Depowering Risk: Vehicle Power Restriction and Teen Driver Accidents in Italy*

Silvia Balia[†] Rinaldo Brau[†] Marco G. Nieddu[†‡]

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Abstract

This paper investigates the road safety impact of a power restriction on novice drivers. The restriction, introduced in Italy in 2011, prevents drivers from using high-performance vehicles during the first license year. To estimate the effect on teen traffic accidents we leverage the between-cohorts difference in the exposure to the reform. We find that, when prevented from using high-powered cars, teens are 13% less likely to cause an accident and 28% less likely to cause a fatal accident, mainly because of fewer speed violations. This effect persists even after the one-year restriction expires. Our findings highlight the importance of targeted policies – directed at those generating the largest harm – limiting exposure to specific high-risk settings. These policies stand out as an effective, yet feasible alternative to deterrence-based strategies and screening mechanisms, which are often difficult to enforce and sustain.

JEL Classification: D04; I12; I18; K32

Keywords: youth road accidents; driving restriction; graduated licensing; risky behaviors; risk exposure

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1 Introduction

Motor vehicle traffic accidents represent a leading cause of death and disability globally. Even in developed countries, vehicle accidents are responsible for about one out of three violent deaths WHO (2018). The figures regarding young drivers are even more alarming. Road crashes represent the biggest killer of 15-24-year-olds, and this age group exhibits the highest road mortality rate in many industrialized countries ITF (2018). Consequently, lowering the number of road traffic injuries and fatalities, especially among young drivers, has been placed at the top of the policy agenda in all countries.

Improving young drivers' records is a challenging policy goal. Teen drivers are the most inexperienced road users and show the highest propensity to engage in risky behaviors, such as drink-driving or excessive speeding (Anderson et al., 2013). The efficacy of deterrence policies in the form of monitoring and sanctions for hazardous driving depends on sustained enforcement (DeAngelo and Hansen, 2014) and is often short-lived (Abouk and Adams, 2013). Furthermore, regulations must confront the fact that individual risk attitude is non-uniformly distributed in the population nor directly observable. Because of this, untargeted policies are likely to generate welfare gains that do not offset the losses from affecting an inefficiently large population share.

Among the interventions aimed at reducing teen driver crashes, the so-called Graduated Driver Licensing (GDL) programs have gained increasing popularity over time (see Williams et al., 2016).[1] Instead of trying to deter risky driving behaviors, GDLs operate by limiting novice drivers' exposure to specific circumstances where these behaviors are more likely to occur. The restrictions, which include, for instance, late-night driving or the carrying of peer passengers, are progressively lifted with license seniority and thus allow young drivers to gain experience while being constrained to a low-risk setting. GDLs have been found to reduce road accidents and fatalities. Nonetheless, several important research questions remain unanswered. First, the evidence on the channel through which they work is scarce and mixed: do they effectively and permanently improve teens' driving

[1] GDLs are commonly implemented in the US, Canada, New Zealand and Australia, while their adoption in Europe is partial and occasional (Helman et al., 2017). However, the European Commission has listed them among the possible key actions in a roadmap on safe road use (EC2018, 2018).

behavior or discourage young individuals from getting a license? Second, the optimal design of these restrictions has received little attention. The literature on corrective policies highlights that these should be directed toward the individuals generating the largest externalities and internalities to balance welfare losses and gains (Allcott et al., 2019). This is also the underlying rationale for policies such as GDLs: not only teen drivers represent the highest-risk group, but the harms they can generate (to others and themselves) also depend on specific conditions or circumstances. How to identify these factors, and thus the restrictions to be imposed on teens' driving, becomes crucial to predict the success of GDL policies.

In this paper, we provide novel evidence in this direction by studying the road safety impact of an Italian reform that restricts new drivers from using high-power vehicles. The law – introduced in 2011 – forbids drivers from using vehicles whose engine power exceeds 70 kilowatts (about 94 horsepower) or whose power-to-weight ratio exceeds 55 kilowatts (about 74 horsepower) per ton during their first license year. The restriction aims to prevent teens' opportunities for risky driving and speeding in particular, the latter being the main factor for nearly one-fourth of all teen accidents. To assess the impact of the power restriction on road safety, we combine unique and rich administrative data on traffic accidents (which allows us to identify the at-fault driver in each crash) for the period 2006-2016 with the census of Italian driving licenses. The resulting pseudo-panel enables us to estimate the causal effect of being subjected to the power limit on the likelihood of causing a severe traffic accident or a fatality both during and after the restriction period. Specifically, our difference-in-differences research design compares the evolution of accident rates across different age groups of drivers, leveraging the between-cohorts differences in the exposure to the reform.

We find that exposure to the vehicle power limit significantly improves teen road safety. *Per-capita* teen accident rates fall by 18% (from 4.4 to 3.6 accidents per 1,000 people) and fatal accidents by about 28%. In principle, such a drop could also be explained by a lower inflow into the pool of road users, as even the number of licenses issued post-reform decreases by 19%. However, we find that the power limit reduces *per-licensee* teen accident rates by 13% (from 8.3 to 7.2 accidents per 1,000 drivers). This reduction translates into 520 fewer severe crashes, 6,200 fewer injuries and about 95 fewer deaths in the five years after its introduction (in the five years before the reform the number of injuries and deaths caused by teen drivers was 45,000 and 670, respectively). Consistent with the power limit operating by discouraging teens from engaging in risky driving behaviors, we find that nearly half (44%) of this effect is explained by fewer accidents due to excessive speed violations, although these represent only about 25% of all teen accidents.

Importantly, our findings are solely driven by fewer accidents caused by vehicles exceeding the maximum consented power. This result is key to our identification, as it confirms that our strategy detects the causal effect of the power restriction rather than that of other (confounding) traffic safety policies. The power limit and the contemporary introduction of a more demanding written exam could have discouraged potentially *risky* drivers from applying for a license, leading to fewer teen accidents through a composition effect. However, we show that this is unlikely the case as the effect of the reform on licensing is homogenous across cells with different *ex-ante* teen accident rates. We also show that the impact of the reform on license probability is stronger among those with the lowest propensity to drive (that is, those who, in the absence of the reform, would have earned the license but driven rarely) and that the decrease in new licensees does not differ by gender, which is a well-acknowledged determinant of risky driving behavior.

We also find that drivers who underwent the one-year restriction exhibit lower accident rates even after the restriction has been lifted. We reconcile this finding with the fact that the power limit has affected car choices. As long as cars are not replaced in the short-term, novice drivers are likely locked into a low-risk setting for a relatively long time horizon. We provide direct evidence in this direction by studying the consequences of the reform on car sales. Using a regression-discontinuity framework, we show that the market of vehicles barely complying with the power limit increases post-reform at the expense of higher-powered models.

This study mainly relates to the literature on the road safety effect of graduated licensing programs. Constraining novice drivers in a low-risk setting is one of the features of GDL schemes that can make them a powerful policy tool to reduce teen accidents (Dee and Evans, 2001). However, most studies point to an "incapacitation channel" – GDLs have a negative impact on teen licensing, and this lead to fewer crashes – as the main mechanism at work (Dee et al., 2005; Karaca-Mandic and Ridgeway, 2010). Others highlight the mixed efficacy of the set of norms and restrictions encompassed within the term GDL. For instance, Gilpin (2019) analyzes the various teen driver licensure provisions in the US and finds that the minimum age to apply for the intermediate licensing stage has the largest impact on teen vehicular fatalities, but only through a reduction in licensing rates. When looking at the impact of specific restrictions on fatalities per licensee, he does not find any significant effect of banning nighttime driving or carrying teenage passengers, and only partial (limited to females) impact of cellphone or texting bans.

Such mixed evidence further confirms that the choice of the constraints to be imposed on teen drivers is a crucial aspect of the design of effective GDL schemes. The rationale for these interventions is that the policy target can hardly be the teens' actual risky behavior but rather the specific circumstances or settings that foster such behavior. In other words, their success depends on how these circumstances enter teens' accident probability. The study by Moore and Morris (2021), which analyzes an Australian program preventing teens from driving overnight and carrying teenage passengers, is one of the few works finding a direct effect on crashes per licensee. The authors highlight that the targeted setting (driving late at night with multiple peers) is a major factor in vehicle fatalities, as it explains approximately a fifth of all teen crashes. Our findings can be interpreted along the same line. The Italian vehicle power limit effectively discourages reckless driving, and speeding in particular, by forcibly decoupling teen drivers from high-performance cars. We show that, when unconstrained, teens paired with above-limit vehicles are up to 20% more likely of causing an accident, mostly because this increases the chances of speeding. The same is not true for senior drivers. In this perspective, the Italian reform is efficient as it is directed towards the group generating the largest externalities and internalities. Less targeted policies limiting the use of such vehicles for a larger share of road users (such as power taxes) will likely achieve only small additional gains in terms of road safety at the cost of large welfare losses.

2 Context and Institutional Setting

In many countries, road safety policies use monetary and regulatory incentives to promote safe driving, which ultimately depends on the individual's effort to limit risk exposure and adopt careful driving behaviors. Reckless driving, and speeding in particular, require government regulation as they generate costs not accounted for in the drivers' utility maximization problem. Specifically, hazardous driving induces externalities in the form of injuries and deaths of passengers, other cars' occupants, pedestrians, but also property damages. It also generates additional costs in the form of internalities when drivers do not fully balance unexpected private costs and private benefits. Lack of self-control, myopic behavior, limited information, or cognitive limitation are typical factors that might lead individuals to underestimate their own future costs and costs to society (see Griffith, 2022).

Echoing Kenkel (1993) taxonomy of policies for reducing drunk driving, policies to limit reckless driving can be classified into two broader categories: *direct* interventions targeting specific behaviors, and *indirect* interventions that try to reduce risky driving indirectly. The former category includes, among others, the introduction of tighter speed limits, harsher punishments for using a phone while driving or not using seatbelts (Cohen and Einav, 2003; Carpenter and Stehr, 2008), zero-tolerance laws on drunk driving (Carpenter, 2004; Hansen, 2015; Francesconi and James, 2021), or even more complete reforms of traffic safety regulation (French et al., 2009; Aney and Ho, 2019). These interventions rely on high monitoring and sustained enforcement (Abouk and Adams, 2013) and thus induce large economic costs. Moreover, they often act as ex-post sanctions and may fail to prevent these dangerous behaviors.[2] Examples of indirect interventions include increases in fuel taxation or car ownership, which makes car driving more costly and thus, by reducing car use, also mechanically lower accident rates (see Morrisey and Grabowski, 2011).

In principle, a viable alternative would be to act on the selection channel through the screening process of both prospect and existing drivers. However, asymmetric information does not allow screening candidate drivers based on their risk type (but only their driving skills or knowledge). Penalty-point systems, although generally effective (De Paola et al., 2013), can identify dangerous road users only ex-post.[3] Because risk attitude is not directly observable at the individual level,

[2] An example of (unsuccessful) ex-post sanctions for the case of Italy is the introduction, in March 2016, of a specific offense for vehicular homicide. Bruzzone et al. (2019) and Basili and Belloc (2020) suggest its effects on road accidents are limited.

[3] Drivers' heterogeneity based on their ability or risk attitude is well known in the literature. For example, Bourgeon and Picard (2007) propose a model where drivers' type ("reckless" and "normal") is identified based on their effort

regulatory policies often identify specific groups with the greatest risk propensity and prevent them from driving, rather than trying to deter (or punish) individual risky behaviors. This is the case with Minimum Driving Age (MDA) and GDL policies. While the former fully ban car use for underage teens, the latter still permit them to drive while reducing their exposure to high-risk situations when inexperienced, for example by limiting late-night driving or the carrying of peer passengers. Most GDL programs follow a three-stage scheme: a learner stage, when teens are allowed to drive only supervised until they pass the driving test; an intermediate stage, where unsupervised driving is possible only under specific circumstances; and a full-privilege stage when all restrictions are lifted. Such structure thus allows young drivers to gradually gain experience in a reduced-risk setting.

The Italian traffic law shares many features with analogous models implemented in other countries and, since 2003, is based on a penalty-based points system. Until 2010, the driver licensing system followed a two-stage structure: a supervised learning phase and a full licensing phase. To access the former, learner drivers must apply for a temporary driving-license card ("*foglio rosa*"), whose full eligibility requirements include reaching the minimum driving age (18) and passing a written test. Under the driving card regime – which can last up to 6 months – learners can take driving lessons and drive under the supervision of an experienced accompanying person but only in the daytime and on urban roads. Passing the driving exams grants access to the full licensing phase, where these limits are lifted.[4]

In July 2010, Italy announced a new package of regulations to incentivize safe driving and discourage risky behaviors by beginner drivers (Law No. 160/2010). Starting from February 9th, 2011, new licensees are subject to a vehicle power limit for the first 12 months: they are no longer allowed to drive vehicles with an engine power exceeding 70 kilowatts (kW), which is about 94 horsepower, or a power-to-weight ratio above 55 kW (74 horsepower) per ton. The introduction of the power limit marked a significant change in Italian traffic regulations, as its license system *de facto* moved closer to the typical three-stage structure of the GDL schemes described above.[5]

to drive safely but is unobservable to the regulator.

[4]Still, full licensees are subject to a different regulation for the first three license years: they face double point

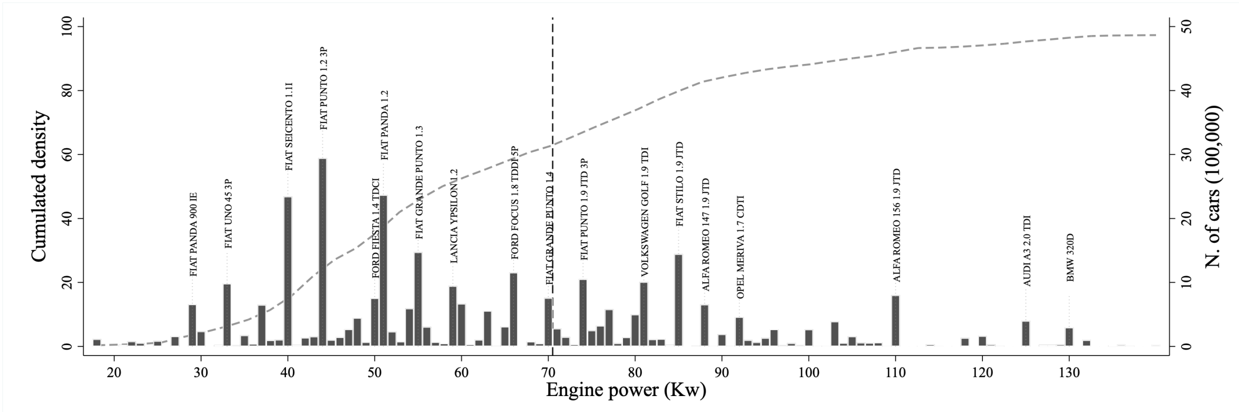


Figure 1. Distribution of passenger cars by engine size in 2009

Figure 1 depicts the stock of passenger cars in Italy before the reform distinguishing between vehicles complying and not complying with the 70kw limit. The number of licensed cars by car manufacturer and model is computed from the ACI Statistical Yearbook for the year 2009. As the ACI data do not include the information on the engine power, these are retrieved from the *Quattroruote* database of car models. The vertical bars indicate the total number of licensed vehicles (in hundreds of thousands of the corresponding engine power. The dashed line indicates the cumulated density (reported on the right y-axis). The green area shows all models complying with one of the two engine power thresholds ($kW \geq 70$), while the red one indicates models exceeding the maximum consented power according to the same criterion. In 2009, nearly two-thirds of all passenger cars complied with such a limit. On the bar labels, the figure also reports the most common car models and configurations. Above-limit cars are mainly those with an engine size larger than 1500 cubic centimeters (cc). The relationship between engine size and engine power is also displayed in the Online Appendix Figure OA1: the vast majority (from 70 to 100%) of cars whose engine size is larger than 1,500cc exceed the 70kw power restriction. This share is much lower for vehicles with an engine size below 1,500cc, and close to zero for engines below 1,300cc.

The power restriction was part of a broader range of measures targeting young drivers. Starting from January 2011, the written exam became more demanding. The number of test questions

deductions and stricter speed limits on extra-urban roads and motorways (90 and 100 km/h, respectively).

[5]Similar engine-power restrictions have been implemented in some Australian states (New South Wales and Victoria). In Europe, a power limit is also in place in Croatia (Genschow et al., 2014).

increased from 30 to 40 – while the number of mistakes allowed (4) remained unchanged – and the number of topics covered by each exam grew from 10 to 25.[6] Moreover, since July 2010, drivers with less than three years of license seniority face a zero-tolerance policy on BAC. This tighter limit substitutes the standard limit of 0.5 grams per liter of blood (already reduced following two reforms in 2007 and 2008). These two policies are contemporaneous with the introduction of the power restriction and could potentially act as confounders. In Section 4, where we discuss our identification strategy, we provide a detailed description of how we address this issue.

3 Data and Descriptive Evidence on Teen Road Accidents

3.1 Data

The empirical analysis in this paper is based on a unique database built gathering together different types of administrative data. Our primary data sources are statistics on road accidents leading to injuries or fatalities, released by the Italian National Statistical Institute (Istat), and the Italian driving license census, released by the Ministry of Transport and Infrastructure (MIT).

The Istat data on road accidents (*Rilevazione degli incidenti stradali con lesioni a persone*) cover all accidents occurring in Italy with at least one driver, a passenger, or a pedestrian injured or dead. The microdata, which have been released annually since 2000, are based on the information collected every month by various police forces, local governments, and organizations.[7] The data provide detailed information about accidents (weekday, hour, location, road type, road and weather condition, type of crash) and the vehicles involved. In addition, they report the gender and the age of vehicle occupants, together with the driver's license type. Notably, the data include information on the accident type (head-on, rear-end or side collisions, road departure, rollover) and each driver's behavior (such as excessive speed, stop sign violation or red traffic light running). Hence, for approximately 90% of the crash episodes, we can identify a single at-fault driver as the person

[6] Before the reform, applicants were tested on their knowledge of (at most) ten topics, with three true/false statements for each of them. After the reform, the questions cover all 25 topics in the syllabus: out of the 40 true/false questions, 30 are devoted to the subjects identified by the Ministry as the most important ones – two questions on each subject – while ten questions cover the remaining ten topics.

[7] Specifically, these are *Automobile Club d'Italia*, Ministry of Interior (national police), Ministry of Defence (*Carabinieri*), provincial and local police, and statistical offices or local monitoring centers.

culpable of a traffic violation, as filed by the police.[8]

Data on the yearly number of licensees come from the census of all Italian driving licenses released by the Ministry of Transport and Infrastructure (MIT). The original data contains all of the 38.7 million licenses active as of May 2017, reporting information on the licensee's demographics – gender, year of birth, municipality of residence – as well as crucial information such as the license type and the exact issue date. We exploit this information to reconstruct the number of licensees in each year and geographical area by gender and birth cohort. Although the dataset includes only licenses in use up to May 2017, we believe that selection is not a major concern for our analysis, as we focus on relatively young individuals and limit our analysis to the period 2006-2016. Moreover, we validate our procedure by comparing our (reconstructed) time series of licensees with the yearly number of licenses resulting from the official MIT statistics on written and driving tests. Since the driving test is the final stage of the driving exam, the number of successful driving tests corresponds to the number of new licenses issued. Unfortunately, we cannot directly use these data for our analysis as they do not include test takers' characteristics. The Online Appendix Figure OA2 reveals that the two time series overlap almost perfectly.[9]

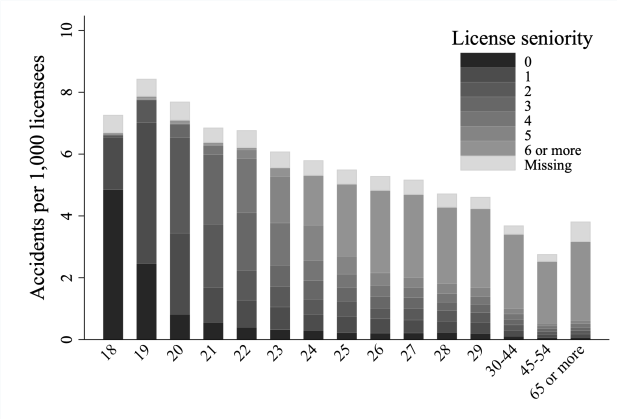
Population data come from the Istat *Intercensal estimates on the resident population.* This dataset is released annually and includes the total population of each municipality, distinguished by age and gender, as of January 1$^{\text{st}}$ of each year. Lastly, in Section 6, we exploit additional data sources. The data on yearly car sales by manufacturer and model specification come from the *Automobile Club d'Italia* (ACI) Statistical Yearbook.[10] Because the car model specifications do not include information on engine power, we match these data with the publicly-available *Quattruote* database, listing all car models available in the Italian market since 1971.[11]

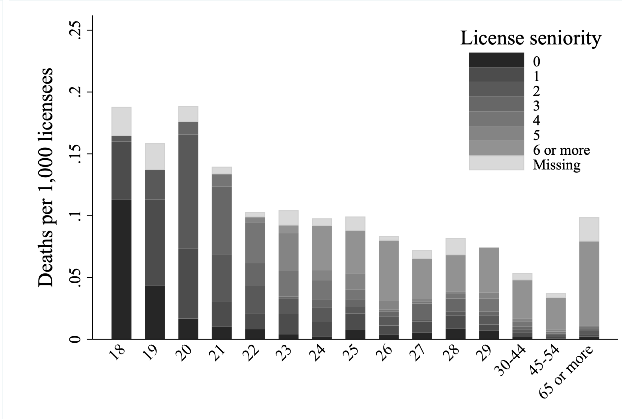
[8]When two at-fault drivers are present (6% of the cases), we focus on the first of the reported vehicle.

[9]A discrepancy between the yearly number of licenses issued as estimated from the license census and the license test data emerges for the years 2006 and 2007 only. This difference is mainly due to the licenses issued in the last quarter of 2006 and in the first quarter of 2007 that, once expired (Italian driving licenses expire in ten years) had not yet been renewed by the publication date of the license census (May 2017). As the census covers valid driving licenses only, these licenses are missing. However, the share of missing licenses is small (less than 9%), and not specific to the treated cohorts, thus being unlikely to represent a significant concern for our analysis.

[10]Source: http://www.aci.it/laci/studi-e-ricerche/dati-e-statistiche/annuario-statistico.html.

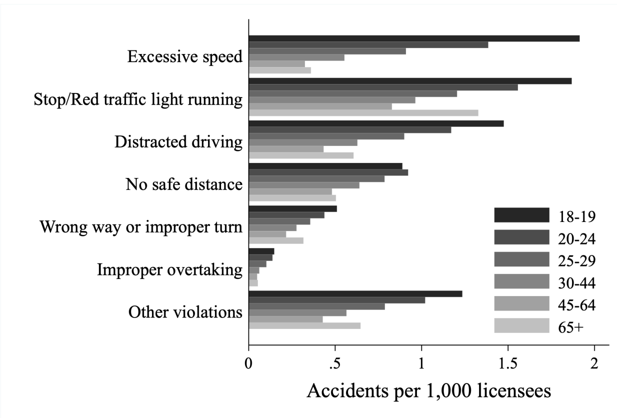
[11]Source: https://www.quattroruote.it/archivio/listino/.

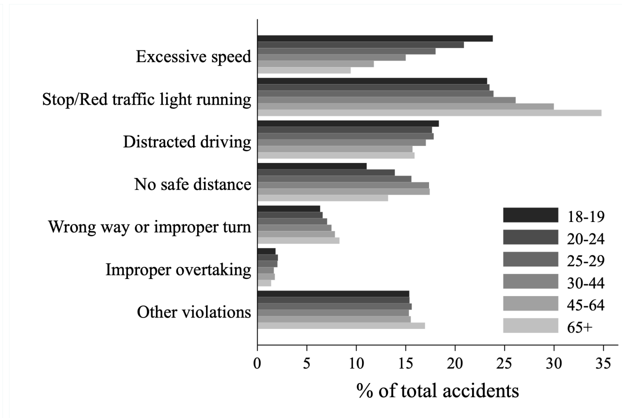




a. Accident rate

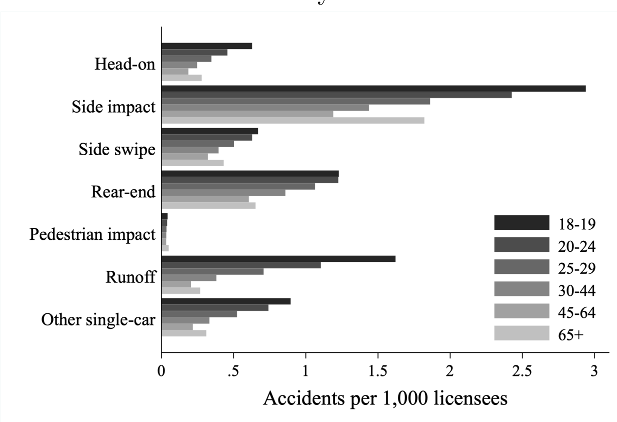
b. Death rate

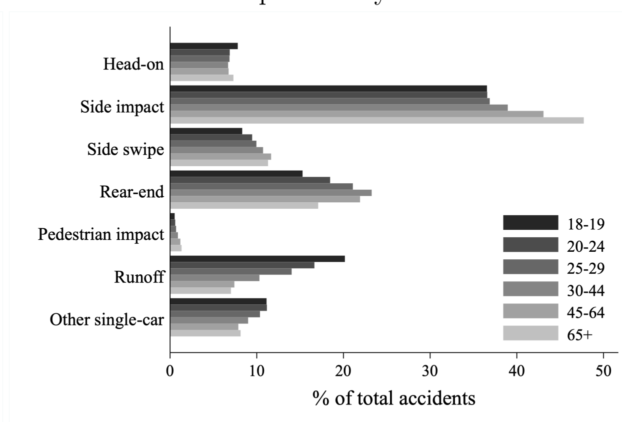




c. Accident rate by traffic violation

d. Accident composition by traffic violation





e. Accident rate by collision type

f. Accident composition by collision type

Figure 2. Age profile of traffic accidents in Italy in 2010

3.2 Descriptive Evidence on Road Accidents

Car crashes among teen drivers represent a major policy concern in Italy. Over the decade before the reform, teen drivers were responsible for nearly 60,000 severe road accidents, resulting in more than 100,000 injuries and 1,700 fatalities.

This steep age profile is highlighted in Figure 2, which reports the number of accidents (Panel A) and deaths (Panel B) per 1,000 drivers separately by driver age in the year 2010. Eighteen to twenty years old drivers exhibit the highest values and are between 2 and 2.3 more likely to cause a severe accident and between 2.6 and 3.8 more likely to cause a fatal accident than those aged 30 to 44 years. In principle, these numbers can be partly explained by a lack of driving experience. However, decomposing the accident rate by license seniority reveals that the youngest drivers exhibit worse traffic records even if compared with older individuals with the same driving experience (the darkest colors in Panel A and B). License seniority is defined based on the license year because the exact license date is not available in the accident data. This explains why 18 years old may have above-zero license seniority even if the minimum driving age in Italy is 18.

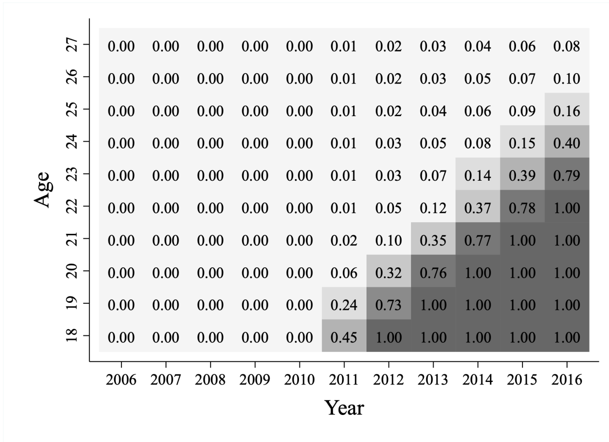
When we separate accidents by type of traffic violation (Panel C and D), we find that risky driving behavior is a major determinant of teen crashes. Compared to older drivers (30 to 44 years), teen licensees are nearly four times more likely to cause an accident due to excessive speed (1.91 *versus* 0.55 accidents per 1,000 licensees), and twice more likely to crash after running a stop sign or a red light (1.87 *versus* 0.96 accidents per 1,000 licensees). Moreover, speeding alone accounts for a quarter of all accidents and nearly half of all the deaths (45%) caused by 18 and 19 years old. These shares are much lower for older drivers: among 30 to 44 years old speeding-caused accidents account for only 15% of the total. The analysis by accident dynamics, reported in **panel D** and F which indicate percentages of total accidents for the corresponding age group, provides further evidence along these lines. The probability of runoffs is between four and eight times higher for teen drivers (1.62 accidents per 1,000 licensees) than for older drivers (0.38 and 0.20 for 30-44 and 45-54 years old drivers, respectively). This type of single-vehicle collision is the second most common type of accident for teen drivers (explaining alone one-fifth of all crashes), while they are only the fourth (or higher) most common accident dynamic for more senior drivers (the share of these accidents in the population of above-thirty drivers ranges between 7 to 10%).

4 Identification Strategy

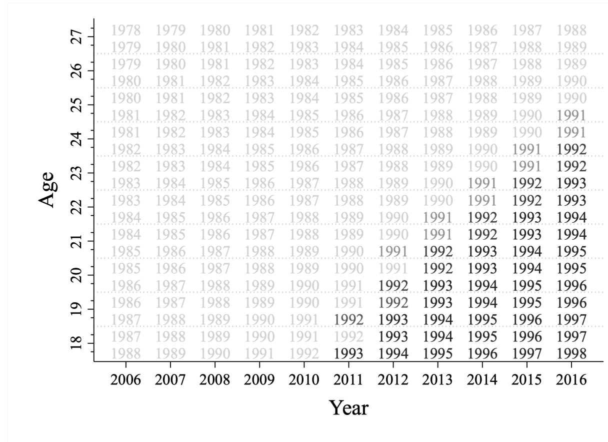
We identify the effect of the introduction of the vehicle power restriction rule on road accidents by exploiting the between-cohort differences in exposure to the reform. Figure 3 provides a visual representation of our identification strategy by plotting the share of post-reform licensees by age and year (Panel A), and by cohort (Panel B). Specifically, each number in Panel A indicates the proportion of licensees of age j in year t who obtained a driving license after February 2011. In Panel B, each number indicates the corresponding birth cohorts, that is, the possible birth years of licensees of age j observed in year t. The new regulation was introduced in February 2011. This implies that, starting from that date, age bins are progressively populated with cohorts of individuals who underwent the one-year power restriction during their first license year. Nearly half of the licensees aged 18 in 2011 (45%) obtained their license under the new rules.[12] From 2012 onwards, this share goes up to 100%, because the 18-years-old bin consists exclusively of cohorts reaching the minimum eligibility age in 2011 or later. Conversely, bins identifying older ages are mostly populated by unexposed cohorts throughout the whole period considered. Even in 2016, only a small share (8 to 10 %) of licensees aged 26 or 27 years got their license before the enforcement of the power restriction, consistent with the average age for license acquisition being 19.2 years. For this reason, we consider individuals of that age an ideal control group.

The above-described identification strategy is a Difference-in-Differences setup, where we compare the evolution of road accidents caused by drivers of different ages before and after the introduction of the vehicle power restriction. Ideally, the implementation of this strategy would require matched licensees-accidents individual-level data. With these data, one would estimate the following equation:

[12] Licensees aged 18 in 2011 could be either born in 1992 or 1993. In the former case, they might have obtained the license in 2010, before the vehicle power restriction was in place. Since the license census does not include information on the age at the time of the license, but rather the birth year, the numbers in Panel A represent an across-cohort average. The cohorts are reported in Panel B.



a. Proportion of post-reform licensees



b. Post-reform cohorts

Figure 3. Cohorts exposure to the reform

$$Y_{iat} = \alpha + \beta Age_a^{18} \times Post_t + \gamma Age_a^{18} + \delta Post_t + \lambda X_i + \epsilon_{iat} \quad (1)$$

where Y_{iat} is the indicator for whether driver i of age a caused a crash in year t, and the term Age_a^{18} identifies 18-year-old individuals, who are fully-exposed to the power limit for $t \geq 2011$ – when the indicator $Post_t$ takes value one – and where X_i are time-invariant individual characteristics such as drivers' gender and municipality.

Since matched driver-accident data are not available, we combine the data on road accidents and the driving licenses microdata into a (pseudo) balanced panel by collapsing observations into cells based on the variables common to the two datasets. Specifically, we aggregate individual observations into cells defined on two-year age groups, gender, and commuting zones, for each year over the period 2006-2016. Choosing two-year age groups limits possible zero-inflation of the data and increases the readability of the results.[13] Aggregating data by commuting zones allows us to overcome a limitation of our data. The Istat data on road accidents do not include information on drivers' residence, but only on the locality where the accident occurred. By using the Italian Labour Market Areas (*Sistemi Locali del lavoro*), which are clusters of municipalities where individuals in

[13] In the Online Appendix Table OA1 we confirm the robustness of our main results by considering one-year age groups, and varying the composition of the treatment and control group.

the local labor force live, work and commute, we lower the chances of erroneously attributing road accidents occurring in a given area to individuals residing elsewhere.[14] Additionally, grouping observation at the commuting zone-level allows us to relax the assumption of independence within groups by using a robust variance matrix calculation.

We estimate the following cell-level equivalent of Equation 1, where the unit of observation is a cell *acst*, defined based on age group (a), commuting zone (c), gender (s) – which represent the time invariant individual characteristics – and year (t):

$$Y_{acst} = \beta Age_a^{18-19} \times Post_t + \phi Female_s + \eta_{ac} + \theta_{ct} + \nu_{acst} \tag{2}$$

In the above equation, Y_{acst} is the cell-specific accident rate, defined either as (i) $\frac{Accidents_{acst}}{Inhabitants_{acst}} \times 1,000$ – the number of accidents per one thousand inhabitants – or (ii) $\frac{Accidents_{acst}}{Drivers_{acst}} \times 1,000$ – the number of accident per one thousand licensees. Age_a^{18-19} is a binary indicator taking the value of 1 for the 18-19 age group which, from 2011 onwards, is progressively populated by individuals belonging to fully-exposed cohorts. In our main specification, the control group is the 26-27 age group, as it is made by individuals who are largely unexposed to the reform for the whole period considered (as shown in Figure 3). Equation 2 also includes CZ×year fixed-effects (θ_{ct}), age group×CZ fixed effects (η_{ac}) and a gender dummy ($Female_s$).[15]

We estimate Equation 2 by Weighted Least Squares using the number of observations in each cell – that is, the denominator of the Y_{ajst} ratio – as weights. Weighting by cell size allows us to interpret the coefficient β as the effect of the reform on the individual likelihood of causing an accident. Hence, the weighted estimates of the model described in Equation 2 yields the same coefficients β that would be obtained by estimating Equation 1 on the (unavailable) underlying matched microdata. Depending on whether the outcome is defined according to (i) or (ii), the estimated DiD coefficient β capture the change in accident probability in the resident population

[14] The *Sistemi Locali del lavoro* are defined by Istat based on the commuting matrices resulting from the 2011 Italian population census.

[15] Grouping by cells defined based on age (2), commuting zones (611), gender (2), and years (10) leads to a sample of 26,884 observations. Under our main specification, we exclude the year 2011 which is only partially treated, thus ending up with a sample of 24,440 cells.

or in the sub-population of licensees, respectively.[16]

We also estimate a fully-dynamic specification of Equation (2) by replacing the term $\beta Age_a^{18-19} \times Post_t$ with $\sum_{t=2006}^{2016} \beta_t Age_a^{18-19} \times d_t$. Under this specification, the coefficients β_t for $t \geq 2011$ capture the effect of the power ban in each of the post-reform years, while for $t < 2011$ they enables us to test for the existence of pre-reform diverging patterns of road accidents between our treatment and control groups, that would pose a threat to identification.

The identification strategy relies on the assumption that no contemporaneous cohort-specific shocks or confounding policies targeting the same age classes are affecting the probability of causing a traffic accident. In Section 2, we have mentioned other policies implemented around, or shortly before, the vehicle power restriction was introduced. In principle, both the written driving test reform (January 2011) and the zero-tolerance law (July 2010) might hamper the interpretation of our estimated coefficients of interest, as they potentially target the same cohorts. To address these concerns, and provide further support for the identification assumption, we complement the analysis by testing whether the reform has a differential impact on road accidents depending on whether the car has a below- or above-limit engine. Because our data do not include information on the power of the vehicles involved in a crash (in kilowatts), we exploit information on the engine displacement in cubic centimeters (cc) which is reported for about 60% of the crash episodes. Hence, we further split accidents in cells based on the engine size of the at-fault driver×car pair and we estimate Equation 2 separately for accidents caused by vehicles with different engine size.[17] As the reform restricts teens from using cars whose engine exceeds 70kw – a limit which we approximate with an engine size of 1,500cc – we expect the power limit to impact accident rates only when the engine size is lower than 1,500cc. Conversely, if either the zero tolerance law or the written driving test reform constitutes a significant confounder – that is, if they play a part in reducing teen accidents – we would expect the estimated coefficient β to be negative and significant even in the subsample

[16] Since we define our outcomes of interest as the number of accidents per 1,000 individuals (either inhabitants or licensees), each β coefficient captures the change in accident probability in per-thousand terms.

[17] Specifically, the outcome of interest is $\frac{Accidents_{ackst}}{Drivers_{acst}}$, where k indicates the lower limit of each of the K engine size groups, and where $\sum_{k=0}^{K} \frac{Accidents_{ackst}}{Drivers_{acst}} = \frac{Accidents_{acst}}{Drivers_{acst}}$. In principle, we could limit the analysis to accidents caused by above-limit vehicles. However, this would lead to a substantial loss in terms of sample size, as information about vehicles' engine size is available only for a subset of the accidents.

of accidents for which the engine of the at-fault car is below 1,500 cc.

We also investigate whether a change in the *composition* of novice drivers might explain our results. The policy package introduced in January 2011 could have discouraged specific groups of individuals from applying for a driving license. Hence, a lower number of accidents per licensee post-reform could be the result of a positive selection of prospective drivers. We devote Section 5.3 to this issue. We study the heterogeneity of the reform impact on license propensity in order to characterize the nature of the compositional change – if any – in place.

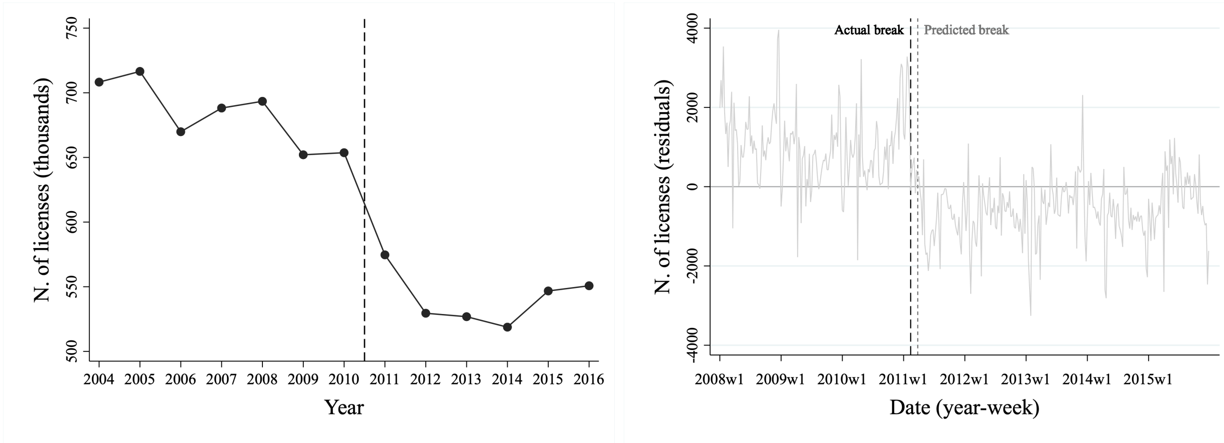
5 Results

5.1 Reform Effect on New Licensees

The 2010 reform could impact road safety in two fundamental ways. On the one hand, it may improve fresh licensees' driving records by preventing them from using high-power vehicles. On the other hand, it may also reduce the number of new licensees and thus mechanically lead to fewer teen crashes. Restrictions on young drivers have been indeed found to lower teens' propensity to obtain a driving license (Gilpin, 2019). In this light, introducing a power limit could discourage those who do not have access to a complying vehicle from obtaining a license, as it implies waiting an extra year before they can start driving. Moreover, the new written driving test could also reduce the inflow of new drivers. The new test format covers more topics and includes more questions, which could have depressed pass rates or even discouraged teens from applying for a driving license.[18]

Panel A of Figure 4 shows that even in our sample, the number of new licensees dropped substantially in the post-reform period: about 79,000 units (-12%) in 2011 and 45,000 in 2012, with an overall reduction of more than 19% compared to 2010. Panel B - where we plot the deseasonalized residuals from an OLS regression of the number of driving licenses issued every week on week-of-the-year fixed-effects - also reveals that the structural break in the number of new permits issued occurs near the date of the reform introduction (the estimated break date is March

[18] The Online Appendix Figure OA3 provides support to this latter hypothesis. The number of test-takers drops but test success rates do not (they actually increase from 2012).



a. Raw data

b. Structural break test

Figure 4. Trends in driver licenses

31st, 2011, while the date of the introduction of the power limit is February 9th, 2011). These trends seem to be driven by a tendency to postpone licensing rather than a permanent reduction in the number of drivers. In the Online Appendix Figure OA4, we depict how the proportion of licensees in each age group varies over time. The share of licensees among 18-year-olds fell from 36% in 2010 to 29.2% in 2011, further declining to 23.5% in 2016. A similar pattern emerges when looking at older individuals, but the gap narrows as the treated cohorts become older and vanishes by the age of 24. The percentage of 24-years-old with a license in 2015 (the last non-exposed cohort) is nearly identical to that of 24-years old in 2016 (the first fully-exposed cohort).

In the Online Appendix Figure OA5, we investigate how the decrease in licensing has affected teen car usage. Using data from the ISTAT *Multipurpose Survey on the Households: Aspects of Daily Life*, which includes questions on mobility habits, we show that although the reform results in a significant increase in the proportion of non-licensees among teenagers, it has only a minor effect on the likelihood they *do not use* a car. The teens who were discouraged from obtaining a license are those with the lowest propensity to drive. This is consistent with the heterogeneity analysis presented in the Online Appendix Figure OA6: the reduction of new licensees is larger in areas that rely less on private car transportation, such as urban commuting zones, where alternative modes of transportation are also available, and in areas with lower mobility rates in general. Moreover, it is more pronounced in wealthier areas, but it does not appear to depend on the characteristics of

Table 1.
The effect of the vehicle power limit on the probability of causing a traffic accident

	Accidents per capita			Accidents per licensee		
	(1) Accidents	(2) Accidents	(3) Fatal Accidents	(4) Accidents	(5) Accidents	(6) Fatal Accidents
Post × Age 18-19	-0.761**	-0.793**	-0.028**	-1.045**	-1.107**	-0.052**
	(0.115)	(0.094)	(0.007)	(0.143)	(0.127)	(0.012)
Post	-1.145**			-1.303**		
	(0.083)			(0.081)		
Age 18-19	-0.123			2.668**		
	(0.092)			(0.118)		
Female	-2.895**	-2.896**	-0.089**	-3.956**	-3.969**	-0.128**
	(0.058)	(0.048)	(0.003)	(0.067)	(0.062)	(0.005)
CZ × Age group FE	No	Yes	Yes	No	Yes	Yes
CZ × Year FE	No	No	Yes	No	No	Yes
Baseline average	4.366	4.366	0.097	8.343	8.343	0.186
Percent change	-17.4	-18.2	-28.9	-12.5	-13.3	-28.0
R^2	0.551	0.710	0.301	0.533	0.681	0.288
Observations	24440	24440	24440	24440	24440	24440

Note. All regressions include CZ fixed-effects. Baseline averages are calculated as the (weighted) mean of the dependent variable for the treatment group in the pre-reform period (2006-2010). Percent changes are calculated by dividing the estimated coefficient of $Post \times Age18-19$ by the baseline average. Robust standard errors in parentheses.
$^{+}$ p<.10
** p< .01
* p<.05

the car population.

5.2 *Reform Effect on Road Accidents*

Table 1 shows the effect of the vehicle power reform on the likelihood of teen drivers causing a road accident. The coefficient $Post \times Age^{18-19}$ captures the average effect of the policy on the number of accidents or fatal accidents per capita (Columns 1-3), and the number of accidents per licensee (Columns 4-6). The estimated effect is negative and statistically significant (at the 99% confidence level) for all outcomes and under different specifications. Exposure to the vehicle power limit reduces road accidents by -0.79 episodes per 1,000 inhabitants and -1.11 episodes per 1,000 drivers under our preferred specification (Columns 2 and 5), which includes both CZ×age-groups fixed effects and CZ×year fixed effects. Importantly, this reduction also translates into a lower number of fatal accidents, which diminish by 0.03 and 0.05 episodes per 1,000 individuals and licensees (Columns 3 and 6). These effects are economically meaningful. Compared to the corresponding baseline average, accidents per capita and per licensee drop by 18% and 13%, respectively. Similarly, fatal accidents drop by 29% (in per capita terms) and 28% (in per licensee terms).[19]

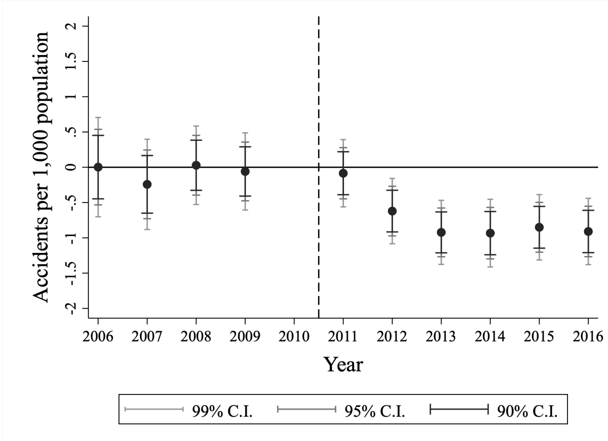
[19] The baseline average is the treatment group mean – the average number of accidents where the at-fault-driver is aged 18 or 19 years – computed over the pre-policy period 2006-2010.

The estimates presented in Columns 1-3 of Table 1 capture the reform's effect on the number of accidents per capita. Hence, they reflect a combination of a direct effect of the vehicle power limit on novice licensees and an incapacitation effect. As discussed above, the inflow of new drivers shrinks post-reform, and thus does the number of potential road crashers in the treated group.[20] However, results presented in Columns 4 to 6, where the outcome considered is the number of accidents per licensee, suggest that the vehicle power limit also directly affects the likelihood that teen drivers cause a traffic accident.

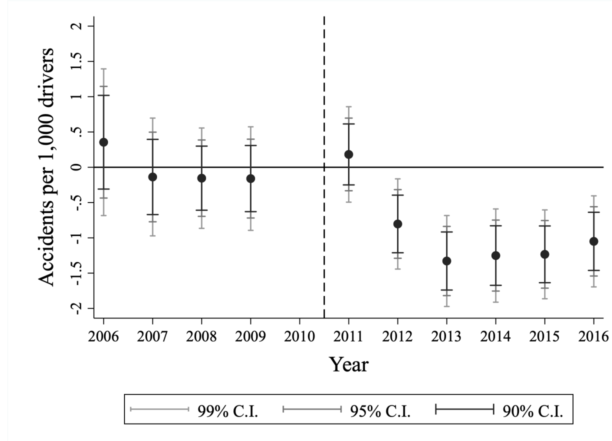
Importantly, these estimates are based on the notion of "at-fault driver" discussed in Section 3, which is based on the police reports. The introduction of the power limit could have also reduced accidents by teens who are not technically at fault, for instance by promoting safer driving behavior in general. We show that this is indeed the case by presenting the analogous of Table 1 but considering a broader definition for the outcome variable (Table OA2 in the Online Appendix). Specifically, we look at the probability of being *involved* in – rather than causing – a traffic accident. The reform induced a reduction of 1.4 teen-involved crashes per 1,000 drivers (about 10% of the baseline average). Comparing the estimates in Columns 4 to 6 of Table OA2 with their analogous in Table 1, we see that about 75% of the reduction in the accidents involving teen drivers is explained by fewer accidents *caused* by teen drivers. This share grows to 91% when considering fatal accidents (Column 6). These numbers are substantially larger than the share of caused accidents over the total number of teen-involved crashes at baseline (62 and 74%, respectively), thus suggesting that the reform operates by discouraging driving behaviors that are a direct cause of vehicle accidents.

Figure 5 depicts the dynamic effects of the power limit on accidents per capita (Panel A) and per licensee (Panel B) separately for each post-reform year. In this Figure, we plot the coefficients β_t of all interaction terms $Age18-19 \times d_t$ resulting from estimating a fully-dynamic specification of Equation 2, where the interaction with pre-reform year 2010 is the omitted term. The estimates,

[20] Besides Gilpin (2019), who shows that GDL programs improve road safety by discouraging teens from driving, a few other studies document how a reduction in the number of road users affects the road accidents rate (Lichtman-Sadot, 2019; Jackson and Owens, 2011). Bertoli et al. (2018) also highlight the existence of a composition effect in road accidents, by showing that the 2008 economic recession in Spain lowered younger (and riskier) individuals' propensity to drive, thus leading to fewer accidents. A similar result emerges from Maheshri and Winston (2016) for the case of the United States.



a. Accidents per capita



b. Accidents per licensee

Figure 5. The effect of the vehicle power limit on road accidents

along with their standard errors and regression statistics, are also reported in the Online Appendix Table OA3. Both panels show that the accident rate of the exposed cohorts (18-19 years old) drops after the introduction of the reform, as compared to the control cohorts (26-27 years old). Consistent with treatment intensity being lower in the reform year (see Figure 3), the effect is not statistically different from zero in 2011. It becomes strongly significant (at the 99% level) and larger in magnitude across all the post-policy years, ranging between -.62 and -.93 accidents per 1,000 inhabitants (Panel A), and between -.80 and -1.33 accidents per 1,000 drivers (Panel B).[21] Figure 5 also provides strong support to our identification strategy. The interaction coefficients β_t are small and not statistically significant from zero for the whole pre-2011 period, revealing the absence of pre-trends. The accidents rate in the treatment and control groups followed a nearly identical pattern before the reform.

To confirm that our DiD estimates capture the treatment effect of interest and are not driven by simultaneous confounding policies we estimate a fully-dynamic specification of Equation 2 separately for vehicles likely complying and not complying with the power limit. We do not observe in our data the engine power (in kw), but only its engine size (in cc). Since the vast majority of

[21] In Table OA1 in the Online Appendix, we show that our results are robust to alternative treatment and control groups specification and the use of single-age cells. Estimates in Columns 1 to 4 are noisier, possibly because of zero-inflated data: in only 35% of the cells the number of accidents caused by a driver aged 18 years old is different from zero. This number grows to 53% when considering the 19 years old as the treatment age group. Because of this, in our preferred specification we group observations in two-year age cells.

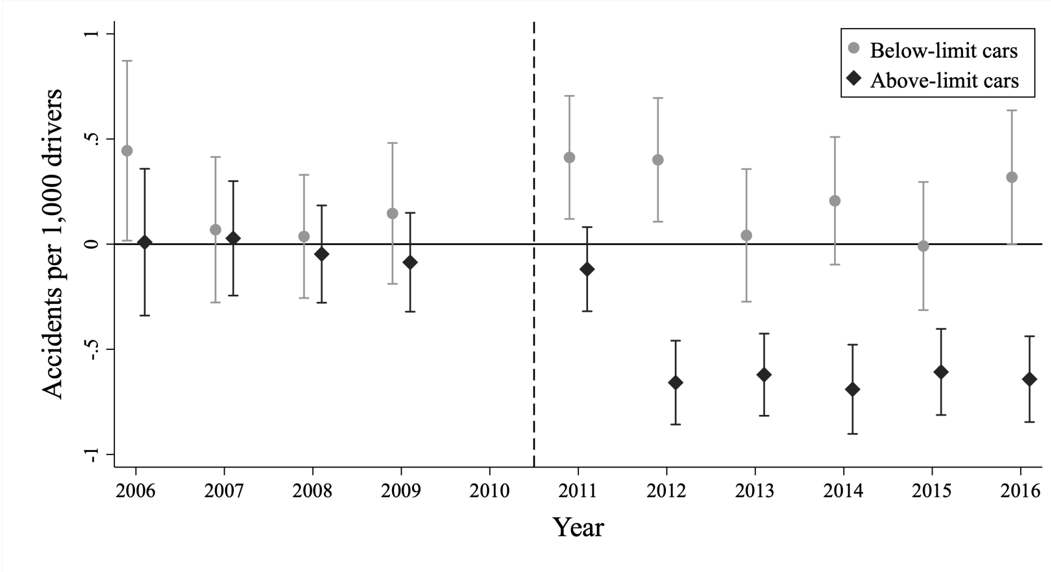


Figure 6. The effect of the power limit on traffic accidents by complying and non-complying vehicles

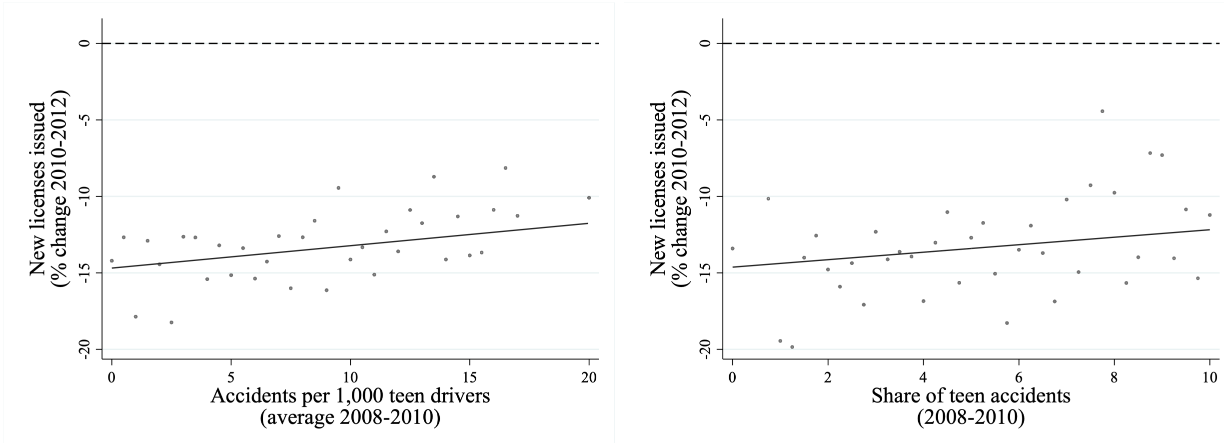
car models with an engine larger than 1,500cc also exceed the 70kw limit (as shown in the Online Appendix Figure OA1), we split road crashes into two groups, depending on whether they are caused by a vehicle with an engine size below or above 1,500cc. If the vehicle power limit is the sole driver of the DiD estimates presented in Table 1 and Figure 5, we expect that this effect is entirely driven by the fewer crashes caused by vehicles with non-complying engines. Figure 6 shows that this is indeed the case. The post-policy interaction coefficients are negative and significant only when limiting the analysis to accidents caused by vehicles exceeding the power limit. Conversely, we observe a zero or marginally positive effect on the probability of crashes by vehicles with a complying engine, a finding that clashes with other policies or confounders explaining our results. The Online Appendix Table OA4 provides further evidence in this direction by presenting estimates separately for five engine size classes. The negative and significant estimates for the interaction coefficients β_t are specific to accidents where the engine of the at-fault car is between 1500 to 1800cc (Column 5) or above 1800cc (Column 6).

The decomposition of the effect on accidents per licensee by accident category, reported in the Online Appendix Table OA5, shows that more than 40% of the overall effect is driven by fewer accidents involving a single vehicle only (mostly run-off collisions) and nearly half (44%) is due to a reduction in the number of accidents caused by excessive speed, which decreased by over a fifth. In addition, about 60% of the overall impact of the reform is explained by a reduction in nighttime accidents, although these account for less than a third of all teen accidents. These findings support our interpretation of the effects of the reform: the power restriction limits dangerous driving behaviors, such as excessive speed, which typically occur at night and during the weekends.

5.3 Alternative Mechanisms and Spillovers

In the previous section, we show that the 2010 reform improved teens' driving records. We interpret this evidence as the *direct* effect of the power limit on teens' driving behavior. In principle, other mechanisms may explain our results. In Section 5.1 we highlight that the number of new licensees also drops post-reform. While we account for the shrinking number of teen drivers by defining accident rates in *per licensee* terms, it is still possible that a composition effect also drives our findings. If the imposition of the power limit – or the simultaneous introduction of stricter test standards – discouraged potentially risky drivers from getting a license, this would ultimately have translated into fewer teen accidents.

To test for this alternative hypothesis, we investigate the characteristics of the marginal teen discouraged from obtaining the license. For the power ban to induce such a composition effect on teen accidents, it should be able to keep potentially *riskier* drivers out of the road. We do not observe this measure at the *individual* level, but our data allow us to compute the baseline accident risk at the aggregate (cell) level. Hence, in Figure 7 we plot the teen accident propensity – defined as the teen accident rate (Panel A) and the incidence of teen crashes on the total accidents (Panel B) – against the percentage reduction in the number of new teen licenses within the same cell over the (pre-reform) years 2008-2010. Each dot indicates the average reduction in the number of licenses issued within a bin, defined based on the pre-reform level of teen accident propensity in a given commuting zone×sex cell. Solid lines display the linear prediction. The pattern depicted in the two panels clashes with alternative mechanisms driving our results, such as a positive selection of prospect drivers. This hypothesis would indeed require a negative relationship between ex-ante accident propensity and license drop. On the contrary, the relationship between these two measures is flat or slightly increasing.



a. Teen accident rate

b. Share of accidents caused by teen drivers

Figure 7. Accident risk and reduction in teen licensing

In the Online Appendix, we provide two additional pieces of evidence against the concern that our findings are driven by a change in the composition of teen drivers. First, in Figure OA7, we study the gender heterogeneity of the reform impact. Gender is indeed a well-acknowledged determinant of road safety and driving behavior (Francesconi and James, 2021; Rhodes and Pivik, 2011; Li et al., 1998).[22] Even in our sample, teen males are much more likely to cause a traffic accident than female drivers, mostly because of a higher propensity to engage in behaviors such as speeding and stop or traffic lights violations. Hence, for a positive selection of prospect drivers to be in place, one would expect the reduction of new licensees to be larger for males rather than females. We show that this is not the case, as the likelihood of earning a driving license decreases homogeneously (by about ten percentage points) among teen males and females. Yet, the effect of the reform on accidents is heterogeneous by gender: Panel D of Figure OA7 suggests that the power limit mostly reduces accidents among teen males.[23] Second, we also find that the reform has a limited impact on the share of teens who do not drive (as shown in the Online Appendix Figure OA5). This implies that the marginal driver eliminated by the reform is someone who, in the absence of the reform, would have gotten the license but rarely used the car, thus constituting a *safe* rather than *risky* driver.

[22] Also, other studies (Lucidi et al., 2010; Cordellieri et al., 2016) highlight that female drivers tend to be much more concerned about the risk of a road accident than their male counterparts.

[23] This result is also confirmed in the Online Appendix Table OA6, reporting the estimates from a triple-difference specification of Equation 2 where we further interact the term $Post \times Age18-19$ with a gender dummy.

Finally, we assess the net effect of the reform by testing whether it generates negative spillovers in the form of more accidents caused by unrestricted vehicles. By limiting the set of cars suitable for teen drivers, the reform could trigger a switch towards alternative modes of transport, such as motorcycles and scooters, thus leading to more non-car crashes. The Online Appendix Figure OA8 suggests that this is not the case. The figure depicts the reform's effect on accidents per capita (Panel A) and per licensee (Panel B) separately for vehicle types. Neither scooter nor motorcycle accidents rise following the introduction of the power limit on cars. Relatedly, in the Online Appendix Table OA7, we document that the power limit does not induce a negative spillover by increasing multi-passenger car accidents. In this case, we decompose the average effect of the reform by the number of occupants in the at-fault car, and we find that the power limit decreases uniformly teen accidents regardless of the number of passengers carried.

6 Effect persistence

6.1 Post-ban Effects

A potential challenge to the usefulness of GDL programs, which establish a staged approach to driver licensing, is that their beneficial effects could vanish once the temporary bans have been removed. Thus, it is worth assessing whether licensees who underwent the power limit during their first license year exhibit lower accident rates even after the ban is lifted.

To do so, we exploit the information on the license issue year, available for more than 80% of crash episodes in the Istat data. We estimate a variant of the fully-dynamic specification of Equation 2, where the age group 20-21 years is the treatment group, and the group 26-27 is the control group. Moreover, we restrict the sample to accidents caused by drivers no longer exposed to the restriction. Being t the year of the accident, we limit the analysis to episodes where the at-fault driver had a license since $t-2$ or earlier.[24] Treated units are those drivers aged 20-21 years with two or three years of license seniority. From 2013 onwards, these units are exposed to the reform – they underwent the power restriction during the first license year – as they obtained the

[24]As we do not know the exact license issue date, we also exclude accidents by drivers who got their license in $t-1$, who may still be under the one-year power restriction.

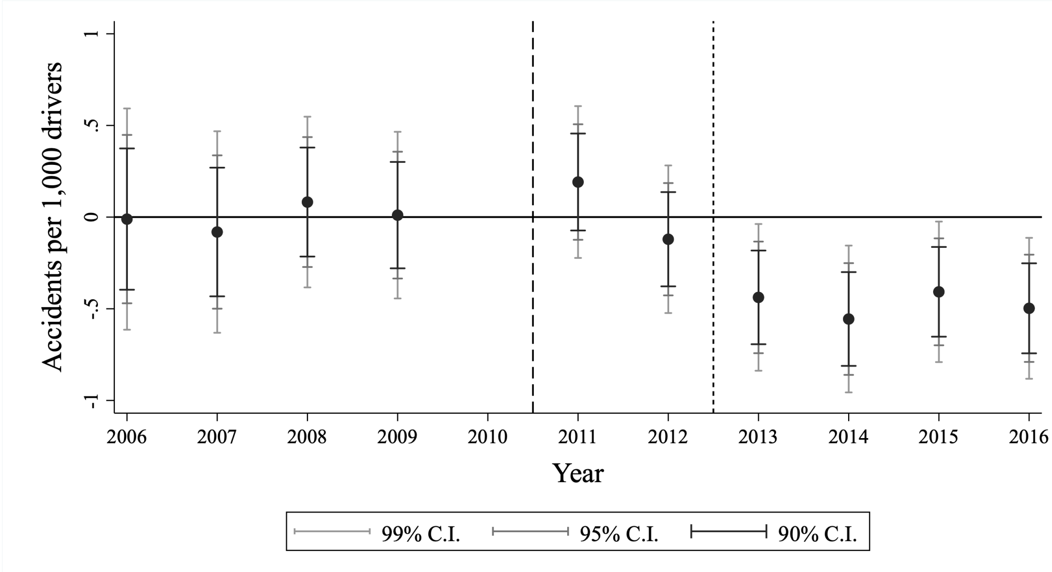


Figure 8. Post-ban effects on road accidents

license in 2011 or later. The estimated coefficients from this regression are depicted in Figure 8. The estimates are negative and significant for $t \geq 2013$, consistent with the reform's impact being long-lived. Drivers subject to the one-year restriction period are less likely to cause a car accident even after this expires.

This result is also presented in the Online Appendix Table OA8, where we restrict (Columns 3 and 4) or do not restrict (Columns 1 and 2) the sample to drivers who have had their license for at least two years. Estimates reveal that the power restriction, over the period 2013-2016, reduces the likelihood of traffic accidents occurring as well as accidents due to excessive speeding among drivers aged 20 or 21 years. Consistent with the proposed mechanism, the interaction coefficient β_{2012} is larger in magnitude in Columns 1 and 2, when the sample also includes those drivers who reached the license eligibility age before the reform but who obtained their license in the post-reform period (in t or $t-1$). By contrast, it is much smaller in Columns 3 and 4, where we limit the sample to drivers who obtained their license in $t-2$ or earlier. These experienced drivers in 2012 were not exposed to the reform and were allowed to drive any type of vehicle during their first license year.

Taken together, these findings also highlight that the reform effectively reduces traffic accidents even if new licensees strategically postpone car use, waiting for the restriction period to expire.

If this were the case, a one-year power limit would simply delay novice drivers' accidents to the unrestricted regime, and we would not observe any lasting effect of the regulation.

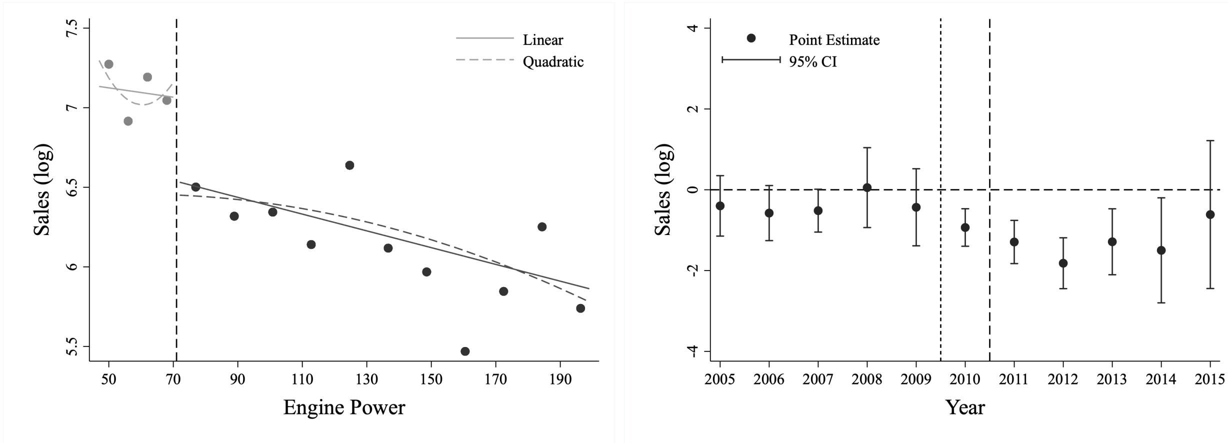
6.2 Long-run Mechanisms

Two main mechanisms may explain the lasting effect of the one-year power limit. First, being constrained within a low-risk setting could encourage enduring virtuous driving habits among young drivers. The role of legal regimes or regulatory policies on habit formation is a common (and debated) issue in various contexts. For example, Severen and Van Benthem (2022) show that individuals who were exposed to the oil crises of the 1970s during their formative driving years are less likely to drive 20 years later. Kaestner and Yarnoff (2011) highlight a similar link between exposure to different drinking-age regimes during youth and later alcohol consumption and traffic fatalities. Additionaly, Williams (2005) shows that students who face stricter drunk driving laws in secondary school tend to consume less alcohol even in college.[25] Applied to driving behavior, this interpretation would be consistent with that of Moore and Morris (2021), who find that the effect of a one-year ban on carrying multiple passengers at night persists even after the ban is lifted.

A second explanation relies on the new rule inducing a change in car choice. At least in the short run, the reform decreases the utility of choosing a car not complying with the power limit, and thus incentivizes the choice of low-powered cars for fresh licensees. Hence, the long-lasting impact of the restriction could also be explained by drivers remaining under a low-risk setting – a less powerful and thus less risky car – even after the restriction is lifted, as cars are typically chosen on a long-time horizon.

While we cannot disentangle the two mechanisms, we provide evidence that the reform does affect car choice by exploiting the ACI data on Italian car sales from 2006 to 2016. We test whether a discontinuity arises around the maximum consented power threshold, that is whether the sales of (barely) complying car models boost compared to sales of (barely) above-limit ones. Figure 9 summarizes the result of our regression-discontinuity (RD) exercise. In Panel A, each circle

[25] For further discussion on the role of stringent policies in establishing good standards of behavior, see the study by Viscusi et al. (2011).



a. Car sales post-reform

b. RD estimates by year

Figure 9. The effect of the power limit on car sales

represents the average sales (in logarithms) of car models in each engine-size bin in the post-reform period 2011-2014. Panel A shows that a negative and discontinuous jump emerges at the 70 kw cutoff, thus confirming that the restriction boosted sales of car models satisfying the engine power threshold, likely at the expense of models with larger engine size.[26] Panel B reports the bias-corrected RD coefficients obtained using the robust estimator proposed in Calonico et al. (2014), along with their confidence intervals, for each year. The estimates are small in magnitude and not statistically different from zero throughout the period 2005-2009. Conversely, after the power restriction was announced (July 2010), they become negative and statistically significant at the 99% confidence level. The new regulation induced a sharp change in the Italian car market. As less powerful cars are both an illiquid asset and a means of transport less suitable for risky driving, this composition effect may explain the long-lasting reduction in teen drivers' accident rates.

7 Discussion and conclusions

Our empirical results show that a targeted restriction on novice drivers, which prevents them from engaging in a high-risk activity such as speeding, can have a large impact on road safety. The introduction of the power limit led to 13% fewer accidents by novice drivers and to 28% fewer fatalities. The analysis of the dynamic effects of the power limit on injuries and deaths – depicted

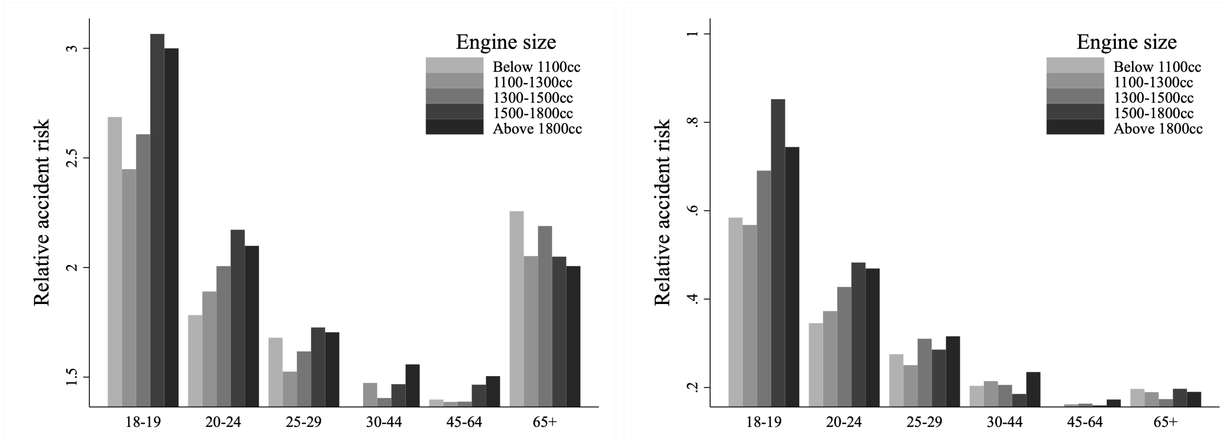
[26]The Online Appendix Figure OA9 is the 2006-2009 analog of the evidence presented in Panel A of Figure 9, and shows that no discontinuity emerges in the pre-reform period.

in the Online Appendix Figure OA10 – reveals that the power limit prevented teen drivers from causing about 6,200 injuries and 95 deaths. This represents a considerable gain in road safety, especially when compared with the total number of injuries (45,000) and deaths (670) due to teen accidents in the five years before the reform. As shown in the Online Appendix Table OA9, a significant share of the lives saved and injuries prevented (30% and 36%, respectively) involves occupants of other (not-at-fault) vehicles and pedestrians. The overall safety gain of the reform is even larger if we also consider the long-lasting effects presented in the previous section, as teens exposed to the reform exhibit diminishing accident rates even when they grow older.

Our findings support the recurring view in the literature that specific vehicles impose a large negative externality on other road users and that limiting their usage can mitigate this cost. For example, White (2004) and Anderson and Auffhammer (2014) studied the consequences of the American "arms race" to heavier cars. While heavier cars are safer to their occupants when an accident occurs, they also increase the probability of a fatality in lighter-struck cars because an increased perceived safety may boost risky driving, as originally pointed out by Peltzman (1975). Our results align with this view, as vehicle power and weight are positively correlated. By preventing teen drivers from using more powerful, heavier cars, the reform generates an *external* safety benefit which, crucially, does not occur at the expense of *internal* safety. The power limit lowers injuries and deaths also in the striking vehicle, thus revealing that the internal benefits outweigh any potential (safety) cost of switching to a lighter car.

Importantly, such welfare gains come with lower costs when compared to alternative regulation policies. In principle, higher taxes on fuel or the ownership of high-power vehicles could improve road safety by (implicitly) increasing the price of speeding. However, previous estimates indicate that achieving the same effect as this policy would require very large increases (at least 50%) in taxation.[27] More in general, the power limit entails relatively low welfare costs for the economy – by limiting private mobility, which is a primary source of utility (for example, social life) and work productivity (for example, timely and flexible mobility) – because it affects only a particular subset

[27] For example, Morrisey and Grabowski (2011) estimate for the US that a 10% increase in gasoline prices would reduce one-year driver fatalities by about 6% for 15-17-year-olds and 3% for 18-20-year-olds. Cross-country estimates for reported deaths after a car crash lie within the same range (Burke and Nishitateno, 2015).



a. All accidents

b. Accidents due to excessive speed

Figure 10. Vehicle engine power and accident risk

of the population. Importantly, this subset is chosen among the individuals who are most likely to cause a traffic accident, that is, those who generate the largest internalities and externalities.

This idea is not novel in the literature. Allcott et al. (2019) mention it among the well-established guiding principles for policymakers to design taxes on sin goods such as sugar-sweetened beverages. They highlight that policies in these contexts should aim to reduce consumption the most among children and adolescents who, for instance, have the greatest lack of self-control.[28] Moreover, these principles are the underlying rationale for all interventions that seek to reduce road accidents by limiting teen driving. These go from minimum driving age rules – which prevent underage teens from driving *at all* – to programs that allow driving but under *specific* limitations (such as the power limit under study and GDL programs in general). In a context of asymmetric information, where individual risk type is not observed, these kinds of policies target the categories with the highest accident rate, such as teens.[29]

Hence, from an *efficiency* standpoint, the identification of a suitable target is crucial for the success of corrective policies that work by discouraging risky behaviors that cannot simply be

[28] Also, Griffith (2022) warns that policies aimed at reducing obesity (such as corrective taxes) may result in additional costs without any benefit if people with high internalities or externalities do not respond.

[29] Our estimates align with previous findings on the effectiveness of such policies. For example, Huh and Reif (2021) estimate for the US that a one-year increase in minimum driving age could reduce motor vehicle fatalities by up to 44%. Likewise, Bostwick and Severen (2022) estimate that increasing the minimum full-privilege driving license age induces a reduction of fatal car accidents among 16-years-old ranging between 17% and 34%.

banned tout-court. Moore and Morris (2021) study the effect of an Australian GDL scheme that prevents novice drivers from carrying peer passengers overnight. They estimate that the restriction led to 57% fewer nighttime multi-passenger accident crashes and 50% fewer casualties. Their findings thus suggest that even a relatively contained restriction in teen mobility opportunities may result in significant safety gains when directed toward those behaviors that originate the largest harm (night multi-passenger accidents account for less than 3% of all crashes, but explain almost a fifth of all teen deaths). We argue that the Italian power limit operates in a similar manner. It (implicitly) discourages teens from engaging in speed driving – a behavior that is a major determinant of teen crashes – by restricting access to a good that may be a *complement* to risky driving.[30] Figure 10 corroborates this hypothesis by showing the driver-vehicle-specific accident risk in the pre-reform period.[31] For most age groups, the relationship between vehicle power and accident risk is flat or slightly increasing, indicating that car engine size is not a major factor in traffic crashes. By contrast, when focusing on young drivers, this relations is positive and steep. For teen drivers in particular, accident risk increases by up to 20% when using cars exceeding 1,500cc (those banned by the reform). The gradient in engine size is even more striking when focusing on accidents caused by excessive speeding. Importantly, these estimates likely represent a lower bound, as our DiD estimates imply up to a 60% increase in teen accident risk associated with the use of high-power vehicles.[32]

To conclude, our findings highlight the importance of defining road safety policies that combine the choice of a narrow target group of individuals (those generating the largest externalities and

[30] This parallels the findings on the effect of changes in the minimum legal drinking age on motor vehicle deaths, as even alcohol is both a cause and a facilitator of dangerous driving (Carpenter and Dobkin, 2009).

[31] For each age group (a) and engine size (k), vertical bars of Figure 10 indicate the ratio $\frac{At-fault_{ak}}{Not-at-fault_{ak}}$ where $At-fault_{ak}$ is the number of accidents caused by a driver of age a who drives a vehicle with engine size k and $Not-at-fault_{ak}$ is the number of accidents involving a not-at-fault driver-car pair ak. This latter term is a proxy for the number of car rides (or driving time) for each driver-car type, under the assumption that the higher the driving time, the higher the likelihood of being involved in an accident caused by someone else. The vehicle-driver pairing of not-at-fault drivers is presented in the Online Appendix Figure OA11. The sample is limited to accidents for which the information on the car engine size is available. Statistics are relative to the year 2010.

[32] The baseline accident rate (8.3) is a weighted average of that of teen drivers using low-power and high-power vehicles, where weights are set to .75 and .25, respectively (Figure 10 highlights that, when unconstrained, 25% of teen drivers use above-limit cars). Thus, the power limit levels the accident probability among all teens to that associated with the use of low-power vehicles. If the accident rate of teen drivers using high-power cars is 20% higher than that of teens using low-power cars, we would expect a decline from 8.3 to 7.9 crashes per 1,000 drivers. This a priori estimate is smaller than what we actually obtain with our DiD strategy (from 8.3 to 7.2 crashes).

internalities) with specific settings correlated with risky driving behavior. In this sense, the power limit displays appealing features from both an effectiveness and efficiency standpoint. A gradual phased-in entitlement of licenses to unrestricted use of vehicles, that does not limit mobility for the majority of teens, effectively prevents traffic-related injuries and deaths. Furthermore, although the restriction lasts only one year, it has persistent positive effects on road safety. These policies have received little attention, particularly in European countries, where most interventions take the form of deterrence strategies targeting fully-licensed drivers (and generally have a short-lived impact). More generally, we emphasize the importance of strategically limiting young drivers' exposure to high-risk settings, particularly when their driving risk and skills are difficult to assess and targeted screening strategies are challenging to implement. Yet, the definition of such high-risk settings requires continuous adjustments. Policy targets should be regularly updated to keep pace with technological advancements, market strategies, and global trends, such as, for instance, the introduction and diffusion of electric vehicles.

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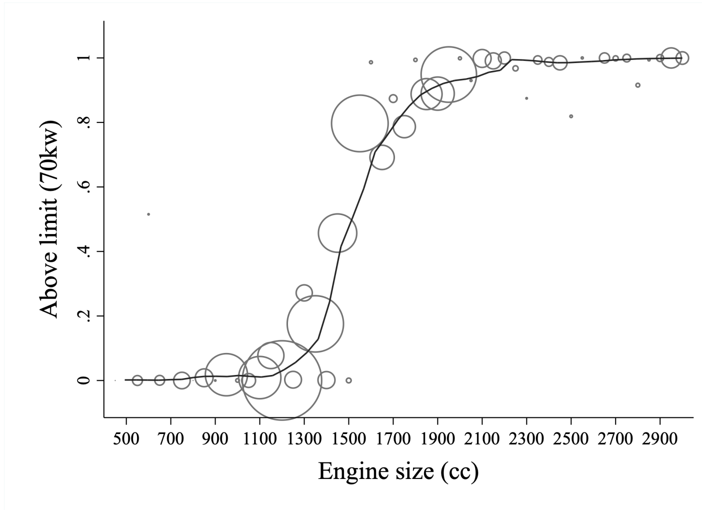
Appendix

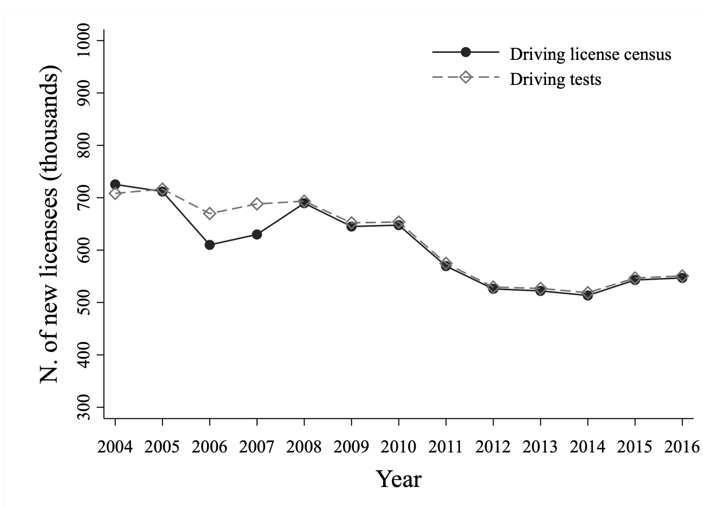
A.1 Figures

Figure OA1. Relationship between engine size and engine power



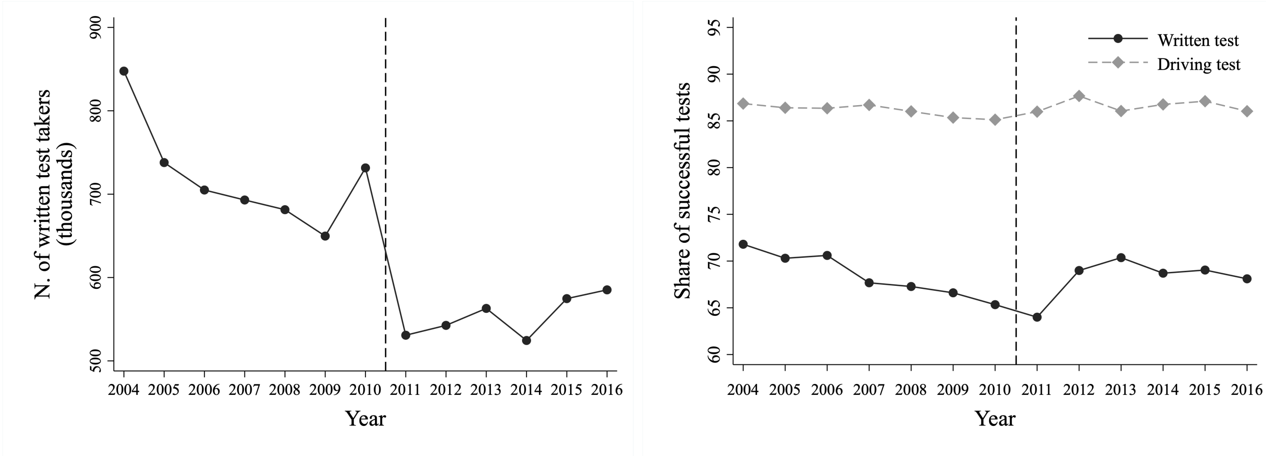
Notes. This figure shows the relationship between engine displacement and engine power for cars registered in the Italian vehicle census over the period 2000-2016. The dataset includes all registered cars as of May 2017. Markers indicate the proportion of car models exceeding the 70kw power limit within each engine displacement bin (of width 50cc). Markers' size indicates the number of observations in each bin. The solid line represents the predicted share of above-limit car models from a kernel-weighted local polynomial regression.

Figure OA2. Official and reconstructed time series of licensees



Notes. This figure depicts the number of driving licenses issued in each year over the period 2004-2016. The solid line indicates the number of new licensees based on the driving license census, while the dashed line is for the number of successful driving tests based on the MIT reports on driving exams.

Figure OA3. Time trends of the driving exams

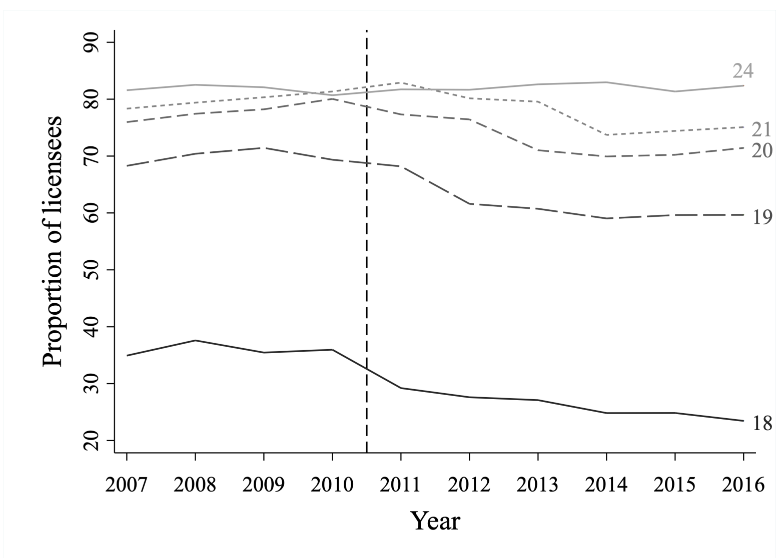


a. Written Test Takers per Year

b. Test Success Rate

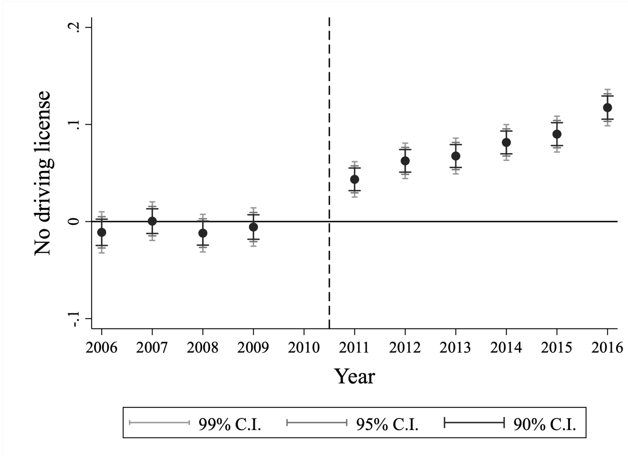
Notes. This figure depicts the evolution of the number of (written) test takers (Panel A) and of the proportion of successful driving and written tests (Panel B) over the years 2004-2016.

Figure OA4. Population with a driver's license by age

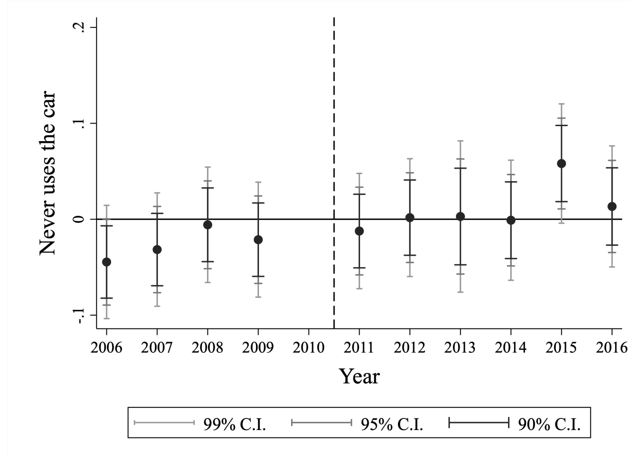


Notes. This figure depicts how the proportion of licensees in the population evolved during the period 2007-2016. Each line indicates, for each of the years considered, the share of individuals of a given age – reported on the right-hand side – who have a driving license.

Figure OA5. Reform effect on licensing and car use



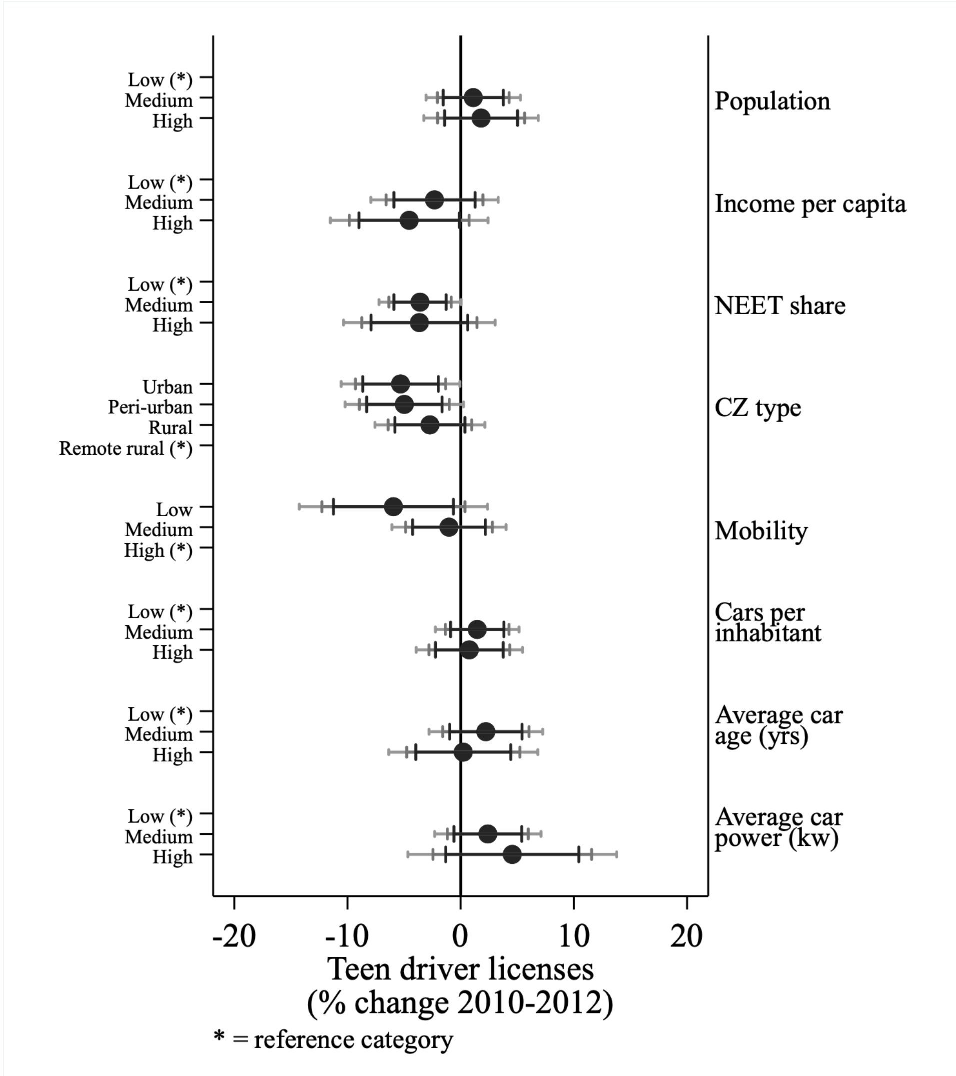
a. No driving license



b. Never use a car

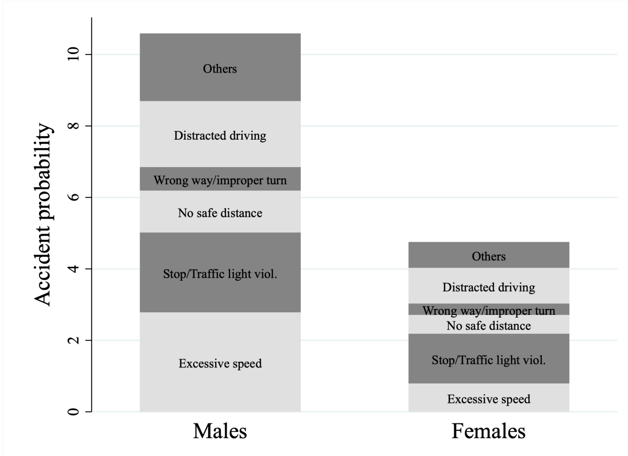
Notes. These figures depict the effect of the reform on the probability of not having a license (Panel A) and of not using a car (Panel B). In each figure, dots indicate the estimated interaction coefficients $Age18 - 19 \times d_t$ – for different values of t, reported on the x-axis – from a regression model of the form of Equation 2. The interaction term relative to the pre-reform year (Age 18-19 × year 2010) is the omitted term. In Panel A, the dependent variable is the population without a driving license in a cell – defined based on age, gender, and year – over the total population of the same cell. In Panel B, it is an indicator taking value one when an individual answers “never” to the question “How often do you drive a car” in the Istat *Multipurpose Survey on the Households: Aspects of Daily Life*. This is a a population survey carried out annually on a sample of about 20,000 households which, among the other questions, asks respondents about how often they use (drive) a car, indicating five possible answers: “Never”, “a few times a year”, “a few times per month, “a few times per week”, and “every day”. The category of non-drivers includes both people without a driver’s license (who cannot drive by law) and those who, although achieved a license, never or very rarely drive. In Panel A, regressions also include regressions include a gender dummy, CZ×age-groups fixed effects, and CZ×year fixed effects. They are estimated by WLS, where weights are the total population in each cell. In Panel B, regressions also include an indicator for a respondent’s age (taking value one for individuals aged 18-19 and zero for those aged 25 to 34), gender, region of residence, and years fixed-effects. Vertical spikes indicate robust confidence intervals at the 90%, 95%, and 99% level.

Figure OA6. Heterogeneous reduction in teen licensing

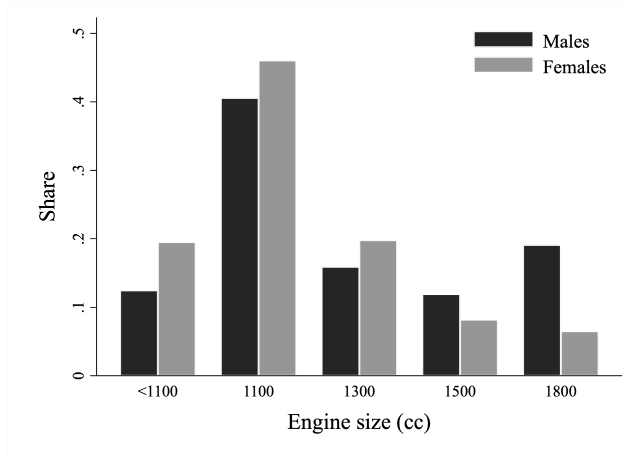


Notes. This figure depicts the geographical heterogeneity in the reduction of new licensees between 2010 and 2012. The figure reports the estimated coefficients and associated confidence interval from a regression where the dependent variable is the percentage change in the number of licenses issued in each commuting zone, and the independent variables are a set of categorical variables capturing commuting zone characteristics. These are *i.* the terciles of the distribution of commuting zones by population, *ii.* income, and *iii.* the proportion of NEET young people; *iv.* a classification of Italian municipalities from *urban* to *remote rural*, as they are defined by the Italian National Governmental Agency For Territorial Cohesion; *v.* the terciles of the distribution of mobility intensity (based on the 2011 Italian National Census), *vi.* number of cars per inhabitant, *vii.* the average age and *viii.* average power of the commuting zone car population. *Low* indicates the bottom tercile – the reference category in all cases but *iv.* – *Medium* the intermediate tercile, and *High* the top tercile. As for measure *iv.*, commuting zones are classified as *urban*, *peri-urban*, *rural*, or *remote rural* based on the share of inhabitants residing in municipalities falling in each category (for instance, a commuting zone is defined as *urban* if the majority of the population lives in an *urban* municipality.

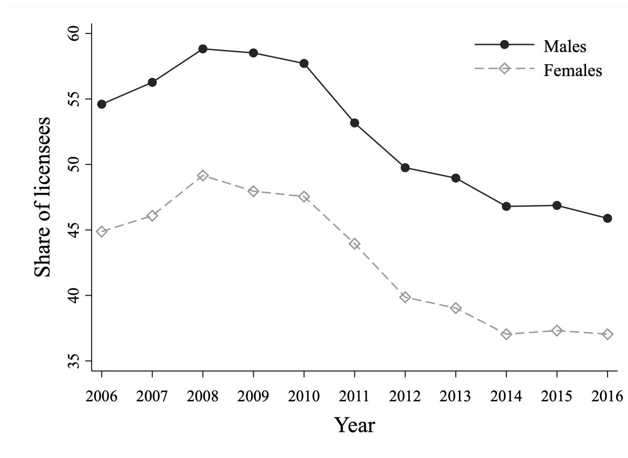
Figure OA7. Gender heterogeneity



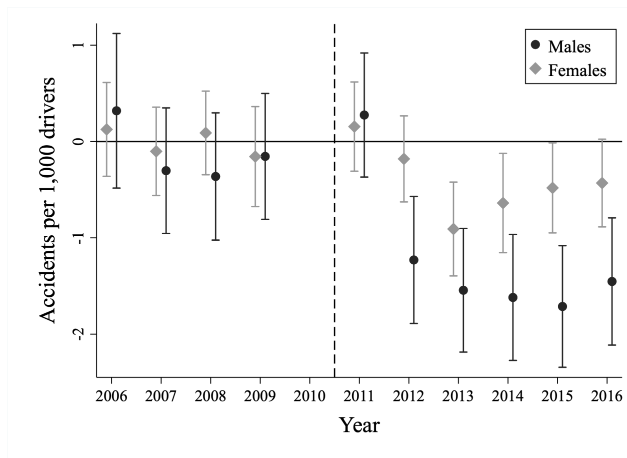
a. Baseline accident probability



b. Vehicle-driver paring



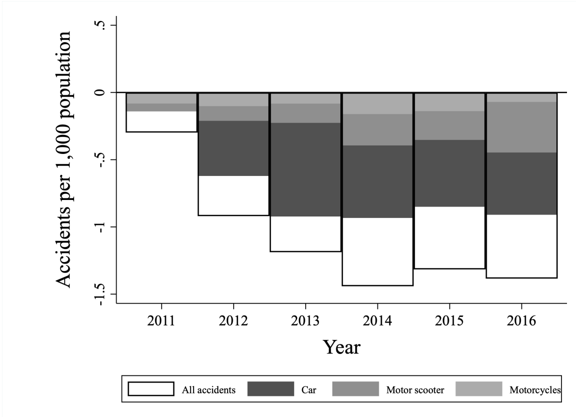
c. Time trends of teen licensees



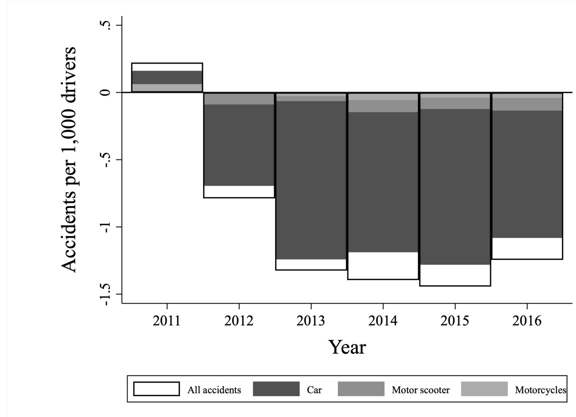
d. Reform impact on accident rates

Notes. This figure depicts the gender differences in the (pre-reform) probability of causing a traffic accident (Panel A), the likelihood of using a low- or high-power car (Panel B), the time trends in teen licensing (Panel C), and the impact of the reform on accidents' probability. In Panel A, each vertical bar represents the accident probability by type of violation committed by at-fault teen male or female drivers per 1,000 licensees of the same sex. In Panel B, vertical bars indicate the share of teen male and female drivers who drive a car of a given engine size (in the x-axis). Shares are calculated using the number of non-at-fault driver-vehicle pairs observed in the database as an approximation of the unobservable number of actual car rides for each pair. In both panels, statistics are relative to the year 2010. In Panel B, the sample is limited to observations for which the information on the car engine size is available. The number of teens driving a car of a given engine size class is approximated by the number of not-at-fault drivers involved in an accident in 2010. Panel C depicts the share of males and females teens (aged 18 or 19) who have a driving license in each year. Panel D depicts the gender heterogeneity of the effect of the power restriction on the likelihood of teen drivers causing traffic accidents. Dots indicate the estimated interaction coefficients $Age18 - 19 \times d_t$ – for different values of t, reported on the x-axis – from regression models of the form of Equation 2. Regressions are estimated separately for female and male drivers. The dependent variable is defined as the number of accidents caused by drivers-car pairs in a cell – defined based on age, gender, commuting zone, engine size, and year – per 1,000 licensees of the corresponding age, gender, commuting zone, and year group. The interaction term relative to the pre-reform year (Age 18-19 × year 2010) is the omitted term. Regressions include CZ×age-group fixed-effects, and CZ×year fixed-effects. Regressions are estimated by WLS, where weights are the number of licensees in each cell. Vertical spikes indicate robust confidence intervals at the 95% level.

Figure OA8. Possible spillover effects: car, motor scooter, and motorcycles accidents rates



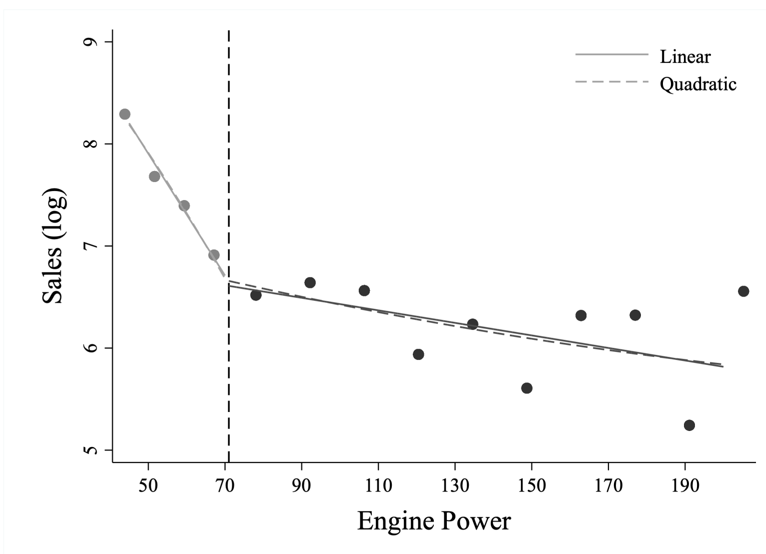
a. Accidents per capita



b. Accidents per licensees

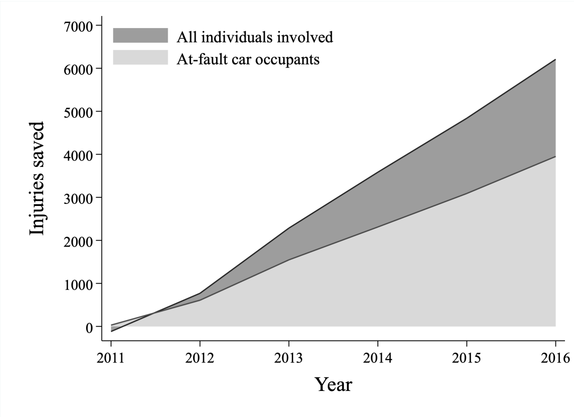
Notes. These figures depict the effect of the car power limit on the likelihood of teens causing a traffic accident when using different types of vehicles. These include cars but also unrestricted vehicles such as motor scooter (engine <125cc), and larger motorcycles (engine ≥125cc). In each figure, vertical bars indicate the estimated interaction coefficients $Age18-19 \times d_t$ – for different values of t, reported on the x-axis – from a regression model of the form of Equation 2. The interaction term relative to the pre-reform year (Age 18-19 × year 2010) is the omitted term. In Panel A, the dependent variable is the number of accidents caused by drivers (of the corresponding vehicle type) in a cell – defined based on age, gender, commuting zone and year – per 1,000 population of the same cell. In Panel B, it is the number of accidents per 1,000 licensees (*i.e.* owners of a car license). All regressions include a gender dummy, CZ×age-group fixed-effects, and CZ×year fixed-effects. Regressions are estimated by WLS, where weights are the total population (Panel A) or the number of licensees (Panel B) in each cell.

Figure OA9. Car sales by model engine power in the pre-reform period

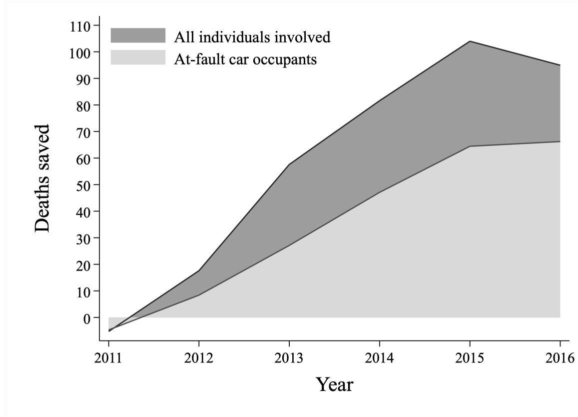


Notes. This figure shows how average car sales vary based on the model engine power. Markers indicate the average number of car sales (in logarithm) in the pre-reform period 2006-2009 within each engine-power bin. The solid (dashed) lines represent the predicted sales from linear (quadratic) regressions estimated separately for observations to the left and to the right of the cutoff (70kw).

Figure OA10. Back-of-the-envelope calculations on the number of prevented injuries and deaths



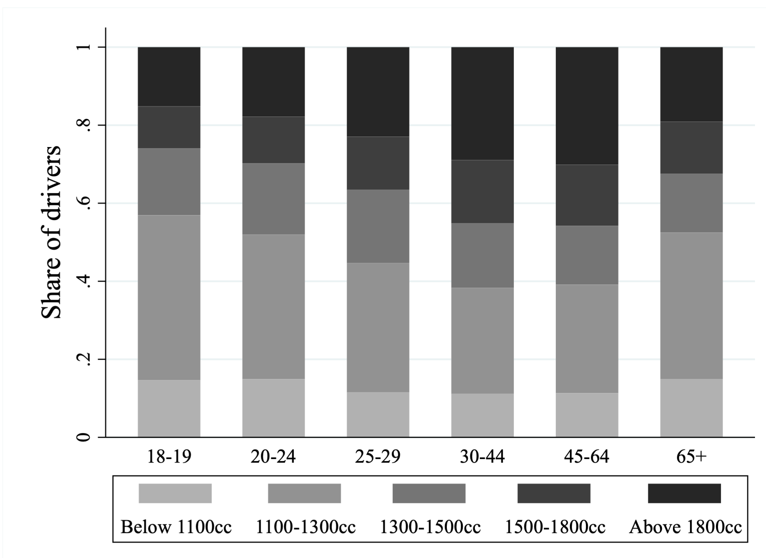
a. Prevented injuries



b. Prevented deaths

Notes. This figure illustrates the number of injuries (Panel A) and deaths (Panel B) saved because of the introduction of the power limit. This is obtained by multiplying, for each year t, the estimated interaction coefficient $Age18-19 \times d_t$ – from a regression model of the form of Equation 2, where the dependent variable is the total number of injuries (deaths) caused by drivers in a specific cell – with the number of licensees of the same cell and year. Cells are defined based on age, gender, and commuting zone. The estimated interaction coefficients are reported in Appendix Table OA9. Darker colors indicate the total value, while lighter ones refer to occupants of the at-fault car only.

Figure OA11. Vehicle-driver pairing



Notes. This figure depicts the share of drivers in each age group (reported on the horizontal axis) who drive a vehicle of a specific engine size range. The shares are computed based on considering only the sample of not-at-fault observations in the road accidents database, that is, drivers× vehicle pairs who are involved in a car accident but are not culpable of any traffic violation, according to the police report. Darker colors indicate larger engine size.

A.2 Tables

Table OA1. Robustness to different age group specifications

	Treatment age group: 18-years-old				Treatment age group: 19-years-old			
	(1) Control: 24	(2) Control: 25	(3) Control: 26	(4) Control: 27	(5) Control: 24	(6) Control: 25	(7) Control: 26	(8) Control: 27
Treatment age × year 2006	0.718	0.497	0.805^{+}	0.950^{+}	-0.071	-0.272	-0.001	0.146
	(0.493)	(0.496)	(0.481)	(0.492)	(0.470)	(0.469)	(0.453)	(0.456)
Treatment age × year 2007	-0.529	-0.678^{+}	-0.823*	-0.726^{+}	0.471	0.326	0.145	0.277
	(0.388)	(0.393)	(0.386)	(0.386)	(0.391)	(0.395)	(0.397)	(0.390)
Treatment age × year 2008	-0.619^{+}	-0.347	-0.646^{+}	-0.427	-0.019	0.253	-0.063	0.189
	(0.363)	(0.365)	(0.359)	(0.359)	(0.322)	(0.334)	(0.331)	(0.324)
Treatment age × year 2009	-0.208	-0.063	0.052	0.067	-0.528	-0.393	-0.303	-0.249
	(0.379)	(0.374)	(0.361)	(0.367)	(0.346)	(0.346)	(0.342)	(0.350)
Treatment age × year 2011	0.664^{+}	0.630	0.881*	1.026**	-0.495^{+}	-0.529^{+}	-0.320	-0.121
	(0.400)	(0.395)	(0.393)	(0.395)	(0.298)	(0.289)	(0.300)	(0.300)
Treatment age × year 2012	-0.949**	-0.927**	-0.903*	-0.920**	-0.893**	-0.812**	-0.833**	-0.837**
	(0.354)	(0.346)	(0.351)	(0.349)	(0.310)	(0.296)	(0.303)	(0.296)
Treatment age × year 2013	-1.404**	-1.720**	-1.577**	-1.588**	-1.077**	-1.387**	-1.293**	-1.295**
	(0.357)	(0.345)	(0.344)	(0.347)	(0.301)	(0.290)	(0.295)	(0.300)
Treatment age × year 2014	-1.141**	-1.204**	-1.476**	-1.357**	-0.962**	-1.013**	-1.354**	-1.180**
	(0.361)	(0.365)	(0.356)	(0.359)	(0.304)	(0.300)	(0.306)	(0.295)
Treatment age × year 2015	-1.294**	-1.384**	-1.399**	-1.406**	-1.128**	-1.195**	-1.242**	-1.257**
	(0.346)	(0.336)	(0.335)	(0.341)	(0.302)	(0.291)	(0.296)	(0.295)
Treatment age × year 2016	-1.399**	-1.553**	-1.411**	-1.504**	-0.909**	-1.046**	-0.966**	-1.018**
	(0.349)	(0.347)	(0.345)	(0.348)	(0.290)	(0.301)	(0.303)	(0.295)
Baseline average	7.269	7.269	7.269	7.269	8.435	8.435	8.435	8.435
R^2	0.542	0.539	0.533	0.531	0.606	0.612	0.608	0.614
Observations	26877	26877	26877	26877	26884	26884	26884	26884

Notes. This table reports estimates from Equation 2 varying the composition of the treatment and the control group. The unit of observation is a cell defined based on age, gender, commuting zone and year. In all columns, the dependent variable is the number of accidents caused by drivers in a specific cell per 1,000 licensees of the same cell. The interaction term relative to the pre-reform year (Age 18-19 × year 2010) is the omitted term. In all columns, regressions include a gender dummy, CZ×age-group fixed-effects, and CZ×year fixed-effects. Regressions are estimated by WLS, where weights are the number of licensees in each cell. Baseline averages are calculated as the (weighted) mean of the dependent variable for the treatment group in the pre-reform year 2010. Robust standard errors in parentheses.

$^{+}$ p<.10
** p< .01
* p<.05

Table OA2. The effect of the vehicle power limit on the probability of being involved in a traffic accident

	Accidents per capita			Accidents per licensee		
	(1)	(2)	(3)	(4)	(5)	(6)
	Accidents	Accidents	Fatal Accidents	Accidents	Accidents	Fatal Accidents
Post × Age 18-19	-0.945**	-0.993**	-0.026**	-1.396**	-1.466**	-0.057**
	(0.165)	(0.127)	(0.008)	(0.185)	(0.157)	(0.013)
Post	-2.203**			-2.504**		
	(0.122)			(0.113)		
Age 18-19	-0.545**			4.023**		
	(0.132)			(0.156)		
Female	-3.847**	-3.850**	-0.115**	-5.100**	-5.110**	-0.165**
	(0.084)	(0.065)	(0.004)	(0.091)	(0.083)	(0.006)
CZ × Age group FE	No	Yes	Yes	No	Yes	Yes
CZ × Year FE	No	No	Yes	No	No	Yes
Baseline average	7.052	7.052	0.131	13.476	13.476	0.251
Percent change	-13.4	-14.1	-19.5	-10.4	-10.9	-22.8
R^2	0.642	0.798	0.350	0.630	0.779	0.337
Observations	24440	24440	24440	24440	24440	24440

NOTES. This table reports the effect of being subject to the power restriction on the likelihood of being involved in a traffic accident. The unit of observation is a cell defined based on age, gender, commuting zone and year. In Columns 1 and 2, the dependent variable is the number of accidents involving drivers (i.e., regardless of whether they is at-fault or not-at-fault-) in a specific cell per 1,000 population of the same cell. In Column 3, it is the number of fatal accidents per 1,000 population. In Columns 4 and 5, it is the number of accidents per 1,000 licensees, while in Column 6 it is the number of fatal accidents per 1,000 licensees. $Post$ is an indicator that equals one for the cells identifying the post-reform years 2012-2016 and zero for the pre-reform years 2006-2010, $Age18-19$ is an indicator that equals one for cells identifying the treatment age group 18-19 and zero for the control group 26-27, and $Post \times Age18-19$ is their interaction. The reform year (2011) is excluded. All regressions include CZ fixed-effects. In Columns 2,3, 5, and 6 they also include CZ×age-group and CZ×year fixed-effects. Regressions are estimated by WLS, where weights are the total population (Columns 1-3) or the number of licensees (Columns 4-6) in each cell. Baseline averages are calculated as the (weighted) mean of the dependent variable for the treatment group in the pre-reform period (2006-2010). Percent changes are calculated by dividing the estimated coefficient of $Post \times Age18-19$ by the baseline average. Robust standard errors in parentheses.
$^+$ p<.10
** p< .01
* p<.05

Table OA3. The effect of the vehicle power limit on road accidents: year-by-year estimates

	Accidents per capita			Accidents per licensee		
	(1) Accidents	(2) Accidents	(3) Fatal Accidents	(4) Accidents	(5) Accidents	(6) Fatal Accidents
Age 18-19 × year 2006	-0.092	0.002	0.004	0.270	0.355	0.046
	(0.304)	(0.273)	(0.018)	(0.418)	(0.403)	(0.032)
Age 18-19 × year 2007	-0.301	-0.242	-0.026	-0.183	-0.137	-0.021
	(0.280)	(0.248)	(0.017)	(0.344)	(0.324)	(0.029)
Age 18-19 × year 2008	0.004	0.029	0.016	-0.185	-0.154	0.031
	(0.256)	(0.216)	(0.016)	(0.305)	(0.276)	(0.026)
Age 18-19 × year 2009	-0.059	-0.059	0.002	-0.212	-0.160	0.002
	(0.244)	(0.212)	(0.015)	(0.303)	(0.285)	(0.025)
Age 18-19 × year 2011	-0.090	-0.084	0.002	0.150	0.182	0.006
	(0.223)	(0.185)	(0.015)	(0.300)	(0.263)	(0.026)
Age 18-19 × year 2012	-0.616**	-0.621**	-0.025	-0.806**	-0.803**	-0.038
	(0.217)	(0.179)	(0.015)	(0.272)	(0.248)	(0.026)
Age 18-19 × year 2013	-0.924**	-0.922**	-0.047**	-1.349**	-1.328**	-0.075**
	(0.220)	(0.176)	(0.015)	(0.273)	(0.250)	(0.025)
Age 18-19 × year 2014	-0.949**	-0.933**	-0.034*	-1.261**	-1.251**	-0.047^{+}
	(0.230)	(0.186)	(0.015)	(0.282)	(0.256)	(0.026)
Age 18-19 × year 2015	-0.878**	-0.850**	-0.029*	-1.247**	-1.234**	-0.041^{+}
	(0.237)	(0.180)	(0.014)	(0.287)	(0.244)	(0.024)
Age 18-19 × year 2016	-0.946**	-0.910**	-0.010	-1.075**	-1.050**	-0.003
	(0.245)	(0.182)	(0.015)	(0.295)	(0.250)	(0.026)
Age 18-19	-0.023			2.762**		
	(0.172)			(0.219)		
Female	-2.886**	-2.887**	-0.088**	-3.938**	-3.948**	-0.126**
	(0.054)	(0.045)	(0.003)	(0.061)	(0.058)	(0.005)
CZ × Age group FE	No	Yes	Yes	No	Yes	Yes
CZ × Year FE	No	Yes	Yes	No	Yes	Yes
Baseline average	4.244	4.244	0.079	8.041	8.041	0.149
R^2	0.554	0.708	0.296	0.539	0.681	0.285
Observations	26884	26884	26884	26884	26884	26884

Notes. This table reports the estimates from Equation 2. The unit of observation is a cell defined based on age, gender, commuting zone and year. In Columns 1 and 2, the dependent variable is the number of accidents (fatal accidents in Column 3) caused by drivers in a specific cell per 1,000 population of the same cell. In Columns 4 and 5, it is the number of accidents per 1,000 licensees, while in Column 6 it is the number of fatal accidents per 1,000 licensees. The interaction term relative to the pre-reform year (Age 18-19 × year 2010) is the omitted term. All regressions include CZ fixed-effects. In Columns 2, 3, 5, and 6 they also include CZ×age-group and CZ×year fixed-effects. Regressions are estimated by WLS, where weights are the total population (Columns 1-3) or the number of licensees (Columns 4-6) in each cell. Baseline averages are calculated as the (weighted) mean of the dependent variable for the treatment group in the pre-reform year 2010. Robust standard errors in parentheses.
$^{+}$ p<.10
** p< .01
* p<.05

Table OA4. The effect of the power limit on traffic accidents by complying and non-complying vehicles

	All	Below Limit			Above Limit		Unknown
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
		0-1100 cc	1100-1300 cc	1300-1500 cc	1500-1800 cc	Above 1800 cc	
Age 18-19 × year 2006	0.355	0.217**	-0.069	0.296**	0.149^{+}	-0.140	-0.099
	(0.403)	(0.072)	(0.120)	(0.097)	(0.089)	(0.110)	(0.184)
Age 18-19 × year 2007	-0.137	0.009	-0.020	0.079	0.074	-0.046	-0.234
	(0.324)	(0.064)	(0.118)	(0.073)	(0.071)	(0.091)	(0.164)
Age 18-19 × year 2008	-0.154	0.048	-0.100	0.088	0.048	-0.095	-0.144
	(0.276)	(0.057)	(0.103)	(0.073)	(0.059)	(0.084)	(0.173)
Age 18-19 × year 2009	-0.160	0.054	0.011	0.081	-0.021	-0.065	-0.220
	(0.285)	(0.056)	(0.112)	(0.072)	(0.064)	(0.081)	(0.163)
Age 18-19 × year 2011	0.182	0.082	0.148	0.182*	-0.048	-0.071	-0.111
	(0.263)	(0.058)	(0.102)	(0.071)	(0.055)	(0.076)	(0.159)
Age 18-19 × year 2012	-0.803**	0.060	0.263*	0.078	-0.238**	-0.420**	-0.546**
	(0.248)	(0.059)	(0.108)	(0.070)	(0.054)	(0.072)	(0.148)
Age 18-19 × year 2013	-1.328**	0.040	0.054	-0.052	-0.278**	-0.343**	-0.749**
	(0.250)	(0.064)	(0.112)	(0.071)	(0.053)	(0.070)	(0.146)
Age 18-19 × year 2014	-1.251**	0.054	0.104	0.048	-0.299**	-0.391**	-0.767**
	(0.256)	(0.061)	(0.109)	(0.068)	(0.056)	(0.075)	(0.145)
Age 18-19 × year 2015	-1.234**	-0.008	0.042	-0.043	-0.273**	-0.334**	-0.617**
	(0.244)	(0.061)	(0.106)	(0.066)	(0.053)	(0.074)	(0.150)
Age 18-19 × year 2016	-1.050**	0.014	0.326**	-0.021	-0.270**	-0.372**	-0.726**
	(0.250)	(0.063)	(0.115)	(0.075)	(0.054)	(0.072)	(0.148)
Baseline average	8.041	0.723	1.865	0.805	0.595	0.822	3.231
R^2	0.681	0.405	0.494	0.417	0.433	0.484	0.629
Observations	26884	26884	26884	26884	26884	26884	26884

NOTES. This table reports estimates from Equation 2 for different subgroups of accidents defined based on the engine size of the at-fault vehicle. In Column 1, the dependent variable is the number of accidents caused by drivers in a specific cell per 1,000 licensees of the same cell. In Columns 2 to 7, the dependent variable is the number of accidents caused by drivers-car pairs in a cell – defined based on age, gender, commuting zone, engine size, and year – per 1,000 licensees of the corresponding age, gender, commuting zone, and year group. Hence, the horizontal sum of the coefficients in Columns 2 to 7 is equal to the estimated effect for the whole sample of accidents (Column 1). The coefficients in Column 7 are relative to accidents caused by cars with unknown engine size. The interaction term relative to the pre-reform year (Age 18-19 × year 2010) is the omitted term. In all columns, regressions include a gender dummy, CZ×age-group fixed-effects, and CZ×year fixed-effects. Regressions are estimated by WLS, where weights are the number of licensees in each cell. Baseline averages are calculated as the (weighted) mean of the dependent variable for the treatment group in the pre-reform year 2010. Robust standard errors in parentheses.
$^{+}$ p<.10
** p< .01
* p<.05

Table OA5. Effect decomposition by accident category

Panel A: By accident type

	All	Multi-car					Single-car	
	(1)	(2) Head-on	(3) Side impact	(4) Side swipe	(5) Rear-end	(6) Pedestrian	(7) Runoff	(8) Other single-car
Post × Age 18-19	-1.107**	-0.084**	-0.313**	-0.118**	-0.132**	-0.006	-0.366**	-0.083*
	(0.127)	(0.024)	(0.065)	(0.028)	(0.039)	(0.006)	(0.041)	(0.033)
Baseline average	8.343	0.668	3.100	0.710	1.235	0.034	1.675	0.906
Percent change	-13.3	-12.5	-10.1	-16.7	-10.7	-19.0	-21.8	-9.2
R^2	0.681	0.365	0.586	0.413	0.490	0.291	0.434	0.400
Observations	24440	24440	24440	24440	24440	24440	24440	24440

Panel B: By at-fault driver's behaviour

	All							
	(1)	(2) Excessive speed	(3) Stop/Traffic light viol.	(4) No safe distance	(5) Wrong way/impr. turn	(6) Impr. overtaking	(7) Distracted driving	(8) Others
Post × Age 18-19	-1.107**	-0.483**	-0.188**	-0.067*	-0.061*	-0.026*	-0.111**	-0.172**
	(0.127)	(0.048)	(0.050)	(0.033)	(0.024)	(0.011)	(0.038)	(0.035)
Baseline average	8.343	2.046	2.057	0.875	0.586	0.156	1.313	1.311
Percent change	-13.3	-23.6	-9.1	-7.6	-10.5	-16.9	-8.4	-13.1
R^2	0.681	0.479	0.556	0.463	0.371	0.290	0.447	0.399
Observations	24440	24440	24440	24440	24440	24440	24440	24440

Panel C: By accident time, day, and location

	All	Time		Day		Location		
	(1)	(2) Day	(3) Night	(4) Weekday	(5) Weekend	(6) Urban	(7) Extra-urban	(8) Highway
Post × Age 18-19	-1.107**	-0.432**	-0.663**	-0.546**	-0.561**	-0.771**	-0.288**	-0.048*
	(0.127)	(0.089)	(0.062)	(0.086)	(0.066)	(0.114)	(0.051)	(0.021)
Baseline average	8.343	5.629	2.663	5.298	3.044	5.788	2.139	0.415
Percent change	-13.3	-7.7	-24.9	-10.3	-18.4	-13.3	-13.5	-11.5
R^2	0.681	0.616	0.552	0.609	0.537	0.693	0.486	0.495
Observations	24440	24440	24440	24440	24440	24440	24440	24440

Panel D: By age of the driver of the struck car

	All	Time		Day		Location		
	(1)	(2) Day	(3) Night	(4) Weekday	(5) Weekend	(6) Urban	(7) Extra-urban	(8) Highway
Post × Age 18-19	-1.107**	-0.111**	-0.090**	-0.016	-0.274**	-0.075+	-0.012	-0.529**
	(0.127)	(0.016)	(0.026)	(0.028)	(0.051)	(0.040)	(0.020)	(0.054)
Baseline average	8.343	0.367	0.724	0.685	1.990	1.424	0.357	2.794
Percent change	-13.3	-30.3	-12.4	-2.3	-13.8	-5.3	-3.5	-18.9
R^2	0.681	0.332	0.406	0.402	0.527	0.456	0.335	0.472
Observations	24440	24440	24440	24440	24440	24440	24440	24440

NOTES. This table reports the effect of being subject to the power restriction on the likelihood of causing a traffic accident, separately by accident category. The unit of observation is a cell defined based on age, gender, commuting zone and year. In Column 1 the dependent variable is the number of accidents caused by drivers in a cell divided per 1,000 licensees of the same cell. In Columns 2 to 8, the dependent variable is the number of accidents of each category caused by drivers in a cell per 1,000 licensees of the same cell. Since the categories are mutually exclusive, the horizontal sum of the coefficients in Columns 2 to 8 is equal to the overall effect (in Column 1). In Panel A, we decompose the effect of the power limit by accident dynamic (multi-car *versus* single-car); in Panel B, by the behavior of the at-fault driver; in Panel C, by accident time (day or night, weekday or weekend); in Panel D, by the age of the drivers in the struck vehicle. $Post \times Age18-19$ is the interaction between $Post$ – an indicator that equals one for the cells identifying the post-reform years 2012-2016 and zero for the pre-reform years 2006-2010 – and $Age18-19$ – an indicator that equals one for cells identifying the treatment age group 18-19 and zero for the control group 26-27. The reform year (2011) is excluded. All regressions include a gender dummy, CZ×age-group fixed-effects and CZ×year fixed-effects. Regressions are estimated by WLS, where weights are the number of licensees in each cell. Baseline averages are calculated as the (weighted) mean of the dependent variable for the treatment group in the pre-reform period (2006-2010). Percent changes are calculated by dividing the estimated coefficient of $Post \times Age18-19$ by the baseline average. Robust standard errors in parentheses.

+ p<.10

** p< .01

* p<.05

Table OA6. Gender heterogeneity

	All							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
		Excessive speed	Stop/Traffic light viol.	No safe distance	Wrong way/impr. turn	Impr. overtaking	Distracted driving	Others
Age 18-19 × Female	-2.320**	-1.213**	-0.317**	0.034	-0.142**	-0.057**	-0.282**	-0.344**
	(0.187)	(0.068)	(0.077)	(0.048)	(0.035)	(0.016)	(0.053)	(0.050)
Post × Age 18-19	-1.340**	-0.734**	-0.197**	-0.015	-0.081*	-0.033$^+$	-0.083	-0.197**
	(0.166)	(0.067)	(0.069)	(0.047)	(0.035)	(0.017)	(0.053)	(0.049)
Post × Age 18-19 × Female	0.772**	0.620**	0.067	-0.070	0.059	0.019	-0.038	0.114$^+$
	(0.227)	(0.088)	(0.096)	(0.063)	(0.047)	(0.022)	(0.074)	(0.067)
Baseline average (Males)	11.161	3.006	2.544	1.143	0.783	0.225	1.690	1.769
Baseline average (Females)	4.716	0.810	1.430	0.529	0.332	0.066	0.826	0.722
R^2	0.706	0.508	0.561	0.468	0.374	0.292	0.452	0.411
Observations	24440	24440	24440	24440	24440	24440	24440	24440

Notes. This table reports estimates from a triple-difference specification of Equation 2, where we further interact the term $Post \times Age18-19$ with a gender dummy. The unit of observation is a cell defined based on age, gender, commuting zone and year. In Column 1 the dependent variable is the number of accidents caused by drivers in a cell divided per 1,000 licensees in the same cell. In Columns 2 to 8, the dependent variable is the number of accidents of each category caused by drivers in a cell per 1,000 licensees of the same cell. The sample includes accidents occurred over the period 2006-2016, but the reform year (2011) is excluded. In all columns, the regressions also include a gender dummy, its interaction with the $Post$ indicator ($Post \times Female$), CZ×age-group fixed-effects, and CZ×year fixed-effects. Regressions are estimated by WLS, where weights are the number of licensees in each cell. Robust standard errors in parentheses.
$^+$ p<.10
** p< .01
* p<.05

Table OA7. Effect decomposition by number of passengers in the striking car

	All	N. of car occupants		
	(1)	(2)	(3)	(4)
		1	2	3 or more
Post × Age 18-19	-1.107**	-0.648**	-0.240**	-0.219**
	(0.127)	(0.102)	(0.038)	(0.028)
Baseline average	8.343	5.932	1.516	0.895
Percent change	-13.3	-10.9	-15.8	-24.4
R^2	0.681	0.655	0.426	0.373
Observations	24440	24440	24440	24440

Notes. This table reports the effect of being subject to the power restriction on the likelihood of causing a traffic accident, separately by the number of occupants in the at-fault vehicle. The unit of observation is a cell defined based on age, gender, commuting zone and year. In Column 1 the dependent variable is the number of accidents caused by drivers in a cell divided per 1,000 licensees in the same cell. In Columns 2, 3, and 4, the dependent variable is the number of accidents where occupants in the struck car are one, two, or three or more (respectively) caused by drivers in a cell per 1,000 licensees of the same cell. $Post \times Age18-19$ is the interaction between $Post$ – an indicator that equals one for the cells identifying the post-reform years 2012-2016 and zero for the pre-reform years 2006-2010 – and $Age18-19$ – an indicator that equals one for cells identifying the treatment age group 18-19 and zero for the control group 26-27. The reform year (2011) is excluded. All regressions include a gender dummy, CZ×age-group fixed-effects and CZ×year fixed-effects. Regressions are estimated by WLS, where weights are the number of licensees in each cell. Baseline averages are calculated as the (weighted) mean of the dependent variable for the treatment group in the pre-reform period (2006-2010). Percent changes are calculated by dividing the estimated coefficient of $Post \times Age18-19$ by the baseline average. Robust standard errors in parentheses.
$^+$ p<.10
** p< .01
* p<.05

Table OA8. Post-ban effects on road accidents

	All		License since t-2 or earlier	
	(1)	(2)	(3)	(4)
	All accidents	Excessive speed	All accidents	Excessive speed
Age 20-21 × year 2006	0.451	0.192^{+}	-0.011	0.036
	(0.406)	(0.107)	(0.234)	(0.069)
Age 20-21 × year 2007	0.195	0.122	-0.081	0.056
	(0.336)	(0.104)	(0.213)	(0.072)
Age 20-21 × year 2008	0.471	0.090	0.082	0.046
	(0.286)	(0.089)	(0.181)	(0.061)
Age 20-21 × year 2009	0.421	0.045	0.011	-0.053
	(0.306)	(0.093)	(0.176)	(0.061)
Age 20-21 × year 2011	0.096	0.054	0.191	0.095
	(0.239)	(0.076)	(0.161)	(0.059)
Age 20-21 × year 2012	-0.441*	-0.076	-0.121	-0.019
	(0.225)	(0.080)	(0.156)	(0.059)
Age 20-21 × year 2013	-0.673**	-0.250**	-0.438**	-0.143*
	(0.222)	(0.074)	(0.155)	(0.057)
Age 20-21 × year 2014	-0.808**	-0.193*	-0.556**	-0.119^{+}
	(0.219)	(0.080)	(0.155)	(0.062)
Age 20-21 × year 2015	-0.544*	-0.247**	-0.408**	-0.157*
	(0.223)	(0.079)	(0.149)	(0.062)
Age 20-21 × year 2016	-0.442*	-0.156*	-0.497**	-0.086
	(0.218)	(0.077)	(0.149)	(0.059)
Baseline average	7.268	1.583	4.171	0.907
R^2	0.712	0.506	0.630	0.421
Observations	26884	26884	26884	26884

NOTES. This table reports estimates from Equation 2 where the age group 20-21 years is the treatment group. The unit of observation is a cell defined based on age, gender, commuting zone and year. In Columns 1 and 3, the dependent variable is the number of accidents caused by drivers in a specific cell per 1,000 licensees of the same cell, while in Columns 2 and 4 it is the number of accidents due to excessive speeding per 1,000 licensees. The interaction term relative to the pre-reform year (Age 18-19 × year 2010) is the omitted term. In Columns 3 and 4, the sample includes only drivers with two years of license seniority or more. In all Columns, regressions include a gender dummy, CZ×age-group fixed-effects, and CZ×year fixed-effects. Regressions are estimated by WLS, where weights are the number of licensees in each cell. Baseline averages are calculated as the (weighted) mean of the dependent variable for the treatment group in the pre-reform year 2010. Robust standard errors in parentheses.
$^{+}$ p<.10
** p< .01
* p<.05

Table OA9. Injuries and deaths in the striking and struck vehicles

	Injuries			Deaths		
	(1)	(2)	(3)	(4)	(5)	(6)
	All	In at-fault car	In other vehicles	All	In at-fault car	In other vehicles
Age 18-19 × year 2006	0.256	0.070	0.186	0.050	-0.003	0.053^{+}
	(0.716)	(0.399)	(0.389)	(0.037)	(0.020)	(0.029)
Age 18-19 × year 2007	-0.214	0.111	-0.325	-0.000	-0.011	0.011
	(0.641)	(0.376)	(0.337)	(0.035)	(0.019)	(0.026)
Age 18-19 × year 2008	-0.743	-0.566	-0.176	0.058^{+}	0.007	0.051^{+}
	(0.548)	(0.347)	(0.281)	(0.034)	(0.018)	(0.028)
Age 18-19 × year 2009	-0.861	-0.406	-0.455	0.000	-0.003	0.003
	(0.563)	(0.348)	(0.293)	(0.031)	(0.018)	(0.022)
Age 18-19 × year 2011	0.203	-0.052	0.255	0.009	0.008	0.001
	(0.553)	(0.352)	(0.277)	(0.031)	(0.017)	(0.023)
Age 18-19 × year 2012	-1.677**	-1.093**	-0.584*	-0.043	-0.025	-0.019
	(0.528)	(0.336)	(0.266)	(0.031)	(0.016)	(0.025)
Age 18-19 × year 2013	-2.986**	-1.849**	-1.137**	-0.079**	-0.037*	-0.042^{+}
	(0.511)	(0.321)	(0.263)	(0.029)	(0.016)	(0.022)
Age 18-19 × year 2014	-2.678**	-1.575**	-1.103**	-0.049	-0.041*	-0.008
	(0.522)	(0.332)	(0.264)	(0.031)	(0.016)	(0.024)
Age 18-19 × year 2015	-2.578**	-1.601**	-0.978**	-0.046	-0.036*	-0.010
	(0.511)	(0.326)	(0.257)	(0.028)	(0.016)	(0.022)
Age 18-19 × year 2016	-2.847**	-1.790**	-1.058**	0.019	-0.004	0.022
	(0.511)	(0.328)	(0.256)	(0.032)	(0.017)	(0.024)
Baseline average	14.809	8.251	6.559	0.169	0.072	0.097
R^2	0.611	0.520	0.591	0.279	0.284	0.269
Observations	26884	26884	26884	26884	26884	26884

NOTES. This table reports the estimates from Equation 2. The unit of observation is a cell defined based on age, gender, commuting zone and year. In Columns 1 to 3, the dependent variable is the number of people injured in accidents caused by drivers in a specific cell per 1,000 licensees of the same cell. In Columns 4 to 6, it is the number of caused deaths per 1,000 licensees. In Columns 2 and 3 (5 and 6), the number of injuries (deaths) per 1,000 licensees is split between injured (dead) people in the striking at-fault car and injured (dead) occupants of other struck vehicles or pedestrians. The interaction term relative to the pre-reform year (Age 18-19 × year 2010) is the omitted term. In all columns, regressions include a gender dummy, CZ×age-group fixed-effects, and CZ×year fixed-effects. Regressions are estimated by WLS, where weights are the number of licensees in each cell. Baseline averages are calculated as the (weighted) mean of the dependent variable for the treatment group in the pre-reform year 2010. Robust standard errors in parentheses.

$^{+}$ $p<.10$
** $p< .01$
* $p<.05$