Potential v. Actual Environmental Regulation: Evidence from Germany's Car Market

Xianru Han*

October 31, 2025

Abstract

Policymakers often announce or debate new regulations long before they are enforced, yet how such credible threats of future policy influence behavior remains understudied. This paper examines how potential v. actual regulatory pressure to reduce diesel emissions affects the new and used car markets in Germany, and consequently affects air pollution. I interpret the lawsuits initiated by an environmental organization against local governments as credible signals of potential future regulation, and exploit the staggered timing of the lawsuits in different German counties to examine their effects on car sales. I find evidence of a decline in the sales of used diesel vehicles in areas with ongoing lawsuits, and modest increases in sales of 7–12-year-old gasoline cars. In the new car market, private households increase gasoline car purchases, but diesel sales to both households and firms remain largely unchanged. These behavioral shifts coincide with a measurable reduction in ambient nitrogen dioxide concentrations. Comparing the effects of these regulatory threats with those of implemented diesel bans, I find that about two-thirds to three-quarters of the total adjustment in used-diesel markets occurs before the enforcement of diesel bans. Using data on more than 30 million used-car listings, I further show that bans reduce the resale value of high-emitting diesels by 3–5 percent and their listing volumes by roughly 20 percent.

Keywords: Regulatory threats, Legal action, Diesel bans, Used and new car markets, Urban air quality, Difference-in-differences

---

* Han: Department of Agricultural and Resource Economics, University of Maryland (email: xhan1236@umd.edu).
I am deeply grateful for the years of support and guidance from Anna Alberini, Maureen Cropper, and Colin Vance. I also thank Valeria Fanghella, Joshua Linn, Louis Preonas, Misato Sato, M.R. Sharan, Cory Smith, Nicole Wagner, Ulrich Wagner, Roberton Williams, and Jiaao Yu, as well as seminar participants at the 1st International Alpine Workshop on Energy Economics and Policy (AWEEP), the 26th CU Environmental and Resource Economics Workshop, the Institute for Humane Studies, the RWI–Leibniz Institute for Economic Research Brown Bag Seminar, the 16th Empirical Methods in Energy Economics (EMEE) Workshop, the Nordic Annual Environmental and Resource Economics (NAERE) Workshop, and the University of Maryland for valuable comments and feedback. I am grateful to RWI–Leibniz Institute for Economic Research for research support and data access, and to AutoScout24 for providing essential data resources. This work received financial support from the Institute for Humane Studies under grant nos. IHS019112 and IHS019441. Any remaining errors are my own.

1. Introduction

Air pollution is the single largest environmental health risk worldwide, responsible for millions of premature deaths each year (Brauer et al., 2024). In Europe, nitrogen dioxide (NO_2) is a particular concern: concentrations in many cities continue to exceed the EU's legal limit of 40 $\mu g/m^3$, and exposure is estimated to cause tens of thousands of premature deaths annually (EEA, 2019). Road transport is the dominant source of urban NO_2, accounting for roughly 40 percent of emissions and is the greatest source of population exposure, since emissions are released at street level, in densely populated areas (EEA, 2019). Diesel engines are the largest contributor within road transport, so reducing their emissions has become central to efforts to improve urban air quality.

Historically, vehicle emission standards in Europe have been less stringent for diesel than for gasoline vehicles (see Appendix Table A.1 for details). EURO emission standards before 2014 allowed diesel cars to emit roughly three times as many oxides of nitrogen (NO_x) per kilometer as gasoline cars. Only in 2014, with the advent of EURO 6 standards, were new diesel passenger vehicles required to limit their emissions to approximately the same level as gasoline vehicles.

Between 2010 and 2014, diesel cars accounted for 42 to 48 percent of new car registrations in Germany. It is therefore not surprising that in 2014 about half of roadside monitoring stations in Germany still recorded annual NO_2 concentrations above the EU limit of 40 $\mu g/m^3$ (AirClim, 2015).

Beginning in 2015, the environmental non-governmental organization (NGO) Deutsche Umwelthilfe (DUH) filed lawsuits against municipalities whose clean air plans failed to bring NO_2 within EU standards. These lawsuits do not simply challenge the general inadequacy of local air quality management. DUH explicitly requests that cities revise their plans to include restrictions on older diesel vehicles, arguing that such bans are necessary to meet the required air quality limits. The lawsuits therefore provide a clear and credible signal that diesel bans are likely, and they increase the probability that cities will eventually adopt such measures (Töller 2021). Beginning in 2018, several German cities, including Hamburg, Stuttgart, Darmstadt, and Berlin, banned high-emitting diesel vehicles from designated zones.

This paper examines how both these potential and actual regulations of diesel vehicles have affected the new and used car markets and air quality in Germany. I ask two research questions. First, can the threat of future regulation, or uncertainty about future regulation, affect markets and local air quality even before regulations are put in place? Second, if and when diesel bans actually start, does it affect the volume and pricing of used cars in the affected cities? If so, by how much? How does the magnitude of this effect compare with that of the threat of future regulation?

Germany is an especially revealing case because its powerful automotive industry has historically resisted stricter environmental regulations, delaying or weakening ambitious standards (Richter and Stegen, 2022). Nevertheless, several municipalities have introduced local diesel bans, making Germany an appropriate setting in which to examine whether local initiatives can reshape markets. Germany is an interesting market for three reasons. First, diesel cars have been losing market share since the Dieselgate scandal, in which Volkswagen and other German automakers were found to have manipulated emissions tests (Strittmatter & Lechner, 2019). Local bans provide a way to test whether municipal programs speed up or slow down this broader process of de-dieselization, or redirect demand toward other vehicle types. Second, because the European Union will prohibit the sales of new internal combustion engine (ICE) vehicles from 2035 onward, any future demand for ICE vehicles will increasingly have to be met in the used-car market. Local diesel bans therefore provide a useful preview, since they restrict the use of certain vehicles in urban areas and show how consumers may reallocate demand within the used-car market when some options become less attractive. Third, the staggered timing of lawsuits and local bans makes it possible to distinguish between expectations of or concerns about future, more stringent regulations and the impact of actual policy implementation.

While most studies evaluate policies only after they are implemented, this paper is among the first to identify how regulatory threats influence markets before bans take effect. Specifically, it examines how regulatory pressure shapes vehicle markets and air quality through two distinct channels: regulatory threats, defined as lawsuits that raise the perceived likelihood of future restrictions, and the implementation of local diesel bans. I exploit the quasi-experimental variation in the rollout of lawsuits and diesel bans across German counties in a difference-in-differences (DID) design.

To identify the effect of regulatory threats, I construct a treatment group comprised of counties that faced legal action but did not introduce bans during the study period, ensuring that the estimated effects capture behavioral responses to the perceived threat of regulation rather than the impact of restrictions themselves. The treatment group I construct to study the implementation channel is comprised of the German cities that actually adopted bans. I estimate how these measures affected both vehicle markets, through the value and availability of high-emitting diesels in the secondary market.

I assemble an original dataset on lawsuits and diesel bans, which I merge with data on the car market and air pollution. I compile information on legal actions and bans from local newspapers, court filings, government websites, and official statements. By tracing lawsuits over time, I classify each case by its legal outcome, whether it was implemented, mandated by a court, remained pending, or was ultimately avoided. This classification makes it possible to assess the credibility of regulatory signals. I link these records to quarterly new and used vehicle registration data from 2014 to 2019. I draw on proprietary microdata from AutoScout24, one of Europe's largest online car marketplaces, covering more than 30 million used car listings between 2019 and 2024 with information on asking price, vehicle attributes such as mileage, fuel type, and emission standard, listing date, and the location of the seller. Finally, I have high-frequency air quality monitoring data, which I use to see if the lawsuits and the proposed and actual bans have an effect on ambient concentrations of air pollutants.

I produce several key findings. First, I find that regulatory signals significantly reduce registrations of diesel cars in the used car market. Registrations decline across all emission standards, falling by 6.7 to 8.8 percent for Euro 1 to Euro 5 vehicles and by 9.4 percent even for Euro 6 models, which are formally exempt from driving bans. By contrast, registrations of 7-12 year-old gasoline cars increase modestly, with Euro 4 and Euro 5 models rising by 7.3 percent and 3.4 percent, respectively, while no significant changes appear for older or newer gasoline vehicles. Taken together, these results suggest that regulatory threats reduce diesel registrations across all emission standards while shifting some transactions toward gasoline models perceived as less exposed to regulatory risk.

I also show that the effects of regulatory signals vary systematically with their credibility, which I proxy with the final outcome of each lawsuit (assuming that the final outcome mirrors the expectations of the public). The assumption is that lawsuits that ultimately result in bans were perceived as stronger threats ex ante than those that were withdrawn, remained pending, or were mandated but not implemented. Using Synthetic Difference-in-Differences estimators for each outcome, I find that used diesel registrations decline most sharply in counties where bans are ultimately implemented, with smaller effects in places where they are not. This effect is particularly strong for Euro 1 to Euro 5 diesels, underscoring the role of perceived likelihood of regulation in shaping anticipatory market responses. In the gasoline market, I find a similar but weaker pattern: substitution is concentrated in Euro 4 models, with the largest increases in counties where bans are implemented and weaker or insignificant effects elsewhere. These results suggest that it is not only the presence of regulatory threats but also their perceived credibility that drives behavioral change, particularly among owners of vehicles viewed as vulnerable to future restrictions.

Turning to the new car market, I find clear differences by ownership type. Company registrations show no significant response to regulatory threats, with purchases of both diesel and gasoline cars essentially unchanged. Private consumers, by contrast, respond more promptly: I estimate a 2.8 percent increase in gasoline registrations, consistent with a cautious shift toward vehicles perceived as less exposed to future restrictions. These results highlight that responsiveness to regulatory pressure differs by ownership type, with private buyers more sensitive to signals while company fleets remain largely inertial.

I conclude the analysis of regulatory threats by examining environmental outcomes. Following the filing of lawsuits aimed at restricting diesel vehicles, ambient nitrogen dioxide concentrations decline by 1.4 micrograms per cubic meter, or about 4 percent of the pre-treatment mean. By contrast, changes in ozone and particulate matter are small and statistically insignificant, which is not surprising given the chemistry of ozone formation (Real et al., 2024) and the fact that nitrate aerosols account for only a fraction of total $PM_{2.5}$ mass (Gu et al., 2023).

Having established the effects of lawsuits, I next turn to the implementation of the diesel bans. Using the data on used car listings, I find that high-emitting diesel vehicles experience both lower prices and fewer listings after local bans are implemented. Asking prices decline by about 3.3 to 5.1 percent relative to unaffected vehicles, while the quarterly number of high-emitting diesel listings falls by 18 to 23 percent. Taken together, these results show that diesel bans exert targeted downward pressure on the resale value of high-emitting vehicles and reduce their availability in the secondary market.

These findings raise a central question: why do lawsuits, even before formal policies take effect, trigger such clear market responses? Lawsuits function as regulatory signals. Evidence from financial markets shows that litigation is often treated as a forward-looking indicator: Sato et al. (2024) document that climate-related lawsuits provoke negative stock market returns upon announcement or judgment, suggesting that investors incorporate such actions as signals of future regulatory risk. Survey evidence provides a parallel perspective in the vehicle market. Götz et al. (2022) report that many diesel car owners expressed concerns about mobility restrictions and depreciation risks associated with proposed bans, indicating that expectations of future regulation shaped perceptions even before bans were implemented. In Germany, lawsuits filed by environmental NGOs were widely reported in local media and accompanied by press releases, ensuring that these signals reached consumers and firms and contributed to shifting expectations about diesel restrictions.[1]

My paper contributes to two strands of literature. First, while a substantial body of research examines the effects of formal regulations on vehicle emissions and market behavior, less attention is paid to the influence of regulatory threats. Prior work has typically focused on two types of policy instruments. One set of policies seeks to reduce emissions per kilometer driven, such as vehicle exhaust standards (Jacobsen et al., 2023; Shu et al., 2025) and vehicle scrappage incentives (Li et al., 2013). A second set targets total emissions by reducing kilometers driven, for example through driving restrictions (Barahona et al., 2020; Davis, 2008; Gallego et al., 2013), fuel taxes (Li et al ., 2014). registration taxes (Cerruti et al., 2019), and import restrictions on used vehicles (Zhou, 2023). By contrast, I emphasize how lawsuits and the public communication surrounding them function as regulatory threats, and document changes in both new and used vehicle markets before formal bans take effect.

A second contribution is to the literature on bottom-up participation in environmental governance. Prior studies show that citizen complaints and appeals can raise the costs of inaction and influence regulatory enforcement (Zhang et al., 2025; Buntaine et al., 2024). My study examines a related but distinct channel: lawsuits filed by an environmental NGO in Germany. Rather than tracing how governments adjust enforcement, I study how these NGO challenges influenced outcomes in both vehicle markets and local air quality. Beyond their legal function, the lawsuits acted as regulatory threats that shifted policymakers' perceived options, raised the

[1] Lawsuit filings are observable events that are systematically publicized. For example, Deutsche Umwelthilfe (DUH) issues press releases announcing each case (see DUH, 2015 press release), and these are regularly covered in national and international outlets, such as *Clean Energy Wire*'s factsheet on diesel driving bans (Clean Energy Wire, 2018) and *Reuters* reports on the Federal Administrative Court's 2018 ruling (Reuters, 2018a, Reuters, 2018b). This public visibility makes lawsuits a consistent and measurable proxy for threats of future restrictions, in contrast to less systematic sources such as political debate or local media coverage.

political costs of inaction, and broadened debate at both local and state levels (Bothner et al., 2022). Such inaction is common in environmental regulation, since strict enforcement often entails substantial political and economic costs (Greenstone, 2002; Greenstone et al., 2012; Walker, 2013; He et al., 2020). By linking bottom-up legal pressure to observable changes in market behavior, my study complements prior work on citizen-driven oversight and shows how civic action can reshape economic activity and environmental outcomes even before formal policies are implemented.

2. Institutional Background: Diesel Bans

Beginning in 2018, several German cities, including Hamburg, Stuttgart, Darmstadt, Berlin, and later Munich, introduced diesel bans that restricted access for older, higher-emitting diesel vehicles into the city limits. These measures followed a landmark ruling by the Federal Administrative Court in February 2018, which confirmed that municipalities were authorized to ban older diesel cars in order to comply with EU limits on nitrogen dioxide (NO_2) concentrations. The bans were defined using the European "Euro" emission standards: most prohibited diesels certified under Euro 1 to Euro 4, and in some cases Euro 5, while generally exempting newer Euro 6 models.

The most important reason for considering or implementing diesel bans is that many German cities continued to exceed EU nitrogen dioxide limits, even as new diesel cars were getting cleaner. As of 2017, 65 cities recorded annual concentrations above the legal threshold of 40 µg/m³ (Umweltbundesamt, 2018). Although diesel vehicles were widely recognized as the main contributor, policymakers at both the federal and local levels were reluctant to impose diesel bans, citing potential economic costs for the automotive industry and diesel vehicle owners (ClientEarth, 2018). In this context, Deutsche Umwelthilfe (DUH), an environmental NGO, emerged as a central source of pressure. Beginning in 2015, the NGO filed lawsuits in nearly 40 cities with persistently high pollution, formally challenging inadequate clean-air plans but in practice pressing municipalities to adopt diesel restrictions as one of the few effective measures available.[2] Prior studies document that DUH litigation played an important role in shaping the adoption of bans

[2] Since a 2013 ruling by the Federal Administrative Court, recognized environmental organizations have legal standing to bring cases against noncompliance with environmental protection laws. The ruling, won by Deutsche Umwelthilfe (DUH), affirmed the right of environmental NGOs to challenge violations of air quality standards. DUH remains the only organization in Germany that systematically pursues litigation to enforce clean-air regulations (Deutsche Umwelthilfe, 2019).

across German cities (Töller, 2021) and was framed by observers as a way to enforce the "right to clean air" (ClientEarth, 2018). The lawsuits elevated air quality on local political agendas and received media coverage, making them a credible proxy for the threat of future bans.

Before any lawsuits or outright bans, diesel cars were a popular choice in the EU and in Germany, due to tax breaks for their purchase, the lower price of diesel fuel, the higher fuel efficiency of diesel engines, and their lower CO_2 emissions (Del Rosal, 2022). From 2000 to 2014, the share of diesel cars in new passenger vehicle registrations across Western Europe increased markedly, rising from 32 percent in 2000 to a peak of 56 percent in 2011, before stabilizing at around 53% by 2013–2014 (European Automobile Manufacturers Association, n.d.).[3] In Germany, the largest car market in Europe, the diesel share rose from 30.3% in 2000 to 48.1% in 2012.[4]

However, the increase in diesel vehicles had unintended consequences for local air quality as diesel engines of early Euro standard vintages emit higher levels of nitrogen dioxide (NO_2) and particulate matter (PM_{10}) compared to gasoline engines. Between 2005 and 2007, 52 large cities in Germany with populations over 100,000 exceeded the daily PM_{10} limit of $50\ \mu g/m^3$ and 54 cities exceeded the annual NO_2 limit of $40\ \mu g/m^3$. In response, the German federal states and local governments adopted Clean Air Action Plans (CAAPs) aimed at improving urban air quality. These plans typically included four main components: investment in public transportation, the construction of ring roads to divert traffic from city centers, and improvements to traffic flow, but the most prominent intervention was the introduction of Low Emission Zones (LEZs), which proved more effective than other measures in reducing local pollution (Wolff, 2014).[5]

3. Data

[3] Western Europe, as defined in the source data, includes the 15 European Union member states prior to the 2004 enlargement (EU-15), comprising Austria, Belgium, Denmark, Finland, France, Germany, Greece, Ireland, Italy, Luxembourg, the Netherlands, Portugal, Spain, Sweden, and the United Kingdom, as well as the three member countries of the European Free Trade Association (EFTA): Iceland, Norway, and Switzerland.

[4] In 2014, diesel penetration in new passenger car registrations varied substantially across Western European countries—from 73% in Ireland, 72% in Luxembourg, and 71% in Portugal, to just 27% in the Netherlands (European Automobile Manufacturers Association, n.d.).

[5] Diesel bans are more stringent than the earlier Low Emission Zones (LEZs), which Germany introduced in 2008. LEZs restrict access based on a vehicle's emission class and are enforced through a color-coded sticker system displayed on windshields (see Appendix Tables A.1–A.2 for details). Vehicles meeting Euro 4 or higher standards, covering most diesels registered after 2006, receive a green sticker and are permitted to enter all stages of LEZs. As a result, LEZs excluded only the oldest, highest-emitting vehicles, primarily diesel cars certified under Euro 1 to Euro 3, whereas diesel bans directly targeted a much larger share of the active fleet that continued to generate high NO_2 emissions.

To measure regulatory signals, I compile data on DUH lawsuits, which serve as a proxy for the threat of future diesel restrictions.[6] I link this information to quarterly new and used vehicle registration records from IHS Markit (2014–2019). To assess environmental outcomes, I gather data on air pollutant concentrations from monitoring stations. My dataset is a county-by-quarter panel, to which I add socioeconomic and demographic characteristics from the Federal Institute for Research on Building, Urban Affairs, and Spatial Development (BBSR). To study the implementation channel, I draw on proprietary microdata from AutoScout24, one of Europe's largest online car marketplaces, covering more than 30 million used car listings between 2019 and 2024. The dataset includes asking prices, vehicle characteristics (e.g., mileage, fuel type, age, and emission standard), listing dates, and seller location. Together, these sources provide a rich empirical setting that allows me to separately identify the anticipatory effects of lawsuits, which signal potential future restrictions, and the implementation effects of local bans once they are enacted.

3.1 Diesel Bans

I compile information on lawsuits and diesel bans from local newspapers, government websites, court announcements, and official statements. These sources provide city-specific details on the timing of actions related to the lawsuits and diesel restrictions. I focus on lawsuits filed by Deutsche Umwelthilfe (DUH), the only environmental NGO that systematically pursued such cases across German cities. I code the filing date of each lawsuit as the point at which regulatory pressure becomes salient locally, providing a consistent and verifiable proxy for the threat of future diesel bans.[7]

My analysis focuses on 39 cities where an environmental NGO filed lawsuits between 2014 and 2019, challenging inadequate clean air plans and in practice pressing for diesel driving bans

[6] In this paper, lawsuits are used as a proxy for regulatory pressure rather than as an object of study in their own right. Public expectations about future diesel restrictions can be shaped by many channels, including media reports and political debate, but these are difficult to track systematically across cities and over time. Lawsuits provide a consistent and verifiable measure of regulatory pressure: they are observable events that escalate policy debate, receive local media attention, and can be coded uniformly across locations.

[7] DUH is the only environmental NGO that systematically pursued lawsuits over municipal clean air plans. Between 2011 and 2019, it initiated 47 of 49 relevant cases, making it the dominant actor in this area of litigation (Töller, 2021). While the lawsuits formally targeted the adequacy of clean air plans, they frequently raised the prospect of diesel bans as one of the few effective compliance measures. This record underscores DUH's central role in shaping local clean-air policy and supports the use of filing dates as a credible proxy for the threat of future restrictions.

as a compliance measure.[8] I aggregate city-level lawsuit information to county level to match the rest of my data. In total, the 39 cities in my sample are located within 38 distinct counties.[9]

Figure 1 maps the 39 German cities. In these cases, lawsuits initiated by the environmental NGO between 2014 and 2019 occurred before the adoption of diesel bans. Circles indicate cities with lawsuits only, triangles represent cities with both lawsuits and subsequent bans, and stars mark cities that introduced bans without prior lawsuits. The cities are geographically dispersed across Germany, with some clustering in urbanized and industrial areas such as North Rhine–Westphalia and Baden-Württemberg, where air quality problems are most acute. Most cities in the sample are large urban centers, with 38 out of 39 having populations exceeding 100,000, reflecting the concentration of regulatory pressure in densely populated areas. Notably, Hamburg is excluded from the sample, since a different environmental organization filed its two cases, which do not meet the inclusion criteria of my empirical design.[10]

Figure 2 illustrates the timing of lawsuits filed in 39 German cities. The first major wave occurred in the fourth quarter of 2015, when lawsuits were initiated in 11 cities, followed by a second cluster of 11 filings in the first quarter of 2018. The final legal outcomes varied across cities: some courts ordered the implementation of diesel restrictions, while others approved settlements or dismissed the cases after negotiated adjustments. By the end of 2019, 39 cities had been sued. This staggered timing generates the identifying variation that I exploit in the difference-in-differences design.

3.2 IHS Markit Data

Data on new and used passenger car registrations in Germany are obtained from IHS Markit. The dataset reports the number of registrations for each passenger car make-model at the county-by-

---

[8] While Töller (2022) documents 47 DUH-led lawsuits filed between 2011 and 2019, my dataset restricts attention to post-2013 cases, after ENGOs had clearly established legal standing to challenge municipal air quality plans under federal administrative law. I further limit the sample to cases where DUH's legal demands directly or indirectly targeted diesel traffic restrictions, thereby excluding earlier or more general challenges to pollution control plans.

[9] One county, home to both Ludwigsburg and Marbach, contains two treated cities. As both cities had lawsuits initiated on the same date (March 29, 2018), I treat the entire county as entering the treatment period in that quarter.

[10] Unlike other cities, two lawsuits in Hamburg were filed by a different environmental organization, Bund für Umwelt und Naturschutz Deutschland (BUND). The first, initiated on April 5, 2013, addressed general NO_2 pollution on Max-Brauer-Allee and predates the 2013 legal ruling that granted ENGOs standing to sue over air quality violations. The second was filed on July 23, 2018, after the city had already implemented a partial diesel ban on May 31, 2018. I exclude both cases because my design focuses on DUH-led litigation filed after 2013 that either preceded or plausibly triggered local diesel driving restrictions.

quarter level from 2014 to 2019. Each vehicle is defined by a unique combination of attributes, including make and model, body type, fuel type (diesel, gasoline, hybrid, plug-in hybrid, or electric), engine size, horsepower, drivetrain (front-, rear-, or all-wheel drive), gross vehicle weight, year of first registration, age, and seller type (private or dealer). For new car registrations, the dataset distinguishes between private and company ownership, whereas this distinction is not available for used cars. In the used car data, "year of first registration" refers to the year the vehicle was originally registered as new, so the age of each vehicle is easily calculated. The used-car registration data do not include information on vehicle prices, emissions, or fuel economy.

I restrict the used-car registrations to models that are representative of the active passenger car market, excluding models whose production ended before 1995, limited editions, high-performance luxury cars, racing and rally vehicles, pickups, trucks, and vans. At the same time, I retain vehicles up to 30 years old. .

Figure A.1 shows the annual number of new and used passenger car registrations in Germany between 2014 and 2019. New car sales increase steadily over this period, while used car registrations remain roughly stable at about 6.8 million per year, compared to 3.3 million new cars. Figure A.2 shows that the composition of the used car market changes little: gasoline and diesel vehicles continue to account for nearly all registrations. In contrast, Figure A.3 reveals a more pronounced shift in the new car market. The diesel share falls from about 50 percent in 2015 to 30 percent in 2019, offset first by an increase in gasoline cars and later by the rise of hybrid and electric vehicles. Overall, these figures indicate that the transition toward cleaner technologies begins in the new car market but is not yet evident in the used car market.

Figure A.5 compares used car registration trends in counties facing the threat of future diesel bans with those that did not face such legal pressure. To account for staggered timing, I align each county's data relative to the quarter in which the first lawsuit was filed. The vertical axis shows average registrations per quarter, while the horizontal axis indicates event time in quarters relative to this regulatory shock. Figure A.6 presents a similar comparison for new car registrations, disaggregated by fuel type and ownership type. In general, counties facing lawsuit pressure show substantially higher levels of both used and new car registrations throughout the sample period. The patterns across fuel types, ownership types, and emission standards appear mixed, and it is difficult to isolate causal effects without controlling for county characteristics and other confounding factors. I examine these relationships more formally in the next section.

3.3 Used Car Sale Listings

My AutoScout24 dataset covers all listings posted on the platform in Germany between 2019 and 2024, comprising approximately 26.6 million listings in total, including 6.9 million in 2019 alone. Each observation is a vehicle listing posted by either a private individual or a dealer, and includes the asking price, detailed attributes such as make, model, year of first registration, mileage, fuel type, and emission category, and the seller's zip code and listing date.

This dataset complements the registration data by capturing a later stage of market adjustment. The registration data from 2014 to 2019 allow me to examine responses to potential regulation, while the listing data from 2019 to 2024 capture outcomes after the enforcement of diesel bans. They provide detailed information on prices and availability in the secondary market. Because listings begin only in 2019, they mainly reflect the period after bans were implemented, starting with Hamburg in mid-2018, followed by Stuttgart, Darmstadt, and Berlin in 2019, and Munich in 2023. The listing data therefore offer rich post-ban coverage but limited information on pre-ban dynamics.

To provide context on the platform, Figure A.4 compares quarterly used car listings from AutoScout24 with used car registrations from IHS Markit. Although the two datasets overlap only in 2019, the quarterly series display very similar seasonality. Registrations remain relatively stable in the pre-pandemic period, averaging 6.6 million per year between 2014 and 2019, whereas listings exhibit much stronger fluctuations. In 2019, annual listings amounted to about 6.1 million, declined to 4.9 million in 2020, and then stabilized at lower levels of 3.2 to 3.7 million per year between 2022 and 2024. Over the full period from 2019 through 2024, AutoScout24 contains approximately 26.6 million listings, which averages to about 4.4 million listings per year.

3.4 Air Pollution Data

I obtain ambient concentrations of air pollution data from the German Environment Agency (UBA). The dataset provides daily average concentrations of key pollutants, including nitrogen dioxide, ozone, and particulate matter, from 659 stations across Germany. These stations are located in 272 counties. I aggregate station readings to the county–quarter level to match the panel used for vehicle registrations. While 128 counties lack monitoring stations, every county facing threats of diesel bans contains at least one station. Figure A.7 compares the air pollution in counties facing threats of diesel bans with those not subject to such pressures. On average, counties exposed to threats of diesel bans show higher concentrations of nitrogen dioxide and particulate matter, as well as higher overall Air Quality Index (AQI) values, while ozone concentrations are lower compared to other counties.

3.5 County Characteristics

My sample includes data for 38 counties where environmental lawsuits were filed seeking diesel restrictions, as well as 360 counties without such actions during the study period. These two groups of counties differ systematically in several baseline characteristics, including pollution levels and socioeconomic factors prior to the lawsuits. Counties facing legal pressure tend to be more urbanized, with higher population density and lower car density. To ensure comparability, I control for a range of county-level characteristics in all regressions, drawing on data from the Federal Institute for Research on Building, Urban Affairs and Spatial Development (BBSR). These variables include population density, car density, GDP per capita, unemployment rate, the density of students in higher education, and the proportion of residents aged 65 and over. Figure 3 shows the spatial distribution of these characteristics across Germany in 2013, well before any proposed or actual diesel bans.

4. Research design

4.1 Basic Research Design

I interpret two conceptually distinct interventions as channels of regulatory pressure: lawsuits initiated by environmental NGOs, which I treat as threats of future policy, and the subsequent implementation of formal diesel driving bans.

My analysis is based on a difference-in-differences design. To isolate the signaling channel, I exclude from the analysis the four counties where diesel bans had already been implemented by 2019: Berlin, Hamburg, Stuttgart, and Darmstadt. Counties without lawsuits are candidate members of the control group. Because counties with and without lawsuits may differ systematically, I construct alternate control groups, including buffer and doughnut samples that exclude counties geographically close to treated ones, a large-city sample restricting attention to urban centers, and a matched-sample design. I also explore alternate constructions of the control group that exclude locations with preexisting policies, such as low emission zones (LEZs) or Clean Air Action Plans (CAAPs). The unit of analysis is the county-by-quarter, and treatment is defined by the filing of lawsuits across counties. I examine three outcomes: registrations of used cars, registrations of new cars, and local air quality.

The second part of the analysis focuses on the actual policy intervention. Treatment is defined as the introduction of local restrictions on older diesel vehicles, while other cities remain unrestricted. Because my outcomes of interest are the asking prices and listing volumes of used cars, the unit of analysis is the individual vehicle listing on the AutoScout24 platform. I compare changes in the prices and volumes of non-compliant diesel listings before and after local bans relative to unaffected vehicles and unaffected regions.

4.2 Empirical Strategy: Lawsuits as a Signal of Future Policy

To estimate the effect of lawsuits filed by the environmental NGO on county-level outcomes, I begin with a two-way fixed effects (TWFE) specification:

$$Y_{tk} = \beta \times Lawsuit_{tk} + \alpha_k + \delta_t + \varepsilon_{tk} \qquad (1)$$

where Y_{tk} is the outcome in quarter by year t, for county k. Outcomes include (i) registrations of used vehicles, (ii) registrations of new vehicles, and (iii) local air pollution levels. $Lawsuit_{tk}$ is an indicator equal to one if a county k has experienced its first lawsuit by quarter-year t, demanding compliance with air quality standards through diesel driving restrictions. The terms α_k and δ_t are county and quarter-year fixed effects, respectively. County fixed effects account for unobserved time-invariant differences across counties, and quarter-year fixed effects net out time shocks and trends common to all counties. Identification comes from within-county changes over time, comparing outcomes before and after the filing of lawsuits. The coefficient of interest, β, provides the DID estimate of how the threats of future policy, captured by lawsuits, affected used car registrations, new car registrations, or local air pollution. This research design relies on the identifying assumption that the timing of lawsuits is uncorrelated with unobserved, time-varying factors that influence the outcomes of interest. In other words, in the absence of lawsuits, outcomes in counties that experience lawsuits and those that do not would have followed parallel trends.

The expected sign of β may vary by outcome. It can be positive if the proposed or anticipated bans encourage consumers to purchase new vehicles, such as newer diesel or gasoline cars, in response to expected restrictions. It can be negative if owners of older diesel vehicles sell them or postpone purchases due to uncertainty about future bans. For air quality, a negative coefficient would indicate an improvement in local conditions following reduced diesel use. Standard errors are clustered at the county level.

Although TWFE models are widely used in staggered adoption research designs, recent studies have highlighted that the "within" estimator yields consistent estimates only under strong assumptions, namely that the treatment effects are homogeneous across groups and over time (Borusyak, Jaravel, and Spiess 2024; Callaway and Sant'Anna 2021; De Chaisemartin and d'Haultfoeuille 2020; Goodman-Bacon 2021; Sun and Abraham 2021).

As lawsuits seeking diesel driving restrictions were filed at different times across Germany, it is not clear if their effects are constant over time. Nor can I assume that the effect of such legal action is identical across counties, since the lawsuits are not randomly assigned. In this context, I turn to the Callaway and Sant'Anna (2021) difference-in-differences estimator (CS-DD), which allows me to estimate and flexibly aggregate group-time ATTs across multiple treatment groups and time periods (Callaway and Sant'Anna, 2021). When I use "never treated" units as the comparison group, I have that

$$ATT(g,t) = \mathbb{E}\left[Y_t - Y_{g-1} \mid G_g = 1\right] - \mathbb{E}\left[Y_t - Y_{g-1} \mid C = 1\right] \tag{2}$$

where $ATT(g,t)$ is the group-by-time-specific ATT for counties treated in the group g at the time quarter-year t. A group g is formed according to the time in which a county first faced a lawsuit filed by the environmental NGO. G_g denotes whether the county belongs to the group treated for the first time in the period g, and C is a dummy indicating whether a county has never been treated.

Next, I explore how the treatment effects vary with the length of exposure to the treatment. To do so, I get the average treatment effect for exactly e periods after treatment, β^e, which is:

$$\beta^e = \sum_{g \in G} 1\{g + e \leq T\} 1\{\mathrm{t} - \mathrm{g} = e\} ATT(g, g+e) P(G = g \mid G + e \leq T) \tag{3}$$

where $P(G = g \mid G + e \leq T)$ is the probability of being first treated in period g conditional on being observed e periods after the treatment.

Finally, I aggregate the group time average treatment effects into an overall effect of participating in the treatment, according to the following expression:

$$\beta = \frac{1}{\kappa} \sum_{g \in G} \sum_{t=1}^{T} 1\{t \geq g\}\, ATT(g,t) P(G = g \mid G \leq T) \tag{4}$$

where $\kappa = \sum_{g \in G} \sum_{t=1}^{T} 1\{t \geq g\} P(G = g | G \leq T)$. β is a weighted average of each $ATT(g,t)$, which places more weight on $ATT(g,t)$s with larger group sizes.

4.3 Empirical Strategy: Diesel Bans as Local Restrictions

To study the impact of diesel driving bans on the used car market, I estimate models of both listing prices and listing volumes. The unit of observation is the individual vehicle listing drawn from the AutoScout24 platform

For asking prices, I estimate regressions of the form:

$$\begin{aligned} \log(price_{ikmt}) &= \beta_0 Ban_{kt} + \beta_1 (Ban_{kt} \times AffectedDiesel_i) + \beta_2 CarAge_i \\ &\quad + \beta_3 Diesel_i + \beta_4 (Diesel_i \times CarAge_i) + \beta_5 MileagePerAge_i \\ &\quad + \gamma X_i + FEs + \epsilon_{ikmt} \end{aligned} \tag{5}$$

where $price_{ikmt}$ denotes the asking price posted for a used car i of make-model m, listed in city k, at time t. I then construct two variables to capture the rollout of diesel bans. The first variable Ban_{kt} is a time-varying indicator equal to 1 if a diesel driving ban is in effect in the city k at time t, and zero otherwise. The timing is defined by the start and (where relevant) end dates of local bans.[11] The second variable, $AffectedDiesel_i$, identifies the subset of diesel vehicles i directly restricted under these bans.[12] In all cities, Euro 6 diesels remain exempt. The interaction term $Ban_{kt} \times AffectedDiesel_i$ captures the price response of affected diesel vehicles to the implementation of local bans.

I further include vehicle age ($CarAge_i$) and allow its effect to vary for diesel and gasoline vehicles through the interaction term $Diesel_i \times CarAge_i$, where $Diesel_i$ is an indicator for diesel vehicles. I control for vehicle usage intensity through $MileagePerAge_i$, defined as odometer mileage divided by age in years. Finally, X_i is a vector of additional vehicle-level attributes. The regression includes a rich set of fixed effects (FEs), including make-model, time, and location interactions, depending on the specification. The identifying variation thus comes from within-

[11] Hamburg introduced restrictions on May 31, 2018, which remained in effect until September 13, 2023. Stuttgart implemented a ban starting January 1, 2019, which remains in effect throughout the sample period. Berlin introduced restrictions on November 1, 2019, which were lifted on September 1, 2022. Darmstadt imposed restrictions starting June 1, 2019, and these remain in place. Munich began restricting certain diesels on February 1, 2023, and the ban continues thereafter.

[12] In Hamburg, Berlin, and Darmstadt, bans apply to diesels certified under Euro 1 to Euro 5 throughout the restriction period. In Stuttgart, Euro 1 to Euro 4 diesels are restricted beginning in 2019, with Euro 5 added in 2020. In Munich, Euro 1 to Euro 4 diesels are restricted from February 2023, and Euro 5 vehicles are added to the ban from April 2024 onward.

make-model comparisons over time, across locations with and without bans, controlling flexibly for vehicle characteristics and unobserved heterogeneity through detailed fixed effects.

To analyze the effect of diesel bans on the number of listings, I estimate Poisson pseudo–maximum likelihood (PPML) regressions following Silva and Tenreyro (2006, 2011). PPML produces consistent estimates under correct specification of the conditional mean $E[y_i] = e^{x_i\beta}$, remains robust to heteroskedasticity, and does not require the dependent variable to follow a Poisson or any specific distribution. Formally, the conditional expectation of the number of listings is specified as:

$$\begin{aligned} E[listing_{ikmt} \mid \cdot\,] \\ &= \exp\,(\beta_0 Ban_{kt} + \beta_1(Ban_{kt} \times AffectedDiesel_i) + \beta_2 CarAge_i \\ &+\ \beta_3 Diesel_i + \beta_4(Diesel_i \times CarAge_i) + FEs) \end{aligned} \tag{6}$$

Where the specification mirrors the price regression in equation (5), except that mileage is not included as a control. $listing_{ikmt}$ denotes the number of used car listings for vehicle i of make-model m, listed in city k, at quarter t. The interaction term $Ban_{kt} \times AffectedDiesel_i$ captures the effects of diesel bans on the likelihood of observing restricted diesel listings relative to unaffected vehicles. I estimate specifications with increasingly saturated fixed effects and cluster the standard errors at the city level. The coefficient β_1 can be interpreted as a semi-elasticity: the percentage change in the number of restricted diesel listings associated with the implementation of a ban.

5. Results

5.1 Effects of Lawsuits on Used Diesel Car Registrations

Table 1 shows the CS-DD estimates of the impacts of NGO lawsuits on used diesel car registrations. Panel A uses counties that never experience a lawsuit during the study period as the comparison group. Panel B instead uses counties that eventually face a lawsuit, but had not yet been treated at a given point in time (“not-yet treated” units). The results are nearly identical across the two specifications, confirming the robustness of my findings. Column (1) pools Euro 1 to 3 vehicles, which represent the oldest and most polluting diesels, and shows a 6.7 percent decline in registrations following a lawsuit. Column (2) shows that Euro 4 vehicles, often explicitly targeted by proposed restrictions, experience a larger decline of 8.8 percent. Column (3) shows that Euro 5 vehicles, which face partial or phased restrictions depending on location, decline by 7.5 percent.

Column (4) shows that Euro 6 vehicles, although exempt from bans during 2014 to 2019, decline by 9.4 percent.

The decline in Euro 6 registrations highlights the role of litigation as a forward-looking regulatory signal that shapes expectations in the secondary diesel market. Although Euro 6 vehicles are not restricted during the study period, buyers may anticipate that these models will be the next to face bans if lawsuits against earlier standards prove successful. Taken together, the results show that lawsuits reduce registrations across all emission standards, with effects of comparable magnitude in the range of 7 to 9 percent. The Euro 6 effect underscores how litigation transmits regulatory risk beyond the vehicles formally covered by restrictions. As a benchmark, I then estimate a standard TWFE-DD specification (Appendix Table A.3). The results indicate declines in diesel registrations across all emission standards, with coefficients in the range of -0.08 to -0.14. However, the TWFE approach relies on strong assumptions about treatment effect homogeneity. The Goodman-Bacon decomposition (Figure A.8) shows that nearly all 2×2 comparisons yield negative estimates, except for the later-treated versus earlier-treated comparison, which produces a positive coefficient because the earlier-treated group had already adjusted before serving as a control. This highlights anticipatory responses to lawsuits and motivates the use of estimators that better accommodate staggered adoption.

I next verify that the main results for used car registrations are robust to using estimators that make weaker assumptions about parallel trends. Specifically, I implement the Synthetic Difference-in-Differences (SDID) approach of Arkhangelsky et al. (2021), which constructs a synthetic counterfactual that more closely matches the pre-treatment trajectory of treated counties. The method places greater weight on control counties that share similar pre-lawsuit trends in vehicle registrations and on time periods that are most comparable to the treated periods. By aligning pre-trends in a more flexible and "local" manner, the SDID estimator mitigates bias from differential trends that can affect standard two-way fixed-effects models. As shown in Table A.4, the estimated effects on used diesel car registrations remain similar in magnitude and significance, suggesting that the results are not sensitive to the choice of estimator.

My results are robust to a wide range of alternative control group definitions. Figure A.9 illustrates the spatial composition of the main and alternative samples used in my analysis. The Full Sample, used in the primary specification, includes all counties except Hamburg (see footnote 9 for details). To address potential identification concerns, I consider eight alternative control group definitions. The Buffer Sample excludes control counties located within 25 kilometers of treated areas to mitigate potential spillover effects, since consumer responses to litigation may extend beyond county borders. The Doughnut Sample refines this by restricting controls to counties located 25 to 75 kilometers from treated areas, thereby preserving geographic comparability while avoiding immediate neighbors that may be indirectly affected. The Large City Sample restricts the control group to counties with populations exceeding 100,000, ensuring that controls are more similar in urban character to the treated counties where lawsuits occur. The Matching Sample selects counties using coarsened exact matching on pre-treatment characteristics such as population density and GDP per capita, constructing a control group that is comparable to treated counties in terms of baseline socioeconomic conditions. Finally, I construct policy-conditioned samples that either require or exclude the presence of low-emission zones (LEZ) and clean air action plans (CAAP) to account for potential policy overlap. This set of maps clarifies how treatment and control areas are defined under each sample, corresponding to the estimates reported in Figure A.9.

As shown in Figure 4, across nearly all specifications the estimated effects of lawsuits on used diesel registrations stay consistently negative, which confirms the robustness of my findings to alternative control group definitions. The only exception comes from the LEZ-based controls: in these specifications, the estimates for Euro 3 and lower and for Euro 5 do not show statistically significant reductions. This outcome reflects the fact that most counties with lawsuits also operate low-emission zones, so comparing them to other LEZ counties weakens the contrast between treated and control groups. Moreover, the LEZ sample contains the fewest observations, consists mainly of urban counties, and lies geographically close to the treated regions, which raises concerns about comparability, sample size, and potential spillovers. Taken together, these limitations explain the weaker results in the LEZ specifications, while the broader pattern shows that lawsuits impose an additional impact on top of pre-existing LEZs, consistent with litigation acting as a regulatory signal that further discourages diesel registrations.

While Table 1 reports the average effect of NGO lawsuits over the full sample period, it does not show how these effects evolve over time. To examine the dynamics of the treatment effect and assess the validity of the parallel trends assumption, I present event study plots of used diesel car registrations by Euro standard. Figure 5 illustrates the dynamic effects of legal action on quarterly used diesel vehicle registrations for the filing of the lawsuit in each treated county, and the y-axis shows the estimated change in log registrations. The coefficients on pre-treatment quarters, based on the Callaway and Sant'Anna (2021) estimator, are close to zero and show no discernible pre-trends, supporting the parallel trends assumption and indicating that the timing of legal action is plausibly exogenous conditional on the included fixed effects. In contrast, the TWFE estimates for Euro 3 and Euro 4 vehicles exhibit a downward trend prior to treatment. Following the initiation of legal action, I observe a persistent decline in the registration of used diesel vehicles, as shown in Figure 5. For Euro 1–3, Euro 4, and Euro 5 diesel vehicles, the estimated effects become increasingly negative over time, reaching declines of roughly 30 percent after twelve quarters.

This pattern suggests a cumulative behavioral response as consumers anticipate increased regulatory scrutiny for older diesel models. Surprisingly, Euro 6 vehicles, which were formally exempt from diesel driving bans during the study period, also exhibit a statistically significant decline in registrations of about 20 percent, beginning around nine quarters after treatment. A plausible explanation is that litigation created broader policy uncertainty and reputational concerns in the secondary diesel market, leading buyers to anticipate that even newer diesel technologies might eventually face restrictions.

5.2 Effects of Lawsuits on Used Gasoline Car Registrations

Having documented persistent declines in used diesel registrations following lawsuits, I next examine the gasoline market to assess potential substitution. Table 2 reports CS-DD estimates of the impact of NGO legal action on used gasoline registrations by emission standard, with Panel A using never-treated counties as the control group and Panel B using not-yet-treated counties. I find no statistically significant changes for the oldest (Euro 1–3) and newest (Euro 6) gasoline vehicles. In contrast, registrations of Euro 4 and Euro 5 gasoline vehicles rise by 7.3 percent and 3.4 percent, respectively, both statistically significant. These increases likely reflect a shift in the secondary market toward middle-aged gasoline vehicles, which are not targeted by litigation and may be perceived as safer alternatives under evolving regulatory expectations. While I cannot directly observe substitution, the pattern is consistent with a shift in registrations away from diesel toward gasoline vehicles, not directly affected by lawsuits. The results are consistent across Panels A and B. Figure 6 further illustrates these findings, showing statistically significant increases in Euro 4 and Euro 5 gasoline registrations across all specifications, with no significant effects for other emission classes.

Figure 7 presents event-study estimates of the dynamic response of used gasoline registrations to lawsuits. Among gasoline vehicles, Euro 4 models show a statistically significant and steadily increasing rise in registrations after the filing of lawsuits. The effect strengthens over time, reaching more than a 15 percent increase by the end of the post-treatment window, consistent with a persistent and cumulative response. Euro 5 vehicles display a more modest and transitory pattern: registrations increase significantly in the middle quarters but gradually taper off and become statistically insignificant in later periods. For Euro 1–3 and Euro 6 gasoline vehicles, the estimated effects remain small and insignificant throughout. These dynamic patterns align with the baseline estimates and are consistent with substitution away from diesel, most notably toward middle-aged gasoline vehicles not directly targeted by litigation.

My findings on litigation-based regulatory pressure contribute to and extend the literature on how environmental interventions shape vehicle fleets. My study extends this literature in three important ways. First, I move beyond fleet composition to provide evidence on transactions in the secondary market. Second, I show that lawsuits significantly depress registrations of Euro 6 diesels, even though they were exempt from bans, which highlights the forward-looking role of litigation as a regulatory signal. Third, I document larger and more persistent increases in Euro 4 and Euro 5 gasoline registrations, demonstrating that litigation reshapes the secondary market more broadly than prior LEZ evidence suggests.[13]

5.3 Heterogeneous Effects by Legal Outcome

To assess whether the market response varies with the credibility of enforcement, I examine the heterogeneous effects of NGO lawsuits by their eventual legal outcome. Counties are classified into four categories based on their enforcement status as of Spring 2021: (i) bans that are

[13] Prior work on low-emission zones (LEZs) documents behavioral responses to formal regulation. Wolff (2014) shows that LEZs induce substitution toward lower-emitting vehicles (Euro 4 to Euro 6) using city-level registration data from 2008 to 2010. Margaryan (2021) extends this analysis through 2017, finding that LEZs reduce the share of Euro 1 vehicles and increase Euro 4 registrations, with no significant effect on Euro 5 or Euro 6. This study, however, does not distinguish between fuel types. Klauber et al. (2024) separate diesel and gasoline vehicles and document that LEZs lead to a 17 percent decline in Euro 1 diesels, with no effect on the newest diesels, and a modest 1.1 percent increase in Euro 2 to Euro 4 gasoline registrations. Together, these studies show that LEZs primarily reduce the oldest diesel vehicles and modestly increase mid-aged gasoline registrations, while newer standards remain largely unaffected.

implemented, (ii) bans that are court-mandated but not enforced, (iii) lawsuits that remain pending, and (iv) lawsuits that are avoided through settlement or withdrawal. Importantly, market participants could not have known these outcomes in advance. I use the ex post classification only as a proxy for credibility, under the assumption that lawsuits that ultimately led to implemented bans represented more credible threats than those that did not. This approach allows me to test whether markets responded more strongly when the threats of future restrictions were more credible.

To isolate the signaling channel, I exclude post-ban periods in counties where diesel bans are eventually implemented. This ensures that the estimates capture the effect of lawsuits as regulatory signals rather than the later effect of bans themselves. I estimate separate treatment effects for each group using the Synthetic Difference-in-Differences (SDID) estimator of Arkhangelsky et al. (2021), which constructs synthetic controls that match pre-treatment trends and yields average treatment effects on the treated. Figures 8 and 9 present the results for used diesel and used gasoline registrations, disaggregated by Euro standard and final legal outcome.

Figure 8 presents SDID estimates of the effect of lawsuits on used diesel registrations, disaggregated by Euro standard and by the final legal outcome of the cases. The x-axis orders categories from implemented to avoided bans, which reflects a decreasing degree of enforcement credibility. The results reveal a clear gradient across these categories: the largest declines occur in counties where bans are implemented, and the magnitude steadily decreases for court-mandated, pending, and avoided cases. For Euro 3, Euro 4, and Euro 5 vehicles, registrations fall by up to 30 percent in counties with implemented bans, while the effects are progressively smaller in the other categories. By contrast, the pattern for Euro 6 diesel vehicles is less clear and the confidence intervals are wide across all categories. Overall, these results highlight the importance of expectations in shaping market behavior. Consumers respond not only to formal enforcement but also to signals about the credibility and severity of potential restrictions, with older diesel models most affected.

Figure 9 presents SDID estimates of the effect of lawsuits on used gasoline vehicle registrations by emission standard and final legal outcome. Compared with the diesel market, the enforcement gradient is less pronounced, although a clear pattern emerges for Euro 4 gasoline vehicles. The largest increases in registrations occur in counties where diesel bans are fully implemented, consistent with substitution away from diesel in response to credible enforcement.

The effect diminishes somewhat in areas with court-mandated bans and weakens further in counties under legal revision. Interestingly, registrations of Euro 4 gasoline vehicles also rise significantly in counties where bans are ultimately avoided through settlement or withdrawal. This suggests that the uncertainty generated during the litigation period can influence behavior even when no restrictions are eventually enacted. For Euro 5 gasoline vehicles, the estimates are generally positive but smaller in magnitude and imprecisely estimated, with no consistent gradient across implementation categories. Registrations of Euro 3 and Euro 6 gasoline vehicles show little to no response, with estimates close to zero and statistically insignificant across most legal outcomes.

Taken together, these findings indicate that the increases in used gasoline registrations are concentrated among Euro 4 models and are shaped by both the credibility and timing of legal action. The results highlight how anticipatory responses can emerge even in the absence of implemented policy, driven by perceived regulatory risk. They also suggest that the adjustment is selective rather than broad-based, with mid-aged gasoline models perceived as relatively safer choices under evolving restrictions. From a policy perspective, the findings emphasize the importance of legal and regulatory signaling: credible threats of enforcement can influence market outcomes well before formal policies take effect.

5.4 Spillovers

I now examine whether legal threats surrounding diesel bans generate spillover effects in neighboring counties not directly targeted by lawsuits. While my main analysis focuses on the 38 counties sued by Deutsche Umwelthilfe (DUH), residents in adjacent jurisdictions may also update their expectations in response to nearby legal actions, particularly in regions with shared commuting zones or overlapping media coverage. The direction of spillover effects is not predetermined. On one hand, consumers may respond similarly to those in treated areas, anticipating future regulation and reducing diesel purchases. On the other hand, if nearby counties are perceived as safer havens for diesel use and resale, I might observe substitution into diesel in these adjacent markets. My empirical design allows me to test for either direction of effect.

This analysis connects to earlier evidence on the spillover effects of Low Emission Zones (LEZs). Wolff (2014) shows that LEZs not only reshape fleet composition within regulated cities but also reduce the prevalence of high-emitting vehicles in neighboring regions. However, because that study relies on environmental sticker categories that combine diesel and gasoline vehicles, it cannot identify the specific role of diesel. Klauber et al. (2024) extend this literature by separating diesel and gasoline vehicles and find that LEZs reduce the number of banned diesel cars in both treated and neighboring counties, suggesting that policy-induced “de-dieselization” extends beyond directly regulated areas. Building on these insights, I seek to isolate spillover effects from legal action, rather than from policy implementation, and distinguish explicitly between diesel and gasoline vehicles.

To do so, I define a county as indirectly exposed if it lies within 25 kilometers of a treated county but was not itself subject to any DUH-led litigation. For these regressions, the treatment group is redefined to consist of these indirectly exposed counties, while the control group is defined as either never-treated counties (Panel A) or not-yet-treated counties (Panel B).Table 3 reports CS-DID estimates of spillover effects on used diesel vehicle registrations. Across all Euro standards, the coefficients are negative and statistically significant, indicating that residents in counties neighboring treated areas reduce diesel registrations rather than increase them. Using never-treated counties as controls (Panel A), registrations decline by 4.1 percent for Euro 1–3 vehicles and 6.0 percent for Euro 4 vehicles, with similar magnitudes for Euro 5 and Euro 6 models. When not-yet-treated counties are used as controls (Panel B), the estimated effects become larger. These estimates, although smaller in magnitude than the declines in directly treated counties, point to a clear geographic diffusion of regulatory pressure. Although displacement of diesel registrations to neighboring markets is theoretically plausible, I find no evidence of such behavior. Instead, registrations fall even in indirectly exposed areas, reinforcing the conclusion that litigation discourages diesel adoption more broadly rather than displacing it across space.

Turning to used gasoline vehicles (Table 4), I find evidence of spillover effects, although they are weaker and more selective than those for diesel vehicles. Registrations of Euro 4 gasoline vehicles increase by about 1.7 to 1.8 percent across specifications, consistent with a modest shift toward these mid-aged gasoline models in neighboring areas. By contrast, registrations of Euro 1–3 gasoline vehicles show small but statistically significant declines, while Euro 5 and Euro 6 gasoline vehicles display no meaningful changes. Overall, the results suggest that legal threats surrounding diesel bans induce limited adjustments in the gasoline car market, primarily through slight increases in Euro 4 vehicles. These patterns mirror the substitution tendencies observed in directly treated counties (Table 2), although the magnitudes are smaller in neighboring areas.

In summary, the spillover effects move in the same direction as those observed in directly treated counties, although the magnitudes are smaller. Diesel registrations fall across all emission categories in neighboring areas, with no evidence of displacement into these jurisdictions, even among older, more vulnerable diesel models. This absence of relocation suggests that legal threats discourage diesel adoption more broadly rather than simply shifting it across space. At the same time, registrations of middled-aged gasoline vehicles rise modestly, consistent with a cautious adjustment in purchasing behavior. Together, these results indicate that the influence of litigation extends beyond targeted jurisdictions, shaping expectations and discouraging diesel adoption even in areas not directly subject to lawsuits.

5.5 Effects of Lawsuits on New Car Registrations

While the used car market reflects adjustments in secondary transactions, the new car market captures forward-looking purchase decisions. I therefore examine how lawsuits affect new registrations across ownership types and fuel categories. Unlike the used market, the new car market does not differentiate vehicles by Euro standard, as all new cars sold in Germany from 2014 onward are required to meet the Euro 6 standard. Instead, I distinguish between private and company registrations, as these segments follow systematically different decision-making patterns. Prior research shows that private consumers tend to focus on upfront costs (Andor et al., 2020), whereas companies prioritize total cost of ownership (Transport & Environment, 2020). Moreover, company car buyers benefit from a wide range of fiscal incentives, including VAT refunds, depreciation allowances, and tax deductions for employees using company cars (Burra et al., 2024). This heterogeneity motivates my analysis of how lawsuits about diesel bans affect new car registrations across ownership type (private vs. company) and fuel type (diesel vs. gasoline).

Table 5 presents CS-DD estimates of the effect of NGO lawsuits on new car registrations. Among company vehicles, I find no statistically significant changes in either diesel or gasoline registrations, indicating limited responsiveness to litigation-based regulatory signals. One possible explanation is that company fleets operate with longer planning cycles, leasing contracts, and institutional procurement constraints, which reduce short-run flexibility. This muted response is also consistent with Burra et al. (2024), who show that company vehicles in Germany respond much less to financial incentives for electric vehicles than private buyers, underscoring structural differences in purchasing behavior between the two segments. In contrast, private vehicles show a statistically significant 3.3 percent increase in gasoline registrations, while the 2.9 percent increase in diesel registrations is not statistically significant. These results suggest that private consumers are more responsive to regulatory pressure, with the rise in gasoline registrations consistent with a shift toward vehicles perceived as less exposed to future restrictions, although I cannot directly observe substitution decisions.

The absence of significant increases in diesel registrations across both ownership types suggests limited enthusiasm for diesel's long-term viability, even for newer Euro 6 models that remain formally exempt from driving bans. While I cannot directly observe consumer attitudes, this muted response is consistent with concerns about regulatory uncertainty and the possibility of future restrictions. This interpretation stands in contrast to emerging scientific evidence showing that modern gasoline vehicles can emit more carbonaceous particulate matter than diesel vehicles equipped with particulate filters, suggesting that Euro 6 diesels are not necessarily more polluting than their gasoline counterparts (Platt et al., 2017). Consistent with this pattern, the number of diesel make–model variants with new registrations contracts during the study period, falling from 303 in 2014 to 281 in 2019. Over the same period, gasoline variants rise modestly from 464 to 484, while hybrid and electric variants expand more substantially. These descriptive patterns are consistent with both limited consumer uptake and a gradual reduction in diesel offerings by manufacturers. While I cannot fully disentangle demand and supply, the evidence points to a weakening role for diesel in the German new car market during the study period.

To explore the dynamics of the treatment effect and assess the plausibility of the parallel trends assumption, I turn to an event study analysis to trace the timing, persistence, and potential heterogeneity of market responses. Figure 10 presents the event study estimates for new car registrations by fuel type and ownership. For company cars, I find no statistically significant change in diesel or gasoline registrations throughout the post-treatment period, consistent with the static ATT results as shown in Table 3. By contrast, private vehicles exhibit more pronounced, though temporary, adjustments. Registrations of private gasoline vehicles remain stable in the early post-treatment quarters but increase sharply beginning around quarter 12, peaking at roughly 10 to 15 percent before tapering off by quarter 15. This short-lived surge may reflect precautionary purchases in response to heightened regulatory uncertainty surrounding diesel, followed by a return to baseline once expectations stabilize. For private diesel vehicles, I observe a modest increase in registrations in the early post-treatment quarters, peaking around quarter 4, followed by a steady decline. By the end of the event window, the effects converge back to zero, suggesting that any initial adjustment was transitory. Taken together, the event study results highlight ownership-specific heterogeneity: private buyers display delayed but temporary responses to litigation-based regulatory signals, while company registrations remain largely unaffected throughout.

5.6 Effects of Lawsuits on Local Air Pollution

Having examined how legal actions surrounding diesel bans reshape the composition of the car fleet, I now turn to the environmental consequences of these changes. These legal actions re motivated by growing concerns about air pollution, particularly traffic-related pollutants. I therefore focus on nitrogen dioxide (NO_2), particulate matter (PM_{10}), and ozone (O_3), and sulphur dioxide (SO_2). NO_2 and PM_{10} are directly tied to vehicle emissions, making them the most relevant indicators of traffic pollution. O_3 by contrast, is a secondary pollutant formed through atmospheric chemical interactions involving nitrogen oxicdes and volatile organic compounds. SO_2, while less directly tied to road traffic in recent years, provides an additional check on broader air-quality effects.

Table 6 presents CS-DD estimates using interpolated pollution measures, where I aggregate daily station readings to the county-quarter level and impute missing coverage using inverse distance weighting (IDW), following standard practice in the literature (e.g., Currie and Neidell (2005); Karlsson and Ziebarth 2018; Klauber et al. 2024). In Appendix Table A.5, I replicate the analysis without interpolation, relying only on counties with direct monitoring coverage. As expected, the number of observations is much smaller in this specification, since many counties lack pollution monitors, but the results are qualitatively similar. Turning to the main estimates in Table 4, I find that NO_2 concentrations decline by 1.413 $\mu g/m^3$, equivalent to 4.0 percent of the pre-treatment mean. This effect lies within the range of prior studies on the effects of Low Emission Zones (LEZs), which document NO_2 reduction between 0.6 and 4.1 $\mu g/m^3$ depending on the specification and policy scope (Gehrsitz 2017; Klauber et al. 2024; Pestel and Wozny 2021; Sarmiento et al. 2023). The magnitude of my estimated is notable given that the treatment captures the initiation of legal proceedings rather than the enforcement of formal diesel bans. Although the litigation specifically targets NO_2, the impacts may not be limited to this pollutant. However, I find no statistically significant changes in PM_{10}, O_3 or SO_2 concentrations.

The absence of detectable effects on these pollutants is not unexpected, given their complex atmospheric formation processes and potentially slower responsiveness to changes in fleet composition. Nevertheless, the Air Quality Index (AQI) declines by roughly 3.5 $\mu g/m^3$ (7.3 percent), a statistically significant reduction that signals an overall improvement in local air quality conditions (see Appendix Table A.4). These findings suggest that lawsuits aimed at diesel restrictions, even before bans are implemented, can generate meaningful environmental co-benefits, particularly by reducing NO_2 concentrations and improving overall air quality. Future research with longer follow-up periods and more granular pollution data could further illuminate the persistence and breadth of these environmental responses.

To further explore the timing and persistence of air quality effects, I conduct an event study analysis tracing pollution levels before and after lawsuits. Figure 10 shows that NO_2 concentrations begin to decline about four quarters after treatment, with reductions exceeding 3 $\mu g/m^3$ by the 12th quarter. This gradual decline aligns with my earlier car market findings, where I observe persistent reductions in older diesel registrations and increased uptake of cleaner alternatives. PM_{10} estimates remain statistically insignificant across post-treatment periods. Similarly, I detect no significant changes in O_3 or SO_2 concentrations over the post-treatment period. This differs from Sarmiento et al. (2023), who document a rise in ozone following the LEZ implementation in Germany. A key distinction lies in the treatment definition and time horizon: my study captures regulatory signals via legal action over a four-year window, whereas Sarmiento et al. (2023) examine LEZ enforcement over a decade. Although declines in NO_2, a precursor that scavenges ozone are evident, their scale and duration may not be sufficient to drive ozone accumulation. This highlights the nonlinear and context-specific nature of atmospheric responses to emission changes (Kroll et al., 2020).

5.7 Comparing Potential and Actual Regulation (Registrations)

The preceding sections examined how regulatory threats, in the form of lawsuits filed by the environmental NGO, affected used and new car registrations. These estimates capture market responses to the expectation of future restrictions rather than to their actual enforcement. The empirical setting in this study allows for a direct comparison between potential and actual regulation. Some cities, such as Stuttgart, Darmstadt, and Berlin, experienced NGO-led lawsuits that were later followed by diesel bans, while others, such as Hamburg, implemented bans without any prior legal action. This variation allows distinguishing between market adjustments triggered by regulatory threats and those that occur after formal policy implementation.

Figures A.10–A.13 illustrate these dynamics. Each panel presents synthetic difference-in-differences (SDID) event-study estimates by fuel type and Euro standard. The solid vertical line marks the filing of a lawsuit, which represents the onset of potential regulation, and the dashed vertical line marks the start of the diesel ban, which represents actual enforcement. Among these cities, Stuttgart provides the clearest evidence, as it offers the longest post-lawsuit and post-ban observation window. There, diesel Euro 3–5 registrations begin to decline immediately after the lawsuit and fall further once the ban is enforced, showing a two-stage adjustment: an early response to the regulatory threat and an additional decline after enforcement. The absence of pre-filing trends supports the identifying assumptions of the SDID framework. In Darmstadt and Berlin, the dynamics are similar but the post-ban period is shorter, limiting the ability to capture longer-term enforcement effects. The estimates still indicate that most of the decline occurred before the bans took effect. Gasoline registrations show smaller and more variable changes, consistent with limited substitution toward gasoline vehicles except for a modest increase in Euro 4 gasoline cars in Stuttgart.

Hamburg provides a useful contrast. Because its diesel ban preceded any NGO-led lawsuit, the estimates there capture only the immediate effect of enforcement without any preceding regulatory threat. The results in Figure A.13 show little evidence of a significant or sustained response. This suggests that analyses focusing solely on post-enforcement periods may bias the estimated impact of regulation, as they omit earlier market adjustments triggered by credible legal or informational signals.

Table 7 summarizes these dynamics across cities. ATT (Pre-ban) measures the average effect of lawsuits before the implementation of formal diesel bans, while ATT (Full) captures the total effect over the full period including bans. Mean (Pre-ban) and Mean (Post-ban) report average event-study coefficients before and after enforcement, respectively. Across treated cities, the pre-ban effects for used diesel registrations range from −0.19 to −0.37 log points, accounting for roughly two-thirds to three-quarters of the total observed adjustment. These results indicate that regulatory threats were the primary driver of market response, while enforcement reinforced but did not change its direction. A limitation is that the post-ban observation window is relatively short, since the registration data ends in 2019. Future data would allow for assessing whether enforcement effects persist or dissipate over time.

5.8 Effects of Diesel Bans on Used Car Listings

5.8.1 Asking Prices

Table 8 presents estimates of the effect of diesel driving bans on used car listing prices, using listings from 2019 to 2024. The dependent variable is the logarithm of the asking price posted by sellers on the AutoScout24 platform. Across all specifications, I find a statistically significant and economically meaningful reduction in the listing prices of diesel vehicles that are explicitly restricted under local diesel ban regulations. The coefficient on the interaction term *Ban* × *AffectedDiesel* ranges from -0.051 to -0.033, implying a price decline of approximately 3.3 to 5.1 percent relative to unaffected vehicles during the ban period. These estimates are robust to alternative sets of fixed effects, including specifications that absorb granular time trends and county-level variation in brand composition. The coefficients on Ban alone are small and occasionally positive, indicating that unaffected vehicles did not experience comparable price declines and may even have benefited slightly from substitution effects. I also find a significant negative coefficient on the interaction term Diesel × Car Age, indicating that diesel vehicles depreciate more steeply with age than gasoline vehicles. This suggests that sellers adjust asking prices for older diesels more aggressively, consistent with expectations of lower resale value under evolving regulatory environments. Taken together, these results show that diesel driving bans exerted downward pressure on the market value of non-compliant diesel vehicles in treated counties.

5.8.2 Listing Volumes

Table 9 reports the effect of diesel bans on the number of used car listings. Across all specifications, the coefficient on Ban × AffectedDiesel is large, negative, and highly significant, ranging from –0.230 to –0.183. These estimates imply that diesel bans reduced the number of listings for non-compliant diesels by roughly 18 to 23 percent relative to unaffected vehicles. By contrast, the coefficient on Ban alone is negative in the full 2019–2024 sample, suggesting a modest decline inlistings of unaffected vehicles, but becomes statistically insignificant when the analysis is restricted to 2019 only. This pattern indicates that the robust and consistent response is concentrated among restricted diesels, with little evidence of systematic spillovers to unaffected vehicles. Among the control variables, older cars are consistently less likely to be listed, diesel vehicles are listed more frequently than gasoline cars on average, and the negative coefficient on Diesel × Car Age shows that diesel listings decline more steeply with age than gasoline listings. Taken together, the results highlight that d iesel bans substantially reduced the presence of non-compliant diesels in the used car market, while leaving unaffected vehicles largely unchanged.

Taken together, the listing platform evidence shows that diesel driving bans triggered sharp and targeted adjustments on the supply side of the used car market. Sellers of non-compliant diesels responded by lowering asking prices by about 3–5 percent and reducing listings by nearly 20 percent, while unaffected vehicles remained largely stable. These bans amplified an existing pattern in the market, as older diesels were already depreciating more steeply and contracting more rapidly from listings than comparable gasoline vehicles. The findings highlight the enforcement channel of regulation: bans directly eroded the market value and availability of restricted diesels in the domestic secondary market. At the same time, it is important to recognize that the platform records asking prices and listings rather than final transactions, and does not capture whether withdrawn vehicles were ultimately sold, scrapped, or exported abroad. Future work linking listings to transaction and export data would help provide a fuller picture of the longer-run reallocation of restricted diesels.

6. Discussion and Conclusions

This study examines how both regulatory signals and regulatory enforcement shape vehicle market behavior and environmental outcomes in Germany. Leveraging the staggered timing of lawsuits filed by an environmental NGO seeking diesel driving restrictions, I provide evidence that legalthreats alone can significantly influence consumer choices and local air quality. In the used car market, registrations of diesel vehicles declined significantly across all emission standards, with reductions ranging from 7% to 9%. Simultaneously, registrations of Euro 4 and Euro 5 gasoline vehicles increased by 7.7% and 3.5%, respectively, suggesting precautionary substitution toward models perceived as less exposed to regulatory risk. Back-of-the-envelope calculations indicate that these shifts correspond to approximately 18,788 fewer used car registrations across 38 treated counties in a one-year period. In the new car market, lawsuits are associated with a 2.8 precent increase in gasoline registrations among private buyers, equivalent to 3,758 additional vehicles. However, this uptick was not sufficient to offset the decline in used car transactions, resulting in a net contraction in internal combustion engine registrations overall. Beyond market responses, I find that nitrogen dioxide concentrations fell by approximately 8% in treated counties, with corresponding improvements in composite air quality indices, underscoring the tangible environmental co-benefits of legal signals.

Beyond these anticipatory responses, I next examine the consequences of the diesel bans once they were formally implemented. Drawing on granular listing data from AutoScout24, I study how sellers of non-compliant diesels adjusted in response to restrictions enacted between 2019 and 2024. The results point to substantial adjustments that were confined to non-compliant diesels. Their asking prices fell by 3–5 percent and listings contracted by 18–23 percent after bans, in contrast to the relative stability of unaffected vehicles. These findings indicate that bans not only depressed the resale value of restricted diesels but also thinned their presence in the domestic secondary market. At the same time, the platform records listings rather than transactions, and does not reveal whether withdrawn vehicles were ultimately sold, scrapped, or exported abroad. This caveat points to a promising avenue for future work linking listings to transaction and export data to provide a fuller picture of how regulatory enforcement redistributes vehicles across markets.

Taken together, my results show that regulatory pressure influences behavior through two distinct channels. Lawsuits function as credible threats of future regulation, prompting anticipatory responses even before formal rules are enacted, while diesel bans generate direct adjustments in the market for non-compliant vehicles. These findings highlight the importance of both regulatory signals and implemented restrictions, and suggest that effective policy design should account for heterogeneous responses across market segments. Future research could further examine the long-run consequences for resale values, scrappage decisions, cross-border vehicle flows, and broader transportation choices.

REFERENCE

AirClim. (2015). Air pollution prevails in Germany. *Acid News*. https://www.airclim.org/acidnews/air-pollution-prevails-germany

Andor, M. A., Gerster, A., Gillingham, K. T., & Horvath, M. (2020). Running a car costs much more than people think—stalling the uptake of green travel. *Nature*, *580*(7804), 453-455.

Arkhangelsky, D., Athey, S., Hirshberg, D. A., Imbens, G. W., & Wager, S. (2021). Synthetic difference-in-differences. *American Economic Review*, *111*(12), 4088-4118.

Barahona, N., Gallego, F. A., & Montero, J. P. (2020). Vintage-specific driving restrictions. *The Review of Economic Studies*, *87*(4), 1646-1682.

Borusyak, K., Jaravel, X., & Spiess, J. (2024). Revisiting event-study designs: robust and efficient estimation. *Review of Economic Studies*, *91*(6), 3253-3285.

Bothner, F., Töller, A. E., & Schnase, P. P. (2022). Do lawsuits by ENGOs improve environmental quality? Results from the field of air pollution policy in Germany. *Sustainability*, *14*(11), 6592.

Brauer, M., Roth, G. A., Aravkin, A. Y., Zheng, P., Abate, K. H., Abate, Y. H., ... & Amani, R. (2024). Global burden and strength of evidence for 88 risk factors in 204 countries and 811 subnational locations, 1990–2021: a systematic analysis for the Global Burden of Disease Study 2021. The Lancet, 403(10440), 2162-2203. *The Lancet*, *403*(10440), 2162-2203.

Buntaine, M. T., Greenstone, M., He, G., Liu, M., Wang, S., & Zhang, B. (2024). Does the squeaky wheel get more grease? The direct and indirect effects of citizen participation on environmental governance in China. *American Economic Review*, *114*(3), 815-850.

Burra, L. T., Sommer, S., & Vance, C. (2024). Free-ridership in subsidies for company-and private electric vehicles. *Energy Economics*, *131*, 107333.

Callaway, B., & Sant'Anna, P. H. (2021). Difference-in-differences with multiple time periods. *Journal of econometrics*, *225*(2), 200-230.

Cerruti, D., Alberini, A., & Linn, J. (2019). Charging drivers by the pound: How does the UK vehicle tax system affect CO2 emissions?. *Environmental and Resource Economics*, *74*(1), 99-129.

Chay, K., & Greenstone, M. (2003). Air quality, infant mortality, and the Clean Air Act of 1970.

ClientEarth. (2018). *Important judgments for clean air in Germany.* https://www.clientearth.org/projects/access-to-justice-for-a-greener-europe/updates-annual-newsletters/important-judgments-for-clean-air-in-germany

Currie, J., & Neidell, M. (2005). Air pollution and infant health: what can we learn from California's recent experience?. *The quarterly journal of economics*, *120*(3), 1003-1030.

Davis, L. W. (2008). The effect of driving restrictions on air quality in Mexico City. *Journal of Political Economy*, *116*(1), 38-81.

De Chaisemartin, C., & d'Haultfoeuille, X. (2020). Two-way fixed effects estimators with heterogeneous treatment effects. *American Economic Review*, *110*(9), 2964-2996.

Del Rosal, I. (2022). European dieselization: Policy insights from EU car trade. *Transport policy*, *115*, 181-194.

Deutsche Umwelthilfe. (2019). *Right to clean air: European background paper* [PDF]. https://www.duh.de/fileadmin/user_upload/download/Projektinformation/Verkehr/Luftreinhaltung/Right-to-Clean-Air_Europe_Backgroundpaper_EN.pdf

European Automobile Manufacturers Association. (n.d.). *Diesel penetration in Western Europe* [PDF]. https://www.acea.auto/uploads/statistic_documents/Diesel_penetration_in_Western_Europe.pdf

European Environment Agency. (2019). *Air quality in Europe: 2019 report* (EEA Report No. 10/2019). Publications Office of the European Union. https://doi.org/10.2800/822355

Eskeland, G. S., & Feyzioglu, T. (1997). Rationing can backfire: the "day without a car" in Mexico City. *The World Bank Economic Review*, *11*(3), 383-408.

European Parliament. (2022). *EU ban on the sale of new petrol and diesel cars from 2035 explained. European Parliament Topics.* https://www.europarl.europa.eu/topics/en/article/20221019STO44572/eu-ban-on-sale-of-new-petrol-and-diesel-cars-from-2035-explained

Fardella, C., Barahona, N., Montero, J. P., & Sepúlveda, F. (2023). On the geography of vintage-specific restrictions. *Resource and Energy Economics*, *75*, 101405.

Gallego, F., Montero, J. P., & Salas, C. (2013). The effect of transport policies on car use: Evidence from Latin American cities. *Journal of Public Economics*, *107*, 47-62.

Gehrsitz, M. (2017). The effect of low emission zones on air pollution and infant health. *Journal of Environmental Economics and Management*, *83*, 121-144.

Goodman-Bacon, A. (2021). Difference-in-differences with variation in treatment timing. *Journal of econometrics*, *225*(2), 254-277.

Greenstone, M. (2002). The impacts of environmental regulations on industrial activity: Evidence from the 1970 and 1977 clean air act amendments and the census of manufactures. *Journal of Political Economy*, *110*(6), 1175-1219.

Greenstone, M., List, J. A., & Syverson, C. (2012). *The effects of environmental regulation on the competitiveness of US manufacturing* (No. w18392). National Bureau of Economic Research.

Gu, Y., Deakin, E., & Long, Y. (2017). The effects of driving restrictions on travel behavior evidence from Beijing. *Journal of Urban Economics*, *102*, 106-122.

Gu, Y., Henze, D. K., Nawaz, M. O., Cao, H., & Wagner, U. J. (2023). Sources of PM2. 5-associated health risks in Europe and corresponding emission-induced changes during 2005–2015. *GeoHealth*, *7*(3), e2022GH000767.

Guerra, E., & Millard-Ball, A. (2017). Getting around a license-plate ban: Behavioral responses to Mexico City's driving restriction. *Transportation Research Part D: Transport and Environment*, *55*, 113-126.

Guerra, E., Sandweiss, A., & Park, S. D. (2022). Does rationing really backfire? A critical review of the literature on license-plate-based driving restrictions. *Transport Reviews*, *42*(5), 604-625.

He, G., Wang, S., & Zhang, B. (2020). Watering down environmental regulation in China. *The Quarterly Journal of Economics*, *135*(4), 2135-2185.

Hockenos, P. (2018). End of the road: Are diesel cars on the way out in Europe? *Yale Environment 360.* https://e360.yale.edu/features/end-of-the-road-are-diesel-cars-on-the-way-out-in-europe

Isen, A., Rossin-Slater, M., & Walker, W. R. (2017). Every breath you take—every dollar you'll make: The long-term consequences of the clean air act of 1970. *Journal of Political Economy*, *125*(3), 848-902.

Jacobsen, M. R., Sallee, J. M., Shapiro, J. S., & Van Benthem, A. A. (2023). Regulating untaxable externalities: Are vehicle air pollution standards effective and efficient?. *The Quarterly Journal of Economics*, *138*(3), 1907-1976.

Karlsson, M., & Ziebarth, N. R. (2018). Population health effects and health-related costs of extreme temperatures: Comprehensive evidence from Germany. *Journal of Environmental Economics and Management*, *91*, 93-117.

Klauber, H., Holub, F., Koch, N., Pestel, N., Ritter, N., & Rohlf, A. (2024). Killing prescriptions softly: Low emission zones and child health from birth to school. *American Economic Journal: Economic Policy*, *16*(2), 220-248.

Kroll, J. H., Heald, C. L., Cappa, C. D., Farmer, D. K., Fry, J. L., Murphy, J. G., & Steiner, A. L. (2020). The complex chemical effects of COVID-19 shutdowns on air quality. *Nature Chemistry*, *12*(9), 777-779.

Li, S., Linn, J., & Muehlegger, E. (2014). Gasoline taxes and consumer behavior. *American Economic Journal: Economic Policy*, *6*(4), 302-342.

Li, S., Linn, J., & Spiller, E. (2013). Evaluating "Cash-for-Clunkers": Program effects on auto sales and the environment. *Journal of Environmental Economics and management*, *65*(2), 175-193.

Li, S., Wang, B., & Zhou, H. (2024). Decarbonizing passenger transportation in developing countries: Lessons and perspectives1. *Regional Science and Urban Economics*, *107*, 103977.

Margaryan, S. (2021). Low emission zones and population health. *Journal of Health Economics*, *76*, 102402.

Martin, G. J., & Yurukoglu, A. (2017). Bias in cable news: Persuasion and polarization. *American Economic Review*, *107*(9), 2565-2599.

Nishitateno, S., & Burke, P. J. (2024). Effects of low emission zones on air quality, new vehicle registrations, and birthweights: Evidence from Japan. *Environmental and Resource Economics*, *87*(7), 1955-1992.

Pestel, N., & Wozny, F. (2021). Health effects of low emission zones: evidence from German hospitals. *Journal of Environmental Economics and Management*, *109*, 102512.

Platt, S. M., El Haddad, I., Pieber, S. M., Zardini, A. A., Suarez-Bertoa, R., Clairotte, M., ... & Prévôt, A. S. (2017). Gasoline cars produce more carbonaceous particulate matter than modern filter-equipped diesel cars. *Scientific reports*, *7*(1), 4926.

Real, E., Megaritis, A., Colette, A., Valastro, G., & Messina, P. (2024). Atlas of ozone chemical regimes in Europe. *Atmospheric Environment*, *320*, 120323.

Richter, I., & Stegen, K. S. (2022). A choreography of delay: The response of German auto incumbents to environmental policy. *Environmental Innovation and Societal Transitions*, *45*, 1-13.

Sarmiento, L., Wägner, N., & Zaklan, A. (2023). The air quality and well-being effects of low emission zones. *Journal of Public Economics*, *227*, 105014.

Sato, M., Gostlow, G., Higham, C., Setzer, J., & Venmans, F. (2024). Impacts of climate litigation on firm value. *Nature Sustainability*, *7*(11), 1461-1468.

Shu, L., Wang, C., & Wang, W. (2025). Vehicle exhaust standards and urban air quality in China. *Journal of Development Economics*, *172*, 103387.

Silva, J. S., & Tenreyro, S. (2006). The log of gravity. *The Review of Economics and statistics*, 641-658.

Silva, J. S., & Tenreyro, S. (2011). Further simulation evidence on the performance of the Poisson pseudo-maximum likelihood estimator. *Economics Letters*, *112*(2), 220-222.

Strittmatter, A., & Lechner, M. (2020). Sorting in the used-car market after the Volkswagen emission scandal. *Journal of Environmental Economics and Management*, *101*, 102305.

Sun, L., & Abraham, S. (2021). Estimating dynamic treatment effects in event studies with heterogeneous treatment effects. *Journal of Econometrics*, *225*(2), 175-199.

Töller, A. E. (2021). Driving bans for diesel cars in German cities: The role of ENGOs and Courts in producing an unlikely outcome. *European Policy Analysis*, *7*(2), 486-507.

Transport & Environment. (2020). *Company cars: How European governments are subsidising pollution and climate change.* https://www.transportenvironment.org/articles/company-cars-how-european-governments-are-subsidising-pollution-and-climate-change

Umweltbundesamt. (2018). *Air quality 2017: Reduction in nitrogen dioxide pollution is not yet sufficient.* https://www.umweltbundesamt.de/presse/pressemitteilungen/luftqualitaet-2017-rueckgang-der

Walker, W. R. (2013). The transitional costs of sectoral reallocation: Evidence from the clean air act and the workforce. *The Quarterly Journal of Economics*, *128*(4), 1787-1835.

Wang, L., Xu, J., & Qin, P. (2014). Will a driving restriction policy reduce car trips?—The case study of Beijing, China. *Transportation Research Part A: Policy and Practice*, *67*, 279-290.

Wolff, H. (2014). Keep your clunker in the suburb: low-emission zones and adoption of green vehicles. *The Economic Journal*, *124*(578), F481-F512.

Zhang, Y. (2022, March 4). How can low-emission zones drive a just transition to sustainable mobility? *ICLEI Sustainable Mobility.* https://sustainablemobility.iclei.org/how-can-low-emission-zones-drive-a-just-transition-to-sustainable-mobility

Zhang, J., Elliott, R. J., Zhang, B., & Liu, M. (2025). Public environmental complaints and regulatory intensity. *Journal of Environmental Economics and Management*, 103221.

Zhou, H. (2023). *Restricting trade for the environment? Evidence from import restrictions on used vehicles in China* [Working paper].

Figure 1. Cities with Diesel Driving Restrictions (2014–2019)

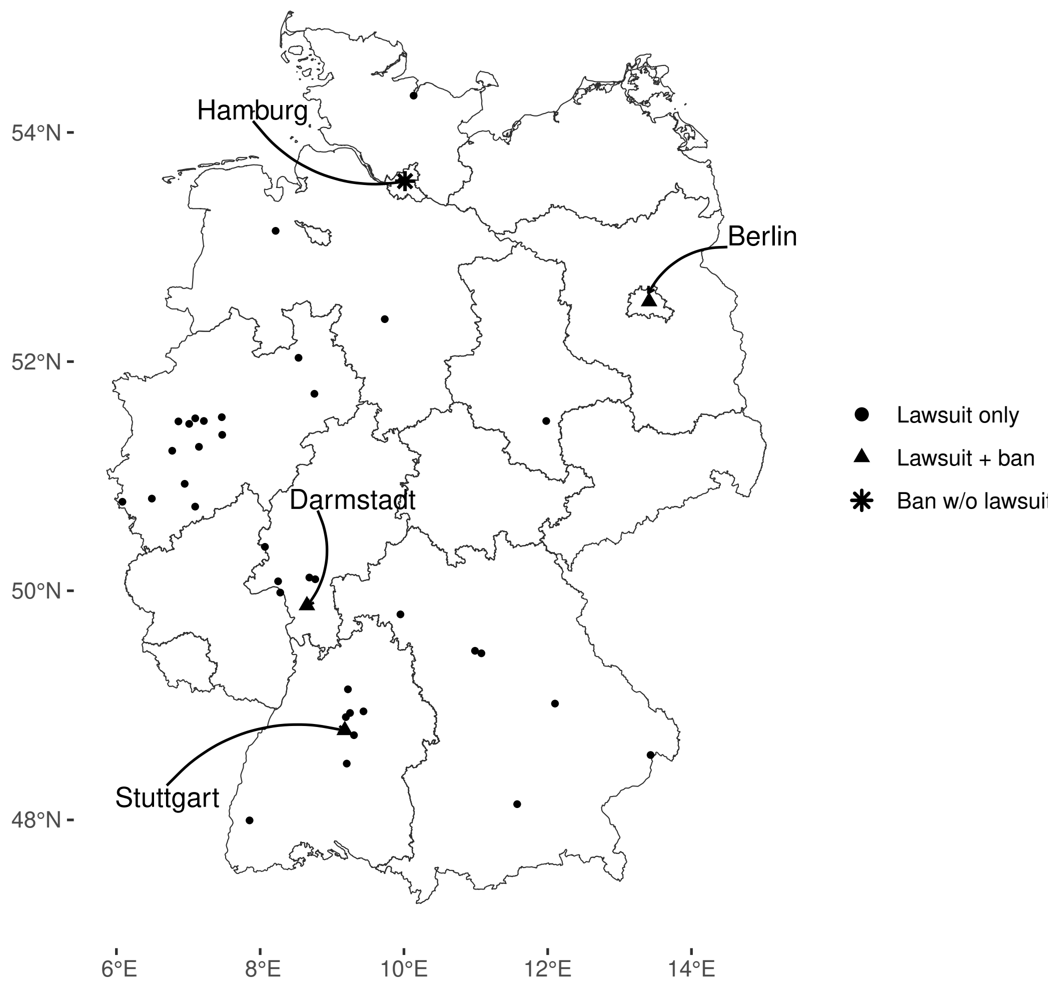


Figure 2. Timing of Lawsuits Related to Diesel Driving Restrictions Across Cities

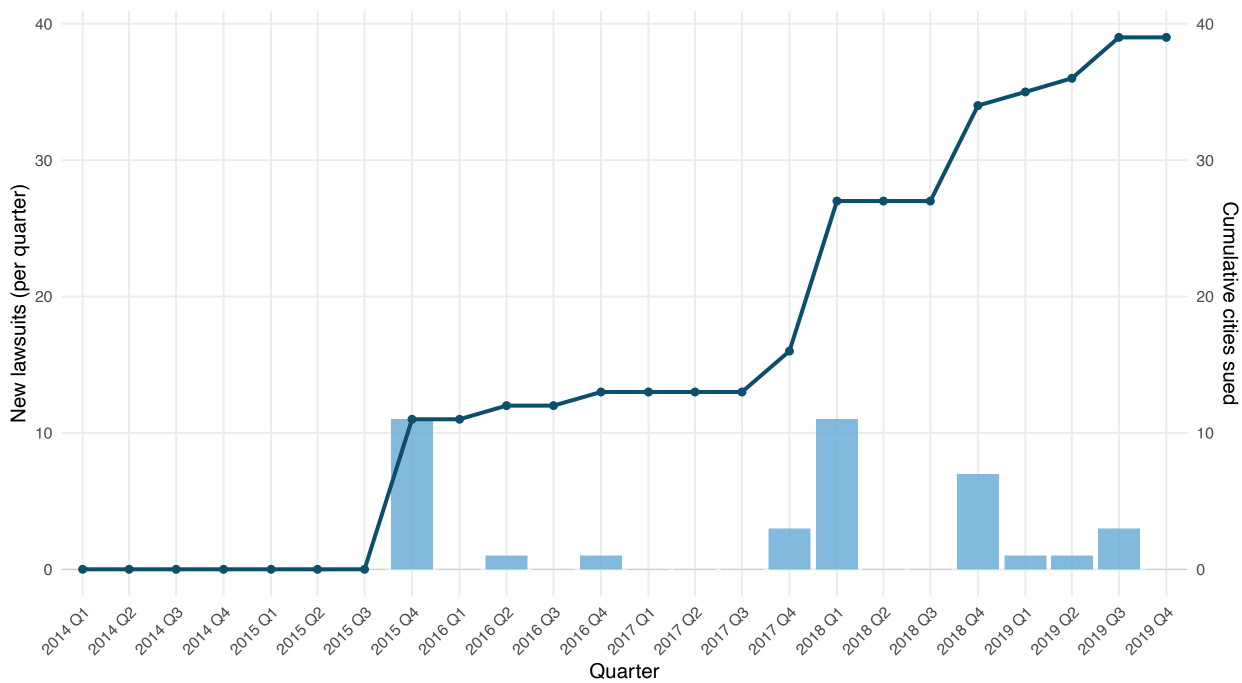
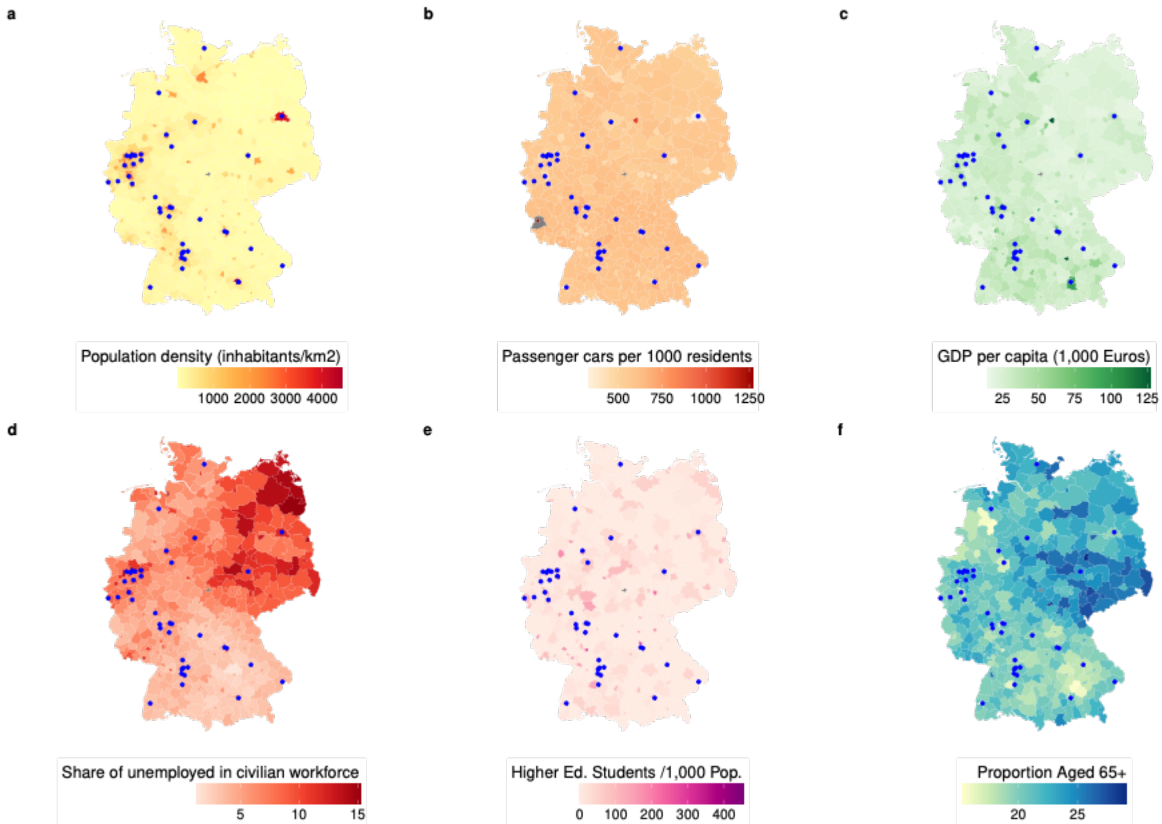
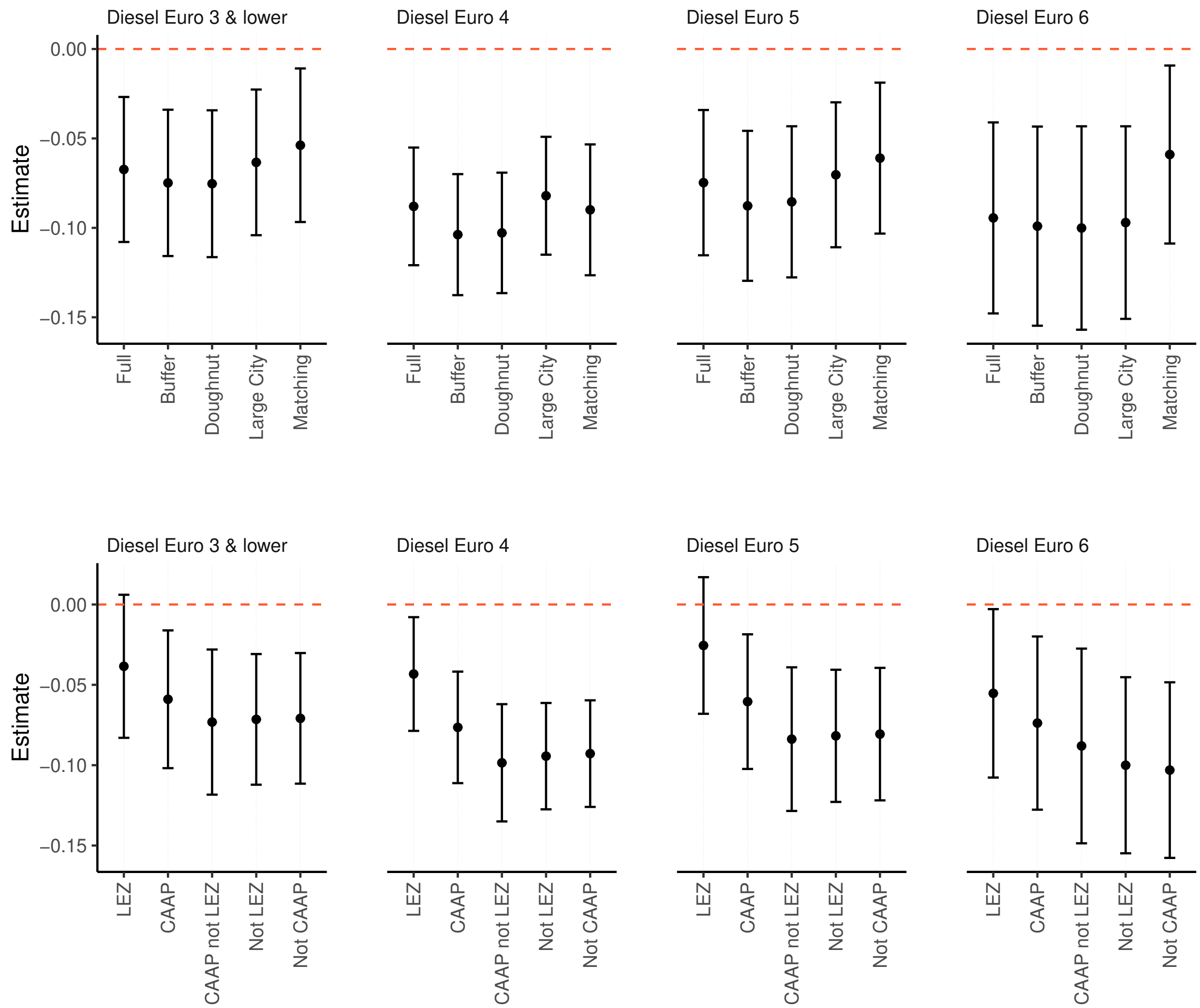


Figure 3: Spatial Distribution of County Characteristics



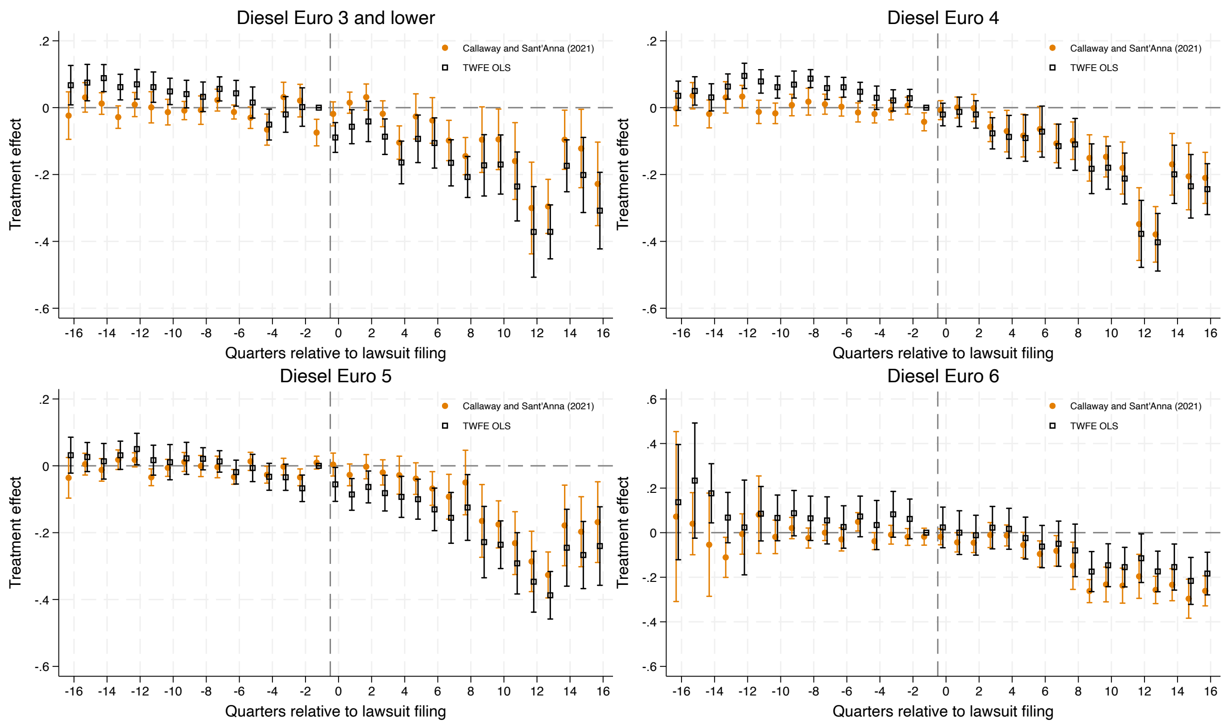
Notes: This figure maps key county characteristics in 2013, prior to any proposed or actual diesel bans. Blue dots mark counties where lawsuits seeking diesel restrictions were later filed. Data are from the Federal Institute for Research on Building, Urban Affairs and Spatial Development (BBSR).

Figure 4: Impact of Lawsuits on Used Diesel Registrations by Control Group



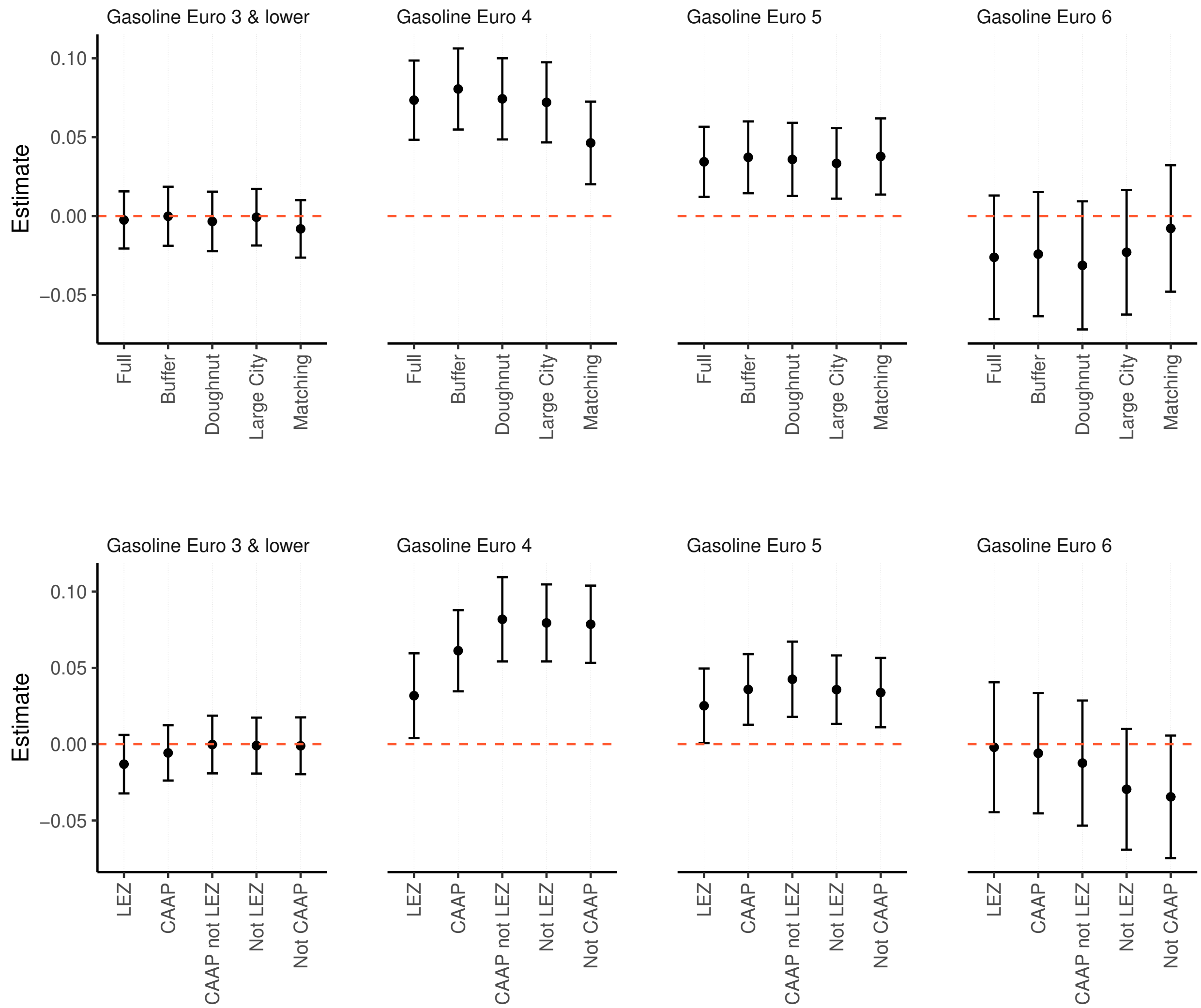
Notes: Each figure plots the CS-DD estimates of the effect of lawsuits filed by Deutsche Umwelthilfe (DUH) seeking diesel driving restrictions on used diesel vehicle registrations, by Euro emission standard, under alternative control group definitions. The top panel reports results for Full, Buffer, Doughnut, Large City, and Matching. The bottom panel reports results for LEZ, CAAP, CAAP not LEZ, Not LEZ, and Not CAAP. All regressions include county fixed effects and quarter-by-year fixed effects. Error bars represent the 95 percent confidence intervals with standard errors clustered at the county level.

Figure 5: Event Study of Used Diesel Vehicle Registrations



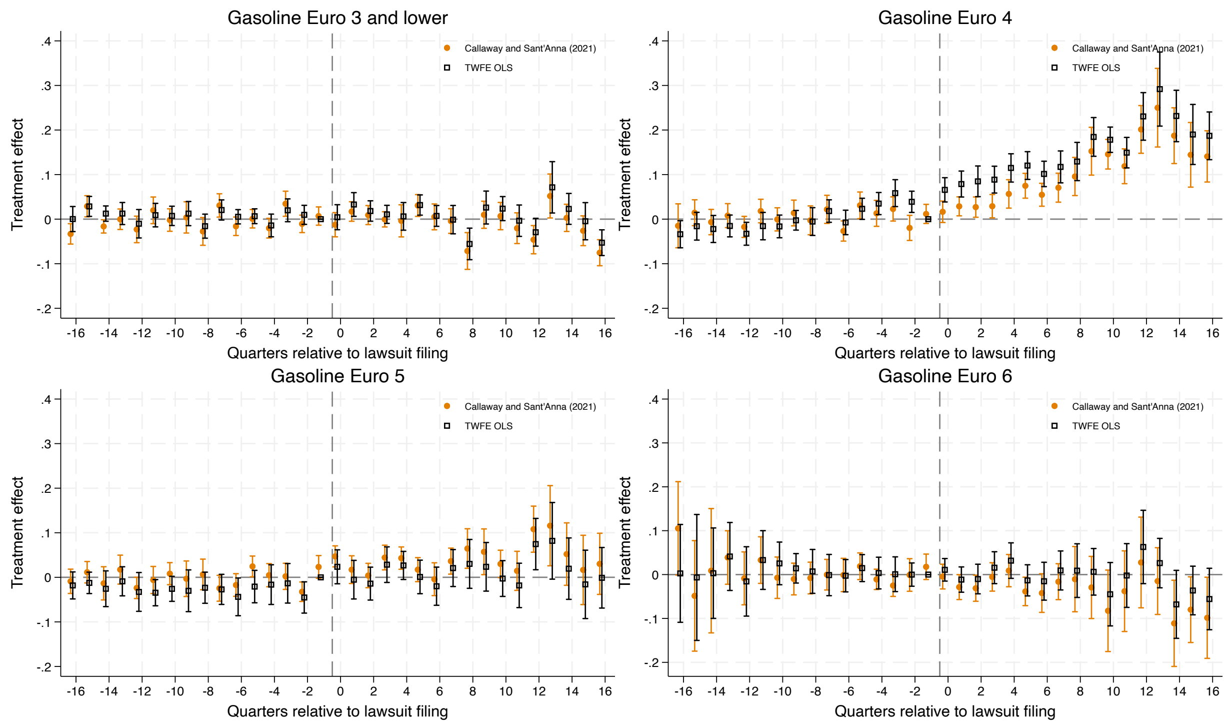
Notes: This figure presents event study estimates of the effect of lawsuits filed by Deutsche Umwelthilfe (DUH) on used diesel vehicle registrations, disaggregated by emission standard. Each panel overlays results from two estimators: a dynamic version of the two-way fixed effects (TWFE) model estimated by OLS (in black square markers), and the estimator proposed by Callaway and Sant'Anna (2021) (in orange circle markers). The outcome variable is the log number of used diesel vehicle registrations at the county-quarter level. The bars represent the 95 percent confidence intervals. Standard errors are clustered at the county level.

Figure 6: Impact of Lawsuits on Used Gasoline Registrations by Control Group



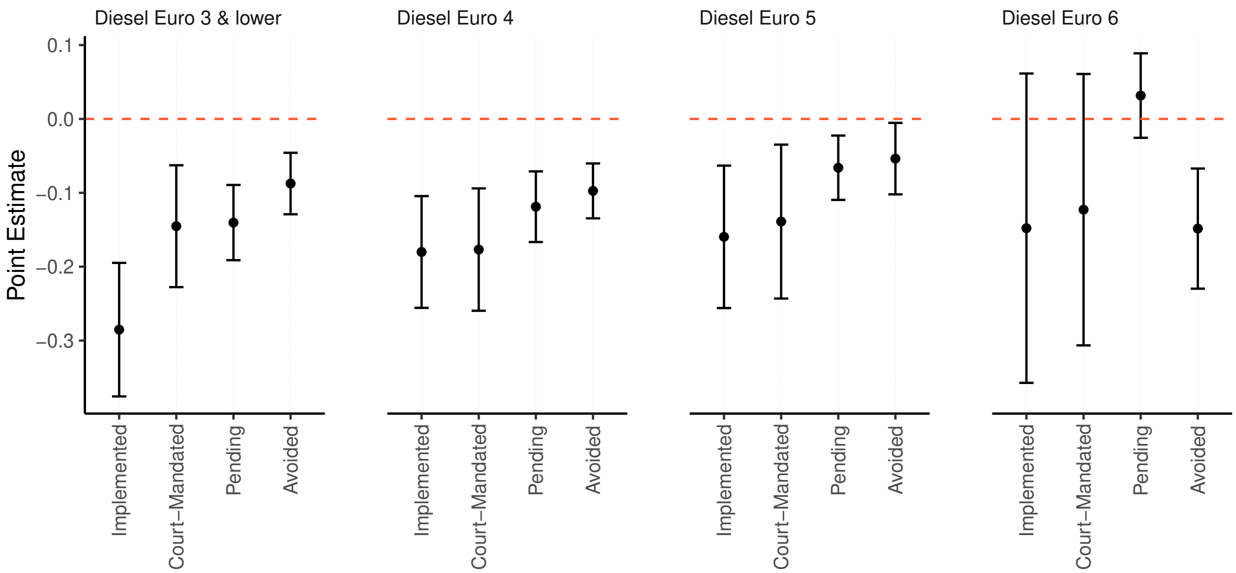
Notes: This figure reports CS-DD estimates of the effect of lawsuits filed by Deutsche Umwelthilfe (DUH) seeking diesel driving restrictions on used gasoline vehicle registrations, disaggregated by Euro emission standard. Each point represents the coefficient estimate from a separate regression using one of five alternative control group definitions: Full, Buffer, Doughnut, Large City, and Matching. All regressions include county fixed effects, and quarter-year fixed effects. Error bars represent the 95 percent confidence intervals with standard errors clustered at the county level.

Figure 7: Event Study of Used Gasoline Vehicle Registrations



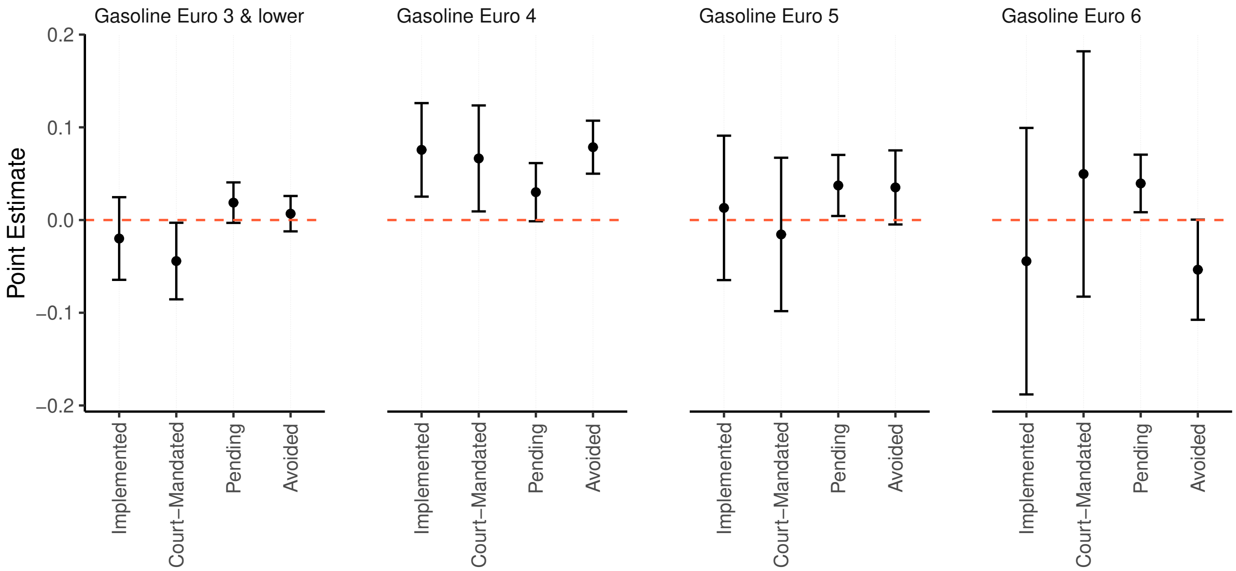
Notes: This figure presents event study estimates of the effect of lawsuits filed by Deutsche Umwelthilfe (DUH) on used gasoline vehicle registrations, disaggregated by emission standard. Each panel overlays results from two estimators: a dynamic version of the two-way fixed effects (TWFE) model estimated by OLS (in black square markers), and the estimator proposed by Callaway and Sant'Anna (2021) (in orange circle markers). The outcome variable is the log number of used gasoline vehicle registrations at the county-quarter level. The bars represent the 95 percent confidence intervals. Standard errors are clustered at the county level.

Figure 8: Effect of Lawsuits on Used Diesel Registrations by Final Legal Outcome



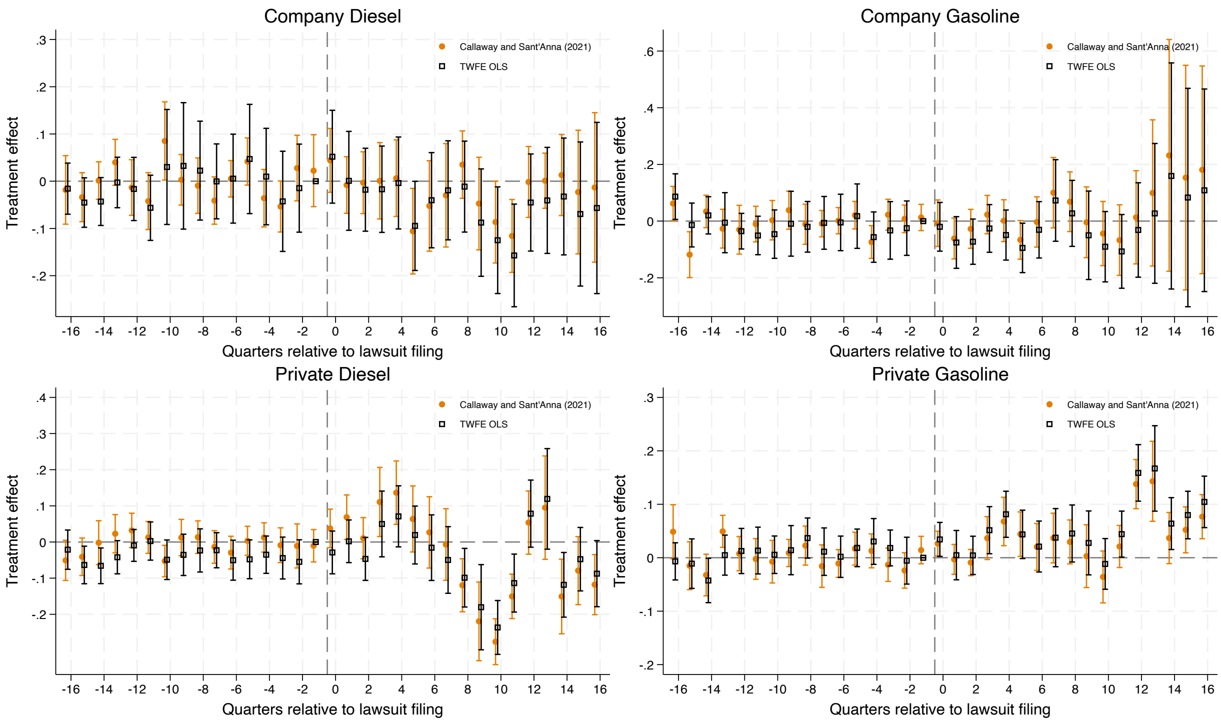
Notes: This figure presents Synthetic Difference-in-Differences (SDID) estimates of the effect of lawsuits filed by Deutsche Umwelthilfe (DUH) seeking diesel driving restrictions on used diesel vehicle registrations. Estimates are disaggregated by the final legal outcome of lawsuits as of Spring 2021 and by Euro emission standard. Lawsuits are grouped into four categories: *Implemented* (bans that were enforced), *Court-Mandated* (bans upheld by courts but not enforced), *Pending* (litigation ongoing), and *Avoided* (cases settled or withdrawn). For the Implemented category, the sample excludes periods after bans took effect to isolate the impact of lawsuits rather than enforcement. The outcome variable is the log number of used diesel registrations at the county-quarter level. The bars represent the 95 percent confidence intervals, with standard errors estimated by the placebo method.

Figure 9: Effect of Lawsuits on Used Gasoline Registrations by Final Legal Outcome



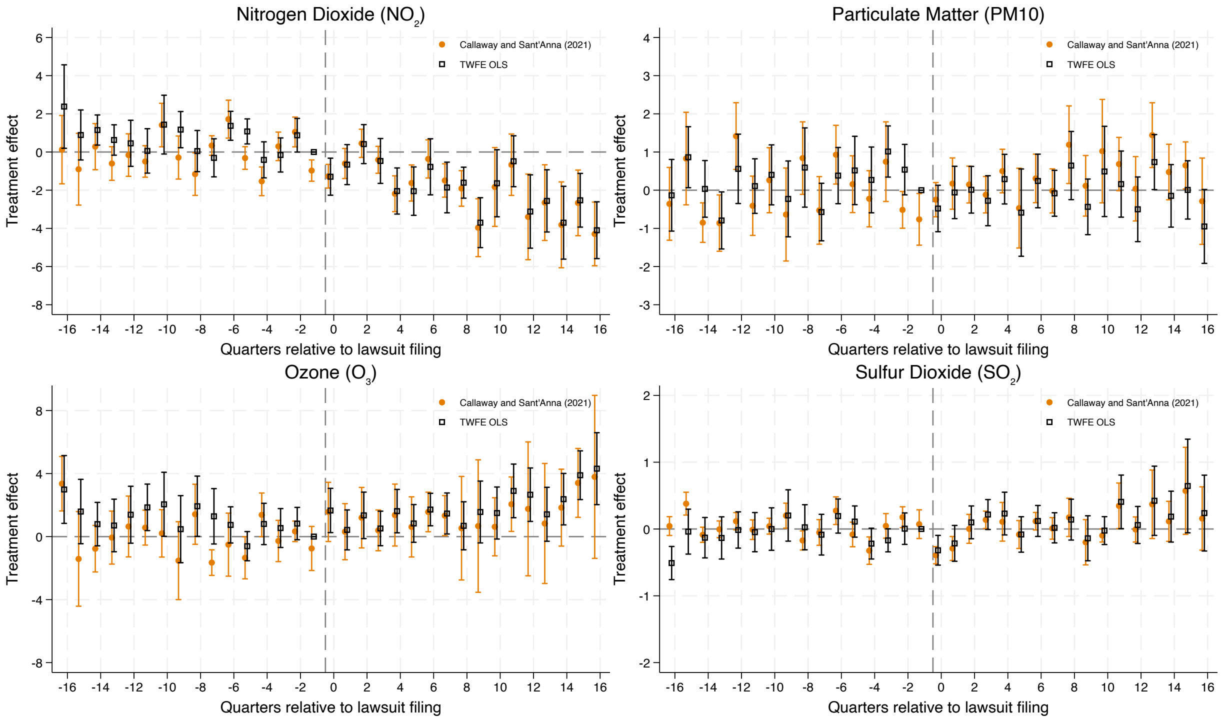
Notes: This figure presents Synthetic Difference-in-Differences (SDID) estimates of the effect of lawsuits filed by Deutsche Umwelthilfe (DUH) seeking diesel driving restrictions on used gasoline vehicle registrations. Estimates are disaggregated by the final legal outcome of lawsuits as of Spring 2021 and by Euro emission standard. Lawsuits are grouped into four categories: *Implemented* (bans that were enforced), *Court-Mandated* (bans upheld by courts but not enforced), *Pending* (litigation ongoing), and *Avoided* (cases settled or withdrawn). For the Implemented category, the sample excludes periods after bans took effect to isolate the impact of lawsuits rather than enforcement. The outcome variable is the log number of used gasoline registrations at the county-quarter level. The bars represent the 95 percent confidence intervals, with standard errors estimated by the placebo method.

Figure 10: Event Study of New Vehicle Registrations



Notes: This figure presents event study estimates of the effect of lawsuits filed by Deutsche Umwelthilfe (DUH) on new vehicle registrations, disaggregated by fuel type and ownership type. Each panel overlays results from two estimators: a dynamic version of the two-way fixed effects (TWFE) model estimated by OLS (in black square markers), and the estimator proposed by Callaway and Sant'Anna (2021) (in orange circle markers). The outcome variable is the log number of new vehicle registrations at the county-quarter level. The bars represent 95 percent confidence intervals. Standard errors are clustered at the county level.

Figure 11: Event Study of Air Pollution



Notes: This figure presents event study estimates of the effect of lawsuits filed by Deutsche Umwelthilfe (DUH) on air pollution. Each panel overlays results from two estimators: a dynamic version of the two-way fixed effects (TWFE) model estimated by OLS (in black square markers), and the estimator proposed by Callaway and Sant'Anna (2021) (in orange circle markers). The outcome variable is the average pollution level at the county-quarter level. The bars represent 95 percent confidence intervals. Standard errors are clustered at the county level.

Table 1: Impact of Lawsuits on Used Diesel Vehicle Registrations

	(1) Euro 1-3	(2) Euro 4	(3) Euro 5	(4) Euro 6
Panel A: Never-Treated Counties as the Control Group				
Lawsuit	-0.067***	-0.088***	-0.075***	-0.094***
	(0.021)	(0.017)	(0.021)	(0.027)
Outcome mean	380	560	818	339
Observations	9480	9480	9480	7900
Number of counties	395	395	395	395
Panel B: Not-Yet-Treated Couties as the Control Group				
Lawsuit	-0.066***	-0.088***	-0.074***	-0.093***
	(0.021)	(0.017)	(0.021)	(0.027)
Outcome mean	380	560	818	339
Observations	9480	9480	9480	7900
Number of counties	395	395	395	395

Notes: The unit of observation is a county-quarter-year. This table reports CS-DD estimates of the effect of lawsuits filed by Deutsche Umwelthilfe (DUH) seeking diesel driving restrictions on used diesel car registrations, disaggregated by emission standard. Specifically, it presents estimates of the coefficient β from equation (4). All regressions include county fixed effects and quarter-year fixed effects. In all panels, the outcome mean reports the mean of the dependent variable in the treated county prior to the filing of the first lawsuit. Standard errors in parentheses are clustered at the county level. Significance level denoted by *** p<0.01; ** p<0.05; * p<0.10.

Table 2: Impact of Lawsuits on Used Gasoline Vehicle Registrations

	(1) Euro 1-3	(2) Euro 4	(3) Euro 5	(4) Euro 6
Panel A: Never-Treated Kreise as the Control Group				
Lawsuit	-0.002	0.073***	0.034***	-0.026
	(0.009)	(0.013)	(0.011)	(0.020)
Outcome mean	2102	885	978	631
Observations	9480	9480	9480	7900
Number of counties	395	395	395	395
Panel B: Not-Yet-Treated Kreise as the Control Group				
Lawsuit	-0.003	0.072***	0.035***	-0.025
	(0.009)	(0.013)	(0.011)	(0.020)
Outcome mean	2102	885	978	631
Observations	9480	9480	9480	7900
Number of counties	395	395	395	395

Notes: The unit of observation is a county-quarter-year. This table reports CS-DD estimates of the effect of lawsuits filed by Deutsche Umwelthilfe (DUH) seeking diesel driving restrictions on used gasoline car registrations, disaggregated by emission standard. Specifically, it presents estimates of the coefficient β from equation (4). All regressions include county fixed effects and quarter-year fixed effects. In all panels, the outcome mean reports the mean of the dependent variable in the treated county prior to the filing of the first lawsuit. Standard errors in parentheses are clustered at the county level. Significance level denoted by *** p<0.01; ** p<0.05; * p<0.10.

Table 3: Spillover Effects of Lawsuits on Used Diesel Vehicle Registrations

	(1) Euro 1-3	(2) Euro 4	(3) Euro 5	(4) Euro 6
Panel A: Never-Treated Counties as the Control Group				
Lawsuit	-0.041***	-0.060***	-0.055***	-0.064***
	(0.009)	(0.010)	(0.011)	(0.015)
Outcome mean	267	387	631	297
Observations	8640	8640	8640	7200
Number of counties	360	360	360	360
Panel B: Not-Yet-Treated Couties as the Control Group				
Lawsuit	-0.041***	-0.057***	-0.052***	-0.066***
	(0.009)	(0.010)	(0.011)	(0.015)
Outcome mean	267	387	631	297
Observations	8640	8640	8640	7200
Number of counties	360	360	360	360

Notes: The unit of observation is a county-quarter-year. This table reports CS-DD estimates of the spillover effect of lawsuits filed by Deutsche Umwelthilfe (DUH) seeking diesel driving restrictions on used diesel car registrations, disaggregated by emission standard. Counties are defined as spillover-affected if they lie within 25 kilometers of a directly treated county but did not themselves receive any DUH-led lawsuits. Specifically, it presents estimates of the coefficient β from equation (4), where the treatment group includes these indirectly exposed counties. All regressions include county fixed effects and quarter-year fixed effects. In all panels, the outcome mean reports the mean of the dependent variable in the spillover counties prior to the filing of the first lawsuit in the nearest treated county. Standard errors in parentheses are clustered at the county level. Significance level denoted by *** p<0.01; ** p<0.05; * p<0.10.

Table 4: Spillover Effects of Lawsuits on Used Gasoline Vehicle Registrations

	(1)	(2)	(3)	(4)
	Euro 1-3	Euro 4	Euro 5	Euro 6
Panel A: Never-Treated Counties as the Control Group				
Lawsuit	-0.011*	0.018***	0.006	-0.010
	(0.006)	(0.017)	(0.008)	(0.010)
Outcome mean	1494	657	716	477
Observations	8640	8640	8640	7200
Number of counties	360	360	360	360
Panel B: Not-Yet-Treated Counties as the Control Group				
Lawsuit	-0.012**	0.017**	0.006	-0.013
	(0.006)	(0.007)	(0.008)	(0.010)
Outcome mean	1494	657	716	477
Observations	8640	8640	8640	7200
Number of counties	360	360	360	360

Notes: The unit of observation is a county-quarter-year. This table reports CS-DD estimates of the spillover effect of lawsuits filed by Deutsche Umwelthilfe (DUH) seeking diesel driving restrictions on used gasoline car registrations, disaggregated by emission standard. Counties are defined as spillover-affected if they lie within 25 kilometers of a directly treated county but did not themselves receive any DUH-led lawsuits. Specifically, it presents estimates of the coefficient β from equation (4), where the treatment group includes these indirectly exposed counties. All regressions include county fixed effects and quarter-year fixed effects. In all panels, the outcome mean reports the mean of the dependent variable in the spillover counties prior to the filing of the first lawsuit in the nearest treated county. Standard errors in parentheses are clustered at the county level. Significance level denoted by *** p<0.01; ** p<0.05; * p<0.10.

Table 5: Impact of Lawsuits on New Vehicle Registrations

	(1)	(2)	(3)	(4)
	Company Diesel	Company Gasoline	Private Diesel	Private Gasoline
Panel A: Never-Treated Counties as the Control Group				
Lawsuit	-0.016	0.013	0.029	0.033***
	(0.026)	(0.034)	(0.032)	(0.012)
Outcome mean	1356	1074	303	807
Observations	9480	9480	9480	9480
Number of counties	395	395	395	395
Panel B: Not-Yet-Treated Counties as the Control Group				
Lawsuit	-0.020	0.011	0.030	0.030***
	(0.024)	(0.031)	(0.032)	(0.011)
Outcome mean	1356	1074	303	807
Observations	9480	9480	9480	9480
Number of counties	395	395	395	395

Notes: The unit of observation is a county-quarter-year. This table reports CS-DD estimates of the effect of lawsuits filed by Deutsche Umwelthilfe (DUH) seeking diesel driving restrictions on new car registrations, disaggregated by fuel type and ownership. Specifically, it presents estimates of the coefficient β from equation (4). All regressions include county fixed effects and quarter-year fixed effects. In all panels, the outcome mean reports the mean of the dependent variable in the treated county prior to the filing of the first lawsuit. Standard errors in parentheses are clustered at the county level. Significance level denoted by *** $p<0.01$; ** $p<0.05$; * $p<0.10$.

Table 6: Impact of Lawsuits on Air Pollution

	(1)	(2)	(3)	(4)
	NO_2	PM_{10}	O_3	SO_2
Panel A: Never-Treated Cities as the Control Group				
Lawsuit	-1.413***	0.199	1.200	-0.005
	(0.371)	(0.200)	(0.930)	(0.062)
Outcome mean	35.298	20.964	44.778	2.036
Observations	9456	9456	9456	9456
Panel B: Not-Yet-Treated Cities as the Control Group				
Lawsuit	-1.386***	0.177	1.244	-0.006
	(0.369)	(0.199)	(0.925)	(0.062)
Outcome mean	35.298	20.964	44.778	2.036
Observations	9456	9456	9456	9456

Notes: The unit of observation is a county-quarter-year. This table reports CS-DD estimates of the effect of lawsuits filed by Deutsche Umwelthilfe (DUH) seeking diesel driving restrictions on air pollution. All regressions include county fixed effects and quarter-year fixed effects. In all panels, the outcome mean reports the mean of the dependent variable in the treated county prior to the filing of the first lawsuit. Standard errors in parentheses are clustered at the county level. Significance level denoted by *** p<0.01; ** p<0.05; * p<0.10.

Table 7. Potential vs. Actual Regulation: SDID Estimates by County, Fuel Type and Euro Standard

County	Euro	ATT (Pre-ban)	ATT (Full)	Mean (Pre-ban)	Mean (Post-ban)
Panel A: Used Diesel Vehicle Registrations					
Stuttgart	1–3	-0.295***	-0.586***	-0.295	-1.533
Stuttgart	4	-0.270***	-0.550***	-0.270	-1.460
Stuttgart	5	-0.243***	-0.336***	-0.245	-0.634
Stuttgart	6	-0.094	-0.110	-0.094	-0.163
Darmstadt	1–3	-0.369***	-0.404***	-0.368	-0.574
Darmstadt	4	-0.140*	-0.227***	-0.140	-0.635
Darmstadt	5	-0.177**	-0.244**	-0.178	-0.553
Darmstadt	6	-0.348**	-0.350**	-0.348	-0.358
Berlin	1–3	-0.248***	-0.266***	-0.248	-0.515
Berlin	4	-0.188**	-0.204**	-0.188	-0.424
Berlin	5	-0.072	-0.077	-0.072	-0.148
Berlin	6	-0.032	-0.036	-0.032	-0.091
Panel B: Used Gasoline Vehicle Registrations					
Stuttgart	1–3	0.006	0.035	0.007	0.127
Stuttgart	4	0.133**	0.177***	0.132	0.320
Stuttgart	5	0.006	0.020	0.006	0.068
Stuttgart	6	-0.024	-0.007	-0.024	0.047
Darmstadt	1–3	-0.017	-0.017	-0.017	-0.018
Darmstadt	4	0.063	0.066	0.062	0.086
Darmstadt	5	0.041	0.046	0.041	0.070
Darmstadt	6	-0.028	-0.030	-0.029	-0.034
Berlin	1–3	-0.047	-0.049	-0.047	-0.081
Berlin	4	0.064	0.066	0.063	0.100
Berlin	5	-0.004	-0.002	-0.004	0.020
Berlin	6	-0.085	-0.087	-0.084	-0.120

Notes: The unit of observation is a county–quarter–year. The dependent variable is the log of used-vehicle registrations. Each entry reports synthetic difference-in-differences (SDID) estimates for treated counties (Stuttgart, Darmstadt, and Berlin). ATT (Pre-ban) reports the estimated average treatment effect of lawsuits in the period before the implementation of the diesel ban, capturing responses to the potential regulation. ATT (Full) reports the total average treatment effect over the entire sample period, combining the effects of both lawsuits and subsequent bans. Mean (Pre-ban) and Mean (Post-ban) report the average event-study coefficients for quarters after the lawsuit filing but before and after the start of enforcement, respectively, summarizing the dynamics of adjustment across the two phases. Significance level denoted by *** $p<0.01$; ** $p<0.05$; * $p<0.10$.

Table 8: Impact of Diesel Bans on Used Car Asking Prices

	(1)	(2)	(3)	(4)	(5)	(6)
	log(price)	log(price)	log(price)	log(price)	log(price)	log(price)
Ban	-0.007	-0.008	-0.005	0.031	0.020*	0.019*
	(0.007)	(0.006)	(0.006)	(0.021)	(0.011)	(0.011)
Ban x Affected Diesel	-0.050***	-0.051***	-0.051***	-0.038***	-0.037***	-0.033***
	(0.013)	(0.016)	(0.013)	(0.009)	(0.011)	(0.010)
Car Age	-0.125***	-0.127***	-0.127***	-0.137***	-0.139***	-0.139***
	(0.000)	(0.000)	(0.000)	(0.001)	(0.001)	(0.001)
Diesel	0.131***	0.126***	0.121***	0.115***	0.105***	0.105***
	(0.003)	(0.002)	(0.002)	(0.005)	(0.003)	(0.003)
Diesel x Car Age	-0.019***	-0.018***	-0.018***	-0.022***	-0.021***	-0.021***
	(0.000)	(0.000)	(0.000)	(0.001)	(0.000)	(0.000)
Mileage	-0.015***	-0.015***	-0.015***	-0.014***	-0.014***	-0.014***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Make x Model	Y			Y		
County	Y			Y		
Month x Year	Y	Y		Y	Y	
County x Make x Model		Y	Y		Y	Y
Make x Model x Quarter x Year			Y			Y
Observations	24M	24M	24M	5.68M	5.68M	5.68M
R-squared	0.901	0.912	0.914	0.899	0.915	0.916

Notes: The unit of observation is a vehicle listing. The dependent variable is the logarithm of the asking price for used cars listed on AutoScout24. All regressions include controls for body type and seller type (dealer or private). Standard errors in parentheses are clustered at the county level. The first three columns use data from 2019 to 2024; the last three columns restrict the sample to listings from 2019 only, excluding the COVID-19 period. Significance level denoted by *** p<0.01; ** p<0.05; * p<0.10.

Table 9: Impact of Diesel Bans on Used Car Listing Volume

	(1)	(2)	(3)	(4)	(5)	(6)
	listing	listing	listing	listing	listing	listing
Ban	-0.181***	-0.204***	-0.225***	-0.050	-0.029	-0.057
	(0.068)	(0.076)	(0.066)	(0.089)	(0.069)	(0.069)
Ban x Affected Diesel	-0.183 ***	-0.203***	-0.203***	-0.221***	-0.232***	-0.230***
	(0.029)	(0.033)	(0.034)	(0.030)	(0.037)	(0.028)
Car Age	-0.054***	-0.051***	-0.052***	-0.062***	-0.057***	-0.062***
	(0.003)	(0.002)	(0.003)	(0.003)	(0.003)	(0.003)
Diesel	0.070**	0.085***	0.068**	0.079*	0.100***	0.067
	(0.029)	(0.022)	(0.028)	(0.042)	(0.029)	(0.044)
Diesel x Car Age	-0.016***	-0.019***	-0.015***	-0.021***	-0.027***	-0.020***
	(0.003)	(0.002)	(0.002)	(0.002)	(0.005)	(0.002)
Make x Model	Y			Y		
County	Y			Y		
Quarter x Year	Y	Y		Y	Y	
County x Make x Model		Y	Y		Y	Y
Make x Model x Quarter x Year			Y			Y
Observations	10.7M	10.7M	10.7M	2.1M	2.1M	2.1M

Notes: The dependent variable is the quarterly number of used car listings, aggregated at the make × model × age group within each county and quarter, and estimated using Poisson pseudo–maximum likelihood (PPML). Standard errors in parentheses are clustered at the county level. The first three columns use data from 2019 to 2024; the last three columns restrict the sample to listings from 2019 only, excluding the COVID-19 period. Significance level denoted by *** p<0.01; ** p<0.05; * p<0.10.

Appendix A. Additional figures and tables

Table A.1: European Union emission regulations for passenger cars

Stage	Date	CO	HC	HC+NOx	NOx	PM	PN†
		g/km					#/km
Positive Ignition (Gasoline)							
Euro 1‡	1992.07	2.72 (3.16)	-	0.97 (1.13)	-	-	-
Euro 2	1996.01	2.2	-	0.5	-	-	-
Euro 3	2000.01	2.30	0.20	-	0.15	-	-
Euro 4	2005.01	1.0	0.10	-	0.08	-	-
Euro 5	2009.09[b]	1.0	0.10[d]	-	0.06	0.005[e,f]	-
Euro 6	2014.09	1.0	0.10[d]	-	0.06	0.005[e,f]	6.0×10^{11} [e,g]
Euro 7	2026.11.29	1.0	0.10[d]	-	0.06	0.0045	6.0×10^{11} [e,g]
Compression Ignition (Diesel)							
Euro 1‡	1992.07	2.72 (3.16)	-	0.97 (1.13)	-	0.14 (0.18)	-
Euro 2, IDI	1996.01	1.0	-	0.7	-	0.08	-
Euro 2, DI	1996.01[a]	1.0	-	0.9	-	0.10	-
Euro 3	2000.01	0.64	-	0.56	0.50	0.05	-
Euro 4	2005.01	0.50	-	0.30	0.25	0.025	-
Euro 5a	2009.09[b]	0.50	-	0.23	0.18	0.005[f]	-
Euro 5b	2011.09[c]	0.50	-	0.23	0.18	0.005[f]	6.0×10^{11}
Euro 6	2014.09	0.50	-	0.17	0.08	0.005[f]	6.0×10^{11}
Euro 7	2026.11.29	0.50	-	0.17	0.08	0.0045	6.0×10^{11}

* At the Euro 1..4 stages, passenger vehicles > 2,500 kg were type approved as Category N_1 vehicles
† Euro 5-6: particles > 23 nm; Euro 7: particles > 10 nm
‡ Values in brackets are conformity of production (COP) limits
a. until 1999.09.30 (after that date DI engines must meet the IDI limits)
b. 2011.01 for all models
c. 2013.01 for all models
d. and NMHC = 0.068 g/km
e. applicable only to vehicles using DI engines
f. 0.0045 g/km using the PMP measurement procedure
g. 6.0×10^{12} 1/km within first three years from Euro 6 effective dates

Notes: Source: https://dieselnet.com/standards/eu/ld.php

Table A.2: Emission standards for LEZ sticker categories

	No sticker	Red	Yellow	Green
Diesel	Euro 1 or older	Euro 2/Euro 1 with particle filter	Euro 3/Euro 2 with particle filter	Euro 4 or better/Euro 3 with particle filter
Gasoline	Without catalytic converter	-	-	Euro 1 with catalytic converter or better

Table A.3: Impact of Lawsuits on Used Vehicle Registrations

	(1) Euro 1-3	(2) Euro 4	(3) Euro 5	(4) Euro 6
A. Used diesel vehicle registrations				
Lawsuit	-0.143***	-0.128***	-0.114***	-0.077***
	(0.021)	(0.023)	(0.022)	(0.026)
Outcome mean	380	560	818	339
Observations	9480	9480	9480	7900
Number of counties	395	395	395	395
R-squared	0.976	0.980	0.977	0.980
B. Used gasoline vehicle registrations				
Lawsuit	0.004	0.099***	0.030**	-0.004
	(0.007)	(0.011)	(0.012)	(0.017)
Outcome mean	2102	885	978	631
Observations	9480	9480	9480	7900
Number of counties	395	395	395	395
R-squared	0.993	0.988	0.987	0.989

Notes: The unit of observation is a county-quarter-year. This table reports two-way fixed effects (TWFE) estimates of the effect of lawsuits filed by Deutsche Umwelthilfe (DUH) seeking diesel driving restrictions on used car registrations, disaggregated by fuel type and emission standard. Specifically, it presents estimates of the coefficient β from equation (1). All regressions include county fixed effects and quarter-year fixed effects. In all panels, the outcome mean reports the mean of the dependent variable in the treated county prior to the filing of the first lawsuit. Standard errors in parentheses are clustered at the county level. Significance level denoted by *** $p<0.01$; ** $p<0.05$; * $p<0.10$.

Table A.4: Impact of Lawsuits on Used Vehicle Registrations

	(1)	(2)	(3)	(4)
	Euro 1-3	Euro 4	Euro 5	Euro 6
Outcome: Used Diesel Vehicle Registration				
Lawsuit	-0.107***	-0.116***	-0.072***	-0.114***
	(0.016)	(0.016)	(0.019)	(0.029)
Outcome mean	380	560	818	339
Observations	9480	9480	9480	7900
Number of counties	395	395	395	395
Outcome: Used Gasoline Vehicle Registration				
Lawsuit	-0.000	0.069***	0.027*	-0.018
	(0.008)	(0.012)	(0.015)	(0.024)
Outcome mean	2102	885	978	631
Observations	9480	9480	9480	7900
Number of counties	395	395	395	395

Notes: The unit of observation is a county-quarter-year. This table reports Synthetic Difference-in-Differences (SDID) estimates of the effect of lawsuits filed by Deutsche Umwelthilfe (DUH) seeking diesel driving restrictions on used car registrations, disaggregated by fuel type and emission standard. Specifically, it presents estimates of the coefficient β from equation (4). All regressions include county fixed effects and quarter-year fixed effects. In all panels, the outcome mean reports the mean of the dependent variable in the treated county prior to the filing of the first lawsuit. Standard errors, shown in parentheses, are obtained from placebo inference based on repeated reassignments of treatment status. Significance level denoted by *** p<0.01; ** p<0.05; * p<0.10.

Table A.5: Impact of Lawsuits on Air Pollution

	(1)	(2)	(3)	(4)	(5)	(5)
	NO_2	PM_{10}	O_3	AQI	NO_2 Annual	Satellite NO_2
Panel A: Never-Treated Cities as the Control Group						
Lawsuit	-2.379***	0.535*	0.772	-3.501***	-1.629***	-3.090**
	(0.483)	(0.273)	(1.197)	(1.088)	(0.406)	(1.254)
Outcome mean	37.549	21.237	43.924	47.767	37.554	19.638
Observations	5429	5028	4917	5750	1355	2370
Panel B: Not-Yet-Treated Cities as the Control Group						
Lawsuit	-2.281***	0.484*	0.838	-3.426***	-1.601***	-3.071**
	(0.475)	(0.274)	(1.192)	(1.097)	(0.402)	(1.255)
Outcome mean	37.549	21.237	43.924	47.767	37.554	19.638
Observations	5429	5028	4917	5750	1355	2370

Notes: The unit of observation is a county-quarter-year. This table reports CS-DD estimates of the effect of lawsuits filed by Deutsche Umwelthilfe (DUH) seeking diesel driving restrictions on air pollution. All regressions include county fixed effects and quarter-year fixed effects. In all panels, the outcome mean reports the mean of the dependent variable in the treated county prior to the filing of the first lawsuit. Standard errors in parentheses are clustered at the county level. Significance level denoted by *** p<0.01; ** p<0.05; * p<0.10.

Figure A.1: Annual Registrations of New and Used Passenger Cars in Germany (2014–2019)

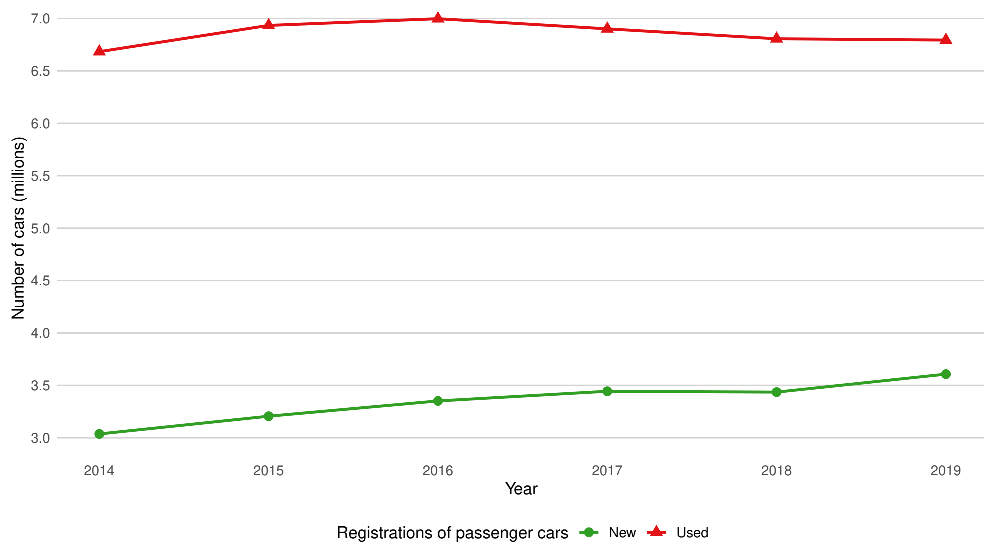


Figure A.2: Fuel Type Distribution of Used Passenger Cars in Germany (2014–2019)

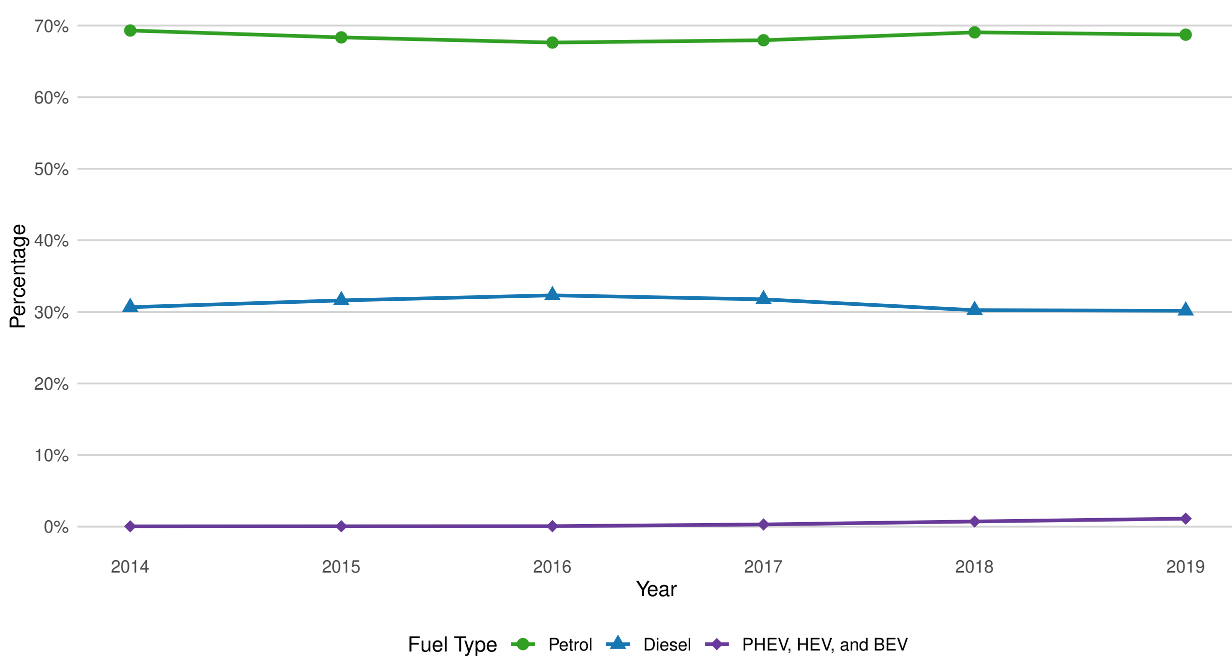


Figure A.3: Fuel Type Distribution of New Passenger Cars in Germany (2014–2019)

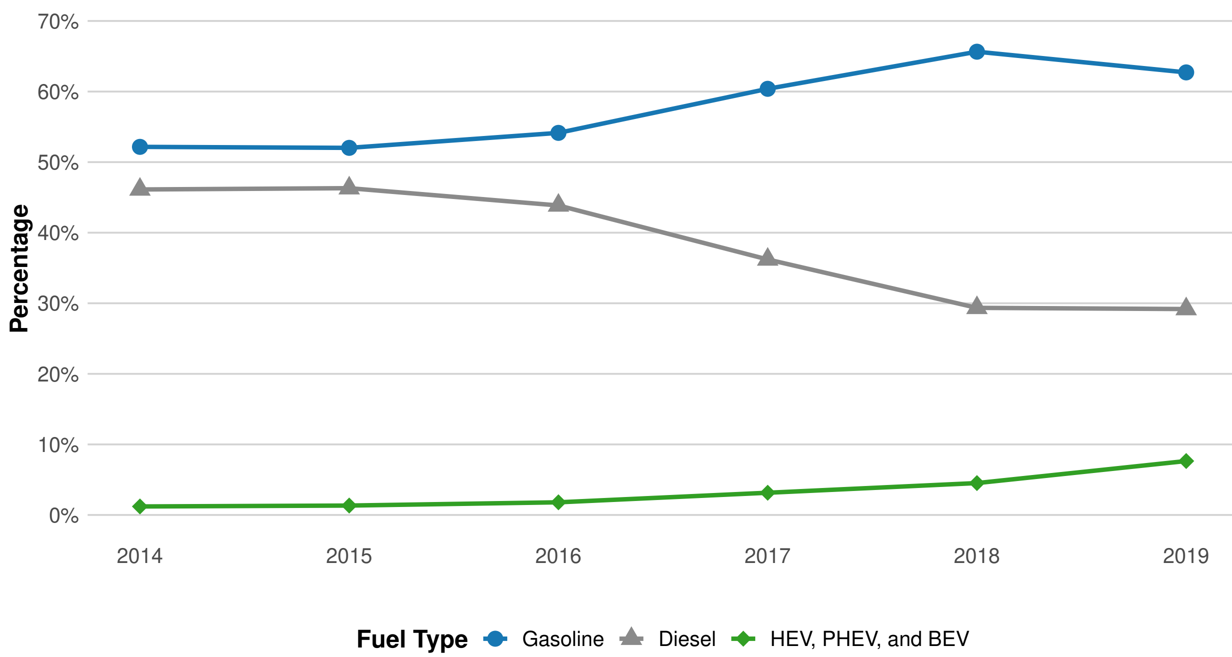
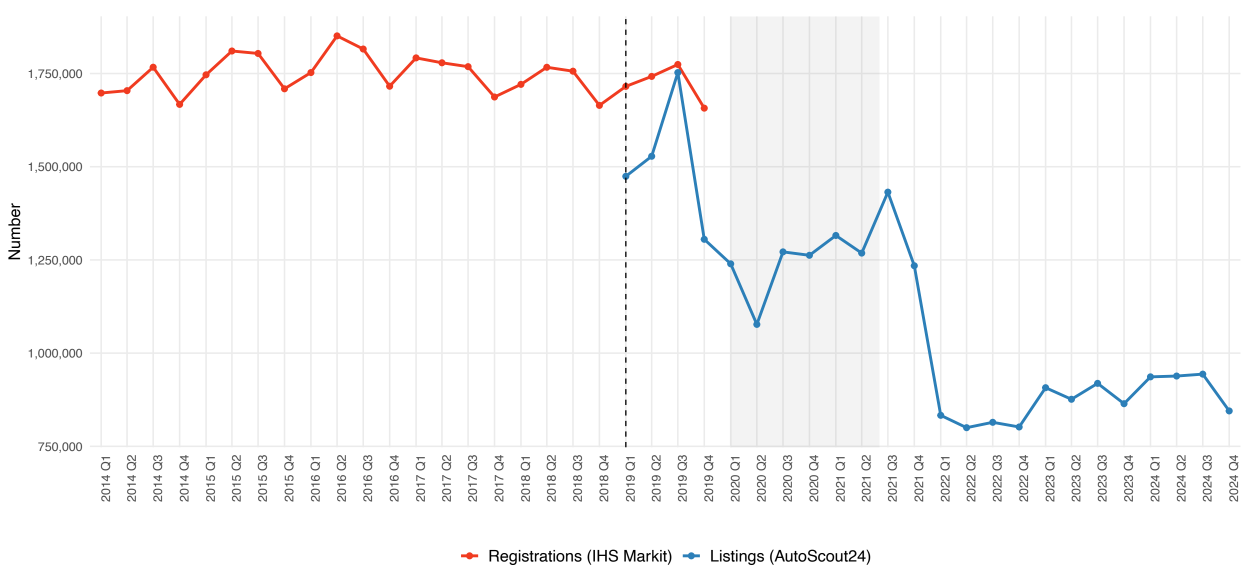


Figure A.4: Quarterly Used Car Listings and Registrations in Germany (2014–2024)



Note: The figure shows the quarterly total number of used car registrations (IHS Markit, red line) and online used car listings (AutoScout24, blue line). The dashed line marks the beginning of AutoScout24 coverage (2019 Q1), and the shaded area indicates the main COVID disruption (from Q1 2020 to Q2 2021).

Figure A.5: Pre- and Post-treatment Averages of Quarterly Used Car Registrations for Treated and Control Counties (2014–2019)

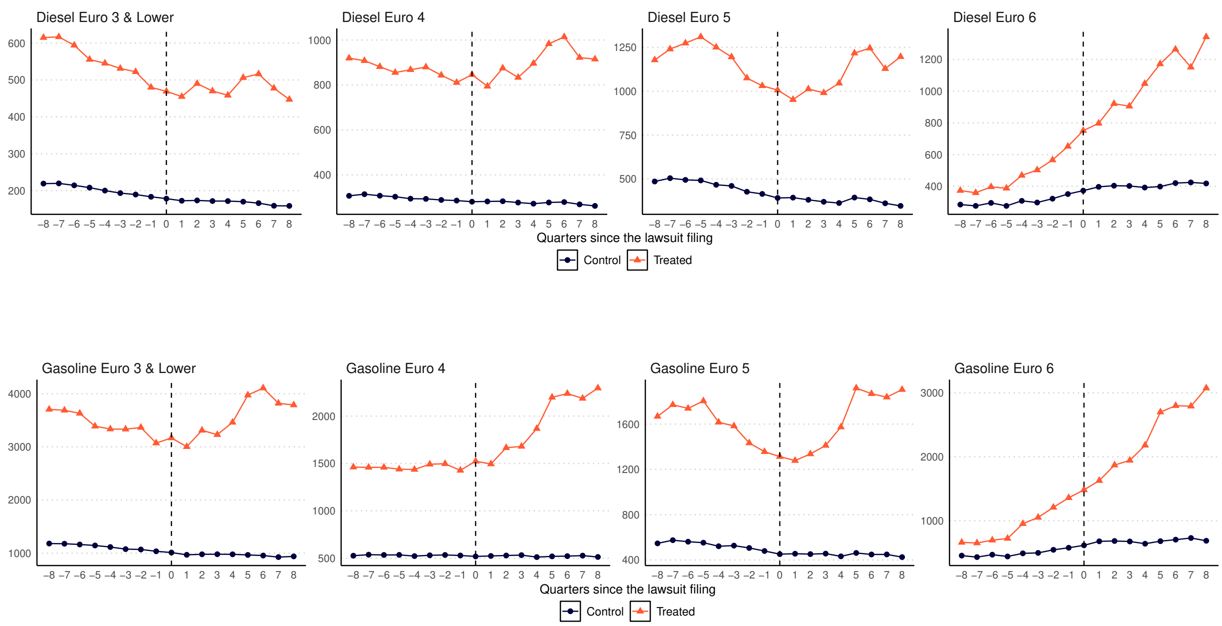


Figure A.6: Pre- and Post-treatment Averages of Quarterly New Car Registrations for Treated and Control Counties (2014–2019)

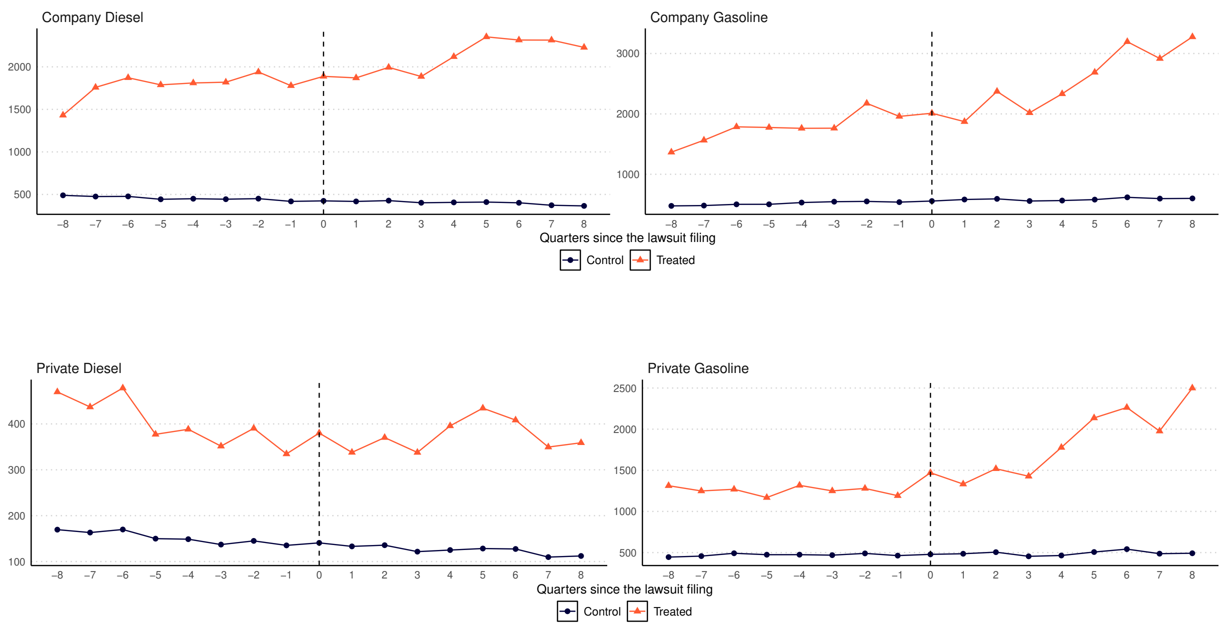


Figure A.7: Pre- and Post-treatment Averages of Quarterly Air Pollution Levels for Treated and Control Counties (2014–2019)

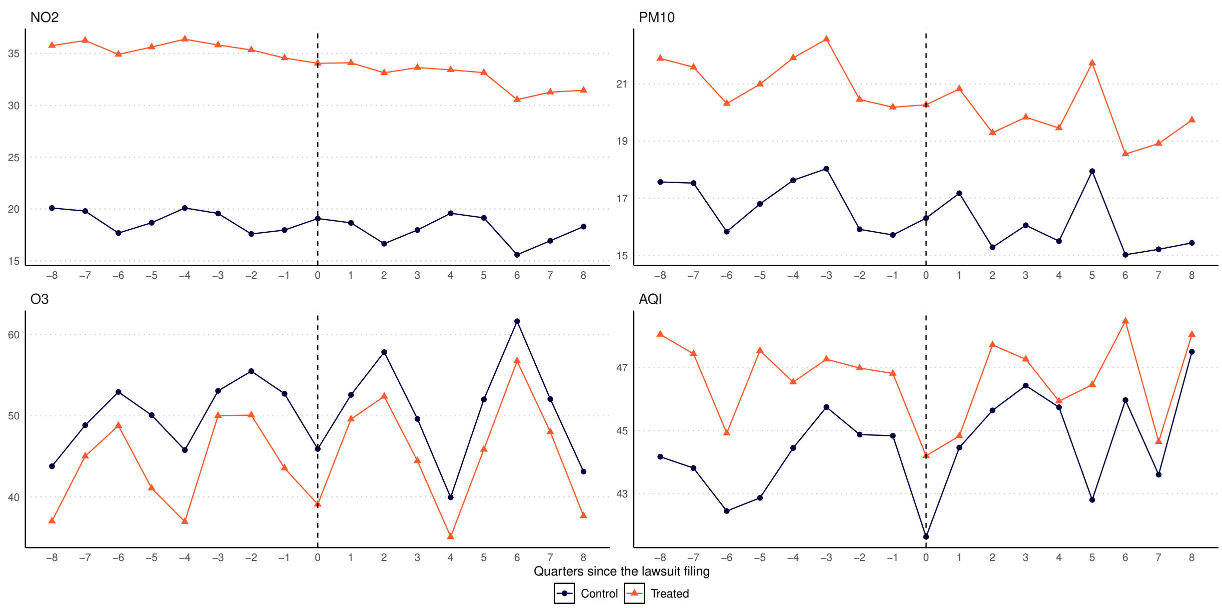
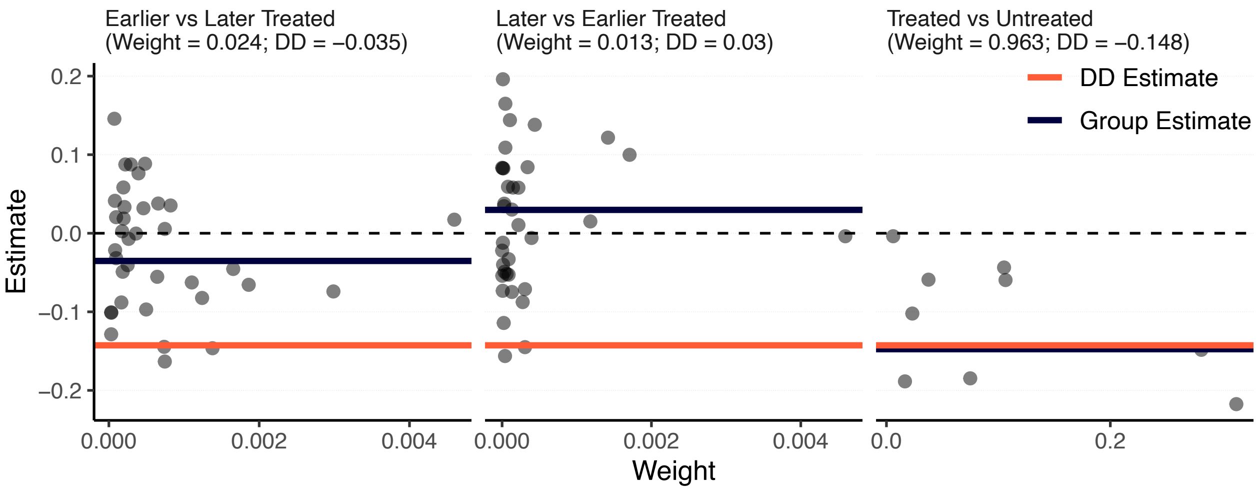
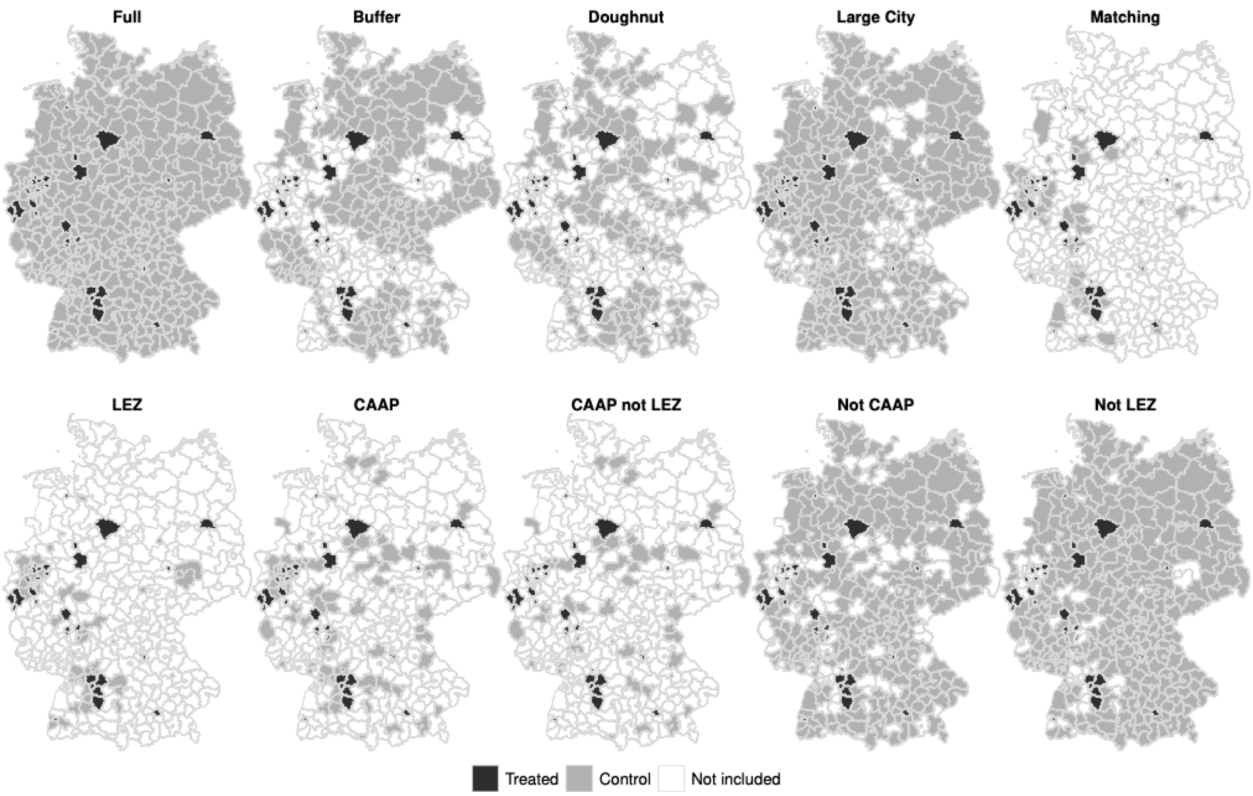


Figure A.8: Goodman-Bacon decomposition for the treatment effect on used diesel Euro 3 registrations



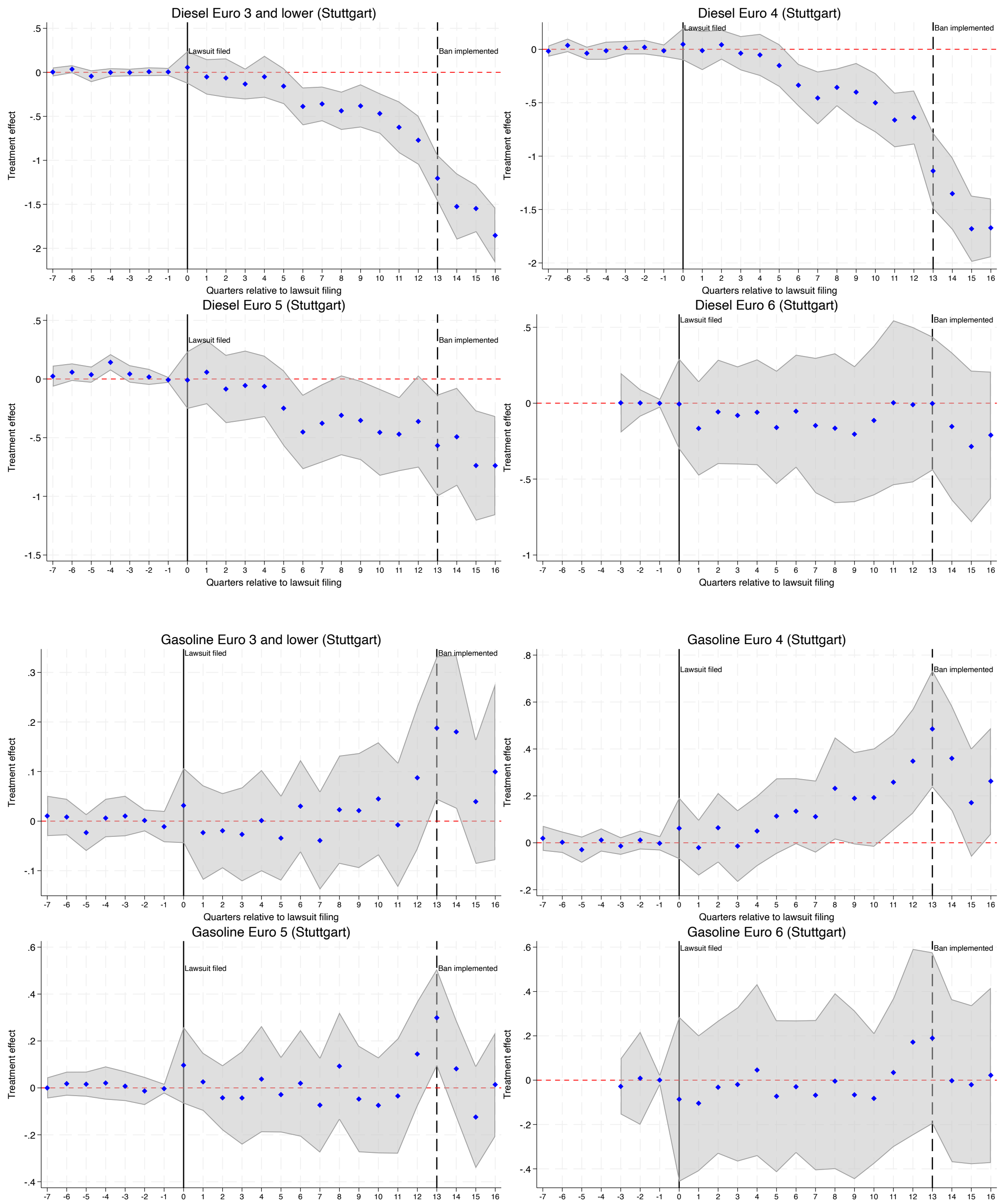
Notes: The figure plots the Goodman-Bacon (2021) decomposition of the TWFE-DD estimates for the impact of lawsuits seeking diesel driving restrictions on used diesel Euro 3 registrations. Each dot corresponds to a two-group two-period comparison with point estimates depicted on the y-axis and weights on the x-axis. The orange line is the TWFE-DD estimate and equals the sum of y-axis values weighted by x-axis values. The solid black lines represent a weighted average of comparisons within each comparison group.

Figure A.9: Spatial Distribution of Treated and Control Counties Across Alternative Sample Specifications



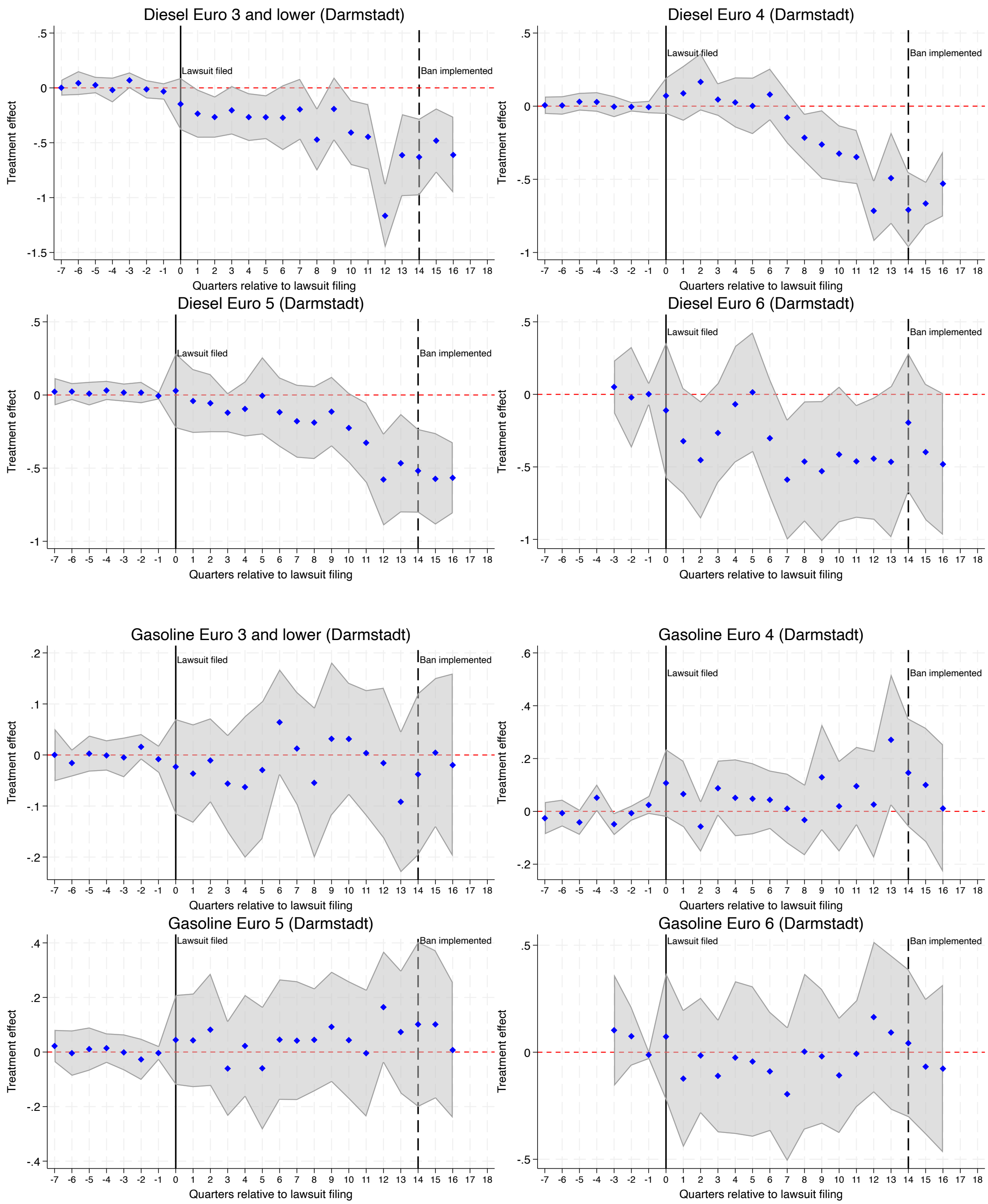
Notes: The maps display the spatial distribution of treated and control counties across alternative sample specifications used in the robustness analysis. Treated counties (dark gray) are those where DUH lawsuits were filed. Control counties (medium gray) vary across specifications. Counties not included in the sample are shown in white. Full includes all never treated counties. Buffer excludes counties located within 25 km of treated counties. Doughnut restricts the control group to counties located 25 to 75 km from treated counties. Large City limits the sample to metropolitan areas. Matching uses matched counties selected on observable characteristics. LEZ and CAAP condition on the presence of a low-emission zone or clean air action plan, while Not LEZ and Not CAAP exclude such counties.

Figure A.10: Dynamic Effects of Potential and Actual Regulation on Used Vehicle Registrations in Stuttgart



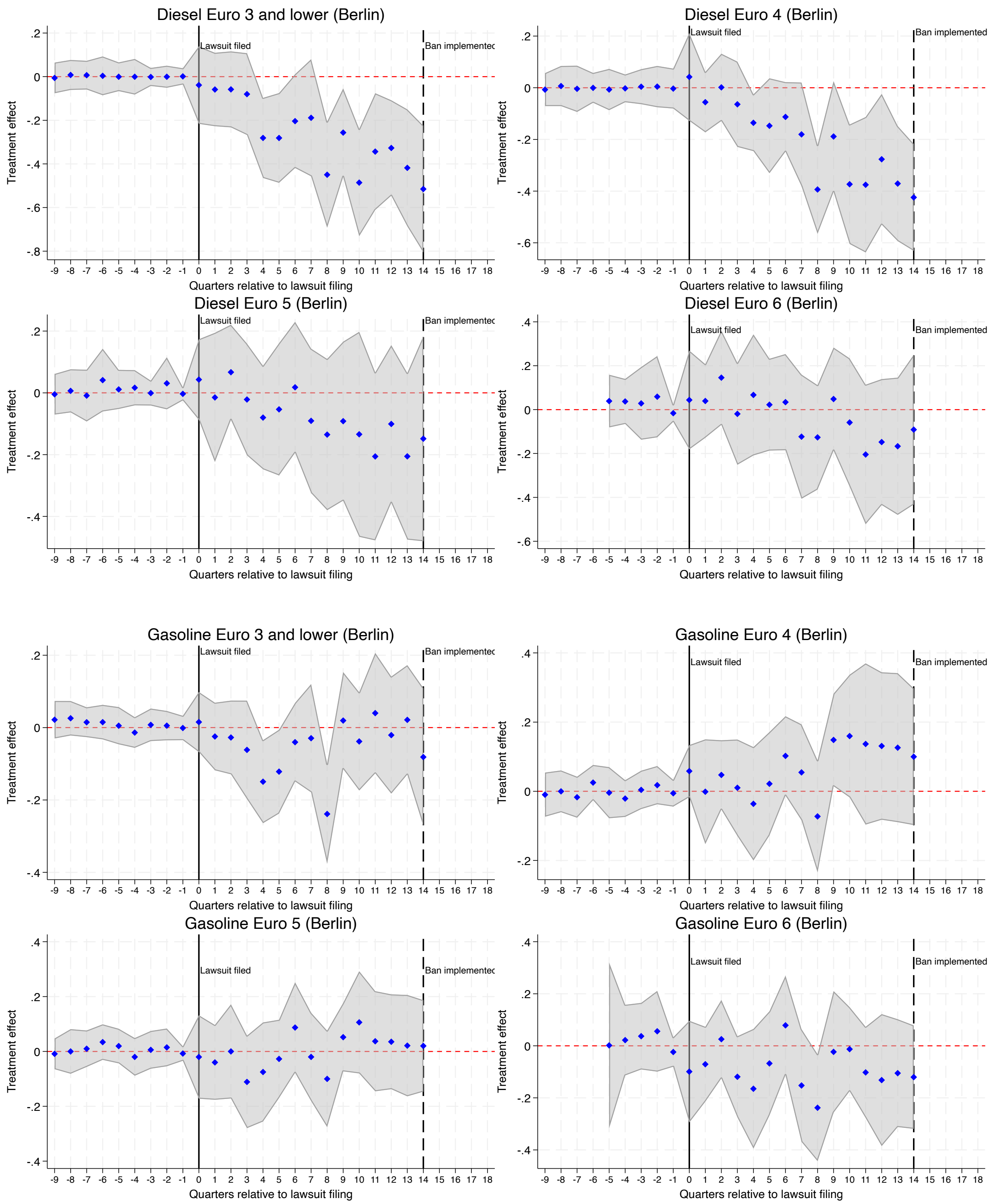
Notes: Each panel reports synthetic difference-in-differences (SDID) event-study estimates for the log of used-vehicle registrations in Stuttgart, by fuel type and Euro standard. The solid vertical line marks the filing of the diesel-ban lawsuit (potential regulation), and the dashed vertical line marks the implementation of the diesel ban (actual regulation). Shaded bands denote placebo-based 95 percent confidence intervals.The x-axis measures quarters relative to the lawsuit filing, and the y-axis reports estimated treatment effects relative to the pre-lawsuit baseline.

Figure A.11: Dynamic Effects of Potential and Actual Regulation on Used Vehicle Registrations in Darmstadt



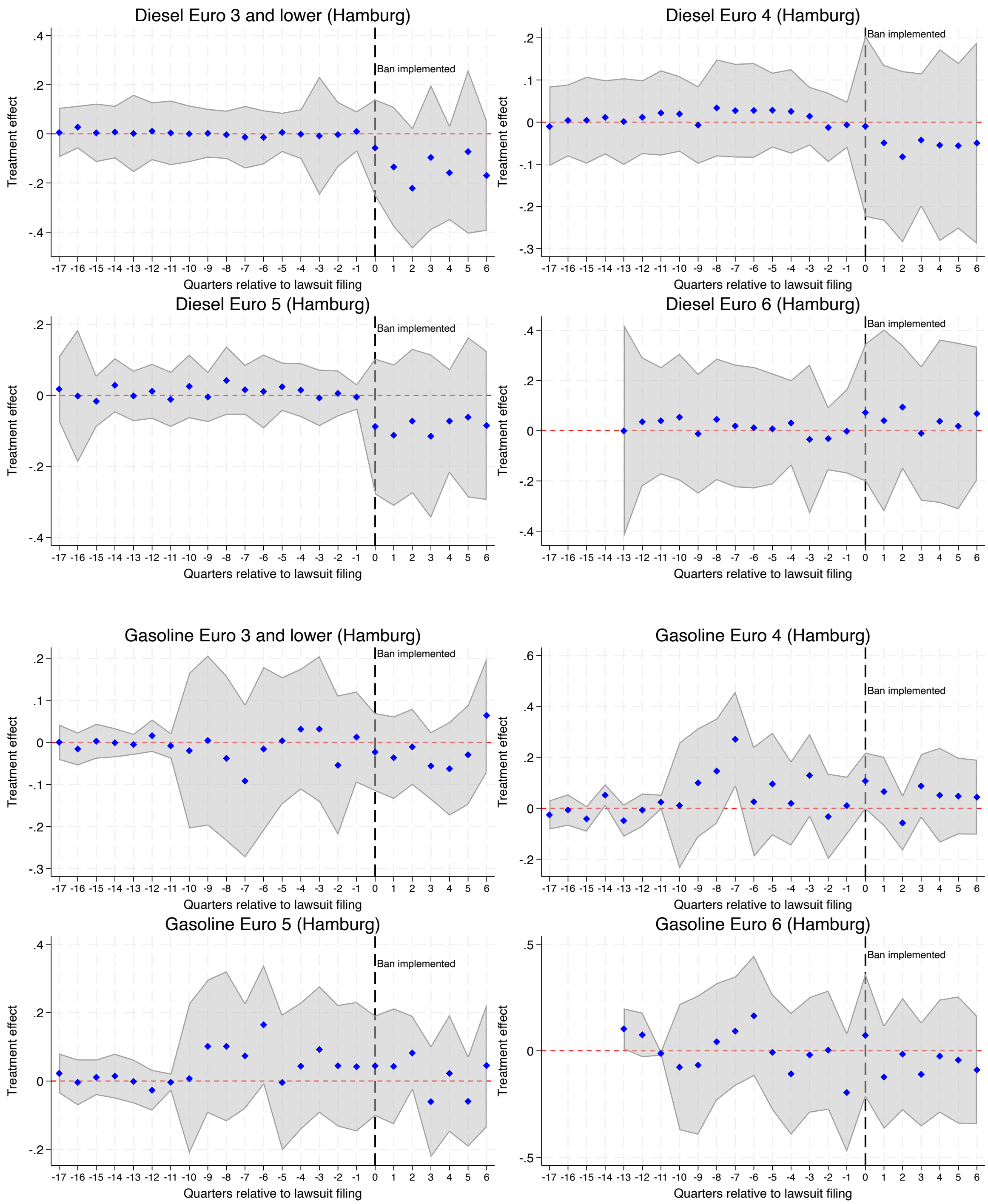
Notes: Each panel reports synthetic difference-in-differences (SDID) event-study estimates for the log of used-vehicle registrations in Darmstadt, by fuel type and Euro standard. The solid vertical line marks the filing of the diesel-ban lawsuit (potential regulation), and the dashed vertical line marks the implementation of the diesel ban (actual regulation). Shaded bands denote placebo-based 95 percent confidence intervals.The x-axis measures quarters relative to the lawsuit filing, and the y-axis reports estimated treatment effects relative to the pre-lawsuit baseline.

Figure A.12: Dynamic Effects of Potential and Actual Regulation on Used Vehicle Registrations in Berlin



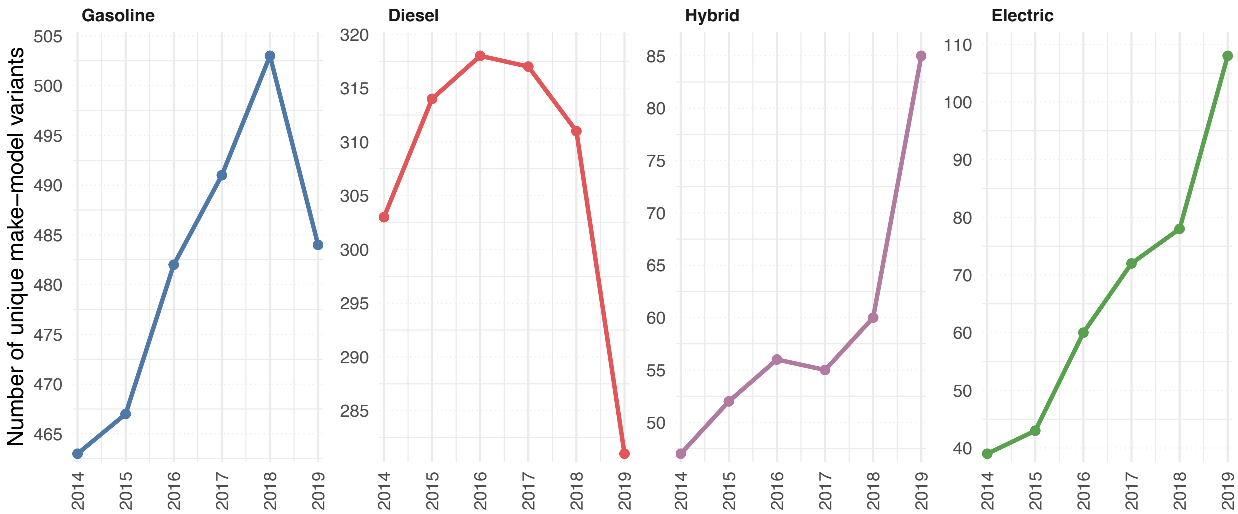
Notes: Each panel reports synthetic difference-in-differences (SDID) event-study estimates for the log of used-vehicle registrations in Berlin, by fuel type and Euro standard. The solid vertical line marks the filing of the diesel-ban lawsuit (potential regulation), and the dashed vertical line marks the implementation of the diesel ban (actual regulation). Shaded bands denote placebo-based 95 percent confidence intervals.The x-axis measures quarters relative to the lawsuit filing, and the y-axis reports estimated treatment effects relative to the pre-lawsuit baseline.

Figure A.13: Dynamic Effects of Actual Regulation on Used Vehicle Registrations in Hamburg



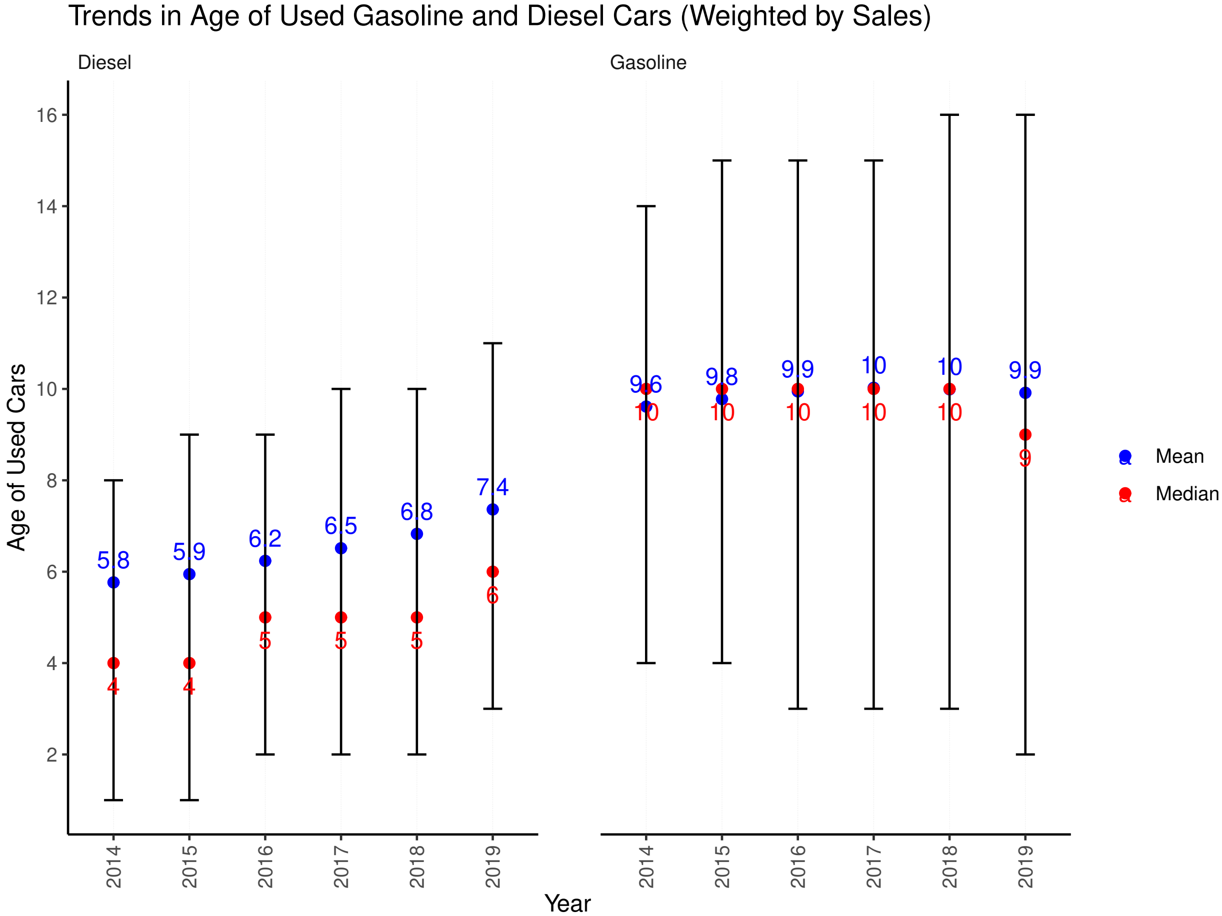
Notes: Each panel reports synthetic difference-in-differences (SDID) event-study estimates for the log of used-vehicle registrations in Hamburg, by fuel type and Euro standard. The dashed vertical line marks the start of the diesel ban (actual regulation). Because the ban preceded any DUH-led litigation, the estimates capture the direct impact of actual regulation. Shaded bands denote placebo-based 95 percent confidence intervals. The x-axis measures quarters relative to the ban implementation, and the y-axis reports estimated treatment effects relative to the pre-ban baseline.

Figure A.14: Trends in the Number of Make–Model Variants by Fuel Type in the New Car Market, 2014–2019



Notes: This figure shows the number of unique make–model variants with new car registrations in Germany between 2014 and 2019, broken down by fuel type (gasoline, diesel, electric, and hybrid). A variant is defined as a unique make–model combination in each year.

Figure A.15: Trends in Sales-Weighted Age of Used Cars by Fuel Type



Notes: This figure plots the trends in the age of used gasoline and diesel cars from 2014 to 2019, weighted by sales. The blue dots indicate the sales-weighted mean age, and the red dots represent the median age. The vertical bars span the interquartile range (25th to 75th percentile) of the sales-weighted age distribution for each year.