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Do lenders price diesel risk? Evidence from Dieselgate and low-emission zones in captive vs. independent banks

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Abstract

Transitioning to a sustainable economy and reducing air pollution hinge on appropriate economic incentives and financing conditions. The auto loan market offers a prime setting, as lenders' credit terms can either discourage or incentivize the purchase of high-pollution vehicles. Using loan-level data, we examine how captive and independent banks adjust lending conditions in response to information and regulatory shocks affecting diesel vehicles. Exploiting the 2015 diesel emissions scandal and the introduction of local circulation restrictions, we show that lending responses differ systematically across lender types, with captive banks tending to weaken, rather than reinforce, the effectiveness of environmental regulation for air pollution.

JEL classification: G21; G51; Q53; Q58.

Keywords: Car Loans; Captive Banks; Independent Banks; Diesel Emissions Scandal; Car Circulation Restrictions.

Non-technical summary

Environmental risks and their adverse health impacts have led governments and supranational authorities to adopt increasingly stringent policies aimed at reducing pollution. The transportation sector has been a central target of these efforts, as diesel vehicles are a major source of hazardous air pollutants such as nitrogen dioxide (NO_2) and particulate matter. Regulatory measures have included tighter emission standards and local circulation restrictions for polluting vehicles, with the objective of internalizing environmental externalities by increasing the private costs of owning and operating high-emission cars.

The effectiveness of such environmental policies depends on how these regulatory measures are transmitted through economic channels and incentives. The financial sector plays a key role in this process, as credit conditions influence households' consumption choices and firms' production decisions. Credit markets may therefore either reinforce environmental regulation or, in some cases, weaken its intended effects. Auto loans provide a particularly relevant setting to study this mechanism, since financing conditions can directly affect the demand for polluting vehicles.

This paper examines how auto lenders adjust credit supply in response to shocks that alter the perceived environmental quality and regulatory exposure of diesel vehicles. We exploit two quasi-natural experiments in European car markets. The first is the 2015 diesel emissions scandal, which revealed that diesel cars emitted substantially higher levels of pollution in real-world driving than previously reported. The second is the staggered introduction and tightening of local low-emission zones, which restrict the circulation of diesel vehicles in affected areas. Both shocks primarily affected diesel cars, while petrol vehicles—otherwise similar in observable characteristics—were not directly exposed and therefore serve as a natural comparison group.

A central contribution of the paper is to analyze how lenders' organizational structure and incentives shape credit supply responses to these environmental shocks. We distinguish between captive lenders, which are financial subsidiaries of car manufacturers, and independent banks. Captive lenders are exposed to the credit and collateral risk of car loans but also have incentives to support the profitability and market value of their parent manufacturers' vehicles. Independent banks, by contrast, face similar credit risks but have no strategic interest in supporting specific car brands. This distinction allows us to identify how vertical integration between production and finance affects the transmission of environmental and regulatory shocks through credit markets.

The analysis is based on a comprehensive dataset of car loans for used vehicle purchases originated by European banks and captive lenders in France, Germany, Italy, and Spain between 2008 and 2018. We identify key vehicle characteristics, including model and fuel type, and aggregate loans into narrowly defined cohorts based on vehicle attributes, lender, borrower income group, and geographic location. This approach enables us to compare loan terms for diesel and petrol vehicles that are otherwise highly similar. The empirical strategy relies on difference-in-differences designs with a dense set of fixed effects, allowing us to isolate diesel-specific shocks from broader trends in credit supply or demand. We also complement the cohort-level design with loan-level regressions that exploit the full data granularity to further explore the heterogeneity across lenders and cars.

The results show that lenders respond very differently to environmental and regulatory shocks depending on their incentives. Following the diesel emissions scandal, captive lenders reduce interest rates and increase loan-to-value ratios for diesel vehicles relative to independent banks. These effects are strongest for diesel cars produced by the captive lenders' own parent manufacturers, suggesting that captive banks use favorable financing conditions to support vehicle sales and resale values after negative environmental information is disclosed.

In contrast, independent banks respond strongly to regulatory shocks stemming from the introduction and tightening of low-emission zones. Focusing on Germany, we find that independent lenders increase loan rates for diesel vehicles in counties subject to stricter circulation restrictions, with larger effects for higher stringency levels. This pattern is consistent with independent banks pricing the increased regulatory and collateral risk associated with diesel vehicles. Captive lenders, by comparison, adjust lending conditions less strongly.

Overall, the findings highlight that credit supply can dampen the transmission of environmental policies when lenders have incentives to support polluting products. Vertical integration between manufacturing and finance may therefore weaken the effectiveness of environmental regulation by offsetting policy-induced increases in the cost of owning high-pollution vehicles. These results are relevant for policymakers seeking to understand how financial intermediation affects the real-world impact of environmental policies.

1 Introduction

Growing awareness of environmental risks and their adverse health impacts has prompted national governments and supranational authorities to adopt stringent policies and regulations aimed at reducing the environmental impact of polluting activities. A central focus of these environmental policies has been the transportation sector, as a major source of toxic air pollutants, particularly from diesel vehicles. Regulatory actions targeting hazardous air pollutants like nitrogen dioxide (NO_2) and particulate matter, mainly emitted through diesel combustion, have included more stringent emission standards for new vehicles and circulation restrictions for older, non-compliant vehicles. These measures aim to internalize environmental externalities of air pollution by increasing the private costs of owning and operating polluting vehicles.

The effectiveness of environmental regulation, however, strongly depends on how these these policies are transmitted through economic channels and incentives. The financial sector plays an important role in this context, as financing conditions set by banks and other intermediaries affect household consumption choices and firms' production decisions. As a result, credit markets can either foster the desired effects of environmental regulations and policies on the behavior of final users or counteract their intended objectives. The loan market for car purchases is a paradigmatic example in this respect, as different lenders set credit conditions which may either discourage or support the purchase of high-pollution vehicles.

In this paper, we study how lenders respond to shocks that alter the perceived environmental quality and regulatory exposure of high-polluting vehicles. We exploit quasi-natural experiments in the diesel car market: the 2015 emissions scandal, which revealed previously unpriced environmental risk, and the staggered introduction of local circulation restrictions due to air pollution. These shocks affected specifically diesel vehicles, while petrol vehicles, otherwise similar in key characteristics, were not directly exposed and serve as a natural comparison group. We also compare the lending behavior of captive intermediaries, which are subsidiaries of car manufacturers, and independent banks, which operate as traditional consumer finance institutions. Captive banks are exposed to collateral risk as lenders, but as subsidiaries of car manufacturers, also have the incentives to support the profitability of their parent groups, potentially influencing the loan terms they offer. Independent banks, in contrast, face the collateral risk of cars but have no strategic incentive to support the market value of the vehicles they finance. This setting allows us to identify a novel channel through which lender incentives and industry structure shape credit supply responses to environmental shocks. In doing so, we show that lending incentives—particularly for captive banks—can counteract, rather than reinforce, the intended objectives of environmental policy. Our findings highlight how organizational structure and vertical integration shape banks' responses to regulatory and informational shocks.

We draw on a comprehensive dataset of car loans for used vehicle purchases extended by European banks and captive lenders in France, Germany, Italy, and Spain between 2008 and 2018.[1] We manually identify key vehicle characteristics, including model and fuel type, based on originator-reported information. To enhance comparability between treated and control groups, we aggregate individual car loans into narrowly defined cohorts based on car fuel type, model, make, lender bank, borrower income group, and geographic location.

[1]The data come from the European Data Warehouse (EDW), a centralized European platform that collects, validates, and disseminates loan-level data on the underlying pools of Asset-Backed Securities (ABS) across asset classes.

For example, one cohort may include loans for BMW X1 diesel vehicles originated by Santander Bank for high-income borrowers residing in Dortmund.

Our baseline empirical strategy compares changes in loan terms for diesel versus petrol vehicles within these cohorts, before and after the diesel emission scandal and the low-emission zone shocks. To isolate the effect of these events, we estimate first-differenced regressions with dense set of fixed effects that absorb persistent cohort-level heterogeneity. Because this design compares outcomes within tightly matched loan groups by car model, location, lender, and borrower profile, observed differences in pricing can plausibly be attributed to diesel-specific shocks rather than to confounding local trends in credit supply or demand.

To investigate the mechanisms underlying these aggregate effects, we complement the cohort-level design with loan-level regressions that exploit existing heterogeneity. These regressions allow us to examine differences across lenders and cars. First, we compare loan terms charged by captive banks for vehicles produced by the lender's parent company (own-brand) with those for other brands. Second, we assess how the presence of captive support influences loan pricing, particularly for independent banks. We proxy captive support with brand-level market penetration, which captures to what extent a model is likely backed by a captive lender. Overall, our setting links loan-level data to variation in brand alignment and captive support, allowing us to test how environmental shocks to diesel vehicles affect credit supply under different incentive structures. By comparing captive and independent lenders for loans on identical vehicles to similar borrowers, where only diesel cars are affected by information shocks (the diesel emissions scandal) and policy shocks (the introduction of circulation restrictions), we isolate how credit supply responds when lenders differ in their incentives to support the brand value.

The diesel emissions scandal represents a sharp information disclosure shock, as it revealed that diesel cars emitted substantially higher levels of pollution in real-world driving than suggested by laboratory tests due to the installation of defeat devices. While the scandal started with Volkswagen in September 2015 (Ater & Yoseph, 2022; Hasan et al., 2023; Strittmatter & Lechner, 2020), it soon extended to other carmakers, which became subject to investigations in the following months and years (Bachmann et al., 2023). Even before these investigations, the Volkswagen scandal had already raised widespread concerns about diesel vehicles produced by other manufacturers. Against this background, in the cohort-level collapsed analysis, comparing the two years before and two years after the Dieselgate scandal, we show that captive banks reduce loan rates for diesel cars by 12 basis points (bps) relative to independent banks. These results are confirmed when we exploit time treatment heterogeneity across carmakers and conduct a staggered analysis using brand-specific treatment dates. The evidence indicates that also the captive banks of other manufacturers adjusted loan rates following the Volkswagen scandal, which was perceived as an anticipatory event for the broader diesel market, while we find no significant effects around brand-specific disclosure events.

The cohort-level results are reinforced by the loan-level analysis, which yields larger effects in magnitude while controlling for model and bank-year fixed effects. We find that - after Dieselgate - captive banks reduce loan rates for diesel cars by 30 bps (relative to a sample mean of 7.23%) and increase loan-to value (LTV) ratios by 3.5 percentage points (pp) (relative to a sample mean of 66.35%). Exploring brand heterogeneity, we show that captive banks reduce loan rates by 20 bps for diesel cars of their own brands relative to other brands following Dieselgate. Moreover, when comparing captive and independent banks, captive lenders decrease loan rates by 31 bps for diesel cars of their own brands relative to the rates charged by independent banks for the same model. We also find evidence on the role of captive support, which mitigates - particularly for independent banks - the impact of Dieselgate on diesel car loans by reducing both the potential rise in loan rates and the potential decrease in LTV ratios. This effect, typically observed for independent banks, is also present for captive banks when financing cars of other brands. Overall, these loan-level results suggest that captive banks have incentives to apply more favorable financing conditions for cars produced by their own parent manufacturers, thereby supporting both current sales and the resale value of the vehicles.

The introduction or tightening of low-emission zones (LEZs) constitutes a policy shock that constrains the effective usability of vehicles in a given county by limiting the circulation of affected cars, specifically those with diesel engines. We focus on Germany, which began introducing LEZs already in 2008 and applies them uniformly, seven days a week. Since LEZs are set at different stringency levels (from 1 to 3), we study the incremental upgrades in restriction intensity over the period 2008-2018. Using a stacked dataset that aligns treated and not-yet-treated counties in event time around each LEZ incremental upgrade, and two-year pre- and post-event windows, we first conduct a cohort-level collapsed analysis. Considering all incremental LEZ events, we find that independent banks price the higher credit risk of diesel car loans in counties with LEZs by raising loan rates by an average of 18 bps. When analyzing transitions to higher stringency levels, independent banks increase loan rates by even larger amounts, particularly following upgrades to Level 2 and Level 3, consistent with the higher risk of loans for diesel cars in counties with tighter restrictions. These results are confirmed in the loan-level analysis, both when comparing treated counties subject to different LEZ levels and when including also never-treated counties.

Finally, we conduct a robustness analysis to assess potential anticipation effects prior to the formal introduction or tightening of LEZs, based on data for locally recorded pollution levels. As an alternative event, we consider instances in which the EU annual-mean NO_2 concentration exceeds the regulatory limit of $40\,\mu g/m^3$ at at least one monitoring station within a county, capturing transition risk related to future restrictions. Exceeding this threshold may increase regulatory pressure and the likelihood of subsequent traffic-related restrictions for diesel vehicles. We find no evidence that the recording of above-threshold pollution levels affect banks' lending behavior, either for captive or independent lenders, in anticipation of future LEZ introductions or tightenings. These results further support the interpretation that the observed changes in diesel car loan conditions are driven by the implementation of LEZs rather than by potential anticipatory events.

Related Literature — Our paper relates to three main strands of the literature: (i) bank lending during the transition away from polluting activities; (ii) the lending behavior of captive financial intermediaries; and, (iii) the role of consumer credit for car market developments.

Our study builds on and expands prior evidence on bank lending in the green transition, encompassing both corporate and household credit. Banks' lending practices are closely linked to economic activities affecting the natural environment (Ivanov et al., 2024; Kacperczyk & Peydró, 2022) and can significantly influence corporate investments in green technologies (Accetturo et al., 2025; De Haas et al., 2025). As climate policies become more stringent, environmental risks may have a significant impact on banks' portfolios and, therefore, should be incorporated into lending decisions (Degryse et al., 2023; Delis et al., 2024; Reghezza et al., 2022). Despite public commitments to sustainability and the potential balance-sheet consequences of transition risk, banks continue to provide large amounts of credit, often at favorable conditions, to highly polluting firms (Beyene et al., 2025; Giannetti et al., 2025), while capital markets tend to price climate and carbon risks more strongly (Bolton & Kacperczyk, 2021; De Haas & Popov, 2023; Krueger et al., 2020; Seltzer et al., 2025). Some banks may face conflicts of interest that could restrict their ability to properly internalize transition risks. These may arise from legacy exposures to polluting firms (Brown et al., 2017; Degryse et al., 2025; Mueller & Sfrappini, 2025) or from cross-border lending in response to stricter climate policies in banks' home countries (Benincasa et al., 2025; Demirguc-Kunt et al., 2024; Laeven & Popov, 2023). Our paper contributes to this literature by examining the conflict of interest faced by captive banks relative to independent banks, and by analyzing how this conflict shapes banks's responses to environmental shocks.

Moreover, our paper relates to the literature on the lending behavior of captive financial intermediaries. When manufacturers also act as lenders, the credit approval process reflects both the profitability of the loan itself and the profit from selling the underlying product financed through the captive loan. A growing body of theoretical and empirical literature has examined the performance of banks and captive finance companies in credit markets (Barron et al., 2008; Bodnaruk et al., 2016; Brennan et al., 1988; Murfin & Pratt, 2019). Some studies analyze how financial shocks to manufacturers (Benetton et al., 2025), trade tariffs (Hankins et al., 2025) and sales force incentives (Ramcharan & Yao, 2022) affect loan terms and conditions under vertical integration between production and finance. Other papers study the expansion of non-bank lending to riskier borrowers in consumer credit markets in response to increased competition (Gissler et al., 2020). We contribute to this literature by showing how product market considerations at the parent-manufacturer level shape the lending behavior of captive banks. In particular, we investigate how car loan conditions can be used to incentivize the sales of products facing weaker consumer demand, such as highly polluting diesel vehicles.

Lastly, this paper contributes to the literature on the role of consumer credit in car markets. A large share of households finances car purchases through bank credit or other forms of asset-based lending. Existing studies document the importance of car loan conditions for auto sales, particularly for borrowers facing credit constraints (Adams et al., 2009; Johnson et al., 2014). In this context, financial shocks that worsen liquidity conditions (Benmelech et al., 2017) or generate losses for consumer lenders (Ramcharan et al., 2016) may tighten loan terms and reduce car sales. Other studies investigate the role of search frictions in consumer loan markets (Argyle et al., 2022), the consumer response to exogenous variations in credit terms (Argyle et al., 2021), and the importance of monthly payments in consumer installment debt (Argyle et al., 2020).

More recently, within the debate on climate change and the carbon transition, some papers explore the pricing of car loans for electric versus internal combustion engine vehicles (Bena et al., 2025; Klee et al., 2024), while others analyze the effects of carbon taxes on automotive innovation in clean technologies (Aghion et al., 2016) and on car loan pricing (Fliegel et al., 2025). Since auto loans are often securitized (Latino et al., 2025a), some studies investigate the role of ESG scores in the funding costs of auto loan securitizations and their pass-through to loan rates (Kontz, 2025), as well as the investment behavior of mutual funds in sustainability-transparent auto ABS (Latino et al., 2025b).

Our paper contributes to the debate on the role of environmental factors in car loan pricing by focusing on another important dimension, distinct from CO_2-driven climate change: the emission of hazardous air pollutants such as NO_2, particularly from diesel vehicles. Unlike electric mobility, which appears only in recent loan data, diesel vehicles have long held a substantial market share, allowing us to exploit a substantially larger sample and richer time variation to explore environmental shocks. While regulatory commitments to net-zero emissions and the transition to electric mobility are more recent developments, regulations targeting diesel emissions have been in place since the late 1990s and then are at a more advanced stage of implementation. We study how auto lenders adjust loan conditions in response to two shocks affecting the environmental quality of diesel cars, i.e., the diesel emissions scandal and the introduction of local circulation restrictions, which arise from the enforcement of these regulations intended to reduce hazardous air pollutants. Moreover, our results show how financial intermediaries with product-market exposure may respond differently to environmental shocks than stand-alone lenders.

The reminder of the paper is structured as follows. Section 2 provides some background on the shocks affecting the environmental quality of diesel cars and discusses the economic intuition underlying banks' lending incentives and behavior. Section 3 describes the dataset. Sections 4 and 5 outline the empirical strategy and present the results for the diesel emission scandal and the introduction of LEZs. Section 6 concludes by summarizing the main findings and discussing their implications.

2 Background and Theoretical Motivation

2.1 Shocks to Diesel Vehicles: The 2015 Emissions Scandal and Low-Emission Zones

Diesel cars are more fuel-efficient and therefore emit less CO_2 than petrol cars, but they produce significantly higher levels of NO_2 and other harmful pollutants, increasing local air pollution and health risks.[2] High levels of air pollution can influence consumers' choices, for instance on health insurance (Chang et al., 2018), and can be highly relevant for the political discussion (Koetter & Popov, 2025). They can also affect financial sector outcomes, like corporate earnings forecasts (Dong et al., 2021), or bank lending to firms (Berger et al., 2025). Also credit markets for car loans are expected to respond to shocks concerning diesel-related pollution, to the extent that these shocks can reduce consumer demand for diesel vehicles, leading to oversupply in the used-car market and a decline in diesel car resale values. This depreciation, in turn, weakens the value of collateral in auto loans and increases credit risk. We exploit two distinct quasi-natural experiments that materialize diesel-related credit risk: (i) the 2015 diesel emissions scandal, and (ii) the staggered introduction of LEZs in Germany.

(i) The 2015 Diesel Emissions Scandal. On September 18, 2015, the U.S. Environmental Protection Agency (EPA) issued a notice of violation to Volkswagen (VW), revealing that the firm had installed software enabling diesel vehicles to pass laboratory emissions tests while emitting four to seven times more NO_2 under real driving conditions. The scandal soon widened to several other European manufacturers, as subsequent investigations involved additional carmakers beyond VW.[3] The revelations sparked a global scandal, exposing widespread emissions manipulation and sharply increasing scrutiny of diesel technology, ultimately disrupting the used diesel car market.

The diesel emissions scandal led to a decline in diesel car registrations for VW and other affected car

[2]According to the World Health Organization, air pollution is the leading environmental risk for non-communicable diseases in Europe, where diesel penetration is high.

[3]Table B.1 and Figure B.1 in the Appendix provide additional details on the timelines related to the unfolding of the diesel emissions scandal.

manufacturers.[4] Beyond the manufacturer-level reputation effects, this information disclosure shock implied that diesel vehicles became substantially riskier assets than petrol vehicles. Consumers were affected through two main channels. First, the scandal increased the financial risk of owning a diesel car. Uncertainty about future environmental regulation and the long-run usability of diesel vehicles reduced resale prices and, in turn, their value as collateral. In the spirit of Akerlof (1970), heightened uncertainty about product quality and regulatory risk exacerbates information asymmetries and can generate adverse selection in the used-car market, as buyers become more cautious when hidden risks loom large. Second, the scandal plausibly shifted consumer preferences away from diesel vehicles. The revelation that even modern diesel cars could be highly polluting increased environmental concerns, particularly in the used-car segment where vehicles are older and more emission-intensive. Combined, these channels reinforced doubts about the long-term viability of diesel technology and likely pushed both buyers and lenders away from diesel vehicles.

(ii) The introduction of Low-Emission Zones in Germany. In recent years, governments have increasingly adopted policies to curb traffic-related pollution, notably through LEZs. These designated areas restrict vehicle access, typically banning high-emission vehicles. LEZs primarily target diesel cars due to their higher emissions of harmful air pollutants. In this context, used diesel vehicles are of particular concern, as they generate disproportionately high levels of NO_2 and particulate matter (PM). Older models, which often lack advanced exhaust treatment systems, are major contributors to urban air pollution and therefore prime targets of LEZ restrictions. Moreover, the diesel emissions scandal further underscored the need for stricter regulations by revealing large discrepancies between laboratory-tested and real-world emissions.

The introduction of LEZs in Germany provides a uniquely useful setting for studying their impact on car loan markets. By 2018, Germany had implemented 52 LEZs, starting in 2008. Unlike in other countries, German LEZs apply uniformly—seven days a week and to all four-wheeled vehicles—ensuring a high degree of regulatory consistency.[5] A car's eligibility to enter a German LEZ is determined by its Euro norm standard, which imposes progressively stricter emission limits.[6] Petrol cars face substantially more lenient restrictions than diesel vehicles. For instance, at the highest stringency level (Level 3), petrol cars are required to meet only Euro 1 standards, introduced in 1992, implying that all petrol cars manufactured after 1992 are unaffected by LEZ restrictions. In contrast, diesel vehicles must comply with Euro 4 standards or higher (or Euro 3 with an approved retrofit).[7]

[4] Ater & Yoseph (2022) show that the scandal reduced transactions and resale prices of manipulated VW cars due to lower willingness-to-pay and adverse selection. Strittmatter & Lechner (2020) find a supply increase and a decrease in prices for used VW diesel cars, particularly those subject to high probability of manipulation. Bachmann et al. (2023) document spillover effects on non-VW German automakers, lowering consumer valuations and sales. Hasan et al. (2023) show that VW registrations declined more in Protestant-majority German counties, reflecting stronger reactions to corporate fraud.

[5] Elsewhere, LEZ rules often vary, applying only on certain days or to specific vehicle types, such as heavy-duty trucks. For example, in Italy, Milan's Area C charges high-emission vehicles to enter the city center, while Rome enforces alternating bans based on license plates during high-pollution periods.

[6] EU legislation mandates emission standards for new vehicles before market entry. Introduced in 1993, Euro standards have evolved from Euro 1 to Euro 6 (2015), with compliance in one EU member state allowing sales across the EU.

[7] Table B.2, as well as Figures B.2 and B.3 in the Appendix provide more information on the location and timeline

2.2 Vertical Integration and Credit Market Responses

These environmental shocks interact with lender incentives in credit markets, which differ systematically across organizational structures. In particular, the car loan market includes two main types of financial intermediaries: captive lenders and independent banks. Captive banks, which are subsidiaries of large car manufacturers, specialize in financing and leasing vehicles produced by their parent companies. Due to vertical integration between production and finance, manufacturers exert direct control over the credit policies of their captive banks. As a result, unlike independent banks, captive lenders consider both the profitability of the loan and the sale of the financed vehicle, aligning lending decisions with the manufacturers' sales incentives.

The literature predicts a relationship between expected price depreciation and captive finance through two key channels. First, captive financing can mitigate information asymmetry in markets where buyers have limited knowledge about product quality. A manufacturer's decision to finance its own products can signal confidence in their quality, as in Emery & Nayar (1998), Lee & Stowe (1993), and Long et al. (1993). Relatedly, Stroebel (2016) shows that vertically integrated lenders possess superior information about collateral values, consistent with vertical integration arising under asymmetric information. In the context of the diesel emissions scandal, captive financing may therefore signal that the manufacturer stands behind the relative quality and long-run value of its diesel vehicles compared with petrol vehicles or newer models that are less exposed to future air-pollution policies.

Second, captive finance can also operate through a commitment channel. By financing their own products, manufacturers internalize the price impact of future production choices and can commit to actions that support used-asset prices, thereby sustaining higher expected resale values and increasing buyers' willingness to pay today (Murfin & Pratt, 2019). In our setting, following an exogenous shock to diesel vehicles, such a commitment to supporting future resale values can prevent a collapse in used diesel car prices and protect new-vehicle pricing strategies.

Empirical predictions. Following the diesel emissions scandal and the introduction of LEZs, declines in diesel car resale values increase credit risk and erode lenders' expected profits. The value of a car loan depends on interest income, loan repayments, and the vehicle's resale value in the event of default. A drop in car values raises default risk and lowers recovery rates, leading to higher expected loan losses. Lenders must therefore trade off interest revenues against increased credit risk and deteriorating collateral values (Stroebel, 2016). In response, they may tighten credit conditions by raising interest rates or requiring larger down payments, thereby reducing LTV ratios (Cerqueiro et al., 2016, 2020). Captive lenders, however, face a different set of incentives. Owing to vertical integration with manufacturers, they internalize the effects of credit supply on new-vehicle demand and on the profitability of car sales (Barron et al., 2008; Benetton et al., 2025). When diesel vehicle values fall, stricter lending conditions would further depress demand for diesel models, undermining manufacturer's pricing strategies and market positions. Moreover, manufacturers and dealers often hold substantial inventories of diesel vehicles, making demand stimulation through pricing and financing particularly valuable in the aftermath of the scandal (e.g. Busse et al., 2006). Captive lenders

of LEZ introductions in Germany at the county level.

may therefore have incentives to offset the shock by easing credit terms to support sales, facilitate inventory clearance, and stabilize expected resale values.

These institutional differences yield two central empirical predictions.

- **Prediction 1 - Baseline credit response.** Environmental shocks that reduce diesel vehicle values lead to tighter lending conditions for diesel cars relative to petrol cars.
- **Prediction 2 - Differential response of captive lenders.** Captive banks respond differently to these shocks, potentially relaxing credit constraints to support manufacturers' pricing and sales strategies.

These predictions guide our empirical strategy. We compare changes in lending conditions for diesel versus petrol vehicles before and after the environmental shocks and examine whether these responses differ systematically between captive and independent lenders.

3 Data and Summary Statistics

Our main dataset comprises car loans securitized by European banks and captive lenders. These data are available from the European Data Warehouse (EDW), a centralized European platform that collects, validates and disseminates loan-level information on the underlying pools of Asset-Backed Securities (ABS) across different asset classes. Issuers of ABSs are required to report loan-level information on the composition and performance of their securitized loan portfolios on a quarterly basis, following a detailed and standardized format set by the European Central Bank (Ertan et al., 2017; Klein et al., 2020). EDW is the primary securitization repository in the European Union, and current regulations require all securitization transactions issued in the EU to report loan-level data to a recognized repository (EU Regulation 2017/2402).

We apply the following filters to the retrieved EDW data. We retain only loans for the purchase of used, non-commercial cars, where ownership is transferred at the time of the purchase.[8] Borrowers must be individuals; loan to legal entities are therefore excluded. To ensure comparability in credit risk characteristics, we focus on amortizing car loans and exclude balloon loans.[9] We further restrict the sample to loans originated between 2008 and 2018 in Germany, France, Italy, and Spain. Crucially, each car underlying a loan must be identifiable by make, model, and fuel type, as our empirical strategy relies on comparing petrol and diesel cars. We manually determine these characteristics using information reported by loan originators and infer fuel type from manufacturers' model naming conventions. For example, in Alfa Romeo's nomenclature, "JTD" in a model description denotes a diesel engine. Accordingly, the model "Alfa Romeo Giulietta 2.0 JTD 150" is classified as diesel. We exclude car models for which the engine type cannot be definitively identified. The final dataset includes vehicles produced by Alfa Romeo, Audi, BMW, Citroën, Dacia, FCA, Peugeot, Renault, SEAT, Škoda, and Volkswagen.[10] Our final dataset consists of a total of 843,889 car loan contracts originated

[8] For this reason, we exclude leasing contracts, in which ownership is transferred only after a pre-determined period, following the exercise of the purchase option by the lessee.

[9] Amortizing car loans account for approximately 76% of the car loan market in our sample countries. Balloon loans are not directly comparable, as they involve additional credit risk related to the principal repayment at maturity.

[10] The Appendix A provides additional details on the make, model, and fuel-type classification.

by European banks between 2008 and 2018 in Germany, France, Italy, and Spain. For each loan, we observe vehicle characteristics (brand, model, and fuel type), lending conditions (loan rate, maturity, LTV ratio, down payment, principal balance, and original car valuation), and borrower characteristics, including income, employment type, and geographical location at the NUTS3 level, corresponding to small administrative units such as counties within a country.[11] Table 1 provides an overview of the main variables and their definitions.

Table 2 presents descriptive statistics for the main car loan variables and other relevant characteristics, offering insights into the dataset's composition in terms of borrowers, countries, and car makes. The average interest rate is 7.2% per annum, the average loan term is 55 months, and the average LTV ratio 66.4%. Borrowers have an average primary income of EUR 26,250. Captive banks originate 68% of car loans in the sample, being defined as lenders in which the manufacturer holds at least a 50% ownership stake. The remaining loans are extended by independent banks. Moreover, 92% of used car loans issued by captive banks finance cars produced by their own manufacturing group. The lower part of Table 2 provides additional details on the distribution of loan contracts across car makes, borrower occupation types, countries, and years.

4 Effects of the Diesel Emissions Scandal

This section analyzes how the September 2015 diesel emissions scandal affected loan conditions for diesel relative to petrol vehicles. The scandal represents a sharp information shock that raised the perceived environmental risk of diesel cars and, in turn, affected their expected resale value. We exploit this shock using a cohort-level first-differences design. Loans are grouped into narrowly defined cohorts—by lender bank, vehicle model, brand, and fuel type, borrower's country and income group —within which diesel and petrol loans are highly comparable. For each cohort, we take the first difference in average loan terms between the pre- and post-scandal periods, thereby differencing out all time-invariant cohort characteristics and isolating the relative shift in loan pricing for diesel versus petrol vehicles.

Our empirical strategy exploits the fact that the diesel emissions scandal directly affected diesel but not petrol vehicles. As explained in Section 2, the scandal revealed the use of defeat devices in diesel engines that understated real-world NO_2 emissions, initially at Volkswagen and subsequently at other manufacturers. These revelations undermined confidence in diesel technology and triggered diesel-focused regulatory and public scrutiny. By contrast, petrol cars were not implicated in the manipulation and did not face comparable depreciation risk, making fuel type a natural treatment dimension and petrol vehicles an ideal control group. Comparing diesel and petrol loans for otherwise identical vehicles— the same make and model with different engines—allows us to isolate the scandal-induced change in loan terms for diesel vehicles.

We construct the estimation sample from amortizing used-car loans extended to individual borrowers between September 2013 and September 2017, yielding a symmetric two-year window around the September 2015 shock.[12] Cohorts are defined as combinations of lender bank, vehicle brand, model, and fuel type, as well as borrower's country and income quintile (constructed within country). To ensure reliable cohort means and

[11] The Nomenclature of Territorial Units for Statistics (NUTS) is the EU's regional classification system with three levels: NUTS1 (major regions), NUTS2 (economic policy regions), and NUTS3 (small administrative units such as counties).

[12] In robustness tests in Section 4.2, we allow for heterogeneous treatment timing across manufacturers.

avoid noisy cohort-level first differences driven by small samples or composition effects, we restrict attention to sufficiently large cells and exclude cohorts with fewer than 50 loans in both the pre- and post-scandal periods. This threshold implies at least 100 underlying loan observations per collapsed cohort and follows standard pseudo-panel practice aimed at enduring reliable cohort means (Deaton, 1985), and corresponds to excluding approximately the bottom 15% of cohorts by size.[13]

For each cohort c, we compute the mean of the outcome variable in the pre- and post-scandal periods and construct the first difference as the change in average loan conditions after versus before the scandal:

$$\Delta Y_c \equiv \bar{Y}_{c,\text{post}} - \bar{Y}_{c,\text{pre}}.$$

Our baseline specification relates this within-cohort change in loan conditions to diesel status:

$$\Delta Y_c = \beta \, \text{Diesel}_c + X_c' \gamma + \mu_{m(c)} + \mu_{bc(c)} + \varepsilon_c, \tag{1}$$

where Diesel_c equals one for diesel cohorts and zero for petrol cohorts, and X_c is a vector of pre-scandal borrower-level controls averaged within cohort c (e.g., log income). The fixed effects $\mu_{m(c)}$ are car-model fixed effects that capture model-specific characteristics (such as demand, supply, and resale values) that may affect loan pricing independently of the scandal. $\mu_{bc(c)}$ are bank-by-country fixed effects, absorbing lender- and country-specific variation in credit supply and pricing conditions. Standard errors are clustered at the model–fuel–bank level, allowing for arbitrary correlation in residuals within these cells. Under this specification, the coefficient β captures the scandal-induced change in loan conditions for diesel relative to petrol vehicles within tightly defined lender–product–borrower cohorts.

Table 3 reports the effects of the diesel emissions scandal on loan contract terms. Panel A shows results for loan interest rates, while Panel B presents results for LTV ratios. Columns 1 and 2 estimate the baseline specification as in equation (1) separately for captive and independent banks, respectively. Column 3 pools all lenders and includes a *Diesel* × *Captive* interaction.

The results show that, following the diesel emissions scandal, captive banks apply more favorable lending conditions for diesel cars relative to independent banks. In particular, captive banks reduce loan rates for diesel cars relative to petrol cars by 9 bps (Column 1), while independent banks exhibit no statistically significant change in loan rates (Column 2). In the pooled specification with an interaction term, captive banks reduce loan rates for diesel cars by 12 bps relative to independent banks (Column 3), corresponding to a decline of 1.7% relative to the sample mean.[14] Consistent patterns emerge for LTV ratios. After the scandal, captive banks increase the LTV ratio for diesel car loans by 1.4 pp, while independent banks raise it by 0.9 pp. This difference can be explained by the ex-post reduction in down payments. As observed in Table C.1 (Appendix), captive banks lower down payment amounts for diesel car loans by 6.5%, thereby

[13] In Figure C.1 and Table C.6 (Appendix) we examine the sensitivity of the main estimate to alternative cohort-size thresholds. In the baseline collapsed sample, cohorts are substantially larger, with the mean number of underlying loans per cohort ranging between roughly 400 and 800 across specifications.

[14] Table C.5 in the Appendix reports a set of robustness checks based on alternative fixed-effect structures, sample restrictions, and the inclusion of additional loan contract characteristics. The estimated Diesel × Captive coefficient remains stable across specifications.

easing borrowing constraints at loan origination.

4.1 Pre-Treatment Differences Between Diesel and Petrol Car Loans

A key identifying assumption underlying the cohort first-difference design in equation (1) is that, absent the diesel emissions scandal, loan conditions for diesel and petrol vehicles would have evolved similarly within cohorts. We assess this assumption by examining whether diesel and petrol loans differ systematically in the pre-scandal period once we condition on high-dimensional fixed effects spanning the same lender, product, and borrower characteristics that define the cohorts in our first-difference design. Focusing on the pre-treatment years 2013 and 2014, we estimate the following loan-level regression:

$$Z_i = \alpha + \beta \, Diesel_i + \eta_{c(i)} + \varepsilon_i, \tag{2}$$

where Z_i denotes a loan-level outcome (loan rates and LTV ratios) observed prior to the scandal. The fixed effects $\eta_{c(i)}$ correspond to model × bank × country × income-group cells, mirroring the cohort structure used in the first-difference analysis. The dummy $Diesel_i$ equals one for diesel car loans and zero for petrol car loans. Standard errors are clustered at the cohort level defined by model × bank × country × income group × fuel type. Under this specification, the coefficient β captures conditional pre-treatment differences between diesel and petrol car loans within the same lender–product–borrower cells.

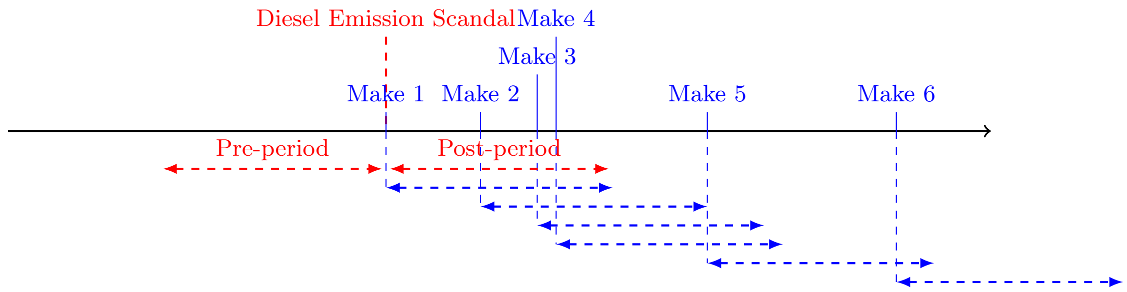
Table 4 compares diesel and petrol car loans in the pre-scandal years 2013 and 2014. For captive banks, diesel–petrol differences in loan rates are very small in both years and show no systematic change over time, while the corresponding LTV gap (about 1.2–1.6 pp) is modest and stable. For independent banks, diesel–petrol differences in loan rates are somewhat larger in levels but remain essentially unchanged between 2013 and 2014, indicating no differential pre-trends prior to the scandal. Overall, while some level differences exist—particularly for LTV ratios—there is no evidence of systematic divergence between diesel and petrol car loans in the pre-treatment period. This stability supports the identifying assumption that post-scandal differences are not driven by pre-existing differential dynamics.

Figure 1 provides additional context by showing that retail prices for diesel and petrol fuel did not exhibit differential movements around the time of the diesel emissions scandal in the countries covered by our sample. This suggests that the estimated changes in loan conditions are unlikely to be driven by contemporaneous fuel price dynamics, further supporting the interpretation of the scandal as a diesel-specific information shock.

4.2 Robustness to Staggered Manufacturer Exposure

The September 2015 investigation of Volkswagen triggered the diesel emissions scandal, but subsequent investigations involved other manufacturers at different points in time.[15] Our baseline specification treats September 2015 as a common information shock for all diesel vehicles. However, the salience and timing of the scandal may have differed across manufacturers: later disclosures could have generated additional brand-specific shocks, or they may have been largely anticipated once Volkswagen's violations became public.

[15] See Table B.1 and Figure B.1 in the Appendix for a detailed timeline of manufacturer-specific investigations.



To assess the robustness of our results to staggered manufacturer exposure, we allow treatment timing to vary across brands. We continue to define cohorts based on vehicle model, make, and fuel type, lender bank, borrower's country, and income quintile, ensuring that diesel and petrol car loans remain comparable within tightly defined lender–product–borrower cells. Diesel cohorts are treated at the manufacturer-specific investigation date, while petrol cohorts serve as a never-treated control group. For each manufacturer, we construct a symmetric two-year window around its investigation date and normalize calendar time into pre- and post-treatment periods. We then stack these manufacturer-level panels and compute cohort-level first differences in loan outcomes. The resulting specification is analogous to our baseline first-difference model as in equation (1), but includes manufacturer fixed effects to absorb persistent brand-level differences in loan pricing, along with bank–country fixed effects to control for time-invariant differences in credit supply across lenders and countries.

Figure 2 plots the estimated coefficients on loan rates for diesel cars across alternative first-difference specifications that differ in their treatment timing assumptions. Column (1) reports the average treatment effect on the treated (ATT) from the baseline model, assuming a common treatment date of September 2015 for all diesel cohorts and a symmetric two-year window before and after the shock. Column (2) presents the ATT from a stacked first-difference model that uses a common pre-period ending in September 2015 but allows the post-period to begin at each manufacturer's investigation date. Column 3 reports estimates from a fully manufacturer-specific stacked design, with both pre- and post-periods defined relative to each brand's investigation date. The comparison between Columns (1) and (2) indicates that the Volkswagen investigation was the dominant information shock, with limited additional effects from later manufacturer-specific disclosures. Consistent with this interpretation, Column (3) shows that brand-specific scandal events do not generate statistically significant changes in diesel car loan rates. In unreported analyses, we exclude VW Group brands to mitigate potential reputational spillovers, but the results for captive banks remain unchanged.

4.3 Loan-level Analysis

To corroborate the collapsed-cohort results in Table 3, we complement the cohort-level analysis with loan-level regressions using the full panel of individual loans. This serves two purposes. First, it verifies that the cohort-level findings are not driven by aggregation or cohort construction. Second, it allows a more granular assessment of how individual loan contract margins adjust following the Dieselgate, and whether these adjustments differ systematically between captive and independent banks.

For the loan-level analysis, we estimate the following specification for each loan i, originated by bank $b(i)$ for vehicle model $m(i)$ in country $c(i)$ at date $t(i)$:

$$Y_i = \beta\big(\text{Diesel}_i \times \text{Post}_{t(i)}\big) + \gamma_1 \,\text{Post}_{t(i)} + \gamma_2 \,\text{Diesel}_i + X_i'\delta + \mu_{m(i)} + \mu_{b(i)\times t(i)} + \varepsilon_i, \tag{3}$$

where Y_i denotes the loan-level outcome of interest (loan rate, LTV ratio, and–as reported in the Appendix–loan maturity, loan amount, and down payment). The indicator $\text{Post}_{t(i)}$ equals one for loans originated after the Dieselgate announcement, while Diesel_i identifies diesel vehicles. The vector X_i contains borrower control variables (e.g., log income). We include car-model fixed effects, $\mu_{m(i)}$, to absorb time-invariant model characteristics, and bank-year fixed effects, $\mu_{b(i)\times t(i)}$, to account for time-varying lender-specific pricing conditions. Standard errors are clustered at the model–fuel–bank level. Under this specification, the coefficient β captures the differential post-scandal change in loan conditions for diesel relative to petrol vehicles of the same model within bank–year cells. Conceptually, this estimator parallels the collapsed-cohort specification but exploits the full loan-level variation rather than changes in cohort means.

Table 5 reports the loan-level estimates for loan rates and LTV ratios, separately for captive banks (Column 1), independent banks (Column 2), and the pooled sample with interactions by captive status (Column 3). For loan rates, the coefficient on *Diesel* × *Post* is negative and statistically significant for captive banks, implying a reduction of 27 bps, while it is small and statistically insignificant for independent banks. Consistent with this pattern, the triple interaction *Diesel* × *Post* × *Captive* in the pooled specification is strongly negative, indicating an ex-post decrease in loan rates by 30 bps for diesel car loans extended by captive banks relative to independent banks. A similar differential response emerges for LTV ratios. Following the scandal, captive banks increase LTV ratios on diesel car loans by 4.9 pp relative to petrol car loans, while independent banks also raise LTV ratios but by a smaller magnitude of 1.9 pp. In the pooled specification, diesel car loans experience a general post-scandal increase in LTV ratios of 1.8 pp, with captive banks raising LTV ratios by an additional 3.5 pp. Table C.2 (Appendix) shows that these LTV effects are driven by adjustments in down payments and loan amounts: after the scandal, captive banks reduce down payments on diesel car loans by 8.7% and correspondingly increase loan amounts by 4.4%, relative to independent banks, consistent with an easing of financing constraints for diesel vehicle purchases.

Overall, the loan-level evidence confirms that the collapsed-cohort results are not driven by aggregation and reflects within-lender adjustments in individual loan contracts. Taken together, the two approaches provide complementary evidence that captive banks actively supported diesel vehicle financing following the diesel emission scandal. The collapsed-cohort specification serves as the baseline empirical design, as it identifies the effect from within-cohort first differences in mean loan terms across tightly comparable lender–product–borrower groups, thereby differencing out all time-invariant cohort characteristics and reducing sensitivity to compositional changes. By contrast, the loan-level regressions exploit within bank–year variation and, by construction, place greater weight on high-volume segments and within-lender pricing adjustments. Differences in identifying variation and weighting can therefore lead to differences in magnitude across the two approaches. The consistency in signs and relative differences across bank types underscores the robustness of the main findings and reinforces the interpretation that captive banks systematically eased financing conditions for diesel vehicles following the Dieselgate shock.

One limitation of the EDW dataset is that it does not contain information on dealer–bank compensation arrangements (e.g., merchant fees or dealer markups), which may prevent a direct analysis of how the overall economic terms of a car purchase may adjust across all the different margins. Prior research shows that both the Dieselgate scandal and environmental regulations such as the Low Emission Zones reduced the market value of the affected diesel vehicles (Ater & Yoseph, 2022; Börjesson et al., 2021) suggesting that the shocks likely exerted downward pressure on diesel vehicle prices, making it less likely that manufacturers were able to sustain higher effective prices through financing adjustments.

Moreover, existing evidence suggests that dealer markup practices are more prevalent in the United States than in European auto finance markets (e.g., Latino et al. (2025a)). In European markets, there is comparatively less evidence of systematic dealer-level interest rate markups, particularly for captive lenders. While we cannot directly observe these additional transaction margins or fully rule out adjustments across them, our analysis focuses on differences in credit terms conditional on the observed loan contracts. The results therefore document clear differences in the loan terms offered by captive and independent lenders following the Dieselgate shock.

While reallocation across margins cannot be ruled out, lower interest rates can stimulate demand by easing liquidity constraints and shifting risk to lenders, thereby reducing buyers' exposure to downside risk in ways that price reductions do not. Consistent with Murfin & Pratt (2019), captive banks' provision of more favorable loan terms reflects incentives to support demand given their exposure to collateral and resale values.

Heterogeneity in Loan Conditions by Lender Type and Own-Brand Financing. To further investigate the mechanisms underlying the more favorable post-Dieselgate lending behavior of captive banks, we examine whether captive lenders respond differently when financing vehicles produced by their own parent manufacturer versus rival brands. Table 6 reports loan-level results by own-brand status, for loan rates and LTV ratios.[16] Column 1 restricts the sample to captive banks and compares own-brand with non–own-brand loans. Columns 2 and 3 expand the comparison to include independent banks: Column 2 contrasts captive own-brand loans with comparable loans by independent banks, while Column 3 compares captive non–own-brand loans with loans originated by independent banks. For loan rates (Panel A), we find that after the diesel emissions scandal captive banks charge significantly lower interest rates (by about 20 bps) for diesel cars of their own brands relative to diesel cars of other brands (Column 1). This effect is even more pronounced when comparing captive and independent lenders: captive banks charge loan rates that are 31 bps lower for diesel cars of their own brand relative to comparable loans by independent banks (for vehicles of the same model) (Column 2). By contrast, we observe no statistically significant difference between captive and independent banks for diesel vehicles of other brands, unrelated to the parent manufacturer of the captive bank (Column 3). The results for LTV ratios (Panel B) mirror this pattern. Captive banks apply significantly higher LTV ratios (by 3.6 pp) for diesel cars of their own brands relative to comparable loans by independent banks (Column 2), while no significant differences emerge for diesel vehicles of other brands (Column 3). These findings support the interpretation that captive banks use lending terms strategically to bolster the sales and residual values of vehicles produced by their own parent manufacturers following the Dieselgate shock.

[16] Table C.3 in the Appendix reports results also on loan maturity, down payment and loan amount.

Heterogeneity in Loan Conditions by Manufacturer Captive Finance Strength. We next examine whether the strength of a manufacturer's captive finance arm affects the loan-level response to Dieselgate, beyond the direct lending behavior of captive banks for cars of their own brands. This exercise is motivated by the idea that manufacturers with a large and well-established captive finance presence may have greater ability and incentives to stabilize diesel car sales after the scandal, either by subsidizing purchases through their lending arm or by coordinating pricing strategies between the manufacturer and its captive bank. To capture ex-ante captive support, we use manufacturer-level financing penetration rates in Western Europe, as reported in firms' annual reports. These penetration rates measure the share of a manufacturer's new vehicle registrations financed by its captive subsidiary. We map the corresponding EU-wide penetration rate, which we define as "captive support", to all loans associated with a given brand i in our data. A higher value of captive support therefore indicates a stronger captive finance presence and a greater capacity to support sales through financing.

In Table 7, we estimate loan-level regressions that interact *Diesel* × *Post* with *Captive support* and run separate specifications for independent banks (Column 1), captive banks financing non–own-brand vehicles (Column 2), and captive banks financing their own-brand vehicles (Column 3). This structure allows us to distinguish between indirect captive support operating through the broader credit market and direct support provided by captive banks to their parent manufacturers' vehicles. We report the results on loan rates and LTV ratios.[17]

We find evidence that captive support mitigates - particularly for independent banks - the impact of Dieselgate on diesel car loans by attenuating the potential rise in loan rates. Following the scandal, independent banks charge lower loan rates for diesel cars produced by manufacturers with stronger captive support: a 1 pp increase in captive support is associated with lower diesel loan rates by 11 bps (Column 1). A similar pattern - of smaller magnitude - is observed for captive banks when financing cars of other brands, suggesting that strong captive finance arms can mitigate the tightening of loan conditions for diesel vehicles, even when loans are originated outside the manufacturer's own captive network (Column 2). By contrast, when captive banks finance cars of their own brands, ex-post loan rates for diesel cars are already lower on average, and the marginal effect of additional captive support is weaker. We observe a similar pattern for LTV ratios. Independent banks apply higher LTV ratios to diesel cars of brands with stronger captive support, and this also holds for captive banks financing non-own-brand vehicles. For own-brand car loans, captive banks again apply higher LTV ratios on average, but this increase in LTV is smaller when captive support is already high.

5 Effects of the Introduction of Low-Emission Zones

This section investigates how diesel circulation restrictions affect car loan conditions in Germany. The introduction of LEZs constitutes a regulatory shock that directly targets diesel vehicles by restricting their usability in affected areas, thereby reducing their market value and resale prospects. To isolate this effect, we compare loan terms for diesel and petrol vehicles across counties that do and do not introduce LEZs.

Table 8 documents the institutional setting and the source of identifying variation. In total, 66 counties

[17] Table C.4 in the Appendix displays findings also for loan maturity, down payment and loan amount.

adopted their first LEZ between 2008 and 2018, with stringency ranging from Level 1 to Level 3. Many of these jurisdictions subsequently upgraded their LEZs in incremental steps ($1 \rightarrow 2$ and $2 \rightarrow 3$), and both adoptions and upgrades occurred at different points in time across counties. This staggered, multi-step rollout implies that counties are exposed to different treatment intensities at any given point in time. As a result, standard two-way fixed-effects estimators risk comparing units at different treatment stages and may yield biased estimates in the presence of heterogeneous treatment effects.

We address these challenges using a stacked Difference-in-Differences design. For each county that introduces or upgrades a LEZ, we assign an event identifier based on the month and year of the change and construct an observation window spanning two years before and two years after the event. All loans issued within this window receive the corresponding LEZ event ID. In parallel, we assign the same LEZ event IDs and windows to "not-yet-treated" counties (and, where relevant, never-treated counties), which serve as control groups. We refer to the set of loans in this LEZ event-specific window—both in the treated county and in the corresponding not-yet-treated (or never-treated) counties—as an LEZ event stack. This stacking procedure aligns treated and control observations in event time, treats each incremental LEZ change as a separate quasi-experiment, and avoids comparisons between units at different treatment stages, ensuring that identification comes from comparisons with appropriate not-yet-treated controls.

While used-car markets are not strictly local and vehicles can in principle be traded across regions, the identification strategy remains economically meaningful because exposure to LEZ restrictions is defined based on the borrower's place of residence rather than the dealer location. As a result, even when a vehicle is purchased in another county, the borrower resides in the treated area, and the usability of the vehicle is directly affected by the introduction or tightening of a LEZ. From the lender's perspective, the financed vehicle serves as collateral; therefore, these restrictions may influence the expected resale value of diesel vehicles that are used by borrowers living in the affected counties. Moreover, used-car markets are subject to search frictions and transportation costs that limit the extent to which vehicles are traded across distant regions. These frictions imply that local regulatory changes can still affect demand conditions and collateral risk for diesel vehicles.

Collapsed cohort first differences. To isolate the discontinuity generated by each LEZ event while ensuring comparisons between observationally similar loan contracts, we collapse the stacked dataset to cohort cells—defined by lender bank, car brand and model, fuel type, borrower's income group, state, LEZ treatment status within the LEZ event stack, and LEZ event stack s; then we compute the first difference in average loan terms between the post- and pre-event periods within each cohort. Formally, for cohort c in event stack s,

$$\Delta Y_c \equiv \bar{Y}_{c,\text{post}} - \bar{Y}_{c,\text{pre}},$$

where Y denotes the loan rate or other loan contract terms. Our baseline specification is

$$\begin{aligned} \Delta Y_c = {} & \beta_1 \, \text{LEZ}_c + \beta_2 \, \text{Diesel}_c + \beta_3 \big(\text{LEZ}_c \times \text{Diesel}_c\big) + X_c' \beta_4 \\ & + \alpha_{s(c)} + \alpha_{b(c)} + \alpha_{m(c)} + \alpha_{r(c)} + \varepsilon_c, \end{aligned} \tag{4}$$

where Diesel_c indicates diesel car loans and X_c includes borrower characteristics averaged within the cohort (in practice, log borrower income). The indicator LEZ_c equals one for cohorts belonging to counties that, within their event stack, experience an incremental one-level tightening of LEZ stringency—from $0 \to 1$, $1 \to 2$, or $2 \to 3$, and zero for cohorts in not-yet-treated (or never-treated) counties assigned to the same event stack. The fixed effects $\alpha_{s(c)}, \alpha_{b(c)}, \alpha_{m(c)}, \alpha_{r(c)}$ absorb heterogeneity across event-stacks, lender banks, car models, and states. Standard errors are clustered at the event-stack–by–state level. Because each event stack isolates a single one-step transition in LEZ stringency, equation (4) identifies the causal effect of incremental LEZ tightening on the relative change in loan conditions for diesel versus petrol vehicles.

To allow the effect of LEZ tightening to differ by the resulting stringency level, we augment equation (4) with indicators for the post-transition LEZ level and their interactions with the LEZ and Diesel dummies. Specifically, $L_{2,c}$ and $L_{3,c}$ indicate that cohort c belongs to an event stack in which the LEZ is tightened to Level 2 or Level 3; transitions to Level 1 serve as the baseline. Thus, $\text{LEZ}_c = 1$ flags any tightening from no LEZ to a positive level (Levels 1–3), and the coefficients β_1 and β_3 capture the effect of a transition from no LEZ to a Level-1 LEZ on petrol and diesel car loans, respectively. The additional coefficients $\beta_{k\ell}$ for $\ell \in \{2, 3\}$ then measure how these effects differ when the LEZ is tightened to Level 2 or Level 3 relative to a transition to Level 1. The heterogeneity specification is:

$$\begin{aligned} \Delta Y_c = {} & \beta_1 \, \text{LEZ}_c + \beta_2 \, \text{Diesel}_c + \beta_3 \big(\text{LEZ}_c \times \text{Diesel}_c\big) \\ & + \sum_{\ell \in \{2,3\}} \Big[\beta_{4,\ell} L_{\ell,c} + \beta_{5,\ell}(\text{LEZ}_c \times L_{\ell,c}) + \beta_{6,\ell}(\text{Diesel}_c \times L_{\ell,c}) + \beta_{7,\ell}(\text{LEZ}_c \times \text{Diesel}_c \times L_{\ell,c})\Big] \\ & + X_c' \beta_8 + \alpha_{s(c)} + \alpha_{b(c)} + \alpha_{m(c)} + \alpha_{r(c)} + \varepsilon_c. \end{aligned} \tag{5}$$

Table 9 reports the collapsed first-difference estimates for loan rates (Panel A) and LTV ratios (Panel B).[18] Columns 1–2 pool all incremental LEZ events and estimate the average effect of a one-step tightening as in equation (4), while Columns 3–4 implement specification (5) and compare transitions that raise stringency to Level 2 or Level 3 relative to transitions to Level 1. All specifications are estimated separately for captive banks (Columns 1 and 3) and independent banks (Columns 2 and 4).

The results for the specification (4) show that, following a tightening in LEZ stringency, independent banks charge higher loan rates for diesel cars, by 18 bps on average across all incremental LEZ events (Panel A, Column 2). In contrast, captive banks display no statistically significant variation in loan rates (Column 1). Allowing the effects to vary by the resulting LEZ level in specification (5) reveals substantial heterogeneity. When comparing the transitions to different levels of LEZ stringency, we find that the upgrades to more stringent LEZs (Level 2 or Level 3) lead to substantially tighter lending conditions for diesel vehicles by independent banks compared with the introduction of a less stringent Level 1 LEZ. Relative to the adoption of a Level 1 LEZ, independent banks increase diesel loan rates by approximately 43 bps when stringency is raised to Level 2, and by a further 32 bps when moving to Level 3 (Column 4). Given that upgrades to higher LEZ levels typically follow earlier transitions, these estimates imply a progressively stronger pricing of local regulatory risk for diesel vehicles by independent banks as circulation restrictions intensify. Captive banks

[18] Table C.7 in the Appendix reports results also on loan maturity, down payment and loan amount.

do not exhibit statistically significant changes in diesel loan rates across higher LEZ levels. The tendency of independent banks to price LEZ-related regulatory risk through higher loan rates aligns with existing evidence from consumer credit markets showing that, in standardized arm's-length lending environments, changes in expected losses are commonly reflected in interest rate adjustments (Einav et al., 2013).

The analysis of loan-to-value ratios (Panel B) reveals a markedly different adjustment pattern for captive banks. On average, captive lenders reduce LTV ratios for diesel car loans following the first introduction of a LEZ, consistent with an early tightening of collateral requirements when regulatory risk becomes salient but remains uncertain. As LEZ stringency increases further, however, captive banks reverse this pattern and raise LTV ratios. Relative to Level 1 LEZs, captive lenders increase LTV ratios on diesel car loans by 13 pp when stringency is raised to Level 2, and by an additional 12 pp when moving to Level 3 (Column 3). This dynamic response suggests that, once diesel-related regulatory risk is largely priced into beliefs, captive banks shift from collateral risk management toward demand support to offset growing sales pressure associated with increasingly binding circulation restrictions.[19]

Taken together, the evidence from Dieselgate and LEZ adoptions shows that lender incentives—shaped by organizational features such as vertical integration—play a central role in determining whether and how adjustment in loan conditions occurs after environmental shocks.

5.1 Loan-level Analysis

We next bring the analysis back to the loan level. The goal is to verify that the diesel pricing effects documented in the collapsed cohort regressions are also present in the underlying loan-level data, and to exploit the richer variation in individual loan contracts to study heterogeneity by LEZ stringency and lender type.

As in the cohort analysis, we consider incremental LEZ transitions ($0\to1$, $1\to2$, $2\to3$), but estimation is now conducted at the level of individual loans. Let Y_{ijst} denote the loan rate (or other contract term) of loan i issued in county j, in event stack s, at calendar month t. The variable Diesel_i indicates diesel vehicles, and Post_{ist} equals one if the loan is originated after the LEZ tightening in its event stack. Because only counties undergoing a one-step change appear in a given event stack, Post_{ist} captures the discrete regulatory shift for the treated counties.

Within each event stack, the treated group consists of loans issued in the county implementing the LEZ tightening, while the control group consists of loans issued contemporaneously in counties that have not yet reached the same LEZ stringency level. Identification therefore comes from comparing diesel and petrol car loans in treated counties to suitably aligned not-yet-treated control counties before and after the transition. Our benchmark loan-level specification is:

$$\begin{aligned} Y_{ijst} = {} & \beta_1 \,\text{Diesel}_i + \beta_2 \,\text{Post}_{ist} + \beta_3 \big(\text{Diesel}_i \times \text{Post}_{ist}\big) + X_i'\beta_4 \\ & + \alpha_{s\times t} + \alpha_{s\times \text{model}(i)} + \alpha_{s\times \text{state}(j)\times \text{bank}(i)\times t} + \varepsilon_{ijst}, \end{aligned} \tag{6}$$

[19] This interpretation is consistent with evidence that suggests that the vertical integration between captive banks and manufacturers may mitigate informational frictions in collateral valuation and thereby facilitate risk management via collateral pricing (Murfin & Pratt, 2019).

where X_i contains borrower characteristics (log income). The fixed effects $\alpha_{s\times t}$, $\alpha_{s\times \text{model}(i)}$, and $\alpha_{s\times \text{state}(j)\times \text{bank}(i)\times t}$ absorb, respectively, stack-by-month shocks, stack-by-model heterogeneity, and stack-by-state-by-bank-by-month pricing differences. Standard errors are clustered at the county-by-stack level. We estimate equation (6) separately for captive and independent banks.

Most incremental LEZ transitions in our data involve upgrades to Level 3 (see Table 8), yielding substantial identifying variation at this stringency level. We therefore first estimate a treated-only version of equation (6), restricting the sample to Level 3 transitions and counties that tighten their LEZ in the corresponding event stack. In these regressions, petrol car loans serve as an internal control group within treated counties, and the coefficient on $\text{Diesel}_i \times \text{Post}_{ist}$ measures the change in the diesel–petrol spread induced by a Level-3 tightening, under the assumption that, absent the policy, diesel and petrol car loan terms would have followed parallel trends within the same county. Table 10 reports the results for captive and independent banks.

The loan-level estimates for Level-3 LEZ tightening (Columns 1 and 2) support the collapsed cohort-level first-difference results, confirming that our main findings are not an artefact of the collapsing and differencing procedure. Independent banks increase loan rates for diesel car loans by about 11 bps relative to petrol car loans following the transition to a Level-3 LEZ, while no statistically significant effect is observed for captive banks. These estimates should be interpreted as within-treated Level-3 effects rather than as fully causal comparisons across LEZ levels, since they do not incorporate not-yet-treated control counties from other stringency levels.

To explore how banks adjust car loan conditions in response to increasing LEZ stringency within treated counties, we extend the baseline loan-level specification by expanding the sample beyond Level 3 transitions to include multiple LEZ levels and interacting the diesel-post indicator with dummies for the post-transition LEZ level. Let $L_{\ell,s}$ denote that event stack s corresponds to an upgrade raising the LEZ to level $\ell \in \{2,3\}$ (transitions from Level 0 to Level 1 are not included in this specification). Restricting the sample to treated counties, we estimate:

$$\begin{aligned} Y_{ijst} = {} & \beta_1 \,\text{Diesel}_i + \beta_2 \,\text{Post}_{ist} + \beta_3 \,(\text{Diesel}_i \times \text{Post}_{ist}) \\ & + \sum_{\ell \in \{2,3\}} \psi_\ell \,(\text{Diesel}_i \times \text{Post}_{ist} \times L_{\ell,s}) + X_i'\beta_4 \\ & + \alpha_{s\times t} + \alpha_{s\times \text{model}(i)} + \alpha_{s\times \text{state}(j)\times \text{bank}(i)\times t} + \varepsilon_{ijst}, \end{aligned} \tag{7}$$

The coefficients ψ_ℓ capture how the diesel–petrol gap in loan conditions changes after the LEZ upgrade to Level 2 or Level 3 - relative to counties with an existing Level 1, within the set of treated counties. The resulting estimates are reported in Columns 3 and 4 of Table 10.

In counties with an existing LEZ, independent banks charge higher loan rates for diesel cars versus petrol cars, by about 20 bps. Notably, independent banks further increase this loan spread for diesel cars relative to petrol cars after the upgrades to more stringent LEZs, respectively by 38 bps in the transitions from Level 1 to Level 2 and by 29 bps in the tightening from Level 2 to Level 3 (Column 4). On the contrary, captive banks - in counties with an existing LEZ - charge lower loan rates for diesel cars versus petrol cars, on average by 25 bps; moreover, they show no statistically significant changes in diesel loan rates after the upgrades to more stringent LEZs (Column 3). Turning to loan-to-value ratios, the loan-level evidence closely mirrors the cohort-level mechanism. Captive banks first reduce LTV ratios at the initial introduction of a LEZ, but then increase LTV ratios following the upgrade to higher LEZ stringency levels.[20]

We further develop our loan-level analysis on LEZs, by estimating - as robustness - stacked DiD specifications that incorporate both treated and not-yet-treated counties and allow the diesel–post effect to vary with LEZ stringency. We present the results in Tables C.9 and C.10 in the Appendix. These regressions estimate a triple-difference coefficient on LEZ event × Diesel × Post and its interactions with LEZ stringency level, providing a causal loan-level analogue to the collapsed cohort-level heterogeneity specification in equation (5). The results are broadly consistent with the findings in Table 9: transitions to more stringent LEZs (Level 2 or Level 3) imply substantially tighter lending conditions for diesel vehicles by independent banks relative to the introduction of a less stringent LEZ (Level 1). In particular, relative to the diesel car loan rates in Level 1 LEZs, independent banks increase diesel loan rates by 48 bps in the upgrade from Level 1 to Level 2, and by a further 37 bps in the transition from Level 2 to Level 3.

Finally, we exploit the loan-level data granularity to explore heterogeneity in bank lending behavior by brand affiliation, paralleling the analysis for the diesel emissions scandal. We augment the specification (6) to investigate whether captive banks set different lending conditions for cars of their own brands relative to cars of other brands, and relative to independent banks. The results are reported in the Appendix, in Table C.11 for the transitions to Level 3 and Table C.12 for all incremental changes in LEZ stringency.[21] They suggest that after a tightening in LEZ stringency captive banks may have some incentive to treat diesel cars of other brands less favorably than those of their own brands. In Table C.12, the quadruple interaction coefficient on LEZ-treated × Post × Diesel × Own-brand in Column 1 indicates a sizeable, though statistically non-significant, reduction in loan rates of about 30 bps for diesel cars of the captive bank's own brand. By contrast, the quadruple interaction on LEZ-treated × Post × Diesel × Captive in Column 3 shows that captive banks apply significantly higher loan rates (by 52 bps) for diesel cars of other brands relative to independent banks financing the same car model.

To summarize, the loan-level LEZ analysis highlights a relevant difference with the diesel emissions scandal. While the information shock due to Dieselgate allows captive banks to influence purchasing decisions via more accommodative financing, LEZs impose direct and binding restrictions on vehicle usage. As a result, the scope for captive banks to offset the policy shock through more favorable lending terms is somehow more limited under circulation restrictions.

5.2 Local Pollution Levels Preceding Low-Emission Zones

The LEZ analysis identifies the impact of realized regulatory risk by examining how banks adjust loan terms when diesel vehicle restrictions are introduced or tightened. To explore potential anticipation effects prior to LEZ implementation, we conduct a robustness check using an alternative event that captures transition

[20] Table C.8 in the Appendix presents the loan-level results under the same specification for loan maturity, down payment, and loan amount.

[21] The corresponding results of the loan-level analysis for loan maturity, down payment and loan amount are shown in Tables C.13 and C.14 in the Appendix.

risk before policy realization: the first year in which local air pollution exceeds the EU annual-mean nitrogen dioxide (NO_2) limit of $40\,\mu g/m^3$ at any monitoring station within a county.

The institutional motivation is that EU air-quality rules set a legally binding annual-mean limit of $40\,\mu g/m^3$ for nitrogen dioxide. Accordingly, we classify a county as non-compliant in a given year if the maximum station-level annual mean NO_2 concentration observed within the county exceeds this threshold. Exceeding the limit marks the onset of heightened public and regulatory pressure and may anticipate the future introduction or tightening of a low-emission zone, as it increases the likelihood of traffic-related interventions affecting diesel vehicles even before a formal decision by the local authorities.[22]

We mirror the baseline LEZ methodology. Specifically, we construct stacked event-time samples around the first breach of the NO_2 limit, using two-year pre- and post-event windows. Counties that never breach the limit are assigned as never-treated controls within each stack. To ensure comparability across events, never-treated counties are re-used across stacks, following the stacked event-study design employed in the LEZ analysis. We then collapse the stacked data to cohort cells defined by lender bank, car model and brand, fuel type, borrower's income group, and county, and compute first differences in average loan terms between the post- and pre-event periods. Formally,

$$\Delta Y_c \equiv \bar{Y}_{c,\text{post}} - \bar{Y}_{c,\text{pre}},$$

where Y denotes the loan-level outcome variables. Our baseline specification is:

$$\begin{aligned}\Delta Y_c = \beta_1\, \mathbf{1}\{NO_2 > 40\}_c + \beta_2\, Diesel_c + \beta_3\big(\mathbf{1}\{NO_2 > 40\}_c \times Diesel_c\big) + X_c'\beta_4 \\ + \alpha_{s(c)} + \alpha_{b(c)} + \alpha_{m(c)} + \alpha_{r(c)} + \varepsilon_c,\end{aligned} \tag{8}$$

where $\mathbf{1}\{NO_2 > 40\}_c$ equals one for cohorts located in counties that breach the limit in their corresponding event stack and zero for cohorts in never-treated control counties assigned to the same stack. The vector X_c includes borrower characteristics averaged within cohort (log income). Standard errors are clustered at the event-stack-by-state level. The coefficient β_3 captures the differential change in loan conditions for diesel relative to petrol vehicles following the observation of NO_2 concentrations above the regulatory threshold.

The results for loan rates and LTV ratios are presented in Table 11, with additional outcome variables (loan maturity, down payment, and loan amount) presented in Table C.15 in the Appendix. Overall, we find no evidence that the recording of above-threshold pollution levels induces anticipation adjustments in bank lending behavior, either by captive or independent banks. In particular, loan conditions for diesel vehicles do not change differentially relative to petrol vehicles following the first breach of the NO_2 limit, prior to the formal introduction or tightening of a LEZ.

This conclusion is confirmed when we replicate the analysis at the loan level, exploiting the full granularity

[22] Under EU Directive 2008/50/EC, persistent exceedances require competent authorities to adopt air-quality plans aimed at restoring compliance "as soon as possible." In Germany, exceedances of the NO_2 limit have been a focal trigger for enforcement and litigation; in particular, Deutsche Umwelthilfe (DUH) has brought numerous cases against cities and states for failing to meet the NO_2 standard, increasing the likelihood of traffic-related interventions (including diesel restrictions) in high-NO_2 areas. Figure B.4 shows a map of the locations in Germany where the NO_2 threshold has been surpassed prior to 2018.

of the data. The corresponding loan-level estimates, reported in Table C.16 (Appendix), likewise show no evidence of anticipation effects in car loan conditions ahead of realized LEZ policy interventions.

6 Conclusions

A central question in environmental policy is whether and how regulatory intervention shapes real economic behavior once financial intermediaries and market responses are taken into account. Environmental regulation aims to reduce socially costly externalities but often relies on market mechanisms for their transmission. For environmental policies to be effective, financing conditions must reinforce, rather than counteract, sustainability objectives. This transmission mechanism is especially important in the transportation sector, where the negative externalities associated with hazardous pollutants—particularly from diesel vehicles—pose significant risks to the environment and public health.

In this paper, we use loan-level data on used car purchases to study how different types of lenders—captive versus independent banks—adjust credit conditions in response to environmental shocks. We exploit two distinct sources of variation affecting diesel vehicles: the 2015 diesel emissions scandal, which represents an information disclosure shock that altered beliefs about environmental quality without immediately constraining vehicle usability; and the staggered introduction of Low-Emission Zones (LEZs), which constitutes a regulatory shock that directly restricts vehicle use. Because both shocks threaten the long-term value of diesel vehicles, through either changes in product demand or direct impairments to asset usability, lenders would be expected to adjust loan conditions to reflect the heightened expected risk of diesel car loans. However, we show that vertical integration between manufacturing and finance systematically dampens—or offsets—the pricing of environmental risk in car loans.

Our results show that the diesel emissions scandal, despite sharply increasing awareness about diesel-related pollution, did not lead to a broad-based tightening of credit conditions for diesel vehicles. Independent banks exhibit no significant repricing of diesel car loans following the information disclosure. Captive banks, by contrast, ease financing conditions by lowering interest rates and relaxing collateral requirements, particularly for vehicles produced by their parent manufacturers. These patterns indicate that increased transparency about environmental performance alone is insufficient to induce market-wide pricing of diesel-related risk especially when lenders face incentives to support product demand and residual values.

In contrast, the introduction of binding circulation restrictions generates a markedly different response. Following the implementation or tightening of LEZs, independent banks raise lending rates for diesel relative to petrol vehicles, with risk premia increasing as regulatory stringency intensifies. This pattern indicates a progressive pricing of realized regulatory risk as circulation restrictions become more binding. Captive banks respond differently, mainly via collateral terms. They tighten LTV ratios at initial LEZ adoption, when regulatory risk becomes salient but remains uncertain. As LEZ stringency increases further, however, captive banks relax collateral requirements and raise LTV ratios, indicating a shift toward demand accommodation once restrictions become more binding and sales pressure intensifies.

Our analysis highlights how lender incentives and organizational structure shape the transmission of environmental policy through credit markets. Environmental risk is not priced uniformly in consumer credit markets: vertically integrated captive finance arms systematically attenuate the pass-through of both informational and regulatory environmental shocks to borrowing conditions. In particular, information disclosure alone—such as the diesel emissions scandal—fails to induce widespread repricing, while binding regulatory interventions, such as the introduction and tightening of low-emission zones, are priced by independent banks but only partially transmitted by captive lenders. As a result, captive finance can weaken the alignment between environmental policy objectives and market outcomes by sustaining credit supply for high-polluting assets precisely when those assets become riskier.

These dynamics have important implications for the design of environmental regulation in sectors - like transportation - where financing plays a central role, especially in the context of increasingly stringent emissions standards, expanding circulation restrictions for high-polluting vehicles, and the prospective phase-out of internal combustion engine technologies.

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Table 1: Variable Definitions and Sources

Variable	Description	Source
Loan characteristics		
Loan rate (% per annum)	Interest rate at origination	EDW
Loan-to-value (%)	Loan amount over car value at origination	EDW
Loan maturity (months)	Contractual maturity at origination	EDW
Down payment (1000 EUR)	Down payment amount at origination	EDW
Loan amount (1000 EUR)	Loan amount at origination	EDW
Borrower characteristics		
Geographical location	Borrower's location at loan origination, specified at the NUTS3 level	EDW
Borrower income (1000 EUR)	Borrower's primary gross annual income	EDW
Employment status	Different dummies indicating the employment status of borrower (employed, self-employed, student, pensioner, unemployed)	EDW
Car characteristics		
Brand	Brand name of car	EDW
Model	Model of car	EDW
Classification	Car type (as defined by the European Commission)	EDW

Notes: This table reports variable definitions and data sources for the loan-level dataset obtained from the European DataWarehouse.

Table 2: Descriptive Statistics

	mean	sd	p25	p50	p75	count
Loan rate (% per annum)	7.23	2.40	5.15	7.00	9.40	843873
Loan maturity (months)	55.21	16.95	48.00	60.00	60.00	843888
Loan-to-value (%)	66.35	34.97	35.00	70.00	100.00	843559
Down payment amount (1000 EUR)	5.53	5.67	0.80	4.00	8.79	843698
Original principal balance (1000 EUR)	8.27	5.34	4.00	7.28	10.94	843889
Original car valuation (1000 EUR)	13.65	5.10	9.99	12.90	16.60	582835
Primary income (1000 EUR)	26.25	17.89	16.00	22.00	32.00	779317
Share captive banks (0/1)	0.68	0.47	0.00	1.00	1.00	843889
Share captive banks' own car brand (0/1)	0.92	0.28	1.00	1.00	1.00	574234
Car make (0/1)						
ALFA ROMEO	0.00	0.06	0.00	0.00	0.00	843889
AUDI	0.04	0.21	0.00	0.00	0.00	843889
BMW	0.07	0.25	0.00	0.00	0.00	843889
CITROEN	0.31	0.46	0.00	0.00	1.00	843889
DACIA	0.01	0.09	0.00	0.00	0.00	843889
FIAT	0.01	0.10	0.00	0.00	0.00	843889
PEUGEOT	0.35	0.48	0.00	0.00	1.00	843889
RENAULT	0.08	0.27	0.00	0.00	0.00	843889
SEAT	0.02	0.15	0.00	0.00	0.00	843889
SKODA	0.03	0.16	0.00	0.00	0.00	843889
VW	0.07	0.26	0.00	0.00	0.00	843889
Share of obs. with car models (0/1)	0.87	0.34	1.00	1.00	1.00	843889
Borrower occupation type (0/1)						
Employed	0.63	0.48	0.00	1.00	1.00	843889
Unemployed	0.01	0.11	0.00	0.00	0.00	843889
Self-employed	0.06	0.24	0.00	0.00	0.00	843889
Pensioner	0.15	0.35	0.00	0.00	0.00	843889
Other	0.14	0.35	0.00	0.00	0.00	843889
Loan origination country (0/1)						
Germany	0.35	0.48	0.00	0.00	1.00	843889
France	0.51	0.50	0.00	1.00	1.00	843889
Spain	0.10	0.30	0.00	0.00	0.00	843889
Italy	0.05	0.21	0.00	0.00	0.00	843889
Share of obs. with county (0/1)	0.97	0.16	1.00	1.00	1.00	843889
Loan origination year (0/1)						
2008	0.01	0.08	0.00	0.00	0.00	843889
2009	0.03	0.18	0.00	0.00	0.00	843889
2010	0.05	0.22	0.00	0.00	0.00	843889
2011	0.08	0.28	0.00	0.00	0.00	843889
2012	0.11	0.31	0.00	0.00	0.00	843889
2013	0.12	0.32	0.00	0.00	0.00	843889
2014	0.14	0.34	0.00	0.00	0.00	843889
2015	0.15	0.36	0.00	0.00	0.00	843889
2016	0.14	0.35	0.00	0.00	0.00	843889
2017	0.12	0.33	0.00	0.00	0.00	843889
2018	0.05	0.23	0.00	0.00	0.00	843889

Notes: This table reports descriptive statistics for the baseline car loan sample over 2008–2018 (see Table 1 for variable definitions). The sample excludes observations with missing identifiers for bank, make, model, county, or fuel type. Statistics include the number of observations, mean, standard deviation, minimum, and maximum. All numeric variables are winsorized at the 0.1/99.9 percentiles, except Loan-to-Value (%), which is winsorized at the 0.1/99.5 percentiles.

Table 3: Effect of Diesel Emission Scandal on Diesel Car Loan Conditions

	Captive (1)	Independent (2)	All (3)
Panel A: Δ Loan Rate (p.p.)			
Diesel	-0.091*	-0.012	0.006
	(0.048)	(0.029)	(0.036)
Diesel × Captive			-0.122**
			(0.055)
Fixed effects: Model & Bank-Country	Yes	Yes	Yes
Borrower control: Income	Yes	Yes	Yes
R-squared	0.818	0.340	0.782
Observations	253	290	545
Mean loans per collapsed cell	848.8	407.5	611.5
Panel B: Δ Loan-to-Value Ratio (p.p.)			
Diesel	1.396***	0.912*	0.910*
	(0.490)	(0.520)	(0.481)
Diesel × Captive			0.868
			(0.712)
Fixed effects: Model & Bank-Country	Yes	Yes	Yes
Borrower control: Income	Yes	Yes	Yes
R-squared	0.567	0.339	0.540
Observations	253	290	545
Mean loans per collapsed cell	848.8	407.5	611.5

Notes: This table reports collapsed-cohort first-difference estimates of the effect of the 2015 Diesel emissions scandal on loan conditions for diesel relative to petrol vehicles. Loans are grouped into narrowly defined cohorts based on lender bank, vehicle brand and model, fuel type, borrower country, and borrower income quintile. For each cohort, the outcome variable is the change in the cohort mean between the pre- and post-scandal periods. Columns (1) and (2) report estimates separately for captive and independent banks; Column (3) pools all lenders and includes an interaction between diesel status and captive bank affiliation. All specifications include car model fixed effects and bank-by-country fixed effects, as well as cohort-level averages of borrower income. Standard errors are robust and clustered at the model × fuel × bank level. Parentheses report standard errors. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 4: Pre-Treatment Differences Between Diesel and Petrol Car Loans Prior to the Diesel Emissions Scandal

	Control mean	Treatment mean	Difference mean	Difference SE	Ftest	Observations
			Captive banks, 2013			
Loan rate (% per annum)	7.96	7.97	0.01	0.02	0.505	54510
Loan-to-value (%)	66.23	67.83	1.6	0.46	<0.001	54510
			Captive banks, 2014			
Loan rate (% per annum)	7.82	7.81	-0.01	0.02	0.453	63572
Loan-to-value (%)	65.4	66.56	1.16	0.4	0.004	63572
			Independent banks, 2013			
Loan rate (% per annum)	5.78	5.84	0.06	0.02	0.003	29309
Loan-to-value (%)	92	93.12	1.12	0.38	0.004	29298
			Independent banks, 2014			
Loan rate (% per annum)	5.54	5.61	0.07	0.02	<0.001	38004
Loan-to-value (%)	90.18	90.62	0.44	0.31	0.161	37998

This table compares pre-treatment loan characteristics between diesel (treatment) and petrol (control) car loans separately by bank type and year. Columns (1)–(2) report group means. Columns (3)–(4) report the estimated diesel–petrol difference and its standard error from loan-level regressions of the respective outcome on a diesel indicator, including model × bank × country × income-group fixed effects, which span the cohort-defining dimensions. Column (5) reports the p-value of the test that the diesel coefficient equals zero. Standard errors are clustered at the cohort level (model × bank × country × income group × fuel type).

Table 5: Effect of Diesel Emission Scandal on Diesel Car Loan Conditions: Loan-Level

	Captive (1)	Independent (2)	All (3)
Panel A: Loan Rate (p.p.)			
Post	-0.104	-0.299***	-0.321***
	(0.064)	(0.043)	(0.044)
Diesel	0.403***	0.348***	0.328***
	(0.068)	(0.079)	(0.084)
Post × Diesel	-0.270***	-0.045	-0.020
	(0.044)	(0.061)	(0.063)
Post × Captive			0.263***
			(0.083)
Diesel × Captive			0.123
			(0.113)
Post × Diesel × Captive			-0.304***
			(0.079)
Observations	253,799	145,949	399,758
Adj. R^2	0.201	0.238	0.396
Fixed effects: Model & Bank-Year	Yes	Yes	Yes
Borrower control: Income	Yes	Yes	Yes
Panel B: Loan-to-Value Ratio (p.p.)			
Post	-2.654**	-0.114	0.018
	(1.284)	(0.497)	(0.500)
Diesel	-2.757***	-0.373	0.453
	(0.758)	(0.495)	(0.613)
Post × Diesel	4.931***	1.942***	1.807***
	(1.279)	(0.442)	(0.469)
Post × Captive			-2.920**
			(1.432)
Diesel × Captive			-3.924***
			(1.062)
Post × Diesel × Captive			3.456**
			(1.401)
Observations	253,799	145,945	399,754
Adj. R^2	0.164	0.026	0.329
Fixed effects: Model & Bank-Year	Yes	Yes	Yes
Borrower control: Income	Yes	Yes	Yes

Notes: This table reports loan-level difference-in-differences estimates of the effect of the 2015 Diesel emissions scandal on loan conditions for diesel relative to petrol vehicles. The sample consists of amortizing used-car loans originated between September 2013 and September 2017. *Post* is an indicator for loans originated after September 2015, and *Diesel* identifies diesel vehicles. Columns (1) and (2) report estimates separately for captive and independent banks; Column (3) pools all lenders and includes interactions with captive bank affiliation. All specifications include car model fixed effects and bank-by-year fixed effects, as well as borrower income as a control. Standard errors are robust and clustered at the model × fuel × bank level. Parentheses report standard errors. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 6: Heterogeneity in Loan Conditions by Lender Type and Own-Brand Financing

	Captive (own-brand + no own-brand) (1)	Captive + Independent (own-brand) (2)	Captive + Independent (no own-brand) (3)
Panel A: Loan Rate (p.p.)			
Post	-0.331***	-0.316***	-0.309***
	(0.105)	(0.044)	(0.044)
Diesel	0.322***	0.342***	0.340***
	(0.098)	(0.088)	(0.077)
Own-brand	-0.213*		
	(0.116)		
Post × Diesel	-0.084	-0.029	-0.034
	(0.103)	(0.064)	(0.062)
Post × Own-brand	0.239**		
	(0.098)		
Post × Captive		0.271***	0.134
		(0.088)	(0.097)
Diesel × Own-brand	0.081		
	(0.120)		
Diesel × Captive		0.118	-0.022
		(0.120)	(0.109)
Post × Diesel × Own-brand	-0.195*		
	(0.113)		
Post × Diesel × Captive		-0.311***	-0.178
		(0.081)	(0.113)
Observations	253,799	383,210	162,497
Adj. R^2	0.201	0.400	0.300
Fixed effects: Model & Bank-Year	Yes	Yes	Yes
Borrower control: Income	Yes	Yes	Yes
Panel B: Loan-to-Value Ratio (p.p.)			
Post	-2.656**	0.041	-0.135
	(1.268)	(0.500)	(0.498)
Diesel	0.910	0.376	-0.291
	(1.283)	(0.626)	(0.488)
Own-brand	4.769***		
	(1.381)		
Post × Diesel	2.440*	1.751***	2.010***
	(1.365)	(0.469)	(0.444)
Post × Own-brand	0.018		
	(1.580)		
Post × Captive		-2.994**	-1.017
		(1.503)	(1.462)
Diesel × Own-brand	-3.838***		
	(1.484)		
Diesel × Captive		-4.019***	0.174
		(1.105)	(1.298)
Post × Diesel × Own-brand	2.651		
	(1.905)		
Post × Diesel × Captive		3.629**	0.317
		(1.475)	(1.370)
Observations	253,799	383,206	162,493
Adj. R^2	0.164	0.334	0.107
Fixed effects: Model & Bank-Year	Yes	Yes	Yes
Borrower control: Income	Yes	Yes	Yes

Notes: This table reports loan-level estimates of the effect of the 2015 Dieselgate shock on loan conditions for diesel relative to petrol vehicles, allowing for heterogeneity by captive bank affiliation and own-brand financing. Column (1) restricts the sample to loans originated by captive banks and includes both own-brand and non–own-brand vehicles. Column (2) compares loans originated by captive banks for own-brand vehicles with loans extended by independent banks for the same car models. Column (3) compares loans extended by captive banks for non–own-brand vehicles with loans originated by independent banks for the same car models. *Own-brand* indicates loans originated by a captive bank for vehicles produced by its parent manufacturer. All specifications include car model fixed effects and bank-by-year fixed effects, as well as borrower income controls. Robust standard errors clustered at the model × fuel × bank level are reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 7: Heterogeneity in Loan Conditions by Manufacturer Captive Finance Strength

	Independent (1)	Captive no own-brand (2)	Captive own-brand (3)
Panel A: Loan Rate (p.p.)			
Post × Diesel	3.478**	1.466	-1.650***
	(1.451)	(0.909)	(0.374)
Captive support	0.048	0.097***	
	(0.061)	(0.037)	
Post × Captive support	0.079***	0.022	-0.006
	(0.030)	(0.022)	(0.012)
Diesel × Captive support	-0.007	-0.025	-0.048***
	(0.056)	(0.020)	(0.011)
Post × Diesel × Captive support	-0.105**	-0.045*	0.044***
	(0.043)	(0.026)	(0.011)
Observations	118,186	15,152	234,236
Adj. R^2	0.255	0.347	0.178
Fixed effects: Model & Bank-Year	Yes	Yes	Yes
Borrower control: Income	Yes	Yes	Yes
Panel B: Loan-to-Value Ratio (p.p.)			
Post × Diesel	-14.637*	-43.128***	25.921***
	(7.957)	(13.040)	(9.408)
Captive support	-0.018	0.406	
	(0.472)	(0.802)	
Post × Captive support	-0.403***	-1.038***	-0.207
	(0.146)	(0.317)	(0.255)
Diesel × Captive support	0.312	-0.691**	0.344**
	(0.317)	(0.327)	(0.144)
Post × Diesel × Captive support	0.492**	1.318***	-0.677**
	(0.235)	(0.376)	(0.273)
Observations	118,185	15,152	234,236
Adj. R^2	0.029	0.214	0.150
Fixed effects: Model & Bank-Year	Yes	Yes	Yes
Borrower control: Income	Yes	Yes	Yes

Notes: This table reports loan-level estimates of the effect of the 2015 Dieselgate shock on loan conditions for diesel relative to petrol vehicles, allowing for the interaction with a proxy for captive finance strength. *Captive support* is measured as the manufacturer-level share (in percent) of new-vehicle financing provided by the manufacturer's captive lender at the European level. This penetration rate is constant within a brand (30.7 for Peugeot/Citroën, 33.6 for VW/SEAT/Škoda/Audi, 36.4 for Renault/Dacia, and 42.2 for Fiat/Alfa Romeo), so variation in captive support arises primarily from differences across manufacturer groups rather than within-brand variation. All specifications include car model fixed effects and bank-by-year fixed effects, as well as borrower income controls. Robust standard errors clustered at the model × fuel × bank level are reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 8: Timing and stringency of Low-Emission Zone (LEZ) adoptions and upgrades

Panel A: First adoption of LEZs by level

	Initial level of first LEZ			
Year	Level 1	Level 2	Level 3	Total
2008	11	0	0	11
2009	11	0	0	11
2010	6	1	0	7
2011	0	4	1	5
2012	11	2	1	14
2013	0	4	4	8
2015	0	0	3	3
2016	0	0	3	3
2017	0	0	2	2
2018	0	0	2	2
Total	39	11	16	66

Panel B: Upgrades of LEZ level

	Upgrades		
Year	L1→L2	L2→L3	Total
2009	1	0	1
2010	5	1	6
2011	5	1	6
2012	16	6	22
2013	12	18	30
2014	0	19	19
2015	0	2	2
2016	0	1	1
Total	39	48	87

Notes: Each observation corresponds to a German county (*Kreis*). Panel A reports the calendar year and initial stringency level of the first Low-Emission Zone (LEZ) introduced in each county. Panel B reports subsequent one-level upgrades from Level 1 to Level 2 and from Level 2 to Level 3. Totals in Panel B sum upgrades across years; a county may contribute with multiple upgrades over time.

Table 9: Effect of LEZ on Diesel Car Loan Conditions

	All incremental LEZ events		By LEZ level (ref: Level 1)	
	Captive Banks (1)	Independent Banks (2)	Captive Banks (3)	Independent Banks (4)
Panel A: Δ Loan Rate (p.p.)				
LEZ event	-0.183	-0.291**	0.084***	0.136***
	(0.149)	(0.123)	(0.030)	(0.031)
Diesel	0.038**	-0.037***	0.041**	-0.038***
	(0.019)	(0.010)	(0.019)	(0.010)
Diesel × LEZ event	0.020	0.182**	-0.093	-0.218***
	(0.120)	(0.071)	(0.276)	(0.011)
Diesel × LEZ event × LEZ Level 2			0.187	0.425**
			(0.317)	(0.204)
Diesel × LEZ event × LEZ Level 3			0.045	0.319***
			(0.375)	(0.097)
R-squared	0.668	0.106	0.669	0.107
Observations	10206	37191	10206	37191
Mean loans per collapsed cell	25.7	26.6	25.7	26.6
Panel B: Δ Loan-to-Value Ratio (p.p.)				
LEZ event	-1.078	-0.572	6.913***	0.621
	(2.243)	(1.830)	(0.914)	(0.516)
Diesel	0.345	0.254	0.484	0.244
	(0.411)	(0.232)	(0.420)	(0.232)
Diesel × LEZ event	-5.278***	-0.874	-14.429***	-2.547***
	(1.601)	(0.624)	(2.456)	(0.226)
Diesel × LEZ event × LEZ Level 2			13.224***	7.415**
			(3.392)	(3.632)
Diesel × LEZ event × LEZ Level 3			12.295***	-0.566
			(3.917)	(1.544)
R-squared	0.089	0.042	0.091	0.042
Observations	10206	37191	10206	37191
Mean loans per collapsed cell	25.7	26.6	25.7	26.6
Borrower income	Yes	Yes	Yes	Yes
Event-stack FE	Yes	Yes	Yes	Yes
Model FE	Yes	Yes	Yes	Yes
Bank FE	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes

Notes: This table reports collapsed-cohort first-difference estimates of the effect of low-emission zone (LEZ) introduction or tightening on loan conditions for diesel relative to petrol vehicles in Germany. The sample is constructed using a stacked difference-in-differences design in which each LEZ adoption or upgrade defines an event stack with a two-year pre- and post-event window. Within each event stack, loans are collapsed to cohort cells and outcomes are measured as first differences between the post- and pre-event periods. Columns (1)–(2) pool all incremental LEZ events and estimate the average effect of a one-step tightening. Columns (3)–(4) allow the effects to vary by the resulting LEZ stringency level; coefficients on triple interactions measure differences relative to transitions to Level 1 LEZs. All specifications include stack-group, car model, bank, and state fixed effects, as well as borrower income controls. In this setting, state refers to a German Land. Robust standard errors clustered at the event-stack–by–state level are reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 10: Effect of LEZ on Diesel Car Loan Conditions: Loan-Level

	LEZ Level 3 only		By LEZ level (ref: Level 1)	
	Captive Banks (1)	Independent Banks (2)	Captive Banks (3)	Independent Banks (4)
Panel A: Loan Rate (p.p.)				
Diesel	-0.113	0.060	-0.254**	0.199***
	(0.091)	(0.041)	(0.114)	(0.071)
Post × Diesel	0.083	0.107*	-0.045	-0.185
	(0.099)	(0.055)	(0.157)	(0.134)
Diesel × LEZ Level 2			0.058	-0.262**
			(0.151)	(0.122)
Diesel × LEZ Level 3			0.139	-0.140*
			(0.147)	(0.083)
Post × Diesel × LEZ Level 2			0.149	0.376**
			(0.196)	(0.170)
Post × Diesel × LEZ Level 3			0.133	0.291**
			(0.187)	(0.145)
Observations	4,014	12,054	9,405	19,185
Adj. R^2	0.405	0.265	0.327	0.304
Panel B: Loan-to-Value Ratio (p.p.)				
Diesel	-2.405	0.748	3.834	1.951
	(2.206)	(1.031)	(2.830)	(1.547)
Post × Diesel	1.217	0.609	-6.583*	-3.893
	(2.771)	(1.269)	(3.588)	(3.192)
Diesel × LEZ Level 2			-5.042	-0.731
			(3.402)	(2.529)
Diesel × LEZ Level 3			-6.251*	-1.219
			(3.598)	(1.872)
Post × Diesel × LEZ Level 2			6.309	4.466
			(4.278)	(3.799)
Post × Diesel × LEZ Level 3			7.829*	4.499
			(4.548)	(3.446)
Observations	4,014	12,052	9,403	19,181
Adj. R^2	0.111	0.030	0.106	0.032
Borrower income	Yes	Yes	Yes	Yes
Event-stack×Month FE	Yes	Yes	Yes	Yes
Event-stack×Model FE	Yes	Yes	Yes	Yes
Event-stack×State×Bank×Month FE	Yes	Yes	Yes	Yes

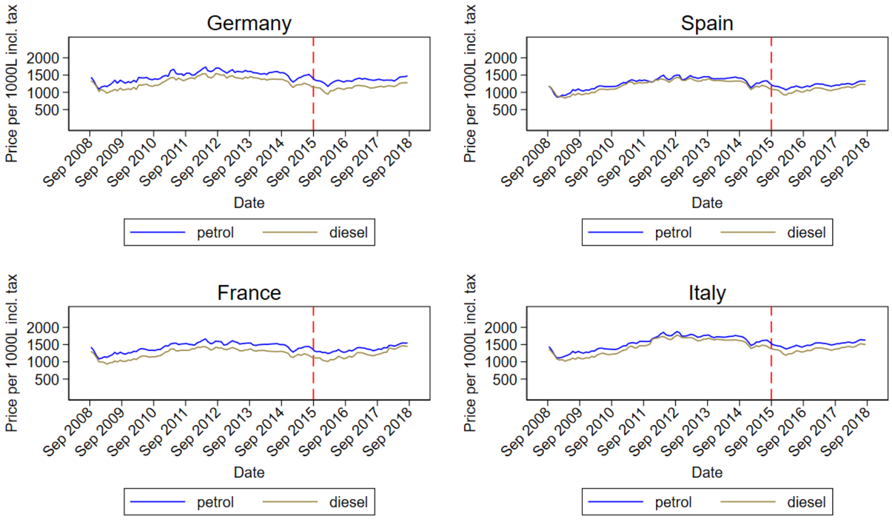
This table reports loan-level difference-in-differences estimates of the effect of LEZ tightening on diesel relative to petrol car loan conditions. Panels A and B report results for loan interest rates and loan-to-value (LTV) ratios, respectively. Columns (1)–(2) restrict the sample to treated counties experiencing a transition to a Level 3 LEZ and compare diesel and petrol car loans within the same county before and after the tightening. Columns (3)–(4) restrict the sample to treated counties with an existing LEZ and allow the diesel–post effect to vary by the post-transition LEZ stringency level; transitions to Level 1 serve as the reference category. All regressions include borrower income controls and a full set of event-stack–by–month, event-stack–by–model, and event-stack–by–state–by–bank–by–month fixed effects. In this setting, state refers to a German Land. Robust standard errors clustered at the county–by–event-stack level are reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 11: Effect of Pollution on Diesel Car Loan Conditions

	Captive Banks (1)	Independent Banks (2)	All (3)
Panel A: Δ Loan Rate (p.p.)			
Diesel	0.117**	-0.016	-0.032*
	(0.048)	(0.019)	(0.018)
$\mathbf{1}\{NO_2 > 40\}$	-0.017	0.029	-0.003
	(0.155)	(0.131)	(0.173)
Diesel × $\mathbf{1}\{NO_2 > 40\}$	0.055	0.033	0.202
	(0.162)	(0.233)	(0.263)
Diesel × Captive			0.171***
			(0.048)
$\mathbf{1}\{NO_2 > 40\}$ x Captive			0.053
			(0.326)
Diesel × $\mathbf{1}\{NO_2 > 40\}$ x Captive			-0.481
			(0.362)
R-squared	0.563	0.076	0.218
Observations	2372	8975	11355
Mean loans per collapsed cell	26.1	26.3	26.2
Panel B: Δ Loan-to-Value Ratio (p.p.)			
Diesel	-1.101	-0.479	-0.574
	(0.786)	(0.565)	(0.568)
$\mathbf{1}\{NO_2 > 40\}$	1.178	-0.712	-0.758
	(1.621)	(1.467)	(1.242)
Diesel × $\mathbf{1}\{NO_2 > 40\}$	-4.669	-2.937	-2.618
	(3.034)	(3.626)	(3.518)
Diesel × Captive			0.312
			(0.861)
$\mathbf{1}\{NO_2 > 40\}$ x Captive			3.598
			(2.427)
Diesel × $\mathbf{1}\{NO_2 > 40\}$ x Captive			-2.885
			(4.711)
R-squared	0.080	0.049	0.046
Observations	2372	8975	11355
Mean loans per collapsed cell	26.1	26.3	26.2
Borrower income	Yes	Yes	Yes
Event-stack FE	Yes	Yes	Yes
Model FE	Yes	Yes	Yes
Bank FE	Yes	Yes	Yes
State FE	Yes	Yes	Yes

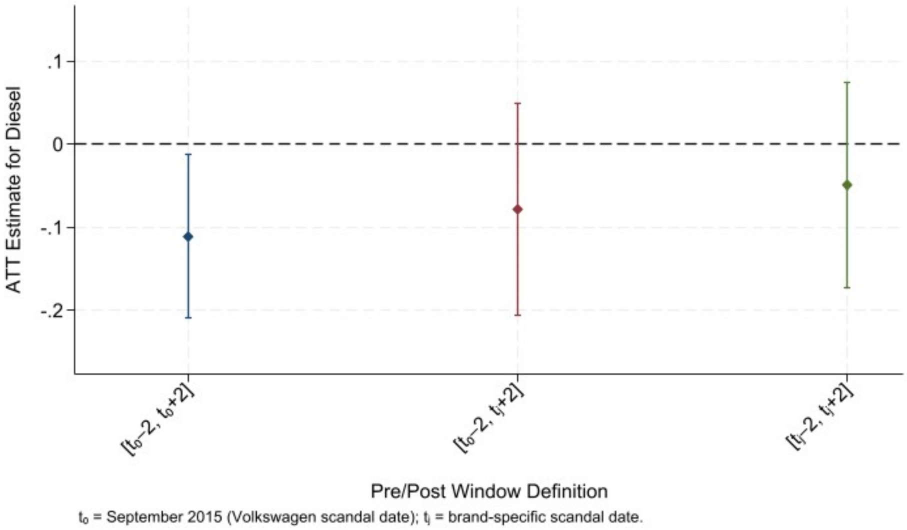
Notes: This table reports collapsed-cohort first-difference estimates of the effect of local air-pollution exceedances on loan conditions for diesel relative to petrol vehicles in Germany. The event is defined as the first year in which the maximum annual mean NO_2 concentration across monitoring stations within a county exceeds the EU limit of $40\,\mu g/m^3$. Event stacks are constructed using two-year pre- and post-event windows, with never-breaching counties serving as controls within each stack. Loans are collapsed to cohort cells defined by lender bank, car model and brand, fuel type, borrower income group, and county, and outcomes are measured as post-minus-pre differences. All specifications include event-stack, car model, bank, and state fixed effects, as well as borrower income controls. In this setting, state refers to a German Land. Standard errors clustered at the event-stack–by–state level are reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Figure 1: Diesel and petrol prices, including tax



This figure plots retail pump prices for Euro-super 95 (petrol) and automotive diesel fuel, including taxes, recorded closest to the 15th of each month. Prices are shown separately for Germany, France, Spain, and Italy. Data are from Eurostat's Weekly Oil Bulletin.

Figure 2: Estimated Treatment Effects under Varying Pre- and Post-Period Definitions



Notes: This figure compares average treatment effects (ATTs) from three first-difference model specifications that differ in their definitions of pre- and post-treatment periods. The first column estimates the ATT assuming a common treatment date $t_0 =$ September 2015 for all cohorts and a symmetric window of two years before and after ($[t_0 - 2, t_0 + 2]$). The second column uses a stacked first-difference model with a uniform pre-period $[t_0 - 2, t_0]$ for all cohorts and a post-period that begins at each brand-specific scandal date t_j ($[t_j, t_j + 2]$). The third column estimates ATTs using fully manufacturer-specific windows, with both pre- and post-periods defined relative to each brand's scandal date ($[t_j - 2, t_j + 2]$).

Appendix

A Additional Figures and Tables: Identification Fuel Type and Car Model

Table A.1: Fuel type identification by brand

Brand	Diesel	Petrol
ALFA ROMEO	JTD, JTDM	JTS, TwinAir, MultiAir
AUDI	TFSI, TDI	TSI, TFSI
CITROEN	HDI	PureTech, VTI
DACIA	DCI	TCE, SCE
FIAT	JTD, Multijet	T-Jet, TwinAir
PEUGEOT	HDI	PureTech, VTI, THP
RENAULT	DCI	TCE, SCE
SEAT	TDI	TSI, TFSI
SKODA	TDI	TSI, TFSI
VW	TDI	TSI, TFSI

Notes: This table summarizes how the fuel type identification, whether a car has a petrol or diesel engine, is derived from car brand's model naming convention. Brands are only included if diesel as well as petrol engines can be indubitably identified. E.g for Toyota only certain diesel cars (D-4D), are easily identifiable, hence Toyota cars are excluded.

Table A.2: Make-Model Overview.

Make	Model	Frequency	Make	Model	Frequency	Make	Model	Frequency
ALFA ROMEO	147	509	AUDI	Q5	1,730	CITROËN	C3	40,799
ALFA ROMEO	159	1,103	AUDI	Q7	1,143	CITROËN	C3 Picasso	17,183
ALFA ROMEO	Brera	102	AUDI	TT	270	CITROËN	C4	96,003
ALFA ROMEO	GT	219	BMW	1 Series	9,470	CITROËN	C5	25,386
ALFA ROMEO	Giulietta	1,255	BMW	2 Series	242	CITROËN	C6	1,500
ALFA ROMEO	MiTo	717	BMW	3 Series	15,814	CITROËN	C8	3,649
ALFA ROMEO	Spider	149	BMW	4 Series	146	CITROËN	DS3	11,449
AUDI	A1	1,779	BMW	5 Series	10,082	CITROËN	DS4	5,576
AUDI	A3	8,966	BMW	7 Series	659	CITROËN	DS5	2,902
AUDI	A4	4,697	BMW	X1	1,001	CITROËN	Xsara Picasso	757
AUDI	A5	2,270	BMW	X3	2,592	DACIA	Duster	2,557
AUDI	A6	3,318	BMW	X5	1,244	DACIA	Lodgy	542
AUDI	A7	216	BMW	X6	366	DACIA	Logan	278
AUDI	A8	422	CITROËN	C1	2,942	DACIA	Sandero	2,122
AUDI	Q3	644	CITROËN	C2	3,520	FIAT	500	550

Notes: This table summarizes how the fuel type identification, whether a car has a petrol or diesel engine, is derived from car brand's model naming convention. Brands are only included if diesel as well as petrol engines can be indubitably identified. E.g for Toyota only certain diesel cars (D-4D), are easily identifiable, hence Toyota cars are excluded.

Make	Model	Frequency	Make	Model	Frequency	Make	Model	Frequency	Make	Model	Frequency
FIAT	500L	307	PEUGEOT	3008	32,215	RENAULT	Koleos	1,240	SKODA	Octavia	3,261
FIAT	Bravo	571	PEUGEOT	307	10,960	RENAULT	Laguna	1,181	SKODA	Rapid	594
FIAT	Croma	391	PEUGEOT	308	61,307	RENAULT	Latitude	105	SKODA	Superb	1,060
FIAT	Freemont	297	PEUGEOT	4007	1,603	RENAULT	Megane	6,447	SKODA	Yeti	2,400
FIAT	Panda	171	PEUGEOT	4008	957	RENAULT	Modus	824	VW	Eos	1,124
FIAT	Punto	398	PEUGEOT	406	236	RENAULT	Scenic	4,651	VW	Golf	15,240
FIAT	Tipo	138	PEUGEOT	407	14,723	RENAULT	Twingo	803	VW	Jetta	734
FIAT	Ulysse	199	PEUGEOT	5008	15,906	RENAULT	Wind	182	VW	Passat	5,516
PEUGEOT	1007	1,400	PEUGEOT	508	13,904	SEAT	Altea	2,671	VW	Phaeton	408
PEUGEOT	107	812	PEUGEOT	607	1,752	SEAT	Cordoba	175	VW	Polo	4,998
PEUGEOT	108	2,548	PEUGEOT	807	4,259	SEAT	Exeo	1,188	VW	Scirocco	158
PEUGEOT	2008	17,183	PEUGEOT	RCZ	2,545	SEAT	Ibiza	2,468	VW	Sharan	2,283
PEUGEOT	206	8,828	RENAULT	Captur	2,653	SEAT	Leon	8,826	VW	Tiguan	5,232
PEUGEOT	207	54,411	RENAULT	Clio	7,412	SEAT	Toledo	338	VW	Touareg	281
PEUGEOT	208	46,515	RENAULT	Kadjar	446	SKODA	Fabia	3,466	VW	Touran	11,247

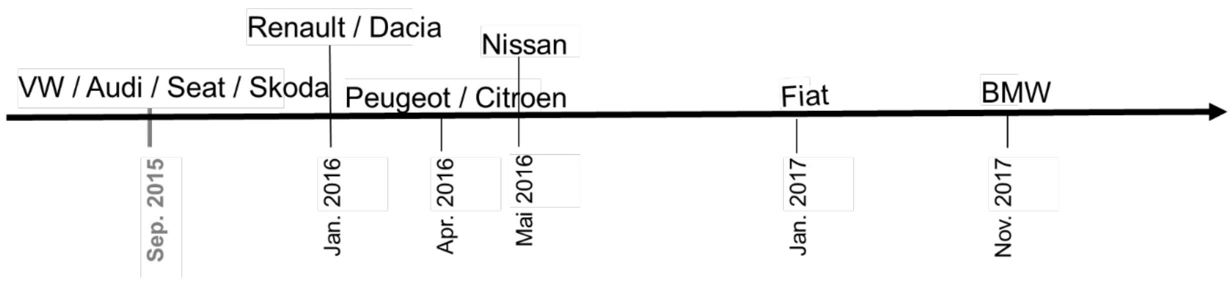
Notes: This table summarizes how the fuel type identification, whether a car has a petrol or diesel engine, is derived from car brand's model naming convention. Brands are only included if diesel as well as petrol engines can be indubitably identified. E.g for Toyota only certain diesel cars (D-4D), are easily identifiable, hence Toyota cars are excluded.

B Additional Figures and Tables: Diesel Emission Scandal and LEZs

Table B.1: Timeline Diesel Emissions Scandal

Date	**Event**
June 2007	The EU introduces regulation banning defeat devices, with Member States required to enforce the ban.
2011	The European Commission's Joint Research Centre finds NO_x emissions exceed EU limits by up to 14 times in various car models under real-world conditions (Weiss et al., 2011).
October 2014	A study by the International Council on Clean Transportation (ICCT) reveals excessive emissions in several VW cars sold in the US (Franco et al., 2014).
September 2015	The US Environmental Protection Agency (EPA) accuses VW of using "defeat devices" to pass diesel emissions tests. VW admits to installing software that reduces emissions during lab tests in 11 million diesel engines worldwide. VW shares drop 40% in two days.
November 2015	The EPA issues a second Notice of Violation for Audi and Porsche.
January 2016	French fraud investigators inspect Renault's headquarters.
April 2016	PSA Peugeot Citroën's headquarters are inspected by French fraud investigators. The US Department of Justice asks Daimler to investigate its diesel emissions certification process.
May 2016	South Korean authorities accuse Nissan of using a defeat device to manipulate emissions data for the Nissan Qashqai. Nissan denies the accusation.
July 2016	German authorities launch investigations into Porsche and Daimler for alleged emissions test fraud.
January 2017	The US EPA issues a violation notice to Fiat Chrysler Automobiles (FCA) for exceeding NO_x pollution limits in over 100,000 diesel SUVs and trucks from 2014–2016.
March 2017	Nissan vehicles are found to produce NO_x levels above legal limits set by the 2009 EU Euro 5 standards.

Figure B.1: Diesel Emission Scandal Timeline by Car Brands



Notes: This figure provides a timeline showing when the manufacturers of the displayed car brands were first publicly linked to the diesel emissions scandal.

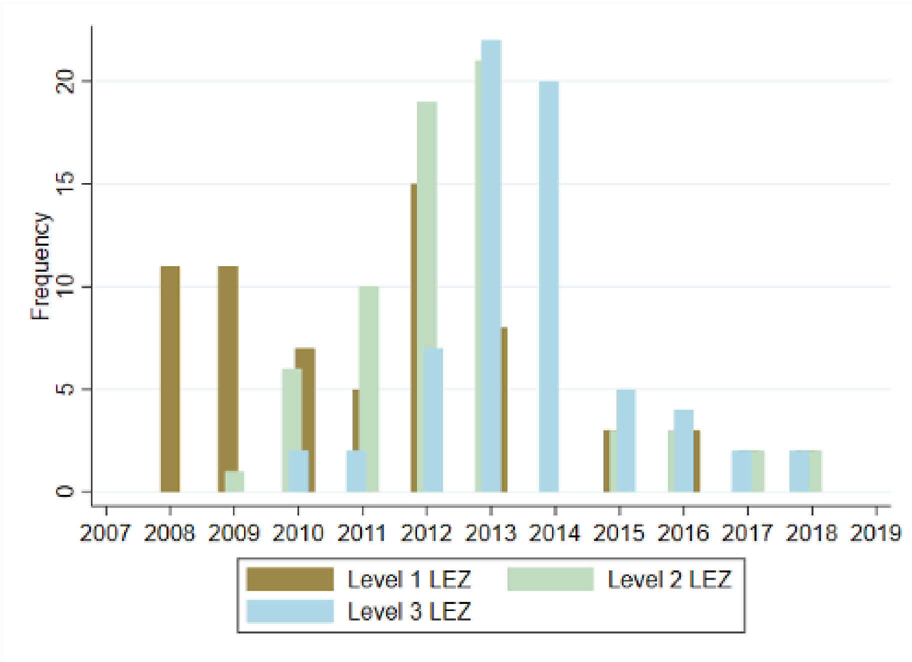
Table B.2: Introduction of LEZs in Various Counties and Municipalities

County	Municipality	Introduction LEZ
Stuttgart	Stuttgart	01.03.2008
Böblingen	Herrenberg	01.01.2009
Esslingen	Wendlingen	02.04.2013
Ludwigsburg	Pleidelsheim	01.01.2013
Rems-Murr-Kreis	Urbach	01.01.2012
Heilbronn	Heilbronn	01.01.2009
Heilbronn	Ilsfeld	01.03.2008
Heidenheim	Heidenheim	01.01.2012
Ostalbkreis	Schwäbisch Gmünd	01.03.2008
Karlsruhe	Karlsruhe	01.01.2009
Karlsruhe	Pfinztal	01.01.2010
Heidelberg	Heidelberg	01.01.2010
Mannheim	Mannheim	01.03.2008
Pforzheim	Pforzheim	01.01.2009
Enzkreis	Mühlacker	01.01.2009
Freiburg im Breisgau	Freiburg	01.01.2010
Rottweil	Schramberg	01.07.2013
Reutlingen	Reutlingen	01.03.2008
Tübingen	Tübingen	01.03.2008
Zollernalbkreis	Balingen	01.04.2017
Ulm	Ulm	01.01.2009
München	München	01.11.2008

County	Municipality	Introduction LEZ
Regensburg	Regensburg	15.01.2018
Augsburg	Augsburg	01.07.2009
Neu-Ulm	Neu-Ulm	01.11.2009
Berlin	Berlin	01.01.2008
Bremen	Bremen	01.01.2009
Darmstadt	Darmstadt	01.11.2015
Frankfurt am Main	Frankfurt am Main	01.11.2008
Offenbach am Main	Offenbach	01.01.2015
Wiesbaden	Wiesbaden	01.02.2013
Limburg-Weilburg	Limburg a.d. Lahn	31.01.2018
Marburg-Biedenkopf	Marburg	01.04.2016
Region Hannover	Hannover	01.01.2008
Osnabrück	Osnabrück	04.01.2010
Düsseldorf	Düsseldorf	15.02.2009
Duisburg	Duisburg	01.01.2012
Essen	Essen	01.01.2012
Krefeld	Krefeld	01.01.2011
Mönchengladbach	Mönchengladbach	01.01.2013
Mülheim an der Ruhr	Mülheim an der Ruhr	01.01.2012
Oberhausen	Oberhausen	01.01.2012
Remscheid	Remscheid	01.01.2013
Wuppertal	Wuppertal	15.02.2009
Mettmann	Langenfeld	01.01.2013
Rhein-Kreis Neuss	Neuss	15.02.2010
Wesel	Dinslaken	01.07.2011
Aachen	Aachen	01.02.2016
Bonn	Bonn	01.01.2010
Köln	Köln	01.01.2008
Aachen	Eschweiler	01.06.2016
Rheinisch-Bergischer Kreis	Overath	01.11.2017
Bottrop	Bottrop	01.01.2012
Gelsenkirchen	Gelsenkirchen	01.01.2012
Münster	Münster	01.01.2010
Recklinghausen	Herten	01.01.2012
Bochum	Bochum	01.01.2012
Dortmund	Dortmund	01.01.2012
Hagen	Hagen	01.01.2012
Herne	Herne	01.01.2012
Siegen-Wittgenstein	Siegen	01.01.2015

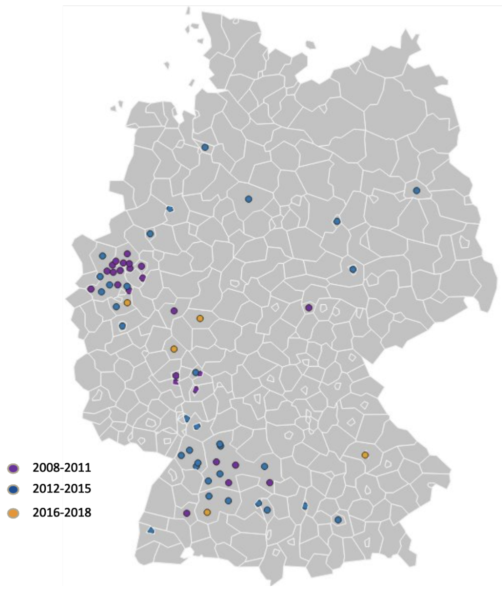
County	Municipality	Introduction LEZ
Mainz	Mainz	01.02.2013
Leipzig	Leipzig	01.03.2011
Halle (Saale)	Halle (Saale)	01.09.2011
Magdeburg	Magdeburg	01.09.2011
Erfurt	Erfurt	01.11.2012

Figure B.2: Implementation Timeline of Low-Emission Zones in Germany



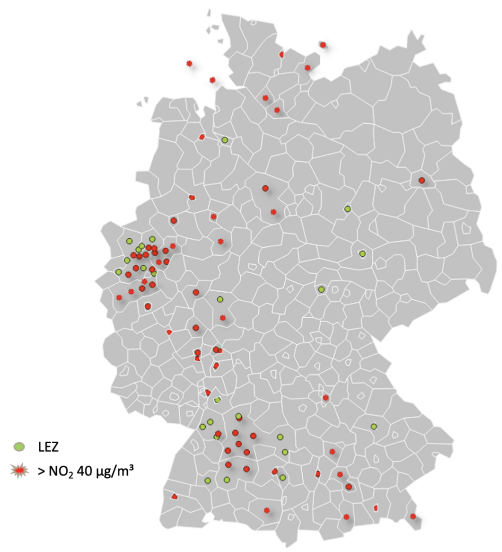
Notes: This figure presents an overview of the timeline for the introduction of low-emission zones in German cities. A vehicle's classification in terms of emission is based on the EC air pollution directive, which specifies emission levels. In 2008, 11 new Low-Emission Zones (LEZs) were introduced, which required a minimum diesel standard of Euro 2 or Euro 1 with retrofit (level 1). Ten years later, 2 new LEZs were established, implementing a direct requirement for diesel cars of Euro 3 or Euro 2 with retrofit (level 2), and 2 new LEZs were introduced with even stricter requirements of Euro 4 and higher or Euro 3 with retrofit (level 3).

Figure B.3: Map of Low-Emission Zones in Germany



Notes: This figure shows the distribution of low-emission zones that have been introduced from 2008 to 2018 in Germany.

Figure B.4: Map of High NO_2 Pollution in Germany



Notes: This figure shows the distribution of locations in Germany where the NO_2 threshold, ($>NO_2$ 40 µg/m^3), has been surpassed prior to 2018. The figure also shows where low-emission zones have been introduced at the same time span.

C Additional Figures and Tables: Robustness and Supplementary Results

Table C.1: Effect of Diesel Emission Scandal on Diesel Car Loan Conditions (cont.)

	Captive (1)	Independent (2)	All (3)
Panel C: Δ Loan Maturity (months)			
Diesel	-0.389	0.058	-0.109
	(0.425)	(0.356)	(0.334)
Diesel × Captive			-0.415
			(0.569)
Fixed effects: Model & Bank-Country	Yes	Yes	Yes
Borrower control: Income	Yes	Yes	Yes
R-squared	0.535	0.459	0.508
Observations	253	290	545
Mean loans per collapsed cell	848.8	407.5	611.5
Panel D: Δ Down Payment Amount (LN EUR)			
Diesel	-0.065***	-0.068	-0.067
	(0.019)	(0.053)	(0.049)
Diesel × Captive			-0.005
			(0.052)
Fixed effects: Model & Bank-Country	Yes	Yes	Yes
Borrower control: Income	Yes	Yes	Yes
R-squared	0.508	0.138	0.180
Observations	253	290	545
Mean loans per collapsed cell	848.8	407.5	611.5
Panel E: Δ Loan Amount (LN EUR)			
Diesel	-0.002	0.023**	0.011
	(0.007)	(0.009)	(0.009)
Diesel × Captive			-0.009
			(0.012)
Fixed effects: Model & Bank-Country	Yes	Yes	Yes
Borrower control: Income	Yes	Yes	Yes
R-squared	0.609	0.542	0.681
Observations	253	290	545
Mean loans per collapsed cell	848.8	407.5	611.5

Notes: This table reports collapsed-cohort first-difference estimates of the effect of the 2015 Diesel emissions scandal on loan conditions for diesel relative to petrol vehicles. Loans are grouped into narrowly defined cohorts based on lender bank, vehicle brand and model, fuel type, borrower country, and borrower income quintile. For each cohort, the outcome variable is the change in the cohort mean between the pre- and post-scandal periods. Columns (1) and (2) report estimates separately for captive and independent banks; Column (3) pools all lenders and includes an interaction between diesel status and captive bank affiliation. All specifications include car model fixed effects and bank-by-country fixed effects, as well as cohort-level averages of borrower income. Standard errors are robust and clustered at the model × fuel × bank level. Parentheses report standard errors. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table C.2: Effect of Diesel Emission Scandal on Diesel Car Loan Conditions: Loan-Level (cont.)

	Captive (1)	Independent (2)	All (3)
Panel C: Loan Maturity (months)			
Post	0.496	-1.067**	-0.955**
	(0.616)	(0.415)	(0.415)
Diesel	1.459***	-0.470	-0.210
	(0.405)	(0.776)	(0.716)
Post × Diesel	-0.808	1.259***	1.224***
	(0.641)	(0.412)	(0.418)
Post × Captive			1.457**
			(0.737)
Diesel × Captive			1.630**
			(0.827)
Post × Diesel × Captive			-2.131***
			(0.764)
Observations	253,799	145,953	399,762
Adj. R^2	0.072	0.045	0.075
Fixed effects: Model & Bank-Year	Yes	Yes	Yes
Borrower control: Income	Yes	Yes	Yes
Panel D: Down Payment Amount (LN EUR)			
Post	0.045	0.014	0.016
	(0.041)	(0.018)	(0.019)
Diesel	0.035	0.001	-0.023
	(0.028)	(0.030)	(0.034)
Post × Diesel	-0.107**	-0.015	-0.018
	(0.047)	(0.019)	(0.020)
Post × Captive			0.027
			(0.045)
Diesel × Captive			0.065
			(0.042)
Post × Diesel × Captive			-0.087*
			(0.050)
Observations	218,850	96,023	314,882
Adj. R^2	0.175	0.127	0.209
Fixed effects: Model & Bank-Year	Yes	Yes	Yes
Borrower control: Income	Yes	Yes	Yes
Panel E: Loan Amount (LN EUR)			
Post	0.016	-0.007	0.001
	(0.013)	(0.011)	(0.011)
Diesel	-0.010	0.057**	0.085***
	(0.012)	(0.025)	(0.025)
Post × Diesel	0.050***	0.025*	0.017
	(0.015)	(0.014)	(0.014)
Post × Captive			0.005
			(0.018)
Diesel × Captive			-0.117***
			(0.029)
Post × Diesel × Captive			0.044**
			(0.021)
Observations	253,799	145,953	399,762
Adj. R^2	0.062	0.219	0.213
Fixed effects: Model & Bank-Year	Yes	Yes	Yes
Borrower control: Income	Yes	Yes	Yes

Notes: This table reports loan-level difference-in-differences estimates of the effect of the 2015 Diesel emissions scandal on loan conditions for diesel relative to petrol vehicles. The sample consists of amortizing used-car loans originated between September 2013 and September 2017. *Post* is an indicator for loans originated after September 2015, and *Diesel* identifies diesel vehicles. Columns (1) and (2) report estimates separately for captive and independent banks; Column (3) pools all lenders and includes interactions with captive bank affiliation. All specifications include car model fixed effects and bank-by-year fixed effects, as well as borrower income as a control. Standard errors are robust and clustered at the model × fuel × bank level. Parentheses report standard errors. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table C.3: Heterogeneity in Loan Conditions by Lender Type and Own-Brand Financing (cont.)

	Captive (own-brand + no own-brand) (1)	Captive + Independent (own-brand) (2)	Captive + Independent (no own-brand) (3)
Panel C: Loan Maturity (months)			
Post	-0.013	-0.973**	-1.030**
	(0.623)	(0.416)	(0.413)
Diesel	0.371	-0.275	-0.320
	(0.674)	(0.732)	(0.743)
Own-brand	-1.819**		
	(0.778)		
Post × Diesel	-0.297	1.238***	1.226***
	(0.633)	(0.417)	(0.411)
Post × Own-brand	0.521		
	(0.824)		
Post × Captive		1.539**	0.511
		(0.756)	(0.855)
Diesel × Own-brand	1.125		
	(0.783)		
Diesel × Captive		1.778**	0.093
		(0.850)	(1.013)
Post × Diesel × Own-brand	-0.522		
	(0.911)		
Post × Diesel × Captive		-2.233***	-0.986
		(0.786)	(0.790)
Observations	253,799	383,214	162,501
Adj. R^2	0.073	0.074	0.057
Fixed effects: Model & Bank-Year	Yes	Yes	Yes
Borrower control: Income	Yes	Yes	Yes
Panel D: Down Payment Amount (LN EUR)			
Post	0.173***	0.013	0.017
	(0.055)	(0.019)	(0.018)
Diesel	0.056	-0.021	-0.002
	(0.058)	(0.034)	(0.030)
Own-brand	0.214***		
	(0.059)		
Post × Diesel	-0.144***	-0.014	-0.020
	(0.055)	(0.020)	(0.019)
Post × Own-brand	-0.134**		
	(0.062)		
Post × Captive		0.027	0.102
		(0.047)	(0.065)
Diesel × Own-brand	-0.018		
	(0.065)		
Diesel × Captive		0.069	0.082
		(0.043)	(0.066)
Post × Diesel × Own-brand	0.037		
	(0.075)		
Post × Diesel × Captive		-0.092*	-0.103*
		(0.053)	(0.061)
Observations	218,850	301,415	109,490
Adj. R^2	0.175	0.212	0.136
Fixed effects: Model & Bank-Year	Yes	Yes	Yes
Borrower control: Income	Yes	Yes	Yes
Panel E: Loan Amount (LN EUR)			
Post	0.041*	0.001	-0.006
	(0.023)	(0.011)	(0.011)
Diesel	0.061**	0.079***	0.065***
	(0.027)	(0.026)	(0.024)
Own-brand	0.098***		
	(0.031)		
Post × Diesel	-0.007	0.017	0.024*
	(0.025)	(0.014)	(0.014)
Post × Own-brand	-0.027		
	(0.025)		
Post × Captive		0.003	0.066**
		(0.019)	(0.028)
Diesel × Own-brand	-0.075**		
	(0.030)		
Diesel × Captive		-0.115***	-0.021
		(0.029)	(0.035)
Post × Diesel × Own-brand	0.060**		
	(0.030)		
Post × Diesel × Captive		0.046**	-0.026
		(0.022)	(0.029)
Observations	253,799	383,214	162,501
Adj. R^2	0.062	0.217	0.239
Fixed effects: Model & Bank-Year	Yes	Yes	Yes
Borrower control: Income	Yes	Yes	Yes

Notes: This table reports loan-level estimates of the effect of the 2015 Dieselgate shock on loan conditions for diesel relative to petrol vehicles, allowing for heterogeneity by captive bank affiliation and own-brand financing. Column (1) restricts the sample to loans originated by captive banks and includes both own-brand and non–own-brand vehicles. Column (2) restricts the sample to own-brand vehicles and pools loans originated by captive and independent banks. Column (3) restricts the sample to non–own-brand vehicles and pools loans originated by captive and independent banks. *Own-brand* indicates loans originated by a captive bank for vehicles produced by its affiliated manufacturer. All specifications include car model fixed effects and bank-by-year fixed effects, as well as borrower income controls. Robust standard errors clustered at the model × fuel × bank level are reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table C.4: Heterogeneity in Loan Conditions by Manufacturer Captive Finance Strength (cont.)

	Independent (1)	Captive no own-brand (2)	Captive own-brand (3)
Panel C: Loan Maturity (months)			
Post × Diesel	7.015	5.653	-0.109
	(9.579)	(7.013)	(6.717)
Captive support	0.126	-1.027***	
	(1.185)	(0.360)	
Post × Captive support	0.194	0.033	-0.088
	(0.208)	(0.177)	(0.128)
Diesel × Captive support	0.244	0.120	-0.162
	(0.273)	(0.145)	(0.124)
Post × Diesel × Captive support	-0.172	-0.149	-0.027
	(0.282)	(0.197)	(0.208)
Observations	118,190	15,152	234,236
Adj. R^2	0.051	0.086	0.045
Fixed effects: Model & Bank-Year	Yes	Yes	Yes
Borrower control: Income	Yes	Yes	Yes
Panel D: Down Payment Amount (LN EUR)			
Post × Diesel	0.374	0.744	-0.629**
	(0.368)	(0.609)	(0.318)
Captive support	-0.068*	-0.089**	
	(0.039)	(0.036)	
Post × Captive support	0.005	0.020	0.001
	(0.008)	(0.015)	(0.008)
Diesel × Captive support	-0.007	0.019	-0.006
	(0.011)	(0.015)	(0.007)
Post × Diesel × Captive support	-0.011	-0.025	0.017*
	(0.011)	(0.017)	(0.009)
Observations	77,459	12,328	204,080
Adj. R^2	0.133	0.171	0.172
Fixed effects: Model & Bank-Year	Yes	Yes	Yes
Borrower control: Income	Yes	Yes	Yes
Panel E: Loan Amount (LN EUR)			
Post × Diesel	0.525*	-0.441*	0.374**
	(0.283)	(0.261)	(0.149)
Captive support	-0.004	0.003	
	(0.035)	(0.012)	
Post × Captive support	0.014**	-0.011*	-0.004
	(0.006)	(0.007)	(0.003)
Diesel × Captive support	0.006	-0.003	0.008**
	(0.008)	(0.006)	(0.003)
Post × Diesel × Captive support	-0.015*	0.013*	-0.010**
	(0.008)	(0.007)	(0.005)
Observations	118,190	15,152	234,236
Adj. R^2	0.231	0.164	0.058
Fixed effects: Model & Bank-Year	Yes	Yes	Yes
Borrower control: Income	Yes	Yes	Yes

Notes: This table reports loan-level regressions of loan conditions on indicators for diesel vehicles, the post-Dieselgate period, and their interactions with a proxy for captive finance strength. *Captive support* is measured as the manufacturer-level share (in percent) of new-vehicle financing provided by the manufacturer's captive lender at the European level. This penetration rate is constant within a brand (30.7 for Peugeot/Citroën, 33.6 for VW/SEAT/Škoda/Audi, 36.4 for Renault/Dacia, and 42.2 for Fiat/Alfa Romeo), so variation in captive support arises primarily from differences across manufacturer groups rather than within-brand variation. All specifications include car model fixed effects and bank-by-year fixed effects, as well as borrower income controls. Robust standard errors clustered at the model × fuel × bank level are reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table C.5: Effect of the Diesel Emissions Scandal on Diesel Car Loan Conditions: Alternative Specifications

	(1) Baseline	(2) Model FE only	(3) Brand + Bank-Country FE	(4) Same-Model	(5) + Loan Controls
Diesel	0.006	0.020	-0.013	0.012	0.015
	(0.038)	(0.054)	(0.037)	(0.038)	(0.039)
Diesel × Captive	-0.122*	-0.154	-0.112*	-0.124*	-0.115*
	(0.063)	(0.117)	(0.066)	(0.064)	(0.060)
Observations	545	545	549	424	545
R-squared	0.782	0.296	0.745	0.764	0.788
Borrower control: Income	Yes	Yes	Yes	Yes	Yes
Mean loans per collapsed cell	848.8	848.8	848.8	848.8	848.8

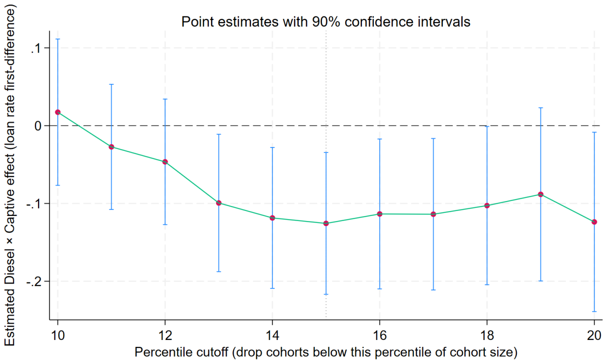
Notes: This table reports estimates of the effect of the Diesel Emissions Scandal on loan interest rates for diesel relative to petrol vehicles, using the collapsed cohort first-difference specification. The dependent variable is the post-minus-pre change in the loan interest rate. *Diesel* is an indicator for diesel-fueled vehicles, and *Captive* indicates captive lenders. Column (1) reports the baseline specification with model and lender-by-country fixed effects. Column (2) includes model fixed effects only. Column (3) replaces model fixed effects with brand fixed effects and lender-by-country fixed effects. Column (4) reports the baseline specification but restricts the sample to models offered with both diesel and petrol engines. Column (5) adds to the baseline specification loan contract characteristics (e.g., maturity, loan-to-value ratio, and loan amount), which are potentially endogenous outcomes of the scandal and therefore constitute "bad controls"; this specification is included for completeness only. All specifications control for average borrower income. Robust standard errors clustered at the model × fuel × bank level are reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table C.6: Effect of the Diesel Emissions Scandal on Diesel Car Loan Conditions: Robustness to Cohort-Size Cutoff

Cutoff percentile	Diesel × Captive	Std. Err.	Observations
p10	0.017	(0.057)	713
p11	-0.027	(0.049)	670
p12	-0.047	(0.049)	640
p13	-0.099*	(0.054)	596
p14	-0.119**	(0.055)	555
p15	-0.126**	(0.055)	529
p16	-0.114*	(0.059)	491
p17	-0.114*	(0.059)	464
p18	-0.103	(0.062)	436
p19	-0.088	(0.068)	410
p20	-0.124*	(0.070)	379

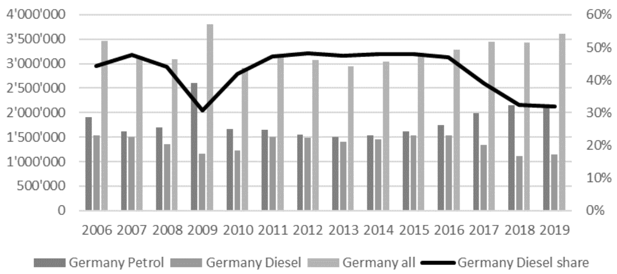
Notes: Each row reports the estimated Diesel × Captive coefficient from the baseline first-difference specification for loan rates after applying alternative minimum-cohort-size filters. Cohorts are defined at the loan level by lender, model, country, income quantile, fuel type, and period (pre/post), and cohort size is measured as the number of underlying loans in that cell. For each percentile p, we exclude loan-level observations belonging to the smallest p percent of cohorts, ranked by their number of underlying loans, and then re-collapse the data to construct post-minus-pre differences. All specifications include model fixed effects and lender-by-country fixed effects and control for average borrower income. Standard errors are clustered at the model × fuel × bank level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Figure C.1: Effect of the Diesel Emissions Scandal on Diesel Car Loan Conditions: Robustness to Cohort-Size Cutoff



Notes: This figure assesses how the estimated Diesel × Captive effect on loan rates varies with the minimum cohort size used in data collapsing. Each point shows the coefficient from the pooled first-difference model, with vertical bars indicating 90% confidence intervals. The horizontal dashed line marks zero, while the vertical dotted line represents the baseline cutoff—excluding roughly the bottom 15% of cohorts (50 loans per cohort) in both periods. Lower percentile cutoffs retain cohorts with very few underlying observations (for example, cohorts below the 10th percentile contain on average about 31 loans, with minimum cohort sizes as low as 29 loans). As expected in pseudo-panel settings, including very small cohorts introduces noise, whereas excluding too many reduces statistical power. The results demonstrate that the effect's magnitude and sign remain stable near the baseline cutoff, indicating robustness to the cohort size threshold.

Figure C.2: New Registrations of Passenger Cars by Fuel Type in Germany



Notes: The variable on the LHS axis indicates the number of registrations in Germany for petrol, diesel, and all cars. The variable on the RHS axis denotes the share of diesel cars out of the total cars in car registrations. Source: own illustration based on Eurostat data.

Table C.7: Effect of LEZ on Diesel Car Loan Conditions (cont.)

	All incremental LEZ events		By LEZ level (ref: Level 1)	
	Captive Banks (1)	Independent Banks (2)	Captive Banks (3)	Independent Banks (4)
Panel C: Δ Loan Maturity (months)				
LEZ event	-0.024	1.286	5.093***	6.002***
	(1.608)	(1.509)	(1.476)	(0.408)
Diesel	0.522*	0.438***	0.592**	0.447***
	(0.270)	(0.147)	(0.275)	(0.148)
Diesel × LEZ event	-1.671	-1.775	-3.034	-9.800***
	(1.202)	(1.697)	(2.545)	(0.179)
Diesel × LEZ event × LEZ Level 2			2.956	8.234***
			(3.555)	(1.806)
Diesel × LEZ event × LEZ Level 3			1.669	10.420***
			(3.183)	(1.686)
R-squared	0.176	0.101	0.177	0.102
Observations	10206	37191	10206	37191
Mean loans per collapsed cell	25.7	26.6	25.7	26.6
Panel D: Δ Down Payment Amount (LN EUR)				
LEZ event	-0.151	0.486***	-1.457**	0.503***
	(0.335)	(0.155)	(0.631)	(0.038)
Diesel	0.023	0.020	0.023	0.024
	(0.051)	(0.020)	(0.052)	(0.020)
Diesel × LEZ event	-0.076	-0.078	1.001	-0.388***
	(0.263)	(0.090)	(0.771)	(0.017)
Diesel × LEZ event × LEZ Level 2			-1.201	0.627*
			(0.948)	(0.375)
Diesel × LEZ event × LEZ Level 3			-1.069	0.713***
			(0.881)	(0.171)
R-squared	0.029	0.023	0.030	0.024
Observations	10206	37191	10206	37191
Mean loans per collapsed cell	25.7	26.6	25.7	26.6
Panel E: Δ Loan Amount (LN EUR)				
LEZ event	-0.013	0.083***	0.157***	0.056***
	(0.055)	(0.025)	(0.018)	(0.009)
Diesel	0.037***	0.019***	0.040***	0.019***
	(0.009)	(0.003)	(0.009)	(0.003)
Diesel × LEZ event	-0.056**	-0.070***	-0.111	-0.042***
	(0.028)	(0.011)	(0.072)	(0.004)
Diesel × LEZ event × LEZ Level 2			0.212**	0.013
			(0.102)	(0.053)
Diesel × LEZ event × LEZ Level 3			-0.065	0.025
			(0.086)	(0.022)
R-squared	0.128	0.088	0.132	0.088
Observations	10206	37191	10206	37191
Mean loans per collapsed cell	25.7	26.6	25.7	26.6
Borrower income	Yes	Yes	Yes	Yes
Event-stack FE	Yes	Yes	Yes	Yes
Model FE	Yes	Yes	Yes	Yes
Bank FE	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes

Notes: This table reports collapsed-cohort first-difference estimates of the effect of low-emission zone (LEZ) tightening on loan conditions for diesel relative to petrol vehicles in Germany. The sample is constructed using a stacked difference-in-differences design in which each LEZ adoption or upgrade defines an event stack with a two-year pre- and post-event window. Within each event stack, loans are collapsed to cohort cells and outcomes are measured as first differences between the post- and pre-event periods. Columns (1)–(2) pool all incremental LEZ events and estimate the average effect of a one-step tightening. Columns (3)–(4) allow the effects to vary by the resulting LEZ stringency level; coefficients on triple interactions measure differences relative to transitions to Level 1 LEZs. All specifications include stack-group, car model, bank, and state fixed effects, as well as borrower income controls. Robust standard errors clustered at the event-stack–by–state level are reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table C.8: Effect of LEZ on Diesel Car Loan Conditions: Loan-Level (cont.)

	LEZ Level 3 only		By LEZ level (ref: Level 1)	
	Captive Banks (1)	Independent Banks (2)	Captive Banks (3)	Independent Banks (4)
Panel C: Loan Maturity (months)				
Diesel	0.480	0.648	-0.850	2.688***
	(1.321)	(0.874)	(1.576)	(0.884)
Post × Diesel	-0.120	1.202	-1.831	-1.207
	(1.434)	(0.872)	(2.276)	(2.457)
Diesel × LEZ Level 2			-2.023	0.213
			(2.144)	(1.862)
Diesel × LEZ Level 3			1.305	-2.043
			(2.058)	(1.258)
Post × Diesel × LEZ Level 2			4.041	-0.204
			(2.947)	(2.725)
Post × Diesel × LEZ Level 3			1.773	2.409
			(2.699)	(2.613)
Observations	4,014	12,054	9,405	19,185
Adj. R^2	0.212	0.071	0.232	0.075
Panel D: Down Payment Amount (LN EUR)				
Diesel	0.072	-0.023	-0.043	0.011
	(0.074)	(0.041)	(0.100)	(0.065)
Post × Diesel	0.033	0.049	0.295**	0.015
	(0.114)	(0.048)	(0.140)	(0.095)
Diesel × LEZ Level 2			0.195	0.049
			(0.150)	(0.089)
Diesel × LEZ Level 3			0.115	-0.032
			(0.125)	(0.077)
Post × Diesel × LEZ Level 2			-0.401**	-0.146
			(0.195)	(0.114)
Post × Diesel × LEZ Level 3			-0.264	0.033
			(0.181)	(0.107)
Observations	1,975	7,848	4,702	12,185
Adj. R^2	0.165	0.142	0.159	0.137
Panel E: Loan Amount (LN EUR)				
Diesel	0.044	0.115***	0.038	0.098***
	(0.044)	(0.018)	(0.047)	(0.023)
Post × Diesel	0.015	-0.021	-0.098*	-0.017
	(0.056)	(0.022)	(0.055)	(0.068)
Diesel × LEZ Level 2			-0.060	0.050
			(0.058)	(0.057)
Diesel × LEZ Level 3			0.006	0.017
			(0.065)	(0.030)
Post × Diesel × LEZ Level 2			0.145*	0.002
			(0.077)	(0.081)
Post × Diesel × LEZ Level 3			0.112	-0.003
			(0.079)	(0.072)
Observations	4,014	12,054	9,405	19,185
Adj. R^2	0.230	0.235	0.255	0.239
Borrower income	Yes	Yes	Yes	Yes
Event-stack×month FE	Yes	Yes	Yes	Yes
Event-stack×model FE	Yes	Yes	Yes	Yes
Event-stack×region×bank×month FE	Yes	Yes	Yes	Yes

This table reports loan-level difference-in-differences estimates of the effect of LEZ tightenings on diesel relative to petrol car loan conditions. Panels A and B report results for loan interest rates and loan-to-value (LTV) ratios, respectively. Columns (1)–(2) restrict the sample to treated counties experiencing a transition to a Level 3 LEZ and compare diesel and petrol loans within the same county before and after the tightening. Columns (3)–(4) restrict the sample to treated counties with an existing LEZ and allow the diesel–post effect to vary by the post-transition LEZ stringency level; transitions to Level 1 serve as the reference category. All regressions include borrower income controls and a full set of event-stack–by–month, event-stack–by–model, and event-stack–by–state–by–bank–by–month fixed effects. Robust standard errors clustered at the county–by–event-stack level are reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table C.9: Effect of LEZ on Diesel Car Loan Conditions: Loan-level Alternative Specification

	All incremental LEZ events		By LEZ level (ref: Level 1)	
	Captive Banks (1)	Independent Banks (2)	Captive Banks (3)	Independent Banks (4)
Panel A: Loan Rate (p.p.)				
Post × Diesel	-0.000	-0.017***	0.003	-0.016***
	(0.008)	(0.005)	(0.008)	(0.005)
Post × LEZ event	-0.033	-0.061	0.008	0.111
	(0.069)	(0.071)	(0.086)	(0.107)
LEZ event × Diesel	-0.050	0.038	-0.067	0.139***
	(0.038)	(0.035)	(0.054)	(0.054)
Post × LEZ event × Diesel	0.077	0.049	0.003	-0.225**
	(0.053)	(0.044)	(0.095)	(0.103)
... × LEZ Level 2 (vs 1)			0.103	0.479***
			(0.143)	(0.155)
... × LEZ Level 3 (vs 1)			0.196	0.369***
			(0.135)	(0.123)
Observations	465,823	1,467,010	465,823	1,467,010
Adj. R^2	0.442	0.335	0.442	0.336
Panel B: Loan-to-Value Ratio (p.p.)				
Post × Diesel	0.013	0.463***	0.037	0.473***
	(0.172)	(0.102)	(0.177)	(0.104)
Post × LEZ event	-0.311	0.308	0.740	0.738
	(1.081)	(0.984)	(1.423)	(1.842)
LEZ event × Diesel	0.111	-0.058	0.893	0.413
	(0.804)	(0.709)	(0.993)	(1.222)
Post × LEZ event × Diesel	-0.229	-0.489	-2.158	-2.867
	(1.233)	(0.894)	(1.811)	(2.249)
... × LEZ Level 2 (vs 1)			3.299	3.843
			(3.065)	(3.022)
... × LEZ Level 3 (vs 1)			3.683	3.052
			(3.110)	(2.631)
Observations	465,623	1,466,006	465,623	1,466,006
Adj. R^2	0.112	0.041	0.112	0.041
Borrower income	Yes	Yes	Yes	Yes
Event-stack×Month FE	Yes	Yes	Yes	Yes
Event-stack×Model FE	Yes	Yes	Yes	Yes
Event-stack×State×Bank×Month FE	Yes	Yes	Yes	Yes

This table reports loan-level stacked difference-in-differences estimates of the effect of LEZ tightenings on diesel relative to petrol car loan conditions. Panels A and B report results for loan interest rates and loan-to-value (LTV) ratios, respectively. Columns (1)–(2) pool all incremental LEZ events and estimate the average diesel–post effect across all transitions. Columns (3)–(4) allow the diesel–post effect to vary by the post-transition LEZ stringency level; transitions to Level 1 serve as the reference category. All regressions include the full set of main effects and interaction terms implied by the specification; for readability, only the Post × Diesel coefficient and selected higher-order interaction terms are reported. All specifications include borrower income controls and event-stack–by–month, event-stack–by–model, and event-stack–by–state–by–bank–by–month fixed effects. Robust standard errors clustered at the county–by–event-stack level are reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table C.10: Effect of LEZ on Diesel Car Loan Conditions: Loan-Level Alternative Specification (cont.)

	All incremental LEZ events		By LEZ level (ref: Level 1)	
	Captive Banks (1)	Independent Banks (2)	Captive Banks (3)	Independent Banks (4)
Panel C: Loan Maturity (months)				
Post × Diesel	0.414***	0.508***	0.467***	0.540***
	(0.116)	(0.079)	(0.120)	(0.080)
Post × LEZ event	-0.893	-0.888	-0.467	-1.222
	(0.693)	(0.886)	(0.867)	(1.289)
LEZ event × Diesel	-0.241	0.382	-0.587	0.125
	(0.542)	(0.517)	(0.764)	(0.706)
Post × LEZ event × Diesel	0.273	0.120	-0.662	-0.590
	(0.766)	(0.596)	(1.198)	(1.770)
... × LEZ Level 2 (vs 1)			3.474*	0.596
			(2.075)	(2.200)
... × LEZ Level 3 (vs 1)			1.323	2.059
			(1.844)	(2.034)
Observations	465,823	1,467,010	465,823	1,467,010
Adj. R^2	0.255	0.075	0.255	0.075
Panel D: Down Payment Amount (LN EUR)				
Post × Diesel	0.002	-0.007*	0.002	-0.007*
	(0.008)	(0.004)	(0.008)	(0.004)
Post × LEZ event	-0.077*	-0.026	-0.169***	0.061
	(0.044)	(0.043)	(0.054)	(0.063)
LEZ event × Diesel	-0.013	-0.025	-0.061	-0.034
	(0.033)	(0.026)	(0.046)	(0.042)
Post × LEZ event × Diesel	0.029	0.023	0.136**	0.064
	(0.051)	(0.033)	(0.069)	(0.065)
... × LEZ Level 2 (vs 1)			-0.339**	-0.142
			(0.137)	(0.092)
... × LEZ Level 3 (vs 1)			-0.067	-0.045
			(0.117)	(0.084)
Observations	265,826	924,158	265,826	924,158
Adj. R^2	0.192	0.138	0.192	0.138
Panel E: Loan Amount (LN EUR)				
Post × Diesel	0.011***	0.005**	0.010**	0.005***
	(0.004)	(0.002)	(0.004)	(0.002)
Post × LEZ event	-0.004	0.002	0.031	0.009
	(0.024)	(0.019)	(0.031)	(0.030)
LEZ event × Diesel	-0.006	0.026**	-0.012	0.006
	(0.018)	(0.012)	(0.025)	(0.016)
Post × LEZ event × Diesel	0.012	-0.012	-0.037	-0.024
	(0.027)	(0.015)	(0.040)	(0.045)
... × LEZ Level 2 (vs 1)			0.107	0.024
			(0.067)	(0.062)
... × LEZ Level 3 (vs 1)			0.035	0.020
			(0.070)	(0.051)
Observations	465,823	1,467,010	465,823	1,467,010
Adj. R^2	0.265	0.244	0.265	0.244
Borrower income	Yes	Yes	Yes	Yes
Event-stack×month FE	Yes	Yes	Yes	Yes
Event-stack×model FE	Yes	Yes	Yes	Yes
Event-stack×State×Bank×Month FE	Yes	Yes	Yes	Yes

This table reports loan-level stacked difference-in-differences estimates of the effect of LEZ tightenings on diesel relative to petrol car loan conditions. Panels A and B report results for loan interest rates and loan-to-value (LTV) ratios, respectively. Columns (1)–(2) pool all incremental LEZ events and estimate the average diesel–post effect across all transitions. Columns (3)–(4) allow the diesel–post effect to vary by the post-transition LEZ stringency level; transitions to Level 1 serve as the reference category. All regressions include the full set of main effects and interaction terms implied by the specification; for readability, only the Post × Diesel coefficient and selected higher-order interaction terms are reported. All specifications include borrower income controls and event-stack–by–month, event-stack–by–model, and event-stack–by–state–by–bank–by–month fixed effects. Robust standard errors clustered at the county–by–event-stack level are reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table C.11: Own-brand Heterogeneity in Loan Conditions: LEZ Level 3

	LEZ Level 3 only		
	Captive (OB + non-OB) (1)	Captive + Indep. OB (2)	Captive + Indep. non-OB (3)
Panel A: Loan Rate (p.p.)			
Post × Diesel	0.175	0.092*	0.101*
	(0.325)	(0.053)	(0.055)
Post × Own-brand	-0.314		
	(0.320)		
Post × Captive		-0.204**	0.803**
		(0.090)	(0.329)
Post × Diesel × Own-brand	-0.056		
	(0.326)		
Post × Diesel × Captive		0.044	0.203
		(0.106)	(0.379)
Observations	4,392	16,441	12,479
Adj. R^2	0.411	0.299	0.270
Panel B: Loan-to-Value Ratio (p.p.)			
Post × Diesel	2.972	0.399	0.354
	(6.148)	(1.202)	(1.248)
Post × Own-brand	-3.118		
	(5.994)		
Post × Captive		0.284	5.989
		(2.551)	(6.638)
Post × Diesel × Own-brand	-2.092		
	(6.699)		
Post × Diesel × Captive		0.825	0.562
		(3.109)	(6.685)
Observations	4,392	16,432	12,472
Adj. R^2	0.107	0.075	0.030
Borrower income	Yes	Yes	Yes
Event-stackxMonth FE	Yes	Yes	Yes
Event-stackxModel FE	Yes	Yes	Yes
Event-stackxBank×YearxState FE	Yes	Yes	Yes

Notes: This table reports loan-level difference-in-differences estimates from a stacked event-based design examining heterogeneity in lending conditions by brand affiliation following transitions to the most stringent Low-Emission Zone (LEZ Level 3). The sample is restricted to observations exposed to an incremental transition to LEZ Level 3. Own-brand indicates vehicles produced by the parent manufacturer of the captive bank, and Captive identifies loans issued by captive banks. All regressions include the full set of main effects and interaction terms implied by the specification; for readability, only Post × Diesel and the selected higher-order interaction coefficients shown in the table are reporte. Robust standard errors clustered at the event-stack × county level are reported in parentheses. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

Table C.12: Own-brand Heterogeneity in Loan Conditions: Pooled LEZ Levels

	All LEZ Levels		
	Captive (OB + non-OB) (1)	Captive + Indep. OB (2)	Captive + Indep. non-OB (3)
Panel A: Loan Rate (p.p.)			
Post × Diesel	-0.045*	-0.016***	-0.016***
	(0.025)	(0.005)	(0.005)
Post × Diesel × LEZ-Treated	0.386**	0.040	0.038
	(0.193)	(0.043)	(0.043)
Post × Diesel × Own-brand	0.047*		
	(0.026)		
Post × Diesel × Captive		0.031***	-0.024
		(0.009)	(0.028)
Post × Diesel × LEZ-Treated × Own-brand	-0.295		
	(0.202)		
Post × Diesel × LEZ-Treated × Captive		0.049	0.518**
		(0.069)	(0.228)
Observations	480,347	1,915,704	1,507,631
Adj. R^2	0.436	0.347	0.337
Panel B: Loan-to-Value Ratio (p.p.)			
Post × Diesel	-0.332	0.500***	0.441***
	(0.430)	(0.100)	(0.101)
Post × Diesel × LEZ-Treated	1.618	-0.883	-0.590
	(3.500)	(0.889)	(0.898)
Post × Diesel × Own-brand	0.315		
	(0.459)		
Post × Diesel × Captive		-0.314*	-0.109
		(0.190)	(0.427)
Post × Diesel × LEZ-Treated × Own-brand	-1.816		
	(3.746)		
Post × Diesel × LEZ-Treated × Captive		1.071	0.065
		(1.571)	(3.442)
Observations	480,147	1,912,749	1,504,917
Adj. R^2	0.130	0.095	0.047
Borrower income	Yes	Yes	Yes
Event-stackxMonth FE	Yes	Yes	Yes
Event-stackxModel FE	Yes	Yes	Yes
Event-stackxBank×YearxState FE	Yes	Yes	Yes

Notes: This table reports loan-level difference-in-differences estimates from a stacked event-based design pooling all incremental LEZ transitions. Own-brand indicates vehicles produced by the parent manufacturer of the captive bank, and Captive identifies loans issued by captive banks. All regressions include the full set of main effects and interaction terms implied by the specification; for readability, only Post × Diesel and the selected higher-order interaction coefficients shown in the table are reporte. Robust standard errors clustered at the event-stack × county level are reported in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% levels.

Table C.13: Own-brand Heterogeneity in Loan Conditions: LEZ Level 3 (cont.)

	LEZ Level 3 only		
	Captive (OB + non-OB) (1)	Captive + Indep. OB (2)	Captive + Indep. non-OB (3)
Panel C: Loan Maturity (months)			
Post × Diesel	3.922	1.017	1.256
	(5.154)	(0.892)	(0.868)
Post × Own-brand	-6.993		
	(5.791)		
Post × Captive		0.422	0.147
		(1.377)	(5.049)
Post × Diesel × Own-brand	-3.570		
	(5.625)		
Post × Diesel × Captive		-0.695	6.322
		(1.387)	(5.551)
Observations	4,392	16,441	12,479
Adj. R^2	0.212	0.110	0.069
Panel D: Down Payment Amount (LN EUR)			
Post × Diesel	0.216	0.064	0.060
	(0.323)	(0.047)	(0.047)
Post × Own-brand	-0.247		
	(0.324)		
Post × Captive		0.093	-0.117
		(0.118)	(0.372)
Post × Diesel × Own-brand	-0.277		
	(0.337)		
Post × Diesel × Captive		-0.135	0.114
		(0.098)	(0.334)
Observations	2,300	10,213	8,150
Adj. R^2	0.154	0.148	0.139
Panel E: Loan Amount (LN EUR)			
Post × Diesel	0.208	-0.026	-0.024
	(0.185)	(0.022)	(0.022)
Post × Own-brand	-0.229		
	(0.177)		
Post × Captive		0.036	0.060
		(0.063)	(0.222)
Post × Diesel × Own-brand	-0.209		
	(0.202)		
Post × Diesel × Captive		0.023	0.280
		(0.054)	(0.176)
Observations	4,392	16,441	12,479
Adj. R^2	0.238	0.296	0.237
Borrower income	Yes	Yes	Yes
Event-stackxMonth FE	Yes	Yes	Yes
Event-stackxModel FE	Yes	Yes	Yes
Event-stackxBank×YearxState FE	Yes	Yes	Yes

Notes: This table reports loan-level difference-in-differences estimates from a stacked event-based design examining heterogeneity in lending conditions by brand affiliation following transitions to the most stringent Low-Emission Zone (LEZ Level 3). The sample is restricted to observations exposed to an incremental transition to LEZ Level 3. Own-brand indicates vehicles produced by the parent manufacturer of the captive bank, and Captive identifies loans issued by captive banks. All regressions include the full set of main effects and interaction terms implied by the specification; for readability, only Post × Diesel and the selected higher-order interaction coefficients shown in the table are reporte. Robust standard errors clustered at the event-stack × county level are reported in parentheses. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

Table C.14: Own-brand Heterogeneity in Loan Conditions: Pooled LEZ Levels (cont.)

	All LEZ Levels		
	Captive (OB + non-OB) (1)	Captive + Indep. OB (2)	Captive + Indep. non-OB (3)
Panel C: Loan Maturity (months)			
Post × Diesel	-0.331	0.507***	0.515***
	(0.361)	(0.078)	(0.078)
Post × Diesel × LEZ-Treated	4.373	0.005	0.177
	(3.113)	(0.610)	(0.588)
Post × Diesel × Own-brand	0.726*		
	(0.380)		
Post × Diesel × Captive		-0.220	-0.447
		(0.137)	(0.389)
Post × Diesel × LEZ-Treated × Own-brand	-4.013		
	(3.265)		
Post × Diesel × LEZ-Treated × Captive		0.139	5.425*
		(0.846)	(3.242)
Observations	480,347	1,915,704	1,507,631
Adj. R^2	0.266	0.119	0.082
Panel D: Down Payment Amount (LN EUR)			
Post × Diesel	-0.025	-0.009**	-0.007*
	(0.024)	(0.004)	(0.004)
Post × Diesel × LEZ-Treated	0.084	0.038	0.030
	(0.191)	(0.032)	(0.032)
Post × Diesel × Own-brand	0.026		
	(0.025)		
Post × Diesel × Captive		0.003	-0.039
		(0.008)	(0.024)
Post × Diesel × LEZ-Treated × Own-brand	-0.089		
	(0.202)		
Post × Diesel × LEZ-Treated × Captive		-0.055	0.049
		(0.055)	(0.204)
Observations	281,238	1,183,045	954,905
Adj. R^2	0.190	0.141	0.138
Panel E: Loan Amount (LN EUR)			
Post × Diesel	-0.009	0.004**	0.005**
	(0.012)	(0.002)	(0.002)
Post × Diesel × LEZ-Treated	0.153	-0.013	-0.010
	(0.098)	(0.015)	(0.015)
Post × Diesel × Own-brand	0.017		
	(0.013)		
Post × Diesel × Captive		-0.003	-0.018
		(0.004)	(0.012)
Post × Diesel × LEZ-Treated × Own-brand	-0.143		
	(0.104)		
Post × Diesel × LEZ-Treated × Captive		0.027	0.166
		(0.028)	(0.103)
Observations	480,347	1,915,704	1,507,631
Adj. R^2	0.263	0.300	0.247
Borrower income	Yes	Yes	Yes
Event-stackxMonth FE	Yes	Yes	Yes
Event-stackxModel FE	Yes	Yes	Yes
Event-stackxBank×YearxState FE	Yes	Yes	Yes

Notes: This table reports loan-level difference-in-differences estimates from a stacked event-based design pooling all incremental LEZ transitions. Own-brand indicates vehicles produced by the parent manufacturer of the captive bank, and Captive identifies loans issued by captive banks. All regressions include the full set of main effects and interaction terms implied by the specification; for readability, only Post × Diesel and the selected higher-order interaction coefficients shown in the table are reporte. Robust standard errors clustered at the event-stack × county level are reported in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% levels.

Table C.15: Effect of Pollution on Diesel Car Loan Conditions (cont.)

	Captive Banks (1)	Independent Banks (2)	All (3)
Panel C: Δ Loan Maturity (months)			
Diesel	0.550	0.752**	0.609*
	(0.588)	(0.353)	(0.345)
$\mathbf{1}\{NO_2 > 40\}$	-0.899	-0.745	-0.952
	(1.984)	(2.073)	(1.863)
Diesel × $\mathbf{1}\{NO_2 > 40\}$	-0.704	-0.743	-0.935
	(2.105)	(4.004)	(4.122)
Diesel × Captive			0.906
			(0.720)
$\mathbf{1}\{NO_2 > 40\}$ x Captive			2.136
			(2.456)
Diesel × $\mathbf{1}\{NO_2 > 40\}$ x Captive			1.401
			(4.161)
R-squared	0.162	0.119	0.114
Observations	2372	8975	11355
Mean loans per collapsed cell	26.1	26.3	26.2
Panel D: Δ Down Payment Amount (LN EUR)			
Diesel	0.134	0.065	0.076
	(0.106)	(0.048)	(0.049)
$\mathbf{1}\{NO_2 > 40\}$	-0.068	0.651*	0.669**
	(0.394)	(0.330)	(0.314)
Diesel × $\mathbf{1}\{NO_2 > 40\}$	-0.011	-0.447	-0.487
	(0.409)	(0.561)	(0.547)
Diesel × Captive			0.008
			(0.096)
$\mathbf{1}\{NO_2 > 40\}$ x Captive			-0.818
			(0.545)
Diesel × $\mathbf{1}\{NO_2 > 40\}$ x Captive			0.575
			(0.708)
R-squared	0.047	0.037	0.036
Observations	2372	8975	11355
Mean loans per collapsed cell	26.1	26.3	26.2
Panel E: Δ Loan Amount (LN EUR)			
Diesel	0.028	0.013	0.010
	(0.018)	(0.008)	(0.008)
$\mathbf{1}\{NO_2 > 40\}$	0.037	0.010	0.012
	(0.047)	(0.064)	(0.060)
Diesel × $\mathbf{1}\{NO_2 > 40\}$	-0.109	-0.078	-0.081
	(0.082)	(0.107)	(0.109)
Diesel × Captive			0.036**
			(0.017)
$\mathbf{1}\{NO_2 > 40\}$ x Captive			0.047
			(0.082)
Diesel × $\mathbf{1}\{NO_2 > 40\}$ x Captive			-0.012
			(0.133)
R-squared	0.160	0.093	0.095
Observations	2372	8975	11355
Mean loans per collapsed cell	26.1	26.3	26.2
Borrower income	Yes	Yes	Yes
Event-stack FE	Yes	Yes	Yes
Model FE, Bank FE, State FE	Yes	Yes	Yes

Notes: This table reports collapsed-cohort first-difference estimates of the effect of local air-pollution exceedances on loan conditions for diesel relative to petrol vehicles in Germany. The event is defined as the first year in which the maximum annual mean NO_2 concentration across monitoring stations within a county exceeds the EU limit of 40 $\mu g/m^3$. Event stacks are constructed using two-year pre- and post-event windows, with never-breaching counties serving as controls within each stack. Loans are collapsed to cohort cells defined by lender bank, car model and brand, fuel type, borrower income group, and county, and outcomes are measured as post-minus-pre differences. All specifications include event-stack, car model, bank, and state fixed effects, as well as borrower income controls. Standard errors clustered at the event-stack–by–state level are reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. t-statistics in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table C.16: Effect of Pollution on Diesel Car Loan Conditions: Loan-Level

	Captive (1)	Independent (2)	All (3)
Panel A: Loan Rate (p.p.)			
Post × Diesel	0.005	-0.001	-0.000
	(0.016)	(0.011)	(0.011)
Post × Diesel × $\mathbf{1}\{NO_2 > 40\}$	-0.024	-0.028	-0.038
	(0.075)	(0.059)	(0.061)
Post × Diesel × $\mathbf{1}\{NO_2 > 40\}$ × Captive			0.003
			(0.097)
Observations	115,457	387,231	502,789
Adj. R^2	0.455	0.333	0.350
Panel B: Loan-to-Value Ratio (p.p.)			
Post × Diesel	-0.382	0.214	0.295
	(0.389)	(0.219)	(0.220)
Post × Diesel × $\mathbf{1}\{NO_2 > 40\}$	0.275	0.610	0.479
	(1.498)	(1.185)	(1.210)
Post × Diesel × $\mathbf{1}\{NO_2 > 40\}$ × Captive			-0.199
			(1.894)
Observations	115,399	386,844	502,335
Adj. R^2	0.136	0.046	0.098
Panel C: Loan Maturity (months)			
Post × Diesel	0.373	0.395**	0.370**
	(0.267)	(0.173)	(0.175)
Post × Diesel × $\mathbf{1}\{NO_2 > 40\}$	-0.672	1.290	1.093
	(1.011)	(0.859)	(0.869)
Post × Diesel × $\mathbf{1}\{NO_2 > 40\}$ × Captive			-1.838
			(1.408)
Observations	115,457	387,231	502,789
Adj. R^2	0.277	0.078	0.120
Panel D: Down Payment Amount (LN EUR)			
Post × Diesel	0.011	-0.005	-0.006
	(0.017)	(0.008)	(0.009)
Post × Diesel × $\mathbf{1}\{NO_2 > 40\}$	-0.032	0.040	0.044
	(0.082)	(0.061)	(0.060)
Post × Diesel × $\mathbf{1}\{NO_2 > 40\}$ × Captive			-0.088
			(0.095)
Observations	66,360	242,721	309,194
Adj. R^2	0.228	0.146	0.152
Panel E: Loan Amount (LN EUR)			
Post × Diesel	-0.001	-0.000	0.001
	(0.009)	(0.004)	(0.004)
Post × Diesel × $\mathbf{1}\{NO_2 > 40\}$	-0.022	0.038*	0.043**
	(0.037)	(0.021)	(0.021)
Post × Diesel × $\mathbf{1}\{NO_2 > 40\}$ × Captive			-0.063*
			(0.038)
Observations	115,457	387,231	502,789
Adj. R^2	0.288	0.246	0.307
Borrower income	Yes	Yes	Yes
Stack×month FE	Yes	Yes	Yes
Stack×model FE	Yes	Yes	Yes
Stack×state×bank×month FE	Yes	Yes	Yes
Clustered SE	County×Stack	County×Stack	County×Stack

Notes: This table reports loan-level difference-in-differences estimates of the effect of local air-pollution exceedances on diesel relative to petrol car loan conditions. The coefficient of interest is the triple interaction *Post* × *Diesel* × $\mathbf{1}\{NO_2 > 40\}$; column (3) additionally reports its differential for captive lenders. All specifications include borrower income controls and a full set of event stack-by-month, event stack-by-model, and event stack-by-state-by-bank-by-month fixed effects. Robust standard errors clustered at the county × event-stack level are reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

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