Shaping the Habits of Teen Drivers*

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Abstract

We show that a targeted law can modify teens' risky behavior. We examine the effects of an Australian intervention banning first-year drivers from driving late at night with multiple peers, which had accounted for one-fifth of their traffic fatalities. Using data on individual drivers linked to crash outcomes, we find the reform more than halves targeted crashes, casualties and deaths. There are large positive spillovers through lower crashes earlier in the evening and beyond the first year, suggesting broad and persistent declines in high-risk driving. When we consider potential explanations for these spillovers, we find results consistent with habit formation aided by the expressive value of the law. Overall, the targeted intervention delivers gains comparable to harsher restrictions that delay teen driving.

Keywords: traffic fatalities, teens, driving restrictions, risky behavior, habit formation, expressive value of law

JEL Classification: I18, K32

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1 Introduction

Teens engage in a wide variety of risky behaviors, including smoking, drinking, drug use, criminal activity, unprotected sex, and unsafe driving (Gruber, 2001). Driving is one of the most common and dangerous of these activities. Studies report that 15–30% of teen drivers have an accident in their first year of driving (McCartt et al., 2009; Curry et al., 2015). Teens have a fatality risk per mile driven several times higher than adults (Tefft et al., 2013), and traffic accidents are teens' leading cause of death in most countries (Kyu et al., 2016).

Governments in many developed countries have introduced restrictions on young drivers in order to reduce these risks (OECD International Transport Forum, 2015). "Graduated driver licensing" laws apply to learners and in a probationary period of unsupervised driving. Such laws include minimum licensing periods, passenger restrictions, nighttime curfews, lower speed and alcohol limits, and cellphone bans. In the US, nighttime curfews and passenger restrictions are particularly common, with over 90% of new teen drivers subject to both (Gilpin, 2019).

While studies find that these laws can reduce accidents, three issues limit their effectiveness. First, reductions mainly come from fewer teens getting driver's licenses (Gilpin, 2019). Incapacitation delays driving experience and the benefits of driving independently, which may be substantial relative to the value of reducing teen crashes (Dee et al., 2005). Second, risky driving still accounts for a large fraction of teen deaths. Peer passengers and nighttime driving elevate crash risks, especially in combination: Tefft et al. (2013) estimate that US teen drivers' fatality rates per mile driven are over 50 times higher for late-night driving with multiple peers than daytime driving without passengers. Between 1996 and 2010, the US went from nine states with nighttime restrictions and none with passenger restrictions to nearly all states having both (Williams et al., 2016). Yet the decline in the fraction of teen traffic fatalities resulting from late-night multi-passenger crashes has been modest, from 16% before 1996 to 13% after 2010.[1] Third, the restrictions have no impact on crashes after they end (Dee et al., 2005; Karaca-Mandic and Ridgeway, 2010; Gilpin, 2019). Thus, there is little support for the US National Highway Traffic Safety Administration's (1998) claim that "by creating safer teen drivers today, we also are helping them become safer, more responsible young adult drivers tomorrow" (p. 16).

[1] Authors' calculations for ages 16–19 from Fatal Accident Reporting System 1975–2018 (`https://www.nhtsa.gov/content/nhtsa-ftp/251`). Passenger and night restrictions apply separately, except in Louisiana. We use 11:00pm–4:59pm to match states' median curfew start and end times (`https://www.ghsa.org/state-laws`).

In this paper, we show that a targeted driving restriction can overcome these issues, resulting in large improvements in road safety. In July 2007, the Australian state of New South Wales banned first-year drivers from carrying two or more passengers under the age of 21 between 11:00pm and 4:59am. As in the US, late-night multi-passenger crashes are high risk: in our setting, they had accounted for only 2.8% of police-reported crashes involving first-year drivers, but 18.3% of fatalities from such accidents.

We examine the impacts of the restriction using administrative data on drivers' licenses linked to detailed crash data. Using multi-passenger crash outcomes per licensed driver, we estimate a difference-in-differences Poisson model that compares changes in the restricted period (11:00pm–4:59am) to daytime hours (8:00am–7:59pm) to control for other factors affecting crash risks. After showing similar pre-treatment trends in daytime and nighttime crashes, we estimate that the restriction reduced reported crashes by 57%, casualties by 50%, and hospitalizations and fatalities by 58%. We find similar reductions using a triple-differences approach with drivers aged 25–39 years as a comparison group. An indication of the extent of the change is that targeted crashes represented 18.3% of first-year fatalities before the restriction and 4.3% afterwards.

We consider contemporaneous spillovers to: (i) multi-passenger crashes by first-year drivers near the 11:00pm–4:59am period; (ii) crashes by first-year drivers with zero or one passenger, during and near 11:00pm–4:59am; and (iii) all crashes with teen passengers during and near 11:00pm–4:59am. We find that there is a reduction in first-year drivers' multi-passenger crashes between 8:00pm and 10:59pm, suggesting that the restriction reduced trips for activities spanning several hours (e.g., driving to movies or parties). We find no effects for the other two spillovers, indicating that there were no large responses in terms of first-year drivers taking fewer passengers or teens taking more trips with older drivers (e.g., with parents or in taxis).

We assess whether the restriction had effects beyond the first year of driving. We find statistically significant reductions in nighttime multi-passenger crashes in the second and third years. There are no discernible differences beyond the third year, but by that time the underlying crash rates are just one-fifth those of first-year drivers. There is no evidence that the restriction delays the development of driving skills, affects the likelihood of obtaining a license, or changes the transition rate to the next license class (where drivers are no longer subject to the restriction).

We conduct several empirical exercises to understand what mechanisms may account for these broad and persistent effects. First, we assess whether the restriction reduced driving or made driving safer by using multi-vehicle crashes to measure driving prevalence (Levitt and Porter, 2001). We show that the effect sizes do not vary by young drivers' likelihood of being responsible for a crash, implying that the reduced crashes come from young drivers reducing their nighttime driving with peers (both in the first year and longer term). Second, we show that there is a strong positive correlation between the short- and long-term effects for different subgroups, consistent with habit formation (Charness and Gneezy, 2009; Fujiwara et al., 2016; Ito et al., 2018). Third, we show that the treatment effects are more persistent among teens always subject to the ban than teens who started driving independently before the restriction went into effect, which highlights how initial compliance shapes longer-term behavior. Fourth, we show that young drivers paying attention to other road laws — by not speeding or drinking alcohol at the time of a crash — are most responsive to the new restriction. This is consistent with the law itself having "expressive value" by providing information about the safety or desirability of nighttime driving with peers, which may have affected norms or social preferences (Sunstein, 1996; Posner, 2002; Benabou and Tirole, 2011; Acemoglu and Jackson, 2017). Finally, consistent with norms or preferences changing gradually after the restriction was introduced, we show that the crash reductions become larger over time, while the fraction of first-year drivers caught violating the restriction declines. These empirical exercises suggest that the broader effects likely come from a combination of habit formation through deterrence and the expressive value of the law itself.

The evening spillovers and persistent effects more than double the impact of the policy. In total, the direct and indirect effects per 100,000 drivers amount to 5.9 fewer deaths, 41 fewer hospitalizations, 98 fewer minor casualties and 164 fewer crashes with property damage. Government estimates of the value of a statistical life and crash costs imply that these improvements are worth approximately A$412 million (US$320 million), or A$738 (US$570) per first-year driver. Reduced harm to passengers and other drivers account for nearly all of the gains, indicating that the restriction substantially reduced the negative externalities of teen driving.

These effects are large relative to policies that substantially reduce teen licenses and driving. The estimated decrease in traffic deaths per teen is 45–90% of the overall impacts of "good" graduated licensing systems found by Dee et al. (2005) and Gilpin (2019), which typically combine the effects of nighttime curfews, passenger restrictions, older licensing ages, and long permit periods (McCartt et al., 2010). The effect on fatalities is also similar to the estimated gains from raising the US minimum driving age by one year (Huh and Reif, 2020).

We make several contributions to the literature. Most directly, we enhance the understanding of driving restrictions used in graduated licensing systems, which are often evaluated as a whole (e.g., Dee et al., 2005; Morrisey et al., 2006; Karaca-Mandic and Ridgeway, 2010). Targeted nighttime passenger restrictions have been introduced in several jurisdictions, including two Australian states (New South Wales, Queensland), four Canadian provinces (Manitoba, Ontario, Quebec, Newfoundland and Labrador), and one US state (Louisiana). However, to the best of our knowledge, we are the first to comprehensively examine the effects of this type of policy. In doing so, we demonstrate the importance of using individual-level administrative data on licensing and crashes to examine persistence in the policy treatment effects. Our results contrast with US research on all-day teen passenger limits and nighttime curfews for teen drivers, which finds no impacts after controlling for the number of licenses (Gilpin, 2019).[2]

We also advance the broader economic literature on traffic safety.[3] First, as discussed above, we show that a targeted driving intervention can have sizable impacts. Second, we highlight the importance of external costs in high-risk teen driving, complementing previous research on externalities and driving (e.g., Levitt and Porter, 2001; Edlin and Karaca-Mandic, 2006; Anderson and Auffhammer, 2014; Barahona et al., 2020). Third, safer driving behavior in our setting persists for two years after treatment, which is much longer than has been found in the literature. For example, Bauernschuster and Rekers (2019) find that speed-monitoring campaigns do not reduce accidents once they end, Abouk and Adams (2013) find that ongoing texting bans decrease crashes for one or two months before returning to baseline levels, and Banerjee et al. (2019) find that drunk-driving enforcement effects persist for three months.

Finally, by showing that a targeted restriction can moderate teens' risky behaviors in a high-stakes setting, we contribute to the broader literature on behavioral change and the deterrence of harmful behaviors.[4] Two features of the policy appear important to explaining why we

[2]For reviews of the broader literature, see Shope (2006), Williams (2017) and O'Neill (2020).

[3]Economics studies have examined drunk-driving laws (Evans et al., 1991; Ruhm, 1996; Carpenter, 2004; Hansen, 2015); minimum legal drinking ages (Dee and Evans, 2001; Carpenter and Dobkin, 2009; Lovenheim and Slemrod, 2010; Lindo et al., 2016); minimum driving ages (Huh and Reif, 2021); vehicle weight (Anderson, 2008; Anderson and Auffhammer, 2014); seatbelt regulations (Peltzman, 1975; Dee and Evans, 2001; Cohen and Einav, 2003; Carpenter and Stehr, 2008); child safety seats (Levitt, 2008; Jones and Ziebarth, 2016; Anderson and Sandholt, 2019); speed limits (Dee and Sela, 2003; Ashenfelter and Greenstone, 2004); police enforcement (DeAngelo and Hansen, 2014; Gallagher and Fisher, 2020); and ride-sharing services (Anderson and Davis, 2021).

[4]For reviews, see Gruber (2001); Cawley and Ruhm (2011); Chalfin and McCrary (2017); Doleac (forthcoming).

find such a broad and persistent effect relative to other studies. The first is its targeted nature. Acemoglu and Jackson (2017) argue that laws are most likely to change norms when they ban fewer behaviors and conflict less with prevailing norms. The restriction we study banned a type of driving that accounts for approximately 0.5% of trips made by adults, and only a slightly higher fraction for teens.[5] The second is that teens are restricted when they first start driving. O'Donoghue and Rabin (2001) argue that teens' direct experience of risky behaviors may increase rather than decrease risk-taking, while recent studies have shown initial teenage behaviors and experiences shape long-term preferences around driving (Severen and Van Benthem, 2022); alcohol consumption (Kueng and Yakovlev, 2021); smoking (Friedson et al., 2021); and criminal activity (Bell et al., 2018; Sviatschi, forthcoming).[6] Thus, our paper highlights the potential of using targeted restrictions to regulate teens' driving and other risky behaviors.

2 Background

New South Wales (NSW) is Australia's most populous state. It has approximately eight million residents, with the majority living in Sydney. The NSW Government is responsible for driving laws, which are enforced by a single state police agency. In 2006, the year before the nighttime passenger restriction was introduced, NSW had 7.4 traffic fatalities per 100,000 residents. This is similar to the recent OECD average (7.0 per 100,000 in 2016). Like Canada, the UK and the US, NSW has seen a steady decrease in traffic fatalities since the 1970s (Appendix Figure A1).

The graduated driver licensing system in NSW has three stages.[7] From age 16, individuals who pass a written test can get a learning permit that allows driving with a fully licensed driver. From age 17, individuals can take computer and driving tests to get a probationary "P1" license. As discussed below, they must have held their learning permit for a minimum period and completed a minimum amount of supervised driving. The P1 license allows unsupervised driving, although drivers cannot drive over 90 kmh (56 mph) and must have a blood alcohol concentration of zero.[8] Since July 2007, they are also subject to the nighttime passenger restriction described below. After holding a P1 license for one year, individuals can obtain a provisional "P2" license;

[5] Authors' calculations using 2009 US National Household Travel Survey (`https://nhts.ornl.gov/downloads`).

[6] It is noteworthy that, consistent with O'Donoghue and Rabin (2001), our policy changes persist for longer than directly experiencing a crash, which modifies teens' driving behavior for three months (O'Brien et al., 2017).

[7] More NSW licensing information is available at: `https://www.rms.nsw.gov.au/roads/licence/index.html`.

[8] Lindo et al. (2016) find NSW's legal drinking age of 18 has no effect on traffic accidents.

during our sample period, this involved passing a computer-based "hazard perception test." The P2 license allows drivers to travel up to 100 kmh (63 mph) and removes the nighttime passenger restriction. Drivers can apply for an unrestricted license after holding a P2 license for two years, which removes the speed restriction and increases the maximum blood alcohol concentration allowed from zero to 0.05%. Until they obtain an unrestricted license, drivers must display signs on the front and back of their vehicle showing their licensing stage.

Since July 2007, P1 drivers cannot carry more than one passenger aged under 21 years between 11:00pm and 4:59am. Drivers aged 25 and over are exempt. Exemptions for employment, family or volunteering reasons can be sought in writing, but are rarely granted. The restriction was announced in January 2007. Violations result in a fine — currently A$581 (∼US$450) — and three demerit points for drivers, which is one point below the level at which their license is suspended.[9] Approximately one percent of first-year drivers were penalized for violations in the first year after the restriction was implemented (Hildebrand and Kaye, 2008).

Four other changes were introduced in July 2007: (i) P1 drivers caught speeding had their licenses automatically suspended for three months; (ii) Learner, P1 and P2 drivers were banned from using cellphones; (iii) the minimum period holding a learning permit increased from six to 12 months; and (iv) the minimum hours of learners' supervised driving increased from 50 to 120 hours. Siskind et al. (2019) and Kettlewell and Siminski (2020) examine the impact of the last two restrictions on crash outcomes, with the former comparing two different cohorts of license holders and the latter using a birth-cohort regression discontinuity design. Neither find changes in crash outcomes, although Kettlewell and Siminski (2020) find that requiring 50 hours of supervised driving since 2000 reduced crashes.[10] We compare nighttime to daytime outcomes to separate the effects of the nighttime passenger restriction from other factors affecting crashes.

3 Data

We use two administrative data sets from the NSW Department of Transport. The first is a census of NSW driving license records for everyone born since 1980, which allows us to follow drivers aged under 25 throughout our sample period. It includes sex, age, and the dates that different licenses were obtained. The second data set, *CrashLink*, contains detailed information

[9]Demerit points are also issued in response to traffic infringements, such as speeding or running a red light.

[10]Note that the introduction of this requirement occurred before our sample period.

on crashes reported to NSW Police. Crashes are reported when there are fatalities, injuries, or vehicles towed. The data set includes the month, year and exact time of the crash; each vehicle's passenger numbers; driver characteristics; and the number of fatalities and injuries. Age is not recorded for uninjured passengers and pedestrians. The data sets are linked at the driver level.[11]

Using these data, we can identify drivers and crashes affected by the nighttime passenger restriction. We focus on the first 12 months of P1 driving, which is the minimum length of a P1 license.[12] Our primary sample consists of drivers who obtained their P1 license before age 24, as the restriction does not apply from age 25. Most obtain their P1 license much earlier: in our sample, 58% got their license at age 17 and 84% by age 19. We create a panel at the driver-year-month level for different times of the day from June 2004 to September 2014, during which policies are consistent.[13] We have 37 months before and 87 months after the restriction was introduced. We have 239,158 drivers never subject to the restriction, 76,679 who had a P1 license when the restriction was introduced, and 558,207 always subject to it.

Summary statistics for first-year drivers before the restriction was in place are shown in Appendix Table A1. On average, they are aged 18.8 years and 52% are male. First-year drivers involved in crashes are typically younger, less experienced and more likely to be male than other first-year drivers. These differences are more pronounced for the nighttime multi-passenger crashes that were the target of the restriction. Around two-thirds of these crashes occur on Friday and Saturday nights, and half involve multiple vehicles. They are relatively serious, representing 2.8% of crashes involving first-year drivers but 18.3% of the fatalities.

Before the reform, crashes declined strongly with experience (Appendix Figure A2). Quarterly crashes per 100,000 driver-years halve over the first year of driving, from 8,662 in the first quarter to 4,294 crashes in the fourth. After four years of driving, the crash rate is 69% lower than the initial rate. Nighttime multi-passenger crashes decline even more sharply, dropping by 61% during the first year and 82% after four years. Inexperienced drivers have elevated crash risks, especially for nighttime multi-passenger crashes.

[11] Our analysis is limited to NSW, as other Australian states do not have similar licensing and crash data.

[12] P1 drivers have to visit the licensing agency to get their P2 license, and most do so within three months of eligibility. We consider if the restriction affected the transition from P1 to P2 licenses in Section 5.

[13] In May 2004, probationary drivers' alcohol limit was reduced to zero. From October 2014, police no longer had to attend crashes with no casualties. A ban on P1 drivers using powerful vehicles was introduced in July 2005, but it applies to few vehicles and had no apparent effects in our data.

4 Estimating the policy effects

4.1 Comparing nighttime and daytime crash rates

We estimate the impact of the nighttime passenger restriction by using daytime crashes to account for underlying changes in crash outcomes. Our key identifying assumption is that any underlying changes in crash risks or severity, such as changes in driving activity, traffic rules or vehicle safety, will have similar relative impacts on nighttime and daytime crash rates. We relax this assumption in some specifications by including a comparison group of older drivers not subject to the nighttime passenger restriction, who account for any changes in nighttime crash risks common to younger and older drivers.

We are not the first to use this identification strategy. Studies examining the impact of alcohol taxes and regulations on traffic safety have used a similar approach, as nighttime traffic fatalities are more likely to involve alcohol (e.g., Ruhm, 1996; Dee, 1999; Carpenter and Dobkin, 2011). The key difference is that the restriction we study is directly tied to nighttime driving, while those studies use it as the period most strongly treated by alcohol regulations.

To assess the validity of our approach, we compare the nighttime (11:00pm–4:59am) and daytime (8:00am–7:59pm) crash rates of first-year drivers carrying multiple passengers in Figure 1a. Daytime crash rates are higher than at night because most driving occurs during the day. However, in relative terms, the two series exhibit a similar, slightly downward trend before the restriction. A regression-adjusted version with 95% confidence intervals shows no statistically significant differences across nighttime and daytime relative crash rates before the restriction, and no evidence of announcement-related anticipation effects (Appendix Figure A3a). These results suggest that daytime crashes can account for counterfactual trends in nighttime crashes.

Figure 1a also suggests that the restriction reduced crash rates, with a sharp and persistent reduction in nighttime crashes and little change in the trend for daytime crashes after it was introduced. The regression-adjusted version indicates that these reductions are statistically significant (Appendix Figure A3a). Figure 1b shows the crash rates for first-year drivers carrying zero or one passenger, which provides further information about the comparability of the nighttime and daytime crash rates and the effects of the restriction. The relative changes in daytime and nighttime crashes with zero or one passenger are similar prior to the restriction, which is confirmed in a regression-adjusted version (Appendix Figure A3b). There is no visually apparent change in these nighttime crashes after the restriction was introduced.

4.2 Effects on multi-passenger crashes

We estimate the changes in crash outcomes using a difference-in-differences Poisson specification that allows for spillovers on multi-passenger crashes in hours adjacent to the restricted period. Trips often span several hours; for example, US teen drivers who leave home in the evening are away for 2.3 hours on average.[14] A reduction in such trips could decrease crashes in nearby hours, while shifting trips could increase crashes in nearby hours (e.g., not going to the movies versus attending an earlier screening). We allow for spillover effects three hours either side of 11:00pm–4.59am, but also consider other ranges.[15]

Our primary Poisson specification is:

$$\mathbb{E}[y_{iymp}|Z_{iymp}] = \exp\{\eta_{ym} + \theta_p + \mu_{mp} + \sum_{j=1}^{3} \beta_j \mathbf{1}(\text{Period}_p = j) \times \text{Post}_{ym} + X_{iymp}\lambda\} \qquad (1)$$

For a given outcome y_{iymp} and set of explanatory variables Z_{iymp}, i indexes individuals, y years, m months, and p periods of the day. Four periods are used: restricted (11:00pm–4:59am), evening (8:00pm–10:59pm), morning (5:00am–7:59am), and daytime (8:00am–7:59pm). Indicator variables for the first three periods are interacted with Post_{ym}, which identifies the post-restriction period. The key coefficients are β_j, which give the change in a crash outcome after the restriction was introduced relative to daytime outcomes. The percentage change in each outcome is given by $100 \times (\exp(\hat{\beta}_j) - 1)$. We include year-month fixed effects η_{ym} and period-of-day fixed effects θ_p to control for common crash determinants over time and across the day. Month-period fixed effects μ_{mp} are included to account for seasonal variation in crash outcomes across the day. The vector of individual controls X_{iymp} includes fixed effects for drivers' sex, months of age and months of experience, which are interacted with the period-of-day dummy variables to allow for different impacts of driver composition across the day. We control for the number of days a license is held in the first, partial month of driving. We allow for an arbitrary

[14] Authors' calculations from 2009 US National Household Travel Survey (`https://nhts.ornl.gov/downloads`). We use 16–19-year-olds who leave home 6:00pm–10:59pm and return home by 4:00am (the latest time recorded).

[15] Appendix Figures A3c and A3e show that multi-passenger crashes in these periods also have similar pre-restriction trends to daytime crash rates.

correlation in errors at the individual level.[16]

The results for multi-passenger crashes, casualties and hospitalizations/deaths are shown in columns 1–3 of Table 1. In addition to coefficients and standard errors, we show the implied changes as a percentage and per 100,000 driver-years. In the restricted period, crashes changed by -57%, casualties by -50%, and hospitalizations/deaths by -58% (all $p < 0.01$). In the evening period, crashes and casualties change -15% and -26% respectively (both $p < 0.05$), while there is a statistically insignificant -35% change in hospitalizations/deaths. There are no statistically significant effects in the morning period.

We assess the sensitivity of these results to varying the reference period and the measurement of spillovers. First, we divide the day into three-hour blocks and estimate a version of equation (1) with a reference period of 11:00am–1:59pm. The estimates remain similar, with no evidence of further spillovers (Figure 2). Second, spillover periods of two and four hours produce similar evening estimates and no discernible changes in morning crash outcomes (Appendix Table A2). These results are consistent with the raw data on hourly crash rates before and after the reform, with large decreases in the restricted period and smaller decreases during 7:00pm–10:59pm (Appendix Figure A4).

In a second, complementary approach, we estimate the policy effects using a comparison group of older drivers to control for general changes in the relative risk of nighttime driving compared to daytime driving. Drivers aged 25 years and over are not subject to the nighttime passenger restriction, even if they are a P1 driver. We therefore add the nighttime and daytime crash outcomes of drivers aged 25–39 years who, as a group, had a similar number of nighttime multi-passenger crashes prior to the restriction as first-year drivers (Appendix Figure A5). This allows us to use a "triple differences" specification based on driver types (first year/25–39 years), time of day (nighttime/daytime) and time period (before/after the restriction).

A limitation of this approach is that we have to use crash counts rather than crash rates. Driving license information is only available for individuals born since 1980, which means we cannot track licensing for individuals aged 25 and over throughout our sample period. Therefore, we cannot condition on license numbers or control for the individual characteristics of license holders. However, the trends in first year drivers' nighttime (11:00pm–4:59am) and daytime

[16] We include fixed effects using the Poisson Pseudo-Maximum Likelihood estimator (Correia et al., 2020). We do not include individual fixed effects as age and experience are collinear at the individual level.

(8:00am–7:59pm) crash counts are similar to the crash rates presented in Figure 1a, and equivalent figures for drivers aged 25–39 years show that their nighttime and daytime crash counts evolve smoothly over time (Appendix Figure A5).

We estimate the following Poisson triple differences specification:

$$\mathbb{E}[y_{gymp}|Z_{gymp}] = \exp\{\eta_{gym} + \theta_{gp} + \mu_{gmp} + \sum_{j=1}^{3} \alpha_j \mathbf{1}(\text{Period}_p = j) \times \text{Post}_{ym} + \beta_j \mathbf{1}(\text{Period}_p = j) \times \text{Post}_{ym} \times \text{FirstYr}_g \lambda\} \quad (2)$$

The crash outcomes y_{gymp} are now counts for the treated and comparison groups of drivers, which are indexed by g, at the year-month-period (ymp) level. The same four time periods are used: restricted (11:00pm–4:59am), evening (8:00pm–10:59pm), morning (5:00am–7:59am), and daytime (8:00am–7:59pm) as the reference period. We include a complete set of fixed effects for group-year-month η_{gym}, group-period θ_{gp}, and group-month-period μ_{gmp}. These account for group-specific differences in crashes over time and across the day, and group-specific seasonal variation within each period. The α_j coefficients come from the interaction between the period-of-day indicator variables Period_p and a dummy variable identifying the post-restriction period Post_{ym}. They identify relative changes in crash outcomes in the nighttime and spillover windows after the restriction is introduced that are common to both groups. The key coefficients of interest are β_j, as they identify additional changes specific to first-year drivers (FirstYr_g). The percentage change in each outcome is given by $100 \times (\exp(\hat{\beta}_j) - 1)$. We estimate standard errors that are robust to heteroskedasticity.

The results for multi-passenger crashes, casualties and hospitalizations/deaths from equation (2) are shown in columns 4–6 of Table 1, alongside the results from equation (1). The results across the two specifications are very similar in magnitude and precision. In the restricted period, crashes are estimated to change by -55%, casualties by -44%, and hospitalizations/deaths by -53% (all $p < 0.01$). These closely match the previous estimates for the direct effects. In the evening period, crashes are estimated to change by -11%, casualties by -32% ($p < 0.05$), and hospitalizations/deaths by -39% ($p < 0.05$). The estimates are similar to the previous estimates for the evening spillover, except that the estimate for crashes loses statistical significance and the estimate for hospitalizations/deaths gains it (at $p < 0.05$). As before, there are no statistically significant effects in the morning period.

The consistency of the results provides several insights into the robustness of our estimates. First, the estimated reductions in crash outcomes are not due to changes in nighttime crash risks that are common to new and more experienced drivers. Second, the similarity of the estimates using counts and rates implies that the results are not sensitive to conditioning on license numbers. Third, the results do not depend on the inclusion of individual-level controls or clustering of standard errors. We verify the results are robust to additional alternative specifications and sample choices.[17]

For every 100,000 first-year drivers, the direct effects and evening spillovers amount to reductions of 94 crashes, 98 casualties and 38 hospitalizations/fatalities. We separately estimate the reductions in hospitalizations and fatalities using a single 8:00pm–4:59am treatment period and daytime outcomes from all crashes involving first-year drivers to increase precision. The estimates imply reductions per 100,000 drivers of 5.0 fatalities ($p < 0.05$) and 33.4 hospitalizations ($p < 0.01$).[18] This estimated reduction in fatalities of 69% is similar to the 72% reduction in the raw data (from 7.5 to 2.1 per 100,000 driver-years). The large decline in serious outcomes limits concerns that crash-reporting changes can explain our results.

We examine which road users benefit from the restriction. To do so, we separately estimate the change in casualties for first-year drivers, their passengers, and other road users using a single 8:00pm–4:59am treatment period (Table 2). The point estimates imply that first-year drivers account for 24% of the overall reduction in casualties, while their passengers account for 45% and other road users 31%. For the reduction in hospitalizations/fatalities, the point estimates imply that first-year drivers account for 4%, their passengers 46%, and other road users 50%. The restriction on first-year drivers generates substantial external benefits to other road users.

[17]Using variations of equation (1), we present results using robust standard errors; a negative binomial model; and Poisson and OLS models that use period-month-year counts (Appendix Table A3). We also present results using equation (1) and alternate samples of (a) first-year drivers who begin as teens; and (b) all P1 drivers aged under 25 (Appendix Table A4). The results are qualitatively similar throughout.

[18]The sum of these two estimates closely matches the original point estimate (-38.4 versus -38.2 for hospitalizations/fatalities). This approach also produces similar casualty and crash estimates.

4.3 Effects on other types of crashes

In response to the restriction, first-year drivers may have increased trips alone or with one passenger (e.g., four teens may now travel in two vehicles instead of one). First-year drivers may have also traveled as passengers with an older driver, such as a parent or taxi driver. We examine whether there are any detectable changes in these types of crashes.

We estimate the crash outcomes for first-year drivers with zero or one passenger. We use equations (1) and (2); as shown already, the trends in crash rates and crash counts of first-year drivers and drivers aged 25–39 years suggest that these approaches should identify policy effects (Figure 1b; Appendix Figures A3b, A3d, A3f, A5c, A5d). The estimates, which are shown in Appendix Table A5, are small and not statistically different from zero. The 95% confidence intervals from equation (1) imply that any increase in such crashes between 11:00pm and 4:59am represents at most 17% of the decrease in multi-passenger crashes (15% for casualties). We confirm these results using similar robustness exercises to those for multi-passenger crashes (Appendix Tables A3 and A4).

To examine trips where teens travel as passengers with an older driver, we estimate the effects of the restriction on casualties at ages 16–20 not involving a P1 driver under 25. We use period-month-year casualty counts and a Poisson model using daytime outcomes as controls. The estimated effects are small and not statistically different from zero (Appendix Table A6).

Public transport trips are another potential margin of response. While we cannot observe them in our data, public transport in NSW is extremely safe; for example, no teens died in bus or train accidents — at any time of the day — in the first three years that the nighttime passenger restriction was in place.[19] Thus, any substitution to public transport or other lower-risk trips would not be large enough to meaningfully change the net effects on crash outcomes.

4.4 Persistence in the treatment effects

We examine whether there are treatment effects beyond the first year of driving, once the restriction no longer applies. We use the linked licensing-crash data to create a panel of crash outcomes for the first four years of driving. We continue to identify the policy effects by estimating the post-restriction changes relative to daytime crash outcomes. We estimate the following Poisson

[19]NSW crash statistics are at: `https://roadsafety.transport.nsw.gov.au/statistics/reports.html`.

differences-in-differences specification:

$$\mathbb{E}[y_{iymp}|Z_{iymp}] = \exp\{\nu_{ym}+\theta_p+\mu_{mp}+\text{Post}_i+\sum_{d=1}^{4}\sum_{j=1}^{3}\beta_{dj}\mathbf{1}(\text{Exp}_{iym}=d)\times\mathbf{1}(\text{Period}_p=j)\times\text{Post}_i+X_{iymp}\lambda\} \tag{3}$$

where the index values and controls are the same as in equation (1). Indicator variables measuring each year of experience are interacted with the period-of-day indicator variables and Post_i, which identifies drivers treated by the nighttime passenger restriction. This interaction produces our main coefficients of interest, β_{dj}, which measure the change in crash outcomes in period-of-day j for treated drivers in their d^{th} year of driving relative to daytime outcomes and drivers of the same experience who were never subject to the restriction.[20] We initially exclude partially treated drivers, who started driving independently before the restriction was introduced, but assess their response in the next section.

Figure 3 shows results for multi-passenger crash outcomes during 11:00pm–4:59am by years of experience. Figure 3a shows the implied percentage changes for crashes. The estimates are -59% in the first year of driving, -44% in the second, -24% in the third, and -15% in the fourth. The estimates for the first three years are statistically significant at the 5% level. The results for casualties in Figure 3b display a similar pattern, although the magnitudes decline more quickly and are only statistically significant in the first two years. Figures 3c and 3d show these impacts in absolute terms, by comparing the nighttime multi-passenger crash and casualty rates of drivers subject to the restriction to counterfactual rates implied by our regression estimates. The reform flattened the crash-experience relationship for nighttime multi-passenger crashes. Without the reform, we estimate that crash rates in the fourth year would have been approximately one-fifth those in the first year. This matches how the pre-reform crash rates discussed in Section 3 decline with experience. After the reform, crash rates in the fourth year are only half those in the first year, yet still lower than the counterfactual. The restriction significantly improved outcomes until drivers gained experience and had much lower underlying crash risks.

We quantify the direct and evening-spillover effects in the 2$^{\text{nd}}$ through 4$^{\text{th}}$ years of driving (Appendix Table A7). After the restriction ends, we estimate that, for every 100,000 first-year

[20]We define the second year as the 14$^{\text{th}}$–24$^{\text{th}}$ months of driving, as the 13$^{\text{th}}$ month is partly in the first year for most drivers. We control for the effect in the 13$^{\text{th}}$ month, but exclude it from our β_{dj} estimates and adjust the implied effects in the second year to reflect a full year of driving. The third and fourth years include the 25$^{\text{th}}$–36$^{\text{th}}$ and 37$^{\text{th}}$–48$^{\text{th}}$ months, respectively.

drivers, there are changes of -57 crashes, -39 casualties and -6 hospitalizations/fatalities. These persistent effects meaningfully increase the impact of the nighttime passenger restriction. We value these effects, together with the immediate crash reductions, in the next section.

5 Mechanisms and interpretation

The driving restriction that we study resulted in large reductions in multi-passenger crashes in the targeted period, contemporaneous reductions in evening multi-passenger crashes, and persistent reductions in nighttime multi-passenger crashes for two years after the restriction ends. In this section, we examine why this law produced such large and broad effects relative to other teen driving restrictions studied within the literature. We also consider what teens' responses imply about the costs of the restriction relative to its sizable safety benefits.

Before considering specific mechanisms, it is important to determine whether the restriction reduced driving (fewer miles driven) or made driving safer (fewer crashes per mile driven). Previous studies have used the interaction of "treated" and "untreated" drivers in crashes to separate these factors in the context of drunk driving (Levitt and Porter, 2001) and graduated driving laws (Karaca-Mandic and Ridgeway, 2010). Our approach is to assume that first-year drivers' risk of being in an accident that is caused by other road users is proportional to how much they drive. Thus, we use the changes in multi-vehicle crashes for which first-year drivers are not responsible as a measure of the change in driving prevalence.[21]

We use two measures of crash responsibility. The first is police judgments, who mainly assign responsibility based on the vehicles' maneuvers before the crash (e.g., turning left, overtaking a vehicle). Second, to address concerns that police may be more likely to blame first-year drivers for crashes once the restriction was introduced, we use machine-learning methods and pre-restriction data to predict the most responsible driver in each crash (see Appendix B).

For nighttime multi-passenger crashes involving multiple vehicles, the estimated changes where drivers treated by the restriction are deemed most responsible are similar to those where they are not (Appendix Figure A6). These results are robust to using police reports or the machine-learning approach. The results imply that less nighttime driving with multiple passen-

[21] We do not use crash interactions, as it is necessary to assume equal and independent mixing between driver types, which likely fails in our setting (see Levitt and Porter 2001; Karaca-Mandic and Ridgeway 2010).

gers can account for all of the crash reductions, both in the first year and the longer term.[22] Thus, the restriction reduced nighttime driving with multiple passengers even beyond the first year.

5.1 Habit formation and the expressive value of laws

Habits are typically formed through repetitive behavior, and lead to persistence in behavior once motivation or attention decline. Many economics studies take the persistence of behavior once no longer incentivized to do to — as observed here — as evidence of habit formation.[23] Such studies have found a positive correlation between short- and long-term effects for different subgroups (Fujiwara et al., 2016) and treatments (Charness and Gneezy, 2009; Ito et al., 2018). In our context, this would mean stronger long-term effects for drivers and driving behaviors that were more affected in the first year. For teen driving, a negative correlation is also possible, if the restrictions impede learning by doing (Dee et al., 2005; Karaca-Mandic and Ridgeway, 2010). Delayed skill development could mean that those more affected by the first-year restriction have higher subsequent crash risks. Figure 4 presents the short- and long-term point estimates for 13 types of crashes; results with 95% confidence intervals are presented in Appendix Figure A7. The correlation is 0.94, suggesting that initial changes in behavior are the source of the persistent improvements. For example, the short- and long-term estimates are larger for female drivers, crashes on main roads, crashes involving multiple vehicles, crashes on Friday or Saturday nights, crashes in which the relevant driver was not speeding and crashes during 11:00pm–4:59am compared to 8:00pm–10:59pm. The lack of any negative correlation suggests that restricting nighttime driving with peers does not delay the development of driving skills, implying they can be gained through less risky types of driving (e.g., nighttime driving without passengers or daytime driving with multiple passengers).

We next assess the effects for partially treated drivers, who were allowed a period of nighttime driving with multiple peers before the restriction was introduced (Table 3). Among these drivers, nighttime multi-passenger crashes decline by 40% under treatment in the first year of

[22] This is not surprising, as the alternative would require first-year drivers to keep driving despite the restriction, yet be much safer when doing so. Our findings are consistent with U.S. research that finds graduated driving laws reduce crashes by reducing driving (Karaca-Mandic and Ridgeway, 2010; Gilpin, 2019).

[23] Studies have examined the longer-run effects of temporary interventions on smoking cessation (Giné et al., 2010), electricity consumption (Costa and Gerard, 2021; Ito et al., 2018), water usage (Ferraro and Price, 2013; Byrne et al., 2021), physical exercise habits (Charness and Gneezy, 2009; Royer et al., 2015), commuting behavior (Larcom et al., 2017), political participation (Fujiwara et al., 2016), and hospital hygiene (Steiny Wellsjo, 2022).

driving ($p < 0.05$). This estimate is approximately two thirds of the estimate for fully treated drivers, and the p-value on the hypothesis test of equal effect sizes is 0.08. Over the 2nd through 4th years of driving, the crashes of partially treated drivers decline by a statistically insignificant 17%. This is roughly one half of the estimate for fully treated drivers (p-value for equality of coefficients: 0.07). Partially treated drivers have less persistent effects than fully treated drivers, which is consistent with the restriction affecting the initial development of driving habits or preferences.[24]

Other mechanisms may play a role in long-term behavioral change, including learning, information acquisition, taste discovery and social influences (Volpp and Loewenstein, 2020). A potential mechanisms in our context is that the law itself may have provided information about the safety or desirability of nighttime driving with peers. A literature has developed on the "expressive value of laws," where the act of passing a particular law sends a signal about a norm or prescribed attitude toward behavior (e.g., Sunstein, 1996; Posner, 2002; Benabou and Tirole, 2011; McAdams and Rasmusen, 2007). Empirical examples include finding that voting in Swiss Cantons declined after the repeal of mandatory voting laws with symbolic fines (Funk, 2007); that seatbelt use in the US increases after the introduction of seatbelt laws (separate to enforcement risk) (Wittlin, 2011); and that the introduction of legal same-sex relationship recognition policies improved attitudes toward sexual minorities in Europe (Aksoy et al., 2020).

In our context, teens paying attention to the expressive value of the law we study could be expected to be already paying attention to other road rules around speeding and drinking alcohol. In Table 4, we present results by whether or not the teen driver was speeding or had an illegal blood alcohol concentration at the time of the crash.[25] While both speeding/drinking and non-speeding/non-drinking crashes decline by statistically significant levels in the first year and the longer term, the latter type declines the most in both periods, with respective p-values on hypothesis tests of equal effect sizes across the two types of 0.003 and 0.24. Speeding and drinking account for fewer crashes after the first year, so the reduction in non-speeding/non-

[24]These results are consistent with recent studies of preference formation that point to the importance of the older teenage years, albeit through experience rather than deterrence or information. Severen and Van Benthem (2022) document that gasoline price changes at ages 15–18 affect driving behavior decades later. They attribute this to formative driving experiences affecting long-term preferences. Similarly, Kueng and Yakovlev (2021) find that Russian anti-alcohol campaigns and beer-market expansion permanently changed the alcohol tastes of males who were affected at ages 14–18.

[25]We combine these behaviors into one category, as first-year drivers had an illegal blood alcohol concentration in just 2% of crashes before the restriction. Speeding occurred in 19.3% of crashes.

drinking crashes accounts for around three quarters of the overall reductions in each period.[26]

Two pieces of evidence also indicate increased compliance over time, which is consistent with a change in norms (Ostrom, 2000). First, using publicly available aggregate data on traffic infringements, we show that the fraction of P1 drivers caught breaking the passenger restriction halved over the seven years in our sample period (Appendix Figure A8a). This cannot be explained by declining enforcement of teen road rules, with little change in the infringement rate of P1 drivers not displaying license plates (Appendix Figure A8b) and an increase in infringements for mobile phone use (Appendix Figure A8c). Second, the treatment effects increase over time. The estimated changes in first-year drivers' multi-passenger crashes between 11:00pm and 4:59am are -36% in the first year that the policy was in effect; -47% in the second; and around -60% thereafter (Appendix Figure A3a).[27]

These empirical exercises are not conclusive about the source of the persistence, but they suggest that habit formation and information from the law itself are likely mechanisms for the broader effects. Such mechanisms are consistent with the restriction creating only small costs to young drivers; we now use measures of revealed preferences to consider these costs.

5.2 Inferring costs and valuing benefits

It is not possible to accurately measure the costs of the restriction. However, drivers may reveal their preferences for the banned driving through licensing decisions, which we can examine with our data. Before doing so, three features of the policy are worth noting. First, the persistence in treatment effects — and the relatively smooth decay in the size of the effects — are consistent with first year drivers bearing only small costs, as discussed above. Second, the driving affected should be mainly related to social activities with peers. Effects on work, study or family would require tasks that necessitate driving two or more people aged under 21 between 11:00pm and 4.59am *and* not being able to get a written exemption ahead of time. Third, the strong positive correlation between effect heterogeneity in the first year and longer term shown in Figure 4 and the steady trends in other crash outcomes in Figures 1a and 1b suggest that any costs related

[26] A priori, it not clear that more compliant drivers would experience disproportionate crash reductions, as violating the late-night multi-passenger restriction is relatively easy to detect and may have increased the costs of violating other road rules (e.g., drink-driving), which may be harder for police to observe without a traffic stop.

[27] We find qualitatively similar results if we augment equation (1) with interactions of the treatment effects with linear time trends. The coefficient on the interaction term for the nighttime window is negative and statistically significant (-0.006, $p = 0.040$), implying a modest increase in the absolute value of the effects over time.

to limiting the development of driving skills are small.

We now examine if the restriction affected P1 licensing rates, which we take as a measure of the benefits of independent driving.[28] The nighttime passenger restriction uses age criteria for drivers (under 25 years) and passengers (under 21 years). We cannot use the driver criterion, as we do not have licensing information for individuals aged 25 and over throughout our sample period. However, we can use the latter feature by assuming a positive correlation between a driver's age and those of their (potential) peer passengers. We compare the rates of new P1 licenses for ages 17–19 to ages 20–23 in Appendix Figure A9. The younger group, who would be expected to mainly have friends aged under 21 and therefore be strongly affected by the restriction, have similar licensing trends to the older group, for whom the restriction's impact should be much weaker. Other licensing rules introduced over this period were not based on age, as discussed in Section 2, so this comparison isolates the impact of the nighttime passenger restriction and suggests that it had no observable impact on the value of a P1 license.[29] Thus, the restriction did not appear to decrease the value of a P1 license for teens.

We can further examine the costs imposed on young drivers by how quickly they switch from a P1 to a P2 license, since this action removes the nighttime passenger restriction. During our sample period, this required visiting the licensing agency and passing a computer-based "hazard perception test" after 12 months on a P1 license. Prior to the introduction of the nighttime passenger restriction, graduating to a P2 license allowed driving at 100 kmh (63 mph) rather than 90 kmh (56 mph); it also started the two-year period needed for a full license. Given the relatively low costs of obtaining a P2 license, this is a good measure of how P1 drivers react to the costs of the nighttime passenger restriction.

We can use a regression discontinuity design to estimate if there was a change in the P2 transition rate after the nighttime passenger restriction was introduced. We focus on the fraction of P1 drivers transitioning to P2 in the first calendar month that they are eligible. This measures the response of those seeking to avoid the ban as soon as possible and also avoids anticipation

[28] We have already shown that our main results are similar using crashes per licensed driver or crash counts, which suggests that licensing changes are not an important mechanism for our results.

[29] Given the parallel pre-trends in relative license rates, we can formally estimate the effects on the rate of new P1 licenses among 17–19-year-olds in the post period using a differences-in-differences Poisson specification with individuals aged 20–23 as the comparison group. The estimate implies a statistically insignificant increase in new licenses by 4.5% ($p = 0.484$).

affecting inference.[30] There is no visually apparent change in the transition rate when the restriction was introduced, and no statistically significant discontinuity (Appendix Figure A10). The upper bound on the 95% confidence interval is 1.1 percentage points, or around six percent of the mean transition rate just before the restriction was introduced.[31]

These revealed-preference approaches suggest that the nighttime passenger restriction did not impose large costs on first-year drivers. A related question is the value of the benefits. We value the crash outcomes, which are likely to be the primary benefits of the restriction.[32] To do so, we use NSW Government valuations, which are consistent with international estimates for developed countries (Transport for NSW, 2019). The improvements over the first four years of driving are valued at A$412m (US$320m), or A$738 (US$570) per driver (see Appendix C).

In summary, we find meaningful crash improvements and no evidence of large costs. On top of data limitations, welfare analysis is complicated by potential changes in norms or social influences, but the available evidence suggests that the nighttime passenger restriction we study delivers higher net benefits than many other teen-driving interventions.

6 Conclusion

The introduction of a nighttime passenger restriction for first-year drivers in NSW led to substantial safety improvements relative to other studies. We estimate that the restriction saves 5.0 lives per 100,000 drivers in the first year of driving. We imprecisely estimate that a further 0.9 lives per 100,000 drivers are saved in subsequent years. Approximately 85% of teens are treated by this restriction, so 5.0–5.9 lives per 100,000 teen drivers equates to 77–91% of the Dee et al. (2005) estimate for the lives saved by stringent US graduated licensing systems. Our estimate is 45–53% that of Gilpin's (2019) estimate for more recent and stringent graduated licensing systems. As another comparison, our estimate is 73–102% of Huh and Reif's (2020) estimate

[30] P1 drivers eligible for a P2 license when the law took effect in July 2007 got their P1 license in July 2006, before the policy announcement. Moreover, they have no choice until July 2007, preventing endogenous sorting around the policy introduction date. Individuals get their licenses at a fairly uniform rate within the month, so this measures the fraction who get a P2 license within, on average, the first 15 days of being eligible to do so.

[31] We use the `rdrobust` command from Calonico et al. (2017), with an MSE-optimal bandwidth and uniform kernel. We report the bias-corrected coefficient and robust standard error.

[32] Reduced driving may also reduce teens' criminal activity, as Deza and Litwok (2016) find that graduated driving laws reduce crime at ages 16 and 17. We are unable to explore those effects in our data.

for the mortality effects of increasing the minimum driving age by one year.[33] Unlike in our setting, Gilpin (2019) finds that the gains from graduated licensing come from fewer licenses, while increasing the minimum driving age mechanically prevents teens from driving.

Other crash outcomes are also important, especially as many teens experience major physical and mental trauma in crashes (Holbrook et al., 2005; Gardner et al., 2007). Our estimates indicate that, for every 100,000 first-year drivers, the restriction led to 41 fewer hospitalizations, 98 fewer other injuries, and 164 fewer crashes with property damage. As discussed in the previous section, the improvements over the first four years of driving are valued at A$412m (US$320m), or A$738 (US$570) per driver.

Although our use of individual-level administrative data on licenses and crashes represents an advance over most studies of teen risky behavior, we face some inherent data limitations. We do not have information on teens' individual traffic penalties, their full set of transport options and choices, or the value they place on different types of driving. As a result, it is not possible to accurately measure the welfare costs of the restriction, although the persistent effects suggest that many teens are willing to forgo nighttime driving with peers.

It is also challenging to determine the exact ingredients that contribute to the success of this policy. Some level of enforcement is likely important for getting first-year drivers to comply with the new restriction, and the ability to detect violations was likely aided by first-year drivers having to display "P1" plates on their vehicles. However, this is not an unusual requirement, with probationary drivers subject to similar rules in other Australian states, several countries in Asia and Europe, and jurisdictions in Canada (British Columbia) and the US (New Jersey). As discussed in Section 2, NSW has traffic fatality rates that are comparable to other developed countries. Future research will be needed to determine the specific factors that contribute to the efficacy of targeted restrictions.

Our findings inform fundamental issues related to regulating risky behaviors, especially among teens. Everyday adult activities like driving, sexual activity, and consuming alcohol are especially risky to younger people. When designing laws and information campaigns, policy makers often choose between complete risk avoidance or trying to minimize risks while allowing youths to gain experience. Yet policymakers often fail to recognize that harsh restrictions can

[33] Traffic fatalities account for 84% of their savings, with the rest coming from reducing non-traffic causes like drug overdoses. Our range considers both their overall and traffic-specific estimate.

generate large costs and limit teens' experience.[34] This is evident in the case of driving, where policy makers in the United States, the European Union — and even NSW — have sought ever-stricter limits on teens.[35] Calls for applying graduated approaches to regulating other teen risky behaviors also often ignore this aspect.[36] At the other extreme, UK policy makers recognized these costs and decided against any graduated driving laws.[37]

Our results highlight the potential for using targeted restrictions as an alternative to harsh approaches to regulating risky behaviors (or not regulating them at all). Our study complements research showing targeted information on HIV can reduce teens' risky sex more than abstinence programs (Dupas, 2011), and a growing number of studies identifying policies that increase criminal deterrence without increasing the severity of punishments (e.g., Bhuller et al., 2020; Mueller-Smith and Schnepel, 2021; Rose, 2021). Targeted restrictions may bring about positive behavioral changes among teens that have been difficult to achieve through other means, and deserve more attention as a policy option for reducing risky or undesirable behaviors.

[34]For example, Argys et al. (2019) attribute approximately half of the substantial decline in US teen labor force participation since 1995 to the driving restrictions associated with graduated licensing schemes.

[35]Examples include a 2011 US Congressional Act that sought to introduce national standards that would prohibit drivers under 18 from doing any driving late at night or with more than one peer passenger at any time of the day (see `https://www.congress.gov/bill/112th-congress/senate-bill/528`); a 2006 conference of European transport ministers recommending many tougher restrictions on young drivers (see `https://www.internationaltransportforum.org/Pub/pdf/06YoungDrivers.pdf`); and a 2011 NSW Audit Office report that recommended first-year drivers be banned from carrying multiple peer passengers at any time of the day and also subject to broad nighttime driving curfews (see `https://www.audit.nsw.gov.au/sites/default/files/pdf-downloads/2011_Oct_Report_Improving_road_safety_young_drivers.pdf`). The NSW Audit Office did not evaluate the restriction we study.

[36]For example, a *Lancet* commission on adolescent health and well-being focused only on how graduated driving laws reduce crashes, and recommended that "consideration should now be given to applying graduated approaches to other aspects of adolescent policy" (Patton et al., 2016, p.78).

[37]For example, in 2021 the UK House of Commons Transport Committee decided against a graduated driving scheme for young drivers, citing "concerns over the impact such restrictions could have upon the social and economic opportunities available to young and novice drivers" (see page 4 of `https://committees.parliament.uk/publications/4871/documents/49009/default/`).

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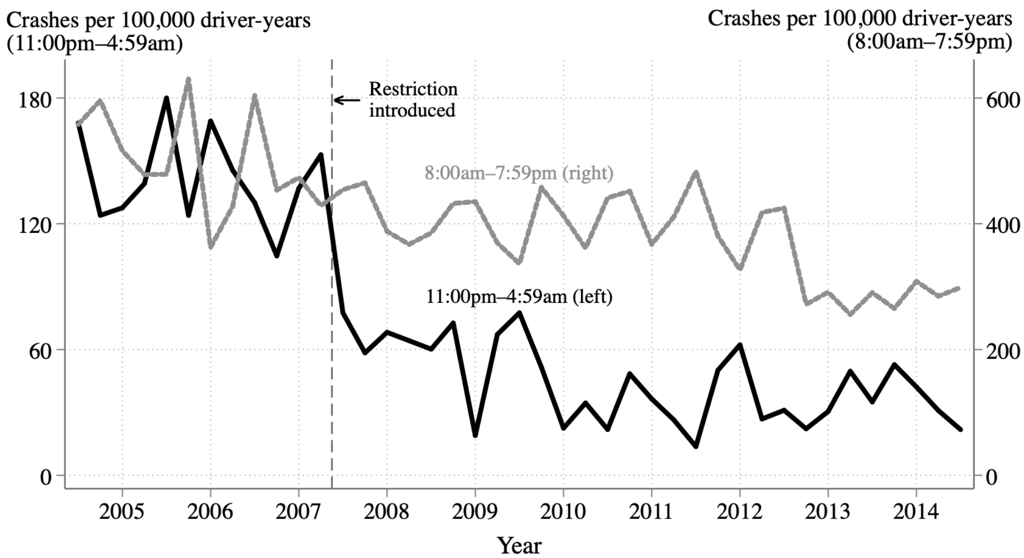
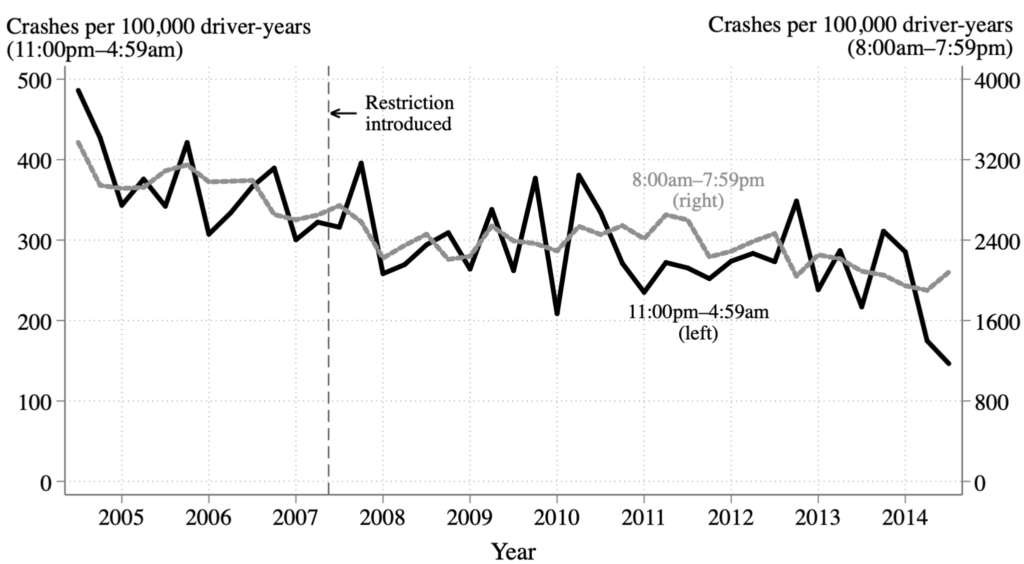
Figure 1: Crash rates of first-year drivers by the number of passengers and time of day

(a) Carrying two or more passengers

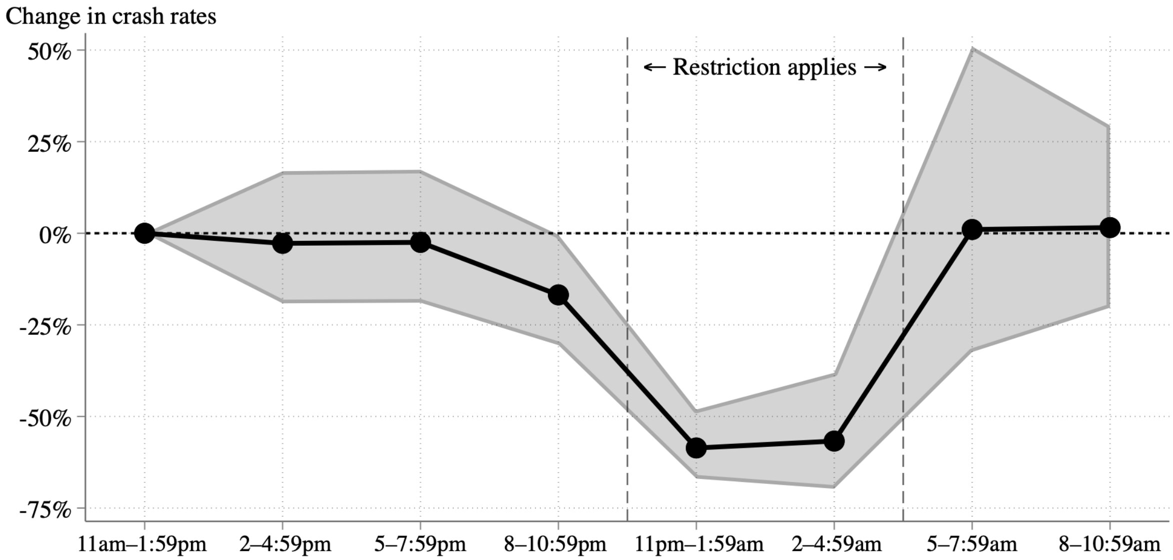


(b) Carrying zero or one passenger



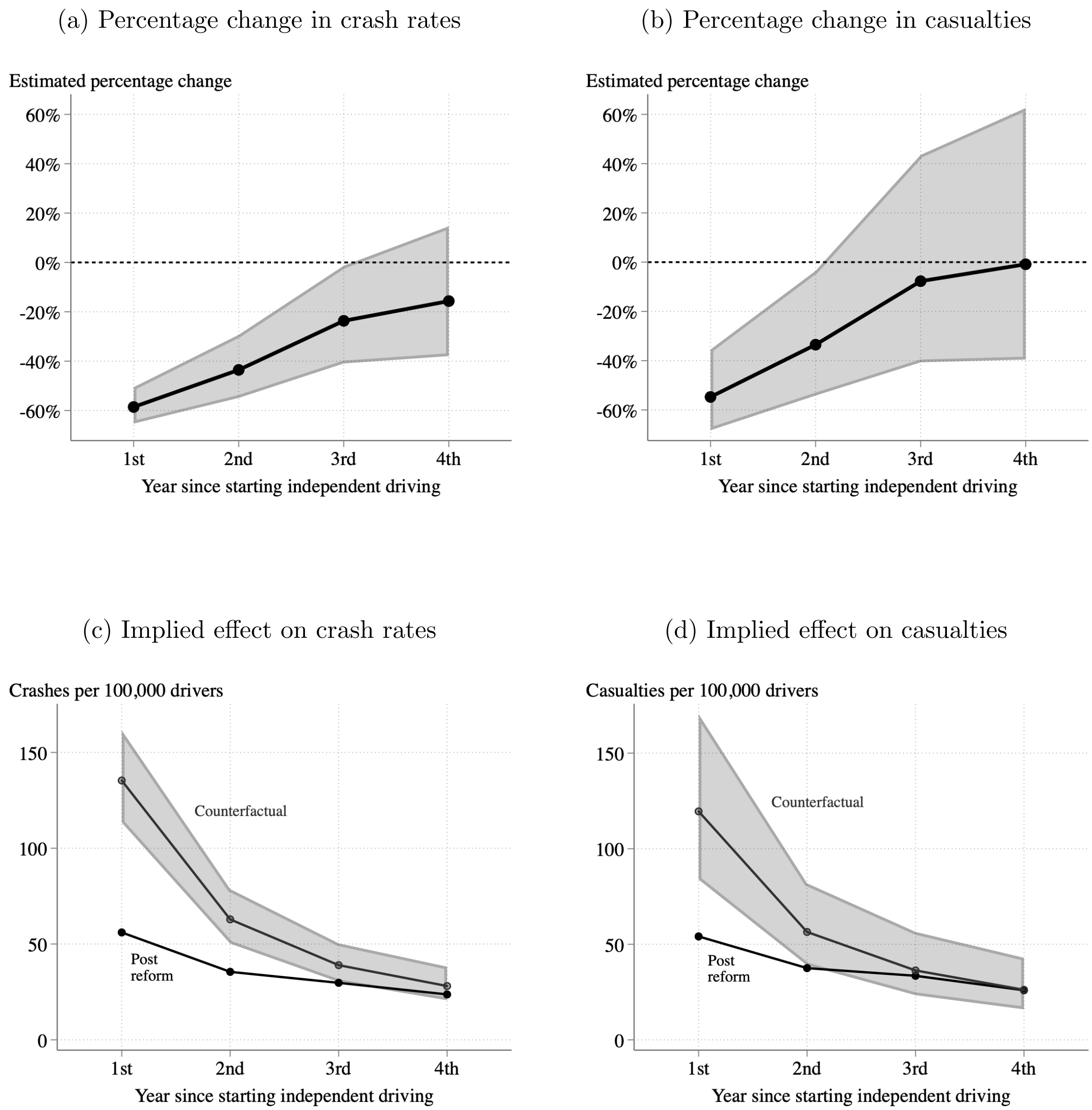
Notes: These figures show the quarterly crash rates of first-year drivers in NSW from July 2004 to September 2014. Panel (a) shows that there are similar trends in multi-passenger crash rates across the nighttime (11:00pm–4:59am) and daytime (8:00am–7:59pm) periods until the restriction is introduced. Immediately after its introduction, there is a reduction in nighttime crash rates relative to daytime crash rates. Panel (b) shows that the trends in crash rates with zero or one passenger are similar across the nighttime and daytime periods both before and after the introduction of the nighttime passenger restriction.

Figure 2: Estimated changes in multi-passenger crash rates of first-year drivers by time of day



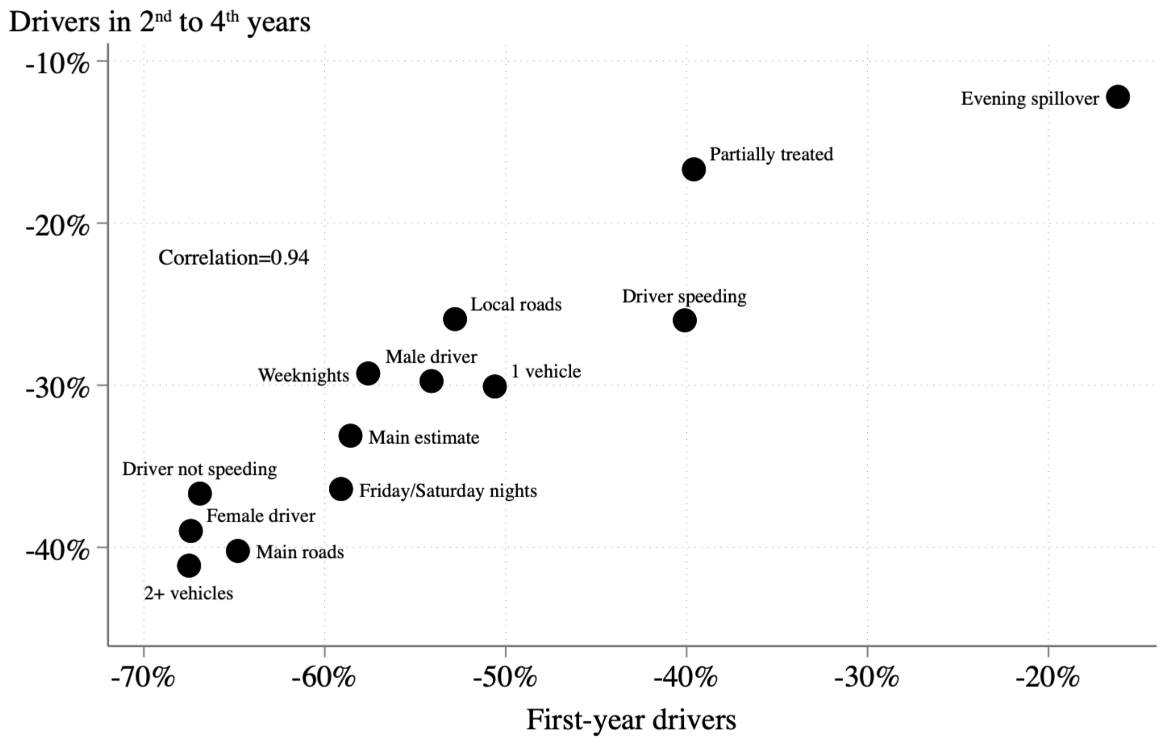
Notes: This figure shows the implied percentage changes and 95% confidence intervals in the multi-passenger crash rates of first-year drivers in three-hour periods of the day after the nighttime passenger restriction was introduced, relative to a 11:00am–1:59pm reference period. The estimates come from a version of equation (1) where the three period-of-day indicator variables are replaced with seven indicator variables identifying three-hour periods across the day. These estimates are based on the same data. The estimates show a similar percentage reduction in multi-passenger crash rates in the first three hours and second three hours of the restricted period, which are also similar to the main estimate in Table 1. There is a reduction in crashes in the three hours before the restricted period. Otherwise, the estimates are close to zero and not statistically significant.

Figure 3: Immediate and subsequent impacts on nighttime multi-passenger crashes



Notes: This figure shows the estimated effects and 95% confidence intervals for nighttime (11:00pm–4:59am) multi-passenger crashes and casualties in the first year and also the following three years (when drivers are not subject to the restriction). The estimates come from equation (3) using driver-license and crash data from June 2004 to September 2014. In panels (a) and (b), the coefficients are translated into percentage changes, while in panels (c) and (d) they are used to create counterfactual rates for drivers in the post-reform period. There are statistically significant reductions in crashes over the first three years and in casualties over the first two years, with a general pattern of the absolute effect size declining with years of driving. Appendix Table A7 presents the full estimates, including for the evening spillover and for hospitalizations/fatalities.

Figure 4: Correlation between first-year and longer-term treatment effect heterogeneity



Notes: The figure shows the correlation between the treatment effect heterogeneity estimates in the first year and next three years of driving for multi-passenger crashes in the restricted period (11:00pm–4:59am). We also present the "evening spillover" effect on multi-passenger crashes in the 8:00pm–10:59pm period to facilitate comparisons to the overall effect in the restricted period ("main estimate"). The estimates are based on equation (3) with a sample of never treated and fully treated drivers (except for the partially treated estimate, which includes partially treated and excludes fully treated drivers). The longer-term estimates on the vertical axis are a weighted average of the effects in years 2–4 based on the crash rates in each year of driving. All estimates use driver-license and crash data from June 2004 to September 2014. The figure shows that there is a strong positive correlation between the size of the treatment effects in the first year and the next three years of driving, when the restriction no longer applies. The 95% confidence intervals for all of these estimates are shown in Appendix Figure A7.

Table 1: Effect of the nighttime passenger restriction on first-year drivers' multi-passenger crash outcomes

	Differences-in-differences			Triple-differences		
	All crashes (1)	No. of casualties (2)	No. in hospital/ killed (3)	All crashes (4)	No. of casualties (5)	No. in. hospital/ killed (6)
Direct effect: 11pm–4:59am	-0.846	-0.696	-0.858	-0.801	-0.575	-0.759
	(0.087)	(0.167)	(0.268)	(0.104)	(0.168)	(0.271)
Implied percent change	-57.1%	-50.1%	-57.6%	-55.1%	-43.7%	-53.2%
Change per 100,000 drivers	-68.1	-57.6	-24.8	-62.9	-44.6	-20.7
Evening spillover: 8–10:59pm	-0.166	-0.300	-0.427	-0.116	-0.380	-0.486
	(0.067)	(0.121)	(0.243)	(0.071)	(0.127)	(0.225)
Implied percent change	-15.3%	-25.9%	-34.7%	-11.0%	-31.6%	-38.5%
Change per 100,000 drivers	-25.4	-40.7	-13.4	-17.4	-54.0	-15.7
Morning spillover: 5–7:59am	0.036	-0.073	-0.042	0.053	-0.237	-0.174
	(0.193)	(0.351)	(0.582)	(0.174)	(0.315)	(0.556)
Implied percent change	3.6%	-7.1%	-4.1%	5.4%	-21.1%	-16.0%
Change per 100,000 drivers	0.5	-1.0	-0.2	0.7	-3.6	-0.9
Observations	38,454,512	38,454,512	38,454,512	992	992	992

Notes: The estimates in columns 1–3 are based on equation (1), which is a Poisson difference-in-differences regression that uses daytime hours (8:00am–7:59pm) as the reference period and allows for spillover effects on nearby hours in the evening and morning. The estimates in columns 4–6 are based on equation (2), which is a Poisson triple-differences regression that uses drivers aged 25–39 as an additional comparison group. The samples come from linked administrative driver-license and crash records from June 2004 to September 2014. The regressions in columns 1–3 are at the driver-month-year-period level, while the regressions in columns 4–6 are at the group-month-year-period level. Standard errors in parentheses are shown, with clustering by driver in columns 1–3, and robust standard errors in columns 4–6. See the text for more details.

Table 2: Impact of the nighttime passenger restriction on different road users

	Total	First-year driver's car: Driver	First-year driver's car: Passengers	Other
	(1)	(2)	(3)	(4)
	Number of casualties			
Effect at 8pm–4:59am	-0.448	-0.340	-0.384	-0.644
	(0.107)	(0.122)	(0.143)	(0.158)
Implied percent change	-36.1%	-28.8%	-31.9%	-47.5%
Change per 100,000 drivers	-98.2	-21.8	-41.3	-28.8
	Number in hospital/killed			
Effect at 8pm–4:59am	-0.628	-0.129	-0.543	-1.277
	(0.206)	(0.306)	(0.264)	(0.355)
Implied percent change	-46.6%	-12.1%	-41.9%	-72.1%
Change per 100,000 drivers	-37.8	-1.6	-17.4	-19.2

Notes: This table presents the estimated changes in crash outcomes after the introduction of the nighttime passenger restriction for different road users. The Poisson estimates are based on a version of equation (1) with a combined evening and restricted period (8:00pm–4:59am) and daytime hours (8:00am–7:59pm) as the reference period. We allow for spillover effects on nearby hours in the morning but do not report these estimates (which are small and not statistically significant). The sample comes from linked administrative driver-license and crash records from June 2004 to September 2014. Estimates are based on separate regressions where a crash outcome is restricted to a specific group of road users (e.g., for the top panel of column (4), the dependent variable is the number of casualties among those outside of vehicles driven by first-year drivers: drivers/passengers in other vehicles and pedestrians). Each regression uses 28,840,884 observations, which is one-quarter less than in Table 1 because of the combined evening and restricted period. The results in this table show that first-year drivers account for a minority of the reductions in casualties and hospitalizations/deaths, with large benefits to other road users.

Table 3: Impact of the restriction on the crash rates of fully versus partially treated drivers

	Fully treated (1)	Partially treated (2)	p-value: (1) = (2)
1st year drivers (under treatment)	-0.887	-0.504	0.077
	(0.085)	(0.213)	
Implied percent change	-58.8%	-39.6%	
Change per 100,000 drivers	-68.3	-44.5	
Drivers in 2$^{\text{nd}}$–4$^{\text{th}}$ years	-0.409	-0.183	0.069
	(0.079)	(0.119)	
Implied percent change	-33.6%	-16.7%	
Change per 100,000 drivers	-47.5	-26.1	

Notes: This table presents the estimated changes in crash outcomes for drivers subject to the nighttime passenger restriction in their first full year of driving (fully treated) compared to drivers already in their first year when the restriction was introduced (partially treated). The Poisson estimates are based on a single regression from a version of equation (3) that allows for different effects for fully and partially treated drivers in each year of driving. The estimated effects on drivers in their 2$^{\text{nd}}$ to 4$^{\text{th}}$ years of driving are averaged to form one estimate based on the crash rates in each year of driving (and standard errors calculated using the delta method). The regression has 144,476,304 observations and is based on linked administrative driver-license and crash records from June 2004 to September 2014. The third column shows p-values from the hypothesis test that the reduction in crashes of fully treated drivers is the same as the reduction for partially treated drivers for a given period of driving experience.

Table 4: Impact of the restriction on crashes by compliance with speeding/alcohol laws

	Driver not speeding or drinking (1)	Driver speeding or drinking (2)	p-value: (1) = (2)
1st year drivers	-1.126	-0.555	0.003
	(0.121)	(0.145)	
Implied percent change	-67.6%	-42.6%	
Change per 100,000 drivers	-48.8	-18.8	
Drivers in 2^{nd}–4^{th} years	-0.482	-0.287	0.248
	(0.099)	(0.136)	
Implied percent change	-38.2%	-25.0%	
Change per 100,000 drivers	-35.2	-12.7	

Notes: This table presents the estimated changes in crash outcomes for drivers who are complying with speeding and alcohol regulations compared to those who are in breach of a regulation at the time of the accident. The Poisson estimates are based on a single regression from a version of equation (3) that allows for different effects on the crash rates of different types of accidents. The estimated effects on drivers in their 2^{nd} to 4^{th} years of driving are averaged to form one estimate based on the crash rates in each year of driving (standard errors are calculated using the delta method). The regression has 258,090,784 observations and is based on linked administrative driver-license and crash records from June 2004 to September 2014. The third column shows p-values from the hypothesis test that the reduction in crashes of each type is the same for a given amount of driving experience.

Web Appendix for "Shaping the Habits of Teen Drivers"

Timothy J. Moore   Todd Morris

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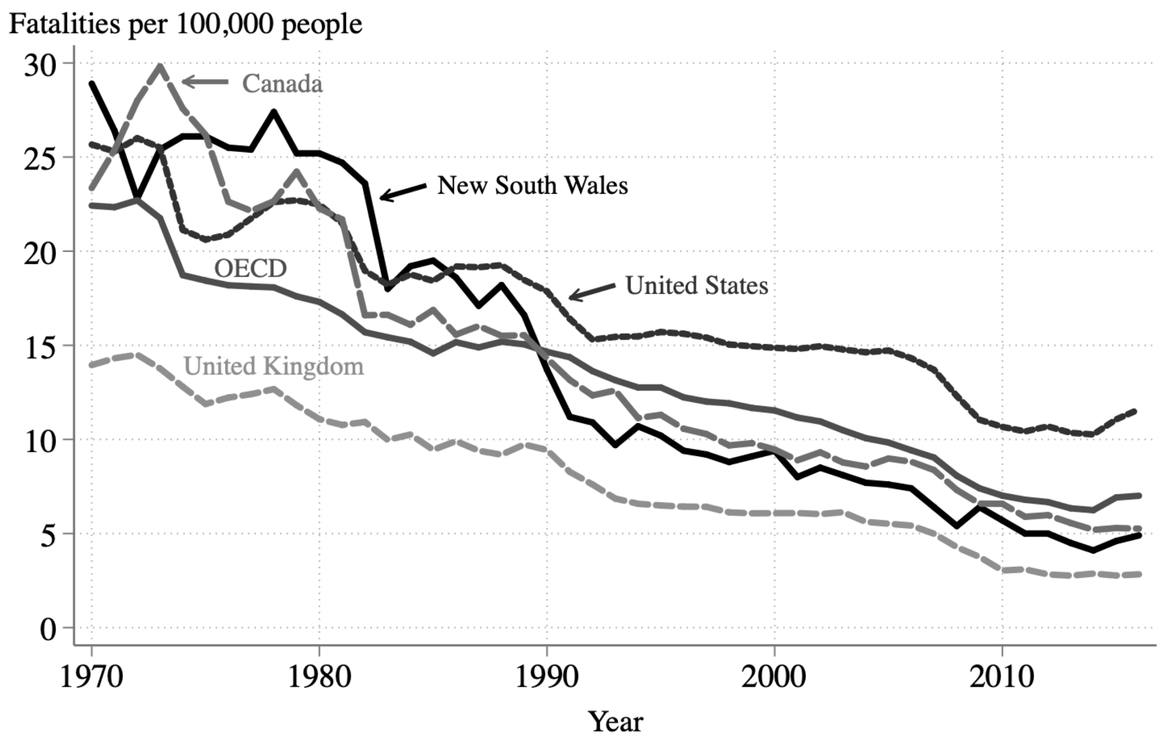
A Additional figures and tables

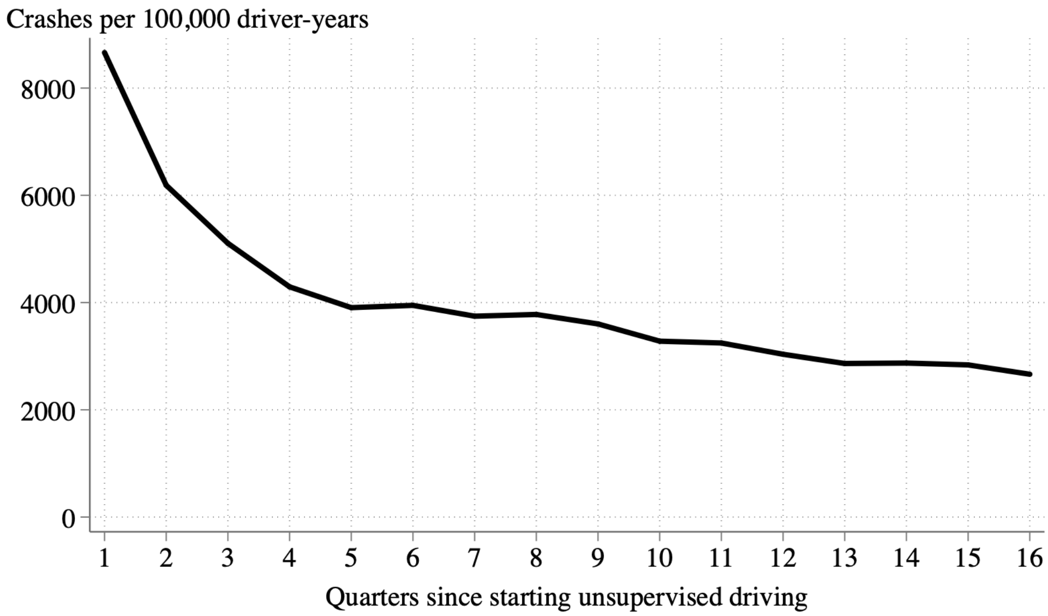
Figure A1: Traffic fatality rates in New South Wales compared to Canada, the United Kingdom, the United States and the OECD average: 1970–2016



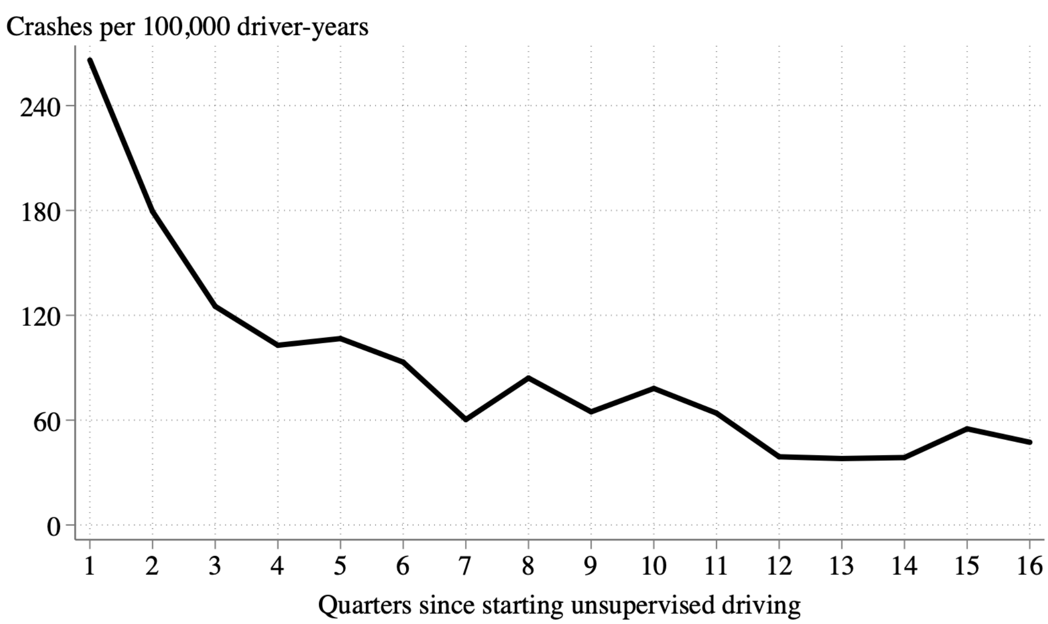
Notes: This figure shows the trends in traffic fatality rates in New South Wales and countries with similar traffic and legal systems. Sources: New South Wales Centre for Road Safety (`https://roadsafety.transport.nsw.gov.au/statistics/index.html`) and authors' calculations using the Organization for Economic Cooperation and Development (OECD) Road Accident and Population databases (`https://data.oecd.org/`). OECD data excludes suicides involving the use of motor vehicles. The OECD average is based on the 25 countries that were members of the OECD by 1973.

Figure A2: The pre-restriction relationship between experience and crash rates in NSW

(a) All crashes



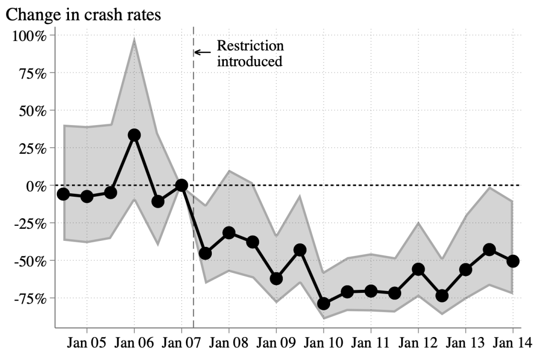
(b) Crashes with 2+ passengers, 11:00pm–4:59am



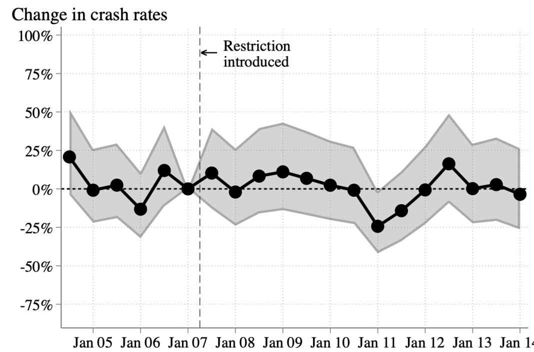
Notes: These figures show the crash rates as a function of driving experience before the introduction of the nighttime passenger restriction in NSW. Experience is the number of quarters since a probation (P1) license was first held. The sample includes crash records from June 2004 to June 2007 for all drivers who obtained a P1 license before age 24, which matches the sample restriction for our main sample.

Figure A3: Changes in crash rates of first-year drivers relative to the six months before the introduction of the nighttime passenger restriction, by number of passengers and time of day

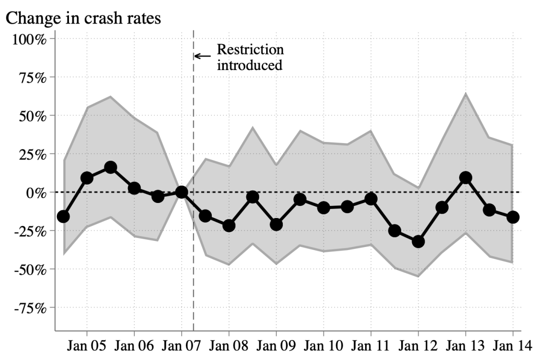
(a) 2+ passengers, 11:00pm–4:59am



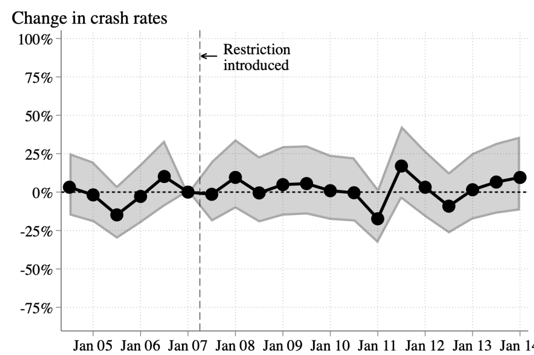
(b) 0/1 passenger, 11:00pm–4:59am



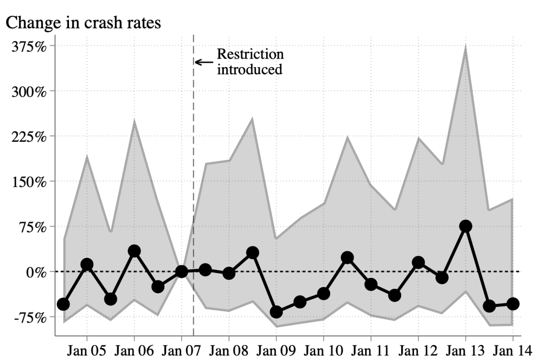
(c) 2+ passengers, 8:00pm–10:59pm



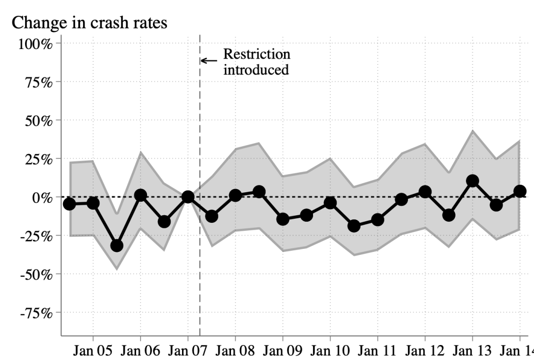
(d) 0/1 passenger, 8:00pm–10:59pm



(e) 2+ passengers, 5:00am–7:59am



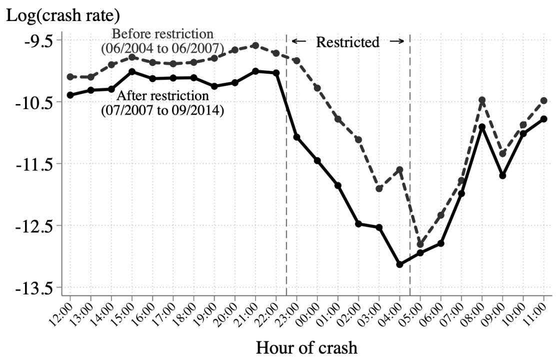
(f) 0/1 passenger, 5:00am–7:59am



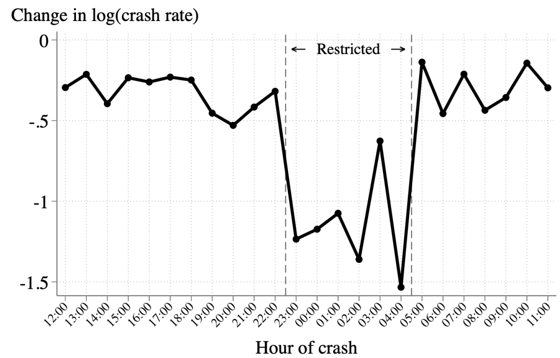
Notes: These figures show the percentage changes in crash rates, with 95% confidence intervals, of first-year drivers in different periods of the day relative to daytime crashes and the six months before the restriction was introduced. The specification is similar to equation (1), except we interact the period-of-day dummy variables with dummies spanning six-month periods. We use driver-license and crash data from July 2004 to June 2014 and our estimates come from separate regressions for multi-passenger crashes [panels (a), (c) and (e)] and crashes with 0/1 passenger [panels (b), (d) and (f)]. Panel (a) shows that there are no changes in multi-passenger crashes during 11:00pm–4:59am before the restriction and large, statistically significant reductions afterwards. Panel (b) shows no changes over time in crashes with 0/1 passenger during 11:00pm–4:59am. Both figures are consistent with the crash rates in Figure 1. The other panels show no statistically significant differences in crash rates in the evening and morning periods, although all but one of the post-restriction estimates for multi-passenger crashes in the evening period imply a reduction in crash rates.

Figure A4: Average hourly crash rates of first-year drivers before and after the reform

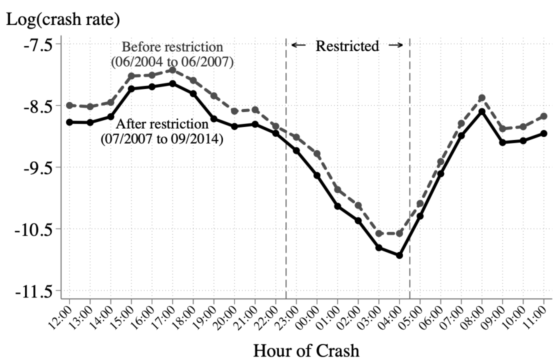
(a) Crash rates: 2+ passengers



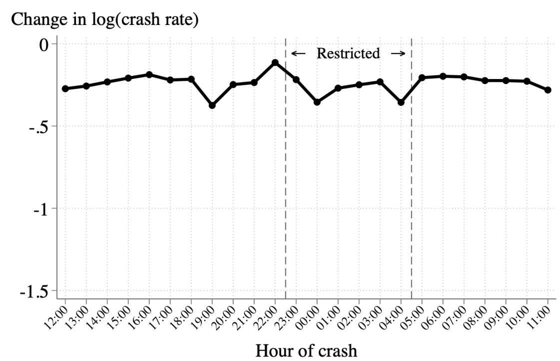
(b) Change in crash rates: 2+ passengers



(c) Crash rates: 0/1 passenger



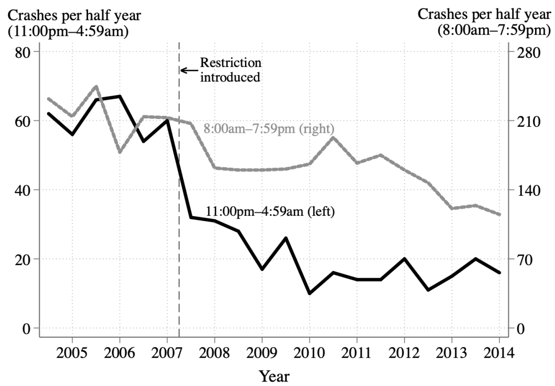
(d) Change in crash rates: 0/1 passenger



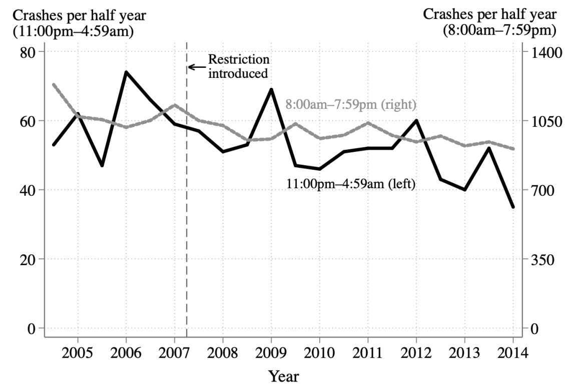
Notes: These figures show the log of the hourly crash rates before and after the introduction of the nighttime passenger restriction in July 2007. Panel (a) shows the rates for multi-passenger crashes. There is a reduction in crashes across the day, but the reduction is much larger in the 11:00pm to 4:59am hours than in the rest of the day. Panel (b) shows the change in the rates in panel (a), which confirms that the largest differences occur in the restricted period. The differences in the earlier evening hours are the next largest, while the differences in other hours are fairly similar. Panels (c) and (d) show the equivalent information for crashes with zero or one passenger. There is little visual evidence of these crashes having a different pattern in or near the restricted period to other hours of the day.

Figure A5: Crash counts of first-year drivers and drivers aged 25–39 by the number of passengers and time of day

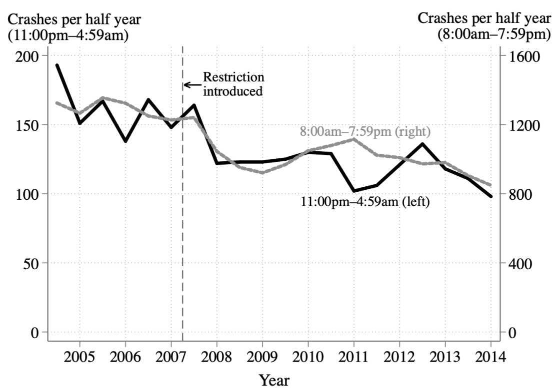
(a) First-year drivers: 2+ passengers



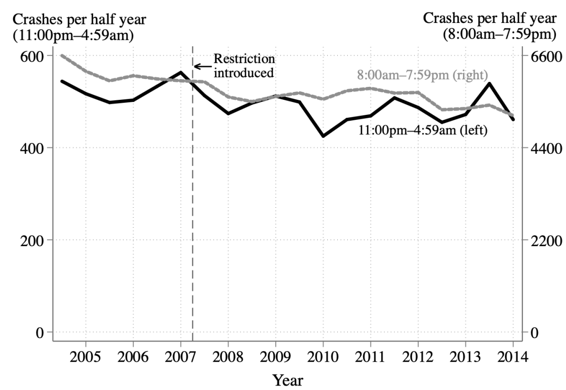
(b) Drivers aged 25–39: 2+ passengers



(c) First-year drivers: 0/1 passenger

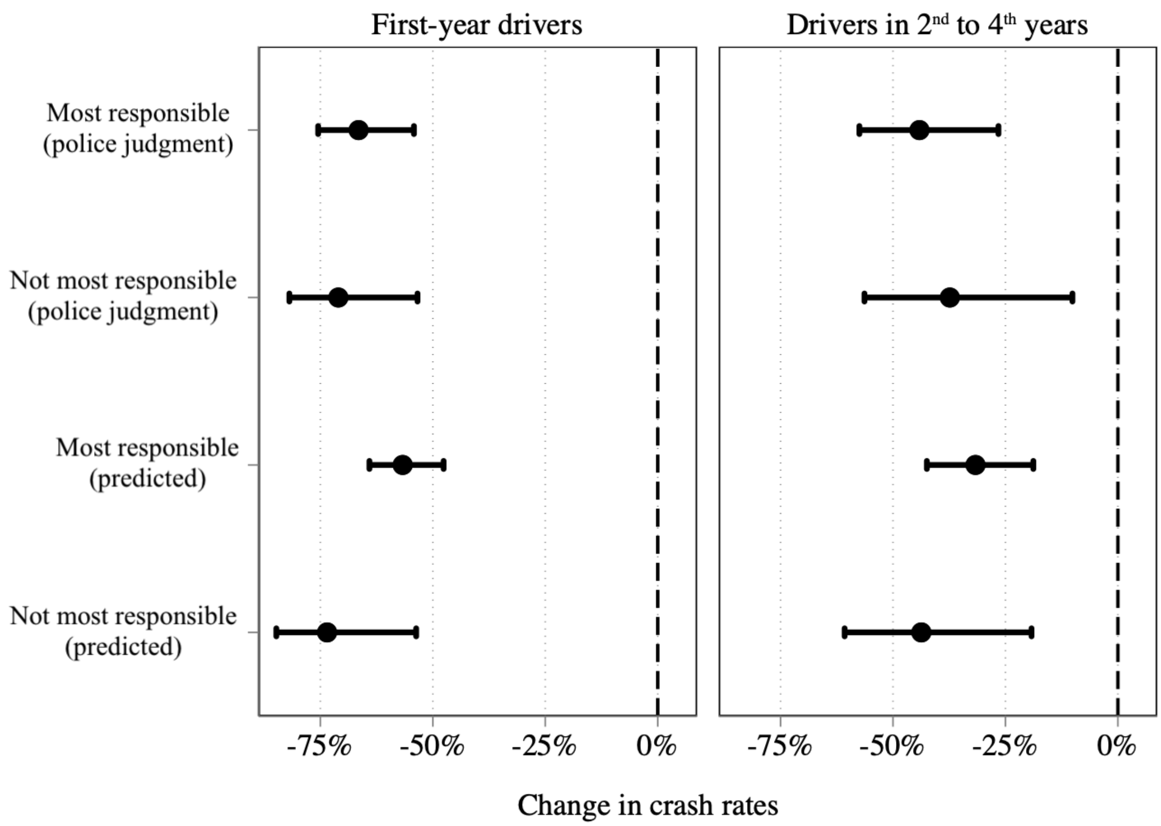


(d) Drivers aged 25–39: 0/1 passenger



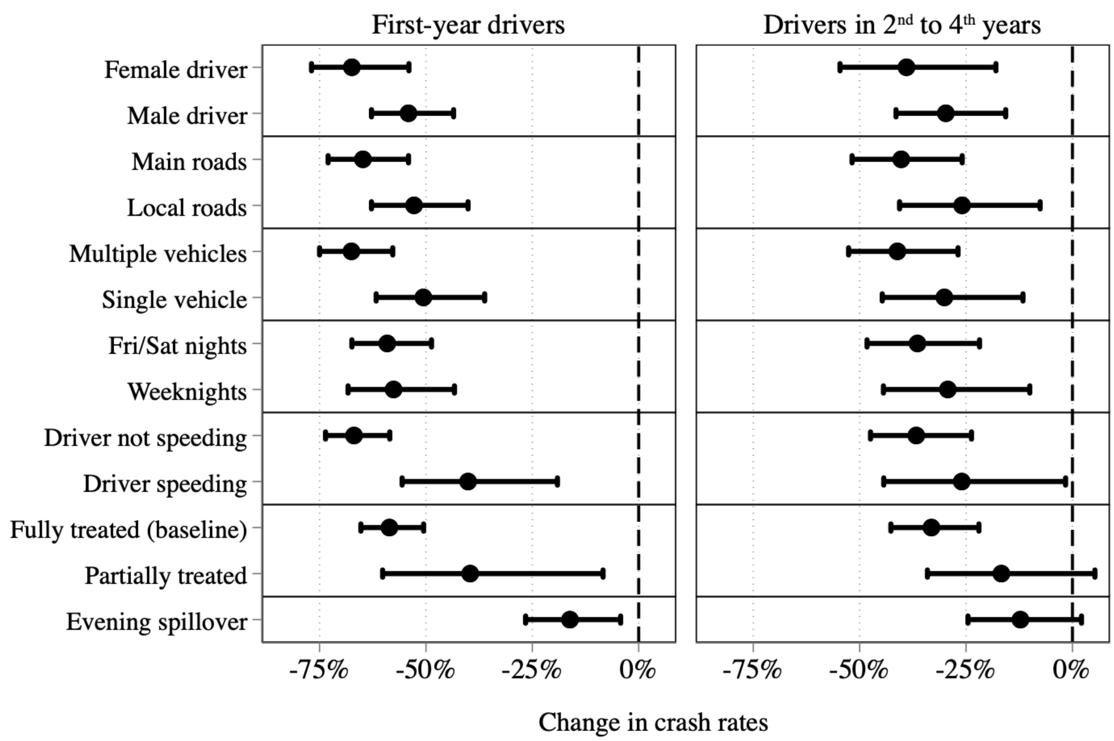
Notes: These figures show the six-monthly crash counts of first-year drivers and drivers aged 25–39 in NSW from July 2004 to June 2014. Panel (a) shows that, among first-year drivers, there are similar trends in multi-passenger crash counts across the nighttime (11:00pm–4:59am) and daytime (8:00am–7:59pm) periods until the restriction is introduced. Immediately after its introduction, there is a reduction in nighttime crashes relative to daytime crashes. Panel (c) shows similar trends in crashes for first-year drivers with zero or one passenger across the nighttime and daytime periods both before and after the introduction of the nighttime passenger restriction. Panels (b) and (d) show the corresponding crash counts over time for drivers aged 25–39. Crashes involving these drivers follow similar trends across the nighttime and daytime periods throughout the sample period (irrespective of the number of passengers).

Figure A6: Using multi-vehicle crash estimates by driver responsibility to measure changes in driving prevalence



Notes: This figure shows the estimated percentage changes and 95% confidence intervals in multi-vehicle crashes in the restricted period (11:00pm–4:59am) based on whether or not the young driver was most responsible for the crash. The estimates are based on equation (3) for fully treated drivers. The longer-term estimates on the right are a weighted average of the effects in years 2–4 based on the crash rates in each year of driving. Standard errors are calculated using the delta method. All estimates use driver-license and crash data from June 2004 to September 2014. Two measures of responsibility are used: (i) police reports and (ii) a machine-learning approach that assigns responsibility based on pre-reform crash data (see Appendix B for details). Both measures result in similar estimates. The figures show that the estimated reductions in nighttime multi-passenger crashes where first-year drivers/former first-year drivers were not responsible are similar to the estimates for multi-passenger crashes where they were responsible. If crashes for which first-year drivers are not responsible measure the amount they drive (rather than crash risks), then these results suggest that both the first-year and longer-term results may be entirely due to reductions in the amount of nighttime multi-passenger driving.

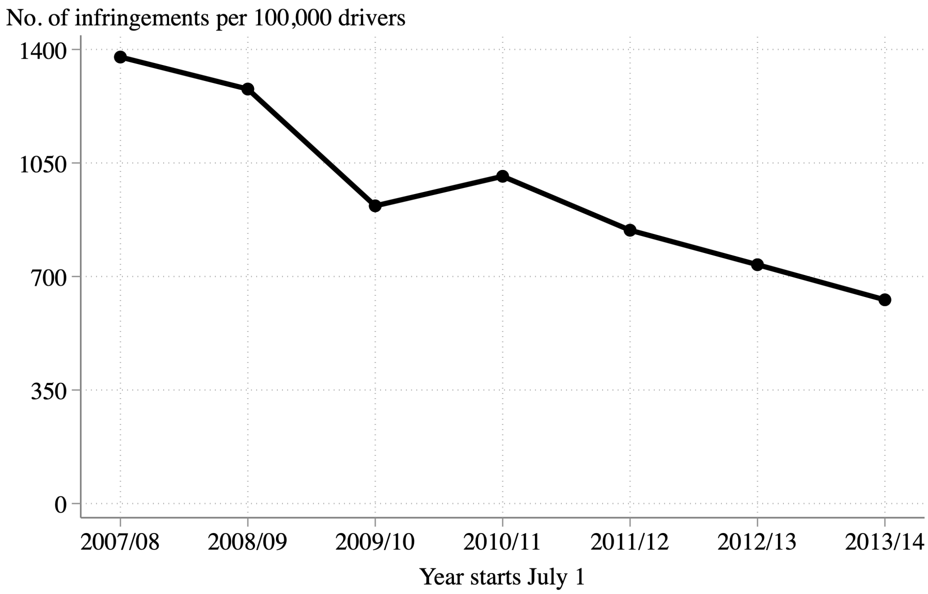
Figure A7: Relationship between first-year and longer-term heterogeneity in treatment effects



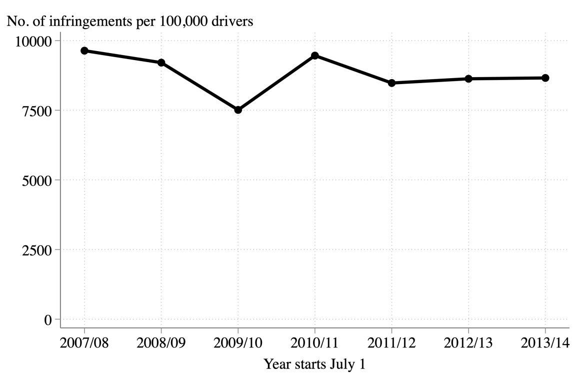
Notes: The figures show the treatment effect heterogeneity estimates for multi-passenger crashes in the restricted window (11:00pm–4:59am) in the first year of driving and for the next three years. We also present the "evening spillover" effect on multi-passenger crashes in the 8:00pm–10:59pm period to facilitate comparisons to the overall effect in the restricted period ("fully treated (baseline)"). The estimates are based on equation (3) with a sample of never treated and fully treated drivers (except for the partially treated estimate, which includes partially treated and excludes fully treated drivers). The longer-term estimates on the right are a weighted average of the effects in years 2–4 based on the crash rates in each year of driving. Standard errors are calculated using the delta method. All estimates use driver-license and crash data from June 2004 to September 2014. The point estimates are the same as in Figure 4; this figure adds the 95% confidence intervals.

Figure A8: Traffic infringement rates of P1 drivers by year and type of infringement

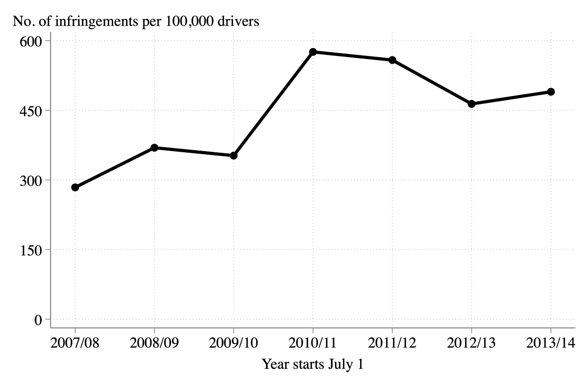
(a) Violating nighttime peer passenger restriction



(b) Not displaying license plates on vehicle

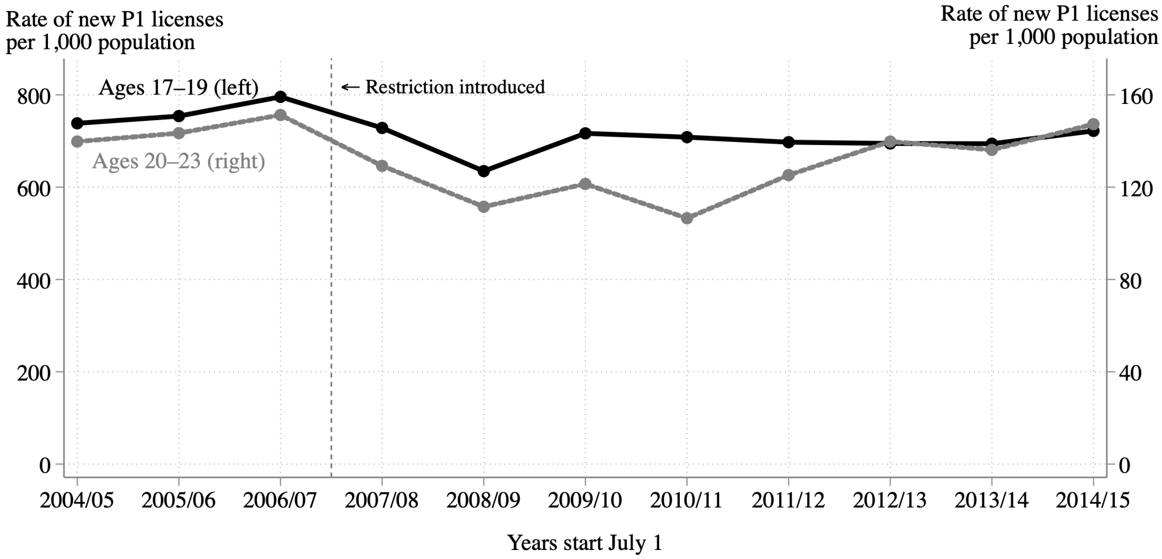


(c) Using mobile phone while driving



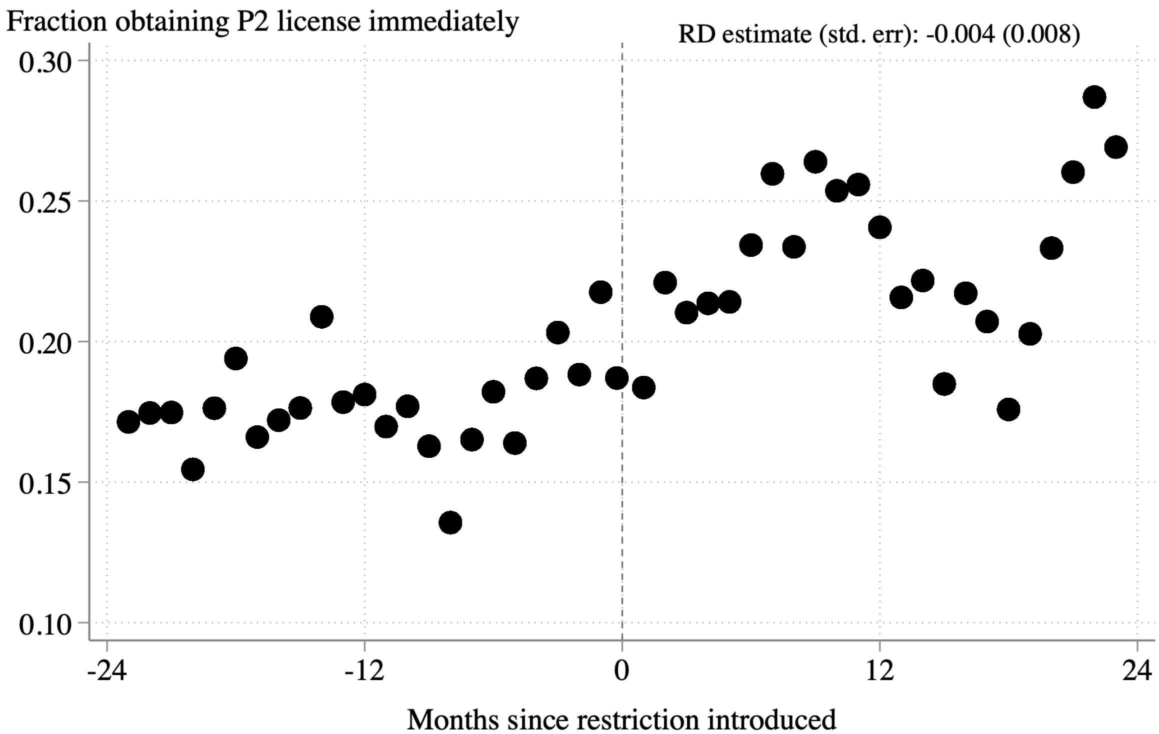
Notes: These figures show trends in the rate of infringements issued to P1 drivers over time in the years since the restriction was introduced. We scale the number of infringements by the number of P1 drivers in each year. Panel (a) shows that the infringement rate for the nighttime passenger restriction has fallen in the seven years since the restriction was introduced. This cannot be explained by a decline in the enforcement of road rules, since Panel (b) shows only a relatively modest decline in the infringement rate for not displaying P1 license plates and Panel (c) shows an increase in the infringement rate for cell phone use. Statistics for each infringement were provided to us after a request to the NSW Government. The denominator is the number of P1 drivers at the end of each year (all ages), available from `https://roads-waterways.transport.nsw.gov.au/cgi-bin/index.cgi?fuseaction=statstables.show&cat=Licensing`.

Figure A9: Trends in the annual rate of new first-year (P1) licenses by age



Notes: This figure shows the annual rate of new P1 licenses per 1,000 population in NSW by age. The figure shows a small decline in the license rate after the nighttime peer-passenger restriction is introduced in July 2007. The relative decline is similar for individuals aged 20–23 years, who are less likely to be constrained by the nighttime peer-passenger restriction (assuming they have friends of a similar age). This suggests that the restriction had no observable impacts on licensing.

Figure A10: Examining changes in the fraction of transitions from P1 to P2 licenses that occur in the first possible calendar month around the date the restriction was introduced



Notes: This figure shows, among individuals transitioning to a P2 license in a given calendar month, the fraction that obtained their P2 license in the first possible calendar month. On the horizontal axis is the number of months since July 2007, when the restriction was introduced. On average, an immediate transition requires drivers to obtain their P2 license within 15 days of eligibility. An increase in this rate after July 2007 could indicate that the nighttime passenger restriction imposes a meaningful cost that teen drivers want to avoid. We estimate the effect at the discontinuity using observations within 36 months either side of the restriction, a uniform kernel and the `rdrobust` command in Stata (Calonico et al., 2017). We present the bias-corrected RD estimate and standard error using the (optimal) bandwidth of 4 months.

Table A1: Pre-reform license and crash characteristics of first-year drivers

		Crashes	
	Licenses (1)	All (2)	2+ passengers & 11pm–4:59am (3)
License characteristics			
Male	51.7%	62.1%	70.8%
Age	18.8	18.1	17.7
(Std. dev)	(1.7)	(1.5)	(1.2)
Months held license	5.4	4.9	4.5
(Std. dev)	(3.5)	(3.3)	(3.3)
Unique drivers	315,837	13,056	377
Crash characteristics			
Number of casualties		0.58	0.85
(Std. dev)		(0.89)	(1.79)
Number hospitalized		0.089	0.220
(Std. dev)		(0.354)	(0.701)
Number killed		0.0054	0.0371
(Std. dev)		(0.0860)	(0.2894)
Number of passengers		0.7	2.8
(Std. dev)		(1.0)	(0.9)
Number of vehicles		1.9	1.6
(Std. dev)		(0.8)	(0.6)
Weekend (Friday 5:00am to Sunday 4:59am)		35.5%	66.0%
Daytime period: 8:00am–7:59pm		69.1%	0%
Restricted period: 11:00pm–4:59am		10.2%	100%
Evening period: 8:00–10:59pm		14.6%	0%
Morning period: 5:00–7:59am		6.1%	0%
In Sydney		55.7%	58.9%
In other urban area		36.6%	34.5%
Main road		51.4%	48.8%
Local road		48.6%	51.2%
First-year driver speeding		19.3%	33.7%
First-year driver had alcohol		2.0%	9.5%
Most responsible driver (police judgment)		76.2%	84.1%

Notes: This table summarizes the driver-license and crash records of first-year drivers prior to the reform. The sample comes from linked administrative driver-license and crash records from June 2004 to June 2007.

Table A2: Effect of varying spillover periods for multi-passenger crash outcomes

	Hours in each spillover period		
	2	3	4
	Crashes		
Evening spillover	-0.133	-0.166	-0.173
	(0.077)	(0.067)	(0.063)
Morning spillover	-0.064	0.036	-0.035
	(0.277)	(0.193)	(0.124)
	Number of casualties		
Evening spillover	-0.311	-0.300	-0.324
	(0.141)	(0.121)	(0.113)
Morning spillover	-0.150	-0.073	-0.158
	(0.523)	(0.351)	(0.223)
	Number in hospital/killed		
Evening spillover	-0.367	-0.427	-0.732
	(0.284)	(0.243)	(0.232)
Morning spillover	-0.783	-0.042	-0.518
	(0.652)	(0.582)	(0.473)

Notes: This table presents Poisson regression estimates using different lengths of time to measure the spillover effects of the nighttime passenger restriction on the multi-passenger crash outcomes of first-year drivers in hours near the restricted period. Equation (1) uses three-hour periods, denoted as the evening and morning periods. We present results using respective periods of two, three and four hours in columns (1), (2) and (3). The first two estimates come from regressions using a daytime reference period of 8:00am–7:59pm, while the third necessarily uses a narrower reference period of 9:00am–6:59pm to avoid overlap with the four-hour spillover periods. We estimate but do not report results for the restricted period. All regressions use the same controls; see the notes below Table 1 and the text for details. The sample comes from linked administrative driver-license and crash records from June 2004 to September 2014, and the number of observations in each regression is 38,454,512. The estimated changes in crash outcomes are similar across periods spanning two, three and four hours, with the estimates increasing in statistical significance as the number of hours increases. These results show that the estimates are robust to the measurement of spillovers. This is consistent with Appendix Figure A4, which shows the hourly crash rates before and after the restriction was introduced.

Table A3: Robustness of the main estimates to choice of regression model

	Main estimates (Carrying 2+ passengers)			Estimates for 0–1 passenger crashes		
	All crashes (1)	No. of casualties (2)	No. in hospital/ killed (3)	All crashes (4)	No. of casualties (5)	No. in hospital/ killed (6)
Panel A: Poisson with robust standard errors (without individual-level clustering)						
Direct effect: 11pm–4:59am	-0.846	-0.696	-0.858	-0.036	-0.064	-0.240
	(0.087)	(0.168)	(0.268)	(0.042)	(0.075)	(0.137)
Evening spillover: 8–10:59pm	-0.166	-0.300	-0.427	0.031	-0.001	-0.067
	(0.067)	(0.121)	(0.242)	(0.036)	(0.063)	(0.134)
Morning spillover: 5–7:59am	0.036	-0.073	-0.042	0.045	0.100	-0.076
	(0.193)	(0.351)	(0.582)	(0.047)	(0.078)	(0.169)
Panel B: Negative binomial						
Direct effect: 11pm–4:59am	-0.845	-0.691	-1.068	-0.036	-0.064	-0.254
	(0.087)	(0.144)	(0.217)	(0.042)	(0.074)	(0.133)
Evening spillover: 8–10:59pm	-0.164	-0.313	-0.550	0.033	-0.005	-0.078
	(0.067)	(0.118)	(0.198)	(0.036)	(0.062)	(0.129)
Morning spillover: 5–7:59am	0.043	-0.115	-0.236	0.044	0.094	-0.080
	(0.193)	(0.283)	(0.411)	(0.047)	(0.077)	(0.160)
Panel C: Poisson (aggregate count)						
Direct effect: 11pm–4:59am	-0.846	-0.703	-0.905	-0.038	-0.065	-0.230
	(0.080)	(0.135)	(0.230)	(0.035)	(0.071)	(0.112)
Evening spillover: 8–10:59pm	-0.170	-0.296	-0.412	0.027	-0.004	-0.062
	(0.054)	(0.102)	(0.196)	(0.031)	(0.055)	(0.122)
Morning spillover: 5–7:59am	0.051	-0.115	-0.109	0.046	0.098	-0.074
	(0.160)	(0.297)	(0.521)	(0.041)	(0.077)	(0.155)
Panel D: Log (aggregate count + 1)						
Direct effect: 11pm–4:59am	-0.735	-0.609	-0.557	-0.027	-0.046	-0.461
	(0.085)	(0.151)	(0.198)	(0.045)	(0.086)	(0.145)
Evening spillover: 8–10:59pm	-0.154	-0.323	-0.416	0.026	0.003	-0.198
	(0.078)	(0.149)	(0.183)	(0.041)	(0.076)	(0.149)
Morning spillover: 5–7:59am	0.156	0.045	-0.192	0.065	0.123	-0.289
	(0.092)	(0.152)	(0.174)	(0.052)	(0.089)	(0.161)

Notes: This table compares the estimates from different specifications of the effects of the nighttime passenger restriction on the crash outcomes of first-year drivers. Panel A reproduces the individual-level Poisson estimates in Table 1. Panel B shows the estimates from an equivalent Negative binomial model (with standard errors clustered at the individual level). Each regression in these panels uses 38,454,512 observations at the driver-month-year-period level. The regressions in Panels C and D use 496 observations at the month-year-period level with robust standard errors. This requires us to drop individual-level controls, but all other controls are maintained. Panel C shows Poisson estimates and Panel D shows OLS estimates where the dependent variable is transformed from y to $\ln(y + 1)$. See the notes in Table 1 and the text for more details. The table shows that the estimates are similar across all specifications.

Table A4: Robustness of main estimates to different samples

	Main estimates (Carrying 2+ passengers)			Estimates for 0–1 passenger crashes		
	All crashes (1)	No. of casualties (2)	No. in hospital/ killed (3)	All crashes (4)	No. of casualties (5)	No. in hospital/ killed (6)
Panel A: First-year drivers (main sample)						
Direct effect: 11pm–4:59am	-0.846	-0.696	-0.858	-0.036	-0.064	-0.240
	(0.087)	(0.167)	(0.268)	(0.042)	(0.075)	(0.137)
Implied percent change	-57.1%	-50.1%	-57.6%	-3.5%	-6.2%	-21.3%
Evening spillover: 8–10:59pm	-0.166	-0.300	-0.427	0.031	-0.001	-0.067
	(0.067)	(0.121)	(0.243)	(0.036)	(0.063)	(0.134)
Implied percent change	-15.3%	-25.9%	-34.7%	3.2%	-0.1%	-6.5%
Morning spillover: 5–7:59am	0.036	-0.073	-0.042	0.045	0.100	-0.076
	(0.193)	(0.351)	(0.582)	(0.047)	(0.078)	(0.169)
Implied percent change	3.6%	-7.1%	-4.1%	4.6%	10.6%	-7.3%
Panel B: First-year drivers who got licenses by age 19						
Direct effect: 11pm–4:59am	-0.880	-0.722	-0.959	-0.034	-0.101	-0.293
	(0.091)	(0.175)	(0.283)	(0.045)	(0.080)	(0.145)
Implied percent change	-58.5%	-51.4%	-61.7%	-3.4%	-9.6%	-25.4%
Evening spillover: 8–10:59pm	-0.162	-0.254	-0.437	0.036	-0.029	-0.044
	(0.069)	(0.125)	(0.253)	(0.038)	(0.068)	(0.143)
Implied percent change	-15.0%	-22.4%	-35.4%	3.6%	-2.9%	-4.3%
Morning spillover: 5–7:59am	0.028	-0.063	-0.073	0.049	0.098	-0.138
	(0.203)	(0.366)	(0.638)	(0.051)	(0.085)	(0.183)
Implied percent change	2.8%	-6.1%	-7.1%	5.0%	10.3%	-12.9%
Panel C: All P1 drivers aged under 25						
Direct effect: 11pm–4:59am	-0.810	-0.722	-0.748	-0.017	-0.022	-0.198
	(0.076)	(0.143)	(0.232)	(0.037)	(0.065)	(0.117)
Implied percent change	-55.5%	-51.4%	-52.6%	-1.7%	-2.2%	-17.9%
Evening spillover: 8–10:59pm	-0.151	-0.297	-0.431	0.025	0.002	-0.114
	(0.061)	(0.109)	(0.215)	(0.031)	(0.055)	(0.114)
Implied percent change	-14.0%	-25.7%	-35.0%	2.5%	0.2%	-10.8%
Morning spillover: 5–7:59am	-0.040	-0.222	-0.458	0.067	0.165	0.118
	(0.164)	(0.288)	(0.451)	(0.040)	(0.067)	(0.142)
Implied percent change	-3.9%	-19.9%	-36.7%	6.9%	17.9%	12.5%

Notes: This table shows that the main estimates in Table 1 are similar if we restrict the sample to first-year drivers who got their license as teenagers (Panel B) or all P1 drivers aged under 25 (Panel C). The number of observations in each regression is 38,454,512 in Panel A, 32,524,248 in Panel B and 59,600,476 in Panel C. See the notes in Table 1 and the text for more details.

Table A5: The effect of the nighttime passenger restriction on the crash outcomes of first-year drivers from trips with zero or one passenger

	Differences-in-differences			Triple-differences		
	All crashes (1)	No. of casualties (2)	No. in hospital/ killed (3)	All crashes (4)	No. of casualties (5)	No. in. hospital/ killed (6)
Direct effect: 11pm–4:59am	-0.036	-0.064	-0.240	-0.055	-0.021	-0.021
	(0.042)	(0.075)	(0.137)	(0.042)	(0.078)	(0.126)
Implied percent change	-3.5%	-6.2%	-21.3%	-5.3%	-2.1%	-2.1%
Change per 100,000 drivers	-11.8	-10.7	-12.7	-18.2	-3.5	-1.0
Evening spillover: 8–10:59pm	0.031	-0.001	-0.067	-0.043	-0.117	-0.136
	(0.036)	(0.063)	(0.134)	(0.037)	(0.063)	(0.134)
Implied percent change	3.2%	-0.1%	-6.5%	-4.2%	-11.1%	-12.7%
Change per 100,000 drivers	15.6	-0.2	-4.3	-22.2	-35.0	-9.1
Morning spillover: 5–7:59am	0.045	0.100	-0.076	0.004	0.003	-0.218
	(0.047)	(0.078)	(0.169)	(0.045)	(0.082)	(0.165)
Implied percent change	4.6%	10.6%	-7.3%	0.4%	0.3%	-19.6%
Change per 100,000 drivers	12.1	14.6	-2.6	1.1	0.4	-8.1
Observations	38,454,512	38,454,512	38,454,512	992	992	992

Notes: This table presents the estimated changes in the crash outcomes of first-year drivers from trips with zero or one passenger after the introduction of the restriction on carrying multiple passengers between 11:00pm and 4:59am. The estimates in columns 1–3 are based on equation (1), which is a Poisson difference-in-differences regression that uses daytime hours (8:00am–7:59pm) as the reference period and allows for spillover effects on nearby hours in the evening and morning. The estimates in columns 4–6 are based on equation (2), which is a Poisson triple-differences regression that uses drivers aged 25–39 as an additional comparison group. The samples come from linked administrative driver-license and crash records from June 2004 to September 2014. The regressions in columns 1–3 are at the driver-month-year-period level, while the regressions in columns 4–6 are at the group-month-year-period level. Standard errors in parentheses are shown, with clustering by driver in columns 1–3, and robust standard errors in columns 4–6. See the text for more details.

Table A6: Impact of the restriction on teen casualties as non-peer passengers and pedestrians

	All non-drivers (1)	Non-peer passengers (2)	Pedestrians (3)
Direct effect: 11pm–4:59am	-0.102	-0.108	-0.065
	(0.058)	(0.070)	(0.090)
Implied percent change	-9.7%	-10.3%	-6.3%
Change per 100,000 drivers	-9.9	-7.9	-1.6
Evening spillover: 8pm–10:59pm	-0.037	-0.046	-0.014
	(0.073)	(0.085)	(0.119)
Implied percent change	-3.6%	-4.5%	-1.4%
Change per 100,000 drivers	-2.4	-2.2	-0.2
Morning spillover: 5–7:59am	-0.019	0.092	-0.294
	(0.104)	(0.125)	(0.163)
Implied percent change	-1.9%	9.6%	-25.5%
Change per 100,000 drivers	-0.5	1.9	-2.3

Notes: This table examines whether there was any increase in casualties among individuals aged 16–20 resulting from additional trips as pedestrians or non-peer passengers. The Poisson estimates are based on a version of equation (1) with casualty counts at the month-year-period level. Individual-level controls are not possible but all other controls are maintained. We exclude casualties from crashes that directly involve a P1 driver aged under 25 to avoid capturing any direct effects of the restriction. The number of observations in each regression is 496. The sample comes from administrative crash records and covers the period from June 2004 to September 2014. The estimates in this table are small and not statistically significant, suggesting that there were no large spillovers on casualties from additional trips as pedestrians or non-peer passengers.

Table A7: Effect on nighttime multi-passenger crashes and casualties by year of driving

	All crashes (1)	No. of casualties (2)	No. in. hospital/killed (3)	Property damage only (4)
Direct effect: 11pm–4:59am				
1st year drivers	-0.881	-0.793	-0.990	-0.961
	(0.090)	(0.181)	(0.277)	(0.119)
Implied percent change	-58.6%	-54.7%	-62.8%	-61.8%
Change per 100,000 drivers	-74.8	-68.6	-31.1	-45.1
2nd year drivers	-0.572	-0.408	-0.210	-0.588
	(0.115)	(0.190)	(0.308)	(0.163)
Implied percent change	-43.5%	-33.5%	-18.9%	-44.4%
Change per 100,000 drivers	-27.2	-18.4	-2.6	-13.5
3rd year drivers	-0.270	-0.080	-0.110	-0.384
	(0.131)	(0.225)	(0.370)	(0.185)
Implied percent change	-23.7%	-7.7%	-10.4%	-31.9%
Change per 100,000 drivers	-9.1	-2.8	-1.4	-6.5
4th year drivers	-0.170	-0.009	0.056	-0.397
	(0.157)	(0.252)	(0.491)	(0.235)
Implied percent change	-15.7%	-0.9%	5.8%	-32.8%
Change per 100,000 drivers	-4.3	-0.2	0.4	-4.8
Evening spillover: 8–10:59pm				
1st year drivers	-0.176	-0.275	-0.447	-0.116
	(0.068)	(0.122)	(0.236)	(0.089)
Implied percent change	-16.2%	-24.1%	-36.0%	-10.9%
Change per 100,000 drivers	-31.7	-43.8	-16.8	-11.4
2nd year drivers	-0.051	-0.141	-0.010	0.044
	(0.106)	(0.181)	(0.322)	(0.146)
Implied percent change	-5.0%	-13.2%	-1.0%	4.5%
Change per 100,000 drivers	-2.8	-7.4	-0.1	1.3
3rd year drivers	-0.168	-0.006	0.041	-0.272
	(0.130)	(0.226)	(0.399)	(0.179)
Implied percent change	-15.5%	-0.6%	4.2%	-23.8%
Change per 100,000 drivers	-5.8	-0.2	0.3	-5.1
4th year drivers	-0.318	-0.527	-0.469	-0.101
	(0.163)	(0.314)	(0.515)	(0.211)
Implied percent change	-27.2%	-40.9%	-37.5%	-9.6%
Change per 100,000 drivers	-8.0	-10.4	-2.7	-1.4

Notes: This table shows the estimated effects for nighttime multi-passenger crash outcomes in the first four years of driving. The Poisson estimates come from equation (3) using driver-license and crash data from June 2004 to September 2014, and each regression uses 129,045,392 observations. The estimates in columns (1) and (2) for the 11:00pm–4:59am period are also presented in Figure 3. In this table, we also estimate the effect on the number of crashes where there is property damage but no casualties, which we include to value the effects of restriction.

B Predicting the most-responsible driver in multi-vehicle crashes

For each crash in our data, the police make a judgment of the key vehicle causing the crash. These judgments are primarily based on the maneuvers of each vehicle prior to the crash (e.g., turning right, proceeding straight, stationary), which are included in our data. To minimize concerns that police judgments may change after the restriction was introduced (conditional on crash characteristics), we predict the most-responsible vehicle in all multi-vehicle crashes involving drivers in their first four years of driving who were carrying multiple passengers.

We use a logit Least Absolute Shrinkage and Selection Operator (LASSO) estimator to predict crash responsibility from pre-reform crashes. We allow the LASSO estimator to use several different types of variables to predict whether the reference vehicle (containing the first-to-fourth-year driver) is most responsible for the crash. We use variables measuring the:

(i) Maneuvers of the reference vehicle: dummy variables for stationary/parked; pulling out from a driveway; turning right or performing a U-turn (Australians drive on the left); turning left; proceeding straight in lane; waiting to turn; veering to change/merge lanes; driving on incorrect side of the road;

(ii) Maneuvers of other vehicles: we include the sum of the equivalent maneuver dummy variables in (i) for all other drivers involved in the crash, since some crashes involve more than two vehicles;

(iii) Type of impact: dummy variables for head-on crashes; rear-end crashes; right-angle crashes; other-angle crashes; vehicle-pedestrian crashes;

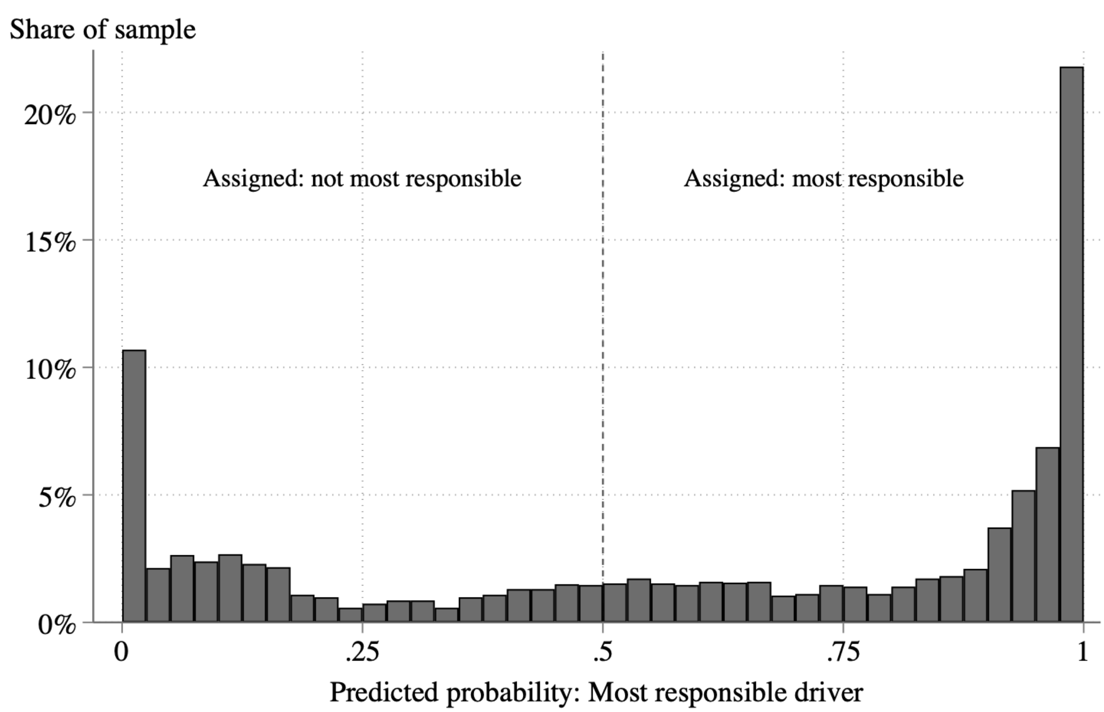
(iv) Characteristics of the reference driver: dummy variables for the different license types (Learner, P1, P2, Unrestricted); a dummy variable for whether the reference driver's vehicle was towed; dummy variables for whether the driver was speeding, fatigued, and had a blood alcohol concentration level above the limit; a male dummy variable; age in months; months of driving experience; dummy variables for whether the driver was killed and injured; and the number of people in the vehicle;

(v) Characteristics of all other drivers: we use the same characteristics as in (iv) and again sum across individual variables;

(vi) Crash characteristics: dummy variables for the type of road (undivided two-way, divided road, dual freeway, T-intersection, other intersection); a dummy variable for being on local roads; dummy variables for the time period of the crash: daytime (8:00am–7:59pm), evening (8:00pm–10:59pm), nighttime (11:00pm–4:59am) or morning (5:00am–7:59am); dummy variables for whether the crash occurred in daylight or at dusk/dawn; dummy variables for crashes in Sydney and rural areas; dummy variables for the different speed limits of the road; the number of vehicles involved; the total number of people involved in the crash; a dummy variable for the crash being on a curved road; dummy variables for the crash involving at least one casualty and at least one serious casualty; and the sum of the age of all drivers involved in the crash.

Appendix Figure B1 shows the distribution of the fitted values from our LASSO model for pre-reform multi-vehicle crashes. Most of the mass is located close to probability 0 (not the most responsible driver) or probability 1 (the most responsible driver).

Figure B1: Histogram of predicted probabilities for the younger driver being most responsible for multi-vehicle crashes before the restriction was introduced

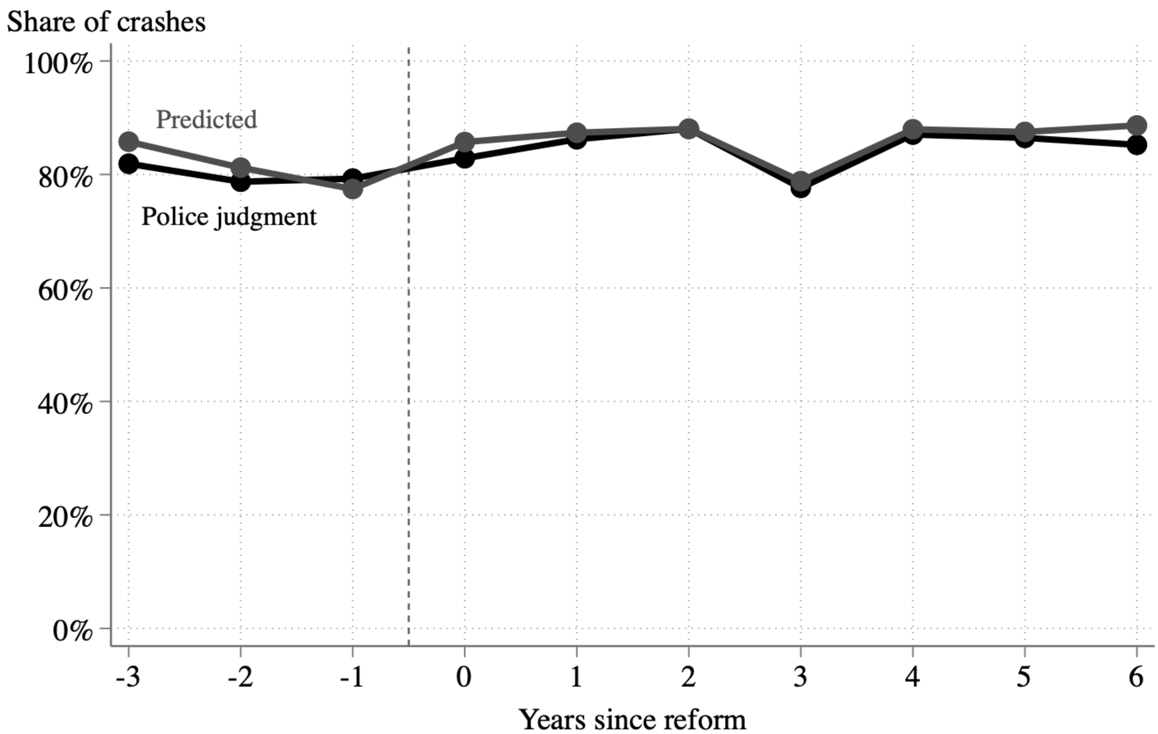


Based on an assignment rule where drivers with predicted probabilities greater than 0.5 are assigned to be the most responsible driver, our machine-learning model predicts that young drivers caused 61.8% of multi-vehicle crashes prior to the reform. This closely matches the 59.6% reported in police judgments. Moreover, the model correctly predicts the most responsible driver in 88.0% of crashes. Importantly, we find that the model predicts the most responsible driver with a similar level of accuracy after the reform (86.9%), suggesting that we are not over-fitting the data. There is also a similarly high level of accuracy before and after the restriction if we focus on crashes during the restricted period (86.0% before and 86.4% after). This consistency minimizes concerns that police judgments may have changed once dealing with first-year drivers who were violating the nighttime passenger restriction.

Overall, given the high predictive power of our LASSO estimates of crash responsibility, it is no surprise that we find little difference in the estimates based on machine learning or police judgments. Appendix Figure A6 shows the estimates for the effects of the restriction on multi-vehicle crashes where the younger driver was and was not the most responsible. Both measures produce similar estimates and imply the crash reductions do not vary much with responsibility.

Consistent with this, there is little change over time in the proportion of nighttime multi-passenger crashes in which the young driver is most responsible. For the 11:00pm–4:59am period, Appendix Figure B2 shows the proportion of multi-passenger crashes caused by the driver in their first four years of driving for the three years before the restriction and seven years after. Using both police judgment and the machine-learning estimates, there is no evidence that the share of crashes caused by younger drivers changed after the restriction was introduced. We find a similar pattern if we restrict the sample to first-year drivers (not shown). Nighttime multi-passenger crashes not caused by younger drivers are declining in proportion to the decrease in all nighttime multi-passenger crashes.

Figure B2: Share of multi-passenger crashes 11:00pm–4:59am caused by the younger driver



C Aggregating and valuing crash outcomes

In this section, we provide more details about how we value the effects of the nighttime passenger restriction. All values are in 2019 Australian dollars, unless otherwise stated.

We use the values used by the NSW Government (Transport for NSW, 2019). Each traffic fatality is valued at \$7.75 million. For comparison, this is slightly lower than the value of statistical life estimate for Australia from Viscusi (2018) of \$10 million in 2015 dollars, or \$10.9 million in 2019 dollars once updated using Australian wage growth (Australian Bureau of Statistics, 2020). Other valuations are for serious injuries requiring hospitalization (\$495,874 each); minor injuries not requiring hospitalization (\$77,472 each); and crashes involving property damage but no injuries or fatalities (\$10,338 each).

We apply these values to the implied reductions from our estimates. For minor injuries and property-damage crashes, we use our estimates in Appendix Table A7 from equation (3). The number of minor injuries is equal to the change in casualties in column (2) minus the change in hospitalizations/deaths in column (3). For hospitalizations and fatalities, our first-year estimates use a single 8:00pm–4:59am treatment window and daytime outcomes from all crashes to increase precision (see Section 4.2). For the longer-term effects, we estimate a single treatment effect for the 8:00pm–4:59am period and use all drivers in their second-to-fourth years of driving. This approach results in estimated reductions for every 100,000 first-year drivers of 0.9 fatalities and 7.1 hospitalizations, neither of which are statistically significant at conventional levels. We obtain similar estimates (0.6 fatalities and 7.4 hospitalizations) if we use the statistically significant effects on casualties in column (2) of Appendix Table A7 and assume that a constant fraction of casualties were hospitalizations and deaths throughout.

Overall, our estimates indicate that for every 100,000 first-year drivers subject to the restriction, there were changes of -163.7 crashes (-77.2 with casualties and -86.5 with just property damage), -97.8 minor injuries, -40.5 hospitalizations, and -5.9 deaths. With a 2% annual discount rate, these reductions are worth \$412 million in total or \$738 per driver subject to the restriction (valued in their first year of driving). This is our preferred estimate. If we only use statistically significant estimates and ignore the implied longer-term reductions in fatalities and hospitalizations, the value of the improvements decreases by 14%. If we use the Viscusi (2018) estimate for the value of a statistical life in Australia, the value increases by 25%.