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Abstract

According to the offsetting effect theory, since drivers wearing seat belts feel more secure, they tend to drive less carefully and may cause more accidents, including those involving pedestrians. Most previous studies have used only state-level accident data, which cannot control for individual characteristics of drivers, vehicles, and the environmental factors surrounding the accidents. This paper uses individual-level accident data to analyze how drivers respond to the laws exploiting changes in the seat belt laws in a number of US states in the last decade. I find that the laws do not cause less careful behavior by drivers. In fact, they drive more carefully when more stringent seat belt laws are in effect, and this leads to less involvement of pedestrians in accidents. These results show that the offsetting effects do not exist when all accidents, including fatal accidents, are considered.

Keywords: Offsetting Effects, Safety Regulation, Seat Belt Laws, Vehicle Accidents

JEL Classification : D01, L62, L51

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I. Introduction

It is widely accepted that mandatory seat belt laws reduce the fatalities of drivers who wear seat belts. However, there have been ongoing discussions regarding the effectiveness of the laws. According to the offsetting effect theory, suggested by Peltzman (1975), since drivers wearing seat belts feel more secure, they drive less carefully, causing more fatal accidents involving pedestrians. If the laws resulted in more pedestrian involvement in accidents and the resulting fatalities were sizeable enough to offset the decrease in the fatalities of drivers and passengers, then the seat belt regulation would be considered ineffective. This paper reinvestigates the existence of the effects.

Many earlier studies have only investigated the effects of the seat belt laws on the fatality rates of drivers and passengers. Some have directly tested the effectiveness of the seat belt laws on the fatalities of the non-occupants who are involved in fatal accidents. These tests show mixed results. Furthermore, even some supporting results do not provide direct evidence on the relationship between the laws and the offsetting behavior, even though many factors are appropriately controlled for in their models. This is mainly because most literature uses only either aggregated state-level or survey data.

Several problems may arise from using state-level data for this type of research. First, the state-level data cannot correctly measure heterogeneity among drivers and their behavioral change. By focusing on the causal relationship between the laws and the fatality at the state level, researchers failed to figure out if the laws caused drivers' adverse behavior, and in turn, if it led to the frequent involvement of non-occupants in accidents. Another issue is that most literature focuses only on fatality rates because of data limit. The overall effects of a regulation on safety should include both the monetary value of injuries and fatalities. In other words, the reduction of accidental harm should include the magnitude of less severe injuries of occupants and non-occupants because of the laws. Therefore, we can still conclude that the overall effect of the primary seat belt laws on accidental harm is ambiguous.

This paper answers three major questions. First, would seat belt laws cause drivers' more aggressive and/or less careful driving behavior? By looking at individual accident data with the specific locations of crashes, I investigate if there is a direct link between the laws and the behavioral change. Second, would less careful driving behavior, if the effects existed, result in the involvement of more pedestrians and other non-occupants in accidents? Third, how does the less careful driving behavior play a role as a link between the laws and the involvement of non-occupants? To answer these questions, this paper develops a unique model of identifying drivers' behavioral changes by observing each individual driver's responses to the change in the laws.

I find that the laws do not cause less careful behavior by drivers. In fact, they drive more carefully when more stringent seat belt laws are in effect, and this leads to less pedestrian involvement in accidents. These results show that the offsetting effects do not exist when all accidents, including fatal accidents, are considered. As a policy implication, I recommend the use of stronger punitive penalties along with the seat belt laws. This policy tool can be used to increase the expected monetary costs of not wearing seat belts so that it induces less careful drivers to wear them.

The paper consists of five sections. In the next section, I review the literature on offsetting behavior. Section III discusses the empirical strategy and the econometric models. Section IV describes data. Section V discusses estimation results as well as sample selection, endogeneity, and sensitivity analysis. The last section draws conclusions and policy implications.

II. Literature

It is widely accepted that seat belt usage reduces fatalities among those wearing seat belts. According to the 2008 survey from the National Highway Traffic Safety Administration (NHTSA), seat belt usage has risen steadily, while there has been a steady decline in passenger vehicle occupant fatalities per mile traveled[1]. Most economic literature has focused on whether the seat belt laws have reduced aggregate fatalities or not, regardless of the type of individuals involved in accidents. Many papers (McCarthy (1999), Derrig et al. (2002)) use time series data and analyze whether there is any statistically significant difference before and after the law enforcement. However, such studies neglect to control for a time trend, and since macro effects unrelated to seat belt laws also affect fatalities, this is an important limitation of these studies. The tests (with mixed results) for offsetting effects are only considered as a secondary concern to those who focus

[1]NHTSA: Traffic Safety Facts - Laws, 2008.

on aggregate fatality.

Other studies try to investigate directly if the offsetting effect in fact exists[2]. Among others, Garbacz(1990) finds a positive relationship between seat belt usage and fatality of non-occupants. Recent studies use panel data models using state level data. Evan and Graham (1991) use pooled data from 50 states. They find that there is weak evidence that fatalities of non-occupants increase and they conclude that offsetting behavior appears to be small, relative to lifesaving effects. Cohen and Einav(2003) investigate it by looking at the effects of the laws on the fatalities of non-occupants. They notice a potential endogeneity problem of seat belt usage and use seat belt laws as an instrument for it. They do not find any significant evidence on the compensating behavior. These studies focus on the factors that affect fatalities and use the increase in the fatality rate as evidence of offsetting behavior, which could not be direct evidence of the behavioral change.

Some of the empirical studies focus on an individual's personal characteristics. They find that heterogeneity across individuals is important. Loeb (1995) uses monthly accident data in only one state to remove the state-wide differences in the laws. He finds that the state's seat belt law results in a reduction in the various driver-involved injury rates. However, as long as they use aggregate data, in particular, state-level data, many problems still persist. After controlling for state-specific characteristics, they could easily find whether the laws in different states reduced fatalities, given a fixed number of fatalities. However, it is very difficult to observe an individual driver's behavioral change and test if this behavioral change affects fatalities using the state-level data. Furthermore, there is no direct link between behavioral change and the laws. State-level data obtained from surveys is subject to serious measurement error concerns and unobserved hetereogeneity given the lack of control of individual-level characteristics regarding the driver, the vehicle, and the environmental conditions surrounding the accidents.

Sobel and Nesbit (2007) use individual-level data to test for individual human responses to safety improvements within a well-controlled environment. They use data from the National Association for Stock Car Auto Racing (NASCAR). Their results strongly support the presence of these offsetting behavioral effects. Since those drivers are always driving at the limit of what

[2]Earlier studies include Peltzman (1975, 1977), Robertson (1977), Crandall and Graham (1984), Garbacz(1990), Evans and Graham (1991), and Loeb (1995).

is safe, it is expected that the authors would find a clear offsetting effect. Professional racecar drivers on a closed course, participating in a competition in which the objective is to beat all other drivers, are certainly not representative of average drivers on our roads.

Many studies only used data from fatal crashes. The use of only fatal crashes may not accurately measure the effectiveness of a safety regulation and it may result in sample selection bias. Some (Levitt et al. (2001)) have proposed a solution to remove this bias by including only crashes in which someone in a different vehicle dies.

Singh and Thayer (1992) use models based on individual-specific survey data to see if seat belt usage affects the number of citations for moving violations. Their results show that the compensating behavior hypothesis only applies to those individuals who are not strongly averse to risk, and that individual risk preferences are an important dimension. They find that drivers' risk preferences may be irrelevant to the behavioral change. They also find that the existence of the offsetting behavior may not necessarily result in the increase in non-occupant involvement.

My paper does not investigate the effects of seat belt laws on traffic fatalities. The question to be answered is whether I can identify the direct link between the behavioral change due to the laws and accidental harm, including fatalities. For this, I use individual-level accident data over a five-year period from the National Highway Safety Administration (NHTSA)[3].

III. Empirical Strategy and Econometric Models

There are two types of law enforcement. One is primary enforcement in which occupants can be ticketed simply for not using their seat belts. The other is secondary enforcement under which occupants must be stopped for another violation, such as a speeding violation, before they can be cited for seat belt nonuse[4]. Thus, the primary enforcement is a much stronger regulatory tool. As of 2010, 32 states had primary seat belt laws in effect (Table 1)[5]. I use this information to see if stronger law enforcement causes careless driving behavior and then if the behavior, if any,

[3]The detailed description of the data is discussed in section V.

[4]NHTSA, "Traffic Safety Use in 2008", DOT HS 811 036.

[5]Even though many states adopt primary seat belt laws, the coverage and the maximum fee differ from state to state. For instance, Texas charges $ 200, while many states charge only $ 10.

results in more involvement of pedestrians.

The estimation strategy consists of two steps. First, I observe each accident that occurred between 2003 and 2007. Since the year 2003, 13 states have changed their seat belt laws from secondary to primary, so I will focus on the accidents that occurred in these states. Within a state, accidents occur either before (pre-accidents) or after (post-accidents) the enforcement date of the primary seat belt law. I test if there are behavioral differences among drivers who have accidents before and after the date. Each observation is a driver who is involved in an accident. It contains a variable, a zip-code, that shows the location of the driver's home address [6]. It also shows when the accident occurred. The observation tells us whether or not the accident occurs under the primary seat belt law. Since accidents occur on different dates within a state as well as across the states, some occur before the primary seat belt laws are adopted, while others occur after them. I use drivers' careless behavior to measure the behavioral difference before and after the enforcement date of the seat belt laws. Each observation shows whether or not the driver was less careful at the time of crash. Careless behavior includes talking on, listening to, or dialing a phone; adjusting climate control, the radio, or a CD; using other devices integral to the vehicle; sleeping, eating or drinking; smoking related distractions; and other distractions or inattention[7].

Second, I test if more non-occupants are involved in accidents as the result of less careful driving when the primary seat belt laws are in force [8]. If the less careful driving behavior does not result from the stronger seat belt laws, then we can conclude that the laws are not one of the determinants of any involvement of non-occupants in accidents.

These two may be correlated. If drivers feel secure because of the seat belt laws, they may drive less carefully by taking some careless actions, which, in turn, may result in greater probability of causing accidents involving more non-occupants. Because this recursive relationship between

[6]Some drivers cause accidents in other states rather than their states. They are travelers and commuters. However, the data set does not include information on the exact locations of crashes. There is no reason to believe that most drivers experience accidents in other states rather than their states. Thus, drivers' addresses are used.

[7]Less careful driving may appear in many different forms. Either drivers raise their travel speed above the maximum speed limit or they show specific careless behavior. They can be used as indicators showing their behavioral change. Other forms can be illustrated by sleeping, being drowsy, less focus on driving, etc. I use the definition of careless behavior from the NHTSA.

[8]Previous literature investigates whether seat belt laws reduce the number of fatal accidents. This paper only focus on behavioral change, conditional on the accidents that are occurred. The GES data contain a sample randomly selected from the population, but it includes all types of injuries, unlike the FAR data, which includes only fatal accidents. Therefore, accidental harm is measured only by injury levels.

laws, driving behavior, and the involvement of non-occupants results in simultaneity, separate estimation could cause biases, and the pooled bivariate probit model is a natural specification to employ[9]. Since this model is qualitatively different from the typical bivariate probit model, it is a recursive, simultaneous-equations model[10].

The bivariate probit model is

$$y^*_{mi} = \beta_i' x_{mi} + \gamma_i y^*_{li} + \epsilon_{mi}, \qquad y_{mi} = 1 \text{ if } y^*_{mi} > 0,$$
$$= 0 \text{ otherwise,}$$

$$E[\epsilon_m] = E[\epsilon_l] = 0, \,\forall\, m \neq l,$$
$$Var[\epsilon_m] = Var[\epsilon_l] = 1, \,\forall\, m \neq l,$$
$$Cov[\epsilon_m, \epsilon_l] = \rho, \,\forall\, m \neq l.$$

The bivariate normal cdf, $\Phi(x_1, x_2, \rho)$, is

$$Prob(X_1 < x_1, X_2 < x_2) = \int_{-\infty}^{x_2}\int_{-\infty}^{x_1} \phi_n(z_1, z_2, \rho) dz_1 dz_2.$$

, where $m = 1$ and 2.

Assume that ε_1 and ε_2 are joint normally distributed with means zero, and covariance matrix, Σ. In summary, the recursive system is:

[9] A panel data model is an ideal model for this research. However, it is not possible to construct panel data from the GES data set. The data set is basically repeated cross-sectional. Recent economic literature has developed an econometric technique (Synthetic (or Pseudo) panel data model) that produces panel data from cross-sectional data. As a sensitivity analysis, I construct a synthetic panel data model to see if its estimation results are consistent with this probit model. The estimation results are presented in section V. For more details, see Bae & Benítez-Silva (2011a).

[10] See Green (2003) and Cappellari and Jenkins (2003) for more details.

$$
\begin{aligned}
B_i &= f(L_i, X_i, X_t, X_s) \\
I_i &= f(B_i, X_i, X_t, X_s)
\end{aligned} \tag{1}
$$

where, L_i = primary seat belt law, B_i = driver's carelessness, I_i = involvement of non-occupants, X_i = exogenous independent variables, X_t and X_s = year- and state-fixed effects.

Control variables (X_i) on the right-hand side include personal characteristics, vehicle factors, road and weather conditions, and other environmental factors. Therefore, if two accidents occur on the same date in the same state, then the behavioral differences between two drivers would only be explained by these control variables (X_i). They are in both equations. Furthermore, the model includes year and state dummies to control for aggregate year and state impacts. To compare the estimation outcomes, both separate probit and bivariate probit models are presented and discussed.

The sample for the analysis contains states. Therefore, the dependent variable might be correlated within a cluster (a state), possibly through unobserved cluster effects (Wooldridge, 2002). This is true even when some control variables are included, so I use the standard errors that allow for within-state correlation, relaxing the usual requirement that the observations be independent. All the t-values are calculated using a robust variance estimate that adjusts for within-cluster correlation.

Various post-estimation issues, such as sample selection, endogeneity, identification, etc, are discussed in section V. The estimation results from a synthetic (Pseudo) panel data model are also presented as a robust test.

IV. Data and Summary Statistics

1. Data and Descriptive Statistics

I use two sources of data. The first source of data is about the seat belt laws. As of 2008, all US jurisdictions except New Hampshire adopted seat belt legislation. Many states, such as

Connecticut, New York, and Texas, adopted primary seat belt laws in the mid-1980s, while some states, such as Kansas, adopted it recently (Table 1). The coverage of seat belt use differs from state to state. Due to changes in law enforcement, seat belt use has increased consistently over time. It reached 83 percent in 2008[11]. However, many states still adopt secondary seat belt laws.

The second source of data is about accidents. It is obtained from the National Highway Traffic Safety Administration (NHTSA). The General Estimates System (GES) in the NHTSA obtains its data from a nationally representative probability sample selected from the estimated six million police-reported crashes that occur annually. This data contains the detailed description of each crash. Each crash includes information on the people and vehicles involved as well as the detailed description of the accident, including environmental factors[12].

There are several advantages of using this data. First, it contains detailed information on individual drivers' behavior, such as careless driving behavior, alcohol consumption, non-occupant involvement and other behavioral change at the time of the crash, before the crash and after the crash so that I can observe each driver's detailed behavior. Second, it contains vehicle characteristics, such as model year, age of car, and vehicle contribution factor. Previous literature shows that the vehicle age affects drivers' behavior (Crandall & Graham (1984)). For instance, I identify what model year vehicles were involved in a particular accident. Third, each observation has a zip code so that I know in which state the accident occurred at that particular time. The zip code for each observation is the main link between the seat belt law in the state and the accident. Furthermore, this data set includes environmental factors, road conditions, weather conditions, and personal characteristics. These characteristics are unique to each observation. They are used as control variables to account for individual (crash-specific) heterogeneity. For instance, suppose that two crashes occur in the same state on a same date. The characteristics explain the variations among them. By controlling for these factors, I can see the effects of primary seat belt laws on drivers' behavior. Otherwise, the coefficient of the seat belt law would be biased because of the heterogeneity affecting the behavior, even though I control for state-fixed and year-fixed effects.

[11] *Click It or Ticket* (CIOT), America's Seat Belt Campaign, NHTSA.

[12] Since the GES contains a sample from all accidents, one cannot use this data to see the effects of seat belt laws on fatalities. However, this also contains accidents with no, or minor injuries as well as fatal injuries. This enables us to see if the laws result in more or less severe accidents, conditional on the accidents that have occurred. Since the GES data contains only accidents, the reduction in accidental harm can be measured by only the reduction of the severity of injuries. The overall benefits should include "no accident", which is unobservable.

Since I focus on the states that have changed their seat belt laws since 2003, I only use accidents that occurred in these states[13]. Over the time period, more than 76,000 people were involved in crashes in these states (Table 2). Drivers consist of 78 percent of them (59,528 individuals). Since non-occupants do not affect drivers' behavioral change due to the laws, I only use the observations for drivers.

One thing to consider is whether the increase in accidental harm from the offsetting behavior is big enough to outweigh the reduction in accidental harm from the seat belt laws, even if the offsetting behavior exists. By simply taking a look at the descriptive statistics of accident data, one could find some intuitive idea on the size of accidental harm of non-occupants. The General Estimates System (GES) has 76,481 individuals who were involved in the accidents that occurred in these states over the period of study[14]. Pedestrians and cyclists consisted of only 1.72 percent of them (Table 2). Drivers and passengers consisted of 98.03 percent of the sample. Therefore, it may not be sizeable, even if the fatality of non-occupants increases because of the offsetting effects. This becomes clear when I focus on the injury severity levels of those non-occupants included in the sample. Among the 1,309 non-occupants involved in accidents over the period, only 80 people had a fatal injury (Table 3). Fifty-five percent of them actually caused the accidents due to their possible mistakes or misbehavior (Table 4). If we include non-motorist vehicle operators and other or unknown action, the percentage increases to 81.25 percent. Only 18.75 percent of them (or 15 non-occupants out of more than 76,000 people involved in accidents) had a fatal injury when they did not take any action. This percentage can be explained by drivers' mistakes or careless (or even intentionally aggressive) behavior. Even so, there is no guarantee that the seat belt laws cause this involvement.

2. Summary Statistics

Table 5 shows summary statistics for the main model. Dependent variables are dummy variables. The dependent variable in the first equation, $CARELESS$, measures whether the driver was distracted at the time of the accident. This behavior is caused by the driver, not by other

[13]In 2010, Kansas changed the law, but I do not use the information because the GES data for the year was pre-estimated one.

[14]There was no change in the laws in 2008. In 2009, 4 states changed their laws. By including these four states, we can compare drivers' behavioral differences between the states with and without the primary seat belt law over the years of study.

people or objects on the road. Therefore, it directly measures the driver's mistakes or careless behavior. If the driver in an accident is careless, then the value is 1. Otherwise, the value is zero. Careless behavior includes the driver's talking on, listening to, or dialing a phone; adjusting climate control, the radio, or a CD; eating or drinking; smoking-related distractions; using or reaching other devices integral to the vehicle; sleeping; and other distractions or inattention[15]. More than 13 percent of the drivers were careless when accidents occurred. The dependent variable in the second equation, $NON\_OCCUPANTS$, measures the involvement of non-occupants, such as pedestrians and bikers, in the accident. If any non-occupant is involved in the accident, then the value is one. Otherwise, the value is zero. About 5.3 percent of the drivers experienced non-occupant related accidents.

Independent variables include three main factors: individual accident-level, state-fixed, and year-fixed factors along with the seat belt law[16]. The main variable, $PRIMARY$, is an indicator. If an accident occurs before the enforcement date in the state, then the value is zero. Otherwise, it is one. More than 45 percent of accidents occurred when the primary seat belt laws were in force. To see drivers' lagged adjustment, I define another variable. The variable, $PRIMARY3$, has the value of zero if the accident occurs before the enforcement date or within the first 3 months since the adoption of the new law. About 3.2 percent of drivers had accidents within the periods, so the variable, $PRIMARY$, has the value of 1 for these observations, but it is 0 in $PRIMARY3$. The variable, $PRIMARY6$, allows three more months.

The drivers' average age is 38 and 61 percent of the drivers in the sample are males. $ALCOHOL$ measures whether alcohol is involved in the accident at the time of the crash. If alcohol is involved, then the value is 1, otherwise it is 0. Slightly more than 10 percent of accidents were related to alcohol. $NIGHT$ measures when the accident occurs. If the accident occurs between 7:00 p.m. and 2:00 a.m., then the value is one. Otherwise, the value is zero. About 21 percent of the accidents occurred at night time. The variable is correlated with alcohol consumption, which may cause careless behavior. $HIGH\_POP$ is a dummy variable that indicates the density of population. If the accident occurs in the area with 100,000 residents or more, then

[15]Some drivers were distracted by other occupants or outside person or object at the time of crashes. However, this type of distraction could have been avoided if they had been attentive. Notice that this is self-reported by the driver, or occupants, and in some cases by witnesses to the accidents

[16]The definition and the description of each variable is in the appendix.

it is one. Otherwise, it is zero [17]. I expect that drivers are more careful in driving in highly populated areas.

Many variables that reflect road and weather conditions are included. If the road surface is dry, then $DRY\_SURFACE$ is one. If it is wet, snowy, or icy, then it is zero. The variable measures road conditions. About 80 percent of drivers had accidents under good surface conditions. $GOOD\_WEATHER$ measures if it is rainy, snowy, sleety, and foggy. If there is no adverse condition, then the value is one. $LIGHT$ measures visual conditions. Sixty-five percent of accidents occurred during the daylight. I also include the vintage variable, $VEHICLE\_AGE$, to measure drivers' behavioral differences, depending on how old their vehicles are. The average vehicle age is 7.2 years. To account for a possible non-linear relationship, I also include $VEHICLE\_AGE\_SQ$. If the accident occurs on the highway, then the variable, $HIGHWAY$, has the value of one. Most accidents occurred on local roads. If the accident occurs in an interchange area, then the value of $INTERCHANGE$ is one. Otherwise, it is zero. Less than 3 percent of the accidents occurred in interchange areas. The variable, $SPEED\_LIMIT$, measures the maximum speed limit at the place of the accident. Since there are different maximum speed limits even within a state, this information helps determine accident-specific variations. The average speed limit is 41 mph[18].

State and year dummies are included in the equations to control for state- and year-fixed factors. The states that changed their laws are: Alaska, Arkansas, Delaware, Florida, Illinois, Kentucky, Maine, Minnesota, Mississippi, North Carolina, South Carolina, Tennessee, and Wisconsin. Florida has the most accidents over the time period. North Carolina and Tennessee have the second and the third most accident, respectively. Alaska has the fewest accidents among the states[19].

[17] The reason that the dummy variable is used is because of data limitations. Furthermore, since each observation has the zip code that the owner of the vehicle resides, the owner's address and the place the accident occurred will be different. Thus, county-level population cannot be used in this case. To control for regional population density, the dummy variable would be fine.

[18] Some accidents, not many, occurred in places where the maximum speed limits were zeros. These places do not have any statutory limit because they are parking lots, alley, or etc.

[19] The GES data is a sampled data from the police accident reports. Therefore, each selected observation has its own weight to be used to get the national estimate. The frequencies in the sample do not have any meaning if the weight is not taken into account. However, it suffices to use the raw data to see the behavioral differences among drivers because I do calculate neither the number of accidents nor fatality rates.

V. Empirical Results

1. Main Estimation Results

The main estimation results are presented in Table 6. The variance-covariance matrix of the cross-equation error terms is estimated and the null hypothesis that $\rho_{12} = 0$ is tested with a Wald test at the 5 percent level. The Wald test shows that there is a correlation between the error terms ($H_o : \rho = 0$, $\chi^2(1) = 12.0355$). Thus, if two equations were estimated separately, then the estimated coefficients would be inefficient. Therefore, the model shows that the involvement of non-occupants is linked to the primary seat belt laws through the careless behavior of drivers. All estimation results are obtained using robust standard errors that adjust for within-cluster correlation. To compare estimation coefficients, I present both separate probit and bivariate probit models.

I focus on the bivariate probit model in the third column, while I present the first two models just for comparison. The first equation tests if the primary seat belt laws induce drivers to drive less carefully, while the second equation tests if careless driving behavior causes more accidents involving non-occupants.

The estimation results show that drivers drive more carefully when the primary seat belt law is in force in a state. Careless behavior is negatively associated with $PRIMARY$ and it is statistically significant at the 5 percent level. This is the opposite result of the offsetting effect theory. Primary seat belt law and seat belt usage are highly correlated (NHTSA). Therefore, more drivers wear their seat belts in the states where stronger law enforcement is in effect. Using their seat belts may remind drivers to be more careful. We do not know whether drivers feel more secure by wearing their seat belts. They drive more carefully under the primary seat belt law. The adoption of the primary seat belt law may warn drivers to be alert for accidents in their state. Considering that the sample contains only accidents reported to the police, drivers are more careful because of the law, even though they are involved in accidents. In that sense, the laws must have played not only a passive protective role but also an active preventive role. Given that accidents occurred, the primary seat belt laws may reduce the severity of injuries that otherwise would have been more severe because of their less careful driving behavior[20].

[20] I cannot directly compare the severity levels among the accidents that are reported to the police and

Among other control variables, careless behavior is caused by neither age nor gender[21]. $ALCOHOL$ is not associated with $CARELESS$, so alcohol involvement itself does not result in careless behavior[22]. Careless behavior is found in the accidents that occurred between 7:00 p.m. and 2:00 a.m. Drivers are more careful when the areas are highly populated. During the daylight, drivers are less careful. When it is dark, dawn or dusk, people drive more carefully. The vehicle age does not affect a driver's behavior. Therefore, I can conclude that there is no vintage effect. $HIGHWAY$ is negatively associated with careless behavior. When drivers drive on highways, they are more careful. A higher speed limit makes drivers less careful. This is a seemingly counterintuitive outcome. Drivers are possibly more careful on the local roads because of frequent obstacles, such as pedestrians. Drivers may focus on driving when the roads have lower maximum speed limits. The coefficients of year dummies are not statistically significant. Most state dummies are statistically significant. The estimation outcome from Probit model 1 in the first column is not quite different from the bivariate probit model in the third column.

The second equation in the third column shows that more careful driving, affected by the primary seat belt laws, reduces the probability of non-occupant involvement. The coefficient of $CARELESS$ is statistically significant at the 5 percent significance level. Thus, in general when drivers are less careful in driving, more pedestrians are involved in accidents. However, the seat belt laws make drivers more careful, and thus, there are fewer accidents involving non-occupants. That means that the offsetting effects do not appear here, so I draw a conclusion: The offsetting effects do not exist when the police accident report is used for the analysis. The law actually reduces non-occupant involvement. The coefficient of $CARELESS$ in the second column is not statistically significant at any level, so if the second equation is independently estimated, then the result shows that careless driving behavior is not associated with the involvement of non-occupants. This is misleading because the model disregards the recursive structure of the model. There are no substantial differences between the second and the third models, except

that are not because of the nature of data.

[21]Most literature show that young male drivers cause more fatal accidents. My study uses individual-level accident data. It includes the accidents with all injury levels and property damages. Therefore, based on the police-reported accidents, the estimation results show that there is no difference in careless behavior among male and female drivers as well as young and old drivers.

[22]This result seems odd. However, it is because of the definition of the variable, $ALCOHOL$. It measures whether alcohol is involved in an accident. Therefore, it is different from the driver's actual drinking. $ALCOHOL$ measures more likelihood of the accident, while the driver's drinking behavior is reflected in $CARELESS$.

CARELESS.

The personal characteristics affect the involvement of non-occupants. Both older and male drivers cause more accidents involving non-occupants. Alcohol-related accidents involve more pedestrians. $NIGHT$ is not associated with pedestrian involvement. This makes sense because not many pedestrians are on the roads during the night. More pedestrians are involved in accidents in highly populated areas. $VINTAGE$ is irrelevant to the dependent variable again. It is also natural to observe that $HIGHWAY$ is not statistically associated with $NON\_OCCUPANT$ since pedestrians are not on the highway. The coefficient, $INTERCHANGE$, is negative and it is statistically significant at the 10 percent level. That means fewer pedestrians are involved in the accidents occurred in the interchange areas. This is because most drivers are more careful when they approach the interchange areas. A higher speed limit is associated with less pedestrian involvement and it is statistically significant at the 5 percent level. This implies that most accidents involving pedestrians occur on the roads with lower maximum speed limits, such as local roads. All the year-dummies are not statistically significant, while most state-dummies are statistically significant at either the 1 percent or the 5 percent significance level. The primary seat belt laws reduce the accidents involving non-occupants by affecting drivers' driving behavior[23].

2. Sample Selection Bias and Endogeneity

In this sub-section, I discuss and answer two potential problems. First, there could be a sample selection bias in my model. Since the sample contains only the accidents that are reported to the police, some accidents with minor or no injury or property damage only are not in the sample. This might cause a sample selection problem. Assuming that the drivers who report to the police are, on average, more likely to be less careful, could it be that accidents are more likely when they aren't wearing their seat belts? Maybe (because of offsetting behavior), the accidents are more likely. I am not able to control for selection, using the traditional way (following Heckman (1979)), because I do not have information on those who do not report accidents. Therefore, to see if this sample selection problem, if any, alters my estimation results, I use an alternative method. I estimate the model using sub-samples. The GES data has information on vehicle role

[23]The marginal effects for a conditional mean function in the bivariate probit model are not reported in this paper. It is available to the author upon request.

in accidents. Many accidents involve multi-vehicle crashes. Each driver (each observation) is in either a striking or non-striking vehicle. Striking vehicles may be associated with drivers' careless behavior. However, struck vehicles are irrelevant to the careless behavior. Therefore, the drivers whose vehicles are struck are not necessarily associated with careless behavior. They would not have been reported to the police if their vehicles had not been involved in accidents.

Therefore, I estimate the model using only these observations. That way, the estimation results can ameliorate the sample selection bias concerns, if the sample includes only struck vehicles[24]. The estimation results will show both samples to compare. If the estimation results were quite different among two specifications, then this would be the indirect evidence of serious selection bias.

Table 7 shows the estimation results to detect any possible sample selection problem. I divide the sample into two sub-samples: Striking vehicle group vs. Non-striking vehicle group. We may consider the striking vehicle group relatively less careful (more risky) than the non-striking group. As the results show, the primary seat belt laws make drivers more careful. There is no big difference between the two sub-samples. The variable, $DRY\_SURFACE$ is positively correlated with $CARELESS$ and is statistically significant at the 1 percent level in the non-striking vehicle group. However, it is not significant in the striking vehicle group. This makes sense because drivers in struck vehicles are less careful when the road condition is good. Other control variables show similar outcomes. The only difference is in state dummies. In some states, drivers show opposite behavior, depending on their vehicle role.

The second equation shows that drivers' careful driving results in less pedestrian involvement and there is no difference between the two groups. Both sub-samples should be estimated by the bivariate probit model. Some control variables that were not statistically significant in Table 6 are now significant. The variable, AGE, shows different behavioral patterns in the two groups. Older drivers in the striking vehicle group experience more accidents involving non-occupants, while younger drivers in the non-striking vehicle group experience more accidents involving themselves. $ALCOHOL$ is positively associated with $NON\_OCCUPANT$ only in the striking vehicle

[24]The information on the vehicle role in the police reports does not identify who caused the accidents. Struck vehicles may cause the accidents. However, striking vehicles may cause more accidents on average. In this sense, this method may not be an ideal way to solve the selection bias problem, but an alternative way to ameliorate it.

group. Drivers in struck vehicles are not related to alcohol involvement. The weather does not affect the involvement of non-occupants in the striking vehicle group. However, when the weather is good, fewer non-occupants are involved in crashes in the non-striking vehicle group. $VINTAGE$ affects the dependent variable in the non-striking vehicle group, while $INTERCHANGE$ affects it in the striking vehicle group. Fewer pedestrians are involved in accidents when the road has a lower speed limit, regardless the vehicle role.

In conclusion, the estimation results do not alter the main estimation results. Furthermore, the selection bias does not seem serious because both sub-samples show similar outcomes. Some control variables explain drivers' behavioral differences in the sub-samples. Still, the primary seat belt laws reduce the accidents involving non-occupants regardless of the drivers with different risk preference.

The second question that I need to answer in this section is why a state changed the law during a particular year. The state might have changed the law because of the increase in the fatality rate over time. If so, then the increase in the fatality rates in previous years may be the source of more careful driving behavior. If this is true, then a possible endogeneity problem may arise. As part of the identification strategy, I need to prove that more or less careful driving behavior should not be caused by the consistent increase or decrease in the fatality rate, but caused by the seat belt laws. If a primary seat belt law in a state were adopted at one point in time because of an increase in fatality rates in previous years, then the non-occupant involvement would be explained by the fatality rates, not by the law itself. As we can see from Table 8, there is no clear trend in fatality rates. For instance, the state of Mississippi experiences a constant decrease in the fatality rates until 2006, when the law began being enforced. Then, the fatality rate reaches the lowest point in 2008. Therefore, the fatality rate is not the source of the law enforcement, but the result of it.

3. Lagged Effects

Another issue is how quickly drivers adjust their driving behavior to the change in the law, if they do at all. If the laws do not have an immediate impact on drivers' behavior, the estimation results may be plagued by measurement error, so I re-estimate the model using another variable for L_i. I consider the accidents that occur within three months (or six months) after the enforcement dates as pre-accidents. During the first three or six months, some drivers may not know whether their states have changed the laws. Alternatively, it takes time for people to adjust their driving pattern (or behavior). With the modification of the variable, the model can explain possible "recursive" correlation between laws, driving behavior, and the resulting involvement of non-occupants in accidents.

To test if there is a lagged effect, I estimate the model using $PRIMARY3$ and $PRIMARY6$. These variables reflect drivers' delayed behavioral change due to the change in the law enforcement. The estimation results are shown in Table 9[25]. The first model uses $PRIMARY3$, while the second model uses $PRIMARY6$. The first model shows almost the same estimation results with Table 6. Both the coefficients, $PRIMARY3$ and $CARELESS$, are statistically significant at the 1 percent significance level. From this result, I can conclude that drivers' behavioral change may not be immediate. The estimation results still remain unchanged.

Now, the second model uses $PRIMARY6$. The coefficient of $CARELESS$ in the second equation is now statistically insignificant at any significance level. Furthermore, the Wald test cannot reject the null of lack of correlation between the equations. From this observation, the effect of the primary seat belt laws on non-occupant involvement is immediate and show some delayed effects too. However, the effects do not last long and fade out over time.

4. Sensitivity Analysis

Drivers' behavioral change may come from other unobservable factors rather than the adoption of the primary seat belt laws. Suppose that two accidents occur on a particular date in a state. Two drivers' behavioral differences must be captured by other individual factors, such as their income levels, education levels, attitudes on risk, and others. This identification issue can be resolved by conducting a sensitivity analysis[26].

For the sensitivity analysis, I observe each state and choose a neighboring state where the primary seat belt law had been enforced before 2003, or where there is no primary law until now[27]. Then, I assign each accident to either a pre- or post-accident as if the state changed the

[25] The year dummies are not shown in the table.

[26] I conduct this sensitivity analysis because the GES data do not have information on each driver's personal characteristics, such as income, education, and etc, except gender and age.

[27] It doesn't matter whether or not the neighboring state has the primary seat belt law. There will be no behavioral change since there is no change in the law.

seat belt law on the enforcement date of the neighboring state. Then, I estimate the model using the bivariate probit model. If this estimation outcome were similar to the main results, then it would be evidence that other unobservable factors rather than the seat belt laws affected the behavioral change as well as the involvement of non-occupants.

Table 10 shows the estimation results using only neighboring states. The error terms are not correlated. Therefore, there is no recursive structure in this model. Two equations should be separately estimated. The first model shows that the primary seat belt laws induce drivers' careless driving behavior, which is an opposite result from Table 6. It is statistically significant but at the marginal level. Then, we can conclude that the offsetting effects appear in the states where there is no change in the seat belt laws. How can we explain this seemingly odd result? In fact, the variable, $PRIMARY$, in this section represents pre- and post-primary seat belt periods in the neighboring states where new laws are adopted. Therefore, the drivers in the states with no legal change may be affected by the states with the legal change. Knowing that the neighboring state changes the law on a particular date, the drivers whose state does not change its law may tend to drive less carefully[28].

According to a report from the US Department of Tranportation, accidents related to careless driving behavior have increased between 2005 and 2009. The report says, "The proportion of fatalities reportedly associated with driver distraction increased from 10 percent in 2005 to 16 percent in 2009. During that time, fatal crashes with reported driver distraction also increased from 10 percent to 16 percent."(US DOT Report, 2011). This implies that, in general, drivers have driven less carefully over time. This is probably because of the improved safety quality of vehicles, and better road conditions over time. Thus, unless there is more stringent safety regulation, drivers may tend to drive less carefully. This is reflected in the coefficient of the variable, $PRIMARY$. It is positively associated with the seat belt laws. This is because the model includes only the states where there is no "actual" change in the law[29].

[28]Interstate commuters may change their driving behavior once they cross the border of the states. This is not empirically proved. However, this is plausible if we consider a similar situation. With regard to maximum speed limits, drivers reduce their travel speed as they pass from the roads with higher to those with lower maximum speed limits.

[29]This implies that the pooled cross-sectional probit model does not effectively control for unobserved factors that change over time and affect the dependent variable. This gives a rationale for the use of the panel data model. However, there is no panel data on individual accidents. In the next section, I present a synthetic panel data model.

In the second equation, the careless behavior is not associated with the involvement of non-occupants in accidents. Therefore, the involvement of non-occupants in accidents must be explained by other factors rather than the primary seat belt laws. This is true because these neighboring states did not change their laws over the period of study.

The estimation results in Tables 6 and 10 are different. When there is no change in the seat belt law, there is no recursive structure. Thus, I can conclude that the primary seat belt law is one of the main determinants for the involvement of non-occupants via the change in drivers' driving behavior.

5. Synthetic Panel Data Model

The bivariate probit model is used as a main model for this research and it is basically a pooled cross-sectional model. One reason for using independently pooled cross sections is to increase the sample size and thus get more precise estimators (Wooldridge, 2002). I use it, not for that reason, but because of the change in the seat belt laws that occurs at different points in time. The ideal estimation strategy is to use panel data. However, it is not possible to have such data at the individual accident level as I explained in the earlier section. As an alternative, I introduce an appropriate econometric technique, a synthetic panel data model[30].

The synthetic panel model groups drivers into several cohorts according to their personal characteristics and observes their behavioral changes over time as well as across the cohorts. I use gender, states, and four age groups to construct cohorts. Each cohort contains drivers whose characteristics are similar. For instance, one cohort contains drivers who are all males between 16 and 25 years old who all live in the same state. Then, I observe this group's behavior over time. I also calculate the ratio of careless drivers to all drivers in the cohort. The ratio is used for the variable, $CARELESS$. This variable is not an indicator, but a continuous variable that shows a rate between zero and one. The higher the ratio is, the less careful the drivers on average in the cohort. From this grouping, 88 different cohorts are created[31]. Since there are five years, the

[30]This econometric technique has recently developed and carefully used in the limited settings, depending on the nature of studies. It is essentially impossible to observe an individual driver's behavior and his or her response to seat belt laws over time. Thus, the technique groups drivers whose characteristics are similar into a type. We then track the drivers' behavior over time through these types. So, each type behaves as if it were an individual. For more details, see Deaton (1985), Deaton & Irish (1985), Verbeek & Nijman (1992), and, recently, Bae & Benítez-Silva (2011a)

[31]Each state has 8 different types. Eleven states are included in this model. Two states, Alaska and

total number of observation is therefore 440. These observations are used for this synthetic panel data model[32].

The econometric equation is

$$CARELESS_{it} = X_{it}\beta + \gamma PRIMARY_{it} + c_i + \varepsilon_{it}, \qquad i = 1, ..., 88 \quad \text{and} \quad t = 2003, ..., 2007 \tag{2}$$

where, i is a type and t is a year. X_{it} includes independent variables that are used to group drivers. The summary statistics are presented in Table 11. Table 11-a uses the panel data from the states that change the laws, while Table 11-b uses it from their neighboring states that do not change them. I call the former primary data and the latter non-primary data. From the primary data, the average of $CARELESS$ is 18 percent. Thus, on average, there are about 1.8 careless drivers out of 10 drivers in each cohort. Between the primary and non-primary data, there is no difference in drivers' careless behavior (.1800 vs. .1853.). The main independent variable, $PRIMARY$, is a dummy variable. Therefore, if all drivers in a group have accidents before the seat belt law is adopted at a particular year, then the value is zero. Otherwise, it is one. Slightly less than 30 percent of the observations have the value of zero. All other independent variables are dummy variables because they are used to construct the panel data.

Both estimation results are presented to compare. If the offsetting effects existed, then the coefficient of the variable, $PRIMARY$, would be positive and statistically significant only in the estimation model that uses the primary data (Table 11-a).

The estimation results are presented in Table 12[33]. The random effect models are used.

Maine, are dropped because of econometric issue. Both states have very few accidents in the original data set. If I grouped them into several cohorts, then the number of observations each cohort would be very small. The synthetic panel data model results in measurement errors if the size of each cohort is too small.

[32]There is a trade-off between the pooled probit and the panel models in terms of econometric benefits. To ameliorate individual unobserved heterogeneity that changes over time, the panel model is more desired. However, there is no panel data. The only way is to construct the synthetic panel model, which may result in another problem. Not all the independent variables used in the bivariate probit model can be used. This, we can't control for these within the model. Only the variables that are used to make cohorts should be used. If other variables were used, by calculating their group averages, then the measurement error problem would arise.

[33]Since these results are used only as a robust test for the main model, it is enough to show the relationship between primary seat belt laws and careless driving behavior.

The first synthetic panel model shows the estimation results from the states where their laws have been changed. The second model shows the estimation results from the neighboring states. The first model shows that drivers in a state drive more carefully when a primary seat belt law is in effect in the state. The coefficient of the variable, $PRIMARY$, is highly significant at the 1 percent level. This result is consistent with the main estimation result from the bivariate probit model(Table 6). When a state adopts a more stringent seat belt law, the ratio of careless drivers decreases by 19 percent. The fit of this model is fairly good, with an R^2 close to 40 percent. The second synthetic panel model shows that the coefficient is not statistically significant at any level. This is for the same reason: The neighboring states have not changed the laws over the periods of study.

Individual characteristics are not statistically significant. This result is also consistent with the estimation results from the pooled probit model. The only difference is year dummies. They are not statistically significant in the probit model, but they are significant in the panel model. This implies that the bivariate probit model fails to control for unobserved factors that change over time and affect the dependent variable. As long as pooled cross-sectional data is used, this is inevitable. The main disadvantage of the synthetic panel model, however, is that many environmental factors that are used in the probit model are not controlled for anymore. Thus, both models (the bivariate probit and synthetic panel models) have their own pros and cons. However, the bivariate probit model in this paper is estimated using the robust variance estimate that adjusts for within-cluster correlation. Therefore, even though the model does not control for unobserved factors well, there is a consistent result from both models.

VI. Concluding Remarks

This paper investigates the effects of the primary seat belt laws on driver behavior and the involvement of non-occupants. I find that the offsetting effects do not exist when I analyze the accidents using all injury levels. Primary seat belt laws rather reduce the predicted probability of less careful driving behavior. The behavior does not even lead to greater involvement of non-occupants. Therefore, the overall effect of the laws is still effective, assuming that the law reduces the fatality of drivers and passengers[34].

The sensitivity analysis shows that the primary seat belt law is one of the main determinants for the involvement of non-occupants via the change in drivers' driving behavior. Such behavioral pattern has not been observed in the neighboring states. Therefore, the coefficient of the variable, $PRIMARY$, is not mixed with other unobservable factors. Regarding the possible lagged behavior, the effect of the primary seat belt laws on the non-occupant involvement is immediate and shows some delayed effects. The estimation results from the synthetic panel model also show consistent outcomes. Both bivariate probit and the synthetic panel data models show that drivers are more careful because of the stringent law enforcement. I can also conclude that there is no presence of the offsetting effects from the seat belt laws.

Currently, 31 states and the District of Columbia adopt the primary seat belt law. Nineteen states still have secondary laws, and New Hampshire has no seat belt law. Some people argue that drivers should choose to wear seat belts as a matter of "personal freedom."[35] However, the primary seat belt laws save the lives of drivers as well as passengers, pedestrians, and bikers. This result, combined with earlier studies, shows that the primary seat belt laws play an important role in improving public safety on the U.S. roads[36].

It is still true that the laws save drivers' lives. As of Jan. 1, 2010, a new state law in Georgia, 'Super Speeder Law', went into effect with substantially higher fines, \$ 200[37]. This law may give drivers stronger incentives to drive more carefully and strengthen the effects of primary seat belt laws. Therefore, a punitive penalty, such as higher fines, would make the laws much more effective, if used together.

For future studies, we may test if joint regulation were more effective in promoting public safety. We can perform this test by comparing different states with and without punitive (monetary) penalty, given that the states have the same level safety enforcement.

[34] A research note (2006) from the NHTSA found that states with primary enforcement laws have lower fatality rates. According to the note, the passenger vehicle occupant fatality rates were 1.03 per 100 million vehicle miles traveled (VMT) and 10.69 per 100,000 population over the period of study. This compares to 1.21 and 13.13 (respectively) for all other states.

[35] For instance, the National Motorists Association(NMA) submitted testimony against a 2003 Wisconsin bill allowing primary enforcement. Seven years later, the state of Wisconsin eventually passed a primary seat belt law in 2009. See more details from "http://www.motorists.org/seat-belt-laws/testimony".

[36] Not only the seat belt laws improve public safety. Vehicle recall regulation reduces accidental harm. See Bae & Benítez-Silva (2011a and 2011b) for more details.

[37] See "http://www.safespeedsgeorgia.org/".

Another possible study can augment my paper. The use of cellular phones has been prevalent in recent years in the U.S. Some states are beginning to prohibit drivers from using the cellular phones to call or send text messages, while driving on highways. Cellular phone usage could be a major distraction of careless driving behavior. My current paper does not incorporate this into the model. Therefore, we could test if there is a relationship between primary seat belt laws, laws banning cellular phones, and their joint impacts on road safety.

Table 1. Primary Seat Belt Laws - States

State	Initial Effective Date	Primary Seatbelt Laws?	Standard Enforcement Date	Who is Covered? In What Seats?	Maximum Fine 1st Offense
Alabama	07/18/91	Yes	12/09/99	15+ years in front seat	$ 25
Alaska	09/12/90	Yes	05/01/06	16+ years in all seats	$ 15
Arizona	01/01/91	No		15+ in front seat; 5 through 15 in all seats	$ 10
Arkansas	07/15/91	Yes	06/03/09	15+ years in front seat	$ 25
California	01/01/86	Yes	01/01/93	16+ years in all seats	$ 20
Colorado	07/01/87	No		16+ years in front seat	$ 15
Connecticut	01/01/86	Yes	01/01/86	7+ years in front seat	$ 15
Delaware	01/01/92	Yes	06/30/03	16+ years in all seats	$ 25
DC	12/12/85	Yes	10/01/97	16+ in all seats	$ 50
Florida	07/01/86	Yes	06/30/09	6+ in front seat; 6 through 17 in all seats	$ 30
Georgia	09/01/88	Yes	07/01/96	18+ in front seat; 6 through 17 in all seats	$ 15
Hawaii	12/16/85	Yes	12/16/85	18+ in front seat; 8 through 17 in all seats	$ 45
Idaho	07/01/86	No		7+ years in all seats	$ 10
Illinois	01/01/88	Yes	07/03/03	16+ in front seat; 18 and younger in all seats if driver is younger than 18 years	$ 25
Indiana	07/01/87	Yes	07/01/98	16+ years in all seats	$ 25
Iowa	07/01/86	Yes	07/01/86	11+ years in front seat	$ 25
Kansas	07/01/86	Yes	06/10/10	18+ in front seat; 14 through 17 in all seats	$ 30
Kentucky	07/15/94	Yes	07/20/06	7+ years in all seats; 6 and younger and more than 50 inches in all seats	$ 25
Louisiana	07/01/86	Yes	01/01/95	13+ years in front seat	$ 25
Maine	12/26/95	Yes	09/20/07	18+ years in all seats	$ 50
Maryland	07/01/86	Yes	10/01/97	16+ years in front seat	$ 25
Massachusetts	02/01/94	No		13+ years in all seats	$ 25
Michigan	07/01/85	Yes	04/01/00	16+ years in front seat	$ 25
Minnesota	08/01/86	Yes	06/09/09	all in front seat; 3 through 10 in all seats	$ 25
Mississippi	07/01/94	Yes	05/27/06	7+ years in front seat	$ 25
Missouri	09/28/85	No		16+ years in front seat	$ 10
Montana	10/01/87	No		6+ years in all seats	$ 20
Nebraska	01/01/93	No		18+ years in front seat	$ 25
Nevada	07/01/87	No		6+ years in all seats	$ 25
New Hampshire	n/a	No law		No law	No law
New Jersey	03/01/85	Yes	05/01/00	18+ in front seat; 8 through 17 in all seats;	$ 20
New Jersey				7 and younger and more than 80 pounds	
New Mexico	01/01/86	Yes	01/01/86	18+ years in all seats	$ 25
New York	12/01/84	Yes	12/01/84	16+ years in front seat	$ 50
North Carolina	10/01/85	Yes	12/01/06	16+ years in all seats	$ 25
North Dakota	07/14/94	No		18+ years in front seat	$ 20
Ohio	05/06/86	No		15+ in front seat; 8 through 14 in all seats	$ 30
Oklahoma	02/01/87	Yes	11/01/97	13+ years in front seat	$ 20
Oregon	12/07/90	Yes	12/07/90	16+ years in all seats	$ 90
Pennsylvania	11/23/87	No		18+ in front seat; 8 through 17 in all seats	$ 10
Rhode Island	06/18/91	No		18+ years in all seats	$ 75
South Carolina	07/01/89	Yes	12/09/05	6+ in front seat; 6+ in rear seat with shoulder belt	$ 25
South Dakota	01/01/95	No		18+ years in front seat	$ 20
Tennessee	04/21/86	Yes	07/01/04	16+ years in front seat	$ 50
Texas	09/01/85	Yes	09/01/85	17+ in front seat; 5 through 16 in all seats; 4 and younger and 36 in or more	$ 200
Utah	04/28/86	No		16+ years in all seats	$ 45
Vermont	01/01/94	No		16+ years in front seat	$ 25
Virginia	01/01/88	No		16+ years in all seats	$ 25
Washington	06/11/86	Yes	07/01/02	16+ years in all seats	$ 124
West Virginia	09/01/93	No		8+ in front seat; 8 through 17 in all seats;	$ 25
Wisconsin	12/01/87	Yes	06/30/09	8+ years in all seats	$ 10
Wyoming	06/08/89	No		9+ years in all seats	$ 25

* Source: Insurance Institute for Highway Safety (IIHS), “http://www.iihs.org/laws/SafetyBeltUse.aspx#OR.”

Table 2. People Involved in Accidents in 13 States over the 5 Years

Person Type	Freq	Percent
Driver of a Motor Vehicle in Transport	59528	77.83
Passenger of a Motor Vehicle in Transport	15446	20.20
Occupant of of a Motor Vehicle Not in Transport	162	0.21
Occupant of of a Non-Motor Vehicle Transport Device	10	0.01
Pedestrian	846	1.11
Cyclist (Pedalcyclist)	463	0.61
Person in or on a Working Vehicle	10	0.01
Other or Unknown	16	0.02
Total	76481	100.00

Note : The data set comes from the GES.

Since the GES data are from a probability sample of police-reported traffic crashes, national estimates can be made from these data. Refer to "NASS GES Analytical Users Manual, 1988 - 2007" regarding the methodology for this.

Table 3. Injury Severity of Individuals

	All individuals		Pedestrians & Cyclists	
Severity level	Frequency	Percent	Frequency	Percent †
No injury	45622	59.65	13	0.99
Possible injury	10797	14.12	55	4.20
Non-incapacitating injury	10532	13.77	793	60.58
Incapacitating injury	6233	8.15	358	27.35
Fatal injury	772	1.00	80	6.11
Injured, Severity Unknown	190	0.25	8	0.61
Died Prior to Crash	4	0.00	0	0.00
Unknown if Injured	2331	3.05	2	0.15
Total	76481	100.00	1309	100.00

† All individuals involved.

Table 4. Non-Occupant Action with Fatal Injury

Non-Occupant Action	Pedesrtians & Cyclists	
	Frequency	Percent
No Action	15	18.75
Non-motorist vehicle operator	7	8.75
Darting or running into road	13	16.25
Improper Crossing of roadway or intersection (Jaywalking)	16	20.00
Jogging	2	2.50
Walking with or against traffic	6	7.50
Playing, working, sitting, lying, standing, etc in roadway	7	8.75
Other or unknown action	14	17.50
Total	80	100.00

† All individuals involved.

Table 5. Summary Statistics

Variable	Obs	Mean	SD	Min	Max	Acronym
Dependent variables:						
Careless Action	59528	.1343	.3410	0	1	*CARELESS*
Non-Occupants	59528	.0533	.2247	0	1	*NON_OCCUPANT*
Independent variables:						
Primary Seat Belt Law	59528	.4549	.4980	0	1	*PRIMARY*
Primary Seat Belt Law (3 months)	59528	.4225	.4940	0	1	*PRIMARY3*
Primary Seat Belt Law (6 months)	59528			0	1	*PRIMARY6*
Age	59528	38.4737	16.3493	16	100	*AGE*
Sex	59528	.6133	.4870	0	1	*MALE*
Alcohol Consumption	59528	.1068	.3089	0	1	*ALCOHOL*
Hour of Crash	59528	.2062	.4046	0	1	*NIGHT*
Population Density	59528	.3611	.4803	0	1	*HIGH_POP*
Road Condition	59528	.8017	.3987	0	1	*DRY_SURFACE*
Weather Condition	59528	.8595	.3475	0	1	*GOOD_WEATHER*
Light Condition	59528	.6492	.4772	0	1	*LIGHT*
Vintage	59528	7.1995	5.2277	0	73	*VEHICLE_AGE*
Vintage Square	59528	79.1610	115.5438	0	5329	*VEHICLE_AGE_SQ*
Inter-State Highway	59528	.0923	.2895	0	1	*HIGHWAY*
Relation to Junction	59528	.0287	.1671	0	1	*INTERCHANGE*
Maximum Speed Limit	59528	40.9272	12.3881	0	75	*SPEED_LIM*
Year Dummy 2003	59528	.1817	.3856	0	1	*YEAR_2003*
Year Dummy 2004	59528	.1701	.3757	0	1	*YEAR_2004*
Year Dummy 2005	59528	.1187	.3235	0	1	*YEAR_2005*
Year Dummy 2006	59528	.1921	.3940	0	1	*YEAR_2006*
Year Dummy 2007	59528	.3374	.4728	0	1	*YEAR_2007*
State Dummy Alaska	59528	.0003	.0179	0	1	*ALASKA*
State Dummy Arkansas	59528	.0032	.0568	0	1	*ARKANSAS*
State Dummy Delaware	59528	.0032	.0561	0	1	*DELARWARE*
State Dummy Florida	59528	.2481	.4319	0	1	*FLORIDA*
State Dummy Illinois	59528	.1601	.3667	0	1	*ILLINOIS*
State Dummy Kentucky	59528	.0670	.2500	0	1	*KENTUCKY*
State Dummy Maine	59528	.0005	.0217	0	1	*MAINE*
State Dummy Minnesota	59528	.0058	.0758	0	1	*MINNESOTA*
State Dummy Mississippi	59528	.0086	.0921	0	1	*MISSISSIPPI*
State Dummy North Carolina	59528	.1978	.3983	0	1	*NORTH_CAROLINA*
State Dummy South Carolina	59528	.0067	.0818	0	1	*SOUTH_CAROLINA*
State Dummy Tennessee	59528	.1776	.3822	0	1	*TENNESSEE*
State Dummy Wisconsin	59528	.1212	.3263	0	1	*WISCONSIN*

Table 6. Primary Seat Belt Law and Offsetting Effects

	Probit Model 1	Probit Model 2	Bivariate Probit Model
	CARELESS		**CARELESS**
PRIMARY	-.4668 (.1804)***		-.4673 (.1846)**
AGE	-.0006 (.0006)		-.0006 (.0006)
MALE	.0230 (.0147)		.0234 (.0149)
ALCOHOL	.1602 (.1731)		.1644 (.1742)
NIGHT	.2340 (.0929)**		.2458 (.0921)***
HIGH_POP	-.1700 (.0468)***		-.1712 (.0479)***
DRY_SURFACE	-.2057 (.2192)		-.2060 (.2170)
GOOD_WEATHER	.2714 (.1894)		.2683 (.1831)
LIGHT	.1452 (.0791)*		.1520 (.0837)*
VEHICLE_AGE	-.0037 (.0102)		-.0037 (.0099)
VEHICLE_AGE_SQ	.0003 (.0003)		.0003 (.0003)
HIGHWAY	-.2974 (.1247)**		-.3057 (.1284)**
INTERCHANGE	.0902 (.0908)		.0907 (.0931)
SPEED_LIMIT	.0109 (.0035)***		.0112 (.0037)***
YEAR_2003	-.0839 (.1635)		-.0877 (.1666)
YEAR_2004	.4056 (.2621)		.4042 (.2632)
YEAR_2005	.3728 (.2879)		.3748 (.2880)
YEAR_2006	.0169 (.1699)		.0114 (.1705)
ALASKA	.5620 (.0934)***		.6106 (.0961)***
ARKANSAS	.3846 (.0191)***		.3852 (.0197)***
DELAWARE	.4500 (.1220)***		.4509 (.1246)***
FLORIDA	-.0762 (.0319)**		-.0871 (.0328)***
ILLINOIS	.4157 (.1703)**		.4180 (.1744)**
KENTUCKY	.3353 (.0738)***		.3315 (.0765)***
MAINE	.6700 (.0493)***		.6679 (.0506)***
MINNESOTA	-.2447 (.0895)***		-.2506 (.0915)***
MISSISSIPPI	.6982 (.0765)***		.6891 (.0776)***
NORTH_CAROLINA	.8014 (.0785)***		.7992 (.0793)***
SOUTH_CAROLINA	.7478 (.0696)***		.7475 (.0703)***
TENNESSEE	.6297 (.1294)***		.6295 (.1312)***
WISCONSIN	-		-

Continued

Table 6. Primary Seat Belt Law and Offsetting Effects

	Probit Model 1	Probit Model 2	Bivariate Probit Model
	NON_OCCUPANT		**NON_OCCUPANT**
CARELESS		.0162 (.1051)	.5705 (.2297)**
AGE		.0004 (.0002)**	.0005 (.0002)**
MALE		.0253 (.0132)*	.0236 (.0130)*
ALCOHOL		.3781 (.1884)**	.3567 (.1672)**
NIGHT		-.3108 (.3333)	-.3277 (.3271)
HIGH_POP		.4166 (.1757)**	.4237 (.1743)**
DRY_SURFACE		.3805 (.1694)**	.3942 (.1587)**
GOOD_WEATHER		-.1703 (.1198)	-.1974 (.1258)
LIGHT		-.5731 (.3455)*	-.5783 (.3446)*
VEHICLE_AGE		-.0101 (.0091)	-.0095 (.0088)
VEHICLE_AGE_SQ		.0002 (.0005)	.0001 (.0004)
HIGHWAY		-.0242 (.3661)	.0022 (.3609)
INTERCHANGE		-.9255 (.5281)*	-.9110 (.5061)*
SPEED_LIMIT		-.0342 (.0177)*	-.0348 (.0176)**
YEAR_2003		.1497 (.1678)	.1367 (.1730)
YEAR_2004		-.1107 (.1220)	-.1589 (.1259)
YEAR_2005		.3252 (.2609)	.2734 (.2494)
YEAR_2006		.2442 (.2515)	.2352 (.2511)
ALASKA		1.2652 (.2524)***	1.2278 (.2439)***
ARKANSAS		.5779 (.1598)***	.5315 (.1478)***
DELAWARE		.4351 (.1197)***	.4178 (.1085)***
FLORIDA		1.1646 (.2103)***	1.1484 (.1999)***
ILLINOIS		.2722 (.0417)***	.2718 (.0397)***
KENTUCKY		-.3718 (.0714)***	-.3895 (.0732)***
MAINE		.3123 (.0915)***	.2404 (.0822)***
MINNESOTA		.3829 (.1366)***	.3951 (.1339)***
MISSISSIPPI		.6004 (.2298)***	.5381 (.2166)**
NORTH_CAROLINA		.1091 (.1797)	.0456 (.1640)
SOUTH_CAROLINA		.6059 (.1667)***	.5446 (.1516)***
TENNESSEE		.4478 (.2498)*	.4154 (.2402)*
WISCONSIN		-	-
NUM of OBS	59,528	59,528	59,528

Note : Standard errors are in parentheses.
Robust variance estimate that adjusts for within-cluster correlation is used.
Wald test of $\rho = 0$: $\chi^2(1) = 12.0355$. Reject the null.
: Significant at the 5-percent level. *: Significant at the 1-percent level.

Table 7. Offsetting Effects with Sub-Sample: Striking vs. Non-Striking

	Striking	Non-striking
	CARELESS	**CARELESS**
PRIMARY	-.4045 (.1884)**	-.5533 (.1745)***
AGE	-.0008 (.0006)	-.0001 (.0007)
MALE	.0177 (.0231)	.0330 (.0233)
ALCOHOL	.0617 (.1437)	.2947 (.2162)
NIGHT	.2025 (.1031)**	.3142 (.0773)***
HIGH_POP	-.1326 (.0501)***	-.1964 (.0427)***
DRY_SURFACE	-.3606 (.2317)	.1757 (.0558)***
GOOD_WEATHER	.4363 (.1882)**	-.1212 (.0741)
LIGHT	.1421 (.0762)*	.1689 (.0914)*
VEHICLE_AGE	.0046 (.0074)	-.0160 (.0129)
VEHICLE_AGE_SQ	.0000 (.0002)	.0006 (.0004)
HIGHWAY	-.2705 (.1226)**	-.2847 (.1303)**
INTERCHANGE	.0832 (.1405)	.0548 (.0733)
SPEED_LIMIT	.0130 (.0036)***	.0072 (.0032)**
YEAR_2003	-.0630 (.1629)	-.1334 (.1818)
YEAR_2004	.4194 (.2121)**	.3213 (.3182)
YEAR_2005	.3591 (.2373)	.3892 (.3546)
YEAR_2006	-.0144 (.1567)	.0571 (.1683)
ALASKA	.6025 (.1076)***	.7079 (.1372)***
ARKANSAS	.4765 (.0371)***	.2881 (.0409)***
DELAWARE	.3334 (.1408)**	.5529 (.1194)***
FLORIDA	-.0739 (.0292)**	-.1513 (.0407)***
ILLINOIS	.3936 (.1617)**	.4801 (.1979)**
KENTUCKY	.3179 (.0578)***	.3488 (.1238)***
MAINE	1.3147 (.0919)***	-.2674 (.0562)***
MINNESOTA	-.5376 (.0959)***	.2643 (.0386)***
MISSISSIPPI	.5565 (.0816)***	.7994 (.1010)***
NORTH_CAROLINA	.7371 (.0743)***	.8723 (.0918)***
SOUTH_CAROLINA	.8363 (.0686)***	.6879 (.0750)***
TENNESSEE	.4803 (.1341)***	.7903 (.1537)***
WISCONSIN	-	-

Continued

Table 7. Offsetting Effects with Sub-Sample: Striking vs. Non-Striking

	Striking	Non-striking
	NON_OCCUPANT	**NON_OCCUPANT**
CARELESS	.6486 (.1883)***	.5357 (.2256)**
AGE	.0018 (.0004)***	-.0012 (.0006)**
MALE	.0319 (.0261)	.0154 (.0136)
ALCOHOL	.5386 (.1692)***	.1517 (.1711)
NIGHT	-.4071 (.3334)	-.2564 (.3354)
HIGH_POP	.3971 (.1695)**	.4470 (.1809)**
DRY_SURFACE	.3515 (.1541)**	.3858 (.1970)**
GOOD_WEATHER	-.0681 (.1759)	-.3253 (.0941)***
LIGHT	-.6760 (.3476)*	-.4521 (.3334)
VEHICLE_AGE	.0021 (.0132)	-.0203 (.0081)**
VEHICLE_AGE_SQ	-.0002 (.0007)	.0003 (.0004)
HIGHWAY	-.2690 (.3028)	.3033 (.4307)
INTERCHANGE	-.9586 (.3945)**	-.8965 (.6432)
SPEED_LIMIT	-.0322 (.0162)**	-.0391 (.0182)**
YEAR_2003	.2216 (.2065)	.0275 (.1437)
YEAR_2004	-.1899 (.1351)	-.1386 (.1374)
YEAR_2005	.4283 (.2280)*	.0790 (.2420)
YEAR_2006	.2936 (.2636)	.1620 (.2267)
ALASKA	.8720 (.0766)***	1.4102 (.3313)***
ARKANSAS	.4817 (.1317)***	.5359 (.1449)***
DELAWARE	.6887 (.0995)***	-.2238 (.1374)
FLORIDA	1.2415 (.1779)***	1.0643 (.2260)***
ILLINOIS	.3045 (.0358)***	.2448 (.0551)***
KENTUCKY	-.4339 (.0997)***	-.2674 (.0562)***
MAINE	.6125 (.1208)***	-5.6038 (.2960)***
MINNESOTA	.2967 (.1381)**	.5176 (.1275)***
MISSISSIPPI	.6181 (.2370)***	.4425 (.2045)**
NORTH_CAROLINA	.0641 (.1882)	.0002 (.1473)
SOUTH_CAROLINA	.3938 (.1202)***	.6534 (.1899)***
TENNESSEE	.4749 (.2179)**	.3206 (.2655)
WISCONSIN	-	-
NUM of OBS	32,738	26,790

Note : Standard errors are in parentheses.
Robust variance estimate that adjusts for within-cluster correlation is used.
Wald test of $\rho = 0$: $\chi^2(1) = 20.0632$ for the first sub-sample. Reject the null.
$\chi^2(1) = 13.8942$ for the second sub-sample. Also reject the null.
: Significant at the 5-percent level. *: Significant at the 1-percent level.

Table 8. Fatality Rate Per 100 Million VMT

	1999	2000	2001	2002	2003	2004	2005	2006	2007	2008
ALASKA	1.74	2.30	1.89	1.82	1.98	2.02	1.45	(1.49)	1.59	1.27
ARKANSAS	2.07	2.24	2.08	2.13	2.09	2.22	2.05	2.01	1.96	1.81
DELAWARE	1.18	1.49	1.58	1.40	(1.57)	1.44	1.40	1.57	1.23	1.35
FLORIDA	2.06	1.99	1.77	1.76	1.71	1.65	1.75	1.65	1.56	1.50
ILLINOIS	1.42	1.38	1.37	1.35	(1.36)	1.24	1.27	1.17	1.16	0.98
KENTUCKY	1.75	1.75	1.83	1.95	1.99	2.04	2.08	(1.91)	1.80	1.74
MAINE	1.28	1.19	1.33	1.47	1.39	1.30	1.13	1.25	(1.22)	1.06
MINNESOTA	1.22	1.19	1.06	1.20	1.18	1.00	0.98	0.87	0.89	0.78
MISSISSIPPI	2.66	2.67	2.18	2.43	2.33	2.28	2.32	(2.20)	2.04	1.79
NORTH_CAROLINA	1.71	1.74	1.67	1.70	1.66	1.64	1.53	(1.53)	1.62	1.40
SOUTH_CAROLINA	2.41	2.34	2.27	2.23	2.01	2.11	(2.21)	2.08	2.11	1.86
TENNESSEE	2.01	1.99	1.85	1.73	1.73	(1.89)	1.79	1.82	1.70	1.50
WISCONSIN	1.31	1.40	1.33	1.37	1.42	1.31	1.36	1.22	1.27	1.05

† Parentheses show the fatality rate of the year when the law began its enforcement.

‡ http://www-fars.nhtsa.dot.gov/Main/index.aspx

Table 9. Offsetting Effects with 3 Month- and 6 Month-Time Lag

	Bivariate Probit Model 1	Bivariate Probit Model 2
	CARELESS	**CARELESS**
PRIMARY	-.4272 (.1236)***	-.6710 (.2618)**
AGE	-.0006 (.0006)	-.0005 (.0006)
MALE	.0226 (.0146)	.0229 (.0151)
ALCOHOL	.1605 (.1776)	.0903 (.1271)
NIGHT	.2849 (.1157)**	.2796 (.1138)**
HIGH_POP	-.1567 (.0480)***	-.1585 (.0455)***
DRY_SURFACE	-.2170 (.2097)	-.2214 (.2182)
GOOD_WEATHER	.2971 (.1715)*	.2783 (.1849)
LIGHT	.1966 (.0988)**	.2317 (.0819)***
VEHICLE_AGE	-.0040 (.0096)	-.0014 (.0081)
VEHICLE_AGE_SQ	.0003 (.0003)	.0002 (.0003)
HIGHWAY	-.3025 (.1298)**	-.2900 (.1259)**
INTERCHANGE	.0689 (.0968)	.0692 (.0866)
SPEED_LIMIT	.0110 (.0038)***	.0101 (.0040)**
ALASKA	.1406 (.0880)	-.0301 (.1736)
ARKANSAS	.3829 (.0208)***	.3885 (.0185)***
DELAWARE	-.0202 (.0785)	-.0955 (.0646)
FLORIDA	-.0976 (.0295)***	-.1024 (.0311)***
ILLINOIS	-.0701 (.0249)***	-.1798 (.0744)**
KENTUCKY	-.1212 (.0955)	-.3567 (.2152)*
MAINE	.2347 (.1479)	-.0214 (.2397)
MINNESOTA	-.2596 (.0827)***	-.2341 (.0628)***
MISSISSIPPI	.2367 (.0762)***	.0762 (.1617)
NORTH_CAROLINA	.2984 (.1107)***	.0919 (.1853)
SOUTH_CAROLINA	.2969 (.0948)***	.1578 (.1019)
TENNESSEE	.1696 (.0635)***	.0485 (.0637)
	NON_OCCUPANT	**NON_OCCUPANT**
CARELESS	.5995 (.1975)***	.3236 (.4282)
AGE	.0005 (.0002)**	.0005 (.0002)**
MALE	.0234 (.0129)*	.0243 (.0133)*
ALCOHOL	.3551 (.1648)**	.3682 (.1870)**
NIGHT	-.3299 (.3250)	-.3240 (.3360)
HIGH_POP	.4237 (.1742)**	.4221 (.1787)**
DRY_SURFACE	.3932 (.1594)**	.3882 (.1607)**
GOOD_WEATHER	-.1957 (.1256)	-.1842 (.1270)
LIGHT	-.5804 (.3440)*	-.5796 (.3474)*
VEHICLE_AGE	-.0095 (.0088)	-.0099 (.0090)
VEHICLE_AGE_SQ	.0001 (.0004)	.0002 (.0004)
HIGHWAY	.0030 (.3592)	-.0099 (.3758)
INTERCHANGE	-.9217 (.5167)*	-.9243 (.5200)*
SPEED_LIMIT	-.0347 (.0176)**	-.0346 (.0179)*
ALASKA	1.2258 (.2462)***	1.2451 (.2447)***
ARKANSAS	.5287 (.1512)***	.5521 (.1422)***
DELAWARE	.4133 (.1092)***	.4252 (.1095)***
FLORIDA	1.1469 (.2012)***	1.1588 (.2009)***
ILLINOIS	.2717 (.0394)***	.2722 (.0401)***
KENTUCKY	-.3887 (.0718)***	-.3819 (.0766)***
MAINE	.2343 (.0878)***	.2662 (.0726)***
MINNESOTA	.3956 (.1325)***	.3917 (.1391)***
MISSISSIPPI	.5341 (.2217)**	.5678 (.2044)***
NORTH_CAROLINA	.0426 (.1668)	.0721 (.1554)
SOUTH_CAROLINA	.5406 (.1564)***	.5708 (.1431)***
TENNESSEE	.4126 (.2433)*	.4302 (.2348)*
NUM of OBS	59,528	59,528

Note : Standard errors are in parentheses.
Robust variance estimate that adjusts for within-cluster correlation is used.
Year dummies are not shown.
Wald tests of $\rho = 0$: $\chi^2(1) = 20.6588$. $\chi^2(1) = .8401$ Reject only the first null.
: Significant at the 5-percent level. *: Significant at the 1-percent level.

Table 10. Offsetting Effects with Only Neighboring States

	Probit Model 1	Probit Model 2	Bivariate Probit Model
	CARELESS		**CARELESS**
PRIMARY	.4391 (.2584)*		.4392 (.2586)*
AGE	-.0002 (.0005)		.0002 (.0005)
MALE	.0129 (.0108)		.0128 (.0109)
ALCOHOL	-.0708 (.0690)		-.0710 (.0689)
NIGHT	.1191 (.0675)*		.1184 (.0681)*
HIGH_POP	-.0207 (.1170)		-.0207 (.1169)
DRY_SURFACE	-.0827 (.2851)		-.0822 (.2867)
GOOD_WEATHER	-.1280 (.2568)		-.1283 (.2578)
LIGHT	.2190 (.0666)***		.2183 (.0670)***
VEHICLE_AGE	.0041 (.0036)		.0041 (.0036)
VEHICLE_AGE_SQ	-.0001 (.0001)		-.0001 (.0001)
HIGHWAY	-.3512 (.0630)***		-.3507 (.0635)***
INTERCHANGE	.0027 (.1115)		.0027 (.1115)
SPEED_LIMIT	.0133 (.0062)**		.0132 (.0062)**
YEAR_2003	.5469 (.3299)*		.5477 (.3318)*
YEAR_2004	.6051 (.2667)**		.6055 (.2673)**
YEAR_2005	.7093 (.3852)*		.7098 (.3853)*
YEAR_2006	.1024 (.1601)		.1031 (.1616)
HAWAII	.2445 (.1388)*		.2460 (.1373)*
ARKANSAS	.3521 (.0468)***		.3518 (.0474)***
MARYLAND	.7312 (.2600)***		.7313 (.2598)***
FLORIDA	-.0553 (.0308)*		-.0548 (.0130)*
INDIANA	-.6816 (.2561)***		-.6816 (.2561)***
OHIO	-.5410 (.0928)***		-.5405 (.0925)***
NEW_HAMPSHIRE	.4286 (.0494)***		.4287 (.0495)***
MINNESOTA	-.2006 (.0700)***		-.1999 (.0702)***
MISSOURI	.0502 (.1215)		.0505 (.1211)
VIRGINIA	.2542 (.0776)***		.2544 (.0773)***
GEORGIA	.1976 (.1559)		.1978 (.1557)
ALABAMA	.2461 (.2171)		.2473 (.2166)
WISCONSIN	-		

Continued

Table 10. Offsetting Effects with Only Neighboring States

	Probit Model 1	Probit Model 2	Bivariate Probit Model
	NON_OCCUPANT		**NON_OCCUPANT**
CARELESS		-.1807 (.1099)	-.2359 (.2627)
AGE		-.0009 (.0006)	-.0009 (.0006)
MALE		.0227 (.0111)**	.0228 (.0112)**
ALCOHOL		.4963 (.2478)**	.4955 (.2465)**
NIGHT		-.0079 (.3368)	-.0070 (.3402)
HIGH_POP		.2187 (.2513)	.2183 (.2525)
DRY_SURFACE		1.2206 (.3431)***	1.2195 (.3445)***
GOOD_WEATHER		-.9153 (.3680)**	-.9171 (.3668)**
LIGHT		-.4776 (.3115)	-.4753 (.3198)
VEHICLE_AGE		.0024 (.0050)	.0024 (.0051)
VEHICLE_AGE_SQ		-.0002 (.0002)	-.0002 (.0002)
HIGHWAY		.4980 (.3061)	.4935 (.3130)
INTERCHANGE		-.8117 (.3384)**	-.8107 (.3395)**
SPEED_LIMIT		-.0618 (.0185)***	-.0617 (.0185)***
YEAR_2003		-.0326 (.3001)	-.0286 (.2851)
YEAR_2004		-.1944 (.1724)	-.1894 (.1545)
YEAR_2005		.1242 (.1415)	.1312 (.1228)
YEAR_2006		.1318 (.2865)	.1323 (.2841)
HAWAII		.0686 (.1052)	.0738 (.0990)
ARKANSAS		.8418 (.2057)***	.8459 (.2062)***
MARYLAND		.3881 (.1268)***	.4061 (.1136)***
FLORIDA		1.1858 (.1924)***	1.1859 (.1928)***
INDIANA		-.1251 (.0630)**	-.1264 (.0620)**
OHIO		.4936 (.1408)***	.4908 (.1501)***
NEW_HAMPSHIRE		.1176 (.0499)**	.1224 (.0333)***
MINNESOTA		.4476 (.0911)***	.4460 (.0938)***
MISSOURI		.3115 (.1291)**	.3138 (.1252)**
VIRGINIA		.4083 (.1431)***	.4124 (.1372)***
GEORGIA		.6640 (.1710)***	.6687 (.1646)***
ALABAMA		.9726 (.2794)***	.9792 (.2571)***
WISCONSIN		-	-
NUM of OBS	88,630	88,630	88,630

Note : Standard errors are in parentheses.
Robust variance estimate that adjusts for within-cluster correlation is used.
Wald test of $\rho = 0$: $\chi^2(1) = .0679$. Cannot Reject the null.
: Significant at the 5-percent level. *: Significant at the 1-percent level.

Table 11-a. Summary Statistics - Synthetic Panel Model(Primary Group)

Variable	Obs	Mean	SD	Min	Max	Acronym
Dependent variable:						
Careless Action	437	.1800	.1787	0	1	*CARELESS*
Independent variables:						
Primary Seat Belt Law	440	.2909	.4547	0	1	*PRIMARY*
Sex	440	.5	.5006	0	1	*MALE*
Age between 16 and 25	440	.25	.4335	0	1	*AGE_1*
Age between 26 and 35	440	.25	.4335	0	1	*AGE_2*
Age between 36 and 49	440	.25	.4335	0	1	*AGE_3*
Age over 50	440	.25	.4335	0	1	*AGE_4*
Year Dummy 2003	440	.2	.4005	0	1	*YEAR_2003*
⋮						
Year Dummy 2007	440	.2	.4005	0	1	*YEAR_2007*
State Dummy Arkansas	440	.0909	.2878	0	1	*ARKANSAS*
State Dummy Delaware	440	.0909	.2878	0	1	*DELARWARE*
State Dummy Florida	440	.0909	.2878	0	1	*FLORIDA*
State Dummy Illinois	440	.0909	.2878	0	1	*ILLINOIS*
State Dummy Kentucky	440	.0909	.2878	0	1	*KENTUCKY*
State Dummy Minnesota	440	.0909	.2878	0	1	*MINNESOTA*
State Dummy Mississippi	440	.0909	.2878	0	1	*MISSISSIPPI*
State Dummy North Carolina	440	.0909	.2878	0	1	*NORTH_CAROLINA*
State Dummy South Carolina	440	.0909	.2878	0	1	*SOUTH_CAROLINA*
State Dummy Tennessee	440	.0909	.2878	0	1	*TENNESSEE*
State Dummy Wisconsin	440	.0909	.2878	0	1	*WISCONSIN*

Table 11-b. Summary Statistics - Synthetic Panel Model (Non-Primary Group)

Variable	Obs	Mean	SD	Min	Max	Acronym
Dependent variable:						
Careless Action	440	.1853	.1883	0	.8642	*CARELESS*
Independent variables:						
Primary Seat Belt Law	440	.2909	.4547	0	1	*PRIMARY*
Sex	440	.5	.5006	0	1	*MALE*
Age between 16 and 25	440	.25	.4335	0	1	*AGE_1*
Age between 26 and 35	440	.25	.4335	0	1	*AGE_2*
Age between 36 and 49	440	.25	.4335	0	1	*AGE_3*
Age over 50	440	.25	.4335	0	1	*AGE_4*
Year Dummy 2003	440	.2	.4005	0	1	*YEAR_2003*
⋮						
Year Dummy 2007	440	.2	.4005	0	1	*YEAR_2007*
State Dummy Arkansas	440	.0909	.2878	0	1	*ARKANSAS*
State Dummy Maryland	440	.0909	.2878	0	1	*MARYLAND*
State Dummy Florida	440	.0909	.2878	0	1	*FLORIDA*
State Dummy Indiana	440	.0909	.2878	0	1	*INDIANA*
State Dummy Ohio	440	.0909	.2878	0	1	*OHIO*
State Dummy Minnesota	440	.0909	.2878	0	1	*MINNESOTA*
State Dummy Missouri	440	.0909	.2878	0	1	*MISSOURI*
State Dummy Virginia	440	.0909	.2878	0	1	*VIRGINIA*
State Dummy Georgia	440	.0909	.2878	0	1	*GEORGIA*
State Dummy Alabama	440	.0909	.2878	0	1	*ALABAMA*
State Dummy Wisconsin	440	.0909	.2878	0	1	*WISCONSIN*

Table 12. Synthetic Panel Data Model

	Synthetic Panel Model 1	Synthetic Panel Model 2
	CARELESS	**CARELESS**
PRIMARY	-.1945 (.0276)***	-.0379 (.0251)
MALE	.0109 (.0135)	.0093 (.0127)
AGE_1	.0264 (.0190)	.0214 (.0180)
AGE_2	.0049 (.0195)	.0154 (.0185)
AGE_3	.0160 (.0197)	.0105 (.0178)
YEAR_2003	-.0620 (.0224)***	.0577 (.0232)**
YEAR_2004	.0762 (.0215)***	.1010 (.0191)***
YEAR_2005	.0654 (.0219)***	.1236 (.0272)***
YEAR_2006	-.0456 (.0173)***	.0055 (.0198)
ARKANSAS	.0655 (.0273)**	.0655 (.0264)**
DELAWARE	.1759 (.0340)***	-
MARYLAND	-	.3902 (.0321)***
FLORIDA	.0587 (.0242)**	.0587 (.0241)**
ILLINOIS	.1544 (.0309)***	-
INDIANA	-	-.0088 (.0241)
KENTUCKY	.1104 (.0198)***	-
OHIO	-	-.0239 (.0161)
MINNESOTA	.0010 (.0236)	.0013 (.0238)
MISSISSIPPI	.2408 (.0352)***	-
MISSOURI	-	.0561 (.0204)***
NORTH_CAROLINA	.2277 (.0164)***	-
VIRGINIA	-	.1057 (.0120)***
SOUTH_CAROLINA	.2253 (.0325)***	-
GEORGIA	-	.1217 (.0267)***
TENNESSEE	.2725 (.0418)***	-
ALABAMA	-	.3250 (.0347)***
Num. of Obs.	437	440
Num. of Groups	88	88
R^2:within	0.2894	.1633
R^2:between	0.7981	.9308
R^2:overall	0.4037	.5229

Note : Robust standard errors are in parentheses.
Two states, Alaska and Maine, are not used for this estimation because the number of observations in each cohort is too small.

: Significant at the 5-percent level. *: Significant at the 1-percent level.

APPENDIX A. Description of Variables

Variable	Description	Dummy
Dependent variables:		
$CARELESS$ §	Careless driving behavior:	Y
	1 if the driver shows careless driving behavior, 0 otherwise	
$NON\_OCCUPANT$	Non-occupants' involvement	Y
	1 if non-occupants are involved, 0 otherwise	
Independent Variables:		
$PRIMARY$ §	Primary seat belt law: 1 if an accident occurs	Y
	in the state with the law, 0 if otherwise	
$PRIMARY3$	Primary seat belt law: 1 if an accident occurs	Y
	after 3 months since the adoption, 0 if otherwise	
$PRIMARY6$	Primary seat belt law: 1 if an accident occurs	Y
	after 6 months since the adoption, 0 if otherwise	
AGE	Age of the person (years)	Y
AGE_1 §	Age: 1 if the driver's age is between 16 and 25	Y
AGE_2 §	Age: 1 if the driver's age is between 26 and 35	Y
AGE_3 §	Age: 1 if the driver's age is between 36 and 49	Y
AGE_4 §	Age: 1 if the driver's age is over 50	Y
$MALE$ §	Gender: 1 if male, 0 if female	Y
$ALCOHOL$	Police-reported alcohol involvement in accidents	Y
	1 if the person had consumed an alcoholic beverage, 0 if not	
$NIGHT$	Hour of crash	Y
	1 if accident occurs between 7:00 p.m. and 2:00 a.m.	Y
$HIGH\_POP$	Population Density:	Y
	1 if within area of population of 100,000 +, 0 if less than 100,000	
$DRY\_SURFACE$	1 if condition of road surface at the time of crash is dry, 0 otherwise	Y
$GOOD\_WEATHER$	General weather conditions:	Y
	1 if it is good, 0 if there was any adverse condition	Y
$LIGHT$	General light conditions: 1 if daylight, 0 otherwise	Y
$VEHICLE\_AGE$	Difference between the current year and the model year	N
$VEHICLE\_AGE\_SQ$	Square of $VEHICLE\_AGE$	N
$HIGHWAY$	Interstate Highway	Y
	1 if the crash occurred on an interstate highway, 0 otherwise	
$INTERCHANGE$	1 if the first harmful event is located	Y
	within an interchange area, 0 otherwise	
$SPEED\_LIMIT$	Actual posted speed limit (miles per hour)	N
$YEAR\_2003$ §	Year dummy	Y
$\vdots$	$\vdots$	
$YEAR\_2007$ §	Year dummy	Y
$ALASKA$ §	State dummy	Y
$\vdots$	$\vdots$	
$WISCONSIN$ §	State dummy	Y

Note : § indicates that the definition of the variable is same in the synthetic panel data model.

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