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The Impact of Car Pollution on Infant and Child Health: Evidence from Emissions Cheating *

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Abstract

Car exhaust is a major source of air pollution, but little is known about its impacts on population health due to socioeconomic selection, measurement error, and avoidance behaviors. We exploit the dispersion of emissions-cheating diesel cars—which secretly polluted up to 150 times as much as gasoline cars—across the United States from 2008-2015 as a unique opportunity to overcome these empirical challenges and measure the health impacts of car pollution. Using the universe of vehicle registrations, we demonstrate that a 10 percent cheating-induced increase in car exhaust increases rates of low birth weight and acute asthma attacks among children by 1.9 and 8.0 percent, respectively. These health impacts occur at all pollution levels and across the entire socioeconomic spectrum.

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1 Introduction

The impacts of car pollution and their optimal regulation are the subject of an ongoing and contentious academic and policy debate in the United States, Europe, and around the world. Yet, little empirical evidence exists on the impacts of car exhaust on health outcomes. Although it is well established that air pollution has negative impacts on population health (Chay and Greenstone, 2003a,b; Currie and Neidell, 2005; Chen et al., 2013; Deschenes et al., 2017; Deryugina et al., 2019), the existing quasi-experimental evidence is largely based on measures of overall air pollution without identifying the contribution of car pollution. Two pioneering papers have studied the health impacts of car pollution on disadvantaged infants—those born to mothers residing next to highway toll stations and those sick enough to die in response to weekly traffic variation (Currie and Walker, 2011; Knittel et al., 2016)—but these estimates might not be generalizable. Whether moderate levels of car pollution impact the health of the general population remains an open question.[1,2]

This gap in knowledge is perhaps surprising, as car exhaust is omnipresent on a daily basis across the entire population. Even for the wealthiest members of society, there is no escape from car exhaust. Moreover, as car pollution can be regulated in many ways and at relatively low costs (Fowlie et al., 2012), accurately measuring its health impacts is crucial to evaluate the costs and benefits of different regulations and their enforcement. Finally, because consumers are exposed at least partly to their own vehicle's pollution (Harik et al., 2017), informing society about the health consequences of car exhaust could profoundly impact consumer choice.

Empirical evidence on the health impacts of car pollution is scarce due to several well-known

[1]Notably, both the current and the previous U.S. administrations have based their car emissions regulations on the notion that the causal evidence linking traffic-related air pollution to health, in particular health at birth, is "insufficient" and "inadequate"(US EPA, 2012, 2018). These regulations draw on a meta study (HEI, 2010) that concludes the evidence of impacts on birth outcomes is "inadequate and insufficient" and the evidence of impacts on mortality is "suggestive but not sufficient." The evidence is deemed sufficient to conclude a causal relationship only in the case of the exacerbation of asthma. For recent quasi-experimental evidence of traffic-related pollution impacts on asthma, see Marcus (2017) and Simeonova et al. (2018).

[2]Anderson (forthcoming)—another related paper studying a relatively disadvantaged population—documents mortality impacts of car pollution for elderly Los Angeles residents living next to highways, exploiting the fact that downwind neighborhoods get more pollution on average. He et al. (forthcoming) focus on the high pollution environment of a mega city, studying the health impacts of pollution from diesel cargo trucks in São Paulo.

threats to causal inference, including socio-economic selection, avoidance behavior, and measurement error. In this paper, we exploit a unique natural experiment that overcomes these empirical challenges.

In 2008, a new generation of supposedly clean diesel passenger cars was introduced to the U.S. market.[3] These new diesel cars were marketed to environmentally conscious consumers, with advertising emphasizing the power and mileage typical for diesel engines in combination with unprecedented low emissions levels. Clean diesel cars won the Green Car of the Year Award in 2009 and 2010, and quickly gained market share. By 2015, over 600,000 cars with clean diesel technology were sold in the United States. In the fall of 2015, however, it was discovered that these cars covertly activated equipment during emissions tests that reduced emissions below official thresholds, and then reversed course after testing. In street use, a single "clean diesel" car could pollute as much nitrogen oxide (NO_X; a precursor to fine particulate matter and ground-level ozone) as 150 equivalent gasoline cars.[4] Hereafter, we refer to cars with "clean diesel" technology as cheating diesel cars.

The dispersion of these cheating diesel cars across the United States gives us a unique opportunity to measure the causal effect of car pollution on infant and child health. This natural experiment provides several unique features. First, it is typically difficult to infer causal effects from observed correlations of health and car pollution, as wealthier individuals tend to sort into less-polluted areas and drive newer, less-polluting cars. The fast roll-out of cheating diesel cars provides us with plausibly exogenous variation in car pollution exposure across the entire socio-economic spectrum of the United States. Second, it is well established that people avoid known pollution, which can mute estimated impacts of air pollution on health (Neidell, 2009). Moderate pollution increases stemming from cheating diesel cars, a source unknown to the population, are less likely to induce avoidance behaviors, allowing us to cleanly estimate the full impact of pollution. Third, air pollution comes from a multitude of sources, making it difficult to identify

[3]These cars were first introduced by Volkswagen (Volkswagen AG, Audi AG, and Volkswagen Group of America, collectively "VW") in 2008 with their TDI Clean Diesel series. Fiat Chrysler Automobiles (FCA) entered the "clean diesel" market in 2013 with their EcoDiesel series. For a complete list, see Table A.15.

[4]See Sections 2.2 and 2.3 for details on the emissions scandal and on the emissions levels of cheating diesel cars.

contributions from cars, and it is measured coarsely with pollution monitors stationed in a minority of U.S. counties. This implies low statistical power and potential attenuation bias for correlational studies of pollution (Lleras-Muney, 2010). We use the universe of car registrations to track how cheating diesel cars spread across the country and link these data to detailed information on each birth conceived between 2007 and 2015. This setting provides rich and spatially detailed variation in car pollution.

We find that counties with increasing shares of cheating diesel cars experienced large increases both in air pollution and in the share of infants born with poor birth outcomes. We show that for each additional cheating diesel car per 1,000 cars—approximately equivalent to a 10 percent cheating-induced increase in car exhaust—there is a deterioration of 2.0 percent in air quality indices for fine particulate matter ($\mathrm{PM}_{2.5}$) and a 1.9 percent increase in the rate of low birth weight. We find similar effects on larger particulates (PM_{10}; 2.2 percent) and ozone (1.3 percent), as well as reductions in average birth weight (-6.2 grams) and gestation length (-0.016 weeks). Effects are observed across the entire socio-economic spectrum, and are particularly pronounced among advantaged groups, such as non-Hispanic white mothers with a college degree. Effects on pollution and health outcomes are approximately linear and not affected by baseline pollution levels. Overall, we estimate that the 607,781 cheating diesel cars sold from 2008 to 2015 led to an additional 38,611 infants born with low birth weight.[5] Finally, we also find an 8.0 percent increase in asthma emergency department (ED) visits among young children for each additional cheating diesel car per 1,000 cars in a subsample of five states.

A potential concern is that our estimates may be confounded by changes in county or maternal characteristics that are correlated with increasing cheating diesel shares. We address this concern by analyzing the impact of gasoline versions of cheating diesel cars (hereafter “cheating” gas) that were marketed to and purchased by a similar population, but which did not pollute above emissions standards (that is, they did not cheat). We find that neither pollution nor birth outcomes are affected by a county’s share of “cheating” gas cars, even though mothers giving birth in counties

[5] This number is equivalent to 1.7 percent of the overall 2.22 million low birth weight singleton births in the United States over the same period.

with high "cheating" gas shares have similar socioeconomic characteristics as mothers in counties with high cheating diesel shares. We further show that maternal characteristics do not change systematically over time in counties with increasing cheating diesel shares. These results suggest that our estimates are not driven by compositional changes in the type of mothers giving birth, nor by unobserved characteristics correlated with preferences for new car types or particular brands.

To benchmark our pollution estimates, we use emission measures from tail-pipe tests of cheating diesel cars to calculate how much we would expect ambient air pollution to increase due to their introduction.[6] This calculation suggests an increase in $\mathrm{PM}_{2.5}$ of 0.2 to 6 percent for each additional cheating diesel car per 1,000 cars. Our estimate of a 2.0 percent increase in ambient $\mathrm{PM}_{2.5}$ lies squarely within this range, and implies that passenger cars contribute around 20 percent of the overall fine particulate matter in complier counties. Our estimated impacts on birth outcomes are large. The implied causal health effects of car pollution from an IV specification are four to eight times larger than the pollution-health relationship estimated in cross-sectional studies (e.g. Hyder et al. (2014)), largely due to measurement error that attenuates cross-sectional estimates. We further show that our estimated impacts are similar or stronger than quasi-experimental studies that have focused on rarer outcomes and more disadvantaged populations (Chay and Greenstone, 2003a,b; Currie and Walker, 2011; Knittel et al., 2016).

This paper makes several contributions to the existing literature. We provide the first causal evidence that moderate variation in car pollution impairs fetal development and child health across the entire population. This finding builds on a large body of correlational as well as quasi-experimental studies linking overall air pollution to population health.[7] Our estimates demonstrate that car pol-

[6]We build on existing studies which have used pollution estimates from cheating diesel car tail-pipe tests to predict county-level excess $\mathrm{NO_X}$ pollution (Barrett et al., 2015; Holland et al., 2016; Chossière et al., 2017). These papers then use air pollution integrated assessment models to predict how excess $\mathrm{NO_X}$ pollution transforms into $\mathrm{PM}_{2.5}$ and ozone, and then impacts mortality. Mortality impacts are predicted using estimates of the mortality-pollution relationship from correlational studies. Estimates range between 46 and 59 U.S.-wide excess deaths caused by VW's cheating diesel cars. Although a direct comparison with our results is difficult (as these studies only report estimates for $\mathrm{NO_X}$ but not for $\mathrm{PM}_{2.5}$ or ozone), we show that our estimates are in line with the car tail-pipe test parameters upon which these papers are based.

[7]See Pope et al. (2002); Peters et al. (2004); Ponce et al. (2005); Stieb et al. (2012); Volk et al. (2013); Vrijheid et al. (2016); Cohen et al. (2017); Heft-Neal et al. (2018), Ransom and Pope (1992); Pope et al. (1992); Schwartz (1994); Bell et al. (2004); Neidell (2004); Luechinger (2014); Schlenker and Walker (2016); Currie and Schwandt (2016); Halliday et al. (2018); Anderson (forthcoming), Dominici et al. (2014), Currie et al. (2014) among others, for

lution plays an important causal role in this relationship, and suggest that correlational studies severely underestimate the true health costs of car pollution. Our results suggest that the strong direct health impacts of car exhaust need to be accounted when creating regulations aimed at reducing car emissions.

Second, the existing literature often finds pollution effects that are concentrated on disadvantaged populations, and suggests several potential mechanisms for this observed effect heterogeneity: poorer populations might be more exposed to pollution, they might be more susceptible to health impairments due to lower baseline health, or they might have less access to health care to treat the symptoms of exposure (Currie et al., 2014; Bell et al., 2005). We find health impacts that are not limited to disadvantaged groups, which demonstrates that good baseline health and health care access do not fully buffer the impacts of car pollution, and emphasizes the role of exposure. Reductions in car pollution are likely to provide society-wide benefits.

Third, while much of the existing literature estimates pollution effects net of protective responses to observable changes in pollution, we interpret our estimates as the full, unmuted impact of car pollution exposure. In line with this interpretation, our results imply health elasticities that are at the upper end of the range of estimates provided by the quasi-experimental pollution literature (Chay and Greenstone, 2003a,b; Currie and Walker, 2011; Knittel et al., 2016), despite the focus of this literature on more disadvantaged complier populations. Our estimates are particularly relevant for settings when the harm of pollution exposure is unknown to the population either due to unawareness of the pollution (Moretti and Neidell, 2011) or because the pollution level is considered safe, as tends to be the case for moderate levels of car pollution.

Fourth, the existing quasi-experimental literature often relies on daily or weekly variation in pollution (e.g. Knittel et al. (2016)). This focus on short-term exposure can overstate long-term effects if it captures "harvesting," or understate them if impacts increase with length of exposure (a notable exception is Anderson (forthcoming)). Our setting provides medium-term pollution variation which allows us to compare exposure differences across entire pregnancy periods. Given our

additional references. An important related literature analyzes the health impacts of retrofitting school buses (Beatty and Shimshack, 2011; Austin et al., 2019) and the removal of cargo trucks in a high pollution city (He et al., 2018).

focus on health at birth, we essentially estimate the impacts of life-time exposure since conception. Moreover, our estimates likely imply costly long-term impacts of pollution, as poor health at birth has been linked to negative effects on health, human capital, and economic outcomes throughout the life-cycle.[8] At the same time, our estimates represent lower bounds for the overall effect on the population as there are likely to be additional negative impacts on health and productivity of pollution exposure at older ages.[9]

Our analysis provides important insights for policy makers and society at large. First, the approximately linear health effects of pollution in the observed range calls into question the notion that there is a "safe level" of car pollution. We show that moderate variation in car pollution harms health even in counties with pollution levels below the EPA's safety threshold. This insight is informative for the current policy debate in the U.S. over whether regulators should account for environmental harm from pollution that occurs at levels below the EPA threshold (Friedman, 2019). More broadly, our results speak to the efficacy of regulation aimed at reducing emissions from diesel and fossil fuel powered cars, e.g. the banning of high emissions cars. Such bans are becoming increasingly popular—already Brussels, Hamburg, Paris, and Madrid have implemented limited bans on diesel cars, and dozens of other cities are planning to implement bans on cars powered by diesel or all fossil fuels over the next decade. Our results suggest that the implied improvements in air pollution and population health may justify even relatively distortive policies.

Second, our study is the first to show that cheating diesel cars had measurable impacts on ambient air pollution and population health. This is important information for a prominent industry scandal that already has been one of the costliest in recent history. To date, Volkswagen has paid about $25 billion in fines to the U.S. government and in compensation to owners of cheating diesel cars. Our results demonstrate that the group of individuals harmed by the emissions cheating

[8]See Almond et al. (2005); Russell et al. (2007) for direct medical costs of low birth weight. See McCormick (1985); Aylward et al. (1989); Roth et al. (2004); Currie and Almond (2011); Case et al. (2005); Black et al. (2007); Oreopoulos et al. (2008); Almond et al. (2018); Elder et al. (2019), among others, for long-term effects. Isen et al. (2017) directly link adult productivity to pollution exposure during pregnancy.

[9]See Currie et al. (2009a); Zivin and Neidell (2012); Adhvaryu et al. (2016); Lavy et al. (2016); Chang et al. (2016); Meyer and Pagel (2017); Archsmith et al. (2018); Borgschulte et al. (2018); Bishop et al. (2018); Austin et al. (2019); Chang et al. (2019); He et al. (2019); Heissel et al. (2019); Kahn and Li (2019); Hollingsworth and Rudik (2019); Zheng et al. (2019), among others.

widely expands beyond Volkswagen's customer base.

Finally, we note that the diesel emissions cheating scandal was not a result of insufficient regulation but of insufficient enforcement. Our results emphasize that resources spent on the enforcement of car pollution regulation are well invested if they decrease the likelihood that car makers will cheat, and are in line with other recent work showing strategic responses to uneven enforcement of environmental regulations (Zou, 2018). This conclusion is particularly relevant in the face of recent budget cuts to regulatory bodies in the United States, and the current trend toward deregulation and industry self-regulation.

The remainder of the paper is organized as follows. Section 2 provides background information on diesel pollution and emissions cheating. Sections 3 and 4 describe our data sources and the empirical strategy. Results are presented in Section 5; Section 6 concludes.

2 Background

2.1 Diesel versus gasoline

The two predominant fuel technologies worldwide for passenger cars and light-duty trucks are diesel and gasoline. Both diesel and gasoline originate as crude oil, which refineries then process into different types of fuel. Diesel, however, is less refined, heavier and oilier than gasoline, and has a higher energy density due to its longer carbon chains. This higher energy density, combined with the compressed combustion technology of diesel engines, means that diesel engines are more powerful and run more efficiently, using less fuel than gasoline engines. This efficiency advantage suggests that diesel engines emit less carbon dioxide ($\mathrm{CO_2}$), an important greenhouse gas, though the extent to which $\mathrm{CO_2}$ improvements are actually realized remains controversial (Helmers et al., 2019).

The main drawback to diesel engines is that diesel fuel does not burn as cleanly as gasoline, emitting high levels of particulate matter ($\mathrm{PM_{2.5}}$ and $\mathrm{PM_{10}}$) and nitrogen oxides ($\mathrm{NO_X}$). $\mathrm{NO_X}$ is a precursor compound to both additional particulate matter and ground-level ozone, the main component of smog.[10] Both particulate matter and ozone are associated with decreases in lung function, asthma exacerbations, increases in hospital visits for respiratory causes, and mortality.[11]

Although diesel cars have enjoyed high popularity and strong political support in Europe since the 1990s (with a market share of above 50 percent among new cars (Cames and Helmers, 2013)), historically there has been very low demand for them in the United States, due both to consumer preferences and the lack of favorable regulation as found in Europe. Moreover, tightening U.S. air pollution standards made it increasingly difficult for manufacturers to produce competitively priced diesel cars meeting those standards.

2.2 The diesel emissions cheating scandal

In the mid-2000s, VW engineers began developing a new diesel engine (TDI Clean Diesel) designed to meet tightening U.S. emissions standards for passenger vehicles (Department of Justice, 2017). These new engines appeared to have all the benefits of diesel vehicles—strong performance and fuel efficiency—without the downside of high pollution. VW heavily advertised this new diesel line in national U.S. print, television advertisements (including the 2010 Super Bowl), and social media campaigns, promoting it to environmentally conscious consumers some of whom began buying diesel vehicles for the first time. TDI Clean Diesel models won the "Green Car of the Year" award both in 2009 and 2010, and VW quickly became the largest seller of light-duty diesel vehicles in the United States. Among advertised claims for the emissions system of the new clean diesel line were that it "reduces smog-causing nitrogen oxides by up to 90 percent when compared with past generations of diesel technologies sold in the United States," and it has "fewer NO_X emissions than comparable gasoline engines." Advertisements began with headings such as "Hybrids? They're so last year [...] now going green doesn't have to feel like you're going green" (Federal Trade Commission, 2016). Fiat Chrysler Automobiles (FCA) entered the market in 2013 with their EcoDiesel series. By the end of 2015, nearly 550,000 of VW's TDI Clean Diesel

[10] See Hodan and Barnard (2004); Lin and Cheng (2007).

[11] See, for example, Di et al. (2017); Gent et al. (2003); Jerrett et al. (2009); Mar and Koenig (2009); Medina-Ramon et al. (2006); Pope et al. (2002).

vehicles and 60,000 of FCA's EcoDiesel vehicles were registered across the United States.

In the fall of 2015, the Environmental Protection Agency (EPA) made public that VW's TDI Clean Diesel models were far from clean, emitting $\mathrm{NO_X}$ at as much as 4,000 percent above the legal limit, evidently as a trade-off for enabling more powerful and durable engine performance (Department of Justice, 2017). Despite their dramatic pollution levels, these vehicles had previously passed standard EPA drive cycle tests due to "defeat devices," i.e. illegal software designed to cheat emissions tests. Specifically, the software recognized when a car was undergoing an emissions test, and adjusted components (such as catalytic converters or valves used to recycle some of the exhaust gases) to reduce pollutant emission to legally compliant levels for the duration of the test. As these additional procedures lowered the engine's performance and were costly to maintain at a permanent level, they were switched off by the software during regular driving. The EPA's probe into the TDI Clean Diesel cars led it to conclude in January 2017 that cars equipped with FCA's EcoDiesel engine contained similar illegal software. A list of all models issued EPA violations is found in Table A.15.

2.3 Emissions from cheating diesel cars

To our knowledge, this analysis is the first to empirically measure the effect of the cheating diesel cars on ambient air pollution. However, on-road tail pipe emissions of the cheating diesel cars were measured by researchers at West Virginia University, who first noted on a dynamometer the large $\mathrm{NO_X}$ emissions discrepancies between on-road and standard EPA tests (CAFEE, 2014). This study found that depending on the test route, the cheating diesel cars emitted $\mathrm{NO_X}$ at 5 to 35 times the amount permitted by the US-EPA Tier2-Bin5 standard (0.07 g/mi). The full extent of the cheating was revealed during follow-up testing conducted by the California Air Resources Board in conjunction with the the EPA, which found that VW cheating diesel vehicles emitted 10 to 40 times the $\mathrm{NO_X}$ emissions permitted by the EPA (0.7-2.4 g/mi). In contrast, a typical new gasoline car in this period emitted $\mathrm{NO_X}$ at well below the EPA limit; the median among light-duty make-models was just 0.016 g/mi for model year 2010 vehicles.[12] Thus, a single cheating VW diesel car emits as much $\mathrm{NO_X}$ as 44 to 150 gasoline cars of equivalent model years, and an increase of one cheating car per thousand can be thought of as an equivalent increase in car pollution by approximately 4.3 to 14.9 percent.[13]

To assess the plausibility of our estimates, it is helpful to consider the contribution of car exhaust to overall air pollution. In the EPA's National Emissions Inventory Report (United States Environmental Protection Agency, 2014), 43 percent of $\mathrm{NO_X}$ emissions were attributed to on-road mobile emission sources. The total contribution of on-road mobile sources to $\mathrm{PM_{2.5}}$ and ozone, however, are more difficult to quantify.

$\mathrm{PM_{2.5}}$ is both emitted directly from cars and created by secondary formation from precursor emissions such as $\mathrm{NO_X}$ (Hodan and Barnard, 2004; Lin and Cheng, 2007). In the 2014 National Emissions Inventory Report, 5.6 percent of $\mathrm{PM_{2.5}}$ emissions were attributed to on-road mobile sources. However, this number does not take into account the secondary formation of $\mathrm{PM_{2.5}}$, which has been shown to contribute more to ambient $\mathrm{PM_{2.5}}$ than primary emissions (Zawacki et al., 2018). An alternative, reduced-form way to quantify the contribution of cars to ambient pollution is to look at the change in pollutants throughout a given day, measuring the increase in pollution during rush hour compared to midday levels. Such an analysis results in a 43 percent change in cars' contribution to $\mathrm{PM_{2.5}}$.[14] Using the most and least conservative estimates for the amount of $\mathrm{NO_X}$ emissions from the cheating cars and the contribution of cars to overall $\mathrm{PM_{2.5}}$ generates a range of expected increases from an additional cheating car per 1,000 cars from 0.2 to 6 percent.[15]

[12]Authors' calculations from the EPA's Certified Vehicle Test Results Report Data files for cars and light trucks (https://www.epa.gov/compliance-and-fuel-economy-data/annual-certification-data-vehicles-engines-and-equipment).

[13]We assume that each cheating diesel car replaces a gasoline car, hence it would increase car pollution by the equivalent of 43 to 149 gasoline cars. It is difficult to know, however, the $\mathrm{NO_X}$ emissions of a typical non-cheating diesel car, as the EPA data on $\mathrm{NO_X}$ emissions by make-model is only comprehensive starting in 2010, when nearly all of the diesel cars were cheating. However, the non-cheating diesel car tested by CAFEE (2014) was found in on-road testing to emit $\mathrm{NO_X}$ at around the threshold allowed by the EPA of 0.07 g/mi, which would imply that the cheating diesel cars emitted as much $\mathrm{NO_X}$ as roughly 10 to 40 non-cheating diesel cars.

[14]Authors' analysis using data from the subsample of $\mathrm{PM_{2.5}}$ monitoring stations submitting hourly readings from 2015-2017, weekdays only, calculating annual averages of hourly pollution readings and then calculating ((pollution reading in highest hour)-(pollution reading in lowest hour))/(mean pollution). Hourly readings were not commonly reported until 2015.

[15]Cheating VW vehicles emitted 10 to 40 times the $\mathrm{NO_X}$ emissions permitted by the EPA, or 0.7-2.4 g/mi. Cheating

Although the precise contribution of on-road sources to ambient $\mathrm{PM}_{2.5}$ is thus unknown, we would expect a 10 percent increase in car exhaust to translate to an increase in measured $\mathrm{PM}_{2.5}$ of around 2 percent if 20 percent of $\mathrm{PM}_{2.5}$ near monitoring stations was caused by on-road mobile sources.

The processes of ground-level ozone formation and accumulation are likewise complex reactions between $\mathrm{NO_X}$ and volatile organic compounds (VOC) in the lower atmosphere, in the presence of sunlight (EPA, 2018). $\mathrm{NO_X}$ emissions generally contribute more to ozone than VOC emissions, however, suggesting a large potential role for the cheating diesel cars (Zawacki et al., 2018). As with particulate matter, we do not attempt to model these pathways directly, but rather the link between $\mathrm{NO_X}$emissions from cars and ambient ozone levels.

3 Data

3.1 Vehicle registration data

Our main independent variables of interest come from vehicle registration data purchased from IHS Markit. These data contain county-level vehicle registration snapshots of the entire stock of passenger cars and light trucks (referred to collectively as "cars" throughout the paper) from 2007, 2011, and 2015.[16] Each snapshot contains the county-level car total, the number of cars of each diesel make-model-vehicle year (which allows us to identify cheating diesel cars—listed in Table A.15), and the number of analogous gas cars for each make-model-vehicle year with a diesel option

FCA diesel cars emitted 5 to 20 times the EPA $\mathrm{NO_X}$ limit, or 0.35-1.2 g/mi. Estimates of the fraction of $\mathrm{PM}_{2.5}$ attributable to cars vary from 5.6 percent using EPA estimates of primary emissions to 43 percent using variation between rush hour peaks and troughs. Using conservative estimates of $\mathrm{NO_X}$ emissions from cheating cars and the contribution of cars to overall $\mathrm{PM}_{2.5}$ (0.7 g/mi and 5.6 percent, respectively), an additional cheating car per thousand would increase $\mathrm{PM}_{2.5}$ by 0.2 percent (one cheating car is equivalent to 44 comparable gas cars, which is equivalent to increasing car pollution by 4.3 percent; 0.043 x 0.056). Using the high end of the range ($\mathrm{NO_X}$ emissions of 2.4 g/mi and 40 percent of $\mathrm{PM}_{2.5}$ from cars), an additional cheating car per thousand would increase $\mathrm{PM}_{2.5}$ by 6 percent (one cheating car is equivalent to 150 comparable gas cars, which is equivalent to increasing the fleet by 14.9 percent; (0.149 x 0.40)).

[16]Light trucks are defined as those with gross vehicle weight ratings from 1 to 3, which covers vehicles up to 14,000 pounds. Examples of vehicles in the highest weight class included in our data are the Ford F-350, the Ram 3500, and the Hummer H1.

(to identify "cheating" gas cars—gas versions of cheating models).[17]

We use the information on the vehicle model year and county of registration to interpolate diesel registrations and gas car analogues between snapshot years. For example, we assume that a 2009 model year vehicle (first sold in 2008) that was registered in a county in 2015 was also registered in that county from 2008 or 2009 to 2014.[18] The total number of cars registered in each county is imputed linearly between 2007, 2011, and 2015, as we only have the make-model-year subtotals for cars with a diesel option. We construct the county-level number of cheating diesel cars, non-cheating diesel cars, and "cheating" gas cars per 1,000 cars.

We consider the cheating diesel cars' share of the vehicle stock as our explanatory variable for two main reasons. First, we believe the most relevant counterfactual to purchasing a cheating car is purchasing a non-cheating car. Thus, we want our measure of exposure to be a measure of the composition of the passenger vehicle stock rather than a measure of the absolute number of cars. Second, we want a measure of an average individual's (or pollution monitor's) exposure to the pollution of a cheating car. Our measure captures the fact that an extra cheating car in a uniformly densely populated county such as Cook County, IL is less likely to drive by any particular individual in the county, compared to the same car in the similarly sized but less evenly populated Champaign County, IL. However, we also show the robustness of our main results to using the number of cheating diesel cars per county-level population and per square mile of county land area.

[17]The IHS Markit Vehicles in Operation data used in Figures 1-7, A.1-A.5, and Tables 1-7 and A.1-A.12 are based on a snapshots taken by IHS Markit (12/2007, 12/2011, 12/2015). These figures reflect a total vehicle population based on the location of the vehicle operators as reported in vehicle registrations, and not based on where the vehicle is primarily driven. These figures also include complete information on light vehicle registrations, including rentals, fleets, and retail.

[18]We use sales data to assign a fraction of cars to the year before the model year, to take into account the fact that 75 percent of new cars were purchased in the model year, and 25 percent were purchased in the previous year. For more details, see Section A.1.2.2. The model-year based extrapolation works if the number of passenger vehicles that are sold or moved out of county is small relative to the stock of registered vehicles. We can check the validity of this extrapolation by comparing the 2011 counts constructed by rolling back the 2015 data by county and model year to the actual counts of registered vehicles in 2011. The process works very well—the correlation between the predicted number of emissions-cheating diesels registered in 2011 and the actual number in 2011 is 0.992.

3.2 Pollution data

Our measures of air pollution are constructed from air quality data from the EPA. The Clean Air Act requires every state to establish a network of air monitoring stations that take daily readings of criteria pollutants, of which we consider fine particulate matter ($\mathrm{PM}_{2.5}$), particulate matter (PM_{10}), nitrogen dioxide (NO_2), carbon monoxide (CO), and ozone (O_3). For each pollutant, we construct three county-month level measures: the average concentration, an air quality index, and the number of poor air quality days. The air quality index is scaled from 0–500, and is based on methodology from the EPA (see Section A.1.2). Poor air quality days are defined as days when the air quality index exceeds 51.

There are three main caveats with these data. First, the number of air monitoring stations varies across pollutants. Coverage is best for $\mathrm{PM}_{2.5}$ and ozone, with monitors in 556 and 731 counties in 2015, respectively, whereas fewer monitoring stations measure NO_2 (monitors in 232 counties in 2015). Thus, we have qualified information about the direct effect of the cheating diesel cars on NO_2, a component of NO_x, which was the main pollutant excessively emitted by cheating diesel cars. However, as mentioned above, NO_2 is a primary precursor for $\mathrm{PM}_{2.5}$ and ozone (Hodan and Barnard, 2004; Lin and Cheng, 2007). Lin and Cheng (2007) find that the majority of NO_2 converts to particulate nitrate (a component of $\mathrm{PM}_{2.5}$) within a few days. Hence, increases in NO_x will directly translate into increases in $\mathrm{PM}_{2.5}$ and ozone, pollutants that are well monitored and that are the most relevant for health impacts.[19]

Second, monitors and monitoring stations are added and discontinued frequently over the sample time. Furthermore, Grainger et al. (2016) provide evidence that regulatory agencies in compliance with federal air quality standards strategically avoid pollution hotspots when choosing new monitor locations. Systematic placement of new monitors in areas with less pollution could distort the time trend of county-level pollution if we averaged across all available monitors in each period. As a conservative approach, we use only one monitoring station per county to hold fixed the phys-

[19] $\mathrm{PM}_{2.5}$ and ozone are considered the most relevant in terms of health impacts of air pollution (for reviews, see Hoek et al. (2013); World Health Organization (2013)), and are to a large extent caused by road transportation (Cames and Helmers, 2013; Chossière et al., 2017).

ical area where pollution is being measured over time. For each county and pollutant, we choose the monitoring station with the most readings over our period and construct a county-month level panel of pollution measures from 2007 to 2015. We show that results are very stable if we use alternative measures based on several monitors per county.[20]

A third caveat is that even the longest-running monitor locations are not randomly assigned within a county. Monitors are often located in or near cities (Muller et al., 2018), so that the sample of counties with monitors tends to be more urban than the overall U.S. population. Moreover, if regulators on average strategically place monitors away from pollution sources, and car pollution is very local, we could underestimate the effect of the cheating cars on pollution. However, the high pollution levels of cheating diesel cars were not known to regulators, and over 85 percent of the monitors we use predate the emissions cheating cars. Hence, it is unlikely that regulators strategically placed monitors in order to avoid the pollution generated by cheating diesel cars.[21]

For $\mathrm{PM}_{2.5}$, we can complement the monitor-based measure with an alternative which combines satellite observations of aerosol optical depth, simulations from a chemical transport model, and information from EPA monitors (van Donkelaar et al., 2019).[22] There are advantages and disadvantages to this alternative measure. On the positive side, the satellite-based measure offers estimates of monthly $\mathrm{PM}_{2.5}$ for all counties in the US, rather than just the subsample with a $\mathrm{PM}_{2.5}$ monitoring station. On the negative side, satellite-based estimates are not direct measures, and prediction errors appear both to be important and not well understood. In particular, Fowlie et al. (2019) suggest that satellite estimates are biased down at high $\mathrm{PM}_{2.5}$ concentrations.

[20] We develop a procedure which averages across multiple monitoring stations at the county-month level, while holding the geographical distribution of the monitoring stations fixed. In particular, we allow more than one monitor per county (for example up to two monitors per county), but only use observations where all monitors report pollution measures to avoid compositional changes from new monitors being added in different parts of the county.

[21] Relatedly, Zou (2018) shows that when pollution is measured intermittently (on six-day cycles), air quality is worse on unmonitored days—suggesting strategic behavior over time as well as over the initial location of the monitor. As the source of pollution is unknown in our context and the monitoring data aggregated to the monthly level, we do not believe this type of high-frequency, strategic behavior will impact our analysis.

[22] We are grateful to Wes Austin for providing us with cleaned county-month-level PM 2.5 satellite data.

3.3 Birth data

Our first set of health outcomes comes from the U.S. National Vital Statistics System's birth records from 2007 to 2015. These data contain detailed information on all births in the United States, including county and month of birth, demographic information about the mother, and health outcomes for the infant, and are collapsed to the county-conception month. Conception month is calculated by taking the gestational age in weeks and dividing by 4.345, rounding gestation months to the nearest integer, and subtracting months of gestation from the birth month.

Our preferred measures of infant health are county-month average birth weight and the county-month level fraction of babies born at low birth weight (less than 2500 grams). Birth weight is often used as a summary measure of infant health, and low birth weight in particular is associated with a range of poor outcomes, both health-related and economic, such as schooling and earnings (Black et al., 2007; Almond et al., 2018). We further analyze county-month average gestational age in weeks and the rate of premature birth (gestational age of less than 37 weeks). Birth weight and gestational age are both commonly used summary measures of infant health.

3.4 Emergency department discharge data

Our second set of health outcomes comes from State Emergency Department Databases (SEDD) from the Agency for Healthcare Research and Quality's Healthcare Cost and Utilization Project (HCUP), which contain the number of emergency department (ED) visits over time, by diagnosis, from 2007 to 2015. We focus on the number of visits with a primary diagnosis of asthma, which is known to be triggered by poor air quality and has been shown to be correlated with exposure to traffic-borne pollutants (Gauderman et al., 2005).[23] In particular, we construct the number of ED visits for asthma per 1,000 people at the county-quarter level. We also break down this measure by patient age, as young children are most likely to be affected.

Although the HCUP data are very detailed, there are two important caveats. First, the ED discharge data do not include records for patients who were admitted through the ED. This limitation

[23] We use ICD-9 code 493 and ICD-10 code J45.

means that the data are not well suited for analyzing more-severe health outcomes, such as strokes or heart attacks, which typically result in a hospital admission. Second, we are limited by financial considerations to a small subsample of states: Arizona, New Jersey, Kentucky, Rhode Island, and Florida, providing us with 228 counties.

3.5 Other data

Our analysis further includes annual data on county characteristics from the U.S. Census Bureau's Small Area Income and Poverty Estimates program (SAIPE) and the Census Bureau's Population Estimates program (county-year level population, median income, fraction in poverty, fraction of children in poverty, and fraction white), as well as the annual number of vehicle-miles driven by state according to the Federal Highway Administration.

3.6 Cheating diesel over space and time

Figures 1A and 1B show the distribution of non-cheating diesel cars in 2007 and 2015. Non-cheating diesel is strongly clustered in the western part of the United States, particularly in less-populated rural states (these are mainly light-duty trucks). While there has been a slight increase in the fraction of non-cheating diesel cars between 2007 and 2015, the spatial distribution is stable over time. The pattern looks very different for the distribution of cheating diesel cars in Figures 1C and 1D. The 2007 map is blank as the first cheating diesel cars were not sold until 2008. By 2015, over 600,000 cheating diesel cars were sold (Figure A.1 shows annual registration counts), clustered on the West Coast, the upper Midwest, and New England–areas that are typically thought of as relatively wealthy and environmentally conscious. The distribution of "cheating" gas models looks very similar to the cheating diesel map (see Figure A.2) indicating that diesel and gas model were marketed to similar areas (they were also similarly priced; for details see Table A.14).

Figure 2 shows how the distribution of different car types is related to counties' median income in the 2015 cross-section. The left figure shows average median income across percentiles of counties, ranked by their share of non-cheating diesel cars. Counties with high shares of non-cheating diesel have somewhat lower median income than counties with fewer diesel cars (in line with income gradients for overall air pollution (Muller et al., 2018)). The blue dots in the right panel show that this relationship is reversed for cheating diesel cars. Counties with higher shares of cheating diesel cars in 2015 have higher median incomes than those with lower shares. The relationship is close to linear across the entire distribution, and it is strong: the median income in the top percentile is almost twice as high as in the bottom percentile. The hollow green circles in the right panel of Figure 2 show median income when counties are ranked instead by their share of "cheating" gas models. T'he relationship is very similar to the cheating diesel gradient, suggesting that areas in which people purchased many cheating diesel cars are similar to areas with many "cheating" gas models. The same holds true for a broad set of maternal characteristics (Figures 7 and A.7).

3.7 Summary statistics

Table 1 shows summary statistics at the county-year-month level for the overall sample of counties, for the sample of counties with a $\mathrm{PM}_{2.5}$ monitor, and for terciles ordered by the share of cheating diesel in 2015.[24] The upper panel shows counties' car registration characteristics, demographics, and pollution outcomes. The lower panel shows birth outcomes and maternal characteristics, restricted to county-year-month observations with non-zero births. We include observations with zero births in the main pollution regressions in order to maximize the power of the analysis, though results are unchanged if only county-year-months with non-zero births are used.

Comparing the first two columns, $\mathrm{PM}_{2.5}$ monitors are placed in counties that are more populated and that have slightly lower poverty rates and higher median incomes than the full sample of counties. Birth outcomes and maternal characteristics are relatively similar between the full sample and the $\mathrm{PM}_{2.5}$ monitor sample. In line with Figure 2, median income increases across terciles

[24]Table A.1 shows summary statistics for the subsample of the five states for which we have HCUP data on emergency department discharges. As in the overall sample, counties with higher shares of cheating diesel cars tend to have higher median incomes and lower poverty. Asthma rates are hump-shaped, with the highest occurrence in the second tercile and slightly lower rates in the bottom tercile than in the top tercile.

of the 2015 cheating diesel share. A higher cheating diesel share is also associated with a larger population, a higher number of cars and miles driven, and lower poverty rates. Despite having more cars, $\mathrm{PM}_{2.5}$ and PM_{10} levels are lower on average in counties with a higher cheating diesel share. Mothers giving birth in counties with higher cheating diesel shares are more likely to be white, married, more educated, and less likely to smoke during pregnancy. Not surprisingly, given the positive selection of mothers, these counties have better average birth outcomes: higher birth weight and longer gestation length.

4 Empirical strategy

We seek to estimate the effect of car pollution on health. Given well-measured experimental variation in car pollution at the county-time level (P_{ct}), we would run the following regression

$$Health_{ct} = \alpha + \beta ln\left(P_{ct}\right) + \varepsilon_{ct} \tag{1}$$

with β measuring the effect of a percent increase in car pollution on health. Running this regression in available observational data likely results in a biased estimate due to endogeneity and measurement error, and is nearly impossible to run due to lack of data on car pollution. As discussed previously, we argue that the number of cheating diesel cars provides a well-measured (conditionally) exogenous source of car pollution. Our preferred measure of exposure to cheating diesel cars is the number of such cars per 1,000 cars in a county (cD_{ct}).

How do changes in cD_{ct} relate to changes in total car pollution within a county? First, the pollution stemming from one gasoline car (p_i) can be decomposed into miles driven times the pollution per mile

$$p_{it} = m_{it} * \left(poll/m\right)_i \tag{2}$$

Then, assuming that cars are either gasoline or cheating diesel cars, that all cars in a county drive the same average miles $\overline{m}_c$, and that cheating diesel cars pollute as much as 100 gasoline cars per mile (see Section 2.3), we can express a county's total car pollution (P_{ct}) as:

$$
\begin{aligned}
P_{ct} &= \underbrace{\left(1 - \frac{cD_{ct}}{1000}\right) * \overline{p}_c * C_{ct}}_{pollution\ from\ gas\ cars} + \underbrace{100 * \left(\frac{cD_{ct}}{1000}\right) * \overline{p}_c * C_{ct}}_{pollution\ from\ cheating\ cars} \\
&= \underbrace{\left(1 + \frac{99cD_{ct}}{1000}\right) * \overline{p}_c * C_{ct}}_{total\ pollution\ from\ cars}
\end{aligned}
\tag{3}
$$

with $\overline{p}_c$ referring to the pollution stemming from a gasoline car driving $\overline{m}_c$ miles and C_{ct} referring to the total number of cars in a county.[25] Thus, an increase of one cheating diesel car per 1,000 cars increases the total car pollution by about 10 percent for small baseline shares.[26]

$$
\frac{\delta ln\left(P_{ct}\right)}{\delta cD_{ct}} = \frac{1}{9.9 + cD_{ct}} \approx 0.1 \tag{4}
$$

We also show results using alternative exposure measures, such as the number of cheating diesel cars per 1,000 people or per square mile. However, these alternative measures do not relate as directly to equation (1).

The strong spatial clustering of cheating cars described in the previous section implies that simply comparing areas with higher and lower shares would not be informative. Our empirical strategy therefore focuses on within-area comparisons over time. The fast roll-out of cheating diesel cars into higher-income areas in combination with the deception of consumers regarding their actual

[25]While we do not have data on miles driven by make of car, there is little relationship between average miles driven per capita at the state level and share of cheating diesel cars, supporting the assumption that cheating diesel cars are driven as similar amount as the average car.

[26]Note that all counties start with a share of zero cheating cars and the median county has a share of only 1.6 per 1,000 cars in 2015.

pollution levels provides us with identifying variation for a complier population of particular interest. We will run several versions of the following regression:

$$Outcome_{ct} = \alpha + \beta_1 cD_{ct} + \beta_2 cG_{ct} + \lambda_c + \lambda_t + \delta X_{ct} + \varepsilon_{ct} \tag{5}$$

where c indicates the county and t the time period (either monthly or annually). The dependent variable $Outcome$ refers to pollution, birth, or health outcomes (for birth outcomes t refers to the conception month or year). The main regressor of interest is cD, referring to the number of cheating diesel cars per 1,000 registered cars in a county. cG is the share of "cheating" gas cars per 1,000 cars. λ_c and λ_t are county and time fixed effects and X_{ct} are time-varying county characteristics.[27] The data are collapsed at the county-month level (we also show results using the micro-level data and data collapsed to the county-year), and standard errors are clustered at the county level. Observations in birth outcome regressions are weighted by the number of births. When we focus on subgroups of mothers with various demographic characteristics, we use the individual level micro data.

Finally, we present results from instrumental variable (IV) specifications in which we use the cheating diesel share as instrument for $\mathrm{PM}_{2.5}$ and ozone. In regressions that include both $\mathrm{PM}_{2.5}$ and ozone, we interact the cheating diesel share with county-specific weather conditions (which differently mediate the transformation of car exhaust into $\mathrm{PM}_{2.5}$ and ozone) to obtain additional instruments (Knittel et al., 2016).

4.1 Identification

The inclusion of county and time fixed effects means that we compare changes in areas with increasing cheating diesel shares (treatment counties) to overall time trends in the data. This is

[27] Included characteristics are share of non-cheating diesel cars per 1,000 cars, log total cars, log population, poverty rate, child poverty rate, and median income. Birth outcome regressions include additional controls for the following maternal characteristics: fraction of mothers that are hispanic, black, married, smoking during pregnancy, average age, and education bins.

essentially a difference-in-difference approach, with the identifying assumption that any trend deviations in the outcomes of treatment counties are driven by the changes in the cheating diesel share. There is a common set of threats to this framework.

Increases in cheating diesel shares might be correlated with or driven by simultaneous socio-economic changes that increase pollution and worsen health outcomes. A direct way to test for such violation of the exclusion restriction is balancing regressions that use socio-economic indicators as dependent variables on the left of the regression equation (Pei et al., 2018). We will show balancing results both in binned scatter plots and in regression form.

Our estimates could also be confounded by selection due to unobserved characteristics, such as tastes for new cars. We report the effects of counties' "cheating" gas shares to explore the role of such factors. As discussed above, the type of counties with high cheating diesel shares are very similar in terms of socio-economic characteristics to counties with high "cheating" gas shares. If our results were driven by selection, we would expect to find similar effects of cheating diesel and "cheating" gas cars on health outcomes.

While we think of the share of "cheating" gas cars as akin to a placebo conceptually, we are only able to separately identify the effects of the two different types of cars because they have slightly different patterns of dispersion. As we show in Figure A.3, both cheating diesel cars and the equivalent gas models achieve higher market shares in counties with stronger preexisting demand for these Volkswagen models. However, counties with both strong preexisting Volkswagen demand and higher initial diesel shares accumulate somewhat more cheating diesel models relative to "cheating" gas cars.[28] Intuitively, the accumulation of both cheating diesel and "cheating" gas cars are driven by brand preferences, while the difference between the two types is driven by preferences for and availability of diesel fuel. Importantly, we will show that only cheating diesel cars are associated with higher pollution and worse health outcomes, and this relationship is virtually identical regardless of whether we also control for "cheating" gas cars.

[28] Using data on diesel fuel availability in North Carolina, we can further show that both initial diesel shares and ratios of cheating diesel to "cheating" gas models in a county are correlated with the fraction of gas stations with a diesel pump (see Figure A.4).

Another potential concern are differential trends in treatment and control counties occurring already before the treatment. Similarly, a spurious relationship in our setting could be caused by strong outliers in single time periods. It would not be plausible if effects were driven by an individual period despite the gradual dispersion of cheating diesel cars. We explore pretrends and the role of individual years using an event study approach.

Finally, effects could be driven by changes in control areas, for example, if poorer counties with few cheating diesel cars ("control counties") experienced improvements in pollution and health for reasons unrelated to diesel car penetration. Figure A.1 shows no evidence of a trend change in pollution for such control counties. We also present robustness checks in which we exclude counties with few cheating diesel cars.

5 Results

5.1 Effect of diesel emissions on pollution

We find large and statistically significant effects of the cheating diesel cars on air pollution. This relationship is demonstrated semi-parametrically in Figure 3A, which shows binned scatter plots of fine particulate matter air quality plotted against the fraction of cheating diesel, with county fixed effects, time fixed effects, and vehicle composition variables partialled out. While there is a strong relationship between the fraction of emissions cheating cars and $\mathrm{PM}_{2.5}$, there is no such relationship for "cheating" gas cars (3B shows analogous plots with the "cheating" gas share on the y-axis).

The first panel of Table 2 reports effects on the air quality index (AQI) for the five analyzed pollutants. We find strongly significant effects of cheating diesel cars on $\mathrm{PM}_{2.5}$, PM_{10}, and ozone. An additional cheating car per 1,000 increases the AQI for those three pollutants by 1.99 percent, 2.20 percent, and 1.33 percent, respectively. Effects on CO and NO_2 are not significant at the 5 percent level. The second row shows the coefficients on the share of "cheating" gas cars. Point estimates are small and imprecisely estimated but negative across all pollutants, which is what one would expect given that these are newer models, and, absent the emissions cheating scandal, newer models tend to be cleaner. The last line of each panel reports the p-value of a test of equality between the cheating gas and cheating diesel coefficients; in nearly every case where the effect of the cheating diesel cars on pollution is significant, we can also reject that the coefficients on the diesel and gas versions are the same.[29]

The air quality index is particularly useful for comparing the magnitudes of the effects across the pollutants, which have very different mean concentrations. Figure 4 plots the point estimates in Table 2A, which helps summarize the results visually: there is a large effect of the fraction of cheating diesel cars on air pollution, in particular on $\mathrm{PM}_{2.5}$, PM_{10}, and ozone. Conversely, when a larger share of the vehicle stock is made up of "cheating" gas cars, there is if anything slightly less pollution.[30]

Panels B and C of Table 2 show results for mean pollutant concentration and for the number of days with an AQI above 51. For the mean concentration, we are able to use both a monitor and satellite-based measure of $\mathrm{PM}_{2.5}$. Columns 1 and 2 of Panel B show that our results are nearly identical across the two measures of fine particulate matter, despite the large change in the sample of counties. For the coarsest measure of air quality (number of poor air quality days), we also find that the cheating diesel cars are associated with more days with elevated levels of NO_2. The effects on $\mathrm{PM}_{2.5}$ and ozone remain highly significant with similar magnitudes in percentage terms across all three measures, while the effects on the other pollutants are less robust—perhaps due to a smaller sample of counties with monitoring stations.

5.1.1 Spatial lag model

One potential weakness of our empirical strategy is that it could underestimate the effects of cheating diesel cars on pollution where substantial spillovers of pollution exist across counties. Such spillovers could occur either because many cars drive across county borders, or because the air

[29] For brevity we do not show separate regressions including and excluding the control for "cheating" gas cars; however, excluding this variable from the regression has no effect on the point estimate for cheating diesel cars.

[30] Non-cheating diesel cars are also associated with slightly worse pollution outcomes (see Figure A.5).

pollution quickly spreads across space. We investigate the importance of spillovers empirically in Table A.3, which examines the effects of cheating cars in neighboring counties on pollution levels.

For counties with a $\mathrm{PM}_{2.5}$ monitor, we run a horse race between the effect of the fraction of cheating cars in the county itself and the fraction of cheating cars in counties within varying distance bands of the monitor county (1–20 miles, 20–40 miles, and 40–60 miles). This strategy strongly decreases the number of observations, as counties are fairly large, and we can only conduct this analysis on counties with a $\mathrm{PM}_{2.5}$ monitor that have neighboring counties within the different distance bands.

Columns 1 and 4 of Table A.3 replicate our main results from Table 2 for $\mathrm{PM}_{2.5}$ concentration and air quality index, and columns 2 and 5 show that our results are nearly identical within the subsample of counties with neighboring counties in the three distance bands. The stability and precision of our baseline effects in the smaller sample suggest that the model is informative, despite the fairly severe data restriction. Columns 3 and 6 show that the effect of cheating diesel cars on pollution loads entirely onto the market penetration within the county, and there is no statistically significant effect of cheating cars in neighboring counties. These results suggest that effects of cheating diesel cars on pollution are largely concentrated on the county of the owners' residence, bolstering the validity of our identification strategy.

Together, Tables 2 and A.3 show strong evidence that cheating diesel cars increased air pollution. In the next section we consider whether these increases in local air pollution affect fetal development.

5.2 Effect of diesel emissions on health at birth

We find that the market share of cheating diesel cars is associated with worse birth outcomes. This relationship is again first demonstrated semi-parametrically in Figure 5A, which shows binned scatter plots of birth outcomes plotted against the fraction of cheating diesel, with county fixed effects, time fixed effects, and vehicle composition variables partialled out. As with particulate matter in Figure 3, for both average birth weight and average fraction born at low birth weight, there is a clear and striking linear relationship between the fraction of emissions cheating vehicles and worse birth outcomes. Again, this relationship only exists with the fraction of cheating diesel cars—the line for "cheating" gas in Panel B of Figure 5 is essentially flat, suggesting that there is no correlation with birth weight despite very similar patterns of selection.

The effects of cheating cars on birth outcomes are presented in regression form in Table 3, with the addition of time-varying county-level controls. The first two columns of Table 3, Panel A show that cheating diesel cars have a strongly negative impact on average birth weight. There is no such association, however, for the share of "cheating" gas cars. When we include both shares in the same regression in column 2, the coefficient on "cheating" gas cars is small and insignificant, while the coefficient on cheating diesel cars is essentially unchanged (again, we can formally reject equality between the coefficients on cheating diesel and "cheating" gas cars in all cases). The coefficient in the second column indicates that each additional cheating diesel car per 1,000 cars decreases birth weight by about 6.2 grams, or 0.19 percent. This effect is highly significant with a t-value of 8.3.

The next two columns of Table 3, Panel A show cheating diesel cars also strongly affect low birth weight rates, with an impact of 0.12 percentage points (1.9 percent) for every additional car and a t-value of 5.5. The fraction of cheating diesel vehicles is also associated with lower gestational age at birth and higher rates of preterm birth, though with a considerably smaller magnitude when compared to birth weight (see Table 3, Panel B). An additional cheating diesel car per thousand decreases gestational age by 0.016 weeks. However, these effects are much smaller in magnitude (a 0.04 percent decrease for every additional cheating car) compared to the impacts on birth weight. The effects of cheating cars on prematurity rates are larger (0.07 percentage points, or 0.7 percent), though less precisely estimated, with a t-value of 1.7. Figure 6 plots the point estimates from Table 3, again emphasizing the large effect of the fraction of cheating diesel cars on birth outcomes, relative to the economically and statistically insignificant effects of the share of the "cheating" gas cars. Figure A.6 shows the corresponding binned scatter plot for gestational age: as suggested by the regression results in Table 3, Panel B, the relationship between gestational age and the cheating cars, though present, is smaller and less precise.[31]

5.2.1 Balancing regressions

Our analysis has shown that increases in the share of cheating diesel vehicles are associated with lower birth weight. This relationship is not present for the fraction of "cheating" gas vehicles, suggesting that effects are not simply driven by differential trends in affluent areas with preferences for newer cars or particular brands. However, areas with increasing cheating diesel shares might undergo socioeconomic changes in other dimensions that are not captured by the comparison with the "cheating" gas areas. Figure 2 shows that the selection into these two car types is similar, but there is not a one-to-one relationship (otherwise we also could not estimate the effects independently) and the selection into cheating diesel that is independent from the selection into "cheating" gas potentially could be sufficient to create an association with birth outcomes.

Our regressions control for a broad set of time-varying county characteristics, and the finding that their inclusion has little effect on our estimates suggests that omitted variable bias does not play a big role (Tables A.10 and A.11). As Pei et al. (2018) show, however, a more powerful test of the orthogonality condition is provided by balancing regressions with county characteristics as dependent variables. Table A.2 shows corresponding balancing regressions using the baseline specification described in equation 1. As one would expect from the patterns in Figure 7, the coefficients on the cheating diesel share are small and insignificant in most cases. Only for college education is the effect significant at above the 5 percent level (though very small compared to the unconditional relationship). Importantly, however, the effect is positive, a selection that would work against our finding negative impacts on birth outcomes.

In a similar vein, Figure 7 shows binned scatter plots of maternal characteristics on the cheating diesel share, both for the cross-sectional unconditional relationship in 2015 (on the left) as well as from 2007 to 2015 after partialling out county and time fixed effects (on the right). As reflected

[31] Note that the "cheating" gas share has a positive (though quantitatively small) impact on gestation length, which is in line with the potential reversed effects on pollution discussed in the previous section. However, unlike the relationship between cheating diesel cars and birth outcomes, this positive association is not robust across specifications.

in the summary statistics, the unconditional relationship is strongly positive for all indicators of higher socioeconomic status. However, when county and time effects are partialled out, the relationship becomes flat in all cases. These patterns are similar when ordering counties by the share of "cheating" gas, as shown in Figure A.7.

5.2.2 Event study figures

Figure A.8 shows the coefficients of interaction terms of year dummies with counties' 2015 share of cheating diesel and "cheating" gas from regressions that include the baseline set of controls with the sample period running from 2004 to 2015 (2006 is the reference period). As this figure shows, the impacts on birth weight and the low birth weight rate systematically increase along with the gradual roll-out of cheating diesel cars (Figure A.1), and this pattern does not follow a pretrend. For "cheating" gas, on the other hand, there is no systematical pattern and most confidence intervals include zero. The pattern for $\mathrm{PM}_{2.5}$ and ozone shown in the two bottom figures is more noisy, in line with the poor measurement of local pollution. However, the overall pattern is similar, with positive effects systematically appearing in the later years and no evidence of pretrends.

5.3 Effects for socio-economic subgroups

Panel A of Table 4 shows effects of cheating diesel on birth weight for socioeconomic subgroups, using disaggregated individual-level data. Column 1 shows effects for 16.5 million births to non-Hispanic white mothers, while the second column focuses on 4.6 million births to black mothers. Existing literature indicates that more disadvantaged minority mothers typically are more affected by adverse pregnancy conditions, with larger negative effects on birth outcomes (Currie et al., 2014). This is not the case here: effects of cheating diesel cars on birth weight are more than 50 percent larger for non-Hispanic white mothers than for black mothers. One reason for the effect difference might be spatial segregation within counties, with higher effective shares of cheating diesel cars driving in areas with more non-Hispanic white mothers.

Columns 3 and 4 of Table 4 restrict the sample by education, comparing mothers with a college degree to mothers without any college education. Point estimates are slightly larger for mothers with more education, but the difference is quantitatively small and not statistically significant. Comparing this effect difference to the estimates across the racial subgroups suggests that racial segregation is stronger than spatial sorting based on education.

The last two columns of Table 4 combine racial and educational characteristics to focus on particularly advantaged and disadvantaged mothers. Restricting the college-educated subgroup to non-Hispanic white mothers somewhat decreases the sample size (8.87 million to 6.3 million births), but the estimated effect barely changes. However, the group of black mothers without a college degree is reduced to 2.45 million births and the average birth weight is substantially lower than in the other subgroups. The effect of the cheating diesel share for this particularly disadvantaged group is less than half the size of the baseline effect, and it is not significantly different from zero.

Panel B of Table 4 shows corresponding results for the low birth weight rate. For this outcome, effects are more similar across more and less disadvantaged subgroups. However, low birth weight baseline rates in those groups are very different, with much larger rates of low birth weight babies among disadvantaged groups. This implies that the same estimated effect in terms of percentage points reflects a much smaller relative effect for more disadvantaged subgroups. In other words, an additional cheating diesel car per 1,000 increases low birth weight cases by a similar number among advantaged and disadvantaged mothers, even though there are many more births close to the low birth weight cutoff at the risk of being pushed below the threshold among disadvantaged mothers.

5.4 Interaction with pollution baseline levels

The subgroup analysis suggests that more advantaged mothers are particularly affected by cheating diesel cars. These results already indirectly suggest that the effects of the cheating cars are not limited to areas with very high baseline pollution, as low income mothers disproportionately reside in these areas. However, we can directly investigate the role of baseline pollution and nonlinear effects in our data. For our main birth outcomes, in Table 5 we interact the cheating diesel share with two measures of baseline $\mathrm{PM}_{2.5}$ pollution: the normalized average level in 2007 and 2008 (mean zero and standard deviation one), and whether a county is designated by the EPA as out of compliance with air quality standards.

As foreshadowed by the linearity of the cheating diesel effect documented in Figure 3, we find that the effects of cheating diesel cars on birth outcomes do not differ across areas with higher or lower baseline levels of $\mathrm{PM}_{2.5}$, or in areas that are or are not in attainment with EPA-allowed levels of $\mathrm{PM}_{2.5}$ (Table 5). These results concord nicely with the fact that damages from car pollution occur across the entire socioeconomic spectrum, and emphasize the importance of car pollution and $\mathrm{PM}_{2.5}$ for population health even below EPA-allowed concentrations.

5.5 Instrumental variable regressions

Under the arguably plausible assumption that health outcomes are impacted by cheating diesel cars only through their effect on pollution, we can also use an IV strategy to benchmark the health effects of a car pollution-induced change in ambient air quality. Table 6 shows IV regressions of birth outcomes on car pollution with the cheating diesel share as instrument, as well as analogous cross-sectional OLS regressions with month-by-year fixed effects for comparison. As ozone and particulate matter are not always measured in the same places, we maximize sample size by using the satellite-based measure of $\mathrm{PM}_{2.5}$ (which is available for all counties), and the monitor-based measure ozone. We start with regressions that only consider $\mathrm{PM}_{2.5}$—the car pollutant most relevant for health and that we find most consistently affected by cheating diesel cars—and then add ozone.

We find that the IV point estimates of the effect of $\mathrm{PM}_{2.5}$ on birth outcomes are four to eight times larger than the OLS point estimates. As columns 1 and 4 in Panel A show, a 1 $\mu g/m^3$ increase in mean PM 2.5 decreases birth weight by 4.4 grams in the OLS regression and by 31 grams in the IV (0.18 and 0.7 percentage points for low birth weight, respectively, Panel B). Socioeconomic selection would typically bias the OLS estimate upwards, while measurement error has the potential to cause attenuation toward zero. In Table A.7, we provide evidence in support of the role of measurement error; limiting measurement error by focusing on small counties or satellite data substantially increases the OLS but has little impact on the IV point estimates. This finding of OLS estimates that are small compared to IV is very common in the quasi-experimental pollution literature (Chay and Greenstone, 2003b; Knittel et al., 2016; Schlenker and Walker, 2016; Deryugina et al., 2019).

Although it is standard in the literature to focus on a single pollutant when considering the effect of pollution on health, car pollution contains more than particulate matter; the IV estimate of the $\mathrm{PM}_{2.5}$ effect might be too large if cheating diesel cars also impact birth outcomes via other pollutants, such as ozone (Benmarhnia et al., 2017). Although including ozone measurements decreases the sample from around 190,000 to 50,000 observations, we show in columns 2 and 5 that this sample restriction does not change the estimated effect of $\mathrm{PM}_{2.5}$. Column 6 includes both $\mathrm{PM}_{2.5}$ and ozone as endogenous explanatory variables. As an additional instrument, we use the share cheating diesel cars interacted with the maximum monthly temperature, as ozone formation is influenced by ambient temperature.

Results suggest that our measured impacts on infant health run through $\mathrm{PM}_{2.5}$. The point estimate of the effect of $\mathrm{PM}_{2.5}$ on birth outcomes is essentially unchanged when ozone is included, while the IV coefficient on ozone is insignificant and wrong-signed.[32] Taking column 4 as our baseline specification, we find that a car pollution-induced increase in mean $\mathrm{PM}_{2.5}$ by one $\mu g/m^3$ (or 12.25 percent) reduces birth weight by 31 grams (or 0.93 percent), while it increases the rate of low birth weight by 0.7 percentage points (or 13.44 percent), implying elasticities of 0.09 and 1.10, respectively.

[32]Note that point estimates have large magnitudes due to the low mean ozone level in the sample. Further note that the first stage F-stat is 6.1, indicating that instruments are weak. This is mainly due to the weak relationship of cheating diesel cars with mean ozone levels (see Table 2). The relationship is stronger for ozone AQI, and in Table A.8 we repeat the OLS and IV regressions using the pollutant AQI rather than their mean concentration levels. The first stage F-stat is close to 10, but the IV estimate of the effect of ozone on birth weight remains insignificant and reversely signed.

5.6 Effect of diesel emissions on asthma

As our emergency department discharge data cover just five states, we first verify that cheating diesel cars are still associated with higher concentrations of fine particulate matter pollution in this subsample. In column 1 of Table 7, we replicate the effect of the cheating cars on $\mathrm{PM}_{2.5}$ from Table 2, and in column 2 we run the same regression on the subsample of counties for which we have emergency department discharge data. Even in this small subsample we find strong effects of the cheating cars on $\mathrm{PM}_{2.5}$, with a point estimate even larger than that for the entire United States.

Cheating cars are associated with more emergency department visits for asthma among young children. Columns 3 through 6 of Table 7 consider the effect of cheating diesel cars on the rates of emergency department visits for asthma—first for everyone, and then separated by age groups. The overall relationship between cheating diesel cars and emergency department asthma visits is not statistically significant (column 3). However, we see large and precisely measured increases in the number of emergency department asthma visits for young children. An additional cheating diesel car per thousand is associated with an increase of 0.27 visits per quarter per 1,000 children age 0 to 4—an increase of 8 percent.

5.7 Robustness

Section A.1.1 shows that our main results are robust to a broad set of alternative specification choices, different levels of aggregation, choices about weighting, varying the number of monitoring stations used, alternative controls, separating the diesel VW and FCA cars, splitting the sample by the market penetration of cheating cars, dropping the least exposed counties, dropping counties out of compliance with the National Ambient Air Quality Standards, and alternative ways of defining exposure.

5.8 Discussion of magnitudes

In this section we discuss the magnitudes of our effect sizes, placing them in context with what is already known about these and similar relationships. The discussion is complicated by missing general information as well as uncertainty about the specifics of the underlying mechanisms, both for the pollution and the health impacts. Despite the lack of a direct comparison, our reading of the current evidence suggests that our effect sizes are reasonable, considering what prior work has found for related outcomes and sub-populations.

An additional cheating diesel car per thousand cars increases the air quality indices of $\mathrm{PM}_{2.5}$, PM_{10}, and ozone by 2.0 percent, 2.2 percent, and 13 percent, respectively (see Table 2A). Using a range of estimates from the environmental science literature on the contribution of cars to overall particulate matter, in Section 2.3 we show a back-of-the-envelope calculation based on existing estimates that suggest a range of a 0.2 to 6 percent increase in $\mathrm{PM}_{2.5}$ and ozone pollution for each cheating diesel car. Although this calculation relies on many simplifying assumptions to fill gaps in the literature, it places our estimates within a reasonable range. In addition, our estimates suggest that approximately 20 percent of overall ambient $\mathrm{PM}_{2.5}$ originates from passenger cars in complier counties.

It is more difficult to benchmark our effects on birth outcomes, as little is known about the parameters of the biological mechanisms that translate $\mathrm{PM}_{2.5}$ and ozone into fetal and infant health. Moreover, to the best of our knowledge, our study provides the first quasi-experimental (or instrumental variable) estimate of $\mathrm{PM}_{2.5}$ on birth outcomes so we cannot compare our results directly to previous quasi-experimental estimates. One natural benchmark are the cross-sectional OLS estimates presented in Tables 6 and A.7. Similarly to the epidemiological literature on pollution and birth outcomes, we find that a unit increase in a county's $\mathrm{PM}_{2.5}$ pollution is associated with birth weight reductions of 2.5 to 8.2 grams (see Table A.7). Similarly, Hyder et al. (2014) report reductions in birthweight of 2.5 to 7.9 grams for the same change in $\mathrm{PM}_{2.5}$. However, our IV estimates are many times larger than the OLS coefficient, and we argue that the latter are attenuated by measurement error while our IV strategy corrects for this bias.

A number of important quasi-experimental studies have looked at the impact of overall pollution and high-frequency variation in traffic on infant mortality. Although these studies look at a (fortunately) rare outcome in a more disadvantaged population, we can still compare the relative effects on infant mortality to our estimated impact on low birth weight. Knittel et al. (2016) report a 1.03 elasticity of infant mortality with respect to traffic-induced weekly PM_{10} changes, and Chay and Greenstone (2003a,b) find elasticities of 0.35 percent to 0.45 percent with respect to recession-induced total suspended particulates variation.[33] Our estimated elasticity of low birth weight with respect to $\mathrm{PM}_{2.5}$ of 1.10 is at the upper end of this range.

Another closely related quasi-experimental study is Currie and Walker (2011), which shows that the decrease in traffic congestion caused by the introduction of electronic toll collection is associated with a decrease in low birth weight of 12 percent for mothers in direct proximity to toll stations. Although no direct comparison to our effect magnitudes is possible as pollution impacts are not reported, the authors cite a government report stating that delays (a proxy for traffic) dropped by 85 percent after the introduction of electronic toll collection. Our IV estimate suggests that it would take a 60 percent decrease in car pollution to decrease low birth weight by 12 percent. Again, our estimate is if anything larger than the implied IV in Currie and Walker (2011), despite the strongly negative selection of mothers living next to highway toll stations.

Finally, our largest effects in percent terms are those on asthma visits by young children to emergency departments. The most closely related study in this regard is a recent working paper looking at the effect of congestion pricing in Stockholm (Simeonova et al., 2018). This study found that congestion pricing led to reductions in $\mathrm{PM}_{2.5}$ of 15 to 20 percent and decreases in emergency department visits for young children of 12 to 47 percent (Simeonova et al., 2018). Scaled by the change in pollution, our magnitudes are similar to those found in Stockholm: for a 7.5 percent increase in $\mathrm{PM}_{2.5}$, we find an 8 percent decrease in the rate of asthma emergency department visits for young children. A few other papers look at the effect of air pollution on asthma and find similarly large effects, but focus on different pollutants. He et al. (forthcoming) document that

[33] Currie and Neidell (2005) and Currie et al. (2009b) find impacts for CO, a pollutant our study is not suited to identify and for which Knittel et al. (2016) do not find traffic-related impacts.

a 17.86 percent reduction in NO_x reduces respiratory admissions among children by 9.1 percent in the of a highly polluted megacity. Marcus (2017) finds that regulations in California requiring cleaner-burning gasoline decrease NO_2, CO, and SO_2 by 2, 6, and 11 percent, and also decrease asthma hospitalizations by 3 to 8 percent for children living near highways. Schlenker and Walker (2016) use air traffic network delays originating in the eastern United States as an instrument for pollution in California, and find that a one standard deviation increase in CO pollution is associated with a 37 percent increase in asthma hospital visits for children under 5 years.[34] Lastly, using variation in PM_{10} from seasonal openings and closings of a steel mill in Utah, Pope (1989) finds that in months with average PM_{10} levels greater than or equal to 50 $\mu g/m^3$, average hospital admissions for children for respiratory disease (including asthma) increased by 89 percent. Thus, although large, we believe our results on asthma are plausible.

6 Conclusion

In this paper, we use emissions cheating diesel cars as a natural experiment to measure the effects of car pollution on population health. We find that an additional cheating diesel car per thousand, which can be thought of as an increase in passenger car pollution of around ten percent, increases $\mathrm{PM}_{2.5}$ and ozone by 2.0 and 1.3 percent, respectively. The same increase in the effective level of car pollution increases the low birth weight rate by 1.9 percent and ED visits for young children by 8 percent. We contribute to the literature on pollution and health by showing in a causal framework that car exhaust is an important contributor to overall ambient pollution and that car pollution impairs population health at all pollution levels and across the entire socio-economic spectrum.

We focus on health at birth and in early childhood because of its importance over the entire life-cycle, and because exposure to pollution affects these outcomes over relatively short time horizons. Exploring health as well as productivity impacts among individuals exposed at older ages would

[34] A 1 standard deviation increase in $\mathrm{PM}_{2.5}$ would be an increase of 3.8$\mu\mathrm{g/m}^3$, which is six times the effect of an additional cheating car in the HCUP sample. Thus, a 1 standard deviation increase in $\mathrm{PM}_{2.5}$ from cheating cars would be associated with a 48 percent increase in asthma ED visits.

be a fruitful path for future research, though it might take time for the impacts on some of those outcomes to be measurable in data.

Our paper has three main takeaways for policy makers. First, car pollution is a society-wide health threat. Although more research is needed to measure and explore the entire distribution of damages, our results suggest that a singular focus on disadvantaged populations living close to pollution hotspots misses the larger picture. Second, regulators, consumers, and communities need to be informed about these broader health costs of car pollution. Diesel cars, which if insufficiently filtered, cause more $\mathrm{PM}_{2.5}$ and ozone than gasoline cars, should be a primary focus. Third, strong regulation needs to be paired with strong enforcement to be successful, as tightening regulations can increase the returns to cheating (Reynaert and Sallee, 2018). In the past, most of the policy discussion around limiting emissions from cars has focused on how strict to make emissions limits rather than on how to ensure they are followed. This deficit in enforcement has been laid bare as a result of the emissions cheating scandals, and as a result, both the United States and the European Union are making changes to how pollution limits are enforced.

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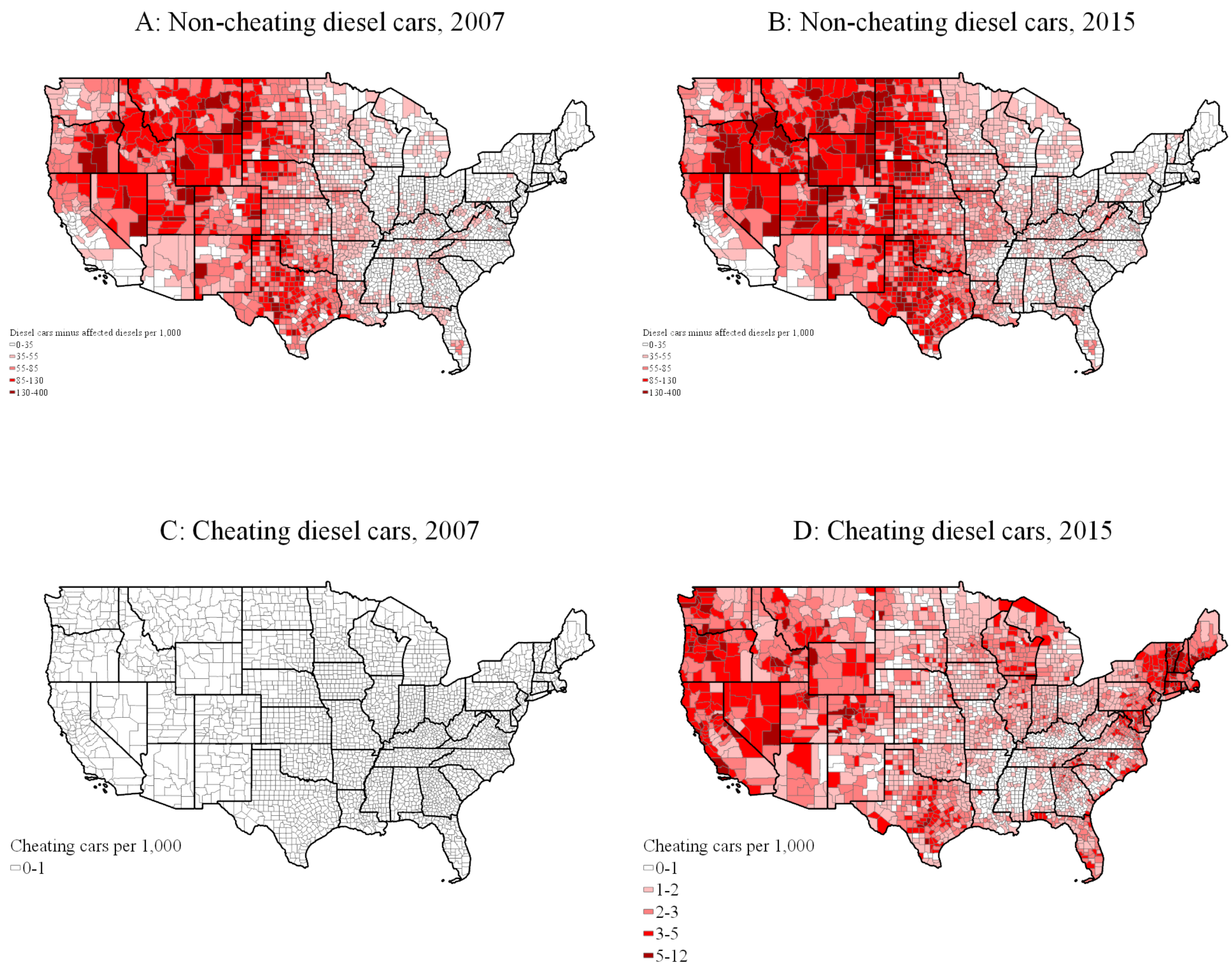
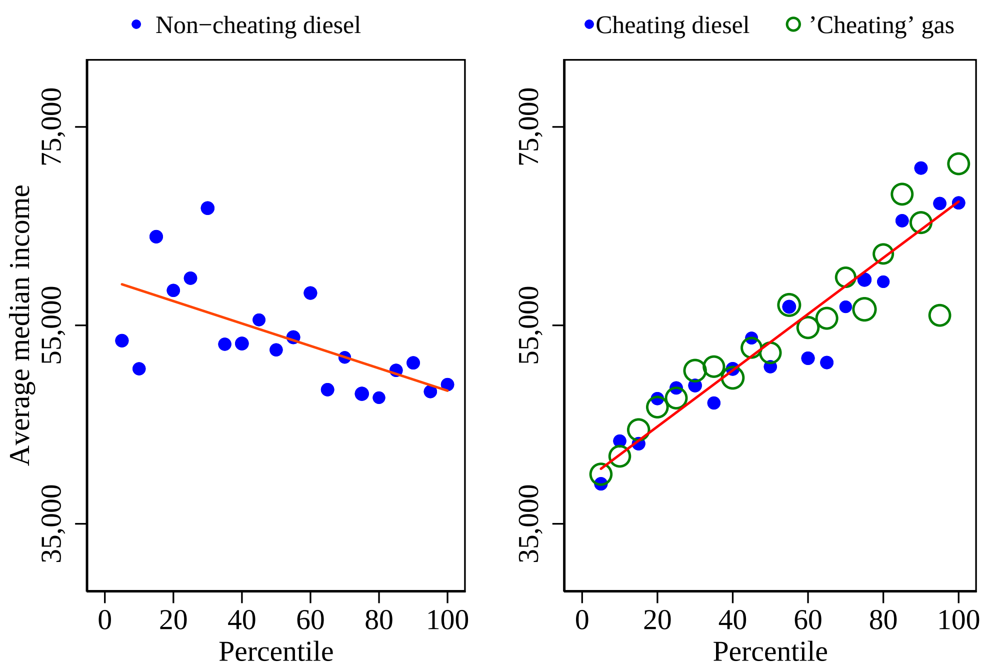
7 Figures

Figure 1: County-level distribution of diesel cars



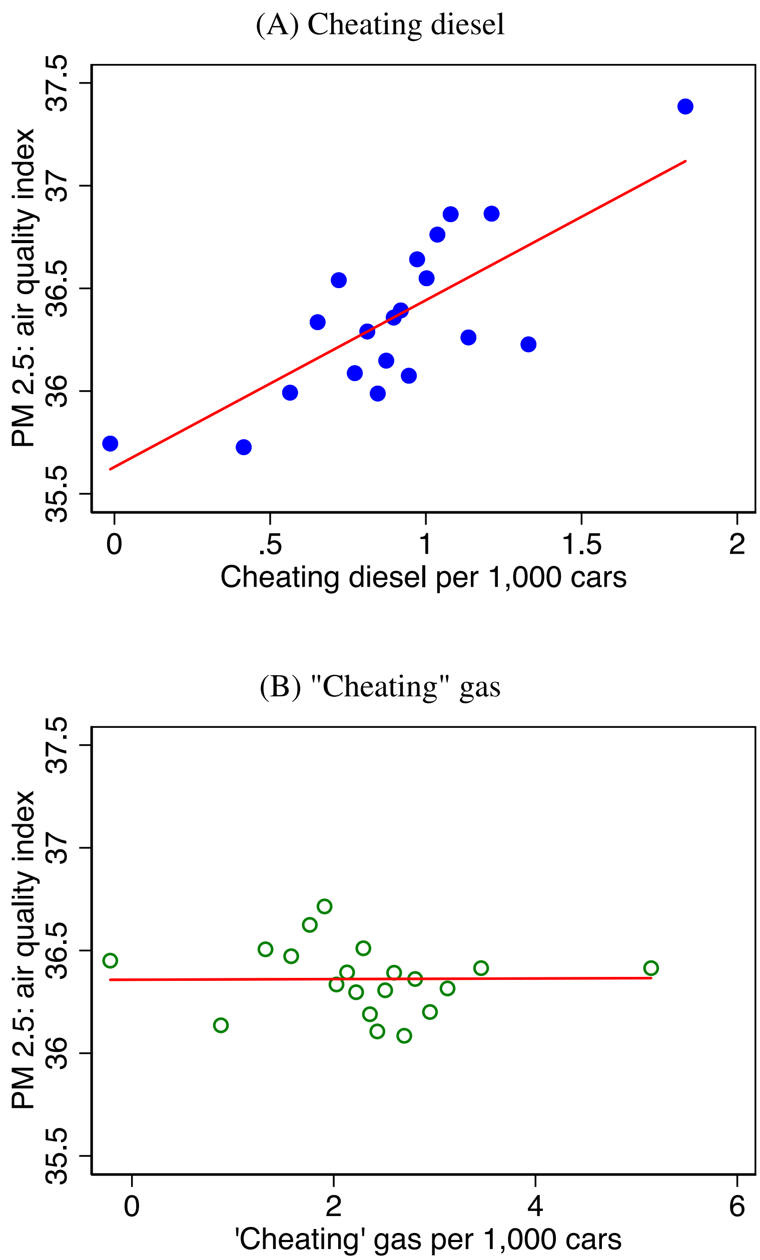
Notes: Number of non-cheating and cheating diesel cars per 1,000 cars and light-duty trucks registered at the county-level in 2007 and 2015. Cheating diesel cars are make-model-model years exposed in the emissions cheating scandals, from IHS Markit (for a complete list of cheating cars see Table A.15). Non-cheating diesel cars are diesel cars not included in the emissions cheating scandals.

Figure 2: Average median income in 2015 across county groups ranked by diesel fraction, and by "cheating" gas fraction



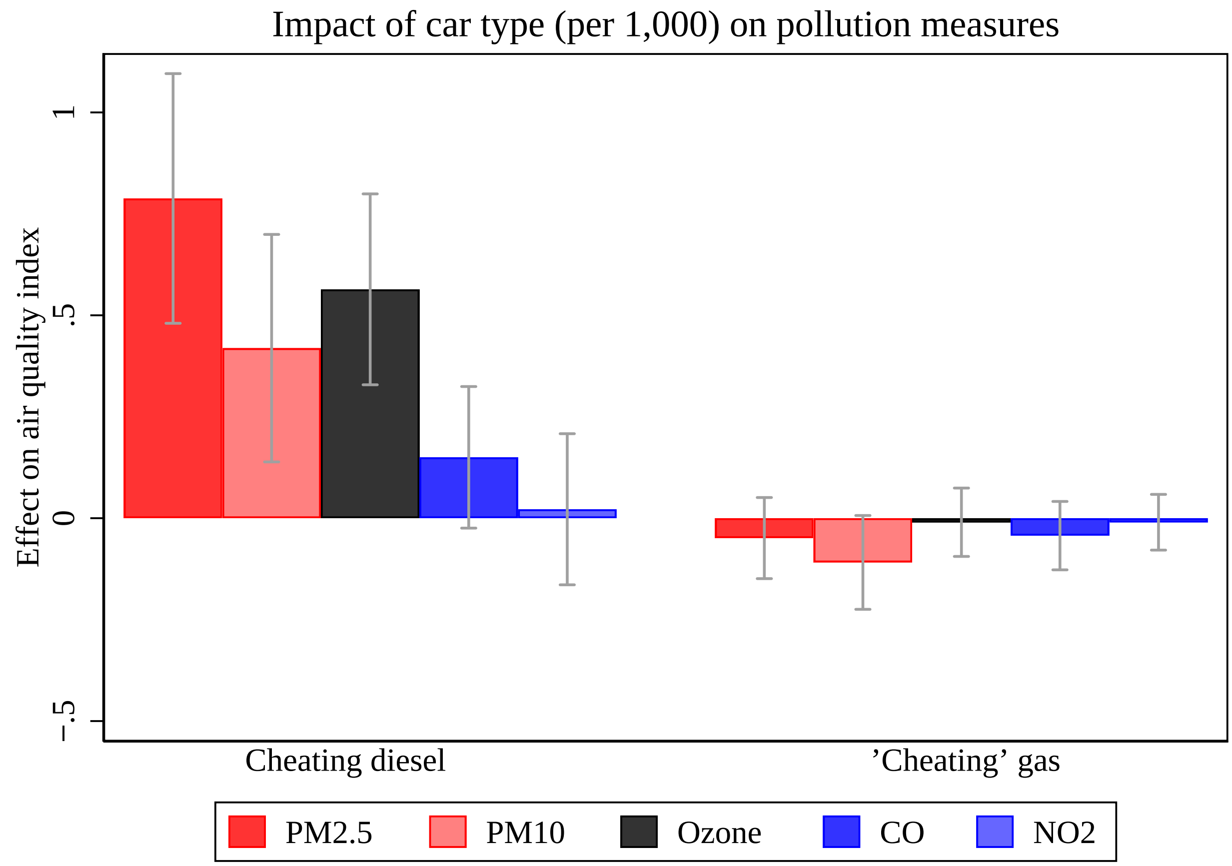
Notes: Median income from the census. Scatter plots divide counties into ventiles based on vehicle composition, and then plot the average median income in each bin. For the left figure, counties are ranked by their fraction of non-cheating diesel models. For the right figure, counties are ranked by their fraction of cheating diesel models and "cheating" gas models, respectively. Cheating diesel cars are make-model-model years exposed in the emissions cheating scandals; "cheating" gas refers to the gas versions of the cheating diesel cars; non-cheating diesel cars are diesel cars not included in the emissions cheating scandals, all from IHS Markit (for a complete list of cheating cars see Table A.15). Observations at the county-month level in 2015, weighted by number of births.

Figure 3: Binned scatter plots of $\mathrm{PM}_{2.5}$pollution against the share of cheating diesel and "cheating" gas, controlling for county and year fixed effects



Notes: Pollution outcomes from the EPA. Cheating diesel cars are make-model-model years exposed in the emissions cheating scandals; "cheating" gas refers to the gas versions of the cheating diesel cars, both from IHS Markit (for a complete list of cheating cars see Table A.15). Binned scatter plots divide counties into ventiles by the number of cheating diesel cars per 1,000 cars (panel A) and the number of "cheating" gas cars per 1,000 cars (Panel B). The average of vehicle composition in each bin is plotted on the x-axis, and the mean pollutions outcomes plus the residual in each bin is plotted on the y-axis, after partialling out county and year fixed effects, the log number of total cars, the number of non-cheating diesel cars per 1,000 cars, and the number of "cheating" gas cars per 1,000 cars (Panel A), and number of cheating diesel cars per 1,000 cars (Panel B). Observations at the county-year level, from 2007-2015.

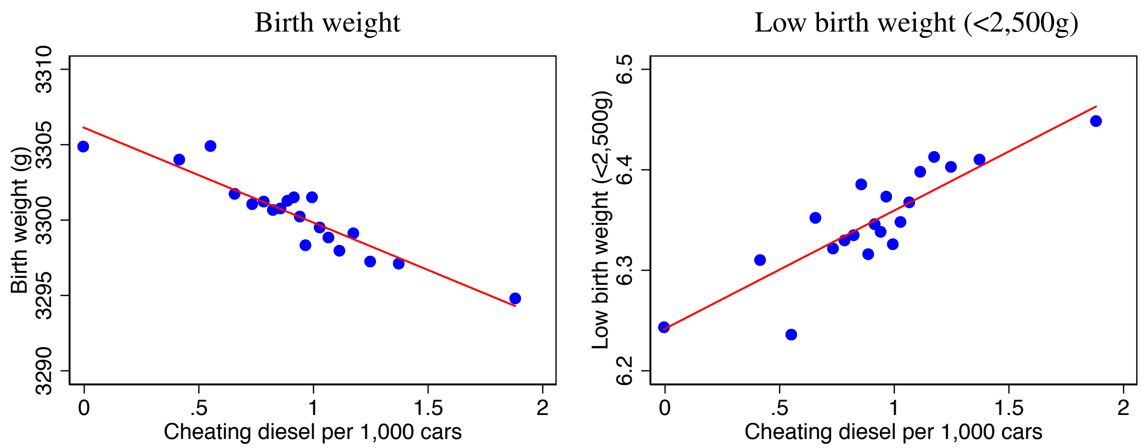
Figure 4: Effect of vehicle composition on air quality indices for different pollutants



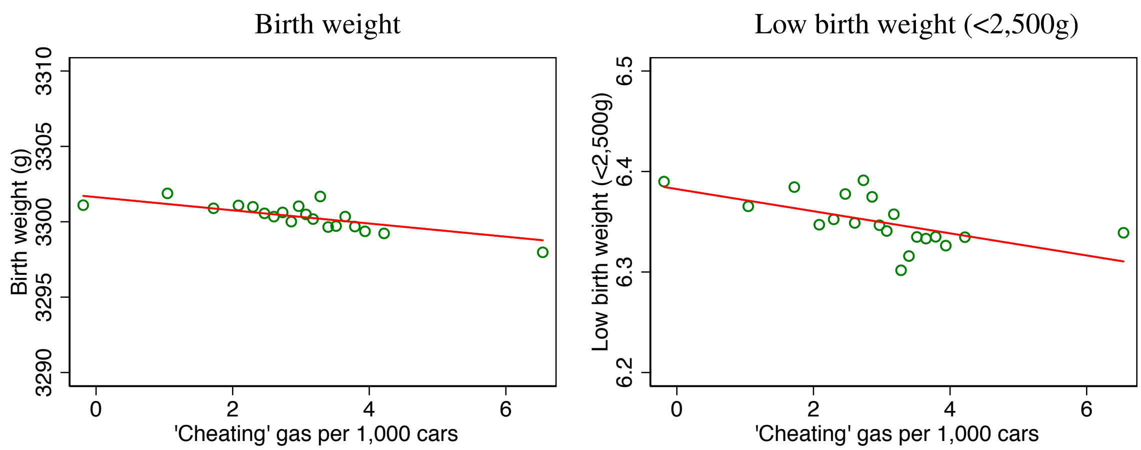
Notes: Coefficients are plotted from regressions of air quality indices on the number of cheating diesel cars per 1,000 cars and the number of "cheating" gas cars per 1,000 cars, as reported in Table 2B. Separate regressions are run for each pollutant. Pollution data is from the EPA; information on the construction of the air quality indices is given in section A.1.2. Cheating diesel cars are make-model-model years exposed in the emissions cheating scandals; "cheating" gas refers to the gas versions of the cheating diesel cars, both from IHS Markit (for a complete list of cheating cars see Table A.15). Additional controls are: population, poverty rate, child poverty rate, and median income from the census; and the log total cars registered and the fraction of non-cheating diesel cars from IHS Markit. County and month-by-year fixed effects also included. Observations at the county-month level, from 2007-2015. Standard errors clustered at the county level.

Figure 5: Binned scatter plots of birth weight and low birth weight rates against the share of cheating diesel and "cheating" gas, controlling for county and year fixed effects

(A) Cheating diesel

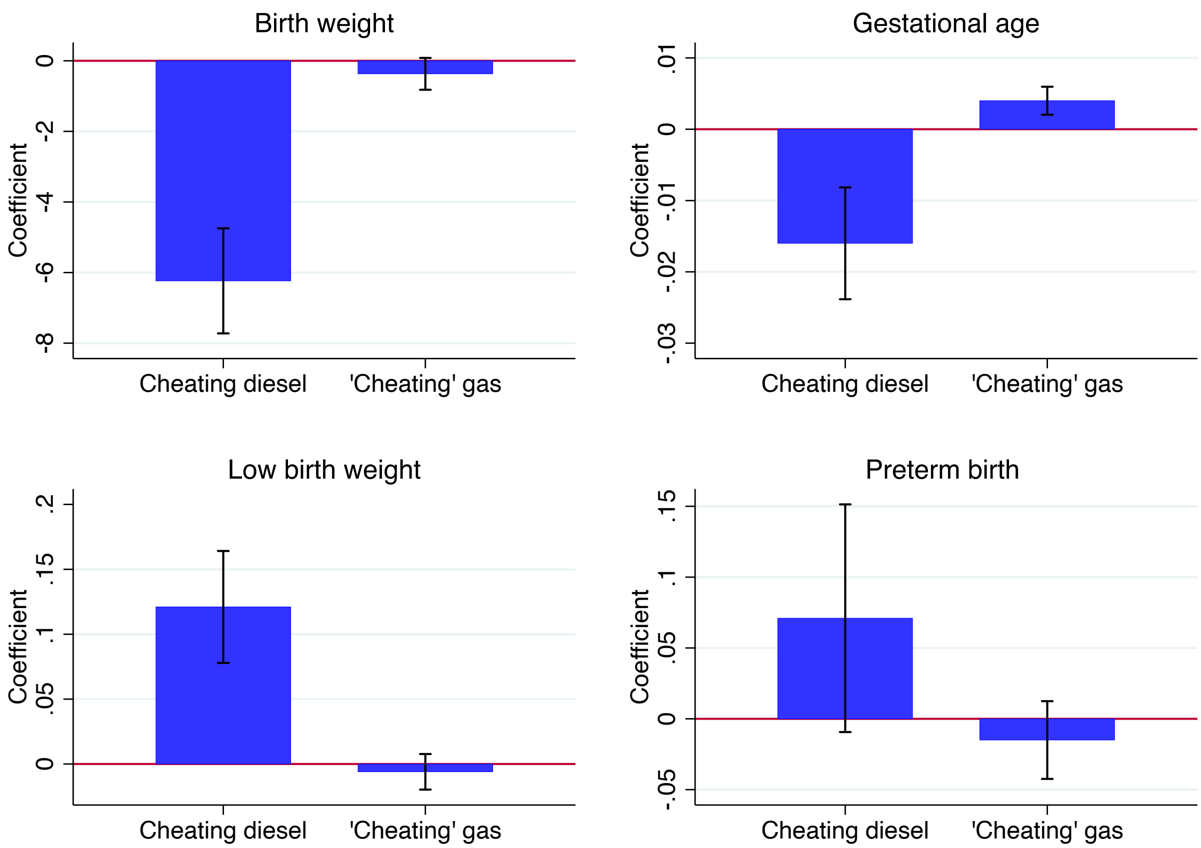


(B) "Cheating" gas



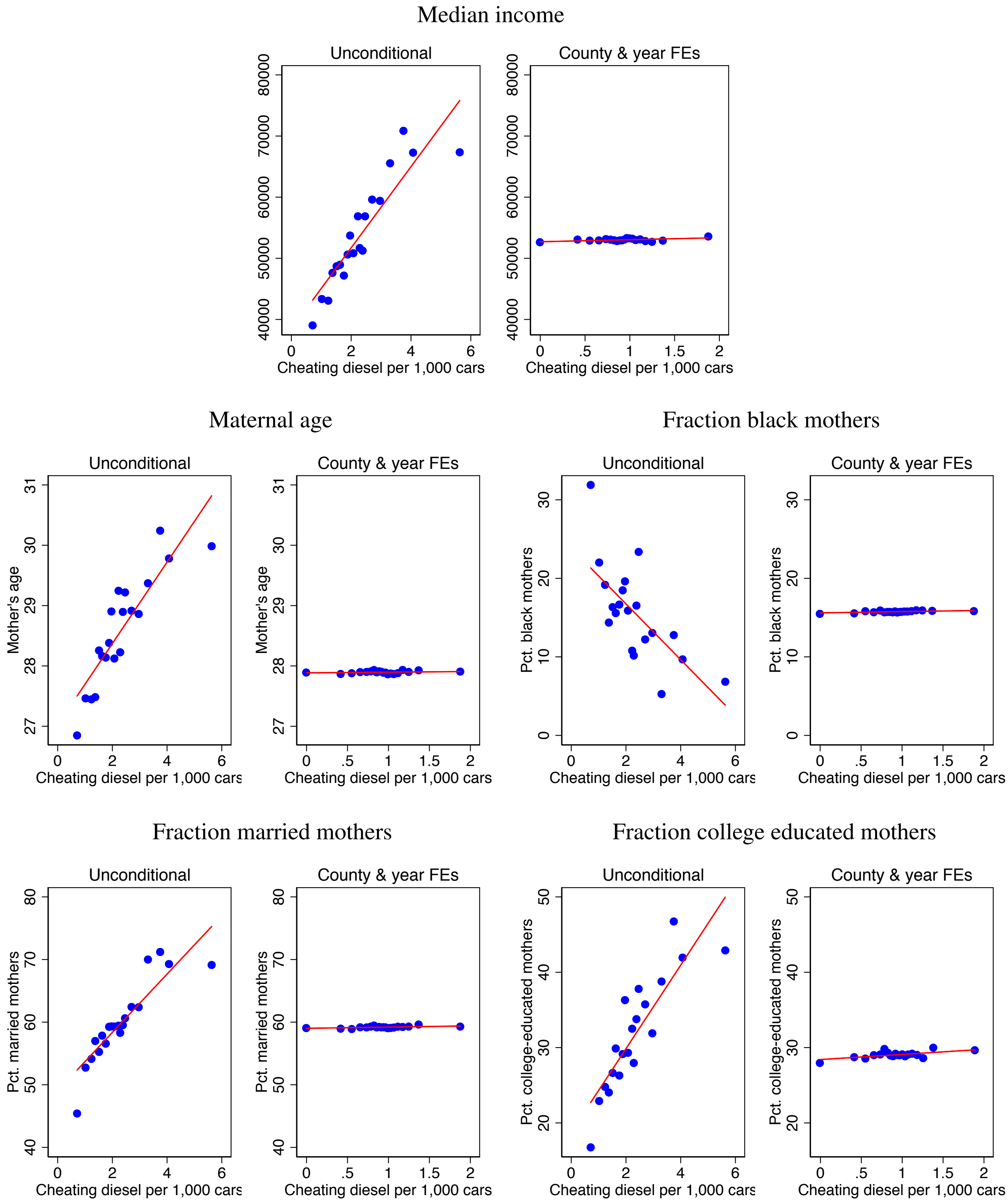
Notes: Birth weight and fraction low birth weight from birth certificate data. Cheating diesel cars are make-model-model years exposed in the emissions cheating scandals; "cheating" gas refers to the gas versions of the cheating diesel cars, both from IHS Markit (for a complete list of cheating cars see Table A.15). Binned scatter plots divide counties into ventiles by the number of cheating diesel cars per 1,000 cars (panel A) and the number of "cheating" gas cars per 1,000 cars (Panel B). The average of vehicle composition in each bin is plotted on the x-axis, and the mean birth outcomes plus the residual in each bin is plotted on the y-axis, after partialling out county and year fixed effects, the log number of total cars, the number of non-cheating diesel cars per 1,000 cars, and the number of "cheating" gas cars per 1,000 cars (Panel A), and number of cheating diesel cars per 1,000 cars (Panel B). Observations at the county-year level, from 2007-2015, weighted by the number of births in the county-year.

Figure 6: Effect of vehicle composition on birth outcomes



Notes: Coefficients are plotted from regressions of birth outcomes on the number of cheating diesel cars per 1,000 cars and the number of "cheating" gas cars per 1,000 cars, as reported in Table 3. Birth weight, fraction low birth weight, gestation age in weeks, and fraction born premature from birth certificate data. Cheating diesel cars are make-model-model years exposed in the emissions cheating scandals; "cheating" gas refers to the gas versions of the cheating diesel cars, both from IHS Markit (for a complete list of cheating cars see Table A.15. Regressions control for county and month-by-year fixed effects. Additional controls are: population, poverty rate, child poverty rate, and median income from the census; the log total cars registered and the fraction of non-cheating diesel cars from IHS Markit; fraction Hispanic, black, married, smoked during pregnancy, mothers' average age, and fraction in education bins from birth certificate data. Observations at the county-month level, from 2007-2015, weighted by number of births. Standard errors clustered at the county level.

Figure 7: Binned scatter plots of maternal characteristics against the share of cheating diesel



Notes: Median income from the census, maternal characteristics from birth certificate data. Cheating diesel cars are make-model-model years exposed in the emissions cheating scandals, from IHS Markit (for a complete list of cheating cars see Table A.15). The left plot of each pair is a binned scatter plot with the number of cheating diesel cars per 1,000 cars in 2015 on the x-axis and the maternal characteristic on the y-axis. The right plot shows a binned scatter plot of the same maternal characteristic against the number of cheating diesel cars per 1,000 cars, but now using the entire time period (2007-2015), and partialling out county and year fixed effects. Observations at the county-year level, weighted by the number of births in the county-year.

8 Tables

Table 1: Summary statistics: area characteristics

	All counties	PM 2.5 monitor	By frac. cheating in 2015 Tercile 1	Tercile 2	Tercile 3
Vehicle characteristics					
Cheating diesel per 1,000 cars	0.70	0.89	0.28	0.60	1.21
Non-cheating diesel per 1,000 cars	44.83	29.76	40.02	43.45	51.04
'Cheating' gas per 1,000 cars	1.48	2.27	0.95	1.44	2.05
Fraction cheating diesel in 2015	1.84	2.25	0.79	1.59	3.15
Total cars (thousands)	80.7	287.4	31.9	82.3	127.9
Total miles driven (millions)	82,042	90,686	65,670	82,747	97,712
Census/SAIPE					
Total population	99,125	370,406	38,398	100,972	158,013
Pct. in poverty	16.45	15.08	20.20	15.75	13.41
Pct. children in poverty	23.34	20.91	28.80	22.41	18.83
Median income	44,040	50,041	37,493	43,770	50,846
Pollution outcomes (1 monitor/county)					
PM 2.5: $\mu g/m^3$	9.34	9.34	9.90	9.93	8.69
PM 2.5: air quality index	36.35	36.35	38.74	38.53	33.82
PM 10: $\mu g/m^3$	19.72	19.95	21.08	21.14	18.52
Ozone: ppm	0.031	0.030	0.030	0.031	0.031
CO: ppm	0.33	0.33	0.33	0.32	0.34
NO2: ppm	8.73	9.51	8.72	8.31	9.04
N	339,240	55,940	113,088	113,076	113,076

	All counties	PM 2.5 monitor	By frac. cheating in 2015 Tercile 1	Tercile 2	Tercile 3
Birth outcomes					
Birth weight (g)	3,300	3,294	3,249	3,286	3,321
Low birth weight (<2,500g)	6.35	6.63	7.39	6.70	5.90
Gestational age in weeks	38.74	38.71	38.60	38.67	38.81
Preterm birth	10.05	10.28	11.57	10.72	9.30
Birth characteristics					
Number of births (county-month)	1,537	1,969	523	1,172	1,992
Pct. hispanic mothers	24.10	26.50	13.19	24.15	26.44
Pct. black mothers	15.74	16.71	26.74	17.34	12.31
Pct. white mothers	75.93	73.73	66.15	76.16	77.91
Pct. married mothers	59.19	59.80	48.71	56.68	63.08
Mother's age	27.90	28.24	26.37	27.44	28.52
Pct. mothers smoked during pregnancy	8.83	6.99	14.55	10.56	6.57
Pct. college-educated mothers	29.04	31.23	17.80	25.93	33.40
Pct. mothers w/ some college	27.69	26.86	28.72	28.48	26.98
Pct. mothers w/ < high school	17.58	17.44	21.82	18.42	16.15
Pct. high school-educated mothers	26.14	25.02	31.66	27.79	23.91
N (births>0)	209,533	51,765	57,134	74,229	78,170

Notes: Vehicle characteristics from IHS Markit; county-level characteristics from the census; pollution outcomes from the EPA; birth outcomes and characteristics from birth certificate data. Means of birth outcomes and characteristics are weighted by number of births. Cheating diesel cars are make-model-model years exposed in the emissions cheating scandals from IHS Markit (for a complete list of cheating cars see Table A.15). Observations at the county-month, restricted to months with non-zero births, from 2007-2015.

Table 2: Effect of vehicle composition on pollution

A: Air quality index

	PM 2.5 (1)	PM 10 (2)	Ozone (3)	CO (4)	NO2 (5)
Cheating diesel per 1,000 cars	0.79***	0.42***	0.56***	0.15*	0.022
	(0.16)	(0.14)	(0.12)	(0.089)	(0.095)
'Cheating' gas per 1,000 cars	-0.049	-0.11*	-0.010	-0.043	-0.010
	(0.051)	(0.059)	(0.043)	(0.043)	(0.035)
FEs: county, time	Yes	Yes	Yes	Yes	Yes
Observations	55,940	34,766	64,206	18,392	23,401
R^2	0.532	0.582	0.613	0.684	0.867
Mean dep. var.	39.72	19.12	41.99	8.513	20.29
P-value ($\beta_1 = \beta_2$)	0.000	0.002	0.000	0.079	0.772

B: Mean pollutant concentration ($\mu g/m^3$ for $\mathrm{PM}_{2.5}$ and PM_{10}, ppm for CO and NO_2)

	PM 2.5 (1)	PM 2.5, sat. (2)	PM 10 (3)	Ozone (4)	CO (5)	NO2 (6)
Cheating diesel per 1,000 cars	0.26***	0.23***	0.30	0.00015*	0.015**	0.024
	(0.049)	(0.015)	(0.22)	(0.000079)	(0.0068)	(0.066)
'Cheating' gas per 1,000 cars	-0.016	-0.018***	-0.070	0.000045	-0.0042	-0.027
	(0.016)	(0.0059)	(0.081)	(0.000030)	(0.0031)	(0.029)
FEs: county, time	Yes	Yes	Yes	Yes	Yes	Yes
Observations	55,940	336,576	34,766	64,206	18,392	23,401
R^2	0.483	0.588	0.474	0.686	0.599	0.861
Mean dep. var.	10.46	8.905	21.01	.03026	.4309	10.58
P-value ($\beta_1 = \beta_2$)	0.000	0.000	0.164	0.276	0.020	0.538

C: Number of days with air quality index>51

	PM 2.5 (1)	PM 10 (2)	Ozone (3)	CO (4)	NO2 (5)
Cheating diesel per 1,000 cars	0.39***	0.045	0.29***	0.0039	0.060**
	(0.11)	(0.039)	(0.061)	(0.0027)	(0.027)
'Cheating' gas per 1,000 cars	-0.063*	-0.016	0.031	0.0013	-0.011
	(0.033)	(0.020)	(0.024)	(0.0019)	(0.015)
FEs: county, time	Yes	Yes	Yes	Yes	Yes
Observations	55,940	34,766	64,206	18,392	23,401
R^2	0.498	0.463	0.567	0.134	0.395
Mean dep. var.	4.344	.5336	5.42	.08829	.7376
P-value ($\beta_1 = \beta_2$)	0.000	0.193	0.001	0.404	0.051

Notes: Pollution monitor data from the EPA, satellite data from van Donkelaar et al. (2019); information on the construction of the air quality indices in section A.1.2. Cheating diesel cars are make-model-model years exposed in the emissions cheating scandals; "cheating" gas refers to the gas versions of the cheating diesel cars, both from IHS Markit (for a complete list of cheating cars see Table A.15). "FEs: county, time" refer to county and month-by-year fixed effects. Additional controls are: population, poverty rate, child poverty rate, and median income from the census; and the log total cars registered and the fraction of non-cheating diesel cars from IHS Markit. Observations at the county-month level, from 2007-2015. Standard errors clustered at the county level.

Table 3: Effect of vehicle composition on birth outcomes

Panel A: Birth weight

	Birth weight (in grams)		Low birth weight ($< 2,500$ grams)	
	(1)	(2)	(3)	(4)
Cheating diesel per 1,000 cars	-6.739***	-6.236***	0.116***	0.121***
	(0.704)	(0.759)	(0.020)	(0.022)
'Cheating' gas per 1,000 cars		-0.370		-0.006
		(0.230)		(0.007)
FEs: county, time	Yes	Yes	Yes	Yes
Observations	191,082	191,082	191,082	191,082
R^2	0.733	0.734	0.559	0.559
Mean dep. var.	3,302	3,302	6.314	6.314
P-value ($\beta_1 = \beta_2$)		0.000		0.000

Panel B: Gestational age

	Gestational age (in weeks)		Preterm birth (< 37 weeks)	
	(1)	(2)	(3)	(4)
Cheating diesel per 1,000 cars	-0.012***	-0.016***	0.055	0.071*
	(0.004)	(0.004)	(0.043)	(0.041)
'Cheating' gas per 1,000 cars		0.004***		-0.015
		(0.001)		(0.014)
FEs: county, time	Yes	Yes	Yes	Yes
Observations	191,102	191,102	191,102	191,102
R^2	0.616	0.617	0.534	0.534
Mean dep. var.	38.74	38.74	9.975	9.975
P-value ($\beta_1 = \beta_2$)		0.000		0.056

Notes: Birth weight, fraction low birth weight, gestation age in weeks, and fraction born premature from birth certificate data. Cheating diesel cars are make-model-model years exposed in the emissions cheating scandals; "cheating" gas refers to the gas versions of the cheating diesel cars, both from IHS Markit (for a complete list of cheating cars see Table A.15). "FEs: county, time" refer to county and month-by-year fixed effects. Additional controls are: population, poverty rate, child poverty rate, and median income from the census; the log total cars registered and the fraction of non-cheating diesel cars from IHS Markit; fraction Hispanic, black, married, smoked during pregnancy, mothers' average age, and fraction in education bins from birth certificate data. Observations at the county-month level, from 2007-2015, weighted by number of births. Standard errors clustered at the county level.

Table 4: Effect of vehicle composition on birth weight by subgroups (micro data)

Panel A: Birth weight (in grams)

	(1) nHwhite	(2) Black	(3) College degree	(4) No college	(5) nHwhite college	(6) Black no college
Cheating diesel per 1,000 cars	-6.66***	-4.23**	-6.33***	-6.10***	-6.25***	-2.61
	(0.79)	(1.86)	(0.85)	(0.91)	(0.84)	(1.94)
'Cheating' gas per 1,000 cars	-0.27	-0.16	-0.44	-0.17	-0.56*	-0.31
	(0.25)	(0.41)	(0.31)	(0.28)	(0.29)	(0.52)
FE: county, time	Yes	Yes	Yes	Yes	Yes	Yes
Observations	16,536,223	4,587,491	8,870,725	13,333,095	6,298,914	2,452,613
R^2	0.043	0.028	0.036	0.048	0.013	0.029
Mean dep. var.	3366.0	3122.0	3379.7	3246.7	3431.1	3086.1
P-value ($\beta_1 = \beta_2$)	0.000	0.051	0.000	0.000	0.000	0.310

Panel B: Low birth weight (<2,500g)

	(1) nHwhite	(2) Black	(3) College degree	(4) No college	(5) nHwhite college	(6) Black no college
Cheating diesel per 1,000 cars	0.12***	0.13*	0.091***	0.16***	0.076***	0.087
	(0.022)	(0.070)	(0.024)	(0.029)	(0.023)	(0.089)
'Cheating' gas per 1,000 cars	-0.016***	0.0029	-0.0041	-0.0087	-0.0045	0.028
	(0.0057)	(0.017)	(0.0073)	(0.011)	(0.0058)	(0.024)
FE: county, time	Yes	Yes	Yes	Yes	Yes	Yes
Observations	16,536,223	4,587,491	8,870,725	13,333,095	6,298,914	2,452,613
R^2	0.016	0.012	0.009	0.018	0.004	0.013
Mean dep. var.	5.15	11.0	4.46	7.50	3.67	11.9
P-value ($\beta_1 = \beta_2$)	0.000	0.101	0.000	0.000	0.002	0.557

Notes: Birth weight and low birth weight indicator from birth certificate data. nHwhite refers to non-Hispanic white mothers. Cheating diesel cars are make-model-model years exposed in the emissions cheating scandals; "cheating" gas refers to the gas versions of the cheating diesel cars, both from IHS Markit (for a complete list of cheating cars see Table A.15). "FEs: county, time" refer to county and month-by-year fixed effects. Additional controls are: population, poverty rate, child poverty rate, and median income from the census; the log total cars registered and the fraction of non-cheating diesel cars from IHS Markit; Hispanic, black, white, married, age, and education bins from birth certificate data. Observations at the individual level, from 2007-2015. Standard errors clustered at the county level.

Table 5: Main birth results by baseline pollution levels

	by baseline PM 2.5				by EPA nonattainment status			
	(1) Birth weight	(2) Low birth weight	(3) Gestational age	(4) Preterm birth	(5) Birth weight	(6) Low birth weight	(7) Gestational age	(8) Preterm birth
Cheating diesel per 1,000 cars	-6.140***	0.116***	-0.015**	-0.013	-6.193***	0.123***	-0.016***	0.086**
	(1.051)	(0.027)	(0.006)	(0.063)	(0.735)	(0.022)	(0.004)	(0.040)
Cheating diesel * 07/08 mean pm25	0.112	-0.011	0.000	-0.073**				
	(0.377)	(0.009)	(0.003)	(0.032)				
Cheating diesel * out of compliance					-0.366	-0.017	-0.000	-0.125**
					(0.942)	(0.021)	(0.005)	(0.053)
Observations	51,958	51,958	51,961	51,961	190,967	190,967	190,987	190,987
R^2	0.839	0.715	0.722	0.689	0.734	0.559	0.617	0.534
Mean dep. var.	3,294	6.639	38.71	10.24	3,302	6.314	38.74	9.975

Notes: Birth weight, fraction low birth weight, gestation age in weeks, and fraction preterm from birth certificate data. Cheating diesel cars are make-model-model years exposed in the emissions cheating scandals; "cheating" gas refers to the gas versions of the cheating diesel cars, both from IHS Markit (for a complete list of cheating cars see Table A.15); 07/08 mean pm25 refers to counties' normalized (mean zero, std. dev. one) average $\mathrm{PM}_{2.5}$ level in 2007 and 2008. The non-interacted main effect is absorbed by the county fixed effects. "FEs: county, time" refer to county and month-by-year fixed effects. Additional controls are: population, poverty rate, child poverty rate, and median income from the census; the log total cars registered and the fraction of non-cheating diesel cars from IHS Markit; fraction Hispanic, black, married, smoked during pregnancy, mothers' average age, and fraction in education bins from birth certificate data. Observations at the county-month level, from 2007-2015, weighted by number of births. Standard errors clustered at the county level.

Table 6: OLS and IV regressions of birth weight on mean PM 2.5 ($\mu g/m^3$) and ozone (ppm)

Panel A: Birth weight (in grams)

	Cross-sectional OLS			Instrumental variable		
	(1)	(2)	(3)	(4)	(5)	(6)
Mean PM 2.5	-4.386***	-4.011***	-4.120***	-30.92***	-30.72***	-33.09***
	(0.553)	(0.885)	(0.849)	(5.687)	(6.768)	(8.064)
Mean ozone			-414.8			7190.8
			(339.3)			(5171.6)
Ozone sample	No	Yes	Yes	No	Yes	Yes
FEs: time	Yes	Yes	Yes	No	No	No
FEs: county, time	No	No	No	Yes	Yes	Yes
First stage F-stat				186	69.3	6.1
Mean dep. var.	3,341	3,324	3,324	3,341	3,324	3,324
Mean PM 2.5	8.16	8.43	8.43	8.16	8.43	8.43
Mean ozone	0.031	0.031	0.031	0.031	0.031	0.031
Observations	189,711	50,714	50,714	189,711	50,714	50,714

Panel B: Low birth weight (<2,500g)

	Cross-sectional OLS			Instrumental variable		
	(1)	(2)	(3)	(4)	(5)	(6)
Mean PM 2.5	0.184***	0.162***	0.154***	0.699***	0.694***	0.760***
	(0.0127)	(0.0208)	(0.0207)	(0.192)	(0.234)	(0.261)
Mean ozone			-27.45***			-131.3
			(7.669)			(139.1)
Ozone sample	No	Yes	Yes	No	Yes	Yes
FEs: time	Yes	Yes	Yes	No	No	No
FEs: county, time	No	No	No	Yes	Yes	Yes
First stage F-stat				186	69.3	6.1
Mean dep. var.	5.2	5.74	5.74	5.2	5.74	5.74
Mean PM 2.5	8.16	8.43	8.43	8.16	8.43	8.43
Mean ozone	0.031	0.031	0.031	0.031	0.031	0.031
Observations	189,711	50,714	50,714	189,711	50,714	50,714

Notes: Birth weight and fraction low birth weight from birth certificate data; mean PM 2.5 from van Donkelaar et al. (2019) satellite data; mean ozone from the EPA data. Cross-sectional OLS refers to regressions of birth outcomes on the pollution measure and month-by-year fixed effects. Instrumental variables (IV) refers to IV regressions of birth outcomes on pollution with the cheating diesel share as instrument. Column 6 includes the interaction of the cheating diesel share with the maximum monthly temperature as an additional instrument. "FEs: county, time" indicates inclusion of county and month-by-year fixed effects. IV regressions further control for: population, poverty rate, child poverty rate, and median income from the census; the log total cars registered and the fraction of non-cheating diesel cars from IHS Markit; fraction Hispanic, black, married, smoked during pregnancy, mothers' average age, and fraction in education bins from birth certificate data. Observations at the county-month level, from 2007-2015. Standard errors clustered at the county level.

Table 7: Effect of vehicle composition on asthma visits to the emergency department

	PM 2.5 (mean)		Visits to ED per 1,000			
	(1)	(2)	(3)	(4)	(5)	(6)
	Full sample	HCUP sample	All ages	0-4	5-24	25-44
Cheating diesel per 1,000 cars	0.26***	0.64***	-0.04	0.27**	-0.01	-0.01
	(0.05)	(0.15)	(0.02)	(0.12)	(0.04)	(0.03)
'Cheating' gas per 1,000 cars	-0.02	0.01	-0.00	-0.06**	-0.01	-0.00
	(0.02)	(0.02)	(0.01)	(0.03)	(0.01)	(0.01)
FEs: county, time	Yes	Yes	Yes	Yes	Yes	Yes
Observations	55,940	5,657	6,756	6,756	6,756	6,756
R^2	0.483	0.629	0.864	0.789	0.752	0.828
Mean dep. var.	10.1	8.556	1.212	3.385	1.727	1.193
States	51	5	5	5	5	5
Counties	685	67	228	228	228	228
P-value ($\beta_1 = \beta_2$)	0.000	0.000	0.201	0.023	0.952	0.904

Notes: Column 1 repeats column 2 from Table 2A. Column 2 replicates this specification for for states included in the HCUP sample (AZ, RI, NJ, FL, KY). Controls in columns 1 and 2 are the same as in Table 2. Number of asthma emergency department visits per 1,000 people from the HCUP ED data (columns 3-6). When the dependent variable is the number of visits for a specific age range, the denominator is the number of people in the same age range from the census. Cheating diesel cars are make-model-model years exposed in the emissions cheating scandals; "cheating" gas refers to the gas versions of the cheating diesel cars, both from IHS Markit (for a complete list of cheating cars see Table A.15). "FEs: county, time" refer to county and quarter-by-year fixed effects. Additional controls are: population, population in relevant age group, poverty rate, child poverty rate, and median income from the census; the log total cars registered and the fraction of non-cheating diesel cars from IHS Markit; average fraction of ED visits that are black, Hispanic, female, covered by Medicaid, covered by private insurance, the average length of stay, and the average number of deaths from the HCUP data. Observations in columns 3-6 at the county-quarter level, from 2007-2015, weighted by county-level population in the relevant age group. Standard errors clustered at the county level.

A.1 Appendix (for online publication)

A.1.1 Additional robustness exercises

Table A.4 shows the robustness of our main results to a broad set of alternative specification choices. Columns 1 through 8 show results for birth weight, while columns 9 through 16 present results for $\mathrm{PM}_{2.5}$. Columns 1 and 9 repeat our preferred specification, for comparison.

A.1.1.1 Different car manufacturers

First, in columns 2 and 10, we estimate the effect of the cheating diesel VW and FCA cars separately. For both the pollution and birth outcomes, the effects of an additional cheating VW diesel car per thousand are consistent with our main results, and precisely measured. The effect of an additional cheating FCA car per thousand is very imprecise, which is not surprising given that these cars only entered the market in model year 2014 and our data only go through 2015.

A.1.1.2 Different types of counties

Second, we show that the effect size per car is similar in counties with higher and lower market penetration of the cheating vehicles. The regressions in columns 3–4 and 11–12 divide the sample into counties with an above or below median concentration of cheating diesel vehicles in 2015; the similarity of the point estimates across these two samples suggests that the effects of pollution from the cheating diesel cars on birth outcomes are relatively linear. Columns 5 and 13 drop states in the bottom quintile of the fraction cheating diesel cars in 2015. Columns 6 and 14 drop counties designated to be in non-attainment status with the National Ambient Air Quality Standards in 2005. TOur pollution results are also unaffected when we limit the sample to include only county-months with non-missing birth information (the overlap between the pollution monitor sample and the birth outcomes sample in Table 3).

A.1.1.3 Different exposure measures

Third, we show that our results are robust to alternative ways of defining exposure. Our preferred specification uses the fraction of the vehicle fleet that is cheating as the exposure measure. Alternatively, we could have defined the variable as the number of cheating cars per square mile. We prefer our measure, as there are many counties where only part of the county is populated and thus scaling by area would distort the exposure measure toward an artificially lower exposure in more rural areas. However, we show in columns 7–8 and 15–16 that the results are similar when we define cheating diesel by cheating cars per square mile, or cheating cars per 1,000 people.Our results are also robust to varying the number of monitoring stations included in our pollution outcome variables, though most counties have at most one monitoring station (results available on request).

A.1.1.4 Different weighting and aggregation

Although our primary specification is weighted (by the number of births for birth outcomes and by population for the number of emergency department visits), the results are very similar if we leave the regressions unweighted. Table A.5 show our main results on birth outcomes using an unweighted specification. The resulting estimates for the effect of cheating diesel cars on health outcomes are nearly identical.

Our results are also robust to different levels of aggregation. Table A.9 shows that our results are very similar if we collapse our data to either the annual or commuting zone level, while regressions using disaggregated individual-level data are shown in Table 4.

A.1.1.5 Different controls

In addition to varying the specification, Tables A.10 and A.11 show the robustness of our results to different sets of fixed effects and controls. The first column shows the raw association between the fraction of cheating diesel cars and health outcomes. For all birth outcomes, more cheating cars is associated with better birth outcomes, reflecting the fact that these cars are being purchased in generally wealthier areas (see Figure 2). This positive correlation is eliminated when county-level controls are added (column 2). When county and month-by-year fixed effects are added, we see a strong negative association emerge between the share of cheating diesel cars in a county and birth weight. In columns 3 and 4 we add county and month-by-year fixed effects, first alone, and then with the county-level controls. Although the county-level controls are not very important as long as we include county fixed effects, using within-county variation is important. Our results are also robust to including a wide range of county-month temperature and precipitation controls, and our results for ozone are stronger in the summer than the winter, which makes sense as ozone requires sunlight and heat for production (see Table A.12).[35] Our results are also robust to including various economic controls, such as unemployment rate and refinancing volume, to control for the any potential confounding effects of the subprime mortgage crisis and the great recession (results available upon request). Finally, the share of cheating diesel cars does not affect birth rates in our sample, in line with the lack of compositional impacts shown in Figure 7 (results available upon request).

A.1.2 Technical details

A.1.2.1 Air quality index

The EPA's air quality index is scaled from 0-500 and broken into levels of health-risk categories: 0-50 – Level of health concern = Good; 51-100 – Level of health concern = Moderate; 101-150 – Level of health concern = Unhealthy for sensitive groups; 151-200 – Level of concern = Unhealthy; 301-500 – Level of concern = Hazardous.

For each pollutant, "cut-points" are used to determine what the value of the AQI will be for a given measurement of that pollutant. While the scale has remained the same over time (100 is always considered the cutoff beyond which the air quality is unhealthy), the relevant cut-points

[35] Additionally, we have used a spatial first differences (SFD) specifications, a recently developed approach which analyzes differences between adjacent counties instead of deviations from country-wide trends (Druckenmiller and Hsiang, 2018). In our setting, the SFD approach has limited statistical power due to sample reductions in first differences (all counties adjacent to small counties with zero births are dropped) as well as potential treatment spillovers between counties. Despite this loss in power, the SFD estimates are similar to our baseline results (available upon request).

have changed over the time period studied in this paper. Thus, we construct an internally consistent air quality index for each pollutant using the bins given in the most recent AQI documentation and actual pollution measurements (May 2016, see Figure A.13), rather than relying on the AQI numbers published by the EPA.

The formula for converting daily pollution measures to a daily air quality index is reproduced below. First, the maximum concentration of the pollutant is determined (for particulate matter, this is also the mean), the relevant cut-points are found, and the index is calculated according to the formula:

$$AQI_p = \frac{AQI_{High} - AQI_{Low}}{BP_{High} - BP_{Low}} * (Concentration_p - BP_{Low}) + AQI_{Low} \tag{A.1}$$

with AQI_{High} = AQI for upper cut-point ;AQI_{Low} = AQI for lower cut-point ; BP_{High} = Upper cut-point concentration ; and BP_{Low} = Lower cut-point concentration.

The maximum and mean are the same for particulate matter because of the way it is measured; a filter is placed outside for the relevant time period, and then the amount of particulates collected is measured. Thus, there is just one measure per day, and the maximum is the same as the mean.

A.1.2.2 Cheating diesel cars per county per year

When we roll back the make-model-model year dataset of the stock of registered vehicles per county in 2015 to construct an annual measure, we need to consider that model years are sold both in the calendar year corresponding with the model year and in the previous year. Considering that a sizable fraction of new cars are purchased prior to the model year is important, because our goal is to correctly construct the number of cheating cars in each county and each year.

We use a secondary dataset of car sales at the county-make-model level (Experian Autocount data, from 2008-2017) to construct make-model specific measures of the share of vehicles purchased in the vehicle year (relative to the previous year) for make-models with cheating diesel cars. These shares range from 0.56 to 0.9 (available on request). For each make-model, we assign this fraction of make-models to the model year, and the remaining cars to the prior year. The

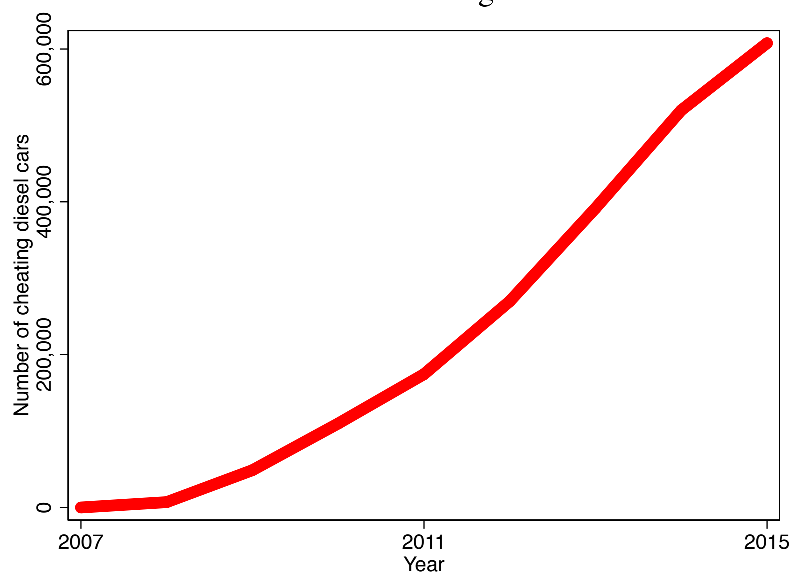
Autocount data does not allow us to separate sales of diesel and gas powered cars, so the fraction purchased in the model year is the same for the cheating diesel and "cheating" gas cars.

For the number of non-cheating diesel cars, we use a similar smoothing procedure. Again using the 2008-2017 Autocount sales data, we calculate the fraction of all vehicles purchased in the model year (0.7518) and use this share to roll back the number of diesels. As is the case with the model-specific measure, this statistic combines gas and diesel vehicles.

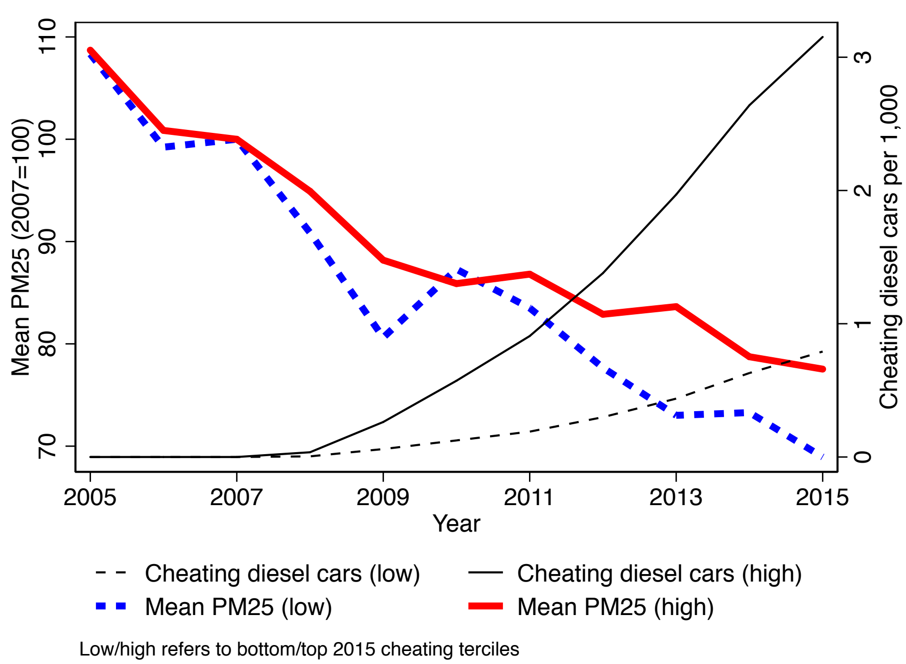
A.2 Appendix Figures

Figure A.1: Cheating diesel cars and particulate matter over time

Panel A: Stock of cheating diesel cars



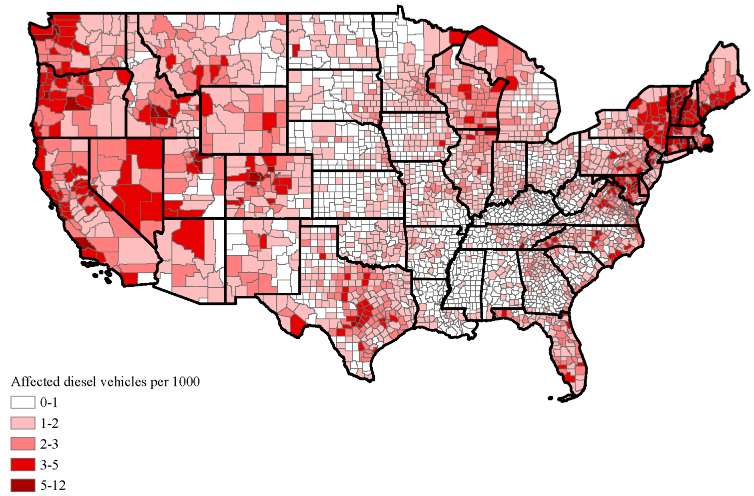
Panel B: Share cheating diesel cars and mean PM2.5



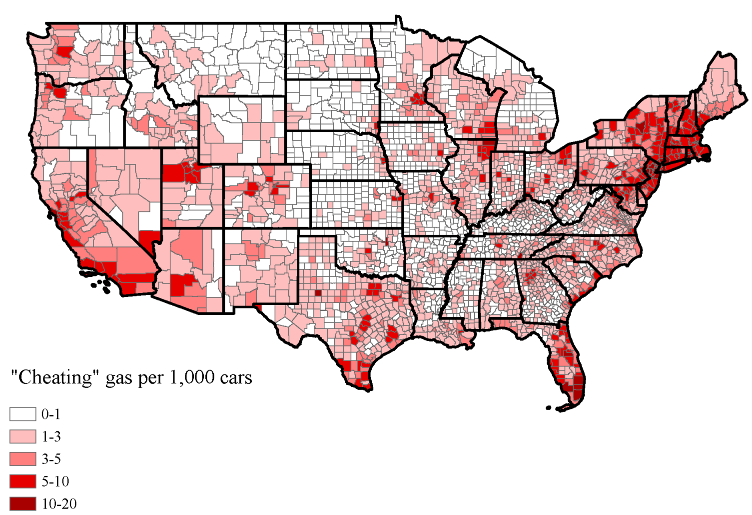
Notes: Panel A shows the total number of cheating diesel cars registered in the United States by year. In panel B, the solid lines show the mean PM 2.5 level and the average cheating diesel share in the top tercile of counties in terms of the 2015 cheating diesel share. The dashed lines show the respective averages for the bottom tercile. Cheating diesel cars are make-model-model years exposed in the emissions cheating scandals, from IHS Markit (for a complete list of cheating cars see Table A.15). Mean $\mathrm{PM}_{2.5}$ from the EPA data, normalized to the 2007 average. “Low diesel” counties are those in the bottom tercile of fraction of cheating diesel, “high diesel” counties are those in the top tercile of fraction of cheating diesel. Data at the county-year level, from 2005-2015.

Figure A.2: County-level distribution of cheating diesel and “cheating” gas cars in 2015

A: Cheating diesel cars, 2015

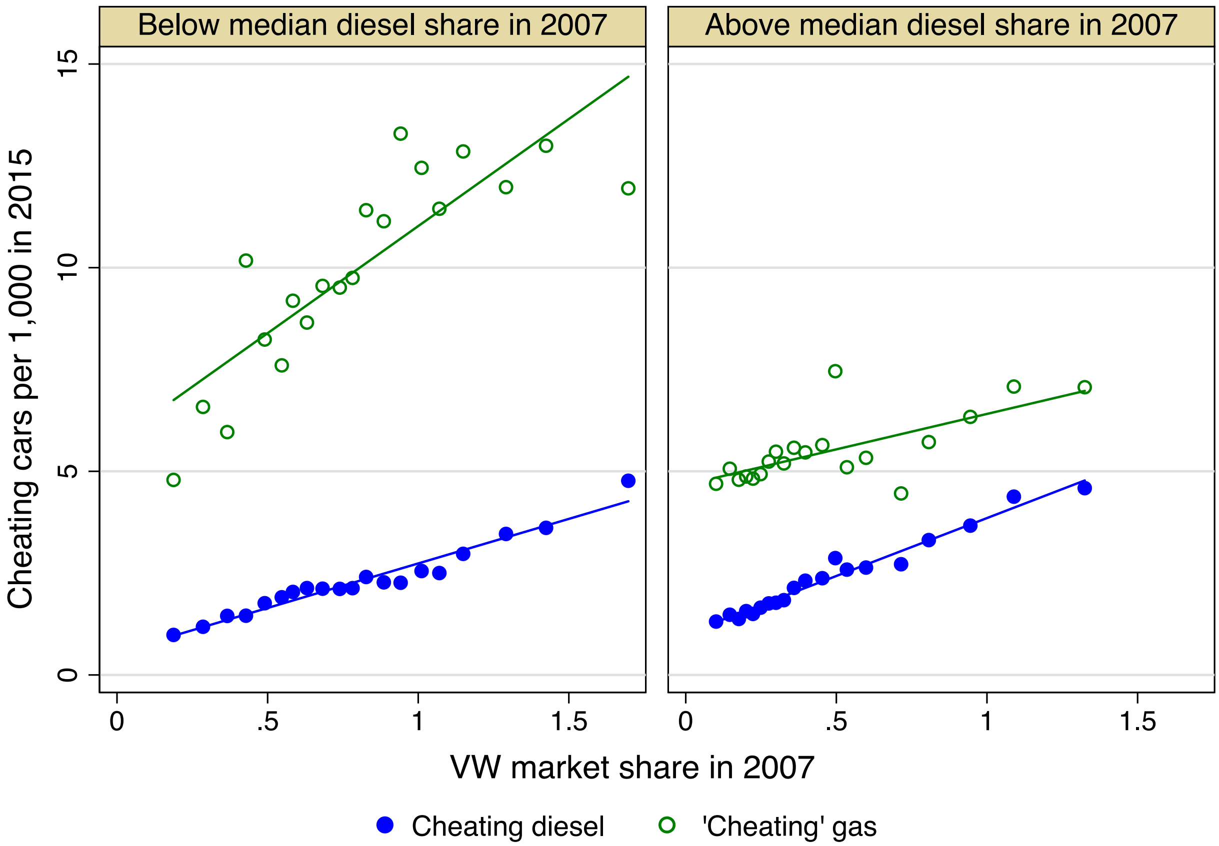


B: “Cheating” gas cars, 2015



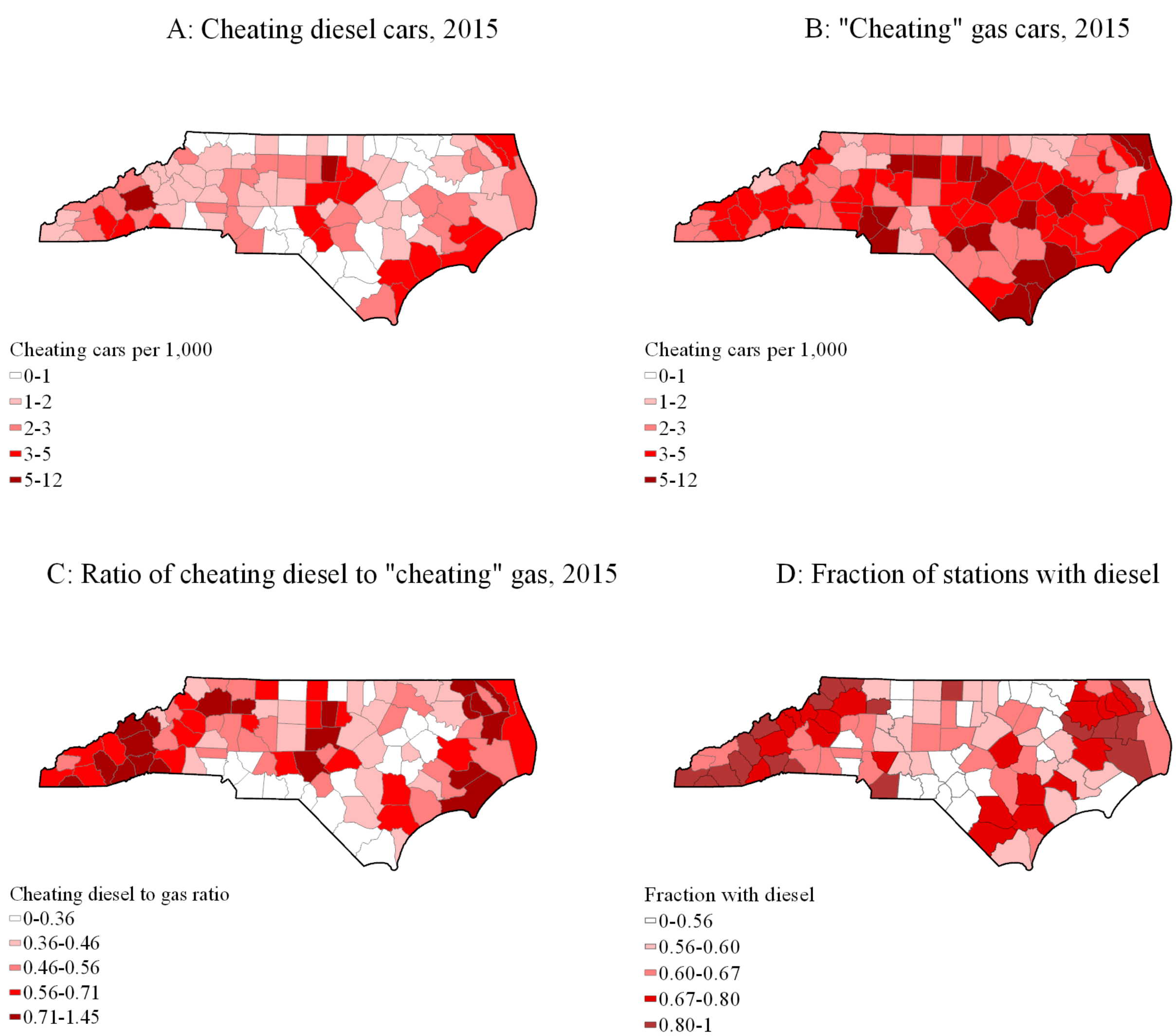
Notes: Number of cheating diesel and “cheating” gas cars per 1,000 cars and light-duty trucks registered at the county-level in 2015. Cheating diesel cars are make-model-model years exposed in the emissions cheating scandals, from IHS Markit (for a complete list of cheating cars see Table A.15). Non-cheating diesel cars are diesel cars not included in the emissions cheating scandals.

Figure A.3: Binscatter plots of 2015 cheating diesel and "cheating" gas across 2007 VW market shares, by 2007 diesel share



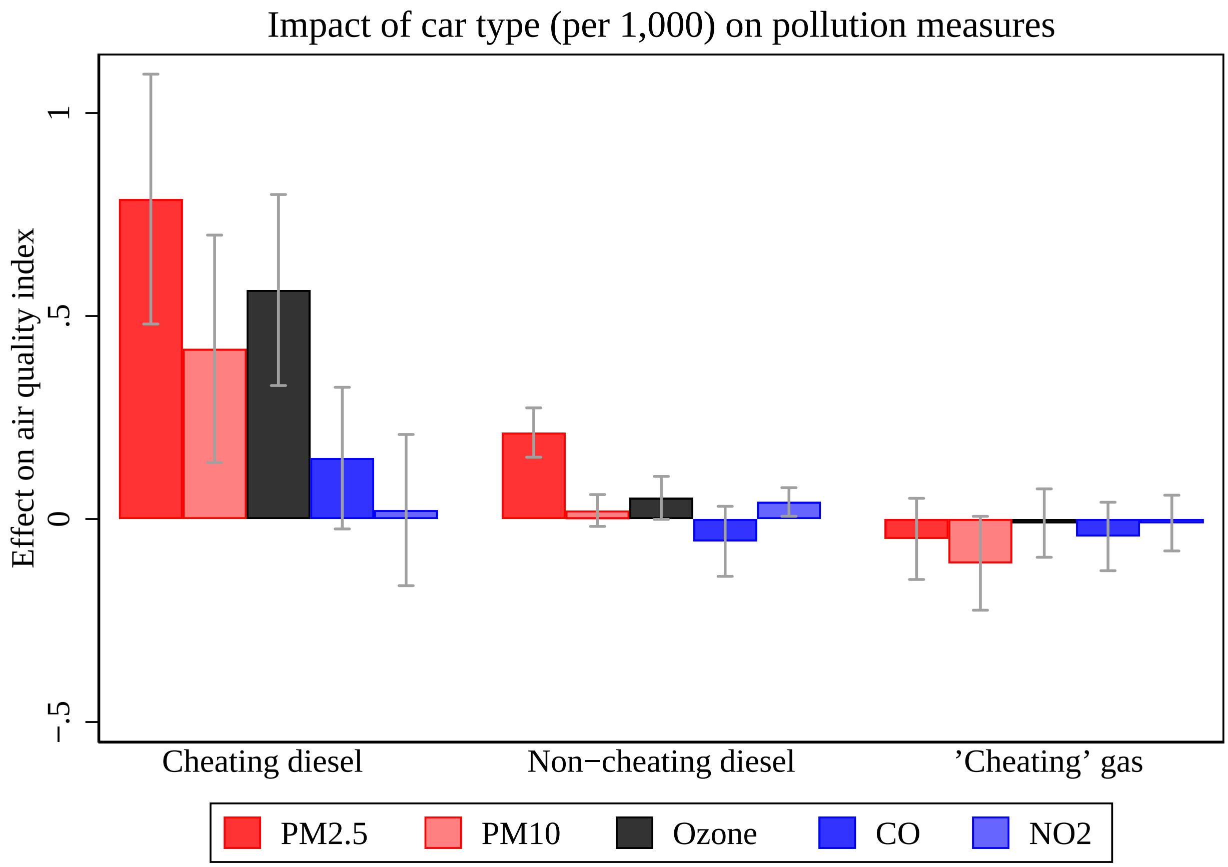
Notes: Separate Binscatter plots by Volkswagen 2007 market share, for counties below and above 2007 (non-cheating) diesel share. Cheating diesel and "cheating" gas shares increase with the 2007 VW market share but the effect is stronger in counties with low (non-cheating) diesel shares for "cheating" gas. Cheating diesel cars are make-model-model years exposed in the emissions cheating scandals; "cheating" gas refers to the gas versions of the cheating diesel cars, both from IHS Markit (for a complete list of cheating cars see Table A.15).

Figure A.4: Distribution of cheating diesel cars and diesel fuel in North Carolina



Notes: Number of cheating diesel and "cheating" gas cars per 1,000 cars and light-duty trucks registered at the county-level in 2015. Cheating diesel cars are make-model-model years exposed in the emissions cheating scandals, from IHS Markit (for a complete list of cheating cars see Table A.15). "Cheating" gas cars are the gasoline versions of the cheating make-model-years. Data on diesel fuel availability for North Carolina is provided by the North Carolina Department of Agriculture and Consumer Services, shared with GIS through the NC OneMap program, and is the fraction of gas stations with at least one diesel fuel nozzle at the county level in 2017.

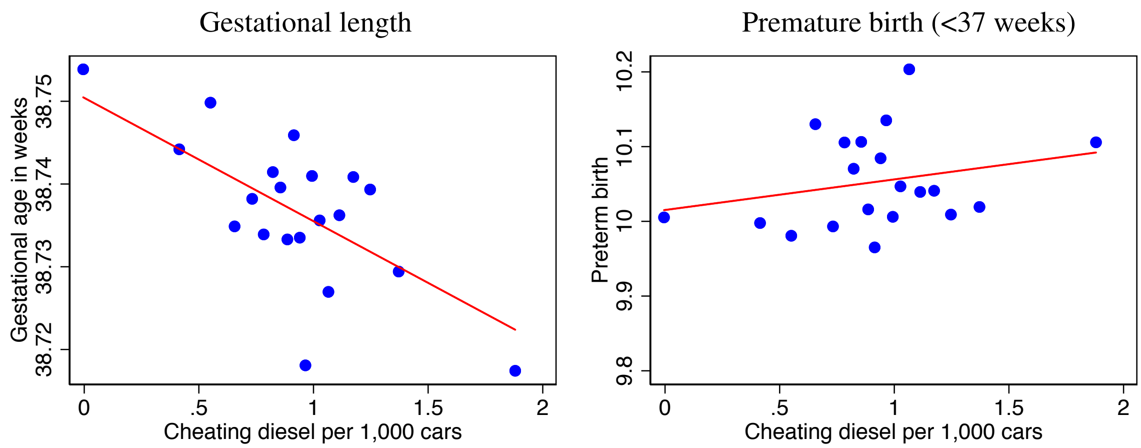
Figure A.5: Effect of vehicle composition on air quality indices for different pollutants (including non-cheating diesel)



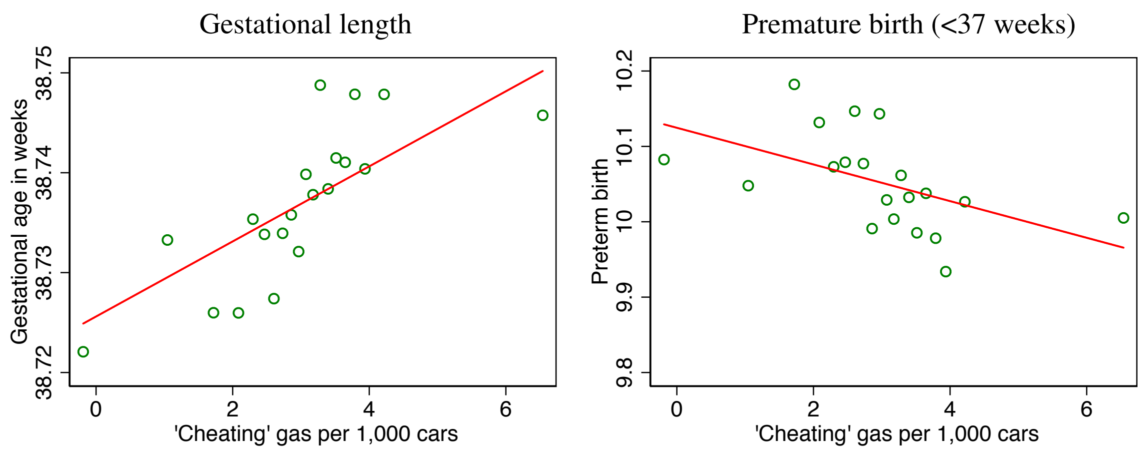
Notes: Coefficients are plotted from regressions of air quality indices on the number of cheating diesel cars per 1,000 cars and the number of "cheating" gas cars per 1,000 cars, reported in Table 2B. Separate regressions are run for each pollutant. Pollution data from the EPA; information on the construction of the air quality indices in section A.1.2. Cheating diesel cars are make-model-model years exposed in the emissions cheating scandals; "cheating" gas refers to the gas versions of the cheating diesel cars, both from IHS Markit (for a complete list of cheating cars see Table A.15). Additional controls are: population, poverty rate, child poverty rate, and median income from the census; the log total cars registered and the fraction of non-cheating diesel cars from IHS Markit. County and month-by-year fixed effects also included. Observations at the county-month level, from 2007-2015. Standard errors clustered at the county level.

Figure A.6: binned scatter plots of gestation length and premature birth rates against the share of cheating diesel and "cheating" gas, controlling for county and year fixed effects

(A) Cheating diesel

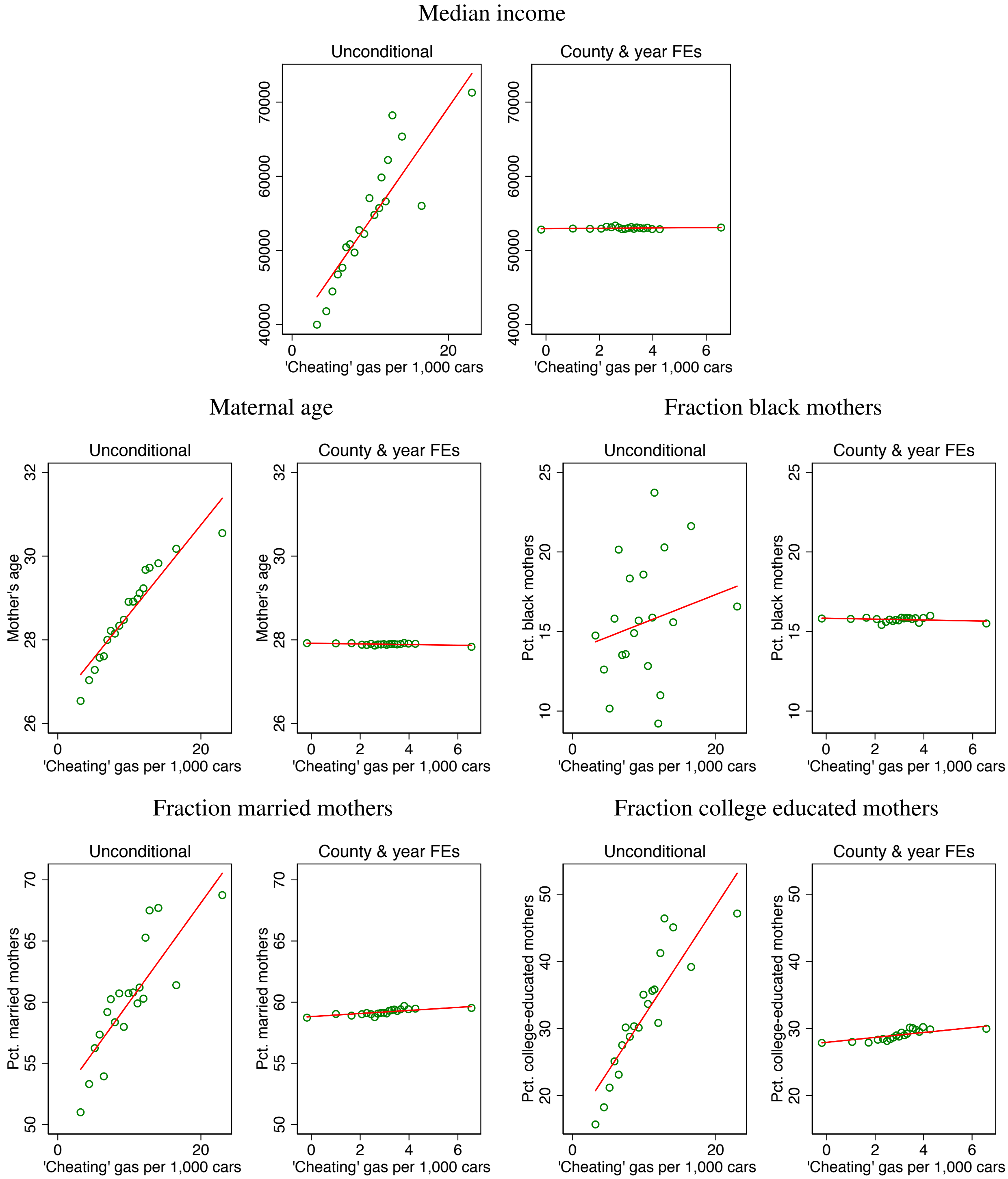


(B) "Cheating" gas



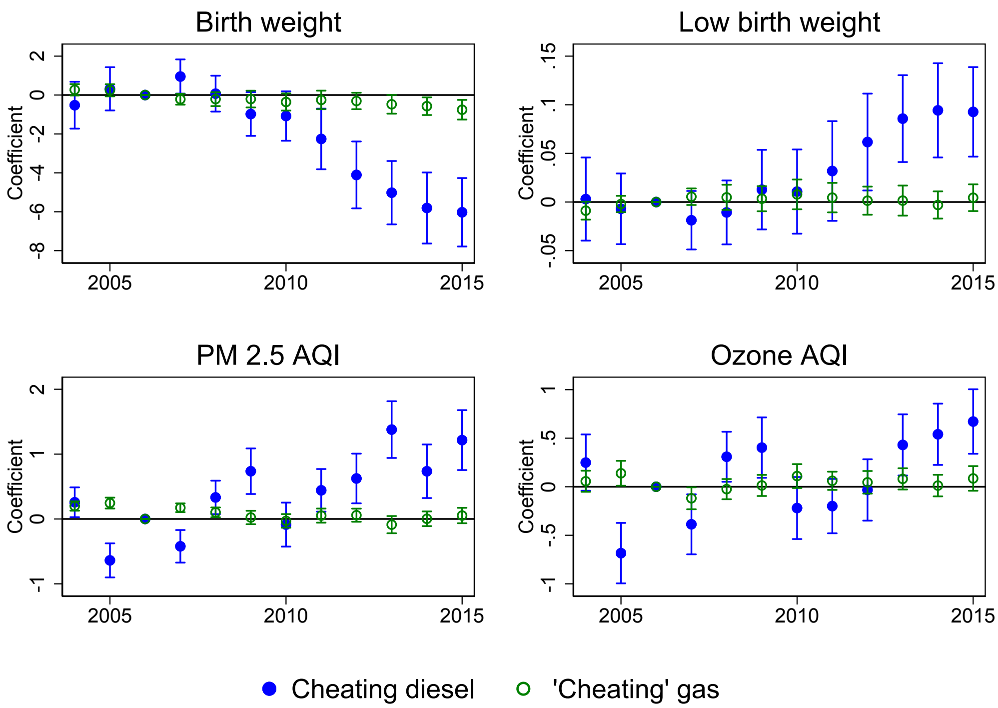
Notes: Gestation age and fraction born preterm from birth certificate data. Cheating diesel cars are make-model-model years exposed in the emissions cheating scandals; "cheating" gas refers to the gas versions of the cheating diesel cars, both from IHS Markit (for a complete list of cheating cars see Table A.15). Binned scatter plots divide counties into ventiles by the number of cheating diesel cars per 1,000 cars (panel A) and the number of "cheating" gas cars per 1,000 cars (Panel B). The average of vehicle composition in each bin is plotted on the x-axis, and the mean birth outcomes plus the residual in each bin is plotted on the y-axis, after partialling out county and year fixed effects, the log number of total cars, the number of non-cheating diesel cars per 1,000 cars, and the number of "cheating" gas cars per 1,000 cars (Panel A), and number of cheating diesel cars per 1,000 cars (Panel B). Observations at the county-year level, from 2007-2015, weighted by the number of births in the county-year.

Figure A.7: Binned scatter plots of maternal characteristics against the share of "cheating" gas



Notes: Median income from the census, maternal characteristics from birth certificate data. Cheating diesel cars are make-model-model years exposed in the emissions cheating scandals; "cheating" gas refers to the gas versions of the cheating diesel cars, both from IHS Markit (for a complete list of cheating cars see Table A.15). The left plot of each pair is a binned scatter plot with the number of "cheating" gas cars per 1,000 cars in 2015 on the x-axis, and the maternal characteristic on the y-axis. In the right plot is a binned scatter plot of the same maternal characteristic against the number of "cheating" gas per 1,000 cars, but now using the entire time period (2007-2015), and partialling out county and year fixed effects. Observations at the county-year level, weighted by the number of births in the county-year.

Figure A.8: Event study figure



Notes: Plotted are coefficients along with 95% confidence intervals on the interactions of year dummies with counties' 2015 share of cheating diesel (blue dots) and "cheating" gas (green circles). The same controls as in the baseline regressions are included. Cheating diesel cars are make-model-model years exposed in the emissions cheating scandals from IHS Markit (for a complete list of cheating cars see Table A.15). Data are at the county-year level from 2004-2015.

A.3 Appendix Tables

Table A.1: Summary statistics: HCUP sample characteristics

	All counties	PM 2.5 monitor	By frac. cheating in 2015: Tercile 1	Tercile 2	Tercile 3
Health outcomes					
Asthma ED visits per 1,000	1.21	1.25	1.00	1.48	1.07
Asthma ED visits per 1,000, ages 0-4	3.37	3.52	2.26	4.21	2.95
Asthma ED visits per 1,000, ages 5-24	1.73	1.78	1.37	2.03	1.58
Census/SAIPE					
Total population	1,055,605	1,365,429	35,781	1,061,646	1,128,399
Pct. in poverty	15.0	14.8	23.7	16.0	13.7
Pct. children in poverty	21.4	21.0	32.7	22.7	19.7
Pct. white	61.4	56.3	88.6	49.1	66.8
Median income	51,143	51,812	34,517	48,069	54,255
Vehicle characteristics					
Cheating diesel per 1,000 cars	0.84	0.83	0.23	0.64	1.01
Non cheating diesel per 1,000 cars	18.38	15.65	31.55	16.13	18.76
'Cheating' gas per 1,000 cars	3.63	3.96	0.87	3.94	3.65
Observations	8,208	1,882	2,736	2,736	2,736

Notes: Health outcomes from HCUP; county-level characteristics from the census; vehicle characteristics from IHS Markit. Cheating diesel cars are make-model-model years exposed in the emissions cheating scandals; "cheating" gas refers to the gas versions of the cheating diesel cars, both from IHS Markit (for a complete list of cheating cars see Table A.15). Observations at the county-quarter level from 2007-2015, weighted by population.

Table A.2: Maternal characteristics balancing regressions

	(1) Pct. hispanic	(2) Pct. black	(3) Pct. white	(4) Pct. married	(5) Avg. age	(6) Pct. smoked	(7) Pct. college	(8) Pct. high school	(9) Pct. < high school
Cheating diesel per 1,000 cars	-0.11	-0.05	-0.21*	0.04	0.00	0.10	0.34***	-0.09	-0.03
	(0.09)	(0.09)	(0.11)	(0.09)	(0.01)	(0.10)	(0.11)	(0.12)	(0.13)
FEs: county, time	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	191,102	191,102	191,102	191,102	191,102	191,102	191,102	191,102	191,102
R^2	0.986	0.989	0.986	0.912	0.939	0.774	0.931	0.791	0.876
Mean dep. var.	24.56	15.12	76.37	59.44	27.92	8.828	28.89	25.95	17.54

Notes: Cheating diesel cars are make-model-model years exposed in the emissions cheating scandals, from IHS Markit (for a complete list of cheating cars see Table A.15). "FEs: county, time" refer to county and month-by-year fixed effects. Additional controls (with the exception of the dependent variable) are: population, poverty rate, child poverty rate, and median income from the census; the log total cars registered, the number of "cheating" gas cars per 1,000 cars, and the number of non-cheating diesel cars per 1,000 cars IHS Markit; fraction Hispanic, black, married, smoked during pregnancy, mothers' average age, and fraction in education bins from birth certificate data. Observations at the county-month level, from 2007-2015, weighted by number of births. Standard errors clustered at the county level.

Table A.3: Effect of vehicle composition on pollution by distance

	PM 2.5 AQI			PM 2.5 concentration		
	(1)	(2)	(3)	(4)	(5)	(6)
Cheating diesel per 1,000 cars	0.80***	0.94***	0.77*	0.27***	0.32***	0.23*
	(0.16)	(0.28)	(0.46)	(0.05)	(0.09)	(0.14)
Cheating diesel per 1,000 cars: 1-20 mi			0.41			0.10
			(0.54)			(0.15)
Cheating diesel per 1,000 cars: 20-40 mi			-0.01			0.04
			(0.50)			(0.16)
Cheating diesel per 1,000 cars: 40-60 mi			-0.06			0.05
			(0.51)			(0.16)
'Cheating' gas per 1,000 cars	-0.02	0.07	0.06	-0.01	0.02	0.02
	(0.05)	(0.07)	(0.12)	(0.02)	(0.02)	(0.04)
'Cheating' gas per 1,000 cars: 1-20 mi			-0.04			0.01
			(0.14)			(0.04)
'Cheating' gas per 1,000 cars: 20-40 mi			-0.07			-0.03
			(0.14)			(0.04)
'Cheating' gas per 1,000 cars: 40-60 mi			0.26*			0.06
			(0.15)			(0.05)
FEs: county, time	Yes	Yes	Yes	Yes	Yes	Yes
Observations	55,940	12,602	12,602	55,940	12,602	12,602
R^2	0.531	0.625	0.628	0.482	0.611	0.614
Mean dep. var.	36.35	39.63	39.63	9.338	10.26	10.26
Monitors	824	151	151	824	151	151

Notes: Pollution data from the EPA. AQI stands for air quality index; information on the construction of the air quality indices in section A.1.2. Distance between counties calculated as the distance between county centroids using geodetic distances. The vehicle composition variables in the distance bands are constructed by dividing the total number of each car type among all counties in the distance band by the total number of cars among all counties in the distance band. Additional controls are: the log total cars registered and the fraction of non-cheating diesel cars from IHS Markit, as well as month by year fixed effects and county fixed effects. Columns 2 and 4 replicate columns 1 and 3 in the subsample with at least one neighbor with a county centroid within 20 miles. Cheating diesel cars are make-model-model years exposed in the emissions cheating scandals; "cheating" gas refers to the gas versions of the cheating diesel cars, both from IHS Markit (for a complete list of cheating cars see Table A.15). "FEs: county, time" refer to county and month-by-year fixed effects. Also included in columns 3 and 6: log(total cars), log(total cars): 1-20 miles, log(total cars): 20-40 miles, log(total cars): 40-60 miles, non-cheating diesel, non-cheating diesel: 1-20 miles, non-cheating diesel: 20-40 miles, non-cheating diesel: 40-60 miles. Observations at the county-month level, from 2007-2015. Standard errors clustered at the county level.

Table A.4: Alternative specifications

	Birth weight								PM 2.5							
	(1) Main result	(2) By scandal	(3) Above median	(4) Below median	(5) Drop low aff.	(6) Drop nonat.	(7) Cars/ area	(8) Cars/ pop	(9) Main result	(10) By scandal	(11) Above median	(12) Below median	(13) Drop low aff.	(14) Drop nonat.	(15) Cars/ area	(16) Cars/ pop
Cheating diesel per 1,000 cars	-6.24***		-6.20***	-9.09*	-7.00***	-8.02***			0.26***		0.26***	-0.12	0.26***	0.29***		
	(0.76)		(0.86)	(5.05)	(1.26)	(1.36)			(0.05)		(0.06)	(0.22)	(0.05)	(0.05)		
Cheating VW per 1,000 cars		-6.43***								0.30***						
		(0.81)								(0.05)						
Cheating FiatC per 1,000 cars		-0.99								-0.31						
		(4.80)								(0.24)						
'Cheating' gas per 1,000 cars	-0.37	-0.35	-0.36	-0.39	0.46	0.94			-0.02	-0.02	-0.04*	0.02	-0.00	-0.06***		
	(0.23)	(0.23)	(0.22)	(0.51)	(0.48)	(0.64)			(0.02)	(0.02)	(0.02)	(0.03)	(0.02)	(0.02)		
Cheating diesel per 10 mi^2							-15.29***								0.21	
							(4.22)								(0.22)	
'Cheating' gas per 10 mi^2							-0.57								0.02	
							(0.55)								(0.05)	
Cheating diesel per 1,000 pop								-6.78***								0.30***
								(0.86)								(0.05)
'Cheating' gas per 1,000 pop								0.12								-0.02**
								(0.22)								(0.01)
FEs: county, time	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	191,082	191,082	101,489	87,277	156,414	171,999	191,082	191,082	55,940	55,940	34,759	20,051	46,678	41,483	55,940	55,940
R^2	0.734	0.734	0.756	0.665	0.350	0.344	0.733	0.733	0.483	0.483	0.460	0.560	0.468	0.433	0.480	0.482
Mean dep. var.	3,302	3,302	3,314	3,272	3,348	3,345	3,302	3,302	9.338	9.338	8.929	10.08	9.34	8.672	9.338	9.338
P-value ($\beta_1 = \beta_2$)	0.000	0.000	0.000	0.099	0.000	0.000	0.001	0.000	0.000	0.000	0.000	0.555	0.000	0.000	0.428	0.000

Notes: Birth weight from birth certificate data; $\mathrm{PM}_{2.5}$ from the EPA data. Above and below median refers to the county-level fraction of cheating diesel cars in 2015. The "Drop low aff." specification drops states in the bottom quintile of fraction cheating diesel cars in 2015; "Drop nonat." drops counties designated to be in non-attainment status with the National Ambient Air Quality Standards in 2005. Cheating diesel cars are make-model-model years exposed in the emissions cheating scandals; "cheating" gas refers to the gas versions of the cheating diesel cars, both from IHS Markit (for a complete list of cheating cars see Table A.15). Cheating VW cars are cheating diesel cars from the Volkswagen emissions cheating scandal; cheating FiatC are are cheating diesel cars from the Fiat-Chrysler emissions cheating scandal. "FEs: county, time" refer to county and month-by-year fixed effects. Additional controls are: population, poverty rate, child poverty rate, and median income from the census; the log total cars registered and the fraction of non-cheating diesel cars from IHS Markit; fraction Hispanic, black, married, smoked during pregnancy, mothers' average age, and fraction in education bins from birth certificate data (controls from birth data only included in columns 1-8). Observations at the county-month level, from 2007-2015. Columns 1-8 weighted by number of births. Standard errors clustered at the county level.

Table A.5: Effect of vehicle composition on birth outcomes (unweighted regressions)

Panel A: Birth weight

	Birth weight (in grams)		Low birth weight ($< 2,500$ grams)	
	(1)	(2)	(3)	(4)
Cheating diesel per 1,000 cars	-6.757***	-7.339***	0.158***	0.178***
	(1.158)	(1.243)	(0.042)	(0.044)
'Cheating' gas per 1,000 cars		0.349		-0.014
		(0.470)		(0.016)
FEs: county, time	Yes	Yes	Yes	Yes
Observations	191,082	191,082	191,082	191,082
R^2	0.347	0.347	0.222	0.222
Mean dep. var.	3,342	3,342	5.199	5.199
P-value ($\beta_1 = \beta_2$)		0.000		0.000

Panel B: Gestational age

	Gestational age (in weeks)		Preterm birth (< 37 weeks)	
	(1)	(2)	(3)	(4)
Cheating diesel per 1,000 cars	-0.009	-0.012*	0.086	0.063
	(0.006)	(0.007)	(0.064)	(0.067)
'Cheating' gas per 1,000 cars		0.003		0.028
		(0.002)		(0.023)
FEs: county, time	Yes	Yes	Yes	Yes
Observations	191,102	191,102	191,102	191,102
R^2	0.235	0.235	0.209	0.209
Mean dep. var.	38.92	38.92	8.854	8.854
P-value ($\beta_1 = \beta_2$)		0.046		0.652

Notes: Birth weight, fraction low birth weight, gestation age in weeks, and fraction born premature from birth certificate data. Cheating diesel cars are make-model-model years exposed in the emissions cheating scandals; "cheating" gas refers to the gas versions of the cheating diesel cars, both from IHS Markit (for a complete list of cheating cars see Table A.15). "FEs: county, time" refer to county and month-by-year fixed effects. Additional controls are: population, poverty rate, child poverty rate, and median income from the census; the log total cars registered and the fraction of non-cheating diesel cars from IHS Markit; fraction Hispanic, black, married, smoked during pregnancy, mothers' average age, and fraction in education bins from birth certificate data. Observations at the county-month level, from 2007-2015. Standard errors clustered at the county level.

Table A.6: Effect of vehicle composition on asthma visits to the emergency department (unweighted regressions)

	PM 2.5 (mean)		Visits to ED per 1,000			
	(1)	(2)	(3)	(4)	(5)	(6)
	Full sample	HCUP sample	All ages	0-4	5-24	25-44
Cheating diesel per 1,000 cars	0.26***	0.64***	0.03	0.28***	0.08**	0.03
	(0.05)	(0.15)	(0.02)	(0.11)	(0.04)	(0.03)
'Cheating' gas per 1,000 cars	-0.02	0.01	-0.01	-0.05	-0.03***	-0.00
	(0.02)	(0.02)	(0.01)	(0.03)	(0.01)	(0.01)
FEs: county, time	Yes	Yes	Yes	Yes	Yes	Yes
Observations	55,940	5,657	6,756	6,756	6,756	6,756
R^2	0.483	0.629	0.682	0.420	0.527	0.500
Mean dep. var.	9.338	8.804	1.026	2.628	1.447	1.097
States	51	5	5	5	5	5
Counties	685	67	228	228	228	228
P-value ($\beta_1 = \beta_2$)	0.000	0.000	0.183	0.012	0.022	0.375

Notes: Column 1 repeats column 2 from Table 2A. Column 2 replicates this specification for for states included in the HCUP sample (AZ, RI, NJ, FL, KY). Controls in columns 1 and 2 are the same as in Table 2. Number of asthma emergency department visits per 1,000 people from the HCUP ED data (columns 3-6). When the dependent variable is the number of visits for a specific age range, the denominator is the number of people in the same age range from the census. Cheating diesel cars are make-model-model years exposed in the emissions cheating scandals; "cheating" gas refers to the gas versions of the cheating diesel cars, both from IHS Markit (for a complete list of cheating cars see Table A.15). "FEs: county, time" refer to county and quarter-by-year fixed effects. Additional controls are: population, population in relevant age group, poverty rate, child poverty rate, and median income from the census; the log total cars registered and the fraction of non-cheating diesel cars from IHS Markit; average fraction of ED visits that are black, hispanic, female, covered by Medicaid, covered by private insurance, the average length of stay, and the average number of deaths from the HCUP data. Observations in columns 3-6 at the county-quarter level, from 2007-2015. Standard errors clustered at the county level.

Table A.7: OLS and IV regressions of birth weight on PM 2.5 ($\mu g/m^3$) across different samples and measures

Dependent variable: Birth weight (in grams)

	Cross-sectional OLS				Instrumental variable			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Baseline	Small counties	Baseline	Small counties	Baseline	Small counties	Baseline	Small counties
Mean PM 2.5 (monitor)	-2.558***	-4.897***			-36.37***	-50.20**		
	(0.593)	(1.018)			(8.153)	(24.00)		
Mean PM 2.5 (satellite)			-5.581***	-8.217***			-30.92***	-30.08***
			(0.549)	(0.802)			(5.687)	(11.51)
FEs: time	Yes	Yes	Yes	Yes	No	No	No	No
FEs: county, time	No	No	No	No	Yes	Yes	Yes	Yes
First stage F-stat					33.9	5.98	186	51.7
Mean dep. var.	3,314	3,310	3,338	3,344	3,316	3,312	3,341	3,349
Observations	51,760	25,728	207,852	104,726	46,265	22,496	189,711	93,384

Notes: Birth weight from birth certificate data; mean PM 2.5 monitor data from the EPA data; mean PM 2.5 satellite data from van Donkelaar et al. (2019). Columns 2, 4, 6, 7 restrict the sample to counties smaller than the median county area. Cross-sectional OLS refers to regressions of birth weight on the pollution measure and month-by-year fixed effects. Instrumental variables (IV) refers to IV regressions of birth outcomes on pollution with the cheating diesel share as instrument. "FEs: county, time" indicates inclusion of county and month-by-year fixed effects. IV regressions further control for: population, poverty rate, child poverty rate, and median income from the census; the log total cars registered and the fraction of non-cheating diesel cars from IHS Markit; fraction Hispanic, black, married, smoked during pregnancy, mothers' average age, and fraction in education bins from birth certificate data. Observations at the county-month level, from 2007-2015. Standard errors clustered at the county level.

Table A.8: OLS and IV regressions of birth weight on PM 2.5 AQI and ozone AQI

Panel A: Birth weight (in grams)

	Cross-sectional OLS			Instrumental variable		
	(1)	(2)	(3)	(4)	(5)	(6)
PM 2.5 (AQI)	-1.172***	-1.081***	-1.002***	-9.507***	-9.647***	-12.32***
	(0.150)	(0.244)	(0.239)	(1.775)	(2.203)	(3.522)
Ozone (AQI)			-0.907***			4.927
			(0.192)			(3.119)
Ozone sample	No	Yes	Yes	No	Yes	Yes
FEs: time	Yes	Yes	Yes	No	No	No
FEs: county, time	No	No	No	Yes	Yes	Yes
First stage F-stat				153	55.1	9.43
Mean dep. var.	3,341	3,324	3,324	3,341	3,324	3,324
Mean PM 2.5	33.6	34.7	34.7	33.6	34.7	34.7
Mean ozone	40.2	40.2	40.2	40.2	40.2	40.2
Observations	189,711	50,714	50,714	189,711	50,714	50,714

Panel B: Low birth weight (<2,500g)

	Cross-sectional OLS			Instrumental variable		
	(1)	(2)	(3)	(4)	(5)	(6)
PM 2.5 (AQI)	0.0493***	0.0443***	0.0443***	0.215***	0.218***	0.277**
	(0.00338)	(0.00562)	(0.00569)	(0.0594)	(0.0748)	(0.108)
Ozone (AQI)			-0.0000445			-0.0958
			(0.00471)			(0.0831)
Ozone sample	No	Yes	Yes	No	Yes	Yes
FEs: time	Yes	Yes	Yes	No	No	No
FEs: county, time	No	No	No	Yes	Yes	Yes
First stage F-stat				153	55.1	9.43
Mean dep. var.	5.2	5.74	5.74	5.2	5.74	5.74
Mean PM 2.5	33.6	34.7	34.7	33.6	34.7	34.7
Mean ozone	40.2	40.2	40.2	40.2	40.2	40.2
Observations	189,711	50,714	50,714	189,711	50,714	50,714

Notes: Birth weight and fraction low birth weight from birth certificate data; mean PM 2.5 from van Donkelaar et al. (2019) satellite data; mean ozone from the EPA data. Cross-sectional OLS refers to regressions of birth outcomes on the pollution measure and month-by-year fixed effects. Instrumental variables (IV) refers to IV regressions of birth outcomes on pollution with the cheating diesel share as instrument. Column 6 includes the interaction of the cheating diesel share with the maximum monthly temperature as an additional instrument. "FEs: county, time" indicates inclusion of county and month-by-year fixed effects. IV regressions further control for: population, poverty rate, child poverty rate, and median income from the census; the log total cars registered and the fraction of non-cheating diesel cars from IHS Markit; fraction Hispanic, black, married, smoked during pregnancy, mothers' average age, and fraction in education bins from birth certificate data. Observations at the county-month level, from 2007-2015. Standard errors clustered at the county level.

Table A.9: Robustness: Alternative levels of aggregation

Panel A: Annual Data

	(1) Birth weight	(2) Low birth weight	(3) Gestational age	(4) Preterm birth
Cheating diesel per 1,000 cars	-6.06***	0.12***	-0.02***	0.07
	(0.83)	(0.02)	(0.00)	(0.04)
'Cheating' gas per 1,000 cars	-0.46*	-0.00	0.00**	-0.02
	(0.28)	(0.01)	(0.00)	(0.01)
FEs: county, time	Yes	Yes	Yes	Yes
Observations	21,469	21,469	21,472	21,472
R^2	0.944	0.907	0.876	0.864
Mean dep. var.	3,302	6.316	38.74	9.979
P-value ($\beta_1 = \beta_2$)	0.000	0.000	0.000	0.069

Panel B: Commuting Zone Level Data

	(1) Birth weight	(2) Low birth weight	(3) Gestational age	(4) Preterm birth
Cheating diesel per 1,000 cars	-7.82***	0.13***	-0.02***	0.07
	(0.96)	(0.03)	(0.01)	(0.06)
'Cheating' gas per 1,000 cars	-0.43	0.00	0.00	-0.00
	(0.29)	(0.01)	(0.00)	(0.02)
FEs: cz, time	Yes	Yes	Yes	Yes
Observations	58,959	58,959	58,959	58,959
R^2	0.839	0.646	0.737	0.629
Mean dep. var.	3,302	6.313	38.74	9.977
P-value ($\beta_1 = \beta_2$)	0.000	0.000	0.001	0.207

Notes: Birth weight, fraction low birth weight, gestation age in weeks, and fraction preterm from birth certificate data. Cheating diesel cars are make-model-model years exposed in the emissions cheating scandals; "cheating" gas refers to the gas versions of the cheating diesel cars, both from IHS Markit (for a complete list of cheating cars see Table A.15). "FEs: county, time" refer to county and year fixed effects. Additional controls are: population, poverty rate, child poverty rate, and median income from the census; the log total cars registered and the fraction of non-cheating diesel cars from IHS Markit; fraction Hispanic, black, married, smoked during pregnancy, mothers' average age, and fraction in education bins from birth certificate data. Observations in Panel A at the county-year level, observations in Panel B at the commuting zone-month level, from 2007-2015, weighted by number of births. Standard errors clustered at the county or commuting zone level. 2010 commuting zone definitions from Fowler et al. (2018) used in Panel B.

Table A.10: Effect of fraction diesel on birth weight, using different sets of controls

Panel A: Birth weight

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Cheating diesel per 1,000 cars	28.321***	0.311	-6.820***	-6.739***	29.914***	2.191	-6.326***	-6.236***	-5.678***
	(3.517)	(2.641)	(0.721)	(0.704)	(3.334)	(2.700)	(0.811)	(0.759)	(0.745)
'Cheating' gas per 1,000 cars					-1.577*	-1.934***	-0.369	-0.370	-0.445*
					(0.837)	(0.422)	(0.233)	(0.230)	(0.237)
County covars	No	Yes	No	Yes	No	Yes	No	Yes	Yes
FEs: county, time	No	No	Yes	Yes	No	No	Yes	Yes	Yes
Extended controls	No	No	No	No	No	No	No	No	Yes
Observations	209,511	191,082	209,511	191,082	209,511	191,082	209,511	191,082	165,564
R^2	0.061	0.437	0.721	0.733	0.094	0.439	0.721	0.734	0.734
Mean dep. var.	3,300	3,302	3,300	3,302	3,300	3,302	3,300	3,302	3,302
P-value ($\beta_1 = \beta_2$)					0.000	0.148	0.000	0.000	0.000

Panel B: Low birth weight

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Cheating diesel per 1,000 cars	-0.581***	0.078	0.105***	0.116***	-0.492***	0.112	0.117***	0.121***	0.104***
	(0.092)	(0.065)	(0.021)	(0.020)	(0.086)	(0.069)	(0.023)	(0.022)	(0.023)
'Cheating' gas per 1,000 cars					-0.073***	-0.035***	-0.011*	-0.006	-0.003
					(0.020)	(0.013)	(0.006)	(0.007)	(0.007)
County covars	No	Yes	No	Yes	No	Yes	No	Yes	Yes
FEs: county, time	No	No	Yes	Yes	No	No	Yes	Yes	Yes
Extended controls	No	No	No	No	No	No	No	No	Yes
Observations	209,511	191,082	209,511	191,082	209,511	191,082	209,511	191,082	165,564
R^2	0.026	0.334	0.551	0.559	0.090	0.334	0.551	0.559	0.560
Mean dep. var.	6.35	6.314	6.35	6.314	6.35	6.314	6.35	6.314	6.31
P-value ($\beta_1 = \beta_2$)					0.000	0.047	0.000	0.000	0.000

Notes: Birth weight and fraction low birth weight from birth certificate data. Cheating diesel cars are make-model-model years exposed in the emissions cheating scandals; "cheating" gas refers to the gas versions of the cheating diesel cars, both from IHS Markit (for a complete list of cheating cars see Table A.15). "FEs: county, time" refer to county and month-by-year fixed effects. County-level covariates are: population, poverty rate, child poverty rate, and median income from the census; the log total cars registered and the fraction of non-cheating diesel cars from IHS Markit; fraction Hispanic, black, married, smoked during pregnancy, mothers' average age, and fraction in education bins from birth certificate data. Additional controls in column 9 are county-level unemployment rates and the ratio of new cars from Experian autocount data (available from 2009-2015) to total registered cars. Observations at the county-month level, from 2007-2015, weighted by number of births. Standard errors clustered at the county level.

Table A.11: Effect of fraction diesel on gestational age, using different sets of controls

Panel A: Gestational age

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Cheating diesel per 1,000 cars	0.113***	0.040***	-0.011**	-0.012***	0.114***	0.043***	-0.015***	-0.016***	-0.015***
	(0.012)	(0.010)	(0.004)	(0.004)	(0.013)	(0.011)	(0.005)	(0.004)	(0.004)
'Cheating' gas per 1,000 cars					-0.002	-0.001	0.004***	0.004***	0.003**
					(0.003)	(0.002)	(0.001)	(0.001)	(0.001)
County covars	No	Yes	No	Yes	No	Yes	No	Yes	Yes
FEs: county, time	No	No	Yes	Yes	No	No	Yes	Yes	Yes
Extended controls	No	No	No	No	No	No	No	No	Yes
Observations	209,535	191,102	209,535	191,102	209,535	191,102	209,535	191,102	165,584
R^2	0.065	0.286	0.614	0.616	0.090	0.287	0.614	0.617	0.620
Mean dep. var.	38.74	38.74	38.74	38.74	38.74	38.74	38.74	38.74	38.75
P-value ($\beta_1 = \beta_2$)					0.000	0.000	0.000	0.000	0.000

Panel B: Preterm births

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Cheating diesel per 1,000 cars	-1.134***	-0.188**	0.011	0.055	-1.080***	-0.202**	0.041	0.071*	0.043
	(0.123)	(0.095)	(0.047)	(0.043)	(0.138)	(0.103)	(0.045)	(0.041)	(0.040)
'Cheating' gas per 1,000 cars					-0.043	0.023	-0.026*	-0.015	-0.011
					(0.040)	(0.029)	(0.014)	(0.014)	(0.012)
County covars	No	Yes	No	Yes	No	Yes	No	Yes	Yes
FEs: county, time	No	No	Yes	Yes	No	No	Yes	Yes	Yes
Extended controls	No	No	No	No	No	No	No	No	Yes
Observations	209,535	191,102	209,535	191,102	209,535	191,102	209,535	191,102	165,584
R^2	0.074	0.309	0.526	0.534	0.097	0.309	0.526	0.534	0.538
Mean dep. var.	10.05	9.975	10.05	9.975	10.05	9.975	10.05	9.975	9.882
P-value ($\beta_1 = \beta_2$)					0.000	0.057	0.168	0.056	0.225

Notes: Gestation age in weeks and fraction born premature from birth certificate data. Cheating diesel cars are make-model-model years exposed in the emissions cheating scandals; "cheating" gas refers to the gas versions of the cheating diesel cars, both from IHS Markit (for a complete list of cheating cars see Table A.15). "FEs: county, time" refer to county and month-by-year fixed effects. County-level covariates are: population, poverty rate, child poverty rate, and median income from the census; the log total cars registered and the fraction of non-cheating diesel cars from IHS Markit; fraction Hispanic, black, married, smoked during pregnancy, mothers' average age, and fraction in education bins from birth certificate data. Additional controls in column 9 are county-level unemployment rates and the ratio of new cars from Experian autocount data (available from 2009-2015) to total registered cars. Observations at the county-month level, from 2007-2015, weighted by number of births. Standard errors clustered at the county level.

Table A.12: Robustness to weather controls

	Main results				Including weather controls				By season	
	(1) PM 2.5 AQI	(2) Ozone AQI	(3) Birth weight	(4) Low birth weight	(5) PM 2.5 AQI	(6) Ozone AQI	(7) Birth weight	(8) Low birth weight	(9) Ozone: winter	(10) Ozone: summer
Cheating diesel per 1,000 cars	0.79***	0.58***	-6.19***	0.12***	0.75***	0.40***	-6.19***	0.12***	-0.13	0.80***
	(0.16)	(0.12)	(0.76)	(0.02)	(0.14)	(0.11)	(0.76)	(0.02)	(0.12)	(0.23)
'Cheating' gas per 1,000 cars	-0.03	-0.01	-0.37	-0.01	-0.03	-0.05	-0.37	-0.01	-0.08**	-0.12
	(0.05)	(0.04)	(0.23)	(0.01)	(0.05)	(0.04)	(0.23)	(0.01)	(0.04)	(0.08)
FEs: county, time	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Weather controls	No	No	No	No	Yes	Yes	Yes	Yes	No	No
Weighted by population	No	No	Yes	Yes	No	No	Yes	Yes	No	No
Observations	55,120	63,903	189,540	189,540	55,120	63,903	189,540	189,540	10,323	19,241
R^2	0.533	0.613	0.733	0.560	0.583	0.679	0.733	0.560	0.708	0.698
Mean dep. var.	36.53	40.35	3,302	6.317	36.53	40.35	3,302	6.317	29.02	47.22
P-value ($\beta_1 = \beta_2$)	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.677	0.001

Notes: Cheating diesel cars are make-model-model years exposed in the emissions cheating scandals; "cheating" gas refers to the gas versions of the cheating diesel cars, both from IHS Markit (for a complete list of cheating cars see Table A.15). "FEs: county, time" refer to county and month-by-year fixed effects. "Weather controls" include information on temperature and precipitation from the PRISM climate group at Oregon State; these statistics are at the county-month level and are calculated from daily data. Weather data covers all US counties except those in Hawaii and Alaska; columns 1-4 replicate our main results in this subsample. Weather controls included in columns 5-8 are precipitation (maximum, mean, minimum, standard deviation), maximum temperature (maximum, mean, minimum, standard devation), and mean temperature (maximum, mean, minimum, standard devation). Precipitation unit is millimeter, and temperature unit is Celsius. Additional controls are the same as in Table 2 for the pollution outcomes and Table 4 for the birth outcomes. Observations at the county-month level, from 2007-2015, and birth outcome regressions are weighted by number of births. Standard errors clustered at the county level.

Table A.13: Current EPA Breakpoints for the Air Quality Index (May 2016)

These Breakpoints...							...equal this AQI	...and this category
O_3 (ppm) 8-hour	O_3 (ppm) 1-hour[1]	$PM_{2.5}$ ($\mu g/m^3$) 24-hour	PM_{10} ($\mu g/m^3$) 24-hour	CO (ppm) 8-hour	SO_2 (ppb) 1-hour	NO_2 (ppb) 1-hour	AQI	
0.000 - 0.054	-	0.0 – 12.0	0 - 54	0.0 - 4.4	0 - 35	0 - 53	0 - 50	Good
0.055 - 0.070	-	12.1 – 35.4	55 - 154	4.5 - 9.4	36 - 75	54 - 100	51 - 100	Moderate
0.071 - 0.085	0.125 - 0.164	35.5 – 55.4	155 - 254	9.5 - 12.4	76 - 185	101 - 360	101 - 150	Unhealthy for Sensitive Groups
0.086 - 0.105	0.165 - 0.204	(55.5 - 150.4)[3]	255 - 354	12.5 - 15.4	(186 - 304)[4]	361 - 649	151 - 200	Unhealthy
0.106 - 0.200	0.205 - 0.404	(150.5 - (250.4)[3]	355 - 424	15.5 - 30.4	(305 - 604)[4]	650 - 1249	201 - 300	Very unhealthy
([2])	0.405 - 0.504	(250.5 - (350.4)[3]	425 - 504	30.5 - 40.4	(605 - 804)[4]	1250 - 1649	301 - 400	Hazardous
([2])	0.505 - 0.604	(350.5 - 500.4)[3]	505 - 604	40.5 - 50.4	(805 - 1004)[4]	1650 - 2049	401 - 500	Hazardous

Notes: table reproduced from (United States Environmental Protection Agency, 2016).

Table A.14: Original manufacturer's suggested retail price (MSRP) of cheating diesel models

Make/model	Model year	Gas model, original MSRP	Diesel model, original MSRP
VW Jetta	2009	17340	21990
VW Beetle	2013	19795	23295
VW Beetle Convertible	2013	24995	27895
VW Passat	2012	19995	25995
Audi A3	2010	27270	29950
VW Jetta Sportwagen	2009	18999	23590
VW Golf	2010	17620	22155
VW Golf SportWagen*	2015	22215	25415
VW Touareg	2009	39300	42800
Audi A7 Quattro	2014	64500	66900
Audi A8	2014	75100	82500
Audi Q5 Quattro	2014	37300	46500
Audi Q7	2009	43500	50900
Porsche Cayenne	2013	48850	55750
Jeep Grand Cherokee	2014	29945	34445
RAM 1500 (crew cab)	2014	24500	27300

Notes: Unless otherwise specified, original MSRP was taken from www.newcartestdrive.com/reviews; we were unable to find the original MSRP for the 2014 Audi A6 Quattro.
2015 VW Golf SportWagen: https://www.caranddriver.com/news/a15357836/2015-volkswagen-golf-sportwagen-pricing-announced-starts-at-22215/
2014 Audi A8: https://www.autotrader.com/Audi/A8/2014
2014 Audi Q5 Quattro: https://www.autotrader.com/Audi/Q5/2014
2013 Porsche Cayenne: https://www.autotrader.com/Porsche/Cayenne/2013
2014 RAM 1500: https://www.kbb.com/ram/1500-crew-cab/2014/

Table A.15: Cheating diesel make-model-model years

Panel A		Panel B		Panel C	
Make-model	Year	Make-model	Year	Make-model	Year
VW Jetta	2009-2015	VW Touareg	2014	VW Touareg	2009-2016
VW Jetta SportWagen	2009-2014	Porsche Cayenne	2015	Porsche Cayenne	2013-2016
VW Golf	2010-2015	Audi A6 Quattro	2016	Audi A6 Quattro	2014-2016
VW Golf SportWagen	2015	Audi A7 Quattro	2016	Audi A7 Quattro	2014-2016
VW Beetle	2012-2015	Audi A8	2016	Audi A8	2014-2016
VW Beetle Convertible	2012-2015	Audi A8L	2016	Audi A8L	2014-2016
VW Passat	2012-2015	Audi Q5	2016	Audi Q5	2014-2016
Audi A3	2010-2015			Audi Q7	2009/2016

Panel D	
Make-model	Year
FAC Dodge Ram 1500	2014-2016
FAC Jeep Grand Cherokee	2014-2016

September 18, 2015: EPA informs Volkswagen that it has violated the Clean Air Act by installing defeat devices on several 2009-2015 model year vehicles. The 2.0 liter diesel vehicles (and model years) that were listed in this letter as being in violation of the Clean Air Act are listed in Table, Panel A.
November 2, 2015: The EPAs investigation into Volkswagen reveals that the auto manufacturer also installed defeat devices on several model year 2014-2016 3.0 liter diesel vehicles. These vehicles are listed in Table, Panel B.
November 19, 2015: During a meeting with the EPA, Volkswagen officials reveal that the use of defeat devices in its 3.0 liter diesel vehicles extends beyond what was listed in the Clean Air Act violation the EPA issued to Volkswagen on November 2, 2015. Volkswagen admitted that all of their 3.0 liter diesel vehicles from 2009 to 2016 were implanted with defeat devices. These vehicles/model years are listed in Table, Panel C.
January 12, 2017: The EPA notified Fiat Chrysler of Clean Air Act violations. The EPA discovered that like Volkswagen Fiat Chrysler had equipped several of its 3.0 liter diesel vehicles with defeat devices. These vehicles/model years are listed in Table, Panel D.