Do Seat Belt Laws Still Work? Replication and Re-evaluation of Recent Evidence

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Abstract

Recent work suggests that mandatory seatbelt laws are associated with lower motor vehicle accident mortality, but it is unclear whether this is due to increased enforcement. We study this association using 2001-2010 US data on traffic fatalities among individuals ages 10 and over and we test the robustness of unstated assumptions by replicating and extending prior work. Using Poisson regression adjusted for state-level traffic safety policies (seat belt laws, blood alcohol laws, speed limits), miles traveled, and median income, we replicate the covariate-adjusted association seen in prior work (Rate Ratio [RR]=0.80, 95%CI 0.74,0.87). Further adjustment for state and time fixed effects showed no evidence of a causal effect (RR=1.00, 95%CI 0.95,1.06). We find little evidence that seatbelt laws reduce traffic fatalities, and prior work was confounded by general improvements in other environmental determinants of motor vehicle accident mortality. Replications studies have the potential to increase the integrity of research findings.

Introduction

The United States has made great and ongoing strides in reducing deaths from motor vehicle accidents (MVA), even as the number of vehicle miles traveled has increased (1, 2). These reductions have come from several different sources, including improved road design, changes in vehicle safety and driver behavior, and safety legislation (3, 4). Continued reductions in MVA death rates likely will require additional interventions, thus it is important to understand "what works" for optimizing prevention strategies and policy choices. In a recent study Lee and colleagues (5) reported that states with mandatory seat belt laws that require primary enforcement had 17% lower rates of motor vehicle accident mortality compared to states with secondary enforcement. Primary enforcement means that drivers may be directly cited for seat belt non-use, whereas secondary enforcement means that non-belted drivers may be additionally cited after another traffic infraction. Because of the direct penalty imposed by primary enforcement there are reasons to hypothesize that primary laws may be more effective than secondary laws in reducing accidental deaths, especially given the evidence that primary laws increase seat belt use more than secondary laws (6, 7).

Lee and colleagues also report that, based on their calculations, and estimated 6634 persons (95% confidence interval [CI], 5060 to 8208) would not have died each year if all states had mandatory laws with primary enforcement. An accompanying editorial by Baldwin and Houry goes so far as to say that, based on the accumulated evidence, the effectiveness of primary laws on saving lives should be considered "unequivocal" (8, p.234). These are strong counterfactual claims, later echoed in the media (9), about what *would* have happened to MVA death rates *if* all states had upgraded from secondary to primary enforcement. However, these claims rest on strong assumptions, chief among them that states with primary laws and secondary laws are exchangeable apart from their policy status (10). Because the strength of assumptions is often inversely related to the credibility of the evidence it is important to try and rule out alternative explanations, particularly for policy analysis (11).

In particular, observational studies of policies need to careful about confounding factors that may be mistaken for the effect of the policy. Lee and colleagues controlled for several covariates, but states with primary enforcement may differ from states without primary enforcement for reasons that may be associated with MVA death rates and difficult to capture with covariate adjustment. Moreover, given the evidence above on secular declines in death rates it is important to control for any population-wide sources of changes in MVA death rates that may have nothing to do with mandatory seat belt laws. In this paper we replicate the results of Lee and colleagues and provide a number of additional robustness tests to see whether the lower risk of traffic accident deaths in states with primary enforcement laws is, in fact, due to the laws, or whether other factors can account for the observed association. We also use a credible design for estimating the causal effect of upgrading from secondary to primary enforcement on MVA death rates.

Methods

Data

Given that the analysis of Lee and colleagues was based on publicly available data and their methods were reported with considerable detail, we attempted to use the same sources and case definitions to reproduce their findings. We obtained data on fatal accidents from the Fatal Analysis Reporting System (FARS) for 2001-2010 (12), and data on population estimates by age (10-14, 15-19, 20-24, 25-34, 35-44, 45-54, 55-64, 65-74, and 75 years and over), state (excluding the District of Columbia), and year from the US Census Bureau (13). We restricted our dataset to fatalities among individuals aged 10 and older that were drivers or passengers in motor vehicles. We abstracted data on mandatory seat belt policies, including the date and type of enforcement (secondary or primary) from the Insurance Institute for Highway Safety (14).

Because other aspects of safety legislation also changed over this period and could be associated with changes in seat belt laws, we also obtained data on laws pertaining to maximum speed limits and legal limits for blood alcohol concentration (15). For these laws we used data on annual vehicle miles traveled from the FARS website (16), and state median household income (constant dollars) from the US Census (17).

Statistical Analysis

We first attempted to replicate the estimates of Lee and colleagues, then we made minor adjustments to their initial models to test the robustness of some of the underlying assumptions. For the replication we assigned primary enforcement status to states in a given year if they upgraded their law before June 30 (5), but in other analysis we also used the exact date of the law to calculate the proportion of months in a given year a state had a primary law in effect. We calculated MVA death rates per 100,000 population by age group, and we collapsed the dataset to 500 state-year observations (10 observations for each state) and used generalized estimating equations and Poisson regression to assess the effect of primary enforcement. We used the total population aged 10 years and over as the offset in regression models for the whole population and also repeated these analyses restricting to specific age groups (10-24 years and 25 years and over).

After replicating the main results we made some model adjustments. We further adjusted for age, for overall time trends using indicator variables for each year, and fixed characteristics of states by including indicator variables for each state. Because state indicator variables mean that the analysis is conditional on state, comparisons of primary and secondary laws must come from within-state comparisons, helping to control for any unmeasured factors that may be correlated with MVA risk. In the same sense, the set of indicator variables for each year control for any population-wide factors that may be affecting MVA death rates in both primary and secondary enforcement states. This design is also commonly known as "difference-in-differences," (18, 19, 20) since the key idea is to compare the difference in MVA death rates before and after upgrading to primary enforcement with the difference in rates among states that did not upgrade to primary enforcement. The latter serves as a control group and accounts for any changes that may be affecting MVA rates in the absence of policy changes.

In addition to adjustments for state and time fixed effects, we also ran additional models that used alternative assumptions about how MVA rates were correlated among states. Lee and colleagues used an "exchangeable" correlation matrix, which assumes that MVA rates are correlated among states but that this correlation is constant over time. However, in the context of strong secular trends in outcomes an exchangeable correlation not be ideal, since measures further apart in time are likely to be less correlated (21, 22). We therefore tested whether using an autoregressive correlation structure, which assumes that observations within clusters that are closer in time are more correlated, could affect the estimates. All analyses were conducted with Stata (version 14), and standard errors for the Poisson regression models were clustered at the state level (23, 24). This study used de-identified data and did not require ethics review.

Role of the Funding Source

We did not receive any external funding for this study.

Table 1: Standardized differences in mean covariates for 50 states with primary vs. secondary enforcement of mandatory seat belt laws.

	Remained Secondary		Remained Primary		Upgrade to Primary		Secondary vs. Primary	Secondary vs. Upgrade
Variable	Mean	SD	Mean	SD	Mean	SD	d*	d*
Age group, %								
10-24 years	24.4	1.8	25.1	1.4	23.9	1.4	-33.0	19.1
25-44 years	31.1	2.2	33.0	2.0	31.4	1.7	-54.7	-1.3
45-64 years	29.1	2.0	28.2	1.8	29.0	1.6	33.7	4.7
65+ years	15.1	1.8	13.7	1.5	15.6	2.3	61.6	-18.0
Speed limit, %								
$\leq$ 65 MPH	58.8	49.4	32.6	47.0	34.3	47.6	38.3	35.7
70 MPH	11.1	31.5	48.9	50.1	59.8	49.2	-63.8	-83.4
>70 MPH	30.1	46.0	18.5	38.9	5.9	23.6	19.3	46.9
BAC law, %								
BAC <0.10	14.5	33.2	10.5	29.3	10.5	28.0	9.1	9.4
BAC <0.08	83.3	35.1	93.0	23.9	88.1	29.7	-22.8	-10.5
Median income (1000s)	485.3	63.7	478.5	71.3	448.4	65.3	7.1	40.4
VMT (10 billions)	679.4	350.5	1626.3	1073.1	946.3	617.7	-83.9	-37.6

SD=standard deviation; MPH = miles per hour; BAC = blood alcohol concentration; VMT = vehicle miles traveled. *d=Difference in means divided by pooled standard deviation ×100.

Results

Table 1 shows means and standard deviations for covariates for states that maintained secondary enforcement, already had primary enforcement, and upgraded from secondary to primary enforcement. Standardized differences in mean values of age, other elements of road safety legislation, income, and vehicle miles traveled indicate substantial differences (25) in the kinds of states that did and did not alter their policy, and also raises concerns that these groups of states are likely to differ on other covariates that may be associated with MVA death rates.

Appendix Tables 1 and 2 reproduce the published Tables 2 and 3 of Lee and colleagues (5) and show, respectively, descriptive age-specific MVA rates and MVA rates in states with primary vs. secondary enforcement, by age group. These tables are consistent with Lee and colleagues though our rate estimates are slightly lower. Table 2 shows comparisons between the estimated associations given by Lee and colleagues (5) and our own estimates using the same data source. Adjusted generalized estimating equation (GEE) models for the population ages 10 and over show that states with primary enforcement had 13% lower MVA death rates compared to states with secondary enforcement (RR=0.87, 95% CI 0.82 to 0.92). Our estimates are slightly weaker but very similar to those of Lee and colleagues, as is the patterning for other age groups (slightly stronger effects for adolescents and young adults, slightly weaker for ages 25 and over). In Appendix Table 3 we show that our coefficients for 65 mile-per-hour speed limit laws, 0.08 blood alcohol concentration laws, and vehicle miles traveled are also similar to those of Lee and colleagues.

Table 2: Replication of Lee and colleagues' (5) main findings of the association between mandatory seat belt laws with primary enforcement and motor vehicle accident mortality, US states 2001-2010.

	Primary vs. Secondary Enforcement					
	Crude		Adjusted*		Adjusted† (Lee et al.)	
Population	RR	95% CI	RR	95% CI	IRR	95% CI
Ages 10 and over	0.75	(0.71,0.80)	0.87	(0.82,0.92)	0.83	(0.78,0.90)
Ages 10-24	0.71	(0.67,0.76)	0.84	(0.78,0.90)	0.80	(0.74,0.87)
Ages 25 and over	0.77	(0.72,0.82)	0.88	(0.83,0.94)	0.85	(0.79,0.91)

Estimated using generalized estimating equations with exchangeable correlation structure (N=500 for all models). RR=rate ratio; CI=confidence interval. *Adjusted for presence or absence of speed limit ($\leq$65 miles per hour), maximum blood alcohol content of 0.08 g/dL to drive, median income and vehicle miles driven per person. †Adjusted for presence or absence of speed limit ($\leq$65 miles per hour), maximum blood alcohol content of 0.08 g/dL to drive, graduated driver's license law, immediate administrative license suspension law, median income, vehicle miles driven per person.

Table 3 shows estimates of the impact of upgrading from secondary to primary enforcement of mandatory seat belt laws based on the exact date of passage. The crude and covariate-adjusted associations are consistent with Lee et al.'s results, suggesting around a 10-15% decrease in MVA death rates for states with primary versus secondary enforcement. Further adjustment for time trends in reduces the effect to a 7% decrease. The final column shows the difference-in-differences model (full model results are shown in Appendix Table 4) . After controlling for both time trends and for fixed characteristics of states there is little evidence that upgrading from secondary to primary enforcement of mandatory seat belt laws reduces MVA mortality (RR=1.00, 95% CI 0.95 to 1.06). In Appendix Table 5 we also show that the covariate-adjusted GEE models of Lee et al. are also effectively null when adjusted for broad age groups (RR=0.95, 95% CI 0.87 to 1.04), for time trends (RR=0.99, 95% CI 0.95 to 1.03) or when their basic covariate-adjusted model is specified using an auto-regressive rather than exchangeable correlation structure (RR=0.97, 95% CI 0.91 to 1.04).

Lee and colleagues did not adjust for age but estimated the association in broad age groups. We also estimated the impact of upgrading to primary enforcement within each age group using covariate-adjusted difference-in-differences models. In Appendix Figure 1 we show that there is no evidence for any effect of upgrading to primary enforcement in any age group, and a pooled random-effects meta analysis (26) estimate is null and precise (RR=1.00, 95% CI 0.98 to 1.03), with no evidence of heterogeneity across age groups (Cochran's Q=2.71, p = 0.951). Finally, analyses using negative binomial model to account for overdispersion (RR=1.00, 95% CI 0.95 to 1.06) or a 1-year lagged policy variable (RR=1.00, 95% CI 0.94 to 1.06) were identical (not shown).

Table 3: Regression estimates of the effect of upgrading mandatory seat belt laws to primary enforcement on motor vehicle accident mortality.

	Exposure contrast for primary vs. secondary enforcement							
	Est	95% CI	Est	95% CI	Est	95% CI	Est	95% CI
Rate Ratio	0.84	(0.67,1.05)	0.91	(0.81,1.03)	0.93	(0.82,1.05)	1.00	(0.95,1.06)
Rate Difference	-1.96	(-4.40,0.47)	-1.23	(-2.77,0.31)	-0.94	(-2.52,0.65)	0.05	(-0.73,0.82)
Covariates*		No		Yes		Yes		Yes
Year FEs†		No		No		Yes		Yes
State FEs‡		No		No		No		Yes

Estimated using Poisson regression with population as the exposure and standard errors clustered at the state level. N=4500 for all models. Est=estimate; CI=confidence interval; FE=fixed effect. *Covariates included age group indicators, maximum speed limit ($\leq$65, 70, or 75 miles per hour), presence of maximum blood alcohol content of 0.08 g/dL to drive, presence of maximum blood alcohol content of 0.10 g/dL to drive, and vehicle miles driven per person. †Separate indicator variables for each year. ‡Separate indicator variables for each state.

Discussion

Estimating the impact of policy changes on health outcomes requires a number of important assumptions, especially when using observational study designs. We found that after controlling for time trends and fixed state characteristics primary seat belt laws have no detectable impact on MVA mortality over the period 2001-2010. Lee and colleagues also decomposed their effect into within- and between components, which effectively controls for fixed characteristics of states (21, 27) and reported that the within-state effect was still protective. Similarly, we found that when controlling only for covariates and fixed state characteristics primary enforcement was still associated with reduced MVA mortality, which suggests that failing to control for secular trends is the most likely source of bias in prior estimates.

Controlling for time trends helps to account for any unmeasured common factors affecting MVA death rates in states with both primary and secondary enforcement. In particular, we found that MVA mortality rates began declining sharply in 2007 in all states, regardless of their enforcement status, and by 2010 rates were roughly 40% lower than in 2001 (see Appendix Table 4). A number of possible factors could account for this decline. The most recent decline in MVA mortality coincides with the onset of the Great Recession, and several other studies have found that rates decline when the economy contracts (28, 29, 30), chiefly as a result of reductions in the amount of discretionary driving (31, 32). Additionally, there is evidence that improvements in road and vehicle safety and design over the past several years have made important contributions to reductions in MVA mortality (33, 34, 35). In a recent paper Farmer and Lund estimated that approximately 7,700 fewer driver deaths occurred in 2012 than would have been expected as a result of improvements in vehicle safety (35). Many of these changes are safety innovations such as mandatory frontal airbags, side and rollover airbags, electronic stabilization control, automatic crash warnings, improved vehicle lighting, and shorter vehicle design cycles. Lee and colleagues did report that they found no differences between primary and secondary enforcement states in the fraction of MVA deaths involving vehicles older than 5 years old, but it is unlikely that this covariate would be sufficient to account for the multitude of other factors unrelated to mandatory seat belt laws that have improved death rates over this period.

In noting their study's limitations Lee and colleagues mention that "conclusions about causality" cannot be made due to the cross-sectional, ecological design. We agree, but nevertheless, strong causal claims are evident in their assertion that approximately 6600 deaths would be prevented each year if all states upgraded to primary enforcement laws. We find that these claims are not robust to straightforward adjustments for either age or, as noted above, broad secular trends. Moreover, the estimates of Lee and colleague are also heavily influenced by the method of accounting for the fact that they have repeated measures on states. Using a correlation structure that allows state-level correlations in MVA death rates to change over time, as seems likely in the context of secular declines, effectively nullified the prior association even without adjustment for time trends. The sensitivity of results to different modeling assumptions should temper the strength of any causal conclusions.

Moreover, our estimates are also more consistent with prior studies that have attempted to control for secular trends and unmeasured state-level characteristics. Cohen and Einav (36) also found no impact of upgrading from secondary to primary enforcement on MVA death rates, despite the fact that upgrading did increase seat belt usage. However, their analysis was limited since only a few states had upgraded to primary enforcement during their period of observation (1983-1997). Houston and Richardson (37) analyzed the period from 1990 to 2002 using a design similar to ours and did find some evidence that upgrading reduced mortality rates per vehicle miles traveled; however their estimate of the magnitude of the policy impact (a decline 4.7%) was considerably less than the 17% reported by Lee and colleagues. Our results suggest that the effectiveness of primary enforcement laws may have diminished in recent years. Seat belt use has plateaued near 90%, so other environmental factors may now be playing a more prominent role.

It is also worth noting that the effects of other time-varying policy factors remained important in our analysis even after conditioning on state and time factors and mandatory seat belt laws. We found that speed limits in excess of 65 miles per hour increased MVA mortality rates by 15-20%, and other quasi-experimental research suggests that higher speed limits increase the probability of crashes, especially fatal ones (38). To be clear, we agree that seat belts save lives, and it remains important to further increase belt use in the United States. Given a good deal of evidence that upgrading from primary to secondary enforcement increases seat belt use, even in places where belt use is already high (39), there are likely some important benefits to upgrading. Nevertheless, some potential drawbacks of these policies have also been raised–particularly concerns that laws with primary enforcement may increase police harassment of minorities (40). Though there is currently little evidence that primary enforcement laws lead to racial differences in police enforcement (41), it remains a concern in some communities, and policymakers must nevertheless consider both benefits and drawbacks when considering whether or not to upgrade to primary enforcement.

Of course our analysis has limitations. We were not able to replicate exactly the results of Lee and colleagues and did not have data on graduated driver's license or administrative license suspension laws. Failure to account for these laws could lead to bias, but the bias would have to be very large to make our null estimates more consistent with those of Lee and colleagues. And though fixed state characteristics and time trends remove important sources of confounding, it of course remains possible that our analysis is confounded by other time-varying factors that are associated with the adoption of upgrades to primary enforcement and affect MVA death rates. Our difference-in-differences analysis would also be augmented by having additional data on MVA trends in states that did and did not upgrade to primary enforcement, where more rigorous testing of parallel trends prior to any policy changes could be evaluated (19, 42).

Finally, our study has implications for the ongoing "crisis" of reproducibility in biomedical and social science research. Much has been made of the problem of research findings that are fragile, do not replicate, or are later overturned by alternative study designs or analytic methods (43). There are multiple reasons for non-reproducibility, including poor design and measurement, excessive focus on statistical significance, conflicts of interest, and a misalignment between career incentives for researchers and the integrity of research output (44). Manski has argued that the incentive structure for making strong claims is particularly problematic in policy research, where policy analysis with "incredible certitude" (45) leads researchers to systematically downplay the role of uncertainty. When studies aimed at the answering the same question use different analytic methods and find strikingly different answers, there may be good reason for caution about the robustness of research findings. Studies that can successfully replicate earlier work and provide additional testing of critical assumptions have the potential to improve the overall quality of research output (46, 47, 48, 49), and we hope that future work evaluating the impact of safety legislation on MVA mortality will benefit from our evaluation.

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Reproducible Research Statement

A replication data set including the raw data and statistical code is publicly available and posted on Dr. Harper's Dataverse: https://dataverse.harvard.edu/dataverse/samharper.

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Appendix Tables

Replication of Lee and colleagues Table 2:

Table A.1: Average MVA rates by age group, 2001 and 2010

Age group	Year of death		
	2001	**2010**	**Total**
	Mean	Mean	Mean
10-14 y	3.3	1.9	2.6
15-19 y	23.8	13.2	18.5
20-24 y	25.2	16.8	21.0
25-34 y	15.4	11.3	13.3
35-44 y	12.1	8.4	10.3
45-54 y	11.2	7.3	9.3
55-64 y	11.2	7.5	9.3
65-74 y	14.5	8.6	11.5
75+ y	21.1	15.0	18.0
Total	15.3	10.0	12.7

Source: Author's calculations

Lee and colleagues Table 2:

Table 2. Motor Vehicle Crash Fatality Rates for 2001 and 2010*

Variable	**Mean Fatalities per 100 000 Persons, *n***	
	2001	**2010**
Entire study cohort aged ≥10 y	14.6	9.7
Adolescents and young adults aged 10–24 y	17.5	11.0
Adults aged ≥25 y	13.6	9.3
Age group		
10–14 y	3.3	1.9
15–19 y	24.1	13.3
20–24 y	25.6	17.2
25–34 y	15.6	11.4
35–44 y	12.2	8.5
45–54 y	11.4	7.4
55–64 y	11.6	8.0
65–74 y	14.5	8.9
≥75 y	21.8	15.7

* 50 states.

Replication of Lee and colleagues Table 3:

Table A.2: Unadjusted fatality rates and rate difference in states with primary and secondary laws, by age group

	All		10-24y		25+y	
	Rates	RD	Rate	RD	Rate	RD
Secondary	12.3		15.5		11.3	
	[10.7,13.9]		[13.6,17.4]		[9.8,12.8]	
Primary	10.1	-2.2	12.2	-3.2	9.3	-1.9
	[8.2,12.0]	[-4.6,0.1]	[10.1,14.3]	[-5.9,-0.5]	[7.5,11.2]	[-4.2,0.3]
N	4500	4500	1500	1500	3000	3000

Poisson regression, standard errors clustered at the state level. 95% confidence interval in brackets.

Lee and colleagues Table 3:

Table 3. Motor Vehicle Crash Fatality Rates, by Type of Law

Variable	**Mean Fatalities per 100 000 Persons, *n***		**Rate Difference (95% CI)**
	Primary Seat Belt Law	**Secondary Seat Belt Law**	
Entire study cohort aged ≥10 y	11.5	14.0	−2.5 (−3.5 to −1.4)
Adolescents and young adults aged 10–24 y	13.9	17.2	−3.3 (−4.5 to −2.1)
Adults aged ≥25 y	10.8	13.0	−2.2 (−3.2 to −1.2)

Replication of Lee and colleagues Appendix Table 2:

Table A.3: GEE models for primary law by age group

	Ages 10+ IRR	Ages 10-24 IRR	Ages 25+ IRR
Primary law	0.87	0.84	0.88
	[0.82,0.92]	[0.78,0.90]	[0.83,0.94]
Speed limit <=65 mph	0.74	0.77	0.72
	[0.64,0.85]	[0.67,0.89]	[0.62,0.84]
BAC 0.08 law	0.97	0.94	0.99
	[0.92,1.04]	[0.89,1.00]	[0.93,1.05]
VMT (10 billions)	0.99	0.99	0.99
	[0.99,1.00]	[0.99,1.00]	[0.99,1.00]
Median household income (10000s)	0.73	0.72	0.74
	[0.69,0.78]	[0.67,0.77]	[0.70,0.78]
N	500	500	500

Exponentiated coefficients; 95% confidence intervals in brackets

Lee and colleagues Appendix Table 2:

Appendix Table 2. Statistical Models of the Analyses

State-Level Factor	Adjusted IRR (95% CI)
GEE model for adolescents and young adults aged 10-24 y*	
Primary seat belt law	0.80 (0.74–0.87)
Speed limit ≤65 mph	0.72 (0.60–0.88)
BAC law†	0.89 (0.83–0.96)
GDL law	0.89 (0.81–0.97)
Immediate administrative license suspension law	0.96 (0.90–1.03)
Median income‡	0.84 (0.77–0.91)
Vehicle miles driven per person§	1.00 (0.99–1.00)
GEE model for adults aged ≥25 y*	
Primary seat belt law	0.85 (0.79–0.91)
Speed limit ≤65 mph	0.70 (0.56–0.88)
BAC law†	0.94 (0.89–1.00)
GDL law	0.90 (0.82–0.97)
Immediate administrative license suspension law	0.97 (0.93–1.02)
Median income‡	0.86 (0.81–0.93)
Vehicle miles driven per person§	1.00 (0.99–1.00)

BAC = blood alcohol concentration; GDL = graduated driver licensing; GEE = generalized estimating equation; IRR = incidence rate ratio; mph = miles per hour.
* 500 observations.
† Maximum legal BAC of 0.08 g/dL to drive.
‡ Indexed in $10 000 intervals.
§ Average 10 000 mi for a given state.

Difference-in-differences model

Table A.4: Incidence rate ratios and 95% confidence intervals from difference-in-differences models

	Ages 10 and over		Ages 10-24		Ages 25 and over	
	IRR	95%CI	IRR	95%CI	IRR	95%CI
Primary law	1.00	[0.95,1.06]	0.99	[0.92,1.07]	1.00	[0.95,1.06]
Speed limit 65 mph	1.00	[1.00,1.00]	1.00	[1.00,1.00]	1.00	[1.00,1.00]
Speed limit 70 mph	1.17	[1.12,1.22]	1.15	[1.10,1.21]	1.17	[1.12,1.22]
Speedlimit 75 mph	1.29	[1.17,1.42]	1.23	[1.10,1.38]	1.31	[1.19,1.45]
BAC 0.10 law	1.00	[0.94,1.06]	1.04	[0.98,1.11]	0.98	[0.91,1.05]
BAC 0.08 law	0.98	[0.92,1.03]	0.98	[0.92,1.04]	0.97	[0.91,1.03]
VMT (10 billions)	0.97	[0.93,1.00]	0.98	[0.94,1.03]	0.96	[0.93,0.99]
Median income (10000s)	1.02	[0.96,1.08]	1.07	[0.99,1.16]	1.00	[0.94,1.06]
Ages 10-14y	1.00	[1.00,1.00]	1.00	[1.00,1.00]		
Ages 15-19y	8.28	[7.68,8.93]	8.29	[7.68,8.94]		
Ages 20-24y	9.27	[8.57,10.02]	9.29	[8.59,10.05]		
Ages 25-34y	5.52	[5.15,5.91]			1.00	[1.00,1.00]
Ages 35-44y	4.22	[3.98,4.47]			0.77	[0.75,0.78]
Ages 45-54y	3.87	[3.66,4.09]			0.70	[0.68,0.72]
Ages 55-64y	3.82	[3.60,4.05]			0.69	[0.66,0.72]
Ages 65-74y	4.59	[4.28,4.93]			0.83	[0.78,0.89]
Ages 75+y	7.42	[6.82,8.08]			1.35	[1.26,1.45]
2001	1.00	[1.00,1.00]	1.00	[1.00,1.00]	1.00	[1.00,1.00]
2002	1.03	[1.01,1.06]	1.07	[1.03,1.12]	1.02	[0.99,1.04]
2003	1.01	[0.98,1.05]	1.02	[0.97,1.06]	1.01	[0.98,1.05]
2004	1.00	[0.95,1.06]	1.01	[0.94,1.09]	1.00	[0.95,1.06]
2005	0.98	[0.92,1.04]	0.95	[0.88,1.03]	0.99	[0.93,1.06]
2006	0.94	[0.88,1.01]	0.92	[0.85,1.01]	0.95	[0.88,1.02]
2007	0.89	[0.82,0.96]	0.86	[0.78,0.95]	0.89	[0.82,0.97]
2008	0.77	[0.71,0.83]	0.69	[0.62,0.77]	0.80	[0.74,0.86]
2009	0.69	[0.64,0.75]	0.61	[0.55,0.67]	0.73	[0.67,0.79]
2010	0.65	[0.59,0.71]	0.56	[0.49,0.63]	0.69	[0.63,0.75]
N	4500		1500		3000	

State fixed effects not shown, standard errors clustered at the state level. 95% confidence interval in brackets.

Alternative GEE models:

Table A.5: GEE models with trends and alternative auto-regressive correlation structure

	Exchangeable IRR	Exchangeable IRR	Exchangeable IRR	Autoregressive(1) IRR	Autoregressive(1) IRR
Primary law	0.95	0.99	0.98	0.97	1.02
	[0.86,1.04]	[0.95,1.03]	[0.94,1.03]	[0.90,1.04]	[0.97,1.08]
Speed limit <=65 mph	0.85	0.70	0.71	0.71	0.70
	[0.69,1.04]	[0.55,0.89]	[0.54,0.92]	[0.59,0.85]	[0.56,0.88]
BAC 0.08 law	1.07	1.00	1.00	0.98	0.99
	[1.00,1.14]	[0.95,1.05]	[0.95,1.05]	[0.93,1.03]	[0.95,1.04]
VMT (10 billions)	0.99	0.99	0.99	0.99	0.98
	[0.98,1.00]	[0.98,1.00]	[0.98,1.00]	[0.98,0.99]	[0.97,0.99]
Median income (10000s)	1.00	1.00	0.99	0.94	1.01
	[0.95,1.05]	[0.95,1.06]	[0.94,1.04]	[0.90,0.98]	[0.98,1.04]
% Ages 10-24y	1.10		1.02		1.01
	[1.06,1.14]		[0.97,1.08]		[0.93,1.08]
% Ages 25-44y	1.08		1.03		1.02
	[1.05,1.10]		[0.98,1.08]		[0.96,1.09]
% Ages 65+y	0.93		1.00		1.00
	[0.83,1.05]		[0.94,1.07]		[0.92,1.09]
Year fixed effects					
2002		1.02	1.04		1.04
		[1.00,1.05]	[1.00,1.09]		[0.99,1.09]
2003		1.00	1.04		1.03
		[0.97,1.03]	[0.97,1.11]		[0.95,1.11]
2004		0.98	1.04		1.02
		[0.94,1.03]	[0.94,1.15]		[0.91,1.14]
2005		0.96	1.03		1.00
		[0.90,1.02]	[0.90,1.18]		[0.86,1.17]
2006		0.92	1.01		0.97
		[0.86,0.98]	[0.87,1.18]		[0.82,1.16]
2007		0.87	0.97		0.92
		[0.80,0.93]	[0.81,1.16]		[0.75,1.12]
2008		0.75	0.85		0.80
		[0.70,0.81]	[0.70,1.03]		[0.65,0.99]
2009		0.68	0.78		0.73
		[0.63,0.73]	[0.63,0.97]		[0.58,0.92]
2010		0.63	0.74		0.69
		[0.59,0.69]	[0.59,0.93]		[0.53,0.88]
N	500	500	500	500	500

Exponentiated coefficients; 95% confidence intervals in brackets

Forest plot of age-specific effects:

Figure A.1: Age-specific effects of primary vs. secondary enforcement on MVA accidents