THE EFFECTS OF MANDATORY SEAT BELT LAWS ON DRIVING BEHAVIOR AND TRAFFIC FATALITIES

Alma Cohen and Liran Einav*

Abstract—This paper investigates the effects of mandatory seat belt laws on driver behavior and traffic fatalities. Using a unique panel data set on seat belt usage in all U.S. jurisdictions, we analyze how such laws, by influencing seat belt use, affect the incidence of traffic fatalities. Allowing for the endogeneity of seat belt usage, we find that such usage decreases overall traffic fatalities. The magnitude of this effect, however, is significantly smaller than the estimate used by the National Highway Traffic Safety Administration. In addition, we do not find significant support for the compensating-behavior theory, which suggests that seat belt use also has an indirect adverse effect on fatalities by encouraging careless driving. Finally, we identify factors, especially the type of enforcement used, that make seat belt laws more effective in increasing seat belt usage.

I. Introduction

TRAFFIC accidents are a major source of fatalities and serious injuries. Every day more than 100 Americans are killed in motor vehicle crashes. One important policy tool that has been used to combat this problem is the passage of mandatory seat belt laws. Indeed, the federal government set in 1997 an ambitious goal of increasing seat belt usage from the 1996 national level of 68% to 85% by the year 2000 (a target that was not achieved) and to 90% by 2005. To increase seat belt usage, the federal government has been encouraging states to adopt stronger mandatory seat belt laws.

The aim of this paper is to empirically investigate the effectiveness of mandatory seat belt laws in reducing traffic fatalities. We use a unique data set on state-level seat belt usage that enables us to improve upon previous work. This data set allows us to break up the effects of mandatory seat belt laws into two components: the effectiveness of the laws in increasing seat belt usage and the effectiveness of seat belt use in reducing traffic fatalities. Using the seat belt laws as instruments for the usage rate, this work is the first to address the endogeneity of usage. Our findings have substantial implications for policymaking in this area.

Our data set contains panel data on the 50 U.S. states and the District of Columbia for the years 1983 to 1997. Although mandatory seat belt laws were adopted in Europe and Australia as early as the 1970s, it was not until December 1984 that such laws were adopted in the United States, New York being the first state to do so. During our observation period, all other U.S. jurisdictions, except New Hampshire,[1] gradually adopted seat belt legislation.[2] This pattern of adoption makes it possible to obtain a clean identification of the effects of these laws, controlling for year and state fixed effects. Given the wide variation in usage rates across states, we can also allow the effect of mandatory seat belt legislation to depend on the usage rate that prevailed when the law was passed.

The seat belt usage rate data also allow for a direct investigation of the compensating-behavior theory (Peltzman, 1975). According to this theory, because drivers wearing seat belts feel more secure, they drive less carefully, leading to more traffic accidents. Thus, although the use of seat belts decrease fatalities among drivers wearing them, fatalities among other individuals go up, offsetting the beneficial effects of seat belts. To test the compensating-behavior theory, it is necessary to identify the effects of an increase in usage rate on driving behavior.

We distinguish, following the literature, between fatalities among car occupants, who may be directly affected by using seat belts, and fatalities among nonoccupants (pedestrians, bicyclists, and motorcyclists), who do not use seat belts and can thus be affected by seat belt use only indirectly. The compensating-behavior theory suggests that traffic fatalities are influenced by seat belt use in two ways: the direct effect, which operates to reduce the probability that a car occupant wearing a seat belt will be killed in the event of an accident, and the indirect effect, which operates to increase the incidence of accidents by inducing less careful driving. Whereas car occupants might be subject to both of these effects, nonoccupants are subject only to the indirect effect. Thus, the compensating-behavior theory predicts positive correlation between seat belt usage and fatalities among nonoccupants.

Our findings indicate that seat belt use significantly reduces fatalities among car occupants, but does not appear to have any statistically significant effect on fatalities among nonoccupants. Thus, we do not find significant evidence for compensating behavior. In the course of our analysis, we replicate some of the findings of studies that concluded that such behavior exists, and we show that, once we allow for the endogeneity of usage, the effects of seat belt use on nonoccupant fatalities is insignificant.

Overall, we find that seat belt legislation unambiguously reduces traffic fatalities. Specifically, we estimate that a

Received for publication October 30, 2001. Revision accepted for publication July 11, 2002.

* National Bureau of Economic Research and Stanford University, respectively.

This paper was written while both authors were graduate students at Harvard University. We are grateful to Lucian Bebchuk, Gary Chamberlain, David Cutler, John Graham, Shigeo Hirano, Caroline Hoxby, Lawrence Katz, Ariel Pakes, Jack Porter, Manuel Trajtenberg, Kip Viscusi, seminar participants at Harvard University and at "Econometrics in Tel-Aviv," two anonymous referees, and the editor for helpful comments. We thank the NHSTA and the Highway Safety Offices of many states for providing us with the data on seat belt usage. Any remaining errors are our own.

[1] In fact, New Hampshire passed a mandatory seat belt law with secondary enforcement in 1999, but it requires only drivers and passengers under the age of 18 to wear seat belts.

[2] Within this period, in the late 1980s, four states—Massachusetts, Nebraska, North Dakota, and Oregon—passed a mandatory seat belt law, repealed it, and reinstated it again after a short period (up to two years).

The Review of Economics and Statistics, November 2003, 85(4): 828–843

1-percentage-point increase in usage saves 136 lives (using a linear specification), and that a 1% increase in usage reduces occupant fatalities by about 0.13% (using a log-log specification). To illustrate, this implies that moving from the 68% national usage level to the 90% target level will save annually about 1500–3000 lives (4% to 8% of all traffic fatalities). Interestingly, although this estimate of the effect of increased seat belt usage on saved lives is substantial, it is considerably smaller than the estimate used by the federal government, which is 5,536 saved lives annually (approximately 14% of total fatalities).

The first-step regressions of our analysis also allow us to analyze which elements of seat belt legislation make it effective in increasing usage rate. The element that we find to be most important in this respect is having primary enforcement (i.e., allowing the police to stop and fine violators even if they do not engage in other offenses) rather than secondary enforcement (i.e., allowing the police to fine violators only when they are stopped for some other offense). While observers and policymakers have noticed that states with primary enforcement have on average higher usage rates, we are able to identify and estimate the effects of primary enforcement in a statistically more reliable way.

We find that, whereas a mandatory seat belt law with secondary enforcement increases the usage rate by about 11 percentage points, a mandatory seat belt law backed by primary enforcement increases usage by about 22 percentage points. This finding supports the recent initiative undertaken by the federal government to encourage states to adopt primary enforcement. Indeed, we estimate that if all 34 states now having secondary enforcement were to switch to primary enforcement, the national usage would increase from the current 68% to about 77%, producing an annual saving in the range of 500 to 1,200 lives.

The paper is organized as follows. Section II discusses the literature, section III describes the data, and section IV discusses and motivates our estimation strategy. Section V presents our results and discusses their robustness, and section VI concludes.

II. Literature and Motivation

It is widely agreed that, holding the number of accidents fixed, the direct effect of seat belt use is to reduce fatalities among those wearing seat belts. A survey of laboratory evidence concluded that seat belt use by front seat passengers can prevent 40% to 50% of the fatalities among such passengers involved in an accident (Department of Transportation, 1984). Furthermore, most of the empirical papers that investigated this direct effect found results consistent with the laboratory studies (Evans, 1986, Graham et al., 1997, and Levitt and Porter, 2001).

Although it is widely accepted that seat belt use reduces the fatality risk among car occupants in the event of an accident, it has been argued that its overall effect on fatalities might be insignificant or even positive. This argument was put forward by Peltzman (1975), who argued that seat belt use might produce careless driving and in turn greater risks for nonoccupants. As a result, mandatory seat belt laws might increase total fatalities rather than reduce them.[3]

There is a large empirical literature that tries to estimate the effect of mandatory seat belt laws on fatalities. Some of the existing papers consider the effect of such laws on aggregate fatalities without trying to distinguish between fatalities among those seated in a car who can wear seat belts (occupants) and those not in a car (nonoccupants).[4] Other papers have tried to test for compensating behavior by making this distinction, which enables focusing on nonoccupant fatalities. Nonoccupants cannot be affected directly by seat belts but only through changes in driving behavior. The studies that used this approach reached mixed results.[5]

The existing empirical work has substantial limitations that the present study seeks to overcome. To start with, many of the papers surveyed use time series data and look at traffic fatalities before and after a mandatory seat belt law was passed.[6] Such studies could not take into account other macro effects, such as other laws, public campaigns, or technological changes, which are unrelated to the mandatory seat belt laws but might have affected the changes in the time trend of fatalities. Second, the results of most studies cannot be used for further policy evaluations, because they depend only on one change in the law.[7] As discussed later on, the comprehensive panel data that we use in this paper allow us to overcome many of these limitations.[8]

[3] In the psychology literature, Wilde (1982) takes this argument to an extreme with his theory of risk homeostasis. He argues that individuals adopt a fixed target level of fatality risk and adjust their driving accordingly.

[4] McCarthy (1999) finds that a mandatory seat belt law increases the number of fatal accidents, whereas Bhattacharyya and Layton (1979) and Houston, Richardson, and Neeley (1995) find that seat belt laws significant reduce traffic fatalities. Derrig et al. (2002) find no statistically significant effect.

[5] Garbacz (1991), Loeb (1995), and Wagenaar, Maybee, and Sullivan (1988) find a significant effect of mandatory seat belt laws in reducing occupant fatalities, whereas Evans and Graham (1991) and Harvey and Durbin (1986) find a significant positive effect in *increasing* nonoccupant fatalities. Asch et al. (1991) find that, while the number of fatalities per accident decreased after the passage of the mandatory seat belt law, there was a significant increase in the number of accidents. The most puzzling results are found by Garbacz (1990a, 1990b, 1992) and Risa (1994); they find that seat belt usage is either insignificant or *positively* associated with occupant and nonoccupant fatalities.

[6] See, for example, Bhattacharyya and Layton (1979), Garbacz (1991), Harvey and Durbin (1986), and Wagenaar et al. (1988).

[7] The same argument also applies when panel data are used, if the law or the change in the law does not vary across the different groups. This is the case, for example, in Risa (1994), Asch et al. (1991), and McCarthy (1999).

[8] Evans and Graham (1991), Houston et al. (1995), and Sen (2001) do use the variation in the mandatory seat belt laws across U.S. states or Canadian provinces. However, these papers do not use data on usage and thus can only analyze the reduced-form, indirect effect of mandatory seat belt laws on fatalities. As we discuss next, such analysis may be significantly improved upon by using data on seat belt usage.

Furthermore, and perhaps most importantly, the existing empirical work has failed to break up the law's effect, as we do, into the effect of the law on usage and the effect of usage on traffic fatalities. Mandatory seat belt laws presumably do not affect fatalities directly but only through their effect on usage, which in turn affects traffic fatalities. Thus, to conclude that mandatory seat belt laws are beneficial, we must first find that the laws are effective in increasing seat belt usage, and then investigate how usage affects fatalities. By analyzing the two effects separately, as our data permit us to do, our approach has several advantages. First, the effect of a mandatory seat belt law might depend substantially on the initial seat belt usage at the time the law was passed.[9] The typical analysis in earlier studies, which uses only dummy variables for the existence of mandatory seat belt laws restricts the law to have an impact on fatalities that is constant across different initial levels of usage. In contrast, by incorporating data on seat belt usage, we allow for the law's effect to depend on the initial usage level, which seems plausible and important. Moreover, usage data allow for a more direct test of the theory of compensating behavior. The theory suggests that careless driving is associated with seat belt use, not with the existence of mandatory seat belt laws, which can only be used as a proxy. Thus, testing the theory on the basis of usage is preferable. Finally, separately estimating the effect of seat belt usage on traffic fatalities allows us to evaluate the benefits associated with other policy measures aimed at increasing seat belt use, such as advertising campaigns.

The few studies that used seat belt usage data in their analysis did not take into account that the decision to wear a seat belt is a choice variable and therefore likely to be endogenous (Garbacz, 1990a, 1991, 1992; Risa, 1994).[10] For example, if the probability of an accident is high, individuals will be more likely to protect themselves and to use seat belts. Hence, on regressing fatalities on usage without allowing for this endogeneity, we would expect to find a positive correlation between the usage and the error term, which would lead to an upward bias in the coefficient on usage. Indeed, this is probably the reason why Garbacz (1990a, 1992) and Risa (1994) obtain positive coefficients on usage, leading them to accept the compensating-behavior theory. As we show later on, this bias disappears once we allow for the endogeneity of usage.

Finally, a wide existing literature focuses on the impact of mandatory seat belt laws on seat belt usage, as we do in our first-step regressions. This question is of interest to federal and state officials who have been investing a great deal of effort in trying to increase seat belt use. Most of the empirical studies focus on the effects that the two different types of enforcement have on usage. The general findings are that laws increase usage, and that primary enforcement does it significantly better.[11] The existing studies generally focus on the short-term effects of adopting seat belt laws, because they do not have panel data sets that are sufficiently long. Our longer observation period may help generalize these findings also for the longer run.[12]

Some of the empirical studies also investigate how individuals' characteristics affect the individuals' decisions whether to use seat belts. It was found that heterogeneity across individuals is important, and that more risk-averse individuals are more likely to comply with the law.[13] This heterogeneity introduces another difficulty in measuring the impact of the law. It might suggest, for example, that the individuals who begin to wear seat belts when the usage increases from 40% to 60% are of a different type than the individuals who begin to wear seat belts when the usage rate increases from 80% to 100%; this might lead the two increases to have different effects on fatalities. We address these issues in section V, when we investigate different specifications and functional forms.

III. Data

We use a panel of annual state-level variables for all U.S. jurisdictions—the 50 states and the District of Columbia. Unless otherwise noted, all variables cover the period between 1983 and 1997. The Data Appendix defines all the variables, describes their sources and their relevance, and provides descriptive statistics. In this section we focus on the two most important variables for our analysis, namely, traffic fatalities and seat belt usage.

We obtained data on the annual number of occupant and nonoccupant[14] traffic fatalities from the Fatality Analysis Reporting System (FARS).[15] There are roughly 35,000 occupant fatalities and 5,000 nonoccupant fatalities every year. Figure 1 shows the trend in occupant and nonoccupant

[9] For example, such legislation might have a big effect in states in which the seat belt usage was low to begin with, but only a small effect in states where everybody was already using seat belts prior to the passage of the law. Seat belt usage varies a great deal across states. In our data set, usage levels in state-years without any mandatory seat belt law vary between 4% and 59%, with a mean and median of 30% and standard deviation of about 13%, implying that wide variation in initial usage levels is likely to lead to a wide variation in the impact of seat belt laws on traffic fatalities.

[10] See, for example, Traynor (1993), who provides evidence that drivers are more likely to take various precautions, such as wearing a seat belt, when driving conditions are bad.

[11] See, for example, Campbell (1988), Campbell, Stewart, and Campbell (1986), Dee (1998), Evans and Graham (1991), Wagenaar et al. (1988), and Patryka (1987).

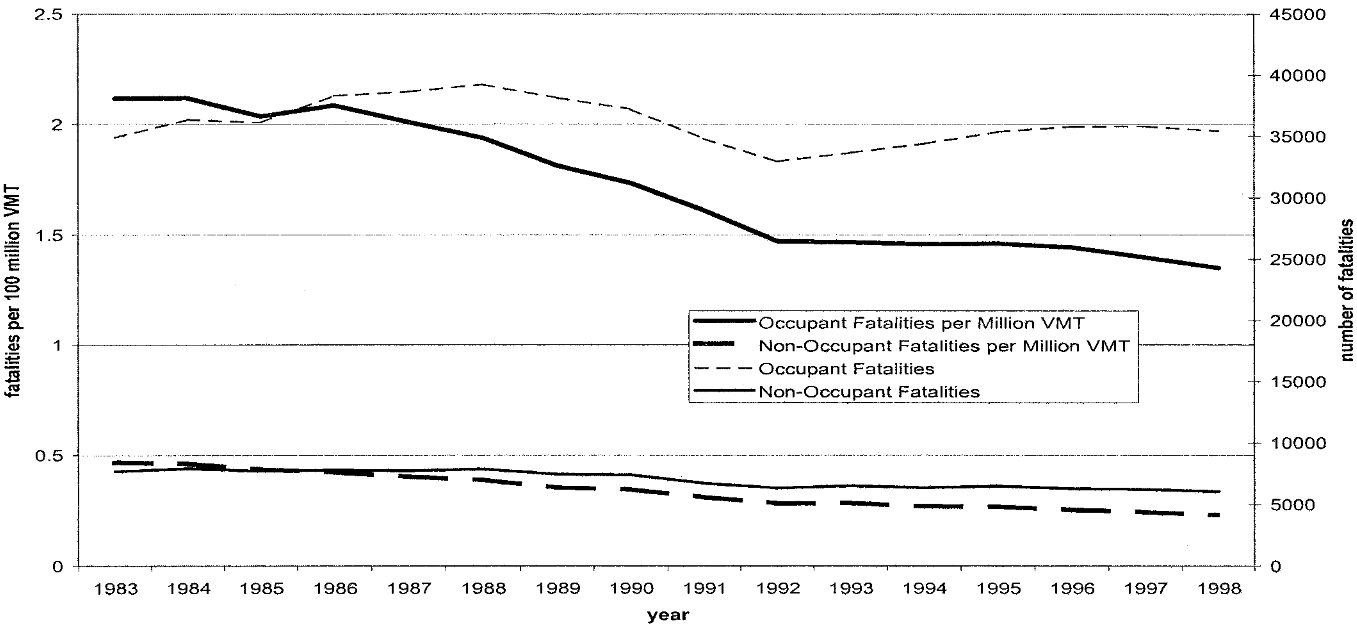
[12] As various writers have emphasized, the long-term effects of the considered legislation might differ from the short-term effects. See, for example, Peltzman (1977), and Evans and Graham (1991).

[13] See Center for Disease Control (1986), Dee (1998), Evans (1987), Evans and Wasielewski (1983), Hunter et al. (1990), Houston et al. (1995), Levitt and Porter (2001), and Singh and Thayer (1992).

[14] We do not distinguish between front seat and rear seat passengers, although it may be argued that rear seat passengers, in many states, are not required to use seat belt, and hence should be treated as nonoccupants. If this is the case, then it only makes our results stronger.

[15] FARS contains detailed information on all fatal traffic accidents within the United States. A fatal accident is defined as any traffic accident that results in fatality to a vehicle occupant or nonmotorist, or in fatality from injuries resulting from a traffic accident that occurs within 30 days of it.

FIGURE 1.—TOTAL OCCUPANT AND NONOCCUPANT FATALITIES



The graph shows the trends in occupant and nonoccupant fatalities in the United States over the observation period. It can be seen that while total fatalities are roughly constant over the years, there is significant trend downwards once fatalities are normalized by vehicle miles traveled.

fatalities during our sample period. It indicates that there is no clear trend over time. Once we normalize by vehicle mile traveled (VMT), we observe a drop in fatalities over the sample period for both occupants and nonoccupants.

An important and unique element of our data set is the state-level data on seat belt usage. We obtained such data from the following three different sources:

(i) *Data from the Highway Safety Office of each state.* The states obtain their estimates of seat belt usage rate by conducting periodical observational surveys.[16] Most of the states had separate estimates for front seat occupants and for rear seat occupants. We use only the information on front seat occupants, which is available for all states for which we have data. Consistent with NHTSA guidelines, the data are then weighted to reflect the regional sampling design and the average daily traffic volume.[17]

(ii) *Data from the National Highway Traffic Safety Administration (NHTSA).* These data include annual state-level usage rates, from 1990 until 1999, for all 51 states. The NHTSA data that we use are also the ones used by the federal government to estimate the effects of seat belt legislation and to allocate the federal budget among states.

(iii) *Data from the Behavioral Risk Factor Surveillance System (BRFSS).* These data are based on a state-level telephone survey conducted by the Center for Disease Control (CDC). One of the questions asked in this survey is: "How often do you use seat belts when you drive or ride in a car?" There are five possible answers: never, seldom, sometimes, almost always, and always. We assigned a weight to each one of the answers[18] and aggregated them by state over all surveyed individuals, adjusting for sampling weights.[19] The BRFSS data are available from 1984 until 1997, with varying numbers of states surveyed each year.[20]

We combined the first two sources of data to obtain a usage data set that is as full as possible, and we used the BRFSS data mainly for robustness checks, addressing concerns about usage measurement from the observational surveys. To combine the data from the first two sources, we

[16] The way the observational survey is conducted is the following: each state chooses a number of counties, which usually account for more than 85% of the population. In each county the survey is held at several chosen sites (intersections). The sites are chosen from a list of potential sites by a standard unbiased sampling procedure, which is recommended by the NHTSA. Potential sites are places where the driver has to either slow down or to stop completely, so that the observation is made easier and more accurate. After a short training period, each observer is randomly assigned to a specific site and to a specific time of day (the observation slots are generally a 40–60 min. time window during daylight hours on midweek days).

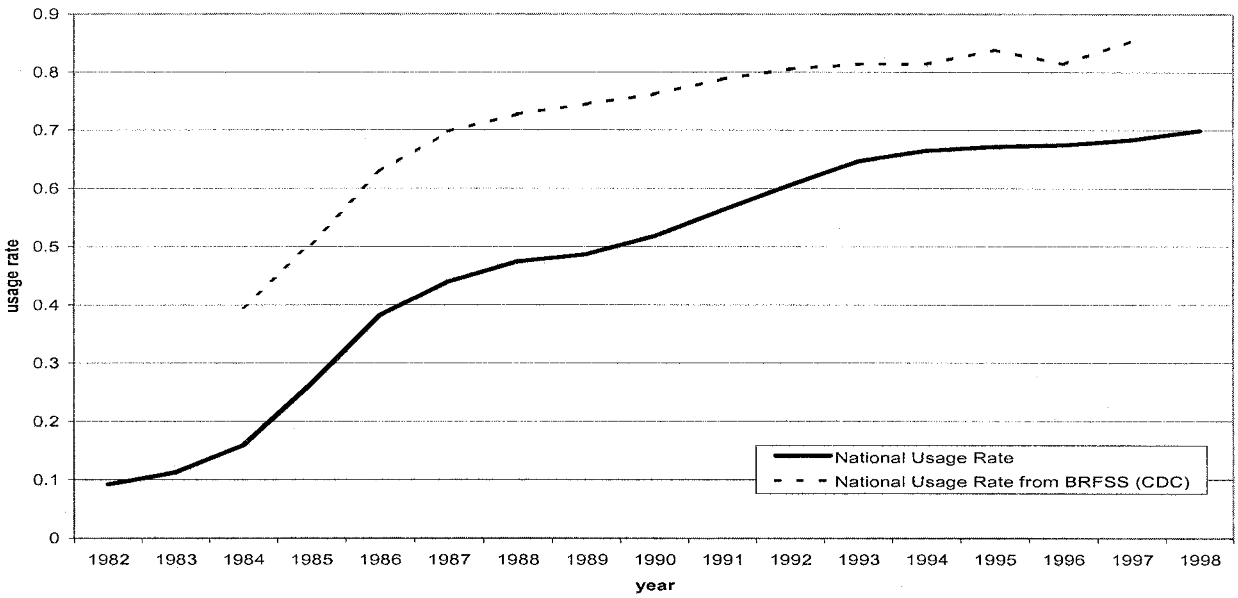
[17] Thirty-seven states provided us with a full set of data that includes annual usage starting in the year prior to the passage of the law and until 1998, five states provided us with incomplete data for some of the years in our observation period, and nine states provided us with none.

[18] The weights we have used were (0, 0.1, 0.3, 0.75, 1). We tried a few other quintuplets of weights, without much difference in the results.

[19] The BRFSS is a random telephone survey. Hence, sampling weights try to adjust mainly for the number of telephone lines and the number of persons in each household.

[20] One problem with the use of these data is that it limited our number of observations substantially. Another problem is that this variable is far from being optimal in that it suffers from all the problems that arise from self-reporting, and from subjective answers.

FIGURE 2.—AVERAGE SEAT BELT USAGE OVER TIME



The solid line represents the average (equal weights) of seat belt usage, as reported by the observational surveys. This variable is the one used in our reported regressions. The dashed line represents the average of seat belt usage, as implied by the BRFSS survey of the CDC. Note that the averages for 1994 and 1996 are based only on 10 states, as explained in the text (section III).

use as a basis the NHTSA data and add to them the additional information obtained from each state's highway safety office. This seems reasonable to do in that the two data sources provide similar figures. Comparing the comparable figures obtained from the two sources, we find that the differences are not more than 1–2 percentage points in more than 95% of the cases. In most cases, the figures were actually identical, which is not surprising, because the states' offices are generally the source for the NHTSA data. Throughout our analysis we use this combined data set for the reported results.

Some questions might be raised with respect to the reliability of the figures we obtained from each state's highway safety office and the NHTSA. One possible concern is that states might elect to report a higher usage than the actual one in order to win federal budgets that are promised to states that reach threshold levels. Another concern might be with respect to the comparability of the usage figures across states and over time; states use observational methods that vary somewhat even if they are similar in spirit, and some states have also changed over time the ways in which they conduct their observational surveys.

We take several steps to address some of these concerns. First, we try to mitigate any estimation problems that might arise from the nature of the usage data by using state and time fixed effects. We do so in order to control for state-specific biases that are fixed over time, as well as for biases that might result from changes in reporting requirements by NHTSA. Second, we use instrumental variables for the usage rate, thereby addressing any nonsystematic measurement errors. Finally, the use of the BRFSS data, which provide a similar but independent set of estimates, enables us to further verify the robustness of our results.[21]

IV. Empirical Strategy

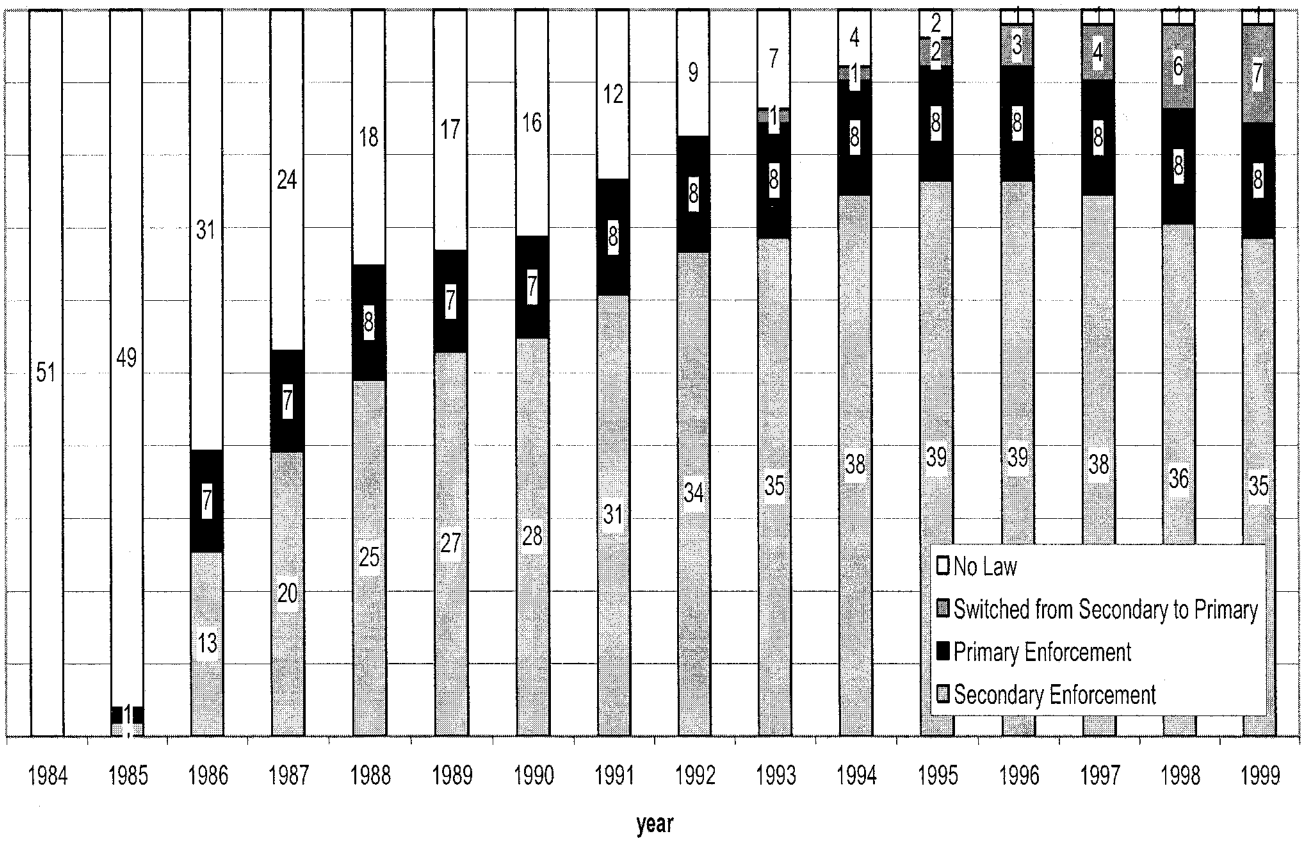
As discussed above, during our observation period, all U.S. states except New Hampshire passed mandatory seat belt laws. The variation in our data comes from the fact that states passed such laws at different times and adopted laws with different types of enforcement. Another variation comes from the fact that several states revised their laws, moving from secondary enforcement to primary enforcement.[22] Figure 3 displays the number of states with mandatory seat belt laws and the type of enforcement, as it evolved during our observation period. The fact that the adoption of mandatory seat belt laws was quite gradual helps us to identify their effects.

Our basic approach is to estimate a simple linear equation, with traffic fatalities as the dependent variable and with usage rate, control variables, and year and state fixed effects on the right-hand side.

[21] Because the BRFSS data were obtained independently of our actual usage data, a strong relationship between the two data sets confirms the reliability of our original usage data and the robustness of the results. Although the BRFSS data, with the weights we use, suggest significantly higher usage than the data we end up using, the correlation between the two data sets is remarkably high (correlation coefficients of 0.7 to 0.9 across different dimensions of the data). Figure 2 shows the increasing national level of usage over time, with the very similar trend in the usage as calculated from the BRFSS survey.

[22] The two other papers that use similar panel data sets did not have such richness, because of the time at which they were written. Evans and Graham (1991) had only data for 1984 to 1987, and Houston et al. (1995) covered the period from 1967 to 1991.

FIGURE 3.—LEGISLATION OVER TIME



The numbers inside the bars are the numbers of states having a particular type of law in a given year. Alabama switched from secondary to primary enforcement in October 1999 (not reflected in the graph). New Hampshire is the only state that has no general mandatory seat belt law.

We use year fixed effects to control for any time-specific *macro effects* that shift the level of traffic fatalities for all states. In our context, such macro effects might involve technological changes that introduced safer cars or national campaigns that affected the behavior of drivers across the nation. The time effects also capture the increased penetration of air bags over time.[23]

We use state fixed effects to capture any unobserved state characteristics that are fixed over time, such as population characteristics, general weather conditions, traffic conditions, and so forth. As pointed out in section II, these state fixed effects are important for mitigating the upward bias associated with the likely endogeneity of seat belt usage. For example, this bias can arise from the fact that in states with more dangerous traffic conditions (due to weather, say, or road conditions) people are more likely to use seat belts and are also more likely to be involved in a traffic accident.

Adding state fixed effects cannot, of course, completely eliminate endogeneity problems. The probable positive correlation between usage and the error term is likely to be lower once fixed effects are controlled for, but it might well not disappear. Conditions in any given state change over time. For example, states that experienced an increase in traffic fatalities might invest in promoting seat belt use. Such investments might lead to an increase in usage, which again might generate a positive correlation between usage and the error term and thereby introduce an upward bias to our estimated coefficient.

Therefore, it is worthwhile instrumenting for the usage. In our case, variables that are related to the mandatory seat belt laws are natural candidates for instrumental variables. Such laws are likely to be correlated with usage (after all, this is what the laws are for), and it also seems reasonable to assume that they are not correlated with the error term. As discussed earlier, mandatory seat belt laws are likely to affect traffic fatalities only through their effect on usage.

Still, a concern might remain with respect to the possible endogeneity of the mandatory seat belt laws. In particular, it might be argued that states that faced an increase in their traffic fatalities had a higher propensity to pass such laws. Although the above concern might be important for cross-sectional analysis,[24] we believe that, once we control for

[23] Air bag effects would not be captured completely by the year effects if there were cross effects between seat belts and air bags. However, it was suggested that these two protection devices are almost independent, in the sense that each is found useful in different types of accidents. See, for example, footnote 19 in Levitt and Porter (2001).

[24] For example, the results in Garbacz (1992) are likely to be driven by such endogeneity bias.

state and year fixed effects, this concern becomes much less important. Recall that all states except New Hampshire eventually passed a law, so that the considered concern might arise only with respect to the type of law passed and the time at which it was passed. The passage of the law is a political process that is likely to take some time and whose outcome may well depend on various political factors that are unlikely to be related to fatalities.[25]

The main opposition to the seat belt laws was based on arguments related to individual rights[26] and to discriminatory enforcement,[27] not to traffic fatalities. Thus, political, administrative, and ideological factors that had little to do with fatalities are likely to have been the primary factors that determined the time in which the legislation was passed in any given state. Indeed, a detailed survey of the process producing mandatory seat belt legislation (NHTSA, 1999) does not even mention high levels of traffic fatalities as having any influence on the passage of such legislation. Rather, this survey indicates that political and administrative factors played a decisive role in the timing of the passage of the law:

> Traffic safety measures were introduced when the agenda for the legislative session allowed it. Some sessions, highly influenced by the Governor's agenda, were dedicated to gun control issues or revenue concerns while others were concerned with traffic safety measures, making it the right time for the introduction of safety belt laws. Timing of legislative priorities was crucial to passage. In most cases, legislators who supported traffic safety issues were able to generate the necessary votes for only a limited number of such measures in any given legislative session. These issues included child passenger safety and speed limit initiatives. (NHTSA, 1999, p. 24)

In general, the procedure involved in passing seat belt legislation is long and complicated, making the timing of the law independent of the error term on fatalities. The independence assumption is especially reasonable once state and year fixed effects are included. It is also worth noting that the fatalities series are quite noisy, so that even a short delay in passing the law is sufficient to make the actual timing of the passage of the law satisfy standard exogeneity requirements.

Although there is no direct statistical test for the validity of the instruments, we run several tests and obtain results that are consistent with the above arguments. It is also important to note that the use of instrumental variables does not only address the endogeneity problem but also solves any estimation problems that might result from nonsystematic measurement errors in the usage variable (see section III).

We follow the existing literature and estimate the basic equation twice. We first use the number of occupant fatalities as the dependent variable, and then the number of nonoccupant fatalities. In the first regression we expect to obtain a negative coefficient on usage, which would be in line with the expected direct effect of seat belts as a protection device. The second regression tests for the compensating-behavior hypothesis. A positive coefficient on usage in the second regression would be consistent with the compensating-behavior hypothesis, whereas an insignificant or a negative coefficient would be inconsistent with the hypothesis.[28,29]

Our first-step regression, in which usage is regressed on the mandatory seat belt law variables (as well as on the other controls), also allows us to investigate which elements make seat belt laws more effective. The main difference among states' laws is in the type of enforcement (primary or secondary). In addition, some states have switched their type of enforcement from secondary to primary, providing an additional layer of variation in the data. This variation across states and over time enables us to perform our analysis. Of the 16 states with primary enforcement, 8 states passed the law with primary enforcement to begin with, and 8 states switched from secondary to primary enforcement after the initial adoption (see table 1). As mentioned earlier, there is a wide variation in usage rates among the different states. Laws also differ in which passengers are required to wear seat belts. Most states require only front seat passengers to wear seat belts, but a significant number of states (13 at present) require all passengers to do so. Fines also vary across states, from $0 in Rhode Island (verbal warning only) to $100 in Virginia. In addition, in some states auto insurance coverage for an accident will be reduced if the seat belt law was not complied with at the time of an accident. Therefore, we also estimate how usage levels are affected by the different elements of the law, the passage of time

[25] See Levitt (1996) for a similar argument, when using prison-overcrowding litigation as an instrument for the number of prisoners.

[26] The argument is that unlike other traffic violations, seat belt law violators do not put anyone else at risk, and hence should be free to choose whether to use a seat belt or not.

[27] The term "driving while black," was used in the media to bring attention to this. It is argued that police officers use mandatory seat belt laws, and in particular primary enforcement, in order to stop African-Americans and harass them. In practice, statistical studies have shown that this is not true.

[28] Note that much of the endogeneity problem for usage that is discussed in the text is less severe for the nonoccupant regression than it is for the occupant one. It is not obvious how an increase in nonoccupant fatalities would make drivers use more seat belts. However, it is likely that people do not always obtain (through the media, for example) separate statistics on traffic fatalities for occupants and nonoccupants. Hence, an increase in nonoccupant fatalities is predicted to affect seat belt decisions in a similar way to an increase in occupant fatalities, so endogeneity may still be an issue.

[29] Another way to test the Peltzman effect would be to look at changes in the *number of accidents* that result from an increase in the use of seat belt. The past work has not used changes in the number of accidents, because data on the number of accidents are viewed as problematic (in that "accidents" are not well defined). As will be discussed in section V, we also tested the Peltzman hypothesis using a limited panel data set on number of accidents and obtained results consistent with those we obtained using the number of nonoccupant fatalities.

TABLE 1.—A SUMMARY OF THE LAWS IN ALL STATES

State	Secondary Enforcement	Primary Enforcement	Usage: Before Law	Usage: Immediately After Law	Usage: Before Change in Law	Usage: Immediately After the Change	Usage: In 1998
AK	9/12/90		45%	66%			57%
AL	7/1/92	12/10/99	47%	58%			52%
AR	7/15/91		34%	52%			62%
AZ	1/1/91		55%	65%			62%
CA	1/1/86	1/1/93	26%	45%	67%	81%	89%
CO	7/1/87		NA	NA			66%
CT		1/1/86	NA	NA			70%
DC	12/12/85	10/9/97	NA	NA	66%	80%	80%
DE	1/1/92		54%	70%			62%
FL	7/1/86		34%	55%			57%
GA	9/1/88	7/1/96	20%	28%	51%	61%	74%
HI		12/16/85	33%	73%			81%
IA		7/1/86	18%	43%			77%
ID	7/1/86		24%	27%			57%
IL	7/1/85		16%	36%			65%
IN	7/1/87	7/1/98	20%	37%	62%		62%
KS	7/1/86		10%	24%			59%
KY	7/1/94		42%	58%			54%
LA	7/1/86	11/1/95	12%	35%	59%	63%	66%
MA	2/1/94[c]		34%	52%			51%
MD	7/1/86	10/1/97	18%	55%	71%	80%	83%
ME	12/27/95		50%	50%			61%
MI	7/1/85		26%	46%			70%
MN	8/1/86		20%	33%			64%
MO	9/28/85		NA	NA			60%
MS	3/20/90		NA	23%			58%
MT	10/1/87		40%	59%			73%
NC		10/1/85	26%	42%			77%
ND	7/14/94[c]		32%	42%			40%
NE	1/1/93[c]		33%	54%			65%
NH							56%
NJ	3/1/85		18%	40%			63%
NM		1/1/86	NA	NA			83%
NV	7/1/87		21%	34%			76%
NY		12/1/84	16%	52%			75%
OH	5/6/86		19%	49%			61%
OK	2/1/87	11/1/97	16%	35%			56%
OR		12/7/90[c]	50%	70%			83%
PA	11/23/87		NA	NA			58%
RI	7/1/91		28%	32%			59%
SC	7/1/89		NA	49%			65%
SD	1/1/95		40%	40%			46%
TN	4/21/86		NA	NA			57%
TX		9/1/85	NA	NA			74%
UT	4/28/86		NA	NA			67%
VA	1/1/88		33%	63%			74%
VT	1/1/94		54%	68%			63%
WA	6/11/86		36%	52%			79%
WI	12/1/87		26%	56%			62%
WV	9/1/93		33%	55%			57%
WY	6/8/89		NA	NA			50%
Total	42	16[a]					
Average			31%	48%	63%	73%	65%[b]

[a] 8 out of the 16 states which have primary enforcement first adopted secondary enforcement and then switched to primary enforcement.
[b] The sample mean is not weighted, and therefore differs from the national usage, which is 68% in 1998.
[c] In Massachusetts, Nebraska, North Dakota, and Oregon mandatory seat belt laws were passed, repealed, and reinstated again. In all these cases the time period between the original passage of the law and its repeal did not exceed two years. The dates provided above refer to the dates of the second passage of the law. Although not reflected in this table, these changes are taken into account in the empirical analysis.

TABLE 2.—THE EFFECT OF SEAT BELT USAGE ON *OCCUPANT* FATALITIES

Dependent Variable:	Occupant Fatalities per VMT			log(Occupant Fatalities per VMT)		
Independent Variable	OLS	State FE	IV	OLS	State FE	IV
Seat belt usage	0.0026**	−0.0027***	−0.0052***			
	0.0011	0.0010	0.0019			
log(seat belt usage)				0.114***	−0.053**	−0.133***
				0.029	0.022	0.047
log(median income)	−0.0162***	0.0125***	0.0122***	−0.988***	0.657***	0.624**
	0.0013	0.0043	0.0044	0.074	0.248	0.254
log(unemployment rate)	0.0018***	−0.0014***	−0.0015***	0.072***	−0.126***	−0.132***
	0.0004	0.0005	0.0004	0.026	0.029	0.030
log(mean age)	0.0072**	0.0009	0.0041	0.424**	1.072	1.343*
	0.0034	0.0145	0.0151	0.190	0.790	0.810
log(% blacks)	0.0006***	0.0019	0.0019	0.041***	0.080	0.075
	0.0002	0.0016	0.0016	0.011	0.084	0.086
log(% Hispanics)	0.0003*	−0.0010	−0.0008	0.012	−0.079	−0.0803
	0.00018	0.0012	0.0012	0.010	0.063	0.065
log(traffic density rural)	−0.0016***	−0.0022	−0.0020	−0.130***	−0.191*	−0.200*
	0.0003	0.0019	0.0020	0.019	0.104	0.106
log(traffic density urban)	0.0019**	0.0006	0.0005	0.177***	−0.151*	−0.167**
	0.0008	0.0013	0.0013	0.046	0.080	0.082
log(violent crimes)	0.0027***	−0.0005	−0.0006	0.151***	−0.037	−0.035
	0.0005	0.0011	0.0010	0.028	0.055	0.055
log(property crimes)	−0.0025***	0.0028**	0.0027**	−0.119***	0.280***	0.279***
	0.0006	0.0013	0.0013	0.032	0.080	0.081
log(VMT rural)	0.0013***	−0.0043**	−0.0042**	0.101***	0.051	0.100
	0.0002	0.0021	0.0021	0.013	0.116	0.118
log(VMT urban)	−0.0024***	−0.0082***	−0.0081***	−0.138***	−0.267***	−0.247***
	0.0004	0.0016	0.0016	0.023	0.084	0.085
log(fuel tax)	−0.0014***	−0.0019***	−0.0019***	−0.112***	−0.110***	−0.114***
	0.0004	0.0005	0.0005	0.024	0.030	0.031
65-mph speed limit	−0.0003	−0.0004	−0.0003	−0.028	−0.014	
	0.0004	0.0003	0.0003	0.026	0.021	0.022
70-mph speed limit or above	0.0008**	0.0012***	0.0012***	0.055**	0.087***	0.089***
	0.0004	0.0003	0.0003	0.025	0.016	0.017
MLDA of 21 years	−0.0015**	−0.0015***	−0.0014***	−0.048	−0.055***	−0.051**
	0.0008	0.0005	0.0005	0.034	0.021	0.023
BAC = 0.08	−0.0004	−0.0002	−0.0002	−0.011	−0.009	−0.012
	0.0003	0.0003	0.0003	0.018	0.016	0.017
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
State FE	No	Yes	Yes	No	Yes	Yes
N	556	556	556	556	556	556
Adj. R^2	0.7751	0.9305	0.9294	0.787	0.936	0.934

For exact definitions of the variables, refer to the data appendix.
***, **, *: Significant at 1%, 5%, and 10% confidence level, respectively.
The variables that we used as instruments for the third column are three dummy variables that stand for mandatory seat belt laws with secondary enforcement, primary enforcement, and primary enforcement that follows a switch from initially adopting secondary enforcement.
Robust standard errors below estimates.
As described in the text, note that the panel is not full for the early years (before 1990), which is why the number of observations is 556 rather than 765 (51 states over 15 years, 1983–1997).

since the adoption of the law, and the initial level of seat belt usage. We use a simple linear specification to do so, as suggested by a Box-Cox regression.[30]

V. Results

A. *The Effect of Seat Belt Use on Occupant Fatalities*

Table 2 presents a set of linear and log-log regressions of occupant fatalities on usage and other controls. The first and fourth columns report OLS regressions without controlling for state fixed effects. In both specifications the coefficient on usage is positive and significant, indicating that higher seat belt usage increases occupant fatalities. As argued before, this is likely to be the result of strong endogeneity of the usage variable. Indeed, once we control for state fixed effects, as reported in the second and fifth columns of table 2, the coefficient on usage changes its sign and becomes negative and statistically significant. However, the inclusion of state fixed effects corrects only for that part of the endogeneity problem that arises from cross-sectional differences across states. The usage variable is still likely to be positively correlated with the error term, and hence to be biased upwards, towards 0. Another potential source of bias in the coefficient on usage is that of measurement errors.

[30] An alternative specification would treat seat belt use as a dynamic decision by adding to the estimated first-step equation the lagged usage as a regressor. We briefly discuss this specification in section V. However, note that when using fixed effects one should be concerned that the coefficient on the lagged dependent variable would be biased because of the correlation between the lagged usage and the within error term. To remedy the bias, we instrument for it by using the lagged difference in usage.

This source of bias would also lead to a bias towards 0 in the coefficient.

Fortunately, by instrumenting for usage using the dummies of mandatory seat belt law and type of enforcement, we can solve both problems. Indeed, as reported in the third and sixth columns of table 2, once instrumented for, the usage coefficient becomes higher in absolute value, with a negative coefficient of -0.0052 for the linear specification and elasticity of about -0.13 for the log-log specification.[31]

The coefficients on the control variables deserve attention as well. Note the extreme change in most of them once we move from the simple OLS regression in the first and fourth columns to the fixed effects specification in the second and fifth columns. Because most of the variation in these variables is between states rather than within states (see the Data Appendix), the simple OLS results are driven by the cross-state variation, while the fixed-effects results are driven by within-state variation. Some of the views expressed in policy discussions seem to be driven by the cross-sectional variation, and they disappear once state fixed effects are controlled for.

The third and sixth columns of table 2 indicate that the coefficient on income is positive and that the coefficient on unemployment rate is negative. This suggests that traffic fatalities are lower in bad times, which is consistent with Ruhm's (2000) findings that mortality rates are lower during recessions. Note, however, that traffic density and VMT are controlled for, so Ruhm's suggestion that people drive more during booms cannot fully explain the results. Another possible factor might be that the opportunity cost of time is higher during booms, inducing faster driving.

The coefficients on the other demographic variables are not statistically significant once fixed effects are controlled for. This result is inconsistent with the widespread perception that African-Americans are involved in more fatal traffic accidents. That popular view shows up in the OLS results, but disappears once we control for state fixed effects.

Traffic density, in both rural and urban roads, has no significant effect in the linear specification but has a negative effect on fatalities in the log-log specification. Denser traffic might result in slower or more careful driving and hence less accidents. The interpretation of the coefficients on VMT is indirect because VMT is also used in the construction of the dependent variable. The effect of urban VMT is negative, and the effect of rural VMT is insignificant; this suggests that a mile traveled on rural roads is more dangerous than a mile traveled on urban roads, which seems plausible.

B. *The Effect of Seat Belt Use on Nonoccupant Fatalities*

Table 3 is identical to table 2 except for the dependent variable, which is now the nonoccupant fatalities. Again, it is easy to notice the dramatic changes in the coefficient on usage once we control for state fixed-effects. In the simple OLS regressions, for both specifications, the coefficient on usage is positive and statistically significant, which could be interpreted as an indication of compensating behavior. We view this as a replication of the results reported by Garbacz (1992b) and Risa (1994). Once state fixed effects are controlled for, however, the coefficient on usage becomes negative (and statistically significant in the log-log specification), suggesting the opposite story. This result is consistent with the story that the use of seat belts makes drivers more conscious of safety issues and thereby induces them to drive more carefully. Once we treat the usage as endogenous, the coefficient on usage becomes statistically insignificant in both specifications. Thus, we conclude that the effect of usage rate on nonoccupant fatalities is nonpositive, and that there is no strong support for the Peltzman effect.

Table 3 indicates that, whereas many of the coefficients on the control variables obtain significant coefficients when we do not control for state fixed effects, almost all of them become insignificant once we do so. This suggests that the within variation in nonoccupant fatalities is quite noisy, and it cannot be explained by our control variables. Another possibility is that we need other controls to explain nonoccupant fatalities, such as the number of bicyclists or pedestrians in each state or the level of activity (i.e., the equivalent of VMT) per bicyclist and pedestrian in each state. Such data, however, are not available. The only two controls that do have significant coefficients are on age and the dummy for high speed limits. The latter is positive, suggesting that higher speed limits are likely to lead to more nonoccupant fatalities. The coefficient on age is positive and very high. This may be the result of higher likelihood of nonoccupant fatalities (mainly of pedestrians) among elderly persons.

It might be suggested that bicyclists are subject to technological changes such as better bicycles and the introduction of bike helmets in the 1980s. Such changes might affect the interpretation of the results we obtain for nonoccupant fatalities. To deal with this concern, we ran the above regressions only for pedestrians, whose activity was presumably not subject to technological changes. The coefficient we obtain on usage remains insignificant as before.

The Peltzman effect could in theory be tested also by looking at the effects of seat belt laws on the *number of accidents* rather than the number of nonoccupant fatalities. The number of accidents presumably reflects changes in driving behavior. However, data on the number of accidents are viewed as problematic and have not been used heavily in the literature, because, unlike fatalities, "accidents" are not

[31] In fact, our data potentially allow us to distinguish between the two sources of bias. To eliminate the bias caused by measurement error without changing the endogeneity bias, we use the other measure of usage, the one obtained from the CDC, as an instrument for the usage rate. By doing so in the log-log specification, we obtain a coefficient of -0.097 on the usage, suggesting that about half of the bias in the fixed-effects OLS coefficient may be attributable to measurement errors, whereas the other half comes from endogeneity.

TABLE 3.—THE EFFECT OF SEAT BELT USAGE ON *NONOCCUPANT* FATALITIES

Dependent Variable:	Nonoccupant Fatalities per VMT			log(Nonoccupant Fatalities per VMT)		
	OLS	State FE	IV	OLS	State FE	IV
Seat belt usage	0.0011***	−0.0001	0.0007			
	0.0004	0.0004	0.0007			
log(seat belt usage)				0.158***	−0.119**	−0.042
				0.056	0.058	0.121
log(median income)	−0.0028***	−0.0018	−0.0017	−0.981***	−0.303	−0.270
	0.0005	0.0014	0.0013	0.149	0.555	0.558
log(unemployment rate)	0.0001	−0.0005***	−0.0005	0.108**	−0.037	−0.031
	0.0001	0.0002	0.0005	0.051	0.064	0.065
log(mean age)	0.0051***	0.0210***	0.0199***	0.522	6.089***	5.833***
	0.0017	0.0065	0.0063	0.456	2.210	2.153
log(% blacks)	0.0002***	0.0006	0.0006	0.074***	−0.391*	−0.386
	0.0006	0.0005	0.0005	0.022	0.234	0.236
log(% Hispanics)	0.0007***	0.0014***	0.0012**	0.206***	0.173	0.177
	0.0006	0.0004	0.0004	0.018	0.157	0.157
log(traffic density rural)	0.0002**	−0.0001	−0.0001	0.099***	−0.065	−0.057
	0.0001	0.0008	0.0008	0.034	0.303	0.301
log(traffic density urban)	0.0011***	0.0001	0.0001	0.415***	−0.239	−0.223
	0.0003	0.0007	0.0007	0.087	0.165	0.170
log(violent crimes)	0.0008***	0.0001	0.0001	0.313***	0.088	0.087
	0.0001	0.0003	0.0003	0.053	0.134	0.134
log(property crimes)	−0.0012***	−0.0001	−0.0001	−0.452***	−0.250	−0.249
	0.0002	0.0005	0.0005	0.068	0.179	0.179
log(VMT rural)	−0.0002***	−0.0008	−0.0008	−0.075***	−0.384	−0.431
	0.00008	0.0007	0.0007	0.024	0.272	0.275
log(VMT urban)	−0.0006***	−0.0006	−0.0006	−0.188***	0.091	0.072
	0.0001	0.0006	0.0006	0.052	0.185	0.187
log(fuel tax)	−0.0008***	−0.0001	−0.0001	−0.090*	0.039	0.043
	0.0002	0.0002	0.0002	0.049	0.062	0.062
65-mph speed limit	0.0001	−0.0001	−0.0001	−0.050	−0.034	−0.047
	0.0002	0.0002	0.0002	0.047	0.056	0.059
70-mph speed limit or above	0.00002	0.0004***	0.0004***	0.043	0.177***	0.174***
	0.0001	0.00009	0.0001	0.054	0.040	0.040
MLDA of 21 years	0.00003	0.000008	−0.00002	−0.016	0.016	0.013
	0.0003	0.0002	0.0002	0.075	0.055	0.055
BAC = 0.08	0.00018*	−0.0001	−0.00008	0.071	−0.050	−0.047
	0.0001	0.0001	0.0001	0.045	0.037	0.037
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
State FE	No	Yes	Yes	No	Yes	Yes
N	556	556	556	556	556	556
Adj. R^2	0.6805	0.9031	0.9018	0.686	0.881	0.880

For exact definitions of the variables, refer to the data appendix.
***, **, *: Significant at 1%, 5%, and 10% confidence level, respectively.
The variables that we used as instruments for the third column are three dummy variables that stand for MSBL with secondary enforcement, primary enforcement, and primary enforcement that follows a switch from initially adopting secondary enforcement.
Robust standard errors below estimates.
As described in the text, note that the panel is not full for the early years (before 1990), which is why the number of observations is 556 rather than 765 (51 states over 15 years, 1983–1997).

well defined.[32] Nevertheless, we used such data to check our previous results. Using panel data on 17 states over six years for the number of accidents (see Data Appendix), we find that the coefficient on usage was insignificant. This result is similar to the results presented in table 3, and it is consistent with those findings of ours that suggest that compensating behavior does not have a significant effect.

[32] In this case, for example, what we have is the count of police accident reports. While police are likely to be present at any serious accident, the arrival of police to the scene of a minor accident may depend greatly on the region, the time of day, the day of the week, and many other factors. Many crashes are not reported to police and therefore go undetected in state records. Studies have concluded that these cases make up a sizable portion of motor vehicle crashes [see Blincoe and Faigin (1992) and NHTSA (1994)].

C. *The Effect of Mandatory Seat Belt Laws on Seat Belt Usage*

Table 4 reports the results of regressing seat belt usage on the law variables and different controls. The first regression does not use state fixed effects, and the second regression does. We also consider several functional forms for the usage variable, but a Box-Cox regression, allowing the dependent variable to have flexible functional form, suggests that a linear specification is the most suitable functional form, with $L = 1.025$, insignificantly different from 1 ($p > 0.65$). It is worth noting that the linear specification can also be easily seen from looking at plots of the usage series state by state.

TABLE 4.—THE IMPACT OF MANDATORY SEAT BELT LAWS ON SEAT BELT USAGE

Dependent Variable: Independent Variable	Seat Belt Usage OLS	State FE
Secondary enforcement	0.131***	0.112***
	0.012	0.012
Primary enforcement	0.286***	0.219***
	0.015	0.024
Secondary to primary enforcement	0.122***	0.135***
	0.023	0.017
log(median income)	0.259***	0.015
	0.043	0.158
log(unemployment rate)	0.038***	0.006
	0.015	0.024
log(mean age)	0.111	0.418
	0.092	0.508
log(% blacks)	−0.008	0.048
	0.007	0.052
log(% Hispanics)	−0.003	−0.016
	0.005	0.052
log(traffic density rural)	−0.022**	0.067
	0.009	0.094
log(traffic density urban)	0.112***	0.025
	0.021	0.060
log(violent crimes)	−0.008	−0.036
	0.015	0.032
log(property crimes)	0.055***	−0.033
	0.018	0.051
log(VMT rural)	0.024***	0.087
	0.006	0.092
log(VMT urban)	0.015	0.027
	0.013	0.055
log(fuel tax)	−0.007	−0.043**
	0.015	0.022
65-mph speed limit	0.052***	0.023
	0.018	0.015
70-mph speed limit or above	−0.014	−0.005
	0.014	0.012
MLDA of 21 years	−0.024	0.011
	0.022	0.025
BAC = 0.08	0.012	−0.007
	0.011	0.013
Year FE	Yes	Yes
State FE	No	Yes
N	556	556
Adj. R^2	0.803	0.912

For exact definitions of the variables, refer to the data appendix.
***, **, *: Significant at 1%, 5%, and 10% confidence level, respectively.
In unreported regressions we run identical regressions using the CDC usage data and obtain similar results.
As described in the text, note that the panel is not full for the early years (before 1990), which is why the number of observations is 556 rather than 765 (51 states over 15 years, 1983–1997).
A Box-Cox regression (as well as inspection of the graphs) suggests that a linear specification of the dependent variable is much more appropriate.

As expected, mandatory seat belt laws significantly increase the seat belt usage rate, and primary enforcement does this more effectively than secondary enforcement. Primary enforcement increases usage by about 22 percentage points, whereas secondary enforcement increases it by only half as much. Switching from secondary to primary enforcement increases the usage by about 13 percentage points. Our results suggest that adopting primary enforcement from the beginning has a similar ultimate effect on usage to that of first adopting secondary enforcement and then switching to primary enforcement. Indeed, we cannot reject the hypothesis that both strategies have the same ultimate effect—the sum of the coefficients on secondary enforcement and on the switch to primary enforcement is not significantly different from the coefficient on primary enforcement.

It is also worthwhile reporting the variables that we exclude from our final regression. Some researchers suggest that the short-term effects of mandatory seat belt laws might differ from the long-term effects. We therefore defined new variables that were equal to the time passed since the law was adopted for each one of the enforcement types. None of these variables was found to be significant. Thus, the effect of the law appears to be *immediate* and *permanent*. This phenomenon can also be verified by looking at usage plots state by state.

We also tested the significance of other components of the law. We found that the coefficient on who is required to use seat belts is not significant.[33] Similarly, the coefficients on whether or not wearing a seat belt will reduce insurance coverage in the event of an accident, and the coefficient on the amount of the fine, turn out to be insignificant as well.

In addition, most of our control variables lose significance once state fixed effects are included. Moreover, the percentage of African-Americans or Hispanics is not significant, even when state fixed effects are not included. This contradicts the common view that minorities are more likely not to use seat belts and hence should be specifically targeted in seat belt campaigns.

Although traffic fatalities seem to be a static variable, seat belt usage might have some dynamic effects as well. Therefore, we also estimated a regression using lagged seat belt usage as an explanatory variable. We find that the lagged usage variable turns out to be significant, taking about one-third of the explanatory power from the law-related dummies, and suggesting some habitual behavior. However, given that the laws generally moved in one direction, from not having a law to having one or from having secondary enforcement to having primary enforcement, it is not clear that our data can distinguish between dynamic and static effects. Ideally, one would use individual-level data to make that distinction. Alternatively, one may want to examine the four cases in which the law was repealed. In all those cases, unfortunately, the time between the changes did not exceed two years, which precluded using it for identification given our annual usage data. Still, the few data from these episodes are consistent with the view that static decisions are more important than dynamic habits; usage rates dropped drastically with the repeal of the laws and increased immediately after the laws were reinstated.

D. *Robustness and Specification Tests*

We have used different specifications and have run different tests to check whether our results are robust. We have

[33] Recall that our usage data are on front seat passengers only, so this result is not very surprising. It is quite likely that this component of the law would affect the usage rate of rear seat passengers, but the available data do not allow us to test this.

also done so to examine whether our assumptions regarding the validity of the instruments and the functional form are supported by the data.[34]

First, we tested different sets of controls by omitting and adding variables to the set of controls reported in tables 2 and 3. Additional controls were proxies for the state population's level of risk aversion, such as diet, smoking habits, and frequency of athletic exercising, that were obtained from the BRFSS data. Other controls that we included are the number of licensed drivers and registered vehicles. We also included the fraction of new car registrations in order to capture differences in the safety level of cars resulting from different distributions of vintages of cars across states, and the fraction of trucks among all registered vehicles. None of the additional controls significantly influenced our estimates. We also ran the same specification after replacing our usage data with those of the BRFSS. The results had the same patterns for the coefficients, as well as similar point estimates. In addition, we ran a first-difference regression instead of a fixed-effects regression to take into account serial correlation problems, and the results were not much changed (although their significance levels decreased).

We have also devoted attention to the issue of functional form. Earlier studies showed that less careful drivers are less likely to use seat belts. Thus, drivers that are least likely to use seat belts might be those that are more likely to be involved in an accident. If this is the case, then it can be expected that increasing seat belt usage from, say, 80% to 90% will save more lives than the lives saved by increasing usage from 20% to 30%. In other words, it might be argued that the relationship between fatalities and usage rate is concave. In contrast, our log-log specification assumes a convex relationship; the log-log specification implies that the same percentage point increase in usage is more effective at low levels of usage. In examining whether this poses a problem for our conclusions, note first that the linear specification provides very similar results to the log-log specification. Moreover, as a test, we included in our log-log specification two more variables—a linear term for usage, and the logarithm of 1 minus the usage. The coefficients on these variables turned out to be very low and completely insignificant, without much change in the estimated coefficient on the original logarithmic term, thus providing support for the log-log specification.

Another possible concern is that our identification for the usage variable might be driven by long-term within-state differences and not by the actual change in the mandatory seat belt laws. In unreported regressions we estimate the coefficients on usage for both occupant and nonoccupant regressions for different choices of time windows around the passage (or change) of the mandatory seat belt law. Although the coefficients are naturally not the same, their magnitude and their significance level are quite stable over the different time windows, and are quite similar to those obtained in our reported regressions (third and sixth columns of tables 2 and 3). This is also the case if we omit from our sample the years around the law, which addresses any possible concern that the exact timing of the law is not well identified by the annual level dummy variable.

TABLE 5.—REDUCED-FORM REGRESSION

Dependent Variable:	Fatalities per VMT		log(Fatalities per VMT)	
Independent Variable	Occupant Regression	Nonoccupant Regression	Occupant Regression	Nonoccupant Regression
Secondary dummy	−0.0007***	0.00007	−0.042***	0.0005
	0.0002	0.00009	0.013	0.032
Primary dummy	−0.0012***	−0.0001	−0.061***	0.084**
	0.0005	0.0002	0.022	0.042
Secondary to primary dummy	−0.0002	0.0003	−0.030	0.086
	0.0004	0.0002	0.024	0.063
N	765	765	765	765
Adj. R^2	0.9227	0.8849	0.929	0.875

The regressions are identical to those reported in tables 2 and 3, but use the instrumental variables (the law dummies) as regressors, rather than the (endogenous) seat belt usage. All other control variables and state fixed-effects are included.
***, **, *: significant with 1%, 5% and 10% confidence level, respectively.
Robust standard errors below estimates.

We also investigate statistically the validity of the instruments. We argued earlier that the timing of the passage of the law is exogenously determined and is unrelated to preceding trends in fatalities. To test this, we run different hazard models, looking for a relationship between the passage of the law and some trend in fatalities that may have preceded it (controlling for observable variables and state fixed effects). We find no statistically significant evidence that such a relationship exists, which is consistent with our discussion in section IV and with the assumption that the timing of the passage of seat belt legislation is exogenous.

Similarly, one might be concerned that mandatory seat belt laws have effects on fatalities other than their effect through the increase in usage. For example, the passage of mandatory seat belt laws might be accompanied by a general campaign for traffic safety, thus making drivers more attentive to safety issues. Although there is no formal test to check whether this is the case, table 5 reports the reduced-form coefficients when fatalities are regressed directly on the instruments, namely the law dummy variables. If the assumptions are correct and the laws affect fatalities only through seat belt usage, then it will be possible to approximate their effect on fatalities by calculating it indirectly through the effect on usage (as can be calculated from the results reported in tables 2, 3, and 4). Indeed, back-of-the-envelope calculations suggest that there is no major difference in the order of magnitude of the *average* effect of the law if we calculate it from this regression rather than from the results reported in tables 2, 3, and 4.

The only remaining puzzle concerns the positive and highly significant coefficient on the primary enforcement dummy for the nonoccupant regression in the logarithmic regression reported in table 5. This coefficient seems quite

[34] Full regression reports of these tests will be provided by the authors upon request.

robust, even when we include usage in the regression, as well as for different choices of subsamples.[35] Taken at face value, this coefficient might suggest that some sort of Peltzman effect does exist, and it replicates some of the results in the existing literature. However, the result disappears with a linear specification, and given our results for usage, it seems that this is not the right interpretation, but rather that there is something else, left unexplained, that makes nonoccupant fatalities go up when primary enforcement laws are passed. This highlights the importance of our point that incorporating usage data is essential for a rigorous test of the Peltzman effect.

VI. Conclusions

This paper uses a unique data set on seat belt usage in order to estimate the effectiveness of mandatory seat belt laws. In contrast to earlier work, we analyze separately the effect of such laws on seat belt usage levels and the effects of usage on fatalities, we allow for the endogeneity of usage, and we take advantage of the variation in the laws across states. Along the way, we replicate certain results obtained in past work and show why the inference of significant compensating behavior drawn from them was not always warranted.

Our data set and empirical strategy enable us to test directly the theory of compensating behavior suggested by Peltzman (1975). In contrast to the predictions of the theory, we do not find any evidence that higher seat belt usage has a significant effect on driving behavior.

Our results indicate that, overall, mandatory seat belt laws unambiguously reduce traffic fatalities. We estimate that a 1-percentage-point increase in usage saves 136 lives annually (using a linear specification), and that a 1% increase in usage reduces annual fatalities by about 0.13% (using a log-log specification). These estimates imply that about 1500–3000 lives would be saved annually if the national seat belt usage were to increase from 68% to the (still unattained) target level of 90%.

Our estimates of the potential savings in lives from increased seat belt usage are less than half of the estimate used by the National Highway Traffic Safety Administration (NHTSA). The significant difference between these estimates results from the fact that the NHTSA uses 45% as the estimated elasticity of seat belt usage. This value is based on the actual usage as estimated from that of drivers involved in traffic fatalities. This estimate differs from the one calculated in observational studies and used for the calculation of the national usage rate. Thus, our estimates provide a better guide for policymaking in this area than the estimates currently used by policymakers.

Finally, our work enables us to identify and measure the effects of various features of mandatory seat belt laws on the laws' effectiveness in increasing seat belt use. In particular, having primary enforcement can considerably enhance this effectiveness. Our estimates indicate that the national usage would increase from 68% to 77%, and 500–1200 lives would be saved annually, if all states now having secondary enforcement moved to primary enforcement. Thus, the recent initiative by the federal government to encourage states to adopt primary enforcement is worthwhile.

[35] In fact, the significance of the primary dummy in the nonoccupant regression, together with the insignificance of the usage rate in the IV regression, makes the primary dummy invalid as an instrument in the nonoccupant regression that is reported in table 3. Still, the secondary dummy remains a valid instrument, and using it alone as an instrument for the nonoccupant regression does not alter the results.

REFERENCES

Asch, Peter, David T. Levy, Dennis Shea, and Howard Bodenhorn, "Risk Compensation and the Effectiveness of Safety Belt Use Laws: A Case Study of New Jersey," *Policy Sciences* 24:2 (1991), 181–197.

Bhattacharyya, M. N., and Allan P. Layton, "Effectiveness of Seat Belt Legislation on the Queensland Road Toll—An Australian Case Study in Intervention Analysis," *Journal of the American Statistical Association* 74:367 (1979), 596–603.

Blincoe, Lawrence J., and B. M. Faigin, "The Economic Costs of Motor Vehicle Crashes 1990," HS 807-876, *U.S. Department of Transportation* (1992).

Campbell, B. J., "The Association between Enforcement and Seat Belt Use," *Journal of Safety Research* 19 (1988), 159–163.

Campbell, B., J. Richard Stewart, and Frances A. Campbell, "Early Results of Seat Belt Legislation in the United States of America," University of North Carolina Highway Safety Research Center working paper no. A-123, Chapel Hill, NC (1986).

Center for Disease Control, "Behavioral Risk Factor Surveillance—Selected States, 1984," *Morbidity and Mortality Weekly Report* 35 (1986), 253–254.

Dee, Thomas S., "Reconsidering the Effects of Seat Belt Laws and Their Enforcement Status," *Accident Analysis and Prevention* 30 (1998), 1–10.

Department of Transportation, "Regulatory Impact Analysis of FMVSS 208: Occupant Crash Protection," Washington, DC (1984).

Derrig, Richard A., Maria Segui-Gomes, Ali Abtahi, and Liu Ling-Ling, "The Effect of Population Safety Belt Usage Rate on Motor Vehicle–Related Fatalities," *Accident Analysis and Prevention* 34 (2002), 101–110.

Evans, Leonard, "The Effectiveness of Safety Belts in Preventing Fatalities," *Accident Analysis and Prevention* 18 (1986), 229–241.

——— "Belted and Unbelted Drivers Accident Involvement Rates Compared," *Journal of Safety Research* 18 (1987), 57–64.

Evans, Leonard, and Paul Wasielewski, "Risky Driving Related to Driver and Vehicle Characteristics," *Accident Analysis and Prevention* 15 (1983), 121–136.

Evans, William N., and John D. Graham, "Risk Reduction or Risk Compensation? The Case of Mandatory Safety-Belt Use Laws," *Journal of Risk and Uncertainty* 4:1 (1991), 61–73.

Garbacz, Christopher, "Estimating Seat Belt Effectiveness with Seat Belt Usage Data from the Center for Disease Control," *Economics Letters* 34 (1990a), 83–88.

——— "How Effective is Automobile Safety Legislation?" *Applied Economics* 22 (1990b), 1705–1714.

——— "Impact of the New Zealand Seat Belt Law," *Economic Inquiry* 29 (1991), 310–316.

——— "More Evidence on the Effectiveness of Seat Belt Laws," *Applied Economics* 24 (1992), 313–315.

Graham, John D., Kimberly M. Thompson, Sue J. Goldie, Maria Segui-Gomez, and Milton C. Weinstein, "The Cost-Effectiveness of Air Bags by Seating Position," *Journal of American Medical Association* 278:17 (1997), 1418–1425.

Harvey, Andrew C., and James Durbin, "The Effect of Seat Belt Legislation on British Road Casualties: A Case Study in Structural Time Series Modeling," *Journal of the Royal Statistical Society Series A—Statistics in Society* 149:3 (1986), 187–210.

Houston, David J., Lilliard E. Richardson Jr., and Grant W. Neeley, "Legislation Traffic Safety: A Pooled Time Series Analysis," *Social Science Quarterly* 76:2 (1995), 328–345.
Hunter, William W., Jane C. Stutts, J. Richard Stewart, and Eric A. Rodgman, "Characteristics of Seat Belt Users and Non-users in a State with a Mandatory Belt Use Law," *Health Education Research: Theory and Practice* 5 (1990), 161–173.
Insurance Information Institute, *The Fact Book: Property/Casualty Insurance Facts,* Annual Publication (1990–1998).
Levitt, Steven D., "The Effect of Prison Population Size on Crime Rates: Evidence from Prison Overcrowding Litigation," *The Quarterly Journal of Economics* 111:2 (1996), 319–351.
Levitt, S. D., and J. Porter, "Sample Selection in the Estimation of Air Bags and Seat Belt Effectiveness," this REVIEW 83:4 (2001), 603–615.
Loeb, Peter D., "The Effectiveness of Seat Belt Legislation in Reducing Injury Rates in Texas," *Economic Analysis of Safety and Health Regulation* 85:2 (1995), 81–84.
McCarthy, Patrick S., "Public Policy and Highway Safety: A City-Wide Perspective," *Regional Science and Urban Economics* 29 (1999), 231–244.
National Highway Traffic Safety Administration, *Estimating the Benefits from Increased Safety Belt Use* (Washington, DC, Department of Transportation, 1994).
——— *Legislative History of Recent Primary Safety Belt Laws* (Washington, DC, Department of Transportation, 1999).
Patryka, S., "Mandatory Belt Use Laws in 1985," *Journal of The American Association for Automotive Medicine* 9 (1987), 10.
Peltzman, Sam, "The Effects of Automobile Safety Regulation," *Journal of Political Economy* 83 (1975), 667–725.
——— "The Effects of Automobile Safety Regulation: Reply," *Journal of Economic Issues* 11:3 (1977), 672–678.
Risa, Alf Erling, "Adverse Incentives from Improved Technology: Traffic Safety Regulation in Norway," *Southern Economic Journal* 60:4 (1994), 844–857.
Ruhm, Christopher J., "Are Recessions Good for Your Health?" *The Quarterly Journal of Economics* 115:2 (2000), 617–650.
Sen, Anindya, "An Empirical Test of the Offset Hypothesis," *Journal of Law and Economics* 44:2 (2001), 481–510.
Singh, Harinder, and Mark Thayer, "Impact of Seat Belt on Driving Behavior," *Economic Inquiry* 30 (1992), 649–658.
Traynor, Thomas L., "The Peltzman Hypothesis Revisited: An Isolated Evaluation of Offsetting Driver Behavior," *Journal of Risk and Uncertainty* 7:2 (1993), 237–247.
Wagenaar, Alexander C., Richard G. Maybee, and Kathleen P. Sullivan, "Mandatory Seat Belt Laws in Eight States: A Time-Series Evaluation," *Journal of Safety Research* 19 (1988), 51–70.
Wilde, Gerald J. S., "The Theory of Risk Homeostasis: Implications for Safety and Health," *Risk Analysis* 2 (1982), 209–225.

DATA APPENDIX

In this appendix, we describe all the variables used in the analysis (those which are used in the reported regression and those which were eventually excluded), motivate their use when necessary, and provide descriptive statistics for the continuous variables.

1. Fatalities

1.a Data obtained from the Fatalities Analysis Reporting System (FARS)

- *Nonoccupant fatalities*—the number of traffic fatalities of pedestrians and bicyclists.
- *Occupant fatalities*—the number of traffic fatalities of drivers and passengers (of any seating position) of a motor vehicle in transport.

2. Controls

2.a Data obtained from the U.S. Census

- *% Blacks*—the percentage of African-Americans in the state population.
- *% Hispanics*—the percentage of people of Hispanic origin in the state population.
- *Mean age*—in years.
- *Median income*—in current U.S. dollars.

2.b Data obtained from the annual publication Highway Statistics

- *Traffic density rural*—registered vehicles per unit length of rural roads in miles.
- *Traffic density urban*—registered vehicles per unit length of urban roads in miles.
- *VMT rural*—vehicle miles traveled on rural roads.
- *VMT urban*—vehicle miles traveled on urban roads.

2.c Data obtained from the Bureau of Labor Statistics (BLS)

- *Unemployment rate.*[36]

2.d Data obtained from the Department of Justice

- *Violent crimes*—number of violent crimes per capita (homicide, rape, and robbery).[37]
- *Property crimes*—number of property crimes per capita (burglary, larceny, and auto theft).[37]

3. Other Related Laws[38]

3.a Data obtained from Insurance Information Institute (1990–1998)

- *65 mph speed limit*—a dummy variable that is equal to 1 for 65-mph top speed limit in the state (55 mph is the base category).
- *70 mph speed limit or above*—a dummy variable that is equal to 1 for 70-mph or higher top speed limit in the state (55 mph is the base category).
- *BAC is 0.08*—a dummy variable that is equal to one for a maximum of 0.08 blood alcohol content (0.1 is the base category).
- *Fuel tax*—the tax on fuel (in current cents).
- *MLDA of 21 years*—a dummy variable that is equal to one for a minimum legal drinking age of 21 years (18 years is the base category).

4. Elements of the Mandatory Seat Belt Law

4.a Data obtained from Insurance Information Institute (1990–1998)

- *Secondary dummy*—a dummy variable that is equal to 1 for the periods in which the state had a secondary-enforcement mandatory

[36] State-level data on unemployment rates are supposed to capture general economic conditions, and they indeed proved to be important in previous papers in explaining fatalities.
[37] We obtained data on crime (split between violent crime and property crime) in order to capture unobserved characteristics of the population, as well as police activity or law enforcement level. There can be two different interpretations for the crime data as a proxy for police activity. One might suggest that a higher crime rate is likely to be associated with a larger police force, which in turn makes violating the traffic laws more difficult. One might also suggest that a higher crime rate shifts police away from enforcement of traffic laws, and hence makes violations easier. Given that we use the crime rate only as a control variable, we do not discuss these two effects.
[38] We obtained data on the status of other relevant laws—such as speed limits, limits on alcohol drinking while driving, minimum legal drinking age, and tax on fuel—in order to isolate the effect of mandatory seat belt laws from those of other laws that might have a direct or indirect effect on driving.

TABLE A1.—DESCRIPTIVE STATISTICS FOR THE CONTINUOUS VARIABLE

Variable	Mean	Std. Dev.	Min	Max	Within Std. Dev.	Number of Observations[a]
% Blacks	10.79%	12.05%	0.25%	68.86%	0.43%	765
% Hispanics	5.44%	7.45%	0.47%	39.92%	0.85%	765
Mean age	35.14	1.70	28.23	39.17	0.68	765
Median income	17,992	4,811	8,372	35,863	3,852	765
Traffic density rural	0.33	0.22	NA[b]	1.11	0.05	765
Traffic density urban	1.52	0.52	0.62	3.74	0.15	765
VMT rural	16,566	12,588	NA[b]	64,939	2,252	765
VMT urban	24,882	33,246	980	230,541	6,108	765
Unemployment rate	6.25	2.05	2.23	18.02	1.53	765
Violent crimes[c]	0.30	0.65	0.02	5.06	0.14	765
Property crimes[c]	3.00	5.71	0.13	42.78	0.53	765
Fuel tax	16.24	4.96	5.00	39.00	3.60	765
Seat belt usage	52.89%	17.02%	6.00%	87.00%	13.43%	556
CDC usage	71.04%	15.50%	27.78%	95.24%	11.62%	485
Occupant fatalities	707.85	695.80	24.00	4,398.00	101.38	765
Nonoccupant fatalities	139.12	188.96	3.00	1,220.00	26.20	765
Occupant fatalities per VMT[d]	18.34	5.53	6.34	37.52	3.41	765
Nonoccupant fatalities per VMT[d]	3.15	1.63	0.46	10.27	0.94	765

[a] All variables have 765 observations, which stand for 15 years (1983–1997), in the 51 states. The seat belt usage is the only exception, and is not fully covered for the early years (see section III).
[b] There are no rural areas in the District of Columbia.
[c] Number of crimes per 1,000 people.
[d] Fatalities per 1,000 vehicle miles traveled.

seat belt law, or a primary-enforcement law that preceded by a secondary-enforcement law (no seat belt law is the base category).

- *Primary dummy*—a dummy variable that is equal to 1 for the periods in which the state had a primary-enforcement mandatory seat belt law that was *not* preceded by a secondary-enforcement law (no seat belt law is the base category).
- *Secondary-to-primary dummy*—a dummy variable that is equal to 1 for the periods in which the state had a primary-enforcement mandatory seat belt law that was preceded by a secondary enforcement law (no seat belt law is the base category).

5. Usage

5.a *Data obtained from states' observational surveys, from the National Highway Traffic Safety Administration (NHTSA), and from the Center for Disease Control (CDC) (for more details see Section III).*

- *CDC seat belt usage*—the frequency of seat belt usage, as self-reported by state population surveyed.
- *Seat belt usage*—the observed percentage of front-seat passengers who use seat belts.