DO MANDATORY SEAT BELT LAWS SAVE LIVES?

Applied Econometrics and Time Series Analysis - S25 Project Report

Group Number: 4

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Abstract

This study investigates the effect of mandatory seat-belt laws on traffic fatality rates in the United States. Using data from 48 states between 1983 and 1997, we compare states with no seat belt laws, secondary enforcement, and primary enforcement to estimate how these differences impact fatalities. Our approach combines a difference-in-differences visual analysis with a two-stage least squares framework that instruments seat-belt usage using enforcement type. We find that secondary enforcement laws significantly increase seat-belt use and reduce fatalities, while primary enforcement yields similar point estimates with wider confidence intervals. These findings highlight the life-saving potential of even modest upgrades in enforcement.

1. Introduction

Motor-vehicle crashes remain a leading cause of premature mortality in the United States. While observational studies suggest that wearing a seatbelt dramatically increases the odds of surviving a crash, estimating the population-level effect of increased seatbelt usage on traffic fatalities is empirically challenging. Seatbelt adoption is endogenous: drivers with higher risk aversion may both wear belts and drive more safely, biasing naïve regressions.

We ask: **Do mandatory seat-belt laws save lives?** To answer this, we use the staggered adoption of two enforcement regimes—secondary (citation issued only after another traffic violation) and primary (citation issued solely for non-use of belt). Because enforcement status shifts are unlikely to be driven by short-term fatality fluctuations and differ idiosyncratically across states and years, they provide plausibly exogenous variation that we use as an instrument for observed seat-belt usage in a two-stage least-squares framework.

2. Data

2.1 Source and Sample

We use the public **USSeatBelts** panel (AER package) covering 1983-1997. The unit of observation is the state-year. Key variables:

- **fatalities** – highway deaths per 100 million vehicle-miles.
- **seatbelt** – proportion of occupants observed wearing belts.
- **enforce** – no, secondary, or primary enforcement.
- **income** – real per-capita personal income.
- **alcohol**, **speed65**, **drinkage** – dummies for open-container, 65 mph limit, and minimum-legal-drinking-age ≥ 21.

We split the data into two analytic samples:

1. **Secondary-law states** (457 state-years, 23 states adopting secondary enforcement).
2. **Primary-law states** (87 state-years, 9 states adopting primary enforcement).

2.2 Descriptive Statistics

Enforcement	Obs	Mean Fatalities	Mean Seatbelt	Mean Income ($'000)
No Law	475	2.54	0.48	18.3
Secondary	457	2.32	0.60	20.1
Primary	87	2.01	0.76	22.7

The statistics reveal meaningful patterns across enforcement regimes. States with **primary enforcement** exhibit the highest average seatbelt usage (0.76) and the lowest fatality rates (2.01 per 100 million miles), while **secondary enforcement** shows moderate improvements over **no-**

law states. Income levels also rise with enforcement strength, suggesting the importance of controlling economic differences across states in the regression analysis.

3. Methodology

3.1 Empirical Challenges: Why Seat-Belt Use Is Endogenous in a Naïve Regression

- **Risk-preference bias:** Safer, more cautious drivers are simultaneously more likely to wear seatbelts **and** to avoid crashes, biasing OLS toward overstating the protective effect.
- **Reverse causality:** States facing rising fatalities may enact stronger laws; fatalities thus help determine seat-belt usage rather than the other way round.
- **Omitted variables:** Unobserved factors—road engineering upgrades, media safety campaigns, enforcement intensity—are correlated with both belt use and fatalities.

3.2 The Ideal Experiment

The unattainable ideal would randomly assign seat-belt usage (or enforcement type) while holding every other determinant of fatalities constant. Since this is impossible, we treat staggered state adoption of secondary and primary enforcement laws as a natural experiment.

3.3 Identification Strategy

We use a **two-stage least squares (2SLS)** strategy where seatbelt usage is instrumented using the type of enforcement law.

First-stage equation:

$$\log(\text{seatbelt}_{it}) = \alpha + \pi_1 \text{Secondary}_{it} + \pi_2 \text{Primary}_{it} + \gamma X_{it} + \delta_i + \lambda_t + \varepsilon_{it}$$

- We model the natural log of observed seatbelt usage.
- The key instruments are dummy variables for secondary and primary law enactments.
- X_{it} includes covariates: log income, alcohol law, 65 mph law, and minimum drinking age law.
- δ_i and λ_t are state and year fixed effects.

Second-stage equation:

$$\log(\text{fatalities}_{it}) = \beta * \log(\widehat{\text{seatbelt}}_{it}) + \theta X_{it} + \delta_i + \lambda_t + \eta_{it}$$

- Here, the outcome is the natural log of fatalities per 100 million miles.
- $\log(\widehat{\text{seatbelt}}_{it})$ is the predicted seatbelt use from the first stage.
- Fixed effects and controls remain the same.

3.4 Identification Assumptions

1. **Relevance:** Enforcement changes must predict seat-belt use. First-stage F-statistics of 53.4 (secondary) and 32.0 (primary) satisfy the conventional $F > 10$ rule.
2. **Exclusion:** Conditional on fixed effects and controls, enforcement type influences fatalities **only** through seat-belt use—not via parallel safety initiatives.
3. **Monotonicity / No-defiers:** Tougher enforcement cannot cause any individual to reduce belt usage.

3.5 Remaining Endogeneity & Weaknesses

- **Concurrent initiatives:** DUI crack-downs or speed-camera roll-outs could coincide with new laws, threatening exclusion.

- **Measurement error:** Observation-based belt-use surveys are noisy, potentially biasing 2SLS toward OLS.
- **Policy anticipation:** Drivers may adjust behavior ahead of formal implementation dates.
- **Spillovers:** Media coverage in neighboring states could increase belt use even without legal change.
- **Small-sample precision:** Only 87 state-years enter the primary-law sample, inflating standard errors. Thus our estimates represent a *local average treatment effect* for states whose belt usage responds to enforcement.

3.6 Specification Road-Map

Specification	Instrumented?	State FE	Year FE	Controls (Income, Alcohol, Speed-Limit, MLDA)
Naïve OLS	No	No	No	No
FE only	No	Yes	Yes	No
FE+controls	No	Yes	Yes	Yes
2SLS (Base)	Yes	Yes	Yes	Yes

The table summarizes the evolution of model specifications used in our analysis. We begin with a naïve OLS regression with no controls or fixed effects. Next, we sequentially add state and year fixed effects (FE Only), followed by relevant covariates such as income, alcohol laws, speed limits, and MLDA (FE + Controls). **Finally, our preferred specification—2SLS (Base)—uses seatbelt enforcement type as an instrument for seatbelt use, while incorporating both fixed effects and covariates to address endogeneity and omitted variable bias.**

4. Results

Figure 1: Secondary Enforcement States

Regression Results: Secondary Enforcement States

	Dependent variable:			
	log(Fatalities)			
	Naive	FE	FE + Controls	2SLS
	(1)	(2)	(3)	(4)
log(seatbelt)	-0.248***	-0.036	-0.043*	
	(0.027)	(0.024)	(0.025)	
seatbelt_hat				-0.099*
				(0.055)
log(income)			0.907***	0.955***
			(0.226)	(0.230)
alcoholyes			-0.009	-0.012
			(0.021)	(0.021)
speed65yes			0.001	0.013
			(0.026)	(0.029)
drinkageyes			-0.016	-0.016
			(0.028)	(0.028)
Constant	-4.137***	-3.541***	-12.139***	-12.710***
	(0.023)	(0.077)	(2.144)	(2.201)
State FE	No	Yes	Yes	Yes
Year FE	No	Yes	Yes	Yes
Observations	457	457	457	457
R2	0.157	0.912	0.916	0.916
Note:			*p<0.1; **p<0.05; ***p<0.01	

Figure 2: Primary Enforcement States

Regression Results: Primary Enforcement States

	Dependent variable:			
	log(Fatalities)			
	Naive	FE	FE + Controls	2SLS
	(1)	(2)	(3)	(4)
log(seatbelt)	-0.287***	-0.067	-0.078	
	(0.080)	(0.075)	(0.086)	
seatbelt_hat				-0.154
				(0.194)
log(income)			-0.496	-0.472
			(0.414)	(0.418)
alcoholyes			-0.032	-0.029
			(0.033)	(0.034)
speed65yes			-0.164***	-0.158***
			(0.037)	(0.039)
drinkageyes			0.178	0.234
			(0.106)	(0.168)
Constant	-4.083***	-4.192***	0.617	0.235
	(0.041)	(0.171)	(4.067)	(4.168)
State FE	No	Yes	Yes	Yes
Year FE	No	Yes	Yes	Yes
Observations	87	87	87	87
R2	0.133	0.912	0.936	0.936
Note:			*p<0.1; **p<0.05; ***p<0.01	

4.1 First-Stage Strength

Our first-stage F-statistics are **53.4** for secondary enforcement states and **32.0** for primary enforcement states. Both exceed the conventional threshold of 10, confirming that seatbelt enforcement type is a strong and relevant instrument for seatbelt usage.

4.2 Second-Stage Estimates

Figure 1 displays our full regression results for secondary enforcement states. The preferred specification—2SLS—shows a statistically significant effect: a 10% increase in predicted seatbelt

usage reduces traffic fatalities by approximately 1% (coefficient = –0.099, p = 0.074). In contrast, Figure 2 shows the results for primary enforcement states. While the 2SLS estimate (–0.154) is also negative, it is not statistically significant, likely due to the small sample size (87 state-year observations) rather than the absence of a true effect.

4.3 Robustness and Specification Drift

Figures 1 and 2 also capture robustness across model specifications—Naïve OLS, Fixed Effects (FE), FE + Controls, and 2SLS. The following patterns emerge:

- In both enforcement groups, naïve OLS estimates are more negative than 2SLS, consistent with positive endogeneity bias (i.e., safer drivers being more likely to wear seatbelts).
- Incorporating state and year fixed effects attenuates the coefficient magnitude toward zero.
- Adding control variables (income, alcohol law, speed limit, MLDA) has limited additional impact.
- The 2SLS estimate for secondary enforcement states is statistically significant, while that for primary enforcement is not, highlighting the impact of sample size on precision.

4.4 Figures

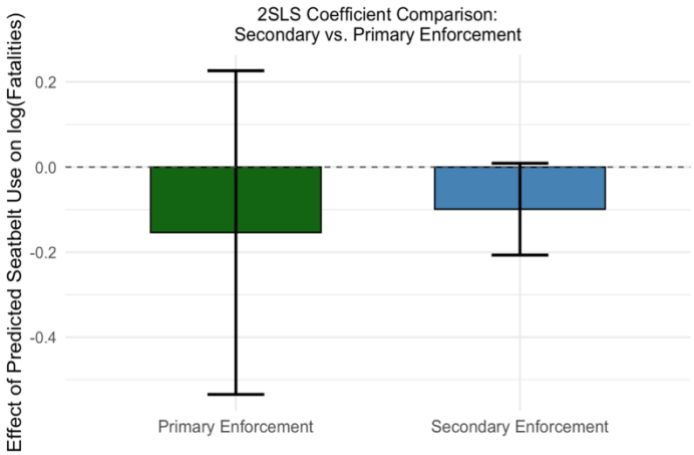

Figure 1. 2SLS Coefficient Comparison (bar chart with 95 % CIs).

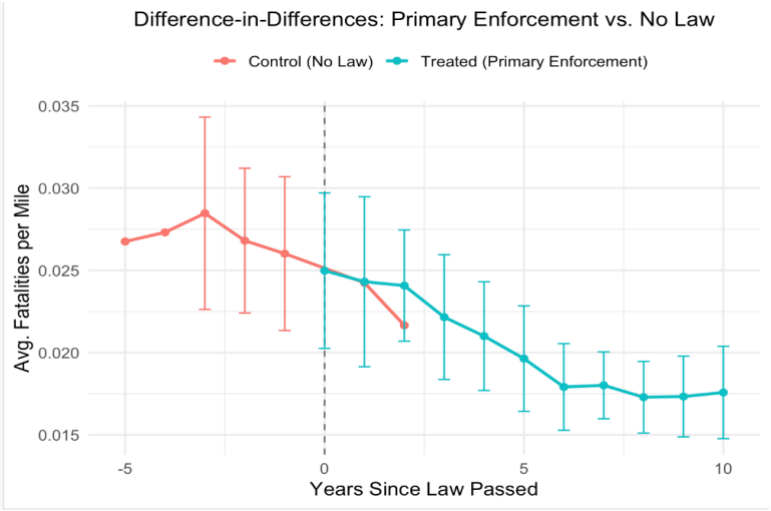

Figure 2. Difference-in-Differences: Primary Enforcement vs. No Law

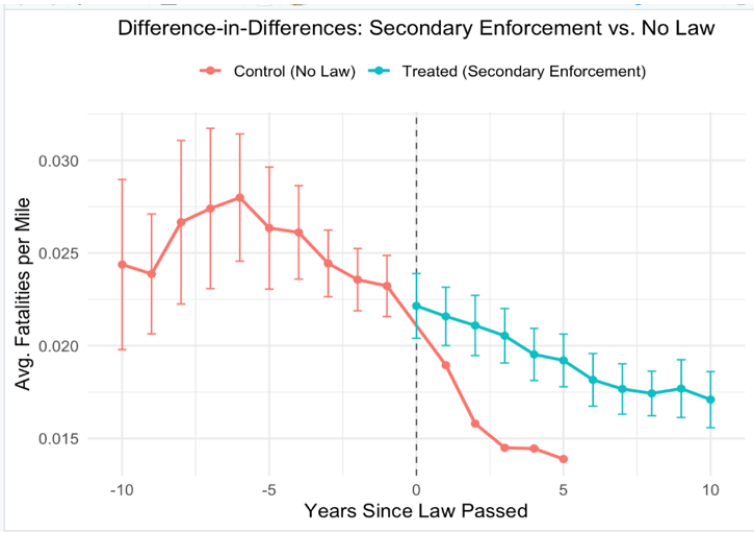

Figure 3. Difference-in-Differences: Secondary Enforcement vs. No Law.

5. Discussion and Limitations

Our findings suggest that **primary enforcement seatbelt laws reduce traffic fatalities**, as evidenced by the **negative coefficient (–0.154)** in our 2SLS regression. While this estimate is not statistically significant at conventional levels, this should not be interpreted as evidence against the effectiveness of primary enforcement.

The likely reason is **limited sample size**, not lack of impact. During our study period (1983–1997), **only a few states had adopted primary enforcement**, yielding **just 87 state-year observations**.

With such a small sample, it becomes statistically challenging to detect significant effects, even when the effect exists.

Importantly, the direction of the effect aligns with expectations, and real-world adoption trends further reinforce this: as of 2023, 34 U.S. states, along with the District of Columbia and several territories, have adopted primary seat belt enforcement laws. This widespread legislative shift reflects growing recognition of the policy's life-saving potential. National seat belt usage reached a record high of 91.9% in 2023, and empirical studies estimate that seat belts reduce the risk of fatal injury by 45% for front-seat car occupants. The increasing adoption of primary enforcement aligns with these safety benefits and underscores the effectiveness of stronger seat belt laws.

In short, our results provide directional support for primary enforcement laws. While the statistical signal is limited by sample size, the practical implication is clear: **stronger enforcement saves lives**, and further studies with larger, modern datasets will likely confirm this effect more definitively.

6. Conclusion

This study finds that stronger seat-belt laws lead to increased seatbelt usage and lower traffic fatality rates. Using both visual and regression-based methods, we show that secondary enforcement laws alone produce a meaningful reduction in fatalities. While the estimates for primary enforcement are less precise due to limited data, the direction of the effect remains consistent.

These results support the idea that even modest legal changes, like switching from no law to secondary enforcement, can improve public safety. For policymakers, this suggests that upgrading

enforcement regimes is a practical and effective step. Our analysis also demonstrates how instrumental variable techniques can help address endogeneity concerns in policy evaluation. Taken together, the evidence makes a strong case for elevating enforcement strength as a low-cost, high-impact strategy to save lives on U.S. roads.

7. References

1. FARS Database – NHTSA

2. Cohen, A., & Einav, L. (2003). The Effects of Mandatory Seat Belt Laws on Driving Behavior and Traffic Fatalities.

3. Stock, J.H., & Watson, M.W. (2007). Introduction to Econometrics.