The Effect of Mandatory Seat Belt Laws on Seat Belt Use by Socioeconomic Position

Sam Harper
Erin C. Strumpf
Scott Burris
George Davey Smith
John Lynch

Abstract

We investigated the differential effect of mandatory seat belt laws on seat belt use among socioeconomic subgroups. We identified the differential effect of legislation across higher versus lower education individuals using a difference-in-differences model based on state variations in the timing of the passage of laws. We find strong effects of mandatory seat belt laws for all education groups, but the effect is stronger for those with fewer years of education. In addition, we find that the differential effect by education is larger for mandatory seat belt laws with primary rather than secondary enforcement. Our results imply that existing socioeconomic differences in seat belt use would be further mitigated if all states upgraded to primary enforcement.

INTRODUCTION

Mandatory seat belt laws, steadily implemented in the United States since the mid-1980s, increase seat belt use and reduce fatalities (Carpenter & Stehr, 2008; Cohen & Einav, 2003; Shults, Nichols, et al., 2004). Laws with *primary enforcement*, which allow unbelted drivers to be cited without being stopped for another offense, increase belt use and reduce fatalities to a greater extent than do laws with *secondary enforcement*, for which a driver cannot be stopped or ticketed for failing to use seat belts alone (Cohen & Einav, 2003; Dee, 1998; Farmer & Williams, 2005; Houston & Richardson, 2006a, 2006b).

As a consequence, U.S. rates of seat belt use have increased dramatically—from roughly 20 percent in the mid-1980s to 85 percent in 2012 (Dee, 1998; Escobedo et al., 1992; Nelson et al., 2002; Pickrell & Ye, 2012; Wagenaar, Maybee, & Sullivan, 1988). However, studies have consistently found that individuals of lower socioeconomic position, measured by either education or income, are less likely to wear seat belts (Beck et al., 2007; Blomquist, 1991; Braver, 2003; Chaudhary, Solomon, & Cosgrove, 2004; Fhaner & Hane, 1973; Goldbaum et al., 1986; Helsing & Comstock, 1977; Hersch, 1996; Hunter et al., 1990; Leigh, 1990; Nelson, Bolen, & Kresnow, 1998; Robertson, Oneill, & Wixom, 1972; Strine et al., 2010; Wells, Williams, & Farmer, 2002). For example, Beck et al. (2007) found that self-reported rates of "always" using seat belts were 15 percentage points lower among those with less than a high school compared to college graduates (82 vs. 67 percent) in states

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with secondary enforcement, and 3 percentage points lower in states with primary enforcement (88 vs. 85 percent). These differential rates of seat belt use likely make an important contribution to substantial socioeconomic differences in mortality from motor vehicle accidents (Cubbin, Leclere, & Smith, 2000a, 2000b; Steenland et al., 2003; Whitlock et al., 2003), differences that are notably absent across race-ethnic groups (Centers for Disease Control and Prevention, 2010). It's been hypothesized that the better-educated are more efficient at processing information about the risks and benefits of seat belts, or that they may have lower rates of time or risk preference (Blomquist, 1979, 1991; Hersch, 1996; Leigh, 1990; O'Conor, Blomquist, & Miller, 1996; Rogers, 2003). Others have suggested that lower seat belt use among low socioeconomic groups reflects a more general pattern of multiple risk behaviors, including risky driving (Wilson, 1990), and there is indeed evidence that individuals from lower socioeconomic strata are more likely to exhibit multiple risk behaviors (Berrigan et al., 2003; Hahn, Vesely, & Chang, 2000; Rogers, Hummer, & Nam, 2000). Still others have suggested that disadvantaged individuals may be less likely to invest in prevention, be more fatalistic and less likely to believe traffic accidents are preventable, or may use belts less often because their parents did so (Becker, 2007; Girasek, 2001; Shin, Hong, & Waldron, 1999).

These hypothesized mechanisms for socioeconomic inequalities in rates of seat belt usage suggest a potential for heterogeneous effects of seat belt laws on different socioeconomic groups, though in different directions. If the underlying mechanism leading to lower rates of usage among disadvantaged groups is that they are generally less risk averse—and there is evidence that disadvantaged groups exhibit behaviors associated with risky driving regardless of belt use (Braver, 2003; Foss, Beirness, & Sprattler, 1994)—then mandatory seat belt laws may lead to widening inequalities in seat belt use. This may occur because of selective recruitment—the process by which safer-than-average drivers are more likely to switch to using seat belts in response to a policy change (Evans, 1985; Hemenway, 1993). Prior work suggests that drivers at greater risk for accidents (e.g., men, intoxicated drivers) are less likely to respond to mandatory seat belt laws (Dee, 1998). If the reason low-educated drivers buckle up less frequently is a greater general propensity for risky driving, the existing evidence on selective recruitment suggests that mandatory laws would be less effective in this population. Alternatively, if individuals of lower socioeconomic position are more sensitive to the perceived costs (legal or financial) of violating mandatory seat belt laws, it is also possible for their rates of usage to increase more in response to legislation.

Despite the potential for heterogeneous effects by socioeconomic position, most research on mandatory seat belt laws has focused on their overall effectiveness in raising seat belt use and reducing mortality (e.g., Cohen & Einav, 2003; Dinh-Zarr et al., 2001; Houston & Richardson, 2006a). Estimating differential effects on mortality is largely impossible because between the mid-1980s and the mid-1990s, when the majority of laws were first passed, valid and consistent measures of education are unavailable on U.S. death certificates (Jemal et al., 2008). But given the well-established causal relationship between seat belt use and traffic accident mortality, estimating the causal impacts of these laws on seat belt use by socioeconomic position has important implications for inequalities in traffic accident mortality. However, the four previous studies that have looked at whether socioeconomic differences in belt use are affected by legislation were either simple pre–post designs (Kristiansen, 1985) or cross-sectional studies that cannot account for unobserved state characteristics or temporal trends (Beck et al., 2007; Strine et al., 2010; Wells, Williams, & Farmer, 2002).

In this paper, we evaluate the differential effect of mandatory seat belt laws on seat belt use among socioeconomic subgroups using data from the 1984 to 2008

U.S. Behavioral Risk Factor Surveillance System (BRFSS) surveys. We exploit the variation among states in the timing of the passage of these laws and interact this difference-in-differences (DD) estimate with measures of educational attainment to identify the effect of legislation on socioeconomic differences in seat belt use. We find consistent evidence that mandatory seat belt laws, especially with primary enforcement, increase population-wide seat belt use and reduce socioeconomic differences in belt use.

METHODS

Data

To estimate the effect of mandatory seat belt laws on socioeconomic inequalities in seat belt use, we used pooled data from the 1984 to 2008 waves of the BRFSS. Details of the design and methodology of the BRFSS have been published previously (Gentry et al., 1985; Marks et al., 1985; Remington et al., 1988). Briefly, the BRFSS is an ongoing random-digit dialed telephone survey conducted monthly within each state, the results of which are compiled by the Centers for Diseases Control and Prevention (CDC). A systematic review of methodological studies concluded that most self-reported health measures in the BRFSS are reliable and valid (Nelson et al., 2001), and there is generally agreement between aggregate BRFSS data and data from nationally representative surveys (Nelson et al., 2003).

Measures

We used years of completed education as our measure of socioeconomic position, largely because it has been repeatedly associated with seat belt use and is less susceptible to reverse causation since it is typically completed by early adulthood. We categorized individuals as having completed <12, 12, 13 to 15, or 16 or more years of education. We were also interested in differential effects by household income as a measure of socioeconomic position. However, inconsistencies in the coding of BRFSS income categories across surveys (especially upper-income categories) and an inability to adjust for changes in purchasing power over time precluded analysis of differential effects by income.

Questions about seat belt use were asked in all states in 1984 to 1993 (annually), 1995, 1997, 2002, 2006, and 2008. In 1994, 1996, and 1998, questions on seat belt use were not part of the core questionnaire and given only as a module to 9, 10, and 7 states, respectively. Self-reports of seat belt use are well known to overestimate actual use when compared with directly observed measurements (Centers for Disease Control and Prevention, 1988), though in more recent years estimates have converged (Ibrahimova, Shults, & Beck, 2011). However, state-level surveys with direct observation are of little utility here because they do not disaggregate belt use by individual socioeconomic characteristics. In addition, despite the fact that self-reports overestimate actual use, secular trends in self-reported and observed use are similar (Cohen & Einav, 2003) and the extent of overestimation is substantially reduced when self-reported use is defined as always using a seat belt (Nelson, 1996). Thus, we defined our main measure of seat belt use as whether the respondent reported that they always use their seat belt when driving or riding in a car, and we model the probability of individuals reporting they always use seat belts. However, if the impact of seat belt laws varies across levels of reported use, this definition may be unnecessarily restrictive. Therefore, in additional to always use we also estimated impacts on changes in "infrequent use" (seldom or never) and a pseudo-continuous measure of seat belt use.[1]

Estimates of the timing of seat belt legislation, type of enforcement (secondary or primary), and the maximum fine for the first offense were taken from Cohen and Einav (2003) and supplemented with updated data from the Insurance Institute for Highway Safety (Insurance Institute for Highway, 2012). Because the BRFSS contains the date of interview, we are able to assign exposure to seat belt laws based on the exact effective date of the law as described by the Insurance Institute for Highway Safety. Figure A1[2] shows the timing of the passage of seat belt laws in each state. Because other aspects of safety legislation also changed over this period and could be associated with changes in seat belt laws, we also obtained data on laws pertaining to maximum speed limits and legal limits for blood alcohol concentration (BAC),[3] overall motor vehicle accident mortality rates (Centers for Disease Control and Prevention & National Center for Health Statistics, 2010), and some limited data on enforcement of seat belt legislation (Tison & Williams, 2010).

Aggregating all surveys from 1984 to 2008 gave an overall sample size of 1,937,578. In order to limit the target population to the United States, we eliminated all observations from Puerto Rico, Guam, and the U.S. Virgin Islands (n = 25,170, 1.3 percent). We also excluded individuals less than 25 years of age that may not have yet completed their education (n = 151,556, 7.8 percent). Limiting the analysis to individuals with complete data on all covariates produced an analytic sample size of 1,371,731 individuals.

Table 1 shows characteristics of the BRFSS sample for selected years. The sample size increased from 8,877 in 1984 when only 15 states were participating to 313,996 in 2008 when all 50 states and the District of Columbia participated. Changes in the socioeconomic characteristics of the sample over time reflect U.S. secular trends, with increases in the fraction of the population with advanced education and declines in the fractions of the population that are white, not Hispanic, married, and current smokers. Seat belt use increased substantially, as the fraction of the population reporting always wearing a seat belt increased from 22 percent in 1984 to 86 percent in 2008. Figure 1 shows trends in rates of always wearing a seat belt among education subgroups. Rates of use increased from 14 to 86 percent among those with less than 12 years of education, and from 32 to 89 percent among those with 16 or more years of education. Thus, over the entire period higher educated groups have consistently reported the highest rates of always wearing seat belts, but there is some evidence of convergence in more recent years.[4]

Table 2 shows descriptive information on state-level policy variables, including seat belt legislation. Over the entire period, roughly 15 percent of the population had no seat belt law, 50 percent had a secondary enforcement law, 20 percent moved directly from no law to primary enforcement, and 15 percent upgraded from a secondary to a primary enforcement law.

[1] To create a continuous measure we follow Cohen and Einav (2003) and Carpenter and Stehr (2008) and assign values of 0, 0.1, 0.3, 0.75, and 1 to self-reports of never, rarely, seldom, usually, and always using seat belts.

[2] All appendices are available at the end of this article as it appears in JPAM online. Go to the publisher's Web site and use the search engine to locate the article at http://www3.interscience.wiley.com/cgi-bin/jhome/34787.

[3] We are grateful to Christopher Carpenter for supplying us with the data on maximum speed limits and BAC laws.

[4] The increase in rates appears stronger from 1984 to 1987, but this is partially an artifact of states with higher rates of seat belt use being added to the BRFSS sample. A simple decomposition (Das Gupta, 1978) showed that 25 percent of the decline from 1984 to 1987 was due to changing composition of the BRFSS sample rather than actual improvements in rates (results not shown).

Table 1. Individual characteristics for selected years, BRFSS, 1984 to 2008 ($n = 1{,}371{,}731$).

	1984		1995		2008	
	Weighted percent	Standard error	Weighted percent	Standard error	Weighted percent	Standard error
Age						
25 to 34 years	30.2	0.49	25.3	0.15	21.2	0.07
35 to 44 years	20.3	0.43	25.9	0.15	22.5	0.07
45 to 54 years	17.4	0.40	18.2	0.13	22.4	0.07
55 to 64 years	15.7	0.39	12.4	0.11	16.4	0.07
65+ years	16.3	0.39	18.2	0.13	17.5	0.07
Male	49.2	0.53	49.0	0.17	49.8	0.09
Household size (mean)	2.3	0.01	2.1	0.00	2.2	0.00
Race						
White	91.0	0.30	85.8	0.12	80.0	0.07
Black	6.9	0.27	9.3	0.10	10.5	0.05
Other	2.1	0.15	5.0	0.08	9.6	0.05
Hispanic	4.7	0.22	7.0	0.09	12.0	0.06
Marital status						
Married	73.6	0.47	68.0	0.16	67.7	0.08
Divorced/widowed/separated	17.3	0.40	19.9	0.14	18.5	0.07
Never married	9.1	0.31	12.1	0.11	13.8	0.06
Education						
<12 years	18.5	0.41	13.9	0.12	9.5	0.05
12 years	32.0	0.50	32.4	0.16	26.6	0.08
13 to 15 years	27.0	0.47	25.6	0.15	26.1	0.08
16+ years	22.6	0.44	28.1	0.16	37.8	0.09
Employment						
Employed	63.0	0.51	65.6	0.17	63.9	0.09
Not employed	18.8	0.41	16.3	0.13	19.4	0.07
Retired	18.1	0.41	18.0	0.13	16.7	0.07
Body mass index (BMI)						
Normal (<25 kg/m^2)	56.7	0.53	45.8	0.17	33.9	0.08
Overweight (25 to 30 kg/m^2)	33.1	0.50	37.3	0.17	37.8	0.09
Obese (<30 kg/m^2)	10.3	0.32	16.9	0.13	28.3	0.08
Smoking						
Never	44.7	0.53	49.9	0.17	54.8	0.09
Current	29.2	0.48	23.0	0.15	18.1	0.07
Former	26.2	0.47	27.1	0.15	27.1	0.08
Alcohol use						
Past-month alcohol use	56.5	0.53	48.7	0.17	53.7	0.09
Past-month binge drinker	15.7	0.39	12.1	0.11	14.6	0.06
Heavy alcohol use	11.0	0.33	3.6	0.06	5.1	0.04
Ever driven intoxicated	3.9	0.20	2.2	0.05	2.0	0.02
Seat belt usage						
Always	21.8	0.44	70.6	0.16	86.0	0.06
Nearly always	14.9	0.38	14.5	0.12	8.2	0.05
Sometimes	18.9	0.42	7.2	0.09	3.0	0.03
Seldom	18.1	0.41	3.6	0.06	1.3	0.02
Never	26.3	0.47	4.1	0.07	1.5	0.02
Observations	8,877		82,518		313,996	
Number of states in sample	15		50		51	

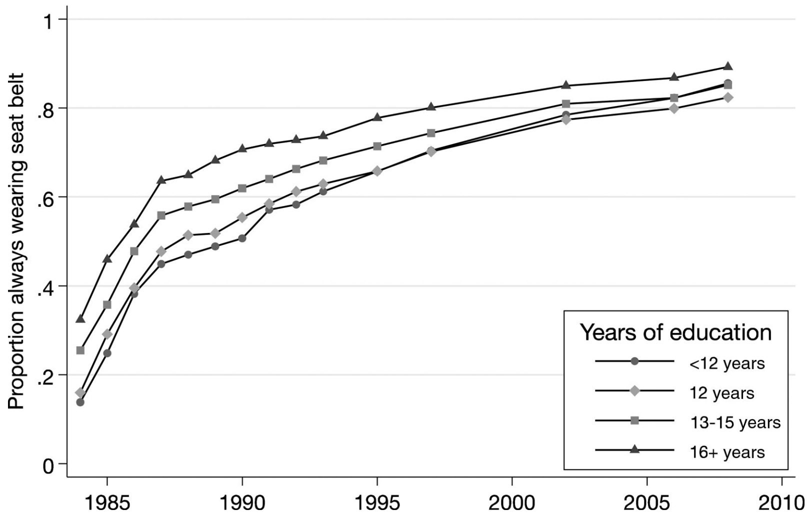

Figure 1. Trends in the Rates of Reporting Always Wearing a Seat Belt Among Education Groups, 1984 to 2008 BRFSS.

Empirical Strategy

To estimate the differential effect of seat belt laws on seat belt use, we used DD models (Angrist & Pischke, 2009; Meyer, 1995) and allowed the treatment effect to vary by education. We fit a series of linear probability models of the sort below:

$$Y_{ist} = \beta_0 + \beta_1 S_{ist} + \beta_2 L_{st} + \beta_3 S_{ist} L_{st} + \beta_4 X_{ist} + \sigma_s + \tau_t + \sigma_s S_{ist} + \tau_t S_{ist} + \varepsilon_{ist}$$

where Y_{ist} represents seat belt use for individual i in state s at time t, S indicates education group (indicator variables, 16+ years as the reference), L represents a series of dummy variables for the presence of a seat belt law and enforcement type (secondary or primary, no law as reference),[5] X is a series of individual-level covariates[6] (age, age squared, gender, marital status, race, ethnicity, employment, smoking, body mass index, past-month alcohol use, binge drinking and chronic drinking, and having ever driven while intoxicated), σ and τ are state- and year-fixed effects, and ε is an error term. Under this specification, β_3 represents our coefficients of interest, capturing whether legislation affects education groups differentially. β_1 estimates the effect of socioeconomic position on seat belt use in the absence of any seat belt law; β_2 estimates the effect of seat belt laws in the omitted category of education; β_4 captures the effects for a vector of covariates; σ_s and τ_t control for, respectively, time-invariant differences in seat belt use between states and overall

[5] Because during the period of observation some states upgraded from secondary to primary enforcement, we also estimate the effect of switching from secondary to primary enforcement relative to secondary enforcement only. Similar to Cohen and Einav (2003), we included three dummy variables for legislation: a secondary dummy equal to 1 in each period a state had either a secondary law or a primary law preceded by a secondary law; a primary dummy equal to 1 in each period a state had primary law not preceded by a secondary law; and a "switch" dummy equal to 1 in each period a state had a primary law preceded by a secondary law.

[6] We did not include income as a covariate since it is likely to be strongly determined by education (Card, 1999). Nevertheless, our results are generally unaffected by adjustment for individual-level covariates, including income.

Table 2. State-level policy variables, 1984 to 2008.

	Years covered	Weighted mean	Standard deviation	Minimum	Maximum
Seat belt law (percent of population covered)					
No law	1984 to 2008	13.9	34.6	0.0	100.0
Secondary enforcement	1984 to 2008	52.0	49.9	0.0	100.0
Primary enforcement	1984 to 2008	20.2	40.2	0.0	100.0
Upgraded from secondary to primary enforcement	1984 to 2008	13.9	34.6	0.0	100.0
Maximum speed limit (miles/hour)	1984 to 2008	64.3	5.5	55.0	75.0
Zero tolerance blood alcohol concentration law	1984 to 2008	55.1	49.1	0.0	100.0
0.10 Blood alcohol concentration law	1984 to 2008	59.6	48.7	0.0	100.0
0.08 Blood alcohol concentration law	1984 to 2008	34.2	47.1	0.0	100.0
Motor vehicle accident mortality rate (deaths/100,000 population)	1984 to 2008	17.2	4.8	7.2	35.1
Paid media for seat belt enforcement (nominal $/1,000 population)	1999 to 2006	0.8	1.1	0.0	10.6
Belt citations per 10,000 population	1999 to 2006	22.1	12.9	0.0	65.0

secular changes in belt use; $\tau_t S_{ist}$ captures any population-wide secular changes in belt use that differ by socioeconomic position; and $\sigma_s S_{ist}$ controls for any differences between states in the effect of socioeconomic position on belt use that do not change over time.

Under this model, the differential effect of legislation is identified by comparing within-state changes in seat belt use across socioeconomic groups after legislation, relative to changes in other states over the same period. An important assumption of this strategy is that the precise timing of legislation is exogenous (i.e., unrelated to seat belt usage or factors that may be associated with usage, such as fatalities). Cohen and Einav (2003) provide evidence that the timing of legislation is unrelated to prior levels of fatalities, and there is additional evidence that the timing of seat belt legislation is largely determined by political and administrative issues that are unlikely to be correlated with seat belt usage (Russell, Dreyfuss, & Cosgrove, 1999). We provide several sensitivity analyses of the above model below. We used the BRFSS sample weights and adjusted for state clustering in all models to allow

Table 3. Marginal effects of state seat belt laws on measures of seat belt use, by socioeconomic group, 1984 to 2008 BRFSS (n = 1,371,731).

	Always use		Infrequent use		Overall use[a]	
	Estimate[b]	95 percent CI[c]	Estimate[b]	95 percent CI[c]	Estimate[b]	95 percent CI[c]
Seat belt law and education						
No law → secondary						
<12 years	23.0	(17.0, 29.0)	−21.4	(−27.2, −15.7)	24.1	(18.2, 30.0)
12 years	20.7	(16.0, 25.4)	−16.1	(−20.7, −11.6)	19.9	(14.9, 24.9)
13 to 15 years	17.2	(13.1, 21.2)	−12.0	(−15.3, −8.8)	15.3	(11.6, 19.0)
16+ years	17.0	(12.0, 21.9)	−10.1	(−13.5, −6.7)	13.8	(9.6, 17.9)
No law → primary						
<12 years	44.4	(37.2, 51.6)	−30.4	(−37.2, −23.6)	38.7	(32.0, 45.5)
12 years	35.6	(29.5, 41.6)	−21.6	(−28.3, −14.9)	29.8	(22.7, 37.0)
13 to 15 years	28.5	(22.6, 34.5)	−14.5	(−19.0, −9.9)	20.9	(15.0, 26.7)
16+ years	19.7	(12.2, 27.2)	−10.5	(−17.7, −3.4)	15.0	(7.6, 22.3)
Secondary → primary						
<12 years	9.8	(7.7, 11.9)	−3.1	(−5.0, −1.3)	5.9	(4.2, 7.6)
12 years	7.5	(6.2, 8.8)	−2.0	(−3.0, −0.9)	4.3	(3.2, 5.4)
13 to 15 years	6.2	(4.6, 7.8)	−1.5	(−2.3, −0.8)	3.2	(2.3, 4.1)
16+ years	2.6	(1.3, 4.0)	−0.5	(−1.1, 0.1)	1.2	(0.3, 2.0)

[a]Continuous measure of seat belt use as defined (see text) by Cohen and Einav (2003).
[b]Marginal effect (percentage points) of mandatory law, controlling for individual covariates (age, age squared, sex, race, ethnicity, marital status, employment, current smoking, body mass index (BMI), past-month alcohol use, past-month binge drinking, past-month heavy drinking, ever driven while intoxicated), and the full set of interactions between education and state- and year-fixed effects.
[c]95 percent CI, estimated by the delta method.

for nonindependence of observations within the same state (Bertrand, Duflo, & Mullainathan, 2004). All analyses were conducted using Stata 12 software.

RESULTS

Differential Effects by Education

Table 3 reports the marginal effects (in percentage points) from linear probability models and conditional on other demographic and behavioral covariates. The first column shows the results for always use as the outcome. We find strong effects of mandatory seat belt laws for all education groups, but the effect is stronger for those with fewer years of education. For example, moving from no seat belt law to a law with secondary enforcement increased reports of always using seat belt use by 23 percentage points among those with <12 years of education, compared to a 17 percentage point increase for those with 16 or more years of education. The impact of laws with primary enforcement is larger, particularly for the lower educated population. Moving directly to a law with primary enforcement increased always use by nearly 45 percentage points among the lowest educated, an effect that was more than 20 percentage points greater than among those with the highest level of education. The effects of upgrading from secondary to primary enforcement were smaller in magnitude, but showed a similar gradient, with about a 10 percentage point increase in always use for the lowest educated and a 3 percentage point increase among the highest educated. For brevity we do not focus on the effects for other covariates, but the associations with seat belt use were similar to those

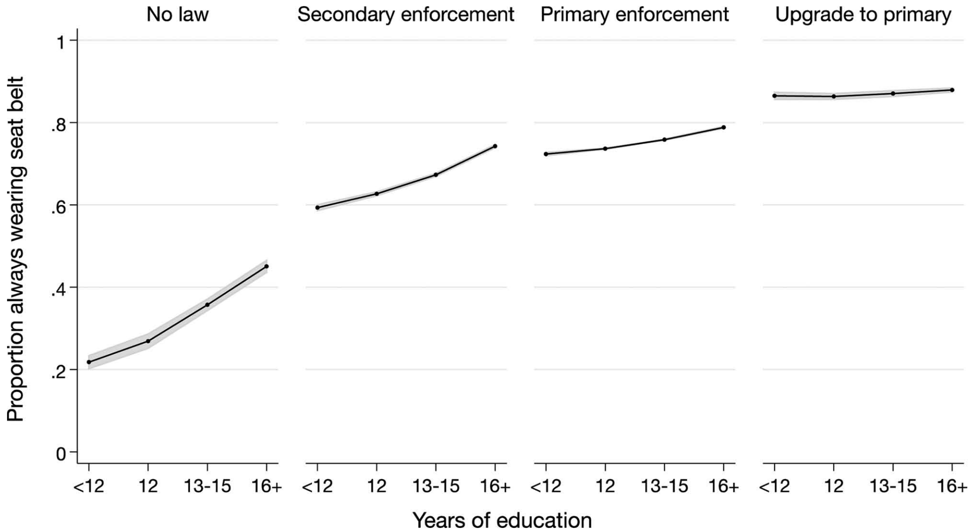

Figure 2. Effect of Mandatory Seat Belt Laws on the Predicted Probability of Always Wearing a Seat Belt Among Education Groups, BRFSS Surveys, 1984 to 2008 ($n =$ 1,371,731).

observed in other observational studies (Beck et al., 2007; Dee, 1998; Goldbaum et al., 1986; Nelson, Bolen, & Kresnow, 1998).[7]

The second and third columns of Table 3 show results for alternative indicators of seat belt use. These estimates speak to whether the differential effect of seat belt laws is observed for increasing always use or whether the effects may be more general across the range of seat belt use frequency categories. Generally speaking, the magnitude and socioeconomic patterning of effects are similar for both infrequent use and our "continuous" measure of seat belt use, with some minor exceptions. Because always use represents the target behavior for most public health campaigns, for the remainder of the paper, we restrict our focus to always use.

Figure 2 shows the effect of laws on the probability of always using seat belts. In the absence of any law, the predicted probability of use was 45 percent for those with >16 years of education, compared to just 22 percent for those with <12 years of education—a difference of roughly 23 points. Moving directly to secondary enforcement increases use for all groups, but because the laws have stronger effects on the lower educated, this reduces the gradient to around 15 points (74 vs. 59 percent). Primary laws go even further, reducing this gap to seven points if implemented directly (79 vs. 72 percent), and the education gap is essentially eliminated when states upgrade from secondary to primary enforcement (88 vs. 87 percent).

[7] Estimates for the effect of individual covariates on always belt use are presented in Table A1. For example, from the models in Table 3 we estimated that the probability of always wearing seat belts were roughly 8 percentage points lower for males, 5 points lower for past month binge drinkers, 10 points lower for those reporting ever having driven while intoxicated, and 7 points lower for current smokers. Blacks were just 2 percentage points less likely than whites to report always wearing seat belts, and Hispanics were slightly more likely to report always wearing seat belts than non-Hispanics. All appendices are available at the end of this article as it appears in JPAM online. Go to the publisher's Web site and use the search engine to locate the article at http://www3.interscience.wiley.com/cgi-bin/jhome/34787.

Table 4. Effect of mandatory seat belt laws on the probability of always wearing a seat belt when driving or riding in a car among selected demographic groups, 1984 to 2008 BRFSS ($n = 1{,}371{,}731$).

	None → Secondary law		None → Primary law		Secondary → Primary	
Comparison[a]	Estimate[b]	95 percent CI[c]	Estimate[b]	95 percent CI[c]	Estimate[b]	95 percent CI[c]
Race						
White	20.0	(15.1, 24.9)	30.6	(23.7, 37.4)	5.3	(3.8, 6.8)
Black	16.7	(12.0, 21.5)	38.3	(31.7, 44.8)	9.3	(5.8, 12.7)
Other	19.1	(7.8, 30.3)	35.6	(12.1, 59.2)	7.2	(3.3, 11.0)
Binge alcohol use[d]						
No	20.6	(15.8, 25.3)	32.2	(23.9, 40.5)	5.4	(3.8, 7.0)
Yes	14.3	(9.0, 19.5)	26.0	(20.5, 31.6)	9.9	(7.2, 12.6)
Heavy alcohol use[d]						
No	20.0	(15.3, 24.8)	32.3	(23.9, 40.6)	5.7	(4.3, 7.2)
Yes	15.5	(9.7, 21.2)	21.7	(14.0, 29.4)	10.1	(7.5, 12.7)
Intoxicated driving						
Never	20.0	(15.2, 24.8)	31.6	(24.0, 39.3)	5.9	(4.4, 7.4)
Ever	8.0	(3.5, 12.4)	25.0	(15.1, 34.8)	7.5	(4.0, 11.0)
Gender						
Female	21.8	(17.4, 26.3)	33.7	(24.5, 43.0)	4.7	(2.7, 6.7)
Male	17.5	(12.3, 22.7)	29.1	(22.7, 35.6)	7.3	(5.5, 9.0)
Smoking						
Never	20.7	(15.4, 26.1)	31.3	(21.4, 41.2)	4.7	(3.0, 6.3)
Current	18.4	(13.3, 23.4)	35.7	(29.4, 42.1)	9.8	(7.8, 11.9)
Former	18.5	(14.7, 22.4)	27.6	(21.3, 33.9)	5.7	(4.4, 7.0)
Body mass index (BMI)						
Normal/under	20.3	(15.3, 25.3)	30.3	(21.8, 38.8)	4.6	(3.1, 6.0)
Overweight	19.7	(14.7, 24.8)	33.3	(25.9, 40.8)	6.0	(4.3, 7.7)
Obese	18.6	(14.5, 22.6)	32.4	(27.0, 37.7)	8.2	(6.1, 10.3)

[a]Each comparison shows the results from a separate model that included state- and year-fixed effects and all individual-level covariates and education and household income (see notes on Table 3 for listing).
[b]Marginal effect (percentage point change).
[c]95 percent CI calculated by the delta method.
[d]Past-month use.

Selective Recruitment

If selective recruitment explained lower seat belt use among disadvantaged socioeconomic groups, we would expect to see a widening of the socioeconomic gap (or at the very least similar effects) after law implementation, but we see the opposite. One explanation could be that selective recruitment is not operating in our sample more generally, perhaps because it applies only in cases where seat belt use is very low. To test whether our finding of larger increases in seatbelt use among the less educated could be due to the absence of selective recruitment in our sample, we ran additional DD models (conditional on individual demographic and behavioral characteristics) interacted with indicators for other demographic groups (e.g., men, heavy drinkers) that have been shown previously to be less responsive to mandatory seat belt laws (Dee, 1998). Table 4 shows the results of a series of separate models of the effect of seat belt laws on differences in the predicted probability of not always wearing seat belts among demographic groups.

In contrast to the patterns for education and income in Table 3, seat belt laws appear to be somewhat *less* effective among other demographic groups typically associated with lower rates of seat belt use. For example, moving from no law to a secondary enforcement law increased always belt use by 22 percentage points among women and 18 points among men, and the corresponding increases in belt use for moving directly to a primary enforcement law were 34 and 29 percent, respectively.

This provides some evidence that women are more responsive to mandatory seat belt laws than are men. Stronger effects of selective recruitment can be seen among individuals reporting on heavy alcohol consumption and intoxicated driving in the past month, demonstrating that this pattern is indeed present in our sample and that its absence is unlikely to explain our main finding. Differential effects of mandatory seat belt laws were less evident for comparisons among individuals categorized by race, smoking status, or levels of body mass index.

Sensitivity Analyses

As a check on the sensitivity of our estimates to the key identifying assumptions of the DD model, we also estimated models that included state-specific linear and nonlinear trends (Angrist & Pischke, 2009). These models try and control for any unobserved state-specific factors that may be changing gradually over time and affect seat belt use (e.g., enforcement). The first three models of Table 5 show the impact of including state-specific linear, quadratic, and cubic time trends in our DD models, and in each case the estimate is similar in magnitude and not statistically different from the basic fixed effects DD model. For the effects of primary enforcement laws and upgrades from secondary to primary, we find similarly consistent effects. Our basic model shows a differential effect of about 25 (44.4 vs. 19.7) and 7 (9.8 vs. 2.6) percentage points, respectively, for primary enforcement laws and upgrades for the lowest educated group, and including state-specific time trends has little impact on these estimates.

One concern about changes in mandatory seat belt laws over this period is that they may be plausibly correlated with changes in other traffic safety legislation that may also affect seat belt use. Model 4 of Table 5 shows results from models that included dummy variables representing changes over time among states in maximum speed limits (55, 60, 65, 70, 75 miles/hour), the presence of a zero tolerance drunk driving law, and laws specifying the legal BAC limit for drivers (0.10 or 0.08). The inclusion of these other traffic safety laws do not appear to have much impact on our estimates, and we still find considerably stronger effects of mandatory laws among lower educated groups. It also seems plausible that legislation could follow from increased attention to motor vehicle accident rates. We therefore estimated models that also included traffic accident mortality rates lagged by one or two years (models 5 and 6) and their inclusion did not change our basic estimates from Table 3. Model 7 includes all of the above factors and gave similar results.

It is difficult to disentangle the effects of laws from those of enforcement, which often increases concurrently with legislation. Unfortunately, there is limited data with which to estimate the effects of heightened enforcement. Carpenter and Stehr (2008) used data on state seat belt incentive grant funding from 1999 to 2005 and found little evidence of any effect on youth seat belt usage, nor did they find it had any impact on their estimated effects of seat belt legislation. In order to measure the effects of enforcement, we obtained annual data on paid media advertising (nominal dollars per capita) and the number of seat belt citations per capita for the only years we could find them, 1999 to 2006 (Tison & Williams, 2010). Unfortunately, during this period the BRFSS only collected information on seat belt use in 2002 and 2006, so we have only two periods to see whether our estimates are robust to measures of enforcement. Furthermore, the only policy changes during this period were upgrades from secondary to primary enforcement (this reduced our sample size to 268,951). Nevertheless, controlling for state-level paid media enforcement and belt citations, we still find evidence that upgrading to primary enforcement increases always use of seat belts by 7.2 percentage points among those with <12 years of education (95 percent confidence interval [CI]: 3.9, 14.0) compared to 5.4

Table 5. Sensitivity analyses for the differential effect by education on always using seat belts.

	DD estimates	State-specific time trends: Linear 1	Quadratic 2	Cubic 3	Changes in speed limits and BAC laws 4	Lagged motor vehicle mortality rates: 1 year 5	2 years 6	All additional controls 7
Secondary law								
<12 years	23.0 (17.0, 29.0)	22.6 (15.9, 29.3)	21.3 (15.6, 27.0)	20.6 (14.5, 26.7)	22.7 (17.2, 28.1)	23.0 (17.0, 29.1)	23.0 (16.9, 29.2)	21.0 (15.4, 26.7)
12 years	20.7 (16.0, 25.4)	20.6 (14.9, 26.2)	19.4 (14.5, 24.3)	18.6 (13.2, 23.9)	20.4 (16.2, 24.7)	20.7 (15.9, 25.6)	20.7 (15.8, 25.7)	19.1 (14.2, 23.9)
13 to 15 years	17.2 (13.1, 21.2)	17.2 (12.0, 22.4)	16.1 (11.6, 20.6)	15.3 (10.7, 19.8)	16.9 (13.3, 20.4)	17.1 (13.1, 21.2)	17.1 (12.9, 21.3)	15.8 (11.5, 20.0)
16+ years	17.0 (12.0, 21.9)	17.0 (10.8, 23.2)	15.7 (10.5, 20.9)	14.6 (9.5, 19.8)	16.6 (12.2, 20.9)	17.0 (12.0, 22.1)	17.0 (11.9, 22.2)	15.5 (10.5, 20.5)
Primary law								
<12 years	44.4 (37.2, 51.6)	44.8 (27.1, 52.5)	40.0 (34.9, 45.2)	34.3 (28.4, 40.3)	44.1 (38.1, 50.0)	44.3 (37.1, 51.5)	44.4 (37.2, 51.6)	43.6 (38.2, 48.9)
12 years	35.6 (29.5, 41.6)	36.3 (29.5, 43.1)	32.8 (29.6, 35.9)	27.7 (23.3, 32.2)	35.7 (31.0, 40.3)	35.6 (29.2, 41.9)	35.8 (29.6, 42.0)	35.7 (30.8, 40.6)
13 to 15 years	28.5 (22.6, 34.5)	29.4 (22.7, 36.2)	26.4 (23.2, 29.6)	21.4 (17.6, 25.3)	28.9 (24.1, 33.7)	28.5 (22.3, 34.8)	28.8 (22.9, 34.8)	28.9 (23.6, 34.2)
16+ years	19.7 (12.2, 27.2)	20.4 (12.1, 28.7)	17.4 (12.6, 22.1)	12.3 (7.7, 16.9)	20.0 (13.9, 26.1)	19.9 (12.3, 27.5)	20.2 (12.8, 27.6)	20.3 (13.7, 26.9)
Primary upgrade								
<12 years	9.8 (7.7, 11.9)	9.8 (6.6, 13.0)	10.0 (8.1, 11.9)	11.0 (9.0, 12.9)	9.4 (7.3, 11.4)	10.0 (7.8, 21.1)	10.3 (8.1, 12.5)	9.4 (7.0, 11.7)
12 years	7.5 (6.2, 8.8)	7.6 (5.1, 10.1)	8.0 (6.6, 9.4)	9.0 (7.4, 10.6)	7.1 (5.4, 8.7)	7.4 (5.8, 9.1)	7.7 (6.2, 9.3)	6.8 (4.8, 8.8)
13 to 15 years	6.2 (4.6, 7.8)	6.5 (3.7, 9.4)	6.8 (4.9, 8.7)	7.7 (5.9, 9.6)	5.8 (4.1, 7.5)	6.7 (4.8, 8.6)	7.1 (5.3, 8.8)	6.3 (4.0, 8.6)
16+ years	2.6 (1.3, 4.0)	2.8 (0.2, 5.4)	3.2 (2.0, 4.5)	4.2 (2.7, 5.8)	2.2 (0.5, 3.9)	3.1 (1.6, 4.6)	3.4 (1.9, 4.9)	2.5 (0.7, 4.3)
State-specific time trends								
Linear	N	Y	Y	Y	N	N	N	Y
Quadratic	N	N	Y	Y	N	N	N	Y
Cubic	N	N	N	Y	N	N	N	Y
Max. speed limit	N	N	N	N	Y	N	N	Y
Zero tolerance/BAC laws	N	N	N	N	Y	N	N	Y
Motor vehicle accident rates								
One-year lag	N	N	N	N	N	Y	N	Y
Two-year lag	N	N	N	N	N	N	Y	N
Obese	1,371,731	1,371,731	1,371,731	1,371,731	1,316,444	1,057,735	1,057,735	1,012,979

Notes: Each column shows the marginal effect (percentage point change and 95 percent CI) from a separate model that included state- and year-fixed effects (and two-way interactions with education) and all individual-level covariates (see notes on Table 3 for listing).

points (95 percent CI: 1.8, 9.0) among those with 16 or more years of education (results not shown). The enforcement variables were not statistically distinguishable from zero: The percentage point effect (95 percent CI) for paid media enforcement was 0.57 (−2.2, 3.3) and for belt citations per capita was −0.02 (−0.22, 0.18).

To assess the impact of the changing composition of BRFSS states, Table 6 shows estimates from models where we restricted the sample to states that had continuously participated in the survey over a given time period. Model 1 shows the effects for the 15 states that have participated continuously since 1984. The point estimates are similar in both magnitude and socioeconomic patterning to our main results, but with increased uncertainty. However, for the impact of primary enforcement laws the estimated differential effects among lower educated groups are quite similar to the full sample. Restricting to states participating since 1990 (model 2) produced generally similar estimates. We also looked at whether our full sample estimates might differ in the early (1984 to 1995) versus later period of observation (1995 to 2008) and found little evidence for heterogeneity.

In models 5 to 7 of Table 6, we provide some further evidence that effects of seat belt laws are specific to seat belt use by using outcomes that are unlikely to be affected by changes in mandatory seat belt laws: self-reports of current smoking, past-month binge alcohol consumption, and obesity. We estimate our basic DD models allowing for heterogeneous effects by education for these outcomes conditional on our other demographic and behavioral controls. For all three placebo outcomes we find null or nearly null effects.

Timing of Effects

Previous research has generally shown that the overall effects of mandatory seat belt laws occur shortly after legislation is passed, if not immediately thereafter (Cohen & Einav, 2003; Dee, 1998). We modified our DD models by including dummy variables for one and two years prior to the year of legislation, and one, two, or more than two years after legislation (Angrist & Pischke, 2009). Figure 3 shows these estimates separately by education group. The x-axis shows time (in years) relative to the year of adoption, and the y-axis shows the estimated change in always using seat belts. These estimates indicate that the differential effect of mandatory seat belt laws occurs in the year of adoption and generally persists for several years thereafter, and do not provide much evidence that changes in belt use occurred prior to legislative changes.

DISCUSSION

The major contribution of this paper is the new finding that mandatory seat belt laws differentially increase reported seat belt use among disadvantaged socioeconomic groups. The differential effect by education was considerably larger for states moving directly from no law to a law with primary enforcement (25 percentage point difference), but was also evident for states that upgraded from secondary to primary enforcement (7 percentage point difference).

Given that much of the past literature (e.g., Blomquist, 1991; Braver, 2003; O'Conor et al.; Wilson, 1990) emphasized generalized differences in risky behavior as a potential explanation for socioeconomic differences in seat belt use, our results are somewhat surprising, as the theory of selective recruitment would suggest that seat belt laws would widen socioeconomic differences in use. In fact, our finding of a narrowing of socioeconomic differences in seat belt use suggests that individuals with lower socioeconomic status are more responsive to seat belt laws. This could be due to the inherently regressive nature of any fixed fine or to the

Table 6. Panel restrictions and placebo tests for the differential effect by education on always using seat belts.

		Restrictions on the panel of states or time period[a]				DD for placebo outcomes[b]		
	DD estimates	15 states participating since 1984 1	45 states participating since 1990 2	Early period (1984 to 1995) 3	Later period (1995 to 2008) 4	Current smoking 5	Binge drinking 6	Heavy alcohol use 7
Secondary								
<12 years	23.0 (17.0, 29.0)	18.2 (8.7, 27.8)	23.0 (17.1, 29.0)	23.2 (16.1, 30.4)	26.0 (23.5, 28.5)	−0.2 (−1.4, 1.1)	0.2 (−0.5, 1.0)	0.2 (−0.3, 0.7)
12 years	20.7 (16.0, 25.4)	17.5 (10.9, 24.0)	20.7 (16.0, 25.3)	21.5 (15.2, 27.8)	15.8 (14.7, 16.8)	0.1 (−0.7, 0.9)	0.1 (−0.7, 1.0)	0.0 (−0.4, 0.4)
13 to 15 years	17.2 (13.1, 21.2)	16.2 (11.1, 21.3)	17.2 (13.2, 21.2)	17.2 (11.6, 22.9)	12.4 (11.2, 13.5)	0.7 (−0.2, 1.5)	0.2 (−0.5, 1.0)	0.3 (−0.1, 0.8)
16+ years	17.0 (12.0, 21.9)	14.0 (7.8, 20.1)	17.0 (12.1, 21.8)	17.8 (11.3, 24.3)	10.4 (9.5, 11.3)	0.3 (−0.5, 1.1)	0.4 (−0.4, 1.1)	0.2 (−0.3, 0.7)
Primary								
<12 years	44.4 (37.2, 51.6)	41.2 (32.0, 50.4)	44.4 (37.4, 51.5)	44.5 (36.5, 52.4)	n/a	−1.1 (−5.3, 3.1)	0.8 (−0.4, 2.0)	0.1 (−0.9, 1.1)
12 years	35.6 (29.5, 41.6)	35.4 (30.2, 40.6)	35.5 (29.6, 41.5)	36.5 (29.2, 43.7)	n/a	0.9 (−0.2, 2.1)	1.8 (0.4, 3.2)	0.7 (−0.4, 1.7)
13 to 15 years	28.5 (22.6, 34.5)	30.8 (26.1, 35.6)	28.6 (22.7, 34.4)	30.1 (23.2, 37.1)	n/a	−0.1 (−1.8, 1.6)	0.8 (−0.8, 2.5)	0.5 (−0.2, 1.3)
16+ years	19.7 (12.2, 27.2)	19.4 (12.8, 26.0)	19.7 (12.3, 27.0)	21.9 (13.9, 29.9)	n/a	−1.3 (−2.7, −0.0)	1.1 (0.3, 2.0)	−0.1 (−0.6, 0.5)
Switch								
<12 years	9.8 (7.7, 11.9)	8.6 (5.7, 11.6)	9.8 (7.7, 11.9)	11.3 (8.7, 13.9)	9.3 (5.8, 12.7)	−0.5 (−2.0, 1.1)	0.8 (−0.2, 1.7)	0.4 (−0.1, 0.9)
12 years	7.5 (6.2, 8.8)	6.4 (4.2, 8.6)	7.6 (6.3, 8.9)	9.1 (7.0, 11.2)	7.9 (6.0, 9.9)	−0.4 (−1.2, 0.4)	1.0 (−0.1, 2.2)	0.5 (0.1, 0.9)
13 to 15 years	6.2 (4.6, 7.8)	5.0 (2.4, 7.5)	6.4 (4.9, 8.0)	8.0 (5.6, 10.4)	6.0 (3.9, 8.1)	−0.8 (−1.7, 0.1)	1.0 (−0.1, 2.0)	0.4 (−0.2, 1.0)
16+ years	2.6 (1.3, 4.0)	1.1 (−1.2, 3.4)	2.7 (1.3, 4.0)	5.5 (3.3, 7.7)	2.8 (1.2, 4.4)	0.0 (−0.7, 0.6)	1.3 (0.2, 2.4)	0.7 (−0.0, 1.5)
Obese	1,371,731	449,015	1,261,142	512,211	942,038	1,371,731	1,371,731	1,371,731

[a]Each column shows the marginal effect (percentage point change and 95 percent CI) from a separate model that included state- and year-fixed effects (and two-way interactions with education) and all individual-level covariates (see notes on Table 3 for listing).
[b]Marginal effect (percentage point change and 95 percent CI) for separate logistic models run for each education group including state- and year-fixed effects and individual-level covariates (see notes on Table 3 for listing).

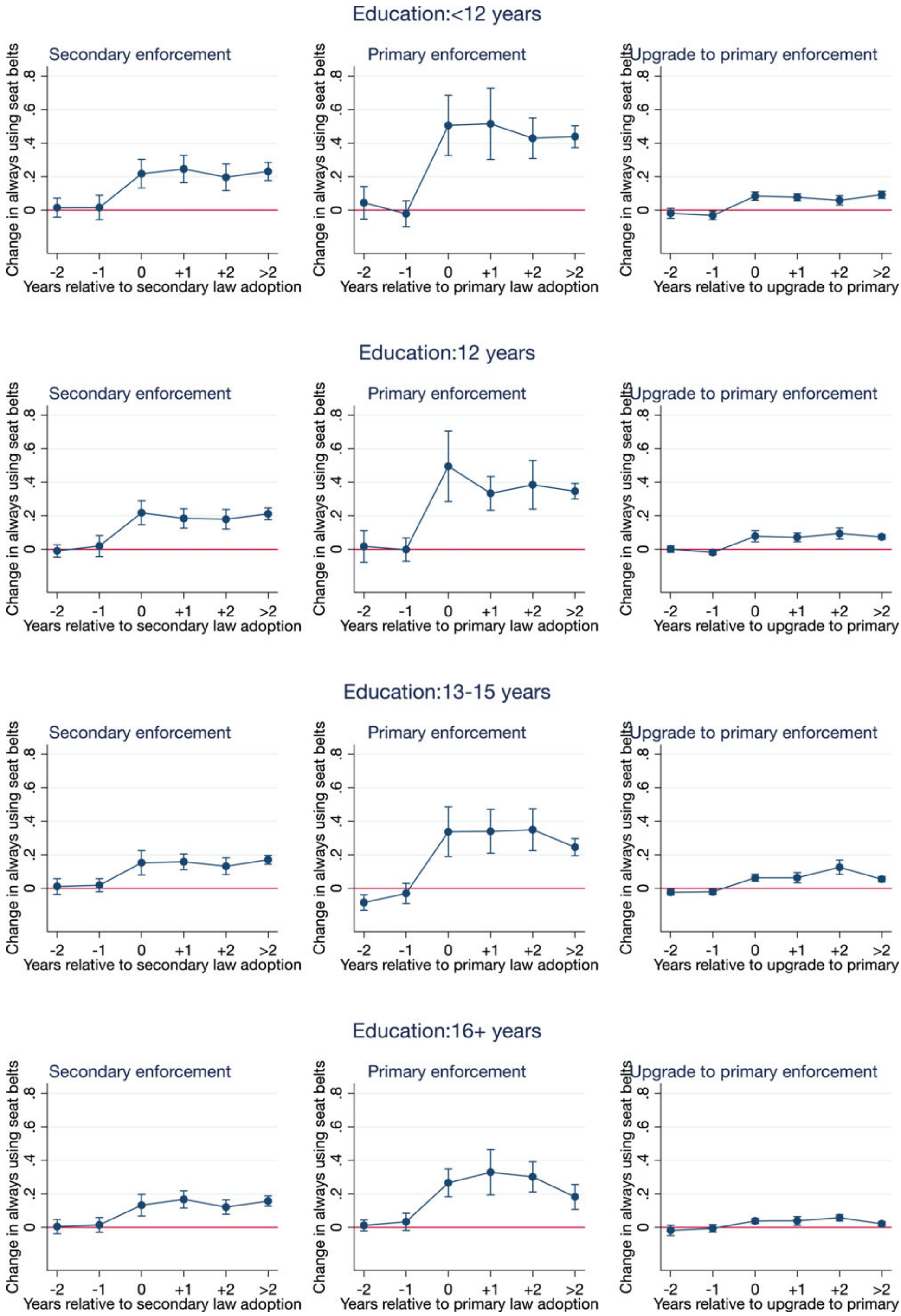

Figure 3. Lead and Lagged Effects of Mandatory Seat Belt Laws on the Probability Always Wearing a Seat Belt by Education, BRFSS Surveys, 1984 to 2008 ($n =$ 1,371,731).

potentially higher costs of interacting with law enforcement (e.g., if individuals with lower education are less likely to have insurance, up-to-date car inspection, or readily verifiable landed-immigrant status). It could also be the result of different distributions of risk preferences in more and less educated subgroups, which imply differential changes in seat belt use for a shift in the risk aversion threshold for wearing a seat belt common across groups.[8]

Since we found supporting evidence for selective recruitment among other subpopulations in our sample (e.g., widening gaps by gender and drinking behavior), we are more confident that any selective recruitment effect and differences in risk aversion by education do not explain the differential response to seat belt laws by education.

Another possible explanation of this differential responsiveness is that rates of seat belt use are approaching the feasible maximum among the more advantaged groups, though overall rates in the United States remain low by international standards (European Transport Safety Council, 2006). This seems possible, but there are some reasons to think otherwise. First, our results in Table 3 demonstrate that the effects of legislation are strong even for the most advantaged socioeconomic groups. Second, our estimation strategy allows for the possibility that socioeconomic differences may be changing over time for reasons unrelated to legislation by including a socioeconomic group-by-time interaction. Finally, we restricted our sample to later years of data (after the strongest increases in belt use), which yielded effects that remained statistically different from zero and were similar to the full sample results. This is consistent with other evidence that primary seat belt laws are effective even among states with initially higher levels of belt use (Shults, Elder, et al., 2004).

It could also be argued that it is not the law itself, but the economic consequences of being ticketed that lead individuals of lower socioeconomic position to increase seat belt usage more in response to seat belt laws, as fines are often adopted concurrently with legislation. We attempted to incorporate data on the maximum fine for the first offense of failure to wear a seat belt (ranging from US $10 to $75). However, there is little temporal variation among states in maximum fines over this period (Insurance Institute for Highway, 2012), thus additional evidence will be necessary to rule out such an explanation.

The above sensitivity analyses suggest that the presence of the law itself may be more likely to increase use among lower socioeconomic groups. Surveys conducted by the U.S. National Highway Traffic Safety Administration (Boyle & Lampkin, 2008; Boyle & Sharp, 1997) show that lower educated individuals are more likely than the better-educated to cite the law as the most important reason for wearing seat belts, and are less likely to cite avoiding injury (also see Hunter et al., 1990). In the 1996 survey, 14 percent of those with <12 years of education cited the law as the most important reason for wearing seat belts, compared to only 5 percent of college graduates. In contrast, only 63 percent of those with <12 years of education cited avoiding injury, compared to 72 percent of college graduates (Boyle & Sharp, 1997). This patterning was similar in the 2007 survey—13 and 59 percent of the least educated cited the law and avoiding injury, respectively, compared to 5 and 72 percent for college graduates (Boyle & Lampkin, 2008).

Mandatory seat belt laws, while effective, are probably not sufficient to increase belt use alone—enforcement is crucial and other mechanisms may continue to generate inequalities in seat belt use where primary enforcement is the norm (Leveque, Humblet, & Lagasse, 2004). Socioeconomic inequalities in motor vehicle accidents

[8] We thank an anonymous reviewer for this suggestion.

remain even in countries where mandatory seat belt laws have been in place for decades (Borrell et al., 2005). And even if socioeconomic differences in belt use are eliminated, it is likely that socioeconomic differences in motor vehicle accidents will persist, though at reduced levels (Braver, 2003). This suggests that mechanisms other than seat belt use, such as differences in vehicle quality (Girasek & Taylor, 2010), access to effective trauma care, driver behavior, or crash severity may also play a role in generating socioeconomic differences in motor vehicle accident mortality. Thus, an important question for future study is whether seat belt laws reduce socioeconomic differences in motor vehicle accident mortality. However, the potential for answering this question is hampered by the historical lack of accurate information on socioeconomic position in U.S. motor vehicle accident and death certificate data (Sorlie & Johnson, 1996). Given more widespread reporting of education on death certificates since the mid-1990s, it may be possible to assess the impacts of upgrades to primary enforcement laws.

CONCLUSIONS

It is well known that mandatory seat belt laws increase seat belt use, but our results indicate that such laws, particularly those with primary enforcement, also reduce the gap in seat belt use between low and high socioeconomic groups by 15 to 25 percentage points. It thus appears that seat belt laws have played a key role in the reductions in socioeconomic inequalities in seatbelt use over the past two decades. Despite secular declines, socioeconomic differences in seat belt use are still evident, and, given that currently only half of U.S. states have mandatory seat belt laws with primary enforcement, upgrading from secondary to primary enforcement is likely to be an effective policy for further reducing socioeconomic inequalities in seat belt use.

SAM HARPER is Assistant Professor of Epidemiology at McGill University, 1020 Pine Avenue West, Room 36B, Montreal, QC H3A 1A2, Canada.

ERIN C. STRUMPF is Associate Professor of Economics and Epidemiology, McGill University, 1020 Pine Avenue West, Montreal, QC H3A 1A2, Canada.

SCOTT BURRIS is Professor of Law, Temple University, 1719 North Broad Street, Philadelphia, PA 19122.

GEORGE DAVEY SMITH is Professor of Clinical Epidemiology, University of Bristol, Office OS10, Oakfield House, Oakfield Grove, Clifton, BS8 2BN, U.K.

JOHN LYNCH is Professor of Public Health, University of Adelaide, 178 North Terrace & Stephens Place, Level 7, MDP DX 650 550, South Australia 5005, Australia.

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APPENDIX

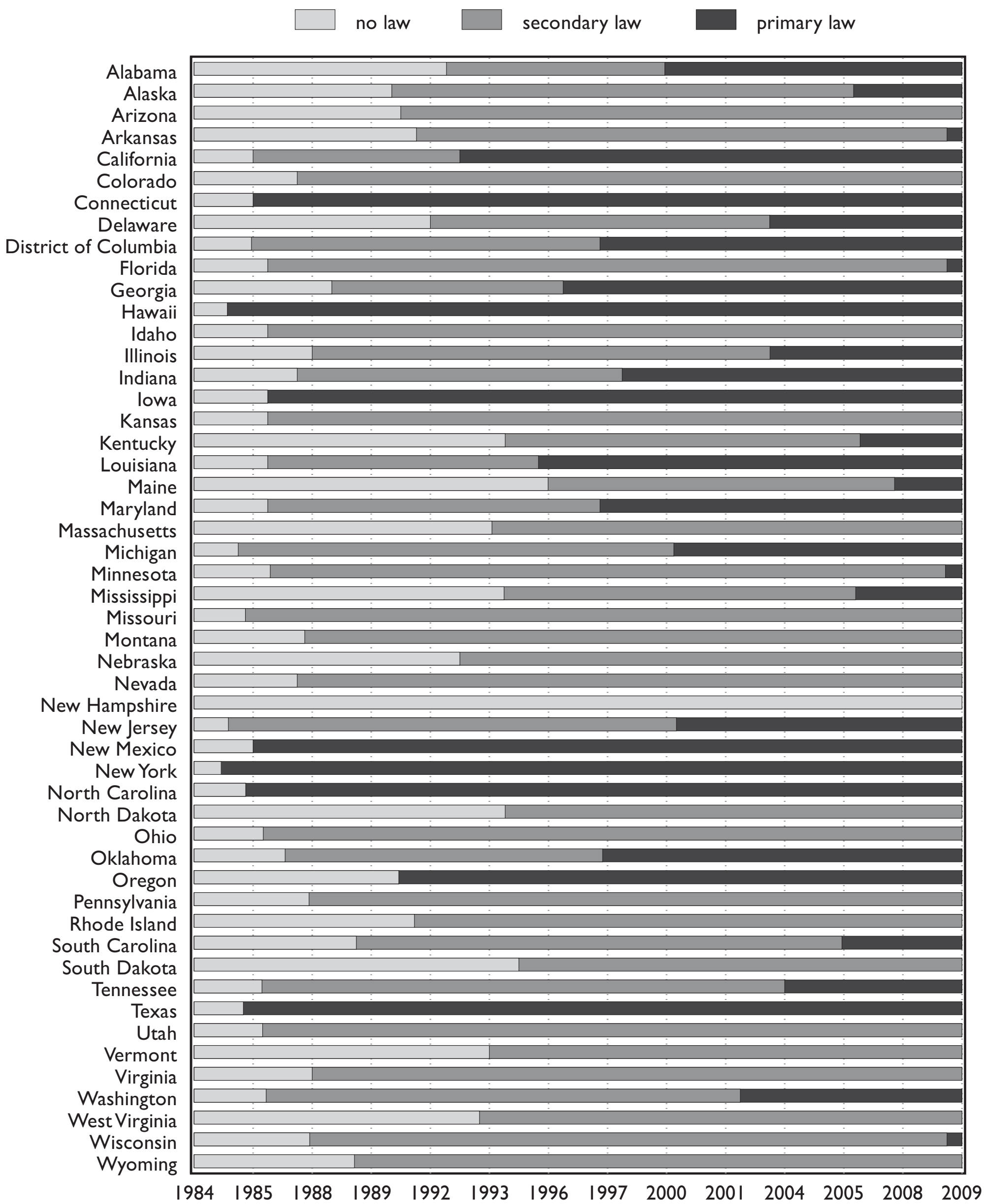

Figure A1. Changes in Mandatory Seat Belt Laws Among U.S. States, 1984 to 2009 (Cohen & Einav, 2003; Insurance Institute for Highway Safety, 2012).

Table A1. Full regression results for education: Always belt use, BRFSS 1984 to 2008 (N = 1,371,731).

Variable	Coefficient	Standard error	t	P-value	Lower 95 percent CI	Upper 95 percent CI
Education (reference = 16+ years)						
<12 years	−0.195	0.024	−8.24	0.000	−0.243	−0.148
12 years	−0.123	0.014	−8.52	0.000	−0.152	−0.094
13 to 15 years	−0.034	0.019	−1.8	0.078	−0.072	0.004
Mandatory law (reference = none)						
Secondary enforcement	0.170	0.025	6.86	0.000	0.120	0.219
Primary enforcement	0.197	0.037	5.28	0.000	0.122	0.272
Upgraded to primary enforcement	0.026	0.007	3.88	0.000	0.013	0.040
Education × secondary law						
<12 years	0.061	0.018	3.43	0.001	0.025	0.096
12 years	0.037	0.013	2.87	0.006	0.011	0.064
13 to 15 years	0.002	0.009	0.22	0.830	−0.017	0.021
Education × primary law						
<12 years	0.247	0.024	10.18	0.000	0.198	0.296
12 years	0.159	0.011	14.65	0.000	0.137	0.181
13 to 15 years	0.089	0.012	7.33	0.000	0.064	0.113
Education × primary upgrade						
<12 years	0.071	0.009	7.93	0.000	0.053	0.090
12 years	0.049	0.005	10.03	0.000	0.039	0.059
13 to 15 years	0.036	0.006	5.82	0.000	0.024	0.048
Age	0.000	0.001	0.78	0.441	−0.001	0.002
Age^2	0.000	0.000	0.36	0.719	0.000	0.000
Household size	−0.007	0.001	−4.98	0.000	−0.010	−0.004
Male	−0.084	0.005	−18.41	0.000	−0.094	−0.075
Hispanic	0.014	0.005	2.82	0.007	0.004	0.023
Race (reference = white)						
Black	−0.017	0.007	−2.3	0.025	−0.032	−0.002
Other race	0.021	0.007	3.03	0.004	0.007	0.035
Marital status (reference = married)						
Divorced/widowed	−0.032	0.003	−10.4	0.000	−0.039	−0.026
Never married	−0.036	0.003	−10.56	0.000	−0.043	−0.030
Employment status (reference = employed)						
Not employed	0.011	0.004	3.03	0.004	0.004	0.018
Retired	0.034	0.005	6.21	0.000	0.023	0.045
Smoking status (reference = never smoker)						
Current smoker	−0.068	0.004	−18.64	0.000	−0.076	−0.061
Former smoker	−0.008	0.002	−3.59	0.001	−0.012	−0.003
Body mass index (BMI) category (reference = normal)						
Overweight (25 to 30 kg/m^2)	−0.030	0.003	−11.85	0.000	−0.035	−0.025
Obese (<30 kg/m^2)	−0.078	0.004	−17.7	0.000	−0.087	−0.069
Past-month alcohol use	0.002	0.003	0.66	0.514	−0.004	0.007
Past-month binge drinker	−0.052	0.003	−15.8	0.000	−0.058	−0.045
Heavy alcohol use	−0.034	0.004	−9.37	0.000	−0.041	−0.026
Ever driven intoxicated	−0.097	0.006	−15.92	0.000	−0.110	−0.085
Constant term	0.361	0.017	20.97	0.000	0.326	0.396

Notes: State- and year-fixed effects (and their two-way interactions with education) not shown.