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Hands on the Wheel, Eyes on the Phone: the Effect of Smart Phone Usage on Road Safety

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We provide novel evidence on the effect of smart phone use on road accidents. We exploit variation in phone usage fees in the Netherlands following a change in European Union (EU) roaming regulations implemented in 2017. The growth rate of mobile data roaming increased substantially after the change, which allows us to estimate a difference-in-differences model where non-Dutch drivers from the EU are treated, while Dutch drivers serve as control group. Our results suggest that around 10% of vehicles involved in accidents can be explained by the use of smart phones, and that these accidents mainly happen on urban roads.

Keywords: road safety, accident risk, smart phones, urban roads
JEL Codes: I12, I18, K32, K42, R41

1. Introduction

Traffic accidents are an important loss to society. In the European Union (EU) for example, about 25,000 road users lost their lives due to traffic accidents in 2018. For every death on European roads, there are an additional 50 injuries of which 8 are severe

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and 4 cause permanent disability (European Commission, 2019b). Next to this physical harm, accidents also cause psychological suffering to those directly involved and to friends and relatives of the victims. Traffic accidents also lead to monetary losses due to damages to private and public property, and are a major cause of traffic congestion. The total costs of traffic accidents in the EU are estimated to be about €280 billion, or 2% of GDP, which makes it the most important external cost of transportation (European Commission, 2019a). Similar numbers can be found for the United States and other countries (Blincoe et al., 2015).

These high costs explain the vast body of scientific literature on traffic accidents that exists today, including important contributions from the field of economics on related topics such as the risk of drunk driving (Levitt and Porter, 2001a), the size of the accident externality caused by one typical additional driver (Edlin and Karaca-Mandic, 2006), and the effect of mandatory seatbelt laws on traffic fatalities (Cohen and Einav, 2003).[1] The substantial costs of accidents also provide governments with a strong rationale to prioritise safety in road design, and in traffic and vehicle related regulation. Safety concerns in this respect largely shape policy decisions on aspects such as speed limits, road geometry, obligatory usage of seatbelts, and factors that affect the ability of road users to maintain attention on the driving task. This includes prohibiting the use of alcohol and cell phones by drivers. Figure 1 indicates that stricter safety regulations over the past two decades have had a promising impact on the number of road fatalities in the EU. However, progress in terms of reductions in road fatalities, as compared to the EU policy target formulated by the European Commission, began to diverge and stagnate in 2013, even after accounting for vehicle kilometres travelled.[2]

Despite regulations that forbid car drivers from using mobile phones while driving, effective regulation has proved to be difficult, and technological progress in recent years

[1] Other notable contributions include: Levitt and Porter (2001b), Adams and Cotti (2008), Jacobsen (2011), and DeAngelo and Hansen (2014).

[2] Data on vehicle kilometres travelled for all EU countries does not span back until 2000, so we plot fatality rates per million passenger km for four major EU countries in Figure A1 of Appendix A, which shows a similar trend.

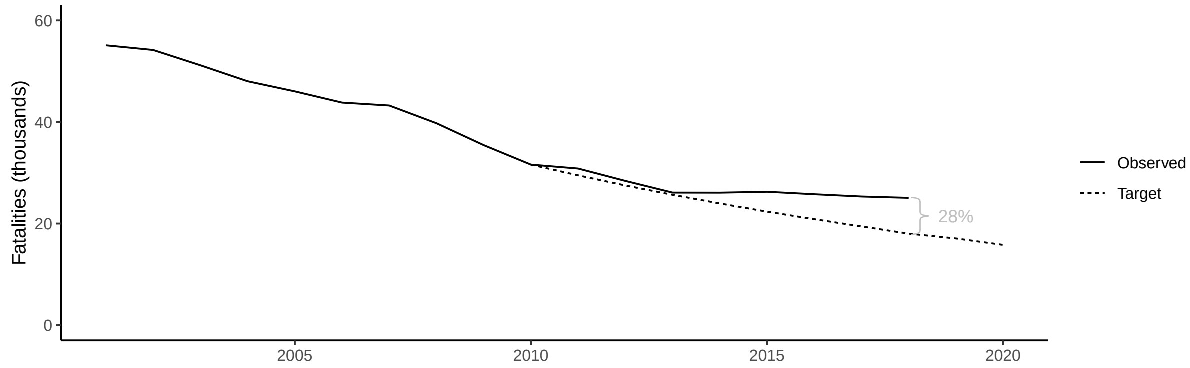


Figure 1: Road fatalities in the EU and 2020 policy target (European Commission, 2019b)

has transformed cell phones into omnipresent devices that can be seen as a major cause of distraction in traffic. Smart phones stand out as a major culprit, as they have enabled various novel distractions, including sending and receiving messages via numerous applications, news updates, video calling, and receiving notifications from social media platforms. In experimental settings, this has been shown to cause visual, cognitive, and physical distractions which result in longer reaction times, less awareness, and various other deficiencies which restrict full control of the vehicle (Zhao et al., 2013, Young et al., 2014, Haque and Washington, 2015).

Findings from the lab are generally corroborated by observational studies in naturalistic settings and crash-based studies (see e.g. Dingus et al., 2016, Redelmeier and Tibshirani, 1997, and McEvoy et al., 2005). However, various studies using field data fail to conclusively prove this relation.[3] In the first large scale field study of its kind, Bhargava and Pathania (2013) estimate the effect of mobile calls on accidents using a discontinuity in the price scheme at 9pm between 2002 and 2005. They find a 7.2 percent increase in call likelihood after the price drop but no corresponding increase in the number of accidents at the 9pm threshold. Further research on the effect of state wide mobile phone bans in the US indicates that the effects are short lived, if detectable at all (Abouk and Adams, 2013; Burger et al., 2014).

[3] Drivers may also be able to navigate streets more easily using navigation applications, hence the effect of phone use on traffic accidents is not per se negative.

The most recent studies that focus on smart phones find more conclusive negative safety effects. Hersh et al. (2019b) exploit temporal variation in 3G coverage in California between 2001 and 2013 to study the effect of gaining access to mobile data on vehicle accidents. After controlling for vehicle kilometres travelled and road segment fixed effects, the authors find that crash rates increase by 1.1 percentage points when roads receive 3G coverage. Furthermore, Faccio and McConnell (2019) find that locations with a lot of activity of Pokémon Go (a popular video game app on the smart phone at the time) faced more vehicle accidents after the introduction of the game, suggesting that 136 of the total 2850 nation wide crashes (approximately 5%) in the five months after the introduction of the game could be attributed to it.

Although numerous studies have investigated the link between phone use and accidents, a substantial research gap prevails.[4] Most existing estimates are dated, while mobile phone use has dramatically changed since the turn of the century in terms of adoption, exposure and capabilities.[5] For example, in the much cited study by Redelmeier and Tibshirani (1997), only 18% of drivers owned mobile phones which had limited capabilities, while in more recent studies, Bhargava and Pathania (2013) only focus on mobile calling and Hersh et al. (2019b) end their study in 2013. Furthermore, studies that do address the interaction between modern smart phones, with data usage, and accidents, either focus on very specific non-generalisable phone-use (Pokémon Go in Faccio and McConnell, 2019), or only focus on highways (Hersh et al., 2019b). In addition, most studies do not account for unobserved factors that may be correlated to both phone use and accident likelihood, such as risk preferences at the individual level and demand factors at the aggregate level. Finally, as sample sizes were often small in experimental and crash-based studies, generalisation to aggregate effects is often problematic. Therefore, an important and ongoing research question is to what extent does mobile phone use while driving affect the number and likelihood of traffic accidents.

[4] See e.g. reviews by WHO (2011), Oviedo-Trespalacios et al., 2016, and Lipovac et al. (2017).

[5] Mobile phone subscriptions per capita have been above one in the world since 2016 (World Bank, 2019) and in 2018 smart phone penetration was above 70% in many developed nations (Newzoo, 2018).

We propose a novel approach based on field data and a natural experiment induced by a change in EU roaming regulations. The specific policy, imposed in June 2017, mandated mobile phone operators to abolish all roaming surcharges for EU customers travelling outside their home country network within the EU. The policy, dubbed *Roam Like at Home* (RLAH), implied that people travelling abroad within the EU now face their home fee, which is substantially lower than pre-policy charges. As a consequence, growth in roaming cellular traffic increased sharply after the policy. Mobile data use while roaming grew by over 200 percentage points, whereas local usage was not affected by the policy and faced stable growth rates.[6] We hypothesize that, as of June 2017, EU citizens driving abroad are more likely to be distracted by their phone, while nothing changed for local usage.

We use micro data on all police reported road accidents in the Netherlands from 2014 until 2018. We then use vehicle registration information to classify which (foreign) drivers are plausibly treated by the RLAH policy. The causal effect of phone use on road accidents is then estimated using a difference-in-differences (DiD) approach, where we use the RLAH policy as treatment, and local users as control group. This allows us to overcome endogeneity issues from earlier studies due to measurement error in phone use and omitted variables. Our key identification assumption is that in the absence of the policy, the number of vehicle accidents by roaming users should follow similar trends to local drivers, for which we provide evidence in our parallel trends plot.

Our findings imply that the increase in phone use due to the policy causes the number of accidents to increase by around 10%. Under plausible assumptions, this implies a crash risk odds ratio of around 3.8. Under the assumption that this mechanism also carries over to local drivers and holds for other EU countries, our results then imply that each year as many as 2,500 road fatalities in the EU can be attributed to phone use. This suggests that about one third of the gap between the EU target and the observed number of fatalities shown in Figure 1 could be reduced by successfully banning mobile phone

[6] Growth rates have been calculated using information from the International Roaming BEREC Benchmark Data Reports (for roaming) and the Dutch Authority for Consumers and Markets (for locals).

use of drivers.

This study contributes to the existing literature in five ways. First, our results provide a causal estimate of phone use on road safety based on a novel method. Second, because our identifying variation comes from a very recent policy intervention, our estimates take into account modern distractions of smart phones, and particularly changes in mobile data use. Third, because our analysis is based on revealed and non-experimental field data of all registered accidents in the Netherlands, we are able to estimate an aggregate effect. This is especially relevant given the urgency of road safety issues, and the rapid growth in cellular traffic. Fourth, with our approach we can estimate how smart phone distractions affect accidents for different severity levels and on different road types. We show that phone distractions increase accident risk predominantly on local urban roads, which highlights that studies focusing solely on highways underestimate the total effect. In addition, our results indicate that both light accidents as well as fatal accidents increase due to smart phone use. Fifth, we introduce an identification strategy that is directly applicable to *all* other countries in the European Union, allowing for convenient cross validation of our results using data from other countries in future research.

The rest of this paper is structured as follows. Section 2 describes the policy context, Section 3 explains the methods employed, and Section 4 presents the data we use. Section 5 discusses our results, robustness checks, and implications. Finally, Section 6 concludes.

2. The Roam Like at Home Policy

On 27 October 2015, the European Parliament adopted regulation No. 2015/2120 which prescribed that all roaming surcharges should be abolished within the EU.[7] Following a decade of EU roaming regulations which aimed to gradually reduce roaming fees within the EEA, the *Roam Like at Home* (RLAH) policy meant that, effective 15 June 2017, telecommunication network providers were required to abolish all roaming surcharges in

[7]Roaming refers to mobile phones connecting to a cellular network abroad. In the absence of regulation, mobile network operators generally charge additional fees for using this service.

addition to domestic retail prices for EU roaming customers.

The policy dramatically reduced the costs of phone use abroad, both compared to the gradual reductions prior to RLAH and compared to the pre-RLAH prices. For example, leading mobile operators such as Vodafone Germany, offered daily roaming packages such as EasyTravel in early May of 2017 providing "phone calls, texting and surfing abroad [within the EU] just like at home" for a price of €2.99 per day. This equates to around €90 per month and is over four times more than standard domestic packages offering calls, texts and data at the time (Vodafone, 2017).[8] The special Eurobarameter (2018) survey, carried out one year after RLAH, suggests that awareness of RLAH was already high with 62% of Europeans that travelled in the previous 12 months being aware that roaming charges had been eliminated, and only 19% of travellers claiming to never use mobile data (down from 42%). Nevertheless, around 50% of the respondents still claim to restrictively use mobile data while abroad, suggesting that EU roaming users still use their mobile phones comparatively less than locals.

To evaluate the effect of RLAH, we collect data on mobile phone usage of roaming users in the EU from the International Roaming BEREC Benchmark Data Reports and local usage from the Dutch Authority for Consumers and Markets (ACM).[9] Figure 2 plots the average monthly data traffic in MB's per roaming user for each quarter between 2012 and 2018, with the shaded region representing when RLAH was active.[10] It indicates that since RLAH was introduced, roaming usage appears to catch up with developments in local cellular data traffic.[11] It also shows that cellular roaming traffic exhibits a strong

[8]Regulated wholesale data rates were capped at €0.05 per MB or €50 per GB, so using data outside of a data bundle may have been restrictively expensive.

[9]BEREC only includes information on the number of active roaming users, referred to as roaming subscribers in the BEREC reports, since the second quarter of 2016. BEREC considers a subscriber to be a roaming subscriber if roaming services were active *at least once* in the concerned period. In order to calculate the average monthly usage before this period, we predict the number of subscribers using a log-linear model with a time trend and quarter dummies. Using total data usage gives almost identical results (available from BEREC upon request). We document this in Appendix A.1.

[10]Note that the second quarter of 2017 already contains 15 days during which the policy was active, namely the second half of June. Furthermore anticipating RLAH, several large network providers dropped roaming chargers earlier in the year, such as Vodaphone UK in April (CNET, 2017).

[11]The average trip duration for European tourists outside their country of residence is about 8.4 nights in 2017 (Eurostat, 2019), which explains why roaming usage is about four times lower than local usage after the policy.

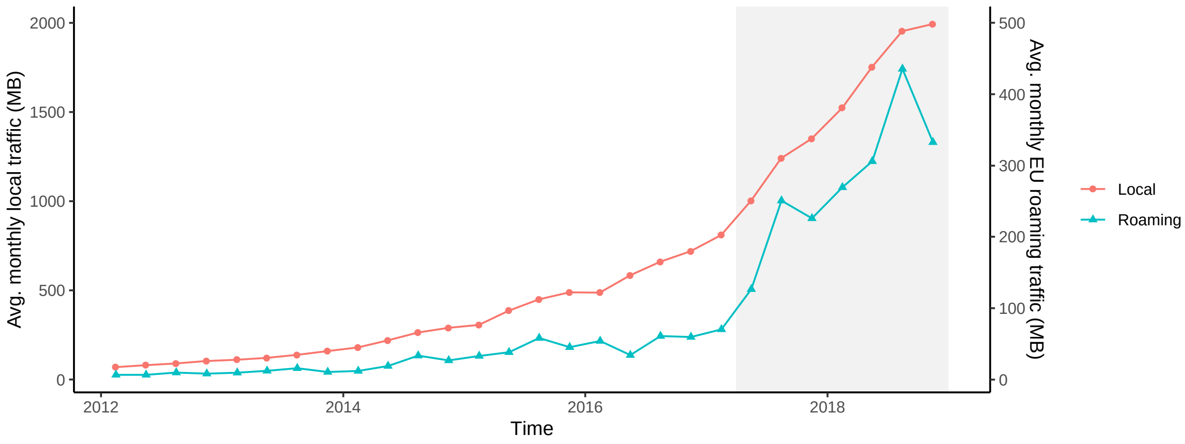


Figure 2: Average monthly data traffic per quarter.

upward growth trend for both groups and demonstrates a high degree of seasonal variation for roaming users. This is not surprising as technological advancements (e.g. introduction of 4G-network) and the increased adoption of smart phones has resulted in higher speeds, lower prices, and more demand, while tourism, and therefore roaming usage, tends to be seasonal. It is therefore useful, when comparing the annual growth rates of cellular traffic, to compare each quarter with the same quarter in the previous year.

Figure 3 illustrates that the RLAH policy resulted in a very large increase in the growth rate of phone use of roaming users one year after the policy, while having *no discernible effect* on locals. Table A4 in Appendix A documents the average annual growth rate before and after the policy for roaming users as compared to locals. It indicates a substantial increase in the growth rate of roaming data usage by 200 percentage points, while texts and calls also increased by around 20 to 80 percentage points, relative to locals. This further demonstrates that the policy had large effects on the overall phone use of roaming users, while especially effecting data usage.

3. Empirical methods

Our aim is to estimate the causal effect of phone usage on traffic accidents. Because data on phone use of drivers is privacy sensitive and not made available for research purposes, we use the implementation of the RLAH policy as a source of exogenous variation. We

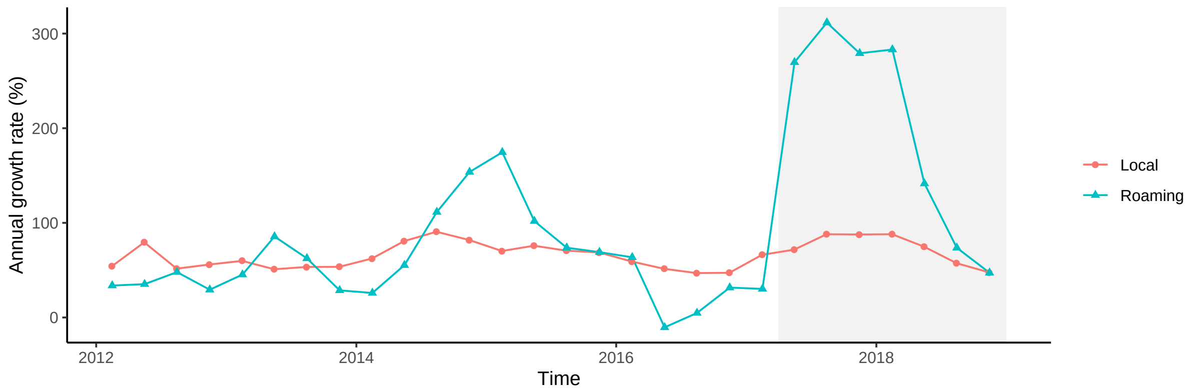


Figure 3: Annual growth rates in cellular data traffic per quarter.

hypothesise that a substantial reduction in phone usage fees induces more phone use while driving, which in turn leads to an increase in accidents due to driver distraction. We analyse the Dutch context and exploit identifying variation from a sharp drop in mobile phone charges following the EU RLAH policy, introduced on June 15, 2017. Unique for this price change, and essential for our identification strategy, is that fees for domestic phone use (i.e. within the home country) are not affected by the policy. This allows us to define a control group, in our case drivers with a Dutch phone subscription, and a treatment group, drivers with a phone subscription from any other EU country. As a consequence, we can employ a difference-in-differences (DiD) approach to estimate the effect of the policy-induced increase in phone use on road safety. Below, we first introduce the general statistical model, and subsequently discuss how we deal with the statistical challenges that arise in our setting.

3.1. *Statistical model*

We use a standard DiD approach, where we estimate how the RLAH policy affects the number of vehicles involved in road accidents. We define V_{it} as the number of vehicles involved in accidents for each country-group in each province, indexed by i, at time t.[12]

[12]Because we essentially have a count model, our temporal and spatial resolutions are arbitrary. We aim for the most fine-grained resolution to maximally use variation over time and space. We are in this respect constrained by the resolution of the essential control variables. We aggregate at the province-month level because this is the most fine grained resolution for which we can control for country-specific VKT.

We consider the general following model:

$$\log(V_{it}) = \beta T_{it} + \gamma \mathbf{H}_{it} + \delta \mathbf{W}_{it} + \phi_i + \kappa_t + \epsilon_{it}, \tag{1}$$

where log denotes the natural logarithm. The treatment effect, denoted by T_{it}, is a dummy equal to one after the policy was introduced for vehicles from roaming countries. We proxy for traffic intensity using vector $\mathbf{H}_{it}$, which contains separate control variables, in logs, for the number of hotel nights of locals and roaming users, and a dummy in case we observe a zero.[13] Further, vector $\mathbf{W}_{it}$ contains weather controls, that we include to improve the efficiency of the estimator.[14]

Finally, we include panel and time fixed effects. Time invariant characteristics of drivers and the area in which they drive, such as the road network, attractiveness to tourists, and number of car users, are captured by a country-group province fixed effect, ϕ_i, which represents the panel element in our analysis. We also control for any unobserved time trends affecting all drivers, for instance due to road maintenance or infrastructure improvements, by including a time fixed effect, κ_t, for each year-month.

We note that using a cell phone was rather costly for roaming users before the policy. It might therefore be useful to assume that before the policy roaming drivers did *not* use their phone at all while driving. However, if roaming users *did* use their phones while driving prior to new roaming regulations, we still accurately estimate the effect of the price drop, but underestimate the total effect of phone use. Our estimates should therefore be considered as a lower bound of the total effect of phone distractions on road accidents.

[13] We obtain hotel nights per province per country of origin from Statistics Netherlands (2019a), which is measured in thousands. In case of a zero, which we only observe for roaming countries, we set the value to one (so that the log is zero) and use a dummy to control for these cases separately.

[14] These include for each province and month the average temperature, average rainfall, and number of days with temperatures below 0 °C.

3.2. *Measurement error*

Measurement error poses a statistical challenge in our setting, because we do not directly observe within-vehicle phone use, nor the type of phone subscription drivers have. Below, we identify three implications of this challenge, and discuss how we deal with them.

First, for multi-vehicle accidents, we cannot identify which driver caused the accident, if any at all. This means that we have measurement error in the dependent variable, which makes our estimates potentially imprecise, albeit still unbiased if the measurement error is random. We address this issue by focussing on vehicles rather than accidents, because multi-vehicle accidents might include both treated and control-group drivers. In addition, we also perform a robustness check where we consider a subsample with single-vehicle accidents (e.g. a car crashing into a tree). This approach rules out measurement error of this sort, but comes at the cost of having less statistical power, as only a small fraction of the accidents in the data are single-vehicle accidents (17.58%). As it is a priori not possible to decide which is the preferred approach, we report results for both estimation strategies.[15]

Second, some drivers of vehicles that are registered abroad might still have a Dutch phone subscription. For instance, drivers that live in bordering regions in Belgium or Germany and often work in the Netherlands. These drivers will be erroneously classified as treated, and will bias our estimates downwards.[16] To address this issue, we will run a robustness check where we exclude all border provinces, as it is likely that this measurement problem is most pronounced in those regions.

Third, some roaming users may not respond at all to price changes if they do not have to pay the mobile phone charges themselves. One can think of unlimited subscriptions paid by drivers' employers or having a Dutch subscription while living just across the

[15] Another related issue which is solved by taking single-vehicle accidents is that roaming accidents may result in more multi-vehicle accidents. This would violate the SUTVA, but it is unlikely to be problematic in this setting due to the size of the control group; around 95% of vehicles in our accidents sample are part of the control group.

[16] Additionally, some roaming users might be driving a Dutch car, for instance a rental car, and will hence be erroneously designated as untreated. This may lead to a small downward bias, however due to the large number of accidents in the control group (local users) it is unlikely to have a substantial effect.

border. This insensitivity to the price would also result in a downward bias of the estimate. We address this concern in two ways. Firstly, we re-estimate our main model on a sub-sample where we exclude trucks and vans, assuming that drivers of these vehicles are most likely to have such arrangements with their employer, and secondly, on a sub-sample without bordering countries or typical labour migration countries.

3.3. *Trends in vehicle kilometres travelled*

A potential confounding factor is vehicle kilometres travelled (VKT) by roaming drivers. For instance, because countries and provinces vary in their popularity as a holiday destination over time (Taylor and Ortiz, 2009), there may be more roaming accidents due to increased tourism rather than due to increased phone distractions. Another potential reason for temporal variation in VKT by roaming drivers could come from changes in trade and business trips as a result of ongoing globalization. Because these trends affect treated drivers (e.g. tourists) but not local drivers, it poses a potential threat to our identification strategy and may lead to overstating the effect of phone distraction on road safety.

Ideally, one would want to directly control for VKT to avoid any bias from traffic intensity. However this information is only available at an annual level, aggregated into Dutch and non-Dutch VKT.[17] We show that the number of hotel nights per country of origin is a good proxy for both tourism and business related traffic (see section 4.2.3 for an extensive discussion on the quality of this proxy). This implies that, if the relation between traffic and hotel nights is stable over time, controlling for hotel nights will absorb a bias that stems from VKT trends of roaming drivers.[18] Nevertheless, we also perform two additional robustness checks. Firstly, we include a roaming specific linear time trend which captures nationwide trends in accidents of roaming users. This is how-

[17]For non-Dutch vehicles, Statistics Netherlands only provides imputed annual figures of VKT for the whole country. For all traffic combined, there are intensity measures available at the province-month level. These will be used to validate our VKT proxy (hotel nights).

[18]This is a reasonable assumption over a five year period but may not hold in the long run (e.g. if cheap flights and high-speed trains make cars a less attractive mode).

ever potentially a bad control as it also picks up part of the treatment effect. Secondly, we re-estimate our models using only two years of observations between July 2016 and July 2018 (i.e. one year pre and one year post policy), for which it is implausible that there are major trends in tourism transport modes conditional on hotel nights.

3.4. *Standard errors*

In our setting, the number of observations depends on an arbitrary temporal and spatial resolution and hence we aggregate vehicle data to province-month observations to align the resolution with our control variables. However, if accidents are serially positively correlated, ordinary least squares (OLS) standard errors may be too small (Bertrand et al., 2004). To address this issue, we cluster our standard errors at the time invariant level of a province and country-group, which leaves us with $12 \times 6 = 72$ clusters (12 provinces and 6 country groups). In addition, we run a robustness check where we ignore all time series variation and aggregate our data into two periods, pre and post policy. This rules out any autocorrelation in error terms and the outcome highlights that our results and standard errors are hardly affected by serial correlation.

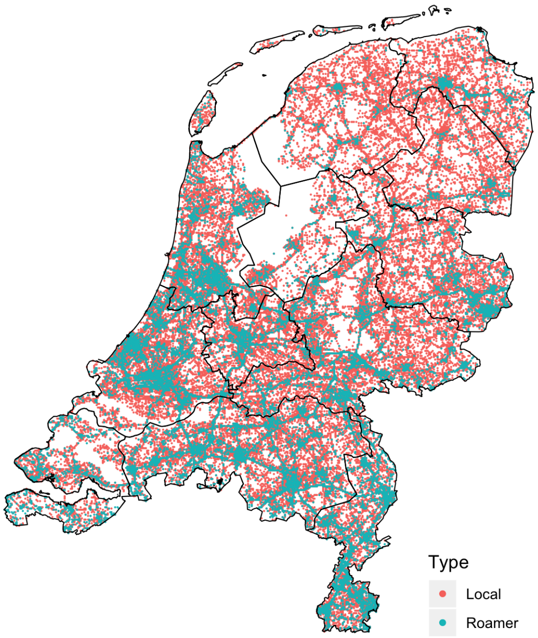
4. Data and context

4.1. *Road safety data*

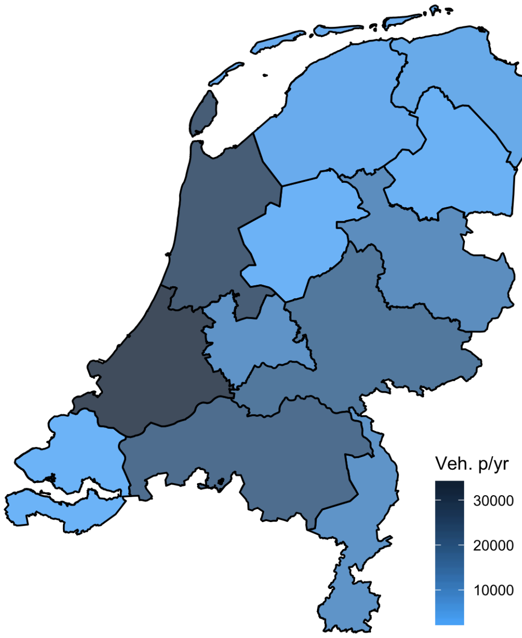
We observe police reported accidents in the Netherlands as published by the Dutch Ministry of Infrastructure and Water Management (specifically 'Rijkswaterstaat'). The maps in Figure 4 plot the locations and annual counts of vehicles involved in accidents per province. The maps highlight that accidents are spread across the country, but more concentrated around urban areas and highways.

Our data contains characteristics of road accidents and of the parties involved.[19] For each accident, we observe accident circumstances, such as day of the week, time of

[19] We use the full dataset available to researchers as we require privacy sensitive information on vehicle registration nationality. A publicly available version of the data is available on `data.overheid.nl`, but does not contain all party characteristics.



(a) Accident locations.



(b) Vehicles in accidents per year per province.

Figure 4: Maps of the Netherlands with accident locations and counts per province.

the day, road type, weather conditions, and road surface conditions. Furthermore, the dataset contains vehicle related characteristics, such as vehicle type, vehicle manoeuvre just before the crash, sex and age of the driver, and the country in which the vehicle is registered.[20] Finally, party related variables are also reported and provide information such as age and sex of involved parties, casualty severity and whether the casualty was a driver, passenger, cyclist, or pedestrian.

We directly observe the vehicles' country of registration. Drivers of cars registered in EU countries, but outside of the Netherlands are likely to reside in those EU countries. Therefore, vehicle registration is a good proxy of whether the driver incurs roaming costs (before RLAH) or uses the local network instead.[21] To abstract from long term trends,

[20]For our particular application we cannot use most of these characteristics as they are often missing for non-local cars. This is because these data stem from the car registry in the Netherlands, which is not connected to databases from other countries. The data does not contain information on whether a car is rented or leased.

[21]Dutch law requires that any vehicle staying in the Netherlands for more than six months must obtain a Dutch licence plate. Note that, due to our difference-in-difference method, misclassification can pose a problem for the efficiency of our estimator, but will not bias our estimates under the plausible assumption that misclassification is not correlated to the roaming regulation.

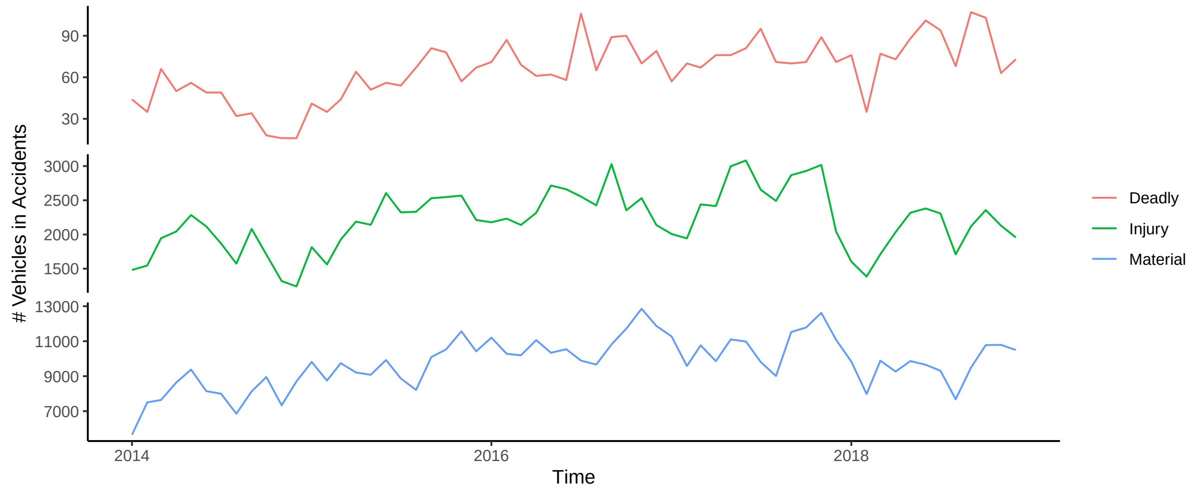


Figure 5: Number vehicles involved in accidents per month by severity.

we use data for the years 2014 until 2018, which contains 0.76 million vehicles involved in 0.44 million accidents. Most accidents have more than one vehicle involved (78%), therefore we use information at the party level to avoid measurement error which may be present at the accident level, as police reports do not indicate which party was at fault. We discuss this issue and how we deal with it in more detail in Section 3.2.

4.1.1. *Trends in road safety*

Figure 5 shows that there appears to be an increase in the number of vehicles involved in accidents over all levels of severity. Over the period of study, our data shows that the annual number of deadly accidents increased by around 20%, while the number of accidents involving injury and material damage increased by about 50%, with most of the change between 2014 and 2016. In an average month there are around 74 vehicles involved in deadly accidents, 2,381 vehicle accidents involving injury and 10,280 vehicle accidents involving material damage.

4.1.2. *Grouping roaming drivers*

We combine observations in our sample into six country groups for our main analysis. The aim of this grouping is to strike a balance between, on the one hand, optimally controlling for unobserved heterogeneity per country of origin (by means of group fixed effects), and on the other hand, preserving statistical power by avoiding zero counts (which are omitted due to the log transformation of the dependent variable, see Section 3 for a discussion).

The first group contains vehicles with a Dutch registration and are our control group (95.12% of sample). Second and third, are the two adjacent countries, with 1.76% of German vehicles and 1.04% of Belgian vehicles, respectively. The fourth group contains other western European countries which account for 0.42% of vehicles in accidents. Drivers from these countries often visit the Netherlands as tourists.[22] The fifth group contains Romanian, Polish, and Bulgarian vehicles (1.32%) which are relatively common on Dutch roads due to joint economic activity and labour migration. More than for other cases, drivers from these labour migration countries may have a Dutch phone subscription and thus might not be treated by the RLAH policy. Therefore, it is important to include a separate fixed effect for vehicles from these countries. It also allows us to run a robustness check where we exclude vehicles from these countries which highlights that vehicles from these countries do *not* drive our overall results (see Section 5.3.1). The sixth group contains all remaining EU countries (0.33%).

4.2. *Descriptive statistics*

4.2.1. *Vehicles involved in accidents*

Around 5% of vehicles involved in accidents are from roaming users, 46% of drivers are female and the average age is 42 years old. Of the total number of accidents, 0.58% are deadly, 18.7% result in injury, and 80.72% cause material damage only.[23]

Local and roaming drivers involved in accidents are roughly comparable, but roaming users tend to be younger, male, and drive more on fast roads than local drivers.[24] In terms of the damage reported, the share of material damage is relatively large for roaming

[22] These are: France, Great-Britain, Denmark, Spain, Austria, Portugal, Luxembourg, Sweden, Italy, Ireland, Norway, and Finland.

[23] Table A1 in Appendix A presents the descriptive statistics for vehicles involved in accidents.

[24] Table A2 in Appendix A provides more detailed descriptives of vehicles involved in accidents by group.

Table 1: Descriptive statistics for province-month data

Statistic	N	Mean	St. Dev.	Min	Max
Panel A: Locals					
Vehicles in accidents	720	953.11	757.40	84	3,297
log(Vehicles in accidents)	720	6.52	0.86	4.43	8.10
No trucks	720	189.13	133.80	26	564
Single vehicle accidents (SV)	720	742.60	590.43	72	2,503
Hotel Nights ($\times$ 1000)	720	148.99	118.92	11	565
Panel B: Roamers					
Vehicles in accidents	3,600	9.78	13.41	0	92
log(Vehicles in accidents)	3,032	1.79	1.19	0.00	4.52
No trucks	3,600	1.68	2.53	0	20
Single vehicle accidents (SV)	3,600	6.16	9.54	0	71
Hotel Nights ($\times$ 1000)	3,600	22.21	72.31	0	707

vehicles. This may be a reporting bias, as language barriers can make it more likely for the police to be called in these situations with only material damage, whereas locals may more easily settle without police present. Importantly, dissimilarities between local and roaming drivers do not threaten our identification of the average treatment effect on the treated (ATET) under the plausible assumption that the RLAH policy does not induce sorting that considerably affects the composition of the group of roaming drivers.[25] These dissimilarities become more relevant when generalizing estimated effects to the untreated population. We discuss the assumptions required to attribute the estimated effect to all drivers in Section 5.4.

4.2.2. *Distribution of accidents*

Our dependent variable is the number of vehicles involved in accidents, aggregated by province, month and country group. Table 1 presents descriptive statistics for various subsets. Naturally, the mean of the count of vehicles involved in accidents is in levels much larger for locals than for roaming users. In logs, however, the figures are more

[25]Figure A3 in Appendix A shows that the age distribution of roaming users does not change considerably after the policy was implemented. We note, however, that even if we find a policy-induced sorting in the distribution of drivers in accidents, this does not necessarily bias our estimates, as it may be a result of the policy e.g. younger drivers may be more likely to use their phone and therefore be more represented in accidents, while the distribution of age groups in kilometres travelled may be the same.

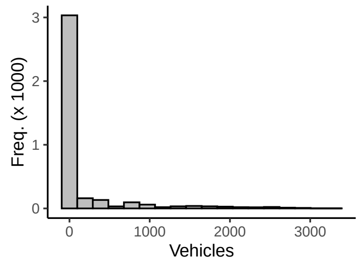
comparable and the standard deviation is in the same ballpark. This means that after controlling for the different mean levels —as we do by including country fixed effects—the treated and control group appear to have very similar support.

Figure 6 shows histograms of the dependent variable after log transformation and after demeaning for fixed effects. Panels (a)–(c) indicate that these empirical distributions are left-skewed, as to be expected from count data. Similarly, panels (d)–(f) show that after taking logs of these counts, distributions still seem to be slightly skewed to the left. However, if we demean by our panel and time fixed effects, as in panels (g)–(i), distributions seem quite symmetric, albeit with a larger variance for roaming compared to local users. This is non-problematic, however, when using standard errors that are robust to heteroskedasticity.

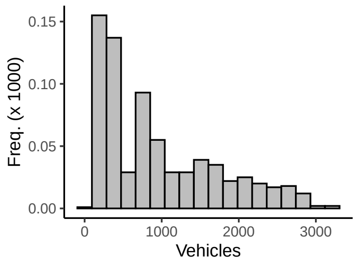
4.2.3. *Hotel nights data as proxy for traffic intensity*

An important concern with our approach may be that country specific trends in traffic intensity, or vehicle kilometres travelled (VKT), might drive our results. For example, an increase in tourism over time may result in relatively more VKT by roaming users and therefore increase the likelihood of a roaming accident after the introduction of RLAH. We do not observe VKT for each drivers' country at the required level of temporal (monthly) and spatial (province) disaggregation. Instead, we use overnight stays in hotels, obtained from Statistics Netherlands (2019a), to proxy for changes in tourism and thereby monthly traffic intensity. For each province, we observe the number of overnight stays per month, disaggregated into guests' country of origin. We assess the quality of this proxy in two ways.

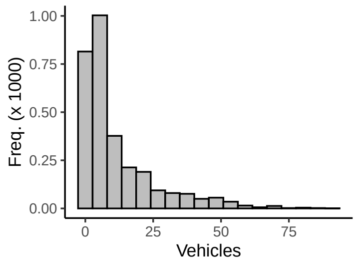
First, we observe country wide VKT at the annual level for locals and non-locals. Figure 7 shows annual growth rates of hotel nights and VKT for local and roaming (non-local) drivers. The figure highlights that over the course of the five years prior to the treatment, VKT by roaming drivers grew more compared to local VKT. However, a similar, yet even stronger trend is visible for hotel nights. Even when we exclude the



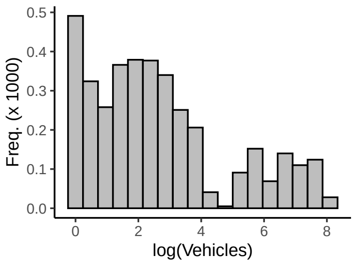
(a) In levels.



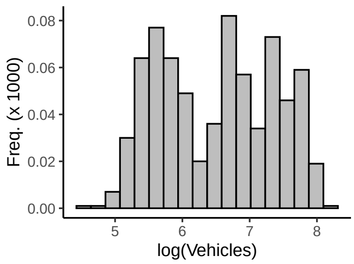
(b) In levels (locals).



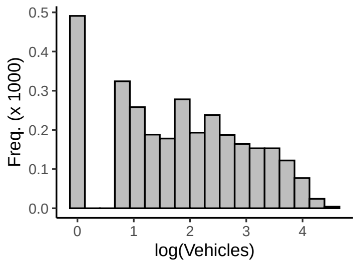
(c) In levels (roaming users).



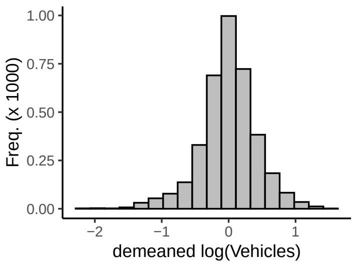
(d) In logs.



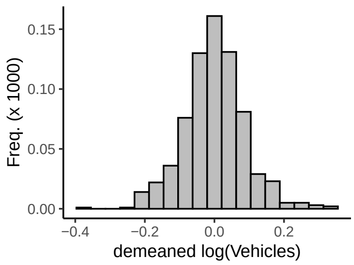
(e) In logs (locals).



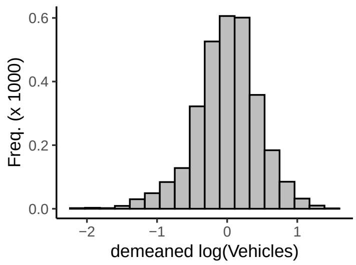
(f) In logs (roaming users).



(g) In logs demeaned.



(h) In logs demeaned (locals).



(i) In logs demeaned (roaming users).

Figure 6: Histograms of vehicles per month per province.

province containing Amsterdam, an obvious hot spot of growth in hotel nights, we see a similar pattern. This suggests that we can capture trends in VKT with hotel nights, albeit potentially overestimating changes in VKT.

Second, we analyse how traffic intensity and the number of vehicles involved in accidents are related to hotel nights for Dutch drivers, for which we observe traffic intensities on highways at the province-month level (Statistics Netherlands, 2019b). Table A5 in Appendix A shows that, after controlling for time and panel (in this case simply province) fixed effects, there is no statistically significant effect of hotel nights for Dutch nationals with respect to traffic intensity, or number of vehicles involved in accidents. Importantly however, we do find a statistically significant and robust effect for the case of roaming drivers and the number of vehicles involved in accidents. This suggests that hotel nights

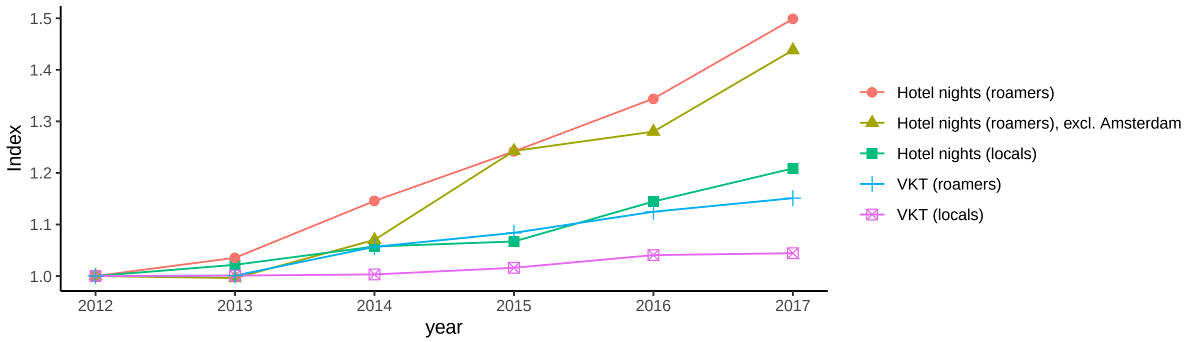
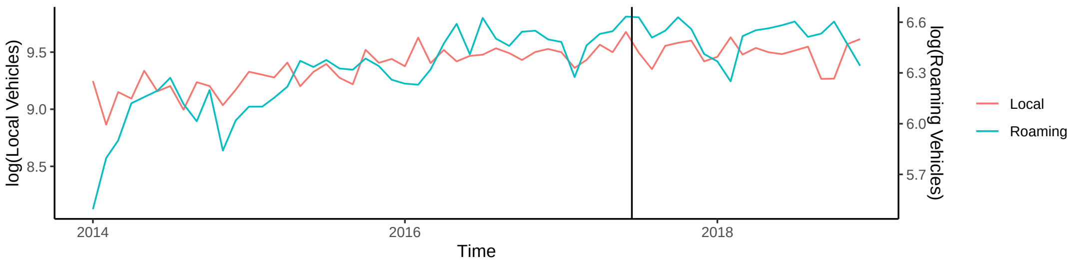


Figure 7: Trends in hotel nights and vehicle kilometers



(a) Roaming and local user vehicles in accidents.

Figure 8: Graphical representation of common trends in aggregated province-month data.

are a good proxy for country specific changes in VKT from tourism and business related trips. Furthermore, the R^2 in column (2) is 0.99, which indicates that almost all of the variation in the traffic intensity can be explained by our fixed effects, suggesting that group specific changes in traffic intensity are unlikely to effect our estimates.[26]

5. Results

5.1. *Parallel trends*

We first examine overall trends of local vehicles (control group) and roaming vehicles (treated group) involved in accidents. Figure 8a shows that nationwide accident counts for these groups follow similar trends. The figure also highlights that these measures are

[26]Note that we find a borderline significant (significant only at the 10% level) negative estimate for hotel nights of locals in column (8). This might be an indication that drivers who are staying in a hotel, are driving more safely because they are unfamiliar with the area. This would be in line with findings in observational studies. Another possible explanation could come from region specific holidays that vary in timing between years for given regions, and between regions for given years.

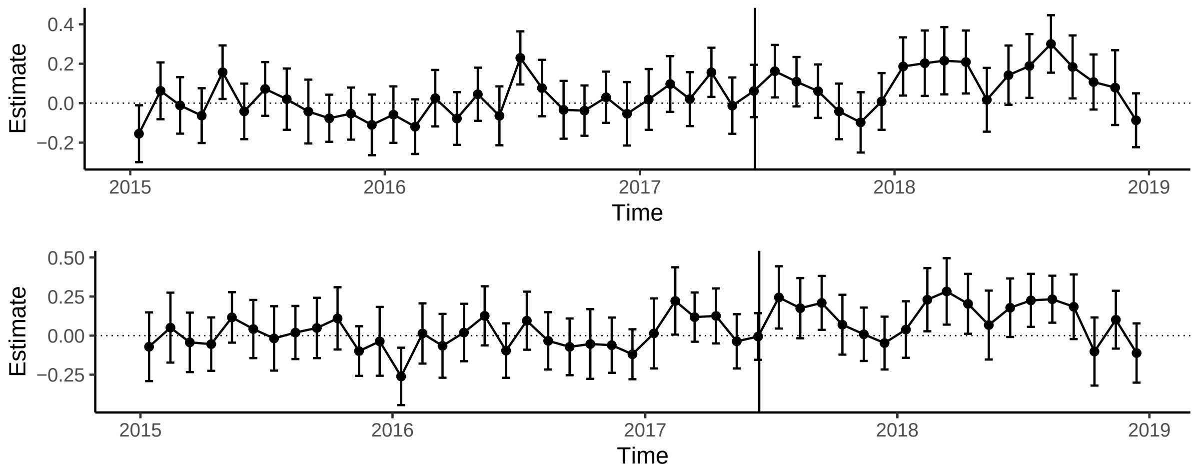


Figure 9: Treatment effect per month for full sample (top) and single-vehicles (bottom).

quite noisy and that no clear jump is observable around the policy introduction in 2017.

For a more rigorous analysis of a common trend, in Figure 9 we plot estimates of a monthly treatment effect, while including all controls and fixed effects as in our preferred specification in (1). Here, the coefficients are estimated using an indicator for whether the province-month count of vehicle accidents are for roaming users, interacted with year-month dummies.[27] The results in Figure 9 indicate that no clear pre-trend exists and that local drivers are a suitable control group for roaming drivers, conditional on controls and fixed effects. Furthermore, after the policy, there is a clear positive impact on accidents as indicated by the increased proportion of positive and statistically significant estimates.[28] This pattern is even more pronounced in the bottom panel of the plot where we focus specifically on single-vehicle accidents.

[27]Specifically, the figure plots the β_τ coefficients from estimating:

$$\log(V_{it}) = \sum_{\tau=-41}^{60} \beta_\tau R_{i,t-\tau} + \gamma \log(H_{it}) + \phi_i + \kappa_t + \epsilon_{it}, \tag{2}$$

where $R_{i,t-\tau}$ is an indicator variable for whether the vehicle count is for roaming users or not, interacted with a year-month dummy, and β_τ is the effect of the policy for each year-month t. To be able to include the seasonality fixed effect κ_t in this setting, we omit the treated×year-month dummies for the first full year; otherwise perfect multicollinearity emerges. The error bars represent robust 95% confidence intervals for each monthly point estimate.

[28]44% of the coefficients are positive and statistically significant post-policy as compared to only 10% pre-policy.

Table 2: Main regression results

	log(# Vehicles in Accidents)				
	(1)	(2)	(3)	(4)	(5)
Treatment effect	0.124***	0.125***	0.176***	0.089***	0.094***
	(0.034)	(0.035)	(0.025)	(0.030)	(0.031)
Roamer × trend				0.003**	
				(0.001)	
log(Hotel nights roamers)					0.298***
					(0.076)
log(Hotel nights locals)					-0.089
					(0.063)
Temperature		0.057***	-0.005*	-0.005*	-0.004*
		(0.016)	(0.003)	(0.003)	(0.002)
Rain		0.061	-0.001	-0.001	0.003
		(0.061)	(0.012)	(0.012)	(0.013)
# Frost days		0.157***	0.025*	0.025*	0.019
		(0.040)	(0.013)	(0.013)	(0.012)
Time FE		Yes	Yes	Yes	Yes
Panel FE			Yes	Yes	Yes
Clusters	72	72	72	72	72
Local vehicles	686k	686k	686k	686k	686k
Roaming vehicles	35k	35k	35k	35k	35k
Observations	3,688	3,688	3,688	3,688	3,688
R^2	0.729	0.748	0.965	0.965	0.967

Notes: Column (1) is a basic DiD regression which includes a dummy for roaming user, policy and the interaction between roaming user and policy (denoted treatment effect). Robust standard errors in parentheses are clustered at the province and country-group level. Hotel nights are split into two orthogonal variables for local and roaming users. A dummy is included when hotel nights were inflated (only for roaming users). ***, **, * indicate significance at 1%, 5%, and 10%.

5.2. *Estimation results*

Table 2 shows the estimation results with incremental levels of controls and fixed effects. Column (1) shows that with only the minimal DiD controls, we find a statistically significant effect of over 12%.[29] Column (2) shows that overall time trends (captured by year×month fixed effects), and weather controls hardly change the estimated treatment effect. In column (3) we add panel fixed effects, where our panel identifier is a province-country group. This increases the point estimates and lowers the standard errors, indicating that these fixed effects improve the efficiency of the estimator and suggests

[29]Here we run the most simple DiD regression, which includes a dummy for the RLAH policy, a dummy for whether the country group consists of roaming users, and the treatment effect is the interaction between these two dummy variables.

that accident counts are heterogeneous across provinces and country-groups. Column (4) shows that the estimated treatment effect declines significantly when we add a linear roaming-specific monthly time trend. This is potentially a bad control that can also pickup part of the treatment effect, but the results here imply that any major nationwide trends in accidents of roaming users only partially affect the results.

Our preferred specification is the one used in column (5), in which we include controls for hotel nights as a proxy for traffic intensity. We find a point estimate of 0.094 with a standard error of 0.031. This implies that the policy-induced increase in phone use leads to an increase in the number of vehicles involved in accidents of 9.91%, with a 95% confidence interval of $3\% - 17\%$. The point estimate declines as compared to (3) and the hotel nights elasticity of roaming users has the expected sign. It indicates that a 1% increase in hotel nights for roaming users is associated with an increase of around 0.3% in the number of vehicles involved in accidents. The hotel nights effect is insignificant for locals, conditional on our set of fixed effects. This makes sense as traffic intensity for roaming users is likely to follow seasonal tourist trends while most local traffic is generated by work commutes and other daily activities. Importantly, fixed effects already absorb overall trends in VKT, heterogeneity across provinces, and heterogeneity across vehicle countries. Therefore, statistical significance of the hotel nights elasticity, and the fact that the point estimate of the treatment effect is smaller when we include hotel nights, highlights that we indeed capture country specific long term trends in VKT.

5.3. *Robustness checks*

In this section we perform a vast range of robustness checks. Tables with results are available in Appendix B.

5.3.1. *Measurement error*

One type of measurement error arises because we do not accurately observe which vehicle potentially caused the accident. Table B1 in Appendix B shows estimation results using different subsets of accident types and vehicle involvement. Columns (1–2) show that focusing on different types of accidents yields very similar results. Excluding trucks and focusing on single-vehicle accidents leads to very similar or only slightly stronger point estimates. Focusing on single vehicle accidents may suggest we reduce measurement error slightly, but again the point estimates are not statistically different from the main estimate.

As discussed before, our analysis may suffer from measurement error in the treatment assignment, for instance by having a Dutch phone subscription while still driving a non-Dutch car or vice versa. It is likely that measurement error is most pronounced in bordering provinces and for drivers with a close connection to The Netherlands. This can either be due to proximity (like bordering regions or countries) or due to strong economic links (e.g. labour migration). If we exclude bordering countries, we find somewhat larger effects while if we remove bordering provinces or drivers from labour migration countries, we find only slightly smaller effects.[30] These results indicate that our results do not suffer from a severe downward bias from measurement error.

5.3.2. *Accounting for VKT trends*

So far, we have assumed that country-of-origin specific trends in VKT are well-captured by our hotel nights proxy. Results from Section 4 suggest that this is a plausible assumption. Nevertheless, to further rule out any issue with long-term trends in non-local road traffic as a potential confounder, in columns (1–2) of Table B2, we restrict our sample to one year before and one year after the policy (i.e. from June 2016 to July 2018). This approach yields an estimate of 6.8% for all vehicles and 14% for single-vehicle accidents which are very comparable to our main results. This highlights that long term trends in VKT cannot explain the observed increase in vehicles involved in accidents.

[30]Excluding border provinces also mitigates potential concerns that border provinces face more VKT due to the policy, e.g. if people are more likely to go shopping across the border because phone usage is cheaper. Such an endogenous response might induce sorting and thereby poses a threat to our identification strategy.

5.3.3. *Accounting for auto-correlation in error structure*

In our main analysis, we use the number of vehicles involved in accidents per province per month as observational unit. If there is strong serial correlation, then OLS standard errors may be incorrect, even when clustering at a time-invariant level as we do (Bertrand et al., 2004). To deal with this issue in the most conservative way, we re-estimate our main models on data aggregated to pre and post-policy averages.[31] Columns (3–4) in Table B2 show that the statistical significance is only slightly lower as compared to our main analysis (the t-statistic = 2.1 as compared to 3.1 in our preferred specification). This provides strong evidence that serial correlation does not pose a threat to our statistical inference.

5.3.4. *Weighting*

Our aim is to approximately recover the phone-use effect *per driver*, rather than at a province level. This suggests that we should use sample weights for VKT at the individual level.[32] Because these data are not available on the vehicle accident level, we test the robustness of our results to four weighting schemes that are closely related to VKT.[33] As regions differ in the total number of roaming drivers involved in accidents, this also allows to assign higher weights to provinces that tend to have relatively more roaming drivers and therefore may be more informative. Table B3 shows that our main results hardly change if we use weights based on 1) roaming accident numbers, 2) total accident numbers, 3) traffic intensity, and 4) hotel nights. This suggests that our fixed effects and log-level specification already sufficiently account for differences in VKT between regions.

[31] After aggregating, the data represents the log number of vehicles, hotel nights, and weather conditions, by country group and province, for an average month in the pre and post data.

[32] Note however that weighting might lead to erroneously small standard errors when there is clustering in the disturbances (Solon et al., 2015). Therefore, as the latter is likely to be the case in our setting, we are cautious with weights and report the more conservative estimates (without weighting) as main results.

[33] Note that for accident numbers we use the time invariant pre-policy number of roaming and total accidents.

5.3.5. *Accounting for zero counts*

In our main analyses we use a log-linear specification, which performs well with a sufficient number of accidents. However, during some months, for some country-groups, we observe few or even zero vehicles in accidents (14.02% pre and 4.95% post policy). These cases are naturally excluded from our log-linear regressions. However, they might be less likely to occur after the policy due to policy-induced phone distractions. As a consequence, our estimations might suffer from a slight downward bias by excluding more zero counts before than after the RLAH policy introduction for treated vehicles. To test if such a bias exists, we re-estimate our main specification as in (1) using a Poisson pseudo-maximum likelihood count model. Table B4 presents the results from this re-estimation, which allows us to include all province-month observations.[34] The coefficients are remarkably similar and in column (5), our preferred specification with hotel nights, the results indicate that the policy caused 9.4% more accidents and is statistically significant at the 1% level.[35]

5.3.6. *Heterogeneous effects*

In addition to the average treatment effect that we estimate in our main analysis, we test for measurable heterogeneity in the effect of phone use, for various subgroups of drivers and road characteristics.

We first test whether the effect size varies by age group. Table B5 in Appendix B suggests that our main effect predominantly applies to drivers in the age group between 30 and 50. We find statistically insignificant effects for age groups below 30 and above 50. However, as the 95% confidence intervals overlap, we cannot conclusively determine that the effects are statistically different, which might be due to less precision. Lab based studies also tend to be inconclusive on the performance differences of distracted driving across age groups. Oviedo-Trespalacios et al. (2016) synthesize the most recent literature, and find that although "older drivers tend to engage less in a secondary task like using

[34] This means we have 4,248 province-month observations as compared to 3,688 in column (5) of Table 2.

[35] Column (4) of this specification indicates that it indeed appears that the roamer specific time trend is a bad control, as could be expected.

mobile phones while driving [...], the performance of younger drivers, who are inclined to use a mobile phone while driving, has been reported to be less affected by mobile phone tasks than older drivers" (p. 369). It is therefore not surprising that many studies report a negligible effect of age differences.

We also investigate the treatment effect on different road types. Phone distractions may disproportionately impact the likelihood of causing an accident in more challenging road conditions, such as in urban areas and on local roads where drivers often share the road with other vehicles and modes (e.g. pedestrians and cyclists). To test this hypothesis, we split the sample into three road types based on the speed limit. To assure sufficient statistical power, we define the following three road classes with roughly equal numbers of accidents: below 50 km/h, between 50 km/h and 100 km/h, and above 100 km/h. These groups roughly represent local roads in urban areas, local roads in rural areas, and highways. Similarly, we test whether our estimates are different for vehicles involved in more severe accidents (fatal or injury) versus accidents with only material damage. Results of these estimations are presented in Table 3.

Columns (1–3) indicate that most of the estimated effect comes from local roads, and we do not find evidence of a reduction in road safety on highways. This suggests that phone distractions are either more risky on local roads (e.g. due to crossings and traffic lights), or that drivers use their phone less frequently on highways (e.g. because it is perceived as more dangerous).[36]

Finally, columns (4–5) indicate that the main result holds, regardless of accident severity, suggesting that mobile phone distractions play an important role in accidents with varying degrees of severity. Our results do not support the hypotheses that phone distractions predominantly increase accidents with material damage, for instance, if people mostly use phones in low-speed, low-risk, situations like traffic jams.

[36] We cannot fully isolate the effect of phone usage from that of increased car navigation, but the fact that we find only an effect on urban roads may indicate that car navigation does not increase safety in urbanized areas.

Table 3: Estimation results using subsamples of road types and severity).

	log(# Vehicles in Accidents)				
	< 50km/h	50km/h - 100km/h	>100km/h	Fatal/Injury	Material
	(1)	(2)	(3)	(4)	(5)
Treatment effect	0.098**	0.050	-0.065	0.115**	0.083**
	(0.038)	(0.040)	(0.049)	(0.054)	(0.033)
log(Hotel nights roamers)	0.178***	0.260***	0.220***	0.130**	0.294***
	(0.058)	(0.078)	(0.045)	(0.051)	(0.075)
log(Hotel nights locals)	-0.031	0.004	-0.125	0.214***	-0.144**
	(0.054)	(0.065)	(0.116)	(0.077)	(0.067)
Weather controls	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes
Panel FE	Yes	Yes	Yes	Yes	Yes
Weather controls	Yes	Yes	Yes	Yes	Yes
Clusters	72	72	72	70	72
Local vehicles	368k	135k	101k	133k	554k
Roaming vehicles	14k	8k	9k	3k	32k
Observations	3,083	2,796	2,818	2,136	3,636
R^2	0.964	0.955	0.934	0.961	0.965

Notes: Robust standard errors in parentheses are clustered at the province and country-group level.***, **, * indicate significance at 1%, 5%, and 10%.

5.4. *Implications*

Our robustness checks indicate that the effect of phone use generally falls within the 95% confidence interval $3\% - 17\%$ of our main estimate. Furthermore, 9.91% is likely to be a conservative estimate of the total effect of phone use because we only estimate the effect induced by the price drop, while roaming users where likely to use their phones, albeit infrequently, prior to the policy. In this section we calculate the total number of accidents and the relative risk of phone use implied by our main estimate.

5.4.1. *Total number of accidents caused by phone use*

To calculate the number of accidents associated with phone use, we compare the observed number with a counter-factual situation where all drivers face phone usage fees equal to the pre-policy roaming charges. In other words, we consider how many accidents could be avoided if all drivers faced higher phone usage costs and thereby used their phones less.[37]

[37] This could be enforced, for example, by imposing more stringent regulation that increases the costs of being caught using a mobile phone while driving.

Importantly, the RLAH policy abolished additional roaming surcharges, such that after the policy, roaming and local users face the same phone use costs and accident risk.[38]

Our results are applicable to all road users under the assumption that the mechanism identified for roaming users carries over to all drivers. In other words, the average treatment effect for roaming users (ATET) should be representative of the average treatment effect (ATE) for all road users. Extrapolating the ATET to the ATE therefore requires the key assumption that the treatment and control group are sufficiently comparable.

Based on observable driver characteristics, roaming users tend to be younger and drive on faster roads than local drivers (see discussion in Section 4.2). Nevertheless, our analysis on heterogeneous effects across age groups suggests that differences in driver age leads to similar results, while highways tend to be safer than local roads with respect to the accident risk of phone distractions (see Table A3 in Appendix A). Therefore, based on observable characteristics, our ATET may in fact underestimate the ATE because roaming users are more likely to drive on highways.

One remaining concern might be that unobservable driver characteristics, such as familiarity with roads and other infrastructure makes roaming and local drivers not comparable. For instance, if driving on unfamiliar roads increases accidents risk, then this may be further exacerbated by phone distraction. However, Intini et al. (2018) find no clear evidence for increased accident risk due to unfamiliarity with the road network. On the contrary, they find that familiarity is associated with increased accident risk. More research is required to understand the interaction between driver distractions and road familiarity, but at this stage there seems to be no clear indication that our ATET overestimates the ATE due to road familiarity.

In sum, it seems plausible that our estimated ATET is roughly similar to the ATE. Our results then imply that phone use causes 13,563 additional accidents annually in

[38]In other words, the RLAH policy caused roaming drivers to 'catch up' with local drivers' smart phone usage and the distractions and associated accident risk. There may still be variations across mobile phone plans and across countries, but these no longer depend on roaming or local use. In addition, these differences are most likely fairly constant over time in the short run and are more related to local demand and supply conditions than to the RLAH policy.

the Netherlands, of which about 2,536 result in injury and 79 are fatal. Furthermore, if the ATE is applicable to other EU countries, this would imply that around 2,500 road fatalities in the EU in 2018 may be attributable to phone use.[39] As shown in Figure 1, the gap between the EU 2020 target and actual fatalities was 28% (7,044 cases). Our results then suggest that around one third of this gap could be closed by successfully banning mobile phone use qhile driving.

5.4.2. *External effect*

We do a back-of-the-envelope calculation to calculate the external effect based on average accident characteristics. Let us assume that in each accident just *one* driver was potentially causing the accident due to phone distraction. Then, out of 764k drivers involved in accidents in our data, 334.89k (43.8%) of them were involved in a crash without contributing to the cause of the accident themselves. If we focus on local roads—where we find the strongest effect of distraction—we find a similar figure of 43.9%.

We use these figures to calculate a simple external effect of phone use, expressed in terms of vehicles involved in accidents. Starting with our main estimate of a 9.91% increase in vehicles involved in accidents due to phone distractions, we calculate that in all accidents, on average about 4.1% of vehicles were affected due to distraction of *other* drivers. Note that this calculation crucially hinges on the assumption that in each phone-induced accident only *one* driver was distracted. This may seem plausible, but may be violated in rare cases.

5.4.3. *Crash risk odds ratio*

We follow Bhargava and Pathania (2013) and translate our estimate for the effect of the change in mobile phone use, due to the RLAH policy, on the number of vehicles involved in accidents to the crash risk odds ratio (or 'relative risk') which allows us to compare our results to the existing literature. This requires two key parameters, the percentage of roaming users that are on their phone while driving or the 'baseline prevalence', and

[39] This can be calculated by multiplying our main estimate by the total number of fatal vehicle accidents in 2018, so 9.91% × 25,058 = 2,470.

the change in phone use due to the policy, denoted by b and c respectively.

Observational studies, based on roadside surveys, indicate that average phone use in the car ranges between $1-11\%$ (European Road Safety Observatory, 2015).[40] These field studies do not distinguish between roaming and local drivers, however there is a good reason to expect that the baseline prevalence is overestimated for roaming users because roaming was very costly before RLAH. Therefore we consider a range of $\mathrm{b} \in [0.01, 0.10]$, in the sensitivity analysis, but note that lower values are more likely.

As for the increase in phone use due to the policy, Table A4 suggests that RLAH induced an increase in the annual growth rate of mobile data of around 200 percentage points, and calls and texts of around 80 and 20 percentage points, respectively. We assume that aggregate changes in roaming use also apply to drivers visiting the Netherlands and consider a range of $\mathrm{c} \in [0.5, 2]$. It is possible that most of this 200 percentage points increase comes from watching videos and playing songs, which may not (fully) translate to an equivalent increase in distractions while driving. This would imply that the lower values in the specified range for c are more relevant and more applicable to our setting.

Using these parameters, we can calculate a range of possible relative risk factors, denoted by RR, implied by our preferred estimate, $\hat{\beta}$, using the formulation:

$$\hat{\beta}[1 \times (1-b) + RR \times b] = RR \times bc - bc. \tag{3}$$

To reflect the uncertainty of these assumptions, Table 4 illustrates how our key parameters influence the implied RR estimates. It indicates that RR is decreasing in the baseline prevalence and in the change in phone use due to the policy. In other words, if the policy had a small impact on phone use and roaming drivers used their phone very little prior to the policy, our estimate implies larger risks associated with phone use while driving.

[40] Based on a naturalistic driving setting between 2012 and 2015, Dingus et al. (2016) observe handheld cell phone prevalence in the US to be about 6.3%. There is no reason to expect that prevalence is substantially different in the Netherlands and therefore we expect that the findings in European Road Safety Observatory (2015) captures a meaningful range for our study.

That said, we take a conservative estimate for the baseline prevalence of 3% and the change in phone use due to the policy of 100%. This would imply a relative risk of phone use of 3.8.[41] We consider this to be a conservative estimate as it is unlikely that roaming drivers used their phones as intensively as local drivers due to the high pre-policy roaming costs.

Table 4: Sensitivity of implied accident risk.

	Baseline prevalence, b				
Δ phone use due to RLAH, c	1%	2%	3%	5%	10%
50%	17.10	8.90	6.20	4.00	2.30
80%	11.70	6.30	4.40	3.00	1.90
100%	9.80	5.30	3.80	2.60	1.70
150%	7.00	4.00	2.90	2.10	1.50
200%	5.60	3.30	2.50	1.80	1.40

Notes: This table presents the relative accident risk implied by our baseline estimate from column (5) in Table 2. The relative risk is calculated by re-arranging equation (3) such that: $RR = \frac{\hat{\beta}-\hat{\beta}b+bc}{b(\hat{\beta}+c)}$. Baseline prevalence reflects the percentage of time roaming drivers spend on the phone while in the car. An illustration is outlined in the text.

Comparing these estimates to the existing literature suggests that our conservative estimate of the crash risk associated with modern smart phone usage is similar to earlier crash-based studies, but are significantly larger than recent field studies.[42] This suggests that the crash risks of phone use are slightly lower in magnitude than those found for positive levels of blood alcohol.[43] As mentioned earlier, previous research focuses mainly on the effects of calling, or focuses on specific road types and phone use, however modern smart phones offer substantially more usability and potential for distraction, and our findings suggest that these effects are more likely to be present on local urban roads. Our estimates for the change in mobile phone use due to the RLAH policy suggest that we mainly pick up an effect from using more mobile data (increase in growth rate of about 200 percentage points as compared to local drivers) which may explain why we find larger

[41] Re-arranging terms, we can find $RR = \frac{\hat{\beta}-\hat{\beta}b+bc}{b(\hat{\beta}+c)}$. Plugging in $b = 0.03$ and $c = 1$ gives: $RR = 3.8$.

[42] Redelmeier and Tibshirani (1997) find a RR of about 4.3, Dingus et al. (2016) find the RR of cell phone use to be 3.6, and Bhargava and Pathania (2013) do not find any effect. Hersh et al. (2019b) do not calculate the RR, however their main estimate of 1.1% is far lower than our main estimate of 9.91%.

[43] Levitt and Porter (2001a) finds a crash risk of 7 and 13 for positive levels of blood alcohol and illegal levels respectively.

implied relative risk estimates than some earlier field studies.

6. Conclusion

In this research we provide novel evidence on the effect of cell phone use on car accidents. We exploit variation in the cell phone usage fees in the Netherlands following the *Roam Like at Home* (RLAH) policy introduced by the European Union (EU) in 2017. This intervention is used as a treatment, and applies to roaming users—non-Dutch drivers from the EU—which allows us to employ a difference-in-differences approach.

We show that the growth rate of mobile calls, texts, and particularly data usage increased substantially after the change in roaming regulations, making roaming phone use more in line with usage in home countries. While we do not directly observe actual phone use of drivers, the observed increase in usage is likely to (partly) carry over to phone use while driving. We estimate that increased phone use due to the policy causes an increase in the number of vehicles involved in accidents of 9.91% (95% confidence interval $3\% - 17\%$), which is the average treatment effect on the treated (ATET). This is likely to be an underestimate of the *total effect* of phone use while driving, as our estimates capture the effect of an *increase* in phone use, which was not fully absent before the policy.

Under the assumption that the identified mechanism carries over to all EU drivers, our estimate implies that, in 2018, around 2,500 road fatalities in the EU could be attributed to phone use. Our results then suggest that around one third of the gap between realised safety improvements on roads and the EU 2020 target can be attributed to mobile phone use.[44]

Our findings indicate that the existing literature may underestimate the risks associated with modern smart phone usage while driving. Our main result implies a crash risk odds ratio associated with mobile phone use of around 3.8, which is likely to be a conservative estimate. All in all, our results suggest that smart phones are making roads

[44] In 2018, the EU was 28% away from their 2020 target (see Figure 1).

less safe, and this has important implications for road safety policies.

Our paper provides an estimate of the *average* effect of smart phone usage on the number of vehicles involved in traffic accidents, which may conceal considerable differences between specific groups of drivers. We look into heterogeneous effects by estimating models for different sub-samples (e.g. for different age groups, or excluding trucks). Future research could delve into this further, by estimating propensities of specific groups of drivers to use their phone while driving. Ride hailing drivers, for example, may have a relatively high propensity to be distracted by their phone, which might be an important factor in explaining the results of Barrios et al. (2020), who find that ride hailing services increased the number of traffic accidents in the US. Such evidence could provide valuable input for related regulation and policies.

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A. Additional descriptives

Table A1: Descriptive statistics: Vehicles in accidents.

Statistic	N	Mean	St. Dev.	Min	Max
Roaming	764,065	0.046	0.210	0	1
Age	561,136	42.488	15.015	0.000	110.000
Female	764,065	0.455	0.707	0	10
Maximum speed (km)	653,055	63.726	26.823	15.000	130.000
Deadly	764,065	0.006	0.076	0	1
Injury	764,065	0.187	0.390	0	1
Material	764,065	0.807	0.394	0	1

Table A2: Descriptive statistics by group: Vehicles in accidents.

Variable	Roaming	Local	Diff	Tstat
Age	40.903	42.566	-1.663	18.998
Female	0.383	0.459	-0.075	21.007
Maximum speed (km)	74.511	63.200	11.312	-62.898
Deadly	0.006	0.006	-0.000	0.503
Injury	0.088	0.192	-0.103	65.301
Material	0.906	0.802	0.104	-63.736

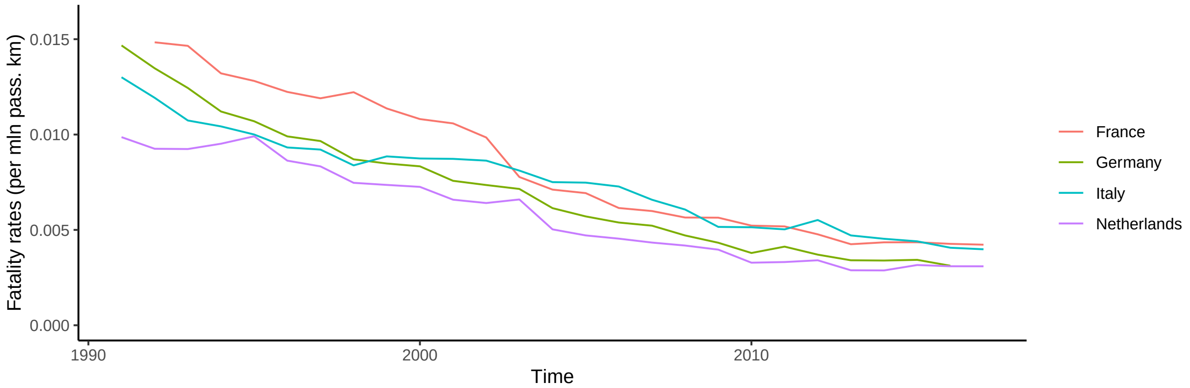

Figure A1: Fatality rates in road accidents over time in major EU countries and the Netherlands

Table A3: Relative frequencies of road types by severity

	Local roads	Major roads	Highways
Fatal/injury	14.1 %	4.3 %	2.0 %
Material damage	46.8 %	17.9 %	14.8 %
Total	60.9 %	22.2 %	16.9 %

Table A4: Difference-in-differences in annual growth of phone use.

		Annual growth rate (%)		Δ Annual growth rate (p.p.)	
Usage	User	Pre	Post	Diff	DiD
Calls	Local	4.29	-2.49	-6.78	
Calls	Roaming	-1.06	71.16	72.21	78.99
Data	Local	67.05	83.82	16.76	
Data	Roaming	68.09	285.89	217.80	201.04
Texts	Local	-18.24	-1.80	16.45	
Texts	Roaming	-22.01	18.37	40.38	23.93

Notes: Pre-policy refers to the the average annual growth rates of cellular traffic comparing each quarter with the same quarter in the previous year, over three years (Q1 2014 − Q1 2017) prior to the implementation of RLAH. Post-policy is one year, Q2 2017 − Q1 2018, after RLAH.

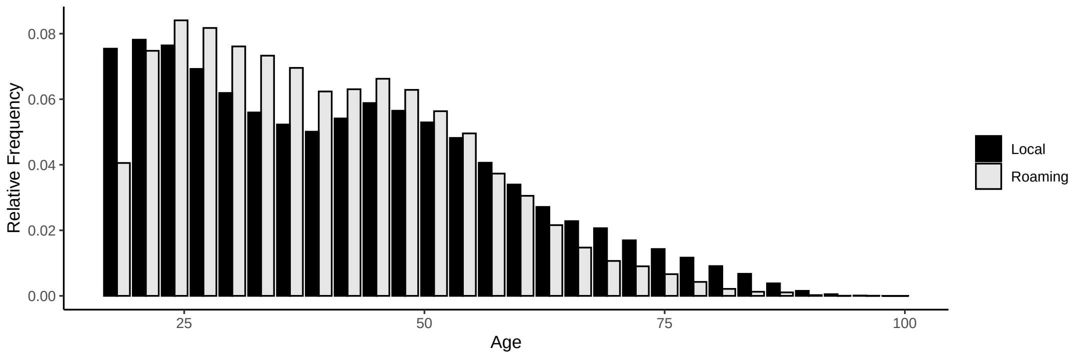

Figure A2: Age of local and roaming users.

A.1. *Predicting phone use per subscriber pre-2016*

We obtain roaming usage data from the EU Body of European Regulators for Electronic Communications (BEREC). Their reports include the time series "EEA average consumption per month per total number of roaming subscribers (in GB)" from the second quarter of 2016 onwards. Therefore, in order to get a better picture of the long term changes in roaming data usage, we use data on the total "EEA Retail data traffic (millions of GB)"

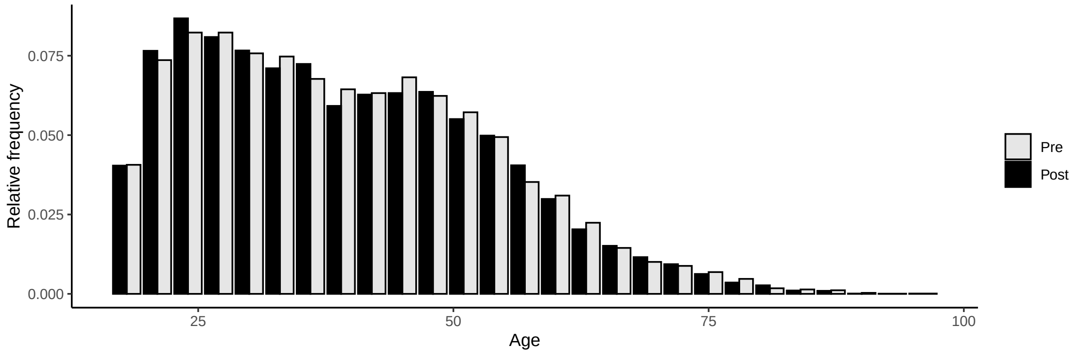

Figure A3: Age of roaming users pre and post policy.

(available as of 2007) and predict the number of subscribers in earlier periods using a simple model. The advantage of this approach is that the number of subscribers appears to follow a rather simple dynamic process and means that we only need to predict the denominator. We can then also compare the growth in our metric to the total growth in mobile data use which gives us more confidence that the predictions are as close as possible to actual figures.

We observe quarterly data on the number of roaming subscribers from the second quarter of 2016 until the first quarter of 2019. The top panel in Figure A4 indicates that the number of subscribers appears to follow a somewhat log-linear growth trend with a strong seasonal pattern which is likely related to summer tourism. We therefore estimate the number of subscribers using the following regression equation:

$$\log(S_t) = \gamma \text{Trend}_t + \phi_{q(t)} + \epsilon_t, \tag{4}$$

where $\log(S_t)$ is the natural logarithm of the number of subscribers, Trend_t is a linear time trend capturing the growth over time, and $\phi_{q(t)}$ are quarter dummies that capture seasonal variations. The resulting model has an $R^2 = 0.92$, which suggests that it captures the vast share of roaming subscriber dynamics. This is further confirmed by the bottom panel of Figure A4 which compares the actual and predicted number of subscribers and the resulting calculation of data roaming per subscriber. Finally, Figure A5 compares the difference between growth in roaming data per subscriber and the total roaming data use. While the trends are almost identical, it indicates a larger growth in total data use which is likely a result of capturing overall trends in growth in subscribers (which is relatively constant) and may also be a result of the RLAH policy that causes the number of people actively using roaming while travelling to increase. Overall, it suggests that the predicted change in data usage is a conservative estimate of the effect of the policy.

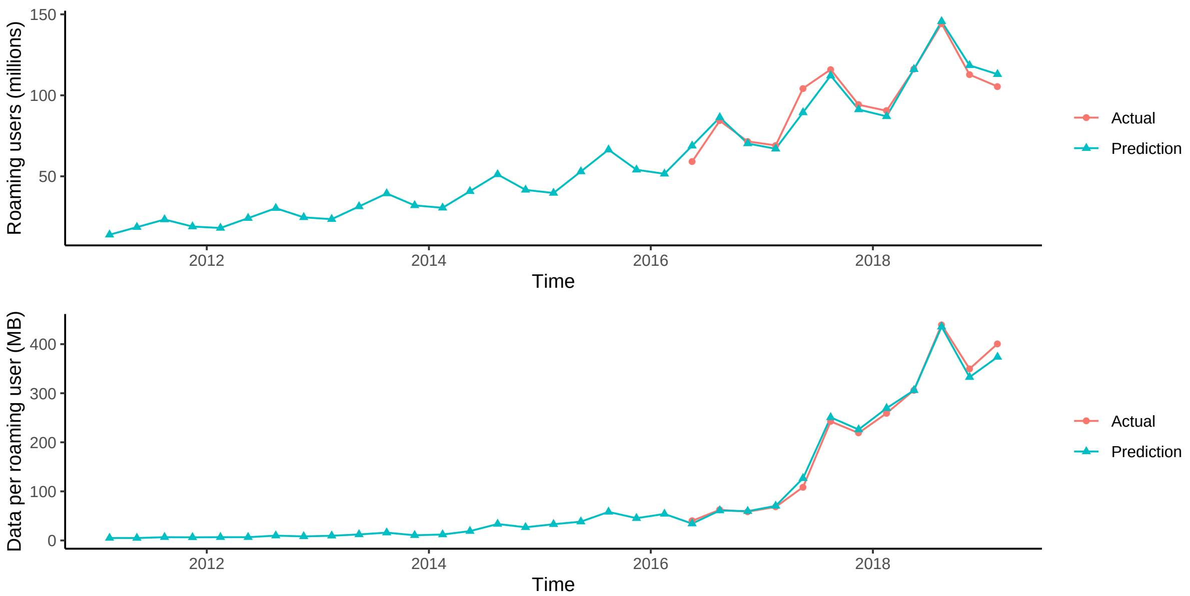

Figure A4: Predicting number of EU roaming subscribers and data consumption

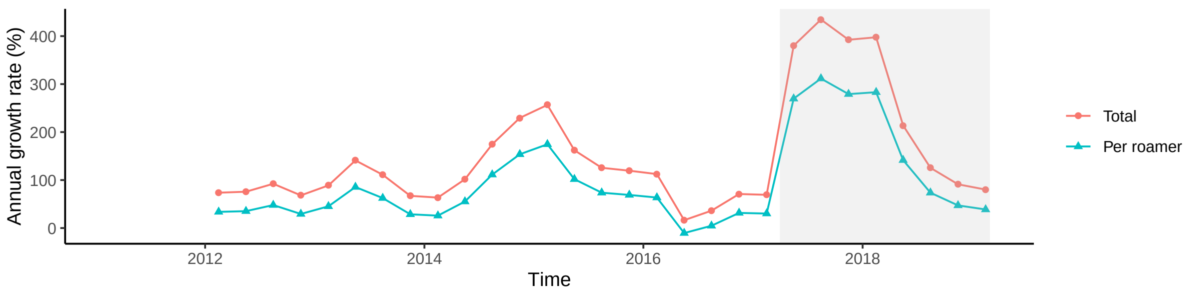

Figure A5: Growth in roaming data usage per subscriber as compared to the total

A.2. *Analysis of hotel nights as proxy for vehicle kilometres travelled*

Table A5: Regression results for analysing traffic and hotel nights for Dutch drivers.

	log(Traffic intensity)		log(# Vehicles in accidents)					
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
log(Hot. loc.)	0.240**	0.042	0.786***	0.109			0.628***	-0.135*
	(0.109)	(0.065)	(0.117)	(0.083)			(0.069)	(0.075)
log(Hot. roam.)					0.287***	0.318***	0.302***	0.306***
					(0.086)	(0.077)	(0.082)	(0.079)
Time FE		Yes		Yes		Yes		Yes
Panel FE		Yes		Yes		Yes		Yes
Subsample	Loc.	Loc.	Loc.	Loc.	Roam.	Roam.	All	All
Within R2	0.232	0.016	0.660	0.023	0.242	0.045	0.806	0.046
Observations	576	576	576	576	3,032	3,752	3,752	3,752
R^2	0.232	0.992	0.660	0.990	0.242	0.966	0.806	0.966

Notes: Robust standard errors in parentheses are clustered at the province and country-group level.***, **, * indicate significance at 1%, 5%, and 10%.

B. Robustness checks and sensitivity analyses

Table B1: Results correcting for sources of measurement error

	log(# Vehicles in Accidents)				
	No trucks	SV	No border prov.	No border countr.	No BG/PL/RO
	(1)	(2)	(3)	(4)	(5)
Treatment effect	0.086**	0.096***	0.072*	0.134***	0.077**
	(0.034)	(0.033)	(0.037)	(0.030)	(0.034)
log(Hotel roam.)	0.297***	0.147**	0.424***	0.132***	0.335***
	(0.093)	(0.056)	(0.121)	(0.037)	(0.084)
log(Hotel loc.)	-0.117*	-0.167***	0.087	-0.038	-0.071
	(0.061)	(0.054)	(0.082)	(0.072)	(0.064)
Weather controls	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes
Panel FE	Yes	Yes	Yes	Yes	Yes
Clusters	72	72	30	48	60
Local vehicles	535k	136k	356k	686k	686k
Roaming vehicles	22k	6k	12k	15k	26k
Observations	3,303	2,658	1,523	2,458	3,026
R^2	0.966	0.962	0.968	0.977	0.972

*Notes:*Robust standard errors in parentheses are clustered at the province and country-group level.***, **, * indicate significance at 1%, 5%, and 10%.

Table B2: Results using only one year pre/post (1–2), and data aggregated to two periods (3–4).

	log(# Vehicles in accidents)			
	All	Single vehicle	All	Single vehicle
	(1)	(2)	(3)	(4)
Treatment effect	0.065**	0.131***	0.148**	0.162*
	(0.029)	(0.036)	(0.070)	(0.091)
log(Hotel nights roamers)	0.316***	0.192***	0.114	-0.082
	(0.094)	(0.064)	(0.094)	(0.109)
log(Hotel nights locals)	0.036	-0.053	0.148	-0.037
	(0.069)	(0.078)	(0.424)	(0.409)
Weather controls	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes
Panel FE	Yes	Yes	Yes	Yes
Clusters	72	72	72	72
Local vehicles	319k	59k	23k	4k
Roaming vehicles	18k	3k	1k	0k
Observations	1,593	1,162	144	143
R^2	0.969	0.962	0.999	0.998

Notes: Robust standard errors in parentheses are clustered at the province and country-group level. Columns (1–2) are obtained using data from June 2016 until July 2018. Columns (3–4) are obtained after aggregating the data into two periods, one before the policy and one after. For interpretation purposes, after aggregation, variables are then rescaled to their initial units (e.g. monthly averages).***, **, * indicate significance at 1%, 5%, and 10%.

Table B3: Regression results using weighted least squares.

	log(# Vehicles in Accidents)			
	(1)	(2)	(3)	(4)
Treatment effect	0.111***	0.128***	0.110***	0.101***
	(0.030)	(0.033)	(0.028)	(0.037)
log(Hotel nights roamers)	0.196***	0.185***	0.255***	0.256***
	(0.061)	(0.057)	(0.059)	(0.092)
log(Hotel nights locals)	-0.212***	-0.173*	-0.083	-0.221*
	(0.078)	(0.094)	(0.066)	(0.118)
Weather controls	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes
Panel FE	Yes	Yes	Yes	Yes
Weights	Total veh.	Roaming veh.	Avg traf. intens.	Avg hotel nights
Clusters	72	72	72	72
Local vehicles	686k	686k	686k	686k
Roaming vehicles	35k	35k	35k	35k
Observations	3,688	3,688	3,688	3,688
R^2	0.970	0.969	0.966	0.968

Notes: Estimated using weighted least squares, with pre-policy total number of (roaming) vehicles as weights. Robust standard errors in parentheses are clustered at the province and country-group level.***, **, * indicate significance at 1%, 5%, and 10%.

Table B4: Estimation results using Poisson regression.

	# Vehicles in Accidents				
	(1)	(2)	(3)	(4)	(5)
Treatment effect	0.186***	0.186***	0.187***	0.0381	0.0899***
	(0.0234)	(0.0210)	(0.0223)	(0.0262)	(0.0298)
Roamer × trend				0.00520***	
				(0.000921)	
log(Hotel nights roamers)					0.335***
					(0.0709)
log(Hotel nights locals)					-0.000314
					(0.0689)
Temperature		0.0366	-0.00100	-0.00101	-0.00108
		(0.0264)	(0.000852)	(0.000851)	(0.000850)
Rain		0.198**	0.0134***	0.0132***	0.0139***
		(0.0788)	(0.00447)	(0.00443)	(0.00452)
# Frost days		0.0571	-0.000380	-0.000526	-0.000896
		(0.0631)	(0.00301)	(0.00297)	(0.00352)
Time FE	No	Yes	Yes	Yes	Yes
Panel FE	No	No	Yes	Yes	Yes
Clusters	72	72	72	72	72
Local vehicles	686	686	686	686	686
Roaming vehicles	35	35	35	35	35
Observations	4,248	4,248	4,248	4,248	4,248

Notes: Robust standard errors in parentheses are clustered at the province and country-group level.***, **, * indicate significance at 1%, 5%, and 10%.

Table B5: Estimation results for subsamples with different age groups.

	log(# Vehicles in Accidents)					
	All	Age ≤ 30	30 <Age< 50	Age≥ 50	Age≥ 65	Age unknown
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment effect	0.094***	0.039	0.099**	0.058	0.055	0.193**
	(0.031)	(0.038)	(0.039)	(0.035)	(0.051)	(0.087)
log(Hotel nights roamers)	0.298***	0.201***	0.200***	0.204***	0.027	0.181**
	(0.076)	(0.039)	(0.050)	(0.064)	(0.049)	(0.072)
log(Hotel nights locals)	-0.089	0.058	-0.046	0.016	0.063	-0.238
	(0.063)	(0.070)	(0.080)	(0.069)	(0.096)	(0.166)
Weather controls	Yes	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes	Yes
Panel FE	Yes	Yes	Yes	Yes	Yes	Yes
Clusters	72	72	72	72	62	72
Local vehicles	686k	189k	221k	172k	59k	104k
Roaming vehicles	35k	7k	12k	6k	1k	9k
Observations	3,688	2,638	3,072	2,572	1,422	2,822
R^2	0.967	0.959	0.954	0.959	0.970	0.924

Notes: Robust standard errors in parentheses are clustered at the province and country-group level.***, **, * indicate significance at 1%, 5%, and 10%.