Web Appendix for "Shaping the Habits of Teen Drivers"

Timothy J. Moore Todd Morris

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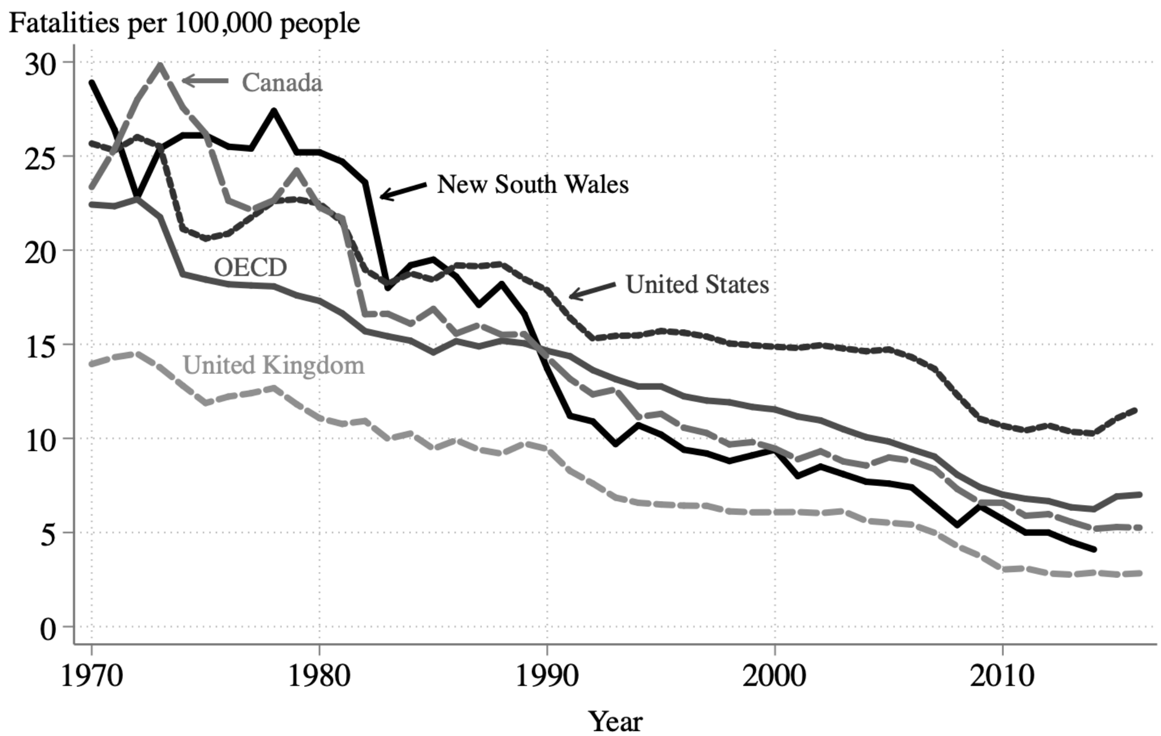
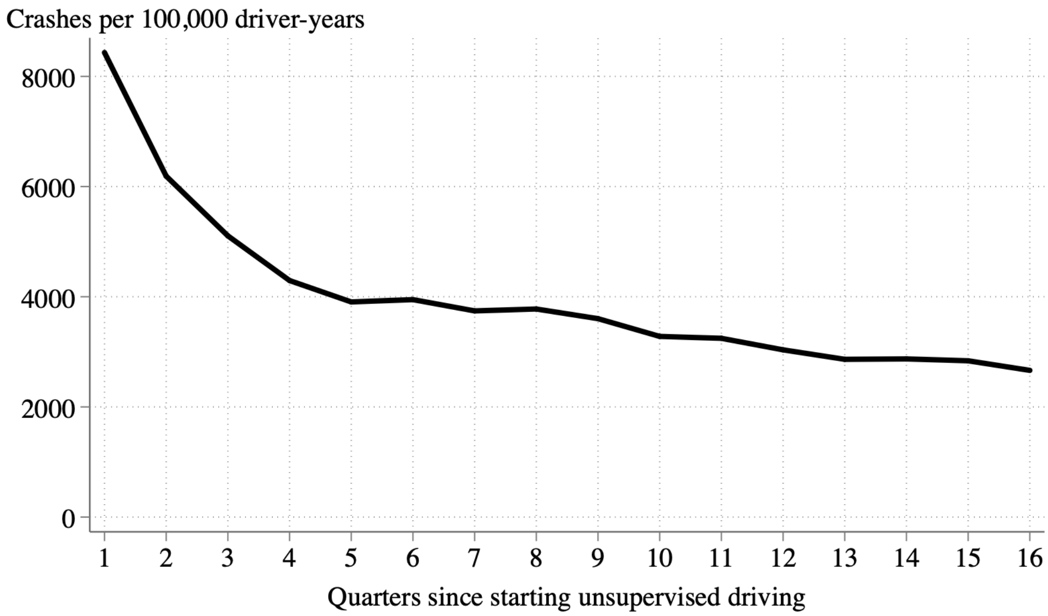
Figure A1: Traffic fatality rates in New South Wales compared to Canada, the United Kingdom, the United States and the OECD average: 1970–2016



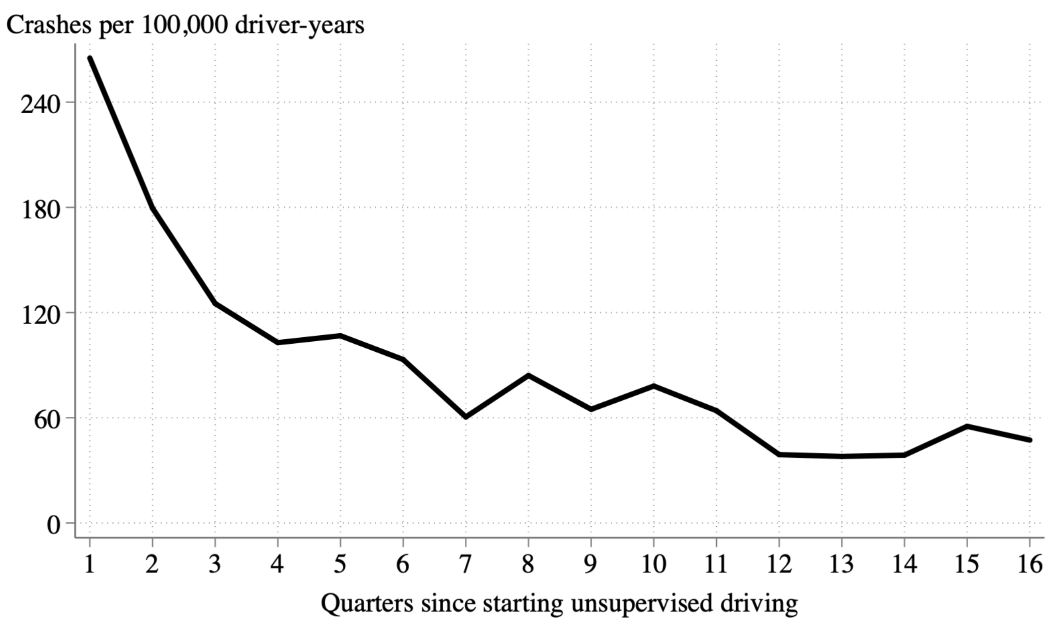
Notes: This figure shows the trends in traffic fatality rates in New South Wales and countries with similar traffic and legal systems. Sources: New South Wales Centre for Road Safety (Transport for NSW, 2022*d*) and authors' calculations using OECD data on countries' annual traffic fatalities (OECD, 2022*b*) and population (OECD, 2022*a*). 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



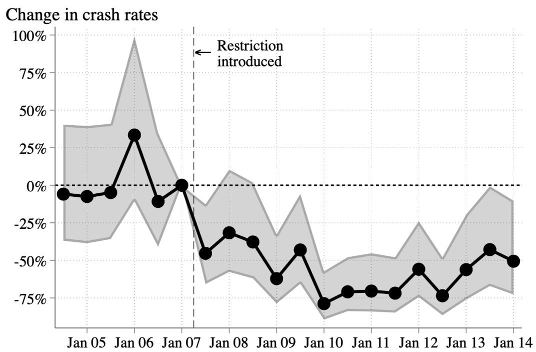
(b) Crashes with 2+ passengers, 11:00pm–4:59am



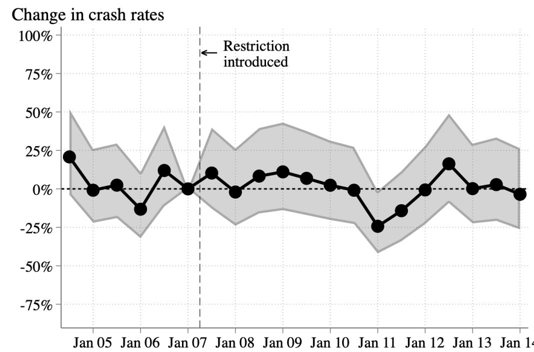
Notes: These figures show the crash rates as a function of driving experience before the introduction of the nighttime passenger restriction in NSW. Experience is the number of quarters since a probation (P1) license was first held. The sample includes crash records from June 2004 to June 2007 for all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

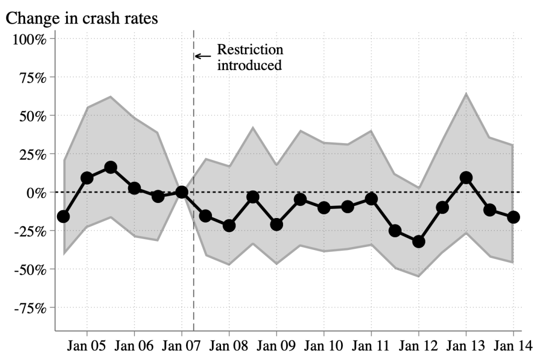
(a) 2+ passengers, 11:00pm–4:59am



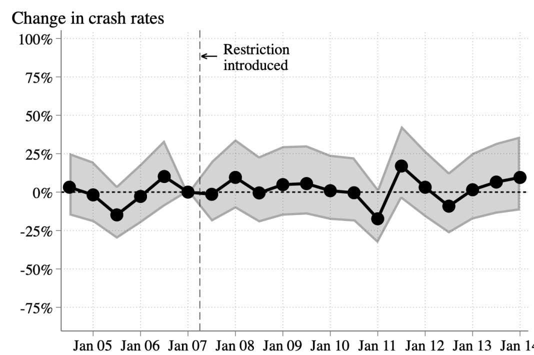
(b) 0/1 passenger, 11:00pm–4:59am



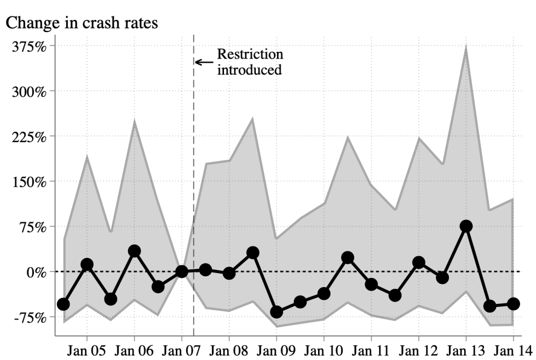
(c) 2+ passengers, 8:00pm–10:59pm



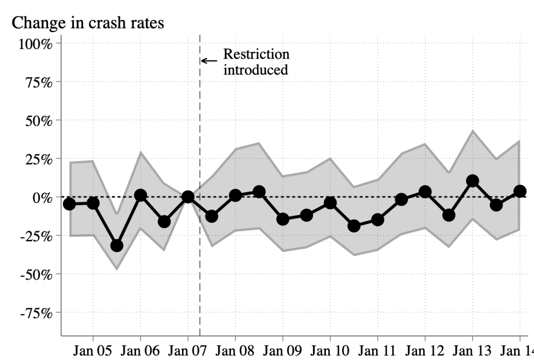
(d) 0/1 passenger, 8:00pm–10:59pm



(e) 2+ passengers, 5:00am–7:59am



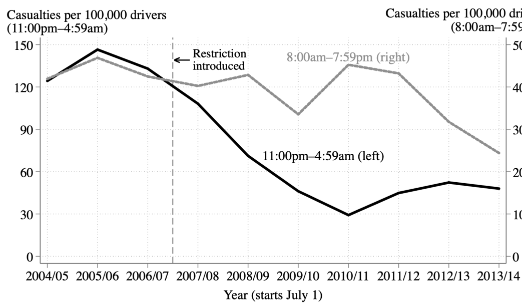
(f) 0/1 passenger, 5:00am–7:59am



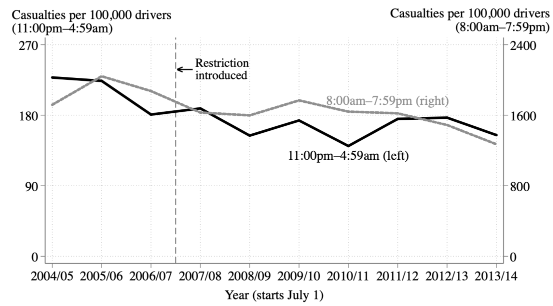
Notes: These figures show the percentage changes in crash rates, with 95% confidence intervals, of first-year drivers in different periods of the day relative to daytime crashes and the six months before the restriction was introduced. The specification is similar to equation (1), except we interact 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: Other crash outcomes of first-year drivers by number of passengers and time of day

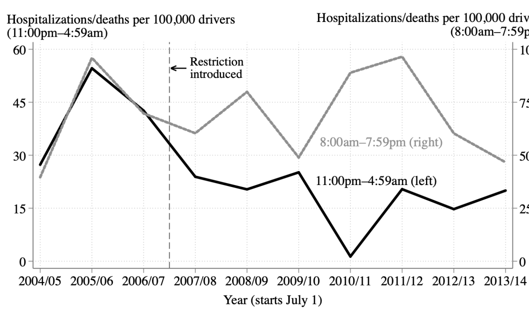
(a) Casualties: 2+ passengers



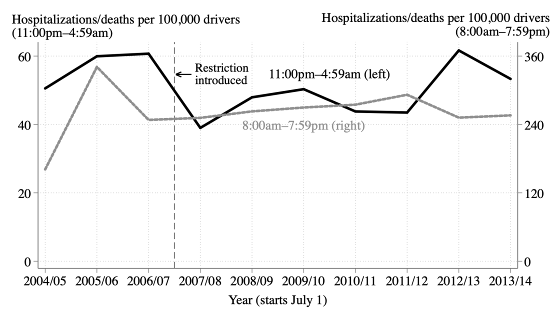
(b) Casualties: 0/1 passenger



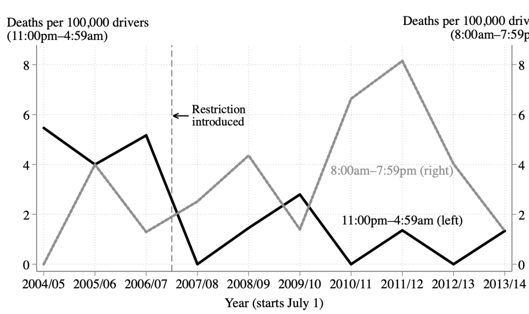
(c) Hospitalizations/deaths: 2+ passengers



(d) Hospitalizations/deaths: 0/1 passenger



(e) Fatalities: 2+ passenger

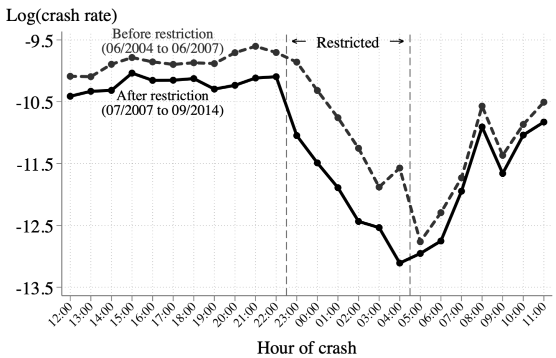


(f) Fatalities: 0/1 passenger

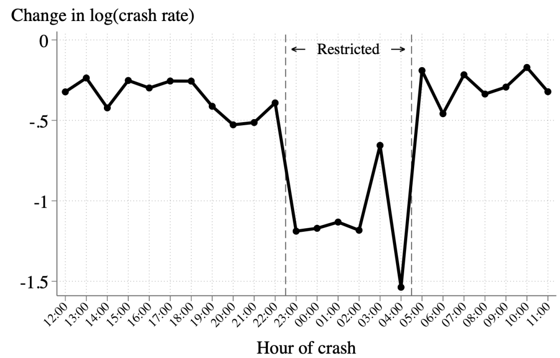
Notes: These figures show the annual crash outcomes (casualties, hospitalizations/deaths and deaths) of first-year drivers in NSW from July 2004 to June 2014. These figures are analogous to Figure 1, which shows the corresponding crash rates at a quarterly frequency. Panels (a), (c) and (e) show that there are similar trends in multi-passenger crash outcomes across the nighttime (11:00pm–4:59am) and daytime (8:00am–7:59pm) periods until the restriction is introduced. After its introduction, there is a reduction in the rate of nighttime casualties, hospitalizations/deaths and deaths relative to daytime outcomes. Panels (b), (d) and (f) show the trends in crash outcomes with zero or one passenger. The trends are broadly similar across the nighttime and daytime periods both before and after the introduction of the nighttime passenger restriction, although the series become noisier for more severe outcomes.

Figure A5: Average hourly crash rates of first-year drivers before and after the reform

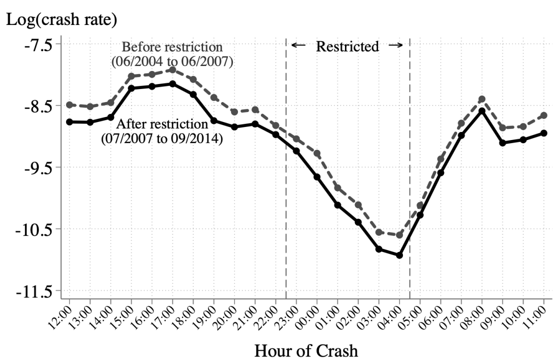
(a) Crash rates: 2+ passengers



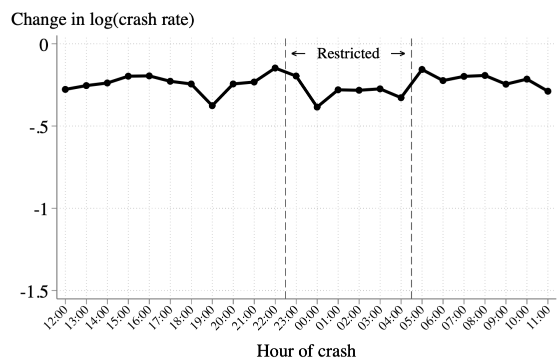
(b) Change in crash rates: 2+ passengers



(c) Crash rates: 0/1 passenger



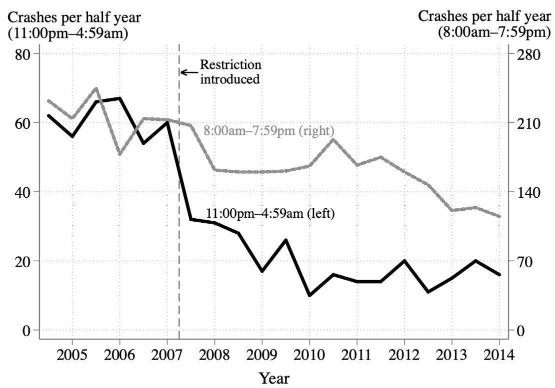
(d) Change in crash rates: 0/1 passenger



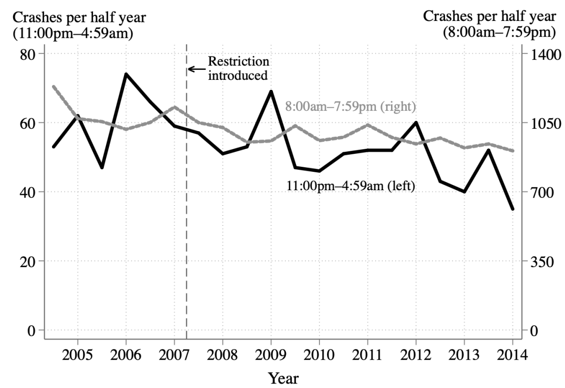
Notes: These figures show the log of the hourly crash rates before and after the introduction of the nighttime passenger restriction in July 2007. Panel (a) shows the rates for multi-passenger crashes. There is a reduction in crashes across the day, but the reduction is much larger in the 11:00pm to 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 A6: Crash counts of first-year drivers and drivers aged 25–39 by the number of passengers and time of day

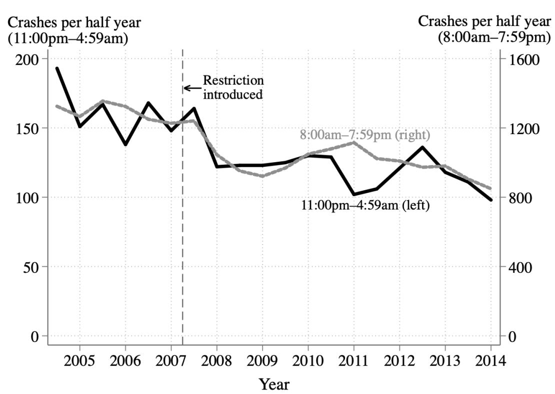
(a) First-year drivers: 2+ passengers



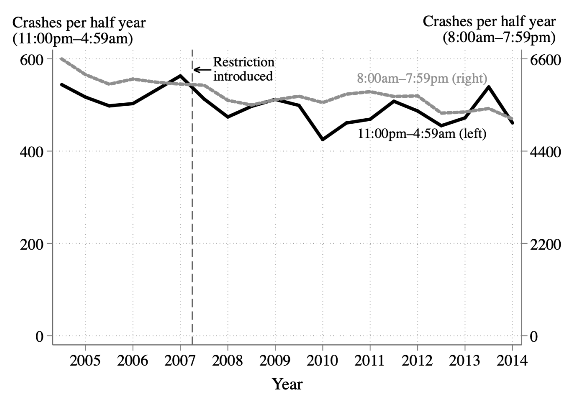
(b) Drivers aged 25–39: 2+ passengers



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

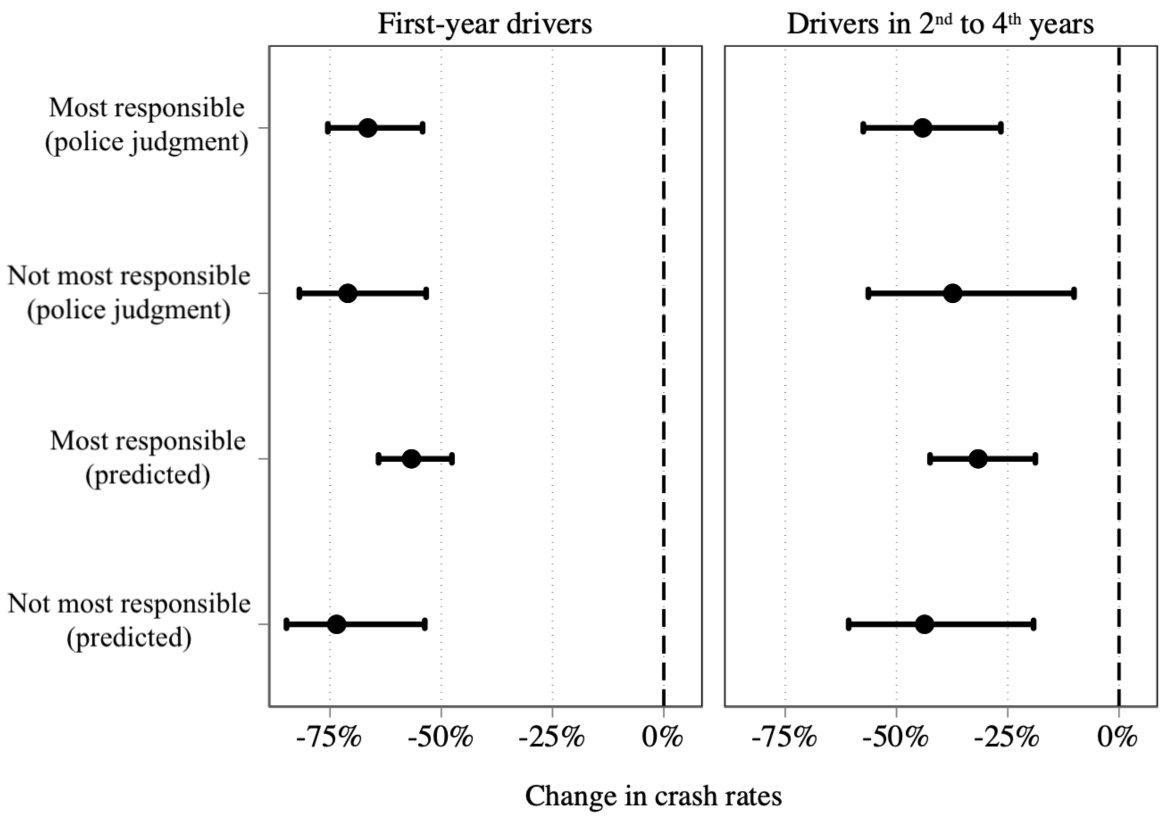


(d) Drivers aged 25–39: 0/1 passenger



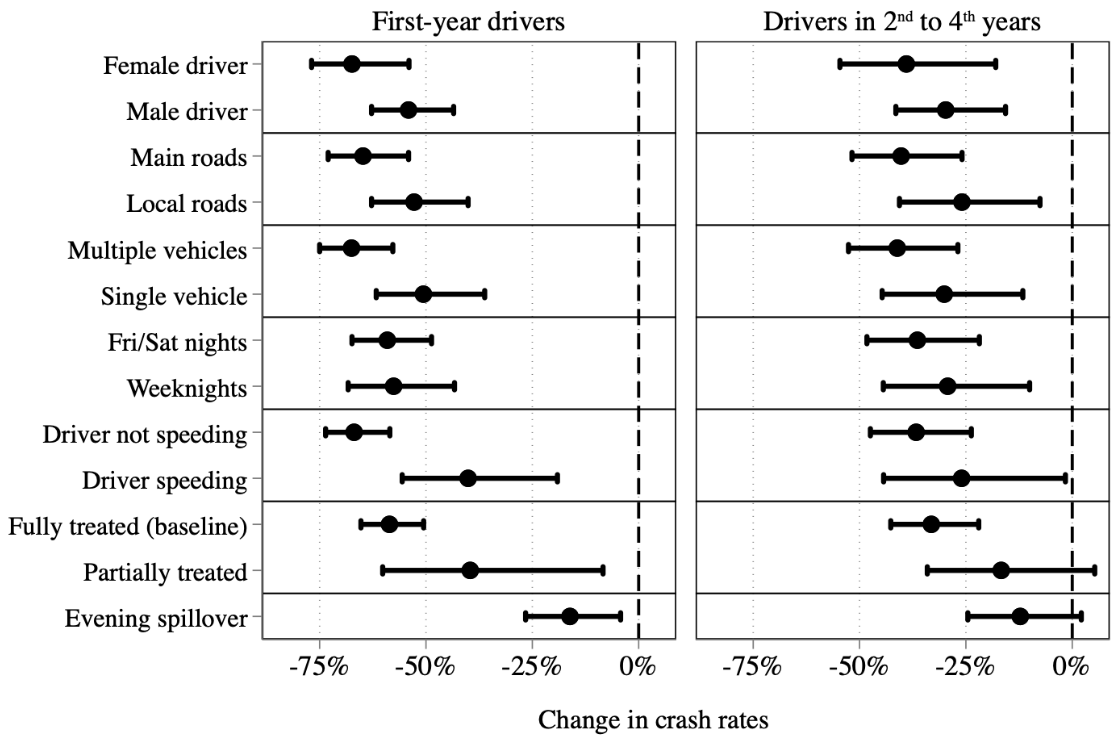
Notes: These figures show the six-monthly crash counts of first-year drivers and drivers aged 25–39 in NSW from July 2004 to June 2014. Panel (a) shows that, among first-year drivers, there are similar trends in multi-passenger crash counts across the nighttime (11:00pm–4:59am) and daytime (8:00am–7:59pm) 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 A7: 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 C 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.

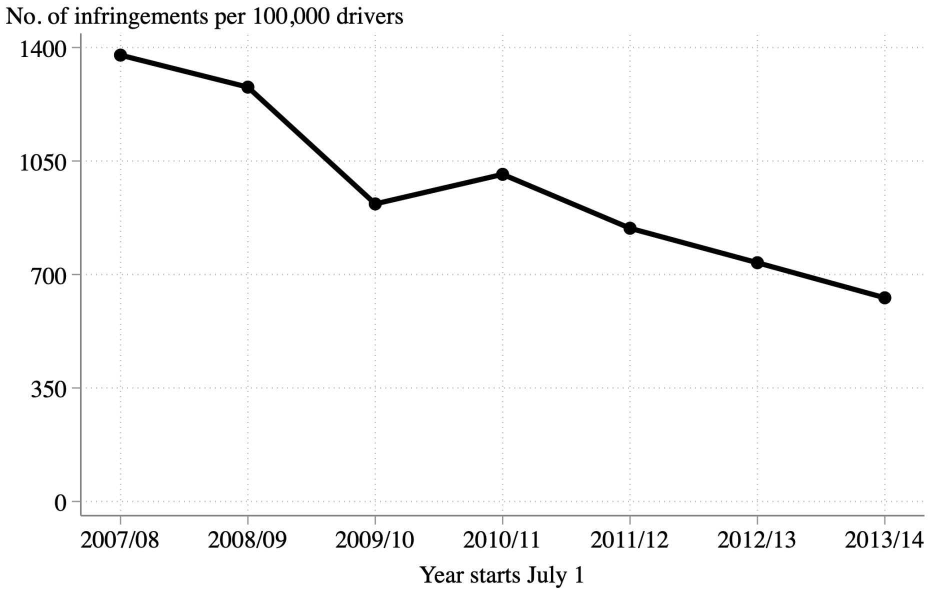
Figure A8: Relationship between first-year and longer-term heterogeneity in treatment effects



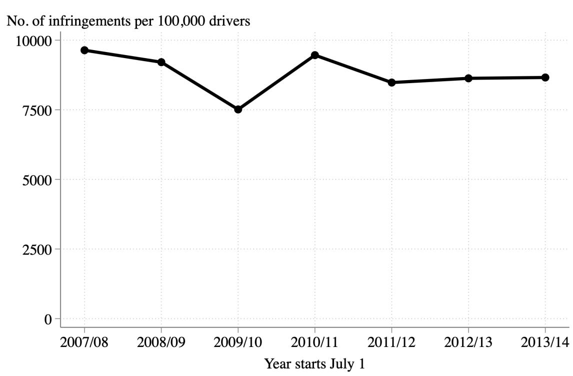
Notes: The figures show the treatment effect heterogeneity estimates for multi-passenger crashes in the restricted window (11:00pm–4:59am) in the first year of driving and for the next three years. We also present the "evening spillover" effect on multi-passenger crashes in the 8:00pm–10:59pm period 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 A9: Traffic infringement rates of P1 drivers by year and type of infringement

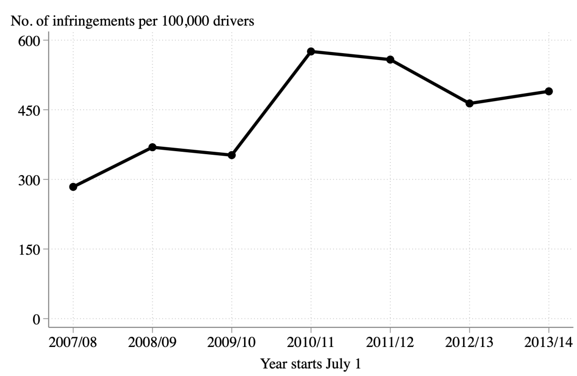
(a) Violating nighttime peer passenger restriction



(b) Not displaying license plates on vehicle

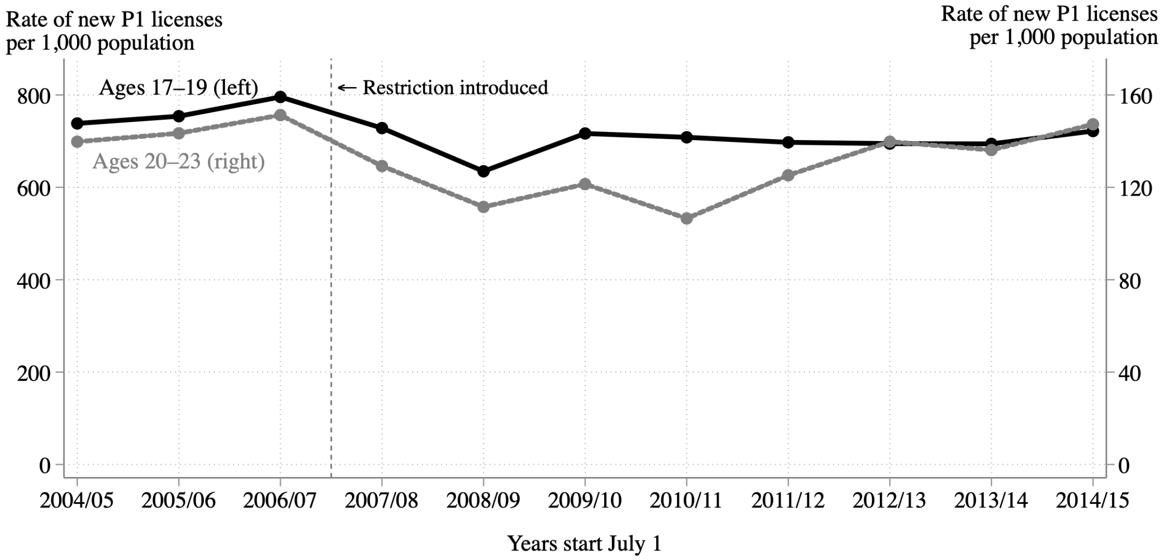


(c) Using mobile phone while driving



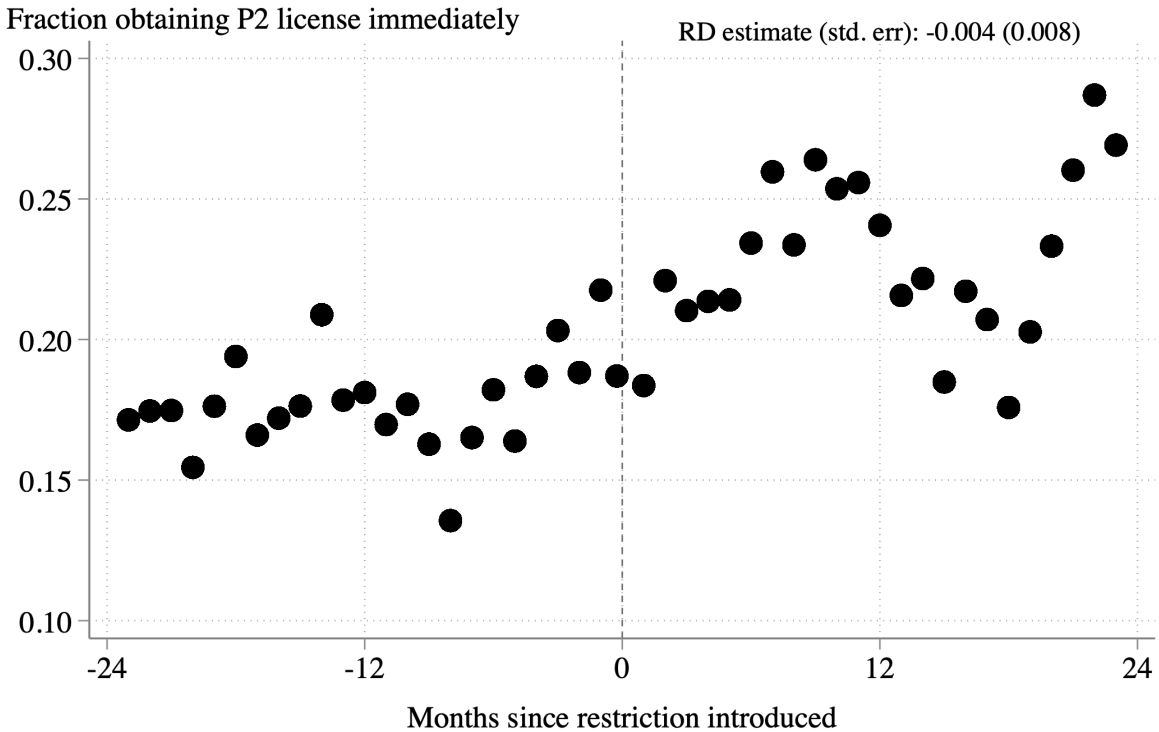
Notes: These figures show trends in the rate of infringements issued to P1 drivers over time in the years since the restriction was introduced. We scale the number of infringements by the number of P1 drivers in each year. Panel (a) shows that the infringement rate for the nighttime passenger restriction 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 A10: 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 A11: 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

	Licenses	Crashes	
		All	2+ passengers & 11pm–4:59am
	(1)	(2)	(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 A5, 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	No. of casualties	No. in hospital/ killed	All crashes	No. of casualties	No. in hospital/ killed
	(1)	(2)	(3)	(4)	(5)	(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 Summary of related studies

Table B1: Summary table of related studies

Study	Setting & policies	Data	Empirical approach	Crash outcomes: Key findings	Crash outcomes: Spillovers?	Crash outcomes: Persistence in effects?	Crash outcomes: Externalities to others?	Effects on licensing?
Overall analysis of Graduated Driving Law (GDL) systems								
Langley, John D., Alexander C. Wagenaar, and Dorothy J. Begg. 1996. "An evaluation of the New Zealand graduated driver licensing system." *Accident Analysis & Prevention*, 28(2), 139–146.	New Zealand. Examine effects of GDL adoption in 1987.	Data on hospitalizations from traffic accidents, 1979–92. Monthly license counts by age.	Time-series analysis comparing hospitalization rates before and after 1987 for different age groups.	Injury reduction is larger for 15–19-year-olds (23%) than other groups (12% for 20–24-year-olds and 16% for 25+).	Not assessed.	Not assessed.	Not assessed.	Licensed drivers aged 15–19 fell by 25% in the 2 years post reform. No change in injuries per licensed driver.
Ulmer, Robert G., et al. 2000. "Effect of Florida's graduated licensing program on the crash rate of teenage drivers." *Accident Analysis & Prevention*, 32(4), 527–532.	Florida, US. Examine effects of GDL adoption in 1996.	Data on crashes for Florida and Alabama, 1995–1997.	Compare crash rate among teens in Florida to those aged 25–54 in the same state, before and after the policy. Alabama is used as a 'placebo'.	Estimate a 19% reduction in Florida crash rates at age 15, 11% at age 16 and 7% at age 17. No significant effects at age 18 or in Alabama.	Not assessed.	Not assessed.	Not assessed.	No evidence that the introduction of GDL diminished the rate at which teens held licenses.
Shope, Jean T., et al. 2001. "Graduated driver licensing in Michigan: Early impact on motor vehicle crashes among 16-year-old drivers." *JAMA*, 286(13), 1593–1598.	Michigan, US. Examine effects of GDL adoption in 1997.	Data on crashes from Michigan state police for 1996, 1998 and 1999.	Similar to Ulmer et al. (2000), compare crash rates among 16-year-olds in Michigan to those 25 years or older before and after GDL adoption.	Crash involvements among 16-year-olds fell by 25%; larger reductions found late at night, where driving was only allowed with experienced adults.	Not assessed.	Not assessed.	Not assessed.	Share of 16-year-olds who had license fell from 59.7% in 1996 to 37.5% in 1999.
Foss, Robert D., John R. Feaganes, and Eric A. Rodgman. 2001. "Initial effects of graduated driver licensing on 16-year-old driver crashes in North Carolina." *JAMA*, 286(13), 1588–1592.	North Carolina, US. Examine effects of GDL adoption in December 1997.	Data on crashes from North Carolina Crash Data File for 1996, 1997 and 1999.	Similar to Ulmer et al. (2000) and Shope et al. (2001), compare crash rates among 16-year-olds in North Carolina to those 25–54 before and after GDL adoption.	Crash rates among 16-year-olds fell by 23% in raw data, 27% with older control group. Larger reductions at night, where curfew applied.	Not assessed.	Not assessed.	Not assessed.	Not assessed.

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Table B1: Summary table of related studies (continued from previous page)

				Crash outcomes				
Study	**Setting & policies**	**Data**	**Empirical approach**	**Key findings**	**Spillovers?**	**Persistence in effects?**	**Externalities to others?**	**Effects on licensing?**
Dee, Thomas S., David C. Grabowski, and Michael A. Morrisey. 2005. "Graduated driver licensing and teen traffic fatalities." *Journal of Health Economics*, 24(3): 571–589.	US. Examine effects of GDLs on crashes by system strength and specific features.	State-year panel data on fatal crashes from Fatality Analysis Reporting System (FARS), 1992–2002.	Difference-in-differences based on state GDL introduction. Add a triple-difference approach with older drivers as an extra control group.	For ages 15–17, GDLs reduce traffic fatalities by at least 6%. Reductions of ∼19% for stricter GDL policies.	Not assessed.	No statistically significant effects on fatalities at ages 18–20.	All traffic fatalities aged 15–17 includes passengers/ pedestrians. Broader externalities not assessed.	Not assessed.
Morrisey, Michael A, et al. 2006. "The strength of graduated drivers license programs and fatalities among teen drivers and passengers." *Accident Analysis & Prevention*, 38(1): 135–141.	US. Examine effects of GDL adoption, allowing for differences by system strength.	Data on fatal crashes from FARS, 1992–2002.	Similar to Dee et al. (2005), differences-in-differences based on state GDL adoption.	Reduction in fatalities at ages 15–17 by 19% for "good" GDL programs. Larger reductions during the day than at night.	Not assessed.	Not assessed.	Large reductions in fatalities for the teenage passengers of teen drivers.	Not assessed.
Trempel, Rebecca E. (2009). "Graduated driver licensing laws and insurance collision claim frequencies of teenage drivers." *Arlington, USA: Highway Loss Data Institute.*	US. Examine effects of GDL stringency on insurance claim frequencies of teen drivers.	State-year-age data on automobile collision claim frequencies for 1996–2008.	Poisson regression models used to estimate effects of GDL laws on claim frequencies, with controls for frequency among 35–55-year-olds in same state.	GDL laws rated 'good' (vs. poor) have 20% fewer claims at age 16. Higher licensing age, practice requirement, passenger restrictions and night curfews associated with lower claims.	Not assessed.	Not assessed.	Not assessed.	Not assessed.
Karaca-Mandic, Pinar, and Greg Ridgeway. 2010. "Behavioral impact of graduated driver licensing on teenage driving risk and exposure." *Journal of Health Economics*, 29(1): 48–61.	US. Examine effects of GDLs on crashes by system strength and specific features.	State-year panel data on fatal and non-fatal police-reported crashes from 12 states, 1990–2015.	Model based on staggered introduction of laws and computing changes in driving prevalence based on mix of teen-teen, teen-adult and adult-adult crashes.	For ages 15–17, no change in crash per mile driven. Proportional decreases in crashes and driving prevalence of 5% during day and 15% at night.	Not assessed.	No statistically significant effects on crash risks or driving prevalence at ages 18–20.	Not assessed.	Not assessed.
McCartt, Anne T., et al. 2010. "Graduated licensing laws and fatal crashes of teenage drivers: A national study." *Traffic Injury Prevention*, 11(3): 240–248.	US. Examine effects of GDL stringency on teen driver fatalities.	Data on fatal crashes from FARS, 1996–2007.	Poisson regression with multiple treatments, controlling for state-year crash rate of 30–59-year-olds.	GDL systems rated good (vs. poor) are associated with 30% lower fatal crash rates among 15–17 year-olds.	Not assessed.	Laws rated good associated with 9% lower fatalities among 18–19-year-olds.	Not assessed.	Not assessed.

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Table B1: Summary table of related studies (continued from previous page)

Study	Setting & policies	Data	Empirical approach	Crash outcomes: Key findings	Crash outcomes: Spillovers?	Crash outcomes: Persistence in effects?	Crash outcomes: Externalities to others?	Effects on licensing?
Masten, Scott V., Robert D. Foss, and Stephen W. Marshall (2013). "Graduated driver licensing program component calibrations and their association with fatal crash involvement." *Accident Analysis & Prevention*, 57, 105–113.	US. Examine effects of GDL components on fatalities.	Data on fatal crashes from FARS, 1986–2007.	Poisson regression with multiple treatments, state fixed effects and state time-varying controls.	Fatality rates among 16–17-year-olds negatively associated with longer learner period, longer nighttime curfew period, higher independent driving age.	Not assessed.	Not assessed.	Not assessed.	Not assessed.
Gilpin, Gregory. 2019. "Teen driver licensure provisions, licensing, and vehicular fatalities." *Journal of Health Economics*, 66: 54–70.	US. Examine effects of specific GDL provisions on fatalities, licenses and fatalities per licensed driver.	Data on fatal crashes from FARS, and state-year-age license counts, 1996–2015.	Difference-in-differences based on state GDL introduction. Add a triple-difference approach with older drivers as an extra control group.	For ages 16–17, higher minimum licensing ages of 16.5+ decrease fatalities by 23%, driver's education rules by 6%, and "no pass, no drive" laws by 7%. More supervised hours increase fatalities by 6%.	Not assessed.	No statistically significant effects at ages 18–20.	All traffic fatalities ages 15–17 includes passengers/ pedestrians. Broader externalities not assessed.	For ages 16–17, licensing rates fell by 33%. 16.5+ licensing ages reduce it by 20 p.p. No change in fatalities per licensed driver.
Studies of minimum legal driving age								
Huh, Jason, and Julian Reif. 2021. "Teenage driving, mortality, and risky behaviors." *American Economic Review: Insights*, 3(4): 523–39.	US. Examine effects of min. driving age, which varies across states.	Fatalities from CDC mortality data (1983–2014); license data from AddHealth survey.	Regression discontinuity design of mortality rates around the minimum legal driving age.	Traffic fatality rates increase by 4.92 per 100,000 person-years at minimum driving age.	Not assessed.	Not assessed.	Not assessed.	Driver licensing increases by 18.6 percentage points at minimum driving age.
Studies of learning permit-related rules								
Kettlewell, Nathan, and Peter Siminski. 2020. Optimal model selection in RDD and related settings using placebo zones. *IZA Discussion Paper No. 13639.*	Australia (NSW). Examine increases in minimum supervised driving hours for learners from 0 to 50 hours and 50 to 120 hours.	Administrative individual-level data on licensing and police-reported crashes, for birth cohorts 1980+.	Use regression discontinuity and kink designs using date-of-birth thresholds and a month-of-birth 2SLS instrumental variable.	In 1^{st} year of unsupervised driving, crash rates per licensed driver decrease by 21% for 0-to-50 hours change, no statistically significant effects for 50-to-120 hours change.	Not assessed.	Treatment at learner stage assessed in first year after that stage. No additional testing of persistence.	Not assessed.	Suggestive evidence of small delay in obtaining a license. Adding controls reduces effects slightly.

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Table B1: Summary table of related studies (continued from previous page)

Study	Setting & policies	Data	Empirical approach	Crash outcomes: Key findings	Crash outcomes: Spillovers?	Crash outcomes: Persistence in effects?	Crash outcomes: Externalities to others?	Effects on licensing?
Studies of vehicle-power restrictions for teens								
Balia, Silvia, Rinaldo Brau, and Marco G Neiddu. 2021. "Depowering risk: Vehicle power restriction and teen driver accidents in Italy." *The University of York HEDG Working Paper No. 21/06*.	Italy. Examine a vehicle-power limit on first-year drivers.	Data on fatal and non-fatal crashes from the Istat data; license counts by age, geographical area, sex and birth cohort.	Differences-in-differences models exploiting cohort-variation in reform exposure; some regressions estimate effects by vehicle power (comparing high to low power).	Reductions in at-fault accidents per capita of 18% and per license of 13%. Larger reductions for fatal crashes and those involving speeding.	Not assessed.	Estimate a negative effect on drivers aged 20–21, possibly due to types of vehicles owned.	Estimate 37% of reduced fatalities/ injuries are people in other vehicles or pedestrians.	Licensing declines ∼19%. Other contemporaneous changes in licensing system may contribute to this decline.
Studies on safety impacts of other state-level policies on teens								
Dee, Thomas S, and William N Evans. 2001. "Behavioral policies and teen traffic safety." *American Economic Review*, 91(2): 91–96.	US. Examine effects on teens of state laws on seat belt use, minimum legal drinking ages, and highway speed limits.	Data on fatal crashes from FARS, 1977–1992.	Differences-in-differences with multiple treatments, exploiting variation from states' adoption and removal of individual laws.	Mandatory seat belt use reduced fatalities among 18–19-year-olds by 7–10%; drinking ages of 18 or 19 associated with 5% higher fatality rates at age 18–19. No effect of higher maximum speed limits.	Not assessed.	Suggestive evidence of a negative effect of lower drinking ages on fatalities at age 22 and 23.	Not assessed.	Not assessed.
Eisenberg, Daniel. 2003. "Evaluating the effectiveness of policies related to drunk driving." *Journal of Policy Analysis and Management*, 22(2), 249–274.	US. Examine effects of several policies on traffic fatalities, including GDL.	Data on fatal crashes from FARS, 1982–2000. License counts by state.	Differences-in-differences with multiple treatments, exploiting variation from states' adoption and removal of individual laws.	GDL is associated with a 9% decrease in fatal crash rates per capita among drivers under age 21.	Not assessed.	Not assessed.	Not assessed.	Not assessed.
Carpenter, Christopher S, and Mark Stehr. 2008. "The effects of mandatory seatbelt laws on seatbelt use, motor vehicle fatalities, and crash-related injuries among youths." *Journal of Health Economics*, 27(3): 642–662.	US. Effect of seatbelt laws on youths' traffic fatality rates.	Data on fatal crashes from FARS, 1991–2005.	Differences-in-differences with multiple treatments, state-time trends.	Mandatory seat belt laws are associated with reductions in fatality rates among 14–18-year-olds by 8%.	Not assessed.	Not assessed.	Not assessed.	Not assessed.

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Table B1: Summary table of related studies (continued from previous page)

Study	Setting & policies	Data	Empirical approach	Crash outcomes: Key findings	Crash outcomes: Spillovers?	Crash outcomes: Persistence in effects?	Crash outcomes: Externalities to others?	Effects on licensing?
Studies of nighttime curfews on teens								
Preusser, David F., et al. 1990. "City curfew ordinances and teen motor vehicle injury." *Accident Analysis & Prevention*, 22(4): 391–397.	In US, examine city curfews that limit the late-night activities of minor teenagers in public places, including driving on highways.	Police-reported crash data for Michigan and Ohio, 1985–1987.	Compare crashes with curfews in Detroit, Cleveland, and Columbus to crashes in Cincinnati, which does not have such a curfew.	For ages 13–17, curfews associated with a 23% reduction in injuries as drivers, passengers, pedestrians, or bicyclists during the curfew hours.	Not assessed.	Not assessed.	Focus on injuries at ages 13–17, includes other road users. No assessment of broader externalities.	Not assessed.
Preusser, David F., Paul L. Zador, and Allan F. Williams. 1993. "The effect of city curfew ordinances on teenage motor vehicle fatalities." *Accident Analysis & Prevention*, 25(5): 641–645.	In US, examine city curfews that limit the late-night activities of minor teenagers in public places, including driving on highways.	Data on fatal crashes from FARS, 1984–1990. Surveyed 149 cities and found 72 with curfews; focused on 47 with curfews for all under 18.	Compared cities with curfews to those without, and assessed effects at night and daytime.	For ages 13–17, curfews associated with a 23% reduction in fatalities between 9pm and 4.59am.	No effects on daytime accidents.	Not assessed.	Focus on fatalities at ages 13–17, includes other road users. No assessment of broader externalities.	Not assessed.

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C 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;

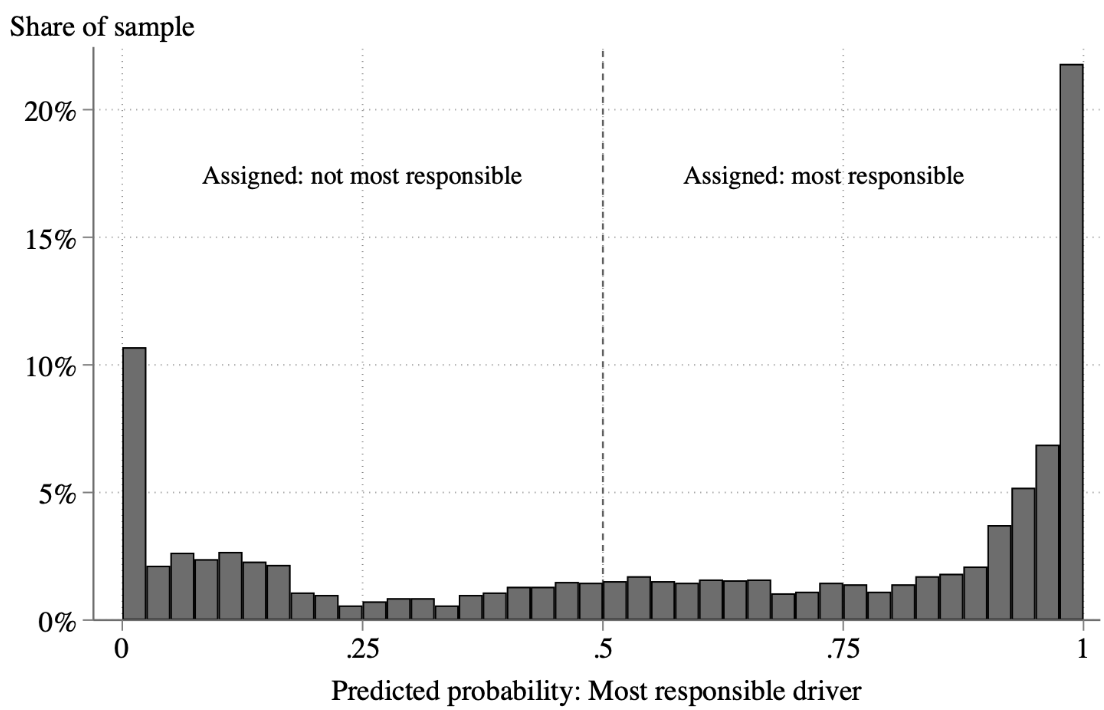
(iv) Characteristics of the reference driver: dummy variables for the different license types (Learner, P1, P2, Unrestricted); a dummy variable for whether the reference driver's vehicle was towed; dummy variables for whether the driver was speeding, fatigued, and had a blood alcohol concentration level 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 C1 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 C1: Histogram of predicted probabilities for the younger driver being most responsible for multi-vehicle crashes before the restriction was introduced

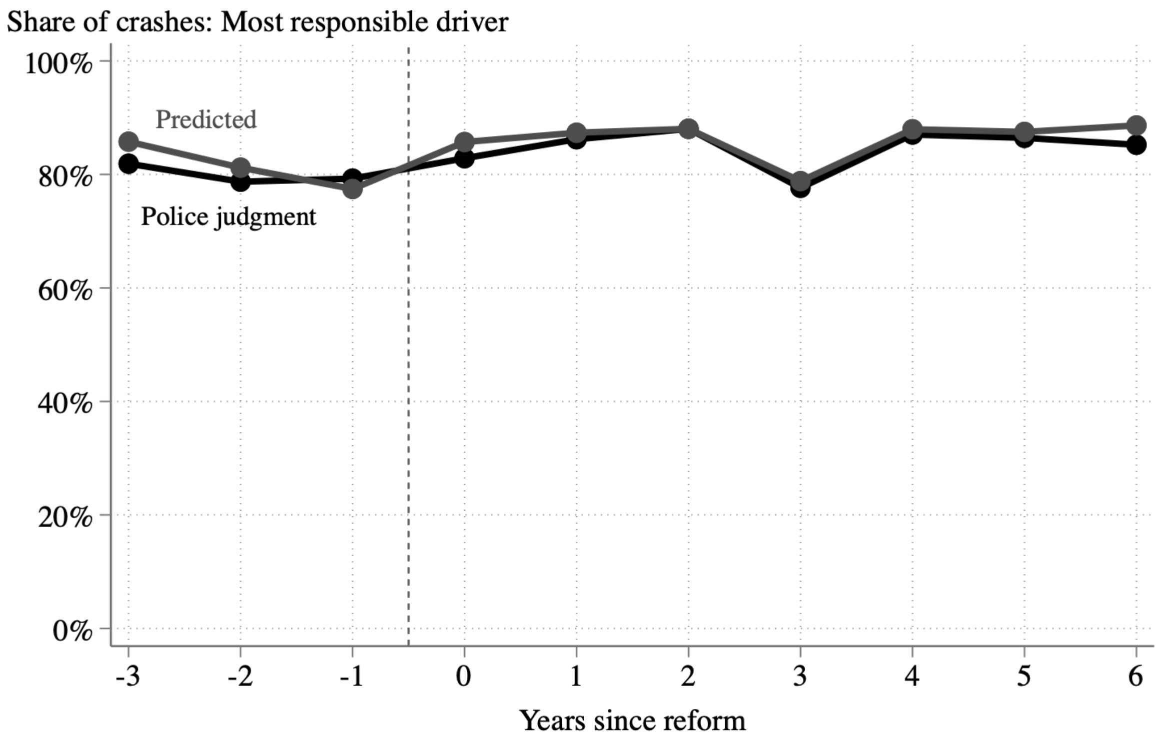


Based on an assignment rule where drivers with predicted probabilities greater than 0.5 are assigned to be the most responsible driver, our machine-learning model predicts that young drivers caused 61.8% of multi-vehicle crashes prior to the reform. This closely matches the 59.6% reported in police judgments. 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 A7 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 C2 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 C2: Share of multi-passenger crashes 11:00pm–4:59am caused by the younger driver



D Aggregating and valuing crash outcomes

In this section, we provide more details about how we value the economic benefits of the estimated reductions in crashes, injuries and fatalities from 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).[D1]

We apply these values to the implied reductions from our estimates in each of these outcomes among fully treated drivers:

- For property-damage crashes (no injuries or fatalities), we use our estimates in column (4) of Appendix Table A7 (from equation (3)). These estimates imply a change of -86.5 property-damage crashes per 100,000 drivers over the first four years of driving.

- For minor injuries, we use our estimates in columns (2) and (3) of Appendix Table A7 (from equation (3)). Specifically, the estimated change in minor injuries in each period-of-day by year-of-driving cell is equal to the estimated change in casualties in column (2) minus the estimated change in hospitalizations/deaths in column (3), since casualties include both minor injuries and hospitalizations/deaths. This approach results in an estimated change per 100,000 first-year drivers of -97.8 minor injuries over the first four years.

- For hospitalizations and fatalities, our estimates for first-year drivers use a single 8:00pm–4:59am treatment window and daytime outcomes from all crashes (0–1 passenger and multiple passengers) as the control group to increase precision (see Section 4.2). This approach implies changes per 100,000 drivers of -5.0 fatalities and -33.4 hospitalizations. For the longer-term effects, we estimate a single treatment effect for the 8:00pm–4:59am period among drivers in their second-to-fourth years of driving (again using daytime outcomes

[D1]The values for injuries and fatalities includes the value of the property damage in such crashes.

from all of their crashes as the control group). This approach results in estimated changes 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. In the 7.25 years following the restriction, we observe 558,207 drivers that were always subject to the restriction ($\sim$77,000 per year). Thus, the total estimated effects are 5.58-times larger (-913.8 crashes, -545.9 minor injuries, -226.1 hospitalizations, and -32.9 deaths), while the annual effects are slightly smaller (-126.0 crashes, -75.3 minor injuries, -31.2 hospitalizations, -4.5 deaths). With a 2% annual discount rate, these reductions are worth \$56 million for each year the restriction has been in place (\$412 million in total), or \$738 for each driver subject to the restriction (valued in their first year of driving). These are our preferred estimates. If we only use statistically significant estimates and ignore the implied longer-term reductions in fatalities and hospitalizations, the value of the crash reductions decreases by approximately 14%. If we use the Viscusi (2018) estimate for the value of a statistical life in Australia, the value of the crash reductions increases by approximately 25%.

The value of these benefits include the direct benefits of the restriction (reduced crash outcomes in the first year in the 11:00pm–4:59am window) and its spillover effects (reductions earlier in the evening and beyond the first year). Here, we decompose these gains. The key challenge in doing so is that our preferred estimates for hospitalizations and fatalities among first-year drivers use a combined 8:00pm–4:59am treatment window to increase precision, and this period will capture both the direct effects and evening spillovers. To resolve this issue, we use our estimates in column (3) of Appendix Table A7, which separately estimate the effect on hospitalizations/fatalities in the 11:00pm–4:59am and 8:00–10:59pm periods. These estimates imply that 65% of the reduction in hospitalizations/fatalities occurs in the restricted period and 35% occurs earlier in the evening.

We then make assumptions about the probability of a fatality conditional on a hospitalization/fatality. Our baseline assumption is that this probability is the same in the two periods. This means that, of the estimated change in fatalities of -5.0 per 100,000 first-year drivers, -3.25 (65%) is in the restricted period and -1.75 (35%) is earlier in the evening. With this approach, we estimate direct effects per 100,000 drivers of -45.1 property-damage crashes, -37.5 minor injuries, -21.7 hospitalizations and -3.25 fatalities. The estimated spillover effects (in the evening and beyond the first year) are -41.4 property-damage crashes, -60.3 minor injuries, -18.8 hospitalizations and -2.65 fatalities. Valuing these reductions, we estimate that the spillovers are worth $345 per first-year driver, or 47% of the total benefits.

We examine the sensitivity of this estimate. If the conditional probability of a fatality is higher in late-night crashes, our approach will underestimate the value of the direct effects of the restriction and overestimate the value of the spillover effects. This is the case prior to the reform, when 14.4% of hospitalizations/fatalities were fatalities in the restricted period compared to 4.7% in the evening spillover period. Thus, we re-estimate the effects, allowing the conditional probability of a fatality to be three-times higher in the restricted window. Under this assumption, the value of the spillovers declines to $267 per first-year driver, or 36% of the total benefits.