Did the 65 mph Speed Limit Save Lives?

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The University of California Transportation Center

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DID THE 65 MPH SPEED LIMIT SAVE LIVES?

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Abstract—In 1987, most states raised the speed limit from 55 to 65 mph on portions of their rural interstate highways. There was intense debate about the increase, and numerous evaluations were conducted afterwards. These evaluations share a common problem: they only measure the *local* effects of the change. But the change must be judged by its *system-wide* effects. In particular, the new 65 mph limit allowed the state highway patrols to shift their resources from speed enforcement on the interstates to other safety activities and other highways—a shift many highway patrol chiefs had argued for. If the chiefs were correct, the new allocation of patrol resources should lead to a reduction in *statewide* fatality rates. Similarly, the chance to drive faster on the interstates should attract drivers away from other, more dangerous roads, again generating system-wide consequences. This study measures these changes and obtains surprising results. We find that the 65 mph limit reduced statewide fatality rates by 3.4% to 5.1%, holding constant the effects of long-term trend, driving exposure, seat belt laws, and economic factors.

I. INTRODUCTION

In 1987, amid widespread controversy, 40 states raised the speed limit to 65 mph on sections of their rural interstate roads. Anticipation of the consequences varied widely: some predicted carnage, others said fatalities would decline. As might be expected, there have been numerous studies of the new speed limit. (See, for example: Baum, Wells, and Lund 1990; NHTSA 1989; Gallagher et al. 1989.)

Most of these studies looked at the *number* of fatalities, before and after the increase to 65 mph. The number usually increased since traffic usually increased—but we should be looking at *rates*, i.e. fatalities per vehicle mile traveled (VMT). And all the studies have confined themselves to looking at local effects: did raising the speed limit on highway X affect fatalities on that highway?

But there are theoretical reasons to believe that the effect of the 65 mph limit will be felt across the entire highway system: (i) enforcing the 55 mph limit on the interstate highways required a substantial amount of highway patrol resources: the new 65 mph limit allows highway patrols to shift these resources to other safety activities and other highways—something they wished to do; (ii) the new 65 mph limit might produce a shift of traffic from rural roads to rural interstates; (iii) higher speeds on the rural interstates might have psychological "spill-overs" that encourage faster driving on other roads. Thus, changing the speed limit on the rural interstates is likely to have consequences for other highways, so we should take account of these broader effects.

This study analyzes the statewide consequences of raising the speed limit, treating highways and enforcement as a total system. We find that the 65 mph speed limit reduced the statewide fatality rate by 3.4%–5.1%, compared to those states that did not raise their speed limit.

II. THEORY: POLICE ENFORCEMENT AND TRAFFIC BEHAVIOR AS A SYSTEM

Highway patrol resources are limited. If more officers are assigned to enforce speed limits on rural interstate highways, fewer can be assigned to other safety activities such as truck safety inspections or drunk driving checkpoints. In the absence of any external political pressure, police administrators try to balance their resources across the alternative safety activities. But when the federal government threatens to impose serious financial penalties on states that do not meet a particular speed limit criterion, the states may respond by altering the balance of their patrol activities.

Highways are also an interdependent system: "restrictions" on one road will cause some drivers to switch to other roads. A restriction might be a construction project, an accident, or an "unreasonable" speed limit. We would expect interactions between policing activity and drivers' highway choice as well.

The 55 mph limit and the misallocation of police resources

Although the federal government lacks direct power to set speed limits, it did so indirectly through financial pressure. It threatened a reduction in federal highway funding for any state that did not establish and enforce a 55 mph speed limit. There were detailed compliance requirements: speed monitoring programs were established, and states were required to report the proportion of drivers who exceeded the new limit. If the majority of a state's drivers did so, then its highway funds would be cut.

Given such financial pressure, it is reasonable to suppose that state governments might ask their highway patrols to give extra attention to enforcing speed limits so as to generate favorable compliance statistics. In response, highway patrols were likely to concentrate resources on the interstate highways since these highways have the densest concentration of high-speed traffic, and hence a patrol-hour of activity there will control the greatest number of potential speeders. On the other hand, the interstates are only a small proportion of total highway miles, and they were already the safest roads in the state. So it is possible that overall safety would decrease if patrol resources were shifted from other duties to controlling speeders on the interstates.

That is, it is possible that the federal mandates might cause patrol resources to be used in a suboptimal manner (Lave 1988). In fact, the National Research Council's commission to study the effects of the 55 mph limit found that 29% of patrol staff hours were devoted to rural interstate highways, though these highways accounted for only 9% of fatalities (NRC 1984, p. 227). Commenting on the allocation of highway patrol resources, the NRC commission said: "This (existing) allocation . . . is not entirely optimal either from the standpoint of total travel on these highways, or total motor vehicle deaths" (NRC 1984, p. 226).

The Commissioner of the California Highway Patrol, testifying before the Surface Transportation Subcommittee of the House of Representatives, said:

> Speed enforcement is important . . . However, I cannot look at this problem with blinders. My resources, like yours are limited. I must search for the most effective use of these funds. My responsibility to the citizens of California dictates that I achieve the maximum impact by setting priorities. And although speed enforcement is important, it is not our only priority. It is part of our balanced and comprehensive approach to traffic safety. (Hannigan 24 April, 1990, pp. 6–7)

The International Association of Chiefs of Police (IACP) reported the results from a survey of its members in testimony before the same subcommittee on March 22, 1990:

> In states where compliance figures are satisfactory, often times this is because other safety priorities such as DWI and drug interdiction are relegated to secondary and tertiary priority. Thus, we in law enforcement are in the classic Catch-22. If we don't comply, safety is negatively impacted because our funds are reduced. If we do comply, safety is negatively impacted because other priorities are reduced. It's hard to see how we can win this one, Gentlemen. (Tippet 1990, p. 2)

Attached to their testimony was a copy of an official IACP resolution, passed in 1988, which says in part:

> (Federal financial) sanctions also force the overconcentration of limited resources for the express purpose of attaining compliance rather than application of resources in a manner most effectively enhancing total highway safety . . ."

Thus, as measured by either comparative ratios of patrol resources to fatalities or by the opinions of state safety professionals, the federal sanctions associated with the 55 mph speed limit produced a misallocation of highway patrol resources. The increase to a 65 mph limit in some states would have reduced the pressure to concentrate on speeders (because federal compliance requirements were relaxed in these states) and would have allowed patrol resources to be shifted to activities that the patrols believed would have greater impact on safety.

These new patrol activities would be spread across all highway types, hence the effects of the new speed limit would be spread across all highway types. Thus, to measure the impact of the change to 65 mph, we must look at the change in statewide fatalities.

The 55 mph limit and the misallocation of vehicle traffic

Obviously, some of the traffic diverted from one highway will show up on others. Before the imposition of the 55 mph limit in 1974, we would have expected many drivers to go out of their way to use the high-speed interstate highway system. After the new limit was passed a driver choosing between a 55 mph rural interstate and a 55 mph country road was more likely to select the country road: it is usually more scenic, more direct, and less heavily policed. We would expect a similar effect for urban interstates.

The overall safety effect of raising the speed limit

The misallocation of traffic and the misallocation of police resources combine to produce measurement bias in the reported safety statistics. They overstate the apparent effect of the 55 mph speed limit on *rural interstate* highways: extra policing lowers the fatality rate below what it would be with a 55 mph limit and normal policing; and artificially decreased traffic volumes lower the fatality rate below what it would be with normal traffic.

What happens on *other roads*? If patrol resources had been misallocated in response to federal pressure to enforce the 55 mph limit, then removing the federal pressure should cause a better use of patrol resources and a decrease in fatalities on noninterstate roads. Likewise, any diversion of traffic onto the rural interstates should decrease fatalities on noninterstate roads (Kamerud 1988; Lave 1988, 1989). And McKnight and Klein (1990, p. 77) comment: "In the face of widespread noncompliance with the 55 mph limit, raising the limit on rural Interstates may benefit safety by diverting some speeders to the highways best able to accommodate them."

Although the competing effects make it difficult to predict the net result of the new speed limit, the theory does lead to one absolutely unambiguous conclusion:

> To evaluate the effect of the increase to 65 mph, we must look at total fatality rates for the entire state.

III. METHODOLOGY

The estimation of fatality rates is highly sensitive to sample size. If some stretch of highway normally has, say, ten fatalities per year, a few random individual accidents can greatly affect the apparent fatality rate. Such fluctuations cause serious problems when we are trying to evaluate the effects of a safety intervention policy: suppose the fatality rate falls 5%, does that mean the new policy worked? or suppose the rate remains the same, does that mean the new policy failed? The answer in both cases is: we just do not know because the expected yearly fluctuations in these rates are larger than the probable effects from the new policy. Yet a number of studies of the new speed limit have relied on small samples from a specific highway type within a specific state.

This study analyzes the effect of the new speed limit using two independent methodologies. First, in Sections IV and V we compare the experience of the entire group of states that raised speeds against the experience of the states that did not. This resembles the familiar test group versus control group methodology, though obviously it is not a random sample. Second, in Sections VI and VII we analyze the data on a state-by-state basis using regressions on monthly time-series data. This enables us to incorporate the effects of background variables that might differ across states.

Table 1. Change in statewide fatality rates

	The change (%) in statewide fatality rates		
	1986->1987	1987->1988	Overall change 1986->1988
The 65 mph states	−4.68	−1.55	−6.15
The 55 mph states	−.07	−2.55	−2.62

Both methodologies use the theoretically correct dependent variable: statewide fatalities divided by statewide VMT.

IV. AGGREGATE METHODOLOGY

We aggregate states into two large groups: those that raised the speed limit to 65 mph in 1987 versus those that did not. For each group of states, we compute the total fatality rate: the sum of overall statewide fatalities across the entire group, divided by the sum of statewide VMT. We do this for 1986, the last full year of data before the change in the speed limit, and for 1988, the first full year of data afterwards. To evaluate the effects of the new speed limit, we compare the change in fatality rates for the 65 mph states against the change in fatality rates in the 55 mph states.

In effect, we are comparing a test group to a control group. The time period is the same for both groups so we are holding constant many of the effects that might be operating on the fatality rate: long-term trends, improvements in auto safety features, roads, driving habits, or the influence of general economic changes.* Furthermore, the aggregation into groups of states enhances reliability for the same reason that an average is a more reliable estimator than a sample of one: the effect of a positive idiosyncratic influence on the fatality rate in one state, will tend to be canceled by the effect of a negative idiosyncratic influence in another state.

The analysis is based on data compiled by the National Highway Traffic Safety Administration (NHTSA 1989, pp. 33–44). Table 1 shows the basic

* The procedure cannot correct for any causal factor that differs systematically between the two groups of states. We were able to eliminate one such possibility: seat belt usage is similar. The 55 mph states had an average belt usage rate of 20.8% in 1986 and 48.2% in 1988, a 27.5% improvement. The 65 mph states had a belt usage of 24.1% in 1986 and 51.5% in 1988, also a 27.5% improvement.

results. Looking at the states that raised their speed limits in 1987: the overall fatality rate fell by 4.68% in 1987 (compared to the year before when the limit had been 55 mph), and then fell an additional 1.55% in 1988 for an overall drop of 6.15%. Looking at the states that did not change their speed limits: fatality rates were essentially unchanged in 1987 compared to the year before, and then fell by 2.55% the next year for an overall drop of 2.62%.

Obviously we cannot attribute the entire 6.15% fatality drop (in the 65 mph states) to the new speed limit. Fatality rates might have declined even if the limit had not changed. There is no absolutely certain way to calculate such a contrafactual estimate, but there is a way to estimate it by using data from the states that did not change the speed limit. Consider them a control group for the 65 mph speed limit experiment, and use their experience to estimate what would have happened to the test group—the states that did raise the limit. The difference in fatality rates between the two groups of states is 3.62%. (calculated as $[100/(100 - 2.62)] \times [6.15 - 2.62]$). That is, we estimate that the 65 mph speed limit reduced the fatality rate by 3.62% compared to those states that did not raise their speed limit.

How certain is this result? Its accuracy depends on the truth of the control group assumptions: are the states that retained the 55 limit generally comparable to the states that changed to 65 mph? Many factors can influence the fatality rate. To be absolutely certain of these conclusions, we would need enormously more data than are available, to hold all those factors constant (Kamerud 1988).

But despite data limitations, this estimate has several significant advantages: (i) it uses a better evaluation criterion—the effect on the statewide fatality rate; (ii) it aggregates the data into large groups, to produce far more reliable estimates of fatality rates—they are more stable, and the implicit averaging process helps to compensate for the effects of excluded idiosyncratic variables as well; (iii) the use of a control group should take care of many of the remaining problems from excluded variables.

V. ADDITIONAL EVIDENCE

We posit a connection between the misallocation of police resources, the misallocation of traffic, and the overall statewide death rate. The relative decline in fatality rates (for those states that increased the speed limit) supports the theory. Is there any microlevel evidence as well? For example, are there data to show that police actually did reallocate resources, or that traffic actually did move between highway types?

Table 2. Reallocation of traffic in the states that raised the speed limit to 65 mph

Part A: Using own-growth rate (65 mph states) as baseline	
% Increase in rural interstate VMT divided by % increase in statewide VMT	= 1.73
% Increase in urban interstate VMT divided by % increase in statewide VMT	= 1.13
% Increase in non-interstate VMT divided by % Increase in Statewide VMT	= .89
Part B: Using 55 mph state growth rates as baseline	
Rural interstate traffic: VMT growth rates in 65 mph states divided by 55 mph states	= 1.62
Urban interstate traffic: VMT growth rates in 65 mph states divided by 55 mph states	= 1.32
Noninterstate traffic: VMT growth rates in 65 mph states divided by 55 mph states	= 1.36

Source: NHTSA (1989).

Evidence for the reallocation of police resources. There is evidence that the police are feeling resource pressures. Freedman and Paek (1993, p. 5) survey highway and city police across the country and discover, "[Their ability] to respond to the rising demand for traffic enforcement has clearly diminished over time. This suggests that more efficient allocation of police resources is needed in many places."

Appendix A in (NHTSA 1989) includes state responses to a request for information. Although the tone of NHTSA's questions seems to ask the states for confirmation that they take speed enforcement very seriously, several states chose to address other issues. Nevada says it shifted resources to other enforcement activities after the 65 mph limit was passed (page A-96). California, Montana, West Virginia, and Wyoming imply that they have changed also. And in a personal communication, Ron Sostkowski, Director of State and Provincial Police within the International Association of Chiefs of Police, confirms: (i) this has been a frequent topic of conversation at the annual meetings of state police chiefs, and (ii) the highway patrols in the states that raised their speed limits did use the opportunity to shift resources into activities that they thought would have greater safety impact.

Evidence for the reallocation of traffic. Is there evidence that traffic did shift back to the high-speed highways after the increase in speed limits? We cannot use the simple, year-to-year change because there are strong overall trends in travel as well. So we need to measure change relative to some expected baseline. Table 2 does this in two different ways.

Part A, at the top concentrates on the states that increased speed limits to 65 mph. For these states, it compares the VMT growth rate on specific highway types to the overall VMT growth rate in the state. For example, it shows that traffic on the rural interstate highways in the 65 mph states, grew 1.73 times faster than the overall VMT growth in those states. Traffic on the noninterstate highways grew at only 89% of the overall VMT growth rate for these states. Both results are consistent with our theory.

Part A makes internal VMT growth comparisons: highway type versus the state average. Part B compares the VMT growth in the 65 mph states to the growth in the 55 mph states, keeping highway type constant. Thus, it can be seen that on rural interstate highways, VMT grew 1.62 times faster in the 65 mph states than it did in the 55 mph states. And so on. These numbers are consistent with the expected pattern of traffic shifts that would be expected if the underlying theory were correct.

To summarize: there is strong evidence that state highway patrols wanted to reallocate resources in the hypothesized way, and there is some evidence they actually implemented this intention. And there is evidence that traffic patterns actually did shift in the manner we hypothesized.

VI. STATE-BY-STATE REGRESSION ANALYSIS

The results in Section IV contradict the intuition of many observers and must be examined carefully. The results rely on a comparison in fatality trends between the states that did and those that did not raise their speed limits. The basic assumption behind such a test-group/control-group evaluation is that the two groups are otherwise comparable. Perhaps that is not true here: perhaps some economic factor led one group of states to stick with the old speed limit and also affected their fatality rate. The obvious way around this possibility is to disaggregate the data and explicitly model the determinants of the fatality rate in individual states; then, holding other factors constant, measure any change in the state's fatality rate that occurred after the speed limit was raised. There is already such a state-by-state analysis in the literature, and we will build on it.

The most sophisticated analysis of the effects of the 65 mph limit has been done by Garber and Graham (1990). Using monthly fatalities on rural interstate highways as their dependent variable, they run separate regressions for each state that adopted the 65 mph limit. They measure the effect of the new speed limit with a dummy variable that is 0 in the pre-65 mph months, and 1 thereafter; they hold constant the effects of unemployment, seat belt laws, linear time trend, monthly traffic patterns, and weekday/weekend patterns.

If the new speed limit reduces safety, the 65 mph dummy variable should be significantly positive. Figure 1 (adapted from their Figure 6) shows their major result. These are the *t*-ratios for the estimated speed limit dummy: the length of a bar measures statistical significance, the direction shows whether the estimated effect was to increase fatalities (up direction) or to decrease them. The estimated effect varied considerably across states—some states actually show a fatality decrease following the change to 65 mph. But most of the estimates show an increase in fatalities, and nine of these positive coefficients were statistically significant. Garber and Graham (1990) conclude (page 145): "The new 65 mph limit appears to be increasing rural interstate fatalities in some states, reducing them in others, and having no detectable effect (given the experience to date) in the remainder. The number of states experiencing increased fatalities exceeds the number experiencing reduced fatalities . . ."

Although their analysis and presentation are thorough and ingenious, it has two potential problems. First, they are modeling the effects of the speed limit change only on rural roads, while the theory developed above clearly indicates that there will be system-wide effects. Second, they are unable to control for exposure: monthly VMT statistics are not reported separately for the rural interstate highways. They compensate for this by devising proxies for VMT—time trend and the unemployment rate. These should be good proxies, but they are not a perfect substitute for the missing VMT data. We deal with these problems in a two step process.

Step 1: We began by reestimating the Garber/Graham model on each 65 mph state, but used *statewide* fatalities as the dependent variable. We changed only the dependent variable.* Running these new regressions, the effects of the speed limit variable disappeared: there was no statistically discernible effect of the higher speed limit on statewide fatalities.

Step 2: Next, taking advantage of the availabil-

*Steven Garber generously provided a copy of their data set. We added in the newly available data to extend their time series through 1990; statewide fatality data were taken from the federal Fatal Accident Reporting System (FARS) data base. Georgia and Virginia raised their limits in 1988; we adjusted their speed dummies for this.

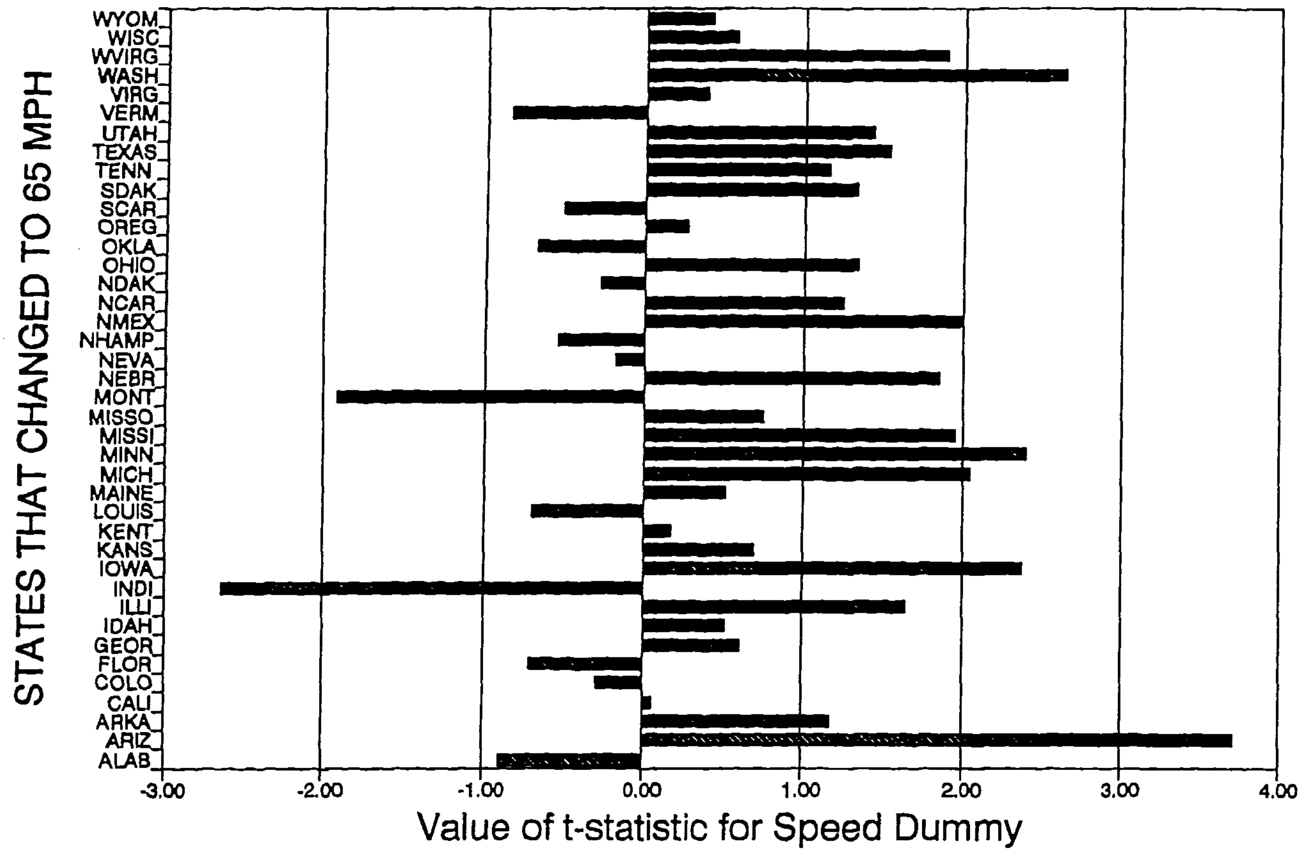

Fig. 1. Garber and Graham (1990) estimate of the effect of increasing speed limits.

ity of statewide VMT data, we changed the dependent variable to the fatality *rate:* fatalities divided by VMT.* We ran new regressions using these statewide fatality rates as the dependent variables, but kept the same Garber/Graham independent variables. Table 3 shows the results from the new regressions. The fit of the model was good: mean R-square was 0.59; the estimated coefficients of the unemployment variable were negative as expected (increased unemployment decreases the fatality rate); and the estimated coefficients of the seat belt dummy variable were mostly negative (seat belt laws decrease the fatality rate).

Figure 2 is set up the same way as the Garber/Graham figures. It displays the t-ratios associated with the 65 mph speed limit dummy: bar direction shows whether the new speed limit decreased (to the left) or increased the fatality rate; bar length shows the statistical significance of the estimated effect. Most of the bars point left, indicating that statewide fatality rates decreased after the 65 mph speed limit was adopted. Figure 3 shows the quantitative magnitude of the effect: we divide the estimated speed limit dummy by the mean fatality rate in that state; this produces an estimate of the percentage change in the fatality rate, and these are then plotted as bar length. Most of the bars indicate that the fatality rate fell by 5%–10%.

To illuminate these results we confine our attention to states where the estimated speed limit dummy is statistically significant. Figures 4 and 5 report the same regression data as Figs. 2 and 3, but we omit the states whose estimated coefficients were insignificant. The pattern is clear: the effect of the higher speed limit was to decrease the statewide fatality rate—exactly the change that would have been predicted from the arguments of the highway patrol chiefs.

Taking an unweighted average of the speed limit dummies across all the states, we estimate that the mean change in the fatality rates was −3.43% following the introduction of the new higher speed limit. This figure is strikingly close to the −3.62% estimate we obtained in Section IV, when we com-

*We obtained monthly VMT data as follows: the FHWA reports yearly VMT for each state, and the federal Nationwide Personal Transportation Survey (NPTS) reports the quarterly pattern of VMT throughout the year. So, for each state, we multiplied the known yearly VMT by the monthly VMT proportions to estimate monthly statewide VMT.

Table 3. Results from regression on states adopting 65 mph limit (1976–1990 data)

State	65 mph spd lim dummy	t ratio	Percent unemploy	t ratio	Belt use dummy	t ratio	r^2	Mean of fatality rate
Alabama	−0.0014	−0.89	−0.00054	−2.98			0.650	0.031
Arizona	−0.0099	−5.81	−0.00243	−8.29			0.761	0.041
Arkansas	−0.0005	−0.24	−0.00005	0.12			0.516	0.034
California	−0.0018	−2.28	−0.00134	−8.11	−0.00127	−1.51	0.859	0.028
Colorado	−0.0025	−0.76	−0.00024	−0.60	0.00411	1.20	0.724	0.027
Florida	−0.0034	−2.70	−0.00093	−3.78	−0.00355	−2.70	0.747	0.033
Georgia	0.0008	0.53	−0.00221	−7.59	−0.00382	−2.41	0.774	0.029
Idaho	−0.0026	−0.56	−0.00061	−0.93	0.00388	0.98	0.569	0.036
Illinois	0.0022	1.93	−0.00047	−2.46	0.00070	0.61	0.808	0.026
Indiana	0.0025	0.53	−0.00011	−0.63	−0.00453	−0.99	0.680	0.026
Iowa	0.0047	2.01	−0.00060	−1.37	0.00005	0.02	0.667	0.028
Kansas	−0.0016	−0.75	−0.00097	−1.69	−0.00105	−0.50	0.604	0.028
Kentucky	0.0025	1.56	0.00026	1.22			0.662	0.030
Louisiana	−0.0017	−0.57	−0.00044	−1.07	−0.00065	−0.22	0.689	0.038
Main	−0.0015	−0.66	−0.00021	−0.37			0.444	0.026
Michigan	0.0005	0.61	−0.00034	−3.14	−0.00030	−0.29	0.821	0.025
Minnesota	0.0017	1.08	−0.00153	−5.25	−0.00222	−1.36	0.817	0.022
Mississippi	−0.0703	−1.96	−0.01293	−2.38	−0.02880	−0.74	0.089	0.065
Missouri	0.0044	2.14	−0.00099	−2.33	0.00044	0.18	0.465	0.030
Montana	−0.0094	−1.70	−0.00111	−1.26	0.00528	0.92	0.562	0.037
Nebraska	−0.0018	−0.80	−0.00111	−1.97			0.466	0.025
Nevada	−0.0096	−1.36	−0.00179	−3.51	0.01067	1.53	0.534	0.041
New Hampshire	−0.0051	−2.57	−0.00053	−1.33			0.520	0.024
New Mexico	−0.0013	−0.45	−0.00089	−1.76	0.00012	0.04	0.674	0.045
North Carolina	−0.0026	−2.23	−0.00047	−1.96	0.00081	0.61	0.770	0.032
North Dakota	0.0149	0.56	0.00829	0.93	−0.00624	−0.17	0.108	0.041
Ohio	0.0002	0.21	−0.00053	−3.72	−0.00084	−0.72	0.757	0.024
Oklahoma	−0.0109	−0.17	−0.00762	−1.63	−0.01796	−0.28	0.090	0.050
Oregon	0.0004	0.26	−0.00055	−2.32	0.00088	0.57	0.678	0.030
South Carolina	−0.0033	−1.97	−0.00081	−3.19	−0.00473	−2.85	0.592	0.036
South Dakota	0.0043	1.23	−0.00048	−0.40			0.458	0.028
Tennessee	−0.0026	−1.68	−0.00047	−2.29	0.00101	0.66	0.695	0.032
Texas	−0.0029	−2.66	−0.00131	−5.21	−0.00288	−2.60	0.859	0.030
Utah	−0.0047	−1.90	−0.00098	−2.33	0.00223	0.91	0.535	0.028
Vermont	−0.0000	−0.01	−0.00243	−3.16			0.429	0.028
Virginia	0.0004	0.30	−0.00090	−3.22	−0.00135	−0.90	0.780	0.023
Washington	−0.0015	−0.97	−0.00132	−6.20	−0.00078	−0.53	0.777	0.025
West Virginia	0.0001	0.02	−0.00019	−0.77			0.485	0.038
Wisconsin	−0.0018	−0.81	−0.00040	−1.93	0.00170	0.78	0.742	0.024
Wyoming	0.0031	0.86	−0.00111	−1.65	−0.00264	−0.70	0.713	0.038

Linear regressions set up as in Garber & Graham, including dummy variables for months.
Dependent variable = Monthly statewide fatality rate, that is: (monthly fatalities on all road types)/(statewide monthly VMT).

pared the change in aggregate fatalities between the states that did and did not adopt the new speed limit.

The results in Section IV rely on aggregate analysis: we compute the overall fatality rate for the combined 65 mph sample of states before and after the new speed limit; then this is compared to the time trend in the overall fatality rate for the states that did not adopt the new limit, using them as a control group to hold other factors constant. The results in this section compute the change in fatalities on a state-by-state basis, while explicitly holding constant the effects of time trends, unemployment, seat belt laws, and traffic patterns. Given the substantial difference in methodologies, the similarity of results lends confidence to the conclusions.

Step 3: As an additional check on the regressions, we examined an alternative hypothesis. Suppose there had been a nationwide break in fatality trends starting in 1987—for some reason other than the new speed limit—and that fatality rates had begun an overall drop after that time. If this were true, then the 65 mph speed limit dummy would pick up the effect of this trend break and be spuriously negative. The significance of the dummy would reflect the overall break in fatality trends, not the change in the speed limit laws.

If there had been such a spurious break in the time trend, we would detect its effect in data from states that did not raise speed limits. So we did the following: using the states that had not raised their

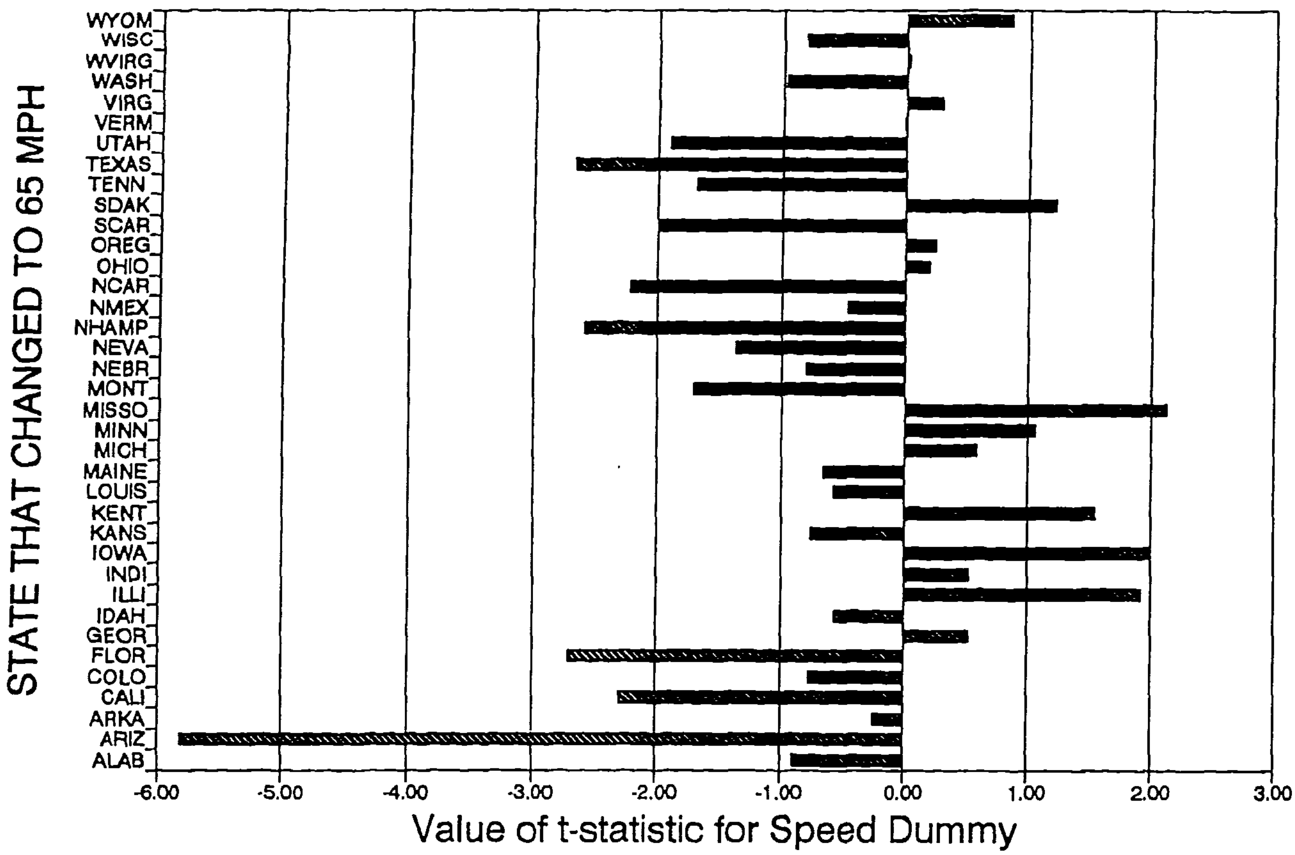

Fig. 2. *T*-value of 65 mph speed limit. Coeff; Dep. Var = Fatality rate 1976–1990.

speed limits, we created a fake-65 mph dummy variable that was 0 before June 1987 and 1 afterwards. If there actually had been a break in historic fatality trends, then this dummy would pick up its effects, and we would see significant negative estimates for the fake-65-mph dummy. If there were no break in the trend, the estimated coefficients would be near zero and insignificant.

Table 4 reports the results of these regressions. Figures 6 and 7 plot them for the subset of states where the fake 65 mph dummy was approximately significant. There is no evidence for the hypothesized spurious break in fatality trends: the fake-65 mph dummy was negative and near significant in 2 states, but positive and near significant in the other 4. This supports our interpretation of the results in Step 2.

VII. A MORE EFFICIENT REGRESSION MODEL

In Section VI we fitted a separate regression to each state. This allows for variation in speed limit effects between states, but it may be an inefficient use of the available data: the speed limit dummy is estimated using only a portion of the total available data. Might we combine the data from all the states to produce a better estimate? Although the individual estimates of the 65 mph dummy did differ from state to state, this may be just the consequence of statistical variation. Suppose the effect of the 65 mph limit were uniform across states: we could fit a single regression to their combined data and produce a more precise estimate of the effect of the 65 mph limit. In this section we test the hypothesis of uniformity across states. We fit a "restricted" model to the combined data sets that restricts the speed limit coefficient to be identical across states; and we fit an "unrestricted" model that allows the coefficients to vary. We then test the residuals from the two models to compare their fit.

The restricted model

We use the same model as in Table 3, and the same time period, but for interpretive convenience we use the log of the dependent variable. (Garber/Graham ran their regressions both ways—log and linear—and got essentially identical results with both forms of the dependent variable.) That is, we allow the other coefficients to vary across the

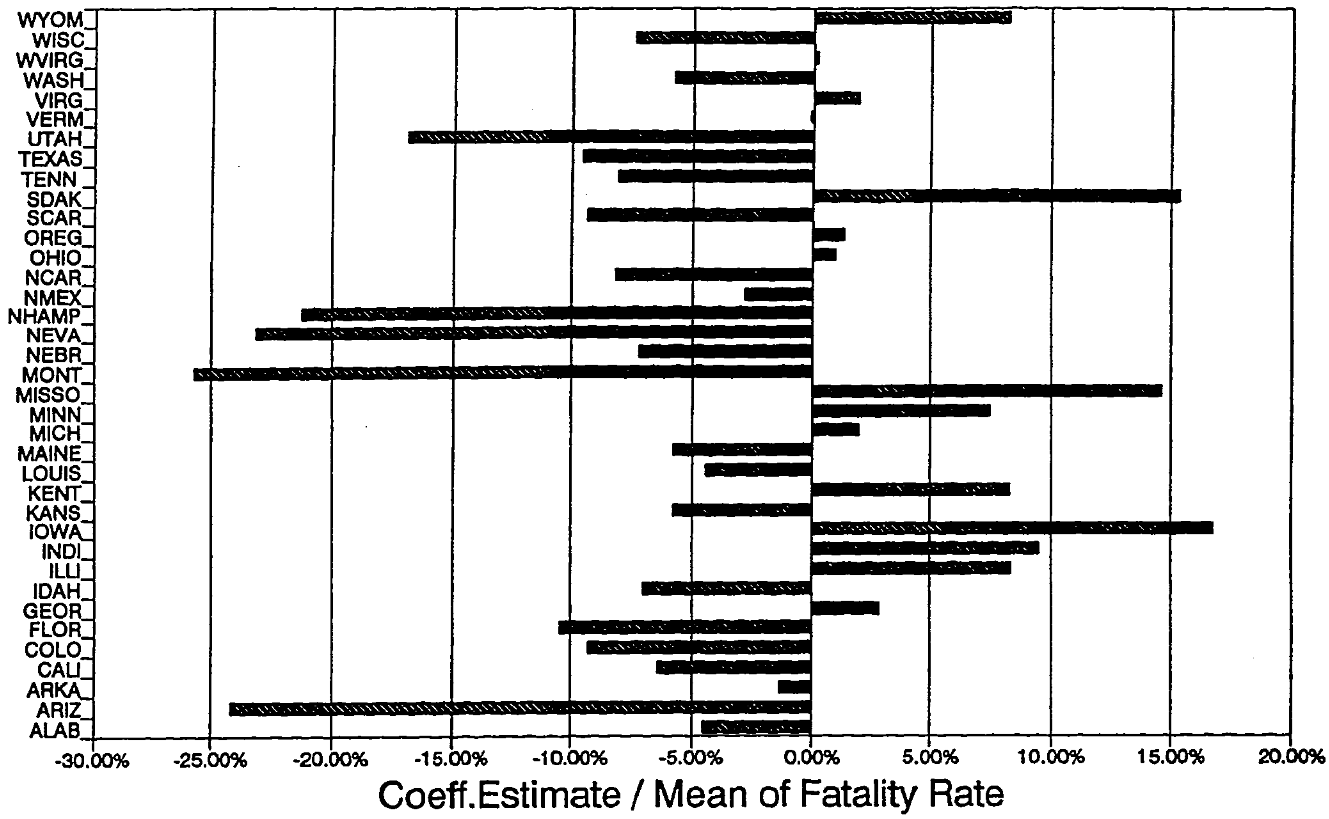

Fig. 3. Estimated % change in fatality rates after increase to 65 mph limit.

states—they are allowed to have different time trends, seat belt effects, unemployment effects, and so on—but we estimate a single 65 mph dummy for the entire 40-state sample. Observations span the period from January 1976 to December 1990. Thus there were 7,200 observations, 180 from each state.

Since the dependent variable is in logs, the estimated coefficient for the speed limit variable is the percentage change in fatality rate, nationwide. Results of the restricted model are: a 5.06% decrease in fatality rate, with a t-ratio of 3.19. R-squared is 0.61.

The unrestricted model

The unrestricted model is identical with the restricted model with one change: the coefficient of the speed limit dummy is allowed to vary across states—and each of the unrestricted regressions is fitted to a much smaller data base, only 180 observations. The results of the unrestricted regressions are shown in Table 5.

To evaluate the hypothesis of uniform effect, we test the fit of the restricted model against the unrestricted model. We perform an F-test on the residuals, as follows:

$$F = \frac{(e_{*}'e_{*} - e'e)/(g - j)}{e'e/j}$$

Where: $e_{*}'e_{*}$ is the residual sum of squares of the restricted regression, and g is the degrees of freedom associated with the restricted regression (equal to the number of observations minus the number of coefficients estimated). And in the denominator: $e'e$ is the residual sum of squares of the unrestricted regression, and j is the degrees of freedom associated with this regression. This produces the F-statistic 1.14, with 39 degrees of freedom in the numerator and 6,520 degrees of freedom in the denominator. That is, we conclude at the 95% level of significance that the explanatory power of the model is not compromised by assuming a uniform effect for the new speed limit across all the states that adopted it. This result also strengthens the conclusion that the new speed limit decreased the fatality rate.

How robust are these results? Do they really present a general indication of the effects of the new speed limit, or are they just the result of the particular combination of data in our sample. One way to check the solidity of the results is to alter the sample, reestimate the regressions and use a

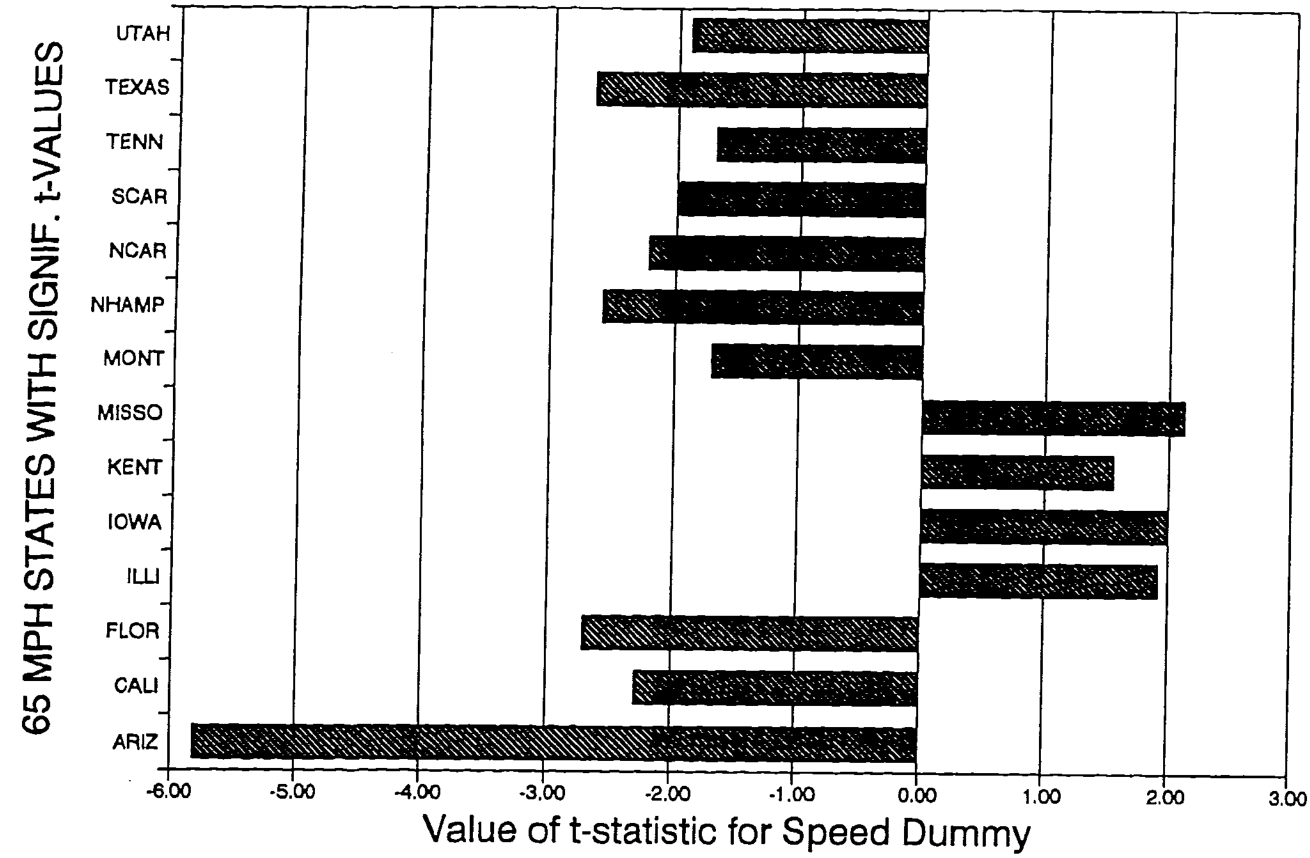

Fig. 4. T-value of 65 mph speed limit. Coeff; Dep. Var. = Fatality rate 1976–1990.

Chow test to see whether the new estimates are similar to the originals. If the two regressions produce widely divergent parameter estimates, this casts doubt on the reliability of the model; if the two regressions produce similar parameter estimates, this lends credibility to the basic estimates.

We performed the test as follows. First, seven representative states were removed from the sample,* and the restricted model was reestimated from the 5,940 observations on the remaining 33 states. The resulting regression coefficients were similar, adjusted R-squared was 0.54, and the 65 mph speed limit coefficient was estimated at -4.40%, with a t-ratio of 2.38.

Next, analysis of variance techniques are employed to test the consistency of the two results. An F-statistic is computed as follows:

$$F = \frac{(e_*'e_* - e'e)/(m - p)}{e'e/p}$$

Where: $e_*'e_*$ is the residual sum of squares of the original 40-state regression; and m is the degrees of freedom associated with that regression. And in the denominator: $e'e$ is the residual sum of squares of the 33-state regression, and p is the associated degrees of freedom. This produces an F-statistic of 1.07, with 591 degrees of freedom in the numerator and 6,531 degrees of freedom in the denominator.

That is, the parameter estimates are similar despite the large change in the data sample. Thus there is further reason to believe in the generality of the results.

*Randomness being impossible to achieve from such a small population, we instead chose a representative sample: each major region of the United States was represented, as were urban and rural states, states that did and did not enact seatbelt laws, and states whose individual regressions got high and low R-squares. We removed: California, Georgia, Louisiana, Minnesota, Nebraska, New Hampshire, and New Mexico.

VIII. SUMMARY AND CONCLUSIONS

Prior studies of the 65 mph limit have only measured the *local* effects of the change. But enforcement and highways are integrated, interactive systems: extra policing resources used to reduce speeding must be diverted from other kinds of safety activities; drivers discouraged from using interstate

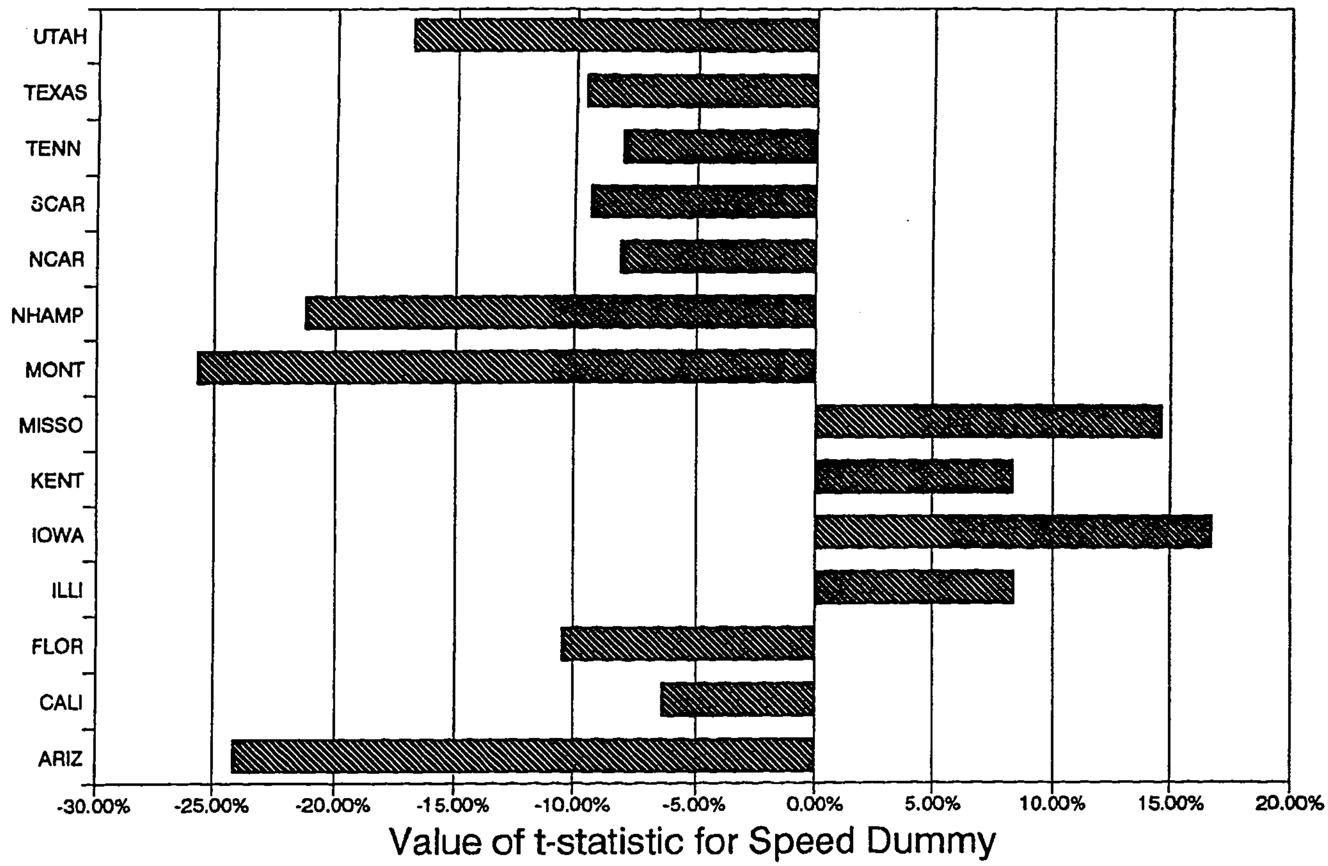

Fig. 5. Estimated % change in fatality rates after increase to 65 mph limit.

highways may move to other, more dangerous roads. A decrease in fatalities in an area where resources have been concentrated may be offset–even overwhelmed–by the effects on the rest of the highway system. Thus, a new speed limit must be evaluated by its *system-wide* consequences. We must use overall statewide fatality rates as the dependent variable.

The new 65 mph limit allowed state highway patrols to shift resources from speed enforcement on the interstates to other safety activities and other highways—a shift many highway patrol chiefs had argued for. If the chiefs were correct, the reallocation of patrol resources should lead to a reduction in statewide fatality rates. In addition, the higher speed limit on the interstates might attract drivers

Table 4. Regression on states with 55 mph limit, using "fake" 65 mph dummy

State	Fake 65 mph speed limit dummy var.	t-ratio	% unemployed	t-ratio	Belt use dummy	t-ratio	R-sq	Mean of fatality rate
Alaska	−0.00632	−1.38	−0.00073	−0.69	0.00168	0.29	0.437	0.032
Connecticut	−0.00236	−1.55	−0.00111	−3.39	−0.00310	−1.92	0.565	0.022
Delaware	0.00381	1.50	0.00023	0.38			0.279	0.026
Hawaii	0.00565	1.92	−0.00154	−2.02	−0.00049	−0.16	0.474	0.027
Maryland	−0.00200	−1.48	−0.00022	−0.77	0.00200	1.39	0.617	0.023
Massachusetts	0.00157	1.46	−0.00111	−5.45	0.00124	0.90	0.661	0.020
New Jersey	−0.00045	−0.61	−0.00052	−2.70	−0.00061	−0.73	0.688	0.020
New York	0.00205	2.32	−0.00083	−2.91	−0.00175	−1.78	0.826	0.027
Pennsylvania	0.00238	1.88	−0.00040	−2.89	−0.00331	−2.79	0.786	0.026
Rhode Island	−0.00520	−0.34	−0.00122	−0.45			0.053	0.033

Linear regressions set up as in Garber and Graham, including dummy variables for months.
Dependent variable = Monthly statewide fatality rate, that is: (monthly fatalities on all road types)/(statewide monthly VMT).

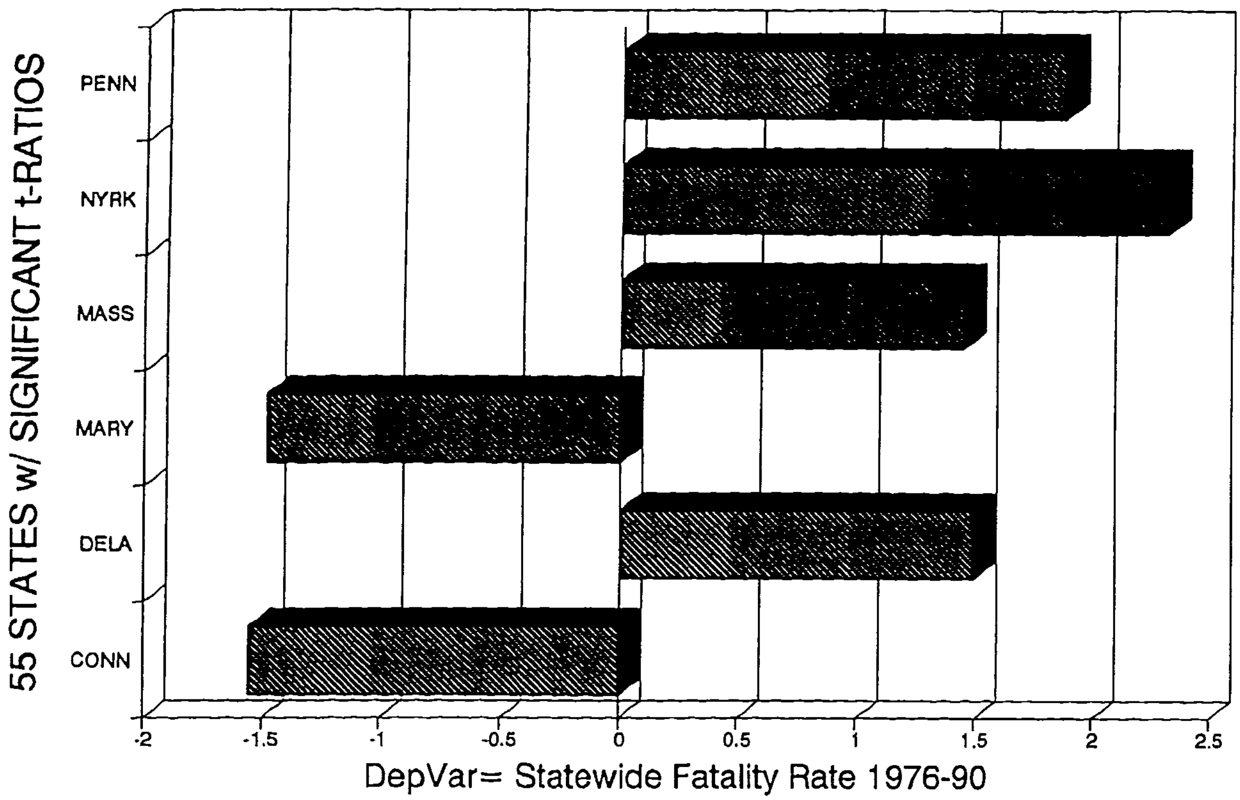

Fig. 6. *T*-ratio of fake 65 mph dummy for states with 55 mph speed limit.

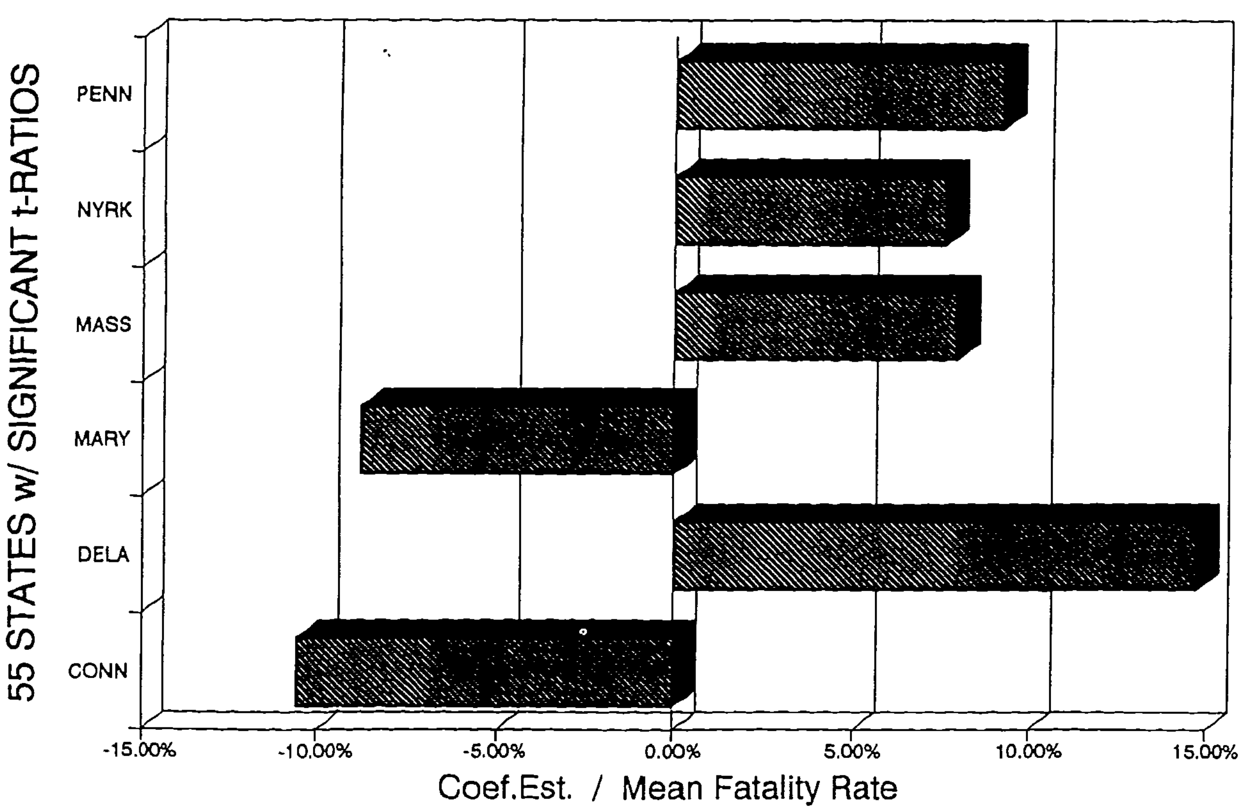

Fig. 7. Estimated change in fatalities in period following fake 65 mph dummy.

Table 5. Unrestricted regression

State	65 mph dummy	t-ratio
Alabama	−0.053	−1.09
Arizona	−0.295	−6.81
Arkansas	−0.003	−0.05
California	−0.0074	−2.88
Colorado	−0.11	−0.91
Florida	−0.117	−3.07
Georgia	0.022	0.41
Idaho	−0.133	−0.87
Illinois	0.104	2.45
Indiana	0.103	0.53
Iowa	0.218	2.51
Kansas	−0.078	−0.90
Kentucky	0.093	1.61
Louisiana	−0.057	−0.83
Maine	−0.127	−1.29
Michigan	0.024	0.71
Minnesota	0.106	1.53
Mississippi	−0.444	−1.97
Missouri	0.115	1.78
Montana	−0.217	−1.25
Nebraska	−0.066	−0.73
Nevada	−0.188	−0.97
New Hampshire	−0.257	−2.93
New Mexico	−0.035	−0.57
North Carolina	−0.115	−3.18
North Dakota	0.306	1.17
Ohio	0.014	0.30
Oklahoma	0.13	0.27
Oregon	0.006	0.11
South Carolina	−0.091	−1.95
South Dakota	0.138	1.10
Tennessee	−0.081	−1.64
Texas	−0.111	−3.43
Utah	−0.21	−2.24
Vermont	−0.089	−0.78
Virginia	0.038	0.60
Washington	−0.062	−1.04
West Virginia	0.004	0.06
Wisconsin	−0.006	−0.67
Wyoming	0.091	0.82

Dependent variable = log of fatality rate.

away from other, more dangerous roads; again, leading to a change in the statewide fatality rate.

To test these ideas we began by examining aggregate data. We combined the states into two groups: a test group, the states that raised their speed limits; and a control group, the states that did not. We calculated the change in overall fatality rates that occurred after the new speed limit: the test group improved 3.6% more than the control group.

We also found evidence that highway patrols had, in fact, shifted resources and drivers had shifted roads in the manner predicted by our theory.

We then turned to a regression analysis of the data from the individual 65 mph states. We used the basic model developed by Garber and Graham (with rates as the dependent variable) that holds constant the effects of long-term trend, driving exposure, seat belt laws, and economic factors; and fitted this to monthly time series data on a state-by-state basis. Using the statewide fatality rate as the dependent variable reversed the Garber/Graham results: the average decline in state-by-state fatality rates following the new speed limit was 3.4%.

Did the 3.4% decline in the 65 mph states represent the result of the new speed limit, or was it possibly the effect of some downward break in the long-term trend? To check this possibility, we ran the same regression model on the 55 mph states, giving them fake-65 mph dummy variables starting in June 1987. The estimated coefficients of the fake-65 mph dummies contradicted this alternative explanation—there was no evidence for a general shift in long-term fatality trend.

To obtain more efficient estimates of the speed limit effects, we then ran the analysis on the combined sample of 65 mph states, restricting them to a common estimate of the 65 mph dummy variable. This indicated that the new speed limit had produced a 5.1% decline in the fatality rate. To test the robustness of these results, we performed a Chow test by removing seven representative states from the sample: the resulting regression estimates were statistically similar to those of the full sample.

Taken as a whole, these different analyses lead to the conclusion that overall statewide fatality rates fell by 3.4% to 5.1% for the group of states that adopted the 65 mph limit.

Why did the new speed limit lower the fatality rate? (i) Drivers may have switched to safer roads—the aggregate data support this; (ii) highway patrols may have shifted resources to activities with more safety payoff—testimony by highway patrol chiefs supports their intention to do so; (iii) finally, although we had no data to examine it in this study, it is also possible that the new law caused a decline in speed variance: it might decline on the interstates as law-abiding drivers caught up with the speeders, and it might decline on other highways as their speeders switched to the interstates. Future research ought to be directed toward disentangling the relative contribution of these three factors.

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