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RECONSIDERING THE EFFECTS OF SEAT BELT LAWS AND THEIR ENFORCEMENT STATUS

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Abstract—The debate over the benefits of mandatory seat belt laws and their enforcement status has focused on a controversial empirical enigma: why have these policies, which appear to have increased belt use sharply, had a relatively small impact on traffic fatalities? In this paper, I offer new insights into this question by examining panel data on observed belt use from the National Highway Traffic Safety Administration and self-reported data on belt use from pooled cross-sections of the Centers for Disease Control and Prevention's 1985–1993 Behavioral Risk Factor Surveillance System. By exploiting the panel nature of these data, I demonstrate that prior estimates, which have not conditioned on the unobserved time-varying determinants of belt use, have dramatically overestimated the impact of seat belt laws and their enforcement status on belt use. The true effects are more consistent with the modest impact these policies have had on traffic fatalities without having to appeal to the possibility of risk compensation by drivers. However, I find strong evidence in support of the selective recruitment hypothesis. Belt use among those most likely to be involved in traffic accidents (e.g. males, drinkers of alcohol, the young) has been significantly less responsive to seat belt laws and their enforcement status. © 1998 Published by Elsevier Science Ltd. All rights reserved

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INTRODUCTION

Over the past 30 years, state and Federal agencies have instituted a variety of standards and laws designed to improve traffic safety. For vehicle producers, these mandates have included production criteria for crash avoidance, crashworthiness, seat belts and air bags. For drivers, these interventions have meant the required use of seat belts as well as limits on maximum speeds and blood alcohol content. The efficacy of these safety interventions in reducing traffic fatalities has been controversial (Robertson, 1996). Some researchers have argued that the impact of these interventions has been offset or, at least, attenuated by a compensating increase in risk-taking by drivers (Peltzman, 1975; Garbacz, 1990; Chirinko and Harper, 1993). One important empirical justification for the risk compensation hypothesis is the perception that mandatory seat belt laws have generated large increases in seat belt use but only relatively modest reductions in traffic fatalities[1]. The research presented in this paper offers insights into this question by presenting new evidence on how the introduction and enforcement of seat belt laws have affected use.

In the early 1980s, the U.S. Department of Transportation (DOT) and a number of key national organizations endorsed the adoption of mandatory seat belt laws (Campbell and Campbell, 1988). In December of 1984, New York became the first state with such a law. By the end of 1993, 42 more states and the District of Columbia had adopted similar measures[2]. A large increase in rates of belt use has been attributed to the introduction of these laws. For example, based on comparisons of roadside observations before and after the introduction of a seat belt law, Campbell and Campbell (1988) report a doubling or even tripling of belt use. More specifically, Evans and Graham (1991) compare similar data and find that use rose an average of 28 percentage points after the law went into effect. The effects appeared even larger in states that enacted a primary enforcement status for the law (i.e. stopping motorists for no offense other than not wearing a seat belt).

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[1]Other empirical evidence in support of this view has consisted of controversial research that has linked safety interventions to increased injuries among non-occupants like pedestrians and bicyclists (Chirinko and Harper, 1993; Peltzman, 1975). Evans and Graham (1991) find only weak evidence in support of this phenomenon.

[2]This tally includes Nebraska which adopted a seat-belt law in 1985, repealed it in 1987 and reinstated it in 1993 but excludes Massachusetts which had a seat-belt law during 1986 but did not reinstate it until 1994.

The use of seat belts is known to reduce the risk of dying in a crash. Analysts at the U.S. DOT (1984) concluded that 40–50% of fatalities among drivers and front-seat passengers could be prevented by the use of a lap and shoulder seat belt[3]. Based on these numbers, one might expect the introduction of mandatory seat belt laws to reduce fatalities by as much as 14% (0.28×0.50) in the absence of a compensatory response by drivers. The observed reduction in fatalities, though significant in every sense of the word, has been much lower. For example, Evans and Graham (1991) find that the introduction of the seat belt laws reduced fatalities by only 8%[4]. These simple calculations form the basis of an important policy question: why weren't the benefits of seat belt laws greater? One consistent but controversial explanation is that a compensatory increase in risk-taking by drivers reduced the law's benefits.

The research presented here offers new evidence on this question by reconsidering how seat belt laws and enforcement actually affected the level and pattern of use. The standard approach which has consisted of comparing pre- and post-law use may generate a biased and incomplete picture of the law's true effects for two important reasons. One source of concern is that this approach may overestimate the law's effect by confounding the advent of the law with other independent, time-varying determinants of belt use. For example, the period during which states were adopting seat belt laws was also one during which information about the life-saving benefits of seat belts was being widely disseminated. This information may have had an effect on belt use that was independent of the enactment of seat belt laws. An unbiased estimate of the effect of seat belt laws on belt use requires distinguishing its timing from that of other unobserved time-varying determinants of belt use. A second reason to be concerned that comparing pre- and post-law use does not give an accurate picture is that the law's effect may not be homogenous: unsafe drivers may be the least likely to adjust their belt use after the introduction of the law. If the selective recruitment hypothesis were true, the reduction in traffic fatalities associated a seat belt law would appear relatively modest because those most likely to be in accidents (e.g. males, drinkers of alcohol, the young) would have had the smallest behavioral response[5].

These issues were addressed by analyzing two data sets that contain both cross-sectional and time-series variation in belt use. One data set consists of information on belt use observed by the National Highway Traffic Safety Administration (NHTSA) in 19 metropolitan areas over the 1985–1991 period. The second data set employed in this study consists of the 1985–1993 Behavioral Risk Factor Surveillance System (BRFSS) surveys conducted by the Centers for Disease Control and Prevention. These annual, national telephone surveys contain self-reported information on seat belt use and a variety of other health behaviors. Because both data sets pool the time-series variation in several states, the estimations presented here can compare traditional evaluations of seat belt policies with new evaluations that condition on the unobserved, time-varying determinants of belt use. Furthermore, because the BRFSS data contain rich individual-level information, the hypothesis of selective recruitment can be directly tested by comparing the responsiveness of belt use among accident-prone samples to that of the general population.

OBSERVED BELT USE

Over the 1985–1991 period, the NHTSA gathered data on belt use in 19 metropolitan areas. More specifically, these frequently used data were gathered by direct observation at several randomly selected intersections and freeway exits in each metropolitan area (U.S. DOT, 1989). The 19 cities included in this survey were Atlanta, Baltimore, Birmingham, Boston, Chicago, Dallas, Fargo/Moorhead, Houston, Los Angeles, Miami, Minneapolis/St. Paul, New Orleans, New York, Phoenix, Pittsburgh, Providence, San Diego, San Francisco and Seattle. The Fargo–Moorhead metropolitan area was deleted from the sample because it covers two states. The total number of remaining observations for this 7-year period is 126. These data track belt use during a period of considerable variation in belt laws. All but three of the 18 cities were in states that introduced seat belt laws over this period[6]. The exceptions are New York

[3]Using data on actual crashes, Evans (1986) generates similar estimates.

[4]This estimate was based on a panel of states from 1975 to 1987 and was conditioned on unobserved state-specific and time-varying determinants of fatalities. Other researchers have concluded that fatality reduction ranged from 5 to 8% (Campbell et al., 1986; Wagenaar et al., 1987; Partyka, 1985).

[5]The selective recruitment hypothesis is not a novel one (Campbell and Campbell, 1988). However, the estimations presented here provide a new and direct tests of the existence and magnitude of this phenomenon.

[6]Most laws became effective in the middle of calendar years. The coding convention employed here was that if a law became effective on or before 1 July, it was considered in effect for that year. The correspondence between the subsequent results and prior research suggests that this approach is not problematic. Additionally, estimations with the BRFSS data which were matched to law introductions on a monthly basis generate similar results. Information on the seat belt laws in the 50 states and the District of Columbia has been drawn from Evans and Graham (1991), the Statistical Abstract of the United States (1995) and U.S. DOT (1993).

and Chicago which had seat belt laws throughout the 1985–91 periods and Birmingham which had none[7]. In Boston, respondents faced a seat-belt law only during 1986. The results to be presented are robust to excluding Boston from the data set.

The effect of mandatory seat belt laws and their enforcement on use has been identified by ordinary least squares (OLS) estimation of eqn (1).

$$B_{ct} = \gamma L_{ct} + v_c + u_t + \epsilon_{ct} \tag{1}$$

Eqn (1) is a linear probability specification for a two-way fixed-effects model. B_{ct} is the rate of observed belt use in city c in year t. The term, v_c, represents unobserved city-specific determinants of belt use. The term, u_t, represents unobserved year-specific determinants of belt use and ϵ_{ct} is a mean-zero random error. L_{ct} is a binary indicator for the presence of a seat belt law in a given state at time t[8]. There are other reasonable functional forms for eqn (1). For example, an explicit concern for the binary nature of belt use would suggest a 'log–odds' specification (Maddala, 1983). However, the results presented here are robust to both log–odds and log-linear formulations of eqn (1). The linear probability specification in eqn (1) is preferred because it facilitates a comparison of these results with those of prior researchers. More specifically, the traditional approach to evaluating the efficacy of seat belt laws and their enforcement levels has consisted of comparing pre- and post-law belt use. In the context of eqn (1), this amounts to an OLS estimate of γ in a specification that omits the year-specific controls, u_t. A specification concern raised here is that such estimates might confound the true effect of the law and its enforcement with unobserved, time-varying determinants of belt use.

Results

The estimates of eqn (1) which are presented in Table 1 indicate that this specification concern is valid[9]. Model (1), which omits year effects, suggests that the introduction of seat belt laws increased the rates of belt use by 26.9 percentage points. This estimate is similar to the 28 percentage point increase reported by Evans and Graham (1991) who compared pre- and post-law belt use in 31 states. The estimates in model (2), which also omits year effects, suggest that belt use rose 46.4 percentage points in states adopting primary enforcement and 24.4 percentage points in states adopting secondary enforcement[10].

Models (3) and (4) add year effects to these models. The reference year in these estimations is 1985. The estimated year effects in both models indicate a statistically significant and positive national trend towards increased belt use over this period[11]. For example, model (3) indicates that belt use in 1987 was over 10 percentage points higher than in 1985, independent of the introduction of seat belt laws. More importantly, model (3) demonstrates that traditional evaluations of the effect of seat belt laws have confounded the timing of these laws with this national trend towards increased use. The point estimates in model (3) indicate that seat belt laws increased belt use in these 18 cities by only 17.2 percentage points. Though this effect is still large and statistically significant, it demonstrates that the traditional approach to evaluating the effect of seat belt laws has overestimated their impact by 56.4% [(0.269 − 0.172)/0.172]. A similar pattern emerges in comparing models (2) and (4). The conventional result that the enforcement status of belt laws affects belt use is robust to the inclusion of year effects. A belt law with primary enforcement status improved belt use more than one with secondary enforcement status. However, the omission of year effects implies a substantial upward bias in estimates of these effects.

This new estimate of how seat belt laws affected belt use has important implications for the debate over risk compensation by drivers. Though belt use reduces fatality risk by 40–50%, a fatality reduction of roughly 8% has been attributed to the introduction of seat belt laws (Evans and Graham, 1991). However, the expectation of a larger fatality reduction has been based on the notion that belt laws increased use by much more than 17.2 percentage points. Given that belt use rose only 17.2 percentage points in response to the law, a fatality reduction of around 8% is entirely consistent with the technological efficacy of seat belts and requires no appeal to the possibility of compensating risk-taking by drivers[12]. In other words, this new estimate of how seat belt laws affected belt use implies that drivers did not

[7]Nonetheless, these observations are included in the subsequent estimations since they help identify the national time-series variation in belt use.

[8]When addressing the effect of the law's enforcement status, there will instead be two indicators: one for primary enforcement and one for secondary enforcement. The reference in both specifications is the absence of any seat belt law.

[9]All four models fit the data well, explaining roughly 80–90% of the variation in observed belt use.

[10]Some caveats are appropriate for these estimates. They may not be as generalizable since only one of the states represented in this data set adopted primary enforcement (Texas).

[11]The year effects are jointly, as well as individually, significant. Using the R^2 from models (1) and (3) in Table 1, the test F-statistic for the hypothesis that the coefficients on the year effects are zero is $[(0.900 - 0.826)/6]/[(1 - 0.900)/101] = 12.45$. Since this value easily exceeds the standard critical values of the F-statistic, the hypothesis is rejected.

[12]More specifically, the expected fatality reduction would range from 6.9% (0.172×0.40) to 8.6% (0.172×0.50).

Table 1. Linear probability models: the policy determinants of seat belt use, NHTSA 1985–1991*

	With city effects		With city and year effects	
Independent variables	model (1)	model (2)	model (3)	model (4)
Mandatory seat belt law	0.269 (14.6)	—	0.172 (8.9)	—
Primary enforcement	—	0.464 (9.1)	—	0.351 (8.3)
Secondary enforcement	—	0.244 (13.4)	—	0.158 (8.8)
1986	—	—	0.056 (2.6)	0.043 (2.2)
1987	—	—	0.108 (5.0)	0.095 (4.8)
1988	—	—	0.135 (6.1)	0.122 (6.0)
1989	—	—	0.133 (5.9)	0.121 (5.8)
1990	—	—	0.158 (7.0)	0.146 (7.0)
1991	—	—	0.159 (6.6)	0.148 (6.8)
Intercept	0.316 (10.1)	0.338 (11.4)	0.292 (11.7)	0.315 (13.5)
R^2	0.826	0.850	0.900	0.918

*Absolute values of t-statistics are reported in parentheses.

substantially attenuate the safety benefits of the new laws by increasing their risky driving.

The empirical relevance of the year effects in these estimations also begs the question as to why there were sharp rises in belt use independent of the enactment and enforcement status of seat belt laws. One possible scenario is that public debate about these laws effectively disseminated information to the driving public regarding the health benefits of belt use. However, traditional public education campaigns regarding belt use have been notoriously ineffective (Williams and Lund, 1988). Nonetheless, the first mandatory seat belt laws were preceded by several high-profile endorsements from organizations like the American Public Health Association, the Consumer's Union, the U.S. DOT, the American Automobile Association, the American Medical Association and General Motors (Campbell and Campbell, 1988). Furthermore, in the years following these endorsements, almost every state discussed and approved a mandatory seat belt law. The estimated year effects in Table 1 provide some casual support for the hypothesis that this early national discourse substantially influenced belt use. The independent nationwide increases in belt use were particularly large in the mid-1980s and small in the 1990 and 1991. However, other hypotheses might also be consistent with this empirical evidence. Furthermore, with regard to the research questions addressed here (i.e. the impact of state policies), this question is largely moot. The year effects control for the unobserved, time-varying determinants of nationwide belt use that would otherwise bias the evaluations of interest.

SELF-REPORTED BELT USE

One of the drawbacks of the grouped data employed in the previous section is that they cannot identify how patterns of belt use have varied by important demographic characteristics like age, gender, race and ethnicity and by other salient behaviors like alcohol consumption. In this section, I present empirical models of belt use that are based on nationally representative individual-level data which do include such information. These rich data allow the hypothesis of selective recruitment to be tested directly. Additionally, because these data contain cross-sectional and time-series variation, the estimations based on these data can provide further evidence on the importance of unobserved, time-varying determinants of belt use.

Data

The Center for Disease Control and Prevention's (CDC) annual BRFSS surveys have gathered a wide variety of information on individual health behaviors and knowledge. Each BRFSS survey is a "state-based random-digit-dialed telephone survey that collects self-reported data from a representative sample of civilian, non-institutionalized persons aged greater than or equal to 18 years."[13] In the 1985–1993 surveys, over half a million respondents were asked certain 'core' questions. One of the health behaviors about which all respondents were consistently asked was their use of seat belts.

The five salient responses to this question ranged from 'always' to 'never'[14]. Seat belt use for respondents to the BRFSS surveys has been defined as a binary indicator that is equal to one for those who

[13]Some states have used clustered or stratified sample designs in order to oversample minority populations of interest. Some of the subsequent estimations condition on these stratifying variables; sample weights are not used.

[14]Respondents who were 'not sure', who refused to answer or who claimed they never drive or ride in cars have been deleted from our sample.

Table 2. Descriptive statistics by year, BRFSS surveys, 1985–1993

		Proportion of annual respondents facing:				
Survey year	Mean belt usage (%)	Mandatory belt law* (%)	Primary enforcement (%)	Secondary enforcement (%)	Number of states in survey	Number of observations in survey
1985	25.7	8.1	5.8	2.2	23	24,609
1986	38.3	47.7	20.5	27.2	26	33,030
1987	43.9	56.9	18.0	38.9	33	48,172
1988	48.7	66.7	20.5	46.2	37	54,404
1989	51.7	70.5	19.0	51.5	40	64,478
1990	54.8	70.0	17.6	52.3	45	77,799
1991	58.7	78.2	20.7	57.5	48	84,447
1992	62.0	83.6	21.9	61.7	49	92,328
1993	64.2	86.5	20.5	65.9	50	98,155
All years	54.1	70.9	19.4	51.5	50	577,422

*Primary and secondary enforcement may not sum to this column due to rounding.

responded that they 'always' used their seat-belts and zero otherwise. This is an admittedly ad hoc construction. However, this definition of belt use provides a close match between what nationally representative data on observed belt use exists and the contemporaneous rate of belt use among BRFSS respondents[15]. Furthermore, the subsequent estimations demonstrate that evaluations based on this definition correspond closely with those based on observational data. Nonetheless, the pattern of results to be presented are robust to other reasonable constructions of the dependent variable[16]. The full sample consists of the 577,422 respondents aged 18–80. Fifty states are represented in these pooled surveys. However, each state is not represented in each survey year. The mean belt use, sample size and number of surveyed states are listed by survey year in Table 2.

The empirical models of belt use have included as covariates some inarguably exogenous, individual demographic characteristics: age; gender; race; and ethnicity. Because the relationship between age and belt use may be non-linear, age squared is also included as a covariate. It is possible that one of the reasons that the traditional comparisons of pre- and post-law belt use generate biased estimates of the law's impact is that they ignored the within-state variation in these demographic characteristics over time. However, that does not prove to be the case; evaluations of seat belt laws are robust to the inclusion of such demographic variables. Nonetheless, understanding the pattern of belt use indicated by these demographic variables could prove useful for policy-makers interested in designing policies that increase belt use.

Because the BRFSS interviews occur on a monthly basis throughout the calendar year, respondents can be matched with unusual accuracy to the timing of new state laws. Respondents have been coded as facing a new law and its enforcement status if their interview occurs in or after the first full month during which the law is effective. With two exceptions, seat belt laws were not repealed after becoming effective. As mentioned earlier, Massachusetts drivers faced a seat belt law only during 1986. Nebraska had a seat belt law from September of 1985 through 1986 and in 1993. The results to be presented are robust to excluding respondents in these two states. Only five states (Kentucky, Maine, New Hampshire, North Dakota and South Dakota) had no seat belt laws over this period[17]. Nine of the 46 states with seat belt laws mandated primary enforcement while the remaining 37 had secondary enforcement. However, there are no BRFSS respondents in this sample from Wyoming which had secondary enforcement over this period. The means in Table 2 indicate that there has been substantial within-state variation in these policy instruments over the 1985–1993 period. In 1985, only 8.1% of BRFSS respondents faced a seat belt law and the mean rate of belt use was 25.7%. In 1993, nearly 87% of respondents faced a seat belt law and belt use had risen to 64.2%.

In order to address the selective recruitment

[15]The 19-city survey, which was discontinued after 1991, was not nationally representative (U.S. DOT, 1996). However, the population-weighted national rate of observed belt use in 1992 was 62% (U.S. DOT, 1993). In that same year, the rate of belt use among BRFSS respondents was also 62% according to the definition employed here (Table 2).

[16]For example, if those who 'nearly always' use seat belts are also defined as users (Robertson, 1992) or if belt use is defined by the unadjusted ordered response to the survey question, similar results are generated.

[17]Interestingly, though they never adopted mandatory belt laws, reported belt use grew sharply in these states over this period. This independent trend underscores the importance of controlling for the time-varying, nationwide determinants of belt use.

hypothesis, particular attention will be paid to belt use among several groups that are known to be involved in a high proportion of traffic accidents. For example, alcohol is involved in over 40% of all fatal crashes (U.S. DOT, 1995). Several samples have been defined by their alcohol consumption. A drinker is a respondent who reports having had any alcoholic drink in the past month. Frequent drinkers are those who report having had 10 or more drinks in the past month. A binge drinker reports having had five or more drinks in a row sometime in the last month[18]. Nearly half of the BRFSS respondents are drinkers (286,086 observations) while roughly 25% are frequent drinkers (146,846 observations). Nearly 14% (78,333 observations) are binge drinkers. Drivers under the age of 25 also have one of the highest rates of involvement in fatal crashes (U.S. DOT, 1995). Therefore, another sample consists of the 75,644 respondents who were aged 25 or less when interviewed. Since the male involvement rate in fatal crashes is three times that of females (U.S. DOT, 1995), another sample consists of the 245,851 respondents who are male. The remaining samples are based on the 34,265 respondents who are both young and male. Since rates of alcohol use and traffic accidents are particularly high among young males, additional samples of young males have been defined by their alcohol consumption. The selective recruitment hypothesis is directly supported if the belt use among these samples has been relatively less responsive to mandatory seat belt laws and their enforcement status.

The quality of self-reported data

An important advantage of self-reported data is that information on belt use can be matched to other policy-relevant individual characteristics. However, there has been some question about whether these self-reported data on belt use accurately represent actual behavior. In particular, Robertson (1992) has suggested that the belt use data from telephone surveys are not predictive of actual belt use. This conclusion was based on observing that, in 1988, the average reported belt use in 13 states consistently overstated average observed use in those states. However, that exercise compared use in the metropolitan areas observed by the NHTSA to self-reported data that were gathered throughout the given states. Garbacz (1990) has suggested that the moderate fit between self-reported and observed belt use may be due to the possibility that observational data for a given city are not representative of the rates of use in the entire state. Using carefully gathered observational data for North Carolina, Garbacz (1990) finds a strong fit between observed use and that reported by BRFSS respondents.

Furthermore, there is additional evidence that the comparisons of Robertson (1992) understate the fit between observed belt use and that reported by BRFSS respondents. I matched the 126 city-by-year observations of belt use used in the previous section to the corresponding state-by-year rates of reported use from the BRFSS surveys[19]. The correlation coefficient between these two measures ($\rho=0.81$) implies a strong, positive relationship. However, the state-level rates of reported belt use were, on average, 10 percentage points higher than the contemporaneous rates of belt use observed in a state's cities. This difference could reflect a bias towards overstating actual belt use. However, it could also reflect the differing scope of each data set's sample design. The strong fit between more recent, nationally representative observational data on belt use and that reported by BRFSS respondents lends support to the latter hypothesis (see footnote 15).

Nonetheless, even if we were to assume this difference reflected a reporting bias, the self-reported belt use data from the BRFSS surveys might still be useful for evaluation research. In order to evaluate the impact of seat belt laws and their enforcement, the salient question is not whether these self-reported data present an accurate snapshot of use in a state at a given moment. Instead, it is whether changes in self-reported use within a state accurately track changes in actual behavior. The use of the self-reported data for evaluation research would only be problematic if a presumed bias towards overstating belt use increased with the introduction of seat belt laws or their enforcement status. Therefore, perhaps the most convincing evidence on the propriety of using the BRFSS data comes from comparing the evaluations based on those data to the evaluations from the previous section which were based on observational data. Estimations based on the self-reported BRFSS data are presented in Tables 3 and 4. The strong correspondence between those results and those based on observed data (Table 1) implies that self-reported belt use accurately tracks changes in actual behavior, particularly in response to within-state policy changes.

[18]These definitions do not explore all the heterogeneity in alcohol consumption. However, these definitions are standard in the literature on the welfare consequences of alcohol use (Grossman et al., 1993).

[19]Since the BRFSS surveys do not cover every state and year, the number of matched observations was 107.

Specification

This section presents estimates of the determinants of belt use based on the individual-level self-reported data from the BRFSS surveys. The basis for these estimations is a linear, latent variable model in which the net benefit of belt use, B^*, to person i in state s at time t:

$$B^*_{ist} = W_{ist}\Pi + L_{st}\gamma + v_s + u_t + \epsilon_{ist} = X_{ist}\beta + \epsilon_{ist} \quad (2)$$

is a function of individual attributes, W_{ist}, and unobserved state-specific and time-specific attributes (v_s and u_t). The term, ϵ_{ist}, is a mean-zero random error. As in eqn (1), the term, L_{st}, is a binary indicator for the presence of a seat belt law in state s at time t. The net benefits to using seat belts are not directly observed. However, the decision to use seat belts is. Let that decision be defined by:

$$B_{ist} = 1 \text{ if } B^*_{ist} \geq 0$$
$$B_{ist} = 0 \text{ if } B^*_{ist} < 0 \quad (3)$$

Assuming that ϵ_{ist} is normally distributed and that $\Phi(.)$ is the cumulative distribution function, the probability of seat belt use can be expressed as:

$$\text{Prob}(B^*_{ist} \geq 0) = \text{Prob}(B_{ist} = 1) = \text{Prob}(\epsilon_{ist} \geq -X_{ist}\beta)$$
$$= \Phi(X_{ist}\beta) \quad (4)$$

This probability provides a familiar basis for a probit estimation of the determinants of seat belt use. However, estimations based on the logistic distribution or on a linear probability model generate results very similar to those reported for this probit specification. I report the marginal effects of the key components of X on the probability of belt use. These marginal effects $[\partial(\text{Prob}(B=1))/\partial X = \beta\phi(X\beta)]$ have been defined for the mean level of belt use in the given sample[20]. The marginal effects are more useful than the probit coefficients since they can be directly compared to the OLS estimates in Table 1 as well as to the estimates reported in prior research.

The results of estimating the probit models for the entire BRFSS sample are reported in Tables 3 and 4. These probit models fit the belt use data well. More specifically, the accuracy of these models can be judged by the fit between reported belt use and that predicted by the models (Maddala, 1983). As a baseline, it is useful to consider a naive model of belt use. Since the mean level of belt use in these data is 54.1%, such a naive model would always predict that $B_{ist} = 1$ and would be correct over 54 times out of 100. However, these probit models generate many more correct predictions. Even the simplest probit specification [model (1) in Table 3] generates correct predictions over 64 times out of 100[21]. Including additional covariates improves the predictive ability of the model.

Demographic patterns

These estimations reported in Tables 3 and 4 indicate a consistent and statistically significant pattern of belt use across demographic groups. Females are nearly 11 percentage points more likely than males to use their seat belts. Blacks and Hispanics are, respectively, around 5 and 2 percentage points less likely than whites to use seat belts. Furthermore, belt use is an increasing function of age until roughly age 57. Elderly drivers are less likely to use their seat belts. These patterns are important information for policy-makers interested in designing measures that will increase belt use. However, since the within-state trends in these characteristics are uncorrelated with the timing of seat belt laws, their omission does not appear to be a problematic feature of prior work that has addressed the overall impact of those laws.

The importance of year effects

The first estimation reported in Table 3 replicates the standard approach for evaluating the effect of a seat belt law on use. By conditioning on state attributes and the timing of the law but not on year effects, this specification simply compares belt use before and after the introduction of the law. Model (1) of Table 3 indicates that, after the introduction of a seat belt law, respondents were 30.1 percentage points more likely to use their seat belt. This large and statistically significant estimate is quite similar to the estimated effect of 26.9 percentage points that was based on observational data (Table 1) and the effect of 28 percentage points reported by Evans and Graham (1991) who compared pre- and post-law rates of belt use based on observational data. What small difference exists between these estimates could be due to biases in self-reported data. However, it could also be due to the longer time period covered by the BRFSS data or to the possibility that the self-reported data are more representative of belt use in the population than the observational data. Regardless, the small magnitude of the difference between these estimates places a reasonable bound on the bias that could possibly be due to any flaws in self-reported belt use.

[20] More specifically, they have been defined for probability density function, $\phi(z)$ evaluated where $\Phi(z)$ = the sample mean of belt use.

[21] This calculation is based on a cross-tabulation of reported belt use and predicted belt use based on the estimated index function, $X_{ist}\beta$.

Table 3. Probit marginal effects: the determinants of seat belt use, BRFSS 1985–1993*

Independent variables	With state effects		With state and year effects	With state and month effects
	model (1)	model (2)	model (3)	model (4)
Mandatory seat belt law	0.301 (124.8)	0.304 (125.4)	0.185 (62.2)	0.183 (60.7)
Age	—	0.004 (17.5)	0.004 (16.0)	0.004 (15.9)
Age squared	—	−0.000038 (14.9)	−0.000035 (13.5)	−0.000035 (13.5)
Female	—	0.107 (77.8)	0.109 (78.8)	0.109 (78.8)
Black	—	−0.053 (20.7)	−0.056 (21.9)	−0.056 (21.9)
Hispanic	—	−0.020 (5.8)	−0.024 (7.0)	−0.024 (7.0)
Other race	—	0.044 (10.5)	0.043 (10.1)	0.043 (10.1)
Log likelihood	−367,539	−363,936	−360,853	−360,721

*Absolute values of t-statistics are reported in parentheses. The marginal effects are defined for the mean level of belt use. This data set consists of 577,422 observations.

Table 4. Probit marginal effects: the determinants of seat belt use, BRFSS 1985–1993*

Independent variables	With state effects		With state and year effects	With state and month effects
	model (1)	model (2)	model (3)	model (4)
Primary enforcement	0.370 (56.9)	0.376 (57.6)	0.262 (37.6)	0.260 (37.2)
Secondary enforcement	0.290 (111.7)	0.292 (112.1)	0.174 (56.2)	0.172 (54.7)
Age	—	0.004 (17.5)	0.004 (16.0)	0.004 (15.9)
Age squared	—	−0.000038 (14.9)	−0.000035 (13.5)	−0.000035 (13.5)
Female	—	0.107 (77.9)	0.109 (78.9)	0.109 (78.8)
Black	—	−0.053 (20.8)	−0.056 (21.9)	−0.056 (21.9)
Hispanic	—	−0.020 (5.9)	−0.024 (7.0)	−0.024 (7.0)
Other race	—	0.044 (10.5)	0.043 (10.1)	0.043 (10.1)
Log likelihood	−367,473	−363,864	360,778	−360,646

*Absolute values of t-statistics are reported in parentheses. The marginal effects are defined for the mean level of belt use. This data set consists of 577,422 observations.

The central concern raised about these estimates is that they may be biased because they have confounded the timing of the new law with a significant overall trend towards increased belt use. Models (3) and (4) which condition on unobserved time-varying determinants of belt use indicate that this concern was valid. As in the empirical models based on observational data, the year effects in model (3) are both individually and jointly significant. Similarly, these effects also point to a monotonic increase in belt use over this period that was independent of the enactment of seat belt laws. More importantly, after conditioning on these year effects, the estimated impact of a seat belt law on use fell to 18.5 percentage points[22]. The standard approach which did not control for year effects overestimates the impact of these laws by nearly 63% [(0.301 − 0.185)/0.185]. Given the empirical importance of the year effects, it is important to ask whether the 8 year effects in model (3) provide adequate proxies for the unobserved, time-varying determinants of belt use. Model (4), which substitutes month effects for year effects, suggests that they do. The use of over 100 month effects did not substantively alter the results generated by model (3).

The estimations reported in Table 3 indicate that a similar pattern emerges if the effect of the laws' enforcement status is estimated. Models (1) and (2) indicate that after the introduction of primary enforcement, seat belt use increased by 37.0 percentage points. In states with secondary enforcement, use rose by 29.0 percentage points. These effects are similar to the comparisons reported by Evans and Graham (1991) who noted that use appear to rise by 34 percentage points in primary enforcement states and by 25 in secondary enforcement states. After conditioning on year effects, the estimated impacts of primary and secondary enforcement fall to 26.2 and 17.2 percentage points, respectively. This implies that the traditional comparisons which ignore the unobserved time-varying determinants of belt use overestimate the true effect of primary enforcement by more than 41% [(0.370 − 0.262)/0.262] and of secondary enforcement by over 66% [(0.290 − 0.174)/0.174].

The selective recruitment hypothesis

Another concern raised about the standard approach of comparing average pre- and post-law

[22]This estimate is also strikingly close to that based on observational data (Table 1).

Table 5. Probit marginal effects: the policy determinants of seat belt use by sample, BRFSS 1985–1993*

Sample	model (1)	model (2)		Number of observations
	Mandatory seat belt law	Primary enforcement	Secondary enforcement	
All respondents	0.185 (62.2)	0.262 (37.6)	0.174 (56.2)	577,422
Drinkers	0.159 (38.0)	0.234 (25.0)	0.148 (33.7)	286,086
Frequent drinkers	0.153 (26.7)	0.227 (17.5)	0.142 (23.8)	146,846
Binge drinkers	0.153 (19.0)	0.242 (12.5)	0.141 (16.8)	78,333
Young	0.145 (17.8)	0.229 (11.7)	0.134 (15.9)	75,644
Males	0.168 (36.5)	0.232 (21.8)	0.158 (32.9)	245,851
Young males	0.117 (9.7)	0.171 (5.9)	0.110 (8.7)	34,264
Young male drinkers	0.114 (7.8)	0.192 (5.4)	0.104 (6.8)	22,349
Young male frequent drinkers	0.105 (6.2)	0.167 (4.2)	0.097 (5.5)	15,719
Young male binge drinkers	0.110 (5.9)	0.215 (4.7)	0.086 (5.0)	13,209

*Absolute values of t-statistics are reported in parentheses. The marginal effects are defined for the mean level of belt use in each sample. All models include state and year effects, age, age squared and indicators for gender, race and ethnicity.

use was that it could not identify a pattern of heterogeneity in how drivers might respond to state policies. More specifically, there is concern that the individuals most likely to be involved in traffic accidents might be the least likely to adjust their belt use in response to a seat belt law and its level of enforcement. Unfortunately, this question cannot be easily addressed with observational data on belt use. However, the estimations reported in Table 5 provide direct evidence on whether this hypothesis is accurate by estimating the effect of these state policies on belt use among samples of drinkers, frequent drinkers, binge drinkers, the young, males and young males and young males who are drinkers, frequent drinkers and binge drinkers.

The results in Table 5 demonstrate that seat belt laws and their enforcement status did have a large and statistically significant effects on belt use among these important samples. For example, the enactment of a seat belt law raised the probability of belt use 15.9 percentage points among drinkers, 15.3 percentage points among frequent and binge drinkers, 14.5 percentage points among the young, 16.8 percentage points among males and 11.7 percentage points among young males. However, the magnitudes of these effects were uniformly smaller than that identified for the entire sample. In other words, the behavioral response of the entire sample significantly overestimates the response in these policy-relevant groups[23]. These estimations, therefore, demonstrate that the selective recruitment hypothesis is accurate: belt use among these important populations has been significantly less responsive to state policies.

[23]For example, the t-statistic for the null hypothesis that the overall effect of the seat belt law equaled 0.168 (the response of males) is 5.7 [(0.185 − 0.168)/0.0030]. The hypothesis that the response of the entire sample accurately measures that of the males is rejected. Replicating this exercise for the other samples and the enforcement status generates similar conclusions.

Another policy-relevant pattern to these results is that this difference is particularly pronounced for young males and young males who use and abuse alcohol. The response of the entire sample to seat belt laws overestimates the response of young males by more than 58% [(0.185 − 0.117)/0.117]. However, the effect of a seat belt law with primary enforcement status appears relatively robust. For example, the responsiveness of the entire sample to secondary enforcement of a seat belt law overstates that of young males who are binge drinkers by 102% [(0.174 − 0.086)/0.086]. The effect of a belt law with primary enforcement is only overstated by 22% [(0.262 − 0.215)/0.215] for that sample. In other words, belt use among young males who were binge drinkers was relatively responsive to a seat belt law if that law had primary enforcement status. These results reinforce the impression created by estimations with the full sample: though almost all states have seat belt laws, policy-makers may be able to generate further increases in belt use and traffic safety through changes in the enforcement status of these laws.

Nonetheless, taken together, these results imply that the existence of selective recruitment has attenuated the safety benefits of seat belt laws. Belt use among the accident-prone was less responsive to the enactment of seat belt laws and their enforcement status. However, these estimates also suggest that the magnitude of this attenuation has been relatively small since even accident-prone respondents had a significant behavioral response to state seat belt policies. For example, suppose all drivers and passengers shared the relatively weak response of young males to the enactment of a seat belt law (i.e. an 11.7 percentage point increase in belt use). A fatality reduction of roughly 5.3% (0.45 × 0.117) would still follow the enactment of such a law. Similarly, further evidence on the small impact of selective recruitment comes from the fact that the observed fatality reductions associated with seat belt laws are roughly consistent with the changes in belt use among the general population.

CONCLUSIONS

The results presented in this paper lend further support to the conventional wisdom that the introduction and enforcement status of mandatory seat belt laws have significantly affected belt use. However, this research has also demonstrated that the standard comparisons of pre- and post-law use dramatically overestimated the true magnitude of these effects by confounding the timing of the state law with a strong, contemporaneous trend towards increased use. More specifically, the results presented here demonstrated that traditional evaluations overstated the impact of seat belt laws on belt use by roughly 60%. Furthermore, these results illustrated that the effect of state seat belt policies on use was not homogenous. Those most likely to be involved in traffic accidents were significantly less responsive to the enactment of seat belt laws and their enforcement status.

Both of these results have important policy implications. First, the more modest effects of seat belt laws and their enforcement status on belt use are entirely consistent with the technological efficacy of seat belts and the observed reduction in traffic fatalities. The experience with seat belt laws in the United States does not support the hypothesis that drivers dramatically attenuate the benefits of traffic safety interventions by increasing their risk-taking. However, these results do imply that the benefits of these policies were reduced by the weaker behavioral response among the most accident-prone drivers. But, because even accident-prone drivers substantially increased their belt use in response to seat belt laws and their enforcement status, the magnitude of this attenuation has been relatively small.

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