

Doctoral thesis

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Lana Krehić

Three Essays on Transport Economics and Policy

NTNU
Norwegian University of Science and Technology
Thesis for the Degree of
Philosophiae Doctor
Faculty of Economics and Management
Department of Economics



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ISBN 978-82-326-5286-0 (printed ver.)
ISBN 978-82-326-6226-5 (electronic ver.)
ISSN 1503-8181 (printed ver.)
ISSN 2703-8084 (online ver.)

Doctoral theses at NTNU, 2021:57

Printed by NTNU Grafisk senter

Preface

This thesis consists of three independent essays addressing different topics within transport economics and policy. All chapters are based on data from several sources that are combined for the purpose of this thesis. The essays in Chapters one and three are authored by myself. The essay in Chapter two is joint work with Professor Colin Green at the Department of Economics at the Norwegian University of Science and Technology (NTNU).

Acknowledgements

During the three years of my PhD, I have gotten to know many professors and fellow PhD students who have created a supportive environment that has helped me finish my thesis. First, I would like to thank my supervisor, Colin Green, who has always had time for my questions and treated every concern I had seriously. He has provided invaluable feedback and supplied me with encouragement, in addition to teaching me a lot about economics, research and Australian peculiarities, for which I am grateful. I would also like to extend my gratitude to co-supervisor Jørn Rattsø, who has read my work in progress and widened my perspective.

I thank my other colleagues at the Department of Economics for interesting general discussions, constructive feedback on my presentations and for inspiring me with your research and participation in various public debates. I would also like to credit the administrative staff for their helpfulness in any needed situation. Further, I am grateful for being trusted with working in the seminar committee and various students support assignments.

The PhD environment during the last three years has been sublime, and I want to especially thank Sigrid, Irmelin, Isabel, Mia and Haakon. I have appreciated the big and small discussions about important and unimportant topics, seminars, quizzes, workouts, coffee tastings and leisure activities, which has filled me with joy and motivation to keep working. Importantly, the knowledge and passion you exhibit for your research areas have inspired me in my own project.

Last, but not least, I wish to thank my family for supporting and believing in me, and especially Einar, for his patience and counsel.

Contents

Introduction	1
Chapter 1	
A free rider problem? The effect of electric vehicles on urban toll prices in Norway	7
Chapter 2	
Bar Closing Hours and Traffic Accidents: Evidence from Municipal Variation in Norway	41
Chapter 3	
Safety first? The effect of studded tyres on traffic accidents and local air pollution	72

Introduction

Transport choices are classic examples of individual decisions being made without fully accounting for the effect it has on society. For example, a person considering riding their car to work will only account for the direct costs of the trip, such as gasoline and parking expenses. However, for society as a whole, a car trip has additional costs like road wear and tear, traffic congestion and air pollution. Similarly, a person that drink-drives considers only the risk of being caught by the police, whereas the cost inflicted on the society encompasses the risk of injuring others, the risk of increasing pressure on healthcare systems, and increased policing expenses. This inconsistency of costs carried by the individual and the society, namely negative externalities, represents a welfare loss for the society as a whole.

A common approach to internalise these externalities is to introduce taxes and regulations to the market as suggested by Pigou (1920). In transport, but also in other areas of economics, determining the optimal tax relies heavily on comprehending the damages associated with the negative externality (Vickrey, 1963). In practice, however, there are multiple factors that complicate the process of deciding on the proper market intervention. First, it is challenging to identify the extent of the negative externality. Second, the design of a policy depends on factors that are not associated with the externality itself. For example, it is influenced by what level of government it is enacted, the political composition of policy-makers and which laws and taxes are already in place. This creates a demand for research that seeks to identify negative externalities and that thoroughly covers the causal effects of policies that have been or will be introduced.

Causal relationships are useful for making predictions about the consequences of an intervention (Angrist & Pischke, 2009), which in turn enables predictions

that can be used to form evidence-based policy recommendations. Therefore, it is important for studies undertaking empirical methods to identify causal effects. In general however, there is a lack of causal evidence in many aspects of transportation research. One reason for this is that policies rarely are introduced alone. For example, the introduction of congestion charges are often accompanied by increased public transport supply, and alterations in drink-driving laws are supplemented with changes in night-time policing activities. This makes it difficult to disentangle the impact of one particular regulation, and consequently prevent the identification of causal effects.

In this thesis, which consists of three independent essays, I aim to empirically identify the causal effects of different policies on transport related outcomes. I do this by studying policies that have been enacted in multiple local jurisdictions in Norway over several years. This allows me to extract information on different levels, both cross-sectionally and over time. Moreover, by using the appropriate research designs, I am able to separate the effect of a policy from other effects and thereby identify the causal parameters of interest.

In the first essay, I investigate how the national electric vehicle policy in Norway effects the prices in local urban toll rings. Numerous cities around the world have attempted to internalise congestion costs from road traffic by instituting charges for entering their city centres. Traditional factors included in the costs of congestion typically entail delay costs and occasionally the cost of accidents (Vickrey, 1963). In Norway however, tolls have primarily been used as a means to finance new roads and not as a instrument for reducing the congestion and the pollution levels (Larsen & Østmoe, 2001; Small & Verhoef, 2007). Concurrently, from the 1970s and onward there was a strong desire to establish a production of electric vehicles in Norway. In an effort to increase demand, owners of electric vehicle were granted numerous tax exemptions relating to the purchase and the use of electric vehicles. These exemptions are still in place today and equates to 5.78 billion NOK (approximately 613 million USD) in 2017 alone (Ministry of Finance, 2017). One of the tax benefits of owning an electric vehicle is the exemption of toll charges, which translates to an income loss for the local urban toll rings.

Using panel data of Norwegian cities with toll rings, I exploit regional varia-

tion in an instrumental variable approach and find that a higher share of electric vehicles increases toll charges. The result implies that owners of conventional cars pay more per passing because of the national exemption. While this may be an outcome that is advantageous with respect to the discouragement of the use of fossil fuelled cars, it has an additional consequence that has implications for social welfare. As the majority of electric vehicle owners have an above-average income, exempting electric vehicle owners from toll charge suggests a distribution effect where low income groups end up paying the largest part of the increased toll price.

In the second essay, written in collaboration with Colin Green, we study the relationship between bar closing hours and night-time traffic accidents over a ten year period in Norway. According to the World Health Organization (2007), approximately 20 percent of fatally injured drivers in high-income countries have a blood alcohol concentration above the legal limit. While changes in bar closing hours and its effect on traffic accidents have received some attention in the literature (see for example Vingilis et al. (2005); Green et al. (2014); Biderman et al. (2010)), the studies have been limited to exploring changes in one direction and of the same magnitude, with results been inconclusive.

Municipalities in Norway are free to choose opening hours within quite broad nationally set limits, namely between midnight and 3am. Consequently, closing hours are subject to local public debates and serve as political planks that lead to substantial variation both across municipality and time. We utilise this variation and find that, on average, the effects of closing hour on traffic accidents is zero. However, when accounting for population differences among municipalities, we find that the effect in fact is heterogeneous. Most notably, longer hours in highly populated areas are associated with a reduction in accidents whereas the opposite is true for less populated municipalities. The probable mechanisms producing these results are that unified and early closing hours increase the risks for multiple drinkers driving at the same time. Moreover, it makes multiple vehicle accidents more likely due to higher underlying traffic flows that naturally occur earlier at night. Indeed, later closing hours may also limit alternative modes of transport or make it more expensive, such as public transport or taxis, which is germane to less populated municipalities. The results imply that, depending on the context,

employing closing hours as a policy instrument can have large effects on night-time traffic safety.

In the third essay, I investigate how a particular pollution-restricting policy, namely the annual studded tyre ban in Norway, impacts traffic accidents and pollution levels. Although offering superior traction on icy roads, the use of studs is frequently debated. By tearing off micro-particles from the road surface, studded tyres contribute to increase pollution levels considerably. According to the World Health Organization (2018), particulate matter pollution is believed to have caused as many as 4.2 million deaths globally in 2016. Recognising this risk, policy-makers aim at curtailing pollution by reducing the use of studded tyres. The objective of the final essay is to assess the societal costs of reducing studs use, namely how the number of traffic accidents are effected, and compare them to the societal benefits of reducing pollution.

I study the effects of studded tyres by utilising a periodical ban on studded tyres in Norway, which is reinstated every year after Easter. Due to this rule, the ban start date can vary by a month from one year to another. This greatly reduces the threat of other variables changing simultaneously with the ban, thus creating a setting suitable for a regression discontinuity design. More precisely, I compare the accident and the pollution levels one week before and one week after the ban starts. I find that, although there is evidence of an increased safety effect when using studded tyres, the costs of increased pollution outweighs the benefits of studded tyres. I argue that today's car and road maintenance technology goes a long way in providing a sufficient safety.

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Chapter 1

A free rider problem?

The effect of electric vehicles on urban toll prices in Norway

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Abstract

Numerous cities around the world have attempted to internalise congestion costs from road traffic by instituting charges for entering their city centres. The collected revenues are often redistributed as investments in the road infrastructure. Many of these schemes allow for the exemption of cleaner vehicles, which might offset the reduction in congestion and reduce revenue. This paper assesses the effects of exempting electric vehicles from the charge that is levied on other drivers. Using panel data of Norwegian cities that have urban toll rings, I exploit regional variation in electric car adaption and find that owners of conventional cars pay 3.3 NOK (0.36 USD) more per passage due to the exemption. As the majority of electric vehicle owners have above-average incomes, the exemption implies a distributional effect in which low income groups pay the largest portion of the increased toll price.

JEL Classification: H230, R40, R42

Keywords: Electric vehicles, Congestion, Toll roads, Distributional effects

*I thank Colin Green, Askill Halse and Jørn Rattsø for comments that substantially improved this paper. I also benefited from comments that were made by participants at the 2019 Annual Meeting of the Norwegian Association of Economists and the Scottish Economic Society Annual Conference in April 2019. Finally, I am grateful for data on toll prices from Ferde, Fjellinjen, Nye Kristiansand Bompengeselskap and Vegamot. **Correspondence:** Department of Economics, Norwegian University of Science and Technology, Norway. *E-mail address:* lana.krehic@ntnu.no

1 Introduction

Increasing air pollution and congestion from road traffic are challenges that are faced by many cities. These challenges have led to the development of a variety of schemes that aim at curbing private vehicle usage, where a main motivation has been alleviating congestion externalities (Pigou, 2017; Vickrey, 1963). For example, in some cities, such as Stockholm, London, Singapore and Milan, commuters must pay to enter a predefined congestion area. Another common approach for addressing increasing local air pollution is to incentivise the purchase and the use of cleaner vehicles. This can be realised by offering tax credits, providing access to high-occupancy vehicle lanes, or exempting less-polluting vehicles from congestion charging, which is the focus of this paper.

The combination of charging for driving in city areas and exempting some vehicles from this charge implies a threefold function of congestion charges. First, they alleviate congestion and pollution. Second, they raise funding for road construction and public transport. Finally, they incentivise the use of exempt vehicles, such as electric cars. While these functions are not entirely consistent, the combination of congestion charging and exemptions is a well-established strategy for limiting the exhaust of harmful pollutants. Proponents argue that these exemptions are crucial for reducing the pollution that arises from road traffic. Opponents argue that exempt vehicles contribute to both congestion and road deterioration, and that the policy reduce revenue from toll collection. Furthermore, electric vehicle owners tend to be well-educated, have above-average income and belong to multiple-car households. This raises the question of whether the combination of congestion charging and electric vehicle exemptions have unintended consequences for social welfare where the tax that is collected from conventional motorists is used to subsidise well-off electric vehicle owners.

I examine this issue by focusing on Norway, which provides an interesting study for a variety of reasons. A number of cities in Norway have implemented “city packages”. These include projects that aim at improving roads and public transport, funded by revenues from urban toll rings. These tolls are similar to congestion charges in that crossing a ring involves a charge, however, at least historically the motivation has been to finance road infrastructure rather than to reduce congestion and pollution. Moreover, throughout the past decade, there

has been a substantial and rapid rise in the use of electric cars in many countries. This rise has arguably been most rapid in Norway, which is reported to have the world's highest rate of electric vehicles. For example, the electric vehicle share grew from one percent to one-third of new car sales between 2011 and 2018.

Since the early 1990s, the Norwegian government has allowed several generous tax exemptions for electric vehicle owners, including the exemption from all toll road charges. The cost of the toll exemption scheme has risen proportionally with the number of exempt vehicles, and is frequently a subject of debate. For example, the toll charge exemption amounted to a loss in revenue of 550 million NOK (approximately 64 million USD) in 2016 (The Norwegian Public Roads Administration, 2018). The presence of several toll rings and a non-trivial increase in the share of electric vehicles allows for a study that covers multiple settings within a country over a period of time.

In this paper, I focus on two objectives. The first objective is to explore how the share of electric vehicles affects urban toll ring charges in Norwegian cities. The dramatic increase in electric vehicle ownership represents a potential income loss for local governments and their road infrastructure projects. The question of interest is whether local governments raise urban toll ring charges to compensate for this income loss. The second objective recognises that the characteristics of conventional and electric vehicle owners may differ. Consequently, I examine whether increases in toll price have heterogeneous effects among income groups.

I utilise data on the numbers of electric vehicles in cities with urban toll rings and their adjacent municipalities from 2010 to 2017, which is a period that coincided with increased electric car ownership. A concern is that higher toll charges and other policies that are included in the city packages may affect the uptake of electric vehicles. This is a challenge to causal interpretation. To address this issue, I employ an instrumental variable strategy where I use the variation in another location's electric vehicle share as a source of exogenous variation. This variation will reflect underlying factors that drive electric vehicle uptake, net of local factors that affect the local electric vehicle uptake. A remaining concern is that some toll pricing decisions may depend on other toll rings' pricing strategies, thereby creating a learning effect that challenges the identification strategy of the main instrument. As an alternative instrument, I use the electric vehicle share

in areas of Norway that are not adjacent to an urban toll ring, which produces no change in the estimated effect.

The baseline instrumental variable estimate implies that a one percentage point larger electric vehicle share leads to a toll charge increase of between 2.9 and 4.8 percent. A back-of-the-envelope calculation based on estimates in this paper suggests that in 2017, a conventional vehicle owner paid 3.3 NOK (0.36 USD) more every time she crossed an urban toll ring compared to the counterfactual case in which electric vehicles were not exempted from the charge.

After conducting a range of robustness tests, I extend the analysis to examine heterogeneous effects. The results suggest that the effect of electric vehicle shares on toll charges is slightly stronger in more populous cities. In addition, I find that increasing the toll price is subject to the discretion of the political wing that holds the majority in the toll ring municipality. Only left-wing local governments react to a larger share of electric vehicles by raising the toll price. This suggests that right-wing parties balance their budgets in other ways, for example by reducing road infrastructure investments or financing road infrastructure investments in an alternative way.

The distributional analysis that is conducted at the end of the paper suggests that the increase in toll price translates into a regressive tax. This outcome mainly derives from the difference in the ownership share of the two car types among income groups. Overall, the results from this paper suggest that the distributional effect of exempting electric vehicles from tolls is heterogeneous, where low-income individuals who reside in big cities potentially experience the largest cost of electric vehicle exemption. In contrast, the above-average income group receives a net benefit from the exemption.

The remainder of the paper is organized as follows. In Sections 2 and 3, I present the related literature and the institutional background. Section 4 presents the data and the empirical method. Sections 5 and 6 present the main findings and robustness tests, respectively. In Sections 7 and 8, I analyse the heterogeneous and distributional effects. In Section 9, I present the conclusions of this study.

2 Related literature

Road pricing schemes can be regarded as serving two objectives. First, they align the cost of private vehicle use in congested areas with the social cost of using the road. Second, they provide funding for transport infrastructure improvements. Indeed, researchers have found that tolls improve air quality, reduce congestion and, consequently, encouraging labour supply, in addition to increasing traffic safety (Fu & Gu, 2017; Gibson & Carnovale, 2015; Hymel, 2009; Green, Heywood, & Navarro, 2016). However, other economists have raised concerns regarding the negative effects that road pricing mechanisms can have on economic activities, such as discouraging labour force participation, adding congestion to areas in which no charge is levied and reducing retail real estate prices (Parry & Bento, 2001, 2002; Agarwal, Koo, & Sing, 2015).

Even though exempting cleaner vehicles from road pricing is common, the literature has paid little attention to its consequences. Nevertheless, the experience from several pricing schemes is that the number of exempted vehicles that enter the charged area dramatically increases after implementation, thereby partially offsetting congestion decreases and reducing revenue (Börjesson, Eliasson, Hugosson, & Brundell-Freij, 2012; Leape, 2006; Rotaris, Danielis, Marcucci, & Massiani, 2010). In addition, Green, Heywood, and Navarro (2020) find that NO₂ emissions increased after the London congestion charge was imposed, which was presumably due to a shift towards exempted diesel vehicles. In a paper that is closely related to this one, Bento, Kaffine, Roth, and Zaragoza-Watkins (2014) find that allowing single-occupant hybrid vehicles access to high-occupancy vehicle (HOV) lanes, as a way of increasing the hybrid vehicle demand, resulted in a substantial welfare loss. They conclude that the losses that are incurred due to unpaid congestion in the HOV lanes far outweigh the benefits of increasing the number of cleaner hybrid vehicles.

An important aspect of exempting selected vehicles from charges is the question of who the owners of electric vehicles are. Borenstein and Davis (2016) find a strong correlation between income and electric vehicle take-up in the US. Offering an income tax credit to all vehicle buyers in the US led to the top income quintile receiving 90 percent of all credits. In the case of Norway, electric vehicle owners often have above-average income and belong to a two-car household

(Institute of Transport Economics, 2016). Thus, the toll charge exemption has the potential to have negative distributional effects, with an inverse relationship between the payers of the largest share of the tax and income level.

This paper contributes to our knowledge regarding the interaction between policies that aim at increasing the demand for cleaner vehicles and road charging policies. I demonstrate that incentives aimed at increasing electric vehicle adoption can have unintended distributional effects. These findings can influence how welfare effects of pricing schemes are assessed, and contributes to the extensive literature on incidence. Moreover, this paper is the first to examine urban road pricing in multiple cities over time, whereas earlier studies have mostly focused on a single city. Last, using the estimates from this paper, I offer a unique figure of the extent to which electric vehicle exemption has increased the toll charge for owners of conventional vehicles. This contributes to policy debates that are likely to occur in many cities around the world that are dealing with challenges relating to congestion and pollution.

3 Institutional background

3.1 Electric vehicle policy in Norway

Norway has a long history with electric vehicles. From the 1970s onward, there was a strong desire to establish Norwegian electric vehicle production, and attempts at commercialisation were conducted. In the 1990s, an electric vehicle association was established, of which the lobbying activities focused on removing barriers to electric vehicle adoption (Figenbaum, Assum, & Kolbenstvedt, 2015). These activities resulted in many benefits being implemented in the late 1990s and early 2000s that remain in place today. Table 1 presents an overview of all of the policies and when they were implemented. For instance, drivers who buy electric vehicles are exempt from registration and value added tax, have a reduced annual road tax, can recharge and park in public parking spaces for free, have access to public transport lanes and, critically for this paper, can pass through road tolls free of charge.¹

¹In the later years, some municipalities began charging a full or reduced price for parking for electric vehicles compared to conventional cars due to overcrowding of electric vehicles.

Table 1: Overview of electric vehicle promotion policies

Exemption from registration tax	1996
Reduced annual vehicle tax	1996
Exemption from road tolls	1997
Free parking (on public parking spaces)	1999
Reduced company car tax	2000
VAT exemption	2001
Access to public transit lanes	2005
Exemption from paying ferry fees	2009

Note: The stated year is the year the policy became permanent. For example, the exemption from registration tax was under trial from 1990 to 1995.

Nevertheless, the ownership of electric vehicles did not change substantially after the implementation of these incentives. Figure 1 provides an overview of the shares of electric vehicles in cities that are within commuting distance to a toll ring in Norway, between 2010 and 2017. The share of electric vehicles was essentially zero until 2010, when ownership started to grow at an exponential rate. Figenbaum et al. (2015) explain this growth in terms of increased international focus on greenhouse gas mitigation and improved battery technology. In addition, from 2010 onwards, many of the large international car companies introduced their electric vehicles into the Norwegian market. The vast increase in availability of electric cars led to a rapid expansion in sales and increased price competition among manufacturers, which further fuelled the acquisition of electric vehicles.² According to the Norwegian Road Federation, five percent of the car fleet was battery-only by the end of 2017, and by August 2018, almost 30 percent of new car sales consisted of electric vehicles.

All electric vehicles have a registration number that starts with the letters EL or EK, which offers a convenient way of differentiating between toll paying conventional cars and free passing electric cars. When driving through a toll, the vehicle’s registration number is automatically photographed. Every month, the local toll company sends an invoice to the vehicle’s registration address, where the number of passes, toll price and rebates are summed.

²The price competition that followed the entry of international car companies into the Norwegian market led to the end of the Norwegian electric vehicle production.

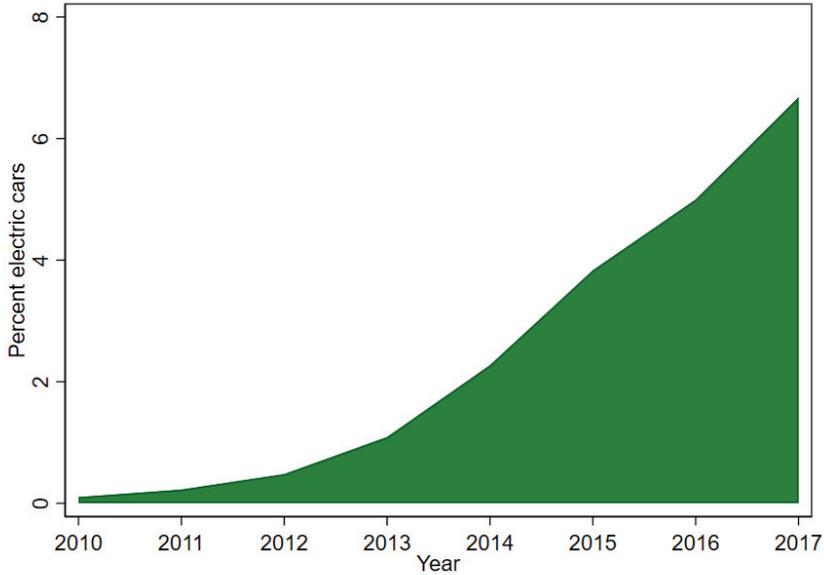


Figure 1: Average share of electric vehicles in municipalities with or within commuting distance to an urban toll ring from 2010 to 2017 in Norway. *Source: Statistics Norway.*

3.2 Urban toll rings

The first urban toll ring in Norway opened at the end of the 1980s, and the number of rings has been increasing steadily ever since. Initially, the objective of urban toll rings was not to reduce traffic flows or pollution but to finance new road projects and to improve established roads to handle growing traffic flows (Ramjerdi, Minken, & Østmoe, 2004). This was manifested in low toll rates and little or no variation in rates according to the degree of congestion or the time of day.

Today, urban toll rings still serve as a mechanism for raising revenue for road infrastructure investments, some of which operate similarly to a congestion charge with rush-hours charges. There are several rationales for revenue raising using this strategy. First, the city infrastructure is improved at a faster rate than if municipalities were to wait for national government funding. Second, there is a principle of fairness that is based on a user-pay concept. The driving population in and around the city pays the road tolls, but also benefits from the revenue

in the form of investment in the local road infrastructure. Third, municipalities often arrange a revenue matching scheme with the central government. The revenue that is raised in each toll ring is matched with an equal amount of government funding, which provides additional incentives for local governments to implement toll rings (Larsen & Østmoe, 2001).

The process of instituting an urban toll ring begins with the affected municipalities and counties initiating a project. The objectives of the project could be to enhance a road or the public transport system, to improve safety for cyclists and pedestrians, and to finance the project by using toll road revenue. If a majority among the representatives of the local government councils is obtained, an independent public company is established. The company's responsibility is to manage the finances, which includes obtaining loans that are related to the planned projects, administering the toll collection and managing revenue. After paying for operating costs, the profits are limited to the expenditures on the predefined projects.

The initial toll charge is computed based on the predicted costs of the planned projects and the expected revenues over the collection period. The typical factors that are considered when selecting the toll charge include the financial contribution of the central government, the number of paying cars in the focal urban area and the duration of the collection period (which cannot exceed 15 years). Two types of price increases are possible during the urban toll ring's operation period. The first type is price adjustment changes. For example, charges are raised to adjust for increases in the general price level. The second type of price change is politically decided, and often involves larger price changes. Municipalities consider increasing the toll price when additional revenue is needed either to add projects to the city package or if the planned projects are underfinanced. All of these changes must win the local government majority's vote for the price change to be implemented. Consequently, the political composition of the parties and their attitude towards the use of urban toll rings for financing are likely to be important factors that determine whether and when toll prices are changed.

By 2018, eleven cities in Norway had urban toll rings, and this number is

Table 2: Overview of cities with an urban toll ring, years of implementation, prices, population and electric vehicle shares

City	Year opened	Price (2010) ¹	Price (2017)	Population (2017)	Electric vehicle share (2017)
Bergen	1986	10.31	23	278 556	11.25%
Oslo (Bærum)	1990 (2008)	23.68 (11.61)	36.65 (12.83)	666 759 (124 008)	9.97% (11.5%)
Trondheim ²	1991-2005, 2010	8.8	12.93	190 464	6.64%
Kristiansand	1997	12.03	12.37	89 268	9.06%
Stavanger area	2001	13.75	16	208 225	6.12%
Namsos	2003	8.6	10.2	13 051	1.16 %
Tønsberg	2004-2016	8.6	7.61 ³	44 922	4.93 %
Haugalandet	2008	6.86	6	91 485	5.3 %

Note: The prices are specified in NOK and calculated as average hourly real prices to include hours with higher or zero charge.

1) In terms of 2017 prices.

2) Trondheim has a scheme with one inner and one outer ring, and commuters pay when both entering and leaving the rings. The price differs between the two rings, hence, the stated price is the average price.

3) The stated price is the price that was valid in 2016, which was the last year that the ring was in operation.

Source: *Fjellinjen AS, Ferde AS, Agder Bomdrift AS, Vegamot AS, The Norwegian Public Roads Administration, Statistics Norway.*

expected to double over the next decade.³ An overview of the cities with toll rings, the years of implementation, prices and population is presented in Table 2. The number of urban toll rings has been increasing steadily during the last three decades. The prices that are listed in columns two and three are twenty-four-hour averages, which are stated in 2017 prices. Averaging is conducted to ensure that any changes in the ring operation hours or rush hour charges are reflected in the price. The real prices have increased most prominently in the largest cities, whereas smaller and more recently opened rings have experienced moderate price increases.

Ever since the urban toll rings opened, toll companies have offered subscriptions to toll ring passers. The discounts were implemented to incentivise prepayment, and to give the perception that toll ring costs were maintained at reasonable levels for frequent passers, which likely contributed to lower public resistance (Larsen & Østmoe, 2001). The discount schemes have varied between cities and over time. The discounts range between 10 and 50 percent per passage, and approximately 80 percent of crossings are conducted by vehicles with a subscription (The Norwegian Public Roads Administration, 2018). This has at least three implications: First, most vehicle owners face a lower price in practice than the stated prices. Thus, the incentive for switching to an electric vehicle or an alternative transport mode is reduced. Second, in some cases, the toll companies reduced the maximum discount instead of increasing the toll price. Such changes will not be reflected in the stated prices, whereas most commuters experienced a price increase in practice. Third, the discounted price represents the true income of the toll companies. Therefore, the rebated prices are a more accurate measure of the revenue than the stated prices. Thus, I estimate the effect of the electric vehicle share on the urban toll ring price under the assumption that the price commuters face is the price that corresponds to the maximal discount.

³The cities that had toll rings in 2017 are Bergen, Bodø, Bærum/Oslo, Førde, Haugesund area, Harstad, Namsos, Kristiansand, Trondheim, Skien/Porsgrunn and Stavanger area. I am excluding the toll rings in Bodø, Førde, Harstad and Skien/Porsgrunn from of the analysis because they all opened late in the observation period (2015). Thus, these toll companies have more information about the relationship between the toll price and the electric vehicle share and can set a high initial price to prepare for a large electric vehicle share. However, I do include the toll ring in Tønsberg, which closed down in 2016 because the collection period ended.

4 Theoretical motivation and empirical approach

To examine the composition of the toll company's revenue, I establish a simple theoretical model. Suppose that the toll companies have a target revenue and the only variable they can change to reach the target revenue is the toll price. The revenue constraint is formulated as follows:

$$R_{it} = (1 - EV_{it}) \cdot TV_{it} \cdot P_{it} \cdot N_{it} \quad 0 \leq EV_{it} \leq 1 \quad (1)$$

where R_{it} is the revenue in toll ring i in year t , EV_{it} is the share of electric vehicles and TV_{it} is the total number of vehicles in operation in toll ring i in year t . Furthermore, P_{it} is the toll price and N_{it} is the number of times a vehicle passes toll i in year t .

How will a shock in the number of electric vehicles affect the price in the toll ring, subject to a revenue constraint? Assume that the total number of vehicles in operation and the number of times a vehicle passes the toll are constant over time, namely, assume that TV_{it} and N_{it} are fixed. For simplicity, suppose that the number of electric vehicles is not a function of the toll price. The total differential of Equation (1) is:

$$dR_{it} = (1 - EV_{it})TV_{it} \cdot N_{it} \cdot dP_{it} - TV_{it} \cdot N_{it} \cdot P_{it} \cdot dEV_{it} \quad (2)$$

Setting the change in revenue to zero and solving for the change in the toll price yields the following expression:

$$dP_{it} = \frac{P_{it}}{(1 - EV_{it})}dEV_{it} \quad (3)$$

According to Equation (3), if the electric vehicle share changes by dEV_{it} , the toll price must be changed by dP_{it} to hold total revenue constant. The magnitude of dP_{it} depends on the initial toll price and the electric vehicle share. For example, suppose that $dEV_{it} = 0.01$, which represents an increase in the share of electric vehicles by one percentage point. With an initial toll price of 20 NOK and an electric vehicle share of 4 percent, the toll price must increase by 0.21 NOK, or by one percent from the initial price, to keep the revenue from toll charging unaltered.

To calculate the relevance of the relationship between electric vehicles and the toll price more rigorously, I estimate a model that aims at identifying the causal effect of the electric vehicle share on the toll price. Information on the toll prices between 2010 and 2017 in Norwegian urban toll rings has been provided by the respective toll companies. Data on the number and types of vehicles that are registered, population, income, share of the urban population, unemployment and education for all 419 municipalities in Norway are acquired from Statistics Norway (SSB).⁴ Since the data are provided on an annual basis, the prices of the toll rings are weighted annual averages, where the weights are the numbers of days for which the corresponding prices are in operation. This is done to capture price changes that occur during a calendar year. Using these data, I explore whether the penetration of electric vehicles into the Norwegian car market contributes to increasing the price in the toll rings. I estimate the following equation:

$$\ln(\text{realprice})_{it} = \alpha_1 \text{ShareEV}_{it} + \alpha_2 X_{it} + \alpha_i + \varepsilon_{it} \quad (4)$$

where $\ln(\text{realprice})_{it}$ is the natural logarithm of the real price of a toll ring in municipality i in year t , ShareEV_{it} is the number of electric vehicles over all vehicle types in municipality i at time t , X_{it} is a vector of observed covariates for municipality i at time t , α_i denotes unobserved time-constant effects in municipality i and ε_{it} denotes the time-varying idiosyncratic errors.⁵ The key parameter of interest is α_1 , which provides an estimate of the effect of an increase in the share of electric vehicles on the price of the toll ring.

Several econometric challenges arise when identifying the effect of the electric vehicle share on the toll rates. First, the main explanatory variable is the number of registered vehicles and is therefore a proxy for the actual usage of a specified vehicle type. Newer vehicles may be more frequently used than older vehicles due to improvements in fuel efficiency, safety and appearance. This might be especially true for electric vehicles in areas where an urban toll ring has been established. According to the toll company in Oslo (Fjellinjen), the share of

⁴There were 426 municipalities in 2017. There have been several splits and mergers of municipalities during the last decade in Norway and the seven missing municipalities from my data are a consequence of this.

⁵The included vehicles are passenger cars, which are divided in five fuel categories: petrol, diesel, gas, electric and other.

electric vehicles that passed through the toll ring in 2017 was 10.41 percent, while the share of registered electric vehicles was approximately 8.4 percent. This is a source of measurement error, which implies that the true effect of the electric vehicle share on toll charges may be underestimated when using the number of registered vehicles. However, the number of toll ring crossings is a more responsive and potentially noisy measurement compared to using the number of registered cars. For example, if taxes on conventional car use or petrol prices increase, it is easier to drive less than to buy a new car. Thus, the use of the stock of cars can be advantageous in this analysis.

Furthermore, it is possible that the most frequent toll passers in Oslo, for example, are not people who live in Oslo. People who live in the adjacent municipalities and commute to Oslo for work are more likely to face the toll charge. For example, the neighbouring municipality Bærum had an electric vehicle share of 11.5 percent in 2017, which seems to be closer to the crossing share that was observed in the Oslo toll ring. This emphasises the importance of including close-by municipalities in the analysis. Consequently, I include municipalities that have strong proximity and economic and labour market attachments to the municipality where the toll ring is located. For example, if a city has an urban toll ring, the inhabitants of neighbouring municipalities will be assumed to face the same price as the inhabitants of the ring city.⁶

An additional challenge is that the number of planned projects and, thus, the cost of the projects will vary among city packages. Moreover, the topography and car culture may influence car use, toll cordon placement and the number of cordons that are required for the formation of a ring around the city. These time-invariant variables affect both the share of electric vehicles and the toll price. Failing to control for the time-fixed variables will thus result in a biased estimate of α . To address this concern, all regressions are estimated with municipality fixed effects. In addition, a number of time-varying variables, such as population size, share of urban population, average income and unemployment levels are

⁶Two additional groupings have been applied in the analysis. One follows the structure of Statistics Norway's economic regions, which are based labour and commodity market flows, and on population levels. The other follows the proposed groups of Bhuller (2009) where the main grouping criterion is the commuting patterns in the municipality. Neither of the two alternative groupings yielded any significant differences in the results. Therefore, they are excluded from the paper.

included in the model.

Finally, one might expect higher toll prices to incentivise consumers to buy electric vehicles, since passing the tolls with an electric vehicle is free of charge. If this is the case, OLS estimates of the effect of the electric vehicle share on the toll price may overstate the true impact on the toll price. To identify the causal effect of a rising electric vehicle share on the toll price, I instrument the electric vehicle share in one area by the average share of electric vehicles in other Norwegian areas.⁷ The government’s electric vehicle policy, as well as supply and technological progress of electric vehicles, is homogeneous within the country. Consequently, changes to any of these components will cause analogous effects across the country. This can be exploited to instrument for the endogenous electric vehicle share in a city. The identifying assumption that underlies this strategy is that other regions’ electric vehicle uptake should not be functions of the focal city’s tolls. Moreover, area specific market conditions will provide independent variations across regions. The empirical specification is as follows:

$$\ln(\text{realprice})_{it} = \alpha_1 \text{ShareEV}_{it} + \alpha_2 X_{it} + \alpha_i + \varepsilon_{it} \quad (5)$$

where

$$\text{ShareEV}_{it} = \beta_1 \overline{\text{ShareEV}_{-it}} + \beta_2 X_{it} + \beta_i + \epsilon_{it} \quad (6)$$

where $\overline{\text{shareEV}_{-it}}$ is the average electric vehicle share in all municipalities except for municipality i . In discussing the results, I consider potential threats to identification, such as learning effects across urban toll ring areas and the choice of the cluster level of the standard errors, and seek to address these. Importantly, I show that the key results are robust to a range of exercises that seek to address these concerns.

5 Results

I start this section by estimating the relationship between electric vehicle share and urban toll ring prices with a fixed-effects approach. There are a total of

⁷Similar approaches have been applied by Hausman, Leonard, and Zona (1994), Nevo (2001), Autor, Dorn, and Hanson (2013) and Hausman and Ros (2013).

Table 3: Influence of the share of electric vehicles on the urban toll ring prices (2010 – 2017)

	(1)	(2)	(3)	(4)	(5)	(6)
	OLS	FE	FE	FE	FE	FD
Share electric vehicles	0.060*** (0.013)	0.042*** (0.008)	0.032*** (0.008)		0.037** (0.018)	0.031** (0.014)
Population			0.022 (0.015)	0.069** (0.029)	0.013 (0.015)	0.008** (0.003)
Share urban population log (Income)			0.007 (0.005)	0.011** (0.006)	0.005 (0.005)	-0.002*** (0.001)
Unemp. rate			0.000 (0.000)	0.000** (0.000)	0.000 (0.000)	0.000 (0.000)
Higher education			-0.018 (0.014)	-0.065** (0.031)	(0.000) (0.014)	-0.005 (0.003)
Electric vehicles				0.050** (0.022)		
Conv. vehicles				-0.018** (0.008)		
Constant	2.537*** (0.069)	2.581*** (0.019)	-5.711 (3.724)	-11.067** (5.248)	0.242 (2.909)	0.021* (0.011)
Year dummies	No	No	No	No	Yes	Yes
R ²	0.114	0.357	0.413	0.310	0.456	0.190
N	491	491	491	491	491	491

Note: The dependent variable is the natural logarithm of the urban toll ring price. The standard errors in parentheses are clustered at the municipality level. ***, **, * indicate statistical significance at the 1%, 5% and 10% levels, respectively.

62 municipalities with or adjacent to an urban toll ring in the sample. In all regressions, the standard errors are clustered at the municipal level, which is motivated by the concern that the error terms might be correlated within municipalities across years.⁸

In columns (1) and (2) in Table 3, I present a model that is estimated with

⁸The selection of the cluster level is not straightforward in this case. One could argue that standard errors should be clustered at the urban toll ring level, namely including all municipalities that commute through a selected toll ring, or at the county level. However, there are only 8 toll rings and 13 counties in the analysis, and clustering in few numbers is advised against (Cameron & Miller, 2015). An alternative is to cluster at lower levels, for instance municipalities, where a greater number of clusters are available. I explore the effects of alternative approaches to clustering standard errors in subsection 6.

OLS and fixed-effects, respectively. The difference between these two columns demonstrates a slight upward bias in the OLS estimates. The estimate from the fixed-effects model suggests that an increase in the share of electric vehicles of one percentage point will increase the toll ring price by approximately 4.2 percent. Thus, a toll charge of 20 NOK (2.35 USD) will increase to 20.84 NOK (2.45 USD). Controlling for population growth, increased urbanisation, income level, unemployment rate and education level in column (3) does not substantially effect the coefficient of interest.

To disentangle the effects of electric vehicles and conventional vehicles on the toll ring price, I estimate a model in which they enter as separate variables. The results are presented in column (4). Indeed, separating the variables suggests that having more electric vehicles increases the toll prices, whereas having more paying conventional vehicles has a negative effect on the toll price, keeping all else constant. More specifically, an increase of a thousand electric cars will increase the price by 0.46 percent, whereas an increase of a thousand conventional vehicles will decrease the price by 0.2 percent. This asymmetry suggests that toll prices are more readily increased in the case of lost income than decreased in the case of increased income. In column (5), I re-estimate the main model and add year dummies to control for national factors that change each year and may effect toll prices. The coefficient for the electric vehicle share increases somewhat compared to column (3) and is less precisely estimated. However, the estimate remains statistically significant at the five percent level.

To examine whether the coefficient of interest is sensitive to estimation method, I estimate the model using a first-difference approach. If the first differences estimator provides results that differ substantially from the estimate that was obtained from the fixed-effects model, this can indicate misspecification of the model, which, in turn, can produce inconsistent estimates. The result of the first difference estimation is presented in column (6). Virtually no change is observed in the estimated coefficient of the electric vehicle share.⁹

⁹In unreported estimates I also used a one-step Arellano-Bond estimator, with two lags on the dependent real price variable and the share of electric vehicles. There was no substantive change in either the significance or magnitude of the effect of electric vehicles on toll prices.

Table 4: IV estimation of the effect of the electric vehicle share on urban toll ring prices

IV estimates			
First stage:	Share electric vehicles	Standard error	F-statistic
ShareEV _{-it}	2.48***	(0.214)	134.49
Second stage:	Leave one area out	Distant municipalities	
	(1)	(2)	(3)
Share electric vehicles	0.028*** (0.007)	0.082*** (0.025)	0.029*** (0.007)
Year dummies	No	Yes	No
Area time trends	No	Yes	No
First stage F-statistic	134.49	13.92	160.61
R ²	0.410	0.693	0.412
N	491	491	491

Note: The dependent variable is the natural logarithm of the urban toll ring price. All regressions include controls for the population level, share of urban population, income level, unemployment rate and education level. The standard errors in parentheses are clustered at the municipality level. ***, **, * indicate statistical significance at the 1%, 5% and 10% levels, respectively.

The prior analysis suggests that a one percentage point higher electric vehicle share increases the price in urban toll rings by approximately 3.3 percent. However, a fundamental concern is that an increase in the toll price may itself lead to an increase in the share of electric vehicles. The instrumental variable strategy, which is outlined in Section 4, identifies the component of electric vehicle take-up that is due to overall reduced usage costs and improved technology, thereby circumventing the potential bias that is caused by reverse causality in the fixed-effects estimates.

Table 4 presents the estimates of the relationship between the electric vehicle share and the toll price obtained by using an instrument variable approach. In the top part of the table, the first stage estimates from the model in column (1) is presented. The coefficient suggests that the electric vehicle share increases by 2.5 percentage points when other municipalities have increases of one percentage point in their shares of electric vehicles. This implies that the increase in the overall electric vehicle share in Norway is slower than in cities with toll rings. Moreover, the coefficient is statistically significant at the one percent level. An F-statistic above 10 is obtained, and thus, following Staiger and Stock (1997) the instrument passes the standard thresholds for detecting weak instruments. The estimates in column (1) are the IV counterpart of the fully specified fixed-effects model in column (3) in Table 3. The coefficient is slightly reduced compared to the fixed-effects estimate, but remains statistically significant at the one percent level.

There is a concern that macroeconomic cycles and infrastructure development might affect both toll prices and electric vehicle adoption. Economic downturns might cause stagnant toll prices or affect electric vehicle adoption. In addition, improvements in the charging station infrastructure in an area might not only increase the electric vehicle share in that area, but also possibly lead to spill-over effects that increase the demand in other areas. If that increase of electric vehicles raises the toll price in the other areas, then the exclusion assumption for a valid instrument is violated. I approach this concern by estimating a model that includes both year fixed-effects and toll-ring-specific time trends. The results are presented in column (2). The coefficient of interest increases substantially compared to column (1) and is statistically significant at the one-percent level. The

instrument loses some explanatory power but still passes the standard threshold for a valid instrument. However, in controlling for the macroeconomic variables, I also take out the underlying factors that affect national electric vehicle take-up, which is a crucial deflator in the instrumental variable approach. Thus, knowing that the model is robust to accounting for broad macroeconomic effects, I will follow the structure in column (1) in the subsequent analysis.

An additional cause of concern is that a learning effect might occur between cities with urban toll rings. Imagine a shock that affects the number of electric vehicles in an area. Other areas can observe the consequences of this shock and, in turn, alter the prices in their toll rings to prepare for a similar shock. This learning effect, if present, can only occur between areas where an urban toll ring has been established. To determine the extent to which a learning effect is present, I construct a second instrument, which includes only the average share of electric vehicles in areas without urban toll rings. Small changes in the estimated coefficients compared to the results in column (1) in Table 4, suggests that a learning effect is not present in the data. Moreover, because this approach removes the effects of tolls on the whole, it serves as a robustness test of the initial instrument. The results that are obtained using the alternative instrument are presented in column (3). The effect of the electric vehicle share on the urban toll ring price is negligibly higher than its counterpart in column (1), and the effect is positive and statistically significant. This relaxes the concern of a learning effect being present in the main estimates. In conclusion, the instrument variable approach suggests that an increase in the share of electric vehicles of one percentage point will increase the prices in urban toll rings by approximately 3 percent.

6 Robustness

A common subject of concern in empirical analysis is the level of clustering of the standard errors. The objective of clustering is to allow model errors to be correlated within clusters (Cameron & Miller, 2015). In the main analysis, the clustering is at the municipal level. Since the price of the toll rings applies for several municipalities, one might argue that the urban toll ring area is the

suitable level of clustering. Column (1) in Table 5 presents the coefficients and standard errors when estimating with the main instrument variable approach with clusters on the toll-ring-area level.

Table 5: Robustness tests

	(1)	(2)
	Alternative cluster level	Group level regression
Share electric vehicles	0.029* (0.015)	0.031*** (0.008)
First stage F-statistic	44.34	
R ²	0.408	0.351
N	491	63

Note: The dependent variable is the natural logarithm of the urban toll ring price. The standard errors in parentheses are clustered at the urban toll ring level (column 1). ***, **, * indicate statistical significance at the 1%, 5% and 10% levels, respectively.

Compared with the baseline estimate, the coefficient of interest remains statistically significantly different from zero. Nevertheless, the standard errors have increased such that the effect is now only statistically significant at the 10 percent level. Clustering at this level may not be optimal for multiple reasons. First, the number of clusters in this case is reduced to the number of toll rings, and few clusters can lead to poor estimates of the standard errors (Cameron & Miller, 2015). Second, Abadie, Athey, Imbens, and Wooldridge (2017) argue that clustering at levels that are too aggregated can have adverse effects in the sense that the standard errors can be unnecessarily conservative. Clustering on ring level is based on geography, which traverses county borders and enfolds various types of municipalities in the same group.

Therefore, an alternative strategy for handling challenges that are related to within-group correlation is to estimate a regression using group averages instead of estimating on the municipal level. The normal standard errors that are obtained via grouped estimation are presumably more reliable than clustered standard errors in samples with few clusters (Angrist & Pischke, 2009). Consequently, I aggregate the data on the toll ring level by averaging over all municipalities that have or are adjacent to a selected toll ring. At this point, I have a panel that consists of one observation per eight toll ring regions per year, from 2010 to 2017. This low number of observations provide a poor basis

for instrumental variable estimation, since the estimated coefficient is likely to be biased in small samples (Bound, Jaeger, & Baker, 1995). At the same time, the results that were obtained in the fixed-effect analyses in Table 3 closely resemble the estimates that were obtained in the instrumental variable estimations in Table 4. Consequently, estimating a fixed-effects regression using group-level averages provides an test of stability across model specifications.

I estimate a fixed-effects regression on the group averages, and control for the average income, which is the only statistically significant control variable in the main analysis. The result is presented in column (2) in Table 5, and is similar to the estimates in the main analysis both in terms of magnitude and precision. This similarity provides robustness to the instrumental variable approach in the main model. Hence, in the remaining analysis the standard errors are clustered at the municipality level.¹⁰

7 Heterogeneity in toll price effects

7.1 City size differences

The effect of the electric vehicle share on toll ring prices most likely varies between cities and toll rings. Toll companies might have different budgetary constraints or project durations, and, therefore, react differently to a reduction in revenue. With sufficient statistical power, one could run a separate regression and identify a separate effect for every urban toll ring area. A second-best approach is a resampling method, which is often referred to as jackknifing, that involves estimating the benchmark model while excluding one urban toll ring at a time. This procedure constitutes a test on whether some toll rings are outliers that drive the general results.

Table 6 presents the results estimated with the jackknifing approach, where the first column indicates the city that was excluded from the regression. The effect of electric vehicle share is stable across all regressions, and they are in general similar in magnitude to the estimates obtained in the main results. At the same time, when omitting Oslo from the sample, as presented in the first row,

¹⁰Subsequent analyses have also been conducted with ring number clusters, without changing the results substantially.

Table 6: Jackknife analysis

Excluded area	Share electric vehicles	N	R ²
Oslo	0.021** (0.009)	379	0.363
Bergen	0.015*** (0.006)	403	0.353
Trondheim	0.021*** (0.006)	443	0.402
Stavanger	0.036*** (0.007)	435	0.473
Kristiansand	0.033*** (0.007)	443	0.434
Namsos	0.031*** (0.007)	435	0.421
Haugalandet	0.033*** (0.007)	443	0.483
Tønsberg	0.031*** (0.007)	456	0.434

Note: The dependent variable is the natural logarithm of the urban toll ring price. The standard errors in parentheses are clustered at the municipality level. ***, **, * indicate statistical significance at the 1%, 5% and 10% levels, respectively.

the effect of the electric vehicle share weakens and is less precisely estimated. This implies that the electric vehicle share in Oslo and its adjacent municipalities affects the associated toll ring price the most. The effect is also smaller in magnitude when Bergen or Trondheim is excluded from the regression. In summary, there seems to be a tendency towards somewhat smaller estimates when larger cities are excluded. This implies that the toll ring prices in larger cities have a propensity to react more strongly to an increased electric vehicle share. A possible explanation may be that larger cities have more extensive provision of public transport. Therefore, price increases can more easily be justified. Nevertheless, the pattern from the main model persists and all coefficients are statistically significant, which implies that no urban toll ring is solely responsible for the results in the main models.

7.2 Political heterogeneity

The main result suggests that when the share of toll-exempt vehicles increases, the toll charge increases. However, there is a possibility that raising the toll charge may be subject to the discretion of the municipal council. For example, some municipalities indeed respond to a loss in revenue by increasing the toll charge. Others might be more reluctant to increase tolls and instead seek funding elsewhere or drop planned projects. As discussed in subsection 3.2, price setting and price changes depend heavily on the incumbent parties in local governments. The left-wing parties in Norway are positive towards the use of toll charges to finance road construction. This includes the Labour party, which is the largest party in Norway.¹¹ The largest right-wing party, namely the Conservative Party, has a stated objective of reducing the share of road construction that is financed by toll road revenue, whereas the second largest right-wing and third largest party in Norway, the Progress Party, is strongly against toll rings.

¹¹An exception is the Red Electoral Alliance, which is the smallest and leftmost party, that argues that toll roads are inequitable.

Table 7: Analysis of whether the majority or mayor's affiliation affects urban toll ring pricing strategies.

	Left wing municipalities		Right wing municipalities	
	(1)	(2)	(3)	(4)
	Left majority	Left mayor	Right majority	Right mayor
Share electric vehicles	0.026*** (0.007)	0.034*** (0.008)	-0.007 (0.004)	-0.002 (0.005)
First stage F-statistic	9.16	12.04	118.98	90.35
R ²	0.547	0.522	0.317	0.338
N	80	166	337	325

Note: The dependent variable is the natural logarithm of the urban toll ring price. The regression is estimated using 2SLS and includes the same controls as in the baseline estimation (Table 4). The standard errors in parentheses are clustered at municipality level. ***, **, * indicate statistical significance at the 1%, 5% and 10% levels, respectively.

Through the course of the sample period, three election results are available (2007, 2011 and 2015). I re-estimate the main regression for urban toll ring cities according to which wing holds the majority and according to mayoral affiliation.¹² Table 7 examines how political affiliations are associated with toll pricing strategies.¹³ The estimates suggest that left-wing majorities react to increased electric vehicle share by increasing the toll price, whereas right-wing majorities or mayors do not. Overall, these results suggest that the wings react differently to revenue deficiency and that there is discretion for local municipalities in setting toll charges and financing road infrastructure. This suggests that right-wing parties balance their budgets in other ways, for example by reducing road infrastructure investments or financing road infrastructure investments in an alternative way.

8 Distributional effects

As a final point I consider the distributional consequences of the electric vehicle exemption policy. I approach this exercise by first presenting a back-of-the-envelope calculation of the counterfactual toll price if there were no exemption policy. Second, I explore how the ownership of electric and conventional vehicles varies by income group to assess whether some income groups receive a net benefit from the policy.

How much more is a conventional vehicle owner paying per urban toll ring passage as a consequence of the larger electric vehicle share? In 2017, the mean electric vehicle share in municipalities that are adjacent to a toll ring was 6.7 percent. The average toll ring price was 23 NOK (2.5 USD). The result that was obtained in the main analysis suggests the toll price increases by approximately 3 percent per percentage point increase in the electric vehicle share. Suppose

¹²It is likely that the wing that holds a majority also have the mayor. However, the mayor's political affiliation does not imply a majority for that wing. For instance, a wing that is close to obtaining a majority can collaborate with either a small party of the opposite wing or a party that is categorised as neither left- nor right-wing and bargain over the position of mayor.

¹³The left-wing parties are the Red Electoral Alliance, the Socialist Left Party, the Labour Party and the joint lists of left-wing parties. Right-wing parties include the Liberal Party, the Centre Party, the Christian Democrats, the Conservative Party, the Progress Party and the joint lists of the right-wing parties. The data on local government election are provided by Fiva, J. H., Halse, A.H., and Natvik, G. J., (2017): Local Government Dataset.

Table 8: Distributional effect and tax burden according to income groups

A: Electric vehicles	Income group		
	Below average	Average	Above average
Ownership share (%)	5	20	75
Value exempted tolls (million \$)	-4.4	-17.4	-65.3
B: Conventional vehicles	Below average	Average	Above average
Ownership share (%)	18	28	53
Value excess tolls (million \$)	15.7	24.4	46.1
C: Tax distribution	Below average	Average	Above average
Net cost (million \$)	11.3	7	-19.2

Note: The below average (<399,999 NOK), average (400-499,999 NOK) and above average (>500,000 NOK) income groups relate to commuters who travel by car daily.

instead that every electric car is a paying vehicle, all other things being equal. The average price of the toll rings would be 19.17 NOK (2.09 USD).¹⁴ This naïve calculation suggests that a conventional vehicle owner pays 3.3 NOK (0.36 USD) more per toll ring crossing today compared to the counterfactual scenario in which there is no exemption policy.

Next, I divide the ownership shares of the vehicle types by three income groups.¹⁵ By the end of 2017 there were approximately 95,000 electric vehicles in and around municipalities with toll rings, whereas 1.07 million cars were running on conventional fuel types. Assuming that all cars pass a toll ring every day for a year and using the mean toll ring price for 2017 (2.5 USD), the lost revenue that is attributable to electric cars is 87 million dollars.¹⁶ Dividing this loss by the number of conventional vehicles yields an increase of 0.2 dollars in excess payment in 2017.

Table 8 provides an overview of vehicle type ownership according to income

¹⁴The following formula is applied: $P_{EV} = (1 + \alpha \cdot ShareEV_1)P_C$, where P_{EV} and $ShareEV_1$ are the current toll ring price and electric vehicle share, respectively, α is the coefficient that was obtained in the main analysis and P_C is the counterfactual price.

¹⁵The statistics on ownership shares and income are derived from the National Travel Survey from 2013/14 (Institute of Transport Economics, 2016), and include subjects driving to work five times a week with cars as the main mode of transportation. The income statistic is only available in nine intervals.

¹⁶Although it is inaccurate to assume that car owners drive to work every day, it is likely that, when accounting for trips undertaken during leisure time, the average number of toll crossings over a year approach one per day.

group. The first row in section A suggests that the above-average income group owns the majority of electric vehicles, whereas the below-average and average income groups constitute 25 percent of electric vehicle owners. In the second row, I calculate the value of the exemption according to the ownership share in each income group. In section B, I calculate the value of the excess payment by the conventional car owner to bridge the toll company's revenue gap according to income group. In section C, the values of the exempted tolls and excess tolls by income group are summed to obtain the net cost of the policy according to income group. Table 8 reveals that individuals who earn less than average have a net cost of approximately 11 million dollars in 2017 due to the electric vehicle exemption policy. Furthermore, individuals who belong to the average income group have a net cost of 7 million dollars, whereas the above-average earners receive a net benefit of 19 million dollars.

This analysis identifies nontrivial differences in the net cost among income groups as a consequence of the exemption policy. The increased toll price is a burden that is shifted mostly onto low-income non-electric vehicle owners, thereby resulting in an inverse relationship between cost and income. This suggests that the policy is regressive and implies an unfavourable distributional effect. To correct the undesirable distributional aspects of the policy, the government can perform the appropriate transfers among income groups. This might translate into cash or in-kind transfers that benefit low income groups more than high income groups. A relevant example of such equalising policy is public transportation improvements, which will disproportionately benefit people with lower income.

9 Concluding remarks

The development of measures for addressing increasing traffic levels in urban areas has become progressively important for policy-makers. Several road pricing mechanisms have been implemented to restrict traffic growth. Charging motorists to enter city centres can reduce congestion, and, thus, reduce air pollution, improve road safety and serve as a mechanism for revenue collection. However, many schemes exempt less-polluting vehicles from charge to obtain a cleaner car

fleet, which might offset some of these effects.

This paper is the first to explore the effects of exempting selected vehicle types from charges. By analysing the prices in several urban toll rings in Norway over time, I find that an increase in the share of electric vehicles increases the toll charge. The result is robust to alterations in the sample and the estimation method, although the effect seems to be stronger in the largest cities. Finally, I find that whether an increase in electric vehicle share increases toll prices is subject to the discretion of the political wing that holds the majority in the toll ring municipality. A back-of-the-envelope calculation conducted in this paper suggests that a conventional vehicle owner pays 3.3 NOK (0.36 USD) more per toll ring crossing as a consequence of a higher vehicle share and the exemption policy.

An important aspect of assessing the consequences of electric vehicle exemption is the income level of electric vehicle owners. After analysing of the distributional effects of the exemption policy, I find that a substantial part of the cost is shifted onto the below-average income group. This implies that exempting electric vehicles from toll charges has a negative distributional effect. However, the total welfare effect of the exemption of electric vehicles from toll charges is uncertain. The number of electric vehicles most likely influences both congestion and pollution, along with public budgets and the durations of toll ring projects. Therefore, the net effects of electric vehicle exemption on society and heterogeneous groups should be explored in future research.

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Chapter 2

Bar Closing Hours and Traffic Accidents: Evidence from Municipal Variation in Norway

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Abstract

Driving while under the influence of alcohol is a leading cause of fatalities worldwide. This has led to a range of legislation and policy interventions, with bar opening hours being a particular focus. Existing evidence on the effect of bar opening hours on traffic accidents is mixed. We explore this issue studying traffic accidents in Norway over a ten year period. This setting is advantageous due to marked temporal and regional variation in closing times. We demonstrate an overall reduction in traffic accidents of longer opening hours. However, this pattern is sensitive to population density, with the pattern reversed (longer hours, more accidents) in less populated municipalities. Moreover, we provide evidence that liberalising bar closing hours decreases serious and fatal accidents, irrespective of municipal size. This suggests that for small municipalities, extending openings hours involves a trade-off between decreasing infrequent but serious accidents, at the potential cost of increased occurrence of more common, less serious, accidents.

JEL Classification: I18, R41

Keywords: Closing hours, Alcohol policy, Traffic accidents

*We are thankful for data provided by the Norwegian Public Roads Administration, the Norwegian Institute of Public Health, and the Central Mobile Police Service. We are also grateful to seminar participants at the Norwegian University of Science and Technology for helpful comments. **Corresponding author:** Lana Krehic, Department of Economics, Norwegian University of Science and Technology, Norway. *E-mail address:* lana.krehic@ntnu.no

1 Introduction

Driving while under the influence of alcohol remains a leading cause of fatalities and serious injuries worldwide. According to the World Health Organization (2007), approximately 20 percent of fatally injured drivers in high-income countries have a blood alcohol concentration above the legal limit. Furthermore, over half of fatal accidents in the US happen at night-time, where 54 percent of accidents are alcohol-related (Forbes, 2009). As a consequence, policies aiming to prevent drink-driving and reduce the risk of accidents remain a central part of the debate on appropriate alcohol policies.

This has led many jurisdictions to introduce restrictions that aim to reduce drink-driving through lowering the blood alcohol level permissible while driving, and to attach large penalties to breaches of these restrictions. At the same time, a broader range of legislation on alcohol consumption has the potential to substantially impact drink-driving while also affecting associated societal harms. One such area is on-premise licensing laws. Where and when individuals can purchase and consume alcohol, such as at a bar, restaurant or hotel, has a natural link with drink-driving.

While drink-driving laws have become stricter, many jurisdictions have become more liberal with respect to the opening hours of licensed premises. In practice, the relationship between opening hours, drink-driving, and traffic injuries is complicated by several factors. On the one hand, longer opening hours are linked to greater alcohol consumption with the associated heightened drink-driving risk. However, early closing times are often thought to result in so-called drinking 'against the clock'. This has the potential to lead to increased inebriation at closing time and heightened risk of traffic accidents. More generally, unified and early closing hours increase risks related to multiple drinkers driving at the same time (Levitt & Porter, 2001). Stringent closing hours may also make multiple-vehicle accidents more likely due to higher underlying traffic flows that naturally occur earlier at night. Yet, later hours may limit alternative modes of transport (public transport) or make it more expensive (taxis). In summary, the direction of effect between opening hours and traffic accidents is unclear and, as we discuss later, the existing evidence often reflects this point.

We study this issue focusing on Norway, which provides an interesting focal

point for a variety of reasons. On-premise opening hours are set at a highly disaggregated municipal level and vary considerably over time. This provides substantial variation in opening hours with changes in different directions and at different margins. At the same time, many other potential policy changes likely to confound estimates of the effect of on-premise hour do not vary at the municipal level. For example, in many settings, drink-driving penalties, drink-driving limits, and off-premise alcohol laws may be changed at the same time, or as a result of, on-premise law changes. This makes it difficult to disentangle the effect of changes in opening hours from other changes in alcohol related policies. In Norway, these policies are set nationally and simply do not vary in our period of analysis. Similarly, one response to changes in opening hours may be to change policing patterns. Again, in our case policing decisions are taken at a different, higher level, than bar opening hours and are unlikely to be varied with opening hour changes. Together, we argue that this provides a clean setting to isolate the effect of on-premise opening hours on individual behaviour. On top of this, there is marked regional variation in population density and accidents rates with commensurate differences in the density of off-premise venues and availability of public transport. We utilise these differences to understand likely mechanisms generating our results, which is important when discussing the implications of our results for other jurisdictions.

Municipalities in Norway are free to choose opening hours within quite broad nationally set limits, namely between midnight and 3am. Moreover, local parties frequently include changes in bar hours in their platforms. This leads to substantial variation both across municipality and time. Our main approach utilises this within municipal across time variation to estimate the effect of different opening hours on a range of accident types and injury outcomes. We use panel data on closing hours covering 2009 to 2018 for 424 municipalities in Norway, and combine it with detailed data on all reported night-time traffic accidents. We demonstrate an average zero effect that hides marked variation across jurisdictions. Most notably, longer hours in highly populated urban areas are associated with pronounced lower accident and injury rates. This fits with previous evidence from urban settings. In contrast, longer hours increase accidents in less populated areas. These results are robust to a range of likely confounding influences,

and remain in alternative data sources such as police DUI reports.

Combining the detailed data on accident outcomes with the marked regional variation allows for an extended investigation of heterogeneous effects of closing hour. We demonstrate that the effect is concentrated in the urban areas of a municipality, and has particularly large effects on the rate of multiple-car accidents. This has implications both for policing decisions, while also being relevant for -makers as two-car accidents involve higher average societal costs than single-car accidents. Furthermore, we utilise the advantage of having both extensions and restrictions of closing hour in our sample to show that these have asymmetric effects. Restrictions appear to have no effect on accidents, while liberalisations decrease accidents in populous, urban settings. Finally, we demonstrate important trade-off effects for less populated municipalities, where extending closing hours will increase more common, but less serious accidents, and decrease costly and infrequent accidents that involves serious injuries or fatalities. These effects suggest an important role for context in which opening hours are chosen and changed.

In what follows, we briefly review the literature on the consequences of alcohol access policies on traffic accidents. We then describe the institutional framework and outline our data. This is followed by a description of our empirical methodology, our main results, robustness checks and examination of heterogeneity in treatment effects. We then provide a conclusion.

2 Previous literature

There is a large literature on the effect of alcohol policies on traffic accidents. This covers a range of different policies and can be summarised as demonstrating mixed results. For example, Saffer (1997) finds that a ban on broadcast advertising on beer and wine reduced the number of fatal traffic accidents in the US. While, Ruhm (1996) demonstrates that many alcohol-control policies have, at best, a limited effect on the number of traffic fatalities. He shows that increasing beer taxes and raising the legal drinking age are the two strategies that reduce vehicle fatalities. Dee (1999), however, finds no relationship between beer taxes and youth traffic fatalities, but again demonstrates substantial reductions

in traffic fatalities when the minimum legal drinking age is higher. Carpenter and Dobkin (2009) use regression discontinuity approaches to demonstrate an increase in traffic fatalities at the legal drinking age limit of 21. On the other hand, Lindo, Siminski, and Yerokhin (2016) use a similar approach and demonstrate no effect of minimum legal drinking age in Australia on traffic accidents and fatalities, despite it having a large effect on alcohol consumption. Related to this, Lovenheim and Slemrod (2010) emphasise the importance of standard minimum legal drinking age across all states, after their results revealed that unequal policies across bordering counties can lead to increased number of teenager-involved fatalities. Hansen (2015) shows that more stringent punishments to breaches of DUI regulations reduce recidivism of drink-drivers.

Other research examines off and on-premise alcohol availability. Baughman, Conlin, Dickert-Conlin, and Pepper (2001) investigate local alcohol policy changes in Texas counties, and find that switching from a dry county to allowing for off-premise sale of beer and wine reduced the number of alcohol-related accidents, whereas allowing both off- and on-premise sales of all liquor types had no statistically significant effect on the number of accidents. Cotti and Walker (2010) explore how the emergence of casinos in the U.S. affected alcohol-related fatal traffic accidents. The opening of a casino increased the number of fatal traffic accidents, however, the relationship was negatively related to county population size. The authors argue that patrons have to drive longer distances to rural casinos, which in turn increase accidents. The opposite is true for urban casinos, where patrons are likely to reside close-by and have access to public transport.

Most closely related to our paper is research exploring the effect of changes of on-premise alcohol serving and bar closing hours. Often these papers have examined individual events of licensing changes. For example, Vingilis et al. (2005) investigate the effect on road safety of an extension in on-premise alcohol sales from 1 to 2am in Ontario, Canada. They find no impact on traffic fatalities with positive blood alcohol concentration after the extension. Green, Heywood, and Navarro (2014) explore the effects of a large liberalisation in bar closing hours that occurred simultaneously across all of England and Wales. They demonstrate marked reductions in traffic accidents and injuries. At the same time, Biderman, De Mello, and Schneider (2010) examine the effect of a restriction of bar closing

hours in the São Paulo Metropolitan Area, and demonstrate a reduction in fatal traffic accidents. Together, this highlights the mixed evidence of the effect of closing hours on traffic accidents.

Our paper adds to this literature in a number of ways. First, unlike previous research, we examine changes in opening hours that are spread over a country and across a substantial period of time. The latter in particular, we argue, reduces concerns that our results reflect other variations that change simultaneously with closing hours. At the time, these changes occur across a variety of opening times, and include both extensions and restrictions in opening hours. In addition, Norway has a national alcohol policy regarding off-premise alcohol sales that remains unchanged across the period. Furthermore, the restrictiveness of off-premise sales in Norway prevents direct substitution between on and off premise drinking, for example around bar closing hours. In particular, beer (up to 4.7 percent) cannot be purchased off-premise after 8pm on weekdays, 6pm on Saturdays and not at all on Sundays. Other stronger alcohol can only be purchased from the government run monopoly which exists in few locations (for example, Trondheim with 200,000 inhabitants has eight of these shops) and with very limited opening hours (weekdays til 6pm, 10am-4pm on Saturdays and closed on Sundays). This makes it more difficult to shift to off-premise drinking at bar closing time.

3 Institutional framework and the data

According to Norwegian law, on-premise alcohol sales are permitted between 8am to 3am for beverages with an alcohol content up to 22 percent, such as beer and wine. Spirits containing between 23 and 60 percent alcohol can be served between 1pm and 3am.¹ Municipalities are free to decide serving regulations within these hours, and serving hours can differ between beer and wine, and hard liquor, in the same municipality. Serving hours can also differ between weekdays and weekends. Our main approach, unless stated otherwise, is to use weekend closing hours for hard liquor. We stress, however, that our results are unchanged if we, instead, use the beer and wine serving hours.

¹Patrons can stay up to 30 minutes after alcohol sales time in order to finish their drinks.

Changes to on-premise closing hours in Norwegian municipalities are frequent and often subject to political debate and media attention (Rossow & Baklien, 2014). In addition, changes in closing hours often take place shortly after a new council has been elected, suggesting that alcohol policy often is a part of local election campaigns. In general, the reasoning behind the decision to alter closing hours are multi-factorial. The public debate is often divided between public health and industry interests. Proponents of liberal closing hours argue that it contributes to the liveliness of cities and increases revenue for hotels, restaurants and bars. Opponents argue that earlier closing hours decrease street violence and a range of other social externalities including drink-driving. Support for both of these arguments can be found in the small, associative, literature that exists in Norway. For instance, Melberg and Schøyen (2012) find that a reduction in alcohol serving time by one hour is associated with higher revenue of between 9 and 12 percent. On the other hand, Rossow and Norström (2012) find that a one hour extension in closing hours is associated with an increase in assaults by 13 percent.

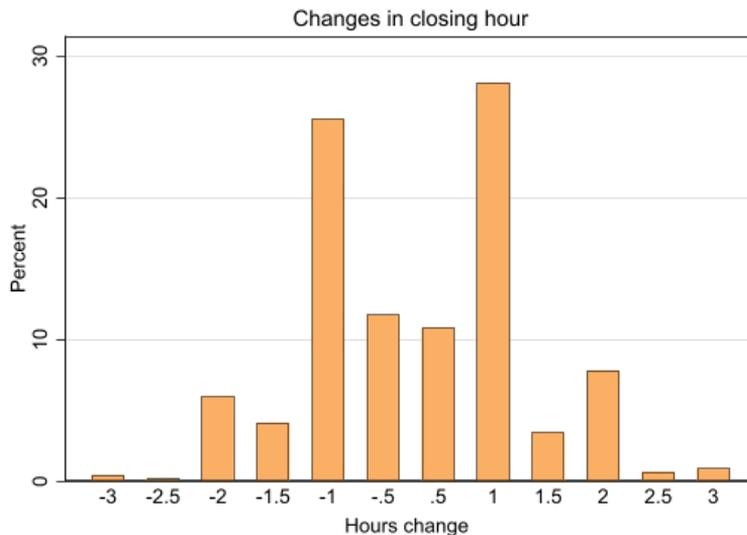


Figure 1: An overview of the direction of closing hours change in the period between 2009 and 2018. *Source: Norwegian Institute of Public Health.*

Data on closing hours come from local councils' response to the Alcohol Act

survey, conducted by the Norwegian Institute of Public Health. The questionnaire is sent to all Norwegian municipalities every year, with a very high response rate of between 93 to 99 percent. We use the information on closing hours from 2009, the earliest year for which information on municipal closing hour is available, to 2018. During this period there were 434 individual municipal changes in on-premise serving hours. This is equivalent to each municipality changing their closing hours, on average, one time over the ten year period. Figure 1 shows the distribution of changes in closing hours in terms of size and direction. These changes are quite evenly split between reductions and extensions. Around 55 percent of changes involve increases or decreases of on-premise sales by one hour. 22 percent constitute 30 minute changes, and there is a non-trivial amount of changes of more than one hour in terms of both extensions and restrictions. This means that we can meaningfully provide estimates and explore heterogeneous outcomes of the effects of opening hours on traffic accidents that come from a range of changes both in direction and size.

Figure 2 provides geographic information on these changes. The upper panel displays municipalities that at some point during the sample period extended on-premise serving hours.² In the lower panel we plot municipalities that have restricted their closing hours. Two points can be made from these illustrations. First, municipalities that liberalised or restricted hours are spread across the country. This reduce concerns that changes are geographically clustered in some manner, for instance around major cities, in areas with strong religious preferences, or in areas with a strong brewing industry. Second, and although more difficult to see graphically, a non-trivial number of municipalities have both extended and restricted closing hours during the period. This has some implications for the examining of heterogeneous effects, which will be discussed later.

Our road accident data come from the The Norwegian Public Roads Administration (NPRA) and contain all motor vehicle accidents reported to the police from 2009 to 2018 for all 429 municipalities. We have information on the date and time of accident, severity, road speed limit and accident area. This data allows us to match accident location to the corresponding municipality's closing hours. Furthermore, we match the accidents and closing hour data to population

²The municipal structure in Norway has changed during the last decade. Municipalities that have merged are challenging to map, and are therefore marked as missing.

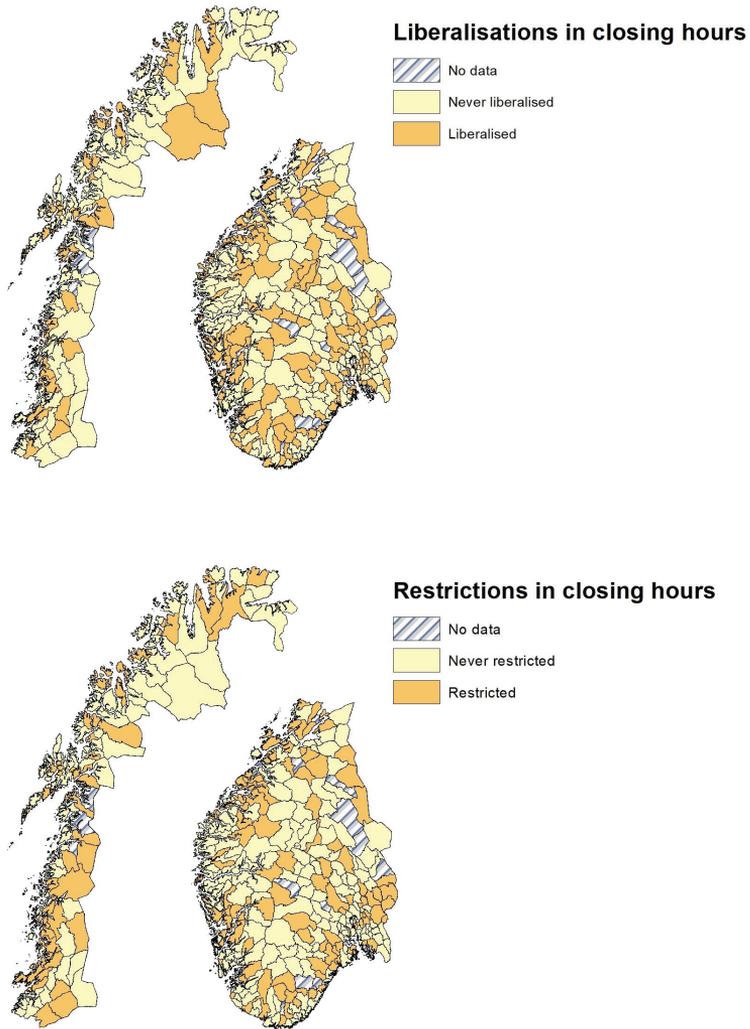


Figure 2: Geographical distribution of municipalities that ever liberalised and restricted their closing hours. *Source: Norwegian Institute of Public Health.*

level from Statistics Norway. We also collect data on the number of individuals between the age of 18 and 25 and construct a variable of the share of young adults in the municipality. This reflects that young drivers are both much more likely to be involved in a traffic accident than other sober drivers, and also may be more affected by changes in on-premise closing hours.

Table 1: Descriptive statistics by closing hours status (2009-2018)

	(1)	(2)	(3)
Variable	All	Unchanged	Changed
Municipalities	423	204	219
Accidents	0.82 (2.60)	0.96 (3.37)	0.68 (1.58)
Closing hour (beer/wine)	2.02 (0.60)	2.05 (0.57)	1.98 (0.63)
Closing hour (spirits)	1.73 (0.82)	1.78 (0.79)	1.67 (0.86)
Population	11,943 (36,561)	14,553 (48,031)	9,508 (20,495)
Young adults	1,247 (4,056)	1,540 (5,287)	972 (2,362)

Note: The variable *Accidents* is the number of traffic accidents occurring weekends between 10pm and 5am in a municipality over the course of one year. Standard deviations in parentheses.

Systematic differences between those municipalities that did and those did not change closing hours can lead to biased estimates. Table 1 displays summary statistics of the variables included in our analysis, divided by whether or not municipalities have changed their closing hours. Approximately half of the municipalities have not altered closing hours during the sample period, whereas the other and slightly larger half have. Overall, the two groups are similar. No change municipalities have slightly more accidents and have somewhat more liberal closing hours. It is worth noting that population size is fairly bigger for municipalities that do not change closing hours. Population is a slow changing variable, so controlling for population and including municipality fixed-effects should account for potential confounding factors that might bias the estimates of the effects of closing hour.

Figure 3 illustrates variation in the timing of accidents according to when municipal on-premise alcohol serving stops. The figure demonstrates that, irre-

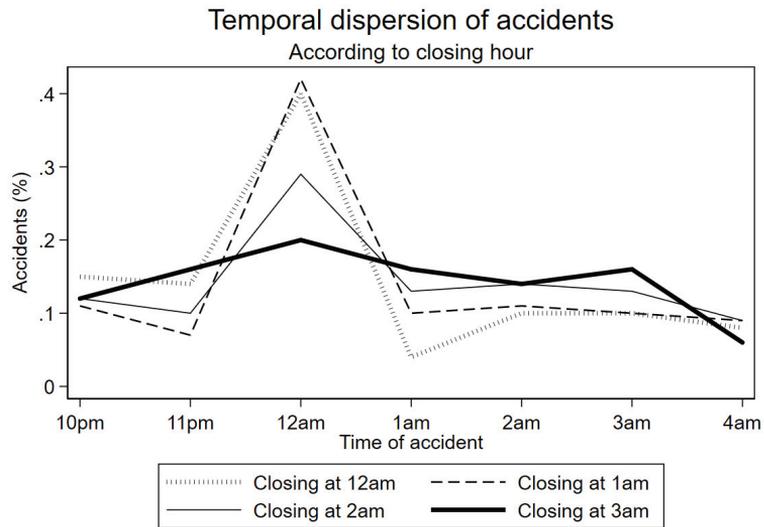


Figure 3: An overview of the temporal dispersion in average yearly municipal accidents rate according to closing hours. Note: "Closing at 12am" includes municipalities that stop serving alcohol at 12:30 am. The same is true for closing at 1 and 2 am.

spective of closing hour, there is a peak of accidents at midnight. More precisely, of all accidents happening weekends between 10pm and 4am, 20 to 40 percent happen around midnight. The peak may reflect an aspect of Norwegian drinking culture where people spend the first part of an evening at private parties, then commute to pubs and bars around midnight. In addition, there is an inverse relationship between hour at night and underlying traffic flow that might be the reason for higher accidents numbers earlier at night. In general, the plot illustrates that the later the closing hour, the more temporally dispersed are the accidents. We now move on to study how closing hour affects the number of traffic accidents.

4 Methodology

Our main approach is to estimate variants of:

$$Acc_{it} = \alpha_i + \beta_1 ClosingHour_{it} + \beta_2 ClosingHour_{it} \cdot Population_{it} + \gamma X_{it} + \delta T_t + \varepsilon_{it} \quad (1)$$

where Acc_{it} is the number of weekend accidents happening between 10pm and 5am in municipality i in year t . $ClosingHour$ is the maximum allowed on-premise alcohol serving hour during weekends in municipality i in year t , ranging from midnight to 3am. Vector X includes the population level and the number of young adults (aged 18 to 25) in municipality i in year t . α_i captures municipal fixed effects, T_t is a set of year dummies, and ε_{it} is random idiosyncratic error. Hence, our estimate of interest β_1 is identified by within municipality variation in opening hours holding constant nationwide annual patterns in accidents. In extensions, and as explained further, we allow this estimate to vary by population size of municipality by including $ClosingHour_{it} \cdot Population_{it}$, the interaction term between closing hours and population.

Our choice of municipal fixed effects approach is motivated by a range of concerns regarding cross municipal variation in time-invariant factors influencing alcohol consumption, bar opening hours, and underlying risk of traffic accidents. For instance, there is variation across Norway in the strength of religious attitudes, which simultaneously influences factors such as drinking culture, opening

hours, and night time activities. Some of these areas are in locations where the difficulty of driving, and the risk of accident, is higher. This leads to a concern that OLS estimation will overstate the effect of bar closing hours on traffic accidents. Alternatively, some municipalities in rural areas might have a strong drinking culture with liberal bar closing hours, but also a higher accident prevalence because of lesser provision of night-time public transport services. Again this may lead to OLS estimates being upwardly biased.

A further concern, not mitigated by this approach, is the potential for time-varying factors correlated with both closing hours and traffic accidents. For example, nation-wide shocks, caused by government awareness campaigns or increased taxation on alcohol, could reduce closing hours and the number of traffic accidents in several municipalities within the same year. We include year fixed effects to capture such influences that are common for all municipalities. Municipal specific time varying factors are more difficult to address, and it is unclear in what direction these may bias our results. For example, restrictions of hours and the election of municipal governments that favour restrictions may gain more traction in instances where there have been increases in underlying problems related to alcohol consumption in the local area. At the same time, increases in local economic activity may increase the number of licensed venues, and increase pressure on municipal governments to extend hours. We adopt a number of approaches aimed at assessing the sensitivity of our main estimates to these types of factors. These range from including municipal specific time trends to estimating disaggregated models for liberalisations and restrictions. While these tests do not directly address time-varying unobservables, they provide some gauge of the sensitivity of our main estimates.

Finally, we suspect that any effect of bar closing hours is heavily dependent on municipal population size. For example, more populous municipalities are likely to have higher concentrations of bars, and relatively more people enjoying night-time activities. Changing alcohol serving hours in more densely populated municipalities could be viewed as a more intense treatment. At the same time, they have substantially greater public transport availability, especially late at night. To model this heterogeneity, our initial approach is to simply include an interaction term of bar closing hours and population in our main model.

5 Results

Column (1) in Table 2 provides initial estimates of the relationship between closing hours and night-time traffic accidents. This and all regressions henceforth are estimated with municipal fixed-effects, year dummies and standard errors that are clustered at the municipal level. The result suggest a negative, however small and not statistically significant, relationship between longer opening hours and accidents. However, this estimate provides the linear effect of increasing opening hours by one, and may hide marked non-linearities across actual closing time. To examine this, we additionally estimated an analogue of (1) where we replaced closing hours with a series of dummy variables to indicate closing hour, where midnight is the omitted category. These estimates are summarised in Figure 4. They suggest no difference between closing times of midnight, 1am or 2am, but some suggestion of lower accidents at the latest closing time. However, none of these coefficients are statistically significant at standard levels, and the estimate for 3am closing is particularly imprecise.

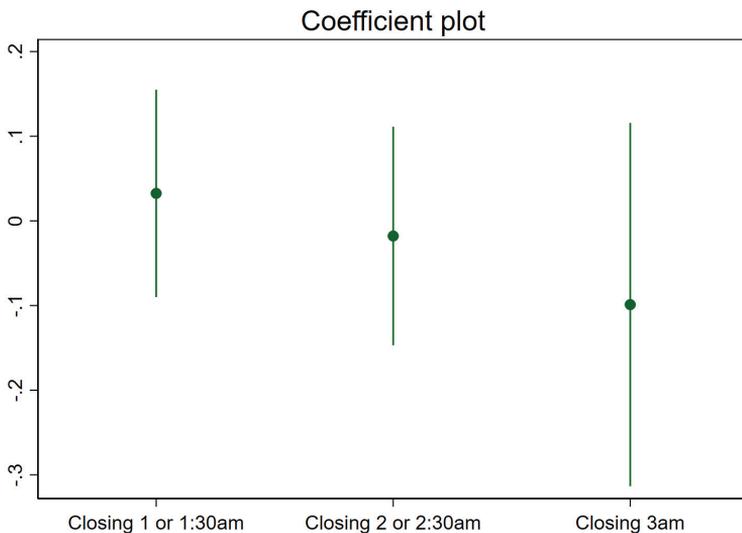


Figure 4: Estimated impact of different closing hours on traffic accidents.

As discussed earlier, it is likely that any relationship between closing hours and accidents may vary according to population levels and the number of young

Table 2: The influence of bar closing hours on traffic accidents between Friday and Sunday (2009-2018)

	Hard liquor			Beer and wine
	(1) FE Accidents	(2) FE Accidents	(3) FE Accidents	(4) FE Accidents
Closing hour	-0.004 (0.033)	0.382*** (0.071)	0.137*** (0.046)	0.128*** (0.043)
Closing hour × population		-0.536*** (0.091)	-0.187*** (0.064)	-0.236*** (0.052)
Population (/10,000)			-3.496*** (0.510)	-3.397*** (0.439)
Number of young adults (/10,000)			13.195* (7.150)	13.552** (6.872)
Constant	1.123*** (0.090)	1.795*** (0.177)	3.890*** (0.361)	3.878*** (0.393)
Year dummies	Yes	Yes	Yes	Yes
R ²	0.019	0.116	0.167	0.169
Observations	4039	4039	4039	4039
Municipalities	423	423	423	423

Note: The dependent variable is the number of traffic accidents occurring weekends between 10pm and 5am. ***, **, * indicate statistical significance at 1%, 5% and 10%, respectively.

adults in a given municipality. As a first step to investigating this, we allow the effect of opening hours to vary by the population size of the municipality. In column (2), we present the results where we include an interaction between municipal population and bar closing hours. This dramatically changes the estimate of interest. The initial effect of liberalising bar closing hours is positive and statistically significant at the one percent level. The coefficient implies an increase in 0.4 accidents when extending bar closing hours by one hour. The interaction term between bar closing hours and population is negative, statistically significant, and also sizeable. This positive effect of bar closing hours decreases as population size increases, with the effect on average being zero at approximately 7,100 inhabitants and turning negative beyond this point. This provides an initial suggestion of marked differences in the effect of closing hours by municipal setting. For example, for the most populous municipality in Norway (Oslo), the results suggest approximately 37 accidents less per year after liberalisation,

or three night-time accidents less per month.³ At the same time, there are 230 municipalities in Norway with less than 5,000 inhabitants. Extending opening hours would increase accidents with 30 percent for these municipalities, adding up to 24 accidents for all municipalities. We next add population level and the number of young adults to the model, as these are variables that are likely to have an effect on the number of night-time traffic accidents. The result is presented in column (3), which is equivalent to equation 1. The magnitude of both closing hours and the interaction term is reduced, but both direction and statistical significance are unaltered.

Municipalities have the ability to differentiate between serving hours for spirits, and for beer and wine, allowing beverages with lower alcohol levels to be sold later than hard liquor. The correlation between the two closing hours is 73 percent. For simplicity we have used hard liquor opening hours up to this point. In column (4), we present estimates where we use the applicable serving hours for beverages with a lower alcohol content. The key estimates of interest are essentially unchanged by this. As a result, we focus solely on the hard liquor hours from this point on but stress that all following estimates are largely unchanged if we use these alternative hours.

³Oslo has approximately 674,000 inhabitants as of 1st of January, 2018.

Table 3: Alternative specifications of the effect of closing hours on traffic accidents

	Logs (1)	Weighted (2)	County cluster (3)	Trends (4)	Fixed population (5)
Closing hour	1.92** (0.889)	0.722*** (0.186)	0.137*** (0.045)	0.101** (0.040)	0.162*** (0.050)
Closing hour × population	-0.225** (0.110)	-0.469*** (0.107)	-0.187** (0.075)	-0.178*** (0.053)	-0.231*** (0.075)
Weighted	No	Yes	No	No	No
County cluster	No	No	Yes	No	No
Municipal trend	No	No	No	Yes	No
R ²	0.029	0.424	0.167	0.324	0.168
Observations	4039	4039	4032	4039	4039
Municipalities	423	423	423	423	423

Note: All regressions are estimated with fixed effects and include controls for population, number of young adults and year effects. ***, **, * indicate statistical significance at 1%, 5% and 10%, respectively.

6 Robustness

6.1 Alternative specifications

We next test the sensitivity of these results to alternative specifications of the main model. In column (1) in Table 3, we modify all variables, with the exception of closing hours, to be expressed in natural logarithms and replace the zeros in the dependent variable with 0.001. The result supports the findings from the main model, suggesting that an increase in closing hours increases the number of traffic accidents, but the effect decreases with population size. Next, we re-estimate our main model using population levels as weights.⁴ Comparing unweighted and weighted regressions is a useful diagnostic for model misspecification, as suggested by Solon, Haider, and Wooldridge (2015). The coefficients of interest in column (2) increase somewhat compared to the main model, but the sign and significance of the coefficients persist.

In column (3), we allow standard errors to be correlated within counties and cluster at that level. The standard errors are near to unaltered, suggesting that within-county correlation between clusters is not cause for concern in the main estimates. There might be some unobserved trending factors that affect both closing hours and the number of traffic accidents, such as attitudes towards alcohol or local economic factors. Failing to control for these may lead to biased and inconsistent estimates of the effect of closing hours on traffic accidents. To examine this, we estimate our main model including municipal time trends. The coefficients presented in column (4) are marginally smaller in magnitude compared to the main result, yet both estimates are sharply estimated. An alternative approach is to include county trends. We do not display the results in this paper, however doing this produces somewhat larger and more precise estimates compared to the main model, but does not change the interpretation of the results.

A final test of robustness is related to our interaction of closing hour and population. As both variables vary over time it may be that some of the effect of the interaction term derives from variation in population levels instead of changes

⁴We use population levels from the first year in the sample in order not to place more weight on the latter years because of population growth.

in closing hour. We explore this by fixing population levels in the interaction term at the first year of observation, so that only closing hour changes over time. The results of this approach, displayed in column (5), replicates the coefficients in the main results both in terms of magnitude and statistical significance.

6.2 Examining threats to identification

In this section we provide further checks aimed at examining the robustness and threats to our identification strategy. One concern is that people living in rural areas may be more affected by closing hours in nearby cities than in their own municipality. This would mean that our estimates of closing hour effects are biased towards zero, as inhabitants do not react to changes in bar closing hours in their municipality of residence. At the same time, an alteration in closing hours that increases patronage from neighbouring municipalities might lead to more accidents in the neighbouring city. If this is the case, the estimate of the interaction term between closing hours and population will be biased upwards. To explore this we aggregate the data to the economic regions level and assign the closing hours of the biggest city to all municipalities in the region.⁵ The result of this exercise is presented in column (1). Neither the coefficient of the initial effect nor the interaction term is statistically significant, suggesting that big city changes in closing hour is not a cause for concern in our main results.

An additional examination entails placebo testing where we estimate our main model only on accidents during rush hours between Monday and Thursday.⁶ The idea is that accidents in these times should not be influenced by local closing hours. If the main results are causal, one would expect weekend on-premise alcohol serving hours to have no explanatory power with respect to accidents taking place on weekdays. The result of this placebo exercise is presented in column (2). Neither the estimated coefficient for closing hours nor the interaction between closing hours and population are significantly different from zero. This provides supportive evidence again that on-premise closing hours have a causal

⁵The classification of economic regions follows the structure of proposed by Statistics Norway, which is based on labour and commodity market flows, and population levels.

⁶The reason we do not include Fridays is its proximity to the weekend. Changes in weekend closing hours could lead to changes in travel patterns during rush hour on Fridays, such as leaving at a different time than the rest of the week.

Table 4: Examination of threats to identification

	Economic regions (1)	Rush hour accidents (2)	DUI (3)	DUI (weighted) (4)
Closing hour	0.151 (0.258)	0.164 (0.137)	0.605*** (0.190)	1.944*** (0.462)
Closing hour × population	-0.061 (0.057)	-0.290 (0.215)	-0.915*** (0.263)	-1.322*** (0.232)
Weighted	No	No	No	Yes
R ²	0.206	0.263	0.084	0.320
Observations	890	4040	4036	4036
Municipalities	89	423	424	424

Note: All regressions are estimated with fixed effects and include controls for population, number of young adults and year effects. ***, **, * indicate statistical significance at 1%, 5% and 10%, respectively.

effect on night-time traffic accidents.

A feature of the accidents data is that we do not observe whether the driver was impaired and whether or not their blood alcohol concentration level was above the legal limit.⁷ This means that the effect of closing hours on the number of accidents may reflect drinking behaviour, or potentially other factors such as driver fatigue. To explore this further, we utilise police report data regarding motorists driving under the influence.⁸ There are however two challenges to using the police report data. First, some charges are dropped if later testing (typically at a hospital) clears the driver of wrong doing, yet these reports will remain in our data. Second, we cannot differentiate whether the driver was under the influence of alcohol or drugs, or both. Nonetheless, using this data provides additional information on the likely channels of the effect that we observe of closing hours on traffic accidents. We re-estimate our main models with our dependent variable being the number of night-time DUI reports. Column (3) in Table 4 presents these estimates. The estimated relationship between closing hours and DUI reports follows that for accidents. Longer hours lead to more DUI reports, but

⁷The legal blood alcohol concentration level is 0.02 in Norway.

⁸An incident can be reported to the police in three different circumstances. First, if a traffic accident happens, police will report the driver if they suspect that they were under the influence at the time of the accident. Second, police may stop and report a driver if the car is observed driving in a suspicious way. Third, a motorist is reported if an alcohol blood level concentration above the legal limit is revealed after roadside breath-testing.

this effect decreases and becomes negative in more populous municipalities. The turning point of this relationship, approximately 6,600 inhabitants, is very similar to that for our main models. This further suggests that there is substantial heterogeneity between the effect of opening hours in large urban municipalities, and smaller rural municipalities. At the extreme, these results imply a decrease of 61 police reports per year in Oslo when closing hours are extended by one hour.

A further concern with these estimates is the potential for scale effects in the measurement of DUI across municipalities of differing size. For example, a roadside breath-test on 50 motorists in a less populated municipality, compared to a large one, will lead to a oversampling of drivers in the small municipality. We follow the approach suggested by Solon et al. (2015) and estimate a weighted regression with robust standard errors in order to obtain consistent coefficients. The results are presented in column (4). Once again, the direction of the estimates are the same as the results obtained in the main model. The coefficients are larger and statistically significant at the one percent level. These results suggest that a likely channel of the effect of closing hours on night-time traffic accidents works through changes in people driving under the influence.

7 Heterogeneity

7.1 Heterogeneous treatment effects

We further explore potential heterogeneity in treatment effects. This is potentially important for policy reasons, as changes in opening hours may change drink-driving risks differently and alter, for example, optimal policing responses. It is also informative regarding the underlying mechanisms that generates our results. Our data record information on the location and speed limit of the road where the accident happened. We exploit this information in columns (1) and (2) in Table 5, and differentiate between accidents occurring on urban roads where the maximum speed limit is up to, and over, 50 kilometres per hour, respectively. The results indicate that extending closing hours increases the number of accidents on urban roads for municipalities of a smaller size. On the other hand, the average municipality experiences a reduced number of accidents in urban areas

Table 5: Differences in treatment effect of changes in on-premise alcohol serving hours

	Urban roads		Persons		Injuries	
	Up to 50 km/h (1)	Over 50 km/h (2)	One (3)	Two or more (4)	No or minor (5)	Serious or fatal (6)
Closing hour	0.102*** (0.039)	0.035 (0.027)	0.071** (0.034)	0.066*** (0.021)	0.117*** (0.039)	0.020 (0.016)
Closing hour × pop.	-0.124** (0.059)	-0.064* (0.036)	-0.095** (0.045)	-0.093*** (0.030)	-0.155*** (0.056)	-0.033** (0.016)
Mean	0.35	0.47	0.54	0.28	0.64	0.18
R ²	0.200	0.041	0.043	0.224	0.197	0.014
Obs.	4035	4035	4035	4035	4035	4035
Muns.	423	423	423	423	423	423

Note: The dependent variable is the number of traffic accidents occurring weekends between 10pm and 5am. All regressions are estimated with fixed effects and include controls for population, number of young adults and year effects. ***, **, * indicate statistical significance at 1%, 5% and 10%, respectively.

when closing hours are extended. The effects of closing hours is notably weaker for accidents on higher-speed roads.

An additional and related question is how many cars or people are involved in an accident. This is motivated by the findings of Levitt and Porter (2001) that drivers with alcohol in their blood are seven times more likely to be involved in a fatal car crash. One-car accidents have societal costs, yet, two-car accidents involve an additional externality where drinking drivers may injure others. In our data, we are able to separate between single-car accidents and accidents involving one car colliding with another car, a cyclist or a pedestrian.⁹ The results are presented in columns (3) and (4), and suggest that changing closing hours have the same effect on one and several people accidents. In terms of percentage point effects, the magnitude on both the initial effect and the interaction term are essentially the same. However, if one considers the large differences in the underlying frequency of these two events, this implies much larger effects of opening hours on multiple people accidents. Provided that changes in closing hour mostly affect accidents on urban roads where road user density is higher,

⁹The last category will hereby be referred to as accidents involving several people.

which is the result obtained in the first two columns, it is likely that the reduction in accidents is stronger for accidents involving several people. It also suggests that extended closing hours allows patrons to disperse at more diverse times. This reduces the number of people leaving bars simultaneously with a resultant decline in accidents.

An important remaining question is how altering bar closing hours affects the severity of traffic accidents. More specifically, changing closing hours may affect the number of serious traffic accidents, which are subject to higher social costs than accidents where no one is hurt. Consequently, the treatment effect on accident severity may be of greater policy interest. Additionally, there is likely less measurement error of more serious accidents. Columns (5) and (6) in Table 5 presents the results when we split the number of accidents by the degree of severity.¹⁰ The effect of extending closing hours on no and minor injury accidents reflects the evidence found in the main results. Moving on to column (6), once again direction of the coefficients mimics the results seen throughout the paper. Nevertheless, the initial effect of closing hours is not statistically significant. This result suggests that extending closing hours will lead to fewer accidents with serious or fatal injuries, irrespective of city size. Yet, the negative effect is growing with population size. On the basis of the mean number of accidents and the sample mean population size, liberalising closing hours will reduce the number of accidents with minor injuries by 12 percent, whereas the number of serious or fatal injuries will fall by 25 percent. Consequently, the findings in the two latter columns imply a trade-off for the smallest municipalities, in that increasing closing hours will increase accidents with no and minor injuries, whereas it may decrease serious and fatal accidents. The magnitude of the increase in less serious accidents is smaller, and the social cost of traffic accidents of a more serious manner is larger. In contrast, less serious accidents are more common, making it a question of assigning priorities for policy-makers.

The monetary consequences associated with altering bar closing hours and the effect it has on traffic accidents can be roughly calculated from the societal value of avoiding traffic accidents. This value encompasses both direct costs such

¹⁰In our data, minor injuries are scratches and fractures. Serious injuries include injuries that require hospital admissions, with the potential of being permanently injured.

as production loss, medical and material expenses, and indirect costs including pain, grief, reduced health or reduced years of life (The Norwegian Public Roads Administration, 2018). A statistical life's value is calculated to be approximately \$3,408,239 (in 2019 US Dollars), whereas the value of avoiding an accident involving a serious injury is \$1,263,983. The value of preventing accidents involving a minor injury or material damages is \$82,384 and \$4,288, respectively. Our results indicate that extending closing hours for an averagely populated municipality will save \$2,990 worth of damages for accidents involving a minor injury or material damages. For serious and fatal accidents, the effects of extending closing hours will avoid accidents corresponding to a value of \$93,444. In summary, a one hour extension aggregates to \$96,434 in avoided costs per year for a municipality with the average of 12,000 inhabitants. Assuming that this effect is applicable for all Norwegian municipalities, the value of avoided accidents is \$40,8 million per year.

Nevertheless, our analyses show that there is a heterogeneous effect of closing hours depending on population size. For a municipality of a smaller size, extending closing hours yields a trade-off between an increase of less serious accidents, which are more common, and a decrease in rare but serious accidents. For example, for municipalities with 5,000 inhabitants, liberalising closing hours will lead to an increase in less serious accidents corresponding to a cost of \$1,733 per year. On the other hand, liberalisation will decrease serious and fatal accidents by a value of \$46,722 per year. To sum up, the value of avoiding traffic accidents for less populated municipalities by changing bar closing hours appear to speak in favour for liberalisation. At the same time, there are a number of other costs related to increased bar closing hours that is not included in this rough calculation, such as increased policing and possibly increased associated societal harms, which may alter the net welfare benefit of extended closing hours.

We further explore treatment intensity by using the number of liquor licenses in each municipality. In addition, utilising the number of liquor licenses holding population levels constant can be viewed as a proxy for night-life culture in the municipality.¹¹ We investigate how different numbers of liquor licenses affects

¹¹We estimated all equations in the main model controlling for the number of licensed places in each municipality. The variable did not contribute to explain the variation in the number of traffic accidents, which is likely due to limited variation in licenses over time.

Table 6: Estimating the effect of changes in on-premise alcohol sales on the number of traffic accidents by the number of serving licences in the municipality

	Less than 6 (1)	Between 6 and 12 (2)	More than 12 (3)
Closing hour	0.040 (0.056)	0.010 (0.068)	0.332*** (0.121)
Closing hour × population	0.024 (0.177)	-0.033 (0.137)	-0.245*** (0.064)
R ²	0.019	0.019	0.254
Observations	1429	1639	1437
Municipalities	156	170	145

Note: The dependent variable is the number of traffic accidents occurring week-ends between 10pm and 5am. All regressions are estimated with fixed effects and include controls for population, number of young adults and year effects. ***, **, * indicate statistical significance at 1%, 5% and 10%, respectively.

treatment intensity by dividing the sample by license tertiles. The results presented in Table 6 show that there is no effect of opening hours in municipalities with few liquor licenses (up to 12). In those where the number of licences exceeds twelve, the effect of liberalising closing hours has a substantial impact on accidents.

In general, the results from the main model persist in column (3), even though the initial effect of liberalising closing hours is somewhat stronger than what previous results have shown. In addition, the coefficient of the interaction term is smaller than the coefficient of the initial effect. This implies that population levels need to be even larger for the benefit of a decrease in traffic accidents to outweigh the initial increase in traffic accidents. The mean population in municipalities where the number of issued licenses exceed 12 is 25,000, and the corresponding accident mean is 1.46. Liberalising closing hours by one hour reduces the number of accidents with 0.3 per municipality per year, or 43.5 accidents a year for the relevant municipalities. Comparing to the mean number of accidents, the effect translates to a 20 percent reduction following a liberalisation in closing hours. Thus, it seems that treatment intensity has been identified to be stronger in municipalities with a higher number of liquor licenses, and that the effect of changing on-premise alcohol serving hour is most evident in populous municipalities.

7.2 Symmetric effects

Table 7: Estimated effect of on-premise alcohol sales on the number of traffic accidents, separated by whether hours were extended or restricted, separately (2009-2018)

	Only Liberalised (1)	Only Restricted (2)
Closing hour	0.531*** (0.176)	-0.104 (0.131)
Closing hour \times population	-0.501* (0.253)	0.080 (0.271)
R ²	0.083	0.120
Observations	472	389
Municipalities	52	40

Note: The dependent variable is the number of traffic accidents occurring weekends between 10pm and 5am. All regressions are estimated with fixed effects and include controls for population, number of young adults and year effects. ***, **, * indicate statistical significance at 1%, 5% and 10%, respectively.

A key advantage of our setting is that we observe both liberalisation and restriction of closing hours. This has two implications. First, it reduces the risk of unobserved variables confounding the interpretation of our estimates of closing hours. Second, it allows us to split the sample between municipalities that have liberalised and municipalities that have restricted their bar closing hours, providing the basis of a symmetry testing exercise. From a policy perspective, it is interesting to uncover whether an alteration in closing hour of a particular direction maximises the reduction in the number of traffic accidents. There is one challenge regarding splitting up the sample in this manner. During the ten-year period in our sample, many municipalities changed closing hours in both directions, as pointed out in Section 3. Municipalities that only liberalised or only restricted closing hours constitute around 28 and 25 percent of the sample, respectively. Our approach is to estimate our main model where we split the sample into municipalities that either only extended or only restricted bar closing hours.

The estimates are presented in Table 5. Column (1) reveals that the effects found in the baseline estimates are also found in municipalities that have

liberalised closing hours. The coefficients of interest are larger relative to the main model, and statistically significant. Turning to column (2) reveals smaller and not statistically significant effects of closing hour restrictions on traffic accidents. These results suggest asymmetric effects of closing hours changes on traffic accidents. Reductions in opening hours have no effect on traffic accidents, but liberalisation increase the number of accidents in smaller municipalities and decreases those in more populous municipalities.

8 Conclusion

There is ongoing debate regarding the regulation of alcohol availability, where bar opening hours is a main focus. This debate reflects a range of issues and interest groups, including perceived trade-offs between costs associated with health and public disorder, and benefits from greater individual liberty and economic activity.

A particular focus is the link between bar opening hours and traffic safety. Existing evidence in this area, which primarily comes from one-off extensions or restrictions, paints a mixed picture. We return to this issue focusing on Norway where municipalities are free to choose opening hours within quite large margins set nationally. Moreover, they exercise this choice, and frequently change these hours. This provides a setting where we observe many changes, both extensions and liberalisations, across a variety of time margins. Critically, this occurs in a setting where other relevant policy decisions are set nationally, and do not vary in our time of analysis.

We demonstrate average zero effects of opening hours on traffic accidents that mask large variations across municipalities, where the key sources of variation relates to population. Later closing hours increase accidents in smaller, less populated, municipalities, while substantially decreasing accidents in average and more populated municipalities. This result is shown also in estimates of police reports of driving under the influence. In general, we find sizeable effects despite studying a setting with low accidents numbers and high policing. Importantly, the results in this paper suggest that the effect of closing hour on traffic accidents is heterogeneous and highly context-specific.

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Chapter 3

Safety first? The effect of studded tyres on traffic accidents and local air pollution

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Abstract

Trade-offs between different social outcomes are a feature of many environmental policies. One example that applies to several countries is road safety measures during winter. These take the form of regulating the use of studded tyres, which offer superior traction on icy roads, but also contribute to harmful particulate matter pollution. This paper offers novel empirical insights into the compromise between local air pollution and traffic accidents by exploiting an annual studded tyre ban in Norway. The ban's reinstatement date varies according to when Easter falls, which can differ by over a month. Exploiting this variation in a regression discontinuity design, I find that the social cost of pollution outweighs the safety effects obtained from using studded tyres, even during the coldest winters.

JEL Classification: I18, R41, Q53

Keywords: Traffic accidents, Pollution, Regulation

*I am thankful to Colin Green and Jørn Rattsø for valuable suggestions and advice, and grateful for data access from the Norwegian Public Roads Administration, Finance Norway and Air Quality in Norway. **Correspondence:** Department of Economics, Norwegian University of Science and Technology, Norway. *E-mail address:* lana.krehic@ntnu.no

1 Introduction

Particulate matter is a pollutant that poses a great risk to health due to its ability to penetrate peoples' lungs and enter their bloodstream. According to the World Health Organization (2018), particulate matter pollution is believed to have caused as many as 4.2 million deaths globally in 2016. Recognising this, there have been a range of policies aimed at reducing pollution, ranging from driving restrictions to congestion charges. In this context, the use of studded tyres is frequently debated and is considered a double-edged sword. On the one hand, studded tyres provide superior traction under snowy and icy conditions, greatly increasing road safety under demanding driving conditions. On the other hand, the studs contribute considerably to pollution by tearing off micro-particles from the road surface.

While studs on tyres are entirely forbidden in many countries, other areas experiencing tougher winters, such as Canada, the northernmost states in the US, and several European countries, allow studded tyres only between specific dates due to concerns about their effects on pollution. The Norwegian Environment Agency recently recommended reducing the use of studded tyres as a means of reducing PM₁₀ pollution. While a decrease in particulate matter pollution is considered a social benefit, it may be outweighed by potential increases in traffic accidents, leaving the net social benefit in doubt.

This paper provides estimates of the effect of studded tyres on both accidents and pollution levels. I exploit the fact that for most of Norway, the yearly reinstatement of the studded tyre ban depends on when Easter falls, a date that can vary by up to a month from one year to another and is independent of, for example, prevailing weather conditions. The three northernmost counties, however, have a fixed ban date. This causes the actual ban date to vary both within Norway and across years.

I focus on two main questions. First, do traffic accidents increase after the ban? Second, do particulate matter pollution levels decrease following the ban? By employing a regression discontinuity design, I compare accidents and pollution levels one week before and after the ban within the same year, with the assumption that unobservable variables such as driver behaviour and driving conditions are unaltered due to the ban. Specifically, as the date of the ban changes

from year to year, this greatly reduces the threat of other variables changing simultaneously with the ban. I translate these findings into societal costs and benefits, offering novel insights into the trade-offs between decreases in local air pollution and enhancements in traffic safety. If the net social benefit of studded tyre use is positive, the ban date should be postponed and policy-makers should consider deregulating the use of studded tyres. If the opposite is true, areas allowing studded tyres should consider restricting the use of studded tyres.

I use daily data on police-reported traffic accidents for all Norwegian counties between 2008 and 2019, and daily pollution level data from the ten largest cities over the same time period. The results imply an increase in traffic accidents following the ban's start. Moreover, the effect remains when estimating the effect of the ban using alternative data on accidents reported to insurance companies. A closer investigation reveals that there is an increased safety effect of studded tyres during the coldest years in the sample. Furthermore, I find that the studded tyre ban decreases pollution, a result that is robust to variations in controls and alternative functional forms. I also perform two placebo tests. First, I change the ban date and second, I replace the dependent variable with NO_2 levels, a pollutant that should not be affected by the change in studded tyre use. The effect of the ban is not statistically significant in either of the two placebo tests which is an important validation of the design.

A cost-benefit analysis shows that the cost of particulate matter pollution created by studded tyres far outweighs the safety benefits of studded tyres. This conclusion holds even in the the coldest years in the sample, when studded tyres have a stronger safety effect. The results in this paper represent an important public health finding, and raise the question of whether policy-makers should take other measures to increase traffic safety during winter.

In what follows, I briefly review the literature on the consequences of traffic regulation policies, particularly on road safety and pollution. Furthermore, I explain the institutional settings of the studded tyre ban. I then outline the data used in the analysis and follow this with a description of the methodology and key results. In addition, I present a series of robustness tests and discuss the findings. I conclude with suggestions for further research.

2 Related literature

This paper is related to a broader literature assessing the effects of policies that aim to restrict emissions via reduced congestion and that also have an impact on traffic safety. Typically, researchers find that reductions in congestion reduce both pollution and traffic accidents. For instance, Green, Heywood, and Navarro (2016, 2020) utilise a difference-in-differences analysis to assess how the number of traffic accidents and pollution were affected by the introduction of the London congestion charge. The authors find a decrease in the number of traffic accidents and in most pollutants after comparing observations from London to observations from the 20 largest cities in Great Britain before and after the institution of the congestion charge. However, NO_2 levels increased following the charge, which the authors attribute to drivers substituting towards exempt diesel vehicles. Wolff (2014) and Pestel and Wozny (2019) use the same approach to evaluate the effect on PM_{10} pollution and the health effects of low-emission zones in Germany. Both studies find statistically significant reductions in particulate matter levels, whereas Pestel and Wozny (2019) more specifically find a reduction in incidents of chronic diseases of the circulatory and respiratory system.

Other studies apply the regression discontinuity design to explore the impact of specific pollution-restricting policies. Davis (2008) studies the change in several pollutants after the "Hoy No Circula" was introduced in Mexico City, a policy that forbids driving certain days of the week, dependent on the car owner's licence plate number. He finds no evidence of improved air quality, which he discovers is due to inter-temporal substitution towards driving during hours when the restriction is not in place, and that some inhabitants purchased a second, older and more polluting car to have another license plate. A similar driving restriction scheme was introduced in Beijing in 2008. Viard and Fu (2015) utilise a regression discontinuity design and find that pollution, in addition to labour supply, decreased after the policy was implemented.

More closely related to this paper is research that is concerned with studded tyres specifically. Although studded tyres are widely used in several European countries, Canada and the northernmost states in the US, few studies exist on the costs and benefits of studded tyres. Even among studies considering the road safety benefits of studded tyres find mixed evidence. According to Elvik (1999)

there are two main approaches commonly used when evaluating the safety effects of studded tyres. One approach is to study the number of accidents involving cars using studded tyres and determine whether there are differences in accident rates compared to cars without studded tyres. Using this approach, Malmivuo, Luoma, and Porthin (2017) find that the overall risk of fatal road accidents in winter does not differ statistically significantly between vehicles with and without studded tyres. On icy roads however, the authors find a higher accident risk for vehicles without studded tyres. A concern with this particular approach is the risk of selection in people driving with studded tyres, which may confound the true effect on road safety. Indeed, the authors of the aforementioned study find that motorists driving with non-studded tyres were more experienced and drove newer cars. If less experienced drivers and older cars are more likely to have studded tyres, then the observed safety effect of studded tyres is likely to be deflated.

A second approach is to compare the number of traffic accidents following a general ban on studded tyres with the number of accidents before the ban or to explore whether a general change in observed studded tyre use is associated with a change in traffic accidents over time. Elvik, Fridstrøm, Kaminska, and Meyer (2013) estimate the use of studded tyres and traffic accidents in five cities in Norway between 1991 and 2009. They find that accidents involving injuries increase by five percent when the use of studded tyres decreases by 25 percentage points. A challenge associated with exploiting general variation in the percentage of cars using studded tyres over several years is that factors such as car safety features, road maintenance and other relevant variables are likely to have also changed over time. In the absence of counterfactuals, it is difficult to disentangle the effect of changes in studded tyre use and the other confounding factors.

This paper differs in several ways from the aforementioned literature, first and foremost because I analyse a particular type of pollution-restricting policy. Being that the ban I study is cyclical and follows a fixed rule, the date the ban begins varies from year to year, and, more importantly, no other policy is enacted at the same time as the ban on studded tyres. This is in contrast to the aforementioned papers studying driving restrictions and congestion charges, which are typically accompanied by programmes designed to, for example, increase the use of public

transportation, cycling and walking. While I do not have information on tyre type, I conduct a range of placebo tests aimed at testing the validity of the main results and assuring that the only factor changing after ban start is the use of studded tyres. In addition, by utilising a regression discontinuity design to compare changes in accidents and pollution only a short period before and after the ban, unobserved characteristics are likely to remain unchanged, strongly reducing the risk of confounding factors. This also means that the characteristics of the people driving and of their cars should be the same before and after the ban, making the selection problem in this study less salient than that of previous studies.

With this paper, I contribute to the empirical literature by assessing both the potential benefits and costs of using studded tyres, and provide a broader understanding of the societal net benefit. In particular, to my knowledge there exists no other research that assesses the increase in roadside particulate matter due to studded tyre use in a non-laboratory setting. Identifying the overall effect of studded tyres is important for policy-makers considering reducing pollution with the help of restricting the use of studs. Likewise, for areas experiencing a spike in accidents during winter this study can help decide whether studs in tyres should be encouraged or if other safety measures such as increased road maintenance are preferable.

3 Background

Particulate matter pollution and the negative health effects associated with increased pollution are a major concern worldwide. The case of pollution is particularly challenging as individuals do not account for cost they inflict on society. This failure to align private and social marginal costs is a classic example of a negative externality that needs to be internalised in order to avoid welfare losses. Aligning these costs hinges upon proper pricing of the externality, which is achieved by revealing and understanding the real economic costs of pollution (Vickrey, 1963).

Car traffic is the main source of PM_{10} pollution in urban areas, deriving mainly from road and tyre wear and tear. According to the Norwegian Institute of

Public Health (2013) the use of studded tyres during winter in Norway and similar countries results in these countries having a considerably higher prevalence of ambient air particulate matter pollution than other parts of the world. It is widely known that long-term exposure to particulate matter pollution directly contributes to the development of lung and cardiovascular diseases. In addition, research shows that health risks accrue at lower concentration levels and after shorter exposure than previously thought (World Health Organization, 2006).

In an effort to limit particulate matter pollution, many authorities including the Norwegian government, restrict the use of studded tyres to be within certain dates. Because the climate in Norway varies considerably from the north to the south, the ban varies across counties. In the northernmost counties, which encompass approximately ten percent of the population and 35 percent of the landmass, studded tyres are permitted from the 16th of October through the 30th of April every year. For the rest of the country, the ban ends every year from the 1st of November and is reinstated the first Monday after Easter Monday. Since Easter falls on the first Sunday after the first full moon following the spring equinox, the actual date of the ban implementation varies greatly from year to year. For example, in 2008, the last date studded tyres use was permitted was March 30th, whereas May 1st was the corresponding date in 2011.¹

The date when the ban is lifted are merely days when the use of studded tyres is allowed and do not represent a period where use is mandatory. A concern is therefore that the end of the ban is not as binding as the beginning of it. Drivers will not be fined for a lack of studded tyres, and can wait until the driving conditions demand studs. In that respect, the use of studded tyres around the ban end date is endogenous to weather conditions, and thus does not serve as a good assignment variable. On the other hand, if one is caught driving studded tyres outside of the permitted period, a fine amounting to 1,000 NOK (~106 USD) is issued by the authorities. With this in mind, I focus my analysis on the days around the Easter dependent start of the ban, and not the end of the ban.

A concern about employing the ban start date as the assignment variable is that, if conditions appear dry, car owners may remove the studded tyres and

¹The following list presents the ban start dates in chronological order, from 2008 to 2019: March 31st, April 20th, April 12th, May 2nd, April 16th, April 8th, April 28th, April 13th, April 4th, April 24th, April 9th and April 29th.

switch to summer tyres before the ban starts. However, there are several examples of sudden temperature drops and heavy snowfalls in both late April and May are not uncommon anywhere in Norway.² Therefore, it is not obvious why a driver would risk his or her safety by switching earlier than necessary, especially since there are no apparent individual disadvantages to driving with studs. In addition, the use of tyre hotels has become increasingly popular, which imply reduced flexibility in the timing of tyre switching. Nonetheless, if people were to change tyres before the ban starts, it would simply mean that the estimates found in the analysis represent a lower bound on the true effect of studded tyres on traffic accidents and pollution.

4 Data and method

The data used in this paper come from several sources. I draw daily accident data from 2008 and 2019 from the Norwegian Public Roads Administration, containing all motor vehicle accidents reported to the police for all 18 counties.³ Pollution data are drawn from the national service Air Quality in Norway, which collects pollution levels from fixed-location monitoring stations in several Norwegian cities. The information collected by each monitoring station varies; however, two relevant road traffic pollutants are consistently recorded, namely, PM₁₀ and NO₂. The first is impacted by the use of studded tyres, whereas the second is not. NO₂ therefore is used to conduct a placebo test later in the analysis. According to the Norwegian Institute of Public Health (2013), the size of the particulate matter is of significance for the diffusion of the pollution. Because of its size, PM₁₀ spends less time in the air and spreads less than, for example, PM_{2.5} particles. This implies that the measured level of PM₁₀ originates from local emissions, which is an important assumption for the identification strategy of this paper.

Observations on pollution over a suitable time period are recorded for ten

²According to meteorologist Kristian Gislefoss, May is a month that has historically experienced both snow and 30 degree Celsius days, even from one year to the next (Ertesvåg, 2018).

³I use the county structure valid in 2019, meaning that Nord-Trøndelag and Sør-Trøndelag denote one county.

cities in Norway, starting from 2008.⁴ For this reason, the analysis of the accident data will be performed both for all counties and for the ten cities for which I have pollution data to ensure that the results are comparable. On the one hand, it is advantageous to study accidents at the county level because the risk of local confounding factors influencing the outcome is reduced. On the other hand, disaggregated data allow me to study local outcomes such as local air pollution, and to explore how and whether weather conditions impact these results. This, in turn, allows for a number of extended robustness tests. When estimating the accidents and pollution effects in cities, I include weather variables on precipitation, temperature and wind speed drawn from the Norwegian Meteorological Institute. Finally, all regressions include population-level data attained from Statistics Norway.

The main equation is a flexible model estimated using a regression discontinuity design to assess the impact of the studded tyre ban on accidents and pollution:

$$y_{it} = \delta Ban_{it} + f(DaysSinceBan_{it}) + DOW_t \alpha + \beta_1 H_t + \beta_2 p_{it} + W_{it} \gamma + \lambda_t + u_{it} \quad (1)$$

where y_{it} is the logarithm of the number of accidents or the pollution level in local authority i on day t , Ban is a treatment indicator equal to 1 on the days the ban is in effect in county i in year t , and zero otherwise, and $f(DaysSinceBan)$ is a flexible function that includes the running variable indicating the number of days since the ban was reinstated. For example, the variable $f(DaysSinceBan)$ takes the values of -5 and 5; five days before and after the ban is reinstated, respectively. To examine the robustness of the main result, this variable will be estimated both as a linear and nonlinear function.⁵ Furthermore, DOW_t is a vector of dummies indicating the day of the week, and H_t is a dummy indicating public holidays. The inclusion of these controls is crucial as traffic flows vary

⁴The cities included are Bergen, Drammen, Fredrikstad, Kristiansand, Lillehammer, Oslo, Stavanger, Tromsø, Trondheim and Ålesund.

⁵The specific form of $f(DaysSinceBan)$ depends on the polynomial order with which I model the regression. For example, when modelling a linear model, the function can be written as $\alpha_0 + \beta DaysSinceBan$, whereas when including a second-order polynomial, the function becomes $\alpha_0 + \beta_1 DaysSinceBan + \beta_2 DaysSinceBan^2$. See Angrist and Pischke (2009) for a more detailed explanation.

greatly between workdays and non-workdays. The variable p_{it} is the natural logarithm of the population in local authority i in year t , and W_{it} is a vector of weather variables. I also include year dummies, represented by the coefficient λ_t .

Comparable studies using regression discontinuity design to examine the effect of pollution-restricting policies, such as Viard and Fu (2015) and Davis (2008), have used 20 or 30 days around the cut-off. In this paper, I use an estimation window that includes one week before and one week after the ban. The reasons for this somewhat narrow estimation window are threefold. First, in contrast to the aforementioned papers where the cut-off is one event in time, the studded tyre ban is repeated every year, thus increasing the number of observations close to the cut-off. Second, the regression discontinuity design hinges upon comparable conditions before and after the cut-off to obtain valid estimates. Expanding the estimation window would entail an enhanced risk of, for example, substantial weather differences. Last and most importantly, although the studded tyre rule forces the ban *date* to vary from year to year, the ban date always happens one week after Easter, a period comprised of several public holidays. Consequently, expanding the analysis to encompass the two or three weeks before the ban would not constitute a valid comparison group for the two or three weeks after the ban. Although the consequence of exclusively using one week before and after the ban is lower precision, it is also likely to reduce the risk of bias due to confounding factors (Lee & Lemieux, 2010).

The parameter of interest is δ , which captures the average difference in accidents and pollution when studded tyres are allowed and when studded tyres are banned. The identifying assumption is that observing the number of accidents and the pollution levels just before the ban provides a valid measurement of the average number of accidents and the average pollution levels that would have been if the ban had not been reinstated. More specifically, this requires that no other observable or unobservable factors that influence accidents or pollution jump at the cut-off point. In addition, as the accident data do not include information on whether the involved cars were using studded tyres or not, it is crucial to identification that the only change around the cut-off is drivers switching from studded to summer tyres. In discussing the results, I recognise this and other potential threats to identification and seek to address these. Checks for robustness

in the analysis include varying the control variables and the polynomials, changing the bandwidth, analysing the covariates and performing two placebo tests, in line with Angrist and Pischke (2009), Cattaneo, Titiunik, and Vazquez-Bare (2020) and Lee and Lemieux (2010).

Table 1: The mean and standard deviation (in parentheses) of each variable one week before and after the yearly studded tyre ban (2008-2019).

Variable	Before	After
Panel A: Daily county data (N=18)		
Accidents	0.69 (0.96)	0.72 (0.95)
Population (/1000)	281.6 (161.6)	281.7 (161.6)
Panel B: Daily city data (N=10)		
Accidents	0.35 (0.78)	0.40 (0.81)
Population (/1000)	165.9 (168.8)	165.9 (168.9)
PM ₁₀	31.46 (21.28)	28.3 (21.1)
NO ₂ *	32.39 (16.96)	31.62 (15.67)
Temperature (°C)	5.74 (4.18)	6.12 (3.31)
Precipitation (mm)	1.85 (3.96)	1.69 (3.93)
Wind speed (m/s)	3.63 (2.03)	3.7 (1.91)

Note: *Panel A*: Observations from the 18 counties existing in 2019. *Panel B*: Cities include Bergen, Drammen, Fredrikstad, Kristiansand, Lillehammer, Oslo, Stavanger, Tromsø, Trondheim and Ålesund. PM₁₀, NO₂, temperature and wind are 24-hour averages. Precipitation is the 24-hour total. *: NO₂ levels are not available for Ålesund.

Table 1 displays summary statistics on accidents, population and pollution levels one week before and after the ban in the years between 2008 and 2019. Temperature, precipitation and wind speed statistics are also displayed, as these variables are known to influence pollution level metering. In the sample period from 2008 to 2019 there is a total of 2.486 and 680 accidents over the two-week period on the county and city level, respectively. In general, the average number of accidents increases both in the county and city samples after the ban starts. However, the difference in means is not statistically significant. Note that the accident numbers are only accidents reported to the police and may be an underrepresentation of the actual number of accidents. On the other hand, the accidents reported here are likely to be of a more serious manner. Thus, they represent an important segment of accidents with respect to costs and may

simply be of greater policy interest. However, in the robustness section in this paper, I analyse the effect of the ban on an alternative data source, namely traffic accidents reported to the insurance companies. Generally, the only two variables that are significantly different before and after the ban, are the decrease in particulate matter pollution and the increase in temperature.

5 Results for traffic accidents

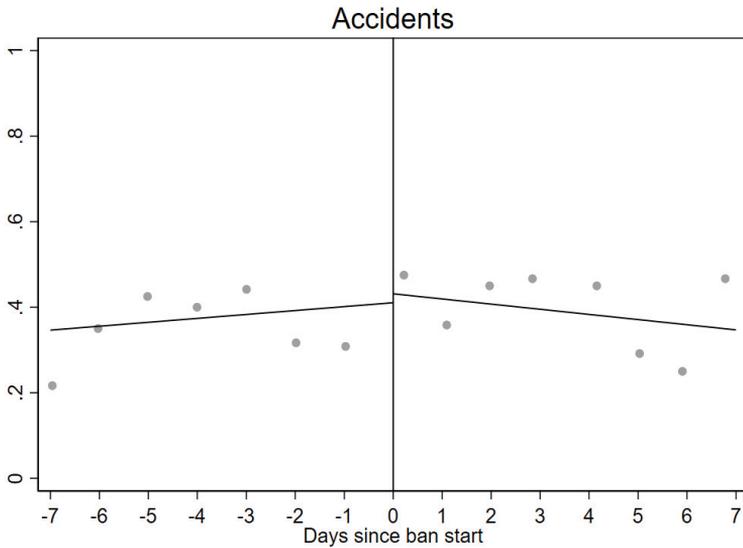


Figure 1: Regression discontinuity plot of daily traffic accidents in one-day bins.

In this section, I present the results for the relationship between the studded tyre ban and traffic accidents estimated according to Equation 1. I start the regression discontinuity analysis with a graphical illustration.⁶ Figure 1 shows the average number of accidents in one-day bins, where the solid line represents a fitted value from a global linear polynomial. I control only for the day of the week and allow for an intercept shift at the cut-off point that represents the ban start. Visual inspection shows that there is a small jump at the ban start, but

⁶In the plot I use the city data for accidents in order for it to be commensurable with the pollution plot.

in general, the graphical analysis suggests no clear evidence of an increase in accidents following the ban. In what follows I examine the data numerically in a more structured manner by adding a flexible polynomial and multiple controls, and I examine the robustness of the results by applying different bandwidths.

Table 2 displays the regression discontinuity results using county-level data. In this table and in all estimations henceforth, I control for population levels, holidays, day of the week and year. I start off by presenting the results from a fixed-effects regression as a benchmark for assessing potential biases. The result is presented in column (1), where the effect of the ban is expressed as a dummy equal to one if the accident happened when the ban was in force. The estimated coefficient suggests that accidents increase by 17 percent the week following the ban. Nevertheless, the coefficient is not statistically significant.

Table 2: The effect of the studded tyre ban on traffic accidents in Norwegian counties.

Dep. var.:	FE		RD		
	(1)	(2)	(3)	(4)	(5)
ln(accidents)					
Ban effect	0.17 (0.192)	0.378 (0.403)	0.315 (0.490)	0.364 (0.403)	0.18 (0.490)
Polynomial Bandwidth		First 2.43	Second 2.43	Second 2	Second 3
Observations	3,510	3,510	3,510	3,510	3,510
Counties	18	18	18	18	18

Note: The dependent variable is the logarithm of the number of daily traffic accidents one week before and after the ban starts. All regressions are estimated controlling for population, day of the week, and holidays and include year fixed effects. Heteroscedasticity-robust standard errors in parentheses. ***, **, * indicate statistical significance at 1%, 5% and 10%, respectively.

I move on to estimate the relationship using the regression discontinuity method. Column (2) reports the results when the model is estimated with a linear polynomial. The point estimate increases to 38 percent, suggesting a negative bias in the fixed-effects results. Nevertheless, the estimate is not statistically significant.⁷ A concern using the regression discontinuity design may be that

⁷All regressions have also been estimated as level models with no change in results compared to estimating on logarithm form. I prefer the logarithmic presentation to simplify the interpretation of the regression coefficients.

$f(\text{DaysSinceBan}_{it})$ is incorrectly specified. Although the use of high-order polynomials in the regression discontinuity setup is common in the literature, I prefer a linear or quadratic polynomial of the running variable, following Cattaneo et al. (2020) and Gelman and Imbens (2019). Therefore, I include a second-order polynomial to test whether the estimate is sensitive to different specifications of the control function. The result is shown in column (3). The effect of the ban changes neither in direction nor in size, implying that the studded tyre ban does not lead to a statistically significant increase in accidents on the county level.

The choice of bandwidth is a crucial step when estimating with a regressing discontinuity design, and the standard procedure is to follow the mean squared error criterion (Cattaneo et al., 2020). For the models in columns (2) and (3), the optimal bandwidth is 2.43. Consequently, a common test of result sensitivity is to vary the bandwidth. In general, decreasing the bandwidth tends to decrease the possibility of specification error and decrease precision, whereas increasing the bandwidth has the opposite effect. In columns (4) and (5), I estimate the model choosing a bandwidth of two and of three, respectively. Decreasing the bandwidth changes the result slightly, and increasing the bandwidth leads to a large reduction in the estimated coefficient. Altogether, I find no robust evidence that the studded tyre ban increases accidents on the county level.

Table 3: The effect of the studded tyre ban on traffic accidents in Norwegian cities.

Dep. var.:	FE		RD			
	(1)	(2)	(3)	(4)	(5)	(6)
ln(accidents)						
Ban effect	0.213 (0.134)	0.774 (0.552)	0.766* (0.392)	0.785** (0.392)	0.538 (0.328)	0.806** (0.392)
Polynomial		First	Second	Second	Second	Second
Weather controls	No	No	No	Yes	Yes	Yes
Bandwidth		2.7	2.6	2.6	2	3
Observations	1,800	1,800	1,800	1,761	1,761	1,761
Cities	10	10	10	10	10	10

Note: The dependent variable is the logarithm of the number of daily traffic accidents one week before and after the ban starts. All regressions are estimated controlling for population, day of the week, and holidays and include year fixed effects. Heteroscedasticity-robust standard errors in parentheses. ***, **, * indicate statistical significance at 1%, 5% and 10%, respectively.

I repeat the accident analysis on the city-level data, and the results are presented in Table 3. Column (1) displays the fixed-effects result, suggesting an increase in accidents of 21 percent the week following the ban. Again, the effect is not statistically significant. Moving on to the estimates of the relationship using regression discontinuity in column (2), the estimate increases substantially to 77 percent. Although 77 percent may appear to be a large effect, the average daily number of accidents reported to the police is 0.37 accidents, meaning that the observed effect translates to an increase of 0.29 accidents per day per city. Nevertheless, the estimate is not statistically significant. Including a second-degree polynomial as shown in column (3) does not change the size of the estimate considerably, but increases the precision somewhat. The coefficient suggests that the ban is associated with an increase in traffic accidents of 76 percent the week following the ban, a result that is statistically significant at the 10 percent level. In column (4), I add weather controls to the model. As explained in Section 4, one test for whether the regression discontinuity setup is valid entails adding controls to ensure that the estimate does not change considerably, and that only the precision increases. The inclusion of weather controls increase precision so that the estimate is now statistically significant at the five percent level. Moving on to columns (5) and (6), I repeat the exercise of deviating from the optimal bandwidth and find that the estimates are sensitive both in terms of magnitude and precision.

In short, I have estimated the effect of the studded tyre ban on traffic accidents using two different samples. The coefficients from the county and the city sample are different both in terms of magnitude and precision, but not in terms of the directions of the coefficients. There are several possible explanations for why this difference exists. First, it may be that there are heterogeneous effects of studded tyres on urban and rural area accidents, arising from differences in road infrastructure or driving culture. Aggregating accidents to the county level might hide these variations and give an average zero effect on accidents. Second, a feature of the data is that it only includes police-reported accidents. Thus, it may be that the level of reported accidents simply are too low. Alternatively, it is possible that the type of accident that is mostly affected by the studded tyre ban is of a less serious manner, which are prone to being underreported in the

police-reported data.

To explore this last concern, I re-estimate the main model with a third, different, sample. In particular, I utilise data over traffic accidents reported to the largest insurance companies in Norway.⁸ These contain accidents that exclusively involve material damages and presumably are not reported to the police. The insurance data will supplement the police-reported accidents and serve as a robustness test for whether the effect of the ban remains in different data sources. There is however one challenge to using this data. The insurance companies do not consistently record the location of accidents. Consequently, the analysis is performed on the country level, which implies the number of observations is reduced and I lose the ability to control for weather effects. In addition, as explained in Section 3, the three northernmost counties have a fixed ban date start, which is in contrast to rest of the country. Since I cannot differentiate where the accidents took place, I will apply the varying ban date that affects the largest part of the country in this analysis.

Table 4: The influence of the studded tyre ban on traffic accidents using insurance data (country level).

Dep. var.:	RD: Material damages (insurance data)			
ln(accidents)	(1)	(2)	(3)	(4)
Ban effect	0.670*** (0.050)	0.778*** (0.142)	0.670*** (0.050)	0.990*** (0.067)
Polynomial	First	Second	Second	Second
Bandwidth	1.92	3.36	2	4
Observations	180	180	180	180

Note: The dependent variable is the logarithm of the number of daily traffic accidents one week before and after the ban starts. Heteroscedasticity-robust standard errors in parentheses. ***, **, * indicate statistical significance at 1%, 5% and 10%, respectively.

Table 4 present the estimates of the effect of the ban obtained by using insurance-reported accidents. Overall, the coefficients follow the results observed in the main results. Moreover, the estimates are robust to alterations in polynomials and bandwidth, and suggest an increase of approximately 250 accidents for the whole county following the ban. To summarise, the effect of the ban

⁸The largest insurance companies cover approximately 80 percent of the insurance market in Norway.

remains in an alternative data source, which adds robustness to the main result. In addition, it implies that less and more serious accidents are homogeneously affected by the studded tyre ban.

I return to using the police-reported accidents data, which in the main result implied a positive, although not robust, safety effect of studded tyres. This result might derive from average driving conditions in the period around the ban being such that studded tyres are no longer advantageous. More precisely, if the roads are on average too bare, I am not able to detect the safety effect of studded tyres. In an effort to examine whether this could be the case, I run a regression on a sub-sample consisting of the coldest half of the years in the sample. The average temperatures around the ban period range from 2.45 to 9.41 degrees Celsius, and the coldest years are 2008, 2010, 2012, 2013, 2017 and 2018. Although low temperatures do not necessarily mean icy roads, they are an adequate proxy for the purpose of this test. An alternative approach is to run the regression on early Easter years because temperatures are expected to be lower earlier in the year. However, there are several examples of cold but late Easters, for example, in the year 2017, which is the fourth latest ban start and the third coldest winter, making the timing of Easter a poor proxy for cold winters.

Table 5: The effect of the studded tyre ban on traffic accidents during cold winters in Norwegian cities.

Dep. var.:	RD: Cold years			
ln(accidents)	(1)	(2)	(3)	(4)
Ban effect	1.95*** (0.461)	1.767*** (0.540)	1.941*** (0.461)	1.671*** (0.539)
Polynomial	First	Second	Second	Second
Bandwidth	1.98	2.6	2	3
Observations	872	872	872	872
Cities	10	10	10	10

Note: The dependent variable is the logarithm of the number of daily traffic accidents one week before and after the ban starts. All regressions are estimated controlling for population, day of the week, and holidays and include year fixed effects. Heteroscedasticity-robust standard errors in parentheses. ***, **, * indicate statistical significance at 1%, 5% and 10%, respectively.

Table 5 displays the results from the regression discontinuity analysis on the cold winters in the city sample. The first column shows the results when estimat-

ing the regression with a linear polynomial. With an average of 0.4 accidents, the coefficient implies an increase of 0.8 accidents per day per city. The effect is statistically significant at the one percent level. I move on to estimate the relationship including a second-order polynomial, which only negligibly changes the result. In columns (3) and (4), I test whether the estimate is robust to changes in bandwidth. Neither decreasing nor increasing the bandwidth changes the coefficient or the statistical significance considerably. The estimates indicate that there is heterogeneity in the treatment effect of studded tyres on traffic accidents, and as anticipated, it is during the coldest winters that accidents increase the most after switching to summer tyres. Thus, it seems that the studs contribute to increasing road safety, but the ban is on average reinstated in a period when the studs' superior traction is not exploited. This examination of treatment heterogeneity might have policy implications that will be addressed in the discussion section of this paper.

6 Results for pollution

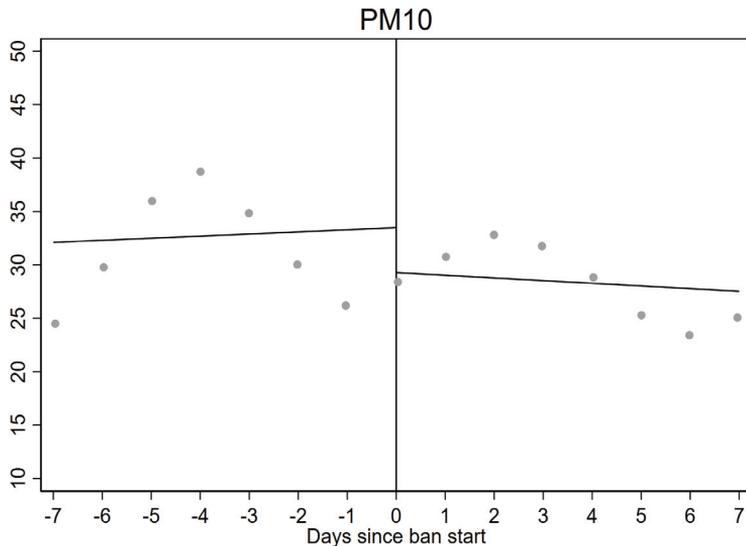


Figure 2: Regression discontinuity plot of daily particulate matter pollution levels (PM_{10}) in one-day bins.

In this section, I present the results from estimating the relationship between the studded tyre ban and particulate matter pollution. Similar to the previous section, I start by illustrating the regression discontinuity for the pollution data around ban start, controlling for the day of the week only when estimating the fitted line. Figure 2 demonstrates a clear drop in pollution levels at the cut-off point. I go on to analyse the discontinuity statistically, beginning with a fixed-effects model. The result is presented in column (1) in Table 6. The coefficient for the ban is negative and suggests a decrease of 13 percent in PM_{10} pollution following the ban. Moving on to estimating the effect using regression discontinuity analysis, the coefficient increases in absolute value to 42 percent. In columns (3) and (4), I estimate the relationship including a second-order polynomial and weather controls, respectively. The preferred model in column (4) suggests that pollution decreases by 21 percent as a consequence of the ban, an effect that is statistically significant at the one percent level. Once again, I test the stability of the results by estimating the model with bandwidths that are both lower and higher than the optimal bandwidth. Columns (5) and (6) suggest that the estimate is stable across this particular variation in the estimation method.

Table 6: The effect of the studded tyre ban on PM_{10} levels in Norwegian cities.

Dep. var.:	FE		RD			
	(1)	(2)	(3)	(4)	(5)	(6)
$\ln(PM_{10})$						
Ban effect	-0.128*** (0.031)	-0.422*** (0.069)	-0.354*** (0.083)	-0.207*** (0.073)	-0.269*** (0.061)	-0.182*** (0.073)
Polynomial		First	Second	Second	Second	Second
Weather	No	No	No	Yes	Yes	Yes
controls						
Bandwidth		1.9	2.6	2.6	2	3
Observations	1,749	1,749	1,749	1,710	1,710	1,710
Cities	10	10	10	10	10	10

Note: The dependent variable is the logarithm of daily 24-hour average PM_{10} pollution one week before and after the ban starts. All regressions are estimated controlling for population, day of the week, and holidays and include year fixed effects. Heteroscedasticity-robust standard errors in parentheses. ***, **, * indicate statistical significance at 1%, 5% and 10%, respectively.

Table 7: Robustness tests: Different ban date, NO₂ and co-variate analysis.

	Placebo		RD on covariates		
	(1)	(2)	(3)	(4)	(5)
	PM ₁₀	NO ₂	Prec.	Temp.	Wind
Ban effect	-0.022 (0.057)	-0.056 (0.069)	-0.146 (0.610)	0.026 (0.170)	0.044 (0.087)
Observations	1,583	1,321	1,785	1,723	1,776
Cities	10	9	10	10	10

Note: All the dependent variables are expressed as natural logarithms and are estimated with the optimal bandwidth found in the main results and a second order polynomial. Heteroscedasticity-robust standard errors in parentheses. ***, **, * indicate statistical significance at 1%, 5% and 10%, respectively.

There is a concern that the statistically significant reduction in pollution identified in the main results is found by chance, or that there exists some other variable that is correlated with both the ban date and pollution levels. For example, even though the ban date can vary by up to a month from one year to another, it always occurs during springtime, when the preferred mode of transport might shift to less polluting methods such as biking or walking. I approach this concern by performing two placebo tests.

First, I replace the ban date and randomly set a placebo ban date to be three weeks after the actual ban. The result of this exercise is presented in column (1) in Table 7. The effect of this placebo ban date is negative, albeit the coefficient is close to zero and not statistically significant. In a second placebo test, I replace the dependent variable with nitrogen-dioxide. This pollutant is formed from car emissions, and its levels are thus independent of the asphalt wear and tear produced by studded tyres. If we do observe a change in this pollutant, it could be a sign that there are some other omitted variables changing concurrently with the ban, which would invalidate the results from the main specification. The result is presented in column (2) in Table 7 and implies that the effect of the ban on nitrogen-dioxide is small in magnitude and not statistically significantly different from zero. Overall, the results of these two placebo tests increase the confidence of the regression discontinuity design applied in the main estimates.

An additional assumption for a valid regression discontinuity analysis is that

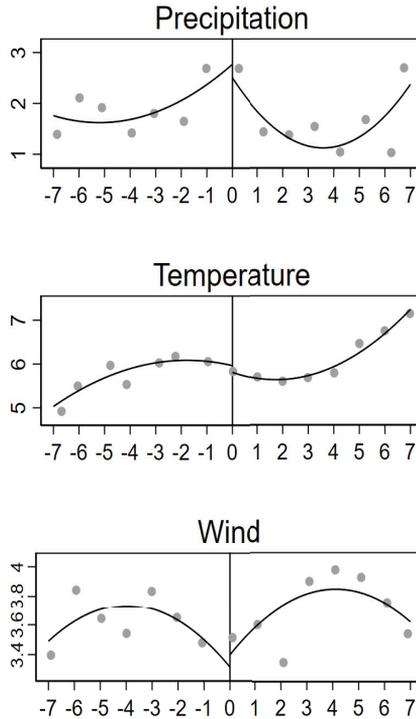


Figure 3: Robustness on covariates.

no covariate should jump around the cut-off of the assignment variable (Lee & Lemieux, 2010). To test whether the setup in the main estimates complies with this particular assumption, I replace the main dependent variable PM_{10} with the covariates, iteratively treating each control variable as an outcome. The results of this exercise are presented graphically in Figure 3 and analytically in columns (3) through (5) of Table 7. Reassuringly, there is no evidence of a sharp discontinuity in precipitation, temperature or wind at the cut-off date. The exogenous nature of the ban rule and the potentially large variation in the ban date relieve concerns that any of the control variables seriously bias the estimates in the main results.

The estimates of the previous section confirm that studded tyres have a stronger safety effect during cold years. To examine whether this heterogeneous effect corresponds to particulate matter pollution, I run the analysis for the same sub-sample of cold years as for the accident data. The results are presented in

Table 8: The effect of the studded tyre ban on particulate matter pollution during cold winters in Norwegian cities.

Dep. var.:	RD: Cold years			
$\ln(\text{PM}_{10})$	(1)	(2)	(3)	(4)
Ban effect	-0.678*** (0.163)	-0.772*** (0.110)	-0.706*** (0.09)	-0.762*** (0.111)
Polynomial	First	Second	Second	Second
Bandwidth	2.6	2.6	2	3
Observations	823	823	823	823
Cities	10	10	10	10

Note: The dependent variable is the logarithm of the number of daily 24-hour average PM_{10} pollution one week before and after the ban starts. All regressions are estimated controlling for population, day of the week, and holidays and include year fixed effects. Heteroscedasticity-robust standard errors in parentheses. ***, **, * indicate statistical significance at 1%, 5% and 10%, respectively.

Table 8, where the pollution level is regressed with a first- and second- order polynomial and is later estimated with sub-optimal bandwidths. The estimates suggest that pollution decreases by approximately 75 percent following the studded tyre ban during cold years, an effect stronger than the effect observed in the main results.

This increase in the ban effect might be due to two reasons. First, it may indicate that the ban start day is more binding when temperatures are cold, compared to milder winters when people might switch to summer tyres before the ban starts. A more binding date implies that studded tyres will be used until the latest permitted day, increasing pollution before the ban which in turn leads to a greater reduction in pollution when studs are no longer allowed. Second, during cold winter days when the air is dry, there is little air change, which in turn leads to an increased concentration of particulate matter pollution. This unfavourable combination of higher studded tyre use and accumulated particulate matter increases the concentration of harmful pollutants during cold winters before the ban starts. In general, despite not having information on studded tyre use, this examination of heterogeneous effects suggests that the treatment intensity varies between years, and supports the main results in terms of direction.

7 Discussion and policy implications

An appealing feature of the estimates found in Sections 5 and 6 is that they provide a basis for calculating the costs and benefits of the use of studded tyres. While I cannot perform a full welfare analysis, these figures can serve as an input into one. To provide an approximate guide to the economic significance of the results in the main models, I combine the empirical point estimates with the average cost of a traffic accident and the statistical value of a life, and compute a back-of-the-envelope cost-benefit assessment of the use of studded tyres.

The cost of banning studded tyres is a potential increase in the number of traffic accidents. Because these estimates did not imply any effect of studded tyres on county-level accidents, and since pollution data on the county level does not exist I confine the calculations to be on the average city level. According to The Norwegian Public Roads Administration (2018), the average cost of a traffic accident is 3.36 million in 2019 NOK (362,679 USD). The average city in the data experiences 0.4 accidents per day before the ban starts, or approximately three accidents per week. Although the estimates on accidents in the city sample are unstable, the upper limit of the results implies an increase in accidents of 79 percent compared to when studded tyres are allowed. This translates to an increase of 2.4 accidents and amounts to a cost of 8 million NOK (862,100 USD) per city per week if studded tyres are banned. Assuming that the effect applies to all 104 cities in Norway, the overall cost is 1.12 billion NOK (87 million USD).

Similarly, I find that pollution decreases by 20 percent after the studded tyre ban starts. This translates to a reduction in daily average pollution of approximately $6 \mu\text{g}/\text{m}^3$ from the sample average of $31.5 \mu\text{g}/\text{m}^3$ the week before the ban. The societal costs of particulate matter pollution are multiple. For example, researchers have found that PM_{10} levels affect labour supply, children's health at birth, children's mortality and all-cause mortality (Viard & Fu, 2015; Currie, Neidell, & Schmieder, 2009; Chay & Greenstone, 2003; He, Fan, & Zhou, 2016). For the cost-benefit analysis in this paper I choose to focus only on the effects on mortality. In one sense, using the overall death rate is not optimal, because children and elderly are individuals that affected by particulate matter to a higher degree than adults individuals. On the other hand, the death rate for individuals with existing respiratory and cardiovascular diseases when pollution

increases is considerably higher. Thus, focusing on the overall death rate might provide a good proxy or even a lower bound on the total cost.

According to the Norwegian Environment Agency (2020), an increase in PM_{10} exposure of $10 \mu\text{g}/\text{m}^3$ leads to an increase in the all-cause risk of death by 0.07 percent. The average death rate in Norway for 2008 to 2019 was 8 per 1,000 inhabitants, and the average population in the city sample is 166,000 people. Assuming that the effect of pollution on health is symmetric, a reduction of $6 \mu\text{g}/\text{m}^3$ translates to one life saved per week in an average city. The value of a statistical life is 33.6 million NOK (3.63 million USD). Thus, the benefit of reducing PM_{10} pollution in all Norwegian cities would save lives worth 201.6 million NOK (378 million USD) per week.

What is important to keep in mind is that the effect of the studded tyre ban on traffic accidents was on average not statistically significant. However, whether we choose to interpret the effect as not statistically significantly different from zero, as the upper bound of 79 percent, or something in between, the benefit of reducing particulate matter is in any case higher than the cost of increased accidents. Not even during the coldest winters, where the results suggest a robust and statistically significant safety effect, does the benefit of studded tyres exceed the costs of pollution.

There are several potential reasons why the benefit that arises from using studded tyres is lower than the cost. While early research found that studs decreased the risk of accidents, much has happened over the last decades both with respect to car safety systems and road maintenance equipment. It appears that this has enhanced safety to a point where studded tyres are superfluous. Accordingly, winter tyres without studs appear to be an alternative that is sufficiently safe throughout the winter.

Alternatively, the safety effect of zero observed in the main results might stem from the fact that the studded tyre ban in general is reinstated too late in the year. In this paper, I focus on the effect of the ban in the two weeks around its start, a time period that encompasses days falling between late March and early May. Even though the exact date of the ban varies within these months, it may be the case that driving conditions in general in that period are not icy, and thus I am not able to clearly identify a change in accidents following the

ban. The heterogeneity in treatment found in Section 5 strengthens the belief that this indeed might be the case. One policy implication would be to push forward the ban date to a period where the safety effect of studded tyres is stronger and the cost of pollution decreases accordingly. On the other hand, this means that drivers using studded tyres should switch to summer tyres before mid-March. The result from the heterogeneity analysis suggest that this could be a bad solution because it would greatly increase accidents during long or cold winters. Pushing the ban start date forward can accordingly be hazardous, and stands as a poor option.

In summary, use of studded tyres with the ban policy that is in place today does not pass a cost-benefit test when applying the estimated benefits and costs found in this study. A possible solution is to set the ban date earlier or ban studded tyres entirely, in combination with increasing road maintenance when conditions demand it. The cost-benefit calculation performed in this paper may provide a useful benchmark as to how much road maintenance expenditures can be increased to obtain a net benefit from studded tyres.

8 Conclusion

This paper provides empirical evidence on the traffic safety and pollution effects of a specific pollution-restricting policy, namely the periodic studded tyre ban in Norway. I utilise a regression discontinuity design to identify the extent to which traffic accidents increase and pollution decreases following the ban. In general, I find evidence of an increase in traffic accidents. At the same time, PM_{10} levels are statistically significantly reduced following the ban. After conducting an analysis of the costs and benefits, I find that the use of studded tyres creates more damage through particulate matter pollution than the benefits through increased traffic safety.

The net costs might arise because the ban is reinstated too late, when roads are bare and studded tyres are no longer advantageous. This is confirmed in the finding that studded tyres significantly contribute to traffic safety during colder years. Thus, one policy implication is to reduce the period when studs are allowed. This will reduce particulate matter emissions and better coordinate the

use of studded tyres with the period when they are most effective. This can be supplemented with increased road maintenance when the ban is reinstated. The back-of-the-envelope calculations performed in this paper can serve as a basis for calculating an increase in the winter maintenance budget while still obtaining a net welfare benefit.

Finally, the evidence found in this paper on traffic accidents diverges somewhat between the aggregated county sample and the city sample. This may be due to the lack of, for example, weather controls on at county level. It might also be that the traffic safety effects of studded tyres are systematically different between cities and the country as a whole when including rural areas due to differences in driver characteristics, road quality and upkeep. Future research should seek to address these concerns, preferably by obtaining more detailed data on actual studded tyre use and municipal expenditure on winter road maintenance.

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ISBN 978-82-326-5286-0 (printed ver.)
ISBN 978-82-326-6226-5 (electronic ver.)
ISSN 1503-8181 (printed ver.)
ISSN 2703-8084 (online ver.)



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