchange rate during the sample period

[12] VW diesel vehicles that were manufactured between 2009 and 2015 and that contain the software that circumvented the emissions standards. Our data set does not explicitly indicate whether a car includes the cheating device, so we screen vehicles based on brand, the year of production, engine displacement (1.2L, 1.6L, 2.0L and 3.0L) and specific models mentioned in media reports (e.g., Skoda Octavia, Skoda Fabia, Skoda Rapid, Skoda Superb, Volkswagen Jetta, Volkswagen Passat, Volkswagen Golf).

primary market until 12 months prior to that month. According to the figure, an average of 2.7% of Dieselgate Volkswagens' stock was involved in monthly transactions during the baseline period, while an average of 2.3% of other brands' stock was involved in monthly transactions during the baseline period. In the follow-up period, Dieselgate Volkswagens' share-of-stock decreased to 2.3%, whereas other brands' share-of-stock increased to 2.6%.

<Place Figure 4 about here>

Turning to the effect of Dieselgate on prices, Figure 2 shows that after September 2015 the resale value of VW diesel vehicles substantially decreased. According to the figure, in the baseline period, the average monthly asking price of Dieselgate Volkswagens was $24,100, whereas the average monthly asking price of other brands was $29,400. In the follow-up period, the average asking price of Dieselgate Volkswagens decreased to $20,500 (a decrease of $3,600), whereas the average asking price of other brands decreased to $28,400 (a decrease of $1,000). Although vehicles in both groups experienced a decrease in the average asking price, the relative decrease in Dieselgate Volkswagens was much larger: 14.8% vs. 3.7%.

Overall, Figures 1, 2 and 4 strongly suggest that Dieselgate negatively affected both the number of transactions and the resale value of VW-manipulated cars. Nevertheless, these results might also be driven by changes in the characteristics of cars sold before and after September 2015. For instance, if cars sold in the follow-up period were older or had traveled longer distances, then these characteristics might explain why the prices of VW cars decreased. To address such concerns, we conduct regression analyses in which we control for these and other characteristics.

<Place Table I about here>

# IV Estimation and results

Our empirical strategy compares outcome variables in the treatment group – VW diesel vehicles that included the software that circumvented EPA emissions standards – and outcome variables in the control group – non-VW diesel vehicles that were manufactured in the same years as the vehicles in the treatment group. The identifying assumption is that the underlying trends for these two groups would have been similar after September 2015 if the scandal had not occurred; Figures 1-4 show that the pre-trends were approximately parallel. A separate concern is that the control group is not a valid control, since the vehicles in that group might be substitutes for the vehicles in the treatment group. If this is indeed the case, then we may obtain biased estimates in our difference-in-differences analysis. We address this concern in section IV(v).

## IV(i) Dieselgate's effect on the volume of transactions

We use the MOT data to estimate the following difference-in-differences specification:

$$y_{i,t} = \beta_0 + \beta_1(Dieselgate\,Volkswagens_i) \times (POST_t) + \gamma_i + \delta_t + \epsilon_{i,t} \tag{1}$$

The dependent variable $y_{i,t}$ is either $ln(Transactions)_{i,t}$ – the natural logarithm of the number of transactions in month $t$ involving vehicles of brand $i$, or *Share-of-stock*$_{i,t}$ – the number of transactions in month $t$ involving vehicles of brand $i$ divided by the total stock of vehicles of brand $i$ in month $t$. $POST_t$ is a dummy variable that equals one if the transaction occurred during the follow-up period, i.e. between October 2015 and September 2016. *Dieselgate Volkswagens*$_i$ is a dummy variable indicating whether a vehicle belongs to the treatment group, and *(Dieselgate Volkswagens*$_i$*)*$\times$*(POST*$_t$*)* is an interaction variable. Finally, $\gamma_i$ and $\delta_t$ are brand- and month-level fixed effects, respectively. These fixed effects control for prevailing market conditions and time-invariant brand preferences. We cluster the standard errors at the brand level. The coefficient on the interaction term, $\beta_1$, is the main coefficient of interest, and it captures the effect of Dieselgate on the number of transactions involving VW-manipulated vehicles after September 2015.

The results, presented in Table II, suggest that Dieselgate had a statistically significant negative effect of nearly 18% on the number of transactions involving VW-manipulated vehicles. The analysis of Dieselgate's effect on the share-of-stock yields similar qualitative results. Following the Dieselgate scandal, the share of transactions in VW-manipulated vehicles relative to its stock declined by 43 basis points.

<Place Table II about here>

## IV(ii) Dieselgate's effect on resale value

To estimate the effect of Dieselgate on the resale value of VW-manipulated vehicles, we use the Yad2 data and estimate the following difference-in-differences regression:

$$\begin{aligned} ln(price)_{i,t} = {} & \beta_0 + \beta_1(Dieselgate\,Volkswagens_i) \times (POST_t) + \beta_2 X_i + \\ & \gamma_i + \delta_t + \epsilon_{i,t} \end{aligned} \tag{2}$$

In this analysis each observation is a single listing. The dependent variable, $ln(price)_{i,t}$, is the natural logarithm of the asking price of listing $i$ that was posted in month $t$; $\gamma_i$ is brand fixed of listing $i$; $X_i$ is a vector of a the specific car's characteristics in each listing that includes: gear type (automatic or manual), age (years), engine displacement (liters), distance traveled (10,000 kilometers) and the number of previous owners. $X_i$ also includes a category-level fixed effect (e.g., SUV, compact, commercial vehicle) that controls for unspecified category-related factors such as quality and target markets. The other variables are defined as in section IV(i). The baseline period includes listings placed between September 2014 and August 2015; the follow-up period includes listings placed between September 2015 and August 2016.[13] We cluster the standard errors at the brand level.

The estimation results are reported in Table III. Recall that the Yad2 data only cover private sellers. In column 1, we estimate the base specification without the vehicle characteristics and obtain a difference-in-differences coefficient of -0.09, implying a statistically significant 9% drop in the resale price of VW-manipulated cars after Dieselgate. After adding the vehicles' characteristics (column 2), we find that the coefficient on the change in resale value drops to -0.06 (6%). The signs of the coefficients on the vehicle characteristics are as expected. For example, an increase of one year in a car's age is associated with a decrease of 11% in the vehicle's value, and an additional owner is associated with a decrease in the value of the car by 5%. The decline in the resale value of VW vehicles, and the drop in the volume of transactions involving those vehicles documented in section IV(i), suggest that buyers' willingness-to-pay for VW vehicles was adversely affected by the scandal. However, the decline in volume and in prices may have been also affected by changes in sellers' behavior after Dieselgate. In the next section we examine this possibility.

<Place Table III about here>

## IV(iii) Dieselgate's effect on transaction volume by seller type

We investigate whether different types of sellers—namely, private versus non-private sellers—responded differently to the scandal. Specifically, we compare the change in the number of transactions of VW-manipulated vehicles across private and non-private sellers before and after Dieselgate. To this end, we estimate the following equation:

$$\begin{aligned} y_{i,s,t} \quad = \quad & \beta_0 + \beta_1(Dieselgate\,Volkswagens_i) \times (Private_s) \times (POST_t) + \\ & \beta_2(Dieselgate\,Volkswagens_i) \times (POST_t) \; + \beta_3(Private_s) \times (POST_t) + \\ & \beta_4(Dieselgate\,Volkswagens_i) \times (Private_s) + \beta_5(Private_s) + \gamma_i + \delta_t + \epsilon_{i,s,t} \end{aligned}$$

(3)

The dependent variable $y_{i,s,t}$ is either $ln(Transactions)_{i,s,t}$, which is the natural logarithm of the number of transactions in month $t$ involving vehicles made by brand $i$ and sold by a seller of type

[13]We include listings created in September 2015 in the follow-up period because the dependent variable in this analysis is the last price that a seller sets. We also note that sellers can update their listings at any time, and that the average duration of listings is well over one month. Our results are robust to the definition of the follow-up period and do not change if we assume that the follow-up period starts in October 2015.

$s$ (i.e. private or non-private); or the $\textit{Share-of-stock}_{i,s,t}$, which is the number of transactions in month $t$ involving vehicles of brand $i$ and sold by seller $s$ divided by the total stock of vehicles made by brand $i$ and held by seller $s$ in month $t$.[14] $POST_t$, $\textit{Dieselgate Volkswagens}_i$, $\delta_t$ and $\gamma_i$ are the same as in equation 1. $Private_s$ is a dummy variable that indicates whether the vehicle's seller is private or non-private. We cluster the standard errors at the brand level. The coefficient on the triple indicator variable, $(\textit{Dieselgate Volkswagens}_i)\times(POST_t)\times(Private_s)$, is our main coefficient of interest, and it captures the differential effect of Dieselgate on the number of transactions by different types of sellers.

The results, presented in Table IV, indicate that Dieselgate had a large effect on private sellers and a negligible effect on non-private sellers. When we use the number of transactions as the dependent variable, the difference-in-difference-in-differences coefficient, $\beta_1$, is -0.31, and it is -68 basis points when we use the share-of-stock as the dependent variable.[15] In a separate analysis, we also examined the impact of Dieselgate on the number of vehicles purchased by non-private entities. We find that dealers bought significantly fewer VW-manipulated cars after the scandal. Accordingly, it seems likely that the effect we report (i.e. that the volume of sales by non-private sellers was not substantially affected by Dieselgate) is driven by the sales of the existing stock of vehicles held by dealers in September 2015, and by sales by other non-private sellers (leasing firms and companies).

<Place Table IV about here>

Why did private sellers experience a large drop in transactions, whereas non-private sellers only witnessed a negligible change? We claim that increased adverse selection in the used-car market can explain this difference. Specifically, we contend that Dieselgate damaged VW's reputation and raised concerns about the reliability of its used cars mostly among potential buyers. In contrast, owners of the used-cars, who have first-hand information on the reliability of their cars, were less sensitive to these concerns. Therefore, owners prefer to continue using their car rather than to sell it at a relatively low price.[16] Unlike private sellers, non-private sellers can better handle informational asymmetries (e.g., through warranties and repeat-business concerns) and

[14] The total stock of vehicles made by *Brand i* and held by *Seller s* in *Month t* was calculated as the accumulated sales of new cars of that brand until one year before (until *t-12*) while also taking into account transactions in the secondary market that may change the type of ownership.

[15] 42% of the non-private transactions are by leasing companies (lessors) who mostly serve corporations with a fixed term lease of 3 years. Another 21% of non-private transactions are by corporations and governmental agencies who own their vehicles, and the rest (37%) are by car dealers. We get similar results when we drop transactions made by dealers.

[16] Our findings are also consistent with Hendel and Lizzeri [1999] who show that in the presence of adverse selection, goods produced by unreliable brands are likely to experience greater decreases in price in volume compared with goods produced by reliable brands. Accordingly, assuming that Dieselgate harmed the reputation of VW then we should experience a drop in volume and in prices.

this difference could explain why they do not experience a significant drop in their volume of transactions after Dieselgate.[17]

## IV(iv) Additional results

### IV(iv)(a) Dynamics of the Dieselgate effect

To examine the dynamic effect of Dieselgate, we break down the difference-in-differences coefficient in equations 1 and 2 to the month level. Figure 5 shows the monthly effects that Dieselgate had on the number of transactions and on the share-of-stock involving VW-manipulated vehicles. The figure indicates that the negative effect of Dieselgate gradually increased over the first few months of the follow-up period. In addition, the effect remained negative throughout the entire year following the scandal, suggesting that the market for VW diesel vehicles did not recover. Figure 6 presents a similar breakdown of the Dieselgate effect on resale price. Like Figure 5, the figure shows that the negative effect on the resale value of VW's manipulated cars remained negative during most of the follow-up period.

<Place Figure 5 about here>

<Place Figure 6 about here>

### IV(iv)(b) Dieselgate effect on price update

To further study the effects of Dieselgate on sellers' pricing behavior, we analyze changes in the asking price within listings on Yad2. Specifically, we examine whether the average percentage change in the asking prices for VW-manipulated cars changed after September 2015. To do so, we use another dataset from Yad2 that also includes the first price of each listing (the price of the listing when it was posted). This information is available for only 85% of the listings in the main analysis (8,268 observations). We use this dataset to estimate the following OLS equation:

$$\begin{aligned} \Delta price_{i,t} &= \beta_0 + \beta_1 (Dieselgate\,Volkswagens_i) \times (POST_t) + \beta_2 X_i + \\ &\quad \gamma_i + \delta_t + \epsilon_{i,t} \end{aligned} \tag{4}$$

The dependent variable, $\Delta price_{i,t}$, is the percentage change between the initial and the final price of listing $i$ that were posted in month $t$. The other explanatory variables are similar to

[17] A related explanation for the differential impact of Dieselgate on private and non-private sellers would emphasize the opportunity cost of keeping a used car longer. Arguably, private sellers can continue driving their used cars and face lower opportunity costs compared with non-private sellers who keep the used cars in large parking lots. In this case, we can expect that private sellers will have a more elastic response to changes in the market compared to non-private sellers. Therefore, the impact of Dieselgate on the number of transactions by private owners will be larger than the corresponding impact on non-private sellers.

those used in equation 2. The results are shown in Table V. According to this analysis, after Dieselgate the magnitude of the average negative price update in listings for VW-manipulated vehicles increased by nearly 100 basis points. In our sample, the average price update of a VW-manipulated vehicles in the baseline period was -2.1%. Our estimate implies that following the scandal, the average price update increased by nearly 50%. This result suggests that following the scandal, sellers of VW used cars had to make larger "compromises" relative to their initial asking prices compared with owners of non-VW vehicles. This result also indicates that sellers did not instantly and accurately update their vehicles' resale value.

<Place Table V about here>

#### IV(iv)(c) Dieselgate effect on the number of new listings

We also examine the effect of Dieselgate on the number of new listings uploaded by private sellers to the Yad2 website. The results of this analysis, reported in Table A.1 in the online appendix, suggest that the number of listings for VW-manipulated vehicles posted by private sellers decreased by nearly 9% following the scandal (p-value = .12). These results support the findings of our difference-in-differences analysis regarding the decrease in transaction volume among private sellers.

### IV(v) Robustness

#### IV(v)(a) Substitution to non-VW cars

After Dieselgate, potential buyers of VW cars that had their emission system manipulated may seek to buy other cars. Among these alternatives, we mention new cars, non-diesel used car and non-VW used diesel cars. In the main empirical analysis, we included non-VW used diesel cars in the control group. This seems intuitive since these cars are probably the closest substitutes to the cars in the treatment group, which likely have similar underlying demand and supply trends. Accordingly, our estimation likely controls reasonably well for common unobserved market level changes. Nevertheless, a potential concern with our estimates may arise if many buyers substitute to non-VW diesel cars after Dieselgate. Such a shift may significantly raise the price and quantity of cars in the control group and indirectly bias our estimates upward.

<Place Table VI about here>

To show why this concern is unlikely to affect our results, we repeat the analyses using treatment and control groups which arguably exhibit lower level of substitution. By reducing the degree of substituability between the control and treatment groups, we can examine how sensitive our estimates to substitution concerns. In particular, we expect low substitutability between passenger cars (subcompact, compact, mid-size and full-size in our samples) and non-passenger cars (commercial vehicle, SUV, minivan and other in our sample).[18] Thus, we can use VW-manipulated passenger cars as the treatment group, and non-VW non-passenger cars as the control group. This analysis also takes advantage of the size of the underlying groups of cars, where VW is the dominant car manufacturer among passenger cars whereas other manufacturers are dominant in the non-passenger segment.[19]

Given the low substitutability between passenger and non-passenger cars, and the differences in the shares of VW and non-VW in these categories, we expect that this analysis will not be subject to the substitution concern. Accordingly, if the initial estimates we obtained are biased upward then the estimates in these alternative specifications should be smaller. Column 2 in Tables VII, VIII and IX show the results of this analysis. For ease of comparison, column 1 in each of these tables includes the estimates from our main specifications (shown in Tables II and III). In all cases, the estimated difference-in-differences coefficients remain negative and statistically different from zero. In all cases the magnitude of the coefficients somewaht increased though the difference vis-a-vis the original estimation is statistically insignificant. In columns 3-7 we consider other derivations of the treatment and control groups and obtain similar results. For example, in column 3 the treatment group includes only (VW-manipulated) passenger cars and the control group includes only (non-VW) commercial vehicles and minivans, the two non-passenger car categories with the lowest substitutability for passenger cars. In column 4 we add the full-size vehicles to the control group, so the treatment group includes subcompact, compact and mid-size (VW-manipulated) cars, and the control group includes all non-passenger cars and full-size (non-VW) cars. Finally, the online appendix presents an analysis in which the treatment and control groups are defined so as to increase substitutability. The results of these analyses are all similar to those in our main analysis. Taken together, the results presented in this section suggest that spillover concerns are unlikely to have biased our results.

### IV(v)(b) Additional robustness

We performed additional analyses to further examine the robustness of our findings. For instance, we verified that the results are unchanged when we excluded transactions conducted in September 2015 and listings uploaded in August or in September 2015. In a separate analysis, we included vehicles manufactured prior to 2009 and obtained similar results. These additional results are

[18] In recent years some SUVs became substitutes for passenger cars. However, those SUVs, which are known as "Crossover SUVs" by professionals usually use gasoline engine.

[19] Table VI shows that transactions involving passenger cars account for 82% and 69% of transactions and listings of VW cars in the MOT and Yad2 samples, respectively. Non-passenger cars, in turn, are more common among other car manufacturers, accounting, respectively, for 69% and 76% of transactions and listings in the MOT and Yad2 samples for non-VW manufacturers. The number of transactions involving non-VW non-passenger cars is roughly 34 times larger than the number of transaction involving VW non-passenger cars.

reported in the online appendix. Finally, we also calculated the standard errors of our results using different types of bootstrapping techniques and verified that the results did not change.

<Place Table VII about here>

<Place Table VIII about here>

<Place Table IX about here>

## V Concluding remarks

In this paper, we have shown that a fraudulent behavior by manufacturer may have non-trivial and long-lasting effects on secondary markets. Specifically, we study the effects of Dieselgate, one of the major industrial scandals in recent history, on the used car market in Israel. Analyzing a comprehensive sample of administrative and proprietary data from the two-year period surrounding Dieselgate, we find that both transaction volume and price of VW-manipulated cars dropped considerably after the scandal. The drop in transaction volume was concentrated among private sellers, and persisted throughout the year after the scandal began.

Overall, our findings are consistent with lower willingness-to-pay for VW's cars and with increased adverse selection in VW's used-car market. Our analysis that distinguishes between private and non-private sellers is useful in showing that a change in buyers' willingness-to-pay cannot solely drive our results. if the drop in the volume of transactions was driven primarily by changes in buyers' willingness-to-pay, then the drop in the volume of transactions should be roughly similar across private and non-private sellers. In contrast, if adverse selection is also driving the drop in transactions, then we should expect that the drop in volume will be concentrated among private owners. Notably, private sellers are more prone to adverse selection concerns compared to non-private sellers who can offer warranties or other means to overcome informational frictions.

The Dieselgate scandal involved violations of environmental regulations and probably resulted in damage to the environment. Given the nature of these violations, we can assume that buyers and sellers of VW polluting cars do not fully internalize the social costs of VW's fraudulent behavior. Accordingly, one may question whether the decisions of individuals to trade in secondary markets would have been different had the underlying fraudulent behavior involved safety issues, with no externalities. We leave this to future research.

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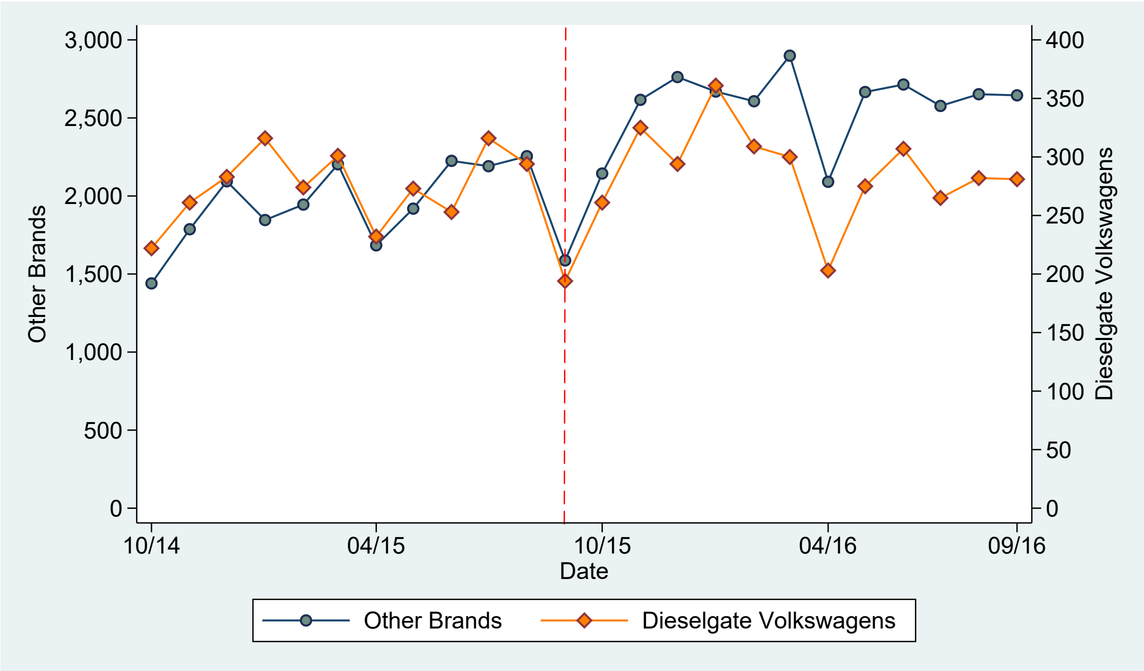
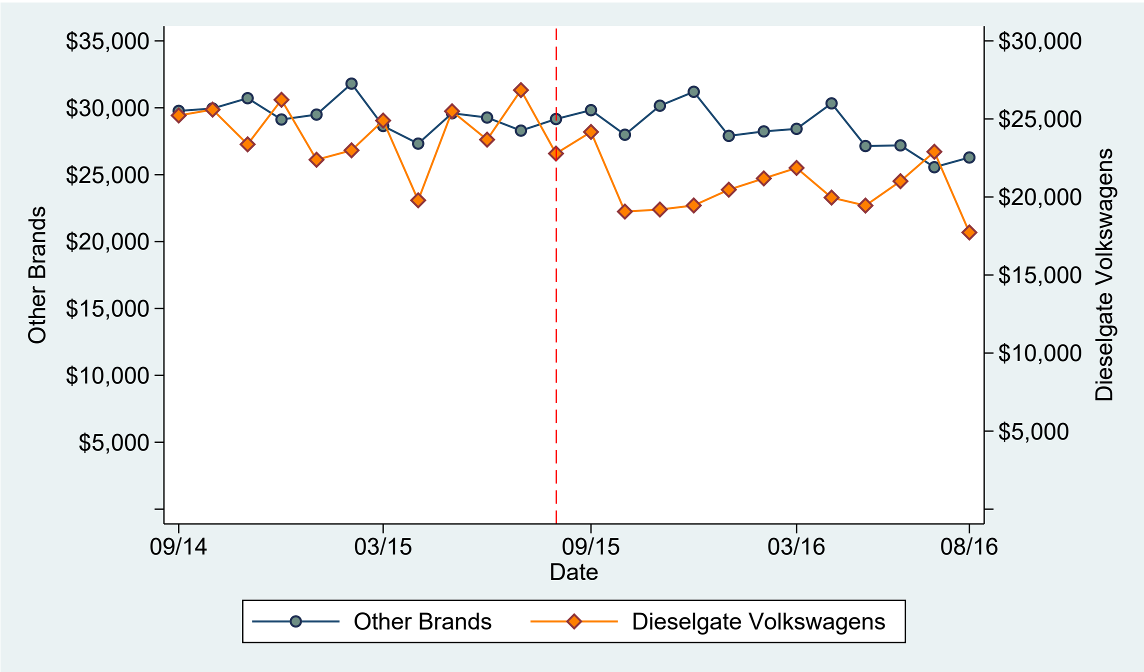
Figure 1: Number of transactions, Dieselgate Volkswagens vs. other brands




The figure separately shows the number of monthly transactions involving diesel vehicles for Dieselgate Volkswagens (the treatment group) and for other brands (the control group) during the baseline period (October 2014 to September 2015) and during the follow-up period (October 2015 to September 2016). The left axis corresponds to the number of transactions in the control group, while the right axis is for the treatment group. The two groups show similar patterns in the baseline period. The number of transactions involving vehicles in the treatment group decreased in the follow-up period compared to the number of transactions in the control group. The decline in the number of transactions in September 2015 for both the treatment and control groups is due to the High Holy Day season in Israel.

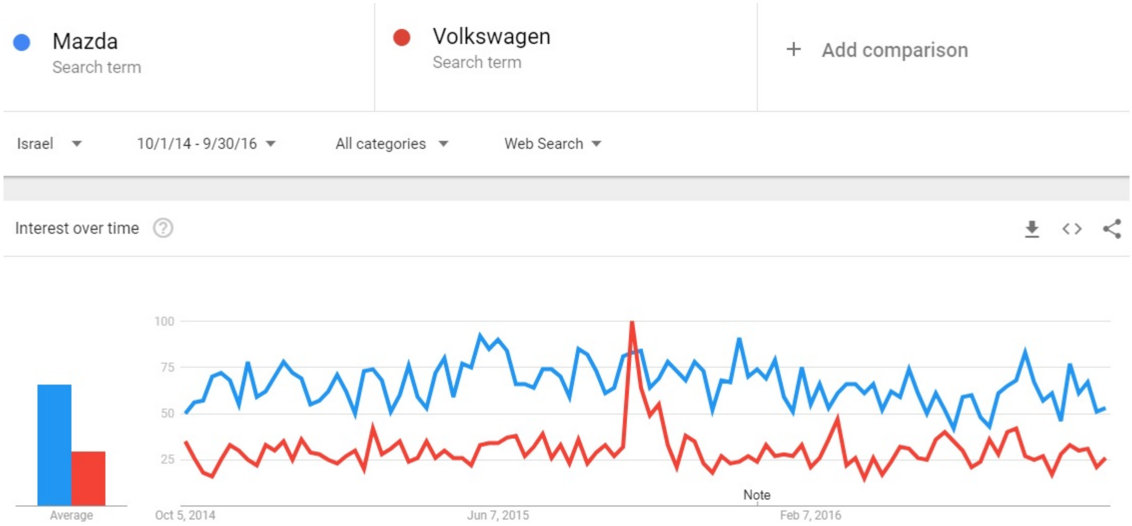
Figure 2: Average asking price ($), Dieselgate Volkswagens vs. other brands




This figure separately shows the monthly average asking price of diesel vehicles for Dieselgate Volkswagens (the treatment group) and for other brands (the control group). As the figure shows, the average asking price of Dieselgate Volkswagens decreased during the follow-up period (September 2015 to August 2016) compared to the trend in the average asking price of other brands. The drop in the asking price is observed for listings uploaded already in August 2015, because we calculate the average asking price for these listings based on the final listing price.

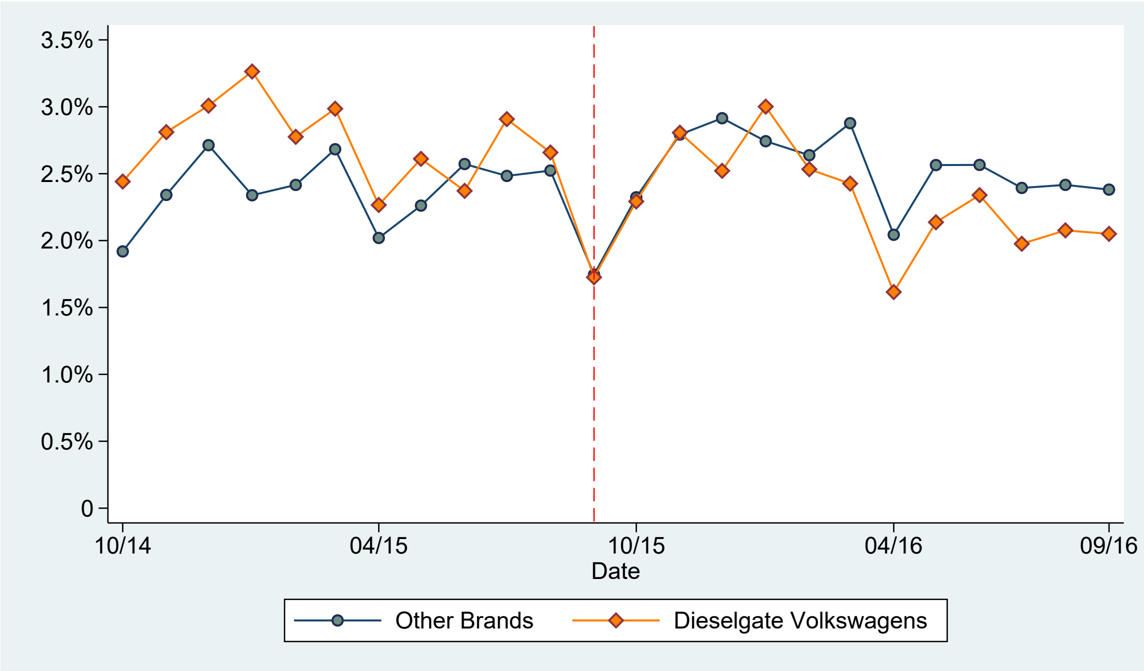
Figure 3: Public interest in Volkswagen

The figure shows the Google Trends index of the term “Volkswagen” (red line) and compares it to the Google Trends index of the term “Mazda” (blue line) for the period from October 2014 to September 2016, for searches made within Israel.




Source: Google Trends. The index results are based on searches for the terms “Volkswagen” and "Mazda" (for the period from October 2014 to September 2016) that were conducted from within Israel.

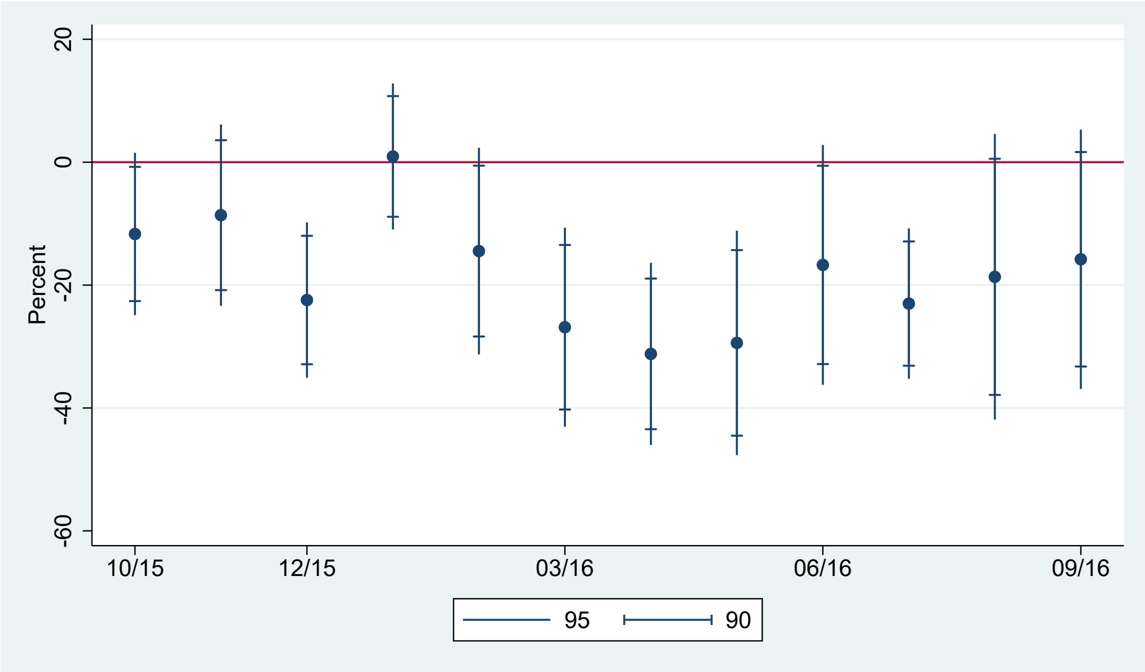
Figure 4: Share of transactions out of the total stock, Dieselgate Volkswagens vs. other brands



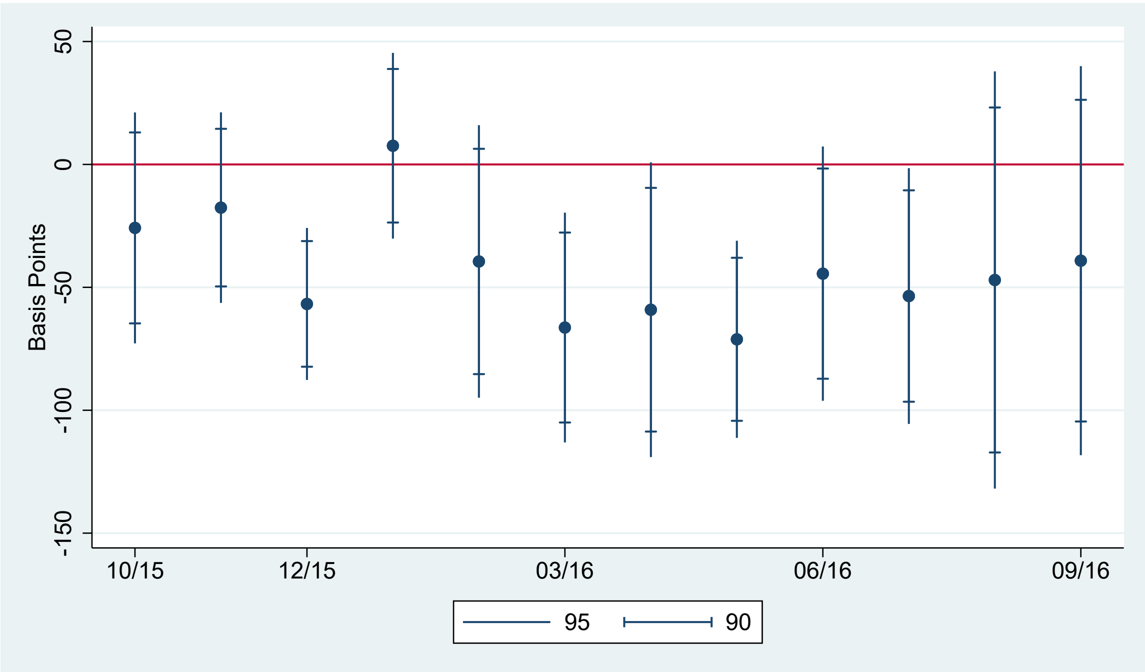

The figure separately shows the percentage of vehicles involved in transactions out of the total available stock of those vehicles (share-of-stock), for Dieselgate Volkswagens (the treatment group) and for other brands (the control group), during the baseline period (October 2014 to September 2015) and during the follow-up period (October 2015 to September 2016). As the figure shows, Dieselgate Volkswagens were traded more frequently during the baseline period compared to other brands. In contrast, in the follow-up period Dieselgate Volkswagens were traded less frequently than vehicles of other brands. The decline in the number of transactions in September 2015 for both the treatment and control groups is due to the High Holy Day season in Israel.

Figure 5: The dynamic effect of Dieselgate on transactions

Panel A: The dynamic effect of Dieselgate on the number of transactions

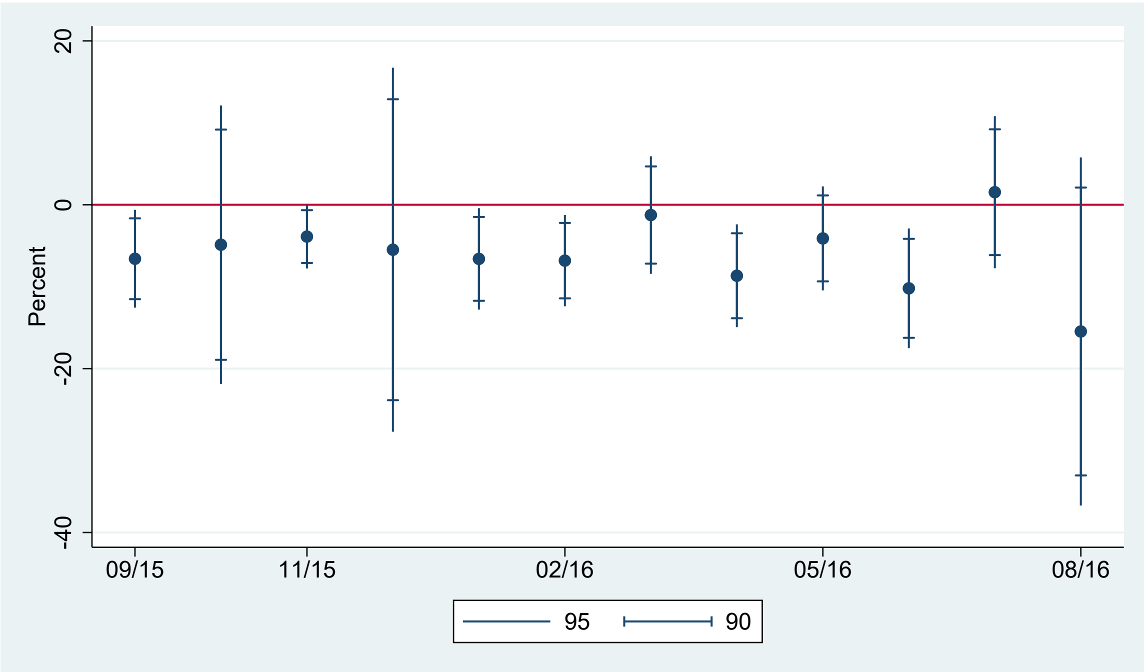



Panel B: The dynamic effect of Dieselgate on share-of-stock




The figure shows the coefficients of the "Dieselgate Effect" on the number of transactions (Panel A) and on the share of vehicles involved in transactions out of the total available stock of those vehicles (Panel B) in each month, and their 90% and 95% confidence intervals. We calculate the coefficients by estimating a version of equation 1 and adding an interaction term of the *Dieselgate Volkswagens* variable with the month fixed effect variable ($\delta_t$). The figure shows that the negative effect of Dieselgate on the transaction volume of VW-manipulated cars persisted throughout the follow-up period.

Figure 6: The dynamic effect of Dieselgate on price




The figure displays the coefficients of the "Dieselgate Effect" on the resale value of VW cars for each month and their 90% and 95% confidence intervals. We calculate the monthly effect by estimating a version of equation 2, and adding an interaction term of the *Dieselgate Volkswagens* variable and the month fixed effect variable ($\delta_t$). The figure shows that the negative effect of Dieselgate on the listing price persisted throughout the follow-up period. The drop in August 2015 arises because the calculation of the average asking price in each month is based on the final listing price, which often occurred afterwards (i.e. after Dieselgate was exposed.)

Table I: Descriptive statistics for the MOT and Yad2 data

**Panel A: MOT data**

| **Variable** | **Mean** | **Median** | **Min** | **Max** |
|---|---|---|---|---|
| Total transactions, per month | 2,538 | 2,506 | 1,662 | 3,199 |
| Total transactions, per manufacturer | 2,900 | 2,567 | 293 | 10,240 |
| Share of transactions per manufacturer | 4.8% | 4.2% | 0.5% | 16.8% |
| Share of private sellers per manufacturer | 53.2% | 55.9% | 23.4% | 91.9% |

**Panel B: Yad2 data**

| **Variable** | **Mean** | **Median** | **Min** | **Max** |
|---|---|---|---|---|
| List price (usd) | 28,208 | 23,077 | 1,026 | 98,718 |
| Car age (years) | 4.8 | 5.0 | 1.0 | 8.0 |
| Distance traveled (10,000 km) | 13.8 | 12.5 | 0.5 | 49.5 |
| # of previous owners | 1.6 | 1.0 | 1.0 | 9.0 |
| Engine displacement (liters) | 2.4 | 2.0 | 1.0 | 7.2 |

**Panel C: Comparison of car categories and brands between samples**

| **Car category** | MOT | Yad2 | **Brand** | MOT | Yad2 |
|---|---|---|---|---|---|
| Commercial vehicle | 40% | 38% | Renault | 17% | 10% |
| Compact | 25% | 17% | Citroen | 14% | 10% |
| SUV | 17% | 27% | KIA | 7% | 7% |
| Full-size | 5% | 5% | Toyota | 7% | 9% |
| Minivan | 4% | 5% | Skoda | 7% | 5% |
| Mid-size | 3% | 4% | Mercedes | 6% | 8% |
| Other categories | 6% | 5% | Mitsubishi | 5% | 5% |
| | | | Isuzu | 5% | 3% |
| | | | Peugeot | 5% | 4% |
| | | | Fiat | 5% | 6% |
| | | | VW | 4% | 5% |
| | | | Hyundai | 4% | 7% |
| | | | Ford | 3% | 3% |
| | | | Chevrolet | 2% | 7% |
| | | | BMW | 2% | 3% |
| | | | Nissan | 2% | 2% |
| | | | Sanyang | 1.3% | 1.8% |
| | | | Opel | 1.0% | 1.0% |
| | | | Rover | 0.8% | 0.9% |
| | | | Chrysler | 0.6% | 1.0% |
| | | | Volvo | 0.5% | 0.6% |

Notes:
The MOT data set is based on 60,900 observations, and the Yad2 data set is based on 11,648 observations.
The listing price was calculated using a fixed exchange rate of 3.9 NIS per 1 USD (which was approximately the exchange rate through the sample period).

Table II: Dieselgate's effect on monthly transaction volume

| | (1) $ln(Transactions)_{i,t}$ | (2) $Share\text{-}of\text{-}stock_{i,t}$ |
|---|---|---|
| (Dieselgate Volkswagens)×(POST) | $-0.18^{***}$ | $-0.43^{**}$ |
| | (0.06) | (0.20) |
| Month Fixed Effect | YES | YES |
| Brand Fixed Effect | YES | YES |
| $R^2$ | 0.96 | 0.54 |
| N | 504 | 504 |

Standard errors in parentheses are clustered at the brand level. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$
In both columns an observation is at the brand-month level. In column 1 we present the estimation results when using $ln(Transactions)_{i,t}$ as the dependent variable. In column 2 we present the estimation results using $Share\text{-}of\text{-}stock_{i,t}$ as the dependent variable. $Share\text{-}of\text{-}stock_{i,t}$ is given by $\frac{Transactions_{i,t}}{Stock_{i,t}} \times 100$, where $Transactions_{i,t}$ is the number of transactions in month $t$ of vehicles produced by brand $i$ and $Stock_{i,t}$ is the cumulative number of diesel vehicles that were sold in the primary market between the years 2009 and 2015 up to 12 months before month $t$. The baseline period is October 2014 to September 2015, and the follow-up period is October 2015 to September 2016.

Table III: Dieselgate's effect on Volkswagen‘s resale price

| | $ln(price)_{i,t}$ | |
|---|---|---|
| | (1) | (2) |
| (Dieselgate Volkswagens)×(POST) | $-0.09^{***}$ | $-0.06^{***}$ |
| | (0.02) | (0.02) |
| Car's Age (years) | | $-0.11^{***}$ |
| | | (0.02) |
| Distance Traveled (10,000 KM) | | $-0.02^{***}$ |
| | | (0.00) |
| # of Owners | | $-0.05^{***}$ |
| | | (0.01) |
| Engine Displacement (Liters) | | $0.16^{***}$ |
| | | (0.05) |
| Automatic Gear | | 0.09 |
| | | (0.08) |
| Category Fixed Effect | NO | YES |
| Month Fixed Effect | YES | YES |
| Brand Fixed Effect | YES | YES |
| $R^2$ | 0.52 | 0.79 |
| N | 11,648 | 11,648 |

Standard errors in parentheses are clustered at the brand level. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$
In both columns an observation is a listing. The dependent variable $ln(price)_{i,t}$ is the natural logarithm of the last asking price of those listings. The baseline period is September 2014 to August 2015, and the follow-up period is September 2015 to August 2016.

Table IV: Dieselgate's effect on monthly transaction volume by seller type

| | (1) $ln(Transactions)_{i,s,t}$ | (2) $Share\text{-} of\text{-}stock_{i,s,t}$ |
|---|---|---|
| (Dieselgate Volkswagens)×(Private)×(POST) | $-0.31^{***}$ | $-0.68^{**}$ |
| | (0.07) | (0.27) |
| (Dieselgate Volkswagens)×(POST) | −0.01 | 0.15 |
| | (0.05) | (0.26) |
| (Private)×(POST) | $0.12^{**}$ | 0.09 |
| | (0.05) | (0.25) |
| (Dieselgate Volkswagens)×(Private) | $1.65^{***}$ | 1.12 |
| | (0.41) | (1.00) |
| Private | −0.27 | $-2.27^{**}$ |
| | (0.26) | (0.96) |
| Month Fixed Effect | YES | YES |
| Brand Fixed Effect | YES | YES |
| $R^2$ | 0.68 | 0.58 |
| N | 816 | 816 |

Standard errors in parentheses are clustered at the brand level. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$

In column 1 we present the estimation results when using $ln(Transactions)_{i,s,t}$ as the dependent variable. $ln(Transactions)_{i,s,t}$ is the natural logarithm of the total number of transactions in month $t$ of vehicles produced by brand $i$ and sold by seller $s$. In column 2 we present the estimation results using $Share\text{-}of\text{-}stock_{i,s,t}$ as the dependent variable. $Share\text{-}of\text{-}stock_{i,s,t}$ is given by $\frac{Transactions_{i,s,t}}{Stock_{i,s,t}} \times 100$, where $Transactions_{i,s,t}$ is the number of transactions in month$t$ of vehicles produced by brand $i$ and sold by seller $s$, and $Stock_{i,s,t}$ is the cumulative number of diesel vehicles that were sold in the primary market between the years 2009 and 2015 to seller $s$, plus (minus) vehicles purchased (sold) from (to) different types of seller in the secondary market up to 12 months before month $t$. The baseline period is October 2014 to September 2015, and the follow-up period is October 2015 to September 2016.

Table V: Dieselgate's effect on Volkswagen's listing's price update

| | $\Delta(price)_{i,t}$ | |
|---|---|---|
| | (1) | (2) |
| (Dieselgate Volkswagens)×(POST) | $-1.02^{**}$ | $-0.99^{**}$ |
| | (0.36) | (0.40) |
| Car's Age (years) | | 0.10 |
| | | (0.07) |
| Distance Traveled (10,000 KM) | | $-0.07^{***}$ |
| | | (0.01) |
| # of Owners | | $-0.34^{***}$ |
| | | (0.11) |
| Engine Displacement | | 0.10 |
| | | (0.11) |
| Automatic Gear | | 0.23 |
| | | (0.26) |
| Category Fixed Effect | NO | YES |
| Month Fixed Effect | YES | YES |
| Brand Fixed Effect | YES | YES |
| $R^2$ | 0.03 | 0.04 |
| N | 8,269 | 8,269 |

Standard errors in parentheses are clustered at the brand level. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$
In both columns an observation is a listing. The dependent variable $\Delta price_{i,t}$ is the percentage change between the initial and the final listing price ($\frac{Final\ Price}{First\ Price} - 1$). The baseline period is September 2014 to August 2015, and the follow-up period is September 2015 to August 2016.

Table VI: Distribution of car categories between groups and data sets

| | **MOT** | | **Yad2** | |
|---|---|---|---|---|
| | VW | Non-VW | VW | Non-VW |
| *Passenger Cars* | | | | |
| Subcompact | 10% | 3% | 6% | 2% |
| Compact | 60% | 21% | 46% | 14% |
| Mid-size | 11% | 2% | 17% | 2% |
| Full-size | 0% | 5% | 0% | 6% |
| | 82% | 31% | 69% | 24% |
| *Non-Passenger Cars* | | | | |
| Commercial vehicle | 11% | 43% | 18% | 40% |
| SUV | 7% | 18% | 14% | 28% |
| Minivan | 0% | 5% | 0% | 5% |
| Other categories | 0% | 3% | 0% | 3% |
| | 18% | 69% | 31% | 76% |

Notes:
This table shows the percentage of observations in the MOT data set and the Yad2 data set, divided by car category and group (VW vehicles and non-VW vehicles). We use these data sets to estimate our main results in subsections IV(i) and IV(ii).
Other categories include large vans and sport vehicles.

Table VII: Spillover analysis of the dieselgate's effect on the number of monthly transactions using different treatment and control groups

| | $ln(Transactions)_{i,t}$ | | | | | | |
|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
| (Dieselgate Volkswagens)×(POST) | $-0.18^{***}$ | $-0.26^{***}$ | $-0.20^{***}$ | $-0.24^{***}$ | $-0.21^{**}$ | $-0.20^{**}$ | $-0.28^{**}$ |
| | (0.06) | (0.06) | (0.06) | (0.05) | (0.08) | (0.07) | (0.12) |
| Month Fixed Effect | YES | YES | YES | YES | YES | YES | YES |
| Brand Fixed Effect | YES | YES | YES | YES | YES | YES | YES |
| Car Categories in Treatment | ALL | Passenger Cars | Passenger Cars | Subcompact, Compact, Mid-size | Compact | Compact, Mid-size | Compact, Mid-size |
| Car Categories in Control | ALL | Non-Passenger Cars | Commercial, Minivan | Non-Passenger Cars, Full-size | Mid-size, Full-size, SUV | Commercial | Commercial, SUV, Full-size, Subcompact, Minivan |
| $R^2$ | 0.96 | 0.95 | 0.96 | 0.96 | 0.96 | 0.96 | 0.93 |
| N | 504 | 456 | 384 | 480 | 288 | 480 | 360 |

Standard errors in parentheses are clustered at the brand level. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$
Column (1) is identical to the first column in Table II. The baseline period is October 2014 to September 2015, and the follow-up period is October 2015 to September 2016.

Table VIII: Spillover analysis of the dieselgate's effect on the monthly share of transacted vehicles using different treatment and control groups

| | *Share-of-stock*$_{i,t}$ | | | | | | |
|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
| (Dieselgate Volkswagens)×(POST) | $-0.43^{***}$ | $-0.65^{***}$ | $-0.48^{***}$ | $-0.61^{***}$ | $-0.62^{***}$ | $-0.57^{***}$ | $-0.75^{***}$ |
| | (0.20) | (0.15) | (0.16) | (0.15) | (0.15) | (0.13) | (0.13) |
| Month Fixed Effect | YES | YES | YES | YES | YES | YES | YES |
| Brand Fixed Effect | YES | YES | YES | YES | YES | YES | YES |
| Car Categories in Treatment | ALL | Passenger Cars | Passenger Cars | Subcompact, Compact, Mid-size | Compact | Compact, Mid-size | Compact, Mid-size |
| Car Categories in Control | ALL | Non-Passenger Cars | Commercial, Minivan | Non-Passenger Cars, Full-size | Mid-size, Full-size, SUV | Commercial | Commercial, SUV, Full-size, Subcom-pact, Minivan |
| $R^2$ | 0.68 | 0.70 | 0.51 | 0.68 | 0.68 | 0.67 | 0.62 |
| N | 504 | 456 | 384 | 480 | 288 | 480 | 360 |

Standard errors in parentheses are clustered at the brand level. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$
Column (1) is identical to the second column in Table II. The baseline period is October 2014 to September 2015, and the follow-up period is October 2015 to September 2016.

Table IX: Spillover analysis of the Dieselgate's effect on Volkswagen‘s resale price using different treatment and control Groups

| | $ln(price))_{i,t}$ | | | | | | |
|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
| (Dieselgate Volkswagens)×(POST) | $-0.06^{***}$ | $-0.09^{***}$ | $-0.08^{***}$ | $-0.09^{***}$ | $-0.10^{***}$ | $-0.11^{***}$ | $-0.14^{***}$ |
| | (0.02) | (0.02) | (0.02) | (0.02) | (0.02) | (0.01) | (0.01) |
| Car's Age (years) | $-0.11^{***}$ | $-0.12^{***}$ | $-0.14^{***}$ | $-0.12^{***}$ | $-0.14^{***}$ | $-0.12^{***}$ | $-0.08^{***}$ |
| | (0.02) | (0.02) | (0.03) | (0.02) | (0.03) | (0.02) | (0.01) |
| Distance Traveled (10,000 KM) | $-0.02^{***}$ | $-0.01^{***}$ | $-0.01^{***}$ | $-0.01^{***}$ | $-0.01^{***}$ | $-0.01^{***}$ | $-0.02^{***}$ |
| | (0.00) | (0.00) | (0.00) | (0.00) | (0.00) | (0.00) | (0.00) |
| # of Owners | $-0.05^{***}$ | $-0.05^{***}$ | $-0.08^{***}$ | $-0.05^{***}$ | $-0.08^{***}$ | $-0.05^{***}$ | $-0.02^{***}$ |
| | (0.01) | (0.01) | (0.02) | (0.02) | (0.02) | (0.01) | (0.01) |
| Engine Displacement (Liters) | $0.16^{***}$ | $0.14^{***}$ | $0.18^{***}$ | $0.14^{***}$ | $0.21^{*}$ | $0.15^{***}$ | $0.36^{***}$ |
| | (0.05) | (0.04) | (0.07) | (0.05) | (0.10) | (0.05) | (0.10) |
| Automatic Gear | $0.09^{***}$ | $-0.02$ | $-0.05$ | $-0.03$ | $-0.07$ | $-0.02$ | $-0.06^{*}$ |
| | (0.08) | (0.10) | (0.11) | (0.10) | (0.11) | (0.10) | (0.03) |
| Month, Brand and Category | YES | YES | YES | YES | YES | YES | YES |
| Car Categories in Treatment | ALL | Passenger Cars | Passenger Cars | Subcompact, Compact, Mid-size | Compact | Compact, Mid-size | Compact, Mid-size |
| Car Categories in Control | ALL | Non-Passenger Cars | Commercial, Minivan | Non-Passenger Cars, Full-size | Mid-size, Full-size, SUV | Commercial | Commercial, SUV, Full-size, Subcompact, Minivan |
| $R^2$ | 0.79 | 0.81 | 0.70 | 0.79 | 0.70 | 0.80 | 0.81 |
| N | 11,648 | 8,719 | 5,492 | 9,351 | 4,885 | 9,222 | 4,329 |

Standard errors in parentheses are clustered at the brand level. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$
Column (1) is identical to the second column in Table III. The baseline period is September 2014 to August 2015, and the follow-up period is September 2015 to August 2016.

# A Online Appendix for The Impact of Environmental Fraud on the Used Car Market: Evidence from Dieselgate

Itai Ater and Nir S. Yoseph

Table A.1 includes the estimation results using the number of new Yad2 listings as the dependent variable.

Similarly to the analysis presented in section IV(v), in Tables A.2 - A.4 we repeat our main analyses while restricting the data sets by car category. However, in this case we intend to increase the substitutability between the treatment group and the control group. In particular, we omit from both groups transactions made in car categories where VW's market share is low, so that the share of the dominant car categories among VW vehicles will increase within the control group.

Tables A.5 to A.7 include the estimation results of the effect of Dieselgate using alternative samples of cars, based on the vintage of the cars. In particular, we include VW diesel cars that were manufactured before 2009 and therefore were not included in the recall. We find that the number of transactions of these cars also declined after Dieselgate but that the magnitude of the decline is somewhat smaller compared with that of VW-manipulated cars.

Table A.1: Dieselgate's effect on new listings of Dieselgate Volkswagens in Yad2 website

| | $ln(new\, lists)_{i,t}$ | |
|---|---|---|
| | (1) | (2) |
| (Dieselgate Volkswagens)×(POST) | −0.09 | −0.09 |
| | (0.06) | (0.06) |
| Dieselgate Volkswagens | −2.17 | |
| | (0.04) | |
| Month Fixed Effect | YES | YES |
| Brand Fixed Effect | NO | YES |
| $R^2$ | 0.996 | 0.749 |
| N | 48 | 264 |

In column 1 we report robust standard errors.
In column 2 standard errors are clustered at the brand level.
$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$
In column 1 we aggregate the number of new lists by (Dieselgate Volkswagens and other brands) and in column 2 we aggregate the number of new lists by month and manufacturer. $ln(new\, lists)_{i,t}$ is the natural logarithm of the new listings in *month t* of vehicles produced by either group or brand *i*. The baseline period is October 2014 to September 2015, and the follow-up period is October 2015 to September 2016.

Table A.2: Robustness of the Dieselgate's effect on the number of monthly transactions to the exclusion of car categories

| | $ln(Transactions)_{i,t}$ | | | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| (Dieselgate Volkswagens)×(POST) | $-0.18^{***}$ | $-0.31^{***}$ | $-0.20^{***}$ | $-0.34^{***}$ |
| | (0.06) | (0.10) | (0.06) | (0.10) |
| Month Fixed Effect | YES | YES | YES | YES |
| Brand Fixed Effect | YES | YES | YES | YES |
| Excluded Categories | NON | Commercial | Full-size, Minivan, Other categories | Commercial, Full-size, Minivan, Other categories |
| $R^2$ | 0.96 | 0.94 | 0.96 | 0.93 |
| N | 504 | 432 | 480 | 408 |

Standard errors in parentheses are clustered at the brand level. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$
Column (1) is identical to the first column in Table II. The baseline period is October 2014 to September 2015, and the follow-up period is October 2015 to September 2016.

Table A.3: Robustness of the Dieselgate's effect on the monthly share of transacted vehicles to the exclusion of car categories

| | *Share-of-stock*$_{i,t}$ | | | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| (Dieselgate Volkswagens)×(POST) | $-0.43^{**}$ | $-0.50^{***}$ | $-0.50^{**}$ | $-0.61^{***}$ |
| | (0.20) | (0.25) | (0.21) | (0.27) |
| Month Fixed Effect | YES | YES | YES | YES |
| Brand Fixed Effect | YES | YES | YES | YES |
| Excluded Categories | NON | Commercial | Full-size, Minivan, Other categories | Commercial, Full-size, Minivan, Other categories |
| $R^2$ | 0.68 | 0.49 | 0.56 | 0.90 |
| N | 504 | 432 | 480 | 408 |

Standard errors in parentheses are clustered at the brand level. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$
Column (1) is identical to the second column in Table II.The baseline period is October 2014 to September 2015, and the follow-up period is October 2015 to September 2016.

Table A.4: Robustness of The Dieselgate's effect on Volkswagen's resale price to the exclusion of car categories

| | $ln(price))_{i,t}$ | | | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| (Dieselgate Volkswagens)×(POST) | $-0.06^{***}$ | $-0.07^{***}$ | $-0.06^{***}$ | $-0.08^{***}$ |
| | (0.02) | (0.02) | (0.02) | (0.03) |
| Car's Age (years) | $-0.11^{***}$ | $-0.11^{***}$ | $-0.07^{***}$ | $-0.07^{***}$ |
| | (0.02) | (0.02) | (0.01) | (0.01) |
| Distance Traveled (10,000 KM) | $-0.02^{***}$ | $-0.02^{***}$ | $-0.02^{***}$ | $-0.03^{***}$ |
| | (0.00) | (0.00) | (0.00) | (0.00) |
| # of Owners | $-0.05^{***}$ | $-0.06^{***}$ | $-0.02^{*}$ | $-0.03^{***}$ |
| | (0.01) | (0.01) | (0.01) | (0.01) |
| Engine Displacement (Liters) | $0.16^{***}$ | $0.16^{***}$ | $0.17^{***}$ | $0.24^{***}$ |
| | (0.05) | (0.05) | (0.03) | (0.07) |
| Automatic Gear | 0.09 | 0.08 | $0.14^{***}$ | $0.14^{***}$ |
| | (0.08) | (0.09) | (0.03) | (0.04) |
| Category Fixed Effect | YES | YES | YES | YES |
| Month Fixed Effect | YES | YES | YES | YES |
| Brand Fixed Effect | YES | YES | YES | YES |
| Excluded Categories | NON | Commercial | Full-size, Minivan, Other categories | Commercial, Full-size, Minivan, Other categories |
| $R^2$ | 0.79 | 0.84 | 0.81 | 0.87 |
| N | 11,648 | 7,274 | 10,199 | 5,825 |

Standard errors in parentheses are clustered at the brand level. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$
Column (1) is identical to the second column in Table III. The baseline period is September 2014 to August 2015, and the follow-up period is September 2015 to August 2016.

Table A.5: Robustness of the Dieselgate's effect on the number of monthly transactions to vehicles' vintage

| | $ln(Transactions)_{i,t}$ | | | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| (Dieselgate Volkswagens)×(POST) | $-0.18^{***}$ | $-0.16^{***}$ | $-0.09^{*}$ | $-0.13^{*}$ |
| | (0.06) | (0.06) | (0.05) | (0.07) |
| Month Fixed Effect | YES | YES | YES | YES |
| Brand Fixed Effect | YES | YES | YES | YES |
| Production Year | 2009-2015 | 2000-2015 | 2000-2008 | 2007-2008 |
| $R^2$ | 0.96 | 0.99 | 0.98 | 0.96 |
| N | 504 | 504 | 456 | 408 |

Standard errors in parentheses are clustered at the brand level. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$
Column (1) is identical to the first column in Table II. The baseline period is October 2014 to September 2015, and the follow-up period is October 2015 to September 2016.

Table A.6: Robustness of the Dieselgate's effect on the monthly share of transacted vehicles to vehicles' vintage

| | *Share-of-stock*$_{i,t}$ | | |
|---|---|---|---|
| | (1) | (2) | (3) |
| (Dieselgate Volkswagens)×(POST) | $-0.43^{***}$ | $-0.34$ | $-0.35^{**}$ |
| | (0.20) | (0.22) | (0.15) |
| Month Fixed Effect | YES | YES | YES |
| Brand Fixed Effect | YES | YES | YES |
| Production Year | 2009-2015 | 2007-2015 | 2007-2008 |
| $R^2$ | 0.68 | 0.55 | 0.72 |
| N | 504 | 504 | 408 |

Standard errors in parentheses are clustered at the brand level. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$
Column (1) is identical to the second column in Table II. The baseline period is October 2014 to September 2015, and the follow-up period is October 2015 to September 2016.

Table A.7: Robustness of the Dieselgate's effect on Volkswagen's resale price to vehicles' vintage

| | $ln(price))_{i,t}$ | | | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| (Dieselgate Volkswagens)×(POST) | $-0.06^{***}$ | $-0.06^{***}$ | $-0.06^{***}$ | $-0.05$ |
| | (0.02) | (0.01) | (0.02) | (0.03) |
| Car's Age (years) | $-0.11^{***}$ | $-0.16^{***}$ | $-0.16^{***}$ | $-0.19^{***}$ |
| | (0.02) | (0.01) | (0.01) | (0.02) |
| Distance Traveled (10,000 KM) | $-0.02^{***}$ | $-0.01^{***}$ | $-0.01^{***}$ | $-0.01^{***}$ |
| | (0.00) | (0.00) | (0.00) | (0.00) |
| # of Owners | $-0.05^{***}$ | $-0.02$ | $-0.02^{***}$ | $-0.02^{***}$ |
| | (0.01) | (0.01) | (0.00) | (0.01) |
| Engine Displacement (Liters) | $0.16^{***}$ | $0.13^{***}$ | $0.14^{*}$ | $0.12^{***}$ |
| | (0.05) | (0.04) | (0.03) | (0.03) |
| Automatic Gear | 0.09 | $0.17^{***}$ | $0.12^{***}$ | $0.15^{***}$ |
| | (0.08) | (0.05) | (0.03) | (0.03) |
| Category Fixed Effect | YES | YES | YES | YES |
| Month Fixed Effect | YES | YES | YES | YES |
| Brand Fixed Effect | YES | YES | YES | YES |
| Production Year | 2009-2015 | 2000-2015 | 2000-2008 | 2007-2008 |
| $R^2$ | 0.79 | 0.86 | 0.84 | 0.84 |
| N | 11,648 | 38,320 | 25,652 | 8,268 |

Standard errors in parentheses are clustered at the brand level. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$
Column (1) is identical to the second column in Table III. The baseline period is September 2014 to August 2015, and the follow-up period is September 2015 to August 2016.