Effects of Converted Primary Enforcement Seat Belt Laws on Traffic Fatalities

By Christopher L. McCall

Christopher L. McCall completed his Master of Public Policy at the Georgetown Public Policy Institute in 2012. Omar Robles, PhD, served as his thesis advisor. McCall also holds a BA in Political Science from the University of Minnesota. He recently began work as a Legislative Assistant in the Minnesota State Senate, working for Sen. Ann Rest, the Chair of the Tax Reform Division.

ABSTRACT

As states continue to strive for safer roadways, seat belt laws remain a major policy issue in state legislatures across the country. The effectiveness of seat belt usage in saving lives during motor vehicle accidents is well documented, but enforcement methods vary among states. Some states and other actors continue to resist tougher seat belt laws on the grounds that they are either ineffective or violate personal freedoms. As of 2011, 24 states and the District of Colombia have upgraded from secondary to primary enforcement of existing seat belt laws. This paper analyzes the impact of tougher enforcement laws on reducing roadway fatality rates. This research builds upon previous works as it considers an additional 10 states that have converted to primary enforcement since the last known study. Using state-level panel data collected from the National Highway Traffic Safety Administration and the United States Census Bureau, this paper employs a series of fixed-effect regression models to determine if there are significant benefits observed in states that adopt stricter seat belt enforcement laws. The results indicate that, in terms of both lives and money saved, states experience significant benefits after upgrading existing secondary laws to primary enforcement. Closer examination reveals that this impact is not homogenous across states and that adopting primary laws may have outsized benefits for highly-populous states and specific geographic regions such as the Southeast and Pacific Coast.

I. INTRODUCTION

Sheer numbers demonstrate the true devastation of motor vehicle fatalities in the United States. From 1994 to 2009, the National Highway Traffic Safety Administration recorded 661,403 fatalities linked to motor vehicle accidents (NHTSA 2011). This signifies an average of 41,338 Americans killed each year by preventable accidents. In light of this continuing threat to public safety, policymakers must identify and replicate the laws most effective in reducing deaths caused by car crashes.

Unlike some policy arenas, a majority of traffic safety laws fall squarely in the jurisdiction of state-level statutes. The resulting diversity among states means that some successful policies may be underutilized. While there are certain factors (i.e., weather, congestion, and infrastructure) that cause states to implement different laws in pursuit of effective highway safety policy, many high-risk behavioral patterns are exhibited by vehicle occupants throughout the nation, and thus can be countered with the same legislation. Of these behaviors, failure to wear a seat belt is among the most dangerous.

The argument for stricter seat belt legislation relies on the consensus view that buckling up saves lives. NHTSA estimates that seat belts provide the most protection of any single safety apparatus, reducing potential deaths in crashes by 45-60 percent (2002). In light of this evidence, New York became the first state to pass a law mandating seat belt usage in 1984. Since then, every state except New Hampshire has passed some form of mandate for adult occupants, and all states require the use of seat belts by minors.

> "The argument for stricter seat belt legislation relies on the consensus view that buckling up saves lives."

While adoption of adult seat belt laws has been almost universal, state statutes still vary in their design. Among the most pronounced differences is the degree of power given to state and local officers in enforcing the law. In some states, seat belt mandates are subject to primary enforcement, meaning that vehicle operators who do not buckle up can be stopped and cited solely for that offense. However, other states only make these laws subject to secondary enforcement, prohibiting officers from pulling over or fining a driver for not wearing a seat belt unless another infraction has also occurred. Secondary enforcement is a relatively rare legal practice that relies largely on the theory that compliance with a statute will occur simply because of its existence.

Advocates of primary enforcement contend that the mandate gives vehicle occupants added incentive to buckle up due to the heightened risk of being pulled over. This in turn makes them safer in the event of an accident, reducing bodily harm and preventing deaths. In the past, state legislatures were also enticed to adopt primary enforcement because doing so would unlock conditional highway funds from the federal government. As this program expired in 2009, advocates

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now focus on publicizing the fiscal savings to state budgets generated by preventing highway deaths.

Advocates of secondary laws tend to utilize two lines of argument. The first is that primary enforcement, along with the mere idea of mandating seat belt usage at all, is an infringement on personal liberty. This contention is value-driven and loses much of its merit if the driver is not alone in his or her vehicle. Statistical evidence demonstrates that unrestrained vehicle occupants pose an immediate threat to other passengers since their bodies may become dangerous projectiles in the event of a high-speed accident (Redelmeier 2004).

The second argument against upgrading to primary enforcement provides the null hypothesis for this paper: that primary laws will not create a meaningful difference in traffic fatalities for states already using secondary enforcement. If this contention can be rejected with quantitative analysis, it would indicate that adopting primary enforcement actually enhances public safety. Specifically, a statistically significant difference between the impacts of secondary and converted primary laws would imply that the latter is a better policy mechanism for reducing roadway fatalities.

II. MOTIVATION

Past studies rely on panel data that is no longer up to date, thus creating a need for renewed research on seat belt legislation. My research utilizes data from 1994 through 2009 while other reports only contain observations through 2002. In that time period an additional 10 states have converted from secondary to primary enforcement, doubling the number of jurisdictions that have taken such action and providing a more robust sample for measuring the impact of upgraded enforcement mechanisms. Further, by using 1994 as the first year of data, I am able to use secondary enforcement as a nationwide baseline as opposed to a now defunct model where states have no seat belt laws.

The continuing political debate surrounding this issue suggests that this research may have real policy implications. As of my writing, 17 states have yet to adopt primary enforcement, representing roughly a quarter of the nation's population. Research accounting for the additional states that have adopted primary statutes in the last decade will enhance decision making for those legislatures deciding whether to upgrade existing laws. Furthermore, updated research can benefit states that are currently debating whether to repeal or downgrade existing seat belt laws. While my research does not directly test the consequences of this kind of policy reversal, my findings could help determine whether such actions would be detrimental to public safety.

III. LITERATURE REVIEW

Seat belt laws receive a large amount of attention in transportation policy. However, the focus of research has shifted over time as the national debate has moved from the actual utility of seat belts to the effectiveness of different policies encouraging their usage. Research specifically measuring the impact of converted primary enforcement laws has only emerged in the last decade.

Cohen and Einav (2003) run regression models on the effectiveness of varying seat belt laws using panel data from all 50 states for the years 1983-1997. Using these laws as instruments for actual usage rates, they find a 13.5 percent increase in seat belt usage for states that convert from secondary to primary enforcement, but their study lacks a direct measurement of the impact that tougher laws have on fatality rates. Research by Farmer and Williams (2004) specifically addresses this question by analyzing a sample of states from 1989 to 2003. Their results suggest there is approximately a 10 percent reduction in fatality rates among the treatment group compared to the control group.

Houston and Richardson (2006) also focus their research on the benefits of upgrading existing secondary laws, using panel data from 1990 to 2002. They control for demographic data, fixed effects, and policy controls such as graduated driver's license programs and blood-alcohol content limits for drunken driving arrests. They find that for the 10 states (along with the District of Columbia) that upgraded to primary laws between 1990 and 2002, annual fatality rates decreased by an average of 4.7 percent for all vehicle occupants and 5.1 percent for drivers.

IV. DATA

All data used in my models regarding state-level traffic deaths can be found in the Fatality Analysis Reporting System (FARS), a database maintained by NHTSA. Specifically, FARS provides state-by-state data for the annual rates of fatalities per 100 million vehicle miles travelled (VMT). I utilize public data available online for the years 1994 through 2009.

The Insurance Institute for Highway Safety provides a timeline for the adoption of seat belt laws in every state, allowing me to create indicator variables for the panel data. I ultimately choose to drop state-year observations for which there is no seat belt law in place, eliminating the state of New Hampshire from the panel data as well as another six observations from states that did not pass some form of mandate prior to 1994.[1] This is done to avoid having a control group that is too small to provide a reliable baseline. Instead, state-year observations with secondary enforcement laws become the baseline

[1] Initial seat belt laws fail to predate 1994 in Kentucky, Maine, Mississippi, North Dakota, and South Dakota

to which those with converted primary laws are compared.

While there may be concerns about bias or incorrect measurement in the NHTSA data, any problems would be systemic rather than varying by state due to a uniform reporting method at the federal level. Regardless, FARS provides the best available data due to federal reporting mandates, hence its usage here. Using the VMT rate rather than raw fatality totals provides a built-in control mechanism to account for any change in deaths that may be unrelated to occupant behavior, such as variance in population or changes in the level of roadway usage.

The state-level demographic data used in my models are publicly available online from the US Census Bureau. Populations are estimated for each year using projections calculated by the Bureau. Additional descriptive statistics taken from this source include median household income and population proportions for race and gender.

V. METHODOLOGY

I utilize my panel data to establish a difference-in-differences model, creating two control groups: 1) states that have only used secondary enforcement, and 2) pre-treatment observations from states that have converted from secondary to primary laws. My model also applies state and time fixed effects to control for national trends and unique state characteristics.[2]

[2] An OLS version of each model is also used to help understand the impact of omitting state fixed effects from the regressions.

Examples of year-based fixed effects include improvements in vehicle safety features and national economic shocks, which could trigger systemic increases or decreases in fatality rates. State fixed effects related to roadway fatalities may include climate, quality of infrastructure, and differences in "driving cultures" that cause drivers to be more or less responsible behind the wheel due to varying behavioral norms. The initial model used to measure the impact of converting to primary enforcement on fatality rates (*FatalitiesVMT)* is:

$$FatalitiesVMT_{st} = \beta_0 + \beta_1 SecToPrim_{st} + \Sigma\beta_s FE_s + \Sigma\beta_t FE_t + \varepsilon$$

The indicator variable *SecToPrim* has been coded to "1" only when observations have a primary enforcement law on the books that was updated from a previously standing secondary enforcement statute, as opposed to states where the original seat belt law utilized primary enforcement.[3] It should be noted that states that convert to primary laws will activate both the standalone primary variable and *SecToPrim*.[4] The latter

[3] For states with effective starting dates for their primary law that are later than April 30, the appropriate binary variable(s) for the new law will be activated beginning in the following calendar year to ensure that the law's immediate impact is not underestimated.

[4] While a "primary" variable is created, the way I construct my dataset means that all observations that activate this variable are already controlled for in fixed-effects regressions. This occurs through one of two ways. First, states whose original seat belt law used primary enforcement will have a value of "1" for all of their observations, making the primary law a fixed effect which is already controlled for in my models. Second, states for which the variable is

Table 1. Descriptive Statistics for State Time Series Data: 1994-2009 (n=794)

Variable	Mean	Std. Dev.	Minimum	Maximum
VMT Fatality Rate	1.57	0.43	0.61	2.94
Total Fatalities	827.57	814.13	29	4333
Seat Belt Law: Original Primary Enforcement	16.12%	36.80%	0	1
Seat Belt Law: Secondary Enforcement	65.62%	47.53%	0	1
Seat Belt Law: Converted Primary Enforcement	18.26%	38.66%	0	1
Median Household Income (2010 dollars)	$50,820.58	$7,677.52	$34,280.75	$73,598.42
State Population	5,683,292	6,240,953	474,982	36,961,229
Black (%)	11.79	11.69	0.34	64.9
Hispanic (%)	8.05	8.95	0.52	44.9
Male (%)	49.12	0.87	46.7	52.70

variable's coefficient (β_1) is of the most interest to my research because it examines whether states experience a statistically significant decline in the rate of fatalities per 100 million VMT after upgrading from secondary seat belt enforcement laws.

I then add demographic controls to this model to evaluate the robustness of my findings. In full, my final model will take on the following form:

$$FatalitiesVMT_{st} = \beta_0 + \beta_1 SecToPrim_{st} + \beta_2 Population_{st} + \beta_3 lnIncome_{st} + \beta_4 Black_{st} + \beta_5 Hispanic_{st} + \beta_6 Gender_{st} + \Sigma\beta_s FE_s + \Sigma\beta_t FE_t + \varepsilon$$

All of the variables added in this model are controls generated using demographic data from the US Census Bureau.[5] *Population* utilizes the Bureau's annual projections and accounts for differences in fatality rates that may be associated with having more or less total people on the roadways. I take the log of median household income to create *lnIncome,* which is included to account for the theory put forth by Christopher Ruhm that economic conditions in a state may influence the likelihood of motor vehicle accidents or crash-related fatalities (Cohen and Einav 2003).[6]

Values for *Black* and *Hispanic* are calculated by utilizing data compiled by the US Census Bureau to find the proportion of a state's population that identifies as each of those particular races in a given year. These variables are included due to observational data suggesting lower compliance rates for

activated upon upgrading a secondary law will have perfect correlation between that variable and *SecToPrim*.

5 I initially attempted to use state median age as an additional control but ultimately omitted it. The measurement is not sophisticated enough to produce significant results because it does not tease out the possible impacts caused by high concentrations of particularly young or old drivers.

6 Log is used to create more easily interpreted results, which display the impact of a percentage increase in income.

Table 2. Comparative Mean Statistics for State Time Series Data: 1994-2009

Variable	Secondary (n=521)	Primary (n=128)	Converted Primary (n=145)	National Mean (n=794)
VMT Fatality Rate	1.64	1.43	1.45	1.57
Total Fatalities	701.33	1342.45	826.64	827.57
Black (%)	9.92	7.33	23.42	11.79
Hispanic (%)	5.98	18.83	5.99	8.05
Male (%)	49.17	49.31	48.75	49.12
Median Household Income (2010 dollars)	$50,314.02	$52,714.30	$50,969.00	$50,820.58
State Population	4,424,147	10,918,542	5,586,069	5,683,292

seat belt usage among both of these groups, particularly young passengers of both races (American Academy of Pediatrics 2011). Finally, *Gender* is calculated as the male percentage of a state's population and included due to lower seat belt usage rates among men and a demonstrated higher risk of them being killed in motor vehicle accidents (Borenstein 2007).

VI. HYPOTHESIS

In all models that control for state fixed effects, I hypothesize that the coefficient for *SecToPrim* (β_1) will be statistically significant with a negative relationship. This result would indicate that upgrading existing secondary enforcement laws to primary enforcement is associated with reductions in occupant fatality rates. It is difficult, however, to predict whether the magnitude of these correlations will be similar to past research.

VII. DESCRIPTIVE STATISTICS

Table 1 provides descriptive statistics for the data relevant to my research. For this dataset, I have dropped all observations for which there is no adult seat belt law, removing 22 observations from my model.

The staggeringly high standard deviations for fatalities and population highlight the importance of using the VMT fatality rate as my dependent variable rather than raw totals. There are also notably large standard deviations for the two race variables. The remaining demographic variables are far less volatile across state lines.

Table 2 displays the means for selected variables when the data is divided into subgroups based on enforcement law.[7]

This table demonstrates how relying upon total fatalities would give

[7] California was the first state to convert to a primary law and the only one to do so before 1994. I have coded the state's observations as if there was always a primary law because my model cannot capture a before-and-after effect.

misleading results. The nation's three largest states by population (California, Texas, and New York) all adopted primary legislation before the time frame of this study, meaning their larger fatality counts would be wrongfully linked to primary enforcement if the data were not standardized using VMT fatality rates. These three states also skew the average proportion of Hispanics living in jurisdictions with primary laws.

The stratified data for VMT fatality rates, found in Table 2, tell a story much closer to my hypothesis. Secondary states have the highest average rate of fatalities, while those with converted primary laws produce figures similar to states whose seat belt legislation originated with primary enforcement. However, regression analysis is still necessary to establish causation.

On average, African Americans represent a much higher proportion of residents in states with converted primary laws. This finding may not be coincidental. Since African Americans have lower observed compliance rates with seat belt laws than the national average, primary laws are often advertised to policymakers as a

Table 3. The Impact of Converted Seat Belt Enforcement Laws on Traffic Fatalities

Dependent Variable:	Fatalities per 100 Million VMT (National Average = 1.571)			
Independent Variable	Model 1 OLS	Model 2 State FE	Model 3 OLS	Model 4 State FE
Secondary to Primary Enforcement	-0.037	-0.049	-0.111***	-0.049*
	(0.039)	(0.030)	(0.027)	(0.025)
Log (population)	--	--	-0.028***	-0.744**
			(0.010)	(0.354)
Log (median income)	--	--	-2.000***	0.266*
			(0.064)	(0.152)
% Blacks	--	--	0.013***	0.034***
			(0.001)	(0.010)
% Hispanics	--	--	-0.001	-0.002
			(0.001)	(0.014)
% Male	--	--	0.250***	-0.107**
			(0.015)	(0.051)
Year FE	Yes	Yes	Yes	Yes
State FE	No	Yes	No	Yes
n	744	794	744	794
Adj. R-squared	0.117	0.596	0.673	0.628

Note: For exact definitions of the variables, refer to the methodology section.
Observations clustered by state.
Significance of the coefficient estimate at the 0.01 level ***, at the 0.05 level **, and at the 0.10 level *.

solution to improving usage among this demographic (Ellis et al. 2000). Despite concerns raised about the potential for increased racial profiling by law enforcement officers, research from states with upgraded laws suggests that primary enforcement does not lead to a disproportionate number of citations being issued to black or Hispanic drivers (NHTSA 2006).

VIII. REGRESSION RESULTS

Table 3 presents results for my proposed regressions of traffic fatality rates on enforcement laws and demographic controls. Models 1 and 3 use Ordinary Least Squares (OLS), which do not control for state fixed effects. Both models find negative relationships between fatality rates and upgrading to primary laws, but the coefficient for the *SecToPrim* indicator variable is only statistically significant in Model 3. The coefficient is also greater in magnitude in Model 3 than in Model 1. The only difference between these models is the inclusion of basic demographic controls in Model 3, suggesting that their omission makes it more difficult to identify the true impact of primary enforcement laws and creates an overall positive bias on the coefficient of interest when using OLS.

The OLS coefficient in Model 3 suggests that converting to primary enforcement of seat belt laws is correlated with a 0.111 decrease from the average fatality rate in states with secondary enforcement. The implied result is a 6.99 percent decrease in total motor vehicle deaths, which matches my expectations of a negative correlation. The five demographic controls also increase the overall explanatory power of the model (adjusted R-squared) by a large degree.

In Models 2 and 4, state fixed effects are taken into account such as climate and infrastructure. The *SecToPrim* coefficient (-0.049) does not change after adding an array of demographic controls in Model 4, suggesting that this result is robust. However, while the coefficient is not significant in Model 2, it is marginally significant at the 5.8 percent level after controlling for demographic variables. The different coefficient values for *SecToPrim* between Models 3 and 4 suggest that omitting state fixed effects creates a negative bias on the coefficient. If we choose to accept the results in Model 4, then the decision to control for state and time fixed effects allows us to conclude that converting to a primary enforcement law results in a 0.049 decrease from the average VMT fatality rate in secondary states, a 3.09 percent reduction.

Models 3 and 4 both control for the same demographic factors and produce similar findings with regard to statistical significance. Contrary to expectations from past research, neither regression shows a significant relationship between Hispanics and statewide traffic fatalities. Compared to Cohen and Einav's model, my results suggest a more robust relationship between the proportion of African Americans and traffic fatalities. One possible explanation for this finding comes from NHTSA observational data that demonstrates that adult black vehicle occupants have the lowest usage rate of seat belts. This problem persists even when children (age 12 and younger) are in the vehicle, meaning young passengers will likely emulate such risky behavior and face an increased probability of death in an accident (Glassbrenner 2008).

Table 4. Select Results of Using Dynamic Estimates

Independent Variable	Model 5 OLS	Model 6 State FE
-3 years from passage	-0.045	-0.026
	(0.061)	(0.038)
-2 years from passage	-0.105*	-0.069*
	(0.059)	(0.040)
-1 year from passage	-0.052	-0.013
	(0.058)	(0.038)
0 (year enacted)	-0.084	-0.046
	(0.058)	(0.038)
+1 year from passage	-0.105*	-0.081*
	(0.058)	(0.046)
+2 years from passage	-0.149**	-0.090*
	(0.058)	(0.045)
+3 years from passage	-0.157**	-0.076*
	(0.062)	(0.042)
+4 or more years	-0.157***	-0.054
	(0.033)	(0.045)
n	744	794
Adj. R-squared	0.677	0.630

Note: Control variables from Models 3 and 4 also included. Significance of the coefficient estimate at the 0.01 level ***, at the 0.05 level **, and at the 0.10 level *.

The male coefficient changes signs after applying fixed effects, though it is significant in both Models 3 and 4. Studies have indicated that men are more prone to severe crashes than women, but in crashes of equal severity women are more likely to be injured or killed (IIHS 2006). Thus, the true effect of this variable is hard to estimate and an omitted variable bias likely exists in the coefficient. Regardless, the impact of gender rates has been isolated from the true effect of seat belt laws in my model.

The variable for logged median household income is positive and statistically significant after accounting for state fixed effects. This also occurs in Cohen and Einav's regression analysis, and they partially attribute the finding to the theory that better economic conditions lead to increased car usage, making drivers more vulnerable to accidents and casualties (2003).

Finally, the coefficient for logged population indicates a negative relationship between population size and traffic fatalities, a finding that increases in significance after controlling for state fixed effects. This finding may defy expectations, as having more drivers on the road would increase traffic, creating more opportunities for motor vehicle accidents. However, the negative correlation could have several legitimate explanations. For instance,

Table 5. Reduced-Form Results for Era of Passage		
IV: Era Law was Upgraded	OLS Coefficient	State FE Coefficient
Early (Pre-2002)	-0.417*	-0.043
	(0.245)	(0.031)
Late (Post-2002)	-0.333	0.002
	(0.247)	(0.051)
T-test Early=Late	0.069	0.428
n	744	794
Adj. R-squared	0.675	0.628

Note: All variables from Models 3 and 4 also included.
Significance of the coefficient estimate at the 0.01 level ***, at the 0.05 level **, and at the 0.10 level *.

states with lower populations are likely more rural, requiring residents to spend more time driving on highways and operating their vehicles at faster speeds. This leads to a higher risk of being involved in fatal accidents as opposed to fender benders, which are more common on local roads with lower speed limits. Additionally, a higher percentage of residents in populous, urbanized states are likely using alternative modes of transportation such as public transit, bicycles, or walking.

IX. SUPPORTING EVIDENCE

The following tests intend to address potential challenges to my initial findings and search for differing trends in the impact of converted primary laws based on divergent state characteristics.

Dynamic Effects

To test the exogeneity of upgraded seat belt laws, I apply dynamic estimates to Models 3 and 4 to reveal if my main coefficient captures preexistent trends in fatality rates rather than acting as an independent effect. Unlike Models 1 through 4, the *SecToPrim* variable is replaced with indicator variables that denote the year that state observations occurred relative to when a converted primary law was passed.

Table 4 presents the results of the dynamic estimates test. When applying state fixed effects in Model 6, I find small, negative coefficients prior to states converting to primary enforcement. However, fatality rates fall at a larger and more consistent pace in the years immediately after those primary laws are enacted. Only the +4 year variable fails to show a significant reduction in fatality rates, indicating a possible diminishment in the effect of the law over time. This may be evidence that the ability of seat belt laws to change behavior is not consistent as the overall compliance rate rises. The riskiest drivers will be tougher to sway into abiding by the law than those who immediately start buckling up following the adoption of primary enforcement (Dee 1998).

Table 6. ***SecToPrim*** Results Stratified by State Size (Population)							
State Size in Population	Avg.VMT Fatal	Total States	Applicable States	N	Observations w/*SecToPrim*=1	*SecToPrim* OLS	*SecToPrim* State FE
Small	1.693	21	5	329	26	-0.051	-0.046
(under 3 million)						(0.076)	(0.078)
Medium	1.516	19	10	330	87	-0.130***	-0.052
(3-9 million)						(0.031)	(0.032)
Large	1.409	10	4	135	32	-0.131***	-0.079**
(over 9 million)						(0.035)	(0.030)

Note: Control variables from Models 3 and 4 also included.

Significance of the coefficient estimate at the 0.01 level ***, at the 0.05 level **, and at the 0.10 level *.

Overall, these findings suggest the possible existence of a small systemic decline in fatalities per VMT regardless of state legislation. However, following the adoption of converted primary laws, the rate of this decline noticeably increases in magnitude and significance, suggesting desirable behavioral changes in the wake of stricter regulations.

Adopting Statutes in Differing Eras

I also stratify states based on when laws were adopted to test for endogeneity between the impact of converted primary laws and the era in which those statutes were adopted. This test was conducted to detect any outsized effect for early adopting states, which may have passed stricter laws sooner because they had a larger problem with compliance or traffic fatalities. However, Table 5 shows that when controlling for fixed effects, no significant difference is detectable.

Differing Effects by Population Size

While my base model shows an inverse relationship between state population and fatality rates, it fails to capture any differing impact in upgrading seat belt laws based on state size. I

Table 7. ***SecToPrim*** Results Stratified by Census Region						
Region	Total States	Applicable States	N	Observations w/ *SecToPrim*=1	*SecToPrim* OLS	*SecToPrim* State FE
Northeast	8	2	126	11	0.105**	0.015
					(0.049)	(0.03)
Midwest	12	3	190	27	-0.054	-0.078
					(0.052)	(0.084)
South	17	12	270	97	-0.133***	-0.078**
					(0.035)	(0.028)
West	13	2	208	10	-0.087	-0.022
					(0.077)	(0.066)

Note: Control variables from Models 3 and 4 also included.

Significance of the coefficient estimate at the 0.01 level ***, at the 0.05 level **, and at the 0.10 level *.

stratify the states into three groups based on whether they make up less than 1 percent of the national population, more than 3 percent, or some proportion in between. The base regression models are then run separately for each group. Table 6 demonstrates the results.

After controlling for state fixed effects, small and medium states each produce coefficients similar to the aggregate figure of -0.049 from Table 3, though neither is statistically significant. However, the coefficient in large states is highly significant and exceeds the magnitude captured in Table 3, indicating a reduction of -0.079 from the average fatality rate in secondary enforcement states. This amounts to a 5.28 percent reduction in traffic deaths among the nation's most populous states that adopt primary enforcement. These results suggest that upgraded seat belt laws are more effective in highly populated states than they are in smaller ones, meaning larger states may have more incentive to change their policies than less populous ones.

Table 8. Projected Average Annual Impact of Upgrading Seat Belt Laws for States Still Using Secondary Enforcement

(Simulating a 3.09% reduction in fatalities for year 2009)

State	Projected Reduction in Annual Fatalities	Economic Savings*
Arizona	23.62	$22,446,287.11
Colorado	16.54	$14,177,103.14
Idaho	6.25	$6,216,649.28
Massachusetts	10.67	$9,047,763.40
Missouri	28.74	$22,229,846.19
Montana	6.52	$5,386,655.78
Nebraska	7.25	$5,889,169.13
Nevada	7.12	$6,573,922.31
North Dakota	4.08	$3,771,666.51
Ohio[a]	57.84	$47,957,954.98
Pennsylvania[a]	65.42	$59,823,257.78
South Dakota	4.38	$3,265,537.06
Utah	7.29	$7,252,993.59
Vermont	2.13	$2,210,152.76
Virginia	20.79	$20,658,480.55
West Virginia	11.2	$9,723,691.11
Wyoming	4.19	$3,216,896.68
Total:	**284.03**	**$261,701,915.05**

[a] Calculated at a reduction rate of 5.28% due to differing measured impact of laws in states with large populations.

*Economic savings calculated using Center for Disease Control state-level data from 2005 on total costs of traffic deaths.

Source: http://www.cdc.gov/motorvehiclesafety/statecosts/

Regional Differences

While the base models in Table 3 control for state fixed effects, they do not detect possible regional differences resulting from varying driving cultures or conditions. I thus group states into Northeast, Midwest, South, and Western regions as defined by the US Census Bureau, and replicate my models separately for each area.

Table 7 shows that roughly two-thirds of state-year observations with converted primary laws occur in the South. As a result, the sample size of applicable observations is rather small in other regions and it is difficult to discern a noticeable effect for upgraded enforcement statutes. However, in Southern states where there are a number of converted primary observations, there is a significantly larger decrease in fatality rates than the national average demonstrated in Table 3.[8] This suggests an outsized effect

[8] I also stratify states using the Census Bureau Divisions, which compartmentalize each region into more homogenous subgroups. Doing so reveals statistically significant drop-offs in fatality rates for states with converted primary laws in the Southeast and along the Pacific coast.

“These results suggest a sizeable potential impact for states that choose to upgrade their seat belt laws. In addition to saving lives, states and families also stand to experience a large reduction in the economic costs suffered from fatal roadway crashes.”

for converted primary laws in this part of the country. It is hard to make sweeping conclusions about differing regional effects from this test. However, in light of the interesting results, I would encourage further research on this topic.

X. DISCUSSION

In Table 8, I indicate the potential implications of my results for states that, as of this writing, still utilize secondary enforcement seat belt laws.[9] Using the output from Model 4 in my primary results, as well as the findings from Table 6 on state size, I project the reduction in annual deaths for these states based on a fixed percentage reduction of their 2009 fatality counts. For most states I calculate reductions using an estimated decline of 3.09 percent in fatality rates. For the two large states of Ohio and Pennsylvania, I use my findings from Table 6 to apply an estimated 5.28 percent decrease in deaths per 100 million VMT.

Table 8 also displays my projected fiscal savings using CDC data from 2005 on the state-level financial burdens of highway deaths. The price faced by state governments when a traffic fatality occurs includes medical expenses, legal processes, and work-loss costs. Since these figures are slightly outdated, my estimated economic savings may actually be somewhat conservative due to inflation.

These results suggest a sizeable potential impact for states that choose to upgrade their seat belt laws. In addition to saving lives, states and families also stand to experience a large reduction in the economic costs suffered from fatal roadway crashes. The implications of primary enforcement are particularly powerful for populous states like Ohio and Pennsylvania. Even if the projections for these two states utilized the original coefficient from Model 4, they would still be projected to save a combined 74 lives and roughly $68.7 million annually.

XI. CONCLUSIONS

The results from this paper indicate that states that convert from secondary to primary enforcement of seat belt laws experience a significant decrease in traffic fatality rates and related economic costs. This effect is exogenous from subtle pre-existing trends. Furthermore, additional analysis shows that this drop in fatalities is more prominent in specific subgroups of states, including those with the largest populations and those in the South or the West Coast. States that adopt tougher seat belt

[9] States that have converted to primary enforcement between the end of my data period and the writing of this paper include Arkansas, Florida, Kansas, Minnesota, Rhode Island, and Wisconsin.

laws also stand to experience notable financial savings by foregoing the heavy economic costs associated with traffic deaths.

The implications of this research are important for state policymakers. While the sunset date has passed for receiving federal funds in exchange for adopting primary laws, my research provides evidence that such laws are still worthwhile because they significantly reduce traffic deaths and fiscal costs to state governments. If every state currently using secondary enforcement were to upgrade their statutes, my analysis suggests that it would save approximately 284 lives annually. These figures, even when given some room for error, are sizeable and worth consideration by all policymakers.

Further research on this topic is warranted. States that have only recently adopted stricter laws have had little time to measure the results of their new policies. By replicating my regression analysis in the future, more robust data can be used to verify my findings. I also recommend that future researchers with more time and resources attempt to add controls for several policies that work in conjunction with seat belt mandates to reduce fatalities. These laws include speed limits, drunk driving statutes, and distracted driving restrictions.

Even without controlling for these factors, it is clear from this research that converting seat belt laws from secondary to primary enforcement significantly reduces statewide traffic fatalities. Therefore, continued efforts to adopt stricter seat belt legislation in the remaining states with secondary enforcement are both admirable and worthwhile.

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