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

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Abstract

This paper investigates how a vehicle power limit on young novice drivers impacts teen traffic accidents in Italy. First introduced in 2011, the reform prevents drivers from using high-performance vehicles during their first license year. We combine rich administrative data on severe accidents over the period 2006-2016 with the driving license census to assess whether undergoing the power limit lowers the likelihood of causing a traffic accident. Our difference-in-difference estimates – we leverage on the between-cohort differences in the exposure to the reform – reveal that the power limit reduces road accidents per capita by about 18%, and accidents per licensee by 13%. The effect is entirely determined by a drop in accidents caused by above-limit vehicles and is primarily driven by fewer speed violations. Moreover, the beneficial impact of the one-year restriction period persists even after its expiration. Our findings highlight the importance of policies that, instead of directly targeting risky behaviours, are aimed at reducing exposure to high-risk settings. In frameworks where deterrence policies and screening mechanisms are hard to implement and maintain, these policies stand out as an effective, yet feasible strategy to increase teen road safety.

JEL Classification: D04; I12; I18; K32

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

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I Introduction

Motor vehicle traffic accidents are a leading cause of death and disability globally. Even in developed countries, vehicle accidents are responsible for one out in five 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 industrialised countries (ITF, 2018). Consequently, lowering the number of road traffic injuries and fatalities, especially among young drivers, has been placed at the top of policy agenda in all countries.

Improving young drivers' behaviour is a challenging goal. Teen drivers are the most inexperienced road users and show the highest propensity to engage in risky behaviours, such as drink-driving or excessive speeding (Anderson et al., 2013). The efficacy of deterrence policies in the form of monitoring, bans, and sanctions for risky driving behaviours depends on sustained enforcement (DeAngelo and Hansen, 2014) and is often short-lived (Abouk and Adams, 2013).[1] Moreover, in a context of asymmetric information where ex-ante ability and attitude to risky behaviours are hard to observe, simple gate-keeping mechanisms for selecting future drivers – such as driving license examinations – are likely to constitute an imperfect screening tool. Because of this, targeted restrictions and schemes providing gradual access to a full driving license – the so-called Graduated Driver Licensing (GDL) programs – have become increasingly popular, especially in the US and Australia.[2] Instead of directly targeting novice drivers based on their risky driving behaviour, these programs are designed to reduce drivers' exposure to high-risk settings while inexperienced, for example by limiting late-night driving or the carrying of peer passengers. The restrictions are progressively lifted as drivers gain experience. Although similar schemes have been found to reduce road accidents and fatalities, the evidence on the channel through which they work is scarce and mixed. Specifically, whether they effectively and permanently improve the driving behaviour of new licensees or whether they simply discourage young individuals from driving remains an open question.

[1]On the comparison between deterrence and control policies see also Kenkel (1993).

[2]In the latter case, the term Graduate Driver Licensing system (scheme) is sometimes used.

This paper studies the road safety effects of an Italian reform designed to reduce new drivers' exposure to specific high-risk setting. Introduced in 2011, the law restricts drivers from using high-performance vehicles during their first license year. Such vehicles include those whose engine power exceeds 70 kilowatts or whose power-to-weight ratio exceeds 55 kilowatts per ton.[3] To assess the impact of the power restriction on road safety, we combine unique and rich administrative Italian 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. We group the total number of accidents and the population of licensees in cells defined by commuting zone, gender, and age of the at-fault driver. The resulting pseudo-panel (with group sizes as weights) allows us to estimate the causal effect of undergoing the power limit on the likelihood of causing a traffic accident 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 on the between-cohorts differences in the exposure to the reform and providing a full dynamic specification of the effect.

We find that exposure to the vehicle power limit significantly lowers teen driver accidents throughout the entire post-reform period. Although a lower inflow into the pool of road users can partly explain the reduction in the number of accidents per capita (-18%) – the number of licenses issued post-reform drops by 19% – the reform also reduces the likelihood of teen licensees causing a severe traffic accident (-13%). Importantly, we show that the latter effect is entirely driven by a decrease in accidents caused by vehicles exceeding the maximum consented power. This finding confirms that our strategy identifies the causal effect of the power restriction rather than that of other (confounding) traffic safety policies. An effect decomposition analysis highlights that the power limit operates by discouraging risky driving behaviours. Nearly half (44%) of the overall effect is explained by fewer accidents due to excessive speed violations, although these are responsible for only about 25% of all teen accidents. Furthermore, the restriction is particularly

[3] In the paper, we provide direct evidence that the use of high-performance vehicles constitutes a high-risk factor for young drivers. Using (pre-reform) data on traffic accidents in Italy, in Section III we show the existence of a positive relationship between the likelihood of causing an accident and vehicle engine displacement for under-25 drivers. This view is also in line with the psychological literature, which highlighted that vehicle performance and drivers' risk-taking behaviour are positively related (see, e.g., Horswill and Coster, 2002). Furthermore, by considering high-performance vehicles that were subject to restrictions in Australia, Keall and Newstead (2013) estimate a 69% higher probability of crash involvement than is the case with lower power vehicles that were not subject to restrictions.

effective in lowering accident rates among male drivers, who exhibit the highest propensity to engage in risky driving behaviours. Finally, the reform's impact is long-lasting: drivers who underwent the one-year restriction exhibit lower accident rates even after the restriction has been lifted.

Our study contributes to the literature on the effectiveness of teen driving restrictions by showing that: (i) preventing fresh and inexperienced licensees from driving a powerful car effectively improves their driving behaviour, and (ii) this effect endures with license seniority. Several studies (Gilpin, 2019; Dee et al., 2005; Karaca-Mandic and Ridgeway, 2010) have shown that imposing limits on future drivers affects road safety through an "incapacitation channel" (i.e. by discouraging teens from obtaining a driving license). However, these studies find no evidence that these kinds of programs directly affect exposed new licensees, as we do in our study. To the best of our knowledge, the only evidence in this direction is provided by Moore and Morris (2020), who studied an Australian policy banning first-year drivers from carrying multiple passengers during night hours. A few distinctive features of the Italian driving restriction scheme can explain its effectiveness in lowering the number of road accidents among fresh licensees. First, the imposition of a power limit implicitly targets dangerous behaviour – speeding – which is the leading cause of injuries and deaths among teen drivers.[4] Second, unlike other similar programs, the restriction is tied to license seniority rather than age, which implies that the one-year probation period cannot be avoided by simply postponing licensing. Our finding that the beneficial effects of the restrictions do not diminish once the restriction period expires is indeed novel in the literature. We argue that two main mechanisms can explain this. On the one hand, being constrained to safety-enhancing restrictions while inexperienced might promote the formation of good driving habits (Moore and Morris, 2020). On the other hand, the existence of the power limit can direct car choices towards less-powerful vehicles, thus locking-in novice drivers in a low-risk setting. As long as car owners do not replace their vehicles in the short-term, the reform indirectly forces drivers to be paired with vehicles less suitable for spirited driving (e.g., excessive speed) for a long time horizon. Using a regression-discontinuity framework, we show that the reform indeed induces a change in car choices: the market of vehicles barely complying with the power limit increases post-reform, possibly at the

[4] As we document in Section V, excessive speeding alone is responsible for over a quarter of all severe teen accidents.

expense of higher-powered models.

More generally, this paper contributes to the vast literature on road safety policies using monetary and non-monetary incentive mechanisms to target unsafe driving. Many studies document the effectiveness of policies introducing penalty point systems (De Paola et al., 2013; Bourgeon and Picard, 2007), mandatory seat-belt wearing (Cohen and Einav, 2003; Carpenter and Stehr, 2008), texting bans (Abouk and Adams, 2013) as well as more complete reforms of traffic safety regulation (Aney and Ho, 2019).[5] Different types of public interventions have also tried to induce changes in driver behaviour through alcohol control policies, for example by banning late-night alcohol sales (Marcus and Siedler, 2015), reducing blood alcohol concentration (BAC) limits (Hansen, 2015), or even setting them to zero (Carpenter, 2004).[6]

Taken together, our findings highlight the importance of regulatory schemes acting on risk exposure. In frameworks characterised by individuals who are heterogeneous in their driving behaviour and skills, the optimal policy would be to target those who have the ex-ante highest risk attitude or the least developed driving skills.[7] However, when faced with asymmetric information, similar, first-best policies are difficult to implement, while (second-best) policies limiting drivers' exposure to high-risk settings may represent a viable and successful alternative. Our empirical results directly support this view. First, when discussing the (pre-reform) statistics on road accidents (Section III), we show that using high-performance vehicles represents a high-risk setting for novice drivers. While driving a more powerful car (i.e. a car with larger engine displacement) is not associated with a higher likelihood of senior drivers causing an accident, a positive relationship emerges when considering under-25 and especially teen drivers. Second, when decomposing the effect of reform depending on vehicle engine size, we show that accidents caused by drivers using relatively low-powered vehicles increased post-reform. We interpret this finding as indirect evidence of a positive sorting between driver type and vehicle power: reckless drivers – who, if allowed, would be paired

[5]While the majority of these studies focus on car crashes, French et al. (2009) analyzes the efficacy of different policies on motorcycle safety.

[6]For a review of the effects of reducing the consented BAC for driving, see also Burton et al. (2017).

[7]Drivers' heterogeneity based on their ability or risk attitude is well known in the literature. For example, Bourgeon and Picard (2007) propose a simple model where two types of drivers exist, "reckless" and "normal" drivers, and they are identified based on their effort to drive safely. The regulator cannot observe the driver's type.

with high-performance cars – have to switch to low-performance ones under the policy constraint, thereby raising the accident rates within this group of vehicles. However, forcibly decoupling risky drivers from their preferred car types makes incautious driving less likely, determining a negative net effect of vehicle power restrictions on accident rates. Once again, this confirms that restricting early driving to less powerful vehicles is an an effective, yet feasible strategy for enhancing road safety.

II Institutional Setting

In many countries, road safety policies use both monetary and regulatory incentive mechanisms to promote safe driving, which ultimately depends on the individual's effort to limit risk exposure and adopt careful driving behaviours. Italy is no exception, with its broad set of measures addressing road safety and preventing road traffic injuries and fatalities.

Italian traffic law is based on a penalty-based points system and on a two-stage driver licensing scheme. The penalty-based system shares many features with similar models implemented in other countries. Depending on the severity of their traffic violations, drivers start eroding their initial endowment of 20 points. Points deduction can be associated with monetary sanctions and even suspension or withdrawal of the license in the case of serious infringements.[8] The driver licensing system consists of 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 lasts up to six months – learners can take driving lessons and drive under the supervision of an accompanying person, but can carry passengers only in the daytime and on urban roads.[9] Passing the driving exams grants access to the full licensing phase, where these limits are lifted. However, new licensees are still subject to a specific regulation. Point

[8]The penalty-based system was established in 2003 (Law no. 214/03). On the effectiveness of its institution in Italy, see De Paola et al. (2013).

[9]The accompanying person must be younger than 65 and have at least ten years of driving experience.

deductions are doubled for traffic violations by drivers who have had a license for less than three years. Moreover, these drivers are subject to stricter speed limits, as they cannot exceed 90 and 100 km/h on extra-urban roads and motorways, respectively.

In 2010, Italy introduced a further package of regulations to incentivise safe driving and discourage risky behaviours by beginner drivers.[10] These measures were motivated by the alarming incidence of car crashes with injuries and fatalities among young drivers: data show that, in 2010, accident and mortality rates of under-21 drivers were more than a third higher compared with those of drivers aged 25 to 29 years.[11] In particular, the 2010 measures imposed tighter restrictions on novice drivers. Starting from February 9th, 2010, new licensees are subject to a vehicle power limit for the first twelve months: they are no longer allowed to drive vehicles with an engine power exceeding 70 kilowatts or with a power-to-weight ratio above 55 kilowatts per ton.[12] The introduction of the vehicle power limit marked a significant change in Italian traffic regulations. After its implementation, the Italian license system moved closer to the three-stage structure which is typical of the graduate driver licensing schemes widely used in US and Australia. These schemes involve a learner stage, where only supervised driving is allowed, an intermediate stage, where (typically) driving constraints apply on late-night driving and carrying peer passengers, and a full license stage where all restrictions are lifted (Gilpin, 2019).

It is worth stressing that 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 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.[13] Moreover, since July 2010 drivers with less than three years of license seniority faced a zero-tolerance policy

[10] More recent legislative innovations include the Road Homicide penalty system introduced in March 2016. However, Bruzzone et al. (2019) suggest its effects on road accidents are limited.

[11] Reported figures are from the *Rilevazione sugli Incidenti Stradali con Lesioni a Persone* (Istat).

[12] The law was announced on July 29, 2010 (Law No. 160/2010).

[13] 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 the 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 10 questions cover the remaining 10 topics.

on BAC. Such tighter limit substitutes the standard limit of 0.5 grams per litre of blood.[14] These two policies are thus contemporaneous to the introduction of the power restriction and could act as potential confounders.[15] In Section IV, where we discuss our identification strategy, we detail how we deal with the issue of confounding policies so as to pinpoint the effect of the power restriction.

III Data and Summary Statistics

III.a Data

The empirical analysis in this paper is based on a unique database built gathering together different types of administrative data. Our primary sources of data 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.[16] 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. Importantly, the data include information on the accident type (head-on, rear-end or side collisions, road departure, rollover) and on each driver's behaviour (e.g. excessive speed, stop sign or red traffic light running). Hence, for approximately 90% of the crash episodes we can identify a single at-fault driver as the person culpable of a traffic violation, as filed by the police.[17]

[14] The BAC consented levels have been already tightened by two reforms in 2007 and 2008.

[15] The 2010 reform also extended the learning phase to 17 year olds owning a permit to ride motorcycles, under the obligation of practicing for 10 hours with a professional instructor and taking some lessons at night and on non-urban roads.

[16] 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.

[17] When two at-fault drivers are present (6% of the cases), we consider the first – the one reported as "vehicle A" –

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 MIT reports on driving exams, finding little discrepancy between the two figures (see Appendix Figure A1).[18] [19]

We combine these two datasets into a (pseudo) balanced panel of road accidents and driving licenses by defining groups of observations that share the same common characteristics. As road accidents and the number of licensees are reported separately by gender, age, and municipality, we collapse the micro data into cells based on these dimensions. Specifically, we aggregate all observations into cells defined on two-year age groups (18-19, 20-21, 22-23, 24-25, 26-27), 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.[20] Our choice to aggregate the data by commuting zone, rather than municipalities, aims to minimise errors in the matching between (cells of) licensees and accidents. The Istat data on road accidents do not include the information on the municipality of residence of the drivers involved, but only the municipality where the accident occurred. Hence, we consider commuting zones – which are clusters of munici-

as the relevant one.

[18] The MIT reports include the number of successful written and driving tests, where the latter corresponds to the number of new licenses issued. We cannot directly use these data for our analysis as they do not include personal information on the test takers (age, gender, geographical area).

[19] 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 had not yet been renewed by May 2017 (Italian driving licenses usually expire in ten years) and thus are not included in the license census which covers all licenses active as of May 2017. 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.

[20] In the Appendix Table A3 we confirm the robustness of our main results by considering one-year age groups.

palities where individuals in the local labour force live, work and commute – to lower the chances of erroneously attributing road accidents occurring in a given area to individuals residing elsewhere. Our definition of commuting zones follows the Istat Italian Labour Market Areas (*Sistemi Locali del Lavoro*) which are based on the commuting matrices resulting from the 2011 Italian population census. In order to compute the number of accidents per capita and the proportion of licensees in the population we combine the resulting balanced panel with Istat's intercensal estimates on resident population, which are also available by gender, age and municipality.

Lastly, in Section VI, 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.[21] 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.[22]

III.b Summary Statistics on Road Accidents in Italy

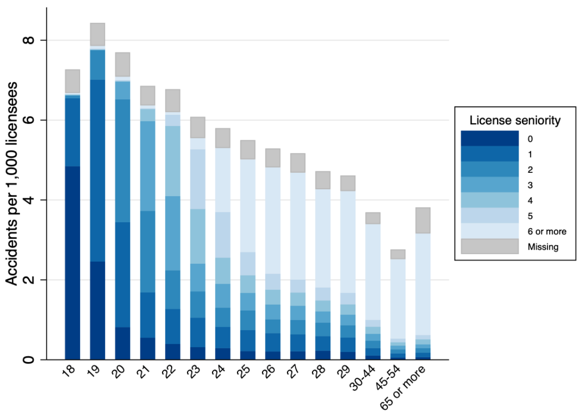
When the reform was introduced, in 2010, teen road safety was still an issue of great concern in Italy. Over the previous decade, teen drivers were responsible for nearly 60 thousand severe road accidents, resulting in more than 100,000 injuries and 1,700 fatalities. Figure I reports the number of accidents (Panel A) and deaths (Panel B) per 1,000 drivers separately by driver age. Young drivers exhibit the highest values. Compared to those aged 30 to 44 years, teen drivers are 2.2 times more likely to cause a severe accident and 3.1 times more likely to cause a fatal accident.[23] In principle, these numbers can be partly explained by a lack of driving experience. However, decomposing the accident rate by type of traffic violation (Panel C) reveals that risky driving behaviours are a major determinant of teen crashes. Excessive speeding alone accounts for a quarter of all the accidents and nearly half of all the deaths (45%) caused by 18 and 19 year olds.

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

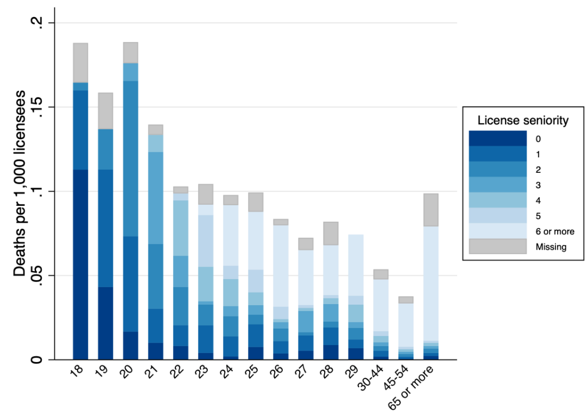
[22] Source: https://www.quattroruote.it/archivio/listino/.

[23] The number of accidents (deaths) caused by 18-19 years old per 1,000 licensees is 8.04 (.17), while that by 30-44 is 3.69 (.05).

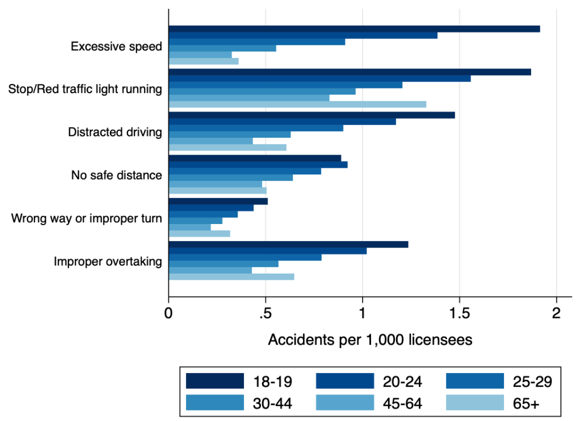
Figure I: Summary Statistics



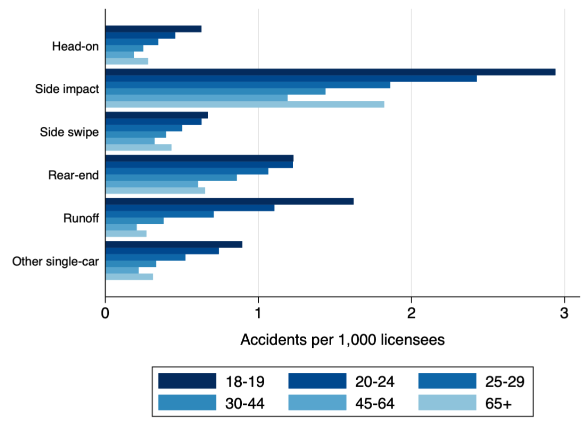
a. Accident rate



b. Death rate



c. Accident rate, by traffic violation



d. Accident rate, by collision type

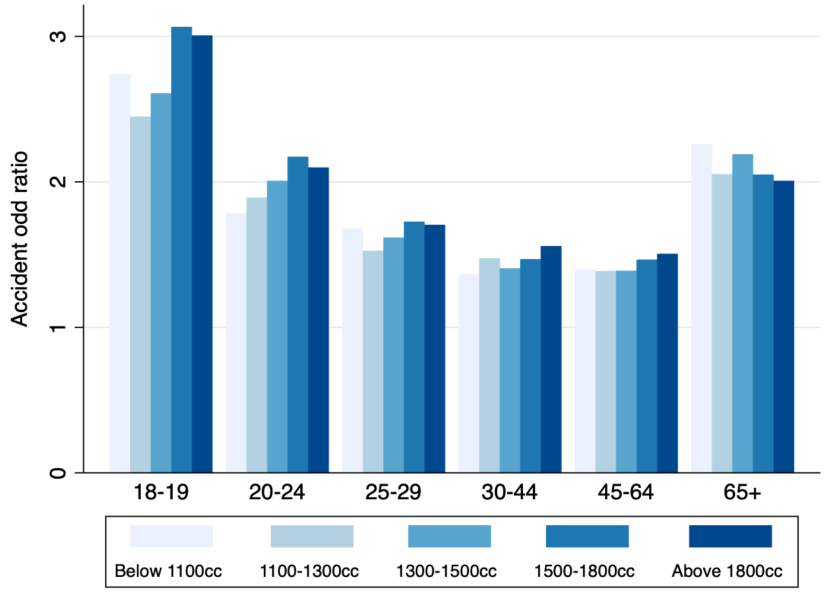
NOTES. These figures depict accident rates in Italy by age and license seniority of the at-fault driver (Panel A and B), traffic violation category (Panel C), and collision type (Panel D). In all panels, bars indicate the number of accidents per 1,000 licensees. Statistics are relative to the year 2010.

Furthermore, as shown in Panel D, single-car accidents such as run-off-road collisions are much more common among young (20% of all accidents) than older drivers (5% in the age group 30-44).

In Figure II, we also explore the relationship between vehicle power and the likelihood of a road accident before the power limit was introduced (2010). Specifically, each vertical bar indicates the ratio $\frac{At-fault_{ak}}{Not-at-fault_{ak}}$, that is, the ratio between the number of accidents caused by a driver of age a who drives a vehicle with engine size k, and the number of accidents involving a not-at-fault driver-car pair ak.[24] Dividing the number of accidents by the term $Not-at-fault_{ak}$ allows us to

[24]In line with the definition of at-fault drivers, not-at-fault drivers are those who, according to the police report,

Figure II: Accident risk, by engine displacement



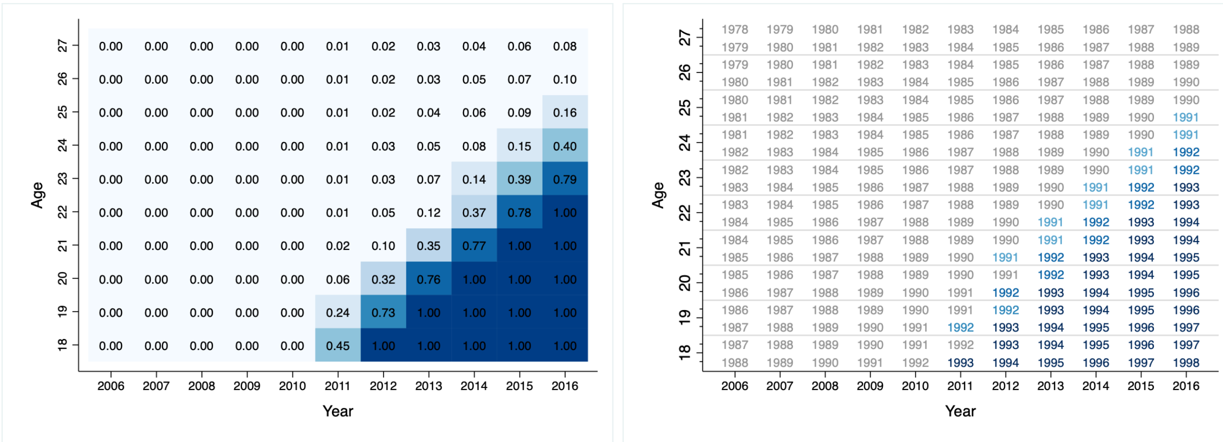
NOTES. This figure depicts the relationship between vehicle power and the likelihood of a causing a road accident by age group of the at-fault driver. For each age group (a) and engine size (k), vertical bars indicate the ratio between the number of accidents caused by driver-car pairs ak and the number of accidents involving – but not caused by – driver-car pairs ak (that is, accidents where the driver-car pair ak is not at-fault. The sample is limited to accidents for which the information on the car engine size is available. Statistics are relative to the year 2010.

account for the fact that younger drivers were less likely to drive a high-powered vehicle even before the introduction of the power restriction. In this sense, the denominator should approximate the number of car rides by age and vehicle power, a measure which is barely observed. The patterns depicted in Figure I highlight that while the relationship between vehicle power and accident risk is mostly flat for older age groups, it has a positive slope for young or inexperienced drivers. For teen drivers in particular (given that they already exhibit the highest accident rates), driving a high-powered car is associated with a higher likelihood of causing an accident and thus potentially represents a high-risk setting.

committed no violation.

IV Identification Strategy

Figure III: Post-reform Licensees



a. Proportion of post-reform licensees

b. Post-reform cohorts

NOTES. This figure depicts the proportion of post-reform licensees (Panel A) and their birth cohort (Panel B). Specifically, each number in Panel A indicates the proportion of licensees of age j (on the y-axis) in year t (on the x-axis) 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 (on the y-axis) observed in year t (on the x-axis).

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 III 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). The new regulation was introduced in February 2011. This implies that, starting from that date, cells 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.[25] From 2012 onwards, this share goes up to 100%, because the 18-years-old cell consists exclusively of cohorts reaching the minimum eligibility age in 2011 or later. Conversely, cells identifying older ages are mostly populated by unexposed cohorts throughout the whole period considered. Even in 2016, only a small share (8 to 10 percent) of licensees aged 26 or 27 years got their license before the

[25] 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.

enforcement of the power restriction, consistent with the average age for license acquisition being 19.2 years. For this reason, we consider these age classes an ideal control group.

To implement the above-described identification strategy, we use a Difference-in-Differences setup, where we compare the evolution of road accidents caused by drivers in different age classes before and after the introduction of the vehicle power restriction. Specifically, we estimate variants of the following linear equation:

$$Y_{ajst} = \alpha + \sum_{t=2006}^{2016} \beta_t Age_a^{1819} \times d_t + \delta T_{jt} + \gamma X_{asj} + \nu_{ajst} \tag{1}$$

In the above equation, Y_{ajst} is the cell-specific accident rate in year t, defined either as (i) $\frac{Accidents_{ajst}}{Population_{ajst}}$ – the number of accidents per one thousand inhabitants – or (ii) $\frac{Accidents_{ajst}}{Drivers_{ajst}}$ – the number of accident per one thousand licensees. We define cells based on age (a), gender (s), commuting zone (CZ) j, and year (t). Equation (1) includes a vector of year×CZ fixed effects (T_{jt}), which account for shocks affecting asymmetrically different commuting zones, a gender dummy and age-group×CZ fixed-effects (the vector X_{asj}). Age_a^{1819} is a binary indicator taking the value of 1 for the 18-19 age group and 0 for the 26-27 age group.[26] The term d_t is a vector of year dummies. From 2011 onwards, the 18-19 age group is progressively populated by individuals belonging to fully-exposed cohorts. Hence, our coefficients of interest are β_t for $t \geq 2011$.

We estimate weighted regression models where the weights are the number of observations in each cell, that is, the denominator of the Y_{ajst} ratio. Weighting by cell size allows us to interpret each β_t coefficient as the effect of the reform on the individual likelihood of causing an accident. Depending on whether the outcome is defined according to (i) or (ii), the estimated DiD coefficients β_t capture the change in accident probability in the resident population or in the sub-population of licensees, respectively.[27] Besides providing a fully dynamic specification, Equation (1) also enables us to test for the existence of diverging patterns of road accidents between our treatment and

[26] In Appendix Table A3 we also estimate Equation 1 varying the composition of the treatment and control group.

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

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 II, we have detailed which other policies have been implemented in the same period of, or shortly before, the vehicle power restriction. 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 the 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. Appendix Figure A5 depicts the relationship between engine size and engine power, based on the Italian vehicle census (May 2017).[28] The vast majority (from 70 to 100%) of cars whose engine size is larger than 1,500cc exceed the 70-kilowatt power restriction. This share is much lower for cars with an engine size below 1,500 cc, and it is close to zero for engines below 1,300cc.

Hence, we further split accidents in cells based on the engine size of the at-fault driver$\times$car pair and we estimate Equation 1 separately for accidents caused by vehicles with different engine size. Specifically, the outcome of interest is $\frac{Accidents_{ajkst}}{Drivers_{ajst}}$, where k indicates the lower limit of each of the K engine size groups, and where $\sum_{k=0}^{K} \frac{Accidents_{ajkst}}{Drivers_{ajst}} = \frac{Accidents_{ajst}}{Drivers_{ajst}}$. As the reform restrict 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 $k \geq 1,500$. Conversely, if either the zero tolerance law or the written driving test reform constitute a significant confounder – that is, if they play a part in reducing teen accidents – we would expect the estimated DiD coefficients β_t to be negative and significant even in the subsample of accidents for which $k < 1,500$.[29]

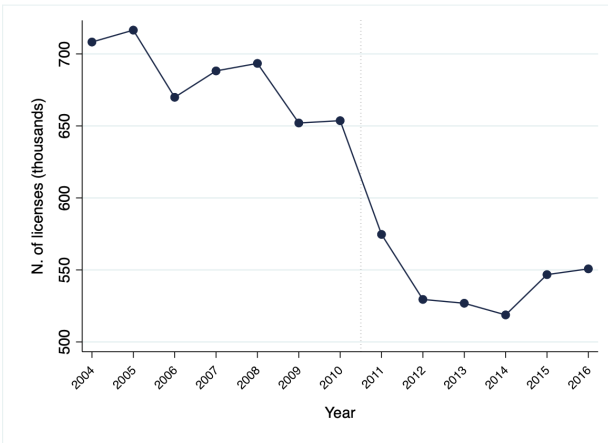
[28] We obtain a nearly identical figure if we use the *Quattroruote* car model database, which includes all car models available in the Italian market in each year.

[29] In principle, we could limit the sample of accidents caused by above-limit vehicles throughout the whole analysis. However, this would lead to a substantial loss in terms of sample size, as the information about vehicles' engine

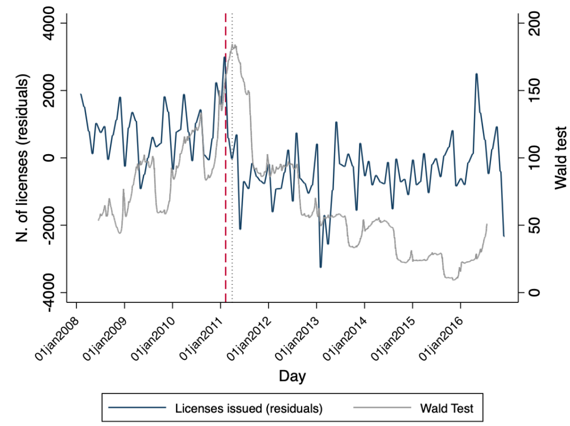
V Results

V.a Reform Effect on New Licensees

Figure IV: Reform Effect on the Number of New Licensees



a. Raw data



b. Structural break test

Notes. These figures depict how the number of driving licenses issued vary before and after the reform. Dots in Panel A indicate the number of individuals (in thousands of people) who obtained a driving license in each year over the period 2004-2016. The darker line in Panel B indicates the deseasonalised residuals from a OLS regression of the number of driving licenses issued every week on week-of-the year fixed-effects. The lighter line indicates the statistics from a Wald test of whether the coefficients from the above-mentioned regression vary over the periods defined by an unknown break date. The dashed vertical line indicates the date of the introduction of the power limit (9 February 2011), while the dotted vertical line indicates the estimated break date (31 March 2011).

Our analysis of the impact of the vehicle power limit starts by investigating its effect on the number of driving licenses issued. Restrictions on young drivers are found to lower teens' propensity to obtain a driving license (Gilpin, 2019). Similarly, the introduction of 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.

Panel A of Figure IV shows that the number of new licensees dropped substantially in the post-reform period. It fell by about 79,000 units (-12%) in 2011 and by 45,000 in 2012, with an overall reduction of more than 19% compared to 2010. In Panel B, we plot the deseasonalised residuals from regressing the number of licenses issued weekly on week-of-the-year fixed effects. The weekly time series highlight that the drop in the number of new licenses occurs exactly around the date of

size is available only for 60% of the accidents.

the introduction of the power restriction. This visual impression is confirmed when performing a Wald test for a structural break, which delivers March 31st, 2011, as the estimated break date. In Figure A2 (in Appendix), 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 vanishing by the age of 24.[30] Thus, these numbers reveal a tendency to postpone licensing rather than a permanent reduction in the number of drivers.

In principle, the introduction in January 2011 of the new written driving test could also explain the reduction in the number of teen licensees. The driving test trends reported in Appendix Figure A3 (Panel A) reveal that this reduction is driven by fewer test takers rather than by a higher failure rate in the written test. Still, our data do not allow to disentangle the impact of the two reforms, as the new test could have discouraged teens from applying to a driving license. Pass rates have been rising after 2011 (as shown in Panel B) and this might be suggestive of a possible change in the composition of test-takers. In the next section, we detail how we deal with the shrinking number of teen drivers, and we provide direct evidence that possible changes in the composition of teen licensees are not a threat to our identification of the effect of the power limit.

V.b Reform Effect on Road Accidents

Table I 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 -0.81 episodes per 1,000 inhabitants and -1.02 episodes per 1,000

[30] The percentage of 24-years-old with a license in 2015 is nearly identical to that of 24-years old in 2016. The former reached the minimum driving age in the pre-reform period (2010), while the latter is the first fully-exposed cohort.

Table I: Reform Effect on Road Accidents

	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***
	(0.120)	(0.118)	(0.007)	(0.132)	(0.127)	(0.012)
Post	-1.145***			-1.303***		
	(0.064)			(0.065)		
Age 18-19	-0.123			2.668***		
	(0.100)			(0.111)		
Female	-2.895***	-2.896***	-0.089***	-3.956***	-3.969***	-0.128***
	(0.058)	(0.055)	(0.003)	(0.058)	(0.055)	(0.005)
CZ × Age group FE	No	Yes	Yes	No	Yes	Yes
CZ × Year FE	No	Yes	Yes	No	Yes	Yes
Baseline average	4.366	4.366	0.097	8.343	8.343	0.186
R^2	0.374	0.481	0.152	0.391	0.499	0.150
Observations	39192	39192	39192	39192	39192	39192

NOTES. This table reports the effect of being subject to the power restriction on the likelihood of causing 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 caused by drivers 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). Robust standard errors in parentheses. *** p< 0.01, ** p<0.05, and *p<0.10.

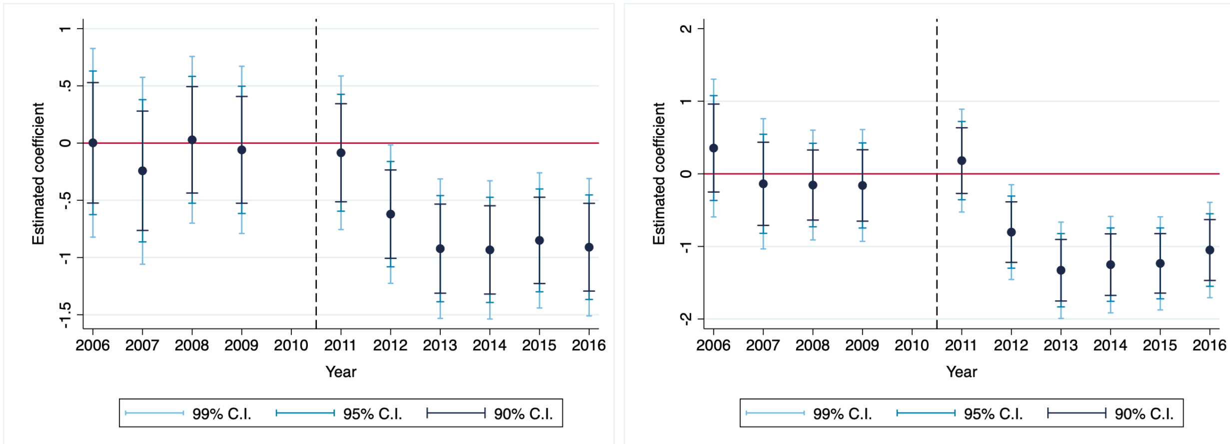
drivers under our preferred specification (Columns 2-3 and 5-6), which includes both CZ×age-groups fixed effects and CZ×year fixed effects. Importantly, this reduction also translates into a lower number of fatal accidents, which diminish by 0.03 and 0.05 episodes in per capita and per licensee terms, respectively. These effects are economically meaningful. Accidents per licensee drop by 13% when compared to the corresponding baseline average, while fatal accidents by 28%.[31] Back-of-the-envelope calculations – which are illustrated in Appendix Figure A4 – suggest that from its introduction to 2016 the power limit prevented teen drivers from causing about 6,300 injuries and deaths, a large share of which (37%) are occupants of other (not-at-fault) vehicles and pedestrians.[32]

The estimates presented in Columns 1-3 of Table I capture the effect of the reform on the

[31] 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.

[32] We calculate the number of saved injuries and deaths as follow: *i)* we estimate a set of regressions of the form of Equation 1 where the dependent variable is the total number of injuries and deaths (in the at-fault car, in other vehicles, or total) caused by licensees of a specific cell; *ii)* we multiply each of the estimated interaction coefficient $Age18-19 \times d_t$ by the total number of teen licensees in the corresponding year t; *iii)* we aggregate the resulting (yearly) figures over the whole post-reform period considered (2011-2016). The estimates from the regressions mentioned in *i)* are reported in Appendix Table A2.

Figure V: Reform Effect on Road Accidents



a. Accidents per capita

b. Accidents per licensee

NOTES. These figures depict the effect of the power restriction on the likelihood of teen drivers causing a traffic accident. 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 1. 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 in a cell – defined based on 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. In both panels, 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. Vertical spikes indicate robust confidence intervals at the 90%, 95%, and 99% level.

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 in the above paragraphs, the inflow of new drivers shrinks post-reform, and thus the number of potential road crashers in the treated group drops. This result is in line with the evidence provided by Gilpin (2019), who shows that GDL programs improve road safety by discouraging teens from driving.[33] 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 has a direct effect on the likelihood that teen drivers cause a traffic accident.

Figure V 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 all the interaction coefficients β_t resulting from estimating Equation 1, where the pre-reform year 2010 is

[33] A few other studies document how a reduction in the number of road users – following changes in the supply of public transport – affects the road accidents rate (Lichtman-Sadot, 2019) and (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.

the omitted term.[34] The corresponding estimates, along with their standard errors and regression statistics, are reported in the Appendix Table A1. Both panels of Figure V 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 – as documented in Figure III – 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).[35] Results presented in Figure V also provide 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 Equation (1) separately for vehicles likely complying and not complying with the power limit. We do not observe in our data the vehicle engine power (in kw), but only its engine size (in cc). We thus split road crashes into two groups, depending on whether they are caused by a vehicle with an engine size below or above above 1,500 cc.[36] If the vehicle power limit is the sole driver of the DiD estimates presented in Table I and Figure V, we expect that this effect is entirely driven by the fewer crashes caused by vehicles with non-complying engines.

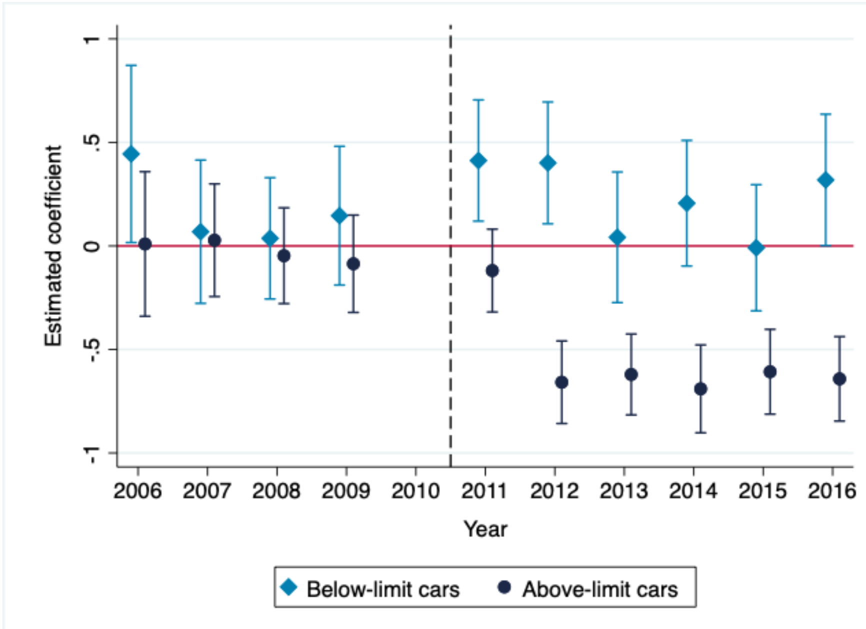
Figure VI shows that this is indeed the case. The post-policy interaction coefficients are negative and significant only when limiting the analysis to accidents caused by vehicles exceeding the power limit. Conversely, we observe a zero or marginally positive effect on the probability of crashes by

[34] We obtain nearly identical results when excluding any alternative pre-reform year (2008 or 2009).

[35] In Table A3 in the 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-inflation: only in 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.

[36] Appendix Figure A5 depicts the relationship between engine size and engine power, and show that the vast majority of car models with an engine larger than 1,500cc also exceed the 70kw limit.

Figure VI: Reform Effect by Engine Displacement



NOTES. This figure depicts the effect of the power restriction on the likelihood of teen drivers causing a traffic accident separately for vehicles below and above the power limit. 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 1. Regressions are estimated separately depending on the engine size of the at-fault car. 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. Darker dots indicate cars with an engine size above 1,500cc, while lighter dots indicate cars with an engine size below 1,500cc. In both regressions, the interaction term relative to the pre-reform year (Age 18-19 × year 2010) is the omitted term. 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. Vertical spikes indicate robust confidence intervals at the 95% level.

vehicles with a complying engine, a result that clashes with other policies or confounders explaining our results. Appendix Table A4 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).[37] The estimates in Figure VI also suggest that the likelihood of causing a crash when driving a relatively low-powered car increases post-reform. We interpret this finding as evidence that the power restriction affects the vehicle-driver pairing. As driving high-performance vehicles is no longer possible, novice drivers who would have driven

[37] In this table, the horizontal sum of the coefficients in Columns 2 to 7 is equal to the estimated effect for the whole sample of accidents, that we report in Column 1 for the sake of comparison. The coefficients in Column 7 are relative to accidents caused by cars with unknown engine size. Since this subgroup likely includes also accidents caused by high-power vehicles, the estimates are negative and significant for the post-reform years.

such cars – and are unwilling to wait for the restriction to be lifted – have to switch to low-powered cars. If high-performance car users are also riskier drivers – that is, if vehicle power and individual risk are positively correlated – their inflow into the pool of low-powered car users would result in an increased accident rate in this group.

Importantly, this result alleviates the concern that the observed decrease in teen accidents could be driven by a change in the composition of novice drivers due to the reform of the license written test. In Section V.a we show that the number of licenses issued shrinks from early 2011, which is the date when both the power limit and the new test were implemented. In our main specification, we account for the lower inflow of teen licensees by defining the outcome as the number of accidents per licensee. Still, the new test could have changed the composition of the pool of (successful) test takers – for instance, by discouraging riskier individuals from applying to a driving license – thus hampering the interpretation of our findings. However, this hypothesis is hardly compatible with the evidence that only accidents caused by drivers using above-limit cars decreased post-reform. If the new test helped selecting safer drivers, this would have translated into a lower likelihood of causing an accident regardless of the vehicle power, or at least not specific to high-powered cars.

V.c Effect Heterogeneity and Decomposition by Accident Characteristics

In this section, we decompose the effect of the reform by accident category. The rich set of accident characteristics included in the Istat microdata enables us to classify accidents based on the time, the day of the week, the location (urban area, non-urban area, highway), the collision dynamics, and the driving behaviour of each driver. We explore which accident categories are the most affected by the vehicle power limit by estimating a set of regressions of the form of Equation (1), where the dependent variable is $\frac{Accidents_{ajtc}}{Drivers_{ajt}}$ and c is the accident category. Results are reported in Table II. The horizontal sum of the coefficients is equal to the overall effect because the c categories are mutually exclusive.[38]

[38]For a limited sample of accidents, information on time, day, or location is not available, which explains possible (small) inconsistencies between columns.

Table II: Reform Effect on Accidents per Licensee by Accident Category

Panel A: By Accident Type								
	All	Multi-car					Single-car	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
		Head-on	Side impact	Side swipe	Rear-end	Pedestrian	Runoff	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
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)	(3)	(4)	(5)	(6)	(7)	(8)
		Excessive speed	Stop/Traffic light viol.	No safe distance	Wrong way/impr. turn	Impr. overtaking	Distracted driving	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
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)	(3)	(4)	(5)	(6)	(7)	(8)
		Day	Night	Weekday	Weekend	Urban	Extra-urban	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
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

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 category c 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). Robust standard errors in parentheses. *** p< 0.01, ** p<0.05, and *p<0.10.

In Panel A of Table II, we estimate the effect of the reform on a licensee's probability of causing a multi-car *versus* single-car crash. More than 40% of the overall effect is driven by a reduction in accidents involving a single vehicle only. Overall, compared to the baseline average, the power restriction mainly lowers run-off (-22%) and head-on collisions (-13%). These accident types are the the potentially most severe ones. They exhibit the highest fatality rates, being, on average, three to four times more likely to result in deaths than side-impact or rear-end collisions.[39]

In Panel B, we focus on the behaviour of the at-fault driver. We group possible drivers violations into seven categories: excessive speed, stop or red traffic light running, failure to keep safe-following distance, wrong way or improper turn violations, improper overtaking, distracted driving, and other violations. Nearly half (44%) of the overall effect is due to a reduction in the number of accidents caused by excessive speed. Compared to the baseline average, these accidents decreased by over a fifth. Even this finding is consistent with our interpretation of the effects of the reform: the power limit has hampered hazardous driving behaviours, which are typically tied to the availability of high-performance vehicles.

Finally, in Panel C, we estimate the effect of the reform on a licensee's probability of causing night or day, weekday or week-end, urban or non-urban accidents. The post-reform interaction coefficients is negative and significant in all columns, thus confirming that the power restriction significantly reduced all these types of accidents. In relative terms – that is, if compared with the baseline average – the drop in accidents occurring at night (-25%) and on the week-ends (-18.5%) is moderately larger, which might be again consistent with the power restriction limiting dangerous driving behaviours such as excessive speed.[40]

In Appendix Table A5, we also explore the heterogeneity of the reform effect by estimating a set of regressions where the term $Post \times Age^{18-19}$ is interacted with a gender dummy (Column 1) and different CZ characteristics (Columns 2-5). We find that, in absolute terms, the restriction

[39] In Italy, the fatality rates of head-on and run-off-road collisions, computed over the period 2006-2010, are 0.043 and 0.041, respectively. These figures are much higher than those of less-severe accidents, such as side-impact (0.014) and rear-end collisions (0.011).

[40] Over 22% of all accidents due to excessive speed occur at night, a figure which is much higher than is observed for other types of violations (in 2010, 15% of all severe accidents occurred at night.)

lowers accident rates more among male teen than female drivers. However, at baseline, the number of accidents per licensee is two times higher among male teen drivers (10.59) than females (4.75), which, in relative terms, makes the effects similar across the two groups. When investigating the geographical heterogeneity, we find the impact of the reform to be more pronounced in urban, more populated, and wealthier areas, although the triple-interaction coefficients are not always significant.

V.d Persistence of the Effect

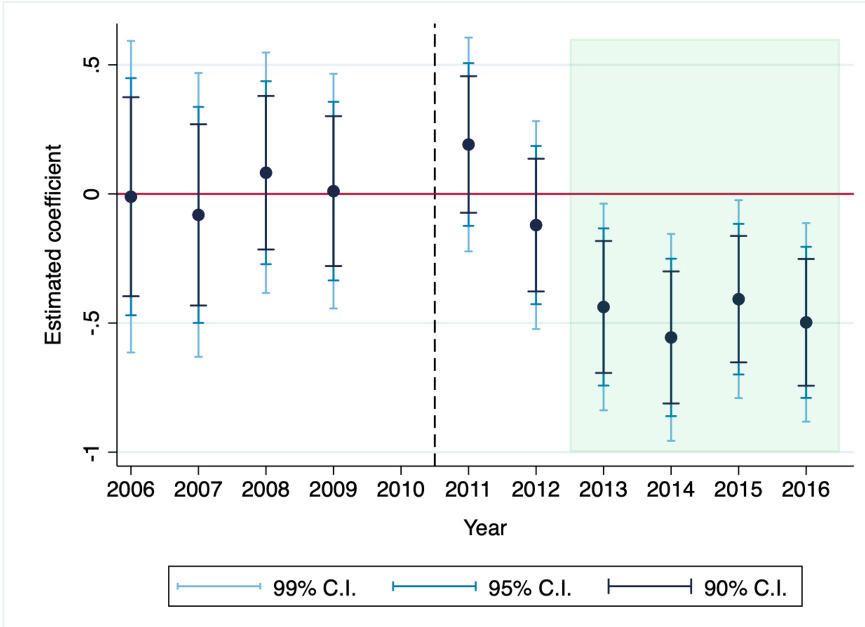
A potential challenge to the usefulness of GDL programs, which establish a staged approach to driver licensing, is whether their beneficial effects persist once the temporary bans have been removed. Thus, it is worth assessing whether licensees who underwent the power limit during their first license year exhibit lower accident rates even after it 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 Equation (1), 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.[41] 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 license in 2011 or later. The estimated coefficients from this regression are depicted in Figure VII. 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 Appendix Table A6, 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

[41] 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 VII: Reform Effect on Road Accidents by Unrestricted Drivers



Notes. This figure depicts the effect of the power restriction on the likelihood of teen drivers causing a traffic accident after the power restriction expires. Dots indicate the estimated interaction coefficients $Age21 - 21 \times d_t$ – for different values of t, reported on the x-axis – from regression models of the form of Equation 1, where treated units are those drivers aged 20-21 years. The dependent variable is defined as the number of accidents caused by drivers in a cell – defined based on age, gender, commuting zone, and year – per 1,000 licensees of the corresponding age, gender, commuting zone, and year cell. In both regressions, the interaction term relative to the pre-reform year ($Age21 - 21 \times d_{2010}$) is the omitted term. The sample includes only drivers with two years of license seniority or more. 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. Vertical spikes indicate robust confidence intervals at the 99%, 95%, and 90% level.

two years.[42] Estimates reveal that the power restriction reduces the likelihood of traffic accidents occurring as well as accidents due to excessive speeding each year over the period 2013-2016 for 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.

[42]In all columns, the control units are those belonging to the age group 26-27.

These results also highlight that the reform is effective in reducing traffic accidents even in the presence of strategic behaviour. In principle, new licensees could respond to the power limit by delaying car use for the first license year, waiting for the restriction period to expire. If this was the case, the presence of a one-year power limit could simply have the effect of delaying novice drivers' accidents until they access the unrestricted regime. However, this hypothesis is hard to reconcile with the observed lasting effects of the regulation.

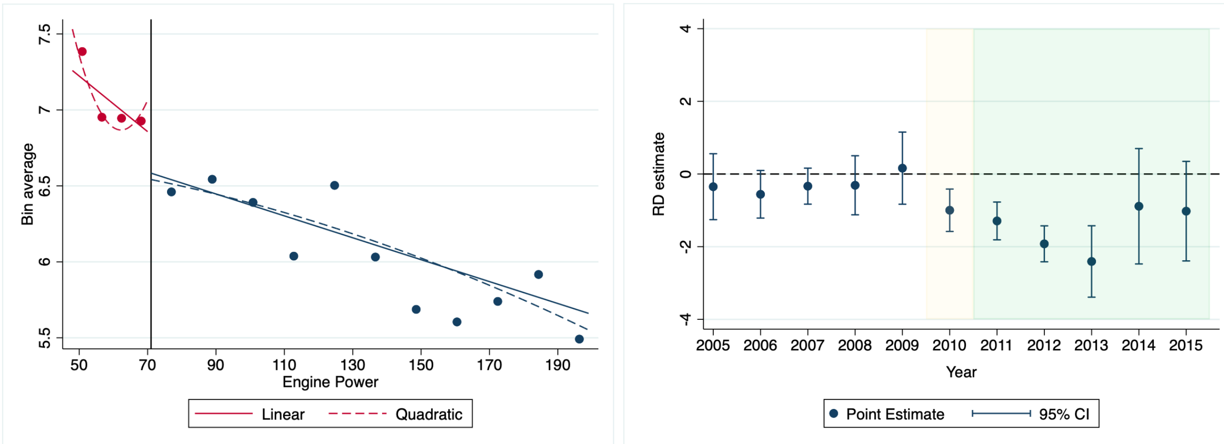
VI Discussion and Mechanism

Two main mechanisms are likely to have concurred to affect drivers' behaviour over a period longer than the one-year power limit itself. First, being constrained within a low-risk setting could encourage enduring virtuous driving habits among young drivers. The role played by legal regimes or regulatory policies on habit formation and their consequent long-run effects is a common (and debated) issue in various contexts. For instance, Kaestner and Yarnoff (2011) highlight a close link between the exposure to different drinking-age regimes while young and later in life alcohol consumption and traffic fatalities. Similarly, Williams (2005) shows that students who face stricter drunk driving laws while in secondary school tend to consume less alcohol even when in college.[43] Applied to driving behaviour, this interpretation would be in line with that of Moore and Morris (2020), who also find that the effect of a one-year ban for 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 could incentivise the choice of low-powered cars for fresh licensees. Under this second hypothesis, the long-lasting impact of the short-lived restriction would be explained by drivers remaining under a low-risk setting – a less powerful car – even after the restriction is lifted, as typically cars are chosen on a long-time horizon.

While we cannot disentangle the two mechanisms, we provide evidence that the reform does

[43] On the role of stringent policies to ensure the establishment of good standards of behaviour, see also the study of Viscusi et al. (2011) on bottle recycling in the US.

Figure VIII: Power Limit Effect on Car Sales



a. Car sales post-reform

b. RD estimates by year

Notes. These figures show how average car sales vary based on the model engine power. In Panel A, markers indicate the average number of car sales (in logarithm) in the post-reform period 2011-2014 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). Panel B depicts the estimated coefficients from a set of RD regressions, where the dependent variable is the logarithm of car sales in a given year – reported on the x-axis – and the running variable is the distance (in kw) from the 70kw power threshold. Each dot indicates the bias-corrected regression-discontinuity estimate obtained using the robust estimator proposed in Calonico et al. (2014). Vertical spikes indicate confidence intervals at the 95% level.

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, i.e., whether the sales of (barely) complying car models boost compared to sales of (barely) above-limit ones. The two panels of Figure VIII summarise the result of our regression-discontinuity (RD) exercise. Each circle in Panel A represents average sales (in logarithms) of car models in each engine-size bin in the post-reform period. The solid and the dashed line are the prediction respectively from a linear and quadratic regression of the logarithm of car sales on engine power (in kilowatts), computed separately for the group of car models satisfying (left-hand-side) and not satisfying (right-hand-side) the power restriction. Hence, the vertical distance between the left-hand-side and the right-hand-side intercepts in each graph represents the effect of crossing the engine power threshold on car sales.

We can see that a negative and discontinuous jump emerges, thus confirming that the power restriction boosted sales of car models satisfying the kilowatt threshold, possibly at the expense of models with larger engine size.[44] The robustness of this result is confirmed by the evidence presented

[44] Appendix Figure A6 is the 2006-2009 analog of the evidence presented in Panel A of Figure VIII, and shows that

in Panel B, where we plot the RD coefficients, alongside their confidence intervals, separately for each year. The estimated coefficients are small in magnitude and not statistically different from zero throughout the whole period 2005-2009. On the contrary, after the power restriction was announced (July 2010), the estimated discontinuity becomes negative and statistically significant at the 99% confidence level. Therefore, the new regulation induced a sharp change in the Italian car market. To the extent that less powerful cars are both an illiquid asset and a means of transport less suitable for risky driving, this composition effect may partly explain the long-lasting reduction in teen drivers' accident rates.

VII Conclusions

This paper studies the impact of vehicle power restrictions affecting novice licensees on traffic accidents. We use Italian data on road accidents with injuries and fatalities from 2006 to 2016, combined with the driving permit census, to estimate the causal effects of a reform which constrains first-year licensees to use cars not exceeding specific power thresholds. We find that the power restriction lowers road accidents per capita among individuals aged 18-19 by approximately 18%. This result is partly due to an incapacitation effect: driving license rates drop following the reform. However, we also find that teen drivers subject to the regulation are significantly less likely to cause a road accident (-13%) and a fatal accident (-28%), and that such reduction is entirely driven by fewer accidents caused by above-limit engines. Back-of-the-envelope calculations suggest that the power limit has prevented above 1,200 injuries and deaths a year, leading to 6,300 less accident victims in the five years after its introduction.

Our results show that the reform successfully limits young drivers' exposure to a high-risk circumstance of driving a high-powered car when young and inexperienced. Consistent with this factor, we find that over 44% of the effect is the result of fewer accidents caused by excessive speeding. We thus argue that banning high-performance vehicles can promote lower-risk settings

no discontinuity emerges in the pre-reform period.

in two ways. First, less powerful cars have most likely resulted in less risky driving *per se*, merely because of limited speed capability. Second, the power restriction quite probably affected reckless drivers' vehicle choices by making cars more suitable for spirited driving unavailable to them, thereby thus, in turn, lowering their utility from hazardous driving and speeding. Loosely speaking, as also revealed by the models' names, driving a FIAT *Panda* is likely to provide a somewhat different sensations than driving a *Jaguar*!

Furthermore, our study shows that the vehicle power restriction on novice drivers, although lasting only one year, has persistent positive effects on road safety. This finding is particularly relevant if we consider that similar legislative measures generally have a short-lived impact. A plausible explanation is that the has led to virtuous habit formation, thus conserving its effect even when young drivers become entitled to drive high-performance cars. However, in-depth scrutiny of the data at hand discloses a more tangible effect yielding a basic (though effective) hysteresis mechanism. Our RD analysis shows that the new legislation boosts sales for car models satisfying the kilowatts threshold. Because cars are non-durable goods, mostly used for longer than one year, the increased sales of cars complying with the power limit mechanically lengthens the effect of the restriction, notably until the car owner becomes a more experienced driver.

Overall, our findings highlight the need to control how future drivers join the pool of road users. We show that a gradual phased-in entitlement of licensees to unrestricted use of vehicles can be effective in preventing traffic-related injuries and deaths among young drivers. Still, these policies have received little attention, especially in European countries, with most efforts directed towards interventions targeting the hazardous driving behaviours of already-licensed drivers. More generally, we emphasise the importance of strategically limiting young drivers' exposure to high-risk settings, especially when their risk type and skills are hard to observe, and risk-targeted screening strategies are challenging to implement.

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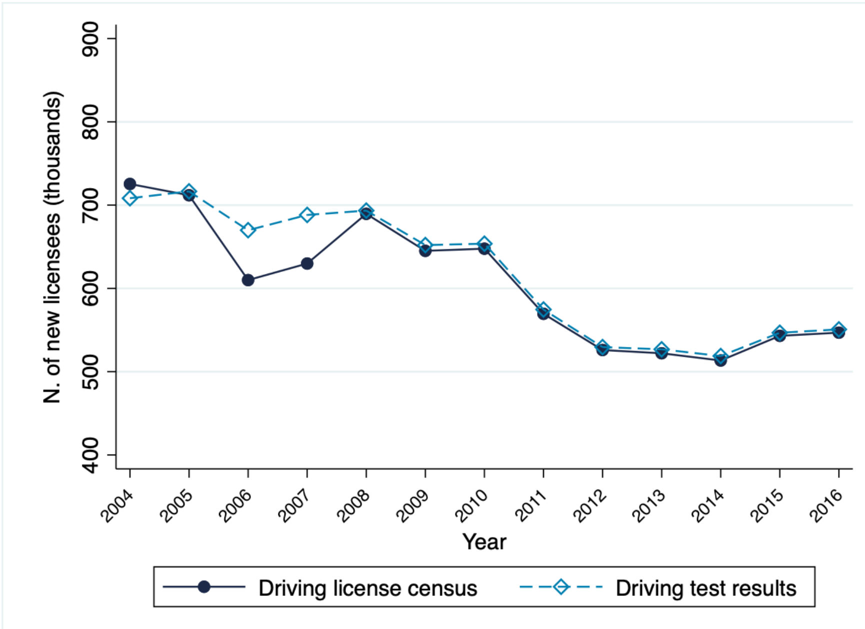
Appendix

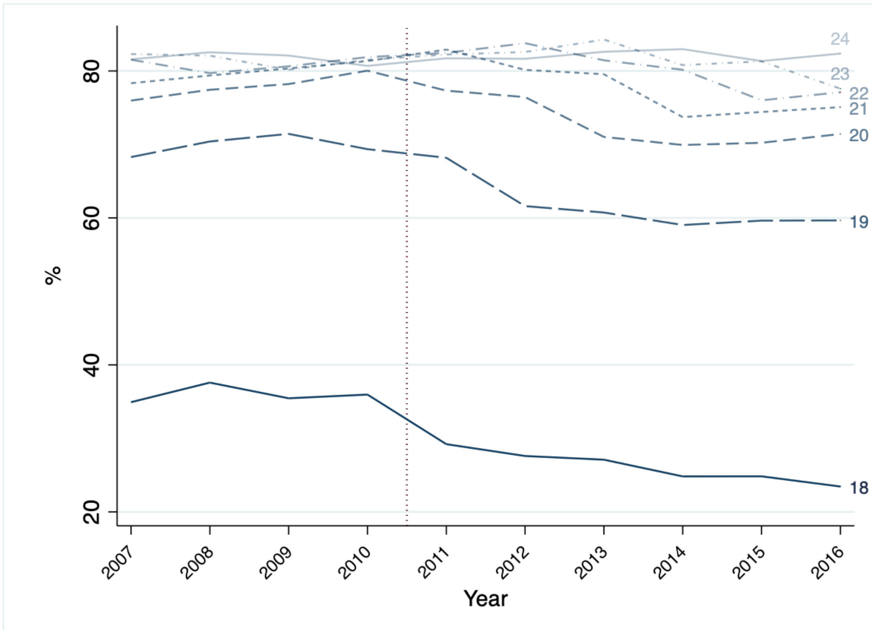
A.1 Figures

Figure A1: Coverage of the License Census Database



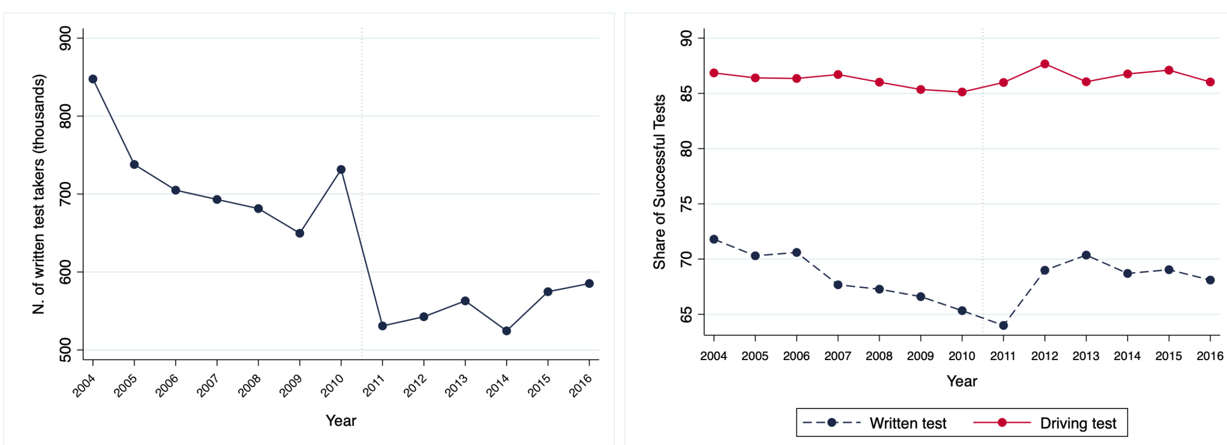
NOTES. This figure depicts the number of driving licenses issued in each year over the period 2004-2016. The solid line indicates the number of new licensees based on the driving license census, while the dashed line the number of successful driving tests based on the MIT reports on driving exams.

Figure A2: Population with Driver 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 A3: Driving Test Trends

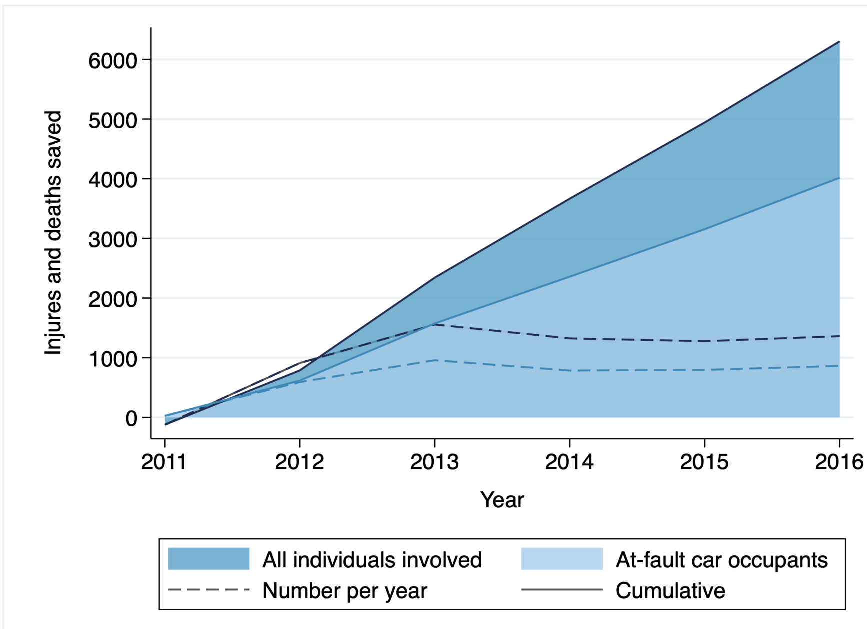


a. Written Test Takers per Year

b. Test Success Rate

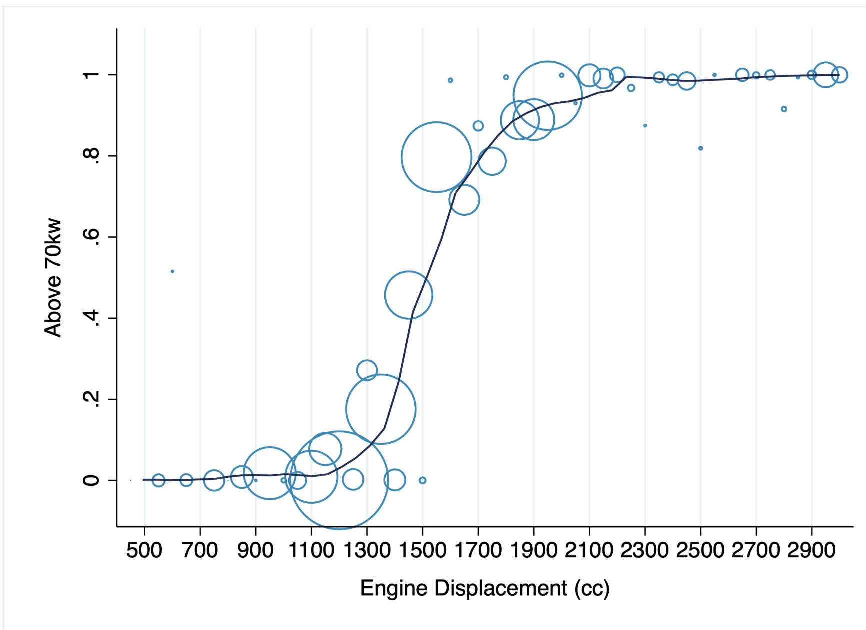
Notes. This figure depicts the evolution of the number of (written) test takers (Panel A) and of the proportion of successful driving and written tests (Panel B) over the years 2004-2016.

Figure A4: Prevented Injuries and Deaths



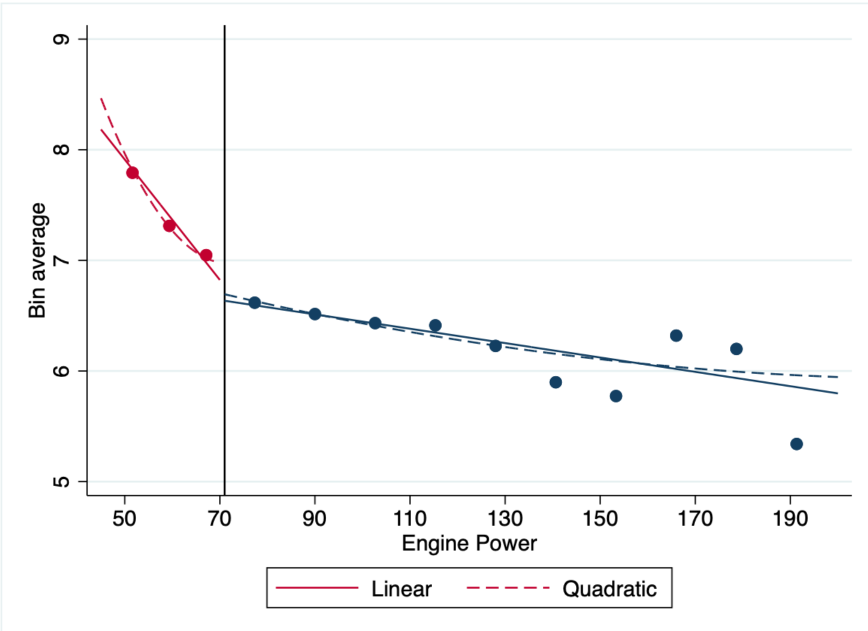
Notes. This figure illustrates the number of injuries and deaths saved because of the introduction of the power limit. The dashed lines indicate the number of injuries and deaths saved each year. This obtained by multiplying, for each year t, the estimated interaction coefficient $Age18 - 19 \times d_t$ – from a regression model of the form of Equation 1, where the dependent variable is the total number of caused injuries and deaths by drivers in a 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 A2. The solid lines indicate the cumulative number of injuries and deaths saved. For both the yearly and the cumulative number of injuries and deaths saved, darker colours indicate the total value, while lighter ones refers to occupants of the at-fault car only.

Figure A5: Relationship between Engine Displacement 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 A6: Pre-Reform Car Sales



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).

A.2 Tables

Table A1: Reform Effects on Road Accidents - Year-by-year estimates

	Accidents per capita			Accidents per licensee		
	(1)	(2)	(3)	(4)	(5)	(6)
	Accidents	Accidents	Fatal Accidents	Accidents	Accidents	Fatal Accidents
Age 18-19 × year 2006	-0.092	0.002	0.004	0.270	0.355	0.046
	(0.316)	(0.317)	(0.019)	(0.369)	(0.365)	(0.033)
Age 18-19 × year 2007	-0.301	-0.242	-0.026	-0.183	-0.137	-0.021
	(0.311)	(0.314)	(0.017)	(0.338)	(0.344)	(0.028)
Age 18-19 × year 2008	0.004	0.029	0.016	-0.185	-0.154	0.031
	(0.282)	(0.279)	(0.016)	(0.291)	(0.290)	(0.026)
Age 18-19 × year 2009	-0.059	-0.059	0.002	-0.212	-0.160	0.002
	(0.279)	(0.281)	(0.016)	(0.293)	(0.296)	(0.025)
Age 18-19 × year 2011	-0.090	-0.084	0.002	0.150	0.182	0.006
	(0.258)	(0.257)	(0.015)	(0.277)	(0.271)	(0.026)
Age 18-19 × year 2012	-0.616***	-0.621***	-0.025*	-0.806***	-0.803***	-0.038
	(0.237)	(0.231)	(0.015)	(0.253)	(0.250)	(0.025)
Age 18-19 × year 2013	-0.924***	-0.922***	-0.047***	-1.349***	-1.328***	-0.075***
	(0.241)	(0.233)	(0.015)	(0.256)	(0.254)	(0.024)
Age 18-19 × year 2014	-0.949***	-0.933***	-0.034**	-1.261***	-1.251***	-0.047*
	(0.241)	(0.231)	(0.015)	(0.257)	(0.254)	(0.026)
Age 18-19 × year 2015	-0.878***	-0.850***	-0.029**	-1.247***	-1.234***	-0.041*
	(0.241)	(0.226)	(0.014)	(0.259)	(0.245)	(0.024)
Age 18-19 × year 2016	-0.946***	-0.910***	-0.010	-1.075***	-1.050***	-0.003
	(0.247)	(0.230)	(0.014)	(0.265)	(0.251)	(0.025)
Age 18-19	-0.023			2.762***		
	(0.190)			(0.201)		
Female	-2.886***	-2.887***	-0.088***	-3.938***	-3.948***	-0.126***
	(0.054)	(0.052)	(0.003)	(0.054)	(0.052)	(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	0.374	0.478	0.149	0.395	0.498	0.147
Observations	43120	43120	43120	43120	43120	43120

NOTES. This table reports the estimates from Equation 1. 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< 0.01, ** p<0.05, and *p<0.10.

Table A2: Intensive Margin of the Reform Effect

	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 1. 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 injures (deaths) per 1,000 licensee is split between injured (dead) people in the at-fault car and injured (dead) occupants of other 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< 0.01, ** p<0.05, and *p<0.10.

Table A3: Reform Effect on Accidents per Licensee: Robustness to Different Age Group Specifications

	Treatment age group: 18-years-old				Treatment age group: 19-years-old			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Control: 24	Control: 25	Control: 26	Control: 27	Control: 24	Control: 25	Control: 26	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 1 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< 0.01, ** p<0.05, and *p<0.10.

Table A4: Reform Effect on Accidents per Licensee, by Vehicle Engine Displacement

	All	Engine Displacement Below Limit			Engine Displacement Above Limit		Unknown ED
	(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 1 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 variabile 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. 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< 0.01, ** p<0.05, and *p<0.10.

Table A5: Effect Heterogeneity

	Gender heterog.	Commuting zone heterogeneity			
	(1)	(2)	(3)	(4)	(5)
Age 18-19 × Female	-2.454***				
	(0.147)				
Post × Age 18-19	-1.375***	-0.836***	0.383	1.355	-2.007***
	(0.150)	(0.150)	(1.245)	(1.117)	(0.260)
Post × Age 18-19 × Female	0.839***				
	(0.195)				
Post × Age 18-19 × Urban commuting zone		-0.367*			
		(0.222)			
Post × Age 18-19 × Population (log)			-0.120		
			(0.107)		
Post × Age 18-19 × Per capita income (thousands)				-0.109**	
				(0.052)	
Post × Age 18-19 × Unemployment rate					7.270***
					(1.659)
R^2	0.738	0.681	0.681	0.681	0.681
Observations	24440	24440	24440	24440	24440

NOTES. This table reports estimates from a triple-difference specification of Equation 1, where we further interact the term $Post \times Age18-19$ with a gender dummy and a set of CZ characteristics. These are: i) a binary indicator for urban areas, defined as those CZ that include at least a municipality classified as a "central city" by the Italian National Governmental Agency For Territorial Cohesion; ii) the CZ population (in logarithm); iii) the CZ average per capita income (in thousands euro); iv) the CZ unemployment rate. 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 sample include accidents occurred over the period 2006-2016, but the reform year (2011) is excluded. In Column 1, the regression include gender×year, CZ×age-group, and CZ×year fixed-effects. In Columns 2 to 5, regressions include a gender dummy, the interaction between the binary indicator $Age18-19$ and the corresponding CZ characteristic, CZ×age-group, 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< 0.01, ** p<0.05, and *p<0.10.

Table A6: Effect Persistency: Estimates by License Seniority

	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 1 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< 0.01, ** p<0.05, and *p<0.10.