Congestion pricing with electric vehicle exemptions: Car-ownership effects and other behavioral adjustments

Elisabeth T. Isaksen   Bjørn G. Johansen*

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Abstract

Decarbonizing transportation requires a shift from conventional to zero-emission vehicles. We examine whether congestion pricing with electric vehicle (EV) exemptions accelerates this transition by encouraging a shift toward cleaner cars. To identify causal effects, we combine administrative data on car ownership with a triple-differences design that exploits household-level variation in policy exposure across metropolitan areas and work commutes. We find that higher rush hour charges for conventional vehicles significantly increase EV adoption, primarily through replacement rather than fleet expansion. However, responses vary by socioeconomic characteristics, with higher-income and well-educated households more likely to adopt EVs. Beyond car ownership, we document behavioral adjustments, including relocation to avoid tolls, re-routing around the cordon, and shifting travel timing. Overall, congestion pricing reduced traffic volumes and improved air quality. Our findings offer insights for designing equitable and effective transportation policies.

Keywords: congestion pricing, electric vehicles, car ownership, transportation policy, traffic, air pollution
JEL codes: H23, R41, R48, Q58, Q52, Q55

*First version: May 2021. Isaksen (corresponding author): The Ragnar Frisch Centre for Economic Research, Oslo, Norway, and Grantham Research Institute on Climate Change and the Environment, London School of Economics, United Kingdom (Visiting Associate) (email: e.t.isaksen@frisch.uio.no); Johansen: Institute of Transport Economics, Oslo, Norway and University of Oslo, Department of Economics, Oslo, Norway (email: bgj@toi.no). An earlier version of this paper was previously circulated under the title "Congestion pricing, air pollution, and individual-level behavioral responses". We are grateful for comments and suggestions by anonymous reviewers, Laura Grigolon, Erin Mansur, Katinka Holtsmark, Andreas Moxnes, Gregor Singer, and audiences at the London School of Economics, the University of British Columbia, University of Manitoba, Manchester University, the Norwegian School of Economics, SLU Uppsala, the 25th EAERE, and the CER-ETH Workshop on Energy, Innovation, and Growth. This research was supported with funding from the Grantham Foundation (through the Grantham Research Institute on Climate Change and the Environment, London School of Economics) the Centre for Climate Change Economics and Policy, and the Research Council of Norway (grants no. 267942, 302059, 295789, 336324). Administrative registers made available by Statistics Norway have been essential. Replication package: https://doi.org/10.7910/DVN/GHZDDG.

1 Introduction

Cordon-based congestion pricing is increasingly used to mitigate air pollution and traffic congestion in urban centers.[1] By imposing higher costs on driving in congested areas – especially during rush hours – these policies ensure that private costs better reflect overall social costs by accounting for congestion and emission externalities. Differentiating charges by vehicle type further incentivizes drivers to switch to cleaner cars. Electric vehicles (EVs) are often highlighted as a key technology for reducing emissions in this hard-to-abate sector, and combining traditional transportation policies with lower rates for cleaner cars may accelerate the green transition.[2] Such a shift may enhance the benefits of congestion pricing by reducing emissions beyond the cordon and outside peak hours. However, granting lower rates or exemptions for EVs may counteract other policy objectives by dampening congestion reduction and potentially increasing car ownership.

This paper examines the car-ownership effects of a time-varying congestion charge with exemptions for electric vehicles, by combining individual-level administrative data with a novel research design. We complement the main analysis with evidence on various – both intended and unintended – adjustment mechanisms, such as sorting behavior and changes in driving patterns. Specifically, we ask: What are the impacts of increased rush hour congestion charges on household-level EV adoption and car ownership? How do household responses vary with socioeconomic characteristics and substitution possibilities? And to what extent do households adjust through channels other than car ownership?

Increased rush hour rates for conventional vehicles may lead to multiple adjustments. First, individuals may respond by adopting an electric vehicle. The impact on a household's car ownership depends on whether they dispose of their existing vehicle, replace a conventional car with an electric one, or add an EV to their existing fleet. Household adjustments may also vary based on socioeconomic characteristics, such as the ability to purchase a new car, and substitution possibilities, such as the quality of public transit. Second, individuals may avoid the congestion charge by re-routing, changing their departure time, relocating their workplace or residence, or switching to alternative modes of transportation, such as public transit, cycling, walking, or working from home. While some of these behaviors align with policy objectives – such as avoiding rush hours or shifting to public transit – others, like driving around the toll cordon, are largely unintended. Previous studies

[1] Major cities such as London, New York, Milan, Singapore, and Stockholm have implemented a cordon congestion charge, where drivers pay a fee to enter a designated zone within a city.

[2] Throughout this paper, "electric vehicles" refer specifically to battery-electric vehicles.

have shown that policies aimed at mitigating congestion and urban air pollution can sometimes lead to unintended consequences (Davis, 2008; Auffhammer and Kellogg, 2011; Bento et al., 2014; Gibson and Carnovale, 2015; Barahona et al., 2020), occasionally even resulting in net welfare losses (Davis, 2008). These unintended effects may arise from drivers' substitution behavior or the exploitation of policy loopholes. As a result, the net benefits of rush hour congestion charges with EV exemptions, as well as their distributional consequences, remain an open question.

To examine the car-ownership effects of congestion charging with EV exemptions, we take advantage of a sharp increase in rush hour cordon charges in 2016 in the second largest city in Norway (Bergen). The policy raised the price of entering the city center toll cordon during rush hours by 80% for gasoline and diesel vehicles. Before the policy change, toll payments did not vary by time of day. Electric vehicles have been exempt from paying congestion charges and road tolls in Norway since 1997 and throughout our study period, and the policy hence significantly increased the relative price of driving a high-emission versus a low-emission vehicle during rush hours.

The magnitude of the rush hour charges was substantial; for a daily commuter driving during rush hours, the costs roughly equated to the annual fuel expenses of an internal combustion engine vehicle (see Appendix Table A.4). Consequently, drivers of conventional cars had a strong financial incentive to switch to an electric vehicle. Access to electric vehicles was limited before 2010, and policies favoring these cars likely had a modest impact on adoption. However, the roll-out of several high-quality models over the past decade made electric vehicles a viable option, thereby expanding the opportunity set for drivers (Figenbaum et al., 2015). National tax exemptions and low electricity prices also made electric vehicles competitive with conventional cars in the time period analyzed; the total annual ownership cost of a new electric vehicle was roughly equivalent to that of a 10-year-old gasoline vehicle before considering the congestion charge (see Appendix Table A.4).

To examine household-level responses to the cordon congestion charge, we exploit exceptionally detailed data on car ownership and commuting routes. Specifically, we combine administrative data on the universe of cars in Norway with detailed socioeconomic information on households, including the workplaces of household members. By linking data on the locations of individuals' homes and workplaces with the locations of cordon toll gates, we identify households that need to cross the cordon when traveling between home and work. Given that work-related trips account for approximately one-third of the kilometers driven by commuting households (Grue et al., 2021), we expect exposure to congestion charges on the work commute to be an important factor in households' car ownership decisions.

To recover causal effects, we construct treatment and control groups in a triple differences framework. We define the treatment group as households exposed to the Bergen congestion charge on their way to work in 2014 (*paying commuters*) and the first control group as households in Bergen where the 2014-work route did not have toll gates (*non-paying commuters*). By including household and neighborhood-year fixed effects, we compare within-household changes in car ownership for those living in the same neighborhood, where households are differently exposed to the policy due to different work locations. Due to different toll exposure in the pre-period, however, paying commuters had a higher EV acquisition rate also before the policy change. To address the non-parallel trends, we subtract a similar difference between paying and non-paying commuters in three control cities that have a toll cordon, but where rush hour charges did not change. The triple difference framework thus exploits variation across i) paying versus non-paying commuters, ii) Bergen versus control cities, and iii) before versus after the increase in congestion charges.

Results from the empirical examination show that households exposed to the Bergen congestion charge were nearly 3 percentage points more likely to adopt an electric vehicle. This estimated treatment effect explains more than 1/5 of the increase in electric vehicle adoption in the treatment group from 2014 to 2017. The increase in EVs is mirrored by a negative effect on gasoline and diesel vehicle ownership, resulting in no net change in total car ownership. Hence, *on average*, households replaced their fossil fuel car with an electric one.

Examining heterogeneous effects, we find strong disparities across several socioeconomic dimensions. While the policy had no effect on EV adoption among households in the lowest income quintile, ownership rates in the highest income quintile increased by 5.4%. Furthermore, treatment effects are larger for university-educated couples with children and for households with longer commutes and poor public transit quality. The latter finding suggests that the availability of transportation substitutes influences household adaptation responses. For the bottom quintile, however, the probability of adopting an EV remains zero, regardless of education, age, or public transit quality. While heterogeneous responses may partially reflect differences in preferences, financial constraints likely play a key role in limiting EV adoption. During the study period, the second-hand market for electric vehicles was nearly non-existent, making EV adoption effectively synonymous with purchasing a new car.

Overall, our findings on car ownership suggest that cordon-based congestion charging combined with exemptions for electric vehicles can be a powerful tool for promoting electric vehicle adoption, though there are substantial differences in how households respond to the policy.

To gain a more comprehensive understanding of the implications of time-varying congestion charges, we complement our car ownership analysis with an examination of their effects on sorting behavior, traffic patterns, and air pollution. Using residential relocation and job changes as outcome variables, we find evidence of sorting behavior to avoid the toll cordon. On average, treated households are 0.3 percentage points more likely to move out of the toll cordon and 0.7 percentage points more likely to relocate their workplace outside the cordon. However, these sorting responses have only a minor impact on our car ownership estimates.

To examine impacts on traffic and air pollution we combine high-frequency sensor and monitoring station data with a difference-in-differences strategy that compares outcomes across cities. Results from this supporting analysis indicate that the policy reduced rush hour traffic into the city center by 14% and daily traffic by 5.5%. We find evidence of inter-temporal substitution, with increased traffic in the 15–30 minutes before and after rush hours, as well as spatial substitution towards lower-priced roads. However, the net effect on traffic remains negative and substantial, suggesting that a significant share of drivers switched to other modes of transportation or canceled their trips altogether. Furthermore, we find suggestive evidence that the policy improved air quality, with an 11% reduction in NO_2 concentrations during midday hours and a daily decrease of 9.6% (or 4.1 μg/m^3). A similar percentage decline is observed for PM_{10}, though estimates are too noisy to draw firm conclusions.

Taken together, our findings highlight both the effectiveness and limitations of congestion pricing schemes with EV exemptions. While these policies accelerate EV adoption, they may also introduce equity concerns and unintended behavioral responses.

Our findings offer several insights for policymakers. First, we demonstrate that differentiating driving costs by vehicle type and time of day can encourage a shift toward cleaner cars. As Norway has been a front-runner in EV adoption, our results may be particularly relevant for anticipating the future effects of driving-related incentives in other countries, where EVs are expected to become more competitive with conventional vehicles. Second, while imposing higher costs on driving often raises distributional concerns, empirical evidence on how households are differently affected is often lacking. The richness of our data allows us to shed light on heterogeneous responses across household types, providing valuable insights for policymakers seeking to balance efficiency and equity in transportation policies.

1.1 Related literature

Our paper contributes to the empirical literature on how governmental policies influence the adoption of low-emission vehicles, particularly electric vehicles. Existing studies often focus on purchase-related incentives, such as tax credits and purchasing subsidies (Gallagher and Muehlegger, 2011; Clinton and Steinberg, 2019; Muehlegger and Rapson, 2022), while a few examine driving-related incentives, including charging infrastructure (Springel, 2021; Li et al., 2017), low emission zones (Wolff, 2014; Aydin et al., 2024), and vintage-specific driving restrictions (e.g., Barahona et al., 2020). As far as we are aware, this is the first study to examine the effects of a rush hour congestion charges with an EV exemption on household-level EV adoption and car ownership.[3] Previous studies also typically focus on new vehicle registrations, often at the zip code, metropolitan, or state level. In contrast, we use household-level car ownership data and exploit within-household variation in policy exposure across metropolitan areas and work commutes, allowing us to more credibly recover causal effects. By examining car ownership rather than new registrations, we can assess whether EVs replace or expand the vehicle fleet – an essential distinction for evaluating net environmental and climate benefits. Additionally, the richness of our data allows us to explore distributional aspects of congestion charging, extending beyond the existing literature (Börjesson and Kristoffersson, 2018; West and Börjesson, 2020).

The closest study to ours is Halse et al. (2025), which can be seen as a companion paper. The paper examines the effects of bus lane access and road toll rates across Norway on car ownership using a fixed effects specification. While road tolls primarily aim to generate revenue for infrastructure investments in rural and urban road networks, rush hour charges are designed to address congestion and pollution externalities in urban areas. As a result, behavioral responses may differ. Additionally, while Halse et al. (2025) primarily rely on cross-sectional variation, we exploit a substantial increase in congestion charges within a triple-differences framework. By focusing on cordon-based charging, we are able to examine behavioral adjustments such as re-routing, departure time shifts, sorting in and out of the cordon, and the broader effects on traffic and air pollution.

Our paper also complements the empirical literature on the effects of transportation policies on driving-related externalities such as air pollution and traffic congestion. Previous studies have shown that e.g., low emission zones (Wolff, 2014;

[3] While some studies examine the effects of congestion charging on car ownership (e.g., Morton and Ali, 2025; Gonzalez et al., 2021), these policies do not include EV incentives, and the papers do not analyze EV adoption. Krehic (2022) documents that the EV ownership rate has implications for the toll rates for diesel and gasoline cars, but does not study how toll rates affect car ownership.

Gehrsitz, 2017; Pestel and Wozny, 2019; Zhai and Wolff, 2021; Green et al., 2020; Rivera, 2021; Klauber et al., 2024), road tolls (Fu and Gu, 2017), and congestion charges (Gibson and Carnovale, 2015; Simeonova et al., 2019) can help improve urban air quality, with resulting health benefits (Currie and Walker, 2011; Gehrsitz, 2017; Simeonova et al., 2019; Pestel and Wozny, 2019; Klauber et al., 2024). While these studies provide important estimates on the environmental and health effects of transportation policies, very few studies examine the *underlying mechanisms* through which individuals respond to these policies, as well as how mechanisms differ across households.

2 Background

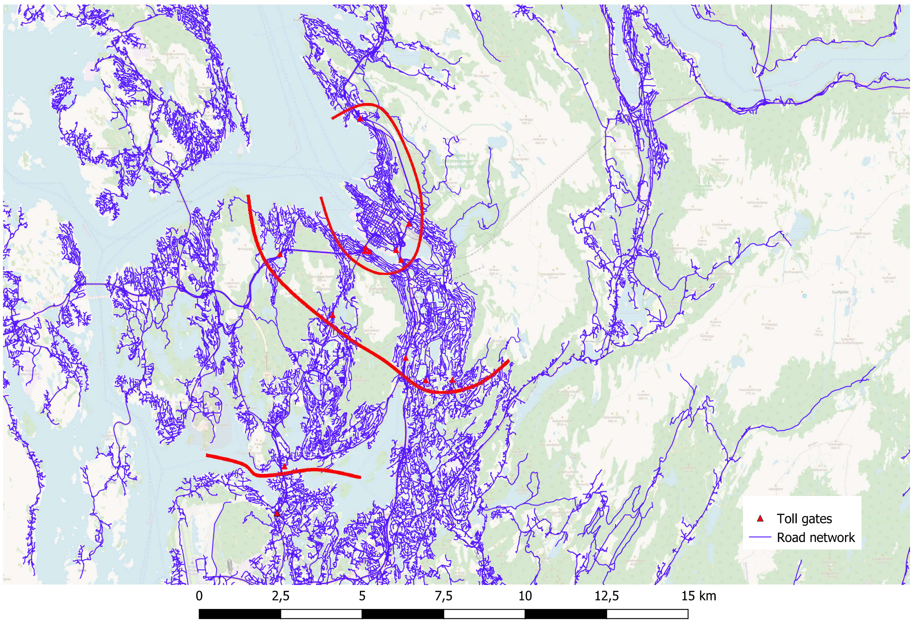
The Bergen congestion charge was announced in February 2015 and implemented one year later, on February 1, 2016. The policy aimed to mitigate rush hour congestion and reduce inner-city air pollution, particularly NO_X concentrations. The stricter regulations were largely prompted by a 2015 EFTA court ruling, which convicted Norway of violating the EU's ambient air quality standards.[4] The increased rush hour rates were also designed to accelerate EV adoption. At the time, Norway aimed for a 100% EV market share by 2025 (NTP, 2017), and the exemptions from congestion charges were among several incentives for EV owners (see Appendix A for a full list of incentives). Since nearly 98% of Norway's electricity is generated from renewable sources (Statistics Norway, 2020), EVs cause minimal indirect CO_2 emissions from driving.

The Bergen congestion charge was electronically collected via the existing automated toll gates in and around the city center. Figure 1 illustrates the location of the toll cordon borders in Bergen, and shows that in order to reach the city center a driver would need to pass the toll cordon. Vehicles were charged when first entering the toll cordon, and a vehicle was only charged once when passing the cordon several times within an hour.

Table 1 presents the details of the congestion charging policy. Prior to February 1, 2016, passenger vehicles passing through the toll cordon paid NOK 25 (approximately \$3) regardless of the time of day. After February 1, 2016, passenger cars faced a rush-hour rate of NOK 45 (approximately \$5.40) on weekdays, during the hours 06:30-09:00 and 14:30-16:30, representing an 80% price increase. Simultane-

[4] In 2015, Norway was found guilty in the EFTA court for breaching EU ambient air quality standards in several areas, including Bergen. Most violations were linked to excessive NO_2 concentrations in urban regions. As a result, Norway was required to implement measures to comply with the EU Air Quality Directive.

Figure 1: Toll cordon borders in Bergen



Notes: The figure illustrates the toll cordon borders in Bergen. Red triangles mark the locations of toll gates, while red lines indicate roads where passing through without encountering a toll cordon is impossible. Thin blue lines represent the road network in and around Bergen.

ously, the non-rush rate was reduced to NOK 19 (approximately \$2.30), reflecting a 24% price decrease. Electric vehicles were exempt from tolls both before and after the introduction of the time-varying congestion charge. Consequently, the policy further increased the relative cost of driving a diesel or gasoline vehicle compared to an electric vehicle.[5]

[5] Appendix Figure A.1 depicts the development in toll rates in Bergen from 2005 to the end of 2017. The figure shows that rates were fixed at NOK 15 for all hours of the day from 2005 to 2013. In 2014, the rate for all hours increased to NOK 25. In 2016, separate rates were introduced for rush and non-rush hours, set at NOK 45 and 19, respectively, throughout 2017.

Table 1: Congestion charging in Bergen

Date announced	Feb 18, 2015
Date implemented	Feb 1, 2016
Morning rush	06:30-09:00
Afternoon rush	14:30-16:30
Price pre Feb 1, 2016	NOK 25 (∼\$3)
Price post Feb 1, 2016: rush hour	NOK 45 (∼\$5.4)
Price post Feb 1, 2016: non-rush	NOK 19 (∼\$2.3)

Notes: The table provides details of the congestion charging scheme in Bergen, with rates given in NOK (10 NOK ≈ 1 EUR ≈ 1.2 USD). These rates correspond to the initial implementation levels and apply to small passenger vehicles (< 3500 kg). For large vehicles (> 3500 kg), the toll was 50 NOK before February 1, 2016, and increased to 90 NOK during rush hours and 38 NOK outside rush hours after policy implementation. Electric vehicles were exempt from congestion charges and tolls throughout the study period, while hybrid electric vehicles were subject to the same rates as internal combustion engine vehicles (ICEVs). A monthly cap was set on the total toll costs per vehicle, allowing a vehicle to enter the toll cordon free of charge once the cap was reached. However, this cap was too high to be binding for regular commuters (60 entries per month), meaning a vehicle would need to cross the toll cordon more than twice per day for the cap to take effect.

3 Data

In this paper, we aim to estimate the effects of the congestion charge on household-level car ownership using rich administrative data. Below, we describe the key data sources and the approaches used to construct our main outcome variables and proxies for policy exposure.[6]

Car ownership: We compile data from the National Motor Vehicle register on the full population of vehicles registered in Norway between 2011 and 2017. The register includes technical vehicle details and ownership information, such as dates of first registration, ownership changes, scrapping, and de-registration. We focus on privately owned passenger vehicles and vans registered for non-commercial purposes, measuring ownership as of December 31st each year. Although cars are registered at the individual level, we treat car acquisitions as a household-level decision. Thus, our main outcome variable is household-level car ownership. This results in a panel of car ownership data at the household×year level, with each observation representing the snapshot of cars owned at the end of each year. From December 2011 to December 2017, the share of Norwegian households owning an electric vehicle rose from approximately 0% to 4.5%. By 2017, Norway had the highest ownership share of electric vehicles globally (IEA, 2018).

Socioeconomic characteristics: We integrate the car ownership data with detailed socioeconomic information on individuals and households, including age, gen-

[6] For a detailed description of the data sources, see Fevang et al. (2021).

der, household size (number of adults and children), employment and retirement status, income, wealth, education, and ownership of a second home (e.g., cabin). Crucially, the data also includes the location of each residence at the basic statistical unit level, which is the smallest geographical unit at which the micro-data is available to researchers. We refer to these units as "neighborhoods". There are approximately 14,000 neighborhoods in Norway, each with an average population of around 400 individuals, or fewer than 200 households.[7] This granular information allows us to control for various characteristics that might influence car ownership and to examine heterogeneous effects of the policy in our empirical analysis.

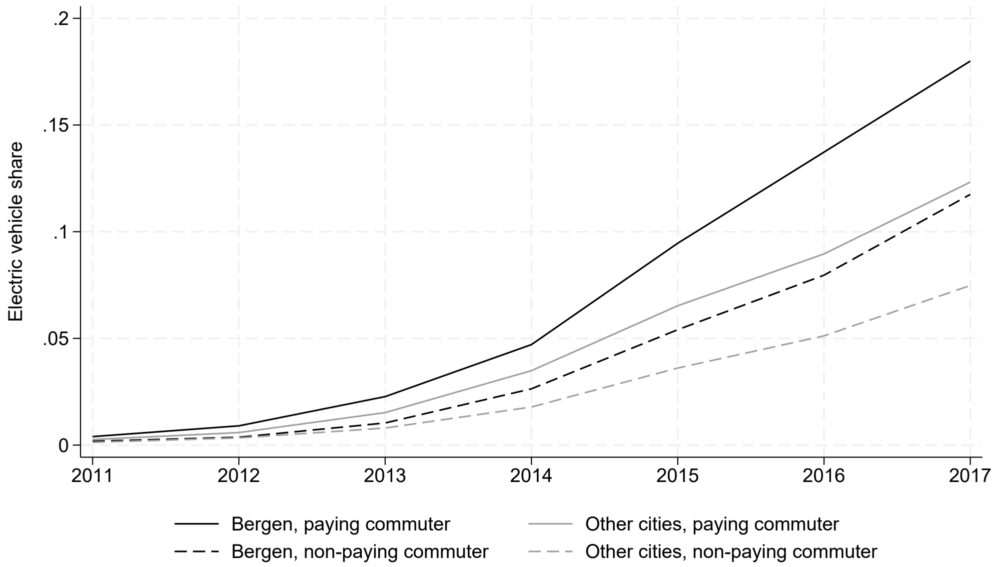
Work commute and policy exposure: By linking employed individuals to their employer, we can identify the neighborhood of their workplace. We then combine this information with road network data (The Norwegian Mapping Authority, 2019) to determine the fastest route between the centroids of residential and workplace neighborhoods. Using toll gate coordinates and rates from the Norwegian Public Roads Administration, we also calculate the toll payments associated with each route. These toll payments serve as a measure of individual-level exposure to the cordon congestion charge for work trips. As discussed in Section 1 and 2, work-related trips are likely a significant factor in households' responses to the policy. In addition to toll exposure, we calculate other work trip-related variables such as driving time, distance to and from work, and door-to-door time when using public transit. For additional details, see Appendix B.

4 Empirical strategy

To identify causal effects on household-level car ownership, we need to disentangle the policy effects from other confounding trends, such as the increased supply of electric vehicles, national EV policies, and expanding charging infrastructure. We start by defining two groups of households: *paying commuters* and *non-paying commuters*. Paying commuters are households where at least one individual crosses the toll cordon between their residence and workplace. Non-paying commuters are households where none of the working individuals are exposed to toll payments on their work commute. These definitions are based on residential and workplace locations in 2014 – one year before the announcement of the time-varying congestion change – to ensure that treatment status does not change in response to potentially endogenous sorting. The effects on relocation and job changes are analyzed separately (see Section 7.1).

[7]See Appendix B.1 for an illustration of neighborhood size.

Figure 2: Share of households owning an electric vehicle (2011–2017)



Notes: The figure plots the share of households that own at least one electric vehicle on December 31 each year over the period 2011-2017, by four different groups. The first observation reflects the electric vehicle share on December 31, 2011, and the last observation reflects the electric vehicle share on December 31, 2017. The time-varying congestion charge in Bergen was announced in February 2015 and implemented in February 2016. Paying commuters are households where at least one individual passes the toll cordon between the residence and the workplace. Non-paying commuters are households where none of the working individuals are exposed to toll payments on the home-work route. The definitions are based on the residence and work locations in 2014. Other cities include Stavanger, Kristiansand, and Haugesund; see Appendix B for details.

Based on the two groups of households, a potential identification strategy could be a Difference-in-Differences (DiD) framework. This approach would absorb any time-invariant difference between the two groups. However, since *paying commuters* were also exposed to road tolls before the sharp increase in rush hour charges, they might follow a different trend than *non-paying commuters* in the pre-period. Specifically, given the increased supply, variety, and quality of electric vehicles over time, and the fact that electric vehicles have been exempt from toll payments since the 1990s, we might expect to see a larger increase in electric vehicle ownership among paying commuters compared to non-paying commuters even in the absence of the rate increase. This concern is supported by Figure 2, which shows that paying commuters in Bergen have been acquiring EVs at a faster rate than non-paying commuters since 2011. Additionally, a dynamic DiD specification comparing paying to non-paying commuters in Bergen confirms that the difference in pre-trends persists (see Appendix Figure C.1).[8]

To address the problem of non-parallel trends, we utilize variation from other selected cities with a toll cordon around their centers in a triple differences (DiDiD)

[8] A dynamic DiD specification comparing paying commuters in Bergen to paying commuters in other cities also reveals non-parallel pre-trends. Results are available upon request.

framework. During our study period, electric vehicles were exempt from all road tolls in Norway, allowing them to pass any toll cordon free of charge. We identify Stavanger, Kristiansand, and Haugesund as suitable control cities due to their size, location, and the absence of significant changes in their toll rate schemes throughout our sample period (see Appendix B.2 for details). Figure 2 shows that paying commuters in these control cities also increased their electric vehicle ownership beyond that of non-commuters even in the absence of any toll increase. Estimating a placebo DiD regression for the control cities confirms this picture (see Appendix Figure C.1). The DiDiD setup enables us to recover a causal estimate of the increased congestion charges in Bergen by subtracting the estimated difference between paying and non-paying commuters in the control cities.

More formally, our DiDiD estimator is written as:

$$y_{it} = \beta \text{post}_t \times c_i \times B_i + \alpha_t c_i + \theta_{nt} + \eta_i + X_{it}'\gamma + \varepsilon_{it}, \tag{1}$$

where i indicates household, t indicates year, y_{it} is the household level outcome variable in a given year, post_t is a dummy variable equal to 1 for years after the policy change, c_i is a dummy variable equal to 1 for paying commuters, and B_i is a dummy variable equal to 1 for households in or near Bergen. The coefficient of interest, β, reflects the effect of the policy change on paying commuters in Bergen.

The term $\alpha_t c_i$ captures any differential trends in car ownership over time between paying and non-paying commuters. Since paying commuters are exposed to stronger EV incentives than non-paying commuters, even in the absence of the treatment, and because the variety and quality of EVs have improved over time, we expect paying commuters to adopt EVs at a faster rate. The term $\alpha_t c_i$ will absorb this trend based on the difference between paying and non-paying commuters in control cities. The neighborhood $\times$ year fixed effects θ_{nt} account for any neighborhood-level variation over time that affects both paying and non-paying commuters. This includes variations in other local electric vehicle incentives that potentially vary over time and across neighborhoods, such as the availability of charging stations and parking spaces.[9] We fix n to the households' 2014 residence location to ensure that neighborhood fixed effects are not affected by endogenous sorting in response to the policy. Household fixed effects η_i capture any time-invariant difference across households, including those related to being a paying commuter in Bergen. We also

[9]Note that the neighborhood $\times$ year fixed effects will not be able to address potential spillover effects from paying to non-paying commuters within neighborhoods, such as peer effects in car ownership (see e.g., Isaksen et al., 2022; Tebbe, 2023). If peer effects among neighbors are substantial, our estimates likely represent a lower bound on the true effect of the congestion charge on EV adoption.

include a vector of time-varying household-level controls, X'_{it}, to account for other factors that may influence car ownership, such as income and employment status (see Appendix B.3 for further details). ε_{it} represents the idiosyncratic error term.

A key identifying assumption is that, in the absence of the congestion charge, the difference in outcomes between paying and non-paying commuters in Bergen versus other cities would have evolved in a similar manner. This assumption is conditional on the control variables and fixed effects included in our specification. Although the parallel trends assumption is inherently untestable, we assess its validity by estimating a dynamic version of our DiDiD estimator, where treatment effects are allowed to vary over time. By defining the year prior to the policy announcement (2014) as the reference year, the dynamic DiDiD estimator can be specified as:

$$y_{it} = \sum_{s \in \{T | s \neq 2014\}} \left[\alpha_t c_i + \beta_t c_i \times B_i \right] \times \mathbb{1}\{t = s\} + \eta_i + X'_{it}\gamma + \theta_{nt} + \varepsilon_{it}, \qquad (2)$$

where β_t captures the annual treatment effects. Under the parallel trends assumption, we expect $\beta_t \approx 0$ for years prior to 2014.

4.1 Interpretation and threats to identification

As our empirical strategy relies on policy exposure on the work commute, our estimated treatment effect should be interpreted as a local treatment effect for the subpopulation of households where at least one individual is employed. For households where none of the individuals are employed (e.g., students, retirees, unemployed), the effects of the congestion charge may be very different.

Importantly, our triple differences estimate is an intention-to-treat (ITT) measure. That is, we do not observe whether households actually paid the congestion charge; rather, we use their potential *exposure* based on residential and workplace locations recorded in 2014. Consequently, some households classified as "treated" may not regularly use a car for commuting.[10] Treated households' exposure to toll payments may also evolve over time due to endogenous behavioral responses, such as relocating or switching jobs to avoid the congestion charge. Any resulting adjustments in car ownership are part of the overall treatment effect captured by our ITT estimate. In Section 7, we explicitly study several other margins of adjustment, including changes in traffic patterns and households' relocation choices.

[10]According to the National Travel Survey 2018/2019 (Grue et al., 2021, Table 7.4), around 43 percent of those living in the city center of Bergen commute by car, while 77 percent of those living in the suburbs around Bergen commute by car. As we restrict our sample to individuals where the work commute is more than 5 kilometers (see Section 5), the share of individuals commuting by car is likely higher in our sample.

As the policy both increased rush hour rates and slightly lowered non-rush rates, the average price signal faced by households is uncertain. However, we argue that the small rate reduction outside peak hours should have negligible effects on our results. First, while traffic during rush hours was significantly reduced after the congestion charge was implemented, there was almost no change in traffic during non-rush hours in the two years after the policy change; see Appendix Figure F.1. We consider it unlikely that a price change with no impact on traffic would significantly affect car ownership.

Second, our triple difference estimate is designed to net out confounding effects from non-work-related trips, which are likely to occur outside peak hours. Specifically, households living in the same neighborhood are likely to have similar local travel patterns for leisure or errands. By including neighborhood-year fixed effects, our identification strategy absorbs these within-neighborhood variations in non-work trip behavior, thereby isolating the effect of the work commute.

Third, those who commute to work will often have limited flexibility to adjust their travel times due to fixed work schedules or other commitments, such as child-care or co-commuting with partners. The relatively broad rush hour pricing window (06:30–09:00 and 14:30–16:30) suggests that even jobs with flexible hours typically require commuting during peak periods. In Norway, the standard workday is 7.5 hours, and even positions with flexible schedules generally include core working hours from 9:00 to 15:00. This leaves only 1.5 hours for schedule adjustments, making it difficult for most workers to entirely avoid rush hour tolls. Additionally, during our study period (pre-pandemic), the incidence of remote work was low, further limiting the feasibility of avoiding congestion charges.

Taken together, these patterns suggest that rush hour rates play a more important role in influencing car ownership decisions than non-rush rates – in particular when considering work commutes.

4.2 Heterogeneous effects

To examine how different types of households respond to the congestion charge, we estimate a variant of the DiDiD estimator that allows treatment effects to vary across different socioeconomic groups. Let $k \in \mathcal{K}$ denote a specific group (e.g., income quintile). The heterogeneous DiDiD can be expressed as:

$$\begin{aligned} y_{it} = &\sum_{k \in \mathcal{K}} \left[\beta_k \text{post}_t \times B_i \times c_i + \alpha_{tk} c_i + \delta_{tk} \right] \mathbb{1}\{i \in k\} \\ &+ \eta_i + X_{it}' \gamma + \theta_{nt} + \varepsilon_{it}, \end{aligned} \tag{3}$$

where β_k represents the treatment effect for group k. All coefficients, except for the demographics and neighborhood $\times$ year fixed effects, are specific to each group k. This approach ensures a flexible model that accounts for various group-specific time-varying factors.

5 Sample restrictions and descriptives

Sample restrictions: In our analysis, we focus on households as the unit of observation. Based on our empirical strategy, we restrict the sample to households located within 50 kilometers of Bergen, Stavanger, Kristiansand, or Haugesund in 2014, and where at least one household member was employed in 2014. Further, we focus on households where the 2014 work distance was between 5 and 50 kilometers (km). We apply a 5 km cut-off to ensure that driving is a relevant commuting option. For very short commutes, our proxy for policy exposure may be less applicable, as households are more likely to use alternative modes of transportation, such as walking, biking, or public transportation.[11]

Based on the sample of households with work commutes between 5 and 50 km, we define *paying commuters* as households where at least one household member crosses the toll cordon (in Bergen or the control cities) on his/her way to work in 2014, the year prior to the rush hour congestion charge in Bergen being announced. We also ensure that these households do not pass additional toll gates. *Non-paying commuters* are defined as households where *all* household members had zero toll payments on their work routes in 2014. The trimmed sample leaves us with 146,782 households observed over a period of 7 years, resulting in a panel of 717,892 household $\times$ year observations. The sample is unbalanced as our unit of observation is households, and these may arise and dissolve over time.

Descriptives: Table 2 presents summary statistics for 2014 by city and commuter group, based on our estimation sample. Panel A illustrates the differences in EV ownership between paying and non-paying commuters, while Panel B compares toll rates faced by these groups. For both Bergen and the control cities, the observed differences are relatively similar.

Panel C provides summary statistics on various socioeconomic variables. In

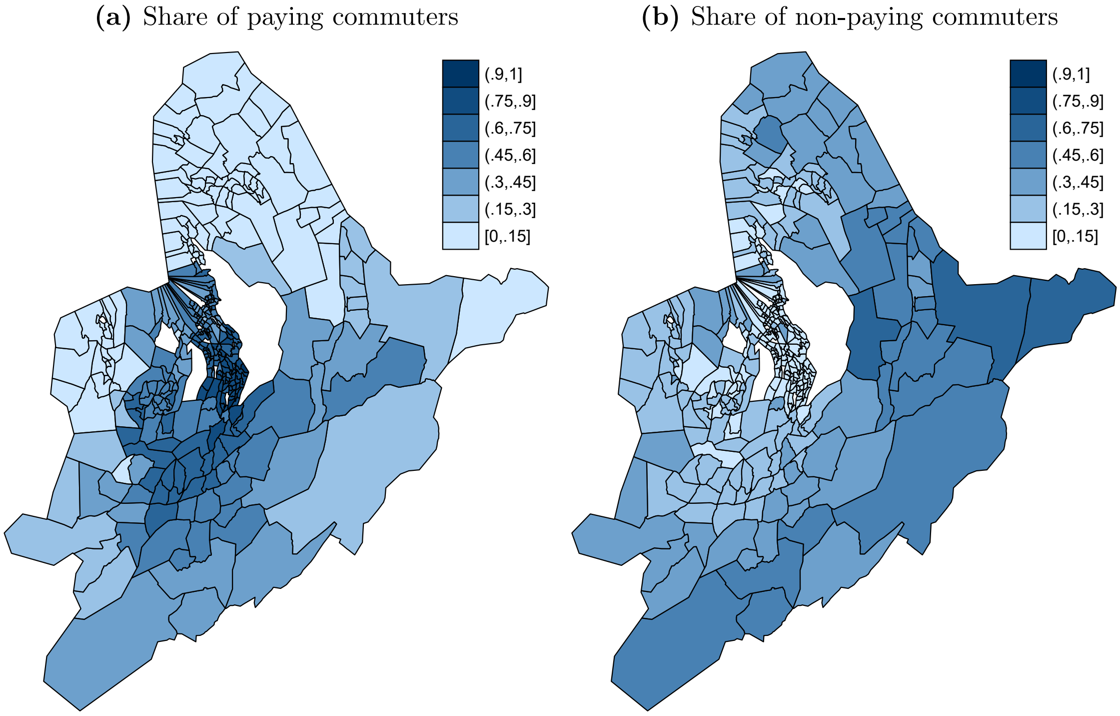
[11]For such short distances, households living and working on opposite sides of the toll cordon are not necessarily more intensively exposed than the control households. Given that we include neighborhood times year fixed effects, we would then compare treated and control households that live very close to the toll cordon, making them more likely to be similarly affected. In Appendix D we estimate a separate DiDiD regression for households with a work commute of less than 5 km and show that effects for all car-ownership outcomes are small and insignificant, as expected.

Table 2: Summary statistics by city and commuter group (2014)

	Bergen				Other cities			
	Paying		Non-paying		Paying		Non-paying	
	mean	sd	mean	sd	mean	sd	mean	sd
Panel A: Outcomes								
Electric vehicle (0/1)	0.047	0.212	0.026	0.160	0.035	0.184	0.018	0.132
Number of electric vehicles	0.049	0.221	0.027	0.167	0.036	0.191	0.018	0.138
Number of ICE vehicles	1.108	0.838	1.397	0.877	1.432	0.846	1.385	0.866
Total number of vehicles	1.157	0.858	1.424	0.883	1.468	0.852	1.403	0.871
Panel B: Journey to work variables								
Toll rate (NOK/individual)	30.38	12.76	0.00	0.00	21.11	9.52	0.00	0.00
Toll rate (NOK/household)	42.68	20.72	0.00	0.00	30.49	15.94	0.00	0.00
Driving distance (km)	12.57	7.55	14.00	8.81	14.65	8.15	12.07	7.32
Driving time (min)	13.51	8.39	14.86	9.58	14.75	8.01	12.66	7.68
PT time minus driving time (min)	36.57	23.04	64.63	50.73	52.52	33.34	69.96	74.06
PT time divided by driving time	3.41	0.99	5.32	2.98	4.46	2.06	6.45	6.02
Panel C: Socio-economic variables								
Couple (0/1)	0.68	0.47	0.66	0.47	0.74	0.44	0.66	0.47
Children living at home (0/1)	0.39	0.49	0.41	0.49	0.46	0.50	0.41	0.49
Persons in household	2.52	1.33	2.58	1.38	2.75	1.37	2.58	1.39
Age	42.71	12.25	43.71	12.62	42.92	11.79	43.15	12.42
Female (0/1)	0.49	0.29	0.48	0.30	0.48	0.26	0.48	0.30
Owns second home	0.11	0.32	0.10	0.31	0.11	0.31	0.11	0.31
Employed (0/1)	0.94	0.16	0.91	0.19	0.93	0.17	0.91	0.19
Retired (0/1)	0.05	0.19	0.06	0.21	0.04	0.17	0.05	0.19
Income (100,000 NOK/individual)	4.11	2.97	3.85	2.30	4.29	2.45	4.06	2.48
Income (100,000 NOK/household)	6.97	4.85	6.40	4.45	7.47	4.85	6.74	4.83
Wealth (mill NOK/individual)	1.45	5.18	1.20	3.51	1.31	2.92	1.34	4.22
Wealth (mill NOK/household)	2.49	8.12	2.03	6.01	2.32	5.47	2.28	7.99
Education:								
Unknown (0/1)	0.19	0.39	0.19	0.39	0.15	0.35	0.19	0.39
Less than high school (0/1)	0.07	0.25	0.12	0.33	0.10	0.30	0.12	0.32
High school (0/1)	0.21	0.41	0.35	0.48	0.31	0.46	0.33	0.47
University <4 years (0/1)	0.30	0.46	0.26	0.44	0.30	0.46	0.25	0.43
University >4 years (0/1)	0.23	0.42	0.08	0.27	0.14	0.35	0.11	0.31
Observations	23616		25538		60238		37390	

Notes: The table presents summary statistics for 2014, based on the estimation sample. "Paying" refers to paying commuters, while "Non-paying" refers to non-paying commuters. "ICE" denotes internal combustion engine vehicles, and "PT" represents public transit. All variables, except "children" and "number of household members," are individual-specific but averaged across spouses. In the empirical estimation, we control for the following variables: gender (female), employment status, retirement status, second home ownership, presence of children, education level, household size, two polynomials in age, income, wealth, commuting distance, and driving time to work. Additionally, we include two polynomials for absolute and relative commuting time differences between public transit and private car travel. If data for a given variable is missing for one spouse, we use the other spouse's value as a proxy for the household average. We allow coefficients for all variables to differ based on household composition (i.e., whether the household has one or two adult members). For regressions including household fixed effects, all time-invariant control variables will drop out. More detailed variable descriptions can be found in Appendix Table B.1.

Figure 3: Share of population in Bergen that is treated and non-treated (2014)



Notes: Figure shows the share of households that is classified as "paying" and "non-paying" commuters in Bergen in 2014, by neighborhood. The maps display neighborhoods located in Bergen Municipality, where Bergen city center is located in the middle of the maps where neighborhoods are the smallest.

general, paying commuters in Bergen and in the control cities exhibit similar demographic characteristics. However, paying commuters in the control cities are more likely to be part of two-adult households and have slightly higher incomes. Compared to non-paying commuters, paying commuters are generally more likely to be couples and tend to be wealthier with higher educational attainment.

Figure 3 displays the share of the population in each neighborhood of Bergen Municipality classified as paying (Panel a) or non-paying (Panel b) commuters. The maps illustrate that the proportion of paying commuters increases with proximity to the city center. As population density rises closer to the city center, neighborhoods also become progressively smaller. In the inner city center, more than 90% of residents are classified as paying commuters, whereas in the northern outskirts of Bergen, the share drops to below 15%. Among paying commuters, 56% commute *into* the city center, 14% commute *through* the toll cordon, and 30% reside in the city center and commute *out of* the cordon. In contrast, only 2% of non-paying commuters live inside the toll cordon.

The maps illustrate that comparing paying and non-paying commuters in Bergen over time without accounting for the differences in residence location will likely lead to biased estimates. In our empirical strategy, we absorb all (unobserved) time-varying variables across neighborhoods and only use within-neighborhood variation over time. Further, by including other cities as an additional control group, we are able to include year fixed effects that are specific to paying commuters. The maps hence show where the largest share of paying commuters is located, but do not reflect the variation used for identifying causal effects of the cordon congestion charge.

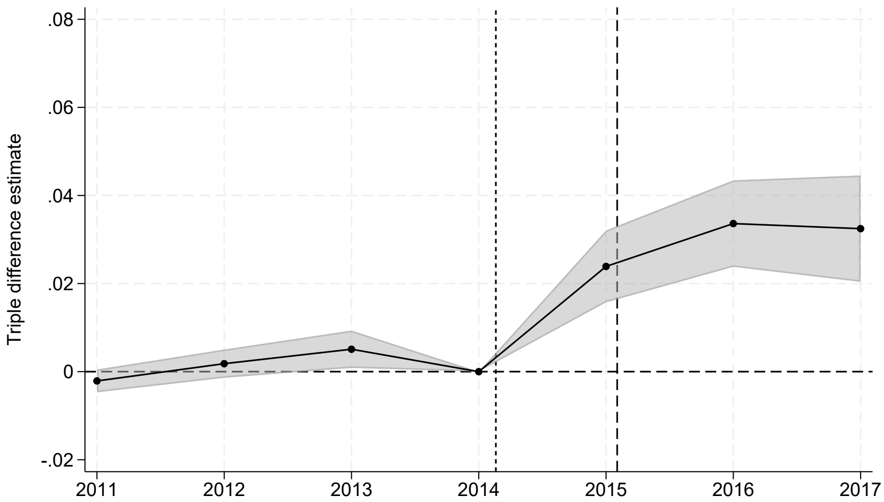
6 Results on car-ownership

Figure 4 presents the annual treatment effects estimated from the DiDiD specification in Equation 2. Panel (a) indicates that households exposed to the Bergen congestion charge were approximately 3 percentage points more likely to own an electric vehicle by the end of 2016 and 2017. We also find a positive and significant treatment effect on electric vehicle ownership at the end of 2015, suggesting that households responded to the policy announcement in February 2015. This anticipation effect is unsurprising given the durable nature of cars and the fact that acquisition decisions often account for future expectations. Examination of the pre-intervention period (2011-2014) shows estimated coefficients close to zero, supporting the validity of the parallel trends assumption.

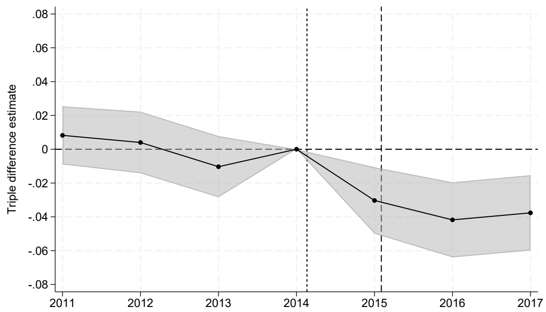
Panel (b) shows negative treatment effects on the number of ICE vehicles, which mirrors the observed positive effect on electric vehicle ownership. Panel (c) shows point estimates close to zero for the total number of cars owned by a household. This indicates that, on average, households replaced their conventional vehicles with electric ones.

Figure 4: DiDiD estimates on car ownership

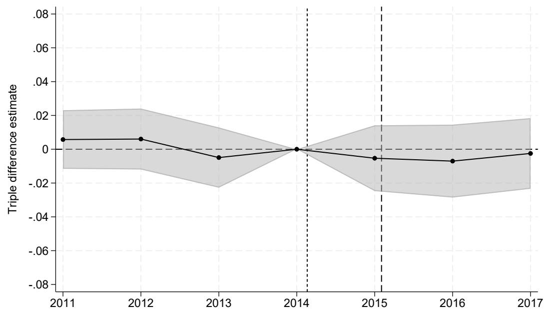
(a) Electric vehicle (0/1)



(b) Number of ICE vehicles



(c) Total number of vehicles



Notes: The figure plots coefficients β_t estimated from Equation 2, where β_{2014} is normalized to zero. Gray areas indicate 95% confidence intervals. Panel (a) shows the annual treatment effect on the probability of a household owning an electric vehicle. Panel (b) shows the annual treatment effect on the number of internal combustion engine (ICE) vehicles owned by a household. Panel (c) shows the annual treatment effect on the total number of vehicles owned by a household. All regressions include the following set of time-varying individual and household-level controls: employed, retired, second home, children, education level, number of persons registered at the household, income, and wealth. Standard errors are clustered at the 2014 neighborhood level.

Table 3 presents average treatment effects on car ownership based on Equation 1, excluding the announcement year (2015) from the estimations. The coefficient in column (1) indicates that the congestion charge led to an average increase of 2.9 percentage points in the probability of owning an EV. To put this effect into perspective, the congestion charge accounts for over 20% of the increase in EV ownership for the treatment group from 2014 to 2017.[12] The effect on the *number* of electric vehicles owned is slightly larger (0.031), reflecting that some households own more than one electric vehicle. The reduction in the number of ICE vehicles is

[12]This proportion is derived from the observed EV shares in 2014 and 2017 reported in column (1) of Table 3 and is illustrated in Appendix Figure C.2.

Table 3: DiDiD estimates on car ownership

	Probability	Number of vehicles		
Dependent variable:	Pr(BEV) (1)	BEV (2)	ICEV (3)	Total (4)
Post × Paying commuters × Bergen	0.0293*** (0.00494)	0.0308*** (0.00542)	-0.0326*** (0.00883)	-0.00179 (0.00840)
Observations	707952	707952	707952	707952
Mean depvar 2014 (paying commuters, Bergen)	0.0491	0.0506	1.1427	1.1933
Mean depvar 2017 (paying commuters, Bergen)	0.1800	0.1907	1.1099	1.3006
Paying commuter × year FE ($\alpha_t c_i$)	✓	✓	✓	✓
Household FE (η_i)	✓	✓	✓	✓
Household characteristics ($X'_{it}\gamma$)	✓	✓	✓	✓
Neighborhood × year FE (θ_{nt})	✓	✓	✓	✓

Notes: The table plots the coefficient β estimated from Equation 1. The dependent variable is indicated by the column heading. "BEV" refers to battery electric vehicles, "ICEV" refers to internal combustion engine vehicles, and "Total" refers to the total number of vehicles owned by the household. The sample is restricted to the years 2011-2017, where 2016-2017 denotes the "post" period and 2015 is excluded due to potential anticipation effects. Standard errors are clustered at the 2014 neighborhood level (1,786 clusters). * p<0.10, ** p<0.05, *** p<0.01.

0.033 cars (column 3), corresponding to a proportional effect of -2.9%.[13] We find a small and statistically insignificant decrease in total car ownership of -0.002 (column 4), which translates to a proportional effect of -0.15%.

A priori, one might expect that increased congestion charges for ICE vehicles would lead to a reduction in car ownership by a household. However, as different adaptation mechanisms pull in different directions, the net effect is an empirical question. Households might choose to i) dispose of their existing ICE vehicle, ii) replace their existing ICE vehicle with an EV, iii) or acquire an EV while retaining their existing car. As we will show in Section 6.2, we observe that some households indeed fall into the former category: the poorest quintile of households shows no significant change in EV adoption but a reduction in car ownership. Potential rationales for falling into the latter category – not getting rid of their existing car when acquiring an EV – include transaction costs, sentimental value, or "range anxiety" where the existing car is retained for longer trips that exceed the range of the EV.[14] The null effect of increased costs of driving ICE vehicles on car ownership is also consistent with concurrent literature (Halse et al., 2025).

In summary, the congestion charge led to a transition from conventional vehicles to electric vehicles while keeping the total number of cars owned relatively constant. In the following, we examine the robustness of these findings, as well as heterogeneous responses by household type and work-commute characteristics.

[13] This is calculated as the treatment effect (-0.0326) divided by the 2014 mean for paying commuters (1.1427).

[14] Findings in Johansen and Munk-Nielsen (2022) lend support to the "range anxiety" hypothesis.

6.1 Robustness

Our main findings are consistent across various robustness checks, including different sample restrictions and specifications. Below, we summarize the key findings focusing on EV ownership, while additional details on other car-ownership outcomes are provided in Appendix D.

Sample restrictions: In our main estimation, we drop households with a short work commute (below 5 km). As discussed in Section 5, our empirical strategy relies on driving being a relevant option on the work commute. To test the sensitivity of our main results, we estimate effects for samples with a 4, 3, and 2 km cut-off instead. Including shorter work commutes monotonically lowers the effect on EV ownership, from 2.9 pp (5 km) to 2.3 pp (2 km); see Appendix Table D.1. The lower effect could partly be explained by the intent-to-treat interpretation of our estimate; households with a short work commute are less likely to actually pay congestion charges.[15] By splitting the sample according to the area of residence (see Appendix Table D.3), we see that the results are mainly capturing effects for households living outside the city center – excluding paying and non-paying commuters living *inside* the toll cordon gives an estimate that is similar to, and insignificantly different from, our main estimate. The estimated effect for households living inside the toll cordon is zero.[16] As expected, the estimate for pass-through commuters is lower (2.2 pp) compared to the full sample (2.9 pp). Excluding pass-through commuters gives a slightly higher estimate (3.2 pp). This is further discussed in relation to circumvention behavior in Section 7.3.

Fixed effects and controls: In Appendix Table D.4 we show that our main results are relatively stable across different fixed effects specifications and demographic controls, as long as neighborhood$\times$year fixed effects are taken into account (columns 3-5). The importance of accounting for neighborhood trends comes as no surprise as the supply of electric vehicles is increasing over time, and the adoption rate is likely to be affected by several local aspects of residential neighborhoods such as access to parking and charging stations, accessibility, travel demand, etc. Accounting for differential trends across neighborhoods is therefore crucial. When including neighborhood$\times$year fixed effects, adding household-level and work commute con-

[15] If we only focus on households with a work commute of *less than 5 km* we find small and insignificant effects on car-ownership outcomes - as expected (see Appendix Table D.2). In the heterogeneity analysis we show that treatment effects are relatively similar across work distances above 5 km (Figure 5, Panel (e)).

[16] This estimate is also less precise due to the sample composition: 30 percent of paying commuters, but only 2 percent of non-paying commuters in Bergen live inside the toll cordon.

trols has little impact on the estimated treatment effect. Including household fixed effects only marginally lowers the treatment effect, from 2.99 pp to 2.93 pp. Lastly, to account for potentially differential effects of fuel prices across work commutes, we interact the neighborhood-specific trends with the household-level work commute distance in 2014 (Appendix Table D.5). These neighborhood-year-specific "work distance slopes" will absorb any local trends that potentially affect neighborhoods differentially by virtue of the length of the commute (e.g., local variation in fuel prices, road congestion, etc.). The estimated effect on EV ownership is slightly smaller, but this could also be explained by the local trends absorbing parts of the (true) treatment effect.

6.2 Heterogeneous effects

The estimated average treatment effects may mask heterogeneity. In the following, we examine how the treatment effects of the congestion charge vary across different socioeconomic dimensions. Key results are presented in Figure 5, while supporting results are available in Appendix E.

Income: Allowing the treatment effect to vary by household income reveals a clear income gradient in electric vehicle adoption. Panel (a) in Figure 5 illustrates that households in the highest quintile are 4.8 percentage points more likely to adopt an electric vehicle in response to the policy. In contrast, the likelihood of electric vehicle adoption for households in the lowest income quintile is nearly zero. This suggests that higher-income households are more responsive to the policy. Further analysis in Appendix E shows that the lowest income quintile is the only group where the effect on total car ownership is significantly different from zero and negative, with a reduction of approximately 0.04 cars, or 5.3%. This indicates that while wealthier households predominantly replaced their conventional vehicles with electric ones, poorer households reduced their car ownership.

The observed income gradient in car ownership responses could have various explanations. First, *treatment intensity* may differ across income groups. Specifically, a lower fraction of lower-income households commutes by car compared to higher-income households. According to a Norwegian travel survey, less than 50% of the lowest income quintile commutes by car, whereas 57–65% of higher income quintiles do (Grue et al., 2021, Table 7.5, p. 55). This suggests that treatment intensity is a relevant factor, but that it is not likely to fully explain the income gradient. The negative effect on ICE vehicle ownership for the lowest income quintile indicates that these households are indeed impacted by the policy but adjust along other margins.

Second, high-income households might have a stronger *preference* for purchasing an electric vehicle in response to the policy, whereas low-income households might prefer to switch to alternative modes of transportation, such as public transit or cycling. This preference disparity could reflect differences in the utility of electric vehicles, time value, or preferences for other transportation modes.

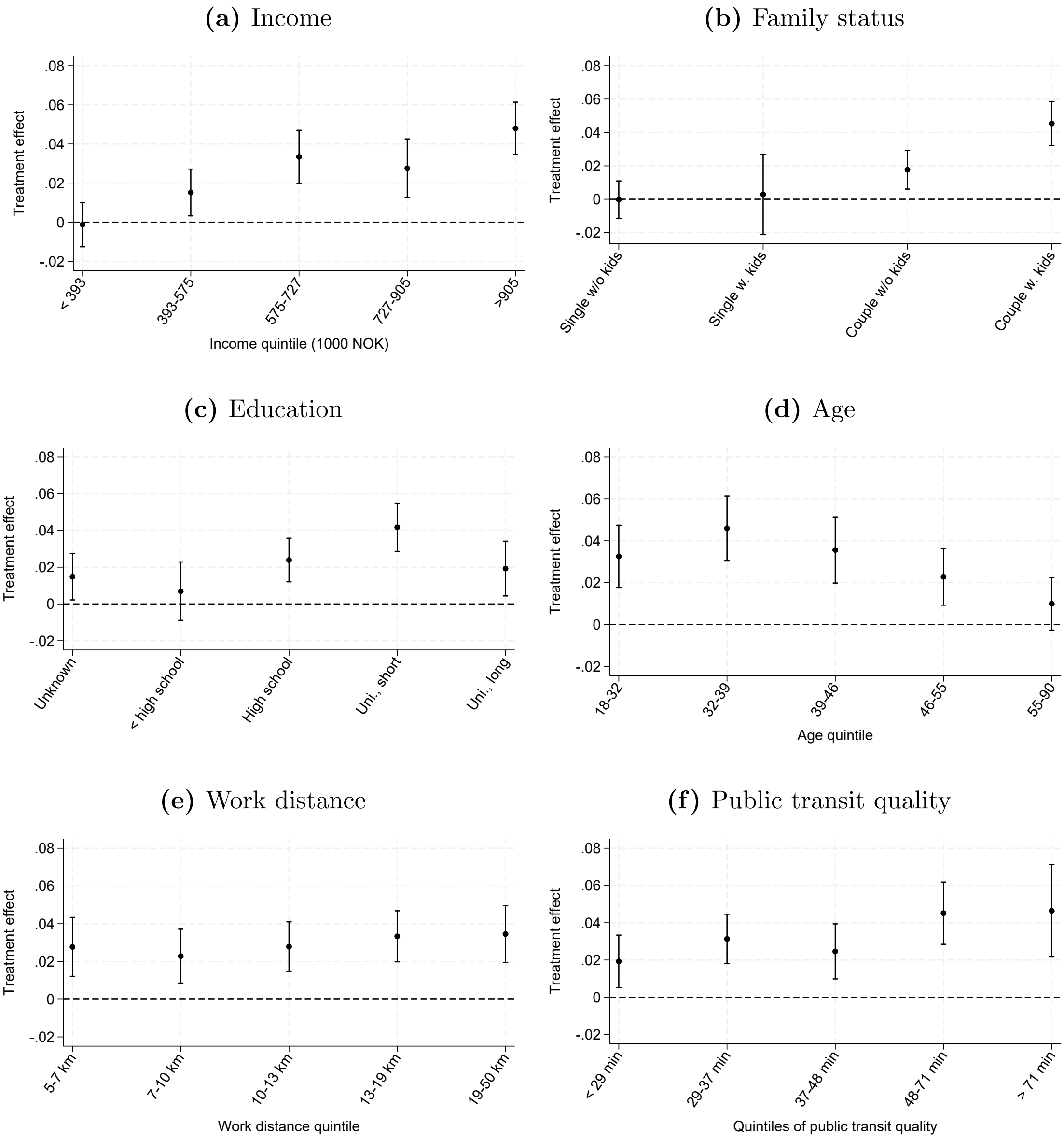
Third, *financial constraints* might limit the ability of low-income households to purchase electric vehicles. During the study period, acquiring an electric vehicle typically meant buying a new car, as the market for used electric vehicles was negligible. Although electric vehicles were cheaper than comparable conventional cars due to various tax exemptions (see Appendix Table A.3), low-income households are generally less likely to purchase new cars compared to high-income households (Fevang et al., 2021). Even if an electric vehicle could be financially advantageous in the long run (see Appendix Table A.4), the immediate financial outlay might act as a barrier for lower-income households.

Household type, education, and age: Couples with children show a significantly higher probability of adopting an electric vehicle in response to the policy (Panel b). This suggests that households with children may prioritize the benefits of EVs more highly, perhaps due to the need for frequent, reliable transportation for family activities. There may also be economies of scale that make it more cost-efficient for these households to invest in an electric vehicle. Further, the likelihood of adopting an electric vehicle increases with educational attainment, peaking at households with up to 4 years of university education (Panel c).[17] This trend may reflect a greater awareness of the benefits of EVs or a higher capacity to navigate the adoption process among more educated households. Previous literature also suggests that individuals tend to "undervalue" future fuel savings when purchasing a vehicle (see e.g., Allcott and Wozny, 2014), a tendency that may diminish with higher levels of education. Lastly, we find that EV adoption is highest among households in their 30s and lowest among those in the top age quintile. The higher adoption rate among younger households may be driven by a combination of factors, including financial stability, openness to new technology, and greater transportation needs.

Since education and age co-vary with income, the observed heterogeneous patterns likely reflect a combined effect of income and the demographic factors in question, along with other correlated variables. In an attempt to disentangle the income channel from other mechanisms, we estimate heterogeneous effects separately for

[17] This group includes individuals with a Bachelor degree (or equivalent) as well as those with 1 to 4 years of undergraduate credits irrespective of receiving a degree or not. More than 4 years include those with a Masters degree or higher (Ph.D.).

Figure 5: Heterogeneous DiDiD: electric vehicle ownership



Notes: The figure plots the coefficients β_k estimated from Equation 3, where k refers to group (e.g., income quintile). Each panel (a-f) plots coefficients estimates from a separate regression. Whiskers indicate 95 % confidence intervals. The dependent variable is a dummy variable equal to 1 if the household owns an electric vehicle in year t and 0 otherwise. Groups are based on 2014 demographics. "Income" is summed over spouses, "education" is the maximum value in each household, and "age", "work distance" and "public transit quality" are averaged over spouses. Public transit quality is defined as time to work by public transit minus time to work by car in minutes. "Uni, long" is more than 4 years of higher education, and "Uni., short" is 1-4 years. The sample is restricted to the years 2011-2017, where 2016-2017 denotes the "post" period and 2015 is excluded due to potential anticipation effects. Standard errors are clustered at the 2014 neighborhood level. See Appendix Table E.1 for coefficients in table format.

the top and bottom income quintile (see Appendix Figure E.1). The findings reveal that households in the bottom income quintile are unresponsive to the policy, regardless of educational attainment or age. In contrast, for high-income households, who are more likely to afford a new electric vehicle, treatment effects increase with educational attainment and decrease with age. These patterns suggest that age and education influence EV adoption beyond the income effect.

Public transit and commute distance: Households' adaptation decisions may also depend on the quality of transportation substitutes. Here, we proxy public transit quality by the *additional* time required to commute by public transit compared to driving a private car. We find that households with the poorest public transit connectivity ($>$ 71 minutes) are more than twice as likely to adopt an EV compared to those in the top quintile of public transit connectivity ($<$ 29 minutes) (Panel f). However, these estimated treatment effects are not statistically different from one another. With this caveat in mind, our results suggest that poor public transit options may drive households to seek alternative, reliable modes of transportation, such as EVs. Additionally, there is a slight tendency for EV adoption to increase with driving distance to work (Panel e), although differences across quintiles are relatively small and statistically insignificant. This implies that commute distance likely plays a minor role in the decision to adopt an EV.

Again, commuting distance and public transit quality may co-vary with income. In Appendix Figure E.1, we show that households in the lowest income quintile are unresponsive to public transit quality and work distance. In contrast, high-income households with the poorest public transit quality are more likely to purchase an EV compared to similarly wealthy households with the best public transit options. For work distance, the difference between the high- and low-income households is most pronounced for those with the longest work commute.

These findings highlight the varied factors influencing household decisions to adopt electric vehicles in response to congestion charges. While suggestive, our findings add to the evidence base suggesting that low-income households are to a larger extent locked into existing behavioral patterns.

7 Other adjustment margins and outcomes

Our results so far indicate that, on average, the time-varying congestion charge increased EV adoption while leaving household car ownership levels unchanged. However, drivers of combustion engine vehicles have several alternative ways to adapt beyond paying the additional charge or acquiring an EV. Here, we examine various adjustment mechanisms as well as their implications for interpreting our main car ownership effects.

First, households can adjust on the intensive margin by reducing their driving

(Section 7.2), for example, by commuting to the city center via alternative modes of transportation. This may lead to an extensive margin response, such as disposing of an existing combustion vehicle. Second, they can shift their departure times to avoid higher-priced time intervals (Section 7.2). Third, pass-through drivers can re-route to bypass the toll cordon entirely, albeit at the cost of a longer trip (Section 7.3). Finally, individuals may relocate or change workplaces to avoid the toll charges (Section 7.1).

If the congestion charge could be easily avoided through some of these mechanisms, we would expect a smaller effect on EV adoption. Lastly, we analyze the policy's impact on air pollution concentrations (Section 7.4). Together with our main analysis on car ownership and the examination of other adjustment margins, this provides a more comprehensive understanding of the overall effects of the congestion charge.

7.1 Moving and job change

In our main analysis, we define treatment and control groups based on residential and workplace locations in 2014. This approach ensures that policy-induced sorting, such as changing residence or workplace to avoid the congestion charge, does not affect treatment status. Neighborhood fixed effects are also based on 2014 residential locations to prevent right-hand side variables from being influenced by the congestion charge. Consequently, any subsequent household responses – such as relocation to avoid the congestion charge and the resulting changes in car ownership – are considered part of the policy's overall impact.

Here, we explicitly test for moving and job change as adaptation mechanisms by using these behaviors as outcome variables. Panel A of Table 4 presents DiDiD estimates for the probability of moving (column 1), moving into the toll cordon (column 2), and moving out of the toll cordon (column 3). While we find no significant effect on the overall probability of moving, treated households were 0.4 percentage points less likely to move into the toll cordon and 0.3 percentage points more likely to move out. These effects correspond to a 31% decrease and a 25% increase, respectively, relative to the 2014 mean.

When examining job changes instead of residential moves (Panel B), we observe a similar pattern: treated households were 1.6 percentage points less likely to move into the toll cordon and 0.7 percentage points less likely to move out. These effects correspond to a 19% decrease and a 16% increase, respectively, relative to the pre-treatment mean.

Overall, these findings suggest a statistically significant and non-trivial sorting

Table 4: DiDiD estimates on moving and job change

Dependent variable:	Any-where (1)	Into toll cordon (2)	Out of toll cordon (3)
Panel A: Moving			
Post × Paying commuters × Bergen	-0.00326	-0.00422***	0.00275**
	(0.00371)	(0.00113)	(0.00121)
Observations	707952	707952	707952
Mean depvar 2014 (paying commuters, Bergen)	0.0834	0.0137	0.0108
Panel B: Job change			
Post × Paying commuters × Bergen	0.00835*	-0.0158***	0.00714***
	(0.00435)	(0.00265)	(0.00226)
Observations	707952	707952	707952
Mean depvar 2014 (paying commuters, Bergen)	0.2661	0.0837	0.0527
Paying commuter × year FE ($\alpha_t c_i$)	✓	✓	✓
Household FE (η_i)	✓	✓	✓
Household characteristics ($X'_{it}\gamma$)	✓	✓	✓
Neighborhood × year FE (θ_{nt})	✓	✓	✓

Notes: This table reports the coefficient β estimated from Equation 1. The dependent variables indicate changes in a household's residential location (Panel A) and changes in the work location of *either* household members (Panel B). In column 1, the dependent variable equals one if the location differs from the previous year and zero otherwise. In column 2 (3), the dependent variable equals one if the new location is inside (outside) the toll cordon, and the previous year's location was outside (inside) the toll cordon. The sample is restricted to the years 2011-2017, where 2016-2017 denotes the "post" period and 2015 is excluded due to potential anticipation effects. Standard errors are clustered at the neighborhood level (1,786 clusters). * p<0.10, ** p<0.05, *** p<0.01.

response induced by the policy change, with paying commuters more likely to relocate out of the city center. This relocation likely results in a lower estimated EV adoption effect than if work commutes had remained unchanged since 2014, as relocated households have weaker incentives to acquire an EV after moving. However, as the share of households relocating in response to the congestion charge is small compared to the share that acquired an electric vehicle, the impact of relocation on the car ownership estimates is minimal.[18]

7.2 Driving into the city center

To what extent did drivers respond on the intensive margin by reducing driving to the city center, or by changing their departure time?

To examine these questions, we move from individual-level registry data to high-frequency sensor-level traffic data. Specifically, we collect restricted data on the number of cars passing the city-center toll cordon every 15 minutes for Bergen, Sta-

[18]Re-estimating the effect on EV ownership using a subsample that excludes households that moved or changed work locations after 2014 yields a slightly higher treatment effect (3.14 percentage points) compared to our main estimate (2.93 percentage points), aligning with expectations. However, these results should be interpreted with caution, as the sample selection is conditioned on behavior that is endogenous to the policy.

vanger, Haugesund, and Kristiansand from a regional road toll company (Ferde).[19] The data cover the period 2014 to 2018. We focus on a sample of two years before and after February 1, 2016. We aggregate the sensor-level data to the city level and measure the total number of cars passing any toll gate in the direction of the city center. Appendix F provides additional data details and descriptives.

To identify a causal effect of the policy change on driving into the toll cordon, we employ a differences-in-differences (DiD) framework where we compare traffic volumes across Bergen and the three other cities before and after February 1, 2016. When $Bergen$ is a dummy variable equal to 1 and $post_t$ is a dummy variable equal to 1 after February 1, 2016, the DiD estimator can be written as:

$$y_{ist} = \beta Bergen \times post_t + X'_{ist}\gamma + \sigma_s + \lambda_{ywd} + \theta_{dis} + \varepsilon_{ist}, \quad (4)$$

where y_{ist} denotes an outcome (traffic, air pollution) observed at time interval i (15 minutes, hourly) at time t (date) measured at location s (cordon, station, sensor). While traffic is measured at 15-minute intervals and aggregated to the cordon level, air pollution (Section 7.4) is measured at hourly intervals at the station level. X'_{ist} is a vector of location-specific weather controls, σ_s are location fixed effects, λ_{ywd} denotes year×week number×day-of-week fixed effects, θ_{dis} denotes location-specific day-of-week×time-of-day fixed effects, and ε_{ist} is the idiosyncratic error term. The DiD estimate is captured by the coefficient β. The key identifying assumption is that changes in omitted time-varying variables, such as unobserved technological trends, economic activity, and policies, affect Bergen and our control cities in a similar way. Appendix F.2 provides additional details on the empirical strategy.

Figure 6 shows DiD results for 15-minute time intervals (Panel a), as well as yearly DiD estimates for rush hours (Panel b). We see a clear decrease in traffic during rush hours, as well as some inter-temporal substitution towards the 15-30 minutes before and after rush hours. In the remaining hours of the day, the effect of the congestion charge on traffic is close to zero, despite a decrease in toll rates. The latter finding suggests that drivers are relatively insensitive to lower rates during non-rush hours. Overall, we find a daily traffic reduction of 5.5% (Appendix Table F.1, Panel A).[20] This corresponds to a reduction of 8,160 cars per day. The traffic reduction is primarily caused by fewer passenger vehicles rather than trucks (Appendix Figure F.3). Figure 6, Panel (b) shows that rush hour traffic was sharply

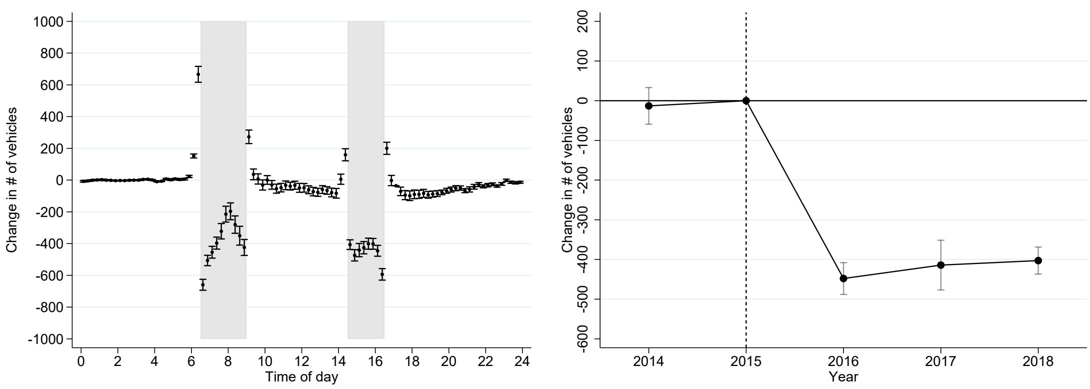
[19] The data is split by passenger vehicles (vehicles that weigh less than 3.5 tonnes), and trucks (vehicles weighing 3.5 tonnes or more).

[20] If there had been a zero increase in the number of cars during the 30 minutes before and after rush hours, we would have seen a 6.5 % daily decrease instead of the actual 5.5 % decrease.

Figure 6: DiD estimates on driving into the city center

(a) DiD effects for 15 min intervals

(b) Yearly DiD effects for rush hours



Notes: Panel (a) plots treatment effects estimated from Equation 4, where regressions are run separately for each 15 minute increment. Gray shaded areas indicate rush hours. Traffic is measured as the total number of cars passing the toll cordon every 15 minutes. Sample period is restricted to 2 years before and 2 years after policy implementation (Feb 1 2016), and to weekdays. Panel (b) plots the coefficients β_t estimated from the following regression: $y_{ist} = \sum_{t=-2}^{3}[\beta_t Bergen \times year_t] + X'_{ist}\gamma + \sigma_s + \lambda_{ywd} + \theta_{dis} + \varepsilon_{ist}$, where the sample is restricted to weekdays and rush hours. Figure F.4 shows effects for non-rush hours and all day traffic. Standard errors are clustered on week in both regressions.

reduced in the year of the intervention and remained low in the following two years.

These findings suggest that the policy worked as intended, by lowering driving and shifting traffic towards non-rush hours. The negative effects on traffic are robust across various fixed effects specifications (Appendix Figure F.5).

7.3 Driving around the toll cordon

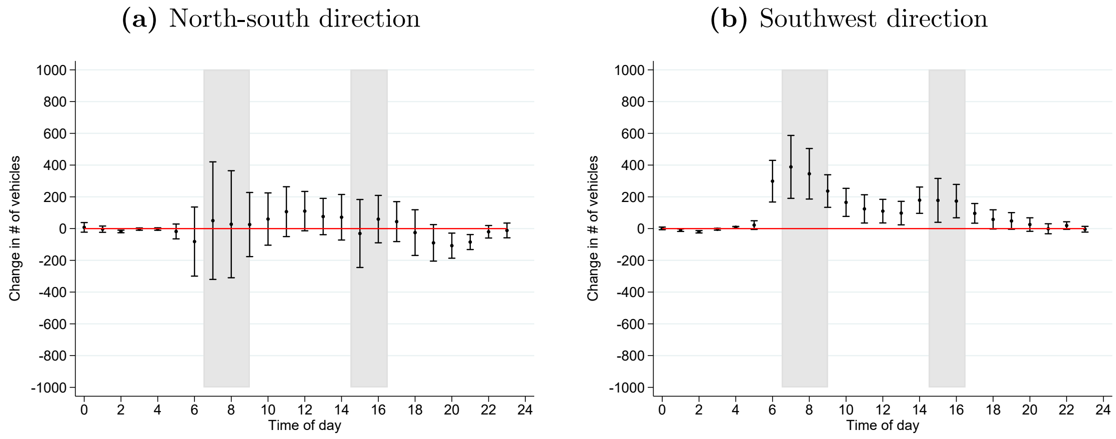
Cars entering downtown Bergen cannot avoid toll charges. However, cars passing through Bergen can opt for longer alternative routes that bypass the toll cordon entirely. For transit traffic, there are two main routes to bypass the toll gates, depending on the direction of travel (Appendix Figure F.6). Vehicles traveling through Bergen in the north-south direction can take a detour that extends travel time by around six minutes. Meanwhile, those traveling in the southwest direction can avoid the toll cordon by taking either a 9-minute detour (for cars arriving from the southwest) or a 1-minute detour (for cars arriving from the southeast).[21]

To examine spatial spillovers, we collect publicly available hourly vehicle count data from traffic sensors located outside the Bergen toll cordon. Our analysis focuses on sensors located along the two routes where drivers can bypass the toll cordon, provided they do not need to enter the city center.[22] Drivers circumventing the toll

[21] Detailed descriptions of these routes are provided in a report by the Norwegian Public Roads Administration (NPRA, 2018).

[22] Two sensors along these detour routes have pre-treatment data, allowing us to analyze changes

Figure 7: DiD estimates on driving around the city center



Notes: Figure shows DiD estimates on hourly traffic measured by sensors located along alternative detour routes around the toll cordon. Coefficients are treatment effects estimated from Equation 4, where β is allowed to vary by hourly increments. Whiskers indicate 95% confidence intervals. Gray shaded areas indicate rush hours. Traffic is measured as the total number of cars registered by the traffic censors in both directions each hour. Standard errors are clustered at the week level.

cordon will need to pass at least one of these sensors, depending on their chosen route. Note that this traffic includes *all* drivers passing through Bergen, not just local traffic from paying and non-paying commuters.

Figure 7 presents hourly DiD estimates of the congestion charge's impact on traffic along detour routes, derived from Equation 4. For the north-south direction, we find no evidence of spatial spillovers during rush hours (Panel a). However, for the southwest direction, we observe clear spatial spillovers (Panel b), with the largest treatment effects occurring during morning rush hours. We estimate that approximately 2,700 vehicles per day and 1,714 vehicles during rush hours used detour routes instead of entering the toll cordon (Appendix Table F.2).

These findings suggest that around one-third of the reduction in daily traffic into the city center may be attributable to drivers circumventing the toll cordon. This substitution pattern does not necessarily represent an undesirable effect of the policy, as it redirects traffic away from congested roads to areas where it likely has a smaller impact on overall traffic flows. However, the installation of new toll gates along the detour routes in 2019 suggests that such circumvention was unintended.[23] Additionally, the longer detour routes imply higher emissions.

These traffic spillovers could bias our EV estimates if redirected traffic disproportionately affects the work commutes of non-paying commuters. If the increased

over time. We use data from the sensors "Kråkenes" and "Indre Arna EV16", which can be accessed at: https://www.vegvesen.no/trafikkdata/start/. Data is available from September 2015 for the north-south direction and from September 2014 for the southwest direction.

[23] In April 2019, 15 new toll gates were installed along the alternative routes shown in Appendix Figure F.6, effectively eliminated the possibility of bypassing the toll cordon.

traffic makes non-paying commuters less likely to own a car, this would introduce an upward bias in our EV adoption estimate. However, we consider such spillover effects on car ownership to be minimal. Congestion is generally less severe outside the city center, reducing the likelihood of a significant impact. Moreover, since we compare households within the same neighborhood, treated and control units are less likely to be differentially affected by these spillovers.

However, the ability to circumvent the toll cordon likely reduces the incentive for paying commuters to switch to an EV. Based on the administrative data, we find that approximately 56% of paying commuters in Bergen had a work commute requiring entry into the toll cordon, 30% required exit through the toll cordon, and 14% could avoid it entirely, as neither their residence nor workplace was located inside. Estimating EV ownership effects separately for paying commuters who can bypass the toll cordon, we find a slightly lower treatment effect of 2.2 percentage points (Appendix Table D.3, column 5). When excluding pass-through commuters from the main specification, the treatment effect increases to 3.2 percentage points (Appendix Table D.3, column 4). While these differences are not statistically significant, the point estimates suggest that the effect for transit commuters is only two-thirds that of other commuters. A likely explanation is that some commuters with the option choose to drive around the city center in a combustion vehicle rather than acquiring an EV.

In summary, this section provides evidence of circumvention behavior, which appears to have dampened the policy's effect on EV ownership for transit commuters, as expected. Since the impact of congestion charging on EV adoption depends on how easily the cordon can be bypassed, findings should always be interpreted within their specific local context.

7.4 Air pollution

To examine the effects of the policy on ambient air quality, we collect hourly atmospheric pollution data for the period 2014-2018 from the Norwegian Institute for Air Research (NILU). Our sample includes 14 municipalities across Norway and 15 monitoring stations, all of which are located near roads.[24] Air pollution is measured in micrograms per cubic meter of air (μg/m^3). We focus on pollutants strongly influenced by traffic: nitrogen dioxides (NO_2) and particulate matter (PM_{10}).[25] We

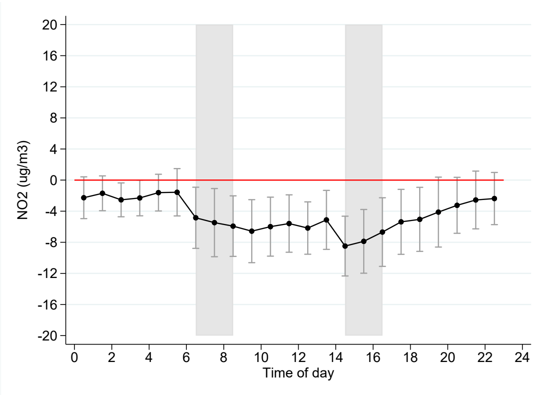
[24] The municipalities include Bergen, Stavanger, Kristiansand, Bærum, Drammen, Fredrikstad, Gjøvik, Grenland, Hamar, Lillehammer, Moss, Sarpsborg, Skedsmo, and Tromsø.

[25] The primary source of NO_2 in Norway is exhaust emissions from internal combustion engine vehicles. In contrast, the main sources of PM_{10} in urban areas include road wear, tire and brake abrasion, sand used to increase friction on icy roads during winter, and emissions from wood-fired

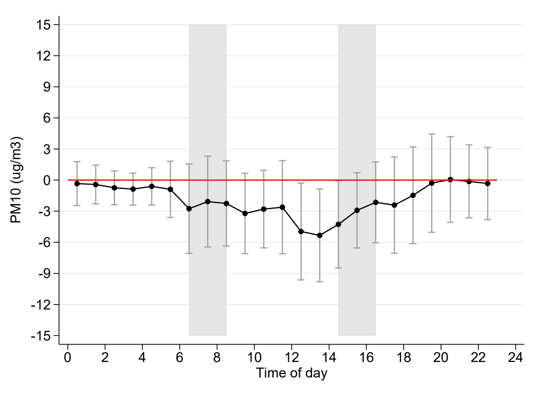
also collect monitor-level weather data from the Norwegian Meteorological Institute for 2014-2018. See Appendix F.1 for further details on the data and descriptive statistics.

Figure 8: DiD estimates on NO_2 and PM_{10}

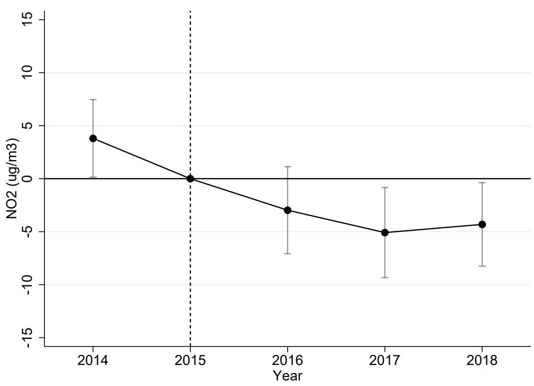
(a) NO_2: 60 min. intervals



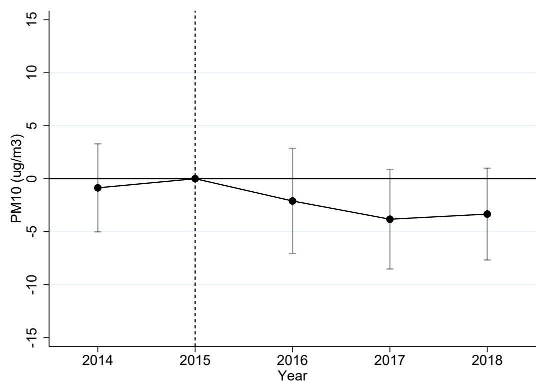
(b) PM_{10}: 60 min. intervals



(c) Yearly rush hour NO_2



(d) Yearly rush hour PM_{10}



Notes: Panels (a) and (b) plot treatment effects estimated from Equation 4, where regressions are run separately for each 60 minute increment. Sample period is restricted to 2 years before and 2 years after policy implementation (Feb 1 2016), and to weekdays. Gray shaded areas indicate rush hours. Pollution is measured as micrograms per cubic meter of air (μg/m^3). Panels (c) and (d) plot the coefficient β_t estimated from the following regression: $y_{ist} = \sum_{t=-2}^{3}[\beta_t Bergen \times year_t] + X'_{ist}\gamma + \sigma_s + \lambda_{ywd} + \theta_{dis} + \varepsilon_{ist}$, where the sample is restricted to weekdays and rush hours. Period is restricted to 2 years before and 3 years after policy implementation Standard errors are two-way clustered on week and station in all regressions.

To estimate the causal effect of the congestion charge on air pollution, we apply Equation 4 using hourly air pollution concentrations as the outcome variable. Since we compare air pollution levels within the toll cordon across treated and control cities, our estimated treatment effect will not be biased by the documented traffic spillovers to areas outside the cordon. However, because monitors are only located inside the city, we are unable to measure potential increases in pollution outside the cordon.

ovens (NILU, 2019).

Figure 8, Panel (a) presents hourly DiD estimates for NO_2. The results indicate that the congestion charge significantly reduced NO_2 concentrations during most hours between 6 AM and 7 PM. Since air pollutants persist in the air after their release, we do not expect to see sharp differences between rush hours and non-rush hours. On average, the congestion charge reduced NO_2 concentrations by 4.1 μg/m^3 per day, representing a 9.5% decrease (Appendix Table F.3, Panel A). During rush hours, the reduction was 6.4 μg/m^3, corresponding to an 11% decrease.

Figure 8, Panel (c) presents yearly DiD estimates for rush hour NO_2 pollution, and suggests the presence of a pre-trend. The DiD effect on NO_2 should therefore be interpreted with caution. This pre-trend may partly reflect the early shift to EVs, which began following the policy announcement in February 2015.

For PM_{10}, we observe a treatment effect of similar magnitude to that of NO_2; however, the estimates are noisier and statistically significant only for a few hours during the day (Figure 8, Panel b). Unlike NO_2, there is no indication of a pre-trend (Figure 8, Panel d). Since PM_{10} is not generated from exhaust emissions, a shift toward EVs is expected to have a smaller impact on this pollutant. While we estimate an average daily reduction of approximately 10.5% (Appendix Table F.3, Panel B), the imprecision of the estimate makes it difficult to draw firm conclusions. The findings on air pollution remain relatively robust across different fixed effects specifications and weather controls (Appendix Figure F.8).

7.5 Trade-offs with multiple policy goals

Congestion charges are typically designed to reduce traffic volumes and air pollution in inner-city areas during rush hours, addressing both congestion and emission externalities. However, congestion pricing schemes that exempt EVs may be less effective at reducing congestion. While we find that the Bergen congestion charge significantly reduced traffic during rush hours, the decline would likely have been greater had EVs also been subject to the charge. At the same time, a shift toward EVs may lower emissions both within the cordon and in neighboring areas, especially if the transition from ICE vehicles to EVs affects not only work commutes but also leisure trips. Thus, replacing ICE vehicles with EVs may provide additional benefits by reducing emissions beyond the cordon.

As EVs constitute an increasing share of the vehicle fleet, the impact of road tolls on traffic volumes is expected to weaken. Exemptions for EVs will also reduce toll revenues as the fleet becomes increasingly electric, necessitating an increase in other taxes. Consequently, optimal tolling rates for EVs will need to be adjusted over time. This consideration is also reflected in Norway's electrification strategy, where EV incentives are gradually being scaled back.[26]

8 Conclusion

Electric vehicles are often highlighted as a key technology to decarbonize private transportation and reduce urban air pollution. This paper demonstrates that differentiating driving costs by vehicle type and time of day can shift the composition of the vehicle fleet towards electric vehicles while maintaining a constant fleet size. By exploiting a policy that combined rush hour cordon charging with EV exemptions, we find that households exposed to the policy were nearly 3 percentage points more likely to adopt an EV. This effect accounts for over one-fifth of the observed increase in EV ownership during the study period.

High-income households responded more strongly to the EV incentives, with responses varying substantially across education, age, and public transit quality. In contrast, low-income households were unlikely to adopt an EV, regardless of these factors. Instead, they responded by reducing car ownership. These findings suggest that financial constraints may drive differential car ownership effects across income groups, though differences in preferences or treatment intensity could also contribute. The strong income gradient in EV adoption must also be considered in light of the negligible second-hand market for EVs at the time.

We also document that households responded to the policy through various adjustments – both intended and unintended – including changing their place of residence and workplace, shifting departure times, reducing trips to the city center, and rerouting around the toll cordon. While some of these behaviors counteracted the policy's objectives, we find an overall negative effect on traffic levels, air pollution, and fossil-fuel vehicle ownership.

As the share of EVs continues to grow in Norway and globally, the benefits of a cleaner vehicle fleet must be weighed against its impact on traffic congestion and toll revenue losses. While optimal tolling rates for EVs will need to adapt over time, we leave the quantification of these rates across time and vehicle types to future research. Nonetheless, our findings offer novel insights into household car ownership decisions and other behavioral adjustments that could inform such calculations.

[26]Starting in 2018, local governments were permitted to charge EVs up to 50% of the standard toll rate. In Bergen, EVs remained exempt until April 2019, after which they were charged approximately one-third of the rate applied to conventional passenger cars (Sand et al., 2022).

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Online supporting material

Congestion pricing with electric vehicle exemptions: Car-ownership effects and other behavioral adjustments

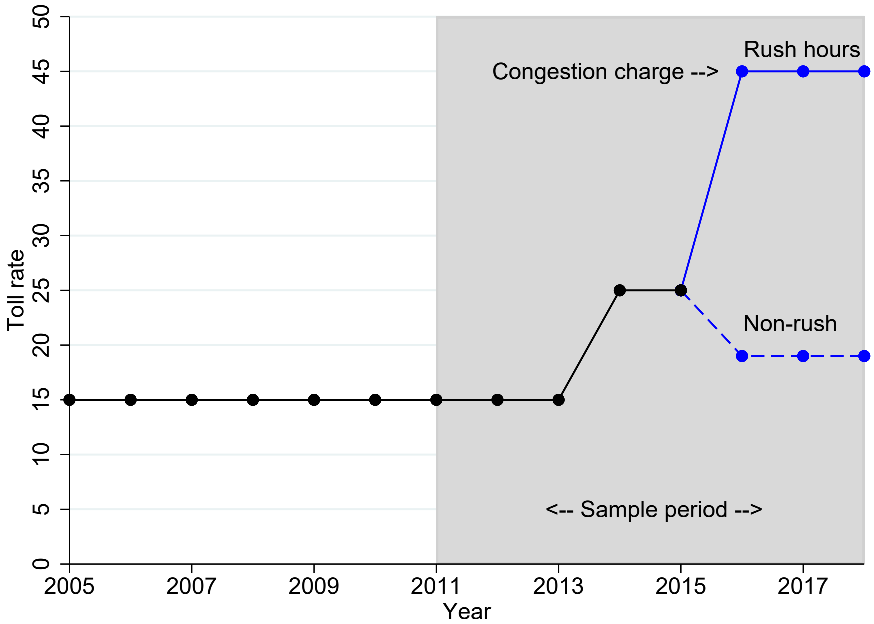
Authors: Elisabeth T. Isaksen and Bjørn G. Johanssen

Replication files: Isaksen and Johansen (2025).

Contents

Appendix A Background

Figure A.1: Toll rates in Bergen, 2005-2018



Notes: The figure illustrates the development of toll rates in Bergen from 2005 to the end of 2017. The time-varying congestion charge was introduced on February 1, 2016.

Table A.1: Cordon-based congestion pricing in Norway. 2013-2019

	Kristiansand	Trondheim	Bergen	Oslo	Stavanger
Date implemented	Nov 19, 2013	Mar 10, 2014	Feb 1, 2016	Nov 1, 2017	Oct 1, 2018
Morning rush	6:30-9:00	7:00-9:00	6:30-9:00	6:30-9:00	07:00-09:00
Afternoon rush	14:30-17:00	15:00-17:00	14:30-16:30	15:00-17:00	15:00-17:00
Price pre	21	0	25	35	20
Price post: rush hour	21	22	45	54/59*	44
Price post: non-rush	14	11	19	44/49**	22

Notes: The table presents Norwegian cities that implemented a cordon-based congestion charge between 2013 and 2019. Three of these policies are unsuitable for analysis due to data limitations: for Oslo and Stavanger, post-period car ownership data is unavailable, while in Kristiansand, the policy did not introduce higher rush hour rates. Prices are listed in NOK (10 NOK ≈ 1 EUR ≈ 1.2 USD) and reflect the rates for small passenger vehicles at the time of implementation. *Gasoline cars: NOK 54, diesel cars: NOK 59. **Gasoline cars: NOK 44, diesel cars: NOK 49.

Table A.2: The Norwegian EV incentives (as of January 2020)

Year	Instrument	Local incentive?
1990	Exempt from purchase/import taxes	
1996	Exempt from annual road tax	
1997	**Exempted from road toll**[1]	Yes
1997	Exempt from ferry charges[2]	Yes
1999	Free municipal parking[3]	Yes
2000	50 % reduced company car tax[4]	
2001	Exempt from 25% VAT on purchase	
2005	Access to bus lanes[5]	Yes
2015	Exempt from 25% VAT on leasing	
2018	Fiscal compensation for scrapping fossil car when switching to a zero-emission car	
2019	Holders of driver license class B allowed to drive electric car class C1 (light lorries)	

Notes: Table lists the different national and local electric vehicles incentives in Norway as of January 2020. At the national level, electric vehicles are exempt from purchase taxes and value-added tax (VAT). At the local level, electric vehicles benefit from exemptions from road toll and congestion charges, access to bus lanes, free parking, and free charging. *Source:* https://elbil.no/english/norwegian-ev-policy/. Year refers to the year implemented.
[1] From 2019: local authorities allowed to impose a rate of maximum 50% of the toll road.
[2] From 2018: local authorities allowed to impose a rate of maximum 50% of the ferry fares.
[3] From 2018: parking fees for EVs introduced locally. Upper limit of 50% of full price.
[4] From 2018: company car tax reduction reduced to 40%.
[5] From 2016: local authorities allowed to limit access to bus lanes to EVs that carry one or more passengers.

Table A.3: Vehicle prices (NOK)

	Prod. price	VAT	Reg. tax	MSRP
Compact cars				
VW Golf Sportsvan (gasoline)	196,456	49,114	73,230	318,800
Nissan Leaf (electric)	245,090	0	0	245,090
Luxury cars				
BMW 640i xDrive Coupe (gasoline)	505,434	126,358	503,007	1,134,800
Tesla Model S (electric)	655,000	0	0	655,000

Notes: The table displays prices in NOK for the modal compact electric car (Nissan Leaf, battery range ≈ 240 km) and the modal luxury electric car (Tesla Model S, battery range ≈ 460 km) in 2016, as well as comparable gasoline cars with similar engine effects. The last column is the manufacturer's suggested retail price (MSRP), publicly accessible at: https://www.skatteetaten.no/globalassets/tabeller-og-satser/listepris-bil/bilpriser-2016.pdf (accessed August, 2020). Based on the MSRP and national tax rates, we have backed out the producer's price excluding taxes, the VAT and the registration tax for the gasoline cars. Both tax components are zero for BEVs. 10 NOK ≈ 1 EUR and ≈ 1.2 USD.

Table A.4: Annual private ownership costs

Cost element	(1) New BEV	(2) New ICEV	(3) Used ICEV
Ownership costs			
Purchase price/value (NOK)	245,090	318,080	50,000
Annual depreciation rate (share)	0.12	0.12	0.12
Annual depr. cost, 5 year avg. (NOK)	23,150	30,112	4,723
Ownership tax (NOK)	445	3,135	3,135
Annual ownership cost (NOK)	**23,595**	**33,246**	**7,858**
Driving costs			
Driving (km)	11,680	11,680	11,680
Cost per kilometer (NOK)	0.16	0.68	0.76
Annual driving cost (NOK)	**1,869**	**7,942**	**8,877**
Toll payments			
Annual toll payments (NOK)	**0**	**9,900**	**9,900**
Total annual cost (NOK)	**25,464**	**51,206**	**30,605**
Value of other BEV incentives			
Free parking (NOK)	-2,349	0	0
Reduced ferry rates (NOK)	-579	0	0
Bus line time savings (NOK)	-4,498	0	0
Annual sum of incentives (NOK)	**-7,426**	**0**	**0**
Total annual cost incl. incentives (NOK)	**18,038**	**51,206**	**30,605**

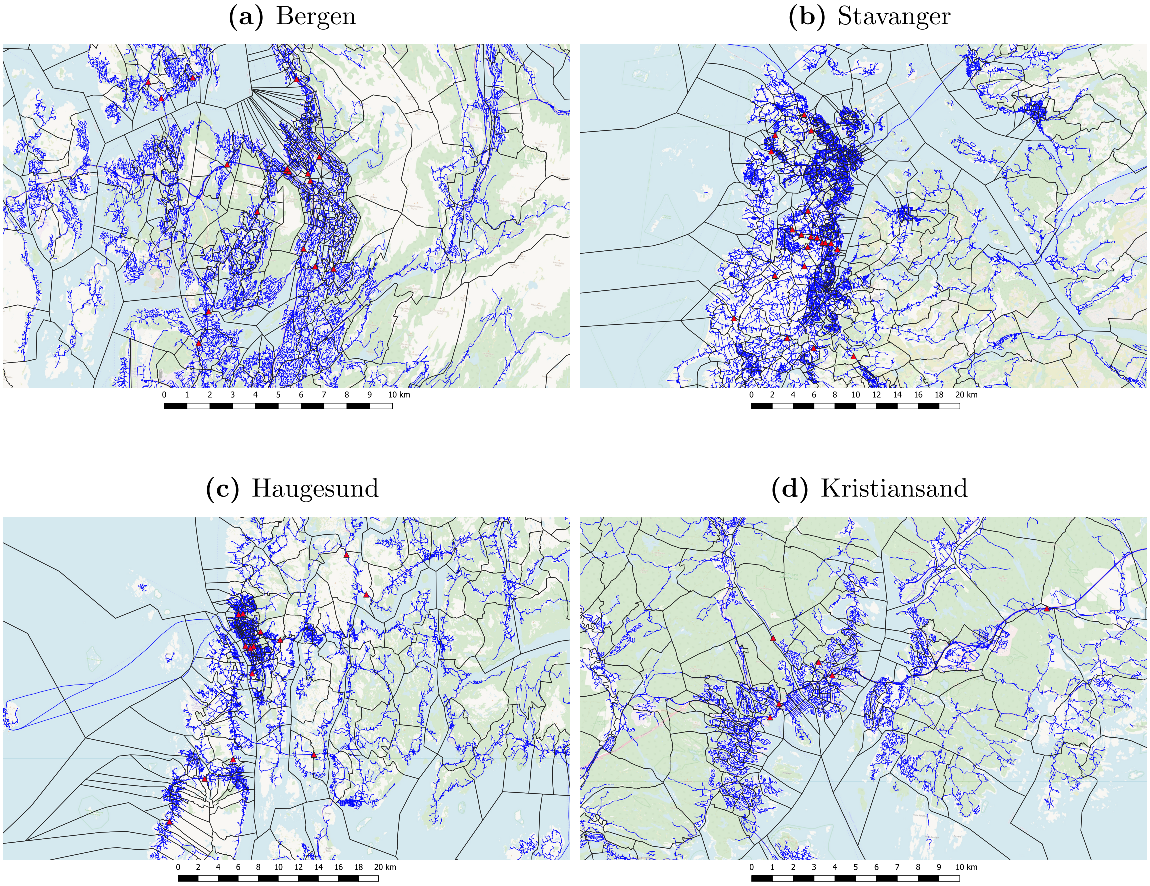
Notes: The table presents simplified calculations of the annual cost of car ownership for three different cars; a new Nissan Leaf (column 1), a new Volkswagen Golf Sportsvan (column 2), and a comparable 10-12 year old small gasoline car (column 3). See Table A.3 for different price components. We assume a depreciation of 12 % per year, and calculate annual depreciation as the average annual value loss over a five year period. Kilometers driven corresponds to average driving per car per year, as reported by Statistics Norway. The number reflect s the average of all passenger cars registered in Hordaland country in 2016, publicly accessible at: `https://www.ssb.no/statbank/table/12576/tableViewLayout1/` (accessed August, 2020). Price per kilometer is based on Norwegian gasoline and kWh prices and fuel/energy efficiency of compact cars. Annual toll payments is for paying commuters in Bergen when the congestion charge is active, assuming a household member drives to work each day (45 NOK×220 days). The annual value of other BEV incentives are based on a national survey among BEV owners in Norway; see Figenbaum and Kolbenstvedt (2016), p. 53 for details. We disregard service, maintenance and insurance costs, which are not necessarily differentiated by propulsion systems but positively correlated with the age of the car. 10 NOK ≈ 1 EUR and ≈ 1.2 USD.

Appendix B Data and descriptives

B.1 Neighborhood size

In our analysis, we compare households in Bergen to those in three control cities: Stavanger, Kristiansand, and Haugesund. Bergen is the second largest city in Norway (with 255,464 inhabitants in 2017), while Stavanger is the third largest city (with 222,697 inhabitants in 2017). Kristiansand and Haugesund are smaller cities (107,157 and 37,166 inhabitants in 2017, respectively). All cities are located in the south-west of Norway, along the coast.

Figure B.1: Neighborhood borders



Notes: The figure illustrates neighborhood size in four cities. Black lines denote neighborhood borders, while blue lines in the background is the road network. The densest urban areas are where the neighborhoods are smallest and the road network most dense. Small red triangles are the toll gates.

Figure B.1 illustrates the various sizes of neighborhoods across Bergen and the three control cities. The neighborhoods reflect the level of the geography-year fixed effects in our main regressions. While some of the neighborhoods in the outskirts of the cities are several kilometers in diameter (these are typically areas that include water, forests, mountains, or green spaces), neighborhoods in the more densely populated parts of the cities are often 100 by 100 meters or smaller. On average, there are around 80 households per neighborhood for the geographical areas included in our main analysis.

B.2 Toll cordon locations and toll rate developments in four cities

Figure B.2 illustrates the borders of the toll cordons in Bergen and the three control cities. A common feature across all four cities is that in order to reach the city center you need to pass a toll cordon. However, in several cities, these cordons have more than one layer. This is of limited concern for the interpretation of the estimated treatment effects due to the hour rule; you only pay once per hour irrespective of how many toll gates you cross as a part of the toll cordon.

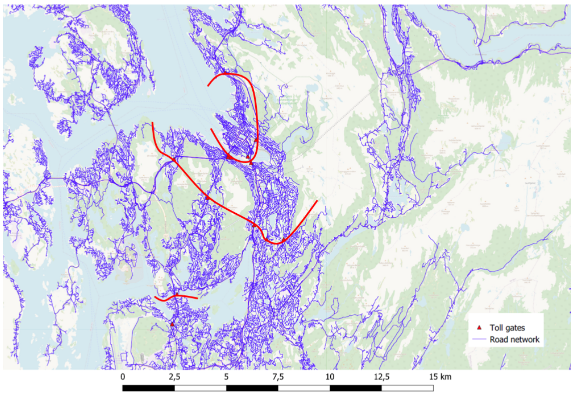
Kristiansand is the only city where the toll cordon only has one ring. Bergen has two rings, in addition to a toll gate in relation to a bridge over a strait. Stavanger has toll gates that block off the city center from the northern and southern directions, as well as additional toll gates to the south that make up a semi-circle around Sandnes, south of Stavanger. Haugesund has a ring around the city center in addition to strategically located toll gates at major roads further out.

Figure B.3 compares the toll rates in all four cities during rush hours from 2006 to 2018. In our sample period (end of 2011 to end of 2017), rush hour rates were constant for Stavanger and Kristiansand (around NOK 20), while there was a very small increase in the rush hour rates in Haugesund from around NOK 12 to NOK 14. By contrast, there was a large increase in rush hour rates in Bergen in the same period; first a small jump from NOK 15 to NOK 25 in 2013, then a large jump from NOK 25 to NOK 45 in 2016. See also Figure A.1 for a more detailed illustration of the rush and non-rush rates in Bergen.

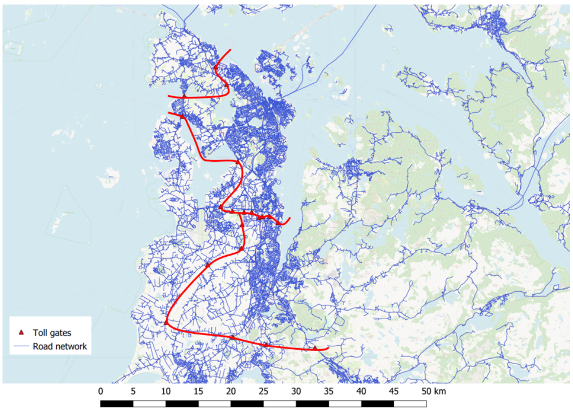
The small increase in toll rate in 2013 means that estimated treatment effects might reflect a response to both these jumps in toll rates. If we observe a positive treatment effect in 2015 on car ownership, this might reflect both anticipation effects of the congestion charge announcement on February 1, 2015, as well as a potential delayed response to the small jump in toll rates in the summer of 2013.

Figure B.2: Toll cordon borders

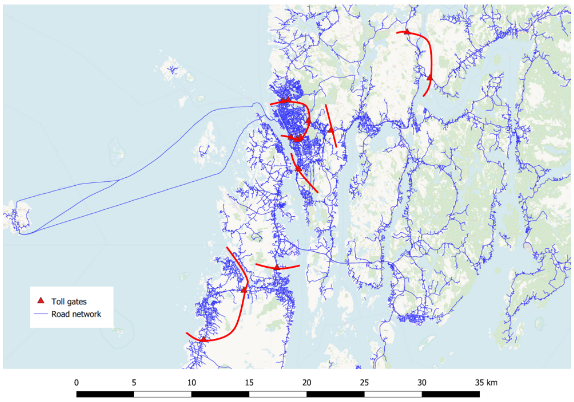
(a) Bergen



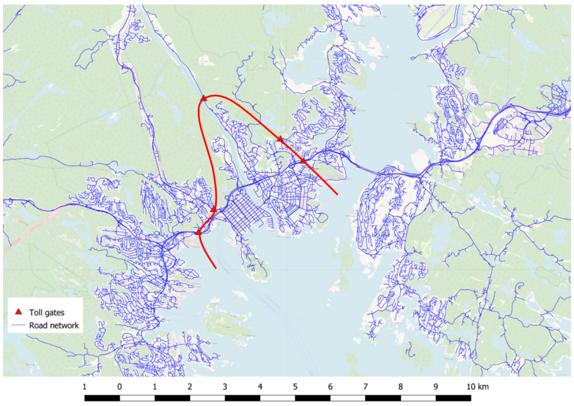
(b) Stavanger



(c) Haugesund

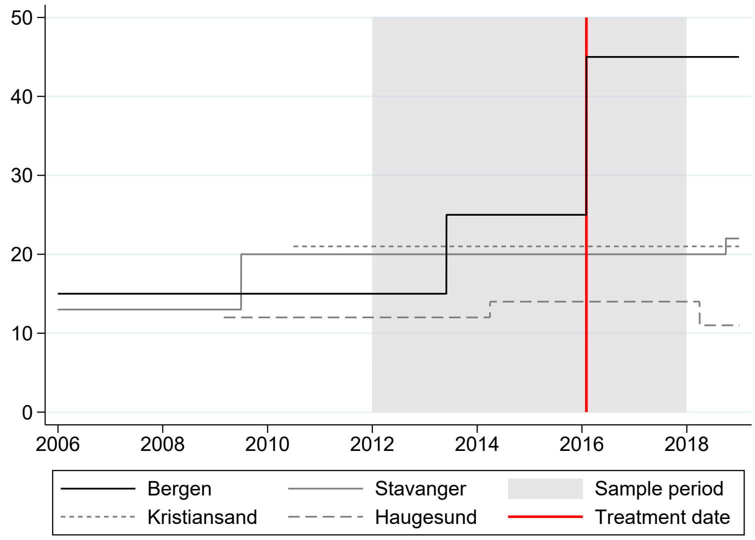


(d) Kristiansand



Notes: The figure illustrates the toll cordon borders in four cities. The red lines indicate where it is impossible to drive through without having to pass a toll cordon. “Paying commuters” live and work on opposite sides of one or more such lines, while “non-paying commuters” live and work on the same side. Thin blue lines show the road network in and around each city.

Figure B.3: Development in rush hour toll rates



Notes: The figure displays the development in rush hour toll rates over time for the cities Bergen, Stavanger, Kristiansand, and Haugesund. The toll rate in Bergen outside rush hours is not displayed to enhance readability. Time is presented continuously rather than annual, which means that the points on the x-axis where the rates change correspond to the actual date. For Kristiansand and Haugesund, the start of the line marks the date when the toll cordon was introduced. The gray shaded area marks the sample period used in the analysis.

B.3 Details on variables and model specification

Table B.1 presents a description of all relevant variables used in our analysis.

Work commute characteristics: We obtained a measure of public transit time from the Norwegian regional transportation models (RTM). These are national transportation models frequently used by policy makers. The variable we use is the sum of time on board, waiting time (calculated as a function of the frequency), transit time, and access/egress time (i.e. walking to/from stations). For a more detailed description of how public transit routes are coded in RTM, see Kwong and Ævarsson (2018). All neighborhood and work commute variables are fixed to 2014 to address potential endogenous sorting.

Control variables: In regressions without household fixed effects, we control for the following set of variables: Dummy variables for being female, being employed, being retired, owning a second home, having children below the age of 18, and separate dummies for education levels (unknown, less than high school, high school, college, and university). A continuous variable for the number of persons (adults and children) registered at the household. Two polynomials in age, net income, net wealth, distance to work, and time to work by car. We also include two polynomials for two variables that are meant to capture the workplace's accessibility by public transit – these are the absolute and the relative time differences to get to work by public transit versus private car. All variables except "children" and "number of household members" are individual specific, but averaged across spouses. If a variable is missing for one of the spouses, the other spouse's value is used. If a variable is missing for both spouses, that household is omitted from the regression. Finally, we let the coefficients for all variables be couple and single specific (i.e. whether the household has one or two adult members). Our main regressions include household fixed effects, which means that time-invariant controls such as female, educational attainment, and (2014) work commute controls will drop out.

Table B.1: Description of variables

Variable	Description
Panel A: Outcome variables	
BEV_{it}	Dummy variable indicating whether household i owns a battery electric vehicle
NumBEV_{it}	The number of battery electric vehicles owned by household i
ICEV_{it}	The number of internal combustion engine vehicles owned by household i
cars_{it}	Total number of vehicles owned by household i
Panel B: Treatment variables	
B_i	Dummy variable, 1 if household lives in the vicinity of Bergen; 0 if the household lives in the vicinity of control cities
c_i	Dummy variable, 1 if at least one household member pass the toll cordon; 0 if no household members are exposed to tolls on their commute
post_t	Dummy variable for 2016 and later
Panel C: Control variables	
couple_{it}	Dummy variable indicating whether there is more than one adult household member
age_{it}	Average age of adult household members
female_{it}	Share of adult household members that are females
employed_{it}	Share of adult household members that are employed
retired_{it}	Share of adult household members that are retired
secondhome_{it}	Dummy variable for whether household owns second home
persons_{it}	Number of household members, adults and children
children_{it}	Dummy variable for having children <18 years living at home
income_{it}	Average net income of adult household members. Labor and capital income net of taxes plus other transfers
wealth_{it}	Average net wealth of adult household members. Value of capital stock (including property) and financial assets net of outstanding debt
educ0_{it}	Dummy: all household members have unknown education
educ1_{it}	Dummy: highest education in household is less than high school
educ2_{it}	Dummy: highest education in household is high school
educ3_{it}	Dummy: highest education in household is 1-4 years of university (undergraduate level)
educ4_{it}	Dummy: highest education in household is more than 4 years of university (gradate and post graduate level)
wd_{it}	Average work distance of adult employed household members in kilometers. Fastest route between centroids of working and residence neighborhoods
time_{it}	Time spent in minutes associated with the commute above, according to the speed limit
PT_diff_{it}	Time to work by public transit (including expected waiting, transit and access/egress time) minus time spent by car
PT_share_{it}	Time to work by public transit (including expected waiting, transit and access/egress time) divided by time spent by car
θ_{nt}	Neighborhood by year fixed effects for the household's residence location

Appendix C Supporting results

C.1 Difference-in-differences (DiD) estimates

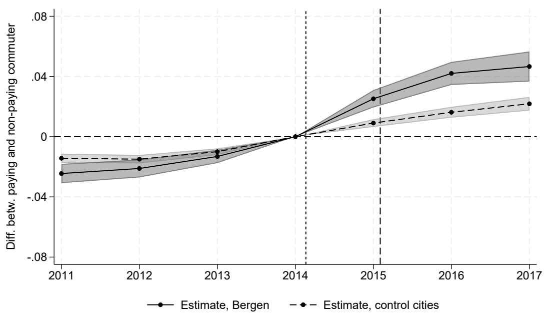
Here, we show estimated treatment effects from two separate DiD regressions for Bergen and the control cities, where we use variation over time between paying and non-paying commuters. These regressions take the following form:

$$y_{it} = \sum_{s \in \{T | s \neq 2014\}} \alpha_t c_i \times \mathbb{1}\{t = s\} + \eta_i + X'_{it}\gamma + \theta_{nt} + \varepsilon_{it} \tag{1}$$

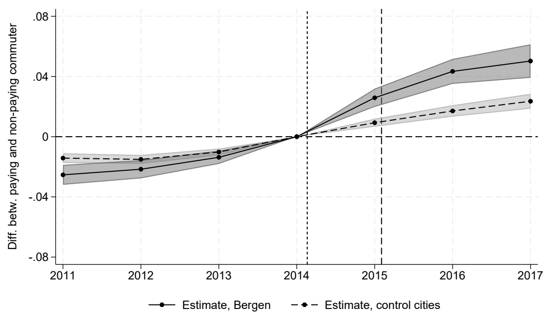
The household fixed effects η_i will absorb any time-invariant household-specific effects (including the difference between paying and non-paying commuters), θ_{nt} indicates neighborhood $\times$ year fixed effects and X'_{it} is a vector of demographics and work route specific controls; see Section 4 for details. The dynamic DiD estimates are captured by α_t^1 (α_t^0) and reflect the estimated "paying commuter" effect for Bergen (control cities) in year t. The triple difference estimate in a given year can be derived from $\alpha_t^1 - \alpha_t^0$.

Figure C.1: DiD estimates for Bergen and control cities.

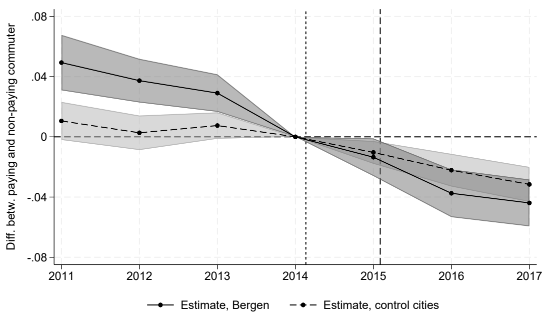
(a) Pr(BEV)



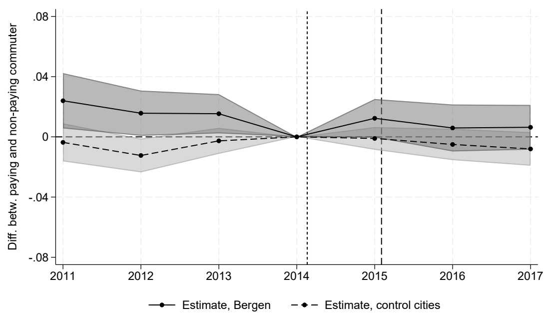
(b) Number of BEVs



(c) Number of ICEs



(d) Number of cars



Notes: The figure shows coefficients α_t estimated from Equation 5, where α_{2014} is normalized to zero. Grey areas indicate 95% confidence intervals. The outcome is indicated by the sub-figure heading. Vertical dotted lines denote the announcement date (Feb 18th 2015), while vertical dashed lines denote the implementation date (Feb 1st 2016). Standard errors are clustered at the neighborhood level.

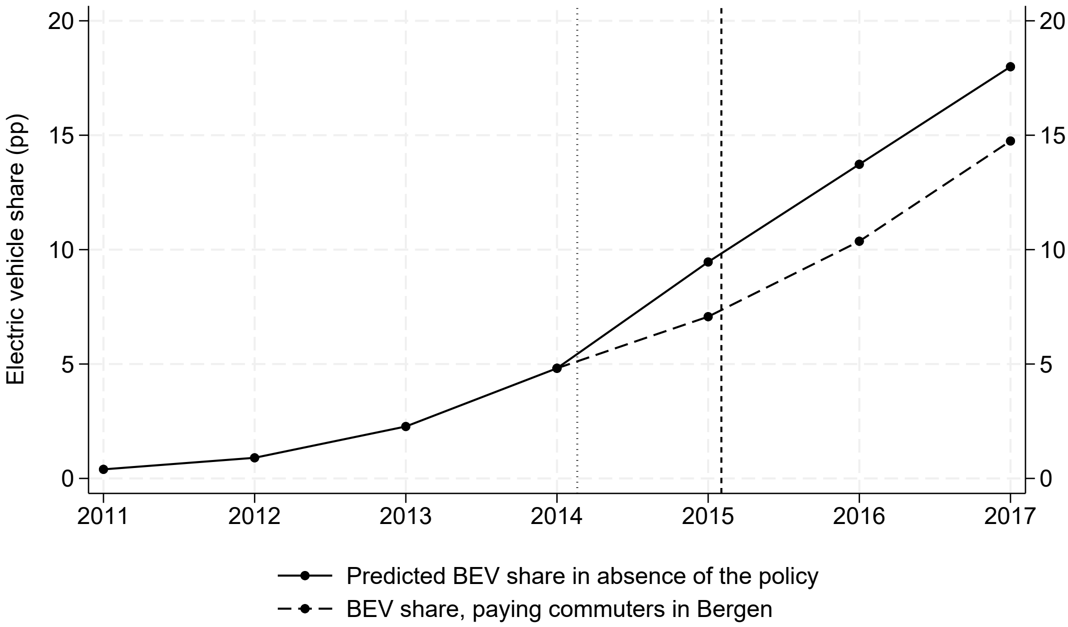
Figure C.1 shows the DiD estimates for Bergen and the control cities. The estimated coefficients in Panel (a) show that paying computers in both cities experience an increase in the electric vehicle ownership share relative to non-paying computers. Panel (b) shows a similar pattern when using the number of electric vehicles as the outcome variable, while Panel (c) shows a declining trend in conventional vehicle ownership prior to the policy. While the pre-treatment trend for the total number of vehicles is relatively parallel in the DiD set-up, this trend masks the differential computational change in car ownership.

By subtracting the estimated effects for the control cities from the true treatment effects for Bergen ($\alpha_t^1 - \alpha_t^0$), we get a triple differences estimate that is approximately the same as our DiDiD estimates presented in Figure 4 in the main text. In contrast to the DiD estimates, the triple difference estimates indicate parallel trends in the pre-treatment period.

C.2 Predicted electric vehicle ownership

Figure C.2 shows the observed and predicted electric vehicle ownership in the period 2011-2017.

Figure C.2: Observed and predicted levels of electric vehicle ownership



Notes: The solid line shows the share of households among paying commuters in Bergen that owned an electric vehicle in the period 2011-2017. The dashed line shows the predicted share of households among paying commuters in Bergen that would have owned an electric vehicle in absence of the congestion charge, based on the treatment estimates reported in Figure 4, panel (a). Car ownership is measured at the end of the year The vertical distance between the two lines indicate the annual treatment effects. The vertical dotted line denotes the announcement date (Feb 18th 2015) and the vertical dashed line denotes the implementation date (Feb 1st 2016).

Appendix D Robustness checks

This appendix presents robustness checks of our main results on EV ownership.

D.1 Sample restrictions

Table D.1 shows the sensitivity of our main result to including shorter work commutes. Table D.2 shows results when restricting the sample to work commutes *shorter than 5 km*. Table D.3 shows results when focusing on individuals that commute *into or through the toll cordon* (as opposed to out of the toll cordon).

Table D.1: DiDiD estimates on EV ownership, by different work commute cut-offs

Sample includes workdistances gt.:	2 km	3 km	4 km	5 km
Dependent variable:	Pr(BEV)	Pr(BEV)	Pr(BEV)	Pr(BEV)
	(1)	(2)	(3)	(4)
Post × Paying commuters × Bergen	0.0232***	0.0264***	0.0283***	0.0293***
	(0.00393)	(0.00432)	(0.00467)	(0.00494)
Observations	941446	858388	782885	707952
Mean depvar 2014 (paying commuters, Bergen)	0.0442	0.0452	0.0469	0.0491
Mean depvar 2017 (paying commuters, Bergen)	0.1654	0.1694	0.1746	0.1800
Paying commuter × year FE ($\alpha_t c_i$)	✓	✓	✓	✓
Household FE (η_i)	✓	✓	✓	✓
Household characteristics ($X'_{it}\gamma$)	✓	✓	✓	✓
Neighborhood × year FE (θ_{nt})	✓	✓	✓	✓

Notes: Table plots the coefficient β estimated from Equation 1. The sample is restricted to the years 2011-2017, where 2016-2017 denotes the "post" period and 2015 is excluded due to potential anticipation effects. Each column present results from a different sample. Column (4) presents our main estimates, where we restrict the sample to households with a work commute distance of at least 5 km. Column (3) presents results when using a cut-off of 4 km, etc. Standard errors are clustered at the 2014 neighborhood level (1,786 clusters in the last column). * p<0.10, ** p<0.05, *** p<0.01.

Table D.2: DiDiD estimates on vehicle ownership for households with a work commute of *less than 5 km*

	Probability	Number of vehicles		
Dependent variable:	Pr(BEV) (1)	BEV (2)	ICEV (3)	Total (4)
Post × Paying commuters × Bergen	0.00213 (0.00676)	0.00376 (0.00734)	0.0101 (0.0151)	0.0138 (0.0144)
Observations	336998	336998	336998	336998
Mean depvar 2014 (paying commuters, Bergen)	0.0192	0.0199	0.7059	0.7258
Mean depvar 2017 (paying commuters, Bergen)	0.0926	0.0976	0.7430	0.8406
Paying commuter × year FE ($\alpha_t c_i$)	✓	✓	✓	✓
Household FE (η_i)	✓	✓	✓	✓
Household characteristics ($X'_{it}\gamma$)	✓	✓	✓	✓
Neighborhood × year FE (θ_{nt})	✓	✓	✓	✓

Notes: Table plots the coefficient β estimated from Equation 1. The dependent variable is indicated by the column heading. BEV refers to battery electric vehicles, ICEV refers to internal combustion engine vehicles, and Total refers to the total number of vehicles owned by the household. The sample is restricted to households with a work commute distance of less than 5 km, as well as the years 2011-2017, where 2016-2017 denotes the "post" period and 2015 is excluded due to potential anticipation effects. Standard errors are clustered at the 2014 neighborhood level (1,569 clusters). * p<0.10, ** p<0.05, *** p<0.01.

Table D.3: DiDiD estimates by location of residence and workplace

Dependent variable: Pr(BEV)	Base reg. (1)	Live outside (2)	Live inside (3)	No transit (4)	Transit (5)
Post × Paying commuters × Bergen	0.0293*** (0.00494)	0.0258*** (0.00541)	-0.00392 (0.0111)	0.0319*** (0.00544)	0.0216*** (0.00814)
Observations	707952	536859	163496	620010	295684
Mean depvar 2014 (paying commuters, Bergen)	0.0491	0.0587	0.0262	0.0497	0.0480
Mean depvar 2017 (paying commuters, Bergen)	0.1800	0.2059	0.0902	0.1802	0.1850
Paying commuter × year FE ($\alpha_t c_i$)	✓	✓	✓	✓	✓
Household FE (η_i)	✓	✓	✓	✓	✓
Household characteristics ($X'_{it}\gamma$)	✓	✓	✓	✓	✓
Neighborhood × year FE (θ_{nt})	✓	✓	✓	✓	✓

Notes: Table plots the coefficient β estimated from Equation 1.Column (1) replicates our main specification, based on the full sample. Column (2) excludes paying and non-paying commuters living inside the toll cordon. Column (3) only includes households living inside the toll cordon. Column (4) excludes paying commuters that live and work on opposite sides of the toll cordon (i.e., pass-through commuters are excluded). Column (5) only contains households that live and work outside the toll cordon (i.e., all paying commuters are pass-through commuters). The sample is restricted to the years 2011-2017, where 2016-2017 denotes the "post" period and 2015 is excluded due to potential anticipation effects. Standard errors are clustered at the 2014 neighborhood level (725-1,649 clusters, depending on specification).

D.2 Fixed effects and controls

Table D.4 shows the robustness of our main result across different fixed effects specifications. Table D.5 shows results when interacting the neighborhood-specific trends with the household-level work commute distance in 2014.

Table D.4: DiDiD estimates with different fixed effects

DiDiD specification	Simple (1)	w/year FE (2)	w/θ_{nt} (3)	w/HH ctrl. (4)	w/HH FE **(5)**
Panel A: Pr(BEV)					
Post × Paying commuters × Bergen	0.0127***	0.0134***	0.0290***	0.0299***	0.0293***
	(0.00488)	(0.00485)	(0.00481)	(0.00479)	(0.00494)
Observations	717892	717892	717777	713873	707952
Mean depvar 2014 (paying commuters, Bergen)	0.0471	0.0471	0.0471	0.0471	0.0491
Mean depvar 2017 (paying commuters, Bergen)	0.1800	0.1800	0.1800	0.1800	0.1800
Panel B: Number of BEVs					
Post × Paying commuters × Bergen	0.0133**	0.0140***	0.0306***	0.0316***	0.0308***
	(0.00523)	(0.00519)	(0.00527)	(0.00525)	(0.00542)
Observations	717892	717892	717777	713873	707952
Mean depvar 2014 (paying commuters, Bergen)	0.0485	0.0485	0.0485	0.0485	0.0506
Mean depvar 2017 (paying commuters, Bergen)	0.1906	0.1906	0.1907	0.1907	0.1907
Panel C: Number of ICEVs					
Post × Paying commuters × Bergen	-0.0206**	-0.0232**	-0.0340***	-0.0299***	-0.0326***
	(0.00937)	(0.00926)	(0.00994)	(0.00919)	(0.00883)
Observations	717892	717892	717777	713873	707952
Mean depvar 2014 (paying commuters, Bergen)	1.1081	1.1081	1.1083	1.1083	1.1427
Mean depvar 2017 (paying commuters, Bergen)	1.1098	1.1098	1.1099	1.1099	1.1099
Panel D: Number of cars in total					
Post × Paying commuters × Bergen	-0.00732	-0.00923	-0.00337	0.00171	-0.00179
	(0.00848)	(0.00845)	(0.00999)	(0.00914)	(0.00840)
Observations	717892	717892	717777	713873	707952
Mean depvar 2014 (paying commuters, Bergen)	1.1566	1.1566	1.1568	1.1568	1.1933
Mean depvar 2017 (paying commuters, Bergen)	1.3004	1.3004	1.3006	1.3006	1.3006
Post	✓				
Paying commuter	✓				
Paying × Post	✓				
Bergen	✓				
Bergen × Post	✓				
Bergen × Paying commuter	✓	✓	✓	✓	
Year FE		✓			
Bergen × Year FE		✓			
Paying commuter × Year FE		✓	✓	✓	✓
Neighborhood × Year FE (θ_{nt})			✓	✓	✓
HH and work commute controls				✓	✓
Household FE (η_i)					✓

Notes: BEV refers to battery electric vehicles, while ICEV refers to internal combustion engine vehicles. Regression (1) estimates the triple difference with dummies for "post", "Bergen" and "paying commuters". Regression (2) makes time controls year-specific, alleviating the need for the "post" dummy. Regression (3) adds neighborhood×year fixed effects, alleviating the need for a "Bergen" dummy. Regression (4) adds household and work commute controls, and finally, regression (5) adds household fixed effects, making "Paying commuter×Bergen" a redundant variable. Regression (5) is the same specification as in Equation 1 and Table 3. Standard errors are clustered on 2014 neighborhoods (1,786 clusters). * p<0.10, ** p<0.05, *** p<0.01.

Table D.5: DiDiD estimates: sensitivity to fuel prices

	Probability	Number of vehicles		
Dependent variable:	Pr(BEV)	BEV	ICEV	Total
	(1)	(2)	(3)	(4)
Post × Paying commuters × Bergen	0.0222***	0.0226***	-0.0298***	-0.00714
	(0.00520)	(0.00575)	(0.00929)	(0.00913)
Observations	707952	707952	707952	707952
Mean depvar 2014 (paying commuters, Bergen)	0.0491	0.0506	1.1427	1.1933
Mean depvar 2017 (paying commuters, Bergen)	0.1800	0.1907	1.1099	1.3006
Paying commuter FE × year FE ($\alpha_t c_i$)	✓	✓	✓	✓
Household FE	✓	✓	✓	✓
Household characteristics ($X_{it}'\gamma$)	✓	✓	✓	✓
Neighborhood FE × year FE (θ_{nt})	✓	✓	✓	✓
Neighborhood FE × year FE × work distance ($\Theta_{nt} dist_i$)	✓	✓	✓	✓

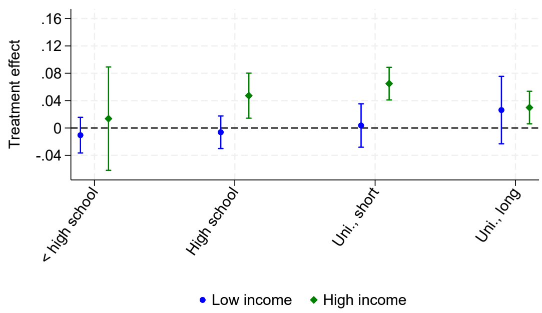
Notes: BEV refers to battery electric vehicles, while ICEV refers to internal combustion engine vehicles. Compared to the main specification, these regressions absorb an additional set of neighborhood × year fixed effects that are interacted with the work distance of each household (as a continuous variable). These neighborhood-year-specific "work distance slopes" will absorb any local trends that potentially affect neighbors differentially by virtue of the length of the commute (e.g. local variation in fuel prices, congestion, etc.). Such local trends might be conflated with the treatment effect in case there is systematic within-neighborhood work distance variation between paying and non-paying commuters. Standard errors are clustered on 2014 neighborhoods (1,786 clusters). * p<0.10, ** p<0.05, *** p<0.01.

Appendix E Heterogeneous effects

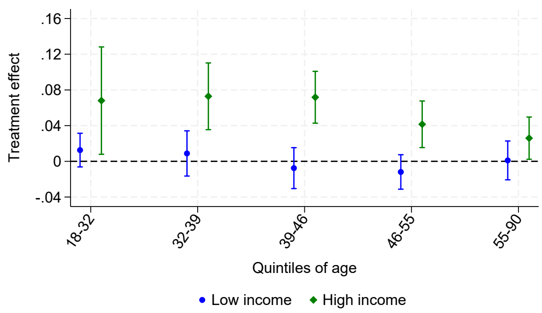
Table E.1 shows heterogeneous treatment effects for education, age, public transit quality and work distance, by the top and bottom income quintile. Table E.1 shows our main heterogeneous DiDiD estimates in table format. Figure E.2 and E.3 show heterogeneous DiDiD estimates for ICEV ownership and the total number of cars, respectively.

Figure E.1: Heterogeneous DiDiD estimates on Pr(BEV), by top and bottom income quintiles

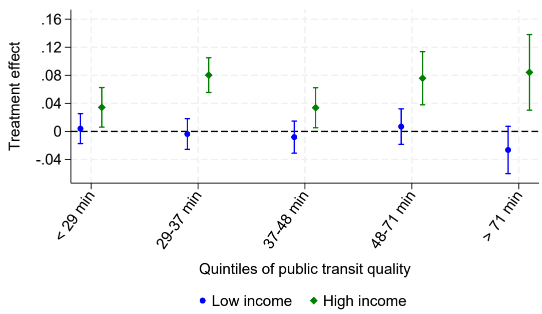
(a) Education



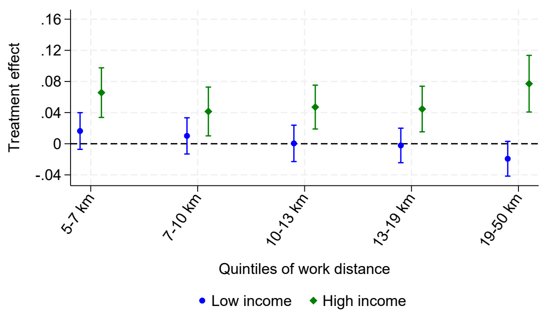
(b) Age



(c) Public transit quality



(d) Work distance



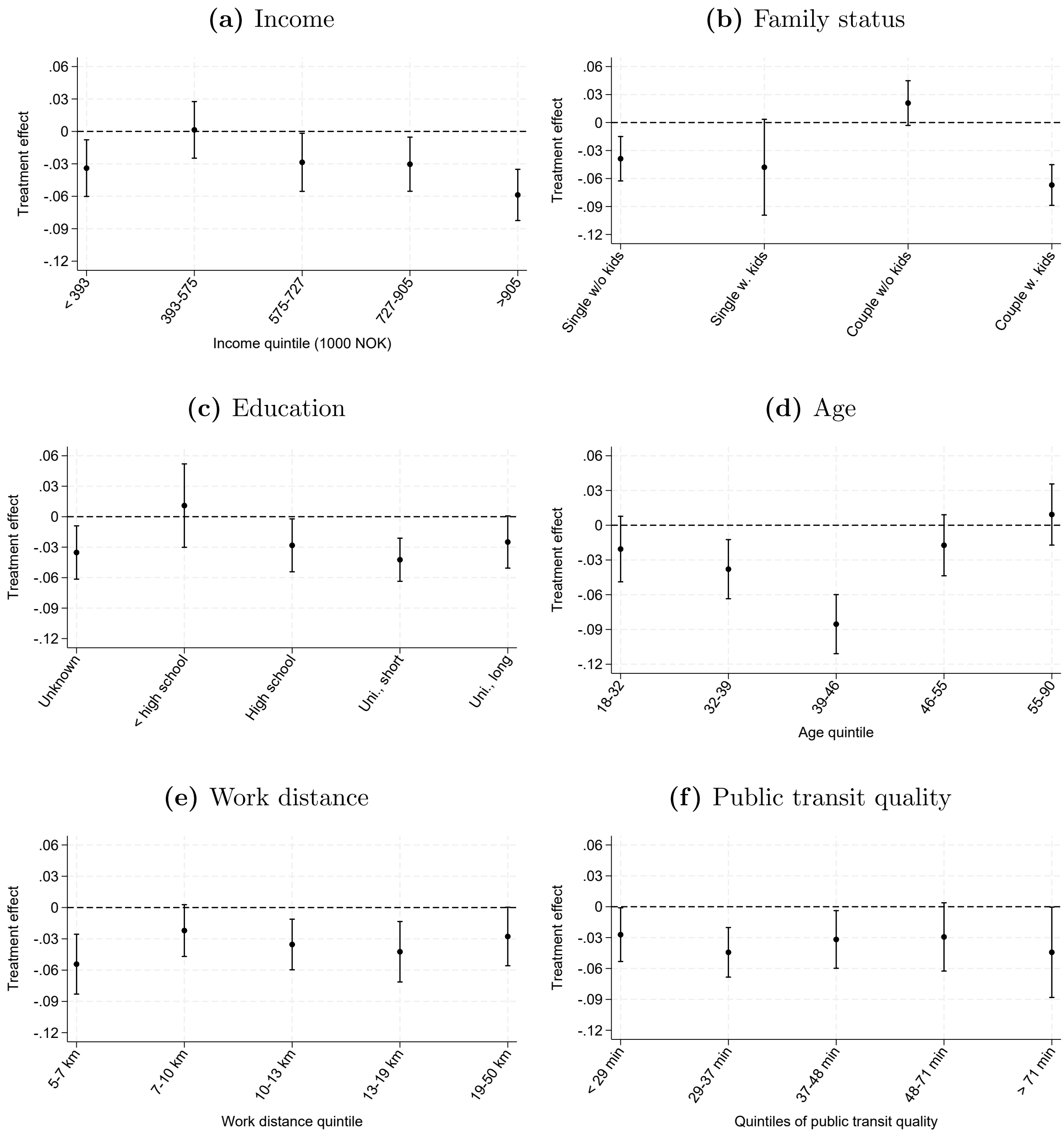
Notes: The figure plots the coefficients β_k estimated from Equation 3, where groups are defined as interactions between the top or bottom income quintile, and education level (Panel a), age (Panel b), quintiles of public transit quality (Panel c), or quintiles of work distance (Panel d). Whiskers indicate 95 % confidence intervals. The dependent variable is a dummy variable equal to 1 if the household owns an electric vehicle in year t and 0 otherwise. Group allocation is household specific and based on 2014 values. Educational attainment refers to the maximum level observed within the household, income is summed over spouses and other variables are averaged across spouses. "Public transit quality" relates to the commute, and is measured as the difference in commute time between driving a private car and public transit. Households in income quintiles 2-4 are dropped from the regression. Standard errors are clustered at the neighborhood level.

Table E.1: Heterogeneous DiDiD estimates on Pr(BEV)

Dependent variable: Pr(BEV)	Estimate for group number:				
Measured in percentage points	(1)	(2)	(3)	(4)	(5)
Panel A: Income†					
Post × Paying commuters × Bergen	-0.00128	0.0152**	0.0334***	0.0276***	0.0480***
	(0.00575)	(0.00610)	(0.00692)	(0.00766)	(0.00684)
Mean depvar 2014	0.01	0.02	0.04	0.06	0.10
Mean depvar 2017	0.05	0.10	0.17	0.22	0.30
Household income (1000 NOK)	344.79	495.12	641.47	779.50	1168.43
Households per group	109913	134387	149941	155970	157741
Panel B: Family status††					
Post × Paying commuters × Bergen	-0.000254	0.00284	0.0177***	0.0454***	
	(0.00572)	(0.0123)	(0.00592)	(0.00673)	
Mean depvar 2014	0.01	0.01	0.04	0.08	
Mean depvar 2017	0.06	0.10	0.15	0.27	
Households per group	155498	28060	228877	295517	
Panel C: Education‡					
Post × Paying commuters × Bergen	0.0148**	0.00696	0.0239***	0.0417***	0.0193**
	(0.00644)	(0.00810)	(0.00605)	(0.00670)	(0.00759)
Mean depvar 2014	0.02	0.02	0.04	0.06	0.08
Mean depvar 2017	0.07	0.09	0.15	0.23	0.23
Households per group	98709	72549	225871	209519	101304
Panel D: Age†					
Post × Paying commuters × Bergen	0.0325***	0.0459***	0.0356***	0.0228***	0.00996
	(0.00757)	(0.00784)	(0.00805)	(0.00690)	(0.00643)
Mean depvar 2014	0.03	0.06	0.07	0.05	0.04
Mean depvar 2017	0.14	0.22	0.23	0.18	0.12
Average age	28.31	35.68	42.38	50.24	60.56
Households per group	110849	139679	152045	156234	149145
Panel E: Work distance†					
Post × Paying commuters × Bergen	0.0277***	0.0228***	0.0278***	0.0333***	0.0346***
	(0.00797)	(0.00729)	(0.00673)	(0.00689)	(0.00771)
Mean depvar 2014	0.03	0.05	0.05	0.06	0.07
Mean depvar 2017	0.14	0.17	0.19	0.20	0.21
Work distance (kilometers)	6.10	8.40	11.18	15.34	26.61
Households per group	135798	141078	142657	143676	144743
Panel F: Public transit†					
Post × Paying commuters × Bergen	0.0193***	0.0313***	0.0246***	0.0451***	0.0464***
	(0.00718)	(0.00677)	(0.00754)	(0.00853)	(0.0126)
Mean depvar 2014	0.04	0.05	0.06	0.06	0.07
Mean depvar 2017	0.14	0.18	0.20	0.24	0.23
Time public transport minus time car (minutes)	22.87	32.97	42.56	57.95	125.55
Households per group	134114	140444	143787	145017	144590

† Column number refers to quintiles of the population.
†† 1: Single without kids; 2: Single with kids; 3: Couple without kids; 4: Couple with kids.
‡ 1: Unknown; 2: Less than high school; 3: High school; 4: Higher education ($\leq$4 years); 5: Higher education (>four years).
Notes: The table shows the coefficient β_k estimated from Equation 3, where k refers to group (e.g., income quintile). All coefficients presented in a panel is from the same regression. The dependent variable is a dummy variable equal to 1 if the household owns an electric vehicle in year t and 0 otherwise. Group allocation is based on 2014 values, which means that households will not move between groups over time. The sample consists of years 2011-2017, where 2016-2017 denotes the "post" period and 2015 is excluded due to potential for anticipation effects. Standard errors are clustered on 2014 neighborhoods (1,786 clusters). * p<0.10, ** p<0.05, *** p<0.01.

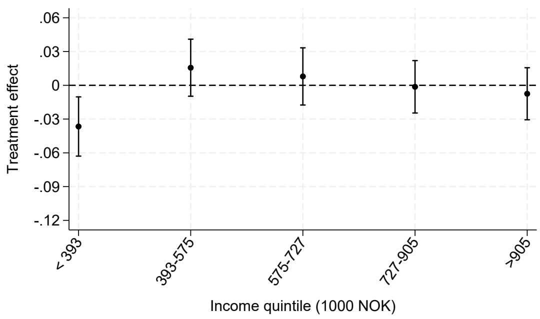
Figure E.2: Heterogeneous DiDiD on ICEV ownership



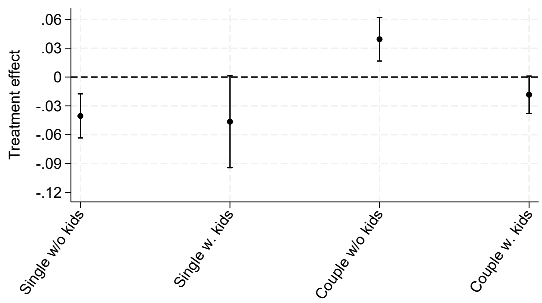
Notes: The figure plots the coefficients β_k estimated from Equation 3, where k refers to group (e.g., income quintile). Each panel (a-f) plots coefficients estimated from a separate regression. Whiskers indicate 95% confidence intervals. The dependent variable is number of internal combustion engine vehicles owned by the household. Groups are based on 2014 demographics. "Income" is summed over spouses, "education" is the maximum value in each household and "age", "work distance" and "public transit quality" are averaged over spouses. Public transit quality is defined as "time to work by public transit minus time to work by car" in minutes. The sample is restricted to the years 2011-2017, where 2016-2017 denotes the "post" period and 2015 is excluded due to potential anticipation effects. Standard errors are clustered at the neighborhood level.

Figure E.3: Heterogeneous DiDiD: Car ownership.

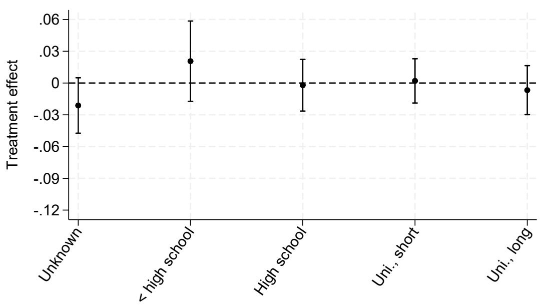
(a) Income



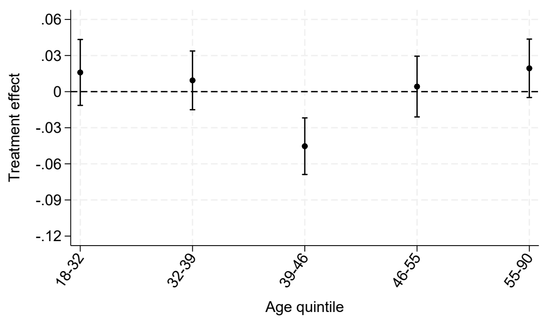
(b) Family status



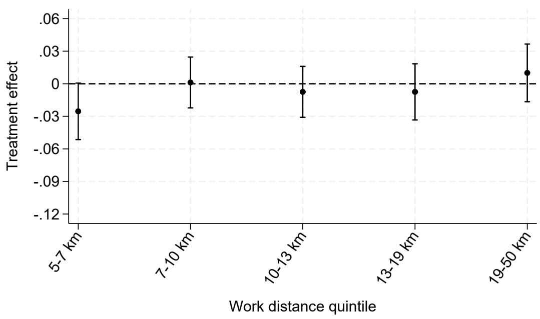
(c) Education



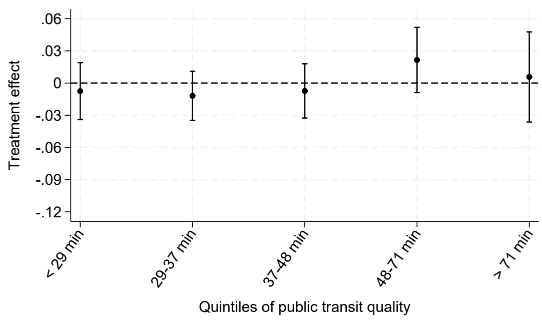
(d) Age



(e) Work distance



(f) Public transit quality



Notes: The figure plots the coefficients β_k estimated from Equation 3, where k refers to group (e.g., income quintile). Each panel (a-f) plots coefficients estimates from a separate regression. Whiskers indicate 95% confidence intervals. The dependent variable is the total number of cars owned by the household. Groups are based on 2014 demographics. "Income" is summed over spouses, "education" is the maximum value in each household and "age", "work distance" and "public transit quality" are averaged over spouses. Public transit quality is defined as "time to work by public transit minus time to work by car" in minutes. The sample is restricted to the years 2011-2017, where 2016-2017 denotes the "post" period and 2015 is excluded due to potential anticipation effects. Standard errors are clustered at the neighborhood level.

Appendix F Traffic and air pollution

This appendix provides supporting material and results for the estimated effects of the time-varying congestion charge on traffic and air pollution.

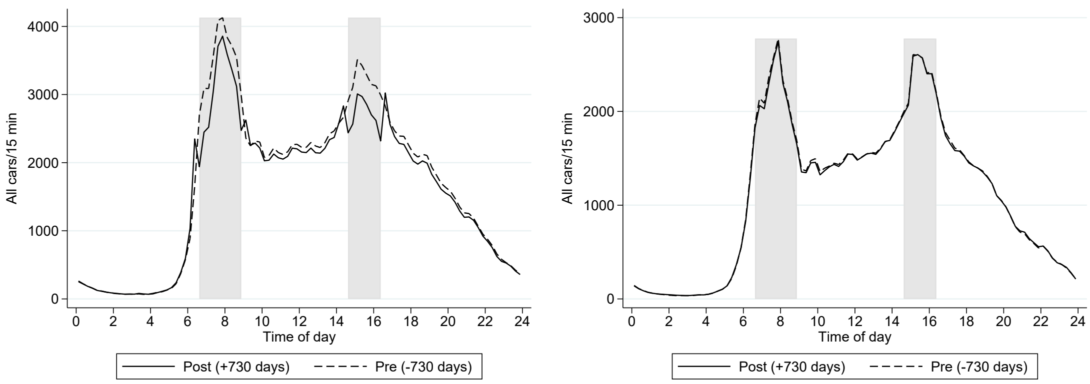
F.1 Data and descriptives

Driving into the city

Figure F.1 shows how vehicles passing the toll gates in Bergen (Panel a) and the other cities (Panel b) are distributed over the course of the day two years before (dashed line) and two years after (solid line) the Bergen congestion charge was implemented. Panel (a) shows a clear decline in rush hour traffic in Bergen, while the same is not observed for the control cities. For Bergen, we also see a small increase in the number of cars right before and after rush hours, suggesting that the policy induced some drivers to change their departure time to avoid the increased cost.

Figure F.1: Traffic volume two years before and after Feb 1 2016

(a) Bergen **(b)** Control cities



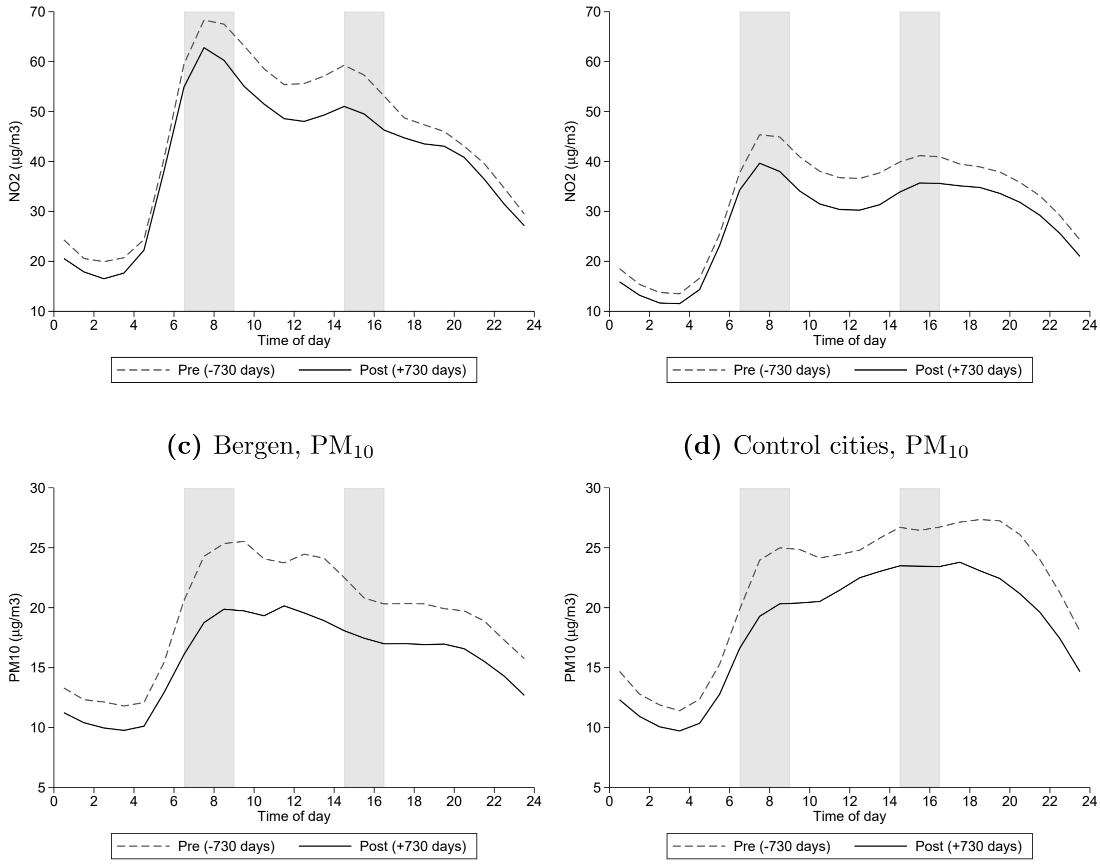
Notes: Figure shows the average number of vehicles passing the toll cordon over the course of a weekday (Monday-Friday) based on 15 minute intervals. The total number of cars are measured in the direction of the city center. Panel (a) shows averages for Bergen and Panel (b) shows averages for three control cities (Stavanger, Haugesund, Kristiansand). Dashed lines indicate averages for the 730 days (2 years) prior to policy implementation. Solid lines indicate averages for the 730 days (2 years) post policy implementation. Gray shaded areas indicate rush hours (06:30-09:00 and 14:30-16:30).

Air pollution

Figure F.2 shows how ambient levels of NO_2 and PM_{10} vary over the course of 24 hours in the two years before (dashed line) and after (solid line) policy implementation for Bergen (Panels a and c) and the control cities (Panels b and d). Gray shaded areas indicate rush hours. To put the levels of air pollution into context, the WHO Air Quality Guidelines for NO_2 and PM_{10} are 40 $\mu g/m^3$ annual mean and 20 $\mu g/m^3$ annual mean, respectively (WHO, 2006). By comparing the average pollution levels pre and post policy, we see that there is a clear decline in ambient air pollution for both Bergen and other cities.

Figure F.2: Air pollution two years before and after Feb 1 2016

(a) Bergen, NO_2 **(b)** Control cities, NO_2 **(c)** Bergen, PM_{10} **(d)** Control cities, PM_{10}



Notes: Figure shows average ambient air pollution over the course of a day for the pollution monitoring stations in Bergen (Panels a and c) and other cities (Panels b and d). Values are based on 60 minute intervals. Sample is restricted to weekdays. Dashed lines indicate averages for the two years prior to policy implementation (Feb 1 2016). Solid lines indicate averages for the two years post policy implementation. Gray shaded areas indicate rush hours. Pollution is measured as micrograms per cubic meter of air(μg/m3).

Weather

To control for the effects of weather on traffic and air pollution outcomes, we collect monitor-level weather data from the Norwegian Meteorological Institute for the years 2014-2018. We focus on hourly measures of temperature, precipitation, wind speed, and wind direction. The weather data is linked to a pollution monitoring station by calculating the inverse distance weighted average of observations from all weather stations within a 50-kilometer radius of a pollution monitoring site. Based on hourly wind data, we construct four wind direction categories: northern $\in$ [0-45] and (315,360], eastern $\in$ (45,135], southern $\in$ (135,225], western $\in$ (225,315].

F.2 Empirical strategy: additional details

In the main traffic regressions, y_{ist} denotes total traffic volume (passenger vehicles and trucks) that pass the toll cordon in Bergen during a 15-minute interval. For air pollution regressions, y_{ist} denotes the concentration of NO_2 or PM_{10} measured at hourly intervals. Both regression specifications include the same vector of weather controls. The vector of weather controls includes a third-order polynomial of air temperature, a second-order polynomial of precipitation, the interaction of temperature and precipitation, a second-order polynomial of wind speed, four dummies for wind direction (north, south, east, and west) as well as their interaction with wind speed. In the traffic regressions, standard errors are clustered at the weekly level, while for air pollution, standard errors are two-way clustered on week and station.

We also estimate a dynamic version of the DiD where we allow treatment effects to vary by year. Specifically, we estimate the following equation:

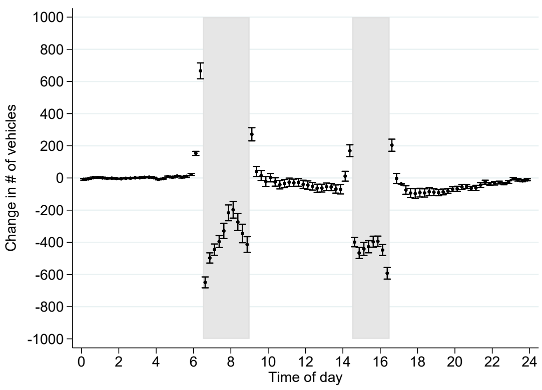
$$y_{ist} = \sum_{t=-2}^{3} [\beta_t Bergen \times year_t] + X'_{ist}\gamma + \sigma_s + \lambda_{ywd} + \theta_{dis} + \varepsilon_{ist}, \tag{2}$$

where the annual DiD estimate is captured by the coefficients β_t. The annual DiD estimates allow us to examine the pre-treatment trends, as well as examine how the treatment effects unfold over time.

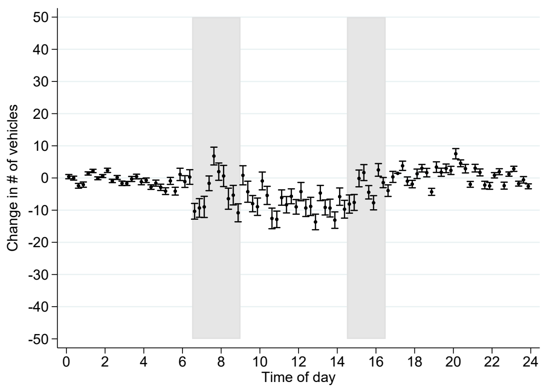
F.3 Results on driving into the city

Figure F.3: DiD estimates on traffic volume by 15 min. intervals and vehicle type

(a) Passenger cars



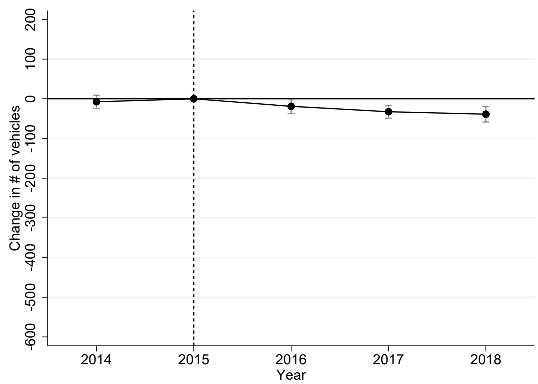
(b) Trucks



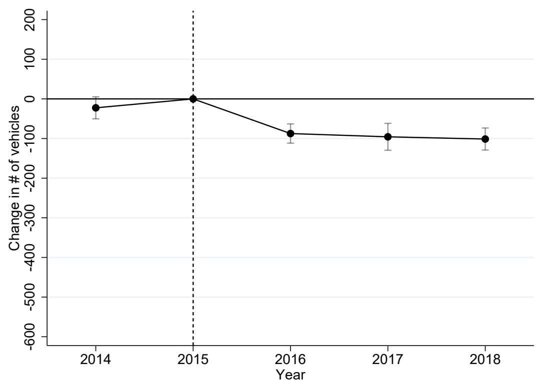
Notes: Figure plots treatment effects estimated from Equation 4, where regressions are run separately for each 15 minute increment. Panel (a) shows results for passenger cars and Panel (b) shows results for trucks. Whiskers indicate 95% confidence intervals. Gray shaded areas indicate rush hours. Traffic is measured as the total number of cars passing the toll cordon every 15 minutes. Passenger cars: all vehicles < 3500 kg. Trucks: all vehicles > 3500 kg. Standard errors are clustered at the week level and are not adjusted for multiple hypothesis testing.

Figure F.4: Annual DiD effects on traffic. Non-rush hours and all day

(a) Non-rush hours



(b) All day



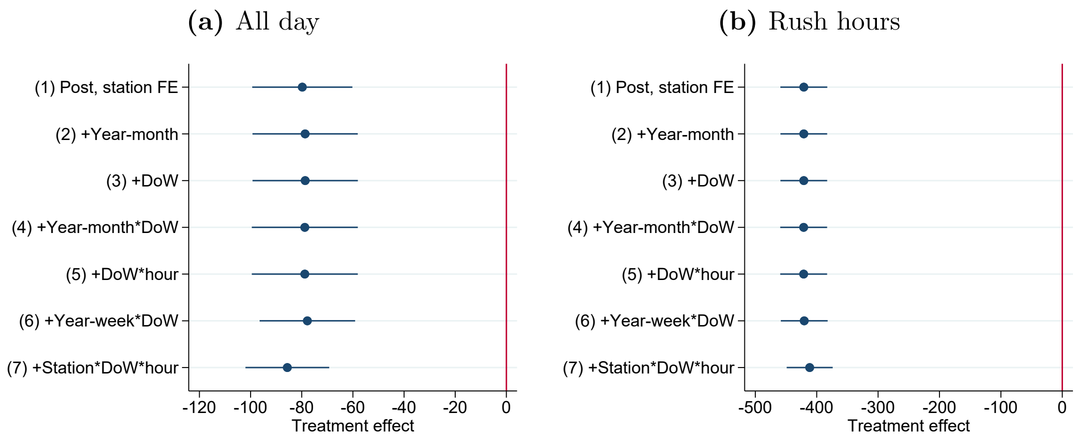
Notes: Figure plots the coefficients β_t estimated from the following regression: $y_{ist} = \sum_{t=-2}^{3}[\beta_t Bergen \times year_t] + X'_{ist}\gamma + \sigma_s + \lambda_{ywd} + \theta_{dis} + \varepsilon_{ist}$, where the sample is restricted to weekdays. Panel (a) shows effects for non-rush hours and Panel (b) shows effects for daily traffic. Traffic is measured as the total number of vehicles passing the toll gates into the city center during a 15-minute interval. Standard errors are clustered on week.

Table F.1: DiD estimates on traffic volume

Dependent variable: # vehicles/15 minute interval	All day (1)	Rush hours: All (2)	Rush hours: Morning (3)	Rush hours: Evening (4)	Non-rush hours: +/-30 min (5)	Non-rush hours: Other (6)
Panel A: All cars						
Bergen × Post	-85.54***	-410.2***	-380.3***	-448.0***	186.8***	-33.24***
	(8.157)	(18.48)	(21.25)	(16.78)	(15.71)	(5.336)
Mean depvar (pre, Bergen)	1,557	2,877	2,928	2,812	2,189	1,145
Change (%)	-5.50	-14.26	-12.99	-15.93	8.53	-2.90
Panel B: Passenger cars						
Bergen × Post	-83.13***	-406.3***	-375.9***	-444.7***	189.7***	-31.28***
	(7.880)	(17.96)	(20.65)	(16.29)	(15.30)	(5.154)
Mean depvar (pre, Bergen)	1,451	2,687	2,724	2,641	2,026	1,068
Change (%)	-5.73	-15.12	-13.80	-16.84	9.36	-2.93
Panel C: Trucks						
Bergen × Post	-2.412***	-3.892***	-4.386***	-3.245***	-2.951***	-1.968***
	(0.428)	(0.896)	(1.047)	(0.830)	(0.792)	(0.309)
Mean depvar (pre, Bergen)	106	190	204	172	163	77
Change (%)	-2.29	-2.05	-2.15	-1.89	-1.81	-2.55
N	387,456	72,648	40,360	32,288	32,288	282,520
Weather controls (X_{ist})	✓	✓	✓	✓	✓	✓
Station FE (σ_s)	✓	✓	✓	✓	✓	✓
Post	✓	✓	✓	✓	✓	✓
Day-of-week × week × year FE (λ_{ywd})	✓	✓	✓	✓	✓	✓
Day-of-week × time-of-day FE (θ_{dis})	✓	✓	✓	✓	✓	✓

Notes: Table shows the β coefficient estimated from Equation 4, running 18 (6 × 3) separate regressions. Dependent variable is the aggregate number of vehicles passing the toll gates in a city during a 15 minute interval. Column headings indicate the sample used in each regression. "Rush hours" refer to the intervals 06:30-08:59 (morning) and 14:30-16:29 (evening). For non-rush hours, "+/- 30 min" refers to the 30 minute intervals right before and after rush hours. "Other" refers to the remaining non-rush hours (i.e., 9:30-13:59 and 17:00-05:59). Sample is restricted to 730 days (2 years) pre and post policy implementation. Standard errors clustered at the weekly level in parentheses. * p<0.10, ** p<0.05, *** p<0.01.

Figure F.5: DiD estimates on traffic volume, by different model specifications

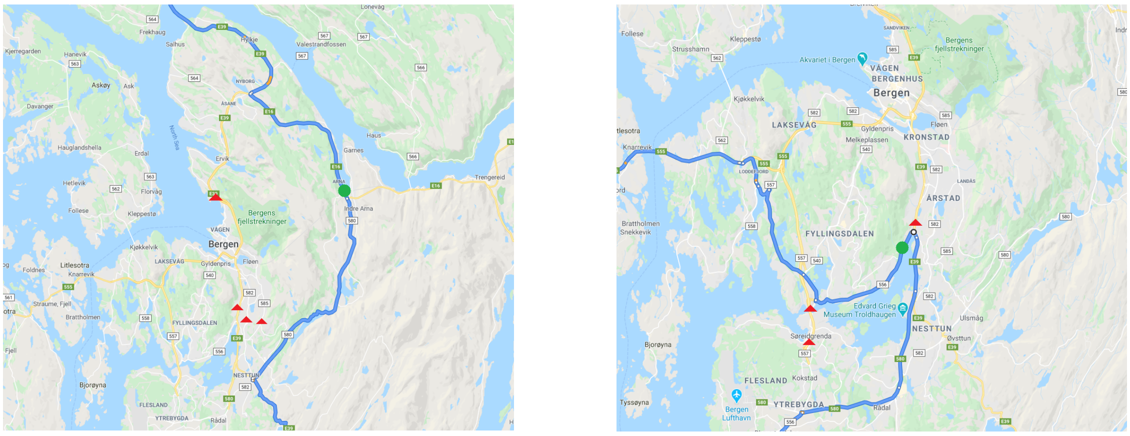


Notes: Figure plots treatment effects estimated from Equation 4. Each panel shows results from 7 separate regressions. Dependent variable is traffic measured as the number of vehicles during a 15 minute interval. Panel (a) shows daily average effects as fixed effects are progressively included. while Panel (b) shows results for rush hour traffic. Whiskers show 95 % confidence intervals. Except for the fixed effects, the model specification is the same as in the main traffic regression table (Table F.3). DoW is short for day of week. Standard errors are clustered on week.

F.4 Spatial spillovers

Figure F.6: Alternatives for bypassing toll gates for transit cars

(a) North-south direction **(b)** South-west direction



Notes: Figure shows the two main routes to avoid toll payments, depending on which direction the car is coming from. Blue lines are routes around Bergen that avoid the cordon toll completely. The toll gates avoided by the alternative routes are marked as red triangles. Green circles mark traffic censors from which we obtain traffic data. Cars passing Bergen in the north-south direction have two alternatives: either driving on E39 through the center of Bergen city and the cordon toll, or following the road E16/580 to the west of Bergen avoiding the cordon toll completely. This route is 6 minutes longer. Cars passing Bergen in the south-west direction are also able to avoid the toll cordon by taking roads E39/556 rather than the direct tunnel under the strait (road 557). This is a detour of 9 minutes for cars arriving from the south-west. However, for cars arriving from the south-east (e.g. from E39) this route is only about one minute longer. Source: Google Maps.

Table F.2: DiD estimates on traffic volume on two alternative routes

Dependent variable:	All day	Rush hours			Other
# vehicles per hour	0:00-23:59	Both	6:00-8:59	14:00-16:59	17-06, 09-14
	(1)	(2)	(3)	(4)	(5)
Panel A: North-south					
Bergen × Post	8.499	22.44	23.45	51.51	1.734
	(48.86)	(125.1)	(162.8)	(79.75)	(28.93)
N	56,957	14,236	7,109	7,127	42,721
Mean depvar (pre, Bergen)	721	1,284	974	1,590	534
Change (%)	1.18	1.75	2.41	3.24	0.32
Panel B: South-west					
Bergen × Post	104.1***	263.3***	342.2***	191.9***	49.89***
	(27.45)	(63.14)	(80.86)	(49.97)	(16.97)
N	71,913	17,980	8,989	8,991	53,933
Mean depvar (pre, Bergen)	959	1,771	1,527	2,015	688
Change (%)	10.86	14.86	22.40	9.52	7.25
Weather controls (X_{ist})	✓	✓	✓	✓	✓
Station FE (σ_s)	✓	✓	✓	✓	✓
Post	✓	✓	✓	✓	✓
Day-of-week × week × year FE (λ_{ywd})	✓	✓	✓	✓	✓
Day-of-week × time-of-day FE (θ_{dis})	✓	✓	✓	✓	✓

Notes: Table shows DiD estimates on traffic volume on two alternative routes for selected time intervals. The dependent variable is the total number of vehicles driving on the road in both directions. The specifications are the same as the main specifications in Table F.1, but time periods are defined differently since only hourly data is available. Standard errors are clustered on week. * p<0.10, ** p<0.05, *** p<0.01.

F.5 Results on air pollution

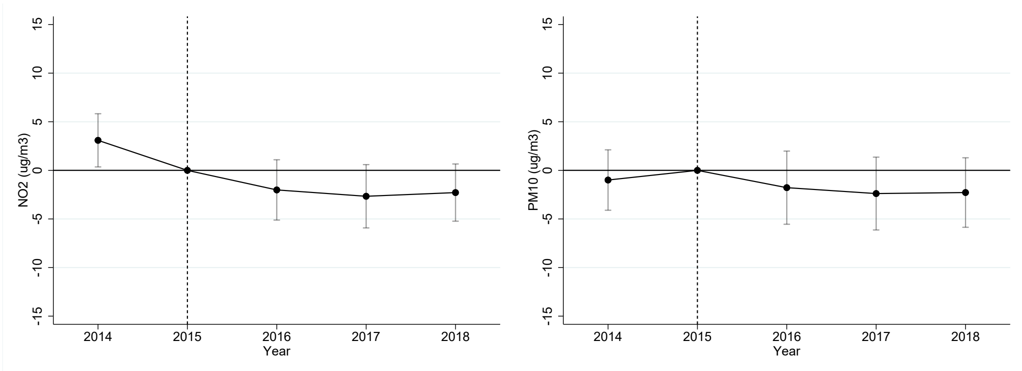
Table F.3: DiD estimates on NO_2 and PM_{10}

	24 hours	Daytime	Midday	Rush	Evening	Night
Dependent variable:	00-23	05-22	06-17	6-9,14-16	18-23	00-05
ambient air pollution (μg/m^3)	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: NO_2						
Bergen × Post	-4.126***	-5.027***	-6.077***	-6.363***	-3.091	-2.035*
	(1.341)	(1.601)	(1.673)	(1.740)	(1.765)	(1.072)
N	273,533	204,628	135,480	79,421	69,172	68,881
Mean depvar (pre, Bergen)	43.20	50.40	55.44	57.91	38.73	23.68
Change (%)	-9.55	-9.97	-10.96	-10.99	-7.98	-8.59
Panel B: PM_{10}						
Bergen × Post	-1.847	-2.236	-3.019	-2.665	-0.570	-0.639
	(1.452)	(1.704)	(1.750)	(1.683)	(1.827)	(0.850)
N	295,828	222,072	147,754	86,281	74,417	73,657
Mean depvar (pre, Bergen)	17.65	19.60	20.83	20.54	17.15	11.79
Change (%)	-10.46	-11.41	-14.49	-12.97	-3.32	-5.42
Weather controls (X_{ist})	✓	✓	✓	✓	✓	✓
Station FE (σ_s)	✓	✓	✓	✓	✓	✓
Post	✓	✓	✓	✓	✓	✓
Day-of-week×week×year FE (λ_{ywd})	✓	✓	✓	✓	✓	✓
Day-of-week×time-of-day FE (θ_{dis})	✓	✓	✓	✓	✓	✓

Notes: Table shows average daily treatment effects (column 1) as well as treatment effects for 5 different time intervals (columns 2-6) for NO_2 (Panel A) PM_{10} (Panel B). Table shows results from 12 separate regressions. NO_2 and PM_{10} are measured as mean levels of (μg/m3) during a 60 minute interval. Post × Bergen refers to the coefficient β estimated from Equation 4. Column headings indicate the sample used in each regression. Rush hours refers to the intervals 06:00-09:59 (morning) and 14:00-16:59 (evening). Non-rush hours, +/- 60 min refers to the 60 minutes right before and after rush hours (i.e., 05:00-05:59, 10:00-10:59, 13:00-13:59, 17:00-17:59). Non-rush hours, other refers to the remaining non-rush hours (i.e., 10:00-12:59 and 18:00-04:59). Sample is restricted to 2 years pre and post policy implementation. All fixed effects are interacted with a holiday dummy. Standard errors are two-way clustered on week and station.

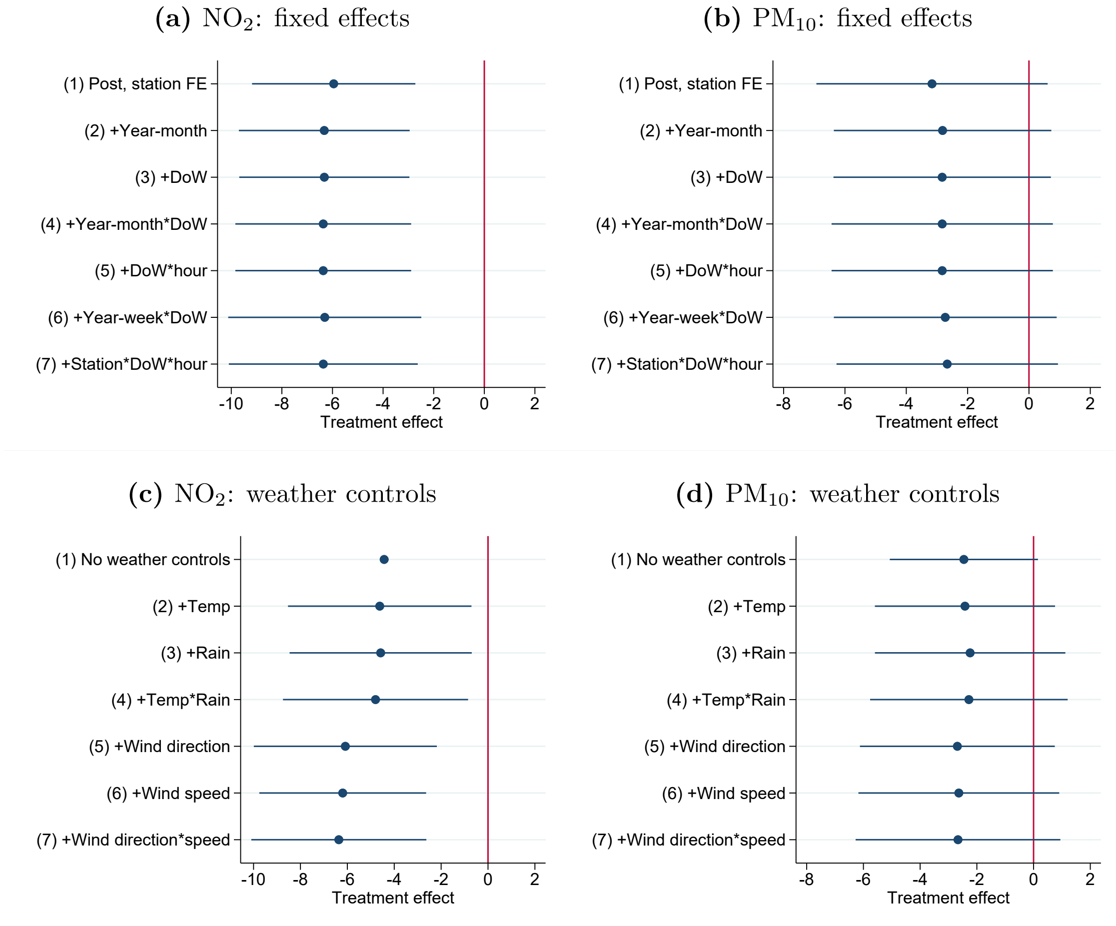
Figure F.7: Annual DiD effects on air pollution.

(a) Daily NO_2 **(b)** Daily PM_{10}



Notes: Figure plots β_t estimated from the following regression: $y_{ist} = \sum_{t=-2}^{3}[\beta_t Bergen \times year_t] + X'_{ist}\gamma + \sigma_s + \lambda_{ywd} + \theta_{dis} + \varepsilon_{ist}$. Sample is restricted to weekdays and to the years 2014–2018. Panel (a) shows results for daily NO_2 concentrations while Panel (b) shows results for daily PM_{10}. Pollution is measured as micrograms per cubic meter of air (μg/m3). Standard errors are two-way clustered on week and station.

Figure F.8: DiD estimates on rush hour air pollution (μg/m3). Different fixed effects and weather controls



Notes: Each panel shows results from 7 separate regressions. Dependent variable is rush hour air pollution measured as mean levels of NO_2 or PM_{10} (μg/m3) during a 60 minute interval. Panels (a) and (b) show robustness across fixed effects specifications, with additional fixed effects progressively included as one moves down the y-axis. Panels (c) and (d) show robustness across specifications with different weather controls, with additional controls progressively included when moving from (1) to (7). Whiskers show 95 % confidence intervals. DoW is short for day of week. Standard errors are two-way clustered on week and station.

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