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Congestion pricing, air pollution, and individual-level behavioural responses

Elisabeth T. Isaksen Bjørn G. Johansen*

June 2021

Abstract

This paper shows that differentiating driving costs by time of day and vehicle type help improve urban air quality, lower driving, and induce adoption of electric vehicles. By taking advantage of a congestion charge that imposed spatial and temporal variation in the cost of driving a conventional vehicle, we find that economic incentives lower traffic and concentrations of NO_2. Exploiting a novel dataset on car ownership, we find that households exposed to congestion charging on their way to work were more likely to adopt an electric vehicle. We document strong heterogeneous patterns of electric vehicle adoption along several socioeconomic dimensions, including household type, income, age, education, work distance and public transit quality.

Keywords: air pollution, electric vehicles, transportation policies, congestion charging
JEL codes: C33, H23, Q53, Q55, Q58, R41, R48

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1 Introduction

Transportation is a major contributor to urban air pollution and greenhouse gas emissions. Despite substantial improvements in the energy efficiency of vehicles, a long tradition of imposing air quality standards, and increased attention towards climate change mitigation, most countries around the world still struggle with the dual challenge of poor ambient air quality and high levels of carbon emissions from transportation (WHO, 2016; EEA, 2019).[1] While more ambitious policies are needed to curb emissions, imposing higher costs on driving is often met with substantial public opposition, where critics point to unfavorable distributional properties of such policies. Previous studies also show that regulations aimed at mitigating air pollution and other driving-related externalites can have unintended consequences (Davis, 2008; Auffhammer and Kellogg, 2011; Bento et al., 2014; Gibson and Carnovale, 2015), sometimes even leading to net welfare losses. Unintended consequences may arise due to drivers' substitution behavior, or by exploitation of policy loopholes. Understanding the impacts of transportation policies aimed at mitigating local and global externalities, as well as their distributional implications, is hence crucial in order to facilitate an efficient and equitable low-carbon transition in the transportation sector.

In this paper, we combine highly detailed data on air pollution, traffic, and car ownership to shed light on efficiency and equity impacts of a congestion charge that increased the costs of driving gasoline and diesel vehicles during rush hours. While command-and-control type of regulations such as low-emission zones and license plate-based driving restrictions are often used to combat urban air pollution, with mixed success (Davis, 2008; Wolff, 2014; Zhang et al., 2017; Zhai and Wolff, 2020), market-based policies such as congestion charging have recently been implemented in several major cities around the world (e.g., Stockholm, Zürich, Milan, London, Singapore). Still, there are few empirical studies exploiting quasi-experimental variation to estimate effects of these types of policies on travel behavior and emissions.[2] Are these types of market-based polices able to mitigate air pollution and induce a shift towards greener modes of transportation? Or are drivers simply substituting towards lower priced hours or roads, potentially leaving the total traffic volume unchanged? What are the distributional consequences of increasing the price of driving a high-emission vehicle, and to what extent are low-income households able to adapt

[1]According to the World Health Organization (WHO), over 90% of the world's population live in places where air quality levels exceeds the health-based guidelines (WHO, 2016).

[2]Notable exceptions include e.g., Gibson and Carnovale (2015) and Simeonova et al. (2019). A more comprehensive literature review is provided towards the end of the introduction.

by adopting costly electric vehicles exempted from congestion charging?

To examine these issues, we exploit a congestion charge implemented in 2016 in the second largest city in Norway (Bergen) that raised the price of entering the city center toll cordon during rush hours by 80 %. The congestion charge only applied to weekdays, and only to gasoline and diesel vehicles. While the main goal of congestion charging is usually to lower traffic volumes during rush hours, the Bergen congestion charge was to a large extent motivated by an aim of improving air quality and to speed up the adoption of battery-electric vehicles (electric vehicles in the following), which have been exempted from paying congestion charges and road toll in Norway since 1997.[3] The policy hence increased the relative price of driving a high- vs. low-emission vehicle. Before 2010, access to high-quality electric vehicles were limited, and polices favoring these cars likely had a modest impact on adoption. However, with the roll-out of several high quality models over the past decade, electric vehicles have become a feasible option, thereby expanding the opportunity set of drivers (Figenbaum et al., 2015). Given the exceptionally high market penetration of electric vehicles in Norway, the Bergen congestion charge makes for an interesting study case to examine the margins of adjustment when drivers face a time-of-day and vehicle-specific charge on driving.[4]

As a first step, we examine the overall effect of the congestion charge on traffic volume and ambient air quality using high-frequency sensor and monitoring station level data. To identify causal effects of the policy, we exploit two sources of variation across time: pre and post policy and weekday vs. weekend.[5] Results from the empirical examination show a negative and significant effect on both traffic volume and air pollution; increasing the rush hour rate of entering the toll cordon by around 80 % led to a 14 % decrease in cars entering the congestion zone during rush hours and an 11 % reduction in concentrations of NO_2 during midday hours.[6] While we find evidence of inter-temporal substitution towards the 15-30 minutes right before and after rush hours, as well as spatial substitution towards lower priced roads, the overall change in traffic is dominated by the large reductions on treated roads during rush hours. These findings suggest that drivers primarily substituted towards other modes of transportation. Averaging effects over the course of a day, we find

[3]From 2019 and onward the policy was changed, and electric vehicles were charged with ∼20 percent of the standard rate. This policy change, however, is outside the time frame of our dataset.

[4]In the first quarter of 2019, over 50% of all new passenger vehicles sold in Norway were electric vehicles; see elbil.no. In the year of the congestion charge implementation (2016), the share of electric vehicles of all new passenger vehicles sold in Norway was around 16%.

[5]The congestion charge was only active during weekdays.

[6]We find similar sized effects for NO_2 when applying a difference-in-differences specification that exploits variation across cities instead of weekday vs. weekend. A similar specification is not feasible for traffic volumes due to lack of comparable data across cities.

that daily traffic volume on rush-hour priced roads decreased by around 4.8 % and ambient levels of NO_2 decreased by 6.5 % (or 3.1 μg/m^3). We find a similar-sized percentage decline in PM_{10}, but estimates are too noisy to draw firm conclusions. As PM_{10} is largely generated from wear and tear from roads, tires and break blocks rather than vehicle exhaust, a change in fleet composition towards electric vehicles is likely to lower NO_2 concentrations but not necessarily PM_{10}.

To further examine behavioral responses to the congestion charge, we exploit a novel data set that combines registry data on the full population of cars in Norway with detailed socioeconomic information on households, including the neighborhood-level location of individuals' home and workplace. Combining this exceptionally detailed data with information on the road network and the location of toll gates, we are able to identify the toll payments faced by each individual household when traveling between home and work – provided that they choose the shortest route. Based on these datasets, we construct treatment and control groups in a triple differences framework. Specifically, we define the treatment group as households exposed to congestion charging on their way to work and the control group as households where the work route does not have toll gates.[7] We then compare the treatment and control groups pre and post policy and across two similar-sized cities in Norway (Bergen and Stavanger), where Stavanger serves as the "placebo" case. By comparing the development of similar types of households across two cities, we are able to control for differential, time-varying effects of the increased availability of electric vehicles on households that pay and do not pay road toll. Identification is further strengthened by the inclusion of neighborhood-year level fixed effects, household level demographics and travel time between home and work with both car and public transit.

Results from the empirical examination suggest that households respond to the congestion charge by substituting towards electric vehicles. We find that households exposed to the Bergen congestion charge were around 4.2 percentage points more likely to adopt an electric vehicle. This estimated treatment effect explains around 1/3 of the increase in electric vehicle adoption in the treatment group from 2014 to 2017.[8] Further, we find that the positive effect on electric vehicle adoption is mirrored by a negative effect on the adoption of gasoline and diesel vehicles, leading to a close to zero effect on the total number of cars owned by a household. This

[7]This definition serves as a proxy for the overall costs faced by households from congestion charging.

[8]From the end of 2014 to the end of 2017, the share of toll-paying commuters in Bergen that owned an electric vehicle increased by 13 percentage points, from 4.7 percent to 17.7 percent. In the absence of the congestion charge, we predict that the electric vehicle share in 2017 would have been 13.5 percent.

suggests that households, on average, replaced their fossil fuel car by an electric one.

Examining heterogeneous effects, we find strong gradients along several socioeconomic dimensions. While the policy had no effect on electric vehicle adoption among households in the lowest income quintile, the electric vehicle share for households in the highest income quintile increased by around 7 percentage points as a consequence of the policy. We also find that treatment effects are larger for university-educated couples with kids, and for households with a longer work commute and poor public transit quality. The latter implies that the quality of transportation substitutes plays a key role in households' adaptation responses. While the heterogeneous effects may be explained by differences in preferences, parts may be due to financial constraints in purchasing an electric vehicle.

Overall, our findings on car ownership suggest that congestion charging combined with exemptions for electric vehicles can be a powerful tool to promote electric vehicle adoption, but that there are systematic differences in how households respond to the policy. Back-of-the-envelope welfare calculations suggest that the policy led to a net welfare gain with a benefit to cost ratio of around 3:1.

The magnitude of our treatment estimates must be seen in context of Norway's other existing electric vehicle incentives, such as exemptions from purchasing tax and value-added tax. These strong financial incentives have contributed to an exceptionally high market share of electric vehicles in Norway and a relatively well-developed charging infrastructure. In absence of these favorable conditions, we would likely have seen a lower effect of the congestion charge on electric vehicle adoption. Despite the specific features of our research context, we argue that our findings may help shed light on expected impacts of congestion charging in other countries in a *future* scenario where electric vehicles are more competitive to internal combustion engine vehicles, e.g. due to policies or technological improvements, and the charging infrastructure more developed than today.

Our paper complements the empirical literature on the effects of transportation polices on air pollution, congestion, and other driving-related externalities. Previous studies have shown that e.g., low emission zones, road tolls and congestion charges can help improve urban air quality (Wolff, 2014; Gibson and Carnovale, 2015; Fu and Gu, 2017; Gehrsitz, 2017; Simeonova et al., 2019; Pestel and Wozny, 2019; Zhai and Wolff, 2020), with resulting health benefits such as lower asthma rates in children (Simeonova et al., 2019), lower infant mortality (Currie and Walker, 2011), and fewer hospital admissions related to chronic cardiovascular and respiratory diseases (Pestel and Wozny, 2019). While these studies provide important estimates on environmental and health effects of transportation policies, very few studies combine highly detailed data with a quasi-experimental design to examine underlying mechanisms through which individuals respond to these policies, as well as how these mechanisms differ across households. The majority of papers also focus on command-and-control instruments; by contrast we provide estimates on the effects of a marked-based policy implemented in several major cities over the past decade.

Our paper also contributes to a small but growing quasi-experimental literature on electric vehicle adoption. Existing studies focus on the effects of purchasing subsidies (Muehlegger and Rapson, 2018; Clinton and Steinberg, 2019), charging infrastructure (Li et al., 2017) and low emission zones (Wolff, 2014) on new vehicle registrations, usually at the zip-code, metropolitan, or state level. By contrast, we examine effects of a congestion charge paired with electric vehicle exemptions on household-level car ownership. Compared to previous studies, we use exceptionally detailed data, were we are able to locate the residence and workplace of each individual living in Norway. This allows us to construct a policy exposure measure that vary substantially across space and time, which helps to develop a more credible identification strategy. Further, by using data on households' car portfolio rather than just new car sales, we are able to examine whether the policy increased or decreased the total number of cars – a crucial aspect to understand the net environmental and climate benefits of electric vehicle incentives.

The remainder of the paper is organized as follows: Section 2 provides background information on the policy and the broader institutional setting. Section 3 describes the data and results for pollution and traffic. Section 4 describes data and results for household-level transportation behavior. Section 5 provides a discussion of the net welfare effects and distributional concerns. Section 6 concludes.

2 Background

The congestion charge in Bergen was announced in February 2015 and implemented one year later, on February 1st 2016; see Table 1. The congestion charge was electronically collected via the existing automated toll gates in and around the city center of Bergen; see Figure 1. Before implementation, small passenger vehicles passing the toll cordon paid an amount of NOK 25 (∼\$3) irrespective of time of day. After the introduction of congestion pricing, small passenger vehicles faced a rush-hour rate of NOK 45 (∼\$5.4) in the hours 06:30-09:00 and 14:30-16:30, equivalent to an 80 % price increase. The rush hour rates were only active on weekdays. Rates in non-rush hours were lowered to NOK 19 (∼\$2.3), representing a 24 % price decrease. Vehicles were charged when entering the toll cordon. If a vehicle passed

Table 1: Congestion charging in the city of Bergen

Date implemented	Feb 1, 2016
Date announced	Feb 18, 2015
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: Rates are given in NOK. 10 NOK ≈ 1 EUR and ≈ 1.2 USD. Rates correspond to the levels at the time of implementation and reflect rates faced by small passenger vehicles (< 3500 kg) . For large vehicles (> 3500 kg) the price was 50 NOK before Feb 1st 2016, and 90 NOK during rush hours (38 NOK outside rush hours) after policy implementation. Battery electric vehicles were exempted from the congestion charge and toll rates throughout the period analyzed. Hybrid electric vehicles were subject to the same rates as internal combustion engine vehicles (ICEVs). Appendix Figure A.3 shows the development of toll rates in Bergen over the period 2005 to 2017.

the toll cordon several times within an hour, it was only charged once.[9] Battery-electric vehicles were exempted from toll rates both before and after the introduction of the congestion charge. The congestion charge hence further increased the relative cost of driving a diesel or gasoline vehicle compared to a battery-electric vehicle.[10]

While the main goal of rush hour pricing is usually to mitigate congestion, the introduction of the Bergen congestion charge was to a large extent motivated by air quality concerns. In the years leading up to implementation, Bergen together with a handful of larger cities in Norway struggled with poor urban air quality, and in 2015 Norway was convicted in the EFTA court for violating EU's ambient air quality standards in several parts of the country.[11] The majority of the violations were linked to excess concentrations of NO_2 in urban areas, where exhaust from road traffic is usually a major source; see Section 3.1 for details. As a consequence of the court decision, Norway was required to initiate measures to meet the requirements of the EU Air Quality Directive.

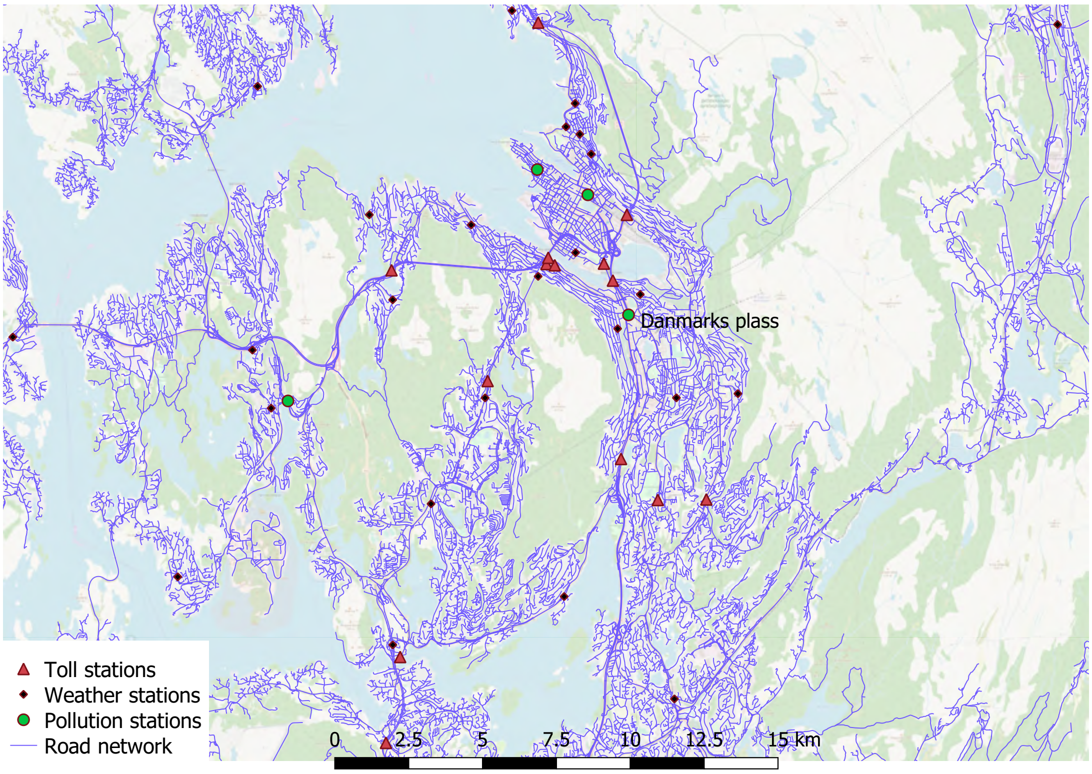
Beyond meeting air quality requirements, the introduction of the congestion charge was also seen as an instrument to lower CO_2 emissions and facilitate the shift towards greener modes of transportation. As almost 98 % of Norway's electricity

[9]There was also a monthly cap on the overall cost per vehicle. Once the cap was reached, the vehicle was allowed to enter the toll cordon free of charge. However, this cap was set too high to be binding for regular commuters.

[10]See e.g., NPRA (2018) for more details on the policy, and for a descriptive analysis of traffic volumes after the introduction of the policy. Congestion charging has also been implemented in four other cities in Norway; see Appendix Table A.1. In this paper, we focus on the Bergen congestion charge as we either lack sufficient air pollution data on the other cities, or lack information on car ownership for the post period.

[11]See e.g., regjeringen.no and nrk.no.

Figure 1: Map of toll gates, pollution monitoring stations and weather stations in Bergen



Notes: The map shows toll gates, weather stations, and pollution monitoring stations in and around the city centre of Bergen. The only pollution monitoring station with a sufficiently long time series to use in the analysis is the monitoring station labeled as *Danmarks plass*. See Appendix A for additional maps of Bergen and the road network.

production is renewable (Statistics Norway, 2020), electric vehicles cause very low indirect CO_2 emissions from driving. Norway has an ambitious goal of increasing the market share of electric vehicles to 100 % by 2025 (NTP, 2017, p. 224), and the congestion charge exemption was one of many benefits granted to electric vehicle owners over the time period analyzed. At the national level, electric vehicles are exempted from purchase taxes and 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. See Appendix Table A.3 for a complete list of electric vehicle incentives.

The strong incentives have contributed to an exceptionally high market share of electric vehicles in Norway - the highest in the world in 2017 (IEA, 2018). The high share has been facilitated by a dramatic increase in the supply of electric vehicle models since 2010.[12] While many of the policies promoting electric vehicles have been in place since the 1990s, their impact likely increased as several high-quality

[12]See Figenbaum et al. (2015) for an overview of electric vehicles introduced in the Norwegian market.

electric vehicle models became available. When estimating effects of the congestion charge on individual-level behavior, it will therefore be important to control for the potentially differential time trends across households with different levels of exposure to electric vehicle benefits. For instance, households paying road toll on their way to work will have a stronger incentive to adopt an electric vehicle, and the response is likely to increase as more electric vehicles become available - also in the absence of an *increase* in road toll. In this paper, we aim to disentangle the effect of the Bergen congestion charge from other policy and technology trends by constructing a control group that faced similar local and national policies and incentives - with the exception of congestion charging.

The introduction of congestion charges in Norway has sparked a lot of public discontent, where critics often point to unfavorable distributional properties of the policy. As all drivers face the same rate, those from lower-income households will necessarily spend a larger share of their overall budget if similarly exposed to the congestion charge. Critics have also pointed to the lack of high-quality substitutes for many households, locking them into existing behavioral patterns. Purchasing a battery-electric vehicle to avoid road toll and congestion charges is still out of reach for many households – despite the large tax exemptions. In the spring and summer of 2019, there were several mass protests around the country against higher toll rates and congestion charging, which resulted in the formation of a new, single-cause political party pledging to remove all toll gates. The new “road toll party” got a substantial share of the votes in the local elections in the fall of 2019, leading to the cancellation of planned congestion charges and new toll gates.[13] Despite claims of the policy disproportionately harming low-income households, there is little evidence on how individuals actually adapted to the policy and to what extent they seemed to be locked into behavioral patterns due to e.g., income and limited public transit options.

3 Part I: air pollution and traffic volume

What was the effect of the congestion charge on rush hour and daily traffic? And to what extent did the policy improve ambient air quality? In the following, we examine these questions using high-frequency data from traffic sensors and pollution monitoring stations.

[13] See e.g. https://www.nrk.no/osloogviken/bomringen-i-drammen-skrotes-1.14560632 (accessed September 11, 2020; Norwegian only).

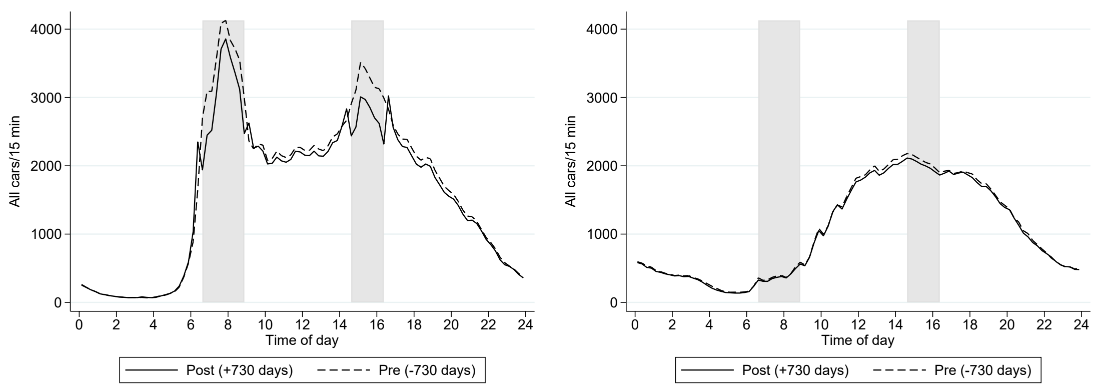
3.1 Data and descriptives

3.1.1 Traffic volume

To investigate effects of the congestion charge on traffic, we collect sensor level data on traffic volume and composition from the local road toll company in Bergen (Ferde). A map of the 14 automated toll gates, which indicate the congestion charging area, is provided in Figure 1. The sensor level data contains information on all cars passing the automated toll road gates in the period 2014 to 2018, with a 15-minute resolution. The number of cars within each 15 minute interval is further split into vehicles weighing less than 3.5 tonnes (referred to as “passenger vehicles”) and vehicles weighing 3.5 tonnes or more (refereed to as “trucks”).[14] In the main analysis, we focus on the total number of cars passing any toll gate in or out of Bergen in a given time period. By aggregating traffic to the city level, we eliminate the toll gate dimension of the data and are left with a high-frequency time series of total traffic.[15] Further, we focus on a period covering two years before and two years after the congestion charge was implemented, i.e., Feb 1 2014 to Feb 1 2018. This ensures that toll rates were constant in the pre-treatment period; see Appendix Figure A.3.

Figure 2: Traffic volume two years before and after Feb 1 2016

(a) Weekday **(b)** Weekend



Notes: Figures show the average number of vehicles passing the toll cordon over the course of a day based on 15 minute intervals. Panel (a) shows averages for weekdays (Monday-Friday) and panel (b) shows averages for the weekend (Saturday-Sunday). 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). Note that the congestion charge was not active during weekends. See Appendix Figure B.1 for traffic volume presented separately for passenger vehicles and trucks. See Appendix Figure B.2 for similar figures using a period of 365 days pre/post policy implementation. See Appendix Figure B.3 for traffic volume 365 days pre and post Feb 1 2015 (“placebo intervention”).

[14] Note that vehicles weighing less than 3.5 tonnes also consist of taxis, vans, and service vehicles. Vehicles weighing 3.5 tonnes or more also consist of buses and emergency vehicles in addition to trucks.

[15] Main results are not sensitive to the level of aggregation, as shown in Appendix B.2.3.

Figure 2 shows how vehicles passing the toll gates are distributed over the course of a day two years before (dashed line) and two years after (solid line) the congestion charge was implemented. Panel (a) clearly indicates that traffic volumes peak around rush hours, and that rush hour traffic declined in the two years after policy implementation.[16] The figure also shows 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. However, this substitution towards non-rush hours seems to be limited to a 15 minute interval before and after rush hours. The traffic pattern during weekends, when the congestion charge was not active, looks very similar in the two years before and after the policy; see panel (b) in Figure 2.[17]

3.1.2 Air pollution

To examine effects of the policy on ambient air quality, we collect hourly data on atmospheric pollution for the period 2014-2018 from the Norwegian Institute for Air Research (NILU), which operates a number of air monitors across Norway. Air pollution is measured as micrograms per cubic meter of air ($\mu g/m^3$). Figure 1 shows a map with the location of monitoring stations in the inner city of Bergen. While there are several monitoring stations located within the congestion zone, only one monitoring station has a sufficiently long time series to examine effects on the policy (the station labeled *Danmarks plass*). We hence limit the analysis to this station only when estimating effects on air pollution.

In the analysis, we focus one two key air pollutants: nitrogen dioxides (NO_2) and particulate matter with a diameter between 2.5 and 10 micrometers (PM_{10}).[18] NO_2 is one of a group of highly reactive gases known as nitrogen oxides (NO_x). The most important source of NO_2 in Norway is exhaust from vehicles with an internal combustion engine, i.e., gasoline and diesel vehicles (NILU, 2019).[19] High ambient levels of NO_2 is usually an urban phenomenon, and in the period analyzed several of the larges cities in Norway violated the national air quality standards for NO_2. Epidemiological studies have documented several adverse health effects of exposure to NO_2, such as aggravation of asthma and bronchitis, impaired respiratory functions, and mortality (see e.g., Lipsett et al., 1997; Shima and Adachi, 2000).

[16]The reduced traffic during rush hours are primarily driven by passenger vehicles; see Appendix Figure B.1.

[17]See Appendix B.1 for additional descriptives.

[18]These are the two pollutants most relevant to road traffic, and are also the ones where we have sufficient data to perform the analysis. An overview of the relative contribution of different sources to six different air pollutants is provided in Appendix Table A.2.

[19]Other less important sources of NO_2 are manufacturing industry and ship traffic.

Recent studies also suggest that NO_2 have adverse health effects also for ambient levels well below national ambient air quality standards (Simeonova et al., 2019; Breivik et al., 2020).

Particulate matter is a mixture of solid particles and liquid droplets such as dust, dirt, soot, or smoke. PM is usually divided into two categories according to the size of the particles. PM_{10} refers to particles with diameters between 10 and 2.5 micrometers.[20] The most important sources of PM_{10} in urban areas in Norway are wear and tear from roads, car tires and break blocks, sand added to roads to increase friction of icy surfaces in the winter, and wood-fired ovens (NILU, 2019). The epidemiological literature has documented several adverse health effects from exposure to PM_{10} and $PM_{2.5}$, such as premature death in people with heart and lung disease, aggravated asthma, and decreased lung growth and lung function in children (see e.g., Avol et al., 2001).

Figure 3 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. Gray shaded areas indicate rush hours. Both pollutants show a peak during the weekday morning rush, with a more pronounced peak for NO_2. By comparing the average pollution levels pre and post policy, we see that there is a clear decline in ambient air pollution on both weekdays and weekends. However, the decline in $\mu g/m^3$ seems to be largest for weekdays.[21] 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). See Appendix C.1 for additional descriptives and summary statistics.

3.1.3 Weather

To control for the effects of weather on traffic and 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.[22] Additionally,

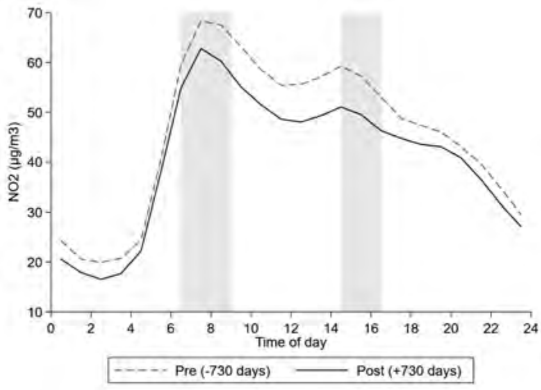
[20] $PM_{2.5}$ refers to particles with a diameter of 2.5 micrometers or smaller.

[21] If we restrict the sample to one year pre and post policy implementation, there appears to be no reduction in NO_2 on weekends and a large reduction on weekdays; see Appendix Figure C.2. For PM_{10} there is a similar, but less striking pattern.

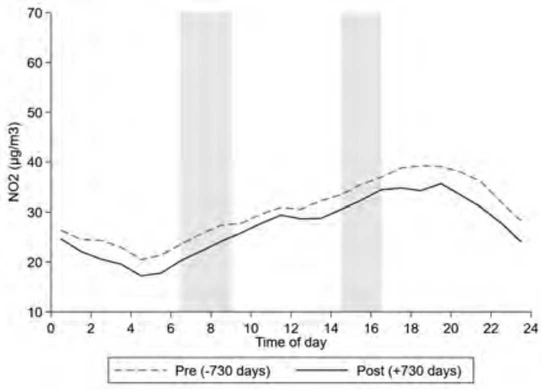
[22] Categorization is based on wind direction in degrees. Northern $\in$ [0-45] and (315,360], Eastern $\in$ (45,135], Southern $\in$ (135,225], Western $\in$ (225,315].

Figure 3: Air pollution two years before and after Feb 1 2016

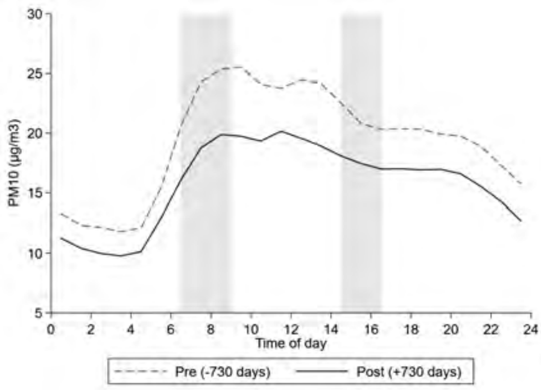
(a) Weekday NO_2



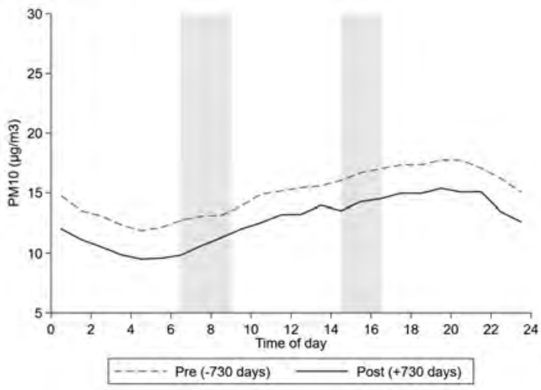
(b) Weekend NO_2



(c) Weekday PM_{10}



(d) Weekend PM_{10}



Notes: Figure shows average ambient air pollution over the course of a day for the pollution monitoring station located at Danmarksplass in Bergen. Values are based on 60 minute intervals. Panels (a) and (c) show averages for weekdays (Monday-Friday) and panels (b) and (d) show averages for weekends (Saturday-Sunday). 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. Note that congestion charging is not active during weekends. Pollution is measured as micrograms per cubic meter of air(μg/m3). See Appendix Figure C.3 for ambient air pollution one year pre and post Feb 1 2015 ("Placebo intervention"). See Appendix Figure C.2 for similar figures using a period of one year pre and post policy intervention.

we collect data on temperature inversion episodes in Bergen from the Nansen Environmental and Remote Sensing Center.[23] The dataset contains temperature for Bergen recorded at different altitudes, allowing us to identify inversion episodes, i.e., periods in which the temperature is increasing in altitude. As cold air is heavier than warm, air inversion episodes tend to reduce air circulation close to the surface and thus trap the pollutants produced by vehicles (and other sources) close to the ground. In the 2 years pre and post policy, inversion episodes occurred for around 4 % of the hourly observations; see Appendix C.1.

[23]The data is available from the following website: https://veret.gfi.uib.no/?action=download.

3.2 Empirical strategy

In this subsection, we provide an empirical framework to help estimate a causal effect of the congestion charge on traffic volume and air pollution. While the descriptive evidence presented in Section 3.1 suggest a decline in traffic and air pollution after the introduction of the congestion charge, the reduction might have been due to other factors than the policy, such as weather conditions, the use of wood-fired ovens, road construction, the supply of low and zero emission vehicles, etc. To identify the causal (short-run) effect of the policy, we employ a differences-in-differences (DiD) framework, where we exploit the fact that rush hour charges were not active during weekends.[24] By defining weekdays as our treatment observations and weekends as our control observations, we mitigate the risk of estimates being confounded by other factors that change over time and affect traffic or pollution simultaneously. The key identifying assumption is that changes in omitted time-varying variables, such as unobserved technological trends, economic activity and local policy initiatives, affect weekday and weekend traffic and air pollution similarly.

Our main regression equation can be written as:

$$y_{ikt} = \beta \text{post}_t \times \text{weekday}_t + X'_{it}\gamma + \lambda_{ym} + \theta_{di} + \varepsilon_{ikt}, \tag{1}$$

where y_{ikt} denotes the outcome of type k observed at time interval i on date t; X'_{it} is a vector of weather controls; λ_{ym} denotes year×month fixed effects; θ_{di} denotes day-of-week×time-of-day fixed effects; and ε_{ikt} is the idiosyncratic error term. Finally, post_t is a dummy variable equal to 1 after February 1st 2016, and weekday_t is a dummy variable equal to 1 during weekdays, meaning that β is the coefficient of interest. Seasonal variation and long-term time trends are absorbed by λ_{ym}, while pre-policy weekday-weekend differences are absorbed by θ_{di}. Thus, the variation left to identify $\hat{\beta}$ is the pre-post difference between weekdays and weekends. In the main specifications standard errors are clustered at the weekly level.

For the main traffic regressions, y_{ikt} denotes the total traffic volume of type $k \in \{\text{all vehicles, passenger vehicles, trucks}\}$ passing the toll cordon in Bergen during a 15 minute interval. For air pollution regressions, y_{ikt} denotes the concentration of pollutant $k \in \{NO_2, PM_{10}\}$ measured at hourly intervals. Both regression specifications include the same vector of weather controls.[25] Our main specification is

[24] Looking at Figure 2, panel (b) it does not seem like the traffic pattern during weekends changed visibly pre vs. post policy implementation.

[25] While weather controls are arguably more important in the air pollution regressions, weather conditions may also affect traffic volume. For consistency reasons, we use the same vector of weather controls in both regressions: three polynomials of air temperature; two polynomials of precipitation; the interaction of temperature and precipitation; two polynomials of wind speed;

based on a sample consisting of 730 days (two years) before and after the policy implementation. This is the longest time interval our data allows that ensures that seasonal trends in traffic or pollutants are balanced pre and post. We remove irregular days, such as vacations and summer months. Alternative specifications and robustness checks are provided in Appendix B.2.

We consider our DiD strategy to give us a conservative treatment estimate, for at least two reasons. First, as some pollutants can stay in the air for several hours (depending on weather conditions among other factors), a policy-induced reduction in weekday pollution may lower ambient levels of air pollution on weekends as well. In the presence of such positive spillovers from weekdays to the weekend, we expect our DiD strategy to downward bias treatment effects. Second, if the policy leads to behavioral changes that are carried over to weekends, our treatment estimate will difference out these effects. For instance, one might suspect that the policy led to more cycling, walking and use of public transit, and that individuals that got accustomed to these modes of transportation were also more inclined to change their behavior during weekends. Furthermore, if households bought electric vehicles as a response to the policy, they likely also used these vehicles during weekends. Our DiD estimate should therefore be interpreted as a lower bound on the causal effect of the policy.[26]

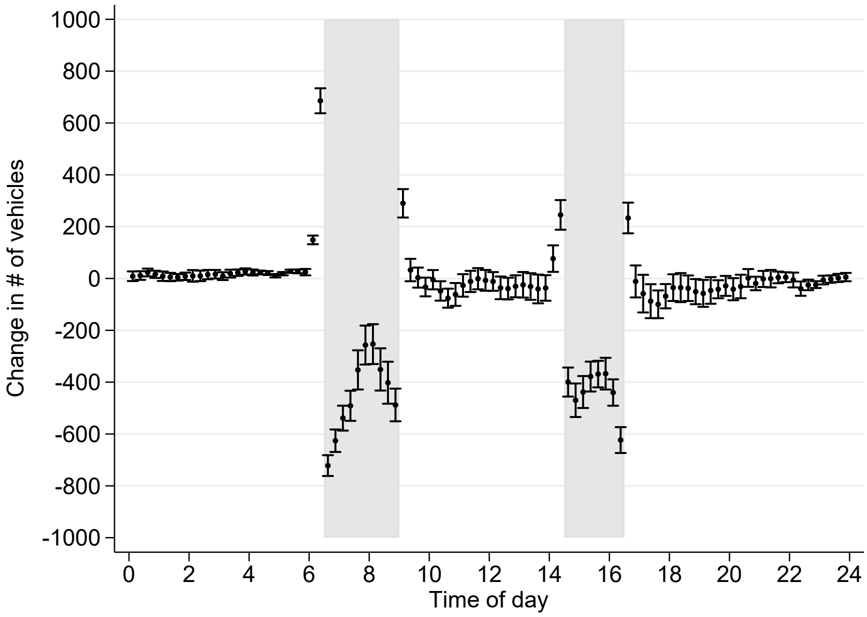
3.3 Results on traffic volume

Figure 4 displays the estimated treatment effects of the policy, where estimates are allowed to vary by 15 minute increments. Comparing these 96 different treatment effects allows us to identify the time intervals with the largest treatment effects, as well as explicitly examine intertemporal substitution. The figure clearly follows the same pattern as Figure 2; traffic shows a sharp decline during the morning and evening rush hours and an increase in 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. These findings imply that the policy worked as intended, by inducing drivers to either change their mode of transportation or substitute towards lower priced hours. The increase in traffic right before and after rush hours are clearly dominated by the reduction during rush hours, implying an overall reduction in daily traffic.

four dummies for wind direction (north, south, east and west) as well as their interaction with wind speed; and finally, a dummy for inversion episodes. We estimate two sets of each of these weather control variables; one for weekdays and one for weekends.

[26]For air pollution, the data permits us to present an alternative DiD estimate based on differences across cities and over time; see Appendix C.3 for details.

Figure 4: DiD estimates on traffic volume by 15 min. intervals. 2 years pre/post



Notes: Figure plots treatment effects estimated from from equation 1, where regressions are run separately for each 15 minute increment. Whiskers indicate 95% confidence intervals. Standard errors are not adjusted for multiple hypothesis testing. Gray shaded areas indicate rush hours. Traffic is measured as the total number of cars passing the toll cordon every 15 minutes. Standard errors are clustered at the week level.

Table 2 shows the average daily treatment effect (column 1), as well as treatment effects for five different time periods of the day (columns 2-6). Overall, the congestion charge led to a 4.8 % reduction in daily traffic volume during weekdays (around 78 vehicles per 15 minute interval, or 7,456 vehicles per day).[27] Note that this estimated effect incorporates intertemporal spillovers within a day. Columns (2)-(4) indicate that traffic during rush hours was significantly reduced by 14.4 % (around 447 vehicles per 15 minute interval). Column (5) shows a 9 % increase in traffic in the 30 minutes before and after the morning and evening rush, indicating intertemporal substitution. Column (6) shows a small reduction in traffic during other non-rush hours (1.3 %) that is significant at the 10 % level.[28] See Appendix Table B.1 for results split by passenger vehicles and trucks.

To illustrate the magnitude of the intertemporal substitution, we translate the 15 minute effects to the total increase or decrease in the number of vehicles within a given time period. While rush hour traffic decreased by (447×18 quarters =) 8,046 cars, there was an increase of (207.8×8 quarters =) 1,662 cars in the 30 minutes

[27] 77.67 cars per quarter × 96 quarters per day = 7,456 cars per day.

[28] This might be related to the small reduction in fees outside rush hours. However, the reduction in fees outside rush hours also applies to weekends, and will therefore to some degree be differenced out. Note, however, that traffic during weekends did not seem to decrease (see Figure 2. Thus, it seems like the reduction in fees outside rush hours had limited impact on overall traffic volume.

Table 2: DiD estimates on traffic volume. 2 years pre/post

		Rush hours			Non-rush hours	
Dependent variable: # vehicles/15 minute interval	All day (1)	All (2)	Morning (3)	Evening (4)	+/-30 min (5)	Other (6)
Post × weekday	-77.67*** (9.889)	-447.0*** (23.18)	-445.7*** (29.33)	-436.9*** (25.68)	207.8*** (19.89)	-15.42* (7.971)
Observations	87518	16416	9122	7294	7294	63808
Mean depvar (pre, weekday)	1632	3104	3239	2936	2316	1175
Change (%)	-4.76	-14.40	-13.76	-14.88	8.97	-1.31
Weather controls ($X'_{it}\gamma$)	✓	✓	✓	✓	✓	✓
Month × year FE (λ_{ym})	✓	✓	✓	✓	✓	✓
Day-of-week × time-of-day FE (θ_{di})	✓	✓	✓	✓	✓	✓

Notes: Table shows results from 6 separate regressions. Dependent variable is vehicles passing toll gates in Bergen during a 15 minute interval. Post×weekday refers to the β coefficient estimated from Equation 1. 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 pre and post policy implementation. Standard errors clustered at the weekly level in parentheses. * p<0.10, ** p<0.05, *** p<0.01. Standard errors are not adjusted for multiple hypothesis testing.

before and after rush hours. If there had been a zero increase in traffic during these 30 minutes before and after rush hours, we would have seen a 5.8 % decrease in daily traffic instead of the actual 4.8 % decrease.[29] Drivers' substitution towards lower priced hours hence lowers the daily treatment effect by around 1 percentage point.

In Appendix B.2, we show that our baseline results on traffic are robust to: (i) trimming the sample to one year pre and post policy, (ii) using three different aggregation levels of traffic, (iii) using different levels of fixed effects, and (iv) performing placebo tests using Feb 1 2015 as the treatment date.

In Appendix B.3, we examine potential spatial substitution towards roads outside the congestion zone. There are primarily two detours drivers can make to avoid the toll cordon: either when driving from north to south, or from south to west. Using hourly data from traffic sensors mounted in the roads, we find evidence of spatial substitution behavior during rush hours, in particular in the south-west direction. We estimate that the spatial spillover is around 1054 vehicles per day for the two road sections combined. This substitution pattern does not necessarily represent an undesirable effect of the policy, as traffic is diverted away from the areas where it likely imposes a larger impact on traffic flows and air quality. If we take these spatial spillovers into account, however, the estimated daily reduction in traffic during weekdays changes from 4.8 % (baseline estimate) to 4.1 %.[30] Unfortunately,

[29]Calculations: (7,456-1,662)/(1,632×96) = 5.8 % reduction in daily traffic.

[30]The net daily reduction in traffic in and around the congestion zone is around (7,456–1,054=) 6,402 vehicles, which corresponds to a (6,402/1,632×96=) 4.1 % reduction in daily traffic.

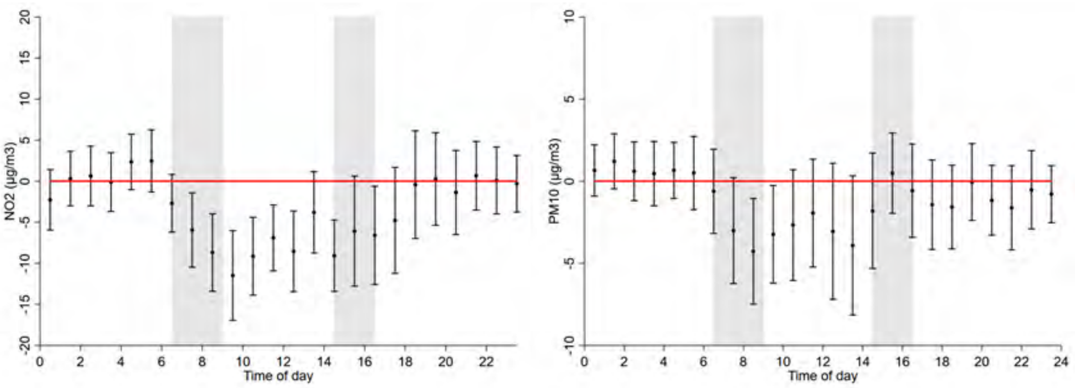
our data does not allow us to examine whether the deterred trips are not taking place at all, or whether people are substituting towards other modes of transport such as public transit, walking, cycling or car pooling.

3.4 Results on air pollution

Figure 5 shows hourly DiD estimates for NO_2 and PM_{10}. From Figure 5 panel (a), we see that the congestion charge led to significant reductions in NO_2 most hours between 6 am and 5 pm. The largest reductions occur in the time period right after the morning rush. As air pollutants can stay in the air for a period of time after they are released, we do not expect to see the same sharp differences between rush hours and non-rush hours as in Figure 2.[31] If we compare the estimated treatment effects to the raw means presented in Figure 3, the estimated effects are very similar to the observed difference in NO_2 concentrations before and after the policy, indicating that most of the change in NO_2 over time can be attributed to the congestion charge.

Figure 5: DID estimates on NO_2 and PM_{10} by 60 min. intervals. 2 years pre/post

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



Notes: Figure plots the coefficient β estimated from Equation 1. Each coefficient reflects the estimated treatment effect from a separate regression, where the sample is restricted to the 1 hour interval indicated on the x-axis. Sample period is restricted to 2 years before and 2 years after policy implementation (Feb 1 2016). Gray shaded areas indicate rush hours. Pollution is measured as micrograms per cubic meter of air (μg/m^3). See Appendix Figure C.4 for results based on a sample restricted to 1 year before and after policy intervention. See Appendix Figure C.6 for "placebo" estimates based on 1 year before and after Feb 1 2015.

Table 3, panel (a) shows average daily treatment effects for NO_2 (column 1) together with average treatment effects for 5 different time intervals (columns 2-6). The coefficient in column (1) suggests that the congestion charge lowered NO_2 concentrations by 3.1 μg/m^3 on average during a day, corresponding to a 7 % reduction. The effect is driven by reductions during and between rush hours; NO_2 concentrations were 6.8 μg/m3 lower during rush hours, corresponding to a 11 % reduction,

[31] The length of the period air pollutants stay in the air will vary with weather conditions, such as precipitation, wind speed and inversion episodes.

while estimated effects on NO_2 during the evening and night time are close to zero and insignificant.[32]

Table 3: DID estimates on NO_2 and PM_{10}. 2 years pre/post

	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/m3)	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: NO_2						
Post × weekday	-3.064**	-4.334***	-6.719***	-6.813***	-0.182	0.323
	(1.369)	(1.576)	(1.634)	(1.658)	(1.915)	(1.300)
Observations	21438	16041	10637	6227	5403	5398
Mean depvar (pre-weekday)	47.01	55.06	60.86	63.78	42.30	24.58
Change (%)	-6.52	-7.87	-11.04	-10.68	-0.43	1.32
Panel B: PM_{10}						
Post × weekday	-1.185	-1.690*	-2.084*	-1.735	-0.868	0.565
	(0.809)	(0.955)	(1.163)	(1.115)	(0.850)	(0.699)
Observations	21624	16219	10794	6314	5429	5401
Mean depvar (pre-weekday)	18.81	21.02	22.29	21.76	18.67	12.01
Change (%)	-6.30	-8.04	-9.35	-7.97	-4.65	4.70
Weather controls ($X'_{it}\gamma$)	✓	✓	✓	✓	✓	✓
Month×year FE (λ_{ym})	✓	✓	✓	✓	✓	✓
Day of week×time-of-day FE (θ_{di})	✓	✓	✓	✓	✓	✓

Notes: Table shows results from 12 separate regressions. Dependent variable is ambient air pollution measured as mean levels of NO_2 or PM_{10} (μg/m3) during a 60 minute interval. Post×weekday refers to the coefficient β estimated from equation 1. Column headings indicate the sample used in each regression. Rush hours refer to the intervals 06:00-09:59 (morning) and 14:00-16:59 (evening). Sample is restricted to two years pre and post policy implementation. Standard errors clustered at the weekly level in parentheses. * p<0.10, ** p<0.05, *** p<0.01

Panel (b) in Figure 5 and Table 3 present results for PM_{10}. While the estimated treatment effects show a relatively similar pattern as for NO_2, with reductions during the daytime, the imprecisely estimated coefficients make it hard to draw firm conclusions. There are several reasons why we would expect PM_{10} estimates to be more noisy and smaller in magnitude than the NO_2 estimates. First, PM_{10} is influenced by multiple sources, and is therefore not as tightly linked to traffic as NO_2. In particular, during wintertime PM_{10} is heavily influenced by the use of wood-fired ovens. Second, as the main source of traffic-related PM_{10} is wear and tear from roads, tires and break blocks rather than exhaust, a potential policy-induced increase in the share of electric vehicles during weekdays would likely have little effect on PM_{10}. By contrast, traffic-related NO_2 is primarily generated from exhaust and would therefore be more influenced by a change in the composition of the car fleet.

In Appendix C.2, we show that the negative and significant effect of congestion

[32] Our rush hour estimate of -11 % corresponds to a road toll elasticity of air pollution of around -0.14. This figure is relatively similar to the toll elasticity reported in Fu and Gu (2017) (-0.15), where the authors estimate the impact of highway toll on an air pollution index reflecting NO_2, PM_{10} and SO_2.

charging on concentrations of NO_2 is robust to: (i) trimming the sample to one year pre and post policy, (ii) using different levels of fixed effects, (iii) using different combinations of weather controls, and (iv) performing placebo tests using Feb 1 2015 as the intervention date. The same robustness checks also confirm a negative but non-significant effect on PM_{10}.

As discussed in Section 3.2, our treatment estimates on NO_2 and PM_{10} should be interpreted as lower bound estimates of the true effect of the policy. One reason for this is that we are differencing out effects of changes in travel habits if these spill over to weekends, such as a shift from driving to cycling. Further, as we will show in Section 4, the congestion charge led to an increased adoption of electric vehicles, which may have lead to a higher share of electric vehicles on the road during both weekdays and weekends.

In an attempt to incorporate these types of behavioral shifts in our treatment estimate, we present findings from an alternative DiD strategy where we compare air pollution levels across cities, pre and post the policy. A similar strategy has been used in previous empirical papers examining effects of various transportation policies on air pollution (see e.g., Simeonova et al., 2019; Zhai and Wolff, 2020). Note however that exploiting differences across cities is only feasible for air pollution and not traffic volume, as we only have access to traffic data from toll gates in Bergen. By contrast, air pollution readings are available for several cities around the country. By focusing on weekdays only and using differences across cities, we circumvent potential problems of spillovers between weekdays and weekends. At the same time, different cities may be subject to different local policies and time trends that might confound the treatment effect, and that are hard to control for. The key identifying assumption is that time-varying omitted variables relevant to air pollution affect all cities similarly. Estimation results from the spatial DiD strategy are presented in Appendix C.3 and show that the congestion charge lowered concentrations of NO_2 by 4 $\mu g/m^3$, or 8.4 percentage points, which is 1.9 percentage points larger than the main results. Again, we find no significant effect on PM_{10}.

4 Part II: Household-level behavior

In this part of the paper, we further examine effects of the congestion charge by moving from station and sensor level data to rich registry data on household level car ownership. The disaggregated data allows us to ask questions such as: How do different types of households adapt to the congestion charge? To what extent do households purchase an electric vehicle in response to the policy? Do households simply add a new car to their portfolio, or do they switch from "brown" to "green"? By estimating a rich set of socioeconomic gradients, we also aim to unmask potential behavioral differences in how households adapt to rising driving costs.[33]

4.1 Data sources

To construct a dataset on car ownership, household demographics, and congestion charge exposure, we combine data from several sources, which are described below.[34]

4.1.1 Car ownership

We collect data on the full population of vehicles registered in Norway over the period 2011-2017 from the National Motor Vehicle register. The register contains technical vehicle information on each car, such as model and fuel type. From the register, we also collect information on current and previous owners of each vehicle, as well as the timing of several acquisition and disposal events, including the first registration date, date of the previous ownership change, scrapping date and/or de-registration dates. We restrict our dataset to privately owned passenger vehicles and vans registered for non-commercial purposes, and stock-sample car owners from the register at the end of each year (December 31st). Even though cars are registered at the individual level, we consider car acquisitions a household level decision and hence focus on households' car ownership in the analysis. This leaves us with a panel of car ownership at the household×year level, where each observation is a snapshot of cars owned at the end of each year. See Appendix D for more information.

Figure 6 displays the annual share of households that owns an electric vehicle. From December 2011 to December 2017, the share of Norwegian households that owned an electric vehicle increased from around 0 % to around 4.5 % (dashed line). The ownership share in 2017 was by far the highest in the world at the time.[35] For Bergen municipality, the share of households that owned an electric vehicle by the end of 2017 was around 8 % (solid line).[36] In the empirical analysis, we aim to disentangle effects of the Bergen congestion charge on electric vehicle ownership

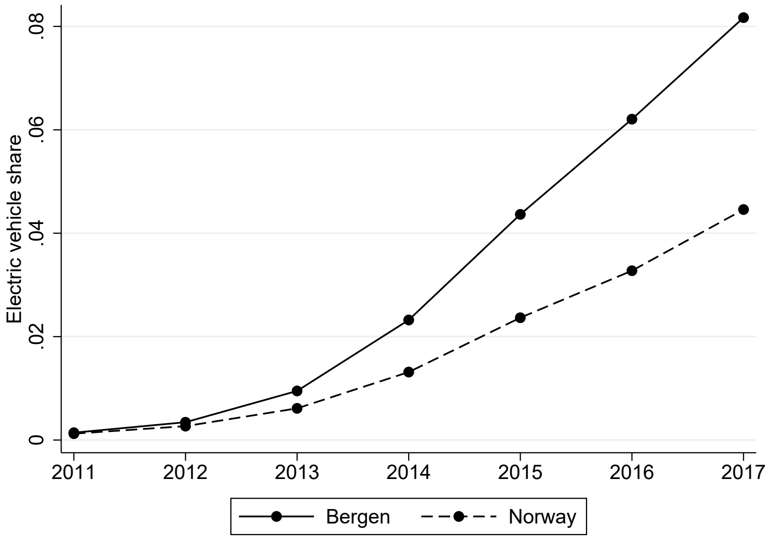
[33] Ideally we would also like to examine the effect of the policy on household-level driving. However, data availability prevents us from investigating this margin of adjustment.

[34] See also Fevang et al. (2021) for a detailed description of the different data sources.

[35] According to IEA (2018), Norway had the world's highest number of battery-electric vehicles and plug-in hybrid as a share of the vehicle stock in 2017 (6.4 %). Only two other countries show a stock share of 1 % or higher: Netherlands (1.6 %) and Sweden (1.0 %). Battery-electric vehicles (BEVs) account for around two-thirds of the world's electric car fleet.

[36] Note that the ownership share of electric vehicles is significantly higher in cities than in rural areas, likely due to e.g., stronger local incentives, better accessibility of charging stations, and shorter distances.

Figure 6: Electric vehicle ownership



Notes: Figure plots the share of households that own a battery electric vehicle on December 31 each year over the period 2011-2017. The first observation reflects the electric vehicle share on December 31st 2011 and the last observation reflects the electric vehicle share on December 31st 2017.

from other confounding trends, such as the increased supply of electric vehicles and national EV policies.

4.1.2 Household characteristics

The car ownership data described above is linked to detailed socioeconomic data on individuals and households from various Norwegian registers, such as the national population register and tax records. Specifically, we collect information on age, gender, number of persons and children in the household, employment and retirement status, income, wealth, education, and ownership of a second home (e.g., cabin). The registry data contains information about the location of each household at the basic statistical unit level – the smallest geographical unit for which we have micro-data. We refer to these units as "neighborhoods". There are in total more than 14,000 neighborhoods in Norway, with an average population of around 400 individuals, or less than 200 households.[37] The detailed information on households allows us to control for several characteristics in the empirical analysis that might influence car ownership, as well as explore heterogeneous effects of the policy.

4.1.3 Journey to work and associated toll payments

In addition to socioeconomic information on individuals and households, all employed individuals are matched to their employer, allowing us to identify the place

[37] By comparison, there were 426 municipalities and 4,856 zip codes in Norway in 2017.

of work at the neighborhood level. By combining the matched employer-employee data with information on the road network, we calculate the fastest route between centroids of the residential and workplace neighborhoods.[38] We also collect data on toll rates and coordinates of all toll gates in Norway from the Norwegian Public Roads Administration.[39] Toll gates are then mapped to each road link, allowing us to calculate toll payments associated with the fastest route between all possible combinations of neighborhood pairs. The calculated toll payments provide a measure of individual-level work trip exposure to the congestion charge. Lastly, 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.[40]

4.2 Empirical strategy

To identify causal effects of the congestion charge on household-level car ownership, we aim to exploit quasi-random variation in individuals' exposure to higher toll rates on the road section between home and work.[41] We start by defining two groups of households which we refer to as *paying commuters* and *non-paying commuters*. Paying commuters are households where at least one individual passes the toll cordon on the (time-minimizing) route between home and work. Non-paying commuters are households where none of the working individuals have toll payments associated with the (time-minimizing) route between home and work. After the introduction of the congestion charge in February 2016, the first group of households (*paying commuters*) faced an increased cost of driving to work during rush hours. We view this increased cost as a proxy for policy exposure.

Based on these two groups of households, a potential identification strategy could be to compare the two groups before and after the policy in a Difference-in-Differences (DiD) framework. Any time-invariant difference between the two groups

[38]To derive the fastest route, we use a publicly available dataset on the Norwegian road network (https://register.geonorge.no/geodatalov-statusregister/elveg/ed1e6798-b3cf-48be-aee1-c0d3531da01a) and find the route along the network that minimizes the sum of link-specific travel time according to the speed limit.

[39]See Appendix Figure D.2 for toll road developments in Bergen and Stavanger over time.

[40]"Public transit time" is obtained 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 the stations). Note that these numbers are not necessarily based on the shortest public transit route; they are the output of a transportation model where route choice is partly based on minimization of generalized travel costs, and partly calibrated to fit observed data. The data on public transit time is static. For a more detailed description of how public transit routes are coded in the transportation models, see Kwong and Ævarsson (2018).

[41]While individuals may be exposed to congestion charging on non-work trips as well, we assume that rising driving costs on the road section between home and work will play an important role in households' response to the policy.

would then be differenced out. We argue, however, that the two types of households faced different trends that would violate the parallel trends assumption and hence bias the estimates. As described in Section 2, electric vehicles were exempted from toll payments also before the introduction of the congestion charge. These incentives for buying electric vehicles likely interacted with the increased availability and improved quality of electric vehicles over time. As a consequence, "paying commuters" faced a stronger incentive to buy an electric vehicle which has increased over our sample period. This implies that we would expect to see a larger increase in electric vehicle ownership for "paying commuters" compared to "non-paying commuters" also in absence of the congestion charge.

To overcome the potential problem of non-parallel trends, we employ a triple differences (DiDiD) framework, where we compare the difference between paying and non-paying commuters in Bergen to a similar difference between households located in a city without congestion charging.[42] As several cities in Norway have a toll cordon, we can define neighborhood pairs that are located on opposite sides of the tolled area. We identify Stavanger as a similar sized city that has a toll cordon, but where congestion charging was not introduced until October 2018. The rates were similar to Bergen in the pre-period, and remained unchanged in the time period analyzed.[43] Our triple differences strategy hence aims to exploit variation along three dimensions: (i) pre vs. post, (ii) paying commuters vs. non-paying commuters, and (iii) Bergen vs. Stavanger.[44]

4.2.1 Estimating equation

More formally, our DiDiD estimator is written as:

$$y_{it} = \beta \text{post}_t \times c_i \times B_i + \alpha_t c_i + \eta c_i \times B_i + X'_{it}\gamma + \theta_{nt} + \varepsilon_{it}, \tag{2}$$

where i indicates household, t indicates year, y_{it} is a placeholder for a relevant household level outcome (e.g., electric vehicle ownership) in a given year, post_t is

[42] Appendix D.2 shows the result of running two separate DiD regressions for these two cities. As expected, paying commuters in both cities experienced an increase in electric vehicle ownership relative to non-paying commuters (Appendix Figure D.4). Note that the vertical difference between the estimated coefficients in the two DiD models is approximately the same as the triple difference coefficients presented in Figure 7 and Table 5 in our main analysis.

[43] 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). Both cities are located in the south-west of Norway, along the coast. See Appendix Table A.1 for an overview of cities with congestion charging, and Appendix Figure D.2 for an overview of toll rates in Bergen and Stavanger in the time period analyzed.

[44] See Appendix D.1 for details on sample restrictions and definitions of treatment and control groups.

a dummy variable equal to 1 after policy implementation, c_i is a dummy variable equal to 1 if the household is classified as a "paying commuter", and B_i is a dummy variable equal to 1 if the household is located in or close to Bergen (as opposed to Stavanger). The coefficient of interest is β and reflects the triple differences estimate (DiDiD). θ_{nt} indicates neighborhood×year fixed effects, and will absorb any time-variant variation within narrowly defined areas in Bergen and Stavanger that are common to paying commuters and non-paying commuters. This includes the effect of other local electric vehicle incentives that potentially vary over time, such as availability of charging stations and parking spaces. The α_t parameters will absorb the year specific effects of being a commuter, and η will absorb the effect of a being paying commuters in Bergen pre policy implementation. ε_{it} is the idiosyncratic error term and X'_{it} is a vector of demographics and work route specific controls; see Appendix D.1 for details.

4.2.2 Identifying assumptions

A key identifying assumption underlying our empirical strategy is that paying commuters in Bergen and Stavanger would have experienced parallel trends in the outcome variable in absence of the congestion charge - conditional on control variables and fixed effects. While this assumption is inherently untestable, parallel pre-treatment trends suggest that the assumption is more likely to hold. To examine the validity of the parallel trends assumption, we estimate a version of our DiDiD estimator where treatment effects are allowed to vary over time. By defining the year prior to the announcement of the policy as the reference year (2014), the dynamic DiDiD estimator can be written as:

$$y_{it} = \sum_{s \in \{T | s \neq 2014\}} \left[\alpha_t c_i + \beta_t c_i \times B_i\right] \times \mathbb{1}\{t = s\} + \eta \left[c_i \times B_i\right] + X'_{it}\gamma + \theta_{nt} + \varepsilon_{it}, \quad (3)$$

where annual treatment effects are captured by β_t. Parallel trends imply that $\beta_t \approx 0$ for the years prior to 2014. Note that η will absorb the 2014 level difference between paying commuters in Bergen and Stavanger. Hence the annual treatment effects β_t are identified from the annual deviations from 2014 levels.

4.2.3 Interpretation of the DiDiD estimate

As our empirical strategy relies on exposure to the policy on the road section between home and work, our estimated treatment effect should be interpreted as a local treatment effect for the sub-population of households where at least one individual is employed. For households where none of the individuals are employed (e.g., students, retirees, unemployed), effects of the congestion charge may be very different. Further, we use work-trip exposure to the congestion charge as a proxy for policy exposure, while the congestion charge may affect non-work trips as well. Our empirical strategy could hence be interpreted as a form of treatment intensity, were we assume that households that face a congestion charge on their way to work will be more intensively exposed than those that do not. This means that households in the control group (non-paying commuters) will potentially also be exposed to higher driving costs, but presumably to a lesser extent than the treatment group. As a result, our estimated treatment effect should be interpreted as a lower bound of the causal effect of the policy for the particular sub-population defined.

4.2.4 Heterogeneous effects

To examine how different types of households respond to the congestion charge, we estimate a version of the DiDiD estimator where we allow treatment effects to vary by different socioeconomic groups. When $k \in \mathcal{K}$ denotes group (e.g., income quintile), the heterogeneous DiDiD can be written as:

$$\begin{aligned} y_{it} = &\sum_{k \in \mathcal{K}} \left[\beta_k \text{post}_t \times B_i \times c_i + \alpha_{tk} c_i + \delta_{tk} + \eta_k B_i \times c_i + \psi_k B_i \right] \mathbb{1}\{i \in k\} \\ &+ X_{it}' \gamma + \theta_{nt} + \varepsilon_{it}, \end{aligned} \tag{4}$$

where the treatment effect for group k is captured by β_k. Note that all coefficients except demographics and neighborhood×year fixed effects are k specific. This ensures a flexible model were we account for several group-specific time-varying factors.[45]

4.3 Descriptives based on the estimation sample

Based on our empirical strategy, we restrict our data set to households located in or close to Bergen and Stavanger, and where at least one household member is employed. See Appendix D.1 for more details on the sample restriction. The trimmed sample leaves us with 76,088 households observed over a period of 7 years, resulting in an unbalanced panel of 448,196 household×year observations.

Table 4 shows summary statistics for 2014 by city and commuter group based on the estimation sample.[46] The electric vehicle share among paying commuters is similar across the two cities in 2014 (5 % in Bergen and 4 % in Stavanger). We also

[45]A fully flexible model where all variables are k specific would be equivalent to estimating separate regressions of Equation 2 for each group k.

[46]See Appendix Table D.4 for the same summary statistics in 2017.

Table 4: Summary statistics, by city and commuter group. 2014

	Bergen				Stavanger			
	Paying		Non-paying		Paying		Non-paying	
	mean	sd	mean	sd	mean	sd	mean	sd
Panel A: Outcomes								
Electric vehicle (0/1)	0.047	0.211	0.028	0.164	0.036	0.185	0.020	0.138
Number of electric vehicles	0.048	0.220	0.028	0.171	0.037	0.195	0.020	0.141
Number of ICE vehicles	1.173	0.838	1.447	0.872	1.483	0.830	1.357	0.849
Total number of vehicles	1.221	0.856	1.476	0.877	1.520	0.836	1.377	0.854
Panel B: Journey to work variables								
Toll rate (NOK/individual)	23.51	7.43	0.00	0.00	17.69	6.23	0.00	0.00
Toll rate (NOK/household)	34.80	17.43	0.00	0.00	26.53	13.13	0.00	0.00
Driving distance (km)	12.37	8.10	14.07	8.83	13.63	6.68	10.45	5.85
Driving time (min)	13.25	8.57	14.93	9.60	13.80	6.81	11.70	7.35
PT time minus driving time (min)	56.65	43.77	90.33	79.61	76.70	53.50	76.05	77.45
PT time divided by driving time	5.37	2.71	7.48	5.36	6.98	4.61	8.04	8.15
Panel C: Socio-economic variables								
Couple (0/1)	0.71	0.45	0.69	0.46	0.76	0.43	0.67	0.47
Children living at home (0/1)	0.41	0.49	0.42	0.49	0.47	0.50	0.41	0.49
Persons in household	2.62	1.34	2.66	1.38	2.84	1.38	2.63	1.41
Age	44.37	12.09	45.01	12.43	44.01	11.44	44.48	12.28
Female (0/1)	0.49	0.28	0.49	0.29	0.48	0.25	0.48	0.29
Owns second home	0.11	0.32	0.11	0.31	0.11	0.31	0.11	0.31
Employed (0/1)	0.94	0.17	0.90	0.20	0.94	0.16	0.92	0.19
Retired (0/1)	0.06	0.20	0.07	0.22	0.05	0.18	0.06	0.20
Income (100,000 NOK/individual)	4.17	3.14	3.90	2.36	4.70	2.87	4.45	2.97
Income (100,000 NOK/household)	7.17	4.73	6.60	4.60	8.31	5.64	7.47	5.79
Wealth (mill NOK/individual)	1.64	6.16	1.32	3.43	1.72	3.02	1.84	5.21
Wealth (mill NOK/household)	2.81	8.74	2.26	6.17	3.08	5.55	3.14	10.06
Education:								
Unknown (0/1)	0.17	0.37	0.17	0.38	0.14	0.34	0.19	0.39
Less than high school (0/1)	0.07	0.26	0.12	0.33	0.10	0.30	0.12	0.32
High school (0/1)	0.22	0.42	0.35	0.48	0.30	0.46	0.29	0.45
College (0/1)	0.31	0.46	0.26	0.44	0.29	0.45	0.26	0.44
University (0/1)	0.23	0.42	0.08	0.28	0.17	0.38	0.15	0.35
Observations	12244		21742		23747		18355	

Notes: Table shows summary statistics for 2014 based on the estimation sample. Paying refers to paying commuters. Non-paying refers to non-paying commuters. ICE refers to "internal combustion engine", and PT refers to "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 set of variables: female, employed, retired, second home, children, education level, the number of persons registered at the household, two polynomials in age, income, wealth, distance and driving time to work, two polynomials in the absolute and relative time differences to get to work by public transit versus private car. If a variable is missing for one of the spouses, the other spouse's value is used as a proxy for the household average. We let the coefficients for all variables be couple and single specific (i.e., whether the household has one or two adult members). More detailed variable descriptions can be found in Appendix Table D.3. Descriptives for 2017 can be found in Appendix Table D.4.

see that the electric vehicle share among non-paying commuters is lower for both cities (3 % in Bergen and 2 % in Stavanger). In the DiDiD strategy, we will compare the development in car ownership for the two types of household groups in Bergen, to the equivalent difference in ownership share for the two types of households in Stavanger.

From Panel B in Table 4, we see that average toll rates associated with the journey to work is NOK 0 for non-paying commuters (by construction), NOK 23.5 for paying commuters in Bergen and NOK 17.7 for paying commuters in Stavanger. Appendix Figure D.2 shows the development in toll rates over time, and reveals that paying commuters in Stavanger were subject to the same toll level in the period 2013 to 2017.[47] Paying commuters in Bergen, however, experienced an increase in toll rates over the same period. From Appendix Figure D.2, Panel A we see that there was a small increase in toll rates in Bergen the year before the congestion charge was introduced (2014), and then a larger jump in 2016 when rush hour pricing was implemented.[48] This means that estimated treatment effects will likely reflect a response to both these jumps in toll rates (i.e., the effect of changing the toll rate from NOK 13 all day to NOK 45 (19) during (non-) rush hours). If we observe a positive treatment effect in 2015 on car ownership, this might reflect effects of both the congestion charge announcement on February 1 2015 as well as a potential delayed response to the jump in toll rates in 2014.

Panel C shows summary statistics for various socioeconomic variables. Overall, paying commuters in Bergen and Stavanger are relativity similar across most demographics – although paying commuters in Stavanger are more likely to be two-adult households and have slightly higher income and education levels. If we compare paying computers to non-paying commuters, the former are more likely to be couples and tend to be richer and with a higher education level.

4.4 Results on car ownership

Figure 7 displays annual treatment effects estimated from the DiDiD specification in Equation 3. From panel (a), we see that households exposed to the Bergen congestion charge were more than 4 percentage points more likely to own an electric vehicle by the end of 2017. The treatment effects in 2016 and 2017 are roughly of the same magnitude and both effects are clearly significantly different from zero. We also find a positive and significant treatment effect on electric vehicle ownership in the end of 2015, suggesting that households responded to the announcement of the policy in

[47] Panel A of Appendix Figure D.2 shows how toll rates in Bergen and Stavanger has changed over time, while Panel B of Appendix Figure D.2 shows the average toll exposure per year for paying and non-paying commuter households in Bergen and Stavanger. Note that the average household level toll exposure in Appendix Figure D.2, Panel B is slightly lower than the actual toll rates as several households only have one spouse that is exposed to toll charges on his/her way to work, while the other spouse live and work on the same side of the toll cordon.

[48] The congestion charge was implemented early in the year (marked by the vertical line), but as our data is at the annual level the jump in toll rates does not show up until the observation at Dec 31st, 2016.

the beginning of the year (in February 2015). The presence of an anticipation effect is unsurprising as cars are durable goods and car acquisition decisions are typically made with future expectations taken into account. Furthermore, the electric vehicle market in the period around policy announcement was characterized by excess demand and long waiting lists, meaning that households would have difficulties in timing the car acquisition to a specific date. Looking at the pre-intervention period (2011-2014), estimated coefficients are close to zero, supporting the validity of the parallel trends assumption. While we do find a statistically significant effect in one of the pre-treatment years, this deviation appears small compared to the large jump in the electric vehicle share in the post-treatment period.

Panel (b) in Figure 7 shows treatment effects on the number of internal combustion engine (ICE) vehicles owned by a household. The negative treatment effects on ICE vehicles are close to a mirror image of the positive effects on electric vehicle adoption, suggesting that households switched from "green" to "brown" rather than adding an additional vehicle to their household. This is also confirmed by the non-significant treatment effects on the total number of vehicles owned by a household; see Figure 7, panel (c).

Table 5: DiDiD estimates on vehicle ownership

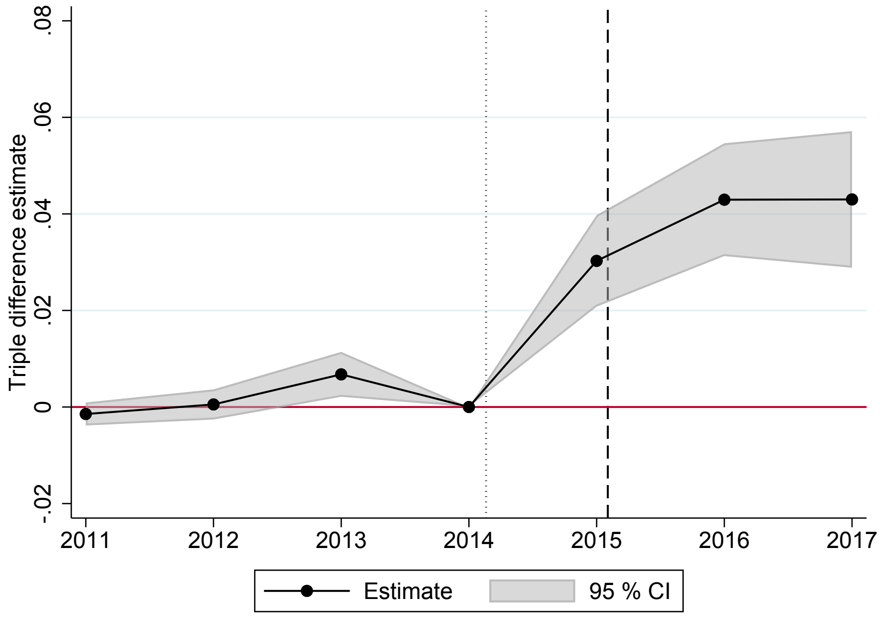
	Probability	Number of vehicles		
Dependent variable:	Pr(BEV) (1)	BEV (2)	ICEV (3)	Total (4)
Post $\times$ Paying commuters $\times$ Bergen	0.0419***	0.0451***	-0.0422***	0.00288
	(0.00593)	(0.00652)	(0.0119)	(0.0116)
Observations	376914	376914	376914	376914
Mean depvar 2014 (paying commuters, Bergen)	0.0469	0.0482	1.1730	1.2212
Mean depvar 2017 (paying commuters, Bergen)	0.1774	0.1872	1.1555	1.3427
Paying commuter $\times$ year FE ($\alpha_t c_i$)	✓	✓	✓	✓
Paying commuter $\times$ Bergen FE ($\eta c_i \times B_i$)	✓	✓	✓	✓
Household characteristics ($X'_{it}\gamma$)	✓	✓	✓	✓
Neighborhood $\times$ year FE (θ_{nt})	✓	✓	✓	✓

Notes: Table plots the coefficient β estimated from Equation 2. 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 neighborhood level. * p<0.10, ** p<0.05, *** p<0.01.

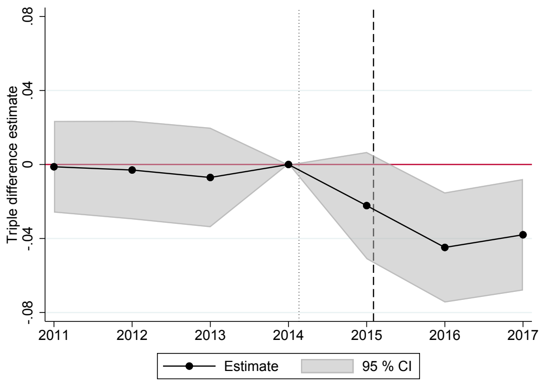
Table 5 shows average treatment effects for the car ownership variables when we restrict the post period to 2016-2017 and the pre period to 2011-2014. The estimate in column (1) suggests that the congestion charge on average induced a 4.2 percentage point increase in the probability of owning an electric vehicle in the post implementation years. To illustrate the magnitude of this effect, we find

Figure 7: DiDiD estimates on vehicle ownership

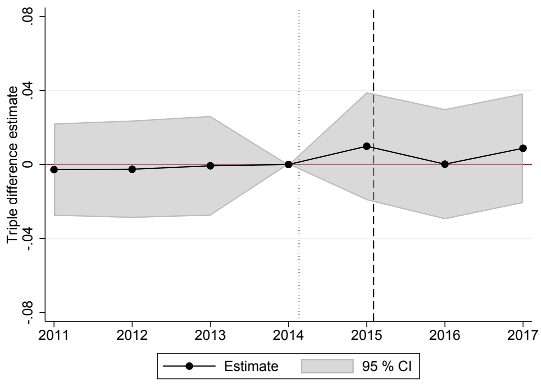
(a) Electric vehicle (0/1)



(b) Number of ICE vehicles



(c) Total number of vehicles



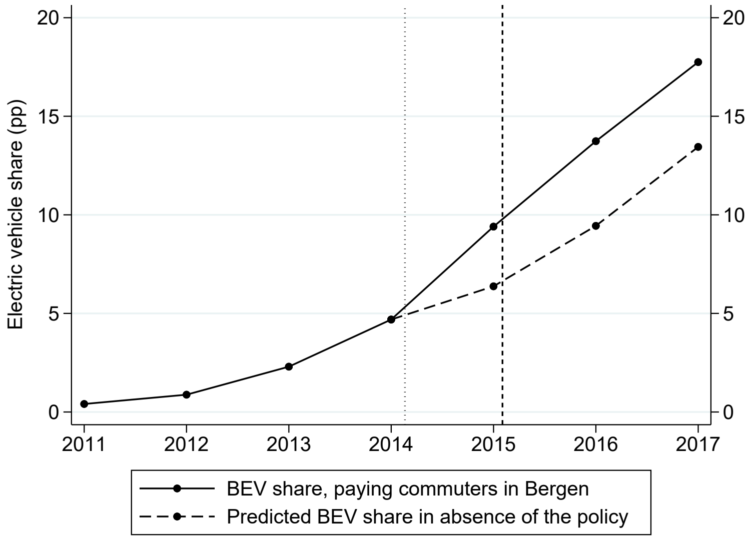
Notes: Figure plots coefficients β_t estimated from equation 3, where β_{2014} is normalized to zero. Panel (a) shows the annual treatment effect on the probability of a household owning a battery electric vehicle. Panel (b) shows the annual treatment effect on the number of internal combustion engine (ICE) vehicles owned by a household. Panel (c) shows the annual treatment effect on the total number of vehicles owned by a household. Standard errors are clustered at the neighborhood level. All regressions include the following set of controls: female, employed, retired, second home, children, education level, number of persons registered at the household, two polynomials in age, income, wealth, distance and driving time to work, two polynomials in the absolute and relative time differences to get to work by public transit versus private car. All controls are single/couple specific.

that the congestion charge can explain around 1/3 of the increase in electric vehicle ownership for the treatment group from 2014 to 2017.[49] See also Figure 8 for an illustration of the treatment effect. At the same time, we find that the policy reduced the average number of ICE vehicles owned by a household by 0.042 (see column 3),

[49]This can be derived from the observed electric vehicle shares in 2014 and 2017 reported in column (1) of Table 5.

resulting in a zero effect on the total number of vehicles owned by a household (see column 4). Our findings hence suggest that the congestion charge helped induce a substantial increase in the number of electric vehicles, while at the same time leaving the average number of cars owned unchanged.

Figure 8: Observed and predicted levels of electric vehicle ownership



Notes: Solid line shows the share households among paying commuters in Bergen that owned an electric vehicle in the period 2011-2017. 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 7a. 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).

Next, we examine the sensitivity of our main results to various specifications of fixed effects and demographic controls. In particular, we show that the inclusion of neighborhood×year fixed effects are important for the magnitude of our estimates (Appendix Table D.5). This comes as no surprise as the demand for electric vehicles is increasing over time and likely to be affected by several local aspects of the residential neighborhood, such as access to parking and charging stations. Moreover, accessibility, travel demand and exposure to toll charges and other local electric vehicle incentives that are unrelated to the commute are likely to be captured by these fixed effects. Appendix Figure D.6 shows that our main result on electric vehicle adoption is robust to the inclusion of demographics and journey to work controls. The stability of our treatment estimate across nine different specifications suggest that our neighborhood-year fixed effects do a good job in controlling for various socioeconomic characteristics. For ICE vehicles, the estimated treatment effect is larger (i.e., more negative) for the specification without any demographic controls. However, including journey to work controls seem to be sufficient to arrive at a robust treatment effect; adding seven additional sets of demographic controls has little effect on the estimated treatment effect.

4.4.1 Heterogeneous effects

The estimated average treatment effects presented in the previous section are likely to mask substantial heterogeneity. In the following, we examine heterogeneous treatment effects along six socioeconomic dimensions: income, family size, education, age, commuting distance, and public transit quality. Key results are presented in Figure 9, while supporting results are available in Appendix D.3.

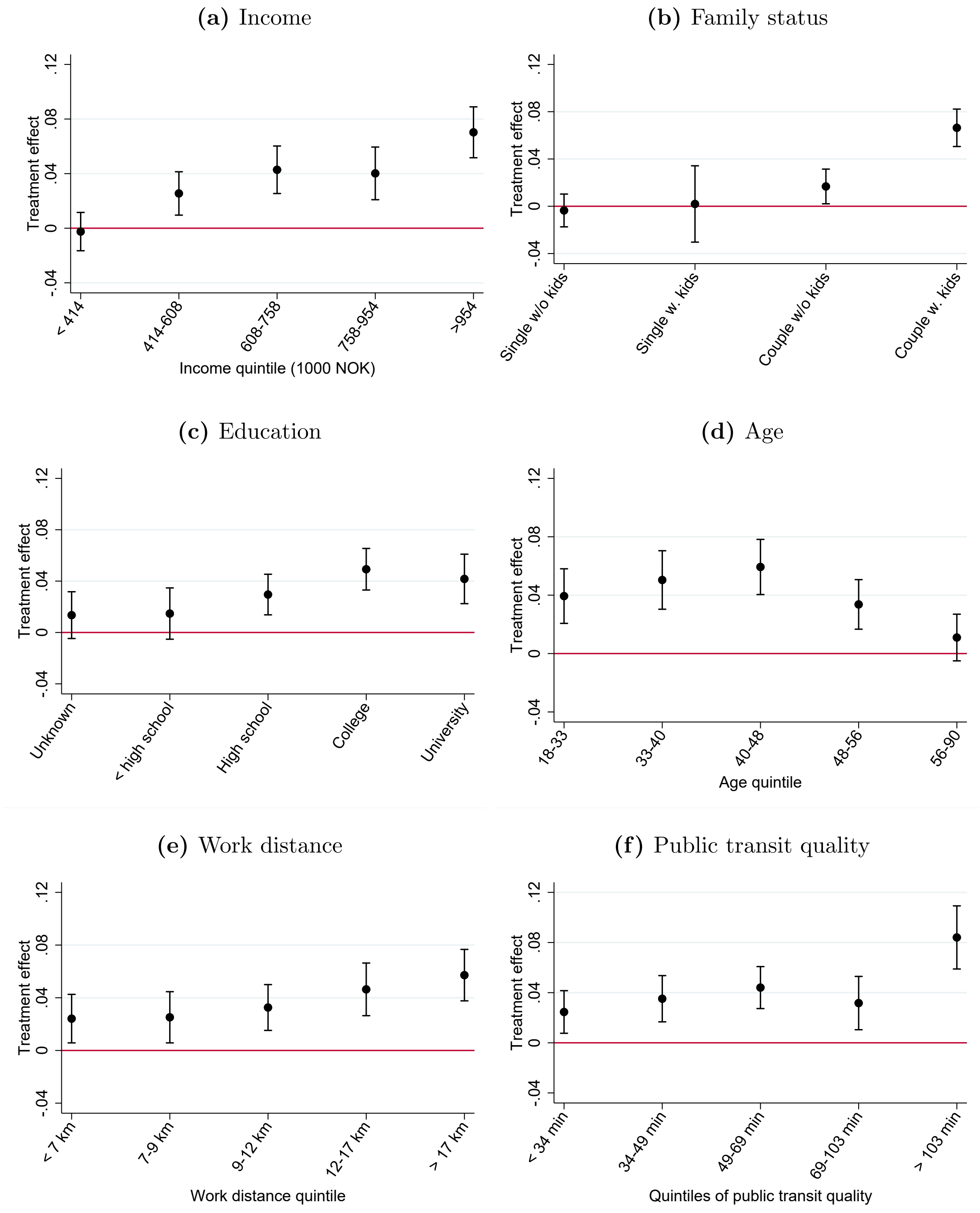
Income: Allowing the treatment effect to vary by household income, we find a clear income gradient in electric vehicle adoption; see panel (a) in Figure 9. While households in the highest quintile are 7 percentage points more likely to adopt an electric vehicle in response to the policy, the corresponding number for the lowest income quintile is close to zero and non-significant.[50] This observed heterogeneous pattern could be due to both *preferences* and *financial constraints*. First, high-income households might prefer to purchase an electric vehicle in response to the policy, while low-income households prefer to change their mode of transportation to e.g., public transit or cycling. Differences in the margin of adjustment could reflect different preferences for adopting new technology, differences in the value of time, or differences in utility from cycling or using public transit. At the same time, the heterogeneous pattern may also reflect financial barriers; purchasing an electric vehicle in the time period we are considering is synonymous with purchasing a new car (due to the negligible market for used electric vehicles). Low-income households may therefore in practice have a more limited opportunity set than high-income households, as the only used cars available are internal combustion engine vehicles.[51] Even if a low-income household considers it cheaper in the long-run to purchase an electric vehicle rather than paying road toll, financial constraints may prevent the household from pursing its optimal adaptation behavior.[52]

[50]See Appendix Table D.6 for coefficients in table format. In Appendix Figure D.8 we also show that high-income households are less likely of adopting an internal combustion engine vehicle, which suggest that the switch from brown to green is driven by the same type of households. This interpretation is also confirmed by looking at Appendix Figure D.9, which shows a close to zero effect on total car ownership for all income quintiles.

[51]Note that purchasing a new electric vehicle in Norway is not necessarily more expensive than purchasing a new conventional vehicle. As electric vehicles are exempted from both the value added tax and the registration tax, electric vehicles actually tend to be *cheaper* than comparable conventional cars; see Appendix Table E.2 for an illustration. Purchasing new cars in general, however, occurs eight times more frequently within the top income decile compared to the bottom one; this holds for both battery-electric and conventional vehicles (see Fevang et al., 2021, Figure 5c).

[52]In a simplified calculation presented in Appendix Table E.3, we show that purchasing a new electric vehicle might in fact be a cheaper option than purchasing a used internal combustion engine vehicle when taking into account both congestion charges and fuel costs.

Figure 9: Heterogeneous DiDiD: electric vehicle adoption.



Notes: Figure plots the coefficients β_k estimated from equation 4, 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. 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. See Appendix Table D.6 for coefficients in table format.

Household type, education, and age: In a next step, we examine how treatment effects vary by household type, education, and age. We find that the probability of adopting an electric vehicle in response to the policy is significantly higher for couples with children below 18 years; see Figure 9, panel (b). For single adult households, the treatment effect is close to zero - irrespective of children in the household. The heterogeneous pattern may reflect that couples with kids are less flexible in changing their mode of transportation. A larger family involves more logistics, which might make it harder to switch to e.g., public transit. There may also be economies of scale that makes it more cost efficient for these households to invest in an electric vehicle. Further, we find that the treatment effect is increasing in educational attainment, with the largest effect for college and university educated households (panel c). This pattern could potentially reflect a correlation between educational attainment and preferences for new technologies. It might also indicate a higher awareness of environmental and climate benefits of driving an EV among higher educated households. Previous literature also suggest that individuals tend to "undervalue" future fuel savings when purchasing a vehicle (see e.g., Allcott and Wozny, 2014), and this tendency might weaken with education. Lastly, we document an inverse u-shaped relationship between electric vehicle adoption and age (panel d), with households close to retirement age as the least responsive group.

As education and age covary with income, the observed heterogeneous patterns are likely to reflect a combined effect of income and the demographic in question - in addition to other correlated variables. In an attempt to disentangle the income channel from other mechanisms, we estimate heterogeneous effects separately for the lowest and highest income quintile; see Appendix Figure D.7. Findings show that households in the lowest income quintile are non-responsive to the policy irrespective of educational attainment and age. This suggests that financial barriers may play an important role in EV adoption. For high-income households, who are more likely to afford a new electric vehicle, treatment effects are increasing in educational attainment and decreasing in age. These patterns suggest that age and education have an effect on EV adoption that go beyond the income effect.

Commuting distance and public transit quality: A household's adaptation decision may also depend on the quality of transportation substitutes. From Figure 9, we see that the probability of EV adoption is increasing in the driving distance to work (panel e) and decreasing in public transit quality (panel f). Public transit quality is proxied by the additional time it takes to get to work by public transit compared to driving a private car.[53] While these findings suggest that transportation

[53]Time by public transit includes average waiting time and walking time to and from stations.

substitutes influence a household's adaptation response, the distributional implications are not clear cut. On the one hand, the driving distance to work and the quality of public transit may reflect a sorting process where individuals choose their preferred neighborhood based on amenities - including public transit access. To the extent that this process is voluntary (i.e., not constrained by financial barriers), and public transit quality is capitalized into housing prices and rents, the distributional impacts of the policy may be less of a concern. On the other hand, the heterogeneous pattern may reflect a "lock-in" effect for households living in rural areas with poor public transit options. As switching to cycling or public transit is less feasible, these households are likely to face a higher adaptation cost as they are disproportionately "forced" to use the electric vehicle channel to avoid congestion charges.

Again, commuting distance and public transit quality may covary with income. In Appendix Figure D.7, we show that households in the lowest income quintile are non-responsive to public transit quality and work distance. By contrast, high-income households with poor public transit quality are more likely to purchase an electric vehicle compared to similarly wealthy households with better public transit options. A similar pattern emerges for work distance. While suggestive, these findings add to the evidence base suggesting that low-income households are to a larger extent locked into existing behavioral patterns.

5 Discussion

This section provides some simple back-of-the-envelope welfare calculations, as well as a short discussion of the distributional implications of the congestion charge.

5.1 Welfare effects

To give a rough estimate of the net welfare effect of the policy, we combine the treatment effects presented in previous sections with a set of assumptions and cost estimates. In our calculations, we consider three categories of social benefits (improved air quality, lower CO_2 emissions, time savings due to less congestion) and contrast these to the private adaptation costs. Focusing on marginal changes in equilibrium quantities induced by the policy, the net welfare effect can be written as:

$$\Delta \mathrm{W} = \underbrace{\overbrace{\Delta \mathrm{NO}_2 \cdot \mathrm{MC}_{\mathrm{NO}_2} + \Delta \mathrm{PM}_{10} \cdot \mathrm{MC}_{\mathrm{PM}_{10}}}^{\text{Local pollutants}} + \overbrace{\Delta \mathrm{CO}_2 \cdot \mathrm{MC}_{\mathrm{CO}_2}}^{\text{Global pollutant}} + \overbrace{\Delta \mathrm{Time} \cdot \mathrm{VoT}}^{\text{Time savings}}}_{\text{Social Benefits}} - \underbrace{\Delta \text{Adaptation costs}}_{\text{Social costs}}, \tag{5}$$

where MC indicates marginal cost, $Time$ indicates driving time, and VoT indicates the value of travel time. Below we give a short description of the welfare calculations, while more details are provided in Appendices E.1–E.5.

Adaptation costs: A congestion charge imposes private costs because the policy prevents drivers from choosing their preferred travel mode, route, or time. We consider four ways in which individuals may adapt to the congestion charge: (1) not drive at all (e.g., change mode of transportation, or work from home), (2) shift driving to non-rush hours (intertemporal substitution), (3) drive around the toll cordon (spatial substitution), and (4) buy an electric vehicle to avoid charges. Note that the actual toll payments made by drivers is simply a transfer from households to the government; if we disregard transaction costs, these payments have no associated social costs.[54] To quantify the adaptation costs related to (1)-(3), we combine the estimated reduction in cars passing the toll cordon during rush hours (Table 2) with derived adaptation costs per trip using the triangle area formula. To quantify the private costs of substituting towards electric vehicles (4), we combine estimates from our household-level ownership regressions (Table 5) with various assumptions.

Local and global pollutants: To quantify social benefits of reduced air pollution, we combine our estimates on changes in ambient levels of NO_2 and PM_{10} (Table 3) with estimates on the social cost of exposure to air pollution, which we compile from the literature. To quantify benefits of lower CO_2 emissions, we consider the effects of reduced driving – taking into account intertemporal and spatial substitution (Table 2, Table B.5) – as well as the substitution towards electric vehicles (Table 5).

Time savings: A key benefit of congestion charging is time savings due to less congestion. Unfortunately our data lacks a relevant congestion measure such as driving speed or time spent in traffic. As a second best solution, we combine our detailed traffic data with before-after estimates of average time savings on different routes collected from a descriptive report (NPRA, 2016).

[54] The flat rate, however, implies taking a larger percentage of income from low-income earners, which has distributional implications; see Section 5.2.

Net welfare effect: Table 6 gives an overview of the calculated social benefits and costs of the policy. Overall, we find that the Bergen congestion charge led to a positive and economically significant net welfare gain of around NOK 49 million per year, equivalent to around USD 5.92 million per year. The welfare calculations imply a benefit to cost ratio of 3.2. Our conclusion of a net welfare gain is robust to excluding any one of the four social benefit components, or assuming that adaptation costs are twice as high as our preferred estimate.

Table 6: Welfare effects of the congestion charge

	mill. NOK/year
Total social benefits	**71.57**
Lower NO_2 concentrations	9.05
Lower PM_{10} concentrations	16.14
Lower CO_2 emissions	4.12
Less congestion (saved travel time)	42.26
Total adaptation costs	**22.22**
Changing mode, route, or time	18,51
Changing vehicle type (to electric)	3.72
Net welfare effects (NOK)	**49.34**
Net welfare effects (USD)	**5.92**

5.2 Distributional concerns

While we find a net welfare gain of the policy, benefits and costs are not necessarily evenly distributed across different population groups. In Section 4.4.1, we show that there are systematic differences in how households adapt to the policy. In particular, we find that high income households are more likely to adopt an electric vehicle in response to the policy. We further find that a long work commute and poor access to public transit increase the chances of adopting an electric vehicle - but only for high-income households. The latter suggests that low-income households may to a larger extent be locked into existing behavioral patterns.

Another dimension affecting the distributional profile of the policy is the actual toll payments. As congestion charges are imposed as a flat rate, they make up a larger percentage of the budget for low-income earners, meaning that the charges are regressive. Based on some simple calculations presented in Appendix Table E.3, the sum of congestion charges payed on the work commute over a year are likely to be in the same order of magnitude as the annual fuel costs. The congestion charges hence make up a non-negligible share of the car ownership costs.

There are, however, several aspects that dampen the regressivity of the congestion charge. First, the regressive profile only applies to the part of the population that actually owns a car. In 2016, the car ownership rate in Norway was around 30 %, and those that do not own a car are on average 30 % poorer and have less education (see Fevang et al., 2021, Table 1 for a detailed overview). This means that the congestion charge will be less regressive when looking at the full population and not only car owners. Second, congestion charges are usually implemented around cities, where income levels tend to be higher. Moreover, households exposed to congestion charges on their work commute are on average slightly richer and more educated than non-exposed households, as illustrated in Table 4. The fact that policy exposure is positively correlated with income dampens the regressivity of the policy. Lastly, the net distributional effects of congestion charges will depend on how the proceeds from the policy are used. Around 70 % of the revenues from congestion charges in Bergen is budgeted for public transit purposes,[55] which tends to disproportionally benefit lower-income groups. Accounting for all the elements mentioned above will tend to dampen the regressivity of the policy.

6 Conclusion

Combating climate change and poor urban air quality will require a fundamental shift towards greener modes of transportation. Policies that incentivize individuals to choose low-emission transportation alternatives will likely play a key role in this transition. To ensure that market-based policies work as intended and retain public support, there is a need to better understand behavioral responses to these types of policies, and effects on emissions.

This paper shows that differentiating driving costs by time of day and vehicle type can help reduce traffic, improve urban air quality, and shift the composition of the car fleet towards electric vehicles. Exploiting a congestion charge in Norway that imposed spatial and temporal variation in the costs of driving a high-emission vehicle, we find that the policy reduced rush hour traffic by 14.5 % and daily traffic by around 4.8 %. The lower traffic translated into cleaner air: we estimate a 6.7 μg/m^3 reduction in ambient levels of NO_2 during midday hours, equivalent to an 11 % decrease. Examining adaptation responses, we find that the congestion charge increased the probability of a household owning an electric vehicle by around 4.2 percentage points – explaining around 1/3 of the observed increase in electric

[55]This percentage applies to the budget period 2018-2037; see: https://www.regjeringen.no/contentassets/66644bf4b3e642acaf10bea324af42b8/byvekstavtale-bergen-2017-2023.pdf, pg. 26 (accessed August, 2020).

vehicle ownership over the same period. The increase in electric vehicle ownership was mirrored by a similar decrease in the ownership of fossil fuel vehicles, leaving the total number of cars unchanged. We document strong heterogeneous patterns along several socioeconomic dimensions, with high-income households responding more strongly to the electric vehicle incentives. Overall, we find that the congestion charge led to a net welfare gain, with a cost ratio of 3.2.

The magnitude of our findings should be interpreted in light of Norway's exceptionally high electric vehicle share and well-developed charging infrastructure. Estimated effects, however, may be informative of expected impacts of raising driving costs for fossil fuel cars in other countries at a future point in time when electric vehicles are more competitive (e.g., due to technological improvements). Further, while we highlight strong socioeconomic gradients in the adoption of electric vehicles, our findings may reflect both different preferences as well as barriers that limit the opportunity set of households. We leave it up to future research to further disentangle these two mechanisms, and to shed additional light on distributional effects of transportation policies aimed at mitigating local and global externalities.

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Online supporting material

Congestion pricing, air pollution, and individual-level behavioral responses

Authors: Elisabeth T. Isaksen and Bjørn G. Johanssen

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Appendix A Background

Figure A.1: Location of Bergen

Figure A.2: Location of Bergen city center and major roads

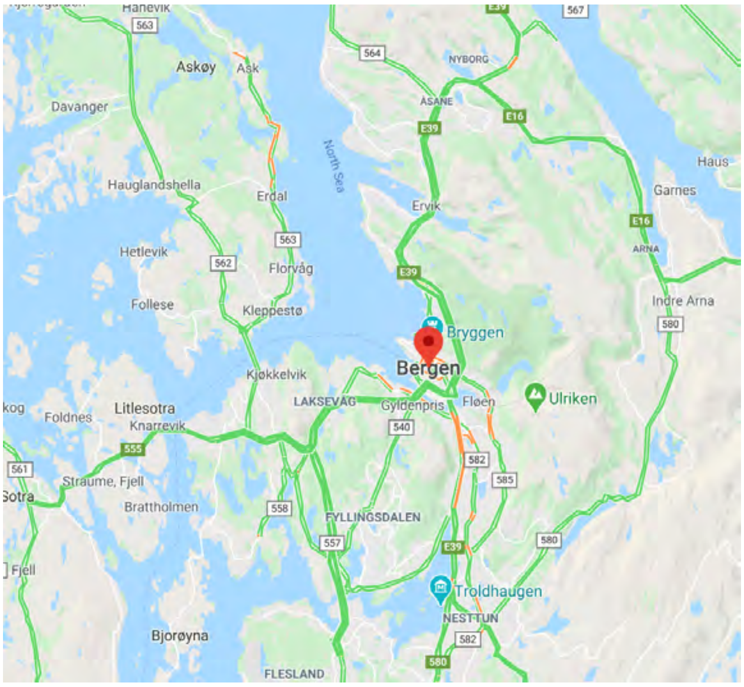
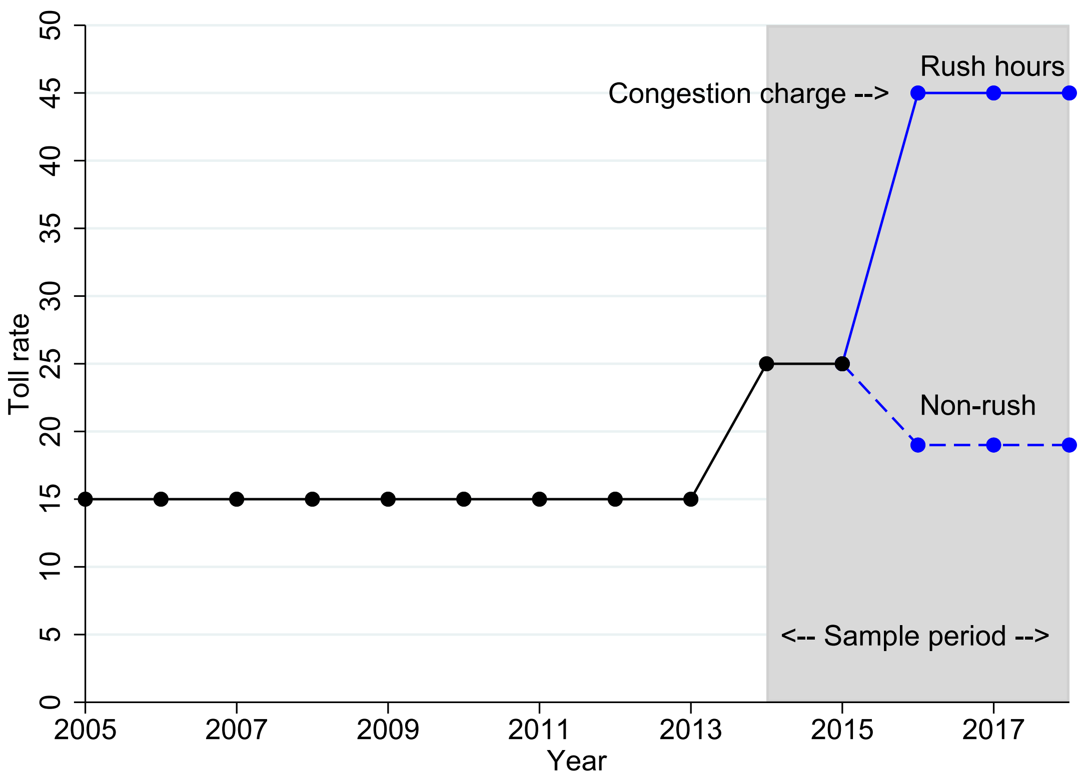


Table A.1: Congestion pricing in Norway. 2013-2019

	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: Prices are given in NOK. 10 NOK ≈ 1 EUR and ≈ 1.2 USD. Prices reflect the rate 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

Figure A.3: Toll road in Bergen. 2005-2018



Notes: Figures shows toll rates for Bergen for the period 2005 to 2017. The congestion charge was introduced on February 1 2016.

Table A.2: Key air pollutants and the relative contribution of different sources.

Source	**NO_2**	**PM_{10}**	**$PM_{2.5}$**	**SO_2**	**CO**	**O_3**
Exhaust	VH	M	H		S	
Wear and tear from roads, tires, and breaks		VH	M			
Sand added to increase friction of icy road surfaces		VH	M			
Wood-fired ovens		H	H			
Manufacturing industry	M	M	M	M		
Ship traffic	M	S	S	M		
Long-range pollution	S	M	H	S		VH

Notes: VH refers to very high contribution, H refers to high contribution, M refers to medium contribution, S refers to small contribution. The contribution of different sources are specific to Norway, and may differ from the most important sources in other countries. Note that the levels of CO in Norway are generally too low to represent any threat to human health. *Source:* NILU (2019).

Table A.3: The Norwegian EV incentives (as of January 2020)

Year	Instrument	Local incentive?
1990	Exempted from purchase/import taxes	
1996	Exempted from annual road tax	
1997	**Exempted from road toll**[1]	Yes
1997	Exempted from ferry charges[2]	Yes
1999	Free municipal parking[3]	Yes
2000	50 % reduced company car tax[4]	
2001	Exempted from 25% VAT on purchase	
2005	Access to bus lanes[5]	Yes
2015	Exempted 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)	

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.

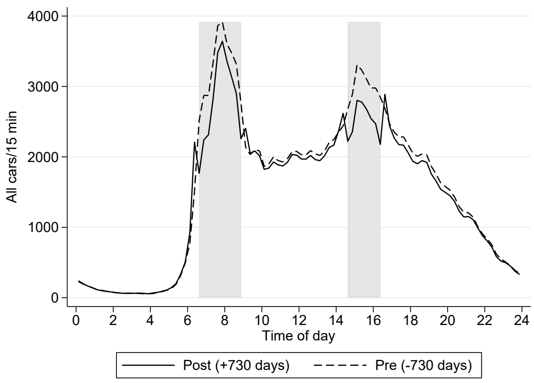
Appendix B Traffic

B.1 Data and descriptives

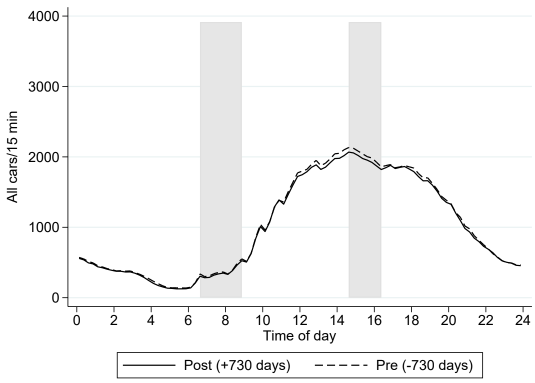
B.1.1 Raw means two years pre and post, by small and large vehicles

Figure B.1: Traffic volume by 15 min. intervals, 2 years pre and post Feb 1 2016.

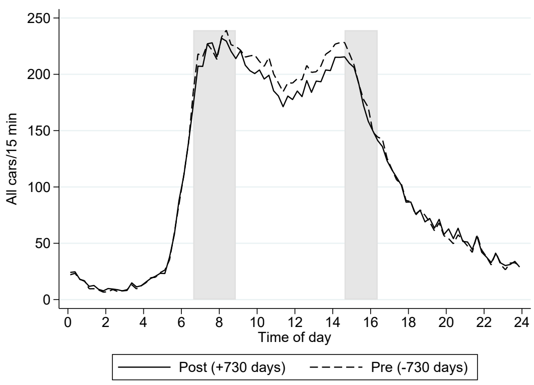
(a) Passenger cars. Weekday



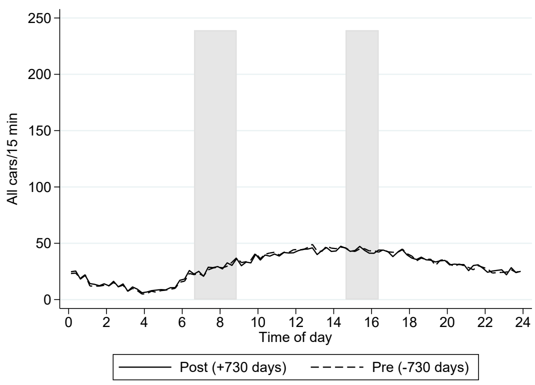
(b) Passenger cars. Weekend



(c) Trucks. Weekday



(d) Trucks. Weekend

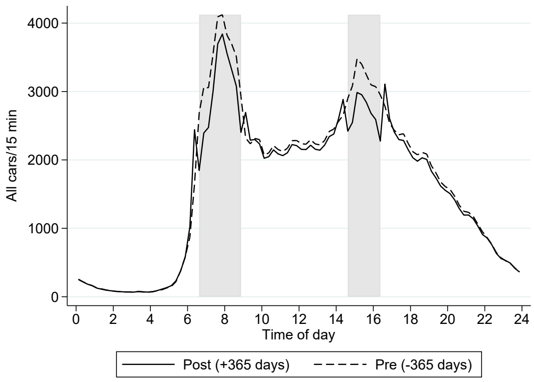


Notes: Figures show the average number of vehicles passing the toll cordon over the course of a day, based on 15 minute intervals. Panels (a) and (c) show averages for weekdays and panels (b) and (d) show averages for the weekend. Dashed lines indicate averages for the 730 days prior to policy implementation. Solid lines indicate averages for the 730 days post policy implementation. Gray shaded areas indicate rush hours. Passenger cars: all vehicles < 3500 kg. Trucks: all vehicles > 3500 kg.

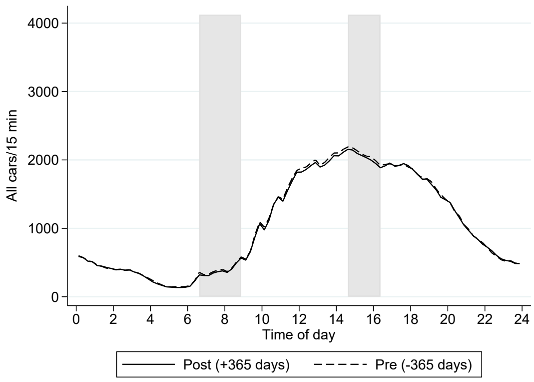
B.1.2 Raw means one year pre and post

Figure B.2: Traffic volume by 15 min. intervals, 1 year pre and post Feb 1 2016.

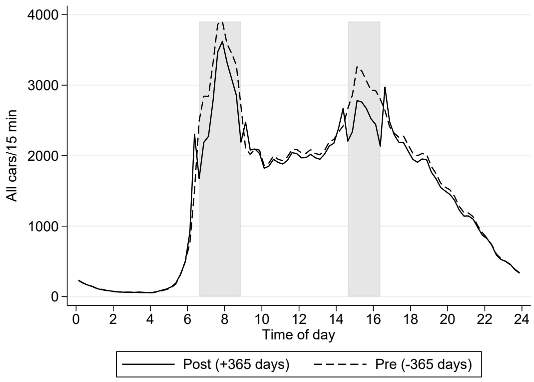
(a) All cars. Weekday



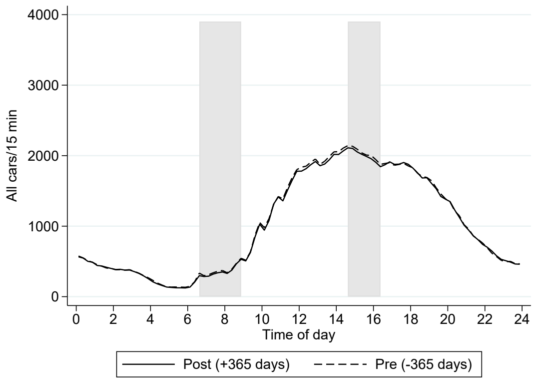
(b) All cars. Weekend



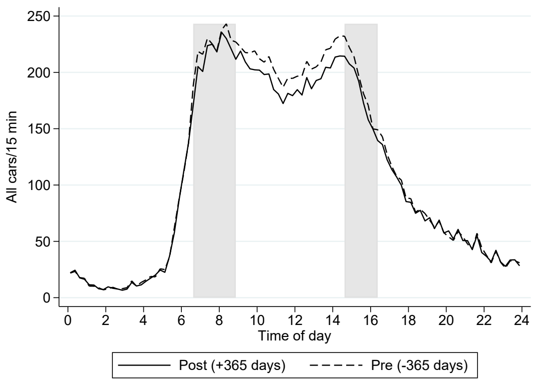
(c) Passenger cars. Weekday



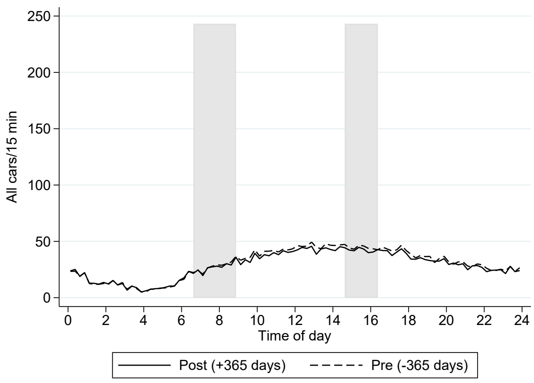
(d) Passenger cars. Weekend



(e) Trucks. Weekday



(f) Trucks. Weekend

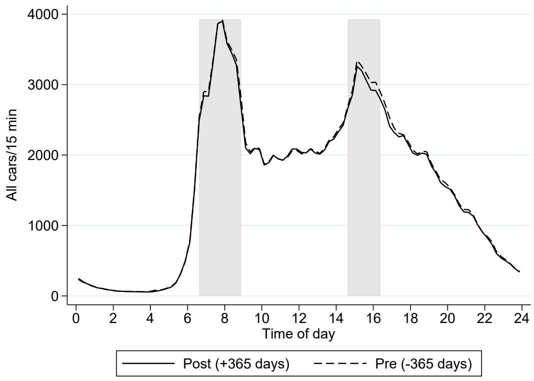


Notes: Figures show the average number of vehicles passing the toll cordon over the course of a day, based on 15 minute intervals. The left side panels show averages for weekdays and the right side panels show averages for the weekend. Dashed lines indicate averages for the 365 days prior to policy implementation. Solid lines indicate averages for the 365 days post policy implementation. Gray shaded areas indicate rush hours. Passenger cars: all vehicles < 3500 kg. Trucks: all vehicles > 3500 kg.

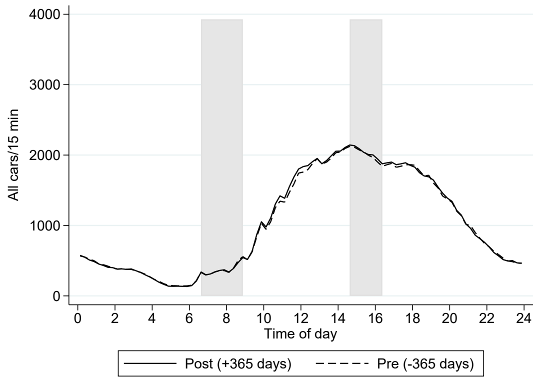
B.1.3 Placebo intervention: 1 year pre and post Feb 1st 2015

Figure B.3: Traffic volume by 15 min. intervals, 1 year pre and post Feb 1 2015 ("Placebo intervention").

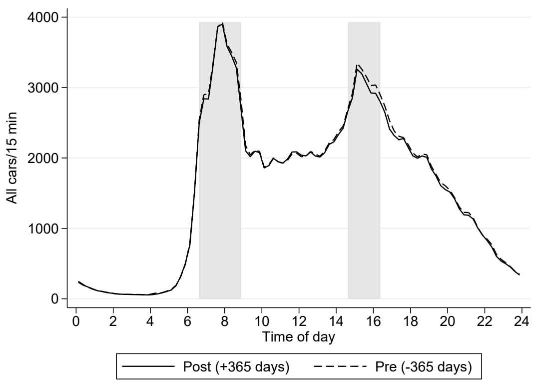
(a) All cars. Weekday



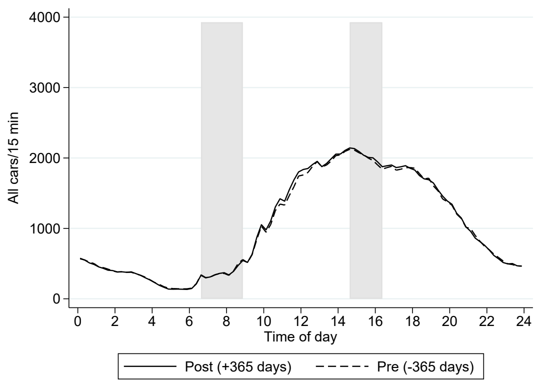
(b) All cars. Weekend



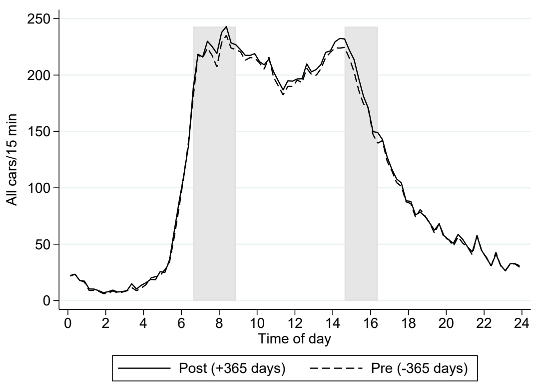
(c) Passenger cars. Weekday



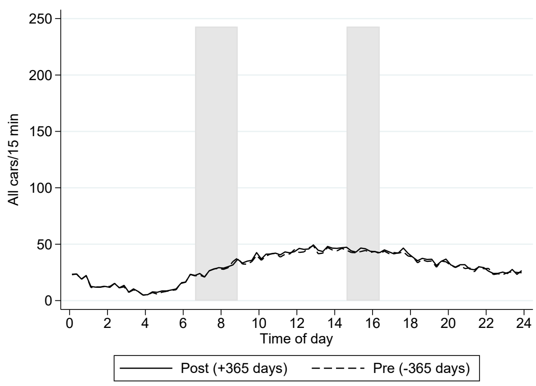
(d) Passenger cars. Weekend



(e) Trucks. Weekday



(f) Trucks. Weekend



Notes: Figures show the average number of vehicles passing the toll cordon over the course of a day, based on 15 minute intervals. The left side panels show averages for weekdays and the right side panels show averages for the weekend. Dashed lines indicate averages for the 365 days prior to Feb 1 2015. Solid lines indicate averages for the 365 days post Feb 1 2015. Gray shaded areas indicate rush hours. Passenger cars: all vehicles < 3500 kg. Trucks: all vehicles > 3500 kg.

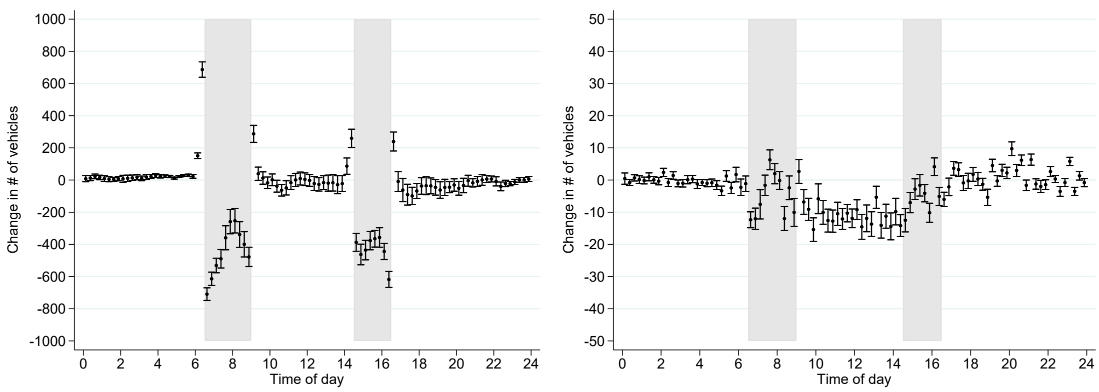
B.2 Supporting results and robustness checks

B.2.1 Results for small and large vehicles

Figure B.4 splits the effect between small and large vehicles. The effect for small vehicles follows the trend in Figure 4 closely, while the effect for large vehicles is more evenly spread throughout the day. There could be several reasons for this: Large vehicles consist mainly of trucks, which may to a greater extent be bound by delivery times and the truck drivers' work schedules, making avoiding the congestion charge difficult. The general decrease may stem from lower commercial activity in the city center during weekdays due to the congestion charge, reducing the demand for freight. Alternatively, the congestion charge could have incentivized shippers to optimize their consolidation routines, thereby reducing the number of trucks required. However, we do not have appropriate data to examine these mechanisms further. Figure B.5 displays similar results for a shorter time time frame of plus/minus one year of the date of policy implementation.

Figure B.4: DiD estimates by vehicle type and 15 min. intervals. 2 years pre/post

(a) Passenger cars. **(b)** Trucks.



Notes: Figure plots treatment effects estimated from from equation 1, where regressions are run separately for each 15 minute increment. Whiskers indicate 95% confidence intervals. Gray shaded areas indicate rush hours. Traffic is measured as total number of cars passing the toll cordon every 15 minutes. Standard errors are clustered on week. Passenger cars: all vehicles < 3500 kg. Trucks: all vehicles > 3500 kg.

Table B.1: DiD estimates on traffic volume by 15 min. intervals. 2 years pre/post

		Rush hours			Non-rush hours	
Dependent variable: # vehicles/15 minute interval	All day (1)	All (2)	Morning (3)	Evening (4)	+/-30 min (5)	Other (6)
Panel A: Passenger cars.						
Post × weekday	-74.52***	-442.1***	-440.7***	-432.1***	212.5***	-12.92
	(9.701)	(22.53)	(28.62)	(25.50)	(19.65)	(7.933)
Observations	87518	16416	9122	7294	7294	63808
Mean depvar (pre, weekday)	1520	2902	3021	2753	2143	1093
Change (%)	-4.90	-15.23	-14.59	-15.70	9.92	-1.18
Panel B: Trucks.						
Post × weekday	-3.146***	-4.920***	-4.999***	-4.840***	-4.686***	-2.507***
	(0.494)	(0.963)	(1.138)	(1.013)	(0.827)	(0.396)
Observations	87518	16416	9122	7294	7294	63808
Mean depvar (pre, weekday)	112	202	218	183	173	81
Change (%)	-2.81	-2.43	-2.30	-2.64	-2.70	-3.08
Weather controls ($X_{it}'\gamma$)	✓	✓	✓	✓	✓	✓
Month × year FE (λ_{ym})	✓	✓	✓	✓	✓	✓
Day-of-week × time-of-day FE (θ_{di})	✓	✓	✓	✓	✓	✓
Length of time interval (hours)	24	4.5	2.5	2	2	17.5

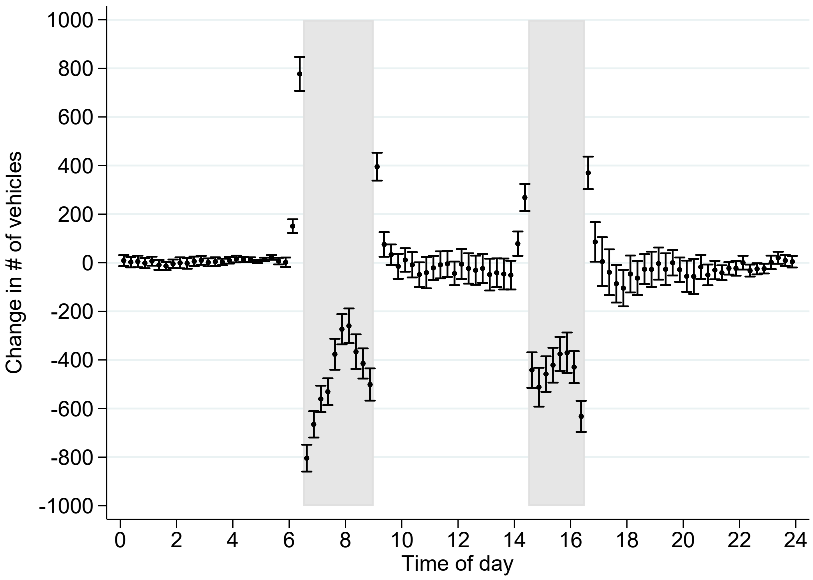
* p<0.10, ** p<0.05, *** p<0.01. Standard errors are clustered on week.
Notes: Table shows results from 2×6 separate regressions. Dependent variable is the number of vehicles passing the toll gates in Bergen during a 15 minute interval. Post×weekday refers to the β coefficient estimated from Equation 1. 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 pre and post policy implementation. Passenger cars: all vehicles < 3500 kg. Trucks: all vehicles > 3500 kg.

B.2.2 1 year before and after Feb 1 2016

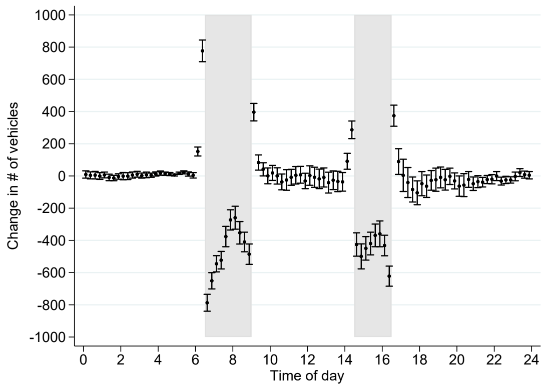
Table B.2 shows results when the sample is restricted to 365 days pre/post policy implementation. Results from this table are almost identical to Table 2.

Figure B.5: DiD estimates on traffic volume by 15 min. intervals. 1 year pre/post

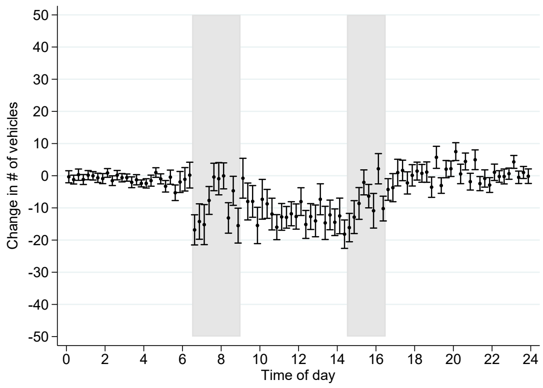
(a) All vehicles, 365 days pre/post Feb 1 2016.



(b) Passenger cars, 365 days pre/post Feb 1 2016.



(c) Trucks, 365 days pre/post Feb 1 2016.



Notes: Figures plots treatment effects estimated from from equation 1, where regressions are run separately for each 15 minute increment. Sample includes +/- 365 days of the treatment date. Whiskers indicate 95% confidence intervals. Gray shaded areas indicate rush hours. Traffic is measured as total number of cars passing the toll cordon every 15 minutes. Standard errors are clustered at the week level. Passenger cars: all vehicles < 3500 kg. Trucks: all vehicles > 3500 kg.

Table B.2: DiD estimates on traffic volume by 15 min. intervals. 1 year pre/post

		Rush hours			Non-rush hours	
Dependent variable: # vehicles/15 minute interval	All day (1)	All (2)	Morning (3)	Evening (4)	+/-30 min (5)	Other (6)
Panel A: All vehicles.						
Post × weekday	-74.70***	-468.0***	-475.4***	-458.2***	277.3***	-13.24
	(11.94)	(23.00)	(26.39)	(30.83)	(20.11)	(10.92)
Observations	43948	8244	4580	3664	3664	32040
Mean depvar (pre, weekday)	1627	3072	3205	2906	2329	1174
Change (%)	-4.59	-15.24	-14.83	-15.77	11.91	-1.13
Panel B: Passenger cars.						
Post × weekday	-70.60***	-459.5***	-466.4***	-450.1***	282.7***	-10.41
	(11.56)	(22.17)	(25.65)	(30.17)	(19.46)	(10.70)
Observations	43948	8244	4580	3664	3664	32040
Mean depvar (pre, weekday)	1514	2868	2986	2721	2155	1093
Change (%)	-4.66	-16.02	-15.62	-16.54	13.11	-0.95
Panel C: Trucks.						
Post × weekday	-4.107***	-8.484***	-8.928***	-8.089***	-5.327***	-2.826***
	(0.736)	(1.350)	(1.498)	(1.429)	(1.186)	(0.595)
Observations	43948	8244	4580	3664	3664	32040
Mean depvar (pre, weekday)	112	203	219	184	174	82
Change (%)	-3.66	-4.17	-4.08	-4.38	-3.06	-3.46
Weather controls ($X'_{it}\gamma$)	✓	✓	✓	✓	✓	✓
Month × year FE (λ_{ym})	✓	✓	✓	✓	✓	✓
Day-of-week × time-of-day FE (θ_{di})	✓	✓	✓	✓	✓	✓
Length of time interval (hours)	24	4.5	2.5	2	2	17.5

* p<0.10, ** p<0.05, *** p<0.01. Standard errors are clustered on week.

Notes: Table shows results from 18 separate regressions. Dependent variable is vehicles passing toll gates in Bergen during a 15 minute interval. Post×weekday refers to the β coefficient estimated from Equation 1. 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 365 days pre and post policy implementation. Passenger cars: all vehicles < 3500 kg. Trucks: all vehicles > 3500 kg.

B.2.3 Different aggregation levels

Table B.3 shows results for three different aggregation levels: daily observations ($N = 656$); toll gate specific observations on a daily level ($N = 9,182$); and toll gate specific observations on a 15 minute resolution ($N = 850,004$). The point estimates will naturally be very different since the observational unit is changed, but the table indicates that predicted percentage changes are almost identical. Furthermore, all coefficients except those in column (6) are significant at the 0.01 level.

Table B.3: DiD, different aggregation levels. 2 years pre/post.

		Rush hours			Non-rush hours	
Dependent variable: # vehicles	All day (1)	All (2)	Morning (3)	Evening (4)	+/-30 min (5)	Other (6)
Panel A: Daily.						
Post × weekday	-7218.6***	-7982.8***	-4444.7***	-3533.6***	1662.1***	-394.0
	(1099.9)	(446.5)	(302.1)	(216.1)	(162.7)	(760.1)
Observations	915	914	913	912	914	915
Mean depvar (pre, weekday)	156666	55873	32392	23486	18534	82020
Change (%)	-4.61	-14.29	-13.72	-15.05	8.97	-0.48
Panel B: Toll gate level, daily.						
Post × weekday	-517.8***	-567.9***	-323.9***	-248.2***	119.6***	-68.52
	(76.58)	(31.58)	(20.23)	(15.08)	(11.46)	(47.53)
Observations	12807	12790	12766	12758	12791	12807
Mean depvar (pre, weekday)	11193	3992	2316	1679	1324	5877
Change (%)	-4.63	-14.22	-13.98	-14.79	9.03	-1.17
Panel C: Toll gate level, 15 minute resolution.						
Post × weekday	-5.796***	-32.58***	-33.43***	-30.74***	15.16***	-1.200**
	(0.719)	(1.626)	(2.035)	(1.800)	(1.411)	(0.575)
Observations	1186729	228343	126367	101976	101165	857221
Mean depvar (pre, weekday)	120.91	222.01	231.68	209.92	166.02	88.19
Change (%)	-4.79	-14.68	-14.43	-14.65	9.13	-1.36

* p<0.10, ** p<0.05, *** p<0.01. Standard errors are clustered on week.
Notes: The outcome variable is the total number of vehicles passing toll gates. The specifications are the same as the main specifications in Table 2, but fixed effects are modified to take into account differences in aggregation levels. In panel A, observations are daily and regressions include month×year and day-of-week fixed effects. In panel B, observations are daily on the toll gate level, and regressions include month×year×toll gate and day-of-week×toll gate fixed effects. In panel C observations are per toll gate per 15 minute interval, and regressions include month×year×toll gate and day-of-week×time-of-day×toll gate fixed effects.

B.2.4 Different model specifications

Table B.4 shows that the main results are not sensitive to the level of the fixed effects.

Table B.4: DiD, different fixed effects. 2 years pre/post.

	Different levels of fixed effects:				
# vehicles	(1)	(2)	(3)	(4)	(5)
All day	-82.17***	-75.39***	-74.45***	-77.09***	-70.43***
	(27.25)	(10.90)	(9.192)	(9.810)	(9.326)
Rush hours:					
All	-440.7***	-436.1***	-432.1***	-444.9***	-421.3***
	(30.09)	(29.46)	(22.11)	(22.20)	(21.80)
Morning rush	-445.6***	-448.1***	-425.0***	-448.4***	-422.0***
	(30.11)	(30.20)	(30.03)	(29.78)	(30.94)
Afternoon rush	-437.6***	-435.6***	-427.6***	-432.4***	-411.4***
	(25.78)	(25.79)	(24.48)	(26.01)	(25.28)
Non-rush hours:					
+/-30 min	192.5***	210.8***	212.2***	210.8***	221.9***
	(29.60)	(20.94)	(18.46)	(19.47)	(18.36)
Other	-19.69	-14.39	-15.26**	-15.40*	-13.27*
	(27.60)	(9.985)	(7.249)	(7.989)	(7.235)
Weather controls	✓	✓	✓	✓	✓
Year×month	✓	✓		✓	
Year×week			✓		✓
DoW	✓	✓			
ToD		✓			
DoW×ToD			✓		
Month×DoW×ToD				✓	
Week×DoW×ToD					✓

* $p<0.10$, ** $p<0.05$, *** $p<0.01$. Standard errors are clustered on week.

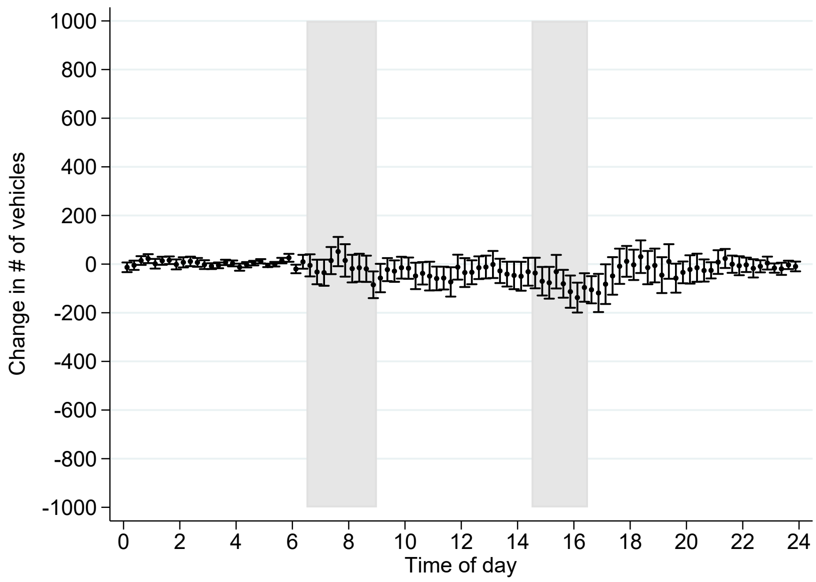
Notes: The outcome variable is the total number of vehicles passing toll gates each 15 minute interval. The specifications are the same as the main specifications in Table 2, but with varying levels of fixed effects column-wise. Each coefficient is from a separate regression (30 regressions in total). DoW: "day-of-week", ToD: "Time-of-day". Columns (1) and (2) have more aggregated (i.e. fewer) fixed effects than the main specification. Columns (3)-(5) have additional fixed effects. Note for example that the most detailed specification (5) will have at most four observations for each fixed effect in the second set (time-of-day×day-of-week×week-of-year; one observation for each time interval each year).

B.2.5 Placebo intervention: 1 year pre/post Feb 1st, 2015.

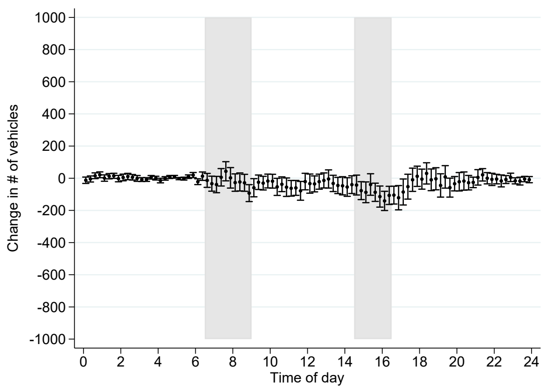
Figure B.6 presents estimated effects of placebo treatments, assuming that the congestion charge took place February 1st 2015, i.e. one year earlier. These placebo treatments are estimated on a dataset that includes 365 days pre/post the placebo treatment date, since we don't have access to traffic data earlier than 2014. The estimated effect of the placebo treatment is insignificantly different from zero for most of the 96 estimated coefficients, increasing our confidence in the fact that the estimated pre/post weekend/weekday difference in our main specification is not driven by changes over time in unobservables affecting traffic during weekends and weekdays differently.

Figure B.6: Total traffic volume by 15 min. intervals, DiD estimates.

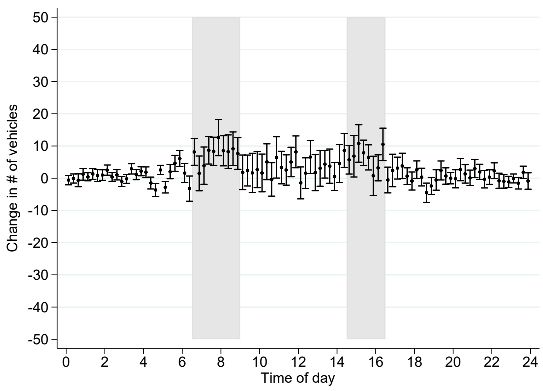
(a) All vehicles. Placebo (1feb2015).



(b) Passenger cars. Placebo (Feb 1, 2015).



(c) Trucks. Placebo (Feb 1, 2015).



Notes: Figures plots treatment effects estimated from from equation 1, assuming the policy took place Feb 1 2015 ("placebo treatment" one year earlier'). Regressions are run separately for each 15 minute increment. Sample includes +/- 365 days of the placebo treatment date. Whiskers indicate 95% confidence intervals. Gray shaded areas indicate rush hours. Traffic is measured as total number of cars passing the toll cordon every 15 minutes. Standard errors are clustered on week. Passenger cars: all vehicles < 3500 kg. Trucks: all vehicles > 3500 kg.

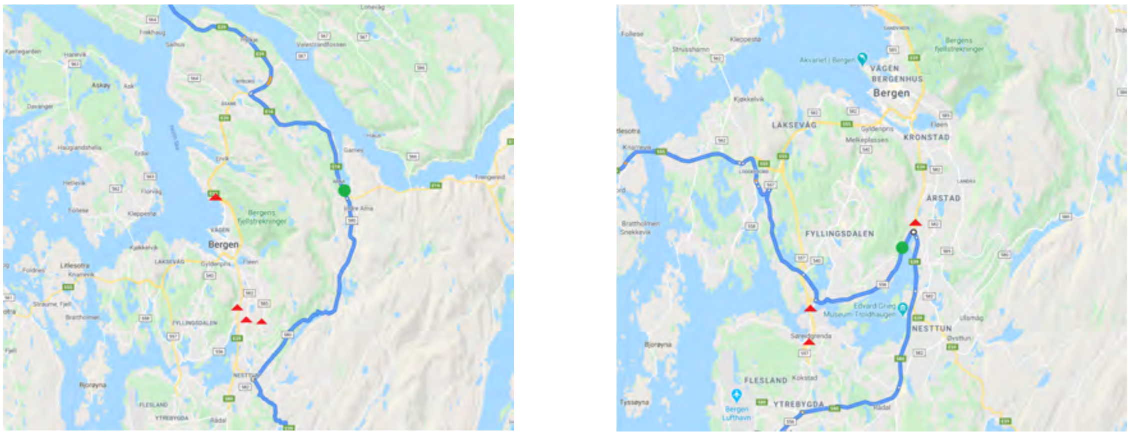
B.3 Spatial spillovers

Cars driving into the center of Bergen are not able to avoid toll charges. However, cars passing Bergen have alternative routes to avoid the cordon toll completely. This allows us to examine potential spatial spillover effects.

The two main routes to avoid toll payments, depending on which direction the car is coming from, are displayed in Figure B.7.[56] The toll gates avoided by the alternative routes are marked as red triangles (See Figure 1 for the location of all toll gates of the cordon toll).

Figure B.7: Alternatives for bypassing toll gates for transit cars

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



Notes: Blue lines are routes around Bergen that avoid the cordon toll completely. Red triangles display the relevant toll gates. Green circles mark traffic censors from which we obtain traffic data. Source: Google Maps.

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 – see Figure B.7a. This route is 6 minutes longer.

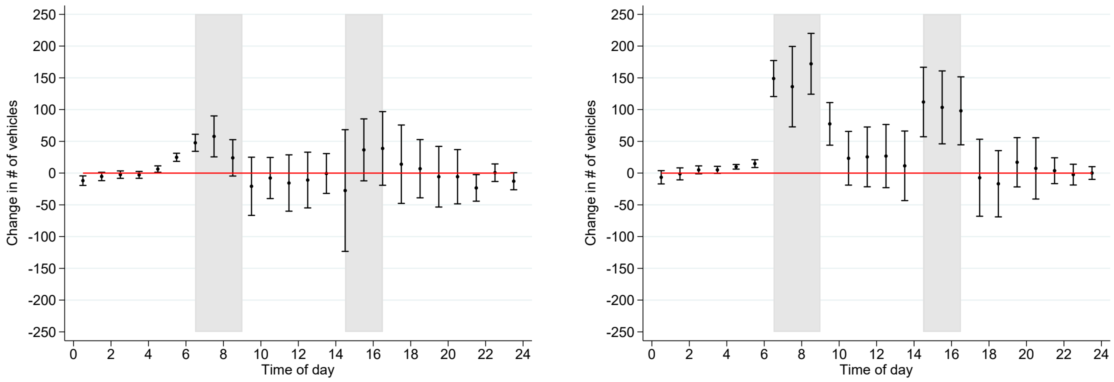
Cars passing Bergen in the south-west direction are also able to avoid the toll cordon by taking roads E39/556 rather than the direct tunnel under the strait (road 557). This is a detour of 9 minutes for cars arriving from the south-west, as the route illustrated in Figure B.7b. However, for cars arriving from the south-east (e.g. from E39) this route is only about one minute longer.

To examine spatial spillovers we collect vehicle count data from the traffic sensors that are marked as green dots in the figure above. These data are hourly and publicly

[56]We are grateful to the Norwegian Public Roads Administration for making us aware of these alternative routes, see e.g. NPRA (2018). Note also that as of April 6th 2019, 15 new toll gates were put in operation. These toll gates cover the alternative routes displayed in Figure B.7, making it infeasible to avoid tolls under the current scheme.

Figure B.8: DiD estimates on traffic volume by hourly intervals.

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



Notes: Figure plots treatment effects estimated from from equation 1, where β is allowed to vary by hourly increments. Whiskers indicate 95% confidence intervals. Gray shaded areas indicate rush hours. Traffic is measured as total number of cars registered by the traffic censors in both directions each hour. Standard errors are clustered at the week level.

available.[57]

A limiting factor is that not much traffic count data is available pre policy implementation. For the north-south direction, we have hourly data from September 9th 2015 and onwards. To balance out seasonal variation pre-post, we therefore focus on the period September 9th to January 31st. This gives us one pre period and two post periods. For the south-west direction, we have data for a whole year pre policy and two whole years after.[58] Note that as the data is hourly, several of the observations contain 30 minutes during rush hours and 30 minutes during non-rush.

For the north-south direction, Figure B.8 indicates small treatment effects that are similar in magnitude during the morning and evening rush hours. However only the effects during the morning rush are significantly larger than zero. The effect is more marked and larger in magnitude for the south-west direction, and all hourly estimates that (partly) cover rush hours are significantly larger than zero. The effect is slightly larger during the morning rush.

Table B.5 illustrates effects for selected time intervals. We estimate the spatial spillover to be around 1,000 vehicles per day.[59]

[57] We use data from the censors "Kråkenes" and "Indre Arna EV16", which can be accessed here: `https://www.vegvesen.no/trafikkdata/start/`. Note that data from several other traffic sensors along the same routes are available; however, the time periods for which data can be accessed is relatively limited and different for each sensor. We found these sensors to have data available for the most sensible time periods pre/post policy implementation.

[58] Apart from the period length, data selection follows the same steps as the toll gate data. This means that the same observations are removed due to holidays.

[59] The "all day" regression indicates that the increase in numbers of vehicles per day is $(5.65 + 38.27) \times 24 = 1,054$. The regression for the six hourly intervals covering rush hours indicates that the number of vehicles per day during rush is $(31.4 + 125.3) \times 6 = 940$.

Table B.5: DiD estimates on traffic volume

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.					
Post × weekday	5.653	31.40*	42.87***	21.03	-2.104
	(7.762)	(15.55)	(8.977)	(26.30)	(6.594)
Observations	8385	2097	1043	1054	6288
Mean depvar (pre, weekday)	718.40	1329.67	1042.33	1612.77	514.92
Change (%)	0.79	2.36	4.11	1.30	-0.41
Panel B: South-west.					
Post × weekday	38.27***	125.3***	153.5***	103.6***	10.10
	(8.632)	(18.24)	(22.32)	(22.63)	(7.975)
Observations	16063	4016	2007	2009	12047
Mean depvar (pre, weekday)	1014.38	1918.47	1703.28	2133.35	712.85
Change (%)	3.77	6.53	9.01	4.86	1.42
Weather controls ($X'_{it}\gamma$)	✓	✓	✓	✓	✓
Month × year FE (λ_{ym})	✓	✓	✓	✓	✓
Day-of-week × time-of-day FE (θ_{di})	✓	✓	✓	✓	✓

* p<0.10, ** p<0.05, *** p<0.01. Standard errors are clustered on week.
Notes: The outcome 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 2, but time periods are defined differently since only hourly data is available.

Appendix C Air pollution

C.1 Data and descriptives

C.1.1 Summary statistics

Table C.1: Summary statistics air pollution and weather. All days of the week. Bergen

	mean	sd	min	max	count
NO2	38.83	30.44	0	235	33,332
PM10	16.53	13.74	0	851	33,600
air temperature	8.92	5.78	-9	31	33,903
precipitation	0.28	0.77	0	27	33,179
wind speed	3.56	2.35	0	20	33,903
wind direction	197.57	80.98	0	360	33,903
wind direction (1-4)	2.76	0.98	1	4	33,903
inversion	0.04	0.20	0	1	33,903

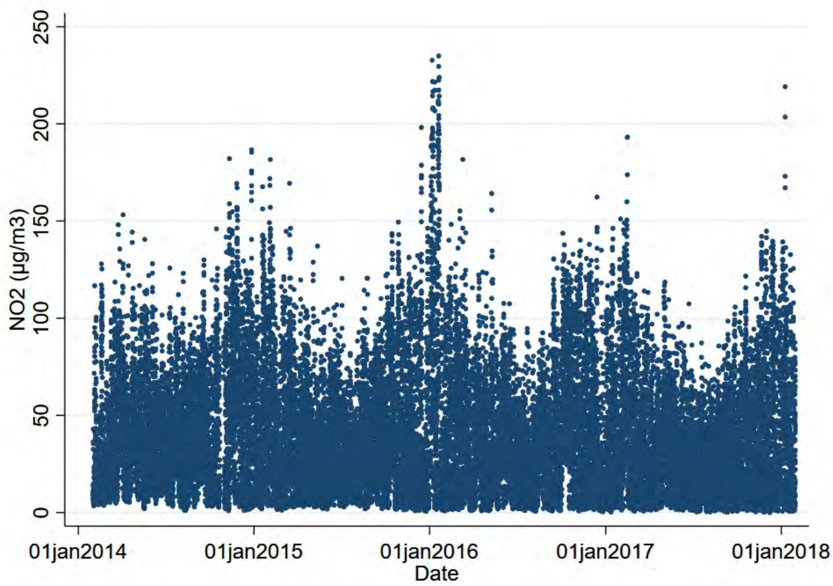
Table C.2: Summary statistics air pollution and weather. Tuesday-Thursday. Bergen

	mean	sd	min	max	count
NO2	43.61	32.35	0	235	14,102
PM10	18.06	14.48	0	230	14,343
air temperature	8.96	5.76	-9	31	14,514
precipitation	0.29	0.81	0	27	14,191
wind speed	3.53	2.32	0	17	14,514
wind direction	197.25	81.09	0	358	14,514
wind direction (1-4)	2.75	0.99	1	4	14,514
inversion	0.05	0.22	0	1	14,514

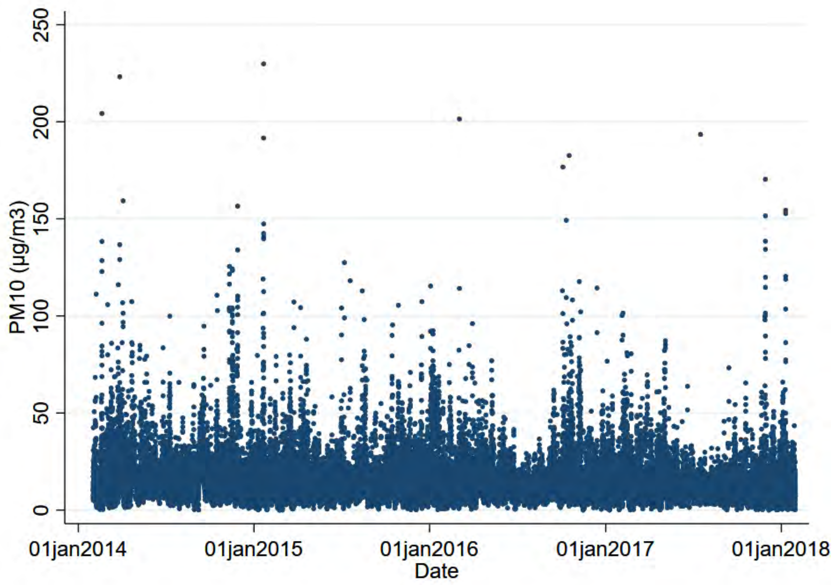
C.1.2 Scatter plots

Figure C.1: Hourly observations of air pollution. 2 years before and after Feb. 1 2016.

(a) NO_2



(b) PM_{10}

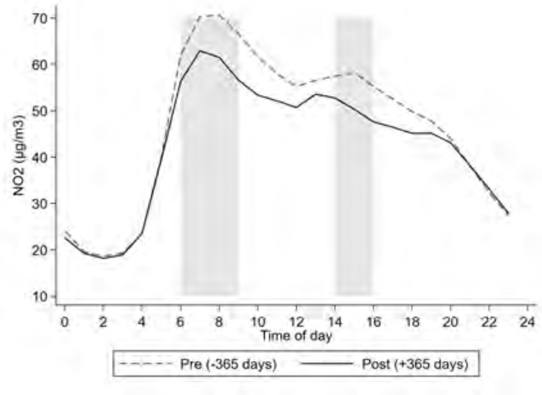


Notes: Figures show hourly air pollution readings from the station “Danmarksplass” in Bergen.

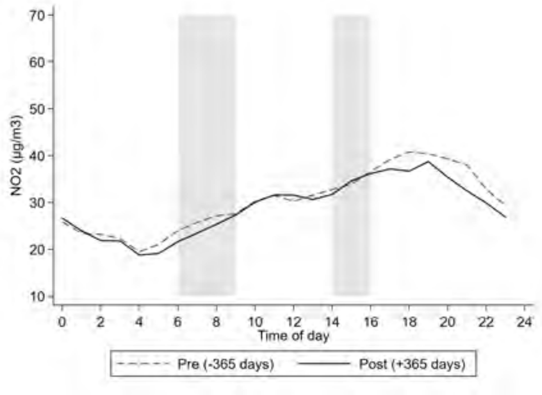
C.1.3 Air pollution 1 year before and after Feb 1 2016

Figure C.2: Ambient air pollution 1 year before and after Feb 1 2016

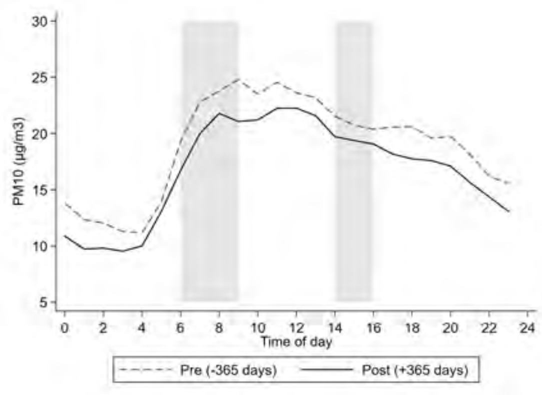
(a) NO_2. Weekday



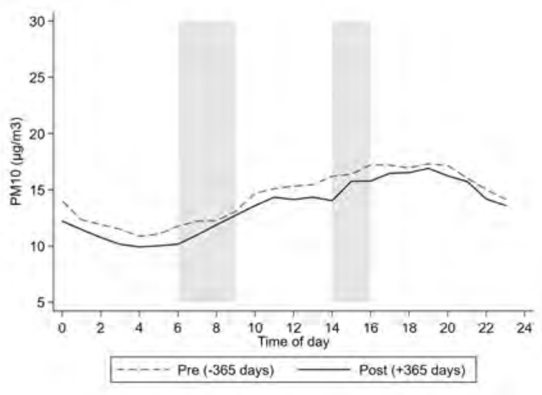
(b) NO_2. Weekend



(c) PM_{10}. Weekday



(d) PM_{10}. Weekend

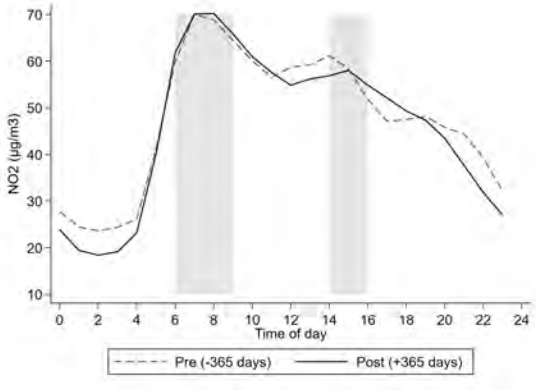


Notes: Figure shows average ambient air pollution over the course of a day for the pollution monitoring station located at Danmarksplass in Bergen. Values are based on 60 minute intervals. Panel (a) and (c) show averages for weekdays (Tuesday-Thursday) and panels (b) and (d) show averages for weekends (Saturday-Sunday). Dashed lines indicate averages for the 365 days prior to policy implementation (Feb 1 2016). Solid lines indicate averages for the 365 days post policy implementation. Gray shaded areas indicate rush hours. Note that congestion charging is not active during weekends. Pollution is measured as micrograms per cubic meter of air(μg/m3). See Appendix Figure C.3 for ambient air pollution 365 days pre and post Feb 1 2015 (”Placebo intervention”).

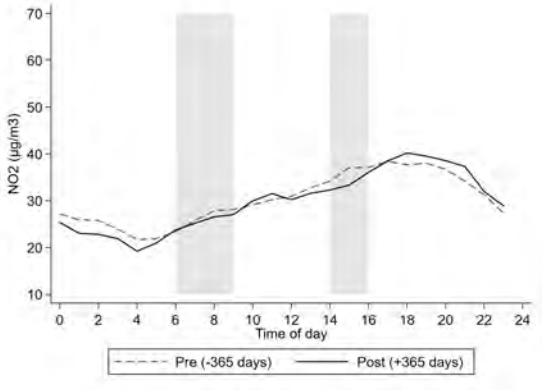
C.1.4 Placebo intervention

Figure C.3: Ambient air pollution, 1 year before and after Feb 1 2015 ("Placebo intervention")

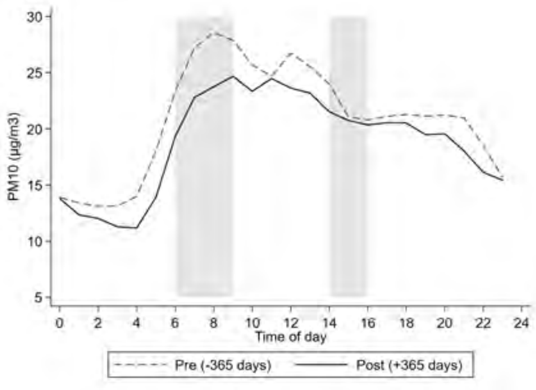
(a) NO_2. Weekday



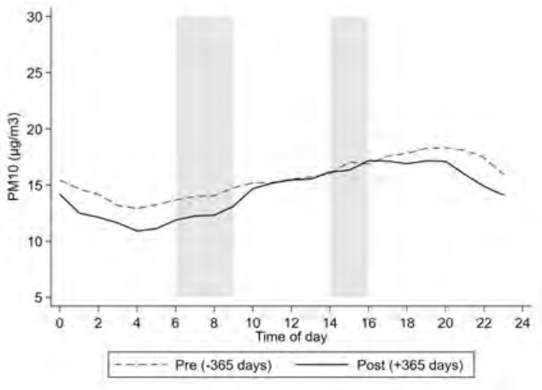
(b) NO_2. Weekend



(c) PM_{10}. Weekday



(d) PM_{10}. Weekend



Notes: Figure shows ambient air pollution on weekdays (Tuesday-Thursday) for the pollution monitoring station located at Danmarksplass in Bergen. Pollution is measured as micrograms per cubic meter of air(μg/m3). Ambient air quality standards for NO2 (annual mean): 40 μg/m3. Ambient air quality standards for PM10 (annual mean): 25 μg/m3. The variable "hour" on the x-axis indicate the start of the time interval, e.g., hour 6 indicates the time interval 06:00-06:59.

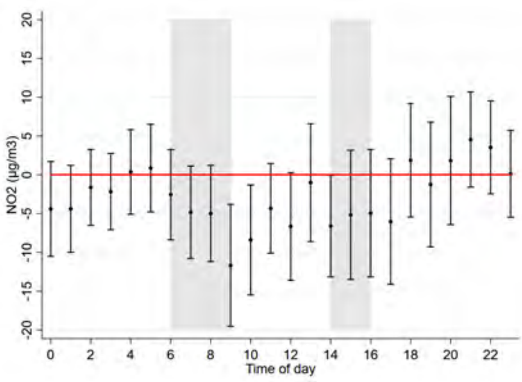
C.2 Supporting results and robustness checks

C.2.1 Results based on 1 year pre and post Feb 1 2016

Figure C.4 and Table C.3 shows results when the sample is restricted to 365 days pre/post policy implementation. Estimated daily treatment effects on NO_2 are very similar to results in our main specification, where we use a period of 2 years before and after policy implementation. As is our main results, we find no significant effects on PM_{10}.

Figure C.4: DiD estimates on air pollution (μg/m3), by 60 min. intervals. 1 year pre/post

(a) NO_2: Treatment date: Feb 1, 2016



(b) PM_{10}: Treatment date: Feb 1, 2016

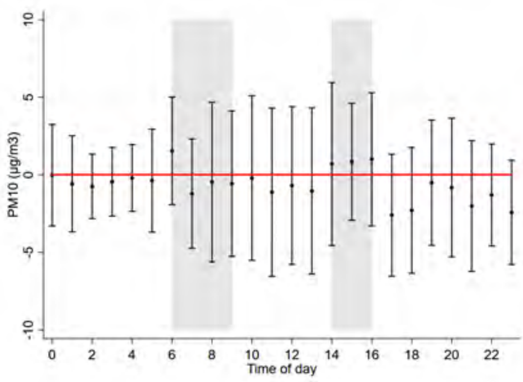


Table C.3: DID estimates on NO_2 and PM_{10}. 1 year pre/post

	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/m3)	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: NO_2						
Post × weekday	-3.089*	-3.064	-5.387**	-5.702***	1.991	-2.218
	(1.812)	(1.945)	(2.038)	(2.066)	(2.686)	(1.979)
Observations	10865	8132	5396	3157	2736	2733
Mean depvar (pre-weekday)	47.40	55.60	61.40	64.49	42.65	24.64
Change (%)	-6.52	-5.51	-8.77	-8.84	4.67	-9.00
Panel B: PM_{10}						
Post × weekday	-0.864	-0.725	-0.260	0.250	-1.597	-0.396
	(1.337)	(1.541)	(1.665)	(1.586)	(1.689)	(1.016)
Observations	10892	8170	5436	3180	2737	2719
Mean depvar (pre-weekday)	18.19	20.32	21.63	21.22	17.91	11.57
Change (%)	-4.75	-3.57	-1.20	1.18	-8.92	-3.43
Weather controls ($X'_{it}\gamma$)	✓	✓	✓	✓	✓	✓
Month×year FE (λ_{ym})	✓	✓	✓	✓	✓	✓
Day of week×time-of-day FE (θ_{di})	✓	✓	✓	✓	✓	✓

Notes: Table shows results from 12 separate regressions. Dependent variable is ambient air pollution measured as mean levels of NO_2 or PM_{10} (μg/m3) during a 60 minute interval. Post × Weekday refers to the coefficient X estimated from equation X. 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 365 days pre and post policy implementation. Standard errors are clustered on week.

C.2.2 Different model specification: fixed effects

Table C.4 shows average daily treatment effects for NO_2 and PM_{10} using different specifications of fixed effects. From Panel A, we see that our main results on NO_2 is not sensitive to the level of the fixed effects; the treatment effect ranges from 3.064 to 3.180 $\mu g/m^3$ (or 6.52 to 6.77%, respectively). For PM_{10} the estimated treatment effect becomes significant under the most conservative specification (column 7), where we only exploit variation within a week instead of within a month (see Panel B).

Table C.4: DID estimates on NO_2 and PM_{10}. 2 years pre/post

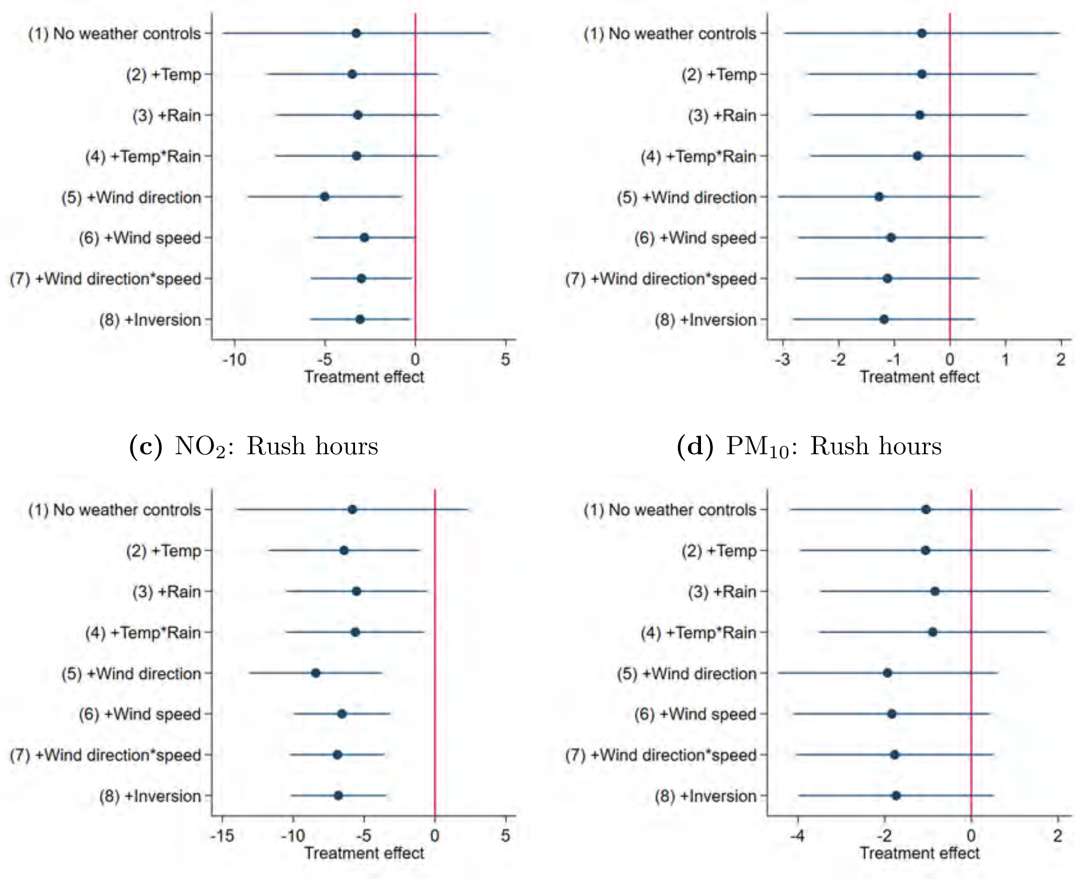
Dependent variable:	Daily treatment effects						
ambient air pollution (μg/m3)	(1)	(2)	(3)	(4)	**(5)***	(6)	(7)
Panel A: NO_2							
Post × weekday	-3.180**	-3.082**	-3.099**	-3.075**	-3.064**	-3.120**	-3.153**
	(1.502)	(1.495)	(1.497)	(1.387)	(1.369)	(1.339)	(1.324)
Observations	21438	21438	21438	21438	21438	21438	21109
Mean depvar (pre-weekday)	47.01	47.01	47.01	47.01	47.01	47.01	46.99
Change (%)	-6.77	-6.56	-6.59	-6.54	-6.52	-6.64	-6.71
Panel B: PM_{10}							
Post × weekday	-1.311	-1.174	-1.173	-1.187	-1.185	-1.389	-1.634**
	(0.920)	(0.841)	(0.840)	(0.816)	(0.809)	(0.848)	(0.807)
Observations	21624	21624	21624	21624	21624	21624	21358
Mean depvar (pre-weekday)	18.81	18.81	18.81	18.81	18.81	18.81	18.83
Change (%)	-6.97	-6.24	-6.23	-6.31	-6.30	-7.38	-8.68
Weather controls	✓	✓	✓	✓	✓	✓	✓
Post	✓	✓	✓	✓	✓	✓	✓
Weekday	✓	✓	✓	✓	✓	✓	✓
Month × year		✓	✓	✓	✓		
DoW			✓	✓			
ToD				✓			
DoW × ToD					✓	✓	
Week × DoW × ToD							✓
Week × year						✓	✓

Notes: Table shows results from 12 separate regressions. Dependent variable is ambient air pollution measured as mean levels of NO_2 or PM_{10} (μg/m3) during a 60 minute interval. Estimated effects reflect daily average effects. DoW is short for day of week (Monday-Thursday). ToD is short for time of day (24 hours). Sample is restricted to 2 years pre and post policy implementation. Standard errors are clustered on week. The coefficients in column (5) correspond to the main estimates in column (1) in Table 3.

C.2.3 Different model specifications: weather controls

Figure C.5 plots treatment effects for NO_2 and PM_{10} using different combinations of weather controls. Corresponding regression coefficients are shown in Table C.5 and Table C.6. The magnitude of the estimated treatment effects are relatively stable across the different specifications. What the weather variables are primarily doing is tightening the confidence intervals. However, adding wind direction to the set of controls (specification #5) does seem to increase the magnitude of the treatment coefficients somewhat.

Figure C.5: DiD estimates on air pollution (μg/m3). Different weather controls



(a) NO_2: All day

(b) PM_{10}: All day

(c) NO_2: Rush hours

(d) PM_{10}: Rush hours

Notes: Each subfigure shows results from 8 separate regressions. Dependent variable is ambient air pollution measured as mean levels of NO_2 or PM_{10} (μg/m3) during a 60 minute interval. Panel (a) and (b) show daily average effects while Panel (c) and (d) show results for rush hours. Whiskers show 95 % confidence intervals. Except for weather controls, the model specification is the same as in the main regression table (Table 3). Standard errors are clustered on week. Temp refers to a polynomial of air temperature of degree 3; Rain refers to a polynominal precipitation of degree 2; Temp*Rain refers to an interaction of temperature and precipitation; Wind speed refers to a polynominal of wind speed of degree 2; WInd direction refers to four dummies for wind direction (north, south, east and west); Wind direction * speed refers to an interaction between wind direction and wind speed; Inversion refers to a dummy variable for inversion episodes. We estimate two sets of each of these weather control variables; one for weekdays and one for weekends.

Table C.5: DID estimates on NO_2 and PM_{10}. 2 years pre/post

Dependent variable:	Daily treatment effects							
ambient air pollution (μg/m3)	(1)	(2)	(3)	(4)	(5)	(6)	(7)	**(8)***
Panel A: NO_2								
Post × weekday	-3.276	-3.494	-3.187	-3.255	-5.022**	-2.821*	-2.985**	-3.064**
	(3.660)	(2.334)	(2.222)	(2.218)	(2.101)	(1.396)	(1.376)	(1.369)
Observations	21530	21530	21438	21438	21438	21438	21438	21438
Mean depvar (pre-weekday)	46.95	46.95	47.01	47.01	47.01	47.01	47.01	47.01
Change (%)	-6.98	-7.44	-6.78	-6.92	-10.68	-6.00	-6.35	-6.52
Panel B: PM_{10}								
Post × weekday	-0.506	-0.505	-0.542	-0.581	-1.273	-1.061	-1.124	-1.185
	(1.222)	(1.017)	(0.955)	(0.949)	(0.900)	(0.828)	(0.814)	(0.809)
Observations	21716	21716	21624	21624	21624	21624	21624	21624
Mean depvar (pre-weekday)	18.83	18.83	18.81	18.81	18.81	18.81	18.81	18.81
Change (%)	-2.69	-2.68	-2.88	-3.09	-6.77	-5.64	-5.98	-6.30
Temperature		✓	✓	✓	✓	✓	✓	✓
Precipitation			✓	✓	✓	✓	✓	✓
Temperature × precipitation				✓	✓	✓	✓	✓
Wind direction					✓	✓	✓	✓
Wind speed						✓	✓	✓
Wind direction × wind speed							✓	✓
Inversion								✓

Notes: Table shows results from 16 separate regressions. Dependent variable is ambient air pollution measured as mean levels of NO_2 or PM_{10} (μg/m3) during a 60 minute interval. Estimated effects reflect daily average effects. Sample is restricted to 2 years pre and post policy implementation. Except for the weather controls, the model specification is the same as in the main regression table (Table 3). Standard errors are clustered on week. Temp refers to a polynomial of air temperature of degree 3; Rain refers to a polynominal precipitation of degree 2; Temp*Rain refers to an interaction of temperature and precipitation; Wind speed refers to a polynomial of wind speed of degree 2; Wind direction refers to four dummies for wind direction (north, south, east and west); Wind direction×speed refers to an interaction between wind direction and wind speed; Inversion refers to a dummy variable for inversion episodes. We estimate two sets of each of these weather control variables; one for weekdays and one for weekends.The coefficients in column (8) correspond to the main estimates in column (1) in Table 3.

Table C.6: DID estimates on NO_2 and PM_{10}. 2 years pre/post. Rush hours only

Dependent variable:	Daily treatment effects							
ambient air pollution (μg/m3)	(1)	(2)	(3)	(4)	(5)	(6)	(7)	**(8)***
Panel A: NO_2								
Post × weekday	-5.828	-6.406**	-5.540**	-5.614**	-8.413***	-6.564***	-6.880***	-6.813***
	(4.028)	(2.641)	(2.451)	(2.416)	(2.324)	(1.687)	(1.648)	(1.658)
Observations	6255	6255	6227	6227	6227	6227	6227	6227
Mean depvar (pre-weekday)	63.70	63.70	63.78	63.78	63.78	63.78	63.78	63.78
Change (%)	-9.15	-10.06	-8.69	-8.80	-13.19	-10.29	-10.79	-10.68
Panel B: PM_{10}								
Post × weekday	-1.045	-1.054	-0.839	-0.891	-1.933	-1.834	-1.768	-1.735
	(1.543)	(1.430)	(1.308)	(1.297)	(1.252)	(1.122)	(1.119)	(1.115)
Observations	6341	6341	6314	6314	6314	6314	6314	6314
Mean depvar (pre-weekday)	21.76	21.76	21.76	21.76	21.76	21.76	21.76	21.76
Change (%)	-4.80	-4.84	-3.86	-4.09	-8.89	-8.43	-8.13	-7.97
Temperature		✓	✓	✓	✓	✓	✓	✓
Precipitation			✓	✓	✓	✓	✓	✓
Temperature × precipitation				✓	✓	✓	✓	✓
Wind direction					✓	✓	✓	✓
Wind speed						✓	✓	✓
Wind direction × wind speed							✓	✓
Inversion								✓

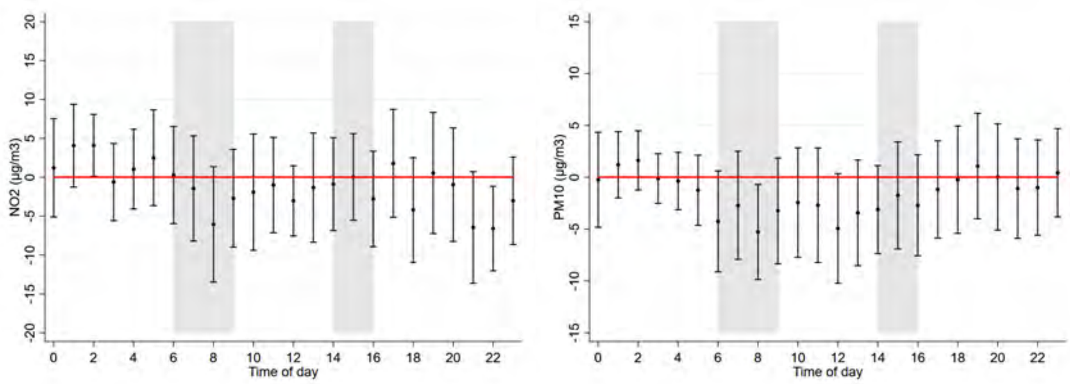
Notes: Table shows results from 16 separate regressions. Dependent variable is ambient air pollution measured as mean levels of NO_2 or PM_{10} (μg/m3) during a 60 minute interval. Estimated effects reflect daily average effects. Sample is restricted to 2 years pre and post policy implementation and restricted to rush hours only. Except for the weather controls, the model specification is the same as in the main regression table (Table 3). Standard errors are clustered on week. Temp refers to a polynomial of air temperature of degree 3; Rain refers to a polynominal precipitation of degree 2; Temp*Rain refers to an interaction of temperature and precipitation; Wind speed refers to a polynomial of wind speed of degree 2; Wind direction refers to four dummies for wind direction (north, south, east and west); Wind direction×speed refers to an interaction between wind direction and wind speed; Inversion refers to a dummy variable for inversion episodes. We estimate two sets of each of these weather control variables; one for weekdays and one for weekends.The coefficients in column (8) correspond to the main estimates in column (1) in Table 3.

C.2.4 Placebo intervention: 1 year pre/post Feb 1st, 2015

Figure C.6 and Table C.7 presents estimated effects of placebo treatments on air pollution, assuming that the congestion charge took place February 1st 2015, i.e. one year earlier. These placebo treatments are estimated on a dataset that includes 365 days pre/post the placebo treatment date. For each of the panels in Figure C.6, estimated effect of the placebo treatment is insignificantly different from zero for 23 of the 24 estimated coefficients, increasing our confidence that our main results are not driven by changes over time in unobservables affecting air pollution during weekends and weekdays differently.

Figure C.6: DID estimates on air pollution by 60 min. intervals. 1 year pre/post

(a) NO_2: Placebo: Feb 1, 2015 **(b)** PM_{10}: Placebo: Feb 1, 2015



Notes: Figure plots the coefficient β estimated from equation ??, but where β is allowed to vary by the hour of the day. Sample is restricted to 2 years before and after policy implementation (Feb 1 2016). Gray shaded areas indicate rush hours. Note that congestion charging is not active during weekends. Pollution is measured as micrograms per cubic meter of air(μg/m3). Standard errors are clustered on week.

Table C.7: DID estimates on NO_2 and PM_{10}. Placebo intervention

	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/m3)	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: NO_2						
Post × weekday	0.274	-0.877	-0.245	-0.0626	-3.196	2.330
	(1.656)	(1.789)	(1.832)	(1.920)	(2.456)	(1.967)
Observations	10683	7995	5301	3102	2693	2689
Mean depvar (pre-weekday)	49.18	57.54	64.03	67.06	43.56	25.70
Change (%)	0.56	-1.52	-0.38	-0.09	-7.34	9.07
Panel B: PM_{10}						
Post × weekday	-1.097	-1.825	-2.596	-2.703	-0.121	0.362
	(1.726)	(1.926)	(1.968)	(1.965)	(2.187)	(1.308)
Observations	10910	8186	5449	3188	2738	2723
Mean depvar (pre-weekday)	20.52	22.93	24.38	23.87	20.32	13.00
Change (%)	-5.35	-7.96	-10.65	-11.32	-0.60	2.78
Weather controls ($X'_{it}\gamma$)	✓	✓	✓	✓	✓	✓
Month×year FE (λ_{ym})	✓	✓	✓	✓	✓	✓
Day of week×time-of-day FE (θ_{di})	✓	✓	✓	✓	✓	✓

Notes: Table shows results from 12 separate regressions. Dependent variable is ambient air pollution measured as mean levels of NO_2 or PM_{10} (μg/m3) during a 60 minute interval. Post × Weekday refers to the coefficient X estimated from equation X. 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 365 days pre and post policy implementation. Standard errors are clustered on week.

C.3 Alternative DiD strategy: Bergen vs. other cities

In this section, we estimate effects of the congestion charge on air pollution by exploiting differences across cities, pre and post policy implementation. When $Bergen_i$ is a dummy variable equal to 1 for the monitoring station located in Bergen and $post_t$ is a dummy variable equal to 1 after February 1st 2016, the DiD estimator can be written as:

$$y_{ikst} = \beta post_t \times Bergen + X'_{ist}\gamma + \sigma_s + \lambda_{ywd} + \theta_{dis} + \varepsilon_{ikt}, \tag{6}$$

where y_{ikst} denotes hourly (i) concentrations of pollutant $k \in \{\text{NO2, PM10}\}$ measured at station s on date t; X'_{it} is a vector of station-specific weather controls; σ_s are monitoring station fixed effects; λ_{ywd} denotes year×week number×day-of-week fixed effects; θ_{dis} denotes station-specific day-of-week×time-of-day fixed effects; and ε_{ikt} is the idiosyncratic error term. The DiD estimate is captured by the coefficient β.

We use an estimating sample consisting of 26 pollution monitoring stations located near roads in different areas around Norway. We restrict the sample to weekdays only (Monday-Friday) and drop holidays and summer months. As a similar policy was introduced in Oslo in November 2017, we restrict our sample to 1 year pre and post Feb 1 2016. This means that our sample covers the period Feb 1 2015 to January 31 2017. The vector of controls are similar to the main specification in Section 3.4, with the exception that we allow effects of weather controls to vary by station. We also need to drop the inversion control as we only have data for Bergen from our data source. Standard errors are clustered on week number and station ID.

Table C.8 shows results from the DiD estimation. From panel (a) column (1) we see that the congestion charge led to a 4.2 μg/m3 decrease in the concentration of NO_2, corresponding to a 9% decline. This estimate is around 1.1 μg/m3, or 2.5 percentage points, higher that the main result presented in Table 3. While the decrease in μg/m3 is highest during rush hours, the percentage reductions are relatively similar for the 6 time period throughout the day. For PM_{10}, we find no significant effect of the Bergen congestion charge; see panel (b) in Table C.8.

Table C.8: DID estimates on NO_2 and PM_{10}. Bergen vs. other cities. Level

	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						
Post $\times$ Bergen	-3.990**	-4.351**	-4.992**	-5.327**	-3.364*	-2.162
	(1.502)	(1.591)	(1.786)	(2.000)	(1.737)	(1.479)
Observations	177545	132772	87891	51509	44888	44766
Mean depvar (pre-weekday)	47.40	55.60	61.41	64.49	42.65	24.64
Change (%)	-8.42	-7.83	-8.13	-8.26	-7.89	-8.77
Panel B: PM_{10}						
Post $\times$ Bergen	-0.420	-0.260	-0.193	1.162	0.0710	-0.346
	(2.066)	(2.609)	(2.710)	(2.653)	(2.885)	(0.769)
Observations	179934	135221	89996	52561	45275	44663
Mean depvar (pre-weekday)	18.29	20.46	21.85	21.58	17.91	11.57
Change (%)	-2.29	-1.27	-0.88	5.38	0.40	-2.99
Weather controls ($X_{it}'\gamma$)	✓	✓	✓	✓	✓	✓
Station FE (σ_s)	✓	✓	✓	✓	✓	✓
Post	✓	✓	✓	✓	✓	✓
Day of week×week×year FE (λ_{ywd})	✓	✓	✓	✓	✓	✓
Station×day of week×time-of-day FE (θ_{dis})	✓	✓	✓	✓	✓	✓

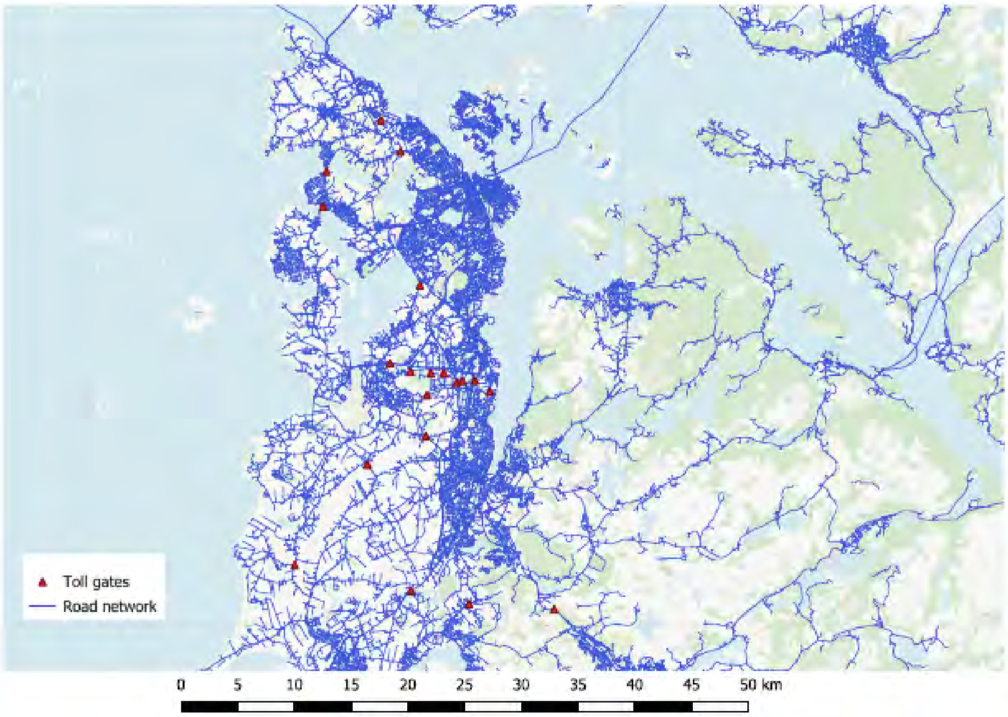
Notes: Table shows results from 12 separate regressions. Dependent variable is ambient air pollution measured as mean levels of NO_2 or PM_{10} (μg/m3) during a 60 minute interval. Post $\times$ Bergen refers to the coefficient β estimated from equation 6. Column headings indicate the sample used in 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 365 days pre and post policy implementation. Standard errors are clustered on week.

Appendix D Car ownership

D.1 Data and descriptives

D.1.1 The Stavanger toll cordon

Figure D.1: Map of toll gates around Stavanger



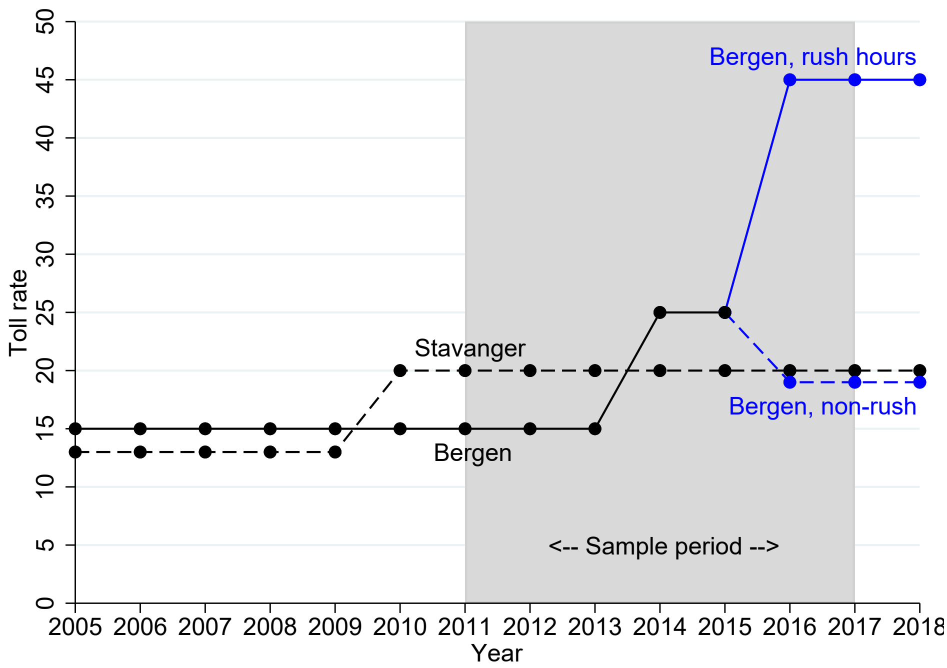
Notes: This map displays the road network in and around Stavanger, as well as the toll gates that were part of the Stavanger cordon toll in 2014. Note that several routes into the city will pass multiple toll gates. However, just as in Bergen cars will only pay once as long as they pass the toll gates within the same hour.

D.1.2 Toll rates in Bergen and Stavanger 2005-2017

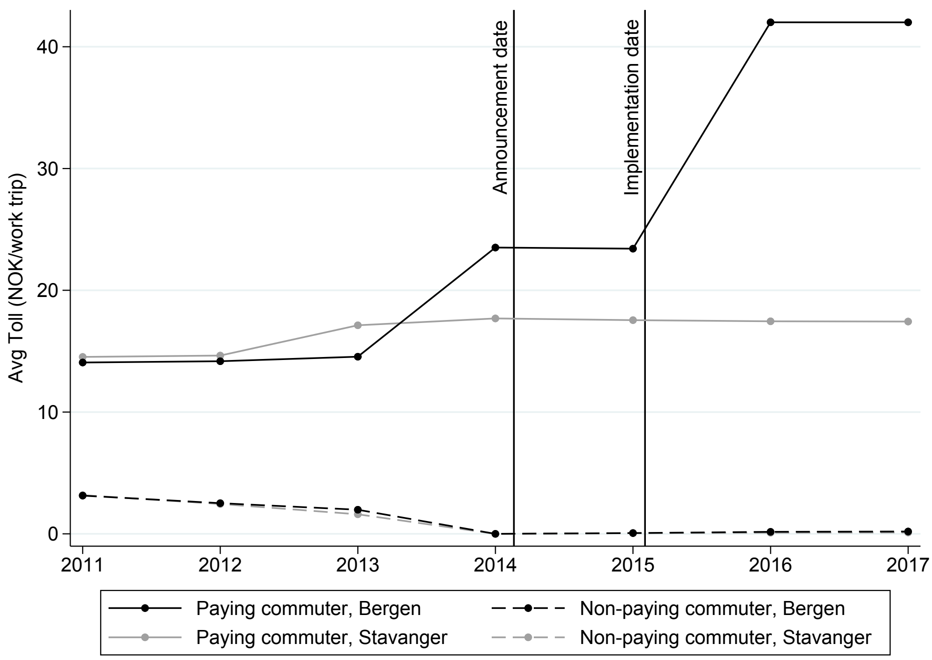
Figure D.2 shows the toll rates in Bergen and Stavanger over the period 2005 to 2017. The first panel displays the rates, while the second panel is the average toll exposure within the household for paying and non-paying commuters. The toll exposure is slightly lower than the rates because in some households not all adult members are exposed to the cordon toll on their way to work.

Figure D.2: Toll rates in Bergen and Stavanger

(a) Toll rates in Bergen and Stavanger



(b) Average toll exposure by treatment and control groups



D.1.3 Definition of treatment and control group

Paying commuters are defined as households where at least one household member passed the toll cordon to Bergen or Stavanger (but no other toll gates) on his/her way to work in 2014. *Non-paying commuters* are defined as households where *all* household members had zero toll payments on their work routes in 2014. The allocation of households to *paying commuters* and *non-paying commuters* is done based on toll payments in 2014 - the year before the congestion charge in Bergen was announced.

D.1.4 Sample selection

Our dataset consists of 441,451 households in either Hordaland or Rogaland county (the counties of Bergen and Stavanger) in 2014, resulting in 3,047,853 annual observations over the years 2011-2017. Based on our empirical strategy, we restrict our sample in the following way:

1. Household must have existed in 2014.
2. At least one household member must be employed.
3. At least one household member must have a workplace where the geographical location is observed (i.e. non-missing work route).
4. Households must be located within 50 kilometers of the city of Bergen or Stavanger.
5. Households must have an average work distance between 5 and 50 kilometers.
6. Households cannot have moved between 2014 and 2017.
7. Households must fall within the definitions of "paying commuter" or "non-paying commuter" as outlined in Section D.1.3.

As treatment is defined as a time-invariant attribute on the household level, the household must have existed in 2014 to be part of the analysis. We consider work distances below 5 km as walking and cycling distance and hence less likely to be affected by toll rates. The 50 km cutoff is done to ensure comparable work distances for paying and non-paying commuters. Restriction #6 is done to ensure that all households are assigned to the treatment or control group in a consistent manner and have not self-selected out of the treatment group. We do not require households to be observed during all years 2011-2017 to be included in our sample, meaning that the dataset is an unbalanced panel.[60]

[60]Restricting the sample to households observed in all years 2011-2017 reduces the number of observations significantly. However, results based on a balanced sample are very similar to our main results. Results can be provided upon request.

Applying the sample restrictions listed above leaves us with a dataset consisting of 76,088 households over 7 years, resulting in a total of 448,196 annual observations. Table D.1 shows how each sample selection criterion affects number of observations, and Table D.2 show the effect on selected variable means in 2014.

Table D.1: Observations by year and sample selection criteria

Year	(1)	(2)	(3)	(4)	(5)	(6)	(7)
2011	406363	370232	254648	197985	133008	112947	53043
2012	414484	377820	262262	204554	137977	115072	57905
2013	422965	385675	269801	210634	142258	115898	64130
2014	441451	402609	276970	215960	146686	116540	76088
2015	447332	406767	273130	212631	145500	114320	69419
2016	451399	408620	268936	209241	143962	112629	64627
2017	463859	419919	269322	209369	144545	113296	62984
Total	3047853	2771642	1875069	1460374	993936	800702	448196
Share of population	1.00	0.91	0.62	0.48	0.33	0.26	0.15

Notes: This table shows how observations per year are reduced as various sample selection criteria are imposed. (1) All households in the counties Hordaland and Rogaland; (2) Removing households where no individuals work; (3) Removing households where no individuals are matched with workplace locations; (4) Removing households located more than 50 kilometers away from Bergen/Stavanger; (5) Removing households where work distance is not between 5 and 50 kilometers; (6) Removing households that moved between 2014 and 2017; (7) Removing households not covered by treatment and control definitions. Column (7) is our final sample.

Table D.2: Summary statistics by sample selection criteria, 2014

Sample mean of variables	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: Outcomes							
Electric vehicle (0/1)	0.020	0.021	0.029	0.032	0.037	0.040	0.031
Number of electric vehicles	0.021	0.022	0.029	0.033	0.038	0.041	0.032
Number of ICE vehicles	1.038	1.031	1.260	1.207	1.298	1.374	1.392
Total number of vehicles	1.058	1.053	1.290	1.240	1.336	1.415	1.425
Panel B: Journey to work variables							
Toll rate (NOK/individual)	20.79	20.81	20.81	21.78	17.18	16.84	9.30
Toll rate (NOK/household)	28.39	28.46	28.46	29.92	24.02	24.07	13.88
Driving distance (km)	34.55	34.56	34.56	33.96	14.31	14.33	12.79
Driving time (min)	32.53	32.55	32.55	31.65	14.73	14.77	13.53
PT time minus driving time (min)	73.74	73.87	72.85	64.69	75.18	77.85	77.22
PT time divided by driving time	7.62	7.63	7.64	6.74	6.56	6.81	7.12
Panel C: Socio-economic variables							
Couple (0/1)	0.53	0.49	0.65	0.64	0.68	0.72	0.71
Children living at home (0/1)	0.28	0.30	0.39	0.39	0.41	0.44	0.43
Persons in household	2.16	2.15	2.53	2.49	2.59	2.71	2.70
Age	48.74	46.83	43.04	42.69	42.91	44.63	44.47
Female (0/1)	0.50	0.50	0.48	0.48	0.48	0.49	0.49
Owns second home	0.10	0.10	0.11	0.11	0.11	0.11	0.11
Employed (0/1)	0.65	0.71	0.91	0.91	0.92	0.92	0.92
Retired (0/1)	0.21	0.16	0.05	0.05	0.05	0.06	0.06
Income (100,000 NOK/individual)	3.52	3.62	4.08	4.16	4.21	4.32	4.32
Income (100,000 NOK/household)	5.58	5.65	6.81	6.92	7.11	7.45	7.44
Wealth (mill NOK/individual)	1.42	1.37	1.33	1.43	1.43	1.66	1.62
Wealth (mill NOK/household)	2.23	2.07	2.25	2.42	2.45	2.84	2.82
Education:							
Unknown (0/1)	0.28	0.30	0.20	0.21	0.18	0.16	0.16
Less than high school (0/1)	0.16	0.13	0.10	0.10	0.10	0.10	0.11
High school (0/1)	0.25	0.25	0.29	0.27	0.29	0.29	0.30
College (0/1)	0.21	0.21	0.27	0.27	0.28	0.29	0.28
University (0/1)	0.10	0.11	0.14	0.15	0.15	0.16	0.15
Observations	441451	402609	276970	215960	146686	116540	76088

Notes: (1): All households in the counties Hordaland and Rogaland; (2): Removing households where no individuals work; (3): Removing households where no individuals are matched with workplace locations; (4) Removing households located more than 50 kilometers away from Bergen/Stavanger; (5): Removing households where work distance is not between 5 and 50 kilometers; (6) Removing households that moved between 2014 and 2017; (7) Removing households not covered by treatment and control definitions. Column (7) is our final sample.

D.1.5 Model specification and description of variables

Our dataset contains information at both the individual and household level. Individual-level data is restricted to persons above the age of 18. In the analysis, we focus on households as the unit of observation. This means that individual-level characteristics are aggregated to the household level as described in this section.

In the main analysis, 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,

Table D.3: 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 Stavanger
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 college
educ4_{it}	Dummy: highest education in household is university
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

less than high school, high school, college and university). A continuous variable for 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). Descriptions of the variables are included in Table D.3.

D.1.6 Summary statistics

Table D.4: Summary statistics 2017

	Bergen				Stavanger			
	Paying		Non-paying		Paying		Non-paying	
	mean	sd	mean	sd	mean	sd	mean	sd
Panel A: Outcomes								
Electric vehicle (0/1)	0.178	0.382	0.117	0.321	0.121	0.326	0.074	0.263
Number of electric vehicles	0.187	0.415	0.124	0.351	0.127	0.352	0.078	0.280
Number of ICE vehicles	1.156	0.788	1.486	0.854	1.489	0.809	1.421	0.815
Total number of vehicles	1.343	0.831	1.610	0.858	1.616	0.815	1.499	0.824
Panel B: Journey to work variables								
Toll rate (NOK/individual)	42.01	14.62	0.19	2.73	17.43	6.62	0.11	1.70
Toll rate (NOK/household)	64.32	32.57	0.37	5.22	27.02	13.59	0.21	3.24
Driving distance (km)	12.42	7.98	14.17	8.76	13.57	6.67	10.53	5.91
Driving time (min)	13.28	8.29	15.03	9.58	13.77	6.81	11.77	7.40
PT time minus driving time (min)	60.00	51.03	94.82	82.68	79.85	57.24	80.16	79.50
PT time divided by driving time	5.62	2.76	7.85	5.59	7.27	4.77	8.51	8.53
Panel C: Socio-economic variables								
Couple (0/1)	0.77	0.42	0.76	0.43	0.82	0.39	0.74	0.44
Children living at home (0/1)	0.42	0.49	0.42	0.49	0.48	0.50	0.42	0.49
Persons in household	2.75	1.33	2.77	1.37	2.96	1.35	2.77	1.40
Age	48.45	11.15	48.38	11.57	47.48	10.78	48.15	11.47
Female (0/1)	0.49	0.25	0.48	0.26	0.48	0.22	0.48	0.26
Owns second home	0.13	0.33	0.12	0.32	0.13	0.33	0.13	0.33
Employed (0/1)	0.93	0.18	0.89	0.20	0.92	0.18	0.90	0.20
Retired (0/1)	0.08	0.23	0.08	0.24	0.06	0.21	0.08	0.24
Income (100,000 NOK/individual)	4.50	3.43	4.12	2.20	4.81	2.78	4.62	4.62
Income (100,000 NOK/household)	7.95	5.11	7.22	4.41	8.73	5.46	8.06	8.52
Wealth (mill NOK/individual)	2.13	6.02	1.67	5.02	1.94	3.46	2.00	5.55
Wealth (mill NOK/household)	3.76	11.37	2.91	9.50	3.49	6.29	3.48	10.71
Education:								
Unknown (0/1)	0.14	0.34	0.13	0.34	0.10	0.30	0.14	0.35
Less than high school (0/1)	0.06	0.24	0.11	0.31	0.09	0.28	0.11	0.31
High school (0/1)	0.23	0.42	0.37	0.48	0.31	0.46	0.30	0.46
College (0/1)	0.32	0.47	0.29	0.45	0.31	0.46	0.28	0.45
University (0/1)	0.25	0.43	0.10	0.29	0.19	0.39	0.16	0.37
Observations	10005		17906		20105		14968	

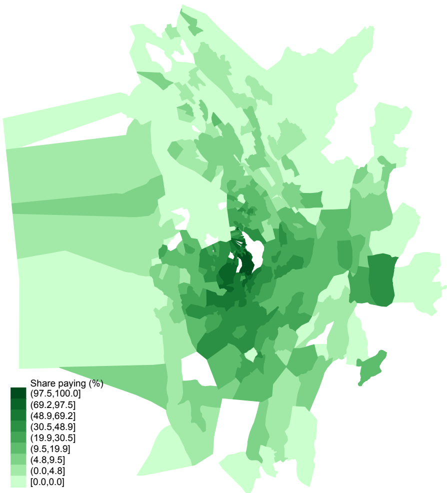
Notes: (1): Paying commuters, Bergen; (2): Non-paying commuters, Bergen; (3): Paying commuters, Stavanger; (4): Non-paying commuters, Stavanger.

D.1.7 Paying and non-paying commuters by area

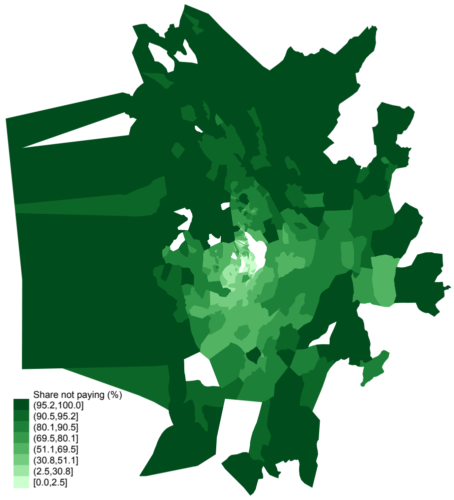
Maps display neighborhoods located within 50 kilometers of Bergen and Stavanger by the share of the final sample that are paying/non-paying commuters. The share of paying commuters is increasing by proximity to the toll cordon. Note that the areas of some neighborhoods to the west mainly consist of open sea. Neighborhoods with less than 50 households (mainly parks, woodland, mountains and bodies of water) are removed for confidentiality reasons. Note that several neighborhoods in or close to the city centers are too small to be visible.

Figure D.3: Share of population that is treated/non-treated

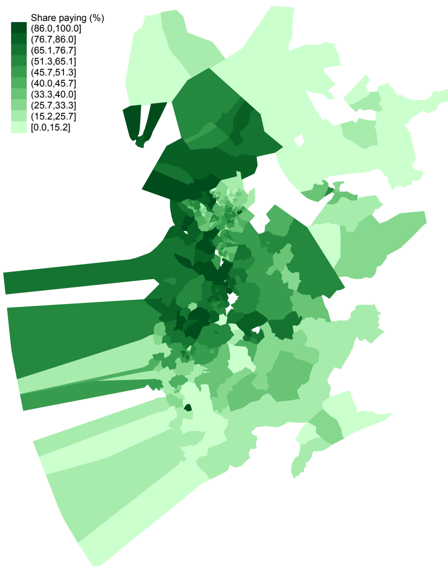
(a) Bergen, paying commuters



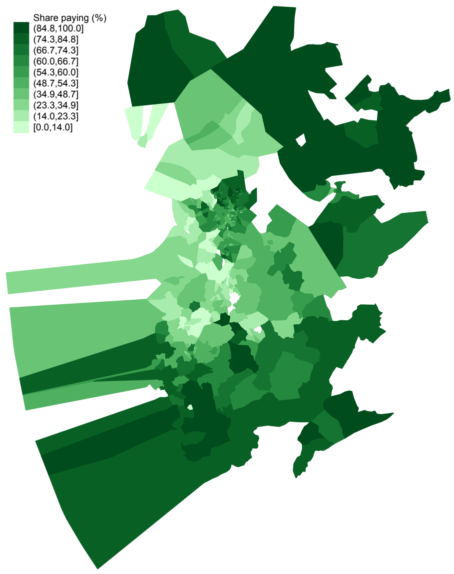
(b) Bergen, non-paying commuters



(c) Stavanger, paying commuters



(d) Stavanger, non-paying commuters



Notes: The share of the final sample that is classified as “paying” and “non-paying” commuters in 2014, by neighborhood.

D.2 Supporting results and robustness checks

D.2.1 DiD and DiDiD estimates: supporting figures

Here, we show estimated treatment effects from two separate DiD regressions for Bergen and Stavanger, where the two differences are "over time" and "between paying and non-paying commuters". These regressions take the following form:

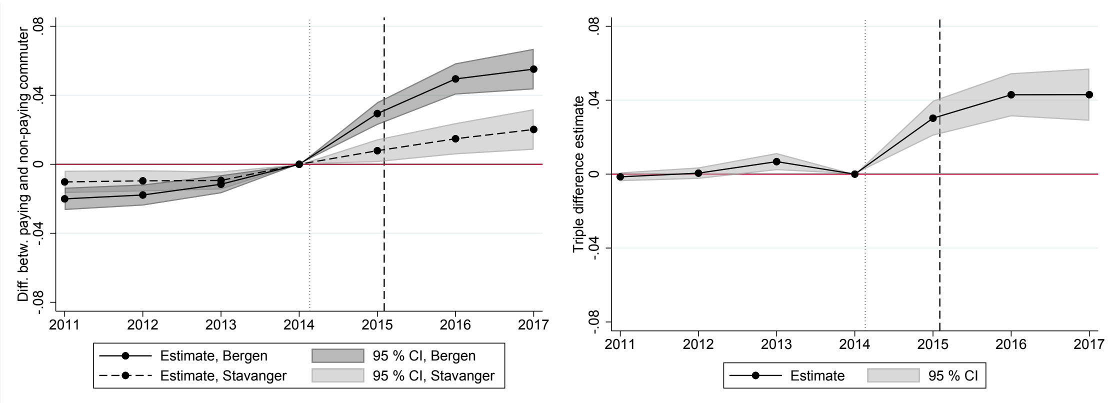
$$y_{it} = \sum_{s \in \{T | s \neq 2014\}} \alpha_t c_i \times \mathbb{1}\{t = s\} + \eta c_i + X'_{it}\gamma + \theta_{nt} + \varepsilon_{it}. \tag{7}$$

η will absorb the effect of being a paying commuter in 2014. If we let α_t^1 (α_t^0) denote the estimated "paying commuter" effect for Bergen (Stavanger) in year t, the triple difference estimate in a given year can be derived from $\alpha_t^1 - \alpha_t^0$.

Figure D.4, panel (a) shows the DiD estimates for Bergen and Stavanger. The estimated coefficients show that paying computers in both cities experiences an increase in the electric vehicle ownership share relative to non-paying computers. By subtracting the estimated effects for Stavanger from the true treatment effects for Bergen, we arrive at our DiDiD estimates presented in panel (b). Figure shows corresponding treatment effects for three additional outcome variables.

Figure D.4: Double and triple differences: Probability of owning an electric vehicle.

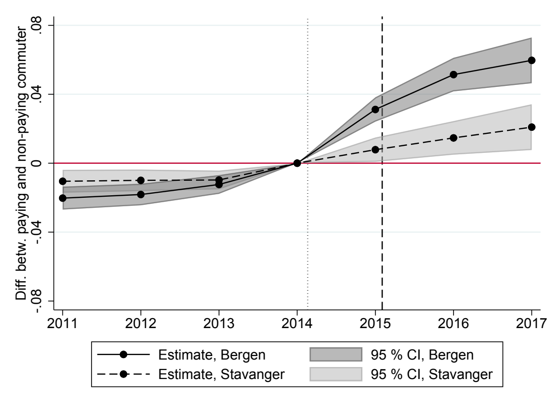
(a) Pr(BEV): DiD **(b)** Pr(BEV): DiDiD



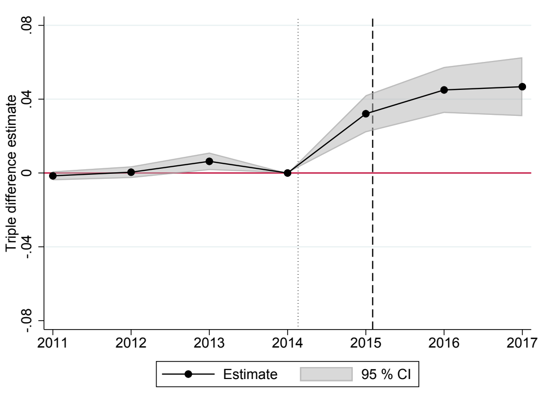
Notes: Figures on the left side plot coefficients α_t estimated from equation 7, where α_{2014} is normalized to zero. Figures on the right side plots the coefficients β_t estimated from equation 3, where α_{2014} and β_{2014} are normalized to zero. The outcome is a dummy variable indicating battery electric vehicle (BEVs) ownership per household. 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 D.5: Double and triple differences: Number of vehicles owned by vehicle type.

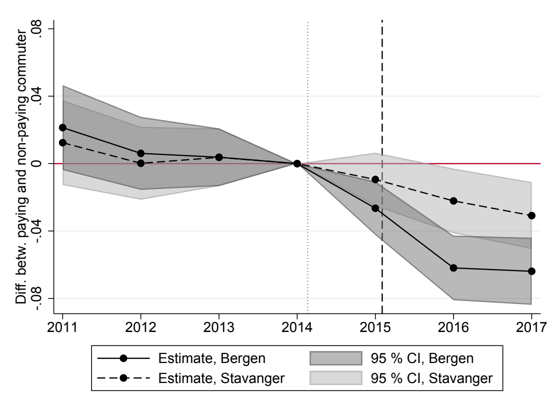
(a) BEV: DiD



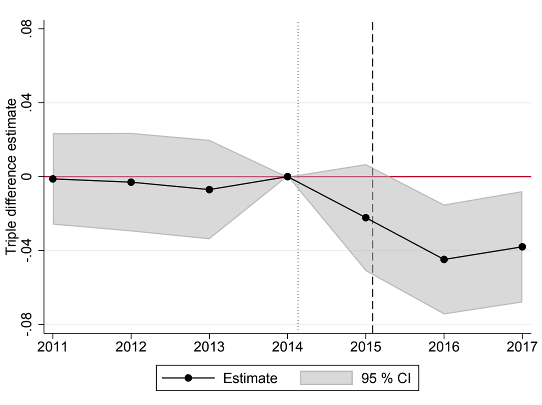
(b) BEV: DiDiD



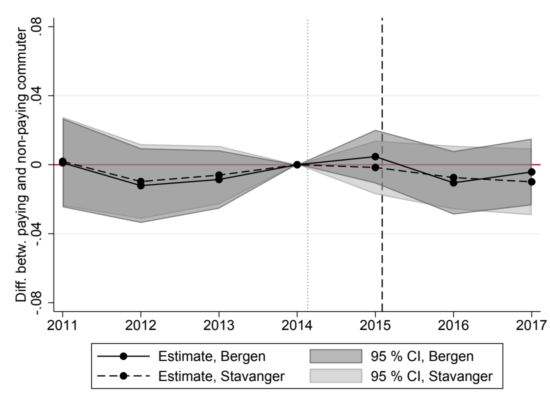
(c) ICE: DiD



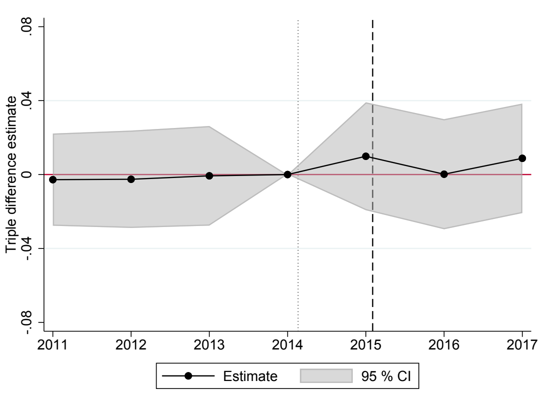
(d) ICE: DiDiD



(e) All cars: DiD



(f) All cars: DiDiD



Notes: Figures on the left side plot coefficients α_t estimated from equation 7, where α_{2014} is normalized to zero. Figures on the right side plots the coefficients β_t estimated from equation 3, where α_{2014} and β_{2014} are normalized to zero. The outcome is number of battery electric vehicles (BEVs) owned per household, number of internal combustion engine vehicles (ICEVs) owned per household and total number of vehicles owned per household for the first, second and third row, respectively. 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.

D.2.2 Robustness

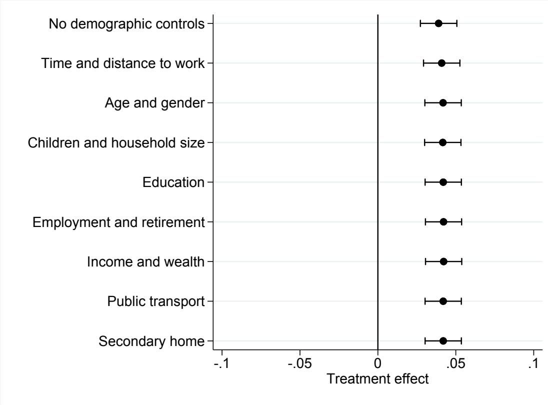
Table D.5: DiDiD estimates with different fixed effects

	(1)	(2)	(3)	(4)
Panel A: Pr(BEV)				
Post × Paying commuters × Bergen	0.0130**	0.0134**	0.0393***	0.0419***
	(0.00585)	(0.00583)	(0.00589)	(0.00593)
Observations	376998	376997	376914	376914
Mean depvar 2014 (paying commuters, Bergen)	0.0469	0.0469	0.0469	0.0469
Mean depvar 2017 (paying commuters, Bergen)	0.1775	0.1775	0.1774	0.1774
Panel B: Number of BEVs				
Post × Paying commuters × Bergen	0.0136**	0.0140**	0.0424***	0.0451***
	(0.00621)	(0.00619)	(0.00648)	(0.00652)
Observations	376998	376997	376914	376914
Mean depvar 2014 (paying commuters, Bergen)	0.0482	0.0482	0.0482	0.0482
Mean depvar 2017 (paying commuters, Bergen)	0.1873	0.1873	0.1872	0.1872
Panel C: Number of ICEVs				
Post × Paying commuters × Bergen	-0.0429***	-0.0392***	-0.0360***	-0.0422***
	(0.0102)	(0.0102)	(0.0121)	(0.0119)
Observations	376998	376997	376914	376914
Mean depvar 2014 (paying commuters, Bergen)	1.1731	1.1731	1.1730	1.1730
Mean depvar 2017 (paying commuters, Bergen)	1.1554	1.1554	1.1555	1.1555
Panel D: Number of cars in total				
Post × Paying commuters × Bergen	-0.0293***	-0.0252***	0.00633	0.00288
	(0.00956)	(0.00944)	(0.0117)	(0.0116)
Observations	376998	376997	376914	376914
Mean depvar 2014 (paying commuters, Bergen)	1.2214	1.2214	1.2212	1.2212
Mean depvar 2017 (paying commuters, Bergen)	1.3427	1.3427	1.3427	1.3427
Year FE	✓	✓		
Paying commuter	✓	✓	✓	
Paying commuter × Post	✓	✓	✓	
Paying commuter × year FE ($\alpha_t c_i$)				✓
Paying commuter × Bergen ($\eta c_i \times B_i$)	✓	✓	✓	✓
Bergen	✓			
Bergen × Post	✓	✓		
Neighborhood FE		✓		
Neighborhood × year FE (θ_{nt})			✓	✓
Household characteristics ($X'_{it}\gamma$)	✓	✓	✓	✓

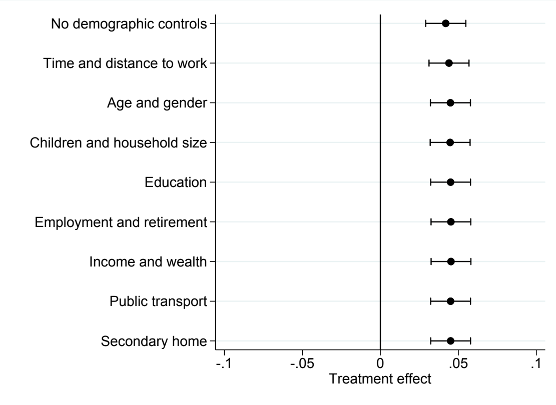
* p<0.10, ** p<0.05, *** p<0.01. Standard errors are clustered on neighborhoods.
Notes: BEV refers to battery electric vehicles, while ICEV refers to internal combustion engine vehicles. Regression (1) estimates the triple difference with dummies for "post", "Bergen", "paying commuters" and year fixed effects. Regression (2) adds neighborhood fixed effects, alleviating the need for a "Bergen" dummy. Regression (3) interacts neighborhood and year fixed effects, alleviating the need for "Bergen×post". Regression (4) interacts "paying commuters" with year fixed effects, alleviating the need for "Paying commuter×post". This is the same regression as in Equation 2 and Table 5.

Figure D.6: DiDiD: Effect of demographics.

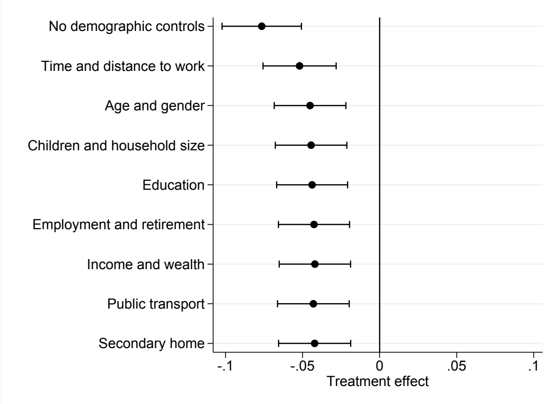
(a) Pr(BEV)



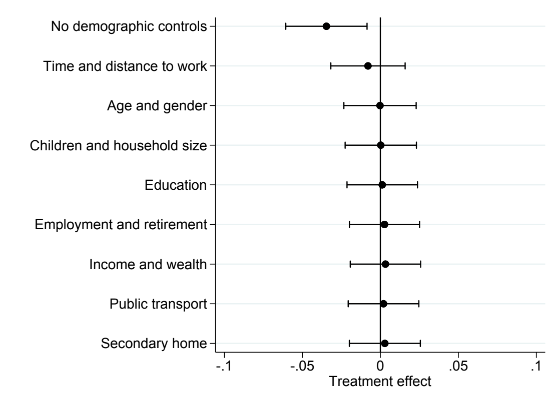
(b) Number of BEVs



(c) Number of ICEVs



(d) Number of cars

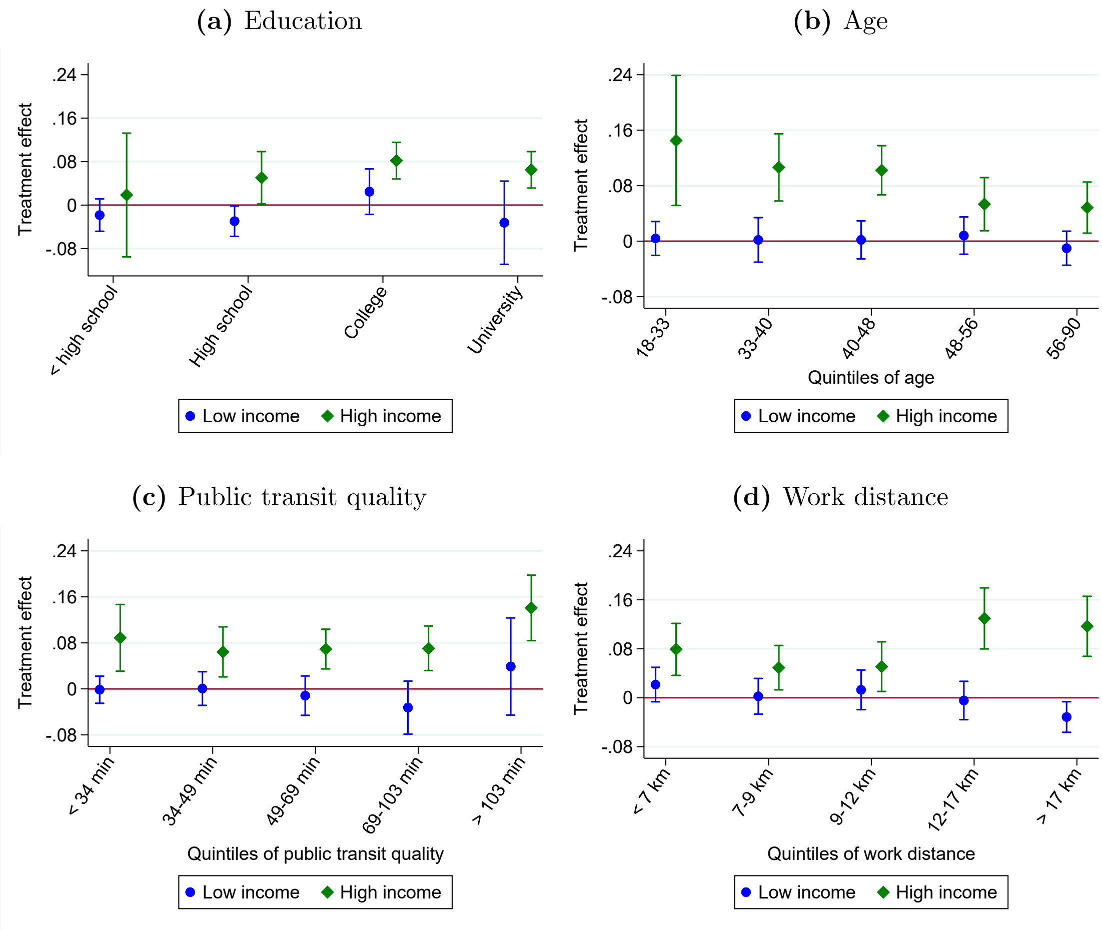


Notes: BEV refers to battery electric vehicles, while ICEV refers to internal combustion engine vehicles. Whisker plots are triple difference estimates from Equation 2 clustered at the neighborhood level, with an increasing number of demographic controls. The top estimate of each panel is unconditional on demographics, while the bottom estimate is the same as in Table 5.

The following variables are added in sequence: (1) single/couple specific first and second order polynomials of time and distance to work by car; (2) single/couple specific controls for average age and share of females in the household; (3) number of household members and single/couple specific dummies for kids less than 18 years old living at home; (4) single/couple specific dummies for education level (the highest education level of spouses); (5) single/couple specific controls for the share of household members that are employed and retired; (6) single/couple specific first and second order polynomials of average net income and average wealth; (7) single/couple specific first and second order polynomials for public transit accessibility to work (time by public transit minus time by car, and time by public transit divided by time by car); and (8) a dummy variable for whether any household members own a secondary home.

D.3 Heterogeneous effects

Figure D.7: Heterogeneous DiDiD estimates on Pr(BEV), by top and bottom income quintiles



Notes: Figure plots the coefficients β_k estimated from equation 4, where groups are defined as interactions between the top or bottom income quintile (the three middle income quintiles are excluded), 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.

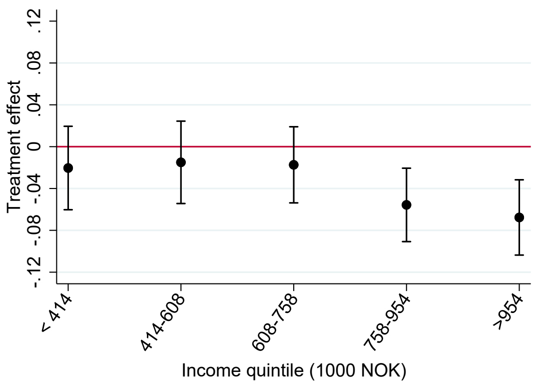
Table D.6: Heterogeneous DiDiD estimates on Pr(BEV)

Dependent variable: Pr(BEV)	Estimate for group number:				
Measured in percentage points	(1)	(2)	(3)	(4)	(5)
Panel A: Income†					
Post × Paying commuters × Bergen	-0.247	2.547***	4.282***	4.018***	7.027***
	(0.716)	(0.811)	(0.888)	(0.984)	(0.951)
Mean depvar 2014	0.97	2.31	4.13	6.16	10.46
Mean depvar 2017	4.00	10.11	16.21	22.05	31.49
Household income (1000 NOK)	350.40	521.31	672.00	817.13	1244.59
Households per group	61069	72408	79541	81564	82332
Panel B: Family status††					
Post × Paying commuters × Bergen	-0.354	0.195	1.676**	6.637***	
	(0.708)	(1.648)	(0.749)	(0.807)	
Mean depvar 2014	1.24	1.59	3.61	8.24	
Mean depvar 2017	4.21	8.39	14.02	27.79	
Households per group	79150	14203	125064	158497	
Panel C: Education‡					
Post × Paying commuters × Bergen	1.350	1.472	2.954***	4.924***	4.171***
	(0.929)	(1.019)	(0.807)	(0.826)	(0.983)
Mean depvar 2014	1.66	2.12	3.34	5.99	7.31
Mean depvar 2017	5.90	8.20	14.90	22.24	23.49
Households per group	51147	40605	117360	109901	57901
Panel D: Age†					
Post × Paying commuters × Bergen	3.937***	5.041***	5.933***	3.367***	1.099
	(0.954)	(1.022)	(0.964)	(0.866)	(0.816)
Mean depvar 2014	2.15	6.40	6.97	4.72	3.43
Mean depvar 2017	14.49	22.14	23.19	16.51	11.02
Average age	29.51	37.24	43.81	51.49	61.27
Households per group	61527	73636	80071	82488	79192
Panel E: Work distance†					
Post × Paying commuters × Bergen	2.417**	2.518**	3.261***	4.638***	5.719***
	(0.939)	(0.991)	(0.887)	(1.019)	(0.996)
Mean depvar 2014	2.93	4.43	5.06	4.95	6.76
Mean depvar 2017	12.95	15.99	18.86	20.20	22.73
Work distance (kilometers)	6.68	8.53	10.85	14.35	24.12
Households per group	72634	75333	75951	76261	76735
Panel F: Public transit†					
Post × Paying commuters × Bergen	2.458***	3.515***	4.404***	3.169***	8.404***
	(0.867)	(0.941)	(0.854)	(1.085)	(1.285)
Mean depvar 2014	2.00	4.80	6.06	6.57	7.90
Mean depvar 2017	9.33	16.92	20.17	23.18	28.20
Time public transport minus time car (minutes)	29.58	45.19	60.41	83.67	168.87
Households per group	67481	71827	77013	80418	79952

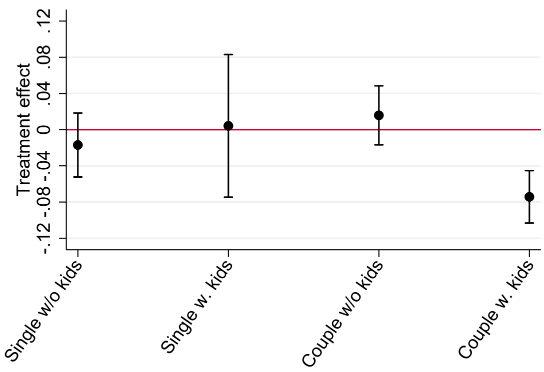
† Column number refers to quintiles of the population.
†† 1: Single without kids; 2: Single with kids; 3: Couple without kids; 4: Couple with kids.
‡ 1: Unknown; 2: Less than high school; 3: High school; 4: College; 5: University.
Notes: Table shows the coefficient β_k estimated from equation 4, where 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. Note that estimated effects are given in percentage points. 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 at the neighborhood level. * p<0.10, ** p<0.05, *** p<0.01.

Figure D.8: Heterogeneous DiDiD: ICEV ownership.

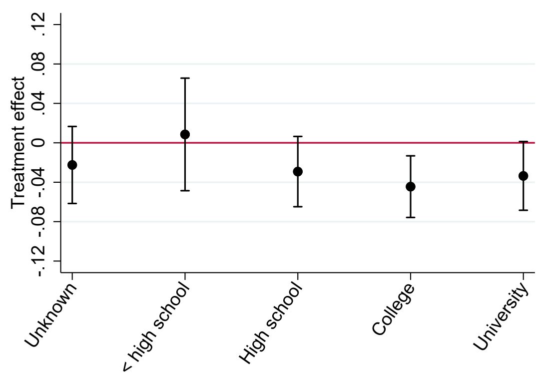
(a) Income



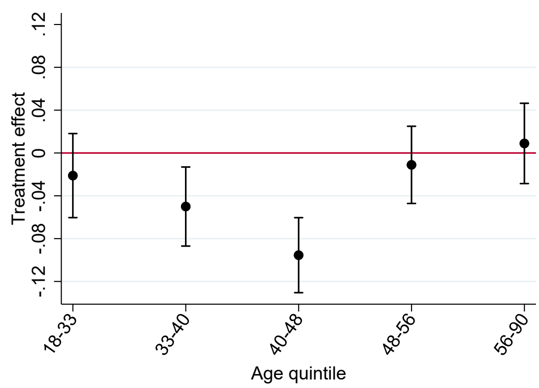
(b) Family status



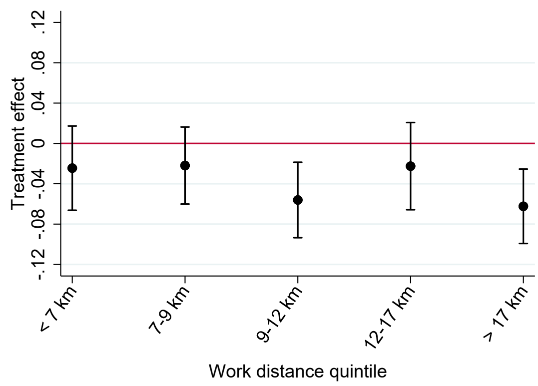
(c) Education



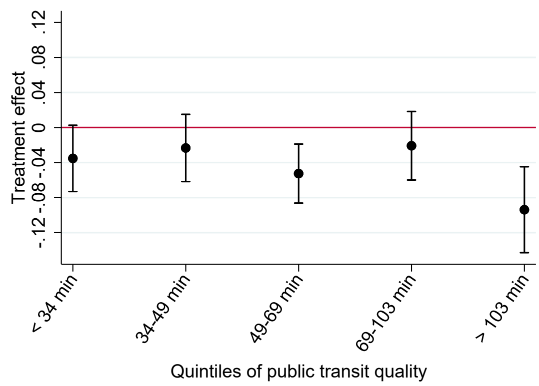
(d) Age



(e) Work distance



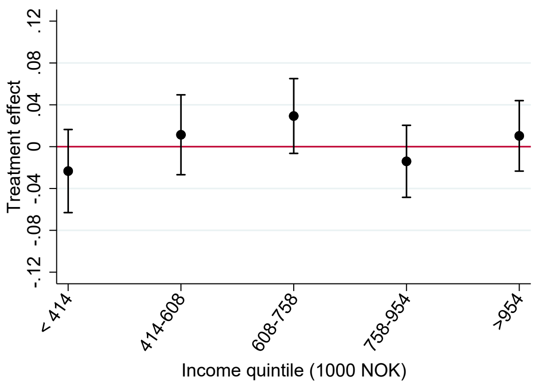
(f) Public transit quality



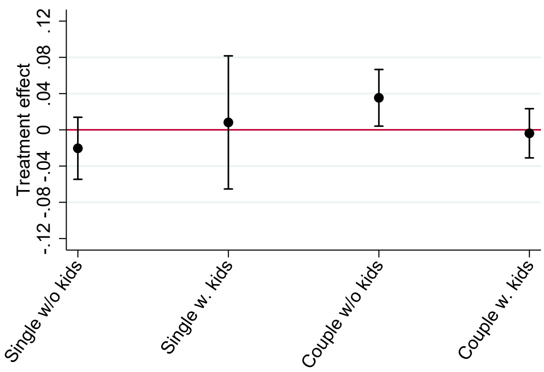
Notes: Figure plots the coefficients β_k estimated from equation 4, where k refers to group (e.g., income quintile). Each panel (a-f) plots coefficients estimated from a separate regression. Whiskers indicate 95% confidence intervals. The dependent variable is number of internal combustion engine vehicles 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 D.9: Heterogeneous DiDiD: Car ownership.

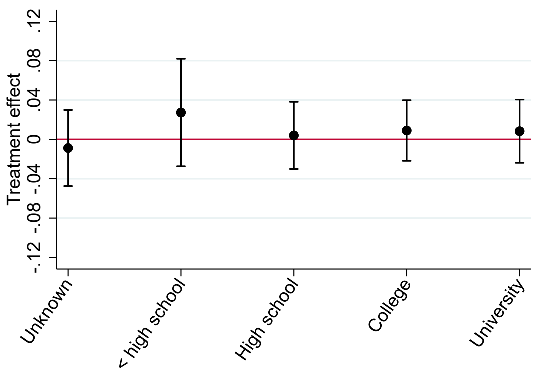
(a) Income



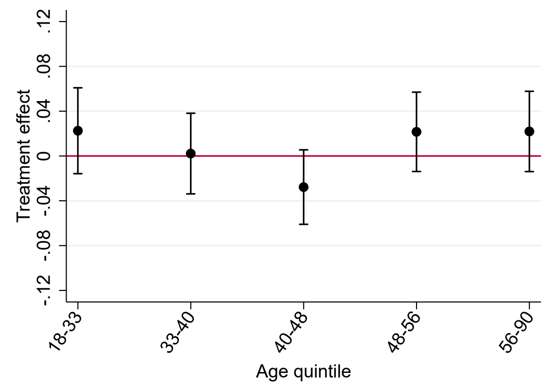
(b) Family status



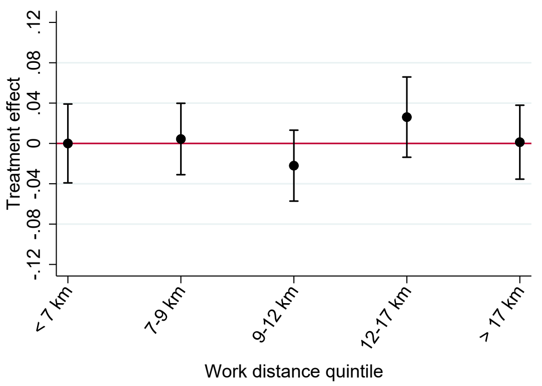
(c) Education



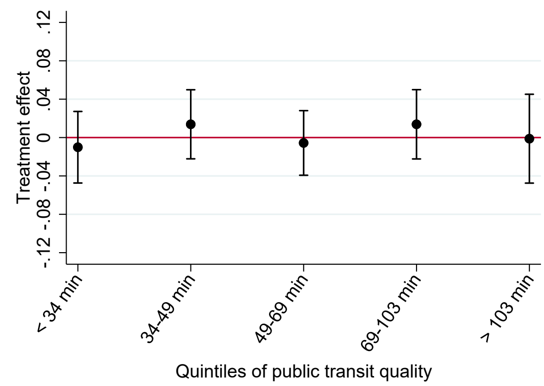
(d) Age



(e) Work distance



(f) Public transit quality



Notes: Figure plots the coefficients β_k estimated from equation 4, 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 E Welfare calculations

In this appendix, we provide more details on the welfare calculations presented in Section 5.1 in the paper.

E.1 Adaptation costs

Households that continue to drive their diesel or gasoline car through the toll cordon during rush hours after the policy was implemented will incur a higher private cost due to higher toll rates. As these payments are simply a transfer to the government, the impact on aggregate welfare is assumed to be zero. Households that adapt their behavior to avoid the congestion charge, however, will incur a private adaptation cost that is welfare-reducing. There are four ways in which this can occur: (1) not drive at all (e.g., change mode of transportation or work from home), (2) shift driving to non-rush hours (temporal adaptation), (3) drive around the toll cordon (spatial adaptation), or (4) buy an electric vehicle.

To quantify the total adaptation costs related to (1)-(3), we combine the estimated reduction in cars passing the toll cordon during rush hours with assumptions on the adaptation cost per trip. The traffic reduction during rush hours is estimated to be around 447 cars per 15-minute interval, or 8,046 cars per workday (see Table 2, Column 2). As an upper bound on adaptation costs per trip, we use the 20 NOK increase in toll rate during rush hours (from NOK 25 to NOK 45); if the cost of avoiding the congestion charge was higher than NOK 20, individuals would prefer to continue to drive through the toll cordon after the implementation of the congestion charge. As a lower bound on the adaptation costs per trip, we use NOK 0; if an individual was indifferent to passing the toll cordon during rush hours or not prior to the policy change, the adaptation costs for this individual will be zero. By assuming that demand is locally linear between these two extremes, we can apply the standard triangle formula to calculate the private deadweight loss, $\frac{1}{2}(p_1 - p_0)(q_0^{\text{cars}} - q_1^{\text{cars}})$, where $(p_1 - p_0)$ is the maximum adaption cost and $(q_0^{\text{cars}} - q_1^{\text{cars}})$ is the total number of cars substituting away from the congestion charge (see Figure E.1). Given 230 working days each year, we find an adaptation costs resulting from (1)-(3) of **NOK 18.51 million per year** (0.5×NOK 20×8,046 cars per day×230 working days).[61]

However, the welfare analysis is complicated by the presence of BEVs and the fact that they cannot be disentangled from other cars in the traffic data. Individuals

[61] Note that we simplify these calculations somewhat by focusing on the change in toll road during rush hours and disregarding the small decrease in toll levels outside rush hours.

Figure E.1: Illustration of adaptation costs

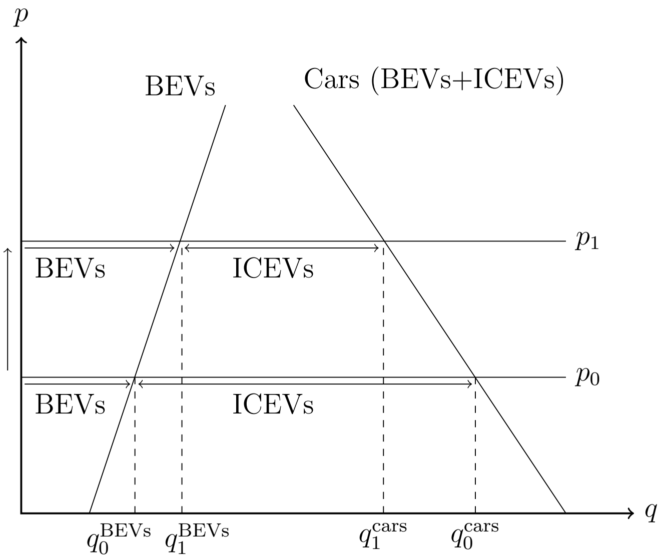


Figure illustrates cars (q) driving through the toll cordon during rush hours as a function of the toll intensity (p). The number of cars in total during rush hours, which is what we observe for the traffic regressions, is downward sloping in toll payments. The number of BEVs is upwards sloping. The number of ICEVs, which are required to pay tolls, is the difference between these two curves. The congestion charge implies increased tolls from p_0 to p_1 during rush hours, fewer cars in total but additional EVs and a change in total toll payments from $p_0(q_0^{\text{cars}} - q_0^{\text{BEVs}})$ to $p_1(q_1^{\text{cars}} - q_1^{\text{BEVs}})$.

substituting towards BEVs as a response to the policy will also incur an adaptation cost $\geq$ NOK 0 per day (otherwise they would have bought a BEV irrespective of the congestion charge) and $\leq$ NOK 20 per day (otherwise they would have continued to drive an ICEV). To quantify this adaptation cost (4), we again employ the triangle formula to calculate $\frac{1}{2}(p_1 - p_0)(q_1^{\text{BEVs}} - q_0^{\text{BEVs}})$ (see Figure E.1), but rely on estimates from the individual-level regressions. The regression coefficient in Table 5, Column 1 indicates that 4.2 percent of paying commuters avoided the congestion charge by purchasing an electric vehicle. We define the population of paying commuters to be the total number of households were at least one household member works and lives on opposite sides of the toll cordon, and the work distance is between 5 and 50 kilometers. This gives us a population of 38,482 households.[62] Based on our regression estimate, 1,616 households adapted to the policy by purchasing an electric vehicle (38,482 households$\times$0.042). Assuming 230 working days each year, the private adaptation cost amounts to **NOK 3.72 million per year** (0.5$\times$NOK 20$\times$230 working days$\times$1,616 households).

[62]Note that this number is higher than the sample size of "paying commuters" in the analysis due to the sample selection criteria imposed in the regressions. However, we still consider the 38,482 households to be a conservative estimate of the affected population, as the congestion charge also affects the remaining population of Bergen – although their increased likelihood of purchasing an electric car is probably smaller than 4.2 percent.

E.2 Time savings

A key benefit of congestion charging is time savings from reduced congestion. Unfortunately, our data lacks a relevant congestion measure such as driving speed or time spent in traffic, meaning that we are unable to obtain a causal estimate on time savings. As a second best approach, we apply descriptive estimates of time savings from other relevant studies. NPRA (2016) reports average time in traffic during rush hours for the main corridors through the Bergen toll cordon during weekdays in April 2015 and April 2016. We use these before/after measurements of driving time, which average out to 2.3 minutes across all corridors, as a proxy for average time savings. From our detailed driving data, we know that an average of 51,534 cars per day drive through the toll cordon during rush hours on weekdays post policy implementation. To convert this to monetary units, we use a recent Norwegian time value study (Flügel et al., 2020), where the time value for commuting car drivers is estimated to NOK 93 per hour by means of stated preference experiments. Note that this number is significantly lower than the average net wage in Norway, which is often used as an opportunity cost for time spent in traffic in other studies. We also disregard time savings for passengers. Our calculations will hence give us a conservative estimate of the time savings induced by the policy. Given 230 working days per year, the value of the time savings amounts to **NOK 42.26 million annually** (51,534 cars per day$\times$2.3 minutes per car$\div$60 minutes per hour$\times$NOK 93 per hour$\times$230 working days).

E.3 Local pollutants

To quantify social benefits of lower local pollution, we combine our estimates on changes in ambient levels of NO_2 and PM_{10} with assumptions about the affected population and estimates on social benefits per $\mu g/m^3$ in the existing literature. From Table 3, Column 1, we find that the congestion charge led to a reduction in daily average levels of NO_2 and PM_{10} of 3.064 and 1.185 $\mu g/m^3$, respectively.[63] As we only observe concentrations of local air pollutants at a single monitoring station (Danmarks plass - see Figure 1), we have limited ability to examine the spatial dispersion of pollutants. Given that our one monitoring station is located at the border of the inner city center of Bergen, we opt for a conservative approach and define the inhabitants of the inner city center as the affected population (29,287 individuals).[64]

[63] Note that while our estimated effect of changes in PM_{10} concentrations is economically significant, the daily estimate is not statistically significant and should therefore be interpreted with caution.

[64] As a point of comparison, there are around 70,000 individuals living within the borders of the Bergen toll cordon and around 270,000 individuals living in the municipality of Bergen.

In reality, individuals working in the city center of Bergen as well as those living in close proximity to the main roads running through the toll cordon are also likely to be affected by the air quality improvements.[65]

Next, we need to provide an estimate of the social costs of air pollution. A growing literature has documented a wide range of channels through which air pollution has adverse effects on societal outcomes, such as low birth weight (Currie and Walker, 2011), respiratory diseases (Jans et al., 2018), lower productivity in both physical and high-skilled work (Graff Zivin and Neidell, 2012; Chang et al., 2016; Ebenstein et al., 2016; Archsmith et al., 2018), criminal activity (Bondy et al., 2020), etc. Factoring in all these different channels, and adapting them to our research context, is not necessarily straight forward. E.g., the effect of exposure to one μg/m^3 may be non-linear and depend on the average level of air pollution, and the outcome reported in studies is not necessarily measured in monetary units. In this analysis, we rely on a recent Norwegian report providing estimates of the social costs of one additional individual being exposed to 1 μg/m^3 of NO_2 and PM_{10} over the course of one year (Rødseth et al., 2019). Taking into account a wide range of short- and long-term effects related to e.g., respiratory diseases, cardiovascular diseases, and excess mortality, the study gives cost estimates of NOK 160 per μg/m^3 of NO_2 and NOK 738 per μg/m^3 of PM_{10}. As the congestion charge is not active during weekends, we scale these estimates by the share of working days (230/365). Note that the cost estimates only cover some of the channels by which air pollution may have adverse effects, and should therefore be seen as conservative estimates.

Based on the estimates and assumptions outlined above, we quantify an annual social benefit of **NOK 9.05 million for NO_2** and **NOK 16.14 million for PM_{10}**.

E.4 Global pollutants

The two most important parameters for determining the benefit of CO_2 reductions are the social cost of CO_2 and the average CO_2 intensity of (non-electric and electric) cars. Relying on estimates from Rødseth et al. (2019), we assume a social cost of carbon of NOK 508 per metric ton of CO_2 ($\sim$\$ 61). When quantifying the CO_2 intensity of non-electric cars, we focus on the direct emissions from fuel combustion. As a measure of the CO_2 intensity, we use the average intensity of the passenger car fleet of non-electric vehicles in Norway in 2018 (149 gCO^2/km) (Rødseth et al.,

[65]Due to lack of data, we are not able to measure ambient air quality along the routes around the city center where we see increased traffic. Our welfare calculations hence do not take into account a potential worsening of air quality in these areas. At the same time, the routes around the city center are generally less congested and have a lower population density - both suggesting a lower social cost of traffic.

2019). When quantifying the CO_2 intensity of electric cars, we consider the indirect emissions related to electricity production. As 95% of Norway's electricity is produced from hydropower, we simplify calculations somewhat by assuming a zero CO_2 intensity for electric vehicles.[66]

To quantify changes in CO_2 emissions induced by the congestion charge, we consider three different channels. First, the congestion charge led to lower CO_2 emissions due to lower traffic volumes. As the congestion charge did not change driving costs for electric vehicles, we can safely assume that the reduction in daily traffic volume measured at the toll gates (7,456 cars per day; see Table 2, Column 1), as well as the increase in daily driving around the toll gate (1,054 cars per day; see Table B.5, Column 1), are explained by non-electric cars. Combined, these numbers indicate the net total reduction in cars on the relevant road sections around Bergen per working day. As a proxy for trip length, we use the average work-home distance from the register data (12.37 kilometers). Given 230 working days per year, this amounts to about 36 million fewer kilometers driven annually. Given the assume CO_2 intensity and social cost, this translates to **NOK 2.76 million per year.**

Second, we take into account that the trips around the toll cordon are longer. Of the 1,054 additional trips per working day, 136 are in the north-south direction while 918 are in the south-west direction. Since the north-south route is 4.1 kilometers longer and the north-south route is 3.5 kilometers longer, we estimate that the spatial adaptation increased driving by 0.867 million kilometers annually, which translates to a social cost of **NOK 0.066 million per year.**

Third, and importantly, households adapted to the policy by acquiring an electric car. As explained above, we assume a zero CO_2 emission intensity for electric cars. To simplify the exposition, we assume that the electric cars induced by the policy replaced non-electric cars 1-to-1. This assumption is in line with our findings in Table 5, which indicates that the increase in electric vehicle ownership is very similar in magnitude to the decrease in non-electric car ownership, resulting in a net zero effect on total car ownership. According to Statistics Norway, a passenger car in Hordaland county (where Bergen is located) was driven 11,680 kilometers per year on average in 2016, and according to Section E.1 the congestion charge led 1616

[66]Note that the CO_2 intensity of the electricity consumed in Norway is exceptionally low also when taking into account electricity imports from other countries. In 2019, the CO_2 intensity of consumed electricity in Norway was estimated to around 17g CO_2e/kWh; see `nve.no/energiforsyning/kraftproduksjon/hvor-kommer-strommen-fra/`. By comparison, the average CO_2 intensity of electricity from coal power plants in Europe was around 900g CO_2/kWh in 2019; see IEA (2020). Note that we simplify calculations by disregarding equilibrium price effects on international markets for power and CO_2 quotas. We also disregard indirect CO_2 emissions related to the production and scrapping of cars.

households in Bergen to acquire electric cars. This translates to about 19 million kilometers annually, with a monetary value of around **NOK 1.43 million per year** due to lower CO_2 emissions. Note that we disregard second-order equilibrium effects working through the second-hand car market.

Combining the three sources of changes in CO_2 emissions gives an annual social benefit of **NOK 4.12 million per year**.[67]

E.5 Assumptions and estimates

Table E.1 gives an overview of all values used in the welfare calculation, including references where appropriate.

Table E.1: Assumptions and estimates used in welfare calculations

Description	Source	Value
Panel A: Adaptation costs		
Congestion charge (additional NOK per trip during rush hours)	Table 1	20
Working days per year	Norwegian average	230
Change in the number of cars during rush hours per day	Table 2, Column 2	-8,046
Paying commuting households exposed	Register data	38,482
Probability of acquiring electric car due to exposure (percent)	Table 5, Column 1	4.2
Panel B: Time savings		
Average time savings during rush hours (minutes per car)	NPRA (2016), pg. 14	2.3
Cars affected (cars per day during rush post policy)	Traffic data	51,534
Value of time for drivers (NOK per hour)	Flügel et al. (2020), pg. 59	93
Number of passengers per car	Conservative assumption	0
Panel C: Local pollutants		
ΔNO_2 (μg/m3)	Table 3, Column 1	3.06
ΔPM_{10} (μg/m3)	Table 3, Column 1	1.19
Cost per year per individual per μg/m3 of NO_2 exposure (NOK)	Rødseth et al. (2019), pg. 23	160
Cost per year per individual per μg/m3 of PM_{10} exposure (NOK)	Rødseth et al. (2019), pg. 22	738
Population exposed to lower air pollution	Register data	29,287
Panel D: Global pollutants		
Cost per tonne of CO_2 (NOK)	Rødseth et al. (2019), pg. 17	508
Average CO_2 intensity of non-electric cars (g CO_2/km)	Rødseth et al. (2019), pg. 31	149
Average CO_2 intensity of electric cars (g CO_2/km)		0
Average driving per car per year (kilometers)	Statistics Norway*	11,680
Average work distance (kilometers)	Register data	12.37
Change in the number of cars passing the toll cordon (cars/day)	Table 2, Column 1	-7,456
Additional cars driving around, north-south direction (cars/day)	Table B.5, Column 1	135.6
Additional cars driving around, south-west direction (cars/day)	Table B.5, Column 1	908.5
Additional trip length, north-south direction (kilometers)	Google maps	4.1
Additional trip length, south-west direction (kilometers)	Google maps	3.5

* Number reflects the average of all passenger cars registered in Hordaland county in 2016, publicly accessible at: https://www.ssb.no/statbank/table/12576/tableViewLayout1/ (accessed August, 2020).

[67] Note that this figure is based on the exact calculations rather than the three rounded numbers given above.

E.6 Additional calculations for private costs

Table E.2: 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: This table displays prices in NOK for the modal compact electric car (Nissan Leaf, battery range $\approx$ 240 km) and the modal luxury electric car (Tesla Model S, battery range $\approx$ 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 $\approx$ 1 EUR and $\approx$ 1.2 USD.

Table E.3: 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**
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 (NOK)	**18,037**	**51,206**	**30,605**

Notes: This 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 E.2 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 is from Table E.1, while 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$\times$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 $\approx$ 1 EUR and $\approx$ 1.2 USD.