Stay or Flee? Probability versus Severity of Punishment in Hit-and-run Accidents

Stefano Castriota – University of Pisa*
Mirco Tonin – Free University of Bozen-Bolzano**

December 2019

Abstract

The empirical literature testing the economic theory of crime has extensively studied the relative importance of the probability and the severity of punishment with reference to planned criminal activities. There are, however, also unplanned crimes and in this paper we focus on a very serious and widespread one, hit-and-run road accidents. In fact, it is not only unplanned, but also largely committed by citizens without criminal records and the decision whether to stay or run must be taken within a few seconds. Using Italian data for the period 1996-2016, we rely on daylight as an exogenous source of variation affecting the probability of apprehension and find that the likelihood of hit-and-run conditional on an accident taking place increases by around 20% with darkness. Relying on two legislative reforms which increased the penalties in case of hit-and-run, we find no significant effect on drivers' behavior. Our results show that criminal activities in unplanned circumstances and under intense time pressure and emotional distress are deterred more by the certainty rather than the severity of legal sanctions.

Keywords: Crime, hit-and-run, road accidents, punishment.
JEL codes: D91, K14, K42, R41.

* Stefano Castriota, Department of Political Sciences, University of Pisa, Via Serafini 3, 56126, Pisa (Italy). stefano.castriota@unipi.it
** Mirco Tonin, Faculty of Economics and Management, Free University of Bozen-Bolzano, piazza Università, 1, 39100, Bolzano (Italy). mirco.tonin@unibz.it

We gratefully acknowledge financial support by the Free University of Bozen-Bolzano CRC grant. We would like to thank Milo Bianchi, Nadia Campaniello, Claudia Di Caterina, Francesco Drago, Davide Dragone, Erdal Ekin, Martin Halla, Benjamin Hansen, Giovanni Mastrobuoni, Alex Moradi, Franco Peracchi, Paolo Pinotti, Paolo Roberti, Steve Stillman, Christian Traxler, Paolo Vanin and participants at various conferences and workshops for useful comments.

1. Introduction

Since Becker's (1968) seminal paper, which first applied an expected utility model to criminal behavior, economists have extensively analyzed incentives to violate the law, both theoretically and – particularly in more recent times – empirically. The theoretical framework by Becker, with its focus on weighting the expected benefit of committing a crime against the expected cost, is particularly well suited to study crimes that require planning, a vast set going from insider trading to pickpocketing.

The empirical literature testing the economic theory of crime has extensively studied the deterrent and incapacitation effects of prison (Drago et al. 2009; Buonanno and Raphael, 2013; Barbarino and Mastrobuoni, 2014) or the impact of the probability of detection (Kleven et al, 2011; Doleac and Sanders, 2015; Mastrobuoni and Rivers, 2019).[1] These studies have typically focused on general crime or on specific felonies, e.g. bank robberies or tax evasion, that are planned. This type of criminal activity does not, however, exhaust the category, as there exist crimes that are unplanned. It is therefore of interest to investigate whether the predictions of the economic model of crime hold also in these circumstances. While for some types of crime planned and unplanned cases probably co-exist (e.g. in the case of homicides, murders vs. involuntary manslaughter), in other instances criminal activity is characteristically unplanned.

This is the case for the crime we study in this paper, namely hit-and-run road accidents with injured or dead victims. This crime is by its nature unplanned as it follows an unforeseen event, a car accident involving the death or injury of another person, and the decision is adopted under strict time constraints and dramatic psychological conditions which could compromise the agent's judgment (Hammond, 2000). This would make it less likely to have a rational response to incentives. Another peculiarity of hit-and-run is that it is largely committed by citizens without

[1] Freeman (1999) provide an overview on the economics of crime literature, while Chalfin and McCrary (2017), Dobb and Webster (2003) and Durlauf and Nagin (2011) focus on crime deterrence.

criminal records, who have higher discount factor (Åkerlund et al., 2016; Mastrobuoni and Rivers, 2016) and higher risk aversion (Block and Gerety, 1995) with respect to criminals.[2] Economic theory predicts that agents with such a psychological profile should react more to penalties (Becker, 1968; Chalfin and McCrary, 2017). Therefore, if there is a rational response to incentives, it should be easier to detect it.

Beside these aspects, hit-and-run road accidents with injured or dead people is a very serious crime that is worthy of investigation *per se*. In fact, around 35% of victims die within 1-2 hours from the accident, therefore delaying the emergency response dramatically reduces survival rates (Tay et al., 2009). Hit-and-run is also very common around the world. According to the AAA Foundation for Traffic Safety, in the US more than one hit-and-run crash – with or without serious consequences – happens every minute, while in 2015 these types of accident were responsible for 1,819 fatalities (5.1% of total in road accidents) and 138,500 serious injuries (5.9%), with a steady increase in recent years in both absolute and relative terms.[3] In selected EU countries, the share of fatal accidents with hit-and-run over the period 2009-2014 ranged between 1% and 6%, while that of injuries between 2% and 14%, with the United Kingdom presenting the highest incidence in both cases.[4]

In this work, we use data on the universe of Italian road accidents with injured or dead people from 1996 to 2016 to study the responsiveness of hit-and-run to a higher probability of apprehension and to higher penalties, the two key variables in the economic model of crime. Building on a recently developed methodology (Doleac and Sanders, 2015; Taulbee, 2017; Domìnguez and Asahi, 2017; Chalfin et al., 2019), we use variation in daylight to show that the likelihood of hit-and-run conditional on an accident taking place gets higher when the probability of being identified decreases.

[2] In his large literature review of sanctions, perceptions and crime, Apel (2013) concludes that people who commit crimes and avoid being arrested tend to lower their subjective probability of apprehension. Since hit-and-run is an unplanned crime largely committed by citizens without criminal records, in our sample the number of repeated offenders with a decreased subjective probability of apprehension is most likely almost null.

[3] See https://aaafoundation.org/hit-and-run-crashes-prevalence-contributing-factors-and-countermeasures/.

[4] See http://traffic-psychology-international.eu/wp-content/uploads/2016/01/Hit-Run-Overview.pdf, fig. 1 and 2.

Then, we study the impact of two legislative reforms introduced in 2003 and 2016 which increased the penalty for hit-and-run to show that harsher penalties do not produce any significant short-term effects on hit-and-run.

This paper contributes to the economics literature on crime by studying a so far neglected felony that is of interest both for its dire societal consequences, as well as for its unplanned nature. One further advantage of studying hit-and-run road accidents is the absence of some of the issues that usually make the identification of the effects of harsher penalties and increased probability of detection difficult.

One such issue is crime displacement (see, for instance, Pricks, 2015). Having a serious road accident and running away are undesired and unplanned events, therefore a policy successful in decreasing the share of hit-and-run would not cause a spatial displacement or a parallel increase in other types of offence which could potentially leave the overall number of victims unchanged. In other words, while it is possible that harsher penalties for robberies could increase, say, burglaries, this is unlikely to be a concern for hit-and-run.

Another issue is that it is often difficult to distinguish between the deterrence and incapacitation effect of penalties (Levitt, 1998), that is, the effect on crime of the change in behavior of free citizens from that of the increased number of incarcerated people who cannot repeat the same or other offences. If we cannot disentangle deterrence from incapacitation, the impact of policies aimed at reducing crime would mix the two, making it difficult to measure their cost, since incapacitation implies additional incarceration costs. For hit-and-run accidents, however, the percentage of repeated offenders is most likely extremely low. For the few drivers who could repeat the same offence, it should be kept in mind that in Italy on average it takes four years to get a final penal sentence. Therefore, the effect of incapacitation on these few repeated offenders would eventually be observed with a long delay from the legal reforms introduced in 2003 and 2016, while in the short-term it would not mix with deterrence. So, as incapacitation plays a negligible role in our context, we are able to identify the effect of deterrence alone.

Beside the economics of crime literature, this paper is also related to the literature studying driving behavior. For instance, De Paola et al. (2013) investigate the effect on accidents of the penalty point system, while De Angelo and Hansen (2014) focus on the effect of policing, and Traxler et al. (2018) look at the impact of penalties on speeding. The focus of our study, however, is not driving behavior or the car accidents in itself, but rather the decision on whether to stay or flee after the accident has already happened.

The rest of the paper is structured as follows. Section 2 discusses the identification strategy. Section 3 describes the dataset and reports descriptive statistics. Section 4 analyzes the impact of a higher probability of detection, while Section 5 studies the short-term impact of the 2003 and 2016 legal reforms that increased the penalties. Section 6 performs a number of robustness checks. The last section discusses the policy implications of the empirical estimates and offers a brief conclusion.

2. Identification strategy

To identify the impact of the probability of detection on hit-and-run, we adopt two different identification strategies based on daylight. The idea is that daylight makes identification of the car by law enforcement more likely, for instance by making it easier for victims or witnesses to read the car plate or to identify the make or color of the vehicle, therefore increasing the probability of apprehension. The importance of light for the likelihood of driver identification in hit-and-run road accidents has been shown by MacLeod et al. (2012) with US data on single pedestrian-motor vehicle fatal crashes over the period 1998-2007.

The effect of light on crime is very clearly identified by Chalfin et al. (2019) who use random variation in streetlights in New York City to show a sizeable reduction in outdoor crimes connected with more lightning. Using exogeneous changes in daylight to study the effect of a higher probability of detection on criminal activity solves the issue of reverse causality that would be present, for instance, when using police presence as a measure of enforcement.[5]

In this paper, we first consider the 7 days (one whole week) or 5 days (excluding weekends) before and after moving to Winter Time (WT, at the end of October) and to Daylight Saving Time (DST, at the end of March). Due to the change to WT we earn one hour of daylight in the morning and lose one hour in the afternoon, and vice versa due to the change to DST. Specifically, when moving to Winter Time, in Italy we lose one hour of darkness (-1) from 7AM to 9AM, while from 4PM to 6PM we earn it (+1); when moving to Daylight Saving Time, we earn one hour of darkness (+1) from 6AM to 8AM, while we lose one hour (-1) from 6PM to 8PM (see Table 1 for a summary). We exploit the changes around these dates to identify the effect of daylight.

In particular, we compare the incidence of hit-and-run, that is, the likelihood of running away conditional on having had an accident, at the same two-hours slots in two adjacent weeks. The type of driving-related activities that are conducted in these two hours slots is presumably very similar across the two weeks, in terms, for instance, of commuting or drinking behavior. This should be particularly so when looking only at weekdays, when people have typically less flexibility regarding the scheduling of their day due to their working activity. Of course, in these specific periods of the year, these two hours slots differ in terms of daylight, and therefore a change in the incidence of hit-and-run can be attributed to that.

One disadvantage of looking at time changes is that in the short term sleeping patterns may be altered, as people haven't had time to adapt, and this may affect, for instance, the decision-making process (Harrison and Horne, 2000) and, therefore, the propensity to hit-and-run. However, gaining or losing one hour of sleep is not collinear

[5] There are of course other solutions to this issue. See, for instance, Di Tella and Schargrodsky (2004).

with the variable described in Table 1. Indeed, each transition from WT to DST or vice versa involves either one hour more sleep or one hour less sleep, but it always involves both a time slot with more darkness and a time slot with less darkness, so it is possible to disentangle the effect of darkness from that of having one hour more sleep. Finally, to confirm that the difference is indeed due to daylight, we also conduct placebo tests, applying the very same methodology to the earlier and later two-hours slots, where there is no effect due to daylight, but where effects due to other concurrent factors, like the change in sleeping patterns, should be present.

This identification strategy has long been used in medicine and engineering to test the effect of daylight on road accidents with mixed results (for a review, see Carey and Sarma, 2017). More recently, economists have used the time change to show the negative impact of daylight on criminal behavior. Doleac and Sanders (2015) use Regression Discontinuity and Differences-in-Differences applied to US data to show the negative impact of Daylight-Saving Time on robberies. This conclusion is confirmed with similar methodologies by Taulbee (2017) with other US data, and by Domìnguez and Asahi (2017) with Chilean data.

We complement the analysis with a second approach that exploits more long-term changes in daylight. We exclude mid-seasons and consider only winter (from November to February) and summer (from May to August) months and focus on three time windows, namely 2PM-4PM, 6PM-8PM, and 10PM-12PM. In Italy in both seasons between 2PM-4PM it is bright, between 10PM-12PM it is dark, while between 6PM-8PM it is dark during wintertime and bright during summertime. Therefore, we can identify the impact of daylight in a differences-in-differences framework.

This focus on the longer term allows drivers to adapt to time changes and get used to darkness/brightness, while the previous approach focuses on the short-term, when people are more likely to maintain the same driving habits (Doleac and Sanders, 2015). The threat of this second approach is, of course, that people may have systematically different behavior in terms of driving-relevant activities between winter and summer, but looking at the two time slots adjacent to 6PM-8PM allow us to check whether this is indeed the case.

To identify the effect of higher sanctions, we exploit two legislative reforms that affected the penalty for hit-and-run accidents. In particular, we focus on two changes introduced by Law No. 72/2003 and Law No. 41/2016. The first raised the penalties for hit-and-run from up to 3 to up to 12 months of prison or, alternatively, from up to 310 Euros to up to 2.500 Euros. With the second reform, the increase in the penal sanctions imposed for road homicide and serious injuries jumped, in case of hit-and-run, by a minimum of one third and maximum of two thirds of the original penalty, with a minimum overall verdict of 5 years.

One advantage of our study compared to other studies using legislative reforms is that, given the unforeseen nature of the circumstances under which fleeing after a car accident happens, anticipation effects are unlikely. In fact, the two reforms became effective on a certain date and, conditional on having had a car accident, there was nothing agents could do in advance to reduce the consequences of the new laws. Therefore, the identification of the effect of the reforms by looking at the discontinuity around the thresholds is not hindered by a gradual change in agent's behavior regarding hit-and-run before the new laws took force.

This methodology allows us to test the short-term effects of the reforms. Ideally, to test for the long-term effects we would need a control group for which the law did not change in order to apply a differences-in-differences methodology. This is, however, not feasible as both laws applied to the whole population. One potential threat to this identification strategy is a change in the composition of drivers having accidents. Our data allows us to check, at least partially, whether this is indeed the case and we will discuss the results in the following sections.

3. Dataset and summary statistics

Data come from the Italian Institute of Statistics (ISTAT) which provided us with the records of every single road accident with injured or dead people from 1996 to 2016, with over 4.5 million observations. The database contains information on the time (hour) and location (province) of the accident; on some characteristics of drivers, passengers, pedestrians and bikers involved (gender, age) and of their transport vehicles (e.g. whether it is a car or a motorbike); on the road type and characteristics (e.g. rural or urban, number of lanes, conditions,…); on climate conditions; on the total number of injured and dead people. We know whether somebody flees after an accident, but have no information about the subsequent identification of the driver and his or her characteristics.[6]

When looking at the impact of the probability of detection, our outcome variable is a dummy variable taking the value of one if an accident was a hit-and-run. Control variables include dummy variables for region and for day of the week, month, year and national holiday that capture factors like the seasonality connected to traffic, patterns in alcohol consumption related to weekends and other unobservables that are invariant to region or time. Furthermore, to take into account the effect of macroeconomic variables which might affect traffic and accidents, we added real monthly per-capita income (in 2016 constant Euros), regional monthly unemployment rate (obtained from linear interpolation of quarterly data), real monthly insurance price index (net of CPI, January 1996=1), and the real daily Brent oil price (in 2016 constant Euros).

When looking at the impact of the penalty, our outcome variable is instead the share of hit-and-run accidents, that is, the total number of hit-and-run accidents involving at least one dead or injured person that occurred in a given time period (a day or a

[6] Data is collected by the police immediately after the accident and is not integrated with subsequent information on the use of drugs and alcohol, former criminal records or traffic violations.

fraction of a day) over the total number of accidents involving at least one dead or injured person that occurred in the same time period.

Table 2a describes the variables and reports the summary statistics. The share of hit-and-run is equal to 0.6% of accidents. June and July are the months with the highest share of accidents, while the distribution over the week is quite constant apart from Sunday, with less accidents. Most of accidents occur in urban roads, then in regional roads, and seldom in highways. The number of victims can vary dramatically, with an average of 0.02 dead and 1.42 injured per accident.

While there is no clear geographical pattern in terms of the incidence of hit-and-run, with high and low incidence regions present in both the North and the South of Italy (see Figure A1 in the Appendix), there appear to be an increasing time trend. Figure A2 reports the scatterplot of the daily share of hit-and-run from January 1996 to December 2016, together with a time trend obtained through a smooth local linear polynomial. What emerges is a sharp increase over the last fifteen years, from the minimum of 0.4% in the early 2000s to almost 1% in 2016, an increase of around 150% in relative terms. Even if Italy had lower percentages when compared with other Western countries, the problem is becoming more serious over time, a general trend observed also in the US. The two red vertical lines indicate the legal reforms implemented in 2003 and 2016.

Within a day, the distribution of hit-and-run is not uniform. Table 2b displays by time windows of two hours the total number of road accidents and that of hit-and-run, plus the relative share of hit-and-run over the total number of accidents. The evidence shows that both the number of accidents and of hit-and-run is higher between 8AM-8PM, while the share of hit-and-run is higher between 8PM-8AM.

4. Effect of probability of apprehension on hit-and-run accidents

In this section, we study the effect of probability of apprehension on hit-and-run accidents by using exogenous variation in daylight. The analysis relies on logit regressions with robust standard errors and dummy variables to control for time and space fixed effects (national holiday, day of the week, month, year, region). The dependent variable is a dummy equal to 1 if the accident was a hit-and-run. We multiply the coefficients, standard errors and marginal effects by 100 to make the results easier to read, so they should be interpreted as the change in the likelihood of a hit-and-run in percent.[7]

As a descriptive exercise on the correlates to hit-and-run, Table A1 in the Appendix reports the marginal effects of the logit regressions for the full sample (column 1), for accidents between a vehicle and a pedestrian (column 2) and for accidents involving at least one motorbike (column 3). Looking at the full database (column 1), in line with the literature (see Tay et al., 2009; MacLeod et al., 2012; Jiang et al., 2016), the likelihood of hit-and-run increases during national holidays, in the week-ends, by night and during wintertime, when presumably the consumption of alcohol and leisure time are higher and when it is dark. Drivers run away more often in urban areas and when the counterpart can hardly chase (accidents involving a motorbike and, mostly, a pedestrian).

Results are similar also when we restrict the analysis to the two subsets of accidents with pedestrians and motorbikes involved (columns 2 and 3). The coefficient on night (defined as 10PM-5AM) is positive, but it may capture not only changes in the probability of apprehension due to the absence of daylight, but also, for instance, changes in alcohol and drug consumption that are correlated with time of the day.

---

[7] Hit-and-run accidents are relatively rare. To check whether this represents an issue for the estimation, we implemented the R package "bgeva" (Marra et al., 2013) that fits regression models for binary rare events where the link function is the quantile function of the generalized extreme value (GEV) random variable.
The fully parametric univariate GEV model is coherent with the logistic analysis and does not improve the goodness of fit to the data (results available upon request). Therefore, we implement the more commonly used logistic regression.

To identify the effect of daylight we use the two strategies described in Section 2. Before moving to the regression analysis, we first report some descriptive statistics on the share of hit-and-run. Looking at the periods around the change to WT and DST (Table 2c, upper part), we can see that, compared to the seven days before the time change, there is a lower incidence of hit-and-run when there is more daylight (0.47% instead of 0.54%) and a higher incidence when there is more darkness (0.71%). Results are very similar in the panel just below, where we exclude the week-end (5 days).

Looking at accidents in the three selected time windows during winter and summer (in the lower part of Table 2c), we see that there is no significant difference between the two seasons in the time windows 2-4 PM and 10-12 PM, while there is a significant difference in the 6-8 PM window, with hit-and-run accidents rising from 0.47% in summer to 0.79% in winter. So, while winter and summer are not different in the time slots when light is present in both or in none, they differ in the time slot when it is bright in summer, but dark in winter.

Table 3a reports the regression results on the short-term effects of darkness on the probability to hit-and-run. In column 1 we identify the short-term effects of daylight by considering the 7 days before and 7 days after the Spring and Fall equinoxes. We consider only the two-hour time window when there is an increase or decrease of darkness. We exclude all the observations outside this window and run a logit regression on the hit-and-run dummy variable over the same standard set of controls of Table A1, column 1. The control variable of interest is "change in hours of darkness" which is equal to 0 in the 7 days before the time change and equal to +1 or -1 afterwards, depending on whether we are moving to WT or DST and whether it is the morning or evening time slot (see Table 1 for a summary).

Results in column 1 of Table 3a show that there is a significant effect of one additional hour of darkness on this type of crime, with a 0.11% higher probability to run after an accident which, applied to the 0.57% average, implies a 19% short-term increase.

To better understand the source of this effect and see whether it is concentrated in a particular period or not, we also run a specification (not reported) with dummies for the four different periods indicated in Table 1. From this, it emerges that the effects of the two periods with an additional hour of light (morning of WT and evening of DST) are not statistically different from each other. The same for the two periods with one additional hour of darkness (evening of WT and morning of DST).

The effect is also present when controlling separately for one additional hour of sleep. The results in Table A2 show that the variable capturing one additional hour of sleep is not significant, while the marginal effect of darkness increases, thus showing that our findings are not due to changes in sleeping hours. In columns 2 and 3 of Table 3a, we perform two placebo tests by applying the very same methodology to the earlier and later two-hours slots, when there is no variation in daylight. The fact that the coefficients are in these cases insignificant confirms that it is indeed daylight affecting the likelihood of hit-and-run and not some other concurrent event that affect differentially the weeks before or after the switch to WT or DST. Finally, in the lower panel, in columns 4-6, we repeat the analysis by excluding the weekends from the sample, confirming the previous findings.

With the second methodology (Table 3b), we exclude mid-seasons and consider only four months around the winter and summer solstices, namely November-February for the winter and May-August for the summer. We consider only those accidents that occurred in the time windows 2PM-4PM (column 2), 6PM-8PM (column 3), and 10PM-12PM (column 4) and the full sample of accidents in the three mentioned time windows (column 1).

These specifications allow us to isolate the effect of daylight. In fact, in the first case (2PM-4PM) it is always bright both during winter and during summer, in the third (10PM-12PM) it is always dark, while in the second (6PM-8PM) it is always bright during the summer and always dark during the winter. In column 1 we apply a differences-in-differences model with interacted winter and time dummy variables

("DID-Winter-6PM-8PM" and "DID-Winter-10PM-12PM"), while in column 2-4 we run separate regressions, thus allowing all coefficients to differ across the three time slots.

Results of column 1 show that, net of all the standard control variables already included in column 1 of Table A1, the interacted dummy "DID-Winter-6PM-8PM" is statistically and economically significant, which is not true for the other interacted dummy variable "DID-Winter-10PM-12PM".[8] The insignificant coefficient of this latter variable indicates that there is no differential trend in how the incidence of hit-and-run changes with time of the day between summer and winter, when we consider time windows for which there is no change in darkness. The significant coefficient of the former variable captures the effect of the different brightness of the 6PM-8PM windows between the two seasons.

The separate regressions of columns 2-4 confirm the long-term effects of daylight; in these specifications the variable of interest is "winter", which is significant only in the time window 6PM-8PM (column 3), when it is bright during the summer and dark during the winter. The effect of daylight is not only statistically, but also economically significant. The marginal effect for two additional hours of darkness ranges between 0.16% (column 1) and 0.21% (column 3), which for one hour is 0.08-0.10%. Since the average rate of hit-and-run is 0.57%, this means that one additional hour of darkness generates a long-term increase of 14-18%.

Both methodologies thus confirm that an increase in darkness, associated with an increase in the probability of detection, increases the likelihood of hit-and-run accidents.

[8] A post-estimation chi2-test confirms that the two diff-in-diff coefficients are statistically different (chi2-test that the difference between the coefficients of DID-6PM-8PM - DID-10PM-12PM = 0 is equal to 22.69, P-Value=0.0000).

5. Effect of harsher penalties on hit-and-run accidents

Here we move to the second part of the analysis and focus on the intensity of the penalty. First, we calculate the daily share of hit-and-run which is used as a dependent variable in a Regression Discontinuity in Time (RDiT), or event study, analysis around two reforms implemented in 2003 and 2016.

The analysis is then carried out in two-stages. In the first, we run OLS regressions of the daily share of hit-and-run over a set of seasonal dummy variables (national holiday, day of the week and month of the year) and the daily real oil price in constant 2016 Euros. Results are omitted for reasons of space, but are available upon request. In the second stage, we run local linear regressions of the residuals of the first stage and show that there is no discontinuity around the threshold and, therefore, the two reforms do not change behavior, at least in the short-term time frame we consider.

We use symmetric windows of different length around the cutoff (300 days+300 days, 200 days+200 days and 100 days+100 days) and the results are robust[9]. Since the second policy was implemented on 25th March 2016 and we have data until the end of the year, in the analysis of the effect of the road homicide the window is actually restricted to 282 days instead of 300 days.

Looking at Figure 1a we see that the first reform *increased* – rather than decreased – the share of hit-and-run, even though the confidence intervals overlap. In Figure 2a, it is evident that there is no jump once penalties became more severe. At least in the short-term, it thus appears that harsher penalties do not cause any reduction in the share of hit-and-run. When we restrict the sample to more homogeneous types of accidents, like accidents involving pedestrians (Figures 1b and 2b) and accidents involving motorbikes (Figures 1c and 2c), there is again no evidence of discontinuity.

[9] Results on the 200 days+200 days and 100 days+100 days are not shown for reasons of space, but are available upon request.

One may worry that harsher penalties may be associated with weaker enforcement, for instance in terms of police patrols in the streets, therefore explaining the lack of behavioral response by drivers. From 2005 onwards, we have daily data from the Italian State Police on the number of police patrols.[10] Applying the same RDiT methodology described above using daily number of police patrols in the Italian roads as dependent variable, we find that there is no discontinuity around the 2016 reform (Figure 2d). Moreover, controlling for the number of police patrols does not change the results (not reported). Due to data availability we cannot look also at the 2003 reform, but this result suggests that contemporaneous changes in enforcement are not behind the lack of responsiveness to harsher penalties.

There are two possible explanations for the ineffectiveness of harsher penalties. As pointed out by Polinski and Shavell (2000, p. 68), "individuals may have incomplete knowledge of the true magnitudes of sanctions, particularly if sanctions are not fixed by law, but are to some degree discretionary". In our specific case, the two reforms have been extensively discussed during the political debate and reported by the press and the media, also through communication campaigns promoted by the Italian Ministry of Transport. Therefore, drivers' unawareness about the change in the penalty is unlikely, but people may still not focus on information about increased penalties under the stressful circumstances of an accident. The uncertainty about the judges' leanings, moreover, could also have played some role, inducing the individual's perceived expected penalty to move less than the actual change.

A second possible explanation is stigma. Convicted criminals suffer not only from public penalties and limited labor market opportunities, but also from the reluctance of people to interact with them from an economic and social point of view (Rasmusen, 1996; Funk, 2004). While there is evidence that length of sentence has an impact on prisoners' utility (Campaniello et al., 2017), for ordinary citizens – as in our case – the disutility from the stigma of going to jail might depend much more on having

---

[10] https://www.poliziadistato.it/pds/stradale/archivio/. Notice that in Italy other police forces (e.g. municipal police or Carabinieri) are also active on the roads. Thus, we have data on an (important) subset of road patrolling activities.

spent any time there rather than on the length of imprisonment itself, which is the margin affected by the increase in penalties. In any case, it is important to show that there is little reaction in both instances of increase in penalties.

6. Robustness checks

In order to test the robustness of our findings we run a number of additional regressions, with results available in the Appendix.

First of all, the null effect we find for harsher penalties using a RDiT methodology could be due to heterogeneous effects of the legislative reforms in terms of drivers' behavior in different hours of the day. So, for instance, harsher penalties could be conductive to a lower incidence of hit-and-run, as predicted by the economic model of crime, during the day, but to a higher incidence during the night, when drunkenness and substance abuse have a bigger role and can impair drivers' judgement.

To see whether this is indeed the case, we check the effect of legislative reforms in different times of the day. In particular, we distinguish between day and night by looking separately at the time windows 6AM-9PM and 9PM-6AM (Figures A3a-A3d). The insignificant discontinuities found in Figures 1-2 are confirmed. Therefore, the null effect of legislative reforms does not appear to be due to responses by drivers to harsher penalties that are of opposite sign in different times of the day.

One important issue that could affect our results is a change in the composition of drivers having accidents. The propensity to flee after an accident may be systematically different among different types of drivers (e.g. males vs. females or young vs. old). If this is indeed the case, then the effect we detect could be the sum of a behavioral effect and a compositional effect, that is, changes in the population of drivers having accidents that could be due to changes in the population of drivers or to changes in the likelihood of having an accident.

Looking at the literature using data on hit-and-run where the driver was subsequently identified, something that happens in approximately half of the cases, male and young drivers appear to have a higher propensity to run after an accident than female and elderly people. This is the case in the US (see Solnick and Hemenway, 1995, for accidents involving pedestrians, and Tay et al., 2008, for all accidents), as well as in the UK (Broughton, 2004). Albeit based on less systematic evidence, this appears to be the case also for Italy.[11] To see whether a compositional effect is driving our results, we now explore the demographic characteristics of those involved in accidents, looking especially at gender and age. Unfortunately, additional demographic characteristics, for instance regarding education or socio-economic background, are not available in the dataset.

We focus first on the probability of detection that was identified through variation in daylight. In particular, we check whether there are potential compositional effects affecting our estimates based on time shifts. Table 2d displays the statistics for the share of accidents with at least one male driver aged 18-45 – the typical profile of identified people in case of hit-and-run – over the total in the two time windows and days of interest. It appears that, for any given time shift, the share of accidents involving this category of drivers does not change before or after the time shifts.

Moving to the effect of harsher penalties, we look at whether the share of accidents involving young male drivers changes with the change in legislation. What we find is that the legal reform of 2003 had indeed an impact on the share of accidents involving a young male (Figures A4a and A4b). In particular, there is a significant *reduction* in the share of accidents involving a young male. Taking into consideration that this is the category of drivers with a higher propensity to flee, we can therefore exclude that the null effect we observe is due to a compositional change (at least along the dimensions we observe) compensating a behavioral change going in the direction

[11] Source: Associazione Sostenitori ed Amici della Polizia Stradale (ASAPS) - 2017 Annual Report, see https://www.asaps.it/62779-_osservatorio_asaps_pirateria_stradale_2017_calano_gli_episodi__-66_e_i_feriti_-.html.

predicted by economic theory (a reduction in hit-and-run). If anything, the decrease in the share of accidents involving young male drivers should make it more likely to observe an overall decrease in hit-and-run.

7. Policy implications and conclusions

In this work we analyze the effect of probability of apprehension versus intensity of penalty for hit-and-run road accidents, a specific kind of felony that is not planned and follows another not planned event, that is, a road accident with seriously injured or dead people. As such, it is largely committed by ordinary citizens without criminal experience, who must decide within a few seconds from the crash whether to stay or run. We show that there is a reaction to the probability of apprehension, finding instead no evidence of a reaction to the intensity of the penalty. There is thus some support for the economic model of crime, even in circumstances where one could have expected agents not to be responsive to incentives at all, due to the intense emotional stress and the short decision time available.

Since incarceration is very expensive and, in our analysis, harsher penalties do not seem to produce any remarkable deterrent effect, while drivers respond to the probability of apprehension, a policy implication of this study is that it would be more efficient to use resources to implement policies that can increase the probability of identification instead of incarcerating people. An improvement in street lighting would represent such a policy, as in Chalfin et al. (2019).[12] Another would be increased police manpower, policing intensity and investigation technologies, which have been proved to be effective (see Di Tella and Schargrodsky, 2004; De Angelo and Hansen, 2014; Chalfin and McCrary, 2017; Mastrobuoni, 2019), but also expensive. Since the technology is steadily evolving and is becoming cheaper, automatic monitoring and identification systems like Lojack (Ayres and Levitt, 1998) and CCTV

[12] See Farrington and Welsh, 2002, for a review.

(Priks, 2015) – especially with facial recognition[13] – can be a more efficient and sustainable solution.

[13] In his study on the effects of surveillance cameras on crime in the underground of Stockholm, Priks (2015) estimates that the cost of preventing one crime – pickpocketing and robbery – is approximately US$ 2,000. With the improvement of technology and the support of automatic face recognition, these costs could significantly decrease over time.

References

1. Åkerlund, D., Golsteyn, B.H., Grönqvist, H. and Lindahl, L. (2016), "Time Discounting and Criminal Behavior", *Proceedings of the National Academy of Sciences of the United States of America*, Vol. 113, No. 22, pp. 6160-6165.
2. Apel, R. (2013), "Sanctions, Perceptions, and Crime: Implications for Criminal Deterrence", *Journal of Quantitative Criminology*, Vol. 29, No. 1, pp. 67-101.
3. Ayres, I. and Levitt, S.D. (1998), "Measuring Positive Externalities from Unobservable Victim Precaution: An Empirical Analysis of Lojack", *Quarterly Journal of Economics*, Vol. 113, No. 1, pp. 43-47.
4. Barbarino, A. and Mastrobuoni, G. (2014), "The Incapacitation Effect of Incarceration: Evidence from Several Italian Collective Pardons", *American Economic Journal: Economic Policy*, Vol. 6, No. 1, pp. 1-37.
5. Becker, G.S. (1968), "Crime and Punishment: An Economic Approach", *The Journal of Political Economy*, Vol. 76, pp.169-217.
6. Block, M.K. and Gerety, V.E. (1995), "Some Experimental Evidence on Differences between Student and Prisoner Reactions to Monetary Penalties and Risk", *The Journal of Legal Studies*, Vol. 24, No. 1, pp. 123-138.
7. Broughton, J. (2004), "Hit and run accidents, 1990-2002", TRL REPORT 612 https://trl.co.uk/reports/TRL612.
8. Buonanno, P. and Raphael, S. (2013), "Incarceration and Incapacitation: Evidence from the 2006 Italian Collective Pardon", *American Economic Review,* Vol. 103, No. 6, pp. 2437-2465.
9. Campaniello, N., Diasakos, T.M. and Mastrobuoni, G. (2017), "Rationalizable Suicides: Evidence from Changes in Inmates' Expected Length of Sentence", *Journal of the European Economic Association*, Vol. 15, No. 2, pp. 388-428.
10. Carey, R.N. and Sarma, K.M. (2017), "Impact of Daylight Saving Time on Road Traffic Collision Risk: a Systematic Review", *BMJ Open*, Vol. 7, No. 6, pp. 1-14, https://bmjopen.bmj.com/content/7/6/e014319.
11. Chalfin, A., Hansen, B., Lerner, J. and Parker, L. (2019), "Reducing Crime Through Environmental Design: Evidence from a Randomized Experiment of Street Lighting in New York City", National Bureau of Economic Research WP No. 25798.
12. Chalfin, A. and McCrary, J. (2017), "Criminal Deterrence: A Review of the Literature", *Journal of Economic Literature*, Vol. 55, No. 1, pp. 5-48.
13. De Angelo, G. and Hansen, B. (2014), "Life and Death in the Fast Lane: Police Enforcement and Traffic Fatalities", *American Economic Journal: Economic Policy*, Vol. 6, No. 2, pp. 231-257.
14. De Paola, M., Scoppa, V., and Falcone, M. (2013), "The Deterrent Effects of the Penalty Points System for Driving Offences: a Regression Discontinuity Approach", *Empirical Economics*, Vol. 45, No. 2, pp. 965-985.
15. Di Tella, R. and Schargrodsky, E. (2004), "Do Police Reduce Crime? Estimates Using the Allocation of Police Forces after a Terrorist Attack", *American Economic Review*, Vol. 94, No. 1, pp. 115-133.

16. Dobb, A.N. and Webster, C.M. (2003), "Sentence Severity and Crime: Accepting the Null Hypothesis", *Crime and Justice*, Vol. 30, pp. 143-195.
17. Doleac, J.B. and Sanders, N.J. (2015), "Under the Cover of Darkness: How Ambient Light Influences Criminal Activity", *Review of Economics and Statistics*, Vol. 97, No. 5, pp. 1093-1103.
18. Domìnguez, P. and Asahi, K. (2017), "Crime Time: How Ambient Light Affect Criminal Activity", Working Paper, https://ssrn.com/abstract=2752629.
19. Drago, F., Galbiati, R. and Vertova, P. (2009), "The Deterrent Effects of Prison: Evidence from a Natural Experiment", *Journal of Political Economy*, Vol. 117, No. 2, pp. 257-280.
20. Drago, F., Galbiati, R. and Sobbrio, F. (2018), "The Political Cost of Being Soft on Crime: Evidence from a Natural Experiment", Working Paper, https://ssrn.com/abstract=2875317.
21. Durlauf, S.N. and Nagin, D.S. (2011), "Imprisonment and Crime", *Criminology & Public Policy*, Vol. 10, No. 1, pp. 13-54.
22. Farrington, D.P. and Welsh, B.C. (2002), "Improved Street Lighting and Crime Prevention", *Justice Quarterly*, Vol. 19, No. 2, pp. 313-342.
23. Freeman, R. B. (1999), "The economics of crime", *Handbook of labor Economics*, Vol. 3, pp. 3529-3571.
24. Funk, P. (2004), "On the Effective Use of Stigma as a Crime-Deterrent", *European Economic Review*, Vol. 48, No. 4, pp. 715-728.
25. Hammond, K.R. (2000). *Judgments Under Stress*. Oxford University Press: New York.
26. Harrison, Y., and Horne, J.A. (2000), "The impact of sleep deprivation on decision making: a review", *Journal of Experimental Psychology: Applied*, Vol. 6, No. 3, pp. 236-249.
27. Jiang, C., Lu, L., Chen, S. and Lu, J.J. (2016), "Hit-and-run Crashes in Urban River-Crossing Road Tunnels", *Accident Analysis and Prevention*, Vol. 95, pp. 373-380.
28. Kleven, H.J., Knudsen, M.B., Kreiner, C.T., Pedersen, S., and Saez, E. (2011), "Unwilling or Unable to Cheat? Evidence from a Tax Audit Experiment in Denmark", *Econometrica*, Vol. 79, No. 3, pp. 651-692.
29. Levitt, S. (1998), "Why Do Increased Arrest Rates Appear to Reduce Crime: Deterrence, Incapacitation, or Measurement Error?", *Economic Inquiry*, Vol. XXXVI, pp. 353-372.
30. MacLeod, K.E., Griswold, J.B., Arnold, L.S. and Ragland, D.R. (2012), "Factors Associated with Hit-and-run Pedestrian Fatalities and Driver Identification", *Accident Analysis and Prevention*, Vol. 45, pp. 366-372.
31. Marra, G., Calabrese, R. and Osmetti, S.A. (2013), "bgeva: Binary Generalized Extreme Value Additive Models", *R package version 0.2*.
32. Mastrobuoni, G. and Rivers, D.A. (forthcoming), "Optimizing Criminal Behavior and the Disutility of Priso", *The Economic Journal*.
33. Mastrobuoni, G. and Rivers, D.A. (2016), "Criminal Discount Factors and Deterrence", IZA DP No. 9769, http://ftp.iza.org/dp9769.pdf.

34. Mastrobuoni, G. (2019), “Crime is Terribly Revealing: Information Technology and Police Productivity”, Working Paper.
35. Polinsky, A.M. and Shavell, S. (2000), “The Economic Theory of Public Enforcement of Law”, *Journal of Economic Literature*, Vol. 38, No. 1, pp. 45-76.
36. Posner, R.A. (2010). *How Judges Think*. Harvard University Press: Cambridge, MA.
37. Priks, M. (2015), “The Effects of Surveillance Cameras on Crime: Evidence from the Stockholm Subway”, *The Economic Journal*, Vol. 125, No. 588, pp. 289-305.
38. Rasmusen, E. (1996), “Stigma and Self-Fulfilling Expectations of Criminality”, *Journal of Law and Economics*, Vol. 39, No. 2, pp. 519-543.
39. Solnick, S. J., and Hemenway, D. (1995), “The Hit-and-run in Fatal Pedestrian Accidents: Victims, Circumstances and Drivers”, *Accident Analysis and Prevention*, Vol. 27, No. 5, pp. 643-649.
40. Taulbee, L. (2017), “Night Crimes: the Effects of Evening Daylight on Criminal Activity”, Working Paper, http://people.wku.edu/alex.lebedinsky/KEA_papers/TAULBEE.pdf.
41. Tay, R., Barua, U. and Kattan, L. (2009), “Factors Contributing to Hit-and-run in Fatal Crashes”, *Accident Analysis and Prevention*, Vol. 41, pp. 227-241.
42. Tay, R., Rifaat, S. M., & Chin, H. C. (2008), “A logistic model of the effects of roadway, environmental, vehicle, crash and driver characteristics on hit-and-run crashes”, *Accident Analysis & Prevention*, Vol. 40, No. 4, pp. 1330-1336.
43. Traxler, C., Westermaier, F. G. and Wohlschlegel, A. (2018), “Bunching on the Autobahn? Speeding Responses to a ‘Notched’ Penalty Scheme”, *Journal of Public Economics*, Vol. 157, pp. 78-94.
44. Zamanaha, B.Z. (2009). *Beyond the Formalist-Realist Divide: The Role of Politics in Judging*. Princeton University Press: Princeton, New Jersey.

Table 1: Change in hours of darkness

When moving to…	Time window	Change in hours of darkness
Winter Time (WT)	7AM-9AM	-1
Winter Time (WT)	4PM-6PM	+1
Daylight Saving Time (DST)	6AM-8AM	+1
Daylight Saving Time (DST)	6PM-8PM	-1

Table 2a: Accidents with injured and/or dead people in Italy, 1996-2016 - Descriptive statistics

Variable	Description	Obs	Mean	S.D.	Min	Max
Hit-and-run	DV=1 in case of hit-and-run	4,549,445	0.006	0.076	0	1
January	DV=1 if accident occurred in January	4,549,445	0.073	0.260	0	1
February	DV=1 if accident occurred in February	4,549,445	0.068	0.252	0	1
March	DV=1 if accident occurred in March	4,549,445	0.080	0.271	0	1
April	DV=1 if accident occurred in April	4,549,445	0.082	0.274	0	1
May	DV=1 if accident occurred in May	4,549,445	0.093	0.290	0	1
June	DV=1 if accident occurred in June	4,549,445	0.095	0.293	0	1
July	DV=1 if accident occurred in July	4,549,445	0.097	0.296	0	1
August	DV=1 if accident occurred in August	4,549,445	0.077	0.267	0	1
September	DV=1 if accident occurred in September	4,549,445	0.086	0.280	0	1
October	DV=1 if accident occurred in October	4,549,445	0.089	0.284	0	1
November	DV=1 if accident occurred in November	4,549,445	0.083	0.276	0	1
December	DV=1 if accident occurred in December	4,549,445	0.078	0.268	0	1
Monday	DV=1 if accident occurred on Monday	4,549,445	0.146	0.353	0	1
Tuesday	DV=1 if accident occurred on Tuesday	4,549,445	0.146	0.353	0	1
Wednesday	DV=1 if accident occurred on Wednesday	4,549,445	0.147	0.354	0	1
Thursday	DV=1 if accident occurred on Thursday	4,549,445	0.148	0.355	0	1
Friday	DV=1 if accident occurred on Friday	4,549,445	0.154	0.361	0	1
Saturday	DV=1 if accident occurred on Saturday	4,549,445	0.144	0.351	0	1
Sunday	DV=1 if accident occurred on Sunday	4,549,445	0.115	0.319	0	1
Night	DV=1 if accident occurred between 10PM and 5AM	4,549,445	0.181	0.385	0	1
Holiday	DV=1 in case of holiday	4,549,445	0.024	0.151	0	1
Good weather	DV=1 in case of good weather	4,549,445	0.791	0.407	0	1
Ruined road	DV=1 in case of ruined road	4,549,445	0.007	0.083	0	1
Non paved road	DV=1 in case of non paved road	4,549,445	0.002	0.048	0	1
Two ways	DV=1 in case of two-ways	4,549,445	0.682	0.466	0	1
Two lanes	DV=1 in case of two-lanes	4,549,445	0.136	0.342	0	1
More than two lanes	DV=1 in case of more than two lanes	4,549,445	0.024	0.152	0	1
Urban	DV=1 in case of urban road	4,549,445	0.759	0.428	0	1
Regional	DV=1 in case of regional road	4,549,445	0.183	0.387	0	1
Highway	DV=1 in case of highway	4,549,445	0.058	0.233	0	1
Total dead	Number of dead people	4,549,443	0.024	0.171	0	40
Total injured	Number of injured people	4,549,443	1.421	0.891	0	72
Nr other vehicles	Number of other vehicles involved	4,549,445	0.026	0.431	0	99
Motorbike	DV=1 in case motorbike was involved	4,549,445	0.401	0.490	0	1
Pedestrian	DV=1 in case a pedestrian was involved	4,549,445	0.084	0.278	0	1
Real pc income	Real monthly per-capita income in 2016 constant €	4,549,445	26,918	1,103	24,814	28,699
Unemployment	Regional monthly unemployment (from quarterly data)	4,549,445	8.208	4.945	1.761	27.105
Real IPI	Real monthly insurance price index (January 1996=1)	4,549,445	1.715	0.270	0.995	2.025
P gas real	Real daily Brent oil price in 2016 constant Euro terms	4,549,445	1.451	0.156	1.216	1.888

Table 2b: Accidents and hit-and-run by time window

Time window	Nr. of road accidents	Nr. of hit-and-run	Share of hit-and-run	95% c.i. of the share of hit-and-run	
12 PM - 2 AM	180,394	1,455	0.0081	0.0077	0.0085
2 AM - 4 AM	127,647	996	0.0078	0.0073	0.0083
4 AM - 6 AM	90,431	676	0.0075	0.0069	0.0080
6 AM - 8 AM	156,633	955	0.0061	0.0057	0.0065
8 AM - 10 AM	493,514	2,480	0.0050	0.0048	0.0052
10 AM - 12 AM	509,648	2,789	0.0055	0.0053	0.0057
12 AM - 2 PM	581,027	2,697	0.0046	0.0045	0.0048
2 PM - 4 PM	515,386	2,370	0.0046	0.0044	0.0048
4 PM - 6 PM	570,484	2,911	0.0051	0.0049	0.0053
6 PM - 8 PM	671,283	3,980	0.0059	0.0057	0.0061
8 PM - 10 PM	384,811	2,778	0.0072	0.0070	0.0075
10 PM - 12 PM	229,621	1,845	0.0080	0.0077	0.0084

Table 2c: Descriptive statistics for hit-and-run over selected days

Share of hit-and-run around the change to WT and DST (7+7)

Season	Nr. of accidents	Nr. of hit-and-run accidents	Share of hit-and-run accidents [95% C.I.]
0 (before time change)	32,862	179	0.0054 [0.0046 0.0062]
-1 (more daylight)	18,922	89	0.0047 [0.0037 0.0056]
+1 (more darkness)	13,112	93	0.0071 [0.0056 0.0085]

Share of hit-and-run around the change to WT and DST (5+5)

Season	Nr. of accidents	Nr. of hit-and-run accidents	Share of hit-and-run accidents [95% C.I.]
0 (before time change)	25,565	150	0.0059 [0.0049 0.0068]
-1 (more daylight)	15,065	68	0.0045 [0.0034 0.0056]
+1 (more darkness)	9,754	76	0.0078 [0.0060 0.0095]

2PM-4PM time window

Season	Nr. of accidents	Nr. of hit-and-run accidents	Share of hit-and-run accidents [95% C.I.]
Winter	177,332	792	0.0045 [0.0041 0.0047]
Summer	158,409	773	0.0049 [0.0045 0.0052]

6PM-8PM time window

Season	Nr. of accidents	Nr. of hit-and-run accidents	Share of hit-and-run accidents [95% C.I.]
Winter	205,353	1,626	0.0079 [0.0075 0.0083]
Summer	250,052	1,168	0.0047 [0.0044 0.0049]

10PM-12PM time window

Season	Nr. of accidents	Nr. of hit-and-run accidents	Share of hit-and-run accidents [95% C.I.]
Winter	90,145	748	0.0083
			[0.0077 0.0088]
Summer	65,101	481	0.0074
			[0.0067 0.0080]

Table 2d: Descriptive statistics for the share of accidents with 18-45 y.o. male drivers, before and after time change

Time window	Change in hours of darkness	Share of Accidents	[95% Conf. Int.]	
Shift to Winter Time				
7 days before shift to WT, 7AM-9AM	0	69.6%	68.1%	71.0%
7 days after shift to WT, 7AM-9AM	-1	70.7%	69.2%	72.3%
7 days before shift to WT, 4PM-6PM	0	68.3%	67.0%	69.6%
7 days after shift to WT, 4PM-6PM	+1	67.7%	66.1%	69.2%
5 days before shift to WT, 7AM-9AM	0	69.0%	67.4%	70.6%
5 days after shift to WT, 7AM-9AM	-1	70.7%	69.0%	72.5%
5 days before shift to WT, 4PM-6PM	0	66.9%	65.3%	68.4%
5 days after shift to WT, 4PM-6PM	+1	66.7%	65.0%	68.3%
Shift to Daylight Saving Time				
7 days before shift to DST, 6AM-8AM	0	76.9%	74.8%	79.1%
7 days after shift to DST, 6AM-8AM	+1	76.7%	74.8%	78.7%
7 days before shift to DST, 6PM-8PM	0	73.2%	72.1%	74.3%
7 days after shift to DST, 6PM-8PM	-1	74.5%	73.4%	75.6%
5 days before shift to DST, 6AM-8AM	0	75.4%	72.7%	78.0%
5 days after shift to DST, 6AM-8AM	+1	74.4%	72.0%	76.9%
5 days before shift to DST, 6PM-8PM	0	72.7%	71.4%	73.9%
5 days after shift to DST, 6PM-8PM	-1	74.3%	73.0%	75.6%

Note: The table summarizes descriptive statistics for the days before and after the time shift with weekend (7+7) and without weekend (5+5).

Table 3a: Short-term effect of daylight

Marginal effects of the determinants of hit-and-run in Italy, 1996-2016
(Logit models)

VARIABLES	(1)	(2)	(3)
		7+7 days	
	2h around time change	**Placebo forward (+2h)**	**Placebo backward (-2h)**
One hour more darkness	**0.11445*** **	0.02537	-.03845
	(0.04)	(0.04)	(0.04)
N	63,812	77,314	49,959

VARIABLES	(4)	(5)	(6)
		5+5 days	
	2h around time change	**Placebo forward (+2h)**	**Placebo backward (-2h)**
One hour more darkness	**0.15689*** **	0.04946	-.0678
	(0.05)	(0.04)	(.06)
N	49,568	61,914	34,693

Notes: Robust standard errors in parentheses. Regressions include the same control variables of column 1, Table A1.
Marginal effects and standard errors have been multiplied by 100 to make the results easier to read.

*** p<0.01, ** p<0.05, * p<0.1

Table 3b: Long-term effect of daylight

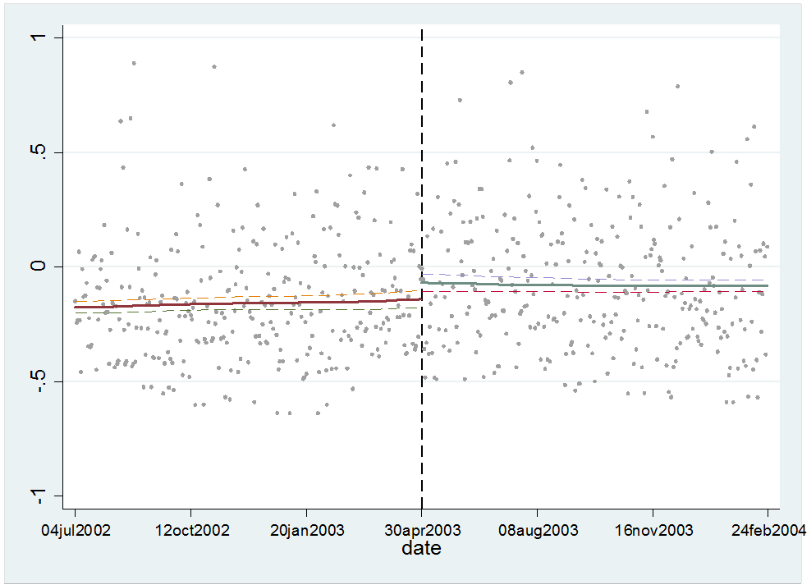
Marginal effects of the determinants of hit-and-run in Italy, 1996-2016
(Logit models)

VARIABLES	(1)	(2)	(3)	(4)
	Only winter and summer			
	2-4, 6-8, 10-12 PM	2PM - 4PM	6PM - 8PM	10PM - 12PM
Winter	0.01717	-0.01136	**0.21171*** **	-0.03548
	(0.03)	(0.02)	**(0.03)**	(0.05)
6PM-8PM	-0.00593			
	(0.02)			
10PM-12PM	0.38759***			
	(0.03)			
DID-Winter-6PM-8PM	**0.15655*** **			
	(0.03)			
DID-Winter-10PM-12PM	-0.04104			
	(0.04)			
N	946,392	335,741	455,405	155,246

Notes: Robust standard errors in parentheses. Regressions include the same control variables of column 1, Table A1. Marginal effects and standard errors have been multiplied by 100 to make the results easier to read.

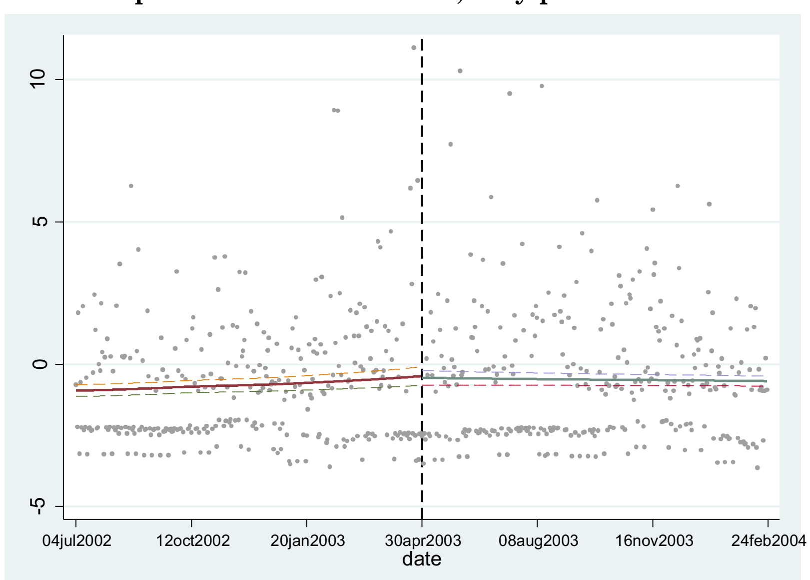
*** p<0.01, ** p<0.05, * p<0.1

Figure 1a: Local linear polynomial regressions of the impact of Law No. 72/2003, full sample



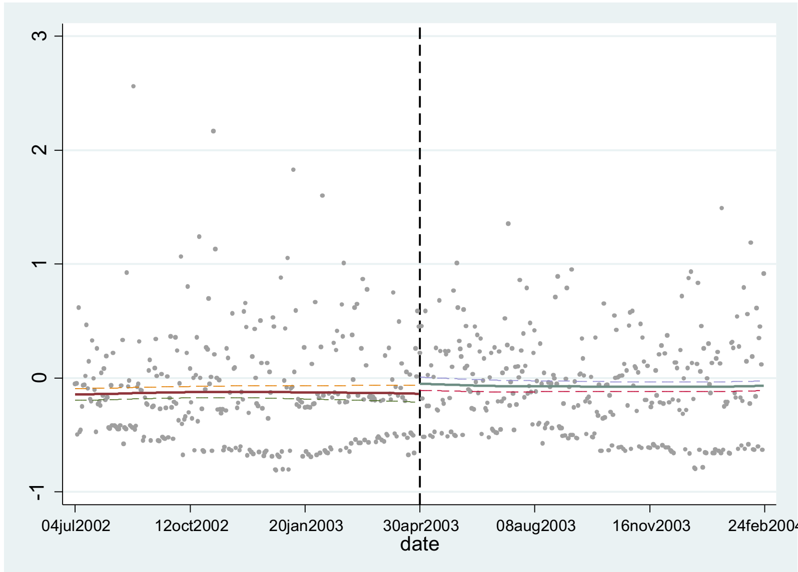
Note: Results come from local linear regressions of the residuals of the first stage, which in turn is an OLS regressions of the daily share of hit-and-run over a set of seasonal dummy variables (national holiday, day of the week and month of the year) and the daily real oil price in constant 2016 Euros. The symmetric window includes 300 days before and 300 days after the cutoff.

Figure 1b: Local linear polynomial regressions of the impact of Law No. 72/2003, only pedestrians



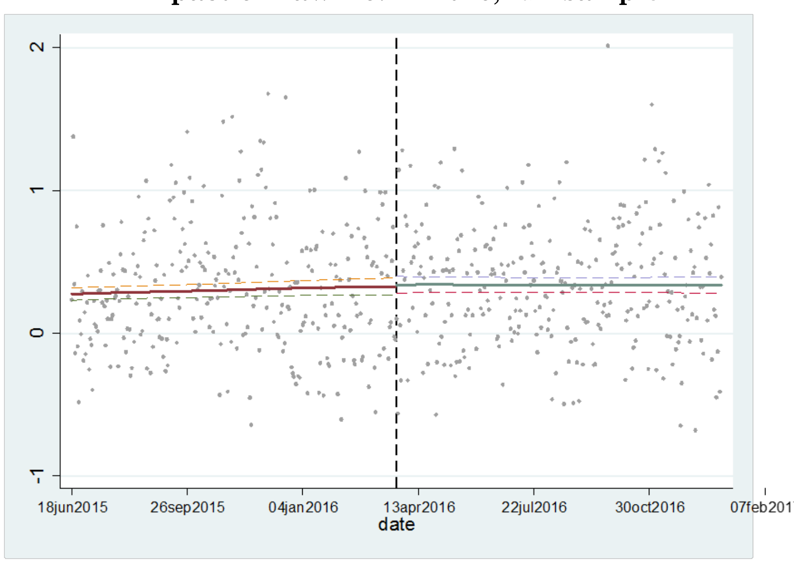
Note: Results come from local linear regressions of the residuals of the first stage, which in turn is an OLS regressions of the daily share of hit-and-run over a set of seasonal dummy variables (national holiday, day of the week and month of the year) and the daily real oil price in constant 2016 Euros. The symmetric window includes 300 days before and 300 days after the cutoff.

Figure 1c: Local linear polynomial regressions of the impact of Law No. 72/2003, only motorbikes



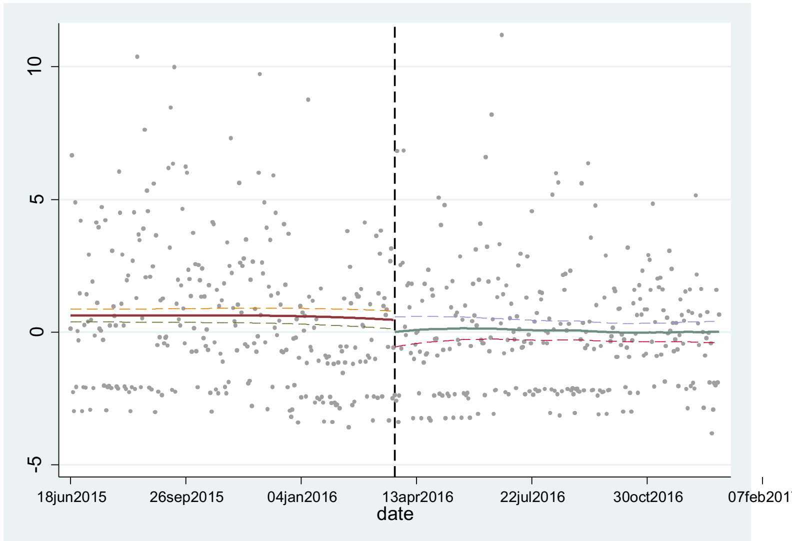
Note: Results come from local linear regressions of the residuals of the first stage, which in turn is an OLS regressions of the daily share of hit-and-run over a set of seasonal dummy variables (national holiday, day of the week and month of the year) and the daily real oil price in constant 2016 Euros. The symmetric window includes 300 days before and 300 days after the cutoff.

Figure 2a: Local linear polynomial regressions of the impact of Law No. 41/2016, full sample



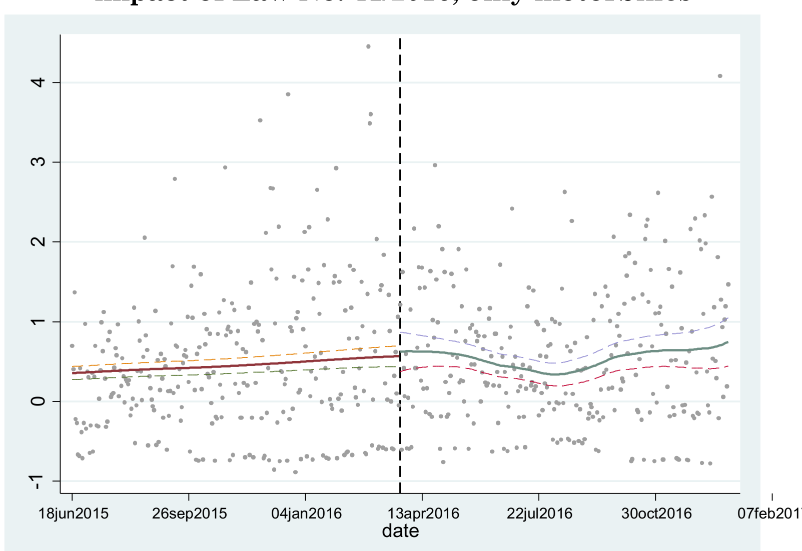
Note: Results come from local linear regressions of the residuals of the first stage, which in turn is an OLS regressions of the daily share of hit-and-run over a set of seasonal dummy variables (national holiday, day of the week and month of the year) and the daily real oil price in constant 2016 Euros. The symmetric window includes 282 days before and 282 days after the cutoff.

Figure 2b: Local linear polynomial regressions of the impact of Law No. 41/2016, only pedestrians



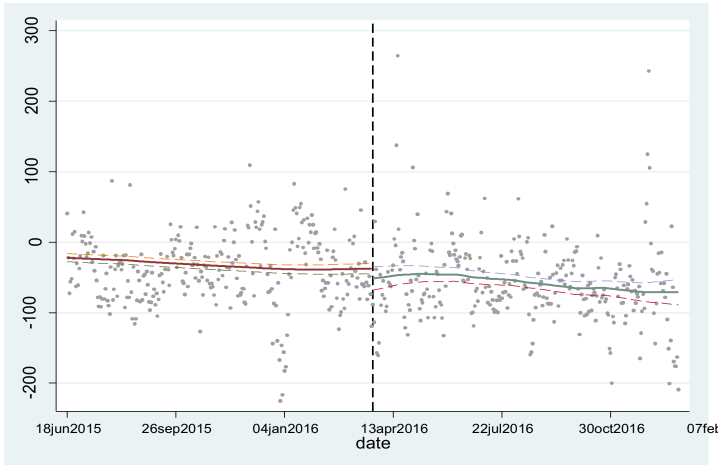
Note: Results come from local linear regressions of the residuals of the first stage, which in turn is an OLS regressions of the daily share of hit-and-run over a set of seasonal dummy variables (national holiday, day of the week and month of the year) and the daily real oil price in constant 2016 Euros. The symmetric window includes 282 days before and 282 days after the cutoff.

Figure 2c: Local linear polynomial regressions of the impact of Law No. 41/2016, only motorbikes



Note: Results come from local linear regressions of the residuals of the first stage, which in turn is an OLS regressions of the daily share of hit-and-run over a set of seasonal dummy variables (national holiday, day of the week and month of the year) and the daily real oil price in constant 2016 Euros. The symmetric window includes 282 days before and 282 days after the cutoff.

Figure 2d: Local linear polynomial regressions of the nr. of police patrols around Law No. 41/2016



Note: Results come from local linear regressions of the residuals of the first stage, which in turn is an OLS regressions of the nr. of police patrols over a set of seasonal dummy variables (national holiday, day of the week and month of the year), the daily real oil price in constant 2016 Euros. The symmetric window includes 282 days before and 282 days after the cutoff.

APPENDIX

Table A1: Marginal effects of the determinants of hit-and-run in Italy, 1996-2016 (Logit models)

VARIABLES	(1) All accidents	(2) Pedestrians	(3) Motorbikes
Holiday	0.11192***	0.73502***	0.12745*
	(0.02)	(0.16)	(0.04)
Tuesday	-0.01004	-0.0383	-0.00944
	(0.01)	(0.09)	(0.02)
Wednesday	0.00609	0.06166	-0.01232
	(0.01)	(0.09)	(0.00)
Thursday	0.0000558	0.0057	-0.00461
	(0.01)	(0.09)	(0.02)
Friday	-0.00596	-0.12042	0.00481
	(0.01)	(0.09)	(0.02)
Saturday	0.001027	0.04948	0.00855
	(0.01)	(0.09)	(0.02)
Sunday	0.06642***	0.52369***	0.02845
	(0.01)	(0.10)	(0.02)
February	-0.02392	0.10357	-0.02384
	(0.02)	(0.11)	(0.03)
March	-0.06204***	-0.06079	-0.09069***
	(0.02)	(0.11)	(0.03)
April	-0.07227***	-0.23807*	-0.11266***
	(0.02)	(0.12)	(0.03)
May	-0.06739***	-0.20096*	-0.10237***
	(0.02)	(0.12)	(0.03)
June	-0.06253***	-0.4179***	-0.08865***
	(0.02)	(0.12)	(0.03)
July	-0.10311***	-0.44973***	-0.14599***
	(0.02)	(0.13)	(0.03)
August	-0.11839***	-0.45234***	-0.18223***
	(0.02)	(0.13)	(0.03)
September	-0.0596***	-0.30719**	-0.10433***
	(0.02)	(0.12)	(0.03)
October	-0.02229***	-0.26653**	-0.00194
	(0.02)	(0.11)	(0.03)
November	-0.03214*	-0.34372***	-0.000465
	(0.02)	(0.11)	(0.03)
December	-0.02102	-0.27981***	0.01455
	(0.02)	(0.11)	(0.03)
Night	0.356***	1.81562***	0.40413***
	(0.01)	(0.06)	(0.01)
Good weather	0.1767***	0.77757***	0.11602***
	(0.01)	(0.07)	(0.02)
Ruined road	0.22201***	2.82957***	-0.25666***

	(0.04)	(0.21)	(0.08)
Two ways	-0.15149***	-0.31574***	-0.16448***
	(0.01)	(0.06)	(0.01)
Two lanes	0.15467***	0.73541***	0.1805***
	(0.01)	(0.08)	(0.02)
More than two lanes	0.20042***	0.52172***	0.22169***
	(0.02)	(0.13)	(0.03)
Urban	0.85522***	0.64637**	1.45454***
	(0.03)	(0.30)	(0.16)
Regional	0.59188***	0.75403**	1.35302***
	(0.03)	(0.32)	(0.16)
Total dead	-0.6043***	-3.65192***	-0.65463***
	(0.03)	(0.20)	(0.06)
Total injured	-0.45551***	-3.07274***	-0.45311***
	(0.01)	(0.12)	(0.02)
Nr other vehicles	-0.17416***	0	-0.28561**
	(0.04)	(omitted)	(0.11)
Motorbike	0.08406***		
	(0.01)		
Pedestrian	0.88936***		
	(0.01)		
Ln realpcincome	-1.41149***	-6.83096*	-1.87646*
	(0.61)	(3.97)	(1.06)
Unemployment	-0.00503**	-0.01402	-0.01309***
	(0.00)	(0.02)	(0.00)
P_gas_real	-0.09487	-0.14262	-0.04011
	(0.08)	(0.50)	(0.13)
N	4,549,443	383,403	1,822,121

Notes: Robust standard errors in parentheses. Regressions include regional and year fixed effects.
The omitted DVs are Monday, January, Day, Bad weather, Good road, One way, One lane, Highway.
*** p<0.01, ** p<0.05, *p<0.1

Table A2: Determinants of hit-and-run in Italy, with a variable equal to 1(-1) after a shift to WT(DST), 1996-2016 (marginal effects)

Logistic regression		
Number of obs	=	63,812
Log pseudolikelihood	=	-1978.2307
Pseudo R2	=	0.1121

hitandrun	Coef.	Robust Std. Err.
holiday	0.01079	0.17325
tuesday	-0.12764	0.10132
wednesday	-0.01546	0.09605
thursday	-0.11103	0.10082
friday	-0.22248	0.10606
saturday	-0.08721	0.10986
sunday	-0.17103	0.13163
one_h_more_darkness	**0.15016**	**0.04969**
one_h_more_sleep	**-0.0783**	**0.05101**
goodweather	0.14921	0.07918
ruinedroad	0.34984	0.28841
twoways	-0.17662	0.07628
twolanes	0.11688	0.09962
morethantwolanes	0.17955	0.14969
urban	1.68847	0.56432
regional	1.31454	0.57136
total_dead	-0.35936	0.20855
total_injured_comparable	-0.58452	0.1055
nr_other_vehicles	-0.53814	0.47715
motorbike	-0.04089	0.0619
peaton	0.75966	0.07358
ln_realpcincome	-0.38226	4.79373
unemployment	-0.00536	0.02078
p_gas_real	-0.13202	0.85144

Notes: Regressions include regional and year fixed effects and make use of robust standard errors.
The omitted DVs are Monday, January, Day, Bad weather, Good road, One way, One lane, Highway.
*** p<0.01, ** p<0.05, *p<0.1

Figure A1: Share of hit-and-run in accidents with injured and/or dead people by region in Italy, 1996-2016

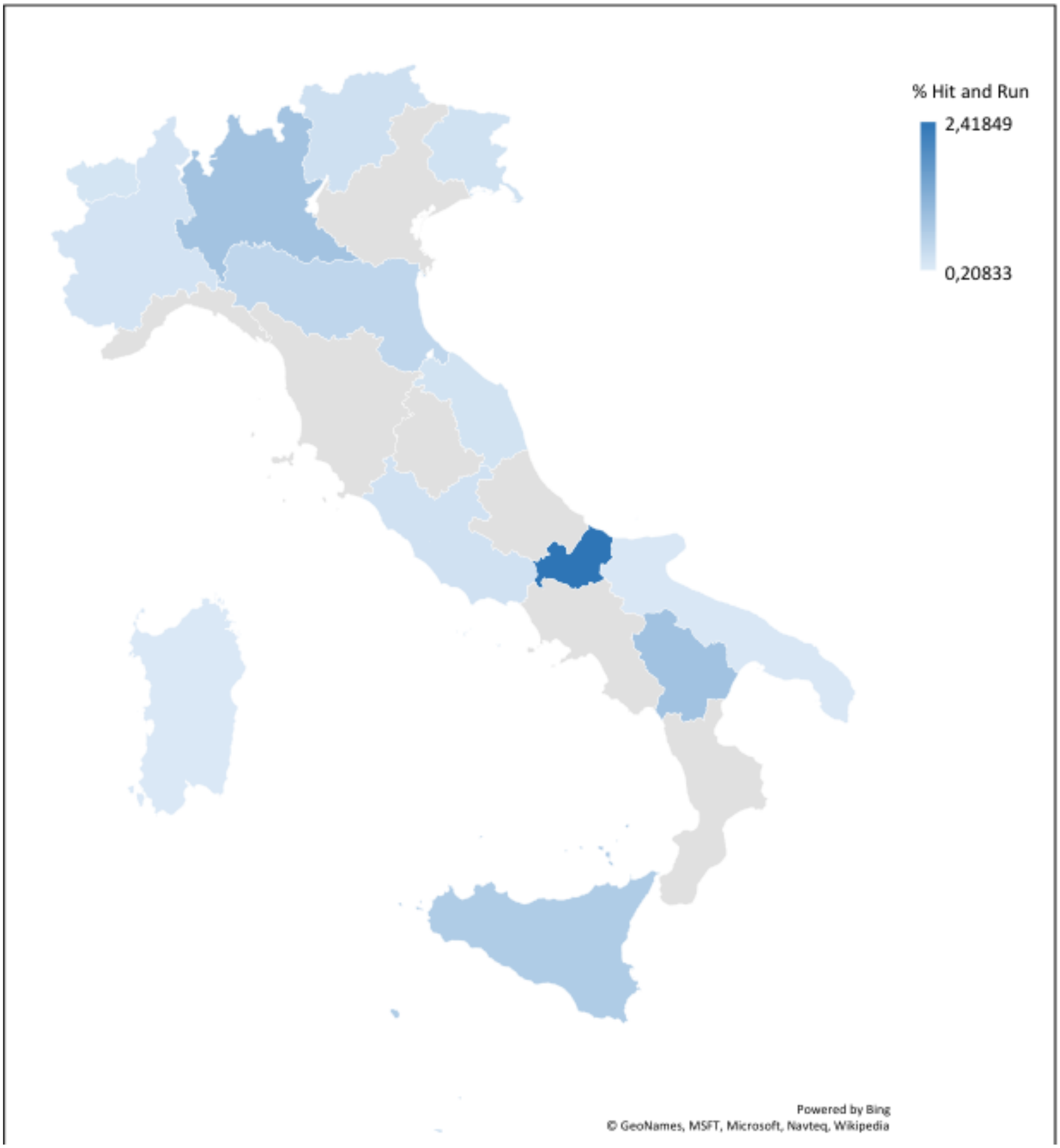
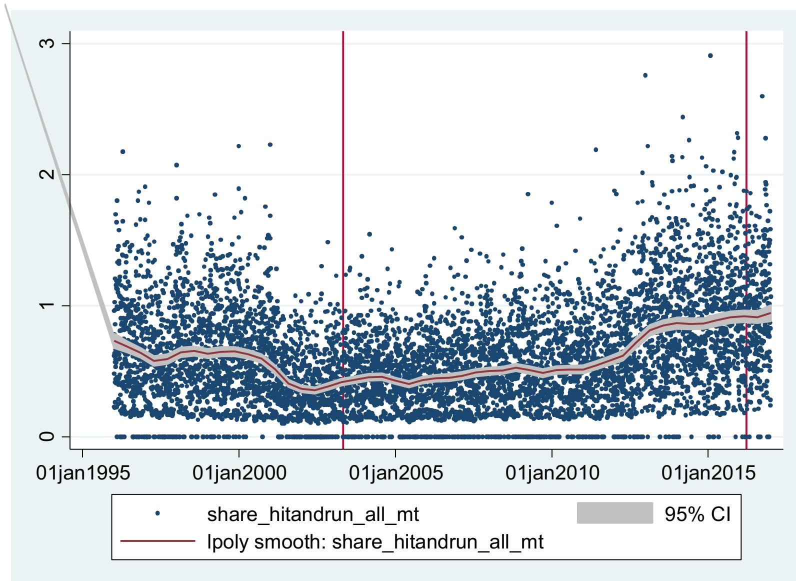


Figure A2: Daily share of hit-and-run in Italy, 1996-2016



Note: The figure reports the scatterplot of the daily share of hit-and-run from 1996 to 2016 and the local linear time trend with the 95% confidence intervals. The two red vertical lines indicate the legal reforms implemented in 2003 and 2016.

Figure A3a: Local linear polynomial regressions of the impact of Law No. 72/2003, 6 AM – 9 PM

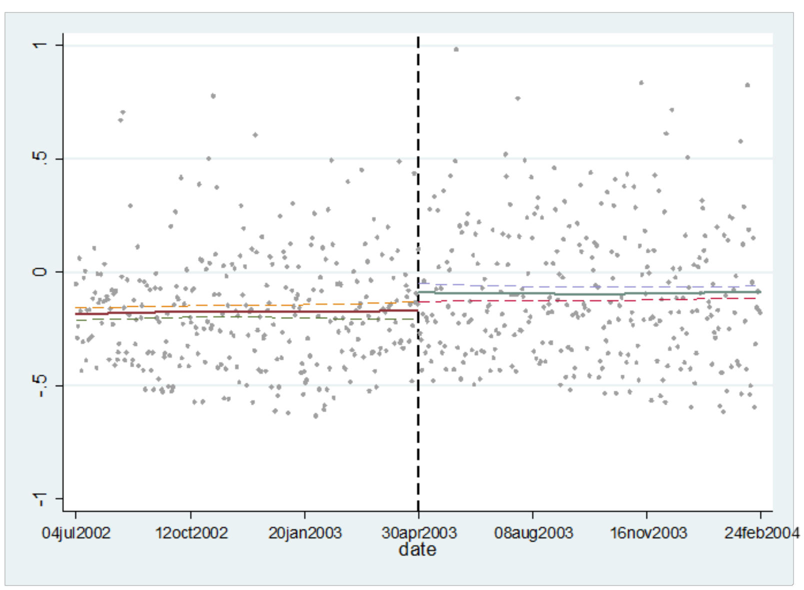
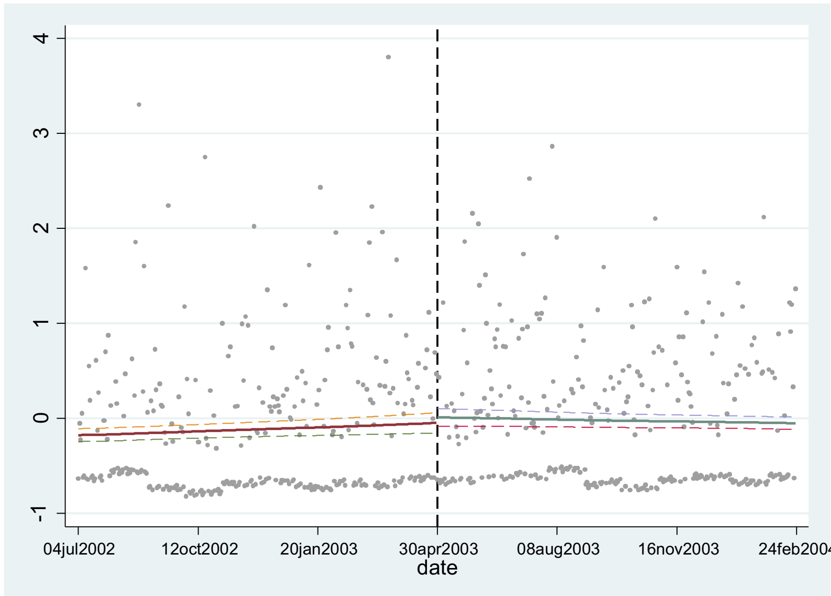
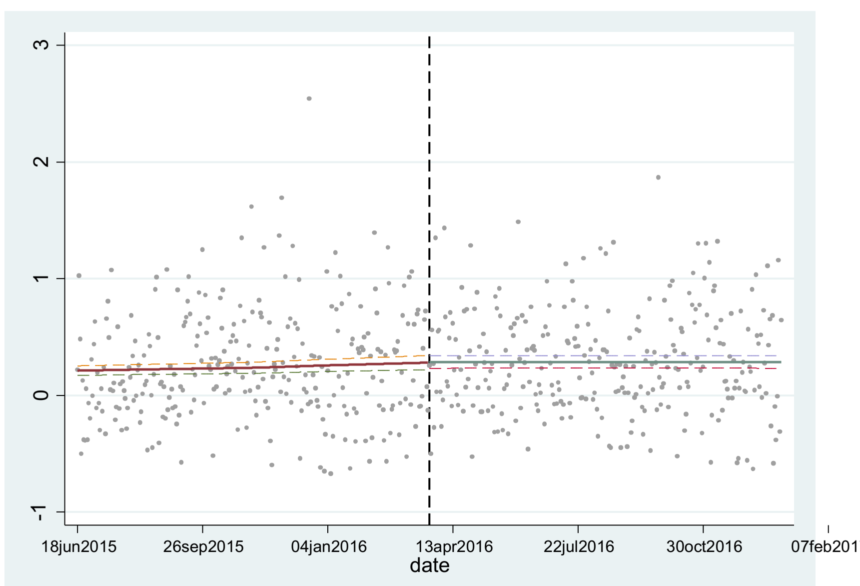


Figure A3b: Local linear polynomial regressions of the impact of Law No. 72/2003, 9 PM – 6 AM



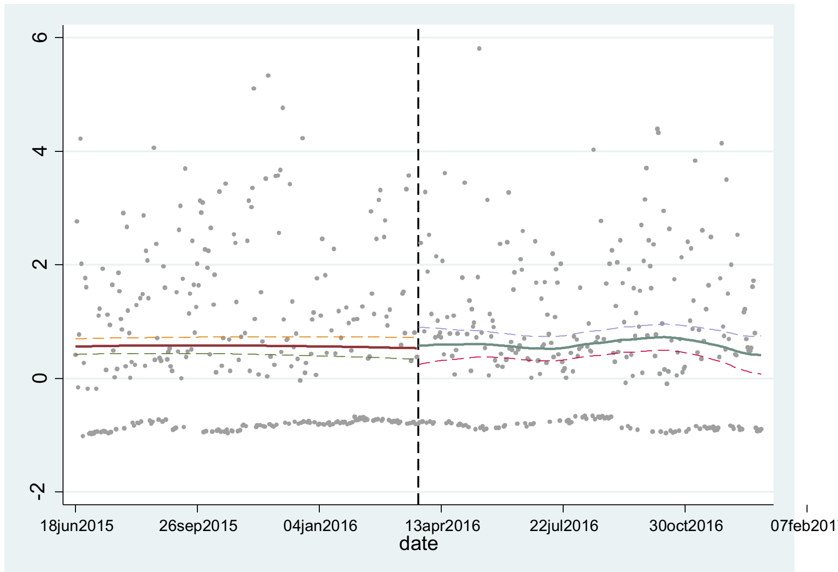
Note: Results come from local linear regressions of the residuals of the first stage, which in turn is an OLS regressions of the daily share of hit-and-run over a set of seasonal dummy variables (national holiday, day of the week and month of the year) and the daily real oil price in constant 2016 Euros.

FigureA3c: Local linear polynomial regressions of the impact of Law No. 41/2016, 6 AM – 9 PM



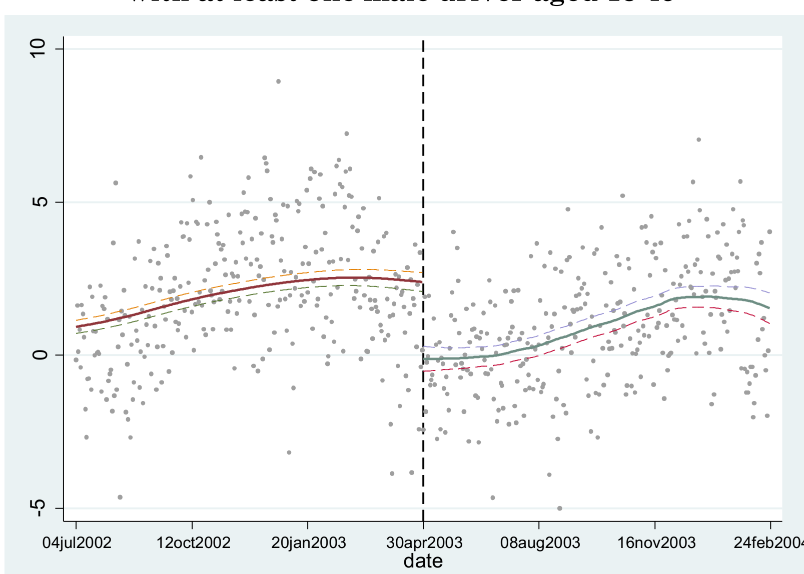
Note: Results come from local linear regressions of the residuals of the first stage, which in turn is an OLS regressions of the daily share of hit-and-run over a set of seasonal dummy variables (national holiday, day of the week and month of the year) and the daily real oil price in constant 2016 Euros.

Figure A3d: Local linear polynomial regressions of the impact of Law No. 41/2016, 9 PM – 6 AM



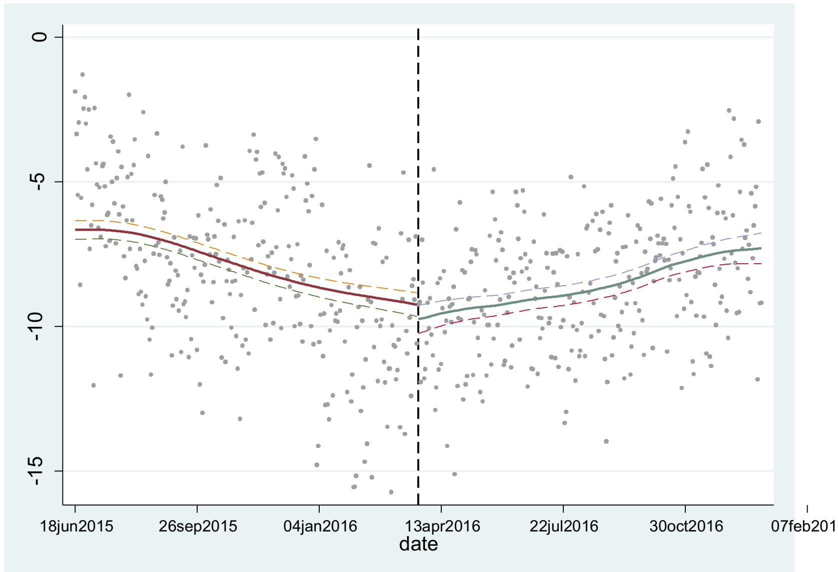
Note: Results come from local linear regressions of the residuals of the first stage, which in turn is an OLS regressions of the daily share of hit-and-run over a set of seasonal dummy variables (national holiday, day of the week and month of the year) and the daily real oil price in constant 2016 Euros.

Figure A4a: Local linear polynomial regressions of the impact of Law No. 72/2003 on the daily share of accidents with at least one male driver aged 18-45



Note: Results come from local linear regressions of the residuals of the first stage, which in turn is an OLS regressions of the daily share of accidents with at least one male driver aged 18-45 over a set of seasonal dummy variables (national holiday, day of the week and month of the year) and the daily real oil price in constant 2016 Euros.

Figure A4b: Local linear polynomial regressions of the impact of Law No. 41/2016 on the daily share of accidents with at least one male driver aged 18-45



Note: Results come from local linear regressions of the residuals of the first stage, which in turn is an OLS regressions of the daily share of accidents with at least one male driver aged 18-45 over a set of seasonal dummy variables (national holiday, day of the week and month of the year) and the daily real oil price in constant 2016 Euros.