Cheap talk: the effect of cell phone usage on road accidents*

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We provide novel evidence of the effect of cell phone use on car accidents. We exploit variation in the cell phone usage fees in the United Kingdom (UK) following European Union (EU) roaming regulations in 2017. This policy intervention is used as a treatment that only applies for non-UK drivers from the EU, which allows us to use a difference-in-differences approach. We find that phone use causes the number of accidents to increase with at least 10%. It is plausible that this mechanism results carry over to home drivers. Our results then imply that each year in the UK as many as 17,093 accidents are associated with phone use.

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

1. Introduction

Traffic accidents led to severe injuries for an estimated number of 135,000 road users in the European Union in 2017, while another 25,300 road users lost their lives (European Commission, 2018b). Next to physical harm, these accidents cause psychological harm, not only to those directly involved, but also to friends and relatives of the victims. Furthermore, traffic accidents lead to monetary losses due to damages to private and public property, and are a major cause of traffic congestion. These negative outcomes provide governments with a 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 and obligatory usage of seatbelts. Policy makers also attempt to control and regulate factors that affect the ability of road users to react appropriately to sudden changes in the traffic situation. In this

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light, governments prohibit driving under influence and increasingly ban the use of cell phones by drivers.

Despite regulations that forbid car drivers to use their cell phones, technological progress in recent years has transformed cell phones into an omnipresent device that can be a major cause of distraction in traffic. Thereby, a growing source of distraction for (car) drivers is the increasing online activity of their cell phones. This includes sending text messages and receiving notifications from social media. At the same time, after years of gradual reduction in the number of accidents per year, 2016 and 2017 were marked by relative stagnation of a downward trend (European Commission, 2018a). Therefore, an important and ongoing research question is to what extent cell phone usage of car drivers affects the number of road accidents.

Various studies have investigated the link between phone use and accidents (see e.g. reviews by Dragutinovic and Twisk, 2005; Young et al., 2007; WHO, 2011; Lipovac et al., 2017). 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. One important caveat of both types of studies is that drivers may adjust behaviour in response to being monitored.

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. The earliest study by Violanti and Marshall (1996) examines 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 past two years (case) and 100 drivers not involved in an accident over the last decade (control). The findings suggest that drivers using a mobile phone for more than 50 minutes per month are more likely to be in an accident.

Redelmeier and Tibshirani (1997) collect mobile phone data from 699 drivers involved in vehicle accidents in Canada between 1994 and 1995.[1] 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 around a fourfold increase in the likelihood of an accident. 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.

Finally, 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 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.

These studies pose several issues concerning the measurement, identification and validity of the causal effect of mobile phone use on accidents. Firstly, mobile phone use has dramatically changed since the turn of the century in terms of adoption, exposure and capabilities. For example, in the studies by Redelmeier and Tibshirani (1997), Laberge-Nadeau et al. (2003) and McEvoy et al. (2005), only 18%, 25%, and 72% of drivers owned a mobile phones respectively,

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

as compared to almost 100% today.[2] Smart phones have drastically affected our exposure to and use of mobile phones. While drivers can navigate streets more easily using navigation applications, there is also an increasing number of potential mobile phone distractions: such as incoming video calls, text messages and various other applications providing frequent notifications. Secondly, many studies do not account for unobserved factors that may be correlated to both phone use and accident likelihood. Finally, as sample sizes are small, it is difficult to generalise the results to a larger population.

We propose an alternative research method based on field data, which allows us to get a more reliable estimate of the causal impact of phone use on vehicle accidents. We exploit a change in the European Union (EU) roaming regulation to analyse the effect phone use on road accidents. This EU roaming regulation, that was imposed in June 2017, mobile phone operators had to abolish all roaming charges for customers that use their phone abroad within any EU country. As a result, the charges for phone use drastically decreased for EU citizens visiting any other EU country. In contrast, users of the home network were not affected by this policy and their fees for phone usage did not change. It seems therefore plausible that, as of June 2017, EU citizens driving abroad are more likely to be distracted in the car due to phone use, while for domestic drivers nothing changed.

We use micro data on all police reported road accidents in the UK from 2014 till 2017. From these data we focus on single-vehicle accidents which ensures that potentially distracted drivers are exactly identified. We then classify those drivers that are plausibly treated by the EU roaming regulation based on vehicle information. The causal effect of phone use on road accidents is then estimated using a difference-in-differences approach, where we use the EU roaming regulation as treatment, and domestic drivers as control group.

This rest of this paper is structured as follows. Section 2 explains the methods employed. Section 3 describes the data and presents descriptives statistics. Section 4 discusses the results and robustness checks. Section 5 concludes.

[2] Mobile cellular subscriptions per capita were above 1 in the world since 2016 (World Bank, 2019).

2. Empirical methods

Our aim is to identify the causal effect of cell phone usage on the number of road accidents. Because phone usage is not observed in the field, we use a structural change in the European Union (EU) roaming regulation as exogenous variation.[3] This policy came into place in 2017 on June 15th and prescribed that all roaming surcharges should be abolished within EU.

The new regulation implied that visitors of any EU country with an EU cell phone subscription (henceforth: roamers or roaming drivers) had a vast drop in their usage fees. However, and importantly, the policy did *not* affect cell phone users subscribed with a domestic operator (henceforth: home users). This implies that we can exploit variation in cell phone charges as a treatment for roaming drivers to estimate a difference-in-difference (DD) model. Because using a cell phone was rather costly for roamers, we assume that before the change in regulation they did not use their phone at all while driving. We emphasize that if roamers *did* use their phones while driving prior to new roaming regulations, then we underestimate the real effect. In that sense our estimates should be considered as a lower bound of the actual effect of phone use on road accidents.

One challenge is to identify precisely which accidents are treated by the regulation. This is because there are often several vehicles involved in road accidents, but based on available data (police reports) we cannot determine which driver is the one that was distracted prior to an accident (i.e. caused the accident). To avoid measurement error of the type of accident, we focus on single-vehicle accidents (e.g. a collision between a car and a tree). This allows us to have an exact classification of roaming accidents versus home-user accidents.

2.1. *Estimation in levels and log*

Let us start with a standard DD approach, where we estimate how the number of accidents A is affected by the roaming regulation R. We consider the following model:

$$A_{r,d,t} = \eta_d + \kappa_t + \beta R_{d,t} + \epsilon_{r,d,t} \tag{1}$$

[3] Roaming refers to mobile phones connecting to a foreign cell network. Network operators generally charge additional fees for using this service. The regulation considered here abolished these charges for EU phone subscriptions

where r denotes a region (UK district), d driver type (roaming or home user), and t the time window. Initially we will use monthly time periods to estimate equation (1). One concern is that accidents of roamers typically follow different (seasonal) trends compared to home users. There are, for instance, more roaming drivers present during holidays, and over the course of a decade, countries can vary in their popularity as a holiday destination (Taylor and Ortiz, 2009). In section 3 we will show, however, that the number of foreign cars visiting the UK is very stable over the years. Nevertheless, to account for this potential confounder, we estimate a variant of (1) where we include month-of-the-year-driver-type fixed effects and month fixed effects.

Furthermore, because we have a treated period of only a half year, one might be worried that our results are driven by seasonal effects. We do correct for seasonal and time fixed effects in most of our analyses, but in order to rule out seasonal effects completely, as a robustness check, we re-estimate the above mentioned specifications on a subsample that contains only the second half of each year (July to December).

Another issue might be misspecification. Therefore, we re-estimate the two models as mentioned above, but now with the log of accidents as dependent variable. This can alleviate misspecification due to the skewed distribution in the number of accidents per region. A disadvantage of this approach is, however, that we loose power as we cannot include regions with zero accidents in either group.

Finally, it is well known that the standard errors of ordinary least squares (OLS) tend to over-reject the null hypothesis in DD estimates due to autocorrelation (Bertrand et al., 2004). To avoid this issue, we estimate other variants of (1). First, we further aggregate to quarterly data and re-estimate the same model with all additional fixed effects included. Compared to previous analyses, using a coarser temporal resolution reduces autocorrelation in the number of accidents per region, which thereby alleviates problems of over-rejection. Second, we ignore all time series information and take monthly and quarterly accidents averaged for the whole pre-treatment and post-treatment period. Thereby, we rule out the possibility that auto correlation problems drive our results.

2.2. *Binary response*

As an alternative identification strategy, we focus on road segments; a much more spatially disaggregated level. This allows us to estimate how the average probability of an accident occurring per road segment is affected by phone use. We consider the following model:

$$\mathrm{P}[Accident_{r,d,t} = 1|\eta, \kappa, R] = \eta_d + \kappa_t + \beta R_{d,t} + \epsilon_{r,d,t} \tag{2}$$

where r now denotes a road segment, and other indices are as above. The biggest challenge of this approach is to find such a temporal resolution that strikes a balance between: (a) having few enough time periods to avoid autocorrelation problems (i.e. the bigger the time window, the better), and (b) having a time window that is as small as possible to have maximally one accident per region-time. The latter issue is to avoid lumping together cases with multiple accidents into a binary variable. We will estimate six variants of (2) while aggregating to monthly, quarterly, or half yearly time periods. For each version we will estimate an additional specification with season-driver fixed effects (e.g. month of the year $\times$ driver type dummies).

3. Data

The analysis of this paper is based on one main dataset. The dataset contains detailed characteristics of personal injury road accidents in Great Britain and of the parties involved. It comprises road accidents that have been reported to and recorded by the police, where the latter has been done using a standardized form. These data have been published by the Department for Transport of the government of the United Kingdom and are made publicly available under an Open Government License on `data.gov.uk`. 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 whether the car was left hand drive or not. Finally, casualty related variables are also reported and provide information such as age and sex of casualty, casualty severity and whether the casualty was a driver/rider, passenger or pedestrian.

We do not directly observe the driver's country of residence, which would be a good proxy of whether the driver incurs roaming costs or uses the home network instead. However, we do have data on whether a car is a left-hand-drive vehicle. Drivers of these vehicles are likely to reside in other EU countries, where traffic drives on the right hand side, instead of Great Britain, where traffic drives on the left hand side of the road. This makes it likely that drivers of left hand drive vehicles incur roaming costs before the change in policy, which is what we assume in our analysis.[4]

To abstract from long term trends, we use data of the years 2014 until 2017.[5] We observe a total number of 0.55 million personal injury accidents for these years. We exclude accidents involving trucks (11.11% of the 0.55 million accidents), as truck drivers are likely to have company phones and would consequently not be affected by the roaming policy. Furthermore, to mitigate reporting errors, self reported cases that were not filed by a police officer are excluded as well (23.04% of the 0.55 million accidents). Accidents that are reported by the Metropolitan Police Service are also excluded from our analysis (18.47% of the 0.55 million accidents), because of a structural change in reporting around the time of policy implementation.[6] Finally, we focus on single-vehicle accidents. The reason for this is that for these accidents we know for sure who caused the accident, which is not the case for multi-vehicle accidents. This leaves us with 97,222 observations, 17.58% of the initial set of 0.55 million accidents.

Our main variable of interest is the number of accidents per UK district per month, which is the dependent variable in our model captured by equation (1). This variable varies between 0 and 61 accidents per UK district per month for home-user drivers, with a mean of 5.8 and a standard deviation of 5.1. The number of accidents by roaming drivers varies between 0 and 2 per UK district per month, with a mean of 0.02 and a standard deviation of 0.14).

Figure 1 provides a first look at the dynamics of the total number of monthly accidents for the two groups. A comparison of the time series of the roaming and home-user drivers suggests that they follow similar trends prior to the policy change. After the policy change there seems to be a subtle increase in the number of roaming accidents, whereas the number of home accidents

[4]Note that, due to our DD method, misclassification can pose a problem for the efficiency of our estimator, but will not bias our estimates under the plausible assumption than misclassification is not correlated to the roaming regulation.

[5]Additionally, we will also include the 2018 data in our analysis once they become available in the spring of 2019.

[6]See Department for Transport (2017a) for more details. Figure A1 in Appendix A illustrates this structural change in reporting.

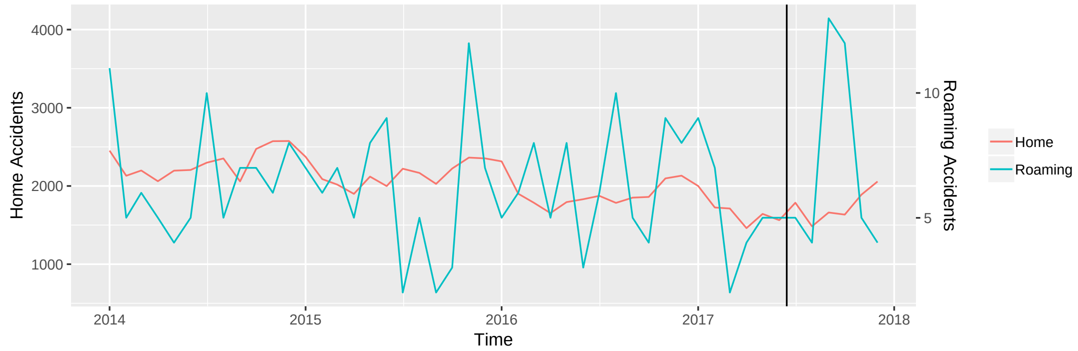

Figure 1: Number of roaming and home accidents per month.

does not deviate considerably from its initial trend. We will use methods discussed in Section 2 to analyse this pattern in a rigorous way and test whether there is indeed a significant change.

One important concern regarding our methodology may be that, if other (unobserved) factors cause the number of roaming drivers to increase around the same time as the change in roaming policy, our results may be driven by these factors instead of the actual change in regulation. However, Figure 2 shows that the number of roaming drivers did not change considerably around the time of the change in policy (June 2017). In contrast, the proportion of foreign cars was the lowest in 2017, indicating that the number of home users (slightly) increased cars. Importantly, this suggest that our approach yields conservative estimates.

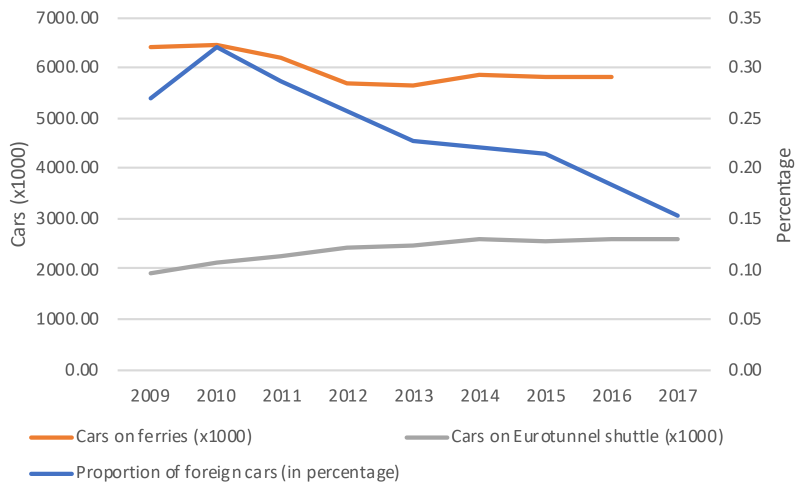

Figure 2: Trend in foreign cars in the UK

Note: Data on the number of cars that make use of the Eurotunnel shuttle services come from Getlink (nd), for number of cars on ferries from Department for Transport (2017b), and estimates of the proportion of foreign cars in UK traffic come from Department for Transport (2018).

Table 1: Regression results for month-district observations using levels and logs.

	Accidents		log(Accidents)	
	(1)	(2)	(3)	(4)
Post regulation	-1.289***		-0.151***	
	(0.331)	(0.000)	(0.015)	(0.000)
Roaming accidents	-9.167***	-10.145***	-1.879***	-1.967***
	(0.796)	(0.832)	(0.057)	(0.080)
Post × Roaming accidents	1.297***	1.862***	0.172***	0.236***
	(0.334)	(0.287)	(0.050)	(0.042)
Constant	9.196***		1.918***	
	(0.797)		(0.060)	
Season-driver FE	No	Yes	No	Yes
Month FE	No	Yes	No	Yes
Observations	33,408	33,408	16,278	16,278
R^2	0.390	0.400	0.128	0.149

Notes: Estimated using weighted least squares, with total no. of accidents per district as weights. Robust standard errors in parentheses are clustered at the district level. Season-driver FE contain (month of the year)×(type of driver dummies).***, **, * indicate significance at 1%, 5%, and 10%.

4. Results

4.1. *Main results*

Table 1 shows the results of four specifications using monthly data per region (UK district). In all models we find a statistically significant increase in the number of accidents following the roaming regulation. In column (1) we find a point estimate of 1.297, which implies an increase in the number of accidents per region per month of over 10%. When we control for diver-type-specific seasonal effects and month fixed effects, the point estimate increases to 1.862. The difference between columns (1) and (2) is, however, within two standard errors of either estimate. Columns (3) and (4) show that using log(accidents) as dependent yields similar, but somewhat stronger effects.

We note that specifications with time and seasonal fixed effects, as in columns (2) and (4), yield the strongest impact. This suggests that seasonal effects (e.g. a higher share of roamers during the summer holiday) do not drive our main results. Nevertheless, since roamers are treated for only half a year in our data, we re-estimate all models using a subsample containing just observations from July till December. The results of this estimation, as presented in Table B1 in Appendix B, show that the results hardly change compared to initial results as presented above.

Table 2: Regression results using coarser temporal resolution.

	Accidents (monthly)		Accidents (quarterly)	
	(1)	(2)	(3)	(4)
Post regulation	-1.289***	-1.289***	-3.866***	-3.866***
	(0.222)	(0.223)	(0.668)	(0.668)
Roaming accidents	-9.167***	-9.167***	-27.502***	-27.502***
	(0.784)	(0.785)	(2.353)	(2.355)
Post × Roaming accidents	1.297***	1.297***	3.892***	3.892***
	(0.222)	(0.222)	(0.666)	(0.667)
Constant	9.196***	9.196***	27.588***	27.588***
	(0.786)	(0.787)	(2.358)	(2.360)
Time series variation	Yes	No	Yes	No
Observations	33,408	1,392	11,136	1,392
R^2	0.390	0.438	0.415	0.438

Notes: Estimated using weighted least squares, with total no. of accidents per district as weights. Robust standard errors in parentheses are clustered at the district level.***, **, * indicate significance at 1%, 5%, and 10%.

So far, we used region-month aggregations of accidents. A potential issue with that approach is over-rejection due to autocorrelation (see the discussion in Section 2). Table 2 presents results that deal with this issue. In column (1), for ease of comparison, we again present the same monthly results as before. In contrast, the results in column (2) stem from an estimation where we ignored all time-series variation and aggregated the data into two periods: pre-treatment and post treatment. By construction, the point estimates are the same. Interestingly however, is that the standard errors are hardly affected. This suggests that autocorrelation does not play a large role in the monthly observations, which implies that our initial approach might not be prone to over-rejection.

Similarly, in column (3) we present results using quarterly aggregated number of accidents per district, while in column (4) we further aggregate to two time-periods. Again, we see only a slight increase in the standard errors of the estimates, suggesting that autocorrelation does not pose a threat to the accuracy of our standard errors and confidence intervals.

As above, we have re-estimated these specifications using observations from July till December only. Results of these estimations, as presented in Table B2 in Appendix B, show that the results are again very similar in this case.

Table 3: Regression results using binary response per road.

	Monthly		Quarterly		Half-yearly	
	(1)	(2)	(3)	(4)	(5)	(6)
Post regulation	-0.006***	-0.008***	-0.018***	-0.023***	-0.036***	-0.041***
	(0.000)	(0.000)	(0.001)	(0.001)	(0.002)	(0.002)
Roaming accidents	-0.041***	-0.045***	-0.117***	-0.115***	-0.221***	-0.215***
	(0.000)	(0.000)	(0.000)	(0.001)	(0.001)	(0.001)
Post × Roaming accidents	0.006***	0.008***	0.018***	0.023***	0.036***	0.041***
	(0.000)	(0.000)	(0.001)	(0.001)	(0.002)	(0.002)
Constant	0.041***	0.045***	0.117***	0.116***	0.221***	0.216***
	(0.000)	(0.000)	(0.000)	(0.001)	(0.001)	(0.001)
Season-driver FE	No	Yes	No	Yes	No	No
Share >1 accident	3.00%	3.00%	7.55%	7.55%	12.82%	12.82%
Observations	4,152,168	4,152,168	1,325,160	1,325,160	618,408	618,408
R^2	0.020	0.021	0.061	0.061	0.121	0.121

Notes: Estimated using weighted least squares, with total no. of accidents per district as weights. Robust standard errors in parentheses are clustered at the level of a road segment. Season-driver FE contain (month of the year)×(type of driver dummies). Share > 1 accidents denotes the share of observations where there were more than 1 accident in the total of observations where there was at least one accident.***, **, * indicate significance at 1%, 5%, and 10%.

4.2. *Binary response*

Table 3 presents the regression results using a linear probability model on road-segments as observations. For all specifications we find a statistically significant increase in the probability of an accident occurring at a road segment after the regulation. In columns (1) and (2) we find that phone use is associated with a 0.6 or 0.8 percentage point increase, which implies an increase of 14.6% and 17.7% respectively. We find similar, and even stronger, effects when we aggregate to quarterly and two-period observations in column (3) and (4).

4.3. *Implications*

Our results show that phone use is associated with an increase in the number of accidents of 14%, when we use our most conservative estimate (first column in Table 1). As a counterfactual, we can calculate total number of accidents in the UK for a situation where all drivers face a phone usage fee equal to the average pre-regulation roaming charges. This is a valid generalization under the assumption that the mechanism identified for roamers carries over to all drivers. Under this assumption, our results then indicate that each year in the UK, 3,005 singe-vehicle accidents are caused by phone use. Furthermore, if we assume that the identified effect also applies to multi-vehicle cases, we find that phone use leads to 17,093 additional accidents per year, of which 205 were fatal.

5. Conclusion

We provide novel evidence of the effect of cell phone use on car accidents. We exploit variation in the cell phone usage fees in the United Kingdom (UK) following European Union (EU) roaming regulations in 2017. This policy intervention is used as a treatment that only applies for non-UK drivers from the EU, which allows us to use a difference-in-differences approach. We find that phone use causes the number of accidents to increase with at least 10%. It is plausible that this mechanism results carry over to home drivers. Our results then imply that each year in the UK as many as 17,093 accidents are associated with phone use.

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A. Additional descriptives

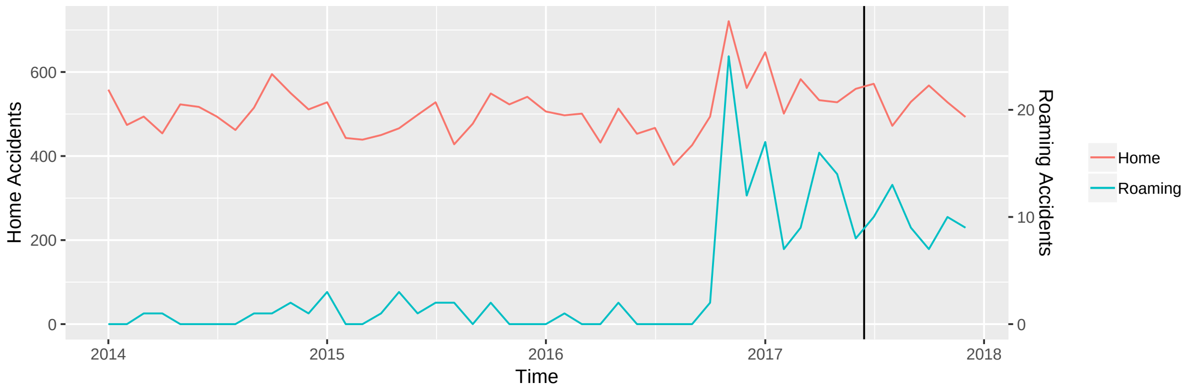

Figure A1: Number of roaming and home accidents per month reported by London Metropolitan Police.

B. Robustness checks

Table B1: Regression results for month-district observations second half of each year.

	Accidents		log(Accidents)	
	(1)	(2)	(3)	(4)
Post regulation	-1.853***		-0.211***	
	(0.391)	(0.000)	(0.017)	(0.000)
Roaming accidents	-9.769***	-9.562***	-1.962***	-1.992***
	(0.869)	(0.839)	(0.063)	(0.067)
Post × Roaming accidents	1.861***	1.861***	0.250***	0.235***
	(0.393)	(0.289)	(0.046)	(0.043)
Constant	9.798***		1.984***	
	(0.871)		(0.060)	
Season-driver FE	No	Yes	No	Yes
Month FE	No	Yes	No	Yes
Observations	16,704	16,704	8,158	8,158
R^2	0.391	0.399	0.148	0.163

Notes: Estimated using weighted least squares, with total no. of accidents per district as weights.***, **, * indicate significance at 1%, 5%, and 10%.

Table B2: Regression results using only for second half of each year.

	Accidents (monthly)		Accidents (quarterly)	
	(1)	(2)	(3)	(4)
Post regulation	-1.853***	-1.853***	-5.559***	-5.559***
	(0.289)	(0.290)	(0.818)	(0.547)
Roaming accidents	-9.769***	-9.769***	-29.307***	-29.307***
	(0.856)	(0.857)	(2.694)	(1.315)
Post × Roaming accidents	1.861***	1.861***	5.584***	5.584***
	(0.289)	(0.289)	(0.820)	(0.541)
Constant	9.798***	9.798***	29.394***	29.394***
	(0.858)	(0.859)	(2.699)	(1.317)
Time series variation	Yes	No	Yes	No
Observations	16,704	1,392	5,568	1,392
R^2	0.391	0.437	0.416	0.437

Notes: Estimated using weighted least squares, with total no. of accidents per district as weights.***, **, * indicate significance at 1%, 5%, and 10%.