Cheap talk: The effect of smartphone usage on road safety*

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We provide novel evidence on the effect of cell phone use on car accidents. We exploit variation in cell phone usage fees in the Netherlands following European Union (EU) roaming regulations in 2017, which abolished all roaming surcharges for EU citizens. This unique price change allows us to estimate a difference-in-differences model where non-Dutch drivers from the EU are treated, while local drivers are not affected and hence can be used as a control group. We first document a substantial increase in the growth rate of mobile data usage by 200 percentage points and of calls and texts by around 40 percentage points. We find that this increase in phone use due to the policy causes the number of accidents to increase by around 16%. Under plausible assumptions, this implies a relative risk or odds ratio of at least 5.3. It is plausible that this mechanism roughly carries over to local drivers. Our point estimates then imply that each year in the Netherlands as many as 19,487 road accidents are associated with phone use, of which about 3,644 result in injury and 113 are fatal.

Keywords: road safety, accident risk, cell phones
JEL Codes: I18, J24, K32, R41

1. Introduction

In 2018, 25,000 road users lost their lives due to traffic accidents in the European Union (EU). For every death on European roads, there are an additional 4 permanently disabling injuries, 8 serious injuries and 50 minor injuries (European Commission, 2018). Next to physical harm, accidents also cause psychological harm to those directly involved and to friends and relatives of the victims (Brom et al., 1993). Furthermore, traffic accidents lead to monetary losses due to damages to private and public property, and are a major cause of traffic congestion. In total,

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the economic costs of traffic accidents in the EU are estimated to be about €280 billion, or 2% of GDP (European Commission, 2019).

These substantial negative costs provide governments with a strong rationale to prioritise safety in road design, and traffic and vehicle related regulation. Safety concerns in this respect partly shape policy decisions on aspects such as speed limits, road geometry, obligatory usage of seatbelts, and factors that affect the ability of road users maintain attention on the driving task. This includes prohibiting driving under influence and cell phone usage by drivers. However, as shown in Figure 1, despite substantial reductions in fatality rates over the past few decades, progress in terms of fewer road fatalities, in line with the EU policy target, began to diverge since 2013.

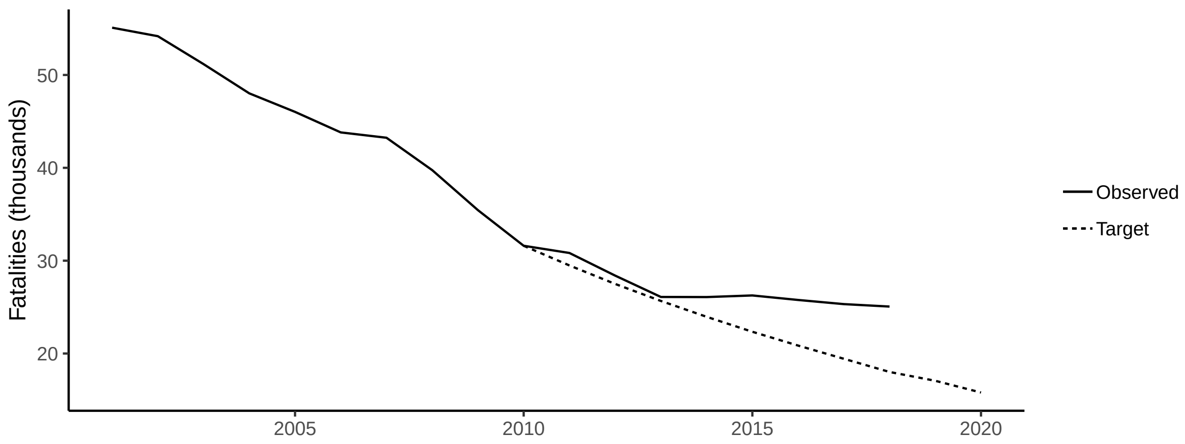

Figure 1: Road fatalities in the EU and 2020 policy target

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 has transformed cell phones into an omnipresent device that can be a major cause of distraction in traffic. Novel distractions that smartphones entail include sending text messages via messaging apps, news updates, taking pictures, and receiving notifications from various social media platforms. Therefore, an important and ongoing research question is *to what extent does mobile phone usage of car drivers affect the number and likelihood of traffic accidents.*

Numerous studies have investigated the link between phone use and accidents, yet despite this vast literature, which we discuss in more detail in Section 2, a substantial research gap prevails.[1] Firstly, mobile phone use has dramatically changed since the turn of the century in terms of adoption, exposure and capabilities. For example, in seminal studies by Redelmeier and Tibshirani (1997), Laberge-Nadeau et al. (2003) and McEvoy et al. (2005), only 18%, 25%, and 72% of drivers owned mobile phones respectively, as compared to nearly 100% today.[2] The adoption of smart phones has also drastically affected our exposure to and use of mobile phones. While previous research mainly focuses on the effects of mobile phone use in terms of of calls and texts, smart phones present an increasing number of potential distractions: such as incoming video calls, text messages and various other applications providing frequent notifications.[3] Secondly, many studies do not account for unobserved factors that may be correlated to both phone use and accident likelihood. Finally, as sample sizes were often small in previous studies, generalisation to aggregate effects is often problematic.

We propose an alternative approach based on field data and a natural experiment induced by a change in EU roaming regulations. The policy, imposed in June 2017, mandated mobile phone operators to abolish all roaming charges for EU customers 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, with mobile data use of roamers growing by over 200 percentage points more than locals who were not affected by the policy and faced stable growth rates. 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 users.

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 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 study contributes to the existing literature in four ways. First, our results provide a causal estimate of phone use on road safety based on a novel method. Second, 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. Third, because our identifying

[1] See e.g. the reviews by Dragutinovic and Twisk, 2005; Young et al., 2007; WHO, 2011; Lipovac et al., 2017.
[2] Mobile cellular subscriptions per capita have been above one in the world since 2016 (World Bank, 2019).
[3] Drivers may also be able to navigate streets more easily using navigation applications.

variation comes from a very recent policy intervention, our estimates take modern distractions of smart phones, and particularly changes in mobile data use, into account. This is especially relevant given the urgency of the road safety issues, and the rapid growth in cellular traffic. Fourth, 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 other contexts in future research.

Our findings imply that the increase in phone use due to the policy causes the number of accidents to increase by 15.81%. Under plausible assumptions, this implies a relative risk or odds ratio of at least 5.3. It is plausible that this mechanism also carries over to local drivers. Our results then imply that each year in the Netherlands as many as 19,487 road accidents are associated with phone use, of which about 3,644 result in injury and 113 are fatal.

The rest of this paper is structured as follows. Section 2 gives an overview of the existing literature. Section 3 describes the data we use and presents descriptives statistics. Section 4 explains the methods employed. Section 5 discusses the results and robustness checks. Section 6 concludes.

2. Related literature

Experimental approaches have shown that phone use causes visual, cognitive, and physical distractions. These distractions result in longer reaction times, inability to maintain correct lane positioning and appropriate speed, reduced field of view, less awareness, increased mental workload, and restrict full control of the vehicle. While these experimental studies have the advantage that they can directly observe the mechanism through which phone use affects driving performance, the lab setting and small sample sizes make accurately quantifying the role of the relevant mechanism in an accident and generalizing the results to other settings difficult. Observational studies in naturalistic settings generally corroborate findings from the lab. However, these studies also suffer from a lack of generalisability, are based on small samples and do not quantify actual effect sizes.[4] One important caveat of both types of studies is that drivers may adjust behaviour in response to being monitored.[5]

[4]For example, the largest study in a naturalistic setting was still limited to 905 crash events due to the large costs of collecting data (Dingus et al., 2016).

[5]Exceptions are studies carried out at fixed locations with hidden observers, but these studies are often not focusing on road safety. For instance, Huth et al. (2015) find that phone use induces delays at traffic lights using

Crash-based studies have the advantage that the effect of phone use on accident likelihood can be measured. However, these studies may suffer from under-reporting as most studies rely on the content of police reports and drivers may be reluctant to admit that they were using their phone before the accident happened. To get around this issue, several crash-based epidemiological studies have aimed to quantify the effect of phone use on accidents using various *artificial* control group methods. In an early study of this kind, Violanti and Marshall (1996) find that that drivers using a mobile phone for more than 50 minutes per month are more likely to be in an accident. For this study, the authors examined the association between mobile phone use and accidents using a case-control design. Between 1992 and 1993, the authors collect a random sample of 100 drivers from New York State that had been involved in an accident over the previous two years (case) and 100 drivers not involved in an accident over the previous decade (control).

For their seminal study, Redelmeier and Tibshirani (1997) collected revealed mobile phone usage data from drivers involved in vehicle accidents.[6] Using a case-crossover design, the authors compare phone records of calls and text messages from drivers 10 minutes before the accident with a comparable preceding day. Identification is based on changes in the relative risk associated to differences in phone use immediately before the accident, compared to normal conditions. This technique is useful as drivers serve as their own controls, thereby eliminating potential confounders associated to characteristics of the driver such as age, sex, experience or risk preferences. They find evidence that phone use, measured as whether the subject was texting or calling 10 minutes prior to the accident, was higher than in the control period and is associated with a likelihood of being involved in a crash that is 4.3 times higher. McEvoy et al. (2005) replicate the analysis in Perth, Australia between 2002 and 2004 for 456 accidents requiring hospital attendance and also find an effect size of about a fourfold increase in risk.

Laberge-Nadeau et al. (2003) perform a large questionnaire on driving habits from around 36,000 driving licence holders in Quebec between 1996 and 2000. They collect cell phone data and police records on accidents and compare the ownership and intensity of cell phone users to determine to what extent drivers are more likely to be in an accident. The findings suggest much smaller effects than earlier literature. Male and female drivers owning a mobile phone

observational data.

[6]Drivers suffered material damage but no serious injury.

are 1.1 and 1.2 times as likely to be in an accident in a given year respectively, controlling for number of kilometres travelled annually and other demographic characteristics. Furthermore, drivers that make over 100 calls per month are around 2 times as likely to be in an accident as compared to drivers without a mobile phone.

Our study is most closely related to Bhargava and Pathania (2013), who study the effect of phone influence 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 drops but find no corresponding increase in the number of accidents at the 9pm threshold. Given their upper bound estimates, the authors reject the 4.3 increase in accident likelihood found by Redelmeier and Tibshirani (1997). In another related study, Faccio and McConnell (2018) find that locations with a lot of activity of Pokmon Go (a popular video game at the time) faced significantly more vehicle accidents after the introduction of the game. Finally, several studies investigate how texting bans affected road accidents (Abouk and Adams, 2013; Burger et al., 2014) and indicate that the effects are short lived, if detectable at all.

3. Data and context

3.1. *Road safety data*

Data on police reported accidents in the Netherlands are published by the Dutch Ministry of Infrastructure and Water Management (specifically Rijkswaterstaat). These data contain characteristics of road accidents and of the parties involved.[7] For each accident, we observe accident circumstances, such as day of the week, time of the day, road type, weather conditions, and road surface conditions. Furthermore, the dataset also contains vehicle related characteristics, such as vehicle type, vehicle manoeuvre, sex and age of the driver, and the country in which the vehicle is registered. 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/rider, passenger or pedestrian.

We can directly observe the vehicles country of registration. Drivers of cars registered outside of the Netherlands are likely to reside in other EU countries, therefore it is a good proxy of

[7] 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.

whether the driver incurs roaming costs (before RLAH) or uses the local network instead.[8] To abstract from long term trends, we use data of the years 2014 until 2018, which contains 0.76 million vehicles involved in 0.44 million accidents. Each accident may have multiple vehicles involved, therefore we use information at the party level to avoid measurement error at the accident level as police reports do not indicate which party was at fault. In section 4 we discuss how we handle measurement error in more detail.

3.1.1. *Trends in road safety*

Figure 2 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, 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.

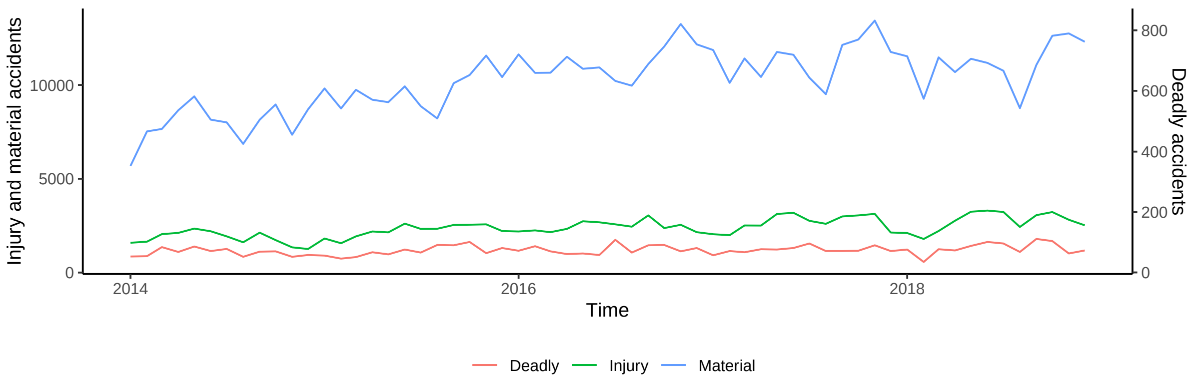

Figure 2: Number vehicles involved in accidents per month by severity.

3.2. *Roam Like at Home Policy*

Our aim is to identify the causal effect of cell phone usage on the number of road accidents. Because data on phone use of drivers is privacy sensitive and not made available for research

[8]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.

purposes, we use the implementation of the RLAH policy as a source of exogenous variation.[9] The policy came into effect on the 15th of June, 2017, and prescribed that all roaming surcharges should be abolished within the EU.

Visitors of an EU country with a cell phone subscription in any of the other EU countries are henceforth referred to as roaming users. Importantly, the policy did *not* affect the costs of using the domestic network of the operator to which the user is subscribed. Users of their domestic network will be further referred to as local users.

Figure 3 presents the annual growth rate in phone use between 2012 and 2018.[10] It illustrates that the RLAH policy resulted in a very large increase in the growth rate of phone use for roamers one year after the policy, while having *no discernible effect* on locals.[11] Table A1 in the Appendix documents the average annual growth rate before and after the policy for roamers as compared to locals. It indicates a substantial increase in the growth rate of mobile data usage by 200 percentage points and of calls and texts by around 40 percentage points, which further demonstrates that the policy had large effects on the overall phone use of roamers, while especially effecting data usage.

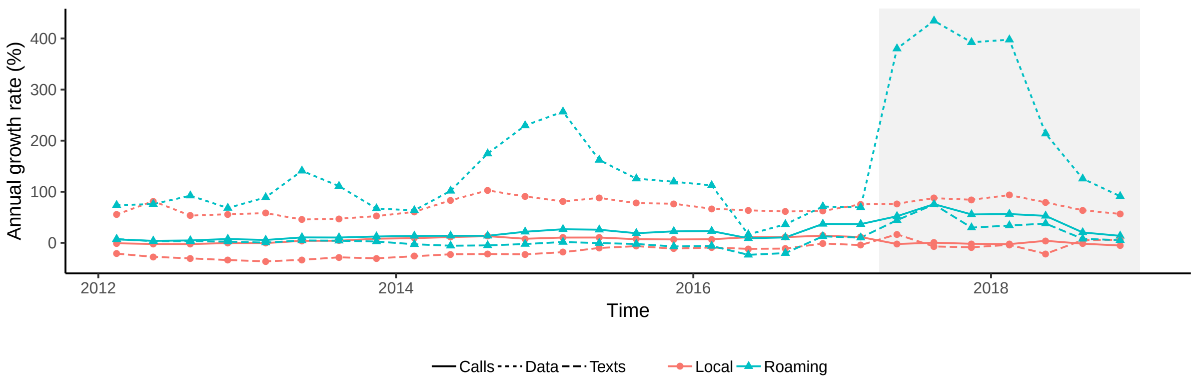

Figure 3: Growth rates in cellular traffic per quarter compared to previous year.

[9]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.

[10]It is useful to compare the growth rates of cellular traffic, rather than levels because technological advancements (e.g. introduction of 4G-network) and increased popularity of smart phones have led to substantial growth in all types of cellular traffic. Importantly, the results of this study rely on comparison between a treated and control group, that both face these developments.

[11]Data sources are the International Roaming BEREC Benchmark Data Reports for roaming users and the Dutch Authority for Consumers and Markets (ACM) for local users. Note that the second quarter of 2017 already contains 15 days during which the policy was active, namely the second half of June.

3.2.1. *Grouping roaming drivers*

We group vehicles 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 discussed in section 4).

The first group contains vehicles with a Dutch registration and are our control group (95.05% of sample). Second and third, are the two adjacent countries, with 1.80% of German vehicles and 1.06% of Belgian vehicles, respectively. The fourth group contains other western European countries which account for 0.42% of vehicles in accidents.[12] The fifth group contains Romania, Poland, and Bulgaria (1.35%) 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 5.3.6). The sixth group contains all remaining EU countries (0.33%).

3.3. *Descriptive statistics*

3.3.1. *Vehicles involved in accidents*

Table 1 presents the descriptive statistics for vehicles involved in accidents. Around 5% of vehicles are roamers, 46% 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.

Table 1: Descriptive statistics: Vehicles involved 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

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

Table 2 highlights that local and roaming drivers involved in accidents are roughly comparable, but roamers tend to be younger, male, and drive on faster roads than local drivers. This makes sense, as both tourists as well as roaming drivers with a business purpose are more likely to spend a larger share of their trip on highways. In terms of the damage reported, the share of material damage is relatively large for roaming vehicles. This is most likely a reporting bias, as language barriers make it more likely for the police to be called in these situations. 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.[13] These dissimilarities become more relevant when generalizing estimated effects to the untreated population. We discuss the assumptions required to attribute the effect to all drivers in section 6.

Table 2: 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

3.3.2. *Dependent variable*

Our dependent variable is the number of vehicles involved in accidents per province per month.[14] We calculate this variable by aggregating the number of vehicles involved in accidents by province, month and country group. The variable varies between 118 and 3,297 vehicles involved in accidents per province per month for local users, with a mean of 972.17 and a standard deviation of 768.37. The number of vehicles involved in accidents by roaming users varies between 0 and 92 per province per month, with a mean of 10.14 and a standard deviation of 13.8.

[13] 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.

[14] We aggregate our data at the province-month level as this is the most detailed information we can obtain for hotel nights. See discussion on this in section 3.3.3.

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

An important concern with our approach may be that group specific trends in traffic intensity or vehicle kilometres travelled (VKT) might drive our results. For example, increases in tourism over time may cause relatively more VKT by roamers and therefore increase the likelihood of a roaming accident after the introduction of RLAH. We do not directly observe VKT separately for each group of drivers. Instead, we use overnight stays in hotels, obtained from Statistics Netherlands (2019a), as 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, Figure 4 shows annual growth rates of hotel nights and VKT for local and roaming guests or drivers. The figure highlights that over the course of the five years prior to the treatment, roaming VKT grew more compared to local VKT. However, a similar, yet even stronger trend is visible for hotel nights. Even when we exclude the province of 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.

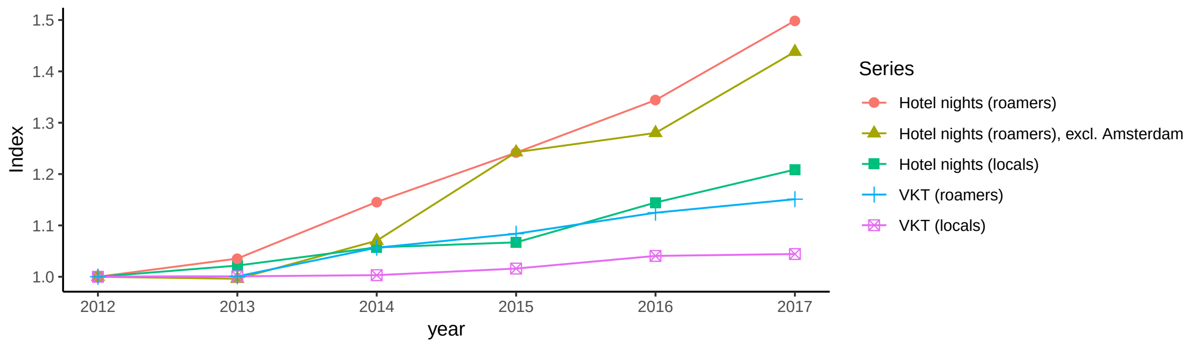

Figure 4: Trends in hotel nights and vehicle kilometers

Second, we analyse how traffic intensity and accidents are related to hotel nights for Dutch drivers, where we observe traffic intensities at the province-month level (Statistics Netherlands, 2019b). Table A2 in Appendix A shows a statistically significant positive elasticity of hotel nights (for Dutch nationals) with respect to traffic intensity (measured on highways). Importantly, we find a statistically significant effect *even* when controlling for year-month and province fixed effects. This suggests that hotel nights are a good proxy for change 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 hotel nights and our fixed effects, suggesting that group specific changes in traffic intensity are unlikely to effect our estimates. In addition, Table A2 also shows that there is a strong association between hotel nights and accidents both for locals and roamers. Assuringly, the estimates for local and roaming users are very similar. In light of the above association between hotel nights and traffic intensity, it thus seems plausible that hotel nights can accurately capture group specific trends in VKT and accidents accordingly.[15]

[15]Note that we find a slightly stronger effect for hotel nights on accidents than on vehicle kilometres. This seems to suggests that hotel guests are overrepresented in in road accidents.

4. Empirical methods

Our aim is to estimate the causal effect of phone usage on road safety. We hypothesise that a substantial reduction in phone 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 a home country) are not affected by the policy. This allows us to define a control group, drivers with a Dutch phone subscription, and a treatment group, drivers with a phone subscription from another 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.

4.1. *Statistical model*

We use a standard DiD approach, where we estimate how the RLAH policy affects the number of vehicles involved in road accidents, V_{grt}, from country g, in region r, at time t.[16] We then consider the following model:

$$\log(V_{grt}) = \phi_r + \eta_g + \kappa_t + \beta R_{gt} + \log(H_{grt}) + \epsilon_{grt}, \tag{1}$$

where log denotes the natural logarithm and R_{gt} denotes the roaming policy. Further, H_{grt} denotes the number of hotel nights, which is included as a proxy for vehicle kilometres travelled by drivers from country of origin g (discussed in detail below). Finally, we include the following fixed effects. First, ϕ_r, is a region fixed effect, which captures time invariant characteristics of an area, such as the road network, attractiveness to tourists, and number of car users. Second, η_g captures country-specific characteristics of drivers to control for differences in vehicle and phone use characteristics between countries. Third, κ_t, denotes a year-month fixed effect that controls for any unobserved time trends affecting all drivers, for instance, weather conditions,

[16] 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.

road maintenance, or infrastructure improvements.[17]

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. We emphasize however, that 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. In that sense, our estimates should be considered as a lower bound of the total effect of phone distraction on road accidents.

4.2. *Measurement error*

There are several sources of measurement error that can arise in our setting. These stem from the fact that we do not directly observe within-vehicle phone use, nor the type of phone subscription drivers have. Here we discuss all four sources of potential measurement error, and how we deal with them.

First, for multi-vehicle accidents, we cannot identify which driver caused the accident, if any. This means that we face measurement error in the dependent variable, which makes our estimates potentially imprecise, albeit still unbiased. We address this issue by focussing on vehicles rather than accidents, so that in multi-vehicle accidents there might be both treated and control-group drivers involved. In addition, we also consider a subsample where we focus on 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.

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. 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

[17] Our approach thus does not rely on controlling for weather directly for unbiasedness or consistency, but doing so may improve the efficiency of our estimator. However, in a robustness check we find that including weather hardly lowers the standard errors (see Table B8 in Appendix B), so that we do not use it for our main analyses.

the mobile phone charges themselves. One can think of unlimited subscriptions paid by drivers' employers. This would also result in a downward bias of the estimate. To address this concern, we re-estimate our main model on sub-samples where we exclude trucks and vans, assuming that drivers of these vehicles are most likely to have such arrangements with their employer.

Fourth, some roaming users might be driving a Dutch car, for instance a rental car. Hence, if there is an increase in the number of local accidents due to increased roaming in local cars, we will underestimate the total effect due to a violation of the stable unit treatment value assumption (SUTVA). We cannot address this issue directly, which implies that our estimates should be considered as a lower bound. However, it is unlikely to be a problematic issue due to the large number of accidents in the control group (local users) for which the average number of accidents will hardly be affected by roaming in local vehicles.

Another violation of SUTVA can be due to increased multi-vehicle accidents caused by roaming drivers, so that more local cars are involved in multi-vehicle accidents, without being the initial cause of the accident. As before, this is unlikely to be problematic due to the size of the control group. In addition, our estimates based on single-vehicle accidents are arguably not affected by this problem.[18]

4.3. *Trends in vehicle kilometres travelled*

A potential confounding factor is vehicle kilometres travelled (VKT) by roaming drivers. For instance, because countries vary in their popularity as a holiday destination over time, there may be more roaming vehicles in accidents due to increased tourism rather than due to phone distraction (Taylor and Ortiz, 2009). 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, we could 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.[19] We argue that hotel nights per country of origin is a good proxy for both tourism and

[18]The only reason for our results based on single-vehicles to be affected by this issue would be if local drivers face an increase in single-vehicle accidents due to unexpected manoeuvres made by roaming drivers that also lead to single-vehicle accidents which seems unlikely.

[19]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).

business related traffic (see section 3.3.3). In combination with country-of-origin fixed effects, hotel nights explains almost all of the variation in VKT, given the relation between traffic and hotel nights is proportionally constant over time. In other words, that there are no divergent trends in travel modes over a five year period, conditional on hotel nights.[20] Nevertheless, we also re-estimate our models using only two years of observations between July 2016 and July 2018 (i.e. one year prior and one year after introduction of the policy), for which we argue it is implausible that there are major trends in tourism transport modes conditional on hotel nights.

4.4. *Standard errors*

In our setting, the number of observations depends on an arbitrary temporal and spatial resolution. We aggregate vehicle data to month-province observations to align the resolution with our control variables. If accidents are serially correlated, the standard errors of ordinary least squares (OLS) may be too small, and will lead to over-rejection of the null hypothesis (Bertrand et al., 2004). To address these issues, we cluster our standard errors at the time invariant level of a region and vehicle-country group, which leaves us with $12 \times 6 = 72$ clusters. In addition, we run a robustness check where we ignore all time series information and aggregate to two periods, pre and post policy. This analysis rules out any autocorrelation in error terms, and highlight that our results are standard errors are hardly affected by serial correlation.

4.5. *Weighting*

Because regions differ greatly in the total number of roaming drivers involved in accidents, it seems natural to use weighted least squares (WLS) with some measure or proxy of roaming vehicles' VKT as sample weights. That is, in regions where there is more traffic, the number of vehicles involved in accidents might be more informative. In contrast, as shown by Solon et al. (2015), weighting might lead to erroneously small standard errors when there is clustering in the disturbances. Because 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. However, as a robustness check we re-estimate our models using weighted least squares, where we use total pre-policy number of vehicles involved in accidents of roaming drivers as weights per

[20] 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).

province. This implies that we assign higher weights to provinces that tend to have relatively more roaming drivers.

4.6. *Zero counts*

In our main analyses we use a log-log specification, which performs well with sufficient number of accidents. However, during some months, for some country groups, we observe few or even zero vehicles in accidents (13.77% pre and 5.79% post policy). These cases are naturally excluded from our log-log regressions. However, they might be less likely to occur after the policy due to policy-induced phone distraction. 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 bias affects our results, we re-estimate our main specification as in 1 using a Poisson regression.

4.7. *Country-group and regional subsamples*

One might be worried that our results are driven by vehicle-country or province specific unobserved trends in VKT that are not well captured by hotel nights. Therefore, we test the sensitivity to various sub samples.

First, because for Amsterdam most tourists will not come by car, we re-estimate our main specification while excluding the province of North-Holland, which contains Amsterdam. Second, there might be a small endogenous response in VKT in border regions induced by the RLAH. For instance, when German or Belgian drivers are more likely to cross the Dutch border due to the more favourable phone use fees. To address this issue, we estimate the treatment effect using non-bordering provinces only. As a by product, this analysis also alleviates potential problems with hotel nights, which might fall short as a VKT proxy for border provinces, where drive-through traffic may cause roaming users to take up a large share of total VKT. Third, for completeness we also test sensitivity to selecting only border provinces.

We also test for sensitivity to specific selections of vehicle-countries. We estimate the treatment effect while excluding labour migration countries, while excluding vehicles from bordering countries (German and Belgium) and while only including those countries. Similarly, to have a more flexible control for country-specific rather than country-group specific unobserved heterogeneity, we also estimate a Poisson regression with a fixed effect for each vehicle country separately.

4.8. *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.

First, we will 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 respectively: 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. Finally, we also look into age groups, and test the treatment effect for drivers under 30, between 30 and 50, and over 50 years old.

5. Results

5.1. *Parallel trends*

In Figure 5 we aggregate country groups into roamers and locals to provide evidence to support the parallel trends assumption. The trend in the number of accidents of both groups shows substantial seasonal variation. If we control for this seasonality, the parallel trends assumption becomes even more plausible, as can be seen from Figure 6. A comparison of the time series of the roaming and local-user accidents suggests that they follow similar trends prior to the implementation of the RLAH policy. After the policy change there seems to be a jump in the number of roaming user accidents, whereas the number of local user accidents does not deviate considerably from its initial trend. We will use methods discussed in Section 4 to analyse this pattern in a rigorous way and test whether there is indeed a significant change.

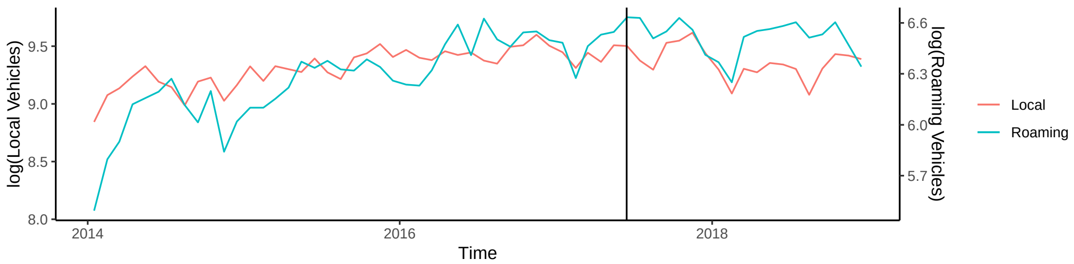

Figure 5: Number of roaming and local user vehicles involved in accidents per month.

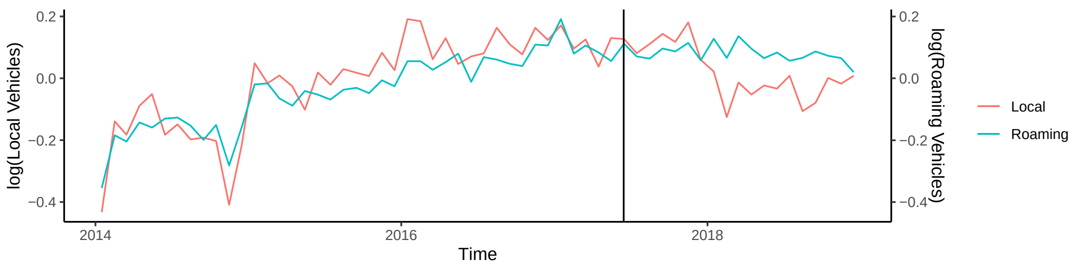

Figure 6: Number of roaming and local user vehicles involved in accidents per month: controlled for seasonality.

5.2. *Estimation results*

Table 3 shows the main estimation results using different subsets of accidents types and vehicle involvement. In all estimations we find a statistically significant increase in the number of

Table 3: Main regression results

	log(Vehicles)			
	All	No Trucks	Single Vehicle (SV)	SV-No Trucks
	(1)	(2)	(3)	(4)
Treatment effect	0.147***	0.160***	0.161***	0.164***
	(0.025)	(0.028)	(0.033)	(0.036)
log(Hotel Nights)	0.064**	0.051**	0.030	0.021
	(0.025)	(0.026)	(0.024)	(0.026)
Country of vehicle FE	Yes	Yes	Yes	Yes
Province FE	Yes	Yes	Yes	Yes
Year-Month FE	Yes	Yes	Yes	Yes
Clusters	72	72	72	72
Local vehicles	630k	491k	124k	111k
Roaming vehicles	33k	21k	6k	4k
Observations	3,323	2,980	2,405	2,079
R^2	0.922	0.915	0.921	0.928

Notes: Standard errors in parentheses are clustered at the level of a province and country of origin of the driver.***, **, * indicate significance at 1%, 5%, and 10%.

accidents following the roaming regulation. In column (1), using all observations, we find a point estimate of 0.147 with a standard error of 0.025. This implies that the policy-induced price drop in phone use leads to an increase in the number of vehicles involved in accidents of 15.81%, with a 95% confidence interval of $10\% - 22\%$. Our estimated hotel nights elasticity has the expected positive sign, and indicates that a 10% increase in hotel nights is associated with a 0.638% increase in the number of vehicles involved in accidents.

Column $(2-4)$ show that focusing on different types of accidents hardly changes the results. Excluding trucks, focusing on single-vehicle accidents, or both, leads to only slightly stronger point estimates. For column (2) this might suggest that truck drivers were less responsive to the RLAH policy, but the estimate is not statistically different from our main estimate. Similarly, focusing on single vehicle accidents as in column $(3 - 4)$ may suggest we reduce measurement error slightly, but it is again only the point estimates that differ from the main estimate.[21] Finally, the estimated hotel nights elasticity has the expected sign, but is only statistically significant when for multi-vehicle cases as in column (1) and (2). This makes sense, as an increase in hotel nights is associated with higher traffic intensity such that single-vehicle accidents are not as likely as multi-vehicle accidents.

[21]The slightly stronger point estimate may also be the results of the SUTVA assumption as the increase in phone use for roamers may also cause more accidents for local users.

5.3. *Robustness checks*

5.3.1. *Accounting for long term trends*

So far, we have assumed that country-of-origin specific trends in VKT are well-captured by our hotel nights proxy. Results from previous analyses suggest that this is a plausible assumption (see Section 3 for a discussion). Nevertheless, to further rule out any issue with long-term trends in non-local road traffic as a potential confounder, we restrict our sample to only one year before and one year after the policy (i.e. from June 2016 to July 2018). This approach yields estimates that are somewhat smaller, but still very comparable to our main results (see Table B2 in Appendix B). This highlights that long term trends in VKT cannot explain the observed increase in vehicles involved in accidents.

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

In our main analysis, we use region-month observations. If there is strong serial correlation, then the normal standard errors may be incorrect, even when clustering at a time-invariant level (see the discussion in Section 4). 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. Table B1 in Appendix B presents the results of this robustness check and shows that the standard errors are only slightly larger compared to the ones in our main analysis. This indicates that serial correlation does not seem to be a threat to our statistical inference.

5.3.3. *Accounting for heterogeneity in traffic using sample weights*

Provinces with more traffic might be more informative for our estimation than quieter provinces. In addition, to approximately recover the phone-use effect *per driver*, rather than at regional level, one should use sample weights for VKT at the individual level. Because these data are not available, we test the robustness of our results to four weighting schemes, that are closely related to VKT. Table B4 in Appendix B shows our main results hardly change if we use weights based on 1) accident numbers, 2) traffic intensity or 3) hotel nights. This suggests that our fixed effects and log-level specification already sufficiently account for differences in VKT among observed regions.

5.3.4. *Testing for sorting*

In our main estimation, we include observations from all provinces in the Netherlands. One might be worried that border provinces might face more VKT induced by the policy, e.g. if people are more likely to go shopping across the border because phone usage is cheaper. Such endogenous response might induce sorting and thereby poses a threat to our identification strategy. In addition, our hotel nights proxy might break down for the province of Noord-Holland, because its capital (Amsterdam) is a popular tourist destination, but few tourists come by car. However, Table B3 in Appendix B shows that the results appear robust to excluding these potentially problematic regions, and highlights that sorting does not drive our results.

5.3.5. *Accounting for zero counts*

In the previous analyses we use a log-level specification. One might be concerned that excluding observations with zero accidents may bias our estimates downwards as it is less likely to have zero observations post-policy due to more phone use. Table B6 presents the results from re-estimating our main regression results using a Poisson regression which allows us to include all province-month observations (this means we have 4,248 province-month observations as compared to 3,323 in column (1) of Table 3). The coefficients are remarkably similar, and are slightly larger overall suggesting that there are indeed more zero observations before the policy which results in a small downward bias. Column (1) indicates that the increase in phone use due to the policy resulted in 18.77% increase in vehicle accidents and is statistically significant at the 1% level.

5.3.6. *Testing for country-specific effects*

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. It is most likely that measurement error is most pronounced for drivers with a close connection to The Netherlands. This can be either due to proximity (like bordering regions) or due to strong economic links (e.g. labour migration). To address these issues, we estimate our results on sub samples without bordering countries (Belgium and Germany) or typical labour migration countries (Poland, Bulgaria, Romania). Table B5 in Appendix B shows that excluding vehicles from these countries leads to nearly identical results. If, instead, we focus only on bordering countries, we find a somewhat smaller point estimate (roughly 10%), suggesting that for these countries measurement error plays a slighly bigger role, as expected. Overall, however, these results highlight that our results do not suffer from a severe downward bias from measurement error.

In related robustness check, we use a Poisson regression without any country grouping, thereby controlling for any country-specific heterogeneity. The results in Table B9 are essentially identical to Table B6 in Appendix B suggesting that our country grouping sufficiently controls for any country-specific heterogeneity.

5.3.7. *Heterogeneous effects*

Table 4: Estimation results using subsamples of road types and severity).

	log(# Vehicles in Accidents)				
	< 50km/h	50km/h - 100km/h	>100km/h	Severe	Material
	(1)	(2)	(3)	(4)	(5)
Treatment effect	0.138***	0.074**	0.029	0.156***	0.142***
	(0.037)	(0.036)	(0.042)	(0.055)	(0.025)
log(Hotel Nights)	0.033	0.054**	0.015	-0.029	0.063***
	(0.024)	(0.023)	(0.021)	(0.030)	(0.024)
Country of vehicle FE	Yes	Yes	Yes	Yes	Yes
Province FE	Yes	Yes	Yes	Yes	Yes
Year-Month FE	Yes	Yes	Yes	Yes	Yes
Clusters	72	72	72	69	72
Local vehicles	339k	124k	94k	121k	509k
Roaming vehicles	13k	7k	8k	3k	30k
Observations	2,785	2,544	2,562	1,931	3,282
R^2	0.919	0.909	0.892	0.930	0.920

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

We also test whether the effect of phone use on accidents is heterogeneous over different road types and accident severity, with results in Table 4. Columns $(1-3)$ indicate that most of the estimated effect comes from local road, and we do not find evidence reduction of road safety on highways. This suggests that phone distraction is 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 more dangerous).

Importantly, columns (3) and (4) highlight that our results hold, regardless of accident severity. One might think that phone distraction mainly increases accidents with material damage, For instance, if people are mostly using phones in low-speed situations like traffic jams. However, our results clearly speak against that hypothesis, and indicate that severe accidents are affected just as well.

5.4. *Event Study and anticipation*

To assess potential anticipation and to test for pre-trends, we interact the the indicator for roaming drivers, D_g, with year-month dummies to get an estimate for each month. Therefore we estimate the parameters in the following equation

$$\log(V_{rgt}) = \phi_r + \eta_g + \beta_t(D_g \times \kappa_t) + \log(H_{rgt}) + \epsilon_{rgt}, \tag{2}$$

where $D_g \times \kappa_t$ is an indicator variable for whether the vehicle count is for roamers or not interacted with a year-month dummy, and β_t is the effect of of the policy for each year-month t.[22]

As can be seen from Figure B1 in Appendix B, before the policy there are no clear pre-trends suggesting that locals are a suitable control group for roaming drivers, after controlling for hotel nights and region, time, and country group fixed effects. Furthermore, after the policy there is a clear positive impact on accidents as the proportion of statistically significant estimates increases substantially.

5.5. *Implications*

Our results show that increase in phone use induced by the RLAH policy is associated with a 15.81% increase in accidents, when using our preferred estimate (first column in Table 3). In this section we calculate the total number of accidents and the relative risk of phone use implied by our main estimate.

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

To calculate the number of accidents associated with phone use, we compare the observed count with a counter-factual situation where all drivers face phone fees equal to the pre-policy roaming charges. This is a valid generalization under the assumption that the mechanism identified for roamers carries over to all drivers. In other words, that the average treatment

[22] One limitation of this regression is that because we estimate a coefficient for each year-month, we cannot simultaneously control for seasonality using fixed effects unless we exclude one month from each year.

effect for roamers (ATET) should be representative of the average treatment effect (ATE) for for all road users. Under this assumption, our results then indicate that phone use causes 19,487 additional accidents annually in the Netherlands, of which about 3,644 result in injury and 113 are fatal.

5.5.2. *Relative risk of phone use*

We follow Bhargava and Pathania (2013) and translate our estimates for the effect of the change in number of accidents due to the RLAH policy to the relative risk (or 'odds ratio') which allows us to compare our results to the existing literature. This requires two key parameters, the baseline average phone use for roaming users while in the car, and 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).[23] As for the increase in phone use due to the policy, Table A1 suggests that the policy induced increase in mobile data by around 200%, and calls and texts by around 40%. We consider $b \in [0.01, 0.10]$ and $c \in [0.5, 2]$.

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 5 illustrates how our key parameters influence the implied RR estimates. It indicates that the RR is decreasing in the baseline proportion of roamers using their phone while driving and the change in phone use due to the policy. In other words, if the policy had a small impact on phone use and few roaming drivers used their phone prior to the policy, it implies substantial risks associated with phone use while driving.

Taking a conservative estimate for the percentage of roaming drivers using their phone while driving, b, of 3% and the change in phone use due to the policy, c, of 100%, this would imply a relative risk of phone use of 5.3.[24] We consider this to be a conservative estimate as it is

[23]Note this does not distinguish between roaming and local drivers.

[24]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: $RR = 5.3$.

unlikely that roaming drivers used their phones as intensively as local drivers due to the high pre-policy roaming costs.

Table 5: Sensitivity of implied accident risk.

	Baseline phone use of roaming drivers, b				
Δ phone use due to RLAH, c	1%	2%	3%	5%	10%
50%	24.60	12.50	8.50	5.30	2.90
80%	17.20	8.90	6.20	4.00	2.30
100%	14.40	7.60	5.30	3.50	2.10
150%	10.40	5.60	4.00	2.70	1.80
200%	8.20	4.50	3.30	2.30	1.60

Notes: This table presents the relative accident risk implied by our baseline estimate from column (1) in Table 3. The relative risk is calculated by re-arranging equation (3) such that: $RR = \frac{\hat{\beta}-\hat{\beta}b+bc}{b(\hat{\beta}+c)}$. An illustration is outlined in the text.

Comparing these estimates to the existing literature suggests that earlier studies may have underestimated the risks associated with modern smart phone usage while driving.[25] As mentioned above, previous research focuses mainly on the effects of calling, however modern smart phones offer substantially more usability and potential for distraction. Our estimates for the change in mobile phone use due to the RLAH policy suggests 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 implied relative risk estimates. Experimental studies find that texting impairs drivers cognitive capacities more than other types of mobile phone usage, suggesting that modern smartphone apps, which increase the prevalence of this type of mobile activity, may be cause for additional safety concerns (Dingus et al., 2016).

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 installed 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 use a difference-in-differences approach.

We estimate that phone causes for an increase of 15.81% of vehicles involved in accidents, which is the average treatment effect on the treated (ATET). Our DiD approach assures the

[25] Redelmeier and Tibshirani (1997) finds a RR of about 4.3, Dingus et al. (2016) find the RR of various cell phone distractions to range between $2.2 - 12.2$ with the latter reflecting the risk of cell phone dialing, while Bhargava and Pathania (2013) do not find any effect.

causality of this estimate, but requires two main assumptions for extrapolation to an average treatment effect (ATE). We discuss these here.

First, the driver characteristics should be roughly comparable between control and treatment group. Looking at the age distribution indicates that treated and control-group drivers roughly comparable in that dimension, although treated drivers were slightly younger (see Figure A2 in Appendix A). In addition, our analysis on heterogeneous effects across age groups suggests that differences in driver age will lead to similar results.

Second, a concern might be that 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 and distraction and road familiarity, but at this stage, there seems to be no suggestions that our ATET overestimates the ATE due to road familiarity.

Importantly, the RLAH policy abolished additional roaming surcharges, so that after the policy, roaming users and domestic users face the same phone use cost.[26] Therefore, the relevant counter-factual situation is one where both local and roaming use face pre-RLAH phone charges. Assuming that our ATET extends to the ATE, our estimate then implies a 15.81% reduction in vehicles involved in accidents. In other words, roaming drivers are arguably not more likely to cause accidents than local drivers, rather the RLAH made roaming drivers to 'catch up' with local drivers' smartphone distraction and associated accident risk.

In sum, it seems plausible that our estimated ATET is roughly similar to the ATE. Our results then imply that each year in the Netherlands as many as 20945 road accidents are associated with phone use, of which 121 were fatal. These figures indicate that a successful ban on within-car phone use will lead to substantial improvements of road safety, even to point where EU road safety targets will be met.

[26] There may be 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 more related to local demand and supply conditions than the RLAH policy.

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Table A1: Difference-in-differences in annual growth of phone use.

		Annual growth rate (%)		Annual growth rate (p.p.)	
Usage	User	Pre	Post	Diff	DiD
Calls	Local	9.11	-0.09	-9.19	
Calls	Roaming	23.15	60.08	36.92	46.12
Data	Local	76.13	86.06	9.93	
Data	Roaming	137.04	359.58	222.54	212.61
Texts	Local	-11.50	-10.36	1.14	
Texts	Roaming	-0.50	44.08	44.58	43.44

Notes: Pre-policy refers to the average annual quarterly growth rate over three years, Q1 2014 − Q1 2017, prior to the implementation of RLAH. Post-policy is one year, Q2 2017 − Q1 2018, after RLAH.

A. Additional descriptives

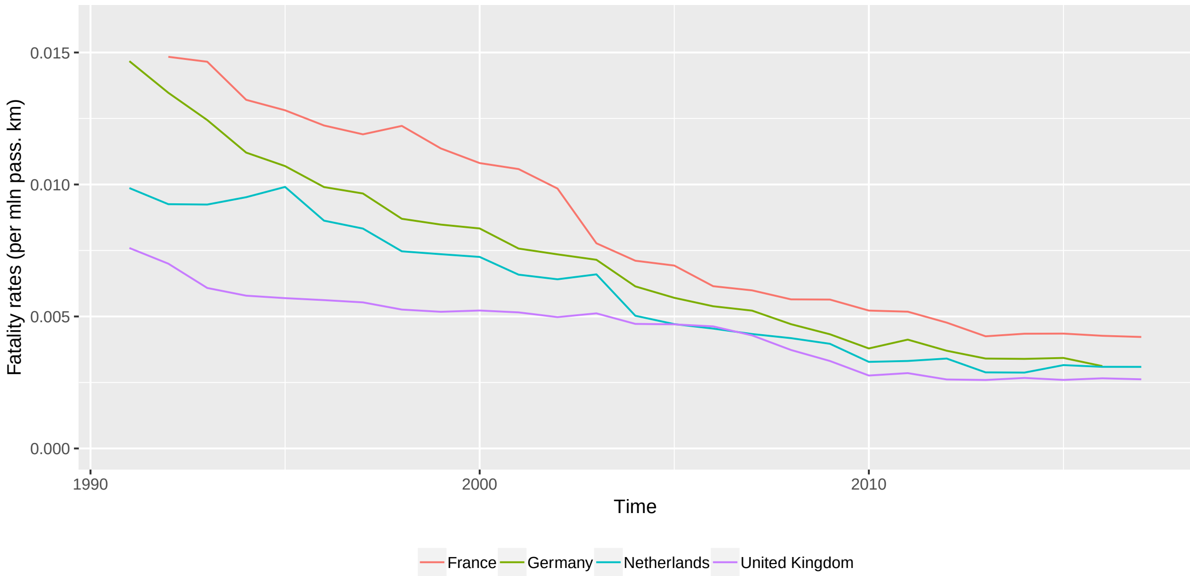

Figure A1: Number of fatal accidents over time in select EU countries

A.1. *Analysis of hotel nights as proxy*

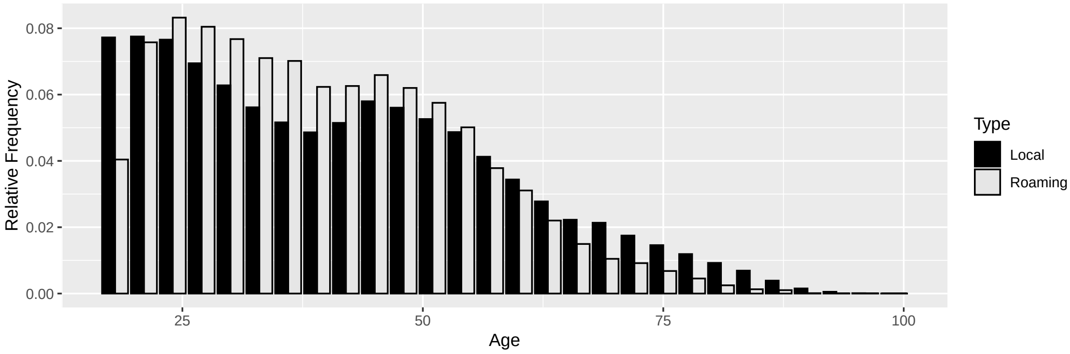

Figure A2: Age of local and roaming users.

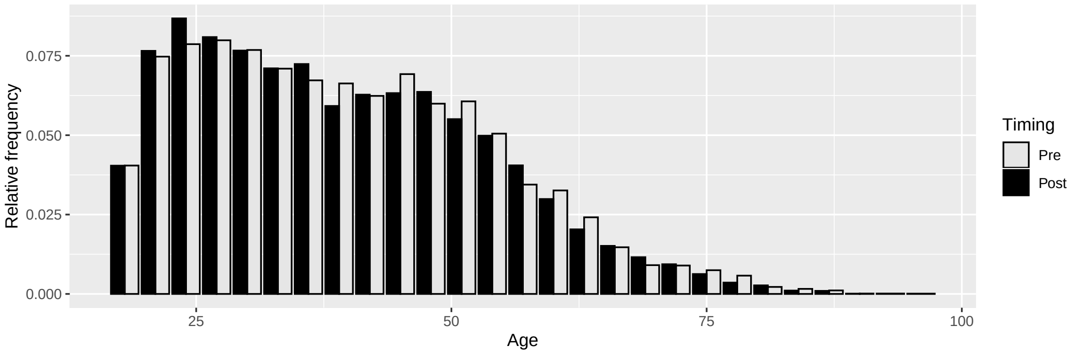

Figure A3: Age of roaming users pre and post policy.

Table A2: Regression results for analysing traffic and hotel nights for Dutch drivers.

	log(Traffic intensity)		log(#Vehicles in accidents)		
	(1)	(2)	(3)	(4)	(5)
log(Hotel nights)	0.240***	0.042**	0.786***	0.109***	0.076***
	(0.016)	(0.021)	(0.020)	(0.037)	(0.004)
Region FE	No	Yes	No	Yes	Yes
Year-month FE	No	Yes	No	Yes	Yes
Subsample	Locals	Locals	Locals	Locals	Roamers
Within R2	0.232	0.016	0.660	0.023	0.081
Observations	576	576	576	576	3,032
R^2	0.232	0.992	0.660	0.990	0.518

Notes:.***, **, * indicate significance at 1%, 5%, and 10%.

B. Robustness checks

Table B1: Robustness check aggregating to two periods.

	Vehicles			
	All	No Trucks	Single Vehicle (SV)	SV-No Trucks
	(1)	(2)	(3)	(4)
Treatment effect	0.102***	0.123***	0.073***	0.093***
	(0.026)	(0.026)	(0.028)	(0.030)
log(Hotel Nights)	0.083**	0.051	0.021	0.009
	(0.041)	(0.044)	(0.037)	(0.035)
Country of vehicle FE	Yes	Yes	Yes	Yes
Province FE	Yes	Yes	Yes	Yes
Pre-Post FE	Yes	Yes	Yes	Yes
Clusters	72	72	72	72
Local vehicles	630k	491k	124k	111k
Roaming vehicles	33k	21k	6k	4k
Observations	144	144	143	139
R^2	0.956	0.947	0.954	0.956

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

Table B2: Robustness check using subsample with one year pre and one year post policy.

	log(Vehicles)			
	All	No Trucks	Single Vehicle (SV)	SV-No Trucks
	(1)	(2)	(3)	(4)
Treatment effect	0.108***	0.148***	0.179***	0.168***
	(0.028)	(0.035)	(0.038)	(0.045)
log(Hotel Nights)	0.064**	0.037	0.017	0.006
	(0.027)	(0.029)	(0.026)	(0.028)
Country of vehicle FE	Yes	Yes	Yes	Yes
Province FE	Yes	Yes	Yes	Yes
Year-Month FE	Yes	Yes	Yes	Yes
Clusters	72	72	72	72
Local vehicles	319k	248k	59k	53k
Roaming vehicles	18k	11k	3k	2k
Observations	1,593	1,447	1,162	999
R^2	0.923	0.915	0.921	0.930

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

Table B3: Robustness check varying the geographic context.

	log(# Vehicles in Accidents)			
	All regions	Amsterdam excl.	Border regions excl.	Border regions
	(1)	(2)	(3)	(4)
Treatment effect	0.147***	0.137***	0.146***	0.140***
	(0.025)	(0.026)	(0.037)	(0.036)
log(Hotel Nights)	0.064**	0.079***	0.020	0.089***
	(0.025)	(0.024)	(0.025)	(0.033)
Country of vehicle FE	Yes	Yes	Yes	Yes
Province FE	Yes	Yes	Yes	Yes
Year-Month FE	Yes	Yes	Yes	Yes
Clusters	72	66	30	42
Local vehicles	630k	517k	327k	303k
Roaming vehicles	33k	29k	11k	22k
Observations	3,323	3,006	1,378	1,945
R^2	0.922	0.921	0.958	0.912

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

Table B4: Regression results using weighted least squares.

	log(Vehicles)				
	(1)	(2)	(3)	(4)	(5)
Treatment effect	0.147***	0.159***	0.152***	0.152***	0.169***
	(0.025)	(0.028)	(0.026)	(0.026)	(0.031)
log(Hotel Nights)	0.064**	0.041	0.060**	0.047**	0.040
	(0.025)	(0.025)	(0.028)	(0.022)	(0.031)
Country of vehicle FE	Yes	Yes	Yes	Yes	
Province FE	Yes	Yes	Yes	Yes	Yes
Year-Month FE	Yes	Yes	Yes	Yes	Yes
Weights	No weighting	Total veh.	Roaming veh.	Avg traf. intens.	Avg hotel nights
Clusters	72	72	72	72	72
Local vehicles	630k	630k	630k	630k	630k
Roaming vehicles	33k	33k	33k	33k	33k
Observations	3,323	3,323	3,323	3,323	3,323
R^2	0.922	0.927	0.911	0.926	0.930

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

Table B5: Estimation results varying the vehicles' country of origin.

	log(# Vehicles in Accidents)			
	All	BG PL RO excl.	No border countr.	Only border countr.
	(1)	(2)	(3)	(4)
Treatment effect	0.147***	0.144***	0.152***	0.105***
	(0.025)	(0.027)	(0.028)	(0.041)
log(Hotel Nights)	0.064**	0.052*	0.018	0.444***
	(0.025)	(0.031)	(0.016)	(0.153)
Country of vehicle FE	Yes	Yes	Yes	Yes
Province FE	Yes	Yes	Yes	Yes
Year-Month FE	Yes	Yes	Yes	Yes
Clusters	72	60	48	36
Local vehicles	630k	630k	630k	630k
Roaming vehicles	33k	24k	14k	19k
Observations	3,323	2,726	2,212	1,747
R^2	0.922	0.926	0.969	0.928

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

Table B6: Estimation results using Poisson regression.

	# Vehicles in Accidents			
	All	No Trucks	Single Vehicle (SV)	SV-No Trucks
	(1)	(2)	(3)	(4)
Treatment effect	0.172***	0.180***	0.178***	0.184***
	(0.0258)	(0.0294)	(0.0338)	(0.0365)
log(Hotel nights)	0.0694	0.114**	0.0616	0.103*
	(0.0441)	(0.0557)	(0.0434)	(0.0530)
Country of vehicle FE	Yes	Yes	Yes	Yes
Province FE	Yes	Yes	Yes	Yes
Year-Month FE	Yes	Yes	Yes	Yes
Clusters	72	72	72	72
Local vehicles	686k	535k	136k	122k
Roaming vehicles	35k	22k	6k	4k
Observations	4,248	4,248	4,248	4,248

Notes: Standard errors in parentheses are clustered at the level of a province and country of origin of the driver.***, **, * indicate significance at 1%, 5%, and 10%.

Table B7: Estimation results using country-month fixed effects.

	log(Vehicles)			
	All	No Trucks	Single Vehicle (SV)	SV-No Trucks
	(1)	(2)	(3)	(4)
Treatment effect	0.149***	0.152***	0.160***	0.167***
	(0.026)	(0.028)	(0.034)	(0.038)
log(Hotel Nights)	0.062**	0.050*	0.029	0.022
	(0.025)	(0.026)	(0.025)	(0.027)
Country of vehicle FE	Yes	Yes	Yes	Yes
Province FE	Yes	Yes	Yes	Yes
Year-Month FE	Yes	Yes	Yes	Yes
Clusters	72	72	72	72
Local vehicles	630k	491k	124k	111k
Roaming vehicles	33k	21k	6k	4k
Observations	3,323	2,980	2,405	2,079
R^2	0.924	0.918	0.924	0.930

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

Table B8: Estimation results when including weather as control.

	log(Vehicles)			
	(1)	(2)	(3)	(4)
Treatment effect	0.147***	0.147***	0.147***	0.148***
	(0.025)	(0.025)	(0.025)	(0.024)
log(Hotel Nights)	0.064**	0.064**	0.064**	0.064**
	(0.025)	(0.025)	(0.025)	(0.026)
Temperature		-0.006**	-0.005*	
		(0.003)	(0.003)	
Rain		-0.002	0.001	
		(0.014)	(0.019)	
Frostdays			0.022	
			(0.015)	
HeavyRainDays			-0.003	
			(0.010)	
Country of vehicle FE	Yes	Yes	Yes	Yes
Province FE	Yes	Yes	Yes	Yes
Year-Month FE	Yes	Yes	Yes	Yes
Weather dummies	No	No	No	Yes
Clusters	72	72	72	72
Local vehicles	630k	630k	630k	630k
Roaming vehicles	33k	33k	33k	33k
Observations	3,323	3,323	3,323	3,323
R^2	0.922	0.922	0.922	0.925

Notes: Robust standard errors in parentheses are clustered at the district-country-of-origin level. Weather data is obtained from Royal Netherlands Meteorological Institute (2019).***, **, * indicate significance at 1%, 5%, and 10%.

Table B9: Regression results using Poisson regression.

	# Vehicles in Accidents			
	All	No Trucks	Single Vehicle (SV)	SV-No Trucks
	(1)	(2)	(3)	(4)
Treatment effect	0.175***	0.181***	0.180***	0.184***
	(0.0260)	(0.0305)	(0.0343)	(0.0382)
log(Hotel nights)	0.0727	0.120**	0.0672	0.113**
	(0.0447)	(0.0600)	(0.0447)	(0.0570)
Country of vehicle FE	Yes	Yes	Yes	Yes
Province FE	Yes	Yes	Yes	Yes
Year-Month FE	Yes	Yes	Yes	Yes
Clusters	360	336	336	312
Local vehicles	686k	535k	136k	122k
Roaming vehicles	35k	22k	6k	4k
Observations	21,240	19,824	19,824	18,408

Notes: Standard errors in parentheses are clustered at the level of a province and country of origin of the driver.***, **, * indicate significance at 1%, 5%, and 10%.

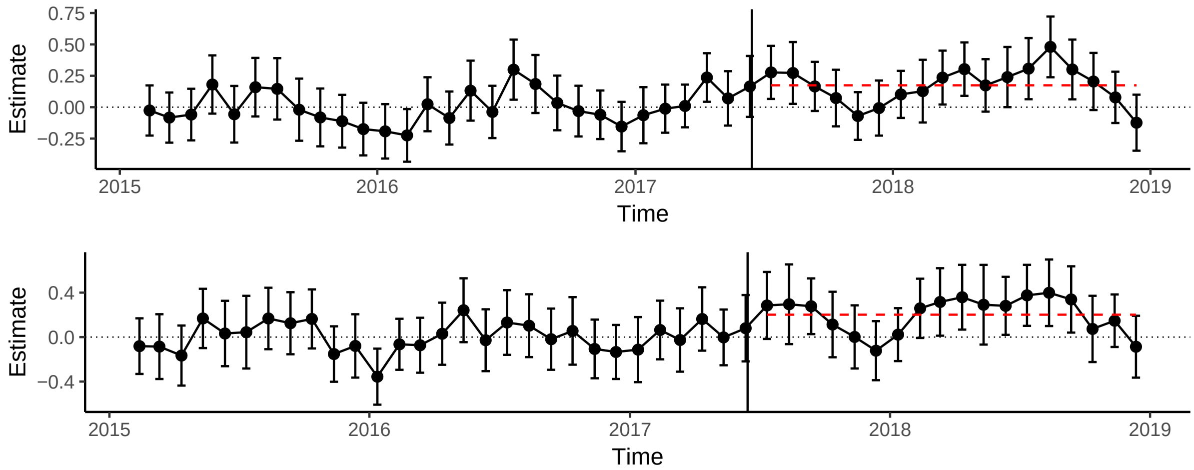

Figure B1: Treatment effect per month for full sample (top) and single-vehicles (bottom).

B. Additional sensitivity analyses

Table B10: Estimation results for subsamples with different age groups.

	log(# Vehicles in Accidents)				
	All	Age $\leq$ 30	30 <Age< 50	Age$\geq$ 50	Age unknown
	(1)	(2)	(3)	(4)	(5)
Treatment effect	0.147***	0.092**	0.160***	0.142***	0.137*
	(0.025)	(0.036)	(0.035)	(0.034)	(0.077)
log(Hotel Nights)	0.064**	0.033	0.046**	-0.004	0.021
	(0.025)	(0.024)	(0.022)	(0.024)	(0.024)
Country of vehicle FE	Yes	Yes	Yes	Yes	Yes
Province FE	Yes	Yes	Yes	Yes	Yes
Year-Month FE	Yes	Yes	Yes	Yes	Yes
Clusters	72	72	72	72	71
Local vehicles	630k	178k	207k	163k	82k
Roaming vehicles	33k	7k	12k	6k	8k
Observations	3,323	2,393	2,791	2,354	2,529
R^2	0.922	0.921	0.910	0.922	0.867

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

B.1. *List of sensitivity checks not possible due to data limitations*

- age of car (only for Dutch cars we know the age)
- only frontal crashes, too few observations (less than 10k accidents) often the impact location is missing
- urban vs rural (often missing)