Would Stronger Seat Belt Laws Reduce Motor Vehicle Crash Deaths? A Semi-Bayesian Analysis

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Abstract

Background: For policy questions where substantial empirical background information exists, conventional frequentist policy analysis is hard to justify. Bayesian analysis quantitatively incorporates prior knowledge, but is not often used in applied policy analysis. **Methods:** We combined 2000-2016 data from the Fatal Analysis Reporting System with priors based on past empirical studies and policy documents to study the impact of mandatory seat belt laws on traffic fatalities. We used a Bayesian data augmentation approach to combine information from prior studies with difference-in-differences analyses of recent law changes to provide updated evidence on the impact that upgrading to primary enforcement of seat belt laws has on fatalities. **Results:** After incorporating the evidence from past studies, we find limited evidence to support the hypothesis that recent policy upgrades affect fatality rates. We estimate that upgrading to primary enforcement reduced fatality rates by 0.37 deaths per billion vehicle miles traveled (95% posterior interval -0.90, 0.16), or a rate ratio of 0.96 (95% posterior interval 0.91, 1.02), and increased the proportion of decedents reported as wearing seat belts by 7 percentage points (95% posterior interval 5, 8), or a risk ratio of 1.18 (95% posterior interval 1.13, 1.24). **Conclusion:** Bayesian methods can provide credible estimates of future policy impacts, especially for policy questions that occur in dynamic environments, such as traffic safety.

Introduction

Despite long-term progress in reducing deaths from motor vehicle crashes in the United States, there remains substantial room for improvement. The US still registers more than 30,000 deaths and 2 million non-fatal injuries annually, and motor vehicle crashes remain a leading cause of death for young people. Moreover, the United States lags behind many other rich countries in measures of traffic safety. A recent comparison [1] found the US had the highest traffic crash death rate per 100,000 population among 20 high-income (as defined by the World Bank) country members of the Organization of Economic Co-operation and Development, and ranked 18th of 20 in terms of observer-reported seat belt use.

The poor international ranking and substantial ongoing burden of crash-related injuries and deaths in the US has prompted a number of calls to implement stronger safety legislation [1]. A good deal of safety legislation is enacted at the state level, and states have a number of potential policy options. A recent report by the RAND organization [2] and a tool [3] developed by RAND for the US Centers for Disease Control and Prevention (CDC) provide detailed cost-effectiveness estimates for range of policies such as increasing sobriety checkpoints, automated speed-trap and red-light cameras, and higher fines for not wearing seat belts.

One policy that is routinely encouraged [4] and considered cost-effective [2] is primary enforcement of mandatory seat belt laws. Primary enforcement means drivers and passengers may be ticketed directly for not using seat belts, rather than being additionally cited after another traffic violation (i.e., secondary enforcement). The RAND report and the CDC's *Motor Vehicle Prioritizing Interventions and Cost Calculator for States* presently use estimates for the effectiveness of upgrading seat belt laws to primary enforcement based on studies conducted in the 1980s and 1990s. In light of past findings, both the CDC and the National Highway Transportation Safety Association have emphasized the need for all states with secondary laws to upgrade to primary enforcement.

Currently, for adults 35 states and the District of Columbia have a law with primary enforcement and 15 states have secondary enforcement (New Hampshire currently has no law covering adults, see eFigure 1). A number of states may be considering passing legislation to upgrade their mandatory seat belt laws from secondary to primary enforcement. Since the last rigorous studies of primary enforcement upgrades were conducted in the early 2000s [5,6], another 16 states have upgraded their laws to primary enforcement, which provides an opportunity to revise estimates of their impact.

A typical frequentist policy analysis would take advantage of the recent changes in state laws to estimate their impact on crash deaths. However, it seems odd to adopt a purely frequentist stance when considering the question of whether states with secondary enforcement laws should upgrade to primary enforcement. A frequentist analysis takes no account of background information and would thus consider it equally likely that upgrading to primary enforcement decreases death rates by a factor of 100 or 10, or that upgrading actually *increases* death rates by a factor 10 or 100. This is empirically absurd, given what is already known from prior studies about how primary enforcement affects crash deaths. We used Bayesian methods to combine prior information about the effect of upgrading with a difference-in-differences analyses of recent law changes to provide updated evidence on the impact that upgrading to primary enforcement has on motor vehicle crash deaths.

Methods

Prior evidence

To incorporate prior information on the impact of seat belt law upgrades on fatalities, we reviewed prior empirical studies (see eAppendix for study details), paying particular attention to those cited as strong evidence for upgrading to primary enforcement [1,4,7–9]. Most of these studies have focused on two outcomes: fatality rates per vehicle mile traveled, and the proportion of fatalities wearing seat belts.

A key challenge in estimating the impact of primary enforcement upgrades is the non-random nature of policy adoption. For this reason we prioritized studies that used a credible design for estimating our

parameter of interest, typically an interrupted time series with an unaffected control group [10], or a difference-in-differences design [11, 12] that took advantage of multiple policy changes.

To inform our empirical priors we used three studies of primary law upgrades [13–15] that estimated the impact on fatality rates per vehicle mile traveled, and one report [16] of six separate studies on the proportion of fatal occupants wearing seat belts. We took these estimates and conducted random effects meta-analyses [17], which treat each study as a separate estimate and combine them into an average effect estimate, assuming that the studies are sampled from a distribution of possible studies. We tested for heterogeneity across studies using Cochran’s Q statistic [18] and used 95% prediction intervals to estimate the expected range of effect sizes for a new study [17].

eFigure 2 shows forest plots of prior studies for the impact on fatality rates. The average estimate for fatality rates is 0.5 fewer deaths per billion vehicle mile traveled (roughly 1500 deaths per year), with a 95% confidence interval of (-0.7 to -0.3), and there is not enough evidence to reject the assumption of homogeneity in the effects $Q = 1.78$, ($df = 2$), $p = 0.411$. On the rate ratio scale the average estimate is 0.95 (0.93, 0.97), again with little evidence against homogeneity across studies. eFigure 3 shows the average estimate for the proportion of deaths wearing seat belts and implies that upgrading to primary enforcement increases the proportion of fatal occupants wearing seat belts by 13 percentage points (95% CI: 10 to 16). However, there is evidence of between-study heterogeneity $Q = 21.18$, ($df = 5$), $p = 0.001$.

Construction of priors

We constructed three priors for the effect (rate ratio) of upgrading for each of our outcomes: non-informative, empirical, and subjective. A “non-informative” prior provides minimal information, akin to a frequentist analysis, which we implemented as a lognormal distribution centered at 0 (i.e., a rate ratio of 1) with a large variance: $N(\ln(1), 100)$. We used 95% prediction intervals from the meta-analysis results to inform our empirical priors, accounting for the uncertainty within and between prior studies [17, 19]. For mortality we take as plausible priors the 95% limits of the prediction interval, representing a 95% chance that the true RR lies between 0.83 and 1.09. This leads to a prior centered at

$$\beta_{prior} = \frac{\ln(0.83) + \ln(1.09)}{2} = \ln(0.95) = -0.05 \text{ with a prior variance of } v_{prior} = \left[\frac{|\ln(0.83) - \ln(1.09)|}{2 \cdot 1.96} \right]^2 = 0.005,$$

i.e., $N(\ln(0.95), 0.005)$. For the proportion belted we used the same non-informative prior. The meta-analysis estimate of the increase in proportion belted due to primary enforcement was 0.13, with a prediction interval of 0.03 to 0.23. Using a base rate of 40% of belted deaths in our sample, this translates into a risk ratio of 1.30 with limits of 1.08 and 1.58. We therefore used an empirical prior of $N(\ln(1.30), 0.009)$, which translates into a 95% chance that the true parameter lies between an 8% and a 58% increase in the proportion of deaths wearing seat belts.

Finally, we also used subjective priors based on claims about the magnitude of primary enforcement laws in the policy literature [3, 4]. Estimates quoted for the impact of upgrading from secondary to primary enforcement are around 7-8% on fatalities, and 12 to 18 percentage points on seat belt use. We added some additional uncertainty to these estimates and used 0.8 and 1.0 as plausible priors for the RR of primary vs. secondary on mortality to generate a lognormal prior of $N(\ln(0.90), 0.003)$. For the proportion belted the estimates of 12 to 18 percentage point increases correspond to approximate risk ratios of 1.3 and 1.45. We added some additional uncertainty to these estimates to derive a subjective prior based on a 95% chance that the true RR lies between 1.15 and 1.60, implemented as a lognormal prior $N(\ln(1.36), 0.007)$.

Data

This study used de-identified data and did not require ethics review. We obtained person-level data on fatal crashes among drivers and passengers from the Fatal Analysis Reporting System [20] for the years 2000-2016. We gathered information on the date and type (secondary or primary enforcement) of mandatory seat belt policies from the Insurance Institute for Highway Safety [21]. We included all states except New Hampshire, which is the only state without a mandatory seat belt law covering adults. We

aggregated the person-level crash data by state, age group, and year of crash, and we used the effective date of each law to calculate the proportion of months in a given year a state had a primary law in effect (effective dates are given in eTable 1).

Crash risk strongly increases with the amount of driving. To account for travel exposure we calculated annual estimates of state-specific vehicle miles traveled from the Fatal Analysis Reporting System website [22], which provides the number of fatalities and the fatality rate per 100 million vehicle miles traveled annually for each state from 2000-2016. Estimates travel exposure were not available by age at the state level. To control for potential confounding by other policies we also obtained time-varying data on laws pertaining to maximum speed limits, legal limits for blood alcohol concentration, and graduated driver’s license programs [23], and, additionally, annual state police per capita (2000-2015) as a proxy for traffic safety enforcement [24], state average alcohol consumption from 2000-2015 [25], and annual state median household income (constant dollars) from the US Census [26]. Police per capita and alcohol consumption were linearly extrapolated to 2016 from 2000-2015 data.

Statistical Methods

Models for likelihood

Similar to prior studies, we used a difference-in-differences design [11, 12] to estimate the effects of upgrading to primary enforcement. That is, we compared the difference in the outcome before and after upgrading to primary enforcement among states that upgraded with the difference in the outcome among states that did not upgrade their laws during the same period. We implemented this using Poisson regression to model the number of crash deaths with the state-year-specific estimate of vehicle miles traveled as the offset. We assume the number of fatalities in age group a in state s at time t is conditionally Poisson, i.e., $y_{ast} \sim \text{Poisson}(\mu_{ast})$:

$$\ln(\mu_{ast}) = \alpha + \beta \times \text{Primary}_{st} + \gamma \mathbf{A}_{ast} + \delta \mathbf{Z}_{st} + \sigma_s + \tau_t + \ln(VMT_{st})$$

where β is our coefficient of interest ($\text{Primary} = 1$ when a primary enforcement law is in effect in state s at time t), and γ , δ , σ , and τ represent vectors of coefficients for age groups, other time-varying state covariates, state fixed effects, and year fixed effects, respectively. State fixed effects control for any time-constant unobserved differences across states that may also be correlated with death rates. Year fixed effects flexibly control for any unmeasured factors that may be affecting death rates in all states.

To estimate the effect on the proportion of belted fatalities we used a grouped logistic regression model since we have—for each age group, state, and year—the number of all fatalities where decedents were not wearing seat belts as well as the total number of fatalities, so we modeled the proportion p_{ast} of all fatalities n_{ast} where occupants were wearing seat belts:

$$\ln(p_{ast}/[1 - p_{ast}]) = \alpha + \beta \times \text{Primary}_{st} + \gamma \mathbf{A}_{ast} + \delta \mathbf{Z}_{st} + \sigma_s + \tau_t$$

where p_{ast} is the probability that a fatality was wearing a seat belt and the model coefficients are defined similarly as for the Poisson regression but on the log odds scale. The grouped logistic regression is fit using maximum likelihood [27], and standard errors were clustered at the state level [28, 29]. We used coefficients from these models to generate, respectively, marginal predicted motor vehicle crash death rates (Poisson model) and proportions of fatalities wearing seat belts (grouped logit model). Marginal predictions were restricted to the observed data (i.e., we did not average predictions over the prior data). We calculated differences and rate ratios of our marginal predictions and used the delta method to calculate standard errors for these quantities. We also checked for compatibility between the prior and the data using the approach outlined by Greenland [30], akin to a test of homogeneity, in which a low p -value may indicate incompatibility between the prior and the observed data.

Construction of the prior data

Improvements in computing power and the incorporation of Bayesian analytic techniques in commercial and open source software have improved the prospects for utilizing Bayesian analysis in policy research [31], usually via repeated sampling such as Markov chain Monte Carlo. However, Bayesian analysis may also be implemented via data augmentation [30], which adds observations representing the prior information to the empirical dataset. The data augmentation approach imposes a prior distribution on our coefficient of interest (β) of the generalized-conjugate form that allows considerable flexibility to incorporate diverse priors [32]. We adopted a data augmentation approach for two reasons. One is the ease with which data augmentation simplifies the analysis, since it may be implemented using traditional likelihood-based algorithms [33]. The second reason is that analysis of state-level policy changes typically need to incorporate cluster robust inference [29] and, to our knowledge, developing priors (or hyperpriors) to account for alternative correlation structures (e.g. serial correlation) in state-level panel data is still an area of active research [34].

Bayesian analysis via data augmentation proceeds similarly to traditional likelihood-based regression, but the information encoded in the priors is added to the dataset as additional observations. Since our only interest is in the parameter on primary enforcement, we only added a single additional observation for each prior, making our analysis semi-Bayesian rather than fully Bayesian (see eAppendix for a comparison with fully Bayesian models). In essence, constructing the prior data translates the prior into the number of events (crash deaths or belted deaths) that would have come from a prior experiment on the question at hand. That is, we ask, what previous experiment would convey the same information as our priors? For Poisson models A equals the observed count of deaths for observed records, and $A = S^2/v$ gives the number of additional deaths represented by the prior data record, where S is a rescaling factor to improve asymptotic approximation [35] and v is the prior variance. The offset term is $\log(\text{vehicle miles traveled})$ for observed records and calculated as $A/e^{m/S}$, where m is the prior mean. For the prior observations the exposure of interest (primary law) is set to $1/S$ and all other covariates are set to 0. Table 1 provides an overview of the parameters resulting from our selection of priors.

We analyzed the augmented data using maximum-likelihood Poisson regression with cluster-robust variance (clustered at the state level [29]). The maximum likelihood estimate of the mean of β gives the approximate posterior median and the 95% confidence limits for β are the approximate posterior limits (2.5th and 97.5th posterior percentiles) [33].

Sensitivity analysis

Because Poisson regression assumes the mean and variance are constant, we also ran a negative binomial model to account for overdispersion, though we note that unconditional negative binomial fixed effects models may be subject to the “incidental parameters” problem and conditional negative binomial models are not true fixed effects models [36,37]. In additional models, we tested for lagged policy effects by allowing the impact of the law to occur 1 year after implementation. We also investigated different control groups and conducted analyses restricting the control group to states that either maintained exclusively secondary or exclusively primary enforcement throughout the entire period of analysis. Additionally, given the decrease in national motor vehicle crash death rates after the Great Recession [38], which began in December 2007, we estimated models that only used the pre-recession time period from 2000-2007. For each of these models we used subjective priors since they represent the strongest prior information about the policy impact. We also considered whether the fraction of fatal crashes occurring on rural roads or involving alcohol may be time-varying confounders and ran additional models that: a) included these two covariates measured in the same state-year of the crash; b) excluded both covariates; and c) included 1-year lagged values of both covariates to avoid conditioning on factors potentially affected by the treatment.

Results

Table 2 shows means and standard deviations for covariates among states that maintained secondary enforcement, already had primary enforcement, and upgraded from secondary to primary enforcement. Notable differences in, for example, exposure to speed limit and blood alcohol policies, between the kinds of states that did and did not alter their seat belt policy suggests that unmeasured fixed state characteristics could confound the effect of upgrading to primary enforcement. Our difference-in-differences analysis should help to mitigate this source of bias.

Table 3 displays our semi-Bayesian estimates of the impact of upgrading (eFigure 4 shows the prior, likelihood, and posterior distributions on the rate difference and rate ratio scale). The non-informative prior generates posterior estimates similar to a frequentist analysis, which show little evidence for any effect on fatality rates. Upgrading to primary enforcement reduced fatality rates by 0.24 deaths per billion vehicle miles traveled (95% posterior limits -0.72, 0.24). Using our empirical prior, which was centered on a 5% reduction in crash fatality rates, pulls the posterior estimate slightly further from the null (Rate Difference [RD] -0.32, 95% posterior limits -0.82, 0.17), but because the likelihood estimate provides more information, it tends to dominate the posterior and still suggests limited evidence of an impact on crash deaths. Using our subjective prior provides qualitatively similar results (RD -0.37, 95% posterior limits -0.90, 0.16, RR of 0.96, 95% posterior interval 0.91, 1.02), and p -values for tests of compatibility between the data and prior were 0.99 for non-informative, 0.72 for empirical, and 0.18 for subjective priors.

Table 4 shows the posterior estimates from the grouped logistic regression for the proportion of deaths wearing seat belts (eFigure 5 shows the prior, likelihood, and posterior distributions on the risk difference and risk ratio scale). In contrast to the results for fatality rates, the posterior estimates using a non-informative prior do suggest that upgrading to primary enforcement increases the proportion of deaths wearing seat belts by around 6 percentage points (95% posterior limits 5, 7). The posterior estimates using both empirical and subjective priors are nearly identical (posterior limits of 5 and 8 percentage points for both estimates), and p -values for tests of compatibility between the data and prior were 0.99 for non-informative, 0.26 for empirical, and 0.07 for subjective priors.

Sensitivity analyses (eTables 2 and 3) generally supported the main results, showing limited evidence of impact on crash death rates, and similar positive estimates for the impact on the proportion of decedents wearing seat belts. The fully Bayesian analysis also produced estimates similar to our semi-Bayesian results (eTable 4), and our estimates were not sensitive to the inclusion of alcohol-involved or rural road deaths (eTable 5). Complete regression results for the fully-adjusted frequentist difference-in-difference models are given in eTables 6 and 7).

Discussion

In this study we provide a semi-Bayesian analysis of the impact on fatalities of upgrading seat belt laws to primary enforcement that explicitly accounts for prior evidence through the use of data augmentation. We encoded information from prior studies on primary enforcement upgrades using empirical priors from random-effects meta-analysis, as well as subjective priors based on policy documents. For the outcome of fatality rates, the posterior distribution suggests a weak impact of upgrading, on the order of a 3% decrease or 0.3 fewer deaths per billion vehicle miles traveled, even with priors centered at a 10% reduction. To achieve posterior estimates more consistent with current claims [1,8,9] about the expected mortality benefits of upgrading to primary enforcement on crash fatality rates would require considerably stronger, and perhaps unrealistic, priors. With respect to the impact on the proportion of deaths wearing seat belts, our posterior estimates using either empirical or subjective priors are a relatively precisely estimated 6 percentage point increase. This is consistent with a beneficial impact of upgrading, but the magnitude of this estimate is notably smaller than the 12 to 18 percentage point increase cited in some policy documents [16].

Overall, our analysis suggests that current policy documents [1,3,8,9] could be updated to reflect the totality of evidence on primary enforcement of seat belt laws. For the 15 states with secondary enforcement

that may be considering upgrading to primary, the potential benefits of increasing seat belt use on reducing mortality should be considered in light of other potential policy impacts. In this paper we are chiefly concerned with providing an estimate of the impact of upgrading to primary enforcement on occupant fatalities and seat belt use, rather than an all-things-considered judgement. Concerns that primary enforcement laws may lead to increases in police harassment of minority drivers are longstanding, but there is little rigorous empirical evidence on this question [39]. Clearly, concerns about paternalism, police harassment [40], and the additional costs of enforcement (such as "Click-It-Or-Ticket" or other media campaigns) are relevant to any policy decision, but a full accounting of those impacts is beyond the scope of this paper.

We do find evidence that upgrading to primary enforcement led to increases of roughly 6 to 7 percentage points in the proportion of crash deaths wearing a seatbelt. Although this effect is smaller than the estimates usually given in policy documents regarding the benefits of primary enforcement, it nevertheless does provide additional evidence that primary enforcement increases seat belt use. More intriguing is the combined evidence that, despite the increase in seat belt use, we do not estimate important decreases in crash death rates. This suggests that further increases in seat belt use may not provide the same degree of benefit today as was seen in the past. There are several potential reasons why this may be true, one of which is effect measure modification. It is relatively well established in the traffic safety literature that early adopters of seat belts tended to be less risky drivers [41, 42]. That is to say, when the first mandatory seat belt laws were implemented in the 1980s and seat belt use was infrequent, those who complied with mandatory laws tended to be drivers that were less likely to be involved in more severe crashes [43]. Now that seat belt use is much more common, the mortality benefits of recent upgrades to primary enforcement may be diluted because those recently converting to seat belt use are no longer among the population of safer-than-average drivers, although there is little direct evidence to that effect.

Another potential explanation for the reduced impact on crash deaths relative to prior literature is that the traffic safety environment has changed in several ways since the 1990s when many initial studies of primary enforcement were first conducted. US seat belt use is now approaching 90% [44] and other environmental factors such as changes in road design and additional vehicle safety features (front and side air bags, electronic stability control, daytime running lights, lane control, etc.) may now be playing a more prominent role. Recent studies of changes in vehicle design suggest strong impacts on motor vehicle crash death rates [5, 6], so it is not clear whether states presently considering upgrading their existing seat belt laws to primary enforcement can expect the benefits seen in prior studies.

Our analysis has limitations. The difference-in-differences model we used assumes that pre-intervention trends between states that did and did not change policy are parallel, and, of course, that there are no unmeasured factors that are associated with the timing of policy adoption and also affect outcomes. Our current estimates, as well as prior frequentist results [45], were robust to alternative control groups, lagging the policy change by one year, and restriction to years prior to the Great Recession, but unmeasured confounding can never be ruled out. We did not conduct a systematic review to inform our empirical priors and we used our subjective assessments of prior study quality to arrive at the meta-analyzed sample. It is possible that we may have missed other relevant studies, but given the extent to which the likelihood dominates our inference, it seems unlikely that any missed studies would lead to substantively different conclusions about the magnitude or sign of the effect [46]. Our results reflect the subjectivity of our chosen priors, and we have made our computing code and data publicly available (<https://osf.io/em2y7/>) to allow others to examine the influence of using different credible priors [31].

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Table 1: Prior record construction for (a) Poisson regression models and (b) grouped logistic regression models

(a) Poisson model: Fatalities per vehicle mile traveled (VMT)

Prior	RR	$Var(\ln[RR])$	95% limits	Deaths	Implied VMT (billions)
Non-informative	1.0	100.0	3e-9, 4e+8	6.25	6.51
Empirical	0.95	0.005	0.83, 1.09	125000	129841
Subjective	0.90	0.003	0.80, 1.00	208333	215970

(b) Grouped logit model: Proportion of deaths wearing seat belts

Prior	RR	$Var(\ln[RR])$	95% limits	Deaths	Implied offset
Non-informative	1.0	100.0	3e-9, 4e+8	4	0
Empirical	1.30	0.009	1.08, 1.58	22222	-0.0693
Subjective	1.36	0.007	1.15, 1.60	28571	-0.1099

Note: Both models use a scaling factor of $s=25$. RR = Rate Ratio

Table 2: Covariates for states by enforcement of mandatory seat belt laws, 2000-2016.

Variable	Remained Secondary		Remained Primary		Upgrade to Primary	
	Mean	SD	Mean	SD	Mean	SD
Age group, %						
0-10 years	13	33	14	34	13	33
10-14 years	7	25	7	26	7	25
15-19 years	7	26	7	26	7	25
20-24 years	7	25	7	26	7	25
25-34 years	13	34	14	35	13	34
35-44 years	14	34	14	35	14	34
45-54 years	14	35	14	35	14	35
55-64 years	12	32	11	31	11	32
65-74 years	7	26	7	25	8	26
75+ years	6	25	6	23	7	25
Max speed limit, %						
<70 MPH	60	49	30	46	36	48
70 MPH	9	29	48	50	57	49
>70 MPH	30	46	22	41	7	26
BAC law, %						
BAC<0.10	13	32	5	20	18	37
BAC<0.08	85	34	95	20	87	31
Graduated license, %						
Night restriction	90	29	95	20	90	29
Passenger restriction	63	48	85	35	57	49
Alcohol per capita, gallons	3	0	2	0	3	0
Police per capita	16	6	16	6	17	4
Median income (10,000s)	6	1	6	1	6	1
VMT (billions)	73	32	175	112	89	57
Unbelted deaths, %	58	16	45	17	52	18

SD=standard deviation; MPH = miles per hour; BAC = blood alcohol concentration; VMT = vehicle miles traveled.

Table 3: Estimated effect of upgrading from secondary to primary enforcement of seat belt laws on motor vehicle crash death rates per billion VMT, US states, 2000-2016.

	Non-informative prior: $N(\ln(1),100)$		Empirical prior: $N(\ln(0.95),0.005)$		Subjective prior: $N(\ln(0.90),0.003)$	
	Secondary	Primary	Secondary	Primary	Secondary	Primary
Estimated Rates	9.7 [9.5,10.0]	9.5 [9.3,9.7]	9.8 [9.5,10.0]	9.5 [9.2,9.7]	9.8 [9.5,10.1]	9.4 [9.2,9.7]
Effect measures						
Rate difference		-0.24 [-0.72,0.24]		-0.32 [-0.82,0.17]		-0.37 [-0.90,0.16]
Rate ratio		0.98 [0.93,1.02]		0.97 [0.92,1.02]		0.96 [0.91,1.02]

Note: Poisson regression with vehicle miles traveled (billions) as offset. Standard errors clustered by state. 95% posterior limits in brackets. Adjusted for age group, presence or absence of speed limit laws (≤ 65 , 70, >70), graduated driver's license laws (night restrictions, passenger restrictions), maximum blood alcohol content laws (0.08, 0.10 g/dl), alcohol consumption per capita, police officers per capita, state median income, and state and year fixed effects. (n=8501).

Table 4: Estimated effect of upgrading from secondary to primary enforcement of seat belt laws on the proportion of crash deaths wearing seat belts, US states, 2000-2016.

	Non-informative prior: $N(\ln(1),100)$		Empirical prior: $N(\ln(1.30),0.009)$		Subjective prior: $N(\ln(1.36),0.007)$	
	Secondary	Primary	Secondary	Primary	Secondary	Primary
Estimated Risk	0.36 [0.35,0.37]	0.42 [0.42,0.43]	0.36 [0.35,0.37]	0.42 [0.42,0.43]	0.36 [0.35,0.37]	0.43 [0.42,0.43]
Effect measures						
Risk difference		0.06 [0.05,0.07]		0.06 [0.05,0.08]		0.07 [0.05,0.08]
Risk ratio		1.17 [1.13,1.21]		1.17 [1.13,1.22]		1.18 [1.13,1.24]

Note: Grouped logistic regression. Standard errors clustered by state. 95% posterior limits in brackets. Adjusted for age group, presence or absence of speed limit laws (≤ 65 , 70 , > 70), graduated driver's license laws (night restrictions, passenger restrictions), maximum blood alcohol content laws (0.08, 0.10 g/dl), alcohol consumption per capita, police officers per capita, state median income, and state and year fixed effects (n=8501).

eAppendix: Would Stronger Seat Belt Laws Reduce Motor Vehicle Crash Deaths? A Semi-Bayesian Analysis

Sam Harper

2019-03-21

1 Current state policies

eFigure 1: Current mandatory seat belt laws for adults as of 2018

2 Prior literature

To incorporate prior information on the impact of seat belt law upgrades on fatalities, we reviewed prior studies, paying particular attention to those cited as evidence for upgrading to primary enforcement [1–5]. Early studies compared the impact of moving from no law to a law with either primary or secondary enforcement, and found benefits for any legislation, but stronger benefits for primary laws [6–9]. However, because all states except New Hampshire presently have a secondary law, our prior should reflect the potential impact of upgrading from secondary to primary enforcement.

A key challenge in estimating the impact of primary enforcement upgrades is the non-random process of policy adoption. States that upgrade their policy from secondary to primary enforcement are likely to be different from those that don’t upgrade in ways that are correlated with MVC rates, leading to biased estimates of policy impact. As an example, California was the first state to upgrade to primary enforcement in 1993. Thus, simply comparing fatality rates in states with primary vs. secondary enforcement laws is likely to overestimate any benefit. For example, a recent study by Lee and colleagues [10] found that states with primary enforcement has fatal crash rates that were 17% lower than secondary states, an estimate considerably higher than those we describe below. We therefore did not include this estimate because key sources of confounding, namely unmeasured state characteristics and time trends, were not controlled for in the study, though perhaps it could be considered as an upper bound for our prior.

2.1 Effect on MVC fatality rates

Two systematic reviews of mandatory seat belt laws were reported in the early 2000s by the Guide to Community Preventive Services Task Force [11, 12]. Dinh-Zarr et al. [11] cited 5 studies as providing evidence on the “incremental effectiveness” of primary vs. secondary laws. However, 4 of these studies [6–9] used time series data prior to the first state upgrading from secondary to primary in 1993 and the other [13] captures the change in California, but with only a single year of post-law data. Shults et al. [12] report between 8% and 15% decreases in fatal and non-fatal injuries combined, citing Jonah and Grant [14] and Farmer and Williams [15]. Only the Farmer and Williams study estimated the impact of upgrading from secondary to primary enforcement.

More recently, Nichols et al. [2] reported a median of 7-8% decline based on upgrades (no standard error was reported). Salzberg and Moffat [16] reported a 13.4% decrease (no uncertainty estimate) in occupant fatalities after an upgrade to primary enforcement in Washington state. Voas et al. [17] also report “median 15% reduction in alcohol-related deaths and a median 5% reduction in non-alcohol-related deaths in five case study states that implemented primary law upgrades.” Given that National Highway Traffic Safety Administration reports that approximately 30% of fatalities can be considered alcohol-related, this would suggest an overall impact of around 8%, consistent with prior estimates.

We used 3 studies to inform our empirical priors, all of which used an interrupted time series with an unaffected control group [18] or a difference-in-differences [19, 20] design. These studies all use within-state variation in mandatory laws (i.e., “switching” from a law with secondary to primary enforcement) and control for secular trends, either using fixed effects for time or ARIMA models, and at the very least are protected from bias due to fixed state characteristics or common time-varying factors.

- Cohen and Einav [21] used a difference-in-differences design to estimate the impact of mandatory laws on MVA fatality rates using 1983-1997 FARS data on all 50 states and DC (additionally, they instrumented for seat belt use to assess its impact as well). In addition to state and year fixed effects they also control for a large number of time-varying covariates (demographics, median income, unemployment, crime rates, speed limit laws, traffic density, drunk driving laws). They report (Table 4) that switching to primary enforcement increases seat belt use by 13.4 percentage points (no standard error but $p < 0.01$) and the reduced form estimate of the effect on occupant fatality rates (Table 5) is -0.0002 (SE = 0.0004) fatalities per VMT and -0.030 (SE = 0.024) per log(fatalities per VMT). We converted their estimate per VMT (which is technically per million VMT since their

estimates of VMT are in millions as reported by “[Highway Statistics](#)”) to 0.2 deaths per billion VMT. Their estimates of log VMT can be converted to estimated RR s by taking the 95% confidence limits for the $\log RR$ implied by the standard error and exponentiating, which gives $RR = 0.97$ and $95\%CI = 0.93, 1.02$.

- Farmer and Williams [15] used 1989-2003 FARS data for 10 states that upgraded and 14 that did not and employed a time-series cross sectional regression model (adjusted for the unemployment rate) to estimate the effect of upgrading to primary enforcement on fatality rates per billion VMT. Because they estimated proportional reduction in driver fatality rates as $E = 1 - \exp(\beta)$ this suggests they used a log-linear model. They report (Table 3) a model estimate of -0.07248 (SE=0.0217), implying a 7% reduction in driver fatality rates per billion VMT. Using the point estimate and implied confidence limits for their effect, we calculated a $RR = 0.93$ and $95\%CI = 0.89, 0.97$. To provide an estimate of the absolute reduction, we took the observed fatality rate in the treated states and the end of follow-up (6.36 per billion VMT) and estimated the counterfactual rate implied by the law effect: $6.36 * 1.07248 = 6.82$ and subtracted the difference, which provided an estimate of $6.36 - 6.82 = -0.497$ per billion VMT. Repeating this exercise with the 95% limits of the effect provided a 95% CI of -0.827 to -0.196.
- Houston and Richardson [22] used a similar design as Cohen and Einav [21] with 1990-2002 FARS data for all 50 states and DC. They also include in their fixed effects model controls for age distribution, income per capita, population density, alcohol consumption, speed limit, drunk driving, and graduated driver’s license laws. They report (Table 4) an effect estimate of -6.647 occupant deaths per 10 billion VMT in states that upgrade to primary (vs. remaining secondary), with a SE of 2.014, which we converted to 95% confidence limits of -10.59 and -2.70. Houston and Richardson report (Table 3) similar estimates for driver deaths per 10 billion VMT (estimate = -5.126, SE = 1.549). They also report that the relative impact of upgrading on occupant fatalities (p.655) is a 4.7% decrease (implying a $RR = 0.953$), which suggests a base rate of $6.647/.047 = 141.43$ deaths per 10 billion VMT. We used this implied base rate and the 95% confidence limits of the absolute effect to estimate the 95% limits of the RR , which we calculated as $(141.43 - 10.59)/141.43 = 0.93$ and $(141.43 - 2.70)/141.43 = 0.98$.

We took these 3 sets of estimates and conducted a random effects meta-analysis [23], which treats each study as a separate estimate and combines them into a summary measure, assuming that the studies are sampled from a distribution of possible studies. We tested for heterogeneity across studies using Cochran’s Q statistic [24]. Below (eFigure2) we show forest plots for both the impact on MVC fatality rates and on the proportion of fatalities belted. The summary estimate for MVC fatality rates is 4.75 fewer deaths per 10 billion VMT, with a 95% confidence interval of (-6.78 to -2.72), and there is not enough evidence to reject the assumption of homogeneity in the effects $Q = 1.78$, ($df = 2$), $p = 0.411$. On the rate ratio scale the summary estimate is 0.95 (0.93, 0.97), again with little evidence against homogeneity across studies.

eFigure 2: Random effects meta-analysis (with 95% prediction interval) of prior studies of upgrades to primary enforcement on fatalities per billion VMT

(a) Rate Difference (RD) per billion VMT

(b) Rate Ratio (RR)

2.2 Effect on the proportion of fatalities wearing seat belts

With respect to the proportion of occupant fatalities wearing seat belts, the report by Nichols et al. [2] provides estimates for the proportion of individuals killed wearing seat belts. Among occupants killed (using FARS data) they report (in their Table 2 on p.10) an average of a 7 percentage point increase in belt use for the early upgrades (1993-1998) and an average of 10 percentage points for upgrades occurring during the more recent period (1999-2004). No estimates of uncertainty are provided. Calculations in the Appendix of Nichols et al. suggest that these estimates were based on simple pre- and 2-year post-intervention data, without control for underlying secular trends, so it seems likely that they may be overestimates of the treatment effect if the proportion of fatalities wearing seat belts is generally increasing over time. Hedlund et al. [25], however, use ARIMA models to conduct interrupted time series analyses on each of 6 upgrades that happened between 2000 and 2004 (Michigan, New Jersey, Washington, Delaware, Illinois, and Tennessee) using monthly crash data from FARS for 1994-2005. Although these estimates do not include the use of an untreated control group, they are likely less biased than simple pre/post comparisons. We extracted the point estimates and standard errors for each estimate of the effect of upgrading on the risk difference in the proportion of fatalities wearing seat belts among front-seat occupants.

As for MVC fatality rates, we conducted a random effects meta-analysis on the 6 estimates of the impact of upgrading on the proportion of fatal occupants wearing seat belts. eFigure 3 shows the forest plot and the summary estimate. The summary estimate implies that upgrading to primary enforcement increases the proportion of fatal occupants wearing seat belts by 13 percentage point, with a 95% CI of 10 to 16.

However, there is evidence of heterogeneity in the study estimates $Q = 21.18$, ($df = 5$), $p = 0.001$. In particular the effects appear weaker for later studies. Hedlund et al. note in their report that the limited duration of post-intervention follow-up time may be a reason for the weaker effects in Illinois and Tennessee.

eFigure 3: Random effects meta-analysis (with 95% prediction interval) of prior studies of upgrades to primary enforcement on the proportion of fatal occupants wearing seat belts.

eTable 1: Date of mandatory seat belt laws and motor vehicle crash death rates 2000-2016.

State	Secondary Law	Primary Law	Rate per 100,000 population	Rate per billion vehicle miles traveled
Alabama	July 18, 1992	December 9, 1999	16.7	12.6
Alaska	September 12, 1990	May 1, 2006	7.4	10.3
Arizona	January 1, 1991		9.9	10.3
Arkansas	July 15, 1991	June 30, 2009	15.5	13.6
California	January 1, 1986	January 1, 1993	6.1	6.8
Colorado	July 1, 1987		8.1	8.4
Connecticut		January 1, 1986	5.3	5.9
Delaware	January 1, 1992	June 30, 2003	9.4	8.9
District of Columbia	December 12, 1985	October 1, 1997	3	5
Florida	July 1, 1986	June 30, 2009	9.4	8.9
Georgia	September 1, 1988	July 1, 1996	11.6	9.8
Hawaii		February 16, 1985	4.7	6.3
Idaho	July 1, 1986		12.3	12
Illinois	January 1, 1988	July 3, 2003	6.3	7.6
Indiana	July 1, 1987	July 1, 1998	8.3	7
Iowa		July 1, 1986	9.9	9.5
Kansas	July 1, 1986	June 10, 2010	12	11.3
Kentucky	July 15, 1994	July 20, 2006	14.9	13.4
Louisiana	July 1, 1986	September 1, 1995	13.3	13.2
Maine	December 27, 1995	September 20, 2007	9.9	8.9
Maryland	July 1, 1986	October 1, 1997	6.5	6.6
Massachusetts	February 1, 1994		3.9	4.6
Michigan	July 1, 1985	April 1, 2000	7.5	7.5
Minnesota	August 1, 1986	June 9, 2009	6.9	6.4
Mississippi	July 1, 1994	May 27, 2006	21.8	16.1
Missouri	September 28, 1985		13.1	11.1
Montana	October 1, 1987		18.6	15.9
Nebraska	January 1, 1993		11	10.3
Nevada	July 1, 1987		8.1	9.4
New Hampshire				
New Jersey	March 1, 1985	May 1, 2000	4.6	5.5
New Mexico		January 1, 1986	14.1	11.2
New York		December 1, 1984	3.8	5.6
North Carolina		October 1, 1985	11.1	10
North Dakota	July 14, 1994		13.9	11
Ohio	May 6, 1986		7.5	7.8
Oklahoma	February 1, 1987	November 1, 1997	14.7	11.5
Oregon		December 7, 1990	7.9	8.6
Pennsylvania	November 23, 1987		7.8	9.5
Rhode Island	June 18, 1991	June 30, 2011	4.5	5.8
South Carolina	July 1, 1989	December 9, 2005	15.5	14.1
South Dakota	January 1, 1995		14.5	13.1
Tennessee	April 21, 1986	January 1, 2004	14	12.2
Texas		September 1, 1985	10.2	10.5
Utah	April 28, 1986	May 12, 2015	7.4	7.5
Vermont	January 1, 1994		8.7	7.2
Virginia	January 1, 1988		8.2	8
Washington	June 11, 1986	July 1, 2002	5.8	6.8
West Virginia	September 1, 1993	July 1, 2013	15.1	13.9
Wisconsin	December 1, 1987	June 30, 2009	8.9	8.4
Wyoming	June 8, 1989		21.6	12.7

3 Visualization of prior, likelihood, and posterior for motor vehicle crash deaths

eFigure 4: Prior, likelihood, and posterior distributions of the effect of upgrading to primary enforcement on MVC fatality rates.

4 Visualization of prior, likelihood, and posterior for proportion belted

eFigure 5: Prior, likelihood, and posterior distributions of the effect of upgrading to primary enforcement on the proportion of death wearing seat belts.

5 Sensitivity analyses for motor vehicle crash deaths

eTable 2: Sensitivity analysis using lagged exposure, alternative control groups, and temporal restriction for the effect of upgrading to primary enforcement of seat belt laws on motor vehicle crash death rates per billion VMT, US states, 2000-2016.

	Negative binomial regression	Lagged policy change by 1 year ^a	Secondary enforcement only as control group ^b	Primary enforcement only as control group ^c	Years prior to Great Recession ^d
Primary vs. secondary					
Rate difference	-0.12 [-0.60,0.35]	-0.18 [-0.66,0.30]	-0.37 [-0.90,0.16]	-0.58 [-1.11,-0.05]	-0.04 [-0.63,0.55]
Rate ratio	0.99 [0.94,1.04]	0.98 [0.93,1.03]	0.96 [0.91,1.02]	0.94 [0.89,0.99]	1.00 [0.95,1.05]
<i>N</i>	8501	8000	8501	5951	4000

Note: All models used VMT (billions) as offset and subjective log-RR prior as $N(\ln(0.90), 0.003)$. Except for negative binomial model all models were Poisson. Standard errors clustered by state. 95% posterior limits in brackets. Adjusted for age group, presence or absence of speed limit laws (≤ 65 , 70, > 70), graduated driver's license laws (night restrictions, passenger restrictions), maximum blood alcohol content laws (0.08, 0.10 g/dl), alcohol consumption per capita, police officers per capita, state median income, and state and year fixed effects. ^a Date of change from secondary to primary enforcement lagged by 1 year. ^b Excluded states that had already upgraded to primary enforcement by 2001. ^c Excluded states that maintained secondary primary enforcement through 2014. ^d Using 2000-2007 data only (Great Recession began in December of 2007).

6 Sensitivity analyses for proportion of deaths belted

eTable 3: Sensitivity analysis using lagged exposure, alternative control groups, and temporal restriction for the effect of upgrading to primary enforcement of seat belt laws on the proportion of crash deaths wearing seat belts, US states, 2000-2016.

	Lagged policy change by 1 year ^a	Secondary enforcement only as control group ^b	Primary enforcement only as control group ^c	Years prior to Great Recession ^d
Primary vs. secondary				
Rate difference	0.06 [0.04,0.07]	0.07 [0.05,0.08]	0.07 [0.05,0.09]	0.04 [0.02,0.06]
Rate ratio	1.16 [1.12,1.20]	1.18 [1.13,1.24]	1.18 [1.12,1.24]	1.12 [1.07,1.17]
<i>N</i>	8000	8501	5951	4000

Note: Grouped logistic regression. Standard errors clustered by state. 95% posterior limits in brackets. Adjusted for age group, presence or absence of speed limit laws (≤ 65 , 70, > 70), graduated driver's license laws (night restrictions, passenger restrictions), maximum blood alcohol content laws (0.08, 0.10 g/dl), alcohol consumption per capita, police officers per capita, state median income, and state and year fixed effects. ^a Date of change from secondary to primary enforcement lagged by 1 year. ^b Excluded states that had already upgraded to primary enforcement by 2001. ^c Excluded states that maintained secondary primary enforcement through 2014. ^d Using 2000-2007 data only (Great Recession began in December of 2007).

7 Comparison with fully-Bayesian analysis

In this paper we used a semi-Bayesian data augmentation approach. As we noted in the manuscript our chief reason for adopting a semi-Bayesian approach was our interest only in the policy parameter. We were not particularly interested in the parameters on, say, year terms, fixed state intercepts, or the covariates. The non-policy parameters are necessary parts of our identification strategy, but we did not see great value in specifying priors for these other parameters, particularly given a large sample size. The rationale for using data augmentation approach rather than a fully-Bayesian approach was that it would allow us to use cluster robust variance, which is standard in the program evaluation literature when treatments are assigned at the cluster (i.e., state) level [26].

We also thought a demonstration of the semi-Bayesian approach applied to a specific policy question could be useful for more applied researchers. This may be particularly useful from the perspective of conceptualizing prior evidence as data from an ‘informationally equivalent’ experiment with known results before our study. As Greenland and others have noted [27], although many researchers see the potential benefits of adopting a Bayesian approach, technical hurdles and the complexity of MCMC are likely barriers to adoption. In addition to our substantive interest in the question of stronger seat belt laws, we also thought it may be useful to demonstrate to readers how to quantitatively incorporate prior information without going “full Bayes”. Data augmentation provides a mechanism for doing so that still allows researchers to utilize existing software for running analysis, provided the priors are specified with care and appropriately rescaled.

However, much of the literature on data augmentation approaches was developed prior to computational improvements for Bayesian analysis, so below we also include a table showing results from fully Bayesian analyses (R code for models given below using the *Bayesian Regression Models using Stan* `brms` package) using default priors (a half Student-*t* prior with 3 *df*, which has been shown to lead to better convergence than the traditional half Cauchy prior [28]), as well as a comparison with our semi-Bayesian analysis using subjective priors. The code we used to fit the model appears below, as well as some graphical evidence from the last model that the chains are well-mixed. From the table below it does not appear that the “fully Bayesian” model would provide substantially difference inference regarding the impact of the policy, which is perhaps not surprising since we have a lot of data that dominates the posterior. We do note that that fully Bayesian model tends to generate slightly more precise estimates relative to the semi-Bayes models fit using maximum likelihood with cluster robust standard errors (clustered by state), which are designed to be conservative in the case of clustered data. In both cases we would still interpret this as evidence of considerable weaker impacts relative to prior evidence and what is provided in existing policy documents, so we think that the fully Bayesian results support our overall conclusions.

eTable 4: Comparison of semi-Bayesian data augmentation and fully Bayesian models

Model	Posterior estimates from Poisson		
	Mean	95% LL	95% UL
<i>Non-informative priors for all parameters</i>			
Semi-Bayes	-0.023	-0.073	0.027
Full Bayes*	-0.016	-0.029	-0.003
<i>N(−1, 0.05) for law, default priors for all others</i>			
Semi-Bayes	-0.037	-0.092	0.019
Full Bayes*	-0.034	-0.047	-0.021

R code for fully Bayesian models:

```
# Load required packages
library(haven)
```

```

library(rstanarm)
library(brms)
library(rstan)
library(bayesplot)

# STAN options
rstan_options (auto_write=TRUE)
options (mc.cores=parallel::detectCores ()) # Run on multiple cores

d <- read_dta ( "../data/derived/mvc-laws-prior-data.dta" )

# 1
# default priors

bglm <- brm ( deaths ~ factor ( year ) + factor ( stfipsn ) + factor ( agegp )
            + factor ( speedlimit ) + bac_10 + bac_08 + gdlpr + paid + rural
            + etohpc21 + pe1000i + medinc + plaw + offset ( log ( evmtb ) ) ,
            data = d , family = poisson )

print(bglm, digits=3)

#2
# our subjective prior for primary law coefficient

bglm2 <- brm ( deaths ~ factor ( year ) + factor ( stfipsn ) + factor ( agegp )
            + factor ( speedlimit ) + bac_10 + bac_08 + gdlpr + paid + rural
            + etohpc21 + pe1000i + medinc + plaw + offset ( log ( evmtb ) ) ,
            data = d , family = poisson ,
            prior = set_prior ( " normal ( -1 , 0.05 ) " , coef = " plaw " ) )

print(bglm2, digits=3)

posterior <- as.array(bglm)
mcmc_dens(posterior, pars = c("b_plaw"))
mcmc_trace(posterior, pars = c("b_plaw"))

```

Density and trace plots for the `plaw` parameter from the fully Bayesian analysis with subjective prior for the `plaw` coefficient (model `bglm2` above, 4 chains, each with `iter = 2000`; `warmup = 1000`; `thin = 1`; total post-warmup samples = 4000):

8 Impact of alcohol-involved and rural road deaths

The models below include different parameterizations of the annual proportion of alcohol-involved and rural road deaths. In our basic Poisson model for motor vehicle crash deaths including our coefficient of interest (primary law upgrade), state and year fixed effects, other time-varying state laws and covariates and log-billions of vehicle miles traveled as an offset, we ran 3 models: 1) without any adjustment for the annual state percentage of alcohol-involved and rural road deaths; 2) with both covariates measured the same year as the outcome; and 3) lagged 1 year. Stata code and results are given below.

Stata code for sensitivity analysis:

```
* load data
use "$cdir/data/derived/mvc-laws-prior-data.dta", clear

* rescale values of VMT
replace evmtb = evmtb / 10

* model for overall death rates (flat priors)
poisson deaths plaw i.year ib2.stabbn ib25.agegp i.speedlimit ///
    bac_10 bac_08 gdlnr gdlpr paid rural etohpc21 pe1000i medinc, exp(evmtb) ///
    vce(cl stfipsn) nolog
estimates store m0

* now run model without covariates "paid" and "rural"
poisson deaths plaw i.year ib2.stabbn ib25.agegp i.speedlimit ///
    bac_10 bac_08 gdlnr gdlpr etohpc21 pe1000i medinc, ///
    exp(evmtb) vce(cl stfipsn) nolog
estimates store m1

* now lag values by 1 year
* pct alcohol-involved deaths in prior year
sort stfipsn agegp year
bysort stfipsn agegp: gen paidL1=paid[_n-1]

* pct deaths on rural roads in prior year
sort stfipsn agegp year
bysort stfipsn agegp: gen ruralL1=rural[_n-1]

* now run model with lagged values
poisson deaths plaw i.year ib2.stabbn ib25.agegp i.speedlimit ///
    bac_10 bac_08 gdlnr gdlpr paidL1 ruralL1 etohpc21 pe1000i medinc, ///
    exp(evmtb) vce(cl stfipsn) nolog
estimates store m2
```

eTable 5: Different parameterizations of percentage of rural and alcohol-involved deaths as covariates

Model	Treatment of alcohol-involved and rural road deaths covariates		
	Included	Excluded	1 year lagged values
IRR for primary law	0.98	0.97	0.99
(95% CI for IRR)	(0.93, 1.03)	(0.93, 1.03)	(0.94, 1.04)
Observations	8500	8500	8000

9 Frequentist regression estimates for motor vehicle crash deaths

eTable 6: Frequentist difference-in-differences Poisson regression results for the effect of upgrading to primary enforcement of seat belt laws on motor vehicle crash death rates per billion VMT, US states, 2000-2016.

	Coefficients		Incidence Rate Ratios	
	Coefficient	95%CI	Coefficient	95%CI
Primary law upgrade	-0.025	[-0.075,0.025]	0.976	[0.928,1.025]
Per capita alcohol consumption	0.165	[0.001,0.330]	1.180	[1.001,1.391]
Police employees per capita	0.009	[-0.003,0.020]	1.009	[0.997,1.020]
Median income (10000s)	0.037	[-0.017,0.091]	1.038	[0.983,1.096]
Max speed limit				
Speed limit <70 mph	0.000	[0.000,0.000]	1.000	[1.000,1.000]
Speed limit 70 mph	0.067	[0.022,0.112]	1.069	[1.022,1.118]
Speedlimit >70 mph	0.187	[0.042,0.332]	1.206	[1.043,1.394]
BAC law				
BAC 0.10 law	0.011	[-0.080,0.102]	1.011	[0.923,1.108]
BAC 0.08 law	0.013	[-0.082,0.108]	1.013	[0.922,1.114]
Graduated license law				
GDL night restriction	0.021	[-0.042,0.083]	1.021	[0.959,1.087]
GDL passenger restriction	-0.016	[-0.067,0.034]	0.984	[0.935,1.035]
Year of death				
2000 (ref)	0.000	[0.000,0.000]	1.000	[1.000,1.000]
2001	-0.022	[-0.053,0.010]	0.978	[0.948,1.010]
2002	-0.014	[-0.058,0.029]	0.986	[0.944,1.030]
2003	-0.042	[-0.096,0.012]	0.959	[0.909,1.012]
2004	-0.082	[-0.143,-0.020]	0.922	[0.867,0.980]
2005	-0.102	[-0.162,-0.041]	0.903	[0.850,0.960]
2006	-0.147	[-0.214,-0.080]	0.863	[0.807,0.923]
2007	-0.212	[-0.281,-0.143]	0.809	[0.755,0.867]
2008	-0.319	[-0.384,-0.254]	0.727	[0.681,0.776]
2009	-0.388	[-0.455,-0.320]	0.679	[0.634,0.726]
2010	-0.437	[-0.508,-0.367]	0.646	[0.602,0.693]
2011	-0.468	[-0.547,-0.389]	0.626	[0.579,0.678]
2012	-0.452	[-0.543,-0.362]	0.636	[0.581,0.697]
2013	-0.493	[-0.582,-0.403]	0.611	[0.559,0.668]
2014	-0.508	[-0.611,-0.405]	0.602	[0.543,0.667]
2015	-0.479	[-0.558,-0.400]	0.620	[0.573,0.671]
2016	-0.472	[-0.541,-0.402]	0.624	[0.582,0.669]
Age group				
0-9y	-1.878	[-1.963,-1.793]	0.153	[0.140,0.166]
10-14y	-2.435	[-2.500,-2.369]	0.088	[0.082,0.094]
15-19y	-0.323	[-0.365,-0.281]	0.724	[0.694,0.755]
20-24y	-0.165	[-0.194,-0.136]	0.848	[0.824,0.873]
25-34y (ref)	0.000	[0.000,0.000]	1.000	[1.000,1.000]
35-44y	-0.237	[-0.261,-0.213]	0.789	[0.770,0.808]
45-54y	-0.325	[-0.359,-0.291]	0.723	[0.699,0.748]
55-64y	-0.591	[-0.635,-0.548]	0.554	[0.530,0.578]
65-74y	-0.845	[-0.916,-0.773]	0.430	[0.400,0.461]
75+y	-0.525	[-0.636,-0.414]	0.592	[0.530,0.661]
N	8500		8500	

Note: Poisson regression with estimated vehicle miles traveled (billions) as offset. Standard errors clustered by state. 95% confidence interval in brackets. State fixed effects not shown.

10 Frequentist regression estimates for proportion belted

eTable 7: Frequentist difference-in-differences grouped logistic regression results for the effect of upgrading to primary enforcement of seat belt laws on the proportion of crash deaths wearing seat belts, US states, 2000-2016.

	Coefficients		Incidence Rate Ratios	
	Coefficient	95%CI	Coefficient	95%CI
Primary law upgrade	0.277	[0.213,0.342]	1.320	[1.237,1.407]
Per capita alcohol consumption	-0.065	[-0.231,0.100]	0.937	[0.794,1.106]
Police employees per capita	0.005	[-0.009,0.019]	1.005	[0.991,1.020]
Median income (10000s)	-0.018	[-0.078,0.043]	0.983	[0.925,1.044]
Max speed limit				
Speed limit <70 mph	0.000	[0.000,0.000]	1.000	[1.000,1.000]
Speed limit 70 mph	0.113	[0.052,0.174]	1.119	[1.053,1.190]
Speedlimit >70 mph	0.247	[0.076,0.418]	1.280	[1.079,1.519]
BAC law				
BAC 0.10 law	-0.027	[-0.284,0.230]	0.973	[0.753,1.259]
BAC 0.08 law	-0.086	[-0.346,0.173]	0.917	[0.708,1.189]
Graduated license law				
GDL night restriction	-0.037	[-0.105,0.031]	0.964	[0.901,1.031]
GDL passenger restriction	-0.051	[-0.122,0.020]	0.950	[0.885,1.021]
Year of death				
2000 (ref)	0.000	[0.000,0.000]	1.000	[1.000,1.000]
2001	0.031	[-0.022,0.083]	1.031	[0.978,1.087]
2002	0.105	[0.061,0.150]	1.111	[1.062,1.162]
2003	0.183	[0.127,0.239]	1.201	[1.136,1.270]
2004	0.255	[0.204,0.305]	1.290	[1.226,1.357]
2005	0.253	[0.177,0.329]	1.287	[1.193,1.389]
2006	0.252	[0.160,0.345]	1.287	[1.174,1.411]
2007	0.283	[0.198,0.369]	1.328	[1.219,1.446]
2008	0.269	[0.167,0.370]	1.308	[1.182,1.448]
2009	0.289	[0.165,0.414]	1.336	[1.180,1.512]
2010	0.322	[0.197,0.447]	1.380	[1.218,1.563]
2011	0.311	[0.171,0.450]	1.364	[1.187,1.568]
2012	0.327	[0.173,0.481]	1.387	[1.189,1.618]
2013	0.376	[0.231,0.521]	1.457	[1.260,1.684]
2014	0.417	[0.269,0.565]	1.518	[1.309,1.760]
2015	0.418	[0.304,0.531]	1.518	[1.356,1.700]
2016	0.414	[0.283,0.545]	1.513	[1.328,1.725]
Age group				
0-9y	-0.515	[-0.720,-0.311]	0.597	[0.487,0.733]
10-14y	0.210	[0.098,0.321]	1.233	[1.103,1.379]
15-19y	0.174	[0.136,0.212]	1.190	[1.146,1.236]
20-24y	-0.002	[-0.034,0.029]	0.998	[0.967,1.030]
25-34y (ref)	0.000	[0.000,0.000]	1.000	[1.000,1.000]
35-44y	0.172	[0.153,0.192]	1.188	[1.165,1.211]
45-54y	0.460	[0.423,0.498]	1.585	[1.527,1.645]
55-64y	0.789	[0.737,0.841]	2.201	[2.091,2.318]
65-74y	1.114	[1.058,1.169]	3.046	[2.881,3.220]
75+y	1.398	[1.312,1.485]	4.049	[3.712,4.416]
N	450006		450006	

Note: Grouped logistic regression with total number of deaths as denominator. Standard errors clustered by state. 95% confidence interval in brackets. State fixed effects not shown.

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