Available online at www.sciencedirect.com
SCIENCE DIRECT
Economics Letters 79 (2003) 401–408
economics letters
www.elsevier.com/locate/econbase

The fatality effects of highway speed limits by gender and age[☆]

Thomas S. Dee[a,*], Rebecca J. Sela[b]

[a]Department of Economics, Swarthmore College and NBER, Swarthmore, PA 19081, USA
[b]Federal Reserve Bank of New York, New York, NY, USA

Received 24 July 2002; received in revised form 31 October 2002; accepted 16 December 2002

Abstract

This study presents panel-based evidence on the overall fatality consequences of recent speed-limit increases in the United States. The results suggest that higher speed limits had highly heterogeneous effects, generally increasing fatalities among women and the elderly but reducing them among males.
© 2003 Elsevier Science B.V. All rights reserved.

Keywords: Speed limits; Traffic fatalities; Age; Gender

JEL classification: I18; H73; K00

1. Introduction

In 1974, the federal government established a National Maximum Speed Limit (NMSL) of 55 miles per hour (MPH) in response to concerns about energy conservation. In 1987, the NMSL was increased to 65 MPH and, by the end of that year, 38 states increased their maximum speed limit to this amount. A second round of speed limit increases occurred after the NMSL was entirely repealed in 1995, allowing states to choose their own maximum speed limits. By 1999, 29 states had maximum speed limits of 70 MPH or higher.[1] The public perception of the desirability of these policies has

[☆]The views expressed in the article are not necessarily reflective of the views at the Federal Reserve Bank of New York or the Federal Reserve System.

*Corresponding author. Tel.: +1-610-690-5767; fax: +1-610-328-7352.
E-mail address: dee@swarthmore.edu (T.S. Dee).

[1]And, in Montana, the current law only requires that drivers use ‘reasonable and prudent’ speeds on interstates during daytime (65 MPH at night). In the evaluation results presented here, we treat Montana as if it had a 70 MPH or higher speed limit. However, we found that our results were not sensitive to simply excluding observations from this state.

0165-1765/03/$ – see front matter © 2003 Elsevier Science B.V. All rights reserved.
doi:10.1016/S0165-1765(03)00026-0

simply focused on a perceived tradeoff between saving time and saving lives (Haight, 1998).[2] In other words, this discussion has been based on the implicit assumption that 'speed kills.' However, the contentious academic literature in this area has focused instead on the more fundamental issue of whether higher speed limits have positive or negative effects on traffic safety.

The conventional view is that the higher speed limits reduce traffic safety by increasing the frequency and severity of collisions. However, there are several ways in which higher speed limits could actually promote traffic safety. For example, several studies (e.g., Lave, 1985; Graves et al., 1993) have noted that higher speed limits may instead reduce the probability of collisions by reducing the variance of highway speeds (i.e., 'variance kills'). Lave and Elias (1994, 1997) also criticized earlier empirical studies of the fatality consequences of the new 65 MPH speed limits for focusing only on the 'local' consequences for the highways directly affected by the limits (typically, rural interstates). They conclude that the overall effect of higher limits has been to reduce fatalities. They also discuss some supporting evidence that these results may be due to two 'system-wide' effects. One is that reducing the compliance requirements associated with low, federally mandated speed limits may have allowed state police forces to focus their scarce resources on activities that were more effective in reducing fatalities (e.g., enforcing drunk-driving laws). Second, they suggest that higher speed limits may have diverted traffic from relatively hazardous secondary roads to interstates.[3]

This study presents new evidence on the effects of increased speed limits on statewide fatalities. These results are based on annual state-level panel data on traffic fatality rates from 1982 to 1999. The results based on these extended panel data make three distinct contributions to this literature. First, we examine the possible sensitivity of speed limit evaluations to the variety of empirical specifications employed in prior studies. Second, we consider the fatality effects of the most recent speed limit increases (to 70 MPH or higher), which occurred since the NMSL repeal.[4] Third, we present evidence on whether the fatality effects of higher speed limits varied by the age and gender of the victims. This existence of this sort of response heterogeneity is plausible since key driving behaviors (e.g., types of risk-taking), driving abilities (e.g., experience, vision and motor skills) and patterns of road usage (e.g., secondary roads versus interstates) differ considerably by these demographic traits (e.g., Grabowski and Morrisey, 2001).

2. Data and specifications

We generated state-by-year data on total traffic-related fatalities for 48 states (Alaska and Hawaii excluded) over the 1982–1999 period (864 observations) from the Fatality Analysis Reporting System

[2] A recent empirical study by Ashenfelter and Greenstone (2002) examines this tradeoff and concludes that the value of time saved because of the movement to 65 MPH speed limits (and the number of lives lost) suggests that states placed a value of $1.54 million (1997 $) on a statistical life.

[3] In the most recent contribution to this literature, Greenstone (2002) finds, using 1982–1990 data, that the introduction of a 65 MPH speed limit increased fatalities on rural interstates but reduced them on urban non-interstates. However, Greenstone (2002) rejects the hypothesis that this decline can be attributed to police reallocations since state police have limited enforcement responsibilities for these urban roads.

[4] Farmer et al. (1999), using data that extend only 2 years after the NMSL repeal, conclude that these new speed limits increased overall fatality rates by 2.9%. However, those results are based exclusively on time-series models and include limited controls for other state policy changes.

(FARS). The FARS, which is administered by the US Department of Transportation, contains fairly detailed data on every traffic accident involving a fatality in the US Combining the FARS data with the Census Bureau's state, year, age and gender-specific population estimates, we created 11 different traffic fatality rates (Table 1).[5] The two key independent variables in these evaluations are binary indicators equal to one for states in years when they have a maximum speed limit of 65 and 70 MPH or higher (Table 1).[6] For states and years, where the limits become effective at some point during the year, we generated fractional values. Our control variables include two binary indicators for mandatory seat belt laws, which most states also introduced over this period. Seat belt laws with primary enforcement allow the police to stop a motorist for not wearing a seat belt. Secondary enforcement implies that a violation can only be assessed if the driver were pulled over for some other

Table 1
Variable means, State Panel data, 1982–1999

Variable	Mean (S.D.)
Traffic fatality rates per 100 000 in population	
Total	22.740 (7.219)
Males	32.788 (10.845)
Males, aged 16–24	54.275 (17.761)
Males, aged 25–44	30.649 (11.552)
Males, aged 45–64	21.699 (8.565)
Males, aged 65 and over	31.622 (10.304)
Females	13.401 (4.286)
Females, aged 16–24	20.686 (7.697)
Females, aged 25–44	11.079 (4.697)
Females, aged 45–64	9.991 (3.832)
Females, aged 65 and over	16.187 (5.137)
65 MPH speed limit	0.501 (0.483)
70+ MPH speed limit	0.120 (0.316)
Mandatory Seat Belt Law—Primary Enforcement	0.200 (0.400)
Mandatory Seat Belt Law—Secondary Enforcement	0.443 (0.497)
State Unemployment Rate	0.061 (0.022)
Administrative License Revocation	0.529 (0.493)
Illegal per se at 0.10 or higher BAC	0.724 (0.437)
Illegal per se at 0.08 BAC	0.128 (0.330)
Number of observations	864

[5] Several studies in this area use vehicle miles traveled instead of population in calculating a fatality rate. However, since miles traveled may respond to changed speed limits and this study focuses on age- and gender-specific responses, population is the more appropriate exposure variable.

[6] We identified these state-year limits by drawing on several sources including Farmer et al. (1999), the Statistical Abstract of the United States, the Insurance Institute for Highway Safety and state-specific searches of newspaper accounts in Lexis-Nexis.

reason. Most prior research finds that these laws increased belt usage and reduced fatality risk (e.g., Sen, 2001). The next control variable, the state unemployment rate, is frequently included in such evaluations because it captures the cyclical variation in traffic safety related to the amount of road usage (e.g., Evans and Graham, 1988). We also include as controls three variables related to the key drunk-driving laws that changed over this period (DeJong and Hingson, 1998).[7] One binary variable identifies whether, in a given year, the state licensing authority is allowed to suspend a driver's license prior to any court action related to a charge of drunk driving (i.e., an 'administrative license revocation'). Two other binary indicators identify the existence of a state law establishing a minimum blood alcohol concentration (BAC) at which it is 'illegal per se' to drive.

The preferred empirical specification for these data is a two-way fixed effect model taking the following basic form:

$$Y_{st} = X_{st}\boldsymbol{\beta} + \boldsymbol{\gamma}\mathbf{Z}_{st} + w_s + v_t + \varepsilon_{st} \qquad (1)$$

where Y_{st} is the dependent variable, X_{st} contains the control variables, $\mathbf{Z}_{st}$ consists of the speed limit variables. The terms, w_s and v_t, are state-specific and year-specific fixed effects and ε_{st} is a mean-zero random error. The results reported here are based on a semi-log model in which Y_{st} is the natural log of the fatality rate per 100 000 in the population in a given state and year.[8] Since the units of observation are of varying sizes, the error term in this model is likely to be heteroscedastic. Furthermore, a recent study by Bertrand et al. (2002) suggests that, in evaluations of this sort, serial correlation can lead to overstated statistical precision. Following one of their suggestions, we report standard errors based on an arbitrary variance-covariance matrix that is consistent in the presence of any correlation pattern within states over time. Given that this correction is only valid asymptotically and some of these models have relatively few degrees of freedom, a finite sample correction that increases the standard errors is also applied (Davidson and MacKinnon, 1993).

In this type of fixed effects specification, the consequences of higher speed limits are effectively identified by comparing the within-state changes in states that raised their limit to the contemporaneous changes within states that did not. However, the prior evaluations of speed limits have generally adopted a different approach. For example, some studies (e.g., Garber and Graham, 1990; Lave and Elias, 1997) rely on time-series data specific to a state and only include controls for a linear trend and a few other variables (e.g., seat belt laws, unemployment rate). Conditional on these controls, such models effectively rely on a comparison of traffic fatalities before and after the speed limit increases. However, the inferences based on this approach may be substantively biased if the actual state trend is nonlinear or if the timing of new speed limits is closely related to that of other contemporaneous state policy changes that were omitted (e.g., stricter drunk-driving laws). Some studies have also examined panel data that varies along both a cross-sectional dimension (e.g., state or counties within a state) and a time-series dimension (e.g., monthly, quarterly or yearly). However, these models generally include

[7] We have also replicated this study's results in models that include the state minimum legal drinking age (MLDA). However, that policy variable is excluded in the results reported here because the state increases in MLDA began well before the study period and had ended by 1988.

[8] The results from linear probability models are quite similar. Because North Dakota had zero fatalities among 45–64-year-old women in 1998, a single fatality was artificially introduced to define the logarithmic transformation. However, the results reported here are also robust to excluding all observations from North Dakota.

a shared linear trend variable instead of less restrictive year fixed effects to control for unobserved time-series determinants (e.g., Lave and Elias, 1997; McCarthy, 1994). Furthermore, the inferences from some of these panel-based models may also be biased because they often do not include fixed effects specific to the cross-sectional unit (e.g., Lave and Elias, 1997; McCarthy, 1994).[9] Relatively few studies have simultaneously included as controls fixed effects specific to each cross-sectional and each time-series unit.[10] In fairness, it is important to note that two-way fixed effects models also rely on maintained assumptions that may not be accurate. In particular, these models implicitly assume that the time-series changes within states that did not raise their speed limits provide a valid proxy for the independent time-series variation in states that did. Furthermore, there may still be omitted variable biases if the timing of new speed limits coincides with the timing of other relevant state policies.[11] We present some evidence on the empirical relevance of these specification choices by comparing the results of models that replicate the varying identification strategies.

3. Results

In Table 2, we present the key results from regressions where the natural log of the total fatality rate is the dependent variable. The first two specifications are similar to those used by prior studies based on panel data. Specifically, these models include a shared linear trend variable instead of year fixed effects. Model (2) also includes state fixed effects. Model (3) is similar to the prior time-series evaluations based on data from a single state. Specifically, this model includes both state fixed effects and linear trend variables unique to each state. The final five models include both state and year fixed effects and also incrementally introduce all of the additional state-year control variables. The results from Model (1), which do not include state fixed effects, suggest that higher speed limits had implausibly large and positive effects on traffic fatality rates. In the remaining models, the estimated effect of moving to a 65 MPH speed limit is consistently negative. However, these estimates are statistically indistinguishable from zero in all but the final specification (P value = 0.053). Models (2), (3) and (4) combine state fixed effects with trend controls or year fixed effects but include few other controls for variables changing within states over time. These models uniformly suggest that the recent movement to speed limits above 65 MPH had relatively large, positive and statistically significant effects on fatality rates. In the subsequent models, which introduce the remaining controls, the estimated effect of 70 MPH or higher speed limits remains uniformly positive. However, in models that include state-specific trends, these estimates are smaller and statistically indistinguishable from zero.

The results from Table 2 suggest that the initial movement to 65 MPH speed limits reduced fatality rates and that the subsequent increases to higher limits increased fatality rates. But most of these

[9] Houston (1999) does include state fixed effects but excludes both a trend variable and year fixed effects.

[10] Lave and Elias (1997) present a simple 'difference-in-differences' comparison that is similar in spirit to two-way fixed effect models. The results in Greenstone (2002) are also an exception. However, those models were only based on data up to 1990 so they cannot assess the effects of the recent NMSL repeal. Furthermore, they do not include controls for the drunk-driving laws varying within states over the same time period.

[11] In this study, we assess this concern directly by estimating models that exclude and, then, introduce other variables that vary by state and year (i.e., key drunk-driving laws and state-specific trend variables).

Table 2
Least squares estimates, semi-log models for traffic fatality rates

Independent variable	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
65 MPH speed limit	0.401‡	−0.004	−0.004	−0.003	−0.030	−0.025	−0.034	−0.033*
	(0.070)	(0.016)	(0.014)	(0.022)	(0.022)	(0.021)	(0.022)	(0.017)
70+ MPH speed limit	0.813‡	0.086‡	0.075‡	0.097†	0.057	0.065*	0.015	0.024
	(0.123)	(0.028)	(0.025)	(0.046)	(0.035)	(0.033)	(0.037)	(0.031)
Mandatory Seat Belt Law— Primary Enforcement	−0.029	−0.019	0.006	–	−0.043	−0.040	–	−0.026
	(0.082)	(0.028)	(0.031)		(0.031)	(0.031)		(0.029)
Mandatory Seat Belt Law— Secondary Enforcement	−0.042	0.006	−0.023	–	−0.009	−0.009	–	−0.033†
	(0.060)	(0.017)	(0.031)		(0.019)	(0.018)		(0.014)
State Unemployment Rate	1.861*	−3.92‡	−3.67‡	–	−3.93‡	−3.59‡	–	−3.17‡
	(1.11)	(0.329)	(0.235)		(0.510)	(0.480)		(0.428)
Administrative License Revocation	–	–	–	–	–	−0.057‡	–	−0.049‡
						(0.018)		(0.017)
Illegal Per Se at 0.10 or higher BAC	–	–	–	–	–	−0.040	–	−0.060†
						(0.028)		(0.024)
Illegal Per Se at 0.08 BAC	–	–	–	–	–	−0.059	–	−0.042
						(0.048)		(0.032)
State fixed effects?	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects?	No	No	No	Yes	Yes	Yes	Yes	Yes
Trend variable?	Yes	Yes	No	No	No	No	No	No
State-specific trend variable?	No	No	Yes	No	No	No	Yes	Yes
R^2	0.286	0.931	0.953	0.919	0.935	0.938	0.950	0.959

The dependent variable is the natural log of the traffic fatality rate per 100 000 persons. Heteroscedastic-consistent standard errors are reported in parentheses.
*Statistically significant at the 10% level; †statistically significant at the 5% level; ‡statistically significant at the 1% level.

estimates are statistically indistinguishable from zero. However, these results may mask the heterogeneous effects of speed limits by gender and age. In Table 3, we present the estimated effects of increased speed limits on fatality rates for particular age and gender groups. The specification used to generate these estimates includes state and year fixed effects, state-specific trends and all the other state-year controls (i.e., as in Model (8) from Table 2). These results suggest that the fatality effects of higher speed limits did vary sharply by age and gender. For example, these estimates indicate that the post-NMSL speed limit changes increased fatality rates among women by approximately 9.9% but had small and statistically insignificant effects among men. These results also suggest that these effects varied sharply by age. Specifically, the most recent increases in speed limits significantly increased fatality rates among the elderly (approximately 13.2%) and women aged 25–44 (approximately 14.8%). In contrast, the movement to speed limits of 65 MPH significantly reduced fatality rates among men aged 25–44 (approximately 10.3%). But the estimated fatality consequences of these policies for men were otherwise statistically insignificant.

4. Conclusions

In this study, we present new evidence on the fatality consequences of increased speed limits. First, our results suggest that the estimated effects of these policies are quite sensitive to the empirical

Table 3
Estimated effects of higher speed limits on traffic fatality rates by age and gender

By age	By gender					
	Total		Males		Females	
	65 MPH	70+ MPH	65 MPH	70+ MPH	65 MPH	70+ MPH
Total	−0.033*	0.024	−0.037*	−0.013	−0.027	0.099†
	(0.017)	(0.031)	(0.020)	(0.036)	(0.022)	(0.039)
Aged 16–24	0.028	−0.010	0.022	−0.028	0.053	0.045
	(0.035)	(0.057)	(0.038)	(0.067)	(0.052)	(0.076)
Aged 25–44	−0.081‡	−0.011	−0.103‡	−0.074	−0.027	0.148†
	(0.027)	(0.045)	(0.032)	(0.054)	(0.048)	(0.071)
Aged 45–64	−0.010	0.083	0.019	0.063	−0.087*	0.097
	(0.048)	(0.063)	(0.065)	(0.070)	(0.047)	(0.105)
Aged 65 and over	−0.025	0.132†	−0.0001	0.136	−0.048	0.153*
	(0.027)	(0.065)	(0.045)	(0.086)	(0.035)	(0.087)

The dependent variable is the natural log of the traffic fatality rate per 100 000 persons. Heteroscedastic-consistent standard errors are reported in parentheses. All models include controls for seat belt laws, unemployment rate, drunk-driving laws and state-specific trends (i.e., as in Model (8) in Table 2).
*Statistically significant at the 10% level; †statistically significant at the 5% level; ‡statistically significant at the 1% level.

specifications employed in prior studies. In particular, these results suggest the importance of eliminating the biases that may be due to unobserved, state-specific determinants of traffic safety as well as to other policy changes that occurred contemporaneously within states (e.g., stricter drunk-driving regulations). Second, this study also provided new evidence that the overall effect of recent speed limit increases that have occurred since the repeal of the NMSL is statistically indistinguishable from zero. Third, we presented evidence that these increases did generate large and statistically significant increases in fatality rates among women and the elderly. We also found that the earlier movement to 65 MPH speed limits reduced fatality rates among men. An understanding of these heterogeneous effects should be an important part of an informed public debate on the desirability of these policy changes. However, the pattern of these reduced-form results also suggests the need for a better understanding of the structural relationship between speed limits, driving behavior, driving ability and traffic safety. In particular, a clear understanding of why higher speed limits created particular fatality risks for women and the elderly may be quite useful since it could suggest ways in which the unfortunate tradeoffs associated with higher speed limits could be attenuated.

References

Ashenfelter, O., Greenstone, M., 2002. Using mandated speed limits to measure the value of a statistical life, National Bureau of Economic Research Working Paper No. 9094.

Bertrand, M., Duflo, E., Mullainathan, S., 2002. How much should we trust differences-in-differences estimates?, National Bureau of Economic Research Working Paper No. 8841.

Davidson, R., MacKinnon, J.G., 1993. In: Estimation and Inference in Econometrics. Oxford University Press, Oxford, New York, Toronto and Melbourne.

DeJong, W., Hingson, R., 1998. Strategies to reduce driving under the influence of alcohol. Annual Review of Public Health 19, 359–378.

Evans, W.N., Graham, J.D., 1988. Traffic safety and the business cycle. Alcohol, Drugs and Driving 4, 31–38.
Farmer, C.M., Retting, R.A., Lund, A.K., 1999. Changes in motor vehicle occupant fatalities after repeal of the national maximum speed limit. Accident Analysis and Prevention 31, 537–543.
Garber, S., Graham, J.D., 1990. The effects of the new 65 mph speed limit on rural highway fatalities: a state-by-state analysis. Accident Analysis and Prevention 22, 137–149.
Grabowski, D.C., Morrisey, M.A., 2001. The effect of state regulations on motor vehicle fatalities for younger and older drivers: a review and analysis. The Milbank Quarterly 79, 517–545.
Graves, P.E., Lee, D.R., Sexton, R.L., 1993. Speed variance, enforcement, and the optimal speed limit. Economics Letters 42, 237–243.
Greenstone, M., 2002. A reexamination of resource allocation responses to the 65-MPH speed limit. Economic Inquiry 40, 271–278.
Haight, F.A., 1998. Letter to the editor. Journal of Safety Research 29, 141–143.
Houston, David J., 1999. Implications of the 65-mph speed limit for traffic safety. Evaluation Review 23, 304–315.
Lave, Charles A., 1985. Speeding, coordination, and the 55 MPH Limit. American Economic Review 75 (5), 1159–1164.
Lave, C., Elias, P., 1994. Did the 65 mph speed limit save lives? Accident Analysis and Prevention 26, 49–62.
Lave, C., Elias, P., 1997. Resource allocation in public policy: the effects of the 65-mph speed limit. Economic Inquiry 35, 614–620.
McCarthy, P.S., 1994. An empirical-analysis of the direct and indirect effects of relaxed interstate speed limits on highway safety. Journal of Urban Economics 36, 353–364.
Sen, A., 2001. An empirical test of the offset hypothesis. The Journal of Law and Economics 44, 481–510.